

# Experiential and Social Learning\*

Agha Ali Akram, Gabriella Fleischman, Reshmaan Hussam, and Akib Khan †

January 19, 2026

## Abstract

Behavioral change can arise from learning through personal experience or learning from others, but how do these forms of learning interact? We conduct a field experiment on household water chlorination in Pakistan, where a randomized group learns through experience by tracking their children’s diarrhea before and after chlorine distribution. Learning-arm households with learning-arm neighbors chlorinate their water at a significantly higher rate for one year after the learning intervention. Their children’s health improves by 0.10 SD relative to all other households receiving chlorine. Neither learning households without learning-arm neighbors, nor non-learning households with learning-arm neighbors, exhibit sustained behavioral change.

## 1 Introduction

Learning as a shared experience is common, whether socialized through sharing new activities together or institutionalized through education in the classroom. Yet the economics literature typically considers the diffusion of information from one person to another as distinct from individual learning, ignoring the possibility of “joint learning,” or the complementarity between learning from one’s own experience and what one learns from others. This paper asks: What is the role of joint learning in the decision to adopt new technologies?

We study the role of joint learning in the adoption of a health technology among 1800 caregivers of children in the peri-urban slums of Karachi, Pakistan. We conduct a randomized controlled trial that has four arms: control households, who receive no intervention; chlorine households,

---

\*We are thankful for useful comments and suggestions from Marcella Alsan, Lori Beaman, Jon Denton-Schneider, Eliana La Ferrara, Mushfiq Mobarak, Martina Björkman Nyqvist, Frank Schilbach, Anna Tompsett, Zachary Wagner, and seminar participants at Network Science in Economics Conference 2025, PacDev 2025, EAYE 2025, Y-RISE 2024, MISUM/Stockholm School of Economics, NEUDC 2024, and AFE 2023. Fatimeh Munawar, Syed Ali Rehan, and Ibadullah Channa provided invaluable research assistance. We gratefully acknowledge financial support from the United States Agency for International Development (USAID) and the World Bank. Special thanks to Jed Friedman, who played a critical role at the beginning of this project. This project would not have been possible without Interactive Research and Development (IRD) Pakistan. The contents are the authors’ sole responsibility and do not necessarily reflect the views of USAID or the United States Government. The experiment was approved by IRD-IRB under IRD-IRB-2019-12-009 and was pre-registered under RCT ID AEARCTR-0003673.

†Akram: [agha.ali.akram@gmail.com](mailto:agha.ali.akram@gmail.com); Fleischman: Harvard Kennedy School, [g.fleischman@g.harvard.edu](mailto:g.fleischman@g.harvard.edu); Hussam: Harvard Business School, [rhusam@hbs.edu](mailto:rhusam@hbs.edu); Khan: MISUM, Stockholm School of Economics, [akib.khan@hhs.se](mailto:akib.khan@hhs.se).

who receive free chlorine tablets for water purification; incentivized households, who receive free chlorine tablets and small daily financial incentives for chlorine use; and learning households, who receive free chlorine tablets and a visual tool to track their household-specific health returns to chlorine use. Incentives and the learning tool are withdrawn after three to six months, but we continue providing chlorine tablets and tracking chlorine in drinking water for over twelve months following their withdrawal.

The educational tool we employ in our learning arm builds on the work of [Akram and Mendelsohn \(2021\)](#) (hereafter, AM). AM design a visual aid (hereafter referred to as the Info-Tool) that plots the health returns to water treatment by utilizing household-specific data on the frequency of child diarrhea, providing a community-specific child diarrhea benchmark, and tracking multiple observations over time per household before and after chlorine tablet provision. Figures [G.1](#) and [G.2](#) depict this Info-Tool, which we provide to all learning-arm households for three months prior to and three months following free chlorine distribution.<sup>1</sup>

To identify the *individual experiential learning* treatment effect, we compare the behavior and health of learning-arm households who are not proximate to other learning-arm households against those in the free chlorine arm. We then test for information diffusion through *social learning* by measuring the spillover effect of being a non-learning household but having learning-arm neighbors (i.e., the cross-treatment spillover effect). We test for a *complementarity* between experiential and social learning by measuring the spillover effect of being a learning household with learning-arm neighbors (i.e., the within-treatment spillover effect) and comparing this with the sum of the previous two estimates.

Our remaining treatment arms enable us to disentangle mechanisms and compare our estimates to policy-relevant benchmarks. Incentive provision and information provision may both raise the consumption stock of chlorine use in the short run, but only the latter can alter the perceived health returns to use; the incentives arm therefore enables us to disentangle the mechanism of habit formation from that of learning. The chlorine-only arm provides a measure of the degree to which access to chlorine is a barrier to use, while the pure control arm enables us to measure the impact of chlorine alone, an important policy benchmark against which to interpret the value of the learning intervention.

We find that, on average, all three interventions (chlorine, incentives, and learning) significantly increase chlorine presence in drinking water for the fourteen month duration over which we provide chlorine tablets, relative to households who did not receive free chlorine. While incentive households chlorinate more than learning households during the period of incentive and Info-Tool provision (the “short run”), learning households outperform incentive households

---

<sup>1</sup>Community health workers (CHWs) present households with the community-specific benchmark rate twice each month for six months: three months prior to chlorine tablet provision and three months following provision. Caregivers are asked to color in the blocks according to frequency of their children’s loose stool themselves, but CHWs also assist if needed during their bi-weekly visits (which they make to all sample households). Designed through extensive piloting both in AM and for the present study, the tool is easily usable and comprehensible to the low-literacy and low-numeracy households in our sample.

thereafter, with child diarrhea rates exhibiting the largest drop for learning households following the withdrawal of interventions (the “long run”).

These average effects mask important heterogeneity that speaks directly to the role of joint learning. Chlorination among households in the learning arm who have no neighbor in the learning arm, who only experience individual learning, converges to the behavior of chlorine-only households immediately after the Info-Tool is withdrawn. Households who are not in the learning arm but have a learning-arm neighbor, who only experience social learning, never chlorinate more than chlorine-only households without learning arm neighbors. In contrast, among learning arm participants who *do* have a neighbor in the learning arm, chlorine use remains stable and significantly higher ( $p = 0.006$ ) than their counterparts over the course of the study. In our context, experiential and social learning *rely* on each other. Individual learning does not persist unless it is reinforced through neighbors’ experiences, and caregivers cannot learn from their neighbors’ experiences unless they have first gone through the same learning experience themselves.

The consequences of this complementarity between experiential and social learning are substantial. One year after the withdrawal of the interventions, children in the learning arm with neighbors in the learning arm show the largest improvements in health. Among households in the chlorine-only group without learning-arm neighbors, the intent-to-treat (ITT) estimate of the treatment on an index of child anthropometric measures is a 0.10 SD ( $p < 0.05$ ) increase relative to the pure control. This is statistically equivalent to the 0.08 SD ( $p < 0.10$ ) increase in child health for learning-arm participants who do not have a learning-arm neighbor and the 0.04 SD increase in child health for non-learning-arm participants with a learning neighbor. In contrast, learning-arm households with a learning-arm neighbor experience a 0.16 SD ( $p < 0.01$ ) increase in child health relative to the pure control after one year. When we pool all other households who receive free chlorine to maximize power and identify the policy-relevant effect, we find that these learning-arm households with learning-arm neighbors exhibit a 0.10 SD ( $p < 0.05$ ) higher treatment effect in child health, doubling the impact of free chlorine distribution on child health.

Beyond the direct public health implications, this phenomenon of joint learning has significant implications for how research design choices can shape treatment effect estimates: joint learning implies that average treatment effects increase with treatment saturation. We can get a sense of how much joint learning may impact treatment effect estimates by comparing our results with those from [Akram and Mendelsohn \(2021\)](#): while we randomize our interventions at the household level, AM cluster-randomize their learning intervention at the neighborhood-block level with full treatment saturation in villages comparable to those we operate in. Fifteen months after withdrawing their Info-Tool treatment, AM finds a 197% increase in the rate of chlorine detection in Info-Tool households’ water relative to households who received free chlorine alone. In contrast, our long run estimates show *no* effects of individual learning alone,<sup>2</sup>

---

<sup>2</sup>Our long run individual learning estimate is a 4% increase in water chlorination that is not statistically

suggesting that estimates of learning in fully saturated cluster-randomized trials risk conflating the effects of joint learning with those of individual learning, and individually-randomized trials risk underestimating the impact of the policy if it were to be fully saturated in a community.

To discipline our exploration of the learning process that learning households may be engaging in, we outline a model of experiential learning. Participants follow a Bayesian learning process when forming their beliefs about the efficacy of a health technology. However, unlike standard models, individuals weigh signals with weights that depend on experience with the signal-generating technology when choosing to adopt the technology. In the context of our experiment, the learning intervention provides caregivers with firsthand experience in acquiring information through the Info-Tool. By engaging with the Info-Tool themselves, these participants become more responsive to any signal generated through the Info-Tool, including those generated by their neighbors.

If behavioral changes are driven by the weights assigned to signals in posterior beliefs, then adoption should be heterogeneous by the sign of the signals that participants observe. To test this mechanism, we use a machine learning algorithm on a host of baseline variables to identify households that are predicted to experience health improvements during the intervention period. These households are ex-ante more likely to acquire positive signals about chlorine efficacy while using the Info-Tool and should be more likely to alter their behavior. Indeed, we find that, over the three months that learning households are receiving chlorine tablets and the Info-Tool, the effects of the learning intervention on chlorination are entirely explained by households who are predicted to improve ( $p = 0.041$ ), suggesting that participants indeed respond to the information that they observe in their environment.<sup>3</sup>

Next, we check whether learning-arm participants are responding to their learning-arm *neighbors'* health signals. Similarly, we find that the within-treatment spillover effect is entirely explained by learning-arm neighbors whose diarrhea rates were ex-ante predicted to improve. Learning-arm participants do not respond to learning-arm neighbors whose health was not predicted to improve, nor do they respond to non-learning-arm neighbors whose health was predicted to improve. This bolsters our assertion that the spillover is driven by higher weights placed on learning-arm neighbors' health signals, rather than proximity to people with positive attributes that are correlated with health improvements.

Finally, we rule out a series of competing mechanisms for our results. Using the incentives arm, we are able to rule out that behavior change is driven by habit formation through the accumulation of consumption (chlorination) stock, a competing mechanism to learning-through-adoption that is often conflated with the effects of learning in the literature (Caro-Burnett et al., 2021).<sup>4</sup> We document that participants in the incentives arm use chlorine tablets at higher rates in the short-term, thereby building up a greater “consumption stock” of chlorination. However, they

---

distinguishable from that of chlorine-only households.

<sup>3</sup>This effect continues but diminishes over the course of the fourteen months, likely because the other Info-Tool households are also learning from their neighbors over time.

<sup>4</sup>See Section K.2 for a detailed review of the existing literature.

converge to the chlorination rates of chlorine-only households immediately following the withdrawal of incentives, suggesting that water chlorination in our context is not a habit-forming activity. As detailed in the paper, we are able to rule out a variety of other competing mechanisms as explanations for the social learning we observe, including changes in beliefs about the expected returns to water chlorination, social network formation, differential communication, mimicry, and evolving social norms.

Our study makes three contributions. First, we build upon the empirical and theoretical literature on technology adoption and behavior change. We identify what, to our knowledge, is both the first theoretical formulation and empirical evidence that information acquired from individual experience and from social learning may interact *multiplicatively* to shape behavior.<sup>5</sup> A large body of literature identifies social learning—learning from other’s information or experiences, which an individual did not herself learn from or partake in—as an important driver of technology adoption and behavioral change (Arrow, 1962; Beaman et al., 2021; Bikhchandani et al., 2024; Breza and Chandrasekhar, 2019; Conley and Udry, 2010; de Janvry et al., 2016; Dupas, 2014; Foster and Rosenzweig, 1994; Khandelwal, 2024; Kondylis et al., 2023). The questions that this literature raises are primarily geared toward understanding how to disseminate information broadly and cost-effectively to uninformed individuals. A growing body of work demonstrates that social learning is limited when agents are unwilling to share information, receive information from certain people, or internalize information acquired by others (BenYishay and Mobarak, 2019; BenYishay et al., 2020; Banerjee et al., 2024; Chandrasekhar et al., 2022; Conlon et al., 2022). We provide novel evidence that, when social learning is predicated on personal experience – for example, if social learning can effectively confirm beliefs established from personal observation, but is ineffective at conveying novel information – social learning from an information intervention will *only* arise as a complement to individual learning.

Second, we speak to the literature on the design of randomized controlled trials (Athey and Imbens, 2017; Duflo et al., 2007). Our findings demonstrate the significant implications of interpreting estimates of treatment effects from cluster-randomized designs as purely individual treatment effects. Cluster-randomized designs with full treatment saturation are often motivated by the desire to eliminate cross-treatment spillovers and thereby identify individual treatment effects. However, in the absence of random variation in exposure to other treated units among treated units themselves, within-treatment spillovers are not identified and the threat of SUTVA violation remains (Hudgens and Halloran, 2008). As such, estimates from cluster-randomized designs with full treatment saturation conflate individual treatment effects with within-treatment spillovers. Extrapolating these estimates to other settings (for example, settings with sparse treatment implementation that may not generate the same within-treatment

---

<sup>5</sup>In a model of simple contagion, information that is learned once cannot be enhanced (although it can be forgotten and relearned). In a model of complex contagion, information learned from multiple sources can be more influential than information from a single source, but this model does not differentiate between learning from multiple people or learning from the self. We show that both *who* information comes from and *the order* in which one receives information from different sources can be critical. In other words, both simple and complex contagion models are consistent with our results, but *only among Info-Tool participants*: social learning becomes relevant to one’s behavior only upon experiencing personal learning.

spillovers) can overestimate treatment effects.

Our study enables us to distinguish within-treatment spillover effects from individual treatment effects and compare these estimates with those of a cluster-randomized test of the same intervention. Comparing our results with those of [Akram and Mendelsohn \(2021\)](#)—which, aside from its level of randomization (clustered vs. our individual-level design), implements an intervention identical to our learning arm—we find that their estimate, which conflates individual learning with joint learning and maximizes the potential for joint learning through full treatment saturation, is staggeringly large. Consistent with these large effects, our estimates of joint learning treatment effects are at least six times larger than those of individual learning alone. Notably, technology adoption and communication are subjects of empirical economics research whose experimental designs are more often cluster-randomized with full treatment saturation *than any other experimental design*, thereby concealing within-treatment spillovers.<sup>6</sup>

Third, these lessons on research design in turn have important implications for policy. When an intervention’s success relies on within-treatment spillovers rather than individual treatment effects, achieving efficacy, even at the individual level, requires a sufficient degree of treatment saturation. In other words, the size of the within-treatment spillovers of an intervention has direct implications for how a social planner administers a policy. Existing literature on employing social networks for information dissemination has focused on seeding information ([Banerjee et al., 2019](#); [Beaman et al., 2021](#); [Chandrasekhar et al., 2022](#)). This literature tests methods through which broader or stronger network reach from a seed can strengthen intervention effects, but often omits the value of the experiences of the person receiving the information. Our findings suggest that, for some types of learning, these assumptions could seriously undermine the potential for individuals to learn from disseminated information. In our setting, personal experience is essential to acting on information acquired by others. *Own experience cannot be diffused through seeding*, suggesting that there may be large efficacy costs to seeding, rather than saturating, certain types of information.

The rest of the paper proceeds as follows: Section 2 provides the details of the experimental design, sample, and data; Section 3 presents a model of complementarities in experiential and social learning; Section 4 presents overall results on chlorine use, and spillover results on chlorine use; Section 5 discusses mechanisms; Section 4.3 presents results on child health; and Section 6 concludes.

## 2 Experimental Design

We conduct a randomized controlled trial in Ibrahim Hyderi, a peri-urban neighborhood of Karachi, Pakistan. We identify 1,800 eligible households through a census. Households are eligible if there is at least one child between the ages of six months and five years old at baseline, if the caregiver (typically the mother) is generally home during the day, and several dimensions

---

<sup>6</sup>See Section J for an analysis of trials registered with the American Economics Association RCT Registry.

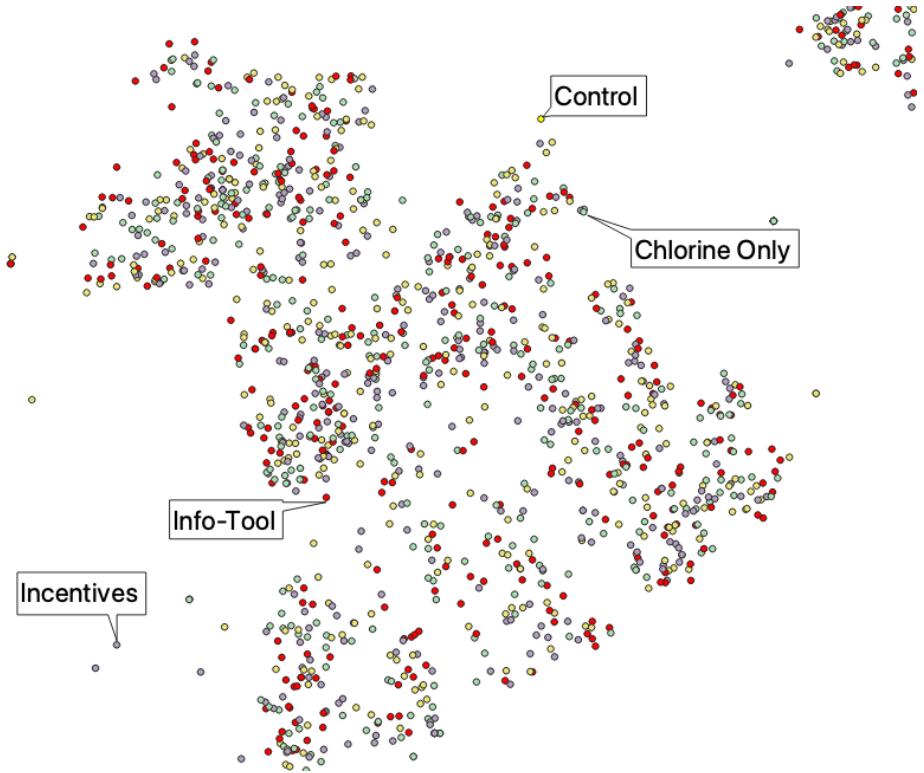


Figure 1: Distribution of Households Across Treatment Arms

of water usage to ensure that chlorine is useful and effective for improving children’s health.<sup>7</sup> 79% of households drink centrally-delivered water that is piped into their own household or plot at baseline. By endline, 70% of households have changed their water delivery method (non-differential by treatment group), with 56% of households now receiving water from a public tap or standpipe and 32% from water piped into the household.

## 2.1 Baseline Survey, Randomization, and Balance

Our baseline survey ran from May to June of 2022. We collect data on chlorine exposure, chlorine knowledge, diarrhea prevalence, and child anthropometrics.<sup>8</sup> As expected, there is not a single household whose water vessel tests positive for chlorine residual at baseline, with 98% of households reporting that chlorine tablets are not available in local markets. Awareness of chlorine tablets in this community is likewise low at baseline: 20% of the sample report having heard of chlorine tablets, but 40% of this subsample cannot properly identify what chlorine tablets are used for. Despite low awareness, 87% percent report that they are open to using chlorine tablets after enumerators explain the purpose of chlorine for purifying water.

We electronically randomize households to treatment groups between the baseline visit and the first biweekly visit. Eligible households are individually randomized into the four 450-person experimental arms as follows:<sup>9</sup>

---

<sup>7</sup>See Appendix Section A for more details on screening criteria.

<sup>8</sup>See Appendix Section A for more details on how we measure self-reported diarrhea prevalence and child anthropometrics.

<sup>9</sup>Treatment groups are balanced on most, but not all, baseline measures. In Tables B.1 and B.2, we compare

*Control (C): No chlorine tablets (Pure control)*

*Treatment 1 (T1): Free chlorine tablets (Chlorine only)*

*Treatment 2 (T2): Free chlorine tablets + Info-Tool (Info-Tool)*

*Treatment 3 (T3): Free chlorine tablets + financial incentive (Incentives)*

		Phase 1		Phase 2		
		Round 1	Round 2			
		Month 1	Pre-Treatment: Months 2-4	Treatment: Months 5-7	Post-Treatment: Months 8-18	
Chlorine	Baseline Survey			Chlorine Distribution	Chlorine Distribution	
Incentives				Chlorine Distribution + Incentives		Endline Survey 1
Info-Tool		Info-tool Training		Chlorine Distribution + Info-tool		Endline Survey 2
Control						
		Twice-monthly visits for child health data	Twice-monthly visits for child health and chlorine presence data	Once-monthly visits for child health and chlorine presence data		

Figure 2: Study Timeline

## 2.2 Phase 1: Biweekly Visits

Phase 1, during which we introduce the behavioral interventions and visit households every two weeks, consists of two rounds. In Round 1, households are made aware of their treatment status and T2 households are trained on how to use the Info-Tool. In Round 2, we distribute chlorine tablets to all three treatment groups and deliver the incentives treatment to T3 households, while T2 households continue to use the Info-Tool. Figure 2 lays out the study timeline and key events. We describe in greater detail the activities of each round below.

### 2.2.1 Round 1 of Phase 1

Throughout Round 1 of Phase 1 (June to August 2022), surveyors visit households every two weeks for a short round of data collection. These surveys are standardized and include

each treatment group with the pure control group. We control for unbalanced variables in all of our empirical specifications. Children from T1 are reported to have fewer diarrhea days than the control group both when considering a binary measure for whether or not any child in the household had diarrhea in the past fourteen days (Table B.1), or when considering both a binary measure and the number of diarrhea-days at the child level (Table B.2). However, there are no significant differences in child anthropometrics, nor do we see lower levels of diarrhea among T1 households in the following visit (before households had received any treatment interventions), indicating these differences are likely spurious.

measuring child diarrhea days over the past two weeks, testing for the presence of chlorine residual, and training caregivers in the T2 group on how to use the Info-Tool chart.<sup>10</sup><sup>11</sup>

In the first Round 1 visit, we reveal treatment status to caregivers. We explain each treatment group to caregivers and inform them that a lottery will determine their treatment status. We explain that the lottery will run through the enumerator's tablet, with the number that appears on the screen, which was pre-assigned to properly stratify by neighborhood block, determining their treatment status.

#### *Info-Tool*

Upon completing the first Round 1 survey, caregivers randomized to T2 receive the Info-Tool.<sup>12</sup> The Info-Tool chart is a simple pen-and-paper chart that allows caregivers to track their children's diarrhea (Figure G.1). Each chart consists of two bars to represent each month: one bar in which caregivers fill in a square for each child-day with diarrhea (for example, if two children had diarrhea on the same day, the caregiver would fill in two boxes), and one bar in which the enumerator fills in a benchmark diarrhea rate. The benchmark diarrhea rate is calculated in two ways: initially, by using the fourteen-day diarrhea rate in the pure control group across all children, multiplied by the number of children in the household; and subsequently, by using the monthly incidence of childhood diarrhea from the epidemiological literature ([Luby et al., 2006](#)).<sup>13</sup>

### **2.2.2 Round 2 of Phase 1**

In Round 2 of Phase 1 (August to November 2022), we continue to visit households every two weeks and launch the distribution of chlorine tablets to all T1, T2, and T3 households.

---

<sup>10</sup>In the first biweekly survey, we also collect data on household bargaining power, as we were unable to incorporate this into our first baseline visit. Since we ask caregivers questions about decision-making power before revealing treatment status, we consider this as a baseline measure of household bargaining power. There are no differences across treatment groups in being the sole decision-maker, or a part of decision-making, in issues of child health, household purchases, or household visits. We also collect anthropometric data for 338 households for whom we were not able to collect anthropometric data in the baseline visit.

<sup>11</sup>Since chlorine distribution had not yet begun, and chlorine tablets are not widely available in local markets, ten percent of households were randomly selected for chlorine testing during every other visit of Round 1 of Phase 1. We test chlorine in this pre-distribution period to ensure that knowledge of treatment status does not affect people's ability to procure chlorine tablets on their own.

<sup>12</sup>The study was non-blind with respect to study subjects and treatment administrators (CHWs). Enumerators were not informed by the research team on the specific treatment status of the households at baseline. While all enumerators were hired through our partner organization, Interactive Research and Development (IRD), we recruited a different team for the endline survey to ensure objectivity, and these enumerators were not informed of the participants' treatment status before beginning the survey. However, participants were not blind to their own treatment status, so it is possible that they discussed their treatment status with the enumerators and effectively rendered the endline survey non-blind. Community health workers used electronic tablets and smartphones to collect data.

<sup>13</sup>When we use the diarrheal incidence in the Control group, the benchmark is updated daily to reflect the past fourteen days of data collection from the pure control households. We switch to the diarrhea rate in the epidemiological literature out of concern that the incidence of diarrhea in the Control group may be impacted by a reduced disease environment due to the presence of treated households in the community. Enumerators explain to participants that the benchmark rate is the average rate of diarrhea from people in the community who do not use chlorine to purify their water.

We continue to collect information on diarrhea prevalence and test water for the presence of chlorine in all households. We test for chlorine residual every other visit (once per month). Incentives begin and the Info-Tool continues.

#### *Incentives*

Caregivers are offered tokens redeemable for child and household goods based upon the number of chlorine tablet wrappers they show enumerators as proof of usage. The chlorine tablets come individually wrapped and participants are instructed to save the empty wrappers in a pouch provided to all participating households. Each daily reward for proper chlorine use is equal to approximately 0.05 USD (with ‘proper use’ calibrated to the household’s pre-intervention water consumption). To hold income effects constant, we also provide comparable products to participants in the remaining groups – *unconditioned* on chlorine tablet wrappers – framed as a token of appreciation for participating in our surveys. Since T3 households can redeem tokens for household goods that have varying values, we implement a lottery to determine the value of the unconditional gifts that non-T3 households received.

#### *Info-Tool*

At the end of Round 2, we aggregate the monthly Info-Tool chart statistics across three-month intervals and present the aggregated data to T2 participants visually (Figure G.2). T2 participants are able to visualize their own children’s diarrhea rate relative to the average diarrhea rate among households that do not use chlorine in the three-month interval before chlorine tablet distribution, and make the same visual comparison for the three-month interval following chlorine tablet distribution: in effect, a household-specific difference-in-differences estimate.

Throughout Round 2, we conduct random unscheduled audits where CHWs test water for the presence of chlorine residual. These audits are conducted to ensure that households are not chlorinating their water in anticipation of our bi-weekly visits and to increase confidence that our scheduled monthly chlorine tests serve as a good proxy for chlorine use throughout the month (audit visits are discussed in more detail in Section L).

### **2.3 Phase 2: Monthly Visits**

In Phase 2 (November 2022 to October 2023), we reduce our household visits to once monthly. We continue to distribute chlorine tablets and test water for the presence of chlorine residual, but we cease providing incentives or gifts, information to Info-Tool participants of the benchmark diarrhea rate, Info-Tool sheets, or assistance to Info-Tool participants to track their children’s diarrhea rates.

## 2.4 Endline Visits

In our first endline survey (December 2023 to January 2024), we collect all measures from baseline, including diarrhea rates, presence of chlorine residual, and child anthropometrics. We additionally elicit willingness-to-pay for chlorine tablets using a take-it-or-leave-it offer with a randomized price<sup>14</sup> and detailed social network data to understand potential mechanisms for spillovers, including directly serving neighbors chlorinated water and indirectly through conversations about the project and lessons learned through the Info-Tool.

We achieve an 87% follow-up rate in the first endline. While attrition is non-differential across learning, chlorine, and pure control arms (our margins of comparison in our primary analyses), incentive households are 4.2 percentage points more likely to attrit than pure control households ( $p < 0.05$ ; Table B.1). However, across a range of baseline observables, the endline incentives respondents do not statistically differ from their counterparts in the other arms (Tables F.5 and F.6).

We conduct a final short follow-up survey in June 2024 to provide participants with chlorine tablets for the summer months (the period in which diarrhea rates peak), monitor stockpiling of chlorine tablets, and ask additional survey questions to disentangle mechanisms.<sup>15</sup>

Taken together, we administer 26 surveys per household: a baseline survey, eleven rounds of bi-weekly visits in Phase 1, twelve monthly visits in Phase 2, one endline visit immediately after Phase 2, and a second endline visit four months later.

## 2.5 Outcomes

Our primary outcomes are chlorine use and child health. Our measure for chlorine use is a binary indicator for the presence of chlorine residual in drinking water. CHWs test for chlorine residual in each household's drinking water vessel using a simple test strip that, when dipped into a small cup of water, turns shades of blue depending on the chlorine concentration in the water (the minimum amount of chlorine detectable by the test strips is 0.5 parts per molecule). Chlorine residual only presents itself if the chlorine tablet was added to the water within twenty-four hours of testing, meaning we are likely to underestimate true chlorination given the preponderance of households who imperfectly chlorinate their water.

Our measures of child health are self-reported diarrhea incidence and child anthropometrics.

---

<sup>14</sup>After providing respondents with a one-month supply of chlorine tablets, free of charge, we offer them a second month's supply of chlorine at a randomized price (market price, a 29% subsidy, or a 53% subsidy).

<sup>15</sup>We sought to monitor stockpiling because there was a sharp drop-off in rates of chlorine detection in the last two rounds of surveying (the final survey of Phase 2, and the first endline survey), which was precisely when we began to tell respondents that the trial was nearly complete and that we would soon cease to provide free chlorine tablets. Since we ended the survey during the winter, the time of year when diarrhea rates are lowest, stockpiling chlorine tablets for the summer would have been a rational response from participants. We find some evidence of this: although, by the time of this survey, only 3% of respondents reported that they still had chlorine tablets remaining, 44% of respondents reported that they ran out of their chlorine tablets later than they should have if they were chlorinating their water daily, and for 24% of households the discrepancy was by more than one month.

Diarrheal incidence is measured by the number of days of diarrhea across all children under five years old, which we collect in every visit. However, this measure is subject to measurement and misclassification errors. The Info-Tool intervention is likely to change the *reporting* of diarrhea in children, since we explicitly encourage participants to track diarrhea within this arm. We may thereby be capturing a treatment effect on reporting, rather than a treatment effect on the actual incidence of diarrhea. Further, recent literature has suggested that the presence of non-infectious diarrhea and asymptomatic infection makes diarrhea presence a poor outcome for measuring the effectiveness of water and sanitation interventions ([Watson et al., 2022](#)).

Our preferred child health measure is instead child anthropometrics. Frequent diarrhea can result in under-absorption of nutrients, leading to suboptimal physical development in children. We therefore collect height-for-age, weight-for-age, weight-for-height, and mid-upper-arm-circumference-for-age for all children under 80 months old.<sup>16</sup>

### 3 A Model of Joint Learning in Technology Adoption

Before turning to the empirical analysis, we offer a conceptual framework to motivate the presentation of results. We present a Bayesian model of learning about a technology, following the notation of [Kondylis et al. \(2023\)](#). However, we modify the model to include “experience weights,” which influence how strongly individuals integrate beliefs into their actions. In contrast to standard Bayesian models, which describe how individuals update their mean beliefs given a signal, experience weights capture how individuals *act* on those beliefs. Experience weights depend on the technology that generates a signal and on personal experience with that signal-acquisition technology.

Let  $A$  be a distribution of prior beliefs about the efficacy of a health technology:

$$A \sim \mathcal{N}(\mu_0, \sigma_0^2)$$

In each period, individuals receive a health signal  $Y$  with precision given by  $\sigma_Y^2$ :

$$Y|A = a \sim \mathcal{N}(a, \sigma_Y^2)$$

Individuals then update their posterior beliefs about the efficacy of the health technology according to Bayes rule:

$$A|Y \sim \mathcal{N}(M, \sigma^2)$$

where  $M \equiv \ell Y + (1 - \ell)\mu_0$  represents the updated expected returns to using the technology (in our case,  $M$  represents posterior beliefs about chlorine efficacy). The weight  $\ell \equiv \sigma_0^2 / (\sigma_0^2 + \sigma_Y^2)$  represents standard weight that agents assign to signal  $Y$  in their posterior beliefs, considering only the precision of signals. Then, participants’ updated uncertainty is  $\sigma^2 \equiv (1 - \ell)\sigma_0^2$ .

---

<sup>16</sup>These children are up to 80 months at endline because they were included in our baseline survey if they were up to 60 months old.

We consider two margins along which information can affect technology adoption. First,  $\ell$  determines how individuals update their explicit beliefs. This knowledge will be reflected in their stated beliefs and depends on the precision of the signal itself, modeled in the variance  $\sigma_Y^2$ . Second, we introduce “experience weights”  $\alpha_Y \in [0, 1]$ , which depend on experience with the signal-acquisition technology. People can intellectually understand and articulate the returns to a technology, but it is their experience-weighted beliefs that affect their decision to act on that knowledge. When  $\alpha_Y = 1$ , individuals behave as in the canonical Bayesian model, acting fully on their updated beliefs. When  $\alpha_Y = 0$ , individuals are sufficiently untrusting, inexperienced, or detached from the signal-acquisition process that they fail to act on the signal altogether, even if they superficially believe and recognize a signal to be positive and precise.  $M$  represents *stated* posterior beliefs about the returns to the technology (or explicit knowledge). We define  $M_\alpha \equiv \ell\alpha_Y Y + (1 - \ell\alpha_Y)\mu_0$  to be the experience-weighted posterior belief. An individual adopts the technology when  $M_\alpha > C$ , where  $C$  is the cost of adopting the technology.<sup>17</sup>

Let  $\gamma_Y \in \{\Gamma\}$  denote the source of the signal  $Y$  from a set of potential sources  $\Gamma$  (for example, the Info-Tool, community observation, or messages from a community health worker). Each individual accumulates experience with each technology over time, denoted  $E(\gamma_Y)$ . We assume that an individual’s behavioral responsiveness to a signal depends on the technology that generates that signal and on the individual’s cumulative experience with that technology.

Formally, the experience weight associated with signal  $Y$  is

$$\alpha_Y = \alpha(\gamma_Y, E(\gamma_Y)),$$

with  $\frac{\partial \alpha}{\partial E(\gamma_Y)} > 0$ . That is, the more familiar or practiced an individual is with a given signal-acquisition process, the more she relies on signals produced by that process when deciding whether to act.

The function  $\alpha(\cdot)$  can reflect a wide range of behavioral processes: learning to trust the technology, developing ownership over the signals one generates, or becoming more willing to act on risky or uncertain signals as experience accumulates. We describe several potential micro-foundations for  $\alpha_Y$  in Appendix Section I, illustrating how the functional form of  $\alpha_Y$  could vary if experience weights arise from trust, ownership, risk-taking, or perceived dispersion of signals. Crucially, experience weights are defined at the level of the *technology*, not the signal realization. Two individuals who receive the same signal  $Y$  may therefore behave differently if one has prior experience with the technology that produced it.

## First period adoption

Initial priors are diffuse: at baseline, 12.7% of respondents report having ever heard of chlorine and knowing that its purpose is water purification. We therefore consider information sent by the Community Health Worker (CHW) as the first signal that participants receive about

---

<sup>17</sup>In a standard model, participants adopt the technology when  $M > C$ .

chlorine,  $Y_{CHW}$ . It is likely that  $\sigma_{Y_{CHW}}^2$  is low, as the CHWs represent a known NGO and they have been visiting households for three months prior to chlorine distribution. It is also likely that  $C$  is close to zero in the first period, since the largest reported cost associated with using chlorine is an unpleasant taste if the chlorine dose is incorrectly titrated, which caregivers would not yet have experienced.

## Interventions

All chlorine groups: We reduce the cost of using chlorine  $C$  in all time periods through free distribution and delivery.

Incentives: We further reduce the cost of using chlorine  $C$  in the Incentives group for the first three months of chlorine distribution by providing monetary incentives for use.

Info-Tool: We change the signal-acquisition technology  $\gamma_Y$  in the first three months of chlorine distribution. This in turn raises cumulative experience with the Info-Tool,  $E(\gamma_Y) \mid \gamma_Y = Info-Tool$ , in perpetuity, and increases  $\alpha_Y$  for all signals personally observed via the Info-Tool and signals sent by other Info-Tool participants for whom the receiver is aware that the sender observed their signal through the Info-Tool.

## Comparative statics

**Prediction 1 (Incentives: Shock to  $C$ )**: *Increased contemporaneous adoption relative to chlorine only households*

A negative shock to  $C$  increases the probability in any period that  $M_\alpha > C$ , leading to increased contemporaneous chlorine adoption. This effect is immediate from the first period of chlorine distribution but fades out as soon as the CHW stops providing chlorination incentives.

**Prediction 2 (Info-Tool: Shock to  $\gamma_Y$ )**: *Increased contemporaneous adoption relative to chlorine only households among households with larger positive early-period health signals, with fade-out*

The Info-Tool leads to an improvement in the signal-acquisition technology in the first three months of chlorine adoption. Consequently, participants place a contemporaneously higher weight  $\alpha_Y$  on signals acquired through observation during the treatment period. After the CHW stops assisting participants in the Info-Tool, costs are the same for all groups, but Info-Tool participants' experience-weighted prior is higher. Thus, we should observe higher average use following the withdrawal of the Info-Tool.

Since individuals learn from short-term health signals, we should expect heterogeneity by the sign on individual contemporaneous health signals, and the Info-Tool group should be the most sensitive to small differences in health signals. This effect should fade out as all participants acquire new signals—signals upon which the Info-Tool group, no longer using the tool, places no special weight.

**Prediction 3 (Info-Tool: Shock to  $E(\gamma_Y) \mid \gamma_Y = \text{Info-Tool}$ ):** *Increased adoption relative to chlorine only households among Info-Tool households with connections to other Info-Tool households*

As soon as an Info-Tool participant has used the Info-Tool, any other Info-Tool-acquired signals, including those acquired by *other* Info-Tool participants, likewise carry additional weight. Info-Tool caregivers with an Info-Tool member in their network will be more responsive to those connections' health signals than caregivers in other treatment groups. This can have effects after the Info-Tool period has ended, if participants share cumulative rather than period-specific information about their experiences.

## Simulated Model

As a check on the plausibility of our theoretical framework, in Appendix H, we plot chlorine adoption patterns predicted by our model using simulated data. Under a set of reasonable parametric assumptions outlined in Appendix H, we document adoption patterns that closely follow our empirical results, which we present below.

# 4 Results: Chlorination

## 4.1 Empirical Specification

We consider two ways in which learning spillovers may transpire. Experiential learning may be infectious in a social network: non-learning-arm households may be more likely to use chlorine when they have learning-arm neighbors, diluting our ability to detect treatment effects (cross-treatment spillovers). However, information diffusion by social learning *does not* always fit epidemiological models of information transmission, wherein information spreads “infectiously” through proximate nodes as long as the information is sufficiently valuable and the cost of spreading information is sufficiently low. In an alternative manifestation of learning spillovers, experiential learning and social learning are complements: learning-arm households are more likely to use chlorine when they have learning-arm neighbors (within-treatment spillovers). In this case, individual randomization reduces the number of learning-arm neighbors a learning household has, thereby weakening the potential impact of the treatment.

Households are, on average, 13.6 meters from the closest other participant; 16.0 meters from the closest participant in any chlorine treatment group; and 27.5 meters from the closest Info-Tool participant.<sup>18</sup> We define a “nearby neighbor” as a participant within 20 meters of a respondent. Under this definition, 43% of participants have a nearby neighbor in the Info-Tool group. When we recenter this measure to account for endogenous neighborhood density (following Borusyak and Hull (2023)), 35% of participants are exposed to a nearby neighbor in the Info-Tool group

---

<sup>18</sup>The median distance to the closest household is 10.4 meters; the median distance to the closest participant in any chlorine treatment group is 12.1 meters; and the median distance to the closest Info-Tool households is 22.7 meters.

by random variation in the treatment assignment (Table B.3 demonstrates that this measure is balanced across treatment arms).

We compute exposure to Info-Tool neighbors in the AM data using the same spillover definition. In that experiment, 49% of Info-Tool participants are randomly exposed to other Info-Tool participants by variation in the treatment assignment, while 2% of Chlorine Only participants are.<sup>19</sup> This means both that, in our trial, non-Info-Tool participants are *more* exposed to other Info-Tool participants than in AM, and that Info-Tool participants are *less* exposed to other Info-Tool participants. If there is a learning spillover from Info-Tool participants onto non-Info-Tool neighbors, our individual-level randomization conceals Info-Tool treatment effects; and if there is a learning spillover from Info-Tool participants onto other Info-Tool neighbors, the treatment is less powerful in our setting.

First, we define our spillover measure. Because randomization is conducted at the individual level, individuals are randomly exposed to Info-Tool neighbors. We consider somebody as being in the “spillover sample” if they have at least one Info-Tool neighbor within an  $r$ -meter radius of where they live. First, we determine the number of Info-Tool participants within each radius  $r \in \{20(20)200\}$  meters. Next, we recenter these measures to purge our estimates of omitted variable bias that may arise from neighborhood density or other endogenous environmental factors, following [Borusyak and Hull \(2023\)](#). We construct  $\text{Any}T2_i^r$ , a binary measure that indicates if the recentered number of Info-Tool participants within radius  $r$  of individual  $i$  is greater than 0. We interpret this variable as indicating if participants were randomly more exposed to an Info-Tool neighbor than they would be in expectation based on endogenous spatial factors. Following [Egger et al. \(2022\)](#), we select the  $R$  that minimizes the Schwarz Bayesian Information Criterion (BIC) for each  $r \in \{20(20)200\}$  meters. Then,  $\sum_{r=20}^R \theta_r(\overline{\text{Any}T2_i^r})$  is our estimate of learning spillovers. For all of our specifications, the smallest radius (20 meters) minimizes the Schwarz BIC. The spillover and non-spillover samples are balanced on observable baseline variables, across the full sample (Table F.1), and within each individual treatment group (Tables F.2, F.3, F.4).

We test for the presence of learning spillovers using the following specification:<sup>20</sup>

$$Y_i = \theta_0 + \theta_{1,r} T1_i + \theta_{2,r} T2_i + \sum_{r=20}^R \theta_{3,r} \text{Any}T2_i^r \times T1_i + \sum_{r=20}^R \theta_{4,r} \text{Any}T2_i^r \times T2_i \\ + \sum_{r=20}^R \theta_{5,r} \text{Any}T2_i^r + NV_i + X_{i0} + \gamma_b + \epsilon_i$$

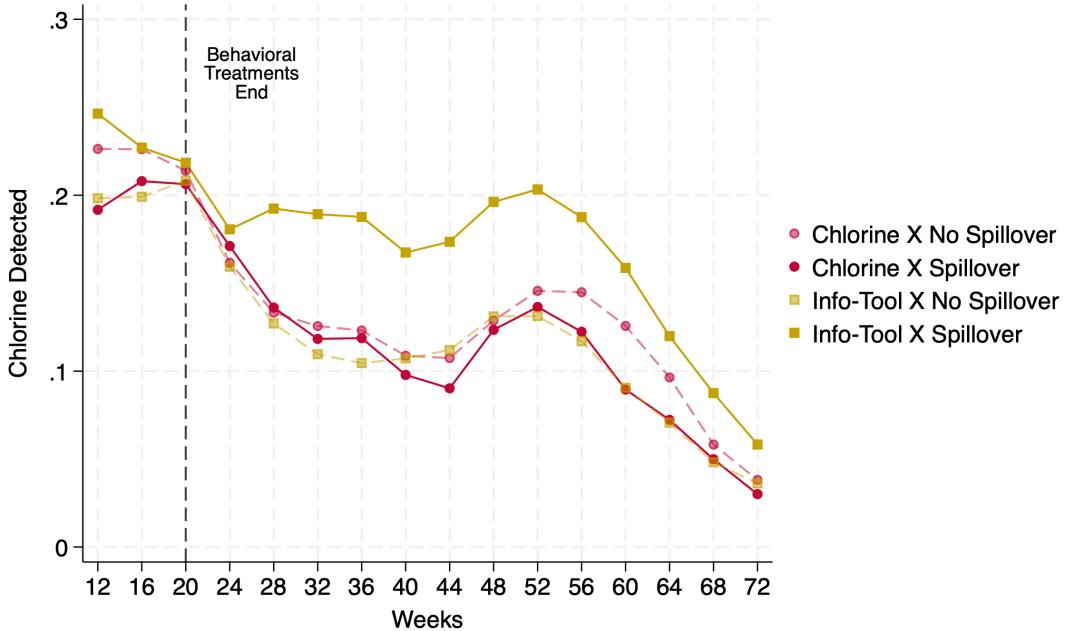
where  $Y_i$  represents the aggregated outcome measurements over relevant visits (e.g., total num-

---

<sup>19</sup>There are no Incentives or Pure Control groups in AM.

<sup>20</sup>We pre-specified an investigation of learning spillovers by conducting a heterogeneity analysis by exposure to Info-Tool households. In our pre-specified empirical estimation, we would analyze the impact of the number of Info-Tool neighbors on the whole sample. We did not pre-specify interacting exposure to Info-Tool neighbors with individual treatment status, which is the main empirical specification we use. Estimating the spillover within each treatment group is a natural extension to the empirical estimation we pre-specified.

Figure 3: Seasonal Time Trends in Chlorine Detection (Moving Averages, Raw Data) where “Spillover” is exposure to any Info-Tool neighbor



ber of visits with chlorine presence detected in the water);<sup>21</sup>  $T1_i$  and  $T2_i$  are binary variables representing Chlorine Only and Info-Tool participants, respectively;  $NV_i$  captures the number of visits with non-missing outcome measurements;  $X_{i0}$  is a vector of unbalanced baseline covariates, the density of study participants within twenty meters<sup>22</sup>, and other covariates that we select using the double-lasso method proposed by [Urminsky et al. \(2016\)](#); and  $\gamma_b$  are block fixed effects (geographic units of stratification). We cluster standard errors at the household level. Then  $\theta_{2,r}$  is our estimate of individual learning;  $\theta_{3,r}$  is our estimate of social learning; and  $\theta_{4,r}$  is our estimate of the complementarity between individual and social learning.

## 4.2 Results: Chlorine Use

We detect chlorine residual in the water of over 20% of treatment households during the short-run (Figure 3). Chlorine use declines among participants in the Chlorine Only arm and participants in the Info-Tool treatment group without any Info-Tool neighbors. Info-Tool participants with Info-Tool neighbors continue chlorinating at a stable high rate until week 52 of the experiment, after which their rate of use also slowly and steadily declines. While our trial is not designed to identify the causes of chlorine use patterns across time, the serial pattern follows diarrhea rates, with all groups chlorinating at higher rates during the high-diarrhea season.<sup>23</sup>

While the presence of Info-Tool participants within twenty meters does indeed increase chlorine

<sup>21</sup>We choose the household-level specification where the outcome is the total number of times we detect chlorine across all visits in each time period, following [McKenzie \(2012\)](#), to maximize power. For robustness, we also conduct our regressions using household-survey panel specification, where the results hold (Appendix Section E).

<sup>22</sup>Section 4.1 details the selection of this bandwidth following [Egger et al. \(2022\)](#).

<sup>23</sup>Overall trends in chlorine use might be explained by seasonality and by experimental changes:

Table 1: Chlorine Detection  
where “Spillover” is exposure to any Info-Tool neighbor

	(1)	(2)	
	Short-Run	Long-Run	
Chlorine × Spillover	-0.008 (0.070)	0.071 (0.172)	
Info-Tool × No Spillover	-0.034 (0.058)	0.055 (0.142)	
Info-Tool × Spillover	-0.033 (0.069)	0.528*** (0.170)	
Observations	1234	1282	
Chlorine No Spillover Mean	0.645	1.543	
<b>P-values:</b>			
Info-Tool: Spillover = No Spillover	0.993	0.006	
Info-Tool × Spillover = Info-Tool × No Spillover + Chlorine × Spillover	0.928	0.092	

Standard errors in parentheses

Each observation is at the household level. The outcome is the aggregate of the total number of times that chlorine was detected across all visits in the specified time period. The Chlorine Only treatment group without spillover neighbors is the omitted group. An indicator for Pure Control participants, who are included in the sample, is in the estimating equation but not reported. All specifications include a control for the number of visits that the household was surveyed during the specified time period, neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Long-run is defined as the remainder of the trial (twelve months). The Spillover Sample is defined as participants who were randomly more exposed to an Info-Tool neighbor (someone within 20m) than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

detection, this is only true for *other Info-Tool participants* (Table 1). This pattern emerges immediately after the behavioral treatments end and persists through the end of the study. After the behavioral treatments end through the end of the study, Info-Tool participants with another Info-Tool participant neighbor chlorinate 30% more often ( $p = 0.006$ ) than participants with no Info-Tool neighbor. There are no meaningful differences in use (statistically or in magnitude) by this spillover measure in the Chlorine Only.<sup>24</sup> While we use an indicator for *any* random exposure to Info-Tool neighbors in our main specifications, we find that the number of times that an Info-Tool participant uses chlorine across the whole study is indeed increasing in the number of Info-Tool neighbors (Figure D.2).

We conduct two additional placebo tests for robustness. First, we replicate our analysis to test for spillovers from Chlorine Only or Incentives participants onto their neighbors. There is no time period in which we find spillover treatment effects from Chlorine Only participants

---

Seasonality: Diarrheal prevalence peaks in the summer, during the hottest and雨iest months. We began our study at the peak season, which allows chlorine use to generate starker changes in diarrhea rates and gives the Info-Tool a greater chance of showing the efficacy of chlorine. Chlorine use tracks closely with self-reported diarrhea (Figure D.1). In the second summer of the experiment, diarrhea rates remain relatively low. This could be due to herd immunity generated via the intervention, or a milder monsoon season.

Experimental Changes: Chlorine rates fall sharply after the behavioral interventions end (week 20), including in the Chlorine Only group, which did not receive a behavioral intervention. There were two experimental changes in week 20: (1) we began visiting households every month, rather than every two weeks; and (2) we stopped providing gifts to all households. In the last two months of the study, chlorine rates dropped to almost zero. These were the only visits since the beginning of the trial when we explicitly reminded participants that the intervention was about to end. It is possible that households stockpiled chlorine tablets, knowing that they would soon lose access to a free supply of chlorine tablets and that the high diarrhea season was still several months away. We found some evidence of stockpiling in our second endline: 44% of respondents reported that they ran out of their chlorine tablets later than they should have if they were chlorinating their water daily, and for 24% of households the discrepancy was by more than one month.

<sup>24</sup>There is no difference in the Incentives group either. We report Incentive group outcomes in Section 5.2.1.

or Incentives participants onto any treatment group, including their own (Tables D.2 and D.3). Second, we show that, among non-Info-Tool participants, chlorine use remains constant in the number of Info-Tool neighbors (Figure D.2). These placebo tests rule out alternative mechanisms driven by the diarrheal disease burden or observation of others using chlorine, strengthening our assertion that Info-Tool neighbors impact one another by sharing information about health signals.<sup>25</sup> Third, in Appendix C, we reanalyze the AM data to test for within-treatment Info-Tool spillover effects and find comparable effects.

## 4.3 Results: Child Health

### 4.3.1 Diarrhea

Diarrhea rates over time offer a lens into how the various interventions impact children's health dynamically over the course of the experiment. Treatment group participants report 36% (Chlorine Only), 26% (Info-Tool), and 38% (Incentives) reductions in diarrhea in the short run relative to the Pure Control group (Table M.1). Reductions in diarrhea rates mirror relative levels of chlorine use throughout the experiment, with the Info-Tool group experiencing the largest reductions in diarrhea prevalence in the long-run. More details on measurement and results on child diarrhea are reported in Section M. However, as a self-reported measure, children's diarrhea rate is subject to respondent bias; that it is directly connected to the substance of the Info-Tool also leaves room for concerns of endogeneity in outcome reporting.

### 4.3.2 Child Anthropometrics

As an objective and therefore our preferred measure of health, we measure child anthropometrics at endline. To analyze the impact of our treatment on child anthropometrics, we use the following specification:

$$Y_{c,h} = \beta_0 + \beta_1 T_h + X_{h0} + \gamma_b + \epsilon_{c,h}$$

where  $Y_{c,h}$  is health outcome  $Y$  for child  $c$  in household  $h$ ,  $T_h$  is treatment status of household  $h$ , and  $X_{h0}$  are household-level baseline control variables. We cluster standard errors at the household level for all child-level specifications.

Following Anderson (2008), we create a summary index combining height-for-age (HAZ), weight-for-height (WHZ), weight-for-age (WAZ), and MUAC-for-age z-scores (mid-upper arm circumference-for-age). For households in any of the three treatment arms, the index increases by over 7% of a standard deviation ( $p < 0.05$ ) (Appendix Table M.3).<sup>26</sup>

---

<sup>25</sup>In the short-run, Incentives participants use chlorine at a higher rate, so any mechanism related to aggregate chlorine use in the community should bear out in the short-run for participants with more exposure to Incentives participants. If the number of signals matter, rather than access to individual experiential learning specifically, then non-Info-Tool participants with access to *two or more* Info-Tool neighbors should respond similarly as Info-Tool participants with access to *one* Info-Tool neighbor.

<sup>26</sup>For households receiving any of the three treatments, the estimates for height-for-age and weight-for-height are close to zero, whereas those for weight-for-age and MUAC-for-age are 0.112 ( $p < 0.1$ ) and 0.055 (not

Table 2: Child Index of Anthropometry (ITT):  
where “Spillover” is exposure to any Info-Tool neighbor

	(1) Omitted: Other Chlorine Only & IT Groups	(2) Omitted: All Other Treated Groups	(3) Omitted: Pure Control	(4) Omitted: Chlorine × No Spillover
Chlorine × No Spillover			0.099** (0.045)	
Chlorine × Spillover			0.040 (0.056)	-0.051 (0.060)
Info-Tool × No Spillover			0.079* (0.044)	-0.012 (0.049)
Info-Tool × Spillover	0.079* (0.044)	0.100** (0.042)	0.158*** (0.056)	0.066 (0.060)
Observations	2616	2616	2616	2616
<b>P-values:</b>				
Chlorine × Spillover = Chlorine × No Spillover			0.331	
Chlorine × Spillover = Info-Tool × No Spillover			0.519	0.523
Chlorine × Spillover = Info-Tool × Spillover			0.081	0.087
Info-Tool × No Spillover = Info-Tool × Spillover			0.186	0.196
Info-Tool × Spillover = Chlorine × Spillover + Info-Tool × No Spillover				0.126

Standard errors in parentheses

Child-level cross-section of the endline survey. Standard errors are clustered at the household level. All regressions include baseline unbalanced household-level controls, baseline child anthropometrics and diarrhea rates, child gender, child age, neighborhood block fixed effects, the number of study participants within twenty minutes, and lasso-selected controls. The regression in column (1) controls for indicator variables for being in the following groups: Incentives × No Spillover, and Incentives × Spillover. The regression in columns (2) and (3) control for indicator variables for being in the following groups: Control, Incentives × No Spillover, and Incentives × Spillover. The index is constructed using following Anderson (2008) from the following variables: WAZ, WHZ, HAZ, and Muac-for-age. The Spillover Sample is defined as participants who were randomly more exposed to an Info-Tool neighbor (someone within 20m) than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

We find that joint learning leads to improvements in child health relative to free chlorine distribution alone, social learning, or experiential learning. The gain to joint learning over treatment alone is a 0.079 SD ( $p < 0.10$ ) increase in an objective measure of child health, doubling the effect of free chlorine distribution alone (Table 2). Comparing the spillover-sample Info-Tool participants with the rest of the sample (including Incentives and Pure Control) to estimate the total treatment effect of joint learning, there is a 0.100 SD increase in the index of child anthropometry ( $p < 0.01$ ).

#### IV Results: Effects of Water Purification

Treatment compliance is not perfect – we are only able to detect chlorine in the water of our treated participants 13% of the time across the whole study, yet 37% of treatment participants self-report that they are using chlorine to treat their water at endline. To better understand the effects of the intervention on child health among households that actually purify their water, we use an instrumental variables specification. We consider compliers to be any individual who, at endline, is induced by their treatment status to boil, bleach, or chlorinate their water according to their self-report (48% of the sample). We use this self-reported measure to proxy for compliance rather than objective chlorine residual detection because participants may have learned about the negative effects of impure water through the study and changed their water treatment behavior in some way *other* than chlorinating their water, which would violate the

---

significant). Appendix Table M.3 reports treatment effects on each index component for all treatment groups pooled, while Appendix Table M.4 reports treatment effects on each index component separated by treatment groups.

exclusion restriction. Indeed, we find that Control participants are more likely to boil their water at endline. If we instead consider treatment to be an instrument for using any effective water purification technique, which includes boiling, then higher rates of boiling in the Control group is not a threat to the exclusion restriction and the Control participants who boil their water are always-takers.

Our instrumented measure is self-reported and is an extensive margin measure of using an effective water purification method, which does not imply perfect compliance.<sup>27</sup> Furthermore, even with perfect compliance, individuals who use effective water purification methods at endline may have purified their water at different rates throughout the study; since health is a stock, this historical use matters. The Info-Tool treatment, and particularly being in the Info-Tool group with a neighbor in the Info-Tool group, leads to a higher rate of cumulative use of chlorine throughout the study. It is likely that someone in these groups who reports purifying her water at endline has a higher rate of historical use than other treated participants.

To address this challenge, we consider the IV impacts of water chlorination on child health using three different measures of treatment as our instrument, which is equivalent to measuring the IV impacts in three distinct complier samples: (1) adopters of effective water purification technologies among households assigned to any treatment group, (2) adopters of effective water purification technologies among households assigned to the Info-Tool treatment group, and (3) adopters of effective water purification technologies among households assigned to the Info-Tool who are also randomly assigned an Info-Tool neighbor. We should expect that the more limited complier samples should have larger IV treatment effects, since the binary self-reported measure of using an effective water purification method represents a larger stock of historical chlorine use in these samples, which should have a greater impact on child health.

There are two threats to the exclusion restriction in our instrumental variables analysis. First, it is possible that the treatment leads households to make other changes in household behavior that affect child health. For example, if households that adopt chlorine are also more likely to be motivated to practice better sanitation during the experiment, we cannot exclusively identify the effect of water purification. Second, Info-Tool households with Info-Tool neighbors could experience dual protection from their own chlorine usage and proximity to other chlorine users. This reduced disease environment, driven by herd immunity, may contribute to the IV treatment effects.

Column (1) of Table 3 reports the IV treatment effect of water purification, using assignment to any treatment group as the instrument for using an effective water purification method.<sup>28</sup> The compliers are any participants in the Chlorine Only, Incentives, or Info-Tool group who are induced to report using an effective water purification method due to random assignment into any treatment group. In Table 3 column (2), the instrument is an indicator for being in the

---

<sup>27</sup>Among the people who reported that they chlorinate their water at endline, we only detected chlorine in the water of 3.8%.

<sup>28</sup>Appendix Table M.5 reports the IV treatment effects on each index component using this same complier sub-sample.

Table 3: IV Impacts on Endline Child Health:  
where “Spillover” is exposure to any Info-Tool neighbor

	(1)	(2)	(3)
	Instrument: Any Treatment	Instrument: Info-Tool	Instrument: Info-Tool × Spillover
Boils, Bleaches, or Chlorinates Water	0.240** (0.113)	0.329*** (0.126)	0.506*** (0.185)
Observations	2616	2616	2616
Endline Control Mean	-0.017	0.023	0.031
First-Stage Coefficient	0.312	0.329	0.312
Weak-IV robust F statistic	119.931	86.571	35.258
C-statistic p-value	0.027	0.005	0.002

Standard errors in parentheses

Child-level cross-section of the endline survey. Standard errors are clustered at the household level. Any treatment (i.e., received chlorine) is an instrument for if the respondent reported at endline that she boils, bleaches, or chlorinates her water. All regressions include baseline unbalanced household-level controls, baseline child anthropometrics and diarrhea rates, child gender, child age, neighborhood block fixed effects, the number of study participants within twenty meters, and lasso-selected controls. The index is constructed using following Anderson (2008) from the following variables: WAZ, WHZ, HAZ, and Muac-for-age. The Spillover Sample is defined as participants who were randomly more exposed to an Info-Tool neighbor (someone within 20m) than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Info-Tool group, and the compliers are individuals who are induced to report using an effective water purification method due to random assignment into the Info-Tool group (the regression controls for being in the Incentives or Chlorine-Only groups). The estimate is 37% higher (0.33 SD increase in child anthropometrics), suggesting that the higher intensity of chlorine use that we detect among Info-Tool throughout the study period translates into greater improvements in child health. Finally, in Table 3 column (3), the instrument is an indicator for being in the Info-Tool group *and* having random exposure to an Info-Tool neighbor, and the compliers are individuals who are induced to report using an effective water purification method due to randomly being in the Info-Tool group and having an Info-Tool neighbor (the regression controls for being in the Incentives or Chlorine-Only groups, and for being in the spillover sample). The estimate of water purification on child health is 111% higher than the any-chlorine-group treatment effect, and 54% higher than the average Info-Tool treatment effect (0.51 SD increase in child anthropometrics).<sup>29</sup>

These heterogeneous results further point towards full saturation of the Info-Tool treatment as the optimal policy. Comparing our results on anthropometrics with results from other recent programs in South Asia, our results on weight-for-age are significantly larger than those from handwashing, hygiene, nutrient supplements, or early childhood education programs (Table 4). We do not see any results on height-for-age, unlike the handwashing, hygiene, and nutrient supplement programs.

#### 4.3.3 Discussion

Is it plausible that the effects on chlorination that we document, while economically meaningful, can yield the large-magnitude effects on child anthropometrics that we find? We believe so. Our

---

<sup>29</sup>Table M.6 reports the corresponding estimates for the four components of the index.

Table 4: Benchmarking Child Health Estimates

Intervention	Paper	HAZ	WHZ	WAZ	Muac-for-age
Chlorine: Average	Appendix Table M.5	0.050	-0.017	0.371*	0.198
Chlorine: Saturated Info-Tool	Appendix Table M.6	-0.083	0.584	0.789**	-0.428
Handwashing	Hussam et al. (2022)	0.272	–	0.203	0.078
Hygiene	Bennett et al. (2018)	0.290	–	0.270	–
Nutrient Supplements	Sazawal et al. (2013)	0.180	–	0.030	–
	Soofi et al. (2022)	0.290	0.050	0.260	–
ECD	Bos et al. (2024)	-0.024	0.230	0.137	–

measure of chlorination is a flow, while anthropometrics is a stock: even modest increases in water purification, when accumulated over an extended period of time, may have large effects. Furthermore, our measures of chlorine use are almost certainly underestimates, as participants need to have used chlorine in the past 24 hours in order for enumerators to detect chlorine presence in each visit.<sup>30</sup> Finally, there may exist a complementarity between herd protection and personal protection, leading to a further amplification of the health impacts of increased chlorine use (Deutschmann et al., 2024; Duflo et al., 2015; Fuller and Eisenberg, 2016). Info-Tool households with Info-Tool neighbors are the participants most likely to both use chlorine *and* have neighbors using chlorine, meaning their children are the most likely to be protected from infection transmitted through their drinking water *and* their environment.

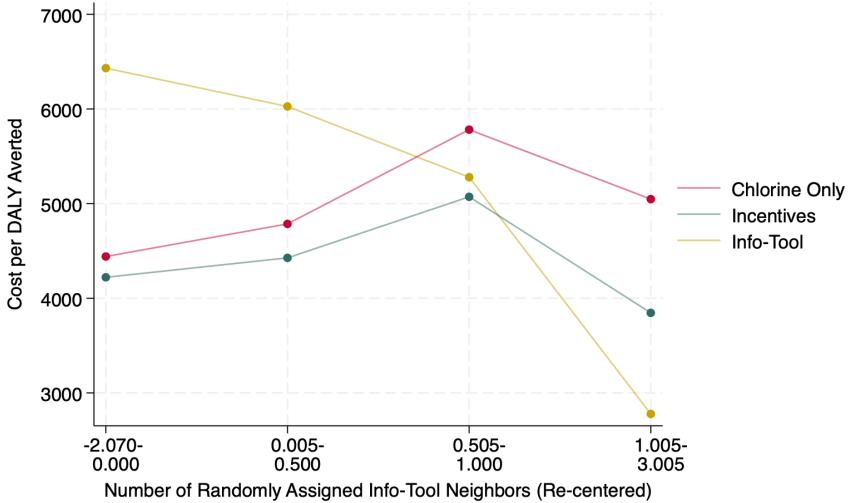
#### 4.4 Cost-Benefit Analysis

The Info-Tool is non-trivially more expensive than the other treatment arms because it is the only treatment that requires CHW time prior to chlorine distribution, implying three months of additional labor costs. Furthermore, the Info-Tool is only effective when offered to many people, implying higher total costs. Are the benefits of the within-treatment spillovers large enough to offset these costs?

We assume that every time we detect chlorine is equivalent to 30 days of water chlorination, and that 11,000 days of water chlorination equates to 1 DALY averted (International, 2017). Each treatment household costs 24.78 USD. Each Info-Tool household cost an additional 7.84 USD (paper materials and salaries for CHW time), and each Incentives household cost on average an additional 0.88 USD (short-term incentives provision). We estimate that the cost per DALY averted for an Info-Tool participant with at least one other Info-Tool neighbor is 2778 USD, while the cost per DALY averted for all other participants is 38% to 131% larger (depending on the treatment group and density of Info-Tool neighbors). Indeed, we find that the cost per DALY averted is *decreasing* in the density of Info-Tool participants in an area (Figure 4). It is *only* cost-effective to implement the Info-Tool if the treatment is highly saturated in a geographic area.

<sup>30</sup>We detect chlorine 13% of the time across the whole study, but 37% of treatment participants *report* that they were using chlorine to treat their water at endline. Furthermore, we detect higher rates of chlorine use in our unscheduled audit studies during the treatment period than we do during regularly scheduled visits.

Figure 4: Cost-Effectiveness Analysis



## 5 Mechanisms

Why does a simple pen-and-paper chart lead to behavioral change? In our model, experience generating health signals with the Info-Tool induces caregivers to place higher weights on any signal acquired from the Info-Tool when deciding to act on beliefs. Crucially, these behavioral changes need not be reflected in stated explicit beliefs or knowledge about the expected returns to chlorine tablet adoption.

We report estimates for our main model parameters in Table 5: explicit knowledge or beliefs about chlorine efficacy  $M$ , experience-weighted beliefs  $M_\alpha$ , and behavioral decisions determined by  $\mathbb{1}(M_\alpha > C)$ . We find *no differences* in participants' incentivized stated beliefs that their children's diarrhea rates decreased due to chlorine, our measure of  $M$  (we discuss this measure and its validity in detail in Section 5.2.4). However, we do find differences in the degree to which participants report that they are *motivated* to use chlorine to attain health for their family. We use motivation as our stated measure of  $M_\alpha$  because experience weights affect behavior (or motivation to act), but not explicit knowledge (we discuss this measure and its validity in detail in Section 5.1.2). This variation in  $M_\alpha$  then translates into differences in chlorine adoption, which represents  $\mathbb{1}(M_\alpha > C)$  (we discuss our results on chlorine adoption in Section 4).

### 5.1 Experience Weights

We show two pieces of evidence that jointly suggest that Info-Tool participants' adoption decisions are driven by weights applied to health signals. First, consistent with Prediction 2 in our model, we show that Info-Tool participants' chlorine use is heterogeneous by the sign on their health signals in the short-run (the expected rate of decline in diarrhea). Second, consistent with Prediction 3 in our model, we show that the Info-Tool to Info-Tool spillover is completely explained by Info-Tool neighbors whose diarrhea rate is ex-ante predicted to improve, indicating that Info-Tool participants only act on signals generated by their Info-Tool neighbors

Table 5: Model Parameters

	(1) Stated $M$ : Diarrhea Went Down	(2) Stated $M_\alpha$ : Motivation: Health	(3) Observed $\mathbb{1}(M_\alpha > C)$ : Times Detected Chlorine
Chlorine $\times$ Spillover	0.022 (0.048)	-0.094 (0.092)	0.071 (0.172)
Info-Tool $\times$ No Spillover	0.021 (0.040)	0.122 (0.075)	0.055 (0.142)
Info-Tool $\times$ Spillover	-0.003 (0.048)	0.222** (0.090)	0.528*** (0.170)
Observations	1123	703	1282
Chlorine $\times$ No Spillover Mean	0.711	6.081	1.543
<b>P-values:</b>			
Info-Tool $\times$ Spillover = Info-Tool $\times$ No Spillover	0.621	0.278	0.006
Info-Tool $\times$ Spillover = Info-Tool $\times$ No Spillover + Chlorine $\times$ Spillover	0.491	0.127	0.092

Standard errors in parentheses

Each observation is at the household level. The omitted group is Chlorine  $\times$  No Spillover. The outcome for column (1) is an indicator for participants who said they believe that their childrens' diarrhea rate dropped after using chlorine in an incentivized elicitation about the efficacy of chlorine. The outcome for column (2) is a rating between 1 and 7 for how true the following statement felt: I use chlorine to achieve a standard of health for my family. This regression controls for the average motivation score the respondent gave across all motivation questions, and the order of questions (randomized). The outcome for column (3) is the number of times that we detected chlorine residual in the participant's water during the post-treatment period. This regression controls for the number of times we tested their water during the post-treatment period, and an indicator variables for belonging to the following groups: Control  $\times$  No Spillover (except column (2) – the outcome variables was not collected for Control participants), and Control  $\times$  Spillover (except column (2) – the outcome variables was not collected for Control participants). All specifications include neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. The Spillover Sample is defined as participants who were randomly more exposed to an Info-Tool neighbor (someone within 20m) than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

who receive positive signals. Next, we use evidence from stated beliefs to argue that Info-Tool participants apply higher weights to Info-Tool signals when deciding to act, and not because the Info-Tool affects signal uncertainty and explicit posterior beliefs about the mean returns to chlorine tablet adoption.

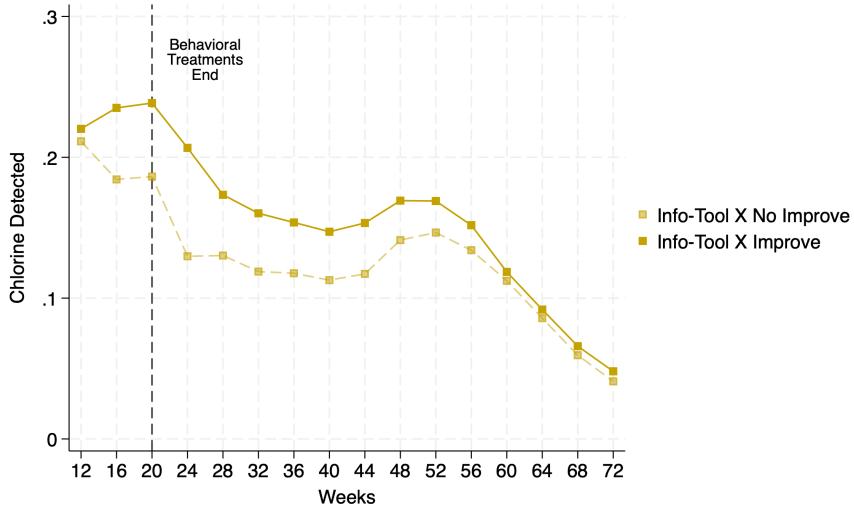
### 5.1.1 Responsiveness to Health Signals

#### Average Effects

First, we show that Info-Tool participants who are predicted to experience improvements in their diarrhea rate during the time period where they are tracking their data – or, Info-Tool participants who we expect to have a positive sign on the health signals they observe in their environment and place higher weights on – chlorinate at a higher rate immediately after the Info-Tool treatment ends (Table 6 and Figure 5). This effect is very valuable in the short-run: predicted-improved Info-Tool participants chlorinate 29% more often than not-predicted-improved Info-Tool participants in the short-run ( $p$ -value = 0.043). In the long-run, this effect becomes smaller and noisier. However, predicted-improved and not-predicted-improved Info-Tool participants' chlorination rates converge not because people in the predicted-improved sample are learning less, but because people in the not-predicted-improved sample are learning more from their Info-Tool neighbors, as evidenced by Figure D.5.

Actual health will be correlated with treatment adoption. To understand how treatment re-

Figure 5: Info-Tool Chlorine Detection by Predicted Health Improvement (Moving Averages, Raw Data)



sponds to health signals that are exogenous to treatment status, we use a predicted measure of health improvement. First, we define “health improvement” as the number of days of diarrhea in the three months before chlorine adoption minus the number of days of diarrhea in the first three months of chlorine adoption. The first three months of chlorine adoption is the relevant period of time to analyze health signals, because this is the time in which Info-Tool participants gain experience acquiring information about their health via the Info-Tool. We consider someone to be “predicted improved” if the predicted diarrhea rate improvement is above the median.

We use lasso with the Pure Control sample to construct a measure of predicted health improvement (as defined above), using variables collected before treatment status was revealed to predict this measure (all variables from the baseline survey and the first half of the first follow-up survey). To avoid bias that can arise from endogenous stratification, we use the leave-one-out procedure proposed by [Abadie et al. \(2018\)](#).<sup>31</sup>

To test for individual learning from one’s own health signals in the Info-Tool group, we use the following specification:

$$Y_i = \theta_0 + \theta_1 T1_i + \theta_2 T2_i + \theta_3 \hat{I}_i + \theta_4 \hat{I}_i \times T1_i + \theta_5 \hat{I}_i \times T2_i + Y_{i0} + X_{i0} + \gamma_b + \epsilon_i$$

where  $\hat{I}_i = 1$  if an individual is ex-ante predicted to improve (continuous predicted health improvement is above the median). Then  $\theta_5$  is our object of interest. We use  $\theta_4$  to test if there are differences in long-term use between those predicted to improve and those not predicted to improve when the information is not acquired through experience with the Info-Tool.

We find that Info-Tool participants whose health is ex-ante predicted to improve in the first

<sup>31</sup>The correlation between “actual health improvement” and “predicted health improvement” is 0.39 in the Pure Control group, and 0.40 in the treatment groups.

Table 6: Chlorine Detection (Household-Survey Panel)  
by predicted health improvement  
Omitted group: Chlorine  $\times$  Not Improved

	(1)	(2)	
	Short-Run	Long-Run	
Chlorine $\times$ Improved	0.104 (0.066)	0.271 (0.166)	
Info-Tool $\times$ Not Improved	-0.046 (0.066)	0.190 (0.163)	
Info-Tool $\times$ Improved	0.107 (0.067)	0.468*** (0.169)	
Observations	1192	1216	
Chlorine $\times$ Not Improved Mean	0.557	1.399	
<b>P-values:</b>			
Info-Tool $\times$ Improved = Info-Tool $\times$ Not Improved	0.024	0.097	

Standard errors in parentheses

Each observation is at the household level. The outcome is the aggregate of the total number of times that chlorine was detected across all visits in the specified time period. The Chlorine Only treatment group without spillover neighbors is the omitted group. An indicator for Pure Control participants, who are included in the sample, is in the estimating equation but not reported. All specifications include a control for the number of visits that the household was surveyed during the specified time period, neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months). Improved is a binary indicator for if the participant's predicted improvement in health after the beginning of chlorine distribution was above the median. See Section 6 for a detailed explanation for how the predicted-health-improvement measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

three months of chlorine distribution use chlorine at a higher rate than Info-Tool participants whose health is not predicted to improve. In the short run, predicted-improved Info-Tool participants use chlorine 29% more often than Info-Tool participants who are not predicted to improve. The two groups only diverge in their chlorine use in the third month, by which time they receive and are able to respond to health signals acquired with the Info-Too. There are no differences between predicted-improved and not-predicted-improved participants in any other treatment group in the short-run.

In the quarter immediately after behavioral treatments end, the predicted-improved Info-Tool respondents continue using chlorine at a higher rate than not-predicted-improved Info-Tool participants (23% higher rate of use, Table 6). However, we are not able to rule out equality of these differences. In this quarter, differences in use by predicted-improvement in the Chlorine Only group also emerge. This could be explained by some learning that happens through observation without the Info-Tool, or by selection. For example, perhaps the characteristics of people who are predicted to improve also incline them more towards longer-term use without any learning. While these patterns fade in both groups in the long-run, the magnitude of the difference remains slightly larger in the Info-Tool group.

### Spillovers

If Info-Tool participants are able to act on information from their Info-Tool neighbors to a

Table 7: Chlorine Detection (Household-Survey Panel)  
where “Spillover” is exposure to any Info-Tool neighbor predicted to improve  
Omitted group: Chlorine  $\times$  No Spillover

	(1)	(2)	
	Short-Run	Long-Run	
Chlorine $\times$ Spillover	-0.084 (0.079)	0.083 (0.196)	
Info-Tool $\times$ No Spillover	-0.054 (0.053)	0.070 (0.132)	
Info-Tool $\times$ Spillover	-0.011 (0.079)	0.645*** (0.197)	
Observations	1192	1216	
Chlorine $\times$ No Spillover Mean	0.643	1.538	
<b>P-values:</b>			
Info-Tool: No Spillover = Spillover	0.588	0.004	
Info-Tool $\times$ Spillover = Info-Tool $\times$ No Spillover + Chlorine $\times$ Spillover	0.247	0.070	

Standard errors in parentheses

Each observation is at the household level. The outcome is the aggregate of the total number of times that chlorine was detected across all visits in the specified time period. The Chlorine Only treatment group without spillover neighbors is the omitted group. An indicator for Pure Control participants, who are included in the sample, is in the estimating equation but not reported. All specifications include survey-round fixed effects, neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, lasso-selected baseline controls, and indicators for being in the Pure Control group with or without a learning-arm neighbor. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months). The Spillover Sample is defined as participants who were randomly more exposed to an Info-Tool neighbor (someone within 20m) whose health was predicted to improve than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined. See Section 6 for a detailed explanation for how the predicted-health-improvement measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

greater degree than other participants are, then the spillover should be explained by cases where information shared between two participants is likely about chlorine being highly efficacious. To test if the spillover treatment effect can be explained by positive signals from neighboring Info-Tool participants, we limit  $AnyT2_i^r$  to only be equal to 1 if there is any Info-Tool participant within radius  $r$  of participant  $i$  who was predicted to see their health improve. When we change the definition of the spillover from *any Info-Tool neighbor* to *any predicted-improved Info-Tool neighbor* the spillover estimates remain almost unchanged (comparing estimates in Table 7 with estimates in Table 1, estimates are *at least as big* in magnitude to the spillover treatment effect with the broader definition of spillovers).

It is possible that participants who are predicted to improve have characteristics that make them more likely to influence their neighbors, regardless of treatment status or whether they share information that can be interpreted as a positive signal about chlorine. To rule out this possibility, we conduct a placebo test where we test heterogeneity by the total number of predicted-improved treated neighbors. In this placebo test, there are no differences in use by exposure to this spillover measure in the short-run or medium-run, in any group (Table D.4).

### 5.1.2 Stated Beliefs

We ask respondents several questions to interrogate three ways in which the Info-Tool could have led participants to apply higher weights on health signals and increase their use of chlorine: (1) Info-Tool increases participant’s belief about the mean efficacy of chlorine tablets for water purification; (2) Info-Tool increases the probability that participants believe that their disease

environment poses a risk to children’s health; and (3) Info-Tool increases the probability that participants act on their beliefs, without affecting beliefs themselves. We find evidence contrary to mechanisms (1) and (2), and supportive evidence of mechanism (3).

### Motivation

Although at endline households in all treatment groups non-differentially report that they believe chlorine to be an effective technology, and unclean water to be a source of sickness, Info-Tool participants are more likely to report that attempting to achieve health for the family is the *motivating reason* for why they use chlorine (Table D.5).<sup>32</sup> Info-Tool households rate “to attain a certain level of health for my family” as a stronger motivating factor for using chlorine, controlling for the total level of motivation they give across all potential motivating factors (or, propensity to report high levels of motivation broadly). Although almost everyone believes that chlorine is an effective technology, and that they live in a risky disease environment where that technology is needed, the Info-Tool gives people the motivation to act upon this information.

Furthermore, the treatment effect on the motivation score that participants give “health” is twice as large in the spillover sample in the Info-Tool group, as compared to Info-Tool participants who are not in the spillover sample (Column 2, Table 5). These treatment effects are not statistically distinguishable and therefore are not conclusive on their own, but it is encouraging that the patterns we see are aligned with our model predictions. Chlorine Only and Incentives participants do not rate health as a motivating factor differentially by access to neighbors from the Info-Tool group.

### Behavior Depends on Information-Acquisition Technology

Why does the Info-Tool motivate people to act upon information that, on the surface, appears widely known? To understand if behavioral change depends on the signal-acquisition process itself, we elicit stated beliefs about trust in information acquisition technologies. We take trust in the Info-Tool, or understanding that the Info-Tool generates valuable signals, as a lower bound on a deep understanding of the Info-Tool’s data-generating process.<sup>33</sup> We remind all participants about the three treatment groups in our study: people receiving chlorine only, people receiving chlorine as well as gifts in exchange for using chlorine, and people receiving chlorine as well as a pen and paper chart with which they tracked their children’s diarrhea

---

<sup>32</sup>At endline, we asked caregivers a series of questions to understand their motivation behind using chlorine (question formulation adapted from Tremblay et al. (2009)). We read several statements describing reasons why someone might use chlorine, and asked women to rate on a scale from 1 to 7 how true that statement was for them during the times when they have ever used chlorine. The statements described a motivation related to health (“Because using treatments like chlorine tablets help me to attain a certain level of health for my family”), habit formation (“Because it has become a fundamental part of my routine”), income generation (“Because it could allow me to earn money”), and intrinsic motivation (“Because I derive pleasure from trying new things”, “Because I want to be very good at taking care of my family, otherwise I would be very disappointed”, and “For the satisfaction I experience when I am successful at doing difficult tasks”).

<sup>33</sup>We piloted questions that directly measured participants’ ability to interpret data from the Info-Tool, but found that participants who were not in the Info-Tool group were unable to even guess at an interpretation. It is possible that participants outside the Info-Tool group are still able to understand that information generated by the Info-Tool is valuable. Thus, we view trust in information gathered from the Info-Tool as a lower bound on deeply understanding the Info-Tool data-generating process.

Table 8: Endline Stated Trust  
Omitted group: Pure Control

	(1) Info-Tool Knows Most	(2) Chlorine Knows Most	(3) <b>Robustness:</b> Incentives Knows Most
Chlorine Only	-0.043 (0.027)	0.122*** (0.034)	-0.079** (0.031)
Incentives	-0.004 (0.027)	-0.085** (0.034)	0.088*** (0.031)
Info-Tool	0.222*** (0.027)	-0.138*** (0.034)	-0.083*** (0.031)
Observations	1516	1516	1516
Control Mean	0.196	0.527	0.276
<b>DID Estimate:</b>			
Info-Tool picks Info-Tool			
– Other Group picks Own Group		0.100 [p= 0.046]	0.133 [p= 0.004]

Standard errors in parentheses

Each observation is at the household level. The outcome for column (1) is an indicator for if the respondent chose a hypothetical Info-tool respondent as the person most likely to be knowledgeable about child health (rather than a Chlorine Only or Incentives participant). The outcome for column (2) is an indicator for if the respondent chose a hypothetical Incentives respondent as the person most likely to be knowledgeable about child health (rather than a Chlorine Only or Info-Tool participant). The outcome for column (3) is an indicator for if the respondent chose a hypothetical Chlorine Only respondent as the person most likely to be knowledgeable about child health (rather than an Incentives or Info-Tool participant). All specifications include neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

rates. We then ask them to imagine a neighbor from each group and indicate which one they believe has the most knowledge about children’s health. Differencing out the propensity to choose one’s own group using Incentives households, participants in the Info-Tool group were 13.3 percentage points ( $p = 0.004$ ) more likely to list the Info-Tool respondent (Table 8).<sup>34</sup>

Our model suggests that experience with the Info-Tool alone, regardless of exposure to other Info-Tool participants, should generate higher weights on signals acquired through the Info-Tool. Indeed, we find that Info-Tool participants trust other Info-Tool participants’ knowledge about child health more than other groups do, regardless of whether they have access to an Info-Tool spillover. Interestingly, Info-Tool participants *without* access to an Info-Tool spillover are even more likely to rate other Info-Tool participants as the most knowledgeable (61% increase,  $p = .094$ ), indicating that personal experience with the Info-Tool itself is enough to change participants’ trust in the Info-Tool as an information source (Table D.6). Then, Info-Tool participants are uniquely primed to apply heavier weights to signals from other Info-Tool participants when deciding to act.

As a final check to ensure that the learning complementarity arises from the shared experience of learning about child health through collecting data via the Info-Tool, rather than merely recognizing that the Info-Tool generates precise signals, we compare households based on their

<sup>34</sup>The corresponding estimate is 10 percentage points ( $p = 0.046$ ) if we use the Chlorine Only households to net out the propensity to choose one’s own group.

level of independent engagement with the tool. Specifically, we contrast those who ever filled out the Info-Tool without the CHW’s help with those who only completed it during CHW visits. Both groups have access to equally precise information, but differ in how much they participated in the shared experience of data collection. Indeed, we find that the spillover effect is driven almost entirely by individuals who have at least one Info-Tool neighbor who independently filled out the Info-Tool chart for at least one two-week period, without the CHW’s help (Appendix Table D.7).

## 5.2 Competing Mechanisms

### 5.2.1 Early Adoption

Alternatively, the Info-Tool might lead participants to adopt chlorine early on at a higher rate than they would without the Info-Tool. Long-term adoption could then be explained by early adoption leading to habit formation, or intertemporal complementarities in chlorine use. We use the Incentives arm to rule out stories of chlorine use related to higher rates of early adoption.

Our Incentives arm builds on [Hussam et al. \(2022\)](#), in which persistence in handwashing behavior is engendered through exogenous short-run financial incentives to handwash in West Bengal.<sup>35</sup> Our Incentives arm serves as a parallel intervention: households are incentivized to chlorinate their water daily, receiving tokens that can be exchanged for household goods for each day of empty chlorine wrappers they present to enumerators. Should water chlorination, which is a repeated act performed at the same time and place each day, indeed be habit-forming, then this exogenous increase in initial consumption stock via financial incentives will activate the intertemporal complementarities in use, thereby generating long-run use even after incentives are withdrawn. Alternatively, a higher rate of use may lead households to acquire more signals in a shorter period of time than in the other two groups, leading participants to learn from their own experience how the tablets improve their children’s health. We do not attempt to distinguish between habit formation and learning through rapid accumulation of health signals in the Incentives arm. Instead, we use the Incentives arm to rule out either of these two mechanisms.

During the short-run, Incentives households chlorinate significantly more than those in Info-Tool or Chlorine Only. However, immediately following incentive withdrawal (week 20), chlorination rates for incentive households plummet, while the decay among Info-Tool households is gradual. Figure D.8 plots the regression coefficients for the impact of Incentives and Info-Tool, relative to Chlorine Only, on chlorination using the panel dataset from household visits (also reported in Table D.1). Consistent with the observations from the raw chlorination plots, during the short-run period (Quarter 1), households assigned to Incentives outperform those in Info-Tool ( $p = 0.014$ ) and Chlorine Only ( $p = 0.090$ ). In the immediate post-intervention period (Quarter

---

<sup>35</sup>[Hussam et al. \(2022\)](#) finds that households who experienced larger health improvements (either across weeks or in aggregate) from the intervention did not exhibit differentially greater persistence in handwashing, and therefore attribute the long run behavior change to habit formation rather than learning.

2), households in the Info-Tool group chlorinate at a higher level than Incentives and Chlorine; however, contrary to the findings in AM, the difference in treatment effects between Info-Tool and Chlorine is not statistically distinguishable ( $p=0.142$ ). Effects converge thereafter.

In order to ensure that Incentives households are actually chlorinating at a higher rate, rather than chlorinating just on the days where the enumerator is present, we do not directly incentivize the water chlorination test (household incentives are tied to presenting empty chlorine tablet wrappers), and we conduct unscheduled audit tests. In these audit tests, we detect chlorine at a higher rate in Incentives than in Info-Tool. Furthermore, we detect chlorine at a much higher rate during the audit visits than during the regularly scheduled visits (45% detection rate in the Incentives arm during audit visits), further suggesting that Incentives participants are not more likely to use chlorine when they expect that we will come to test their water than at other times.

Since the Incentives arm builds up a higher stock of chlorine use in the first three months of chlorine distribution, any theory of sustained behavioral change relating to early adoption will be borne out in the long term in the Incentives arm. The Incentives group immediately reverts to the same rate of chlorine use as the Chlorine Only group when the behavioral treatments end, so the mechanism that helps the Info-Tool sustain chlorine adoption in the months following the behavioral interventions cannot be explained by habit formation or any story related to early adoption.

### **5.2.2 More Interactions and Mimicry**

It is possible that the novelty of the Info-Tool leads participants to interact with one another more when they are both in this group, and that they simply mimic one another's behaviors rather than learn from their information. For example, perhaps they assist one another in filling out the chart. However, we do not have any evidence to suggest that participants interacted in this way, with only 12% of Info-Tool respondents reporting that they ever discussed the Info-Tool with anybody else. Info-Tool participants' endline social networks are not larger than any other groups' (Table D.8). Furthermore, we have no evidence of Info-Tool participants mimicking one another on other behaviors, including take-up of other health programs that are present during our study trial, or choice of household savings technology (Table D.12).

### **5.2.3 More Conversations**

We ask participants if they have ever discussed health or water purification with members of their social network. On average, participants discussed health with one person, and this did not differ across any treatment group. Control group participants are much less likely to discuss water purification than treated participants, but Info-Tool participants are no more likely to discuss water purification with their social network connections than the other two treated groups. If anything, the Incentives group participants are the most likely to discuss water purification with others (Table D.8).

#### 5.2.4 Signal Uncertainty

In an alternate model, the Info-Tool acts as an education tool that increases participants' ability to comprehend signals that they observe or hear. Although this alternate model is attractive in its simplicity, since this mechanism could alone explain the complementarity result, the data do not support it. In this model, the Info-Tool first facilitates participants in comprehending the signals that they observe, leading them to update their beliefs, whereas the participants in other treatment groups do not; then, because Info-Tool participants' beliefs now differ from the beliefs of participants in other groups, the information they share about chlorine efficacy also differs. Because Info-Tool participants gain skills in comprehension from the treatment in this model, they are also uniquely enabled to understand the signals from their Info-Tool neighbors, generating the complementarity that we observe.

If this is the true model that explains our results, the following conditions need to be true: (1) the Info-Tool leads participants to better comprehend the signals about chlorine that they observed; and (2) the Info-Tool participants share different information than participants in the other treatment groups.

##### *No Evidence of Differential Comprehension*

Halfway through Phase 2 (the week 44 survey), we conduct an incentivized belief elicitation activity. We simply asked participants to tell us whether their children's diarrhea rates increased, decreased, or remained the same during the three months following the start of chlorine distribution, compared to the three months prior. If participants answer correctly, they receive a gift (children's goods such as pencils or a notepad). If the Info-Tool improves signal comprehension, then participants should be more likely to answer the question correctly. However, there are no differences in correctly answering the question across any groups, with or without access to Info-Tool neighbors (Table 9). Access to Info-Tool neighbors also does not change which answer Info-Tool participants give, indicating non-differential knowledge or optimism about chlorine efficacy.<sup>36</sup>

To ascertain that answering the question correctly is sensitive to comprehension skills, we test whether response accuracy is differential by education. Reassuringly, participants in any treatment group who had ever been to school (sixty-four percent of the sample had received zero years of education) are 5.9 percentage points (12%,  $p = 0.061$ ) more likely to answer the question correctly (Table D.10).<sup>37</sup> Interestingly, Control participants with any education are *less* likely to answer the question correctly, partially explained because educated Control participants are more likely to answer "I don't know" than any other group. This suggests that access to technological experimentation alone *does* lead participants to increase the attention they pay to signals from their observed environment, especially among people with education. Jointly,

---

<sup>36</sup>Control participants were less likely to say that their diarrhea rate had improved than the treatment groups (accurately so), but there were no differences between any groups that received chlorine on average.

<sup>37</sup>The education treatment effect was non-differential across treated arms among participants who received any chlorine treatment.

Table 9: Midline Memory/Beliefs about Chlorine Efficacy

	(1) Correct Answer	(2) Went Down	(3) Didn't Change	(4) Went Up	(5) Don't Know
Chlorine × Spillover	0.009 (0.049)	0.083* (0.044)	-0.043 (0.027)	-0.019 (0.038)	-0.020 (0.014)
Info-Tool × No Spillover	0.054 (0.040)	0.081** (0.036)	-0.017 (0.022)	-0.038 (0.031)	-0.030*** (0.011)
Info-Tool × Spillover	0.027 (0.049)	0.057 (0.045)	-0.024 (0.028)	-0.014 (0.039)	-0.021 (0.014)
Observations	1123	1123	1123	1123	1123
Control Mean	0.488	0.711	0.077	0.195	0.014
<b>P-values:</b>					
Info-Tool × No Spillover = Info-Tool × Spillover	0.622	0.628	0.823	0.569	0.535

Standard errors in parentheses

Each observation is at the household level. This regression includes neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. In the week 44 survey, we ask participants to tell us whether their children's diarrhea rates increased, decreased, or remained the same during the three months following the start of chlorine distribution, compared to the three months prior. If participants answer correctly, they receive a gift (children's goods such as pencils or a notepad). The outcome of column (1) takes the value of 1 if respondents report the correct answer based on our own data, and 0 otherwise. The outcome of column (2) takes the value of 1 if respondents report that their child's diarrhea rate decreased, and 0 otherwise. The outcome of column (3) takes the value of 1 if respondents report that their child's diarrhea rate didn't change, and 0 otherwise. The outcome of column (4) takes the value of 1 if respondents report that their child's diarrhea rate increased, and 0 otherwise. The outcome of column (5) takes the value of 1 if respondents report that they do not know what happened to their children's diarrhea rate, and 0 otherwise.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

this set of results demonstrates that the Info-Tool does not actually increase the accuracy of participants' knowledge or the optimism of their beliefs about chlorine efficacy. At endline, we ask several more non-incentivized questions about beliefs in chlorine efficacy and the riskiness of the disease environment, none of which yield differential responses across treatment groups.<sup>38</sup>

### No Evidence of Differential Information Sharing

We can rule out that Info-Tool participants talk to more people, or discuss water purification or child health with more people (see Section 5.2.2 and Section 5.2.3). Since we do not observe participants' conversations, we cannot determine exactly *what* they say about water purification or child health in these conversations. However, recall that Info-Tool participants are no more likely to answer positively or accurately about chlorine efficacy when correct answers are incentivized, and there is no reason to believe that only the Info-Tool group would convey a different set of beliefs to each other within their private conversations. Furthermore, we ask a few questions at endline to understand the nature of participants' conversations about water purification, and there are no differences across treatment groups.

<sup>38</sup>At endline, we find no differences across treatment groups in beliefs about how many child-days of diarrhea a hypothetical household would experience after using chlorine relative to before using chlorine; no differences in the number of child-days of diarrhea participants believe their household would experience in the absence of chlorine, in either the summer or winter seasons; no differences in reporting "unclean water" as a primary cause of illness in the household before using chlorine tablets (unprompted); and no differences in how high they rank unclean water as a primary source of child illness prior to using chlorine tablets, relative to other potential sources of illness (prompted). It is possible that beliefs updated immediately after the Info-Tool treatment but then converged with time, with social learning superseding individual learning, such that we do not observe differences in beliefs at endline, so we take these null results as consistent with our results but not conclusive.

We ask participants to guess the water purification method of each member of their social network. Using each participant’s name, nickname, husband’s name, and neighborhood block, we are able to link social network nodes with participants in our sample. If Info-Tool participants are sharing information about chlorine tablets that is more favorable towards chlorine than other groups, we might expect participants to believe that *their* Info-Tool friends are chlorine users. We see no differences in participants’ beliefs about their treated friends’ chlorine tablet use (Table 10). Interestingly, participants *are* more likely to believe that their friends in *any* treatment group are more likely to use chlorine than friends in the Control group or outside the study sample. This implies that participants have some awareness of who uses chlorine tablets, and that this question captures relevant information about participants’ propensity to share information about the water treatment methods they use.

It is possible that some Info-Tool participants share especially optimistic information about chlorine tablets, and some share especially pessimistic information. Then, these two forces might counteract each other so that participants believe their Info-Tool and other-treatment-group friends use chlorine at the same rates on average. If this is the case, then participants should have more *accurate* information about Info-Tool participants’ chlorine use, even if not more optimistic. Using our participants’ self-reported water purification methods at endline, we can determine if participants accurately guess the water purification method of their within-sample network connections. We find no evidence that participants, regardless of their treatment group, have a better understanding of their Info-Tool friends’ water purification methods than the purification methods that any of their other treatment-group friends use (Table D.13).<sup>39</sup> Furthermore, Info-Tool participants do not do a better job of guessing than any other group.

Finally, we also ask participants how long ago they first and last discussed water purification with each social network connection. Info-Tool participants do not begin engaging in conversations about water purification earlier than any of the other groups (Chlorine begins engaging in these conversations the earliest), nor have they had a conversation more recently (Incentives had the most recent water purification discussions) (Table D.14). This evidence supports our model, where treated participants are all equally aware of the benefits of chlorine tablets for water purification, and share this information non-differentially. Experience with the Info-Tool makes the belief in chlorine’s efficacy actionable.

How come the Info-Tool intervention, which assists participants in generating conclusions based on data-driven evidence, *does not* act as an educational tool and increase data memory or comprehension? Different from other successful interventions in the learning-to-learn literature, our intervention is significantly lighter-touch and is conducted with a sample who are majority-uneducated. [Ashraf et al. \(2021\)](#), for example, successfully improves teacher effectiveness in the classroom by implementing a pedagogy that encourages students to approach learning like

---

<sup>39</sup>The same holds true when we use other measures of “true treatment method” using our objective measures of water chlorine detection.

Table 10: Endline Beliefs About Friends' Chlorine Use

	(1)	(2)	
	Believes Uses Chlorine: Full Sample	Believes Uses Chlorine: Info-Tool Only	
Control Friend	-0.010 (0.038)	0.065 (0.067)	
Chlorine Only Friend	0.086** (0.041)	0.137 (0.087)	
Incentives Friend	0.013 (0.032)	0.016 (0.068)	
Info-Tool Friend	0.035 (0.046)	0.093 (0.089)	
Observations	2614	617	
Non-Participant Friends' Mean	0.085	0.052	
<b>P-values:</b>			
Info-Tool Friend = Chlorine Friend	0.281	0.713	

Standard errors in parentheses

Standard errors are clustered at the household level. Each observation is at the network-link level, for observations where the node is within our sample. This regression include neighborhood block fixed effects, unbalanced baseline controls, the number of study participants within twenty meters, lasso-selected baseline controls, and participant fixed effects. The outcome in column (1) is an indicator for if the participant's guess about if her friend uses chlorine aligns with what that friend reported using in the endline survey. The outcome in column (2) is an indicator for if the participant's guess about if her friend uses chlorine aligns with what we objectively observed (did we ever detect chlorine in the friend's water). The outcome in column (3) is an indicator for if the participant guessed that her friend uses chlorine tablets for water purification, and we detected chlorine in that participant's water at least three times.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

scientists, taking into account “evidence and data gathered from everyday life”.

### 5.2.5 Social Norms

It is possible that participants act on what they think *others* believe, via a preference to conform to social norms, rather than their own beliefs. We ask participants at endline if they believe a guest at their home would drink chlorinated water if they were to serve it. While the majority of the Control group believe a guest would accept chlorinated water (67%), there is a large effect of being in any treatment group on this measure (Table D.15). However, the Info-Tool group is no more likely to believe a guest would accept water than the other two groups. If anything, Incentives participants are the most likely to believe a guest would accept chlorinated water. We also ask participants to report to the best of their ability how each of their network connections purifies their water. Control group participants are very unlikely to believe that their network connections purify their water with chlorine (6.4% believe one network connection purifies their water with chlorine), but the rate is 92.9% higher for participants in any treated group. Again, Info-Tool is no more likely to believe their network connections purify water than any of the other treated groups (Table D.15).

## 6 Conclusion

We study the process of joint learning about health by leveraging a learning intervention whose effectiveness in changing behavior hinges on the interaction between individual experiential learning and reinforcement through social learning. While social learning alone proves insufficient to disseminate novel information or alter behavior, its interaction with individual experiential learning yields significant behavioral change, leading to important downstream improvements in child health outcomes.

We document consistent differences across two objectively measured primary outcomes: water chlorination and child anthropometrics. Relative to all other comparison arms, Info-Tool households with Info-Tool neighbors exhibit higher chlorination rates and larger improvements in child health. All other groups chlorinate at lower, statistically comparable levels, experiencing correspondingly smaller health gains relative to our pure control of no chlorine.

We offer three policy takeaways from this study. The first directly addresses the question of scale. When experiential and social learning are complementary, there is high added value in saturating a learning or information treatment. Indeed, we find that the cost per DALY averted is *decreasing* in the density of Info-Tool participants in an area (Figure 4). It is important that policymakers not assume that these interventions will work similarly if disseminated sparsely within a population, for example, by treating or seeding knowledge among a specific sub-population.

Second, we add to a small but growing body of evidence that the underlying mechanism behind behavioral change or technological adoption is more complex than a mere shift in explicit knowledge about the returns to the behavior or technology (Hussam et al., 2022; Conlon et al., 2022; Fafchamps et al., 2024); rather, in our context, learning appears to be most effective when it is action-based and similarly experienced by others in one's network. This has relevance for the literature documenting the necessity of trust in information that individuals receive about health in order for it to impact behavior, a phenomenon with specific implications for historically marginalized communities. We find that a part of the psychological mechanism underlying an Info-Tool participant's behavioral change is increased trust in other Info-Tool participants' knowledge about child health. We operate in a setting where people lack formal consumer protection and have recent experience with insincere and politicized health campaigns (Martinez-Bravo and Stegmann, 2022).<sup>40</sup>

Our findings also speak to the role of the identity of the transmitter – determined here by how one acquires signals – on the perceived value of the transmitted information. Actors are sensitive to *how* information is acquired and more readily respond to information whose acquisition process they are personally familiar with. This may contribute to why people are hesitant to trust health information relayed by experts (Alsan and Wanamaker, 2018; Banerjee et al., 2023; Darden and Macis, 2024; Lowes and Montero, 2021; Martinez-Bravo and Stegmann, 2022), yet update their beliefs and actions strongly in response to personal experiences and information from in-group members (Alsan et al., 2019; Alsan and Eichmeyer, 2024; Bennett et al., 2018; Conlon et al., 2022; Corno, 2014; D'Acunto et al., 2021; Malmendier and Nagel, 2015; Simonsohn et al., 2008).<sup>41</sup> Our findings suggest that, for certain activities, embedding learning experiences within a community rather than importing information from outside may

---

<sup>40</sup>It is important to note that information coming from neighbors was effective above and beyond the information coming from the Community Health Workers, most of whom live in the same community as the respondents.

<sup>41</sup>Reliance on experts may also inadvertently deepen social and economic inequities given disparities in trust of experts, access to experts (Dussault and Franceschini, 2006), and access to well-informed networks (Banerjee et al., 2019; Calónico et al., 2023; Chen et al., 2022).

maximize long-term behavior change. While the identity of someone delivering information is important for generating trust, *how* they deliver that information is likewise critical. As outsiders in these communities, researchers may not have the relevant knowledge and skills to craft the messages that will be most effective. Allowing community members to experience and learn for themselves, and then craft their own messages, might be a more effective information campaign strategy.

Third, our results speak to public health programs across a wide geographic space. The take-up of water purification products is low in many developing countries. In Pakistan, only 0.3% of the population report usage of chlorine tablets, with 7.1% adopting any purification technology (Pakistan DHS 2017-18). The cost and availability of such tablets is an important barrier to adoption. In our endline take-it-or-leave-it willingness-to-pay exercise, only 2.1% of the sample were willing and able to pay *anything* for a one-month supply of chlorine tablets, and only 1.7% were willing and able to pay the market price. This is likely driven by an inability to pay rather than low valuation of chlorine, because participants demonstrate demand for chlorine in other ways. Seven percent of participants demonstrate willingness to give up their time for the chance to purchase chlorine by asking the enumerator to return at a later date to try selling again, hoping that they would have cash on hand available at another time. In the same visit, 37% report that they currently use chlorine to purify their water,<sup>42</sup> and 77% accept free chlorine tablets.

Many features of our study context are characteristic of low- and middle-income countries across South Asia and Sub-Saharan Africa ([Supply and Programme, 2014](#)). Inadequate public WASH infrastructure and low use of cheap point-of-use water purification technology, such as chlorine tablets, lead to environments with water contamination and high diarrhea prevalence. Full subsidization of these technologies has not been successful in bringing about substantive increases in take-up ([Akram and Mendelsohn \(2021\)](#) in Pakistan; [Dupas et al. \(2016\)](#) in Kenya). The materials that we use in our intervention are reasonable to use outside of an experiment and in other contexts. The Info-Tool is a simple and cheap pencil-and-paper intervention that low-literacy-and-numeracy individuals can easily use. Community health workers are a common feature of health systems across low- and middle-income countries ([Perry and Hodgins, 2021](#)), and our field protocols fit into the typical health worker workflow; by integrating into existing workflows, this intervention does not impose added human resource burdens on institutions working in this space. As such, this learning tool is potentially scalable.

Beyond point-of-use chlorination for water purification, what other technologies may feature returns to joint learning? We argue that the best candidates are technologies that are costly to adopt, require persistent use, and produce observable yet noisy treatment effects. Technologies that require persistent use provide users with many opportunities for adoption and observation. However, with sufficiently noisy treatment effects and sufficiently high costs, experiential learn-

---

<sup>42</sup>The correlation between self-reported chlorine use at endline and the probability that we ever detect chlorine residual in the prior three months is 0.22.

ing may need reinforcement through other trusted sources. Examples of health technologies that meet these criteria include: hygiene (e.g., hand-washing and proper treatment of food), mental health care practices (e.g., therapy and meditation), lifestyle changes (e.g., diet and exercise), and annual vaccinations (e.g., influenza and COVID-19). Examples of non-health technologies that meet these criteria include: agricultural practices (Vasilaky and Islam, 2018; Vasilaky and Leonard, 2018)<sup>43</sup>, childcare practices (Bunting, 2004)<sup>44</sup>, and learning or study habits (List and Uchida, 2024) .<sup>45</sup> A promising avenue for future research is to test for within-treatment spillover effects of programs encouraging the take-up of technologies that, according to our criteria, are candidates for experiential and social learning complementarities.

---

<sup>43</sup>Vasilaky and Islam (2018) find that female Ugandan farmers are more likely to learn new information disseminated between intervention rounds when they face team-based incentives to learn rather than tournament-based (individual) incentives. Consistent with our findings, it is only information that is novel that specifically benefits from joint learning. Vasilaky and Leonard (2018) find that, among farmers in Uganda who go to an agricultural training, being paired with another farmer and encouraged to discuss the techniques they learned throughout the farming season leads to higher productivity only attending the training.

<sup>44</sup>An illustrative example is the parental education groups organized in many countries under the coordination of trained health-workers. These groups provide a centralized source of reliable information, communicated by the worker, while also fostering peer-to-peer learning. Parents share specific details and insights about various practices based on their personal experiences, thereby complementing the centralized guidance with practical, experiential knowledge (Bunting, 2004).

<sup>45</sup>List and Uchida (2024) finds within-treatment (but no cross-treatment) spillover effects of early childhood education on cognition, driven by social network effects. While they do not specify what behaviors drive these cognitive gains, it is conceivable that students learn behaviors that promote cognitive development in preschool, which the students reinforce in one another if they are in classrooms together in kindergarten and grade school.

## References

- Abadie, Alberto, Matthew M. Chingos, and Martin R. West**, “Endogenous Stratification in Randomized Experiments,” *Review of Economics and Statistics*, 2018, *C* (4), 567–580.
- AEA RCT Registry Dataverse*
- AEA RCT Registry Dataverse**, <https://www.socialscienceregistry.org/site/csv> 2024. Accessed: 2024-11-12.
- Aggarwal, Shilpa, Rebecca Dizon-Ross, and Ariel D. Zucker**, “Incentivizing behavioral change: The role of time preferences,” *NBER Working Paper No. w27079*, 2020.
- Akram, A. A. and R. Mendelsohn**, “Diaries to Increase the Adoption of Chlorine Tablets for Water Purification by Poor Households,” *Water Economics and Policy (WEP)*, 2021, *7* (2), 1–34.
- Allcott, Hunt and Todd Rogers**, “The short-run and long-run effects of behavioral interventions: Experimental evidence from energy conservation,” *American Economic Review*, 2014, *104* (10), 3003–3037.
- Alpízar, Francisco, María Bernedo Del Carpio, Paul J. Ferraro, and Ben S. Meiselman**, “Exposure-enhanced goods and technology disadoption,” *Working Paper.*, 2022.
- Alsan, Marcella and Marianne Wanamaker**, “Tuskegee and the Health of Black Men,” *The Quarterly Journal of Economics*, 2018, *133* (1), 407–455.
- and Sarah Eichmeyer, “Experimental Evidence on the Effectiveness of Nonexperts for Improving Vaccine Demand,” *American Economic Journal: Economic Policy*, 2024, *16* (1), 394–414.
- , Owen Garrick, and Grant Graziani, “Does Diversity Matter for Health? Experimental Evidence from Oakland,” *American Economic Review*, 2019, *109*, 4071–4111.
- Anderson, Michael L.**, “Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects,” *Journal of the American Statistical Association*, 2008, *103* (484), 1481–1495.
- Arrow, Kenneth**, “The Economic Implications of Learning by Doing,” *The Review of Economic Studies*, 1962, *29* (3), 155–173.
- Ashraf, Nava, Abhijit Banerjee, and Vesall Nourani**, “Learning to Teach by Learning to Learn,” *Working Paper*, 2021.
- Athey, Susan and Guido W. Imbens**, “The econometrics of randomized experiments,” *Handbook of Economic Field Experiments*, 2017, *1*, 73–140.

**Banerjee, Abhijit, Abhijit Chowdhury, Jishnu Das, Jeffrey Hammer, Reshmaan Hussam, and Aakash Mohpal**, “*The Market for Healthcare in Low Income Countries*,” *Working Paper*, 2023.

— , **Arun G. Chandrasekhar, Esther Duflo, and Matthew O Jackson**, “*Using Gossips to Spread Information: Theory and Evidence from Two Randomized Controlled Trials*,” *The Review of Economic Studies*, 2019, 86 (6), 2453–2490.

— , **Emily Breza, Arun Chandrasekhar, and Ben Golub**, “*When Less is More: Experimental Evidence on Information Delivery during India’s Demonization*,” *Review of Economic Studies*, 2024, 91, 1884–1992.

**Beaman, Lori, Ariel BenYishay, Jeremey Magruder, and Ahmed Mushfiq Mobarak**, “*Can Network Theory-Based Targeting Increase Technology Adoption?*,” *American Economic Review*, 2021, 111 (6).

**Becker, Gary S and Kevin M Murphy**, “*A theory of rational addiction*,” *Journal of political Economy*, 1988, 96 (4), 675–700.

**Bennett, Daniel, Asjad Naqvi, and Wolf-Peter Schmidt**, “*Learning, Hygiene and Traditional Medicine*,” *The Economic Journal*, 2018, 128 (612), F545–74.

**BenYishay, Ariel and Ahmed Mushfiq Mobarak**, “*Social Learning and Incentives for Experimentation and Communication*,” *Review of Economic Studies*, 2019, 86, 976–1009.

— , **Maria Jones, Florence Kondylis, and Ahmed Mushfiq Mobarak**, “*Gender gaps in technology diffusion*,” *Journal of Development Economics*, 2020, 143, 1–27.

**Bikhchandani, Sushil, David Hirshleifer, Omer Tamuz, and Ivo Welch**, “*Information Cascades and Social Learning*,” *Journal of Economic Literature*, 2024, 62, 1040–1093.

**Borusyak, Kirill and Peter Hull**, “*Nonrandom Exposure to Exogenous Shocks*,” *Econometrica*, 2023, 91, 2155–2185.

**Bos, Johannes M., Abu S. Shonchoy, Saravana Ravindran, and Akib Khan**, “*Early childhood human capital formation at scale*,” *Journal of Public Economics*, 2024, 231.

**Breza, Emily and Arun Chandrasekhar**, “*Social Networks, Reputation, and Commitment: Evidence from a Savings Monitors Experiment*,” *Econometrica*, 2019, 87 (1), 175–216.

**Bunting, Lisa**, “*Parenting programmes: The best available evidence*,” *Child Care in Practice*, 2004, 10 (4), 327–343.

**Calónico, Sebastian, Rafael Di Tella, and Juan Cruz Lopez del Valle**, “*The Political Economy of a “Miracle Cure”: The Case of Nebulized Ibuprofen and its Diffusion in Argentina*,” *NBER Working Paper 31781*, 2023.

**Caro-Burnett, Johan, Judith A. Chevalier, and Ahmed Mushfiq Mobarak,** “*Is Habit a Powerful Policy Instrument to Induce Prosocial Behavioral Change?*,” *Cowles Foundation Discussion Papers*, 2021, 2600.

**Celhay, Pablo A., Paul J. Gertler, Paula Giovagnoli, and Christel Vermeersch,** “*Long-Term Effects of Temporary Incentives on Productivity in Medical Care Clinics*,” *American Economic Journal: Applied Economics*, 2015, 11 (3), 92–127.

**Chandrasekhar, Arun G., Esther Dufo, Michael Kremer, João F. Pugliese, Jonathan Robinson, and Frank Schilbach,** “*Blue Spoons: Sparking Communication about Appropriate Technology Use*,” *NBER Working Paper 30423*, 2022.

**Chen, Yiqun, Petra Persson, and Maria Polyakova,** “*The Roots of Health Inequality and the Value of Intrafamily Expertise*,” *American Economic Journal: Applied Economics*, 2022, 14 (3), 185–223.

**Conley, Timothy G. and Chris Udry,** “*Learning about a New Technology: Pineapple in Ghana*,” *The American Economic Review*, 2010, 100 (1), 35–69.

**Conlon, John, Manvika Mani, Gautam Rao, Matthew Ridley, and Frank Schilbach,** “*Not Learning from Others*,” *Revise Resubmit, Econometrica*, 2022.

**Corno, Lucia,** “*Learning (or Not) in Health-Seeking Behavior: Evidence from Rural Tanzania*,” *Economic Development and Cultural Change*, 2014, 63 (1), 27–72.

**D'Acunto, Francesco, Ulrike Malmendier, Juan Ospina-Tejeiro, and Michael Weber,** “*Exposure to Grocery Prices and Inflation Expectations*,” *Journal of Political Economy*, 2021, 129, 1615–1639.

**Darden, Michael E. and Mario Macis,** “*Trust and Health Care-Seeking Behavior*,” *NBER Working Paper 32028*, 2024.

**de Janvry, Alain, Karen Macours Macours, and Elisabeth Sadoulet,** “*Learning for adopting: Technology adoption in developing country agriculture*,” *FERDI*, 2016.

**Deutschmann, Joshua W., Molly Lipscomb, Laura Schechter, and Jessica Zhu,** “*Spillovers without Social Interactions in Urban Sanitation*,” *American Economic Journal: Applied Economics*, 2024, 16 (3), 482–515.

**Duflo, Esther, Michael Greenstone, Raymond Guiteras, and Thomas Clasen,** “*Toilets can work: Short and medium run health impacts of addressing complementarities and externalities in water and sanitation*,” *Technical Report, National Bureau of Economic Research* 2015.

**— , Rachel Glennerster, and Michael Kremer,** “*Using randomization in development economics research: A toolkit*,” *Handbook of development economics*, 2007, 4, 3895–3962.

**Duhigg, Charles**, *The power of habit: Why we do what we do in life and business*, Vol. 34, Random House, 2012.

**Dupas, Pascaline**, “*Short-run subsidies and long-run adoption of new health products: Evidence from a field experiment*,” *Econometrica*, 2014, 82 (1), 197–228.

**— , Vivian Hoffman, Michael Kremer, and Alix Peterson Zwane**, “*Targeting Health Subsidies through a Nonprice Mechanism: A Randomized Controlled Trial in Kenya*,” *Science*, 2016, 353 (6302), 889–895.

**Dussault, Gilles and Maria Cristina Franceschini**, “*Not enough there, too many here: understanding geographical imbalances in the distribution of the health workforce*,” *Human Resources for Health*, 2006, 4 (12).

**Egger, Dennis, Johannes Haushofer, Edward Miguel, Paul Niehaus, and Michael Walker**, “*General equilibrium effects of cash transfers: experimental evidence from Kenya*,” *Econometrica*, 2022, 90 (6), 2603–2643.

**Facchini, Gabriel**, “*Forgetting-by-not-doing: The case of surgeons and cesarean sections*,” *Health Economics*, 2022, 31 (3), 481–495.

**Fafchamps, Marcel, Asad Islam, Debayan Pakrashi, and Denni Tommasi**, “*Diffusion in social networks: Experimental evidence on information sharing vs persuasion*,” *Working Paper*, 2024.

**Foster, Andrew D. and Mark R. Rosenzweig**, “*Learning by Doing and Learning from Others: Human Capital and Technical Change in Agriculture*,” *The Journal of Political Economy*, 1994, 103 (6), 1176–1209.

**Fuller, James A and Joseph NS Eisenberg**, “*Herd protection from drinking water, sanitation, and hygiene interventions*,” *The American journal of tropical medicine and hygiene*, 2016, 95 (5), 1201.

**Halm, Ethan A., Clara Lee, and Mark R. Chassin**, “*Is Volume Related to Outcome in Health Care? A Systematic Review and Methodologic Critique of the Literature*,” *Annals of Internal Medicine*, 2002, 137 (6).

**Hanna, Rema, Sendhil Mullainathan, and Joshua Schwartzstein**, “*Learning Through Noticing: Theory and Evidence from a Field Experiment*,” *The Quarterly Journal of Economics*, 2014, 129 (3), 1311–1353.

**Haushofer, Johannes, Michael Kremer, Ricardo Maertens, and Brandon Joel Tan**, “*Water treatment and child mortality: Evidence from kenya*,” *Technical Report, National Bureau of Economic Research* 2021.

**Hudgens, Michael G. and M. Elizabeth Halloran**, “*Toward Causal Inference With Interference*,” *Journal of the American Statistical Association*, 2008, 103 (482), 832–842.

**Hussam, Reshmaan, Atonu Rabbani, Giovanni Reggiani, and Natalia Rigol**, “*Rational Habit Formation: Experimental Evidence from Handwashing in India*,” *American Economic Journal: Applied Economics*, 2022, 14 (1), 1–41.

**International, Population Services**, “*PSI Impact Calculator*,” <http://impactcalculator.psi.org/>. 2017.

**Khandelwal, Vatsal**, “*Learning in networks with idiosyncratic agents*,” *Games and Economic Behavior*, 2024, 144, 225–249.

**Kondylis, Florence, John Ashton Loeser, Mushfiq Mobarak, Maria Ruth Jones, and Daniel Kevin Stein**, “*Learning from Self and Learning from Others : Experimental Evidence from Bangladesh (English)*.,” *Policy Research working paper* ; no. WPS 10545; *Impact Evaluation series Washington, D.C. : World Bank Group.*, 2023.

**Kremer, Michael, Stephen P Luby, Ricardo Maertens, Brandon Tan, and Witold Wiecek**, “*Water Treatment And Child Mortality: A Meta-Analysis And Cost-effectiveness Analysis*,” *Technical Report, National Bureau of Economic Research* 2023.

**List, John A. and Haruka Uchida**, “*Here Today, Gone Tomorrow? Toward an Understanding of Fade-Out in Early Childhood Education Programs*,” *NBER Working Paper* 33027, 2024.

**Lowes, Sara and Eduardo Montero**, “*The Legacy of Colonial Medicine in Central Africa*,” *American Economic Review*, 2021, 111 (4), 1284–1314.

**Luby, Stephen P, Mubina Agboatwalla, John Painter, Arshad Altaf, Ward Billheimer, Bruce Keswick, and Robert M. Hoekstra**, “*Combining drinking water treatment and hand washing for diarrhoea prevention, a cluster randomised controlled trial*,” *The American Journal of Tropical Medicine and Hygiene*, 2006, 11 (4), 479–489.

**Malmendier, Ulrike and Stefan Nagel**, “*Learning from Inflation Experiences*,” *The Quarterly Journal of Economics*, 2015, 131 (1), 53–87.

**Martinez-Bravo, Monica and Andreas Stegmann**, “*In Vaccines We Trust? The Effects of the CIA’s Vaccine Ruse on Immunization in Pakistan*,” *Journal of the European Economic Association*, 2022, 20 (1), 150–186.

**McKenzie, David**, “*Beyond baseline and follow-up: The case for more T in experiments*,” *Journal of development Economics*, 2012, 99 (2), 210–221.

**Miguel, Edward and Michael Kremer**, “*Worms: Identifying Impacts on Education and Health in the Presence of Treatment Externalities*,” *Econometrica*, 2004, 72, 159–217.

**Morewedge, Carey K**, “*Psychological ownership: implicit and explicit*,” *Current Opinion in Psychology*, 2021, 39, 125–132.

**Muralidharan, Karthik and Venkatesh Sundararaman**, “*Teacher Performance Pay: Experimental Evidence from India*,” *Journal of Political Economy*, 2011, 119 (1), 39–77.

**Neal, David, Jelena Vujcic, Orlando Hernandez, Wendy Wood, J Vujcic, O Hernandez, and W Wood**, “*The science of habit: creating disruptive and sticky behavior change in handwashing behavior*,” *Washington DC, USA. USAID/WASHplus Project*, 2015.

**Perry, Henry B. and Stephen Hodgins**, “*Health for the People: Past, Current, and Future Contributions of National Community Health Worker Programs to Achieving Global Health Goals*,” *Global Health: Science and Practice*, 2021, 9, 1–9.

**Quick, Robert E., Akiko Kimura, Angelica Thevos, Mathias Tembo, Isidore Shamputa, Lori Hutwagner, and Eric Mintz**, “*Diarrhea Prevention through Household-Level Water Disinfection and Safe Storage in Zambia*,” *The American Journal of Tropical Medicine and Hygiene*, 2002, 66 (5), 584–589.

**Quick, Robert E, LV Venczel, ED Mintz, L Soletto, J Aparicio, M Gironaz, L Hutwagner, K Greene, C Bopp, K Maloney et al.**, “*Diarrhoea prevention in Bolivia through point-of-use water treatment and safe storage: a promising new strategy*,” *Epidemiology & Infection*, 1999, 122 (1), 83–90.

**Reller, Megan E, Carlos E Mendoza, M Beatriz Lopez, Maricruz Alvarez, Robert M Hoekstra, Christy A Olson, Kathleen G Baier, Bruce H Keswick, and Stephen P Luby**, “*A randomized controlled trial of household-based flocculant-disinfectant drinking water treatment for diarrhea prevention in rural Guatemala*,” *American Society of Tropical Medicine and Hygiene*, 2003, 69 (4), 411–419.

**Royer, Heather, Mark Stehr, and Justin Sydnor**, “*Incentives, commitments, and habit formation in exercise: evidence from a field experiment with workers at a fortune-500 company*,” *American Economic Journal: Applied Economics*, 2015, 7 (3), 51–84.

**Sazawal, Sunil, AKM Ahsan Habib, Usha Dhingra, Arup Dutta, Pratibha Dhingra, Archana Sarkar, Saikat Deb, Jahangir Alam, Asmaul Husna, and Robert E. Black**, “*Impact of micronutrient fortification of yoghurt on micronutrient status markers and growth – a randomized double blind controlled trial among school children in Bangladesh*,” *BMC Public Health*, 2013, 13 (514).

**Scharf, Rebecca J, Mark D DeBoer, and Richard L Guerrant**, “*Recent advances in understanding the long-term sequelae of childhood infectious diarrhea*,” *Current infectious disease reports*, 2014, 16, 1–7.

**Simonsohn, Uri, Niklas Karlsson, George Loewenstein, and Dan Ariely**, “*The tree of experience in the forest of information: Overweighing experienced relative to observed information*,” *Games and Economic Behavior*, 2008, 62, 263–286.

**Soofi, Sajid Bashir, Gul Nawaz Khan, Shabina Ariff, Yasir Ihtesham, Mahamadou Tanimoune, Arjumand Rizvi, Muhammad Sajid, Cecilia Garzon, Saskia de Pee, and Zulfiqar A. Bhutta**, “Effectiveness of nutritional supplementation during the first 1000-days of life to reduce child undernutrition: A cluster randomized controlled trial in Pakistan,” *The Lancet Regional Health – Southeast Asia*, 2022, 4, 1–11.

**Supply, WHO/UNICEF Joint Water and Sanitation Monitoring Programme**, *Progress on drinking water and sanitation: 2014 Update*, World Health Organization, 2014.

**Tremblay, Maxime A., Céline M. Blanchard, Sara Taylor, Luc G. Pelletier, and Martin Villeneuve**, “Work Extrinsic and Intrinsic Motivation Scale: Its Value for Organizational Psychology Research,” *Canadian Journal of Behavioural Science*, 2009, 41 (4), 213–226.

**UNICEF and WHO**, “*Progress on Sanitation and Drinking Water: 2015 Update and MDG Assessment*,” Geneva, 80 pages, 2015.

**Urminsky, Oleg, Christian Hansen, and Victor Chernozhukov**, “Using double-lasso regression for principled variable selection,” *Working Paper*, 2016.

**Vasilaky, Kathryn N. and Asif M. Islam**, “Competition or cooperation? Using team and tournament incentives for learning among female farmers in rural Uganda,” *World Development*, 2018, 103, 216–225.

— and **Kenneth L. Leonard**, “As Good as the Networks They Keep? Improving Outcomes through Weak Ties in Rural Uganda,” *Economic Development and Cultural Change*, 2018, 66, 755–792.

**Watson, Samuel I, Ryan TT Rego, Timothy Hofer, and Richard J Lilford**, “Evaluations of water, sanitation and hygiene interventions should not use diarrhoea as (primary) outcome,” *BMJ Global Health*, 2022, 7 (5), e008521.

**Wellsjo, Alexandra Steiny**, “Simple Actions, Complex Habits: Lessons from Hospital Hand Hygiene,” *Technical Report, Working paper* 2021.

**WHO**, “Diarrhoeal disease factsheet 2017,” 2017.

**Xu, Zhiwei, Yang Liu, Zongwei Ma, Ghasem (Sam) Toloo, Wenbiao Hu, and Shilu Tong**, “Assessment of the temperature effect on childhood diarrhea using satellite imagery,” *Scientific Reports*, 2014, 4 (5389).

# Online Supplementary Material

## Appendix A Experimental Design Details

*Exclusion Criteria:* Households are excluded if they do not store drinking water in a separate vessel from the water for other uses, if children drink from a separate vessel from adults, if the vessel's capacity is less than 10 liters (to ensure there would be enough water to avoid over-chlorination), or if children frequently drink bottled water (see Figure G.3 for a sample vessel). Of the households we approached, 93.6% were eligible based on these criteria, and 80% of the ineligible households were ineligible on the basis that nobody was home, nobody could regularly be home in the future, or there was no child in the correct age range in the household. Only nineteen households (less than 1% of all households approached) were ineligible based on water infrastructure.

*Diarrhea Prevalence:* We asked respondents to describe their perception of “motions”, or diarrhea. Then, we described diarrhea to respondents as loose or watery stools. For respondents who were not sure what classifies as diarrhea, we also shared a visualization of stool types called the Bristol stool chart. Respondents then reported how many days each child under five years old experienced diarrhea in the past fourteen days. Diarrhea rates were higher than expected during the baseline period. Average rates of diarrhea in May are close to 3% of days per child in the literature, whereas households in our sample experienced diarrhea on 5.9% of child-days ([Luby et al., 2006](#)). There was an extreme heatwave ongoing in Karachi during the baseline period, which we believe explains the high rates of diarrhea ([Xu et al., 2014](#)). Diarrhea rates converged to the expected rate in the following month, without any increase in chlorine use.

*Child Anthropometric Measurements:* Anthropometric outcomes were only recorded at baseline and endline – child weight and mid-upper-arm circumference at baseline and endline, and child height and age at endline only. We calculated the children’s weight as the difference between the weight of the mother alone and the weight of the mother while holding the child. For children older than 6 months, we also collected mid-upper-arm circumference. There were 338 households for whom we did not collect anthropometric data in the baseline visit due to issues with the measurement equipment. As such, we collected anthropometric data for these households in the following visit. We did not reveal treatment status to households until the first Phase 1 visit, and enumerators were blinded to treatment status during the baseline visit, so there is no reason to believe that treatment would differentially affect child anthropometrics between the baseline and the following visit. Furthermore, we visited these households at the beginning of the following visit to minimize the time between the end of the baseline survey and the time of measurement for any anthropometric outcomes that we consider as baseline measures.

## **Appendix B Baseline Balance**

Table B.1: Baseline Balance: Adult Outcomes

Variable	(1) Control Mean/SE	(2) Chlorine-tablets Mean/SE	(3) Incentives Mean/SE	(4) Info-tool Mean/SE	(5) Total Mean/SE	(1)-(2)	T-test Difference (1)-(3)	(1)-(4)
Any Child Had Motions	0.283 (0.021)	0.356 (0.023)	0.304 (0.022)	0.291 (0.021)	0.309 (0.011)	-0.073**	-0.021	-0.008
Reported Highest Diarrhea in Summer	0.836 (0.017)	0.844 (0.017)	0.825 (0.018)	0.820 (0.018)	0.831 (0.009)	-0.008	0.011	0.016
Number of Children <5	1.496 (0.034)	1.517 (0.038)	1.479 (0.031)	1.507 (0.037)	1.499 (0.017)	-0.021	0.017	-0.011
Has Heard of Chlorine	0.188 (0.018)	0.194 (0.019)	0.193 (0.019)	0.240 (0.020)	0.204 (0.009)	-0.006	-0.005	-0.052**
Would consider using chlorine	0.869 (0.016)	0.880 (0.015)	0.887 (0.015)	0.878 (0.015)	0.878 (0.008)	-0.010	-0.017	-0.008
Enumerator Observes Dirt in Water	0.164 (0.017)	0.160 (0.017)	0.173 (0.018)	0.156 (0.017)	0.163 (0.009)	0.003	-0.009	0.008
Reports Dirt in Water	0.752 (0.020)	0.753 (0.020)	0.743 (0.021)	0.751 (0.020)	0.750 (0.010)	-0.001	0.009	0.001
Boils, Bleaches, or Chlorinates Water	0.148 (0.017)	0.131 (0.016)	0.131 (0.016)	0.147 (0.017)	0.139 (0.008)	0.017	0.017	0.002
Strains or Filters Water	0.633 (0.023)	0.657 (0.022)	0.619 (0.023)	0.633 (0.023)	0.635 (0.011)	-0.024	0.014	-0.001
Believes Chlorine is for Water Purification	0.188 (0.018)	0.198 (0.019)	0.220 (0.020)	0.218 (0.019)	0.206 (0.010)	-0.010	-0.031	-0.030
Caretaker Asked about Chlorine Test	0.142 (0.016)	0.176 (0.018)	0.140 (0.016)	0.144 (0.017)	0.150 (0.008)	-0.034	0.002	-0.003
Attrited (endline)	0.106 (0.015)	0.136 (0.016)	0.149 (0.017)	0.113 (0.015)	0.126 (0.008)	-0.030	-0.042**	-0.007
N	452	449	451	450	1802			
F-test of joint significance (F-stat)						0.977	0.748	0.430

Notes: The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. Standard errors are robust. Fixed effects using variable block are included in all estimation regressions. \*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.

Table B.2: Baseline Balance: Child Outcomes

Variable	(1) Control		(2) Chlorine-tablets		(3) Incentives		(4) Info-tool		T-test		
	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	(1)-(2)	(1)-(3)	(1)-(4)
Child Weight (kg)	672 [451]	10.461 (0.111)	675 [448]	10.417 (0.109)	664 [448]	10.621 (0.131)	670 [449]	10.624 (0.111)	0.044	-0.159	-0.163
Child MUAC (cm)	676 [452]	14.504 (0.048)	678 [449]	14.428 (0.042)	666 [450]	14.474 (0.049)	675 [450]	14.546 (0.050)	0.076	0.030	-0.041
Number of Motion Days	676 [452]	0.761 (0.067)	679 [449]	0.928 (0.077)	667 [451]	0.886 (0.080)	672 [450]	0.779 (0.074)	-0.167*	-0.125	-0.019
Child Had > 0 Motion Days	676 [452]	0.214 (0.016)	680 [449]	0.278 (0.019)	667 [451]	0.226 (0.016)	675 [450]	0.230 (0.017)	-0.063***	-0.012	-0.015

Notes: The value displayed for t-tests are the differences in the means across the groups. Standard errors are clustered at variable HHID. Fixed effects using variable block are included in all estimation regressions. \*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.

Table B.3: Treatment Balance: Spillover

	(1)	(2)
	Spillover Sample	Spillover Sample
Any Treatment Group	0.019 (0.025)	
Chlorine Only		0.018 (0.031)
Incentives		0.019 (0.031)
Info-Tool		0.019 (0.031)
Observations	1690	1690

P-values:	
Incentives = Chlorine Only	0.975
Info-Tool = Chlorine Only	0.981
Info-Tool = Incentives	0.995

Standard errors in parentheses

Each observation is at the household level. This regression includes neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## Appendix C Akram and Mendelsohn (2021) Reanalysis

We reanalyze the data from [Akram and Mendelsohn \(2021\)](#), computing the same measure of spillovers that we compute in our data. We find that Info-Tool participants with another Info-Tool participant within twenty meters accept chlorine 17% more often than in other households ([Akram and Mendelsohn \(2021\)](#) used chlorine tablet acceptance as their primary outcome throughout the trial, and only tested water for chlorine residual in the final endline visit.)

Similar to our study, the spillover and non-spillover samples diverge most starkly soon after the period ends where the participants are using the Info-Tool, and after they see the cumulative bar chart (Figure C.1). Also similar to our study, the spillover and non-spillover samples converge towards the end of the trial.

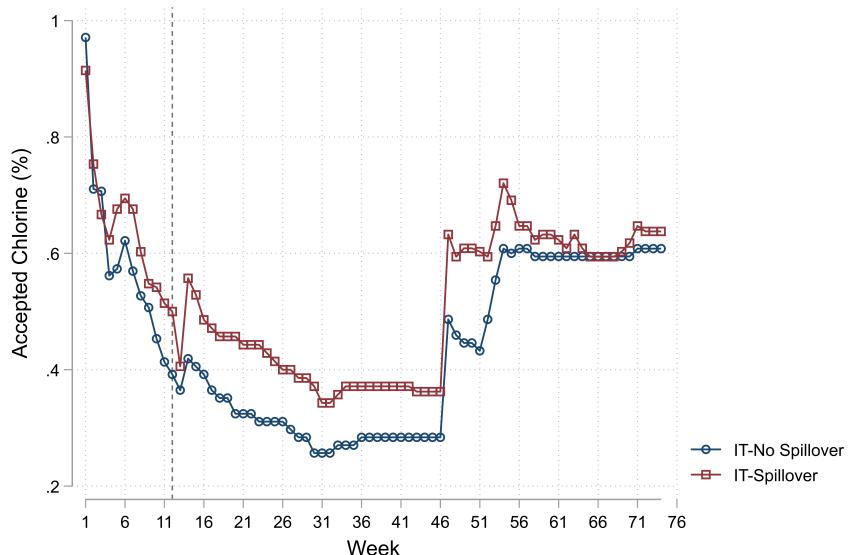


Figure C.1: [Akram and Mendelsohn \(2021\)](#) Reanalysis  
Rates of Chlorine Acceptance  
where “Spillover” is exposure to any Info-Tool neighbor

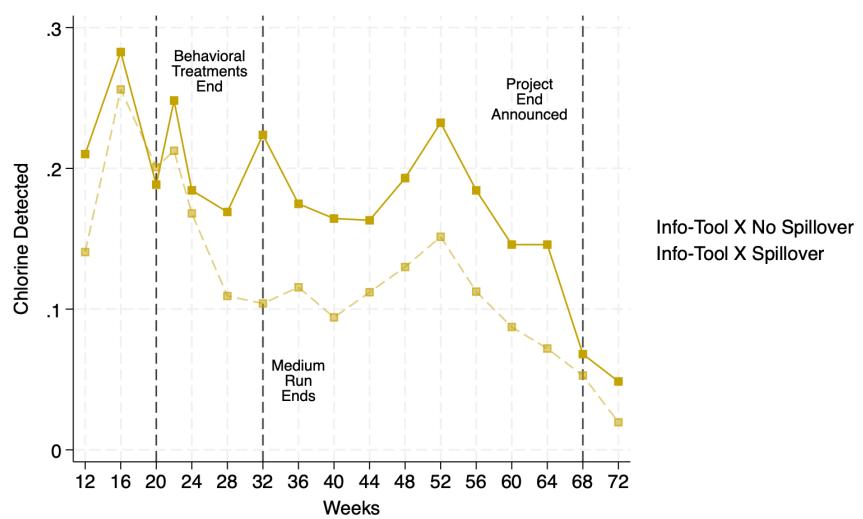


Figure C.2: Our Study  
Rates of Chlorine Detection  
where “Spillover” is exposure to any Info-Tool neighbor

## Appendix D Additional Figures and Tables

Table D.1: Chlorine Detection (Household-Survey Panel)

	(1) Short-Run	(2) Medium-Run	(3) Long-Run
Chlorine Only	0.221*** (0.015)	0.147*** (0.011)	0.108*** (0.009)
Incentives	0.255*** (0.015)	0.142*** (0.010)	0.126*** (0.010)
Info-Tool	0.208*** (0.014)	0.168*** (0.011)	0.122*** (0.009)
Observations	4711	6354	14066
<b>P-values:</b>			
Chlorine = Incentives	0.090	0.689	0.117
Chlorine = Info-Tool	0.484	0.142	0.201
Incentives = Info-Tool	0.014	0.055	0.749

Standard errors in parentheses

Each observation is a household-level survey visit. The outcome is a binary indicator for whether or not chlorine residual was detected in the respondent's water during the survey visit. All specifications include survey-round fixed effects, neighborhood block fixed effects, unbalanced baseline controls, the number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Figure D.1: Raw Moving Averages: Chlorine Detection and Diarrhea Prevalence

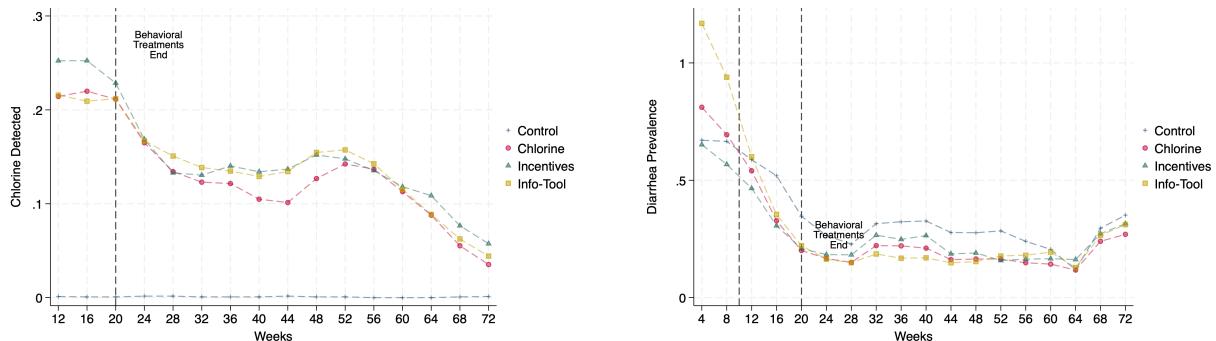


Figure D.2: Raw Rates of Chlorine Detection by Spillover Exposure (Continuous)

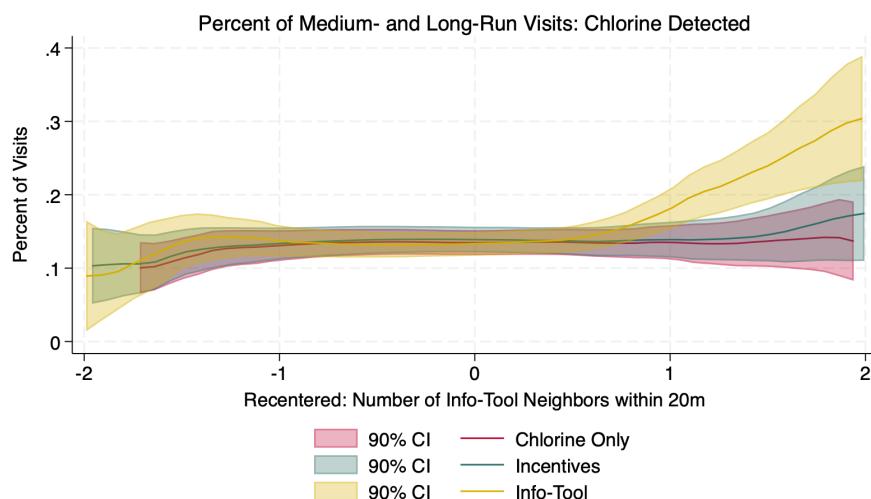


Figure D.3: Heterogeneity: Predicted Health Improvement

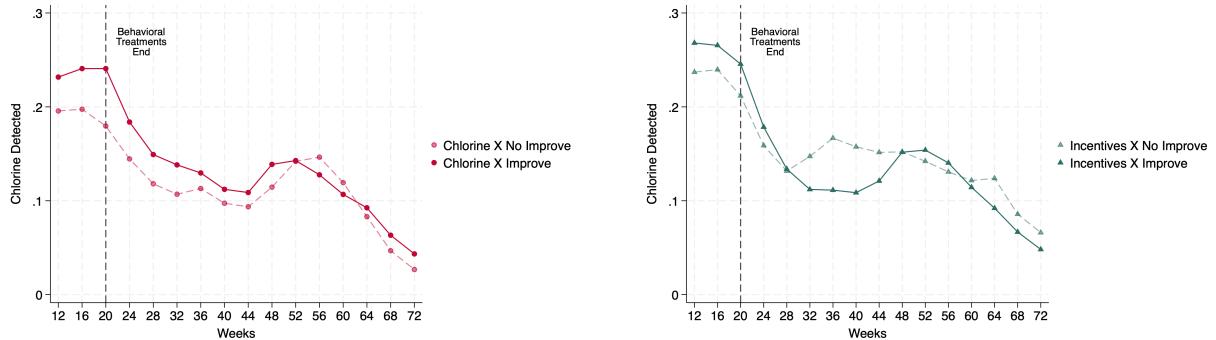


Figure D.4: Heterogeneity: Info-Tool Neighbors (Re-centered)

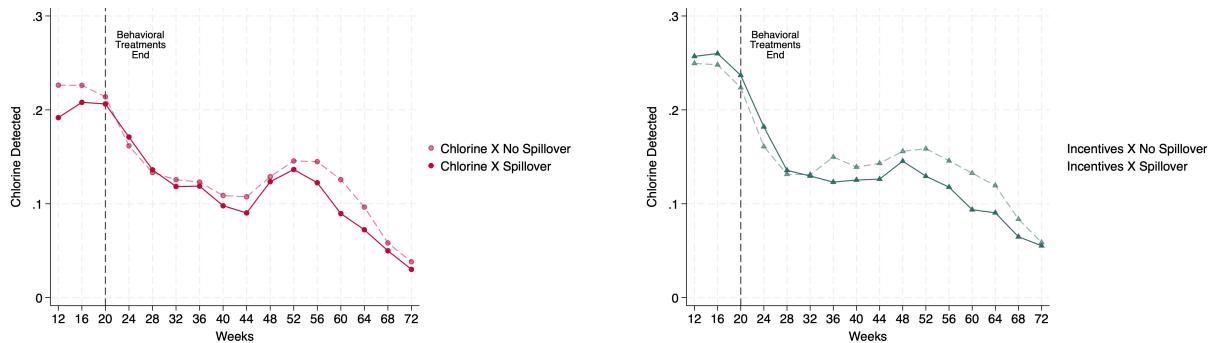


Figure D.5: Heterogeneity: Info-Tool Neighbors (Re-centered)  $\times$  Predicted Health Improvement

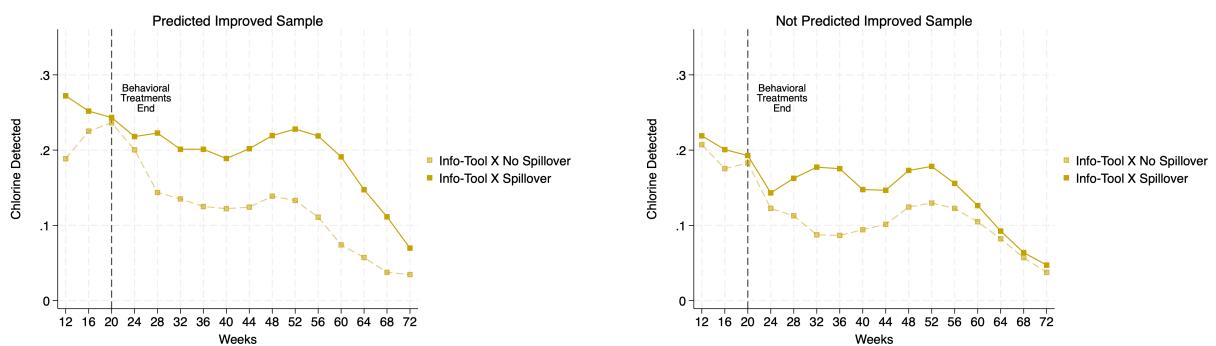


Table D.2: Placebo Test – Chlorine Detection (Household Survey Panel)  
 where “Spillover” is exposure to any Chlorine Only neighbor  
 Omitted group: Chlorine  $\times$  No Spillover

	(1)	(2)	(3)
	Short-Run	Medium-Run	Long-Run
Chlorine $\times$ No Spillover	0.208*** (0.018)	0.133*** (0.013)	0.108*** (0.011)
Chlorine $\times$ Spillover	0.237*** (0.026)	0.169*** (0.017)	0.112*** (0.017)
Incentives $\times$ No Spillover	0.250*** (0.019)	0.136*** (0.012)	0.131*** (0.013)
Incentives $\times$ Spillover	0.255*** (0.025)	0.150*** (0.016)	0.120*** (0.015)
Info-Tool $\times$ No Spillover	0.212*** (0.017)	0.166*** (0.014)	0.127*** (0.012)
Info-Tool $\times$ Spillover	0.193*** (0.022)	0.166*** (0.018)	0.117*** (0.015)
Observations	4711	6354	14066

**P-values:**

Chlorine: No Spillover = Spillover	0.364	0.102	0.831
Incentives: No Spillover = Spillover	0.875	0.456	0.607
Info-Tool: No Spillover = Spillover	0.511	0.999	0.593

Standard errors in parentheses

Each observation is a household-level survey visit. The outcome is a binary indicator for whether or not chlorine residual was detected in the respondent’s water during the survey visit. All specifications include survey-round fixed effects, neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months). The Spillover Sample is defined as participants who were randomly more exposed to a Chlorine Only neighbor (someone within 20m) than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

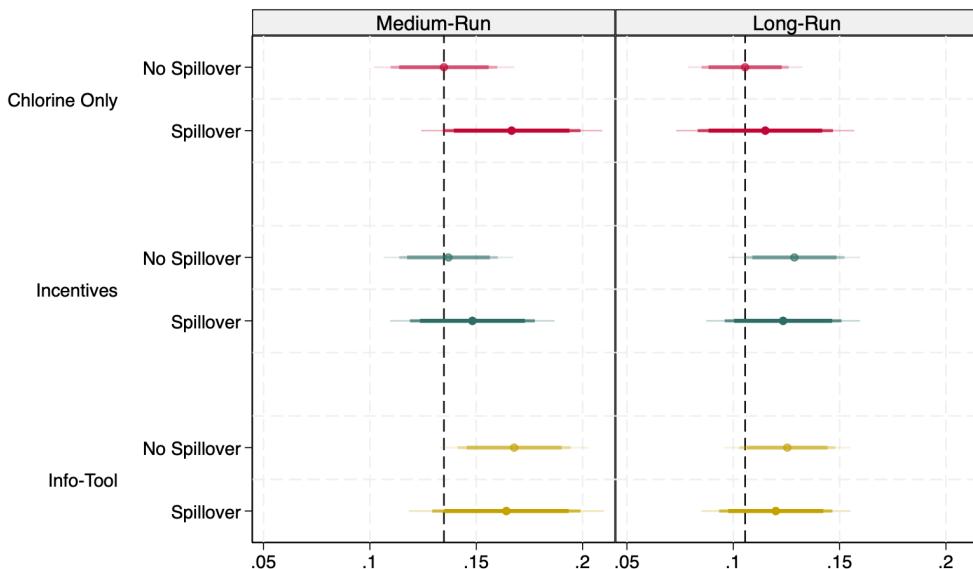


Figure D.6: Placebo Test – Chlorine Detection (Household Survey Panel)  
 where “Spillover” is exposure to any Chlorine Only neighbor

Table D.3: Placebo Test – Chlorine Detection (Panel Specification)  
 where “Spillover” is exposure to any Incentives neighbor  
 Omitted group: Chlorine  $\times$  No Spillover

	(1)	(2)	(3)
	Short-Run	Medium-Run	Long-Run
Chlorine $\times$ No Spillover	0.212*** (0.018)	0.141*** (0.013)	0.101*** (0.011)
Chlorine $\times$ Spillover	0.231*** (0.026)	0.154*** (0.017)	0.123*** (0.018)
Incentives $\times$ No Spillover	0.264*** (0.019)	0.148*** (0.012)	0.135*** (0.013)
Incentives $\times$ Spillover	0.231*** (0.024)	0.130*** (0.015)	0.112*** (0.015)
Info-Tool $\times$ No Spillover	0.194*** (0.016)	0.163*** (0.014)	0.123*** (0.012)
Info-Tool $\times$ Spillover	0.223*** (0.024)	0.172*** (0.019)	0.125*** (0.016)
Observations	4711	6354	14066

**P-values:**

Chlorine: No Spillover = Spillover	0.555	0.564	0.290
Incentives: No Spillover = Spillover	0.291	0.363	0.250
Info-Tool: No Spillover = Spillover	0.326	0.702	0.952

Standard errors in parentheses

Each observation is a household-level survey visit. The outcome is a binary indicator for whether or not chlorine residual was detected in the respondent’s water during the survey visit. All specifications include survey-round fixed effects, neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months). The Spillover Sample is defined as participants who were randomly more exposed to an Incentives neighbor (someone within 20m) than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

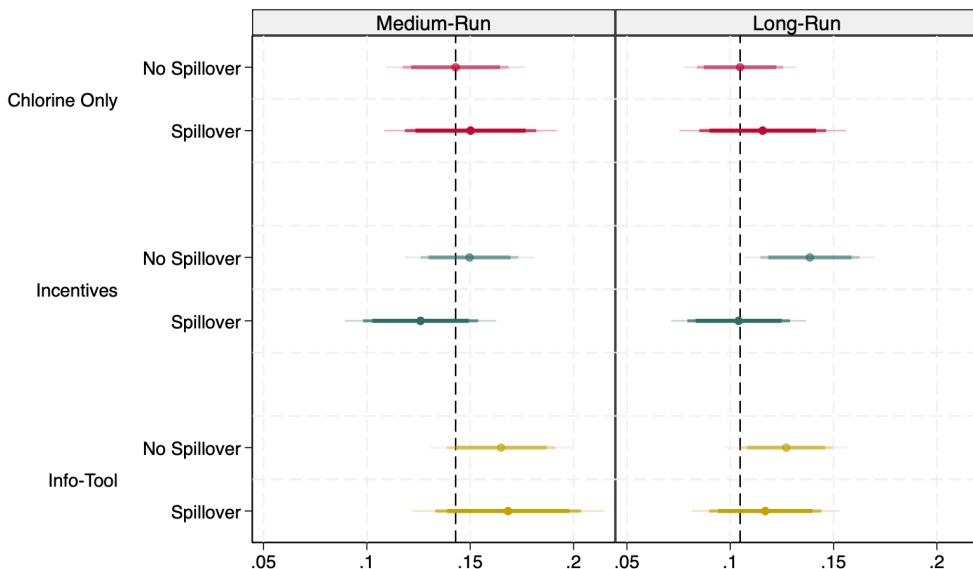


Figure D.7: Placebo Test – Chlorine Detection (Household Survey Panel)  
 where “Spillover” is exposure to any Incentives neighbor

Figure D.8: Regression Coefficients of Chlorine Detection (Omitted: Chlorine Only)

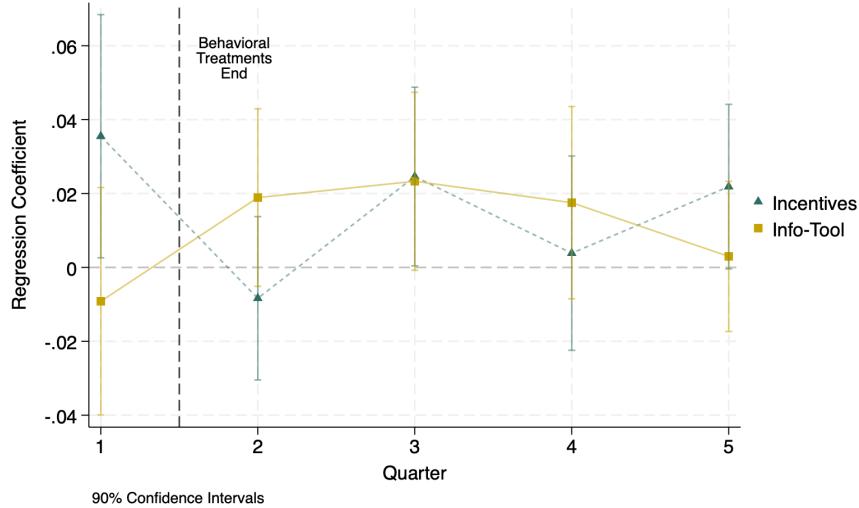


Table D.4: Placebo Test – Chlorine Detection (Household-Survey Panel)  
where “Spillover” is exposure to any treated neighbor predicted to improve  
Omitted group: Chlorine  $\times$  No Spillover

	(1)	Short-Run	(2)	Medium-Run	(3)	Long-Run
Chlorine $\times$ Spillover	0.004	(0.030)	0.008	(0.021)	0.012	(0.016)
Incentives $\times$ No Spillover	0.058**	(0.027)	-0.007	(0.018)	0.030**	(0.015)
Incentives $\times$ Spillover	0.008	(0.028)	-0.002	(0.020)	0.012	(0.017)
Info-Tool $\times$ No Spillover	-0.022	(0.024)	0.013	(0.019)	0.018	(0.014)
Info-Tool $\times$ Spillover	0.014	(0.028)	0.032	(0.023)	0.022	(0.017)
Observations	3463		4689		10472	

**P-values:**

Incentives: No Spillover = Spillover	0.095	0.770	0.275
Info-Tool: No Spillover = Spillover	0.204	0.382	0.845

Standard errors in parentheses

Each observation is a household-level survey visit. The outcome is a binary indicator for whether or not chlorine residual was detected in the respondent's water during the survey visit. All specifications include survey-round fixed effects, neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months). The Spillover Sample is defined as participants who were randomly more exposed to any treatment neighbor (someone within 20m) who was predicted to have their health improve than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined. See Section 6 for a detailed explanation for how the predicted-health-improvement measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.5: Endline Stated Motivations  
Omitted group: Chlorine Only

	(1) Motivation: Health	(2) Motivation: Habit	(3) Motivation: Money	(4) Motivation: Intrinsic
Incentives	0.088 (0.062)	-0.031 (0.064)	-0.020 (0.063)	-0.023 (0.032)
Info-Tool	0.191*** (0.062)	-0.049 (0.064)	-0.068 (0.062)	-0.027 (0.033)
Observations	1049	1057	1051	1058
<b>P-values:</b>				
Incentives = Info-Tool	0.094	0.769	0.444	0.880

Standard errors in parentheses

Each observation is at the household level. The outcome for column (1) is a rating between 1 and 7 for how true the following statement felt: I use chlorine to achieve a standard of health for my family. The outcome for column (2) is a rating between 1 and 7 for how true the following statement felt: Because it has become a fundamental part of my routine. The outcome for column (3) is a rating between 1 and 7 for how true the following statement felt: Because it could allow me to earn money. The outcome for column (4) is the average of the ratings between 1 and 7 for how true each of the following statements felt: Because I derive pleasure from trying new things; Because I want to be very good at taking care of my family, otherwise I would be very disappointed; and For the satisfaction I experience when I am successful at doing difficult tasks. All regressions control for the average motivation score the respondent gave across all motivation questions, the order of questions (randomized), neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.6: Endline Stated Trust  
Omitted group: Pure Control

	(1) Info-Tool Knows Most	(2) Incentives Knows Most	(3) Chlorine Knows Most
Chlorine $\times$ No Spillover	-0.023 (0.034)	-0.105*** (0.038)	0.128*** (0.042)
Chlorine $\times$ Spillover	-0.079* (0.046)	-0.030 (0.053)	0.109* (0.058)
Incentives $\times$ No Spillover	0.001 (0.034)	0.069* (0.038)	-0.070* (0.042)
Incentives $\times$ Spillover	-0.013 (0.046)	0.125** (0.052)	-0.112* (0.058)
Info-Tool $\times$ No Spillover	0.256*** (0.034)	-0.115*** (0.039)	-0.141*** (0.043)
Info-Tool $\times$ Spillover	0.159*** (0.046)	-0.025 (0.052)	-0.134** (0.058)
Observations	1516	1516	1516
<b>P-values:</b>			
Outcome Group: Spillover = No Spillover	0.091	0.394	0.792
<b>DID Estimate:</b>			
Info-Tool Spillover Effect			
– Other Group Spillover Effect		-0.152 [p= 0.110]	-0.078 [p= 0.468]

Standard errors in parentheses

Each observation is at the household level. The outcome for column (1) is an indicator for if the respondent chose a hypothetical Info-tool respondent as the person most likely to be knowledgeable about child health (rather than a Chlorine Only or Incentives participant). The outcome for column (2) is an indicator for if the respondent chose a hypothetical Incentives respondent as the person most likely to be knowledgeable about child health (rather than a Chlorine Only or Info-Tool participant). The outcome for column (3) is an indicator for if the respondent chose a hypothetical Chlorine Only respondent as the person most likely to be knowledgeable about child health (rather than an Incentives or Info-Tool participant). All specifications include neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. The Spillover Sample is defined as participants who were randomly more exposed to an Info-Tool neighbor (someone within 20m) than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.7: Chlorine Detection (Household-Survey Panel)  
where “Spillover” is exposure to any Info-Tool neighbor who filled out the Info-Tool alone  
Omitted group: Chlorine  $\times$  No Spillover

	(1)	(2)	(3)
	Short-Run	Medium-Run	Long-Run
Chlorine $\times$ Spillover	-0.000 (0.031)	0.004 (0.021)	0.028* (0.016)
Incentives $\times$ No Spillover	0.046* (0.025)	-0.009 (0.016)	0.034** (0.014)
Incentives $\times$ Spillover	0.011 (0.031)	-0.001 (0.022)	0.013 (0.017)
Info-Tool $\times$ No Spillover	-0.003 (0.023)	0.006 (0.018)	0.015 (0.013)
Info-Tool $\times$ Spillover	-0.023 (0.030)	0.043* (0.024)	0.043** (0.019)
Observations	3463	4689	10472

P-values:		
Chlorine: No Spillover = Spillover	0.990	0.860
Incentives: No Spillover = Spillover	0.257	0.705
Info-Tool: No Spillover = Spillover	0.480	0.129

Standard errors in parentheses

Each observation is a household-level survey visit. The outcome is a binary indicator for whether or not chlorine residual was detected in the respondent’s water during the survey visit. All specifications include survey-round fixed effects, neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months). The Spillover Sample is defined as participants who were randomly more exposed to an Info-Tool neighbor (someone within 20m) who ever filled out the Info-Tool themselves without enumerator assistance over a two-week period, than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.8: Endline Social Networks

	(1) Number in Social Network	(2) Number Discuss Health	(3) Number Discussed Water Purification
Incentives	-0.051 (0.076)	-0.022 (0.054)	0.114** (0.045)
Info-Tool	-0.119 (0.076)	-0.004 (0.054)	0.005 (0.045)
Observations	1116	1116	1116

P-values:		
Incentives = Info-Tool	0.377	0.730

Standard errors in parentheses

Each observation is at the household level. All specifications include neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.9: Endline Social Networks  
where “Spillover” is exposure to any Info-Tool neighbor

	(1) Number in Social Network	(2) Number Discuss Health	(3) Number Discussed Water Purification
Chlorine × Spillover	0.157 (0.115)	-0.065 (0.081)	0.133** (0.067)
Incentives × No Spillover	-0.019 (0.096)	-0.050 (0.068)	0.148*** (0.056)
Incentives × Spillover	0.046 (0.114)	-0.037 (0.080)	0.186*** (0.067)
Info-Tool × No Spillover	-0.048 (0.097)	0.004 (0.068)	0.037 (0.057)
Info-Tool × Spillover	-0.085 (0.115)	-0.081 (0.081)	0.083 (0.067)
Observations	1116	1116	1116
<b>P-values:</b>			
Incentives × No Spillover = Incentives × Spillover	0.574	0.877	0.576
Info-Tool × No Spillover = Info-Tool × Spillover	0.754	0.297	0.502

Standard errors in parentheses

Each observation is at the household level. All specifications include neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. The Spillover Sample is defined as participants who were randomly more exposed to an Info-Tool neighbor (someone within 20m) than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.10: Midline Memory/Beliefs about Chlorine Efficacy: By Education

	(1) Correct Answer	(2) Went Down	(3) Didn't Change	(4) Went Up	(5) Don't Know
Any Education	-0.128** (0.053)	0.017 (0.048)	0.008 (0.029)	-0.050 (0.042)	0.033** (0.014)
Any Treatment Group	-0.047 (0.036)	0.069** (0.033)	-0.024 (0.020)	-0.032 (0.029)	-0.010 (0.009)
Any Treatment Group × Any Education	0.187*** (0.061)	0.019 (0.055)	-0.033 (0.033)	0.049 (0.048)	-0.041*** (0.016)
Observations	1427	1427	1427	1427	1427
Control Mean (No Education)	0.551	0.650	0.115	0.214	0.019
<b>P-values:</b>					
Any Education +					
Any Treatment Group × Any Education = 0	0.063	0.213	0.154	0.968	0.363

Standard errors in parentheses

Each observation is at the household level. This regression includes neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.11: Mimicry of Other Behaviors

	(1) Take-up of Other Programs: Vitamin A	(2) Take-up of Other Programs: Deworming	(3) Take-up of Other Programs: Iron	(4) Number of Friends Believes Uses Same Savings Technology
Chlorine Only	0.031 (0.031)	0.004 (0.023)	0.011 (0.010)	-0.001 (0.057)
Incentives	0.067** (0.031)	0.001 (0.022)	0.010 (0.010)	0.005 (0.057)
Info-Tool	0.084*** (0.031)	0.004 (0.023)	0.015 (0.010)	-0.114** (0.057)
Observations	1512	1512	1512	1503
<b>P-values:</b>				
Chlorine = Incentives	0.246	0.906	0.889	0.910
Chlorine = Info-Tool	0.085	1.000	0.685	0.051
Incentives = Info-Tool	0.566	0.905	0.584	0.039

Standard errors in parentheses

Each observation is at the household level. All specifications include neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.12: Mimicry of Other Behaviors  
where “Spillover” is exposure to any Info-Tool neighbor

	(1) Take-up of Other Programs: Vitamin A	(2) Take-up of Other Programs: Deworming	(3) Take-up of Other Programs: Iron	(4) Number of Friends Believes Uses Same Savings Technology
Chlorine $\times$ No Spillover	0.069* (0.038)	-0.001 (0.026)	0.013 (0.011)	0.041 (0.065)
Chlorine $\times$ Spillover	-0.041 (0.053)	0.011 (0.032)	0.008 (0.014)	-0.079 (0.080)
Incentives $\times$ No Spillover	0.046 (0.038)	0.017 (0.025)	0.002 (0.011)	0.033 (0.065)
Incentives $\times$ Spillover	0.102* (0.052)	-0.028 (0.031)	0.025* (0.013)	-0.045 (0.080)
Info-Tool $\times$ No Spillover	0.102*** (0.038)	0.016 (0.026)	0.012 (0.011)	-0.086 (0.065)
Info-Tool $\times$ Spillover	0.050 (0.053)	-0.018 (0.032)	0.021 (0.014)	-0.166** (0.080)
Observations	1512	1512	1512	1503
<b>P-values:</b>				
Chlorine $\times$ No Spillover = Chlorine $\times$ Spillover	0.092	0.722	0.689	0.165
Incentives $\times$ No Spillover = Incentives $\times$ Spillover	0.388	0.182	0.104	0.373
Info-Tool $\times$ No Spillover = Info-Tool $\times$ Spillover	0.424	0.314	0.510	0.353

Standard errors in parentheses

Each observation is at the household level. All specifications include neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. The Spillover Sample is defined as participants who were randomly more exposed to an Info-Tool neighbor (someone within 20m) than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.13: Endline Accurate Beliefs About Friends' Water Purification Methods

	(1)	(2)	(3)
	Correct Guess (User or Not: SR)	Correct Guess (User or Not: OB)	Correct Guess (Is a Super User)
Info-Tool Participant	-0.046*** (0.016)	0.011 (0.023)	-0.008 (0.008)
Info-Tool Friend	-0.024 (0.020)	-0.056 (0.036)	-0.007 (0.012)
Info-Tool Participant $\times$ Info-Tool Friend	-0.005 (0.038)	0.044 (0.057)	0.015 (0.020)
Observations	2040	2824	2824
No Info-Tool $\times$ No Info-Tool Friend Mean	0.953	0.440	0.035
<b>P-values:</b>			
Info-Tool Friend + Info-Tool $\times$ Info-Tool Friend = 0	0.378	0.805	0.672

Standard errors in parentheses

Standard errors are clustered at the household level. Each observation is at the network-link level, for observations where the node is within our sample. This regression include neighborhood block fixed effects, unbalanced baseline controls, the number of study participants within twenty meters, lasso-selected baseline controls, participant treatment group fixed effects, and network-link treatment group fixed effects. The outcome in column (1) is an indicator for if the participant's guess about if her friend uses chlorine aligns with what that friend reported using in the endline survey. The outcome in column (2) is an indicator for if the participant's guess about if her friend uses chlorine aligns with what we objectively observed (did we ever detect chlorine in the friend's water). The outcome in column (3) is an indicator for if the participant guessed that her friend uses chlorine tablets for water purification, and we detected chlorine in that participant's water at least three times.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.14: Months Since Water Purification Discussions

	(1)	(2)
	First Water Discussion	Last Water Discussion
Chlorine	0.326 (0.200)	-1.236* (0.703)
Incentives	0.099 (0.157)	-2.404*** (0.715)
Info-Tool	-0.003 (0.165)	-0.987 (0.704)
Observations	1355	1355
Control Mean	1.005	1.005
<b>P-values:</b>		
Incentives = Chlorine Only	0.202	0.114
Info-Tool = Chlorine Only	0.072	0.732
Info-Tool = Incentives	0.471	0.055

Standard errors in parentheses

Each observation is at the household level. This regression include neighborhood block fixed effects, unbalanced baseline controls, the number of study participants within twenty meters, and lasso-selected baseline controls. For links where participants responded that they had never discussed water, we impute with the minimum (0), and with the maximum (24 months).

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.15: Endline Social Norms

	(1)	(2)
	Believes Neighbor Would Accept Chlorinated Water	Number of Friends Believes Chlorinates
Chlorine Only	0.082*** (0.030)	0.041* (0.022)
Incentives	0.139*** (0.030)	0.079*** (0.022)
Info-Tool	0.098*** (0.030)	0.061*** (0.022)
Observations	1503	1690
<b>P-values:</b>		
Chlorine = Incentives	0.061	0.073
Chlorine = Info-Tool	0.601	0.347
Incentives = Info-Tool	0.176	0.403

Standard errors in parentheses

Each observation is at the household level. All specifications include neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.16: Endline Social Norms  
where “Spillover” is exposure to any Info-Tool neighbor

	(1) Believes Neighbor Would Accept Chlorinated Water	(2) Number of Friends Believes Chlorinates
Chlorine × No Spillover	0.079** (0.037)	0.041* (0.025)
Chlorine × Spillover	0.088* (0.051)	0.040 (0.030)
Incentives × No Spillover	0.157*** (0.037)	0.072*** (0.025)
Incentives × Spillover	0.109** (0.050)	0.092*** (0.030)
Info-Tool × No Spillover	0.060 (0.037)	0.049** (0.025)
Info-Tool × Spillover	0.164*** (0.051)	0.082*** (0.030)
Observations	1503	1690

**P-values:**

Chlorine × No Spillover = Chlorine × Spillover	0.892	0.969
Incentives × No Spillover = Incentives × Spillover	0.445	0.531
Info-Tool × No Spillover = Info-Tool × Spillover	0.100	0.316

Standard errors in parentheses

Each observation is at the household level. All specifications include neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. The Spillover Sample is defined as participants who were randomly more exposed to an Info-Tool neighbor (someone within 20m) than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.17: Endline Willingness-to-Pay

	(1) Purchased Tablets	(2) Price Accepted	(3) Hypothetical WTP	(4) Sold or Revisited
Chlorine Only	-0.017* (0.010)	-2.472* (1.444)	0.124 (1.472)	-0.044** (0.019)
Incentives	-0.010 (0.010)	-1.693 (1.448)	0.215 (1.482)	-0.037* (0.019)
Info-Tool	0.004 (0.010)	0.288 (1.444)	0.598 (1.494)	-0.020 (0.019)
Observations	1503	1503	1099	1503
Control Mean	0.027	4.059	5.217	0.106

**P-values:**

Incentives = Chlorine Only	0.474	0.594	0.949	0.685
Info-Tool = Chlorine Only	0.037	0.058	0.742	0.204
Info-Tool = Incentives	0.172	0.175	0.791	0.389

Standard errors in parentheses

Each observation is at the household level. This regression includes neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. The outcome for column (1) is an indicator for if the household ultimately purchased chlorine tablets. The outcome for column (2) is the price at which the household purchased chlorine tablets (the price is 0 for households who did not purchase tablets). The outcome for column (3) is a hypothetical price at which the household said they would be willing to purchase chlorine tablets, if they refused the offered price. The outcome for column (4) is an indicator for if the household purchased the chlorine tablets *or* asked the enumerator to return at a later date when they expected to have cash on hand.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table D.18: Endline Willingness-to-Pay

	(1) Purchased Tablets	(2) Price Accepted	(3) Hypothetical WTP	(4) Sold or Revisited
Chlorine × No Spillover	-0.019 (0.012)	-2.603 (1.802)	-2.103 (1.859)	-0.048** (0.024)
Chlorine × Spillover	-0.013 (0.017)	-2.233 (2.460)	3.970 (2.449)	-0.036 (0.032)
Incentives × No Spillover	-0.007 (0.013)	-1.115 (1.808)	-0.408 (1.867)	-0.064*** (0.024)
Incentives × Spillover	-0.014 (0.017)	-2.681 (2.442)	1.200 (2.437)	0.013 (0.032)
Info-Tool × No Spillover	-0.002 (0.012)	-0.101 (1.803)	-0.838 (1.884)	-0.025 (0.024)
Info-Tool × Spillover	0.014 (0.017)	0.973 (2.453)	3.063 (2.462)	-0.010 (0.032)
Observations	1503	1503	1099	1503
Control Mean	0.027	4.059	5.217	0.106
<b>P-values:</b>				
Chlorine: No Spillover = Spillover	0.779	0.904	0.050	0.773
Incentives: No Spillover = Spillover	0.736	0.607	0.601	0.055
Info-Tool: No Spillover = Spillover	0.460	0.726	0.210	0.712

Standard errors in parentheses

Each observation is at the household level. This regression includes neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. The outcome for column (1) is an indicator for if the household ultimately purchased chlorine tablets. The outcome for column (2) is the price at which the household purchased chlorine tablets (the price is 0 for households who did not purchase tablets). The outcome for column (3) is a hypothetical price at which the household said they would be willing to purchase chlorine tablets, if they refused the offered price. The outcome for column (4) is an indicator for if the household purchased the chlorine tablets *or* asked the enumerator to return at a later date when they expected to have cash on hand.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## Appendix E Main Tables: Robustness to Alternate Specification

For robustness, we also estimate the following specification using household-survey-visit panel specification:

$$\begin{aligned}
Y_i = & \theta_0 + \theta_{1,r}T1_i + \theta_{2,r}T2_i + \sum_{r=20}^R \theta_{3,r}AnyT2_i^r \\
& + \sum_{r=20}^R \theta_{4,r}AnyT2_i^r \times T1_i + \sum_{r=20}^R \theta_{5,r}AnyT2_i^r \times T2_i \\
& + Y_{i0} + X_{i0} + \gamma_b + \epsilon_i
\end{aligned}$$

where  $Y_{i,s}$  is an outcome for individual  $i$  measured in survey-round  $s$ ;  $T1_i$ ,  $T2_i$ , and  $T3_i$  are binary variables representing Chlorine Only, Info-Tool, and Incentive group participants, respectively;  $Y_{i0}$  is the baseline measurement of the outcome (whenever available);  $X_{i0}$  is a vector of unbalanced baseline covariates, the density of study participants within twenty meters<sup>46</sup>, and other covariates that we select using the double-lasso method proposed by [Urminsky et al. \(2016\)](#);  $\delta_s$  denotes survey-round fixed effects; and  $\gamma_b$  are block fixed effects (geographic units of stratification). We cluster standard errors at the household level.<sup>47</sup>

Table E.1: Chlorine Detection: Panel Specification  
where “Spillover” is exposure to any Info-Tool neighbor

	(1)	(2)	
	Short-Run	Long-Run	
Chlorine × Spillover	-0.002 (0.030)	0.007 (0.014)	
Info-Tool × No Spillover	-0.016 (0.024)	0.005 (0.013)	
Info-Tool × Spillover	-0.010 (0.028)	0.041** (0.017)	
Observations	3534	15341	
Chlorine No Spillover Mean	0.227	0.127	
<b>P-values:</b>			
Info-Tool: No Spillover = Spillover	0.834	0.037	

Standard errors in parentheses

Each observation is a household-level survey visit. The outcome is a binary indicator for whether or not chlorine residual was detected in the respondent's water during the survey visit. The Chlorine Only treatment group without spillover neighbors is the omitted group. An indicator for Pure Control participants, who are included in the sample, is in the estimating equation but not reported. All specifications include survey-round fixed effects, neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Long-run is defined as the remainder of the trial (twelve months). The Spillover Sample is defined as participants who were randomly more exposed to an Info-Tool neighbor (someone within 20m) than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

<sup>46</sup>Section 4.1 details the selection of this bandwidth following [Egger et al. \(2022\)](#).

<sup>47</sup>If a participant could not be found or refused to let us test their water in any given survey round, we consider this observation as an attriter and drop it from the analysis.

Table E.2: Chlorine Detection: Panel Specification  
by predicted health improvement

	(1)	(2)	
	Short-Run	Long-Run	
Chlorine × Improved	0.031 (0.029)	0.021 (0.014)	
Info-Tool × Not Improved	-0.028 (0.026)	0.018 (0.014)	
Info-Tool × Improved	0.028 (0.028)	0.036** (0.014)	
Observations	2286	10082	

P-values:		
Info-Tool: Not Improved = Improved	0.043	0.229

Standard errors in parentheses

Each observation is a household-level survey visit. The outcome is a binary indicator for whether or not chlorine residual was detected in the respondent's water during the survey visit. The Chlorine Only treatment group without spillover neighbors is the omitted group. An indicator for Pure Control participants, who are included in the sample, is in the estimating equation but not reported. All specifications include survey-round fixed effects, neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months). Improved is a binary indicator for if the participant's predicted improvement in health after the beginning of chlorine distribution was above the median. See Section 6 for a detailed explanation for how the predicted-health-improvement measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table E.3: Chlorine Detection: Panel Specification  
where “Spillover” is exposure to any Info-Tool neighbor predicted to improve

	(1)	(2)	
	Short-Run	Medium-Run	
Chlorine × Spillover	-0.029 (0.033)	0.017 (0.015)	
Info-Tool × No Spillover	-0.024 (0.022)	0.012 (0.011)	
Info-Tool × Spillover	-0.007 (0.031)	0.043** (0.020)	
Observations	3534	10082	

P-values:		
Info-Tool: No Spillover = Spillover	0.581	0.111

Standard errors in parentheses

Each observation is a household-level survey visit. The outcome is a binary indicator for whether or not chlorine residual was detected in the respondent's water during the survey visit. The Chlorine Only treatment group without spillover neighbors is the omitted group. An indicator for Pure Control participants, who are included in the sample, is in the estimating equation but not reported. All specifications include survey-round fixed effects, neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, lasso-selected baseline controls, and indicators for being in the Pure Control group with or without a learning-arm neighbor. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months). The Spillover Sample is defined as participants who were randomly more exposed to an Info-Tool neighbor (someone within 20m) whose health was predicted to improve than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined. See Section 6 for a detailed explanation for how the predicted-health-improvement measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table E.4: Chlorine Detection: Aggregate  
where “Spillover” is exposure to any treatment neighbor predicted to improve

	(1)	(2)	(3)	
	Short-Run	Medium-Run	Long-Run	
Chlorine × Spillover	-0.006 (0.084)	0.032 (0.075)	0.163 (0.134)	
Incentives × No Spillover	0.185** (0.074)	-0.039 (0.067)	0.269** (0.120)	
Incentives × Spillover	0.006 (0.080)	-0.001 (0.072)	0.083 (0.130)	
Info-Tool × No Spillover	-0.054 (0.073)	0.034 (0.066)	0.168 (0.118)	
Info-Tool × Spillover	0.031 (0.083)	0.125* (0.076)	0.180 (0.136)	
Observations	1188	1261	1261	
<b>P-values:</b>				
Chlorine: No Spillover = Spillover	0.939	0.670	0.225	
Incentives: No Spillover = Spillover	0.028	0.608	0.162	
Info-Tool: No Spillover = Spillover	0.313	0.231	0.930	

Standard errors in parentheses

Each observation is at the household level. The outcome is the aggregate of the total number of times that chlorine was detected across all visits in the specified time period. All specifications include a control for the number of visits that the household was surveyed during the specified time period, neighborhood block fixed effects, unbalanced baseline controls, the total number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months). The Spillover Sample is defined as participants who were randomly more exposed to any treatment neighbor (someone within 20m) who was predicted to have their health improve than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined. See Section 6 for a detailed explanation for how the predicted-health-improvement measure is defined.

\*  $p < .1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table E.5: Chlorine Detection: Aggregate

	(1)	(2)	(3)	
	Short-Run	Medium-Run	Long-Run	
Chlorine Only	0.641*** (0.050)	0.578*** (0.046)	0.933*** (0.090)	
Incentives	0.743*** (0.049)	0.545*** (0.046)	1.085*** (0.089)	
Info-Tool	0.622*** (0.050)	0.647*** (0.046)	1.059*** (0.089)	
Observations	1599	1661	1653	
<b>P-values:</b>				
Chlorine = Incentives	0.039	0.490	0.091	
Chlorine = Info-Tool	0.706	0.139	0.162	
Incentives = Info-Tool	0.015	0.029	0.770	

Standard errors in parentheses

Each observation is at the household level. The outcome is the aggregate of the total number of times that chlorine was detected across all visits in the specified time period. All specifications include a control for the number of visits that the household was surveyed during the specified time period, neighborhood block fixed effects, unbalanced baseline controls, the number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table E.6: Chlorine Acceptance

	(1) Short-Run	(2) Medium-Run	(3) Long-Run
Incentives	0.069*** (0.023)	0.080*** (0.024)	0.063** (0.028)
Info-Tool	0.020 (0.024)	0.016 (0.026)	0.023 (0.029)
Observations	5545	4609	10455
Chlorine-only Mean	0.794	0.756	0.740

Standard errors in parentheses

Each observation is a household-level survey visit. The outcome is a binary indicator for whether or not the respondent accepted free chlorine during the survey visit. All specifications include survey-round fixed effects, neighborhood block fixed effects, unbalanced baseline controls, the number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## **Appendix F Baseline Balance: Spillover and Endline Sample**

Table F.1: Baseline Balance: Spillover Sample

Variable	(1) No Spillover Mean/SE	(2) Spillover Sample Mean/SE	(3) Total Mean/SE	T-test Difference (1)-(2)
Any Child Had Motions	0.295 (0.013)	0.332 (0.019)	0.309 (0.011)	-0.036
Reported Highest Diarrhea in Summer	0.817 (0.011)	0.856 (0.014)	0.831 (0.009)	-0.039
Number of Children <5	1.491 (0.020)	1.514 (0.033)	1.499 (0.017)	-0.023
Has Heard of Chlorine	0.212 (0.012)	0.188 (0.015)	0.204 (0.009)	0.024
Would consider using chlorine	0.872 (0.010)	0.890 (0.012)	0.878 (0.008)	-0.019
Enumerator Observes Dirt in Water	0.164 (0.011)	0.162 (0.014)	0.163 (0.009)	0.002
Reports Dirt in Water	0.750 (0.013)	0.750 (0.017)	0.750 (0.010)	-0.000
Boils, Bleaches, or Chlorinates Water	0.143 (0.010)	0.133 (0.013)	0.139 (0.008)	0.010
Strains or Filters Water	0.620 (0.014)	0.662 (0.019)	0.635 (0.011)	-0.042
Believes Chlorine is for Water Purification	0.217 (0.012)	0.187 (0.015)	0.206 (0.010)	0.030
Caretaker Asked about Chlorine Test	0.143 (0.010)	0.164 (0.015)	0.150 (0.008)	-0.021
Attrited (endline)	0.125 (0.010)	0.128 (0.013)	0.126 (0.008)	-0.003
N	1154	648	1802	
F-test of joint significance (F-stat)				1.110

Notes: The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. Standard errors are robust. Fixed effects using variable block are included in all estimation regressions. \*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.

Table F.2: Baseline Balance: Spillover Sample (Info-Tool Only)

Variable	(1) Info-Tool (no IT neighbor) Mean/SE	(2) Info-Tool (with IT neighbor) Mean/SE	(3) Total Mean/SE	T-test Difference (1)-(2)
Any Child Had Motions	0.253 (0.026)	0.358 (0.037)	0.291 (0.021)	-0.105**
Reported Highest Diarrhea in Summer	0.793 (0.024)	0.867 (0.027)	0.820 (0.018)	-0.074
Number of Children <5	1.498 (0.038)	1.521 (0.076)	1.507 (0.037)	-0.023
Has Heard of Chlorine	0.235 (0.025)	0.248 (0.034)	0.240 (0.020)	-0.013
Would consider using chlorine	0.867 (0.020)	0.897 (0.024)	0.878 (0.015)	-0.030
Enumerator Observes Dirt in Water	0.172 (0.022)	0.127 (0.026)	0.156 (0.017)	0.045
Reports Dirt in Water	0.754 (0.026)	0.745 (0.034)	0.751 (0.020)	0.009
Boils, Bleaches, or Chlorinates Water	0.126 (0.020)	0.182 (0.030)	0.147 (0.017)	-0.056
Strains or Filters Water	0.618 (0.029)	0.661 (0.037)	0.633 (0.023)	-0.043
Believes Chlorine is for Water Purification	0.239 (0.025)	0.182 (0.030)	0.218 (0.019)	0.057
Caretaker Asked about Chlorine Test	0.123 (0.019)	0.182 (0.030)	0.144 (0.017)	-0.059
Attrited (endline)	0.105 (0.018)	0.127 (0.026)	0.113 (0.015)	-0.022
N	285	165	450	
F-test of joint significance (F-stat)				1.382

Notes: The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. Standard errors are robust. Fixed effects using variable block are included in all estimation regressions. \*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.

Table F.3: Baseline Balance: Spillover Sample (Incentives Only)

Variable	(1) Incentives (no IT neighbor) Mean/SE	(2) Incentives (with IT neighbor) Mean/SE	(3) Total Mean/SE	T-test Difference (1)-(2)
Any Child Had Motions	0.315 (0.028)	0.285 (0.035)	0.304 (0.022)	0.030
Reported Highest Diarrhea in Summer	0.836 (0.022)	0.806 (0.031)	0.825 (0.018)	0.030
Number of Children <5	1.486 (0.040)	1.467 (0.050)	1.479 (0.031)	0.019
Has Heard of Chlorine	0.220 (0.025)	0.145 (0.028)	0.193 (0.019)	0.075*
Would consider using chlorine	0.895 (0.018)	0.873 (0.026)	0.887 (0.015)	0.022
Enumerator Observes Dirt in Water	0.185 (0.023)	0.152 (0.028)	0.173 (0.018)	0.034
Reports Dirt in Water	0.773 (0.025)	0.691 (0.036)	0.743 (0.021)	0.082
Boils, Bleaches, or Chlorinates Water	0.140 (0.021)	0.115 (0.025)	0.131 (0.016)	0.025
Strains or Filters Water	0.612 (0.029)	0.630 (0.038)	0.619 (0.023)	-0.018
Believes Chlorine is for Water Purification	0.234 (0.025)	0.194 (0.031)	0.220 (0.020)	0.040
Caretaker Asked about Chlorine Test	0.133 (0.020)	0.152 (0.028)	0.140 (0.016)	-0.019
Attrited (endline)	0.154 (0.021)	0.139 (0.027)	0.149 (0.017)	0.014
N	286	165	451	
F-test of joint significance (F-stat)				0.776

Notes: The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. Standard errors are robust. Fixed effects using variable block are included in all estimation regressions. \*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.

Table F.4: Baseline Balance: Spillover Sample (Chlorine Only)

Variable	(1) Chlorine (no IT neighbor) Mean/SE	(2) Chlorine (with IT neighbor) Mean/SE	(3) Total Mean/SE	T-test Difference (1)-(2)
Any Child Had Motions	0.338 (0.028)	0.388 (0.038)	0.356 (0.023)	-0.050
Reported Highest Diarrhea in Summer	0.835 (0.022)	0.861 (0.027)	0.844 (0.017)	-0.026
Number of Children <5	1.475 (0.042)	1.588 (0.071)	1.517 (0.038)	-0.113
Has Heard of Chlorine	0.190 (0.023)	0.200 (0.031)	0.194 (0.019)	-0.010
Would consider using chlorine	0.870 (0.020)	0.897 (0.024)	0.880 (0.015)	-0.027
Enumerator Observes Dirt in Water	0.137 (0.020)	0.200 (0.031)	0.160 (0.017)	-0.063
Reports Dirt in Water	0.739 (0.026)	0.776 (0.033)	0.753 (0.020)	-0.036
Boils, Bleaches, or Chlorinates Water	0.144 (0.021)	0.109 (0.024)	0.131 (0.016)	0.035
Strains or Filters Water	0.641 (0.029)	0.685 (0.036)	0.657 (0.022)	-0.044
Believes Chlorine is for Water Purification	0.180 (0.023)	0.230 (0.033)	0.198 (0.019)	-0.051
Caretaker Asked about Chlorine Test	0.165 (0.022)	0.194 (0.031)	0.176 (0.018)	-0.028
Attrited (endline)	0.123 (0.020)	0.158 (0.028)	0.136 (0.016)	-0.034
N	284	165	449	
F-test of joint significance (F-stat)				0.636

Notes: The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. Standard errors are robust. Fixed effects using variable block are included in all estimation regressions. \*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.

Table F.5: Baseline Balance: Adult Outcomes (Endline Sample)

Variable	(1) Control Mean/SE	(2) Chlorine-tablets Mean/SE	(3) Incentives Mean/SE	(4) Info-tool Mean/SE	(5) Total Mean/SE	T-test Difference (1)-(2)	T-test Difference (1)-(3)	T-test Difference (1)-(4)
Any Child Had Motions	0.285 (0.022)	0.345 (0.024)	0.292 (0.023)	0.278 (0.022)	0.300 (0.012)	-0.061*	-0.007	0.006
Reported Highest Diarrhea in Summer	0.837 (0.018)	0.832 (0.019)	0.820 (0.020)	0.835 (0.019)	0.831 (0.009)	0.004	0.016	0.002
Number of Children <5	1.493 (0.035)	1.497 (0.039)	1.479 (0.034)	1.511 (0.039)	1.495 (0.018)	-0.005	0.013	-0.019
Has Heard of Chlorine	0.200 (0.020)	0.204 (0.020)	0.201 (0.020)	0.238 (0.021)	0.211 (0.010)	-0.003	-0.000	-0.038
Would consider using chlorine	0.866 (0.017)	0.879 (0.017)	0.880 (0.017)	0.872 (0.017)	0.874 (0.008)	-0.013	-0.014	-0.006
Enumerator Observes Dirt in Water	0.146 (0.018)	0.155 (0.018)	0.169 (0.019)	0.158 (0.018)	0.157 (0.009)	-0.009	-0.023	-0.012
Reports Dirt in Water	0.748 (0.022)	0.747 (0.022)	0.758 (0.022)	0.752 (0.022)	0.751 (0.011)	0.000	-0.010	-0.004
Boils, Bleaches, or Chlorinates Water	0.141 (0.017)	0.121 (0.017)	0.130 (0.017)	0.148 (0.018)	0.135 (0.009)	0.020	0.011	-0.007
Strains or Filters Water	0.651 (0.024)	0.660 (0.024)	0.622 (0.025)	0.639 (0.024)	0.643 (0.012)	-0.009	0.029	0.012
Believes Chlorine is for Water Purification	0.198 (0.020)	0.196 (0.020)	0.232 (0.022)	0.216 (0.021)	0.210 (0.010)	0.002	-0.034	-0.018
Caretaker Asked about Chlorine Test	0.136 (0.017)	0.173 (0.019)	0.143 (0.018)	0.140 (0.017)	0.148 (0.009)	-0.037	-0.007	-0.004
N	404	388	384	399	1575			
F-test of joint significance (F-stat)						0.640	0.606	0.256

Notes: The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. Standard errors are robust. Fixed effects using variable block are included in all estimation regressions. \*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.

Table F.6: Baseline Balance: Child Outcomes (Endline Sample)

Variable	(1) Control		(2) Chlorine-tablets		(3) Incentives		(4) Info-tool		T-test Difference		
	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	(1)-(2)	(1)-(3)	(1)-(4)
Female	572 [386]	0.479 (0.022)	541 [370]	0.457 (0.024)	541 [370]	0.481 (0.022)	565 [383]	0.485 (0.022)	0.022	-0.002	-0.006
Age (in months)	568 [384]	30.885 (0.584)	539 [370]	30.638 (0.563)	539 [368]	30.226 (0.600)	564 [383]	31.599 (0.605)	0.247	0.659	-0.715
Child Weight (kg)	568 [384]	10.562 (0.119)	539 [370]	10.581 (0.122)	539 [368]	10.674 (0.129)	564 [383]	10.691 (0.120)	-0.019	-0.112	-0.130
Child MUAC (cm)	572 [386]	14.345 (0.053)	540 [370]	14.274 (0.049)	542 [371]	14.317 (0.057)	565 [383]	14.395 (0.058)	0.072	0.028	-0.050
Number of Motion Days	572 [386]	0.769 (0.074)	541 [370]	0.852 (0.080)	542 [371]	0.769 (0.081)	565 [383]	0.796 (0.084)	-0.083	-0.001	-0.027
Child Had > 0 Motion Days	572 [386]	0.215 (0.018)	541 [370]	0.262 (0.020)	542 [371]	0.218 (0.018)	565 [383]	0.219 (0.018)	-0.047*	-0.003	-0.004

Notes: The value displayed for t-tests are the differences in the means across the groups. Standard errors are clustered at variable HHID. Fixed effects using variable block are included in all estimation regressions. \*\*\*, \*\*, and \* indicate significance at the 1, 5, and 10 percent critical level.

## Appendix G Photos of Intervention Materials

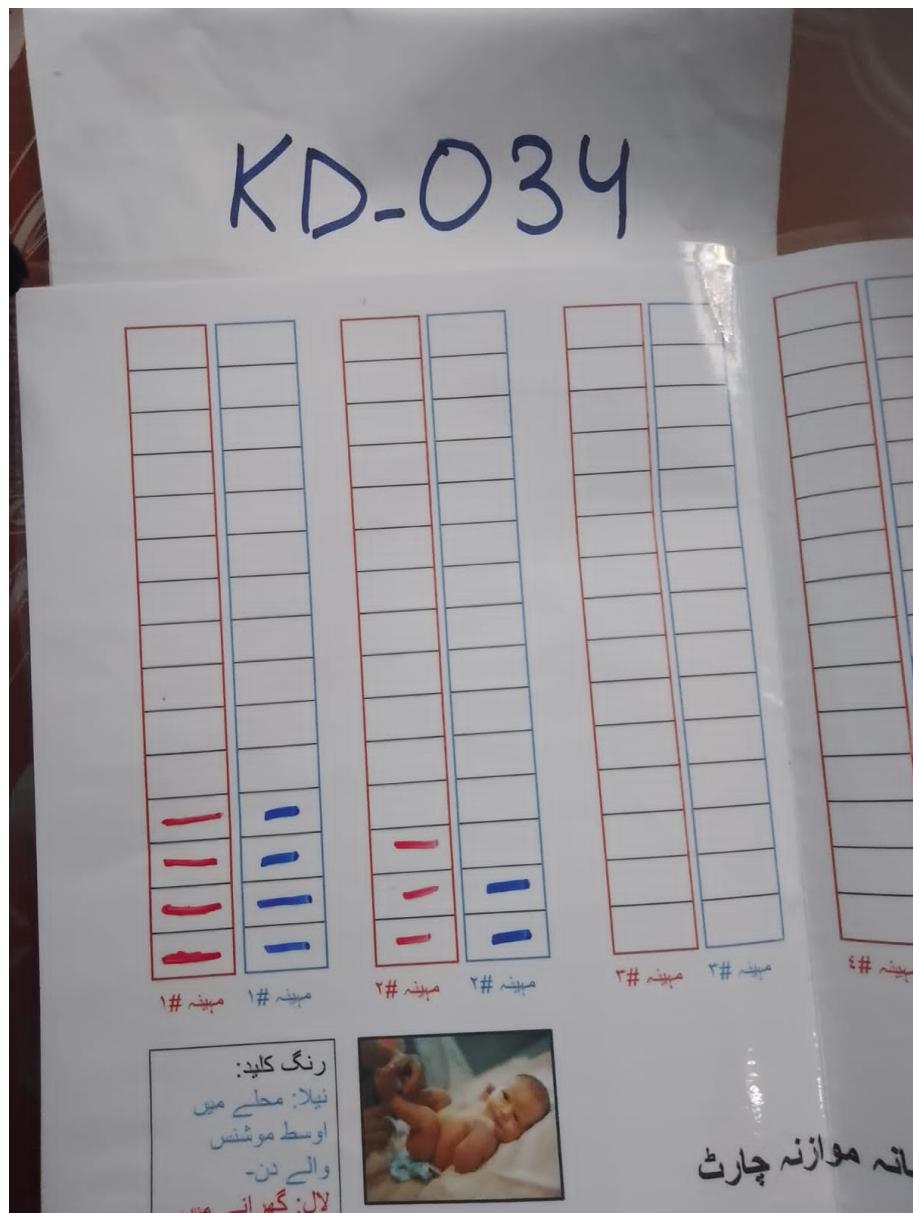


Figure G.1: Experiential Learning Intervention: Info-Tool

Red represents a household's own diarrhea rate, that they fill in on their own in the two weeks between visits. If they did not fill in the data during the previous two weeks, the CHW helps them fill it in retrospectively. During these bi-weekly visits, the CHW also fills in the blue bar with the average rate among people who do not use chlorine (from Luby et al. (2006)).

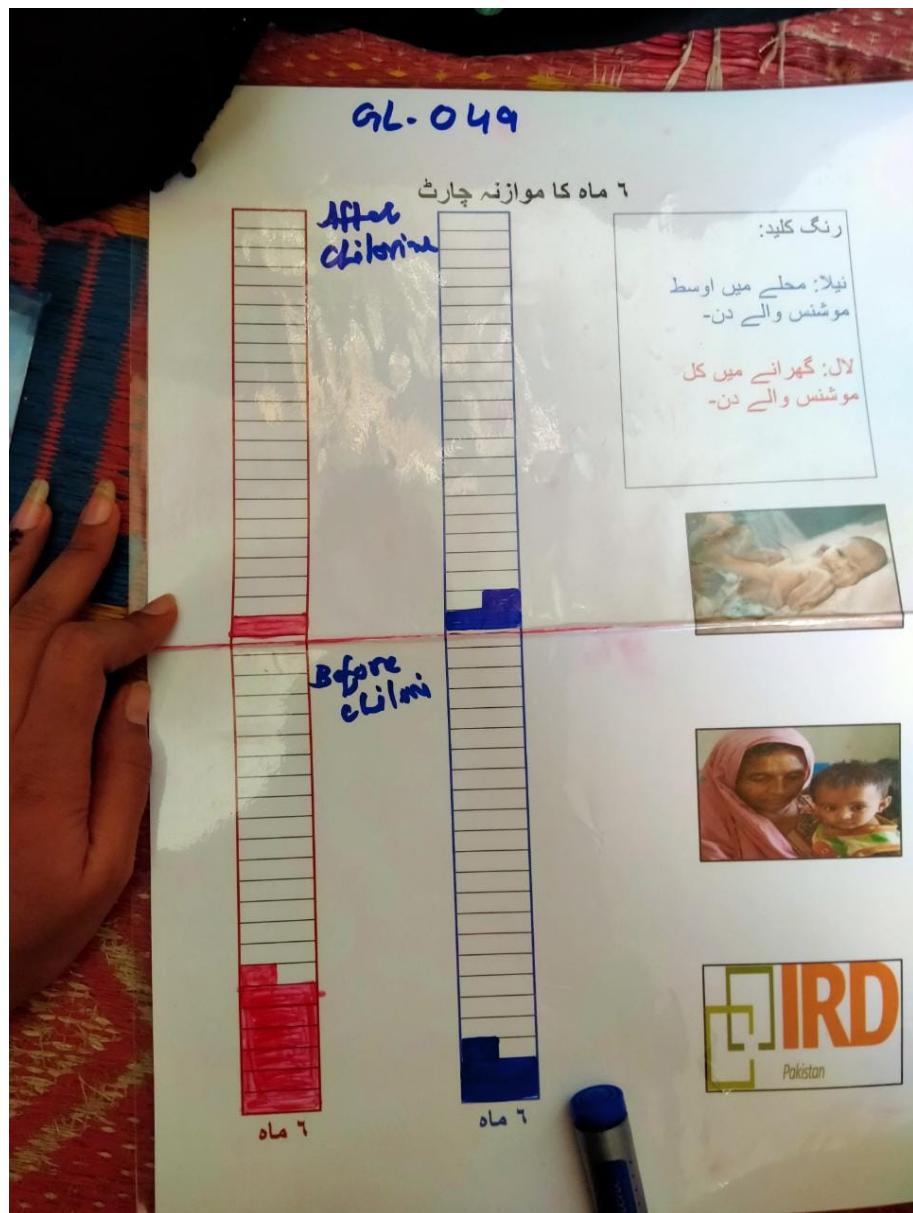


Figure G.2: Experiential Learning Intervention: Info-Tool (Difference-in-Differences Comparison)

Red represents a household's own diarrhea rate, aggregated in the first three months before chlorine was distributed in the bottom panel, and aggregated in the three months after chlorine was distributed in the top panel. The CHW aggregates this data for the respondent. She also fills in the aggregated rate among people who do not use chlorine in blue (from Luby et al. (2006)).



Figure G.3: Sample Water Vessel

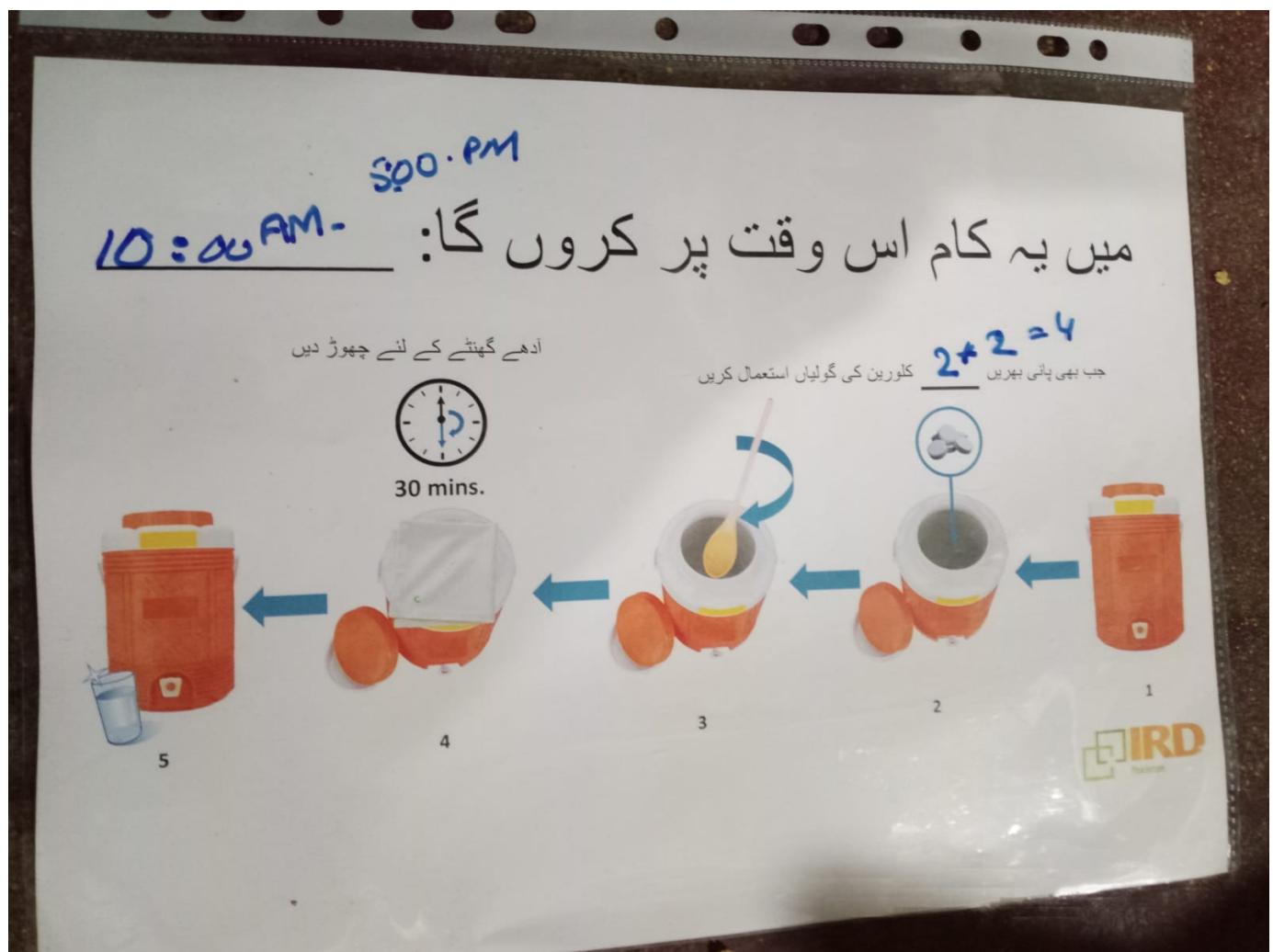


Figure G.4: Leaflet with Instructions on Chlorination

## Appendix H Model Appendix: Simulation

Under a reasonable set of parameters, our model generates dynamic predictions that match our empirical data. We simulate individuals' learning and technology adoption over sixteen periods, where the first four periods represent the "treatment period" (the behavioral interventions), and the following twelve periods represent the "post-treatment period" (free chlorine distribution in all groups). First, we simulate a dataset of 1350 individuals and divide them into three groups (450 per group, as in our experimental design). All three groups undergo the same learning and adoption process, with the following exceptions: one group, modeled on the Incentives group, faces lower costs of technology adoption in the first four periods; and another group, modeled on the Info-Tool group, places heavier experience-weights on the signals observed through the Info-Tool in the first four periods (whether those signals are their own or their Info-Tool friends').

### H.1 Simulated Data Set-Up

Individuals have a prior belief about chlorine efficacy,  $\mu_0$ , which is distributed in the population as:  $\mu_0 \sim \mathcal{N}(.3, .3)$ . We let variance  $\sigma_0^2 = 0.3$  be constant in the population. We assume that prior beliefs are largely positive and certain because most participants' only information about chlorine is from the CHW, who sends a positive signal about chlorine and represents a trusted information source.

In every period where participants adopt chlorine, they receive a signal about chlorine efficacy from nature  $Y \sim \mathcal{N}(0.58, 0.49)$  in the population. This is modeled on the experimental sample mean and standard deviation of the probability that participants' diarrhea rate decreased in the three months after chlorine distribution relative to the three months before. We assume that every participant tries chlorine at least once in period 0 (which represents "day 1" of the treatment period), so that every participant begins period 1 with a signal  $Y$ . We model participants' uncertainty about this belief with  $\sigma_Y^2 = 1$ . Participants apply an *experience-weight* to this belief,  $\alpha_Y = 0.3$ .

#### *Social Learning*

We randomly assign friends using  $Pr(F = 1) = 0.0015$ , sampling with replacement. On average, participants have 2.7 friends each (maximum = 9, minimum = 0), and 0.6 Info-Tool friends each (maximum = 4, minimum = 0). In each period, participants talk with their friends about chlorine with probability  $Pr(T = 1) = 0.2$ . In period  $t$ , the friend sends a signal, which is the average of the signals about chlorine that they received in the previous three periods  $Y_t$ :

$$Y_t = \frac{\sum_{p=t-2}^{p=t} Y_p \cdot \mathbb{1}(M_\alpha^p > C^p)}{\sum_{p=t-2}^{p=t} \mathbb{1}(M_\alpha^p > C^p)}$$

This means that, if a friend has not adopted chlorine in the preceding three periods, they do not share information about chlorine, even if they talk. When participants learn from themselves

(signals from nature), they only update with the information they gain in this particular period,  $Y_t$ . Different from friends, who we assume only learn from each others' signals if they talk (which happens with some random probability), individuals will *always* incorporate information that they generate if they adopt chlorine. In other words, there is no analog for randomly talking in the way that people incorporate their own signals.

### *Technology Adoption*

In each period,  $i$  incorporates signals into her prior (one at a time) from herself and her neighbors. After incorporating all the new signals in the time period, she decides to adopt chlorine if  $M_\alpha > C$ , where we assume that  $C = 0.6$ . Recall that, if she does *not* adopt chlorine, she will not gain any new information about chlorine in the next time period. However, she can still learn from her friends about chlorine (if they talk in that time period, and if her friend has adopted chlorine in the past three time periods).

### *Interventions*

For 450 individuals, we let  $C = 0.5$  in the first four time periods (the Incentives group). For another 450 individuals, we let  $\alpha_Y = 1$  for signals that are acquired with the Info-Tool (signals they receive from themselves in the first four periods, or signals they receive from others in the same group in the first 7 periods, since individuals share prior information up to three periods later). The remaining 450 participants undergo “standard” learning, and represent the Chlorine Only group.

## H.2 Dynamic Simulations

Summary of base parametric assumptions:

$$\begin{aligned}\mu_0 &\sim \mathcal{N}(0.3, 0.3) \\ \sigma_0 &= 0.3 \\ Y &\sim \mathcal{N}(0.58, 0.49) \\ \sigma_Y^2 &= 1 \\ \alpha_Y &= 0.3 \\ C &= 0.6\end{aligned}$$

Summary of interventions:

$$\begin{aligned}\text{Incentives: } C &= 0.5, \forall t \leq 4 \\ \text{Info-Tool: } \alpha_Y &= 1, \forall Y_{t \leq 4} \& i=j \\ \text{Info-Tool: } \alpha_Y &= 1, \forall Y_{t \leq 7} \& j \in \{IT\} \& i \neq j\end{aligned}$$

Our model generates a pattern of adoption that resembles our raw data (Figure H.1).

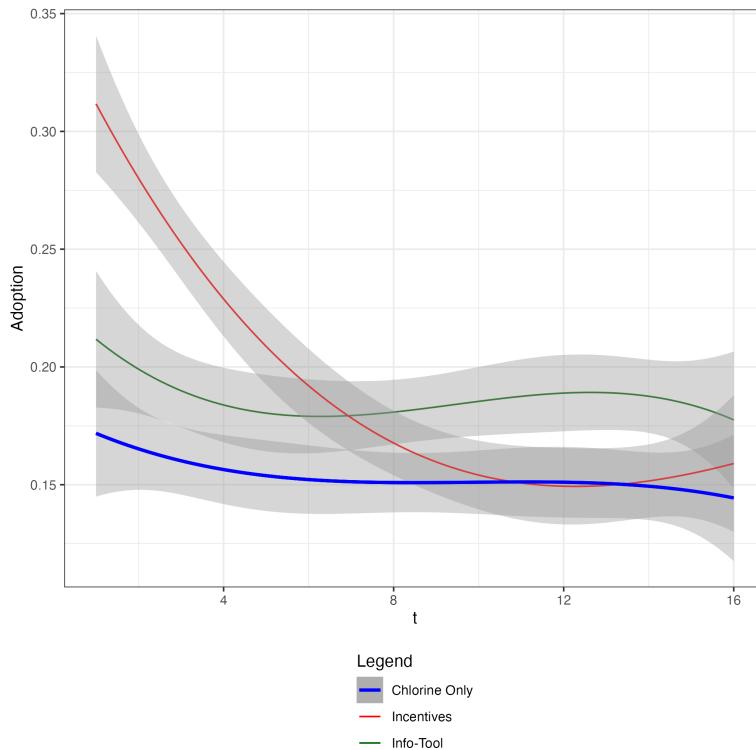


Figure H.1: Model Simulation: Chlorine Adoption Over Time

We observe a positive and *increasing* relationship between the number of Info-Tool friends and chlorine adoption within the Info-Tool group. However, this relationship is not present in the other groups. (Figure H.2).

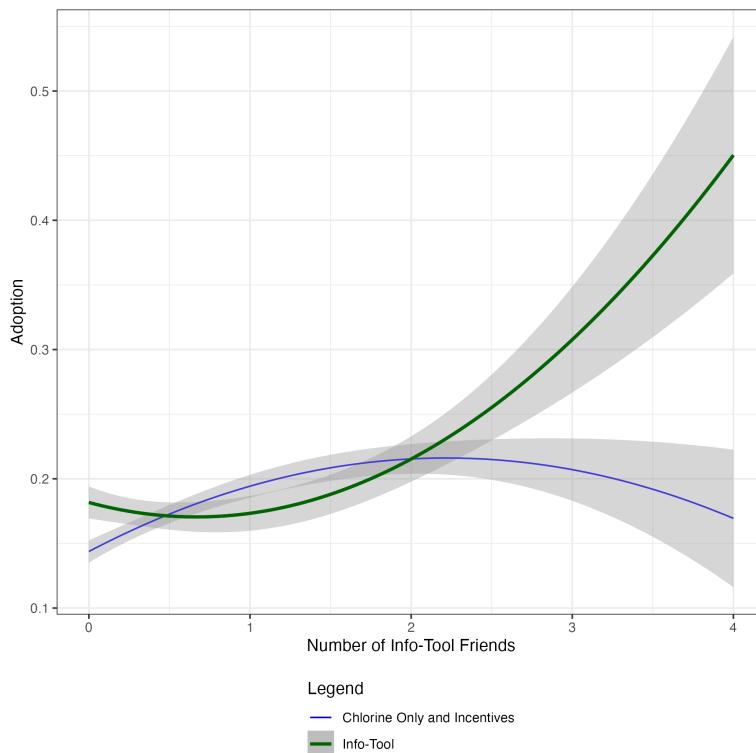


Figure H.2: Model Simulation: Chlorine Adoption as a Function of Info-Tool Friends

While adoption dynamics differ by intervention groups, and according to the number of Info-Tool neighbors, *stated* posterior beliefs  $M$  (i.e. explicit knowledge) do not (Figures H.3 and H.4).

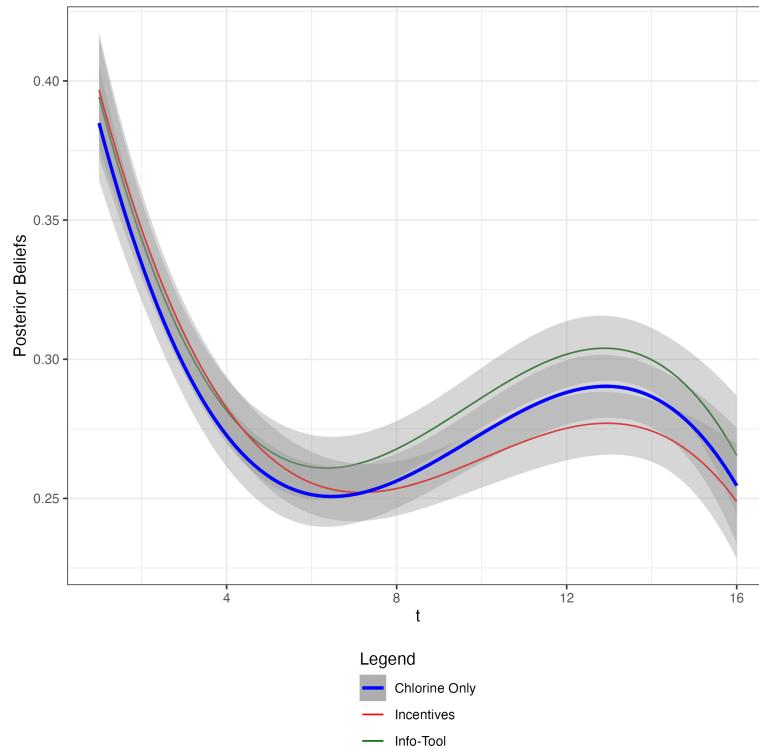


Figure H.3: Model Simulation: Stated Posterior Beliefs Over Time

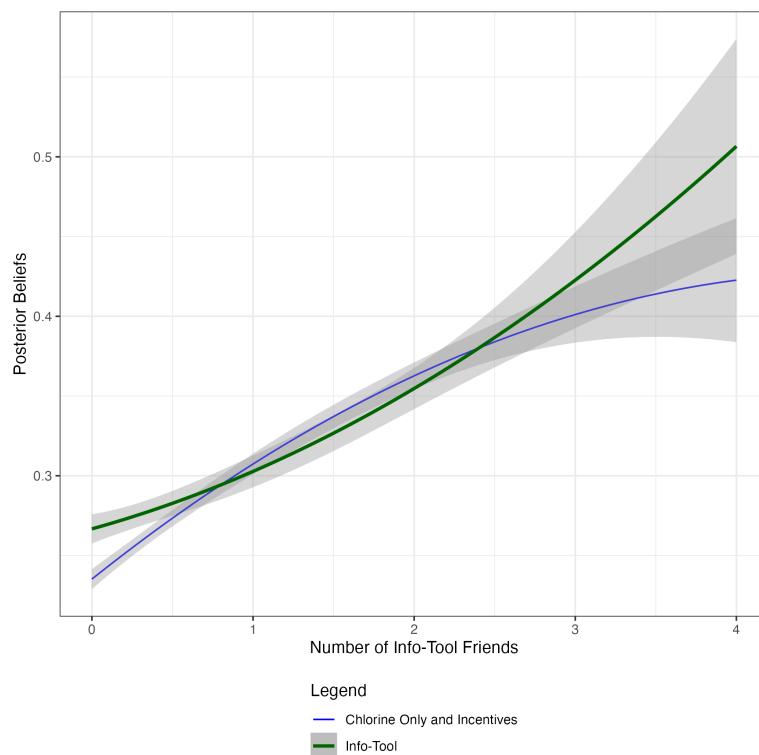


Figure H.4: Model Simulation: Stated Posterior Beliefs as a Function of Info-Tool Friends

## Appendix I Model Appendix: Experience Weights

Experience weights  $\alpha_Y$  can arise through several behavioral mechanisms, each corresponding to a different functional form. Below, we provide four microfoundations that are consistent with our framework and explain how they map to behavior without requiring changes in the informational content or explicit beliefs about the expected returns to the technology, which is captured through changes in  $ell$ .

### (1) Trust

Trust in a signal-acquisition technology increases with experience using that technology. With repeated use, individuals learn that the technology is credible, stable, or familiar, and this experience reduces psychological or emotional distance between the user and the source of information. Even when individuals' statistical understanding of the signal's accuracy (and therefore their posterior mean belief  $M$ ) remains unchanged, they may come to *trust* that information enough to act on it.

Formally, we can represent this process as:

$$\alpha_Y = \alpha(\gamma_Y, \text{Trust}_{\gamma_Y}(E(\gamma_Y))), \quad \text{with } \frac{\partial \text{Trust}_{\gamma_Y}}{\partial E(\gamma_Y)} > 0,$$

where  $\gamma_Y$  indexes the technology that produced the signal  $Y$ ,  $E(\gamma_Y)$  denotes experience with that technology. Experience raises perceived trust, and therefore the experience weight  $\alpha_Y$ , even if the expected value of  $M$  does not change.

### (2) Ownership Effects

Following [Conlon et al. \(2022\)](#), individuals establish psychological ownership over information that they help generate through self-investment (i.e., costly action). We extend this idea to include ownership acquired through *intimate knowledge* of a signal-acquisition process—knowledge gained from observing, engaging with, or understanding how that process produces information.

The psychology literature identifies “self-investment” and “intimate knowledge” as two antecedents of psychological ownership over objects and ideas ([Morewedge, 2021](#)).<sup>48</sup> In our setting, direct engagement with the Info-Tool provides both self-investment and intimate knowledge, allowing participants to “own” the health information they generate. Importantly, ownership extends beyond one’s own data: when an individual understands how a signal is produced, she also places higher behavioral weight on similar signals generated by others using that same technology.

Formally, let  $\gamma_Y \in \{\Gamma\}$  denote the source of the signal  $Y$  from a set of possible sources  $\Gamma$ .

---

<sup>48</sup>[Morewedge \(2021\)](#) also note that “physical control” can generate ownership, but since we discuss ownership over information rather than objects, we omit that channel here.

Let  $c_{\gamma_Y}$  denote the cost-of-action to acquire signal  $Y$  from  $\gamma_Y$ , and let  $IK_{\gamma_Y}$  denote intimate knowledge of that process. We assume:

$$\alpha_Y = \alpha_Y(c_{\gamma_Y}, IK_{\gamma_Y}), \quad \frac{\partial \alpha_Y}{\partial c_{\gamma_Y}} > 0, \quad \frac{\partial \alpha_Y}{\partial IK_{\gamma_Y}} > 0,$$

and that

$$IK_{\gamma_Y} = IK_{\gamma_Y}(E_{\gamma_Y}), \quad \frac{\partial IK_{\gamma_Y}}{\partial E_{\gamma_Y}} > 0.$$

Ownership—through self-investment or intimate knowledge—increases  $\alpha_Y$  without altering  $\ell$ , meaning it changes how readily beliefs are acted upon rather than how they are formed.

### (3) Risk-taking Threshold Model

Experience may also alter the threshold at which individuals are willing to act on uncertain information. Here, the key idea is that experience affects an individual's tolerance for risk rather than the quality of the underlying signal. Consider  $p_\tau = \Pr(Y \geq \tau)$ , the perceived probability that the technology yields a satisfactory outcome. Individuals act only if this probability exceeds an internal threshold  $p^*(E(\gamma_Y))$ , where the threshold declines with experience:

$$p^{*'}(E(\gamma_Y)) < 0.$$

The experience weight can then be written as a smooth threshold function:

$$\alpha_Y = \alpha(\lambda[p_\tau - p^*(E(\gamma_Y))]).$$

Inexperienced individuals require a high perceived probability of success before acting, while experienced users are willing to act even when  $p_\tau$  is modest. Crucially, this formulation does *not* assume that the signal becomes more precise or that  $\ell$  changes. Instead, behavior changes because experience shifts the individual's risk threshold—the same signal is now sufficient to trigger action, even though the perceived mean  $M$  remains constant.

### (4) Dispersion–Sensitivity (Steepness) Model

Experience with the signal–acquisition technology  $\gamma_Y$  can change the perceived dispersion of its signals,  $\sigma_Y = \sigma_Y(E(\gamma_Y))$ , while leaving mean reported beliefs essentially unchanged. In this framework, dispersion  $\sigma_Y$  loads onto two objects: (i) the cognitive updating weight  $\ell$ , which is a *smooth* function of  $\sigma_Y$ ; and (ii) the experience weight  $\alpha_Y$ , which we allow to be a *much steeper* function of  $\sigma_Y$ . This separation permits large changes in behavior through  $\alpha_Y$  even when changes in  $\ell$  (and thus in reported mean beliefs) are too small to detect.

For concreteness, take the standard Gaussian-linear updating weight:

$$\ell(\sigma_Y) = \frac{\sigma_0^2}{\sigma_0^2 + \sigma_Y^2}, \quad \frac{\partial \ell}{\partial \sigma_Y} = -\frac{2\sigma_0^2 \sigma_Y}{(\sigma_0^2 + \sigma_Y^2)^2},$$

which is bounded and varies smoothly in  $\sigma_Y$ . Let the experience weight be

$$\alpha_Y = \alpha(\gamma_Y, \sigma_Y(E_{\gamma_Y})),$$

where:

$$\left| \frac{\partial \alpha_Y}{\partial \sigma_Y} \right| \geq \kappa \left| \frac{\partial \ell}{\partial \sigma_Y} \right| \quad \text{for } \kappa \gg 1,$$

Then an experience shock  $\Delta E_{\gamma_Y}$  implies

$$\Delta \alpha_Y \approx \frac{\partial \alpha_Y}{\partial \sigma_Y} \frac{\partial \sigma_Y}{\partial E_{\gamma_Y}} \Delta E_{\gamma_Y}, \quad \Delta \ell \approx \frac{\partial \ell}{\partial \sigma_Y} \frac{\partial \sigma_Y}{\partial E_{\gamma_Y}} \Delta E_{\gamma_Y},$$

so that  $|\Delta \alpha_Y| \geq \kappa |\Delta \ell|$  locally, and  $|M_\alpha| \geq |M|$ . Hence, behavior (through  $\alpha_Y$ ) can shift markedly while explicit mean beliefs (through  $\ell$ ) change only minimally.

# Appendix J AEA Registry Analysis

## J.1 Background

Randomized controlled trials (RCTs) are a cornerstone of modern empirical research, but their validity often rests on the assumption that individual treatment effects are independent of other individuals' treatment status, commonly referred to as the Stable Unit Treatment Value Assumption (SUTVA). Economists have made substantial progress in analyzing cross-treatment spillovers, where treated units influence control units, to understand general equilibrium effects. However, much less attention has been given to *within*-treatment spillovers, where one treated unit affects *another* treated unit, despite their relevance for economic theory and policy. For individually randomized experiments where there are cross-treatment spillovers, comparing average outcomes between treated and control units may conceal individual treatment effects in the treatment group. A common way to address this is through clustered randomization. In cluster-randomized experiments, researchers randomize clusters of individuals – groups that ostensibly do not interact with each other – to treatment or control, reducing the possibility that treated units' treatment status will influence outcomes among control units. However, cluster-randomized designs with full treatment saturation, where entire clusters are assigned to treatment or control, can *not* address the possibility of complementarity between receiving treatment and being exposed to other people who receive treatment.

Two-stage cluster-randomized designs with varying treatment saturation *can* identify within-treatment spillovers by randomizing clusters to different treated-to-control ratios. This creates random variation in exposure to treated units for both treated and control units, making it possible to measure and understand spillovers comprehensively. However, these designs are rarely used in practice due to logistical challenges and reduced statistical power for detecting individual treatment effects. Moreover, when implemented, they often focus on forces generating cross-treatment spillovers, such as general equilibrium effects in markets, and not on the forces that we argue are similarly likely to generate within-treatment spillovers: the behavioral and social forces behind learning and technology adoption.

Researchers identify or negate cross-treatment spillovers through two primary methods: analyzing the treatment effect of distance (geographic or social) to treated units among control units, and through cluster-randomized designs. In designs with random variation in distance to treated units among control units, cross-treatment spillovers are identified but not neutralized (designs such as [Miguel and Kremer \(2004\)](#)). In cluster-randomized designs where every unit is treated in some clusters and no units are treated in other clusters, cross-treatment spillovers are neutralized but not identified (designs that randomize at a group level such as school-level randomization in [Muralidharan and Sundararaman \(2011\)](#)). Designs that randomize treatment saturation across clusters, and then randomize individuals to treatment or control within clusters according to the predetermined ratio, are cluster-randomized designs that effectively randomize individuals' distance to treated units (designs such as [Egger et al. \(2022\)](#)). Concerns

about the inability to detect true treatment effects due to cross-treatment spillovers have led cluster-randomized designs to be highly prevalent in economics randomized controlled trials, especially within development economics.

## J.2 Methods

We downloaded the AEA RCT Registry data on November 11, 2024 ([AEA RCT Registry Dataverse, 2024](#)). Since there is no single variable indicating the randomization procedure, we utilized ChatGPT 4o to help conduct a text analysis of text-response variables.

Cluster-randomized versus individually-randomized trials: We identify individually-randomized trials from cluster-randomized trials using the “randomization unit”, “sample size number of clusters”, and “sample size number of observations” variables. The assignment rules were as follows.

ClusteredTrial = 0 if any of the following conditions are true:

1. “Sample size number clusters” is equal to any of the following (ignoring cases and punctuation): “no cluster”, “no clusters”, “NA”, “none”, “same as observations”, “no”, “individual”, or is blank
2. “Sample size number clusters” includes any of the following phrases (ignoring cases and punctuation): “treatment is not clustered”, “no clustering”, “there are no clusters”, “equals the total number of participants”, “not clustered”, “not applicable”, “treatment will not be clustered”, “there are no planned clusters”, “this is not cluster-randomized”, “this is not a cluster-randomized”, “NA ”, “N/A ”, “does not have clustered randomization”, “does not cluster randomize”, “there is no need for clustering”, or “no cluster”
3. “Sample size number of clusters” = “Sample size number observations”
4. “Randomization unit” is equal to any of the following (ignoring cases and punctuation): “individual”, “we will randomize individuals”, “individual is the unit of randomization”, “individual level”, “individual randomization for treatments”, “treatments are randomized within-session at the individual level”, “randomization takes part on the participant level”, “individual survey participant”, “individual worker”, “individual participant”, “the randomization was done using stratified random sampling at the individual level”, or “randomization will be done at the individual level”
5. “Randomization unit” includes any of the following (ignoring cases and punctuation): “without clustering”
6. The only difference between the values in “Randomization Unit” and “Sample size number of observations” is that “Sample size number of observations” has a number that is not present in “Randomization Unit”
7. The number in “Sample size number of clusters” is the same as the number in “Sample

size number of observations” and “Randomization unit” includes the word “individual” and “Randomization Unit” does not include the word “cluster”, and “Sample size number of clusters” does not include any words (only numbers)

8. The number in “Sample size number of clusters” is the same as the number in “Sample size number of observations” and “Randomization unit” does not include the word “cluster”, and “Sample size number of observations” does not include any words

Among cases where ClusteredTrial  $\neq 0$ , ClusteredTrial = 1 if any of the following conditions are true:

1. “Sample size number of clusters” includes both a number and a unit (for example, “300 schools”, “100 households” or “50 villages”)
2. “Experimental design” or “Experimental design details” include the words “medium treatment saturation”, “high treatment saturation”, or “low treatment saturation”
3. “Experimental design” or “Experimental design details” include the words “% saturation” or “% treatment saturation”
4. There is a number included in both the “Sample size number of observations” variable and the “Sample size number of clusters” variable, and these numbers are not the same
5. “Sample size number of clusters” and “Sample size number of observations” are both just numbers, and “Sample size number of clusters” is a smaller number than “Sample size number of observations”

After implementing these rules, we are able to classify 96.8% of trials (the treatment design of 304 out of 9494 trials remain unidentified).

Full- vs. partial-treatment saturation: We then classify a trial as cluster-randomized with varying treatment saturation across clusters if the trial is classified as cluster-randomized, and if the registration ever uses terms related to “treatment saturation” or “two-stage randomization” across any variable.

KeywordSat = 1 for observations that mention any word stemming with “saturat” across any variable. Otherwise, KeywordSat = 0.

KeywordTwoStage = 1 if any variable includes the phrase “two stage randomization”, “two-stage randomization”, “2-stage randomization” or “2 stage randomization”. Otherwise, KeywordTwoStage = 0.

We consider a trial to be cluster-randomized with varying treatment saturation if ClusteredTrial = 1 and either KeywordSat = 1 or KeywordTwoStage = 1.

Research Topics: For each trial, researchers select “keywords” associated with their trial. First, we make minor cleaning adjustments for cohesion across variations in specific keywords that people use:

1. “behavior” includes: “behavior”, “behavioral”, “behavioral economics”
2. “environment” includes: “environment”, “climate change”, “environment and energy”
3. “health” includes: “health”, “covid-19”, “mental health”, “nutrition”
4. “crime violence and conflict” includes: “crime violence and conflict”, “post-conflict”
5. “experiment” includes: “experiment”, “rct”, “online experiment”, “survey experiment”, “field experiment”, “lab”
6. “firms” includes: “firms”, “firms and productivity”
7. “productivity” includes: “productivity”, “firms and productivity”

Next, we identify all of the keywords that are associated with at least fifty trials (we do not analyze keywords that are excessively niche). This leaves us with thirty-nine keywords to analyze.

### J.3 Results: Prevalence of Clustered Randomization

Among all projects registered on the American Economics Association RCT Registry, 35% of trials in high-income countries (HICs) featured cluster-randomized designs, whereas 62% of trials in low- and middle-income countries (LMICs) featured cluster-randomized designs. Of trials that did not specify or publish the country, 48% were cluster-randomized. We used the World Bank classification of country income.

Of all the cluster-randomized trials registered to the American Economics Association RCT Registry, only 2.3% were classified as cluster-randomized with varying treatment saturation (98 trials total). An additional 26 trials met the criteria based on the keywords “treatment saturation” or “two-stage randomized”, but were individually randomized trials (a case of type 1 error). Of the cluster-randomized trials with varying treatment saturation, 38% were within LMICs, 9% were within HICs, and 53% did not specify or publish the country.

### J.4 Results: By Topic

For which questions do researchers implement cluster-randomized designs, especially those with varying treatment saturation? To analyze the prevalence of research topics across experimental designs, we use the following regression for each keyword  $K$ :

$$K_t = \beta_0 + \beta_1 C_t + \beta_2 S_t + \beta_3 C_t \times S_t + \gamma_t + \epsilon_t \quad (1)$$

where  $K_t = 1$  if trial  $t$  was registered with keyword  $K$ , and 0 otherwise;  $C_t = 1$  if the trial is cluster-randomized;  $S_t = 1$  if the trial included the terms “treatment saturation” or “two-stage randomization” anywhere in the registration; and  $\gamma_t$  is a world-region fixed effect (HIC, LMIC, or unspecified). Then  $\beta_1$  is the probability that a fully-saturated cluster-randomized

trial is associated with keyword  $K$  relative to individually-randomized trials; and  $\beta_2 + \beta_3$  is the probability that a cluster-randomized trial with varying treatment saturation is associated with keyword  $K$  relative to fully-saturated cluster-randomized trials.

Keywords that are *more* likely to be associated with fully-saturated cluster-randomized trials, relative to individually-randomized trials, include “agriculture”, “cash transfers”, “communication”, “early childhood development”, “education”, “gender”, “health”, “incentives”, “poverty”, and “technology adoption”. Keywords that are *less* likely to be associated with fully-saturated cluster-randomized trials, relative to individually-randomized trials, include “altruism”, “behavior”, “beliefs”, “electoral”, “fairness”, “inequality”, “labor”, “migration”, “redistribution”, “social media”, “trust”, and “other”. There are no statistically significant differences in the probability of individual- or clustered-randomization among the remaining keywords.<sup>49</sup>

Keywords that are more likely to be associated with cluster-randomized trials with varying treatment saturation, relative to fully saturated cluster-randomized trials, are “agriculture” and “migration” (there are no statistically distinguishable differences across any other keywords).

### Keywords Associated with Our Trial

We analyze the prevalence of treatment designs across trials that use top keywords that we group together as “behavioral”, “health”, “technology adoption”, or “communication”. Behavioral trials are most likely individually randomized, and, when cluster-randomized, almost always with full treatment saturation. Health, technology adoption, and communication trials are very likely to be cluster-randomized, but usually with full treatment saturation.

Cluster-randomized trials with full saturation are 11pp (24%,  $p < 0.001$ ) *less* likely to be associated with a top behavioral economics keyword (“behavior”, “beliefs”, “fairness”, or “altruism”) than individually-randomized trials. Cluster-randomized trials with varying treatment saturation are another 8pp (25%,  $p = 0.099$ ) less likely to include a top behavioral economics keyword, relative to fully saturated cluster-randomized trials.

Cluster-randomization with full saturation – the design that allows for the strongest within-treatment spillovers *but conceals them* – is the most common experimental design researchers implement for economics trials focused on technology adoption, health, or communication. Technology adoption trials are 0.7pp (163%,  $p < 0.001$ ) more likely to be cluster-randomized than individually randomized; health trials are 1.9pp (12%,  $p = 0.020$ ) more likely to be cluster-randomized than individually randomized; and communication trials are 0.5pp (55%,  $p = 0.024$ ) more likely to be cluster-randomized than individually randomized.

---

<sup>49</sup>Keywords with no differences in treatment design: “cooperation”, “crime violence and conflict”, “discrimination”, “entrepreneurship”, “environment”, “experiment”, “finance”, “financial literacy”, “firms”, “governance”, “information”, “productivity”, “savings”, “social norms”, “taxation”, “training”, and “welfare”.

# Appendix K Detailed Literature Review

## K.1 Experiential Learning in Health

This paper speaks to a small literature on the value of experiential and social learning in adopting health technology. There exists a large literature investigating how experts themselves learn about health from their own experiences, but this literature is more focused on physician skill development than on belief formation (Halm et al. (2002); Facchini (2022)). While health experts have the information that is most likely to be accurate on average, drawing from clinical trials and personal experiences, there are large inequities in access to experts (Dussault and Franceschini, 2006). Conditional on access to experts, there are still large inequities in how much trust people have in experts. Oftentimes, mistrust is a result of past wrongs committed by the medical community on groups with whom patients identify (Alsan and Wanamaker (2018); Lowes and Montero (2021); Martinez-Bravo and Stegmann (2022)), but there is evidence that knowing that doctors have financial incentives linked with the medical care they provide is enough to sow mistrust (Banerjee et al. (2023)).

Disseminating health information through laypeople or social networks may help solve the problems of trust and access associated with relying on experts to relay health information (Alsan and Eichmeyer (2024), Banerjee et al. (2019)). However, the most socially isolated people may be excluded from information transmission through the social network. Furthermore, the types of information that individuals have access to may differ in quality. Calónico et al. (2023) find that a clinically unsupported treatment for COVID-19 spread through Argentina in a pattern that follows rational learning from neighbors, suggesting that networks can facilitate the spread of medically dubious information. Similarly, Chen et al. (2022) find that access to well-informed networks can generate health inequities. Individuals in Sweden who have a doctor in the family invest more in preventive health, and consequently enjoy healthier and longer lives.

There is scant evidence of experiential learning as a method through which people form beliefs about the efficacy of health inputs and behaviors. Bennett et al. (2018) find that a hygiene course in Pakistan leads to more hygienic behavior when the course includes showing participants microbes under a microscope, suggesting that “seeing is believing.” Corno (2014) finds that individuals in rural Tanzania are more likely to seek clinical care if they previously healed after utilizing clinical care, or if they previously *did not* heal after *foregoing* clinical care. Akram and Mendelsohn (2021), whose treatment arm we replicate in our learning arm, find that the Info-Tool leads to an increase in water chlorination in the long run. We draw from a large literature in development economics showing that individuals learn about agricultural technologies through their own experiences and their neighbors’ experiences (Foster and Rosenzweig, 1994; Hanna et al., 2014; Conley and Udry, 2010). Choosing an agricultural input is similar to choosing a health input in that it is a high-stakes, high-dimensional problem, subject to random shocks and with potential for misattribution.

Globally, 663 million people lack access to an improved source of drinking water while fecal contamination affects 1.8 billion people [UNICEF and WHO \(2015\)](#). Annually, contaminated water contributes to 1.7 billion cases of diarrheal disease and 1.6 million deaths, including half a million under-five children, with most of the disease burden concentrated in developing countries ([WHO, 2017](#)). Safe drinking water results in better health, particularly among young children ([Kremer et al., 2023; Haushofer et al., 2021](#)), and yields long-term health and cognitive improvements ([Scharf et al., 2014](#)). Point-of-use decontamination technologies such as chlorine tablets can drastically reduce the burden of diarrheal disease ([Reller et al. \(2003\)](#), [Quick et al. \(2002\)](#), [Quick et al. \(1999\)](#)).

## K.2 Habit Formation versus Learning

Recent literature on long-term behavior change recognizes habit formation, or the generation of complementarities in use across time, as playing a key role in sustained change (e.g., [Becker and Murphy \(1988\)](#), [Wellsjo \(2021\)](#), [Celhay et al. \(2015\)](#), [Allcott and Rogers \(2014\)](#), [Royer et al. \(2015\)](#), [Aggarwal et al. \(2020\)](#)). In both the case of habit formation and learning, higher initial use implies higher future use: in learning, due to initial exposure generating knowledge of the positive returns to a product and increasing the likelihood of future use; and in habit formation, due to intertemporal complementarities in consumption wherein greater initial consumption stock raises future desire to consume. With a few exceptions, however, these studies make no distinction between these two mechanisms, with persistent behavior change in the post-intervention periods often being explained by one mechanism without consideration of the other.

Recognition that these mechanisms may act in concert or be conflated with one another is rather recent in the literature. [Caro-Burnett et al. \(2021\)](#) examine sanitary latrine use in Kenyan slums and isolate the impact of habit formation interventions above and beyond that of learning by holding short-run use constant across treatment arms and examining only long-run behavior change. They find no detectable difference in long-run behavior across arms, though this is likely due to the study context (with sanitary latrine use less amenable to habit formation) and the nature of the interventions themselves (with a time-constrained subsidy intervention intended to embed the behavior in a habit loop, but challenging to do in practice given the potential unpredictability of defecation). [Hussam et al. \(2022\)](#) consider these mechanisms in the context of handwashing in rural West Bengal and attempt to distinguish habit formation from learning about returns by comparing households who experienced the same level of short-run financial incentives, but varied levels of health returns to their behavior. They find that those who experience larger improvements in health (whether between weeks or in aggregate) are no more likely to persist in their handwashing, suggesting that long-run behavior change is not driven by households independently engaging in learning about the value of handwashing from their health experiences. [Alpízar et al. \(2022\)](#) offer a conceptual framework for how these mechanisms may result in long-term adoption (theoretically distinguishing learning how to use a good and learning the returns to use from changes in taste via habit formation), but are

unable to disentangle these channels empirically in an experiment which generates long run use of water-saving technologies through short run incentives to engage.

Yet the distinction between learning and habit formation is critical to policy, as each mechanism implies a substantively different behavior change process and therefore intervention design. Should learning about returns be more effective at generating sustained behavior change or technology adoption, policy design efforts should focus on developing information campaigns that make returns to a behavior explicit. Should habit formation be more effective in motivating long term change, resources may be better spent incentivizing high short-run engagement, yielding long term intertemporal complementarities, and embedding behaviors within habit loops (a la [Duhigg \(2012\)](#) and [Neal et al. \(2015\)](#)).

Preventive health behaviors are often mundane acts that require repetition in order to generate meaningful health impacts - a setting in which habit-formation may be especially relevant. Our experiment allows us to distinguish between the role of learning through salient signals and learning through early adoption (either through habit formation or through the accumulation of more signals) in the long-term adoption of chlorine tablets.

## Appendix L Audit Visits

Our main outcome, presence of chlorine residual in drinking water, is a good approximation for consistent use of chlorine tablets *unless* participants are more motivated to use chlorine prior to visits from enumerators, in anticipation of the enumerator's visit. There are several reasons we do not expect this to happen. T3 participants received monetary incentives based on the number of empty chlorine tablet wrappers they could present, rather than based on the presence of residual chlorine in the water. If T3 respondents did not like using chlorine tablets, they could throw away the tablets, present the wrappers, and still receive the prize. Thus, there is no pecuniary motivation for any group to make sure their water presents chlorine residual on the day of the enumerator's visit.

However, there may be social desirability motivations for participants to use chlorine in anticipation of a visit from an enumerator. It may still be difficult for participants to plan perfectly. Chlorine residual is only present for 24 hours, so participants would have to plan to the exact day. Furthermore, enumerators tested for chlorine residual once per month, rather than at every visit, and did not explain the results to participants except in the case of over-chlorination. When possible, enumerators were instructed to take a cup of water outside and test for the presence of chlorine residual without the caregiver observing, to further eliminate the possibility that caregivers expected a reward for water quality.

To be sure that caregivers did not plan chlorine use around enumerator visits, we conducted audit visits throughout the behavioral intervention period (Phase 1 Round 2). In these audit visits, an enumerator arrived unexpectedly to test for the presence of chlorine residual. Audits were conducted by a different enumerator than the enumerator who usually visited the house-

hold. During Phase 1, households were visited once every two weeks but chlorine was only tested once per month. We conducted audit visits during the visit period where water was not tested for chlorine residual, and visited selected households several days before or after their assigned survey visit date.

In each month, we randomly selected sixty-five households with whom to conduct audit visits, or just under 5% of treatment group households per month. We were most concerned about anticipatory behavior among households who frequently showed presence of chlorine residual at regularly scheduled visits, but did not want to oversample so heavily from this group that there would be a treatment effect from extra visits. As such, when selecting households for audit visits, we stratified by the frequency of testing positive for the presence of chlorine residual and ensured that no household was audited more than twice.

## Appendix M Child Health

### M.1 Diarrhea

In each visit, we asked participants to recount the number of days in the last week that each child had diarrhea (“experienced motions”). Ultimately, we consider child anthropometrics as our key measure of child health because it is objective. However, since we have panel data on diarrhea, we can use our diarrhea data to understand how child health changed over time with the treatments. We analyze the aggregate of child-diarrhea-days across all children in the household, controlling for the number of children. Household total child-days of diarrhea is the same measure that Info-Tool households recorded as a part of the Info-Tool treatment.

Although we recorded data on child-days of diarrhea during the three months prior to the distribution of chlorine, we do not analyze data from this time period because we consider this a training period for the Info-Tool group. Indeed, we see much higher rates of recorded diarrhea in the Info-Tool group during this period, which we believe is likely over-reporting in response to the Info-Tool treatment. To ensure uniformity across treatment groups in how respondents interpret and report loose stools, we used the Bristol stool chart to help caregivers identify loose stools at baseline. This chart provides illustrations of different potential stool consistencies. At endline, we showed households the chart again and asked them to identify which illustrations they would consider to be motions, to see if the treatments had changed participants’ interpretation of what constitutes a loose stool. Although we expected the Info-Tool treatment to be the most likely treatment to change participants’ interpretation of loose stools (because the treatment would lead participants to more closely attend to children’s stools), it was actually the Incentives group whose interpretation of loose stools appears to have changed. The Incentives group was more likely to consider illustrations of solid stools to be loose, and therefore might have overestimated their children’s rate of diarrhea. There were no differences in the number of illustrations that Pure Control, Chlorine Only, and Info-Tool considered to be loose stools. Consequently, we think that child-days of diarrhea is a good proxy

for child health when comparing the Info-Tool, Chlorine Only, and Control groups. Incentives households may have over-estimated their children's diarrhea rates, so we interpret comparisons between the diarrhea rates in the Incentives group and other groups cautiously.

Table M.1 presents the results. Across all treatment groups, the impact of chlorine tablet dispensation is substantial: diarrhea rates drop by approximately 30% ( $p < 0.005$ ) for Chlorine only, Incentives, and Info-Tool households in the short run. These effects are largely sustained over the medium run, although Incentives households fall short, consistent with their chlorine residual patterns. Treated households broadly continue to demonstrate large and significant reductions in child diarrhea into the long-run, again with the exception of Incentive households.

Average treatment effects suggest that chlorine provision effectively and substantially reduces child diarrhea rates, these effects persist over the course of a year, and those in the Info-Tool arm experience the largest gains, followed by those in Chlorine Only, and lastly the Incentives arm. While these patterns are merely suggestive given the self-reported nature of the outcome, we do observe the same pattern of results in our objective anthropometric measures of child health that we collect at endline, which we report and discuss in Section 4.3. Our findings suggest that continuous chlorine use, even during the season where diarrhea poses the lowest risk (our medium-run period, during which Incentives households most drastically reduce their chlorine use), is important for building young children's stock of health. This pattern also speaks to the importance of tools to help participants attend to subtle health signals. Diarrhea is rare in this season, so differences in health with the introduction of chlorine will be more difficult to observe than during the season when diarrhea rates are higher.

Table M.1: Child-Days of Diarrhea (Household-Survey Panel)

	(1) Short-Run	(2) Medium-Run	(3) Long-Run
Chlorine Only	-0.186*** (0.062)	-0.070 (0.045)	-0.091*** (0.032)
Incentives	-0.198*** (0.057)	-0.027 (0.045)	-0.050 (0.033)
Info-Tool	-0.136** (0.062)	-0.092** (0.042)	-0.091*** (0.031)
Observations	7872	6354	14066
Control Mean	0.515	0.240	0.277
<b>P-values:</b>			
Chlorine = Incentives	0.821	0.271	0.187
Chlorine = Info-Tool	0.378	0.528	0.990
Incentives = Info-Tool	0.228	0.070	0.167

Standard errors in parentheses

Each observation is a household-level survey visit. The outcome is a continuous measure of the total child-days of diarrhea that the household reported over the preceding two weeks (aggregated across all children). All specifications include survey-round fixed effects, neighborhood block fixed effects, unbalanced baseline controls, the total number of children in the household under five in that survey round, the number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## M.2 Anthropometrics

### ITT Results: Any Treatment

Table M.2: Diarrhea: Aggregate

	(1)	(2)	(3)	
	Short-Run	Medium-Run	Long-Run	
Chlorine Only	-0.868*** (0.278)	-0.264* (0.157)	-0.807*** (0.265)	
Incentives	-0.980*** (0.276)	-0.103 (0.156)	-0.444* (0.262)	
Info-Tool	-0.696** (0.279)	-0.334** (0.156)	-0.775*** (0.262)	
Observations	1599	1661	1653	
Control Mean	2.505	0.949	2.348	
<b>P-values:</b>				
Chlorine = Incentives	0.687	0.307	0.172	
Chlorine = Info-Tool	0.542	0.661	0.903	
Incentives = Info-Tool	0.309	0.144	0.208	

Standard errors in parentheses

Each observation is at the household level. The outcome is a continuous measure of the total child-days of diarrhea that the household reported over the entirety of the short-run, medium-run, or long-run period (aggregated across all children). All specifications include a control for the number of visits that the household was surveyed during the specified time period, neighborhood block fixed effects, unbalanced baseline controls, the number of children under five the household had at baseline, the number of study participants within twenty meters, and lasso-selected baseline controls. Short-run is defined as the first three months of chlorine distribution (while the behavioral interventions were ongoing); Medium-run is defined as the next three months of chlorine distribution (immediately after the behavioral interventions ended); and Long-run is defined as the remainder of the trial (nine months).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table M.3: ITT Impacts on Endline Child Health (Pooled Treatments)

	(1) Index (Anthro- pometry)	(2) Height-for- Age	(3) Weight-for- Height	(4) Weight-for- Age	(5) MUAC-for- Age
Any Treatment	0.074** (0.034)	0.016 (0.066)	-0.005 (0.081)	0.112* (0.065)	0.055 (0.054)
Observations	2616	2371	2439	2492	1954
Endline Control Mean	-0.019	-1.773	-0.291	-1.407	-1.453

Standard errors in parentheses

Any treatment is an indicator for not being in the Control group (ever receiving chlorine). Child-level cross-section of the endline survey. Standard errors are clustered at the household level. All regressions include baseline unbalanced household-level controls, baseline child anthropometrics and diarrhea rates, child gender, child age, neighborhood block fixed effects, the number of study participants within twenty meters, and lasso-selected controls. Indexes are constructed using following Anderson (2008).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table M.3 reports the ITT effects of *any* treatment on the various measures of child anthropometry, whereas Table M.4 reports the treatment-specific estimates. Table M.4 shows that the improvements in anthropometric outcomes are consistently higher for households in the Info-Tool group.

Table M.5 reports the results of an IV exercise where we use the assignment to any treatment as an instrument for whether the household reported to be boiling, bleaching, or chlorinating water at endline. We find that treatment compliance leads to a 0.24 SD increase in the index of child anthropometrics ( $p < 0.05$ ).

Table M.4: ITT Impacts on Endline Child Health (Separated by Treatment)

	(1) Index (Anthro- pometry)	(2) Height-for- Age	(3) Weight-for- Height	(4) Weight-for- Age	(5) MUAC-for- Age
Chlorine-Only	0.081** (0.039)	0.020 (0.084)	0.007 (0.099)	0.080 (0.081)	0.025 (0.064)
Incentives	0.030 (0.042)	0.022 (0.078)	-0.098 (0.101)	0.065 (0.079)	0.061 (0.066)
Info-Tool	0.108*** (0.039)	0.006 (0.080)	0.072 (0.101)	0.187** (0.080)	0.080 (0.066)
Observations	2616	2371	2439	2492	1954
Endline Control Mean	-0.019	-1.773	-0.291	-1.407	-1.453
<b>P-values:</b>					
Chlorine = Incentives	0.186	0.979	0.310	0.853	0.572
Chlorine = Info-Tool	0.455	0.867	0.523	0.193	0.386
Incentives = Info-Tool	0.044	0.836	0.104	0.130	0.761

Standard errors in parentheses

Child-level cross-section of the endline survey. Standard errors are clustered at the household level. All regressions include baseline unbalanced household-level controls, baseline child anthropometrics and diarrhea rates, child gender, child age, neighborhood block fixed effects, the number of study participants within twenty meters, and lasso-selected controls. Indeces are constructed using following Anderson (2008).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table M.5: IV Impacts on Endline Child Health (Instrument: Any Chlorine Treatment)

	(1) Index (Anthro- pometry)	(2) Height-for- Age	(3) Weight-for- Height	(4) Weight-for- Age	(5) MUAC-for- Age
Boils, Bleaches, or Chlorinates Water	0.240** (0.112)	0.050 (0.207)	-0.017 (0.260)	0.371* (0.212)	0.198 (0.184)
Observations	2616	2371	2439	2492	1954
Endline Control Mean	-0.019	-1.773	-0.291	-1.407	-1.453
Weak-IV robust F statistic	119.93	127.97	121.36	122.46	95.19
C-statistic p-value	0.027	0.491	0.392	0.139	0.056

Standard errors in parentheses

Any treatment (i.e., received chlorine) is an instrument for if the respondent reported at endline that she boils, bleaches, or chlorinates her water. Child-level cross-section of the endline survey. Standard errors are clustered at the household level. All regressions include baseline unbalanced household-level controls, baseline child anthropometrics and diarrhea rates, child gender, child age, the number of study participants within twenty meters, and neighborhood block fixed effects. Indeces are constructed using following Anderson (2008).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table M.6: IV Impacts on Endline Child Health (Instrument: Info-Tool  $\times$  Spillover)  
 where “Spillover” is exposure to any Info-Tool neighbor

	(1) Index (Anthro- pometry)	(2) Height-for- Age	(3) Weight-for- Height	(4) Weight-for- Age	(5) MUAC-for- Age
Boils, Bleaches, or Chlorinates Water	0.506*** (0.183)	-0.083 (0.376)	0.584 (0.516)	0.789** (0.398)	-0.428 (0.337)
Observations	2616	2371	2439	2492	1954
Endline Control Mean	-0.019	-1.773	-0.291	-1.407	-1.453
Weak-IV robust F statistic	35.26	34.36	33.85	36.26	27.02
C-statistic p-value	0.002	0.986	0.471	0.062	0.379

Standard errors in parentheses

Any treatment (i.e., received chlorine) is an instrument for if the respondent reported at endline that she boils, bleaches, or chlorinates her water. Child-level cross-section of the endline survey. Standard errors are clustered at the household level. All regressions include baseline unbalanced household-level controls, baseline child anthropometrics and diarrhea rates, child gender, child age, the number of study participants within twenty meters, and neighborhood block fixed effects. Indeces are constructed using following Anderson (2008).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$