Education, Implementation, and Teams : 2020 International Consensus on Cardiopulmonary Resuscitation and Emergency Cardiovascular Care Science with Treatment Recommendations

Reference:
Greif Robert, Bhanji Farhan, Bigham Blair L., Bray Janet, Breckwoldt Jan, Cheng Adam, Duff Jonathan P., Gilfoyle Elaine, Hsieh Ming-Ju, Iwami Taku, ...- Education, Implementation, and Teams : 2020 International Consensus on Cardiopulmonary Resuscitation and Emergency Cardiovascular Care Science with Treatment Recommendations
Circulation / American Heart Association - ISSN 0009-7322 - 142:s:[1](2020), p. S222-S283
Full text (Publisher's DOI): https://doi.org/10.1161/CIR.0000000000000896
To cite this reference: https://hdl.handle.net/10067/1742870151162165141

2020 CoSTR

7: Education, Implementation, and Teams

2020 International Consensus on Cardiopulmonary Resuscitation and Emergency Cardiovascular Care Science With Treatment Recommendations

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Key Words: Education, opioid overdose, basic life support

[h1]Abstract

For this *2020 International Consensus on Cardiopulmonary Resuscitation and Emergency Cardiovascular Care Science With Treatment Recommendations*, the Education, Implementation, and Teams Task Force applied the population, intervention, comparator, outcome, study design, time frame format and performed ~~14~~ 15 systematic reviews, applying the Grading of Recommendations, Assessment, Development, and Evaluation guidance. Furthermore, 4 scoping reviews and 7 evidence updates assessed any new evidence to determine if a change in any existing treatment recommendation was required. The topics covered included training for the treatment of opioid overdose; basic life support, including automated external defibrillator training; measuring implementation and performance in communities and cardiac arrest centers; advanced life support training, including team and leadership training and rapid response teams; measuring cardiopulmonary resuscitation performance, feedback devices, and debriefing; and the use of social media to improve cardiopulmonary resuscitation application.

[h1]Introduction

The *2020 International Consensus on Cardiopulmonary Resuscitation* (CPR) *and Emergency Cardiovascular Care Science With Treatment Recommendations* (CoSTR) is the fourth in a series of annual summary publications from the International Liaison Committee on Resuscitation (ILCOR). This 2020 CoSTR for education, implementation, and teams (EIT) includes new topics addressed by systematic reviews (SysRevs) performed within the past 12 months. It also includes updates of the EIT treatment recommendations published from 2010 through 2019,[1-6] as needed, that are based on additional evidence evaluations. As a result, this 2020 CoSTR for EIT represents the most comprehensive update since 2010. The 3 major types of evidence evaluation supporting this 2020 publication are the SysRev, the scoping review (ScopRev), and the evidence update (EvUp).

The SysRev is a rigorous process following strict methodology to answer a specific question, and each of these ultimately resulted in generation of the task force CoSTR included in this publication. The SysRevs were performed by an expert systematic reviewer or by the EIT Task Force, and many have resulted in separate published SysRevs.

To begin the SysRev, the question to be answered was phrased in terms of the PICOST (population, intervention, comparator, outcome, study design, time frame) format. The methodology used to *identify* the evidence was based on the Preferred Reporting Items for Systematic Reviews and Meta-Analyses.[7] The approach used to *evaluate* the evidence was based on that proposed by the Grading of Recommendations, Assessment, Development, and Evaluation Working Group.[8] Using this approach for each of the predefined outcomes, the task force rated as high, moderate, low, or very low the certainty/confidence in the estimates of effect of an intervention or assessment across a body of evidence. Randomized controlled trials (RCTs) began the analysis as high-certainty evidence, and observational studies began the analysis as low-certainty evidence; examination of the evidence using the Grading of Recommendations, Assessment, Development, and Evaluation approach could result in downgrading or upgrading the certainty of evidence. For additional information, refer to Evidence Evaluation Process and Management of Potential Conflicts of Interest in this supplement.[9]

Where a pre-2015 CoSTR treatment recommendation was not updated, the language used differs from that used in the Grading of Recommendations, Assessment, Development, and Evaluation approach, because Grading of Recommendations, Assessment, Development, and Evaluation was not used before 2015.[10-12]

It important to note, that GRADE, which was designed for clinical studies, was applied across different types of literature to maintain consistency across the ILCOR review process. There were challenges in applying GRADE to the evaluation of educational studies, and ILCOR will continue to consider alternative approaches for future evidence reviews.

Draft 2020 CoSTRs for EIT were posted on the ILCOR website[13] for public comment between December 31, 2019, and February 18, 2020, with comments accepted through March 3, 2020. The 14 EIT Task Force draft CoSTR statements received 15277 views and 18 comments. All comments were reviewed by the EIT Task Force, but none of the comments led to any change in the treatment recommendations.

This summary statement contains the final wording of the CoSTR statements as approved by the ILCOR task forces and by the ILCOR member councils after review and consideration of comments posted online in response to the draft CoSTRs. Within this publication, each topic includes the PICOST as well as the CoSTR, an expanded section on justification and evidence-to-decision framework highlights, and a list of knowledge gaps requiring future research studies. An evidence-to-decision table is included for each CoSTR in Appendix A of this publication.

The second major type of evidence evaluation performed to support this 2020 CoSTR for EIT is a ScopRev. ScopRevs are designed to identify the extent, range, and nature of evidence on a topic or a question, and they were performed by topic experts in consultation with the EIT Task Force. The task force assessed the identified evidence and determined its value and implications for resuscitation practice or research. The rationale for the ScopRev, the summary of evidence, and task force insights are all highlighted in the body of this publication. The most recent treatment recommendation is included. The task force notes whether the ScopRev identified substantive evidence that may result in a change in ILCOR treatment recommendations. If sufficient evidence was identified, the task force suggested consideration of a future SysRev to supply sufficient detail to support the development of an updated CoSTR. All ScopRevs are included in their entirety in Appendix B of this publication.

The third type of evidence evaluation supporting this CoSTR for EIT is an EvUp. EvUps are generally performed for topics previously reviewed by ILCOR, to identify new studies published after the most recent ILCOR evidence evaluation, typically through use of search terms and methodologies from previous reviews. Several EvUps for new topics deemed to be important but missing from the existing reviews were also undertaken (based on a PubMed/Medline search only) by one or more of the member resuscitation councils. The EvUps were performed by task force members, collaborating experts, or members of Council writing groups. The EvUps are cited in the body of this publication with a note as to whether the evidence suggested the need to consider a SysRev. The existing ILCOR treatment recommendation was reiterated. In this publication, no change in ILCOR treatment recommendations resulted from an EvUp; if substantial new evidence was identified, the task force recommended consideration of a SysRev. All EvUps are included in their entirety in Appendix C of this publication.

The following topics have been reviewed:

Training for Treatment of Opioid Overdose

Opioid overdose first aid education (EIT 4001: ScopRev)

Basic Life Support (BLS) Including Automated External Defibrillator (AED) Training

Willingness to perform bystander CPR (EIT 626: ScopRev)

Prehospital termination of resuscitation (TOR) (EIT 642: SysRev)

In-hospital termination of resuscitation (TOR) (EIT 4002: SysRev)

Deliberate practice and mastery learning (EIT 4004: EvUp)

Layperson training (EIT 4009: EvUp)

Timing for retraining (EIT 628: EvUp)

Measuring Implementation/Performance in Communities, Cardiac Arrest Centers

System performance improvements (EIT 640: SysRev)

Community initiatives to promote BLS implementation (EIT 641: ScopRev)

Cardiac arrest centers (EIT 624: SysRev, 2019 CoSTR)

Out-of-hospital CPR training in low-resource settings (EIT 634: ScopRev)

Disparities in education (EIT 4003: EvUp)

Advanced Life Support (ALS) Training, Including Team and Leadership Training, and Medical Emergency Teams (METs) and Rapid Response Teams (RRTs)

Spaced learning (EIT 1601: SysRev)

Emergency medical services (EMS) experience and exposure (EIT 437: SysRev)

Cognitive aids during resuscitation education (EIT 629: SysRev)

Team and leadership training (EIT 631: SysRev)

Precourse preparation for advanced courses (EIT 637: SysRev)

Rapid response systems (RRSs) in adults (EIT 638: SysRev)

End-of-course testing versus continuous assessment (EIT 643: SysRev)

Virtual reality, augmented reality, and gamified learning (EIT 4005: EvUp)

In situ training (EIT 4007: EvUp)

High-fidelity manikins for ALS training (EIT 623: EvUp)

Measuring CPR Performance, Feedback Devices, and Debriefing

Debriefing of resuscitation performance (EIT 645: SysRev)

CPR feedback devices during training (EIT 648: SysRev)

Patient outcomes as a result of a member of the resuscitation team attending an ALS course (EIT 4000: SysRev)

Use of Social Media

First responder engaged by technology (EIT 878: SysRev)

[H1] Training for Treatment of Opioid Overdose

[H2] Opioid Overdose First Aid Education (EIT 4001: ScopRev)

[H3] Rationale for Review

In 2015, the ALS Task Force recommended the use of naloxone for individuals in cardiac arrest caused by opioid toxicity (strong recommendation, very low quality of evidence).[14,15] Because of lack of evidence, in 2015 the BLS Task Force did not make a treatment recommendation for using naloxone for suspected opioid overdose. However, the BLS Task Force did suggest offering opioid overdose response education, with or without naloxone distribution, to persons at risk for opioid overdose in any setting (weak recommendation, very low quality of evidence).[16,17] The EIT Task Force chose to identify the scope of current opioid overdose response education programs reporting outcomes to recommend further SysRev or identify gaps in the existing literature on education of the use of naloxone in possible opioid overdose.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: First aid providers responding to opioid overdose

Intervention: Education on response or care of an individual in an opioid overdose emergency

Comparator: Any other or no specialized education

Outcome: Any clinical or educational outcome; survival, first aid provided, skills, attitude, knowledge

Study design: RCTs and nonrandomized studies (interrupted time series, controlled before-and-after studies, cohort studies) were included. Studies that did not specifically answer the question, unpublished studies (e.g. conference abstracts, trial protocols), and studies only published in abstract form, unless accepted for publication, were excluded.

Time frame: All years and all languages were included if there was an English abstract; literature search was updated to November 13, 2019.

[H3] Summary of Evidence

The full ScopRev is included in Appendix B-1.

We found insufficient data to warrant consideration of a SysRev comparing one educational intervention with another or with no education.

Eight[18-25] out of 59 studies finally identified, from a systematic search of 2057, used a comparator group. The 1 RCT reported first aid/naloxone use at 8 of 13 witnessed overdoses within 3 months after interventions; 2 of the 5 overdoses witnessed by an individual in the facilitator-trained group administered naloxone compared with 0 of 3 individuals in the comparison group who received only a pamphlet.[18]

Met opmerkingen [GR1]: Reference 23 (J Am Pharm Assoc (2003)) should be listed as 2019 and not 2003

[H3] Task Force Insights

The EIT Task Force identified several limitations in the evidence relating to opioid overdose education: inconsistent reporting of educational interventions makes comparison between studies challenging. The use of the Guideline for Reporting Evidence-Based Practice Educational Interventions and Teaching checklist for educational interventions would help standardize future analysis.[26]

With only 1 RCT[18] and 7 other studies with control groups,[19-25] a lack of experimental rigor limits comparison and the strength of any future recommendations.

First aid and survival outcomes were self-reported by people generally coming in for a refill of their prescription for naloxone. The verifiability of this data was not reported. A prospective means to validate self-reported use of first aid/naloxone in these emergencies should be developed. For example, if EMS was called, corroborating the status of the poisoned victim, naloxone administration, and outcome could help establish validity. This is challenging because there is debate about the need for hospitalization after reversal of the overdose.

Brief training (less than 15 minutes) for people who use opioids nonmedically without knowing first aid skills appears beneficial for survival, perhaps because of personal and social experience with drugs. Stand-alone education (16–60 minutes) with skill training on administering first aid/naloxone for people who use opioids medically and nonmedically and for first responders is associated with improved outcomes for poisoned victims. The EIT Task Force found no evidence to change the current treatment recommendation.

[H3] Treatment Recommendation

This treatment recommendation from the BLS Task Force is unchanged from 2015.[16,17] We suggest offering opioid overdose response education, with or without naloxone distribution, to persons at risk for opioid overdose in any setting (weak recommendation, very low quality of evidence). In making these recommendations, we place greater value on the potential for lives saved by recommending overdose response education, with or without naloxone, and lesser value on the costs associated with naloxone administration, distribution, or education.

[H1]BLS Including AED Training

[H2] Willingness to Perform Bystander CPR (EIT 626: ScopRev)

[H3] Rationale for Review

The 2010 CoSTR included a narrative review on this topic and described both positive and negative factors impacting the willingness of bystanders (both lay rescuers and healthcare providers) to provide CPR.[1,2] The 2015 CoSTR recommended the use of BLS training interventions that focus on high-risk populations, on the basis of their willingness to be trained and the fact that there is little harm and high potential benefit (strong recommendation, low-quality evidence).[3]

This topic of willingness of bystanders to perform CPR was chosen for a 2020 ScopRev by the EIT Task Force because of the low incidence of provision of CPR and AED use by bystanders in most areas of the world.[27-29] Understanding the barriers and facilitators of

Met opmerkingen [JF2]: ADD REFERNCE #4
#4 Bhanji F, Finn JC, Lockey A, Monsieurs K, Frengley R, Iwami T, Lang E, Ma MH, Mancini ME, McNeil MA, et al; on behalf of the Education, Implementation, and Teams Chapter Collaborators. Part 8: education, implementation, and teams: 2015 International Consensus on Cardiopulmonary Resuscitation and Emergency Cardiovascular Care Science With Treatment Recommendations. *Circulation*. 2015;132(suppl 1):S242–S268. doi: 10.1161/CIR.0000000000000277

Met opmerkingen [GR3]: ADD REFERENCE
Kiguchi T, Okubo M, Nishiyama C, Maconochie I, Ong MEH, Kern KB, Wyckoff MH, McNally B, Christensen E, Tjelmeland I, et al. Out-of-hospital cardiac arrest across the world: First report from the International Liaison Committee on Resuscitation (ILCOR). *Resuscitation*. 2020; In press. doi: 10.1016/j.resuscitation.2020.02.044

https://www.resuscitationjournal.com/article/S0300-9572(20)30129-5/pdf bystander CPR and AED might lead to increased use of AEDs. These facilitators or barriers to perform CPR can be categorized into personal factors, CPR knowledge, and procedural issues.[30]

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Out-of-hospital cardiac arrest (OHCA) bystanders (laypersons)

Intervention: Factors increasing the willingness of bystanders to perform CPR

Comparator: Factors that decrease the willingness of bystanders to perform CPR

Outcome: Resulting in bystander CPR performance in an actual situation and willingness to provide CPR in an actual situation

Study design: RCTs and nonrandomized studies (eg, interrupted time series, controlled before-and-after studies, cohort studies) investigating factors associated with an increase or decrease in bystander CPR in actual settings. Exclusion criteria were simulation studies, unpublished studies (eg, conference abstracts, trial protocols), letters, editorials, comments, case reports, SysRevs, any gray literature, or studies overlapping other ILCOR SysRevs/ScopRevs (eg, dispatcher-instructed CPR, community initiatives to improve CPR, etc).

Time frame: All years and all languages were included if there was an English abstract; literature search was updated to January 4, 2020.

[H3] Summary of Evidence

The full ScopRev is included in Appendix B-2.

We found insufficient data to warrant consideration of a SysRev. Studies had significant heterogeneity among study populations, study methodologies, definitions of factors associated with willingness to provide CPR, outcome measures used, and outcomes reported. There were no

RCTs and 18 observational studies[30-47] reporting factors associated with the willingness of actual bystanders to perform CPR.

[H3] Task Force Insights

The EIT Task Force decided to perform a ScopRev with a narrative summary to gain insight into factors associated with bystanders' actions in real emergencies. ~~The task force categorized the factors associated with bystanders' actions into 3 categories as recommended in a recent review[30]: procedural issues, CPR knowledge, and personal factors.~~

On the basis of this ScopRev and the discussion of the task force, it was suggested that although the 2010 treatment recommendation remains valid, the following proposals should be given further consideration:

- All BLS training, as well as regional and national education programs for lay rescuers, should include information to overcome potential barriers to CPR faced by lay rescuer (eg, panic, disagreeable physical characteristics of the victim, CPR on a female patient)
- When providing CPR instructions, EMS dispatchers should recognize lay rescuers' personal factors (emotional barriers and physical factors that may make them reluctant to perform CPR) and support them in starting and continuing CPR.

[H3] Treatment Recommendation

This treatment recommendation is unchanged from 2010.[1,2]

To increase willingness to perform CPR, laypeople should receive training in CPR. This training should include the recognition of gasping or abnormal breathing as a sign of cardiac arrest when other signs of life are absent. Laypeople should be trained to start resuscitation with chest compressions in adult and pediatric victims. If unwilling or unable to perform ventilation, rescuers should be instructed to continue compression-only CPR. EMS dispatchers should provide CPR instructions to callers who report cardiac arrest. When providing CPR instructions, EMS dispatchers should include recognition of gasping and abnormal breathing.

[H2] Prehospital Termination Of Resuscitation (TOR) (EIT 642: SysRev)

[H3] Rationale for Review

There has been no recent ILCOR recommendation addressing prehospital TOR rules after out-of hospital cardiac arrest (OHCA). Individual TOR rules have been developed and implemented in a variety of emergency medical systems (EMS), but there has been little study of the impact of these rules in prehospital practice. A SysRev addressing the question "Do prehospital TOR rules reliably predict in-hospital outcome following OHCA?" has been completed.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Adults and children in cardiac arrest who do not achieve return of spontaneous circulation (ROSC) in the out-of-hospital environment

Intervention: TOR rules

Comparator: In-hospital outcomes (died/survived), and favorable/unfavorable neurologic outcome

Outcome: Ability of TOR to predict death in hospital (critically important) and unfavorable neurologic outcome (critically important)

Study design: Cross-sectional or cohort studies are eligible for inclusion. Unpublished studies (eg, conference abstracts, trial protocols) were excluded.

Time frame: All years and all languages were included if there was an English abstract. The search was completed on July 10, 2019.

PROSPERO registration CRD42019131010

[H3] Consensus on Science

The SysRev identified 34 studies[48-79] addressing the use of TOR rules. To facilitate improved insight into context and usefulness of the various TOR rules, studies were grouped as follows across the 2 outcomes: 1) prediction of death in hospital and 2) prediction of poor neurologic outcome

Met opmerkingen [GR4]: Add 2 references need to be added:
Marsden AK, Ng GA, Dalziel K and Cobbe SM. When is it futile for ambulance personnel to initiate cardiopulmonary resuscitation? BMJ. 1995;311:49-51
Petrie DA, De Maio V, Stiell IG, Dreyer J, Martin M and O'Brien JA. Factors affecting survival after prehospital asystolic cardiac arrest in a Basic Life Support-Defibrillation system. CJEM, Can. 2001;3:186-92.

[H4] For the Critically Important Outcome of Prediction of Death in Hospital

a) Studies reporting the derivation and internal validation of a TOR rule to predict death after arrival at hospital
b) Studies reporting external validation of a TOR rule to predict death after arrival at hospital
c) Studies reporting clinical validation of a TOR rule to predict death after arrival at hospital

[H5] Studies Reporting the Derivation and Internal Validation of a TOR Rule to Predict Death in Hospital

We identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, indirectness, and imprecision) from 12 nonrandomized studies.[48,51,56,57,65,66,75,76,79]. Between them these studies derived and internally validated 15 distinct TOR rules to predict death after arrival at hospital. Studies by Lee et al {Lee 2019 e134} and Yoon et al { Yoon 2019, 73} derived multiple TOR rules. There was considerable heterogeneity in patient population, clinician

Met opmerkingen [SM5]: Insert ref 60 (Haukoos 2004 145) that was missing from this outcome
Add 2 references:
{Marsden 1995 49} Marsden AK, Ng GA, Dalziel K and Cobbe SM. When is it futile for ambulance personnel to initiate cardiopulmonary resuscitation? BMJ. 1995;311:49-51
{Petrie 2001 186} Petrie DA, De Maio V, Stiell IG, Dreyer J, Martin M and O'Brien JA. Factors affecting survival after prehospital asystolic cardiac arrest in a Basic Life Support-Defibrillation system. CJEM, Can. 2001;3:186-92.

population, and EMS system design; thus, meta-analysis was not appropriate. Reported sensitivities and specificities of included papers are listed in Table 1.

Table 1 Sensitivity and specificity of derivation and internal validation studies (Death)

Author (TOR rule)	Sensitivity [95% CI]	Specificity [95% CI]
Bonnin et al 1993 (no-ROSC TOR){Bonnin 1993 1457}.	0.77 [0.74, 0.79]	0.93 [0.86, 0.98]
Chiang et al 2016 (tCPA TOR){Chiang 2016 39}	0.17 [0.15, 0.20]	1.00 [0.91, 1.00]
Glober et al 2019 (Glob1 TOR){Glober 2019 8}56	0.14 [0.13, 0.16]	1.00 [0.98, 1.00]
Goto et al 2019 (Goto1 TOR){Goto 2018, 240}	0.11 [0.11, 0.11]	1.00 [0.99, 1.00]
Haukoos et al 2004 (Haukoos1 TOR){Haukoos 2004 145}	0.68 [0.64, 0.71]	0.92 [0.78, 0.98]
Lee et al 2019 (KOCARC1 TOR){Lee 2019 e134}	0.31 [0.29, 0.32]	0.97 [0.96, 0.99]
Lee et al 2019 (KOCARC2 TOR){Lee 2019 e134}	0.32 [0.31, 0.34]	0.98 [0.96, 0.99]

Marsden et al 1995 (Marsden TOR) {Marsden 1995 49}	0.58 [0.53, 0.63]	1.00 [0.03, 1.00]
Morrison et al 2007 (ALS TOR)[66]{Morrison 2007 266}	0.51 [0.50, 0.53]	1.00 [0.98, 1.00]
Petrie et al 2001 (Petrie TOR){Petrie 2001 186}	0.39 [0.38, 0.40]	0.98 [0.97, 0.99]
SOS-Kanto 2017 (SOS_Kanto1 TOR){SOS-Kanto 2017 345}	0.50 [0.49, 0.50]	0.95 [0.93, 0.96]
Verbeek et al 2002 (BLS TOR){Verbeek 2002 671}	0.65 [0.62, 0.69]	1.00 [0.75, 1.00]
Yoon et al 2019 (KoCARC1 TOR){Yoon 2019, 73}	0.53 [0.51, 0.54]	0.92 [0.89, 0.94]
Yoon et al 2019(KoCARC2 TOR){Yoon 2019, 73}	0.53 [0.51, 0.54]	0.89 [0.86, 0.91]
Yoon et al 2019(KoCARC3 TOR){Yoon 2019, 73}	0.39 [0.38, 0.41]	0.95 [0.93, 0.97]
TOR indicates termination of resuscitation. [95%CI] – 95% confidence interval		

[H5] Studies Reporting External Validation of a TOR Rule to Predict Death in Hospital

We identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, indirectness, and imprecision) from 24 nonrandomized studies.[49,50,52-55,57-59,61-67,69-71,74,75,77-79]

Between them these studies externally validated 14 distinct TOR rules to predict death after arrival at hospital. There was considerable heterogeneity across TOR variables, patient populations, clinician populations, and EMS systems; thus, meta-analysis was not appropriate. However, performance of 3 TOR rules (BLS TOR rule, ALS TOR rule, universal TOR rule) was reported in multiple papers (see below). Reported sensitivities and specificities of included papers are listed in Table 2.

Table 2. Sensitivity and Specificity of External Validation Studies (Death)

Author (TOR rule)	Sensitivity [95% CI]	Specificity [95% CI]
Cheong et al, 2016 (BLS TOR)[49]	0.66 [0.64, 0.68]	0.93 [0.85, 0.98]
Cheong et al, 2016 (ALS TOR)[49]	0.28 [0.26, 0.30]	0.99 [0.93, 1.00]
Chiang et al, 2016 (BLS TOR)[51]	0.64 [0.62, 0.66]	0.74 [0.67, 0.80]
Chiang et al, 2016 (ALS TOR)[51]	0.58 [0.56, 0.59]	0.76 [0.69, 0.81]
Cone et al, 2005 (NAEMSP TOR)[52]	0.58 [0.54, 0.63]	1.00 [0.74, 1.00]
Diskin et al, 2014 (ALS TOR)[53]	0.27 [0.21, 0.32]	1.00 [0.91, 1.00]
Drennan et al, 2014 (uTOR)[54]	0.43 [0.42, 0.45]	0.89 [0.83, 0.94]
Fukada et al, 2014 (BLS TOR)[55]	0.70 [0.62, 0.78]	0.83 [0.36, 1.00]
Fukada et al, 2014 (ALS TOR)[55]	0.19 [0.08, 0.35]	1.00 [0.40, 1.00]
Goto et al, 2019 (BLS TOR)[57]	0.91 [0.91, 0.91]	0.62 [0.60, 0.63]
Grunau et al, 2017 (Shib 1 TOR)[58]	0.72 [0.71, 0.73]	0.91 [0.89, 0.93]
Grunau et al 2019 (Shib 1 TOR)[47,59]	0.90 [0.89, 0.91]	1.00 [1.00, 1.00]
Jordan et al, 2017 (uTOR)[61]	0.24 [0.16, 0.34]	1.00 [0.83, 1.00]
Kajinno et al, 2013 (BLS TOR)[62]	0.79 [0.79, 0.79]	0.88 [0.87, 0.88]
Kajinno et al, 2013 (ALS TOR)[62]	0.31 [0.30, 0.31]	0.92 [0.92, 0.93]
Kashiura et al, 2016 (BLS TOR)[63]	0.82 [0.81, 0.83]	0.92 [0.88, 0.94]
Kashiura et al, 2016 (ALS TOR)[63]	0.29 [0.28, 0.30]	0.91 [0.87, 0.95]
Kim et al, 2015 (BLS TOR)[64]	0.74 [0.72, 0.75]	0.70 [0.65, 0.74]
Lee et al, 2019 (BLS TOR)[65]	0.72 [0.70, 0.73]	0.78 [0.74, 0.81]
Lee et al, 2019 (ALS TOR)[65]	0.21 [0.20, 0.23]	0.97 [0.95, 0.98]
Lee et al, 2019 (Goto 1 TOR)[65]	0.39 [0.37, 0.40]	0.95 [0.93, 0.97]
Lee et al, 2019 (SOS-Kanto 1 TOR)[65]	0.27 [0.26, 0.28]	0.98 [0.97, 0.99]
Morrison et al, 2007 (BLS TOR)[66]	0.51 [0.50, 0.53]	1.00 [0.98, 1.00]
Morrison et al, 2009 (ALS TOR)[67]	0.33 [0.31, 0.35]	1.00 [0.97, 1.00]
Morrison et al, 2009 (uTOR)[67]	0.57 [0.55, 0.60]	1.00 [0.97, 1.00]
Ong et al, 2006 (BLS TOR)[69]	0.53 [0.52, 0.54]	1.00 [0.99, 1.00]
Ong et al, 2006 (Marsden TOR)[69]	0.19 [0.19, 0.20]	1.00 [0.99, 1.00]
Ong et al, 2006 (Petrie TOR)[69]	0.10 [0.09, 0.10]	1.00 [0.99, 1.00]
Ong et al, 2007 (BLS TOR)[70]	0.69 [0.67, 0.71]	0.81 [0.64, 0.93]
Ong et al, 2007 (Marsden TOR)[70]	0.65 [0.63, 0.67]	0.91 [0.75, 0.98]
Ong et al, 2007 (Petrie TOR)[70]	0.32 [0.30, 0.34]	0.94 [0.79, 0.99]

Met opmerkingen [GR6]: Change to 50

Met opmerkingen [GR7]: Change to 50

Sasson et al, 2008 (BLS TOR)[71]	0.51 [0.49, 0.52]	0.99 [0.97, 1.00]
Sasson et al, 2008 (ALS TOR)[71]	0.23 [0.22, 0.24]	1.00 [0.99, 1.00]
Skrifvars et al, 2010 (ALS TOR)[74]	0.27 [0.26, 0.27]	0.99 [0.97, 1.00]
Skrifvars et al, 2010 (ERC TOR)[74]	0.94 [0.94, 0.95]	0.95 [0.91, 0.97]
Skrifvars et al, 2010 (Helsinki TOR)[74]	0.55 [0.54, 0.56]	0.74 [0.68, 0.80]
SOS-Kanto 2017 (BLS TOR)[75]	0.78 [0.77, 0.79]	0.89 [0.86, 0.91]
SOS-Kanto 2017 (Goto 2 TOR)[75]	0.50 [0.49, 0.51]	0.95 [0.93, 0.96]
SOS-Kanto 2017 (SOS-Kanto 2)[75]	0.44 [0.43, 0.45]	0.97 [0.96, 0.98]
SOS-Kanto 2017 (SOS-Kanto 3)[75]	0.41 [0.40, 0.42]	0.99 [0.97, 0.99]
Verhaert et al, 2016 (ALS TOR)[77]	0.07 [0.05, 0.10]	1.00 [0.96, 1.00]
Yates et al, 2018 (uTOR)[78]	0.34 [0.27, 0.41]	0.17 [0.04, 0.41]
Yoon et al, 2019 (uTOR)[79]	0.70 [0.69, 0.72]	0.81 [0.77, 0.84]

ALS indicates advanced life support; BLS, basic life support; ERC, European Resuscitation Council; uTOR, universal termination of resuscitation; and TOR, termination of resuscitation.

We identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, indirectness, and imprecision) from 13 nonrandomized studies[49,50,55,57,62-65,67,69-71,75,79] reporting the accuracy of the BLS TOR rule to predict in-hospital death. There was considerable heterogeneity across patient populations, clinician populations, and EMS systems; thus, meta-analysis was not appropriate. We calculated estimates of effect per 1000 patients based on the range of sensitivities, specificities, and prevalences in the studies (Table 2).

On the basis of the lowest prevalence of 88.3%,[65] the estimate of false positives (TOR rule predicts death, but patient will survive) per 1000 patients tested ranged from 0 to 36. On the basis of the highest prevalence of 98.6%,[70] the estimate of false positives per 1000 patients tested ranged from 0 to 4.

We identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, indirectness, and imprecision) from 11 nonrandomized studies[49,50,53,55,62,63,65,67,71,77] reporting the accuracy of the ALS TOR rule to predict in-hospital death. There was considerable heterogeneity across patient populations, clinician populations, and EMS systems; thus, meta-analysis was not appropriate. We calculated estimates of effect per 1000 patients based on the range of sensitivities, specificities, and prevalences in the studies (Table 2).

Met opmerkingen [GR8]: One study removed {Yoon 2019 73}(ref79) as measures uTOR not BLS TOR

Met opmerkingen [GR9]: Delete here 79

Met opmerkingen [GR10]: One study added {Skirfvars 2010 679} (ref no 74) as incorrectly recorded as uTOR

On the basis of the lowest prevalence of 84.9%,[77] the estimate of false positives (TOR rule predicts death, but patient will survive) per 1000 patients tested ranged from 0 to 36. On the basis of the highest prevalence of 99.0%,[49] the estimate of false positives (TOR rule predicts death, but patient will survive) per 1000 patients tested ranged from 0 to 3.

Met opmerkingen [GR11]: Delete 49 and replace with {Skrifvars 2010 679} ref nr 74

We identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, indirectness, and imprecision) from 6 nonrandomized studies[54,58,61,67,74,78] reporting the accuracy of the universal TOR rule to predict in-hospital death. There was considerable heterogeneity across patient populations, clinician populations, and EMS systems; thus, meta-analysis was not appropriate. We calculated estimates of effect per 1000 patients based on the range of sensitivities, specificities, and prevalences in the studies (Table 2). On the basis of the lowest prevalence of 82.0%,[61] the estimate of false positives (TOR rule predicts death, but patient will survive) per 1000 patients tested ranged from 0 to 149. On the basis of the highest prevalence of 97.6 %,[74] the estimate of false positives (TOR rule predicts death, but patient will survive) per 1000 patients tested ranged from 0 to 9.

Met opmerkingen [GR12]: Ref 74 Skrifvars removed as incorrect
Ref 79 added {Yoon 2019 73} as correct

Met opmerkingen [GR13]: Incorrect reference removed (Skrifvars 74) replaced with correct reference {Drennan 2014 1488}

[H5] Studies Reporting Clinical Validation of a TOR Rule to Predict Death in Hospital

We identified very-low-certainty evidence (downgraded for indirectness) from 1 nonrandomized study[68] reporting a clinical validation of the universal TOR rule to predict in-hospital death. Sensitivity was 0.64 (95% CI, 0.61–0.68), and specificity was 1.00 (95% CI, 0.92–1.00). Of 954 patients enrolled, the BLS TOR rule recommended transport in 367 cases. Of these, 44 survived to discharge and 323 died in hospital. Of the remaining 586, 388 had resuscitation terminated in the field. Of 198 cases transported to hospital despite termination being recommended, no patient survived.

[H4] For the Critically Important Outcome of Prediction of Poor Neurologic Outcome

a) Studies reporting the derivation and internal validation of a TOR rule to predict poor neurologic outcome
b) Studies reporting external validation of a TOR rule to predict poor neurologic outcome
c) Studies reporting clinical validation of a TOR rule to predict poor neurologic outcome

H5] Studies Reporting the Derivation and Internal Validation of a TOR Rule to Predict Poor Neurologic Outcome

We identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, indirectness, and imprecision) from 6 nonrandomized studies[57,60,65,73,79] Between them these studies derived and internally validated 12 distinct TOR rules to predict poor neurologic outcome. Studies by Haukoos et al {Haukoos 2004 145}, Lee et al {Lee 2019 e134}, Shibahashi et al {Shibahashi 2018 28} and Yoon et al {Yoon 2019 73} derived multiple TOR rules. There was considerable heterogeneity in patient population, clinician population, and EMS system design; thus, meta-analysis was not appropriate. Reported sensitivities and specificities of included papers are listed in Table 3.

Met opmerkingen [GR14]: Add in {Glober 2019 8}

Table 3. Sensitivity and Specificity of Derivation and Internal Validation Studies (Poor Neurologic Outcome)

Author (TOR rule)	Sensitivity [95% CI]	Specificity [95% CI]
Glober et al, 2019 (Glob 2 TOR)[56]	0.19 [0.17, 0.21]	1.00 [0.98, 1.00]
Goto et al, 2019 (Goto 1 TOR)[57]	0.11 [0.10, 0.11]	1.00 [1.00, 1.00]
Haukoos et al, 2004 (Haukoos 2 TOR)[60]	0.57 [0.54, 0.61]	1.00 [0.79, 1.00]
Haukoos et al, 2004 (Haukoos 3 TOR)[60]	0.69 [0.66, 0.72]	1.00 [0.78, 1.00]
Haukoos et al, 2004 (Haukoos 4 TOR)[60]	0.69 [0.65, 0.72]	1.00 [0.48, 1.00]
Lee et al, 2019(KOCARC 4 TOR)[65]	0.30 [0.28, 0.31]	1.00 [0.99, 1.00]
Lee et al, 2019 (KOCARC 5 TOR)[65]	0.31 [0.30, 0.33]	1.00 [0.99, 1.00]
Shibahashi et al, 2018 (Shib1 TOR)[73]	0.39 [0.38, 0.39]	0.95 [0.95, 0.96]
Shibahashi et al, 2018 (Shib2 TOR)[73]	0.59 [0.59, 0.59]	0.89 [0.88, 0.90]
Yoon et al, 2019 (KOCARC1 TOR)[79]	0.52 [0.50, 0.53]	0.99 [0.97, 1.00]
Yoon et al, 2019 (KOCARC2 TOR)[79]	0.52 [0.50, 0.53]	0.98 [0.96, 0.99]
Yoon et al, 2019 (KOCARC3 TOR)[79]	0.38 [0.37, 0.40]	1.00 [0.98, 1.00]

TOR indicates termination of resuscitation.

[H5] Studies Reporting External Validation of a TOR Rule to Predict Poor Neurologic Outcome

We identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, indirectness, and imprecision) from 9 nonrandomized studies[49,59,62-65,72,74,75,79]; externally validating 10 distinct TOR rules to predict poor neurologic outcome. There was considerable heterogeneity across TOR rule variables, patient populations, clinician populations, and EMS systems; thus, meta-analysis was not appropriate. However, performance of 2 TOR rules (BLS TOR, ALS TOR) was reported in multiple papers (see below). Reported sensitivities and specificities of included papers are listed in Table 4.

Met opmerkingen [GR15]: Remove the Grunau study ref 59

Table 4. Sensitivity and Specificity of External Validation Studies (Poor Neurologic Outcome)

Author (TOR rule)	Sensitivity [95% CI]	Specificity [95% CI]
Cheong et al, 2016 (BLS TOR)[49]	0.66 [0.64, 0.68]	1.00 [0.92, 1.00]
Cheong et al, 2016 (ALS TOR)[49]	0.27 [0.25, 0.29]	1.00 [0.92, 1.00]
Kajino et al, 2013 (BLS TOR)[62]	0.78 [0.78, 0.78]	0.97 [0.96, 0.97]
Kajino et al, 2013 (ALS TOR)[62]	0.30 [0.30, 0.30]	0.98 [0.97, 0.99]
Kashiura et al, 2016 (BLS TOR)[63]	0.81 [0.80, 0.82]	0.97 [0.94, 0.99]
Kashiura et al, 2016 (ALS TOR)[63]	0.28 [0.27, 0.29]	0.94 [0.87, 0.98]
Kim et al, 2015 (BLS TOR)[64]	0.72 [0.71, 0.73]	0.90 [0.85, 0.94]
Lee et al, 2019 (BLS TOR)[65]	0.71 [0.70, 0.72]	0.93 [0.89, 0.95]
Lee et al, 2019 (ALS TOR)[65]	0.21 [0.20, 0.22]	0.99 [0.97, 1.00]
Lee et al, 2019 (Goto 1 TOR)[65]	0.27 [0.26, 0.28]	0.98 [0.97, 0.99]
Lee et al, 2019 (SOS-Kanto 1 TOR)[65]	0.39 [0.37, 0.40]	0.95 [0.93, 0.97]
SOS-Kanto 2017 (BLS TOR)[75]	0.77 [0.76, 0.78]	0.96 [0.94, 0.98]
SOS-Kanto 2017 (ALS TOR)[75]	0.49 [0.48, 0.50]	0.98 [0.96, 0.99]
SOS-Kanto 2017 (SOS-Kanto 1)[75]	0.49 [0.48, 0.50]	0.97 [0.95, 0.99]
SOS-Kanto 2017 (SOS-Kanto 2)[75]	0.44 [0.43, 0.44]	0.99 [0.97, 1.00]
SOS-Kanto 2017 (SOS-Kanto 3)[75]	0.40 [0.39, 0.41]	0.99 [0.98, 1.00]
Ruygrok et al, 2008 (ALS TOR)[72]	0.24 [0.21, 0.27]	1.00 [0.92, 1.00]
Ruygrok et al, 2008 (uTOR)[72]	0.34 [0.31, 0.38]	1.00 [0.92, 1.00]
Ruygrok et al, 2008 (Haukoos 3 TOR)[72]	0.06 [0.04, 0.08]	1.00 [0.92, 1.00]
Skrifvars et al, 2010 (ALS TOR)[74]	0.27 [0.26, 0.27]	1.00 [0.97, 1.00]
Skrifvars et al, 2010 (ERC TOR)[74]	0.94 [0.94, 0.95]	0.96 [0.93, 0.98]
Skrifvars et al, 2010 (Helsinki TOR)[74]	0.55 [0.54, 0.56]	0.79 [0.73, 0.85]

Yoon et al, 2019 (uTOR)[79]	0.69 [0.68, 0.71]	0.94 [0.91, 0.96]

ALS indicates advanced life support; BLS, basic life support; ERC, European Resuscitation Council; uTOR, universal termination of resuscitation rule; and TOR, termination of resuscitation.

We identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, indirectness, and imprecision) from 6 nonrandomized studies[49,62-65,75,79] reporting the accuracy of the BLS TOR rule to predict poor neurologic outcome. There was considerable heterogeneity across patient populations, clinician populations, and EMS systems; thus, meta-analysis was not appropriate. We calculated estimates of effect per 1000 patients based on the range of sensitivities, specificities, and prevalences in the studies (Table 4).

Met opmerkingen [GR16]: Remove yoon study ref 79

On the basis of the lowest prevalence of 92.1%, the estimate of false positives (TOR predicts poor neurologic outcome, but patient has favorable neurologic outcome) per 1000 patients tested ranged from 0 to 6. On the basis of the highest prevalence of 98.0%, the estimate of false positives per 1000 patients tested ranged from 0 to 1.

Met opmerkingen [GR17]: Insert ref 65 {lee 2019 e134}

Met opmerkingen [GR18]: Insert ref 49 [Cheong 2016 623}

We identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, indirectness, and imprecision) from 6 nonrandomized studies[49,62,63,65,72] reporting the accuracy of the ALS TOR rule to predict poor neurologic outcome. There was considerable heterogeneity across patient populations, clinician populations, and EMS systems; thus, meta-analysis was not appropriate. We calculated estimates of effect per 1000 patients based on the range of sensitivities, specificities, and prevalences in the studies.

Met opmerkingen [GR19]: Add ref 74 {Skrifvars 2010 679}

On the basis of the lowest prevalence of 92.1%, the estimate of false positives (TOR rule predicts poor neurologic outcome, but patient has favorable neurologic outcome) per 1000 patients tested ranged from 0 to 6. On the basis of the highest prevalence of 98.0%, the estimate of false positives per 1000 patients tested ranged from 0 to 1.

Met opmerkingen [GR20]: Add ref 65 {Lee 2019 e134}

Met opmerkingen [GR21]: Add ref 49 {Cheong 2016 623}

[H5] Studies Reporting Clinical Validation of a TOR to Predict Poor Neurologic Outcome

We identified very-low-certainty evidence (downgraded for indirectness) from 1 nonrandomized study[68] reporting a clinical validation of the universal TOR rule to predict poor neurologic outcome. Sensitivity was 0.63 (95% CI, 0.61–0.68), and specificity was 1.00 (95% CI, 0.92–1.00). Of 953 patients included, the BLS TOR rule recommended transport in 367 cases. Of these, 17 survived with poor neurologic outcome (Cerebral Performance Category 3 or 4) and 323 died in hospital.

[H3]Treatment Recommendations

We conditionally recommend the use of TOR rules to assist clinicians in deciding whether to discontinue resuscitation efforts out of hospital or to transport to hospital with ongoing CPR (conditional recommendation/very-low-certainty evidence).

[H3]Justification and Evidence-to-Decision Framework Highlights

The evidence-to-decision table is included in Appendix A-1. The majority of studies describe either the derivation and internal validation of individual TOR rules or the external validation of previously published TOR rules. We identified only 1 study addressing clinical validation (the use of a TOR rule in clinical practice) of a TOR rule by emergency medical technicians with defibrillators. Robust evidence to support the widespread implementation of TOR rules in clinical practice is therefore weak. Despite several studies reporting a specificity of 1.0, the task force acknowledges that implementation of a TOR rule, in isolation, may result in missed survivors.

The task force recognizes that TOR is common practice in many EMS systems. We support the principle of discontinuing resuscitation when treatment is futile because it preserves the dignity of the recently deceased, reduces risk for EMS providers, and protects scarce healthcare resources. However, the task force also acknowledges that identification of futile cases is challenging and is often informed by both clinical guidelines and clinician insight.

The task force advocates the adoption of TOR guidelines that take into account the patients' prior wishes and/or expectations, consideration of patient pre-existing comorbidities, and quality of life both before and after the cardiac arrest event. Such TOR guidelines may be informed by the inclusion of an evidence-based TOR rule; however, the task force believes a TOR rule should not be the sole determinant of when to discontinue resuscitation.

In those EMS systems that do implement prehospital TOR, the EMS system must ensure that there is no conflict with legislation prohibiting nonphysicians from discontinuing resuscitation and have appropriate governance arrangements to monitor practice. Where an evidence-based TOR rule is included to inform practice, the EMS system should consider the training needs of EMS crews in communicating bad news and supporting the relatives of the recently deceased, in addition to consideration of the generalizability of the chosen TOR rule to its healthcare system. In some healthcare systems, it may be appropriate for EMS systems to communicate with organ donation teams before implementing change.

The task force acknowledges that prehospital TOR may not be feasible in some instances. In some locations, the legal infrastructure may require EMS clinicians to provide resuscitation in all but a very few circumstances (eg, in the presence of rigor mortis). In other areas, it may not be culturally acceptable for nonphysicians to make a clinical decision to stop resuscitation in the prehospital environment. Where this is the case, or where clinical governance arrangements are insufficient to monitor practice, we suggest transport to hospital with ongoing CPR may be preferable.

The 2010 CoSTR recommended validated TOR in adults,[1,2] but the topic was not addressed in 2015. This 2020 CoSTR for EIT softens the recommendation, taking into consideration the social acceptability of excluding potential survivors from in-hospital treatment and the very limited clinical validation of such rules.

[H3] Knowledge Gaps

There is little evidence addressing use of TOR rules in clinical practice. Studies are required to address the following:

- Use of TOR rules in actual clinical practice
- Compliance with out-of-hospital TOR rules
- Implementation strategies of TOR for EMS that are based on evidence
- Health economic implications of TOR implementation
- Societal perceptions and acceptance of TOR rules
- TOR rules specific for children
- Impact of TOR rules on non–heart-beating organ donation

[H2] In-Hospital TOR (EIT 4002: SysRev)

[H3] Rationale for Review

There are no current ILCOR recommendations on clinical decision rules to terminate resuscitation during in-hospital cardiac arrest (IHCA). Almost half of all in-hospital resuscitation attempts are terminated without ROSC. Knowing when to terminate resuscitation is, therefore, an important clinical question. The EIT Task Force defined *clinical decision rules* as cardiac arrest characteristics to be applied during resuscitation to predict survival (ROSC, survival to

Met opmerkingen [JF22]: ADD REFERENCE: Resuscitation Council (UK) and Intensive Care National Audit & Research Centre (ICNARC). Key statistics from the National Cardiac Arrest Audit 2017/18 2019 [Available from: file:///C:/Users/169614A/Downloads/Key Statistics from NCAA 2017-18 (1).pdf.

hospital discharge) and thereby terminate resuscitation if deemed futile. Measures of prediction were negative predictive value, sensitivity, specificity, and positive predictive value.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Adults and children with IHCA

Intervention: Use of any clinical decision rule

Comparator: No clinical decision rule

Outcome: No ROSC, death before hospital discharge, survival with unfavorable neurologic outcome, and death within 30 days

Study design: RCTs and nonrandomized studies (non-RCTs, interrupted time series, controlled before-and-after studies, cohort studies) were eligible for inclusion. Unpublished studies (eg, conference abstracts, trial protocols), animal studies, simulation studies, and studies not in English were excluded.

Time frame: All years until November 11, 2019

[H3] Consensus on Science

We found 3 studies investigating the usability of the UN10 rule to predict survival to hospital discharge on the basis of the unwitnessed arrest, a nonshockable rhythm, and 10 minutes of CPR without ROSC.[80-82] All studies were cohort studies, and no studies used randomization or prospective implementation of a clinical decision rule.

For the critical outcomes of positive predictive value and sensitivity in predicting death before hospital discharge for adults with IHCA, we identified very-low-certainty evidence from 3 historical cohort studies.[80-82] investigating the UN10 rule (downgraded for risk of bias, indirectness, imprecision, and inconsistency). Because of clinical heterogeneity in study cohorts, no meta-analysis was conducted. Positive predictive values and sensitivities are reported in Table 5.

For the important outcomes of specificity and negative predictive value in predicting death before hospital discharge for adults with IHCA, we identified very-low-certainty evidence from 3 historical cohort studies.[80-82] investigating the UN10 rule (downgraded for risk of bias, indirectness, imprecision, and inconsistency). Specificities and negative predictive values are reported in Table 5.

Table 5. Positive Predictive Values, Specificity, Sensitivity, and Negative Predictive Values for Prediction of Death Before Hospital Discharge

	Positive Predictive Value	Specificity	Sensitivity	Negative Predictive Value
Van Walraven, 1999[80]	100% (95% CI, 97.1%–100%)	100% (95% CI, 97.1%–100%)	12.2% (95% CI, 10.3%–14.4%)	10.8% (95% CI, 8.9–12.8%)
Van Walraven, 2001[81]	98.9% (95% CI, 96.5%–99.7%)	99.1% (95% CI, 97.1%–99.8%)	14.4% (95% CI, 12.4%–16.0%)	17.0% (95% CI, 15.3–18.7)
Petek, 2019[82]	93.7% (95% CI, 93.3%–94.0%)	94.6% (95% CI, 94.3%–94.9%)	19.1% (95% CI, 18.8%–19.3%)	22.0% (95% CI, 21.9%–22.0%)

For the important outcomes of positive predictive value, specificity, sensitivity, and negative predictive values in predicting survival to hospital discharge with unfavorable neurologic outcome for adults with IHCA, we identified very-low-certainty evidence from 1 observational study[82] investigating the UN10 rule (downgraded for risk of bias, indirectness, and imprecision). The study reported a positive predictive value of 95.2% (95% CI, 94.9%–95.6%), a specificity of 95.3% (95% CI, 95.0%–95.6%), a sensitivity of 18.8% (95% CI, 18.5%–19.0%), and a negative predictive value of 19.1% (95% CI, 18.8%–19.3%).[82]

We identified no studies predicting no ROSC or death within 30 days. We identified no studies on children with IHCA.

[H3] Treatment Recommendations

We did not identify any clinical decision rule that was able to reliably predict death after IHCA. We recommend against using the UN10 rule as a sole strategy to terminate in-hospital resuscitation (strong recommendation, very-low-certainty evidence).

[H3] Justification and Evidence-to-Decision Framework Highlights

The evidence-to-decision table is included in Appendix A-2. In making this recommendation, the EIT Task Force considered the following: several other scores have been developed that aim at predicting the chance of surviving on the basis of prearrest factors only, including the GO-FAR score[83] and comorbidity scores.[84] While these scores may be suitable to trigger do-not-resuscitate discussions, they are not aimed at deciding when to terminate resuscitation during a resuscitation attempt and were therefore not included in this review.

The Resuscitation Predictor Scoring Scale[85] aimed to identify patients with low likelihood of surviving a cardiac arrest after 15 minutes of resuscitation. This score was not included in the review because the score aimed at identifying patients with low likelihood but not patients with no likelihood of surviving the cardiac arrest.

Several studies (primarily prehospital) have looked at other factors such as end-tidal carbon dioxide (CO_2) and echocardiographic findings to terminate resuscitation. These have been included in reviews by the ILCOR ALS Task Force. End-tidal carbon dioxide and echocardiographic findings may be considered together with other factors to decide when to terminate in-hospital resuscitation.

All identified studies were based on historical cohorts and carry a risk of a self-fulfilling prophecy bias as clinicians may have terminated resuscitation on patients who potentially had a chance of surviving in the observed studies. Prospective studies are needed to reliably assess the effect of such clinical decision rules.

Two of the studies[80,81] included patients resuscitated in the 1980s and 1990s, when resuscitation practices differed from present time and when reported survival rates were lower than now.[86] The third study[82] included patients resuscitated between 2000 and 2016, but a large proportion of the arrests occurred before 2010. As previously stated, survival rates are now higher than in previous decades.

The task force prioritized a perfect positive predictive value (no survivors predicted to be dead) for any clinical prediction rule because of the risk of terminating resuscitation of a patient who could have survived. The task force discussed that it is reasonable not to terminate resuscitation as long as the patient has a shockable rhythm. No single clinical factor or no single decision rule has been identified as sufficient to terminate resuscitation. Therefore, the EIT Task Force members suggested that a decision to terminate an IHCA resuscitation should continue to be based on a combination of factors that are known to be associated with a low chance of survival, eg, end-tidal carbon dioxide, cardiac standstill on echocardiography, duration of resuscitation, patient age, and patient comorbidities.

ILCOR has not previously made a treatment recommendation on an in-hospital TOR rule. Unfortunately, the existing evidence is insufficient to recommend an in-hospital TOR rule. Clinicians have to rely on clinical examination, their experience, and the patient's conditions and wishes to inform their decision to terminate resuscitation efforts.

[H3] Knowledge Gaps

- There are no clinical decision tools to predict the absence of ROSC during in-hospital resuscitation.
- There are clinical decision tools that combine existing decision tool elements such as resuscitation duration and cardiac arrest rhythm with end-tidal carbon dioxide and/or findings on cardiac ultrasound.
- No studies were found on the use of a clinical decision tool to terminate resuscitation for pediatric IHCA.
- There is a lack of prospective clinical validation studies and randomized trials investigating the use of a clinical decision tool to terminate resuscitation during IHCA.
- It is unknown how the use of a clinical decision tool affects resuscitation practices, cost benefit, or how it affects survival outcomes.

[H2] Deliberate Practice and Mastery Learning (EIT 4004: EvUp)

One EvUp (Appendix C-1) identified several studies that suggest the need for consideration of a SysRev, especially because no former assessment of this educational strategy has been done by ILCOR and no treatment recommendation has been made as of January 31, 2020.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Students/healthcare providers taking BLS or ALS training

Intervention: Use of deliberate practice and/or mastery learning

Comparator: No such teaching strategies

Outcome: Improve knowledge/skill performance at course conclusion, knowledge/skill retention beyond course conclusion, clinical performance in actual resuscitations, or patient outcomes (critically important); intact neurologic outcome (critically important)

Study design: Cross-sectional or cohort studies were eligible for inclusion. Unpublished studies (eg, conference abstracts, trial protocols) were excluded.

Time frame: All articles published before 2013 were excluded, and all languages were included if there was an English abstract. The search was completed on October 22, 2019.

An EvUp was conducted for 2020 by the American Heart Association (AHA). A search conducted in PubMed yielded 30 studies, and 12 were identified as relevant. See the complete EvUp in Appendix C-1.

[H3]Treatment Recommendation

The EvUp did not enable a treatment recommendation to be made.

[H2] Layperson Training (EIT 4009: EvUp)

An EvUp was performed (Appendix C-2) and identified several studies suggesting the need to consider a SysRev. To date, no SysRev on the training of laypeople has been done by ILCOR, and no treatment recommendation has been made as of January 31, 2020.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Laypeople (nonprofessional responders)

Intervention: Participating in CPR training

Comparator: Compared with no training

Outcome: Change willingness to perform CPR in actual resuscitations, skill performance quality, and/or patient outcomes

Study design: Cross-sectional or cohort studies are eligible for inclusion. Unpublished studies (eg, conference abstracts, trial protocols) were excluded.

Time frame: All articles published between January 1, 2018, and October 10, 2019, and all languages were included if there was an English abstract.

An EvUp was undertaken by the AHA. A search conducted in PubMed yielded 372 studies, and 25 were identified as relevant. See Appendix C-2 for the full EvUp.

[H3]Treatment Recommendation

The EvUp did not enable a treatment recommendation to be made.

[H2] Timing for Retraining (EIT 628: EvUp)

The topic of timing for retraining was last reviewed in 2015. An EvUp was performed (Appendix C-3) with several studies identified that suggest the need for consideration of a SysRev. The 2015 treatment recommendation[3,4] will then be reevaluated.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Students who are taking BLS courses

Intervention: Any specific interval for update or retraining

Comparator: Compared with standard practice (ie, 12 or 24 monthly)

Outcome: Improve patient outcomes, skill performance in actual resuscitations, skill performance at 1 year, skill performance at course conclusion, and cognitive knowledge

Study design: Cross-sectional or cohort studies are eligible for inclusion. Unpublished studies (eg, conference abstracts, trial protocols) were excluded.

Time frame: All articles published between January 1, 2014, and January 7, 2020, and all languages were included if there was an English abstract

An EvUp was conducted for 2020 by the RCA. A search conducted in PubMed and Embase yielded 1002 studies, and 5 were identified as relevant. See Appendix C-3 for the complete EvUp.

[H3]Treatment Recommendation

The treatment recommendation from 2010 is unchanged.[1,2] There is insufficient evidence to recommend the optimum interval or method for BLS retraining for laypeople. Because there is evidence of skills decay within 3 to 12 months after BLS training and evidence that frequent training improves CPR skills, responder confidence, and willingness to perform CPR, we suggest that individuals likely to encounter cardiac arrest consider more frequent retraining (weak recommendation, very-low-quality evidence).

[H1] Measuring Implementation/Performance in Communities, Cardiac Arrest Centers

[H2] System Performance Improvements (EIT 640: SysRev)

[H3] Rationale for Review

The task force considered improvements at the system level of health care that would have the greatest potential to increase the survival rate after cardiac arrest. Studies associated with system performance improvement for personnel in organizations or systems caring for patients with cardiac arrest were included. *System performance improvement* was defined as hospital-level, community-level, or country-level improvement related to structure, care pathways, process, and quality of care.

[H3] Population, Intervention, Comparator, and Outcome

Population: Resuscitation systems who are caring for patients in cardiac arrest in any setting

Intervention: System performance improvements

Comparator: Compared with no system performance improvements

Outcome: Survival with favorable neurologic outcome at discharge, survival to hospital discharge, skill performance in actual resuscitations, survival to admission, and system-level improvement

Study Designs: RCTs and nonrandomized studies (non-RCTs, interrupted time series, controlled before-and-after studies, cohort studies, case-control studies). All years and all languages were included as long as there was an English abstract associated with system performance improvement for personnel in organizations or systems caring for patients with cardiac arrest. *System performance improvement* is defined as hospital-level, community-level, or country-level improvement related to structure, care pathways, process, and quality of care.

Exclusion: Unpublished studies (eg, conference abstracts, trial protocols), letters, editorials, comments, and case reports.

Time Frame: The new search included studies from November 1, 2013 to November 14, 2019. The studies included in the 2015 SysRev were reviewed against the new inclusion/exclusion criteria and included where appropriate.

[H3] Consensus on Science

The interventions among the studies are summarized in Table 6. For the critical outcome of survival with favorable neurologic outcome at discharge, we identified moderate-certainty evidence from 1 cluster-randomized trial[87] (downgraded for imprecision) and very-low-certainty evidence from 18 non-RCTs[88-105] (downgraded for risk of bias). Among these studies, different interventions for system performance improvement were implemented in different contexts (IHCA versus OHCA); the heterogeneity of the studies precludes any meta-analysis. Thirteen of these studies[88-93,95,96,98,99,101,102,104] showed that patients had significantly higher chance of survival with favorable neurologic outcome at discharge after interventions for system performance improvement were implemented. The other 6 studies,[87,94,97,100,103,105] including 1 cluster-randomized trial,[87] showed no significant improvement after interventions were implemented.

For the critical outcome of survival to hospital discharge, we identified moderate-certainty evidence from 1 cluster-randomized trial[87] (downgraded for imprecision) and very-low-certainty evidence from 21 non-RCTs[88-108] (downgraded for risk of bias). The heterogeneity of the studies precludes any meta-analysis. Fourteen of these studies[88-90,92,93,95,96,98-102,104,107] showed that patients had significantly higher chance of survival to hospital discharge after interventions for system performance improvement were implemented. The other 8 studies,[87,91,94,97,103,105,106,108] including 1 cluster-randomized trial,[87] showed no significant improvement after interventions were implemented.

For the important outcome of skill performance in actual resuscitations, we identified moderate-certainty evidence from 1 cluster-randomized trial[87] (downgraded for risk of bias) and very-low-certainty evidence from 13 non-RCTs[89,95-97,100,102,105,106,108-112] (downgraded for risk of bias). The heterogeneity of the studies precludes any meta-analysis. The interventions of these studies all consisted of strategies to improve the quality of resuscitation, including skills of BLS and ALS. Twelve of these studies,[87,89,95,96,100,102,105,106,108-110,112] including 1 cluster-randomized trial,[87] reported that rescuers had significantly improved skill performance in actual resuscitations after interventions were implemented. The other 2 studies[97,111] showed no significant improvement after interventions were implemented.

For the important outcome of survival to admission, we identified moderate-certainty evidence, from 1 cluster-randomized trial[87] (downgraded for imprecision) and very-low-certainty evidence from 5 non-RCTs[90,91,94,101,107] (downgraded for risk of bias). The heterogeneity of the studies precludes any meta-analysis. Three of these studies[90,101,107] showed that patients had significantly higher chance of survival to admission after interventions for system performance improvement were implemented. The other 3 studies,[87,91,94] including 1 cluster-randomized trial,[87] showed no significant improvement after interventions were implemented.

For the important outcome of system-level improvement, we identified very-low-certainty evidence (downgraded for risk of bias) from 11 non-RCTs.[88,89,91-94,101-103,107,113] The heterogeneity of the studies precludes any meta-analysis. All studies included individual interventions to improve specific system-level variables, and all studies achieved all or partial goals. These system-level variables included rate of bystander CPR or use of AEDs, rate of prehospital or in-hospital therapeutic hypothermia, and the use of automatic CPR devices and CPR feedback devices.

Table 6. Interventions Among Included Studies

Study	Interventions
Hostler, 2011[87] (RCT) (OHCA)	Real-time audiovisual feedback on CPR provided by the monitor-defibrillator among EMS from 3 sites within the Resuscitation Outcomes Consortium in the United States (King County, Washington; Pittsburgh; and Westmoreland County, Pennsylvania) and Canada (Thunder Bay, Ontario)
Adabag, 2017[113] (OHCA)	Minnesota Resuscitation Consortium, a statewide integrated resuscitation program, established in 2011, to provide standardized, evidence-based resuscitation and postresuscitation care
Anderson, 2016[99] (IHCA)	Assess the hospital process composite performance score for IHCA using 5 guideline-recommended process measures
Bradley, 2012 [105] (IHCA)	Get With The Guidelines-Resuscitation (formerly known as the *National Registry of CPR*), a data registry and quality improvement program for IHCA supported by the AHA
Couper, 2015[97] (IHCA)	Phase 1: Quality of CPR and patient outcomes were measured with no intervention implemented Phase 2:

Study	Interventions
	1. Hospital 1: staff received real-time audiovisual feedback 2. Hospital 2: staff received real-time audiovisual feedback supplemented by post-event debriefing 3. Hospital 3: no intervention was implemented
Davis, 2015[88] (IHCA)	Advanced resuscitation training program implementation since Spring 2007
Del Rios, 2019[101] (OHCA)	System-wide initiatives in Chicago since 2013, including telephone-assisted and community CPR training programs; high-performance CPR and team-based simulation training; new postresuscitation care and destination protocols; and case review for EMS providers
Edelson, 2008[108] (IHCA)	Resuscitation with actual performance-integrated debriefing: weekly debriefing sessions of the prior week's resuscitations, between March 2006 and February 2007, reviewing CPR performance transcripts obtained from a CPR-sensing and feedback-enabled defibrillator
Ewy, 2013[104] (OHCA)	Continuous quality improvement, instituted cardiocerebral resuscitation in community and EMS. Community: prompt recognition and activation, CO-CPR, teaching and advocating CO-CPR, CO-CPR for healthcare providers, DA-CPR. EMS: endotracheal intubation delayed, passive ventilations, epinephrine administration
Grunau, 2018[102] (OHCA)	British Columbia OHCA quality improvement strategy, since 2005
Hopkins, 2016[94] (OHCA)	System-wide restructuring high-quality CPR program (CPR Quality Improvement Initiatives, Simplified Medication Algorithm Adopted, EMS Crew Team Training) from the Salt Lake City Fire Department in September 2011
Hubner, 2017[95] (OHCA)	Postresuscitation feedback protocol (implemented on August 1, 2013)
Hunt, 2018[110] (IHCA)	Study of the quality of chest compressions delivered to children during a 3-year period simultaneous with development and implementation of a resuscitation-quality bundle (evolved into the CODE ACES2)
Hwang, 2017[89] (OHCA)	System-wide CPR program in 2011, including DA-CPR protocol, medical control for regional EMS, provision of high-quality ACLS with capnography and extracorporeal CPR, and the standard post–cardiac arrest care protocol
Kim, 2017[92] (OHCA)	Phase 1 (2009–2011): after implementing 3 programs (national OHCA registry, obligatory CPR education, and public report of OHCA outcomes) Phase 2 (2012–2015): after implementing 2 programs (telephone-assisted CPR and EMS quality assurance program)
Knight, 2014[100] (IHCA)	Code team members were introduced to Composite Resuscitation Team Training and continued training throughout the intervention period (January 1, 2010–June 30, 2011)
Lyon, 2012[112] (OHCA)	Resuscitation symposium, collecting transthoracic impedance data via telemetry from ambulance service defibrillators, postresuscitation feedback, and monthly resuscitation training

Study	Interventions
Nehme, 2015[107] (OHCA)	Surveillance in the Australian Southeastern state of Victoria for patients with OHCA of presumed cardiac pathogenesis, with CPR awareness program, telephone-assisted CPR instruction, and prehospital hypothermia
Olasveengen, 2007[111] (OHCA)	Providing CPR performance evaluation
Park, 2018[93] (OHCA)	Implementation of 3 new CPR programs in Seoul Metropolitan City in January 2015: 1. A high-quality DA-CPR program 2. A multitier response program using fire engines or BLS vehicles 3. A feedback CPR program with professional recording and feedback of CPR process
Pearson, 2016[90] (OHCA)	Implementation of team-focused CPR; widespread incorporation began in 2011 with an optional statewide protocol introduced in July 2012
Spitzer, 2019[106] (IHCA)	"Pit crew" model for IHCA resuscitation, including ACLS training and mock code events
Sporer, 2017[91] (OHCA)	Specific implementation of specific therapies focused on perfusion during CPR and cerebral recovery after ROSC (mechanical adjuncts and protective post-resuscitation care with in-hospital therapeutic hypothermia)
Stub, 2015[98] (OHCA)	Assess composite performance score with 5 selected individual ILCOR/AHA guideline recommended, hospital based post-resuscitative therapies performance measures
van Diepen, 2017[103] (OHCA)	HeartRescue project, a multistate public health initiative, established in 5 states (Arizona, Minnesota, North Carolina, Pennsylvania, and Washington) in 2010
Weston, 2017[109] (OHCA)	Initiation of the individualized CPR feedback program
Wolfe, 2014[96] (IHCA)	Structured, quantitative, audiovisual, interdisciplinary debriefing of chest compression events with frontline providers; real-time feedback in actual resuscitation in both periods

ACLS indicates advanced cardiovascular life support; AHA American Heart Association; BLS, basic life support; CO-CPR, chest compression–only CPR; CPR, cardiopulmonary resuscitation; DA-CPR, dispatcher-assisted CPR; EMS, emergency medical services; IHCA, in-hospital cardiac arrest; ILCOR, International Liaison Committee on Resuscitation; OHCA, out-of-hospital cardiac arrest; RCT, randomized controlled trial; and ROSC, return of spontaneous circulation.

[H3] Treatment Recommendations

We recommend that organizations or communities that treat cardiac arrest evaluate their performance and target key areas with the goal to improve performance (strong recommendation, very-low-certainty evidence).

[H3] Justification and Evidence-to-Decision Framework Highlights

The evidence-to-decision table is included in Appendix A-3. The EIT Task Force recognizes that the evidence in support of this recommendation comes mostly from studies of moderate to very low certainty of evidence. However, the majority of studies found that interventions to improve system performance not only improved system-level variables and skill performance in actual resuscitations among rescuers but also clinical outcomes of patients with OHCA or IHCA, such as survival to hospital discharge and survival with favorable neurologic outcome at discharge.

Such interventions need money, personnel, and stakeholder buy-in to improve system performance. Some systems may not have adequate resources to implement system performance improvement. In making this recommendation, EIT Task Force places increased value on the benefits of system performance improvement, which have no known risks, given our knowledge that system performance improvement could show a large effect size in a beneficial direction.

In 2010, the EIT treatment recommendation stated the insufficiency of the evidence to make recommendations supporting or refuting the effectiveness of specific performance measurement interventions to improve processes of care and/or clinical outcomes in resuscitation systems.[1,2] In 2015, a suggestion was made to use performance measurement and quality improvement initiatives in organizations that treat cardiac arrest on the basis of a weak recommendation and very-low-quality evidence.[3,4] The evidence evaluation in 2020 led to a recommendation to evaluate performance, with the goal of improving performance (strong recommendation, very-low-certainty evidence).

[H3] Knowledge Gaps

- Identify the most appropriate strategy to improve system performance.
- Better understand the influence of local community and organizational characteristics.
- Evaluate the cost-effectiveness of the individual interventions for improving system performance.

[H2] Community Initiatives to Promote BLS Implementation (EIT 641: ScopRev)

[H3] Rationale for Review

This evidence evaluation is an update from the 2010 CoSTR.[1,2] In 2015, a SysRev addressed the crucial role of communities in providing and promoting bystander CPR.[3,4] Because several specific interventions have been investigated, the EIT Task Force decided to look into how community initiatives promote BLS implementation. For the purpose of this review, the term *community* was defined as the general population of the studied area (ie, a group of neighborhoods, 1 or more cities/towns or regions, a part of or a whole nation) in which individuals can act as potential witnesses or bystanders of a cardiac arrest (eg, a group of populations with no duty to respond in case of a cardiac arrest). The role of healthcare providers or first responders with any duty to respond was excluded. The term *initiative* includes all interventions aimed at increasing the engagement of the community in providing BLS, including early defibrillation.

Interventions improving the community response to cardiac arrest are evaluated in other specific PICOs of the 2020 evidence evaluation process—like dispatcher-assisted CPR or telephone-CPR; public access defibrillator programs and AED dissemination, including deployment by drones; simplification of CPR protocols (ie, chest compression–only CPR); and apps to localize and engage first responders and/or the nearest AED—and are not addressed in this review.

The aim of this SysRev was to assess the impact of any other intervention involving community, which can affect BLS implementation in terms of bystander CPR and other consistent clinical outcomes. Because of the high heterogeneity among found studies, the task force considered a ScopRev with a narrative description of the results as an appropriate way to summarize the results of this evidence evaluation.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Within the general population of children and adults suffering an OHCA

Intervention: Community initiatives to promote BLS implementation

Comparator: Current practice

Outcome: Survival to hospital discharge with good neurologic outcome, survival to hospital discharge, ROSC, time to first compressions, bystander CPR rate, and proportion of population trained

Study design: RCTs and nonrandomized studies (non-RCTs, interrupted time series, controlled before-and-after studies, cohort studies) are eligible for inclusion.

Time Frame: No limit; search ended November 10, 2019

[H3] Summary of Evidence

The complete ScopRev is included in Appendix B-3.

Of the 17 studies identified, 7 had a cross-sectional design,[47,114-119] 5 were before-and-after studies,[89,120-123] 4 were cohort studies,[124-127] and 1 was an RCT.[128] All OHCA cases included adult populations only. The main settings where the interventions took place were workplaces, schools, governmental offices, major civic events, and community-shared spaces.

[H3] Task Force Insights

Bystander CPR rate was reported in nearly all the studies, and almost all showed a benefit with implementation of community initiatives. This was more pronounced with bundled interventions than with training or mass media, but only 40% of studies reported an increase in survival at hospital discharge. Studies assessing bundle interventions also reported other outcomes that could not be included in the report, because the outcomes could not be associated with a specific intervention.

On the basis of the results of our review, we propose a SysRev be conducted, because it appears that the implementation of community initiatives such as CPR training involving a large portion of the population or bundle of interventions may improve the layperson bystander CPR rate.

[H3] Treatment Recommendation

The treatment recommendation remains unchanged from 2015.[3,4] We recommend implementation of resuscitation guidelines within organizations that provide care for patients in cardiac arrest in any setting (strong recommendation, very low quality of evidence).

[H2] Cardiac Arrest Centers (EIT 624: SysRev, 2019 CoSTR)

Cardiac arrest centers were considered hospitals providing evidence-based postresuscitation treatments, namely targeted temperature management and cardiac intervention (eg, coronary angiography).[14,15] A SysRev on this topic has been published[129] and was included in the 2019 CoSTR summary.[5,6]

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Adults with attempted resuscitation after nontraumatic IHCA or OHCA

Intervention: Treatment at a specialized cardiac arrest center

Comparator: Treatment in a healthcare facility not designated as a specialized cardiac arrest center

Outcome: 30-day survival with favorable neurologic outcome (defined as Cerebral Performance Category 1 or 2, modified Rankin Scale score 0–3), survival at hospital discharge with favorable neurologic outcome, survival at 30 days, and survival at hospital discharge and ROSC after hospital admission

Study Designs: RCTs and nonrandomized studies (non-RCTs, interrupted time series, controlled before-and-after studies, cohort studies) were eligible for inclusion. Unpublished studies (eg, conference abstracts, trial protocols) were excluded, as well as studies reporting pediatric cardiac arrests (18 years old or younger) and cardiac arrest secondary to trauma.

Time Frame: All years and all languages are included, provided there was an English abstract. Unpublished studies (eg, conference abstracts, trial protocols) were excluded. Literature search updated to the August 1, 2018.

[H3] Treatment Recommendations

We suggest adult patients with nontraumatic OHCA be cared for in cardiac arrest centers rather than in non–cardiac arrest centers in settings where this can be implemented (weak recommendation, very-low-certainty evidence).

For patients with IHCA, we found no evidence to support an EIT and ALS Task Force recommendation for or against the intervention.

For patient subgroups with either shockable or nonshockable initial cardiac rhythm, the current evidence is inconclusive, and confidence in the effect estimates is currently too low to support a separate EIT and ALS Task Force recommendation. For regional triage of OHCA patients to a cardiac arrest center by primary EMS transport or secondary interfacility transfer subgroups, the current evidence is inconclusive and confidence in the effect estimates is currently too low to support a separate EIT and ALS Task Force recommendation.[5,6]

[H2] Out-of-Hospital CPR Training in Low-Resource Settings (EIT 634: ScopRev)

[H3] Rationale for Review

Scientific statements and treatment recommendations have in the past been formulated from a perspective of an ideally resourced environment. Little attention has been paid to the applicability of statements from such high-resource or high-income areas in the daily practice of lower-income countries and/or lower-resource emergency care systems. In many parts of the world, the standard of care available in high-resource settings is unavailable because of lack of money. For example, the absence of an EMS system or the low-quality performance of an EMS system[130-133] or an EMS system under development[134] are barriers to the implementation of resuscitation guidelines. ILCOR's aim of creating internationally valid statements should consider that recommendations should also support systems with more limited resources.[135] This ScopRev aims to raise awareness of gaps in emergency care services around the world, to identify gaps in the literature, and to suggest future research priorities to address these gaps.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Adults and children living in low-resource settings

Intervention: Prehospital resuscitation

Comparator: No comparator

Outcome: Improved clinical outcomes

Study design: RCTs and nonrandomized studies (non-RCTs, interrupted time series, controlled before-and-after studies, cohort studies) are eligible for inclusion. Unpublished studies (eg, conference abstracts, trial protocols) were excluded.

Time frame: All years and all languages were included if there was an English abstract.

[H3] Summary of Evidence

The full ScopRev is included in Appendix B-4.

Low-resource settings were defined according to the World Bank definition by gross national income per capita, and all data except those coming from high-income economies were rated as low-resource for this ScopRev. The 24 identified studies[136-159] originated from diverse geographical areas, and there were large differences in the number of studies per region. No studies from low-income countries were eligible; 4 studies were from lower–middle income countries[140,141,154,160] all others were from upper–middle income economies.

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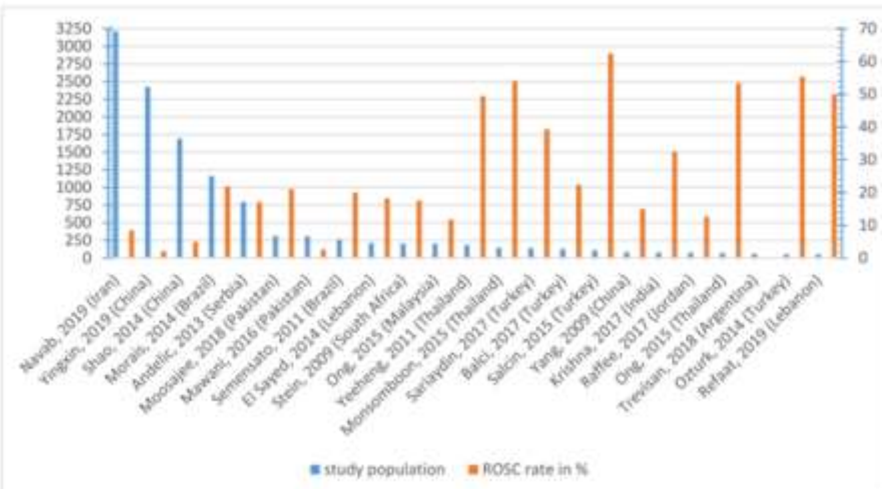
Only 4 studies reported data on over 1000 patients.[136,139,146,150] With the exception of 7 studies,[137,138,144,150,155,156,159] most data were derived from prospective or retrospective observational studies.

The ROSC rates varied considerably between studies, from 0% to 62%. Fifteen studies (63%)[138-142,144,147,150-157] reported on longer-term outcomes such as survival to hospital discharge or neurologic status. Longer-term outcomes were usually worse than those reported in patients from high-resource countries,[161] the Figure shows ROSC rates and the number of patients studied. The 3 largest studies[136,139,150] reported low ROSC rates compared with many of the smaller studies that reported high ROSC rates.

Figure. Number of patients studied (blue) and ROSC rates in % (orange) for included studies. X axis: first author, year of publication (country); Y axis left: number of patients studied; Y axis right: % ROSC. Guo 2017 was excluded from the figure because only a range of ROSC rates were reported.



[H3] Task Force Insights

This ScopRev of prehospital resuscitation in low-resources settings searched for evidence from adult and pediatric studies. Members of the ILCOR EIT Task Force are from mainly high-income settings. Experts with a background in or who are from low-resource settings were consulted and gave their opinions and insights, but they did not participate in the selection of the studies and in the data extraction. For this same reason, we could not consider non-English full-text articles, thereby creating a selection bias.

After the data extraction phase, the EIT Task Force decided to exclude studies on trauma, children, and neonates to reduce the complexity of this review. The EIT Task Force also decided to exclude articles published before January 1, 2009, thereby limiting the results to the last decade (this included 71% of all screened abstracts). We did this because low- and middle-income countries develop over time, and conclusions based on older studies may therefore be no longer relevant. The EIT Task Force acknowledges the heterogeneity of the reported data. This may have derived from the lack of resources that EMS systems, emergency departments, and researchers in low-resource areas can devote to standardize the reporting of outcome after resuscitation. Organizations responsible for emergency care in low-resource environments should be encouraged and supported to introduce measures of data collection, such as registries with outcome documentation, preferably also considering Utstein-style reporting. We acknowledge that there are costs associated with such data collection, and this should be prioritized locally depending on competing health expenditures. Data collection, in turn, may lead to improved comparability of data, support research specific to such settings, and generate scientific statements and recommendations specific for these areas. For future work, regional experts and clinicians should be involved in global initiatives such as ILCOR to maximize both local acceptability and applicability of such recommendations.

The question arises if prehospital resuscitation is feasible, cost-effective, or even ethically justifiable in the regions considered. CPR in OHCA has limited success, even in high-income economies. Considering the scarcity of resources in low-income countries, the feasibility of full ALS and postresuscitation care is controversial. Local determination of where to prioritize health system development should outweigh outside influence to focus on resuscitation to the detriment of other areas of health. So far, the information from the studies identified seems too heterogenous and was considered insufficient to make recommendations on OHCA in low-resource settings.

[H3] Treatment Recommendations

This treatment recommendation is unchanged from 2015.[3,4] We suggest that alternative instructional strategies would be reasonable for BLS or ALS teaching in low-income countries (weak recommendation, very low quality of evidence). The optimal strategy had yet to be determined.

[H2]Disparities in Education (EIT 4003: EvUp)

The topic of disparities in CPR education has not previously been reviewed by ILCOR, and there was no treatment recommendation as of January 31, 2020. An EvUp was performed (Appendix C-4), and several studies were identified that suggest the need for a SysRev.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Laypeople (nonprofessional responders)

Intervention: Racial, ethnic, socioeconomic, or gender disparities

Comparator: None

Outcome: Impact resuscitation education and/or contribute to barriers in bystander CPR

Study design: Cross-sectional or cohort studies are eligible for inclusion. Unpublished studies (eg, conference abstracts, trial protocols), letters, editorials, and pediatric studies were excluded.

Time frame: All articles published before October 8, 2019, and all languages were included if there was an English abstract

An EvUp was conducted for 2020 by the AHA. A search conducted in PubMed yielded 398 studies, and 24 were identified as relevant. The complete EvUp is included in Appendix C-4.

[H3] Treatment Recommendation

The EvUp did not enable a treatment recommendation to be made.

[H1] ALS Training, Including Team and Leadership Training, and METs and RRTs

[H2] Spaced Learning (EIT 1601: SysRev)

[H3] Rationale for Review

The spaced learning principle is supported by evidence from both the cognitive science and neuroscience literature.[162] There are few data to support which method of resuscitation training is most effective.[3,4] Formats employing spaced learning are increasingly being developed, aiming to enhance educational impact and flexibility of teaching. Educational theory strongly supports advantages of spaced learning.[163-167] Potential advantages may include the additional time to reflect and elaborate on the learning content between the learning sessions (eg, constructivist theories) and memory consolidation effects by recall/retraining.

Spaced learning is defined as the following (from the AHA scientific statement "Resuscitation Education Science: Educational Strategies to Improve Outcomes From Cardiac Arrest"[168]): "Spaced or distributed practice involves the separation of training into several discrete sessions over a prolonged period with measurable intervals between training sessions (typically weeks to months), whereas massed practice involves a single period of training [yearly or longer] without rest over hours or days."[168]

Whilst this evidence evaluation did not specifically address the timing of retraining, we included studies comparing spaced with massed learning in contexts of retraining (refresher training).

The comparisons in the literature revealed 2 types: (1) The use of spaced learning, which involved the separation of training into several discrete sessions over a prolonged period with measurable intervals between training sessions (typically weeks to months). The learning content can be distributed across different sessions or repeated at each session. The number of repetitions and time intervals between repetitions can vary. (2) The use of booster training, which describes distributed practice after initial completion of training and is generally related to low-frequency tasks such as the provision of CPR. The terms *just-in-time training*, *just-in-place training*, and *refreshers* describe training that is included in this category.

Because of the high heterogeneity among studies including clinical heterogeneity (such as types, format of intervention, and methods of outcome assessments) and methodologic heterogeneity (outcome assessments, duration of follow-up, and timing of assessment), the EIT Task Force was unable to perform a meta-analysis but reports a narrative synthesis of the findings structured around each outcome; spaced learning and booster training are discussed separately.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: All learners taking resuscitation courses (all course types and all age groups) and/or first aid courses

Intervention: Trained or retrained distributed over time (spaced learning)

Comparator: Compared with training provided at 1 single time point (massed learning)

Outcome: Educational outcomes (skill performance 1 year after course conclusion, skill performance between course conclusion and 1 year, and knowledge at course conclusion) and clinical outcomes (quality of performance in actual resuscitations and patient survival with favorable neurologic outcome)

Study design: RCTs and nonrandomized studies (non-RCTs, interrupted time series, controlled before-and-after studies, cohort studies) are eligible for inclusion. Unpublished studies (eg, conference abstracts, trial protocols) were excluded.

Time frame: All years and all languages were included if there was an English abstract; literature search was updated to December 2, 2019.

PROSPERO registration CRD42019150358

[H3]Consensus on Science

Seventeen studies in courses with manikins and simulation were included in the narrative synthesis: 13 randomized studies[169-181] and 4 nonrandomized studies.[182-185] As shown in Table 7 for spaced learning and 8 for booster learning, the included studies covered a range of resuscitation courses: 8 studies in BLS,[170,171,174,175,177-179,183] with the latter 3 studies reporting results from the same cohort of participants; 3 studies in pediatric ALS[169,172,182]; 5 studies in neonatal life support[173,176,180,181,185]; and 1 study in emergency medicine skills course.[184]

Table 7 - Characteristics of included studies 'Spaced learning'

Author Year Country	Study design	Student	Number of students	Course/Skills taught	Intervention	Control	Primary outcome(s)	Secondary outcomes(s) if any	Conclusion
Patocka 2019 Canada	Single-blinded RCT	Trained EMS providers (EMT or paramedics)	48	AHA/Heart and Stroke Foundation of Canada 2010 PALS curriculum	Spaced course (four 3.5 -h weekly sessions over 1 month)	Massed course (two sequential 7-h days)	Global rating scale (GRS) score for the four individual procedural skills (adult and infant CC, infant BMV and IO) immediately after course and 3 months later	Quantitative metrics of CPR, a multiple-choice question (MCQ) test, and visual analogue scale (VAS) scores for self-efficacy immediately after course and 3 months later	3-month retention of CC skills are, retention of other resuscitation skills may be better in spaced group
Lin 2018 Canada	RCT	Trained Healthcare providers working in the ED	87	Just-in-time CPR training; AHA BLS course	Distributed training at least once a month with real-time feedback without limited practicing time (AHA Resuscitation Quality Improvement (RQI) program)	Annual standardized AHA BLS course once a year	"Excellent CPR"(defined as achieving at least 90% of all AHA standards for CC depth, rate and recoil for each individual criterion.) after one year	Percentage of compression depth > 50 mm for adult/child and compression depth > 40 mm for infant; Percentage of CC with rate of 100–120/min; Percentage of CC with complete recoil. Every 3 months up to 1 year	Spaced training improves quality of CPR.

Patocka 2015 Canada	Prospective cohort	Third-year medical students	45	5 hours Pediatric Resuscitation course based on PALS	4 weekly 1.25 hour sessions (each with one week spacing interval)	Single 5-hour session	Performance on the MCE knowledge assessment and procedural skill global rating scores. 4 weeks following the completion of the last session	Procedural checklist scores and performance on a priori determined critical procedural elements	Spaced format may have better retention of skills and more rapid completion of critical tasks
Kurosawa 2014 Japan	Prospective randomized single-blind trial	Trained PICU-nurses, respiratory therapists, and nurse practitioners.	40	PALS recertification course, based on American Heart Association (AHA) PALS recertification training	Simulation-based modular PALS recertification training (reconstructed into six 30-min sessions conducted monthly) and two 15-minute AED/CPR demonstration sessions, and up to 60 minutes for the written evaluation for a total of 4.5 hours	standard 1-day simulation-based PALS recertification course 7.5 hours	Skill performance measured by a validated Clinical Performance Tool immediately after training	Teamwork (Behavioural Assessment Tool), self-confidence and satisfaction immediately after training	Spaced training more effective for skill performance
Tabangin 2018 Honduras	RCT	Clinic and hospital providers (doctors and nurses)	37	Helping Babies Breathe (HBB)	monthly practice for 6 months after initial training	three consecutive practices at 3, 5 and 6 months	the OSCE B score immediately after training, at 3 and 6 months	passing on thefirst attempt (performing 14 of 18 steps, including the required 4 essential steps) and the number of attempts until passing immediately after training, at 3 and 6 months	Spaced training has better retention of skills

Sullivan 2015 USA	RCT	Trained nurses	66	CPR and defibrillation for IHCA	15 min in-situ IHCA training sessions every two (2M), three (3M) or six months (6M)	standard AHA training (2 years)	Time elapsed from call for help to; (1) initiation of chest compressions and (2) successful defibrillation in IHCA 6 months after initial training	CCF and whether CPR adjuncts (stepstool and backboard) was utilized 6 months after initial training	Spaced training improves initiation of CPR and defibrillation timings
Breckwoldt 2016 Switzerland	quasi-experimental study	5th year medical student	156	Students' procedural knowledge within intensive course in emergency medicine	26 teaching hours in 4.5 days	26 teaching hours in 3.0 days	the difference in overall key-feature test score within 8 days after training		Moderate improvement on learning seen with spaced learning

1

1 Table 8 Characteristics of included studies with 'Booster learning'

Author Year Country	Study design	Student	Number of students	Course/Skills taught	Intervention	Control	Primary outcome(s)	Secondary outcome(s) if any	Main findings
Ernst 2014 USA	RCT	3rd year medical students	110	neonatal intubation	Weekly (practice once/week for four consecutive weeks), or consecutive day (practice once/day for four consecutive days).	standard (control; no practice sessions),	Equipment selection (preparation score), procedural skill steps (procedure score), length of intubation attempts (in seconds), and the number of attempts at 6 weeks		Neither practice superior at 6 weeks
Montgomery * 2012 USA	RCT	Nursing students	606	BLS	6 min of monthly practice on a voice advisory manikin after initial training	no practice after initial training	Survey related to CPR confidence, initial course length, and satisfaction at 1 year		Monthly practice improves confidence.
Kardong-Edgren* 2012 USA	RCT	Nursing students	606	BLS	6 min of monthly practice on a voice advisory manikin after initial training	no practice after initial training	Correctly performed compressions; Correctly performed ventilations at 12 months		Even with monthly practice and accurate voice-activated manikin feedback, some students could not perform CPR correctly.
O'Donnell 1993 UK	RCT	Trained nurses	100	CPR	Group 1: monthly refresher sessions, Group 2: a single refresher at 3 months	Group 3: no refresher training	Knowledge test and pass rate for the skill test 6 months after initial training		Knowledge better in booster training. Skills equally poor in both groups.

Anderson 2019 Canada	RCT	Trained healthcare professionals- ICU, Theatre, ED, ward nurses	244	AHA's Resuscitation Quality Improvement (RQI) program	Workplace-based CPR training at different intervals: Group1- monthly. Group2- 3months. Group3 - 6 months.	Workplace-based CPR training at different intervals: every 12 months	Proportion of participants performed 'Excellent' CPR at the 12-month	Individual CPR performance metrics at 12 month	Booster training is effective in improving CPR performance, with monthly training more effective than training every 3, 6, or 12 months.
Cepeda Brito 2017 USA	Single-blinded, randomized longitudinal study	Trained staff from neonatal intensive care unit	25	NRP	Rolling refresher training at 1-month and 3-month intervals	Rolling refresher training at 6-month interval	Effective chest compressions rate (>90 compressions/min, >1/3 anteroposterior chest wall diameter, full recoil, interruptions <1.5 seconds. Tested at 6 months	Chest compression fraction; chest compression rate; Adjusted chest compression rate (results not given)	No statistically significant difference between groups
Oermann* 2011 USA	RCT	Nursing students	606	BLS	6 min of monthly practice on a voice advisory manikin after initial training	no practice after initial training	Compression rate and depth, percent of compressions performed with adequate depth, percentage with correct hand placement, ventilation rate and volume, and percentage of ventilations with		Booster training may improve skill performance.

							adequate volume. Randomly selected to be tested every 3 months up to 1 year		
Mduma 2015 Africa	Before and After study	midwives, nurse students, operating nurses, and doctors	Number of students not reported. 4894 deliveries before, 4814 post intervention	NRP	Frequent brief (3–5 min weekly) on-site HBB simulation training on newborn resuscitation practices in the delivery room	No booster	Delivery room management of newborns and 24-h neonatal out-comes (normal, admitted to a neonatal area, death, or stillbirths). Observed by research assistants.		The number of stimulated neonates increased from 712(14.5%) to 785(16.3%) (p = 0.016), those suctioned increased from 634(13.0%) to 762(15.8%) (p ≤ 0.0005). Neonates receiving bag mask ventilation decreased from 357(7.3%) to 283(5.9%) (p = 0.005). Mortality at 24-h decreased from 11.1/1000 to 7.2/1000 (p = 0.040).
Bender 2014 USA	RCT	Residents (NICU and non-NICU)	50	NRP	booster simulation 7 to 10 months after NRP.	No booster	Video recordings independently assessed procedural skill and teamwork behavior at 15months		The intervention group demonstrated better procedural skills (71.6 versus 64.4) and teamwork behaviors (18.8 versus 16.2).

Nishiyama 2015 Japan	RCT	University employees and students (non-healthcare)	112	BLS	15min refresher course 6 months after initial 45min training	Initial 45min BLS training. No refresher	The number of appropriate chest compressions during a 2-min test period at 12 months	The number of total chest compressions, the proportion of appropriate chest compressions, and time without chest compressions. Time from starting the presentation to first chest compression and time from arriving at AED beside the participant to the first defibrillation	The number of appropriate chest compressions performed was significantly greater in the refresher training group (68.9 ± 72.3) than in the control group (36.3 ± 50.8, p = 0.009). Time without chest compressions was significantly shorter in the refresher training group (16.1 ± 2.1 s versus 26.9 ± 3.7 s, p < 0.001). There were no significant differences in time to chest compression and AED use between the groups.

1 *same study with different outcomes repor

In all identified studies, practical skills were assessed using manikins.

The overall certainty of evidence was rated as very low for all outcomes primarily because of a very serious risk of bias. The individual studies were all at moderate to serious risk of bias because of confounding. Because of this and a high degree of clinical heterogeneity (such as types, format of intervention, methods of outcome assessments) and methodologic heterogeneity (outcome assessments, duration of follow-up, timing of assessment), no meta-analyses could be performed.

For the critical outcome of skill performance 1 year after course conclusion, we identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, and imprecision) from 4 RCTs,[170,171,175] which all reported the use of spaced learning in BLS to evaluate the number of participants able to provide chest compressions of adequate depth (defined as greater than 50 mm) at 1 year. One RCT[171] (n=87) reported that more participants were able to perform chest compressions of adequate depth with spaced learning than with massed learning. At 12 months' testing, the spaced learning group was superior to the control group for proportion of excellent CPR (control, 6/41 [14.6%], intervention 25/46 [54.3%]; $P<0.001$; odds ratio [OR], 6.94; 95% CI, 2.45–19.69). This study also reported improvement in other measures of quality of chest compressions: percentage of chest compressions at the correct rate (100–120/min) improved from 78.0% (95% CI, 70.8%–85.1%) to 92.7% (95% CI, 86.0%–99.4%), and percentage of chest compressions with complete recoil improved from 86.5% (95% CI, 81.6%–91.4%) to 97.4% (95% CI, 92.8%–100.0%). Similar improvements were also reported in pediatric CPR parameters.

In booster learning, 3 RCTs[170,175,179] (n=790) reported more participants were able to provide chest compressions of adequate depth compared with those who received no booster learning. One RCT[170] compared booster learning of different frequency (monthly, every 3 months, every 6 months, annually). This study reported improved chest compression performance across all booster groups, with monthly booster learning providing the best skill performance but the highest attrition rate. Participants who trained monthly had a significantly higher rate of excellent CPR performance (15/26, 58%) than those in all other groups (12/46, 26% in the 3-month group, *P*=0.008; 10/47, 21% in the 6-month group, *P*=0.002; and 7/48, 15% in the 12-month group, *P*<0.001). *Excellent CPR* was defined as a 2-minute CPR session in which 3 metrics were achieved: (1) 90% of compressions with correct depth (50–60 mm); (2) 90% of compressions with correct rate (100–120/min); and (3) 90% of compressions with complete chest recoil. The Oermann study[175] also reported improved CPR performance in participants who received brief monthly practice compared with no monthly practice. In the booster learning group, students' mean compression depth was within acceptable range (mean, 40.3 mm; standard deviation [SD], 6.6) with 59.2% (SD, 36.6) of compressions with adequate depth and no skill decay over the 12 months (*P*=0.31). In contrast, the control group had a significant loss of ability to compress with adequate depth at 12 months (mean, 36.5 mm; SD, 7.7) and only 36.5% (SD, 33.6) of compressions with adequate depth (*P*=0.004). With booster learning, students in the spaced learning group had significantly higher percentage of ventilations with adequate volume (booster, 52.2%; SD, 30.9 versus no booster, 38.5%; SD 36.1; *P*<0.001). At 12 months, the mean ventilation volume was 565 mL (SD, 148) for the booster group compared with mean ventilation volumes of 431 mL (SD, 232) for no booster group (*P*<0.0001). In a randomized study, Nishiyama et al compared BLS skill retention by laypeople trained with a 45-minute DVD-based program with and without a 15-minute refresher/booster learning at 6 months.[179] During a 2-minute evaluation performed at 12 months, the number of total chest compressions was significantly greater in the booster group than in the no-booster group (booster mean, 182.0 [SD, 41.7] versus no booster mean, 142.0 [SD, 59.1]; $P<0.001$). The number of appropriate chest compressions (with depth over 50 mm, correct hand position, complete recoil) performed was significantly greater in the booster group than in the no-booster group (booster mean, 68.9; SD, 72.3 versus no booster mean, 36.3; SD, 50.8; $P=0.009$). Time without chest compressions was also significantly shorter in the booster group (booster mean, 16.1 [SD, 2.1] seconds versus no booster, 26.9 [SD, 3.7] seconds; $P<0.001$). There were no significant differences in time to first chest compression between the 2 groups (booster mean, 29.6 [SD, 16.7] seconds versus no booster mean, 34.4 ± 17.8 seconds; $P=0.172$) and AED operations.

For the critical outcome of skill performance between course conclusion and 1 year, we identified very-low-certainty evidence (downgraded for risk of bias and imprecision) from 2 RCTs[171,175] (n=201) for number of participants able to perform chest compressions with adequate depth (greater than 50 mm) at 6 months.

In a randomized trial, Lin et al[171] reported the percentage of spaced learning participants who were able to perform chest compressions of adequate depth as mean 83.2 (95% CI, 74.4–92.1) compared with the control group mean 58.0 (95% CI, 48.5–67.4), group difference mean 25.3 (95% CI, 12.0–38.2); the percentage of spaced learning participants able to perform chest compressions of correct rate mean 95.5 (95% CI, 90.0–100.0) compared with the control mean 79.3 (95% CI, 73.3–85.3), group difference mean 16.2 (95% CI, 8.1–24.4); and the percentage of spaced learning participants able to perform chest compressions with complete chest recoil mean 97.4 (95% CI, 94.1–100.0) compared with mean 88.9 (95% CI, 85.3–92.4), group difference mean 8.6 (95% CI, 3.7–13.4). Similar superior performance was reported in the spaced learning group across all testing time points (3, 6, 9, and 12 months).

A second study also reported improved CPR performance in participants who received brief monthly practice compared with no monthly practice.[175] In the booster learning group, the mean compression depths were maintained during 12 months of the study and ranged from 38.6 mm (SD, 6.7) at 3 months to 40.3 mm (SD, 6.6) at 12 months. In the no-booster group, there was significant skill decay with ability to compress with adequate depth, the mean depth at 9 months was 39.6 mm (SD 6.8) and at 12 months was 36.5 mm (SD 7.7, P=0.004). With booster learning, students in the spaced learning group improved their ability to ventilate with an adequate volume (6 months mean ventilation volume, 514.0 mL [SD, 208.4]; 12 months mean ventilation volume, 620.7 mL [SD, 211.0]). In the control group, the mean ventilation volumes remained less than the recommended minimum (500 mL) throughout the 12 months.

[H4] Other Studies Reporting Skill Performance Between Course Conclusion and 1 Year

[H5] Spaced Learning (3 Studies)

Three studies examined spaced learning in pediatric ALS. The first study[172] recruited healthcare providers and found improved clinical performance score: maximum score of 42 made up of 21 items (each item was scored as 0=not performed, 1=performed inappropriately or not in a timely manner, and 2=performed correctly and in a timely manner). Scores in the spaced learning group increased (pre 16.3±4.1 to post 22.4±3.9) compared with scores in the standard massed learning group (pre 14.3±4.7 to post 14.9±4.4; P=0.006). Improvement was also found in the Behavioral Assessment Tool after learning but did not reach statistical significance (P=0.49).

The second study[169] randomized EMS providers to either a spaced (4 weekly sessions) or massed (2 sequential days) format. At 3 months' testing, infant and adult chest compressions were similar in both groups, but bag-mask ventilation and intraosseous insertion performance was superior in the spaced learning group (spaced learning group bag-mask ventilation score mean, 2.2 [SD, 7], *P*=0.005; intraosseous score mean, 3.1 [SD, 0.5], *P*=0.04; massed learning group bag-mask ventilation score mean, 1.8 [SD, 0.5], *P*=0.98; intraosseous score mean, 2.7 (SD, 0.2), *P*=0.98).

In the third study, the same research group randomized medical students to a pediatric resuscitation course in either a spaced or massed format.[182] Four weeks after course completion, participants were tested with a knowledge exam and their ability to perform bag-valve mask ventilation, intraosseous insertion, and chest compressions. The study found no significant difference in knowledge and overall performance, but there was a trend toward more critical procedural steps performed by the spaced learning group.

[H5] Booster Learning (7 Studies)

Sullivan et al randomized nurses into 4 groups: 1 group for standard AHA learning and 3 groups that participated in 15-minute in situ IHCA learning sessions every 2, 3, or 6 months.[174] The study found more frequent learning was associated with decreased median time (in seconds) to starting compressions (standard, 33 [interquartile range—IQR, 25–40] versus 6 months, 21 [IQR, 15–26] versus 3 months, 14 [IQR, 10–20] versus 2 months, 13 [IQR, 9–20]; *P*<0.001) and to defibrillation (standard, 157 [IQR, 140–254] versus 6 months, 138 [IQR, 107–158] versus 3 months, 115 [IQR, 101–119] versus 2 months, 109 [IQR, 98–129]; *P*<0.001])

Randomizing nursing students to monthly booster learning or no booster learning, Kardong-Edgren et al reported a higher percentage of compressions and ventilations without errors in the booster group: percentage of correct mean chest compressions (booster group mean, 49.2 [SD 33.2] versus no-booster group mean, 39.7 [SD 34.8]; *P*=0.003), percentage of correct ventilation (booster group mean, 48.0 [SD, 32.3] versus no-booster group, mean 36.7 [SD 33.7]; *P*<0.0001).[178] In the same cohort, participants also reported high satisfaction with the course.[177]

O'Donnell et al also compared monthly booster learning, booster learning every 3 months, and no booster learning among 100 nursing students undertaking BLS courses.[183] They found improved knowledge in the participant booster learning group but did not find improved skill performance at 6 months (theory score monthly practice mean, 11.5/14; practice every 3 months, 10.68/14; no practice, 9.50/14; *P*=0.05).

Repeated booster practice was tested in neonatal resuscitation by Tabangin, who randomized neonatal hospital providers to monthly practice for 6 months versus 3 consecutive practices at 3, 5 and 6 months.[173] The study concluded that repeated monthly testing resulted in improvements and maintenance of performance. Participants in the monthly practice group scored 1.3 points (SE, 0.42) higher on the objective structured clinical evaluation than those who practiced less frequently. Over 6 months, the monthly practice group had 2.9 times greater odds of passing on the first attempt compared with the group that practiced less frequently.

Ernst et al randomized students training in neonatal intubation to standard training, weekly booster learning, or 4-weekly booster learning.[176] Booster learning improved all aspects of neonatal intubation performance, including choosing the correct equipment, properly performing the skill steps, length of time to successful intubation, and success rate, for novice healthcare providers in a simulation setting. After training, the median preparation score (maximum, 11) for the weekly (median, 9; IQR, 8.0–9.5) and consecutive-day (median, 8.0; IQR, 7.5–9.0) groups was significantly higher than in the control group (median, 7.0; IQR, 6.0–8.0; *P*<0.001). The posttraining performance score (maximum, 8) was also significantly higher in the weekly (median, 7.0; IQR, 6.5–7.5) and consecutive-day (median, 7.0; IQR, 6.0–7.5) groups compared with the control group (median, 5.5; IQR, 4.0–6.0; *P*<0.001). First-attempt intubation success improvements from baseline to the final assessment were as follows: from 3 participants to 11 (20% increase) in the standard group, from 6 participants to 26 (62% increase) in the weekly practice group, and from 4 participants to 29 (67% increase) in the consecutive-day practice group (P<0.001 for all groups). First-attempt intubation times also improved between the baseline and final assessments for participants in the 2 practice groups (weekly mean, 27 seconds decrease from 42.5 to 15.5 seconds; consecutive-day mean, 11.3 seconds decrease from 31.3 to 20.0 seconds; control mean, 6.5 seconds increase from 23.5 to 30.0 seconds; P<0.001). The researchers were unable to demonstrate whether one type of booster learning was superior to the others.

Bender et al conducted an RCT comparing booster learning 9 months after a neonatal resuscitation training program with no booster learning. In simulation testing at 15 months, the booster group scored significantly higher in procedural scores out of a maximum score of 107 (71.6 versus 64.4; P=0.02) and teamwork behaviors out of maximum score of 25 (18.8 versus 16.2; P=0.02). No difference in knowledge scores was found.[181]

Cepeda Brito et al randomized students in a neonatal resuscitation program to rolling refresher booster learning or no booster learning.[180] Participants in booster learning reported higher confidence in their performance at 6 months, but this was not statistically significant.

For the important outcome of knowledge at course conclusion, we found very-low-certainty evidence (downgraded for risk of bias and imprecision) from 3 cohort studies. Breckwoldt et al designed an emergency medicine intensive course of 26 teaching hours and compared the knowledge of 156 students for a course delivered over 4.5 days with a course delivered over 3.0 days.[184] At course conclusion, knowledge was tested with video case-based simulation. After the course, participants' procedural knowledge was assessed by a specifically developed video case-based key-feature test. Participants from the spaced version reached a mean of 14.8 (SD, 2.0) out of 22 points, compared with 13.7 (SD, 2.0) in the massed version (*P*=0.002). In an RCT of spaced versus massed learning in EMS providers, a 33-question standardized Heart and Stroke Foundation of Canada pediatric ALS multiple choice questionnaire (MCQ) test was used immediately after training and 3 months after the course.[169] In the spaced group, there was no decay in the mean MCQ score 3 months after the course compared with the immediate postcourse score (immediately after, 30.3 [SD, 0.5] versus after 3 months, 29.7 [SD 0.5]; *P*=0.39); however, there was a statistically significant decay in the MCQ scores in the massed learning condition (immediately after, 31.1 [SD, 0.5] versus after 3 months, 29.6 [SD 0.5]; *P*=0.04).

O'Donnell compared monthly booster learning, booster learning every months, and no booster learning among 100 nursing students undertaking BLS courses.[183] They found improved knowledge among participants in the booster learning group but did not find improved skill performance at 6 months (theory score monthly practice mean 11.5/14, 3 monthly practice 10.68/14, no practice 9.50/14, *P*=0.05)

For the important outcome of quality of performance in actual resuscitations, we did not identify any studies.

For the important outcome of patient survival with favorable neurologic outcome, we did not identify any studies.

Whilst we did not find any study reporting performance at clinical resuscitation and patient survival with favorable neurologic outcome, there was evidence from 1 observational study on the impact of booster learning on delivery room management of the newborn.[185] This study assessed the impact of frequent brief (3–5 minutes weekly) on-site simulation training on newborn management in the delivery room and the potential impact on 24-hour neonatal mortality. The number of stimulated neonates increased from 712 (14.5%) to 785 (16.3%) (*P*=0.016), and those suctioned increased from 634 (13.0%) to 762 (15.8%) (*P*≤0.0005). Mortality at 24 hours decreased from 11.1/1000 to 7.2/1000 (*P*=0.040).

[H3] Treatment Recommendations

For learners undertaking resuscitation courses, we suggest that spaced learning (training or retraining distributed over time) may be used instead of massed learning (training provided at 1 single time point) (weak recommendation, very-low-certainty evidence).

[H3] Justification and Evidence-to-Decision Framework Highlights

The evidence-to-decision table is included in Appendix A-4. There is growing evidence suggesting that spaced learning can improve skill retention (performance 1 year after course conclusion), skill performance (performance between course completion and 1 year), and knowledge at course completion. We did not find any evidence to support either spaced or massed learning in skill performance during actual resuscitations or patient survival with favorable neurologic outcomes.

In making this recommendation, the EIT Task Force (in collaboration with Neonatal Life Support Task Force) considered the following:

Our review has only found very-low-certainty evidence to support spaced learning in resuscitation education derived mainly from BLS, pediatric, and neonatal life support courses. Nevertheless, the EIT Task Force is of the opinion that the benefits of spaced learning demonstrated in other areas of education would also apply in resuscitation training.

Our review did not evaluate the optimal format of spaced learning or effect of different retraining intervals. Any training intervention should be designed to deliver the learning objectives specific to a course, and it is unlikely that 1 specific format, design, or duration would fit all resuscitation training courses.

There were limited data from 2 studies that reported improved human factors with spaced learning.[172,181]

There may be concerns about increased costs or resource because of the organization required for faculty, equipment, and learners to implement spaced learning.[170] However, there is evidence from the gray literature that spaced learning can lead to cost savings.[186]

Participation in spaced learning requires ongoing motivation. It may be challenging to engage providers in repeated, effortful practice.

Met opmerkingen [JF24]: ADD REFERENCE: Cheng A, Nadkarni VM, Mancini MB, Hunt EA, Sinz EH, Merchant RM, et al. Resuscitation Education Science: Educational Strategies to Improve Outcomes From Cardiac Arrest: A Scientific Statement From the American Heart Association. Circulation. 2018;138:e82-e122.

Met opmerkingen [GR25R24]: That is reference 168 {Cheng e82 2018}

The 2010 CoSTR described insufficient evidence to recommend any specific training intervention, compared with traditional lecture/practice sessions, to improve learning, retention, and use of ALS skills.[1,2] The issue of new teaching strategies was not assessed in 2015, but this 2020 evaluation suggests that spaced learning (distributed over time) may be useful for resuscitation training.

This CoSTR EIT 1601 is a new PICO and refers to the difference in education by a large initial teaching session compared with small inputs separated over time. The CoSTR EIT 628 refers to retraining after initial education. Both are different educational questions and therefore EIT decided to investigate these different questions.

[H3] Knowledge Gaps

- There were no studies examining spaced learning in adult ALS.
- There was a lack of data on the impact of spaced learning on quality of performance in actual resuscitations.

- There was a lack of data on impact of spaced learning on patient survival with favorable neurologic outcome. In neonates, there were limited data on infant mortality at 24 hours after delivery. There are currently no data on survival to hospital discharge or long-term survival in neonates.
- There were insufficient data to examine the effectiveness of spaced learning on skill acquisition compared with maintaining skill performance and/or preventing skill decay.
- There were insufficient data to examine the effectiveness of spaced learning on laypeople compared with healthcare providers.
- There were limited data on impact of spaced learning on human factors (team behaviors and nontechnical skills).
- There was no evidence on cost-effectiveness and resource implications of spaced learning.
- There is a need to understand how to address high attrition rates in spaced learning. For spaced learning to be effective, we will need to understand how to engage learners by using the learners' motivation and reduce their burden.

[H2] EMS Experience and Exposure (EIT 437: SysRev)

[H3] Rationale for Review

There are no current ILCOR recommendations on EMS experience and exposure to resuscitation. Resuscitation knowledge and skills are likely to degrade with time if not refreshed with regular use or training. A SysRev published in 2014[187] found very little evidence; however, several large studies have been published subsequently. EMS experience and exposure was chosen as a topic as there was emerging evidence that EMS exposure to resuscitation varied greatly both within and across organisations, and that there was an association between this and patient outcomes.

The literature defines two main types of comparisons: first, exposure and years of career experience of the team performing resuscitation, and second, exposure and years of career experience of individuals within the team (eg, team leader or treating paramedic). Because of the considerable heterogeneity among studies, the EIT Task Force was unable to perform a meta-analysis but describes the findings in a narrative synthesis.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Adults and children who are in cardiac arrest in the out-of-hospital setting

Intervention: Resuscitation by experienced EMS practitioners or practitioners with higher exposure to resuscitation

Comparator: Resuscitation by less-experienced practitioners or practitioners with fewer exposures

Outcome: Survival to hospital discharge/30 days with good neurologic outcome, survival to hospital discharge/30 days, and survival to hospital (event survival) and prehospital ROSC

Study design: RCTs, nonrandomized studies (non-RCTs, interrupted time series, controlled before-and-after studies, cohort studies), original research articles (both prospective and retrospective) were included with no language restrictions. Unpublished studies (eg, conference abstracts, trial protocols) were excluded.

Time frame: All years and all languages were included if there was an English abstract up to October 14, 2019.

PROSPERO registration CRD42019153599

[H3] Consensus on Science

Very-low-certainty evidence (downgraded for very serious risk of bias) was derived from 7 studies included in this narrative synthesis.[188-193] The critical risk of bias and a high degree of heterogeneity precluded meta-analyses.

Met opmerkingen [JF26]: ADD REFERENCE #194 Lukic 194. Lukić A, Lulić I, Lulić D, Ognjanović Z, Cerovečki D, Telebar S, Mašić I. Analysis of out-of-hospital cardiac arrest in Croatia - survival, bystander cardiopulmonary resuscitation, and impact of physician's experience on cardiac arrest management: a single center observational study. *Croat Med J*. 2016;57:591–600. doi: 10.3325/cmj.2016.57.591

[H4] Studies Examining Exposure to Resuscitation

For the critical outcome of survival with favorable neurologic outcome at discharge/30 days, we identified very-low-certainty evidence (downgraded for risk of bias and imprecision) from 1 non-RCT.[193] This study examined exposure for EMS-physicians and reported unadjusted data with insufficient numbers of events to be confident in the direction of the outcome estimates.

For the critical outcome of survival to discharge/30 days, we identified very-low-certainty evidence (downgraded for risk of bias and imprecision) from 3 non-RCTs.[188,189,193] The largest and highest-quality non-RCT[189] reported adjusted outcomes and examined the whole resuscitating teams' exposure in the preceding 3 years. This study found that higher team exposure in the preceding 3 years was associated with increased survival to discharge: comparing the reference group with 6 exposures or fewer, group with more than 6 to 11 exposures (adjusted OR, 1.26; 95% CI, 1.04–1.54), group with 11 to 17 exposures (adjusted OR, 1.29; 95% CI, 1.04–1.59), and group with more than 17 exposures (adjusted OR, 1.50; 95% CI, 1.22–1.86).

The remaining 2 non-RCTs[188,193] reported unadjusted outcomes and used the average exposure of team leaders to resuscitation over 1-[193]and 3-year study periods.[188] These studies found no association between exposure to resuscitation, at thresholds of 5 exposures over 3 years for EMS-physicians[188] or 10 exposures over 1 year for the lead paramedic, [193] and unadjusted survival to hospital discharge.

Dyson et al[189] also found lower survival to discharge in patients treated by teams without an exposure in the preceding 6 months (adjusted OR, 0.70; 95% CI, 0.54–0.91) compared with those with recent exposure (less than 1 month).

For the critical outcome of event survival, we identified very-low-certainty evidence (downgraded for risk of bias and imprecision) from 2 non-RCTs.[188,193] These 2 studies reported unadjusted outcomes and used the average exposure of team leaders to resuscitation over 1-[193] and 3-year study periods.[188] These studies found no association between exposure to resuscitation, at cutoffs of 5 exposures over 3 years for EMS-physicians[188] or 10 exposures over 1 year for the lead paramedic,[193] and unadjusted event survival.

For the critical outcome of ROSC, we identified very-low-certainty evidence (downgraded for risk of bias) from 2 non-RCTs.[192,193] The largest non-RCT[192] reported adjusted outcomes and examined the primary treating paramedic's exposure in the preceding 5 years. This study found higher exposure of the treating paramedic was associated with increased ROSC, compared with the reference group with fewer than 15 exposures and the group with 15 exposures or more (adjusted OR, 1.22; 95% CI, 1.11–1.36). The other non-RCT[193] also found an unadjusted association between 10 exposures or more for the lead paramedic over a 1-year period and achievement of ROSC (OR, 1.30; 95% CI, 1.01–1.69).

[H4] Studies Examining Years of Career Experience

For the critical outcome of survival with favorable neurologic outcome at discharge/30 days, we identified no studies.

For the critical outcome of survival to discharge/30 days, we identified very-low-certainty evidence (downgraded for risk of bias and imprecision) from 4 non-RCTs.[189,190,194] The largest and highest-quality non-RCT[189] reported adjusted outcomes and examined the treating teams' years of clinical experience and found no association with survival to hospital discharge: reference group with median 5 or fewer career years, group with 5 to 8 years (adjusted OR, 1.17; 0.99–1.39), group with 8 to 11 years (adjusted OR, 1.11; 0.93–1.34), and group with more than 11 years (adjusted OR, 1.09; 0.91–1.29). Two smaller non-RCTs examined subgroups of OHCAs and also found no association with survival to discharge and the experience of the individual treating paramedics or treating EMS team.[190,194] The remaining non-RCT reported an association between increased survival to hospital discharge and technicians with >4 years experience (adjusted OR 2.58, 95% CI 1.11-6.03, P=0.03) and paramedics with >1 year of experience (adjusted OR 2.68, 95% CI 1.05- 6.82, P=0.04). However, this study did not fully account for the experience of the paramedics, as it did not include the previous career experience of paramedics as EMTs.

For the critical outcomes of event survival and ROSC, we identified no studies.

[H3] Treatment Recommendations

We suggest that EMS systems (1) monitor their clinical personnel's exposure to resuscitation and (2) implement strategies, where possible, to address low exposure or ensure that treating teams have members with recent exposure (weak recommendation, very-low-certainty evidence).

Met opmerkingen [JF27]: ADD REFERENCE #191 Soo
191. Soo LH, Gray D, Young T, Skene A, Hampton JR. Influence of ambulance crew's length of experience on the outcome of out-of-hospital cardiac arrest. *Eur Heart J*. 1999;20:535–540. doi: 10.1053/euhj.1998.1334

Met opmerkingen [JF28]: ADD REFERENCE #191
191. Soo LH, Gray D, Young T, Skene A, Hampton JR. Influence of ambulance crew's length of experience on the outcome of out-of-hospital cardiac arrest. *Eur Heart J*. 1999;20:535–540. doi: 10.1053/euhj.1998.1334

[H3] Justification and Evidence-to-Decision Framework Highlights

The evidence-to-decision table is included in Appendix A-5. In making this recommendation, the EIT Task Force prioritized the potential for improved patient outcomes through increased exposure and with the understanding that knowledge and skills degrade over time and without use. We recognize that the evidence in support of this recommendation comes from observational studies of very low certainty.

Potential strategies to improve exposure include rotating EMS personnel through higher OHCA volume areas and ensuring treating teams include EMS personnel with recent exposure. However, the strategies used are likely to vary between EMS systems.

The EIT Task Force discussed the maintenance of resuscitation skills through team simulation. Team simulation has been found to be effective for maintaining ALS skills in hospital settings and is associated with improved patient outcomes.[100,195] Such training may be a useful proxy for exposure in low-exposure settings and for rare OHCA cases (eg, pediatrics and neonates).

The EIT Task Force also discussed the possibility of providing a target level for ideal exposure. However, it was decided that more evidence is needed before exposure can be more accurately defined because the existing studies are conflicting. Dyson et al report a linear relationship between survival to hospital discharge and exposure,[189] whereas Tuttle et al report a leveling of ROSC at more than 15 exposures in the preceding 5 years.[192]

[H3] Knowledge Gaps

- Only short-term outcomes were evaluated. Future studies should document neurologically intact survival to hospital discharge/30 days and adjust for potential confounders.

- There is limited evidence to define low/ideal exposure to OHCA resuscitation.
- There is limited evidence of exposure to rare OHCA cases.
- There is need to study this in other groups of health care professionals.
- There is a need for interventional studies implementing strategies to improve EMS exposure to resuscitation.

[H2] Cognitive Aids During Resuscitation Education (EIT 629: SysRev)

[H3] Rationale for Review

The 2010 CoSTR stated, "It is reasonable to use cognitive aids (eg, checklists) during resuscitation, provided that they do not delay the start of resuscitative efforts." [1,2] Since then, many studies have been published.

For this review, *cognitive aids* were defined as the presentation of prompts aimed to encourage recall of information to increase the likelihood of desired behaviors, decisions, and outcomes.[196] Examples of cognitive aids include checklists, device apps, video clips, and pictures.

Our goal was to describe the impact of cognitive aids used during real CPR attempts; however, no studies were found. Therefore, the task force decided to address the topic in 2 indirect ways: (1) real-life trauma resuscitation, where the clinical environment may be sufficiently similar to cardiac arrest, and (2) simulated cardiac arrest environments. The outcomes listed below refer to these 2 types of studies.

There was high heterogeneity among studies (such as types, format of intervention, methods of outcome assessments, duration of follow-up, timing of assessment). We were unable to perform a meta-analysis and have conducted a narrative synthesis of the findings.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Patients requiring resuscitation or providers learning to deliver resuscitation

Intervention: Use of a cognitive aid

Comparator: No use of a cognitive aid

Outcome:

1. Patient survival
2. Quality of performance in actual resuscitations
3. Skill performance 1 year after course conclusion
4. Time to starting CPR between course conclusion and 1 year in simulated resuscitations
5. Chest compression rate between course conclusion and 1 year in simulated resuscitations
6. Chest compression depth between course conclusion and 1 year in simulated resuscitations
7. Chest compression fraction between course conclusion and 1 year in simulated resuscitations
8. Ventilation between course conclusion and 1 year in simulated resuscitations
9. Time to starting CPR at course conclusion in simulated resuscitations
10. Chest compression rate at course conclusion in simulated resuscitations
11. Chest compression depth at course conclusion in simulated resuscitations
12. Chest compression fraction at course conclusion in simulated resuscitations
13. Ventilation at course conclusion in simulated resuscitations
14. Knowledge at course conclusion

Study design: RCTs and nonrandomized studies (non-RCTs, interrupted time series, controlled before-and-after studies, cohort studies) are eligible for inclusion. Unpublished studies (eg, conference abstracts, trial protocols) were excluded.

Time frame: All years and all languages were included if there is an English abstract. Initial search was run July 17, 2019. The search was updated December 30, 2019.

PROSPERO registration submitted November 23, 2019

[H3] Consensus on Science

1. For the critical outcome of survival to hospital discharge, we identified no studies during cardiac arrest but found very-low-certainty evidence for trauma resuscitation in 3 studies (1 randomized trial[197] and 2 observational studies[198,199]), downgraded for risk of bias, indirectness, and imprecision. These studies enrolled 4659 patients, but not all studies reported numbers of patients who survived, so calculating overall OR was not possible.
2. For the important outcome of quality of performance in actual resuscitations, no studies during cardiac arrest were found, but very-low-certainty evidence for trauma resuscitation (1 randomized trial[197] and 3 observational studies[198-200]), downgraded for risk of bias, inconsistency, indirectness, and imprecision, was identified. These studies enrolled 5094 patients but reported quality of performance using different metrics, so calculating overall OR was not possible.

   Fitzgerald et al[197] reported fewer errors in teams who used a cognitive aid (incident rate ratio [RR], 0.889; 95% CI, 0.793–0.996; P=0.04) but found that compliance to trauma algorithms was not significantly improved with the use of a cognitive aid (incident RR, 1.020; 95% CI, 0.989–1.051; P=0.21).

   Lashosher et al[199] reported that almost all aspects of completing primary and secondary trauma surveys improved with using the cognitive aid and that ordering radiologic investigations improved with using a cognitive aid (P<0.001), except when ordering abdominal CT scans.

Bernhard et al[198] reported that time to completion of required radiologic investigations in trauma patients improved with using a cognitive aid except when ordering chest CTs in the most severely injured subset of patients. However, they found that teams performed more lifesaving interventions (laparotomy and decompressive craniectomy) when using a cognitive aid (19% preimplementation of cognitive aid versus 29% postimplementation; $P<0.05$). Kelleher et al[200] reported that most primary and secondary survey tasks were completed more consistently when teams used a cognitive aid. Primary and secondary survey tasks overall were more likely to be completed (primary survey: adjusted OR, 2.66 [95% CI, 2.07–3.42]; secondary survey: adjusted OR, 2.46 [95% CI, 2.04–2.98]).[200] The average adjusted time to task completion was 9 seconds (–0.15 minutes; 95% CI, –0.23 to –0.08 minutes) faster in the post–checklist implementation period.[200]

3. For the important outcome of skill performance 1 year from course conclusion in simulated resuscitations, we identified no studies.
4. For the important outcome of time to starting CPR between course conclusion and 1 year in simulated resuscitations, we identified very-low-certainty evidence in 1 randomized trial,[201] downgraded for indirectness and imprecision. This outcome was evaluated in only 4 resuscitation teams, and there was no difference (15 seconds without versus 14 seconds with cognitive aid).
5. For the important outcome of chest compression rate between course conclusion and 1 year in simulated resuscitations, we identified very-low-certainty evidence in 2 randomized trials,[202,203] downgraded for risk of bias, inconsistency, indirectness, and imprecision. Ward et al[202] found no significant differences in the percentages of lay provider participants who performed the correct compression rate with no cognitive aid using either a short or long

version of a checklist type of cognitive aid (43% control versus 34% short versus 54% long; not significant [NS]). Williamson et al[203] found a significantly higher chest compression rate in lay provider participants who used a cognitive aid (94.5/min control versus 99.0/min cognitive aid; $P<0.05$), but note that neither group achieved a mean rate within the recommended rates of 100 to 120/min.

6. For the important outcome of chest compression depth between course conclusion and 1 year in simulated resuscitations, we identified very-low-certainty evidence in 2 randomized trials,[202,203] downgraded for risk of bias, indirectness, and imprecision.
Ward et al[202] found no significant differences in the percentage of compressions with proper depth performed by lay provider participants who had access to either a short or long version of a checklist type of cognitive aid (34% control versus 34% short versus 43% long, NS). Williamson et al[203] found no significant differences in the percentage of compressions with proper depth performed by lay provider participants who had access to a cognitive aid (36.6 mm control versus 42.2 mm cognitive aid, NS). Note that neither group achieved a mean depth in the recommended range of 50 to 60 mm.
7. For the important outcome of chest compression fraction (CCF)/hands-off time (HOT) between course conclusion and 1 year in simulated resuscitations, we identified very-low-certainty evidence in 1 randomized trial,[201] downgraded for risk of bias, indirectness, and imprecision. No significant differences in percentage HOT were found when resuscitation teams used a cognitive aid (18.9% when 4 teams did not versus 15.8% when 4 teams did use a cognitive aid, NS).

8. For the important outcome of ventilations between course conclusion and 1 year in simulated resuscitations, we identified very-low-certainty evidence in 2 randomized trials,[202,203] downgraded for risk of bias, indirectness, and imprecision.
   Ward et al[202] found no significant differences in the percentage of ventilations with proper technique performed by lay provider participants who had access to either a short or long version of a checklist type of cognitive aid (50% control versus 47% short versus 56% long; NS).
   Williamson et al[203] found significant differences in the percentage of ventilations with proper tidal volume performed by lay provider participants who had access to a cognitive aid (audio prompts) (55.5% control versus 84.8% cognitive aid; $P<0.01$).
9. For the important outcome of time to start CPR at course conclusion in simulated resuscitations, we identified low-certainty evidence in 4 randomized trials[204-207] (downgraded for risk of bias, indirectness, and imprecision) and 1 observational study[201] (downgraded for risk of bias, indirectness, and imprecision). All studies demonstrated statistically significant and likely clinically significant delays in starting CPR for lay provider participants who used a cognitive aid compared with those who did not (Hunt: 78.2 seconds control versus 159.5 seconds cognitive aid, $P<0.001$[204]; Merchant: 18 seconds [95% CI, 15–21 seconds] control versus 48 seconds [95% CI, 47–49 seconds] cognitive aid[205]; Paal: 93.3 seconds control versus 165.3 seconds cognitive aid, $P<0.001$[206]; Rössler: 23 seconds control versus 63 seconds flowchart, $P<0.0001$[207]).
10. For the important outcome of chest compression rate at course conclusion in simulated resuscitations, we identified very-low-certainty evidence from 6 randomized trials,[202-207] downgraded for risk of bias, inconsistency, indirectness, and imprecision.

Hunt et al[204] reported no significant differences in mean chest compression rate between lay provider participants who used a cognitive aid and those who did not (117/second control versus 127.9/second cognitive aid; NS).

Merchant et al[205] reported a higher mean chest compression rate by lay provider participants who used a cognitive aid compared with those who did not (compression rate: 100/min [95% CI, 97–103/min] versus 44/min [95% CI, 38–50/min]).

Paal et al[206] reported a higher percentage of lay provider participants who used the correct chest compression rate when using a cognitive aid compared with those who did not (14% control versus 44% cognitive aid; *P*<0.001).

Rössler et al[207] reported no significant differences in mean chest compression rate delivered by lay provider participants who used a cognitive aid compared with those who did not (76/min control versus 78/min flowchart; NS).

Ward et al[202] reported no significant differences in percentage of lay provider participants who used a correct chest compression rate when using either a short or long version of a checklist type of cognitive aid compared with those who did not use a cognitive aid (45% control versus 50% short versus 51% long; NS).

Williamson et al[203] reported a higher mean chest compression rate delivered by lay provider participants who used a cognitive aid compared with those who did not (52.3/min control versus 87.3/min cognitive aid; *P*<0.01).

11. For the important outcome of chest compression depth at course conclusion in simulated resuscitations, we found low-certainty evidence from 5 randomized trials,[202,203,205-207] downgraded for risk of bias, indirectness, and imprecision. Only 1 study found a difference in chest compression depth achieved by lay provider participants but not in the recommended

range of depth: control 31 mm (95% CI, 38–44 mm) compared with cognitive aid 41 mm (95% CI, 28–34 mm).[205] All other studies showed no statistically significant difference in compression depth or percentage of compressions in the target range when using cognitive aids compared with not using cognitive aids.[202,203,206,207]

12. For the important outcome of CCF/HOT at course conclusion in simulated resuscitations, we found very-low-certainty evidence from 4 randomized trials,[204,205,207,208] downgraded for risk of bias, inconsistency, and indirectness.

    Hawkes et al[208] reported similar HOT in lay providers with and without a cognitive aid. Hunt et al[204] showed no difference in CCF if lay provider participants did or did not use cognitive aids, but they included time to starting CPR (75.4% control versus 72.2% cognitive aid; NS). However, the time to starting CPR was significantly longer in the cognitive aid group, so it is possible that CCF was actually better in the cognitive aid group, if time to starting CPR was taken into consideration.

    Merchant et al[205] showed a difference in CCF between lay provider participants who did and did not use cognitive aids (50.6% control versus 58.9% cognitive aid), and the use of the cognitive aid was also accompanied by a delay in time to starting CPR.

    Rössler et al[207] showed that if delays in starting CPR were accounted for, lay provider participants had lower HOT when using a cognitive aid compared with not using a cognitive aid (146 seconds control versus 87 seconds cognitive aid; $P<0.0001$).

13. For the important outcome of ventilations at course conclusion in simulated resuscitations, we found low-certainty evidence from 3 randomized trials.[202,203,206] Paal et al[206] reported that there was no difference in the percentage of participants who performed the correct ventilation rate when using or not using cognitive aids (15% control versus 20% cognitive

aid; NS). Ward et al[202] reported no differences in correct ventilations performed by lay provider participants using or not using a checklist type of cognitive aid (44% control versus 44% short versus 51% long; NS). Williamson et al[203] reported more ventilations performed with the correct technique by lay provider participants who used cognitive aids compared with those who did not (control 15% versus 51% cognitive aids; P<0.01).

14. For the important outcome of knowledge at course conclusion in simulated resuscitations, we found no studies.

[H3] Treatment Recommendations

We recommend against the use of cognitive aids for the purposes of lay providers initiating CPR (weak recommendation, low-certainty evidence).

We suggest the use of cognitive aids for healthcare providers during trauma resuscitation (weak recommendation, very-low-certainty evidence). In the absence of studies on CPR, no evidence-based recommendation can be made.

There are insufficient data to suggest for or against the use of cognitive aids in lay provider training.

We suggest the use of cognitive aids for training of healthcare providers in resuscitation (weak recommendation, very-low-certainty evidence).

[H3] Justification and Evidence-to-Decision Framework Highlights

The evidence-to-decision table is included in Appendix A-6. The EIT Task Force prioritized this topic because international resuscitation councils commonly provide cognitive aids to resuscitation course participants and healthcare organizations (algorithms, pocket cards, flowcharts, infographics, etc). However, it has not been determined if they are effective in improving patient outcomes or provider performance during resuscitation.

Cognitive aids may improve performance and patient outcomes by doing the following:

- Decrease cognitive load of individuals or team collectively[209]
- Assist memory; enhancing automatic, fast, subconscious decision-making or cognitive processes; and reducing the impact of stress and distraction on rapid, accurate decision-making[210]
- Standardize communication among resuscitation team members[211]
- Allow for better situation awareness/shared mental model among team members[212]

However, cognitive aids may do the following:

- Promote fixation errors and groupthink[213]
- Impair communication among team members[214]
- Be distracting, especially when not developed well (flow, color, how easy to read, confusing to follow, etc), so they may worsen performance/patient outcomes

Our recommendation has been divided into different contexts, because we believe that the evidence for routine implantation of cognitive aids during resuscitation and training is conflicting. For lay providers, there is consistent evidence that there are potentially clinically important delays in initiating CPR; however, the evidence for impact on other CPR quality metrics (eg, rate, depth, CCF) is less consistent.

There is almost no evidence for the use of cognitive aids by trained healthcare providers during CPR. However, there is substantial evidence, albeit inconsistent, showing that trauma resuscitation teams generally adhere to resuscitation guidelines better, make fewer errors, and perform key clinical tasks more frequently if they use cognitive aids. We believe that the trauma resuscitation environment is sufficiently similar to the CPR environment to enable extrapolation to our recommendation; however we appreciate that others may not agree with this. ~~We acknowledge that our assumption may be incorrect and that there may be important differences between the cardiac arrest and trauma resuscitation clinical environments.~~

When selecting our performance outcomes, we elected to include studies that measured data related to discrete tasks. There were many studies that used composite scores as their primary outcome (eg, score calculated based on completion of several clinical tasks). We excluded these studies for this SysRev, because it was very difficult to compare and consolidate the results.

None of the studies examined provided evidence to describe implementation concerns, eg, training or resource implications. However, it appears feasible to provide cognitive aids for resuscitation providers to use during training and actual resuscitation.

In the 2010 CoSTR, the use of checklists was described as reasonable during adult and pediatric ALS, provided that they do not delay the start of resuscitative efforts.[1,2] This 2020 treatment recommendation provides a more detailed insight into the limited evidence on cognitive aids during resuscitation.

[H3] Knowledge Gaps

- Real-life cardiac arrest studies: Given that resuscitation councils are de facto endorsing the use of cognitive aids by providing pocket cards and algorithm posters, there is an urgent need to adequately study the impact of cognitive aids in the real-world cardiac arrest environment.

- Simulated cardiac arrest studies with healthcare providers using cognitive aids: The 1 study that examines healthcare provider performance[201] is a very small proof-of-concept pilot study and was not sufficiently powered to be able to demonstrate any effects of cognitive aids on performance in this population. Future, larger studies in this area will allow us to strengthen our recommendation for this provider group.
- Human factors: There is no standard format to the types of cognitive aids developed and examined in the studies included in this SysRev. It is likely that providers respond differently to different kinds of cognitive aids, so it is very difficult to consolidate findings from different studies to form a unified conclusion.
- There is much known about how human beings interact with cognitive aids in other clinical (eg, World Health Organization Safe Surgery Checklist) and nonclinical environments (eg, aviation, power plants, and large-scale industry). However, for the scientific community to develop the most effective, targeted cognitive aid for resuscitation, the focus of research should be the impact on human factors, specifically situational awareness (eg, attention/distraction), cognitive load, and communication. This may help us better understand why cognitive aids seem to help providers perform some clinical tasks more completely and efficiently (eg, trauma primary and secondary survey tasks) but seem to impair the ability of providers to perform some other clinical tasks (eg, initiating CPR).

[H2] Team and Leadership Training (EIT 631: SysRev)

[H3] Rationale for Review

This CoSTR for EIT is based on the 2015 CoSTR for team and leadership training[3,4] Evidence for the effect of team and leadership training on educational and clinical outcomes was sought for adult, pediatric, and neonatal courses. The search also included advanced trauma life support courses. Leadership was defined in terms of the attributes of a leader or the process of leadership, and teamwork can be defined as the ability of team members to work together, communicate effectively, anticipate and meet each other's demands, and inspire confidence, resulting in a coordinated collective action.

Met opmerkingen [JF29]: ADD REFERENCE
Norris EM, Lockey AS. Human factors in resuscitation teaching. Resuscitation. 2012;83(4):423-7.
DOI: https://doi.org/10.1016/j.resuscitation.2011.11.001

Met opmerkingen [JF30]: ADD REFERENCE
Salas E, DiazGranados D, Klein C, Burke CS, Stagl KC, Goodwin GF, Halpin SM. Does Team Training Improve Team Performance? A Meta-Analysis. Human Factors. 2008;50(6):903-33.
DOI: https://doi.org/10.1518/001872008X375009

Because teamwork and leadership are increasingly recognized factors contributing to patient safety and outcome in healthcare,[215] these human factors are expected to make a significant contribution to patient outcome in the context of ALS.

Because of the high degree of heterogeneity in context, intervention, and the way outcomes were measured, no meta-analyses could be performed. The results are summarized in a narrative form.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Students who are taking ALS courses in an educational setting

Intervention: Inclusion of specific leadership or team training

Comparator: No such specific training

Outcome: Patient survival, skill performance in actual resuscitations, skill performance at 3 to 15 months (patient tasks, teamwork, leadership), skill performance at course conclusion (patient tasks, teamwork, leadership), and cognitive knowledge

Study design: RCTs and nonrandomized studies (non-RCTs, interrupted time series, controlled before-and-after studies, cohort studies) were eligible for inclusion. Studies evaluating scoring systems (no relevant outcome), studies with self-assessment as the only outcome, reviews, and abstracts without full articles were excluded.

Time frame: Because this is an update of a CoSTR published in 2015, PubMed was searched from January 1, 2014; Embase was searched from January 1, 1999; and the Cochrane database was searched for all years. The literature search was updated to November 28, 2019.

PROSPERO registration submitted January 3, 2020

[H3] Consensus on Science

For the critical outcome of patient survival, we found no randomized clinical trials, but we found very-low-certainty evidence from 3 observational studies (downgraded for risk of bias, indirectness, and imprecision),[195,216,217] all showing improved patient survival. Andreatta et al[195] reported hospital survival from pediatric cardiac arrest over a period of 4 years after implementation of a hospital-wide mock code program, which included team training. These authors found an increase in survival from pediatric cardiac arrest at their hospital during the study period (from 33% to 48% within 1 year) in increments that correlated with the increasing number of mock code events. Neily et al[216] reported hospital mortality in surgical patients at 74 hospitals in the United States that had implemented a surgical team training program. The 74 hospitals in the training program experienced an 18% reduction in annual mortality (RR, 0.82; 95% CI, 0.76–0.91; *P*=0.01) compared with a 7% decrease among the 34 hospitals that had not yet undergone training (RR, 0.93; 95% CI, 0.80–1.06; *P*=0.59). Clarke et al[217] studied if establishing a specialist, second-tier paramedic response for OHCA was feasible and reported a rate of ROSC of 22.5% (the national average was 16%).

For the critical outcome of skill performance in actual resuscitations, we found very-low-certainty evidence from a single RCT, [218] downgraded for risk of bias, indirectness, and imprecision. The study randomized 32 internal medicine residents to receive simulation training with a focus on the role of the resuscitation team leader compared with no additional training but did not find an effect on CPR quality during actual resuscitation of patients. We also found very-low-certainty evidence (downgraded for risk of bias, inconsistency, indirectness, and imprecision) from 4 observational studies[106,219-221] that reported improved CPR depth, rate, ratio, team communication, and improved deployment times of mechanical devices.

For the important outcome of skill performance at 3 to 15 months (patient tasks), we found very-low-certainty evidence from 3 randomized trials (downgraded for risk of bias, inconsistency, and imprecision) that reported improvement in patient tasks.[222-224]

Hunziker et al[222] compared instructions on resuscitation technique with instructions on leadership and communication in medical students during simulated cardiac arrest. Hands-on time was significantly longer in the leadership instruction groups (120 seconds [IQR, 98–135] versus 87 seconds [IQR, 61–108]; $P<0.001$). The time elapsed until CPR was started was significantly shorter in the leadership instruction group ($P<0.018$).

Thomas et al[223] studied interns for pediatrics and combined pediatrics and internal medicine, family medicine, emergency medicine, and obstetrics and gynecology. They compared team training in neonatal resuscitation using high- and low-fidelity manikins. They found no evidence that trained participants maintained more vigilance (median: 100% [control participants] versus 100% [intervention]; $P=0.951$) or workload management (median: 100% [control participants] versus 100% [intervention]; $P=0.549$) than did control participants. The intervention groups had shorter-duration resuscitations compared with control groups immediately after training (mean: 9.3 minutes [control participants] versus 8.3 minutes [intervention]; $P=0.314$).

Blackwood et al[224] randomized pediatric residents to a 1-hour crisis resource management (CRM) instruction or no additional training. The overall Ottawa Global Rating

Scale score (maximum=7) of the CRM group was 1.15 points (95% CI, 0.2–2.1; *P*=0.02) higher than the control group, and this increase was maintained at the 3-month retest scenario. The summative score of all 7 categories (out of 42) was 6.7 points (1.6–11.8; *P*=0.01) higher in the CRM group, and this difference remained at 3 months.

We found no observational studies for this outcome.

For the important outcome of skill performance at 3 to 15 months (teamwork), we found low-certainty evidence from a single randomized trial,[223] downgraded for bias and imprecision. Thomas et al[223] studied interns for pediatrics and combined pediatrics and internal medicine, family medicine, emergency medicine, and obstetrics and gynecology. They compared team training in neonatal resuscitation using high- and low-fidelity manikins. Interns who received team training demonstrated more frequent teamwork behaviors in the 6-month follow-up megacodes than did control participants (mean, 11.8 versus 10.0 behaviors per minute; *P*=0.03).

We also found very-low-certainty evidence (downgraded for risk of bias) from 2 observational studies that reported improved teamwork scores and faculty ratings after CPR team training.[225,226]

For the important outcome of skill performance at 3 to 15 months (leadership), we found moderate-certainty evidence from a single randomized trial, [222] downgraded for risk of bias. Hunziker et al[222] compared instructions on resuscitation technique with instructions on leadership and communication in medical students during simulated cardiac arrest. In the follow-up visit, more leadership utterances (7 [IQR, 4–10] versus 5 [IQR, 2–8]; *P*=0.02) were documented. We also found very-low-certainty evidence from 2 observational studies (downgraded for risk of bias and imprecision) that reported improved checklist scores and self-reported surveys after CPR team training.[226,227]

For the important outcome of skill performance at course conclusion (patient tasks), we found low-certainty evidence from 12 randomized trials,[222-224,228-236]} downgraded for risk of bias and imprecision. Eight of these 12 randomized trials[222-224,228,230-232,236] reported improvement in patient tasks, whereas 4 trials were neutral.[229,233-235]

Hunziker et al[228] compared the performance of teams of general practitioners and hospital physicians in simulated cardiac arrest with and without prior team training. Teams without prior teambuilding had less hands-on time during the first 180 seconds of the arrest (93±37 versus 124±33 seconds; P<0.0001), and they delayed their first defibrillation (67±42 versus 107±46 seconds; P<0.0001).

Thomas et al[223] studied interns for pediatrics and combined pediatrics and internal medicine, family medicine, emergency medicine, and obstetrics and gynecology. They compared team training in neonatal resuscitation using high- and low-fidelity manikins. Teams that had received team training completed the resuscitation an average of 2.6 minutes faster than did control participants, a time reduction of 24% (95% CI, 12%–37%).

Hunziker et al[222] compared instructions on resuscitation technique with instructions on leadership and communication among medical students during simulated cardiac arrest. The leadership instruction group demonstrated a longer hands-on time (120 seconds [IQR, 98–135] versus 87 seconds [IQR, 61–108]; P<0.001) and a shorter median time to start CPR (44 seconds [IQR, 32–62] versus 67 seconds [IQR, 43–79]; P=0.018).

Chung et al[229] compared training using a didactic lecture and simulation with debriefing with training using a resuscitation script among doctors and nurses. After training, there were no differences between the 2 groups in the score for performance in a simulated setting (control, 5.5±11.4 versus script, 4.7±9.6; P=0.838).

Castelao et al[230] compared video-based CRM training embedded in an ALS course for final-year medical students with a control group receiving additional ALS training. No-flow times were significantly lower in the CRM group (31.4 ± 6.1% versus 36.3 ± 6.6%; P=0.014).

Jankouskas et al[231] randomized nursing and medical students to BLS (using a bag-mask device and oxygen) plus CRM training or BLS only. CRM training predicted 13% of the variance in task management (P=0.05), and CRM training and situation awareness predicted 20% of the variance (P=0.04) in response time to chest compressions.

Fernandez et al[232] compared a 25-minute computer-based teamwork training with placebo training in medical students and emergency medicine residents. Teams in the training condition demonstrated better patient care (F1, 42=4.66; P<0.05; η=10%) than did teams in the placebo group.

Blackwood et al[224] randomized pediatric residents to a 1-hour CRM instruction or no additional training. The CRM group placed monitor leads 24.6 seconds earlier (P=0.02), placed an intravenous catheter 47.1 seconds sooner (P=0.04), called for help 50.4 seconds faster (P=0.03), and checked for a pulse after noticing a rhythm change 84.9 seconds quicker (P=0.01). There was no difference in the time to initiation of CPR.

Semler et al[233] compared 3 teamwork teaching modalities for incoming internal medicine interns: didactic, demonstration-based, or simulation-based instruction. Clinical performance scores in a simulated setting were similar between the 3 groups and correlated only weakly with teamwork behavior (coefficient of determination [R_s^2]=0.267; P<0.001).

Castelao et al[234] randomized teams of medical students to CRM team leader training or additional ALS training. In a simulated environment, CRM-trained team leaders showed better adherence to the ALS algorithm (difference, −6.4; 95 % CI −10.3,−2.4; *P*=0.002), but there was no improvement in no-flow time.

Couper et al[235] randomized healthcare providers with intermediate or advanced resuscitation training to receive standard mechanical chest compression device training or pit-crew device training (up to 1 hour). Regarding chest compression flow fraction in the minute preceding the first mechanical chest compression, pit-crew training was not superior to standard training (0.76 [95% CI, 0.73–0.79] versus 0.77 [95% CI, 0.73–0.82]; mean difference, −0.01 [95% CI, −0.06 to 0.03; *P*=0.572]).

Haffner[236] randomized final-year medical students to receive a 10-min computer-based CRM training or a control training on ethics. After the CRM training, team leaders corrected improper chest compressions (35.5%) significantly more often compared with controls (7.7%, *P*=0.03).

We also found very-low-certainty evidence from 4 observational studies[237-240] (downgraded for risk of bias and indirectness) that showed improved resuscitation skills (time to initiation of chest compression, correct positioning of defibrillator electrodes, time to defibrillation, shorter pre-shock pauses etc) and improved simulated survival.

For the important outcome skill performance at course conclusion (teamwork), we found low-certainty evidence from 10 randomized trials,[223,224,229,231-233,235,241-243] downgraded for risk of bias and imprecision. Seven out of these 10 randomized trials showed improved teamwork whereas 3 trials were neutral.[229,233,242]

Thomas[241] randomized interns to receive a neonatal resuscitation course with team training or a standard course. The interns with team training exhibited more frequent team behaviors (number of episodes per minute (95% CI)) than interns in the control group: information sharing 1.06 (0.24, 1.17) versus 0.13 (0.00, 0.43); inquiry 0.35 (0.11, 0.42) versus 0.09 (0.00, 0.10); assertion 1.80 (1.21, 2.25) versus 0.64 (0.26, 0.91); and any team behavior 3.34 (2.26, 4.11) versus 1.03 (0.48, 1.30) (*P*<0.008 for all comparisons).

Thomas[241] studied interns for pediatrics, combined pediatrics and internal medicine, family medicine, emergency medicine, and obstetrics and gynecology. They compared team training in neonatal resuscitation using high and low fidelity manikins. The high-fidelity team training showed more teamwork than control participants (12.8 versus 9.0 behaviors per minute; *P*<0.001). Team training groups had better workload management (control participants: 89.3%; low-fidelity training group: 98.0% [*P*<0.001]; high-fidelity training group: 98.8%; high-fidelity training group compared with control participants [*P*<0.001)].

Met opmerkingen [GR31]: That should be 223

Chung[229] compared training using a didactic lecture and simulation with debriefing with training using a resuscitation script in doctors and nurses. There were no differences in the score improvement after training between the 2 groups in dynamics (C: 9.16±12.6 versus S: 7.4±13.7, *P*=0.715), performance (C: 5.5±11.4 versus S: 4.7±9.6, *P*=0.838) and total scores (C: 14.6±20.1 versus S: 12.2±19.5, *P*=0.726).

Jankouskas[231] randomized nursing and medical students to BLS (using a bag-mask device and oxygen) plus CRM training or BLS only. CRM training predicted 13% in task management (*P*=0.05), 15% of the variance in teamworking (*P*=0.04), and 18% of the variance in situation awareness (*P*=0.03).

Fernandez[232] studied a 25-minute computer-based teamwork training versus placebo training among medical students and emergency medicine residents. Teams in the training group demonstrated better teamwork (F[1, 42]=4.81, *P*<0.05; η=10%).

Blackwood[224] randomized pediatric residents to a 1-hour CRM instruction or no additional training. The intervention group had overall CRM performance scores 1.15 points higher (Ottawa Global Rating Scale) out of 7 (P=0.02).

Semler[233] compared 3 teamwork teaching modalities for incoming internal medicine interns: didactic, demonstration-based, or simulation-based instruction. The average overall Teamwork Behavioral Rater score for those who received demonstration-based training was similar to simulation participation (4.40±1.15 versus4.10±0.95, P=0.917) and significantly higher than didactic instruction (4.40±1.15 versus 3.10±0.51, P=0.045).

Rovamo[242] evaluated the impact of CRM and anesthesia nontechnical skills instruction on teamwork during simulated newborn emergencies performed by doctors and nurses. They could not show that the CRM instruction improved teamwork performance.

Lorello[243] studied mental rehearsal of advanced trauma life support by residents in anesthesiology, emergency medicine, and surgery. The mental practice group engaged in 20 minutes of mental practice, and the control group received 20 minutes of advanced trauma life support training. The mental practice group showed improved teamwork behavior as assessed by the Mayo High Performance Teamwork Scale (r=0.67, P<0.01).

Couper[235] randomized health providers with intermediate or advanced resuscitation training to receive standard mechanical chest compression device training or pit-crew device training (up to 1 h). PIT-crew training did not result in improvement of the global Team Emergency Assessment Tool score (out of 10): PIT-crew training 8.1 (7.2–8.9) versus standard training 7.9 (7.3–8.6); mean difference, 0.15 (95% CI, −0.87 to 1.17), P=0.760.

We also found very-low-certainty evidence from 3 observational studies[225,226,238] (downgraded for risk of bias, inconsistency, indirectness, and imprecision) that found improved teamwork scores and faculty ratings after CPR team training.

For the important outcome skill performance at course conclusion (leadership) we found low-certainty evidence from 6 randomized trials,[222,228,230,234,236,244] downgraded for risk of bias and imprecision. Of these trials, 5 out of 6 showed improved leadership, whereas 1 trial was neutral.[230]

Cooper[244] studied the effect of a 75-minute leadership seminar during an ALS course for doctors, nurses and technicians. The leadership training program improved the leadership performance in a simulated setting.

Hunziker[228] compared the performance of teams of general practitioners and hospital physicians in simulated cardiac arrest with and without prior team training. Teams without prior team training made less leadership statements during simulated cardiac arrest (15±5 versus 21±6, $P<0.0001$).

Hunziker[222] compared instructions on resuscitation technique with instructions on leadership and communication in medical students during simulated cardiac arrest. The leadership instruction group demonstrated more leadership utterances compared with the control group (7 [IQR, 4–10] versus 5 [IQR, 2–8]; $P=0.02$).

Castelao[230] compared video-based CRM training embedded in an ALS course for final year medical students with a control group receiving additional ALS training. They could not show an association between team leader verbalization of instructions and no-flow time.

Castelao et al[234] randomized teams of medical students to CRM team leader training or additional ALS training. Significantly higher team leader verbalization proportions were found for the team leader training group: direct orders (difference, –1.82; 95% CI −2.4,−1.2; $P<0.001$), undirected orders (difference, −1.82; 95 % CI, −2.8, −0.9), $P<0.001$), planning (difference, −0.27; 95 % CI, −0.5,−0.05; $P=0.018$), and task assignments (difference, −0.09 (95% CI, −0.2, −0.01; $P=0.023$).

Haffner et al[236] randomized final-year medical students to receive a 10-minute computer-based CRM or a control training on ethics. Communication quality assessed by the Leader Behavior Description Questionnaire significantly increased in the intervention group by a mean of 4.5 compared with 2.0 ($P=0.01$) in the control group.

We also found very-low-certainty evidence from 3 observational studies [226,227,239] (downgraded for risk of bias, indirectness, and imprecision) that showed improved checklist scores and self-reported surveys after CPR team training.

For the important outcome of cognitive knowledge, we found no evidence.

[H3] Treatment Recommendations

We suggest that specific team and leadership training be included as part of ALS training for healthcare providers (weak recommendation, very-low-certainty evidence).

[H3] Justification and Evidence-to-Decision Framework Highlights

The evidence-to-decision table is included in Appendix A-7. The relevance of this review is further supported by the observations in 1999 by Cooper, who reported that leadership during resuscitation is associated with team performance and that, therefore, leadership training should be provided.[245]

In 2015, the EIT Task Force recommended team and leadership training in ALS courses (weak recommendation, low-quality evidence).[3,4] The current review supports this statement.

Although our current review identified many new studies since the 2015 CoSTR, no RCT addressed the most critical outcome of patient survival. On the other hand, we found 3 observational studies[195,216,217] for this critical outcome of patient survival, but they suffer from risk of bias, indirectness, and imprecision.

In making our recommendation about team and leadership training in ALS courses, we have placed emphasis on the potential benefit, lack of harm, and high level of acceptance of team and leadership training and lesser value on associated costs.

In the studies, many different methods to train leadership and team behavior were reported: through eLearning, video-based training, instruction, demonstration, low-fidelity simulation, or high-fidelity simulation. Team and leadership training may be delivered as an add-on training module to an ALS course, or as an integral part of an ALS course. As such, there was considerable heterogeneity in the studies analyzed. The EIT Task Force was of the opinion that the integration of team and leadership training in ALS courses may promote its sustainability. In addition to team and leadership training, sufficient exposure to resuscitation may be required to achieve improved patient outcome.

This update of the 2015 treatment recommendation[3,4] still favors leadership training during advanced resuscitation education.

[H3] Knowledge Gaps

- What is the most effective/efficient method of team and leadership training (eLearning, instruction, demonstration, simulation training, other) and assessment?
- How do team training and leadership training interact, and what is their relative importance? Is training of the leader more efficient than training of the team?

- What is the effect of team and leadership training on patient outcome (there are no RCTs)?
- How do team/leadership training and provider experience/exposure to resuscitation interact?
- Are there any downsides of leadership training on resuscitation performance (eg, delay of initiating CPR, stress for the leader or the team)?

[H2] Learning Formats Preceding Face to Face Training in Advanced Courses (formerly: Precourse Preparation for Advanced Courses (EIT 637: SysRev)

[H3] Rationale for Review

This review is a follow up to the CoSTR published in 2015 ('Precourse preparation for advanced life support (ALS) courses'), which was based on one study.[3,4] The task force concluded in 2015 that a specific recommendation was too speculative. Since then, blended learning approaches have been developed for ALS courses. As the term 'blended learning' is highly context specific, a clear definition is not possible. From a broad perspective, any type of learning format preceding face to face training may be regarded as part of the course. This topic was prioritized by the EIT Task Force because of the recent dynamic development of online learning (blended learning) with the aim of reducing face to face training time. To account for the different learning formats, we report the results of the search separately for studies (a) comparing the distribution of precourse learning material with no distribution, and (b) comparing any kind of blended learning format that reduces face to face training with traditional courses. Because of the high degree of heterogeneity with context, intervention, and the way outcomes were measured, no meta-analyses could be performed. The results are summarized in a narrative form.

Met opmerkingen [BJ32]: Add referece: Moskal P, Dziuban C, Hartman J. "Blended learning: A dangerous idea?". *Internet and Higher Education*. 2012;**18**:15–23.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Students who are taking ALS courses in an educational setting

Intervention: Precourse preparation for advanced courses (eg, eLearning or pretesting combined with face-to-face training)

Comparator: Traditional course (face-to-face training)

Outcome: Cognitive knowledge, skill performance at course conclusion, skill performance at 1 year, skill performance in actual resuscitations, increased survival rates, and skill performance at time between course conclusion and 1 year

Study design: All comparative, human studies (prospective and retrospective) examining the use of precourse preparation for ALS training and reporting knowledge/skills outcomes. Also, patient outcomes and performance in actual resuscitation situations. Unpublished studies (eg, conference abstracts, trial protocols) were excluded.

Time frame: All years and all languages were included if there was an English abstract. Literature search was updated to November 20, 2019.

PROSPERO registration submitted [160799] December 2, 2019

[H3] Consensus on Science

The question of ***providing learning resources prior to a face to face course*** was addressed by two RCTs [246,247]. One study compared the 2-week access to an online advanced cardiovascular life support [ACLS] simulator with no access to such a simulator, [246] and the other study provided a Microsim CD as precourse material and compared it with no CD distribution. [247] The heterogeneous nature of the studies prevented pooling of data for any outcome; therefore, no meta-analysis was performed.

Neither of the studies addressed the critical educational outcomes of skill performance 1 year after course conclusion and skill performance between course conclusion and 1 year.

Furthermore, neither study addressed the important educational outcomes of quality of performance in actual resuscitations or patient survival with favorable neurologic outcome.

For the important educational outcome of skill performance at course conclusion, we found low-certainty evidence (downgraded for risk of bias and imprecision) from the two RCTs. The first study,[246] with 65 medical students, found no influence on time to initiate chest compressions but significant advantages in the intervention group for the time to defibrillate ventricular fibrillation (112 seconds versus 140 seconds; $P<0.05$) and pacing of symptomatic bradycardia (95 seconds versus 155 seconds; $P<0.05$). The second RCT, with 572 participants of ALS courses[247] distributing a Microsim CD before the course to the intervention group, found no significant differences in performance between intervention and control during a standardized cardiac arrest scenario test at course conclusion (I: 93.6% versus C: 91.8%; $P=0.4$).

For the important educational outcome of knowledge at course conclusion, we found low-certainty evidence (downgraded for risk of bias and imprecision) reported by one RCTs. The 1 RCT, with 572 participants of ALS courses,[247] that distributed a Microsim CD to the intervention group before the face-to-face ALS course found no significant differences of postcourse MCQ scores between the groups (C: 101.9 [SD 13.8] versus I: 101.4 [SD 13.9]; $P=0.7$).

The question of analyzing ***blended learning formats to reduce face to face time in ALS courses*** compared with traditional courses was addressed by one RCT [248] and two non-RCTs.[249,250] The heterogeneous nature of the studies prevented pooling of data for any outcome; therefore, no meta-analysis was performed.

None of the studies addressed the critical educational outcomes of skill performance 1 year after course conclusion and skill performance between course conclusion and 1 year.

Furthermore, no studies addressed the important educational outcomes of quality of performance in actual resuscitations or patient survival with favorable neurologic outcome.

For the important educational outcome of skill performance at course conclusion, we found low-certainty evidence (downgraded for risk of bias and imprecision) from one RCT and two non-RCTs [248 249 250]. The one RCT randomizing 3732 participants of ALS courses to either 6 to 8 hours of e-learning plus 1 day of face to face training or to a traditional 2-day face to face ALS course.[248] This study was inconclusive in demonstrating non inferiority in the intervention group (C: 80.2% versus I: 74.5%; mean difference, −5.7%; 95% CI, −8.8% to −2.7%). The first non-RCT, with 96 ACLS course participants,[249] comparing 6 hours of online lectures plus a 1-day face to face training with a traditional 2-day face to face course, showed that cardiac arrest scenario test pass rates did not differ statistically (C: 87.5% versus I: 95.8%; *P*=0.13). The second non-RCT compared 27170 participants of ALS courses [250] who underwent either 6 to 8 hours of eLearning plus 1 day of face-to-face training or a traditional 2-day face to face ALS course. In this study, the first-attempt cardiac arrest scenario test pass rate was significantly higher in the intervention group (84.6% versus 83.6%; *P*=0.035); however, the absolute educational effect was very low (difference: 1.0% first-attempt cardiac arrest scenario test pass rate).

For the important outcome of knowledge at course conclusion, we also found very-low-certainty evidence (downgraded for risk of bias and imprecision) reported by one RCT and two non-RCTs [248 249 250]. The RCT, randomizing 3732 participants of ALS courses to either 6 to 8 hours of e-learning plus 1 day of face to face training or to a traditional 2-day ALS course,[248] reported no statistical difference for end-of-course MCQ test scores (I: 88.96% versus C: 89.54%; adjusted difference, 0.55%; CI, –1.11% to 0.02%; *P*=0.054). The first non-RCT, with

96 ACLS course participants[249] comparing 6 hours of online lectures plus a 1-day face-to-face course with a traditional 2-day face-to-face course, showed that MCQ pass rates at course conclusion did not differ statistically (C: 85.4% versus I: 95.8%; *P*=0.08). The second study, including 27-170 participants of ALS courses,[250] compared 6 to 8 hours of eLearning plus 1 day of face-to-face training with a traditional 2-day face-to-face ALS training. The intervention group scored significantly higher (I: 87.9% versus C: 87.4%; *P*<0.001); however, the absolute difference of 0.5% was not found to represent educational significance.

[H3] Treatment Recommendations

We recommend distributing precourse learning formats preceding face to face training for participants of ALS courses (weak recommendation, very-low- to low-certainty evidence). In addition, we strongly recommend providing the option of e-learning as part of a blended learning approach to reduce face to face training time ALS courses (strong recommendation, very-low- to low-certainty evidence).

[H3] Justification and Evidence-to-Decision Framework Highlights

The evidence-to-decision table is included in Appendix A-8. Given the higher flexibility for learners and the savings of resources, the EIT Task Force strongly recommends providing the option of such formats for ALS courses (eg, a 1 day's equivalent of eLearning plus 1 day of a face-to-face course). In making this recommendation, the task force takes into account that learning styles may differ substantially and that face-to-face courses may be more effective for some groups of learners.

By implementing such programs, the return of investment of eLearning will be more pronounced if materials can be used by larger groups of learners. It should therefore be considered to develop materials collectively by several providers to save resources (ie, on a national level). However, it should also be taken into account that learners will profit most if the material is produced in the learners' native cultural context. The EIT Task Force emphasizes that close monitoring and evaluation within accredited courses is recommended and appears feasible. The EIT Task Force considers the inclusion of eLearning as a substitute for a part of the ALS course, but the PICOST question left the amount and format of the precourse preparation open. This decision was based on the consideration that the final goal of providing precourse material was to realize an increase of learner flexibility and savings of resources.

For the case of learning formats as a preparation of a traditional course desirable consequences probably outweigh undesirable consequences in most settings while in the case of e-learning formats as part of a blended learning the desirable consequences clearly outweigh undesirable consequences.

In 2015, the EIT Task Force estimated the effect so low that a specific recommendation for or against precourse preparation in ALS courses was too speculative.[3,4] In 2020, the evidence for an effect of precourse preparation is still limited. The TF task force nonetheless recommends providing learning formats as precourse preparation for advanced courses, even though the certainty of the evidence found was very low to low. The TF takes into account that for nearly all ALS courses worldwide, course organizers provide learning formats preceding face to face training as precourse preparation, mostly in form of reading or e-learning. Furthermore, the task force strongly recommends providing the option of e-learning as part of a blended learning approach to reduce face to face training.

[H3] Knowledge Gaps

- No studies were identified evaluating effects of learning formats preceding face to face training on long-term retention or on outcomes related to real life (performance in resuscitations, patient survival).
- Also, no studies addressed different formats of delivery (eg, invested time for preparation, educational involvement of learners, linkage to face-to-face training) or the content covered by the learning formats preceding face to face training.
- Evidence is needed for other formats of resuscitation courses (eg, BLS, pediatric ALS).

[H2] Rapid Response Systems in Adults (EIT 638: SysRev)

[H3] Rationale for Review

Unwell patients admitted to hospital are at risk of deterioration that may progress to cardiorespiratory arrest. Patients commonly show signs and symptoms of deterioration for hours or days before cardiorespiratory arrest.[251] Rapid Response Systems (RRSs) are programs that are designed to improve the safety of hospitalized patients whose condition is deteriorating quickly.[252] A successful RRS may be defined as a hospital-wide system that ensures observations, detection of deterioration, and tailored response to ward patients that may include RRT, also called a Medical Emergency Team (MET).[253] There is uncertainty as to whether these systems are effective in improving patient outcomes (eg, improving patient survival, reducing the number of cardiac arrests).

There was high heterogeneity among studies. The overall certainty of evidence was rated as very low to low for all outcomes primarily because of a very serious risk of bias. The individual studies were all at a serious to critical risk of bias. Because of this and a high degree of heterogeneity, no meta-analyses were performed and, instead, we have conducted a narrative synthesis of the findings.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Adults who are at risk of cardiac or respiratory arrest in hospital

Intervention: Introduction of an RRS (includes Rapid Response Teams (RRT) or MET)

Comparator: No RRS

Outcome: Survival to hospital discharge with good neurologic outcome, survival to hospital discharge, and in-hospital incidence of cardiac/respiratory arrest

Study design: RCTs and nonrandomized studies (non-RCTs, interrupted time series, controlled before-and-after studies, cohort studies) were included. All languages were included if there was an English abstract available.

Time frame: The literature search of the 2015 CoSTR was updated to December 10, 2019.

PROSPERO registration CRD42019160097

[H3] Consensus on Science

For the critical outcome of hospital discharge with favorable neurologic outcome, we did not find any study.

For the critical outcome of survival to hospital discharge, we have found low-certainty evidence (downgraded for risk of bias and inconsistency) from 2 RCTs[254,255] and very-low-certainty evidence (downgraded for risk of bias, inconsistency, and indirectness) from 35 non-RCTs.[256-292]

Of the 2 RCTs, 1 demonstrated no significant difference between control hospitals (functioned as usual) and intervention hospitals (introduced a MET team) for both unadjusted

(P=0.564; Diff, −0.093; 95% CI, −0.423 to 0.237) and adjusted (P=0.752; OR, 1.03; 95% CI, 0.84–1.28) survival.[255] The other study demonstrated a significant difference between control wards and intervention wards (introduction of a critical care outreach service) with all patients (OR, 0.70; 95% CI, 0.50–0.97) and matched randomized patients (OR, 0.52; 95% CI, 0.32–0.85).[254]

Of the 34 nonrandomized studies reporting mortality, no studies reported statistically significant worse outcomes for the intervention. For studies not reporting adjusted outcomes:

- Sixteen studies with no adjustment demonstrated no significant improvement.[259,260,262,264-266,271,272,274,276,278,280-282,287,290]
- Ten studies with no adjustment demonstrated significant improvement.[257,258,273,275,283,286,288,289,291,292]
- One study with no adjustment reported on rates, which improved with MET but did not report on significance.[261]
- One study with no adjustment demonstrated significant improvement for medical patients but not surgical patients (combined significance not reported).[277]

For studies reporting adjusted outcomes:

- Three studies with adjustment demonstrated significant improvement both before and after adjustment. [267,270,284]
- Three studies with adjustment demonstrated significant improvement before adjustment but not after adjustment.[268,285,293]
- Two studies with adjustment demonstrated no significant improvement both before and after adjustment.[256,263]

- One study that reported on both unexpected mortality and overall mortality showed significant improvement both before and after adjustment for unexpected mortality but no significant improvement both before and after adjustment for overall mortality.[269]
- One before-and-after study that presented "after" data for unexpected mortality in 3 separate time bands demonstrated significant improvement in time band 3 before adjustment and in time bands 2 and 3 after adjustment.[279]

The heterogeneous nature of the studies prevents pooling of data; however, there is a suggestion of improved hospital survival in those hospitals that introduce an RRS and a suggestion of a dose-response effect, with higher-intensity systems (eg, higher RRS activation rates, senior medical staff on RRS teams) being more effective.

For the critical outcome of in-hospital incidence of cardiac arrest, we found low-certainty evidence (downgraded for risk of bias and indirectness) from 1 RCT[255] and very-low-certainty evidence (downgraded for risk of bias, inconsistency, and indirectness) from 33 further non-RCTs.[256-262,264,266-270,273-275,277,278,280-284,286,288,294-298]

For the 1 RCT,[255] there was no significant difference between control hospitals and intervention hospitals, for both unadjusted (*P*=0.306; Diff, −0.208; 95% CI, −0.620 to 0.204) and adjusted (*P*=0.736; OR, 0.94; 95% CI, 0.79–1.13) analyses.

Of the 32 observational studies reporting on cardiac arrest rates:

- Seventeen studies with no adjustment demonstrated significant improvement in cardiac arrest rates after the introduction of a MET system.[258,261,262,267,268,270,273,275,277,280,283,290,292,295-297,299]

- Seven studies with no adjustment demonstrated no significant improvement in cardiac arrest rates after the introduction of a MET system[260,264,266,274,278,281,282]
- One before-and-after study using an aggregated weighted scoring system (Modified Early Warning Score) reported significantly higher cardiac arrest rates in Modified Early Warning Score bands 3 to 4 after intervention but not in Modified Early Warning Score bands 0 to 2 or 5 to 15, and overall cardiac arrest rate significance was not reported.[259]
- Three studies with adjustment demonstrated significant improvement in cardiac arrest rates after the introduction of an RRS both before and after adjustment.[257,284,294]
- One study with contemporaneous controls demonstrated no significant improvement in cardiac arrest rates after the introduction of an RRS both before and after adjustment.[256]
- One study with contemporaneous controls demonstrated significant improvement in cardiac arrest rates after the introduction of an RRS both before and after adjustment.[284]
- One study with adjustment demonstrated significant improvement before adjustment for whole of hospital and non–intensive care unit cardiac arrest rates, but only for non–intensive care unit cardiac arrest rates after adjustment.[263]
- One before-and-after study that presented “after” unadjusted data for cardiac arrest in 3 separate time bands demonstrated significant improvement in time bands 2 and 3.[269]

The heterogeneous nature of the studies prevents pooling of data. However, there is a suggestion of a reduced incidence of cardiac arrest in those hospitals that introduce an RRS and a suggestion of a dose-response effect, with higher-intensity systems (eg, higher RRS activation rates, senior medical staff on RRS teams) being more effective.

[H3] Treatment Recommendations

We suggest that hospitals consider the introduction of a rapid response system (RRS) (RRT/MET) to reduce the incidence of IHCA and in-hospital mortality (weak recommendation, low-certainty evidence).

[H3] Justification and Evidence-to-Decision Framework Highlights

The evidence-to-decision table is included in Appendix A-9. The task force places a high value on the outcomes—the prevention of IHCA and death—relative to the likely substantial cost of the system. RRSs have been successfully implemented in many healthcare settings worldwide.[300]

RRS is recommended by the Institute for Healthcare Improvement[301] and other national patient safety initiatives around the world.

There may be a role for an RRS in patients with end-of-life care[302] and also in reduction of medical errors.[303]

Careful consideration needs to be given to the elements of such systems. Effective afferent (detection and activation) and efferent limbs (RRS/MET response) may need the support of administrative and quality improvement strategies.[304]

Adequate resources should be dedicated to such systems to include (a) staff education about the signs of patient deterioration; (b) appropriate and regular vital signs monitoring of patients; (c) clear guidance (eg, alert systems or early warning scores) to assist staff in the early detection of patient deterioration; (d) a clear, uniform system of tiered clinical response; and (e) a clinical response to calls for assistance. The optimal method of patient monitoring and delivery of these components remains unclear.[1,2,305]

The performance of RRSs should be monitored and used as part of a quality improvement program of healthcare organizations. The "Recommended Guidelines for Monitoring, Reporting, and Conducting Research on Medical Emergency Team, Outreach, and Rapid Response Systems: An Utstein-Style Scientific Statement"[306] should be used by hospitals to collect the most meaningful data to optimize system interventions and improve clinical outcomes. This update of the 2015 CoSTR confirms the recommendation to implement RRSs.

[H3] Knowledge Gaps

- There is lack of evidence on long-term survival with favorable neurologic outcomes.
- What is the role of technology in RRSs (eg, remote monitoring, wearable devices)?
- What are the ideal components of the afferent limb of an RRS, eg, which vital signs, observations, and/or laboratory parameters, and with what frequency?
- What are the ideal components of an education program in the recognition of a deteriorating patient?
- What is the ideal mechanism for escalation for assistance (eg, conventional escalation versus automated electronic escalation)?
- What is the ideal makeup of the efferent limb (the response team)?
- What are the causes of failure to rescue or underutilization of RRSs?
- What is the cost-effectiveness of an RRS?

[H2] End-of-Course Testing Versus Continuous Assessment (EIT 643: SysRev)

[H3] Rationale for Review

This PICOST was prioritized by the EIT Task Force on the basis of the ongoing discussion about developing more appropriate assessment methods in resuscitation courses. Current educational literature reports positive educational effects of end-of-course testing.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Participants undergoing BLS/ALS courses

Intervention: End of course testing

Comparator: Continuous assessment and feedback

Outcome: Cognitive knowledge and/or skill performance at course conclusion, skill performance at time between course conclusion and 1 year, skill performance at 1 year, skill performance in actual resuscitations, and increased survival rates

Study design: All comparative, human studies (prospective and retrospective) in ALS training and reporting knowledge/skills outcomes; also, patient outcomes and performance in actual resuscitation situations

Time frame: All years and all languages were included if there was an English abstract; unpublished studies (eg, conference abstracts, trial protocols) were excluded. Literature search was updated to November 28, 2019.

PROSPERO registration submitted December 3, 2019

[H3] Consensus on Science

No studies were found that addressed the PICOST question.

We identified 3 studies[307-309] that analyzed the educational effect of end-of-course testing (without comparing it with continuous assessment).

[H3] Treatment Recommendations

Given that no evidence was identified, we are unable to make a recommendation.

[H3] Knowledge Gaps

- Evidence is needed for the most appropriate way to assess competence of candidates attending resuscitation courses (eg, continuous assessment versus end-of-course testing).

[H2] Virtual Reality, Augmented Reality, and Gamified Learning (EIT 4005: EvUp)

An EvUp was performed (Appendix C-5) with several studies identified that suggest the need for consideration of a SysRev, especially because no former assessment on the training of laypersons was done by ILCOR and no treatment recommendation was issued as of January 31, 2020.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Learners (ie, lay responders and/or healthcare providers) who are taking BLS or ALS training

Intervention: Use of virtual reality/augmented reality/gamified learning

Comparator: None of these

Outcome: Skill performance at course conclusion, skill retention beyond course conclusion, performance in actual resuscitations, or patient outcomes

Study design: All comparative, human studies (prospective and retrospective)

Time frame: All languages were included if there was an English abstract; unpublished studies (eg, conference abstracts, trial protocols) were excluded. Literature search was from January 1, 2013, to September 30, 2019.

No ILCOR review of this topic has been done previously. An EvUp was conducted for 2020 by the AHA. A search conducted in PubMed, Scopius, and Embase yielded 180 studies, and a total of 13 articles were reviewed exploring gamified learning (9) and virtual reality (4). The complete EvUp is included in Appendix C-5.

[H3]Treatment Recommendation

This EvUp does not enable a treatment recommendation to be made.

[H2] In Situ Training (EIT 4007: EvUp)

An EvUp was performed (Appendix C-6) with several studies identified that suggest the need for consideration of a SysRev. No previous review on the training of laypersons has been done by ILCOR, and there was no treatment recommendation as of January 31, 2020.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Healthcare providers

Intervention: In situ (workplace-based) simulation-based resuscitation training

Comparator: No in situ (workplace-based) simulation-based resuscitation training

Outcome: Learning, performance, and patient outcomes

Study design: All comparative, human studies (prospective and retrospective) with all different designs examining the effect of in situ simulation relative to conventional training or no intervention on learning outcome of learners, clinical performance, and patient outcomes

Time frame: All languages were included if there was an English abstract; unpublished studies (eg, conference abstracts, trial protocols) were excluded. Literature search was from January 1, 2013, to October 20, 2019.

An EvUp was conducted for 2020 by the AHA. A search conducted in PubMed yielded 791 studies and 15 were identified as relevant. The complete EvUp is included in Appendix C-6

[H3]Treatment Recommendation

This EvUp does not enable a treatment recommendation to be made.

[H2] High-fidelity manikins for ALS training (EIT 623: EvUp)

The topic of high-fidelity training in advanced life support courses was last reviewed in 2015. An EvUp was performed (Appendix C-7) with several studies identified that suggest the need for consideration of a SysRev.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Participants undertaking ALS training in an education setting

Intervention: Use of high-fidelity manikins

Comparator: Use of low-fidelity manikins

Outcome: Patient outcomes, skill performance in actual resuscitations, skill performance at 1 year, skill performance at time between course conclusion and 1 year, skill performance at course conclusion, and cognitive knowledge

Study design: All comparative, human studies (prospective and retrospective) examining the use high versus low fidelity manikins for ALS training and reporting knowledge/skills outcomes. Also, patient outcomes and performance in actual resuscitation situations.

Time frame: All years and all languages were included if there was an English abstract; unpublished studies (eg, conference abstracts, trial protocols) were excluded. Literature search was from January 1, 2013, to October 2, 2019.

An EvUp was conducted for 2020 by the AHA. A search conducted in PubMed, Scopus, and Embase yielded 109 studies, and 3 were identified as relevant. The complete EvUp is included in Appendix C-7.

[H3]Treatment Recommendation

This treatment recommendation is unchanged from 2015.[3,4] We suggest the use of high-fidelity manikins when training centers/organizations have the infrastructure, trained personnel, and resources to maintain the program (weak recommendations, very-low-quality evidence). If high-fidelity manikins are not available, we suggest that the use of low-fidelity manikins is acceptable for standard ALS training in an educational setting (weak recommendations, low-quality evidence).

[H1] Measuring CPR Performance, Feedback Devices, and Debriefing

[H2] Debriefing of Resuscitation Performance (EIT 645: SysRev)

[H3] Rationale for Review

This PICOST was an update of the 2015 CoSTR,[3,4] which was based on only 2 studies. For the purpose of this review, *briefing* was defined as a process of reviewing and communicating pertinent facts about the resuscitation before the event,[310] and *debriefing* was defined as a postevent discussion between 2 or more individuals in which aspects of performance are analyzed, with the aim of improving future performance.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Rescuers who are caring for patients in cardiac arrest in any setting

Intervention: Briefing or debriefing

Comparator: No briefing or debriefing

Outcome: Survival, skill performance in actual resuscitations, quality of resuscitation (eg, reduce hands-off time, allowing for continuous compressions), and cognitive knowledge

Study design: RCTs and nonrandomized studies (non-RCTs, interrupted time series, controlled before-and-after studies, cohort studies) of healthcare providers, IHCA or OHCA, and debriefing intervention were included. Exclusion criteria were debriefing as part of quality intervention bundle and debriefing after simulated cardiac arrest. All languages were included if there was an English abstract available.

Time frame: Because this is an update of the 2015 CoSTR, the literature search was from January 1, 2014, to September 30, 2019.

PROSPERO registration submitted December 1, 2019

[H3] Consensus on Science

There were no studies comparing briefing as an intervention. For debriefing, data from 3 in-hospital observational before-and-after studies (2 in adults[108,311] and 1 in pediatrics[96]), involving a total of 591 patients, and data from 1 out-of-hospital observational before-and-after study in adults,[312] involving a total of 124 patients, was analyzed. All studies included data-driven debriefing interventions using CPR quality metrics such as chest compression depth, chest compression rate, or CCF.

For the critical outcome of survival with favorable neurologic outcome, we identified very-low-certainty evidence (downgraded for inconsistency, indirectness, and imprecision) from

2 observational studies[96,311] including 367 patients. One study[96] demonstrated significantly increased survival with favorable neurologic outcome from the use of the intervention compared with no debriefing, while the other[311] demonstrated no significant improvement from the use of the intervention compared with no debriefing. Meta-analysis demonstrates no significant effect from the use of debriefing compared with no debriefing on this outcome (RR, 1.41; 95% CI, 0.86–2.32; *P*=0.18; I2=28%).

For the critical outcome of survival to discharge, we identified very-low-certainty evidence (downgraded for indirectness and imprecision) from 4 observational studies[96,108,311,312] including 715 patients. One study[96] reported a trend toward improved survival to hospital discharge from the use of the intervention compared with no debriefing, while 3 other studies[108,311,312] demonstrated no improvement in survival to hospital discharge from the use of the intervention compared with no debriefing. Meta-analysis demonstrates a significant effect from the use of debriefing compared with no debriefing on this outcome (RR, 1.41; 95% CI, 1.03–1.93; *P*=0.03; I2=0%).

For the critical outcome of ROSC, we identified very-low-certainty evidence (downgraded for inconsistency, indirectness, and imprecision) from 3 observational studies[96,108,311] including 591 patients. One study[108] reported improved ROSC from the use of the intervention compared with no debriefing, while the other 2 studies[96,311] reported no improvement in ROSC from the use of the intervention compared with no debriefing. Meta-analysis demonstrates a significant effect from the use of debriefing compared with no debriefing on this outcome (RR, 1.18; 95% CI, 1.03–1.44; *P*=0.02; I2=0%).

For the critical outcome of chest compression depth (mean depth), we identified very-low-certainty evidence (downgraded for inconsistency and indirectness) from 3 observational studies[96,108,311] including 591 patients. One study[108] reported improved mean chest compression depth from the use of the intervention compared with no debriefing, and a second study[311] demonstrated no improvement in mean chest compression depth from the use of the intervention compared with no debriefing. A third study[96] that reported improved compliance with chest compression depth targets from the use of the intervention compared with no debriefing was not included in the meta-analysis because of differing outcome measures. Meta-analysis of 2 studies[108,311] demonstrated a significant effect from the use of debriefing compared with no debriefing on this outcome (mean difference, 4.00 mm; 95% CI, 0.18–7.82; I2=79%).

For the critical outcome of chest compression rate (mean rate), we identified very-low-certainty evidence (downgraded for inconsistency and indirectness) from 4 observational studies[96,108,311,312] including 715 patients. Two studies[108,312] reported improved mean chest compression rate from the use of the interventions compared with no debriefing, while a third study[311] demonstrated no improvement in mean chest compression rate from the use of the intervention compared with no debriefing. The last study[96] reported improved compliance with chest compression rate targets from the use of the intervention compared with no debriefing but was not included in meta-analysis because of differing outcome measures. Meta-analysis of 3 studies[108,311,312] demonstrates no significant effect from the use of the intervention compared with no debriefing on this outcome (mean difference, 5.81 bpm; 95% CI, −0.08 to 11.70; I2, 91%).

For the critical outcome of CCF, we identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, indirectness, and imprecision) from 2 observational studies[311,312] including 397 patients. Whereas one study[312] demonstrated improved CCF from the use of debriefing compared with no debriefing, the other[311] did not. Meta-analysis of these studies demonstrates no significant effect from the use of the intervention compared with no debriefing on this outcome (mean difference, 4.11%; 95% CI, −1.17 to 9.39; I2, 89%).

[H3] Treatment Recommendations

We suggest data-driven, performance-focused debriefing of rescuers after IHCA for both adults and children (weak recommendation, very-low-certainty evidence).

We suggest data-driven, performance-focused debriefing of rescuers after OHCA in both adults and children (weak recommendation, very-low-certainty evidence).

[H3] Justification and Evidence-to-Decision Framework Highlights

The evidence-to-decision table is included in Appendix A-10. Although the certainty of evidence is very low, our recommendations are based on the suggested positive effects of debriefing on patient and process-related outcomes for cardiac arrest.

One limitation is that our analysis revealed high inconsistency (heterogeneity) between studies, reflecting variation in instructional design, provider type, and outcome measures. We have not identified any undesirable effects (ie, emotional trauma) related to debriefing after cardiac arrest in the reviewed studies. Hence, we justify that the reported positive effects outweigh any possible undesirable effects. However, defusing emotions of rescuers after stressful or traumatic events has to be taken into account when assessing any potential risks related to debriefing.

While the certainty of evidence is very low, the associated costs to implement debriefing are likely to be low in many institutions. However, the reviewed studies did not explore the cost-effectiveness of debriefing. This is also applicable, when referring to the required resources for debriefing.

We also consider the high likelihood that this intervention is both acceptable to stakeholders (because of potential benefits, such as improved teamwork, improved communication, or identification of latent safety threats) and feasible in most institutions. This 2020 treatment recommendation supports the treatment recommendation made in 2015.[3,4]

[H3] Knowledge Gaps

- No studies addressed comparisons related to various specifications of debriefing, such as the format (individual feedback versus group debriefings), the timing (hot versus cold debriefings), use of CPR-quality metrics (data-driven versus non data-driven debriefings), or facilitation (facilitated versus nonfacilitated debriefings).
- No study was adequately powered to investigate effects on patient outcome, such as ROSC, survival to discharge, or favorable neurologic outcome at discharge. One study was aimed at assessing the feasibility of intervention delivery rather than effectiveness.[311] Thus, future study design should aim at quantitative and qualitative endpoints related to process measures, such as CPR-quality metrics, and patient outcomes.
- Future research questions may include training of facilitators and impact on debriefings, type of data to be included to improve effectiveness of debriefing, and determination of the optimal length of debriefing, as well as exploration of any possible emotional side effects and their incidence and nature. Related to briefing, future studies may explore effects on rescuers and patients.

[H2] CPR Feedback Devices During Training (EIT 648: SysRev)

[H3] Rationale for Review

CPR quality is a key component in outcome of both OHCA and IHCA. Optimal methods of training both healthcare providers and laypersons are key to improving cardiac arrest outcomes. We searched for studies investigating the use of CPR feedback or guidance device in CPR training published since the last search in 2015.[3,4] We excluded studies that examined the use of CPR feedback devices in performance of CPR (either on real patients or in the simulated environment). We considered both true feedback devices (systems that assess participant performance and provide corrective information) and guidance devices (systems that only provide prompts not based on participant performance, such as a metronome for CPR rate).

There was high heterogeneity among the studies in type of device used, learner demographics, and outcomes. We were unable to perform a meta-analysis, and present the data narratively.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Students who are receiving resuscitation training

Intervention: Use of a CPR feedback/guidance device

Comparator: No use of a CPR feedback/guidance device

Outcome:

1. Patient survival
2. Quality of performance in actual resuscitations
3. Skill performance 1 year after course conclusion
4. Skill performance between course conclusion and 1 year

5. Skill performance at course conclusion

6. Knowledge at course conclusion

Study design: RCTs and nonrandomized studies (non-RCTs, interrupted time series, controlled before-and-after studies, cohort studies) are eligible for inclusion. Unpublished studies (eg, conference abstracts, trial protocols) were excluded.

Time frame: New SysRev search strategy: all years and all languages were included if there was an English abstract; rerunning existing search strategy: January 1, 2014, to November 1, 2019

PROSPERO registration submitted November 9, 2019

[H3] Consensus on Science

We identified 13 randomized studies[313-325] and 1 nonrandomized study[326] examining the effects of CPR feedback/guidance devices on learning CPR skills. All studies were simulation-based studies, and none examined any patient outcomes or performance of teams in actual resuscitations. As a result, all studies were downgraded for indirectness.

[H4] CPR Performance at 1 Year After Training

We identified low-certainty evidence (downgraded for risk of bias and indirectness) from 2 RCTs. The first[325] reported no difference in CPR performance between a group of laypeople trained with a CPR feedback device compared with a control group at 1 year after training. In the second study of CPR training of healthcare providers,[313] both control and feedback groups improved from baseline at 1 year after training, but there was no difference between the control and feedback groups.

[H4] CPR Performance From Training Conclusion to 1 Year After Training

We identified 5 RCTs[318,321,323,325,326] that addressed this outcome. We identified low-certainty evidence (downgraded for risk of bias and indirectness) from 4 RCTs that used true feedback devices.[318,321,323,325] All of these studies were in laypeople or junior healthcare providers, and they reported improvements in retention of CPR skills at 7 days to 3 months after training.

We identified moderate-certainty evidence (downgraded for indirectness) for 1 study[326] that examined the use of a guidance device (a song for compression rate). This study reported an improved compression rate (RR of compression rate between 100 and 120/min, 1.72; 1.17–2.55) compared with learners with no access to a guidance device. We identified 5 RCTs[318,321,323,325,326] that addressed this outcome.

We identified low-certainty evidence (downgraded for risk of bias and indirectness) from 4 RCTs that used true feedback devices.[318,321,323,325] All of these studies were in laypeople or junior healthcare providers, and they reported improvements in retention of CPR skills at 7 days to 3 months after training.

[H4] CPR Performance at End of Training

We identified 8 RCTs[313-317,320,322,324] with moderate to low certainty of evidence downgraded for risk of bias (because of confounding interventions, indirectness, and unclear outcomes) and 1 observational study (very-low-certainty evidence, downgraded for indirectness).[319] Five studies showed improvement in CPR skills at the end of training with the use of feedback devices compared with no feedback device.[313,314,317,322,324] Two studies showed no difference in performance.[316,320] One study showed worse CPR performance at the conclusion of training, although this study has a high risk of bias because of unclear outcome definitions and the use of the audiovisual feedback system to replace an instructor.[315] One observational study found improvements in delivered chest compression rate (118.61 +/10.74 compressions/min versus 137.72 ± 11.14 compressions/min; P<0.001), with the use of a feedback device during training of student teachers.[319]

[H3] Treatment Recommendations

We suggest the use of feedback devices that provide directive feedback on compression rate, depth, release, and hand position during CPR training (weak recommendation, low-certainty evidence). If feedback devices are not available, we suggest the use of tonal guidance (examples include music or metronome) during training to improve compression rate only (weak recommendation, low-certainty evidence).

[H3] Justification and Evidence-to-Decision Framework Highlights

The evidence-to-decision table is included in Appendix A-11. In making this recommendation, the EIT Task Force noted that there have been a number of RCTs examining this topic in simulated settings but none examining patient-related outcomes. These studies have shown positive effects on retention of CPR skills, at least in the short-term, with 1 very-low-certainty study suggesting harm. We recognize that effective feedback devices are only part of an efficient CPR educational strategy. This update confirms the 2015 ILCOR treatment recommendation to use feedback devices during resuscitation training.

[H3] Knowledge Gaps

- Although there are several simulation studies that demonstrate improved CPR performance both immediately after training with a feedback device and short-term retention of CPR skills after training, only 2 studies examined the effect of feedback devices on long-term retention, and none evaluated patient outcomes.

- The use of feedback devices is likely an important component of CPR training, and how it should be integrated with other instructional design elements such as mastery learning and distributive practice needs to be better defined.
- It remains unclear how best to use these devices, how they interact with instructors, and how timing of feedback may impact learning and retention. The use of a team member as a 'CPR coach' dedicated to analyzing feedback data from the device and to provide real-time coaching to team members providing CPR may improve the efficacy of these devices {Cheng 2018 33}.

[H2] Patient Outcomes as a Result of a Member of the Resuscitation Team Attending an ALS Course (EIT 4000: SysRev)

[H3] Rationale for Review

Attendance of participants on an ACLS course comes at a cost—both financial and time—to stakeholders, including participants themselves and their institutions. It is therefore important to show whether this participation has any meaningful impact on patient outcomes. There is likely to be a lack of recent data addressing this question because ACLS training is generally widespread. This ILCOR EIT Task Force review is an "adolopment" of an existing publication,[327] which was a SysRev and meta-analysis of 8 observational studies.[328-335] The literature search was repeated on October 31, 2019, and no additional studies have been identified, making the published work contemporary.

Met opmerkingen [JF33]: ADD REFERENCE
Schünemann HJ, Wiercioch W, Brozek J, Etxeandia-Ikobaltzeta I, Mustafa RA, Manja V, et al. GRADE Evidence to Decision (EtD) frameworks for adoption, adaptation, and de novo development of trustworthy recommendations: GRADE-ADOLOPMENT. Journal of Clinical Epidemiology. 2017;81:101-10.
DOI: https://doi.org/10.1016/j.jclinepi.2016.09.009

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Adult in-hospital patients who have a cardiac arrest

Intervention: Prior participation of 1 or more members of the resuscitation team in an accredited ALS course

Comparator: No such participation

Outcome: ROSC, survival to hospital discharge or to 30 days, and survival to 1 year

Study design: Inclusion: any language, specifically looking at ALS or ACLS, RCTs, and observational; exclusion: other types of life support courses (eg, neonatal life support, ATLS, BLS), studies looking at impact of individual components (eg, airway, drug therapy, defibrillation)

Time frame: "The search dates for the Systematic Review published in Resuscitation extended up until May 2018. The search strategy was rerun July 29, 2019, covering May 2018 onward. No additional papers were identified.

Met opmerkingen [JF34]: ADD REFERENCE 327 HERE: 327: "Lockey A., Lin Y., Cheng A. Impact of adult advanced cardiac life support course participation on patient outcomes-A systematic review and meta-analysis. Resuscitation. 2018;129:48-54".

[H3] Consensus on Science

For the critical outcome of ROSC, we identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, indirectness, and imprecision) from 6 observational studies[328-330,332,334] enrolling 1461 patients showing benefit for ACLS training (OR, 1.64; 95% CI, 1.12–2.41).

Met opmerkingen [JF35]: ADD REFERENCE #335 335. Sodhi K, Singla MK, Shrivastava A. Impact of advanced cardiac life support training program on the outcome of cardiopulmonary resuscitation in a tertiary care hospital. *Indian J Crit Care Med*. 2011;15:209–212. doi: 10.4103/0972-5229.92070

For the critical outcome of survival to hospital discharge or survival to 30 days, we identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, indirectness, and imprecision) from 7 observational studies[328,329,331-335] enrolling 1507 patients showing benefit for ACLS training (OR, 2.43; 95% CI, 1.04–5.70)

For the critical outcome of survival to 1 year, we identified very-low-certainty evidence (downgraded for risk of bias, inconsistency, and imprecision) from 2 observational studies[332,334] enrolling 455 patients showing no benefit for ACLS (OR, 3.61; 95% CI, 0.11–119.42).

[H3] Treatment Recommendations

We recommend the provision of accredited adult ACLS training for healthcare providers (weak recommendation, very-low-certainty evidence).

[H3] Justification and Evidence-to-Decision Framework Highlights

The evidence-to-decision table is included in Appendix A-12. Adult ACLS training improves resuscitation knowledge and skills and is likely to ensure best practice is applied in these emergency situations. We recognize that the evidence in support of this recommendation comes from observational studies of very low quality. However, pooling of the available evidence consistently favors ACLS training, and having ACLS-trained staff present during an attempted adult resuscitation has been found to reduce treatment errors such as incorrect rhythm assessment[330] and time to ROSC.[334] We recognize that the provision of accredited adult ACLS training may not be feasible or appropriate in low-resource settings.

[H3] Knowledge Gaps

- Impact on patient outcomes of prior participation of 1 or more members of the cardiac arrest team for other life support courses (eg, pediatrics, newborns)

[H1] Use of Social Media

[H2] First Responder Engaged by Technology (EIT 878: SysRev)

[H3] Rationale for Review

Bystander CPR/defibrillation improves survival from OHCA, but rates of bystander CPR and performance quality remain low. Engaging volunteer citizens through different social media/technologies could potentially increase rates of bystander CPR/defibrillation and survival. Therefore, this PICOST searched for the role of citizen as first responder, defined as all individuals who were engaged/notified by a smartphone app with mobile positioning system (MPS) or text message (TM)–alert system to attend OHCA events and initiate early CPR and early defibrillation.

[H3] Population, Intervention, Comparator, Outcome, Study Design, and Time Frame

Population: Adults and children with OHCA

Intervention: Having a citizen CPR responder notified of the event via technology or social media

Comparators: No such notification

Outcome: Survival to hospital discharge with good neurologic outcome, survival to hospital discharge/30-day survival, hospital admission, ROSC, bystander CPR rate, and time to first compression/shock

Study design: RCTs and nonrandomized studies (non-RCTs, interrupted time series, controlled before-and-after studies, cohort studies) were eligible for inclusion. Unpublished studies (eg, conference abstracts, trial protocols), animal studies, case series, and simulation studies were excluded.

Time frame: All years and all languages were included if there was an English abstract. The search strategy was performed on the same day (October 25, 2019) for the 3 databases.

PROSPERO registration submitted to PROSPERO on November 12, 2019

[H3] Consensus on Science

Three of the included studies[336-338] assessed the role of a TM-alert system, 3 studies[339-341] assessed the role of a smartphone app with MPS, and 1 study[342] assessed both.

Most studies' outcomes were compared between the intervention and the control period, while 2 studies[339,341] compared the time to compression/shock in the intervention group with that of the EMS.

Studies had covered different search radiuses (ie, 500 m, 1000 m). When it was possible, we extracted only adjusted outcomes from the studies.

The most important confounders (eg, primary rhythm, etiology, witnessed status, location of arrest, gender, age, comorbidities response time, time of the arrest) were controlled for in the multivariable analysis.

However, some studies did not report adjusted data or did so only for certain outcomes (mainly primary outcomes). In these cases, we reported unadjusted RR with 95% CI. In the case of studies assessing the same outcomes, a pooled RR was calculated and reported along with the 95% CI.

For the critical outcome of survival with favorable neurologic outcome at discharge, we identified very-low-certainty evidence from 2 observational studies (downgraded for serious risk of bias) enrolling 2149 OHCAs showing no benefit for having a citizen CPR responder notified of the event via technology or social media (adjusted pooled RR, 1.4; 95% CI, 0.6–3.4).[336,341]

For the critical outcome of survival to hospital discharge/30-day survival, we identified moderate-certainty evidence from 1 RCT (downgraded for serious risk of bias)[340] and very-low-certainty evidence (downgraded for serious risk of bias and serious inconsistency) from 4 observational studies.[336,338,341,342] The RCT reported no benefit in 1-month survival between the intervention and the control group (unadjusted RR, 1.3; 95% CI, 0.8–2.1). The meta-analysis of adjusted data included 2905 OHCAs (4 studies) and showed benefit in survival to hospital discharge when having a citizen CPR responder notified of the event by a smartphone app with MPS or TM-alert system (adjusted pooled RR, 1.70; 95% CI, 1.16–2.48; I2=69%; *P*=0.02)*; 98/1000 more patients benefitted with the intervention (95% CI, 22 more patients/1000 to 208 more patients/1000 when compared with notification by an smartphone's app with MPS or TM-alert system not being offered). These results are confirmed by RRs reported separately in 3 of the 4 studies, showing benefit in survival to hospital discharge when having a citizen CPR responder notified by technology (RR, 1.7 [95% CI, 1.17–2.5] [342]; RR, 2.23 [95% CI, 1.41–3.23][338]; RR, 2.37 [95% CI, 1.07–4.55][341]). One of the studies did not report any significant benefit (RR, 1.06; 95% CI, 0.72–1.51).[336]

For the critical outcome of survival to hospital admission, we identified no studies.

For the important outcome of ROSC, we identified moderate-certainty evidence (downgraded for serious risk of bias) from 1 RCT enrolling 667 OHCAs showing no significant benefit for having a citizen CPR responder notified of the event via technology or social media (0.3 percentage points higher for the intervention group; 95% CI, 6.5 lower–7.3 higher; unadjusted RR, 1.01; 95% CI, 0.79–1.28).[340] We also identified very-low-certainty evidence (downgraded for serious risk of bias) from 3 observational cohort studies enrolling 2571 OHCAs showing no benefit for having a citizen CPR responder notified of the event via technology or social media (unadjusted pooled RR, 0.97; 95% CI, 0.60–1.57).[336,338,341]

For the important outcome of bystander CPR, we identified high-certainty evidence from 1 RCT.[340] This RCT enrolled 667 OHCAs, showing an absolute difference for intervention versus control of 14 percentage points (6 higher to 21 higher; adjusted RR, 1.27; 95% CI, 1.10–1.46); 129/1000 more patients benefitted with the intervention (95% CI, 48 more patients/1000 to 219 more patients/1000 when compared with notification by a smartphone app with MPS or TM-alert system not being offered).[340]

We also identified low-certainty evidence from 1 before-and-after study.[336] This study enrolled 1696 OHCAs, showing benefits for having a citizen CPR responder notified of the event via technology or social media (adjusted RR, 1.29; 95% CI, 1.20–1.37); 160/1000 more patients benefitted with the intervention (95% CI, 110 more patients/1000 to 204 more patients/1000 when compared with no intervention).[336]

For the important outcome of time to first compression/shock delivery, we identified very-low-certainty evidence (downgraded for serious risk of bias and inconsistency) from 4 observational studies enrolling 1833 OHCAs showing that having a citizen CPR responder notified of the event via technology or social media led to significantly lower response times compared with no technology, ie, median response time (minutes:seconds) 6:17 (IQR, 4:49–7:57) versus 9:38 (IQR, 7:14–12:51), Z=−14.498, P<0.0001[339] and median time for defibrillation delivery (minutes:seconds) 8:00 (IQR, 6:35–9:49) versus 10:39 (IQR, 8:18–13:23; P<0.001).[337] Another study showed a significant difference in median response time between mobile rescuers (4 minutes; IQR, 3–6) and EMS teams (7 minutes; IQR, 6–10]), P<0.001.[341] In a comparison of an app-based system with a TM-based system, benefit was found in using the app: responders' median response time 3.5 minutes (IQR, 2.8–5.2) compared with the TM-based system 5.6 min (IQR, 4:2–8:5; *P*=0.0001).[342]

[H3] Treatment Recommendations

We recommend that citizen/individuals who are in close proximity to a suspected OHCA event and willing to be engaged/notified by a smartphone app with an MPS or TM-alert system should be notified (strong recommendation, very-low-certainty evidence).

[H3] Justification and Evidence-to-Decision Framework Highlights

The evidence-to-decision table is included in Appendix A-13. Notifying a citizen CPR responder by a smartphone app with an MPS or TM-alert system to attend OHCA events can lead to an increase in early CPR and defibrillation, improving survival. We considered the improved outcomes in OHCA patients when a citizen CPR responder was notified by a smartphone app or TM for the event and started CPR or delivered defibrillation across most studies.

Even though the certainty of the evidence is very low/low among the observational cohort studies, there was 1 RCT and 1 before-and-after study, reporting improved outcomes when first responders were notified by a smartphone app with MPS or TM-alert system for the OHCA event and started CPR or delivered defibrillation.

Pooled RRs were estimated using a random effect model, because it takes into account the between-studies variability. Heterogeneity between studies was assessed by using the I2 statistics and was evaluated to be moderate (I2=69%, *P*=0.021) for the outcome of survival to hospital discharge. Sensitivity analyses were conducted to investigate the impact each study had on the overall estimate. The presence of the statistical heterogeneity suggests the presence of variability among the clinical characteristics of the studies' populations (ie, comorbidities, cause of cardiac arrest, time and location of the arrest, arrival time of laypersons or first responders at the location) as well as methodological heterogeneity (ie, study design, data collection).

In 2015, the EIT Task Force suggested that individuals in close proximity to a suspected OHCA, and who are willing and able to perform CPR, be notified of the event via technology or social media.[3,4] In 2020, we have made a clear recommendation that a smartphone app with an MPS or TM-alert system should be used to notify potential rescuers.

[H3] Knowledge Gaps

- There is a need for more high-certainty prospective studies including the critical outcome of long-term survival. Risk of bias is a common issue, with studies controlling for confounding factors only for a few outcomes. More RCT studies are needed for more robust evidence.
- There is no evidence of the cost-effectiveness of notifying laypersons through a smartphone app with an MPS or TM-alert system in the case of OHCAs.
- There was only 1 study assessing which of these technologies most improved outcome after OHCA (app versus text message). There is the need for more high-certainty evidence to determine the best technology to use in terms of OHCA outcomes.
- There is a need for the extension of these studies in different social, cultural, ethnic, and geographical contexts.
- The results of the included studies apply only to OHCAs of cardiac origin; there is a need for more evidence in cases of OHCA caused by trauma, drowning, intoxication, or suicide.
- There is a need for more consistent high-certainty evidence on the impact of engaged/notified versus unnotified bystander responses on survival with favorable neurologic outcome at hospital discharge, ROSC, and survival to hospital admission.

- The impact of engaged/notified versus unnotified bystander responses on bystander CPR rates and time to first compressions/shock delivery
- Safety of notifying CPR responders by a smartphone app with an MPS or TM-alert system to attend OHCA events
- The psychological or emotional impact imposed on responders by potential or actual engagement in a call to rescue

[H1]Topics Not Reviewed in 2020

BLS Including AED Training

CPR instruction methods (self-instruction versus traditional) (EIT 647)

Skills testing for resuscitation (EIT 632)

BLS training for high-risk populations (EIT 649)

First aid training (EIT 773)

Chest compression CPR training (EIT 881)

Duration of BLS courses (EIT 644)

ALS Training Including Team and Leadership Training, and METs and RRTs

Timing for advanced resuscitation retraining (EIT 633)

[H1] Acknowledgments

The writing group acknowledges the following contributors/collaborators: Enrico Baldi, MD; Stefanie Beck, MD; Stefan Beckers, MD, PhD, MME; Audrey L. Blewer, PhD, MPH; Adam Boulton, MBChB; Liu Cheng-Heng, MD; Yang Chi-Wie, MD, PhD; Alison Coppola, MClinRes, BSc (Hons); Katie N. Dainty, MSc, PhD; Domagoj Damjanovic, MD; Theresa Djärv,

MD, PhD; Aaron Donoghue, MD, MSCE; Marios Georgiou, CCRN, PhD; Imogen Gunson; Jamillee L. Krob, DHEd, MPH, RDH; Artem Kuzovlev, MD, DrSci; Ying-Chih Ko, MD; Marion Leary, RN, MSN, MPH; Yiqun Lin, MD, MHSc, PhD; Mary E. Mancini, RN, PhD; Tasuku Matsuyama, MD, PhD; Kenneth Navarro, PhD; Aron M. Orkin, MD, MSc, MPH, PhD; Tommaso Pellis, MD; Lucas Pflanzl-Knizacek, BSC, MA; Luca Pisapia, MD; Michela Saviani; Taylor Sawyer, DO, MEd; Andrea Scapigliati, MD; Sebastian Schnaubelt, MD; Barnaby Scholefield, MBBS, PhD; Federico Semeraro, MD; Salma Shammet, MSc; Michael Smyth, MSC, PhD; Abigale Ward; and Drieda Zace, MD, MSc.

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