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TABLE OF CONTENTS

ABSTRACT	1
PLAIN LANGUAGE SUMMARY	2
SUMMARY OF FINDINGS	3
BACKGROUND	5
OBJECTIVES	6
METHODS	6
RESULTS	10
Figure 1.	12
Figure 2.	14
Figure 3.	14
DISCUSSION	16
AUTHORS' CONCLUSIONS	17
ACKNOWLEDGEMENTS	17
REFERENCES	18
CHARACTERISTICS OF STUDIES	27
DATA AND ANALYSES	37
Analysis 1.1. Comparison 1: Professional practice, Outcome 1: Leadership	37
APPENDICES	37
HISTORY	45
CONTRIBUTIONS OF AUTHORS	45
DECLARATIONS OF INTEREST	46
SOURCES OF SUPPORT	46
DIFFERENCES BETWEEN PROTOCOL AND REVIEW	46
NOTES	46
INDEX TERMS	47

[Intervention Review]

Collective leadership to improve professional practice, healthcare outcomes and staff well-being

Jaqueline Alcantara Marcelino Silva¹, Vivian Aline Mininel¹, Heloise Fernandes Agreli², Marina Peduzzi², Reema Harrison³, Andreas Xyrichis⁴

¹Nursing Department, Federal University of São Carlos, São Carlos, SP, Brazil. ²Professional Orientation Department, University of Sao Paulo, Sao Paulo, Brazil. ³School of Population Health, University of New South Wales, Sydney, Australia. ⁴Florence Nightingale Faculty of Nursing, Midwifery & Palliative Care, King's College London, London, UK

Contact: Jaqueline Alcantara Marcelino Silva, jaqueline.alc@gmail.com.

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ABSTRACT

Background

Collective leadership is strongly advocated by international stakeholders as a key approach for health service delivery, as a response to increasingly complex forms of organisation defined by rapid changes in health technology, professionalisation and growing specialisation. Inadequate leadership weakens health systems and can contribute to adverse events, including refusal to prioritise and implement safety recommendations consistently, and resistance to addressing staff burnout. Globally, increases in life expectancy and the number of people living with multiple long-term conditions contribute to greater complexity of healthcare systems. Such a complex environment requires the contribution and leadership of multiple professionals sharing viewpoints and knowledge.

Objectives

To assess the effects of collective leadership for healthcare providers on professional practice, healthcare outcomes and staff well-being, when compared with usual centralised leadership approaches.

Search methods

We searched CENTRAL, MEDLINE, Embase, five other databases and two trials registers on 5 January 2021. We also searched grey literature, checked references for additional citations and contacted study authors to identify additional studies. We did not apply any limits on language.

Selection criteria

Two groups of two authors independently reviewed, screened and selected studies for inclusion; the principal author was part of both groups to ensure consistency. We included randomised controlled trials (RCTs) that compared collective leadership interventions with usual centralised leadership or no intervention.

Data collection and analysis

Three groups of two authors independently extracted data from the included studies and evaluated study quality; the principal author took part in all groups. We followed standard methodological procedures expected by Cochrane and the Effective Practice and Organisation of Care (EPOC) Group. We used the GRADE approach to assess the certainty of the evidence.

Main results

We identified three randomised trials for inclusion in our synthesis. All studies were conducted in acute care inpatient settings; the country settings were Canada, Iran and the USA. A total of 955 participants were included across all the studies. There was considerable variation in participants, interventions and measures for quantifying outcomes. We were only able to complete a meta-analysis for one outcome (leadership) and completed a narrative synthesis for other outcomes. We judged all studies as having an unclear risk of bias overall.

Collective leadership interventions probably improve leadership (3 RCTs, 955 participants). Collective leadership may improve team performance (1 RCT, 164 participants). We are uncertain about the effect of collective leadership on clinical performance (1 RCT, 60 participants). We are uncertain about the intervention effect on healthcare outcomes, including health status (inpatient mortality) (1 RCT, 60 participants). Collective leadership may slightly improve staff well-being by reducing work-related stress (1 RCT, 164 participants). We identified no direct evidence concerning burnout and psychological symptoms. We are uncertain of the intervention effects on unintended consequences, specifically on staff absence (1 RCT, 60 participants).

Authors' conclusions

Collective leadership involves multiple professionals sharing viewpoints and knowledge with the potential to influence positively the quality of care and staff well-being. Our confidence in the effects of collective leadership interventions on professional practice, healthcare outcomes and staff well-being is moderate in leadership outcomes, low in team performance and work-related stress, and very low for clinical performance, inpatient mortality and staff absence outcomes. The evidence was of moderate, low and very low certainty due to risk of bias and imprecision, meaning future evidence may change our interpretation of the results. There is a need for more high-quality studies in this area, with consistent reporting of leadership, team performance, clinical performance, health status and staff well-being outcomes.

PLAIN LANGUAGE SUMMARY

Does collective leadership improve healthcare professionals' actions, patient health care and staff well-being?

Key messages

Collective leadership involves multiple professionals sharing viewpoints and knowledge. Based on the available evidence, we cannot be sure it makes much difference for professional actions, patient health care or staff well-being. Our confidence in these results varies from moderate to very low, severely limited by the low quality and number of included studies.

What did we want to find out?

We aimed to see whether experiences with collective leadership (as opposed to more centralised and hierarchical leadership styles) improve professional actions, patient health care, and staff well-being. We looked for studies where researchers compared collective leadership with centralised leadership.

What did we do?

We collected and analysed all relevant studies with collective leadership interventions characterised by sharing decisions and interactions among health professions.

What did we find?

We found three relevant studies (955 participants). The studies were carried out in hospitals in Canada, Iran and the USA. Collective leadership interventions probably improve leadership (3 studies, 955 participants), may improve teamwork (1 study, 164 participants), and may slightly decrease work-related stress (1 study, 164 participants). We do not know if collective leadership has an effect on these outcomes: clinical performance (1 study, 60 participants), inpatient deaths (1 study, 60 participants), and staff absence (1 study, 60 participants).

What are the limitations of the evidence?

We are moderately confident that collective leadership improves leadership in healthcare settings. The evidence showed that collective leadership had a large effect on leadership strategies. We are less confident in our results about teamwork and work-related stress. We are not confident in the evidence related to clinical performance, inpatient deaths and staff absence. It is possible that people in the studies were aware of which intervention they were getting. Not all studies provided data about everything that we were interested in. The evidence is based on few cases.

How up to date is this review?

We searched for studies published up to January 2021.

SUMMARY OF FINDINGS

Summary of findings 1. Collective leadership to improve professional practice, healthcare outcomes and staff well-being compared with usual centralised and hierarchical leadership

Effect of collective leadership to improve professional practice, healthcare outcomes and staff well-being compared with usual centralised and hierarchical leadership

Patients or population: healthcare professionals

Settings: tertiary care (hospitals: trauma centre and clinical units)

Intervention: collective leadership interventions to improve professional practice, healthcare outcomes and staff well-being

Comparison: usual approaches to leadership identified as centralised and hierarchical

Outcomes	Impact	Number of participants (studies)	Certainty of the evidence (GRADE)*
Professional practice			
Leadership	Average difference (SMD): 0.78 higher (95% CI 0.19 to 1.38 higher) Collective leadership probably improves leadership ^{a,b,c} .	955 participants (3 RCTs)	⊕⊕⊕⊕ Moderate. Downgraded due to unclear risk of bias ^{a,b,c} and serious imprecision. Upgraded due to large magnitude of effect.
Team performance	Average difference (SMD): 0.47 higher (95% CI 0.16 to 0.78 higher) Collective leadership may improve team performance ^a .	164 participants (1 RCT)	⊕⊕⊕⊕ Low. Downgraded due to unclear risk of bias ^a and serious imprecision.
Clinical performance	Average difference (SMD): 0.21 higher (95% CI -0.30 to 0.72) It is uncertain whether collective leadership improves clinical performance ^c .	60 participants (1 RCT)	⊕⊕⊕⊕ Very low. Downgraded due to unclear risk of bias ^c and very serious imprecision.
Health care outcomes			
Inpatient mortality	Relative effect (RR): 0.86 lower (95% CI 0.42 to 1.77) Absolute difference: 17 fewer deaths per 1000 patients (95% CI 73 fewer to 96 more per 1000 patients) It is uncertain whether collective leadership decreases inpatient mortality ^c .	60 participants (1 RCT)	⊕⊕⊕⊕ Very low. Downgraded due to unclear risk of bias ^c and very serious imprecision.

Staff well-being

Work-related stress	Average difference (SMD): 0.17 lower (95% CI -0.48 to 0.13) Collective leadership may slightly decrease work-related stress ^a .	164 participants (1 RCT)	⊕⊕⊕⊕ Low. Downgraded due to unclear risk of bias ^a and serious imprecision.
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Unintended consequences

Staff absence	Average difference (SMD): 0.2 higher (95% CI -0.11 to 0.51) It is uncertain whether collective leadership prevents staff absence ^a .	60 participants (1 RCT)	⊕⊕⊕⊕ Very low. Downgraded due to unclear risk of bias ^a and very serious imprecision.
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GRADE Working Group grades of evidence

High: this research provides a very good indication of the likely effect. The likelihood that the effect will be substantially different is low.

Moderate: this research provides a good indication of the likely effect. The likelihood that the effect will be substantially different is moderate.

Low: this research provides some indication of the likely effect. However, the likelihood that it will be substantially different is high.

Very low: this research does not provide a reliable indication of the likely effect. The likelihood that the effect will be substantially different is very high.

^aWeir 1997, which employed the Work Environment Scale, Peer Cohesion Scale, Work Pressure Scale, and measured paid staff absence hours.

^bShirazi 2016, which employed the Supportive Leadership Behaviour scale.

^cFernandez 2020, which employed the Leadership Taxonomy and Total Patient Care score.

BACKGROUND

Collective leadership is leadership that involves all staff members and patients, where everyone shares responsibility for the success of patient care and the healthcare service. This contrasts with traditional hierarchical leadership approaches, which are focused on individual capabilities. Collective leadership requires leaders to adopt strategies for collaborative working and collective openness. Leaders that work as a collective group may provide improved patient experience and safety, continuing quality improvement in health care, and create a culture of engagement that promotes staff autonomy, accountability and well-being (Eckert 2014; West 2014a).

Collective leadership is not a new concept, but it is increasingly and strongly advocated by international stakeholders as a key approach for health service delivery, as a response to increasingly complex forms of organisation defined by rapid changes in health technology, professionalisation and growing specialisation. Such a complex environment requires the contribution and leadership of multiple professionals sharing viewpoints and knowledge (Raelin 2011; Raelin 2018; West 2014a).

Globally, increases in life expectancy and the number of people living with multiple long-term conditions contribute to greater complexity of healthcare systems. This context requires a collective leadership culture that values and promotes care that is high quality, compassionate (West 2014a; West 2014b), and interprofessional (Folkman 2019; Reeves 2010; Reeves 2018). This is evidenced by a recent Cochrane Review (Reeves 2017). The case for collective leadership is justified by the need to employ professional skills more effectively, and to bring social and healthcare professionals together to improve quality of care (West 2014a; West 2014b). This is an interactive and interdependent process (Orchard 2019).

Description of the condition

Collective leadership is a way of characterising the engagement of multiple healthcare team members to come together to make decisions and strengthen health service and system performance, towards quality of care enhancement. Leadership challenges in health and social care teams arise partly as a consequence of the siloed, mono-professional education of health and social care workers, and of restrictive professional regulation that seeks to define the scope of practice for members of some professional groups while restricting activities to others (Reeves 2010). Moreover, historically, the autonomy and work model that has dominated in medicine generates a hierarchical relationship by subordination, limitation and exclusion, giving rise to leadership challenges. In this context, leaders require a change in posture to move team members away from professional competition and towards a patient-centred and collaborative approach.

Description of the intervention

The review sought to establish the evidence base for collective leadership interventions developed for health and social care professionals, with or without patient involvement, at any level of the healthcare system, including primary, secondary and tertiary care.

Collective leadership is a polysemic concept and the recent literature uses different terms interchangeably, describing

leadership as: 'shared' (Aube 2018; Aufegger 2019; D'Innocenzo 2016; Fischer 2018; Forsyth 2017; Idelji-Tehrani 2019; Jackson 2000; Janssens 2021; Kelley-Patterson 2012; Merckens 1998; Nicolaides 2014; Wang 2014); 'collective' (Denis 2001; Friedrich 2009; McAuliffe 2017; Raelin 2011; Raelin 2018; Ward 2018; West 2014a; Yammarino 2012); 'collectivistic' (De Brún 2019; Yammarino 2012); 'distributed' (Gronn 2002); 'collaborative' (Iachini 2019; Markle-Reid 2017; Okpala 2018; Orchard 2019; Sonnino 2016; VanVactor 2012; Wang 2018); 'team' (Crowe 2017; Farh 2018; Smith 2018); or 'interprofessional' (McGrath 2019; Smith 2018).

For the purposes of the current review, we define collective leadership interventions as those which explicitly aim to improve professional practice, healthcare outcomes, or both, through fostering mechanisms to:

- share decisions in an interactive, interdependent process;
- clarify roles and sharing responsibilities across team members; and
- empower and motivate staff.

Collective leadership can be established in two different ways: either through a non-hierarchical leadership with spontaneous leaders (informal/influential) emerging naturally; or through a hierarchical leadership, with leaders formally appointed by the organisation (D'Innocenzo 2016; De Brún 2019; Nicolaides 2014; Wang 2014). The path to collective leadership normally begins with exchanging information for collective situation analysis, sharing meanings, and setting common goals and understanding among team members. It requires partnership between leaders and team members to solve problems through delegation of responsibilities, empowerment and decision-sharing, to make best use of relevant knowledge and expertise.

In this review, we were concerned with collective leadership interventions that focused on either patient case management, professional management or organisational strategies. Patient case management refers to interventions which involve patients, families, caregivers and professionals. Professional management considers all initiatives that promote professional development through short- or long-term training in general, or specific formal education. This kind of intervention can be organised inside or outside of healthcare facilities in workshops, e-learning, short courses, co-design, educational modules and programmes. Organisational strategies include practices and routines to introduce or improve resources, equipment, work dynamics and new clinical protocols pertaining to collective leadership.

Some examples of collective leadership interventions include:

- a multi-component leadership intervention that involves having team leaders attend a workshop focused on responsive leadership; implementing care-team huddles into daily practice; and implementing a team leaders' support system (Caspar 2017);
- a supportive leadership workshop designed for head nurses (Shirazi 2016);
- a simulation with nurses and doctors sharing leadership during cardiopulmonary resuscitation (Armstrong 2020); and
- a co-designed collective leadership intervention to improve team performance and safety culture (McAuliffe 2017).

How the intervention might work

We followed the initial programme theory for collective leadership, which depicts context-mechanism-outcome configurations (De Brún 2020). De Brún and collaborators described the contextual conditions for collective leadership as team training, co-design strategies, dedicated time for team reflection, inclusive communication, shared decision-making processes and strong interpersonal relationships with teams (De Brún 2020). We used this to inform identification of relevant interventions and outcomes.

Collective leadership interventions may result in improved staff engagement, satisfaction, empowerment, collaborative decision-making, communication, role and goal clarity, understanding about teamwork, mutual respect, trust, self-confidence in contributing to work (Aufegger 2019; De Brún 2019), proactive helping behaviours, patient satisfaction, and improvements in quality and safety care (De Brún 2020). It may also lead to cost reduction (Okpala 2018). Studies have repeatedly suggested a positive relationship between collective leadership approaches and team effectiveness or team performance (Aufegger 2019; D'Innocenzo 2016; De Brún 2019; Nicolaides 2014; Wang 2014).

The dynamics of collective leadership are team-based, involving formal, informal and hierarchical analysis (Yammarino 2012). A key feature of the collective leadership process is having the leader as a facilitator of interactions between team members. This facilitation role aims to generate a common understanding and agreement around team goals (Friedrich 2009; Smith 2018), and identify potential areas of contribution by team members. This collective approach to leadership addresses a limitation identified in traditional hierarchical methods, in which leaders are responsible for taking decisions that impact multiple individuals (Raelin 2018; Yammarino 2012).

The intervention can be applied equally to health settings across lower-, middle-, and high-income countries. We acknowledge that research to date has focused on the latter. There is no evidence that the intervention can disproportionately affect people from a lower- or middle-income country setting. However, the review was sensitive to such issues and we sought to capture any outcomes or implications that may widen health inequity.

Why it is important to do this review

Inadequate leadership weakens health systems and can contribute to adverse events, including refusal to prioritise and implement safety recommendations consistently, and resistance to addressing staff burnout (Joint Commission 2017). Quality of leadership is consistently identified as a factor that contributes to safety and quality failings, but is also central to solutions that minimise preventable health system issues and enhance staff well-being (Eckert 2014; West 2014a; West 2014b). Contemporary health systems operate with multiple leaders throughout every level; thus, the notion of collective leadership holds critical importance for health system enhancement. Yet we lack evidence of the effects of collective leadership interventions in health and social care teams. Therefore, the current review is both timely and important.

Collective leadership is an important component in developing and sustaining a culture of high-quality care, which requires a leadership commitment to:

- translate the collective vision into actions;
- set clear objectives aligned with organisational priorities;
- feed back evidence of positive patient outcomes to the team to inform future actions;
- promote a culture of engagement related to decision-making; and
- foster ongoing learning and quality improvement by listening to patients, caregivers and team members (West 2014a).

Previous attempts to summarise the literature on collective leadership in healthcare settings share some limitations, but have suggested promising outcomes for team engagement (Aufegger 2019; De Brún 2019), and organisational performance (De Brún 2019). For example, Janssens and collaborators completed a review on shared leadership in healthcare action teams, which focused on emergency care teams (Janssens 2021). They considered different forms of shared leadership: institutionalised practices, sharing-to-mentor to develop team member leaders, and spontaneous collaboration to improve ineffective leadership. De Brún and collaborators analysed interventions classified as co-design, co-leadership, service improvement, team training and individual team training in health care. They took out MeSH headings to narrow their search and restricted them to publications since 2000, so they may have missed some studies (De Brún 2019). Moreover, a review of acute healthcare teams suggested that shared leadership enables understanding of roles and tasks, awareness, social support, environmental safety and team satisfaction (Aufegger 2019). However, in these reviews, the authors expressed caution about the limitations of the evidence and their approaches.

We are still lacking a high-quality systematic review that specifically evaluates the effects of collective leadership interventions to improve professional practice, healthcare outcomes and staff well-being. This hinders health professionals, policy makers and researchers' ability to plan effective collective leadership interventions.

OBJECTIVES

To assess the effects of collective leadership for healthcare providers on professional practice, healthcare outcomes and staff well-being, when compared with usual centralised leadership approaches.

METHODS

Criteria for considering studies for this review

Types of studies

We considered any study meeting the study design criteria from the Cochrane Effective Practice and Organisation of Care Group (EPOC 2017a): randomised trials; non-randomised trials; controlled before-and-after studies with contemporaneous data collection and with two or more control and intervention sites; repeated measures studies and interrupted time series (ITS) studies with a clearly-defined point in time when the intervention occurred, and at least three data points before and after implementation of the intervention.

We included studies that were available as a full text, regardless of publication status or language. We included abstracts and associated these with full-text studies. For abstracts of studies

that were potentially eligible, we searched for a corresponding publication and also contacted study authors to obtain any unpublished full study report, if available.

Types of participants

We considered study interventions that targeted any type of health and social care team - uniprofessional, multiprofessional or interprofessional - from different professional areas (e.g. chiropodists or podiatrists, complementary therapists, dentists, dieticians, doctors or physicians, hygienists, midwives, nurses, occupational therapists, pharmacists, physiotherapists, psychologists, psychotherapists, radiographers, social workers or speech therapists), with or without the inclusion of patients. We did not exclude studies on the basis of demographic factors or healthcare setting.

Types of interventions

We considered all collective leadership interventions that:

- had an explicit aim to improve professional practice and healthcare outcomes;
- included a focus on sharing decisions in an interactive and interdependent process;
- fostered role clarification and sharing of responsibilities; and
- aimed to enable staff empowerment and motivation.

Interventions could include workshops, coaching (e.g. Caspar 2017), simulation, protocols (e.g. Armstrong 2020), and role-playing (e.g. Shirazi 2016).

We were interested in professional-directed collective leadership interventions, which may also be referred to as collectivistic leadership, distributed leadership, shared leadership, collaborative leadership, participatory leadership, inclusive leadership, democratic leadership, plural leadership, dispersed leadership, empowering leadership, compassionate leadership, informal leadership, peer leadership and team leadership. We were concerned with collective leadership interventions with a focus on patient case management, professional management or organisational strategies.

For this review, we planned the following comparisons in primary, secondary or tertiary healthcare settings:

- collective leadership versus usual approaches to leadership identified as centralised and hierarchical;
- collective leadership educational interventions with or without organisational restructuring;
- collective leadership interventions targeting frontline teams versus management teams.

We excluded interventions that focused exclusively on strengthening professional collaboration without explicit reference to collective leadership; this allowed us to complement and not overlap with the Cochrane Review on interprofessional collaboration by Reeves 2017.

Types of outcome measures

Primary outcomes

We defined the following primary outcomes.

- Professional practice
 - Leadership, e.g. Distributed Leadership Agency (DLA) (Jönsson 2016)
 - Team performance, e.g. Non-Technical Skills (NOTECHS) (Sevdalis 2008), Team Climate Inventory (TCI) (Anderson 1996)
 - Clinical performance, e.g. time to critical intervention (Janssens 2021)
- Healthcare outcomes
 - Health status (inpatient mortality, mortality within 30 days of discharge)
- Staff well-being
 - Burnout, psychological symptoms (anxiety, depression) and work-related stress, e.g. Professional Quality of Life (ProQOL) (Stamm 2010)
- Unintended consequences
 - Any unintended safety of care events (including errors, healthcare-associated complications), e.g. Patient Measure of Safety (PMOS) (McEachan 2014), or professional, organisational and staff consequences

Secondary outcomes

We included these other patient and organisational outcomes as secondary outcomes:

- Satisfaction
 - Patient satisfaction and experience, e.g. Patient Satisfaction Questionnaire (PSQ) (Ware 1976)
- Organisational
 - Staff turnover (including intention to quit and absenteeism)

We included studies in the review even if they only reported secondary outcomes.

Search methods for identification of studies

We used a combination of search methods for the identification of studies, including electronic database searches, trial registries and grey literature searching.

Electronic searches

The review authors developed the database search strategy in consultation with the EPOC Information Specialist. We searched the Cochrane Database of Systematic Reviews (CDSR) and the Database of Abstracts of Reviews of Effects (DARE) for related systematic reviews.

We searched the following databases for primary studies on 5 January 2021:

- Cochrane Central Register of Controlled Trials (CENTRAL; 2021, issue 1), in the Cochrane Library;
- MEDLINE Ovid (1946 to date of search);
- Embase Ovid (1974 to date of search);

- CINAHL EBSCO (Cumulative Index to Nursing and Allied Health Literature; 1982 to date of search);
- LILACS (Latin American and Caribbean Health Sciences Information database; 1982 to date of search); and
- Web of Science, Conference Proceedings Citation Index - Science (1990 to date of search).

Search strategies were comprised of natural language and controlled vocabulary terms. We applied no limits on language or publication date. In databases where it was possible and appropriate, we used study design filters to limit to the study designs of interest. For randomised trials in MEDLINE, we used a modified version of the Cochrane Highly Sensitive Search Strategy for identifying randomised trials in MEDLINE: sensitivity- and precision-maximising version (2008 revision) (Lefebvre 2021), with additional terms for other relevant study designs. The MEDLINE strategy was peer reviewed by a second Cochrane Information Specialist. We used limits in CINAHL and Embase to remove MEDLINE records in order to avoid duplication in downloaded results. We de-duplicated the remaining results in Endnote against each other. Please see all search strategies used in Appendix 1.

Searching other resources

Trial registries

We searched the following trials registries on 5 January 2021:

- WHO ICTRP (World Health Organization International Clinical Trials Registry Platform; www.who.int/ictrp);
- US National Institutes of Health Ongoing Trials Register (www.clinicaltrials.gov).

Grey literature

We conducted a grey literature search to identify studies not indexed in the databases listed above by searching:

- OpenGrey (www.opengrey.eu);
- Grey Literature Report (New York Academy of Medicine; www.greylit.org);
- Agency for Healthcare Research and Quality (AHRQ; www.ahrq.gov);
- Joanna Briggs Institute (www.joannabriggs.edu.au); and
- National Institute for Health and Care Excellence (NICE; www.nice.org.uk).

We also:

- reviewed reference lists of all included studies and relevant systematic reviews for additional potentially eligible primary studies;
- contacted authors of included studies/reviews to clarify reported published information and to seek unpublished results/data;
- contacted researchers with expertise relevant to the review topic/EPOC interventions;
- conducted cited reference searches for all included studies in ISI Web of Science, Clarivate and screened individual journals and conference proceedings (handsearch);
- provided appendices for all strategies used, including a list of sources screened and relevant reviews/primary studies reviewed.

We contacted experts in the field and authors of included studies for advice on other relevant studies. We conducted handsearches in key e-journals, including the *Journal of Leadership and Organizational Studies*, *BMJ Leader*, *Leadership Quarterly* and *Journal of Management*.

Data collection and analysis

Selection of studies

We downloaded all titles and abstracts retrieved by electronic searching to a reference management database and removed duplicates. Two review author pairs (JAMS with MP, JAMS with VAM) independently screened titles and abstracts for inclusion. We retrieved the full-text study reports/publications and two review author pairs (JAMS with MP, JAMS with VAM) independently applied the eligibility criteria to the full texts, identified studies for inclusion, and identified and recorded reasons for exclusion of ineligible studies. We resolved any disagreement through discussion or, when required, through consulting a third review author (AX or RH).

We listed studies that initially appeared to meet the inclusion criteria but that we later excluded in the 'Characteristics of excluded studies' table, with the reasons for their exclusion. We collated multiple reports of the same study so that each study, rather than each report, is the unit of interest in the review. We also provided any information we could obtain about ongoing studies. We recorded the selection process in sufficient detail to complete a PRISMA flow diagram (Liberati 2009).

Data extraction and management

We used the EPOC standard data collection form and adapted it for study characteristics and outcome data (EPOC 2017b); we piloted the form on at least one study in the review. Two review author pairs (JAMS with MP, JAMS with VAM) independently extracted the following study characteristics from the included studies and enter the data into RevMan Web (RevMan Web 2022).

- Methods: study design, number of study centres and location, study setting, withdrawals, date of study, follow-up.
- Participants: number, mean age, age range, gender, eligibility criteria, other relevant characteristics.
- Interventions: intervention components, comparison, fidelity assessment.
- Outcomes: main and other outcomes specified and collected, time points reported.
- Notes: funding for trial, notable conflicts of interest of trial authors, ethical approval.

Two review author pairs (JAMS with MP, JAMS with VAM) independently extracted outcome data from included studies. We noted in the Characteristics of included studies table if a trial reported outcome data in an unusable way. We resolved disagreements by consensus or by involving a third review author (AX or RH).

Assessment of risk of bias in included studies

Two review author pairs (JAMS with MP, JAMS with VAM, or JAMS with HFA) independently assessed the risk of bias for each study, using the criteria outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* Section 7.1.2 and Chapter 8 (Higgins

2021a), and the guidance from the EPOC Group (EPOC 2017c). We resolved any disagreements through discussion or by involving a third review author (AX or RH). We assessed the risk of bias in randomised trials according to the following domains:

- random sequence generation;
- allocation concealment;
- blinding of participants and personnel;
- blinding of outcome assessment;
- incomplete outcome data;
- selective outcome reporting;
- baseline outcomes measurement;
- baseline characteristics;
- other bias

To analyse the risk of bias in non-randomised studies, we had planned to use the 'Risk of Bias in Non-randomized Studies of Interventions' (ROBINS-I) tool (Sterne 2016). For interrupted time series studies, we had planned to assess the risk of bias according to the following domains (EPOC 2017c):

- independence from other changes;
- specification of the shape of effects before analysis;
- independence from data collection;
- knowledge of intervention allocation;
- incomplete outcome data;
- selective outcome reporting;
- other bias.

We judged each potential source of bias to be high, low or unclear, and where appropriate, provided a quote from the study report with a justification for our judgement in the risk of bias table. We summarised the risk of bias judgements across different studies for each of the domains listed. We assigned an overall risk of bias assessment (high, moderate or low) to each of the included studies, using the approach suggested in Chapter 8 of the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2021a). We considered studies with a low risk of bias for all key domains, or where it seemed unlikely that bias would have seriously altered the results, to have a low risk of bias. We considered studies where risk of bias in at least one domain was unclear, or where we judged it to have some bias that could plausibly raise doubts about the conclusions, to have an unclear risk of bias. We considered studies to have a high risk of bias if they had a high risk of bias in at least one domain, or if we judged them to have serious bias that decreased the certainty of the conclusions.

We considered blinding separately for different key outcomes where necessary. Where information on risk of bias related to unpublished data or correspondence with a trialist, we noted this in the risk of bias table. We did not exclude studies on the grounds of their risk of bias, but clearly reported the risk of bias when presenting the results of the studies. When considering specific effects, we took into account the risk of bias for the studies that contributed to that outcome.

We conducted the review according to our published protocol and report any deviations from it in the 'Differences between protocol and review' section.

Measures of treatment effect

Where possible, we estimated the effect of the intervention using the risk ratio (RR) and risk difference (RD) for dichotomous data, with the appropriate associated 95% confidence interval (CI); and mean difference (MD) or standardised mean difference (SMD) for continuous data, with their 95% CIs (Higgins 2021a). We ensured that an increase in scores for continuous outcomes was interpreted in the same way for each outcome and explained the direction to the reader. For controlled before-and-after, ITS and repeated measures studies, we planned to report measures before and after the intervention, as well as the difference of the periods for specific time points.

Unit of analysis issues

We did not have issues with cluster-randomised trials that did not account adequately for the effects of the clustering on the effect estimate, so adjustments of the analyses to avoid unit of analysis errors was not required (EPOC 2017d).

Dealing with missing data

We attempted to contact the study authors, asking them to provide missing outcome data. Where this was not possible, we concluded that the missing data introduced serious bias impacting on the overall assessment of results (Sterne 2011).

Assessment of heterogeneity

We planned to assess clinical and methodological heterogeneity by considering study design, participants and how the collective leadership intervention was applied. We were only able to identify one outcome (leadership) for which we judged measures, participants, intervention approach and study designs of the included studies were sufficiently similar to conduct a meta-analysis and assess statistical heterogeneity (Borenstein 2009). For this outcome, we assessed statistical heterogeneity using the χ^2 statistic and related P value, alongside the I^2 statistic with associated percentage values (Higgins 2021a).

Assessment of reporting biases

We were not able to pool more than 10 trials so did not create and examine a funnel plot to explore possible publication biases (Sterne 2011).

Data synthesis

We could only undertake a meta-analysis for one outcome, for which pooling made sense. For the remaining outcomes, we provided a narrative description of the results. For the meta-analysis, we combined continuous data from leadership scales that were sufficiently similar (and where direction of effect was the same) using generic inverse variance and standardised mean difference to account for differences in the scales (Higgins 2021a). We used a random-effects model for the meta-analysis to account for possible differences among studies in which conditions of the healthcare setting and approach to collective leadership may have varied. We completed the meta-analysis using the Review Manager (RevMan Web) calculator.

In order to synthesise the remaining outcomes without meta-analyses, we considered the nine items suggested by Campbell 2020 to guide our reporting:

- group studies for synthesis;
- describe the standardised metric and transformation method used;
- describe the synthesis methods;
- describe the criteria used to prioritise results for summary and synthesis;
- investigate heterogeneity in reported effects;
- assess the certainty of the evidence;
- describe the data presentation methods;
- report the results; and
- report the limitations of the synthesis.

We noted the scales used to measure continuous outcomes, including the range of possible scores and direction of the effect, alongside the mean difference and 95% confidence interval, to enable more meaningful interpretation of results. In the summary of findings table, we present the standardised mean difference, calculated using the RevMan Web calculator, to allow easier comparison of the effect size of collective leadership on different outcomes. For one dichotomous outcome (death), we reported the relative risk next to the 95% confidence interval.

Subgroup analysis and investigation of heterogeneity

We included all kinds of health professionals involved across all types of clinical settings, but noted length of the intervention, implementation approach, use of explicit intervention protocols, organisational support and frequency of meetings. We planned for but were unable to carry out any of the following subgroup analyses:

- collective leadership hierarchical interventions subgrouped by setting (primary care, secondary care or tertiary care);
- collective leadership non-hierarchical interventions subgrouped by setting (primary care, secondary care or tertiary care);
- subgroup of collective leadership hierarchical interventions in high-income versus low-income setting;
- subgroup of collective leadership non-hierarchical interventions in high-income versus low-income setting.

To conduct the subgroup analysis, we planned to use all outcomes that were common to at least three studies, performing a separate analysis for each outcome. However, this was not possible with the included studies.

Sensitivity analysis

We planned to perform prespecified sensitivity analyses to assess the robustness of our conclusions and explore their impact on effect sizes. This would have involved the following:

- restricting the analysis to published studies;
- restricting the analysis to studies with a low risk of bias; and
- imputing missing data.

Our included studies and available data did not allow for sensitivity analysis to take place.

Summary of findings and assessment of the certainty of the evidence

Two review authors (JAMS and HFA) independently assessed the certainty of the evidence (high, moderate, low and very low) using the five GRADE considerations (risk of bias, consistency of effect, imprecision, indirectness and publication bias) (Guyatt 2008). We used methods and recommendations described in Chapter 8 and 15 of the *Cochrane Handbook for Systematic Reviews of interventions* (Higgins 2021a), and the EPOC worksheets (EPOC 2017e), and we used GRADEpro software (GRADEpro GDT). We resolved disagreements on certainty ratings through discussion, provided justification for decisions to downgrade or upgrade the ratings using footnotes in the table, and made comments to aid readers' understanding of the review where necessary. We used plain language statements to report these findings in the review (EPOC 2017e).

We summarised our results in 'Summary of findings 1' for the main intervention against each comparison described in the [Types of interventions](#). We included the most important outcomes in order to draw conclusions about the certainty of the evidence within the text of the review.

- Professional practice
 - Leadership
 - Team performance
 - Clinical performance
- Healthcare outcomes
 - Health status (inpatient mortality, mortality within 30 days of discharge)
- Staff well-being
 - Burnout, psychological symptoms (anxiety, depression) and work-related stress
- Unintended consequences
 - Any unintended safety of care events (including errors, healthcare-associated complications) or professional, organisational and staff consequences

We synthesised data on prespecified outcomes of collective leadership along with any other unintended consequences found in the included studies. We used the same eligibility criteria to assess intended (beneficial) and unintended (adverse) effects, in terms of types of studies, types of participants and types of interventions (Peryer 2019).

We considered whether there was any additional outcome information that we could not incorporate into the meta-analysis to note in the 'Comments' section, and stated if it supported or contradicted the information from the meta-analysis.

RESULTS

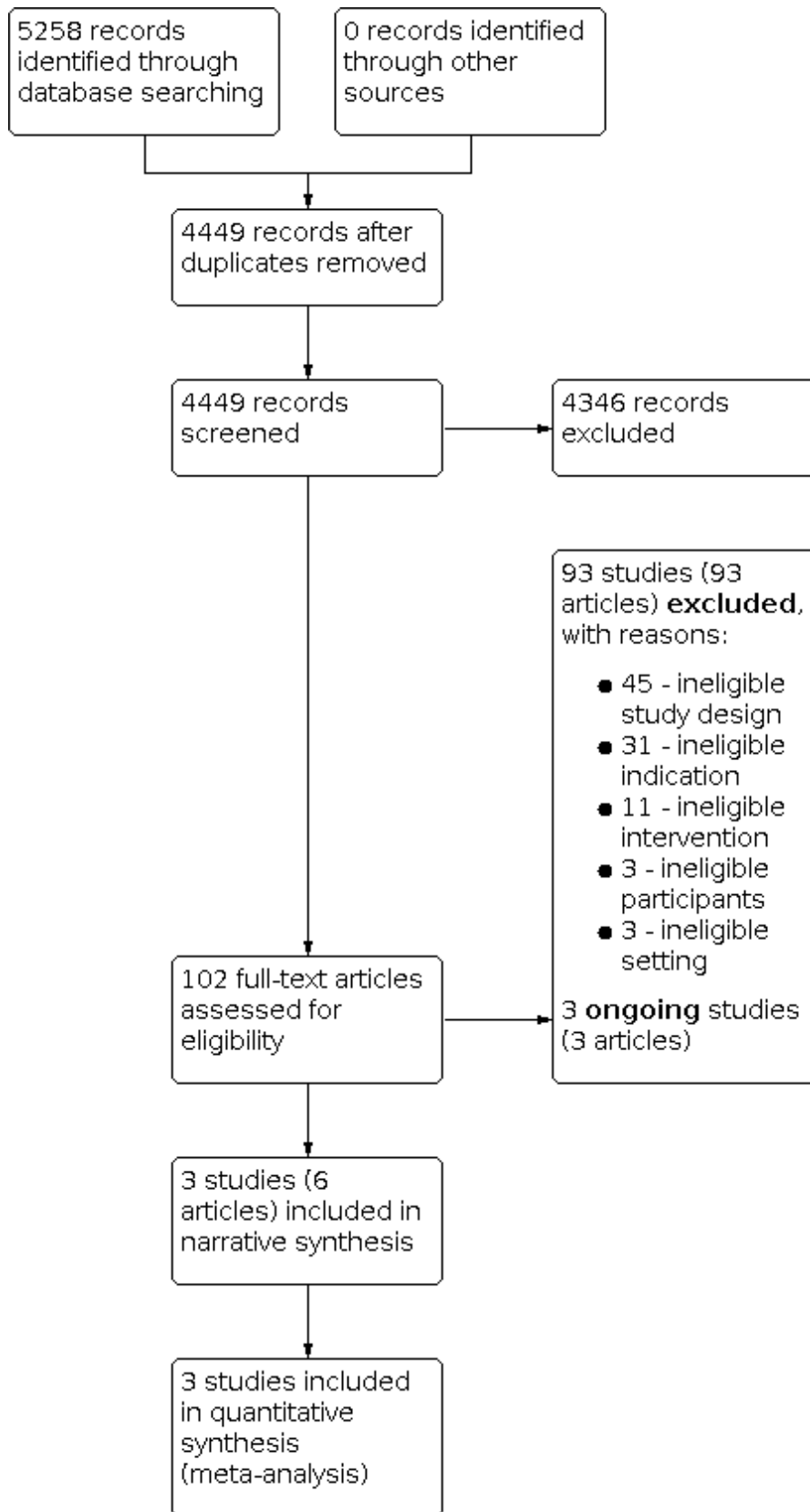
Description of studies

We retrieved 4449 records for title and abstract screening after removing duplicates ($n = 809$). We shortlisted 102 records for independent full-text screening by two review author pairs.

Results of the search

From the full texts, we identified three randomised trials eligible for inclusion. Handsearching did not produce any additional articles. The study selection is summarised in [Figure 1](#).

Figure 1. PRISMA study flow diagram



Included studies

We identified three randomised trials that met the inclusion criteria (Fernandez 2020; Shirazi 2016; Weir 1997). Follow-up measurements varied, while the level of analysis was on individuals rather than on the team or organisational level (Weir 1997; Shirazi 2016; Fernandez 2020).

Locations and participants

Studies originated from three different higher- and upper-middle income countries: Canada (Weir 1997), Iran (Shirazi 2016), and the USA (Fernandez 2020). All studies were conducted in tertiary care, including inpatient clinical units (Weir 1997), an academic hospital (Shirazi 2016), and a trauma centre (Fernandez 2020).

There was a total of 955 participants across all the studies. The number of professionals ranged from 60 (Fernandez 2020) to 731 (Shirazi 2016). The mean age varied between 20 (Fernandez 2020) and 44 years old (Weir 1997). Participants included nurse managers (Shirazi 2016; Weir 1997), nurses (registered nurses and nurse aids) (Shirazi 2016), general surgery and emergency medicine residents (Fernandez 2020), physical therapists, occupational therapists, social workers, clinical dieticians, respiratory technicians, and laboratory technicians (Weir 1997). Two studies included participants from uniprofessional areas such as medicine (Fernandez 2020) and nursing (Shirazi 2016). The remaining study included participants from different professional areas, including nursing, physical therapists, occupational therapists, social workers, clinical dieticians, respiratory technicians and laboratory technicians (Weir 1997).

Interventions

Two studies included professional management by leadership training called supportive leadership (Shirazi 2016) and team leadership (Fernandez 2020). For an organisational strategy, one study aimed to facilitate a decentralised and participatory style of problem-solving management (Weir 1997). The three studies adopted varied intervention strategies, including workshops (Shirazi 2016; Weir 1997), simulation (Fernandez 2020), and problem-solving sessions (Weir 1997).

In Weir 1997, the intervention was to facilitate a decentralised and participatory style of problem-solving management meeting (participative decision-making), with unit staff members and their nurse managers versus the more traditional centralised approach to decision-making. The intervention duration was 12 months, delivered by external consultants with experience in resolving problem situations and developing a helping relationship. All consultants had expertise in mental health consultation and small group work, as well as problem-based learning in professional, undergraduate and graduate programs.

Shirazi 2016 developed an interactive workshop on supportive leadership behaviour for head nurses working in academic hospitals; the workshop included supervisors and nursing personnel. One-day, 8-hour workshops were conducted by experts in the leadership field, utilising teaching methods, role-playing, mini-lectures and the modified 'goldfish bowl technique' (a typical small-group technique used in medical education).

Fernandez 2020 used simulation-based training on team leadership and patient care during trauma resuscitation in a medical trauma centre with emergency medicine residents. Attending physicians and senior residents in both emergency medicine and trauma surgery supervised a 4-hour leadership curriculum focused on assuming a leadership role, sharing information and interpreting data, planning and prioritising tasks, assigning roles and assessing team members' skills, and seeking input and identifying tasks barriers. Sessions began with a 1-hour didactic session, followed by a series of simulations in which one participant served as the team leader while the second participant was observed. An instructor-led debrief occurred after each simulation.

Each included study adopted a slightly different conception of collective leadership, though all were characterised by sharing decisions and interactions among health professions. Fernandez 2020 referred to team leadership focused on team performance by team-oriented goals, facilitating coordination and improving the working environment. Shirazi 2016 applied supportive leadership, as defined by Cummings 2010, as a process in which leaders consider staff needs for motivation, based on intellectual stimulation, individual consideration and effective communication. Weir 1997 adopted a decentralised leadership approach, through participative decision-making towards effective problem-solving and relationship skills.

Excluded studies

We excluded 93 studies (see Characteristics of included studies). The most common reasons for exclusion were: ineligible study design (n = 45); ineligible intervention (n = 11); ineligible indication (n = 31), when the study was not about collective leadership as a topic and did not mention any aspects of collective leadership as a concept; ineligible participants (n = 3); ineligible setting (n = 3). We also identified three ongoing studies.

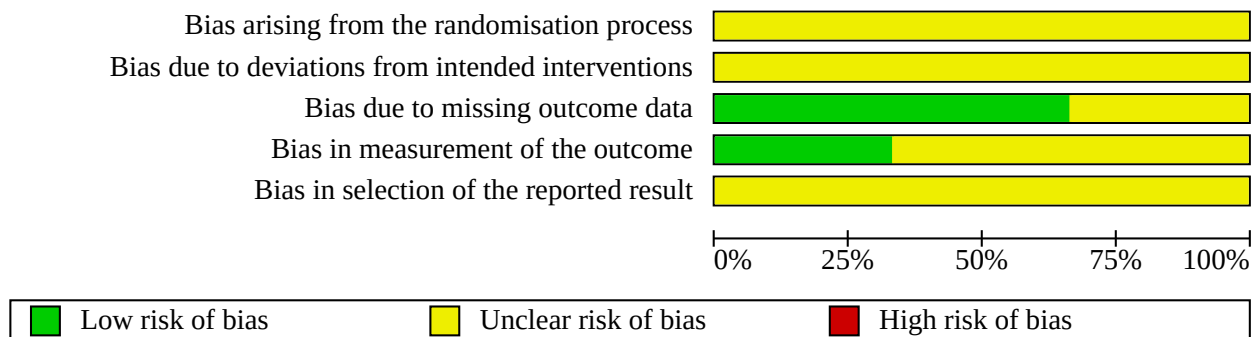
Risk of bias in included studies

We used Cochrane's Risk of Bias 2 (RoB2) tool for assessing the risk of bias in each study. We present a summary of our judgements in Figure 2 and Figure 3. We applied RoB2 for the three included RCTs (Fernandez 2020; Shirazi 2016; Weir 1997), following the guidance for the revised Cochrane risk of bias tool for randomised trials (Higgins 2021a). The risk of bias for each randomised trial included in this review is available in the Characteristics of included studies, and summarised below.

Figure 2. Risk of bias summary: review authors' judgements about each risk of bias item for each included study

	Bias arising from the randomisation process	Bias due to deviations from intended interventions	Bias due to missing outcome data	Bias in measurement of the outcome	Bias in selection of the reported result
Fernandez 2020	?	?	+	+	?
Shirazi 2016	?	?	+	?	?
Weir 1997	?	?	?	?	?

Figure 3. Risk of bias graph: review authors' judgements about each risk of bias item



Allocation

Randomisation process

All studies included presented some concerns with unclear randomisation processes, because of insufficient information provided by the authors (Fernandez 2020; Shirazi 2016; Weir 1997). None of the studies gave information concerning a random component being used in the sequence generation process; the only information about randomisation methods were statements that the studies were randomised. None of the studies gave information about whether the allocation sequence was concealed until participants were enrolled and assigned to the intervention. No baseline imbalances were apparent between groups to suggest a problem with the randomisation process.

Blinding

Deviations from intended interventions

We assessed all three studies as having an unclear risk of bias in this domain because they provided no information about deviations from the intended intervention due to the trial context. Additionally, none of the studies provided information about whether participants were aware of their assigned intervention during the trial. In all three studies, the people delivering the intervention were probably aware of participants' assigned intervention during the trial (Fernandez 2020; Shirazi 2016; Weir 1997). In Shirazi 2016, allocation was concealed from participants; however, the nature of the intervention gives strong reason to believe participants could become aware of their assigned intervention during the trial.

Incomplete outcome data

Missing outcome data

We judged two studies to have a low risk of bias in this domain (Fernandez 2020; Shirazi 2016). Outcome data were available for nearly all randomised participants. These studies gave no information about the reasons for withdrawals, which were balanced between the groups. For all three included studies, there is no evidence that the result was not biased by missing outcome data. It is not probable that missingness in the outcomes could depend on true value. We judged Weir 1997 as having an unclear risk of bias because outcome data were not available for all randomised participants. Missingness in the outcome could depend on its true value, but this is unlikely.

Selective reporting

Measurement of the outcome

In one study (Fernandez 2020), we assessed the risk of bias as low because the methods of measuring the outcomes were appropriate. The measurement of the outcomes did not differ between groups. The raters were blind to study hypotheses and experimental conditions of participants (Fernandez 2020). We assessed the remaining two studies as having an unclear risk of bias because their methods of measuring the outcomes were probably appropriate (Shirazi 2016; Weir 1997). These studies used a validated tool, though details of its validity were not provided. The measurement of the outcome did not differ between groups. Study participants were the outcome assessors, and given the self-reported outcome, were probably aware of intervention status. Assessment of the outcome could have been influenced

by knowledge of the intervention received, but there is no strong reason to believe that it did.

Selection of the reported result

We judged all three studies to have an unclear risk of bias because they did not analyse data in accordance with a prespecified plan finalised before unblinded outcome data were available for analysis (Fernandez 2020; Shirazi 2016; Weir 1997). Authors' intentions for their analysis were unavailable, or their intentions were not reported in sufficient detail to enable an assessment. The results reported were not likely to have been selected, on the basis of the results, from multiple eligible outcome measurements (e.g. scales, definitions, time points) within the outcome domain.

Other potential sources of bias

We did not identify other potential sources of bias.

Effects of interventions

See: [Summary of findings 1 Collective leadership to improve professional practice, healthcare outcomes and staff well-being compared with usual centralised and hierarchical leadership](#)

We were only able to complete a meta-analysis for one outcome (leadership) and completed a narrative synthesis for other outcomes. A meta-analysis of effect estimates was not possible for all outcomes because of incompletely-reported outcome/effect estimates, and different effect measures across studies alongside risk of bias in the evidence (McKenzie 2021).

See [Summary of findings 1](#).

Professional practice: leadership, team performance and clinical performance

Collective leadership interventions probably improve leadership (3 RCTs, 955 participants), and may improve team performance; however, we are uncertain about their effect on clinical performance. The studies included in this review adopted different scales. To facilitate interpretation of the results, we describe these scales below, together with significant results.

Leadership

Three studies reported on leadership outcomes using different scales. Weir 1997 applied a composite measurement with outcome variables from the Work Environment Scale (WES) (Moos 1981). The WES is a 90-item, true-false, self-reported questionnaire consisting of ten subscales; scores on each subscale can range from 0 to 9. The subscale on 'supervisory support' offers an indicator for leadership, for which no statistically significant differences were found (mean difference (MD) 2.73 favouring the intervention, 95% confidence interval (CI) -1.98 to 7.44).

Shirazi 2016 developed a Supportive Leadership Behaviour (SLB) questionnaire, using items from three different leadership scales: Ohio State (OSQ) (Larsson 2006); Developmental leadership (DL) (Hersey 1979), and Hersey and Blanchard's situational theory (HBS) (Halpin 1962). Some items related to SLB were extracted from these questionnaires and some new questions were developed and validated by the authors. The SLB final version is an instrument with 40 items organised in a 5-point Likert scale with four dimensions: support for development, integrity, sincerity and recognition. Each item is scored between 1 and 5, yielding an overall score ranging

from 40 to 200. Statistically significant results were found for overall leadership at three months' follow-up, favouring the intervention (MD 21.41, 95% CI 18.16 to 24.61).

[Fernandez 2020](#) adapted a leadership taxonomy from [Rosenman 2016](#), designed to capture a team leader's best effort when targeting behaviours and communication events. It consists of 10 items grouped under five dimensions, with an overall score ranging from zero to 38. There was a statistically significant improvement in overall leadership score, favouring the intervention (MD 4.06, 95% CI 2.45 to 5.67).

We combined data from the three studies in a meta-analysis using a random-effects model and reporting standardised mean difference, taking into account the differences in the leadership scales used. We found an effect size of 0.78 (95% CI 0.19 to 1.38; $P = 0.01$), which, following the standard rule of thumb, we interpret as moderate difference ([Analysis 1.1](#)).

Team performance

One study provided outcomes indicative of team performance as peer cohesion, relationship and task orientation using the WES ([Weir 1997](#)). A statistically significant result was only found in the peer cohesion subscale, favouring the intervention (MD 7.08, 95% CI 2.45 to 11.71).

Clinical performance

One study reported on a clinical performance outcome as patient care overall score (maximum scores ranging from 20 to 38), but without a statistically significant result (MD 2, 95% CI -2.78 to 6.78) ([Fernandez 2020](#)).

Healthcare outcomes: health status (inpatient mortality, mortality within 30 days of discharge)

We are uncertain of the collective leadership interventions' effect on health care outcomes. Only one study reported patient deaths, but not specifically at 30 days from discharge, with no statistically significant results (relative risk 0.86, 95% CI 0.42 to 1.77) ([Fernandez 2020](#)).

Staff well-being: burnout, psychological symptoms (anxiety, depression) and work-related stress

We are uncertain of the collective leadership interventions' effect on staff well-being. One study described the effects of work-related stress as 'work pressure' using the WES, but without any statistically significant results (MD -2.24, 95% CI -6.18 to 1.70) ([Weir 1997](#)).

Unintended consequences: any unintended safety of care events (including errors, healthcare-associated complications) or professional, organisational and staff consequences

We are uncertain of the effect collective leadership interventions have on unintended consequences. One study reported on staff absences (paid hours) as organisational and staff unintended consequences, without a statistically significant effect (MD 20.35, 95% CI -10.65 to 51.35) ([Fernandez 2020](#)).

DISCUSSION

Summary of main results

Three RCTs met our inclusion criteria. Overall, 955 participants were involved in these studies. Collective leadership interventions in these studies were conducted in Canada, Iran and the USA in tertiary care, including a trauma centre and clinical units. There was variation in participants, interventions and measures for quantifying outcomes. We were only able to complete a meta-analysis for one outcome (leadership); we completed a narrative synthesis for other outcomes.

Collective leadership interventions probably improve leadership (3 RCTs, 955 participants), and may improve team performance (1 RCT, 164 participants). We are uncertain about the collective leadership effect on clinical performance (1 RCT, 60 participants). We are uncertain about the effect on healthcare outcomes, including health status (inpatient mortality) (1 RCT, 60 participants). We identified no direct evidence related to mortality within 30 days of discharge. Collective leadership interventions may slightly improve staff well-being by reducing work-related stress (1 RCT, 164 participants). We are uncertain of the intervention effects on unintended consequences, specifically staff absence (1 RCT, 60 participants). Most of this evidence comes from studies with small numbers of participants.

The majority of interventions were uniprofessional and focused on professional management. There was little consensus about the operationalisation of collective leadership, although the studies shared commonalities in their definition of collective leadership as sharing decisions and interactions among professionals.

Overall completeness and applicability of evidence

Many of the relevant outcomes in our protocol were not reported in the included studies. None of the included studies evaluated the impact of collective leadership interventions on staff well-being (specifically, burnout or psychological symptoms (anxiety, depression)) or unintended consequences in professional or organisational outcomes. We found no studies involving unintended safety of care events (including errors and healthcare-associated complications).

The interventions were heterogeneous in nature. Two studies were conducted in higher-income countries, and one from an upper-middle-income country, which makes inference on the generalisability of the observed effects of the interventions to lower- and middle-income countries difficult.

Quality of the evidence

All included studies were randomised controlled trials (RCTs). Our confidence in the effects of collective leadership interventions on professional practice, healthcare outcomes and staff well-being is moderate in leadership outcomes, low in team performance and work-related stress, and very low for clinical performance, inpatient mortality and staff absence outcomes. The evidence was of moderate, low and very low certainty, meaning future evidence may change our interpretation of the results. Reasons for downgrading the certainty of evidence included risk of bias when all studies were judged at unclear risk of bias; and imprecision, when the absolute number of events was low, confidence intervals were wide and included no difference, or both.

Potential biases in the review process

We carried out the review in accordance with EPOC guidelines and using the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2021a; Higgins 2021b). The COVID-19 pandemic led to significant and unexpected workload increases for all review team members. The scarcity of intervention studies in the collective leadership field led to the inclusion of only three small studies with a negative impact on the certainty of the evidence produced. We could not conduct subgroup analysis and investigation of heterogeneity as planned, because the presentation of the outcomes was diverse among the included studies. We were also unable to retrieve some missing data from study authors. It is possible that non-reporting of information in the published articles may have influenced the risk of bias assessments.

Agreements and disagreements with other studies or reviews

We did not identify any reviews that address the same objectives as this review. Related reviews have pointed to similar results with regard to team engagement and performance outcomes, though our confidence about this evidence is low (Aufegger 2019; De Brún 2019; Janssens 2021). This study substantially expands the scope and comprehensiveness of previous reviews. In our review, we identified different effects of collective leadership interventions, including: professional practice – leadership and clinical performance; healthcare outcomes – inpatient mortality; staff well-being – work-related stress; and unintended consequences – staff absence. Like this review, other reviews were limited by methodological aspects of the included studies, heterogeneous interventions, comparison groups and multiple outcomes.

While collective leadership has the potential for positive influences on professional practice, healthcare outcomes and staff well-being, only three studies reported outcomes potentially attributable to collective leadership and the quality of evidence was moderate, low and very low, respectively.

The low number of included studies is indicative of the limited literature examining the effects of collective leadership models in health care. This paucity could potentially be explained by the challenges inherent in implementing collective leadership approaches in health care due to the established hierarchical leadership models and barriers to implementing change in real-life healthcare contexts (Aufegger 2019; De Brún 2019; Janssens 2021). Further studies on collective leadership are required to develop an understanding of how collective leadership relates to professional practice, healthcare outcomes and staff well-being.

AUTHORS' CONCLUSIONS

Implications for practice

Collective leadership involves multiple professionals sharing viewpoints and knowledge, with the theoretical potential to

influence the quality of care and staff well-being. However, in the studies analysed, we noticed difficulties to advance collective leadership in practice. This review provided limited evidence about the effects of collective leadership interventions on professional practice, healthcare outcomes and staff well-being. Our confidence in the effects of collective leadership interventions on professional practice, healthcare outcomes and staff well-being is moderate in leadership outcomes, low in team performance and work-related stress, and very low for clinical performance, inpatient mortality and staff absence. The evidence was of moderate, low and very low certainty due to risk of bias and imprecision, meaning that future evidence may change our interpretation of the results.

Implications for research

The notion of collective leadership is gaining traction in health service delivery debates. However, there is little consensus about how to conceptualise, define or measure collective leadership. More high-quality studies could shed light on the impact of varied interventions based on different theoretical underpinnings. Further development and evaluation of team-based training interventions would strengthen the evidence base for collective leadership. Further work is required to assess the consequences of collective leadership interventions on: health status, measured as mortality within 30 days of discharge; staff well-being, measured as burnout and psychological symptoms; and unintended consequences, including safety of care events such as errors and healthcare-associated complications. A qualitative evidence synthesis about the factors that influence adoption and sustainability of collective leadership in health care could help inform future intervention development and implementation strategies.

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* Indicates the major publication for the study

CHARACTERISTICS OF STUDIES

Characteristics of included studies [ordered by study ID]

Fernandez 2020

Study characteristics

Methods	<p>Study design: randomised controlled trial</p> <p>Number of study centres: one</p> <p>Location: United States, Harborview Medical Center</p> <p>Study setting: level 1 trauma centre</p> <p>Withdrawals: control N = 9; intervention N = 10</p> <p>Date of study: from April 2016 to December 2017</p> <p>Follow-up: pre- and post-intervention data</p>
Participants	<p>Number: control N = 30; intervention N = 30</p> <p>Mean age: control mean age = 20, SD = 2; intervention mean age = 30, SD = 3</p> <p>Age range: not applicable</p> <p>Gender: control N = 9 (30%) female; intervention N = 11 (37%) female</p> <p>Exclusion criteria: participants were excluded if they were unavailable for the intervention or did not have the appropriate number of resuscitations captured for data analysis. All participants were advanced trauma life support (ATLS) certified and had at least 4 weeks of prior trauma care experience.</p> <p>Other relevant characteristics:</p> <p>Ethnicity in control: Hispanic or Latino = 1 (3%); not Hispanic or Latino = 29 (97%). Ethnicity in intervention: Hispanic or Latino = 2 (7%); not Hispanic or Latino = 28 (93%).</p> <p>The intervention and control groups were similar with respect to demographics, institution and specialty.</p> <p>Postgraduate training year 2: control N = 14 (47%); intervention N = 19 (63%). Postgraduate training year 3: control N = 16 (53%); intervention N = 11(37%).</p>
Interventions	<p>Intervention components: professional management intervention using simulation-based team leadership training, including: 1) establishing the leadership role (assumes leadership); 2) sharing information and interpreting data; 3) planning and prioritising tasks; 4) assigning roles and assessing team members' skills; and 5) seeking input and identifying task barriers</p> <p>Comparison: professional management reviewing responsibilities of the trauma team leader</p> <p>Fidelity assessment: same measures applied in the control and intervention groups</p>
Outcomes	<p>Main and other outcomes specified and collected:</p> <p><u>Patient care score overall:</u> control mean = 60.38, SD = 7.52; intervention mean = 62.38, SD = 11.03; mean difference = 2 (95% CI -2.78 to 6.78)</p> <p><u>Leadership score overall:</u> control mean = 7.23, SD = 2.50; intervention mean = 11.29, SD = 3.64; mean difference = 4.06 (95% CI 2.45 to 5.67)</p> <p><u>Injury severity score:</u> control mean = 22, SD = 14; intervention mean = 20, SD = 15; mean difference = -2 (95% CI -9.50 to 5.50)</p>

Fernandez 2020 (Continued)

Emergency department length of stay: control mean = 211, SD = 130; intervention mean = 245, SD = 151; mean difference = 34 (95% CI -38.82 to 106.82)

30 days intensive care unit (ICU)-free: control mean = 22, SD = 11; intervention mean = 23, SD = 11; mean difference = 1 (95% CI -4.69 to 6.69)

30 days hospital-free: control mean = 14, SD = 19; intervention mean = 17, SD = 16; mean difference = 3 (95% CI -6.08 to 12.08)

Died: control N = 14 (13%); intervention N = 12 (11%); relative risk = 0.86 (95% CI 0.42 to 1.77).

Time points reported: two

Notes

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Notable conflicts of interest of trial authors: the authors have disclosed that they do not have any potential conflicts of interest.

Ethical approval: the University of Washington Institutional Review Board approved this study.

Risk of bias

Bias	Authors' judgement	Support for judgement
Bias arising from the randomisation process	Unclear risk	No information was given concerning a random component being used in the sequence generation process. The only information about randomisation methods was a statement that the study was randomised. No information was given on whether the allocation sequence was concealed until participants were enrolled and assigned to the intervention. No baseline imbalances were apparent between groups to suggest a problem with the randomisation process.
Bias due to deviations from intended interventions	Unclear risk	No information was given on whether participants were aware of their assigned intervention during the trial. The people delivering the intervention were probably aware of participants' assigned intervention during the trial. No information was given about deviations from the intended intervention due to the trial context. Raters were blind to study hypotheses and experimental conditions of participants.
Bias due to missing outcome data	Low risk	Outcome data were not available for all randomised participants, with 19 withdrawals (24%). No information was given about the reasons for withdrawals, which were balanced between the groups. There is no evidence that the result was not biased by missing outcome data. It is not probable that missingness in the outcomes could depend on true value.
Bias in measurement of the outcome	Low risk	The methods of measuring the outcomes were appropriate. The measurement of the outcomes did not differ between groups. The raters were blind to study hypotheses and experimental conditions of participants.
Bias in selection of the reported result	Unclear risk	The data were not analysed in accordance with a prespecified plan finalised before unblinded outcome data were available for analysis. Authors' intentions for their analysis were not available, or their intentions were not reported in sufficient detail to enable an assessment. The results reported were not likely to have been selected, on the basis of the results, from multiple eligible outcome measurements (e.g. scales, definitions, time points) within the outcome domain.

Shirazi 2016
Study characteristics

Methods	<p>Study design: randomised controlled trial</p> <p>Number of study centres: 16 metropolitan academic hospitals</p> <p>Location: Iran</p> <p>Study setting: University Hospitals at the Tehran University of Medical Sciences</p> <p>Withdrawals: N = 39 (control N = 30 and intervention N = 9, all due to changing jobs)</p> <p>Date of study: July 2010 to April 2011</p> <p>Follow-up: 3 months after the intervention ended</p>	
Participants	<p>Number: head nurses N = 110 (55 in intervention group and 55 in control group); nursing personnel (registered nurses and nurse aids): control N = 327; intervention N = 294</p> <p>Mean age: control mean age = 35.87 SD = 8.25; intervention mean age = 36.03 SD 8.54</p> <p>Age range: not mentioned</p> <p>Gender: control female N = 320 (83%); intervention female N = 282 (80%)</p> <p>Exclusion criteria: nurses having no more than 2 months of work experience in the same ward with the intention of staying in that position less than an additional 3 months.</p> <p>Other relevant characteristics: length employed (years): mean Control = 11.19 SD = 8.11; mean Intervention 11.65 SD = 8.18</p>	
Interventions	<p>Intervention components: Supportive Leadership Behaviour (SLB) workshop using a multifaceted learning and teaching style. The content and expected outcomes of the workshop were based on the concepts of different leadership styles, including the fundamentals of SLB.</p> <p>Comparison: no intervention</p> <p>Fidelity assessment: same measures applied in the control and intervention groups</p>	
Outcomes	<p>Main and other outcomes specified and collected:</p> <p><u>SLB scores</u> after three months: control mean = 128.64, SD = 24.27; intervention mean = 150.05, SD = 19.99; mean difference = 21.41 (95% CI 18.16 to 24.61)</p> <p>Time points reported: before the intervention and 3 months after</p>	
Notes	<p>Funding: this work was supported with a grant from the Tehran University of Medical Sciences.</p> <p>Notable conflicts of interest of trial authors: not mentioned</p> <p>Ethical approval: approved by the university's ethical committee and registered as a clinical trial at Clinicaltrials.gov (NCT 01169623)</p>	
Risk of bias		
Bias	Authors' judgement	Support for judgement
Bias arising from the randomisation process	Unclear risk	No information was given on whether a random component was used in the sequence generation process, other than a statement that the study was randomised. There was not enough information on whether the allocation sequence was concealed, other than a statement that the researchers were

Shirazi 2016 (Continued)

		blinded during the process. Baseline differences between groups did not suggest a problem with the randomisation process.
Bias due to deviations from intended interventions	Unclear risk	Allocation was concealed from participants; however, the nature of the intervention gives strong reason to believe participants could become aware of their assigned intervention during the trial. No information was provided on deviations from intended intervention.
Bias due to missing outcome data	Low risk	The outcomes were available for nearly all (95%) participants randomised.
Bias in measurement of the outcome	Unclear risk	The method of measuring the outcome was probably appropriate. A validated tool was used, though details of its validity were not provided. The measurement of the outcome did not differ between groups. Study participants were the outcome assessors, and given the self-reported outcome, were probably aware of intervention status. Assessment of the outcome could have been influenced by knowledge of intervention received, but there is no strong reason to believe that it did.
Bias in selection of the reported result	Unclear risk	The data were not analysed in accordance with a prespecified plan finalised before unblinded outcome data were available for analysis. The result being assessed was unlikely to have been selected, on the basis of the results, from multiple eligible outcome measurements or analyses of the data.

Weir 1997
Study characteristics

Methods	Study design: randomised controlled trial Number of study centres: one Location: Canada Study setting: clinical inpatient units, hospital Withdrawals: not mentioned Date of study: 1996, exact months not mentioned Follow-up: 1 time (12 months)
Participants	Number: N = 164: control = 78, intervention = 86 Profession: nurses Mean age: 44 years old Age range: not mentioned Gender: female N = 163; male N = 1 Exclusion criteria: not nurse managers Other relevant characteristics: mean length employed 12.5 years
Interventions	Intervention components: to facilitate a decentralised and participatory style of problem-solving management meetings (participative decision-making) with unit staff members and their nurse managers. Process consultation, consisting of consultation meetings between nurse manager participants and leadership consultants. The consultants acted as advisors to the nurses concerning leadership,

Weir 1997 (Continued)

communication and decision-making, following a problem-solving approach. Additionally, at 6, 9 and 12 months, there was a group consultation.

Comparison: traditional problem-solving management (control)

Fidelity assessment: not mentioned

Outcomes
Main and other outcomes specified and collected:

Leadership: intervention mean = 1.59, SD = 15.16; control mean = -1.14, SD = 15.39; mean difference = 2.73 (95% CI -1.95 to 7.41)

Relationship: intervention mean = -1.08, SD = 13.48; control mean = -1.08, SD = 12.79; mean difference = 0 (95% CI -4.06 to 4.06)

Task orientation: intervention mean = 2.03, SD = 15.07; control mean = -0.40, SD = 12.99; mean difference: 2.43 (95% CI -1.93 to 6.79)

Work pressure: intervention mean = 2.38, SD = 12.69; control mean = 4.62, SD = 12.99; mean difference = -2.24 (95% CI -6.18 to 1.70)

Physical comfort: intervention mean = -5.77, SD = 14.46; control mean = -3.36, SD = 13.23; mean difference = -2.41 (95% CI -6.70 to 1.88)

Supervisor support: intervention mean = 1.59, SD = 15.16; control mean = -1.14, SD = 15.39; mean difference = 2.73 (95% CI -1.98 to 7.44)

Peer cohesion: intervention mean = 4.63, SD = 15.76; control mean = -2.45, SD = 14.10; mean difference = 7.08 (95% CI 2.45 to 11.71)

Absence (hours): intervention mean = 23.16, SD = 99.38; control mean = 2.81, SD = 102.75; mean difference: 20.35 (95% CI -10.65 to 51.35)

Time points reported: two times, before and after intervention

Notes

Funding: supported by the Ontario Ministry of Health through the Hospital Incentive Fund, project number 28

Notable conflicts of interest of trial authors: not mentioned

Ethical approval: not mentioned

Risk of bias

Bias	Authors' judgement	Support for judgement
Bias arising from the randomisation process	Unclear risk	No information was given concerning a random component being used in the sequence generation process. The only information about randomisation methods was a statement that the study was randomised. No information was given on whether the allocation sequence was concealed until participants were enrolled and assigned to the intervention. No baseline imbalances were apparent between groups to suggest a problem with the randomisation process.
Bias due to deviations from intended interventions	Unclear risk	No information was given on whether participants were aware of their assigned intervention during the trial, but the nature of the intervention implies they were aware. The people delivering the intervention were aware of participants' assigned intervention during the trial. No information was given about deviations from the intended intervention due to the trial context.

Weir 1997 (Continued)

Bias due to missing outcome data	Unclear risk	Outcome data were not available for all randomised participants. There is not evidence that the result was not biased by missing outcome data. Missingness in the outcome could depend on its true value, but this is unlikely.
Bias in measurement of the outcome	Unclear risk	The method of measuring the outcomes was probably appropriate. The measurement of the outcome did not differ between groups. Outcome assessors were probably aware of intervention status. Assessment of the outcome could have been influenced by knowledge of intervention received, but there is no strong reason to believe that it did.
Bias in selection of the reported result	Unclear risk	The data were not analysed in accordance with a prespecified plan finalised before unblinded outcome data were available for analysis. The result being assessed was unlikely to have been selected, on the basis of the results, from multiple eligible outcome measurements or analyses of the data.

SD: standard deviation

Characteristics of excluded studies [ordered by study ID]

Study	Reason for exclusion
Aarons 2017	Ineligible setting
ACTRN12618000191291	Ineligible indication
Ahmed 2020	Ineligible study design
Anjara 2020	Ineligible study design
Armstrong 2020	Ineligible study design
Batcheller 2007	Ineligible indication
Beglinger 2011	Ineligible study design
Beiko 2018	Ineligible study design
Bender 2012	Ineligible intervention
Boyle 2004	Ineligible indication
Bradley 2018	Ineligible study design
Branda 2018	Ineligible intervention
Casady 2005	Ineligible study design
Caspar 2017	Ineligible study design
Chiu 2016	Ineligible study design
Chokrieh 1976	Ineligible study design
Clegg 2000	Ineligible study design

Study	Reason for exclusion
Costanzo 2019	Ineligible intervention
Cummings 2013	Ineligible study design
Currie 2019	Ineligible study design
DeBrún 2020	Ineligible study design
Dickenson 1990	Ineligible study design
Fernandez 2015	Ineligible participants
Fowler 2013	Ineligible study design
Ghazali 2019	Ineligible indication
Gilfoyle 2007	Ineligible study design
Govero 2012	Ineligible study design
Greenwood 2002	Ineligible study design
Hitchcock 2018	Ineligible indication
Hoch 2014	Ineligible setting
Huis 2013	Ineligible indication
Humphreys 2018	Ineligible study design
Hunziker 2018	Ineligible participants
IRCT2017060217756N17	Ineligible intervention
ISRCTN06910890	Ineligible indication
James 2017	Ineligible study design
Jeon 2013	Ineligible intervention
Jeon 2015	Ineligible indication
Johnson 2000	Ineligible study design
Kenaszchuk 2011	Ineligible indication
Krugman 2003	Ineligible study design
Krugman 2013	Ineligible study design
Lake 2014	Ineligible indication
LeComte 2017	Ineligible study design
Leslie 2005	Ineligible intervention

Study	Reason for exclusion
Lieber 2014	Ineligible study design
Llobera 2017	Ineligible indication
Lloyd 1995	Ineligible indication
Macaulay 1992	Ineligible study design
Madrid 2016	Ineligible study design
Malby 2010	Ineligible study design
Manthey 2007	Ineligible indication
Marrone 1999	Ineligible indication
Maynard 2020	Ineligible study design
McAllister 2009	Ineligible study design
McDonagh 2003	Ineligible study design
McFarland 1979	Ineligible indication
Moneke 2013	Ineligible study design
NCT00286975	Ineligible setting
NCT01341821	Ineligible study design
NCT02494128	Ineligible indication
NCT03000829	Ineligible indication
NCT03125330	Ineligible intervention
NCT03639961	Ineligible indication
Nieuwboer 2017	Ineligible study design
Oakley 2014	Ineligible indication
Omar 2015	Ineligible study design
Ottawa Hospital Research Institute 2016	Ineligible indication
Ovens 2018	Ineligible study design
Owen 1994	Ineligible indication
Paterson 2015	Ineligible intervention
Rantz 2012	Ineligible study design
Rantz 2013	Ineligible study design

Study	Reason for exclusion
Richter 2016	Ineligible intervention
Sauer 2011	Ineligible study design
Schmelzer 2009	Abstract and full text not available
Sherrod 2013	Ineligible study design
Shirey 2019	Ineligible study design
Smith 1999	Ineligible study design
Spellerberg 2001	Ineligible indication
Suhovy 2009	Ineligible indication
Sun 2016	Ineligible participants
Ten Have 2013	Ineligible study design
Tomlinson 2020	Ineligible indication
Umble 2005	Ineligible study design
Vestal 2007	Ineligible indication
Wang 2008	Ineligible indication
Ward 2018	Ineligible study design
Weech-Maldonado 2018	Ineligible intervention
Weil 2000	Ineligible indication
West 2016	Ineligible indication
Yura 1984	Ineligible indication
Zablocki 1996	Ineligible indication

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

ACTRN12618000223235

Study name	Who is leading maternity teams? An observational study of leadership sharing in maternity teams and the relationship to teamwork performance
Methods	Observational, cross-sectional, retrospective
Participants	Participants at the "Maternity Emergency Management" (MEM) course at Mater Education. Includes doctors, midwives and midwifery students
Interventions	Leadership within maternity emergency teams

ACTRN12618000223235 (Continued)

Outcomes	Teamwork score - Auckland Team Behaviour Score; clinical teamwork scale; time to critical management (measured in minutes and seconds)
Starting date	01 December 2017
Contact information	sarah.janssens3@mater.org.au
Notes	

ACTRN12619001007123

Study name	What is the effect of a shared leadership system for maternity emergency teams on team performance?
Methods	Randomised controlled trial
Participants	Interprofessional team of clinician educators (midwifery/obstetrics and anaesthetics) with training in simulation delivery and debriefing
Interventions	Simulation training course "Maternity Emergency Management"
Outcomes	Primary: teamwork scores (primary outcome) for shared and singular leadership will be compared within groups. Secondary: clinical performance will be assessed by combination of time to critical intervention (scenario-specific) and completion of checklist of clinical interventions.
Starting date	19 July 2019
Contact information	sarah.janssens3@mater.org.au
Notes	

DRKS00013532

Study name	DRKS00013532
Methods	Non-randomised controlled trial
Participants	Employees at the hospital (all employees of the participating and interviewed departments)
Interventions	Leadership styles and communication. Intervention group: leaders of middle management in hospitals participate in several training seasons with the topic "leadership styles and communication". Control group: leaders of middle management in hospitals do not participate in any training season with the topic "leadership styles and communication".
Outcomes	Reduction of psychological stress in subordinates in hospitals
Starting date	01 August 2018
Contact information	florian.junne@med.uni-tuebingen.de
Notes	Psychological stress in subordinates will be measured at least three times by using standardised questionnaires: prior to the training of their managers, directly after the training of their managers

DRKS00013532 (Continued)

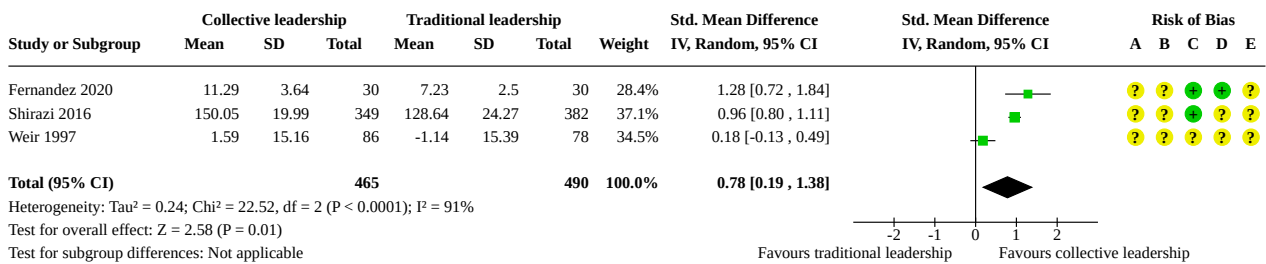
and follow-up measurement three months after the end of the training on leadership and communication.

DATA AND ANALYSES

Comparison 1. Professional practice

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1.1 Leadership	3	955	Std. Mean Difference (IV, Random, 95% CI)	0.78 [0.19, 1.38]

Analysis 1.1. Comparison 1: Professional practice, Outcome 1: Leadership



Risk of bias legend

- (A) Bias arising from the randomisation process
- (B) Bias due to deviations from intended interventions
- (C) Bias due to missing outcome data
- (D) Bias in measurement of the outcome
- (E) Bias in selection of the reported result

APPENDICES

Appendix 1. Search strategies

MEDLINE

Ovid MEDLINE including epub ahead of print, in-process & other non-indexed citations

1	((collectiv* or profession* or disciplinary or interprofessional or multiprofessional or interdisciplinary or multidisciplinary or inter-professional or multi-professional or inter-disciplinary or multi-disciplinary or partner* or distribut* or shar* or collaborat* or participat* or inclusive* or democratic* or plural* or dispers* or empower* or compassion* or informal* or peer or team or cooperat* or co-operat* or group?) adj (leadership or leader?)).ti,ab,kf.	3587
2	co-lead*.ti,ab,kf.	205

(Continued)

3	(inter lead* or interlead*).ti,ab,kf.	146
4	collectivis?.ti,ab,kf.	997
5	or/1-4	4920
6	*leadership/	24252
7	*"organization and administration"/	3661
8	*cooperative behavior/	18073
9	*decision making, shared/	385
10	*patient care team/	27921
11	*interprofessional relations/	21574
12	og.fs.	488714
13	or/7-12	534058
14	6 and 13	8984
15	exp health personnel/	527244
16	(nurse? or doctor? or clinician? or physician? or dentist? or osteopath? chiropodist? or podiatrist? or optometrist? or physician assistant? or midwife or midwives or pharmacist? or general practitioner? or gp or resident? or housestaff or staff or allergist? or anesthesiologist? or anaesthesiologist? or cardiologist? or dermatologist? or endocrinologist? or gastroenterologist? or geriatrician? or hospitalist? or nephrologist? or neurologist? or oncologist? or ophthalmologist? or otolaryngologist? or pathologist? or pediatrician? or paediatrician? or neonatologist? or physiatrist? or pulmonologist? or radiologist? or rheumatologist? or surgeon? or neurosurgeon? or urologist? or health* professional? or provider? or team?).ti,ab,kf.	1932114
17	or/15-16	2171653
18	(5 or 14) and 17	7754
19	randomized controlled trial.pt.	519987
20	controlled clinical trial.pt.	93997
21	multicenter study.pt.	285524
22	pragmatic clinical trial.pt.	1596
23	(randomis* or randomiz* or randomly).ti,ab.	939835
24	groups.ab.	2140903
25	(trial or multicenter or multi center or multicentre or multi centre).ti.	277176
26	(intervention? or effect? or impact? or controlled or control group? or (before adj5 after) or (pre adj5 post) or ((pretest or pre test) and (posttest or post	10017069

(Continued)

	test)) or quasiexperiment* or quasi experiment* or pseudo experiment* or pseudoexperiment* or evaluat* or time series or time point? or repeated measur*).ti,ab.	
27	non-randomized controlled trials as topic/	833
28	interrupted time series analysis/	1077
29	controlled before-after studies/	576
30	or/19-29	11152576
31	exp animals/	23703476
32	humans/	18931093
33	31 not (31 and 32)	4772383
34	review.pt.	2742419
35	meta analysis.pt.	124443
36	news.pt.	204607
37	comment.pt.	885443
38	editorial.pt.	552831
39	cochrane database of systematic reviews.jn.	15152
40	comment on.cm.	885389
41	(systematic review or literature review).ti.	178599
42	or/33-41	8855986
43	30 not 42	7925262
44	18 and 43	2421

Embase, Ovid

1	((collectiv* or profession* or disciplinary or interprofessional or multiprofessional or interdisciplinary or multidisciplinary or inter-professional or multi-professional or inter-disciplinary or multi-disciplinary or partner* or distribut* or shar* or collaborat* or participat* or inclusive* or democratic* or plural* or dispers* or empower* or compassion* or informal* or peer or team or cooperat* or co-operat* or group?) adj (leadership or leader?)).ti,ab,kw.	4837
2	co-lead*.ti,ab,kw.	260

(Continued)

3	(inter lead* or interlead*).ti,ab,kw.	233
4	collectivis?.ti,ab,kw.	1061
5	or/1-4	6372
6	*leadership/	23697
7	*"organization and management"/	62672
8	*cooperation/	14698
9	*shared decision making/	2441
10	or/7-9	78662
11	6 and 10	1824
12	exp health care personnel/	1617784
13	(nurse? or doctor? or clinician? or physician? or dentist? or osteopath? chiropracist? or podiatrist? or optometrist? or physician assistant? or midwife or midwives or pharmacist? or general practitioner? or gp or resident? or housestaff or staff or allergist? or anesthesiologist? or anaesthesiologist? or cardiologist? or dermatologist? or endocrinologist? or gastroenterologist? or geriatrician? or hospitalist? or nephrologist? or neurologist? or oncologist? or ophthalmologist? or otolaryngologist? or pathologist? or pediatrician? or paediatrician? or neonatologist? or physiatrist? or pulmonologist? or radiologist? or rheumatologist? or surgeon? or neurosurgeon? or urologist? or health* professional? or provider? or team?).ti,ab,kw.	2693563
14	or/12-13	3299405
15	(5 or 11) and 14	4874
16	randomized controlled trial/	639714
17	controlled clinical trial/	466484
18	quasi experimental study/	7610
19	pretest posttest control group design/	529
20	time series analysis/	27816
21	experimental design/	19960
22	multicenter study/	274674
23	(randomis* or randomiz* or randomly).ti,ab.	1326411
24	groups.ab.	2995012
25	(trial or multicentre or multicenter or multi centre or multi center).ti.	389713
26	(intervention? or effect? or impact? or controlled or control group? or (before adj5 after) or (pre adj5 post) or ((pretest or pre test) and (posttest or post	12884114

(Continued)

	test)) or quasiexperiment* or quasi experiment* or pseudo experiment* or pseudoexperiment* or evaluat* or time series or time point? or repeated measur*).ti,ab.	
27	or/16-26	14361411
28	(systematic review or literature review).ti.	213703
29	"cochrane database of systematic reviews".jn.	14674
30	exp animals/ or exp invertebrate/ or animal experiment/ or animal model/ or animal tissue/ or animal cell/ or nonhuman/	28520377
31	human/ or normal human/ or human cell/	21953218
32	30 not (30 and 31)	6633812
33	28 or 29 or 32	6860205
34	27 not 33	11131101
35	15 and 34	2703
36	limit 35 to embase	806

CDSR and CENTRAL (Wiley)

1	((collectiv* or profession* or disciplinary or interprofessional or multiprofessional or interdisciplinary or multidisciplinary or inter-professional or multi-professional or inter-disciplinary or multi-disciplinary or partner* or distribut* or shar* or collaborat* or participat* or inclusive* or democratic* or plural* or dispers* or empower* or compassion* or informal* or peer or team or cooperat* or co-operat* or group?) next (leadership or leader?):ti,ab	579
2	co-lead*:ti,ab	26
3	(inter lead* or interlead*):ti,ab	1022
4	collectivis?:ti,ab	28
5	{or #1-#4}	1640
6	[mh "leadership"]	207
7	[mh "organization and administration"]	36837
8	[mh "cooperative behavior"]	936
9	[mh "decision making, shared"]	24
10	[mh "patient care team"]	1724

(Continued)

11	[mh “interprofessional relations”]	569
12	MeSH descriptor: [] explode all trees and with qualifier(s): [organization & administration - OG]	7058
13	{or #7-#12}	42573
14	#6 and #13	121
15	[mh “health personnel”]	8923
16	(nurse? or doctor? or clinician? or physician? or dentist? or osteopath? chiropr podist? or podiatrist? or optometrist? or physician assistant? or midwife or midwives or pharmacist? or general practitioner? or gp or resident? or housestaff or staff or allergist? or anesthesiologist? or anaesthesiologist? or cardiologist? or dermatologist? or endocrinologist? or gastroenterologist? or geriatrician? or hospitalist? or nephrologist? or neurologist? or oncologist? or ophthalmologist? or otolaryngologist? or pathologist? or pediatrician? or paediatrician? or neonatologist? or physiatrist? or pulmonologist? or radiologist? or rheumatologist? or surgeon? or neurosurgeon? or urologist? or health* professional? or provider? or team?):ti,ab	179786
17	{or #15-#16}	182687
18	(#5 or #14) and #17	783

Clinical Trials.gov

1	leadership*	0
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WHO International Clinical Trials Registry Platform (ICTRP)

1	leadership*	123
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CINAHL (EBSCO)

1	TI((collectiv* or profession* or disciplinary or interprofessional or multiprofessional or interdisciplinary or multidisciplinary or inter-professional or multi-professional or inter-disciplinary or multi-disciplinary or partner* or distribute* or shar* or collaborat* or participat* or inclusive* or democratic* or plural* or dispers* or empower* or compassion* or informal* or peer or team or coop-	18
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(Continued)

	erat* or co-operat* or group?) next (leadership or leader?)) OR AB ((collectiv* or profession* or disciplinary or interprofessional or multiprofessional or interdisciplinary or multidisciplinary or inter-professional or multi-professional or inter-disciplinary or multi-disciplinary or partner* or distribut* or shar* or collaborat* or participat* or inclusive* or democratic* or plural* or dispers* or empower* or compassion* or informal* or peer or team or cooperat* or co-operat* or group?) next (leadership or leader?))	
2	TI co-lead* OR AB co-lead*	111
3	TI (inter lead* or interlead*) OR AB (inter lead* or interlead*)	201
4	TI collectivis? OR AB collectivis?	200
5	S1 OR S2 OR S3 OR S4	529
6	(MM "Leadership")	22,522
7	(MM "Management")	5836
8	(MM "Cooperative Behavior")	3349
9	(MM "Decision Making, Shared")	768
10	(MM "Multidisciplinary Care Team+")	15699
11	(MM "Interprofessional Relations+")	12642
12	S7 OR S8 OR S9 OR S10 OR S11	36,246
13	S6 AND S12	951
14	(MH "Health Personnel+")	579,660
15	TI (nurse? or doctor? or clinician? or physician? or dentist? or osteopath? chiropracist? or podiatrist? or optometrist? or physician assistant? or midwife or midwives or pharmacist? or general practitioner? or gp or resident? or housestaff or staff or allergist? or anesthesiologist? or anaesthesiologist? or cardiologist? or dermatologist? or endocrinologist? or gastroenterologist? or geriatrician? or hospitalist? or nephrologist? or neurologist? or oncologist? or ophthalmologist? or otolaryngologist? or pathologist? or pediatrician? or paediatrician? or neonatologist? or physiatrist? or pulmonologist? or radiologist? or rheumatologist? or surgeon? or neurosurgeon? or urologist? or health* professional? or provider? or team?) OR AB (nurse? or doctor? or clinician? or physician? or dentist? or osteopath? chiropracist? or podiatrist? or optometrist? or physician assistant? or midwife or midwives or pharmacist? or general practitioner? or gp or resident? or housestaff or staff or allergist? or anesthesiologist? or anaesthesiologist? or cardiologist? or dermatologist? or endocrinologist? or gastroenterologist? or geriatrician? or hospitalist? or nephrologist? or neurologist? or oncologist? or ophthalmologist? or otolaryngologist? or pathologist? or pediatrician? or paediatrician? or neonatologist? or physiatrist? or pulmonologist? or radiologist? or rheumatologist? or surgeon? or neurosurgeon? or urologist? or health* professional? or provider? or team?)	933,500
16	S14 OR S15	1,293,744
17	(S5 OR S13) AND S16	661

(Continued)

18	S17 Limiters - Exclude MEDLINE records	328
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LILACS (via WHO Global Index Medicus)
pesquisa.bvsalud.org/gim/

<!--td {border: 1px solid #ccc;}br {mso-data-placement:same-cell;}-->

Boolean	Search terms	Results
	“collective leadership” or “collectivistic leadership” or “professional leadership” or “disciplinary leadership” or “interprofessional leadership” or “multi-professional leadership” or “interdisciplinary leadership” or “multidisciplinary leadership” or “inter-professional leadership” or “multi-professional leadership” or “inter-disciplinary leadership” or “multi-disciplinary leadership” or “partner leadership” or “distributed leadership” or “shared leadership” or “collaborative leadership” or “participatory leadership” or “inclusive leadership” or “democratic leadership” or “plural leadership” or “dispersed leadership” or “empowering leadership” or “compassionate leadership” or “informal leadership” or “peer leadership” or “team leadership” or “cooperative leadership” or “co-operative leadership” or “group leadership” or interlead*	
AND	random* or trial* or groups* or multicenter* or multi center* or multicentre* or multi centre* or intervention* or effect* or impact* or controlled* or control group* or before* or after* or pre* or post* or pretest* or or posttest* or quasiexperiment* or quasi experiment* or pseudo experiment* or pseudoexperiment* or evaluat* or time series* or time point* or repeated measure*	
AND	LILACS	122

CRD databases - DARE

1	(((collectiv* or profession* or disciplinary or interprofessional or multiprofessional or interdisciplinary or multidisciplinary or inter-professional or multi-professional or inter-disciplinary or multi-disciplinary or partner* or distribute* or shar* or collaborat* or participat* or inclusive* or democratic* or plural* or dispers* or empower* or compassion* or informal* or peer or team or cooperat* or co-operat* or group?) NEXT (leadership or leader?)))	32
2	(co-lead* OR inter lead* OR interlead* OR collectivis?)	1
3	#1 OR #2	33
4	(#3) IN DARE	21

Conference Proceedings Citation Index- Science (CPCI-S) --1990-present

1	TS=((collectiv* or profession* or disciplinary or interprofessional or multi-professional or interdisciplinary or multidisciplinary or inter-professional or multi-professional or inter-disciplinary or multi-disciplinary or partner* or distribut* or shar* or collaborat* or participat* or inclusive* or democratic* or plural* or dispers* or empower* or compassion* or informal* or peer or team or cooperat* or co-operat* or group?) NEAR/10 (leader?))	1,012
2	TS=co-lead*	68
3	TS= (inter lead* or interlead*)	5,345
4	TS=collectivis?	174
5	#4 OR #3 OR #2 OR #1	6,571
6	TS=(nurse* or doctor* or clinician* or physician* or dentist* or osteopath* chiropodist* or podiatrist* or optometrist* or physician assistant* or midwife or midwives or pharmacist* or general practitioner* or gp or resident* or housestaff or staff or allergist* or anesthesiologist* or anaesthesiologist* or cardiologist* or dermatologist* or endocrinologist* or gastroenterologist* or geriatrician* or hospitalist* or nephrologist* or neurologist* or oncologist* or ophthalmologist* or otolaryngologist* or pathologist* or pediatrician* or paediatrician* or neonatologist* or physiatrist* or pulmonologist* or radiologist* or rheumatologist* or surgeon* or neurosurgeon* or urologist* or health* professional* or provider* or team*)	256,487
7	#6 AND #5	798
8	TS=(random* or trial* or groups* or multicenter* or multi center* or multicentre* or multi centre* or intervention* or effect* or impact* or controlled* or control group* or before* or after* or pre* or post* or pretest* or posttest* or quasiexperiment* or quasi experiment* or pseudo experiment* or pseudoexperiment* or evaluat* or time series* or time point* or repeated measure*)	6,221,165
9	#8 AND #7	659

HISTORY

Protocol first published: Issue 1, 2021

CONTRIBUTIONS OF AUTHORS

- Conceiving, designing and co-ordinating the protocol and review: JAMS, AX
- Providing general advice on the protocol and review: HFA, MP, RH, VAM
- Data collection for the review: JAMS, HFA, MP, VAM
- Data management for the review: JAMS, AX
- Analysis and interpretation of data: JAMS, AX
- Writing the review: JAMS, AX
- AX is the guarantor of the review.

Collective leadership to improve professional practice, healthcare outcomes and staff well-being (Review)

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- All the authors approved the final version submitted for publication.

DECLARATIONS OF INTEREST

- Jaqueline Alcantara Marcelino Silva: none known.
- Heloise Fernandes Agreli: none known.
- Reema Harrison: none known.
- Marina Peduzzi: none know
- Vivian Aline Mininel: none known
- Andreas Xyrichis: none known.

SOURCES OF SUPPORT

Internal sources

- King 's College London. Florence Nightingale Faculty of Nursing, Midwifery and Palliative Care, UK
Infrastructure support and employs review author Andreas Xyrichis
- Federal University of Sao Carlos, Brazil
Employs reviews authors Jaqueline Alcantara Marcelino da Silva and Vivian Aline Mininel
- Cochrane Effective Practice and Organisation of Care (EPOC), UK
Infrastructure support and editorial base
- University of Sao Paulo, Brazil
Employs review author Heloise Fernandes Agreli

External sources

- No sources of support provided

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

Two review author pairs independently screened and selected studies for exclusion; the principal author was part of both for consistency. Three groups of review author pairs independently extracted data from the included studies and evaluated study quality; the principal author took part in all groups.

We specified in our protocol that we would consider non-randomised trials and controlled before-and-after studies with two or more control and intervention sites, and interrupted time series studies with a clearly-defined point in time when the intervention occurred and at least three data points before and after implementation of the interventions. However, we only identified three RCTs suitable for inclusion.

We intended to extract data about patients' severity of condition and diagnostic criteria. However, we were unable to identify this information from the studies.

We intended to analyse the risk of bias in non-randomised studies using the 'Risk of Bias in Non-randomized Studies of Interventions' (ROBINS-I) tool (Sterne 2016). For interrupted time series studies, we planned to assess risk of bias according to the EPOC domains (EPOC 2017c). However, we did not include any non-randomised studies.

We planned to conduct subgroup analyses. However, we were unable to carry these out for: 1. Collective leadership hierarchical interventions subgrouped by setting (primary care, secondary care or tertiary care); 2. Collective leadership non-hierarchical interventions subgrouped by setting (primary care, secondary care or tertiary care); 3. Subgroup of collective leadership hierarchical interventions in high-income versus low-income setting; 4. Subgroup of collective leadership non-hierarchical interventions in high-income versus low-income setting. All three included studies were undertaken in tertiary care (trauma centre and clinical units); none were from low-income settings.

NOTES

This review is based on standard text and guidance provided by Cochrane Effective Practice and Organisation of Care (EPOC).

INDEX TERMS**Medical Subject Headings (MeSH)**

Delivery of Health Care; Health Personnel; *Leadership; *Occupational Stress; Professional Practice; Randomized Controlled Trials as Topic

MeSH check words

Humans