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SYSTEMATIC REVIEW

The effects of obstetric emergency team training on patient outcome: A systematic review and meta-analysis

Lise Brogaard[1,2] | Kasper Glerup Lauridsen[3,4,5] | Bo Løfgren[2,3,5] | Kristian Krogh[3,6] | Charlotte Paltved[7] | Sidsel Boie[8] | Lone Hvidman[1]

[1]Department of Obstetrics and Gynecology, Aarhus University Hospital, Aarhus, Denmark

[2]Department of Clinical Medicine, Aarhus University, Aarhus, Denmark

[3]Research Center for Emergency Medicine, Aarhus University Hospital, Aarhus, Denmark

[4]Department of Critical Care Medicine, Children's Hospital of Philadelphia, Philadelphia, PA, USA

[5]Department of Medicine, Randers Regional Hospital, Randers, Denmark

[6]Department of Anesthesiology and Intensive Care, Aarhus University Hospital, Aarhus, Denmark

[7]Department of Human Resources, Medical simulation in Central Denmark Region (MidtSim), Aarhus, Denmark

[8]Department of Obstetrics and Gynecology, Randers Regional Hospital, Randers, Denmark

Correspondence
Lise Brogaard, Department of Obstetrics and Gynecology, Aarhus University Hospital, Palle Juul-Jensens Boulevard 99, 8200 Aarhus, Denmark.
Email: lbrj@clin.au.dk

Abstract

Introduction: Little is known about the optimal simulation-based team training in obstetric emergencies. We aimed to review how simulation-based team training affects patient outcomes in obstetric emergencies.

Material and methods: Search Strategy: MEDLINE, Embase, Cochrane Library, and Cochrane Central Register of Controlled Trials were searched up to and including May 15, 2021. Selection criteria: randomized controlled trials (RCTs) and cohort studies on obstetric teams in high-resource settings comparing the effect of simulation-based obstetric emergency team training with no training on the risk of Apgar scores less than 7 at 5 min, neonatal hypoxic ischemic encephalopathy, severe postpartum hemorrhage, blood transfusion of four or more units, and delay of emergency cesarean section by more than 30 min. Data collection and analysis: The included studies were assessed using PRISMA, EPCO, and GRADE.

Results: We found 21 studies, four RCTs and 17 cohort studies, evaluating patient outcomes after obstetric team training compared with no training. Annual obstetric emergency team training may reduce brachial plexus injury (six cohort studies: odds ratio [OR] 0.47, 95% CI 0.33–0.68; one RCT: OR 1.30, 95 CI% 0.39–4.33, low certainty evidence) and suggest a positive effect; but it was not significant on Apgar score below 7 at 5 min (three cohort studies: OR 0.77, 95% CI 0.51–1.19; two RCT: OR 0.87, 95% CI 0.72–1.05, moderate certainty evidence). The effect was unclear for hypoxic ischemic encephalopathy, umbilical prolapse, decision to birth interval in emergency cesarean section, and for severe postpartum hemorrhage. Studies with in situ multi-professional simulation-based training demonstrated the best effect.

Conclusions: Emerging evidence suggests an effect of obstetric team training on obstetric outcomes, but conflicting results call for controlled trials targeted to identify the optimal methodology for effective team training.

Abbreviations: OR, odds ratio; RCT, randomized controlled trial.

KEYWORDS
birth, brachial plexus, delivery, emergency teams, obstetric, postpartum hemorrhage, shoulder dystocia, simulation training, systematic review

1 | INTRODUCTION

Every day around the world about 800 women die from preventable causes related to pregnancy and childbirth.[1] It is therefore an important development goal for the World Health Organization to improve maternal, fetal, and neonatal care in childbirth.[2,3] Obstetric emergencies can often be resolved by timely, competent multidisciplinary teamwork.[4,5] Obstetric emergencies do, however, occur infrequently for the individual healthcare provider and consequently it is a challenge for obstetric staff to become experienced in handling these situations on the Labor and Delivery Unit.

Reducing preventable harm to mothers and neonates is a universal goal.[6] Though only 10% of preventable maternal deaths occur in high-resource settings, audits into perinatal and maternal care in high-resource settings have shown that adverse outcomes in emergency obstetrics are frequent and often preventable.[2,6,7] It seems obvious that training maternity care staff in simulated obstetric scenarios in order to establish practiced routines in these clinical challenges would be beneficial and could improve maternal and neonatal outcomes.[8] In many settings, such intrapartum training is recommended or even mandatory.[5] Whereas most staff members appreciate participation in obstetric team training and state after the training that they feel more confident in managing such emergencies in real life, data regarding the actual effects on clinical outcomes are sparse and conflicting.[9]

The objective of this review was to assess the effect of simulation-based team training of healthcare providers in the Labor and Delivery Unit on the outcome of obstetric emergencies.

Key message

Obstetric emergency simulation-based team training may reduce brachial plexus injury with a low certainty level of evidence. Furthermore, our analysis suggests a positive effect on Apgar score less than 7 at 5 min, although not statistically significant.

2 | MATERIAL AND METHODS

2.1 | Protocol and registration

The review was conducted following the protocol for systematic reviews by using the assessment tools PRISMA, EPCO, and GRADE (www.equator-network.com).[10] The full study protocol was designed a priori and published on July 23, 2019 in PROSPERO (CRD42019136775).

2.2 | Identification of studies

The eligibility criteria for included studies were as per protocol.[11] The Population was obstetric emergency teams in hospitals. We considered a team to be at least two healthcare providers working within a team. Teams of either a single professional group or a multiprofessional team were accepted. We included studies conducted in high-income countries, defined by the World Bank classification system of 2019.[12] The healthcare providers could be at any stage of clinical experience. We excluded studies investigating students or non-healthcare professionals. For the Intervention, we considered all types of simulation-based obstetric team training and all types of educational intervention where simulation was used with the aim of improving care of patients in labor. The intervention could be delivered as simulation training alone or in combination with lectures, tutorials, online tests, or workshops. Comparators were teams not exposed to simulation training. All studies with an Outcome of any of the levels of Kirkpatrick[13] were selected for full-text analysis, and studies where patient outcomes related to an obstetrical emergency were reported were selected for further core outcome analysis (see core outcome set below). Eligible study designs were randomized controlled trials (RCT), cluster-randomized trials and cohort studies.

2.3 | Core outcome set

All studies with an evaluation of a patient outcome were included. All predefined core outcomes were selected for the meta-analysis, ie, neonatal asphyxia (defined as Apgar score <7 at 5 min and neonatal hypoxic ischemic encephalopathy), shoulder dystocia (brachial plexus injury at birth), umbilical cord prolapse (with an Apgar score <7 at 5 min), postpartum hemorrhage (blood loss >1500 ml, transfusion of four or more units of red blood cells), delay of birth at an emergency cesarean section (decision-to-delivery time excess of 30 min).

2.4 | Study selection and data extraction

A first literature search was conducted May 23, 2020 and updated on May 15, 2021 (Appendix S1). The databases used were: (a) Ovid MEDLINE (year 1946 to present), (b) Embase (year 1947 to present), and (c) Cochrane Library, including the Cochrane Central Register of Controlled Trials. The literature review was supplemented with studies found by reviewing the reference list of the retrieved studies.

We applied language restrictions to an abstract either in Danish, Swedish, Norwegian, or English. Two authors (LB and LH) independently reviewed all references, read all abstracts and reviewed all full-text studies. Any disagreements between the two reviewers during screening or assessment were resolved in a discussion between the authors. We documented the process using a PRISMA flow chart and kept a record of each full-text study and the reasons for exclusion of studies (Figure 1, Appendix S2).

Three authors (LB, LH, and SB) independently extracted data from each trial included in the final analysis. Any disagreements between the reviewers were resolved in a discussion between the authors. Where multiple publications were identified from the same trial, presenting both the primary analysis and a secondary analysis of the same outcome, only the primary analysis was included in the meta-analysis.

2.5 | Assessment of study quality and bias

Risk of bias assessment was conducted by two authors (LB and LH) who independently assessed all the included studies using the Cochrane Collaboration's tool for assessing risk of bias.[10] As recommended in the Cochrane handbook, other bias tools can be included according to the study design. Therefore, we made a supplementary assessment with the tool MERSQI[14] designed to assess medical educational studies.

2.6 | Statistical analyses

Statistical analysis was conducted using REVIEWMANAGER® software 5.3. As all our outcomes were dichotomous data, we presented results as odds ratio (OR) with 95% CI. In studies where an adjusted analysis was presented, the adjusted result was included. In the meta-analysis, two confidence intervals slightly differ from the authors' reported values as REVIEWMANAGER automatically rounds off to two digits. Therefore, Lenguerrand et al[15] report an effect of OR 0.79 (95% CI 0.63–1.01) where we report the effect as OR 0.79 (95% CI 0.62–1.01) and Fransen et al[16] report an effect of OR 0.96 (95% CI 0.74–1.2) where we report the effect as OR 0.96 (95% CI 0.74–1.25). As a result of the nature of the intervention, there was a significant risk of heterogeneity in the intervention and the timeline. We assessed statistical heterogeneity using the chi-squared test for heterogeneity and defined considerable heterogeneity if I^2 was more than 75%. We addressed heterogeneity in our analysis by using random-effects assessment in our meta-analysis and by downgrading the evidence.[10,17]

2.7 | Quality of evidence

Rating of evidence was done with the GRADE approach, where the initial level of quality was defined by the study design, and then reasons for downgrading or upgrading were assessed. Five factors for downgrading the evidence were assessed: (a) risk of bias by the study design and tools for bias evaluation, (b) inconsistency of results if there was unexplained heterogeneity in the results, (c) indirectness of evidence by whether the correct intervention, population, and outcomes were directly or indirectly compared, (d) imprecision by the width of the confidence intervals, and (e) publication bias evaluated by funnel plots. Three factors could increase the quality of evidence: (a) a large magnitude of effect, (b) plausible confounding that

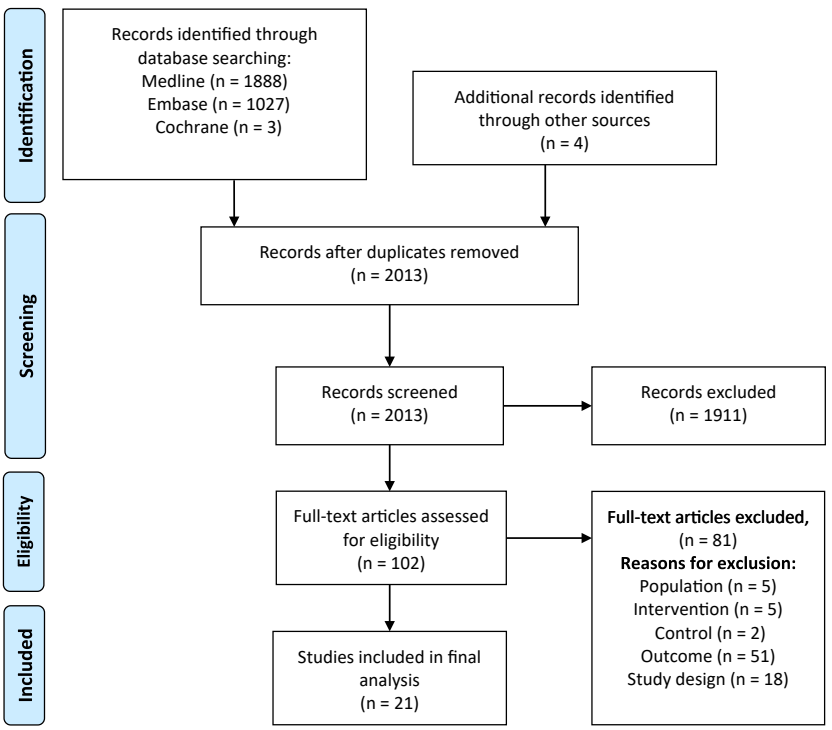


FIGURE 1 Flow diagram of literature selection

would reduce the demonstrated effect, or (c) a dose-response gradient.[8] Two authors (LB and LH) assessed these factors independently and listed arguments for downgrading or upgrading the evidence.

2.8 | Patient involvement

This systematic review was conducted without patient or public involvement.

3 | RESULTS

3.1 | Description of the studies

The literature search identified 2013 references, and after eligibility assessment 102 articles were analyzed in full-text analysis. A total of 21 studies were included[15,16,18-36]and 81 studies were excluded. Study characteristics of excluded and included studies are available in Appendices S2, S3. The selection process is shown in a PRISMA flow-diagram (Figure 1). The included studies consisted of four RCTs[15,16,35,36] and 17 observational cohort studies.[18-36] The settings were Labor and Delivery Units in the USA,[20,23,29,34-36] Australia,[21,25,33] and Europe.[15,16,18,19,22,24,26-28,30-32] The studies were published in 2006–2020. Details of the interventions are listed and compared in Table 1.

3.2 | Risk of bias

Studies with the highest quality design were three open-cluster RCTs[16,35,36] followed by a stepped-wedge RCT.[15] The observational cohort studies had a low to moderate risk of bias. Detailed assessments of each study and the arguments for assessment are described in Appendix S3 and in the risk of bias figure (Figure 2).

3.3 | Effect of intervention

Three studies were excluded from the meta-analysis, as they only reported an adverse outcome index,[34-36] defined by a summative effect measure including maternal and perinatal mortality, transfer to a neonatal intensive care unit, low Apgar scores, uterine rupture, anal sphincter rupture, and blood transfusion.

3.4 | Meta-analysis

Seven studies[16,20,22-26] reported the occurrence of brachial plexus injury at birth before and after training. In one RCT[16] an OR of 1.3 (95% CI 0.39–4.33) was found, whereas a combined OR of 0.47 (95% CI 0.33–0.68) was found in six observational cohort studies[20,22-26] with low heterogeneity (I^2 = 0%). The certainty of evidence was low because the level of certainty was downgraded one level because of risk of bias and one level for imprecision, but upgraded one level for a large magnitude of effect.

Apgar scores less than 7 at 5 min were reported in two RCTs[15,16] with a combined OR of 0.87 (95% CI 0.72–1.05) (I^2 = 13%) and in three observational trials[19-21] with a combined OR of 0.77 (95% CI 0.51–1.19). These observational trials involved considerable heterogeneity (I^2 = 83%). The grade of evidence for Apgar scores less than 7 at 5 min was moderate as the level of certainty was downgraded one level because of inconsistency in the studies.

Neonatal hypoxic ischemic encephalopathy was reported in two studies. In the one RCT[16] an OR of 3.20 (95% CI 0.77–13.30) was found, whereas in the one observational study[19] an OR of 0.50 (95% CI 0.26–0.96) was reported. The certainty of evidence was downgraded to very low because of imprecision and inconsistency.

The effect of training in umbilical cord prolapse was evaluated by Apgar scores less than 7 at 5 min in two observational studies.[32,33] The studies had a combined OR of 1.31 (95% CI 0.11–15.96) with a substantial risk of heterogeneity (I^2 = 62%). The certainty of evidence became very low, because it was downgraded because of risk of bias, inconsistency, and imprecision.

In eight studies, the effect of training on postpartum hemorrhage was evaluated.[16,18,21,25,27-30] Severe blood loss was reported in one RCT[16] with an OR of 2.20 (95% CI 1.24–3.90) and in two observational studies[21,25] there was a combined OR of 1.08 (95% CI 0.96–1.23) (I^2 = 0%). The certainty of evidence was categorized as very low. Transfusion of four or more units of red blood cells was reported from one RCT[16] with an OR of 2.10 (95% CI 1.10–4.01) and in two observational studies,[28,30] with an OR of 0.63 (95% CI 0.38–1.04) (I^2 = 0%). The certainty of evidence was considered very low.

The delay of birth at an emergency cesarean section (decision-to-delivery time excess of 30 min) was evaluated in two observational cohort studies[31,32] and the combined OR was 0.35 (95% CI 0.18–0.71) (I^2 = 0%). The certainty of evidence level was very low.

The effect of simulation-based training is presented by forest plots (Figure 3) with effect stacked by decreasing order of study quality. All studies except the RCT of Fransen et al[16] report a positive effect of simulation-based team training. Studies with in situ multiprofessional simulation-based training demonstrated the best effect.

Detailed assessments for each outcome are shown in Appendix S4, arguments for decision on quality of evidence are listed in the evidence profile in Appendix S5, and main findings in Appendix S6.

4 | DISCUSSION

In this meta-analysis, we found that obstetric emergency simulation-based team training may reduce brachial plexus injury with a low certainty level of evidence. Furthermore, our analysis suggests a positive effect on Apgar score less than 7 at 5 min, although this was not statistically significant. The effect was unclear for hypoxic

TABLE 1 Comparison of studies in the meta-analysis.

				Intervention					Subgroups by outcome						
Study	**Setting**	**Design**	**Time**	**Who?**	**Where?**	**How?**	**Duration**	**When?**	**Apgar**	**HIE**	**BPI**	**PPH**	**DD**	**CP**	**AOI**
Fransen 2017	24 units, Netherlands 27 509 births/year	Open cluster RCT	2 years: 1 year = pre 1 year = post	Delivery ward all staff	Sim center	CRM training, MOET program	1 day	Only once	x	x	x	x			
Lenguerrand 2019	12 units, Scotland 34 881 births/year	Stepped-wedge cluster RCT	2.5 years	Delivery ward all staff	In situ	PROMPT training	1 day	Only once	x						
Draycott 2006	1 unit, England 6000 births/year	Retrospective cohort	6 years: 2 years = pre 1 years = train 3 years = post	Delivery ward all staff	In situ	NA	1 day	Annually	x	x					
Weiner 2016	1 unit, USA 6000 births/year	Retrospective cohort	9 years: 2 year = pre 7 year = post	Delivery ward all staff	In situ	PROMPT training	1 day	Annually	x	x	x				
Shoushtharian 2014	8 units, Australia 12 402 births/year	Retrospective cohort	3.5 years: 18 months = pre 12 months = train 12 months = pos	Delivery ward 50% off staff	NA	PROMPT training	1 day	NA	x			x			
Van de ven 2016	1 unit, Netherlands 1800 births/year	Retrospective cohort	100 months: 38 months pre 24 months train 38 months post	Delivery ward all staff	In situ	2 scenarios	NA	NA			x				
Inglis 2011	1 unit, USA, 3800 births/year	Retrospective cohort	5 years: 2.5 years = pre 2 months = train 2.5 years = post	Delivery ward all staff	NA	NA	NA	NA			x				
Dahlberg 2018	1 unit, Sweden, 3000 births/year	Retrospective cohort	12 years: 4 years = pre, 4 years = train 4 years= post	Delivery ward all staff	Sim center	PROBE training	3 h	Every 1.5 year			x				
Kumar 2018	3 units, Australia 9000 birth/year	Retrospective cohort	5 years: 2 years = pre 1 year = train 2 years = post	Delivery ward all staff	In situ	NA	0.5 day	Every 2. Year			x	x			

(Continues)

TABLE 1 (Continued)

				Intervention					Subgroups by outcome						
Study	Setting	Design	Time	Who?	Where?	How?	Duration	When?	Apgar	HIE	BPI	PPH	DD	CP	AOI
Croft 2016	1 unit, England 6000 births/year	Retrospective cohort	12 years: 4 years = pre 4 years = train 4 years = post	Delivery ward all staff	In situ	PROMPT training	1 day	Annually			x				
Baldvinsdottir 2018	1unit, Sweden 3000 births/year	Retrospective cohort	8 years: 4 years = pre 4 years = post	Delivery ward all staff	In situ	NA	3 h	Every 1.5 year				x			
Egenberg 2015	1 unit, Norway 4800 births/year	Retrospective cohort	3 years: 1 year = pre 1 year = train 1 year = post	Delivery ward all staff	Sim center	NA	6 h	Annually				x			
Egenberg 2017	1 unit, Norway 4800 births/year	Retrospective cohort	4.5 years: 2 years = pre 0.5 year = train 2 years = post	Delivery ward 80% of staff	Off-site	NA	NA	Annually				x			
Lutgendorf 2017	1 unit, California NA births/year	Retrospective cohort	10 months 6 months = pre 4 months = post	NA	In situ	NA	2 day	Only once				x			
Markova 2012	1 unit, Denmark 3500 birth/year	Retrospective cohort	5 years 2 years = pre 1 year = train 2 years = post	NA	NA	Include lecture, multi-professional skills training in PPH, debrief	2.5 h	NA				x			
Fuhrmann 2015	1 unit, Denmark, 4500 births/year	Retrospective cohort	1 year: 5 months = pre 2 months = train 5 months = post	Delivery ward 95% of staff	Sim center	NA	NA	Only once					x		
Siassakos 2009	1 unit, UK 5500 births/year	Retrospective cohort	16 years: 7 years = pre 2 years = train 7 years: = post	Delivery ward 95% of staff	In situ	PROMPT training	1 day	Annually					x	x	
Copson 2017	1 unit, West Australia 6000 births/year	Retrospective cohort	11 years: 3 years = pre, 5 years = train 3 years = post	NA	NA	NA	1 day	Annually						x	

(Continues)

TABLE 1 (Continued)

Study	Setting	Design	Time	Intervention			Duration	When?	Subgroups by outcome						
				Who?	Where?	How?			Apgar	HIE	BPI	PPH	DD	CP	AOI
Phipps 2012	1 unit, USA 9200 births/year	Retrospective cohort	4 years 24 months = pre 6 months = train 18 months = post	Delivery ward 72% of staff	NA	4 h CRM, 4 h Multi professional simulation training	8 h	Only once							x
Nielsen 2007	15 units, USA, 27 509 births/year	Open cluster RCT	1 year: 2 months = prem 4 months = train 5 months = post	Delivery ward all staff	In situ	CRM training, (MedTeams Labor & Delivery Team Coordination Course)	NA	Only once							x
Riley 2011	3 units, USA, 1800/year	Open cluster RCT	3 years: 1 year = pre 1 year = train 1 year = post	NA	In situ	Webinars and simulation Team-STEPPS	2 h 30 month	11 simulations							x

X = mark the outcome/outcomes reported.

AOI, adverse outcome index; Apgar, Apgar score less than 7 at 5 min; BPI, brachial plexus injury at birth; CP, cord prolapse; DD, decision to delivery interval in emergency cesarean >30 min; HIE, hypoxic ischemic encephalopathy; PPH, severe blood loss and/or transfusion four or more units of red blood cells.

ischemic encephalopathy, umbilical prolapse, emergency cesarean section, and severe postpartum hemorrhage, because of few and conflicting studies. Studies with in situ multiprofessional simulation-based training demonstrated the best effect.

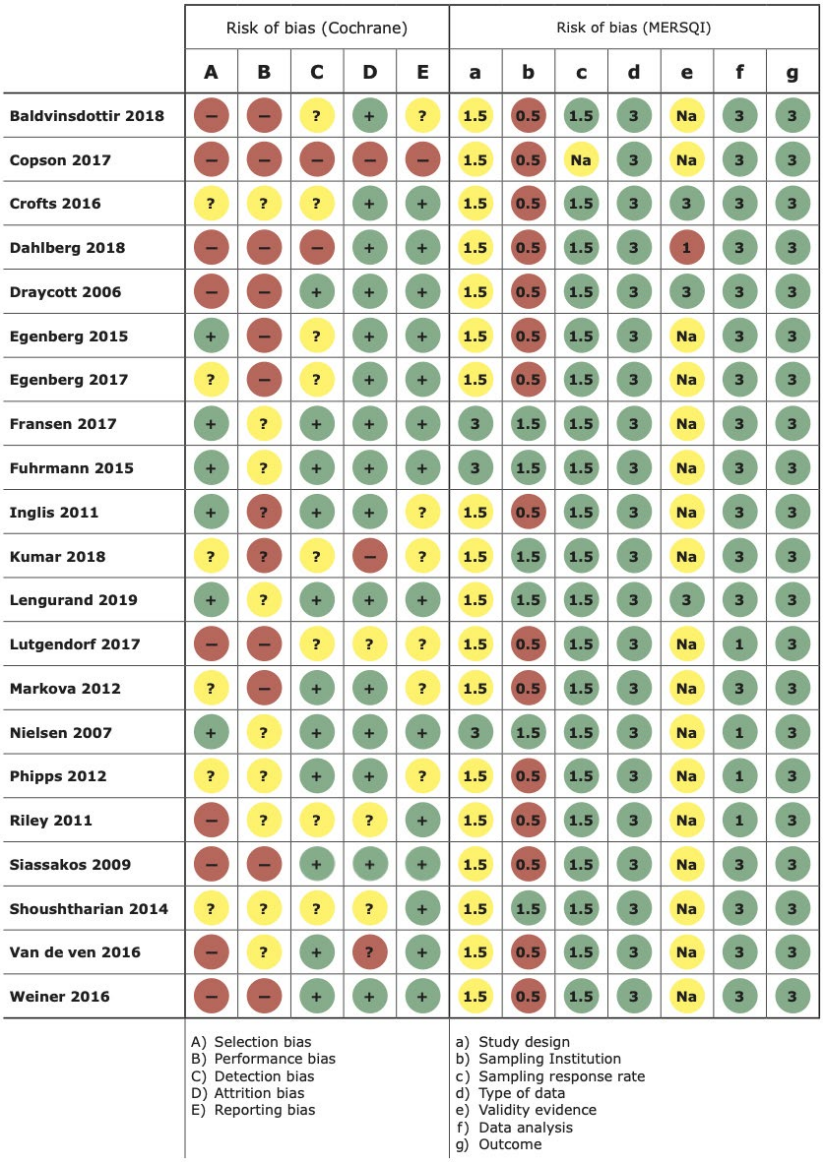
The strength of this systematic review and meta-analysis is the systematic approach by applying the PRISMA, EPCO, and GRADE guidelines in a comprehensive way. We searched multiple databases without date restrictions in four languages to limit bias by identifying all relevant studies. Two authors independently assessed all the published studies and selected the studies for inclusion in order to minimize bias. Three authors performed the data extraction, data synthesis, and quality of evidence assessment. The entire author group discussed the results and the interpretation.

This systematic review included studies with low quality of evidence for several of our core outcomes. In this review, one of the reasons for downgrading the evidence was limitations in study design or execution, also described as risk of bias and inconsistency of results. Further investigation into study design and execution of the intervention revealed differences in the included studies. For instance, only half of the studies encompassed all of the professional staff in the Labor and Delivery Unit,[15,16,18-20,22-27,31,35] whereas in 15% of the studies around half of the staff took part in the training,[21,28,34] and in 25% there was no information of how many participated.[29,30,32,33,36] It was reassuring, however, that the plausible confounding would reduce the demonstrated effect.

Further heterogeneity was found in the context of training, where only 50% used pre-defined guidelines or a previously described program.[15,16,20,21,24,26,35,36] The studies were better aligned with respect to duration of training, with the majority describing a one-day program[15,16,19-21,26,27,32-34] and others using a half-day program (Table 1). The majority trained staff in the Labor and Delivery Unit (in situ training) annually and only a few described training in simulation centers.[16,24,27,31] Hence, the differences in interventions used also limit the conclusions that can be drawn from the meta-analysis.

A further limitation was that only two RCTs could be included in the meta-analysis. One of these impacted heavily in our meta-analysis, as this study[16] was large and evaluated training for all outcomes, but with little or no effect being shown. It is important to evaluate all the included studied effects with regard to the intervention and type of training and the forest plots and not just to consider the summative result of the meta-analysis.

The management of shoulder dystocia was evaluated in six cohort studies and one RCT. The RCT by Fransen et al[16] reported 1 year after training an OR of 1.30 (95% CI 0.39–4.33). In a secondary analysis of the same trial,[37] a significant decrease in brachial plexus injury was reported in the first quarter (0.06% vs. 0.26%, OR 0.19, 95% CI 0.03–0.98), but in the subsequent quarters, no significant reductions were observed. One could argue that the lack of effect was due to loss of obtained skills after 3 months, as reported in other research.[38,39] However, in six observational cohort studies[20,22-26] annual training was also evaluated and a beneficial effect regarding brachial plexus injuries (OR 0.47; 95% CI 0.33–0.68) was



	A	B	C	D	E	a	b	c	d	e	f	g
Baldvinsdottir 2018	−	−	?	+	?	1.5	0.5	1.5	3	Na	3	3
Copson 2017	−	−	−	−	−	1.5	0.5	Na	3	Na	3	3
Crofts 2016	?	?	?	+	+	1.5	0.5	1.5	3	3	3	3
Dahlberg 2018	−	−	−	+	+	1.5	0.5	1.5	3	1	3	3
Draycott 2006	−	−	+	+	+	1.5	0.5	1.5	3	3	3	3
Egenberg 2015	+	−	?	+	+	1.5	0.5	1.5	3	Na	3	3
Egenberg 2017	?	−	?	+	+	1.5	0.5	1.5	3	Na	3	3
Fransen 2017	+	?	+	+	+	3	1.5	1.5	3	Na	3	3
Fuhrmann 2015	+	?	+	+	+	3	1.5	1.5	3	Na	3	3
Inglis 2011	+	?	+	+	?	1.5	0.5	1.5	3	Na	3	3
Kumar 2018	?	?	?	−	?	1.5	1.5	1.5	3	Na	3	3
Lengurand 2019	+	?	+	+	+	1.5	1.5	1.5	3	3	3	3
Lutgendorf 2017	−	−	?	?	?	1.5	0.5	1.5	3	Na	1	3
Markova 2012	?	−	+	+	?	1.5	0.5	1.5	3	Na	3	3
Nielsen 2007	+	?	+	+	+	3	1.5	1.5	3	Na	1	3
Phipps 2012	?	?	+	+	?	1.5	0.5	1.5	3	Na	1	3
Riley 2011	−	?	?	?	+	1.5	0.5	1.5	3	Na	1	3
Siassakos 2009	−	−	+	+	+	1.5	0.5	1.5	3	Na	3	3
Shoushtharian 2014	?	?	?	?	+	1.5	1.5	1.5	3	Na	3	3
Van de ven 2016	−	?	+	?	+	1.5	0.5	1.5	3	Na	3	3
Weiner 2016	−	−	+	+	+	1.5	0.5	1.5	3	Na	3	3

FIGURE 2 Risk of bias

suggested. It is difficult to discount this clear result. Training may therefore reduce the risk of brachial plexus injury; however, certainty of evidence is low.

Regarding Apgar scores less than 7 at 5 min; the two RCTs[15,16] had an OR of 0.87 (95% CI 0.72–1.05) (I^2 = 13%). Although they have a low heterogeneity, the studies differed substantially from each other. The THISTLE trial by Lenguerrand et al[15] used a step-wedge RCT design with in situ training where the PROMPT[40] course methodology was used, whereas the TOSTI trial by Fransen et al[16] was an open-cluster RCT with simulation center training using the MOET[41] program, which has a focus on crisis resource management. The study of Fransen et al[16] is large and of high quality; however, an effect after 1 year was not demonstrated. It has been argued that the intervention is based on off-site training and CRM training, and as a result lacks the introduction to checklists and procedures at the residing hospital.[42] Furthermore, training was not blinded and hospitals not selected for training may have been motivated to improve treatment by other initiatives. The three observational cohort studies[19-21] carried a substantial heterogeneity (I^2 = 83%) and differed in the proportion of the staff that were trained. Shoushtarian et al,[21] who trained 50% of the staff, did not find a reduced effect on the risk of low Apgar scores; however, Draycott et al[19] who trained all the staff in the Labor and Delivery Unit, showed a significant risk reduction of OR 0.51 (95% CI 0.35–0.74). The meta-analysis suggests a positive effect on Apgar score less than 7 at 5 min, although not statistically significant.

Hypoxic ischemic encephalopathy is a rare complication with high morbidity. This outcome was only reported in two studies. Fransen et al[16] reported an OR of 3.20 (95% CI 0.77–13.30). Intuitively, it seems unlikely that training would increase the risk of ischemic encephalopathy and a large degree of uncertainty is also evident from the wide confidence interval. Their findings contradict the observational study of Draycott et al,[19] where a beneficial effect was indicated with an OR of 0.50 (95% CI 0.26–0.96), but the two studies differed with regard to study design and intervention. Overall it must be considered uncertain whether

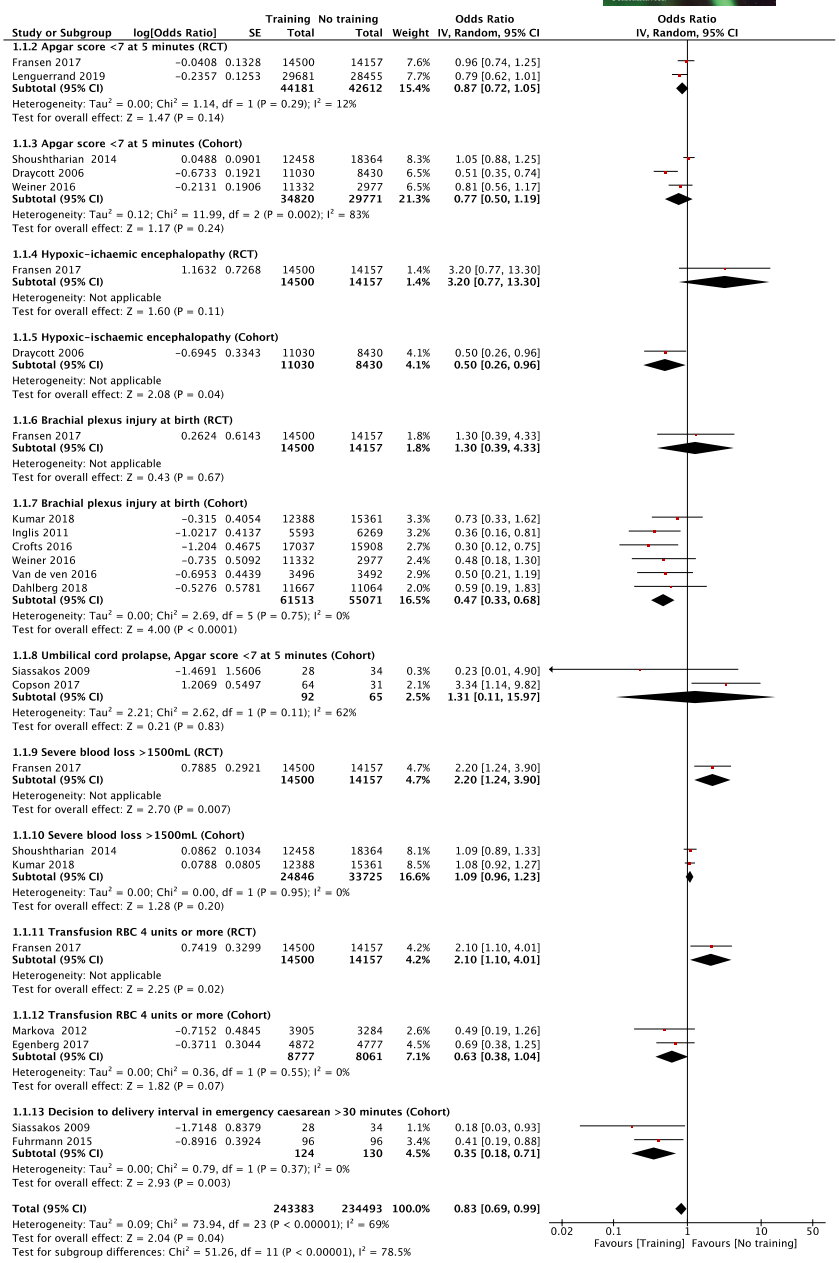


FIGURE 3 Forest plot of studies investigating the risk of: Neonatal outcomes: Apgar score <7 at 5 min (1.1.1-.1.1.2), hypoxic ischemic encephalopathy (1.1.3–1.1.4), brachial plexus injury at birth (1.1.5–1.1.6), cord prolapse (1.1.7). Maternal outcomes: severe blood loss (1.1.8–1.1.9), transfusion red blood cells (RBC) 4 units or more (1.1.10–1.1.11), decision to delivery interval in emergency cesarean >30 min (1.1.12)

the training efforts have an effect on neonatal hypoxic ischemic encephalopathy.

Eight studies addressed the value of training in the management of postpartum hemorrhage.[16,18,21,25,27-30] Fransen et al[16] reported an increase of hemorrhage cases (OR 2.20, 95% CI 1.24–3.90), and Kumar et al[25] and Shoushtarian et al[21] reported an inconclusive result (OR 1.08, 95% CI 0.96–1.23). Underestimation of blood loss is well described,[43] and learning to more accurately assess this will likely raise these rates. Therefore, it seems unlikely that there was a real increase in severe postpartum hemorrhage. The effect on transfusion rates (four or more units) was varied with one RCT[16] showing higher rates and two observational cohort studies[28,30] having lower rates. The inconsistency of these results also leads to uncertainty. We conclude that the effect of training on reducing postpartum hemorrhage and the need for subsequent blood transfusion remains unclear.

Two observational studies[32,33] evaluated the effect of training on the rare event of umbilical cord prolapse using the end-point low Apgar scores at 5 min. The studies had a similar design and were both limited by the inclusion of only a few events. Siassakos et al[32] reported OR of 0.23 (95% CI 0.01–4.90), whereas the study from Copson et al[33] reported a marginally significant increased OR of 2.42 (95% CI 1.03–5.72). This meant that the meta-analysis result was inconclusive, so the effect of training on umbilical cord prolapse remains unclear.

The delay of birth at an emergency cesarean birth (decision-to-delivery time excess of 30 min) was dealt with in two observational cohort studies. Fuhrmann et al[31] evaluated the effect of one training session 1 year later, whereas Siassakos et al[32] trained their staff annually over 16 years. Both studies suggested that training would reduce the proportion of delayed emergency cesarean sections, but because of the design and different observation times, the certainty

of the evidence was very low and a conclusion that there is an effect of training cannot be drawn.

Simulation-based team training is defined by teams applying principles or guidelines in a scenario using a mannequin.[44] Several types of simulation-based training were used in this review.

Based on our review, it seems that local (in situ) multiprofessional training for all staff members is the most beneficial with regards to improving patient outcomes. Not all training is equally effective, and it is noteworthy that none of the two RCTs demonstrates the same effect as cohort studies. It has been speculated whether the lack of blinding may play a role, as the non-training hospitals may train anyhow. Furthermore, a national or regional simulation program may be difficult to implement locally, wherefore training is offered in a simulation center.

Studies included in this review described staff being trained annually for 1 day. Research on resuscitation has suggested that shorter training sessions with shorter intervals can be more efficient.[45-48] However, little is known on how often obstetric training should take place. We anticipated that this review could provide more information on this matter; however, analysis on the frequency was not possible because of the lack of studies using interventions more frequently than yearly.[49]

In this review, we selected patient outcome measures to evaluate the effect of simulation-based training in obstetrical emergencies. We included outcomes that are widely accepted as obstetric quality indicators to cover management, trauma, and injury with regard to both the women and neonates.[50] The strength of the selected outcomes is that they are internationally defined and reported. However, the weakness in several of these outcomes is that multifactorial events can evolve even when the team provides optimal care. Furthermore, some of the included outcomes are considered to be pseudo-outcomes and therefore constitute only an indirect measure. The number of administered blood transfusions is an example of this. A more direct approach would involve auditing the direct performance of the emergency team, such as by live recordings. This is, however, rarely described and not easily used.[51]

In the last decade, use of obstetric simulation training has been increasing as healthcare providers, insurance companies, and hospitals request this provision. Staff use simulation training for improved personal confidence and preparedness.[9,29,52,53] The insurance companies strive for a reduction in malpractice claims[54] and hospitals aim for reduced sick leave among healthcare providers,[55] for higher patient satisfaction,[56] and better obstetric patient safety indicator measures. Training of an entire department is costly, though studies have reported it to be cost-effective.[54,57] Research is therefore needed to ensure effective training in the future and to improve levels of evidence.

5 | CONCLUSION

Emerging evidence suggest an effect of simulation-based obstetric team training for multiprofessional teams trained locally/in situ, but conflicting results call for future controlled trials targeting the methodology for effective team training.

CONFLICT OF INTEREST

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AUTHOR CONTRIBUTIONS

LB, LH, and SB are responsible for acquisition of data and all authors are responsible for the interpretation of data and writing of the article.

ORCID

Lise Brogaard https://orcid.org/0000-0001-5136-9682

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SUPPORTING INFORMATION

Additional supporting information may be found in the online version of the article at the publisher's website.

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