BMJ Global Health

Proactive case detection of common childhood illnesses by community health workers: a systematic review

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To cite: Whidden C, Thwing J, Gutman J, *et al*. Proactive case detection of common childhood illnesses by community health workers: a systematic review. *BMJ Global Health* 2019;**4**:e001799. doi:10.1136/bmjgh-2019-001799

Handling editor Seye Abimbola

Received 28 June 2019
Revised 25 September 2019
Accepted 28 September 2019

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ABSTRACT

Introduction Identifying design features and implementation strategies to optimise community health worker (CHW) programmes is important in the context of mixed results at scale. We systematically reviewed evidence of the effects of proactive case detection by CHWs in low-income and middle-income countries (LMICs) on mortality, morbidity and access to care for common childhood illnesses.

Methods Published studies were identified via electronic databases from 1978 to 2017. We included randomised and non-randomised controlled trials, controlled before–after studies and interrupted time series studies, and assessed their quality for risk of bias. We reported measures of effect as study investigators reported them, and synthesised by outcomes of mortality, disease prevalence, hospitalisation and access to treatment. We calculated risk ratios (RRs) as a principal summary measure, with CIs adjusted for cluster design effect.

Results We identified 14 studies of 11 interventions from nine LMICs that met inclusion criteria. They showed considerable diversity in intervention design and implementation, comparison, outcomes and study quality, which precluded meta-analysis. Proactive case detection may reduce infant mortality (RR: 0.52–0.94) and increase access to effective treatment (RR: 1.59–4.64) compared with conventional community-based healthcare delivery (low certainty evidence). It is uncertain whether proactive case detection reduces mortality among children under 5 years (RR: 0.04–0.80), prevalence of infectious diseases (RR: 0.06–1.02), hospitalisation (RR: 0.38–1.26) or increases access to prompt treatment (RR: 1.00–2.39) because the certainty of this evidence is very low.

Conclusion Proactive case detection may provide promising benefits for child health, but evidence is insufficient to draw conclusions. More research is needed on proactive case detection with rigorous study designs that use standardised outcomes and measurement methods, and report more detail on complex intervention design and implementation.

PROSPERO registration number CRD42017074621.

Key questions

What is already known?

- While many low-income and middle-income countries (LMICs) are adopting community health worker (CHW) programmes as an evidence-based strategy to achieve global health goals, the expected benefits have not been realised in all contexts.
- Recent reviews for developing global guidelines to optimise CHW programmes found a scarcity of evidence on best practices for CHW education, deployment and management.

What are the new findings?

- Proactive case detection of common childhood illnesses by CHWs in LMICs may reduce infant mortality and increase access to effective treatment compared with conventional community-based healthcare delivery (low certainty evidence).
- Studies assessing the effects of proactive case detection showed considerable diversity in terms of participants, interventions, comparisons, outcomes and study quality.

What do the new findings imply?

- Proactive case detection may be more effective than conventional community-based healthcare delivery in achieving child health gains.
- More implementation research is needed with rigorous study designs and standardisation of outcomes to optimise the design and implementation of CHW programmes for impact.

INTRODUCTION

Community health worker (CHW) programmes are experiencing a resurgence as a strategy to achieve health-related sustainable development goals. Many low-income and middle-income countries (LMICs) have implemented integrated Community Case Management (iCCM) of common childhood illnesses,[1 2] a package of services delivered by CHWs to diagnose, treat and refer children under 5 with malaria, diarrhoea, pneumonia and malnutrition in the community setting.[3] This strategy has shown an increase in access to care and reduced child mortality.[4–12] However, the expected benefits have not been realised in all contexts.[13–18] Several recent evaluations of national iCCM programmes in

Burkina Faso, Ethiopia and Malawi did not find impacts on care-seeking or child mortality.[19–22]

These programmes shared certain design features that may have contributed to the lack of overall effects by not addressing barriers to care, such as user fees for services,[23–25] lack of adequate CHW supervision,[26–28] or provision only for patients who sought care from a fixed site. As more countries scale up CHW programmes, it is critical to understand how to best design and implement iCCM, and CHW services more broadly, in order to realise their full potential.

A recent series of systematic reviews to inform WHO guidelines for optimising CHW programmes found a scarcity of evidence on best practices for several key policy areas, including CHW training, supervision and deployment, and calls specifically for more research on CHW workflow.[29] We conducted a systematic review of the evidence for the effectiveness of proactive case detection by CHWs to improve access to care and reduce morbidity and mortality. By proactively seeking out patients at home to offer diagnosis and treatment or referral, a proactive workflow has the potential to overcome barriers to care, including direct and indirect costs, distance, mistrust and gender inequality, reduce the time from onset of a condition to services, and consequently reduce disease progression and mortality.

METHODS

Inclusion criteria

Study designs

Studies from LMICs involving community-based, proactive case detection of common childhood illnesses were identified. Anticipating that randomised trials of healthcare service delivery would be very few, we included a broader range of study designs in line with Cochrane Effective Practice and Organisation of Care (EPOC) group recommendations.[30] These included randomised controlled trials (RCTs) and non-randomised controlled trials (NRCTs), controlled before–after (CBA) studies, interrupted time series (ITS) and repeated measure studies.

Interventions and comparisons

To be eligible for inclusion, studies needed to evaluate a primary healthcare intervention that included proactive case-finding home visits by CHWs for the purpose of searching for and identifying, through history and/or diagnostics, cases of common childhood illness, including malaria, diarrhoea, pneumonia, malnutrition, HIV or tuberculosis. These conditions were chosen because they are covered by international protocols for iCCM of common childhood illnesses[31] and/or contribute a substantial disease burden in LMICs. Studies needed to compare proactive healthcare delivery to usual or supplemented primary care available from facilities and/or CHWs that did not involve home visits for the purpose of identifying sick patients.

CHWs and trial participants

In accordance with earlier reviews, a CHW was defined as any lay health worker who received training to perform tasks related to primary healthcare delivery but had not received professional medical or paramedical education.[32] Recipients of proactive case-finding home visits had to include children under 5 years of age.

Outcomes

We included studies if they assessed any of the following outcomes: (1) mortality among children under 5 years of age or infants aged 0–11 months; (2) prevalence or incidence of disease; (3) hospitalisation; (4) access to healthcare services; (5) harms or adverse effects; (6) costs or economic effects.

Our review focused on assessing proactive case detection as an adjoint to iCCM. As causes of neonatal deaths in LMICs differ from those of post-neonatal child deaths, we did not include studies that were restricted to neonates, that is, intervening solely in the neonatal period and reporting solely on neonatal outcomes. Nevertheless, we retained studies from our search that assessed childhood illness starting from the first day of life and reported outcomes separately for neonates and infants.

Search strategy

We searched the following electronic databases for studies meeting the eligibility criteria, in addition to contacting researchers with expertise relevant to the review topic:

- MEDLINE Ovid (1946 to September Week 4 2017) (searched 10 October 2017);
- Embase (1947 to 2017 October 20) (searched 23 October 2017);
- Global Health Database (1910 to 2017Week 41) (searched 23 October 2017);
- Cochrane Central Register of Controlled Trials (searched 9 November 2017);
- WHO Library (searched 30 November 2017).

The search strategy included terms to capture the following concepts describing the intervention: (i) proactive case detection—broad search terms were used to maximise sensitivity given a lack of MeSH terms for this concept; (ii) CHWs—search terms were adapted from a review by Lewin and colleagues[32] and (iii) condition. A combination of two methodological search filters was adapted to capture a fourth concept for appropriate study design: (iv) the sensitivity-maximising Cochrane MEDLINE filter for RCTs and an EPOC filter for non-randomised trials. The search included publications since 1978, the year of the Alma-Ata Declaration, which marked a restructuring of the global health agenda towards primary healthcare provision by CHWs. No language restrictions were applied. Full strategies and results are provided in online supplementary file 1.

Data collection and analysis

Selection of studies

Studies retrieved from the search were uploaded onto Covidence, a Cochrane technology platform for systematic reviews.[33] Two reviewers (CW and JT or JG) independently screened titles, abstracts and full-text articles

for eligibility. Inclusion was determined by consensus or in consultation with a third reviewer (JT or JG).

Data extraction and quality assessment

Two reviewers (CW and EW) independently extracted data from included studies related to study identification, methods, population, interventions, implementation of intervention, outcomes and results using a data extraction form designed in Covidence. Two reviewers (CW and EW) independently assessed the quality of included studies using the EPOC risk of bias tool for studies with a separate control group;[34] allocation concealment was removed from the quality assessment criteria as reviewers deemed this domain inapplicable due to the nature of the intervention under review. Consensus on data extraction and quality assessment was reached in discussion or in consultation with a third reviewer (JT or JG).

Data synthesis

We reported measures of effect in the same way that study investigators reported them and synthesised them by type of outcome. For studies with a separate control group, we included only the measure of effect derived by comparing the intervention group to the control group, if multiple comparisons were reported. For studies with no separate control group, we included baseline to end-line comparisons. We calculated risk ratios (RRs) for dichotomous data to allow for comparisons across studies. If appropriate denominators (eg, number of live births for mortality outcomes) were not reported, we used population estimates reported in the study to approximate the denominator. We calculated 95% CIs, adjusting for clustering using the intracluster correlation coefficient (ICC) reported in the study, if available.[35] If not available, we used a conservative ICC of 0.05 for all studies with a cluster design, as the ICC was <0.001 in the three studies for which it was reported. We assessed heterogeneity across studies for each outcome type both qualitatively and quantitatively using the I^2 statistic, which describes the percentage of total variation across studies that is due to heterogeneity rather than chance.[36] Two reviewers (CW and JT or JG) independently assessed the certainty of evidence for each analysis using the Grading of Recommendations, Assessment, Development and Evaluation approach,[37 38] which takes into account study design, risk of bias, inconsistency, indirectness/applicability, imprecision and strength of association. Consensus was reached through discussion or in consultation with a third reviewer (JT or JG).

RESULTS

Characteristics of included studies

Excluding duplicates, a total of 442 abstracts were screened for eligibility (figure 1 in online supplementary file 2). Fourteen studies were included, including five cluster RCTs (table 1). Complete information on the characteristics and risk of bias for each study is available in online supplementary file 3.

Study settings

Among the 14 included studies, seven were from Africa (three KwaZulu-Natal, South Africa,[39–41] two Mali,[42 43] one Ethiopia[44] and one Senegal.[45] The two reports from Mali[42 43] and the two from rural South Africa,[40 41] respectively, studied the same interventions delivered to the same populations, differing only with regard to when—and in South Africa, how—impact was assessed. Six studies were from Southeast Asia (three India,[46–48] one Bangladesh,[49] one Nepal[50] and one Pakistan.[51] Two reports from Haryana, India[47 48] evaluated the same intervention delivered to the same population but assessed different outcomes. One study was from the Americas, in Dominican Republic.[52] Four studies took place in urban or periurban settings,[39 42 43 52] and eight in rural settings;[40 41 44–46 49–51] the studies in Haryana[47 48] did not indicate whether the setting was rural or urban.

Study designs and outcomes

The KwaZulu-Natal, South Africa[39–41] and Haryana, India[47 48] studies were cluster RCTs that evaluated a range of access to care, morbidity and mortality outcomes; the rural South Africa study did not report outcomes separately for children under 5 years.[40 41] Two studies were NRCTs that measured morbidity outcomes;[49 52] the Bangladesh study did not report outcomes separately for children under 5 years.[49] The Nepal study[50] that used a non-randomised, stepped-wedge design to assess risk of death among infants and children did not compare results between early and late treatment groups. Instead, it compared annual risks to baseline and used a test for trend to assess programme maturity. This study was therefore considered in this review to be an uncontrolled before–after study from baseline to end-line.

Four studies used a CBA design[44–46 51] and reported percent differences or difference-in-differences for mortality, morbidity or access to care outcomes. However, some did not use the baseline or control group appropriately. The Pakistan study[51] reported different baseline years for intervention and control areas; therefore, this study was deemed a NRCT and only the postintervention comparison between groups was presented in this review. The Ethiopia study[44] presented a number of before–after access to care indicators for the intervention group, but only present before–after data for the comparison group for one outcome, the tuberculosis case notification rate; outcomes were not reported separately for children under 5 years. Finally, the Mali studies[42 43] were included as ITS designs; yet, with only one baseline, they lacked a comparative preintervention trend and thus were treated in the review as uncontrolled before–after studies from baseline to end-line.

Participants

Half of the studies extended CHW services to the entire population,[42–45 49 53 54] among which only the Mali

Table 1 Characteristics of included studies evaluating proactive case detection of common childhood illnesses by community health workers

Study	Study design (period)	Setting + unit of allocation	Participants + CHW sample	Description of proactive case detection intervention				Description of control	Outcomes (timepoints)
				CHW profile	Conditions	Timing	Cointerventions		
Bang, 1999	CBA (1993–1998)	Rural, Maharashtra, India; Area I: n=1 C: n=2	Mother–baby dyads I: n=38 998 C: n=42 149 CHWs I: n=? C: n=?	Educated female VHWs; recruited locally; trained 6 months; supervised fortnightly; performance-linked payment	Doorstep detection of mother and infant danger signs/illnesses, and (in yr. 2–3) home-based management and follow-up of neonatal illnesses and sepsis	Newborn home visits on days 1, 2, 3, 5, 7, 14, 21, 28 and any day called on for 3 year.	▶ Mother's health education (in year 3) ▶ User fee removal for VHW neonatal care ▶ CCM of childhood malaria, diarrhoea, pneumonia by male VHWs and TBAs	Routine care + CCM of childhood malaria and diarrhoea by male VHWs	Infant, neonatal, perinatal mortality (6 months)
Bhandari, 2012	Cluster RCT (2006–2010)	Faridabad, Haryana, India; PHC area I: n=9 C: n=9	Mother–baby dyads I: n=29 782 C: n=30 920 CHWs I: n=601 C: n=?	Anganwadi workers trained additional 8 days in IMNCI; vacant supervisory roles filled (ASHAs); task-based pay; village drug depot	Doorstep detection, treatment and/or referral of newborn danger signs and infection, and infant diarrhoea, pneumonia and malnutrition	Newborn home visits on days 1, 3, 7; again if low birth weight on days 14, 21, 28	▶ IMNCI training for other public and private providers (eg, nurses) ▶ Quarterly mother's health education groups	Routine care at CHW and facility levels	Infant, neonatal, perinatal, postnatal mortality (1, 3, 6, 9, 12 months)
Chen, 1980	Cluster NRCT (05/1978–08/1978)	Rural, Bangladesh; Area I: n=1 C: n=1	Population I: n=157 381 C: n=134 249 CHWs I: n=160 C: n=130	Existing female VHWs trained for two half-days on indications, use, hazards of ORS	Doorstep detection (enquiry) and ORS treatment of simple diarrhoea; referral of severe diarrhoea or complications	Daily household visits	▶ ORS packets provided free-of-charge ▶ Diarrhoeal treatment facility and ambulance	Routine care at VHW and facility levels+diarrhoeal treatment facility and ambulance	Hospital admissions (0–4 months)
Johnson, 2013 and 2018	Repeated cross-sectional (2008–2015)	Periurban, Mali; PHC area I: n=1	Population I: n=56 000 CHWs I: n=20 C: n=NA	Educated female CHWs; recruited locally; 36-day training in iCCM; monthly dedicated supervision; paid monthly stipend	Doorstep detection, referral, follow-up for all cases of disease; doorstep detection and treatment of childhood malaria	Home visits for at least 2 hour/day, 6 days/week, aiming to visit all households 2x per month	▶ User fee removal at CHW and PHC levels ▶ PHC training and infrastructure improved ▶ Adult education and microenterprise groups ▶ LLIN distribution	NA	▶ Child mortality ▶ Prevalence of febrile illness ▶ Treatment rates (0, 12, 24, 36, 48, 60, 72 and 84 months)

Continued

Table 1 Continued

Study	Study design (period)	Setting + unit of allocation	Participants + CHW sample	Description of proactive case detection intervention				Description of control	Outcomes (timepoints)
				CHW profile	Conditions	Timing	Cointerventions		
Khan, 1990	CBA (1984–1987)	Rural, Pakistan; Village I: n=31 C: n=7	Under fives I: n=4665 C: n=1194 CHWs I: n=17 C: n=0	Educated CHWs recruited locally; trained in CCM of symptomatic ARI	Doorstep detection of ARI and treatment or referral for suspected pneumonia	Approx. 200 households visited every 10–14 days	▶ Standardised facility ARI treatment protocol ▶ Maternal health education programmes ▶ Vaccine campaign	Routine care at facility level + vaccine campaign	Infant and child mortality (I: 0–33 months C: quarterly from 0–33 months)
Linn, 2015	CBA (07–11/2013)	Rural, Senegal; Village I: n=15 C: n=15	Population I: n=4217 C: n=4747 CHWs I: n=? C: n=?	HCPs; 1-day training in active case detection; community-level and facility-level supervision; paid for added work	Doorstep detection, treatment and follow-up of malaria for individuals of all ages	Weekly sweeps to every household in the village	▶ Initial community mobilisation and health education ▶ LLIN distribution ▶ SMC	Routine care at HCP and facility levels+LLIN distribution and SMC	▶ Malaria prevalence (I: 0–21 weeks C: 0, 12, 21 weeks) ▶ Care-seeking rates (0–21 weeks)
Mazumder, 2014	Cluster RCT (2006–2010)	Faridabad, Haryana, India; PHC area I: n=9 C: n=9	Mother–baby dyads I: n=29667 C: n=30813 CHWs I: n=601 C: n=?	Same as Bhandari, 2012	Same as Bhandari, 2012	Same as Bhandari, 2012	Same as Bhandari, 2012	Same as Bhandari, 2012	▶ Treatment rates ▶ Hospital admissions ▶ Disease prevalence (1, 6, 12 months)
Navarro, 2013	Cluster NRCT (2005–2007)	Urban, Dominican Republic; Parish I: n=8 C: n=8	Mother–child dyads I: n=266 C: n=337 CHWs I: n=? C: n=0	Community volunteers; mostly female; 60 hours basic training	Doorstep detection (weighing and plotting weight-for-age curve), follow-up and referral for childhood risk of overweight or malnutrition	Fortnightly home visits for first 1.5 month after birth, then monthly until age 2	Women's groups that met fortnightly during pregnancy, then monthly after childbirth, included newborn care and growth monitoring	Routine care at facility level	▶ Prevalence of malnutrition, wasting, stunting, overweight ▶ Hospital admissions (0–24 months)
Pandey, 1991	Non-randomised stepped-wedge trial (1986/1987–1989)	Rural, Nepal; Subdistrict I: n=8 C: n=10	Under fives I: n=3307 C: n=3377 CHWs I: n=1/1000 C: n=0	Literate CHWs recruited locally; 9-day training; supervised fortnightly; stocks ensured; salaried	Doorstep detection and treatment of childhood pneumonia	Daily visits to 10–15 child households, visiting all target homes every 2 weeks	User fees removed for pneumonia treatment	Routine care at facility level	Risk of death (0–36 months)

Continued

Table 1 Continued

Study	Study design (period)	Setting + unit of allocation	Participants + CHW sample	Description of proactive case detection intervention				Description of control	Outcomes (timepoints)
				CHW profile	Conditions	Timing	Cointerventions		
Tomlinson, 2014	Cluster RCT (2008–2010)	Periurban, KwaZulu-Natal, South Africa; Subplaces I: n=15 C: n=15	Mother–baby dyads I: n=1821 C: n=2136 CHWs I: n=? C: n=?	Literate female CHWs recruited locally; 10-day training in IMNCI and PMTCT; salaried	Doorstep detection and help seeking for mother and child danger signs/illnesses	two pregnancy + five postnatal home visits on days 1, 3–4, weeks 2, 3–4, 7–8; 2 extra week 1 if low birth weight	None reported	1 CHW home visit during pregnancy+2 postnatal to help with birth certificates and social grants	► Neonatal mortality ► Disease prevalence ► Care-seeking (Pregnancy+: I: 0–8 weeks C: 0–12 weeks)
Uwimana, 2012	Cluster RCT (2009–2010)	Rural, KwaZulu-Natal, South Africa; Village I: n=3 C: n=3	Population CHWs I: n=39 C: n=50	Former (NGO) CHWs recruited for 1 CCW cadre; 60-day training in TB/HIV/PMTCT; supervised by CHFs at PHC; monthly stipend	Doorstep detection, referral and treatment adherence support for HIV, TB, STIs	Not reported	Initial community mobilisation that included HCT, TB and STI screening and referral	Home visits by CCWs promoting HCT and referring to clinic for HIV testing	Access to screening services (0–10 months)
Uwimana, 2013	Cluster RCT (2009–2011)	Same as Uwimana, 2012	Same as Uwimana, 2012	Same as Uwimana, 2012	Same as Uwimana, 2012	Not reported	None reported	Not reported	Access to screening services and treatment adherence support (14 months)
Yassin, 2013	CBA (2010–2011)	Rural, Ethiopia; Zone I: n=1 C: n=1	Population I: n>3.0 m C: n>1.3 m CHWs 1: n=524 C: n=0	Females HEWs recruited locally; 1 year HSEP training; salaried; supported by lay volunteer CHPs and supervisors	Doorstep TB detection (enquiry), referral and follow-up/treatment adherence support	Not reported	► PHC training and laboratory equipment ► Community education via meetings, radio, etc. ► Contact screening and IPT for asymptomatic children by supervisors	Routine care at facility level	Case detection (0–14 months)

Notes: Unit of allocation is the geographic area allocated between intervention and control groups, even if the intervention was implemented at a smaller level (eg, village). *Participants* are those that received the proactive case detection intervention (n=sample at baseline); the CHW sample is provided where available. Health education/promotion activities are only listed under *cointerventions* if they took place outside of the proactive case detection home visits. *Outcomes* include those that are considered in this review. Fixed *time-points* are the time from intervention roll-out to survey measurement; where outcomes are measured throughout the intervention period from routine data, *time-points* are the range that the intervention was in effect.
ARI, acute respiratory infection; ASHA, accredited social health activists;C, comparison; CBA, controlled before–after; CCM, community case management; CCW, community care worker; CHF, community health facilitator; CHP, community health promoter; CHW, community health worker; HBC, home-based carer; HCP, home care providers; HCT, HIV counselling and testing;HEW, health extension worker; HSEP, health service extension programme;I, intervention; iCCM, integrated community case management; ICDS, integrated child development service; IM(N)CI, integrated management of (neonatal and) childhood illness; IPT, isoniazid preventive therapy; LLIN, long-lasting insecticidal bed net; NRCT, non-randomised controlled trial; ORS, oral rehydration solution; PHC, primary health centre; PMTCT, prevention of mother to child transmission; RCT, randomised controlled trial; SMC, seasonal malaria chemoprophylaxis;STI, sexually transmitted infection; TB, tuberculosis; TBA, traditional birth attendant; VHW, village health worker; yr., year.

studies[42 43] reported outcomes specifically for children under 5 years. Five studies recruited pregnant women and delivered a mother–child intervention during the neonatal period, and in some cases, into infancy and childhood.[39 46–48 52] The remaining two studies tested interventions that targeted children under 5 years of age during a period of 3 years.[50 51]

Characteristics of CHW programmes

The Bangladesh,[49] Ethiopia,[44] Senegal,[45] rural South Africa[40 41] and more recent India[47 48] studies provided supplemental training in the context of the study (two-half days in Bangladesh, 1 day in Senegal, 8 days in India, 60 days in South Africa and unreported in Ethiopia) to CHWs from an already established CHW cadre. The remaining studies evaluated CHW programmes initiated by a research institute, all of which recruited local, literate community members and trained them for a duration of 60 hours[52] to 6 months.[46] In half of all programmes, CHWs were exclusively or predominantly female. Reporting of recipient and CHW sample sizes, and therefore CHW to population ratios, was poor.

Eleven studies reported enhanced CHW supervision as an adjunct to the intervention. However, the supervision strategy and frequency were not adequately described. Supervisors included physicians,[46] nurses,[51] accredited social health activists[47 48] or senior project staff[42 50] who monitored CHW activities periodically. Other studies employed a dedicated cadre of CHW supervisors, either based at the facility[40 41] or in the community.[43 44] Eleven studies paid CHWs for their work, with a salary in-line with government standards,[39 43 44 50] a performance-linked[46] or task-based[47 48] remuneration scheme, or some other form of payment.[40 41 45]

CHWs provided services for the range of conditions eligible for inclusion in the review. CHWs in Mali,[42 43] India[46–48] and periurban South Africa[39] provided integrated management of common neonatal and childhood illnesses. CHWs provided care exclusively for diarrhoea in Bangladesh;[49] for pneumonia in Pakistan and Nepal;[50 51] for malaria in Senegal;[45] for malnutrition and at risk of being overweight in Dominican Republic;[52] for tuberculosis in Ethiopia;[44] and for HIV, tuberculosis, and sexually transmitted infections in rural South Africa.[40 41] In addition to proactive case detection, most studies included doorstep treatment by CHWs and referral to a facility if necessary, with the exception of the studies in Dominican Republic, Ethiopia and periurban South Africa,[39 44 52] which limited postdetection activities to referral for treatment and home-based follow-up.

Most studies compared proactive case detection by CHWs to the standard of care—passive case detection at public or private health facilities; six studies also included passive case detection by CHWs in the control arm. The South African studies included control CHWs who conducted home visits for purposes other than proactive case detection. Control arm CHWs conducted one pregnancy and two postnatal home visits to assist with securing identity documents and social grants in the urban study,[39] and home visits to promote and refer clients to HIV counselling and testing in the rural studies.[40 41]

Risk of bias of included studies

Risk of bias summaries are provided in online supplementary file 2 (figure 2 and figure 3). Risk of bias assessments for each study are provided in online supplementary file 3. These assessments were considered when interpreting the results and certainty of evidence for each outcome.

Selection bias

All studies, with the exception of those in Mali,[42 43] allocated the study area into intervention and control groups. Five studies used cluster randomisation to assign groups.[39–41 47 48] Among seven studies that did not use random allocation, sufficient evidence was provided in only two[45 46] that outcome measurements were similar between groups at baseline, and in only three[46 50 52] that population-level and/or cluster-level characteristics were similar between groups at baseline.

Performance bias and detection bias

Due to the nature of the intervention, blinding of participants and study personnel to allocation assignment was not possible and was scored high risk for all included studies. All six Southeast Asian studies[46–51] and the periurban South Africa study[39] blinded outcome assessors to allocation assignment, earning a low detection bias score.

Attrition bias

Reporting of incomplete outcome data varied considerably between studies. Studies involving pregnant women for a neonatal intervention discussed attrition bias with the use of a trial profile.[39 47 48 52] A Data Safety and Monitoring Board stopped the Haryana, India trials early after the required sample size had been met, but prior to about half of children completing the 12-month assessment.[47 48] Risk of attrition bias was high in the Dominican Republic study where roughly a quarter of mother–child dyads were lost, and there were statistically significant differences in some baseline characteristics that could be associated with the outcome between those who completed follow-up and those who did not.[52] Missing survey data for date of birth and death were imputed in the Mali studies, but the extent and patterns of missing data were explicitly reported.[42 43] Studies from India[46] and Nepal[50] did not comment on completeness of outcome data, but data were collected by an independent set of workers and analysed on an intention to treat basis. CBA studies in Pakistan[51] and Senegal[45] relied on CHWs to collect outcome data in intervention clusters and employed periodic surveys in control clusters. These studies did not discuss incomplete outcome data and were scored high risk due to the differences in data source and methods between the two groups.

Reporting bias

A published protocol was found for only one study.[39] No studies reported outcomes in the methods that were then subsequently omitted from the results and, therefore, no studies were scored as being at high risk of reporting bias. Some studies subsequently added outcomes from posthoc analyses, but provided justifiable reasons for inclusion of the additional outcomes that were not prespecified.[39 47 48]

Protection against contamination

Risk of bias due to contamination was scored as low when large units of allocation were chosen and efforts to minimise contamination were discussed and/or a map was provided showing geographic separation of groups.[44 46–50]

Effects of interventions

Eleven studies assessed the effects of proactive case detection of common childhood conditions by CHWs on mortality, morbidity or access to curative services and were included in the main analysis. Meta-analysis was deemed inappropriate as the studies in each analysis represented considerable clinical diversity with respect to intervention and participant characteristics, methodological diversity with respect to study design and risk of bias, and statistical heterogeneity as quantified by the I^2 statistic. We were unable to explore this heterogeneity by prespecified subgroup analyses due to the limited number of studies. Overall, the certainty of evidence is low or very low because of limitations in study design, indirect measures of effect due to cointerventions or comparisons and unexplained heterogeneity.

Mortality

Seven studies measured mortality outcomes (table 2; Figure 1). Proactive case detection may reduce neonatal mortality (low certainty evidence). However, the effects vary and it is possible that it makes little or no difference to neonatal mortality (calculated RRs: 0.43 to 1.07; I^2=79.1%). Proactive case detection may reduce infant mortality (calculated RRs: 0.52 to 0.94; I^2=61.9%) (low certainty evidence). It is uncertain whether proactive case detection reduces mortality among children under 5 years (calculated RRs: 0.04 to 0.80; I^2=94.4%) because the certainty of this evidence is very low.

Three studies assessed impact on neonatal mortality over a 2–3 year timeframe (table 2; Figure 1). It was the primary outcome in the Maharashtra[46] and Haryana[47] studies of proactive case detection of newborn and infant danger signs, infections and illnesses. In rural Maharashtra, there was a 62% reduction in intervention areas compared with control areas (p<0.001).[46] In Haryana, the neonatal mortality rate beyond the first 24 hours of life was lower in intervention clusters than in control clusters (adjusted HR=0.86; 95% CIs: 0.79 to 0.95), but not the case for the neonatal mortality rate overall—an effect, they explained, due to the higher than expected proportion of neonatal deaths occurring in the first 24 hours on which the intervention was unlikely to have had an effect.[47] In both Maharashtra and Haryana, intervention groups included a mother's education component and system strengthening in terms of user fee removal for CHW care[46] or training of other provider cadres in Integrated Management of Newborn and Childhood Illnesses.[47] An exploratory analysis of the effect of a home visit programme in periurban South Africa to improve appropriate infant feeding and HIV-free infant survival[39] on neonatal mortality showed an increased risk of death in intervention compared with control clusters, although the effect was not statistically significant (RR=1.07; 95% CIs: 0.69 to 1.63).

Four Southeast Asia studies assessed infant mortality. The Maharashtra[46] and Haryana[47] studies found significant reductions (respectively, 45.7%; p<0.001 and AHR=0.89; 95% CIs: 0.78 to 1.00) in infant mortality between intervention and controls. Proactive case detection of childhood respiratory infection and doorstep treatment of suspected pneumonia compared with facility-based care led to reductions in infant mortality in rural Nepal,[50] where cotrimoxazole was provided at home free of charge, and in rural Pakistan,[51] where CHWs treated at home or referred to facilities where treatment protocols had been standardised. In Nepal, the greatest reduction in mortality after 3 years of intervention activities was seen in infants aged 6–11 months (RR=0.36; 95% CIs: 0.24 to 0.56). In Pakistan,[51] the infant mortality rate was 74/1000 in the intervention area during the first 2 years of the study compared with 93/1000 in the control area.

A reduction in mortality was seen for all children under 5 years of age in Nepal, with a relative risk reduction of 0.72 from baseline to year 3,[50] and in Pakistan, with a 26% reduction between intervention (29/1000) and control (39/1000) areas during the first 2 years of the study.[51] In periurban Mali, the under-5 mortality rate declined from 154/1000 at baseline to 25/1000 after 3 years of proactive case detection of common childhood conditions in addition to primary health centre reinforcements and removal of user fees, and to 7/1000 after 7 years.[43]

Morbidity

Six studies assessed prevalence of disease, and four assessed hospitalisation (table 3; Figure 2). Proactive case detection may improve nutritional outcomes (low certainty evidence), although the effects vary, and it is possible that it makes little or no difference to nutritional outcomes (calculated RRs range from 0.61 to 1.16; I^2=61.4%). It is uncertain whether proactive case detection reduces the prevalence of infectious diseases (calculated RRs: 0.06 to 1.02; I^2=90.6%) or hospitalisation (calculated RRs: 0.38 to 1.26; I^2=94.5%) because the certainty of this evidence is very low.

In Mali[42 43] and rural Senegal,[45] proactive case detection of malaria led to significant reductions in the odds of febrile illness among children under five (adjusted OR (AOR) after 7 years=0.45; 95% CIs: 0.32 to 0.62), and symptomatic malaria among the general population in intervention villages compared with control

Table 2 Intervention effects on mortality outcomes

Country	Design*	Reported measure of effect (95% CIs)†	Calculation of risk‡	Calculated RR§
Neonatal mortality				
India[46]¶	CBA	% diff=62.2%; p<0.001	I: 25/979 C: 66/1108	0.43 (0.27, 0.67)
India[47]¶	cRCT	AHR=0.91 (0.80 to 1.03)	I: 1244/29667 C: 1326/30813	0.97 (0.71, 1.33)
SA[39]	cRCT	RR=1.07 (0.69 to 1.63)	I: 20/1821 C: 22/2136	1.07 (0.58, 1.95)
Infant mortality				
India[46]	CBA	% diff=45.7%; p<0.001	I: 38/979 C: 83/1108	0.52 (0.36, 0.75)
India[47]¶	cRCT	AHR=0.89 (0.78 to 1.00)	I: 1925/29667 C: 2136/30813	0.94 (0.73, 1.20)
Nepal[50]	BA	0 to 6 days: RR=0.80 (0.59, 1.10) 0.25 to 5 months: RR=0.74 (0.58, 0.94) 6 to 11 months: RR=0.36 (0.24, 0.56)	I: 236/13406 C: 199/6684	0.60 (0.37, 0.96)
Pakistan[51]¶	cNRCT	% diff=21%; 'not significant'	I: 108/4665 C: 31/1194	0.87 (0.52, 1.46)
Child mortality				
Mali[42]	BA	HR=0.10; p<0.0001	I: 29/1390 C: 38/316	0.17 (0.11, 0.28)
Mali[43]	BA	HR=0.039 (0.013 to 0.116)	I: 5/1023 C: 39/330	0.04 (0.02, 0.10)
Nepal[50]¶	BA	RR=0.72 (0.63 to 0.82)	I: 409/13406 C: 301/6684	0.67 (0.46, 0.98)
Pakistan[51]¶	cNRCT	% diff=26%; p<0.001	I: 149/4665 C: 47/1194	0.80 (0.52, 1.22)

Neonatal period reported is 0–27 days. Infant period is 0–11 months. Child mortality period is 0–59 months. India[46] also reports mortality separately for early (0–6 days) neonates: % diff=57.3%; p<0.001; calculated RR=0.45, and late (7–27 days) neonates: % diff=51.6%; calculated RR=0.31. Study also found a reduction in perinatal mortality % diff=71.0%; p<0.001. A 2005 summary of this field trial reports that reductions in neonatal mortality and infant mortality reached 70% (95% CIs: 59, 81%) and 57% (95% CIs: 46, 68%), respectively, after 8 years postintervention.[65] India[47] also reports mortality for neonates after the first day of life: AHR=0.86 (0.79 to 0.95); calculated RR=0.93. Study also found a reduction in perinatal (AHR=0.89; 95% CIs: 0.78 to 1.00) and postneonatal (AHR=0.76; 95% CIs: 0.67 to 0.85) mortality. Nepal[50] reports no overall infant mortality, only by infant age brackets; denominators for calculated infant and childhood risks are based on study report that initial census registered[66] 84 children (control) and an additional 6722 were born during the study for a total of 13 406 children available (intervention). Pakistan[51] compares mortality rates between intervention and control periods for the 1985–1986 postintervention period; calculated risks are for 1985 only for which the study reports number of children per arm. Nepal[50] and Pakistan[51] also report disease-specific mortality rates; results not shown. The South Africa[39] study found no effect (RR=0.97; 95% CIs: 0.67 to 1.40) on the primary joint mortality–morbidity outcome: HIV-free infant survival at 12 weeks among HIV-positive mothers.

*The study design reported is the nature of the comparative data, not necessarily the design as described by study authors.

†The before–after (BA) studies[42 43 50] reported each annual time point compared with baseline; here we present end-line to baseline risk ratios.

‡Reviewer (CW) calculated risk of death for intervention (I) and comparison (C) groups by taking number of events over number of live births (or, if unavailable, over population). For CBA, cRCT and cNRCT study designs, risks were calculated and compared (ie, calculated risk ratio) for the postintervention period between intervention and control groups; for BA study designs, intervention risk was calculated at end-line and control risk at baseline.

§Risk ratios and 95% CIs are adjusted for clustering.

¶Study primary outcome(s).

AHR, adjusted HR; BA, before–after; CBA, controlled before–after; cNRCT, cluster non-randomised controlled trial; cRCT, cluster randomised controlled trial; RR, risk ratio.

villages (AOR=0.03; 95% CIs: 0.02 to 0.07), respectively. The Haryana[48] study found significant reductions in danger signs (adjusted RR (ARR)=0.82; 95% CIs: 0.67 to 0.99) and local infection (ARR=0.91; 95% CIs: 0.71 to 1.17) among neonates, as well as diarrhoea (ARR=0.63; 95% CIs: 0.49 to 0.80) and pneumonia (ARR=0.60; 95% CIs: 0.46 to 0.78) among infants. The urban South Africa[39] and Dominican Republic[52] studies found no effects on childhood diarrhoea, a secondary intervention outcome.

The Dominican Republic[52] study found that monthly home visits and mother's groups to promote healthy

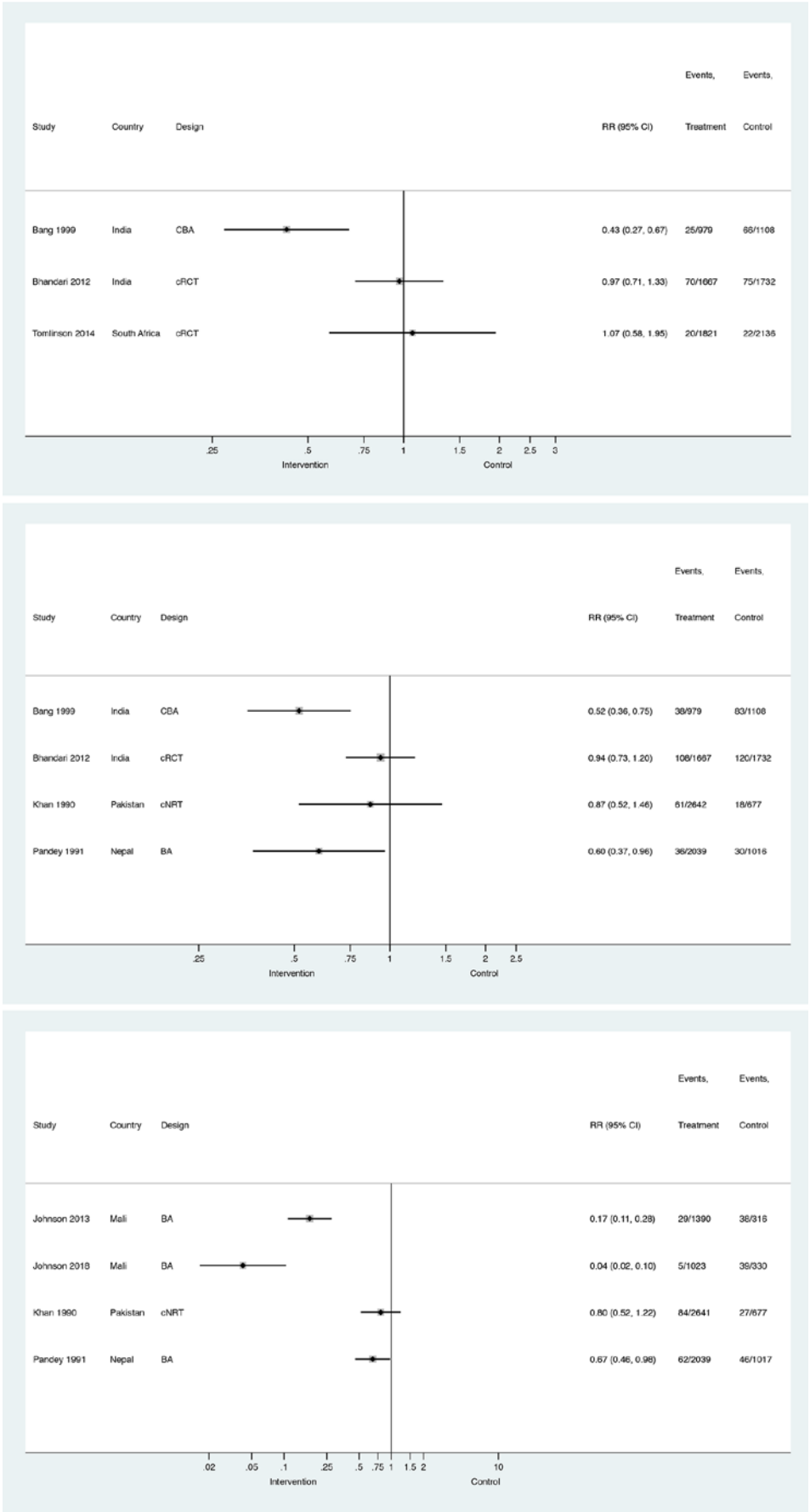

Figure 1 Forest plots for neonatal (top), infant (middle) and under 5 (bottom) mortality. CBA, controlled before–after; RR, risk ratio.

Table 3 Intervention effects on morbidity and access to care outcomes

Country	Design*	Population/condition†	Reported measure of effect (95% CIs)‡	Calculated RR (95% CIs)§
Prevalence of infectious diseases¶				
DR[52]	cNRCT	Diarrhoea, children under two	AOR=0.99 (0.59 to 1.67)	0.95 (0.61 to 1.47)
India[48]	cRCT	Infant** diarrhoea	ARR=0.63 (0.49 to 0.80)	0.63 (0.54 to 0.74)
India[48]	cRCT	Infant** pneumonia	ARR=0.60 (0.46 to 0.78)	0.56 (0.40 to 0.77)
Mali[42]	BA	Childhood febrile illness	PR=0.61; p<0.001	0.61 (0.51 to 0.73)
Mali[43]	BA	Childhood febrile illness	AOR=0.45 (0.32 to 0.62)	0.57 (0.47 to 0.68)
Senegal[45]††	CBA	Malaria, all ages	AOR=0.03 (0.02 to 0.07)	0.06 (0.02 to 0.18)
SA[39]	cRCT	Infant diarrhoea at 12 weeks	RR=1.01 (0.90 to 1.14)	1.02 (0.90 to 1.16)
Prevalence of nutritional outcomes‡‡				
DR[52]††	cNRCT	Stunting, children under 2	AOR=0.50 (0.22 to 1.10)	0.61 (0.33 to 1.11)
DR[52]††	cNRCT	Overweight, children under 2	AOR=0.43 (0.23 to 0.77)	0.69 (0.47 to 1.03)
DR[52]††	cNRCT	LAZ scores, children under 2	MD=0.21 (-0.02 to 0.44)	NA
DR[52]††	cNRCT	BAZ scores, children under 2	MD=−0.31 (-0.49 to -0.12)	NA
India[48]	cRCT	Infant stunting	ARR=0.99 (0.94 to 1.04)	1.03 (0.93 to 1.14)
India[48]	cRCT	Infant wasting	ARR=1.10 (0.90 to 1.36)	1.16 (0.93 to 1.46)
SA[39]	cRCT	Infant LAZ scores at 12 weeks	MD=0.11 (0.03 to 0.19)	NA
SA[39]	cRCT	Infant WLZ scores at 12 weeks	MD=0.01 (-0.07 to 0.09)	NA
SA[39]	cRCT	Infant WAZ scores at 12 weeks	MD=0.09 (0.00 to 0.18)	NA
Hospitalisation§§				
Bangladesh[49]††	cNRCT	For diarrhoea, all ages	% diff=29%; p<0.01	0.38 (0.34 to 0.41)
DR[52]	cNRCT	During first 2 years of life	AOR=1.09 (0.70 to 1.68)	1.07 (0.77 to 1.49)
India[48]	cRCT	During infancy**	ARR=0.67 (0.51 to 0.88)	0.65 (0.46 to 0.91)
SA[39]	cRCT	For infant diarrhoea at 12 weeks	RR=1.28 (0.75 to 2.19)	1.26 (0.67 to 2.39)
Access to effective¶¶ treatment				
DR[52]	cNRCT	Diarrhoea, children under two	AOR=3.86 (1.14 to 13.02)	1.29 (0.79 to 2.12)
India[48]††	cRCT	Infant** diarrhoea	ARR=1.22 (1.06 to 1.42)	1.25 (1.11 to 1.41)
India[48]††	cRCT	Infant** pneumonia	ARR=1.44 (1.00 to 2.08)	1.24 (0.71 to 2.14)
Access to prompt treatment***				
India[48]††	cRCT	Infant** diarrhoea	ARR=0.99 (0.89 to 1.10)	1.00 (0.88 to 1.14)
India[48]††	cRCT	Infant** pneumonia	ARR=1.10 (0.96 to 1.25)	1.01 (0.84 to 1.22)
Mali[42]††	BA	Childhood malaria	PR=1.89; p=0.0195	1.89 (1.18 to 3.05)
Mali[43]††	BA	Childhood malaria	AOR=3.20 (1.75 to 5.85)	2.39 (1.49 to 3.83)

*The study design reported is the nature of the comparative data in this review.
†Neonatal period is 0–27 days, infant period is 0–11 months and childhood is under 5 years of age, unless otherwise indicated.
‡The BA studies[42 43 50] reported each annual time point compared with baseline; here we present effect estimates comparing end-line to baseline.
§For CBA, cRCT and cNRCT study designs, risks were calculated and compared for the postintervention period between intervention and control groups; for BA designs, intervention risk was calculated at end-line and control risk at baseline. Risk ratios and 95% CIs are adjusted for clustering.
¶For the Dominican Republic,[52] India,[48] Mali[42 43] and South Africa[39] studies, prevalence based on mother's reporting of condition during 2 weeks period preceding the interview; for the Senegal[45] study, prevalence measured at each time point by positive rapid diagnostic test of symptomatic community members.
**The India[48] study also reported effects of similar magnitude at 6 months of age; results not shown. Study found a reduction in neonatal morbidity: danger signs (ARR=0.82; 95% CIs: 0.67 to 0.99) and infection (ARR=0.91; 95% CIs: 0.71 to 1.17), and an increase in access to care for neonates: treatment by appropriate provider for danger signs (ARR=1.76; 95% CIs: 1.36 to 2.24), prompt treatment for danger signs (ARR=1.14; 95% CIs: 1.10 to 1.18), treatment by appropriate provider for infections (ARR=4.86; 95% CIs: 3.80 to 6.21) and prompt treatment for infections (ARR=1.97; 95% CIs: 1.71 to 2.27).
††Study primary outcome(s).
‡‡Based on anthropometric measures for all studies.
§§Measure based on mother's recall for Dominican Republic[52] (last 12 months), India[48] (last 3 months) and South Africa[39] (recall period not specified) studies; for the Bangladesh[49] study, measure based on hospital records. CHWs in the Dominican Republic[52] and South Africa[39] studies did not provide doorstep treatment but referred all cases detected; CHWs in the Bangladesh[49] and India[48] studies provided doorstep treatment and referral.
¶¶Defined for the Dominican Republic[52] study as oral rehydration for childhood diarrhoea, and for the India[48] study as treatment from an appropriate provider, which included physicians in government and private facilities, auxiliary nurse midwife, Anganwadi worker (CHW) or ASHA.[48]
***Defined as treatment within 24 hours of symptom onset for all studies.
AOR, adjusted OR; ARR, adjusted risk ratio; ASHA, accredited social health activists; BA, before–after; BAZ, Body Mass Index-for-age; CBA, controlled before–after; CHW, community health worker; cNRCT, cluster non-randomised controlled trial; cRCT, cluster randomised controlled trial; LAZ, length-for-age; MD, mean difference; NA, not applicable; RR, risk ratio; WAZ, weight-for-age; WLZ, weight-for-length.

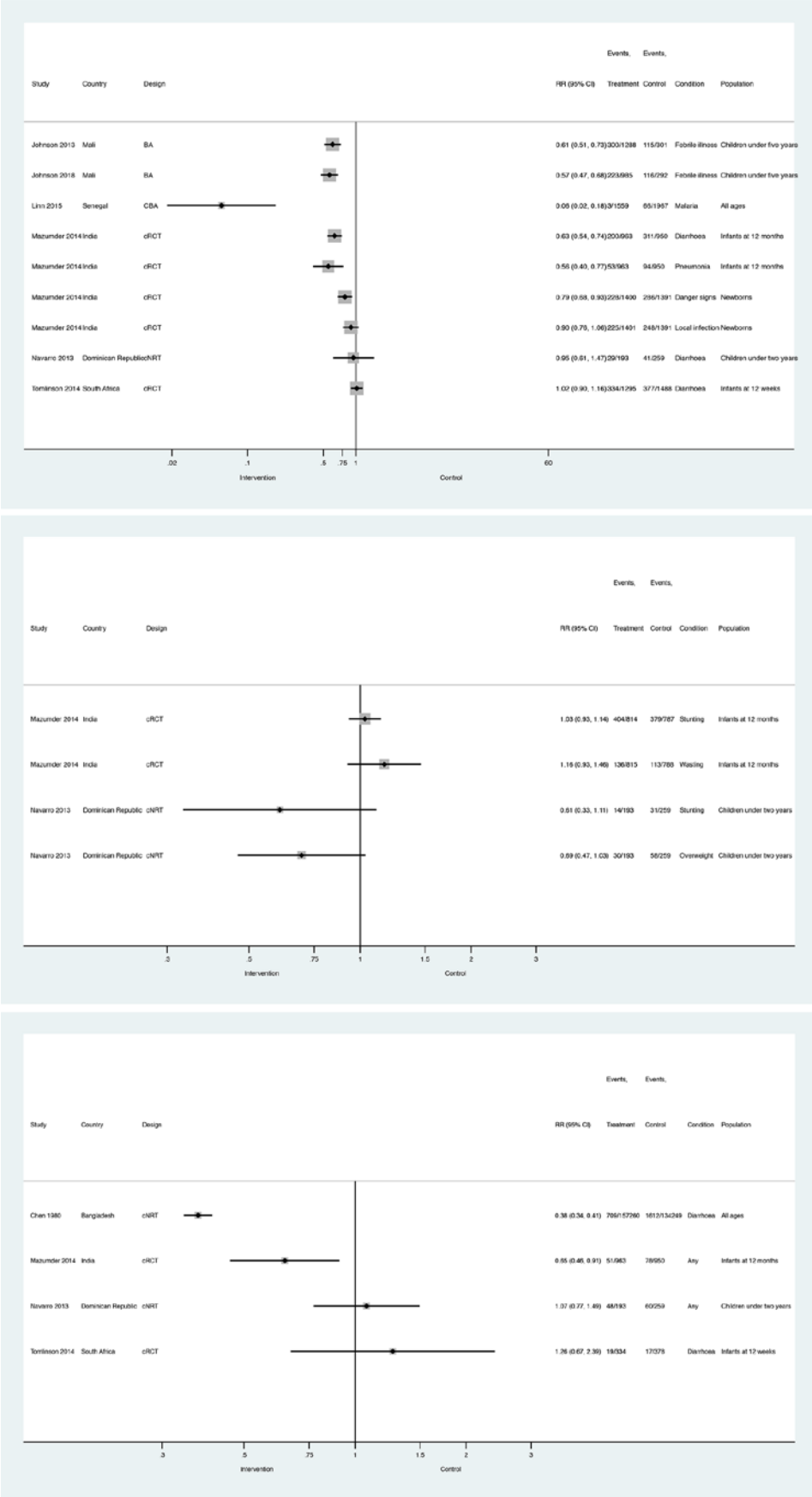

Figure 2 Forest plots for prevalence of common childhood infections (top) and nutritional conditions (middle), and hospitalisation (bottom). BA, before–after; CBA, controlled before–after; RR, risk ratio.

babies and monitor physical growth during the first 2 years of life led to reductions in stunting (AOR=0.50; 95% CIs: 0.22 to 1.10) and risk of overweight (AOR=0.43; 95% CIs: 0.23 to 0.77), compared with standard facility-based controls. The Haryana[48] study found no effect on wasting (ARR=0.99; 95% CIs: 0.94 to 1.04) or stunting (ARR=1.10; 95% CIs: 0.90 to 1.36) at 12 months of age in exploratory analyses. The South Africa[39] study found an increase in infant weight-for-age (mean difference (MD)=0.09; SD: 0.00, 0.18) and length-for-age (MD=0.11; SD: 0.03, 0.19) z-scores, but not weight-for-length (MD=0.01; SD: -0.07, 0.09).

In Bangladesh,[49] CHW home visits to inquire about diarrhoea and offer oral rehydration therapy packets free of charge were associated with a 29% reduction (p<0.01) in hospitalisation for diarrhoea compared with control villages with CHWs doing 'surveillance and health work'. In the Haryana[48] study, in which CHWs assessed newborns for signs of illness at each visit and treated or referred them, caregivers in the intervention clusters reported fewer hospital admissions during infancy (ARR=0.67; 95% CIs: 0.51 to 0.88). In the South Africa[39] and Dominican Republic[52] studies, where proactive CHWs did not offer doorstep treatment but referred all cases detected, caregivers reported more hospital admissions for their children, although results were not statistically significant.

Access to treatment

Four studies assessed access to effective and/or prompt treatment (table 3; Figure 3). Proactive case detection may increase access to effective treatment (calculated RRs range from 1.59 to 4.64; I^2=97.0%) (low certainty evidence). It is uncertain whether proactive case detection increases access to prompt treatment (calculated RRs range from 1.00 to 2.39; I^2=84.9%) because the certainty

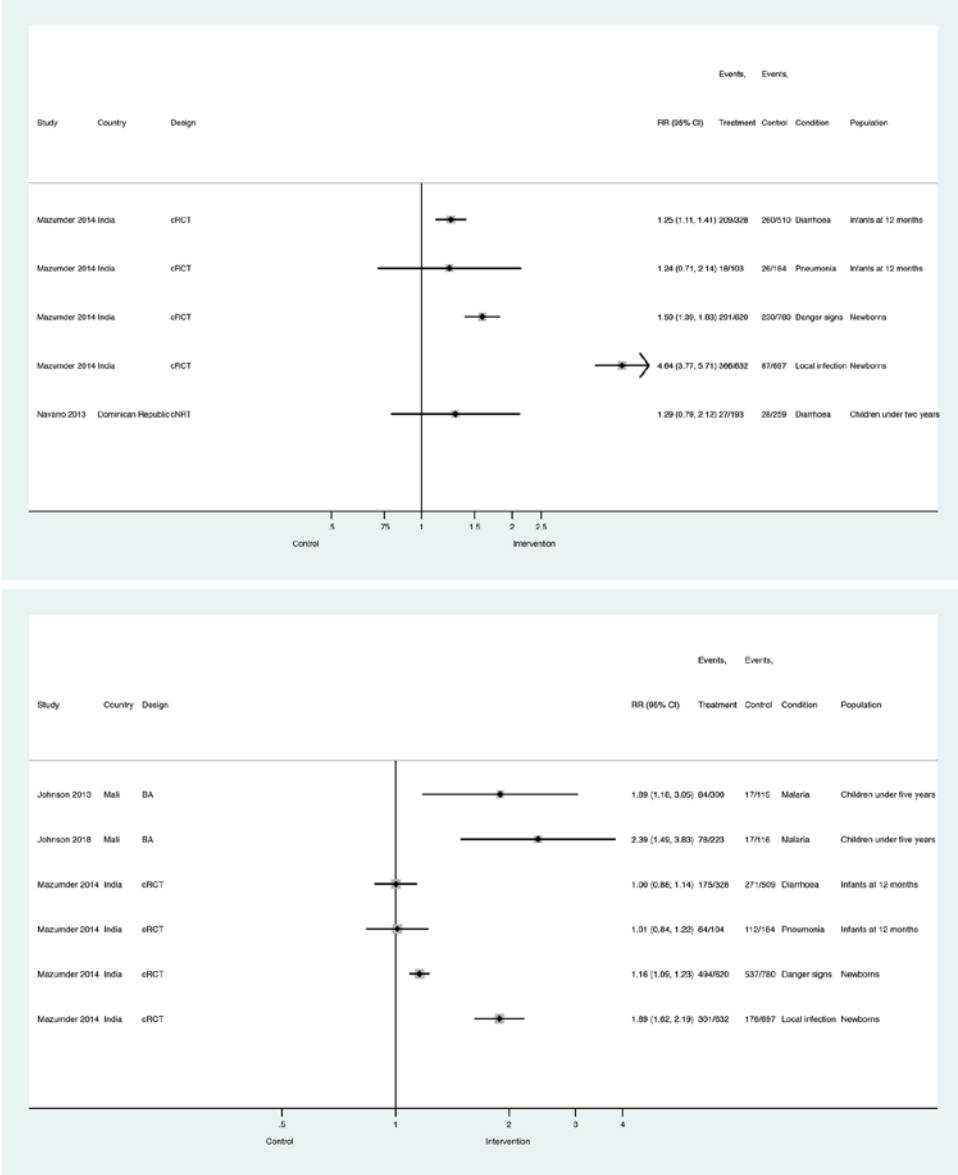

Figure 3 Forest plots for access to effective treatment (top) and prompt access to treatment (bottom). RR, risk ratio.

of this evidence is very low. Three studies assessed the effects of proactive case detection of HIV and/or tuberculosis on access to diagnostic services and/or treatment adherence support; these were excluded from the main analysis and summarised in online supplementary file 4.

In Dominican Republic,[52] proactive home visits increased the proportion of diarrhoeal children who received oral rehydration solution (AOR=3.86; 95% CIs: 1.14 to 13.02). In Haryana,[48] caregivers in intervention clusters were more likely to seek any treatment within 24 hours and treatment from an appropriate provider for newborns with danger signs (respectively, ARR=1.14; 95% CIs: 1.10 to 1.18 and ARR=1.76; 95% CIs: 1.36 to 2.24) and local infections (respectively, ARR=1.97; 95% CIs: 1.71 to 2.27 and ARR=4.86; 95% CIs: 3.80 to 6.21). Caregivers were no more likely to seek any treatment within 24 hours for infants with diarrhoea (ARR=0.99; 95% CIs: 0.89 to 1.10) or pneumonia (ARR=1.10; 95% CIs: 0.96 to 1.25), but more likely to seek treatment from an appropriate provider for diarrhoea (ARR=1.22; 95% CIs: 1.06 to 1.42) or pneumonia (ARR=1.44; 95% CIs: 1.00 to 2.08). In Mali,[42 43] a higher proportion of children with fever received antimalarial treatment within 24 hours of symptom onset compared with baseline (AOR=3.20; 95% CIs: 1.75 to 5.85).

DISCUSSION

Summary and quality of evidence

This review identified 14 studies of 11 different interventions involving proactive case detection of common childhood conditions by CHWs in nine LMICs. Findings are summarized in table 4. Proactive case detection may reduce infant mortality and increase access to effective treatment compared with conventional community-based healthcare delivery (low certainty evidence). Although our review suggests that proactive case detection may also reduce mortality among children under 5 years, prevalence of infectious diseases, hospitalisation and improve access to prompt treatment, it is uncertain because the certainty of this evidence is very low. Proactive case detection may reduce neonatal mortality and improve nutritional outcomes (low certainty evidence), although effects vary and it is possible that it makes little or no difference to these outcomes.

Three high-quality studies from India[46–48] provide evidence that proactive case detection of illnesses among newborns and infants reduced neonatal and infant mortality, morbidity, and improve treatment seeking, compared with a conventional community-based approach. Two moderate quality studies in Senegal[45] and Bangladesh[49] found that proactive case detection and doorstep treatment significantly reduced population-level morbidity, as measured by the prevalence of malarial fever and hospitalisation for diarrhoea, respectively. In these five studies, control groups received passive case detection and management from community-based CHWs and primary health facilities. This provides a more direct assessment of the effectiveness of proactive case detection than studies that had no CHWs in control clusters (which are likely to overestimate its effects) as well as studies with control CHWs who conduct home visits for other purposes (which are likely to underestimate its effects). Activities in control clusters may partially explain the null effects on neonatal mortality and infant morbidity found in the periurban South Africa cluster RCT.[39] Home visits by control CHWs for the purpose of procuring identity documents and social grants may have served in practice to proactively identify sick children and encourage caregivers to seek care.

Our review extracted all study outcomes that met our inclusion criteria, even if those outcomes were the result of exploratory or posthoc analyses. This may account for some of the null effects in studies that reported numerous outcomes for which the study was not powered or for which the intervention had no clear pathway for impact. For example, finding no effect on prevalence of diarrhoea for visits targeting nutrition,[52] and no effect on stunting for visits to detect disease in infants were the results of exploratory analyses and small sample sizes.[47]

Although this review found large inconsistencies in results for hospitalisation, the two studies in which CHWs provided doorstep treatment found a significant reduction,[47 49] whereas the two urban studies[39 52] in which all cases were referred found an increase (although statistically not significant), as might be expected. These were the only studies included in the main analyses in which CHWs did not offer doorstep treatment following proactive detection of uncomplicated cases. In the studies concerning HIV and/or tuberculosis, CHWs referred cases detected and then conducted follow-up home visits for treatment adherence support.

Most studies evaluated complex interventions with multiple components, limiting our ability to draw conclusions about the isolated effects of proactive case detection. At a minimum, all studies likely included—whether or not explicit in the intervention description—health promotion and education messaging by CHWs at the time of home visitation, the benefits of which on child health have been documented.[55–57] Other cointerventions included additional support to proactive CHWs in the form of supervision and/or remuneration; systems strengthening such as facility-level improvements and/or user fee removal; community mobilisation and/or women's groups. Studies that found the intervention effective, such as those in India, Senegal, Bangladesh and Mali, offered more in terms of supportive cointerventions, suggesting these are important design features of successful CHW programmes.

Overall, the quality of studies evaluating proactive case detection was poor. Our review identified only three cluster RCTs that evaluated mortality, morbidity or access to treatment; two of which were the same trial reporting different outcomes.[47 48] Our results show clear design effect, with studies at higher risk of bias showing a larger magnitude of effect than the RCTs (tables 2 and

Table 4 Summary of findings for the main analysis

Proactive case detection of common childhood illnesses by CHWs compared with usual care for reducing mortality and morbidity and improving access to care in children under 5 years of age

Participants: children under 5 years of age accessing primary health services in LMICs
Settings: India (three studies), Mali (two studies), Bangladesh, Dominican Republic, Nepal, Pakistan, Senegal, South Africa
Intervention: home visits by CHWs for proactive detection of common childhood illnesses
Comparison: usual primary care (passive case detection) available from facilities and, in some cases, CHWs

Outcomes	Relative risk	Number of studies	Certainty of the evidence (GRADE)*	Comments
Neonatal mortality Verbal reports Follow-up: 0–12 months	0.43 to 1.07	3†	⨁⨁◯◯ Low‡§	Two Indian studies found proactive case detection of newborn illnesses reduced mortality, although only the non-randomised evidence was statistically significant. Proactive case detection may reduce neonatalmortality. However effects vary, and it is possible that it makes littleor no difference to this outcome.
Infant mortality Verbal reports Follow-up: 0–36 months	0.52 to 0.94	4¶	⨁⨁◯◯ Low**††	Four Southeast Asia studies found reductions in infant mortality, although not all were statistically significant. Two studies targeted various infant conditions, and two specifically targeted pneumonia among children under 5. Proactive case detection may reduce infantmortality.
Child mortality Verbal reports Follow-up: 0–84 months	0.04 to 0.80	4‡‡	⨁◯◯◯ Very low††§§¶¶	Four studies found important reductions in under-5 mortality, although three were uncontrolled before–after analyses. It is uncertain whether proactive case detectionreduces mortality among children under 5.
Prevalence of infectious diseases Verbal reports, diagnostic tests Follow-up: 0–24 months	0.06 to 1.02	6***	⨁◯◯◯ Very low†††‡‡‡§§§¶¶¶	Three West African studies found significant reductions in fever or malarial fever. One study found reductions in both newborn and infant illnesses. Two studies found no effect on child diarrhoea, a secondary intervention outcome. It is uncertain whether proactive case detectionreduces the prevalence of infectious diseases.
Prevalence of nutritional outcomes Athropometric measurement Follow-up: 0–24 months	0.61 to 1.16	3****	⨁⨁◯◯ Low§§§††††	One study targeted childhood nutrition and found positive effects on length and BMI for age. Two studies that targeted various infant conditions found a range of nutritional effects. Proactive case detection may improve nutritional outcomes, although it is possible that it makes littleor no difference to this outcome.
Hospitalisation Verbal reports, hospital records Follow-up: 0–24 months	0.38 to 1.26	4‡‡‡‡	⨁◯◯◯ Very low§§§††††§§§§¶¶¶¶	Hospitalisation may reflect a higher severity of illness, improved treatment seeking, or both. In the two studies where CHWs provided doorstep treatment, hospitalisation significantly declined. In the two studies where all cases detected by CHWs were referred, hospitalisation increased, although results were not statistically significant. It is uncertain whether proactive case detectionreduces hospitalisation.
Access to effective treatment Verbal reports Follow-up: 0–24 months	1.59 to 4.64	2*****	⨁⨁◯◯ Low§§§,††††,†††††,	One study found that treatment was sought more often from an appropriate provider for neonatal illness and infection, and infant diarrhoea and pneumonia. One childhood nutritional intervention improved administration of ORS during diarrhoea. Proactive case detection may increase access to effective treatment.
Access to prompt treatment Verbal reports Follow-up: 0–84 months	1.00 to 2.39	3‡‡‡‡‡	⨁◯◯◯ Very low§.§§§,§§§§§,¶¶¶¶¶	One study found a significant improvement in the speed of treatment for newborns, but no effect for infants with diarrhoea and pneumonia. Uncontrolled before–after analyses in Mali found that the risk of prompt antimalarial treatment among children more than doubled. It is uncertain whether proactive case detectionimproves access to prompt treatment.

⨁⨁⨁⨁ High: this research provides a very good indication of the likely effect. The likelihood that the effect will be substantially different‡ is low.
⨁⨁⨁◯ Moderate: this research provides a good indication of the likely effect. The likelihood that the effect will be substantially different‡ is moderate.
⨁⨁◯◯ Low: this research provides some indication of the likely effect. However, the likelihood that it will be substantially different‡ is high.
⨁◯◯◯ Very low: this research does not provide a reliable indication of the likely effect. The likelihood that the effect will be substantially different‡ is very high.

‡Substantially different=a large enough difference that it might affect a decision.
*GRADE, Working Group grades of evidence.
†Bang 1999, Bhandari 2012, Tomlinson 2014.
‡The quality of evidence was downgraded for indirectness as the interventions in Bang 1999 and Bhandari 2012 included components other than proactive case detection, and comparison CHWs in Tomlinson 2014 conducted home visits for other purposes.
§The 95% CIs included both no effect and appreciable benefit.
¶Bang 1999, Bhandari 2012, Khan 1990, Pandey 1991.
**Risk of bias was assessed as low for Bang 1999 and Bhandari 2012. Khan 1990 and Pandey 1991 employed inappropriate analytical methods for their study designs. The quality of evidence was therefore downgraded for this limitation.
††The quality of evidence was downgraded for indirectness as the interventions in all studies included components other than proactive case detection, such as removal of user fees, facility-level capacity building, and/or women's education.
‡‡Johnson 2013, Johnson 2018, Khan 1990, Pandey 1991.
§§The quality of evidence was downgraded for risk of bias in Johnson 2013 and 2018 (shape of preintervention period not established and no control area for comparison) and in Khan 1990 and Pandey 1991 (inappropriate analyses for study designs).
¶¶The quality of evidence was downgraded for important inconsistency in results; many of the 95% CIs were not overlapping. Heterogeneity may be explained by differences in follow-up times (up to 84 months in Johnson 2018) and/or diseases targeted (only respiratory infection in Khan 1990 and Pandey 1991).
***Johnson 2013, Johnson 2018, Linn 2015, Mazumder 2014, Navarro 2013, Tomlinson 2014.
†††The quality of evidence was downgraded for limitations in design in Johnson 2013 and 2018 (shape of preintervention period not established and no control area for comparison), Navarro 2013 (outcome assessors not blind; high loss to follow-up and differences between lost and retained participants) and Linn 2015 (outcome assessors not blind; potential confounders not controlled for in analysis).
‡‡‡The quality of evidence was downgraded for important inconsistency in results. However, in the studies that showed a significant reduction in prevalence, the intervention targeted the diseases assessed for this outcome; in Navarro 2013 and Tomlinson 2014, CHWs conducted proactive case detection and management of conditions (nutritional and newborn, respectively) other than those assessed for this outcome.
§§§The quality of evidence was downgraded for indirectness due to cointerventions and differences in age groups.
¶¶¶The quality of evidence was upgraded for very large effects found in Linn 2015; weak study design may not explain all of the observed effect.
****Mazumder 2014, Navarro 2013, Tomlinson 2014.
††††Risk of bias assessed as low for Mazumder 2014, unclear for Tomlinson 2014 (no baseline measurement of outcomes; unclear risk of contamination), and high for Navarro 2013 (outcome assessors not blind; high loss to follow-up and differences between lost and retained participants).
‡‡‡‡Chen 1980, Mazumder 2014, Navarro 2013, Tomlinson 2014. In Chen 1980, the intervention targeted and was assessed for the entire population; results not reported in such a way that the outcome could be calculated separately for children under 5.
§§§§Chen 1980 is a non-randomised trial with suggestion of important baseline imbalances in outcomes and confounding factors, although no statistical comparisons made at baseline. The quality of evidence was therefore downgraded for limitations in design.
¶¶¶¶Serious inconsistency in results, although likely due to whether CHWs provided doorstep treatment (likely to reduce hospitalisation), or referral only (likely to increase hospitalisation). The quality of evidence was therefore not downgraded for inconsistency, but for imprecision due to very wide 95% CIs.
*****Mazumder 2014, Navarro 2013.
†††††The quality of evidence was downgraded for imprecision as studies had very wide 95% CIs.
‡‡‡‡Johnson 2013, Johnson 2018, Mazumder 2014.
§§§§Risk of bias was assessed as low for Mazumder 2014, but high for Johnson 2013 and 2018 (shape of preintervention period not established and no control area for comparison). The quality of evidence was therefore downgraded for this limitation in design.
¶¶¶¶The quality of evidence was downgraded for important inconsistency in results; however, it may be explained by differences in intervention, target populations and follow-up times.
BMI, body mass index ; CHWs, community health worker; GRADE, Grading of Recommendations, Assessment, Development and Evaluation; LMICs, low-income and middle-income countries; ORS, oral rehydration solution.

3; Figures 1 and 2). Risk of bias was higher still where inappropriate analytical methods were employed for the study design.[50 51] Additionally, studies published before the year 2000 did not account for clustering in their analytical approaches.[46 49–51]

Limitations

Our synthesis of evidence was limited by the small number of eligible studies, and the considerable diversity between them. With only 11 studies included in the main analyses, we were unable to conduct subgroup analyses that would have tested for differences in effectiveness by features in study and intervention design, including setting, CHW characteristics, target populations, diseases detected or frequency of home visits. We could not explore how different health conditions in different transmission settings or health system contexts would have differential impacts on outcomes. We were also unable to assess publication bias due to the limited number of studies. However, our review included large trials reporting statistically non-significant results, so there are no specific reasons for suspecting a high risk of publication bias.

Our synthesis was further limited by inadequate reporting of methods and results in some studies. We had to make some assumptions in order to calculate a principal summary measure for between study comparisons, such as approximating the denominator or postulating the ICC. Features of CHW intervention design and implementation, including CHW recruitment and training, support and supervision and health system integration, were inadequately described. Comparisons were also inadequately described, making it difficult to understand the differences between the two groups. In some cases, it was not clear whether the control included CHWs at all,[44] what services were offered by control CHWs, including whether they conducted home visits for other purposes,[40 41 49] or whether they received the additional support, such as supervision or payment, offered to intervention CHWs.[45]

As there is no universally adopted terminology or strong indexation in health databases for the concept of proactive case detection, it is possible that some published or unpublished evaluations meeting the inclusion criteria were not identified through the search. There is a large body of evidence for the mortality, morbidity and access to care impacts of comprehensive community-based primary healthcare interventions,[58 59] including household and community integrated management of childhood illness[60–62] that may include home visits by community-based providers for the purpose of health promotion and education, vital registration and/or proactive case detection. Some of these studies[56 57 63] may not have been included because insufficient information was available about the role of home visits in disease detection, study designs did not permit comparisons based on workflow and/or study designs were not sufficiently rigorous.

Implications for research and practice

The review process to inform the WHO guidelines for optimising CHW programmes found a scarcity of evidence for several areas reviewed, including recruitment and training, supervision and management, and health system integration.[29 64] Our review synthesising evidence around CHW workflow yielded similar conclusions regarding inadequate reporting of programme characteristics and lack of robust evidence. These features merit further consideration by programme architects and evaluators.

Standardising impact metrics for evaluating CHW programmes would greatly facilitate the synthesis of evidence in this field. Possible impact metrics include mortality among vulnerable groups, morbidity, as measured by disease prevalence, and access to prompt, effective treatment. Researchers should also consider process outcomes that provide an understanding of why and how a complex intervention did or did not work. None of the studies identified through the search provided a comparative costing analysis, or reported adverse effects of the intervention to patients, providers or the health system. These are important data points for practitioners and policymakers designing, implementing and scaling-up CHW interventions.

Finally, given that neonatal mortality is becoming an increasingly large proportion of mortality among children under 5 years of age, currently accounting for 45% of under-5 deaths,[65] a systematic review dedicated to appraising the evidence of the effects of proactive case detection of neonatal conditions by CHWs in LMICs is merited.

CONCLUSIONS

Proactive case detection by CHWs may reduce child mortality and morbidity and increase access to care. The certainty of this evidence is low due to limitations in study designs, inconsistency in results, indirect measures of effect and important diversity between a small number of included studies. More research is needed on proactive case detection with rigorous study designs, standardised outcomes and measurement, and detail on intervention design and implementation.

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Acknowledgements We are grateful to the London School of Hygiene and Tropical Medicine librarians for their tips on devising and executing the search strategy.

Contributors CW designed the study and wrote the protocol with input from JT, JG, BG, DC, KK and ADJ. CW developed and conducted the database searches. CW, JT and JG screened references for eligibility. CW and EW extracted data from included studies and conducted risk of bias assessments. CW analysed and synthesised data, with input from CL, KK, BG and DC. CW, JT and JG conducted GRADE quality assessments for all outcomes. CW drafted the manuscript. JT, JG, CL, BG and DC provided critical intellectual feedback and assisted in revising the manuscript. All authors read and approved the final manuscript.

Funding The authors have not declared a specific grant for this research from any funding agency in the public, commercial or not-for-profit sectors.

Disclaimer The findings and conclusions presented in this report are those of the authors and do not necessarily reflect the official position of the CDC.

Competing interests CW, KK and ADJ are coauthors on one (CW and KK) or two (ADJ) of the studies included in the review.

Patient and public involvement statement No patient or members of the public were involved in this study.

Patient consent for publication Not required.

Provenance and peer review Not commissioned; externally peer reviewed.

Data availability statement No data are available.

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