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Do community-based midwifery and related community programmes reduce maternal and newborn deaths and illness in underserved settings? A systematic review

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Abstract

Background. Every year hundreds of thousands of women and more than two million newborn babies die from causes that skilled care could prevent. Most of these deaths occur in poor, rural and hard-to-reach communities in low- and middle-income countries (LMICs). Community-based midwifery models and similar programmes bring trained care closer to women, but it is not clear how much they reduce deaths and illness.

Objectives. To assess whether community-based midwifery models and related community-level maternal and newborn programmes reduce (1) maternal mortality, (2) neonatal mortality and (3) maternal and neonatal illness (morbidity) in underserved settings.

Methods. We searched seven databases and grey-literature sources without language or date limits (PROSPERO CRD420261386288; reported according to PRISMA 2020). Two reviewers independently screened records, extracted data and assessed risk of bias (Cochrane RoB 2 for trials; Newcastle-Ottawa Scale for quasi-experimental studies). Because the programmes, populations and outcome measures differed too much to combine statistically, we synthesised results narratively, following the SWiM guidance.

Results. Of 3,214 unique records, 13 studies met the criteria: six cluster-randomised trials (Nepal, India, Bangladesh [two], Ghana, Malawi) and seven quasi-experimental studies (Pakistan, Kenya and five from Nigeria). Eight studies measured neonatal deaths; three measured maternal deaths. All six trials estimated fewer newborn deaths in the intervention areas, but the reduction was statistically convincing in only three: women's groups in Nepal (odds ratio 0.70, 95% CI 0.53 to 0.94) and India (0.68, 0.59 to 0.78), and home visits by community health workers in Sylhet, Bangladesh (risk ratio 0.66, 0.47 to 0.93). Trials in Ghana, Malawi and a second Bangladeshi trial found small reductions that could be due to chance. Maternal mortality results were inconsistent and rested on very few deaths. Evidence on illness was sparse. None of the five Nigerian studies measured mortality.

Conclusions. Community programmes that reach many families and combine women's groups or home visits with links to skilled care can save newborn lives, but the benefit is not automatic. The evidence that community-based midwifery specifically reduces deaths in Nigeria and sub-Saharan Africa is thin, and well-designed evaluations that measure deaths are urgently needed.

Keywords: *community-based midwifery; neonatal mortality; maternal mortality; community health workers; women's groups; underserved settings; low- and middle-income countries; Nigeria*

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1. Introduction

1.1 The scale of the problem

Pregnancy and childbirth should be among the safest events of a woman's life. For millions of women they are not. The World Health Organization (WHO) and partner United Nations agencies estimate that about 260,000 women died from pregnancy-related causes worldwide in 2023, a fall of roughly 40% since 2000, but progress has slowed sharply since 2016 (World Health Organization et al., 2025). About 70% of these deaths occurred in sub-Saharan Africa, and Nigeria alone accounted for about 75,000 deaths (close to 29% of the global total), with a maternal mortality ratio of about 993 deaths per 100,000 live births (World Health Organization et al., 2025). For comparison, the Sustainable Development Goal (SDG) 3.1 target is a global ratio below 70 per 100,000 by 2030. The policy significance of these figures extends beyond the aggregate burden. Maternal and neonatal survival depend on the continuity of care between households, community contacts, primary health centres and emergency referral facilities. The first 28 days of life, particularly the immediate postnatal period, are a critical opportunity for identifying illness, promoting essential newborn practices and facilitating timely treatment. Consequently, the central question is not merely whether community programmes increase contact with health services, but whether they change the timely delivery of effective care sufficiently to influence survival. Newborn babies face similar danger. Roughly 2.3 million babies die in the first 28 days of life every year, and the great majority of these deaths occur in low-income and middle-income settings (United Nations Inter-agency Group for Child Mortality Estimation, 2026). The Nigeria Demographic and Health Survey (NDHS) 2023-24 estimated a neonatal mortality rate of 41 deaths per 1,000 live births, far above the SDG 3.2 aim of 12 or fewer per 1,000 (National Population Commission & ICF, 2024). In plain terms, about one in every 24 Nigerian babies does not survive the first month.

1.2 Why distance, cost and trust matter

Most of these deaths are preventable with well-known care: antenatal check-ups, a skilled attendant at birth, clean cord care, keeping the baby warm, early breastfeeding and prompt treatment of complications such as heavy bleeding, infection, obstructed labour and high blood pressure in pregnancy. The WHO regards a "skilled birth attendant" as an accredited health professional, such as a midwife, doctor or nurse, who is trained to manage normal birth and to recognise and refer complications. Traditional birth attendants are not counted, even if they have received some training (World Health Organization, 2018). Yet in many underserved communities, skilled care is simply not within reach. In Nigeria, the shortage of

skilled birth attendants, long distances, cost, cultural preference for traditional birth attendants and mistrust of formal services have all been documented (Adegoke & van den Broek, 2009; Nyango et al., 2010; Nkwo et al., 2015; Adewemimo et al., 2014; Atuoye et al., 2017; Ntoimo et al., 2022; Fantaye et al., 2019). Skilled attendance alone is not enough: a midwife in an ill-equipped clinic with no way of transferring a patient in an emergency cannot save every life (Adegoke & van den Broek, 2009).

1.3 Community-based models and what is known about them

Community-based models try to close this gap by moving care, and the people who deliver it, closer to women. Figure 1 summarises how the main types of programme are expected to work. Some post trained midwives to rural primary health centres, as in Nigeria's Midwives Service Scheme (MSS), launched in 2009 (Abimbola et al., 2012; Okeke et al., 2017). Others rely on community health workers or volunteers who visit homes during pregnancy and in the first week after birth. A third group, the participatory women's groups, brings local women together in regular meetings to identify problems, plan solutions and act on them, with no clinical care delivered at all. These programme types should be distinguished analytically while being considered within a shared continuum-of-care framework. Community-based midwifery involves skilled clinical personnel and their relationship with primary-care and referral systems; community health-worker home visits principally extend preventive counselling, surveillance and referral into households; participatory women's groups operate through collective problem-solving, social mobilisation and improved care-seeking. Their mechanisms overlap, but they are not interchangeable. This distinction is important when translating evidence from community mobilisation trials into policies concerning midwife deployment and skilled intrapartum care.

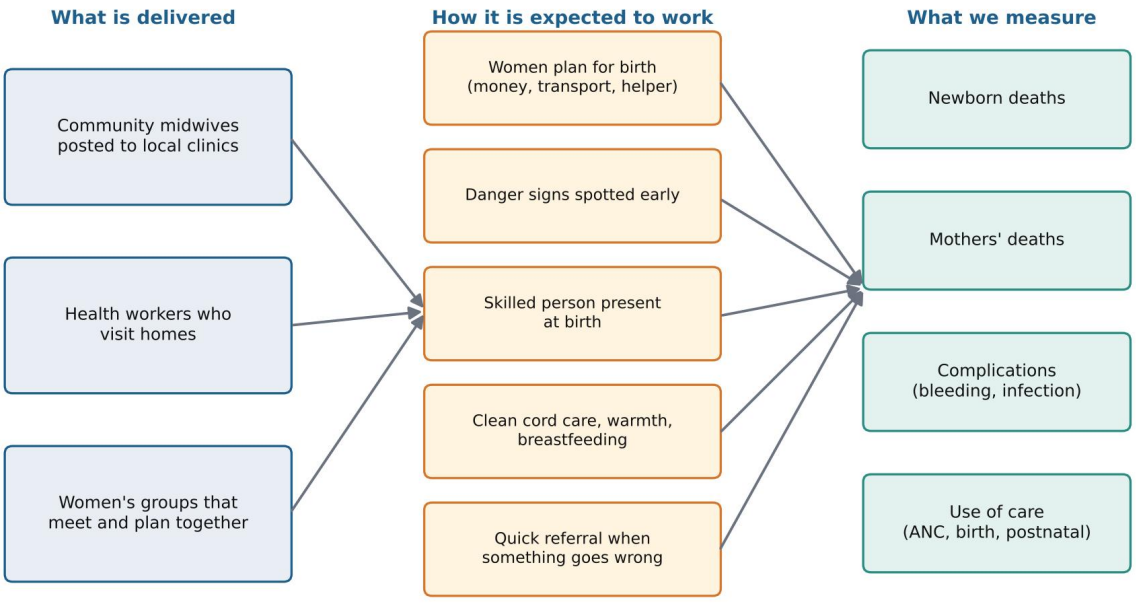


Figure 1. How community-based programmes are expected to improve maternal and newborn health. The programmes (left) work through several practical changes (centre) that, in turn, should reduce deaths and illness and increase the use of health services (right).

Earlier syntheses have mostly examined one of these approaches at a time. A meta-analysis of seven trials of women's groups found 23% lower neonatal mortality (odds ratio 0.77, 95% confidence interval 0.65 to 0.90) and lower maternal mortality (0.63, 0.32 to 0.94), with larger effects where at least 30% of pregnant women took part (Prost et al., 2013). A Cochrane review of community-based packages found 25% lower neonatal mortality (risk ratio 0.75, 0.67 to 0.83) and a possible reduction in maternal mortality (0.80, 0.64 to 1.00) (Lassi & Bhutta, 2015). A review of home-visit trials reported a 38% reduction in neonatal mortality overall, and a much larger reduction (46%) when the programme reached more than half of families (Gogia & Sachdev, 2010). Midwife-led continuity of care has been shown to reduce fetal loss and neonatal death in high-income countries (Sandall et al., 2016), and a major series in *The Lancet* set out the case for midwifery as a pillar of maternal and newborn care worldwide (Renfrew et al., 2014; ten Hoope-Bender et al., 2014).

However, we are not aware of an earlier systematic review that brought together community-based midwifery programmes, health-worker home-visit programmes and women's groups in underserved settings, with particular attention to Nigeria and the wider evidence on skilled midwifery delivery. This matters because Nigerian policy-makers must decide how to invest limited funds, and because the Nigerian studies use designs that are very different from the large trials in South Asia.

1.4 Objectives

This article is the first of two papers from a single registered review. It addresses the review's primary and morbidity questions: whether community-based midwifery models and related community-level programmes, compared with usual care, reduce (1) maternal mortality, (2) neonatal mortality and (3) maternal and neonatal illness in underserved settings. A companion article reports the effects on the use of health services (antenatal care, skilled birth attendance, facility delivery and postnatal care) and their implications for Nigerian health policy.

2. Methods

2.1 Protocol and reporting

The review was registered in advance with PROSPERO (CRD420261386288, 04 May 2026) and no changes were made to the protocol after registration. We followed the PRISMA 2020 reporting guideline (Page et al., 2021). Because a statistical pooling of results was not appropriate (Section 2.7), we reported the narrative synthesis following the Synthesis Without Meta-analysis (SWiM) guideline (Campbell et al., 2020).

2.2 Who and what we looked for

We used the "PICO" framework, which is simply a checklist of Population, Intervention, Comparator and Outcome.

- **Population.** Pregnant women, women in labour, postpartum women and newborns up to 28 days old, living in underserved, rural, peri-urban or hard-to-reach settings in LMICs.
- **Intervention.** Community-based midwifery models, or similar community-level maternal and newborn programmes delivered by midwives or community health workers: home-based care, outreach, midwife-staffed primary health centres, and women's group programmes with health education, support for skilled attendance, postnatal care and referral.
- **Comparator.** Usual care, or facility-based or physician-led care without a structured community-based midwifery component.
- **Outcomes.** *Primary*: maternal mortality and neonatal mortality. *Secondary*: maternal illness (haemorrhage, infection, eclampsia), newborn illness (birth asphyxia, infection, low birth weight) and use of services. This article reports the mortality and illness outcomes.

- **Study designs.** Randomised trials, quasi-experimental studies (including controlled before-and-after studies, interrupted time series and difference-in-differences designs), cohort and case-control studies. We excluded case reports, case series, editorials, conference abstracts without data and purely qualitative studies.

2.3 Searching

We searched MEDLINE, PubMed, Embase, CINAHL, the Cochrane Central Register of Controlled Trials, Scopus and Google Scholar from inception, with no language or date limits. Searches combined subject headings and free-text terms on community midwifery, the Midwives Service Scheme, skilled birth attendance, community health workers, maternal and neonatal mortality and morbidity, antenatal and postnatal care, rural and underserved populations, and the regions of interest (Nigeria, sub-Saharan Africa, South Asia). We also searched ClinicalTrials.gov, OpenGrey, thesis repositories, conference proceedings and institutional reports, checked reference lists, tracked citations forwards in Google Scholar and Scopus, and contacted authors for missing data.

2.4 Selecting studies and collecting data

Two reviewers independently screened titles and abstracts, then full texts, resolving disagreements by discussion or a third reviewer. Two reviewers independently extracted data using a piloted form (author, year, country, design, sample, intervention, comparator, follow-up, outcomes, effect estimates with 95% confidence intervals, and funding). To protect against transcription error, key figures in this article were rechecked against the original publications.

2.5 Risk of bias

Two reviewers independently judged the risk of bias in the cluster-randomised trials with the Cochrane Risk of Bias 2 tool (Sterne et al., 2019), which examines the randomisation process, deviations from the planned intervention, missing outcome data, outcome measurement and selective reporting. The quasi-experimental studies were assessed with the Newcastle-Ottawa Scale (Wells et al., n.d.), which scores selection of groups, comparability and outcome ascertainment out of nine stars.

2.6 Certainty of the evidence

For each outcome we judged how confident we can be in the findings using the GRADE approach, which starts from "high" certainty for randomised trials and "low" for non-randomised studies, and rates down for risk of bias, inconsistency, indirectness, imprecision and publication bias.

2.7 Synthesis

The studies were too different to combine statistically: the interventions ranged from midwife deployment to women's group meetings, the settings from rural Nepal to northern Nigeria, and the outcomes were defined and measured in different ways. We therefore grouped studies by outcome and by type of intervention, tabulated their effect estimates as originally reported, and compared the direction, size and precision of the results. We did not calculate a pooled estimate. The "forest-style" figures in this article display each study's own estimate side by side for easy comparison, not a combined result.

To strengthen the interpretation of heterogeneous evidence, the narrative synthesis considered five dimensions together: intervention mechanism, implementation coverage, baseline service access, follow-up duration and the precision of the reported effect estimate. Randomised and non-randomised findings were considered in separate evidential categories before being brought together in the discussion. The direction of effect was interpreted alongside confidence intervals and study context rather than being treated as sufficient evidence of effectiveness by itself. Because the figures display both odds ratios and risk ratios as originally reported, visual comparisons are descriptive; these measures are not numerically identical and should not be interpreted as a common pooled effect.

2.8 Patient and public involvement

None. This review used only published and publicly available data and did not require ethical approval.

3. Results

3.1 What the search found

The database searches identified 4,817 records and a further 63 came from other sources, giving 4,880 records. After 1,666 duplicates were removed, 3,214 records were screened by title and abstract and 3,079 were excluded. We read 135 full texts and excluded 122, mainly because there was no community component (37), no comparison group (28) or the study was in a high-income setting (21). Thirteen studies were included (Figure 2).

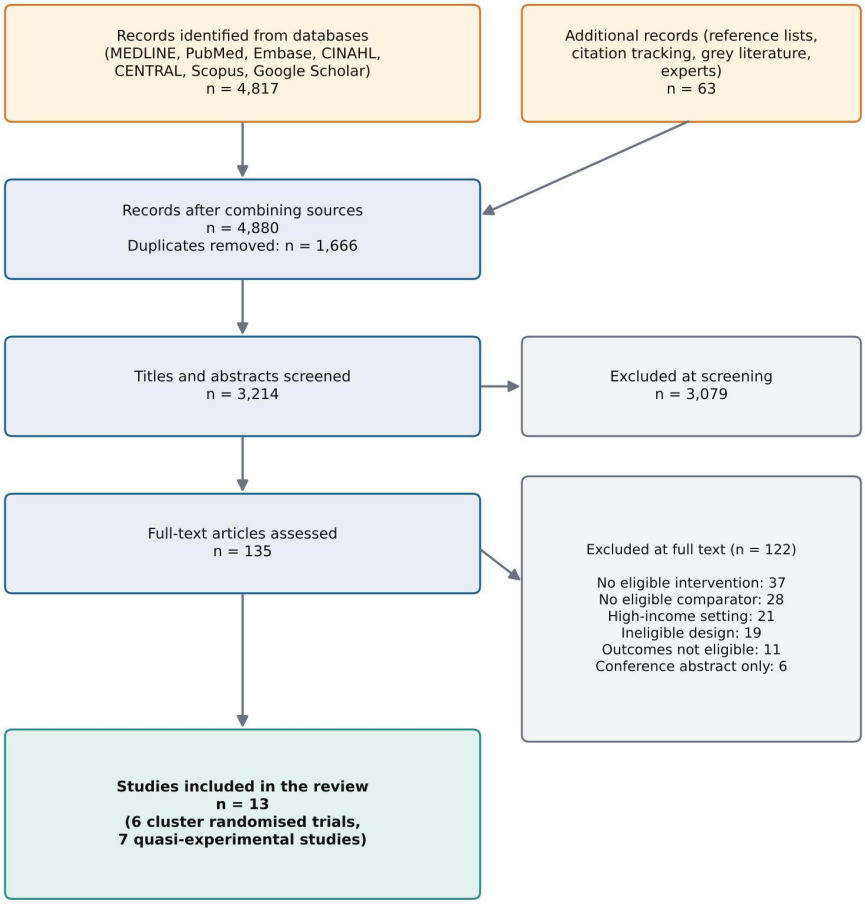


Figure 2. PRISMA 2020 flow diagram showing how 4,880 records were narrowed to 13 included studies.

3.2 The 13 studies

The studies were published between 2004 and 2023 and took place in eight countries: Nigeria (five studies), Bangladesh (two), and one each in India, Nepal, Pakistan, Ghana, Malawi and Kenya. Six were cluster-randomised trials, in which whole communities rather than individual women are randomly assigned, a design that is well suited to community programmes. The other seven were quasi-experimental: communities or clinics were compared before and after a programme, or against similar areas, without random assignment. Table 1 describes each study. The distribution of studies also clarifies the contribution of the review to policy. The six cluster-randomised trials offer direct comparisons of mortality outcomes under community-level intervention conditions, while the seven quasi-experimental evaluations add evidence on programme delivery in routine health systems. The five Nigerian evaluations are especially valuable for examining implementation arrangements, including workforce deployment, facility strengthening, financing, referral and community engagement.

Read together, the two bodies of evidence illuminate both the potential for mortality reduction and the organisational conditions through which community-based approaches may become effective.

The programmes fall into three broad types. *Women's group programmes* (Nepal, India, Bangladesh, Malawi) brought women together in facilitated meetings to work through a "community action cycle": identifying local problems in pregnancy and childbirth, planning solutions, carrying them out and reviewing the results. *Home-visit programmes* (Bangladesh, Ghana, Pakistan) used community health workers or volunteers to visit mothers in pregnancy and in the first days after birth, promoting hygienic cord care, warmth, breastfeeding and early care-seeking. *Midwifery and health-system programmes* (Nigeria, Kenya) placed trained midwives in primary health centres or communities, often alongside facility upgrades or community engagement.

Table 1. Characteristics of the 13 included studies.

Study	Country and design	Participants / scale	Intervention	Comparator
Manandhar 2004	Nepal; cluster RCT (24 clusters)	28,931 women monitored; 6,125 births in mortality analysis	Facilitated women's group meetings (nine per cluster each month) using an action-learning cycle	Control clusters (usual services)
Tripathy 2010	India (Jharkhand, Orissa); cluster RCT (36 clusters)	19,030 births over 3 years	Local facilitators; 20 meetings per women's group; action cycle	Control clusters
Azad 2010	Bangladesh; cluster RCT (18 unions)	36,113 births	Women's groups (about one per 1,400 people); all unions also had health-service strengthening and TBA training	Control unions (same background strengthening)
Baqui 2008	Bangladesh (Sylhet); cluster RCT, 3 arms (24 clusters)	About 15,000 live births per arm	Home-care arm: community health worker home visits (2 antenatal; days 1, 3, 7 after birth), treatment or referral. Community-care arm: community mobilisation meetings only	Comparison clusters with no intervention
Kirkwood 2013	Ghana; cluster RCT (98 zones)	16,168 live births	Volunteers made 2 pregnancy visits and 3 visits in the first week; promoted	Control zones

Study	Country and design	Participants / scale	Intervention	Comparator
			newborn care, recognised danger signs, referred	
Lewycka 2013	Malawi; 2x2 factorial cluster RCT (48 clusters)	26,262 births	Women's groups; volunteer peer counsellor home visits; or both	Control clusters
Bhutta 2008	Pakistan (Sindh); controlled before-after pilot (4 + 4 clusters)	2,932 and 2,610 pregnancies	Extra training of lady health workers in newborn care; dai (TBA) training; community health committees	Four matched control clusters
Okeke 2016	Nigeria (12 states); difference-in-differences	7,104 women in 368 communities	Midwives Service Scheme: midwives posted to rural primary health centres	Communities that received midwives about 3 years later
Willey 2022	Nigeria (Gombe); repeat surveys, 80 clusters	About 3,800 mothers (4 surveys, 2016-2019)	Government-led partnership: midwives, facility strengthening, CHW training, community engagement	Comparison areas (not randomised)
Okonofua 2022	Nigeria (Edo); before-after, no control	1,408 + 1,411 mothers	Seven-part package: community health fund, transport agreement, SMS alerts, drug fund, health talks, advocacy, staff training	None (baseline vs endline)
Grépin 2022	Nigeria (national); difference-in-differences	21,130 births	SURE-P MCH: 1,285 midwives posted to 473 upgraded primary health centres	Comparison catchment areas
Okonofua 2023	Nigeria (referral hospitals); quasi-experimental	752 women (exit interviews)	Intrapartum quality-improvement programme	Control hospitals
Shikuku 2020	Kenya (Busia); before-after, no control	Monthly facility counts, 11 months; 10 community midwives	Retired community midwives linked to six public facilities	None (earlier vs later period)

RCT, randomised controlled trial; TBA, traditional birth attendant; CHW, community health worker; SMS, text message. Sample sizes were rechecked against the original publications; the Willey 2022 figure is derived from the numbers reported in each survey round.

Importantly, the studies differ in how well they can answer the questions in this article. Four of them (all Nigerian) did not measure deaths or clinical illness at all, and two lacked a concurrent comparison group (Table 2).

Table 2. Which studies measured which outcomes.

Study	Neonatal deaths	Maternal deaths	Stillbirth, illness or care practices	Concurrent comparison group
Manandhar 2004	Yes	Yes	Stillbirth	Yes (randomised)
Tripathy 2010	Yes	No	Maternal depression	Yes (randomised)
Azad 2010	Yes	Yes	No	Yes (randomised)
Baqui 2008	Yes	No	Care practices only	Yes (randomised)
Kirkwood 2013	Yes	No	Care practices	Yes (randomised)
Lewycka 2013	Yes	Yes	Reported illness	Yes (randomised)
Bhutta 2008	Yes	No	Stillbirth	Yes (not randomised)
Okeke 2016	No	No	Complications	Yes (not randomised)
Willey 2022	No	No	No	Yes (not randomised)
Okonofua 2022	No	No	No	No
Grépin 2022	No	No	No	Yes (not randomised)
Okonofua 2023	No	No	Care quality only	Yes (hospitals)
Shikuku 2020	Yes	No	Stillbirth	No
Total	8 of 13	3 of 13	8 of 13	11 of 13

3.3 Risk of bias

Among the six trials, four were judged at low risk of bias, one (Malawi) at low to moderate risk because of the complexity of its factorial design and uneven loss of participants, and one (Ghana) as raising "some concerns" about how outcomes were measured. Blinding of participants and health workers was impossible in all trials, which is unavoidable for this kind of intervention. The seven quasi-experimental studies were generally of moderate quality (5 to 6 stars out of 9); the main concerns were that programme sites may not have been chosen at random, that comparison areas could differ from intervention areas in ways that affect outcomes, and, in several Nigerian studies, short follow-up (Table 3).

Table 3. Risk-of-bias assessment.

Study	Design	Tool	Overall rating	Main concerns
Manandhar 2004	Cluster RCT	RoB 2	Low	Blinding of participants not possible
Baqui 2008	Cluster RCT	RoB 2	Low	Minor loss to follow-up; blinding not possible
Tripathy 2010	Cluster RCT	RoB 2	Low	Blinding not possible; minimal attrition
Azad 2010	Cluster RCT	RoB 2	Low	Blinding not possible; minimal attrition
Kirkwood 2013	Cluster RCT	RoB 2	Some concerns	Outcome measurement methods; blinding
Lewycka 2013	Cluster RCT	RoB 2	Low to moderate	Complex factorial design; uneven attrition
Bhutta 2008	Quasi-experimental	NOS	Moderate (5/9)	Small pilot; selection bias; limited generalisability
Okeke 2016	Quasi-experimental	NOS	Moderate (5/9)	Selection bias; non-random site allocation
Shikuku 2020	Quasi-experimental	NOS	Moderate (5/9)	Selection bias; before-after design
Okonofua 2022	Quasi-experimental	NOS	Moderate (6/9)	Selection of comparison facilities; confounding
Grépin 2022	Quasi-experimental	NOS	Moderate to high (6/9)	Strong design; residual confounding possible
Willey 2022	Quasi-experimental	NOS	Moderate (5/9)	Selection bias; possible Hawthorne effect
Okonofua 2023	Quasi-experimental	NOS	Moderate (5/9)	Selection of hospitals; short follow-up

RoB 2, Cochrane Risk of Bias 2; NOS, Newcastle-Ottawa Scale (maximum 9 stars). Ratings are those assigned by the two independent reviewers.

3.4 Neonatal mortality

Neonatal mortality, the risk of dying in the first 28 days of life, was measured in eight studies: all six trials, the Pakistan pilot and the Kenyan study. Figure 3 and Table 4 show the results. A ratio below 1 means fewer deaths in the intervention group; for example, 0.70 means 30% fewer deaths. The contrast between the South Asian trials is substantively informative. Interventions characterised by repeated household contact or sustained participatory engagement produced the clearest mortality improvements, while less intensive or less widely adopted approaches more often improved intermediate care behaviours without demonstrating a corresponding mortality effect during the observation period. This pattern

suggests that coverage, continuity and the capacity to respond to identified complications should be treated as core programme characteristics rather than peripheral implementation details. The absolute differences in neonatal deaths per 1,000 live births are particularly useful for decision-makers because they connect relative effects with the scale of benefit in the populations studied.

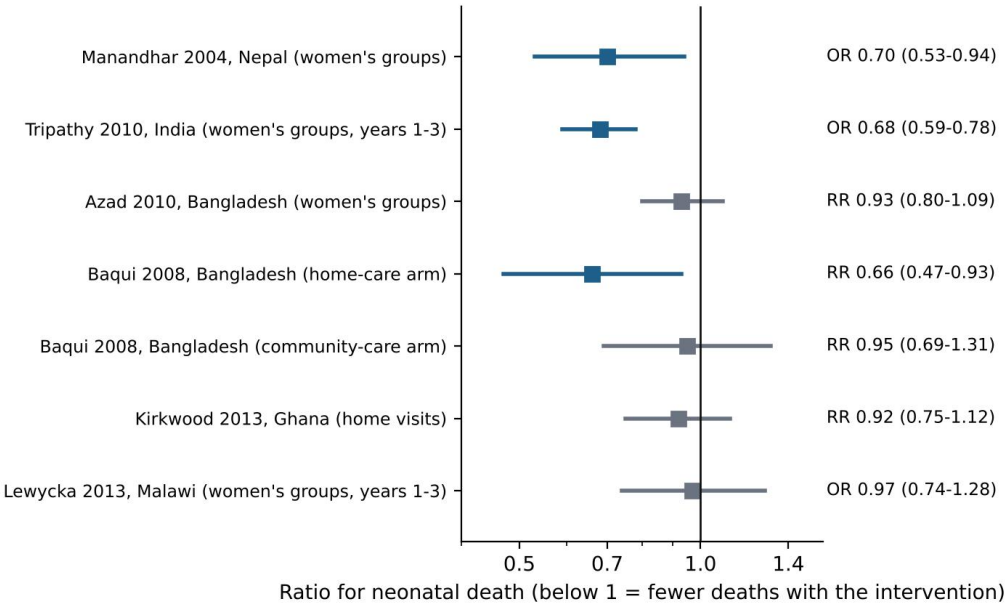


Figure 3. Neonatal mortality in the six cluster-randomised trials. Each square is the study's estimate and each line its 95% confidence interval; blue indicates a statistically convincing reduction and grey an inconclusive result. OR, odds ratio; RR, risk ratio. Estimates are shown as originally reported and are not pooled.

Where the evidence was strongest. In Nepal, intervention areas had 26.2 neonatal deaths per 1,000 live births compared with 36.9 in control areas (odds ratio 0.70; 95% CI 0.53 to 0.94) (Manandhar et al., 2004). In the Indian states of Jharkhand and Orissa, the effect grew over time as women's groups became established: overall across three years the odds of neonatal death were 32% lower (0.68; 0.59 to 0.78), and in years two and three they were 45% lower (0.55; 0.46 to 0.66), with 36.3 neonatal deaths per 1,000 in intervention areas compared with 64.3 in control areas in year three (Tripathy et al., 2010). In Sylhet, Bangladesh, the "home-care" arm, in which a community health worker made two antenatal visits and postnatal visits on days 1, 3 and 7 and treated or referred sick newborns, reduced neonatal mortality by 34% (risk ratio 0.66; 0.47 to 0.93), with 29.2 deaths per 1,000 compared with 43.5 in comparison areas (Baqui et al., 2008).

Where the evidence was inconclusive. In the same Bangladeshi study, the "community-care" arm, which relied on community meetings and messages without home visits, showed

no benefit (0.95; 0.69 to 1.31). A larger Bangladeshi trial that scaled up women's groups across three districts found only a small, inconclusive reduction (risk ratio 0.93; 0.80 to 1.09) (Azad et al., 2010). In Ghana, home visits by existing community volunteers lowered neonatal mortality from 31.9 to 29.8 per 1,000 (risk ratio 0.92; 0.75 to 1.12), a difference that could easily be chance, although the programme clearly improved newborn care practices (Kirkwood et al., 2013). In Malawi, women's groups showed no clear effect on neonatal mortality over the whole study (odds ratio 0.97; 0.74 to 1.28, fully adjusted model); the analysis of years two and three, when groups were better established, gave 0.85 (0.59 to 1.22) (Lewycka et al., 2013). Interestingly, the Malawi trial found that women's groups worked better in areas *without* volunteer peer counsellors (neonatal mortality odds ratio 0.59; 0.40 to 0.86) than in areas that had them (1.38; 0.75 to 2.54), suggesting that the two programmes overlapped and may have competed for the same women's attention.

Non-randomised studies. In a pilot in rural Sindh, Pakistan, in which lady health workers and traditional birth attendants received extra training in newborn care, neonatal mortality in the intervention clusters fell from 57.3 to 41.3 per 1,000 (risk ratio 0.72; 0.56 to 0.91), while it did not fall in the control clusters (risk ratio 1.14; 0.91 to 1.44). The authors stressed that the pilot was not designed for formal statistical testing (Bhutta et al., 2008). In rural Busia County, Kenya, the share of neonatal deaths among registered births fell from 0.54% to 0.18% after community midwives were strengthened, but this change was not statistically significant and there was no comparison group (Shikuku et al., 2020). The Nigerian Midwives Service Scheme evaluation did not examine neonatal mortality (Okeke et al., 2016).

Table 4. Neonatal mortality: results from the eight studies that measured it.

Study	Comparison	Deaths per 1,000 live births	Effect (95% CI)	Reading
Manandhar 2004 (Nepal)	Women's groups vs control	26.2 vs 36.9	OR 0.70 (0.53 to 0.94)	30% fewer deaths; convincing
Tripathy 2010 (India)	Women's groups vs control	Year 3: 36.3 vs 64.3	OR 0.68 (0.59 to 0.78) overall; 0.55 (0.46 to 0.66) in years 2-3	32% fewer overall; 45% fewer later; convincing
Baqui 2008 (Bangladesh)	Home-care arm vs comparison	29.2 vs 43.5	RR 0.66 (0.47 to 0.93)	34% fewer; convincing
Baqui 2008 (Bangladesh)	Community-care arm vs comparison	45.2 vs 43.5	RR 0.95 (0.69 to 1.31)	No clear effect
Azad 2010	Women's groups	33.9 vs 36.5	RR 0.93 (0.80	Small,

Study	Comparison	Deaths per 1,000 live births	Effect (95% CI)	Reading
(Bangladesh)	vs control		to 1.09); adjusted 0.90 (0.73 to 1.10)	inconclusive
Kirkwood 2013 (Ghana)	Home visits vs control	29.8 vs 31.9	RR 0.92 (0.75 to 1.12)	Small, inconclusive
Lewycka 2013 (Malawi)	Women's groups vs control	Not reported as rates	OR 0.97 (0.74 to 1.28), years 1-3; 0.85 (0.59 to 1.22), years 2-3	No clear effect overall; effect differed by presence of peer counsellors
Bhutta 2008 (Pakistan)	Before vs after, intervention clusters	57.3 before; 41.3 after	RR 0.72 (0.56 to 0.91); control clusters 1.14 (0.91 to 1.44)	Favourable; pilot, not powered for testing
Shikuku 2020 (Kenya)	Before vs after, no control	0.54% before; 0.18% after (share of registered births)	Not statistically significant	Inconclusive; no control

OR, odds ratio; RR, risk ratio. Values are as reported by the original authors (Lewycka: fully adjusted model). For Tripathy 2010 the death rates are for intervention vs control areas in year 3. Values are not pooled.

Summary. All six trials produced a point estimate on the favourable side, but only half showed a convincing reduction. The strongest results came from programmes with high participation or intensive home contact, and from South Asia. The differences between trials are discussed in Section 4.2.

3.5 Maternal mortality

Maternal mortality is the death of a woman during pregnancy or within 42 days of its end. Because maternal deaths are rare events, even a trial involving tens of thousands of women records only a handful, and a single extra death can change the result. Three studies reported maternal deaths (Figure 4). Maternal mortality should also be interpreted in relation to the care pathway required to prevent death. Community mobilisation may accelerate recognition of danger signs and decisions to seek help, but survival from severe haemorrhage, eclampsia, sepsis and obstructed labour also depends on timely transport, skilled clinical assessment and functioning emergency obstetric services. Accordingly, the maternal mortality findings are best understood as evidence about the performance of interconnected community and facility systems, not solely the presence or absence of a community intervention.

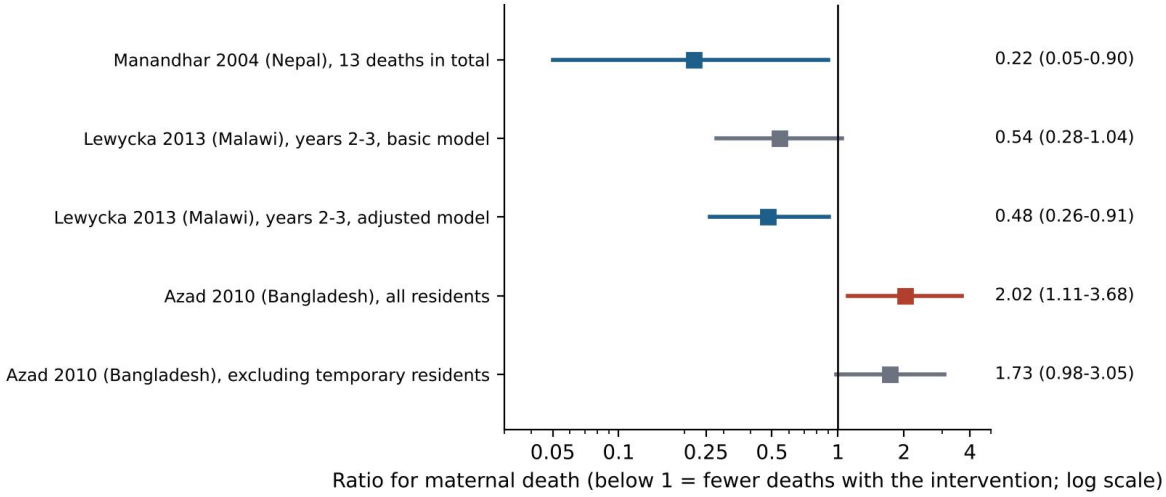


Figure 4. Maternal mortality estimates from the three trials that reported them. Blue, convincing reduction; red, convincing increase; grey, inconclusive. Note the very wide intervals, which reflect the small number of deaths recorded. Estimates are shown as originally reported and are not pooled.

In Nepal, there were 2 maternal deaths in 2,899 live births in the intervention group and 11 in 3,226 in the control group: a maternal mortality ratio of 69 compared with 341 per 100,000, a ratio of 0.22 (0.05 to 0.90). The point estimate is large (a 78% reduction), but because it rests on only 13 deaths the true effect could be anywhere from a 10% to a 95% reduction (Manandhar et al., 2004). In Malawi, women's groups were associated with lower maternal mortality in years two and three, but the result was borderline in the basic model (0.54; 0.28 to 1.04) and nominally significant in the fully adjusted model (0.48; 0.26 to 0.91), and it did not survive correction for the number of comparisons tested (Lewycka et al., 2013). In Bangladesh, the scaled-up women's group trial found the opposite: maternal mortality of 388.9 per 100,000 in the intervention area compared with 189.1 in the control area (risk ratio 2.02; 1.11 to 3.68). The authors judged this result unlikely to be a direct effect of the intervention, since no deaths occurred among group participants, and the difference was no longer statistically significant once temporary residents and tea-garden workers were excluded (1.73; 0.98 to 3.05) (Azad et al., 2010).

Taken together, the three studies point in different directions. For context, the earlier meta-analysis of women's group trials found lower maternal mortality overall (odds ratio 0.63; 0.32 to 0.94) (Prost et al., 2013), but our review adds a reminder that the evidence is imprecise and not uniform. The Nigerian studies did not measure maternal mortality. One hospital-based Nigerian study reported process measures of intrapartum quality, and none reported a maternal mortality ratio (Okonofua et al., 2023).

3.6 Maternal and newborn illness and other health outcomes

Evidence on illness was scattered and used many different definitions, which is why we could not group it by outcome as neatly as deaths (Table 5). Several points are worth noting.

A useful distinction emerges between clinical endpoints and intermediate indicators. Stillbirth, maternal complications and neonatal illness directly concern health status; skin-to-skin care, delayed bathing, early breastfeeding and care-seeking are behavioural or service-process indicators. The latter are important because they represent plausible mechanisms through which programmes influence health, but they should not be presented as equivalent to observed reductions in morbidity or mortality. Preserving this distinction strengthens the clinical interpretation of the evidence and provides a clearer framework for future evaluation.

- **Stillbirths.** The Pakistan pilot reported that stillbirths fell from 65.9 to 43.1 per 1,000 births in intervention clusters (risk ratio 0.66; 0.53 to 0.83), with no change in control clusters (1.04; 0.84 to 1.30) (Bhutta et al., 2008). Stillbirths did not differ between arms in the Nepal trial (Manandhar et al., 2004). Macerated stillbirths in Kenya fell from 0.7% to 0.36%, which was not statistically significant (Shikuku et al., 2020).
- **Maternal illness.** In Malawi, women's groups did not change mothers' reports of health problems (odds ratio 0.80; 0.55 to 1.18), whereas volunteer peer counselling reduced reported infant health problems (0.58; 0.39 to 0.88) (Lewycka et al., 2013). In India, women's groups did not reduce maternal depression overall, but moderate depression was 57% lower in the third year (odds ratio 0.43; 0.23 to 0.80) (Tripathy et al., 2010). In Nigeria's Midwives Service Scheme evaluation, the rates of complications during labour (difference 0.012; −0.028 to 0.051) and after birth (0.002; −0.033 to 0.038) were not different from comparison communities (Okeke et al., 2016).
- **Newborn care practices.** Several trials improved the behaviours that protect newborns. In Ghana, skin-to-skin care rose from 24% to 44% (risk ratio 2.30; 1.85 to 2.87), care-seeking for sick babies from 55% to 77%, and delayed first bathing from 29% to 41% (Kirkwood et al., 2013).
- **Clinical care quality.** A study of referral hospitals in Nigeria found that an intrapartum quality-improvement programme raised an overall quality score by about 6% and improved five of ten quality indicators, but it measured care processes reported in exit interviews rather than complications or deaths (Okonofua et al., 2023).

Overall, evidence about illness is limited. We cannot conclude that community-based programmes consistently reduce maternal or newborn illness; they appear to improve care practices, and in some settings stillbirth, but this needs confirmation.

Table 5. Evidence on stillbirth, illness and care practices.

Study	Outcome	Finding
Bhutta 2008	Stillbirth	65.9 to 43.1 per 1,000 births in intervention clusters (RR 0.66; 0.53 to 0.83); no change in control clusters (1.04; 0.84 to 1.30)
Manandhar 2004	Stillbirth	Similar in both arms
Shikuku 2020	Macerated stillbirth; babies discharged alive	Stillbirth 0.7% to 0.36% (not significant); babies discharged alive 98% to 100%
Lewycka 2013	Reported maternal and infant health problems	Women's groups: maternal problems OR 0.80 (0.55 to 1.18), no clear effect. Peer counselling: infant problems OR 0.58 (0.39 to 0.88), fewer
Tripathy 2010	Maternal depression	No overall effect; moderate depression 57% lower in year 3 (OR 0.43; 0.23 to 0.80)
Okeke 2016	Complications in labour; after birth	No difference from comparison communities (0.012; -0.028 to 0.051, and 0.002; -0.033 to 0.038)
Kirkwood 2013	Newborn care practices	Skin-to-skin care 44% vs 24%; care-seeking for sick babies 77% vs 55%; delayed bathing 41% vs 29%; early breastfeeding 49% vs 41%
Okonofua 2023	Quality of intrapartum care	Composite quality score about 6% higher; 5 of 10 indicators improved (process measures from exit interviews, not complications)

Willey 2022, Okonofua 2022, Grépin 2022 and Baqui 2008 did not report maternal or newborn illness outcomes in the sources checked.

3.7 How certain are we?

Table 6 gives our proposed GRADE ratings. We have moderate confidence that well-implemented women's group and home-visit programmes reduce neonatal mortality, mainly in South Asia. Confidence is low for sub-Saharan Africa, where results were mixed, and very low for maternal mortality and for illness outcomes because the number of events was small, results were inconsistent and few studies measured these outcomes.

Table 6. Proposed certainty of evidence (GRADE) for the main outcomes.

Outcome	Evidence	Main reasons for the rating	Proposed certainty
Neonatal mortality: women's group and home-visit programmes	6 cluster RCTs; 2 non-randomised studies	Results differ between trials (3 convincing, 3 inconclusive); most evidence from South Asia; no blinding possible	Moderate
Neonatal mortality: sub-	2 RCTs (Ghana, Malawi); 1	Inconclusive results; imprecise; context-specific	Low

Outcome	Evidence	Main reasons for the rating	Proposed certainty
Saharan Africa	uncontrolled study (Kenya)		
Maternal mortality	3 cluster RCTs	Very few deaths; very wide intervals; results point in different directions	Very low
Maternal and newborn illness	Scattered; 6 studies	Different definitions; mostly self-reported or uncontrolled; few studies	Very low

4. Discussion

4.1 Main findings

This review brings together 13 studies from eight countries. Its central message is encouraging but more cautious than the headlines of some earlier work. Community programmes can reduce newborn deaths, as in Nepal, India and Sylhet, Bangladesh, where reductions of about one-third were seen. However, the same type of programme did not produce clear reductions in Ghana, Malawi or the scale-up trial in Bangladesh. The evidence on maternal deaths comes from just three studies and is too imprecise to be firm. And although five of the 13 studies were conducted in Nigeria, none counted deaths, so the question that matters most to Nigerian policy-makers remains open.

The synthesis supports a mechanism-oriented interpretation of community programmes. The strongest neonatal survival findings are not attributable simply to moving an activity outside a health facility; they arise where community participation or household contact translates into timely recognition of need, effective newborn practices and practical links to skilled care. This is particularly important for midwifery policy: the most relevant unit of intervention is a functioning care pathway, in which community trust, skilled personnel, referral transport, clinical readiness and postnatal follow-up reinforce one another.

4.2 Why did some programmes work better than others?

Reading across the trials suggests several explanations that future programmes can learn from.

These implementation dimensions can be understood as interacting conditions rather than isolated predictors. High coverage has limited value if contacts are superficial, while intensive counselling may have little population-level impact when few families are reached. Similarly, improved recognition of danger signs can translate into survival gains only where referral and treatment are accessible. Programme planning should therefore assess reach,

intensity, fidelity, clinical linkage and continuity together, and measure each prospectively alongside outcomes.

Reach and intensity. A programme cannot save lives in families it does not reach. In Nepal only 8% of women in the intervention clusters joined women's groups, yet the programme still lowered neonatal mortality, probably by spreading knowledge through the wider community. In India, effects increased as the groups matured over three years. In Sylhet, 97% of families in the home-care arm received a home visit, compared with 46% in the community-care arm that relied on group sessions, and only the high-contact arm reduced deaths (Baqui et al., 2008). This fits the finding of earlier reviews that effects are larger when coverage is higher (Gogia & Sachdev, 2010; Prost et al., 2013).

Time. Behaviour change takes time. The Indian trial showed no benefit in the first year and large benefits in years two and three (Tripathy et al., 2010), and the Malawi trial found stronger effects in its later years. Short evaluations, such as those of 9 to 11 months in some Nigerian and Kenyan studies, may miss gains that appear later, or exaggerate early ones.

Where mortality is high and care is scarce. Programmes tend to show larger absolute benefits where baseline mortality is highest and few alternatives exist. In Sindh and Sylhet, where most women delivered at home, home visits and newborn care education could address a large unmet need. Where other programmes were already running, as in the Bangladesh scale-up trial (in which both arms received health-service strengthening and training of traditional birth attendants), the additional effect of women's groups was smaller.

Overlapping programmes. The Malawi trial showed that two good ideas combined do not necessarily add up: women's groups and peer counselling appeared to dilute each other. Planners should ensure that complementary programmes divide the work sensibly rather than compete for the same women.

Linkage to skilled care. Community workers cannot treat obstructed labour or severe infection. Programmes that include referral and a functioning higher-level facility are more likely to convert early detection into survival (Adegoke & van den Broek, 2009). Most of the included trials relied on referral to existing, sometimes weak, facilities, which may limit how far deaths can fall.

4.3 How these findings compare with earlier reviews

Our results for newborn deaths are consistent in direction with earlier meta-analyses, which found 23% to 25% lower neonatal mortality from women's groups and community-based packages (Prost et al., 2013; Lassi & Bhutta, 2015). What this review adds is (a) a closer look

at the individual trials and why they differ, (b) the inclusion of midwifery-led and health-system programmes in Nigeria and Kenya that earlier reviews of women's groups and community health workers did not cover, and (c) a transparent account of how little is known about maternal deaths and illness.

The present synthesis adds an important distinction to the earlier pooled literature: evidence that a broad class of community programmes can improve newborn survival does not establish that every constituent model, staffing configuration or national implementation will have the same effect. By retaining the original study estimates and examining the programme mechanisms, this review makes the findings more directly useful for implementation decisions. Its value lies in identifying the conditions under which community-based approaches have demonstrated benefit and the kinds of outcome measures required to evaluate their transfer into routine midwifery services.

4.4 What this means for Nigeria

Nigeria's evidence base is both promising and incomplete. The Midwives Service Scheme, which deployed 2,488 midwives to 652 primary health centres across the country, was rigorously evaluated by Okeke and colleagues, who found only a short-lived increase in antenatal care and no increase in skilled birth attendance or facility delivery, and no improvement in complication rates (Okeke et al., 2016; Okeke et al., 2017). The reasons documented include irregular pay, poor housing, and midwives leaving their posts, so that fewer than one in three clinics had its full complement of four midwives by 2014 (Abimbola et al., 2012; Okeke et al., 2017). This is an important lesson: placing midwives in clinics is necessary but not sufficient, and a programme that cannot keep its staff cannot save lives.

For Nigeria, the most actionable implication is to link the deployment of midwives to an explicit community-to-facility delivery model. Posting skilled personnel should be accompanied by community mapping, birth preparedness, early postnatal contact, functional transport arrangements, reliable supplies and referral agreements with higher-level facilities. Monitoring should track not only staffing and service utilisation but also the completeness of the pathway from first contact to definitive care. State and local-government implementation can be strengthened by combining routine service records with community-level reporting of births, deaths and referral completion.

Several newer Nigerian studies suggest that combining midwives with community mobilisation, funding for delivery costs, transport and facility strengthening can increase the use of care, which the companion article examines in detail. But none of these programmes has yet been evaluated for effects on deaths. The results from South Asia, where women's

groups and home visits cut neonatal mortality, are encouraging, but those settings differ from northern Nigeria in culture, religion, gender norms and health-system structure, so adaptation and testing are essential (Doctor et al., 2012; Okereke et al., 2019; Esienumoh & Allotey, 2018).

4.5 Implications for nursing and midwifery practice

- **Community engagement is clinical work.** The most effective programmes were ones in which nurses, midwives and trained facilitators sat with women and families, listened, and helped them plan, not just ones that provided clinical services.

A practical midwifery implementation package can be organised around antenatal identification, birth preparedness, safe delivery, early postnatal surveillance and escalation of complications. At antenatal contact, the midwife should establish a household-specific birth and emergency plan; during community outreach, trained workers should reinforce danger-sign recognition and referral; at birth, skilled attendance and immediate newborn care should be prioritised; and in the first postnatal week, scheduled contact should focus on feeding, warmth, infection signs and timely care-seeking. Supervision, documentation and feedback between community workers and facilities should be embedded throughout these steps. This operational sequence translates the review's evidence into observable service activities without treating non-clinical community workers as substitutes for skilled birth attendants.

- **Prepare every woman for birth and for danger.** Birth preparedness (saving money, arranging transport, choosing a helper and a facility) and knowledge of danger signs in mothers and newborns were common to successful programmes and are within reach of every antenatal clinic.
- **Postnatal contact is a lifesaver.** Home or early postnatal contacts in the first week, when most newborn deaths occur, were central to the Sylhet and Ghana programmes. Midwifery services should plan for these visits and for referral of sick babies.
- **Supervision and support keep workers in post.** Midwives and community health workers need reliable pay, supervision, housing and equipment; otherwise gains are lost to turnover.
- **Record and report.** Midwives and health managers can help future evaluations by counting births, deaths and complications accurately in routine registers.

4.6 Strengths and limitations

Strengths include prospective registration, a wide search with no language limits, duplicate screening and extraction, and rechecking of key figures against the original papers.

Limitations should also be considered. First, only 13 studies were included, and they are very different from one another, which is why we could not pool them; a narrative synthesis is less precise than a meta-analysis, and our judgements about the direction of results involve some interpretation. Second, evidence on maternal mortality and illness is sparse. Third, seven studies were non-randomised and subject to selection bias and confounding; two of them (the Kenyan and Edo State studies) lacked a concurrent comparison group and so, strictly speaking, do not meet our own comparator criterion, while a third compared referral hospitals rather than community programmes. We kept them in the review to reflect the available Nigerian evidence, but conclusions in this article rest mainly on the cluster-randomised trials and on the controlled quasi-experimental studies. Fourth, as in any systematic review, relevant unpublished evaluations, such as government programme reports, may have been missed. Fifth, publication bias could not be formally tested with so few studies.

4.7 Recommendations for research

- Evaluate Nigerian community midwifery programmes using designs that include a comparison group and measure deaths, using routine registration, health-facility records or large household surveys.

Future evaluations would be particularly informative if they pre-specified maternal mortality, neonatal mortality, stillbirth and defined morbidity outcomes; recorded baseline risk; measured implementation coverage and fidelity; and reported cluster-adjusted effect estimates where cluster designs are used. Process evaluation should accompany outcome evaluation so that changes in staffing, referral completion, service readiness and household contact can be linked to observed effects. In Nigeria, multi-site designs with sufficient follow-up and explicit comparison groups would support interpretation across rural, peri-urban and hard-to-reach settings.

- Standardise outcome definitions (for example, WHO definitions of maternal death and neonatal death) so that results can be pooled in future.
- Report coverage and implementation details (how many families were reached, how often workers visited, staff turnover) so that readers can understand why results differ.
- Test combinations of programmes, for example women's groups with midwife-led postnatal visits, in Nigerian states with high mortality, with follow-up long enough to see effects (at least two to three years).

5. Conclusion

Community-based maternal and newborn programmes, especially those that combine strong community engagement or home visits with reliable links to skilled care, can reduce newborn deaths in underserved settings, with reductions of about one-third in the most convincing trials. The evidence for fewer maternal deaths and less illness is limited and uneven, and the evidence that community midwifery models themselves reduce deaths in Nigeria is yet to be generated. Investment in these programmes is justified, but it should come with commitment to retain midwives, to reach many families, and to measure what matters: whether mothers and babies live.

The central policy lesson is therefore one of integration rather than substitution. Community mobilisation, trained household visitors and skilled midwives have complementary roles, but their effectiveness depends on whether they form a continuous, responsive maternal and newborn care system. The strongest contribution of this review is to connect the mortality evidence with concrete implementation features—coverage, continuity, clinical readiness and referral—that can guide nursing and midwifery practice, programme design and future evaluation in underserved settings.

Declarations

Author contributions. CUM conceived the review, designed the methodology, supervised the project and wrote the original draft. CCAN designed and ran the literature searches and, with UUE, screened records, extracted data and assessed risk of bias. UUE contributed to data curation and to the narrative synthesis and prepared the tables and figures. OM acted as the third reviewer to resolve disagreements, validated the extracted data and certainty ratings, and reviewed and edited the manuscript. All authors read and approved the final version and agree to be accountable for the work.

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Data availability. All data are from published studies cited in the reference list. Extraction sheets are available from the corresponding author on request.

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