RESEARCH PAPER

Community Pediatric Nursing Interventions and Their Impact on Child Health Outcomes: A Systematic Review and Meta-Analysis

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Abstract

Background: Community pediatric nurses deliver prevention, surveillance, home visiting, school health, family support, and care coordination, yet effects on child outcomes vary across intervention models.

Methods: PubMed/MEDLINE and Scopus were searched for English-language primary quantitative studies published from 2016 through September 4, 2026. Two authors independently screened records using prespecified PICOS criteria. RoB 2 and ROBINS-I were used for risk-of-bias assessment. Comparable birth outcomes were synthesized with REML random-effects models and modified Hartung-Knapp confidence intervals.

Results: Of 2,351 interface-level records, 1,441 unique retrievable records were screened after deduplication and manual recovery of two PubMed-only reports. Twenty-four reports representing 23 independent studies from 10 countries were included. Three independent studies contributed to each birth-outcome meta-analysis. Low birthweight was not reduced (OR = 0.97, 95% CI [0.85, 1.12], I^2 = 49.7%), and the preterm-birth estimate was imprecise and highly heterogeneous (OR = 0.85, 95% CI [0.44, 1.65], I^2 = 88.4%). Narrative evidence was most consistently favorable for oral-health promotion, repeated infection-prevention programs, selected child-safety pathways, immunization comfort, and some school-based behavioral interventions. Brief smoking, postdischarge utilization, and several nutrition interventions were null or mixed.

Conclusion: Community pediatric nursing can improve selected child-health outcomes, particularly when interventions are repeated, behaviorally specific, and integrated with community systems. However, certainty for pooled birth outcomes was very low, and rigorous comparative trials with standardized child-level outcomes, fidelity reporting, and longer follow-up are needed.

Keywords: community health nursing; pediatric nursing; child health; home visiting; school nursing; systematic review; meta-analysis

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Introduction

Community paediatric nursing is about prevention, surveillance, health promotion, support for children with chronic health conditions and co-ordinating care near the places where children and families live. It brings nursing to the home, school, child-health centers, primary-care services and public-health programs, rather than just to the hospital. It is significant in childhood because the capacity of caregivers, access to preventive services, adherence to services, daily health behaviors, and continuity contribute to health trajectories. The components of child-health surveillance are assessment, growth and development monitoring, health education, anticipatory guidance, referral, and follow-up, whereas public-health nursing is focused on prevention and population-based issues, as well as social context (Uwizeye et al., 2022; Yakuwa et al., 2018).

Community paediatric nursing intervention, therefore, is a general approach and not necessarily one intervention. Interventions may consist of home visiting, school-based disease management, screening by a nurse, vaccine assistance, nutrition and physical activity counseling, telephone/digital follow-up, motivational interviewing, parent counseling, referral, and care coordination. The distinguishing characteristic of their work is to have an overt and meaningful nursing component that alters a child health measure or a proximal determinant of a child health measure. This distinction does not exclude those studies that only address nurse knowledge, student education, workplace outcomes, and/or inpatient processes without a community-oriented child outcome. A systematic review of paediatric nurse-sensitive outcomes also reported a lack of consistency in outcomes and recommended the need for more precise definitions of outcomes (Amatt et al., 2023).

There is evidence that community nursing can have an impact on a number of areas of child health, with varying impacts depending on the condition, intervention intensity, setting and outcome. School- or community-based nurse-led education in asthma has been linked to an increase in disease knowledge, self-management and selected health outcomes; however, there are variations in terms of the content of education and parental involvement (Isik et al., 2019). Nutrition education, physical-activity promotion and behavior change strategies are common components of nurse-led interventions in pediatric overweight and obesity; outcomes associated with behavioral and knowledge change are more consistently sensitive to intervention; clinical heterogeneity is significant (Cheng et al., 2021). These findings provide evidence of plausibility of

benefit and provide insight into the rationale for not combining disparate outcome endpoints in a comprehensive review.

The home visiting model offers another significant model. The Family Connects randomized trial demonstrated that a short-term, universal nurse home-visitation (NHV) intervention can reach a high proportion of families, is well implemented, and links many families to other community resources, with mixed findings regarding the effectiveness on health outcomes (Dodge et al., 2019). Other more intensive models have yielded mixed results. Findings from a large randomized trial of the Nurse-Family Partnership (NFP) did not show a significant difference in the rate of a composite adverse birth outcome (McConnell et al., 2022); follow-up through the first two years of childhood did not show a significant difference in the rate of a prespecified composite of adverse early-childhood outcomes (Swanson et al., 2025). However, in the case of nursing delivery, not all endpoints are necessarily improved by a successful delivery – fidelity, dose, usual services, and choice of outcomes may impact observed effectiveness.

Numerous community-based interventions are also delivered via a caregiver. The use of nurse-led telephone follow-up with motivational interviewing has been implemented to reinforce changes to healthy behaviours among young children promoted by parents (Schlottmann et al., 2019). Reach is one of the key components of effectiveness of these programs because they are complex, and as such, consistency of delivery, family involvement and successful referral/linkage to other services are important. Evaluating outcomes has therefore tended to focus on intervention fidelity assessment in the methodological work done in early-childhood community nursing (Anis et al., 2021).

Though there is a growing body of evidence, previous syntheses have tended to focus on a single condition, risk factor or class of nurse-sensitive outcomes (e.g., asthma, obesity, pediatric nursing quality indicators [Amatt et al., 2023; Cheng et al., 2021; Isik et al., 2019]). A cross-condition synthesis is necessary to identify where quantitative pooling is appropriate, where heterogeneity is a concern for narrative synthesis, and whether there are varying effects for different models of intervention and/or across different settings. This systematic review and meta-analysis currently assesses the relationship between nurse-led and/or substantively nurse-delivered community pediatric interventions and child health outcomes that are measurable and summarizes the consistent effects while maintaining the clinically important differences in outcomes.

Literature Review

School nursing is a significant community paediatric model due to the multiple opportunities for children to be accessed for education, screening, care planning and referral. A number of asthma programs demonstrate this: nurse-led school and community programs typically include education and support to children and parents, but the intensity of the programs and the outcome measures vary (Isik et al., 2019). This is significant because knowledge and symptom control are constructs and should not be combined as one generic health outcome, as is emergency use.

Programs for school and communities promote health as well, before disease occurs. Celik and Bektas (2026) assessed a nurse-delivered cardiovascular program for children using child health-behavior outcomes. These prevention programs can include multiple visits for the nurse, and involvement of school and family, but results may vary with participation, resources, and caregiver involvement; behavior change may occur before changes in physiology.

The situation is comparable with nutrition and obesity prevention. Nurse-led interventions involve nutrition counselling, physical-activity promotion, education and behaviour change techniques; behaviour change outcomes are more likely to be responsive than anthropometric outcomes (Cheng et al., 2021). Telephone approaches have the potential to reach further – Wen et al. (2017) tested infant-obesity prevention advice, and Schlottmann et al. (2019) found motivational interviewing to be helpful in promoting healthy family behaviors. They are influenced by the level of interaction and the involvement of the carer.

Through home visiting, nurses can meet with families in their home environment, educate and set goals with them, monitor health and link families with community resources. Today's trials demonstrate that successful implementation does not necessarily lead to clinical effectiveness. Dodge et al. (2019) showed high reach and fidelity of community-delivered postnatal home visiting services and McConnell et al. (2022) and Swanson et al. (2025) found no significant differences in the pre-specified composite outcomes of birth and early childhood. Intensity of intervention, population risk, exposure and choice of outcome are thus still relevant for interpretation.

Growth and parenting and preventive behavior are also targets of community nursing. Reifsnider et al. (2016), Brown et al. (2023) and

Holmberg Fagerlund et al. (2020) assessed programs across a range of nutrition, family preservation and diet communication. These studies collectively demonstrate the nature of community pediatric nursing in the context of clinical care, family behaviour, and public health and the outcomes this generates that cannot be assumed to be interchangeable.

Screening and surveillance are also integral tasks. Child-health surveillance is defined as assessment, developmental and growth monitoring, education and coordination (Yakuwa et al., 2018). Synthesis should be considered in light of intervention components, intensity, caregiver involvement, and linkage to services, which can only impact outcomes if screening is followed by those actions.

The important aspect of fidelity also comes into play. Anis et al. (2021) created a fidelity checklist for research within early-childhood community nursing, highlighting how inadequate uptake, different training, poor delivery or contamination can undermine observed effects. Therefore, robust implementation metrics can also be observed in the face of small clinical impacts in pragmatic home visit studies (Dodge et al., 2019; McConnell et al., 2022; Swanson et al., 2025).

How to select outcomes is a big synthesis challenge. Pediatric nurse-sensitive outcomes have a wide range of variation and community studies include the uptake of prevention; vaccination; adherence; health behaviours; growth and development; utilization; and quality of life (Amatt et al., 2023). In keeping with this, this review categorizes outcomes according to clinical constructs and pools only sufficiently comparable endpoints and utilises structured narrative synthesis for the rest (Cheng et al., 2021; Isik et al., 2019).

Methodology

Review Design and Reporting Framework

A systematic review and meta-analysis of the impact of community paediatric nursing interventions on measurable child health outcomes was conducted and presented following PRISMA 2020 guidelines (Page et al., 2021). The review question was framed based on Population, Intervention, Comparator, Outcomes and Study design (PICOS). A broad cross-condition approach was selected because community paediatric nursing is not limited to just preventive, behavioural and developmental roles; outcomes were only quantitatively pooled if they were clinically and statistically meaningful and comparable.

Eligibility Criteria

The populations eligible for the study were newborns, infants, children and adolescents aged 18 years or less. When the intervention was aimed at the child, the study was considered eligible if a health, utilization, safety, developmental or behavioral outcome had been measured at the child level. If aimed at the caregiver/family, the study was considered eligible if a child-level health, utilization, safety, developmental or behavioral outcome was measured. Studies were excluded that were conducted in adults only, with maternal outcome only, in nursing students, or included mixed ages with no distinct paediatric outcomes.

The interventions were nurse-led, nurse-delivered, or had a clear substantive community/public health nursing element. Modalities ranged from home visiting to school/kindergarten nursing, child health/primary care counselling, screening, surveillance, preventive education, telephone follow-up, care coordination, family support and community safety promotion. The comparators were usual care, no intervention, delayed intervention, an alternative program or a concurrent comparison group. Uncontrolled evaluations were kept only if they actually evaluated an eligible community nursing program and included a child outcome that was measurable.

Mortality or clinical indicators, health-service utilization, uptake of prevention, nutrition and growth, procedural comfort, infection/exposure prevention, health behaviors, development or psychosocial function, oral health, and child safety/protection were prespecified outcomes. It was necessary for the articles to be primary quantitative studies published in English between the years 2016 and September 4, 2026 and peer-reviewed. Randomized trials, cluster-randomized trials, comparative cohorts, quasi-experimental studies, interrupted time-series evaluations, and directly relevant pre-post studies were evaluated. The following types of reviews were not counted as independent studies: reviews, protocols, qualitative-only reports, descriptive studies with no intervention effect, retracted reports, and duplicate and/or linked reports without unique outcomes. The eligibility framework is summarised in table 1.

Table 1

PICOS Eligibility Criteria

Domain	Included	Excluded
Population	Newborns, infants, children, adolescents ≤18 years; caregiver/family studies with a measurable child-level outcome.	Adult/maternal-only outcomes; nurse/student outcomes; mixed samples without separable pediatric results.

Domain	Included	Excluded
Intervention	Nurse-led/delivered community pediatric/public-health intervention: home visiting, school nursing, child-health/primary care, screening, counseling, prevention, care coordination.	Incidental/nonseparable nursing role; hospital-only intervention without community component; professional education without child outcome.
Comparator	Usual care, no intervention, delayed/attention control, alternative program, or concurrent comparison.	Uncontrolled designs excluded from meta-analysis; retained narratively only when directly relevant.
Outcomes	Clinical/morbidity, utilization, preventive care, growth/nutrition, health behavior, psychosocial/development, oral health, procedural comfort, child safety/protection.	Process/caregiver-only outcome with no measurable child-health endpoint.
Design/limits	Peer-reviewed primary quantitative studies; 2016–Sep 4, 2026; English.	Reviews/protocols, qualitative-only/descriptive reports, retracted reports, duplicate/linked reports without distinct outcomes.

Note. PICOS = Population, Intervention, Comparator, Outcomes, and Study design/limits.

Information Sources and Search Strategy

Searches were conducted in PubMed/MEDLINE and Scopus on 4th September 2026. The PubMed search string combined medical subject headings and title/abstract terms for nursing in the paediatric and/or community setting, paediatric age band, modality of intervention and for outcomes of child health. The Scopus strategy was set up with equivalent concepts using TITLE-ABS-KEY and limited to the years 2016 to 2026, English language, and article/review document types. Reviews returned by the broad strategies were only used for context and citation checking and could not be used as a primary source of evidence. The search strings used are exactly as they were typed and are reproduced in Appendix A.

The number of interface-level hits was 976 in PubMed and 1,375 in Scopus, for a total of 2,351. The 200 citations in the PubMed export and 1,375 in the Scopus export were retained in a structured export. In reference and identifier verification, two PubMed-only reports met the search concepts and were manually recovered, resulting in 1,577 retrievable records, prior to deduplication. 136 duplicate records were identified in the DOI, PMID and normalized-title matching, leaving 1,441 unique retrievable records for screening. 774 PubMed interface hits were not available for record-level reconstruction as the number of hits in the original PubMed interface was higher than the number of hits in the retained structured export. These record-management counts are transparent, and they're presented in table 2.

Table 2

Database Search and Record Management

Source/step	Count	Handling
PubMed interface hits	976	10-year search limit; 200 citations in structured export.
Scopus interface/export records	1,375	2016–2026; English; Article/Review; complete structured export.
Additional PubMed-only reports recovered	2	McConnell et al. (2022) and Swanson et al. (2025), recovered during citation/identifier verification.
Retrievable records before deduplication	1,577	200 PubMed export + 2 recovered reports + 1,375 Scopus.
Duplicates removed	136	134 cross-database duplicate groups + 2 Scopus-internal duplicates.
PubMed interface hits not reconstructable	774	Could not be screened at record level; retained as a transparent limitation.
Unique retrievable records screened	1,441	Screening denominator used for this synthesis.

Note. Interface-level search yield and record-level retrievability are reported separately to avoid treating unreconstructed citations as screened records.

Record Management, Deduplication, and Study Selection

Exact DOI, exact PMID and normalized-title were used for hierarchical deduplication. Titles were lowercased and normalized for Unicode; punctuation was removed, and whitespace was simplified. Ambiguous matches were manually checked and separate reports from the same study were matched to prevent linking them incorrectly. A deduplication audit was conducted which

revealed 134 duplicate groups across the databases and 2 duplicate-title records within Scopus itself.

The titles and abstracts were independently screened by two authors using the prespecified criteria and the potentially eligible full texts were independently assessed. An inter-rater statistic was not computed; instead, disagreements were settled through discussion and consensus. For the purposes of recording reasons for exclusion at eligibility assessment, mutually exclusive primary categories were used. All reports from the same underlying cohort were linked to avoid double counting. Specifically, two reports of one randomized trial of the Nurse-Family Partnership, McConnell et al. (2022) and Swanson et al. (2025), were considered two reports of the same randomized cohort because they described birth and early-childhood outcomes, respectively. The entire selection flow is reported in figure 1.

Data Extraction and Outcome Classification

A structured extraction framework captured author/year, country, design, community setting, participant characteristics, sample size, intervention components, nursing role, comparator, follow-up, outcome definition, group-specific numerical data, effect estimates, confidence intervals, standard errors, and p values. The clinical or utilization outcomes of the child were given first priority. If exact means, standard deviations, and/or arm-level event counts were not available, the most complete estimate reported was retained and no missing numerical values were imputed. In the case of linked reports, outcome data were allocated to the underlying study and report-specific data were retained. The synthesis has been made using the statistical extraction that is provided in Appendix B.

Outcomes were categorized as: Perinatal/birth outcomes, growth/nutrition, procedural pain, infection/exposures, health-service utilization, oral health, child safety/protection, health behaviors, and psychosocial/developmental outcomes. This domain structure was employed to decide if there was a clinically valid rationale for a meta-analysis and to structure the narrative synthesis.

Risk of Bias and Certainty of Evidence

Risk of bias was assessed with RoB 2 for randomized and cluster-randomized trials and ROBINS-I for nonrandomized intervention studies (Sterne et al., 2016, 2019). Judgments considered randomization or confounding, deviations from intended intervention, missing outcome data, outcome measurement, and selective reporting as applicable to each design. Overall judgments were kept on the design-specific scales rather than converted to a numeric score. For pooled outcomes, certainty was evaluated using GRADE across risk of bias, inconsistency, indirectness, imprecision, and publication bias (Balshem et al., 2011).

Statistical Analysis

Only when there were at least two independent studies that evaluated sufficiently similar populations, intervention aims, comparators, and outcomes did a meta-analysis of the study results take place. Odds ratios (ORs) with 95% confidence intervals (CIs) were used for dichotomous birth outcomes. Random-effects models were used as the primary models because there were variations in setting, intensity, implementation and population risk of community nursing interventions. Restricted maximum likelihood (REML) was used to estimate between-study variance (τ^2) and heterogeneity was summarized using Cochran's Q and I^2 (Higgins et al., 2003; Viechtbauer, 2010). The primary uncertainty interval was calculated using a modified Hartung-Knapp method with t-distribution critical values, to account for the fact that three independent studies were used for each pooled birth outcome. Two sensitivity analyses were conducted using conventional normal approximation random-effects and inverse-variance fixed-effect estimates.

Thorland and Currie (2017) were unable to provide a directly poolable OR and rounded percentages of events to match reported sample sizes of the matched samples. Published adjusted ORs were provided by Holland et al. (2018). Event-based ORs were provided by McConnell et al. (2022) based on reported counts of birth-outcomes. The synthesis was therefore deemed exploratory because adjustment strategies and study designs varied. Funnel plots and formal small-study-effect tests were not carried out because fewer than 10 independent studies contributed to any pooled outcome (Egger et al., 1997). For outcomes that could not be meaningfully pooled, effect direction, magnitude, precision, and risk of bias were used to synthesise narratively rather than focusing on statistical significance.

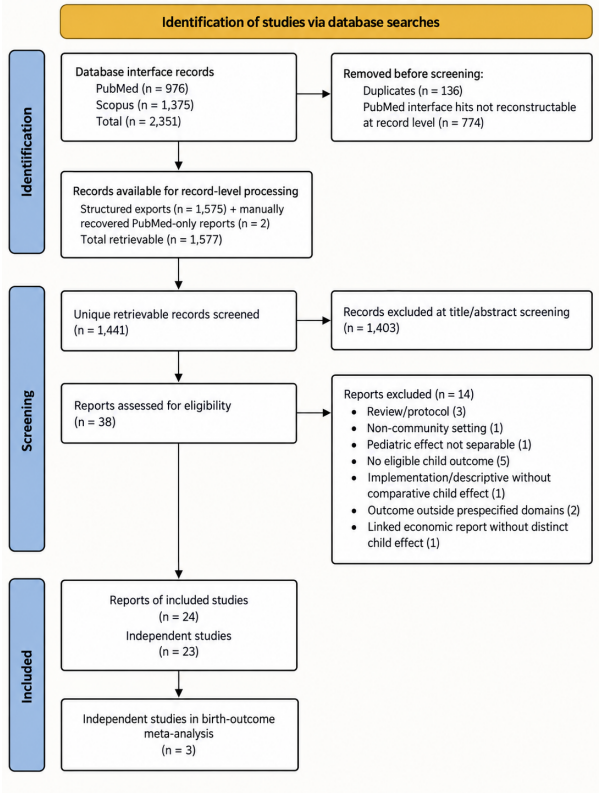
Results

The search interfaces identified 2,351 records (PubMed, n = 976; Scopus, n = 1,375). Of these, 1,575 were in the structured exports and two more PubMed-only exports were manually retrieved during the citation and identifier verification process. One thousand, four hundred and forty-one (1,441) unique retrievable records were screened after excluding duplicate records (136). At title/abstract screening, 1,403 records were excluded, leaving 38 reports for eligibility assessment. Fourteen of the reports were excluded in this stage: three reviews/protocols, one non-community-based intervention, one report that did not address a child-health outcome, one implementation report that did not include an eligible child-health outcome, two with outcomes that were not in the prespecified health domains, and one economic report linked to another intervention report without a distinct extractable child-health outcome. Twenty-four reports, with three independent studies contributing to each birth-outcome meta-analysis, were included. The flow for the selection

of studies and the difference between included reports and independent studies are shown in figure 1.

Figure 1

PRISMA 2020 Flow of Study Identification, Screening, and Inclusion



Note. Twenty-four eligible reports represented 23 independent studies because McConnell et al. (2022) and Swanson et al. (2025) were linked reports from one randomized cohort. The 774 unreconstructable PubMed interface hits were not counted as screened records.

The 23 independent studies spanned the years from 2016 to 2026 and included 10 countries: the United States, Australia, New Zealand, Norway, Sweden, China, Türkiye, the United Kingdom, Japan, and Canada. Included designs were individual randomized trials, cluster-randomized trials, propensity-matched or other comparative cohorts, quasi-experimental studies, pre/post evaluations, or one interrupted time-series analysis. Intensive or universal home visiting, school and kindergarten nursing, public-health nursing in child-care centers, well-child and maternal-child health services, community safety education, oral-health promotion, and postdischarge telephone follow-up were the types of interventions. Child outcomes included Birth Outcomes, growth/BMI, Procedural Comfort, Infection/Exposure Prevention, Hospital/ED use, Oral health, Child Protection, Health Behavior, and Psychosocial Function. Table 3 provides the characteristics of the studies and the key numerical results.

Table 3

Characteristics and Main Findings of Included Studies

Study/country	Design/setting	N	Intervention/comparator	Principal child-health finding
Guo et al. (2016), USA	Retrospective cohort; community home visiting	2,027	Prenatal paraprofessional home visits under RN supervision; dose-response comparison	Each additional visit: +62.20 g birthweight (SE 11.09, p<.001); +0.38 wk gestation (SE 0.03, p<.001); mediated estimates +53.97 g and +0.35 wk.
Reifsnider et al. (2016), USA	Quasi-experimental; WIC/community clinic	174 dyads	Public-health-nursing nutrition/parenting intervention vs comparison	Higher child growth velocity in the intervention; strongest among children of older and less-acculturated mothers.
Desprie & Langeland (2016), Norway	RCT; community health centers	114 (59/55)	30% sucrose vs water during 15-month immunization	Mean crying duration 18 s vs 33 s; significant reduction.

Study/country	Design/setting	N	Intervention/comparator	Principal child-health finding
Anderson et al. (2016), New Zealand	Service evaluation; school nursing	24,497 eligible; 23,756 consented	RN-led school throat/skin screening and treatment	191,423 throat swabs; 11% GAS positive; 20,176 treated; 2013 vs 2014 GAS positivity RR 1.80 (95% CI 1.30–2.30).
Daly et al. (2017), Australia	3-arm cluster RCT; 39 well-child clinics	1,424 parents	Nurse risk feedback/advice ± biochemical feedback vs control	No reduction at 12 mo: T1 OR 1.16 (95% CI 0.73–1.85); T2 OR 1.30 (0.88–1.92).
Thorland & Currie (2017), USA	Propensity-matched cohort; NFP	27,195 + 81,585 controls	Nurse-Family Partnership vs matched controls	LBW 9.4% vs 9.6% (p=.20); preterm 8.7% vs 12.3% (p<.0001).
Stookey et al. (2017), USA	Cluster RCT; child-care centers	43 centers; 791–945 children/year	Healthy Apple + public-health nurse screening vs delayed program	2013–14 BMI percentile difference −2.6 (SE 0.9), p=.003; BMI z-score −0.08 (SE 0.03), p=.007; 60% vs 19% exposed to 3 best practices.
Sezici et al. (2017), Türkiye	Single-blind RCT; kindergarten	79 (39/40); target 95	Nursing-process play therapy vs control	Posttest AA t=2.041, p=.045; SC t=2.692, p=.009; retest AA t=4.538, p<.001; SC t=4.693, p<.001; AW retest t=5.839, p<.001.

Study/country	Design/setting	N	Intervention/comparator	Principal child-health finding
Auger et al. (2018), USA	RCT; postdischarge follow-up	965 analyzed (483/482)	One nurse call within 4 days vs usual discharge	15.9% vs 13.1%; OR 1.26 (95% CI 0.88–1.81), p=.21; warning-sign recall ratio 1.20 (1.10–1.30).
Holland et al. (2018), USA	Secondary cohort from earlier RCT	512 (157/355)	Prior NFP nurse home visiting vs comparison	LBW adjusted OR 0.51 (95% CI 0.27–0.97); preterm OR 1.07 (0.62–1.85); benefit varied by first-child risk.
Sjoberg et al. (2018), Canada	Single-group pilot; school visit	14 siblings	Oncology nurse school visitation program	Fewer school days missed after the visit; no significant PedsQL change; positive interview feedback.
Dodge et al. (2019), USA	RCT; community agency home visiting	936 randomized; impact n=316	Family Connects nurse home visit vs control	CPS investigations mean 0.10 vs 0.18; b=−0.09 (95% CI −0.18 to 0.01), p=.07; ED use 0.51 vs 0.64, p=.25.
Hinde et al. (2017), United Kingdom	Interrupted time series; 2 CCN teams	2 service areas	Children Community Nursing teams introduced vs pre-intervention trend	Estimated reductions of 9,634 and 8,969 bed-days after team introduction; reductions did not reach statistical significance; savings did not offset commissioning cost.

Study/country	Design/setting	N	Intervention/comparator	Principal child-health finding
Holmberg Fagerlund et al. (2020), Norway	Cluster RCT; 10 municipalities	250 endpoint (140/110)	PHN diet communication tool vs standard consultation	Vegetables 64.5±46.8 vs 68.7±83.6 g, p=.54; saturated fat 13.0±2.5 vs 12.9±2.6 E%, p=.78; BMI median 16.4 vs 16.6, p=.19.
Heilbrunn-Lang et al. (2020), Australia	Pre-post pilot; MCH services	1,585; matched follow-up 230	Tooth-Packs + nurse mouth check/referral	1,493 checked; 142 (9.5%) caries identified; oral-health-professional visit OR 28.0 (95% CI 7.40–236.88); assisted brushing OR 1.76 (1.05–3.00).
Brännemo et al. (2021), Sweden	Nonrandom comparative home-visiting cohort	18 mo 72/100; 36 mo 57/77	Six nurse/parent-adviser home visits vs standard one visit	Cavitated caries at 18 mo 0% vs 7%, p=.042; any caries at 36 mo 32% vs 49%, p=.039.
Zhang et al. (2021), China	Cluster RCT; 20 classes/6 kindergartens	582 (289/293)	8-week child hand-hygiene training + parent seminar/WeChat vs control	Improved hand-hygiene behavior and 7-step compliance; fewer infection-related absences; exact abstract effect estimate not reported.

Study/country	Design/setting	N	Intervention/comparator	Principal child-health finding
McConnell et al. (2022) / Swanson et al. (2025), USA	RCT; linked NFP reports	5,670 randomized; birth n=4,966; child-health n=4,932	Intensive Nurse-Family Partnership home visiting vs usual care	Adverse birth composite 26.9% vs 26.1%, adjusted difference 0.5 pp (95% CI −2.1 to 3.1); early-child adverse composite 27.3% vs 26.8%, adjusted difference 0.4 pp (−2.3 to 3.0); ED use −2.9 pp before multiplicity adjustment.
Qian et al. (2023), China	Community pre-post evaluation	Child injury cases 157 (2020), 103 (2021)	Nursing experts delivered repeated community safety education	Injury cases declined from 157 to 103 (p<.05); guardian prevention-knowledge scores increased (p<.01).
Brown et al. (2023), USA	Matched quasi-experimental home visiting	48 vs 150	Tailored public-health nurse support vs matched controls	Open case 2.1% vs 17.3%, p<.01; out-of-home placement 0% vs 10.7%, p<.01; parenting effects d=0.58–0.85.
Williams et al. (2025), United Kingdom	Feasibility pre-post; health visiting	59 baseline; 26 follow-up	Health visitor oral-health advice + oral-health pack	10/26 (40%) increased brushing frequency; 4/26 (15%) reported a dental visit; high attrition limited inference.
Celik & Bektas (2026), Türkiye	Quasi-experimental school study	85 completed	5-week nurse-led cardiovascular education vs no intervention	Group×time health behavior p=.038; visual knowledge p=.001; knowledge gains persisted to 6 months.

Study/country	Design/setting	N	Intervention/comparator	Principal child-health finding
Ninomiya et al. (2017), Japan	Observational preventive home-visiting program	2,252 at-risk parents	Early PHN identification/home visits beginning in pregnancy/infancy	Referral to child protective centers 9.2% for maternity-hospital-identified vs 20.7% for local-health-center-identified families, p<.001.

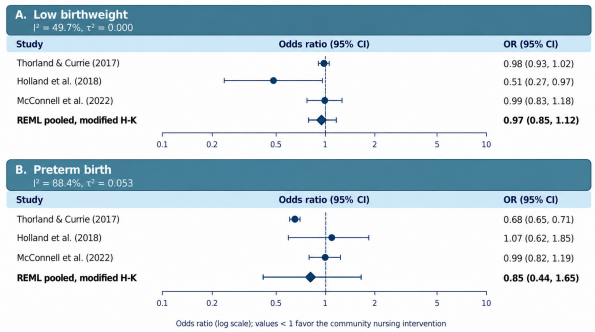
Note. N is the principal recruited or analytic denominator reported for the outcome. Linked McConnell/Swanson publications are displayed in one row to preserve the independent study count. GAS = group A streptococcus; NFP = Nurse-Family Partnership; PHN = public health nurse; CCN = Children Community Nursing.

The largest amount of birth-outcome data came from the home-visiting studies, which varied in types of design and population. Guo et al. (2016) found a dose-response relationship between prenatal visits and birth weight (62.20 g more weight per visit, SE = 11.09, p < .001) and gestation (0.38 weeks longer, SE = 0.03, p < .001). Thorland and Currie (2017) reported comparable low birth weight rates for the matched controls and the Nurse-Family Partnership clients (9.6% vs. 9.4%, p = .20); however, the matched controls had higher rates of preterm births in comparison to the Nurse-Family Partnership clients (12.3% vs. 8.7%, p < .0001). Holland et al. (2018) reported reduced odds of low birthweight in a second birth after prior NFP participation (adjusted OR = 0.51, 95% CI [0.27, 0.97]) but no clear preterm effect (OR = 1.07, 95% CI [0.62, 1.85]). McConnell et al. (2022) did not observe any differences in the adverse birth composite or low birth weight and preterm components in the randomized South Carolina (SC) trial.

A total of three independent studies were deemed to be compatible enough to be explored for pooling. For low birthweight, the REML pooled OR was 0.97 and the modified Hartung-Knapp 95% CI crossed the null (95% CI [0.85, 1.12]); heterogeneity was moderate (I^2 = 49.7%, τ^2 < .001; Q[2] = 3.97, p = .137). For preterm birth, the pooled OR was 0.85 with a wide Hartung-Knapp interval (95% CI [0.44, 1.65]) and substantial heterogeneity (I^2 = 88.4%, τ^2 = 0.053; Q[2] = 17.26, p < .001). The fixed-effect sensitivity estimates were OR = 0.97 (95% CI [0.93, 1.02]) for low birth weight and OR = 0.70 (95% CI [0.67, 0.73]) for preterm birth, the latter of which was heavily influenced by the large observational cohort. Study-specific effects and main random-effects estimates are shown in figure 2.

Figure 2

Random-Effects Meta-Analysis of Low Birthweight and Preterm Birth



Note. Pooled estimates use REML random-effects models with modified Hartung-Knapp 95% confidence intervals. Thorland and Currie (2017) ORs were reconstructed from rounded event percentages; Holland et al. (2018) contributed adjusted ORs; McConnell et al. (2022) contributed event-based ORs. The synthesis is exploratory because study designs and adjustment strategies differ.

The findings relating to child-safety and family preservation were positive, primarily in nonrandomized evaluations. Brown et al. (2023) found a lower rate of open child-protection cases (2.1% vs 17.3%, p<0.01) and a lower rate of out-of-home placements (0% vs 10.7% in matched controls). Fewer unintentional injury cases were found following repeated community safety education by Qian et al. (2023) (157 to 103, p < .05), and lower rates of referral by Ninomiya et al. (2017) were found among families identified earlier in maternity hospitals (9.2% vs. 20.7%, p < .001). Randomized evidence was less clear: Dodge et al. (2019) reported a nonsignificant decrease in investigations of child abuse and neglect by child-protective services, and the NFP two-year follow-up did not report a significant change in its composite adverse event measure in early childhood (Swanson et al., 2025).

There was minimal impact on health-service use from brief counselling interventions. Auger et al. (2018) improved parental warning-sign recall (ratio = 1.20, 95% CI [1.10, 1.30]) but did not reduce 30-day urgent reutilization (OR = 1.26, 95% CI [0.88, 1.81]). Hinde et al. (2017) found that there was a statistically non-

significant but large numerical decrease in paediatric bed days following the delivery of Children Community Nursing teams with estimated savings not outweighing commissioning costs. Previously, Swanson et al. (2025) showed decreased use of emergency departments before multiplicity adjustment and Daly et al. (2017) demonstrated that there was no decrease in the exposure of babies to secondhand smoke after short-term nurse advice and feedback.

More intense preventive programs appeared to be more strongly associated with signals in some areas. Stookey et al. (2017) combined public-health nurse screening with improvements to the child-care environment, and the resulting positive BMI percentile changes (−2.6 points, SE = 0.9, p = .003) and BMI z-score changes (−0.08, SE = 0.03, p = .007) were favorable. However, Holmberg Fagerlund et al. (2020) did not see any diet or BMI effects with a regular communication tool. Reifsnider et al. (2016) found growth velocity to be higher with the community nutrition/parenting intervention. Despriee and Langeland (2016) decreased crying during immunizations when sucrose was used (18 sec vs. 33 sec); and Sezici et al. (2017) found that kindergarten play therapy improved behavioral scores. In a small sibling pilot, Sjoberg et al. (2018) did not observe a significant quality-of-life change, but did observe fewer missed school days.

Some of the consistently best findings for the narrative were for infection prevention and oral-health promotion. The Mana Kidz school-nursing program included screening and treatment and showed less group A streptococcal carriage following the implementation of the program (Anderson et al., 2016), and Zhang et al. (2021) were able to improve hand hygiene and decrease absenteeism due to infection by training children, educating parents, and providing digital reinforcement. Oral-health programs also connected education with supplies, screening and/or referral. 9.5% of children screened were diagnosed with early caries and there were reports of higher dental attendance and assisted brushing by Heilbrunn-Lang et al. (2020). Brännemo et al. (2021) reported a decrease in prevalence of caries after multiple home visits and Williams et al. (2025) noted better brushing practices after multiple visits, even with significant attrition. Figure 3 summarizes direction of effect, without combining clinically heterogeneous endpoints, for each outcome domain.

Figure 3

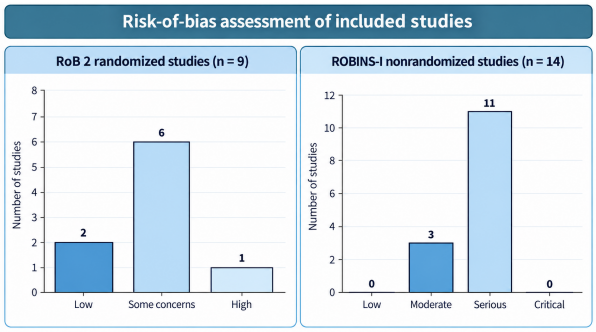
Direction-of-Effect Evidence Map Across Child-Health Outcome Domains

Note. Markers summarize the principal eligible direction of effect and do not encode certainty or sample size. Mixed indicates outcome-, subgroup-, or context-dependent findings. The linked McConnell/Swanson trial is shown as one independent study.

Risk-of-bias assessment indicated that the overall level of evidence was overwhelmingly made up of nonrandomized or methodologically limited assessments. There were nine randomized or cluster-randomized studies of which two were low risk, six had some concerns, and one was high risk. There were 14 nonrandomized studies, 3 of which were considered moderate risk and 11 were considered serious risk. The most frequent concerns were the presence of unmeasured or residual confounding, failure to account for simultaneous controls, attrition, small sample size, subjective outcomes and failure to report cluster-adjusted effects. The distribution of overall judgements by risk of bias tool is shown in Figure 4.

Figure 4

Overall Risk-of-Bias Distribution by Assessment Tool



Note. RoB 2 and ROBINS-I use different overall judgment categories; panels are therefore displayed separately rather than combined into one ordinal scale.

The study-level basis for these judgments is detailed in Table 4. The two low-risk randomized studies were Auger et al. (2018) and the linked McConnell/Swanson NFP trial, whereas the strongest recurring limitations among nonrandomized studies were secular confounding and selection. The cluster trial by Holmberg Fagerlund et al. (2020) was judged high risk primarily because of attrition and missing endpoint data. Because risk categories are instrument-specific, ROBINS-I 'serious' and RoB 2 'high' were not merged into a single numeric score.

Table 4

Study-Level Risk-of-Bias Judgments

Study	Tool	Overall risk	Primary concern
Guo et al. (2016)	ROBINS-I	Serious	Retrospective dose-response; residual confounding and selection.
Reifsnider et al. (2016)	ROBINS-I	Serious	Quasi-experimental allocation and residual confounding.
Despriee & Langeland (2016)	RoB 2	Some concerns	Subjective procedural response and limited blinding detail.
Anderson et al. (2016)	ROBINS-I	Serious	Service evaluation susceptible to secular trends.
Daly et al. (2017)	RoB 2 (cluster)	Some concerns	Cluster design and self-reported household exposure outcomes.
Thorland & Currie (2017)	ROBINS-I	Moderate	Propensity matching reduced but did not eliminate confounding.
Stookey et al. (2017)	RoB 2 (cluster)	Some concerns	Retrospective trial registration and variable center follow-up.
Sezici et al. (2017)	RoB 2	Some concerns	Single kindergarten, attrition from 95 to 79, behavior-rating outcomes.
Auger et al. (2018)	RoB 2	Low	Randomized design with objective primary utilization data.
Holland et al. (2018)	ROBINS-I	Moderate	Secondary selected cohort of mothers with second births.
Sjoberg et al. (2018)	ROBINS-I	Serious	Single-group pilot with n=14 and no concurrent comparator.
Dodge et al. (2019)	RoB 2	Some concerns	Impact subsample and missingness reduce precision.
Hinde et al. (2017)	ROBINS-I	Serious	Two-area interrupted time series; secular confounding remains plausible.
Holmberg Fagerlund et al. (2020)	RoB 2 (cluster)	High	Substantial attrition and missing endpoint data.
Heilbrunn-Lang et al. (2020)	ROBINS-I	Serious	Uncontrolled pre-post analysis with limited matched follow-up.
Brännemo et al. (2021)	ROBINS-I	Serious	Nonrandom reference group and differential follow-up.

Study	Tool	Overall risk	Primary concern
Zhang et al. (2021)	RoB 2 (cluster)	Some concerns	Limited detail on cluster-adjusted reporting and behavioral outcomes.
McConnell/Swanson NFP trial	RoB 2	Low	Randomized design, preregistered administrative outcomes, large analytic cohort.
Qian et al. (2023)	ROBINS-I	Serious	Pre-post/convenience design vulnerable to secular change and confounding.
Brown et al. (2023)	ROBINS-I	Moderate	Matched comparison but small intervention group and residual confounding.
Williams et al. (2025)	ROBINS-I	Serious	Single-group feasibility study with substantial attrition and self-report.
Celik & Bektas (2026)	ROBINS-I	Serious	Nonrandom school allocation and predominantly self-reported outcomes.
Ninomiya et al. (2017)	ROBINS-I	Serious	Observational pathway comparison with baseline-risk and referral differences.

Note. Overall judgments summarize the dominant threat to inference and are not numeric quality scores.

The pooled birth outcomes were rated very low certainty with GRADE. Low birthweight was downgraded because the synthesis mixed randomized and observational designs, included a secondary adjusted cohort estimate, and remained imprecise under Hartung-Knapp inference. Preterm birth was additionally downgraded for very high inconsistency (I^2 = 88.4%). Table 5 summarizes the quantitative findings, heterogeneity, and certainty. Formal publication-bias testing was not undertaken because only three independent studies contributed to each meta-analysis.

Table 5

Meta-Analysis and GRADE Summary of Birth Outcomes

Outcome	Studies	Random-effects OR (95% CI)	Heterogeneity	Fixed-effect sensitivity	GRADE certainty
Low birthweight	3	0.97 (0.85–1.12)	I^2=49.7%; τ^2=0.000; Q(2)=3.97, p=0.137	0.97 (0.93–1.02)	Very low
Preterm birth	3	0.85 (0.44–1.65)	I^2=88.4%; τ^2=0.053; Q(2)=17.26, p<.001	0.70 (0.67–0.73)	Very low

Note. Primary confidence intervals use modified Hartung-Knapp inference. GRADE was downgraded for risk of bias/design mixing and imprecision; preterm birth was additionally downgraded for substantial inconsistency.

Taken together, the quantitative and narrative results do not support a single universal effect of community pediatric nursing. Instead, benefits clustered around interventions that translated nursing contact into repeated, specific actions-such as oral-health routines, infection-prevention practices, safety education, procedural comfort, and selected school-based behaviors-whereas one-off advice, broad diet counseling, and downstream service utilization were less consistently changed. This pattern informed the interpretation of intervention mechanism and intensity in the Discussion.

Discussion

This review identified that community paediatric nursing has the potential to positively affect the selected child-health outcomes but that effects are highly dependent on the mechanism of the intervention and outcome. It was not warranted to have a single pooled estimate from the cross-domains. The only domain that had three independent studies that were sufficiently comparable was birth outcomes. The pooled estimates remained unclear: the point estimate for low birth weight was null, while the point estimate for preterm birth was in favor of intervention but with a large Hartung-Knapp interval and high heterogeneity. The difference between the results of the large propensity-matched Thorland and Currie cohort and the null result from the randomized McConnell trial suggests that good results from observational implementation data may not be replicated

in a randomized implementation in a different service context.

This interpretation is in line with previous syntheses of nurse home visiting. Molloy et al. (2021) detected positive impacts on disadvantaged families, with significant disparities across outcomes and subgroups. Our results advocate for and augment the above conclusion by demonstrating how birth-outcome estimates vary according to study design and study population. Kahraman and Havlıoğlu (2024), on the other hand, yielded positive pooled results for breastfeeding and infant weight. These findings are not mutually exclusive, as breastfeeding and postnatal weight are direct caregiving outcomes and low birthweight and prematurity are multifactorial outcomes which may not be as modifiable with nursing contact alone.

Findings on growth and obesity prevention also were dependent on intervention structure. Stookey et al. (2017) indicated that when nurse screening was combined with environmental quality improvement in child-care centers, BMI changes were favorable, while Holmberg Fagerlund et al. (2020) observed there was no significant diet or BMI change with the use of a routine communication tool. Reifsnider et al. (2016) reported growth velocity improvement but there is some doubt of causation in the quasi-experimental study. This trend is consistent with Cheng et al. (2021) which found significant heterogeneity with greater effects on behavior than anthropometrics. Programs that modify the environment of the child and also reinforce behavior in the various settings may thus have a greater potential than brief counselling alone.

The most clear-cut narrative signals were when the education was integrated with a concrete action pathway. Oral-health programs delivered supplies, screening, referral or re-contact at home, and positive behaviour or caries outcomes were reported by Heilbrunn-Lang et al. (2020), Brännemo et al. (2021) and Williams et al. (2025). Similarly, infection-prevention programs were similar, with Mana Kidz having a nurse in school to conduct screening and treatment, and Zhang et al. (2021) providing a mix of child and parent education plus digital reinforcement. This is similar to the models of child health surveillance that provide assessment and education, referral, treatment, and follow-up (Yakuwa et al., 2018).

Findings of child-safety need to be interpreted with increased caution. Brown et al. (2023), Qian et al. (2023) and Ninomiya et al. (2017) had good child-protection or injury indicators, but were all subject to potential confounding or secular change. Randomized evidence was more mixed: There was only a nonsignificant decrease in child-protective-services investigations with Family Connects (Dodge et al., 2019) and the large NFP follow-up failed to lower its preregistered adverse early-childhood composite (Swanson et al., 2025). Administrative child protection outcomes should thus be viewed with consideration of baseline risk, referral rates and concurrent services.

There was a comparable difference with regard to utilization. Auger et al. (2018) was able to increase parental awareness of signs of warning while not decreasing urgent reutilization, and Hinde et al. (2017) saw a large (but non-significant) numerical decrease in bed days with the introduction of community nursing teams. Swanson et al. (2025) reported a lower use of the emergency department (not corrected for multiple testing). The results indicate that information transfer or service substitution can enhance proximal processes but not necessarily broad utilization, which also can be affected by the severity of the disease, access, parental risk perception and the configuration of services in the local area.

School and psychosocial interventions were promising and require replication. Sezici et al. (2017) showed that play therapy helped to increase social competence and behaviour; Sjoberg et al. (2018), in a very small sibling pilot, showed improved attendance but no significant improvements in their quality of life; and Celik and Bektas (2026) showed that there were sustained improvements in cardiovascular knowledge and behaviour after cardiovascular education. This pattern is consistent with the repeated-contact advantage cited in school-nursing reviews (Isik et al., 2019), though, given the small samples, single-school designs, and self-reported behavior, certainty is low.

There are a number of limitations which restrict the findings. The studies experienced intervention and outcome heterogeneity, and many of the studies were nonrandomized or at high risk of bias. The exploratory birth meta-analysis combined event-based and adjusted estimates and publication-bias testing was inappropriate given that only three independent studies were included and because Hartung-Knapp inference was used to account for uncertainty from the three studies. However, 774 hits on the PubMed interface were unable to be reproduced from the exported information and, as such, full screening of the original interface yield cannot be guaranteed. A full re-export from PubMed should be done prior to submitting to a journal. Searching just PubMed/MEDLINE and Scopus might also have limited recall from a more general approach in searching for nursing databases.

This does not mean that one model of community nursing should be used for everyone. Discrete information needs, referral, or procedural comfort may benefit from brief contacts, whereas entrenched behavior, social adversity, and chronic risk may require repeated contacts, caregiver engagement, cross-sector linkage, and fidelity monitoring. This validates the methodological suggestions by Anis et al. (2021) that community nursing studies should include information on the dose of the intervention, the type of nursing role, uptake, co-interventions, attrition, and arm-level numeric outcomes. Future synthesis would also benefit

greatly from having standardized outcome measures at the child level and equity-focused subgroup analyses.

Conclusion

Community pediatric nursing interventions can improve selected child-health outcomes, particularly when nursing contact is repeated, behaviorally specific, and connected to screening, referral, treatment, family engagement, or environmental change. Favorable evidence was most apparent for oral-health promotion, infection-prevention programs, procedural comfort, selected child-safety pathways, and some school-based behavioral outcomes. In contrast, brief smoking interventions, postdischarge reutilization strategies, and several nutrition interventions were null or mixed. The pooled birth-outcome evidence did not demonstrate a clear reduction in low birthweight or preterm birth, and certainty was very low because of design limitations, imprecision, and heterogeneity. More rigorous comparative trials with standardized child-level outcomes, complete numerical reporting, longer follow-up, and explicit fidelity measures are needed to determine which community pediatric nursing models produce durable and clinically meaningful benefit.

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Appendix A: Database Search Strategies

PubMed/MEDLINE

(
"Pediatric Nursing"[Mesh] OR "Community Health Nursing"[Mesh] OR "Public Health Nursing"[Mesh]
OR "pediatric nursing"[Title/Abstract] OR "paediatric nursing"[Title/Abstract]
OR "community nursing"[Title/Abstract] OR "community health nursing"[Title/Abstract]
OR "public health nursing"[Title/Abstract] OR "community pediatric nurse*"[Title/Abstract]
OR "community paediatric nurse*"[Title/Abstract] OR "pediatric nurse*"[Title/Abstract]
OR "paediatric nurse*"[Title/Abstract]
) AND (
"Child"[Mesh] OR "Infant"[Mesh] OR "Adolescent"[Mesh]
OR child*[Title/Abstract] OR infant*[Title/Abstract] OR pediatric*[Title/Abstract]
OR paediatric*[Title/Abstract] OR adolescent*[Title/Abstract]
) AND (
intervention*[Title/Abstract] OR program*[Title/Abstract] OR programme*[Title/Abstract]
OR "health promotion"[Title/Abstract] OR "health education"[Title/Abstract]
OR "home visit*"[Title/Abstract] OR "home-based"[Title/Abstract] OR "community-based"[Title/Abstract]
OR prevention[Title/Abstract] OR screening[Title/Abstract] OR counseling[Title/Abstract]
OR counselling[Title/Abstract] OR "care coordination"[Title/Abstract] OR "case management"[Title/Abstract]
) AND (
"Treatment Outcome"[Mesh] OR "Health Status"[Mesh] OR "Child Health"[Mesh]
OR outcome*[Title/Abstract] OR "health outcome*"[Title/Abstract]
OR morbidity[Title/Abstract] OR mortality[Title/Abstract] OR hospitalization[Title/Abstract]
OR hospitalisation[Title/Abstract] OR "hospital admission*"[Title/Abstract]
OR "emergency department visit*"[Title/Abstract] OR immunization[Title/Abstract]
OR immunisation[Title/Abstract] OR vaccination[Title/Abstract] OR growth[Title/Abstract]
OR nutrition*[Title/Abstract] OR development*[Title/Abstract] OR "quality of life"[Title/Abstract]
OR adherence[Title/Abstract]
)

Scopus

TITLE-ABS-KEY(
(
"pediatric nursing" OR "paediatric nursing" OR "community nursing"
OR "community health nursing" OR "public health nursing"
OR "community pediatric nurse*" OR "community paediatric nurse*"
OR "pediatric nurse*" OR "paediatric nurse*"
)
AND (child* OR infant* OR pediatric* OR paediatric* OR adolescent*)
AND (

```
 intervention* OR program* OR programme* OR
"health promotion" OR "health education"
 OR "home visit*" OR "home-based" OR "community-
based" OR prevention OR screening
 OR counseling OR counselling OR "care coordination"
OR "case management"
 )
 AND (
 outcome* OR "health outcome*" OR "child health"
OR morbidity OR mortality
 OR hospitalization OR hospitalisation OR "hospital
admission*"
 OR "emergency department visit*" OR immunization
OR immunisation OR vaccination
 OR growth OR nutrition* OR development* OR
"quality of life" OR adherence
 )
)
```

Appendix B: Eligibility-Stage Exclusion Log

Fourteen reports assessed at the eligibility stage did not meet the prespecified PICOS criteria. Table C1 records the primary exclusion reason used for PRISMA accounting.

Table C1

Reports Excluded After Eligibility Assessment

Report	Primary exclusion reason
Wen et al. (2017), CHAT	Protocol; no intervention-result child data.
Cheng et al. (2021), nurse-led obesity review	Scoping review; not primary evidence.
Isik et al. (2019), nurse-led asthma review	Systematic literature review; not primary evidence.
Yoshioka-Maeda et al. (2026), nurse-led DDH ultrasound	Implementation/service report without comparative child-health effect.
TECH2CHECK (2026)	Mixed-age population extending beyond 18 years without separable pediatric effect.
Dardouri et al. (2021), family empowerment for asthma	Hospital-based intervention rather than community setting defined by PICOS.
Söngüt & Altay (2026), first-aid parent training	Caregiver knowledge/skills outcome without child-health endpoint.
Layton et al. (2025), PHN CBT workshop	Maternal mental-health outcomes only.
Lotha Valan et al. (2025), digital support	Parent/eHealth outcomes only; no eligible child endpoint.
Lennartsson et al. (2016), cranial asymmetry education	Parent awareness only; no measured child-health effect.
Galamba et al. (2025), school physical activity	Small feasibility project without comparative/statistically extractable child-health outcome.
Nurse-led digital privacy school program (2025)	Privacy/autonomy literacy outside prespecified child-health domains.
Nurse-led photovoice sustainability program (2026)	Environmental awareness/intentions outside prespecified child-health domains.
Bohingamu Mudiyanselage et al. (2021), right@home economic evaluation	Linked economic secondary report without a distinct extractable child-health effect for this review.