

Effect of Enhanced Recovery After Surgery programs on surgical site infection and 30-day readmission in patients undergoing gastro-intestinal procedures: An umbrella review

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ABSTRACT

Background: Enhanced Recovery After Surgery programs have been implemented across surgical specialties; however, their effectiveness in reducing surgical site infection and 30-day hospital readmission remains unclear. **Objective:** To determine the effect of Enhanced Recovery After Surgery programs on the clinical outcomes of surgical site infection and readmission and identify items of and implementation strategies for such programs. **Design:** An umbrella review of published systematic reviews.

Methods: Systematic searches of MEDLINE, Embase, CINAHL, Cochrane Library, and Scopus databases were conducted in January 2025. Systematic reviews of randomised controlled trials and non-randomised studies of interventions comparing Enhanced Recovery After Surgery programs with standard care in adults after major open or laparoscopic gastrointestinal surgery, focusing on surgical site infection and 30-day hospital readmission, were included. Review quality and certainty of evidence were assessed using A MeaSurement Tool to Assess systematic Reviews (AMSTAR 2) and GRADE (Grading of Recommendations Assessment, Development and Evaluation) tools.

Results: 49 reviews were included in the analysis. Meta-analysis of 42 unique primary randomised controlled trials with 5112 patients suggests a significant association between Enhanced Recovery After Surgery programs use and surgical site infection reduction (estimated odds ratio 0.70, 95 % confidence interval 0.59–0.82, $p < 0.001$, $I^2 = 0\%$). Meta-analysis of 53 primary randomised controlled trials with 7360 patients suggests no significant relationship between Enhanced Recovery After Surgery programs and hospital readmission (estimated odds ratio 1.025, 95 % confidence interval 0.85–1.23, $p = 0.792$, $I^2 = 0\%$). Summary estimates for surgical site infection were similar across non-randomised studies of interventions. GRADE assessments suggest 'very low' certainty across outcomes. Enhanced Recovery After Surgery programs included 5 to 28 components; implementation strategies were not described.

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Discussion: Results suggest an association between Enhanced Recovery After Surgery programs and reductions in surgical site infection, and no relationship between hospital readmissions. However, the body of evidence is weak and inconclusive. These results also indicate heterogeneity in compliance with Enhanced Recovery After Surgery, reflecting the complexity in standardising this surgical care pathway.

Conclusions: Well-controlled primary research is warranted to understand the impact of Enhanced Recovery After Surgery programs.

Registration: PROSPERO registration number: CRD42024503357.

Social media abstract: Enhanced Recovery After Surgery programs may significantly reduce surgical site infections after GI surgery but there is no clear impact on 30-day readmissions.

What is already known

- Enhanced Recovery After Surgery programs have been shown to improve recovery and reduce patients' risk of morbidity and mortality.
- The impact of Enhanced Recovery After Surgery programs on surgical site infections and hospital readmissions remains uncertain, with limited consolidated evidence available.

What this paper adds

- This review found an association between Enhanced Recovery After Surgery programs and reduced surgical site infections, but no association with hospital readmissions.
- Enhanced Recovery After Surgery item compliance varies across studies following gastrointestinal surgeries.
- These findings are important as they demonstrate the potential of Enhanced Recovery After Surgery programs to improve some patient outcomes, highlighting the need to better understand Enhanced Recovery After Surgery implementation.

1. Background

Enhanced Recovery After Surgery programs have significantly transformed the surgery landscape, aiming to optimise patient outcomes by employing a multidisciplinary approach to care (Altman et al., 2019). These programs encompass comprehensive strategies designed to streamline the perioperative care process, from preoperative preparation to postoperative recovery. Strategies within Enhanced Recovery After Surgery programs often include preoperative patient education, standardised anaesthesia and analgesia approaches, minimally invasive surgical techniques, and early postoperative mobilisation (Irani et al., 2023). The emphasis is on evidence-based practices that collectively contribute to reducing stress on patients and enhancing their overall surgical experience. Nonetheless, despite worldwide diffusion of Enhanced Recovery After Surgery guidelines across gastro-intestinal surgical subspecialties (i.e., gastric, oesophageal, hepatobiliary, pancreatic, and colorectal), their universal implementation has been slow (Stone et al., 2018).

The literature is replete with meta-analyses reporting the benefits of Enhanced Recovery After Surgery programs for patients undergoing gastro-intestinal procedures relative to shorter hospital stays (Feng et al., 2023; Huang et al., 2020; Sun et al., 2020; Tan et al., 2021; Xiong et al., 2016; Yuan et al., 2023), and reductions in overall postoperative complication rates (Sun et al., 2020; Tan et al., 2021; Xiong et al., 2016). However, their effectiveness in reducing specific complications like surgical site infections and hospital readmissions up to 30 postoperative days remains equivocal (Feng et al., 2023; Huang et al., 2020). Wang et al.'s (2017) of 28 studies with 4784 patients undergoing liver surgery, reported a significant reduction in surgical site infection (OR 0.59, 95 % CI 0.41–0.84, $p = 0.004$). The Wang review focused solely on patients undergoing liver resections and combined randomised controlled trials (RCTs) and observational studies in the same analysis. Consequently, potential confounders and biases inherent in observational studies

reduce our confidence in the validity of their findings when evaluating the impact of interventions.

In summary, the effects of Enhanced Recovery After Surgery programs on the outcomes of surgical site infection and hospital readmission 30 postoperative days in gastro-intestinal patient cohorts are unclear and there is a lack of a comprehensive consolidated synthesis of evidence. Thus, undertaking an umbrella review may address this uncertainty and advance knowledge. First, the results may enable health-care leaders and frontline clinicians to make evidence-informed decisions about where to best allocate resources, focusing on Enhanced Recovery After Surgery components that are commonly used and have proven benefits. Second, as patients play a crucial role in their care, knowing the impact of Enhanced Recovery After Surgery programs on the incidence of surgical site infection and hospital readmission can enhance patient compliance and satisfaction. Finally, the results of this umbrella review may inform quality improvement activities clinicians can implement to tailor and optimise their Enhanced Recovery After Surgery programs. The aims of this umbrella review were to:

1. Determine the effect of Enhanced Recovery After Surgery programs on surgical site infections and hospital readmission up to 30 postoperative days for patients undergoing gastro-intestinal surgeries;
2. Identify the components of Enhanced Recovery After Surgery programs that are most used across gastro-intestinal surgery subspecialties or procedures; and,
3. Describe the strategies used to implement Enhanced Recovery After Surgery programs across the preoperative, intraoperative, and postoperative periods of surgical care.

2. Methods

Our review protocol was registered with PROSPERO (CRD). We used Belbasis et al. (2022) methodological guidance in conducting umbrella reviews and the *Preferred Reporting Items for Systematic Reviews and Meta-analyses 2020 Statement* (Page et al., 2021) to guide the reporting of this umbrella review. A key advantage of umbrella reviews is their ability to increase statistical power through pooling data from multiple studies (Fernandez et al., 2025). This aggregation enhances the robustness of findings, reduces redundancy by consolidating existing evidence, and streamlines research efforts, saving time and resources that might otherwise be spent on redundant investigations. Ethics approval was not required as all data were obtained from previously published studies. The reporting of health consumer engagement in this review was guided by the *Guidance for Reporting Involvement of Patients and the Public -GRIPP2-SF* (Staniszewska et al., 2017) (Supplementary material Table 1). This guidance encompasses five items where this involvement can occur, including the aims, methods, results, discussion and conclusions, and a critical perspective for reporting patient and public involvement in research. Our health consumer authors (JC, ST) contributed to all except the first item.

2.1. Selection criteria and search strategy

We used the Population, Intervention, Comparator, Outcome,

Timeframe, and Study design (PICOTS) approach to inform the selection criteria (Table 1) and search strategy. Reviews published in peer-reviewed journals between January 2010 and January 2025 were chosen, with the year 2010 selected because of the widespread introduction of Enhanced Recovery After Surgery programs at this time. We included reviews that reported the diagnosis of surgical site infection based on the Centre for Disease Control and Prevention (Mangram et al., 1999) criteria, or however, defined by the review authors. We extracted data on the a priori outcomes, surgical site infection, and hospital readmission up to 30 postoperative days; thus, to be included, reviews had to report on one or both outcomes. If this was not done, the article was excluded. Reviews of intervention studies (RCT or non-randomised studies of interventions) were included. By only including reviews that specifically focused on interventional designs, we reduced the potential for misinterpretation of intervention effects that may arise from merging different types of studies. Owing to limited resources, reviews had to have been published in English.

In consultation with a health librarian, we developed the database searches. Electronic databases included Medical Literature Analysis and Retrieval System Online (MEDLINE) (via EBSCOhost), the Cumulative Index to Nursing and Allied Health Literature (CINAHL), EMBASE, Scopus, and the Cochrane Library. Search terms included combinations of Enhanced Recovery After Surgery, Enhanced Recovery After Surgery pathway, enhanced recovery, fast track, accelerated track, surgical protocols, surgical recovery, hospital-acquired complications, surgical complications, postoperative infections, and Enhanced Recovery After Surgery implementation. Medical subject headings, keywords, and free text words were searched, depending on the database using Boolean connectors ‘AND’ and ‘OR’. (Supplementary material Table 2, provides examples of MEDLINE and CINAHL searches). Searches were conducted on 16 January 2025. Reference lists of included reviews were also examined to identify additional articles for inclusion.

2.2. Study selection

All results were initially exported to EndNote version 20 [EndNote®, Clarivate Analytics, US], and duplicate entries were removed. Covidence (Innovation VH, 2017) was subsequently used for the screening process. One author (MS) initially screened titles and excluded studies that were clearly irrelevant, according to the selection criteria. Then, two authors

Table 1
Inclusion and exclusion criteria.

Inclusion	Exclusion
<p>P: Adult patients (16 years+) undergoing major open or laparoscopic gastro-intestinal surgical procedures on the oesophagus, stomach, liver, gallbladder, pancreas, spleen, small and large bowel, rectum</p> <p>I: ERAS programs covering the preoperative, intraoperative, and postoperative periods</p> <p>C: Standard/usual care interventions, however defined by review authors</p> <p>O: Clinical outcomes</p> <ul style="list-style-type: none"> • surgical site infection • 30-day hospital readmission <p>T: Systematic reviews from January 2010 onwards</p> <p>S: RCTs (including cluster, factorial and crossover trials) or non-randomised studies (before and after studies, time series, case control and cohort studies)</p>	<p>Gastro-intestinal surgical procedures performed for palliation, patients undergoing organ transplantation and bariatric procedures.</p> <p>Reviews that only report on one phase of ERAS care, e.g., intraoperative care</p> <p>No comparisons undertaken (such as after only studies).</p> <p>Studies reporting the frequency of postoperative complications, where the patient is not the denominator, or where the denominator is not reported.</p> <p>Reviews published from 2010 onwards, in which the data was collected pre-2010. Grey literature including abstracts if full text is not available.</p> <p>Uncontrolled studies (no comparison group).</p>

ERAS = Enhanced Recovery After Surgery; RCT = randomised controlled trials.

(RL/KA) independently assessed the remaining titles and abstracts, resolving discrepancies through discussion with an adjudicator. Then, two authors (MS/KA) independently screened full-text reviews, and disagreements were resolved through consensus or consultation with an adjudicator. Cohen's Kappa was used to assess interrater agreement for both title/abstract and full-text screening. A PRISMA flowchart was created to illustrate the study selection process.

2.3. Data extraction

Data extracted included study characteristics such as geographic location, study design, sample size, types of surgical procedures, length of follow-up, statistical results, number of Enhanced Recovery After Surgery components, and funding sources. Where reviews presented duplicate datasets, in meta-analyses, we included the review with the largest dataset. Where reviews included primary studies published before 2010, we presented the characteristics of these reviews as reported. However, only those unique primary studies included in reviews where data were collected from 2010 onwards were included in our meta-analyses. Enhanced Recovery After Surgery program characteristics, including surgery types (e.g., hepatic, colon, pancreatic), and outcomes were also extracted. Data were extracted independently by pairs of two authors (BG/MS, GT/WC, JL/MS, RW/AE, US/WC, JC/MS) with disagreements resolved through discussion or consultation with an adjudicator. Review authors were contacted for further information if required. The data extraction tool was piloted on three reviews, and minor revisions were made.

2.4. Quality appraisal

Following data extraction, pairs of two researchers (BG/MS, GT/WC, JL/MS, RW/AE, US/WC, JC/MS) independently assessed the methodological quality of the included reviews using the A MeaSurement Tool to Assess systematic Reviews 2 (AMSTAR 2) (Shea et al., 2017). The 16-item A MeaSurement Tool to Assess systematic Reviews 2 tool was originally developed to evaluate the methodological quality of systematic reviews, including umbrella reviews. The A MeaSurement Tool to Assess systematic Reviews 2 tool identifies ‘critical’ (items 2, 4, 7, 9, 11, 13 and 15) and ‘non-critical’ (items 1, 3, 5, 6, 8, 10, 12, 14, 16) domains that must be met in a review, and item responses are scaled as either ‘fully performed’, ‘partially performed’, and ‘yes’ or ‘no’ (Shea et al., 2017). Tool developers discourage the summing of items. Rather, appraisers should consider the quality of the overall review based on ‘high’ (no or one non-critical weakness), ‘moderate’ (more than one non-critical weakness), ‘low’ (one critical flaw with or without non-critical weaknesses), and ‘critically low’ (more than one critical flaw with or without non-critical weaknesses). In cases of disagreement, a third reviewer assisted in reaching a consensus.

2.5. Data synthesis

Study characteristics were summarised in tables and synthesised narratively based on study outcomes. Many systematic reviews shared the same primary studies, so to avoid double-counting, we developed ‘decision rules’ about including primary studies across all reviews (Supplementary material Table 3). These decision rules meant that each primary study was counted only once in each meta-analysis. Data from the primary studies were extracted from the systematic reviews and used in our meta-analyses. Based on the extent of heterogeneity, either fixed or random effects meta-analytic models were used to pool the data (Deeks et al., 2023). Fixed effects models were used when the I^2 was < 50 %, signifying low heterogeneity, while random effects models were chosen for I^2 values \geq 50 %, indicating substantial heterogeneity (Deeks et al., 2023). We planned to transform measures of effect size for continuous data (e.g., if hospital readmission was reported in the number of postoperative days), ensuring a uniform scale for

interpretation across different studies. However, this was not necessary as all outcome data were reported as binary. Heterogeneity assessment involved using the Cochrane Q test (with significance set at $p < 0.05$) and the I^2 statistic. Combined outcomes are presented as estimated odds ratios (eOR) with 95 % confidence intervals (CI). The meta R package automatically calculates eORs for binary outcomes, enabling comparison across groups regardless of study design. We performed subgroup analyses of RCTs vs non-randomised studies of interventions study designs, surgical subspecialty (colorectal, upper gastro-intestinal, hepatobiliary), and the number of Enhanced Recovery After Surgery components (< 9 vs ≥ 9 for surgical site infection and < 11 vs ≥ 11 for 30-day readmission). In interpreting the findings from the subgroup analyses conducted, we did not rely solely on a threshold of $P < 0.05$ to determine statistical significance. Instead, we focused on the consistency, direction, and plausibility of effects across subgroups, given the potential for limited power to detect subgroup differences.

We did not perform imputation for missing data. Studies with incomplete reporting or missing follow-up periods were assessed for eligibility, but only those with sufficient data to contribute to the synthesis were included in the analyses. Where necessary, we contacted study authors for clarification; if no response was received, the study was included in the narrative synthesis only. Due to insufficient data, we could not perform planned subgroup analyses based on geographic regions, surgical approach (planned vs emergency; open vs laparoscopic), duration of operation, and studies of the elderly (≥ 65 years). The software package R was used for all meta-analyses, we used the metafor R package.

2.6. GRADE assessment

We assessed the certainty of the evidence using the GRADE approach. GRADE encompasses five domains: risk of bias, inconsistency,

imprecision, indirectness, and publication bias (Balslem et al., 2011; Grade Working Group, 2013), and the overall body of evidence is categorised into high, medium, low, or very low certainty. The criteria for downgrading are as follows: a reduction of one class for a p-value below < 0.10 in an Egger test; a one-class reduction for an I^2 value of 50 % or higher; a downgrade of one or two classes for imprecision, contingent on the number of participants and power; and a one or two-class reduction for studies with less than 75 % or 50 % of participants at low risk of bias, respectively. The *metaumbrella* package (Gosling et al., 2023) in R was used to calculate the GRADE classifications.

3. Results

3.1. Study characteristics

Fig. 1 presents the PRISMA flowchart. Of the 10,633 articles identified, 7798 titles and abstracts were screened, 108 were assessed at full text, and an additional six reviews were assessed using citation searching but were excluded at full text. Overall, 49 reviews were ultimately included. Reasons for exclusions are detailed in Supplementary material Table 4. Using Cohen's Kappa, the level of agreement between independent screeners for title and abstract and full-text screening was $\kappa = 0.56$, and $\kappa = 0.45$, respectively, indicating moderate agreement.

Supplementary material Table 5 summarises the descriptive characteristics of included systematic reviews as reported by review authors for the outcomes surgical site infection and hospital readmission. Across the 49 included reviews, 22 reviews (Amir et al., 2024; Changsheng et al., 2020; Chen et al., 2015; Ding et al., 2017; Ellwanger et al., 2025; Feng et al., 2023; Grant et al., 2017; Huang et al., 2020; Kennelly et al., 2025; Li et al., 2017; McKechnie et al., 2024; Nair et al., 2022; Rouxel and Beloeil, 2019; Song et al., 2016; Thillainadesan et al., 2021; Tian et al., 2024; Wang et al., 2018, 2023; Wu et al., 2024; Yu et al., 2014;

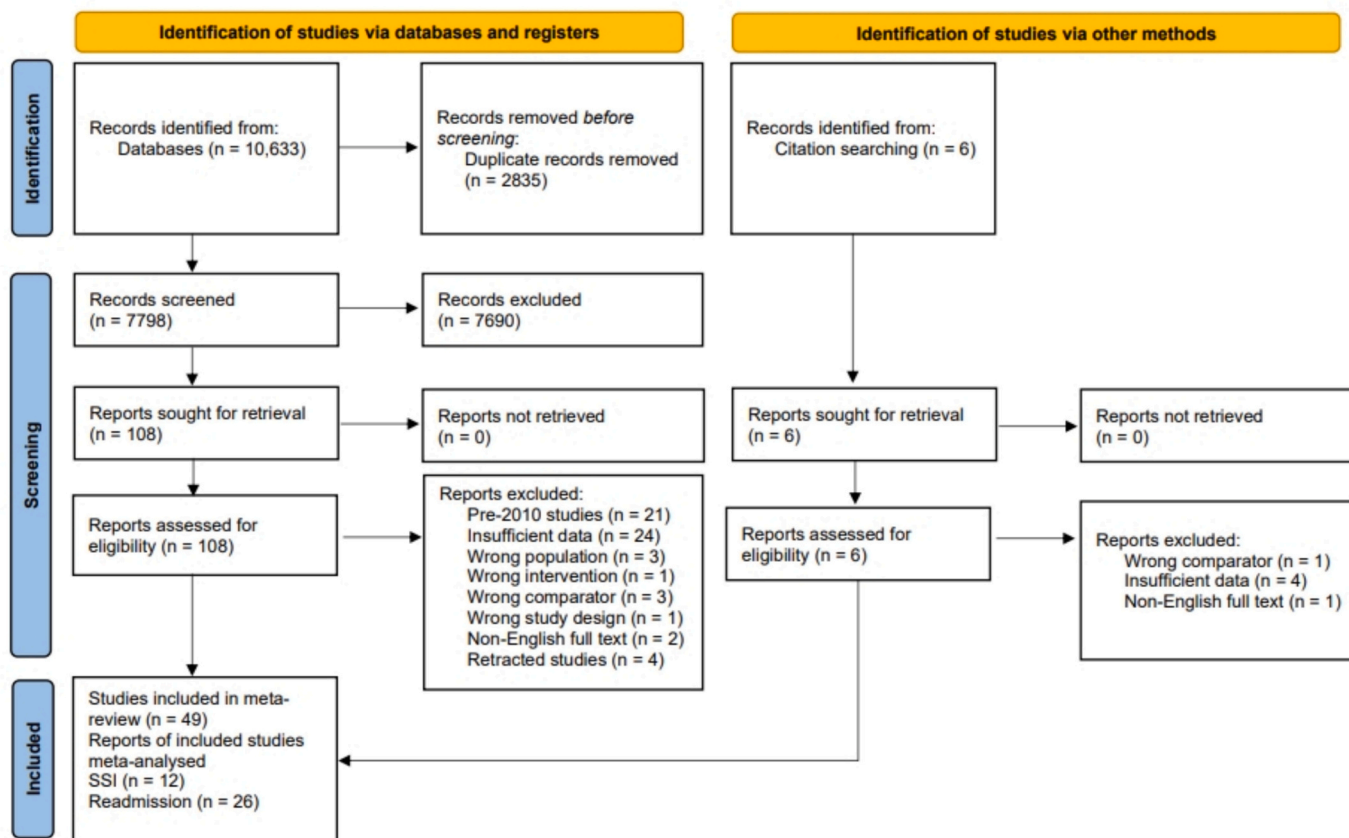


Fig. 1. PRISMA flow chart of selection of reviews.

Zeyara et al., 2024; Zhang et al., 2023) included only RCTs, and five reviews (Cao et al., 2019; Ji et al., 2018; Mao and Huang, 2021; Pisarska et al., 2017; Xiong et al., 2016) included only non-randomised studies of interventions. All systematic reviews except one (Rouxel and Beloeil, 2019) included meta-analyses. For surgical site infection, reviews where authors pooled data, based only on RCTs, sample sizes ranged from 242 (Nair et al., 2022) to 3279 (Grant et al., 2017) patients. For hospital readmission, pooled sample sizes across RCTs ranged from 105 (Li et al., 2017) to 1515 (Feng et al., 2023). Follow-up periods were not reported in 28 reviews (Amir et al., 2024; Beamish et al., 2015; Delabays et al., 2024; Ding et al., 2017; Ellwanger et al., 2025; Feng et al., 2023; Hajibandeh et al., 2020; Ji et al., 2018; Kennelly et al., 2025; Kuemmerli et al., 2024; Liotiri et al., 2024; Mao and Huang, 2021; Nair et al., 2022; Puccetti et al., 2022; Qin et al., 2024; Rouxel and Beloeil, 2019; Tan et al., 2021; Thillainadesan et al., 2021; Trejo-Avila et al., 2019;

Triantafyllou et al., 2020; Visoni et al., 2018; Wang et al., 2020, 2023; Wee et al., 2019; Xiong et al., 2016; Zeyara et al., 2024; Zhang et al., 2023; Zhou et al., 2022). Only two reviews reported funding for included primary studies (Changsheng et al., 2020; Greer et al., 2018).

3.2. Clinical outcomes

Supplementary material Table 6 summarises the number of reviews reporting the effectiveness of Enhanced Recovery After Surgery programs. Forest plots detailing subgroup analyses of RCT vs non-randomised studies of interventions are presented for surgical site infection and hospital readmission (Figs. 2 and 3).

3.2.1. Surgical site infection

11 systematic reviews (Amir et al., 2024; Ellwanger et al., 2025; Feng

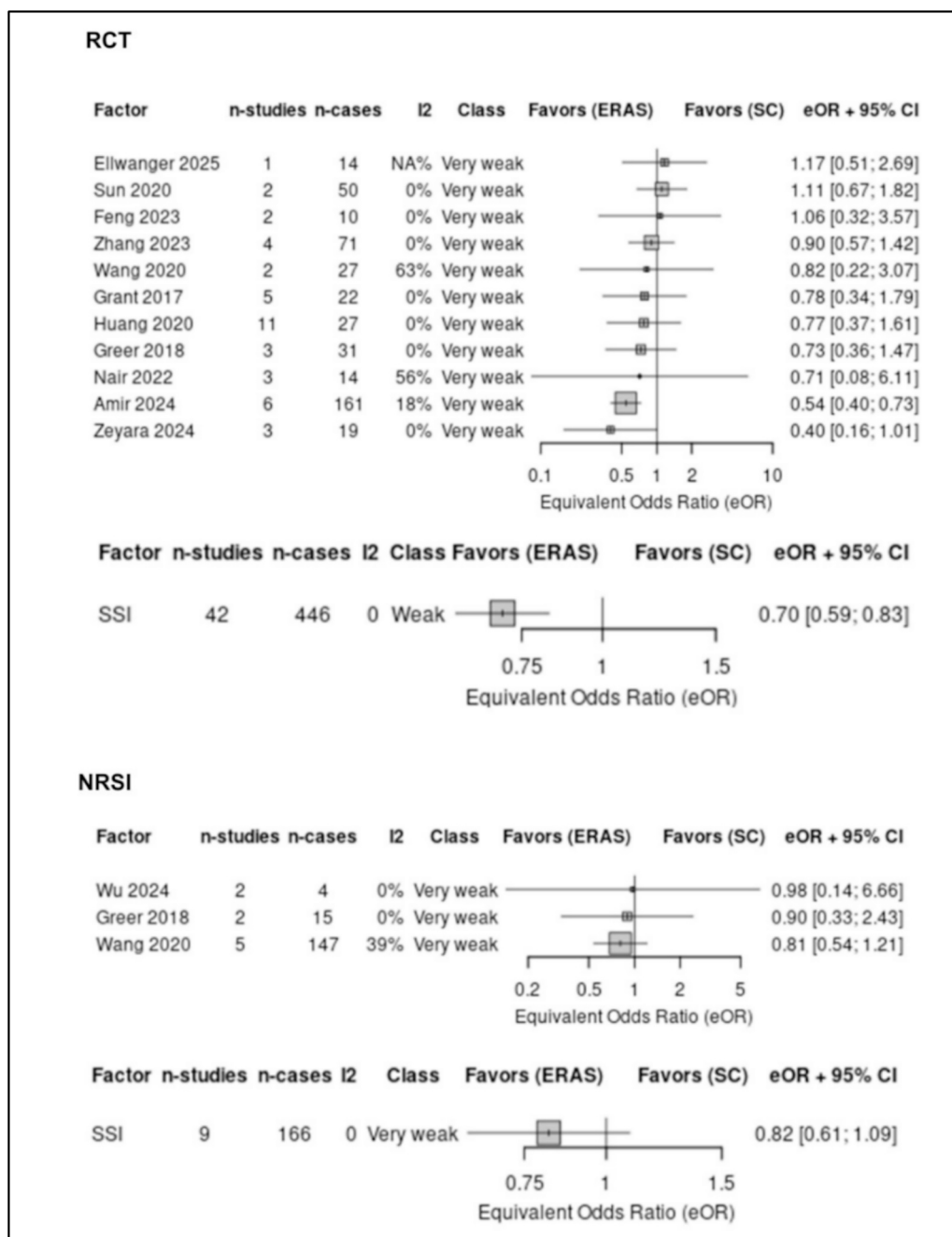


Fig. 2. Surgical site infection (RCT; non-randomised studies of interventions) forest plot.

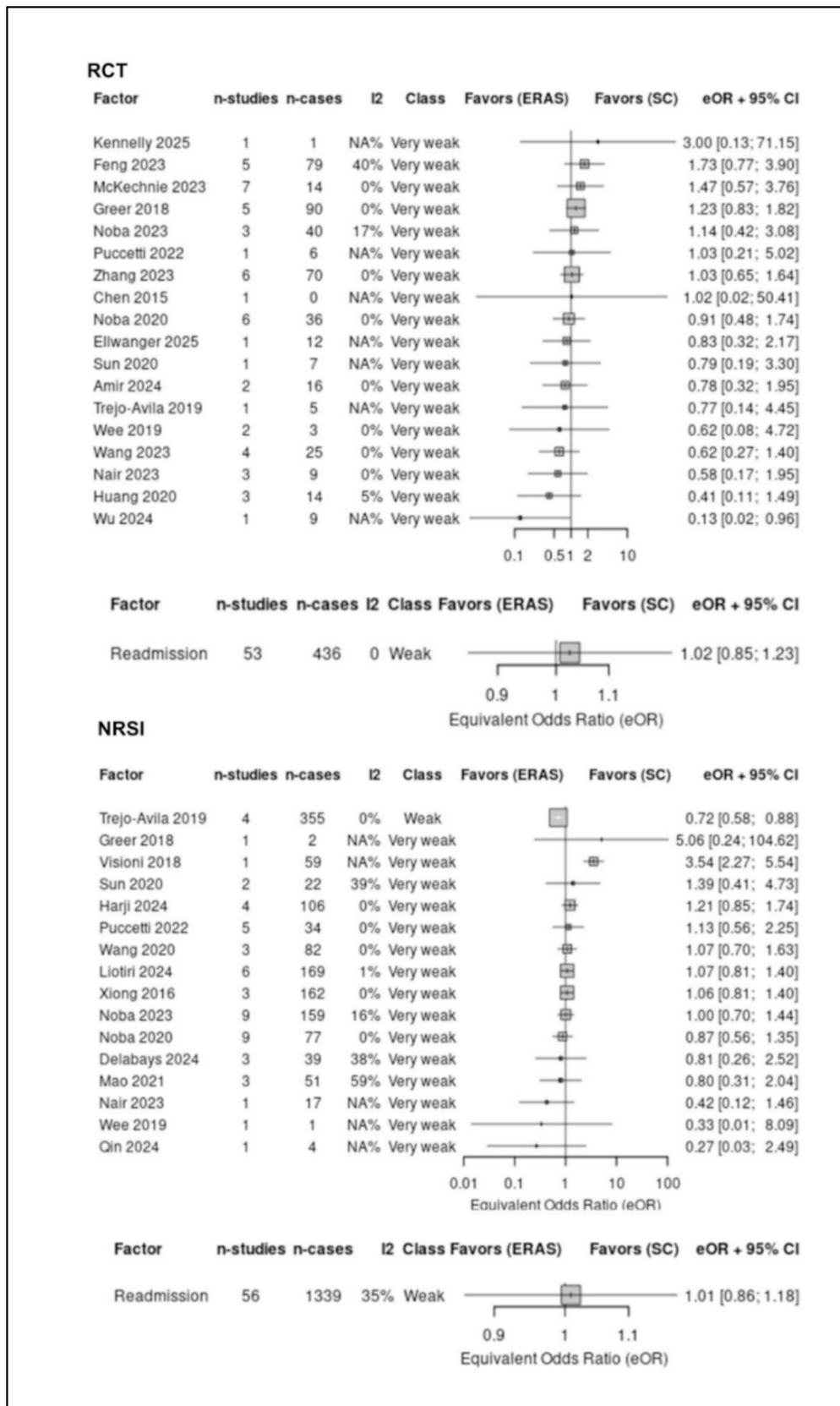


Fig. 3. Hospital readmission (RCT; non-randomised studies of interventions) forest plot.

et al., 2023; Grant et al., 2017; Greer et al., 2018; Huang et al., 2020; Nair et al., 2022; Sun et al., 2020; Wang et al., 2020; Wu et al., 2024; Zeyara et al., 2024; Zhang et al., 2023) based on 42 unique primary RCT studies with 5112 patients were included in the meta-analysis for surgical site infection (Fig. 2). The calculated eOR of RCTs was 0.70 (95 % CI 0.59–0.82, $p < 0.001$), indicating a statistically significant effect with a 30 % reduction in surgical site infections associated with Enhanced Recovery After Surgery use. A fixed effect model was used because heterogeneity was low across studies ($I^2 = 0\%$). Summary estimates were similar across non-randomised studies of interventions, with results suggesting a non-significant decrease in surgical site infection with Enhanced Recovery After Surgery use, and heterogeneity was low at 0 %.

3.2.2. Hospital readmission

18 systematic reviews (Amir et al., 2024; Chen et al., 2015; Ellwanger et al., 2025; Feng et al., 2023; Greer et al., 2018; Huang et al., 2020; Kennelly et al., 2025; McKechnie et al., 2024; Nair et al., 2023; Noba et al., 2020, 2023; Puccetti et al., 2022; Sun et al., 2020; Trejo-Avila et al., 2019; Wang et al., 2023; Wee et al., 2019; Wu et al., 2024; Zhang et al., 2023) comprising 53 unique primary RCT studies, with 7360 patients, were included in the meta-analysis for hospital readmission (Fig. 3). The calculated eOR was 1.03 (95 % CI 0.85–1.23, $p = 0.792$), suggesting a 2.5 % increase in the odds of hospital readmission associated with Enhanced Recovery After Surgery use, although the effect was not statistically significant. A fixed effect model was used because heterogeneity was low across studies ($I^2 = 0\%$). Summary estimates across non-randomised studies of interventions were similar to the pooled results of RCT studies, with results suggesting that Enhanced Recovery After Surgery was associated with a non-significant increase in hospital readmission.

3.3. Subgroup analyses

Forest plots for subgroup analyses based on subspecialty were undertaken for primary studies using RCT designs (Supplementary material Fig. 1a and b). Surgical site infection incidences varied clinically between procedures, where Enhanced Recovery After Surgery appeared to have no significant effect on most surgical procedures. In contrast, the risk of surgical site infection in patients undergoing abdominal surgery appeared to be reduced by 45 % when Enhanced Recovery After Surgery was implemented. Effect estimates were somewhat varied between different surgical procedures, where procedures, like gastrectomy, appeared to be associated with a higher risk of readmission clinically, but the differences were not statistically significant. However, these associations were not statistically significant, most likely due to a lack of power related to small sample sizes.

3.4. Enhanced Recovery After Surgery implementation

Table 2 summarises aspects of Enhanced Recovery After Surgery implementation across included reviews. Across all reviews, there was no information about the health professionals (surgeons/anaesthetists/nurses/others) involved in Enhanced Recovery After Surgery implementation. The number of Enhanced Recovery After Surgery components was not reported or was unclear in thirteen reviews (Amir et al., 2024; Beamish et al., 2015; Ellwanger et al., 2025; Mao and Huang, 2021; Nair et al., 2023, 2022; Puccetti et al., 2022; Qin et al., 2024; Tan et al., 2021; Trejo-Avila et al., 2019; Wang et al., 2020; Zhang et al., 2023; Zhou et al., 2022). Only one review (Greer et al., 2018) reported using audit to check Enhanced Recovery After Surgery compliance; four (McKechnie et al., 2024; Noba et al., 2020, 2023; Tan et al., 2021) reported implementation fidelity, and only two reviews (Feng et al., 2023; Greer et al., 2018) reported loss to follow-up.

Table 2

Enhanced Recovery After Surgery implementation strategies, intervention fidelity, and loss-to-follow-up analyses (n = 49).

ERAS implementation	Number of studies	Studies
Reported number of ERAS items used across studies	36	(Cao et al., 2019; Changsheng et al., 2020; Chen et al., 2015; Delabays et al., 2024; Ding et al., 2017; Feng et al., 2023; Grant et al., 2017; Greer et al., 2018; Hajibandeh et al., 2020; Harji et al., 2024; Huang et al., 2020; Ji et al., 2018; Kennelly et al., 2025; Kuemmerli et al., 2024; Li et al., 2016; Li et al., 2017; Liotiri et al., 2024; Noba et al., 2020; Noba et al., 2023; McKechnie et al., 2024; Pisarska et al., 2017; Rouxel and Beloeil, 2019; Song et al., 2016; Sun et al., 2020; Thillainadesan et al., 2021; Tian et al., 2024; Triantafyllou et al., 2020; Visionsi et al., 2018; Wang et al., 2017; Wang et al., 2018; Wang et al., 2023; Wee et al., 2019; Wu et al., 2024; Xiong et al., 2016; Yu et al., 2014; Zeyara et al., 2024)
Implementation strategies before, during and post-implementation of ERAS	1	(Greer et al., 2018)
ERAS intervention fidelity	4	(McKechnie et al., 2024; Noba et al., 2020; Noba et al., 2023; Tan et al., 2021)
Loss to follow-up; Intention to treat analysis	2	(Feng et al., 2023; Greer et al., 2018)

ERAS = Enhanced Recovery After Surgery.

3.5. Enhanced Recovery After Surgery components

We performed further sensitivity analyses to evaluate the effect of the variation in using Enhanced Recovery After Surgery components on our primary patient outcomes, surgical site infection, and readmission. Based on the number of Enhanced Recovery After Surgery components reported in the primary RCT studies of the included reviews, we found the median cut-offs to be 9 and 11 for surgical site infection and readmission, respectively (Supplementary material Fig. 2a and b). For surgical site infection, four reviews (Feng et al., 2023; Grant et al., 2017; Huang et al., 2020; Zeyara et al., 2024) based on 10 primary RCTs with < 9 components (n = 838) and 16 primary studies with ≥ 9 components (n = 2549), respectively, reported the number of Enhanced Recovery After Surgery components across reviews. Results suggest a 47 % reduction in surgical site infection with < 9 components (eOR = 0.53, 95 % CI 0.28–0.98). For readmission, seven reviews (Ellwanger et al., 2025; Feng et al., 2023; Greer et al., 2018; Huang et al., 2020; Noba et al., 2023; Sun et al., 2020; Wu et al., 2024) based on 11 RCTs with < 11 components (n = 1673) and 27 RCTs with ≥ 11 components (n = 3652 patients), respectively, reported the number of Enhanced Recovery After Surgery components across reviews. These results suggest no association between the number of Enhanced Recovery After Surgery components and reductions in hospital readmission.

3.6. Risk of bias and GRADE

The results for the risk of bias assessment using the A MeaSurement Tool to Assess systematic Reviews 2 (Shea et al., 2017) are reported in Supplementary material Table 7. For reviews where RCTs were included, the proportion of reviews meeting the criteria (“Y” denoting yes) across the seven ‘critical’ domains (Q2, 4, 7, 9, 11, 13, and 15) ranged from 9 % (Q4, Q7) to 90 % (Q11). For reviews that included non-

randomised studies of interventions, 0 %, 20 %, and 20 % met the criteria for the critical domains Q4, Q7, and Q11, respectively. All except one review (McKechnie et al., 2024) were rated as ‘critically low’ as they all had more than *one critical flaw with or without non-critical weaknesses*, thus not meeting the criteria based on the A MeaSurement Tool to Assess systematic Reviews guidance. The results of the GRADE assessment suggest the body of evidence for the effect of Enhanced Recovery After Surgery in reducing the likelihood of surgical site infection and hospital readmission is ‘very low’ (Figs. 2 and 3).

4. Discussion

4.1. Summary of umbrella review findings

The strongest summary evidence from randomised controlled trials demonstrated a statistically significant association between Enhanced Recovery After Surgery implementation and a reduction in surgical site infections, but no significant association with hospital readmission. Except for abdominal surgery, which showed a 45 % reduction in surgical site infections—subgroup analyses by surgical subspecialty did not reveal statistically significant associations. This was likely due to insufficient power to detect differences between groups. Given the very low-quality scores and GRADE assessments, along with meta-analytic results, the overall body of evidence is weak and inconclusive. Further research is needed to determine the true nature of the relationship between Enhanced Recovery After Surgery and its impact on surgical site infections and hospital readmission. These conclusions are based on evidence from RCTs, as the findings from non-randomised studies of interventions are prone to potential confounders, which can lead to overestimation of effects. In cases where reviews reported estimates from both RCTs and non-randomised studies of interventions, we relied on RCTs. This approach is further supported by the fact that none of non-randomised studies of interventions reviews reported adjusted effects; instead, they presented only crude estimates for surgical site infections and hospital readmission associated with Enhanced Recovery After Surgery programs.

Previous clinical trials have demonstrated the clinical benefits of Enhanced Recovery After Surgery programs in reducing postoperative complications in selected patient cohorts (Ripolles-Melchor et al., 2020). Yet, these benefits are not always replicated in real-world settings. A recent step-wedge cluster randomised trial found no beneficial effect on patient outcomes. Instead, there was a non-significant trend toward a higher risk of complications and reoperations, as well as increased hospital readmissions in patients exposed to Enhanced Recovery After Surgery programs (Pagano et al., 2024). Conversely, it may be expected that earlier discharge with better outcomes may result in a small rise in readmissions, highlighting the importance of ensuring patients have good quality discharge education and clear pathways and access to re-enter care. This suggests a need for appropriate transitional care, with support required in the community for patients who are not managing their recovery.

4.2. ERAS implementation and component adherence

Across reviews, there was considerable variation in Enhanced Recovery After Surgery components reported in primary studies, ranging from 5 (Greer et al., 2018; Huang et al., 2020; Wu et al., 2024) to 28 (McKechnie et al., 2024) components. It is more likely that full adherence to all components may achieve benefits comparable to those in highly controlled experimental clinical trials (Duclos, 2024). Pragmatically, in the clinical setting, there is no ‘control’ over the number of ERAS components implemented or the quality and consistency of their implementation, and the reviews did not report on the extent to which components may have been implemented as intended. Further, none of the reviews reported who, when, and how Enhanced Recovery After Surgery was introduced and used. Only four reviews (Greer et al., 2018;

McKechnie et al., 2024; Noba et al., 2020, 2023) reported on Enhanced Recovery After Surgery intervention fidelity across individual studies. So, across reviews, the ‘dose’ of Enhanced Recovery After Surgery interventions could not be determined. The extent to which all Enhanced Recovery After Surgery components are implemented as planned impacts the success in driving patient, systemic, and economic outcomes (Thanh et al., 2016).

4.3. Strengths and limitations

While this review has several strengths, we also acknowledge its limitations relative to the included reviews and the methods used to undertake them. Only 50 % of included reviews reported follow-up periods and definitions for the outcomes of interest. Across reviews, ‘standard care’ was not defined, and the number of Enhanced Recovery After Surgery components implemented varied across reviews. Thus, it is difficult to assess the extent similar components may have been used across control groups. In addition to variation in the components included in standard care, other practice-related differences, such as institutional protocols, clinician expertise, staffing levels, and adherence to interventions, are also likely to have introduced substantial heterogeneity. Furthermore, poor reporting of follow-up periods across studies limits the ability to determine whether outcomes were assessed consistently, adding another layer of uncertainty to the interpretation of the synthesised results.

Further, our analyses based on surgical subspecialty and the number of Enhanced Recovery After Surgery components were underpowered, affecting the precision and generalisability of study results. Despite these methodological variations, our findings suggest that significant statistical heterogeneity was not evident between studies, with almost all individual reviews showing the same direction of effect but not reaching statistical significance. This result may suggest that larger studies, where patients in the control group are not exposed to a ‘partial’ Enhanced Recovery After Surgery intervention, may have sufficient power to demonstrate the true benefits of Enhanced Recovery After Surgery in reducing surgical site infection and readmissions if such benefits exist.

While most of the included reviews were reported according to the PRISMA statement, all but one review (McKechnie et al., 2024) had methodological deficits relative to the A MeaSurement Tool to Assess systematic Reviews 2 critical domains. Most reviews fell short in two or more of the A MeaSurement Tool to Assess systematic Reviews critical domains. For example, only 12 (24 %) reviews comprehensively reported on the search strategies and justified language exclusions, and only three (6 %) reviews provided a list of excluded studies/reasons for exclusion. Despite the robust methods used to conduct this umbrella review, there is always the possibility that we missed potentially eligible reviews for inclusion in our searches. While we achieved moderate agreement during screening, it was not ideal. To mitigate misclassification, all title/abstract and full-text discrepancies were independently adjudicated. Finally, the considerable duplication of primary studies across these systematic reviews was problematic. To ensure primary studies were counted only once, they were included in the largest review and removed from the analyses of other reviews where the same primary study was shared. This may have reduced statistical power.

4.4. Implications

Although Enhanced Recovery After Surgery appears to reduce surgical site infections, its impact on readmission remains unclear. This highlights several practical considerations for implementation. Variability in adherence across settings may influence outcomes, particularly where core components are inconsistently applied or poorly documented (Jones and Kelliher, 2019). Plausibly, optimising the ‘dose’ of Enhanced Recovery After Surgery component delivery involves striking a balance between providing sufficient intensity to achieve

desired outcomes while minimising resource utilisation or patient burden (Ripoles-Melchor et al., 2019). The omission of certain components may reduce the effectiveness of the pathway and heighten patients' risk of adverse outcomes. Further, the lack of effect on readmission may also reflect gaps in discharge education and transitional care planning, especially following early discharge, a known feature of Enhanced Recovery After Surgery pathways (Ljungqvist et al., 2017). Merely having Enhanced Recovery After Surgery programs is not enough; implementation and maintenance of a successful program requires strong clinical and managerial leadership (Jones and Kelliher, 2019). Strengthening these aspects may enhance the effectiveness and sustainability of Enhanced Recovery After Surgery in routine practice.

5. Conclusions

The findings of this umbrella review suggest a statistically significant effect of Enhanced Recovery After Surgery programs in reducing surgical site infection and a non-statistically significant effect on 30-day hospital readmission after gastrointestinal surgical procedures. The primary studies in the included reviews focused on clinical outcomes without clear definitions for surgical site infection and readmission, limiting the conclusions that can be drawn from our analyses. A heterogeneous set of Enhanced Recovery After Surgery components with varying levels of compliance underscores the complexity of standardisation in surgical care pathways. For consumers, this study highlights an important point: Enhanced Recovery After Surgery programs may not always deliver the expected benefits. This information can help patients make informed decisions about their care and discuss potential risks and benefits with their healthcare providers.

CRedit authorship contribution statement

Brigid M. Gillespie: Writing – review & editing, Writing – original draft, Visualization, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Georgia Tobiano:** Writing – review & editing, Investigation, Data curation. **Josephine Lovegrove:** Writing – review & editing, Investigation, Data curation. **R.D. Udeshika Priyadarshani Sugathapala:** Writing – review & editing, Formal analysis, Data curation. **Lukman Thalib:** Writing – review & editing, Investigation, Formal analysis. **Rhea Liang:** Writing – review & editing, Visualization, Investigation. **Rachel Walker:** Writing – review & editing, Data curation. **Annette Erickson:** Writing – review & editing, Data curation. **Joan Carlini:** Writing – review & editing, Investigation, Data curation. **Stanley Thong:** Writing – review & editing, Investigation. **Keith Addy:** Writing – review & editing, Data curation. **Melinda Spencer:** Writing – review & editing, Investigation, Data curation. **Wendy Chaboyer:** Writing – review & editing, Visualization, Validation, Methodology, Investigation, Data curation.

Declaration of competing interest

None.

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Appendix. Supplementary data

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