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Nudging Handwashing among Primary School Students in the Philippines: Evidence from a Cluster Randomized Trial

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Abstract. Handwashing is key to preventing the transmission of various infectious diseases of which school-aged children are particularly susceptible. Traditional, information-messaging campaigns may increase handwashing awareness but have had limited success in promoting behavior change. Behavioral economics “nudges,” which explicitly target the knowledge-behavior gap, is a promising alternative. We evaluate the impact of school-based nudges in the first fully powered cluster randomized controlled trial in the Philippines. Out of our sample of 132 eligible schools, we randomly assigned half to receive nudges, including contextual cues (painted footpath from toilet to handwashing station) and visible reminders (posters and eye sticker), and half to the control group. Four months after implementation, we measured handwashing with soap (HWWS) after toilet use among grades 1–6 students using direct observation and compared this outcome between treatment and control schools. We also assessed whether nudges increased soap availability. The intervention increased HWWS rates by 17.3% points (pp), [95% CI: 4.2, 30.4] in treatment schools from the control group mean of 11.7%. The effect size was comparable across gender and age groups. Access to functioning handwashing facilities with soap increased by 36% (+20.2 pp, 95% CI: 10.9, 29.4). Mediation analysis suggests the program simultaneously nudged students to wash hands with soap in classrooms that already had soap, and nudged teachers to provide soap where it was not already available. These findings demonstrate that behavioral nudges costing less than $70 per school can lead to significant increases in HWWS among students 4 months post-intervention.

INTRODUCTION

Fostering hand hygiene among school-aged children is of critical public health importance. Handwashing has gained renewed global attention in the time of the COVID-19 pandemic as one of the most important methods for slowing its spread.[1] COVID-19 notwithstanding, handwashing with water and soap is key to preventing the transmission of various infectious diseases—including gastrointestinal and respiratory infections, of which school-aged children are particularly susceptible.[2] More than 4.9 million children each die of infectious causes, such as diarrhea and respiratory tract infections.[3]

Intensive interventions aimed to promote hand hygiene in schools has had limited success. In low- and middle-income countries, investments in water, sanitation, and hygiene (WASH) infrastructure are important to allow access to clean water and soap, but often do not lead to behavior change or decreases in disease prevalence.[4–6] Large-scale and often costly education campaigns containing information-rich messages to inform children, parents, and teachers of the benefits of handwashing may increase beneficiaries’ awareness[7] but gaps remain in translating knowledge to sustained behavior and measurable long-term impacts.[8–11] Teacher-led programs that aim to motivate handwashing by eliciting feelings of disgust or social pressure among students (i.e., behavioral motivators) have also had mixed success, as discussed in a recent paper evaluating the impact of a campaign using programming centered around disgust and affiliation to motivate handwashing in Philippines’ schools.[12,13] This study found limited effect sizes of statistical but not practical significance (i.e., increases in handwashing rates from 3% to 6%).

Insights from behavioral economics and neuroscience illustrate why knowledge and infrastructure alone are insufficient to incite behavior change. Behavioral economics finds attention is a limited resource,[14] and is especially constrained in low-resource settings where material scarcity make competing demands on cognition.[15] Individuals also exhibit “present-bias” when making health decisions, tending to value a future good (i.e., remaining disease-free) disproportionately less than the present cost or inconvenience of taking an action (i.e., washing one’s hands).[16,17] Neurobiological studies have further found children’s brains in particular to be underdeveloped in regions associated with attention- and future-oriented behaviors.[18,19] These psychological factors challenge the ability to form healthy habits, even when *knowledge* and *access* to infrastructure is present.

An alternative approach to promoting handwashing uses behavioral nudges, which explicitly addresses the knowledge–behavior gap. Intervening on the *choice architecture* around a behavior can make it more likely to occur in the moment the behavior is needed.[20] This approach, pioneered by Thaler and Sunstein, is characterized by design choices that alter the individual’s context and physical environment to encourage the desired behavior—rather than intervening on conscious decision-making processes.[21,22] These “contextual cues” also map onto Aunger et al.’s model for behavioral response, which highlights psychological processes needed for behavior change.[23] “Reactive” processes are automatic responses “to the presence of a specific cue, such as an object, person, message, or time of day” that serve to reinforce the development of habits over time.

In recent years, several studies have tested the behavioral nudges for promoting handwashing with promising results. In Bangladesh, Dreibelbis et al. implemented contextual cues in the form of bright footpaths with footprints leading from outdoor toilets to a constructed handwashing station in two schools, and found handwashing rates increased from

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18% at baseline (after handwashing infrastructure was built but before nudges were installed) to 74% 6 weeks later.[24] In a follow-up study, they compared these cues to high-frequency lesson plans, and found that the cues were as equally effective as the high-frequency lesson plans in increasing handwashing rates; in this study, nudges alone increased handwashing rates from 24% at baseline to 58%.[25] A similar study using contextual cues in the United States (in the form of red arrows leading from toilet stalls to the sink) increased handwashing by 6 pp, from 43.5% to 49.5%.[26] A different approach was used in Naluonde et al. (2019) a bar of antibacterial soap threaded with a piece of rope functioned as a hall pass and was given to students going to school pit-latrines by a teacher (during class) or student monitor (during breaks); in a randomized trial, authors found handwashing with soap (HWWS) increased by 8%.[27]

We build on this nascent literature by evaluating the impact of a behavioral nudges intervention on student handwashing after toilet use in public elementary schools in the Philippines. In October 2019, we installed a series of handwashing nudges, drawing from previously successful studies and our own formative research, in randomly assigned primary schools in Zamboanga del Norte province. These were contextual cues (including a brightly painted path with footprints) and visual reminders (including an eye sticker near the sink and poster in toilet stall). Four months later, we collected direct-observation data on handwashing and evaluated the interventions' impact. We designed our study to mirror conditions for scale-up in the Philippines, meaning while selected study schools had minimum WASH infrastructure, we did not provide soap and water. This setup allowed us to assess a novel secondary outcome: whether nudges impact teachers' behaviors in ensuring students' access to water and soap.

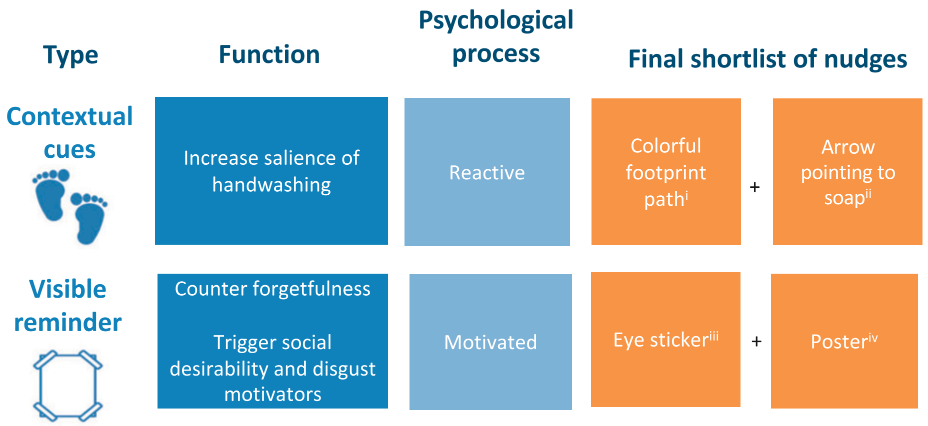
MATERIALS AND METHODS

Study setting and intervention design. This study takes place in elementary schools in Zamboanga del Norte, a province situated in the region of Mindanao in the Philippines. In 2018, 51.6% of the population lived below the poverty line,[28] the highest proportion among regions in the Philippines. Zamboanga del Norte was identified as a priority province for support under UNICEF's Country Program of Cooperation with the Government of the Philippines (2019–2023).

The conceptual framework for the handwashing nudges intervention is based on the psychological determinants of handwashing discussed in Aunger et al.[23] and combines this model with insights from behavioral economics' choice-architecture approach. Our framework also fits into the broader Integrated Behavioral Model for Water, Sanitation and Hygiene.[29] We divide the set of nudges into two types:

1. **Contextual cues** (painted footpath, arrow sticker): The painted footpath and arrow sticker pointing to the soap dish are intended to trigger reactive processes, which are responses "triggered automatically by a particular kind of stimuli"[23] that help to form habits. After making scoping visits to schools and finding that handwashing stations were almost always multipurpose, we added the arrow sticker as an inexpensive way to draw attention to the soap and therefore handwashing in the space.
2. **Visual reminders** (posters, eyes sticker): The posters were not intended to impart new information, but rather serve a reminder function directly counteracting forgetfulness and present bias. They were designed to be highly pictorial and aimed to trigger motivators like disgust (the desire to perform behavior to feel clean) and social affiliation (the desire to behave in similarly to one's peers). Previous studies have found similar visual reminders were effective at increasing handwashing rates in India,[30] England,[31] and the United States.[32,33]

The intervention's conceptual framework, as well as each nudge's theorized pathway to behavioral change, is summarized in Figure 1. In the figure, we also cite the original source of each nudge idea. We selected our nudges by referencing the empirical literature, brainstorming as a team, and conducting scoping visits to Zamboanga del Norte schools. Once we had designed the nudge materials, we piloted them in two schools in Zamboanga del Norte. Our piloting process



i. Blackwell, Goya- Tochetto, and Sturman (2018); Grover et al. (2018)
ii. Brainstorming post-scoping visit
iii. King et al. (2016); Biran et al. (2014)
iv. Biran et al. (2014); Botta et al. (2013); Judah et al. (2009)

Figure 1. Behavioral change framework. This figure appears in color at www.ajtmh.org.

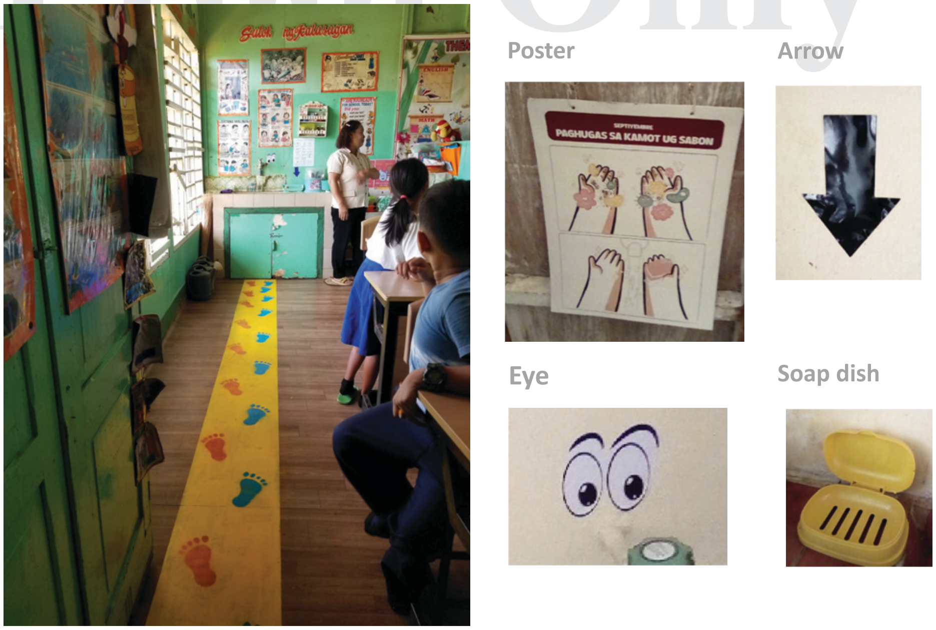


Figure 2. Painted footpath leading from toilet to handwashing station in classroom and other nudges. This figure appears in color at www.ajtmh.org.

consisted of test installations and individual teacher and student interviews (we shared the nudge concepts and poster images with respondents). Based on pilot feedback, we refined the design and installation protocols.

Research design. We conducted a cluster randomized controlled trial (RCT). Across Zamboanga del Norte, 132 Department of Education (DepEd) schools were randomly selected to comprise the study sample based on power calculations. This experiment was powered to detect at least 7% points or greater in handwashing rates between treatment and control schools, where handwashing rate is defined as the percentage of toilet-use instances that led to a student washing their hands with soap and water. Provided the true effect of the program is indeed as small as 7% points, and assuming an intraclass correlation of 0.1, a sample size of 100 schools with 20 handwashing opportunities per school would allow the experiment to be well-powered, or sensitive enough, to detect a difference of this magnitude or greater with an 80% chance of statistical significance and 0.05 significance. To account for likely attrition because of conflict and safety risks in the region (i.e., not being able to observe a school because the area is deemed unsafe), we added a buffer of 32 schools to total 132.

To be eligible for the intervention and study, schools needed to meet the following minimum inclusion criteria:

(i) Water for handwashing was available at the school for at least certain days of the week
(ii) The overall pupil to toilet ratio equaled 100 or lower
(iii) School has at least one individual or group handwashing station
(iv) School does not have any planned WASH programming in the upcoming 2019–2020 school year beyond standard WASH in Schools (WinS) activities

To these infrastructure criteria, we added an additional criterion:

(v) School must be deemed safe by DepEd; that is, there must not be an active presence of armed rebel groups in the nearby community, and the school must be accessible (e.g., no blocked roads because of floods or other weather events).

Items (i) through (iv) were determined based on 2019 data derived from DepEd's WinS Online Monitoring System (OMS), a database updated yearly with detailed school-level WASH indicators. Based on the above, 210 out of 634 (33%) schools were eligible for random selection. We assigned 66 schools to the treatment group to received nudges, and 66 schools to the control group. All schools in ZdN have been oriented on DepEd's national WinS policy and program guidelines. The national WinS programming guidance promotes "correct hygiene and sanitation practices among school children and a clean environment in and around schools" and includes initiatives related to infrastructure, knowledge, and behavior change. There are currently no specific national programs with an explicit focus on handwashing promotion. Classrooms in treatment schools were eligible for nudges if they had: 1) a functional toilet, 2) functional handwashing station (including stored water), 3) a clear, unobstructed path from the toilet to the handwashing station, meaning no large, hard-to-move items such as a bookcase, would break up the footpath. Based on these criteria, we installed nudges in 66.8% of sections, with an average of five sections per school. In total, we installed nudges in 239 classrooms across 58 treatment schools. We did not install nudges in eight treatment schools, because of eligibility criteria (no sections eligible for nudges) or safety restrictions. Among control schools, 65.9% of classrooms were eligible. The CONSORT flow diagram depicting enrollment, allocation, follow-up, and analysis numbers can be found in Supplemental Appendix Section A.

Randomization. We assigned schools to treatment or control group based on a stratified randomization procedure. Strata were determined by the "WinS implementation quality index score" (WinS IQ) and whether a school was considered a "nudge-enabler." Each school was assigned a WinS IQ score using school-level OMS administrative data. These criteria capture whether schools have infrastructure conducive to our outcomes of interest and include having at least one gender-segregated toilet, and government school funding for soap. Based on how many criteria schools met on a series of WASH indicators, schools received WinS IQ scores ranging from 0 to 9. We also determined whether a school was a "nudge enabler" or "non-nudge enabler," based on our hypothesis on which additional features would be conducive to the nudges intervention. Specifically, these features were having no Information, Education, and Communications (IEC) campaign materials on hygiene in toilets or handwashing areas; and having lights in toilets. First, we randomly sampled 100 "nudge enabler" schools. We randomly assigned 50 schools to treatment group and 50 schools to control group, stratifying by three categories of WinS quality index scores (low 2–5, medium 6–7, and high 8–9). An additional 32 schools were randomly drawn from the 96 non-nudge enabler schools remaining in the eligible population. Again, half were randomly assigned to treatment group, and half were randomly assigned to control group within each WinS index strata. We do not use this nudge-enabler variable in our analysis. At the time, it was part of an effort to determine which variables can approximate schools that are more conducive to nudges. However, with more context about WinS policy, we realized indicators chosen may not approximate this well. For example, we presumed having no IEC posters would make the poster reminders more surprising, but IEC posters may actually correlate with other WinS programs like additional WASH infrastructure support, which would be conducive to nudges. We do not include this "nudge-enabler" distinction in our analysis, but control for it as part of strata fixed effects in our regression. Sampling weights also adjust for oversampling of "nudge enabler" schools, such that the final results are generalizable to all Zamboanga del Norte schools that meet the school eligibility criteria. Figure 3 shows the flow of randomization.

Data collection. Nudges were installed in treatment school classrooms in October 2019. Four months later, in February 2020, we returned to all study schools to collect handwashing and facility access outcomes. This study does not include a baseline "preintervention" round of data collection for the following reasons: 1) it would have increased the likelihood of observer bias; schools would become aware that we are measuring handwashing and treated schools may likely adjust their behavior during the follow-up because of this knowledge, introducing bias to our estimate of impact; 2) as described earlier, we had rich administrative data at the school level with key indicators correlated to handwashing that we used to check sample balance and increase the precision of estimates by including as covariates.

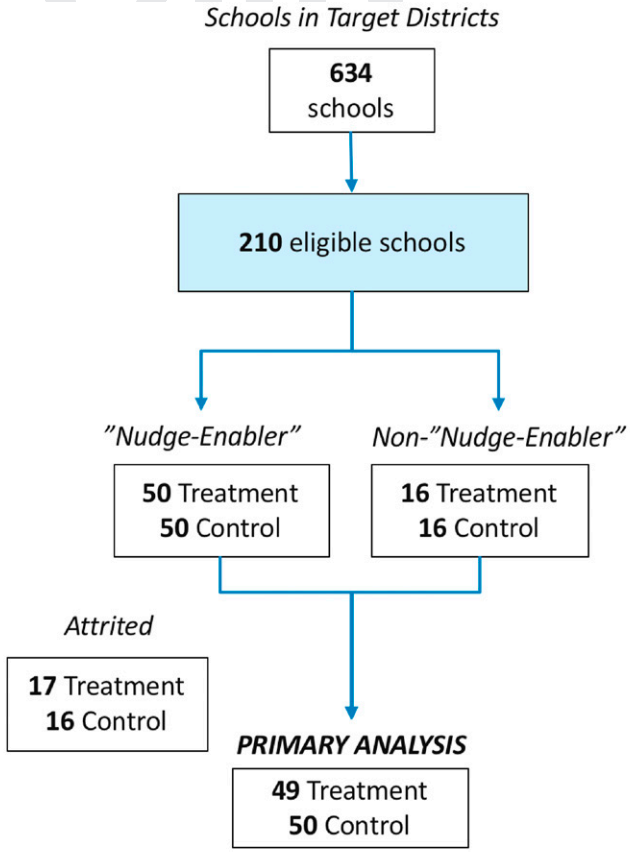


FIGURE 3. Randomization flow chart. This figure appears in color at www.ajtmh.org.

Our primary outcome of interest was student HWWS after toilet use, as observed during classroom observations. Our secondary outcome of interest was students' access to functioning handwashing facilities near toilets, and functioning handwashing facilities with soap near toilets, as observed during structured observations of handwashing facilities. Lastly, we conducted semistructured qualitative interviews in treatment schools to assess perceptions of the nudges among teachers, principals, and students. Facilities observations and qualitative interviews also allowed us to assess implementation quality and whether nudges lasted over the course of the 4-month observation period. Twenty-one local enumerators were recruited and trained on our data collection instruments to conduct data collection activities, summarized in Table 1.

Handwashing after toilet use. To be eligible for handwashing after toilet use observations, sections in both control and treatment schools had to be available, eligible for nudge installations, and observable. We assessed eligibility during endline data collection. Note, this approximates eligibility at baseline, as we did not conduct baseline data collection. This assumes that control school classrooms that are eligible during observation are comparable to those that were eligible 4 months before during installation, and treatment classrooms that were eligible during installation did not become ineligible during observation. We find these assumptions were upheld: only 4% of treatment classrooms became ineligible at endline. Likewise, based on a random sample of 32 Control schools, only two classrooms had changed from ineligible to eligible (0.82%). For a section to be *available*, enumerators had to be able to visit the section when students were present. A section could be unavailable if it was locked, no students are present, the teacher refused to allow the enumerator to stay. Eligibility for nudge installations

TABLE 1
Data collection activities

Activity	Approach	Sample size
Handwashing after toilet use observations	Observed for at least 2 hours in one section of each randomly sampled grade level (1–6) in 99 schools	**99 schools** **(49 T, 50 C)** 298 classrooms (138 T, 160 C) 1,158 HW opportunities (495 T, 663 C)
Handwashing facilities observations	Observed all pupil handwashing facilities (section and group) near toilets	**114 schools** **(54 T, 56 C)** 545 classrooms (266 T, 279 C) 44 group handwashing stations (24 T, 20 C)
Key informant interviews	Conducted semistructured qualitative interviews in T schools	**6 T schools** 36 interviews 4 principals 16 teachers 16 students

followed the same criteria the installation team used to determine which treatment classroom would receive nudges. To be *observable*, enumerators had to be able to sit in a discrete location that gave them clear view of the toilet stall door and handwashing area without disrupting classroom activities. One classroom within each grade level was randomly selected from grades 1 to 6. If the selected section was not eligible for observations, we moved on and observed in the next randomly selected section. If all sections within a grade were not eligible, we observed an additional section in the next grade. To mitigate observer effects, we took the following measures: 1) conducted facility observations after classroom observations, 2) randomly assigned enumerators to control and treatment schools on different days, 3) enumerators explained to teachers that they were there to observe "normal classroom activities" and made no mention of handwashing or sanitation. Handwashing was observed anonymously; potential repeated measures of the same student was not captured.

Handwashing facilities. To be eligible for handwashing after toilet use observations, sections and group handwashing stations had to only be *available* to facilities observations. A section had to be unlocked, so that enumerators could enter and observe the toilet and handwashing area. A group handwashing station had to be approachable and at least one toilet stall unlocked, so that enumerators could approach near it and make observations. Facility observations were conducted after handwashing observations, to mitigate observer effects. The discrepancy between the sample size for handwashing observations and handwashing facilities observations is due to different eligibility criteria for handwashing observation and facilities observations and safety and security concerns (rebel groups are active in areas of Zamboanga del Norte).

Key informant interviews. This assumes that control school classrooms that are eligible now are comparable to those that were eligible 4 months ago during installation, and treatment classrooms did not switch eligibility from eligible to ineligible. We selected two large, two medium, and two small treatment schools, based on school size, from different districts in Zamboanga del Norte for the interviews. At each school, we interviewed the principal, three teachers, and three pupils. On the initial visit during data collection, enumerators obtained permission from the principal to pass out consent forms to pupils, and to return another day. On the second visit, enumerators conducted interviews with all stakeholders. We randomly chose one teacher in grades 1–2, one teacher in grades 3–4, and one teacher in grades 4–5. For pupil interviews, we randomly selected three sections, one each from grades 4 to 6. Pupils from grades 1 to 3 were not interviewed because they would likely have been too young to fully understand the questions. In each chosen section, we randomly selected one pupil out of all those who returned consent forms.

Analytical model. All findings represent intent-to-treat effects, as installation data showed compliance was high (i.e., nudges were installed in at least 90% of eligible classrooms across nudges). Our Pre-Analysis Plan was registered publicly in Clinical Trials. Analyses and data visualizations were conducted in Stata/IC 15.1. The effect of the treatment on the outcome variables for is estimated by the following weighted least squares Linear Probability Model:

$$Y_{ij} = \beta_0 + \beta_1 T_j + \vec{\beta_2}\,\vec{X_{ij}} + \vec{\beta_3}\,\vec{S_{ij}} + \varepsilon_{ij} \tag{1}$$

where

Y_{ij} denotes the outcome variable for pupil i in school j, classified as a binary variable.T_j denotes the treatment variable (binary variable for whether school j received the nudge intervention)

$\vec{X_{ij}}$ represents a vector of pupil, school, and class level covariates: school district dummies, grade dummies, pupil gender, number of pupils in the classroom, school population

$\vec{S_j}$ represents strata fixed effects

ε_{ij} denotes the pupil error term i, clustered at the school level to reflect the fact that the treatment assignment was at the school level

β_n denotes the coefficients determined by the regression model (β_1 is coefficient of interest).

For the main analysis on student handwashing, Y_{ij} was classified as a binary variable equals to 1 if a pupil washed their hands with soap and water (or water only) after exiting the toilet and equal to 0 if they did not. For the secondary analysis on access to handwashing facilities, the same regression specification was used. Y_{ij} equals 1 if toilet i in school j had a functioning handwashing facility nearby, or a functioning facility with soap nearby, and equals to 0 if not. In the context of experimental evaluations, linear probability models provide unbiased estimates of program impact and correctly estimate standard errors.[34] For facility outcomes, class and school level covariates were included but not pupil covariates. Standard errors were clustered at the school level for all specifications. Sample weights equal to the inverse of the probability of a school being selected out of the "nudge-enabler" schools in the eligible school population was applied. For handwashing outcomes, this was multiplied by the inverse probability of a classroom being selected for observation to account for the uneven sampling probability of classrooms in a grade. One deviation from our PAP is that we omitted "nudge conduciveness" index as a control variable. This was done because: 1) It was measured at endline and altered by the intervention, so including biases the main estimate of treatment impact, 2) it is collinear with strata fixed effects. These considerations are further detailed in the Appendix Section B. While Equation (1) is our prespecified model, for the primary outcomes of handwashing we also estimated a multi-level random intercepts model, which accounts for the nested structure of handwashing observations within classrooms within schools. This is described in Supplementary Appendix Section C.

Heterogeneity in the treatment effect was assessed for different groups in the sample. Subgroup analysis was done on pupil gender, pupil grade level (grades 1–3 in comparison to grades 4–6), and WinS Index Scores (as a measure of level of WASH infrastructure, divided into "High" [score of 8–9) versus "Medium to Low" [7 or lower]). For subgroup analysis, a single regression on the full sample with an interaction term between the treatment term and the subsample was used.

Ethical approval. Before observing classrooms, we received verbal consent from the school principal and teacher. For student qualitative interviews, we obtained signed consent from parents and verbal consent from respondents. The stated purpose of our research communicated during the consent process was to observe and learn about student health and sanitation behavior. Our evaluation was granted ethical clearance from the St. Cabrini Medical Center—Asian Eye Institute research ethics committee, which is accredited by the Philippine Health Research Ethics Board, as well as research clearance from the DepEd Zamboanga del Norte.

RESULTS

Final sample. Our baseline sample of 66 schools per treatment arm was well balanced along key observable school-level characteristics. The P value from a joint test of orthogonality of all the balance check variables is 0.85. Over the 4-month study period, 33 schools attrited: 17 (13%) were not visited at either nudge installation or endline data collection because of safety and security concerns, 14 (11%) had no classrooms eligible for the intervention, and two (2%) were dropped after repeated unsuccessful visits. After attrition, our final sample remains well balanced along key characteristics, with a P value on the join test of orthogonality of 0.94 (Table 2).

Impact of behavioral nudges on handwashing after toilet use. The nudge intervention increased the rate of student handwashing after toilet use. The observed rate of handwashing with soap and water after toilet use among treatment students was 17.3% points (pp) (95% CI: 4.2, 30.4) ($P = 0.010$) higher than control school students, for whom the average rate of handwashing was 11.7%. There was also a smaller difference in water-only handwashing (increase of 8.2 pp), leading to a combined difference in at least water handwashing of 25.6 pp [10.8, 40.4] ($P = 0.001$). Additional analysis which models classroom-level random effects in a multilevel model is presented in Appendix Table A2. The results from this specification (a 10 pp increase in handwashing rates) is likely to be less reliable due to small cluster sizes in our dataset (the average number of observations per classroom in our dataset is 3.8, and 70% of classrooms have four observations or fewer) and because multilevel models have been found to overestimate within-cluster correlations and produce less reliable estimates of random intercepts when cluster sizes are less than five.[35–37]

The intervention had no differential handwashing impact on grade groups or genders. Pupils in grades 1–3 and pupils in grades 4–6 responded similarly to the nudges: effect sizes were 17.8 pp and 16.2 pp, respectively (P value of difference = 0.847). Boys and girls also responded similarly: effect sizes were 17.1 pp and 17.5 pp, respectively, P value of difference = 0.926). Among "High" WinS Index schools, nudges increased HWWS by 21.1 pp [2.7, 39.5], and this result was statistically significant (P value = 0.025); among

TABLE 2

Balance check of final sample along key school-level characteristics

Characteristic	Control (N = 50) %	Treatment (N = 49) %	P value
Regular availability of soap	78	80	0.85
MOOE funds for soap	66	69	0.72
Local government unit funds are available for soap	10	8	0.75
No IEC posters in toilets	56	61	0.6
No IEC poster about handwashing in toilets	56	67	0.25
Toilets generally have lights	88	78	0.17
At least one group handwashing facility present	62	67	0.58
At least one classroom handwashing facility present	60	63	0.74
Low WinS Index Score (5 and below)	12	12	0.97
Medium WinS Index Score (6–7)	48	49	0.92
High WinS Index Score (8–9)	40	39	0.9

IEC = Information Education Campaign; MOOE = Maintenance and Other Operating Expenses; WinS = WASH in Schools. The last column shows the P-value from the F statistic of the mean comparison between groups. The P-value from a joint test of orthogonality of all the balance check variables is 0.94.

TABLE 3

Impact of the nudges intervention on handwashing rates after toilet use

	Handwashing after toilet use	
	(1) With water and soap	(2) With at least water
Treatment	0.173**	0.256***
	(0.066)	(0.074)
Female student	0.029	0.011
	(0.025)	(0.022)
Number of students in the classroom	0.005	0.000
	(0.004)	(0.005)
School population (in hundreds)	−0.001**	−0.001**
	(0.000)	(0.000)
Grade dummies	Yes	Yes
District dummies	Yes	Yes
Strata dummies	Yes	Yes
Random effects variance		
School level		
Classroom level		
Residual		
Adj. *R*-squared	0.428	0.396
Observations	1,158	1,158
Control mean	0.100	0.167

Columns (1) and (2) represents regressions of student's handwashing outcome after toilet use on treatment assignment and a vector of covariates and fixed effects, where the unit of observation is at the level of the handwashing observation. Standard errors are clustered at the school level, presented in parenthesis (*** $P < 0.01$; ** $P < 0.05$; * $P < 0.1$). Control mean is the adjusted handwashing rate across control group schools. Sample weights are applied to adjust for unequal sampling probabilites of sections in a grade and oversampling of schools with "nudge enabling" characteristics.

Low-to-Medium WinS Index schools, nudges increased HWWS by 10.7 pp [−2.7, 24.3], but this was not statistically significant (*P* value = 0.12). However, as a result of low sample size in each category, the estimates are not precise; their 95% CIs are quite wide. The 10 pp difference in treatment effect is not statistically significant (*P* = 0.373).

Impact of behavioral nudges on access to handwashing facilities. Access to a functional handwashing facility (a facility with water access) near a toilet was relatively high, but access to soap was significantly lower. In control schools, 84.9% of toilets had a functional handwashing station nearby, but only 55.9% of toilets in control schools had access to a functional handwashing facility with soap (Table 4).

Nudges increased both access to water and to soap at handwashing stations near toilets (Table 4). Compared with control schools, treatment schools were 7.2 pp (0.0, 14.5) (*P* = 0.1) more likely to have a facility near a toilet with water, and 20.2 pp [10.9, 29.4] (*P* = 0.000) more likely to have a facility near a toilet with water *and* soap. In Zamboanga del Norte classrooms, there were two types of water setups at handwashing stations. The first was running water students turned on with a faucet. The second was stored water that students scooped out of a bucket. Subgroup analysis showed that the higher access of functional facilities in treatment schools, as compared with control schools, was driven by greater access to facilities with *stored water.* Treatment schools were 12.6pp (*P* = 0.072) more likely to have access to functional facilities with stored water than control schools; by contrast, there was a slight decline in the proportion of facilities with running water compared with control, but the difference was not statistically significant.

Mediating effect of soap availability. The intervention was designed to impact handwashing rates through student behavioral change. Handwashing nudges increased handwashing rates. However, they also increased access to soap and water availability, which could have also positively affected handwashing rates. To understand whether the increased handwashing rates was driven by students' behavioral change, or by increased opportunity to wash hands with soap—because of increased availability of functional handwashing facilities with soap near toilets—we conducted mediation analysis by running the regression specified in Equation (1) but adding a control variable for soap availability. Treatment effect size reduced slightly, from 17.3 (*P* = 0.010**) to 16.4 (*P* = 0.013**), but remained positive and statistically significant (Appendix Table A1).

Implementation findings. There was a 4-month gap between nudge installation and endline data collection. Despite this significant amount of time, over 98% of classrooms where nudges were installed still had at least one nudge present at endline. More than 75% of each individual nudge (footpath, posters, eye sticker, arrow sticker) installed remained at the end of the 4 months. Additionally, among the

TABLE 4

Impact of the nudges intervention on access to handwashing facilities near toilet

	Access to handwashing facilities near toilet			
	(1) Functioning facility	(2) Functioning stored water facility	(3) Functioning faucet water facility	(4) Functioning facility with soap
Treatment	0.072*	0.126*	−0.039	0.202***
	(0.036)	(0.069)	(0.084)	(0.047)
School population (in hundreds)	0.000	0.000	0.000	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Fixed effects				
Grade dummies	Yes	Yes	Yes	Yes
District dummies	Yes	Yes	Yes	Yes
Strata dummies	Yes	Yes	Yes	Yes
Adj. *R*-squared	0.215	0.314	0.336	0.314
Observations	589	589	589	589
Control mean	0.849	0.431	0.398	0.559

Columns (1) to (4) represents regressions of facility access (presence of functioning handwashing stations near a toilet) on treatment assignment and school-level covariates and fixed effects, where the unit of observation is at the level of the toilet facility. The coefficient for the treatment effect is shown in row "Treatment." Standard errors are clustered at the school level, presented in parenthesis. Control mean is the unadjusted outcome across control group schools. Sample weights are applied to adjust for unequal sampling probabilities of sections in a grade and oversampling of schools with "nudge enabling" characteristics.

*** $P < 0.01$; ** $P < 0.05$; * $P < 0.1$.

remaining nudges, the vast majority remained in good condition, as judged by enumerators according to a provided rubric. The only nudge with significant variation in its condition was the footpath. In 11% of installed classrooms, the footpath was in "bad condition" at endline, and 27% were in "fair condition." The footprints' condition deteriorated because of flaking paint, according to interviewed teachers.

According to interviews, both pupils and teachers had limited interaction with the nudges. According to teachers, some pupils walked on the footpath (both to the handwashing station after toilet use as intended, and for fun at other times), and flipped the posters. Teachers also interacted with the nudges, but primarily to maintain them. They changed the poster displayed monthly; swept up flaked paint from the footpath; and touched the stickers. Broadly, most interviewed principals, teachers, and students were enthusiastic about the nudges. Students mostly liked the nudges because they were colorful and attractive in the classroom. For example, one student said:

> *"I like [the footprints], because it looks good."*

Principals and teachers liked the nudges primarily because they believed the nudges benefited their school and students. As one principal explained:

> *"As a principal for many years, I am happy if there are new things in my school, especially if they are lasting, useable, and useful [like the nudges]."*

DISCUSSION

The results of the evaluation found that the handwashing nudges intervention led to substantial, statistically significant increases in several outcomes of interest. Compared with control schools, in treatment schools, HWWS rates at least doubled. As all outcomes were measured 4 months after implementation, these findings suggest the persistence of the nudges' impact.

The nudges also increased access to soap in functional facilities near toilets by 36%. Teachers are the ones primarily responsible for maintaining facilities and soap availability on a day-to-day basis. Results from mediation analysis suggests the program simultaneously nudged students to wash hands with soap in classrooms that already had soap, and nudged teachers to provide soap where it was not already available. Findings from our secondary analysis that soap and stored water access increased suggest that nudges had not only affected student behavior–as has been the focus of previous papers testing school-based nudges–but also simultaneously changed the behavior of teacher, acting as reminders for them to replenish water and soap supply. These results suggest that nudges can be effective in promoting a more regular supply of soap in settings where there are no *financial* barriers to obtaining soap (i.e., soap is budgeted for as part of school funds) but there may be a *behavioral* barrier in that day-to-day availability relies on teachers or school administrators remembering to replenish the supply.

Our process evaluation findings show that in general, all key stakeholders—principals, teachers, and students—enjoyed and supported the nudges. However, several school staff emphasized that their schools required more consistent water access to improve student hygiene practices; reinforcing that pupils' behavior is not the only barrier to handwashing. Staff also commonly did not know or incorrectly stated the purpose of the eye and arrow stickers.

In designing this study, we drew from previous work pioneering the use of nudges in schools. In comparison to similar studies, our effect size of 17.3 pp on the handwashing with water and soap rate is moderate. It is higher than the results of a study using arrows pointing to handwashing stations in the United States in adult bathrooms,[26] but lower than the impact estimates of several studies testing a variety of nudges in other resource-poor school settings.[24,25] A notable feature of the research design—collecting one round of handwashing data at endline rather than two rounds at baseline and endline as is often standard in randomized trials, was also appropriate in the context of this study. We did not collect baseline handwashing data for two reasons: first, doing so would have increased the likelihood of observer bias; that is, schools would become aware that we are measuring handwashing and treated schools may likely adjust their behavior during the follow-up because of this knowledge, introducing bias to our estimate of impact. Second, we had rich administrative data at the school level with key indicators related to handwashing that we used to both stratify randomization and check sample balance as well as increase the precision of treatment impact estimates by including as covariates. The main threat to internal validity of results is attrition because of safety concerns from armed conflict in the region. We argue that results remain internally robust and attrition was unlikely to cause selection bias in the final sample. The cause of attrition was arguably because of factors unrelated to the intervention or the outcome, we had adjusted initial sample size calculations to account for anticipated attrition, and our final sample was well balanced along key school-level characteristics associated with handwashing.

Our study provides additional support to a growing number of studies that suggest that nudges can be effective in a variety of low middle-income country (LMIC) contexts, and can be implemented at low-cost (our cost estimates totaled about $70 per school). Within the Philippines, we are recommending that the nudges be scaled up as part of the DepEd's overall WinS program, for schools and classrooms that meet minimum eligibility requirements, including water and handwashing station availability. We recommend installation of the nudges tested as a "package" and adapting the nudges only minimally to meet new contexts; for example, by translating the posters into the local language.

Our design and evaluation experience in Zamboanga del Norte suggest for nudges to be successful in a new context, it is important that the nudges are context-appropriate, installed in a setting with enabling conditions (including water availability and at least the possibility of soap availability), and put in place with a plan for their sustained maintenance. Based on our findings on implementation fidelity, we recommend the nudges are repaired and maintained every 6 months.

In both the Philippines and other LMICs, we are recommending considering contextually adapted nudges as part of a package for preparing schools for reopening after COVID-19 quarantines are lifted. Because the nudges require enabling conditions, the nudges may be suitable for some schools or sections, but not others. In our study, the installation success rate in classrooms was 66.8%.

There are several limitations to our study: first, we measured handwashing behavior, not health outcomes. We cannot make claims about the health benefits of HWWS among the target population. Second, we only measured handwashing in schools, so we cannot be sure whether nudges supported habit formation that will carry over to other contexts. Third, we collected fewer handwashing observations than expected; our classroom eligibility and observability requirements reduced the number of available classrooms to observe across schools. Although this did not impact our main analysis, the sample size was too small to assess whether nudges truly had a larger impact in schools with better infrastructure. Fourth, since we relied on classroom observations of handwashing, data collectors were not able to be blinded to the intervention status, which may lead to observer bias. We aimed to mitigate this through a week-long intensive training process and during data collection through conducting high-frequency daily data quality checks to flag data collectors reporting unusually high rates of handwashing; we further alternated enumerator assignments every day to treatment and control schools. Beyond these measures, we are not able to conclusively eliminate this form of bias. Last, we did not examine nudges individually, so we do not know whether the effect size was driven by one or more of the nudges in particular.

The contextual cues tested were based on the behavioral economics approach of using choice architecture to encourage behavior. This approach aligns with the 2010 model of Aunger et al. for behavioral response. The nudges were designed to instill “reactive” or automatic processes for school children to wash hands after they used the toilet. Our finding that the key outcome of handwashing *behavior* improved 4 months after implementation suggests that the nudges were successful in achieving this: the visual cues prompted the behavior of handwashing that lasted over time. However, our evaluation focused on the outcome of handwashing and we did not explicitly measure psychological processes by proxying for reactive internal processes as a process output (e.g., using psychometric scales as in Aunger et al., 2010); this would be interesting area for future work.

There are some potential areas for additional research. Although these handwashing nudges have been replicated in several contexts, it may still be useful to replicate them in other geographies. New replication studies could add to the literature in several ways:

First, if structured as a multi-armed RCT and/or with a large enough sample size, new studies could explore which nudges primarily drive the positive impact on handwashing rates and whether nudges have a differential effect on schools with different levels of infrastructure. Exploring further the effect of nudges on teacher behavior–rather than students–is another potential avenue for future work, both to assess whether our findings hold in other settings and to explore other ways of nudging teachers and principals.

Second, they may help generate additional nudge ideas. We know from our design process that some nudges effective in previous studies were not appropriate for this one, and imagine the opposite may be true in new contexts. Some of these nudges might address more specific barriers to HWWS, or explore how to encourage soap use for pupils who wash only with water when soap is available.

Third, future studies may wish to measure persistence effects of handwashing nudges. To do so, these studies may measure handwashing multiple times by utilizing alternative data collection approaches such as real-time sensor data or liquid soap volume (though these are more expensive than real-time handwashing observations).

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