



Cochrane
Library

Cochrane Database of Systematic Reviews

Hydrocolloid dressings for donor sites of split thickness skin grafts (Protocol)

Derwin R, Moore ZEH, Webster J

Derwin R, Moore ZEH, Webster J.

Hydrocolloid dressings for donor sites of split thickness skin grafts.

Cochrane Database of Systematic Reviews 2017, Issue 4. Art. No.: CD012634.

DOI: 10.1002/14651858.CD012634.

www.cochranelibrary.com

TABLE OF CONTENTS

HEADER	1
ABSTRACT	1
BACKGROUND	1
OBJECTIVES	2
METHODS	2
ACKNOWLEDGEMENTS	6
REFERENCES	6
APPENDICES	8
CONTRIBUTIONS OF AUTHORS	13
DECLARATIONS OF INTEREST	14
SOURCES OF SUPPORT	14

[Intervention Protocol]

Hydrocolloid dressings for donor sites of split thickness skin grafts

Rosemarie Derwin¹, Zena EH Moore¹, Joan Webster²

¹School of Nursing & Midwifery, Royal College of Surgeons in Ireland, Dublin, Ireland. ²Nursing and Midwifery Research Centre, Royal Brisbane and Women's Hospital, Herston, Australia

Contact address: Rosemarie Derwin, School of Nursing & Midwifery, Royal College of Surgeons in Ireland, 123 St Stephens Green, Dublin, D.2, Ireland. rosemariederwin@rcsi.ie.

Editorial group: Cochrane Wounds Group.

Publication status and date: New, published in Issue 4, 2017.

Citation: Derwin R, Moore ZEH, Webster J. Hydrocolloid dressings for donor sites of split thickness skin grafts. *Cochrane Database of Systematic Reviews* 2017, Issue 4. Art. No.: CD012634. DOI: 10.1002/14651858.CD012634.

Copyright © 2017 The Cochrane Collaboration. Published by John Wiley & Sons, Ltd.

ABSTRACT

This is a protocol for a Cochrane Review (Intervention). The objectives are as follows:

To assess the effects of hydrocolloid dressings on the healing of donor sites of split thickness skin grafts (STSGs).

BACKGROUND

Description of the condition

A skin graft involves removal of a section of tissue from one part of the body. The skin graft is composed of two layers of the skin, the epidermis (the outermost layer of skin) and the dermis (lies beneath the epidermis and contains connective tissue, hair follicles, and sweat glands) (Reddy 2014). The graft is then transplanted to another area of the body (Grabb 1991). A split thickness skin graft (STSG) involves excision of the epidermis and part of the dermis (Coull 1991). STSGs are used to close defects in the tissues which cannot be closed by simply bringing together wound edges for closure with stitches (Fowler 1998). STSGs are also used to cover chronic skin defects and can accelerate healing of the wounded area (Voineskos 2009).

The part of the body from which the graft is taken is known as the donor site (Wiechula 2003). The donor site heals by a process of reepithelialisation (restoration of the thin tissue forming the

outer layer of the body's surface). Epithelial cells migrate across the wound surface from the rim of the wound and the edges of various structures in the dermal layer, such as sebaceous glands and hair follicles. This process results in an epithelial cover of the STSG donor site, usually within seven to 14 days. The rate of healing can be variable and is affected by factors such as the depth, site and size of the wound, along with the age of the patient (Joanna Briggs Institute 2002; Wiechula 2003).

The most common reason why a person undergoes a STSG is because of a burn injury (Hop 2014). Burns are the fourth most common form of trauma (Peck 2011). Globally, burns and fires account for over 300,000 deaths each year, however the majority of burns do not result in fatal consequences (WHO 2008). In the USA, in 2004, 32,500 hospital stays were directly related to the management of a burn injury, with almost one in every three of these patients undergoing a skin graft (Milenkovic 2007). In a further study conducted in the Netherlands of 1768 patients referred to a specialist burns centre, 13% underwent reconstructive surgery, and for 32% of these patients, skin grafting was the treat-

ment of choice (Hop 2014). STSGs are also used to treat chronic wounds (Chen 2009; Rose 2014) including leg ulcers and traumatic wounds (Shores 2007). Seghers 2014 conducted a prospective pilot study on recalcitrant leg ulcers, and the findings suggested that STSGs may be beneficial for treating such ulcers. A further study by Anderson 2012 of 107 diabetic patients concluded that STSGs may be useful in the treatment of non-healing diabetic foot ulcers. These findings are supported by Rose 2014 amongst individuals with high risk diabetic foot ulcers.

Description of the intervention

Donor sites are usually covered with a dressing until the donor site is healed. An ideal donor site dressing should accelerate wound healing, cause minimal pain during wound dressing application and prevent infection, which results in minimal scarring (Akita 2006). Dressings can be classified as either moist or non-moist, based on the state of the dressing upon initial application (Voineskos 2009). Hydrocolloids are one of a group of dressings that work on the principle of moist wound healing (Thomas 1992). The concept of moist wound healing arises from the work of Winter 1962 which showed that experimental wounds covered with an occlusive dressing were noted to re-epithelialise at a much faster rate than those exposed to the air. Winter 1962 concluded that wounds need moisture in order to heal at an optimal rate. Moist wound dressings prevent desiccation and the deepening of wounds, reduce the risk of mechanical damage to healing tissue at removal, and provide an environment that results in more rapid healing and reduced pain (Wiechula 2003). Despite the advantages of using this approach, it is evident that non-moist wound healing methods are frequently used in the management of STSG donor sites (Voineskos 2009).

Skin dressings can be further classified into medicated and non-medicated dressings. The dressings most frequently used in the management of STSG donor sites are mesh gauze, polyurethane semipermeable transparent films, fibre dressings, retention dressings and hydrocolloids. Hydrocolloids are classed as a medicated dressing (Wiechula 2003; Haughton 2012). These dressings are made of gel-forming agents and gelatin (Thomas 2008). They are combined with elastomers and adhesives and are then applied to a carrier, for example, a foam or film. The end product is an adhesive, waterproof, wafer-like dressing (Thomas 2008).

How the intervention might work

Hydrocolloids provide a moist wound environment which promotes new tissue formation and wound closure. In addition, because of the occlusive nature of hydrocolloids dressings, they are also thought to offer an effective barrier to the movement of micro-organisms into the wound (Fletcher 2012). When the dressing comes in contact with fluid exuding from a wound, it absorbs the

fluid and forms a gel over the wound. This results in the creation of a moist wound/dressing interface (Thomas 1992). Initially, the dressing is impermeable (does not allow fluid to pass through), however, over time the dressing becomes more permeable, this enables fluid to be released from the dressing through a process known as water vapour transmission (Thomas 1992). The purpose of this is to enhance the ability of the dressing to manage the amount of fluid exuding from the wound (Thomas 1992). If the wound is highly exuding, the dressing will not be able to handle the fluid very well. Hydrocolloid dressings are principally indicated for low to moderately exuding wounds, as they have a limited absorptive capacity (Fletcher 2012; Brölmann 2013).

Why it is important to do this review

A variety of different dressings and topical agents are routinely employed in practice, however, the optimum choice of dressing for donor sites of STSGs remains uncertain (Voineskos 2009; Eskes 2011; Lars 2013). Patient safety is at the heart of healthcare delivery, and avoidance of unnecessary complications associated with clinical care is considered to be a fundamental patient right (WHO 2016). Tan 2009 suggests that any delay in healing of the donor site will result in complications. The donor sites of STSGs are often the source of delayed healing, with considerable pain and discomfort to the patient (Shaileshkumar 2012). Complications can escalate costs, prolonging hospitalisation. There has been no recent previous review to guide practice in this area. Consequently, there is a need to examine current evidence for hydrocolloid dressings for treating donor sites of STSGs.

OBJECTIVES

To assess the effects of hydrocolloid dressings on the healing of donor sites of split thickness skin grafts (STSGs).

METHODS

Criteria for considering studies for this review

Types of studies

We will include published and unpublished randomised controlled trials (RCTs), including trials that randomise by groups (cluster-RCTs), irrespective of language of report. Cross-over trials will be eligible if results are available for the first phase, i.e. before the cross-over to a different treatment occurs. We will also include abstracts from conference proceedings if they meet our inclusion

criteria and we are able to extract data for at least one of our outcome measures. We will not include editorials or letters that refer to RCTs.

Types of participants

People of any age who have one or more donor sites, following a split thickness skin graft (STSG), for any underlying cause. We will not apply any restrictions on wound size or exudate level. We will include individuals who have had a previous skin graft on the donor site.

Types of interventions

The interventions of interest are hydrocolloid dressings. The comparisons for this review are:

- different types of hydrocolloid dressings compared with other types of dressings;
- different types of hydrocolloid dressings compared with another therapy such as topical agents (a topical agent is a cream or an ointment that is applied directly to the wound);
- different types of hydrocolloid dressings compared with no dressing;
- different types of hydrocolloid and another therapy, versus other therapy and
- different types of hydrocolloid dressings compared with other hydrocolloid dressings.

We will include studies which evaluate intervention schedules, including other therapies, provided that these treatments were delivered in a standardised way across the trial arms.

Types of outcome measures

We list primary and secondary outcomes below. If a study is otherwise eligible (i.e. correct study design, population and intervention/comparator), but does not report a listed outcome, then we will contact the study authors, where possible, to establish whether an outcome of interest here was measured but not reported. We will include RCTs that are eligible with respect to participant and intervention characteristics regardless of whether or not they report one of our specified outcomes.

We will report outcome measures at the latest time point available for a study (assumed to be length of follow-up if not specified) and the time point specified in the methods as being of primary interest (if this is different from latest time point available). For all outcomes we will class outcome measures from:

- \leq 1 week to 8 weeks as short-term;
- > 8 weeks to 16 weeks as medium-term; and
- > 16 weeks as long-term.

Primary outcomes

The primary outcome of interest is complete wound healing, and we will include the following:

- time to complete healing; we will record whether this has been correctly analysed, using techniques that account for data censoring, and with adjustment for prognostic covariates such as baseline size;
- the proportion of wounds healed at the completion of the trial period;
- an objective measure of wound healing, such as, absolute or percentage change in wound area.

Secondary outcomes

We will include the following secondary outcomes:

- pain (measured at any time with any validated instrument, e.g. visual analogue scale);
- health-related quality of life (using any validated measure such as the World Health Organization Quality of Life (WHOQOL) - BREF, 36-item Short Form (SF-36), 12-item Short Form (SF12), measured at completion of the study);
- wound infection; we will accept authors' definitions of wound infection;
- safety (treatment-related adverse events, measured at any time);
- cost of treatment (measured at completion of the study).

Search methods for identification of studies

Electronic searches

We will search the following electronic databases to identify relevant RCTs:

- the Cochrane Wounds Specialised Register (to present).
- the Cochrane Central Register of Controlled Trials (CENTRAL; latest issue) in the Cochrane Library.
- Ovid MEDLINE (including In-Process & Other Non-Indexed Citations, MEDLINE Daily and Epub Ahead of Print) (1946 to present).
- Ovid Embase (1974 present).
- EBSCO CINAHL Plus (1937 to present).

The draft search strategy for CENTRAL is presented in [Appendix 1](#). We will adapt this strategy to search Ovid MEDLINE, Ovid Embase and EBSCO CINAHL.

We will combine 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 will combine the Embase search with the Ovid Embase randomised trials filter terms developed by the UK Cochrane Centre ([Lefebvre 2011](#)). We will

combine the CINAHL search with the randomised trials filter terms developed by the Scottish Intercollegiate Guidelines Network (SIGN 2017). We will not impose any restrictions with respect to language, date of publication or study setting.

We will also search the following clinical trials registries for ongoing and unpublished studies:

- ClinicalTrials.gov (clinicaltrials.gov);
- WHO International Clinical Trials Registry Platform (apps.who.int/trialsearch/Default.aspx);
- EU Clinical Trials Register (clinicaltrialsregister.eu).

Searching other resources

We will search reference lists of all included studies and other relevant publications, such as systematic reviews and guidelines. We will contact manufacturers of dressings used in the treatment of STSGs and experts in the field to ask for information relevant to this review. We will also contact the authors of relevant publications to identify any completed or ongoing trials.

Data collection and analysis

Selection of studies

Two review authors (RD and ZM) will independently assess titles and, where available, abstracts of the studies identified by the search strategy against the eligibility criteria for inclusion in the review; discrepancies will be resolved through discussion or by consultation with the third review author (JW). We will obtain full versions of potentially relevant studies, and two review authors will independently screen these against the inclusion criteria. Any differences in opinion will be resolved by discussion and by consultation with the third review author. If deemed necessary, the Cochrane Wounds editorial base will be consulted. We will complete a PRISMA flowchart to summarise this process (Liberati 2009).

Data extraction and management

We will extract and summarise details of the eligible studies. Where possible, we will extract data by treatment group for the prespecified interventions and outcomes in this review. Two review authors (RD and ZM) will independently extract data. Discrepancies will be resolved through discussion or by consultation with the third review author (JW). Where data are missing from reports, we will attempt to contact the study authors and request this information. Where a study with more than two intervention arms is included, we will only extract data from intervention and control groups that meet the eligibility criteria. Where the reported baseline data relate to all patients rather than to those in relevant treatment arms, we will extract the data for the whole

trial and note this. Where studies have been reported in multiple publications/reports, we will obtain all publications. Whilst the study will be included only once in the review, we will extract data from all reports to ensure maximal relevant data are obtained. We will collect outcome data for relevant time points as described in the [Types of outcome measures](#).

Where possible, we will extract:

- bibliographic data, including date of completion/publication;
- country of origin;
- unit of randomisation (participant/wound);
- unit of analysis;
- trial design e.g. parallel, cluster;
- care setting;
- number of participants randomised to each trial arm and number included in final analysis;
 - eligibility criteria and key baseline participant data including cause, depth, extent (area/proportion of total body surface area) and location of split skin graft, ages of patients, and whether they have a diagnosis of infection at baseline;
 - details of treatment regimen received by each group;
 - duration of treatment;
 - details of any co interventions;
 - primary and secondary outcome(s) (with definitions and, where applicable, time points);
 - outcome data for primary and secondary outcomes (by group);
 - duration of follow-up;
 - withdrawals (by group);
 - withdrawals (by group) due to adverse events;
 - adverse events including data on frequency and nature of the events;
 - publication status of study;
 - source of funding for trial.

Assessment of risk of bias in included studies

Two review authors will independently assess the included studies using the Cochrane tool for assessing risk of bias (Higgins 2011a). We will resolve any discrepancies by discussion with a third author (JW). A detailed description of criteria for a judgement of 'low risk', 'high risk' or 'unclear risk' of bias is available (see [Appendix 2](#)). This tool addresses six specific domains: namely, sequence generation, allocation concealment, blinding, incomplete outcome data, selective outcome reporting and other issues (e.g. extreme baseline imbalance). We will assess binding of participants and care providers and blinding of outcome assessors. We will assess selective outcome reporting, by seeking trial protocols, when available, then outcomes in the protocol and the published report will be compared. If the protocol is not available, we will contact the study authors for additional information. We will complete a 'Risk of bias' table for each eligible study and will present an assessment

of risk of bias using a 'Risk of bias' summary figure which will present the judgements in cross tabulation. This display of internal validity indicates the weight the reader may give to the results of each study.

Measures of treatment effect

For dichotomous outcomes, we will calculate the risk ratio (RR) plus 95% confidence intervals (CIs). For continuous outcomes, we will calculate the difference in means plus 95% CIs. Where data are available from trial reports or authors, we will calculate the hazard ratio and corresponding 95% CI to assess the intervention effect of time-to-event data, such as time to complete healing. If data are not available we plan to extrapolate estimates, where possible, using other data from similar studies (Parmar 1998; Tierney 2007). The inclusion of skewed data in a meta-analysis may result in biased estimates of effect, unless 'normalised' by log transformation, however, if scale data have finite upper and lower limits we will apply an easy rule of thumb in order to test for skewness. If the standard deviation, when doubled, is greater than the mean, it is unlikely that the mean is the centre of the distribution and will not be entered into the meta-analysis (Altman 1996). Where continuous data have less obvious finite boundaries the situation is more problematic and may be a matter of judgement. If we find relevant data that are skewed we will present the data in 'Other data' tables. It is also possible that different tools may be used to measure the same outcome (for example, quality of life and pain). We will collect data only from those studies where scales have been validated and are self-reported or completed by an independent rater or relative (not the therapist or investigator). We will use the SMD as the summary statistic in any meta-analysis of such data (Deeks 2011).

Unit of analysis issues

If cluster-RCTs are found and have been analysed using appropriate methods, we plan to follow methods outlined in 16.3.3 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011b). That is, we will extract summary statistics and their standard errors from the paper and meta-analyse data using the generic inverse variance method. Where results in primary studies are not adjusted for clustering we will again extract the summary statistic and standard error and conduct an approximate analysis by multiplying the standard errors of the estimates by the square root of the design effect; where the design effect is calculated as $DE=1+(m-1)*ICC$, where m is the average cluster size. We plan to derive the intracluster correlation coefficient (ICC) from a similar trial in the review but, if this is not possible, will use an ICC of 0.1 and conduct a sensitivity analysis to test variations in the ICC. We will use the generic inverse variance method in Review Manager (RevMan) 5 for any meta-analysis of cluster-randomised designs (Higgins 2011b; RevMan 2014).

A second unit of analysis issue may occur if multiple donor sites in one individual receive the same intervention. In this case, we will consider individuals as clusters, and use the above methods.

Dealing with missing data

We will analyse by intention-to-treat wherever possible. If there is evidence of missing data, we will contact the study authors to request the information. If missing data are not available upon request from authors then the only option is to analyse the available data as reported (Gurusamy 2009). We will make explicit the assumptions of any methods used to cope with the missing data, for example, that the missing values were assumed to indicate a poor outcome. We will perform a sensitivity analysis to assess how sensitive results are to reasonable changes to the assumptions that are made. We will also address the potential impact of the missing data on the findings of the review in the discussion.

Assessment of heterogeneity

We will explore clinical or methodological heterogeneity by examining potentially influential factors, e.g. care setting, patient characteristics, methods, interventions and outcomes of studies. We will assess statistical heterogeneity by visual inspection of the forest plots and by using the Chi^2 test equal to or greater than 0.1, indicating no statistical heterogeneity and the I^2 statistic (Higgins 2003). The I^2 statistic examines the percentage of total variation across studies due to heterogeneity rather than to chance. Values of I^2 over 75% indicate a high level of heterogeneity. We will carry out statistical pooling on groups of studies which are considered to be sufficiently similar.

Assessment of reporting biases

Reporting biases arise when the dissemination of research findings is influenced by the nature and direction of results. Publication bias is one of a number of possible causes of 'small study effects', that is, a tendency for estimates of the intervention effect to be more beneficial in smaller RCTs. Funnel plots allow a visual assessment of whether small study effects may be present in a meta-analysis. A funnel plot is a simple scatter plot of the intervention effect estimates from individual RCTs against some measure of each trial's size or precision (Sterne 2011). We plan to present funnel plots for meta-analyses comprising 10 RCTs or more using RevMan 5 (RevMan 2014).

Data synthesis

Initially we will conduct a structured narrative summary of the studies reviewed and explore potential clinical or statistical heterogeneity. We will enter quantitative data into RevMan 5 and analyse the data using the RevMan 5 analysis software (RevMan 2014).

We plan to group RCTs evaluating different treatment comparisons together and analyse accordingly. We will pool data where trials are similar in terms of population, methods, interventions and outcomes using a fixed-effect model. We plan to test for heterogeneity between studies by performing the Chi² test ($P < 0.1$ signifying significant heterogeneity) and using the I² statistic to quantify the proportion of variation due to heterogeneity (Deeks 2011). In addition, we plan to report heterogeneity as absent (I² = 0% to < 25%); low (I² = 25% to 49%); moderate (I² = 50% to 74%); or high (I² = >75%). If there is evidence of heterogeneity (I² more than 25%), we will explore the source of heterogeneity using sensitivity and subgroup analyses. We will also consider clinical heterogeneity in any planned meta-analysis and, where this exists, we will present data from individual studies in tabular form. For dichotomous outcomes, we will calculate the RR plus 95% CI. For continuous outcomes, we will calculate the difference in means plus 95% CI. If pooling is possible across studies, we plan to present a pooled difference in means with 95% CIs for continuous outcomes. If studies measure the same outcome using different instruments, we will combine data using a SMD estimate. For time to healing, we plan to plot estimates of hazard ratios (HRs) with 95% CIs from study reports using the generic inverse method in RevMan 5 (RevMan 2014). Where hazard ratios are not reported we plan to extrapolate estimates, where possible, using other reported data (Parmar 1998; Tierney 2007).

'Summary of findings' tables

We plan to present the main results of the review in 'summary of findings' tables. These tables present key information concerning the quality of the evidence, the magnitude of the effects of the interventions examined, and the sum of the available data for the main outcomes (Schünemann 2011a). The 'summary of findings' tables also include an overall grading of the evidence related to each of the main outcomes using the GRADE approach. The GRADE approach defines the quality of a body of evidence with regard to the extent to which one can be confident that an estimate of effect or association is close to the true quantity of specific interest. The

quality of a body of evidence involves consideration of within-trial risk of bias (methodological quality), directness of evidence, heterogeneity, precision of effect estimates and risk of publication bias (Schünemann 2011b). We plan to present in the 'summary of findings' tables the following outcomes, which we believe to be the most important, both clinically and to the consumer:

- time to complete healing
- complete healing
- pain
- wound infection
- cost.

Subgroup analysis and investigation of heterogeneity

If sufficient data are available we will undertake the following subgroup analyses:

- type of setting (community, hospital (inpatient), hospital (outpatient))
- aetiological reason for the skin graft.

Sensitivity analysis

We will perform a sensitivity analysis by excluding studies of the lowest quality. In this sensitivity analysis, we will only include studies that are assessed as having a low risk of bias in all key domains, namely adequate generation of the randomisation sequence, adequate allocation concealment and blinding of outcome assessor, for the estimates of treatment effect.

This review is one of a suite of reviews looking at dressings for donor sites of split thickness skin grafts (STSGs).

ACKNOWLEDGEMENTS

The authors would like to acknowledge the contribution of the referees; Kurinchi Gurusamy, Fiona Whiter, Sarah Rhodes, Adolfo Tambella, Dinanda Kolbach, Ashley Yu and Clare Dooley for copy editing this review.

REFERENCES

Additional references

Akita 2006

Akita S, Akino K, Imaizumi T, Tanaka K, Anrako K, Yano H. A polyurethane dressing is beneficial for split-thickness skin-graft donor wound healing. *Burns* 2006;**32**(4):447–51.

Altman 1996

Altman DG, Bland JM. Detecting skewness from summary information. *BMJ* 1996;**313**:1200.

Anderson 2012

Anderson J, Wallin K, Spencer L. Split thickness skin grafts for the treatment of non-healing foot and leg ulcers in patients with diabetes: a retrospective review. *Diabetic Foot and Ankle* 2012;**3**(10):1–18.

Brölmann 2013

Brölmann F, Eskes A, Goslings J, Niessen F, De Bree R, Vahl C, et al. Randomized clinical trial of donor-site wound dressings after split-skin grafting. *British Journal of Surgery* 2013;**100**(5):619–27.

Chen 2009

Chen X, Wang XJ, Wang C, Zhang GA. Repair non-healing wound with artificial dermis and autologous skin graft. *Zhonghua Shao Shang Za Zhi* 2009;**25**(6):444–7.

Coull 1991

Coull A. Making sense of ... split skin graft donor sites. *Nursing Times* 1991;**87**(40):52–4.

Deeks 2011

Deeks JJ, Higgins JP, Altman DG, editor(s). Chapter 9: Analysing data and undertaking meta-analyses. In: Higgins JP, Green S, editor(s). *Cochrane Handbook for Systematic Reviews of Interventions* Version 5.1.0 (updated March 2011). The Cochrane Collaboration, 2011. Available from handbook.cochrane.org.

Eskes 2011

Eskes A, Brolmann F, Gerbens L, Ubbink D, Vermeulen H. Which dressing do donor site wounds need? Study protocol for a randomised control trial. *Trials* 2011;**12**(1):229–34.

Fletcher 2012

Fletcher J, Moore Z, Anderson I, Matsuzaki K. Pressure ulcers and hydrocolloids made easy. *Wounds International* 2012;**2**(4):1–6.

Fowler 1998

Fowler A, Dempsey A. Split thickness skin graft donor sites. *Journal of Wound Care* 1998;**7**(8):399–402.

Grabb 1991

Grabb WC, Smith JW. *Plastic Surgery*. 3rd Edition. Boston: Little Brown, 1991.

Gurusamy 2009

Gurusamy KS, Gluud C, Nikolova D, Davidson BR. Assessment of risk of bias in randomized clinical trials in surgery. *Journal of Surgery* 2009;**96**(4):342–9.

Haughton 2012

Haughton B, Herring C. Wound dressings - guidance for use. Wirral University Teaching Hospitals Foundation Trust. Clinical Guideline V1. August 2012. mm.wirral.nhs.uk/document_uploads/guidelines/Wounddressing-guidanceforuse,V1.pdf (accessed 10 April 2017).

Higgins 2003

Higgins JP, Thompson SG, Deeks JJ, Altman DG. Measuring inconsistency in meta-analyses. *BMJ* 2001;**327**(7414):557–60.

Higgins 2011a

Higgins JP, Altman DG, editor(s). Chapter 8: Assessing risk of bias in included studies. In: Higgins JP, Green S, editor(s). *Cochrane Handbook for Systematic Reviews of Interventions* Version 5.1.0 (updated March 2011). The Cochrane Collaboration, 2011. Available from handbook.cochrane.org.

Higgins 2011b

Higgins JP, Deeks JJ, Altman DG, editor(s). Chapter 16: Special topics in statistics. In: Higgins JP, Green S, editor(s). *Cochrane Handbook for Systematic Reviews*

of Interventions Version 5.1.0 (updated March 2011). The Cochrane Collaboration, 2011. Available from handbook.cochrane.org.

Hop 2014

Hop M, Polinder S, Van Der Vlies C, Middelkoop E, Van Bar M. Costs of burn care: a systematic review. *Wound Repair and Regeneration* 2014;**22**:436–50.

Joanna Briggs Institute 2002

Joanna Briggs Institute. Split thickness skin graft donor sites: post harvest management. Evidence based practice information sheets for health professionals. *Best Practice* 2002;**6**(2):1–6.

Lars 2013

Lars P, Giretzehner M, Trop M, Parvizi D, Spindel S, Schintler M, et al. The properties of the 'ideal' donor site dressing: results of a worldwide online survey. *Annals of Burns and Fire Disasters* 2013;**26**(3):136–41.

Lefebvre 2011

Lefebvre C, Manheimer E, Glanville J. Chapter 6: Searching for studies. In: Higgins JP, Green S, editor(s). *Cochrane Handbook for Systematic Reviews of Interventions* Version 5.1.0 (updated March 2011). The Cochrane Collaboration, 2011. Available from handbook.cochrane.org.

Liberati 2009

Liberati A, Altman D, Tetzlaff J, Mulrow C, Gotzsche P, Ioannidis J, et al. The PRISMA statement for reporting systematic reviews and meta-analyses of studies that evaluate health care interventions: explanation and elaboration. *PLoS Medicine* 2009;**6**:339:b2700.

Milenkovic 2007

Milenkovic M, Russo A, Elixhauser A. Hospital stays for burns, 2004. Healthcare Cost and Utilization Project Statistical Brief #25. January 2007. hcup-us.ahrq.gov/reports/statbriefs/sb25.pdf (accessed 10 April 2017).

Parmar 1998

Parmar MK, Torri V, Stewart L. Extracting summary statistics to perform meta-analyses of the published literature for survival endpoints. *Statistics in Medicine* 1998;**17**:2815–34.

Peck 2011

Peck MD. Epidemiology of burns throughout the world. Part 1: distribution and risk factors. *Burns* 2011;**37**: 1087–100.

Reddy 2014

Reddy S, El-Haddawi F, Fancourt M, Farrant G, Gilkison W, Henderson N, et al. The incidence and risk factors for lower limb skin graft failure. *Dermatology Research and Practice* 2014;**2014**:Article ID 582080. [DOI: 10.1155/2014/582080]

RevMan 2014 [Computer program]

Nordic Cochrane Centre, The Cochrane Collaboration. Review Manager 5 (RevMan 5). Version 5.3. Copenhagen: Nordic Cochrane Centre, The Cochrane Collaboration, 2014.

Rose 2014

Rose JF, Giovinco N, Mills JL, Najafi B, Pappatardo J, Armstrong DG. Split-thickness skin grafting the high risk diabetic foot. *Journal of Vascular Surgery* 2014;**59**(6): 1657–63.

Schünemann 2011a

Schünemann HJ, Oxman AD, Higgins JP, Vist GE, Glasziou P, Guyatt GH. Chapter 11: Presenting results and 'Summary of findings' tables. In: Higgins JP, Green S, editor(s). *Cochrane Handbook for Systematic Reviews of Interventions* Version 5.1.0 (updated March 2011). The Cochrane Collaboration, 2011. Available from handbook.cochrane.org.

Schünemann 2011b

Schünemann HJ, Oxman AD, Higgins JP, Deeks JJ, Glasziou P, Guyatt GH. Chapter 12: Interpreting results and drawing conclusions. In: Higgins JP, Green S, editor(s). *Cochrane Handbook for Systematic Reviews of Interventions* Version 5.1.0 (updated March 2011). The Cochrane Collaboration, 2011. Available from handbook.cochrane.org.

Seghers 2014

Seghers AC, Goh BK, Tan SH, Tang BY. Simplified noncultured autologous cell grafting for the treatment of chronic non healing ulcers: the six-well plate technique. *Clinical and Experimental Dermatology* 2014;**39**(5):620–3.

Shaileshkumar 2012

Shaileshkumar ME, Pramod M, Vishwanath G, Basarkod SI, Chhaya J, Rajani P. A clinical trial to assess the efficacy of hydrocolloid versus parafin gauze dressing for split thickness skin graft donor site treatment. *Journal of Clinical and Diagnostic Research* 2012;**6**(1):72–5.

Shores 2007

Shores JF, Gabriel A, Gupta S. Skin substitutes and alternatives: a review. *Advanced Skin Wound Care* 2007;**20**: 493–508.

SIGN 2017

Scottish Intercollegiate Guidelines Network (SIGN). Search Filters. sign.ac.uk/methodology/filters.html#random (accessed 10 April 2017).

Sterne 2011

Sterne JA, Egger M, Moher D. Chapter 10: Addressing reporting biases. In: Higgins JP, Green S, editor (s). *Cochrane Handbook for Systematic Reviews of*

Interventions Version 5.1.0 (updated March 2011). The Cochrane Collaboration, 2011. Available from handbook.cochrane.org.

Tan 2009

Tan P, Chin Ho W, Song C. The use of Urgotul in the treatment of partial thickness burns and split-thickness skin graft donor sites: a prospective control study. *International Wound Journal* 2009;**6**(4):295–300.

Thomas 1992

Thomas S. Hydrocolloids. *Journal of Wound Care* 1992;**1** (2):27–30.

Thomas 2008

Thomas S. Hydrocolloid dressings in the management of acute wounds: a review of the literature. *International Wound Journal* 2008;**5**(5):602–13.

Tierney 2007

Tierney J, Stewart L, Ghersi D, Burdett S, Sydes M. Practical methods for incorporating summary time-to-event data into meta-analysis. *Trials* 2007;**8**(16):8–16.

Voineskos 2009

Voineskos SH, Ayeni OA, McKnight L, Thoma A. Systematic review of skin graft donor-site dressings. *Plastic and Reconstructive Surgery* 2009;**124**(1):297–306.

WHO 2008

World Health Organization. The global burden of disease: 2004 update. who.int/healthinfo/global_burden_disease/GBD_report_2004update_full.pdf?ua=1 (accessed 10 April 2017).

WHO 2016

World Health Organization. Unsafe medical care is a major source of morbidity and mortality throughout the world. who.int/patientsafety/research/country_studies/en/index2.html (accessed 18 November 2016).

Wiechula 2003

Wiechula R. The use of moist wound-healing dressings in the management of split-thickness skin graft donor sites: a systematic review. *International Journal of Nursing Practice* 2003;**9**:S9–S17. [DOI: 10.1046/j.1322-7114.2003.00417.x]

Winter 1962

Winter GD. Formation of a scab and the rate of epithelialisation of superficial wounds in the skin of the young domestic pig. *Nature* 1962;**193**:293–4.

* Indicates the major publication for the study

APPENDICES

Appendix 1. The Cochrane Central Register of Controlled Trials (CENTRAL) provisional search strategy

#1 MeSH descriptor: [Skin Transplantation] explode all trees
 #2 MeSH descriptor: [Transplantation, Autologous] explode all trees
 #3 MeSH descriptor: [Transplant Donor Site] explode all trees
 #4 (((split next thick*) or split-thick* or “split skin” or split-skin or “partial dermal” or partial-dermal or partial next thick* or partial-thick*) near/3 graft*):ti,ab,kw
 #5 ((skin or derm*) next transplant*):ti,ab,kw
 #6 STSG:ti,ab,kw
 #7 donor site:ti,ab,kw
 #8 {or #1-#7}
 #9 MeSH descriptor: [Bandages, hydrocolloid] explode all trees
 #10 MeSH descriptor: [Colloids] explode all trees
 #11 (hydrocolloid* or “gel forming” or gel-forming or activheal or alione or askina or biatain or comfeel or cutimed or duoderm or flexigran or granuflex or hydrocoll or “nu derm” or nuderm or nu-derm or tegaderm or “ultec pro” or aquacel or urgoclean or versiva or cutinova or medihoney or tegasorb or dermafilm or replicare or signadress or algoplaque or varihesive):ti,ab,kw
 #12 ((carboxymethylcellulose or carboxymethyl-cellulose or gelatin* or NaCMC or CMC or HCD) near/3 (dressing* or bandage*)):ti,ab,kw
 #13 {or #9-#12}
 #14 #8 and #13

Appendix 2. 'Risk of bias' criteria

Random sequence generation	Random sequence generation
Selection bias (biased allocation to interventions) due to inadequate generation of a randomised sequence	Selection bias (biased allocation to interventions) due to inadequate generation of a randomised sequence
Criteria for a judgement of low risk of bias	<p>The investigators describe a random component in the sequence generation process such as:</p> <ul style="list-style-type: none"> ● referring to a random number table; ● using a computer random number generator; ● coin tossing; ● shuffling cards or envelopes; ● throwing dice; ● drawing of lots; ● minimisation*. <p>*Minimisation may be implemented without a random element, and this is considered to be equivalent to being random</p>
Criteria for a judgement of high risk of bias	<p>The investigators describe a non-random component in the sequence generation process. Usually, the description would involve some systematic, non-random approach, for example, generating the sequence:</p> <ul style="list-style-type: none"> ● by odd or even date of birth; ● by some rule based on date (or day) of admission;

(Continued)

	<ul style="list-style-type: none"> • by some rule based on hospital or clinic record number. <p>Other non-random approaches happen much less frequently than the systematic approaches mentioned above and tend to be obvious. They usually involve judgement or some method of non-random categorisation of participants, for example, allocation:</p> <ul style="list-style-type: none"> • by clinician's judgement; • by participant's preference; • based on the results of a laboratory test or a series of tests; • by availability of the intervention. 	
Criteria for a judgement of unclear risk of bias	Insufficient information about the sequence generation process is available to permit a judgement of 'low risk' or 'high risk'	
Allocation concealment Selection bias (biased allocation to interventions) due to inadequate concealment of allocations prior to assignment		Allocation concealment Selection bias (biased allocation to interventions) due to inadequate concealment of allocations prior to assignment
Criteria for a judgement of low risk of bias	<p>Participants and investigators enrolling participants could not foresee assignment because one of the following, or an equivalent method, was used to conceal allocation:</p> <ul style="list-style-type: none"> • central allocation (including telephone, web-based and pharmacy-controlled randomisation); • sequentially numbered drug containers of identical appearance; • sequentially numbered, opaque, sealed envelopes. 	
Criteria for a judgement of high risk of bias	<p>Participants or investigators enrolling participants could possibly foresee assignments and thus introduce selection bias, such as allocation based on:</p> <ul style="list-style-type: none"> • use of an open random allocation schedule (e.g. a list of random numbers); • assignment envelopes without appropriate safeguards (e.g. envelopes were unsealed, non-opaque or not sequentially numbered); • alternation or rotation; • date of birth; • case record number; • any other explicitly unconcealed procedure. 	
Criteria for a judgement of unclear risk of bias	Insufficient information available to permit a judgement of 'low risk' or 'high risk'. 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	
Blinding of participants and personnel Performance bias due to knowledge of the allocated interventions by participants and personnel during the study		Blinding of participants and personnel Performance bias due to knowledge of the allocated interventions by participants and personnel during the study

(Continued)

Criteria for a judgement of low risk of bias	<p>Either of the following.</p> <ul style="list-style-type: none"> • No blinding or incomplete blinding, but the review authors judge that the outcome is 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.
Criteria for a judgement of high risk of bias	<p>Either of the following.</p> <ul style="list-style-type: none"> • No blinding or incomplete blinding, and the outcome 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, and the outcome is likely to be influenced by lack of blinding.
Criteria for a judgement of unclear risk of bias	<p>Either of the following.</p> <ul style="list-style-type: none"> • Insufficient information available to permit a judgement of 'low risk' or 'high risk'. • The study did not address this outcome.

<p>Blinding of outcome assessment Detection bias due to knowledge of the allocated interventions by outcome assessors</p>	<p>Blinding of outcome assessment Detection bias due to knowledge of the allocated interventions by outcome assessors</p>
---	---

Criteria for a judgement of low risk of bias	<p>Either of the following.</p> <ul style="list-style-type: none"> • No blinding of outcome assessment, but the review authors judge that the outcome measurement is not likely to be influenced by lack of blinding. • Blinding of outcome assessment ensured, and unlikely that the blinding could have been broken.
Criteria for a judgement of high risk of bias	<p>Either of the following.</p> <ul style="list-style-type: none"> • No blinding of outcome assessment, and the outcome measurement is likely to be influenced by lack of blinding. • Blinding of outcome assessment, but likely that the blinding could have been broken, and the outcome measurement is likely to be influenced by lack of blinding.
Criteria for a judgement of unclear risk of bias	<p>Either of the following.</p> <ul style="list-style-type: none"> • Insufficient information available to permit a judgement of 'low risk' or 'high risk'. • The study did not address this outcome.

<p>Incomplete outcome data Attrition bias due to amount, nature or handling of incomplete outcome data</p>	<p>Incomplete outcome data Attrition bias due to amount, nature or handling of incomplete outcome data</p>
--	--

Criteria for a judgement of low risk of bias	<p>Any one of the following.</p> <ul style="list-style-type: none"> • No missing outcome data. • Reasons for missing outcome data are unlikely to be related to the outcomes or to the magnitude of the effect.
---	---

(Continued)

	<p>to true outcome (for survival data, censoring is unlikely to introduce bias).</p> <ul style="list-style-type: none"> • 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 the observed event risk is not enough to have a clinically relevant impact on the intervention effect estimate. • For continuous outcome data, the plausible effect size (difference in means or standardised difference in means) among missing outcomes is not enough to have a clinically relevant impact on observed effect size. • Missing data have been imputed using appropriate methods.
<p>Criteria for a judgement of high risk of bias</p>	<p>Any one of the following.</p> <ul style="list-style-type: none"> • Reason for missing outcome data is 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 the observed event risk is enough to induce clinically relevant bias in the intervention effect estimate. • For continuous outcome data, the plausible effect size (difference in means or standardised difference in means) among missing outcomes is enough to induce clinically relevant bias in the observed effect size. • 'As-treated' analysis is done with substantial departure of the intervention received from that assigned at randomisation. • Potentially inappropriate application of simple imputation.
<p>Criteria for a judgement of unclear risk of bias</p>	<p>Either of the following.</p> <ul style="list-style-type: none"> • Insufficient reporting of attrition/exclusions to permit a judgement of 'low risk' or 'high risk' (e.g. number randomised not stated, and no reasons for missing data provided). • The study did not address this outcome.
<p>Selective reporting Reporting bias due to selective outcome reporting</p>	<p>Selective reporting Reporting bias due to selective outcome reporting</p>
<p>Criteria for a judgement of low risk of bias</p>	<p>Either of the following.</p> <ul style="list-style-type: none"> • The study protocol is available and all of the study's prespecified (primary and secondary) outcomes that are of interest in the review have been reported in the prespecified way. • The study protocol is not available but it is clear that the published reports include all expected outcomes, including those that were prespecified (convincing text of this nature may be uncommon).

(Continued)

<p>Criteria for a judgement of high risk of bias</p>	<p>Any one of the following.</p> <ul style="list-style-type: none"> ● Not all of the study's prespecified primary outcomes have been reported. ● One or more primary outcomes is reported using measurements, analysis methods or subsets of the data (e.g. subscales or subgroups) that were not prespecified. ● One or more reported primary outcomes were not prespecified (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.
<p>Criteria for a judgement of unclear risk of bias</p>	<p>Insufficient information available to permit a judgement of 'low risk' or 'high risk'. It is likely that the majority of studies will fall into this category</p>
<p>Other bias Bias due to problems not covered elsewhere in this table</p>	<p>Other bias Bias due to prob</p>
<p>Criteria for a judgement of low risk of bias</p>	<p>The study appears to be free of other sources of bias.</p>
<p>Criteria for a judgement of high risk of bias</p>	<p>There is at least one important risk of bias. For example, the study:</p> <ul style="list-style-type: none"> ● has extreme baseline imbalance; or ● had a potential source of bias related to the specific study design used; ● had an inappropriate influence of funders due to industry-initiated protocols; ● has been claimed to have been fraudulent; or ● had some other problem. <p>Or in cluster-randomised trials there is:</p> <ul style="list-style-type: none"> ● recruitment bias (differential participant recruitment in clusters for different interventions); ● baseline imbalance; ● loss of clusters; ● incorrect analysis; ● comparability with individually randomised trials
<p>Criteria for a judgement of unclear risk of bias</p>	<p>There may be a risk of bias, but there is either:</p> <ul style="list-style-type: none"> ● insufficient information to assess whether an important risk of bias exists; or ● insufficient rationale or evidence that an identified problem will introduce bias.

CONTRIBUTIONS OF AUTHORS

- Rosemarie Derwin: conceived the review question; co-ordinated the protocol development; produced the first draft of the protocol; developed the protocol; contributed to writing and editing the protocol; approved the final version of the protocol prior to submission and is guarantor of the protocol.
- Zena Moore: conceived the review question; produced the first draft of the protocol; developed the protocol; contributed to writing and editing the protocol and approved the final version of the protocol prior to submission.
- Joan Webster: conceived the review question; produced the first draft of the protocol; developed the protocol; contributed to writing and editing the protocol and approved the final version of the protocol prior to submission

Contributions of the editorial base:

Susan 'O' Meara (Editor): edited the protocol; advised on methodology interpretation and content; approved the final protocol prior to submission.

Gill Rizzello (Managing Editor): coordinated the editorial process; advised on content; edited the protocol.

Reetu Child and Naomi Shaw (Information Specialists): designed and edited the search strategy and search methods sections.

Ursula Gonthier (Editorial Assistant): edited the reference sections.

DECLARATIONS OF INTEREST

Rosemarie Derwin: none known.

Zena Moore: has received an honorarium for speaking at a professional meeting for Vancive.

Joan Webster: none known.

SOURCES OF SUPPORT

Internal sources

- School of Nursing, Royal College of Surgeons, Dublin, Ireland.

External sources

- National Institute for Health Research, UK.

This project is supported by the National Institute for Health Research, via Cochrane Infrastructure funding to Cochrane Wounds. The views and opinions expressed herein are those of the authors and do not necessarily reflect those of the Systematic Reviews Programme, NIHR, NHS or the Department of Health.