NEGATIVE BEHAVIORAL TRANSMISSION

RESHMAAN HUSSAM* DAYEA OH‡

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Abstract

Behavior change programs often assume positive transmission of behavior across contexts and therefore evaluate effects only at the site of intervening. We randomize an edutainment program in Bangladeshi schools to trace school-to-home transmission of handwashing and find that children are induced to wash more at school but less at home. This negative transmission impacts non-school days and other household members, yielding a net negative effect of the program. We replicate the conceptual experiment by randomizing the proportion of students receiving handwashing resources at home and tracking home-to-school transmission. Children induced to wash more at home likewise wash less at school.

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*Harvard Business School, Boston MA 02163. rhussam@hbs.edu

†Harvard Kennedy School, Cambridge MA 02139. ohd@g.harvard.edu

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I. INTRODUCTION

Does behavior change transcend context? Education curricula in childhood rely, to a great extent, upon the assumption of positive behavioral transmission. From norms such as respect for authority and friendly play to practices of daily living such as proper hygiene, diet, and exercise, the lessons imparted in educational institutions are crafted not only for the classroom but to shape how individuals operate at home and in the world at large (Campbell et al. 2002; Hahn et al. 2016; Blackman 2002). Conversely, behaviors cultivated at home are often expected to extend to the school or workplace, shaping the culture of these contexts in turn.

Yet evidence of behavioral transmission across contexts is scarce. Should this transmission be positive, educational institutions may be powerful sites for behavior change. Should it be limited or nonexistent, the objectives of education curricula may require reconsideration. Should it instead be negative, with behavioral improvements in one context *reducing* such behavior in another, then programs deemed successful at the site of evaluation may in fact be ineffective or even harm beneficiaries and the communities in which they act.

We design a field experiment around the health practice of handwashing with soap to causally estimate the nature of behavioral transmission between the school and the home. This paper builds off of two recent studies – Hussam, Rabbani, Reggiani, and Rigol (2022a), which examines the habit-forming nature of handwashing – and Hussam, Pandey, Shonchoy and Yamauchi (2022b), which examines how edutainment can alter behavior – to ask a broader question: what happens to behavior when agents move from the site of intervening to an untouched context? Does behavior transmit across these spaces, or does progress in one generate regression in another?

Set in rural Bangladesh, we first experimentally induce behavior change in each context. At school, we vary handwashing rates by randomizing the administration of a hygiene edutainment program across classrooms (referred to as the 'edutainment treatment'). At home, we induce improvements in behavior by distributing handsoap dispensers to a randomized subset of children ('dispenser children'), randomly varying the proportion of students per classroom who have a dispenser installed in their home (referred to as the 'dispenser saturation treatment').

We track handwashing rates for five months in each context to estimate both directions of behavioral transmission. We capture school-to-home behavioral transmission by comparing handwashing performance at home between those who were and were not exposed to the edutainment campaign at school, and we capture home-to-school behavioral transmission by comparing handwashing performance at school between those classrooms with more or fewer children in possession of handsoap dispensers at home. Our measures of handwashing performance are objective and high-frequency as we embed, in both settings, time-stamped sensors within dispensers to track handwashing rates.

We find that the edutainment campaign, despite its low-touch nature of thirty minutes of content weekly and near-zero marginal cost,[1] meaningfully improves behavior in the classroom: students exposed to the campaign wash their hands in school 36% more often ($p < 0.001$) than their unexposed counterparts. Handwashing levels among treated classrooms are sustained over the course of the five month study, while those in control classrooms steadily decline: by the fifth month, students in edutainment-exposed classrooms are washing 106% more ($p < 0.001$) than their control counterparts.

Despite the large positive treatment effect of the edutainment campaign at school however, we find that the campaign has a *negative* impact on behavior at home. The magnitude of the negative impact at home is substantial: the edutainment campaign raises daily school handwashing by an average of 0.27 ($p < 0.001$) washes per student, but reduces daily home handwashing by 0.36 ($p < 0.001$) washes per household, implying a net negative effect of the classroom edutainment program on a treated child and her family. In other words, the public health campaign appears highly effective when evaluated at the site of the intervention by the standards of the literature (Lewis et al. 2018; Bowen et al. 2007; Rosen et al. 2006; Dreibelbis et al. 2014), but in reality reduces total daily handwashing rates for treated students and their families.

The dispenser saturation treatment replicates this conceptual experiment of behavioral transmission in the reverse direction by exogenously shocking home handwashing behavior via dispenser provision and observing performance at school. However, we cannot causally estimate the 'first stage' of this experiment, or the impact of dispensers on home handwashing, because all households in our study sample receive dispensers: given evidence that dispensers significantly reduce child diarrhea and respiratory illness and increase child height and weight in a previous field experiment (Hussam et al., 2022a), we determined it was unethical to deprive any household participating in our study of a dispenser.[2] Reassuringly, data from the present study are consistent with a large 'first-stage': households exhibit 55% higher handwashing rates after dispenser receipt than their self-reported handwashing rates at baseline.

Does this improvement in household hygiene behavior transmit into the classroom? We find that classrooms with a high saturation of students who possess dispensers at home exhibit 12% *lower* ($p < 0.001$) handwashing rates than their low saturation classroom counterparts. Negative behavioral transmission is thus replicated in this direction: classrooms in which more children are enabled to wash hands at home exhibit lower washing rates at school.

The pattern of negative transmission in the dual directions of the school to the home and the home to the school suggests that the effects we observe are not unique to a particular environment

1. We combine edutainment pieces produced by the government of Bangladesh, UNICEF, and other NGOs, resulting in a small cost to curriculum design and near-zero marginal cost to scale.

2. Note that the broader literature on the provision of handwashing supplies or facilities on household behavior and health is far from conclusive: most studies find little, if any, impact of such resource provision on behavior or health. However, none of these studies employ a dispenser attached to the household and equipped with foaming soap, two features that Hussam et al. (2022a) found desirable after extensive piloting, and which resulted in substantial gains in behavior and child health. We therefore rely on evidence about this specific handwashing resource intervention, implemented in a demographically comparable, for the present study.

(school or home) nor a specific intervention (edutainment or dispenser provision), but rather tied to the nature of the handwashing behavior itself. We consider two bodies of behavioral mechanisms through which negative transmission may transpire. First, a set of behaviors we term 'crowd-out mechanisms': among them, constrained effort and moral licensing. In the act of moral licensing, individuals who have already engaged in effortful positive behaviors are more likely to engage in negative ones (or less likely to engage in positive ones) because their good behavior licenses them to become morally lax (Blanken, Ven N., and Zeelenberg 2015; Merritt, Effron, and Monin 2010). Crowd-out due to constrained effort (Luk-Zilberman 2021) posits that individuals possess a limited capacity for effortful behavior regardless of context. Handwashing more in one setting mechanically reduces the quantity washed elsewhere over a given period of time.

We cannot tease apart these sub-mechanisms, nor are they an exhaustive list of behavioral motives behind crowd-out - two features common to this literature due to the complementary and often subconscious nature of such motives. However, this body of motives shares a testable temporal prediction: only once positive action has been undertaken is subsequent action less likely. Our time-stamped data allow us to test this prediction by examining what hours in a day the negative transmission effect arises among edutainment-treated children within the home. Indeed, the bulk of the negative impact on home handwashing transpires *after* children have been exposed to their edutainment-treated classrooms, rather than before they go to school. This is despite the fact that nearly one third of daily handwashing in the household takes place prior to school [among control households], leaving ample opportunity for edutainment-treated students to reduce handwashing within the home during this time, should they wish.

Crowd-out mechanisms deliver a further prediction on the magnitude of behavior change: the degree of crowding out should be commensurate with the degree of prior engagement. In order to test this prediction, we exploit the interruption of the experiment by the end-of-year holiday season as a plausibly exogenous shock to handwashing behavior, as all participants face the holidays regardless of handwashing preferences or performance. Households exhibit a sharp decay in handwashing rates as soon as the holidays begin: while household handwashing rates are more than twice their baseline performance prior to the holidays, they fall to near baseline levels during and after this period. In parallel, the negative behavioral transmission of dispenser children into the classroom is substantial prior to the holidays, but effectively disappears following the holidays. With the rapid decay in household dispenser use and consequently little change in handwashing within the home after holiday season, we observe little crowd-out of the behavior among dispenser children at school.

While we find evidence of crowding-out behavior both temporally and in relative magnitudes, this mechanism alone cannot explain the *net negative* treatment effect we document: at its worst, negative transmission via crowd-out will result in an intervention with zero total impact. A further examination of household behavior on schooldays reveals that the reduction in children's home-washing behavior spills over to other members of the family: even during the hours of the day when the child is at school, the households of edutainment-treated children (adults and non-school-going children) continue to exhibit significantly less washing than their control counterparts, with 0.09 ($p < 0.05$) fewer washes during this time period, or 25% of the daily negative treatment effect. Household members appear to (consciously or otherwise) mimic the behavior of the child, such that the consequences of negative behavioral transmission via crowd-out spreads beyond the treated child alone.

The net negative treatment effect of the edutainment program is further explained by our second proposed mechanism, cue-based habit formation. This behavioral pattern involves repeated actions that become tied to contextual triggers (Laibson 2001; Wood and Neal 2007; Orbell and Verplanken 2010; Wellsjo 2021). A behavior change intervention may amplify the association of a behavior with one context at the expense of others: what before was equally likely in any setting may now be triggered at the site of the intervention and disassociated from alternative environments. A key prediction of this mechanism, as distinct from that of crowd-out, is that the negative treatment effect for edutainment-treated children in the home should manifest on days when children do not have school: even absent direct crowd-out, cue-based habit formation predicts that treated children will come to associate the home setting with reduced handwashing. We test this by examining household handwashing behavior on weekends and holidays. We find that households of edutainment-treated children engage in 0.48 fewer washes ($p < 0.001$) on non-school days relative to their untreated counterparts.

These patterns suggest the following story: children are motivated to wash by the edutainment campaign at school, which raises school-level washing and attaches the school cue to washing. A crowding-out of handwashing, accompanied by the absence of the school cue, then leads to lower washing upon returning home - a behavior that spills over to other members of the family. As the school week proceeds, such repeated daily behavior becomes habitual, reducing handwashing at home over the weekend and subsequent holidays, resulting in a net negative treatment effect of the school edutainment program.

As a mainstay of education curricula, we designed our experiment with the expectation of documenting positive behavioral transmission, and so find our results surprising. We consider two alternative stories that may challenge the behavioral interpretation and generalizability of our results: one that centers the experimental setting rather than the features of the behavior of handwashing, and another that considers compensatory handwashing choices made by the parents of treated students.

Because the edutainment campaign was administered only in school, perhaps edutainment-treated children understood handwashing to be a school-specific behavior, consciously increasing washing at school and reducing washing at home relative to those who received no edutainment. In efforts to correct potential misunderstanding, we supplement the edutainment campaign three months into the experiment with an additional video explicitly underscoring the importance of washing in both locations of the school and the home. An examination of parental hygiene knowledge at endline suggests that this message was effectively conveyed: parents of treated children are significantly more likely to express that the school *and* the home are the two most important locations to handwash. However, though edutainment treated students continue to exhibit substantially higher handwashing rates at school, a difference in differences analysis suggests that this video has no impact at home: treated children continue to wash significantly less than their control counterparts at home. In other words, the negative transmission does not appear to be driven by a conscious misunderstanding about where handwashing may be more and less necessary.

The concern of a context-specific intervention is further allayed by our second experiment. We document negative behavioral transmission not only from school to home, but also from home to school, using a distinct intervention that is not vulnerable to the problem of context-specificity: children were treated at home with handsoap dispensers, a product they had access to in both the home and school contexts. They therefore had no reason to interpret the dispenser as a 'home-specific' technology, yet they exhibit negative behavioral transmission in handwashing nonetheless.

A second alternative narrative involves an intrahousehold substitution away from dispenser soap: perhaps the parents of edutainment-treated students, more attuned to the value of the dispenser soap for their children, choose to substitute away from dispenser soap for their own use, instead using the barsoap that 94% of households already own. However, all households were informed that dispenser soap provision would be unlimited for the duration of the experiment. Empirically, no household ever faced an empty dispenser: soap was topped up every month. The design therefore eliminated the need to be concerned of soap scarcity or a need to save soap for children. Second, when asked in a followup survey to reflect on the various reasons why one may have chosen not to use the dispenser soap, and when offered possible reasons, among which were: "I wished to save the soap for other household members," "I felt it was the child's dispenser, not mine," or "I used hard soap instead," parents of edutainment-treated children do not report these reasons more often than their control counterparts (despite experimenter demand effects alone favoring these answers). We interpret these results as indicating that parents made no conscious decision to substitute away from soap, or alter their engagement with soap, in the households of edutainment treated students; the effects we document appear to be driven by the children themselves.

We highlight two central contributions of this study. Despite serving as the premise for much of early childhood education, literature documenting the transfer of behavior across contexts is limited. Hiramatsu et al. (2014) examines how an energy saving school program affects students' families' energy saving behavior at home, but relies on self-reported survey measures and a pre-post analysis. Tidwell et al. (2020) considers the impact of a school handwashing promotion campaign on handwashing behavior at home, but likewise relies on self-reported measures and a non-randomized comparison arm. Both studies document positive levels of behavioral transmission from the school to the home. We add to this literature by designing a field experiment that implements two distinct interventions to exogenously alter behavior in each context, allowing us to causally estimate bidirectional transmission in behavior and thereby speak to the nature of the observed *behavior* rather than the features of a particular environment or intervention. We augment this design with a critical measurement tool: we are able to precisely and objectively track behavior across contexts by employing handsoap dispensers with time-stamped sensors embedded inside, designed in partnership with the MIT Media Lab (Hussam et al., 2022a). This measurement innovation lies in contrast to self-reports of behavior change or structured observations of hand hygiene, the two measures most frequently employed in existing literature despite their vulnerability to surveyor demand effects (Ram et al. 2010). Our results underscore the importance of this technology: we show that, were we to employ self-reported measures of handwashing (which we also collect), we would – like Hiramatsu et al. (2014) and Tidwell et al. (2020) – document positive behavioral transmission across contexts, and in turn endorse a school-level edutainment campaign for scale that has negative consequences for treated children and their families.

Second, we build upon a complementary body of literature examining the phenomenon of behavioral spillovers. This literature finds evidence of negative spillovers *across* behaviors, whereby increased performance of one behavior reduces performance of a related behavior (irrespective of context). For a recent summary of the empirical work in this space, see Luk-Zilberman (2021), who underscores diversion of attention and depletion of effort or attention as primary mechanisms behind the crowding-out of one behavior vis-à-vis another. This paper, in turn, examines how the *same* behavior may be characterized by negative spillovers over time and space, a phenomenon we term 'behavioral transmission.' We connect the heretofore distinct concept of crowd-out, as explored in the behavioral spillover literature, to cue-based habit formation, a characterization of repeated behaviors across varying sites or environments of action. This connection yields a sobering prediction: not only might crowd-out neutralize the positive impact of an intervention, but cue-based habit formation may further exacerbate this negative transmission. If 'bad' behavior becomes tied to contexts in which more time is spent (such as the home), the impact of a single-site intervention across all sites of action may be negative on net. Our work underscores the need to expand the analytic scope of behavior change interventions to the range of contexts that recipients are expected or likely to operate in, rather than solely the site of intervening.

The methodological contribution of this study is therefore two-pronged. To accurately estimate the impact of behavior change programs, our findings suggest that (1) the impact of the program in all complementary contexts must be measured, and (2) measurement in all contexts must avoid self-reports. Without incorporating both, evaluations may deem a program effective when, in actuality, the intervention reduces total engagement in the desired behavior. In the expansive literature on behavior change interventions, most evaluations do not incorporate either measurement approach. None, to our knowledge, incorporate both.

Are our results unique to the behavior of handwashing? While we cannot speak to other behaviors directly, the underlying mechanisms we propose have each been documented across a range of actions in the literature: behavioral spillovers have been found between meal-logging and meditation (Luk-Zilberman 2021) and water conservation and electricity consumption (Tiefenbeck et al. 2013), among others, and cue-based habit formation has been found in exercising, reading, television (Wood, Tam, and Witt 2005) and drug and food consumption (O'Brien 1976; Fedoroff, Polivy, and Herman 1976). We therefore view the *combination* of these two mechanisms as plausible across a host of common behaviors that require effort and are performed repeatedly across space and time: among them, study habits, healthy food consumption, sustainable environmental practices, and mask-wearing. The range of contexts in which one might expect negative transmission to transpire may also be expansive: consider for example the workplace, in which character or community-building efforts intended for the world at large (such as diversity, equity, and inclusion training) are integrated with the effort or attention of oft-reluctant participants, or religious institutions, which aim to cultivate taxing behaviors to be practiced beyond the houses of worship themselves. We hope this study can offer a framework for estimating the transmission patterns of behaviors and contexts such as these in future work.

II. EXPERIMENTAL DESIGN

II.A. *Study sample and context*

Our experiment was conducted in the Gaibandha District of Bangladesh. We engaged 156 classrooms across 26 public primary schools, paired with 775 rural households (each with at least one child in a sample classroom) across 52 villages. Appendix Table 1 presents sample means for a host of classroom, mother, and child characteristics. Primary schools in our sample have six grade levels, from pre-primary to grade five, with an average class size of 36. The average teacher is a female with thirteen years of formal education and fourteen years of teaching experience, while our average respondent (female head of household) is a 35 year old mother with four years of education.

Hygiene knowledge and practice in our sample population are poor: only 22% of households recognize that a cold can spread between people, and 17% (24%) report using soap before cooking (eating). This lack of soap use is not due to scarcity of the raw materials required for proper handwashing: 94% of households have soap, in the form of barsoap, in the home, and 99% have easy access to a shallow tubewell with potable water.

II.B. *Dispenser and soap features*

We employ a wall-mounted soap dispenser embedded with a time-stamped sensor, designed in partnership with the MIT Media Lab, to measure handwashing behavior within all sample households and classrooms. In households, the dispenser was installed in the verandah, where families typically eat, or near the shallow tubewell, from which families draw their water; in classrooms, it was installed by the door. Because each classroom was not equidistant to a water source, we supplemented the dispenser with adjacent water stations consisting of large water buckets with spouts. Classroom teachers were instructed to permit only their own students to use the dispenser, and we provided teachers with a separate dispenser in the teacher's lounge for their private use.

The sensor in the dispenser is accessible via a key that was not supplied to schools or households, preventing participants from tampering with the sensor inside. Liquid soap was provided throughout the course of the experiment, refilled directly into the dispenser every two weeks for classrooms and monthly for households.

The sensor technology addresses a series of challenges facing public health literature in hygiene and sanitation, which typically employ self-reported outcomes or participant observation to measure hand hygiene (Ram et al. 2010): demand effects and desirability bias, as behavior is self- or enumerator-reported, lack of statistical power, as behavior is infrequently measured, and lack of time-specificity, precluding observation of time trends in behavior change. A consistent and objective method of measurement is particularly important in our dual-context setting, as different contexts are likely to exhibit different levels of demand effects and error in measurement under participant observation or self-reported outcomes.

II.C. *Treatment groups and randomization*

Figure I provides a map of all treatment arms. We first describe the nature of the treatments, and then describe the randomization process.

The edutainment treatment involved a series of children's edutainment programs on hand hygiene, the entirety of which took 10 to 15 minutes to air and was done twice per week in treated classrooms for the duration of the experiment. The videos were shown by the enumerator with the assistance of the teacher during regular class hours, and the windows and door of the classroom were closed during each viewing to ensure no control classroom students had access to the edutainment. The content was sourced from publicly available material (for example, see links to the following: Meena Cartoon, Bangladesh campaign, and Sesame Street), encompassing simple messages describing how, when, and why to wash one's hands. Importantly, the key message of these programs was that one must wash their hands *with soap* in order to effectively remove germs. To maintain interest, we rotated the precise content each session, recycling back every six weeks. Compliance measures reflect high participation: both edutainment days were executed in 92% of the classroom-weeks; one edutainment day in 3% of classroom-weeks, and zero edutainment days in 5%.

The dispenser saturation treatment was administered by selecting a random 25% subsample of students from a randomized set of classrooms (amounting to an average of nine students per treated class) to receive a handsoap dispenser at home. In the remaining comparison classrooms, we randomly selected two students to receive a dispenser at home.[3] To avoid potential confounding effects of greater prestige or popularity that emerge in the classroom due to selection into the home

3. Two students, rather than a percentage of students, was chosen in comparison classrooms because classroom sizes varied significantly. Two students, rather than one, were selected in order to avoid undue attention being focused on a single student per comparison classrooom.

dispenser lottery, children were not notified that their names had been drawn at school; instead, enumerators privately drew names from a classroom roster, secured the household locations of selected students from the teacher, and visited the households independently to recruit and survey the mother and install the dispenser. As such, peers in classrooms could only be affected by a student's household dispenser if the student independently communicated the information to the peer or altered her behavior due to her home environment (a product of behavioral transmission).

Randomization along the two margins of treatment was implemented as follows: for both the edutainment treatment and the saturation treatment, our sample was stratified by school and treatments were randomized at the classroom level. The six classes in each of the 26 primary schools were randomized into three edutainment treatment classes (T) and three control classes (C). Our final analysis sample includes 150 unique classes of which 76 are edutainment treated and 74 control.[45] The saturation treatment was likewise randomized at the classroom level, with 40% of classes randomly allocated to the dispenser saturation treatment and the remainder to the comparison arm.[6] Students in both high and low saturation classes were selected randomly from the class roster conditional on having attended at least 60% of school days in the previous month, a criteria used to ensure that selected students would indeed be engaged in both environments of school and home.

II.D. *Identification of effects*

The experimental design is motivated by the dual objectives of estimating the transmission of handwashing behavior from the school to home and the home to school. In order to do either, the

4. As some schools did not have enough classrooms to accommodate the entire student body at the same time, some classes either worked in two time shifts or arranged the classroom so that two classes would sit in the same classroom simultaneously. For cases where two classes shared the same classrooms in different time periods, we match our time-stamped dispenser presses to the appropriate classes. For cases where two classes' students shared the same classroom simultaneously, it would be impossible to distinguish the presses of students from the two classes; this constraint meant that, if two classes that shared a classroom were initially assigned to different edutainment treatment and control groups for example, we switched the control class to an edutainment treatment class and consider the two classes as a single treated unit. Out of 156 classes, 5 pairs of classes shared the classroom simultaneously and 4 pairs were initially randomly assigned to different treatment groups. In order to maximize our sample size while avoiding problems of imbalance on 'simultaneous shared classroom space,' we do the following: we preserve the one pair of classrooms that were both randomized into the control group, and we randomly preserve one pair from the four mixed classrooms (of which all were functionally treated). We drop the remaining three mixed pairs of classrooms (or six classes) from all analyses. This yields a sample of 150 unique classes of which 76 are edutainment treated and 74 are control. Additionally, because we preserved the treatment status of a classroom as it entered a new school year, an additional pair of classes of mixed treatment status began sharing a classroom simultaneously and are therefore dropped from analysis during the second school year (the last 2.5 months of data). Lastly, we have one pair of classes who are randomized into different saturation treatment statuses and share their classroom simultaneously; we therefore drop only this pair from all saturation-related analyses.

5. Note that the actual number of classes that appear in our data differ by day depending on the number of sensors that were functioning in a given round of data collection.

6. While a balanced randomization would have been optimal for purposes of power, we were constrained by the number of handsoap sensors available to us. We also note that the saturation treatment was not stratified by the edutainment status of a classroom, resulting in a (randomly) higher proportion of saturated classrooms in edutainment-treated classrooms. We control for the saturation (edutainment) treatment of a classroom/child in all edutainment (saturation) analyses, but do not estimate the interaction of the two treatments as per Muralidharan, Wüthrich, and Romero (2020). For more detail on this analysis choice, please refer to footnote 11.

design must first generate exogenous variation in handwashing behavior within each context. This is accomplished at the school level by randomizing the administration of the edutainment campaign across classrooms. At the home level, the exogenous variation is generated by randomizing the distribution of soap dispensers. Paired with a saturation design at the school level and tracking of behavior in both contexts, the design delivers the following causally identified treatment effects:

1. **School-level edutainment effect:** The effect of the hygiene edutainment campaign is identified by comparing the handwashing rates of edutainment treatment classes to those of edutainment control classes. We view this treatment effect as a combination of all channels exogenously moved by the edutainment - individual, peer, and teacher behavior may all change in response to the edutainment and raise students' handwashing rates within the classroom. Given that the purpose of this exercise is to exogenously alter a child's behavior at school, rather than to determine what makes the edutainment campaign effective *per se* (a question explored in Hussam et al. (2022b)), we do not seek to disentangle these channels here.

2. **School-to-home transmission effect:** The behavioral transmission from school to home is identified by comparing the handwashing rate of households with children from edutainment treated classes relative to those of children from control classes. Note that handsoap dispensers cannot feasibly be given to a single child in the household. As such, the estimated household level treatment effect represents the sum of a child's altered behavior in the home and any behavioral spillovers to other household members.

3. **Home-to-school transmission effect:** The behavioral transmission from home to school is identified by comparing the handwashing rates of high-saturation classrooms to those of low-saturation classrooms. As with the household level data, dispenser data captured at the classroom level will represent the combined effect of behavior brought to school by students with a dispenser at home as well as any peer effects generated within the classroom by these students. While the experiment cannot disentangle the two, home-to-school transmission is a prerequisite for peer effects to transpire.

We underscore that the experiment is not designed to capture the direct effect of the handsoap dispenser on household washing (the analagous household-level treatment effect to the direct effect of the edutainment program on school washing). This implies that, if no home-to-school transmission effect manifested (via the saturation treatment at the classroom level), we would not know whether to attribute this to the absence of a direct effect (no impact of dispensers on handwashing rates at the home) or the absence of behavioral transmission from the home into the school. We take this risk intentionally. Two recent studies have documented substantial effects of handsoap dispenser provision at the household level on child handwashing behavior and health. Hussam et al. (2022a) randomly distributed dispensers identical to those used in this study to sociodemographically similar households in West Bengal and documented a 48% decline in loose stool incidence, a 29% decline in ARI symptom incidence, and a 0.2 SD increase in child height-for-age. Hussam et al. (2022b) engaged a non-overlapping but geographically and demographically identical population to that of this study (two years prior) and documented 68% lower loose stool incidence and 52% lower ARI symptom incidence among children in households who received a dispenser. Given the preponderance of evidence of the positive benefits of dispenser provision on child handwashing behavior and health, we determined it unethical to deprive participants in this study of the dispenser. All households and classrooms in our survey sample were therefore provided with handsoap dispensers and liquid soap, precluding the presence of a control group against which to measure the direct household-level dispenser treatment effect. We instead rely on the existing literature, a pre-post dispenser-provision analysis, and the existence of a classroom-level saturation effect to confirm this direct impact, since the former can only transpire if dispenser children are affected by the presence of a dispenser at home.

III. METHODS

III.A. *Outcomes*

Our primary outcome measures are as follows:

1) **Daily handwashing rates** are measured by and collected from the time stamped sensor embedded in each handsoap dispenser. At each press of the dispenser, the sensor records the time of day, to the second, when the button is pressed. The identity of the user is unknown, but we proxy for 'distinct uses' by collapsing presses that occur two or fewer seconds apart into a single press. Daily handwashing rates are then calculated as the sum of all distinct uses over the course of each twenty-four hour period.

   A. School handwashing behavior: Handsoap dispenser data for each class is collected from the field biweekly. For classes who occupy the same classroom in different shifts (eg. 8am-12pm and 12pm-4pm), we match the time-stamped dispenser presses to the class shifts to distinguish between classes.

   B. Household handwashing behavior: Handsoap dispenser data for each household is collected from the field monthly.

2) **Child health** data is collected in the form of self-reported monthly incidence of child diarrhea and respiratory illness. Mothers are asked how many days each child had lose stool motion and symptoms of respiratory illness (cough, runny nose) in the previous two weeks.

III.B. *Data collection and timeline*

Data: Data was collected by local enumerators recruited by our partner survey organization in Gaibandha, Bangladesh. Baseline surveys were conducted in person both at the school and at the household level, but endline surveys were conducted only at the household level via phone due to the closure of schools and field work restrictions resulting from the COVID-19 pandemic.[7] The video intervention at schools was administered twice weekly following the week of the baseline survey, and concluded just prior to COVID school closures. The enumerators in charge of visiting the households to collect sensor data were blind to school-level edutainment and saturation treatments of the sample children in the households.

Timeline: A timeline of the experiment and relevant school events is depicted in Appendix Figure I. The first phase of the experiment was conducted from October to December 2019. The school-level baseline survey was administered in September of 2019 and the household-level baseline survey in October of 2019.[8] Baseline survey administration was accompanied in both schools and homes by the installation of handsoap dispensers. Bangladesh public primary schools are closed on Fridays and national religious holidays. Grade 5 students left school by mid-November in order to prepare for their national advancement exam; all other students took final exams in the first two weeks of December, after which they had winter break for the remainder of the year. We therefore exclude all weekends and holidays from school-level analyses.

The second phase of the experiment commenced in the beginning of the new academic year, lasting from mid-January to mid-March of 2020. We maintained the randomization and all intervention details of the first phase, but we supplemented the school edutainment treatment with an additional video which underscored the importance of handwashing both at school and at home (described in greater detail below). The edutainment treatment assignment followed students into their new grade: because all schools had only one class per grade, classes were preserved as they advanced grade levels.[9]

The final edutainment administration in schools occurred on Mar 14, 2020. While we had intended to continue collecting handwashing data for several months after the conclusion of the edutainment treatment in order to measure long-run effects, the onset of the COVID-19 pandemic led to the closure of all schools in mid-March and precluded further field work at schools or house-

7. Actual prevalence of COVID-19 cases was exceptionally low in rural Bangladesh during this period, and we document no impact of our interventions on likelihood of contracting COVID-19.

8. Due to constraints in the field, the edutainment treatment commenced two weeks before the household-level baseline survey was complete, leaving 520 households to have their baseline surveys administered after the first edutainment treatment had been delivered at school. As such, baseline household characteristics relevant to hygiene knowledge and behavior (the two sets of measures that may be impacted by the edutainment treatment) are dropped from the balance table for this subset of households, and regressions which include hygiene knowledge or behavior controls do not include these values for the 520 households surveyed after the first edutainment treatment.

9. In other words, a treated classroom that was in Grade 3 in 2019 would continue to be treated in Grade 4 in 2020; this posed minimal logistical difficulty since each school only had one class per grade. However, following cohorts over years meant that we lost the Grade 5 classes of 2019 (who moved onto secondary school in 2020), and as such do not appear in the school or household level data from January through March 2020. Grade 0 students of 2020 also do not appear in the data, as those students did not exist at the school during the time of randomization in 2019. Out of 775 household participant students, 119 were Grade 5 students in baseline. From the remaining 656 students, 14 non-Grade 5 students moved to out-of-sample schools and 30 non-Grade 5 students stayed within the same school but did not advance class levels normally (e.g. repeated grade) All these students are dropped from our sample from the new academic year.

holds for the safety of enumerators and study participants. As such, we were unable to continue providing households with soap, collecting sensor data, or conducting the endline survey in person. The endline survey for the household participants was instead conducted via phone in late April; all questions intended for the in-person survey were administered except for child anthropometric measures, which required physical engagement with the children.

III.C. *Empirical Strategy*

A. **Edutainment treatment effect**

To estimate whether the hygiene edutainment campaign shifted behavior within the classroom, we run the following regression:[10]

$$Y_{ct} = \alpha_{ct} + \beta Treated_c + \gamma_t + \lambda_s + \delta_g + X_c + \epsilon_{ct} \tag{1}$$

where Y_{ct} is the classroom level handwashing rate for the classroom c on day t, $Treated_c$ is an indicator variable for whether the classroom is edutainment-treated, γ_t is a day fixed effect, λ_s is a school fixed effect, and δ_g is a grade-level fixed effect. We additionally control for the saturation treatment status of the classroom and employ a cross-fit partialing out estimator to select control variables from our set of classroom characteristics (Chernozhukov et al. 2018).[11]

In order to make comparable to the school-to-home treatment effect, we additionally present results in which the outcome Y_{ct} is divided by the number of students per class, yielding an approximation of the per-student impact of the edutainment treatment at school.

B. **School-to-home transmission**

To estimate the degree of school-to-home behavioral transmission, we run the following re-

10. All specifications and outcome variables were prespecified in our preanalysis plan, available at https://www.socialscienceregistry.org/trials/4746. The minor deviations from this plan are outlined in detail in Appendix Figure III.

11. Despite the two randomizations of the edutainment and saturation treatments, we do not run a long model in which the two treatments are included not only as main effects but additionally as an interaction (Muralidharan, Wüthrich, and Romero 2020). Muralidharan, Wüthrich, and Romero (2020) defines the various cases in which a short model may in fact be appropriate, several of which speak to this experiment. First, ours is a conceptual, rather than a policy, experiment: our experiment is designed to estimate [the direction of] a behavioral phenomenon, rather than evaluate a particular programmatic intervention. Edutainment and dispenser provision are simply instruments to exogenously vary handwashing in two contexts. Second, the dispenser saturation treatment is intended to serve as a reasonable variant of the environment: edutainment campaigns may be administered in classrooms where children are more or less well resourced at home, and a weighted average of these settings is the relevant treatment effect in such a context. Third, we have no distinct theoretical motivation (in the context of behavioral transmission) to examine the interaction between the dispenser saturation and edutainment treatment arms, and therefore do not prespecify an estimation of this effect in our preanalysis plan. As such, the treatment effects we report should be interpreted as a weighted average of the main effect and the interaction with the cross-randomized treatment, the measure we view as most relevant given the objective of this study.

gression:

$$Y_{ht} = \alpha_{ht} + \beta Treated_h + \gamma_t + \lambda_s + \delta_a + X_h + \epsilon_{ht} \quad (2)$$

where Y_{ht} is the household level outcome for the household h on day t, $Treated_h$ is an indicator variable for whether the household has a child who attends an edutainment treated class, γ_t is a day fixed effect, λ_s is a school fixed effect, and δ_a is a child age fixed effect. As above, we additionally control for the saturation treatment status of the child's classroom and employ a cross-fit partialing out estimator to select control variables from our set of baseline characteristics.

C. **Home-to-school transmission**

To estimate the degree of home-to-school behavioral transmission, we run the following regression:

$$Y_{ct} = \alpha_{ct} + \beta Highsat_c + \gamma_t + \lambda_s + \delta_g + X_c + \epsilon_{ct} \quad (3)$$

where Y_{ct} is the class level outcome for classroom c on day t, $Highsat_c$ is an indicator variable for whether classroom c is a high saturation class, γ_t is a day fixed effect, λ_s is a school fixed effect, and δ_g is a grade-level fixed effect. We control for the edutainment treatment status of the classroom and employ a cross-fit partialing out estimator to select additional control variables.

D. **Child health**

To estimate child health impacts, we run the following regression:

$$Y_{ct} = \alpha_{ct} + \beta Treated_c + \gamma_t + \lambda_s + \delta_a + X_c + \epsilon_{ct} \quad (4)$$

where Y_{ct} is the incidence of child loose stool or acute respiratory symptoms, measured in the number of days of symptoms in the previous two weeks, for child c in survey round t, $Treated_c$ is an indicator variable for whether the child attends an edutainment treated class, γ_t is a survey round fixed effect, λ_s is a school fixed effect, and δ_a is an age fixed effect. As in earlier specifications, we control for the saturation treatment status of the child's classroom and employ a cross-fit partialing out estimator to select control variables from our set of baseline characteristics.

IV. RESULTS

IV.A. *Balance*

Appendix Tables II and III present the means of treatment and control groups and p-values for the difference in means, partialling out school and grade/age fixed effects, for a host of baseline observables.

Edutainment-treated classrooms appear no different from control classrooms at baseline. The households of edutainment-treated students are marginally more likely to own their home, own a latrine, and report washing with soap before eating than their control counterparts at baseline.

High saturation classrooms are likewise no different from low saturation classrooms by baseline observables, with the exception of class size: high saturation classrooms are marginally smaller than their counterparts. Households of students from high-saturation classrooms are less likely to treat drinking water, have mothers who are married at a slightly older age, and report higher rates of daily handwashing with soap and washing with soap before cooking.[12]

IV.B. *School edutainment treatment effect*

We first examine whether the hygiene edutainment campaign impacted handwashing behavior in the classroom. Figure II plots the hourly trends of handwashing behavior at schools for the edutainment treated and control classrooms. Handwashing rates across both groups predictably peak during students' two opportunities to eat, snack and lunch. Classes exposed to the edutainment campaign exhibit greater rates of washing throughout the day, both during and between these mealtimes. Figure III zooms out to the full duration of the experiment and plots average daily handwashing rates across treated and control classes: despite considerable variation across days, treated classrooms remain above control classrooms in handwashing performance over the course of the full experiment.

Column 1 of Table I presents the regression analog. Treated classrooms exhibit a 36% higher rate of handwashing than control classrooms ($p < 0.001$). Appendix Table IV examines effects by phase. In phase 1, edutainment treated classrooms exhibit a 20% higher rate of daily handwashing ($p < 0.001$); this effect increases to 106% in phase 2 ($p < 0.001$), during which washing in all classrooms declines, but substantially more so in control classrooms.[13]

12. Were we to document positive behavioral transmission from the household into the school, this imbalance may be concerning: perhaps children selected in high saturation classrooms to receive a dispenser at home simply possessed better hygiene habits. We instead find that high saturation classrooms handwash *less* than their low saturation counterparts. Moreover, we have no specification examining the effect of the saturation treatment at the household level, a treatment effect without an obvious theoretical prediction in the context of behavioral transmission.

13. Hussam et al. (2022a) documents a nearly identical magnitude of decline in handwashing rates in India within households with the same type of dispenser, suggesting that the decline is likely due to the novelty of the dispenser product rather than the context in which it was implemented.

IV.C. *School-to-home transmission effect*

The edutainment campaign significantly altered childrens' behavior of a generalizable skill in the classroom; we now examine whether this behavioral change was transmitted into their home environment. Table II illustrates the impact of the school edutainment treatment at the household level. Having a child from a treated classroom in the household reduces daily handwashing rates in the household by 7% ($p < 0.01$). Figure IV depicts these results graphically over the two phases of the edutainment experiment: households of treated students consistently perform below those of control students.[14]

Is this decline in home handwashing meaningful relative to the increase in handwashing exhibited at school? Column 2 of Table I approximates per-student changes in school-level handwashing due to the edutainment treatment by dividing classroom handwashing rates by the number of students per class. The average student in an edutainment-treated classroom washes 0.27 more times per day at school. According to Table II, however, she and her family wash a total of 0.36 fewer times per school day at home. In other words, the school edutainment intervention leads to a net *negative* effect on handwashing behavior for a treated child and her family.

IV.D. *Home-to-school transmission effect*

An intervention that significantly raises handwashing at school leads to a substantial reduction in handwashing at home. Do we observe a parallel pattern for an intervention that improves handwashing at home, in the form of handsoap dispenser provision? Recall that we cannot directly estimate the impact of dispenser provision on washing at home given the absence of a no-dispenser control arm. We instead rely on previous work establishing the significant health impacts of household-level dispenser provision (Hussam et al., 2022a; Hussam et al., 2022b), as well as a simple pre-post anlaysis: households self-report washing their hands with soap 3 times per day at baseline, while sensors reflect handwashing rates of 5.5 times per day during the first three months of the experiment after dispenser provision. Neither seasonality nor the onset of the COVID-19 pandemic are likely to have raised these rates: households were emerging out of monsoon season and not yet in the winter months (precluding both water-borne illnesses and colds from raising handwashing rates), and fears of COVID did not hit until several months later.

We now examine whether this intervention at the household impacts performance at school. Column 1 of Table III presents the results. Classrooms in which a quarter of all students receive handsoap dispensers at home wash 12% less ($p < 0.01$) than classrooms with only two dispenser children. In other words, classrooms in which a larger proportion of students are enabled to wash their hands at home exhibit significantly lower washing at school.

14. Trends in March should be interpreted with significant caution, as sensor failure rates in households spiked by this month, with only 335 of the 775 original sensors operating (all classroom sensors remained intact as we prioritized reallocating functional sensors to classrooms). Standard errors on treatment effects grow significantly in this final month.

Is this reduction in school-level handwashing meaningful? If we make the conservative assumption that every student in a high-saturation classroom mimics the behavior of their dispensered classmates (an assumption of complete peer effects), being in a high-saturation classroom is associated with a reduction of 0.08 washes per student per school day (Column 2 of Table III). If we instead assume no peer effects, such that the documented classroom level impact is driven solely by a change in the behavior of dispenser children, receiving a dispenser at home is associated with a reduction of 0.51 washes at school per school day.[15] Although *ex ante*, there is no reason that behavioral transmission from home to school caused by dispenser provision must be comparable to that from school to home due to edutainment provision, this bounding exercise is a reassuring check on the magnitude of the effects we document, and suggests that the negative transmission of handwashing behavior from home to school is likewise substantial.

IV.E. Underlying mechanisms

The replication of negative behavioral transmission under two distinct interventions in the dual directions of school to home and home to school suggests that the effects we observe are not unique to a specific environment nor intervention, but rather tied to the behavior of handwashing itself. We consider two sets of mechanisms through which this negative transmission may transpire.

1. *Crowd-out*

We first consider a broad set of mechanisms which we convene under the term 'crowd-out.' Among them are the act of moral licensing and the depletion of cognitive or effort capacity. Moral licensing is a form of psychological bargaining with oneself, in which one permits herself some bad behavior – or absence of good behavior – after having performed a good deed: good behavior licenses one to become subsequently morally lax (Blanken, Ven N., and Zeelenberg 2015; Merritt, Effron, and Monin 2010). In the context of school-to-home transmission of handwashing, children who are exhorted by an edutainment campaign to (and in turn do) handwash at school may feel it acceptable to shirk in the effortful obligation later in the day, such as when they return home. In the case of home-to-school transmission, children who are compelled (either by parents or intrinsically) to use the handsoap dispenser to wash before breakfast may feel licensed to skip a wash before their lunch at school.

Depletion due to limited cognitive, attentional, or effort capacity (Luk-Zilberman 2021) generates a similar story. Limited capacity to perform a given behavior implies that children who are exogenously moved to handwash in the morning at home will have fewer washes remaining until they have exhausted their capacity at school. Likewise, those who are moved to wash at school will have fewer washes remaining before reaching their capacity constraint at home.

15. This back-of-the-envelope calculation is performed as follows: high saturation classrooms exhibit 3.6 more washes per school day than low saturation classrooms and have an average of 7 more students with dispensers at home than low saturation classrooms. $\frac{-3.6}{7} = 0.51$ fewer washes per additional dispenser student.

These two mechanisms are not an exhaustive list of behavioral motives behind crowd-out: target consumption, as a parallel to Camerer et al. (1997)'s target income, in which individuals have a target number of acts they are willing to engage in during a given time period, may play a role. So too may a tradeoff between extrinsic and intrinsic motivation (Bénabou and Tirole 2003) - a channel particularly relevant when teachers (parents) encourage their students (children) to wash, resulting in lower intrinsic motivation by the child to do so thereafter. Teasing apart these mechanisms requires reliable information on the underlying (and often subconscious and intertwined) motivations of our child participants, data that is beyond the scope of this study. We therefore remain agnostic on the underlying sub-mechanism, as is done implicitly by the bulk of behavioral literature on crowd-out: for example, most experimental evidence on the crowding out of intrinsic motivation is also consistent with a crowding out of attention or effort due to limited capacity; Luk-Zilberman (2021) surveys the literature on negative behavioral spillovers attributed to limited attention but makes explicit the inability to distinguish this channel from that of changes in preferences, which in turn determine one's chosen motivation to perform a task, as in Bénabou and Tirole (2003) or Dolan and M.Galizzi (2015).

We instead focus on a key temporal prediction that is shared across this body of crowd-out mechanisms - one that our time-stamped data on handwashing performance offers a unique opportunity to test: once an effortful action has been undertaken, it is less likely to be performed again.

While crowd-out may transpire over a variety of temporal margins (a week, month, or the duration of the experiment), we focus on the natural temporal unit of the day. If the mechanism of crowd-out is operative within the day, then we may expect the negative relationship with home handwashing rates to be strongest after edutainment-treated children have engaged in additional washing at school, rather than before they go to school, on a given day. We examine this pattern in Table IV, in which we divide the daily treatment effect into before school, during school, and after school hours. We find a statistically insignificant reduction in handwashing before school hours (Column 2), at 4% less than the control mean and one fifth of the total daily treatment effect. During school hours (Column 3), when only adults or non-school, and therefore non-treated, children can be using the dispenser at home, we observe a negative effect of similar magnitude, suggesting that at least a portion of the before-school effect is driven by these other household members.[16] Column 4 of Panel A demonstrates that the bulk of the negative transmission effect arises once treated children return home from school, with 9% lower handwashing during this period than the control mean and two thirds of the daily treatment effect ($p < 0.001$) transpiring after school.

Table IV presents results for total handwashing episodes within each specified time period, but results are robust to transforming the outcome to hourly rates (Appendix Table VI): treated

16. The presence of a statistically significant negative impact during these hours is suggestive of behavioral spillovers within the family, a finding explored further in Section IV.G..

households wash a statistically insignificant 1% less than control households per hour in the hours before school, but wash 9% less per hour ($p < 0.001$) in the hours after school. Reductions in handwashing in the homes of edutainment-treated children are driven primarily by the hours *following* exposure to the school environment.

Beyond the temporal prediction, crowd-out mechanisms offer a further prediction on the extent of behavior change in each context: the degree of crowding out should be commensurate with the degree of prior engagement. High engagement in one context should generate high crowd out in another; little engagement in one context should yield little behavior change in the other. In order to test this prediction, we require exogenous variation in the *magnitude* of behavior change in one setting. Our experimental design varies whether or not behavior change transpires, but not the magnitude of change. However, we can exploit the interruption of the experiment by the end-of-year exams and holidays as a plausibly exogenous shock to handwashing behavior, as all participants face the exam period and holidays regardless of handwashing preferences or performance. Households exhibit a sharp decay in handwashing rates as soon as exam period and the holidays begin: while household handwashing rates are more than twice baseline performance prior to the holidays, they fall by nearly 50% during and after the holidays (Appendix Figure II).[17] To get a sense of the degree to which the dispenser 'treatment-effect' changes over this time period, consider that mothers report washing their hands with soap three times per day at baseline: this is likely an overestimate of individual washing due to demand effects that we document below, but an underestimate of total household washing, which we have no reliable means of collecting in the absence of the dispensers. Relative to this, it is plausible that the pattern we observe is one of households eventually returning to near-baseline handwashing patterns, resulting in a zero treatment effect of the dispenser at home during and following the holidays.

How does this large reduction in household handwashing translate into the classroom? Figure V plots the daily handwashing rate for high and low saturation classes over the course of the experiment, with Appendix Table IV presenting the regression analog, estimating effects separately before and after the exam-holiday 'shock' (equivalent to phase 1 and phase 2 for schools). We find that highly saturated classrooms wash substantially less than their low-saturated counterparts prior to the holidays, but this gap nearly closes after the holidays. With the rapid decay in household dispenser use and consequently a small 'first stage' of handwashing within the home after the holidays, we observe very little crowd-out of the behavior among dispenser children at school in phase 2.

We formalize this pattern further in Figure VI, in which we plot household handwashing rates against the size of the school saturation treatment effect, with each observation representing one day in the experiment. We note two features. First, nearly all observations fall in the bottom right quadrant, reflecting negative behavioral transmission. Second, the points are fit by a neg-

17. Notably, the rate of decay in household handwashing exhibited in this study is nearly identical to that exhibited in both the handwashing experiments of Hussam et al. (2022a) and Hussam et al. (2022b), despite occurring in different years, seasons, and in the context of different interventions.

atively sloping line ($\beta = -1.5$, p-value = 0.000): the degree of crowd-out exhibited at school is commensurate with the degree of handwashing (a proxy for the size of the dispenser effect) at home.

Although we have no source of exogenous variation in the degree of edutainment impact on school handwashing rates, precluding a robust way to test the prediction on magnitudes in the reverse direction of school to home, the observed patterns are consistent with the prediction: the impact of edutainment at school on positive behavior change persists throughout the experiment, and the negative transmission into the home persists in parallel.

2. *Cue-based habit formation*

The second mechanism we consider is that of cue-based habit formation (Wood and Neal 2007; Orbell and Verplanken 2010; Laibson 2001), in which repetitive behaviors become triggered by the sensory environments in which they are performed. The converse likewise holds: the absence of triggers can inhibit a behavior, and is therefore a way to break a habit (Duhigg 2012). In other words, a behavior change intervention that is attached to a specific context may succeed in amplifying the behavior in the context of the intervention while diminishing it in all others. In the context of this study, a behavior encouraged in school via the edutainment intervention may be subconsciously tied to the school context and disassociated from the home environment. Likewise, a behavior tied to the home, through mornings in a home equipped with a dispenser, may be subconsciously tied to the home and disassociated from the school.[18]

Recent work (Hussam et al., 2022a; Wellsjo 2021) has established handwashing as a habit forming activity, or a behavior with intertemporal complementarities wherein greater initial engagement generates greater future engagement. In the context of hospitals in the U.S., Wellsjo (2021) further finds that the behavior is cued by entering highly frequented rooms, while in the context of households in rural India, Hussam et al. (2022a) finds that the behavior may be cued by a specific time of year (winter) with its accompanying environmental triggers. We consider whether handwashing behavior, when motivated by an intervention delivered in the school environment, may become attached to the cue of the school at the expense of performance in the home. To our knowledge, previous literature on cue-based habit formation has not examined the impact of an exogenously imposed cue on behavior in *non-cued* settings, making this a first opportunity to explore the potential unintended consequences of nurturing habits that are attached to environmental cues.

A key prediction of the mechanism of cue-based habit formation, as distinct from that of crowd-out, is that the negative treatment effect for edutainment-treated children at the household should manifest even on days when children do not have school: days in which there is no opportunity for crowding out behavior, but [the absence of] the trigger remains. To examine this, we estimate

18. Recall that, while both the home and the school have dispensers, the saturation exercise is comparing children who do and do not have dispensers at home. Any differential increase in use at the home can be *differentially* tied to the home cue relative to the school cue.

the impact of the edutainment campaign on household washing during holidays and weekends. Results are presented in Column 5 of Table IV. We find that households of edutainment-treated children exhibit significantly lower rates of handwashing than their non-treated counterparts even on weekends and holidays, with 0.48 fewer washes ($p < 0.001$) on days when crowding out from school consumption is not possible. The negative treatment effect on weekends and holidays is in fact 41% larger than that on school days, as more time is spent at home. In other words, the absence of the classroom and edutainment cue (or isomorphically, the presence of the home cue, which, over the course of repeated (in)activity has been disassociated from handwashing) leads to lower handwashing in the homes of edutainment-treated children relative to control households regardless of whether the crowd-out mechanism has been directly engaged.

While cue-based habit formation may alone explain the negative behavioral transmission we observe, a crowd-out mechanism may exacerbate it. Children are first moved to wash by the edutainment campaign at school, raising school-level washing and attaching the school cue to washing. Crowd-out behavior, accompanied by the absence of the school cue, then leads to lower washing upon returning home. This reduced washing is in turn adopted by other household members, further reducing washing at home. As the school week proceeds, such repeated daily behavior becomes habitual, reducing handwashing at home on weekends and holidays as well. The combination of these behavioral mechanisms results not only in negative behavioral transmission, but in a net negative total impact on handwashing due to the edutainment intervention.

IV.F. *Potential confounds*

Context-specific intervention While our findings are consistent with a mechanism tied to the behavior of interest - namely that the act of handwashing is characterized by crowd-out and cue-based habit formation - we consider here a feature of the experimental setting that may instead be driving our results.

Because children received the edutainment intervention at school, perhaps they understood hand hygiene to be a school-specific behavior. In other words, perhaps edutainment-treated students interpreted handwashing as an act that was intended for school, and not for the home, resulting in the negative edutainment treatment effect on home washing. We view this story as distinct from that of cue-based habit formation: behavior which is consciously learned (in this case, incorrectly so) can be unlearned through information provision; behavior which is the product of contextual triggers, however, cannot. This distinction between learned and subconscious motives to action is given theoretical exposition in Romer (2000).

We find this alternative story unlikely for several reasons. First, the content of the edutainment was weighted towards material that featured the home: within a typical week of content, 45% of content-time demonstrated washing within a home, 50% in location neutral sites (washing at a sink, playing in dirt), and 5% at a school. Second, the distinction between phase 1 and phase 2 was a direct effort to address this potential confound: in phase 2, we supplemented the existing content with a video featuring one author exhorting the students to wash their hands with soap both at school and at home. If reduced household handwashing was due to incorrect learning regarding the objective of the intervention, a supplementary message conveying the correct information about the home should remedy students' misunderstanding. Table V presents the results of a difference-in-differences regression which estimates the impact of this adjustment to the intervention: we find that the video has no impact on handwashing among edutainment-treated students, neither at school nor within the household. Such students continue to wash significantly more at school and less at home than their control counterparts.

Most critically, we document negative behavioral transmission not only from the school into the home, but also from the home into the school. The latter result employs a distinct intervention that is not vulnerable to the potential artifact of context-specificity: children treated at home were 'treated' with handsoap dispensers, a product that they had access to in both the home and the school. They therefore had no reason to interpret the dispenser as a 'home-specific' technology, yet exhibit negative behavioral transmission of handwashing into the classroom nonetheless.

Intrahousehold substitution of soap Our soap use data, by construction, only captures the use of dispenser soap. 94% of sample households, however, possess barsoap at baseline, a product used primarily for bathing and washing dishes. Perhaps the parents of edutainment-treated students, having been informed of their children's school program and potentially attuned to the value of the dispenser soap for their children, choose to substitute away from dispenser soap for their own use and instead use their existing barsoap?

We view this alternative as unlikely for two reasons. All households were informed that unlimited dispenser soap would be provided for the duration of the experiment, and soap in the household dispenser was publicly topped up every two weeks. Empirically, no household ever presented with an empty dispenser during the monthly visit. There should therefore have been no [differential] concern of soap scarcity nor a need to divert one's soap use in order to save soap for children. Second, we conduct a followup survey one year after the intervention to probe the channels that may have led to reduced soap consumption at home. We ask all respondents the following question: *"Think about all the times you were working around the house, and you knew it would have been helpful to wash your hands with soap, but you decided not to use the dispenser. Why didn't you use it?"*

We then prompted respondents with the following possible answers: *"For example, perhaps you... (A) forgot, (B) didn't have time, (C) found it inconvenient, (D) wanted to save soap for other members of the family, (E) viewed the dispenser as your child's dispenser, (F) used hard soap instead, or (G) does not apply."* The impact of being the parent of an edutainment-treated child on the likelihood of each answer is presented in Table VI. We observe no impact of a child's edutainment status on the likelihood that a parent will report any of these reasons, with the exception of *"I forgot,"* which edutainment parents are 31% more likely to report than control parents, marginally significant when unadjusted for multiple hypotheses.[19] Notably, while imprecise, the magnitude of the coefficients on (D), (E), and (F) are near zero. Given that surveyor demand, were it present, should alone raise the likelihood of reporting these three reasons among parents of edutainment-treated students, we view a near-zero magnitude effect as plausible evidence that this narrative is not a primary driver of the negative transmission we document.

Optimal behavior Perhaps the negative transmission we find is, in fact, optimal? For example, edutainment-treated children may strategically allocate their effort to increase washing in contexts where they believe there are meaningful health returns (at school, where children and germs are abundant) and reduce washing in potentially less consequential spaces such as the home. However, were this the driving force behind our results, it would preclude a negative behavioral spillover in the other direction of home-to-school, which we document.

Alternatively, perhaps edutainment-treated children are shifting those daily activities which require handwashing (such as eating meals or defecating) into school hours, therefore requiring less washing at home? This is also unlikely: all children have unlimited dispenser and soap access in both their school and their home and therefore possess no incentive to shift their activities - a presumably costly act - to the four hours of each weekday they remain at school. Such shifting is also implausible, as defecation is not easily scheduled for young children, and all children eat breakfast and dinner at home. Most decisively, we observe significant reductions in handwashing during holidays and weekends as well, when children have no such opportunity to shift activities into school hours.

IV.G. *Self-reported knowledge and behavior*

Our results and exploration of mechanisms suggest that the behavior of handwashing is vulnerable to negative behavioral transmission across contexts, evidenced in the dual directions of home to school and school to home. We find this result surprising, as our initial motivation behind designing this experiment was to identify the sites of intervening that might generate the largest returns to behavior and health via *positive* transmission. Priors, derived both from the limited existing literature on behavioral transmission (Hiramatsu et al. 2014; Tidwell et al. 2020) and the general nature of education curricula, also favored the existence of positive transmission across contexts. To better understand where positive transmission of behavior from the school into the home may have been hampered, we examine data on hygiene knowledge and behavior collected at endline, six weeks after the conclusion of the intervention.

As is demonstrated in Panel A of Table VII, children treated with the edutainment campaign transmit some information about the edutainment treatment to their mother: mothers of treated children are more likely to report that their child told them about the cartoons they saw at school

19. This reason is consistent with the evidence of parental spillovers, discussed further in Section IV.G.: not seeing one's child utilize the dispenser at home may make it easier to forget to use it oneself.

and to volunteer that the cartoon content was related to handwashing. However, Panel B indicates that little substantive information is conveyed or internalized: mothers of treated children are no more likely to know when their children should wash their hands, what might happen if they do not wash, nor how to wash properly - the central content of the edutainment program. They do exhibit greater awareness that the school and the home are the two most important sites of handwashing for their child.

Did edutainment-treated children learn that handwashing with soap was important but fail to convey it, or not learn from the edutainment at all? While we do not collect data on hygiene knowledge among children, literature on human capital formation suggests that younger children are more amenable to learning from instruction than older children (Heckman and Mosso 2014). Appendix Table VI therefore examines the impact of the edutainment program on school handwashing by grade. We see no obvious pattern: while younger children appear slightly more responsive to the program at school, we cannot reject equality against impacts among the oldest children. The absence of evidence of learning is consistent with the results of Hussam et al., 2022b, which finds that an identical edutainment intervention, when administered via mobile phones to parents and their children at home, results in large changes in handwashing behavior with no corresponding changes in hand hygiene knowledge. Instead, the authors find evidence that behavior change is precipitated by cues (namely exposure to the mobile phone) immediately prior to a handwashing episode - a finding consistent with the underlying mechanism of cue-based habit formation considered here.

Despite no clear change in knowledge about the returns to proper hand hygiene, we do document improvements in self-reported handwashing behavior: Panel C of Table VII shows that mothers of edutainment treated children report washing 0.37 more times per day ($p < 0.05$), are 14% more likely to report washing with soap before they cook ($p < 0.05$), and are 4% more likely to report washing with soap before they eat ($p < 0.05$).

We draw two lessons from this body of self-reported data. First, the self-reports of household handwashing behavior describe a pattern in direct opposition to our sensor data on household handwashing behavior. This inconsistency underscores how vulnerable self-reports are to salience or demand effects: mothers of treated children report significantly better hand-hygiene than their control counterparts, yet our objective and high-frequency measures suggest that these households in fact use the handsoap dispenser significantly *less*, and our follow-up survey indicates no greater use of alternative soap (Table VI). In other words, it appears that respondents either incorrectly believe that they are washing more (perhaps due to greater salience around handwashing in the treatment arm), or they intentionally over-report washing in response to beliefs about surveyor demand. This observation suggests a reevaluation of a number of previous studies that report positive behavioral impacts from handwashing campaigns but rely on self-reported behavior (Contzen, Pasquale, and Mosler (2015), Johansen et al. (2015), and Hovi, Ollgren, and Savolainen-Kopra (2017), among others) and reiterates the necessity of objective data for behaviors otherwise vulnerable to demand effects. Were we to rely on our self-reported measures of handwashing rather than the sensor data in the household, we would document positive behavioral transmission of our edutainment program and, as a result, promote the scale-up of an intervention that in reality results in a net negative impact for treated children and their families.

Second, the absence of evidence of positive knowledge transfer from child to mother highlights a key barrier to the transfer of positive behavior into the home: family members may not gain substantive information from a child's school experiences. Rather, they appear to mimic a child's actions at home, further exacerbating negative transmission. This result contributes to a small body of evidence on the 'reverse' vertical transmission of behaviors from children to parents (Berniell, Mata, and Valdés (2013) and Lundborg and Majlesi (2018)), with existing literature focusing on how positive behavioral change among children can positively influence parents. Here, instead, we find that a reduction in positive behavior among children may likewise be adopted by others in the household. Notably, these results lie in opposition to the increasingly popular label of children as potential "change agents" in the home for health behaviors adopted at school (Olayiwole, Ezirim, and Okoro 2003; Bresee et al. 2016; Burrows 2017).

IV.H. *Impacts on health*

What are the downstream consequences of this negative behavioral transmission across contexts? Results are presented in Table VIII. Our estimates of health impacts are imprecise, but qualitatively consistent with the negative impact of the program on total handwashing: we document a statistically insignificant rise in the incidence of loose stool and respiratory infection symptoms among edutainment-treated children.

The imprecision of our health estimates precludes much interpretation. We posit, however, that the relevant comparison should be - not a null effect on health, but rather - the effect on health that the edutainment intervention would have generated in the absence of behavioral spillovers. While we cannot determine this directly, we draw from an identical edutainment intervention randomized at the household level in neighboring villages two years prior to this experiment (Hussam et al., 2022a). This study documented a 20% increase in the handwashing rates of treated households, which translated into a 54% ($p = 0.011$) decline in loose stool and a 29% ($p = 0.005$) decline in symptoms of acute respiratory infection among exposed children. Why might behavioral transmission have plausibly been absent in such a setting? The study did not intervene in schools, where baseline handwashing with soap was zero: no school carried soap. As such, the household-level intervention did not create an opportunity for crowd-out in complementary contexts: washing at school was zero at baseline and could not go lower, despite an increase in home washing.[20] We therefore view the health impacts found in this earlier study as a reasonable proxy for what the edutainment intervention may have done in the absence of negative behavioral transmission. While our estimates

20. In contrast, the present study, by distributing soap dispensers in both the school and the home among treated and control households, generated an opportunity to wash (which was evidently taken by control households), and therefore an opportunity for crowding out (as evidenced by treated households).

are too noisy to reject health improvements even of the magnitude of Hussam et al. (2022a), this earlier study suggests that the negative behavioral transmission we document may have had meaningful consequences for the health of treated children, who could have benefited from the edutainment in the absence of such transmission.

Beyond the direct impact on child health, because both fecal matter and coughs and colds are common means of spreading bacterial and viral infections, negative behavioral transmission from the school into the home may have important consequences for vulnerable members of the household, whose health status we do not collect in this study. 31% of children in our sample live in a household with an adult over the age of 50 years in a setting where life expectancy is approximately 66 years (Islam et al. 2017). If children respond enthusiastically to a hand hygiene campaign at school, but consequently reduce washing in a home with elderly grandparents, an intervention deemed successful at school may produce substantial negative health spillovers in the home, especially in light of the COVID-19 pandemic.

Conversely, if schools are a center of behavioral spillovers via peer effects as well as health spillovers via physical proximity and play, one may want to ensure that encouraging hygienic practices at home does not unintentionally reduce such behavior in a site with greater externalities. These remain necessarily empirical questions, for which we hope this study can serve as a model, underscoring the necessity of estimating intervention impacts across all sites of action before scaling up behavior change policy.

V. Conclusion

Early education programs are grounded in an assumption of the positive transmission of behavior. Our results suggest that certain behaviors may instead be substitutes across contexts, with engagement in one site crowding-out engagement in another. At its worst, this mechanism may result in the negation of positive behavioral change, or a net-zero impact. However, when paired with cue-based habit formation or spillovers to others, a behavior change campaign that appears successful in one environment may in fact yield a negative impact on net.

In the midst of a global pandemic precipitated by the viral transmission of COVID-19, the cultivation of proper hand hygiene has become imperative for the health and survival of exposed populations around the world. Handwashing with soap has of course long been a necessary tool to reduce infant and child mortality across the developing world, where bacterial and viral contamination end the lives of more than one million individuals per year (WWAP 2019). The COVID-19 pandemic amplifies the need for preventive measures such as handwashing in developing countries, which are characterized by high population densities in urban centers, lack of medical personnel and equipment, scarcity of testing kits, and an absence of proper refrigeration facilities for vaccines. This study estimates the impact of an intervention at school, a site that is a frequent locus for public health programs and should capitalize on economics of scale, on behavior change both at school and at childrens' homes. Notably, we designed the program with this expectation in mind: we hoped to document large positive behavioral spillovers across contexts. Instead, we find our intervention to be quite successful at improving behavior at school, but document significant unintended consequences in the home. As efforts to improve handwashing adherence receive increased attention globally, we hope this study can shape how evaluations of such programs are designed and urge caution in the interpretation of the results of existing studies.

Beyond the application to handwashing, the behavioral mechanisms we propose - crowd out and cue-based habit formation - have each been documented across a wide range of commonplace and high-return behaviors in the literature. We view the combination of these two mechanisms as plausible across a range of behaviors, many of which are consequential to human capital formation, health, and community wellbeing: conservation, diet, and mask-wearing, among others. Likewise, the contexts in which negative transmission may transpire are not limited to the school and the home. From the workplace to the gym to places of worship, we are perpetually engaged – or expected to be engaged – in effortful activity across time and physical space. We view this paper as a proof-of-concept of the phenomenon of behavioral transmission, and hope it may serve as a template for how experiments in other contexts may be designed to estimate the same.

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TABLES

TABLE I. Edutainment treatment at schools

	(1)	(2)
Edutainment treatment	8.963***	0.271***
	(0.975)	(0.027)
Edutainment control mean	25.185	0.752
	[40.530]	[1.234]
Observations	12,427	12,427

Notes. The unit of observation is class by day. Dependent variable for column (1) is daily handwashing rate as measured by total number of soap dispenser presses per day per class, and total number of daily presses per class divided by the number of students in each class for column (2). We control for the saturation status of the classroom in all regressions, and use a cross-fit partialing-out estimator (Chernozhukov et al. 2018) to select from among potential class control variables shown in Appendix Table I. Dates range from Oct 12th – Mar 15th, 2020 (22 weeks, with 5 weeks of vacancy between phase 1 and phase 2 and Fridays, national holidays, and winter break dropped). All include date, school, and grade level FE. Robust standard errors clustered at the class level are reported in parentheses, and standard deviations are reported in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE II. Edutainment treatment at households

	(1)	(2)
Edutainment treatment	-0.362***	-0.069***
	(0.100)	(0.023)
Control mean	5.193	1.173
	[7.112]	[1.629]
Observations	86,610	86,610

Notes. The unit of observation is household by day. Dependent variable for column (1) is daily handwashing rate as measured by total number of soap dispenser presses per day per household, and total number of daily presses per household divided by the number of household members for column (2). Dates range from Oct 12th, 2019 - Mar 12th, 2020 and include both school and non-school days. All models control for the saturation treatment status of the classroom of the dispenser child. We use a cross-fit partialing-out estimator (Chernozhukov et al. 2018) to select from among potential household controls shown in Appendix Table I. All include date, school, and age FE. Robust standard errors clustered at the class level are reported in parentheses, and standard deviations are reported in brackets. *** p<0.01, ** p<0.05, * p<0.1

TABLE III. Saturation treatment at schools

	(1)	(2)
Saturation treatment	-3.582***	-0.083***
	(0.851)	(0.024)
Saturation control mean	30.630	0.873
	[46.136]	[1.337]]
Observations	12,427	12,427

Notes. The unit of observation is class by day. Dependent variable for column (1) is daily handwashing rate as measured by total number of soap dispenser presses per day per class and total number of daily presses per class divided by the number of students in each class for column (2). We control for the edutainment status of the classroom in all regressions, and use a cross-fit partialing-out estimator (Chernozhukov et al. 2018) to select from among potential class control variables shown in Appendix Table I. Dates range from Oct 12th – Mar 15th, 2020 (22 weeks, with 5 weeks of vacancy between phase 1 and phase 2, and Fridays, national holidays, and winter break dropped). All include date, school, and grade level FE. Robust standard errors clustered at the class level are reported in parentheses, and standard deviations are reported in brackets. *** p<0.01, ** p<0.05, * p<0.1

TABLE IV. Edutainment treatment effect at households throughout the day

	School days				Non-school days
	(1) All day	(2) Before school	(3) At school	(4) After school	(5) All day
Edutainment treatment	-0.342***	-0.065	-0.086**	-0.208***	-0.483***
	(0.104)	(0.047)	(0.044)	(0.060)	(0.138)
Control Mean	5.179	1.642	1.313	2.223	5.343
	[6.765]	[3.218]	[3.014]	[3.810]	[8.372]
Observations	68,141	68,141	68,141	68,141	18,469

Notes. The unit of observation is household by day. Dependent variable is daily handwashing rate as measured by total number of soap dispenser presses per day per household within the time range specified (the exact hours are specific to students' grade and school). Dates range from Oct 12th, 2019 - Mar 12th, 2020. All columns control for saturation treatment status, and include date, school, and age fixed effects. We use a cross-fit partialing-out estimator (Chernozhukov et al. 2018) to select from among potential household characteristic controls shown in Appendix Table I for all models. Robust standard errors clustered at the class level are reported in parentheses and standard deviations are reported in brackets. *** p<0.01, ** p<0.05, * p<0.1

TABLE V. Pre-post analysis of edutainment treatment

	(1) School	(2) Household
Edutainment Treatment	0.221**	-0.320
	(0.087)	(0.230)
Post	-0.766***	-1.446***
	(0.075)	(0.174)
Edu-treatment × Post	0.067	0.064
	(0.115)	(0.225)
Control mean	0.752	5.214
	[1.234]	[7.140]
Observations	12,427	89,048

Notes. Dependent variable for column (1) is total number of dispenser presses per day per class divided by the number of students in the class. Dependent variable for column (2) is total number of dispenser presses per day per household. All models control for the saturation treatment status and include school and class level/age fixed effects. Robust standard errors clustered at the class level are reported in parentheses and standard deviations are reported in brackets. *** p<0.01, ** p<0.05, * p<0.1

TABLE VI. Intrahousehold substitution of soap in follow-up survey

	(1) A	(2) B	(3) C	(4) D	(5) E	(6) F	(7) G
Edutainment treatment	0.057	0.023	0.019	0.008	-0.007	-0.013	-0.045
	(0.033)	(0.034)	(0.021)	(0.039)	(0.035)	(0.039)	(0.030)
Control mean	0.186	0.180	0.073	0.268	0.265	0.585	0.220
	[0.390]	[0.385]	[0.261]	[0.444]	[0.442]	[0.493]	[0.415]
Observations	688	688	688	688	688	688	688

Note: The unit of observation is households. The outcome is different possible answers to the question "Think about last year. Think about all the times you were working around the house, and you knew it would have been helpful to wash your hands with soap, but you decided not to use the dispenser. Why didn't you use the dispenser?"
(A): I forgot
(B): I didn't have time
(C): It was inconvenient
(D): I wanted to save the soap for other members of my family
(E): Because I think of it as my child's soap dispenser, not mine
(F): I used a hard soap in our home instead
(G): Does not apply
We control for the saturation treatment status of the household and use a cross-fit partialing-out estimator (Chernozhukov et al. 2018) to select from among potential household characteristic controls shown in Appendix Table I. All models include school and age FE. Robust standard errors clustered at the class level in parentheses and standard deviations are reported in brackets.
*** p<0.01, ** p<0.05, * p<0.1

TABLE VII. Endline measures of edutainment treatment

Panel A: Mother's knowledge of edutainment treatment

	(1) Child talked to mother	(2) Shared the content with mother	(3) Mother described content correctly	(4) Handwashing topic
Edutainment treatment	0.558***	0.384***	0.442***	0.089***
	(0.041)	(0.081)	(0.072)	(0.025)
Control Mean	0.253	0.551	0.551	0.861
	[0.435]	[0.501]	[0.501]	[0.347]
Observations	609	351	351	609

Panel B: Mother's hygiene knowledge

	(1) When wash-child	(2) If no wash may get sick	(3) Handwash procedure	(4) Where wash-child	(5) When wash-mother
Edutainment treatment	-0.019	-0.007	0.003	0.037***	0.027
	(0.016)	(0.021)	(0.017)	(0.014)	(0.020)
Control Mean	0.967	0.930	0.905	0.930	0.912
	[0.179]	[0.255]	[0.294]	[0.255]	[0.284]
Observations	609	609	609	609	609

Panel C: Mother's self-reported hygiene behavior

	(1) Soap per day	(2) Soap before cook	(3) Soap before eat	(4) Child soap before eat
Edutainment treatment	0.370**	0.081**	0.038**	0.018**
	(0.175)	(0.035)	(0.016)	(0.008)
Control Mean	5.553	0.586	0.963	0.978
	[2.794]	[0.493]	[0.188]	[0.147]
Observations	609	609	609	609

Notes. The unit of observation is households. The endline survey was administered via phone at the end of the intervention, five months after the start of the edutainment treatment. All models control for the saturation treatment status and use a cross-fit partialing-out estimator (Chernozhukov et al. 2018) to select from among potential household controls shown in Appendix Table I. Panel C explicitly controls for the baseline hygienic knowledge and behavior outcomes, on top of selecting from among other household controls. All include school and age fixed effects. Standard errors clustered at the class level in parentheses and standard deviations are reported in brackets. *** p<0.01, ** p<0.05, * p<0.1

Panel A

(1): "Did your child ever talk to you about watching a cartoon at the school?"
(2): "If answer to previous question is 'yes', did s/he tell you about the content of the cartoon?"
(3): Content of the cartoon the mother describes is related to handwashing (descriptive)
(4): Mother correctly chose washing hands as the main topic of the cartoons (multiple choice)

Panel B

(1): "When are the most important times for your children to wash their hands?"
(2): "What could happen if your children don't wash their hands?"
(3): "What should you not do when you wash your hands?"
(4): "Where are the two important places for your children to wash their hands at?"
(5): "When should you wash your hands?"

Panel C

(1): "How many times a day do you wash your hands with water and soap? "
(2): "Do you wash your hands with soap before you cook?"
(3): "Do you wash your hands with soap before you eat?"
(4): "Do your children wash their hands with soap before they eat?"

TABLE VIII. Child Health Measures

	(1)	(2)	(3)	(4)
	ARI		Diarrhea	
	Days	Binary	Days	Binary
Edutainment treatment	0.019	0.008	0.027	0.005
	(0.081)	(0.020)	(0.020)	(0.006)
Control mean	0.873	0.242	0.0.021	0.010
	[1.821]	[0.429]	[0.248]	[0.101]
Observations	2,009	2,009	2,009	2,009

Notes. Sample is four rounds of surveys administered every month after the baseline survey to the households. Outcome for columns (1)-(2) is incidence of acute respiratory symptoms (runny nose or cough); outcome for columns (3)-(4) is incidence of loose stool. 'Days' measures the total number of days of symptoms in the two weeks prior to the interview date and 'Binary' equals one if there were any symptoms in the previous two weeks and zero otherwise. All models control for the saturation treatment status and for the baseline health outcome. We use a cross-fit partialing-out estimator (Chernozhukov et al. 2018) to select from among potential household controls shown in Appendix Table I. All include school, age, and survey round fixed effects. Robust standard errors clustered at the class level are reported in parentheses and standard deviations are reported in brackets.
*** p<0.01, ** p<0.05, * p<0.1

FIGURES

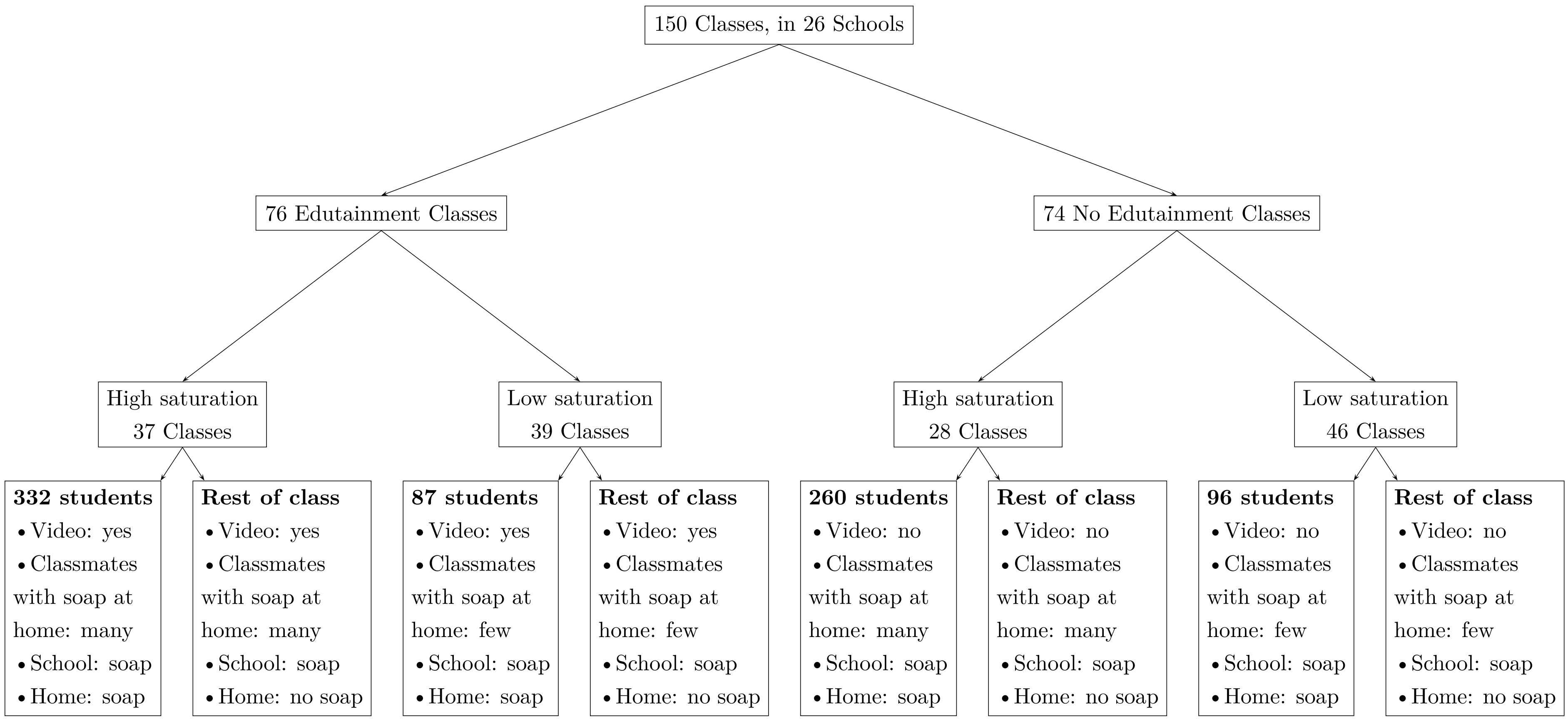


FIGURE I. Description of Full Sample

Notes. Full sample is composed of eight groups, divided according to video exposure (edutainment treatment group vs. edutainment control group), proportion of classmates with dispensers at home (high saturation treatment vs. low saturation treatment group), and whether the students themselves have soap dispensers at home. All groups have access to soap dispensers in classrooms, and 775 randomly selected students have access to soap dispensers at home. At schools, the hand soap usage can thus be measured for all groups aggregated at the class level, while the hand soap usage can be measured for the randomly selected 775 students at the households, aggregated at the family level. The identity of the individual dispenser user is unknown in both classrooms and households, but we proxy for distinct users by collapsing presses that occur two or fewer seconds apart into a single press. All results are reported on this 'extensive' margin of uses, rather than presses.

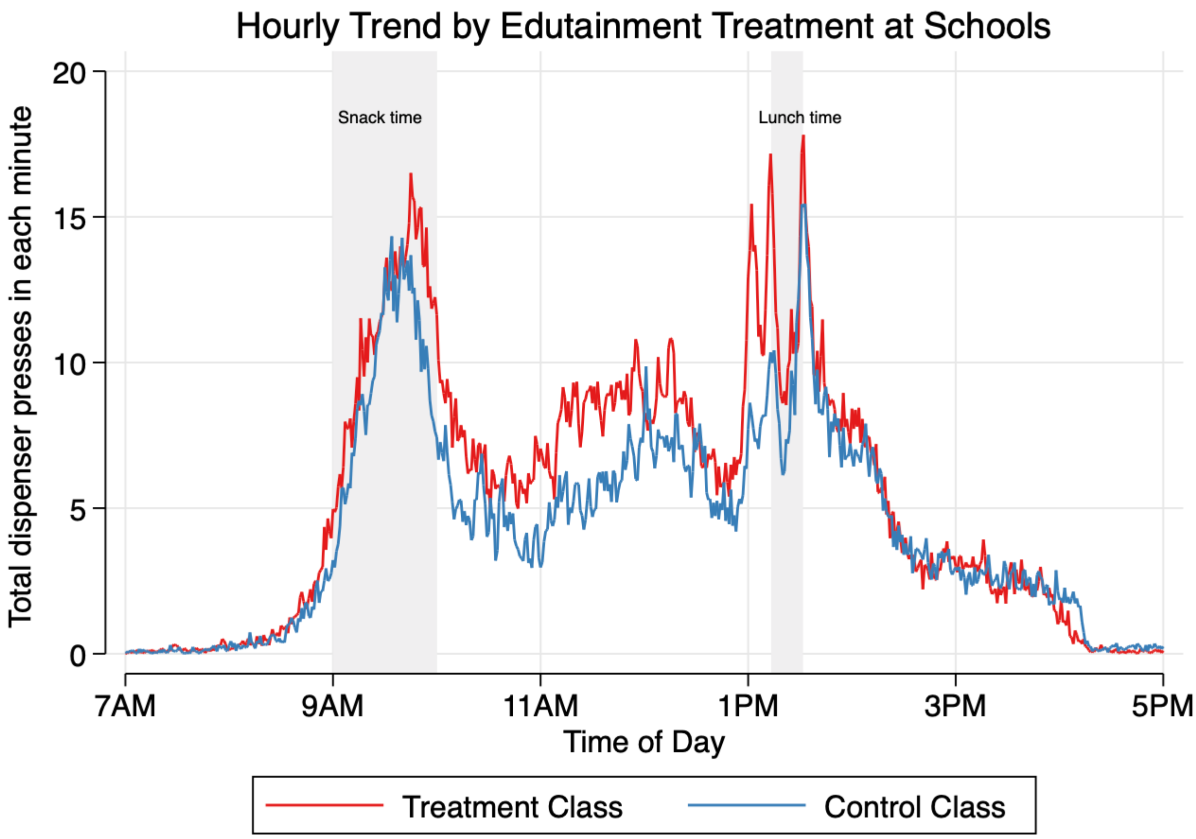


FIGURE II. Hourly trend of classroom dispenser use by edutainment treatment status

Notes. This figure plots the average number of soap dispenser presses in a class throughout a typical day in classrooms according to their edutainment treatment status. Note that only students in grades 3, 4, and 5 have lunch time, and lunch time is either at 1:15pm or 1:30pm depending on the school. Some students eat lunch at home and some at school. All students have snack time, mostly between 9:15am and 12:45pm.

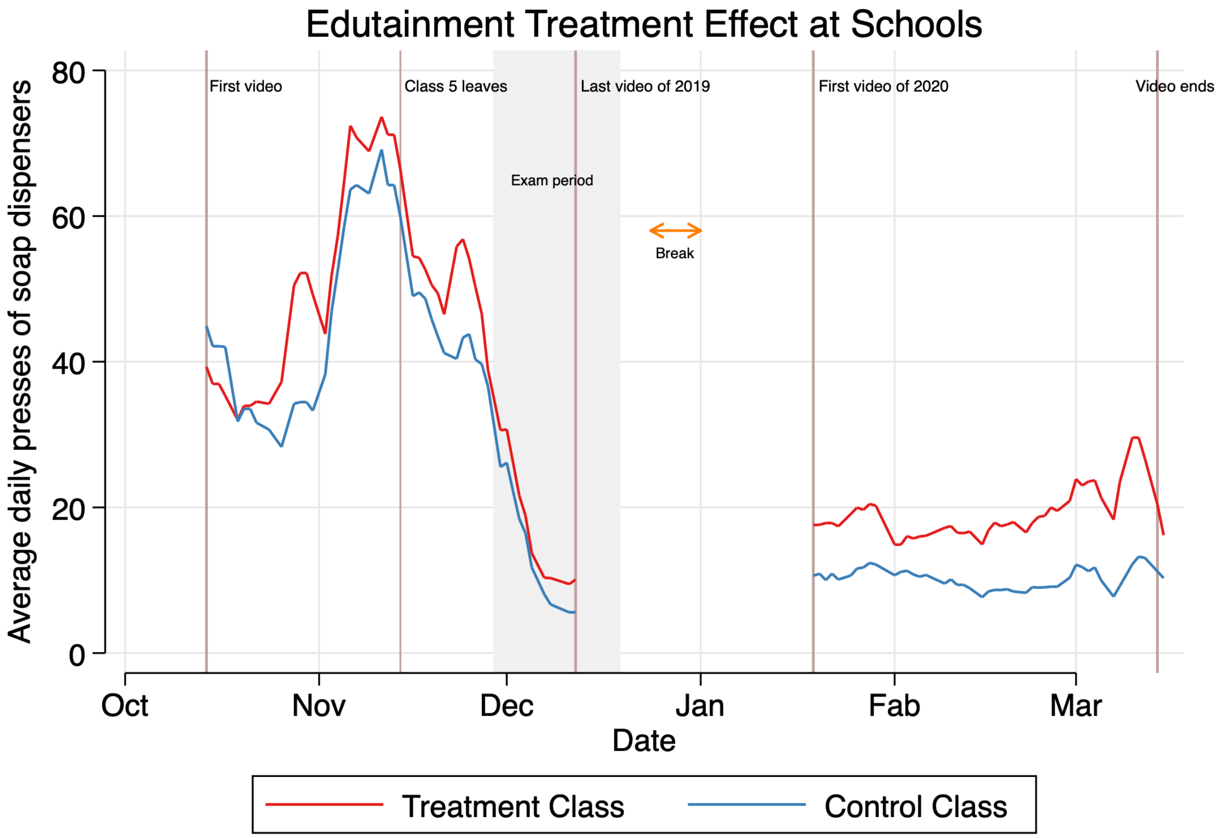


FIGURE III. Average classroom dispenser use by edutainment treatment status

Notes. This figure plots the moving average of total daily soap dispenser presses in classrooms over five days for 76 edutainment treatment classes and 74 edutainment control classes. Important events are marked. Dates range from Oct 12, 2019 to Mar 15, 2020 with non-school days (Fridays, national holidays, and winter break) dropped. Trends in the month of March should be interpreted with caution as our sample size is reduced to 46% of the initial dispenser data due to dispenser sensor failure in the field.

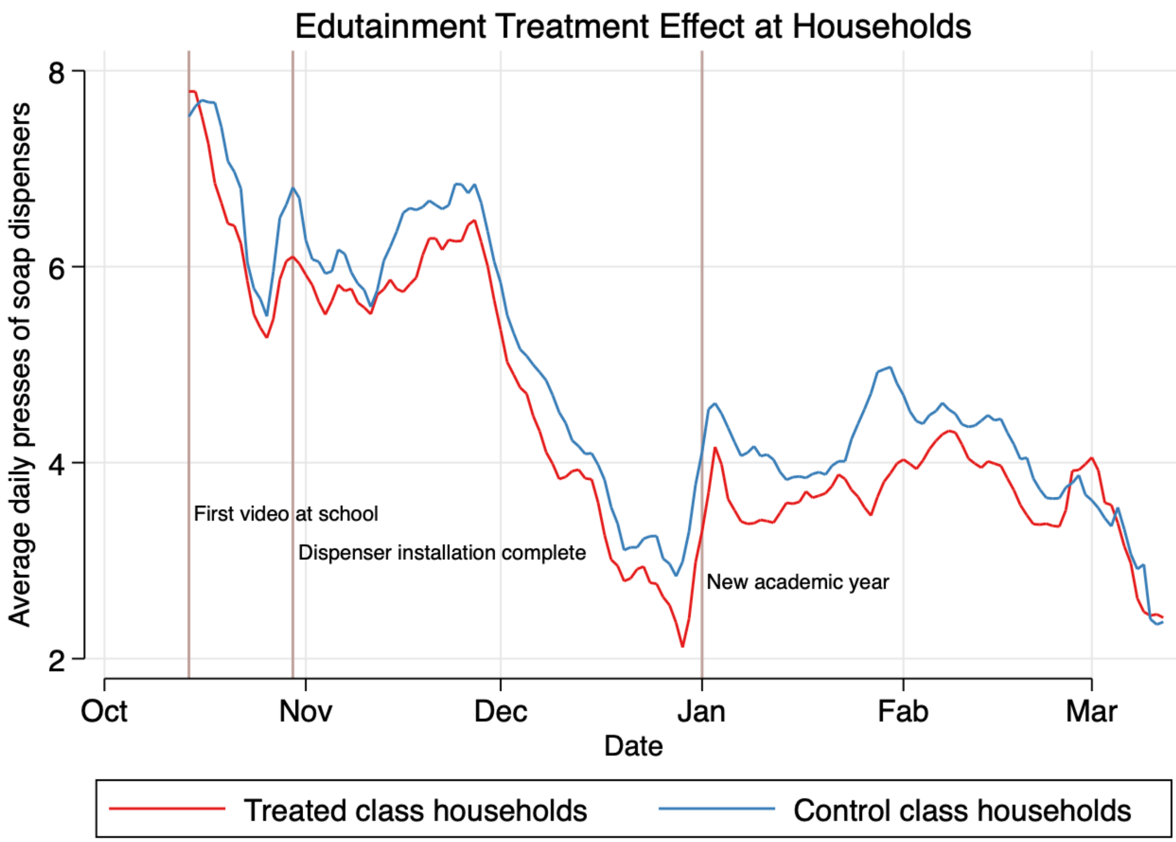


FIGURE IV. Average household dispenser use by edutainment treatment status

Notes. This figure plots the moving average of daily soap dispenser presses over five days in households by their edutainment treatment status. Dates range from Oct 10, 2019 to Mar 12, 2020. Household of students who were in class 5 in 2019, students who move out of our sample schools starting new academic year, and students who did not advance normally to a higher grade no longer appear in the data beginning on Jan 1st, 2020. Trends in the month of March should be interpreted with caution as our sample size is reduced to 46% of the initial dispenser data due to dispenser sensor failure in the field.

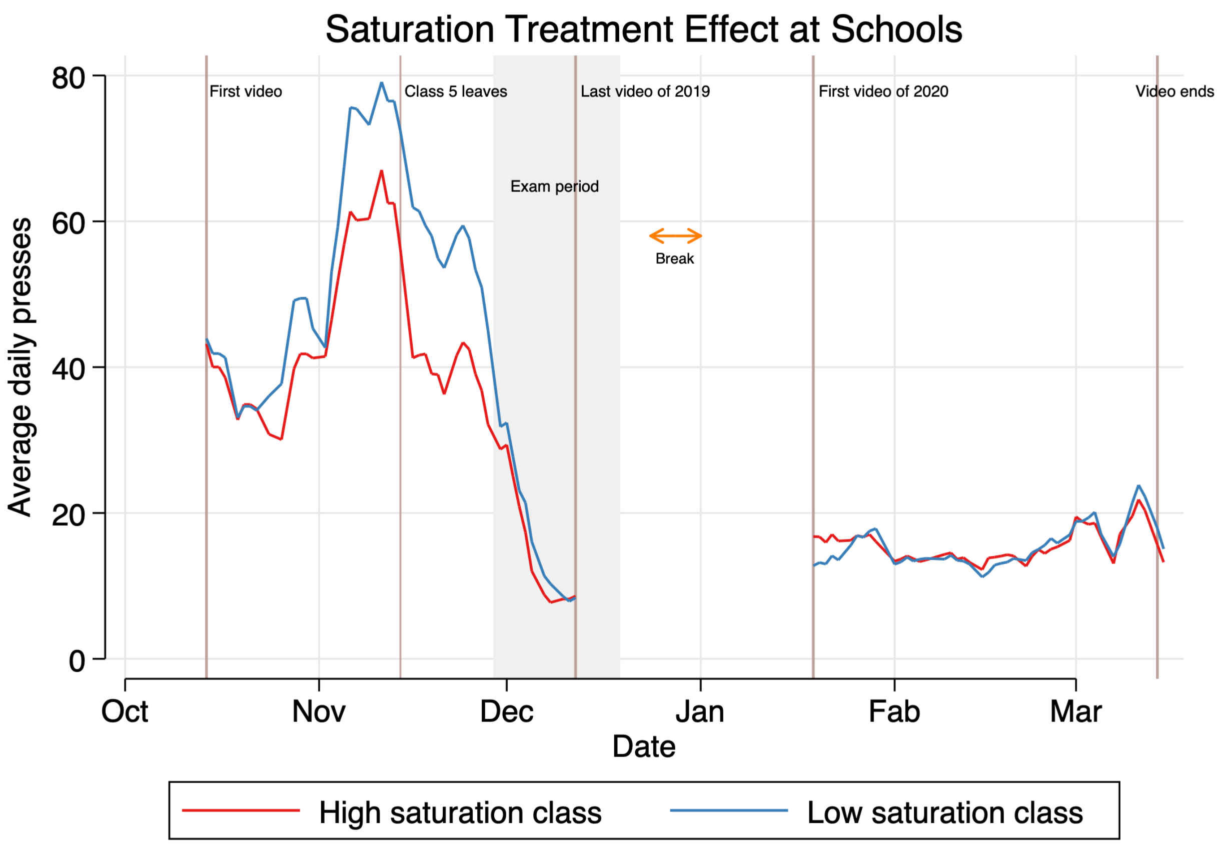


FIGURE V. Average classroom dispenser use by saturation status

Notes. This figure plots the moving average of number of soap dispenser presses over five days for 65 high saturation classes and 89 low saturation classes. Important events are marked. Dates range from Oct 12, 2019 to Mar 15, 2020 with non-school days (Fridays, national holidays, and winter break) dropped. Trends in the month of March should be interpreted with caution as our sample size is reduced to 46% of the initial dispenser data due to dispenser sensor failure in the field.

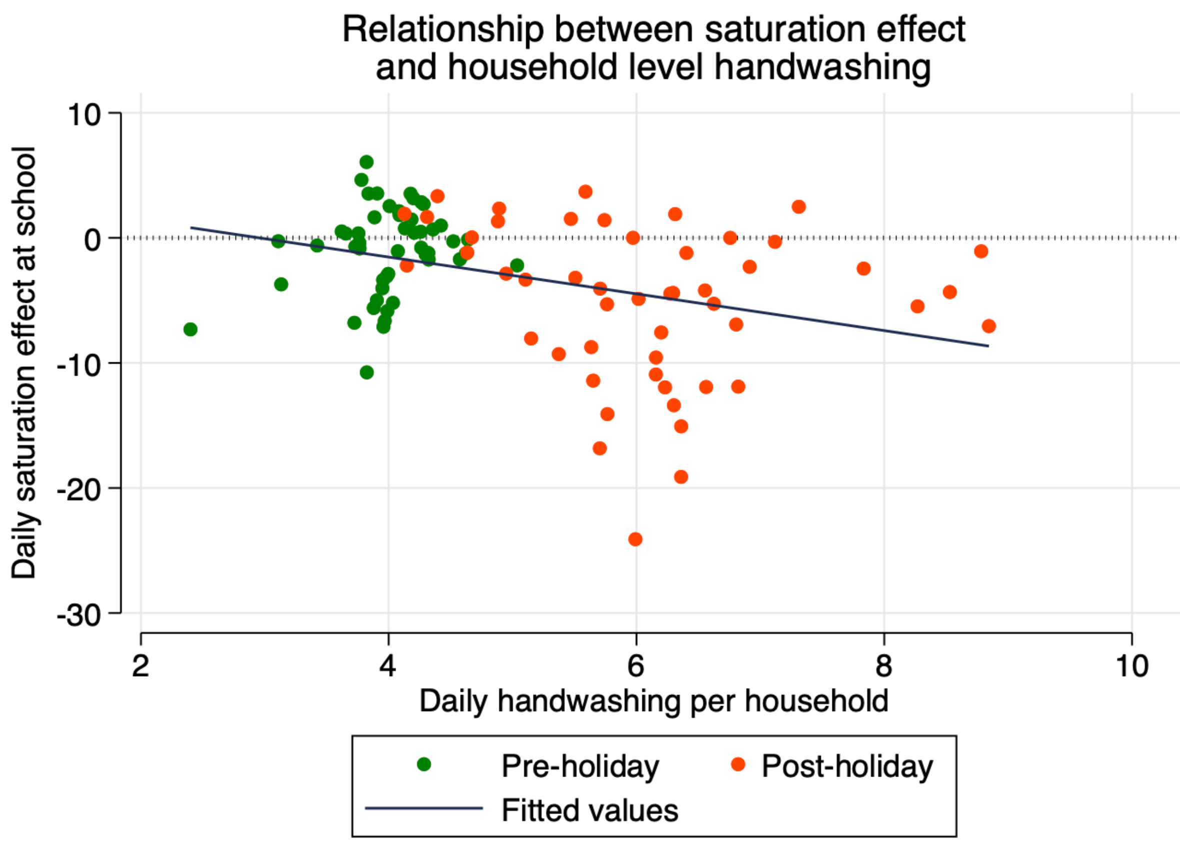


FIGURE VI. Relationship between saturation effect and household level handwashing

Notes. This figure plots the daily saturation effect at the school level against daily handwashing rate per household. Each observation corresponds to one day. Dates range from Oct 12, 2019 to Mar 15, 2020 with non-school days (Fridays, national holidays, and winter break) dropped. The linear fit has a negative slope of −1.47 and is significant at the 1% level.

A APPENDIX

Appendix Table I. Descriptive Statistics

	(1) Mean	(2) SD	(3) Obs
Class characteristics			
Grade	2.51	1.69	150
Female Teacher	0.71	0.46	150
Teacher's age	39.38	8.46	150
Teacher's education	13.17	1.06	150
Teacher's experience	14.94	9.67	150
Number of students	36.11	11.16	150
Household characteristics			
House ownership	0.99	0.10	756
Number of rooms	1.93	0.81	756
Distance to drinking water	2.12	1.24	756
Drinking water source is shallow tubewell	1.00	0.05	756
Doesn't treat drinking water	0.97	0.18	756
Has latrine	0.70	0.46	756
Number of family members	4.56	1.17	750
Agriculture occupation	0.28	0.45	756
Daily labor occupation	0.25	0.43	756
Islam	0.95	0.22	756
Mother's age at marriage	15.96	2.10	756
Mother's age	35.05	9.31	750
Mother is married	0.95	0.22	750
Mother's education	3.82	3.62	750
Age in months	9.02	1.93	756
Female	0.55	0.50	756
Months breastfed	25.99	4.67	756
Runny nose - past 2 weeks	2.59	2.15	756
Cough - past 2 weeks	1.34	1.82	756
Diarrhea - past 2 weeks	0.20	0.80	756
Can cold spread between people	0.22	0.41	251
Soap cleans germs from hands	0.70	0.46	236
Understands when to use soap	0.98	0.14	251
Need soap even when hands seem clean	0.70	0.46	251
Daily handwashing rate with soap	2.98	2.56	251
Wash with soap before cooking	0.17	0.37	251
Wash with soap before eating	0.24	0.43	251
House has soap	0.94	0.23	251
Children use soap before eating	0.27	0.45	251

Notes. Table reports descriptive statistics for the classroom and household samples at baseline. Unit of observation is class for class characteristics and households for the rest. Households whose baseline surveys were conducted after the first edutainment treatment at the schools was administered were excluded from the table for hygiene knowledge and behavior variables.

Appendix Table II. Balance for edutainment treatment

	(1) Control	(2) Treated	(3) P-val	(4) N
Class characteristics				
Female Teacher	0.689	0.723	0.627	150
Teacher's age	40.026	38.75	0.335	150
Teacher's education	13.114	13.217	0.442	150
Teacher's experience	15.460	14.434	0.480	150
Number of students	35.878	36.342	0.719	150
F-test				0.891
Household characteristics				
House ownership	0.983	0.997	0.065*	756
Number of rooms	1.910	1.946	0.520	756
Distance to drinking water	2.198	2.061	0.130	756
Drinking water source is shallow tubewell	0.998	0.997	0.849	756
Doesn't treat drinking water	0.974	0.962	0.138	756
Has latrine	0.669	0.725	0.022**	756
Number of family members	4.539	4.575	0.670	750
Agriculture occupation	0.292	0.266	0.435	756
Daily labor occupation	0.229	0.272	0.134	756
Islam	0.945	0.948	0.847	756
Mother's age at marriage	15.932	15.940	0.961	756
Mother's age	34.984	35.089	0.879	750
Mother is married	0.945	0.952	0.588	750
Mother's education	3.857	3.805	0.859	750
Female	0.551	0.536	0.688	756
Months breastfed	26.102	25.934	0.556	756
Runny nose - past 2 weeks	2.574	2.628	0.698	756
Cough - past 2 weeks	1.423	1.256	0.189	756
Diarrhea - past 2 weeks	0.157	0.238	0.177	756
Can cold spread between people	0.224	0.207	0.722	251
Soap cleans germs from hands	0.691	0.703	0.815	236
Understands when to use soap	0.966	0.991	0.282	251
Need soap even when hands seem clean	0.683	0.714	0.532	251
Daily handwashing rate w/ soap	3.259	2.752	0.132	251
Wash with soap before cooking	0.170	0.170	0.993	251
Wash with soap before eating	0.212	0.277	0.081*	251
House has soap	0.93	0.97	0.296	251
Children use soap before eating	0.263	0.286	0.527	251
F-test				0.125

Notes. This table reports FE-partialled out means of the edutainment control and treated groups (columns 1 and 2), p value of the differences in mean (column 3), and sample size (column 4). Unit of observation is class for class characteristics and households for the rest. Households whose baseline surveys were held after the first edutainment treatment at the schools was administered are excluded from the table for the hygiene knowledge and behavior outcomes. Standard errors are clustered at the class level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Appendix Table III. Balance for saturation treatment

	(1) Control	(2) Treated	(3) P-val	(4) N
Class characteristics				
Female Teacher	0.722	0.687	0.606	150
Teacher's age	39.399	39.354	0.972	150
Teacher's education	13.110	13.242	0.288	150
Teacher's experience	15.423	14.309	0.429	150
Number of students	37.031	34.913	0.074*	150
F test				0.234
Household characteristics				
House ownership	0.986	0.992	0.538	756
Number of rooms	1.968	1.918	0.458	756
Distance to drinking water	2.215	2.100	0.274	756
Drinking water source is shallow tubewell	0.993	0.998	0.448	756
Doesn't treat drinking water	0.984	0.963	0.052*	756
Has latrine	0.684	0.703	0.576	756
Number of family members	4.631	4.537	0.369	750
Agriculture occupation	0.319	0.267	0.199	756
Daily labor occupation	0.271	0.247	0.516	756
Islam	0.958	0.943	0.417	756
Mother's age at marriage	15.685	16.009	0.073*	756
Mother's age	34.564	35.180	0.412	750
Mother is married	0.965	0.944	0.201	750
Mother's education	4.062	3.761	0.377	750
Female	0.575	0.533	0.339	756
Months breastfed	25.892	26.049	0.648	756
Runny nose - past 2 weeks	2.351	2.676	0.096	756
Cough - past 2 weeks	1.270	1.353	0.649	756
Diarrhea - past 2 weeks	0.142	0.216	0.186	756
Can cold spread between people	0.261	0.201	0.276	251
Soap cleans germs from hands	0.701	0.696	0.925	236
Understands when to use soap	0.974	0.981	0.752	251
Need soap even when hands seem clean	0.715	0.696	0.734	251
Daily handwashing rate w/ soap	2.520	3.126	0.072*	251
Wash with soap before cooking	0.105	0.190	0.038**	251
Wash with soap before eating	0.233	0.251	0.678	251
House has soap	0.945	0.953	0.826	251
Children use soap before eating	0.240	0.286	0.355	251
F test				0.050

Notes. This table reports FE-partialled out means of the saturation control and treated groups (columns 1 and 2), p value of the differences in mean (column 3), and sample size (column 4). Unit of observation is class for class characteristics and households for the rest. Households whose baseline surveys were held after the first edutainment treatment at the schools was administered are excluded from the table for the hygiene knowledge and behavior outcomes. Standard errors are clustered at the class level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Appendix Table IV. Edutainment and saturation treatment effect at school by phase

	(1) Phase 1	(2) Phase 2
Edutainment treatment	7.548***	10.577***
	(1.642)	(0.695)
Saturation treatment	-6.250***	-1.320**
	(1.469)	(0.658)
Edutainment control mean	37.510	9.970
	[49.775]	[13.908]
Saturation control mean	44.133	14.595
	[56.601]	[19.463]

Notes. Dependent variable is total number of soap dispenser presses in each class. The unit of observation is class by day. Dates range Oct 12th – Dec 14, 2019 for column (1) and Jan 15th - Mar 15th, 2020 for column (2) (total of 22 weeks, with 5 weeks of vacancy between phase 1 and phase 2 and Fridays, national holidays, and winter break dropped). We use a cross-fit partialing-out estimator (Chernozhukov et al. 2018) to select from among potential class control variables shown in Appendix Table I. All include date, school, and grade level FE. Robust standard errors clustered at the class level are reported in parentheses, and standard deviations are reported in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Appendix Table V. Edutainment treatment effect at households by phase

	(1) Phase 1	(2) Phase 2
Edutainment treatment	-0.385***	-0.486***
	(0.115)	(0.116)
Control mean	5.743	4.335
	[7.854]	[5.655]
Observations	53,330	33,280

Notes. Dependent variable is total number of soap dispenser presses in each household. The unit of observation is household by day. Dates range Oct 12th - Dec 31st, 2019 for column (1) and Jan 1st - Mar 12th, 2020 for column (2). We control for the saturation treatment status of the dispenser child's class in every model. We use a cross-fit partialing-out estimator (Chernozhukov et al. 2018) to select from among potential house characteristics controls shown in Appendix Table I. All include date, school, and age FE. Robust standard errors clustered at the class level are reported in parentheses, and standard deviations are reported in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Appendix Table VI. Hourly edutainment treatment effect at households

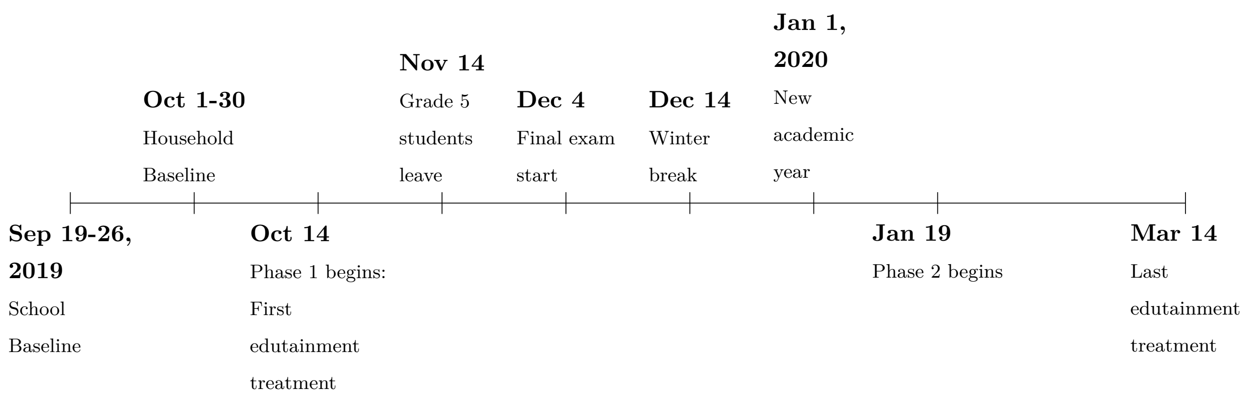
	Panel A: School days				Non school days
	(1)	(2)	(3)	(4)	(5)
	All day	Before school	At school	After school	All day
Edutainment treatment	-0.020***	-0.006	-0.023***	-0.024***	-0.028***
	(0.006)	(0.010)	(0.008)	(0.006)	(0.008)
Control Mean	0.305	0.408	0.290	0.260	0.314
	[0.398]	[0.828]	[0.678]	[0.434]	[0.492]
Observations	68,141	68,141	68,141	68,141	18,469

Notes. Dependent variable is total number of soap dispenser presses in the household during the time of the day specified, divided by the number of hours within that time period. The unit of observation is household by day. Dates range Oct 12th, 2019 - Mar 12th, 2020. All columns control for saturation treatment status of the dispenser child's class and use a cross-fit partialing out estimator (Chernozhukov et al. 2018) to select from among potential household controls shown in Appendix Table I. All include school, date, and age fixed effects. Robust standard errors clustered at the class level are reported in parentheses and standard deviations are reported in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Appendix Table VI. Edutainment treatment effect at school by grade

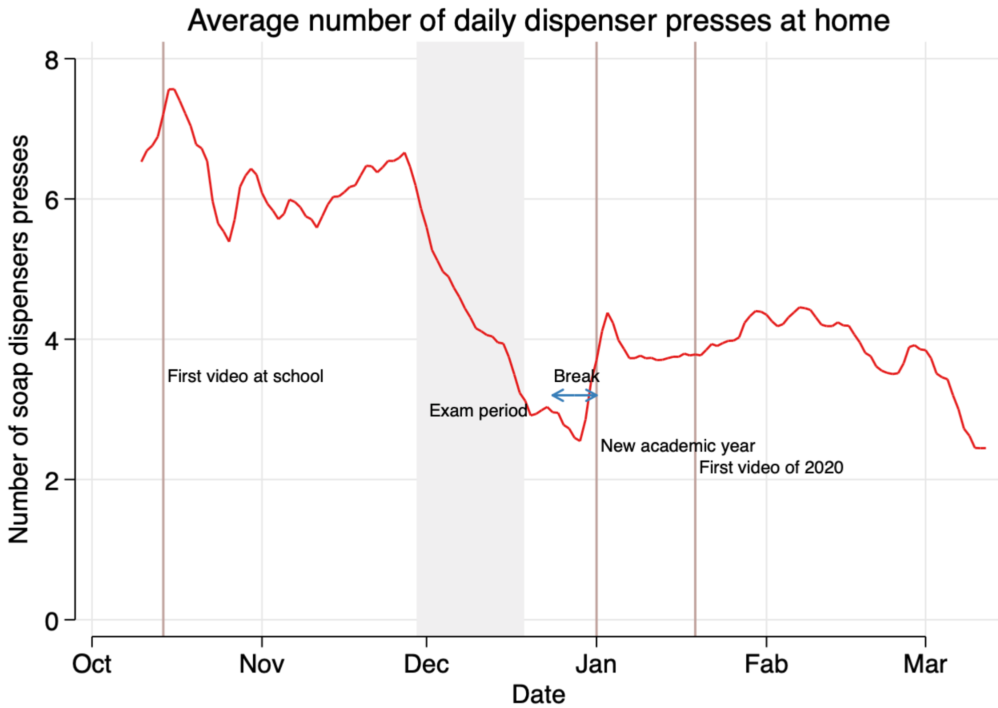
	(1) Grade 0	(2) Grade 1	(3) Grade 2	(4) Grade 3	(5) Grade 4	(6) Grade 5
Edutainment treatment	0.274	0.231*	0.390***	-0.020	0.159	0.251
	(0.188)	(0.127)	(0.116)	(0.095)	(0.138)	(0.311)
Control mean	0.798	0.657	0.552	0.756	0.790	1.287
	[1.501]	[1.037]	[0.943]	[1.137]	[1.108]	[1.945]
Observations	2066	2279	2324	2450	2457	851

Notes. Outcome is daily dispenser presses in classrooms divided by the number of students. All models control for the saturation treatment status of the class and use post-double-selection methodology (Belloni, Chernozhukov, and Hansen 2014) to select from available control variables. All include date FE. Robust standard errors clustered at the class level are reported in parentheses and standard deviations are reported in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$



Appendix Figure I: Timeline of experiment and school events

Notes. This figure shows the timeline of the experiment. Note that the household baseline survey extended beyond the first date of the edutainment treatment due to logistical constraints. This is reflected in our baseline balance table, where the sample is restricted to households that were surveyed before the start of the edutainment treatment for hygienic behavioral and knowledge questions.



Appendix Figure II: Average daily handwashing rate at home

Notes. This figure plots the three day moving average of handwashing rate at the household level throughout the course of the experiment. Dates range Oct 12th, 2019 - Mar 12th, 2020.

Appendix Figure III. Deviation from the pre-analysis plan

Experimental design:

- We add an additional thirty second video to the edutainment intervention at the beginning of the new school year ("Phase 2"), in which one of the authors instructs the students to wash their hands both at the school and home.

Data collection:

- The baseline survey was completed several weeks after the edutainment treatment was launched in the schools due to logistical complications in the field which delayed the administration of the baseline survey to households (but not to schools). Our balance table therefore reflects all sample household baseline values for those outcomes which were plausibly not affected by the treatment (eg. demographic characteristics), but restricts the sample to those households which were surveyed prior to the edutainment intervention launch for those outcomes (hygiene knowledge and behavior) which may have been impacted by the treatment.
- The endline survey was collected via phone, instead of in-person, due to the COVID lockdowns. We were also unable to collect school-level endline data as schools were shut down during this time.
- Because we could not visit in person, we were unable to collect the anthropometric measures of the children at endline, which was prespecified as a primary outcome of interest.

Analysis

- We now control for both treatment statuses of a child in our regression (edutainment treatment and saturation). Our prespecified regression included only the treatment of interest.
- We employ an improved method for control variable selection which we were unaware of when composing the preanalysis plan, the cross-fit partialling-out estimator of Chernozhukov et al. (2018).
- We employ age, rather than grade-level ('class') fixed effects for all individual-level regressions, as this improves our precision. All results are robust to including grade-level rather than age fixed effects as prespecified.
- We do not examine results for the 'conditional household handwashing rate,' one of our prespecified outcome measures in which we divide the outcome of the number of presses by the number of household members. This was done for simplicity of exposition, and we can present these results upon request.