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Cochrane Database of Systematic Reviews 2017, Issue 1. Art. No.: CD012500.

DOI: 10.1002/14651858.CD012500.

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[Intervention Protocol]

Organised trauma systems and designated trauma centres for improving outcomes in injured patients

Michael Mwandri¹, Barclay Stewart^{2,3}, Timothy C Hardcastle^{4,5}, Andres M Rubiano⁶, Russell L Gruen⁷

¹School of Clinical Medicine, University of KwaZulu-Natal, Durban, South Africa. ²Department of Surgery, University of Washington, Seattle, Washington, USA. ³Department of Surgery, Kwame Nkrumah University of Science & Technology, Kumasi, Ghana. ⁴Trauma Service, Inkosi Albert Luthuli Central Hospital, Mayville, South Africa. ⁵Trauma Training Unit, University of KwaZulu-Natal, Durban, South Africa. ⁶MEDITECH Foundation, Neiva, Colombia. ⁷Lee Kong Chian School of Medicine, Nanyang Technological University, Singapore, Singapore

Contact address: Michael Mwandri, School of Clinical Medicine, University of KwaZulu-Natal, 719 Umbilo Road, Durban, 4001, South Africa. mwanrister@gmail.com.

Editorial group: Cochrane Effective Practice and Organisation of Care Group.

Publication status and date: New, published in Issue 1, 2017.

Citation: Mwandri M, Stewart B, Hardcastle TC, Rubiano AM, Gruen RL. Organised trauma systems and designated trauma centres for improving outcomes in injured patients. *Cochrane Database of Systematic Reviews* 2017, Issue 1. Art. No.: CD012500. DOI: 10.1002/14651858.CD012500.

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ABSTRACT

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

To assess the effects of organised trauma systems and designated trauma centres for improving outcomes in injured patients, specifically patient outcomes and adverse effects or harms.

BACKGROUND

Description of the condition

Trauma is the injury of an individual due to a number of potential mechanisms, including road traffic crashes, falls, contact with heat or hot objects or substances, weapons, electricity, bites and stings, and others. Trauma can be intentional (e.g. suicide attempt, assault) or unintentional, and either blunt (e.g. fall) or penetrating (e.g. gunshot wound). Furthermore, injuries can be defined by severity, from minor to severe or life-threatening to uniformly fatal. Given the frequency that these mechanisms occur in societies and the potential for serious consequences from each of them, injury is responsible for a large global health and economic burden.

Almost five million people die from trauma each year, which makes it a serious global health problem (Mock 2015). Additionally, trauma incurs 180 million disability-adjusted life years annually (Mathers 2004). Ninety per cent of this burden occurs in low- or middle-income countries (LMICs) (WHO 2013). In 2005, countries lost an estimated USD 167.8 billion from road traffic injuries alone (Dalal 2013). In response, organised trauma care has developed and evolved over many decades, and has led to the formation of trauma systems (Mullins 1999; Nijs 2003). There is emerging evidence that these systems may be effective in reducing the burden from trauma in LMICs and thus, such systems have been replicated in many high-income countries (HICs) (West 1983; Shackford 1986; Mullins 1994; Nicholl 1997; Atkin 2005). Despite LMICs carrying the greatest trauma burden (WHO 2013;

Hardcastle 2013a; Hardcastle 2013b), few LMICs have adopted a formal trauma system. The World Health Organization (WHO) has recommended developing resource-appropriate trauma systems, as described in their guideline documents, 'Prehospital Trauma Care Systems' and 'Guidelines for Essential Trauma Care' (Mock 2004; Sasser 2005). Further recommendations are based on feasibility of care considering resource limitations, pre-hospital capability, and geographical challenges in LMICs (Hardcastle 2013a). Improvement in the planning and organisation of human and physical resources and institution of systemised trauma care is likely to improve outcomes of injured patients, especially where these services are poor or non-existent (Calvello 2013). However, the quality of evidence used for evaluating these systems remains insufficient.

Description of the intervention

Description of the intervention considers the template for intervention description, evaluation and replication guide (TIDieR) (Hoffman 2014). The name of the intervention is 'organised trauma systems', a pre-planned approach to the provision of the spectrum of trauma care. An organised trauma system stipulates how and when patients are moved to and between providers, who provides care, where it is provided, when it is to be available, and how the costs are recovered. Organised trauma systems offer initial care and triage patients to the most appropriate level of care according to their needs. Organised trauma systems necessarily include pre-hospital and hospital services, and rehabilitation services. Trauma systems may be centralised within a specified geographical area (regionalised). Pre-hospital care is made up of a communication system, initial medical services at the area of injury occurrence, patient transport services (i.e. ambulance services), and includes necessary medical treatment during transportation. Hospital care is provided by hospitals designated as level I to IV trauma centres on the basis of trauma volume, range of available services, staffing requirements, educational/research capacity, and support of injury prevention initiatives. A level I centre has a full range of immediately available services and leadership in teaching, research, and injury prevention and control. Level II centres are able to initiate definitive care for all injuries but with a lesser capacity in research and certain services compared to level I. Level III centres provide assessment, stabilisation, and basic emergency operations. Level IV centres provide basic trauma life support (Mock 2004; Hardcastle 2011; ACS 2014). Additionally, an organised trauma system often includes programmes that support injury prevention initiatives, such as promotion of helmet-wearing among cyclists, appropriate seat belt use, education against alcohol and other illicit substance use among drivers and cyclists, and law enforcement for traffic offenders. Other preventive measures may involve necessary changes in legislation and engineering designs for cars and roads (Rivara 1997; Mock 2001; ACS 2014).

How the intervention might work

An organised trauma system may improve the outcome of severely injured patients by identifying those who require multidisciplinary care and promptly transporting them to an appropriate level of care. This system creates high levels of skills and expertise among providers through high patient volume and concentration of resources and therefore may lead to more intensive utilisation of resources. Regionalised trauma systems and trauma centres provide leadership and organisation of trauma care to the designated population (Cole 2016). Trauma systems often support injury prevention programmes that may reduce the burden of injury. Trauma systems may also reduce barriers to care, improve the quality of care provided, use resources more efficiently, strengthen the trauma provider workforce by offering technical support to lower levels of care (level III and IV), offer a smooth referral mechanism within the system, and improve community health. Generally, centralisation of healthcare systems has been documented to de-skill lower facilities, and delay patients' treatment (Atkinson 2004). However, trauma system centralisation may minimise delays by offering appropriate treatment at the scene and fast transportation, while continuing necessary treatment. Models of trauma systems available in HICs are costly and may adversely affect healthcare provision for other services in low-income countries (Mock 2004).

Why it is important to do this review

Although several studies have reported reductions in the burden of injury and improvements in injury care after the creation of a trauma system, the study designs are weak (e.g. uncontrolled before-and-after studies) (Shackford 1986; Atkin 2005). Furthermore, the relatively successful trauma systems in the USA have not been widely replicated in other regions (West 1983; Guss 1989; Mullins 1994), particularly in LMICs (Nicholl 1997). The lack of support for trauma system development in LMICs may be, in part, due to the lack of evidence for their effectiveness in low-resource settings.

Because of the large burden of trauma globally and high cost of resources for establishing trauma systems, it is important to assess their effectiveness with quality evidence. Doing so is particularly important for LMICs given the greater burden of trauma and critical financial restraints. A quality evaluation of trauma system effectiveness is likely to inform health policy and resource allocation decisions, and ultimately lead to improved care for the injured.

OBJECTIVES

To assess the effects of organised trauma systems and designated trauma centres for improving outcomes in injured patients, specifically patient outcomes and adverse effects or harms.

METHODS

Criteria for considering studies for this review

Types of studies

We will include the following types of studies.

- Randomised trials.
- Non-randomised trials with at least two intervention sites and two control sites.
- Controlled before-after studies that have at least two intervention and two control sites.
- Interrupted time series studies that have a defined point of time when the intervention occurred and must have a minimum of three points before and after the intervention.

Types of participants

We will include healthcare professionals providing care to patients who suffer major trauma (e.g. Injury Severity Score ≥ 15). We will exclude studies that include patients who predominantly suffer fragility fractures and those who have not been admitted to hospital.

Types of interventions

The intervention of interest will be the establishment of an organised trauma system compared to non-trauma system care (current normal standard care for most LMICs). An organised trauma system is defined as a pre-planned approach to the provision of the spectrum of trauma services, including but not limited to, injury prevention and control initiatives, timely transport from the scene of the injury to the trauma care facility, availability of trauma care providers and services when needed, and rehabilitation (WHO 2013).

Types of outcome measures

Primary outcomes

- Patient outcomes (health outcomes, such as, mortality, morbidity, and recovery)
- Adverse effects or harms
 - Clinical, monitoring, or medication errors
 - Delays in standards of trauma care
 - Specific clinical adverse effects, such as sepsis, hospital-acquired or healthcare-associated infections, or surgical complications

Secondary outcomes

- Utilisation and access
 - Utilisation of services. It is expected that more seriously injured patients will be delivered to higher level trauma centres (level I and II) in a more timely fashion and with greater survival in trauma systems than in non-trauma systems. Utilisation of service indicators will include:
 - ◊ volume of trauma patients brought to the appropriate trauma centre
 - ◊ bed occupancy
 - ◊ length of hospital stay (including length of stay in intensive care unit)
 - ◊ appropriate trauma procedures performed
 - Access to services. This includes timely transporting of the severely injured to appropriate care, and it will depend on other services, such as ambulance services, availability of appropriate trauma care providers and services (e.g. trauma surgical services, intensive care, blood bank services). We will measure access to services using indicators such as:
 - ◊ patients' waiting time to access trauma services
 - ◊ injury-appropriate service time
 - ◊ ambulance service call volume, etc.
 - Social outcomes (e.g. community participation or uptake in injury prevention and control initiatives); examples include:
 - ◊ training lay persons to provide pre-hospital care in low- and middle-income countries (LMICs)
 - ◊ bystander care educational programmes
 - ◊ law enforcement for traffic offenders at the community level
- Quality of care provided
 - Adherence to standards of trauma care with tangible patient benefit (e.g. trauma care audit filters proposed by the American College of Surgeons or other groups) (Willis 2008; Juillard 2009; Shafi 2009; ACS 2014; Stewart 2016)
- Equity
 - Timely access to trauma care and differential effects of outcomes across advantaged and disadvantaged populations
- Knowledge
 - Population knowledge regarding injury prevention
 - Healthcare provider knowledge or skill regarding standards of injury care, performance in trauma moulage scenarios

Outcomes involving system organisation in trauma need to be measured over a long period (10 to 16 years) (Mock 2004). However, other outcomes may be measured over a shorter time horizon (a few months to a few years) (Cole 2016).

Reporting of the outcomes listed here will not be an inclusion criterion for the review and we will include studies regardless of the assessed outcomes.

Search methods for identification of studies

We will conduct the searches with the advice and assistance of the Cochrane Effective Practice and Organisation of Care (EPOC) Group. We will not impose any restrictions on publication status, language, or country of publication.

Electronic searches

The Cochrane EPOC Group Information Specialist will develop the search strategies in consultation with the review authors. We will search the Cochrane Database of Systematic Reviews (CDSR) and the Database of Abstracts of Reviews of Effects (DARE) for primary studies included in related systematic reviews. We will search the following databases (from inception).

- Cochrane Central Register of Controlled Trials (CENTRAL), including the Cochrane EPOC Group's Specialised Register.
- MEDLINE (from 1946) In-Process and other non-indexed citations, OvidSP.
- Embase (from 1974), OvidSP.
- Cumulative Index to Nursing and Allied Health Literature (CINAHL) (from 1980), EbscoHost.
- Directory of Online African Journals (DOAJ).

We will use two methodology search filters to limit retrieval to appropriate study designs: a modified version of the Cochrane Highly Sensitive Search Strategy (sensitivity-and precision-maximising version-2008 revision; [Lefebvre 2011](#)) to identify randomised trials ([Higgins 2011](#)), and a Cochrane EPOC Group methodology filter to identify non-randomised trial designs.

Searching other resources

Grey literature

We will conduct a grey literature search to identify studies not indexed in the databases listed above; sources will include the sites listed below. We will document any additional sources in the review.

- OpenGrey (www.opengrey.eu).
- Grey Literature Report (New York Academy of Medicine; www.greylit.org).

Trial registries

- World Health Organization (WHO) International Clinical Trials Registry Platform (ICTRP) (www.who.int/ictrp/en).
- ClinicalTrials.gov, the US National Institutes of Health (NIH) (clinicaltrials.gov).
- Health economic studies database

- National Health Service-Economic Evaluation Database (NHS EED) for identifying studies that meet the study design.

We will also perform the following.

- We will review reference lists of all included studies, relevant systematic reviews/primary studies.
- We aim to contact authors of relevant studies/reviews to clarify reported published information and to seek unpublished results/data.
- We will contact researchers with expertise relevant to the review topic/EPOC interventions.
- We aim to conduct cited reference searches for all included studies in ISI Web of Knowledge; and screen individual journals and conference proceedings (e.g. handsearch).

We will also cross-check the references of included studies and relevant systematic reviews.

We will provide appendices for all strategies used, including a list of sources screened and relevant reviews/primary studies reviewed.

Data collection and analysis

Selection of studies

We will download all titles and abstracts retrieved by electronic searching to a reference management database and remove duplicates. Two review authors (MM and BS) will independently screen titles and abstracts for inclusion. We will code all the potentially eligible studies as either 'retrieve' (eligible or potentially eligible/unclear) or 'do not retrieve'. We will retrieve the full-text study reports/publications. Two review authors (MM and BS) will independently screen the full-text articles and identify studies for inclusion. We will identify and record reasons for exclusion of the ineligible studies. We will resolve any disagreement through discussion or, if required, we will consult a third review author (TH). We will list excluded studies with reasons for exclusion in the 'Characteristics of excluded studies' tables. We will collate multiple reports of the same study so that each study, rather than each report, is the unit of interest in the review. In addition, we will provide any information we can obtain about ongoing studies. We will record the selection process in sufficient detail to complete a PRISMA flow diagram ([Liberati 2009](#)), and a 'Characteristics of excluded studies' tables.

Data extraction and management

We will use a standard data collection form adapted from the Cochrane EPOC Group for extracting study characteristics and outcome data ([EPOC 2013a](#)). Two review authors (MM and BS) will independently extract the following study characteristics from

included studies and transfer the information into Review Manager 5 (RevMan 5) (RevMan 2014).

- Methods: study design, number of study centres and location, study setting, withdrawals, date of study, and follow-up
- Participants: number, mean age, age-range, gender, severity of condition, diagnostic criteria, inclusion criteria, exclusion criteria, and other relevant characteristics
- Interventions: intervention components, comparison, and fidelity assessment
 - We will use the template for intervention description and replication (TIDieR) criteria to assess completeness of reporting interventions (Hoffman 2014); we will use the following criteria
 - ◇ Brief name of the intervention
 - ◇ Description of rationale, theory, or goal of essential elements to the intervention
 - ◇ Description of physical or informational material used for the intervention
 - ◇ Description of each of the procedures, activities or processes used in the intervention, including enabling or supporting activities
 - ◇ Description of providers (background, expertise, training)
 - ◇ Mode of delivery of the intervention (face-to-face, by telephone, internet)
 - ◇ Description of location(s) infrastructure or relevant features of where intervention occurred
 - ◇ Description of: frequency, intensity, duration of the intervention
 - ◇ If intervention is tailored or adapted, describe why, when, and how
 - ◇ If intervention was modified, description of the changes
 - ◇ How well was their intervention planned (if fidelity or adherence was assessed describe how, by who, and if strategies for maintaining fidelity were employed)
 - ◇ How well was the intervention actually carried out, description of how the intervention adhered to the plan
 - Outcomes: main and other outcomes specified and collected, and time points reported
 - For economic outcome of studies focusing on resource utilisation only, we will use the guidance provided by the Cochrane and Campbell Economic Methods Group (methods.cochrane.org/economics), that includes the following selected criteria
 - ◇ Is the chosen time horizon appropriate to include relevant costs and consequences?
 - ◇ Is the actual perspective chosen appropriate?
 - ◇ Are all important and relevant costs for each alternative identified?
 - ◇ Are all costs measured appropriately in physical units?

- ◇ Are all important variables, whose values are uncertain, appropriately subjected to sensitivity analysis?
- ◇ Do the conclusions follow from the data reported?
 - ◇ Does the study discuss the generalisability of the results to other settings and patient/client groups?
 - ◇ Does the article indicate that there is no potential conflict of interest of study researcher(s) and funder(s)?
 - ◇ Are ethical and distributional issues discussed appropriately?
- Notes: funding for trials, notable conflicts of interest of trial authors, and ethical approval

Two review authors (MM and BS) will independently extract outcome data from included studies. We will note in the 'Characteristics of included studies' tables if outcome data were reported in an unusable way. We will resolve disagreements by consensus or by involving a third review author (TH).

Assessment of risk of bias in included studies

Two review authors (MM and BS) will independently assess risk of bias for each study using the criteria outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011), and the guidance from the Cochrane EPOC Group (EPOC 2013b). We will resolve any disagreement by discussion or by involving a third review author (TH). We will assess the risk of bias according to the following domains.

For randomised trials, non-randomised trials, and controlled before-after studies, we will assess the following domains.

- Random sequence generation.
- Allocation concealment.
- Blinding of participants and personnel.
- Blinding of outcome assessment.
- Incomplete outcome data.
- Selective outcome reporting.
- Baseline outcomes measurement.
- Baseline characteristics.
- Other bias.

For interrupted time series studies, we will assess the following.

- If the intervention is independent of other changes.
- If the shape of the intervention effect is prespecified.
- If the intervention is unlikely to affect data collection.
- If knowledge of the intervention is adequately prevented.
- If incomplete outcome data are adequately addressed.
- If the study is free from selective outcome reporting.
- If the study is free from other risks of bias.

We will judge each potential source of bias as either high, low, or unclear and provide a quote from the study report together with a justification for our judgment in the 'Risk of bias' table. We will summarise the 'Risk of bias' judgements across different studies for each of the domains listed. We will consider blinding separately

for different key outcomes, where necessary. Where information on risk of bias relates to unpublished data or correspondence with a trial author, we will note this in the 'Risk of bias' table. When considering treatment effects, we will take into account the risk of bias for the studies that contribute to that outcome.

Measures of treatment effect

We will estimate the effect of the intervention using the following.

- Risk ratios (RRs), adjusting for baseline differences for dichotomous data, together with the appropriate associated 95% confidence interval (CI).
- Mean difference (MD) or standardised mean difference (SMD) for continuous data, together with the 95% CI.

Where appropriate, measurement of treatment effect will use the same scale (i.e. quality of life, disability scales). We will ensure that an increase in scores for continuous outcomes can be interpreted in the same way for each outcome, explain the direction to the reader, and report where the directions were reversed, if this was necessary.

Measurement of treatment effect for cluster-randomised trials, randomised trials, and controlled before-after studies

We will extract the intervention effect estimate reported for outcomes in the included studies along with the P value, 95% CI, and the method used in their calculation. For dichotomous outcomes we will use RRs, and for continuous outcomes we will use SMDs. Ratios greater than 1 and differences greater than 0 between the control and intervention groups will represent benefit for the intervention group.

Measurement of treatment effect for interrupted time series studies

For interrupted time series studies, we will either use a regression analysis with time trends before and after the intervention, adjusted for autocorrelation and any periodic changes, or autoregressive integrated moving average (ARIMA) analysis. We will report outcome results as changes in level and slope. If analysis or reporting is not appropriate, we will re-analyse according to the recommendation given in the Cochrane EPOC Group guideline (Ramsay 2003; EPOC 2013c).

Unit of analysis issues

We will perform analysis at the same level as the allocation for the intervention and control group to avoid unit of analysis errors. For clustering designs, such as cluster-randomised trials, we will perform analysis, adjusting for clustering. We will extract and re-analyse results not adjusted for clustering. If we find a unit of analysis error, and there is insufficient information to allow re-analysis, we will contact the original study authors to obtain necessary

information; if we are unsuccessful, we will not report the CI and P value, and we will describe the incident as a 'unit of analysis error'.

Dealing with missing data

We will state missing data on the collection and extraction form; if data are missing at random, we will ignore their absence and perform analysis. If data are not missing at random, we will contact the original study authors for additional information; if unsuccessful, we will perform a sensitivity analysis to detect the impact of missing data (Higgins 2011).

Assessment of heterogeneity

We will investigate heterogeneity by visual inspection of forest plots and the Chi² test. Where there is no substantial heterogeneity (I² statistic is less than 50%) we will perform a meta-analysis (Higgins 2011). If we identify substantial heterogeneity and if there are an adequate number of included studies (more than 10), we will perform subgroup analyses for prespecified subgroups that are either of the following.

- High-income country (HIC) settings and low- and middle-income country (LMIC) settings.
- Adult trauma patients and paediatric trauma patients.

Assessment of reporting biases

We will assess reporting bias by performing the following.

- We will compare the outcome of studies plotted in a matrix for indicating unreported outcomes.
- We will search protocols, abstracts, or trial registries in databases, such as PubMed, and compare listed outcomes with the reported ones in the related published studies.
- We will compare the methods with result sections of published studies to detected unreported outcomes.

We will contact primary authors to supply the missing information, and if unsuccessful, we will perform a sensitivity analysis (Higgins 2011).

Data synthesis

We will pool data from studies we judge to be clinically homogeneous using RevMan 5 (RevMan 2014). We will undertake meta-analyses only when this is meaningful, i.e. if the treatments, participants, and the underlying clinical question are similar enough for pooling to make sense. When we encounter skewed data we will note that the data are skewed and consider the implication of this. Where multiple trial arms are reported in a single trial, we will include only the relevant arms. If two comparisons must be entered into the same meta-analysis, we will halve the control group to avoid double-counting.

We do not plan to conduct a full economic analysis given the marked variation in studies we will uncover and the inherent risk of significant heterogeneity. Therefore, in accordance with Cochrane guidelines, we will provide a narrative summary of economic results instead of performing pooled analyses.

'Summary of findings' table

We will assess the certainty of evidence across multiple studies with similar interventions and outcomes using the GRADE approach, as described in the Cochrane EPOC Group worksheet for preparing 'Summary of findings' tables (EPOC 2013d). We will rate the certainty of evidence as follows (EPOC 2016).

- High-certainty of evidence: this research provides a very good indication of the likely effect. The likelihood that the effect will be substantially different is low.
- Moderate-certainty of evidence: this research provides a good indication of the likely effect. The likelihood that the effect will be substantially different is moderate.
- Low-certainty of evidence: this research provides some indication of the likely effect. However, the likelihood that it will be substantially different is high.
- Very low-certainty of evidence: this research does not provide a reliable indication of the likely effect. The likelihood that the effect will be substantially different is very high.

We will create a 'Summary of findings' table using the following outcomes: patient outcomes, adverse effects, utilisation of services, access to services, quality of care provided, and knowledge.

Two review authors will independently assess the certainty of the evidence (high, moderate, low, and very low) using the five GRADE considerations (study limitations, consistency of effect, imprecision, indirectness, and publication bias) as it relates to the main outcomes (Guyatt 2008; EPOC 2013d). We will use methods and recommendations described in Section 8.5 and Chapter 12 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011), the Cochrane EPOC Group worksheets (EPOC 2013d), and GRADEpro software (GRADEpro GDT 2014). We will justify all decisions to downgrade or upgrade the quality of the included studies using footnotes and make comments to aid readers' understanding of the review where necessary. We will consider whether there is any additional outcome information that we are unable to incorporate into meta-analyses, will note this in the comments, and will state if it supports or contradicts the information from the meta-analyses. If it is not possible to meta-analyse the data we will present results in a narrative 'Summary of findings' table format (EPOC 2013d).

Subgroup analysis and investigation of heterogeneity

We plan to carry out the following subgroup analyses.

- HIC settings and LMIC settings.
- Adult trauma patients and paediatric trauma patients.

The rationale for the first subgroup analysis is that HIC settings and LMIC settings differ in both human and physical resources. Such differences will almost certainly result in heterogeneity and require separate analyses.

The rationale for the second subgroup analysis is that patients of different ages have particular needs, and therefore require different resources, and this may lead to different outcomes.

For both subgroup analyses, we will assess the following.

- Patient outcomes
- Adverse effects or harms

We will apply a test of interaction to assess statistically significant differences between subgroups.

Sensitivity analysis

We will perform sensitivity analyses by employing multiple imputation methods (Higgins 2011), when the following occur.

- There are 'data missing not at random', and if efforts to obtain additional information from primary study authors are unsuccessful.
- If there are studies with high risk of bias included.
- When we have performed re-analysis (i.e. in a cluster-randomised trial where the intracluster correlation coefficient was not considered initially) for checking the stability of our results.

ACKNOWLEDGEMENTS

We acknowledge the help and support of the Cochrane Effective Practice and Organisation of Care (EPOC) Group. Also, we thank the following people who provided comments to improve the protocol: Denise O'Connor (EPOC Editor), Emma Tavender (EPOC Managing Editor), Clare Dooley (EPOC Assistant Managing Editor), Paul Miller (EPOC Information Specialist), Orlaith Burke (EPOC Statistical Editor), Luke Vale (EPOC Economics Editor), Pierre Durieux (EPOC Editor), Fiona Lecky (Peer Referee), and Deirdre Walshe (Copy-editor). We acknowledge the contribution of Glyn Estebanez who helped immensely in writing the initial draft of this protocol, we also acknowledge Jose Alarcon and Juan Carlos Puyana who contributed to the initial drafts and have continuously improved this manuscript through their comments.

We have based the [Methods](#) section of this protocol on a standard template used by Cochrane EPOC.

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* Indicates the major publication for the study

APPENDICES

Appendix I. Description and inclusion criteria for study designs

Study designs included in Cochrane EPOC reviews			
Suggested terms	Notes	Definition	Exclusions
Randomised trial	Instead of randomised controlled trial , which is redundant.	An experimental study in which people are allocated to different interventions using methods that are random	Studies with only one intervention or control site. We recommend only including cluster randomised trials, non-randomised cluster trials, and controlled before-after studies with at least two intervention sites and two control sites In studies with only one intervention or control site, the intervention (or comparison) is completely confounded by the study site, making it difficult to attribute any observed differences to the intervention rather than to other site-specific variables
Non-randomised trial	Instead of controlled clinical trial. EPOC reviews do not include clinical trials (and randomised trials are also controlled clinical trials). Also instead of 'quasi-randomised controlled trials', which is used to mean different things by different authors	An experimental study in which people are allocated to different interventions using methods that are not random	We recommend only including controlled before-after studies with at least two intervention sites and two control sites
Controlled before-after study	Instead of controlled before-and-after.	A study in which observations are made before and after the implementation of an intervention, both in a group that receives the intervention and in a control group that does not	We recommend only including controlled before-after studies with at least two intervention sites and two control sites Studies in which data collection is not contemporaneous in study and control sites during the pre- and post-intervention periods of the study and/or does not use identical methods of measurement

(Continued)

Interrupted time series study	Use 'study' instead of 'design' or 'analysis'.	A study that uses observations at multiple time points before and after an intervention (the 'interruption'). The design attempts to detect whether the intervention has had an effect significantly greater than any underlying trend over time	Studies that do not have a clearly defined point in time when the intervention occurred, and at least three data points before and three after the intervention
Repeated measures study	Not in the EPOC checklist.	An interrupted time series study where measurements are made in the same individuals at each time point	Not applicable.

Appendix 2. Search strategies

This is the proposed MEDLINE search strategy (EPOC 2013a); we will adjust the strategy, as appropriate, for other databases.

1. ((plan* or develop* or institution or institute? or organi#ation* or organi#e* or designat* or stipulat* or dedicat* or implement*) adj2 (trauma* or polytrauma*)).ti,ab
2. ((polytrauma* or trauma*) adj2 (system? or network?)).ti,ab
3. traumatology/og
4. (trauma* or polytrauma*).ti,ab.
5. Regional Medical Programs/
6. 4 and 5
7. ((impact* or effect* or evaluat* or implement* or develop* or assess* or outcome* or centrali#ation or centrali#ed or regionali#ation or regionali#ed) adj5 (trauma centre* or trauma center* or trauma service? or trauma care)).ti,ab
8. trauma centers/og
9. or/1-3,6-8
10. randomized controlled trial.pt.
11. controlled clinical trial.pt.
12. multicenter study.pt.
13. pragmatic clinical trial.pt.

(Continued)

14. (randomis* or randomiz* or randomly).ti,ab.

15. groups.ab.

16. (trial or multicenter or multi center or multicentre or multi centre).ti

17. (intervention? or effect? or impact? or controlled or control group? or (before adj5 after) or (pre adj5 post) or ((pretest or pre test) and (posttest or post test)) or quasiexperiment* or quasi experiment* or pseudo experiment* or pseudoexperiment* or evaluat* or time series or time point? or repeated measur*).ti,ab

18. non-randomized controlled trials as topic/

19. interrupted time series analysis/

20. controlled before-after studies/

21. or/10-20

22. exp animals/

23. humans/

24. 22 not (22 and 23)

25. review.pt.

26. meta analysis.pt.

27. news.pt.

28. comment.pt.

29. editorial.pt.

30. cochrane database of systematic reviews.jn.

31. comment on.cm.

32. (systematic review or literature review).ti.

33. or/24-32

34. 21 not 33

35. 9 and 34

CONTRIBUTIONS OF AUTHORS

Andres M Rubiano and Russell L Gruen conceived the protocol.

Michael Mwanri and Barclay Stewart designed the protocol.

Timothy Hardcastle co-ordinated the protocol.

Michael Mwanri, Barclay Stewart, and Timothy Hardcastle wrote the protocol.

Russell L Gruen and Andres M Rubiano provided general advice on the protocol.

DECLARATIONS OF INTEREST

Michael Mwandri has no known conflicts of interest.

Barclay Stewart received a US National Institutes of Health (NIH)/Fogarty Global Health Research Fellow Grant (R25TW009345). This poses no conflict of interest to the current work.

Timothy Hardcastle is a consultant to a number of private institutions regarding emergency care or disaster planning for which he was reimbursed for time, travel, and expertise on an ad-hoc basis. In the same way he has received reimbursement for expert case review and testimony as a medico-legal expert in his field, again on an ad-hoc basis. This work was as an expert of the University of KwaZulu-Natal with a PhD in emergency systems development and disaster medicine. He has received speaker invitations to various national or international meetings or courses as an invited plenary speaker and the inviting organisations have funded some or all of his expenses as their guest. For most of these activities there was little profit and the activities are at most peripherally related to the work contained in this Cochrane EPOC review.

Andres M Rubiano has no known conflicts of interest.

Russell L Gruen has no known conflicts of interest.

SOURCES OF SUPPORT

Internal sources

- No sources of support supplied

External sources

- The Australian Satellite of the Effective Practice and Organisation of Care (EPOC) Group receive funding from the National Health and Medical Research Council, Australia.