# Experiential and Social Learning*

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

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**Abstract**

This study examines complementarities between experiential and social learning in health technology adoption. We engage 1800 households in peri-urban Pakistan in a field experiment on water chlorination. Our experiment has four arms: control households, who receive no intervention; households who receive free chlorine tablets; households who receive tablets and small daily financial incentives for chlorine use; and households who receive tablets and an experiential learning intervention. In the learning intervention, participants record and visually track their children's diarrhea rate relative to control households before and after chlorine distribution. While monetary incentives generate higher chlorination than experiential learning and chlorine distribution alone in the short run, these effects quickly dissipate. While there are no differential effects of the learning arm on average, learning arm households who also have a neighbor in the learning arm chlorinate their water at a significantly higher rate for almost one year after the end of the learning intervention. Households *not* in the learning arm exhibit no difference in behavior by whether they have a neighbor in the learning arm. We propose a model of learning whereby "ownership effects", generated by self-investment in learning and intimate knowledge of specific learning processes, give rise to a complementarity between experiential and social learning. We rule out various alternative explanations, including changing beliefs about the returns to chlorine use. The welfare implications are significant: ITT (TOT) estimates suggest that learning households with learning neighbors exhibit a 0.16 SD (0.51 SD) increase in an index of child anthropometrics after one year.

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†Akram: Mathematica, agha.ali.akram@gmail.com; Fleischman: Harvard Kennedy School, gfleischman@g.harvard.edu; Hussam: Harvard Business School, rhussam@hbs.edu; Khan: MISUM, Stockholm School of Economics, akib.khan@hhs.se.

# 1 Introduction

Sharing experiences with others can create powerful incentives for behavioral change, facilitating in-group cooperation and the formation of informal insurance networks (Feigenberg et al., 2013; Goette et al., 2006). Alternatively, shared experiences may introduce negative externalities, such as social image concerns, thereby reducing willingness to seek information or interact (Banerjee et al., 2024b,a). In a fairly small literature, this paper examines an unexplored question: what is the role of shared experiences in the decision to adopt new technologies?

A large body of literature identifies social learning—learning from *other's* information or experiences, which an individual did not herself learn from or take part in—as an important driver of technology adoption and behavioral change (Arrow, 1962; Conley and Udry, 2010; Banerjee et al., 2024a; Beaman et al., 2021; Bikhchandani et al., 2024; Breza and Chandrasekhar, 2019; de Janvry et al., 2016; Foster and Rosenzweig, 1994; Khandelwal, 2024; Kondylis et al., 2023; Dupas, 2014). The questions that this literature raises are primarily geared toward understanding how to disseminate information broadly and cost-effectively to uninformed individuals. However, 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.[1] As such, social learning may play an important role in augmenting learning from oneself, yet the interventions that we most often use to study social learning do not facilitate this learning process.

We study the role of shared experience in the adoption of health technology among caregivers of children in Pakistan. Drawing from a learning intervention used in Akram and Mendelsohn (2021), we test for complementarity between individual experiential learning and social learning and find that the individual treatment effects we estimate are almost entirely attributable to households who have treated neighbors (i.e. within-treatment spillovers). In the long run, experiential and social learning *rely* on each other – neither channel in isolation is sufficient to sustain long-term use. Technology adoption is only sustained when learning is reinforced by somebody with whom individuals have shared a specific, relevant experience. We conduct a field experiment with 1800 households in the peri-urban slums of Karachi, Pakistan. Our experiment has four arms: control households, who receive no intervention; chlorine households, who receive free chlorine tablets; incentivized households, who receive tablets and small daily financial incentives for chlorine use; and learning households, who receive tablets and a visual tool that helps them track their household-specific health returns to chlorine use. Incentive and learning tool provisions are temporary, but we continue providing chlorine tablets (to all treatment households) and tracking chlorine presence in drinking water for over 12 months

[1] This is a variation on learning models of complex contagion. In complex (simple) contagion, nodes need more than one (exactly one) "contact" to become "infected". In learning about new technologies, complex contagion implies that individuals need exposure to information about the usefulness of the technology more than once to adopt the technology. We hypothesize that *who* that information comes from, and the order in which you receive information from different sources, can be likewise critical.

following intervention withdrawal. A comparison of the incentive and learning arms allows us to disentangle habit formation from learning, as both may raise the consumption stock of chlorine use in the short run, but only the latter can alter the perceived health returns to use. A comparison of both of these with the chlorine arm allows us to disentangle the degree to which access to chlorine is a barrier to use. Finally, a comparison of chlorine with pure control offers us a useful policy benchmark against which to measure the relative health impacts of the learning intervention.

Our learning arm builds on the work of Akram and Mendelsohn (2021) (hereafter, AM). They design a visual tool that plots the health returns to water treatment by (1) utilizing household-specific data on the frequency of diarrhea among treated children, (2) providing a community-specific benchmark against which to estimate health returns to chlorination, and (3) generating multiple observations over time per household before and after chlorine tablet provision during the peak diarrhea months of the year, facilitating learning from repeated data and trends in health. The information is presented through a simple visual tool (hereafter called the Info-Tool) in the form of a colorful bar chart that plots days of children's diarrhea for the treated house relative to the expected rate of diarrhea among households that do not use chlorine (Luby et al., 2006). Community health workers (CHWs) present households with the comparison rate twice each month for six months: three months prior to chlorine tablet provision and three months following provision.[2] Fifteen months after ending the Info-Tool treatment, AM finds a 197% increase in the rate of chlorine detection in (formerly) Info-Tool households' water relative to households who received free chlorine alone.

We utilize the same Info-Tool intervention for our learning arm, implementing in areas that are nearby, but not overlapping with, those of AM eight years later. One key difference is that, while AM randomizes the Info-Tool at the community level, we do so at the individual (household) level. As described above, we supplement the learning and chlorine-only interventions with an incentives intervention (through which to disentangle the role of habit formation) and a pure control (against which to benchmark health effects). We find that all three interventions (chlorine, incentives, and learning) significantly increase chlorine presence in drinking water for as long as we provide chlorine tablets, approximately fourteen months. While incentive households perform better than learning households during the period of incentive and Info-Tool provision (the "short-run"), learning households outperform incentive households thereafter, with child diarrhea rates dropping the most for learning households following the withdrawal of interventions. Notably, however, differently from AM, we cannot rule out equality in average chlorination rates between the learning and chlorine-only arms in the long run.

Recall that, while AM saturate the Info-Tool treatment at the neighborhood level, we randomize the learning treatment at the individual/household level. This motivates an examination of learning spillovers through social learning. Our setting allows us to understand the underlying

[2]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.

behavioral mechanisms driving chlorine adoption in Akram and Mendelsohn (2021), while also assessing the broader implications of experimental design choices in the presence of within-treatment spillovers, or a complementarity between receiving treatment and being proximate to others who receive treatment.

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 (Chandrasekhar et al., 2022). 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. This distinction is important for policy. If experiential learning is infectious, the optimal policy would be to seed experiential learning to network-central individuals who can then "infect" others with this information. If experiential and social learning are complements, meaning that participants are most likely to sustainably change their behavior when they receive *both* information from their own experiences and from those of their social connections, the optimal policy may be to geographically saturate experiential learning (given a sufficiently strong within-treatment spillover and a sufficiently cheap intervention). Testing the same intervention at a new level of randomization enables us to do two things: first, to see if our results replicate under a new experimental methodology, and second, to identify the relevance of within-treatment spillovers, which a cluster-randomized methodology conceals.

Our evidence suggests that, in our context, experiential and social learning are indeed complements. Participants in the learning arm who also have a learning-arm neighbor use chlorine at a significantly higher rate than any other group. While, in the short run, those in the learning arm improve their chlorination behavior regardless of the presence of learning neighbors, this effect fades quickly. The complementarity between experiential and social learning appears essential to sustained use: the higher rates of utilization that we observe among participants in the learning arm disappear over time *unless* they have a neighbor who is also in the learning arm. Moreover, the behavior of participants outside of the learning arm is unchanged by having a learning-arm neighbor, suggesting that they cannot learn from their neighbor's experience unless they have experienced such learning 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 show the largest improvement in health. Specifically, the ITT (TOT) effects of the treatment on an index of child health (anthropometric measurements) is a 0.07 SD (0.24 SD, $p = 0.027$)

increase for any chlorine household compared to control (the complier sample for the TOT is any treatment household who reports using effective water purification technologies at endline); a 0.11 SD (0.33 SD, $p = 0.005$) increase for any learning arm participants compared to control (the complier sample for the TOT is learning arm participants who report using effective water purification technologies at endline, and thereby use chlorine more intensively throughout the entire study); and a 0.16 SD (0.51 SD, $p = 0.002$) increase for any learning arm participant with a learning arm neighbor compared to control (the complier sample for the TOT is learning arm participants with a learning arm neighbor who report using effective water purification technologies, the set of compliers who use chlorine *most* intensively throughout the entire study).

To discipline our exploration of the learning process that Info-Tool households may be engaging in, we outline a model of experiential learning. We specify that learning-by-doing might facilitate adoption, but *sustained* adoption from individual learning requires reinforcement. Our model departs from typical learning models by specifying that reinforcement is most effective when it comes from people who have been through a relatable learning experience.

We propose a model where actors are sensitive to the source of the information they receive when deciding to act on that information and are more responsive to signals over which they "take ownership". Following Conlon et al. (2022), individuals establish psychological ownership over information that they acquire through self-investment (i.e., costly action). We build on Conlon et al. (2022) by also allowing individuals to establish ownership over information that they acquire through a learning process about which they have intimate knowledge. The psychology literature establishes "self-investment" and "intimate knowledge" as antecedents to establishing a psychological ownership effect over an object or idea (Morewedge, 2021).[3] We argue that our intervention generates within-treatment spillovers by providing learning-arm participants with firsthand experience in acquiring information through the Info-Tool. By taking the action of completing the Info-Tool, these participants develop an "ownership effect" over that information. Moreover, this experience of engaging with the Info-Tool gives participants "intimate knowledge" of the process by which any signal is acquired via the Info-Tool, including those from their learning-arm neighbors. This allows learning-arm participants to effectively "take ownership" of information generated by their Info-Tool neighbors.

Participants follow a Bayesian learning process when forming their beliefs about the efficacy of a health technology. However, unlike standard models, our model enables individuals to weigh signals with *ownership* weights when choosing to adopt the technology; these weights are independent of signal sign, size, or precision.[4] Following Conlon et al. (2022), signals generated

[3]Morewedge (2021) also notes that "physical control" over an object is another antecedent to psychological ownership of that object. Since we are discussing ownership over information, which cannot be physically held, touched, or possessed, we do not consider this third antecedent to psychological ownership in our setting.

[4]These weights closely follow Conlon et al. (2022), who model an "ownership effect" as a weight placed on signals generated by oneself. Their model interprets action as indicative of beliefs – if information is not worth acting on, it has not truly been internalized. We depart from Conlon et al. (2022), and from standard models, by asserting that our ownership weights determine how readily an individual's beliefs translate into action. This

by oneself when taking a costly action are given higher ownership-weights than other signals. We build on Conlon et al. (2022) by also ascribing higher ownership-weights to signals that are generated (by the self or by others) through methods with which one has intimate knowledge. We interpret the Info-Tool as a technology that increases ownership-weights on information collected through it, as individuals place more weight on the health signals they receive when recording their health status than those that they observe but do not record. The model embeds a complementarity between one's own signals and others' signals by assuming that individuals develop intimate knowledge, and thereby take ownership, of signal observation methods that they have personal experience with. Consequently, if a participant has experience using the Info-Tool, she places more weight on health signals from somebody else whose information was also acquired through the Info-Tool.

Our model argues the mechanism driving changes in chlorination behavior is a change in the weights participants place on health signals when choosing to act on their beliefs. To test this mechanism, we first check whether individuals are responding to health signals at all. Using a machine learning algorithm on a host of baseline variables, we identify households that are *predicted* to experience health improvements during the intervention period. We find that, during the three months that incentives and the learning (Info-Tool) interventions are ongoing, the effects of the learning intervention on chlorination are 29% ($p = 0.049$) greater among households who are predicted to improve relative to those not predicted to improve within the learning arm, suggesting that participants indeed respond to the information that they observe in their environment.[5] This separation does not exist for the same predicted-improved households in the chlorine or incentives arms, suggesting that the pattern we identify is unlikely to be due to selection.[6]

Next, we check whether learning-arm participants are responding to their learning-arm neighbors' health signals. We find that the spillover is entirely explained by learning-arm neighbors whose diarrhea rate was ex-ante predicted to improve. Learning-arm participants do not respond differentially when they have access to non-learning-arm neighbors who were predicted to improve, bolstering 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.

We model the Info-Tool as a technology that generates ownership over any signal acquired with the Info-Tool through "self-investment" (own signals) and "intimate knowledge" (others'

addresses a phenomenon we document whereby individuals update their beliefs about a technology's efficacy in a way that appears sophisticated and rational, demonstrate high levels of certainty in the technology's efficacy, and yet still do not adopt the technology. We model ownership effects as an input into behavioral change, but not explicit knowledge or understanding of a technology's efficacy.

[5]This effect continues but diminishes over the course of the fourteen months, likely because the other Info-Tool households are also learning over time.

[6]Reassuringly, participants who are predicted *ex-ante* to observe null or negative health signals during the facilitated learning experience are *no less likely* to chlorinate their water than other participants outside the learning arm, indicating that the potential for misattribution to lead to negative belief formation about the health technology is not realized.

signals). An alternative, and perhaps a more straightforward, model is one whereby the Info-Tool improves signal precision, but only people who have used it are aware that Info-Tool signals are more precise. If the Info-Tool affects signal precision, then stated beliefs should update in concert with chlorine adoption in the Info-Tool group. These Info-Tool participants should in turn *convey* (in conversation) a different set of beliefs to others. Conversely, if the Info-Tool affects psychological ownership over signals, then stated beliefs and conversational content need not update along with chlorine adoption.

To disentangle these competing mechanisms, we elicit participant's beliefs in and memory about the efficacy of chlorine in reducing children's diarrhea. We find *no* evidence that participants in the learning arm are more likely to believe that chlorine is an effective technology, to have an accurate memory about its effectiveness in their own lived experience, or to share different conversational content about chlorine with their neighbors.[7] In other words, it does not appear that the Info-Tool alters signal precision. Instead, learning arm participants report being significantly more *motivated* to use chlorine to achieve a standard of health for their families; we interpret this change in motivation as a stated belief that directly relates to their desire to act on the information that they have. Learning arm participants are also significantly more likely to believe that other learning-arm participants know more about child health than participants in the chlorine or incentives arms; we interpret this as evidence of more intimate knowledge of the source of learning.[8] While nearly all households express that chlorine is a useful and effective technology, the Info-Tool appears to generate psychological ownership over that information, which thereby motivates action. These results are consistent with recent work from Hussam et al. (2023) and Fafchamps et al. (2024), whose interventions induce changes in behavior with no changes in beliefs or knowledge.

We are able to rule out habit formation, a common confounder to learning-through-adoption in the literature, as a competing mechanism by comparing the learning arm to the incentives arm.[9] Indeed, 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 immediately revert to a lower rate of chlorine use after the withdrawal of incentives, suggesting that water chlorination is not a habit-forming activity in our context. As detailed in the paper,

[7] Our questions about beliefs in chlorine efficacy were simple to answer, incentivized, and asked during the period when the Info-Tool treatment effect is strongest. We document that educated respondents are more likely to answer accurately, suggesting that the questions are sensitive to comprehension skills.

[8] This analysis takes into account the propensity to choose someone from their own treatment group (see Table 6).

[9] While we model the learning arm as a program that facilitates participants in generating an "ownership effect" over health signals gathered from the Info-Tool, it may instead work by leading to higher rates of early adoption which creates habit formation via building a greater 'consumption stock' of chlorination. Incentives for chlorine use should lead to high rates of early adoption *without* helping participants observe more salient health signals, so habit formation (or any technology adoption mechanism that relies on early adoption) should be realized in this treatment arm. We can also use the incentives arm to rule out an alternative learning mechanism, which is that early adoption leads to the accumulation of *more* signals. Salient signals may work more effectively if participants have limited attention (Hanna et al., 2014). However, if participants can attend to and learn from many signals, these signals will be more likely to converge to the truth, which will decrease the potential for misattribution. Our evidence is consistent with Hanna et al. (2014), and suggests that participants are not able to attend to the many signals that they generate without the Info-Tool making these signals salient.

we are able to rule out a variety of other competing mechanisms as explanations for the social learning we observe, including social network formation, differential communication, mimicry, and changing social norms.

Our model of learning and technology adoption is founded in an extensive body of evidence documenting that people are hesitant to trust health information relayed by experts (Alsan and Wanamaker, 2018; Alsan and Eichmeyer, 2024; Darden and Macis, 2024; Lowes and Montero, 2021; Martinez-Bravo and Stegmann, 2022; Banerjee et al., 2023), yet update their beliefs *or actions* strongly in response to personal experiences (Bennett et al., 2018; Conlon et al., 2022; Corno, 2014; Malmendier and Nagel, 2015; D'Acunto et al., 2021; Simonsohn et al., 2008). While there are numerous reasons to be skeptical of learning through one's personal experience – particularly in the health domain, which is precisely why the medical community relies on clinical trial evidence to inform medical and behavioral recommendations – reliance on experts or networks to disseminate information may inadvertently deepen social and economic inequities or undermine intervention efficacy. This is due to 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).[10]

Our study makes three contributions. First, we add to the literature on technology adoption and behavioral change by identifying – what to our knowledge is both the first empirical evidence and theoretical formulation of – a complementarity between individual experience and social learning. In our model, this complementarity is a key underlying mechanism by which repeated exposure to, or experience with, a good or behavior translates into long-term adoption. We generate evidence for this model using a field experiment that suggests that actors are sensitive to *how* information is acquired and more readily take ownership over information whose source they are familiar with. This may contribute to why people more readily trust in-group members with health information (Alsan et al., 2019; Alsan and Eichmeyer, 2024). Furthermore, we show that the learning treatment changes behavior without changing underlying beliefs about the efficacy of chlorine, adding to a small but growing literature that shows that information that individuals possess is not the only, and potentially not the primary, driver of behavior change. Lab-in-the-field evidence suggests that an ownership effect over information that an individual herself generates might be important in determining if this information stimulates behavioral change (Conlon et al., 2022). We document the importance of taking action to generate signals for learning outside the lab in a setting with high-stakes decision-making. We build on Conlon et al. (2022) by showing that, under the right conditions, people can also take ownership of information that comes from other people. Notably, we are one of few papers in the learning-through-adoption literature that can definitively rule out habit formation as

[10]Health is a high-dimensional problem that is subject to random shocks and reliant on many inputs. These inputs are sometimes within an individual's control (for example, compliance with medical treatment, nutrition, and exercise), but oftentimes outside an individual's control (for example, pollution or traffic accidents) and potentially unobservable (for example, pathogens). Furthermore, health outcomes are noisy and rare occurrences that are difficult to observe precisely. Misattributing health inputs to health outcomes could bias beliefs and lead to suboptimal health behavior.

a confounding mechanism to learning, two behavioral processes that are often unknowingly conflated in existing work (see Section I.2 for a detailed review of the existing literature).

Second, we add to the literature on the methodology of randomized controlled trials by demonstrating the significant implications of interpreting estimates of treatment effects from cluster-randomized designs as purely individual treatment effects. Without random variation in exposure to other treated units among *treated units* themselves, within-treatment spillovers are not identified and the threat of SUTVA violation is not eliminated. Thus, estimates from cluster-randomized designs with full treatment saturation conflate individual treatment effects with within-treatment spillovers, and extrapolating these estimates to other settings (for example, settings with sparse treatment implementation that may not generate the same within-treatment spillovers) can bias analyses. 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, in which individual and within-treatment effects are implicitly conflated. Comparing our results with AM—which, aside from its level of randomization (clustered vs. our individual-level design), is similar to our experiment—a back-of-the-envelope calculation suggests that interpreting the estimates from the cluster-randomized experiment as the treatment effect of receiving the intervention without spillover effects (i.e., using the Info-Tool without any neighbors using it) would have led to an overestimation of the individual treatment effect by a factor of six.[11] Technology adoption and communication are fields of economic research whose experimental designs are *more* often cluster-randomized with full treatment saturation *than any other experimental design*, thereby concealing within-treatment spillovers.[12]

Third, this finding has significant implications for scaling policy interventions. 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 and scales an intervention in policy design. We operate in a setting with significant policy implications for scaling education and information campaigns in preventive health. The returns to preventive health behaviors, such as handwashing with soap, using clean cookstoves or bednets, or treating water, are particularly challenging to perceive the returns to. Existing literature on using social networks for information dissemination has focused on seeding information. This literature often assumes that everyone can learn from social connections if the information reaches them, but seeding omits the value of the experiences of the person *receiving* the information. Our evidence suggests that, for some types of learning, these assumptions could seriously undermine the potential

[11] In both trials, we can estimate the combined individual and within-treatment spillover treatment effect. In the individually-randomized trial, we can separately estimate the individual treatment effects and within-treatment spillover effects, and find that the individual treatment effects account for 15% (or less than one-sixth) of the combined individual and within-spillover treatment effect. If we assume that the ratio of individual treatment effects to within-treatment spillover effects is consistent across trials, then only 15% of our estimate of treatment efficacy in the cluster-randomized trial is explained by individual treatment effects.

[12] See Section H for an analysis of trials registered to the American Economics Association RCT Registry.

for individuals to learn from disseminated information. In our setting, personal experience is essential to acting on information disseminated by others. *Own experience cannot be seeded*, suggesting that there might be large efficacy costs in seeding, rather than saturating, certain types of information.[13]

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 6 presents results on child health; and Section 7 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 6 months and 5 years old at baseline, and if the caregiver is generally home during the day. In all but one case the caregiver is a female adult, typically the mother of the children. Furthermore, we screen households on several dimensions of water usage to ensure that chlorine is useful and effective for improving children's health.[14] Although 79% of households primarily drink centrally-delivered water that is piped into their own household or plot at baseline, we do not detect chlorine in the drinking water of a single household. By endline, 70% of households changed their water delivery method (non-differential by treatment group), with the majority of households (56%) now receiving water from a public tap or standpipe (32% still primarily drink piped water into the household).

## 2.1 Baseline Survey, Randomization, and Balance

Our baseline survey of these 1,800 eligible households ran from mid-May to early June of 2022. We collect data on chlorine exposure, chlorine knowledge, diarrhea prevalence,[15] and

[13]Note that subsidizing learning costs through experiential tools, such as the Info-Tool, can generate positive health externalities not only through individual learning but also by enhancing social learning, as households receiving support indirectly reinforce their neighbors' adoption. While the first mechanism justifies, for instance, subsidization of chlorine tablets, the second mechanism makes the case for subsidizing learning costs via offering technologies such as Info-Tool that facilitate both individual and social learning.

[14]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 F.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.

[15]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).

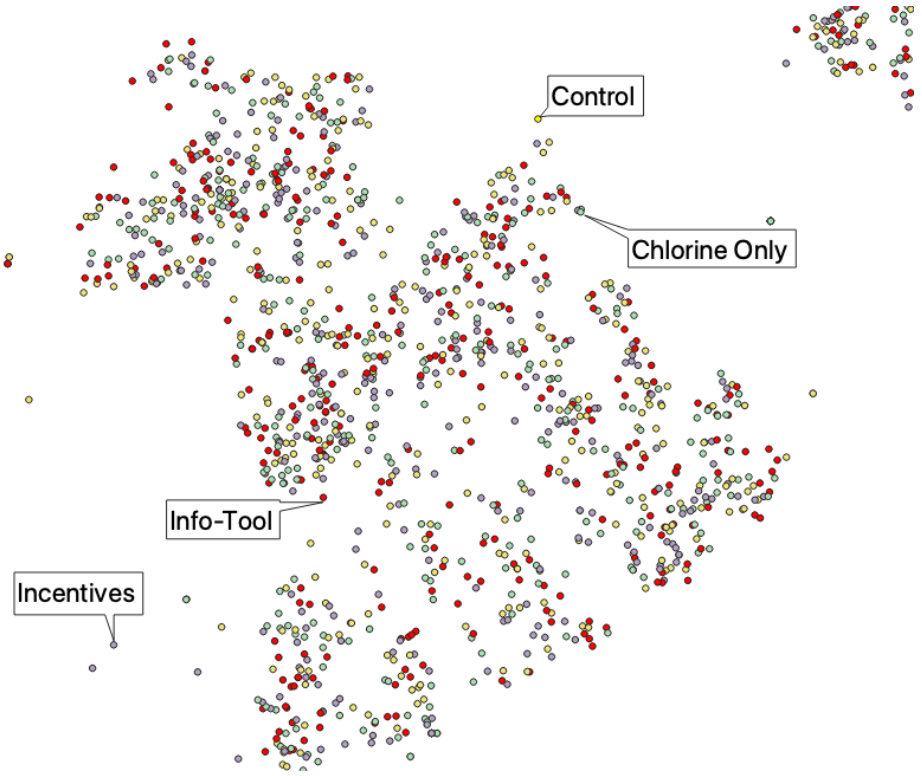



Figure 1: Distribution of Households Across Treatment Arms

child anthropometrics.[16] 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 can not properly identify what chlorine tablets are used for. Despite low awareness, 87% percent report that they are open to using chlorine tablets after enumerators explained 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:

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

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.

[16]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.

*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)

Treatment groups are balanced on most, but not all, baseline measures. In Tables A.1 and A.2, we compare each treatment group with the pure control group. 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 A.1), or when considering both a binary measure and the number of diarrhea-days at the child level (Table A.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.

| | | Phase 1 | | Phase 2 | | |
|---|---|---|---|---|---|---|
| | | Round 1 | Round 2 | | | |
| | Month 1 | Pre-Treatment: Months 2-4 | Treatment: Months 5-7 | Post-Treatment: Months 8-18 | Month 19 | Month 24 |
| **Chlorine** | Baseline Survey | | Chlorine Distribution | Chlorine Distribution | Endline Survey 1 | Endline Survey 2 |
| **Incentives** | | | Chlorine Distribution + Incentives | | | |
| **Info-Tool** | | Info-tool Training | Chlorine Distribution + Info-tool | | | |
| **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. Throughout Round 1, households are aware of their treatment status and we train T2 households on how to use the Info-Tool chart. In Round 2, we distribute chlorine tablets to all three treatment groups and deliver the incentives treatment to T3 participants, while T2 households continue to use the Info-Tool chart. Figure 2 lays out the study timeline and key events.

### 2.2.1 Round 1 of Phase 1

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

involved a few key components: 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.[17] Since chlorine distribution had not yet begun, and chlorine tablets are not widely available in local markets, ten percent of households are randomly selected for chlorine testing during every other visit during Round 1 of Phase 1. Chlorine testing in this period was simply to ensure that knowledge of treatment status does not affect people's ability to procure chlorine tablets on their own.

In the first Phase 1 visit, we reveal treatment status to caregivers. We explain each treatment group to caregivers and tell them that a lottery will determine their treatment status. We explain that the lottery will run through the enumerator's tablet, and that whichever number appears on the screen will determine their treatment status. These numbers are pre-loaded so as to deliver the treatment status we pre-assigned. After treatment status is revealed, we remind participants about what being a part of their treatment group entails.

*Info-Tool*

To activate the 'learning' mechanism, participants in T2 receive the Info-Tool chart on the first visit of Phase 1.[18] The Info-Tool chart is a simple pen-and-paper chart that allows caregivers to track their children's diarrhea (Figure F.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). When we use the diarrheal incidence in the Control group, the benchmark is updated daily so as to reflect the past fourteen days of data collection from the pure control households. We switched from using the diarrhea rate in our Control group to the diarrhea rate in the epidemiological literature out of concern that the incidence of diarrhea in the Control group would be impacted by a reduced disease environment due to the presence of treated households in their community. Enumerators clearly explained to participants that the

[17]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.

[18]The study was non-blinded 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-blinded. Community health workers used electronic tablets and smartphones to collect data.

benchmark rate was the average rate of diarrhea from people in the community who did not use chlorine to purify their water.

This simple, visual paper-and-pencil tool was borrowed from Akram and Mendelsohn (2021), which is comfortable for low-literacy-and-numeracy caregivers to use. In this way, T2 caregivers are able to compare their children's diarrheal incidence with the average diarrheal incidence in households that do not use chlorine tablets, both before they were offered chlorine tablets (months 1-3 of the intervention) and after (months 4-6 of the intervention).

### 2.2.2 Round 2 of Phase 1

Round 2 of Phase 1 began in late August 2022 and continued for three months. Throughout this round, we continue to visit households every 2 weeks and commence the distribution of chlorine tablets to all T1, T2, and T3 households. 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 commence, and the Info-Tool continues.

*Incentives*

Caregivers are offered tokens redeemable for child and household goods if they can show empty chlorine tablet wrappers as proof of usage. The chlorine tablets come individually wrapped and participants are instructed to save the empty wrappers in a pouch provided for them. Each daily reward for proper chlorine use is equal to approximately 5 US cents (with 'proper use' calibrated to the household's pre-intervention water consumption). To hold income effects constant, we also give comparable products to participants in the remaining groups – *unconditioned* on chlorine tablet wrappers – as a token of appreciation for participating in our surveys. Since T3 households can redeem tokens for household goods that had varying values, we implemented 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 F.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 difference-in-differences.

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 J).

## 2.3 Phase 2: Monthly Visits

Phase 2 began in late November 2022 and continued through October 2023. In this phase, we visit households once per month. We continue to distribute chlorine tablets and test water for the presence of chlorine residual but cease provision of incentives or gifts, no longer inform Info-Tool participants of their own children's diarrhea rate relative to the benchmark diarrhea rate, and stop providing Info-Tool sheets or helping Info-Tool participants track their children's diarrhea rates.

## 2.4 Endline Visits

The first endline survey was completed in December 2023 through January 2024. In this round, we collect all measures from baseline, including diarrhea rates, presence of chlorine residual, and child anthropometrics, including weight, mid-upper arm circumference, and height. We additionally elicit willingness-to-pay for chlorine tablets using a take-it-or-leave-it offer with a randomized price[19], and detailed social network data to understand potential mechanisms for spillovers, including directly serving neighbors chlorinated water (through meal-sharing, for example) and indirectly through conversations about the project and lessons learned through the Info-Tool.

We achieve an 87% followup rate in the first endline. Incentives households are most likely to attrit (14.9%), and are 4.2 percentage points more likely to attrit than Control ($p < 0.05$; Table A.1). However, across a range of baseline observables, the endline Incentives respondents do not statistically differ from their counterparts in the other arms (Tables E.5 and E.6). Moreover, none of our primary analyses at endline rely on a comparison between Incentives and Control. Our primary analyses compare Info-Tool with Control or the Chlorine Only group, each of whose endline attrition is non-differentiable from the other.

We conducted a final short followup survey in June 2024 in order to (1) provide participants with chlorine tablets for the summer months, the period in which diarrhea rates peak, (2) monitor stockpiling of chlorine tablets, and (3) ask additional survey questions to disentangle mechanisms.[20]

Taken together, we administer 26 surveys per household: a baseline survey, 11 rounds of bi-weekly visits in months 1-6 of the intervention (Phase 1), 12 monthly visits in months 7-18 of

[19] 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).

[20] 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 only 3% of respondents reported that they still had chlorine tablets remaining in this final followup survey, 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 greater than one month.

the intervention (Phase 2), one endline visit immediately after Phase 2, and a second endline visit four months later. Our primary outcome, chlorine residual in drinking water, is recorded in nineteen of these visits (baseline; bi-weekly visits 6, 8, 10, and 11; and every visit thereafter).

### 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. Upon distributing free chlorine tablets, CHWs test for chlorine residual in each household's drinking water vessel every month 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). The presence of chlorine residual is an objective measure of chlorine tablet use that cannot be manipulated. However, chlorine residual only presents itself if the chlorine tablet was added to the water in the past twenty-four hours, meaning we may not capture a treatment effect for households who imperfectly chlorinate their water.

Our measures of child health are self-reported diarrhea incidence and child anthropometrics. Diarrheal incidence is measured by the number of days of diarrhea across all children under five years old, which we collect in every visit. While we do collect data on the frequency of childhood diarrhea among households, we do not use diarrhea as our only measure of child health because it is subject to measurement and misclassification errors. Firstly, our interventions, particularly Info-Tool, are likely to change the reporting of diarrhea in children, since we explicitly encourage participants to track diarrhea. Thus, we may capture a treatment effect on reporting, rather than a treatment effect on actual incidence of diarrhea. Secondly, the presence of non-infectious diarrhea and asymptomatic infection make diarrhea presence a poor outcome for measuring the effectiveness of water and sanitation interventions (Watson et al., 2022).

Frequent diarrhea can result in poor absorption of nutrients, leading to sub-optimal physical development in children. As such, we supplement diarrhea incidence with child anthropometrics, including height-for-age, weight-for-age, weight-for-height, and mid-upper-arm-circumference-for-age, which we collected at endline among children under 80 months old[21].

## 3 Model of Learning and Technology Adoption

We present a Bayesian model of learning about a technology, following the notation of Kondylis et al. (2023), but modify the model to include "ownership weights," which leads some signals to spur behavior change independent of the signal sign, magnitude, or precision. In our model, ownership weights do not affect explicit knowledge about the technology itself, but instead affect the way that posterior beliefs translate into action. Let $A$ be a distribution of prior

[21]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.

beliefs about the efficacy of the technology:

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

In each period, individuals receive a 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 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). Weights $\ell \equiv \sigma_0^2/(\sigma_0^2 + \sigma_Y^2)$ represent learning. Then, participants' updated uncertainty is $\sigma^2 \equiv (1 - \ell)\sigma_0^2$.

## Ownership Effects

We consider two margins along which information affects technology adoption. First, some information makes people more knowledgeable about the efficacy of the technology, which they incorporate into their posterior beliefs $M$. This knowledge will be reflected in their stated beliefs. Second, psychological ownership over certain pieces of information make their corresponding beliefs more or less actionable. In other words, people can intellectually understand and articulate the returns to a technology, but it is their psychological ownership over the signals that construct their beliefs that affects their decision to act on that knowledge.

Let $\alpha_Y \in [0, 1]$ be a weight determining the psychological ownership of a signal that an individual experiences, where $\alpha_Y = 1$ is a signal over which she experiences complete psychological ownership, and $\alpha_Y = 0$ is a signal over which she experiences no ownership. The psychological ownership that an individual experiences for a signal is not related to the precision of the signal itself, which is modeled in the variance $\sigma_Y^2$. Instead, the ownership weight will translate into an individual's willingness to act on information, *regardless* of the informational content or uncertainty in the information. While $M$ represents *stated* posterior beliefs about the returns to the technology (or explicit knowledge), we now define $M_\alpha \equiv \ell\alpha_Y Y + (1 - \ell\alpha_Y)\mu_0$ to be the ownership-weighted posterior belief. An individual adopts the technology when $M_\alpha > C$, where $C$ is the cost of adopting the technology.[22]

A model that would generate a similar pattern of adoption is one where the intervention leads certain signals to be more precise (lower $\sigma_Y^2$), rather than increasing the ownership-weights $\alpha_Y$ of those signals. However, with a constant $C$, this model requires that stated posterior beliefs and adoption move together. Conversely, our model *does not* necessitate this prediction, and instead allows stated posterior beliefs and adoption dynamics to follow different patterns from

[22] In a standard model, participants adopt the technology when $M > C$

one another.

Let $\gamma_Y \in \{\Gamma\}$ be the source of the signal $Y$ from a set of potential sources $\Gamma$. Let $c_{\gamma_Y}$ denote the cost-of-action that an individual herself bears to acquire signal $Y$ from source $\gamma_Y$ ("self-investment"). Let $IK_{\gamma_Y}$ be "intimate knowledge" of the information acquisition source $\gamma_Y$. We make two key assumptions: ownership weights $\alpha_Y$ are increasing in $c_{\gamma_Y}$ and $IK_{\gamma_Y}$ (ownership effects); and "intimate knowledge" $IK_{\gamma_Y}$ depends on the stock of previous personal experience acquiring signals from $\gamma_Y$, defined as $E_{\gamma_Y}$.

$$\alpha_Y = \alpha_Y(c_{\gamma_Y}, IK_{\gamma_Y}, \cdot)$$

$$IK_{\gamma_Y} = IK_{\gamma_Y}(E_{\gamma_Y}, \cdot)$$

## First period adoption

Initial priors are diffuse: at baseline, 12.7% of respondent 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 chlorine, $Y_{CHW}$. It is likely that $\sigma^2_{Y_{CHW}}$ 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 free 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 induce costly-action to acquire signals in the first three months of chlorine distribution, which increases $c_{\gamma_Y}$ in these months. In changing the signal observation technology $\gamma_Y$ in these months, we also increase $E_{\gamma_Y}|\gamma_Y = IT$ in perpetuity. Consequently, we increase intimate knowledge about the information acquisition process for any Info-Tool signal $IK_{\gamma_Y}|\gamma_Y = IT$. This will be true both for 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 her signal through the Info-Tool.

## Comparative statics:

**Shock to $C$ for Incentives:**

*Prediction 1: Increased contemporaneous adoption relative to Chlorine Only*

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.

**Shock to $c_{\gamma_Y}|\gamma_Y = IT$ for Info-Tool:**

*Prediction 2: Increased contemporaneous adoption relative to Chlorine Only among individuals with larger early-period health signals, with fade-out*

The Info-Tool leads to an increase in $c_{\gamma_Y}$ 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, and stops providing gifts to the Incentives groups, costs are the same for all three groups. However, Info-Tool participants' ownership-weighted prior is higher at the time that the behavioral interventions end. Thus, we should see higher average use immediately after the behavioral interventions end.

Since individuals learn from short-term health signals, we should expect to see 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. However, we should expect this effect to fade out as all participants acquire new signals from their observations – signals upon which the Info-Tool group, no longer using the tool, no longer places higher weights.

Notably, placing higher weights on early health signals is not the only way in which Info-Tool participants differ from the other groups. Info-Tool participants also place higher weights on signals from *other* Info-Tool participants (described in Prediction 3 below), which may supersede heterogeneity by individual early health signals. As such, heterogeneity by early health signals is suggestive of acting on observed signals, but the absence of heterogeneity by health signals is not dispositive.

**Shock to $IK_{\gamma_Y}|\gamma_Y = IT$ for Info-Tool:**

*Prediction 3: Increased adoption relative to Chlorine Only among Info-Tool individuals with connections to other Info-Tool participants*

As soon as an Info-Tool participant has used the Info-Tool, which is a shock to $IK_{\gamma_Y}|\gamma_Y = IT$ through $E_{\gamma_Y}|\gamma_Y = IT$, 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 network connections' health signals than caregivers in other treatment groups. This can have effects after the Info-Tool period has ended, if participants are sharing cumulative, rather than period-specific, information about their experiences.

As a check on the plausibility of our theoretical framework, in Appendix G we plot chlorine adoption patterns predicted by our model using simulated data. Under a set of reasonable

parametric assumptions outlined in Appendix G, we document adoption patterns that closely follow our empirical results, which we present below.

# 4 Results: Chlorination

## 4.1 Average Treatment Effects

We begin by plotting chlorine use by treatment groups across the study period (Figure 3). The short-run is defined as Phase 1 (weeks 12-20). The medium-run spans the first three months of Phase 2 (weeks 22-32). The long-run spans the remainder of Phase 2 (weeks 36-72).

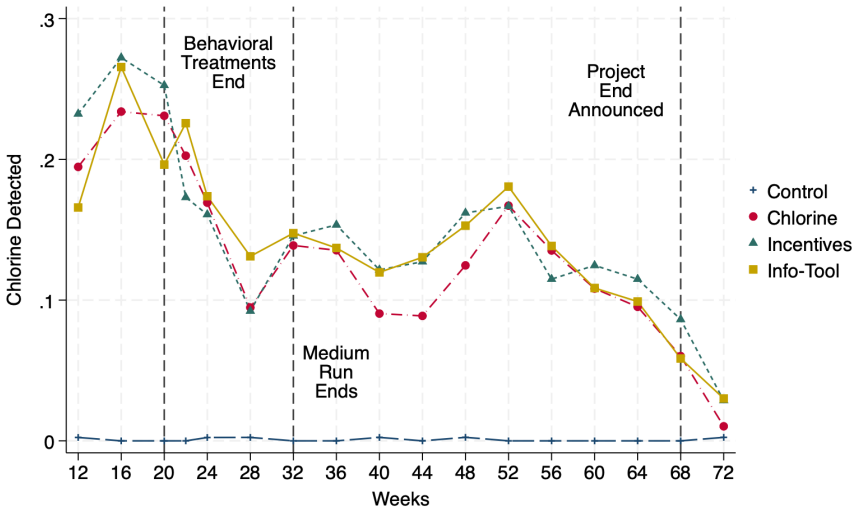



Figure 3: Seasonal Time Trends in Chlorine Detection (Raw Data)

Free chlorine distribution leads to a large increase in chlorination in all treated groups. As we will see in Section 6, there are large decreases in reported child diarrhea and improvements in child anthropometrics in all groups, indicating that free chlorine distribution alone, even with low levels of perfect compliance, can have substantive impacts on child health. On aggregate, the impacts of the behavioral interventions are small in magnitude and at times statistically insignificant. We highlight two important patterns: first, during the short-run, Incentives households chlorinate significantly more than those in Info-Tool or Chlorine Only. Immediately following incentive withdrawal (week 20) however, chlorination rates for incentive households plummet, while the decay among Info-Tool households is gradual. Chlorination patterns across the treatment groups appear, on average, to follow one another thereafter.

We detect chlorine residual in the water of over 20% of treatment households during the short-run. In the medium run, chlorination rates decline. Info-Tool households chlorinate significantly more than their Incentive and Chlorine Only counterparts throughout this period. In the long run, however, Incentives households and Info-Tool household converge around week 32, with households in Chlorine converging by week 52 (eight months after the end of interventions). The last four months of the study period see a decline in chlorination rates across all groups.

While our trial is not designed to identify the causes of chlorine use patterns across time, the serial pattern follows diarrhea rates, with all three groups chlorinating at higher rates during the high-diarrhea season. Chlorination rates fall most sharply in the final two months of the study, which coincide with the moment we inform participants that the study - and therefore chlorine provision - is near its end, which may have led them to ration their existing chlorine tablets.[23]

Our main empirical specification is the following:

$$Y_{i,s} = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + Y_{i0} + X_{i0} + \delta_s + \gamma_b + \epsilon_{i,s}, \tag{1}$$

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[24], 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.[25]

Figure 4 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 C.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 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.

[23]Overall trends in chlorine use might be explained by seasonality and by experimental changes that affected all participants.

Seasonality: Diarrheal prevalence peaks in the summer, during the hottest and rainiest months. We began our study at the peak season, which allows chlorine use to generate starker changes in diarrhea rates (thereby giving the Info-Tool a greater chance of showing the efficacy of chlorine). Chlorine use tracks closely with self-reported diarrhea, when both diarrhea rates and chlorine use rates fall (Figure C.1). In the second summer of the experiment, diarrhea rates chlorine use 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, who 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 that 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 greater than one month.

[24]Section 4.2 details the selection of this bandwidth following Egger et al. (2022).

[25]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.

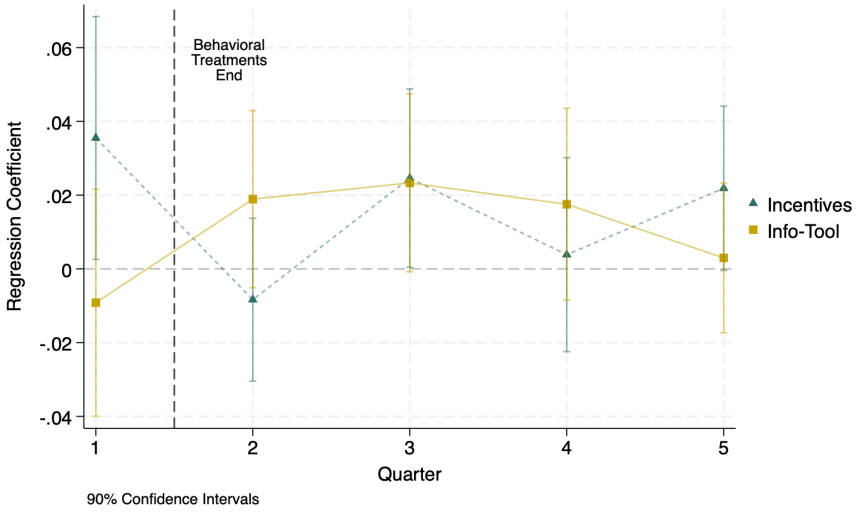



Figure 4: Regression Coefficients of Chlorine Detection (Omitted: Chlorine Only)

## 4.2 Learning Spillovers

In our model, individuals update their beliefs about health technologies when they receive signals from themselves *and others*. Motivated by this theory, we investigate the effects of learning spillovers. Learning spillovers are also a natural potential explanation for why we are not able to replicate the large positive Info-Tool treatment effects that AM find, as we randomize the Info-Tool treatment at the individual level while AM randomizes at the neighborhood-block level.

Households are, on average (median), 13.6 (10.4) meters from the closest other participant; 16.0 (12.1) meters from the closest participant in any chlorine treatment group; and 27.5 (22.7) meters from the closest Info-Tool participant. 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 by random variation in the treatment assignment (Table A.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.[26] 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.

[26]There are no Incentives or Pure Control groups in AM.

Motivated by our theoretical framework of learning and the contextual differences between our study design and AM, we now test for learning spillovers from Info-Tool participants onto their neighbors. We find that the Info-Tool treatment has powerful spillovers, but these spillovers are only absorbed by other Info-Tool participants. In other words, there is an Info-Tool-to-Info-Tool spillover.

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 $AnyT2_i^r$, 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, $\sum_{r=20}^{R} \theta_r(\overline{AnyT2_i^r})$ 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 E.1), and within each individual treatment group (Tables E.2, E.3, E.4).

We test for the presence of learning spillovers using the following specification:[27]

$$\begin{aligned} Y_i =& \theta_0 + \theta_1 T1_i + \theta_2 T2_i + \theta_3 T3_i + \sum_{r=20}^{R} \theta_{4,r} AnyT2_i^r \\ &+ \sum_{r=20}^{R} \theta_{5,r} AnyT2_i^r \times T1_i + \sum_{r=20}^{R} \theta_{6,r} AnyT2_i^r \times T2_i + \sum_{r=20}^{R} \theta_{7,r} AnyT2_i^r \times T3_i \\ &+ Y_{i0} + X_{i0} + \gamma_{1,b} + \epsilon_i \end{aligned}$$

Then $\theta_{6,r}$ is our estimate of Info-Tool to Info-Tool spillovers. We use $\theta_{4,r}$, $\theta_{5,r}$ and $\theta_{7,r}$ to rule in or out spillovers from Info-Tool onto other groups.

We find that, while the presence of Info-Tool participants within twenty meters does indeed increase chlorine detection, this is only true for *other Info-Tool participants*. Figure 5 plots raw chlorination rates across these groups, while Table 1 and Figure 6 present the regression analogs. This pattern emerges in the medium-run (the quarter after behavioral treatments end) and

[27] 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.

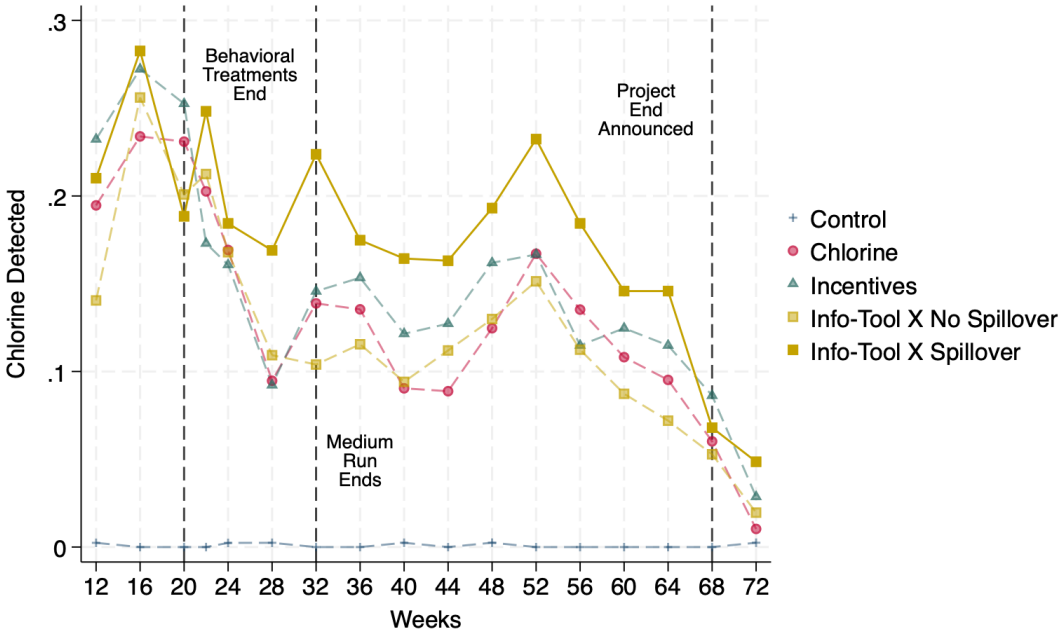



Figure 5: Seasonal Time Trends in Chlorine Detection (Raw Data)
where "Spillover" is exposure to any Info-Tool neighbor

persists through the end of the study. In the medium-run, Info-Tool participants with another Info-Tool participant neighbor chlorinate 26% more often ($p = 0.072$) than participants with no Info-Tool neighbor. There are no meaningful differences in use (statistically or in magnitude) by this spillover measure in either the Chlorine Only or Incentives groups. In the long-run, Info-Tool participants with another Info-Tool neighbor still chlorinate 30% more frequently ($p = 0.061$). Again, there are no differences in the Chlorine Only or Incentives groups by presence of an Info-Tool neighbor. 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 C.2). We pre-specified the panel specification as our main empirical strategy, along with a robustness exercise using a 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)). The results hold in this exercise, and in fact are more precise. Info-Tool participants with another Info-Tool neighbor chlorinate 32% more often in the medium-run ($p = 0.012$), and 31% more often in the long run ($p = 0.034$) (Appendix Section D).

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 or Incentives participants onto any treatment group, including their own (Tables C.3 and C.4). Second, we show that, among non-Info-Tool participants, chlorine use remains constant in the number of Info-Tool neighbors (Figure C.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

Figure 6: Regression Coefficients of Chlorine Detection (Omitted: Pure Control)
where "Spillover" is exposure to any Info-Tool neighbor

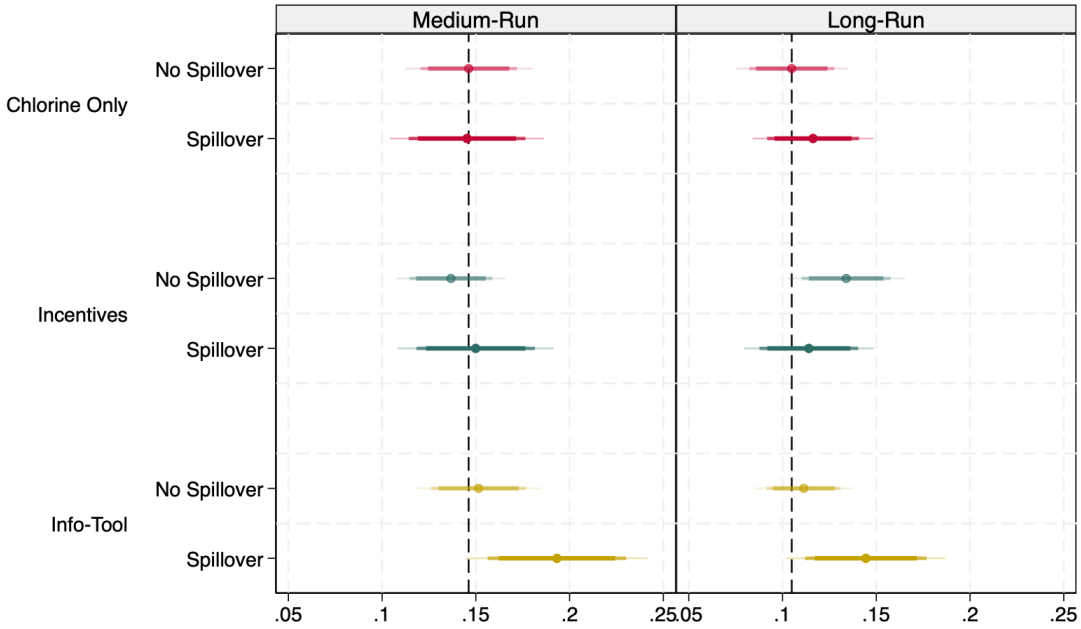



Table 1: Chlorine Detection (Household-Survey Panel)
where "Spillover" is exposure to any Info-Tool neighbor

| | (1) Short-Run | | (2) Medium-Run | | (3) Long-Run | |
|---|---|---|---|---|---|---|
| Chlorine × No Spillover | 0.219*** | (0.018) | 0.146*** | (0.013) | 0.105*** | (0.012) |
| Chlorine × Spillover | 0.219*** | (0.024) | 0.145*** | (0.016) | 0.116*** | (0.013) |
| Incentives × No Spillover | 0.248*** | (0.018) | 0.137*** | (0.011) | 0.134*** | (0.012) |
| Incentives × Spillover | 0.259*** | (0.024) | 0.149*** | (0.016) | 0.114*** | (0.014) |
| Info-Tool × No Spillover | 0.202*** | (0.017) | 0.152*** | (0.013) | 0.111*** | (0.010) |
| Info-Tool × Spillover | 0.211*** | (0.022) | 0.192*** | (0.019) | 0.145*** | (0.016) |
| Observations | 4711 | | 6354 | | 14066 | |
| **P-values:** | | | | | | |
| Chlorine: No Spillover = Spillover | 0.998 | | 0.942 | | 0.450 | |
| Incentives: No Spillover = Spillover | 0.726 | | 0.521 | | 0.232 | |
| Info-Tool: No Spillover = Spillover | 0.742 | | 0.072 | | 0.061 | |

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) than they would be in expectation based on endogenous spatial factors. See Section 5 for a detailed explanation of how the measure is defined.
* $p < .1$, ** $p < 0.05$, *** $p < 0.01$

health signals.[28] Third, in Appendix B, we reanalyze the AM data to test for within-treatment

[28]Because Info-Tool participants are using chlorine at a higher rate in the medium-run, it is possible that the diarrheal burden is lower in neighborhoods with more Info-Tool participants during this time period, which Info-Tool participants are more likely to notice. However, in the short-run, Incentives are using chlorine at higher rates, 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. Similarly, since Info-Tool participants with

Info-Tool spillover effects and find comparable effects.

# 5 Mechanisms

Why does a simple pen-and-paper chart lead to behavioral change? In our model, taking costly action to acquire health signals with the Info-Tool induces an "ownership effect", whereby signals acquired from the Info-Tool are given higher weights than other signals when deciding to act on beliefs. Personal experience using the Info-Tool further allows participants to take ownership over signals acquired through the Info-Tool *by other people* because they have intimate knowledge of the Info-Tool as a signal acquisition technology.

We report estimates for our main model parameters in Table 2: explicit knowledge or beliefs about chlorine efficacy $M$, ownership-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 ownership 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 Ownership 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 who receive positive signals.

Next, we use evidence from stated beliefs to argue that Info-Tool participants apply higher weights to Info-Tool signals because they take ownership over health signals generated via the Info-Tool, and not because the Info-Tool affects signal uncertainty and explicit posterior beliefs.

### 5.1.1 Responsiveness to Health Signals

Average Effects

an Info-Tool neighbor are the most likely participants to use chlorine, it is possible that these neighborhoods have an improved disease environment, and that changes in the disease environment create long-term use. If this is the case, then non-Info-Tool participants with access to *two or more* Info-Tool neighbors should respond similarly as Info-Tool participants with access to at least *one* Info-Tool neighbor.

Table 2: 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 × Spillover | 0.049 | (0.047) | -0.097 | (0.093) | 0.150 | (0.202) |
| Incentives × No Spillover | 0.014 | (0.039) | 0.053 | (0.078) | 0.210 | (0.169) |
| Incentives × Spillover | 0.013 | (0.047) | 0.054 | (0.092) | 0.112 | (0.201) |
| Info-Tool × No Spillover | 0.033 | (0.040) | 0.113 | (0.078) | 0.089 | (0.169) |
| Info-Tool × Spillover | 0.003 | (0.047) | 0.229** | (0.092) | 0.587*** | (0.202) |
| Observations | 1111 | | 1049 | | 1116 | |
| Chlorine × No Spillover Mean | 0.711 | | 6.081 | | 1.543 | |
| **P-values:** | | | | | | |
| Incentives × No Spillover = Incentives × Spillover | 0.987 | | 0.986 | | 0.632 | |
| Info-Tool × No Spillover = Info-Tool × Spillover | 0.530 | | 0.215 | | 0.015 | |

Standard errors in parentheses
Each observation is at the household level. 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. 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.
* $p < .1$, ** $p < 0.05$, *** $p < 0.01$

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 3 and Figure 7). 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 = 0.049). In the medium- and 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 C.5.

Actual health will be correlated with treatment adoption. To understand how treatment responds 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 take costly action to acquire information about their health. 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 im-

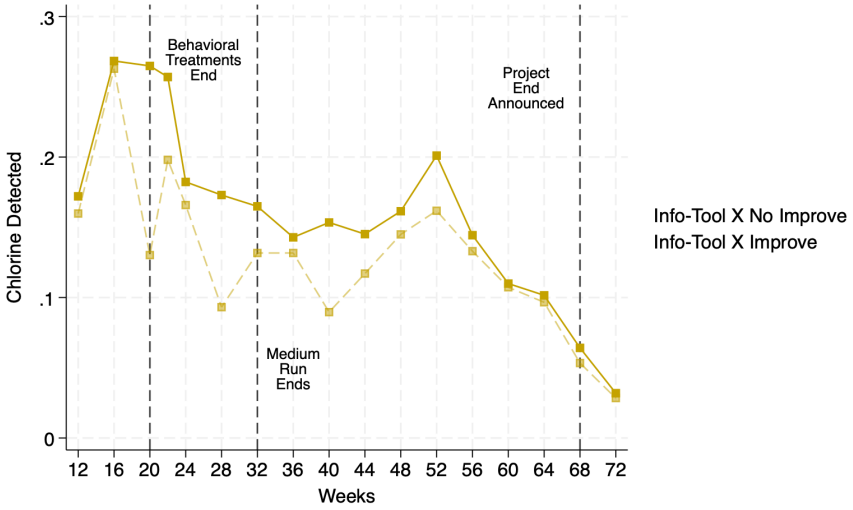



Figure 7: Info-Tool Chlorine Detection by Predicted Health Improvement

provement (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).[29]

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

$$\begin{aligned} Y_i =& \theta_0 + \theta_1 T1_i + \theta_2 T2_i + \theta_3 T3_i + \theta_4 \hat{I}_i \\ &+ \theta_5 \hat{I}_i \times T1_i + \theta_6 \hat{I}_i \times T2_i + \theta_7 \hat{I}_i \times T3_i \\ &+ Y_{i0} + X_{i0} + \gamma_{1,b} + \epsilon_i \end{aligned}$$

where $\hat{I}_i = 1$ if an individual is ex-ante predicted to improve (continuous predicted health improvement is above the median). Then $\theta_6$ is our object of interest. We use $\theta_4$, $\theta_5$, and $\theta_7$ 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 costly action.

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

[29]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 3: Chlorine Detection (Household-Survey Panel)
by predicted health improvement
Omitted group: Chlorine × Not Improved

| | (1) Short-Run | | (2) Medium-Run | | (3) Long-Run | |
|---|---|---|---|---|---|---|
| Chlorine × Improved | 0.033 | (0.029) | 0.035* | (0.021) | 0.013 | (0.016) |
| Incentives × Not Improved | 0.045 | (0.028) | 0.002 | (0.018) | 0.021 | (0.015) |
| Incentives × Improved | 0.058** | (0.029) | 0.019 | (0.019) | 0.027* | (0.016) |
| Info-Tool × Not Improved | -0.019 | (0.026) | 0.020 | (0.019) | 0.013 | (0.015) |
| Info-Tool × Improved | 0.034 | (0.028) | 0.052** | (0.022) | 0.030** | (0.015) |
| Observations | 3463 | | 4689 | | 10472 | |
| **P-values:** | | | | | | |
| Incentives: Not Improved = Improved | 0.659 | | 0.365 | | 0.717 | |
| Info-Tool: Not Improved = Improved | 0.049 | | 0.134 | | 0.277 | |

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

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 3). However, we are not able to rule out these differences at traditional thresholds of statistical significance (p-value = 0.134). [30]

Spillovers

If Info-Tool participants are able to act on information from their Info-Tool neighbors to a 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 4 with estimates in Table 1, estimates are *at least as big* in magnitude to the spillover treatment effect with the broader definition of spillovers).[31]

[30] 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.

[31] In the long-run, a *reverse* pattern emerges in the Incentives group, where people without access to a predicted-improved treated neighbor use chlorine at a higher rate. While we do not have a prediction for this result, these participants were more likely to report at endline that they had shared their tablets with others. Incentives participants in the spillover sample are in an environment where many others have a higher demand

Table 4: Chlorine Detection (Household-Survey Panel)
where "Spillover" is exposure to any Info-Tool neighbor predicted to improve
Omitted group: Chlorine × No Spillover

| | (1) Short-Run | | (2) Medium-Run | | (3) Long-Run | |
|---|---|---|---|---|---|---|
| Chlorine × Spillover | -0.037 | (0.033) | 0.010 | (0.024) | 0.022 | (0.017) |
| Incentives × No Spillover | 0.039* | (0.023) | -0.006 | (0.015) | 0.030** | (0.013) |
| Incentives × Spillover | -0.012 | (0.032) | -0.003 | (0.023) | -0.000 | (0.017) |
| Info-Tool × No Spillover | -0.020 | (0.022) | 0.011 | (0.016) | 0.012 | (0.012) |
| Info-Tool × Spillover | -0.011 | (0.031) | 0.051* | (0.027) | 0.047** | (0.021) |
| Observations | 3463 | | 4689 | | 10472 | |
| **P-values:** | | | | | | |
| Incentives: No Spillover = Spillover | 0.114 | | 0.883 | | 0.074 | |
| Info-Tool: No Spillover = Spillover | 0.781 | | 0.134 | | 0.092 | |

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

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 C.5).

### 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 the probability that participants believe that chlorine is effective; (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 (i.e., an ownership effect). We find evidence contrary to mechanisms (1) and (2), and supportive evidence of mechanism (3).

for chlorine than they do. We do not have any evidence as to why the Incentives group is more responsive to this higher aggregate demand than the Chlorine Only group, but we speculate that it could be due to two possibilities. Firstly, Incentives participants are used to receiving income for their chlorine tablets, so they might have sold their tablets. Only one person reports that they sold chlorine tablets at endline (the rest say they shared them for free), but it could be that participants do not wish to admit they sold tablets or forgot. Secondly, Incentives participants may wish to share chlorine tablets out of equity concerns. Although the participants not in the Incentives group did receive gifts via a lottery, it could be that a feeling of inequity arises between the Incentives group and other households because they were chosen for a group where receipt of gifts was within the caregivers' control and explicitly linked to the treatment assignment.

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 5).[32] 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. Thus, our evidence suggests that Info-Tool influences ownership-weighted beliefs, rather than beliefs alone.

Our model predicts that ownership-weighted beliefs should update more among Info-Tool participants who receive signals about the efficacy of chlorine from other Info-Tool participants, but that participants in other groups should not respond to exposure to Info-Tool neighbors. Indeed, we see that 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 2). 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.

Intimate Knowledge Why does the Info-Tool motivate people to act upon information that, on the surface, appears widely known? To understand if intimate knowledge of the signal-acquisition process is the key mechanism generating an ownership effect over learning-arm neighbors' signals, we elicit stated beliefs about a closely related concept: 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.[33] We remind all participants about the three treatment groups in our study:

[32] 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").

[33] We piloted questions that directly measured participant's 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 intimate knowledge about the Info-Tool data-generating process.

Table 5: Endline Stated Motivations
Omitted group: Chlorine Only

| | (1) | | (2) | | (3) | | (4) | |
|---|---|---|---|---|---|---|---|---|
| | Motivation: Health | | Motivation: Habit | | Motivation: Money | | 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 | |
| **P-values:** | | | | | | | | |
| 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.
* $p < .1$, ** $p < 0.05$, *** $p < 0.01$

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 rates. We then as them to imagine a neighbor from each group and indicate which one they believes 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 6).[34]

Our model suggests that experience with the Info-Tool alone, regardless of exposure to other Info-Tool participants, should generate more intimate knowledge about 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' intimate knowledge about the Info-Tool as an information source (Table C.6). Then, Info-Tool participants are uniquely primed to take ownership over signals from other Info-Tool participants. Behavior changes occur only when an individual gains access to these signals—by being randomly assigned an Info-Tool neighbor—providing them with information they intimately understand and thus take ownership of.

As a final check to ensure that the learning complementarity arises from an ownership effect—driven by 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 level of independent engagement with the tool. Specif-

[34]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.

Table 6: Endline Stated Trust
Omitted group: Pure Control

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

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

ically, 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 C.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.[35] 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

[35]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.

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.

We observe that Incentives arm households use chlorine at a higher rate than either of the Chlorine or Into-Tool arms during the behavioral treatment period. 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 born out in the long term in the Incentives arm. In the three months immediately after the behavioral interventions ended, the Incentives arm reverts to the same rate of use as the Chlorine Only group, while the Info-Tool group chlorinates 18% more often in the medium-run, the quarter after behavioral interventions ended, than the Incentives group ($p = 0.055$; we cannot, however, rule out equality between the Info-Tool group and the Chlorine Only group during this time period with $p = 0.142$). Thus, 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.[36] For a more detailed discussion on the roles of learning and habit formation, see Section I.2.

### 5.2.2 More Interactions and Mimickry

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

[36]Average rates of chlorine use in the Incentives and Info-Tool groups converge at 32 weeks. Notably, the Info-Tool participants in the Spillover sample continue to chlorinate at a higher rate. This could be due to forgetting in the Info-Tool arm without an Info-Tool neighbor, or it could be due to some catching-up in the Incentives group (cyclical habit formation, learning over a different time horizon in the absence of salient signals, etc.). We do not attempt to disentangle potential reasons why Incentives and Info-Tool eventually converge on average because, ultimately, the higher rate of use that Info-Tool experienced, though temporary, resulted in greater child health improvements. Furthermore, the Info-Tool group in the Spillover sample continued to chlorinate at a higher rate after the two groups converged on average.

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 C.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 C.11).

### 5.2.3 More Conversations

We ask participants if they 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 ar the most likely to discuss water purification with others (Table C.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 (Table 7). There are also no

Table 7: Midline Memory/Beliefs about Chlorine Efficacy

| | (1) Correct Answer | (2) Went Down | (3) Didn't Change | (4) Went Up | (5) Don't Know |
|---|---|---|---|---|---|
| Chlorine Only | -0.006 | 0.069** | -0.039** | -0.005 | -0.023** |
| | (0.035) | (0.031) | (0.019) | (0.027) | (0.009) |
| Incentives | 0.034 | 0.064** | -0.036* | 0.001 | -0.027*** |
| | (0.035) | (0.031) | (0.019) | (0.027) | (0.009) |
| Info-Tool | 0.043 | 0.073** | -0.016 | -0.030 | -0.026*** |
| | (0.035) | (0.031) | (0.019) | (0.027) | (0.009) |
| Observations | 1494 | 1494 | 1494 | 1494 | 1494 |
| Control Mean | 0.503 | 0.651 | 0.108 | 0.206 | 0.031 |
| **P-values:** | | | | | |
| Incentives = Chlorine Only | 0.251 | 0.875 | 0.883 | 0.833 | 0.697 |
| Info-Tool = Chlorine Only | 0.155 | 0.891 | 0.218 | 0.367 | 0.743 |
| Info-Tool = Incentives | 0.781 | 0.768 | 0.277 | 0.264 | 0.952 |

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

differences in which answer the participants gives across the three groups that received chlorine, indicating non-differential knowledge or optimism about chlorine efficacy.[37]

To ascertain that answering the question correctly is sensitive to comprehension skills, we test if 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 8).[38] 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, 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.[39]

*No Evidence of Differential Information Sharing*

[37] 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.

[38] The education treatment effect was non-differential across treated arms among participants who received any chlorine treatment.

[39] 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 prior to 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.

Table 8: 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** | 0.017 | 0.008 | -0.050 | 0.033** |
| | (0.053) | (0.048) | (0.029) | (0.042) | (0.014) |
| Any Treatment Group | -0.047 | 0.069** | -0.024 | -0.032 | -0.010 |
| | (0.036) | (0.033) | (0.020) | (0.029) | (0.009) |
| Any Treatment Group × Any Education | 0.187*** | 0.019 | -0.033 | 0.049 | -0.041*** |
| | (0.061) | (0.055) | (0.033) | (0.048) | (0.016) |
| Observations | 1427 | 1427 | 1427 | 1427 | 1427 |
| Control Mean (No Education) | 0.551 | 0.650 | 0.115 | 0.214 | 0.019 |
| **P-values:** | | | | | |
| 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.
* $p < .1$, ** $p < 0.05$, *** $p < 0.01$

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.

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 9). 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

Table 9: Endline Beliefs About Friends' Chlorine Use

| | (1) Believes Uses Chlorine: Full Sample | | (2) 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 | |
| **P-values:** | | | | |
| 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.
* $p < .1$, ** $p < 0.05$, *** $p < 0.01$

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 C.12).[40] Furthermore, Info-Tool participants do not do a better job 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 C.13).

This evidence supports our model, where treated participants are all equally aware of the benefits of chlorine tablet water purification, and share this information non-differentially in frequency or substance. Psychological ownership over signals acquired with the Info-Tool makes the belief in chlorine's efficacy actionable.

How come the Info-Tol 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

[40]The same holds true when we use other measures of "true treatment method" using our objective measures of water chlorine detection.

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 C.14). 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 C.14).

# 6 Child Health

## 6.1 Diarrhea

We find substantial impacts of water chlorination on child health. 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 C.2). These relative reductions mirror relative rates of chlorine use during this time period. Reductions in diarrhea rates continue to mirror relative levels of chlorine use in the medium- and long-run, with the Incentives groups experiencing the smallest reductions in diarrhea prevalence (11% reduction in the medium-run and 18% reduction in the long-run, neither statistically distinguishable from the Pure Control group); and the Info-Tool group experiencing the largest reductions in diarrhea prevalence (38% reduction in the medium-run, and 33% reduction in the long-run, both statistically distinguishable from the Pure Control group at conventional levels of significance). More details on measurement and results on child diarrhea are reported in Section K.

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. As an objective and therefore our preferred measure of health, we measure child anthropometrics at endline in an index that combines four standardized measures: weight-for-age (WAZ), weight-for-height (WHZ), height-for-age (HAZ), and MUAC-for-age (mid-upper arm circumference-for-age) z-scores.

Table 10: 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** | 0.016 | -0.005 | 0.112* | 0.055 |
| | (0.034) | (0.066) | (0.081) | (0.065) | (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. Indeces are constructed using following Anderson (2008).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

## 6.2 Child Anthropometrics

### 6.2.1 ITT Results: Any Treatment

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.

Table 10 reports the ITT effects of *any* treatment on the various measures of child anthropometry, whereas Table 12 reports the treatment-specific estimates. Following Anderson (2008), we create a summary index combining height-for-age, weight-for-height, weight-for-age, and MUAC-for-age z-scores. For households in any of the three treatment arms, the index increases by over 7% of a standard deviation ($p < 0.05$).[41]

### 6.2.2 IV Results: Effects of Water Purification

Treatment compliance is not perfect – we only are able to detect chlorine in the water of our treated participants 13% of the time across the whole study, yet 37% of treatment participants say 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 reports that they boil, bleach, or chlorinate their water. While this is a self-reported measure of compliance, we think this is a better measure of purified water for two reasons: (1) our objective measure of chlorine residual detection is an underestimate and changes with time,

[41] 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 significant).

Table 11: 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** | 0.050 | -0.017 | 0.371* | 0.198 |
| | (0.112) | (0.207) | (0.260) | (0.212) | (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).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

and (2) 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. Indeed, we find that Control participants are more likely to boil their water at endline. If we consider treatment assignment to be an instrument for using chlorine to purify water, then the exclusion restriction does not hold because the treatment also affects participant's use of other water-treatment methods. 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 can be considered always-takers.

We consider the IV impacts of water chlorination on child health using three 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. While we use a binary measure to indicate households who use an effective water purification technology, we know that households in the Info-Tool treatment group use chlorine somewhat more intensively over the course of the preceding year than any treatment household; and that Info-Tool households with an Info-Tool neighbor use chlorine the most intensively over the preceding year. Then, self-reported chlorine users in the Info-Tool group (especially in the spillover sub-sample) should have a larger stock of chlorine use than self-reported chlorine users in other groups, and we can expect that the more limited complier samples should have larger IV treatment effects (since the binary measure of chlorine use represents a larger stock of chlorine use in these samples, and therefore 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 affects 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. While we cannot rule out these threats to the exclusion restriction, and therefore do not claim that the IV effects represent the causal effects of water purification itself, the IV analysis serves an important purpose: It allows us to estimate treatment effects among different complier subgroups. By comparing the IV estimate among compliers in any treatment group with the IV estimate among compliers in the learning arm with learning neighbors, we can understand the potential gains to child health improvements, *even among compliers*, of treating everybody with the learning intervention.

Table 11 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. Forty-eight percent of the sample report using one of these effective water purification technologies at endline. We find that treatment compliance leads to a 0.24 SD increase in the index of child anthropometrics ($p < 0.05$).

## 6.3 Heterogeneity by Treatment

### 6.3.1 ITT Results

Table 12 shows that the improvements in anthropometric outcomes are consistently higher for households in the Info-Tool group. Furthermore, the treatment effects are more than twice the magnitude among Info-Tool households who have another Info-Tool neighbor, relative to Info-Tool households without access to this spillover (Table 13). Comparing only the spillover-sample Info-Tool participants with the rest of the sample, there is a 0.12 SD increase in the index of child anthropometry ($p < 0.05$).

### 6.3.2 IV Results

The IV results may also be heterogeneous with respect to the specific treatment group and access to an Info-Tool neighbor. Our instrumented measure (self-reported use of boiling, bleach, or chlorine for water purification) is self-reported and is an extensive margin measure of using an effective water purification method, which does not imply perfect compliance.[42] 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. However, we cannot use a cumulative measure, such as the number of visits in which we detect chlorine in participants' water, because these measures will not be excludable if the treatments changed water purification methods aside from chlorinating, for which we do not have cumulative measures.

[42] Among the people who reported that they chlorinate their water at endline, we only detected chlorine in the water of 3.8%.

Table 12: 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** | 0.020 | 0.007 | 0.080 | 0.025 |
| | (0.039) | (0.084) | (0.099) | (0.081) | (0.064) |
| Incentives | 0.030 | 0.022 | -0.098 | 0.065 | 0.061 |
| | (0.042) | (0.078) | (0.101) | (0.079) | (0.066) |
| Info-Tool | 0.108*** | 0.006 | 0.072 | 0.187** | 0.080 |
| | (0.039) | (0.080) | (0.101) | (0.080) | (0.066) |
| Observations | 2616 | 2371 | 2439 | 2492 | 1954 |
| Endline Control Mean | -0.019 | -1.773 | -0.291 | -1.407 | -1.453 |
| **P-values:** | | | | | |
| 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 13: Child Index of Anthropometry (ITT): where "Spillover" is exposure to any Info-Tool neighbor

| | (1) Ommitted Group: Pure Control | | (2) Ommitted Group: Everyone Else | |
|---|---|---|---|---|
| Chlorine × No Spillover | 0.105** | (0.045) | | |
| Chlorine × Spillover | 0.068 | (0.056) | | |
| Incentives × No Spillover | 0.039 | (0.045) | | |
| Incentives × Spillover | 0.018 | (0.055) | | |
| Info-Tool × No Spillover | 0.086** | (0.044) | | |
| Info-Tool × Spillover | 0.160*** | (0.058) | 0.118* | (0.061) |
| Observations | 2616 | | 2616 | |
| **P-values:** | | | | |
| Chlorine: No Spillover = Spillover | 0.535 | | | |
| Incentives: No Spillover = Spillover | 0.733 | | | |
| Info-Tool: No Spillover = Spillover | 0.233 | | | |

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

Table 14: IV Impacts on Endline Child Health:
where "Spillover" is exposure to any Info-Tool neighbor

| | (1) Instrument: Any Treatment | (2) Instrument: Info-Tool | (3) Instrument: Info-Tool × Spillover |
|---|---|---|---|
| Boils, Bleaches, or Chlorinates Water | 0.240** | 0.329*** | 0.506*** |
| | (0.113) | (0.126) | (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.
* $p < .1$, ** $p < 0.05$, *** $p < 0.01$

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. Therefore, 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, and that the effect of being someone who reports purifying her water at endline has a different effect on child health. To understand if there is heterogeneity in the causal effect of water purification on child health, we test three sets of compliers by varying our instrument.

Column (1) of Table 14 replicates column (1) of Table 11: assignment to any treatment group is the instrument for using an effective water purification method, and 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 14 column (2), the instrument is an indicator for being in the 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 14 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).[43]

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 15). We do not see any results on height-for-age, unlike the handwashing, hygiene, and nutrient supplement programs.

Table 15: Benchmarking Child Health Estimates

| **Intervention** | **Paper** | **HAZ** | **WHZ** | **WAZ** | **Muac-for-age** |
|---|---|---|---|---|---|
| Chlorine: Average | Table 11 | 0.050 | -0.017 | 0.371* | 0.198 |
| Chlorine: Saturated Info-Tool | Table D.7 | -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 | – |

## 6.4 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 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 use chlorine in the past 24 hours in order for enumerators to detect chlorine presence in each visit.[44] 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 (Fuller and Eisenberg, 2016; Duflo et al., 2015). 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.

# 7 Conclusion

We study the process of learning about health by leveraging a learning intervention whose effectiveness in changing behavior hinges on the interplay 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

[43]Table D.7 reports the corresponding estimates for the four components of the index.

[44]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.

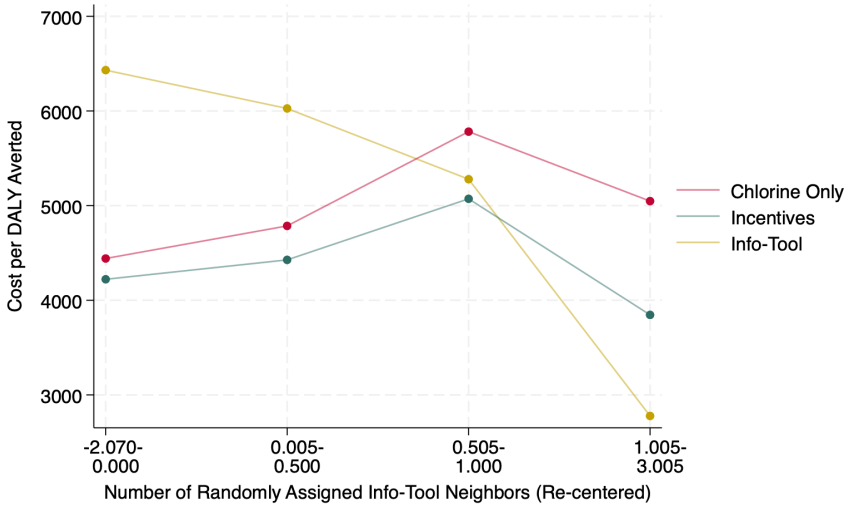



Figure 8: Cost-Effectiveness Analysis

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 8).[45] 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). 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

[45] 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 cost 24.78 USD. Each Info-Tool household cost an additional 7.84 USD (paper materials and CHW time), and each Incentives household cost on average an additional 0.88 USD (short-term incentives provision).

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. While "trust" and "ownership effects" are distinct phenomena, they are tightly linked. Indeed, we find that a part of the psychological mechanism underlying an Info-Tool participant's ability to take ownership of signals from their Info-Tool neighbors 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).[46] This adds to a body of research showing that, 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 demonstrated demand for chlorine in other ways. Seven percent of participants demonstrated 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% reported that they currently used chlorine to purify their water,[47] and 77% accepted 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

[46] 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.

[47] The correlation between self-reported chlorine use at endline and the probability that we ever detected chlorine residual in the prior three months was 0.22.

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 face complementarities between experiential and social learning? Our model suggests 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 learning may need reinforcement through other sources with which one has intimate knowledge. 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, childcare practices[48], and learning or study habits.[49] 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.

[48] 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).

[49] 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.

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# Online Supplementary Material

## Appendix A Baseline Balance

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

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

*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 A.3: Treatment Balance: Spillover

| | (1) Spillover Sample | | (2) 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 B 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 B.1). Also similar to our study, the spillover and non-spillover samples converge towards the end of the trial.

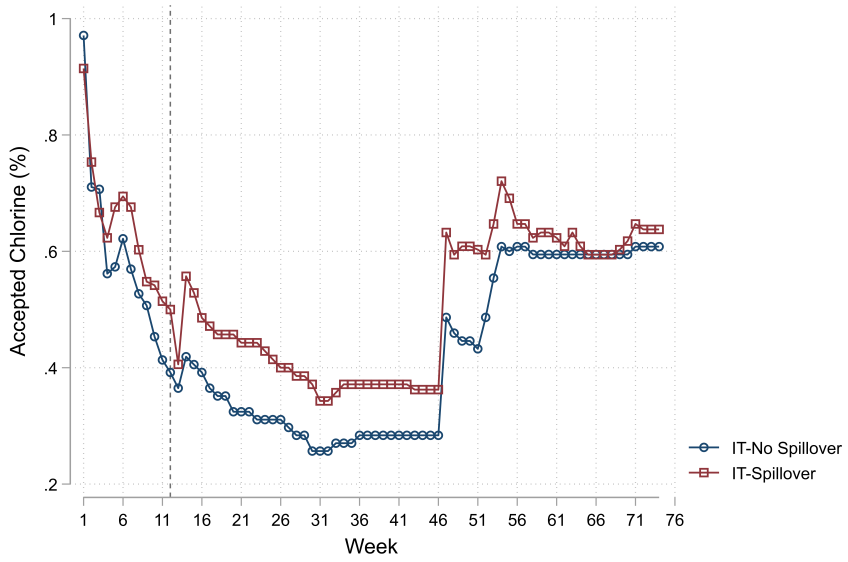



Figure B.1: Akram and Mendelsohn (2021) Reanalysis
Rates of Chlorine Acceptance
where "Spillover" is exposure to any Info-Tool neighbor

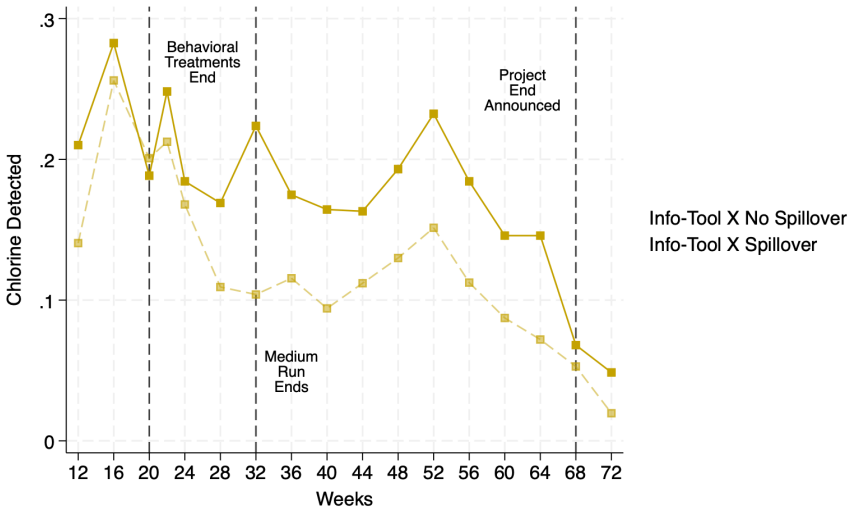



Figure B.2: Our Study
Rates of Chlorine Detection
where "Spillover" is exposure to any Info-Tool neighbor

# Appendix C Additional Figures and Tables

Table C.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 | |
| **P-values:** | | | | | | |
| 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).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: 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 | |
| **P-values:** | | | | | | |
| 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).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

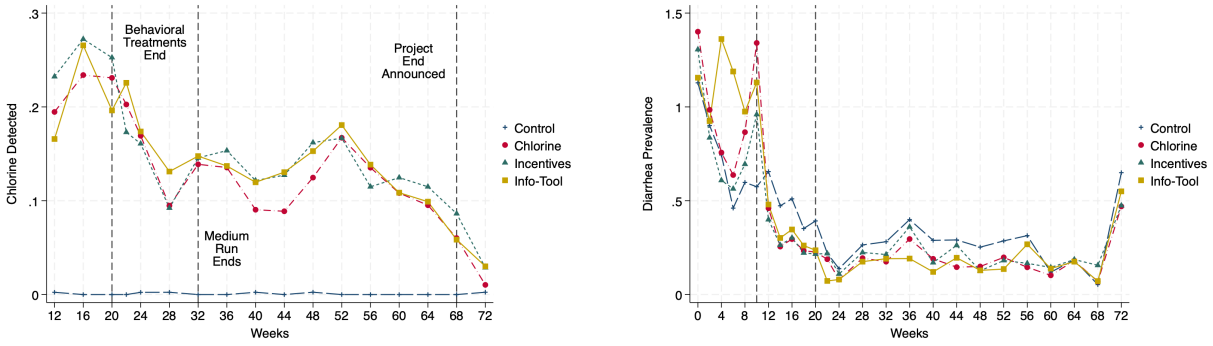



Figure C.1: Raw Average: Chlorine Detection and Diarrhea Prevalence

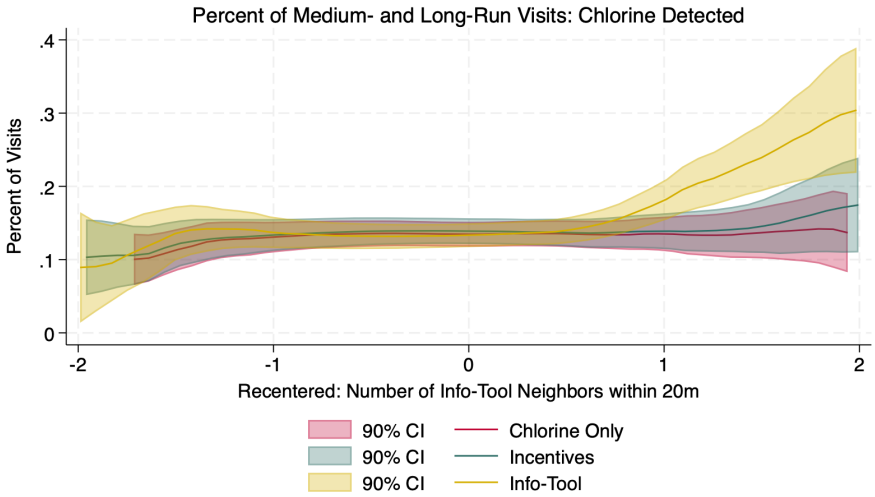



Figure C.2: Raw Rates of Chlorine Detection by Spillover Exposure (Continuous)

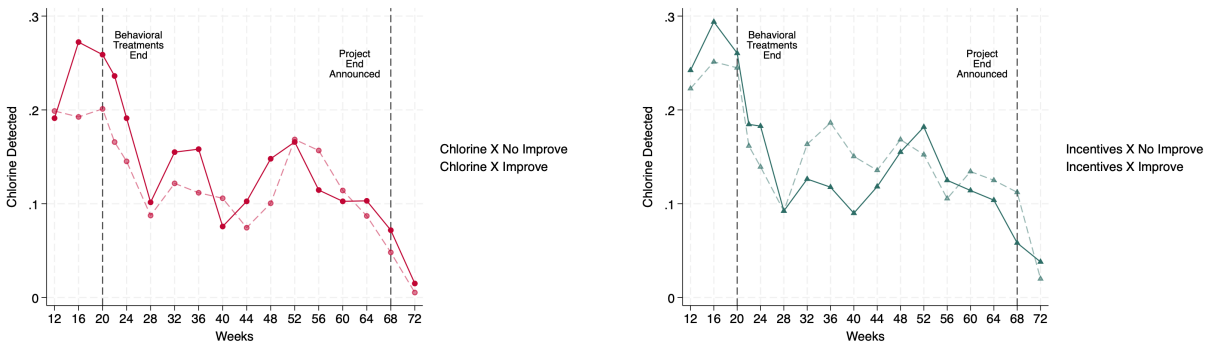



Figure C.3: Heterogeneity: Predicted Health Improvement

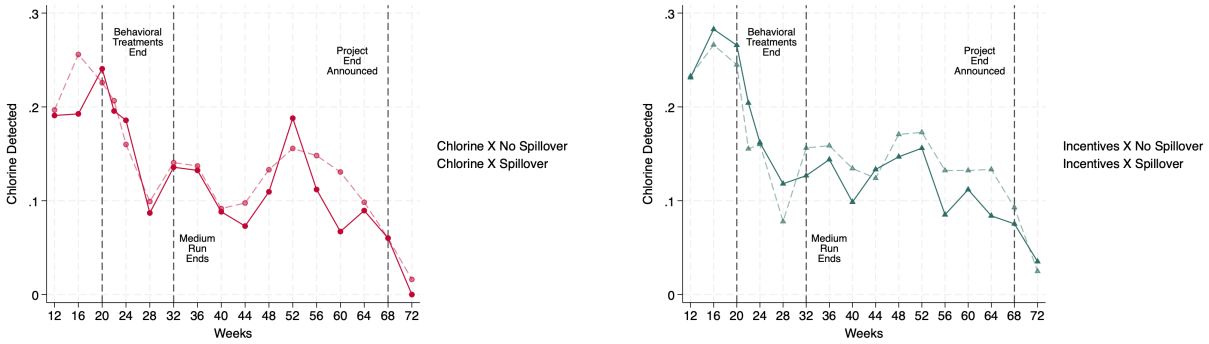



Figure C.4: Heterogeneity: Info-Tool Neighbors (Re-centered)

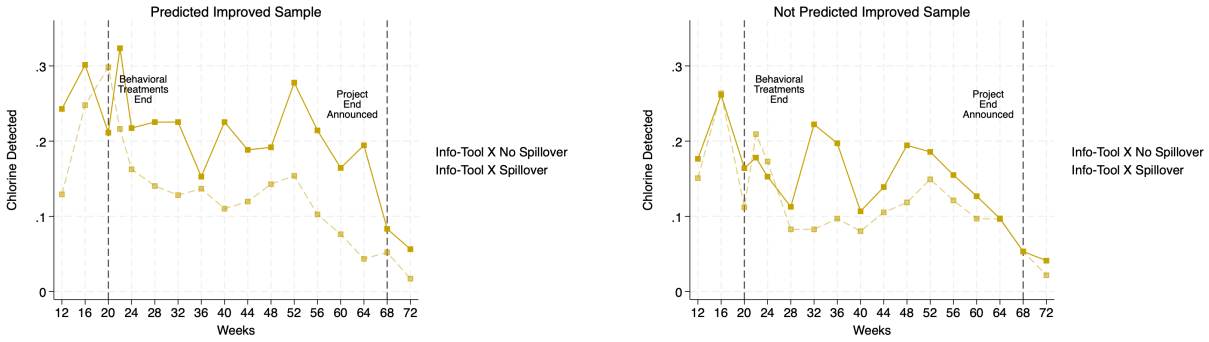



Figure C.5: Heterogeneity: Info-Tool Neighbors (Re-centered) × Predicted Health Improvement

Table C.3: Placebo Test – Chlorine Detection (Household Survey Panel)
where "Spillover" is exposure to any Chlorine Only neighbor
Omitted group: Chlorine × No Spillover

| | (1) Short-Run | | (2) Medium-Run | | (3) Long-Run | |
|---|---|---|---|---|---|---|
| Chlorine × No Spillover | 0.208*** | (0.018) | 0.133*** | (0.013) | 0.108*** | (0.011) |
| Chlorine × Spillover | 0.237*** | (0.026) | 0.169*** | (0.017) | 0.112*** | (0.017) |
| Incentives × No Spillover | 0.250*** | (0.019) | 0.136*** | (0.012) | 0.131*** | (0.013) |
| Incentives × Spillover | 0.255*** | (0.025) | 0.150*** | (0.016) | 0.120*** | (0.015) |
| Info-Tool × No Spillover | 0.212*** | (0.017) | 0.166*** | (0.014) | 0.127*** | (0.012) |
| Info-Tool × 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.
* $p < .1$, ** $p < 0.05$, *** $p < 0.01$

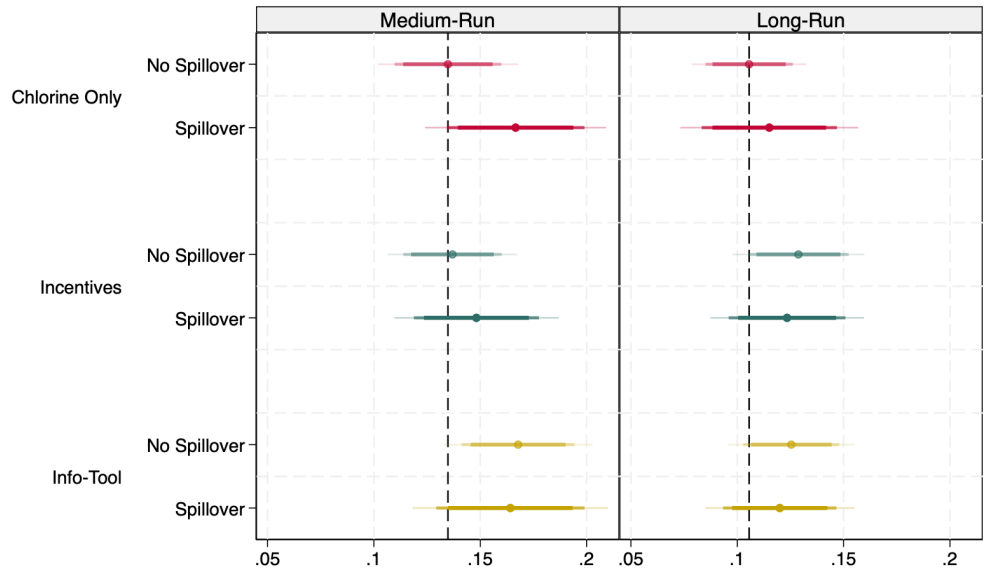



Figure C.6: Placebo Test – Chlorine Detection (Household Survey Panel) where "Spillover" is exposure to any Chlorine Only neighbor

Table C.4: Placebo Test – Chlorine Detection (Panel Specification) where "Spillover" is exposure to any Incentives neighbor Omitted group: Chlorine × No Spillover

| | (1) Short-Run | | (2) Medium-Run | | (3) Long-Run | |
|---|---|---|---|---|---|---|
| Chlorine × No Spillover | 0.212*** | (0.018) | 0.141*** | (0.013) | 0.101*** | (0.011) |
| Chlorine × Spillover | 0.231*** | (0.026) | 0.154*** | (0.017) | 0.123*** | (0.018) |
| Incentives × No Spillover | 0.264*** | (0.019) | 0.148*** | (0.012) | 0.135*** | (0.013) |
| Incentives × Spillover | 0.231*** | (0.024) | 0.130*** | (0.015) | 0.112*** | (0.015) |
| Info-Tool × No Spillover | 0.194*** | (0.016) | 0.163*** | (0.014) | 0.123*** | (0.012) |
| Info-Tool × 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.
* $p < .1$, ** $p < 0.05$, *** $p < 0.01$

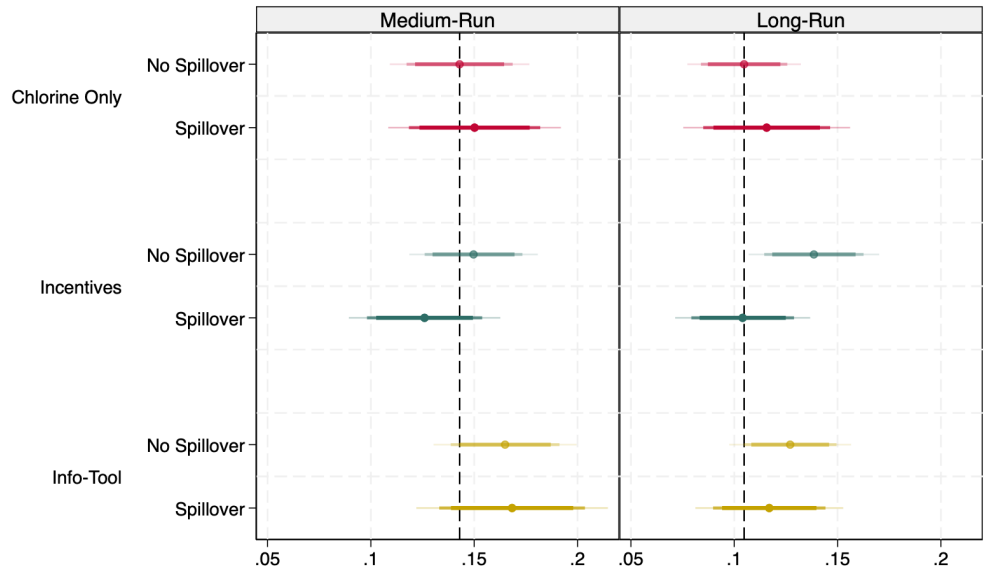



Figure C.7: Placebo Test – Chlorine Detection (Household Survey Panel) where "Spillover" is exposure to any Incentives neighbor

Table C.5: Placebo Test – Chlorine Detection (Household-Survey Panel) where "Spillover" is exposure to any treated neighbor predicted to improve Omitted group: Chlorine × No Spillover

| | (1) Short-Run | | (2) Medium-Run | | (3) Long-Run | |
|---|---|---|---|---|---|---|
| Chlorine × Spillover | 0.004 | (0.030) | 0.008 | (0.021) | 0.012 | (0.016) |
| Incentives × No Spillover | 0.058** | (0.027) | -0.007 | (0.018) | 0.030** | (0.015) |
| Incentives × Spillover | 0.008 | (0.028) | -0.002 | (0.020) | 0.012 | (0.017) |
| Info-Tool × No Spillover | -0.022 | (0.024) | 0.013 | (0.019) | 0.018 | (0.014) |
| Info-Tool × 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 C.6: Endline Stated Trust
Omitted group: Pure Control

| | (1) Info-Tool Knows Most | (2) Incentives Knows Most | (3) Chlorine Knows Most |
|---|---|---|---|
| Chlorine × No Spillover | -0.023 | -0.105*** | 0.128*** |
| | (0.034) | (0.038) | (0.042) |
| Chlorine × Spillover | -0.079* | -0.030 | 0.109* |
| | (0.046) | (0.053) | (0.058) |
| Incentives × No Spillover | 0.001 | 0.069* | -0.070* |
| | (0.034) | (0.038) | (0.042) |
| Incentives × Spillover | -0.013 | 0.125** | -0.112* |
| | (0.046) | (0.052) | (0.058) |
| Info-Tool × No Spillover | 0.256*** | -0.115*** | -0.141*** |
| | (0.034) | (0.039) | (0.043) |
| Info-Tool × Spillover | 0.159*** | -0.025 | -0.134** |
| | (0.046) | (0.052) | (0.058) |
| Observations | 1516 | 1516 | 1516 |
| **P-values**: | | | |
| Outcome Group: Spillover = No Spillover | 0.091 | 0.394 | 0.792 |
| **DID Estimate:** | | | |
| 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 C.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 × No Spillover

| | (1) Short-Run | | (2) Medium-Run | | (3) Long-Run | |
|---|---|---|---|---|---|---|
| Chlorine × Spillover | -0.000 | (0.031) | 0.004 | (0.021) | 0.028* | (0.016) |
| Incentives × No Spillover | 0.046* | (0.025) | -0.009 | (0.016) | 0.034** | (0.014) |
| Incentives × Spillover | 0.011 | (0.031) | -0.001 | (0.022) | 0.013 | (0.017) |
| Info-Tool × No Spillover | -0.003 | (0.023) | 0.006 | (0.018) | 0.015 | (0.013) |
| Info-Tool × 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 | | 0.074 | |
| Incentives: No Spillover = Spillover | 0.257 | | 0.705 | | 0.211 | |
| Info-Tool: No Spillover = Spillover | 0.480 | | 0.129 | | 0.130 | |

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 themself 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 C.8: Endline Social Networks

| | (1) Number in Social Network | (2) Number Discuss Health | (3) Number Discussed Water Purification |
|---|---|---|---|
| Incentives | -0.051 | -0.022 | 0.114** |
| | (0.076) | (0.054) | (0.045) |
| Info-Tool | -0.119 | -0.004 | 0.005 |
| | (0.076) | (0.054) | (0.045) |
| Observations | 1116 | 1116 | 1116 |
| **P-values:** | | | |
| Incentives = Info-Tool | 0.377 | 0.730 | 0.015 |

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 C.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 | -0.065 | 0.133** |
| | (0.115) | (0.081) | (0.067) |
| Incentives × No Spillover | -0.019 | -0.050 | 0.148*** |
| | (0.096) | (0.068) | (0.056) |
| Incentives × Spillover | 0.046 | -0.037 | 0.186*** |
| | (0.114) | (0.080) | (0.067) |
| Info-Tool × No Spillover | -0.048 | 0.004 | 0.037 |
| | (0.097) | (0.068) | (0.057) |
| Info-Tool × Spillover | -0.085 | -0.081 | 0.083 |
| | (0.115) | (0.081) | (0.067) |
| Observations | 1116 | 1116 | 1116 |
| **P-values:** | | | |
| 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 C.10: 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 | 0.004 | 0.011 | -0.001 |
| | (0.031) | (0.023) | (0.010) | (0.057) |
| Incentives | 0.067** | 0.001 | 0.010 | 0.005 |
| | (0.031) | (0.022) | (0.010) | (0.057) |
| Info-Tool | 0.084*** | 0.004 | 0.015 | -0.114** |
| | (0.031) | (0.023) | (0.010) | (0.057) |
| Observations | 1512 | 1512 | 1512 | 1503 |
| **P-values:** | | | | |
| 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 C.11: 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 × No Spillover | 0.069* | -0.001 | 0.013 | 0.041 |
| | (0.038) | (0.026) | (0.011) | (0.065) |
| Chlorine × Spillover | -0.041 | 0.011 | 0.008 | -0.079 |
| | (0.053) | (0.032) | (0.014) | (0.080) |
| Incentives × No Spillover | 0.046 | 0.017 | 0.002 | 0.033 |
| | (0.038) | (0.025) | (0.011) | (0.065) |
| Incentives × Spillover | 0.102* | -0.028 | 0.025* | -0.045 |
| | (0.052) | (0.031) | (0.013) | (0.080) |
| Info-Tool × No Spillover | 0.102*** | 0.016 | 0.012 | -0.086 |
| | (0.038) | (0.026) | (0.011) | (0.065) |
| Info-Tool × Spillover | 0.050 | -0.018 | 0.021 | -0.166** |
| | (0.053) | (0.032) | (0.014) | (0.080) |
| Observations | 1512 | 1512 | 1512 | 1503 |
| **P-values:** | | | | |
| Chlorine × No Spillover = Chlorine × Spillover | 0.092 | 0.722 | 0.689 | 0.165 |
| Incentives × No Spillover = Incentives × Spillover | 0.388 | 0.182 | 0.104 | 0.373 |
| Info-Tool × No Spillover = Info-Tool × 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 C.12: Endline Accurate Beliefs About Friends' Water Purification Methods

| | (1) Correct Guess (User or Not: SR) | | (2) Correct Guess (User or Not: OB) | | (3) 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 × Info-Tool Friend | -0.005 | (0.038) | 0.044 | (0.057) | 0.015 | (0.020) |
| Observations | 2040 | | 2824 | | 2824 | |
| No Info-Tool × No Info-Tool Friend Mean | 0.953 | | 0.440 | | 0.035 | |
| **P-values:** | | | | | | |
| Info-Tool Friend + Info-Tool × 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 C.13: Months Since Water Purification Discussions

| | (1) First Water Discussion | | (2) 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 | |
| **P-values:** | | | | |
| 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 C.14: Endline Social Norms

| | (1) Believes Neighbor Would Accept Chlorinated Water | (2) Number of Friends Believes Chlorinates |
|---|---|---|
| Chlorine Only | 0.082*** | 0.041* |
| | (0.030) | (0.022) |
| Incentives | 0.139*** | 0.079*** |
| | (0.030) | (0.022) |
| Info-Tool | 0.098*** | 0.061*** |
| | (0.030) | (0.022) |
| Observations | 1503 | 1690 |
| **P-values:** | | |
| 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 C.15: 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** | 0.041* |
| | (0.037) | (0.025) |
| Chlorine × Spillover | 0.088* | 0.040 |
| | (0.051) | (0.030) |
| Incentives × No Spillover | 0.157*** | 0.072*** |
| | (0.037) | (0.025) |
| Incentives × Spillover | 0.109** | 0.092*** |
| | (0.050) | (0.030) |
| Info-Tool × No Spillover | 0.060 | 0.049** |
| | (0.037) | (0.025) |
| Info-Tool × Spillover | 0.164*** | 0.082*** |
| | (0.051) | (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 C.16: Endline Willingness-to-Pay

| | (1) Purchased Tablets | (2) Price Accepted | (3) Hypothetical WTP | (4) Sold or Revisited |
|---|---|---|---|---|
| Chlorine Only | -0.017* | -2.472* | 0.124 | -0.044** |
| | (0.010) | (1.444) | (1.472) | (0.019) |
| Incentives | -0.010 | -1.693 | 0.215 | -0.037* |
| | (0.010) | (1.448) | (1.482) | (0.019) |
| Info-Tool | 0.004 | 0.288 | 0.598 | -0.020 |
| | (0.010) | (1.444) | (1.494) | (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 C.17: Endline Willingness-to-Pay

| | (1) Purchased Tablets | (2) Price Accepted | (3) Hypothetical WTP | (4) Sold or Revisited |
|---|---|---|---|---|
| Chlorine × No Spillover | -0.019 | -2.603 | -2.103 | -0.048** |
| | (0.012) | (1.802) | (1.859) | (0.024) |
| Chlorine × Spillover | -0.013 | -2.233 | 3.970 | -0.036 |
| | (0.017) | (2.460) | (2.449) | (0.032) |
| Incentives × No Spillover | -0.007 | -1.115 | -0.408 | -0.064*** |
| | (0.013) | (1.808) | (1.867) | (0.024) |
| Incentives × Spillover | -0.014 | -2.681 | 1.200 | 0.013 |
| | (0.017) | (2.442) | (2.437) | (0.032) |
| Info-Tool × No Spillover | -0.002 | -0.101 | -0.838 | -0.025 |
| | (0.012) | (1.803) | (1.884) | (0.024) |
| Info-Tool × Spillover | 0.014 | 0.973 | 3.063 | -0.010 |
| | (0.017) | (2.453) | (2.462) | (0.032) |
| Observations | 1503 | 1503 | 1099 | 1503 |
| Control Mean | 0.027 | 4.059 | 5.217 | 0.106 |
| **P-values:** | | | | |
| 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 D Main Tables: Robustness to Alternate Specification

For robustness, we also estimate the following specification using an aggregated version of a given outcome:

$$Y_i = \beta_0 + \beta_1 T1_i + \beta_2 T2_i + \beta_3 T3_i + NV_i + X_{i0} + \delta_s + \gamma_b + \epsilon_i, \tag{2}$$

where $Y_i$ represents the aggregated outcome measurements over relevant visits (e.g., total number of visits with chlorine presence detected in the water), and $NV_i$ captures the number of visits with non-missing outcome measurements. All of our tables are replicated using this specification in the Appendix.

Table D.1: Chlorine Detection: Aggregate
where "Spillover" is exposure to any Info-Tool neighbor

| | (1) Short-Run | | (2) Medium-Run | | (3) Long-Run | |
|---|---|---|---|---|---|---|
| Chlorine × No Spillover | 0.641*** | (0.056) | 0.541*** | (0.051) | 0.884*** | (0.099) |
| Chlorine × Spillover | 0.640*** | (0.070) | 0.560*** | (0.062) | 0.955*** | (0.120) |
| Incentives × No Spillover | 0.743*** | (0.056) | 0.506*** | (0.051) | 1.091*** | (0.098) |
| Incentives × Spillover | 0.743*** | (0.068) | 0.528*** | (0.062) | 0.937*** | (0.119) |
| Info-Tool × No Spillover | 0.617*** | (0.057) | 0.543*** | (0.052) | 0.918*** | (0.099) |
| Info-Tool × Spillover | 0.631*** | (0.070) | 0.716*** | (0.063) | 1.200*** | (0.121) |
| Observations | 1599 | | 1690 | | 1690 | |
| **P-values:** | | | | | | |
| Chlorine: No Spillover = Spillover | 0.988 | | 0.779 | | 0.585 | |
| Incentives: No Spillover = Spillover | 0.998 | | 0.740 | | 0.234 | |
| Info-Tool: No Spillover = Spillover | 0.849 | | 0.012 | | 0.034 | |

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 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.2: Chlorine Detection: Aggregate
by predicted health improvement

| | (1) Short-Run | | (2) Medium-Run | | (3) Long-Run | |
|---|---|---|---|---|---|---|
| Chlorine × Improved | 0.088 | (0.082) | 0.150** | (0.073) | 0.152 | (0.132) |
| Incentives × Not Improved | 0.138* | (0.081) | 0.008 | (0.072) | 0.168 | (0.130) |
| Incentives × Improved | 0.168** | (0.081) | 0.079 | (0.074) | 0.231* | (0.132) |
| Info-Tool × Not Improved | -0.044 | (0.081) | 0.088 | (0.073) | 0.104 | (0.131) |
| Info-Tool × Improved | 0.101 | (0.083) | 0.180** | (0.074) | 0.269** | (0.134) |
| Observations | 1188 | | 1261 | | 1261 | |
| **P-values:** | | | | | | |
| Incentives: Not Improved = Improved | 0.707 | | 0.332 | | 0.630 | |
| Info-Tool: Not Improved = Improved | 0.081 | | 0.217 | | 0.222 | |

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). 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 D.3: Chlorine Detection: Aggregate
where "Spillover" is exposure to any Info-Tool neighbor predicted to improve

| | (1) Short-Run | | (2) Medium-Run | | (3) Long-Run | |
|---|---|---|---|---|---|---|
| Chlorine × Spillover | -0.091 | (0.097) | 0.033 | (0.085) | 0.196 | (0.153) |
| Incentives × No Spillover | 0.130** | (0.064) | -0.029 | (0.058) | 0.227** | (0.104) |
| Incentives × Spillover | -0.054 | (0.095) | -0.016 | (0.086) | -0.016 | (0.155) |
| Info-Tool × No Spillover | -0.042 | (0.065) | 0.023 | (0.059) | 0.074 | (0.106) |
| Info-Tool × Spillover | -0.028 | (0.097) | 0.191** | (0.086) | 0.403*** | (0.155) |
| Observations | 1188 | | 1261 | | 1261 | |
| **P-values:** | | | | | | |
| Incentives: No Spillover = Spillover | 0.054 | | 0.874 | | 0.118 | |
| Info-Tool: No Spillover = Spillover | 0.883 | | 0.055 | | 0.035 | |

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 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 D.4: Chlorine Detection: Aggregate
where "Spillover" is exposure to any treatment neighbor predicted to improve

| | (1) Short-Run | | (2) Medium-Run | | (3) 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 | |
| **P-values:** | | | | | | |
| 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 D.5: Chlorine Detection: Aggregate

| | (1) Short-Run | | (2) Medium-Run | | (3) 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 | |
| **P-values:** | | | | | | |
| 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 D.6: Diarrhea: Aggregate

| | (1) Short-Run | | (2) Medium-Run | | (3) 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 | |
| **P-values:** | | | | | | |
| 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 D.7: IV Impacts on Endline Child Health (Instrument: Info-Tool × 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*** | -0.083 | 0.584 | 0.789** | -0.428 |
| | (0.183) | (0.376) | (0.516) | (0.398) | (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).
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.8: 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 E Baseline Balance: Spillover and Endline Sample

Table E.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 | 0.332 | 0.309 | -0.036 |
| | (0.013) | (0.019) | (0.011) | |
| Reported Highest Diarrhea in Summer | 0.817 | 0.856 | 0.831 | -0.039 |
| | (0.011) | (0.014) | (0.009) | |
| Number of Children <5 | 1.491 | 1.514 | 1.499 | -0.023 |
| | (0.020) | (0.033) | (0.017) | |
| Has Heard of Chlorine | 0.212 | 0.188 | 0.204 | 0.024 |
| | (0.012) | (0.015) | (0.009) | |
| Would consider using chlorine | 0.872 | 0.890 | 0.878 | -0.019 |
| | (0.010) | (0.012) | (0.008) | |
| Enumerator Observes Dirt in Water | 0.164 | 0.162 | 0.163 | 0.002 |
| | (0.011) | (0.014) | (0.009) | |
| Reports Dirt in Water | 0.750 | 0.750 | 0.750 | -0.000 |
| | (0.013) | (0.017) | (0.010) | |
| Boils, Bleaches, or Chlorinates Water | 0.143 | 0.133 | 0.139 | 0.010 |
| | (0.010) | (0.013) | (0.008) | |
| Strains or Filters Water | 0.620 | 0.662 | 0.635 | -0.042 |
| | (0.014) | (0.019) | (0.011) | |
| Believes Chlorine is for Water Purification | 0.217 | 0.187 | 0.206 | 0.030 |
| | (0.012) | (0.015) | (0.010) | |
| Caretaker Asked about Chlorine Test | 0.143 | 0.164 | 0.150 | -0.021 |
| | (0.010) | (0.015) | (0.008) | |
| Attrited (endline) | 0.125 | 0.128 | 0.126 | -0.003 |
| | (0.010) | (0.013) | (0.008) | |
| 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 E.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 | 0.358 | 0.291 | -0.105** |
| | (0.026) | (0.037) | (0.021) | |
| Reported Highest Diarrhea in Summer | 0.793 | 0.867 | 0.820 | -0.074 |
| | (0.024) | (0.027) | (0.018) | |
| Number of Children <5 | 1.498 | 1.521 | 1.507 | -0.023 |
| | (0.038) | (0.076) | (0.037) | |
| Has Heard of Chlorine | 0.235 | 0.248 | 0.240 | -0.013 |
| | (0.025) | (0.034) | (0.020) | |
| Would consider using chlorine | 0.867 | 0.897 | 0.878 | -0.030 |
| | (0.020) | (0.024) | (0.015) | |
| Enumerator Observes Dirt in Water | 0.172 | 0.127 | 0.156 | 0.045 |
| | (0.022) | (0.026) | (0.017) | |
| Reports Dirt in Water | 0.754 | 0.745 | 0.751 | 0.009 |
| | (0.026) | (0.034) | (0.020) | |
| Boils, Bleaches, or Chlorinates Water | 0.126 | 0.182 | 0.147 | -0.056 |
| | (0.020) | (0.030) | (0.017) | |
| Strains or Filters Water | 0.618 | 0.661 | 0.633 | -0.043 |
| | (0.029) | (0.037) | (0.023) | |
| Believes Chlorine is for Water Purification | 0.239 | 0.182 | 0.218 | 0.057 |
| | (0.025) | (0.030) | (0.019) | |
| Caretaker Asked about Chlorine Test | 0.123 | 0.182 | 0.144 | -0.059 |
| | (0.019) | (0.030) | (0.017) | |
| Attrited (endline) | 0.105 | 0.127 | 0.113 | -0.022 |
| | (0.018) | (0.026) | (0.015) | |
| 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 E.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 | 0.285 | 0.304 | 0.030 |
| | (0.028) | (0.035) | (0.022) | |
| Reported Highest Diarrhea in Summer | 0.836 | 0.806 | 0.825 | 0.030 |
| | (0.022) | (0.031) | (0.018) | |
| Number of Children <5 | 1.486 | 1.467 | 1.479 | 0.019 |
| | (0.040) | (0.050) | (0.031) | |
| Has Heard of Chlorine | 0.220 | 0.145 | 0.193 | 0.075* |
| | (0.025) | (0.028) | (0.019) | |
| Would consider using chlorine | 0.895 | 0.873 | 0.887 | 0.022 |
| | (0.018) | (0.026) | (0.015) | |
| Enumerator Observes Dirt in Water | 0.185 | 0.152 | 0.173 | 0.034 |
| | (0.023) | (0.028) | (0.018) | |
| Reports Dirt in Water | 0.773 | 0.691 | 0.743 | 0.082 |
| | (0.025) | (0.036) | (0.021) | |
| Boils, Bleaches, or Chlorinates Water | 0.140 | 0.115 | 0.131 | 0.025 |
| | (0.021) | (0.025) | (0.016) | |
| Strains or Filters Water | 0.612 | 0.630 | 0.619 | -0.018 |
| | (0.029) | (0.038) | (0.023) | |
| Believes Chlorine is for Water Purification | 0.234 | 0.194 | 0.220 | 0.040 |
| | (0.025) | (0.031) | (0.020) | |
| Caretaker Asked about Chlorine Test | 0.133 | 0.152 | 0.140 | -0.019 |
| | (0.020) | (0.028) | (0.016) | |
| Attrited (endline) | 0.154 | 0.139 | 0.149 | 0.014 |
| | (0.021) | (0.027) | (0.017) | |
| 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 E.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 | 0.388 | 0.356 | -0.050 |
| | (0.028) | (0.038) | (0.023) | |
| Reported Highest Diarrhea in Summer | 0.835 | 0.861 | 0.844 | -0.026 |
| | (0.022) | (0.027) | (0.017) | |
| Number of Children <5 | 1.475 | 1.588 | 1.517 | -0.113 |
| | (0.042) | (0.071) | (0.038) | |
| Has Heard of Chlorine | 0.190 | 0.200 | 0.194 | -0.010 |
| | (0.023) | (0.031) | (0.019) | |
| Would consider using chlorine | 0.870 | 0.897 | 0.880 | -0.027 |
| | (0.020) | (0.024) | (0.015) | |
| Enumerator Observes Dirt in Water | 0.137 | 0.200 | 0.160 | -0.063 |
| | (0.020) | (0.031) | (0.017) | |
| Reports Dirt in Water | 0.739 | 0.776 | 0.753 | -0.036 |
| | (0.026) | (0.033) | (0.020) | |
| Boils, Bleaches, or Chlorinates Water | 0.144 | 0.109 | 0.131 | 0.035 |
| | (0.021) | (0.024) | (0.016) | |
| Strains or Filters Water | 0.641 | 0.685 | 0.657 | -0.044 |
| | (0.029) | (0.036) | (0.022) | |
| Believes Chlorine is for Water Purification | 0.180 | 0.230 | 0.198 | -0.051 |
| | (0.023) | (0.033) | (0.019) | |
| Caretaker Asked about Chlorine Test | 0.165 | 0.194 | 0.176 | -0.028 |
| | (0.022) | (0.031) | (0.018) | |
| Attrited (endline) | 0.123 | 0.158 | 0.136 | -0.034 |
| | (0.020) | (0.028) | (0.016) | |
| 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 E.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) | (1)-(3) | (1)-(4) |
|---|---|---|---|---|---|---|---|---|
| Any Child Had Motions | 0.285 | 0.345 | 0.292 | 0.278 | 0.300 | -0.061* | -0.007 | 0.006 |
| | (0.022) | (0.024) | (0.023) | (0.022) | (0.012) | | | |
| Reported Highest Diarrhea in Summer | 0.837 | 0.832 | 0.820 | 0.835 | 0.831 | 0.004 | 0.016 | 0.002 |
| | (0.018) | (0.019) | (0.020) | (0.019) | (0.009) | | | |
| Number of Children <5 | 1.493 | 1.497 | 1.479 | 1.511 | 1.495 | -0.005 | 0.013 | -0.019 |
| | (0.035) | (0.039) | (0.034) | (0.039) | (0.018) | | | |
| Has Heard of Chlorine | 0.200 | 0.204 | 0.201 | 0.238 | 0.211 | -0.003 | -0.000 | -0.038 |
| | (0.020) | (0.020) | (0.020) | (0.021) | (0.010) | | | |
| Would consider using chlorine | 0.866 | 0.879 | 0.880 | 0.872 | 0.874 | -0.013 | -0.014 | -0.006 |
| | (0.017) | (0.017) | (0.017) | (0.017) | (0.008) | | | |
| Enumerator Observes Dirt in Water | 0.146 | 0.155 | 0.169 | 0.158 | 0.157 | -0.009 | -0.023 | -0.012 |
| | (0.018) | (0.018) | (0.019) | (0.018) | (0.009) | | | |
| Reports Dirt in Water | 0.748 | 0.747 | 0.758 | 0.752 | 0.751 | 0.000 | -0.010 | -0.004 |
| | (0.022) | (0.022) | (0.022) | (0.022) | (0.011) | | | |
| Boils, Bleaches, or Chlorinates Water | 0.141 | 0.121 | 0.130 | 0.148 | 0.135 | 0.020 | 0.011 | -0.007 |
| | (0.017) | (0.017) | (0.017) | (0.018) | (0.009) | | | |
| Strains or Filters Water | 0.651 | 0.660 | 0.622 | 0.639 | 0.643 | -0.009 | 0.029 | 0.012 |
| | (0.024) | (0.024) | (0.025) | (0.024) | (0.012) | | | |
| Believes Chlorine is for Water Purification | 0.198 | 0.196 | 0.232 | 0.216 | 0.210 | 0.002 | -0.034 | -0.018 |
| | (0.020) | (0.020) | (0.022) | (0.021) | (0.010) | | | |
| Caretaker Asked about Chlorine Test | 0.136 | 0.173 | 0.143 | 0.140 | 0.148 | -0.037 | -0.007 | -0.004 |
| | (0.017) | (0.019) | (0.018) | (0.017) | (0.009) | | | |
| 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 E.6: Baseline Balance: Child Outcomes (Endline Sample)

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

*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 F Photos of Intervention Materials

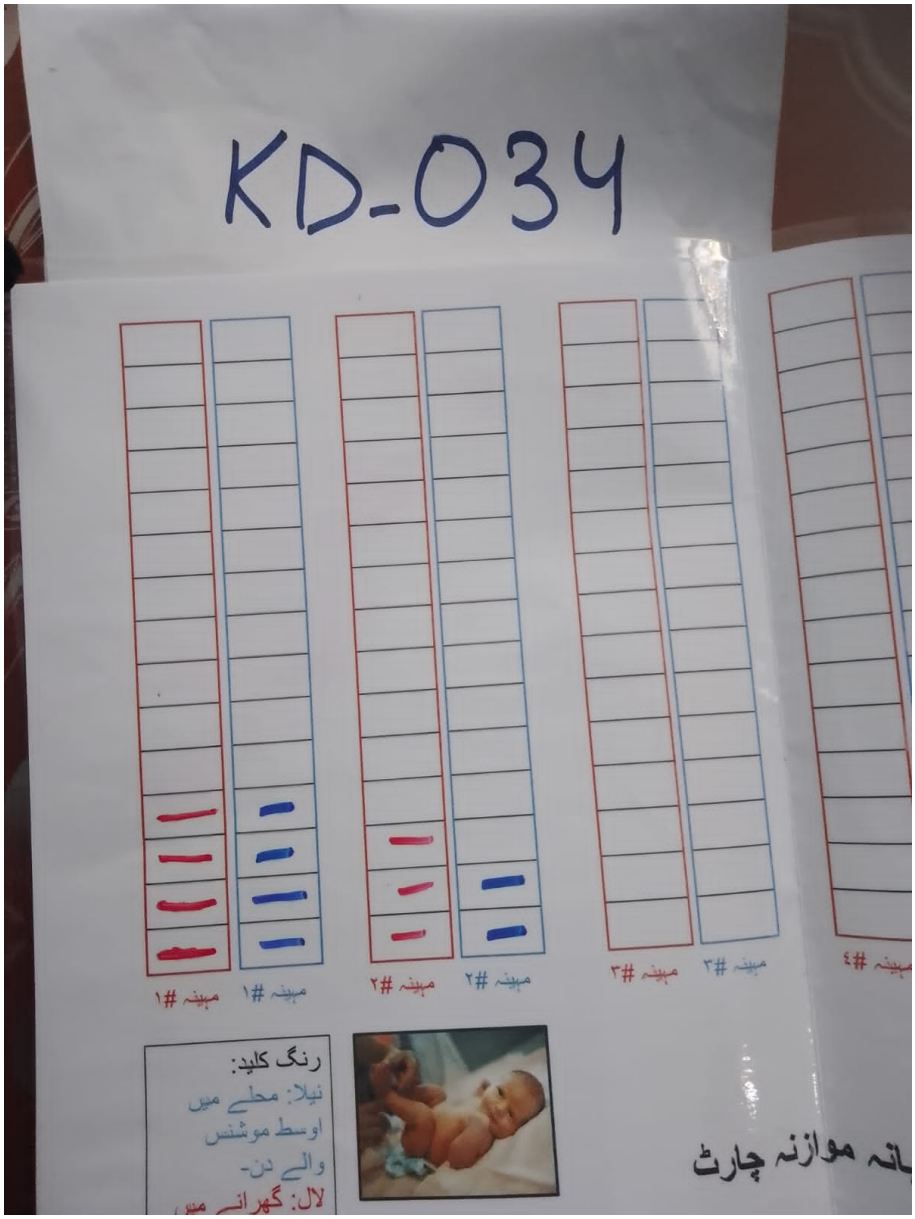



Figure F.1: Experiential Learning Intervention: Info-Tool

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)).

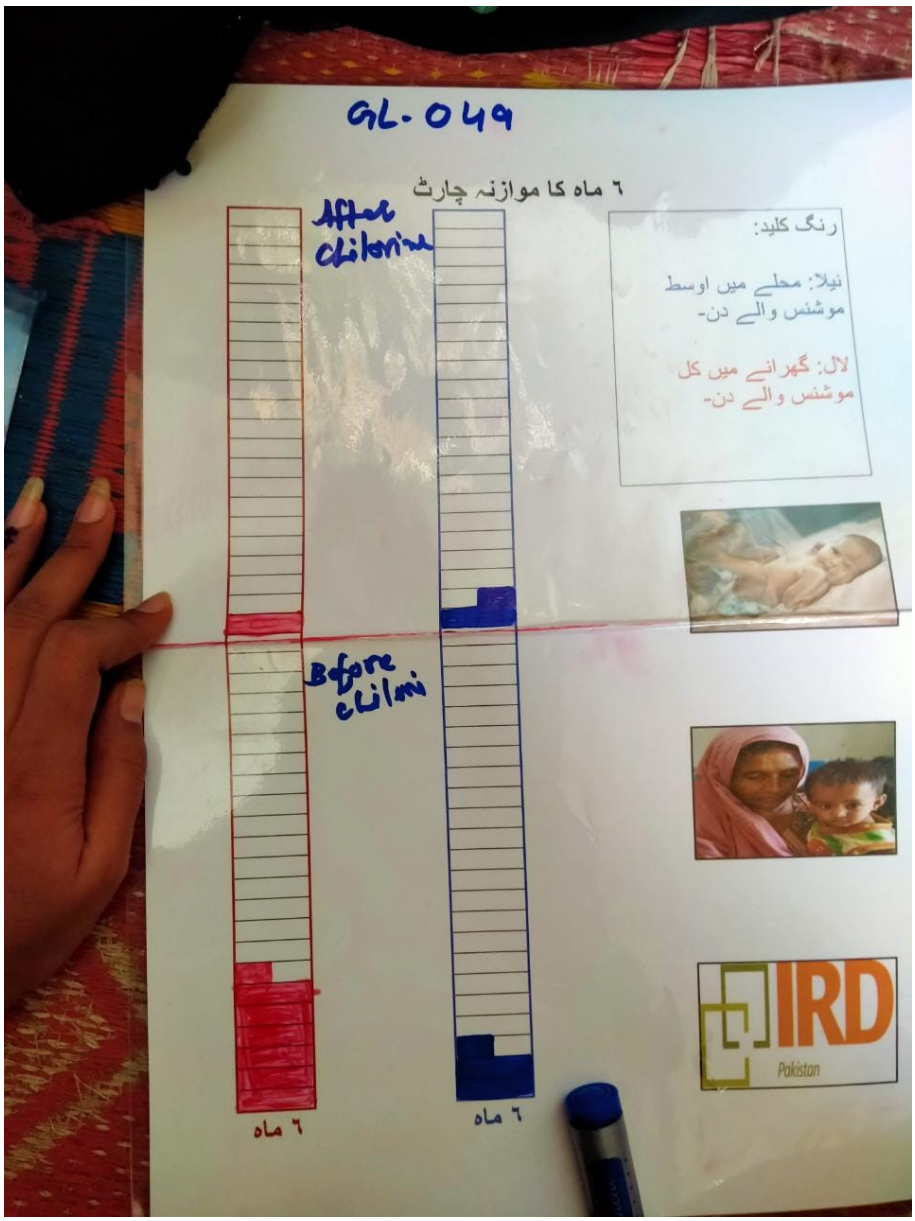



Figure F.2: Experiential Learning Intervention: Info-Tool (Difference-in-Differences Comparison)

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 F.3: Sample Water Vessel

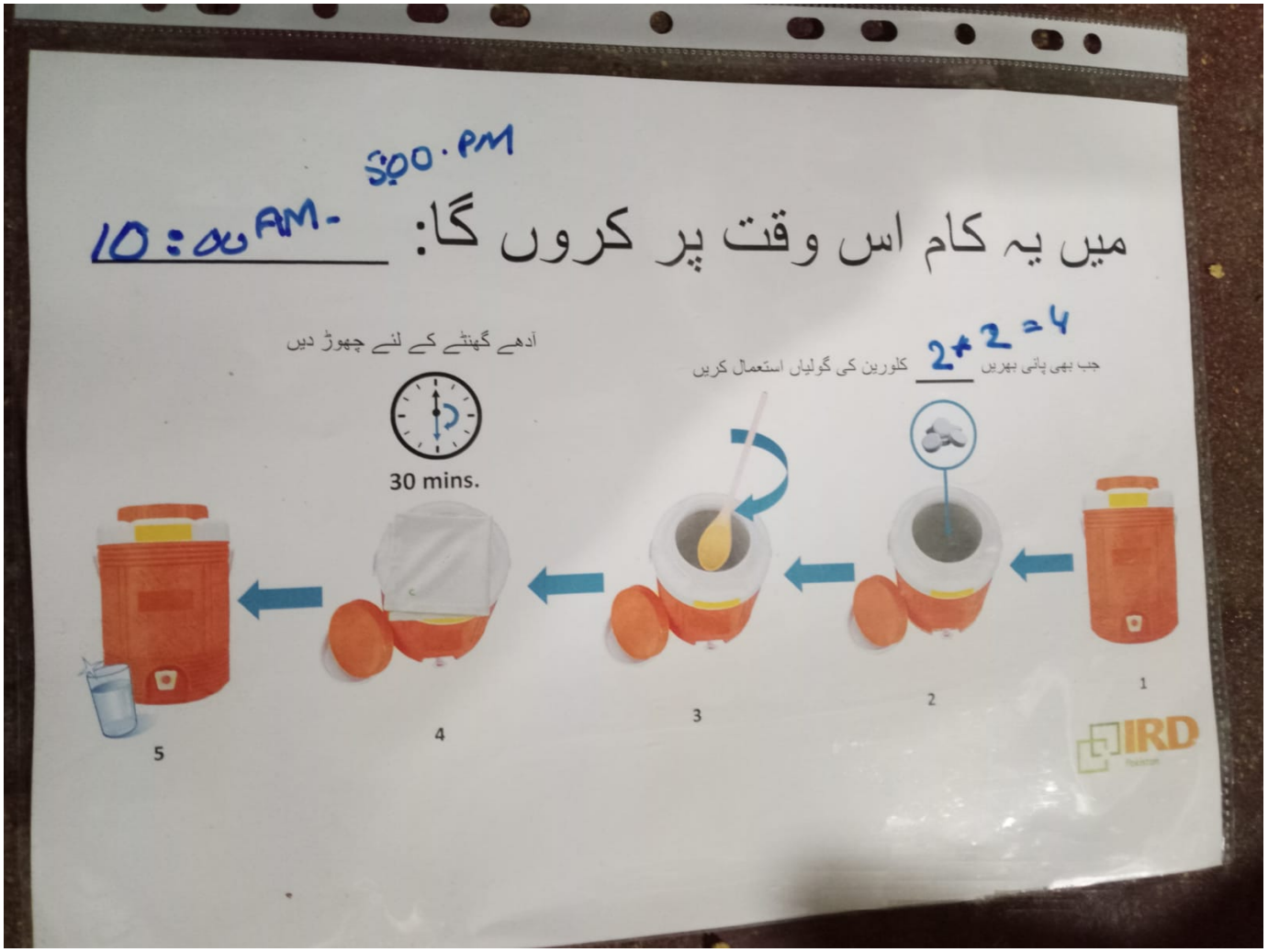



Figure F.4: Leaflet with Instructions on Chlorination

# Appendix G Model 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 ownership-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').

## G.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 *ownership-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 $Y_{\bar{t}}$:

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

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.

## G.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:

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

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

We observe a positive and *increasing* relationship between the number of Info-Tool friends and

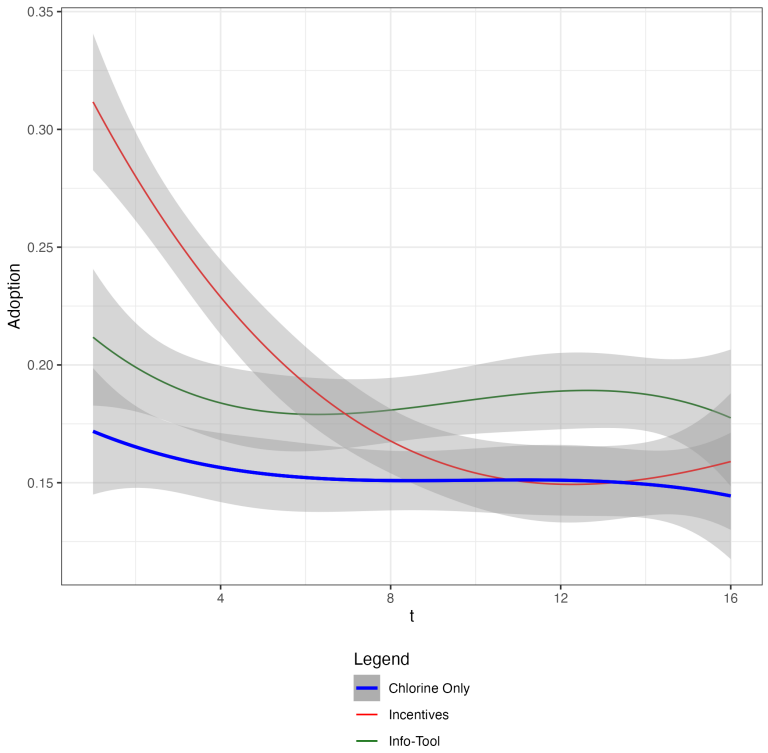



Figure G.1: Model Simulation: Chlorine Adoption Over Time

chlorine adoption within the Info-Tool group. However, this relationship is not present in the other groups. (Figure G.2).

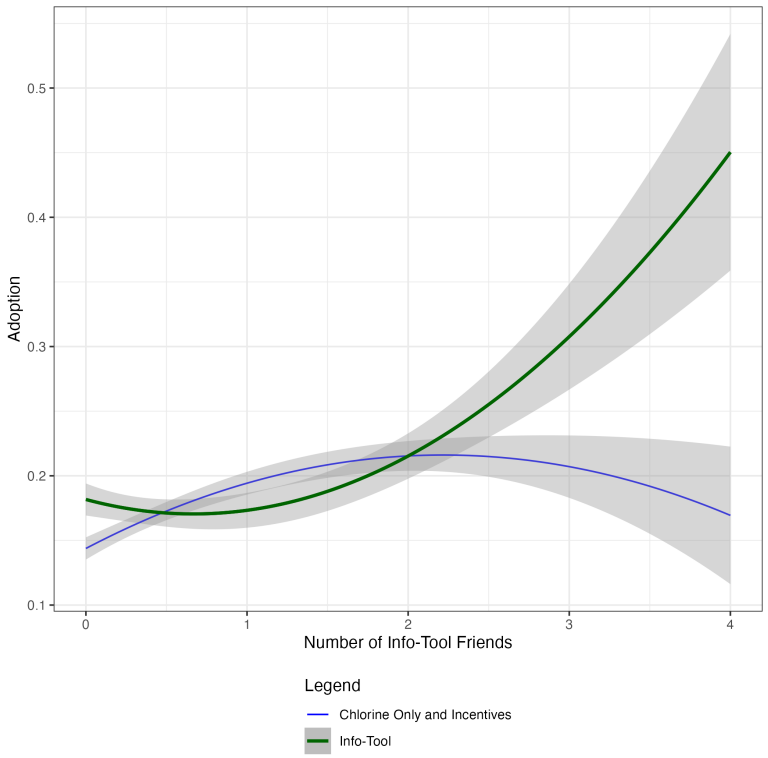



Figure G.2: Model Simulation: Chlorine Adoption as a Function of Info-Tool Friends

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 G.3 and G.4).

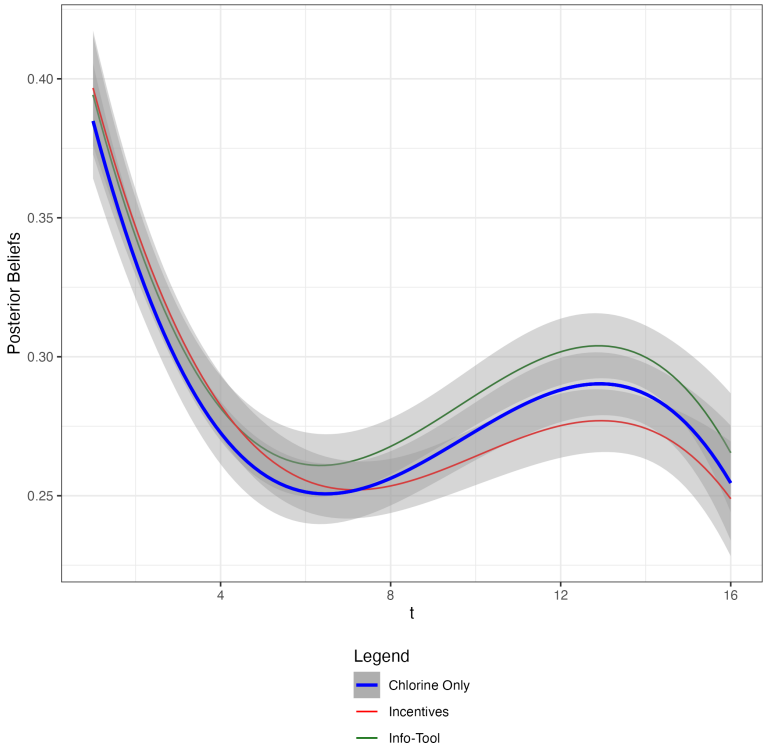



Figure G.3: Model Simulation: Stated Posterior Beliefs Over Time

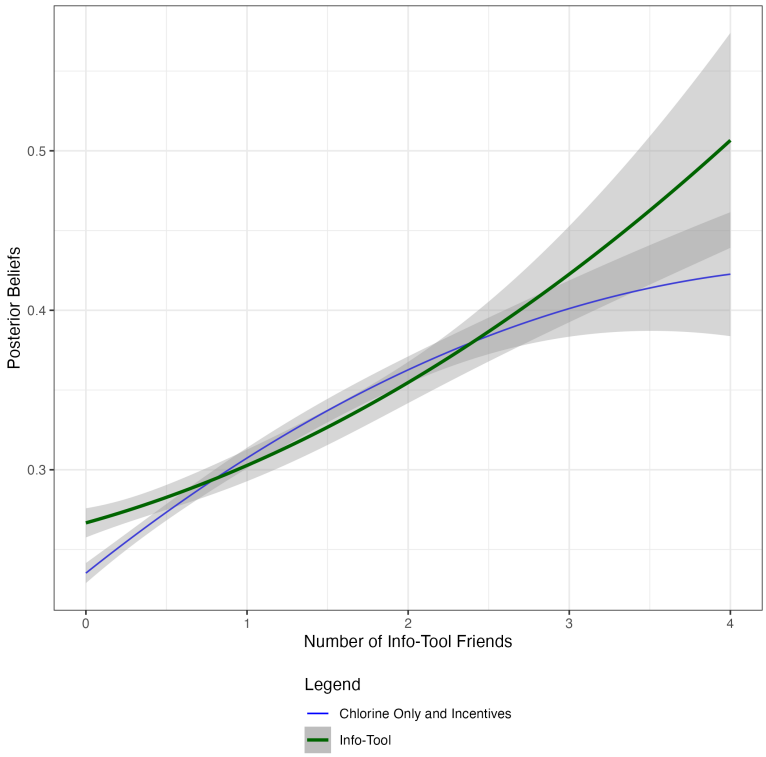



Figure G.4: Model Simulation: Stated Posterior Beliefs as a Function of Info-Tool Friends

# Appendix H AEA Registry Analysis

## H.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.

## H.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:

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 :

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 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.

## H.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.

## H.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 $K$:

$$K_t = \beta_0 + \beta_1 C_t + \beta_2 S_t + \beta_3 C_t \times S_t + \gamma_t + \epsilon_t \tag{3}$$

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.[50]

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.

[50]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 I Detailed Literature Review

## I.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)).

## I.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 J 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 K 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 C.2 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 6. 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.