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Interventions to improve enablers and/or overcome barriers to seeking care during pregnancy, birthing and postnatal period for women living with vulnerabilities in high-income countries: A systematic review and meta-analysis

Emily R Bowden[a,*], Anne B Chang[a,b,c], Gabrielle B McCallum[a]

[a] Child Health Division, Menzies School of Health Research, Charles Darwin University, Darwin, NT, SA
[b] Australian Centre for Health Services Innovation, Queensland University of Technology, Brisbane, SA
[c] Department of Respiratory and Sleep Medicine, Queensland Children's Hospital, Brisbane, SA

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ABSTRACT

Objective: To reduce maternal morbidity and mortality, World Health Organization recommendations include: commencing pregnancy care before 12-weeks', at least eight antenatal and four postnatal visits, and attendance of skilled care at birthing. While lower adherence to the recommendation predominates in low- and middle-income countries, it also occurs in some settings in high-income countries. Globally, various strategies are used to optimise maternity care, in line with these recommendations. This systemic review aimed to determine if enhanced care improves maternal care-seeking, thus improving clinical outcomes for women and babies living with vulnerabilities, in high-income countries.

Design, setting and participants: We searched the Cochrane Central Registers of Controlled Trials and Cochrane Pregnancy and Childbirth, MEDLINE, CINAHL, Proquest Dissertation and Thesis and reference lists of relevant articles. The latest search was performed June 20, 2022. Randomised controlled trials, non-randomised intervention trials and cohort studies comparing effects of interventions designed to increase utilisation of maternal health services with routine care, for women at increased risk of maternal mortality and severe maternal morbidity in high-income countries were included. Two authors selected, extracted, assessed and analysed data. Additional information was sought from study authors. This systematic review and meta-analysis was registered with PROSPERO(CRD42021256811).

Findings: Nine studies with 5,729 participants were included. Interventions to enhance care significantly increased utilisation of health services, increasing attendance at antenatal classes (Odds Ratio[OR]=15·23, 95%Confidence Interval[CI] 10·73–21·61, $p<0·0001$) and postnatal visits by 6–8 weeks (OR=2·66, 95%CI 1·94–3·64, $p<0·0001$), compared to routine care. Infants in the intervention groups were significantly less likely to be: born preterm (OR=0·68, 95%CI 0·56–0·82, $p<0·0001$); low birthweight (OR=0·78, 95%CI 0·64–0·95, $p = 0·01$) or; require neonatal intensive care (OR=0·80, 95%CI 0·66–0·96, $p = 0·02$).

Conclusions and Implications for practice: Among women living with vulnerabilities in high-income countries, interventions to enhance care increases utilisation of maternal health services and improves outcomes.

Introduction

The World Health Organization (WHO) recommends commencing pregnancy care before 12-weeks gestation (WHO, 2017), a minimum of eight antenatal contacts within a midwife-led continuity-of-care model, and a minimum of four postnatal visits (WHO, 2015). These aim to reduce perinatal mortality, severe maternal morbidity and improve women's experience of care (WHO, 2017). Furthermore, attendance of a skilled birth attendant is recognised as critical to improving maternal and neonatal survival (Pathmanathan and Liljestrand, 2003). Globally, many women commence pregnancy care later than recommended, have less

* Corresponding author at: Menzies School of Health Research, PO Box 41096, Casuarina, NT 0811, Australia.
E-mail address: emily.bowden@menzies.edu.au (E.R. Bowden).

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antenatal contacts and/or birth without the support of a skilled birth attendant. These women are at greatest risk of dying from complications of pregnancy or birthing (Sturrock et al., 2021; WHO, 2019). Additionally, for each maternal death, an estimated 50–100 women experience severe maternal morbidity (Howell, 2018).

While the largest burden occurs in low- and middle-income countries (LMICs), certain women from high-income countries (HICs) also experience a disproportionate burden of maternal mortality and severe maternal morbidity. These women include First Nations (DoH, 2020), refugees/migrants (Gibson-Helm et al., 2014), those living with disability (Humphrey, 2016), and adolescents (Feijen-de Jong et al., 2012). They are also more likely to have late commencement or inadequate utilisation of care (Gibson-Helm et al., 2014; Osterman and Martin, 2018). In the 2021 'Australia's mothers and babies' report (AIHW, 2021), adolescents, migrants/refugees, and Australian First Nations women were reported to have lower rates of accessing antenatal care in the first trimester (65%, 74%, 67% respectively) when compared to Australian women collectively (77%). There is lower utilisation of health services throughout pregnancy for Australian First Nations women (12%) and adolescents (9%) who attended less than five antenatal appointments, compared to 4% of other women (AIHW, 2021).

Various strategies which provide care over and above standard care are used globally to optimise pregnancy and postnatal services. These include continuity of carer models, and specific care for women with identified clinical, psychosocial, or sociodemographic risk factors (Symon et al., 2017). In Australia, several models found to be beneficial, and appealing to women, include midwifery group practice (Forster et al., 2016) and culturally tailored continuity-of-carer services (Hartz et al., 2019). Key attributes of standard care compared to enhanced models of care are further described in Table 1. However, despite the availability of these enhanced models of care in HICs, enablers and barriers to accessing these by women living with vulnerabilities, and therefore at increased risk of maternal mortality and severe maternal morbidity, are currently not well understood.

Interventions targeting enablers and/or barriers to improve utilisation of maternal health services could thus result in better clinical outcomes for women and babies (WHO, 2017). The underlying mechanisms on how such interventions improve access or care-seeking are likely multifactorial and include provision of maternal expertise through well trained health professionals, physical factors (e.g. providing transport, reducing wait-times), cognitive reasons (e.g. age-appropriate health education) and socio-cultural factors (e.g. culturally tailored care, female professionals).

While the greater proportion of maternal deaths and severe maternal morbidity occur in LMICs (Sullivan et al., 2017; Alkema et al., 2016) certain women from HICs are also known to have an increased risk of maternal mortality and severe maternal morbidity (Humphrey, 2016; Singh, 2021). Australian First Nations women are nearly four times as likely to experience maternal death compared to non-First Nations women (20·2 and 5·5 per 100,000 respectively) (DoH, 2020). In parts of the United States (US), non-Hispanic Black women are 12 times more likely to experience maternal death than non-Hispanic white women (Howell, 2018). The Netherlands report similar findings with migrant women having a threefold higher risk of maternal death than Dutch women (Kallianidis et al., 2021). Thus, identifying evidence-based interventions which improve enablers and overcome barriers to care-seeking in pregnancy and birthing would be beneficial.

Our aim was to determine if interventions improve care-seeking in pregnancy, birthing, and the postnatal period (up to six weeks), and improve clinical outcomes, for women living with vulnerabilities, and their babies, in high-income countries.

Methods

Study strategy and selection criteria

Our selection criteria initially included all RCTs and was later broadened to include non-randomised trials and cohort studies, comparing the effect of interventions designed to increase utilisation of maternal health services (up to six weeks postpartum), to controls (routine care), conducted in any setting in HICs.

Participants included were women from HICs, known to have an increased risk of maternal mortality and severe maternal morbidity accessing antenatal, intrapartum, or postpartum care.

Our primary outcome was utilisation of maternal health services (up to six weeks). These services were defined as antenatal engagement with a maternal health practitioner (midwife, obstetric nurse and/or doctor) including antenatal clinic visits or classes, intrapartum and/or postpartum care provided by a maternal health practitioner.

Our secondary outcomes were the effect of these interventions (to six weeks postpartum) on:

Maternal outcomes

1. Maternal mortality
2. Severe maternal morbidity (Adane et al., 2020)

Infant outcomes

1. Mortality (stillbirth or neonatal death)
2. Birth outcomes (low birthweight [<2500 gm], preterm birth [<37 weeks])
3. Neonatal morbidity (APGAR score<7 at five minutes, treatment with antibiotics, admission to neonatal intensive care)
4. Infant respiratory events (medically significant infant respiratory events were pre-defined as any of the following: increased cough, increased work of breathing, new abnormal chest exam findings with or without fever or radiographic findings and treated with antibiotics) (McCallum et al., 2020)

The protocol for this systematic review was prospectively registered with PROSPERO in June 2021 however, only included RCTs, as non-randomised trials and cohort studies were not included until June 2022 (CRD42021256811) (Bowden et al., 2020)

The last search was undertaken on June 20, 2022. Databases were searched from inception until search date with publications limited to English. We used the same search terms across databases (Supplementary File, Table 1). The search results were independently reviewed by two authors (ERB and GBM).

The search was conducted using the following databases:

1. The Cochrane Central Registers of Controlled Trials (CENTRAL)
2. The Cochrane Pregnancy and Childbirth Specialised Register
3. MEDLINE (PubMed)
4. CINAHL
5. ProQuest Dissertation and Thesis
6. ClinicalTrials.gov
7. WHO International Clinical Trials Registry Platform Trials Portal
8. Australian and New Zealand Clinical Trials Registry (ANZCTR)

Data analysis

All titles retrieved through the search process were exported to EndNote version 20 for de-duplication and screening. Two review authors (ERB and GBM) independently examined the titles and abstracts of electronic records according to the eligibility criteria and full-text records were obtained for all potentially relevant articles for full review. Reference lists of the included studies were also checked for additional articles. From the full text articles, the same

Table 1
Key attributes of standard care compared to various models of enhanced care.

Care time point	[1]Standard care	[1]Midwifery-led continuity-of-carer	[2]Culturally tailored continuity-of -carer	[3]Group antenatal care	[4]Gift incentive
Antenatally	• Provided by midwives or obstetric doctors rostered on that shift. • Minimal flexibility with appointment locations, days and times. • Short appointment times. • Translators available by appointment only. • Education determined by checklist of requirements.	• Provided by a known midwife or group of midwives throughout the pregnancy. • Locations often in the community rather than at a hospital. • Times and dates negotiable between midwives and women. • Longer appointment times. • Tailored education based on woman's needs. • Accessibility to known midwife in-between appointments.	• Supported by member of cultural group employed by health facility who is able to facilitate translation, access to culturally safe ancillary services, and advocate on behalf of the woman. • Access to and support from other women from same cultural background • Tailored education based on woman's needs. • Often includes continuity of carer by midwife.	• Provided by a known midwife or group of midwives throughout the pregnancy. • Support from women at similar stage of pregnancy, and often from a similar cultural or socio-economic background. • Longer appointment times. • Tailored education based on woman's needs.	• As per standard care. • Women informed that they will receive a gift at a particular milestone.
Intrapartum	• Provided by midwives or obstetric doctors rostered on that shift. • Care provider changes at change of shift.	• Provided by one of the known midwives. • Continuous care by known midwife throughout labour and birthing. • Women's preferences well known to care provider/s.	• Women's preferences able to be made known to care provider/s. • Sometimes includes continuous care by known midwife throughout labour and birthing.	• As per standard care	• As per standard care.
Postpartum	• Provided by midwives or obstetric doctors rostered on that shift. • Education determined by checklist of requirements.	• Provided by one of the known midwives. • Tailored education based on woman's needs. • Women's preferences well known to care provider/s.	• Tailored education based on woman's needs. • Women's preferences able to be made known to care provider/s.	• As per standard care • Postnatal support groups often develop organically from within the antenatal group.	• As per standard care • Women who attend milestone receive gift.

[1] Forster et al., 2016.
[2] Hartz et al., 2019.
[3] Ickovics et al., 2016.
[4] Stevens-Simon et al., 1994.

two reviewers independently assessed studies for inclusion based on eligibility criteria.

We had no disagreements but had planned to resolve disagreements through discussion with another review author (ABC). We extracted data using a standardised data collection form and managed data using the Cochrane software (Review Manager 5·4·1, 2020) in accordance with recommendations provided in the Cochrane Handbook for Systematic Reviews (Higgins and Green, 2008). We recorded the selection process in a PRISMA diagram (Fig. 1).

The same two reviewers independently assessed the risk of bias for each RCT using the criteria outlined in the Cochrane Handbook for Systematic Reviews, according to the domains high, low or unclear (Higgins and Green, 2008), and low, moderate, serious, critical or no information for non-randomised studies (McGuinness and Higgins, 2020). We planned to discuss any disagreement with a third independent reviewer (ABC).

For randomized controlled trials we assessed for:

1. Random sequence generation (selection bias)
2. Allocation concealment (selection bias)
3. Blinding of participants and personnel (performance bias)
4. Blinding of outcome assessment (detection bias)
5. Incomplete outcome data (attrition bias)
6. Selective reporting (reporting bias)
7. Other bias

For non-randomized trials, the same two reviewers assessed bias relating to:

1. Confounding
2. Selection of participants
3. Classification of intervention
4. Deviations from intended intervention
5. Missing data
6. Measurement of outcomes
7. Selection of reported results

Assessment of reporting biases

If reporting bias was suspected, we planned to contact the study authors to ask for missing outcome data. We planned that if necessary data were not provided, and if this was thought to introduce serious bias, the impact of including such studies in the overall assessment would be explored through sensitivity analysis.

For dichotomous variables, we analysed using odds ratios (ORs) and continuous data as mean differences (MDs). We firstly performed meta-analyses on data from RCTs only, and then reanalysed the data including non-randomised trials where appropriate. We combined the results from both randomized and non-randomized studies as there was little heterogeneity between study designs and the interaction between the effect of intervention and the choice of allocation was considered unlikely to al-

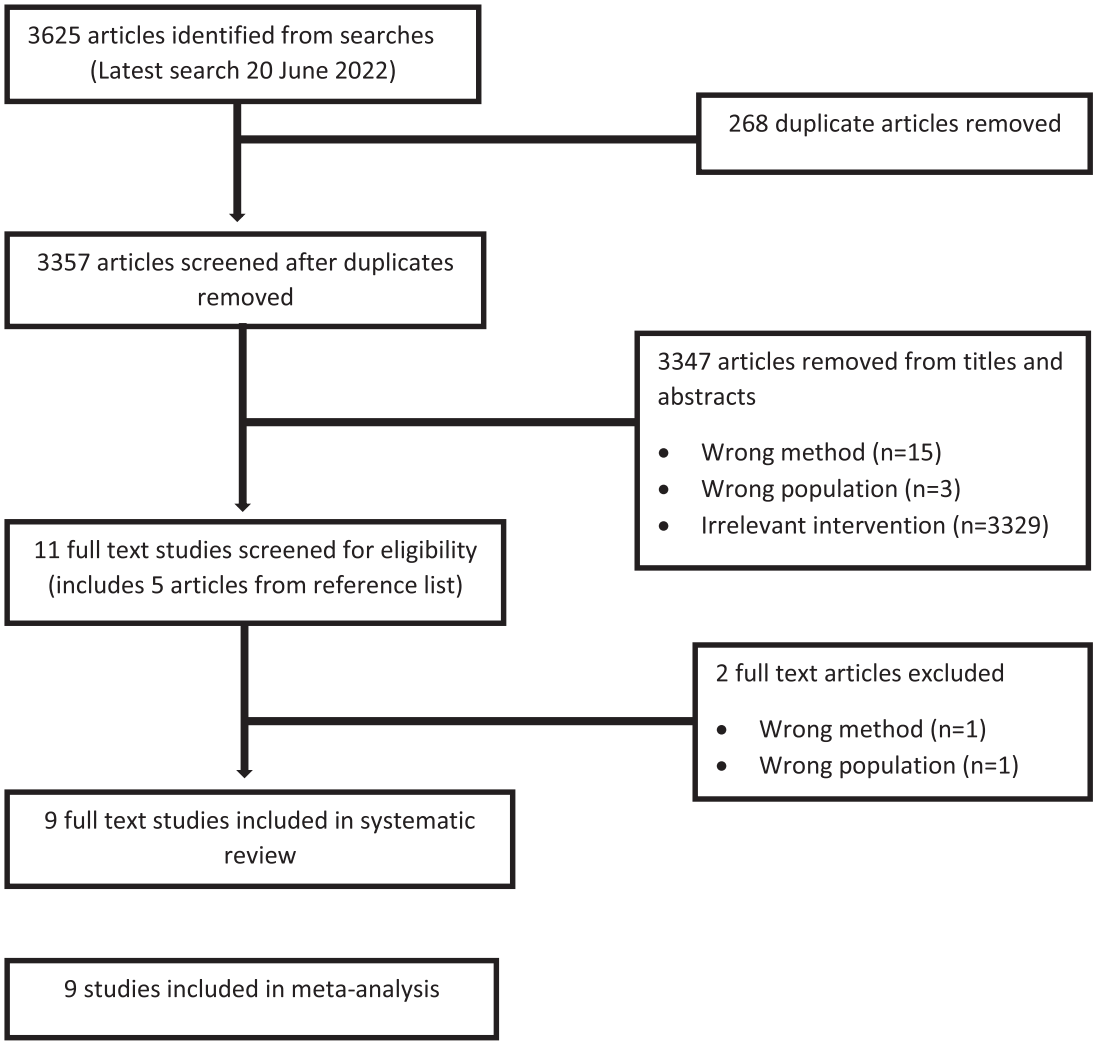

Fig. 1. Study flow diagram.

ter results. Where possible, we also performed meta-analysis on data from non-randomized trials using Cochrane Systematic Review methodology (Higgins and Green, 2008). For our summary of findings table, we used Grade Pro-software (GRADEpro Guideline Development Tool [Software] 2022). A summary of the intervention effect and a measure of quality according to the GRADE approach is presented in the 'Summary of findings' table for each of the above outcomes (Table 2).

We contacted four study authors (Ickovics et al., 2016; Koniak-Griffin et al., 1999; Nguyen et al., 2003; Panaretto et al., 2005) for further study details and received responses from two (Ickovics et al., 2016; Koniak-Griffin et al., 1999).

We described heterogeneity between study results and reported statistical significance ($p<0{\cdot}1$) using the I^2 statistic. If there was substantial heterogeneity, we had planned to explore possible causes using pre-specified subgroup analysis (Higgins and Green, 2008). We also planned to undertake a subgroup analysis on Australian First Nations women versus other First Nations women from HIC to ascertain if there were any global differences between these women.

Risk of bias

Of the RCTs, Hans et al. (2018) and Ickovics et al. (2016) were assessed at low risk of selection bias. Hans et al. (2018) used sealed opaque envelopes and stratified by community. Ickovics et al. (2016) reported that randomisation was performed via computer-generated sequence in stratified blocks. All but two studies (Koniak-Griffin et al., 1999; Nguyen et al., 2003) were assessed as low risk of bias for allocation concealment. Four studies (Hans et al., 2018; Klerman et al., 2001; Marsiglia et al., 2010; Stevens-Simon et al., 1994) described that randomisation occurred after explanation of the study and consent was complete. Ickovics et al. (2016) identified health centres prior to sites being randomised. Nguyen et al. (2003) and Koniak-Griffin et al. (1999) both described randomisation methods but fail to report if this occurred prior to or following consent, thus, were assessed as being at unclear risk of bias (Fig. 2).

Ickovics et al. (2016) was assessed at low risk of outcome bias as sites were identified prior to health centres being randomised. Participants and study personnel of the other RCTs were aware of group allocations throughout the study (Hans et al., 2018; Klerman et al., 2001; Koniak-Griffin et al., 1999; Marsiglia et al., 2010; Nguyen et al., 2003; Stevens-Simon et al., 1994). However, due to the study design it would be unreasonable to expect blinding of participants and thus were assessed as unclear risk. None of the studies, however, described whether outcome assessors were blinded and were classified as an unclear risk of bias (Hans et al., 2018; Ickovics et al., 2016; Klerman et al., 2001; Koniak-Griffin et al., 1999; Marsiglia et al., 2010;

Table 2
Summary of Findings.

Summary of findings:					
Interventions to increase utilisation of maternal health services (up to 6 weeks postpartum) compared to usual care for women living with vulnerabilities in high income countries					
Patient or population: women living with vulnerabilities in high income countries Setting: Any Intervention: Enhanced care Comparison: Usual care					
Outcomes	Anticipated absolute effects* (95% CI)		Relative effect (95% CI)	№ of participants (studies)	Certainty of the evidence (GRADE)
	Risk with usual care	Risk with interventions to increase utilisation of maternal health services (up to 6 weeks postpartum)			
Increased attendance at antenatal classes	148 per 1000	**725 per 1000** (650 to 789)	**OR 15.23** (10.73 to 21.61)	873 (2 RCTs)	⊕⊕⊕⊕ HIGH
Increased attendance at postnatal visit (6–8 weeks)	447 per 1000	**683 per 1000** (611 to 747)	**OR 2.66** (1.94 to 3.64)	680 (2 RCTs)	⊕⊕⊕⊕ HIGH
Perinatal death	4 per 1000	**2 per 1000** (0 to 11)	**OR 0.43** (0.06 to 2.98)	1708 (1 RCT, 1 non-RCT)	⊕⊕⊕⊕ HIGH
Low birth weight (<2500 gm)	108 per 1000	**87 per 1000** (72 to 104)	**OR 0.78** (0.64 to 0.95)	4571 (4 RCTs, 2 non-RCTs)	⊕⊕⊕⊕ HIGH
Preterm birth (<37 weeks)	118 per 1000	**83 per 1000** (69 to 99)	**OR 0.68** (0.56 to 0.82)	4702 (5 RCTs, 2 non-RCTs)	⊕⊕⊕⊕ HIGH
APGAR score <7 at 5minutes	14 per 1000	**20 per 1000** (10 to 40)	**OR 1.50** (0.75 to 2.99)	2029 (1 RCT, 1 non-RCT)	⊕⊕⊕⊕ HIGH
Admission to neonatal intensive care unit (NICU)	157 per 1000	**129 per 1000** (109 to 151)	**OR 0.80** (0.66 to 0.96)	3463 (3 RCTs, 1 non-RCT)	⊕⊕⊕⊕ HIGH

*__The risk in the intervention group__ (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI).
CI: Confidence interval; **OR:** Odds ratio GRADE Working Group grades of evidence.
High certainty: We are very confident that the true effect lies close to that of the estimate of the effectModerate certainty: We are moderately confident in the effect estimate: The true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different.
Low certainty: Our confidence in the effect estimate is limited: The true effect may be substantially different from the estimate of the effect.
Very low certainty: We have very little confidence in the effect estimate: The true effect is likely to be substantially different from the estimate of effect.

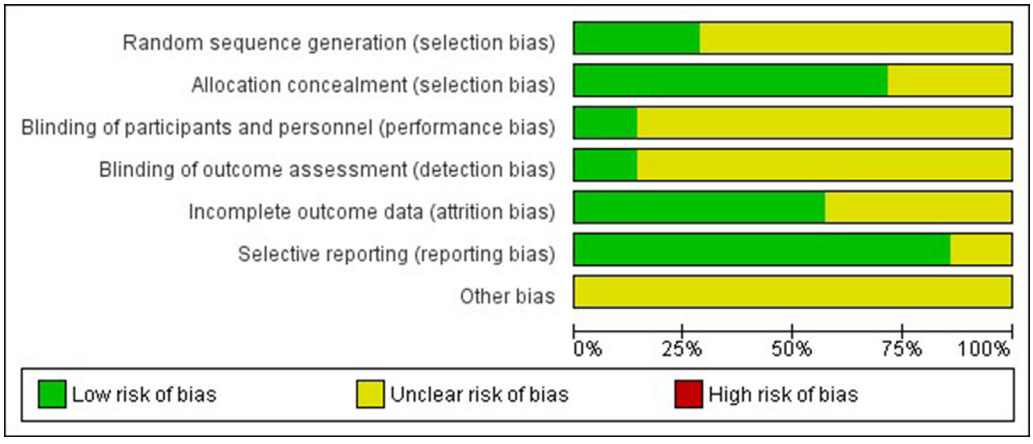

Fig. 2. Risk of bias summary of included studies (RCTs).

Nguyen et al., 2003). The primary outcome in the Stevens-Simon et al. (1994) study was however measured by attendance of a postpartum visit, therefore was assessed as low risk (Fig. 2).

Four studies (Hans et al., 2018; Ickovics et al., 2016; Klerman et al., 2001; Koniak-Griffin et al., 1999) were assessed as low risk of attrition bias. While Klerman et al. (2001) only reported 34% of women for their primary outcome, 92·5% contributed to birth weight, preterm birth, and neonatal intensive care admissions, which were outcomes relevant to this review. Ickovics et al. (2016) adequately described an intention-to-treat and per-protocol analyses. Hans et al. (2018) reported loss to follow up, including 82%, 91% and 89% of participant data at the three pre-specified time periods, as planned. Koniak-Griffin et al. (1999) adequately described reasons for excluded data. Three studies (Marsiglia et al., 2010; Nguyen et al., 2003; Stevens-Simon et al., 1994) were assessed as an unclear risk of bias. Marsiglia et al. (2010) included 85% of women in the analysis but did not provide loss to follow-up data. Although Nguyen et al. (2003) reported loss to follow up, attrition was high with <68% of outcome data available. Stevens-Simon et al. (1994), reported that 73% of participants returned for a postpartum visit but did not describe whether attempts were made to follow up

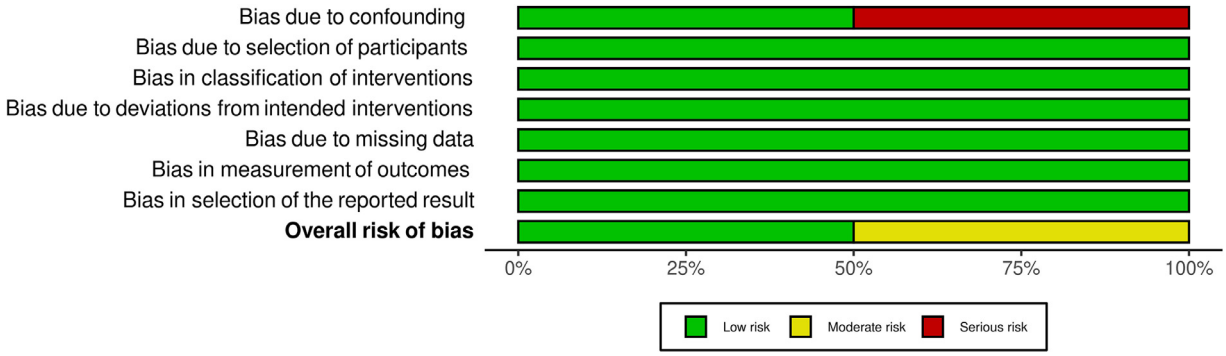

Fig. 3. Risk of bias summary of included studies (ROBINS-i).

participants if they attended a postpartum visit at another facility (Fig. 2).

The primary objective of Nguyen et al. (2003) was "to improve pregnancy outcomes in first-time Hispanic adolescent mothers and their infants". The authors did not describe what the outcomes of interest were, therefore, were assessed as unclear risk of reporting bias. All other studies were assessed as low risk with authors reported on all pre-specified outcomes (Hans et al., 2018; Ickovics et al., 2016; Klerman et al., 2001; Koniak-Griffin et al., 1999; Marsiglia et al., 2010; Stevens-Simon et al., 1994) (Fig. 2).

For the non-randomised studies, Kildea et al. (2021) was assessed as low risk of bias for all domains (Fig. 3). An inverse probability of treatment weighting was used for control of confounding, with characteristics of women in both groups differing only in model of care. There was minimal missing data, and while it was not clear if outcome assessors were blinded, the outcomes relevant to this systematic review are unlikely to be influenced by knowledge of allocation.

Panaretto et al. (2005) was assessed as serious risk for bias as there was inadequate description of the confounding variables. All other domains were however assessed as low risk, giving an overall rating of moderate, meaning that it provided sound evidence for a non-randomised study but cannot be considered comparable to a well-performed randomised trial (Fig. 3).

Further descriptions of the risk of bias for each study are included in Supplementary File, Table 3.

Results

The database searches identified 3625 studies, with nine studies ultimately meeting the inclusion criteria (Fig. 1). These nine studies are described below. (See Supplementary File, Table 2 for description of excluded studies)

Nine studies (Hans et al., 2018; Ickovics et al., 2016; Kildea et al., 2021; Klerman et al., 2001; Koniak-Griffin et al., 1999; Marsiglia et al., 2010; Nguyen et al., 2003; Panaretto et al., 2005; Stevens-Simon et al., 1994) involving 5729 women aged 12–45, met our inclusion criteria (see Supplementary File, Table 3). Seven studies were RCTs (Hans et al., 2018; Ickovics et al., 2016; Klerman et al., 2001; Koniak-Griffin et al., 1999; Marsiglia et al., 2010; Nguyen et al., 2003; Stevens-Simon et al., 1994) conducted in the US, between 1994 and 2015; one was a multicentre cluster RCT (Ickovics et al., 2016). Five studies (Hans et al., 2018; Ickovics et al., 2016; Koniak-Griffin et al., 1999; Nguyen et al., 2003; Stevens-Simon et al., 1994) involved adolescents (12–20 years), three of which focused on one-on-one care during pregnancy, labour and the postpartum period (Hans et al., 2018; Koniak-Griffin et al., 1999; Nguyen et al., 2003), one on provision of group antenatal care (Ickovics et al., 2016), and one used a gift incentive to promote attendance at the 12-week postpartum visit, with a subgroup analysis conducted at 6–8-weeks (Stevens-Simon et al., 1994). One trial included Latina/Hispanic women (Marsiglia et al., 2010) focusing on clarifying health messages using a 'Prenatal Partner', and another in African-American women (Klerman et al., 2001), providing more frequent and intensive antenatal appointments. The two remaining studies (Kildea et al., 2021; Panaretto et al., 2005) were conducted in Australia. One was a non-randomised intervention trial focusing on Australian First Nations women (Kildea et al., 2021) and the other a prospective cohort study, also focusing on Australian First Nations women, with both contemporary and historical control groups (Panaretto et al., 2005).

Utilisation of antenatal care

Overall, three studies (one RCT and two non-randomised studies) reported data on our primary outcome. The RCT (Klerman et al., 2001) reported that the intervention group had a significantly higher mean number of antenatal visits (13·7 [SD 3·8]) compared to the control group (11·9 [SD 3·8]), $p = 0·001$). Of the two non-randomised studies, Panaretto et al. (2005) reported a higher median number of antenatal visits (7[IQR 4–10]) compared to the historical control group (3[IQR 2–6]), whereas Kildea et al. (2021) reported that women in the intervention group were more likely to attend five or more times during the pregnancy compared to the control group (OR=1·49, 95%CI 1·06–2·09, $p = 0·021$).

Attendance at antenatal classes

Data combined from two RCTs (Hans et al., 2018; Klerman et al., 2001), ($n = 873$) showed that those receiving enhanced care were significantly more likely to attend antenatal classes (Odds Ratio [OR]=15·23, 95% Confidence interval [CI] 10·73, 21·61, $p<0·0001$) compared to those receiving routine care (Fig. 4). These results however should be interpreted with caution as there is significant heterogeneity between the two studies.

Attendance at 6–8 week postnatal visit

Two RCTs (Marsiglia et al., 2010; Stevens-Simon et al., 1994) reported on rates of attendance at the 6–8-week postnatal visit. While interventions were vastly different, we combined data from these studies ($n = 680$), which showed those receiving enhanced care were significantly more likely to attend the 6–8-week postnatal visit (OR=2·66, 95%CI 1·94 to 3·64, $p<0·0001$) compared to those receiving routine care (Fig. 5).

Utilisation of intrapartum care

For our primary objective, no studies reported on utilisation of intrapartum care.

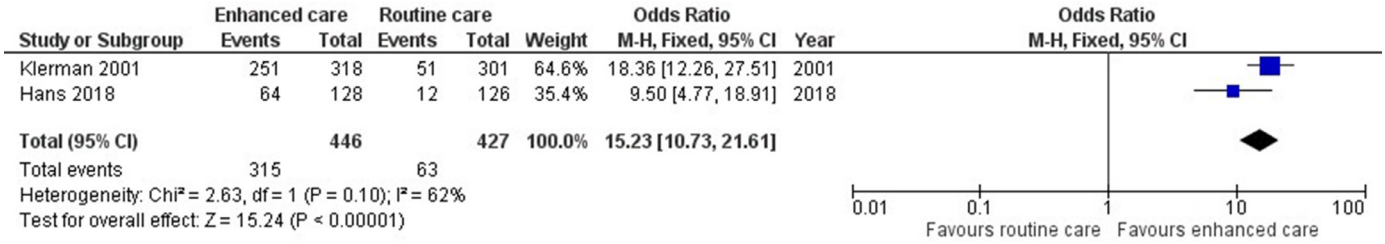

Fig. 4. Forest plot of maternal outcome: attendance at antenatal classes.

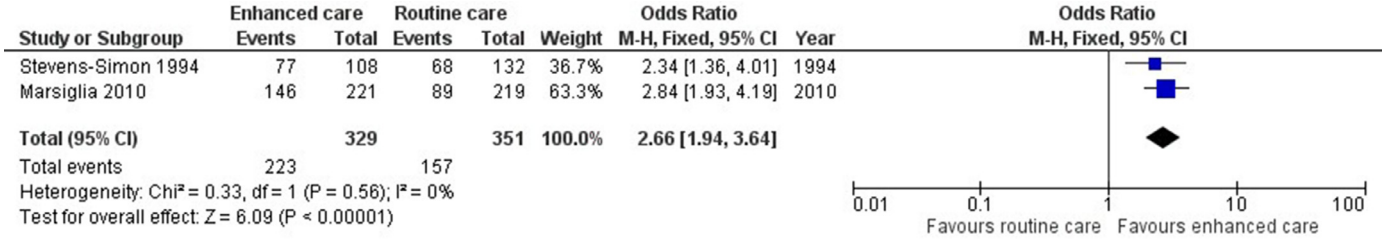

Fig. 5. Forest plot of maternal outcome: attendance at 6–8-week postnatal visit.

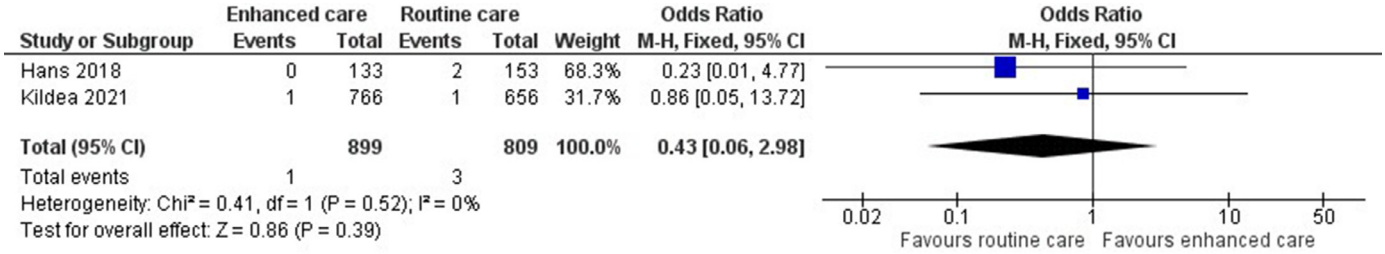

Fig. 6. Forest plot of infant outcome: perinatal death.

Perinatal death

Two studies', one RCT (Hans et al., 2018), and one non-randomised trial (Kildea et al., 2021), reported on perinatal mortality. When combined, no significant difference between intervention and control groups (OR=0·43, 95%CI 0·06–2·98, $p = 0·39$) were found (Fig. 6).

Low birthweight

Combined data from four RCTs (Hans et al., 2018; Ickovics et al., 2016; Klerman et al., 2001; Nguyen et al., 2003) in 2196 neonates did not find any statistically significant differences in the number of neonates with a low birthweight between intervention and control groups (OR=0·90, 95%CI 0·67, 1·19, $p = 0·46$). Combined data from the non-randomised trials (Kildea et al., 2021; Panaretto et al., 2005) in 2375 neonates did show a significant difference (OR=0·69, 95%CI 0·52, 0·91, $p = 0·008$). When all these data were combined in the meta-analysis, neonates ($n = 4571$) in the intervention group continued to be less likely to weigh <2500 gm at birth compared to those in the control group (OR=0·78, 95%CI 0·64–0·95, $p = 0·01$) (Fig. 7).

Preterm birth

Data from five RCTs (Hans et al., 2018; Ickovics et al., 2016; Klerman et al., 2001; Koniak-Griffin et al., 1999; Nguyen et al., 2003) in 2317 neonates did not show a significant difference in the number of preterm births (OR=0·84, 95%CI 0·64–1·11, $p = 0·20$) between groups. However, as above, when data from two non-randomised trials (Kildea et al., 2021; Panaretto et al., 2005) were combined, a statistically significant difference was seen (OR=0·55, 95%CI 0·42–0·72, $p<0·0001$), and the difference remained significant in 4702 neonates when data from all seven studies (Hans et al., 2018; Ickovics et al., 2016; Kildea et al., 2021; Klerman et al., 2001; Koniak-Griffin et al., 1999; Nguyen et al., 2003; Panaretto et al., 2005) were combined, with the likelihood of preterm births being lower in the intervention group (OR=0·68, 95%CI 0·56 to 0·82, $p<0·0001$) (Fig. 8).

APGAR scores

Four studies (two non-randomised and two RCTs) in 2029 neonates reported APGAR scores. Data could not be combined for all four studies due to methodological differences. Data from Kildea et al. (2021) and Klerman et al. (2001) were combined, however no significant difference between the intervention and control groups for APGAR score less than 7 at 5 min were found (OR=1·5, 95%CI 0·75–2·92, $p = 0·25$) (Fig. 9).

Admission to neonatal intensive care

Combined data from three RCTs (Hans et al., 2018; Ickovics et al., 2016; Klerman et al., 2001) for 2041 neonates did not show a significant difference between groups (OR=0·88, 95%CI 0·69, 1·13, $p = 0·33$) for admission to neonatal intensive care. When including a fourth, non-randomised trial (Kildea et al., 2021) a significant difference between groups for 3463 neonates (OR=0·80, 95%CI 0·51–0·92, $p = 0·02$) was observed with neonates in the intervention group less likely to be admitted (Fig. 10).

Maternal mortality, severe maternal morbidity, treatment of neonates with antibiotics and infant respiratory events

For our secondary outcomes, no studies reported maternal mortality or severe maternal morbidity, treatment of neonates with antibiotics, or infant respiratory events.

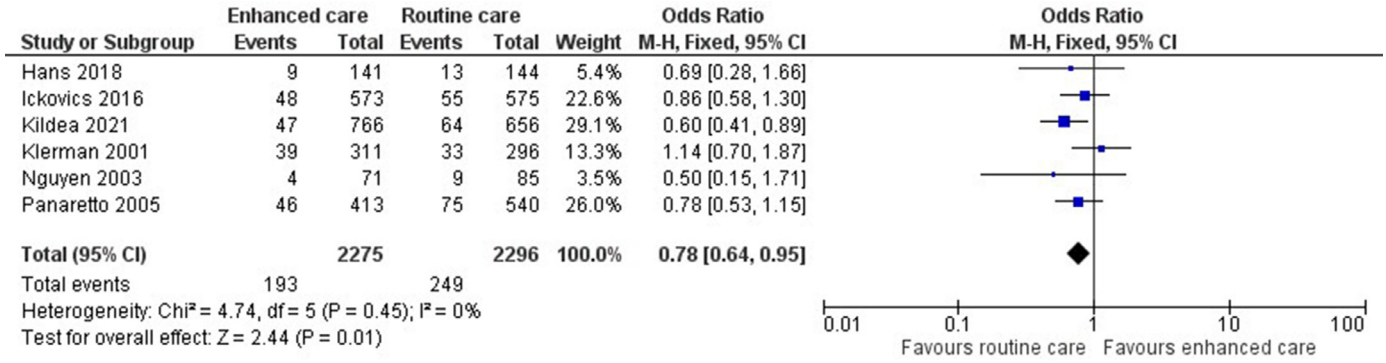

Fig. 7. Forest plot of infant outcome: low birth weight.

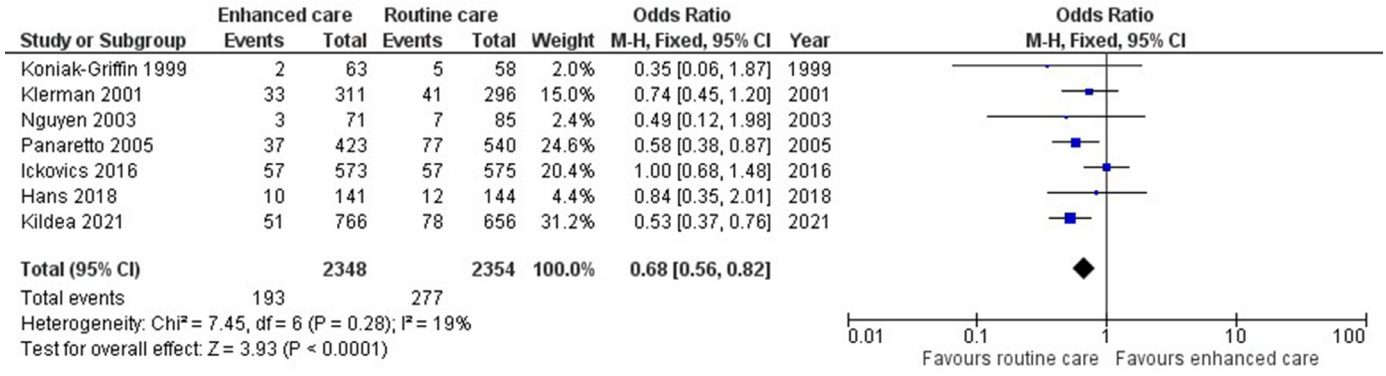

Fig. 8. Forest plot of infant outcome: preterm birth.

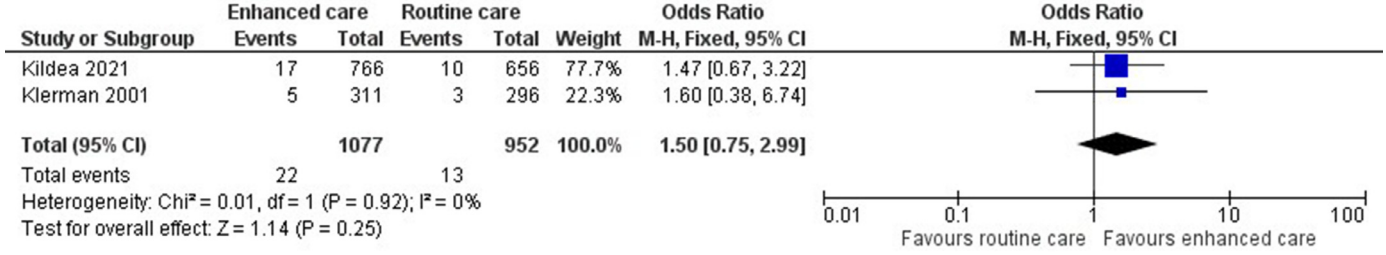

Fig. 9. Forest plot of infant outcome: APGAR score less than 7 at 5 min.

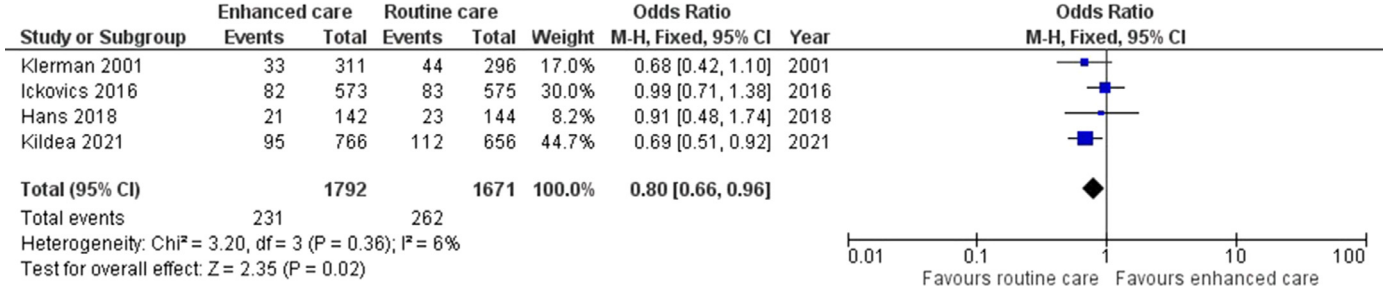

Fig. 10. Forest plot of infant outcome: admission to neonatal intensive care unit.

We did not undertake any of our planned subgroup analysis as there were no appropriate data were available.

Discussion

Nine studies with 5729 participants were included in our study. Importantly, interventions to enhance care significantly increased utilisation of health services, attendance at antenatal classes (Odds Ratio [OR]=15·23, 95%Confidence Interval [CI] 10·73–21·61, $p<0·0001$) and postnatal visits by 6–8 weeks (OR=2·66, 95%CI 1·94–3·64, $p<0·0001$), compared to routine care. Infants in the intervention groups were significantly less likely to be: born preterm (OR=0·68, 95%CI 0·56–0·82, $p<0·0001$); low birthweight (OR=0·78, 95%CI 0·64–0·95, $p = 0·01$) or; require neonatal intensive care (OR=0·80, 95%CI 0·66–0·96, $p = 0·02$).

While limited by the small number of eligible studies and participants, our review demonstrates that enhanced care positively impacts on attendance at the antenatal clinic and antenatal classes.

These data are important as engagement with health services during the antenatal period provides opportunities for detection of complications, focussed health education, emergency preparedness and identification of the onset of labour, thus reducing the risk of perinatal and maternal complications, including severe maternal morbidity and mortality. Antenatal classes also provide greater scope for partners to attend, empowering them to be positive supports during labour, birth and lactation (Barimani et al., 2018). Furthermore, enhanced care also positively impacted attendance at the 6–8-week postnatal visit. This postnatal visit is important as it provides an opportunity for health care providers and the woman to discuss her mental wellbeing (WHO, 2015), including debriefing after the birth, and promoting and encouraging breastfeeding (WHO, 2015), all of which can be a source of trauma for some women, contributing to postnatal depression (Langan and Goodbred, 2016). Between 2009–18, the third most common cause of maternal death in Australia was suicide (DoH, 2020). While not all of these can be directly linked to the development of postnatal depression, it highlights the importance of supporting women's mental health during these critical periods. Furthermore, the 6–8-week postnatal visit provides an opportunity for health care professionals and women to discuss issues such as vaccination of the newborn and contraceptive options (WHO, 2015) as appropriate. The prevention of rapid repeat pregnancies in adolescents is particularly important as babies born to adolescent mothers are more likely to be preterm or have a low birth weight (Nguyen et al., 2003), and adolescent mothers are at greater risk of maternal death (DoH, 2020).

In addition, the data showed fewer low birthweight babies, preterm births and admissions to neonatal intensive care in the enhanced care group, indicating that strategies which address this important public health concern are currently known. Children born with a low birthweight and/or preterm are at increased risk of a myriad of health problems in childhood (e.g. bronchiolitis, hospitalisation) and as adults (e.g. hypertension, diabetes, heart disease, asthma (Johnson and Schoeni, 2011) and chronic respiratory dysfunction (Yang et al., 2020)). These lead to increased health care costs and shorter life expectancies. For example, in Australia, preterm birth and low birth weight is highest amongst Australian First Nations neonates (13% and 13% respectively), compared to 8·6% and 6·6% respectively for all other Australian babies (AIHW, 2021). In addition, Australian First Nations children have high rates of hospitalisation for bronchiolitis, and pneumonia in infancy (Chang et al., 2011; Niu et al., 2020) and the highest reported burden of bronchiectasis in children under 15 globally (McCallum et al., 2020). All these conditions are associated with, amongst other things, low birthweight and preterm birth (Niu et al., 2020), with bronchiectasis accounting for frequent hospitalisations and premature death in the third and fourth decades (Goyal et al., 2019). These data add to previous studies, which have shown that enhanced models of care using midwifery-led continuity-of-carer can significantly decrease the incidence of preterm birth and low birthweight (Mortensen et al., 2019).

Admission to neonatal intensive care often results in separation of mother and baby during a crucial period of bonding and lactogenesis and may inhibit initiation and duration of breastfeeding (Wight, 2015). The benefits of breastfeeding to the neonate are well known and includes improved cognitive development, and a reduction in development of chronic diseases later in life such as diabetes, obesity, hypertension, cardiovascular disease and some types of cancer (Binns et al., 2016). Interventions which reduce the need for neonates to be admitted to neonatal intensive care could improve the short- and long-term health of neonates. This is in addition to the many benefits of breastfeeding to women. These include, but are not limited to, lactational amenorrhoea, faster return to pre-pregnancy weight, reduced risk of breast, ovarian and endometrial cancers, a lower risk of developing osteoporosis, diabetes and even Alzheimer's disease (Del Ciampo and Del Ciampo, 2018).

There are several limitations for this review. Firstly, our systemic review only included a small number of studies likely contributing to a type 1 error. Secondly, most of studies were USA-based, limiting the generalisability of the results. Thirdly, only two studies reported on First Nations women, with none focusing on women with disabilities. Unsurprisingly, we found no eligible studies that examined maternal mortality or severe maternal morbidity. In high-resource settings, maternal mortality is extremely low, and many studies would have trouble powering one large enough to include this, or severe maternal morbidity, as a primary outcome. Further, we found no studies that reported specifically on infant respiratory events.

In Summary, enhanced care increases utilisation of antenatal clinic visits, antenatal classes, and the 6–8-week postnatal visit. Further, the reduction in low birthweight, preterm birth, and admission to neonatal intensive care have potential to improve short- and long-term health outcomes for children and adults, driving down health care costs, and improving health and wellbeing for families. Current evidence demonstrates that enhanced maternal care improves utilisation of maternal health services and birth outcomes for neonates. These data provide much needed evidence to drive policy makers, and educators towards a more woman-centred, sustainable and safe approach to pregnancy care and birthing for the wellbeing of women living with vulnerabilities and neonates from HICs.

Ethics approval and consent to participate

Not applicable

Consent for publication

Not applicable.

Data availability

All data analysed during this current study were available in the public domain in the form of previously published randomised controlled trials. Thus, data sharing is not applicable to this article as no datasets were generated or analysed during the current study.

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Authors contribution

ERB conceptualised the study with GBM and ABC conceptualising the review. ERB and GBM independently undertook the search strategy, selected studies, extracted data and analysed the data. ERB wrote the first draft of the manuscript with substantial revisions by GBM and ABC. All authors approved the manuscript.

Declaration of Competing Interest

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Not applicable.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.midw.2023.103674.

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