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[Intervention Review]

Mechanical versus manual chest compressions for cardiac arrest

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ABSTRACT

Background

Mechanical chest compression devices have been proposed to improve the effectiveness of cardiopulmonary resuscitation (CPR).

Objectives

To assess the effectiveness of resuscitation strategies using mechanical chest compressions versus resuscitation strategies using standard manual chest compressions with respect to neurologically intact survival in patients who suffer cardiac arrest.

Search methods

On 19 August 2017 we searched the Cochrane Central Register of Controlled Studies (CENTRAL), MEDLINE, Embase, Science Citation Index-Expanded (SCI-EXPANDED) and Conference Proceedings Citation Index–Science databases. Biotechnology and Bioengineering Abstracts and Science Citation abstracts had been searched up to November 2009 for prior versions of this review. We also searched two clinical trials registries for any ongoing trials not captured by our search of databases containing published works: Clinicaltrials.gov (August 2017) and the World Health Organization International Clinical Trials Registry Platform portal (January 2018). We applied no language restrictions. We contacted experts in the field of mechanical chest compression devices and manufacturers.

Selection criteria

We included randomised controlled trials (RCTs), cluster-RCTs and quasi-randomised studies comparing mechanical chest compressions versus manual chest compressions during CPR for patients with cardiac arrest.

Data collection and analysis

We used standard methodological procedures expected by Cochrane.

Main results

We included five new studies in this update. In total, we included 11 trials in the review, including data from 12,944 adult participants, who suffered either out-of-hospital cardiac arrest (OHCA) or in-hospital cardiac arrest (IHCA). We excluded studies explicitly including patients with cardiac arrest caused by trauma, drowning, hypothermia and toxic substances. These conditions are routinely excluded from cardiac arrest intervention studies because they have a different underlying pathophysiology, require a variety of interventions specific to the underlying condition and are known to have a prognosis different from that of cardiac arrest with no obvious cause. The exclusions were meant to reduce heterogeneity in the population while maintaining generalisability to most patients with sudden cardiac death.

The overall quality of evidence for the outcomes of included studies was moderate to low due to considerable risk of bias. Three studies (N = 7587) reported on the designated primary outcome of survival to hospital discharge with good neurologic function (defined as a Cerebral Performance Category (CPC) score of one or two), which had moderate quality evidence. One study showed no difference with mechanical chest compressions (risk ratio (RR) 1.07, 95% confidence interval (CI) 0.82 to 1.39), one study demonstrated equivalence (RR 0.79, 95% CI 0.60 to 1.04), and one study demonstrated reduced survival (RR 0.41, CI 0.21 to 0.79). Two other secondary outcomes, survival to hospital admission (N = 7224) and survival to hospital discharge (N = 8067), also had moderate quality level of evidence. No studies reported a difference in survival to hospital admission. For survival to hospital discharge, two studies showed benefit, four studies showed no difference, and one study showed harm associated with mechanical compressions. No studies demonstrated a difference in adverse events or injury patterns between comparison groups but the quality of data was low. Marked clinical and statistical heterogeneity between studies precluded any pooled estimates of effect.

Authors' conclusions

The evidence does not suggest that CPR protocols involving mechanical chest compression devices are superior to conventional therapy involving manual chest compressions only. We conclude on the balance of evidence that mechanical chest compression devices used by trained individuals are a reasonable alternative to manual chest compressions in settings where consistent, high-quality manual chest compressions are not possible or dangerous for the provider (eg, limited rescuers available, prolonged CPR, during hypothermic cardiac arrest, in a moving ambulance, in the angiography suite, during preparation for extracorporeal CPR [ECPR], etc.). Systems choosing to incorporate mechanical chest compression devices should be closely monitored because some data identified in this review suggested harm. Special attention should be paid to minimising time without compressions and delays to defibrillation during device deployment.

PLAIN LANGUAGE SUMMARY

Mechanical chest compression machines for cardiac arrest

Review question

We reviewed which method of chest compressions (applying the traditional hand technique versus using a machine) resulted in more lives saved during cardiopulmonary resuscitation (CPR) for cardiac arrest.

Background

'Sudden cardiac arrest' occurs when someone's heart stops beating unexpectedly. Cardiopulmonary resuscitation, referred to as CPR, involves rhythmical pushing on the chest of a cardiac arrest victim to provide forward blood flow. This can keep blood flowing to the victim's vital organs while the heart is not pumping. CPR has been shown to improve the chance that the heart will restart and the victim will survive. Machines have been developed to take over this chest pumping action using automated pistons, pneumatic vests, or band-like mechanisms. The theory is that these machines should be able to provide a more effective pumping action than is seen in humans because the machines do not pause or get tired. Furthermore, they provide consistent pressure and timing of each chest compression in line with latest evidenced-based practice. Some preliminary studies using these machines have shown that they are easy to use and can save people with cardiac arrest. This is an update of the Cochrane Review on mechanical chest compression devices originally published in 2011 and updated last in 2014.

Study characteristics

The evidence is current to August 2017. We searched the literature and found a total of 2554 citations that were potentially relevant. After reviewing each of these, we found 11 articles describing clinical trials that could help us answer our question. Taken together, these trials included 12,944 adult participants who suffered cardiac arrest either in-hospital or out-of-hospital. The newest studies identified in this update are larger and of higher quality than those that had been identified in prior versions of this review. Several studies were sponsored by device manufacturers.

Key results

We found that available studies have important differences from one another. The most important differences were the type of mechanical device studied and the type of CPR protocol provided for patients assigned to the manual chest compression group. These differences make comparisons across studies challenging. Some studies reported improvements in rate of survival for patients treated with mechanical chest compressions compared to patients treated with manual chest compressions, while others reported no difference or even suggested harm associated with mechanical chest compressions. When considering all of the identified studies together, it seems like mechanical chest compression devices probably have a very similar effect on survival when compared with high-quality manual chest compressions.

Quality of evidence

With the inclusion of several large studies, the overall quality of evidence has improved considerably, and now may be considered to be of low to moderate quality.

SUMMARY OF FINDINGS

Summary of findings for the main comparison. Mechanical chest compressions compared to manual chest compressions for cardiac arrest

Mechanical chest compressions compared to manual chest compressions for cardiac arrest

Patient or population: patients with cardiac arrest
Setting: in-hospital and out-of-hospital cardiac arrest
Intervention: mechanical chest compressions
Comparison: manual chest compressions

Outcomes	Impact	№ of participants (studies)	Quality of the evidence (GRADE)
Survival to hospital discharge with good neurological function	1 study showed a decrease, 1 study showed no difference, and 1 study showed equivalence in this outcome with mechanical devices. No meta-analysis was possible due to heterogeneity.	7587 (3 RCTs) [d]	⊕⊕⊕⊖ Moderate [a]
Survival to hospital discharge	1 study showed a decrease, 4 studies showed no difference, and 2 studies showed an increase in this outcome with mechanical devices. No meta-analysis was possible due to heterogeneity.	8067 (7 RCTs)	⊕⊕⊕⊖ Moderate [a]
Return of spontaneous circulation	1 study showed a decrease, 4 studies showed no difference, and 3 studies showed an increase in this outcome with mechanical devices. No meta-analysis was possible due to heterogeneity.	11,771 (8 RCTs)	⊕⊕⊖⊖ Low [a b]
Survival to hospital admission	All 4 studies showed no difference in this outcome with mechanical devices. No meta-analysis was possible due to heterogeneity.	7224 (4 RCTs)	⊕⊕⊕⊖ Moderate [a]
Sternal or rib fractures	6 studies showed no difference, and 1 study showed an increase in this outcome with mechanical devices. No meta-analysis was possible due to heterogeneity.	7469 (7 RCTs)	⊕⊕⊖⊖ Low [a c]
Haemothorax or pneumothorax	All 5 studies showed no difference in this outcome with mechanical devices. No meta-analysis was possible due to heterogeneity.	7316 (5 RCTs)	⊕⊕⊖⊖ Low [a c]
Internal abdominal organ injury	All 5 studies showed no difference in this outcome with mechanical devices. No meta-analysis was possible due to heterogeneity.	7337 (5 RCTs)	⊕⊕⊖⊖ Low [a c]

GRADE Working Group grades of evidence
High quality: We are very confident that the true effect lies close to that of the estimate of the effect
Moderate quality: We are moderately confident in the effect estimate: The true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different
Low quality: Our confidence in the effect estimate is limited: The true effect may be substantially different from the estimate of the effect
Very low quality: We have very little confidence in the effect estimate: The true effect is likely to be substantially different from the estimate of effect

[a] Downgraded by one level for serious risk of bias in all included studies.
[b] Downgraded by one level for serious risk of inconsistency. Three studies showed benefit, one study showed harm, and four studies showed neither.
[c] Downgraded by one level for serious risk of imprecision. Most studies had wide 95% confidence intervals.
[d] Randomized controlled trials

BACKGROUND

This is the second update of this review, originally published in 2011 (Brooks 2011), and subsequently updated in 2014 (Brooks 2014). It concluded that there was insufficient evidence from high-quality, randomised controlled trials (RCTs) to determine whether mechanical chest compression devices are associated with benefit or harm in the treatment of cardiac arrest.

Description of the condition

Cardiac arrests, both occurring out-of-hospital (OHCA) and in-hospital (IHCA), remain a major health problem in the USA and Canada (Heart and Stroke Foundation 2017; Mozaffarian 2016). Survival rates vary from approximately 5% to 50% depending on location (out-of-hospital versus in-hospital), geographic region and other characteristics of the event (Kleinman 2015). More than half of survivors of sudden cardiac arrest have brain damage of varying degrees (Herlitz 2000; Pusswald 2000; Young 2009). Many patients are comatose after cardiac arrest, with outcomes ranging from brain death to good recovery (Young 2009).

The goal of treatment for cardiac arrest patients is to achieve return of spontaneous circulation and favourable neurological function as early as possible while minimising end-organ damage and dysfunction. Cardiopulmonary resuscitation (CPR), which involves the delivery of rhythmical chest compressions, with or without ventilations, can sustain a minimal but crucial amount of blood flow to vital organs while attempts are made to restore spontaneous circulation.

Description of the intervention

Traditional CPR for cardiac arrest victims includes the delivery of rhythmical manual chest compressions by a human rescuer. An alternative technique is to deliver chest compressions with the use of a mechanical chest compression device. Several types of these are commercially available (Lurie 2002), and they employ a variety of compression mechanisms. Different mechanisms include load-distributing bands, pistons and pneumatic vests. Load-distributing band-CPR involves a wide band of material attached to a short backboard, which is placed around the patient's thorax. The circumference of the band is mechanically and rhythmically shortened and lengthened. The change in circumference of the band simulates compressions delivered in standard manual CPR. Piston devices use compressed gas to drive a piston placed over the lower sternum of the patient. Some of these piston devices use a suction cup attachment to provide active compression/decompression CPR. A pneumatic vest is similar to an oversized blood pressure cuff placed circumferentially around the patient's thorax (Halperin 1993). Chest compression is caused by rapid introduction of air into the vest. Several reports have demonstrated the feasibility of using mechanical chest compression devices in the prehospital setting (Ong 2006; Steen 2005).

How the intervention might work

Several investigations have demonstrated that early CPR is associated with improved survival in both adults and children (Hasselqvist-Ax 2017; Herlitz 1994; Naim 2017; Stiell 2004). For example, in a study of more than 30,000 OHCAs in Sweden between 1990 and 2011, CPR performed before the arrival of emergency medical services personnel was associated with a 30-day odds of survival that was more than twice as high as that associated with no CPR before emergency medical services arrival (adjusted odds ratio 2.15, 95% confidence interval (CI) 1.88 to 2.45; Hasselqvist-Ax 2017). In the Naim 2017 study, bystander CPR was independently associated with improved survival in a cohort of 3900 OHCAs in patients less than 18 (adjusted odds ratio 1.57, 95% CI 1.25 to 1.96).

The quality of chest compressions, as defined by the continuity, rate and depth of compression, may be associated with survival; these characteristics of CPR have been emphasised in the American Heart Association (AHA) guidelines for CPR and emergency cardiovascular care (Kleinman 2015). Several animal studies (Kern 2002; Yu 2002), and at least one human study (Eftestol 2002), have demonstrated an inverse relationship between chest compression interruption duration and short-term survival. Even short pauses in chest compressions for ventilations (four seconds) have resulted in a significant decline in the central haemodynamic pressures necessary for adequate cerebral and coronary perfusion (Ewy 2005). The importance of rate of compression has been demonstrated in several animal (Kern 1986; Maier 1984; Swart 1994), and human studies (Swenson 1988b), which show that higher rates of CPR (120 to 140 compressions/min) improve central haemodynamic measurements. Human observational studies have suggested that return of spontaneous circulation peaks at a compression rate of 125/min (Idris 2012), and that survival to hospital discharge is optimised with compressions between 100/min to 120/min (Idris 2015). Excessive rates may be detrimental to compression depth (Monsieurs 2012). Increased compression depth was related to increased incidence of return of spontaneous circulation in animal models of cardiac arrest (Babbs 1983; Bellamy 1984; Kern 1986). Stiell et al, in a large North American study, including 1029 OHCA participants, demonstrated an association between compression depth and improved return of spontaneous circulation, one-day survival and survival to hospital discharge (Stiell 2012).

Through the use of direct observation of actual cardiac arrests, recordings from automated external defibrillators (AEDs) and accelerometers, several studies have shown that chest compressions performed by trained professionals do not meet recommendations for compression rate, depth and continuity (Abella 2005a; Abella 2005b; Ko 2005; Wik 2005). For example, Wik and colleagues observed that chest compressions were halted for an average of 48% of the time during OHCAs (Wik 2005). While observing 67 IHCAs, Abella and colleagues observed that the chest compression rate was less than 90/min for 27% of the duration of the cardiac arrest, and compression depth was too shallow 37% of the time (Abella 2005a).

Rescuer fatigue has been identified as an important potential contributor to poor CPR quality (Hightower 1995; Ochoa 1998). Hightower and colleagues observed significant fatigue after only one minute of chest compressions on a mannequin. Correct chest compressions were performed 92% of the time during the first minute, 67.1% during the second minute and 39.2% during the third minute. By five minutes, only 18% of chest compressions were being performed correctly. Participants did not accurately identify the point of fatigue (Hightower 1995).

The use of mechanical chest compression devices has been proposed to provide high-quality chest compressions without the interruptions and fatigue associated with human-delivered chest compressions. These devices also liberate human rescuers from the duty of performing chest compressions, allowing them to

perform other resuscitation tasks related to management and transportation of the cardiac arrest victim.

Some data from animal and human observational studies suggest that mechanical chest compressions may be superior to manual chest compressions in cardiac arrest. Animal studies comparing mechanical chest compressions versus manual chest compressions have shown that mechanical chest compressions produce improved cerebral, central and coronary blood flow (Halperin 2002; Halperin 2004; Rubertsson 2005; Timmerman 2003). Evidence also suggests improved survival in animal models of cardiac arrest (Ikeno 2006; Steen 2002). Several observational studies in humans have demonstrated improved outcomes with the use of mechanical chest compression devices (Casner 2005; Ong 2006; Steen 2005; Swanson 2005; Swanson 2006b). A recent meta-analysis of observational studies favoured mechanical compressions (odds ratio (OR) 1.42, 95% CI 1.21 to 1.67) for survival to hospital admission (Bonnes 2016).

Why it is important to do this review

Despite the enthusiasm generated by promising animal and observational human studies of mechanical chest compressions, clinical equipoise persists with regard to the true effectiveness of this therapy (Lewis 2006b). Most data on the effectiveness of mechanical chest compression devices derive from observational data. To date, six systematic reviews have been published (Bonnes 2016; Gates 2015; Li 2016; Ong 2006; Tang 2015; Westfall 2013). A meta-analysis of 12 randomised and observational studies investigating OHCA participants observed a positive association between the use of mechanical chest compression devices and the odds of return of spontaneous circulation (OR 1.53, 95% CI 1.32 to 1.78; Westfall 2013). The authors of this review were employees of or had received a significant amount of research funding from Zoll Medical Corporation. Zoll Medical Corporation makes the AutoPulse mechanical CPR device. A systematic review including non-randomised studies showed no improvement in survival and the potential for worse neurological outcomes with mechanical devices (Ong 2006). A recent meta-analysis of nine prospective studies (both randomised and observational) of OHCAs and IHCAs reported that there was no difference in survival to discharge with good neurological function (risk ratio (RR) 1.11, 95% CI 0.95 to 1.3), but that return of spontaneous circulation was more likely to be achieved with manual compressions in OHCA (RR 0.87, 95% CI 0.81 to 0.94) and IHCA (RR 0.71, 95% CI 0.53 to 0.97) (Li 2016). Three recent systematic reviews performed a meta-analysis of the same five RCTs for OHCAs published between 2006 and 2015 (Bonnes 2016; Gates 2015; Tang 2015). These studies found no difference in survival outcomes, including survival to discharge (Gates: OR 0.89, 95% CI 0.77 to 1.02), survival to hospital admission (Bonnes: OR 0.94, 95% CI 0.84 to 1.05) and survival to discharge with good neurological outcome (Tang: RR 0.80, 95% CI 0.61 to 1.04). One of the reviews (Tang 2015), found a weak association between mechanical compressions and survival to hospital admission (RR of death 0.94, 95% CI 0.89 to 1.00) and to hospital discharge (RR of death 0.88, 95% CI 0.78 to 0.99). The latest 2015 guidelines from the International Liaison Committee on Resuscitation (ILCOR) Advanced Life Support (ALS) Task Force has recommended against the routine use of mechanical chest compressions. However, the authors do suggest that mechanical compressions may be an option in scenarios where sustained high-quality compressions would be otherwise impractical or unsafe (Callaway 2015).

For this 2017 update, the review authors have opted to forgo a quantitative meta-analysis, given the considerable clinical and methodological heterogeneity of the studies identified in the literature. Instead, this update serves to be a single unified review of the existing evidence from the 11 RCTs identified in the literature.

OBJECTIVES

To assess the effectiveness of mechanical chest compressions versus standard manual chest compressions with respect to neurologically intact survival in patients who suffer cardiac arrest.

METHODS

Criteria for considering studies for this review

Types of studies

All studies employing patient-level randomisation or cluster randomisation comparing compressions delivered via any type of powered mechanical chest compression device versus standard manual chest compressions were considered for inclusion in the review. We also included studies that were quasi-randomised, which are controlled clinical trials in which the method of allocation is known but is not considered strictly random. Examples of quasi-random processes for assigning treatments include odd-even numbers, participant social security numbers, days of the week, participant record numbers, ambulance run numbers and participant birth dates, which may appear to represent 'random' phenomena but do not constitute a truly random process of determining group allocation. For a study to be included in the review as a quasi-randomised study, the methodology had to explicitly use the term "quasi-randomisation" to describe the method of allocation. We did not include prospective studies without random or quasi-random allocation in the review. We excluded studies with designs that involved cross-over of individual participants from the manual chest compression arm to the mechanical chest compression arm.

Types of participants

We considered for inclusion in the review patients suffering out-of-hospital cardiac arrest (OHCA) or in-hospital cardiac arrest (IHCA), with resuscitation attempted by trained medical personnel. We excluded studies explicitly including patients with cardiac arrest caused by trauma, drowning, hypothermia and toxic substances. These conditions are routinely excluded from cardiac arrest intervention studies because they have a different underlying pathophysiology, require a variety of interventions specific to the underlying condition and are known to have a prognosis different from that of cardiac arrest with no obvious cause. The exclusions were meant to reduce heterogeneity in the population while maintaining generalisability to most patients with sudden cardiac death.

Types of interventions

We considered for inclusion studies comparing compressions delivered via any type of powered, automatic mechanical chest compression device versus standard manual chest compressions provided by a human.

Types of outcome measures

Primary outcomes

The primary outcome for this review was survival to hospital discharge with good neurological function, equivalent to a Cerebral Performance Category (CPC) one or two (Jennett 1975), as measured by any validated scale.

Secondary outcomes

Secondary survival outcomes included:

- survival to hospital discharge
- return of spontaneous circulation
- survival to emergency department arrival or hospital admission (OHCA only)
- short-term survival (less than or equal to 30 days)
- long-term survival (greater than 30 days)
- adverse effects
  - sternal or rib fractures
  - haemothorax or pneumothorax
  - abdominal organ injury

We also sought to abstract time intervals that may negatively impact outcome, including:

- Emergency telephone call or scene arrival to first shock interval for ventricular fibrillation/ventricular tachycardia
- Emergency telephone call or scene arrival to first CPR interval
- Emergency telephone call or scene arrival to first return of spontaneous circulation interval

Search methods for identification of studies

Electronic searches

We searched the following electronic databases.

- Cochrane Central Register of Controlled Studies (CENTRAL; 2017, Issue 8) in the Cochrane Library (searched 19 August 2017).
- MEDLINE (Ovid, 1946 to August 2017, week 3) on 19 August 2017
- Embase (Ovid, 1980 to 18 August 2017, week 33) on 19 August 2017
- Science Citation Index-Expanded (SCI-EXPANDED) on the Web of Science (Thomson Reuters, 1970 to 19 August 2017) on 19 August 2017
- Conference Proceedings Citation Index–Science (CPCI-S) on the Web of Science (1990 to 19 August 2017) on 19 August 2017
- Science Citation abstracts on the Web of Science (Thomson Reuters, 1960 to 18 November 2009) on 18 November 2009
- Biotechnology and bioengineering abstracts (1982 to 18 November 2009) on 18 November 2009.

The search strategies used for each database are listed in Appendix 1 (for 2017), Appendix 2 (for 2014) and Appendix 3 (for 2011). The original review search strategy for MEDLINE includes the highly sensitive search filter for retrieval of reports of controlled trials (Higgins 2006). The updated MEDLINE and Embase searches include an updated RCT filter. For MEDLINE, we used the sensitivity-maximising version of the Cochrane RCT filter (Lefebvre 2011), and applied adaptations of it to Embase, SCI-EXPANDED and CPCI-S .

We imposed no language of publication restrictions. We sought translations through Cochrane for full-text articles in languages other than English.

Searching other resources

We searched the ClinicalTrials.gov clinical trials registry on 19 August 2017 (www.clinicaltrials.gov), and the World Health Organization International Clinical Trials Registry Platform on 17 January 2018 (www.who.int/ictrp/en). We handsearched bibliographies of included papers. We contacted an expert in the field of mechanical chest compression devices (Dr. James Christensen, University of British Columbia, 19 June 2007) and representatives of chest compression device manufacturers (Zoll, Medtronic, 19 June 2007) about published and unpublished studies on this topic for the original review. We accomplished contact with experts for subsequent searches (2010 and 2017) through author (SCB and LJM) participation in the International Liaison Committee on Resuscitation Advanced Life Support Task Force, which included international experts in OHCA and the use of mechanical chest compression devices. This committee of experts undertook a comprehensive review of the world literature on mechanical chest compression devices.

Data collection and analysis

Selection of studies

Review authors used predefined inclusion criteria to decide on the status of each citation. Two review authors (SCB and BLB in 2011, SCB and NH in 2014, SCB and PW in 2017) screened citations in an independent, hierarchical fashion by title, abstract and then full article for relevance. At each stage of review, we classified citations as 'include', 'exclude' or 'indeterminate'. This process was summarized using a PRISMA flowchart (Moher 2009). We included citations classified as 'include' or 'indeterminate' by at least one of the review authors in the next level of review. We planned to resolve disagreements at the full article stage through consensus and with the assistance of a third reviewer who was an author on prior versions of the review (LJM) if consensus could not be reached. A third review author was not required at any time for the 2017 review. Agreement between review authors at each stage of review was quantified using a kappa statistic. We recorded and reported the reason for exclusion at the full article stage in the results section of the review (Excluded studies).

Data extraction and management

Two review authors (SCB and LJM in 2011, SCB and NH in 2014, SCB and PW in 2017) abstracted data independently using a preformed data abstraction tool developed for the original review. We resolved discrepancies in data abstraction through consensus, but if we could not reach consensus, we planned to involve a third reviewer who was an author on prior updates for this review (BLB) to resolve discrepancies. When available, data abstraction included:

Methods

- year of study
- study design
- study setting

Participants

- sample size

- number of participants per treatment arm
- mean age
- gender
- initial rhythm
- hypothermia treatment post-arrest
- inclusion criteria
- exclusion criteria

Interventions

- intervention
- comparison
- mechanical device employed
- detail of resuscitation protocol

Outcomes

- predefined primary and secondary outcomes

Notes

- notable differences from currently accepted resuscitation guidelines
- comments on unique aspects of study

Assessment of risk of bias in included studies

In the original review and 2014 update, one review author (SCB) evaluated each included study for risk of bias as recommended in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011). In this 2017 update, two review authors (SCB and PW) independently reviewed the risk of bias for all studies. This included making judgements on the following domains as having a low, unclear or high risk of bias: (1) random sequence generation, (2) allocation concealment, (3) incomplete outcome data, (4) cointervention or contamination, (5) blinding of care providers, (6) blinding of outcome assessors, and (7) other potential sources.

Measures of treatment effect

We quantified the primary outcome of survival to hospital discharge with good neurological function and dichotomous secondary outcomes, including survival at different time points and adverse events, as a risk ratio (RR) with 95% confidence intervals (CIs). We planned to compare continuous secondary outcomes (time interval data) by comparing differences in means.

Unit of analysis issues

For cluster-randomised trials, we planned to adjust for clustering by calculating effective sample sizes. We contacted the authors of the Hallstrom study, and determined a mean cluster size for the 223 clusters having at least one primary case was 3.44 (standard deviation 4.82; Hallstrom 2006). The intra-cluster correlation (ICC) was -0.005. For the PARAMEDIC study, the ICC was reported to be negligible at 0.001 (Perkins 2015). Because the calculated design effects for both studies were so close to unity, we entered the data into Review Manager 5 for RR calculation without adjustment for clustering (Higgins 2011; Review Manager 2014). For trials with multiple comparator groups (e.g. different mechanical devices) we combined data from all mechanical device groups into one mechanical chest compression comparator group.

Dealing with missing data

We had planned to contact the study authors directly for any missing or ambiguous data, and to allow for a one-month period for response. If no response, this would be reported as missing data. However, we did not encounter missing data that required us to do so.

Assessment of heterogeneity

We qualitatively explored the clinical heterogeneity of included trials through a detailed examination of study characteristics. We used the I^2 statistic to quantify statistical heterogeneity and to determine the appropriateness of pooling results across studies. We planned a priori not to pool studies if I^2 was > 50%, indicating substantial statistical heterogeneity.

Assessment of reporting biases

We had planned to explore the data graphically with a funnel plot to look for evidence of publication bias, but we found too few studies for this to be useful.

Data synthesis

In this 2017 update, in line with changes in the MECIR standards, we explicitly evaluated the quality of evidence for each outcome using the GRADE approach (Ryan 2016). Given that all outcomes were from RCTs, the starting rating of evidence was 'high quality'. We downgraded quality by one level for serious concerns and two levels for very serious concerns regarding risk of bias, inconsistency, indirectness, imprecision, and publication bias. Two review authors (SCB and PW) independently rated the evidence. We used the GRADE profiler software to generate a 'Summary of findings' table for the following outcomes (GRADEpro GDT 2015).

- Survival to hospital discharge with good neurological function
- Survival to hospital discharge
- Return of spontaneous circulation
- Survival to hospital admission
- Sternal or rib fractures
- Haemothorax or pneumothorax
- Internal abdominal organ injury

We planned to use the DerSimonian and Laird method (random-effects model) to provide a pooled estimate for RR when the data allowed. We planned to use a random-effects model to provide a conservative estimate of pooled effect and anticipated clinical heterogeneity amongst included studies. We made the decision not to pool study results given substantial clinical and statistical heterogeneity.

Subgroup analysis and investigation of heterogeneity

Planned a priori subgroup analyses included analysis by device type (piston versus load-distributing bands versus other), first rhythm analysed (ventricular fibrillation/ventricular tachycardia versus asystole/pulseless electrical activity/non-shockable) and location of arrest (IHCA versus OHCA). We made the decision to not perform pooled subgroup analysis because of issues with heterogeneity and insufficient data.

Sensitivity analysis

We planned for sensitivity analyses including removal of data assessed to be at moderate or high risk of bias during quality review and removal of studies with cluster randomisation. We made the decision to not perform a sensitivity analysis due to heterogeneity preventing meta-analysis and a paucity of quality data in each category.

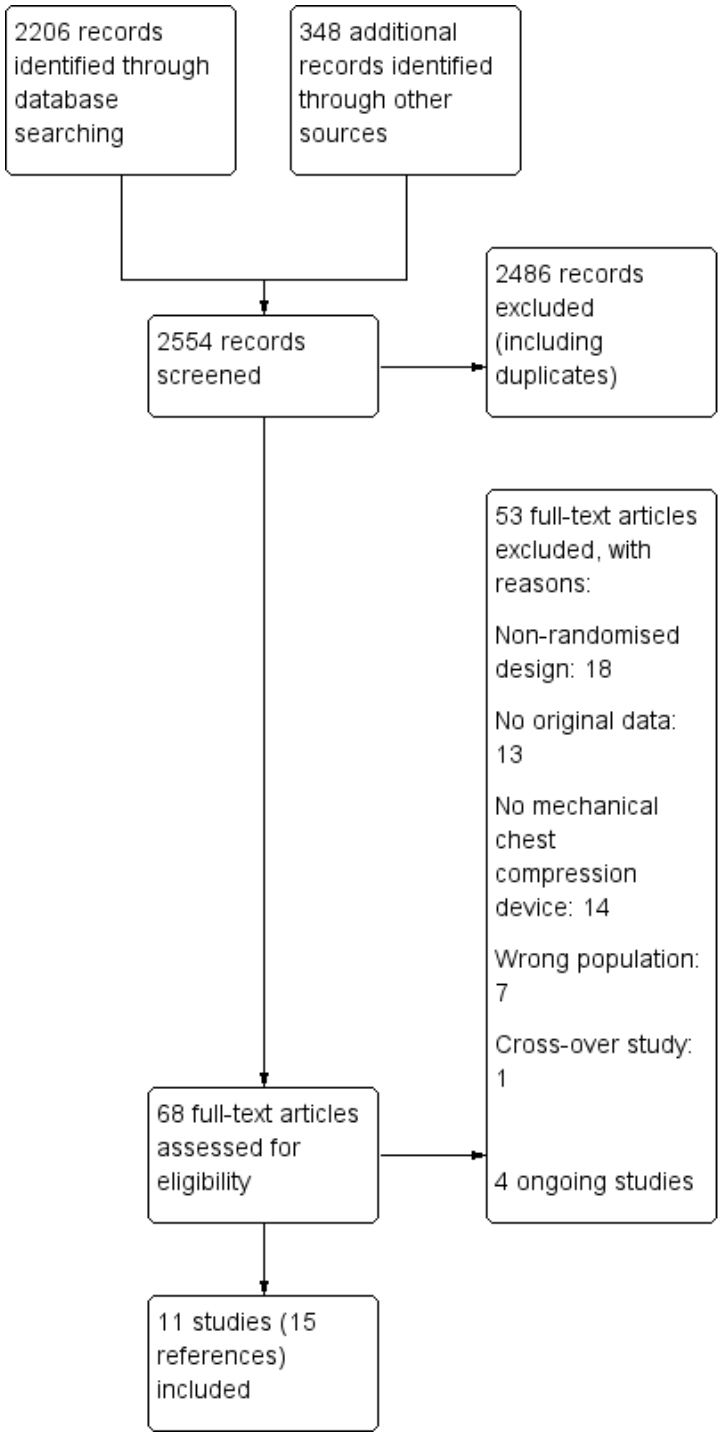
RESULTS

Description of studies

Results of the search

A comprehensive updated search of the literature in 2017 identified 599 additional citations. In the original review, the comprehensive search identified 1561 citations (MEDLINE 516, Embase 517, Science Citation abstracts 275, Biotechnology and bioengineering abstracts 18, Cochrane CENTRAL 78, ClinicalTrials.gov 33, handsearches of references of included papers 7, contact with industry representatives and experts 117). The updated search conducted for the 2014 update identified an additional 390 citations (MEDLINE 40, Embase 50, Conference Proceedings Citation Index–Science and Science Citation Index-Expanded on Web of Science 186, Cochrane CENTRAL 34, ClinicalTrials.gov 80). The present update searched in 2017 further identified 599 citations (MEDLINE 43, Embase 143, Conference Proceedings Citation Index–Science and Science Citation Index-Expanded on Web of Science 258, Cochrane CENTRAL 48, ClinicalTrials.gov 24, World Health Organization (WHO) International Clinical Trials Registry 87). Two independent review authors (SCB and BLB in 2011, SCB and NH in 2014, SCB and PW in 2017) reviewed 2554 citations by title, and categorised 279 of these as 'include or 'indeterminate' and selected them for review by abstract. After review by abstract, we identified 68 citations as potentially relevant and reviewed the full-text. Eleven studies met all inclusion criteria and we included them in this review (Characteristics of included studies; Figure 1).

Figure 1. PRISMA Study flow diagram.



Agreement for relevance between review authors was evaluated by the kappa statistic for this update. Fair agreement was seen for titles and abstracts (0.87) and perfect agreement for full articles (1.0).

Three studies previously identified as ongoing in the 2014 update have now completed and published their results (Perkins 2015; Rubertsson 2014; Wik 2014). We identified four ongoing studies through a search of the WHO International Clinical Trials Registry Platform (CTRI/2013/07/003840; ISRCTN38139840; ISRCTN78354073; NCT01521208; Characteristics of ongoing studies).

Included studies

In total, we included 11 studies in this review (12,944 participants). Of the five new studies included in the 2017 update, three are large-scale randomised controlled trials (RCTs) which account for 90% of all participants included in the review (Perkins 2015; Rubertsson

2014; Wik 2014). Reporting of participant characteristics was incomplete in several of the included studies. Reported mean ages of included participants ranged from 45.5 to 71 years old and the proportion who were males ranged from 58% to 68%. The proportion of participants with initial shockable rhythms (ventricular fibrillation/ventricular tachycardia) varied between studies from 13% to 47%.

While the ethnicity of the participants was not reported, three studies were conducted solely in the USA (Dickinson 1998; Halperin 1993; Taylor 1978), two studies were from China (Gao 2016; Lu 2010), one study was from Sweden (Smekal 2011), one study was from the Netherlands (Koster 2017), and one study was from the UK (Perkins 2015). The remaining studies were multinational collaborations between Sweden/Netherlands/UK (Rubertsson 2014), USA/Austria/Netherlands (Wik 2014), and USA/Canada (Hallstrom 2006).

The use of mechanical cardiopulmonary resuscitation (CPR) for in-hospital cardiac arrest (IHCA) has been studied in four small-scale RCTs (sample size ranged from 34 to 218) with 452 participants total (Halperin 1993; Lu 2010; Taylor 1978; Koster 2017). There is substantial heterogeneity in the methodology, intervention algorithm used and reporting of results. Devices used to deliver mechanical CPR differed in all three studies, including an unspecified piston device (Taylor 1978), the Thumper device (a gas-powered external chest compressor) (Lu 2010), a pneumatic vest-type device (developed by the study authors) (Halperin 1993), and both the LUCAS and AutoPulse devices (Koster 2017).

Out-of-hospital cardiac arrest (OHCA) has been studied in eight RCTs with 12,492 participants (Dickinson 1998; Gao 2016; Hallstrom 2006; Koster 2017; Perkins 2015; Rubertsson 2014; Smekal 2011; Wik 2014). The sample size and quality of the data has improved considerably with the addition of the five recent RCTs newly included in this update.

Of the eight studies, one quasi-randomised trial evaluated the Thumper device (Dickinson 1998), four RCTs evaluated the AutoPulse device (Gao 2016; Hallstrom 2006; Koster 2017; Wik 2014), and three RCTs (Koster 2017; Rubertsson 2014; Smekal 2011), and one cluster-randomised trial (Perkins 2015), evaluated the LUCAS device. The AutoPulse is an automated, battery-powered external chest compressor that uses a load-distributing band to rhythmically compress the chest against a backboard. The LUCAS device employs active compression-decompression of the chest wall by using a silicone rubber cup (which creates a suction effect) and a pneumatic cylinder connected to a stiff back-plate.

In a multicentre, cluster-randomised trial, Hallstrom and colleagues studied 767 participants with OHCA (Hallstrom 2006). The primary outcome of this study was survival to four hours after the emergency call. The trial was stopped at interim analysis by the data safety and monitoring board because of decreased survival to hospital discharge with good neurological function among participants who received mechanical chest compressions.

The CIRC (Circulation Improving Resuscitation Care) trial, using the AutoPulse, was conducted by Wik and colleagues (Wik 2014), and it attempted to address some of the concerns in the Hallstrom 2006 study. Here, they looked at the outcomes of OHCA in 4231 adults. Patients randomised to the intervention arm received what the authors called 'integrated-AutoPulse CPR (iA-CPR)', which was intended to minimise hands-off time by using assigned responder roles and a standardised deployment strategy. This study is also noted for its effort to track and ensure high-quality CPR delivery in both arms of the trial through vigorous training and CPR quality monitoring. The primary outcome in the CIRC trial was survival to hospital discharge.

In a study by Smekal and colleagues (Smekal 2011), 148 cardiac arrest participants were randomly assigned to receive mechanical CPR performed with the LUCAS device or CPR performed with manual chest compressions. Recently, these authors have followed up and reported their results from their much larger LINC trial (LUCAS in Cardiac Arrest) involving 2589 adult OHCAs (Rubertsson 2014). The primary outcome was four-hour survival.

The PARAMEDIC trial investigated the LUCAS device and involved 4471 participants (Perkins 2015). They employed a cluster-randomised open-label controlled trial to compare adult 30-day survival after OHCA between standing manual compressions and using the LUCAS-2 mechanical device. The study authors have emphasised the pragmatic nature of this trial. The primary outcome was 30-day survival.

There is also a smaller, single-centre cluster-RCT (N = 133) that was localised to the northern district of Shanghai, China which used the AutoPulse device (Gao 2016). Ten ambulances from one emergency medical centre were randomised to be equipped with the device or not and then were dispatched in consecutive order (ambulances 1 through 10) for suspected OHCA. The primary outcome for this study was return of spontaneous circulation.

The Koster 2017 study included 374 participants (156 with IHCA and 218 with OHCA) who were randomly assigned to either the AutoPulse, LUCAS or manual compressions. This was a single-centre randomised controlled non-inferiority trial comparing the use of each device against protocols employing manual chest compressions only. This was a study designed to assess safety of the mechanical devices, with the primary outcome being "serious or life-threatening visceral resuscitation-related injury". The devices were not directly compared in this study.

Excluded studies

Common reasons for exclusion were that the article described an experiment involving the use of a non-powered active compression-decompression device as an adjunct to manual CPR (such as a manually operated suction cup) and that the article described a narrative literature review and provided no original data (Excluded studies).

Risk of bias in included studies

The results of our assessment for risk of bias in included studies can be seen in the Characteristics of included studies table and are summarised in Figure 2 and Figure 3. The overall quality of the available data has improved considerably with the three large-scale RCTs included in this update (Perkins 2015; Rubertsson 2014; Wik 2014), along with two smaller studies (Gao 2016; Koster 2017).

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

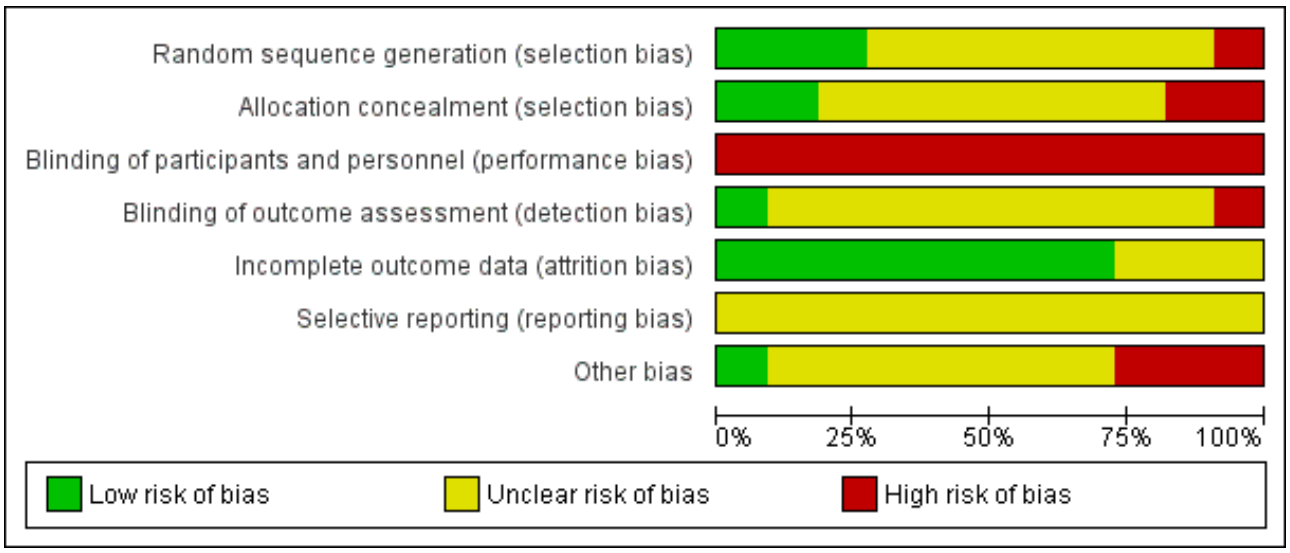
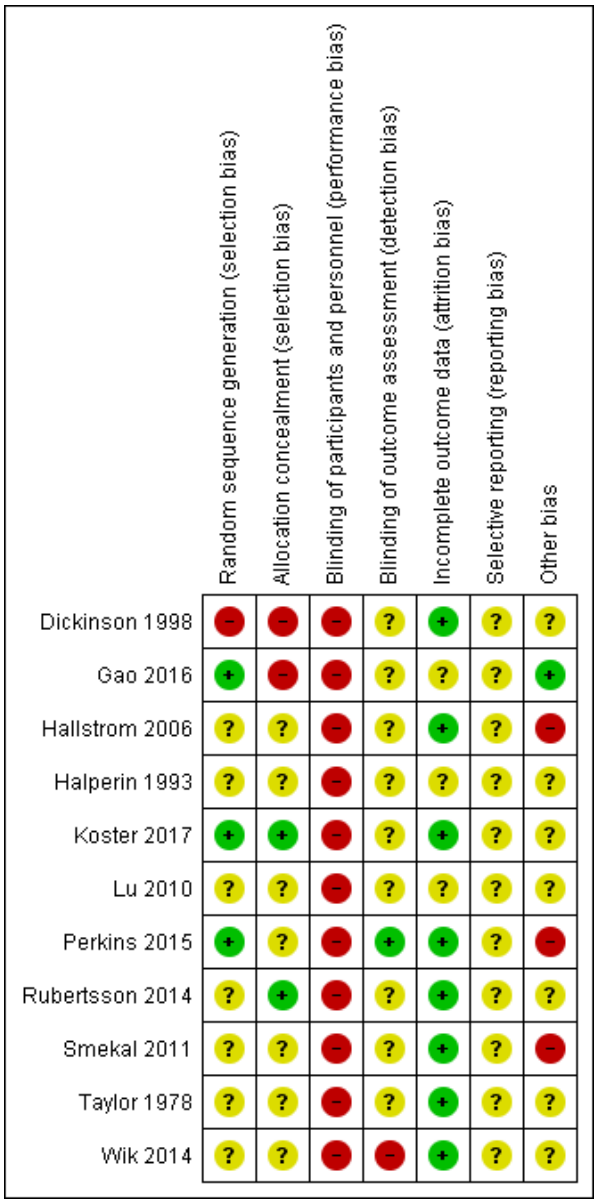


Figure 3. Risk of bias summary: review authors' judgements about each risk of bias item for each included study.



Allocation

Random sequence generation

Three of the newly included studies, using a cluster randomisation study design, described using a computer-generated sequence to randomise ambulance vehicles, placing them at low risk of bias with respect to the randomisation process (Gao 2016; Koster 2017; Perkins 2015). In four studies using simple randomisation (Halperin 1993; Lu 2010; Smekal 2011; Taylor 1978), and one study using cluster randomisation (Hallstrom 2006), the method of random allocation was not adequately reported. Two other studies referred to the use of randomisation envelopes, although they did not report on the specific method of generating the sequence (Rubertsson 2014; Wik 2014). We judged these seven studies to also have an unclear risk of bias. One study was self-described as "quasi-randomised" using odd and even days (Dickinson 1998), placing it at a high risk of bias.

Allocation concealment

Two studies adequately described their method of allocation concealment, including measures to protect against foreknowledge and track attempts to subvert the randomisation by using opaque, sealed envelopes (Koster 2017; Rubertsson 2014). If the use of opaque envelopes was not mentioned explicitly, we deemed the risk of bias to be unclear (Hallstrom 2006; Lu 2010; Perkins 2015; Taylor 1978). For two studies, we felt that the risk of bias was high (Dickinson 1998; Gao 2016). For example, in the Gao cluster-randomised trial, ambulances were randomised to be carrying the mechanical chest compression device or not. Ambulances were dispatched to OHCA emergencies, but it is not clear whether dispatchers might send ambulances carrying the mechanical device to suspected OHCA event with particular characteristics preferentially. Some of the baseline characteristics between treatment groups are different (e.g. initial cardiac arrest rhythm and witness status), suggesting this source of selection

bias might have occurred. The Dickinson 1998 study was quasi-randomised using odd and even days. For studies that did not mention allocation concealment, the risk of bias was unclear (Halperin 1993; Smekal 2011; Wik 2014).

Blinding

In all studies, since the blinding of study personnel was not feasible given the nature of the intervention, we judged these to be at a high risk of performance bias. The CIRC trial mentions that study personnel were not always blinded to the study arm, although the extent and reason were not explained, thus placing it at a high risk of detection bias (Wik 2014). Blinding of outcome assessors was only addressed by the PARAMEDIC study (blinding of research nurses) and the study by Koster et al (blinding of study assessors) (Koster 2017; Perkins 2015). We thus judged the PARAMEDIC study to be at low risk of detection bias (Perkins 2015). However, we determined that the risk of bias for the Koster study was unclear as it mentions that the pathologists performing the autopsies could not be completely blinded due to occasional skin markings on the chest left by mechanical devices (Koster 2017).

Incomplete outcome data

Most trials had excellent follow-up and very few withdrawals for most outcomes, placing them at a low risk of bias (Dickinson 1998; Hallstrom 2006; Koster 2017; Perkins 2015; Rubertsson 2014; Smekal 2011; Taylor 1978; Wik 2014). One study did not report the number of drop outs and as such, the risk was unclear (Halperin 1993). With respect to neurologic outcomes, the CIRC trial notes that this was not known 26% (manual compression) and 30% (mechanical compression) of survivors. However it is unlikely to be a significant source of bias given that the loss is similar in both arms (Wik 2014). Two studies did not explicitly report incomplete or loss to follow-up data, thus the risk is unclear (Gao 2016; Lu 2010).

Selective reporting

Selective reporting bias was difficult to determine because the protocols of several included studies were not available. Two studies chose to report survival to four hours as the primary outcome (Hallstrom 2006; Rubertsson 2014), which is uncommon and not recognised by international consensus groups as a key standard outcome (Utstein 2015). However, all but the Taylor 1978 study and the PARAMEDIC trial (Perkins 2015), reported all outcomes stated in the methods. The PARAMEDIC study did not report on survival to hospital discharge as originally intended in the study protocol because they felt that survival to 30 days was more clinically relevant (Perkins 2010a), and they were unable to completely collect data from hospitals (Perkins 2015).

Seven trials reported adverse effects such as rib or sternal fracture, haemothorax or pneumothorax or other internal abdominal organ injury (Gao 2016; Halperin 1993; Koster 2017; Lu 2010; Rubertsson 2014; Taylor 1978; Wik 2014). Three studies did so with the use of autopsy data, which were carried out on a minority of participants (Halperin 1993; Koster 2017; Taylor 1978). Two studies also did not specify the criteria for autopsy (Halperin 1993; Taylor 1978).The LINC and CIRC trials relied on reports of adverse events by emergency and hospital personnel (Rubertsson 2014; Wik 2014). There is the potential for selective investigation, or reporting of adverse events, or both, which may incorporate significant potential bias into these data.

Other potential sources of bias

Cointervention or contamination

Cointervention or contamination were present in several studies (for studies that were already included in the previous review, see Risk of bias in included studies). Notably, the PARAMEDIC study had unbalanced cross-over (39% mechanical to manual; < 1% manual to mechanical) that may understate the effect of mechanical CPR, if effective (Perkins 2015). This was determined to be at a high risk of bias. The LINC study also used an unconventional CPR algorithm where the mechanical CPR group received a defibrillator shock without pulse/rhythm check and followed three-minute CPR periods (Rubertsson 2014). It is unclear what the impact this cointervention may have had on the measured outcomes.

Differences at baseline and potential confounders

There was inadequate reporting, or significant differences between the study arms with respect to baseline characteristics, or both, that may potentially act as important confounders. Four of the 11 studies failed to report baseline participant characteristics, including the suspected cause of the cardiac arrest, the circumstances of the cardiac arrest (e.g. witnessed versus unwitnessed), the initial cardiac arrest rhythm and other important prognostic factors (Dickinson 1998; Halperin 1993; Lu 2010; Taylor 1978). The impact of these potential confounding variables was impossible to assess, which raises the possibility of selection and allocation bias, and was deemed to be an unclear risk of bias. In general, reporting of the randomisation process was inadequate.

In the Hallstrom 2006 study, the distribution of body habitus was dissimilar between groups. More "thin" and "morbidly obese" participants were included in the mechanical chest compression group. The impact of this imbalance was not explored in the analysis. Intuitively, body shape may interact with treatment effect because of the physics of the mechanical device. Also, among participants with ventricular fibrillation or pulseless ventricular tachycardia, those in the mechanical CPR group had an additional 2.1-minute delay from the emergency call to first defibrillation attempt compared with those in the manual CPR group. This important difference in a well-recognised prognostic factor for survival was not further explored as a potential modifier of effect. Taken together, this study is at a high risk of bias. Two studies had a markedly higher occurrence of ventricular fibrillation or pulseless ventricular tachycardia as the initial rhythm in the mechanical group compared to the manual group (47.1% versus 17% in the Halperin study; and 24% versus 21% in the CIRC study). This was not addressed by Halperin in the text or analysis of the study (Halperin 1993), thus at an unclear risk of bias. In many cases, reporting of baseline characteristics and known prognostic factors for cardiac arrest was insufficient to account for potential confounders.

The quality of CPR, whether or not it incorporated a mechanical device, with respect to depth, rate, consistency of compressions and interruptions, was not reported in nine of 11 studies. Chest compression fraction is the percentage of time during which providers perform chest compressions during a cardiac arrest. In the CIRC trial, CPR fraction was monitored consistently (for 96% of patients) in both the mechanical and manual groups (mechanical 75% versus manual 79%) (Wik 2014). By comparison, the LINC study only monitored 10% of patients with impedence, and reported similar CPR fractions (mechanical 84% versus manual 78%) (Rubertsson 2014). Both are higher than the CPR fraction

reported in the Hallstrom 2006 study (mechanical 59% versus manual 60%, reported for 52% and 45% of patients, respectively). It is recognised that this is an important potential confounder in all cardiac arrest trials, thus the risk of bias for these studies is unclear. The quality of CPR received by participants in the manual CPR group is of paramount importance when assessing the results of comparative analyses. Substandard manual CPR administered by poorly trained providers during a study might inflate the measured relative treatment effect associated with the use of a mechanical CPR device in comparison to the treatment effect observed in a study with higher quality CPR in the manual chest compression control arm.

Effects of interventions

See: **Summary of findings for the main comparison** Mechanical chest compressions compared to manual chest compressions for cardiac arrest

A summary of the data from included studies can be found in Summary of findings for the main comparison. We did not pool data because of substantial clinical heterogeneity in the studies, including in the type of device (Thumper, pneumatic vest and LUCAS), participant (in-hospital, out-of-hospital), timing of device use (after failed conventional CPR versus during first attempts at CPR) and year of publication (studies span 1993 to 2017). Year of publication is important to consider with respect to clinical heterogeneity because recommendations for CPR, including the nature of manual chest compression, have changed drastically over the past 20 years (Perkins 2015a).

We assessed the quality of evidence for each outcome using the GRADE system (GRADEpro GDT 2015), with emphasis on the domains for risk of bias, inconsistency, indirectness, imprecision, and publication bias. For all outcomes, we did not deem that there was evidence of serious risk of indirectness.

Mechanical chest compressions compared to manual chest compressions for cardiac arrest

Survival to hospital discharge with good neurological function

There was moderate-quality evidence (downgraded for serious risk of bias) for the outcome of survival to hospital discharge with good neurological function (primary outcome of this review) reported by three studies involving 7587 patients (Hallstrom 2006; Rubertsson 2014; Wik 2014). Only the Hallstrom 2006 study observed a notable decrease in survival to hospital discharge with good neurological function (defined as Cerebral Performance Category score (CPC) 1 or 2) with the use of mechanical chest compressions as compared with manual chest compressions (3.1% versus 7.5%; risk ratio (RR) 0.41, 95% confidence interval (CI) 0.21 to 0.79) (Analysis 1.1). The LINC trial did not demonstrate a difference between treatment groups for this outcome (CPC 1 or 2; mechanical 8.3% versus manual 7.8%; RR 1.07, 95% CI 0.82 to 1.39) while the CIRC trial (modified Rankin Scale (mRS) 0 to 3; mechanical 4.1% versus manual 5.3%; RR 0.79, 95% CI 0.60 to 1.04) demonstrated equivalence between the two arms (Rubertsson 2014; Wik 2014). The adjusted odds ratio for the mechanical compression arm versus the manual compression arm was 1.06 with a 95% CI of 0.83 to 1.37 which lay fully within the predefined equivalence region (Wik 2014).

Survival to hospital discharge

There was moderate-quality evidence (downgraded for serious risk of bias) for the outcome of survival to hospital discharge reported in seven studies involving 8067 patients (Gao 2016; Hallstrom 2006; Lu 2010; Rubertsson 2014; Smekal 2011; Taylor 1978; Wik 2014). Two studies using the LUCAS device (Rubertsson 2014; Smekal 2011), one study using a piston device (Taylor 1978), and one study using a load-distributing band device (Wik 2014), did not show harm or benefit when compared with manual compressions (Rubertsson 2014: 9.0% versus 9.15%; RR 0.98, 95% CI 0.77 to 1.25; Smekal 2011: 8.0% versus 9.72%; RR 0.82, 95% CI 0.29 to 2.33; Taylor 1978: 12.5% versus 7.7%; RR 1.63, 95% CI 0.30 to 8.90; Wik 2014: 9.34% versus 10.93%; RR 0.85, 95% CI 0.71 to 1.02). Two studies, one using a piston for in-hospital cardiac arrest (IHCA) (Lu 2010), and the other using the AutoPulse for out-of-hospital cardiac arrest (OHCA) (Gao 2016), showed benefit of mechanical compressions (Lu 2010: 32.9% versus 14.7%; RR 2.21, 95% CI 1.18 to 4.17; Gao 2016: 18.8% versus 6.3%; RR 3.01, 95% CI 1.04 to 8.77).

Only one study favoured manual compressions (Hallstrom 2006). Hallstrom and colleagues generated an odds ratio (OR) 0.56 (95% CI 0.31 to 1.00) favouring manual chest compressions using multivariate logistic regression that adjusted for the clustering effect, age, initial rhythm, witness status, emergency medical services site, response time of first vehicle and location of arrest (public versus private) (Hallstrom 2006). (Note: this OR generated by the authors of the Hallstrom study using logistic regression differs slightly from that generated in Analysis 1.2 of this review using raw proportions abstracted from the published manuscript).

Return of spontaneous circulation

There was low-quality evidence (downgraded for serious risk of bias and inconsistency) for the outcome of return of spontaneous circulation reported by eight studies involving 11,771 patients (Dickinson 1998; Gao 2016; Halperin 1993; Lu 2010; Perkins 2015; Rubertsson 2014; Smekal 2011; Wik 2014; Analysis 1.3). Three studies (N = 300) showed benefit with mechanical chest compressions compared with manual compressions (Dickinson 1998: 14.3% versus 0%; RR 4.13, 95% CI 0.19 to 88.71; Lu 2010: 55.3% versus 37.8%; RR 1.46, 95% CI 1.02 to 2.08; Gao 2016: 44.9% versus 23.4%; RR 1.92, 95% CI 1.15 to 3.21). Four studies (N = 7240) showed neither harm nor benefit (Halperin 1993: 47.1% versus 17.6%; RR 2.67, 95% CI 0.85 to 8.37; Perkins 2015: 31.6% versus 31.4%; RR 1.01, 95% CI 0.92 to 1.10; Rubertsson 2014: 35.4% versus 34.6%; RR 1.02, 95% CI 0.92 to 1.14; Smekal 2011: 40.5% versus 31.9%; RR 1.27, 95% CI 0.82 to 1.96). In the large CIRC trial of 4231 patients, the Wik 2014 study calculated a relative risk, which was not adjusted for their interim analysis, of 0.88 (95% CI 0.81 to 0.97), thus favouring manual chest compressions for return of spontaneous circulation.

Survival to hospital admission

There was moderate-quality evidence (downgraded for serious risk of bias) for the outcome of survival to hospital admission reported by four studies involving 7224 patients (Dickinson 1998; Perkins 2015; Rubertsson 2014; Smekal 2011; Analysis 1.4). No studies showed a difference in survival to hospital admission (Dickinson 1998: 14.3% versus 0%; RR 4.13, 95% CI 0.19 to 88.71; Perkins 2015: 22.8% versus 23.3%; RR 0.98, 95% CI 0.87 to 1.09; Rubertsson 2014: 28.2% versus 27.7%; RR 1.02, 95% CI 0.90 to 1.15; Smekal 2011: 24% versus 20.8%; RR 1.15, 95% CI 0.63 to 2.11).

Survival to other time points

The Perkins 2015 study reported ORs (adjusted for age, sex, response time, bystander CPR and initial rhythm, but not clustering design) for survival to three months with good neurological function (OR 0.72, 95% CI 0.52 to 0.99), suggesting harm associated with mechanical chest compression, while survival to three months (OR 0.83, 95% CI 0.61 to 1.12) and to 12 months (OR 0.83, 95% CI 0.61 to 1.11) did not provide evidence for a difference associated with mechanical chest compression. Similarly, the Rubertsson 2014 study reported absolute risk differences (RDs) and found no difference in survival to one month (RD 0.16, 95% CI -2.0 to 2.3) and six months (RD 0.47, 95% CI -1.7 to 2.6).

Among the included studies, a variety of other endpoints were reported for short-term survival (< 30 days). In one trial, the point estimate for survival to 24 hours mildly favoured manual compressions (adjusted OR 0.86, 95% CI 0.74 to 0.998) but the upper limit of the 95% CI was very close to unity (Wik 2014). No difference was found in four-hour survival by either the Rubertsson 2014 study (23.6% versus 23.7%; P < 0.99, 95% CI -3.3% to 3.2%) or the Hallstrom 2006 study (26.4% versus 24.7%; reported P = 0.62).

Adverse effects

Several included studies reported adverse effect and time interval data, but we did not conduct pooled analyses were not done because of considerable clinical heterogeneity. Specifically, the type of device and the mechanism of the device were thought to be of paramount relevance to injury patterns observed in treated participants.

There was low-quality evidence (downgraded for serious risk of bias and imprecision) for the outcome of sternal or rib fractures reported by seven studies involving 7469 patients (Gao 2016; Halperin 1993; Koster 2017; Lu 2010; Rubertsson 2014; Taylor 1978; Wik 2014). Point estimates for RRs derived from the data reported in these studies were divergent. Data from the Wik 2014 study suggested a two-fold increase in the risk for rib or sternal fractures with mechanical chest compressions compared to manual compressions (RR 1.98, 95% CI 1.33 to 2.94; Analysis 1.5).

For the outcome of pneumothorax or haemothorax, there was low-quality evidence (downgraded for serious risk of bias and imprecision) reported by five studies involving 7316 patients (Halperin 1993; Koster 2017; Lu 2010; Rubertsson 2014; Wik 2014). Of these, three studies had very wide CIs due to the low number of events (Analysis 1.6).

For the outcome of other internal organ injury, there was low-quality evidence (downgraded for serious risk of bias and imprecision) reported by five studies involving 7337 patients (Koster 2017; Lu 2010; Rubertsson 2014; Taylor 1978; Wik 2014; Analysis 1.7).

The Koster 2017 study deserves special mention here because it was designed with injury as the primary outcome. This was a three-arm randomised controlled non-inferiority trial with a primary outcome of "serious or life-threatening visceral resuscitation-related damage" detected by post-resuscitation computed tomography (CT) scan, autopsy and clinical follow-up. 'Life-threatening' was defined as "reasonably expected to interfere with cardiovascular or respiratory function, exsanguination in excess of 800 mL" and 'serious' was defined as "demands therapy for repair or for alleviation of pain, expected to prolong hospitalisation". Patients with OHCA or IHCA were randomised to receive standard manual compressions, AutoPulse compressions or LUCAS compressions. The non-inferiority margin was set a priori to be < 10% excess serious or life-threatening resuscitation-related injury. The primary outcome was determined by a panel of clinicians using all available information. Blinding outcome assessors was attempted, but it was admitted by the study authors that the mechanical devices often left characteristic marks on the chest that may have undermined attempts. It was not clear whether outcome assessors were blinded to study design (e.g. non-inferiority trial) which is important to avoid bias and a type I error in non-inferiority studies (New Reference). The risk difference for AutoPulse compared to manual compressions for the primary outcome was +5.3% (95% CI - 2.2% to 12.8%; P= 0.15) which did not satisfy criteria for non-inferiority. The risk difference for LUCAS compressions compared to manual compressions for the primary outcome was +1.0% (95% CI - 5.5% to 7.6%; P= 0.75), which met non-inferiority criteria. The study authors concluded that LUCAS compressions do not cause significantly more serious or life-threatening visceral damage than manual chest compressions. They also concluded that for AutoPulse, significantly more serious or life-threatening visceral damage than manual chest compressions cannot be excluded.

Time interval data

Time interval data were reported by four studies (Hallstrom 2006; Rubertsson 2014; Smekal 2011; Wik 2014). In three studies, there was a delay in the first shock with the mechanical compression group compared to the manual compression group. In the Hallstrom 2006 trial, time from the emergency call to first rhythm analysis was similar in the two groups (8.9 ± 3.0 minutes versus 8.9 ± 2.9 minutes), but the emergency call to first shock interval in those participants with a shockable rhythm was longer in the mechanical chest compression group than in the manual chest compression group (11.8 ± 6.1 minutes versus 9.7 ± 3.1 minutes, reported P = 0.001). In the LINC trial, intervals were measured from the estimated time of cardiac arrest (Rubertsson 2014). The median interval between cardiac arrest and emergency call was two minutes (interquartile range (IQR) 0 to 5) in both arms. Notably, there was a 1.5-minute delay in time from cardiac arrest to first defibrillation in the mechanical group (17, IQR 12 to 22) compared to the manual group (15.5, IQR 11 to 23.5). This delay may be at least partly explained by the protocol which states that the first shock was to be delivered at 90 seconds after the start of mechanical CPR without a rhythm check. In the CIRC trial, the average interval from scene arrival to first shock for patients in ventricular fibrillation/ventricular tachycardia was slightly longer for the mechanical arm (7.5 ± 6.0 minutes versus 6.7 ± 6.2 minutes) (Wik 2014).

The Smekal 2011 trial reported similar scene arrival time to first CPR interval (1.0 ± 1.1 minutes versus 1.1 ± 1.1 minutes). The Smekal 2011 trial also reported cardiac arrest time to start of LUCAS device (13.1 ± 7.2 minutes), cardiac arrest time to start of CPR (10.4 ± 6.6 minutes versus 10.2 ± 5.9 minutes) and call from dispatch centre to start of CPR (8.3 ± 5.8 minutes versus 7.5 ± 3.6 minutes).

Subgroup analyses

Initial cardiac arrest rhythm

We planned a subgroup analysis on the basis of initial cardiac arrest rhythm, however, only two of the included studies reported

outcomes by initial cardiac arrest rhythm (Hallstrom 2006; Perkins 2015). We did not undertake a pooled analysis because of insufficient data and significant heterogeneity. In the Hallstrom 2006 study, four-hour survival was higher in the mechanical group as compared to the manual group (17.2% versus 10.4%). This difference was labelled as a "trend" by authors in the absence of reporting any statistical hypothesis testing being reported in the manuscript. In the Perkins 2015 study, survival was lower among those treated with a mechanical chest compression device compared with those treated with only manual chest compressions in the subgroup of patients having ventricular fibrillation or ventricular tachycardia as their initial cardiac arrest rhythm (OR 0.71, 95% CI 0.52-0.98). There was no difference between treatment groups in the subgroup of patients with pulseless electrical activity (OR 1.38, 95% CI 0.80-2.36).

DISCUSSION

Summary of main results

After an extensive search, we identified 11 randomised trials, with data from 12,944 participants, which included 10 randomised controlled trials (RCTs) (8 randomised at the patient level and two cluster-randomised) and one quasi-randomised trial. The publication dates of these studies span five decades (1978 to 2015), and results demonstrate marked heterogeneity in participant selection (out-of-hospital, in-hospital), cardiac arrest aetiologies, timing of device application (immediate versus delayed after failed traditional cardiopulmonary resuscitation (CPR)), types of mechanical devices used and control group CPR protocols.

This update has added three large RCTs (which account for 90% of all patients in this review). The primary outcome of this review, survival to hospital discharge with good neurological function, has only been reported for out-of-hospital cardiac arrest (OHCA) in the LINC, CIRC and Hallstrom studies (Hallstrom 2006; Rubertsson 2014; Wik 2014). In contrast to the previous results by Hallstrom et al, which suggested that mechanical compressions may be harmful, the new data from the CIRC and LINC trials showed no difference in survival to discharge with good neurological function (Rubertsson 2014; Wik 2014). The CIRC study concluded equivalence between mechanical and manual arms with respect to this outcome (Wik 2014). There was no evidence of a difference between treatment arms in short-term (< 30 days) survival reported in the CIRC, LINC and PARAMEDIC trials (Perkins 2015; Rubertsson 2014; Wik 2014; Characteristics of included studies). However, the PARAMEDIC study did report an adjusted OR for neurologically favourable survival at three months, suggesting superiority of manual chest compressions (Perkins 2015). None of the other large clinical trials reported strong evidence for differences in long-term outcomes. There is no strong evidence to suggest that mechanical chest compression devices cause an excess of adverse effects compared with manual chest compressions, however, most studies included in this review did not report a robust methodology for identifying CPR-related injuries in either arm.

The bulk of available evidence from RCTs included in this review would suggest that CPR protocols involving mechanical chest compression devices produce similar clinical outcomes compared to manual CPR protocols involving very high-quality chest compressions. This finding seems to conflict with the preclinical and observational data demonstrating that mechanical devices can provide superior compressions with respect to rate, depth and consistency resulting in superior haemodynamic effects compared to manual compressions. Many may ask why these findings have not translated into improved clinical outcomes in published randomised clinical trials.

It is possible that the superiority of mechanical devices observed in previous observational studies was spurious and the manifestation of bias. Most importantly, the issue of selection bias needs to be considered. Where patients were not randomised, they may have been selected for mechanical chest compression device use by healthcare providers based on characteristics (measured or unmeasured) favourable for good outcome.

Integration of mechanical devices into the resuscitation algorithms of included studies may have had negative impacts on overall CPR quality (e.g. pauses in chest compressions during device deployment and delayed first defibrillation in patients with ventricular fibrillation) negating any physiologic benefit observed in preclinical studies. Time to first defibrillation was longer in the mechanical chest compression arms of several studies included in this review (Hallstrom 2006; Rubertsson 2014; Wik 2014).

Consideration of the control groups is paramount in interpreting the results of these studies. Conclusions of superiority or equivalence of mechanical chest compression devices to manual chest compressions are entirely dependent on the quality of resuscitation provided in the manual chest compression group. All else being the same, a study with poor-quality manual compressions is more likely to favour mechanical devices. The Wik 2014 study, for example, involved significant training and careful monitoring of CPR quality in both arms; CPR fraction in the control arm was 79%. The equivalence conclusion of this study needs to be carefully qualified because the comparison group received very high-quality manual chest compressions. Had the quality of manual chest compressions been lower, the results may have been different. Similarly, the LINC trial demonstrated a CPR fraction of 78% in the manual chest compression arm (Rubertsson 2014). This high-quality manual chest compression is likely not reflective of most emergency medical services systems in the world, with many prior studies reporting much lower CPR fractions closer to 50% (Wik 2005). Therefore, the relative benefit of implementing mechanical chest compression devices may be different than that demonstrated by the trials included in this review, reporting high CPR quality in the control group.

Improved haemodynamics associated with the use of mechanical chest compression devices may not translate into improved clinical outcomes for several reasons. Although we did not identify any evidence to suggest an overall excess of injuries related to the use of mechanical chest compression devices, there may have been unmeasured and unreported complications from mechanical chest compression devices such as rib, lung, cardiac or intra-abdominal injuries that may have negated the impact of any improvement in haemodynamics during cardiac arrest associated with mechanical devices. As with many other advanced therapies for cardiac arrest, they are often implemented with some delay after the occurrence of cardiac arrest. Perhaps the window of opportunity for improved haemodynamics to impact outcomes is closed by the time mechanical chest devices can be reasonably implemented in most cardiac arrests.

Overall completeness and applicability of evidence

The completion of the CIRC, LINC and PARAMEDIC trials brings in results backed by large sample sizes that more thoroughly address the primary and secondary outcomes defined in this review. With these studies' inclusion, the vast majority of the participants are OHCA patients. We made the decision to exclude non-randomised studies because of their vulnerability to additional biases, including differences in quality of care between crews and emergency medical services systems (non-randomised concurrent controls), changes in practice and in quality of care over time (historical controls), the Hawthorne effect (historical or concurrent controls) and selection bias (preferential application of a device to participants thought to have a very poor prognosis in the hope that use of the device might lead to better outcomes than are attained with standard care). Most studies have focused on two mechanical devices, the LUCAS device and the AutoPulse. To date, four trials are still in progress and have not yet been published (CTRI/2013/07/003840; ISRCTN38139840; ISRCTN78354073; NCT01521208).

Certain potentially important patient subgroups were either under-represented or not reported. For instance, there were less females in the studies included in our review and body mass index was under-reported. There may be important differences in the effectiveness of mechanical chest compression devices in these important and under-represented subgroups (such as females and morbidly obese), but the data did not allow such subgroup analysis. Generalisability of our results for these important subgroups of patients is unclear.

The data identified in this review did not support a robust subgroup analysis of outcomes on the basis of initial cardiac rhythm. This is a major limitation of the data because defibrillation is a crucial and time-dependent element of treatment for patients with shockable initial rhythms. The use of mechanical chest compression devices and their effect on defibrillation must be carefully considered. Only two of the studies included in the review reported outcomes by initial cardiac arrest rhythm (Hallstrom 2006; Perkins 2015). Although this requires further investigation to confirm, results from both of these studies suggest that mechanical chest compression devices may be more beneficial in patients with non-shockable rhythms. The differential effect in rhythm subgroups may relate to delayed defibrillation or some other unmeasured effect of these machines on defibrillation (e.g. pad placement). Further evidence is required on the relative efficacy of mechanical chest compression devices in patients with different initial rhythms.

None of the included studies reported outcomes by response time interval. It may be that mechanical chest compression devices have a differential effect among patients treated earlier in the cardiac arrest as opposed to later in the cardiac arrest. Time to defibrillation among those with shockable rhythms and reduced no-flow time may relate to shorter response time intervals. Available data are unable to shed light on whether mechanical chest compression devices may be more or less effective at different time points along the timeline from collapse to resuscitation initiation.

Quality of the evidence

The quality of the evidence for each outcome ranged from low to moderate quality, assessed using the GRADE approach (GRADEpro GDT 2015) (Summary of findings for the main comparison).

Limitations in study design - risk of bias

The methods of randomisation and allocation concealment were unclear in the majority of studies and we assessed all studies as unclear with regard to selective reporting (Figure 2; Figure 3).

CPR algorithms for both the manual and mechanical compression groups have been described in the new clinical trials added since our last update. The lack of CPR process and quality description in the older studies has been highlighted as a major methodological limitation and potential source of bias in previous versions of this review. These descriptions highlight a number of notable differences in how CPR was delivered across studies.

The Smekal 2011 trial is subject to problems with the cointervention, as a modified CPR algorithm was used in the mechanical chest compression group. The much larger LINC trial by the same authors also used a study-specific algorithm allowing cointervention in the mechanical group where defibrillation was given without rhythm analysis (Rubertsson 2014). The manual CPR group also followed the outdated 2005 European Resuscitation Guidelines. It is difficult to judge how much of the observed treatment effect could be explained by a difference in CPR process rather than by use of the chest compression device alone.

In contrast to the focus on high-quality CPR through training programmes and continuous monitoring of compliance with mechanical device deployment in the CIRC trial, the PARAMEDIC study was designed as a pragmatic trial. As such, they implemented mechanical CPR training according to routine organisation practices (Perkins 2015). It is unclear how much oversight was present to ensure compliance given that 40% of the group assigned to the mechanical chest compression group received manual compressions only. This may be a reflection of real-world challenges in deploying the device. There may be unwillingness among emergency medical services providers to deploy the device on the basis of prejudice (i.e. a belief that manual compressions are superior) or inconvenience. Thus, their results may be interpreted to either reflect an underestimate of any true treatment effect associated with mechanical CPR (efficacy) or an accurate estimate of the effectiveness of mechanical CPR when deployed in the real world.

The Hallstrom 2006 study, which suggested an association between the use of mechanical chest compression devices and worse outcomes, was at risk for bias in several forms. Specifically, the mechanical chest compression group, although balanced with respect to the number of people with ventricular fibrillation or pulseless ventricular tachycardia, had an average 2.1 minutes extra delay to first defibrillation attempt. Because three options were available for integration of the device into the CPR protocol, and because complete CPR process data were lacking, it is unclear whether this delay was a result of time needed for device deployment and to what degree this delay could have been reduced by improved training. It is plausible that this delay is partially, if not completely, responsible for the differences in outcomes between the two groups. A reanalysis of the original trial data has demonstrated that the harm associated with mechanical chest compressions in the analysis was entirely dependent on data arising from the single site (site C) that incorporated a protocol change part-way through the study, which entailed delayed deployment of the device during the resuscitation sequence (Paradis 2010). Although evidence for differential effects of the

AutoPulse device on participants with different body types is not known to the authors of this review, it is possible that the excess of extreme body types in the mechanical CPR group ("thin" and "morbidly obese") may have had an impact on the efficacy of the device that was unaccounted for in the analysis. Prestudy outcomes at each of the participating sites were not reported, so it is difficult to assess whether a Hawthorne effect may have exerted a positive influence on the quality of manual chest compressions in such a way as to make it more challenging for the mechanical chest compression arm to show superiority. Data from the Hallstrom 2006 study highlight the potential importance of device deployment with respect to optimising clinical outcomes.

The CIRC study (Wik 2014), which is the largest and most robust of the identified trials, was designed to overcome some of the limitations of the Hallstrom 2006 study. Specifically, the study protocol involved consistent CPR monitoring (by transthoracic impedance or accelerometer date) and three study phases (in-field training, run-in and statistical inclusion) to minimise biases and learning effects (Wik 2014). Wik and colleagues noted a higher than normal CPR quality which drew them to the conclusion that mechanical CPR was statistically equivalent to high-quality manual CPR.

Several studies did not adequately describe participants included in the study with respect to important covariates known to be associated with survival. For instance, the Lu 2010 study did not report the initial rhythm for included participants. Imbalance between treatment groups with respect to this important prognostic factor may have contributed to observed differences in return of spontaneous circulation.

Inconsistency of results

We did not pool studies on the basis of significant clinical heterogeneity among included studies with regards to population and intervention characteristics. I^2 values for all survival outcomes demonstrated signficant statistical heterogeneity (64% to 75%). Based on assessment of non-overlapping confidence intervals and variance of point estimates for the outcome, return of spontaneous circulation, we downgraded it for inconsistency.

Indirectness of evidence

Although there was some indirectness across studies, we did not feel that there was enough to downgrade our quality of evidence assessments for any of the outcomes. Some studies provided evidence that was indirect with respect to the population studied. For example, the Halperin 1993 study included patients who had failed conventional resuscitation. These patients were much further along in the process towards irreversible death than our target population for this intervention. Some studies provided evidence that was indirect by way of the intervention studied. For instance, some of the devices studied are not commercially available and mechanistically different to those available (e.g. the vest device in the Halperin study).

Imprecision

Imprecision is indicated in several outcomes by excessively wide confidence intervals. This is most pronounced in the outcomes related to adverse events. We downgraded each of the adverse events outcomes for imprecision on this basis.

Publication bias

We have no strong evidence for publication bias and did not downgrade quality assessments on this basis for any of the outcomes assessed. We felt that there were too few studies for funnel plots to be reliable. We found no evidence of unpublished studies in our assessment of clinical trial registries.

Potential biases in the review process

The strengths of this review lie in the comprehensive search of the literature involving several databases, handsearching and contact with experts and industry contacts. Two independent investigators completed reviews for inclusion and data abstraction, reducing the opportunity for some forms of measurement bias.

The decision to limit the content of the review to randomised studies means that we may have missed some important data from non-randomised studies. Our search strategy may have missed some unpublished studies because it focused on databases that primarily include published work.

Agreements and disagreements with other studies or reviews

Since the last update, there have been several additional meta-analyses beyond the Westfall 2013 review that take the CIRC, LINC and PARAMEDIC trials into account (Gates 2015; Tang 2015). We determined at the outset of the original review to not pursue pooling of data if either the descriptive clinical heterogeneity or the statistical heterogeneity were too substantial (I^2 > 50%). Nonetheless, the cumulative data described in this review are in agreement with these other reviews; namely, that mechanical compressions do not offer a short- or long-term survival advantage when compared with manual compressions and may be equivalent to CPR with high-quality manual compressions.

AUTHORS' CONCLUSIONS

Implications for practice

The evidence does not suggest that CPR protocols involving mechanical chest compression devices are superior to conventional therapy involving manual chest compressions only. We conclude on the balance of evidence that mechanical chest compression devices used by trained individuals are a reasonable alternative to manual chest compressions in settings where consistent, high-quality manual chest compressions are not possible or dangerous for the provider (eg, limited rescuers available, prolonged CPR, during hypothermic cardiac arrest, in a moving ambulance, in the angiography suite, during preparation for extracorporeal CPR [ECPR], etc.). Systems choosing to incorporate mechanical chest compression devices should be closely monitored because some data identified in this review suggested harm. Special attention should be paid to minimising time without compressions and delays to defibrillation during device deployment.

Implications for research

Future research should study the effect of mechanical chest compression devices in special scenarios where sustainable high-quality manual chest compressions are challenging or pose a risk to providers. Examples of these scenarios include CPR in a moving ambulance, situations where limited rescuers are available,

during prolonged resuscitations (e.g. accidental hypothermia, toxicological causes), in the angiography suite or during the implementation of extracorporeal cardiopulmonary resuscitation. The relative effect of mechanical chest compressions in important subpopulations requires investigation. For example, patients with non-shockable initial cardiac arrest rhythms may benefit more from mechanical compressions when defibrillation is not indicated and there are no concerns about delay to successful defibrillation associated with implementation of devices.

There is a paucity of data from direct comparisons of different devices with respect to ease of implementation and patient safety. Improved methods of implementation and deployment of the devices which minimise no flow time and delays to defibrillation could be developed. Any research involving mechanical chest compression devices should include the accurate measurement of CPR process in all arms of the study. Data on the use of mechanical chest compression devices in the paediatric population is scant and should be the focus of future investigations.

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* Indicates the major publication for the study

CHARACTERISTICS OF STUDIES

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

Dickinson 1998

Methods	Quasi-randomised trial in the USA Randomisation based on odd/even calendar days
Participants	Adults with out-of-hospital, atraumatic cardiac arrest with resuscitation attempted by paramedics

Dickinson 1998 *(Continued)*

	N = 17 Mechanical mean age: not reported. Male %: not reported Initial rhythm VF: 2/7 (28.5%). PEA: 2/7 (28.5%). Asystole: 3/7 (42.9%) Hypothermia treatment post-arrest: not reported Manual mean age: not reported. Male %: not reported Initial rhythm VF: 4/10 (40%). PEA: 4/10 (40%). Asystole: 2/10 (20%) Hypothermia treatment post-arrest: not reported Exclusions: none reported
Interventions	Mechanical: "Thumper" piston at 80 compressions/min. Compression/ventilation ratio: not reported Manual: chest compressions by paramedic. Compression rate: audio prompt for 80/min Compression/ventilation ratio: not reported
Outcomes	• ROSC: mechanical treatment 1/7 (14.2%); manual 0/10 (0%) • Survival to hospital admission: mechanical 1/7 (14.2%), manual 0/10 (0%)
Notes	Primary outcome of study was end-tidal carbon dioxide. Reporting of baseline characteristics limited to initial rhythm

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	High risk	Quasi-randomisation based on odd/even days
Allocation concealment (selection bias)	High risk	Quasi-randomisation did not allow for allocation concealment
Blinding of participants and personnel (performance bias) All outcomes	High risk	Not possible, given the nature of the intervention
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Not reported
Incomplete outcome data (attrition bias) All outcomes	Low risk	All participants entered into the study had follow-up data
Selective reporting (reporting bias)	Unclear risk	Not reported
Other bias	Unclear risk	Both groups of participants placed on Thumper back-board. Manual CPR (control) group had machine running but not applied to participant to allow human compressions to be delivered at the same rate.

Gao 2016

Methods	Single-centre cluster-RCT in China Clusters were patients treated by a particular ambulance
Participants	Adults with out-of-hospital atraumatic cardiac arrest with resuscitation attempted by EMS who were admitted to the Emergency Medical Centre of one hospital N = 133 Mechanical mean age: 62.6 (14.9). Male: 50/69 (72.5%). VF: 9/69 (13.0%) Initial rhythm VF/VT: 9/69 (13%). PEA: 31/69 (44.9%). Asystole: 24/69 (34.8%) Hypothermia treatment post-arrest: not reported Manual mean age: 64.2 (12.6). Male: 44/64 (68.8%). VF: 8/64 (12.5%) Initial rhythm VF/VT: 8/64 (12.5%). PEA: 20/64 (31.3%). Asystole: 32/64 (50%) Hypothermia treatment post-arrest: not reported Exclusions: pregnant, trauma, patients with advanced cancer, aged < 14 or > 90 years old
Interventions	Mechanical: manual compressions were started while the AutoPulse was being prepared Manual: resuscitation (including defibrillation, drug administration and compression rate) followed the 2010 American Heart Association Guidelines (Berg 2010)
Outcomes	• Survival to hospital discharge: mechanical 13/69 (18.8%); manual 4/64 (6.3%) • Sustained ROSC: mechanical 31/69 (44.9%); manual 15/64 (23.4%) • Survival to 24 hours: mechanical 27/69 (39.1%); manual 14/64 (21.9%) • Survival to hospital discharge: mechanical 13/69 (18.8%); manual 4/64 (6.3%) • Sternal or rib fracture: mechanical 4/60 (6.7%); manual 3/63 (4.8%)
Notes	Both study arms were intubated with ventilation consisting of 100% oxygen. End-tidal carbon dioxide was monitored and recorded. No CPR process or quality data were recorded. CPR according to 2010 American Heart Association CPR guidelines. Concern over selection bias based on how ambulances were dispatched to cardiac arrest. Extremely high survival to hospital rate reported in mechanical compression group in comparison to other published studies. Although "neurological prognosis" in the form of Cerebral Performance Category is reported, this outcome is reported for all patients with ROSC, not in the subset who survived to hospital discharge.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	The 10 ambulances of the Emergency Medical Centre were numbered one through 10. A computer then generated five numbers corresponding to five ambulances which were then equipped with an AutoPulse. The ambulances were then dispatched out sequentially to each patient with OHCA.
Allocation concealment (selection bias)	High risk	There was no allocation concealment as the order of which ambulance will be going out next was known. It is possible that ambulances known to be equipped with the mechanical device were dispatched to OHCA cases viewed as having a better prognosis. There were no mention of efforts to track any subterfuge of the randomisation process.
Blinding of participants and personnel (performance bias) All outcomes	High risk	Not possible given the nature of the intervention

Gao 2016 *(Continued)*

Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Not reported
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	Not reported
Selective reporting (reporting bias)	Unclear risk	Not reported
Other bias	Low risk	All patients were reported to have received only their designated treatment.

Hallstrom 2006

Methods	Multicentre cluster-randomised trial in the USA and Canada Clusters were based on ambulance station or group of stations with cross-over occurring at intervals ranging from four weeks to two months
Participants	Adults with out-of-hospital atraumatic cardiac arrest with resuscitation attempted by EMS N = 767 Mechanical mean age: 66.6 (15.6). Male: 252/394 (64%) Initial rhythm VF/VT: 122/394 (31.0%). PEA: 79/394 (20.1%). Asystole: 164/394 (41.6%). Unknown: 29/394 (7.4%) Hypothermia treatment post-arrest: 30/394 (7.6%) Manual mean age: 66.2 (15.2). Male: 245/373 (66%). VF: 119/373 (31.9%) Initial rhythm VF/VT: 119/373 (31.9%). PEA: 94/373 (25.2%). Asystole: 148/373 (39.7%). Unknown: 12/373 (3.2%) Hypothermia treatment post-arrest: 22/373 (5.9%) Exclusions: prisoner, Do Not Resuscitate order, recent surgery, no study vehicle available, noncardiac etiology, resuscitation > 90 seconds before study vehicle arrival
Interventions	Mechanical: "AutoPulse" load-distributing band at 80 compressions/min. Compression/ventilation ratio: 15 compressions: three-second pause Manual: compressions by EMS personnel (ALS and BLS). Compression rate: 100/min. Compression to ventilation ratio: not reported
Outcomes	• Survival with good neurological function (CPC 1 or 2): mechanical 12/394 (3.1%); manual 28/373 (7.5%) • Survival to four hours after emergency call: mechanical 104/394 (26.4%); manual 92/373 (24.7%) • Survival to hospital discharge: mechanical 23/394 (5.8%); manual 37/373 (9.9%). Odds ratio adjusted for clustering and covariates: 0.57 (95% CI 0.33 to 0.99) • Emergency call to first rhythm analysis time interval, mean minutes (SD): mechanical 8.9 (3.0); manual 8.9 (2.9) • Emergency call to first shock for VF/VT, mean minutes(SD): mechanical 11.8 (6.1); manual 9.7 (3.1) • Emergency call to first EMS CPR, mean minutes (SD): mechanical 7.9 (2.8); manual 7.8 (2.7)
Notes	The study recruited participants in five cities, and the protocol for CPR was not uniform across all sites. In fact, the CPR protocol was changed part-way through the study at one site. The change involved a two-minute delay in applying the mechanical device to the participant, while paramedics administered manual CPR and a first defibrillation if needed. This change was incorporated in response to quality assurance data from the local emergency medical services system showing "prolonged time" without

Hallstrom 2006 *(Continued)*

compressions in the load-distributing band device group. The distribution of participant body type, as judged by treating paramedics, differed in the mechanical and manual groups. The mechanical group had more "thin" participants than the manual group (14.2% versus 8.8%) and more "morbidly obese" participants than the manual group (4.3% versus 2.4%). Trial stopped early at interim analysis for decreased survival to hospital discharge and no difference in primary outcome of survival to four hours after emergency call. Variable CPR protocols used across sites with respect to order of interventions and timing of device application. One of the larger study sites changed CPR protocol to delay application of device two minutes halfway through study

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	Not reported
Allocation concealment (selection bias)	Unclear risk	Not reported
Blinding of participants and personnel (performance bias) All outcomes	High risk	Not possible given the nature of the intervention
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Not reported
Incomplete outcome data (attrition bias) All outcomes	Low risk	99.3% of participants entered into the study had full follow-up data reported
Selective reporting (reporting bias)	Unclear risk	Not reported
Other bias	High risk	Site C of the study changed the protocol halfway through the study; this involved applying the device to participants after a period of CPR and rhythm analysis. This change in CPR technique is likely to have had an impact on outcomes for participants treated at this site with mechanical CPR.

Halperin 1993

Methods	Randomised controlled trial in the USA
Participants	Patients with IHCA after unsuccessful standard CPR for less than 20 minutes N = 34 Mechanical mean age: 61 (16). Male: 10/17 (58.8%). VF: 3/17 (17.6%) Initial rhythm VF/VT: 3/17 (17.6%). PEA: 5/17 (29.4%). Asystole: 6/17 (35.3%) Hypothermia treatment not reported Manual mean age: 69 (18). Male: 10/17 (58.8%). VF: 8/17 (47.1%) Initial rhythm VF/VT: 8/17 (47.1%). PEA: 3/17 (17.6%). Asystole: 3/17 (17.6%) Hypothermia treatment not reported

Halperin 1993 *(Continued)*

	Exclusions: patients given CPR for longer than 20 minutes before randomisation
Interventions	Mechanical: pneumatic vest. Compression rate: not reported. Compression/ventilation ratio: not reported Manual: chest compression provider type not reported. Compression rate: not reported Compression/ventilation ratio: not reported
Outcomes	• ROSC: mechanical 8/17 (47.1%); manual 3/17 (17.6%) • Survival to six hours after resuscitation: mechanical 6/17 (35.3%); manual 1/17 (5.9%) • Survival to 24 hours after resuscitation: mechanical 3/17 (17.6 %); manual 1/17 (5.9%) • Sternal or rib fracture: mechanical 1/4 (25%); manual 2/5 (40%) • Haemothorax or pneumothorax: mechanical 0/4 (0%); manual 1/5 (20%)
Notes	In-hospital study with late randomisation after "failed" standard CPR. Comorbidities, underlying cause of cardiac arrest and important features of cardiac arrest circumstances (witness status, bystander CPR before code team, etc.) not reported

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	Not reported
Allocation concealment (selection bias)	Unclear risk	Sequenced envelopes used. Unclear whether these envelopes were opaque; no mention of a randomisation log to track any subterfuge of the randomisation process
Blinding of participants and personnel (performance bias) All outcomes	High risk	Not practical given the nature of the intervention
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Not reported
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	Number of dropouts from the study not reported
Selective reporting (reporting bias)	Unclear risk	Not reported
Other bias	Unclear risk	Randomisation in cases of unsuccessful manual CPR after cardiac arrest

Koster 2017

Methods	Non-inferiority RCT in the Netherlands
Participants	IHCA or OHCA N = 374

Koster 2017 *(Continued)*

	Mechanical AutoPulse mean age: 65 (15). Male: 75/115 (65.2%). Initial rhythm: VF 30/115 (26%) , VT 7/115 (6%), narrow complex tachycardia 2/115 (2%), PEA 42/115 (37%), asystole 26/115 (23%), unknown not shockable 5/115 (4%). treatment post-arrest: not reported LUCAS mean age: 63 (17). Male: 82/122 (67.2%). Initial rhythm: VF 39/122 (32%) , VT 3/122 (2%), narrow complex tachycardia 6/122 (6%), PEA 36/122 (30%), asystole 22/122 (18%), unknown not shockable 10/122 (8%). treatment post-arrest: not reported Manual mean age: 87 (13.82). Male: 87/137 (63.5%). VF 31/137 (23%), VT 3/137 (2%), narrow complex tachycardia 5/137 (4%), PEA 49/137 (36%), asystole 33/137 (24%), unknown not shockable 9/137 (7%). treatment post-arrest: not reported Hypothermia treatment post-arrest: not reported Exclusions: patients with traumatic cause of arrest, less than 18 years-old, patients with a mechanical chest compression device already applied prehospital by the ambulance crew, patients with ROSC prior to application of study device
Interventions	Patients with either IHCA or OHCA with ongoing CPR upon arrival to hospital were randomised to either the AutoPulse or the LUCAS device. They were then randomised to either mechanical or manual compressions Mechanical: mechanical chest compressions with either the LUCAS chest compression system or the AutoPulse device, which were operated by trained cardiac care unit nurses. Resuscitative protocol, compression rate and compression:ventilation ratio not reported Manual: manual compressions with feedback from sternal displacement transducer from (Philips Heartstart MRx defibrillator). Resuscitative protocol, compression rate and compression:ventilation ratio not reported
Outcomes	**Primary outcome** • Serious or life-threatening visceral organ injury rate difference between mechanical device group and manual group. Autopulse rate difference + 5.3% (95% CI - 2.2% to 12.8%), P= 0.15. Rate difference LUCAS—control þ1.0% (95% CI - 5.5% to 7.6%), P= 0.75 **Secondary outcomes** • Pneumothorax: mechanical AutoPulse 11/103 (10.6%); mechanical LUCAS 3/108 (2.8%); manual 4/126 (3.2%) • Haemothorax: mechanical AutoPulse 2/103 (6.8%); mechanical LUCAS 3/108 (2.8%); manual 4/126 (3.2%) • Sternal or rib fractures: mechanical AutoPulse 47/103 (45.6%); mechanical LUCAS 43/108 (39.8%); manual 52/126 (41.3%) • Internal organ damage: mechanical AutoPulse 1/103 (< 1%); mechanical LUCAS 2/108 (< 1%); manual 0/126 (0%)
Notes	This was a three-arm non-inferiority trial with resuscitation-related injuries as the primary outcome. Randomisation occurred before inclusion/exclusion criteria were applied. Baseline characteristics were balanced except for the exclusion criteria of ROSC on arrival of study resuscitation team, which the authors attribute to the lag time in applying the mechanical device, thus allowing more time for ROSC. Criteria for non-inferiority of LUCAS compared to manual chest compressions on the primary outcome of serious or life-threatening visceral organ injury (< 10% difference) were satisfied. Criteria for non-inferiority of Autopulse compared to manual chest compressions on the primary outcome were not met

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Computer-generated random numbers

Koster 2017 *(Continued)*

Allocation concealment (selection bias)	Low risk	Used opaque sealed envelopes. No mention of a randomisation log to track any subterfuge of the randomisation process or other safeguards against foreknowledge of sequential assignments
Blinding of participants and personnel (performance bias) All outcomes	High risk	Not practical given the nature of the intervention
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	The intention was for all assessors to be blinded to allocation. However, skin markings from device were sometimes apparent to pathologists who were performing the autopsies.
Incomplete outcome data (attrition bias) All outcomes	Low risk	For all outcomes, data were complete. Very few participants (1-2) had unknown hospital course within each treatment arm.
Selective reporting (reporting bias)	Unclear risk	Not reported
Other bias	Unclear risk	Randomisation occurred prior to inclusion/exclusion criteria, which has an unclear impact on allocation.

Lu 2010

Methods	RCT in China
Participants	Confirmed IHCA confirmed by ECG or ECG monitor, cardiac arrest ≤ 10 minutes N = 150 Mechanical mean age: 47.72 (14.25). Male: 46/76 (61%) Initial rhythm: not reported Hypothermia treatment post-arrest: not reported Manual mean age: 45.50 (13.82). Male: 43/74 (58%) Initial rhythm: not reported Hypothermia treatment post-arrest: not reported Exclusions: patients with extrathoracic and abdominal trauma, pregnant women, patients with terminal illness and organ failure (e.g. terminal cancer, heart failure, multiple organ failure, etc.)
Interventions	Mechanical: Thumper Model 1007CCV at 100 compressions/min. Compression/ventilation ratio: five/one Manual: compression/ventilation ratio: not reported
Outcomes	• ROSC: mechanical 42/76 (55.26%); manual 28/74 (37.84%) • Survival to hospital discharge: mechanical 25/76 (32.89%); manual 11/74 (14.86%)
Notes	This study was published in Chinese. All information regarding design, results and risk of bias was obtained with the help of interpreters

Risk of bias

Lu 2010 *(Continued)*

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	Method of randomisation not reported
Allocation concealment (selection bias)	Unclear risk	Not reported
Blinding of participants and personnel (performance bias) All outcomes	High risk	Not practical given the nature of the intervention
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Not reported
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	Not reported
Selective reporting (reporting bias)	Unclear risk	Not reported
Other bias	Unclear risk	No evidence of this reported by translators.

Perkins 2015

Methods	Cluster-RCT in the UK Clusters were patients treated by a particular ambulance
Participants	Adult OHCA of presumed cardiac origin N = 4471 Mechanical mean age: 71.0 (16.3). Male: 1039/1652 (63%) Initial rhythm VF: 364/1652 (22%). VT:12/1652 (1%). PEA: 398/1652 (24%). Asystole: 824/1652 (50%). Unknown: 54/1652 (3%) Hypothermia treatment post-arrest: not reported Manual mean age: 71.6 (16.1). Male: 1774/2819 (63%) Initial rhythm VF: 597/2819 (21%). VT:18/2819 (1%). PEA: 707/2819 (25%). Asystole: 1384/2819 (49%). Unknown: 113/2819 (4%) Hypothermia treatment post-arrest: not reported Exclusions: patients with traumatic cardiac arrest, known or clinically apparent pregnancy
Interventions	Mechanical: initially manual CPR while device powers on. This is followed by LUCAS-2 mechanical chest compressions between 40 mm to 53 mm at a rate of 102/min). Followed 2010 European Resuscitation Council Guidelines (Koster 2010) Manual: compressions by EMS personnel (target compression depth of 50mm to 60 mm, rate 100 to 120/min). Followed 2010 European Resuscitation Council Guidelines (Koster 2010)

Perkins 2015 *(Continued)*

Outcomes	• Survival to 30 days: mechanical 104/1652 (6%); manual 193/2819 (7%) • ROSC: mechanical 522/1652 (32%); manual 885/2819 (31%) • Survived event (ROSC sustained until admission and transfer of care to medical staff at the receiving hospital): mechanical 337/1652 (23%); manual 658/2819 (23%) • Survival to 3 months: mechanical 96/1652 (6%); manual 182/2819 (6%) • Survival to 12 months: mechanical 89/1652 (5%); manual 175/2819 (6%) • Survival with favourable neurological function at 3 months (CPC score 1-2): mechanical 77/1652 (5%); manual 168/2819 (6%)
Notes	CPR quality was not monitored

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Ambulance vehicles were cluster-randomised with a computer generated sequence, stratified by station and vehicle type.
Allocation concealment (selection bias)	Unclear risk	Authors mention that ambulance dispatch staff were unaware of allocation. No mention of a randomisation log to track any subterfuge of the randomisation process or other safeguards against foreknowledge of sequential assignments.
Blinding of participants and personnel (performance bias) All outcomes	High risk	Not practical given the nature of the intervention
Blinding of outcome assessment (detection bias) All outcomes	Low risk	Research nurses assessing patients at follow-up visits were blinded.
Incomplete outcome data (attrition bias) All outcomes	Low risk	For the outcome of survival to 30 days, only 1 patient from the control arm was lost to follow-up. For all other outcomes reported, incomplete data ranged from 0% to 6% and was balanced in both arms.
Selective reporting (reporting bias)	Unclear risk	Not reported
Other bias	High risk	Unbalanced cross-over: mechanical to manual 638/1652 (39%); manual to mechanical 11/2819 (< 1%)

Rubertsson 2014

Methods	RCT in Sweden, the Netherlands, and the UK
Participants	Adult OHCA of presumed cardiac origin N = 2589 Mechanical mean age: 69.0 (range 16-100). Male: 869/1300 (67%) Initial rhythm VF/VT: 374/1300 (29%). PEA: 255/1300 (20%). Asystole: 610/1300 (47%). Unknown: 20/1300 (2%) Hypothermia treatment post-arrest: 198/1300 (15.2%)

Rubertsson 2014 *(Continued)*

	Manual mean age: 69.1 (range 15 to 99). Male: 857/1289 (66%) Initial rhythm VF/VT: 383/1289 (30%). PEA: 254/1289 (20%). Asystole: 594/1289 (46%). Unknown: 16/1289 (1%) Hypothermia treatment post-arrest: 214/1289 (16.6%) Exclusions: patients suffering traumatic cardiac arrest (including hanging), < 18 years old, known pregnancy, too large or small to fit the device, undergoing defibrillation before the device arrived on scene, with crew-witnessed cardiac arrest who achieved ROSC after immediate defibrillation
Interventions	Mechanical: mechanical chest compressions with LUCAS chest compression system with non-standard treatment algorithm. Patients were treated with manual chest compressions while enrolment/randomisation occurred and if randomised to mechanical intervention, device was deployed immediately. Mechanical compressions were initiated and continued for 3-minute cycles. At 90 seconds into the cycle, defibrillation shock delivered without checking heart rhythm. Heart rhythm was checked after each 3-minute cycle. If a shockable rhythm was observed, a new 3-minute cycle was started; counter-shock delivered at 90 seconds without pausing; if no shockable rhythm was observed, a 3-minute cycle was initiated and continued without interruption. Manual: compressions by EMS personnel according to 2005 European Resuscitation Council Guidelines (Nolan 2005)
Outcomes	• Survival to hospital discharge with good neurological function (CPC 1 or 2): mechanical 108/1300 (8.3%); manual 100/1289 (7.8%) • Survival to hospital discharge: mechanical 117/1300 (9.0%); manual 118/1289 (9.2%) • Survival to hospital admission: mechanical 366/1300 (28.2%); manual 357/1289 (27.7%) • Survival at 6 months: mechanical 111/1300 (8.5%); manual 104/1289 (8.1%) • Survival to 4 hours: mechanical 307/1300 (23.6%); manual 305/1289 (23.7%) • ROSC: mechanical 460/1300 (35.4%); manual 446/1289 (34.6%) • Pneumothorax: mechanical 1/1300 (< 1%); manual 1/1289 (< 1%) • Sternal or rib fractures: mechanical 1/1300 (< 1%; noted before application of device); manual 2/1289 (< 1%) • Internal organ damage: mechanical 1/1300 (< 1%, suspected spleen rupture on computed tomagraph scan); manual 0/1289 (< 1%) • Cardiac arrest to emergency call interval, median minutes (IQR): mechanical 2 (0-5); manual 2 (0-5) • Cardiac arrest to start of manual CPR interval, median minutes (IQR): mechanical 11.5 (7-16); manual 11 (7-15) • Cardiac arrest to start of mechanical CPR interval, median minutes (IQR): mechanical 15 (10-20); manual 18 (10-27) • Cardiac arrest to first defibrillation interval, median minutes (IQR): mechanical 17 (12-22); manual 15.5 (11-23.5) • Cardiac arrest to ROSC interval, median minutes (IQR): mechanical 17 (11-25); manual 14 (9-21)
Notes	Mechanical CPR arm followed a novel, study-specific algorithm (defibrillation without pulse/rhythm check and 3-min CPR periods) that may impact treatment effect. CPR quality monitored by impedence data in 10% of patients showing CPR fraction of 0.78 and 0.84 in the manual and mechanical groups, respectively

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	Refers to sealed randomisation envelopes. No other information, including method of randomisation, provided

Rubertsson 2014 *(Continued)*

Allocation concealment (selection bias)	Low risk	Used opaque sealed envelopes. No mention of a randomisation log to track any subterfuge of the randomisation process or other safeguards against foreknowledge of sequential assignments
Blinding of participants and personnel (performance bias) All outcomes	High risk	Not practical given the nature of the intervention
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Not reported
Incomplete outcome data (attrition bias) All outcomes	Low risk	For all outcomes, incomplete data occurred in ˜1% or less of cases. Missing data were imputed as the "worst case" in the intention-to-treat population
Selective reporting (reporting bias)	Unclear risk	Not reported
Other bias	Unclear risk	Cross-over contamination from mechanical to manual arm and vice versa, 64/1300 (4.9%) and 46/1293 (3.6%) respectively. Cointervention present in mechanical CPR arm (study-specific ALS treatment algorithm)

Smekal 2011

Methods	RCT in Sweden
Participants	Out-of-hospital sudden cardiac arrest N = 148 Mechanical mean age: 69 (16). Male %: 50/75 (68%) Initial rhythm VF/VT: VF: 20/75 (27%) Hypothermia treatment post-arrest: not reported Manual mean age: 71 (16). Male %: 50/73 (68%) Initial rhythm VF/VT: 20/73 (27%) Hypothermia treatment post-arrest: not reported Exclusions: known pregnancy, younger than 18 years of age or trauma
Interventions	Mechanical: "LUCAS" piston with active compression-decompression CPR. Participants in the mechanical chest compression group received manual chest compressions while the device was being prepared. They then received 90 seconds of LUCAS compression. The following cycle was then repeated twice: Check rhythm and pulse for a maximum of 10 seconds; if shockable, provide LUCAS for 90 seconds while fixing intravenous line, shock once (200J), provide LUCAS for 60 seconds and repeat. If the participant's rhythm was non-shockable, LUCAS compressions were continued for 90 seconds and repeated. After two cycles of this modified algorithm, traditional CPR, according to European Resuscitation Council 2000 guidelines for advanced cardiac life support, was provided with the use of LUCAS to deliver chest compressions Manual: chest compressions by nurse. Compression rate: performed according to European Resuscitation Council 2000 guidelines (de Latorre 2001), not otherwise reported. Compression/ventilation ratio:

Smekal 2011 *(Continued)*

	performed according to European Resuscitation Council 2000 guidelines (de Latorre 2001), not otherwise reported
Outcomes	• ROSC: mechanical 30/74 (41%); manual 23/72 (32%) • Survival to hospital discharge: mechanical 6/75 (8%); manual 7/72 (10%) • Survival to hospital admission: mechanical 18/75 (24%); manual 15/72 (21%) • Scene arrival to first CPR time interval mean minutes (SD): mechanical 1.0 (1.1); manual 1.1 (1.1)
Notes	Primary outcome of the study was ROSC with BP > 80/50 mmHg for > five minutes. LUCAS algorithm differed from manual CPR algorithm for the first two cycles

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	Not reported
Allocation concealment (selection bias)	Unclear risk	Sequenced envelopes used. Unclear whether these envelopes were opaque; no mention of a randomisation log to track any subterfuge of the randomisation process
Blinding of participants and personnel (performance bias) All outcomes	High risk	Not practical given the nature of the intervention
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Not reported
Incomplete outcome data (attrition bias) All outcomes	Low risk	Following enrolment, only one participant was excluded from the study because he/she was not randomly assigned correctly. For all survival study outcomes, no more than one participant per group had missing data
Selective reporting (reporting bias)	Unclear risk	Not reported
Other bias	High risk	LUCAS algorithm differed from manual CPR algorithm for the first two cycles

Taylor 1978

Methods	RCT
Participants	Patients with IHCA undergoing "prolonged" CPR N = 50 Mechanical mean age: 57 (5.2). Male: not reported Initial rhythm: not reported Hypothermia treatment post-arrest: not reported Manual mean age: 54.8 (6.6). Male: not reported Initial rhythm: not reported

Taylor 1978 *(Continued)*

	Hypothermia treatment post-arrest: not reported Exclusions: patients with greater than 10 minutes of standard CPR
Interventions	Mechanical: piston at 60 compressions/min. Compression/ventilation ratio: five/one Manual: chest compressions by medical house staff. Compression rate: not reported Compression/ventilation ratio: not reported
Outcomes	• Survival to hospital discharge: mechanical 3/24 (12.5%); manual 2/26 (7.7%) • Survival to 24 hours after CPR: mechanical 4/24 (16.7%); manual 4/26 (15.4%) • Sternal or rib fractures: mechanical 10/13 (76.9%); manual 8/17 (47.1%) • Internal organ damage: mechanical 0/13 (0%); manual 2/17 (11.8%)
Notes	Older in-hospital study. Lack of reporting of participant characteristics, reason for arrest, initial rhythm. Potential selection bias in adverse effect risk estimates, as autopsies were not universal and indications for autopsy were not reported

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	Method of randomisation is described as "drawing cards"
Allocation concealment (selection bias)	Unclear risk	Not reported
Blinding of participants and personnel (performance bias) All outcomes	High risk	Not practical given the nature of the intervention
Blinding of outcome assessment (detection bias) All outcomes	Unclear risk	Not reported with respect to pathological examination for injuries caused by the two different types of chest compressions
Incomplete outcome data (attrition bias) All outcomes	Low risk	All participants who entered the study were accounted for with respect to outcomes reported
Selective reporting (reporting bias)	Unclear risk	Not reported
Other bias	Unclear risk	Process of CPR in each of the groups was not reported. Unclear whether protocol related to administration of mechanical CPR; may have altered the process of resuscitation to result in a cointervention

Wik 2014

Methods	RCT in the USA, Austria and the Netherlands
Participants	Adult OHCA of presumed cardiac origin N = 4231 Mechanical mean age: 65.7 ± 16.4. Male: 1295/2099 (61%)

Wik 2014 *(Continued)*

	Initial rhythm VF/VT: 451/2099 (21%). PEA/asystole: 1572/2099 (75%). Unknown: 76/2099 (4%) Hypothermia treatment post-arrest: 690/2099 (32.9%) Manual mean age: 65.6 ± 16.0. Male: 1315/2132 (61%) Initial rhythm VF/VT: 519/2132 (24%). PEA/asystole: 1516/2132 (71%). Unknown: 97/2132 (5%) Hypothermia treatment post-arrest: 800/2123 (37.5%) Exclusions: patients with Do Not Resuscitate orders, who had received mechanical chest compressions prior to randomisation, presumed pregnant, too large for CPR device, prisoners, or if the randomising EMS unit arrived more than 16 mins after emergency call
Interventions	Mechanical: integrated AutoPulse CPR (iA-CPR). Upon arrival, one EMS personnel performed manual CPR and another opened a randomisation envelope. If randomised to receive iA-CPR, the AutoPulse compressions were immediately initiated Manual: compressions by EMS personnel in accordance with the American Heart Association 2005 Guidelines (except resuscitation cycle was 3 mins)
Outcomes	• Survival to discharge with good neurological function (mRS 0-3): mechanical 87/2099 (4.1%); manual 112/2132 (5.3%) • Survival to hospital discharge: mechanical 196/2099 (9.4%); manual 233/2132 (11.0%) • Sustained ROSC: mechanical 600/2099 (28.6%); manual 689/2132 (32.3%) • Survival to 24 hours: mechanical 456/2099 (21.8%); manual 532/2132 (25.0%) • Sternal or rib fracture: mechanical 70/2099 (3%); manual 36/2131 (2%) • Haemothorax or pneumothorax: mechanical 34/2099 (2%); 21/2132 (1%) • Abdominal organ injury: mechanical 1/2099 (< 1%); 0/2132 (0%) • Scene arrival to first shock time interval for VT/VF (mean ± SD): mechanical 7.5 ± 6.0 min; manual 6.7 ± 6.2 mins
Notes	This study design comprised of 3 phases: 1) in-field training where all OHCA patients were treated with AutoPulse device followed by 2) run-in phase to evaluate protocol compliance and minimise Hawthorne effect prior to 3) statistical inclusion phase. Authors emphasised the rigorous training for EMS personnel and continuous monitoring of CPR quality, user compliance. 96% of enrolled cases were monitored for CPR fraction. Both the proportion monitored and the CPR fraction were found to be higher than in previous studies. May reduce generalisability to real-world CPR provider quality

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Unclear risk	Refers to sealed randomisation cards. No other information, including method of randomisation, provided
Allocation concealment (selection bias)	Unclear risk	Sealed envelopes. Does not say if envelope was opaque. No mention of a randomisation log to track any subterfuge of the randomisation process or other safeguards against foreknowledge of sequential assignments
Blinding of participants and personnel (performance bias) All outcomes	High risk	Not practical given the nature of the intervention
Blinding of outcome assessment (detection bias)	High risk	Incomplete blinding as authors state study personnel were “not always blinded to study arm”

Wik 2014 *(Continued)*
All outcomes

Incomplete outcome data (attrition bias) All outcomes	Low risk	Primary outcome (survival to discharge) data available for 4219/4231 (99.1%) Neurological function (mRS scores) of patients who survived to discharge available only for 310/429 (overall 72%; mechanical 70%; manual 74%). Authors state that this was likely not related to their neurological condition, but rather the ability of research co-ordinator to promptly locate and obtain consent from patient prior to discharge. 262 (11%) controls and 260 (11%) iA-CPR participants were excluded post-enrolment
Selective reporting (reporting bias)	Unclear risk	Not reported
Other bias	Unclear risk	Not reported

Abbreviations: ALS = advanced life support; BLS = basic life support; BP = blood pressure; CPC = Cerebral Performance Score; CPR = cardiopulmonary resuscitation; ECG = electrocardiogram; EMS = emergency medical services; IHCA = in-hospital cardiac arrest; IQR = interquartile range; mRS = modified Rankin Scale; OHCA = out-of-hospital cardiac arrest; PEA = pulseless electrical activity; RCT = randomised controlled trial; ROSC = return of spontaneous circulation; VF = ventricular fibrillation; VT = ventricular tachycardia

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

Study	Reason for exclusion
Anonymous 1973	Non-randomised study of technical specifications of several powered and manual devices. No data from use on humans
Arntz 1998	Review
Arntz 2001	No mechanical CPR device; manual active compression-decompression device
Aufderheide 2011	Did not include the use of a mechanical chest compression device
Axelsson 2006	Non-randomised cluster trial; verified with author communication
Axelsson 2009	Prospective observational study inappropriately called a "pseudo-randomised cluster study". As stated in the methods, the clusters or individual patients were not actually randomised. The methodology for allocation used does not fit the definition for quasi-randomisation.
Baubin 1999	No mechanical CPR device; manual active compression-decompression device
Box 2008	Observational study
Dittbrenner 1993	Review of computerised patient record, not cardiopulmonary resuscitation
Dotter 1961	Case series
Elich 1995	Review
Halperin 2000	Animal study
Hampe 2008	Case series
Harkins 1961	Review

Study	Reason for exclusion
Havel 2008	No mechanical CPR device; manual active compression-decompression device
Kern 2001	Review
Knight 1964	Review
Krischer 1989	Included patients with drowning and toxicology as presumed causes of cardiac arrest
Kurowski 2015	No mechanical CPR device. CardioPump CPR feedback device
Lairet 2005	Retrospective chart review
Liu 2010	Study was not randomised
Lurie 1994	No mechanical CPR device; manual active compression-decompression device
Lurie 1997	Review
Lurie 2002	Review
Malzer 1996	No mechanical CPR device; manual active compression-decompression device
Montgomery 1995	Review
Morozumi 2009	Case report
Nachlas 1962	Review
Nachlas 1963	Animal study
Nachlas 1965	Review/case study
Niemann 1984	Abstract on cough CPR
Nishino 1992	Studied anaesthetised patients not in cardiac arrest and the effect of static chest compression on respiratory characteristics
Pearson 1966	Manikin study
Plaisance 1999	No mechanical CPR device; manual active compression-decompression device
Rivers 1993	Non-randomised study
Roberts 1978	Case series
Schwab 1995	No mechanical CPR device; manual active compression-decompression device
Skogvoll 1999	No mechanical CPR device; manual active compression-decompression device
Smekal 2009	Observational study
Stapleton 1991	Manikin study
Stechovsky 2015	Non-randomised study

Study	Reason for exclusion
Swanson 2005a	Non-randomised study
Swanson 2006a	Non-randomised study
Swenson 1988a	Case series
Tucker 1993	No mechanical CPR device; manual active compression-decompression device
Tucker 1994	No mechanical CPR device; manual active compression-decompression device
Vincent 2003	Review
Wang 2007	Non-randomised study looking at CPR quality, not clinical outcomes
Ward 1993	Cross-over study after failed resuscitation. Subjects received both manual and mechanical chest compressions with end-tidal CO^2 as outcome
Weil 2000	Review of manual adjunct device
Wik 2000	Review
Wolcke 2003	No mechanical CPR device; manual active compression-decompression device
Zoll 1966	Review

CPR = cardiopulmonary resuscitation

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

CTRI/2013/07/003840

Trial name or title	Randomised comparison of chest compression using the device AutoPulse with manual chest compressions in patients requiring cardiopulmonary resuscitation (CPR) for in hospital cardiac arrest - CAPCAR
Methods	Randomised controlled trial
Participants	Inclusion criteria: patient with cardiac arrest in the emergency department. Exclusion criteria: < 18 years, chest trauma, patients already on advanced life support, patients achieving ROSC after the initial 5 minutes of manual cardiopulmonary resuscitation according to the 2010 guidelines, predicted futile resuscitation based on fixed dilated pupils, Glascow Coma Scale 3/15, absent pupillary and other brain stem reflexes in the absence of drug intoxications, pregnant females, patients with "Do Not Resuscitate" orders
Interventions	Intervention: AutoPulse: mechanical chest compression device. It will provide chest compression at 100/mins for the duration of resuscitation Control intervention: manual CPR: CPR will be done utilising conventional manual chest compression with hands with targeted rate at least 100/mins for the duration of resuscitation
Outcomes	**Primary efficacy endpoints** • Sustained ROSC: admission to intense care unit/wards with a palpable pulse and measurable blood pressure • Overall survival • Survival to 24 hrs: being alive 24 hrs after the initial arrest

CTRI/2013/07/003840 *(Continued)*

- Survival to hospital discharge: discharge to home or a chronic care facility

Timepoints:

- Sustained ROSC at 24 hours
- Overall survival at hospital discharge

Secondary efficacy endpoints

- In patients with survival to discharge
  - Modified Rankin Scale
  - Cerebral Performance Category
- APACHE III Score

Timepoint:

- Survival at 24 hrs and at discharge

Starting date	01/06/2013
Contact information	Dr. Azharuddin Mohammed Malik Department of Medicine Jawaharlal Nehru Medical College, Aligarh Muslim University 202002 Aligarh, UTTAR PRADESH, India Telephone: 8126320218 e-mail: malikazharuddin@gmail.com
Notes	

ISRCTN38139840

Trial name or title	A feasibility randomised controlled trial of mechanical chest compression devices for in-hospital cardiac arrest (COMPRESS-RCT)
Methods	Randomised
Participants	Adults with in-hospital cardiac arrest
Interventions	Participants are 3:1 randomised to either receive mechanical or manual chest compressions. Mechanical compressions delivered by LUCAS-2 or LUCAS-3 device (Joliffe AB/ Physio-Control, Lund, Sweden)
Outcomes	**Primary outcome** • Proportion of eligible participants randomised over study period **Secondary outcomes** • Measures of feasibility • ROSC • Survival at discharge (with good neurological function) • Survival at 30 days • Survival at 6 months (with good neurological function) • Hospital length of stay • Quality of life measures at discharge and 6 months • CPR quality

ISRCTN38139840 *(Continued)*

	• Adverse events profile
Starting date	October 2015
Contact information	Dr. Keith Couper; phone +44 2476 575923
Notes	Patient recruitment officially began in late February 2017 per study website

ISRCTN78354073

Trial name or title	German Automatic chest compression Resuscitation Trial (German ART)
Methods	Randomised
Participants	Adults aged 18-80 years old with non-traumatic out-of-hospital cardiac arrest Exclusion criteria: body habitus with weight greater than 150 kg or estimated chest circumference greater than 150 cm, pregnancy, time from emergency medical services call to physician arrive greater than 15 min
Interventions	Mechanical group will receive chest compressions with AutoPulse device. Both the mechanical and manual arms will follow the 2005 European Resuscitation Council Guidelines
Outcomes	**Primary outcome** • Rate of survival to admission to hospital with ROSC **Secondary outcomes** • Survival to 24-hours, to discharge from intensive care unit, to discharge from hospital, to three months, to one year • Survival to each of these time points with good neurological function
Starting date	10/01/2008
Contact information	Dr. Andreas Hoeft; email: andreas.hoeft@ukb.uni-bonn.de
Notes	Subgroup analysis will be performed for "fast AutoPulse", i.e. patients who receive mechanical compressions in less than six minutes of the arrival of clinical staff

NCT01521208

Trial name or title	LUCAS continuous chest compressions in out-of-hospital cardiac arrest treatment: the LUCAT Trial
Methods	Randomised
Participants	Adults (> 17 years old and < 81 years old) suffering from non-traumatic or unexpected witnessed (seen, heard or monitored) sudden cardiac arrest, attended by an advanced support ambulance (served by doctor or nurse) in Barcelona city, or in the Girona or Lleida area, with time between the emergency call and reaching patient less than 12 minutes. Exclusion criteria include: • biological signs of death • younger than 18 years or older than 80 years

NCT01521208 *(Continued)*	• trauma caused cardiorespiratory arrest (CRA), including hanging • secondary CRA or intoxication • return of spontaneous circulation previous to arrival of SEM medical team • known pregnancy • inadequate size for LUCAS device • anything in the study that can delay treatment
Interventions	Mechanical chest compressions performed by LUCAS device
Outcomes	**Primary outcomes** • Survival at hospital admittance • Survival on discharge from hospital **Secondary outcomes** • Restoration of spontaneous circulation • End-tidal CO_2 values • SOFA scale values • Days before discharge from intensive care unit/coronary care unit • Metabolic (pH, lactate) and inflammatory (leukocytes, C-reactive protein) parameters • Epidemiology of out-of-hospital cardiac arrest • Blood sample for genetic and biological studies • Left ventricular function
Starting date	January 2012
Contact information	Dr. Francesc Carmona Jiménez; phone +34 607 8477 17; email franciscojosecarmona@gencat.cat Dr. Rosa-Maria Lidón; email rmlidon@vhebron.net
Notes	NCT01521208

DATA AND ANALYSES

Comparison 1. Mechanical chest compressions versus manual chest compressions

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Survival to hospital discharge with good neurological function	3		Risk Ratio (M-H, Random, 95% CI)	Totals not selected
2 Survival to hospital discharge	7		Risk Ratio (M-H, Random, 95% CI)	Totals not selected
3 Return of spontaneous circulation	8		Risk Ratio (M-H, Random, 95% CI)	Totals not selected

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
4 Survival to hospital admission	4		Risk Ratio (M-H, Random, 95% CI)	Totals not selected
5 Sternal or rib fractures	7		Risk Ratio (M-H, Random, 95% CI)	Totals not selected
6 Haemothorax or pneumothorax	5		Risk Ratio (M-H, Random, 95% CI)	Totals not selected
7 Internal abdominal organ injury	5		Risk Ratio (M-H, Random, 95% CI)	Totals not selected

Analysis 1.1. Comparison 1 Mechanical chest compressions versus manual chest compressions, Outcome 1 Survival to hospital discharge with good neurological function.

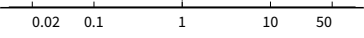
Study or subgroup	Mechanical n/N	Manual n/N	Risk Ratio M-H, Random, 95% CI	Risk Ratio M-H, Random, 95% CI
Hallstrom 2006	12/394	28/373		0.41[0.21,0.79]
Rubertsson 2014	108/1300	100/1289		1.07[0.82,1.39]
Wik 2014	87/2099	112/2132		0.79[0.6,1.04]

Analysis 1.2. Comparison 1 Mechanical chest compressions versus manual chest compressions, Outcome 2 Survival to hospital discharge.

Study or subgroup	Mechanical n/N	Manual n/N	Risk Ratio M-H, Random, 95% CI	Risk Ratio M-H, Random, 95% CI
Gao 2016	13/69	4/64		3.01[1.04,8.77]
Hallstrom 2006	23/394	37/373		0.59[0.36,0.97]
Lu 2010	25/76	11/74		2.21[1.18,4.17]
Rubertsson 2014	117/1300	118/1289		0.98[0.77,1.25]
Smekal 2011	6/75	7/72		0.82[0.29,2.33]
Taylor 1978	3/24	2/26		1.63[0.3,8.9]
Wik 2014	196/2099	233/2132		0.85[0.71,1.02]

Analysis 1.3. Comparison 1 Mechanical chest compressions versus manual chest compressions, Outcome 3 Return of spontaneous circulation.

Study or subgroup	Mechanical n/N	Manual n/N	Risk Ratio M-H, Random, 95% CI	Risk Ratio M-H, Random, 95% CI
Dickinson 1998	1/7	0/10		4.13[0.19,88.71]
Gao 2016	31/69	15/64		1.92[1.15,3.21]
Halperin 1993	8/17	3/17		2.67[0.85,8.37]



Study or subgroup	Mechanical n/N	Manual n/N	Risk Ratio M-H, Random, 95% CI	Risk Ratio M-H, Random, 95% CI
Lu 2010	42/76	28/74		1.46[1.02,2.08]
Perkins 2015	522/1652	885/2819		1.01[0.92,1.1]
Rubertsson 2014	460/1300	446/1289		1.02[0.92,1.14]
Smekal 2011	30/74	23/72		1.27[0.82,1.96]
Wik 2014	600/2099	689/2132		0.88[0.81,0.97]
		Favours manual	0.02 0.1 1 10 50	Favours mechanical

Analysis 1.4. Comparison 1 Mechanical chest compressions versus manual chest compressions, Outcome 4 Survival to hospital admission.

Study or subgroup	Mechanical n/N	Manual n/N	Risk Ratio M-H, Random, 95% CI	Risk Ratio M-H, Random, 95% CI
Dickinson 1998	1/7	0/10		4.13[0.19,88.71]
Perkins 2015	377/1652	658/2819		0.98[0.87,1.09]
Rubertsson 2014	366/1300	357/1289		1.02[0.9,1.15]
Smekal 2011	18/75	15/72		1.15[0.63,2.11]
		Favours manual	0.001 0.1 1 10 1000	Favours mechanical

Analysis 1.5. Comparison 1 Mechanical chest compressions versus manual chest compressions, Outcome 5 Sternal or rib fractures.

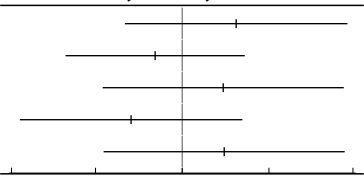
Study or subgroup	Mechanical n/N	Manual n/N	Risk Ratio M-H, Random, 95% CI	Risk Ratio M-H, Random, 95% CI
Gao 2016	4/60	3/63		1.4[0.33,6]
Halperin 1993	1/4	2/5		0.63[0.08,4.66]
Koster 2017	90/211	52/126		1.03[0.8,1.34]
Lu 2010	2/76	8/74		0.24[0.05,1.11]
Rubertsson 2014	1/1300	2/1289		0.5[0.05,5.46]
Taylor 1978	10/13	8/17		1.63[0.91,2.94]
Wik 2014	70/2099	36/2132		1.98[1.33,2.94]
		Favours mechanical	0.01 0.1 1 10 100	Favours manual

Analysis 1.6. Comparison 1 Mechanical chest compressions versus manual chest compressions, Outcome 6 Haemothorax or pneumothorax.

Study or subgroup	Mechanical n/N	Manual n/N	Risk Ratio M-H, Random, 95% CI	Risk Ratio M-H, Random, 95% CI
Halperin 1993	0/4	1/5		0.4[0.02,7.82]
Koster 2017	14/211	7/126		1.19[0.5,2.88]
Lu 2010	1/76	3/74		0.32[0.03,3.05]
Rubertsson 2014	1/1300	1/1289		0.99[0.06,15.84]
Wik 2014	34/2099	21/2132		1.64[0.96,2.82]
		Favours mechanical	0.01 0.1 1 10 100	Favours manual

Analysis 1.7. Comparison 1 Mechanical chest compressions versus manual chest compressions, Outcome 7 Internal abdominal organ injury.

Study or subgroup	Mechanical n/N	Manual n/N	Risk Ratio M-H, Random, 95% CI	Risk Ratio M-H, Random, 95% CI
Koster 2017	3/211	0/126		4.19[0.22,80.52]
Lu 2010	1/76	2/74		0.49[0.05,5.26]
Rubertsson 2014	1/1300	0/1289		2.97[0.12,72.95]
Taylor 1978	0/13	2/17		0.26[0.01,4.94]
Wik 2014	1/2099	0/2132		3.05[0.12,74.76]



APPENDICES

Appendix 1. Search strategies 2017 update

The searches below were performed on 30 April 2015 and repeated on 19 August 2017 to capture records between those two dates.

CENTRAL

#1 MeSH descriptor: [Heart Arrest] explode all trees

#2 cardiac next arrest

#3 heart next arrest

#4 cardiopulmonary next arrest

#5 sudden near/3 death

#6 MeSH descriptor: [Death, Sudden] explode all trees

#7 (#1 or #2 or #3 or #4 or #5 or #6)

#8 MeSH descriptor: [Cardiopulmonary Resuscitation] this term only

#9 MeSH descriptor: [Heart Massage] this term only

#10 cpr

#11 resuscitat*

#12 heart next massage

#13 cardiac next massage

#14 chest next compression

#15 (#8 or #9 or #10 or #11 or #12 or #13 or #14)

#16 (#7 and #15)

#17 piston

#18 autopulse

#19 auto-pulse

#20 thumper

#21 pneumatic

#22 lucas

#23 hands-free

#24 load next distributing

#25 vest

#26 mechanical near/6 compression

#27 mechanical near/6 cpr

#28 automat* near/6 compression

#29 automat* near/6 cpr

#30 device near/6 compression

#31 device near/6 cpr

#32 machine near/6 compression

#33 machine near/6 cpr

#34 MeSH descriptor: [Cardiopulmonary Resuscitation] this term only and with qualifiers: [Instrumentation - IS]

#35 (#17 or #18 or #19 or #20 or #21 or #22 or #23 or #24 or #25)

#36 (#26 or #27 or #28 or #29 or #30 or #31 or #32 or #33 or #34)

#37 (#35 or #36)

#38 (#37 and #16)

MEDLINE Ovid

1. exp Heart Arrest/

2. exp Death, Sudden/

3. cardiac arrest.tw.

4. heart arrest.tw.

5. cardiopulmonary arrest.tw.

6. sudden cardiac death$.tw.

7. sudden death$.tw.

8. or/1-7

9. exp Cardiopulmonary Resuscitation/

10. Heart Massage/

11. cpr.tw.

12. cardiopulmonary resuscitation.tw.

13. chest compression$.tw.

14. resuscitat$.tw.

15. or/9-14

16. Cardiopulmonary Resuscitation/is [Instrumentation]

17. autopulse.tw.

18. auto-pulse.tw.

19. thumper.tw.

20. lucas.tw.

21. hands-free.tw.

22. (pneumatic adj10 (pump or device)).tw.

23. (pneumatic adj10 compression$).tw.

24. (automat$ adj10 compression$).tw.

25. (device$ adj10 compression).tw.

26. (mechanical adj10 compression$).tw.

27. (machine$ adj10 compression$).tw.

28. piston$.tw.

29. load distributing.tw.

30. (vest adj10 compression).tw.

31. (mechanical adj10 cpr).tw.

32. (pneumatic adj10 cpr).tw.

33. (device adj10 cpr).tw.

34. (machine$ adj10 cpr).tw.

35. (vest adj10 cpr).tw.

36. or/16-35

37. 8 and 15 and 36

38. randomised controlled trial.pt.

39. controlled clinical trial.pt.

40. randomized.ab.

41. placebo.ab.

42. drug therapy.fs.

43. randomly.ab.

44. trial.ab.

45. groups.ab.

46. 38 or 39 or 40 or 41 or 42 or 43 or 44 or 45

47. exp animals/ not humans.sh.

48. 46 not 47

49. 37 and 48

50. (2013* or 2014* or 2015*).ed. [*** The repeated search in 2017 featured only the years 2015-2017]

51. 49 and 50

Embase Ovid

1. heart arrest/
2. sudden death/
3. cardiac arrest.tw.
4. heart arrest.tw.
5. cardiopulmonary arrest.tw.
6. sudden cardiac death$.tw.
7. sudden death$.tw.
8. or/1-7
9. resuscitation/
10. heart massage/
11. cpr.tw.
12. cardiopulmonary resuscitation.tw.
13. chest compression$.tw.
14. resuscitat$.tw.
15. or/9-14
16. cardiovascular equipment/
17. autopulse.tw.
18. auto-pulse.tw.
19. thumper.tw.
20. lucas.tw.
21. hands-free.tw.
22. (pneumatic adj10 (pump or device)).tw.
23. (pneumatic adj10 compression$).tw.
24. (automat$ adj10 compression$).tw.
25. (device$ adj10 compression).tw.
26. (mechanical adj10 compression$).tw.
27. (machine$ adj10 compression$).tw.
28. piston$.tw.
29. load distributing.tw.
30. (vest adj10 compression).tw.
31. (mechanical adj10 cpr).tw.
32. (pneumatic adj10 cpr).tw.
33. (device adj10 cpr).tw.
34. (machine$ adj10 cpr).tw.

35. (vest adj10 cpr).tw.

36. or/16-35

37. 8 and 15 and 36

38. random$.tw.

39. factorial$.tw.

40. crossover$.tw.

41. cross over$.tw.

42. cross-over$.tw.

43. placebo$.tw.

44. (doubl$ adj blind$).tw.

45. (singl$ adj blind$).tw.

46. assign$.tw.

47. allocat$.tw.

48. volunteer$.tw.

49. crossover procedure/

50. double blind procedure/

51. randomised controlled trial/

52. single blind procedure/

53. 38 or 39 or 40 or 41 or 42 or 43 or 44 or 45 or 46 or 47 or 48 or 49 or 50 or 51 or 52

54. (animal/ or nonhuman/) not human/

55. 53 not 54

56. 37 and 55

57. (2013* or 2014* or 2015*).em [*** The repeated search in 2017 featured only the years 2015-2017]

58. 56 and 57

Web of Science

7 #6 Databases=SCI-EXPANDED, CPCI-S Timespan=2013 to 28-4-2015 [*** The repeated search in 2017 featured only the years 2015-2017]

6 #5 AND #4

5 TS=(random* or blind* or allocat* or assign* or trial* or placebo* or crossover* or cross-over*)

4 #3 AND #2 AND #1

3 TS=(mechanical* or device* or automat*)

2 TS=(cardiopulmonary resuscitation or cpr or chest compression*)

1 TS=(cardiac arrest or sudden death or cardiopulmonary arrest)

ClinicalTrials.gov

1. Cardiac arrest and device*
2. Cardiac arrest and mechanical
3. 1 or 2

WHO International Clinical Trials Registry Platform

1. Cardiac arrest and mechanical
2. lucas
3. autopulse
4. 1 or 2 or 3

Appendix 2. Search strategies 2014 update

CENTRAL

#1 MeSH descriptor: [Heart Arrest] explode all trees
#2 cardiac next arrest
#3 heart next arrest
#4 cardiopulmonary next arrest
#5 sudden near/3 death
#6 MeSH descriptor: [Death, Sudden] explode all trees
#7 (#1 or #2 or #3 or #4 or #5 or #6)
#8 MeSH descriptor: [Cardiopulmonary Resuscitation] this term only
#9 MeSH descriptor: [Heart Massage] this term only
#10 cpr
#11 resuscitat*
#12 heart next massage
#13 cardiac next massage
#14 chest next compression
#15 (#8 or #9 or #10 or #11 or #12 or #13 or #14)
#16 (#7 and #15)
#17 piston
#18 autopulse
#19 auto-pulse
#20 thumper
#21 pneumatic
#22 lucas
#23 hands-free
#24 load next distributing
#25 vest
#26 mechanical near/6 compression
#27 mechanical near/6 cpr
#28 automat* near/6 compression
#29 automat* near/6 cpr
#30 device near/6 compression
#31 device near/6 cpr
#32 machine near/6 compression
#33 machine near/6 cpr
#34 MeSH descriptor: [Cardiopulmonary Resuscitation] this term only and with qualifiers: [Instrumentation - IS]
#35 (#17 or #18 or #19 or #20 or #21 or #22 or #23 or #24 or #25)
#36 (#26 or #27 or #28 or #29 or #30 or #31 or #32 or #33 or #34)
#37 (#35 or #36)
#38 (#37 and #16)

MEDLINE Ovid

1. exp Heart Arrest/
2. exp Death, Sudden/
3. cardiac arrest.tw.
4. heart arrest.tw.
5. cardiopulmonary arrest.tw.
6. sudden cardiac death$.tw.
7. sudden death$.tw.
8. or/1-7
9. exp Cardiopulmonary Resuscitation/
10. Heart Massage/
11. cpr.tw.
12. cardiopulmonary resuscitation.tw.

13. chest compression$.tw.
14. resuscitat$.tw.
15. or/9-14
16. Cardiopulmonary Resuscitation/is [Instrumentation]
17. autopulse.tw.
18. auto-pulse.tw.
19. thumper.tw.
20. lucas.tw.
21. hands-free.tw.
22. (pneumatic adj10 (pump or device)).tw.
23. (pneumatic adj10 compression$).tw.
24. (automat$ adj10 compression$).tw.
25. (device$ adj10 compression).tw.
26. (mechanical adj10 compression$).tw.
27. (machine$ adj10 compression$).tw.
28. piston$.tw.
29. load distributing.tw.
30. (vest adj10 compression).tw.
31. (mechanical adj10 cpr).tw.
32. (pneumatic adj10 cpr).tw.
33. (device adj10 cpr).tw.
34. (machine$ adj10 cpr).tw.
35. (vest adj10 cpr).tw.
36. or/16-35
37. 8 and 15 and 36
38. randomised controlled trial.pt.
39. controlled clinical trial.pt.
40. randomized.ab.
41. placebo.ab.
42. drug therapy.fs.
43. randomly.ab.
44. trial.ab.
45. groups.ab.
46. 38 or 39 or 40 or 41 or 42 or 43 or 44 or 45
47. exp animals/ not humans.sh.
48. 46 not 47
49. 37 and 48
50. (200911* or 2010* or 2011* or 2012* or 2013*).ed.
51. 49 and 50

Embase Ovid

1. heart arrest/
2. sudden death/
3. cardiac arrest.tw.
4. heart arrest.tw.
5. cardiopulmonary arrest.tw.
6. sudden cardiac death$.tw.
7. sudden death$.tw.
8. or/1-7
9. resuscitation/
10. heart massage/
11. cpr.tw.
12. cardiopulmonary resuscitation.tw.
13. chest compression$.tw.
14. resuscitat$.tw.
15. or/9-14
16. cardiovascular equipment/
17. autopulse.tw.
18. auto-pulse.tw.
19. thumper.tw.
20. lucas.tw.

21. hands-free.tw.
22. (pneumatic adj10 (pump or device)).tw.
23. (pneumatic adj10 compression$).tw.
24. (automat$ adj10 compression$).tw.
25. (device$ adj10 compression).tw.
26. (mechanical adj10 compression$).tw.
27. (machine$ adj10 compression$).tw.
28. piston$.tw.
29. load distributing.tw.
30. (vest adj10 compression).tw.
31. (mechanical adj10 cpr).tw.
32. (pneumatic adj10 cpr).tw.
33. (device adj10 cpr).tw.
34. (machine$ adj10 cpr).tw.
35. (vest adj10 cpr).tw.
36. or/16-35
37. 8 and 15 and 36
38. random$.tw.
39. factorial$.tw.
40. crossover$.tw.
41. cross over$.tw.
42. cross-over$.tw.
43. placebo$.tw.
44. (doubl$ adj blind$).tw.
45. (singl$ adj blind$).tw.
46. assign$.tw.
47. allocat$.tw.
48. volunteer$.tw.
49. crossover procedure/
50. double blind procedure/
51. randomised controlled trial/
52. single blind procedure/
53. 38 or 39 or 40 or 41 or 42 or 43 or 44 or 45 or 46 or 47 or 48 or 49 or 50 or 51 or 52
54. (animal/ or nonhuman/) not human/
55. 53 not 54
56. 37 and 55
57. ("200945" or "200946" or "200947" or "200948" or "200949" or 20095* or 2010* or 2011* or 2012* or 2013*).em.
58. 56 and 57

Web of Science

7 #6 Databases=SCI-EXPANDED, CPCI-S Timespan=2009-11-18 - 2013-01-23
6 #5 AND #4
5 TS=(random* or blind* or allocat* or assign* or trial* or placebo* or crossover* or cross-over*)
4 #3 AND #2 AND #1
3 TS=(mechanical* or device* or automat*)
2 TS=(cardiopulmonary resuscitation or cpr or chest compression*)
1 TS=(cardiac arrest or sudden death or cardiopulmonary arrest)

ClinicalTrials.gov

1. Cardiac arrest and device*
2. Cardiac arrest and mechanical
3. 1 or 2

Appendix 3. Search strategies for original review published in 2011

CENTRAL on the Cochrane Library

#1 MeSH descriptor heart arrest explode all trees
#2 cardiac next arrest in All Text
#3 heart next arrest in All Text
#4 cardiopulmonary next arrest in All Text
#5 sudden near/3 death in All Text

#6 MeSH descriptor Death, Sudden explode all trees
#7 (#1 or #2 or #3 or #4 or #5 or #6)
#8 MeSH descriptor Cardiopulmonary Resuscitation this term only
#9 MeSH descriptor heart massage this term only
#10 cpr in All Text
#11 resuscitat* in All Text
#12 heart next massage in All Text
#13 cardiac next massage in All Text
#14 chest next compression in All Text
#15 (#8 or #9 or #10 or #11 or #12 or #13 or #14)
#16 (#7 and #15) 5
#17 piston in All Text
#18 autopulse in All Text
#19 auto-pulse in All Text
#20 thumper in All Text
#21 pneumatic in All Text
#22 lucas in All Text
#23 hands-free in All Text
#24 load next distributing in All Text
#25 vest in All Text
#26 (mechanical in All Text near/6 compression in All Text)
#27 (mechanical in All Text near/6 cpr in All Text)
#28 (automat* in All Text near/6 compression in All Text)
#29 (automat* in All Text near/6 cpr in All Text)
#30 (device in All Text near/6 compression in All Text)
#31 (device in All Text near/6 cpr in All Text)
#32 (machine in All Text near/6 compression in All Text)
#33 (machine in All Text near/6 cpr in All Text)
#34 MeSH descriptor cardiopulmonary resuscitation this term only with qualifiers: IS
#35 (#17 or #18 or #19 or #20 or #21 or #22 or #23 or #24 or #25)
#36 (#26 or #27 or #28 or #29 or #30 or #31 or #32 or #33 or #34)
#37 (#35 or #36)
#38 (#37 and #16)

MEDLINE OVID

1. exp Heart Arrest/
2. exp Death, Sudden/
3. cardiac arrest.tw.
4. heart arrest.tw.
5. cardiopulmonary arrest.tw.
6. sudden cardiac death$.tw.
7. sudden death$.tw.
8. or/1-7
9. exp Cardiopulmonary Resuscitation/
10. Heart Massage/
11. cpr.tw.
12. cardiopulmonary resuscitation.tw.
13. chest compression$.tw.
14. resuscitat$.tw.
15. or/9-14 (33098)
16. Cardiopulmonary Resuscitation/is [Instrumentation]
17. autopulse.tw.
18. auto-pulse.tw.
19. thumper.tw.
20. lucas.tw.
21. hands-free.tw.
22. (pneumatic adj10 (pump or device)).tw.
23. (pneumatic adj10 compression$).tw.
24. (automat$ adj10 compression$).tw.
25. (device$ adj10 compression).tw.
26. (mechanical adj10 compression$).tw.

27. (machine$ adj10 compression$).tw.
28. piston$.tw.
29. load distributing.tw.
30. (vest adj10 compression).tw.
31. (mechanical adj10 cpr).tw.
32. (pneumatic adj10 cpr).tw.
33. (device adj10 cpr).tw.
34. (machine$ adj10 cpr).tw.
35. (vest adj10 cpr).tw.
36. or/16-35
37. 8 and 15 and 36
38. randomised controlled trial.pt.
39. controlled clinical trial.pt.
40. randomised controlled trials.sh.
41. random allocation.sh.
42. double blind method.sh.
43. single blind method.sh.
44. or/38-43
45. (animals not humans).sh.
46. 44 not 45
47. clinical trial.pt.
48. exp clinical trials/
49. (clin$ adj25 trial$).ti,ab.
50. ((singl$ or doubl$ or trebl$ or tripl$) adj25 (blind$ or mask$)).ti,ab.
51. placebos.sh.
52. placebo$.ti,ab.
53. random$.ti,ab.
54. research design.sh.
55. or/47-54
56. 55 not 45
57. 56 not 46
58. comparative study.sh.
59. exp evaluation studies/
60. follow up studies.sh.
61. prospective studies.sh.
62. (control$ or prospectiv$ or volunteer$).ti,ab.
63. or/58-62
64. 63 not 45
65. 64 not (46 or 57)
66. 46 or 57 or 65
67. 37 and 66

Embase OVID

1. Heart Arrest/
2 . Sudden Death/
3. cardiac arrest.tw.
4. heart arrest.tw.
5. cardiopulmonary arrest.tw.
6. sudden cardiac death$.tw.
7. sudden death$.tw.
8. or/1-7
9. Resuscitation/
10. Heart Massage/
11. cpr.tw.
12. cardiopulmonary resuscitation.tw.
13. chest compression$.tw.
14. resuscitat$.tw.
15. or/9-14
16. Cardiovascular Equipment/
17. autopulse.tw.
18. auto-pulse.tw.

19. thumper.tw.
20. lucas.tw.
21. hands-free.tw.
22. (pneumatic adj10 (pump or device)).tw.
23. (pneumatic adj10 compression$).tw.
24. (automat$ adj10 compression$).tw.
25. (device$ adj10 compression).tw.
26. (mechanical adj10 compression$).tw.
27. (machine$ adj10 compression$).tw.
28. piston$.tw.
29. load distributing.tw.
30.(vest adj10 compression).tw.
31. (mechanical adj10 cpr).tw.
32. (pneumatic adj10 cpr).tw.
33. (device adj10 cpr).tw.
34. (machine$ adj10 cpr).tw.
35. (vest adj10 cpr).tw.
36. or/16-35
37. 8 and 15 and 36
38. clinical trial/
39. random$.tw.
40. randomised controlled trial/
41. trial$.tw.
42. follow-up.tw.
43. double blind procedure/
44. placebo$.tw.
45. placebo/
46. factorial$.ti,ab.
47. (crossover$ or cross-over$).ti,ab.
48. (double$ adj blind$).ti,ab.
49. (singl$ adj blind$).ti,ab.
50. assign$.ti,ab.
51. allocat$.ti,ab.
52. volunteer$.ti,ab.
53. Crossover Procedure/
54. Single Blind Procedure/
55. or/38-54
56. (exp animal experiment/ or nonhuman/) not exp human/
57. 55 not 56
58. 37 and 57

Science Citations Index and Biotech and Bioengineering abstracts

cardiac arrest or sudden death or cardiopulmonary arrest
AND
cardiopulmonary resuscitation or cpr or chest compression*
AND
mechanical* or device* or automat*

ClinicalTrials.gov

1. Cardiac arrest and device*
2. Cardiac arrest and mechanical
3. 1 or 2

Appendix 4. Glossary

The CPC (Cerebral Performance Category) score

The Cerebral Performance Category scale is a simple five-point measurement of cerebral and functional status that ranges from category one to indicate good performance to category five to indicate brain death. The Cerebral Performance Category scale is widely used to evaluate functional outcome in resuscitation research because it requires little training, is brief and can be evaluated with the health record.

The modified Rankin Scale (mRS)

The modified Rankin scale is a single-item, 6-point measurement of primarily functional domains that range from zero (no impairment) to six (deceased). This is usually used on patients who have suffered a stroke or other neurological insult to measure functional dependency postinjury. The mRS may be determined through chart review.

Utstein guidelines

The Utstein style for reporting cardiac arrests arose from a 1990 conference at the ancient abbey of that name on an island near Stavanger, Norway. That conference and another later that year were attended by representatives of the AHA, the European Resuscitation Council, the Heart and Stroke Foundation of Canada and the Australian Resuscitation Council. The major concern was that the results of resuscitation endeavours in different countries, and even within countries, could not be compared meaningfully. Researchers have used disparate endpoints to assess the effectiveness of different systems and interventions. Useful comparisons have been prevented by this lack of uniform definitions and standard methodologies. The Utstein guidelines are a defined set of data elements that are essential or desirable for documenting in-hospital cardiac arrest. Data categories are hospital variables, patient variables, arrest variables and outcome variables. The 'In-Hospital Utstein-Style Template' was developed to summarise these data and recommendations for reporting a specific set of survival rates and outcomes. The Utstein style has attracted wide interest and has become a familiar term among members of the resuscitation community. Many researchers and system directors have adopted the Utstein templates, style and nomenclature to report results of prehospital resuscitation. The success of this international initiative soon led to uniform international styles for reporting the results of paediatric resuscitation and experimental (laboratory) resuscitation.

WHAT'S NEW

Date	Event	Description
31 August 2017	New citation required but conclusions have not changed	We identified five new studies for this update (Gao 2016; Koster 2017; Perkins 2015; Rubertsson 2014; Wik 2014).
31 August 2017	New search has been performed	We updated the searches to August 2017.

HISTORY

Protocol first published: Issue 3, 2008
Review first published: Issue 1, 2011

Date	Event	Description
13 January 2014	New citation required but conclusions have not changed	Two new citations have been identified in this update (Smekal 2011 and Lu 2010), but the data contained within have not changed our conclusion.
7 August 2013	New search has been performed	Review updated with new search dated January 2013. New author Nizar Hassan added.

CONTRIBUTIONS OF AUTHORS

Steven Brooks: guarantor for the review, conception of the study question, protocol draft development and revision, search strategy development and execution, selection of studies, quality review, data abstraction, data analysis, supervised Peter Wang in the process of conducting the review as first author.

Peter Wang: contributed to the 2017 update: quality review, data abstraction, data analysis, manuscript update drafting, editing.

DECLARATIONS OF INTEREST

Steven Brooks has received salary support from the Heart and Stroke Foundation to study the implementation of public access defibrillation. Dr. Brooks receives funding from the Canadian Institutes of Health Research to conduct research on the PulsePoint mobile app for cardiac arrest. Dr. Brooks has received operating funds from the Heart and Stroke Foundation and the Canadian Institutes of Health Research for various other peer-reviewed research studies in cardiac arrest. Dr. Brooks is a paid expert witness for the defense in a variety

of civil cases involving the non-use of a defibrillator in a case of out-of-hospital cardiac arrest, misdiagnosis of chest pain and misdiagnosis of meningitis.

Peter Wang has no conflicts to report.

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Internal sources

- No sources of support supplied

External sources

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INDEX TERMS

Medical Subject Headings (MeSH)

Blood Circulation; Cardiopulmonary Resuscitation [instrumentation] [*methods] [mortality]; Heart Arrest [mortality] [*therapy]; Heart Massage [instrumentation] [*methods] [mortality]; Hospitalization; Patient Discharge; Randomized Controlled Trials as Topic

MeSH check words

Humans