BMJ Global Health

WASH and learn: a scoping review of health, education and gender equity outcomes of school-based water, sanitation and hygiene in low-income and middle-income countries

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To cite: Bick S, Davies K, Mwenge M, *et al*. WASH and learn: a scoping review of health, education and gender equity outcomes of school-based water, sanitation and hygiene in low-income and middle-income countries. *BMJ Glob Health* 2025;**10**:e018059. doi:10.1136/bmjgh-2024-018059

Handling editor Helen J Surana

► Additional supplemental material is published online only. To view, please visit the journal online (https://doi.org/10.1136/bmjgh-2024-018059).

Received 29 October 2024
Accepted 8 April 2025

Check for updates

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ABSTRACT

School-age children in low-income and middle-income countries (LMICs) face health and educational challenges due to inadequate water, sanitation and hygiene (WASH) in schools (WinS). Evidence for the impact of WinS interventions is limited and inconsistent, and previous systematic reviews have faced challenges in synthesising data due to varied interventions, study designs and outcome measures, although most do not examine this variability in more detail. This scoping review identified 83 experimental studies from 33 LMICs measuring a primary or secondary health or educational outcome among pupils, published up to November 2023, using a systematic search of seven databases and searching of reference lists of previous systematic reviews and included articles. These included 65 studies (78%) not included in previous WinS reviews and encompassed 313 intervention effects across 14 outcome domains. Interventions comprised an array of WASH technologies and approaches, often combining infrastructure and behaviour change methods and frequently integrated with other school-based initiatives like deworming. 36 studies (43%) measured only behavioural or knowledge outcomes. Our comprehensive inventory of study outcomes identified 158 unique outcome measures, with 72% measured in exactly one study. Common outcomes included parasitic infections, anthropometric measures and school absence, but approaches to measurement varied widely even for similar outcomes. Only 7% of results were disaggregated by gender, limiting assessment of differential impacts. Our findings underscore the need for standardised outcome measures in WinS research incorporating a complete definition of the assessment and aggregation approach, greater attention to gender-specific impacts, and further exploration of modalities and functions of WinS interventions alongside novel meta-analysis methods to disentangle effects of diverse intervention components.

WHAT IS ALREADY KNOWN ON THIS TOPIC

⇒ Water, sanitation and hygiene in schools (WinS) has well-established theoretical benefits for child health, development and achievement of educational outcomes and gender equity.
⇒ Evidence for the impact of WinS is mixed and heterogeneity in outcomes and interventions often hinders evidence synthesis.

WHAT THIS STUDY ADDS

⇒ Our review expands on previous systematic reviews to scope a diverse WinS evidence base of 83 experimental studies in low-income and middle-income settings, 93 intervention arms and 313 total health and educational outcomes.
⇒ Through a comprehensive inventory of 158 unique outcome measures, we pinpoint aspects of outcome definitions preventing comparability between studies and highlight inadequate reporting of gendered impacts.

HOW THIS STUDY MIGHT AFFECT RESEARCH, PRACTICE OR POLICY

⇒ Findings from this review can aid in the development of standardised outcome measures so that future effects of WinS may be better evaluated, interpreted and synthesised.

BACKGROUND

School-going children in low-income and middle-income countries (LMICs), who may account for a substantial and growing segment of the population,[1 2] are susceptible to morbidity associated with inadequate water, sanitation and hygiene (WASH) in the educational settings where they spend many of their waking hours.[3 4] Frequent diarrhoeal and respiratory infections can spread rapidly in enclosed settings due to social mixing, and outbreaks correlate with absence from school.[5 6] Soil-transmitted helminth (STH) and other parasitic infections peak during school age and cause impairment of immune function, anaemia, and compromised growth and cognitive development.[7–9] Loss of learning capacity and repeated absence negatively impact attainment and retention in the education system,[10–12] with consequences on future economic and health prospects.[13 14] Academic outcomes can also be affected by dehydration due to limited water access,[15] a critical issue for children walking long distances to school. Poor environmental and hygiene conditions in the classroom and grounds can also make teaching and learning difficult[16]; lack of separate, private and secure toilets and washing facilities necessary to effectively manage menstruation is a noted barrier to girls' attendance and progression,[17–19] increasing risks of psychosocial stress and urogenital infection.[20 21] Included in the Sustainable Development Goals, improving WASH in schools (WinS) has therefore been theorised to have diverse and wide-reaching consequences for pupils on health, school absence and education, gender parity, equity and human dignity.

Schools may offer a convenient platform for establishing health behaviours, such as handwashing with soap (HWWS), that adolescents may carry forward into later life,[22] and into their wider communities.[23 24] However, these routine behaviours depend on the sustainable delivery of WASH services[25] in a complex social system where multiple actors—teachers, management staff, administrators, pupils and parents—formal and informal structures, and the physical and social environment shape outcomes.[26] Complex interventions combining improved infrastructure, hygiene promotion, strengthening of governance systems and funding for WASH,[27] and engaging teachers to model normative behaviours[28] may be required to deliver change at multiple levels. There are also school-specific engineering and logistical considerations, such as to account for increased demand on services at break times,[16] group handwashing behaviours[29] and theft or loss of soap.[30] Amidst the challenges of data collection among school-age children,[31] disentangling the effects of WASH in educational settings may be more complex than domestic settings that have been the historical focus of WASH research.

While meta-analyses have demonstrated significant impacts of WASH interventions on morbidity and mortality across educational, childcare and domestic settings,[32–34] evidence from systematic reviews focused specifically on the school setting is more limited. Reviews cover a variety of interventions and outcomes, including impacts of sanitation on cognitive development and absence[35]; impacts of hand hygiene on respiratory infection,[36 37] diarrhoea[37] and absence due to illness[38 39]; varied impacts of water and sanitation in schools[40]; or impacts of a specific technology (rinse-free hand wash) on illness absence.[41] Reviews vary in their inclusion of preprimary education and age ranges they cover. Many reviews predominantly include studies conducted in high-income countries,[38–41] are overly restricted to randomised controlled trials (RCTs)[36–38 41] or include mostly observational studies[40] that do not evaluate the effectiveness of specific WinS interventions. In 2019, McMichael *et al*[42] attempted to address these limitations by including a wider range of experimental study designs and outcomes from studies specific to low-income countries. However, with the exception of the rinse-free hand wash review (limited focus),[41] all have been unable to use standard meta-analysis and synthesis approaches and relied on narrative synthesis. Consequently, it is difficult to draw firm conclusions on the effectiveness of WinS on pupil health and education beyond the 'mixed impacts' often reported.[42]

Most WinS reviews cite heterogeneity in intervention components, study designs, settings, baseline conditions, points of intervention, outcome definitions and methods for measuring outcomes and exposures as barriers to evidence synthesis,[35 38 39 42] but many do not elaborate on specific intervention components or focus on a narrow range of outcomes without adequate detail on their definitions, approaches used for follow-up or outcome construction. Beyond menstrual hygiene interventions, existing reviews have failed to incorporate gender-disaggregated data. A review examining educational outcomes of sanitation[35] notes that few studies attempt to link absence and illness, nor report intermediary outcomes (eg, environmental contamination, sanitation quality) or more specific indicators (eg, menstruation absence) that might help to validate intervention theory; however, these are not typically a focus of WinS reviews. A 2021 evidence map of WASH in LMICs highlighted the need for high-quality systematic reviews of WASH in schools, given the increasing number of publications and focus[43]; indeed, several studies were published in the 5 years since the McMichael *et al*[42] review. However, instead of aiming to more effectively integrate study findings, estimating a mean effect of select components on select outcomes and potentially averaging over important data patterns,[44 45] we used the scoping review framework to assess the breadth and variety of WinS interventions and evaluative approaches. Drawing on previous research,[46] we present a comprehensive inventory of pupil health and educational outcomes to better understand why so few can be combined across studies, aiding future evaluation design and evidence synthesis.

METHODS

The review protocol was preregistered with the Open Science Framework (https://doi.org/10.17605/OSF.IO/AQHNF). Inclusion criteria and data extraction were refined iteratively. This scoping review is reported according to the Preferred Reporting Items for Systematic Reviews and Meta-Analyses extension for Scoping Reviews.[47] Some aspects of the methods were aligned with a recent systematic review on the effectiveness of WASH in domestic settings on diarrhoea by Wolf *et al*,[33] such as included types of study design, search terms and interventions compared. This review was based on the

Arksey and O'Malley framework for scoping reviews, which comprises five stages.[48]

Stage 1: identifying the research question

The primary research questions guiding this scoping review were:

1. 'What are the WASH interventions and strategies used to improve health and education outcomes of school children in LMICs?'
2. 'How is the effectiveness of WinS interventions evaluated in LMICs?'

Stage 2: identifying relevant studies

We searched seven databases (MEDLINE, Embase, Global Health, Web of Science, SCOPUS and the Cochrane Library) on 27 November 2023, with no restrictions on date of publication, and included studies published in a peer-reviewed scientific journal in English or French. We also searched grey literature using the World Bank e-library. The search strategy (online supplemental appendix I) combined terms for WASH interventions and epidemiological studies (refined from the previously mentioned systematic review[33]) with terms identifying schools and school MeSH terms. Terms identifying LMICs adapted from Cochrane guidance[49] to reflect current World Bank definitions at the time of the search were used. The search strategy was refined by review of MeSH terms and text in the title, abstract and keywords of a small sample of documents from the authors' collections to ensure relevant articles were identified by the search strategy. Reference lists of included studies were handsearched for additional studies, and studies included in previous WinS reviews[35 37–42] were also screened for eligibility. We used EndNote (Clarivate, Philadelphia, Pennsylvania, USA) for deduplication, Rayyan for managing blinded title and abstract screening,[50] and Microsoft Excel for data extraction.

Stage 3: study selection

Inclusion criteria for this review were broad to allow characterisation of diverse interventions and outcomes (online supplemental appendix II). Eligible study designs were randomised (including individual-randomised and cluster-randomised) controlled trials; quasi-randomised and non-randomised studies, when baseline data on the main outcome were available before the intervention was implemented (ie, controlled before-and-after studies); case–control and cohort studies; and studies using time-series and interrupted time-series designs. Interventions were implemented principally in a primary or secondary school setting and included at least one component, or any combination of components, aiming to improve school WASH services or promote WASH-related behaviours among schoolchildren, school staff or caregivers (if delivered in the school setting). Menstrual hygiene management (MHM) interventions were only eligible if implemented alongside another WASH component. Studies were eligible if they reported at least one primary or secondary outcome measured on pupils and compared a clearly specified intervention against a usual care or active control group.

Where there were multiple study reports, we included all with pupil outcomes measured in both the intervention and control arm. We excluded study protocols, articles reporting only the design or baseline data, process evaluations, secondary analysis, economic/cost-effectiveness evaluations, commentaries/opinion articles and conference abstracts. Studies that were described as pilot studies but otherwise met inclusion criteria (ie, not limited to feasibility outcomes) were included.

Duplicates were documented and removed, and two reviewers (SB and alternately, KD or MM) independently screened a random 10% sample of titles and abstracts of records identified during searches. Once agreement was reached, one reviewer screened the remaining records. If eligibility was unclear, the article was included in full text review. Two reviewers (SB and alternately, KD, MM or RD) independently screened all full texts. Disagreements between reviewers over title and abstract screening, full-text review and reasons for exclusion were reconciled through discussion with the larger research team. Data were then extracted from all eligible studies.

Stage 4: charting the data

Data were extracted from included studies using a pretested data charting form, piloted by two reviewers (SB and KD) (online supplemental appendix III). Two reviewers (SB, and alternately KD, MM and CM) extracted data from a random sample of 10 included studies, and as agreement was reached, data were extracted from the remaining articles by one reviewer. Any queries regarding data extraction were discussed and resolved by consensus among the research team. Adjustments to the data charting form were made iteratively and documented.

Extracted data included information on interventions, outcomes, study design, contextual factors and reporting of results, in line with the review objectives. Detailed information on study outcomes—including case definitions and measurement, sampling and follow-up of pupils, analytical methods and results—was recorded separately for each health or education outcome measured on pupils preintervention and postintervention (or postintervention for randomised trials). Other outcomes (eg, behavioural assessments, teacher-level outcomes or conditions of school WASH facilities) were listed. As such, for some studies, there were multiple sampling or analytical approaches recorded. For other studies, very limited data were collected, for example, if all outcomes were behavioural. Pathogen detection on hands was considered a proxy for handwashing behaviour and absence due to illness was counted as both a health and educational outcome. When multiple intervention arms were tested against a control, we recorded information on interventions and sample size by intervention arm/comparison separately. For non-randomised studies, we considered only those outcomes measured both before and after intervention. Where there was missing, incomplete or inconsistent information in the included papers, we consulted corresponding sibling publications (such as protocol or baseline papers) to clarify the missing information.

Stage 5: collating, summarising and reporting results

We described interventions by combined and individual components. We categorised interventions in line with a previous systematic review,[33] identifying water interventions as the presence of an intervention component improving either the quantity or quality of water at school (regardless of other components), sanitation interventions as the presence of an intervention component improving either the quantity or quality (eg, cleanliness) of sanitation services at school (regardless of other components), handwashing infrastructure or supplies in the absence of water or sanitation components, and behaviour change promotion in the absence of provision of any infrastructure or consumables. Installation of drinking water stations was counted as both water quantity and quality if intervention descriptions specified some aspect of safe containment. For each category, we examined any reporting of baseline conditions to assess service levels for WASH in schools as defined by the WHO/UNICEF Joint Monitoring Programme.[51] As some studies had multiple interventions that were in different categories (hygiene infrastructure and behaviour change promotion only), we reported findings by intervention arm comparison instead of by study for some analyses. As this was a scoping review, a formal risk-of-bias assessment was not conducted.[47]

For those health or educational pupil outcomes that were plausibly WASH-related (ie, not intended to measure the impact of a non-WASH intervention component), we defined outcomes by four elements, adapted from Mayo-Wilson *et al*[46]: (1) outcome domain, (2) specific measure (including case definition and outcome reporter), (3) metric (for categorical outcomes dependent on baseline status of participants) and (4) method of follow-up and data aggregation. For parasite prevalence outcomes, we defined four metrics: (true) 'reinfection' measured in one study as prevalence of infection among those positive at baseline and subsequently treated (confirmed effective); 'prevalence of incident cases' (sometimes reported as reinfection), among those testing negative at baseline (assumed true negatives) or treated at baseline (treatment assumed effective, and if part of national deworming campaign and not provided by the study assumed to have complete coverage of study population); 'overall prevalence', prevalence regardless of baseline status or where coverage of deworming was unclear; and 'persistence', prevalence among those testing positive at baseline without subsequent specific treatment. Teacher-reported and parent-reported outcomes were sometimes combined when similar: these were measured by trusted adults reporting on child outcomes and often measured collaboratively between reporters. We describe 'unique' outcomes as a defined outcome (with all four elements specified) counted only once, regardless of how many times it appeared across included studies, and 'non-unique' outcomes (or results) as defined outcomes counted each time they appear[46]—what is typically understood as total number of outcomes—including if they appear across multiple intervention arm comparisons within the same study. As studies did not consistently report whether outcomes were primary or secondary, and to avoid bias due to outcome selection,[52] we opted to present all unique outcomes without restriction to primary outcomes. Finally, we identified gender-disaggregated outcome results as those results for which an effect size and measure of precision was reported separately by gender, and 'potentially meta-analysable results' as those results for which sufficient information was provided in the study reports for potential inclusion in a mathematical synthesis with other studies (given alignment on outcome measures): a point estimate and a measure of precision or necessary information to calculate one according to the analytical description (eg, numerators and denominators for simple ratio measures).

We report analysis of key intervention components, study designs and settings, unique and non-unique health and educational outcomes, measurement of outcomes from multiple domains (and intermediary outcomes) simultaneously, and whether results are disaggregated by gender or other key moderators. We cite a single index paper for each study throughout, although data may have been taken from a different report (all references listed in online supplemental appendix IV).

RESULTS

We identified 11 749 records through database searches conducted on 27 November 2023 and 180 records from previous systematic reviews and removed 4570 duplicates (figure 1). We reviewed 214 full-text reports and identified 20 additional relevant reports by searching the reference lists and citing articles of included reports. 83 studies (a study conducted in parallel in Cambodia, Indonesia and Laos reported in one publication[29] was counted and is hereafter reported as three separate studies) met inclusion criteria (referenced in online supplemental appendix IV), reported in 99 publications. Studies excluded during full-text review are listed in online supplemental appendix V with reasons for exclusion. Of the included studies, 18 were common to one of the seven previous WinS reviews identified,[35 37–42] and the remaining 65 were newly identified through our searches, with 34 of these published since the McMichael *et al* review[42] (online supplemental appendix VI). Studies were conducted across 33 different

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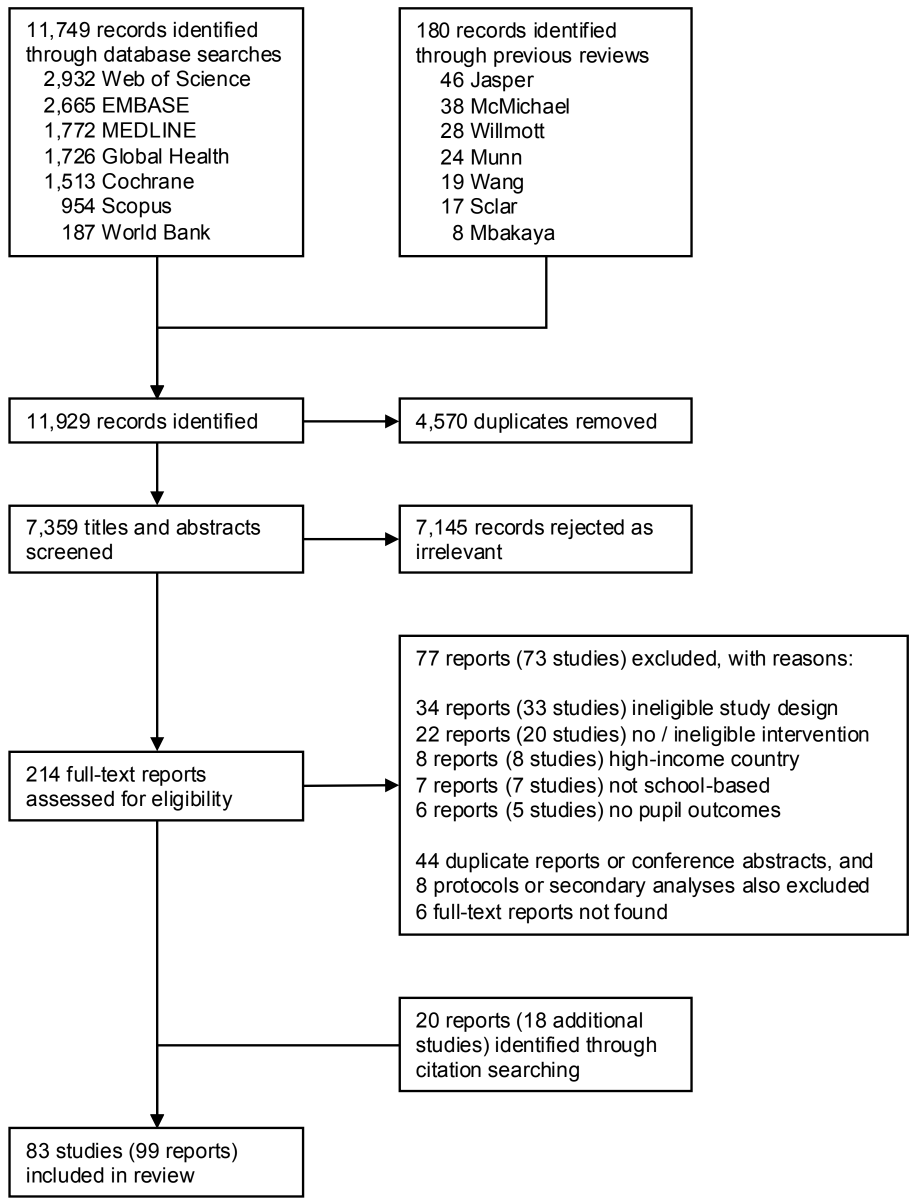

Figure 1 PRISMA flow diagram. PRISMA, Preferred Reporting Items for Systematic Reviews and Meta-Analyses.

countries and various world regions (online supplemental appendix VII), most commonly in Sub-Saharan Africa (33 studies, 40%), East Asia and Pacific (25 studies, 30%) and South Asia (11 studies, 13%). Only 7 were in low-income countries (8%), with others in lower-middle-income (49, 59%) and upper-middle-income (27, 33%) settings.

Across the 83 studies, we identified 93 unique intervention arms/comparisons. There was considerable overlap between interventions addressing the quantity or quality of water, or of sanitation facilities available to pupils (24 comparisons, 26%), with 19 of these delivered in combination with handwashing facilities (HWF) or supplies and 21 alongside behaviour change promotion (figure 2, left). The 16 interventions (17%) providing handwashing infrastructure or supplies in the absence of water or sanitation, and the 53 (57%) addressing behaviour change promotion alone were analysed separately. The study-level dataset, including descriptions of the key features of interventions and their comparison groups, is found in online supplemental appendix VIII. Most interventions (72; 77%) were implemented in primary school settings.

Wide spectrum of WASH technologies and approaches adapted for the school setting

These broad groups encompassed a variety of components and approaches (online supplemental appendix VIII). Two water quality interventions aimed to improve the chemical quality of water,[53 54] and two water quantity interventions were intended to improve adequate

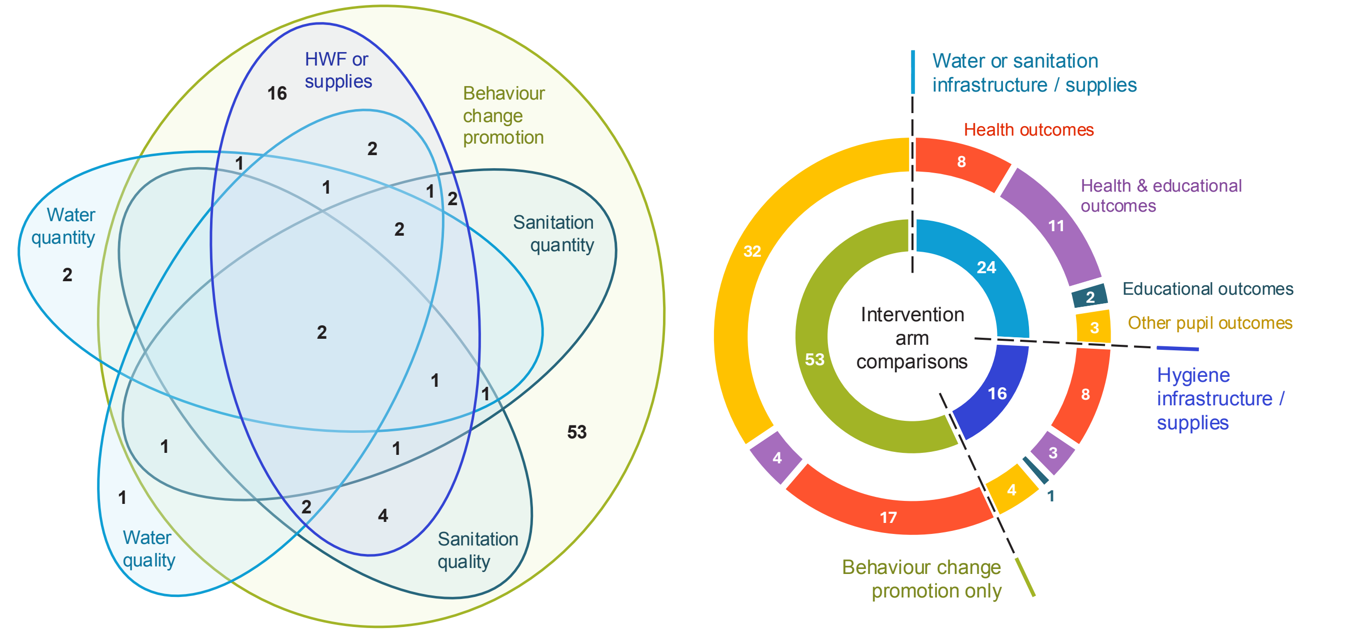

Figure 2 Left: Venn diagram of interventions components compared across included studies. Right: outcomes assessed across three intervention arm categories. HWF, handwashing facilities.

hydration over a short timeframe.[15 55] Other WASH infrastructure interventions focused primarily on reducing infectious disease transmission and were intended to be sustained over time. Water quantity interventions included installation of new water points to supply clean water for drinking and hygiene purposes (to schools with limited water access at baseline),[56–59] water pipes or rehabilitation of water points[56 57 60–62] and water dispensers or water bottles to ensure adequate hydration.[15 63 64] Water quality interventions comprised point-of-use water treatment or safe drinking water storage. Sanitation quantity interventions were most frequently the construction of latrines,[8 9 56–61 65] often separated by gender, and to meet national standards for latrine: pupil ratios[58 59] and with disabled access or separate facilities for disabled pupils.[57 59] Sanitation quality interventions were varied, including cleaning supplies, modifications to toilets to improve privacy, hiring of cleaners or periodic repairs to facilities. Hygiene interventions included provision of HWF (often group handwashing sinks/troughs[29 57 66]), hand sanitiser, soap or materials to make soapy water,[61 67] alongside measures to prevent soap loss or monitor availability of supplies.[68 69] Eight interventions (six studies) involved establishing management systems to ensure sustained financing, monitoring and maintenance of facilities.[56 58 59 66 67 70]

Posters, murals, stickers or signs designed to provide behavioural reminders were included in 27 interventions (online supplemental appendix VIII). Other behavioural 'nudges'[71] included brightly painted paths, footprints and handprints around HWF,[68 72 73] visual modifications to HWF features and materials,[74] soap-on-a-rope as a hall pass (disruptive cue),[75] glo-germ gel for visual feedback on behavioural technique (HWWS)[76 77] and a urine colour chart in lavatories for visual feedback on behavioural consequences (dehydration).[55] Behaviour change components focused primarily on hygiene education, but often mixed with play-based approaches (songs, games, plays and creative contests). 12 programmes involved pupils taking on leadership roles or participating in a 'hygiene club'.

Intersecting and evolving evidence base

Several studies evaluated the same intervention in multiple locations or were nested within larger trials in order to test the impact of additional intervention components. These include parallel trials of comprehensive WASH and school garden interventions in Burkina Faso[61] and Nepal[9]; regional expansion of a programme promoting daily group handwashing and toothbrushing ('FIT for School') developed in the Philippines[66] to Cambodia, Indonesia and Laos,[29] and further development of the approach to target toilet cleanliness ('FIT Plus') in the Philippines[78]; a trial evaluating a latrine cleaning and handwashing programme in Kenya[67] nested within a larger trial evaluating multiple WASH infrastructure improvements ('SWASH+')[58]; adaptation of the 'Magic Glasses' health education for STH prevention programme in China[79] to the Philippines[80]; a non-randomised evaluation of a hygiene programme in Kenya[63] with an embedded cluster-randomised trial testing the impact of a poster contest to improve HWWS[81]; an evaluation of water provision in Zambia[64] that was first tested in a pilot field trial in Mali[15]; and interventions in Nigeria[82] and Indonesia[83] that drew inspiration from an earlier 'worms and ladders' board game for improving helminth control in Nigeria.[84] As such, interventions evaluated built on previous research over time.

As well as the school gardens[9 61] and toothbrushing interventions,[29 66 78] WASH improvements were also delivered alongside vector control measures exploiting the benefits of safe water storage in reducing transmission of both dengue and diarrhoeal pathogens.[85] One study contrasted the effect of WASH improvements against community snail control to prevent schistosomiasis.[86] 18 interventions were implemented alongside school-based deworming in both intervention and control groups, either to all pupils[56 68 70 79 82 87–92] or those found infected at baseline,[58 61 93–96] to estimate impacts of WASH interventions on helminth reinfection. None estimated the combined impact of WASH and deworming against a usual care control group. Only two of the behaviour change promotion interventions included promotion of MHM; none of the interventions providing WASH infrastructure included MHM materials or education. Installation of gender-segregated sanitation facilities was the only intervention component with an explicit gender focus.

Limited shared evaluation methods, outcomes and measurement approaches

Across the 83 studies, a substantial proportion (43%, 36 studies comprising 39 intervention arms) measured only behavioural or knowledge outcomes among pupils, particularly those evaluating behaviour change alone (figure 2, right; online supplemental appendix IX) and were excluded from detailed analysis of outcome measures and assessment. These included the five interventions that were primarily nudge-based.[72–75 81] Of those remaining, 24 were cluster-RCTs (51%), 21 were non-randomised or quasi-randomised studies (45%) and 2 were small-scale individual-RCTs (4%). Detailed information on study designs and follow-up methods is provided in online supplemental appendix VIII. Study quality and reporting varied substantially; those evaluating behaviour change promotion alone included five studies allocating interventions to one intervention and one control school (cluster) preventing separation of treatment effects from systematic differences between clusters, and eight studies not reporting any statistical comparison of study arms. Studies enrolled over 193 000 pupils in total (median 660 per study) across 1398 schools (median 10 per study). Study size varied from a single school in Benin[53] to 200 schools across Mali,[59] and from 80[97] to over 56 000[58] pupils. Duration of data collection periods ranged from a single day[64] to 5 years[86] with a median of 8 months (IQR 5–14.5). Galetti *et al*'s study evaluated the only masked intervention, as participants were unaware whether they were drinking filtered or unfiltered water.[53]

Specific aspects of study designs allowed authors to test hypotheses relevant to research objectives. Five multiarm trials[8 58 67 69 98] were designed to compare different combinations of WASH components. Data from incoming first-grade pupils were used to assess impacts on the force of infection in surrounding communities.[86] Analysis of intracluster correlation coefficients indicated chronic illness or repeat episodes of acute illness.[69]

Among studies reporting statistical comparisons of study arms for at least one relevant outcome, 37 studies measured 313 non-unique (regardless of measurement approach) health or educational outcomes among pupils, spanning 14 outcome domains (figure 3). Common outcome domains varied by the intervention evaluated—parasitic infection status or intensity accounted for over a quarter of total non-unique outcomes (89) and these were mainly impacts of water or sanitation infrastructure or behaviour change promotion alone; hydration, anthropometric and educational outcomes were measured in water or sanitation infrastructure evaluations; and evaluations of hygiene infrastructure/supplies

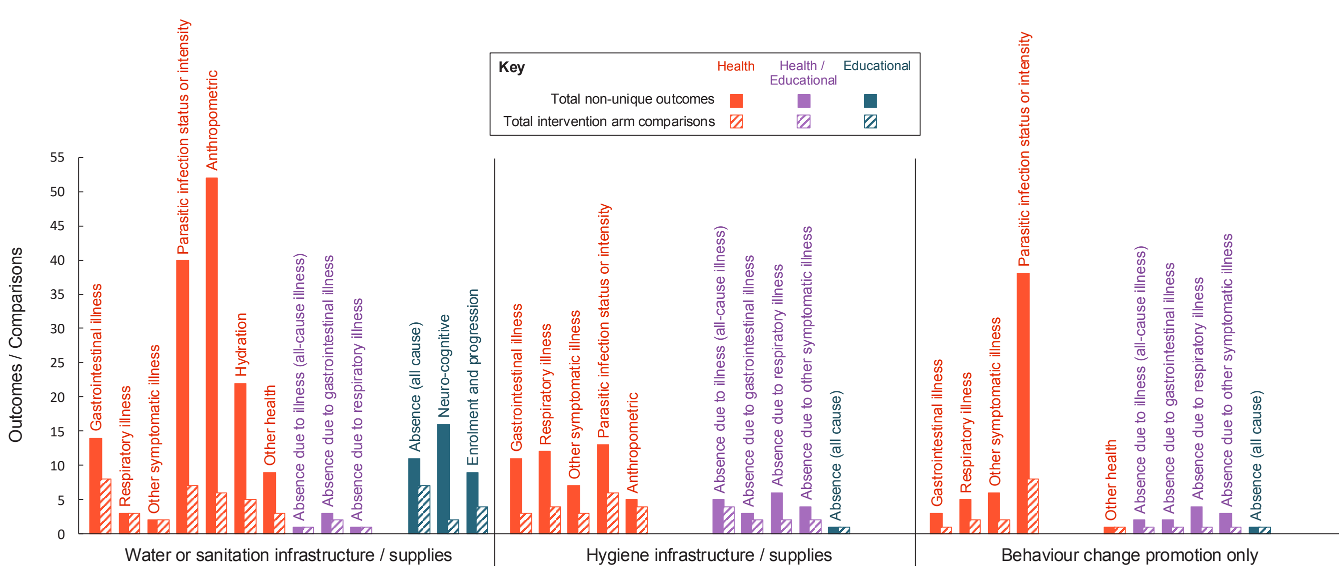

Figure 3 Total non-unique outcomes measured and total intervention arm comparisons by outcome domain, across three intervention categories.

covered a range of symptomatic illness and absence due to illness outcomes (figure 3).

At the study level, we noted differences in the scope and range of outcomes measured (figure 4A). Several studies measured close to one outcome per domain,[29 63 66 99] some measured many outcomes across a few domains (eg, Shrestha *et al*[9] constructed a variety of anthropometric indicators from height and weight measurements) and Bowen *et al*[98] measured many outcomes across many domains (both in-class illness and absence due to illness using reporting of individual symptoms across multiple disease categories).

Within-study overlap between outcome domains (figure 4B; measurement of at least one outcome from the respective domains within the same study) indicated expected relationships between outcomes aligned with intervention theory—for example, between parasitic infection and anthropometric outcomes, various symptomatic illness outcomes, and hydration and cognitive outcomes—and clear gaps—for example, only one study[59] measured both all-cause absence and absence due to illness. Studies measuring gastrointestinal illness, respiratory illness, parasitic infection status or intensity and all-cause absence were also likely to measure school WASH conditions and pupil behaviour as intermediary outcomes. Knowledge was measured alongside impacts on parasitic infection. Five studies[15 61 64 89 99] tested correlations between multiple pupil outcomes. Overgaard *et al*[85] was the only study to assess the relative contribution of illness to absence and include a full list of illness-related absences.

Finally, we examined outcome definitions and methods used for follow-up and analysis, matching similar outcomes across studies, and reporting of effect sizes. Out of the 313 non-unique outcomes, there were 273 potentially meta-analysable outcome results (87%) assuming other aspects of measurement are consistent. However, outcomes were measured using 158 unique outcome measures/assessment approaches (figure 5A; complete inventory of all measures and definitions in online supplemental appendix X), of which 114 (72%) were unique to a single study and only 20 (13%) were assessed similarly across at least three different studies.

14 unique gastrointestinal illness outcomes were used across eight studies (online supplemental appendix X). Although seven measured pupil-reported diarrhoea outcomes using the same WHO definition, recall periods (7-day, 2-day, 1-day) and follow-up methods (single endline measurement vs repeated measures) differed. 12 respiratory illness outcomes were measured across seven studies, with one assessed similarly in two studies[57 59]: pupil-reported respiratory illness defined as any episode of cough, rhinorrhoea, coryza or sore throat in the past 7 days, assessed through repeated measures. Studies defined different combinations of symptoms (eg, acute respiratory illness defined as fever and cough or difficulty breathing[63]) or recorded symptoms separately.[69] Some symptoms were instead observed by teachers or by enumerators, and one study confirmed influenza-like illness reports through detection of influenza A or B viruses in nasal swabs.[100] There were no outcomes of other symptomatic illness measured in more than one study. Outcomes that served as negative control questions about symptoms unrelated to WASH access (cuts/scrapes, toothache)[57] were not included in the inventory.

Infection status or intensity of parasitic infection was measured using 47 unique outcomes (online supplemental appendix X), predominantly through collection of stool samples from pupils. A variety of STH, schistosomiasis, protozoan and other parasitic infections were assessed, and some outcomes combined detection of multiple species. Assessment approaches differed according to whether reinfection, prevalence of incident cases, overall prevalence or persistence was of interest—this distinction contributed 14 of the outcomes. Five studies testing reinfection or incident cases verified the effectiveness of baseline deworming.[68 89 92 93 95] 25 anthropometric outcomes were assessed in 8 studies, all of which were constructed based on standard weight and/or height measurements. Studies constructing categorical outcomes used the same sex-standardised and age-standardised threshold values, but differed by measurement of incidence, prevalence or persistence (contributing six additional outcomes), and follow-up and aggregation method (five additional outcomes). Single endline measurements of height, weight, height-for-age, stunting (low height-for-age) and BMI-for-age were used similarly by three studies.[8 9 61]

Three studies evaluating water quantity interventions measured hydration status[9 53 61] using similar approaches: measurement of specific gravity (Usg) in urine samples at endline, and the same definition of any dehydration based on Usg threshold. Anaemia and haemoglobin concentration were measured by two studies[9 61]; other health outcomes were unique to exactly one study.

Absence due to illness (all-cause illness) was measured in four studies,[62 69 98 99] and three used a combination of teacher-reported absence verified through parents but differed on aggregation of days of absence or any absence in the past week. Teacher-reported or parent-reported absence due to diarrhoea (using the WHO definition of at least three loose stools in a 24-hour period) in the past week was used in three studies. All other absence due to illness outcomes were measured exactly once. None addressed menstruation-related absence. Seven studies measured all-cause absence using six unique outcomes. Roll-call absence assessed at several discrete points (visits by data collection teams) and aggregated over the follow-up period was most used. One study measured both pupil-reported and roll-call absence,[58] however, effect sizes for roll-call absence were not reported. Two studies[57 58] measured total school enrolment with the same approach, and dropout, progression to the next grade, and gender

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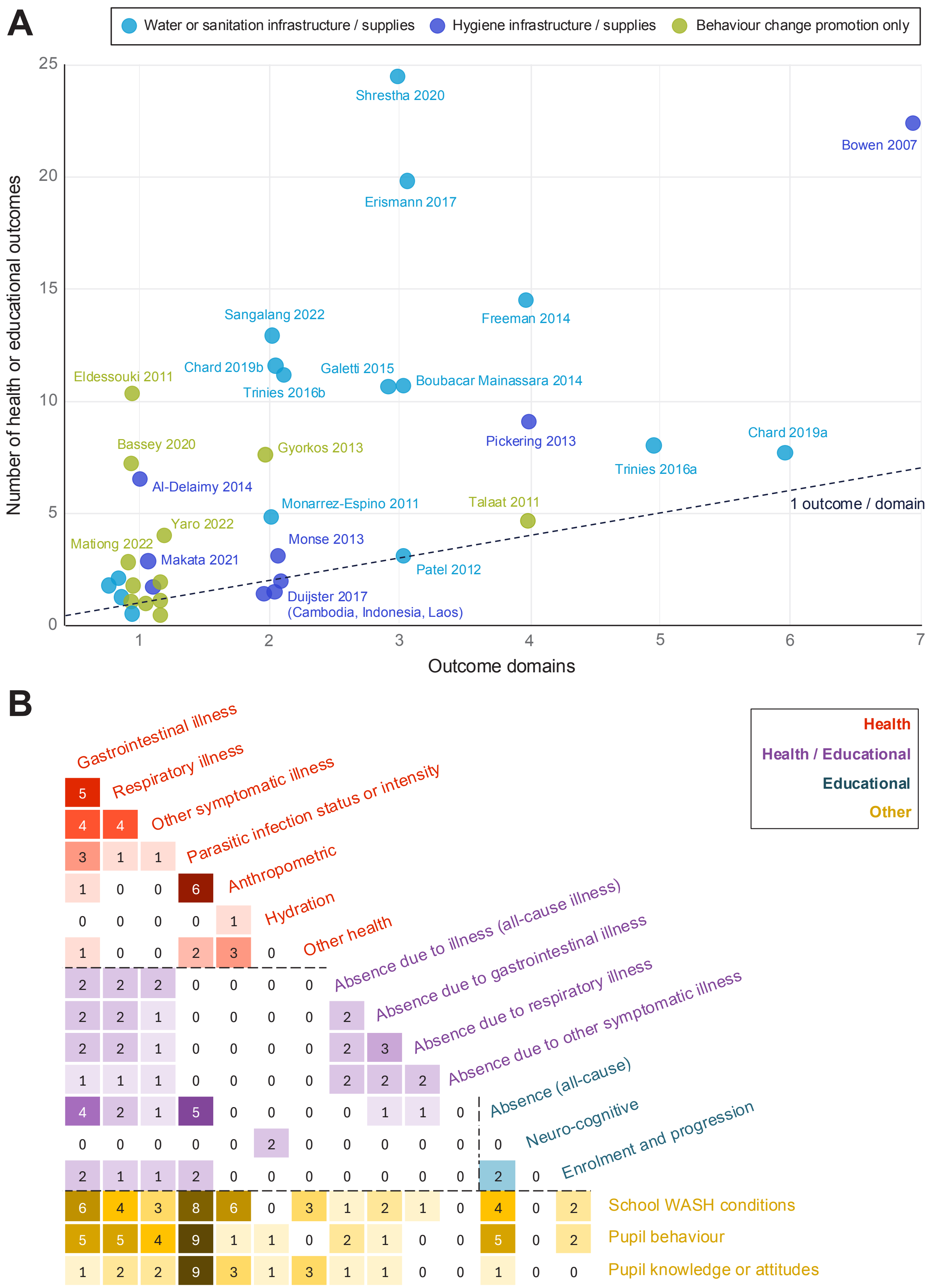

Figure 4 (A) Within-study relationship between number of health or educational outcomes and number of different outcome domains. Overlapping points separated by jitter effect. Studies measuring one or two outcomes in a single domain not labelled. Dashed line indicates one outcome per domain. (B) Within-study overlap between outcome domains, reporting number of studies measuring at least one outcome in both the horizontal and vertical domains. WASH, water, sanitation and hygiene.

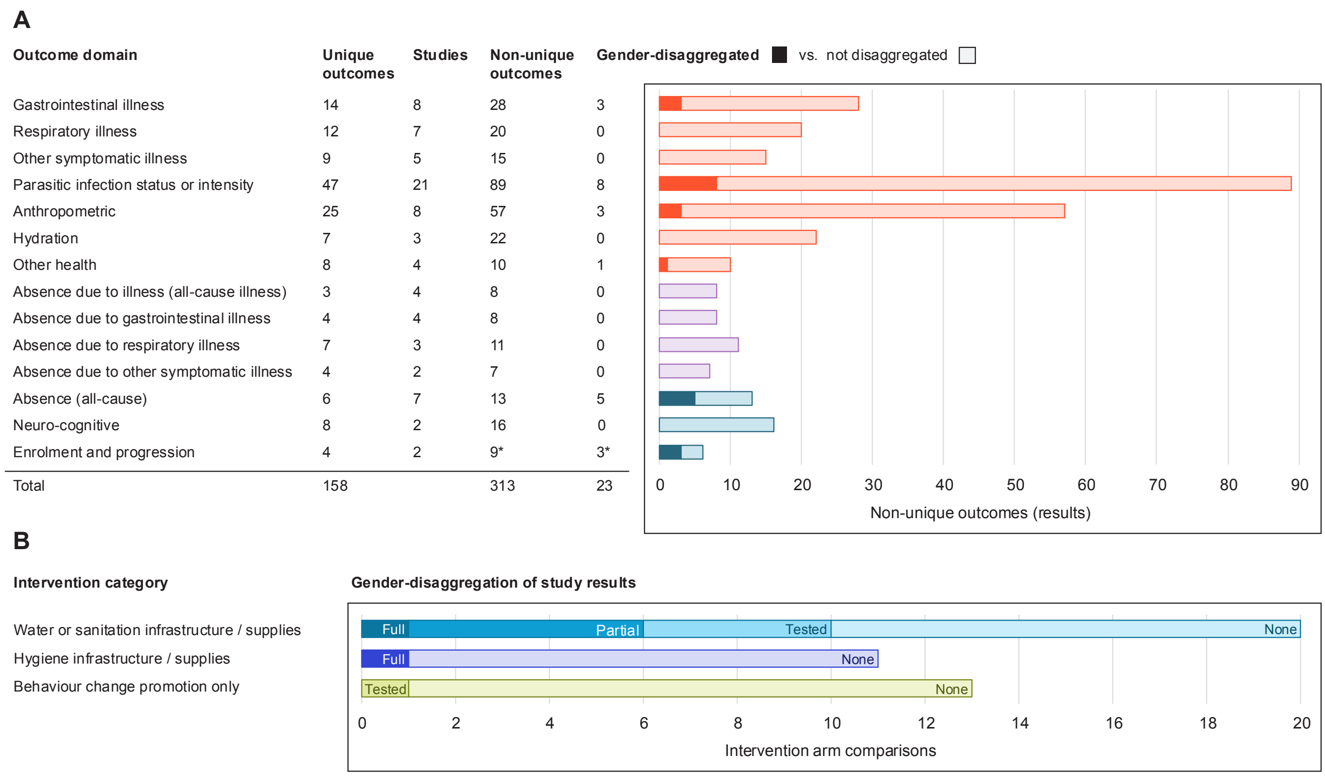

Figure 5 (A) Total number of unique and non-unique outcomes by outcome domain (left) and disaggregation of results by gender at the outcome level (right). Right: non-unique outcomes by domain that were reported disaggregated by gender (solid bars) versus not disaggregated (lighter shading). *Out of six results—the three enrolment results for Freeman *et al*[58] are already disaggregated by means of the gender parity in enrolment outcome and, therefore, not included in the denominator. (B) Disaggregation of study results by gender at the intervention level in the same 37 studies reporting treatment effects on health or educational outcomes. Number of intervention arm comparisons for which results were gender-disaggregated. 'Full': results for all health or educational outcomes were gender-disaggregated; 'partial': disaggregation of some outcomes; 'tested': study authors report conducting subgroup analysis or interaction tests by gender but did not report disaggregated results; 'none': no gender-disaggregated outcomes.

parity in enrolment were measured by individual studies.

Under-reporting of gender-disaggregated results and other subgroup-specific impacts

Of 310 outcomes that were not gender-specific (excluding gender parity outcomes), only 23 results (7%) were disaggregated by gender, in 4 studies.[58 59 61 67] Of these, one study was fully disaggregated[67] and three reported gender-disaggregated effect sizes for some study outcomes.[58 59 61] Five other studies describe gender subgroup analyses/tests for interaction, but do not report gender-disaggregated results.[15 57 64 89 95] Separating by intervention arm (figure 5B; studies double-counted if they had multiple intervention arms), gender-disaggregated results are reported primarily in evaluations of WASH infrastructure or supplies. At the outcome level (figure 5A), gender-disaggregated results are spread over a few health or educational outcomes, with none of the absence due to illness outcomes disaggregated. Four studies tested for interaction with pupil age or grade[59 61 64 95] but do not report disaggregated results. Other tested effect modifiers were more specific to individual study designs and objectives (online supplemental appendix XI). Seven studies examined the effects of intervention fidelity or adherence, or of intermediary outcomes such as school WASH conditions, on study outcomes.[29 54 57–59]

DISCUSSION

We conducted the largest and most comprehensive review of WinS to date, identifying 83 experimental studies in 33 LMICs, with 65 studies not included in previous systematic reviews specific to WinS. Outcomes encompassed over 300 intervention effects on 14 domains of pupil health and education. Broad intervention categories concealed diverse components and school-adapted WASH strategies. Nonetheless, combined infrastructure interventions were prevalent, and behaviour change interventions often functioned in tandem with other school-based programmes, such as nutrition, oral health and deworming, with the latter accounting for a fifth of interventions. Despite challenges in assessing the impact of WASH in the context of regular deworming,[101] there is clear research interest in understanding the effects of integrated school-based programmes, which capitalise on cost-effective, proven platforms for scale-up, with potential synergistic effects.[7,102] Almost half of the studies measured only behavioural or knowledge outcomes. Analysis of outcome domains indicated areas where links between elements of underlying theory have been tested (eg, parasitic infection status and anthropometric outcomes) and where gaps remain (eg, contribution of illness to overall absence). We explored intervention and outcome heterogeneity and confirm prior findings[35,38,39,42] that that meta-analytical methods are not currently straightforward nor appropriate to apply.

Although 87% of outcomes were reported with adequate detail for potential inclusion in meta-analysis, a likely limiting factor is the 158 unique outcome measures, most appearing in exactly one study. A multitude of outcomes measured in different ways, observed across other research areas,[46,103] severely restricts meta-analysis and risks synthesis errors when combining outcomes assumed to be interchangeable.[46] We noted several studies that measured numerous outcomes across few domains. Within one study, a high number of outcomes increases the probability of false positive findings and can lead to bias in selecting outcomes to report; in aggregate, such bias can afflict systematic reviews that synthesise a broad range of outcomes.[52,104] A strength of our review is thus that we present the full list of outcomes without selection (online supplemental appendix X). However, we acknowledge that there are often many potentially relevant outcomes to choose among, due to multiple reporters on child health within the school setting (parents, teachers and pupils themselves), each with varying reliability, and complex pathways to intervention impact; researchers must balance comparability with suitability to the organisational setting, participant capacity and objectives.[31]

For example, some studies used intensive approaches to track pupil absences, verifying illness-related absence with parents against detailed case definitions (eg, Bowen *et al*[98]). Such approaches may improve assessment reliability, particularly when absence rates are high, but may be impractical given resource limitations. Measuring roll-call absence during data collection alongside teacher-reported or pupil-reported absence may be a simpler way to use data from participants otherwise considered lost to follow-up, assess the accuracy of reported outcomes and evaluate potential bias in outcomes measured at school because of differential loss to follow-up, as found in a recent WinS trial.[105] Stool-based pathogen detection has advantages over self-reported outcomes for measuring diarrhoeal disease, particularly in minimising reporting bias[106] and identifying infective causative agents. Included studies commonly used the Kato-Katz technique for detection of intestinal helminths and protozoa in stool, but quantitative molecular diagnostic methods to detect enteropathogens remain unexplored. For symptomatic illness, composite measures can often be inconsistently defined and combined without justification.[52] In the short term, evaluations might prioritise an existing measure, focus on specific symptoms rather than general ones (eg, fever, stomach-ache) and collect illness symptoms separately (examples are studies in Kenya[69] and China[98]) to allow flexibility in outcome construction in the absence of standardised case definitions.

Previous assessments of the WinS evidence base have recommended that standardised outcome measures be defined for health impacts[38] as well as sustained adoption[107] and behaviour change outcomes[108] of WinS programmes. Development of a core outcome set, an agreed on minimum list of outcomes to record in all evaluations in a given field, is widely proposed to address the inconsistencies in outcome measurement that hinder evidence synthesis[52,103] and could apply to WinS. However, the variability in approaches to outcome measurement we found even among outcomes using identical definitions (eg, WHO diarrhoea definition, height-for-age Z-score, or stool-based detection of *Ascaris lumbricoides* eggs) underscores the need for sets of outcomes whose domain, metric, method of aggregation and time point are completely defined,[46] not just the measure/case definition. Aggregation methods matter for outcomes relevant to schoolchildren; included studies recognise that absence at one time point is operationally different from chronic, repeated absence.[69] Conflation of 'absence' and 'absenteeism' in the literature often obscures this reality.[35] To account for the possibility that the inventory of outcomes we conducted may represent outcomes easily measurable or important to researchers and not stakeholders, qualitative research among key stakeholders in school communities[103] could help narrow recommended outcomes for future evaluations.

While downstream effects on school enrolment, academic attainment and gender parity in education are often stated as motivating rationale for WinS programmes, we found that these outcomes are rarely measured or reported. Only 11% of studies reported any gender-disaggregated outcome data, amounting to 7% of total non-unique outcomes, echoing similar findings from WASH interventions in multiple settings.[109] Even evaluations of interventions that are not gender-specific should report gender-disaggregated results, as general interventions can deliver gains to girls' education access and learning comparable to girl-targeted interventions.[110] Mechanisms for achieving gender parity are also underexplored: included studies do not capture sanitation-related psychosocial stressors and subjective menstrual experiences that may be more indicative of a gendered effect,[111,112] nor examine the contribution of menstruation to overall absence, which is debated.[2] However, we note that this review includes no MHM interventions; a recent trial evaluating MHM alongside WASH infrastructure improvements in the Gambia may provide critical evidence to fill this gap.[113] Most of the evaluations of large-scale school-based interventions to improve gender parity (for example, evaluation of school latrine construction in India[114]) may be quasi-experimental[1] and would, therefore, not meet inclusion criteria. Outcomes of initiatives to construct entirely new 'girl-friendly' schools involving sex-segregated facilities[115] are also not captured. No study examined outcomes by pupil disability status. Effect modification by age or school grade was frequently tested among the included studies, but interventions were rarely implemented in secondary schools or among older teenagers, limiting assessment of variation in effects across portions of the school population.

Lastly, we recommend that future evidence synthesis of the diverse components and approaches that comprise WinS interventions makes use of novel meta-analysis methods to disentangle the effects of critical components. Network meta-analysis enables the inclusion of all available evidence—direct comparisons of distinct interventions and indirect comparisons of combinations not yet evaluated—and may be preferable to 'lumping' intervention components or conducting extensive subgroup analyses.[116] We imagine that the limited examination of interventions in the present review may be a starting point for mapping the varied modalities, functions and theories[116,117] underlying WinS interventions. Comparing, for example, knowledge-based versus alternative behaviour change strategies, promotion of group vs individual behaviours, various behavioural cues,[71] helminth-control approaches (reducing pathogen exposure vs reducing contribution to the environmental reservoir[101]), and evaluating the role of community involvement, pupil 'hygiene clubs', general funds for WASH[118] and accountability and information sharing mechanisms[25] that are key drivers of sustainable WinS programmes, may enrich understanding of the effectiveness of WinS in LMICs.

Limitations include restriction to studies published in English or French and focussing primarily on peer-reviewed journals vs grey literature. Despite hand searches of citation lists that yielded 18 additional studies, it is possible that some relevant studies were not identified. We did not formally assess study quality or risk of bias; consequently, studies with a high risk of bias may be over-represented, and the true extent of study heterogeneity may be underestimated. As we analysed three of five elements defining an outcome described in Mayo-Wilson *et al*[46] (excluding time point and change from baseline) and did not further combine definitions with methods of analysis,[46] we have likely undercounted the true number of treatment effects across studies. Studies also occasionally reported additional results that were not prespecified. It is possible that some parasite prevalence outcomes were misspecified as few studies measured the effectiveness of baseline deworming and treatment failure, inadequate coverage of national deworming campaigns, or false negatives at baseline may lead to misclassification as an incident case. In conducting meta-analysis, data are often obtained by contacting authors; our assessment of potentially meta-analysable results based on published reports may be an underestimate. Because primary and secondary outcomes were not always defined, we were unable to explore whether the same variation remains when restricting to primary outcomes. We were unable to explore additional sources of heterogeneity, such as public versus private sector implementing agents[1] and differences between urban and rural settings within countries, within the scope of the review.

CONCLUSIONS

This comprehensive scoping review of 83 school-based WASH evaluations in LMICs revealed a complex and heterogeneous evidence base, with numerous uniquely defined health and educational outcomes complicating meta-analysis, and inconsistent reporting of gendered impacts limiting synthesis of the full range of potential impacts. Our findings call for the development of standardised outcomes accounting for all aspects of outcome assessment, elaboration of school-specific WASH intervention modalities and functions, and greater attention to gender-specific impacts. Despite the challenges identified, the diversity of programmes underscores the potential of integrated, school-based WASH programmes to address multiple aspects of the health and education of schoolchildren.

Acknowledgements Findings from this work were presented at the UNC Water and Health conference in October 2024.

Contributors RD and SB conceptualised the research study, and SB developed the review protocol with input from KD, MM, JC, KC and RD. SB performed searches, and SB, KD, MM and RD screened article titles and abstracts and reviewed full texts. SB, KD, MM and CM extracted data using the data charting form developed by SB and piloted by SB and KD. SB, KD and LB analysed extracted data. SB wrote the first draft of the manuscript. All authors (SB, KD, MM, CM, LB, JC, KC and RD) reviewed and contributed to subsequent drafts, and read and approved the final manuscript. RD is responsible for the overall content as guarantor.

Funding This research was funded by the UK's Foreign, Commonwealth and Development Office (grant code: 301186).

Competing interests None declared.

Patient and public involvement Patients and/or the public were not involved in the design, or conduct, or reporting, or dissemination plans of this research.

Patient consent for publication Not applicable.

Provenance and peer review Not commissioned; externally peer reviewed.

Data availability statement All data relevant to the study are included in the article or uploaded as supplementary information. All data collected for this study are reported in the manuscript and additional files.

terminology, drug names and drug dosages), and is not responsible for any error and/or omissions arising from translation and adaptation or otherwise.

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