The Impact of a School-Based Water, Sanitation, and Hygiene Program on Absenteeism, Diarrhea, and Respiratory Infection: A Matched–Control Trial in Mali

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Abstract. We conducted a matched–control trial in Mali to assess the effectiveness of a comprehensive school-based water, sanitation, and hygiene (WASH) intervention on pupil absence, diarrhea, and respiratory infections. After completion of the intervention, data were collected from 100 beneficiary schools and 100 matched comparison schools in 5–6 sessions over a 14-month period. Data collection included roll calls to assess absenteeism and interviews with a subset of pupils to assess recent absence and disease symptoms. The odds of pupils being absent at roll call were 23% higher in beneficiary schools than in comparison schools (odds ratio [OR]: 1.23, 95% confidence interval [CI]: 1.06, 1.42). The odds of pupils reporting being absent due to diarrhea (OR: 0.73, 95% CI: 0.56, 0.94) or having had diarrhea (OR: 0.71, 95% CI: 0.60, 0.85) or respiratory infection symptoms (OR: 0.75, 95% CI: 0.65, 0.86) in the past week were lower in beneficiary schools compared with comparison schools. We found that a school-based WASH intervention can have a positive effect on reducing rates of illness, as well as absence due to diarrhea. However, we did not find evidence that these health impacts led to a reduction in overall absence. Higher absence rates are less likely attributable to the intervention than the result of an imbalance in unobserved confounders between study groups.

INTRODUCTION

School-aged children are at high risk for water, sanitation, and hygiene (WASH)–related morbidities, including soil-transmitted helminths^{1,2} and trachoma³; this age group experiences over 2.8 billion cases of diarrhea annually.⁴ These infections are associated with increased absence,⁵ which can lead to decreased academic performance and increased likelihood of dropout^{6,7}; this, in turn, prevents children from attaining the numerous economic and health benefits associated with educational attainment.⁸ Poor school WASH environments may facilitate the transmission of WASH-related infections between pupils, and improved access to WASH at school may have the potential to reduce the risk of disease and absenteeism among school-aged children. Although many policy makers and development organizations promote inclusion of school WASH within a rights-based framework, understanding the potential impacts and rigorous evaluation of programs is critical for policy decisions and to ensure effective programming. This is especially true in light of the likely inclusion of school WASH indicators in the post-2015 Sustainable Development Goals.

The evidence from randomized trials of school-based WASH improvements on health outcomes has been mixed. A multiarm trial of comprehensive school WASH interventions in Kenya found reduction in self-reported diarrhea among pupils in school that received water supply compared with controls, although no effect was seen among pupils in schools that received only a water treatment, hygiene promotion, and sanitation intervention.⁹ A handwashing intervention in China found no significant effect on diarrheal illness.¹⁰ Some evidence exists on the impact of school WASH on soil-transmitted helminths.¹¹ Few trials have examined the impact of school WASH on respiratory outcomes. Talat and others showed that pupils who received an

intensive hand-hygiene promotional campaign were less likely to have influenza compared with pupils in control schools, while Bowen and others did not find any significant difference in rates of respiratory infection symptoms.^{10,12} Children spend considerable time at school during the school year, even so, it is not known if reduced exposure to pathogens in the school environment is sufficient to reduce illness when household conditions are poor.

Trials investigating the effect of school WASH on absenteeism have shown equally mixed results. In Cambodia, a quasi-experimental study found reductions in absence from provision of safe drinking water, though only in the dry season.¹³ School-based randomized trials in China and Egypt found lower rates of both absenteeism and absenteeism related to certain illnesses among pupils that participated in handwashing interventions.^{10,12} A handwashing trial among preschool children in Israel found no effect on overall absenteeism or absenteeism due to illness.¹⁴ In Kenya, no overall effect of a comprehensive WASH intervention was found, although a reduction in absence was found among girls.¹⁵ Similarly, null findings were found when schools were provided latrine cleaning and handwashing supplies.¹⁶ Poor WASH conditions may be only one barrier to school attendance, and in many contexts, improvements to school facilities, or school WASH in particular, may not be sufficient to overcome other barriers like income generation or domestic tasks.

Although improvements in pupil diarrheal or respiratory outcomes may lead to reductions in pupil absence, few trials have revealed a consistent relationship between health and absence gains. In several cases, trials have found reductions in absenteeism or illness-related absenteeism without seeing commensurate decreases in health-related outcomes,¹⁰ whereas other studies have found improvements in health-related outcomes without commensurate decreases in absenteeism.^{9,15} The interaction between absenteeism and pupil health is complex; it may be that the provision of improved facilities themselves may improve pupil attendance, independent of detectable impacts on pupil-reported diarrheal or respiratory outcomes. For instance, WASH provisions might improve attendance by decreasing pupils' responsibility to fetch water,

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or by improving girls' ability to privately manage their menstrual periods. Similarly, diarrheal and respiratory-related health effects may not be significant drivers of absenteeism in all contexts. Although improvements to school WASH conditions may reduce overall fecal exposure of children, they may not be sufficient to reduce illness in some contexts if households WASH conditions are poor.

To date, few field trials have examined the impact of a school-based WASH program at scale that includes aspects of water, sanitation, and hygiene. We undertook a longitudinal, matched-control trial in schools throughout Mali to assess the impact of a comprehensive school-based WASH program implemented by five development partners. We quantified the impact of a comprehensive school-based WASH program on absence, diarrhea, and respiratory infection symptoms.

METHODS

Program background. We evaluated a school-based WASH program, known as the Dubai Cares Initiative in Mali (DCIM) for WASH in schools, implemented by CARE Mali, Oxfam GB, Save the Children US, UNICEF-Mali, and WaterAid Mali with financial support from the philanthropic foundation Dubai Cares. The program aimed to support the National Strategic Plan for Hygiene Education in Schools, adopted in 2011 by the Ministry of Education in collaboration with the ministries of WASH. According to statistics from the Ministry of Primary Education, Literacy, and National Languages, only 44.5% of primary schools in Mali have a water point and 58% have at least one latrine. The DCIM program delivered a comprehensive WASH package to 916 primary and secondary schools in six of Mali's nine regions (Bamako District, Koulikoro, Gao, Mopti, Timbuktu, and Sikasso) from 2011 to 2014. The DCIM partners identified public and community schools for inclusion in the program in collaboration with the Ministry of Primary Education. Priority was given to schools in areas where partners had ongoing activities and to schools with larger enrollment to maximize the coverage of the program.

Intervention activities included installing or rehabilitating water points and latrines; distributing WASH supplies including handwashing and drinking water containers, soap, anal cleansing kettles, trash bins, brooms, and disinfectant; and carrying out hygiene promotion activities in and around the schools, training teachers and school management committees, establishing and training school hygiene clubs, and establishing financial, WASH governance and material management systems at the school level. No further financial or technical assistance was provided to schools by the program or the government once the intervention activities were completed; schools were responsible for maintenance of infrastructure and reprovision of supplies. Program partners had standardized outcome-based implementation targets and monitoring and evaluation systems but developed partner-specific implementation strategies. Program outputs will be discussed in a forthcoming paper.

In March 2012, an armed conflict and coup prevented DCIM partners from continuing implementation of the program in Timbuktu, Gao, and parts of Mopti regions. The conflict displaced a number of people, and internally displaced persons (IDPs) moved in and out of study areas over the course of the study.¹⁷

Evaluation design. We used a matched-control, longitudinal design. Beneficiary schools ($N = 100$) were selected for the evaluation from DCIM program records and matched with 100 comparison schools with similar enrollment size and WASH characteristics within the same educational districts. Enumerators visited each school every 6–8 weeks between January 2013 and May 2014, excluding the summer break from June to September 2013, for a total of five to six visits per school. At each visit, enumerators conducted observations of WASH facilities and practices, interviewed the school director, conducted a roll call of all pupils, and conducted interviews with 40 pupils from grades 3 to 6.

Prior to official engagement with Emory University research partners, DCIM organizations collected baseline data on WASH infrastructure among all beneficiary schools in 2011. They also conducted a roll call and pupil interviews in 90 beneficiary and 90 comparison schools to generate pilot data for use in power calculations. These data were intended for use as baseline measures of impact indicators; however, largely due to the ongoing conflict, only 51 of the schools with baseline data met eligibility criteria for participation in the evaluation. As such, we were not able to include baseline data of our impact indicators as part of this analysis.

Sample size and power calculation. We used simulation-based estimates to assess power for our primary outcome of interest¹⁸ using a preintervention prevalence of 13% calculated from pilot data. Our evaluation was powered to detect a significant change of 20% in the odds of roll call absence using 80% power and $\alpha = 0.05$. We calculated this anticipated detectable difference given a sample size of 90 clusters per arm and an estimate of 250 pupils per school. After two rounds of data collection, we calculated the number of follow-up visits necessary using a pupil intracluster correlation (ICC) of 0.262 and a within school ICC of 0.065. Although four follow-up visits were deemed sufficient, given the seasonality of absence and potential delays in observed benefits, we planned for at least five follow-up rounds.

School selection. Beneficiary schools from the DCIM program were eligible for inclusion in the study if they were primary schools, had received or were scheduled to receive the complete DCIM intervention by June 2013, and were located in a secure region of the country. Stratified selection was used to ensure that the evaluation sample was representative of the overall program according to implementing partner, intervention region, school size, and whether implementation was finalized before or after the start of the study period in January 2013. Of the beneficiary schools that had participated in baseline data collection, 42 met eligibility criteria and were included in the study sample. Additional schools were randomly selected from within each stratum, with probability proportional to size sampling, using a random number generator in Microsoft Excel (Redmond, WA). All CARE-supported schools were in the Gao, Timbuktu, and Mopti regions, which were affected by the ongoing insecurity. Ten CARE-supported schools were included from the most secure part of Mopti region to retain partner participation without compromising study power in the event that those schools were lost to follow-up.

Comparison schools that had not participated in any substantial WASH programs in the past 3 years were identified using information from government records. Within each educational district where beneficiary schools were located,

potential comparison schools were stratified by size and WASH characteristics (presence of latrines and improved water points). Nine comparison schools that had participated in baseline data collection met eligibility criteria and were included in the study sample. Additional schools were selected within each stratum using a random number generator in Microsoft Excel so that the overall distribution of comparison schools by size and WASH characteristics matched the beneficiary schools in each district.

Participant selection. Within each school, a sample of 20 boys and 20 girls from grades 3 to 6 were selected at the initial visit for participation in individual surveys. Pupils were systematically selected from class registers using a skip pattern. We interviewed pupils in grades 3–6 based on the ability of children at this grade level to answer the survey questions during piloting. This cohort of pupils was followed throughout the evaluation period. If a pupil in the cohort left the school during the evaluation period, that pupil was replaced by another randomly selected pupil of the same sex in the same class. Pupils in the sixth grade who advanced to the next grade at the end of the 2012–2013 school year were replaced by pupils in the third grade at the start of the 2013–2014 school year. Roll call absence was assessed among all children in all grades.

Data collection and outcomes. Data were collected electronically on Android-enabled devices using Open Data Kit (ODK)¹⁹ by a team of experienced enumerators. Enumerators participated in a 1-week training that included data collection procedures, ethics, minimization of bias, and use of android devices and the ODK data collection software. Enumerators participated jointly in piloting exercises and were asked to independently record answers on their devices. Answers were then compared between enumerators to identify areas where responses differed for further clarification and training. Each enumerator collected data from all schools in one geographic area.

The DCIM partners established four school WASH standards related to water access, sanitation, handwashing, and hygiene kits. These standards were to serve as outcome-based implementation targets considering the minimum school conditions necessary to provide a safe and clean WASH environment for school children. The criteria included for each of these four standards are listed in Table 1. These data were collected using direct observation during unscheduled visits to the school by study enumerators. We asked school directors whether there had been exceptional changes in enrollment, although we did not specifically track whether any IDPs were enrolled.

Our primary outcome was pupil absence that was collected by roll call during unannounced school visits that took place on different days throughout the study. Enumerators used ledgers to collect gender- and grade-level pupil enrollment, pupil absences, and pupil dropout due to abandonment, transfer, or death, as verified by the teachers. School records were deemed unreliable based on piloting and were not used. Current enrollment was calculated by subtracting the number of pupils who had dropped out from the number of total enrollment at each data collection point. Roll call absence percentages were calculated as the number of absent pupils over the number of currently enrolled pupils. In final models, roll call absence was treated as a binary outcome. Absences that were followed by a designation as abandoned

TABLE 1
Water, sanitation, and hygiene standards used by the program

Standard	Criteria
Water access	Water point is improved (tap, borehole well, or covered well) Water point is located on the school grounds Water point is functional (as defined by the school director)
Sanitation	Latrines are located on the school grounds Latrines are improved (cement slab) Latrines are observed to be sex-separated in practice For every 70 pupils, there should be at least one latrine, that is, Safe (no cracks or vibrations in floors, walls, or roof) Clean (no visible signs of feces or pools of urine, the pit is not filled within 50 cm of the top, observed to be “clean” or “somewhat clean”)
Handwashing	At least one handwashing container (including water kettles) available to pupils Presence of water in at least one handwashing container Presence of soap near at least one handwashing container
Hygiene kits	Presence of at least one drinking water container with a tap Presence of at least one kettle for anal cleansing Presence of handwashing soap Presence of either bleach or detergent

were removed from analysis. Data collected on days where enumerators noted teacher absences, festivals, or other events that could impact attendance were excluded from analysis. Data were also excluded of any class where more than 50% of the pupils were absent, on the assumption that this indicated a special circumstance that was not noted.

Data on the secondary outcomes of self-reported absence, diarrhea, and respiratory infection symptoms in the past week²⁰ were collected through pupil interviews. All outcomes were binary. Absence was defined as missing a half day or more. Pupils who reported absence in the last week were asked for the cause or causes of the absence and absence was classified as due to diarrhea, respiratory infection, or other. Pupils who were absent on the day of the survey were included in the calculation of self-reported absence. Pupils were asked if they had had diarrhea using local terminology that referred to loose stool or the physical sensation associated with diarrhea and were also asked how many times they had defecated each day; a pupil was considered to have had diarrhea if they reported the local term and had also defecated three or more times in a day.²¹ Pupils were considered to have a respiratory infection symptom if they reported cough, runny nose, stuffy nose, or sore throat. Data for self-reported diarrhea and respiratory infection symptoms were recorded for pupils who were present on the day of the survey only.

Data analyses. Data were analyzed using Stata Statistical Software: Release 13 (StataCorp LP, College Station, TX). Descriptive statistics were calculated by aggregating individual-level data to the school level where necessary and using two-sample *t* tests to assess the differences in means between beneficiary and comparison schools. Unadjusted mean percentages for all outcome variables were calculated by aggregating individual-level data to the school level.

To quantify the impact of the program, we used intention-to-treat analyses utilizing mixed-effects logistic regression

models that compared beneficiary schools to the matched comparison schools, without regard to program adherence. We included beneficiary schools and their respective matched schools in the analysis once intervention implementation was completed. Models included several design variables, such as fixed effects for the intervention, the educational district matching cluster, and the time of year, and random effects to account for clustering of pupils within schools and repeated measures of pupils over time. We also assessed for effect modifiers and residual confounders for several variables determined a priori, including school size, rural or urban zone, pupil age, pupil sex, pupil grade, and reported latrine presence at home. Covariates were determined to be effect modifiers if there was a statistically significant interaction term ($P < 0.05$) between the covariate and intervention in the full model, or for multicategory indicator variables (i.e., grade, enrollment) if there was a statistically significant likelihood ratio test when assessing the group of categorical interaction terms simultaneously. Covariates were determined as potential confounders if we observed imbalances between potential covariates in the beneficiary and comparison groups. We used fully adjusted models if confounders or effect modifiers were observed. Sex stratified results were reported for the roll call absence outcome, regardless of statistical significance, due to precedence of effect modification in the literature.¹⁵

Since diarrhea and respiratory infection were hypothesized to cause pupils to be absent on the days of the surveys, we ran a sensitivity analysis probabilistically imputing these outcomes to pupils who were not present and therefore not in the dataset for that visit. We estimated and imputed the rate of diarrhea (or respiratory infection) among those absent by summing the average reported diarrhea (or respiratory infection) prevalence for all present pupils plus the percentage of absences in the past week reported as being due to diarrhea (or respiratory infection). We then estimated and reported the effect of the intervention with this absence-adjusted dataset.

Ethical approval. The study protocol was approved by Emory University's Institutional Review Board (Atlanta, GA), the Mali Ministry of Education, and the National Technical and Scientific Research Center (Center National de la Recherche Scientifique et Technique; Bamako, Mali). All three institutions approved consent in loco parentis (in the place of parents) signed by the school director and the school management committee (Comité de Gestion Scolaire). Pupils who were selected for the evaluation provided informed verbal assent. The evaluation was registered at ClinicalTrials.gov, identifier NCT01787058. Privacy of participants was ensured during data collection process. No payment or compensation was offered to respondents.

RESULTS

Demographics and WASH outcomes. Demographic information for the 100 beneficiary and 100 comparison schools at enrollment are presented in Table 2. There was general balance among covariates between the beneficiary and comparison schools, although beneficiary schools were slightly larger ($P = 0.06$) due to larger schools being prioritized for participation in the program within each educational district. Table 3 presents demographic information for the pupils who participated in individual surveys. A total of 4,907 pupils

were recruited among beneficiary schools and 4,823 pupils in the comparison schools. Mean age, mean grade, and household latrine coverage was similar between groups. Baseline data on attendance and health outcomes were not collected, as intervention activities had begun before the evaluation period. Because of the general balance of covariates between beneficiary and comparison groups, we did not include any of these covariates in our final models.

A total of 887 school visits including 772 roll call measures (393 beneficiary, 379 comparison) and 31,178 pupil interviews (15,681 beneficiary, 15,497 comparison; 47% girls) were conducted after the completion of program activities in beneficiary schools and at corresponding time points for the matched comparison schools (Figure 1). Roll calls included 4,420 class-level observations; of these, 101 were excluded due to events that could affect attendance and 55 were excluded due to greater than 50% of pupils being absent. The mean age of children was 11.0 years.

Overall, beneficiary schools met all 15 criteria related to the four DCIM WASH standards during 24.9% of observations; no comparison schools were observed to meet all standards (Figure 2). Beneficiary schools were most likely to meet the water access (81.8%) and hygiene kit (74.0%) standards, and least likely to meet the handwashing (57.9%) and sanitation (47.0%) standards. Beneficiary schools met 10 or more of the 15 WASH criteria at 87% of the visits. Beneficiary schools performed better across all standards compared with comparison schools. Over the course of the evaluation period, 66% of beneficiary schools and 54% of comparison schools reported exceptional increases or decreases in enrollment, in part due to movement of displaced persons from the ongoing conflict in the country.

Absence. During roll call by study enumerators, children in beneficiary schools were absent 8.0% of the time compared with 6.7% in comparison schools (odds ratio [OR]: 1.23, 95% confidence interval [CI]: 1.06, 1.42; Table 4). Crude absence results were similar for both boys and girls, and sex was not an effect modifier in the relationship between the intervention and roll call absence.

The 7-day self-reported absence rate was 17.6% in beneficiary schools and 18.0% in comparison schools (OR: 0.93, 95% CI: 0.79, 1.09; Table 4). Among pupils who reported absence in the past week, 24.1% of pupils in beneficiary schools reported the absence as due to diarrhea compared with 27.4% in comparison schools (OR: 0.73, 95% CI: 0.56, 0.94). There was no difference in reported absence due to respiratory infection symptoms (OR: 0.96, 95% CI: 0.67, 1.38). The odds of pupils in the interview sample being

TABLE 2
Descriptive statistics of sample population by study arm for schools ($N = 200$) at baseline*

	Beneficiary ($N = 100$)	Comparison ($N = 100$)	$P^\dagger$
Mean school population	340 (216)	285 (199)	0.06
Girls	47% (7)	47% (8)	0.75
Schools in rural zones	31% (47)	33% (47)	0.76
Schools with water point	51% (50)	49% (50)	0.78
Schools with latrines	86% (35)	87% (34)	0.84

Results indicate mean (standard deviation [SD]) or mean% (SD).

*Data for the beneficiary schools were collected during the baseline data collection conducted by the partners. Data for the comparison schools were collected during the first round of the study's data collection.

†Values are based on independent samples t tests.

TABLE 3

Descriptive statistics of sample population by study arm for pupils at time of enrollment ($N = 200$ schools)

	Beneficiary ($N = 4,907$)	Comparison ($N = 4,823$)	P^*
Age	11.0 (0.8)	10.9 (0.7)	0.22
Grade	4.3 (0.3)	4.2 (0.2)	0.07
Reported household latrine presence	85.1% (25.5)	82.5% (29.4)	0.50

Results indicate mean (standard deviation [SD]) or mean% (SD).

*Values are based on independent samples t tests on school-level averages.

absent on the day of the survey was higher in beneficiary schools than in comparison schools (OR: 1.26, 95% CI: 1.02, 1.55).

Diarrhea and respiratory infection. The odds of pupils reporting diarrhea in the last week were 29% lower in the beneficiary group than in the comparison group (OR: 0.71, 95% CI: 0.60, 0.85; Table 4). Results for 2-day recall were similar (OR: 0.62, 95% CI: 0.46, 0.82). School size was an effect modifier of the relationship between the intervention and 7-day recall ($P < 0.01$); in a stratified analysis, the effect of the intervention was primarily seen among schools in the bottom two quartiles of population size (Supplemental Table 1). When we ran an absence-adjusted sensitivity analysis probabilistically imputing diarrhea to pupils who were absent on the day of the survey, we continued to see an effect (OR: 0.78, 95% CI: 0.67, 0.91; data not shown).

The odds of pupils in the beneficiary group reporting having had at least one symptom of respiratory infection (cough,

runny nose, stuffy nose, or sore throat) were lower than in the comparison group (OR: 0.75, 95% CI: 0.56, 0.86; Table 4). The absence-adjusted model revealed a similar effect (OR: 0.76, 95% CI: 0.67, 0.87; data not shown).

DISCUSSION

We examined the health and educational impact of a comprehensive outcome-based school WASH intervention that was implemented throughout Mali, in an effort to add to the current evidence base on the role of school WASH in preventing absence and diarrheal and respiratory illness. Our impact evaluation, like other studies before it,^{9–11,14–16} showed mixed results. We found lower rates of self-reported diarrhea and respiratory infection symptoms among beneficiary schools compared with the comparison schools. We also found lower rates of self-reported absenteeism due to diarrhea. However, these positive impacts did not lead to a reduction in self-reported pupil absence (overall or sex segregated), and we found higher rates of roll call absence, the only outcome that was objective and not self-reported, among beneficiary schools compared with comparison schools.

We did not find a significant difference in the rates of self-reported absence between pupils attending beneficiary versus comparison schools, and we found that the odds of pupils in beneficiary schools being absent at roll call were 23% higher than in the matched comparison schools. This contrasts with the previous studies that found lower rates of

FIGURE 1. Flow diagram.

FIGURE 2. Percentage of Schools Meeting.

total absenteeism after the implementation of school WASH programs.^{10,15,22} The fact that we found no effect modification based on sex rules out differential impacts among girls, as found in Kenya.¹⁵ Although it is possible that the increased absence observed in the beneficiary schools was due to some aspect of the delivery of the intervention, it is more likely that this difference was due to suboptimal matching of comparison schools. It is possible that the beneficiary schools had higher rates of absence than comparison schools at the outset of the study. The loss to follow up of schools that participated in the baseline assessment largely due to the ongoing conflict meant that we did not have baseline absenteeism and health measures for the majority of schools in the study, and our matching criteria were insufficient to control for these unobserved confounders. Population movements stemming from the ongoing conflict may have contributed to this confounding effect. As in any resource-poor context, pupils had many reasons to be absent from school, and it is likely that improvements to school WASH access in the context of this project was insufficient to overcome key barriers to school attendance, such as the need to support the family with income generating activities or caring for younger siblings.

Another possible explanation for higher rates of absence among beneficiary schools is the potential for the program itself to have increased exposure to fecal pathogens among pupils. There is some evidence from the literature that improvements to WASH infrastructure and conditions may lead to increased exposure to fecal pathogens. Greene and others found that pupils in schools that received latrines had higher risk of having *Escherichia coli* on their hands,²³ and the authors hypothesized that this may have been due to increased use of latrines that was not accompanied by commensurate increases in handwashing behavior. Although the DCIM program improved WASH outcomes among beneficiary schools compared with comparison schools, beneficiary schools achieved all 15 WASH outcome criteria at only 25% of visits, and the ones that schools were least likely to meet involved providing handwashing stations with soap and ensuring that there were a sufficient number of clean, gender-separated latrines. Pupils did report lower rates of diarrhea and respiratory infections in beneficiary schools, and absence due to illness from diarrhea, indicating that this hypothesis may not be applicable in this setting; however, the potential for a negative relationship between school WASH improvements and absenteeism may be worth exploring.

TABLE 4

Logistic regression models comparing health and educational outcomes between DCIM WASH beneficiary schools and comparison schools ($N = 200$ schools)

	Beneficiary		Comparison		Regression*		
	%	SD	%	SD	OR	95% CI	P
Primary outcome	$N = 4,498$		$N = 4,444$				
Roll call absence, total†‡	8.0	5.5	6.7	5.6	1.23	1.06, 1.42	< 0.01
Roll call absence stratified by sex†							
Girl	8.0	6.0	6.6	5.4	1.24	1.07, 1.43	< 0.01
Boy	8.1	5.2	6.8	5.8	1.24	1.07, 1.44	< 0.01
Secondary outcomes	$N = 4,907$		$N = 4,823$				
7-day absence recall	17.6	8.8	18.0	8.9	0.93	0.79, 1.09	0.38
Absences reported as due to diarrhea	24.1	17.3	27.4	16.1	0.73	0.56, 0.94	0.02
Absences reported as due to respiratory infection symptoms	8.2	9.2	8.2	9.3	0.96	0.67, 1.38	0.83
Pupils in sample absent on day of survey	7.6	6.5	6.3	6.7	1.26	1.02, 1.55	0.03
7-day diarrhea recall	10.0	4.7	13.0	7.9	0.71	0.60, 0.85	< 0.01
2-day diarrhea recall	2.3	2.2	3.6	4.2	0.62	0.46, 0.82	< 0.01
7-day respiratory infection symptom recall	45.2	12.2	51.2	13.0	0.75	0.65, 0.86	< 0.01

CI = confidence interval; DCIM = Dubai Cares Initiative in Mali; OR = odds ratio; SD = standard deviation; WASH = water, sanitation, and hygiene.

*All models included variables for the intervention, the matched cluster, and the time of year, and accounted for clustering of pupils within schools and for repeated measures of pupils over time.

†Percent was calculated by taking the mean of absence aggregated at the school level.

‡The roll call absence model controlled for school size.

We did find evidence that a comprehensive school-based WASH intervention can have a positive impact on pupil health. The odds of pupils in the beneficiary group reporting having had diarrhea in the past week were 29% lower than in the comparison schools, and the odds of pupils in the beneficiary schools reporting having had symptoms of respiratory infections were 25% lower. Our findings are comparable with research showing that school WASH interventions are associated with lower rates of self-reported diarrhea and influenza.^{9,12} In addition, we found that among pupils who had been absent in the last week, the odds of reporting absence due to diarrhea was 27% lower in beneficiary schools than in comparison schools. These data are comparable with findings of reduced absence due to diarrhea and other illness after school WASH interventions.^{10,12} Other studies have also found that WASH interventions can reduce other infectious diseases such as soil-transmitted helminthes.¹¹ However, the observed reduction in absences due to diarrhea did not lead to a reduction of all-causes absenteeism and may not have been great enough to overcome any preexisting imbalances in absence rates between the beneficiary and comparison schools. These findings also raise a question over whether reductions in absence seen in other studies can be solely contributed to health gains. The health impact of this WASH in schools project is also likely due to the background rates of illness in the population and WASH conditions at home. In this context, improvements to school WASH may have been sufficient to improve health, whereas it may not be in other contexts.⁹

Strengths and limitations. One unique strength of this study was that it was an effectiveness evaluation of a school WASH program of over 900 schools led by multiple partners in a variety of settings throughout Mali. Though there was an agreed-upon set of outcomes related to WASH access targets, each partner's approach was slightly different and tailored to the local context. This is in contrast to smaller studies that assess a standard approach, perhaps in only a small number of districts. We were also able to use a prospective, quasi-experimental matched study design, controlling for several important matching variables—an improvement from the many cross-sectional and nonexperimental designs in the literature.

There were also several important limitations. The first was that we were not able to randomly allocate our interventions. Randomization is considered the gold standard of causal evidence and, on average, controls for both measurable and unobserved confounders. However, our use of a matched design was required due to the timing of our engagement in the study and the targeting approach for the program.

A second limitation was the use of self-reported data. Our diarrhea and respiratory infection outcomes relied on self-report and thus were subject to bias. We expect recall bias to be similar between beneficiary and comparison groups and bias our findings to the null, as pupils are more likely to recall more severe cases. However, courtesy bias could result in underreporting of illness in the beneficiary schools and bias away from the null. Controlling for courtesy bias is not possible, though we used shorter recall periods to try to reduce this bias. Our analysis of the 2-day recall data revealed similar findings to the 7-day recall.

Our third limitation was the civil unrest throughout Mali during the study period. The unrest led not only to loss of

target schools but also to considerable migration throughout the study period. The potential impact of a large-scale school WASH program on school attendance may have been limited in this context.

Finally, the impact of our effectiveness evaluation was driven by the fidelity of the intervention, which was inconsistent across the study sites and between schools. This evaluation should be viewed as an assessment of a particular intervention at scale, rather than of the potential for school WASH to have an impact in any context. An evaluation of a school WASH program with higher fidelity would likely result in different outcomes, and more evidence on how to improve hygienic behaviors is warranted. Several recent studies have begun to rigorously evaluate behavior change,^{24,25} and these lessons should be applied more broadly throughout the sector.

CONCLUSIONS

This evaluation of a comprehensive school WASH program provided mixed results. We found evidence that the program had a positive effect on pupils' self-reported prevalence of diarrhea and respiratory infection symptoms and pupils in beneficiary schools were less likely to report absence due to diarrhea than pupils in comparison schools. However, these positive impacts did not result in a subsequent effect on overall self-reported absence in the past week. We also found that pupils in beneficiary school were more likely than pupils in comparison schools to be absent according to roll call data. This increase may be due to underlying differences between the intervention groups or an effect of suboptimal achievement of improved WASH environments or behaviors.

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