

Social Engineering: Evidence from a Suite of Take-up Experiments in Kenya¹

Michael Kremer
Harvard University,
Brookings Institution, and NBER

Edward Miguel
University of California, Berkeley and NBER

Sendhil Mullainathan
Harvard University and NBER

Clair Null
Emory University

Alix Peterson Zwane
The Bill and Melinda Gates Foundation

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Abstract: Many effective health products and behaviors available through the private market are not widely adopted in less developed countries. For example, fewer than 10% of households in our Kenyan study area treat their water with dilute chlorine. Using a suite of randomized evaluations, we find that information and marketing interventions do little to boost use of chlorine. However, chlorine take-up is highly sensitive to price, convenience and social context, with more than half of households using chlorine when an individually-packaged supply is delivered free to the home. The highest sustained take-up is achieved by combining free, convenient, salient, and public access through a point-of-collection chlorine dispenser system and a local promoter. More than half of households treat their water and this use continues 30 months later even though promoters are paid only for the first six months. The estimated long-run costs of this intervention at scale, including administrative costs, are between \$0.25 and \$0.50 per person per year.

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1. Introduction

Two million children die of preventable diarrheal diseases each year and unsafe drinking water is often to blame (Bryce 2005). Randomized controlled trials have documented that point-of-use (POU) chlorination is an effective means of improving water quality and reduces reported child diarrhea by 29%³ Yet despite vigorous social marketing campaigns in many areas, take-up remains low under distribution models that rely on households to purchase individually-packaged chlorine, with under 10% of respondents reporting that their household's stored drinking water was chlorinated at baseline.

In this paper, we report results from a series of randomized evaluations designed to examine the roles of pricing and peers' decisions regarding usage of chlorination. We find that both are important factors. A point-of-collection chlorine dispenser system that includes free, convenient, salient, and public access, as well as a local promoter paid for a short period was able to achieve high levels of take-up that have been sustained more than two years after the program was launched.

There is an active debate on the topic of optimal pricing policies for health inputs in developing countries.⁴ Some argue that it is important to charge clients at least something for health-related goods and services, asserting that a positive price is important for financial sustainability, that it will screen out those who will not use the product, or that it may actually lead consumers to place higher value on the product. Another potential rationale for charging is that it could help to address moral hazard among providers or adverse selection between donors and implementing organizations. For example, a retail distribution model might provide a

³ See Arnold and Colford 2007, Clasen *et al.* 2006, and Fewtrell *et al.* 2005 for reviews of this literature. Schmidt and Cairncross (2009) argue that reported diarrhea measures are problematic because of reporting bias and that more evidence is needed objectively on measured health outcomes.

⁴ See, e.g., <http://www.nytimes.com/2007/10/09/health/09nets.html?ref=science> for a discussion relating to malaria nets.

method of incentivizing workers in the product supply chain or allow donors who subsidize goods and services to assess whether they are in fact valued by the target population.

We find that charging even a modest price sharply reduces demand for dilute chlorine solution. The majority of households that were randomly chosen to receive a free supply of dilute chlorine used it to treat their drinking water, but most households would not pay for the product. We find no evidence that price is an effective screening mechanism to target households with young children who are more likely to benefit from cleaner water. These results are consistent with the steep demand curves for other health technologies recently documented by Kremer and Miguel (2007) and Cohen and Dupas (2008).

One indication that social factors are important comes from testing an approach in which local community members were hired to promote chlorine use among their neighbors. In communities where the NGO gave each household one coupon for a free bottle of dilute chlorine (enough to last one month to six weeks) and paid a community member to serve as a local promoter, take-up was over 25 percentage points higher than in comparison communities at the six months mark and remained roughly 10 percentage points higher two and a half years later. This echoes results from evaluations of education service delivery in developing countries, suggesting that locally-hired workers paid at low wages can be highly effective (for example, Banerjee *et al.* 2007, Duflo *et al.* 2007, Muralidharan and Sundararaman 2008). It is also consistent with other recent evidence that external social pressure can induce pro-hygiene behaviors. For instance, people who were interviewed biweekly were much more likely to be chlorinating their water than those who were only surveyed biyearly (Zwane *et al.* 2011) even though the survey didn't focus entirely on chlorination. We find that incentivizing promoters by explicitly basing their pay on take-up rates has only modest impact on their effectiveness.

We also explored other means of promoting chlorination through social networks. Several different persuasion messages delivered by outsiders (similar to NGO sensitization campaigns) had little incremental effect on chlorine take-up when prices were low, and had only a moderate and short-lived effect at current retail prices. In addition to social effects instigated by these messages, we also tracked diffusion of the chlorine technology through households' own pre-existing social contacts. Specifically, we tested for informal social learning using exogenous variation in the proportion of a household's social network that had access to a free six-month supply of chlorine, extending the approach used in Kremer and Miguel (2007) by randomizing the number of community members who are exposed to treatment. While distribution of free chlorine supplies did generate more conversations about the product, this did not seem to generate changed behavior; individuals whose social contacts received free chlorine were not more likely than others to use the POU product themselves, although there is some evidence that links to community leaders led to somewhat higher take-up.

To combine the advantages of convenience, reduced cost, and social norms for promoting use of chlorination, we developed and tested a community-level chlorine dispenser system that can be installed at water sources (see Figure 1 for a photo). The dispenser is more convenient to use than bottled chlorine since it can automatically provide correct dosages (without risk of exposing skin), and transporting water provides agitation and time for treatment, reducing the wait time at home until water can be consumed. Because the cost of packaging and delivering POU chlorine for household use greatly exceeds the cost of the chemical, the long-run cost of supplying a community with bulk chlorine through a dispenser is a fraction that of supplying it with individually-packaged bottles. In addition, the dispenser provides a physical reminder for households to treat their water at the moment when it is most salient – as water is collected – and maximizes the potential for learning and social network effects by making the dispenser public.

Social impact theory predicts that peoples' behavior will conform more to social norms the more their actions are observed by others (Latané 1981). For instance, Americans are much more likely to wash their hands after going to the bathroom if there is someone else in the room (Pedersen *et al.* 1986). In communities where the dispenser system was in place, the majority of households chlorinate their water, and this was sustained two-and-a-half years later. We estimate that long-term, at scale costs would be as low as \$20-30 per DALY saved, which is extremely cost-effective relative to other public health interventions in less-developed countries.

Section 2 of this paper describes the study context, the POU chlorine product, the household samples and experimental interventions, and data collection procedures. Section 3 presents evidence on the importance of pricing. Section 4 documents that local promoters of the technology, elected by the community and paid by the NGO, were effective at convincing their neighbors to chlorinate in the short run, but that this program did not shift communities to new long-run social norms of use after the promoters were no longer paid for their efforts. Section 5 describes the chlorine dispenser system and shows that high take-up rates can be maintained cost-effectively using this delivery system.

Section 6 discusses the role of social effects in the take-up decision and provides evidence that the chlorine dispenser system induced social effects. We estimate social effects using two innovative research designs. First, we exploit the fact that point-of-use chlorine for home use was randomly distributed to a subset of households in our study communities to estimate the impact of this distribution on the social network of recipients, and find only minor impacts on their take-up. We use a different approach, based on Graham's (2008) excess variance method, to estimate peer effects in the chlorine dispenser communities, and find strong evidence of large peer effects in take-up. The public nature of the chlorine use action in the

context of the chlorine dispenser system, in contrast to the largely private point-of-use home action, is the leading explanation for the pattern of findings.

Section 7 discusses our results in the broader context of related research including cost-effectiveness estimates for investments in child health. The final section concludes.

2 Background

This section describes the setting, the dilute chlorine product, the household samples and experimental interventions, and the data collection procedures. (Detailed descriptions of the sampling strategies and randomizations into treatment groups can be found in Appendix A.)

2.1 *The study setting*

Summary statistics on household characteristics discussed below are drawn from Table 1, Panels A and B.

Our study site is located in rural western Kenya. The daily agricultural wage in the area ranges from US\$1 - US\$2 per day depending on the task; the average mother has a primary school education. Several related households often live together in a fenced compound, and on average there are between one and two children under the age of 3 in every compound. More than 80% of compounds have a latrine and roughly 90% of households have an available supply of soap for washing.

Water is typically collected from wells or springs by women and children, who carry water in plastic jerry-cans. Drinking water is typically then decanted into a wide-mouthed clay storage pot in the home, which keeps water cool. Water is retrieved from the pot using a dipper, typically a plastic cup without a handle. As a result, water often comes in contact with human hands prior to consumption, which can lead to increased contamination or recontamination of water that was safer to drink when collected at the source.

In practice, few households take steps to actively manage water quality. While some households report boiling their drinking water, Kremer *et.al.* (2010) note that the correlation between household water contamination and self-reported boiling is low, suggesting that there is a substantial social desirability bias and that people are over-reporting boiling. Fewer than 10% of sampled households report treating the drinking water currently in their home with chlorine.

2.2 POU chlorination in Kenya: The WaterGuard brand

Chlorine is widely used to disinfect municipal water supplies in developed countries. Population Services International (PSI), an NGO, markets, distributes, and maintains quality control for dilute chlorine (sodium hypochlorite) solutions in over 20 countries worldwide. The product was introduced in Kenya in 2003, branded as WaterGuard. Each 150 mL bottle is enough to treat 1000 L of water, or approximately a one-month to six week supply of drinking water for a household. It has a shelf-life of 18 months. Instructions are provided on the bottle in Swahili and in pictures: add one capful of the solution to 20 L of water (the standard jerry-can size), agitate briefly, and let sit for 30 minutes before consuming.⁵ Immediately after treatment, water smells and tastes strongly of chlorine, though this dissipates over time; if dosed appropriately, only a faint smell and taste remains after a few hours.⁶

At the time of our study, one bottle of WaterGuard sold for 20 Kenyan shillings (or US\$0.30), or about a quarter of the agricultural daily wage. PSI has conducted an extensive multi-year advertising campaign, with ads painted on buildings and broadcast over the radio. WaterGuard is available in many local shops thanks to PSI's distribution network.

⁵ Highly turbid water requires a double-dose. Very few households in our area rely on turbid water for drinking.

⁶ The odor and taste of the product in the bottle also serves as a deterrent against over dosing or children accidentally ingesting WaterGuard directly; though the bottle is not tamper resistant it is extremely unlikely that a child would try to drink from the bottle. The chlorine solution is sufficiently dilute (less than one quarter of the strength of household bleach, which many households also have in the home) that even direct consumption does not pose a serious health risk.

2.3 Project Interventions and Household Samples

This paper draws on data from two phases of project interventions that were designed to test various strategies for increasing take-up of WaterGuard and identify factors that are determinants of that choice.

Sample 1 (2004-2007)

The timing of the intervention and data collection for the first phase of the research is summarized in Panel A of Figure 2. A sample of 1384 households who used 184 naturally-occurring springs as their primary drinking water source were randomized into two cross-cutting treatments: a source water quality intervention (spring protection) and a point-of-use water quality intervention (WaterGuard promotion). This paper focuses on only the latter using data from the last two of four survey rounds; for more information on the spring-protection evaluation see Kremer et al. (2010). Spring communities were first randomized into either the “high-” or “low-intensity” for the WaterGuard intervention, in order to induce exogenous variation in the proportion of a respondent’s contacts who used the chlorine product. In high-intensity communities, 6 out of the 8 sample households were randomized into the WaterGuard treatment arm; in low-intensity communities only 2 of the 8 sample households were randomized into the treatment arm. Across the entire sample, half of the households were randomly selected to receive 7 150 mL bottles of WaterGuard and a voucher for an improved storage pot with a tap and a lid. Free WaterGuard was distributed during the third survey round and the follow-up survey round was collected between 2-7 months later. Additional randomizations selected one-third of the WaterGuard recipients into a treatment in which they were read additional intensive promotion messages by the survey enumerator and another cross-cutting third of households was randomly selected to receive 12 coupons for a 50% discount on WaterGuard at a local shop, redeemable monthly starting two months after households were given the free supply of bottles.

Data on coupon redemptions continued to be collected from shop-keepers until the expiry date for all coupons had passed, one year later.

Sample 2 (2007-2010)

In the second sample, we compare the effect of seven different treatments designed to increase WaterGuard take-up, using data from a baseline survey at the time the interventions were introduced and four follow-up surveys as summarized in Panel B of Figure 2. The seven treatments include variations on external promotional messages (scripts delivered at the community and household levels, as well as the combination: treatments 1A, 1B, and 1C, respectively), paid community-based promoters in combination with one-time subsidies for the product (two payment schemes: flat-fee and incentivized based on chlorine tests, treatments 2A and 2B, respectively)⁷, and community-based promoters in combination with a chlorine dispenser system that provides an unlimited supply of subsidized chlorine at the point of water collection (treatments 3A and 3B for incentivized and volunteer promoters, respectively), along with a comparison group which was not exposed to any treatment. We refer to Treatment 3B and its concurrent comparison group as the “Sample 2 Extension”, since these arms were added to the study almost two years after the baseline survey and interventions in the other communities.

⁷ Compensation schemes were not announced until after the community meeting when promoters were elected, so we measure the treatment, not selection, effect of different compensation schemes. Payments were framed as compensation for six months of effort but promoters were aware that they would not be paid again after the medium-run follow-up even if this was before the end of the 6 month period. In a random half of the promoter communities, the promoter was paid a flat fee of 450 Ksh (US\$6.43) at the short- and medium-run follow-up visits (treatment 2A). In the other half of the promoter communities, payment was based on the number of positive chlorine tests at the sample households during the short- and medium-run follow-up visits (treatment 2B). These “incentivized” promoters received a flat fee of 100 Ksh (US\$1.43) per follow-up visit plus 20 Ksh (US\$0.29) per positive chlorine test. The payment per positive test was calibrated to equate the two payment systems at a take-up rate of 70%, on the basis of pilot results; in practice incentivized promoters earned \$4.27 on average and none earned as much as their flat-fee counterparts. The daily agricultural wage for men in this area is between \$1 to 2, so flat-fee promoters received the equivalent of at least one day of work per month during the study, which is an arguably generous income supplement considering that promoters had all agreed to serve their communities in a volunteer capacity. Both flat-fee and incentivized promoters were informed that all households who had been included in the baseline survey would be revisited at follow-up and that several other community members would also be surveyed at that time in order to motivate the promoters to encourage all members of the community to use WaterGuard.

Importantly, all communities, including the Extension, were randomly selected from the same sampling frame and can thus be considered valid comparisons for one another; the gap in time between Sample 2 and the Extension communities could lead to confounding, but we use the results from the two separate comparison groups to control for this.

Roughly 20 households in each spring community were randomly selected to be surveyed at baseline from a list of all households using the spring. Household-level scripts and coupons were restricted to the baseline sample of households. An additional three households were randomly selected to be added to the sample for the follow-up rounds to assess the effects of the treatments on community members who had not been selected for the interventions.

Short-run follow-up surveys were conducted approximately 3 weeks after the baseline, with the medium-run follow-ups occurring between 3-6 months after baseline. The two long-run follow-up surveys were conducted 16-24 and 28-36 months after baseline, respectively.

2.4 *Data collection procedures*

The target respondent for household surveys was the mother of the youngest child living in the home compound (where extended families often co-reside), or another woman with childcare responsibilities if the mother of the youngest child was unavailable. In addition to the household survey data, we have data on coupon redemptions from shop-keepers' records that allow us to directly track which household redeemed coupons and in which months they did so. (Further details on the data collection protocols are provided in Appendix B.)

We measure chlorine use in household drinking water on the basis of an objective test for the level of total chlorine residual present in the water, rather than self-reported chlorination which likely overestimates the proportion of households who had actually chlorinated their water

because of courtesy or social desirability bias. Chlorine tests are a conservative lower bound on take-up because chlorine levels decline over time.

3 Impact of distributing free supplies of individually-packaged WaterGuard

This section discusses the estimation strategy we use throughout the rest of the analysis and presents the impacts of free WaterGuard distribution on take-up. Impacts of treatment on comparison households, as mediated by social networks, are discussed in Section 6. Thus, results presented in this section understate the effect of the treatment to the extent that comparison households were also affected. We also show that households, who are willing to use the POU product when it is provided for free, nonetheless have very low willingness to pay for WaterGuard.

3.1 Estimation strategy

Equation 1 illustrates an intention-to-treat (ITT) estimator using linear regression.

$$W_{it} = \alpha_t + \delta_i + \beta_1 T_{it} + \varepsilon_{it} \quad (1)$$

W_{it} is the chlorine use measure for household i at time t and T_{it} is a treatment indicator that takes on a value of one after the free WaterGuard intervention was implemented. Random assignment to treatment implies that β_1 is an unbiased estimate of the reduced-form ITT effect of WaterGuard receipt (as opposed to use). Survey-round fixed-effects α_t are included to control for time-varying factors affecting all households. Regression disturbance terms ε_{it} are clustered at the spring level in these regressions, since households using the same spring could have correlated outcomes: they share common water sources, the local sanitation environment, and may have kinship ties.

3.2 WaterGuard take-up

At the unannounced follow-up household visit two to seven months following WaterGuard receipt, most households (79%) that received free WaterGuard reported that their current supply of drinking water was treated and more than half (58%) had detectable levels of chlorine in their drinking water. Factoring in baseline take-up rates and time trends, we estimate the effect of the intervention to be a

52 percentage point increase in validated chlorination in a regression (Table 2, column 1). This is a huge effect relative to control self-reported and validated chlorination rates of just 6% and 4%, respectively.

Notably, low baseline chlorination rates occurred despite the fact that 68% of Sample 1 respondents volunteered drinking “dirty” water as a risk factor at baseline (Table 1, Panel A). Prior to being introduced to WaterGuard through the study, over 70% of respondents recognized the name of the local POU chlorination product and 30% said they had used it at some point in the past (Table 1, Panels A and B). Overall, households had very favorable pre-existing impressions of the product, with over 95% of respondents who were familiar WaterGuard saying that they thought a typical adult in their area would use WaterGuard if it was received as a gift and a similar percentage saying that they thought local households would use it during a cholera epidemic.⁸

Moreover, additional information about the product did nothing to boost demand. Among those who had a free supply of WaterGuard in Sample 1, take-up was no higher among the recipients of the additional social marketing script (Figure 3, Panel A and Table 2, column 2).

3.3 Willingness-to-pay for dilute chlorine solution

Take up of point-of-use chlorine is highly sensitive to price. In Figure 3, Panel B, we plot the proportion of households who use the product at the three prices faced by sample households: zero Ksh per bottle for treatment households at the follow-up survey, 10 Ksh for the subset of treatment households given the 12 months of coupons, and the 20 Ksh market price for comparison households (and treatment households prior to the intervention). As discussed above, demand is high at a price of

⁸ One other factor that could have influenced take-up rates relates to the improved water storage containers that were distributed with the free WaterGuard. Some of these clay pots were poorly manufactured and leaked. Largely as a result of these problems, 30% of households who received pots report not using them. Because households who were given WaterGuard were specifically instructed that it would be most effective if used in the improved containers, when the new pots failed, some households may have decided not to use the WaterGuard. Indeed, both self-reported and validated take-up rates are significantly lower among households who specifically complained about their pot being broken relative to those who did not. Had this aspect of the intervention not been so problematic, perhaps take-up rates would have been even higher.

zero using either self-reported use or positive chlorine test results, but drops off precipitously at even the low price of 10 Ksh per bottle.⁹ An increase in the price from 10 to 20 Ksh barely affects demand.

4 Inducing a social norm of chlorination: The role of promoters

In Sample 2, the fraction of households with residual chlorine in their water was approximately ten times as high in communities with a local promoter (treatments 2A and 2B) relative to comparison households in the short-run 3 week follow-up survey, at 40% roughly versus 4%, respectively (Figure 4 and Table 3 Panel C, Column 1). Eighty-six percent of households who were given a coupon for a free bottle of WaterGuard said they had redeemed it, and 97% of these households reported using it. While take-up fell somewhat at the medium-run (3-6 month) follow-up as coupon recipients used up their free bottles, communities with promoters were nonetheless able to sustain adoption rates between 30-35 percentage points higher than the comparison group take-up rate of 8% (Column 2). Take-up remained significantly higher among the two promoter groups relative to the comparison communities in both long-run follow-ups, at over double the comparison group chlorination rates (Columns 3 and 4), even as take-up rates in the comparison groups increased significantly over time (Panel A). We estimate that somewhat less than half the initial effect of promoters was sustained after payments to promoters ended (A separate paper evaluates compensation strategies of flat fees versus incentives and finds that flat fee promoters perform well.¹⁰).

In contrast to the success of local promoters, additional social marketing scripts read by NGO representatives at the household and community levels, as well as the combination, did not boost

⁹ The low coupon redemption rate among households who had just received a free supply also implies that a lack of familiarity with the product is not the only impediment to product adoption.

¹⁰ Promoters were selected by their communities and initially agreed to serve without considering compensation. This may explain why the compensation system seemed to play a relatively small role in determining promoters' success at increasing take-up rates of WaterGuard

take-up relative to the comparison group in Sample 2, beyond a very short-lived uptick attributable to community scripts in the short-run follow-up (Figure 4 Treatment 1A-C versus Control and Table 3, Panel B).

While the research design does not allow us to precisely identify the particular channels through which local promoters were much more effective than one-time intensive social marketing scripts delivered by a survey enumerator, there are several reasonable possibilities, including the higher frequency of promoter messages, and the fact that promoters are community members, with greater local knowledge, trust and social influence than NGO outsiders.

Our results on the effects of promoters are consistent with several other recent studies that have documented that household POU take-up is responsive to increased scrutiny by others, in some cases even when the extra attention is not intended to change behavior. Kremer *et al.* (2009b) examine the impact of frequency of surveying households about diarrhea prevalence and testing their stored drinking water if they said they were chlorinating. Households randomly selected for surveying on a bi-weekly basis were between two and three times as likely to have chlorine residual in their water relative to households who were surveyed once every six months. Not far geographically from our Kenyan study site, Garrett *et al.* (2008) were able to verify residual chlorine in the stored water of 43% of study households after an intervention in which community health workers promoted household water treatment (but did not offer the product at a discount).¹¹ Data for that study were also conducted during weekly visits by interviewers who asked about diarrhea prevalence.

¹¹ Community health workers also promoted safe water storage containers like those distributed in our intervention, as well as latrines, shallow wells, and rainwater harvesting.

5 The point-of-collection chlorine dispenser system

The chlorine dispenser system builds on the model of a local promoter described in the previous section by providing a convenient and unlimited supply of chlorine available to all members of the community free of charge in addition to the locally-based behavior change campaign implemented by the promoter. By comparing communities with the chlorine dispenser system (treatments 3A and 3B) to those with only promoters (treatments 2A and 2B), we see that the price reduction and convenience associated with the dispenser are important components of increasing demand. Specifically, take-up among communities with the chlorine dispenser system (including a paid promoter) was much higher than among communities with only a promoter (and coupons), (Table 3), and this difference was sustained over time.

That said, we also find evidence that the dispenser hardware itself is not the only explanation for high take-up rates observed in these communities: medium-run take-up was significantly higher among communities where the promoter was paid (treatment 3A) than among those where the promoter was a volunteer (treatment 3B) (Figure 5 and Table 3 Panels C and D, Column 2; p-value for the comparison between treatment 3A and 3B = 0.02). Moreover, whereas take-up increased significantly between the short- and medium-run follow-ups in communities where the dispenser promoter was paid, communities with volunteer dispenser promoters did not see any increase in take-up over this time. While we cannot draw definitive conclusions regarding the role of the promoter, this result could imply that the effect of the dispenser hardware alone was to increase take-up by roughly 30 percentage points over the comparison group (treatment 3B; Table 3 Panel D, Columns 2 and 3) - in contrast, the significant increase take-up over time between the short- and medium-run follow-ups in communities with paid dispenser promoters could reflect the promoters' efforts to help households establish a habit of water treatment (treatment 3A; Table 3 Panel C, Columns 2 and 3).

Interestingly, though, it does not appear necessary to pay promoters indefinitely. Take-up among communities where promoters were paid only for the first six months (treatment 3A) is not significantly lower in either of the long-term follow-up surveys after payments had been suspended (Figure 4 and Table 3 Panel C, Columns 3 and 4).¹² Although paying promoters indefinitely would be a very costly strategy for increasing take-up of chlorination, it appears to be a worthwhile investment in inducing a social norm of dispenser use that can then be sustained with subsidies for the low cost of the chlorine itself.

Taken together, these results underscore the importance of both price subsidies and early promotion as components of the chlorine dispenser system's potential to achieve sustained take-up at low cost.

6 Social effects in chlorine use

6.1 Exploiting exogenous variation in peers' technology adoption (Sample 1)

Using detailed data on conversation frequency and topics collected in the second and fourth survey rounds among Sample 1 households, we find strong evidence that the distribution of free WaterGuard promoted conversations about the product and even more broadly about child health, although to a lesser degree (Table 4 Panel A). In particular, respondents were roughly three times more likely to report ever having a conversation about WaterGuard with the other study participants in their community if the respondent was a member of a treatment household and slightly more than twice as likely if the other household in a relationship pair was in the treatment group (Column 1).^{13,14} Similar increases were also observed for the probability that a

¹² It is possible that with paid promoters, take-up would have continued to grow over time.

¹³ Importantly, while courtesy bias could certainly be inflating the effects of being in the treatment group, since treatment households might feel compelled to tell the enumerator that they discussed the topics of the intervention with other people, the coefficient on the treatment indicator for the non-respondent in the pair is less likely to suffer from such bias.

respondent reported having many conversations about WaterGuard. There was a much smaller but still statistically significant increase in the probability that a respondent in the WaterGuard treatment group had ever spoken with other households about child health (Column 2), suggesting that these households at least partially internalized messages about the connection between water and children’s health that were delivered as part of the intervention.

The exogenous variation in exposure to the product through social networks induced by the high- and low-intensity randomization allows us to directly test for social network effects in the take-up decision. We begin by testing whether households at high-intensity treatment springs were more likely to use WaterGuard (Table 4, Panel B Column 1; using an indicator variable for whether or not household i lives at a high-intensity spring) and then test whether the proportion of treated links has any effect on take-up (Column 2; using the proportion of household i ’s close contacts who received free WaterGuard), always controlling for the household’s own treatment status.¹⁵

We find limited evidence for peer effects (Table 4 Panel B, Columns 1 and 2). Looking across the whole sample, we find no significant effects, and the high vs. low intensity treatment does not seem to affect non-treated households. There is some marginally significant evidence for effects of peer exposure through certain types of community members, although this should be taken with a grain of salt due to potential concerns regarding data mining in subsamples.¹⁶ In

¹⁴ We do not observe significant interactions between the respondent’s treatment status and the paired household’s status in any of these specifications, nor do we find evidence that any additional conversations led to a meaningful increase in the closeness of relationships between study households (results not shown).

¹⁵ In the proportion specification, the baseline total number of close contacts in our sample interacted with a post-intervention indicator is also included as a control variable to account for the fact that more sociable people may more readily adopt new technologies. Likewise, we also include a indicator for households who have no close contacts interacted with a post-intervention indicator to allow for the possibility that non-sociable people might be more or less likely to adopt new technologies.

¹⁶ In addition to those described below, we also tested for the effects of relationships with households who received WaterGuard and were family members, those with whom the respondent had previously discussed WaterGuard, those who are socially well-connected (listed as a close contact by more than the median number of other

particular, households are more likely to use WaterGuard if a higher proportion of the members of the same tribe or of community leaders received WaterGuard (column 3). In addition, there is marginally significant evidence that self-identified community leaders may play a special role (Column 4).¹⁷

Interestingly, we observe much stronger peer effects in these same specifications using self-reported chlorination as the dependent variable rather than verified chlorine residual in the household's stored drinking water as discussed above (results not shown). We interpret this contrast as evidence of social desirability bias, implying that study participants thought chlorination was the right thing to do, even if they didn't actually do it themselves. If this is indeed the case, then the fact that the chlorine dispenser makes the take-up decision public could lead to peer effects being an important driver of the chlorine dispenser system's success at boosting take-up. We test for social effects with the chlorine dispenser in the next section.

6.2 Excess Variance Tests (Sample 2)

The adoption of chlorine dispensers – as measured by chlorine tests of household drinking water – varies substantially across observationally similar communities. For example, at 18 months after the interventions took place, the average take-up rates ranged from 22% to 80%.

One explanation is community-level heterogeneity in household demand for clean water. An alternative explanation for this “excess” take-up variation across communities is that it reflects social interactions or peer group effects. If residents within the same community learn from each other's experiences, then households' take-up will covary positively within a community, and thus display excess variation across communities. We apply the excess variance test in Graham

households at the spring), and those who reported an outbreak of cholera occurring in their community in the past two years, but we find no additional effect from any of these social relationships.

¹⁷ Includes self-identified leaders of women's groups, farmer/agricultural groups, water group/well committee, credit/savings/insurance groups, prayer or bible study groups, burial committees, and school committees or clubs.

(2008) to assess the extent of social effects in both dispenser communities and promoter communities.

Communities vary substantially in size (as measured by the number of households who use the spring as a primary water source), ranging from a minimum of 11 households to a maximum of 60 households with a median of 28 households. Across relatively large communities, random variation between households will generate little random variation in mean peer household demand for treated water. Across spring communities with few households, household variation will generate greater community-level variation in mean peer demand for treated water. As a result, the variance of peer use (as measured by average household demand for treated water) is greater across the set of small communities than it is across the set of large communities. If there are peer effects for usage, then communities with high (low) take up due to individual level variation will endogenously generate even higher (lower) equilibrium take up, so small communities will have even greater variance in community-level take-up than would be predicted based on the assumption of i.i.d. individual decisions.¹⁸ The first piece of suggestive evidence that this may in fact be the case is presented in Figure 6, which non-parametrically displays the community-level chlorine take-up rates for both the larger than median and the smaller than median communities. It is visually evident that there is considerably more dispersion in average community take-up in the small communities. A formal statistical test is required to determine if this dispersion is simply due to greater sampling variation in smaller

¹⁸ Graham (2008) lays out the key assumptions that need to be satisfied for his approach to be valid for community size to provide a plausible source of variation to identify peer effects. First, we need to assume that the distributions of community and promoter characteristics are similar across communities of varying sizes. We have run a variety of tests and do not find statistically significant differences across small and large population communities in a wide range of observable household characteristics. The other three key assumptions – Assumption 1: Pseudo-random Assignment, Assumption 2: Stochastic Separability (individual promoters are allowed to have a comparative advantage in either small or large communities, but the population variance of small and large community promoter effectiveness is restricted to be the same), and Assumption 3: “Peer quality” variation (a testable rank restriction) – are all likely to be satisfied in our case.

communities, and to assess the statistical significance of differences across communities of different sizes. For a certain class of social interaction models (e.g. the linear-in-means model), the unconditional between-group variance of outcomes is the sum of three terms. The first term equals the variance of any group-level heterogeneity (e.g., the effectiveness of an individual chlorine promoter or the extent to which the water supply is contaminated.). The second term equals the between-group variance of any individual-level heterogeneity (e.g., the variance of average household demand for treated water across springs). The third term reflects the social interactions.

Before describing the regressions, it is useful to define two key variables: within-community variance in take-up (G_c^w) and between-community variance in take-up (G_c^b). Given that we observe outcomes for a random subsample of households at communities with more than 25 households, the two variables are defined as follows:

$$G_c^w = \frac{1}{M_c} \frac{1}{M_c^S - 1} \sum_{i=1}^{M_c^S} (Y_{ci} - \bar{Y}_c^S)^2$$

$$G_c^b = (\bar{Y}_c^S - \mu_Y(M_c))^2 - \left(\frac{1}{M_c^S} - \frac{1}{M_c} \right) \frac{1}{M_c^S - 1} \sum_{i=1}^{M_c^S} (Y_{ci} - \bar{Y}_c^S)^2$$

where i indexes the households in each community, and c indexes the community. M_c is the total number of households in community c , M_c^S is the total number of sampled households in community c , Y_{ci} is a binary variable indicating whether household i in community c had a positive chlorine test, and $\mu_Y(M_c)$ is the average community-level take-up for communities of size M_c . In practice, the first term of G_c^b is computed in two steps. First, individual outcomes (Y_{ci}) are regressed on community size (M_c). Second, an average of residuals from the first-step regression is computed for each community, which is then squared.

Intuitively, G_c^w measures the variance in take-up within community c ; while G_c^b measures the deviation in community-level take-up from the average take-up at communities of the same size. Once these two variables are constructed, we can proceed to the IV estimation. In the first stage, we regress G_c^w on community size, including survey-round fixed effects as controls. In the second stage, we regress G_c^b on the predicted values of G_c^w from the first stage, again with survey-round fixed effects as controls. Under the assumptions in Graham (2008), the coefficient on the predicted within-community variance is a consistent estimate of the square of the peer effect γ in the linear-in-means model, where $\gamma=1$ corresponds to the case of no peer effects. An estimated coefficient on the predicted within-community variance that is significantly greater than one indicates that there are positive social effects.

The results applying the method in Graham (2008) are presented in Table 5, where Panel A contains the main IV results testing for the existence of peer effects, Panel B presents the first stage and Panel C the reduced form estimates. Column 1 presents the main result for the dispenser community sample (treatments 3A and 3B): there is a positive coefficient estimate (5.47, s.e. 2.02) on the within-group variance term, and the hypothesis that this coefficient equals one is rejected at over 95% confidence (p-value=0.029). This implies a γ term greater than two in the linear-in-means model, a substantial positive peer effect. The instrumental variable first stage regression is strong (Panel B), with an F-statistic of 71.4, and the reduced form relationship (Panel C) is also statistically significant. Column 2 examines whether this effect is growing over time, for instance, if it takes time for a new social norm of dispenser use to become well-established. While the point estimate on the time trend interacted with the within-group variance term is positive and large, it is not statistically significant at traditional confidence levels.

The next column examines if there are similar peer effects for chlorine use in promoter communities that did not also have a chlorine dispenser installed (treatments 2A and 2B). The leading explanation for such a difference might be the publicly observable nature of dispenser use given their location at well-used community water points, in contrast to the more private nature of WaterGuard use within the home. In Column 3 we find that the point estimate on the within-group variance term is much closer to 1 (at 1.57, s.e. 2.10), and we cannot reject the hypothesis that this estimate equals one (p-value=0.78). Thus, the evidence for positive peer effects is much stronger for the chlorine dispenser communities than for those communities that only had promoters. That said, due in part to limited statistical power given the relatively small sample of communities, we cannot reject that the magnitudes of the social effects in both the chlorine dispenser communities and the promoter-only communities are equal (Column 4, where the interaction term coefficient estimate is negative at -1.22 but not close to traditional confidence levels).

7 Results in the Global Health Context

7.1 *Cost-effective strategies for increasing take-up of POU chlorination*

Preliminary cost estimates based on the documented health benefits of point-of-use chlorination and our best approximation of the long-run cost of bulk chlorine at scale provision suggest that the cost of each dispenser system could be as low as \$0.25-\$0.50 per person per year, including the hardware installation cost of the dispenser (which is expected to last five years) and chlorine delivery.¹⁹ The cost per DALY saved, at scale, is estimated to be \$20-30. This is well below the common benchmark for cost-effective health interventions in developing countries of \$150/DALY.

¹⁹ The authors thank Vivian Hoffman for these cost-effectiveness estimates.

8 Conclusion

We examine the role of pricing and peers in households' demand for chlorine as a point-of-use water treatment technology, using a series of randomized evaluations that generated exogenous variation in each of these factors. We find that price is extremely important for Kenyan households, whereas even intensive person-to-person persuasion campaigns have only a modest and short-lived effect. The provision of a free supply of chlorine led the majority of households to treat their drinking water, even when under 10% were treating their water at market prices. A 50% discount off the retail price was not enough to convince most households to buy the product, and we find no evidence that positive prices serve as an effective means of targeting those households most vulnerable to diarrhea, namely those with small children. Intensive social marketing by an NGO at household visits and community meetings had a modest and short-lived effect on take-up rates and would be extremely expensive to implement on a larger scale.

There is strong evidence of positive social effects in chlorine dispenser communities, and limited evidence for peer effects with individually packaged chlorine. The leading explanation for this divergence is the public nature of the chlorine use action in dispenser communities, given the dispensers' placement at major local water points.

The highest take-up rates were achieved in communities that were provided with a free supply of chlorine via a point-of-collection dispenser in combination with a local promoter. In general, local chlorine promoters were effective at boosting take-up, even after households' free trial supplies of chlorine ran out. Basing promoters' pay on take-up rates did not make a difference in their effectiveness relative to promoters who were paid a flat fee, although there is some evidence that paid promoters were more effective than volunteers. In the individual-supply communities, the long-term effects of promotion were only half of the medium-term effects.

However, take-up in dispenser communities was sustained at high levels even after promoters were no longer paid, perhaps because of the public nature of the chlorine dispenser system.

The point-of-collection chlorine dispenser system cuts the cost of chlorine by reducing packaging, and is designed to boost take up by making water treatment convenient, providing a visual reminder, and facilitating social network effects by making the chlorine treatment decision public. The sustained high take-up leads us to be the most optimistic about this strategy for increasing chlorine take-up as an alternative to the current status quo of individually-packaged point-of-use chlorine distributed through retail channels.

The chlorine dispenser system's real-world potential appears great, and this is one of the most salient findings of the current paper. Yet some important questions remain. In future research, we will address the crucial question of how to design distribution chains for chlorine refills.

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Appendix A: Sample selection and randomization procedures

The current study is one component of a larger project, called the Rural Water Project (RWP), which also examines source water quality improvements and water quantity-related interventions, and which may provide guidance on priorities in the rural water sector. The first sample of households analyzed in this paper were initially identified for another aspect of the RWP, which was concerned with estimating the impacts of spring protection infrastructure. Spring protection seals off the source of a spring and reduces source water contamination from exposure to the environment. Randomization of households into the initial WaterGuard intervention was cross-cut with randomization of their communities (defined by the spring from which they collect their water) into the spring protection intervention. Panel A of Figure 2 summarizes the randomization process and the timing of survey activities for this phase of the research.

As discussed in Kremer, Miguel, Leino, and Zwane (2008), the household sample used in the first phase of the research is representative of users at each source and was constructed according to the following procedure, which we developed to address the fact that there were no administrative records in this rural area that would have identified all households that use a given spring. Instead, survey enumerators interviewed users at each spring, asking their names as well as the names of other household users. Enumerators elicited additional information on spring users from the three to four households located nearest to the spring. Households that were named at least twice among all interviewed subjects were designated as “spring users”. The number of household spring users varied from eight to 59 with a mean of 31. Seven to eight households per spring were then selected (using a computer random number generator) from this spring user list for the household sample used in this paper. In subsequent surveys, over 98% of this spring users sample was later found to actually use the spring at least sometimes, but the few baseline non-user households were nonetheless retained in the analysis.

The spring user list is representative of all households living near sample springs. In a census of all households living within roughly a 20 minute walk at nine sample springs, nearly three quarters of these nearby households were included on the original spring users lists, with even higher rates for those households located within a 10 minute walk of the sample spring. A description of the spring selection and randomization procedures is provided in Kremer *et al.* (2009).

Panel B of Figure 2 summarizes how springs and households were randomized into the various treatment arms in the second phase of the research and documents the timing of the data collection process. Springs were first randomly assigned to each of the five community-level treatments described above (using a computer random number generator). Prior to the baseline visit, an enumerator visited each spring community and asked two community leaders to compile a list of all compounds located near enough to the spring to be able to use it as a water source if they so chose. On the day of the baseline visit, enumerators began by cross-checking these two lists to identify duplicates. Once the list of compounds had been finalized, a random sample for the survey was selected in the field using schedules produced by the research team (with a computer random number generator) that picked compounds based on their position in the list (schedules were stored in sealed envelopes until the list was finalized). This same procedure was used to randomize households into the household script treatment at springs in the control and community script treatment arms.

To address concerns about seasonal variation in water quality and disease burden, all springs were stratified geographically and randomly assigned to an activity “wave,” and all

project activities were conducted by wave (three waves in the first phase of the research and two waves in the second phase).

Appendix B: Measuring chlorination, hygiene knowledge, and social networks

Chlorination

All households were asked whether the water in their primary drinking water storage container was treated with WaterGuard or any other chlorine products. Among respondents who reported treating the drinking water currently in their storage pot, a sample was taken to test for the presence of chlorine residual. The water was tested for total chlorine levels using Pocket Colorimeter II handheld devices, produced by Hach Company, using the following protocol: A 10 mL bottle was rinsed twice with the sample water, and re-filled. The blank was used to reset the machine to zero on the low-range measurement scale and then the contents of one DPD Total Chlorine sachet were added to the sample and agitated gently for 20 seconds. The enumerator recorded the color (clear, light pink, pink) and the sample was then loaded into the machine. After 5 minutes the numeric reading was taken. We test for total chlorine rather than free chlorine, which is the subset of total chlorine that actually disinfects the water, since the primary outcome in this study is take-up. The procedure is equivalent to USEPA Standard Method 4500-CL G for drinking water. The test provides an instantaneous visual confirmation of whether chlorine is present in water; if a sample contains chlorine, the reagent causes the water to turn a shade of pink, with darker colors proportional to higher concentrations of chlorine. In addition, after a short delay, a numeric estimate of the mg/L of chlorine present in the water is produced by the colorimeter. Bi-monthly quality-control checks ensured consistency across the set of colorimeters and each colorimeter's internal consistency was also periodically confirmed.

Depending on the elapsed time since treatment and the characteristics of the storage container, the level of residual chlorine in the water can vary drastically. Experiments conducted in favorable controlled conditions using actual WaterGuard and clay storage containers similar to the type used by the majority of households in our study suggest that residual chlorine may no longer be detectable as few as 12 hours after treatment with WaterGuard following the manufacturer's directions. Details of these experiments are available upon request. Other studies have also noted similar problems with measurement of chlorine in such circumstances (Ogutu *et al.* 2001 and Lantagne 2008). Since we are interested in whether or not the water was *ever* treated with chlorine, rather than the current concentration in the water, we use a definition of take-up that is based on the lowest concentration chlorine (.1 mg/L with pink color) that could not plausibly be a false positive and acknowledge that this cut-off likely leads to false negatives in many cases, given that two-thirds of the respondents who said their water was treated had added chlorine more than 12 hours prior and were using clay storage pots.

Hygiene knowledge and behaviors

In the first sample, a baseline "diarrhea prevention knowledge score", was constructed based on the number of correct responses to an unprompted question on methods to prevent diarrhea; provided. The set of plausible answers include "boil drinking water", "eat clean/protected/washed food", "drink only clean water", "use latrine", "cook food fully", "do not eat spoiled food", "wash hands", "have good hygiene", "medication", or "clean dishes/utensils".

Social network data

In the survey round prior to the WaterGuard intervention in Sample 1, we collected data on each household's relationship to every other sample household living at their spring. Respondents volunteered the nature of their relationship with each of the other survey respondents (e.g. neighbors, familial relationships, community settings in which they primarily interact), as well as whether or not they share the same mother tongue, and how frequently they spoke with the other household in general and on the specific topics of children's health problems, drinking water, and WaterGuard. This social networks module of the questionnaire was repeated in the survey round following the WaterGuard intervention. For the last 40% of the follow-up surveys, additional questions asked whether or not the respondent had received a gift of WaterGuard from the other household or made a gift to them, allowing us to directly observe some of the sharing occurring within the spring community.

Social Network data (Sample 1)

We collected data on a household's social ties with all seven other households surveyed in the same community, for a total of $(8 \text{ respondents}) \times (7 \text{ other households}) \times (2 \text{ directions of a social link, } i \text{ to } j \text{ and } j \text{ to } i) = 112 \text{ relationship pairs per community}$.

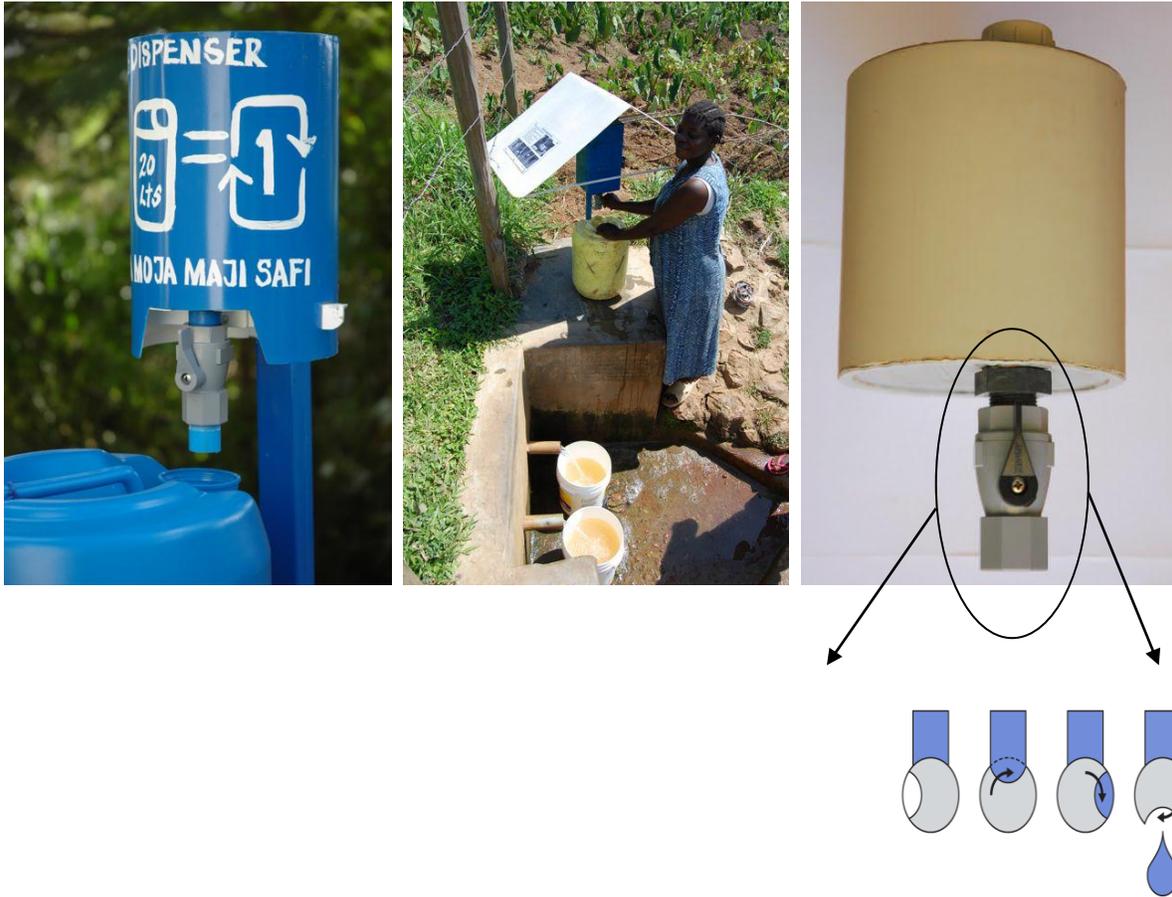
The spring communities in rural western Kenya are quite ethnically homogenous, with three-quarters of all respondent pairs saying that they are members of the same tribe.²⁰ Though different tribes have different mother tongues, communication barriers do not appear consequential – we find that conversation frequency is similar among individuals in the same versus different tribes – most likely due to widespread fluency in Swahili. The majority (59%) of respondent pairs share a family bond, the most common of which are mother in-law/daughter in-law (around 20% of relationships) and sisters in-law (around 25%), a reflection of the social institutions in this area that lead young women to move into their husbands' communities upon marriage, and the fact that our survey protocol interviews the mother of the youngest child in the compound or, if she was unavailable, another woman. Aside from relatives, another common relationship is "neighbors", accounting for 35% of non-family relationships.

We categorize a relationship as "close" if the respondent reports talking to their social link at least two to three times per week. These communities are quite close-knit, with another local household being "unknown" only 14% of the time, and 60% of relationships being "close" as defined above. Thus, the average household identifies roughly 4 of the 7 other local households as close contacts. There are very few households who have no close contacts among the local sample households (just 3% of households are "isolated" in this way) or who have only one close contact (10%). Pre-intervention the average household had 1.8 close connections to households that became part of the free WaterGuard treatment group, or roughly half of all close links (as expected given the randomization), and only 20% of households had no close connections to the eventual free WaterGuard treatment group.²¹

²⁰ In our data, household i 's stated relationship to j and j 's stated relationship to i constitute two "relationship pairs".

²¹ Aspects of network structure could also be relevant determinants of spillover effects but we do not explicitly consider such characteristics in this paper. A related working paper explores the possibility that households discount redundant information received through dense social networks, but fails to find robust patterns using our data and network definitions (see Casaburi 2008).

Figure 1: The Chlorine Dispenser



Notes: The dispenser is installed immediately next to a communal water source and delivers a 3 mL dose of dilute chlorine sufficient to treat 20 liters of water with each turn of the knob. Pictorial instructions are prominently displayed on the housing of the dispenser, which protects the chlorine storage tank inside from the elements. The dispenser tank holds three liters of dilute chlorine solution, enough to treat roughly a month supply for a community of a couple hundred people.

Figure 2: Study timeline
Panel A: Sample 1, 2004-2007

1

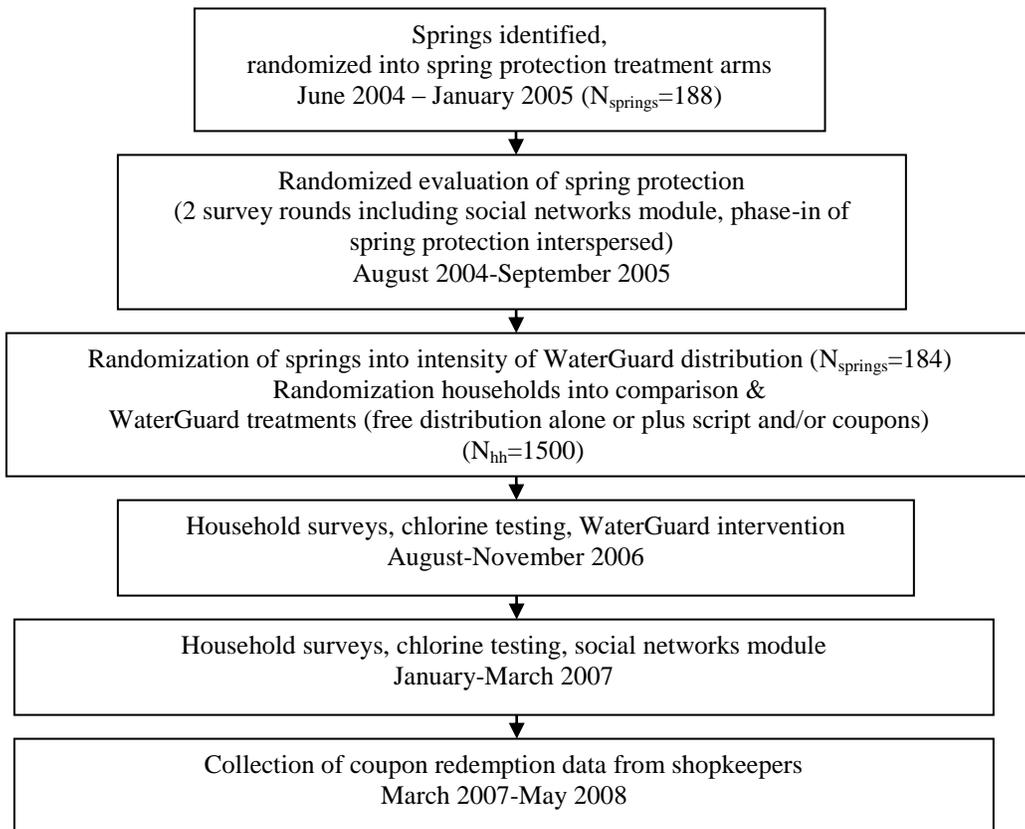


Figure 2: Study timeline
Panel B: Sample 2, 2007-2008

Springs identified, randomized into treatment arms
September 2007 ($N_{\text{springs}}=103$)

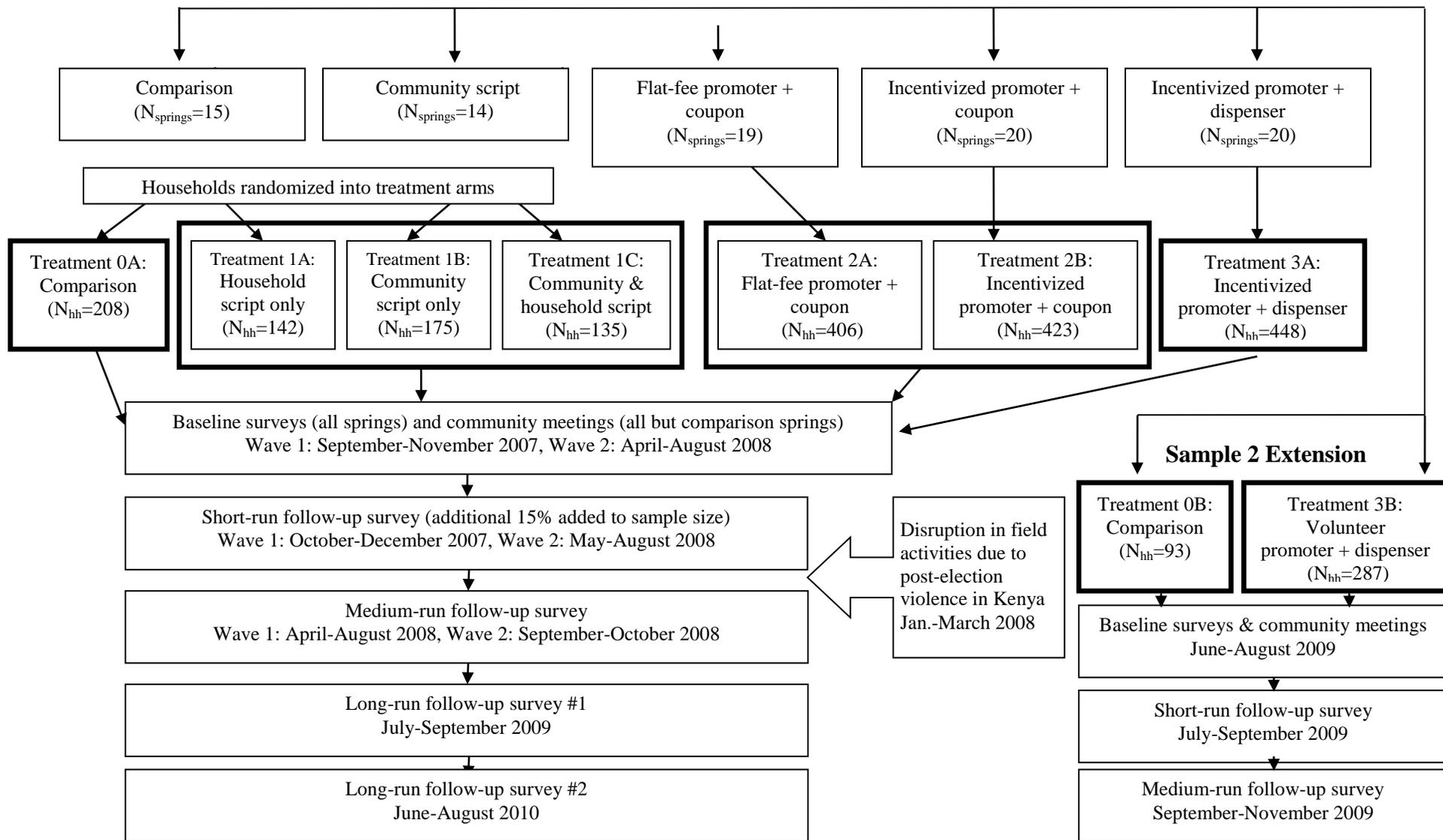
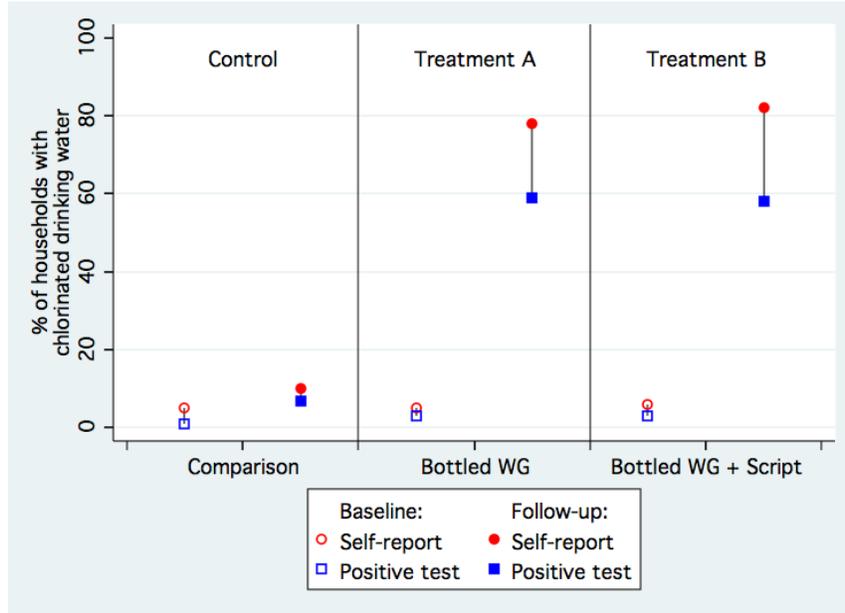
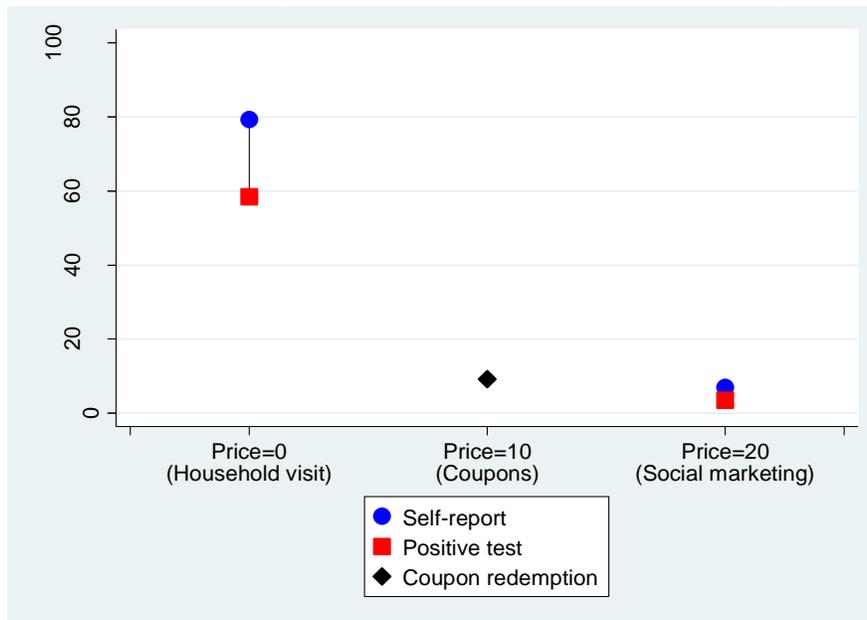


Figure 3: Chlorine Take-up Rates (in Sample 1)
 Panel A: Treatment effects

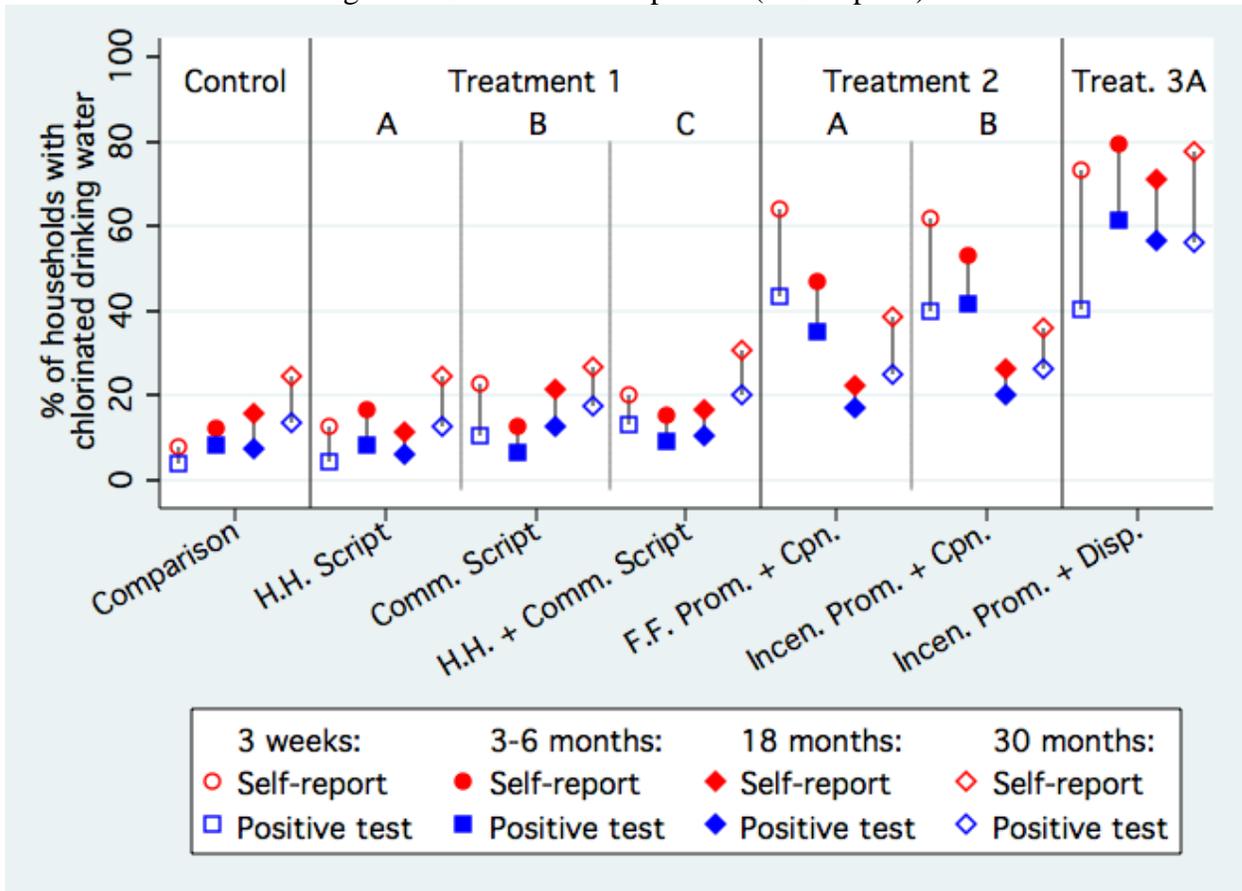


Panel B: Estimating the Demand for WaterGuard (in Sample 1)



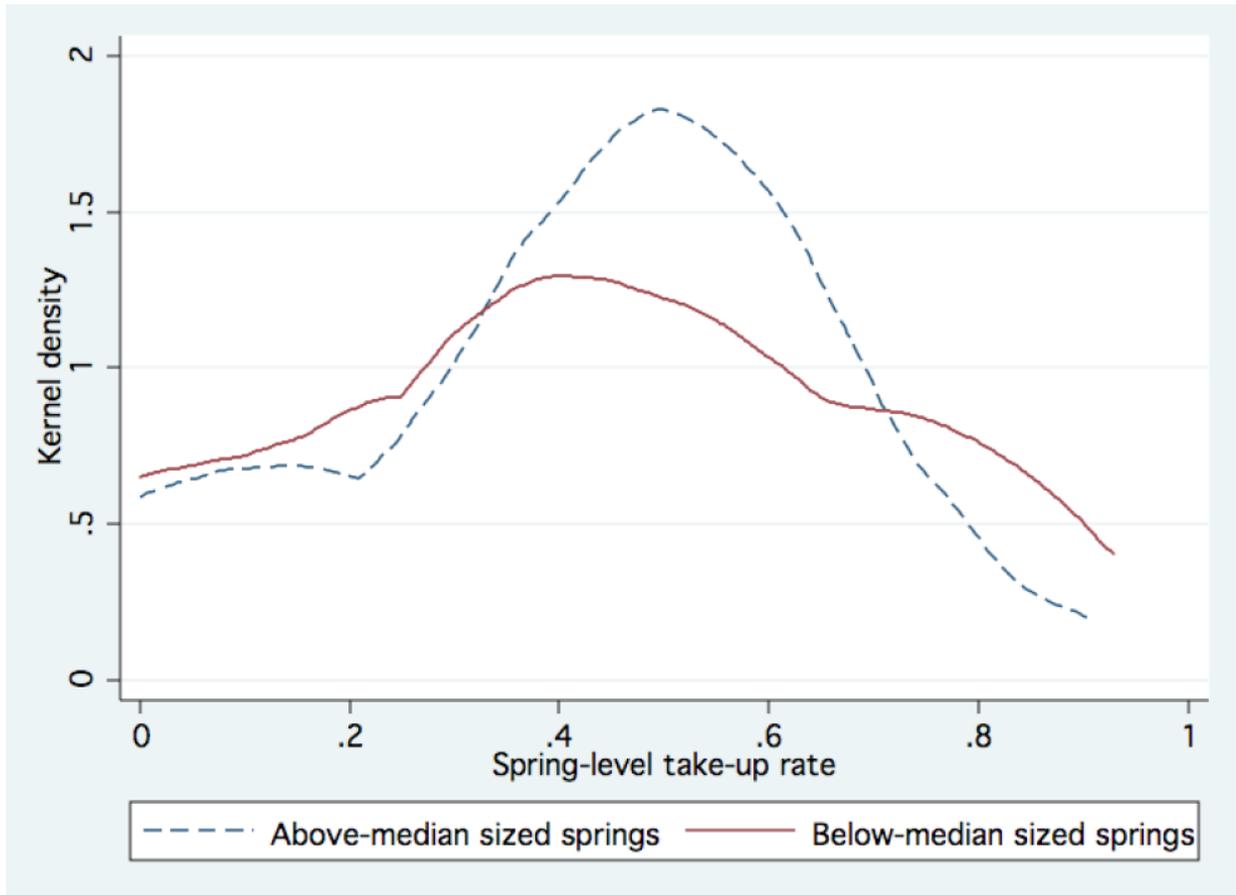
Notes: Bands depicted in graph above are not confidence intervals, but rather reflect an upper and lower bound measure of take-up (self-reported chlorination and positive chlorine tests, respectively). A positive chlorine test result is defined conservatively as sodium hypochlorite of at least 0.1 mg/L with pink color or 0.2 mg/L or greater regardless of color. Data for price = zero are from treatment households in the follow-up survey (n = 628 for self-reports and 627 for test results). Data for price = 10 are from coupons for discounted WaterGuard distributed to subset of treatment households at the time of the intervention (n = 2520; 210 households with 12 coupons each). Coupon redemption data are from shopkeepers' records. Data for price = 20 are from all households prior to the intervention and control households after the intervention (n = 3194 for self-reports and 1942 for test results).

Figure 4: Chlorine Take-up Rates (in Sample 2)



Notes: Bands depicted in graph above are not confidence intervals, but rather reflect an upper and lower bound measure of take-up (self-reported chlorination and positive chlorine tests, respectively). A positive chlorine test result is defined conservatively as sodium hypochlorite of at least 0.1 mg/L with pink color or 0.2 mg/L or greater regardless of color. See section 2.3 for a full description of the treatment arms: 0=Comparison (no intervention), 1A=Household persuasion script, 1B=Community persuasion script, 1C=Both household and community persuasion scripts, 2A=Flat-fee promoter plus one coupon for free WaterGuard per surveyed household, 2B=Incentivized promoter plus one coupon for free WaterGuard per surveyed household, 3=Incentivized promoter plus unlimited supply of free chlorine via a point-of-collection dispenser.

Figure 6: The distribution of spring community-level take-up rates, by community size (in Sample 2 including Treatment Arm T3B Extension)



Notes: Spring-level take-up rate based on positive chlorine test results, defined conservatively as sodium hypochlorite of at least 0.1 mg/L with pink color or 0.2 mg/L or greater regardless of color.

Table 1: Baseline descriptive statistics (Sample 1)

	Household received free WaterGuard		Control		WaterGuard – Control
	Mean (s.d.) (1)	Obs. (2)	Mean (s.d.) (3)	Obs. (4)	(s.e) (5)
Panel A: Sample 1 data					
Respondent years of education	5.66 (3.62)	667	5.71 (3.61)	663	-0.06 (0.20)
Children under age 3 in the compound	1.43 (1.39)	670	1.41 (1.28)	664	0.02 (0.08)
Iron roof indicator	0.70 (0.46)	648	0.70 (0.46)	640	0.00 (0.03)
Household has soap in the home	0.92 (0.27)	669	0.89 (0.31)	663	0.03 (0.02)*
Household has a pit latrine	0.86 (0.35)	669	0.87 (0.34)	662	-0.01 (0.02)
Yesterday's drinking water was boiled indicator	0.25 (0.43)	668	0.29 (0.45)	656	-0.04 (0.03)
Respondent diarrhea prevention knowledge score	3.06 (2.09)	670	3.22 (2.25)	664	-0.17 (0.13)
Respondent said “dirty water” causes diarrhea	0.68 (0.47)	670	0.68 (0.47)	664	0.00 (0.03)
Water at home treated with WaterGuard, self-report ^(b)	0.08 (0.27)	610	0.07 (0.25)	610	0.01 (0.02)
Respondent had ever used WaterGuard ^(b)	0.30 (0.02)	614	0.27 (0.02)	608	0.03 (0.03)
Respondent had heard of WaterGuard ^(b)	0.73 (0.44)	614	0.73 (0.44)	610	0.00 (0.03)

Notes: In the final column, Huber-White robust standard errors are presented (clustered at the spring level), significantly different than zero at * 90% ** 95% *** 99% confidence.

Household data are from the 2004 survey except for variables related to WaterGuard, which are from the 2005 survey. Household survey respondent is the mother of the youngest child in the compound (or the youngest adult woman available).

Table 1: Baseline descriptive statistics (Sample 2) – continued

	Treatment Arm:						
	Control	1A Household Script Only	1B Community Script Only	1C Household + Community Script	2A Flat Fee Promoter + Coupon	2B Incentivize d Promoter + Coupon	3A Incentivized Promoter + Dispenser
Mean, (s.d.)	Treatment Arm – Control (s.e.)						
	[N=137]	[N =118]	[N =115]	[N =120]	[N =341]	[N =337]	[N =334]
Panel B: Sample 2 data	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Respondent years of education	5.30 (3.41)	-0.020 (0.356)	0.109 (0.441)	0.126 (0.420)	0.527 (0.396)	0.243 (0.381)	0.361 (0.390)
Children under age 5 in the compound	1.95 (1.61)	-0.008 (0.205)	-0.036 (0.160)	0.076 (0.180)	0.069 (0.148)	-0.021 (0.164)	-0.200 (0.140)
Iron roof indicator	0.438 (0.498)	0.045 (0.052)	0.151 (0.081)*	0.104 (0.078)	0.041 (0.059)	0.098 (0.066)	0.109 (0.058)*
Household has a pit latrine	0.832 (0.375)	0.015 (0.049)	0.044 (0.056)	0.008 (0.051)	0.032 (0.047)	-0.008 (0.048)	0.045 (0.045)
Water at home treated with any chlorine, self-report	0.073 (0.261)	0.004 (0.025)	0.051 (0.035)	0.071 (0.043)*	0.045 (0.036)	0.011 (0.030)	-0.004 (0.028)
Water at home treated with WaterGuard, self-report	0.058 (0.235)	0.001 (0.020)	0.039 (0.032)	0.042 (0.034)	0.033 (0.031)	0.001 (0.029)	0.002 (0.028)
Respondent had ever used WaterGuard	0.292 (0.456)	0.055 (0.040)	0.097 (0.068)	0.078 (0.065)	0.093 (0.054)*	0.021 (0.051)	0.049 (0.049)
Respondent had heard of WaterGuard	0.891 (0.313)	-0.018 (0.023)	0.039 (0.030)	0.000 (0.036)	0.009 (0.031)	-0.004 (0.031)	-0.001 (0.031)

Notes: In the treatment arm columns, Huber-White robust standard errors are presented (clustered at the spring level), significantly different than zero at * 90% ** 95% *** 99% confidence. Data are from the baseline survey (Sept-Nov 2007 for wave 1 and April-July 2008 for wave 2). Household survey respondent is the mother of the youngest child in the compound (or the youngest adult woman available).

Table 2: Chlorine Take-up Impacts (in Sample 1)

Dependent variable:	Positive chlorine test	
	(1)	(2)
Household received free WaterGuard indicator	0.52 (0.03)***	0.52 (0.04)***
Household received extended social marketing script indicator		-0.02 (0.06)
Household received additional coupons indicator		0.04 (0.06)
Household received extended social marketing script * Household received additional coupons		-0.01 (0.11)
R ²	0.52	0.40
Observations (spring clusters)	2563 (184)	2563 (184)
Number of households	1406	1406
Mean (s.d.) of the dependent variable prior to the intervention	0.02 (0.14)	0.02 (0.14)

Notes: Estimated using OLS, Huber-White robust standard errors (clustered at the spring level) are presented, significantly different than zero at * 90% ** 95% *** 99% confidence. All specifications include household fixed effects, and use data from survey rounds 3 and 4 (for sample 1). A positive chlorine test result is defined conservatively as sodium hypochlorite of at least 0.1 mg/L with pink color or 0.2 mg/L or greater regardless of color. Survey round and month fixed effects included in all regressions but not reported.

Table 3: Chlorine Take-up Impacts, Across Survey Rounds (in Sample 2)

	Short-run survey (3 weeks) (1)	Medium-run survey (3-6 months) (2)	Long-run survey #1 (16-24 months) (3)	Long-run survey #2 (28-36 months) (4)
Panel A	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)
T0: Control	0.041 (0.198)	0.085 (0.280)	0.075 (0.264)	0.138 (0.346)
Panel B	T – C (s.e.)	T – C (s.e.)	T – C (s.e.)	T – C (s.e.)
T1A: Household script only	0.007 (0.020)	-0.003 (0.037)	-0.014 (0.028)	-0.008 (0.027)
T1B: Community script only	0.065 (0.031)**	-0.020 (0.030)	0.052 (0.053)	0.038 (0.051)
T1C: Household + community scripts	0.094 (0.040)**	0.006 (0.040)	0.031 (0.041)	0.064 (0.055)
Panel C				
T2A: Flat-fee promoter + coupon	0.398 (0.050)***	0.268 (0.059)***	0.097 (0.037)***	0.113 (0.053)**
T2B: Incentivized promoter + coupon	0.369 (0.041)***	0.331 (0.047)***	0.127 (0.044)***	0.125 (0.059)**
T3A: Incentivized promoter + dispenser	0.369 (0.038)***	0.528 (0.052)***	0.490 (0.046)***	0.424 (0.054)***
Panel D				
T3B: Voluntary promoter + dispenser	0.301 (0.050)***	0.333 (0.074)***		

Notes: In Panels B, C and D, treatment effects (Treatment Group – Control Group) are shown and Huber-White robust standard errors are presented in parentheses (clustered at the spring level), significantly different than zero at * 90% ** 95% *** 99% confidence. Months to follow-up survey rounds are measured since the baseline visit. T0-T1C received no subsidies on chlorine; households in T2A & B received one coupon for a free bottle of chlorine at their local shop; households in T3A & B had access to an unlimited supply of free chlorine at the dispenser for the duration of the study. A positive chlorine test result is defined conservatively as sodium hypochlorite of at least 0.1 mg/L with pink color, or 0.2 mg/L and greater regardless of color. Panel D compares Treatment 3B (Voluntary promoter plus dispenser) to its contemporaneous control group (control group means not shown).

Table 4: Estimating Social Effects in Chlorine Use with Network Data (in Sample 1)
 Panel A: Changes in conversation patterns following WaterGuard distribution

Dependent variable:	Respondent household reported ever having a conversation with each other survey household in their community about:	
	WaterGuard (1)	Children's health (2)
Free WaterGuard indicator for respondent household in pair	0.20 (0.03)***	0.06 (0.03)**
Free WaterGuard indicator for non-respondent household in pair	0.13 (0.02)***	0.03 (0.02)
Interaction of respondent and non-respondent households' Free WaterGuard indicators	-0.02 (0.03)	0.02 (0.03)
R^2	0.06	0.02
Household pair observations (spring clusters)	6557 (183)	6531 (183)
Mean (s.d.) of the dependent variable in survey round 2	0.10 (0.30)	0.51 (0.50)

Notes: Estimated using OLS. Huber-White robust standard errors (clustered at the spring level) are presented, significantly different than zero at * 90% ** 95% *** 99% confidence. Data on the dependent variable are from the fourth survey round, but include a control for whether or not the respondent reported ever having a conversation on the given topic with the household in question during the second survey round.

Table 4: Estimating Social Effects in Chlorine Use with Network Data (in Sample 1) - continued
 Panel B: Social networks and WaterGuard Take-up

Dependent variable:	Positive chlorine test			
	(1)	(2)	(3)	(4)
Household received free WaterGuard indicator	0.53 (0.04) ***	0.53 (0.06) ***	0.52 (0.03) ***	0.52 (0.03) ***
High-intensity treatment indicator	0.03 (0.03)	0.01 (0.04)	0.00 (0.04)	0.00 (0.04)
Household received free WaterGuard * High-intensity treatment indicator	-0.03 (0.06)			
Interaction with post-intervention indicator:				
Proportion of close contacts who received free WaterGuard ^(a)		0.04 (0.06)	-0.05 (0.07)	-0.02 (0.06)
Proportion of close contacts who received free WaterGuard * Household received free WaterGuard		-0.03 (0.09)		
Proportion of close contacts <i>to same tribe</i> who received free WaterGuard			0.10 (0.06) *	
Proportion of close contacts <i>to community leaders</i> who received free WaterGuard ^(b)				0.07 (0.04) *
R ²	0.52	0.52	0.57	0.57
Observations (spring clusters)	2563 (184)	2230 (184)	3443 (184)	3443 (184)
Number of households	1406	1216	1223	1223
Percentage of relationship pairs of given type	N.A.	60%	21%	36%
Mean (s.d.) of the dependent variable prior to the intervention	0.02 (0.14)	0.02 (0.14)	0.02 (0.14)	0.02 (0.14)

Notes: Estimated using OLS, Huber-White robust standard errors (clustered at the spring level) are presented, significantly different than zero at * 90% ** 95% *** 99% confidence. All specifications include household fixed effects, and use data from survey rounds 3 and 4 (for sample 1). A positive chlorine test result is defined conservatively as sodium hypochlorite of at least 0.1 mg/L with pink color or 0.2 mg/L or greater regardless of color. At “high-intensity” treatment springs 6 of 8 households were assigned to the treatment group whereas only 2 of 8 households were assigned to treatment at the remaining “low-intensity” treatment springs. Additional control variables in all columns include survey round & month fixed effects and the interactions of the post-intervention indicator with baseline total number of close contacts and baseline number of close contacts of a particular type. Columns 2-4 also include indicator variables for zero close contacts and zero contacts of a particular type interacted with the post-intervention indicator.

(a): Close contacts are defined as households with whom the respondent reports talking 2-3 times per week or more.

(b): Includes self-identified leaders of women’s groups, farmer/agricultural groups, water group/well committee, credit/savings/insurance groups, prayer or bible study groups, burial committees, and school committees or clubs.

Table 5: Estimating Social Effects in Chlorine Use with Excess Variance Tests (in Sample 2)

Panel A: IV Results						
Dependent variable:	(1)	(2)	(3)	(4)		
	Between-group variance	Between-group variance	Between-group variance	Between-group variance	Between-group variance	
Within-group variance	5.47 ^{***} (2.02)	2.12 (2.11)	1.57 (2.10)	4.63 ^{**} (1.96)		
Within-group variance*time trend		3.53 (2.56)				
Promoter community indicator				0.016 (0.027)		
Within-group variance*promoter				-1.22 (3.29)		
P-value H ₀ : Within-group variance=1	0.029	0.596	0.780	0.067		
Survey-round FE	Yes	Yes	No	Yes		
Observations	110	110	39	149		
Panel B: First Stage						
Dependent Variable:	(1)	(2A)	(2B)	(3)	(4A)	(4B)
	Within-group variance	Within-group variance	Within-group variance *time trend	Within-group variance	Within-group variance	Within-group variance *promoter
Spring size	-0.204 ^{***} (0.031)	-0.219 ^{***} (0.051)	0.010 (0.033)	-0.315 ^{***} (0.062)	-0.204 ^{***} (0.031)	-0.267 ^{***} (0.062)
Spring size*time trend		0.015 (0.031)	-0.198 ^{***} (0.058)			
Promoter community indicator					0.003 (0.002)	0.018 ^{***} (0.002)
Spring size*promoter					-0.111 (0.069)	-0.315 ^{***} (0.062)
F-statistic	71.4	35.7	25.9	56.8	66.4	109
Survey-round FE	Yes	Yes	Yes	No	Yes	Yes
Observations	110	110	110	39	149	149

(continued on next page)

Table 5: Estimating Social Effects in Chlorine Use with Excess Variance Tests (in Sample 2) - continued

Panel C: Reduced Form	(1)	(2)	(3)	(4)
Dependent variable:	Between-group variance	Between-group variance	Between-group variance	Between-group variance
Spring size	-1.11 ^{***} (0.327)	-0.430 (0.394)	-0.494 (0.640)	-0.943 ^{***} (0.325)
Spring size*time trend		-0.669 ^{**} (0.319)		
Promoter community indicator				0.009 (0.027)
Spring size*promoter				-0.131 (0.795)
Survey-round FE	Yes	Yes	No	Yes
Observations	110	110	39	149

Notes: Robust standard errors in parentheses. ***, **, and * indicate statistical significance at 99%, 95%, and 90% confidence, respectively. Column 1 is estimated using the pooled Sample 2 and Extension Treatments 3A and 3B (Incentivized and Volunteer promoters plus dispensers, respectively) across all follow-up survey rounds. Column 2 is estimated using the same sample, but excluding the medium-run follow-up of the wave-1 Treatment 3A (Incentivized promoter plus dispenser) which may have been affected by election violence. Column 3 is estimated using the same sample as Column 1, but including a time trend, which takes on values 0, 1, 2, 3 for each of the follow-up rounds respectively. Column 4 is estimated using the short-run follow-up data of Sample 2 Treatments 2A & 2B (Flat-fee and Incentivized promoters plus coupons, respectively), which had free bottled WaterGuard at the time of the short-run survey. Column 5 is estimated using a pooled sample of observations used in the estimation of Column 3 and 4.

Table 6: Testing whether price serves as a screening mechanism in chlorine purchase and use, Sample 1

	<u>Price=0</u> Free bottles + household visit		<u>Price=10</u> Shop coupons	<u>Price=20</u> No intervention; purchased from shops		
	(1) <u>Water treated with Cl</u>		(2)	(3) <u>Water treated with Cl</u>		
	A: Self-reported use	B: Positive test	Share of coupons redeemed	A: Self- reported use	B: Positive test	
Panel A: Children under age 3 in the household?						
	Yes	79%	59%	8.3%	6.6%	3.5%
	No	80%	57%	11.4%	8.1%	3.3%
p-value (equality of means)		0.86	0.56	0.01	0.12	0.86
Number of observations:						
	Yes	421	419	1764	2257	1372
	No	207	208	756	937	570
Panel B: Respondent volunteered “dirty water” as a cause of diarrhea?						
	Yes	81%	61%	10.1%	7.8%	3.9%
	No	75%	53%	7.5%	5.8%	3.0%
p-value (equality of means)		0.07	0.07	0.04	0.04	0.34
Number of observations:						
	Yes	402	400	1548	2091	1241
	No	185	187	828	991	591

Notes: Self-reported chlorine use (A) is available for survey rounds 2-4; chlorine tests (B) were only conducted in survey rounds 3-4. A positive chlorine test result is defined conservatively as sodium hypochlorite of at least 0.1 mg/L with pink color or 0.2 mg/L or greater regardless of color. Data for price=0 are from the follow-up survey for households who received free WaterGuard. Data for price=10 are from coupons for discounted WaterGuard (12 per household) distributed to subset of the households who received free WaterGuard; redemption data were collected from shopkeepers for the duration of the coupon program after the household survey rounds had been completed. Data for price=20 are from all households prior to the distribution of free WaterGuard and comparison households at follow-up.