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Emergency training for in-hospital-based healthcare providers: effects on clinical practice and patient outcomes (Protocol)

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[Intervention Protocol]

Emergency training for in-hospital-based healthcare providers: effects on clinical practice and patient outcomes

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ABSTRACT

This is the protocol for a review and there is no abstract. The objectives are as follows:

1. To assess the effects of emergency training for in-hospital-based healthcare providers on patient outcomes
2. To assess the effects of emergency training for in-hospital-based healthcare providers on clinical care practices or organisational practice or both
3. To identify any essential components of effective emergency training programmes for in-hospital-based healthcare providers

BACKGROUND

Healthcare professionals strive to provide safe and effective clinical care, but suboptimal emergency care is a frequently identified factor in adverse outcomes for patients with acute conditions. A number of reports have identified training in emergencies, in particular, as key to improving outcomes for patients (IOM 2000; ERC 2010; CMACE 2011).

Training is a logical way for staff to develop their skills to respond

effectively to relatively rare emergency situations. However, despite more than a decade of research, little evidence exists. The knowledge of the best way to equip staff with the myriad skills they require to deal effectively with stressful live clinical situations remains a challenge (Calvert 2013).

There is an increasing recognition that there needs to be training for both technical skills and human factors in the form of situational awareness and teamwork training (Shapiro 2004; Calvert

2013). In order to achieve these goals, there are a huge number of different, often expensive, training courses available to health professionals, and the way this emergency training is implemented is not uniform (Anderson 2005). This lack of uniformity is further compounded by the availability of adequately trained staff to deliver the training in different locations (Anderson 2005; Calvert 2013).

The effectiveness and limitations of different models of training for these emergency situations remains unclear. This uncertainty arises due to the heterogeneity of training models that are implemented and studied. In addition, there is wide variation in how these training models are evaluated and reported. Currently no standardised evaluation tool exists, and many of the published outcomes are based on self reporting or subjective assessment by observers.

Identifying the most effective methods and essential elements for successful emergency training will provide a useful guide to those designing, implementing, and evaluating training. The utilisation of this knowledge will ensure that healthcare providers are given the best opportunity to gain the skills they need to provide the best possible emergency care to their patients.

Description of the condition

Training of healthcare professionals to effectively manage emergency situations presents different challenges to training staff to provide routine care, in part due to the rarity of cases (Smith 2013). Emergency situations differ between specialities, but all are defined as “serious, unexpected, and often dangerous situations requiring immediate action” (OED 2014). For the purposes of this review, an emergency situation will be one in which immediate action is required. Examples include cardiac or respiratory arrest, failed intubation, major haemorrhage, shoulder dystocia during childbirth, severe sepsis, and tension pneumothorax. These situations can arise either in emergency settings, for example in the emergency department, or in elective settings where staff have to respond to a patient’s evolving condition, for example a failed intubation in theatre.

Training for emergencies is different to that for routine care. This is because whether the training is interactive or didactic, it can be backed up by ‘on the job’ reinforcement. The ability to spend time refining skills outside a high-pressure environment means that a training programme does not have to perform the function of fully preparing staff for a new situation. However, for emergency situations, it is crucial that professionals work efficiently, both individually and as a team, even if it is the first time they have encountered the clinical situation or worked together. This requirement for comprehensive preparation has led to the development of training interventions to address the clinical and human factors in the emergency response.

Description of the intervention

This review will examine training interventions preparing healthcare professionals for emergency situations. The review will consider training for interventions performed within in-hospital settings, as part of the clinical role of staff. We will consider these in-hospital settings to be any facility-based care setting that provides comprehensive secondary or tertiary clinical care. This will include care delivered as a first point of contact in the emergency department.

This review will concentrate on in-hospital emergencies as a subset of all emergency care. There are other settings in which staff are trained to respond to emergencies either in office-based care settings or in the community. However, these settings are very different to the in-hospital environment and present different challenges. Within hospital settings it is usually possible to call upon a broader team of people and specialists to appropriately respond to and comprehensively manage an emergency. The focus in the community or primary care setting may be on the immediate management and transfer to an appropriate facility. Because of these differing priorities the interventions and measures of effectiveness are likely to be different. It is therefore important to consider these areas separately.

In this review, training refers to any form of educational session that has an interactive component. Interactive training courses can have many different formats; courses could, for example, have pre-course e-learning components, case-study discussions, or skills-drills. This presents a challenge when attempting to define or sub-categorise interactive training. We will use a model originally developed by Freeth to categorise the interactive training interventions (Freeth 2005; Hammick 2010), as follows:

- Exchange-based learning (e.g. debates, seminar or workshop discussions, case and problem-solving study sessions);
- Observation-based learning (e.g. work shadowing, joint client/patient consultations);
- Action-based learning (e.g. collaborative enquiry, problem-based learning, joint research, quality improvement initiatives, practice or community development projects); and
- Simulation-based learning (e.g. role-play, experiential group work, the use of clinical skills centres and integrating drama groups within teaching sessions).

In addition to the different types of interactive training, other elements within training programs can vary considerably. Courses may be administered locally, regionally, or nationally. Some high-profile courses conform to strict regulations in terms of content and delivery (ALS 2014), while others may be arranged to suit local needs without national accreditation. Some courses contain an element of assessment (ATLS 2015), while others are attendance based (PROMPT 2012). Courses may be multidisciplinary in faculty and attendees (CAT 2015), while others are run by and for only one profession (TEAM 2015). Some courses vary in duration from half a day, in BLS 2015 to several days, in ATLS 2015. The

speed of deterioration in knowledge and skills of participants and therefore how regularly training is required must also be considered by course conveners (Crofts 2007). Currently, some courses need to be repeated every four years (ATLS 2015), while others are annual (PROMPT 2012).

How the intervention might work

Interactive emergency training sessions enable healthcare professionals to familiarise themselves with required skills in a controlled environment. By having a pre-rehearsed systematic approach to an emergency, staff may then feel more able to concentrate on the current clinical situation rather than panicking about how to approach the emergency. It is this element of rehearsal and planning for emergencies that the interactive elements of the various types of training provide that could be the key to ensuring an appropriate emergency response by each individual and the team as a whole. If a systematic, evidence-based approach towards each in-hospital emergency could be adopted, improved outcomes for patients could result.

Why it is important to do this review

Previous reviews have focused on single aspects of training: modality or speciality (Siassakos 2009; Cook 2011). However, this review will be broad in scope for three reasons. Firstly, there is a paucity of high-quality studies investigating emergency training, so the number of studies to be examined will be increased with a cross-speciality review. Secondly, similar methods of training are applied across a range of emergencies, for example life support courses use similar methods to teach and assess candidates. Finally, although there are differences between training programmes, key essential elements to ensure successful emergency trainings may be clearly illuminated by examining programmes across specialities. This review will consider all interactive training interventions, both medical and surgical, to identify essential components for effective training common to all situations. It will focus on patient and organisational outcomes, rather than on acquisition of knowledge or user rating of training.

A huge number of training courses have been developed worldwide to provide healthcare workers with the skills they require to deal with emergencies. However, as was identified over a decade ago, these courses are often poorly described and even more infrequently studied (Black 2003). From evaluations that have been carried out we have seen some positive patient outcomes (Draycott 2006; Shoushtarian 2014). However, we have also begun to understand that training is not always effective, and in fact on occasion has been shown to coincide with worsening patient outcomes (MacKenzie 2007). If training programmes are evaluated as harmful, they should be quickly modified or abandoned. It is

essential that resources are channeled to increase the effectiveness of staff training and to maximise positive outcomes for patients. The focus of this review will be on changes in staff practice and patient outcomes rather than surrogate outcome measures of change demonstrated by training programmes. An example of a surrogate measure may include change in performance in 'mock code' scenarios (Donoghue 2009). Although these measures do provide a useful way to measure behavioural change as a direct result of the course, they do not represent how these skills translate into actual clinical practice in emergency settings.

Focused on actual behaviour change and patient outcomes in emergency situations, this review will provide an opportunity to identify the essential components of effective emergency training. If this can be achieved, then the factors that are required to deliver the best possible training can be incorporated into emergency training courses to facilitate improvement in patient and organisational outcomes across specialities.

OBJECTIVES

1. To assess the effects of emergency training for in-hospital-based healthcare providers on patient outcomes
2. To assess the effects of emergency training for in-hospital-based healthcare providers on clinical care practices or organisational practice or both
3. To identify any essential components of effective emergency training programmes for in-hospital-based healthcare providers

METHODS

Criteria for considering studies for this review

Types of studies

Few randomised controlled trials (RCTs) have been undertaken to investigate training interventions. Several factors may influence this. One may be that training enthusiasts often implement training sessions with the primary purpose of responding to their local training needs and evaluating impact locally. They may not have the time, resources, or motivation to develop a RCT. Other reasons may include national directives requiring that training in a particular skill be implemented, making it difficult to have a non-intervention control group. An example of this is in the widespread implementation of emergency obstetric training mandated by the NHS Litigation Authority in England (NHSLA 2012). For these reasons, we plan to include the following types of study designs (EPOC 2013):

- RCTs including cluster and step-wedge randomisation for cluster trials
- Non-randomised controlled trials, e.g. intervention allocation by geographical location
- Observational studies including:
 - Controlled before-after studies with a minimum of two intervention and two control groups
 - Interrupted time series, including repeated-measure studies that observe at least three time points before and after the intervention

We will include studies where the comparison is of:

- a group receiving training who are assessed in terms of their skills/ability pre- and postintervention;
- a group receiving a new training intervention compared with a control group receiving current standard training or no training;
- two or more groups receiving different types of training interventions, standard training, or no training, where at least one intervention is interactive.

Types of participants

We will consider healthcare professionals working within an in-hospital environment with potential for life-threatening, time-pressured emergencies in which treatments require rapid physical interventions. We will include studies that have taken place in public or private settings and in low-, middle-, or high-income settings. The healthcare worker can be at any stage of their professional career. We will exclude studies primarily investigating undergraduate/pre-service healthcare students.

We will consider the following specialties:

- Emergency medicine
- Obstetrics and gynaecology
- Anaesthetics
- Intensive care medicine
- Paediatrics, including neonatology
- All medical specialties
- All surgical specialties

We will exclude the following specialties:

- Ophthalmology
- Radiology
- Psychiatry

Types of interventions

We will consider all types of interactive educational intervention with the primary aim of improving the performance of hospital-based healthcare staff acting in in-hospital-based emergency situations, which they respond to as part of their clinical role. For the purposes of this review, we will consider training to be any type of educational intervention with an interactive component as categorised by Freeth (Freeth 2002).

The training course can lead to a recognised qualification, for example an 'Advanced Life Support provider' certificate, however it cannot form part of a primary qualification for health professionals, for example their primary medical or nursing degree.

The intervention can be delivered by a single methodology or by a combination of methods, for example online tutorials, lectures, and workshops. These interventions can take place individually or in groups. The intervention can involve the training of a single professional group or a multiprofessional team. The intervention can be of any duration and frequency and can occur in any setting (for example within the clinical department, local simulation room, or regional/national/international training centre).

Types of outcome measures

We will use Kirkpatrick's model of educational outcomes as modified and used by Freeth to develop a categorisation scheme for outcomes (Freeth 2002). We will only consider studies that examine level 3 (behavioural change) and level 4 (practice and patient outcomes) in this review. We will not include Level 1 (participant reaction) and 2 (acquisition of knowledge and skills) as actual outcomes for the review because despite their usefulness and wide use of the Kirkpatrick model, there remains a lack evidence for a clear causal chain between level 1 and 4 (Bates 2004), therefore the use of level 1 and 2 outcomes as a surrogate for level 3 and 4 outcomes cannot be assumed. In addition, because we are interested in identifying effects of training programmes on outcomes measured during or related to emergency clinical care, we will exclude the level 2 surrogate outcomes of knowledge and skills measured on simulators or actual patients in training and non-emergency settings. However, we will collect data on level 1 and 2 outcomes in the data abstraction form, as this may aid in understanding heterogeneity across studies.

Patient outcomes can include mortality and severe morbidity. In order to demonstrate changes in the management of the relatively rare events leading to these outcomes, studies would be required to have extremely large sample sizes. In response to this, proxy measures of patient outcome are often used in smaller-scale studies, and included in larger studies. These include the quality of clinical care provided or changes in organisational practice, which may be assessed by measuring adherence to guidelines, clinical errors, appropriate escalation to senior colleagues, and number of staff sick days.

The outcome measures addressed by individual studies will vary due to the nature of this review. Instead of an exhaustive list of outcome measures, we have presented in our primary and secondary outcomes a framework, based on the Kirkpatrick model, along which we will consider and categorise outcomes identified in the studies. To facilitate clarity of this framework for this review, we have added examples of outcomes that some studies may consider.

Primary outcomes

- Survival to hospital discharge
- Morbidity rate (e.g. incidence of hypoxic ischaemic encephalopathy in neonates, incidence of sepsis, incidence of residual neurological symptoms) or patient deterioration (e.g. number of cardiopulmonary arrests, requirement for care escalation to a higher dependency setting, Glasgow Coma Scale, deterioration in vital signs) specific to each speciality
- Protocol or guideline adherence (as assessed by observation or review of records, e.g. perimortem caesarean delivery during management of maternal cardiopulmonary resuscitation, time to first defibrillation in cardiopulmonary arrest)

Secondary outcomes

Patient outcomes

- Length of stay
- Patient-reported outcome measures (including complaints and patient satisfaction scales)
- Mortality

Clinical practice outcomes

- Skills during emergency situations (e.g. structured observed assessment of intubation procedure, observation of teamwork skills)
- Clinical endpoint of emergency situation (e.g. success of intubation, correct emergency ultrasound diagnosis)
- Appropriate escalation of care to seniors or different specialities
- Staff attitude (e.g. safety climate, teamwork, satisfaction, level of institutional support)
- Clinical errors (e.g. incorrect drug dosage)

Organisation-of-care outcomes

- Implementation of new systems (e.g. emergency boxes, treatment algorithms or proformas for reference during the emergency, one central emergency number to call)
- Development of local guidelines
- Institutional support (e.g. staff opinion, financial commitment)
- Staffing levels (e.g. workload rating, sick leave, turnover of staff)

Search methods for identification of studies

Electronic searches

We will design a sensitive search strategy to retrieve studies from the following electronic bibliographic databases:

- Cochrane Effective Practice and Organisation of Care (EPOC) specialised register via Reference Manager
- Cochrane Library via Wiley including the Cochrane Central Register of Controlled Trials (CENTRAL) and Database of Reviews of Effects (DARE)
- MEDLINE via OVID (1946 to present)
- EMBASE via OVID (1947 to present)
- CINAHL via Ebsco (1980 to present)
- ERIC via ProQuest (1980 to present)

Trial registries:

- World Health Organization International Clinical Trials Registry Platform (WHO ICTRP) (<http://www.who.int/ictrp/en/>)
- ClinicalTrials.gov, US National Institutes of Health (<http://clinicaltrials.gov/>)

We will use the sensitivity and precision-maximising filter for retrieving RCTs from MEDLINE and EMBASE as recommended in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011). To retrieve non-RCT designs we will use the EPOC methodology filter.

We will apply no language restriction. We will devise the search strategy for the OVID MEDLINE interface and then adapt it for the other databases. A draft electronic search strategy is provided in [Appendix 1](#).

Searching other resources

We will scan reference lists of included studies and any relevant systematic reviews identified. We will consult relevant individuals and organisations for information about unpublished or ongoing studies. We will also scan abstracts from relevant conferences including the AMEE: An International Association for Medical Education and International Conference on Resident Education.

Data collection and analysis

Selection of studies

Two review authors will independently screen all titles and abstracts for eligibility. We will retrieve full-text articles for all studies any review author deems to be potentially eligible. Two review authors will assess the full-text articles against the inclusion criteria. The review team will resolve by discussion any disagreements between two review authors.

We will keep a record of eligibility assessment for each full-text article and will present these in a 'Characteristics of excluded studies' table.

We will document the entire process for the selection of studies using a PRISMA flow chart to demonstrate the initial number of hits, hits after de-duplication, studies excluded at title and abstract screening stage, and finally the total numbers of excluded and included studies (Moher 2009).

Data extraction and management

Two review authors will independently extract data from each study onto a data collection form based upon the Cochrane EPOC data collection checklist (EPOC 2013a). All review authors will be involved in piloting the form on three included studies and amending it as necessary, ensuring that the form is fit for purpose and that there is consistency of approach. Due to the potential variability in assignment of the Kirkpatrick outcomes, it may be useful to consider the level of intraobserver agreement. Although the Kappa statistic will not illuminate the source of any disagreement, it may provide a useful illustration, depending on the number of outcomes in each group (Viera 2005).

We will attempt to contact the original study authors if there is insufficient information in the article text or in an abstract. If we identify multiple publications from one study, we will treat the study as a single entity and extract findings across all publications onto one form.

One review author will enter the data into Review Manager 5.3 (RevMan 5.3), and a second review author will check it for accuracy.

Assessment of risk of bias in included studies

We will use the EPOC 'Risk of bias' tool to assess the risk of bias of all study types (EPOC 2015). The areas of bias addressed by the tool cover the domains outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011).

Two review authors will independently assess the risk of bias of each included study, and assessment will be compared and reconciled, if necessary with the help of an arbitrator. We will categorise each study as having low, high, or unclear risk of bias using the EPOC 'Risk of bias' tool (EPOC 2015). Any disagreements will be resolved by discussion or by consulting the senior review author if necessary.

Measures of treatment effect

From each study we will collect the outcomes relevant to this review, regardless of whether they are the primary outcome for each individual study or not. We will extract the effect estimate of the intervention from the data provided in the publication, including the P value and confidence intervals.

We will present binary outcomes using proportion or rate. We will also report risk ratio or odds ratios as appropriate and their 95% confidence intervals. For continuous outcomes, we will report mean and standard deviation and assess standardised mean

difference for studies evaluating the same outcome in different ways.

Unit of analysis issues

Cluster randomised trials

In order to include these trials in the meta-analysis and in particular to combine them with individually randomised trials, it is important to consider and minimise unit of analysis error. If participants are randomised by cluster, they should be analysed by cluster. However, this is often not the case, and these trials are instead analysed at the level of the individual. This method introduces artificially small P values. If we identify unit of analysis error, we will contact the study authors to request their original data in order to recalculate appropriate study effects using relevant multilevel regression (Higgins 2011). If the information available is not sufficient and/or cannot be obtained, we will report the effect estimate and identify the fact that there is unit of analysis error (that is the data clustering was not accounted for in the original manuscript).

Cross-over trials

Due to the potentially lasting effect of the intervention, cross-over trials in their entirety will not be suitable for this review. If we identify cross-over trials, we will include only the first time period in the analysis.

Studies with more than two intervention groups

We will first assess all studies to decide how many intervention groups are relevant to the review. If more than two groups apply, then we will attempt to follow the recommendations in the *Cochrane Handbook for Systematic Reviews of Interventions* and combine the relevant experimental and control groups to enable a pair-wise comparison. If this is not possible, or if these groups are required in the subgroup analysis, then we will split any groups that are 'shared' in more than one comparison into smaller groups to ensure that their data is not 'double-counted' in any meta-analysis (Higgins 2011).

Interrupted time series

These studies are often not analysed correctly owing to the inappropriate use of t-tests not enabling consideration of the possible secular trends already occurring within their data. Therefore, if time series regression techniques are not used to analyse data in the included study, we will attempt a re-analysis (Ramsay 2003).

Dealing with missing data

We will record if data are missing on the data extraction forms and then contact the authors for further information. We will also consider this information when judging the risk of bias of included studies.

For any trials missing data, we will attempt to carry out analysis for each study on an intention-to-treat basis (attempting to include all participants in the group into which they were randomised). The denominator for each outcome in each study will be the number randomised. Similarly for non-randomised studies, we will carry out analysis based upon the group into which the participants were first allocated, irrespective of whether they actually received the intervention. If possible we will calculate missing summary data from the information provided. We will not impute missing data, but we will report the missing data as a measure of quality.

Assessment of heterogeneity

Due to the nature of this review, we expect significant statistical heterogeneity between studies. In addition, it is difficult to anticipate a priori the sources of heterogeneity. We will therefore extract all important sources of heterogeneity in the data abstraction form, which will include methodological and contextual aspects of the included studies. We will refine the form as we progress in the data extraction process by adding further fields or further categories to the existing fields.

We will investigate the statistical heterogeneity using not only visual inspection of forest plots but also by considering the I^2 statistic. However, due to the review question, there is also likely to be significant diversity in the participants (their healthcare background, their institution and speciality), interventions, length of training, repetitiveness of training, and location of training. We may explore this heterogeneity through subgroup analysis. There is also likely to be some methodological heterogeneity owing to the different study designs included within this review. We will consider the effects of this in the sensitivity analysis following a 'Risk of bias' assessment.

Assessment of reporting biases

If there are sufficient studies to undertake a funnel plot (approximately 5 to 10) for any outcome, we will perform this analysis and then visually examine it for asymmetry (Higgins 2011). If there are fewer studies, consideration will be given to the overall quality of the body of evidence. The strength of evidence will not necessarily be downgraded due to publication bias, as it may not be possible to detect publication bias.

For studies where a protocol has been published, we will compare the predefined outcome measures with those that have been reported. For studies with no protocol, we will examine the outcomes discussed in the methods section of the publication and compare these to the results. If we suspect reporting bias from

these processes, we will contact the authors for further information. If this is not possible, we will undertake a sensitivity analysis to understand the impact of the potential reporting bias on the effect size.

Data synthesis

Due to the nature of the studies likely to meet our inclusion criteria, it may be that different outcome measures and different methods of measuring outcomes will be used, even within a particular type of study design. We will first attempt to group studies of the same design together, and where studies use different scales when investigating the same continuous outcome, we will use standardised mean differences (with 95% confidence intervals) to pool the results of those studies. However, if the number of studies for data to be pooled is insufficient, or if data cannot be combined, we will present the findings in a narrative manner (Higgins 2011).

We will first pool together binary outcomes and therefore odds ratios or risk ratios using a fixed-effect meta-analysis with Mantel-Haenszel model. We will assess heterogeneity using the I^2 statistic. If we find heterogeneity, we will pool the risks together using a random-effects model (DerSimonian-Laird) (Higgins 2011). We will consider study (including sample) and intervention characteristics to investigate the source of heterogeneity, and if enough studies are available, we will consider subgroup analyses based on the categories of relevant study or intervention characteristics.

We will analyse continuous outcomes separately using a similar strategy, a fixed-effect meta-analysis (with inverse-variance weights) or a random-effects model, if we find heterogeneity (DerSimonian-Laird) (Higgins 2011).

However, if there are an insufficient number of studies for data to be pooled, or if data cannot be combined given the diversity of the intervention designs, study designs, specialities covered, and the heterogeneous nature of the outcome, we will present the findings in a narrative manner (Higgins 2011). We will summarise the findings of each relevant included study in tables that include the main characteristics of the study and the results in natural units as reported by the investigators.

We will carry out statistical analysis using Review Manager supplemented by Stata Statistical Software if necessary (RevMan 5.3; STATA 13).

'Summary of findings' table and assessing the certainty of the evidence

We will use the five GRADE considerations (trial limitations, consistency of effect, imprecision, indirectness, and publication bias) to make judgements about the certainty of the available evidence for each main outcome (Guyatt 2011). Two review authors will independently carry out this assessment, with any disagreements being resolved through discussion with a third review author. We will present the information in 'Summary of findings' tables along

with describing key information pertaining to the findings for each outcome including comparative risks, risk ratio, and the number of participants (Higgins 2011). We will justify all decisions to down- or upgrade the certainty of the evidence in relation to each outcome using footnotes.

The 'Summary of findings' tables will present evidence for the three primary outcomes and four secondary outcomes. Due to the lack of certainty over which outcomes we will identify in studies, we will attempt to include one outcome from each of the three broad categories of patient outcomes, clinical practice outcomes, and organisation-of-care outcomes. In addition to displaying the findings by outcome, we will also display the findings by study design. We will use GRADE software to generate the 'Summary of findings' tables (GRADEproGDT 2015).

Subgroup analysis and investigation of heterogeneity

We will investigate statistical heterogeneity across studies using standard Chi^2 tests and the I^2 statistic (Higgins 2003). We will use the inverse-variance weighted method to combine summary measures using random-effects models to minimise the effect of between-study heterogeneity.

We will use the prespecified study-level characteristics and those identified during the data extraction process as characteristics for assessment of heterogeneity. We will use stratified analysis and random-effects meta-regression to examine the difference in pooled risk ratios (Thompson 1999).

As described in [Types of outcome measures](#), we have classified the outcomes as patient, clinical practice, or organisation of care. It is difficult to determine at this stage which outcomes we will use in the subgroup analyses, as this will be dependent on the volume of studies identified and the type of outcomes collected.

It might not be possible to calculate average effects across studies given the diversity of the intervention designs, study designs, specialities covered, and the heterogeneous nature of the outcome. We will therefore perform a narrative synthesis with separate results from each study if necessary. We will summarise the findings of each relevant included study in tables that include the main characteristics of the study and the results in natural units as reported by the investigators.

Due to the anticipated heterogeneity of outcomes and numbers of studies, it is not possible for us to provide an accurate list of subgroup analyses a priori. However, we have listed possible areas for subgroup analyses below.

- Speciality, because different specialities may have different approaches to training or emergencies that are more amenable to short training interventions than others, e.g. shoulder dystocia training versus advanced neonatal resuscitation.
- Composition of the participant group (multiprofessional or single profession), as this will enable an assessment of whether training in multiprofessional or single professional groups delivers improved outcomes. It will also allow a comment in

terms the equity of training interventions between staff groups.

- The frequency of the intervention, e.g. one-off, monthly, annually, as this will allow consideration of whether it is important to have frequent repetitive training or whether one-off training is sufficient.
- Length of training, as this will allow an understanding of whether training interventions need to be long (e.g. one week) or if short interventions (e.g. one hour) can have an impact on patient care.
- Local or off-site training to understand whether training location matters.
- Public or private institution where training occurs to allow consideration of the impact of the setting of the intervention.
- Study design, study quality, degree of adjustment, geographical location to allow an understanding of the impact of the method of investigation on the outcomes.
- Interventions that rely on the actions of a single provider versus a team of providers.
- Outcome types: patient outcomes, clinical practice outcomes, and organisation-of-care outcomes
- Time period, as there may be time trends that increase safety culture.
- Type of health system, e.g. public or private system.
- Other relevant clinical/training/specialty characteristics identified during the data extraction.

Sensitivity analysis

We will perform sensitivity analyses to understand the effects of studies at high risk of bias or unclear risk of bias on the meta-analysis. We will also perform a sensitivity analysis if there is a large amount of missing or imputed data. If we include cluster randomised trials, we will undertake a sensitivity analysis of this group of trials owing to the complexities of possible unit of analysis error. We will also investigate the impact of studies with unit of analysis error by repeating the above analyses without those studies.

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* Indicates the major publication for the study

APPENDICES

Appendix I. MEDLINE Search Strategy

No.	Search terms	Results
1	emergencies/	34835
2	emergency treatment/	8524
3	first aid/	6971
4	cardiopulmonary resuscitation/	11852
5	resuscitation/	22258
6	resuscitation.ti,ab.	40513
7	emergenc*.ti,ab.	245652
8	((urgent or critical or unexpected) adj3 (care or treat*)).ti,ab	27491
9	(adverse adj (outcome* or effect*)).ti,ab.	123817
10	emergency medical services/	33273
11	emergency service, hospital/	46389
12	ot/1-11	481747
13	emergency medicine/ed	3874
14	obstetrics/ed	2589
15	gynecology/ed	2363
16	anesthesiology/ed	3760
17	exp pediatrics/ed	6313
18	exp specialties, surgical/ed	21821
19	health personnel/ed	4845
20	allied health personnel/ed	3431
21	emergency medical technicians/ed	1568

(Continued)

22	nurses' aides/ed	1125
23	physician assistants/ed	1093
24	exp nurses/ed	7375
25	exp medical staff/ed	3293
26	exp nursing staff/ed	13166
27	exp physicians/ed	2599
28	((doctor* or physician* or nurse* or midwife* or midwives or clinician* or consultant* or intensivist* or obstetrician* or gyn? ecologist* or p?ediatrician* or an?esthesiologist* or surgeon* or healthcare assistant* or health care assistant* or health care professional* or healthcare professional* or team* or interprofessional or multiprofessional or inter-professional or multiprofessional or medical or nursing or staff) adj5 (train* or teach* or educat*)).ti,ab	149332
29	or/13-28	202386
30	computer simulation/	147649
31	computer-assisted instruction/	9813
32	education, continuing/	8042
33	education, graduate/	4567
34	exp education, medical/	134077
35	exp education, nursing/	72394
36	education, professional, retraining/	1206
37	education, professional/	2022
38	inservice training/	17758
39	patient simulation/	3446
40	problem-based learning/	5677
41	advanced cardiac life support/ed	191

(Continued)

42	teaching/	43178
43	((in-service or in-service) adj (train* or teach* or educat*)).ti,ab	2012
44	(continuous professional development or cpd).ti,ab.	3769
45	((patient* or computer* or online) adj simulat*).ti,ab.	21171
46	problem based learning.ti,ab.	2349
47	virtual learning.ti,ab.	191
48	(elearning or e-learning or online learning).ti,ab.	1981
49	((experiential or active) adj learning).ti,ab.	2119
50	(skill* adj2 drill*).ti,ab.	31
51	(acls or als or amls or apls or arni or atacc or atls or ccemtp or eals or enpc or epc or epls or fp-c or ils or itls or nls or nrp or pals or pepp or phtls or pils or tncc).ti,ab	31088
52	((advanced or adult or pediatric or paediatric or newborn or neonatal or immediate or trauma or emergency or evaluat* or basic*) adj2 (life support or resuscitation)).ti,ab	8090
53	("anaesthesia trauma and critical care" or critical care emergency medical transport program* or emergency nursing pediatric course* or emergency pediatric care or "hospital and emergency procedures cme course*" or pediatric education for prehospital professionals or trauma nursing core course*).ti,ab	23
54	(emergenc* adj5 train*).ti,ab.	2792
55	or/30-54	461913
56	12 and 29 and 55	8117
57	randomized controlled trial.pt.	397827
58	controlled clinical trial.pt.	89715
59	multicenter study.pt.	188398
60	pragmatic clinical trial.pt.	171

(Continued)

61	(randomis* or randomiz* or randomly).ti,ab.	610748
62	groups.ab.	1462680
63	(trial or multicenter or multi center or multicentre or multi centre).ti	164333
64	(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	6929686
65	non-randomized controlled trials as topic/	18
66	interrupted time series analysis/	45
67	controlled before-after studies/	41
68	or/57-67	7760961
69	exp animals/	18077996
70	humans/	14018139
71	69 not (69 and 70)	4059857
72	review.pt.	1992650
73	meta analysis.pt.	56809
74	news.pt.	169577
75	comment.pt.	632046
76	editorial.pt.	380379
77	cochrane database of systematic reviews.jn.	11489
78	comment on.cm.	632046
79	(systematic review or literature review).ti.	61734
80	or/71-79	6941213
81	68 not 80	5330048

(Continued)

82 56 and 81

3399

CONTRIBUTIONS OF AUTHORS

DS conceived the idea, DS and AM developed the idea. AM, HvdN, EL, and DS prepared the draft of the protocol. YC, JF, SG, TD, CW, and JS commented on the protocol.

DECLARATIONS OF INTEREST

AM is a research fellow funded in part by Ammalife, a UK registered charity working to reduce maternal mortality and a registered member of the PROMPT Maternity Foundation, a UK-based charity.

HvdN is a registered member of the PROMPT Maternity Foundation, a UK-based charity.

EL has no interests to declare.

YC has no interests to declare.

JS is past Chair of the Resuscitation Council (UK) and a current member of its Executive Committee. The Resuscitation Council (UK) is a charity that develops and runs training courses, and JS has no financial interests. JS is an Editor of the journal *Resuscitation* and receives a payment from the publisher Elsevier.

JF and SG has no interests to declare.

CW is the lead research midwife for the PROMPT Maternity Foundation and a part-time employee of the charity.

TD is a trustee of the PROMPT Maternity Foundation, a UK-based charity running maternity training courses. He has no financial interest from this association. He has provided fee-paying services for Limbs and Things and Ferring Pharmaceuticals.

DS has been taken to dinner by Limbs and Things, who have also agreed to sponsor lunch for the course he organises once per year (SMASH: Saving Mothers with Advanced Simulation of High-risk situations). Ferring paid his expenses to attend a Clinical Expert meeting in 2011. He is a registered member of the PROMPT Maternity Foundation, a UK-based charity.

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