Trickle down:
Diffusion of chlorine for drinking water treatment in Kenya*

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Abstract: We study the distribution of dilute sodium hypochlorite to treat drinking water in homes in rural Kenya, and estimate how much households value clean water and how adoption is mediated by social networks. Using a randomized evaluation, we find large impacts of receiving a six-month supply of free chlorine on home water quality and child diarrhea rates. At the time of follow-up, 58% of treatment households had detectable chlorine in home water on an unannounced visit and a 69% reduction in water contamination as measured by the fecal indicator bacteria *E. coli*. Child diarrhea rates fell by 35-40%. However, data from a subset of households given coupons for discounted chlorine in local shops indicate that demand for the product is very low at even a nominal cost. We do not find that households with young children, who stand to benefit most from cleaner water, have a higher valuation for it. Exogenous variation in the proportion of study households in a given community who were included in the treatment group allows us explore the effects of exposure to chlorine on adoption. While network effects are moderate in comparison to the take-up impacts of getting free chlorine, a comparison household whose close contacts all received free chlorine is over twice as likely to adopt product at follow-up relative to baseline.

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1. Introduction

This paper explores the determinants of take-up of an in-home water treatment product, dilute sodium hypochlorite disinfectant, marketed in western Kenya as WaterGuard. When used regularly, chlorine has been shown in numerous randomized trials to be an effective means of preventing child diarrhea, which causes 20% of child deaths each year (Bryce *et al.* 2005), when used regularly. We examine the role of price, knowledge, and social networks as determinants of take-up. We also assess how these variables interact with the presence of children in determining use, a question of particular relevance since households with young children gain the most from cleaner water.

WaterGuard, and similar products, have been developed in response to concerns about the deterioration of water quality in homes where water must be stored prior to consumption. During storage and transport from the water source, touching and handling water can lead to contamination with pathogens that cause diarrhea if ingested. The product we study, if used properly, leaves water with only a slight chlorine taste as it kills many of the pathogens in water. It is less time-intensive than boiling and has the added benefit of leaving a chlorine residual in water that can protect against recontamination of treated water in a way that boiling cannot.

Even with perfect compliance, point-of-use water treatment does not block all paths of diarrheal disease transmission in settings with imperfect hygiene. The classic epidemiological models of diarrhea emphasize four "F"s: food, fingers, flies, feces. Drinking microbacteriologically safe water tackles one means by which fecal matter is ingested and thus only one source of diarrhea (a point emphasized by Esrey (1996) and Curtis, Carincross, and Yonli (2000), among others).

Several recent meta-analyses and a relatively large number of randomized trials (nine as of 2005) indicate a growing consensus that point-of-use (POU) technologies, including chlorination, are an effective means of reducing diarrhea among young children (Arnold and Colford 2007, Clasen *et al*. 2006, Fewtrell *et al*. 2005). POU water treatment methods are particularly appealing in situations where recontamination of water in transport and storage may vitiate many of the benefits of improved source water quality (Wright *et al*. 2004), as is the case in our study area, where water is commonly collected by children, stored in open containers, and retrieved with the use of a dipper which can serve as a vehicle for germs from hands to the water. Using a randomized approach, Crump *et al.* (2005) estimate that the effect of POU water treatment in western Kenya is a 17% reduction in diarrheal prevalence for children under age two. As discussed below, our results are broadly consistent with these findings.

Efforts to market the product and induce widespread take-up however have met with only mixed success, even in regions where diarrhea is a major cause of child morbidity and mortality (Mintz et al. 2001). In our study region, while 87% of households have heard of WaterGuard and 72% volunteer that drinking "dirty" water is a cause of diarrhea, only 5% report that their main supply of drinking water was chlorinated prior to the intervention. Given the low cost of producing the product it seems clear that determining a way to induce high and sustained take-up of WaterGuard in this region and similar environments could be beneficial.

Ashraf, Berry, and Shapiro (2006) have also recently examined the determinants of chlorine take-up, with a particular focus on the role of positive prices as a means of screening out non-users during mass distribution. As discussed in more detail later, we find no evidence that household valuation of the product is higher among households who stand to benefit most from using it. While the large majority of households who received the product for free were willing to use it, even a small cost deters almost all households from purchasing the product for themselves in our sample, in contrast to Ashraf *et al*.

In our study, a six-month supply of WaterGuard was provided to a sample of households in communities defined by access to springs for drinking water using a randomized evaluation approach. At the outset of the larger project of which this study is one part, all local springs were "unprotected". Protection, which seals off the eye of a spring so that water flows through a pipe rather than seeping and pooling on the ground, was then phased in to springs over time in a random order. At the time of this study, one half of the households in the WaterGuard treatment and comparison groups resided near protected springs and the remaining half of households resided near unprotected springs. This cross-cutting design allows us to estimate not only the determinants of WaterGuard take-up but also any interactions between a source water quality improvement (spring protection) and the point-of-use product (WaterGuard) in terms of both take-up and child health.

We find that the provision of WaterGuard results in a 79% self-reported take-up rate at the follow-up survey two to seven months later. Due to concerns with reporting bias, we also directly tested home drinking water for the presence of chlorine and were able to verify usage at 58% of treatment households using a conservative cut-off that likely resulted in some false negatives (since chlorine dissipates during storage). These take-up rates compare favorably to those seen in other studies of this product where follow-up visits are more frequent and closer to the baseline.[1] In our study, take-up is not affected by sanitation coverage, hygiene knowledge, or access to an improved spring with safer water.

The provision of WaterGuard resulted in microbiologically safer drinking water, with treatment households showing 69% less *E. Coli*, a standard indicator bacteria indicating the presence of fecal matter, in their drinking water at follow-up. This is a greater reduction in contamination of water in the home than is achieved by spring protection, the source water quality improvement that is also studied in the larger project. Kremer, Leino, Miguel, and Zwane (2008) find that spring protection is moderately effective at improving household water quality, reducing contamination by 23%. While households that use protected springs have somewhat cleaner home drinking water, spring protection does not reduce WaterGuard's estimated water quality treatment effect. Our findings are also consistent with existing results on child health. In our preferred specification,

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[1] In another study from western Kenya in which households were visited every two weeks for one year, Crump *et al.* (2005) report verified take-up rates for WaterGuard (as measured by the presence of chlorine in drinking water) of between 85% (on regular scheduled visits) and 61% (on unannounced visits).

diarrhea among young children in treatment households falls by 7-8 percentage points, or 35-40% on a base diarrhea prevalence of approximately 20 percent.

The second part of the paper contributes to the economics literature on the role of social networks in the technology adoption decision, building on the approach in Kremer and Miguel (2007). We study whether households with social links to those households that were randomly assigned to receive the product are themselves more likely to use it, and investigate which sorts of links boost take-up most. We find that links to treatment households do lead to somewhat higher take-up, and also present suggestive evidence that members of the same ethnic group (tribe) and community leaders are particularly influential. While network effects are moderate in comparison to the take-up impacts of getting free chlorine, a comparison household whose close contacts all received free chlorine is over twice as likely to adopt product at follow-up relative to baseline.

Section 2 of this paper describes the intervention and data, and presents summary statistics. Section 3 discusses the proportion of treatment households who adopt the technology, and the impacts of receiving WaterGuard on water quality and child health. Section 4 provides evidence on willingness to pay and section 5 presents the social network results. Section 6 concludes.

2. Rural Water Project (RWP) overview and data

This section describes the intervention, randomization into treatment groups, and data collection.

2. 1 Project overview

The current study is one component of a larger project, called the Rural Water Project (RWP), which examines source water quality improvements and water quantity-related interventions, and which may provide guidance on priorities in the rural water sector. The sample of households analyzed in this paper were first identified for another aspect of the RWP, which was concerned with estimating the impacts of source water quality interventions. Thus, we describe briefly the larger RWP data collection procedures.

The area of Kenya in which our study site is located is poor – the daily agricultural wage ranges from US$1 - US$2 per day depending on the task – and few households have access to improved water services. Naturally occurring springs are an important source of drinking water in rural western Kenya, where the region's topography frequently allows ground water to come to the surface. Our respondents report that springs are the main source of water in this area: over 80% of all water collection trips are to springs (either unprotected or protected). The next most common source are shallow wells (at 6%), followed by smaller numbers of water collection trips to boreholes (5%), rivers/streams (2%), lakes, ponds, and other sources. The microbiological quality of the water at these sources varies, but protected springs are among the cleanest sources on average. Households using any of these sources store water in their homes prior to consumption and during this period the water may be come further contaminated as a result of touching it or otherwise introducing pathogens.

In practice, few households take steps to actively manage water quality. Solutions available in this region for the problem of poor water quality include boiling water prior to consumption and treatment of the water with dilute sodium hypochlorite, marketed in the region as WaterGuard. Boiling water is practiced by a quarter of the households in our sample, and self-reported WaterGuard use is about 5% prior to the intervention, with 2% having verifiable chlorine in their drinking water. Moreover, many households do not take steps to limit the consumption of untreated water by children: half of all households with children under age two report that these young children drink unboiled water (in addition to milk or tea, for example).

As mentioned above, the sample of households that we study in this paper were drawn from a representative sample of households that regularly use springs that were a focus of another element of the RWP (see Figure 1 for details). Springs for the RWP were selected from the universe of local unprotected springs by a non-governmental development organization (NGO), International Child Support (ICS). The NGO first obtained Kenya Ministry of Water and Irrigation lists of all local unprotected springs in the Busia and Butere-Mumias districts. NGO field and technical staff then visited each site to determine which springs were suitable for protection.[2]

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 total number of household spring users varied widely, 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 and the other RWP studies.[3]

The NGO planned for the source water quality improvement intervention to be phased in over four years due to their financial and administrative constraints. Following the protection of 93 springs, the cross-cutting WaterGuard provision exercise was undertaken, with households randomized to treatment and comparison groups after being stratified by spring protection status and geographic location.

The data used in this paper include three rounds of survey data collected prior to the provision of WaterGuard and one round of follow-up data. The first round of data was collected from late August 2004 through February 2005. Water quality in household drinking water containers and

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[2] Springs known to be seasonally dry in months when the water table is low were eliminated, as were sites with upstream sources of contamination (e.g., latrines, graves). From the remaining 562 suitable springs, 200 were randomly selected (using a computer random number generator) to receive protection.

[3] The spring user list is quite representative of all households living near sample springs. In a February 2007 census of all households living within roughly a 10 minute walk of seven sample springs, we found that 92% of these nearby households were included on the original spring users lists. Spring user list households are less representative, however, for households living more than 10 minutes away from sample springs.

at springs was tested in local labs as part of each round of data collection, and household data on demographic characteristics, health, anthropometrics, and water use choices were collected, as described further below. 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, including the provision of WaterGuard, were conducted by wave.

A second round of water quality testing at the spring and in homes, spring environment surveys, and household surveys was completed several months after the first round of spring protection (late April-early September 2005). Further spring protection was performed in August-November 2005, and a third survey was administered one year later (August-November 2006). At this time WaterGuard was provided to those households randomly selected to receive the product. The third follow-up survey round took place five months later, from January to March 2007. In total there are 1273 households with at least one observation prior to WaterGuard provision and follow-up data from the final survey round and this is the main analysis sample.

2.2 WaterGuard provision

As described above, the households that were randomly selected to receive WaterGuard were a cross-cutting sample of households that were stratified by spring protection status and location. Households selected to receive the product were given seven 150 mL bottles of the product, an improved drinking water storage container with a tap, and detailed instructions on how to use the product. The provision was designed as a "directed conversation" so that households were invited to discuss the possibilities for water contamination and strategies to prevent this prior to being told how to use WaterGuard. The key elements of this explanation were dosing procedures and the need to wait 30 minutes following treatment before drinking chlorinated water.[4]

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[4] We also experimented with randomly assigning a sub-group of the treatment households to receive an additional message beyond the basic instructions, focusing on taste and the particular benefits of WaterGuard for children. However, we find no differential effect of this additional message on take-up and do not focus on it in the analysis.

The intervention induced exogenous variation in the number of social contacts between a given household and members of the treatment group by varying the "intensity" of the intervention across springs. At half of the springs, two of eight sample households were randomly chosen for treatment (the "low-intensity" treatment), and at remaining springs six of eight sample households were chosen for treatment ("high-intensity" treatment).

2.3 Data collection procedures

WaterGuard use

During the second through fourth survey rounds, all households were asked whether the water in their primary drinking water storage container was treated with WaterGuard or any other chlorine products, the basis for our measure of self-reported take-up. In addition, during the third and fourth survey rounds, 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.[5] The procedure is equivalent to USEPA Standard Method 4500-CL G for drinking water.[6] 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-

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[5] 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 and we are more likely to detect if a household has treated their water using total chlorine due to the broader nature of the test. Our data on E. coli levels, as described in the next subsection, allows us to assess whether or not the water is microbiologically safe.

[6] 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.

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.[7] Other studies have also noted similar problems with measurement of chlorine in such circumstances (Ogutu *et al*. 2001 and Lantagne forthcoming). 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.

In the analysis, we discuss results in terms of both self-reported take-up (which is likely an upper-bound to the extent that there is courtesy bias in reporting) and verified take-up (which is likely a lower-bound to the extent that we are not necessarily able to detect the chlorine depending on the time since treatment). Water quality data from households who report treating their water but have residual chlorine levels below our cut-off for verification lead us to favor the self-reported measure of take-up, given the significantly higher contamination levels among households who did not treat their water. Moreover, responses to open-ended questions about the process of treating the water indicate that households who used the WaterGuard did so appropriately by treating the water

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[7] For these experiments we used narrow-necked pots with lids & spigots, which reduces the amount of airflow that the water is exposed to, relative to the common alternative of a wide-necked uncovered pot which is used in combination with a dipper. In contrast, water from the same treatment batch that was instead left in the jerry can after treatment retained residual chlorine for up to 48 hours.

prior to pouring it into a clay pot for storage, waiting the recommended 30 minutes before consuming treated water, and not mixing untreated water with the treated supply.

Water quality

Water samples were collected in sterile bottles by field staff trained in aseptic sampling techniques.[8] Samples were then packed in coolers with ice and transported to water testing laboratories for same day analysis. The labs use Colilert, a method which provides an easy-to-use, error-resistant test for *E. coli*, an indicator bacteria present in fecal matter.[9, 10] A continuous quantitative measure of fecal contamination is available after 18-24 hours of incubation. Quality control procedures used to ensure the validity of the water testing procedures included periodic positive and negative controls, and duplicate samples (blind to the analyst), as well as monthly inter-laboratory controls. As discussed below, there appears to be mean reversion over time in water contamination, consistent with both some degree of measurement error and natural intertemporal variation.[11, 12]

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[8] At springs, the protocol is as follows: the cap of a 250 ml bottle is removed aseptically. Samples are taken from the middle of standing water and the sterile bottle is dragged through the water so the sample is taken from several locations at unprotected springs, while bottles are filled from the water outflow pipe at protected springs. About one inch of space is left at the top of full bottles. The cap is replaced aseptically. In homes, following informed consent procedures, respondents are asked to bring a sample from their main drinking water storage container (usually a ceramic pot). The water is poured into a sterile 250 ml bottle using a household's own dipper (often a plastic cup). During the follow-up survey round, when it was expected that a large fraction of samples would contain chlorine, the sample bottles were coated on the inside with a 3% solution of sodium thiosulfate, a reducing agent that neutralizes any residual chlorine in the sample, and prevents continued bactericidal action during transit of the sample from the field to the lab for analysis

[9] Our lab procedures were adapted from Environmental Protection Agency Colilert Quantitray 2000 Standard Operating Procedures.

[10] It is common to use *E. coli* as a means of quantifying microbacteriological water contamination in semi-arid regions like our study site. The bacteria *E. coli* is not itself necessarily a pathogen, but testing for specific pathogens is costly and can be difficult. Dose-response functions for *E. coli* have been estimated for gastroenteritis following swimming in fresh water (Kay *et al.* 1994), but such functions are location-specific because fecal matter pathogens vary over space and time. In a district near our study site, a U.S. Centers for Disease Control project finds that the most common bacterial pathogens are Shigella and non-typhoidal Salmonella.

[11] There are several potential sources of measurement error. First, Colilert generates a "most probable number" of *E. coli* coliform forming units per 100 ml in a given sample, with an estimated 95% confidence interval. Second, samples that are held for more than six hours prior to incubation may be vulnerable to some bacterial re-growth/death, making tested samples less representative of the original source. Third, sampling variation is an issue given the small size of the collection bottle (at 250 ml).

[12] In practice, a substantial fraction of water samples were held for longer than six hours, the recommended holding time limit of the U.S. EPA, but we have confirmed that baseline water quality measures are balanced across treatment and comparison groups when attention is restricted to those water samples that were incubated within six

Household survey data

The target household survey respondent was the mother of the youngest child living in the home compound (where extended families often reside together), or another woman with child care responsibilities if the mother of the youngest child was unavailable. The respondent was asked about the health of all children under age five living in the compound, including recent diarrhea episodes.

The household survey also gathered baseline information about hygiene behaviors and latrine use, as well as the frequency of water boiling, home water chlorination and water collection choices. Respondents were asked to give their opinion on methods to prevent diarrhea; they were not given options to choose from, but were prompted three times and their responses recorded. This information was used to construct a baseline "diarrhea prevention knowledge score", namely, the number of correct responses provided.[13] Respondents volunteered three correct preventative activities on average. There is moderate knowledge of water's role: 72% of respondents named avoiding contaminated water (or some variant of this answer) as a way to reduce diarrhea.

The definition of diarrhea in the survey is "three or more loose or watery stools in a 24 hour period," which has been used in related studies (see Aziz *et al.* 1990 and Huttly *et al.* 1987). The questionnaire does not attempt to differentiate between acute diarrhea (an episode lasting less than 14 days) and persistent diarrhea (more than 14 days), but identifies dysentery by asking about blood in the stool. Enumerators used a board and tape measure to measure the height of children older than two years of age, and digital scales for weight. The height of children under two was measured as their recumbent length using a measuring board, and a digital infant scale measured their weight.

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hours of collection, yielding the most reliable estimates (results not shown). Extended holding time increases the noise in the *E. coli* estimate, but there is no clear direction of bias as bacteria both grow and die prior to incubation.

[13] 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", "clean dishes/utensils" or "other valid response". We reviewed all responses other than those listed here and categorized them as valid or invalid.

Social network data

In the survey round prior to the WaterGuard intervention, we collected data on each household's relationship to every other sample household living at their spring. Respondents categorized 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.

2.4 Sample Attrition

We successfully interviewed 87% of the baseline household sample in the second survey round, 89% in the third round (when WaterGuard was distributed), and 85% in the final round. We have data from all four survey rounds for 76% of baseline households and for three survey rounds for an additional 14.5% of households in the baseline sample; thus 90% of baseline households were surveyed in at least two of the three follow-ups. Attrition is not significantly related to spring protection assignment or to assignment to the WaterGuard intervention group: the estimated coefficients on the treatment indicators are 0.01 (p-value=0.3) and 0.06 (p-value 0.58), respectively, and these results are robust to including further explanatory variables as controls (not shown).

The baseline characteristics of households lost over time are typically statistically indistinguishable from those that remain in the sample. Better-off households, proxied as those with iron roofs, are not more likely to attrit, nor are households with better baseline household water quality or hygiene knowledge (not shown). Any sample attrition bias appears likely to be small.

2.5 Baseline descriptive statistics

Table 1 presents baseline summary statistics from the first survey round for households (Panel A) and children under age three (Panel B) where the baseline is defined as the survey round in which WaterGuard was distributed. In regression analysis, we will also use data from the earlier survey rounds as well.

The water quality measure, *E. coli* most probable number (MPN) CFU/100 ml, takes on values from 1 to 2419[14]. We categorize water samples with *E. coli* CFU/100 ml ≤ 1 as "high quality" water. For reference, the U.S. EPA and WHO standard for clean drinking water is zero *E. coli* CFU/100 ml, and the EPA standard for swimming/recreational waters is *E. coli* CFU/100 ml < 126 (in geometric mean over at least five tests).[15] To be conservative, we consider water with counts between 1 and 100 "moderate quality" and values above this to be of poor quality. We rarely observe high quality samples in our data, which is not surprising as source water in this setting (e.g., spring water) is neither in a sterile environment nor has residual chlorine (as treated piped water does).

There is no statistically significant difference between baseline water quality at treatment versus comparison households; (Table 1, Panel A), which implies that the randomization created comparable groups. About 14% of samples meet the stringent U.S. EPA drinking water standards, while around a fifth of samples are "poor" quality. The p-value of the Kolmogorov-Smirnov test for equality of distributions for *E. coli* MPN CFU/100 ml is 0.99 for household water so we fail to reject the null hypothesis that the distributions of our water quality measure at baseline are the same for the treatment and comparison households.

Household water quality is somewhat better than spring water quality on average at baseline: the average difference in log *E. coli* is 0.52 (s.d. 2.64; results not shown). This likely occurs for at

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[14] In the laboratory test results, the *E. coli* MPN CFU can take values from <1 to >2419. We ignore censoring and treat values of <1 as equal to one and values of >2419 as 2419. In practice, there are very few censored observations.

[15] The EPA website has details: http://www.epa.gov/waterscience/beaches/local/statrept.pdf (accessed 11/22/2007).

least two reasons. First, many households collect water from sources other than the sample spring: only half of the household sample gets all their drinking water from their local sample spring at baseline, and overall nearly one third of water collection trips are to other sources. [16] About a quarter of households report boiling their drinking water at baseline.[17] However, the correlation between household water contamination and self-reported water boiling is low, raising the possibility of social desirability reporting bias. While very few households have residual chlorine in their drinking water, the majority of households have heard of WaterGuard and during the third survey round 42% of households reporting having used the WaterGuard product at some point with 30% of households reporting chlorinating their water at least once in the last year; these chlorination levels are higher than usually observed because the government distributed free chlorine in part of our study region following a 2005 cholera outbreak. Treatment households were no more likely than comparison households to report receiving free chlorine (not shown), though a significantly higher fraction of treatment households did say that their community had been affected by cholera in the past two years at the time of the intervention. This discrepancy seems to be caused by idiosyncratic reporting error rather than legitimate differences in cholera exposure since treatment and control households do not differ in reports about whether anyone in the spring community had been affected by cholera.

Most other household and child characteristics are similar across the treatment and comparison groups, further evidence that the randomization was successful. Average mother's education is six years, which is less than primary school completion. Water and sanitation access is fairly high compared to many other less developed countries as about 86% of households report

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[16] Springs are often located in close proximity. Springs in the sample have an average of 1.2 (standard deviation 1.3) other springs within 1 km of the spring, 9.2 (standard deviation 5.8) springs within 3 km of the spring, and 26.5 (standard deviation 14.1) springs within 6 km. Of these, 0.4 (standard deviation 0.6) springs within 1 km are protected springs, 2.8 (standard deviation 3.0) springs are treated within 3 km, and 8.2 (standard deviation 7.9) springs within 6km are treated. There are no significant differences at baseline in the total number of nearby springs for treatment and comparison springs.

[17] This is distinct from boiling water to make tea. It would be possible to drink only tea, and thus effectively drink only boiled water, but we do not find evidence of this coping strategy. 70% of households report that their adult members drank unboiled water the day before they were surveyed and, most importantly, young children are commonly given water to drink directly from the household storage container, not exclusively boiled water.

having a latrine, and the average walking distance (one-way) to the closest local water source is approximately 8 minutes. There are similarly no significant differences across the treatment and comparison groups in terms of the diarrhea prevention knowledge score, knowledge of the relationship between water and sickness, or water boiling behavior, though slightly more treatment households had soap at baseline; this difference did not persist in the survey round in which the intervention was conducted.

There are two variables related to child health and household composition that were different across treatment and comparison groups when the intervention was conducted, though these differences did not exist at baseline. In the third survey round, treatment households have fewer children under age twelve living in them (though the age profile does not differ noticeably), and this difference is significant at 95% confidence. However, there is not a significant difference between treatment and comparison households in the number of children under age three, either at baseline or when the intervention was conducted, and this is the group of children we focus on in the analysis of health impacts. Also, young children in the treatment group are more likely to have diarrhea in the past week; 22% of children under age three in the treatment group report diarrhea in the past week, as compared to 18% in the comparison households. This difference is also significant at 95% confidence. If we use all of the data from earlier rounds, and test for equal diarrhea prevalence between treatment and comparison households we see no evidence of such a difference between the groups, which appear statistically identical along this dimension. There are similarly no statistically significant differences in other non-diarrheal illnesses (e.g., fever, cough, vomiting) or in breastfeeding (which is both curative and preventative for diarrhea) across the two groups (results not reported). In the regression results, we control for the pre-existing difference in diarrhea prevalence rates when estimating the effect of the treatment; ignoring the baseline differences and the estimated treatment effect based only on the cross-section of children at follow-up indicates no reduction in diarrhea. This issue is discussed in much greater detail in Section 3.4.

There is also a slight difference, significant at 90% confidence, between treatment and comparison households in the average number of close contacts prior to the intervention. The average number of contacts that other households have to members of the treatment and comparison groups does not differ significantly, however, and this is the measure that is relevant for estimating the effects of contacts to treatment households since we rely on each household's own report of their connections to other households in order to construct our network measures.[18]

3. Point of use water treatment impacts on water quality and child health

This section discusses the estimation strategy and presents the impacts of WaterGuard distribution on household water quality and child health. Both treatment and comparison households were affected by the intervention. In this section we focus on the effect of the treatment on the treated and return to a regression analysis of the impacts of treatment on comparison households, as mediated by social networks, in Section 4. Thus, results presented in this section understate the effect of the treatment to the extent that comparison households were also affected.

3.1 Estimation strategy

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

$$W_{it} = \alpha_t + \delta_i + \beta_1 T_{it} + (T_{it} * X_i)'\beta_2 + \varepsilon_{it} \qquad (1).$$

W_{it} is the water quality or chlorine use measure for household i at time t ($t \in \{0, 1, 2, 3\}$ for the four survey rounds) and T_{it} is a treatment indicator that takes on a value of one after the intervention. The interaction of treatment status with baseline household characteristics such as sanitation access, respondent's diarrhea prevention knowledge and awareness that "dirty water" causes diarrhea, water boiling (the leading point-of-use water treatment strategy in our study area), an iron roof indicator,

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[18] As discussed in more detail in section 5, we include household fixed effects to control for differences in the permanent sociability of treatment and comparison households and focus on network effects derived from the proportion of close contacts among study households who are members of the treatment group. We also allow for the possibility that more social households might be more likely to adopt the product, all else equal.

years of education, and the number of children under age 12 at baseline allow for differential treatment effects as a function of these characteristics, captured in the vector β_2. We also investigate potential complementarities or substitution patterns between the source water quality improvement from spring protection and chlorination. 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 and the local sanitation environment, and may have kinship ties.

Random assignment 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 also included to control for any time-varying factors affecting all households. Estimates of the average treatment effect on the treated (TOT) in a two-stage procedure (Angrist, Imbens, and Rubin 1996) allow us to estimate the impacts of WaterGuard use on water quality, by instrumenting for the presence of chlorine in drinking water with assignment to treatment. The first stage regression in this exercise is of interest in its own right as the take-up rate for POU water treatment technologies is a key policy concern.

3.2 WaterGuard Take-up

At the unannounced follow-up visit, most households (79%) that received 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, 2-7 months following receipt of the product from field staff. This take-up rate compares very favorably to that achieved in other studies such as Crump *et al.*'s (2005) investigation of WaterGuard medical effectiveness in an area near our study site. Factoring in baseline take-up rates and time trends, we estimate the effect of the intervention to be a 69 percentage point increase in self-reported chlorination and a 52 percentage point increase in validated chlorination (Table 2, columns 1 and 4). These are huge effects relative to baseline self-reported and validated chlorination rates of 6% and 2%, respectively. We see no evidence that either measure of take-up is related to pre-intervention source water quality (Table 2, columns 2 and 5) or other household characteristics aside from whether or not the household boiled drinking water prior to the intervention (Table 2, columns 3 and 6). Households which at the time of the intervention had more children, or more sick children in particular, are no more likely to have detectable levels of chlorine in their water at follow-up.

Over 99% of treatment households report using at least some of the WaterGuard provided, and on average treatment households used slightly less than one bottle per month in the period since WaterGuard was distributed. Because the quantity of WaterGuard required for consistent chlorination depends on the number of household members, and whether or not chlorinated water is reserved for drinking only or consumption by children exclusively, it is hard to say exactly how many households report having used an appropriate amount of their free supply. We estimate that roughly half of the treatment households were chlorinating consistently and appropriately based on the number of bottles they report using and the elapsed time between the intervention and follow-up. This is comparable to the 58% of treatment households who have detectable levels of chlorine in the water when tested at follow-up. While our follow-up survey instrument did not explicitly ask treatment households if they had any of the free supply of WaterGuard remaining, it appears that the take-up rate we observe among treatment households is for the free WaterGuard we distributed, and not for purchased WaterGuard. Very few households accounted for the full supply of seven bottles when asked what they had done with them at follow-up, and the presence of chlorine in the water is not significantly higher among households whose follow-up visit occurred less than three months after the intervention or among those with more than two bottles remaining at follow-up (not shown).

We do not have direct evidence on why households that elected not to use WaterGuard made this decision. However, lack of information regarding the health benefits of using WaterGuard does not seem like a plausible cause since prior to the intervention, 94% of households who had heard of WaterGuard were able to volunteer at least one valid health-related benefit of using the product. 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 a typical household in their area would use WaterGuard during a cholera epidemic. Moreover, among respondents who had previously used WaterGuard, only 11% said they thought that the treated water tasted bad, a characteristic of the product that is often cited as a potential impediment to take-up. In fact, 87% of the respondents said that they thought it tasted good, rather than being indifferent to the strong taste, and "sweetening" water was commonly volunteered as a benefit of using WaterGuard. [19]

3.3 Home water quality impacts

As shown in Figure 2, treatment drastically reduced contamination in households' drinking water supplies. While the cumulative density function of log E. Coli was essentially the same for treatment and comparison households at baseline, after the intervention the distribution for treatment households shifted markedly towards lower levels of contamination but the distribution for control households remained essentially unchanged. Notably, while slightly less than 20% of households had no contamination prior to the intervention, this share rose to over 50% among treatment households after the intervention.

The average impact of treatment on household water quality is positive and relatively large as compared to the impacts of alternative interventions. Table 3 shows that treatment resulted in a 1.370

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[19] One other factor that could have influenced take-up rates relates to the improved water storage container that was given to households in the treatment group. Some of the clay pots that were distributed as part of the intervention were poorly manufactured and leaked. Largely as a result of these problems, 30% of households who received pots report not using them. Because households in the treatment group were specifically instructed that the WaterGuard would be most effective when used in the improved containers, which had design features such as a narrow neck, lid, and tap, intended to prevent recontamination of the treated water, 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 reported not using the pot relative to those who did (self-reported 68% versus 89%, p-value < 0.01) and among those who specifically complained about their pot being broken relative to those who did not (self-reported 68% versus 83%, p-value < 0.01). Had this aspect of the intervention not been so problematic, perhaps take-up rates would have been even higher. On the other hand, the functional pots that were distributed may have been effective at reducing recontamination among households that were using the WaterGuard; average contamination among households that did not use the improved water storage containers was significantly higher than among households that did (log E.Coli MPN of 2.18 versus 1.42, respectively, p-value < 0.01).

log point reduction in E. Coli in household water (column 1). For comparison, spring protection to improve source water quality resulted in home water quality gains of less than one-quarter of the effect size of the WaterGuard intervention. Using assignment to treatment as an instrument for the household's endogenous choice to chlorinate their water supply, we estimate that the effect of the intervention on those who actually used the WaterGuard was a reduction in contamination of 1.936 log points (column 4), larger than the average treatment effect as is consistent with the fact that not all treatment households use the product.[20]

As is clear in Figure 2, assignment to treatment also drastically reduced the share of households who had any contamination at all in their water. As a result of the intervention, an additional 37% of households' drinking water samples were considered safe according to the stringent U.S. EPA standard of zero E. Coli (Table 3, column 5). Instrumenting for WaterGuard use with assignment to treatment, the share of households with contaminated water was 53 percentage points lower among those who used the product (column 8). The effect of treatment in terms of average E. Coli reduction was significantly larger for households who had higher levels of contamination at baseline (column 2), though such households were no more likely to cross the threshold of any contamination in their water as a result of the intervention (column 5).

We again find no evidence of differential treatment effects as a function of baseline household sanitation, diarrhea prevention knowledge, or mother's education (columns 3 and 7).[21] Households living in communities with greater latrine coverage do appear to have less contaminated water overall, but this does not differentially impact the WaterGuard effect. The absence of statistically significant differential effects as a function of pre-existing sanitation access or hygiene

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[20] We also include the spring protection indicator and the number of close household contacts to members of the treatment group, as additional instruments, given the social network results described below.

[21] A direct measure of hygiene, respondents' fingertip fecal contamination, however, is related to observed household water quality: every additional finger testing positive is correlated with a 0.11 log increase in contamination (p-value 0.03) in the cross-section of data from the last survey round when the fingertip contamination data was collected. We find no evidence of differential treatment effects for households with fingertip contamination relative to those without.

knowledge runs counter to claims that water quality improvements are much more valuable when these factors are also in place, although the relatively large standard errors on these interaction terms argue for caution in interpretation. Interestingly, while neither the total number of children nor the number of sick children at baseline was related to the likelihood that a household would have detectable chlorine in their water at follow-up (Table 2, columns 3 and 6), it appears households who had sick young children at baseline benefit slightly less from the receipt of free WaterGuard in terms of water quality improvements (Table 3, column 3).

3.4 Child health impacts

We estimate the impact of WaterGuard receipt on child health in equation 2:

$$Y_{ijt} = \alpha_i + \alpha_t + \beta_1 T_{ijt} + X_{ij}'\beta_2 + (T_{ijt} * X_{ij})'\beta_3 + u_{ij} + \varepsilon_{ijt} \qquad (2)$$

where the main dependent variable is diarrhea in the past week. Future versions of this paper will also use anthropometric data collected by household survey enumerators as dependent variables in equation 2. The coefficient estimate, β_1, on the treatment indicator T captures the WaterGuard treatment effect. An advantage of this experimental design over existing studies, beyond the usual benefits of addressing omitted variable bias, is the ability to avoid measurement error in the key water quality explanatory variable (through use of the treatment indicator). We include child fixed effects (α_i), survey round and month fixed effects (α_t). We also explore heterogeneous treatment effects as a function of child and household characteristics, X_{ij}.

As shown in column 3 of Table 4, using the panel data we find a large and statistically significant effect of WaterGuard receipt on child diarrhea, equivalent to a 35-40% reduction in incidence, which is robust to the inclusion of polynomial controls for gender and age (column 4). We find a very significant and large interaction between the WaterGuard intervention and spring protection (column 5), reflecting the fact that the largest reductions in diarrhea were realized by children in treatment group households living at unprotected springs, who were also the subgroup with the highest diarrhea rates prior to the intervention. [22]

Spring protection and WaterGuard use appear to be substitutes in terms of their effectiveness at preventing diarrhea; health benefits of WaterGuard were relevant only at unprotected springs. The household water quality improvements derived from WaterGuard use did not differ by spring protection status at the source (Table 3, column 1), but it may be the case that the reduction in contamination in water from unprotected springs, where the source water is dirtier to start with, is enough to cross some sort of threshold which water from protected springs is already below.

While our results are consistent with the large existing randomized control literature on point of use water treatment, the pre-existing differences in diarrhea rates between treatment and comparison households in our data could lead to some concerns regarding the causal interpretation of the differences-in-differences estimator we use if this study were alone in its attempt to measure the health benefits of point of use water treatment. If we were to ignore the pre-existing differences in diarrhea prevalence and rely only on the randomization in the cross-section of the follow-up survey data, we would conclude that the intervention had no effect on child health. In regressions similar to Table 4, columns 3-5 but without the fixed effects, the coefficient on the WaterGuard treatment indicator is never significantly different than zero, even when tested for joint significance with the protected spring indicator in column 5 (results not shown). However, prior evidence from randomized control trials confirms the causal relationship that we identify. We have conducted an extensive investigation of alternative explanations for the significantly higher diarrhea rates among

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[22] In the appendix (available upon request), we find no evidence of heterogenous treatment effects by gender, nor are interactions with baseline local sanitation (latrine) coverage, diarrhea prevention knowledge, and education significant, in line with the lack of additional water quality gains for these households. Given the large effects the intervention had on both take-up rates and water quality, we can explore the relationship between these endogenous factors and child health outcomes using the treatment indicator as an instrument. We also include the spring protection indicator and the number of close contacts a household has to members of the treatment group, as additional instruments, given the social network results described in the next section of the paper. Self-reported WaterGuard use seems to reduce diarrhea incidence by slightly more than half the rate that would otherwise have occurred and we also find a strong relationship between E. coli and diarrhea, with each log point reduction in contamination resulting in 6 percentage points less diarrhea.

treatment households, particularly those at unprotected springs, and concluded that there is no reason to believe that the difference is due to something other than chance.[23]

3.5 Water source choice and health behaviors

While the availability of WaterGuard could presumably change the treatment households' optimizing choices of where and how much water to collect and how to store it, we see very little evidence of behaviors that are either substitutes for or complements of the WaterGuard technology (appendix available upon request). There might be a slight tendency for treatment households to make fewer trips to collect water (a reduction of roughly 10% of their original number of trips, significant at 90% confidence), but they do not seem to switch their collection patterns in terms of which sources they visit, whether or not they send children to collect water, or whether or not they drink from the spring in their community. We also see no differences in sanitation between the treatment and comparison groups as measured by the presence of soap in the home or the number of the respondents' fingers which had bacterial contamination.

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[23] Logistically, because of the way the survey forms and other documentation was printed, it would have been extremely difficult for an enumerator to interfere with the randomization into treatment; if such problems had existed, they would have been evident in the follow-up data which had different survey modules for treatment and comparison households and were administered by a different enumerator than the one who distributed the WaterGuard. Reporting bias after seeing enumerators carrying around bottles of WaterGuard is a possibility, but treatment households were no more likely to be visited later in the day or by different survey enumerators than comparison households. On average, WaterGuard take-up rates are the same at protected and unprotected springs, though take-up was significantly higher at the springs that were protected between the first and second survey rounds as reported in Table 2; changes in respondent identities between survey rounds are no more likely at treatment than comparison households; and infants are equally likely to be breastfed at treatment and comparison households. Perhaps most importantly, while the randomization into WaterGuard treatment and comparison groups was done at the household level, because of the high- \ low-intensity aspect of the experimental design, for the purposes of comparing child diarrhea rates, we almost have a spring-level randomization after restricting the sample to children under three (at a given spring, we are typically comparing a group of three children to a group of nine, and at some springs either the treatment or comparison households have no children under age three). With these findings in mind, the differences-in-differences estimator remains our preferred specification. A document detailing the findings of this process are available from the authors upon request.

4. Willingness to pay and valuation of WaterGuard

At the time of the intervention, a random third of households in the treatment group were also given 12 coupons, one per month starting two months after they were given the free supply, for a bottle of WaterGuard at half the retail price.[24] The coupons were redeemable at specified shops in the study area near markets that most women would attend at least once a month. Shopkeeper records allow us to track how many coupons households redeemed and when they did so. These data offer important insights into the medium-run effects of distributing free WaterGuard, extending into the period after participating households were no longer visited by survey enumerators.

Of all the coupons that were distributed, only 10% were redeemed. Less than a third of the 227 households who were given coupons redeemed any of them and on average only four coupons were redeemed by households who redeemed at least one. Though households had only had a few months during which to use coupons by the time of the follow-up survey, we did inquire at that point about reasons for not using the coupons. By far the most common response, given by over 80% of households who said they had not yet used a coupon, was that they still had WaterGuard from the free supply remaining. Nonetheless, this indicates that there was not much permanent demand for WaterGuard, since coupons were only valid for one month but the product can be stored for a long time and could have been stockpiled by households with coupons. Also, interestingly, there were almost no cases of self-reported gift-giving of coupons, in contrast to gifts of the free WaterGuard itself, which were quite common. No identification was necessary to redeem a coupon, so in principle they were fungible, though perhaps households did not realize this.[25]

Only a few respondents said that WaterGuard was still too expensive, even with the 50% discount, and a similarly small fraction reported having some sort of difficulty understanding how the

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[24] Even the “retail” price of WaterGuard in Kenya is subsidized by the organization that promotes and distributes it, Population Services International. At the time of our study, one bottle of WaterGuard sold for 20 Kenyan shillings, or about a quarter of the agricultural daily wage or 50% less than the cost of a 300 mL soda.

[25] Unsurprisingly then, social connections to households who were given coupons have no predictive power in the network regressions discussed in the next section (results not shown).

coupons were to be used or actually redeeming them at the shop. A single respondent said that it was too far for her to travel to a shop, so distance does not seem to be a likely constraint on redemption rates, as would be expected given the attempt to partner with shops near weekly market locations.

Nonetheless, as evidenced by the low overall redemption rates mentioned above, there was only a minimal increase in coupon redemptions as supplies of free WaterGuard were used up, and even this seems to be countered by generally decreasing redemption rates as more time elapsed since the coupons were distributed. Around half of the coupon redemptions occurred between 4 and 7 months after they were distributed, roughly at the same point when the follow-up surveys were conducted. It is possible that the follow-up visits served as implicit reminders to households that they could redeem their coupons, but that this reminder faded from their minds over time.

Our results from this aspect of the intervention are in stark contrast to those of Ashraf *et al.* (2007) and Garrett *et al.* (2008), both of whom find much higher willingness to pay for products like WaterGuard. In a door-to-door marketing study of the WaterGuard product, branded as Clorin in Zambia but otherwise identical, around 70% of sampled households chose to purchase when offered a 50% discount off the retail price. While this could partly be an urban-rural difference (the Ashraf *et al.* study was conducted in Zambia's capital), other possible explanations include heightened social desirability bias since the marketer was directly observing household choices in their case, or the convenience of having the product delivered to the home. Nonetheless, not far from our study in rural western Kenya, Garrett *et al.* 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.[26] On the other hand, data for the Garrett *et al.* study was conducted during weekly visits by interviewers who asked about diarrhea prevalence, which may have increased take-up rates.

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

These findings underscore the differences between free and discounted WaterGuard. While our intervention was very successful at promoting take-up in the short-run, even a 50% reduction in price does not seem to be sufficient in order to induce demand for the product among the rural households participating in our study. These results also suggest that the initially low take-up rates may not have been due to a lack of information about the product's effectiveness; even among households who have experience with WaterGuard after the intervention and who have access to the product at a steep discount, few chose to purchase it.

Using the exogenous variation in prices induced by the intervention, we can explore households' valuations of WaterGuard more formally. In Figure 3, 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 follow-up, 10 Ksh for the subset of treatment households who were given coupons during the 12 months they were valid, and 20 Ksh for comparison households and treatment households prior to the intervention. As discussed earlier, demand is quite high at a price of 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.[27] An increase in the price from 10 to 20 Ksh barely affects demand. Using this reduced-form approach, we find that demand among mothers with below-median education is significantly lower at a price of 20 Ksh, though the difference is small in absolute terms (8.4% of mothers with above median education use WaterGuard compared to 5.9% of those with below median education, p-value 0.01).[28] More concerning, however, is the fact that households who had no young children at baseline actually had significantly higher demand at positive prices than

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[27] In the analysis that follows, we treat each coupon as an observation at 10 Ksh. An alternative approach, which would give an upper bound for demand at this price would be to count any household that redeemed at least one coupon as having positive demand. Even with this more generous definition, demand drops sharply at 10 Ksh to 27% of households using WaterGuard.

[28] Perhaps what matters more than general education is specific knowledge that water is a disease vector. Mothers who volunteered that dirty water is a cause of diarrhea at baseline had significantly higher demand for the product.

households who had young children, suggesting that charging a price for the product may not be an effective way of screening for the households who will benefit most.

In Table 5, we estimate the value of WaterGuard using a discrete choice framework.[29] In column 1 we show that the implied average willingness to pay for WaterGuard in this sample is just over 2 Ksh, based on a conditional logit model in which households choose whether or not to use the product on the basis of which option gives them the greatest indirect utility, assuming a type I extreme value distribution for the error terms. Households at unprotected springs do not value the product any more than those at protected springs (column 2), as is consistent with the fact that both groups experienced equal water quality improvements as a result of assignment to treatment (Table 3, column 1). In columns 3 and 4 we confirm the demand results discussed in the previous paragraph. Older children, rather than younger ones who suffer from the most diarrhea incidents, significantly increase valuation of WaterGuard. Mothers who owned latrines or understood the relationship between dirty water and diarrhea have significantly higher valuations of WaterGuard, while education in general does not seem to affect demand for the product.

Finally, in column 5 we explicitly estimate this heterogeneity using a mixed logit model (Train 2003). Mixed logit allows for random coefficients β on characteristics of the options *use WaterGuard* and *don't use WaterGuard* in the indirect utility function. Simulation techniques are used since there is typically no closed-form solution. We estimate choice probabilities as:

$$P(y_{ijt} \mid X) = \int_{\beta} \frac{\exp(X_{ijt}'\beta)}{\sum_{h} \exp(X_{iht}'\beta)} f(\beta)d\beta$$

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[29] In the appendix (available upon request) we replicate the discrete choice regressions with chlorine test results as the dependent variable instead of self-reported usage. As discussed previously, we prefer the self-reports given measurement problems as the chlorine dissipates over time and in light of the water quality evidence bolstering the self-reports. With fewer observations (since chlorine tests were only conducted in the final two rounds and self-reports were collected for three survey rounds), the results based on chlorine test results are less precise. In the analogous version of Table 5, we find even lower valuation for WaterGuard than is discussed in this section.

where y is either *j=use* or *j=don't use* for household i at time t, X_{ijt} is the set of characteristics including the WaterGuard indicator to capture the benefits of the product and price, and $f(\cdot)$ is the mixing distribution, which we take to be the normal distribution for the coefficient on WaterGuard and triangular distribution (constrained to take on negative values) for price. Bayesian numerical methods allow us to maximize the log-likelihood to estimate the mean and standard deviation of β.

We find significant variation in household tastes for both the benefits of the product and its monetary cost (Table 5, column 5). Using the estimated parameters to calculate each household's valuation, we have a mean willingness to pay of 4.57 Ksh among all study households, quite comparable to the conditional logit specification. These household-specific parameters are consistent with the low demand observed in the data; 95% of households have a valuation somewhere between 1.4 Ksh and 14.6 Ksh.

Combining the results from Tables 4 and 5 yields a bound on the willingness to pay to avert child diarrhea. The average number of averted diarrhea cases due to WaterGuard treatment assignment is (-0.073 cases / child-week) * (1.4 children age 3 and under / household) * (4 weeks / bottle) = -.41 diarrhea cases per household-bottle. Using our estimated mean willingness to pay of 4.6 Ksh, this translates into 11.2 Ksh per case of diarrhea averted, or roughly US$0.16 at prevailing exchange rates, under the assumption that all of WaterGuard's value works through child health gains. This is about half the estimated willingness to pay for averted diarrhea by means of additional travel to cleaner water from the earlier component of the RWP that was focused on spring protection. Depending on the value of time, the average willingness to pay for averted diarrhea from spring protection among this same sample of households is approximately US$0.33. The discrepancy between these two estimates is likely due to the assumption that households' valuations of the two technologies are based solely on child health gains. It is quite likely that the higher willingness to pay to avert diarrhea via spring protection partially reflects the amenity value of the improved infrastructure whereas the lower willingness to pay to avert diarrhea via WaterGuard use captures some households' distaste for the strong flavor and smell of treated water.

5. Social networks and the diffusion of WaterGuard

Even in the context we have just described, in which willingness to pay for WaterGuard appears to be quite low relative to the sizable improvements in water quality and diarrhea prevalence that result from use of the product, higher adoption rates could potentially be achieved in the longer run if other community members eventually learned from early adopters. In this section, we explore this possibility and conclude that such an optimistic scenario is unlikely to unfold in the communities we study. We begin by describing the nature of social networks in our data. We then document that the intervention led community members to engage in more conversations about WaterGuard and, to a lesser degree, children's health.

Next we show that despite talking more about the product, social networks had only moderate effects on take-up. Specifically, we use data on networks prior to the intervention and exploit the exogenous variation in exposure to the treatment generated by the high-/low-intensity randomization in order to estimate the effects of close ties to treatment households on WaterGuard take-up. We also explore what types of relationships and which types of people are particularly influential. Finally, we discuss sharing and social learning as two potential mechanisms through which connections could have facilitated take-up.

5.1 Characterizing social networks

The spring communities participating in the RWP are relatively ethnically homogenous, with three-quarters of all respondent pairs saying that they are members of the same tribe.[30] Though tribe

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we find even lower valuation for WaterGuard than is discussed in this section (by approximately half), and are unable to identify household characteristics that affect valuation (cols. 3 and 4). From the mixed logit specification, we do not find evidence of heterogeneity in preferences over the product's price as is the case using self-reports.

[30] In our data, household A's relationship to B and B's relationship to A constitute two "relationship pairs".

determines mother-tongue, communication barriers do not seem to be an impediment to establishing social contacts as the distribution of conversation frequency is similar among tribally-mixed and same-tribe relationships. The majority (59%) of respondent pairs share some sort of familial bond, the most common of which are mother in-law/daughter in-law (around 20% of relationships) and wife of the brother in-law (around 25%), a reflection of the social institutions in this area that lead young women to move into their husbands' communities and the fact that our survey protocol was to interview the mother of the youngest child in the compound or, if she was unavailable, another woman. Aside from relatives, another common way households describe their relationships is as "neighbors", accounting for 35% of non-family relationships. The prevalence of neighbors and lack of "friends" likely arises from the fact that field staff were instructed to take the first volunteered characterization of the nature of a relationship. In a setting in which people live near each other, neighbor was often volunteered before friend.

We categorize a relationship as "close" if the respondent reports talking to the other household two to three times per week or more. RWP communities are quite close-knit, with only 14% of pairs being with a household the respondent does not know and 60% of relationships being close. Thus, the average household identifies 4 of the 7 other households at their spring as close contacts, and on average a given household is listed as a close contact by 3.5 of the other households at their spring. There are very few households who have no close contacts among the other sample households (3% of households are isolated in this way) or who have only one (10%). Interestingly, a relatively high proportion (18%) of relationships are not mutual, with household A identifying household B as a close connection and household B saying she doesn't even know household A, likely a result of the proliferation of different names for a single individual in these communities.

The average household had 1.8 close connections among the treatment group prior to the intervention. Only 20% of households had no close connections among the treatment group, so there is strong potential for externalities of the treatment through social networks.[31]

5.2 Changes in conversation patterns

We begin our analysis of network effects by exploring changes in conversation patterns that occurred as a result of the intervention (Table 6, Panel A). Using very detailed data on conversation topics and frequencies from the second and fourth survey rounds, we find very strong evidence that the distribution of free WaterGuard promoted conversations about the product as well as conversations about drinking water and, to a lesser degree, children's health. In particular, conversations about WaterGuard were around three times as likely to occur if the respondent was a member a treatment household and slightly more than twice as likely to occur if the other household was a member of the treatment group (columns 1 and 2), with these increases occurring in both the likelihood of frequent conversations as well as the probability that the two households had ever had a conversation about WaterGuard.[32] Conversations about drinking water were also significantly more likely to be reported if either member of the conversation pair was from a treatment household (results not shown). There was also a smaller but statistically significant increase in the probability that a respondent in the treatment group had ever spoken about children's health problems with the other household. This result suggests that the treatment households at least partially internalized messages about the connection between water and children's health that were delivered as part of the intervention. We do

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

[32] 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 drinking water generally and WaterGuard specifically, the coefficient on the treatment indicator for the non-respondent in the pair is less likely to suffer from such bias, since respondents were not likely to recall the treatment status of other households several months after the intervention occurred.

not observe significant interactions between the respondent’s treatment status and the other household’s treatment status in any of these specifications.

As an extension of the investigation of changes in conversation patterns, we also test whether social networks changed in response to the intervention, as would be the case if treatment households became more popular as a result being supplied with WaterGuard. We do not find evidence that the treatment status of either member of a relationship increased the probability of a close connection in the follow-up round, though the probability that a respondent would list another household at least as a distant contact increases slightly if either of them were members of the treatment group (Panel B, columns 5 and 6). This is not qualitatively meaningful however, relative to the 86% of relationships that are categorized as either acquaintances or close contacts prior to the intervention. There is no evidence that two households who were both in the treatment group were any more likely to have a relationship with one another than they were to have a relationship with a household in the comparison group following the intervention.

5.3 Effects on WaterGuard take-up

In Table 7, we report results from estimating Equation (1) including a variety of control variables related to social connections.[33] Our central variable of interest is the proportion of a household’s close contacts among the study households who are members of the treatment group (received WaterGuard). The denominator of this variable, total number of close contracts in our sample, is included as a control variable to account for the fact that more sociable people may more readily

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[33] All of the network regressions are replicated in the appendix (available upon request) with chlorine test results as the dependent variable instead of self-reported usage. As discussed previously, we prefer the self-reports given measurement problems as the chlorine dissipates over time and in light of the water quality evidence bolstering the self-reports. With one third less observations (since chlorine tests were only conducted in the final two rounds and self-reports were collected for three survey rounds), the results based on chlorine test results are less precise. In the analogous version of Table 7, we find no significant evidence of social network effects. The findings from Table 8 that members of the same tribe and community leaders are particularly influential are robust to either measure of chlorine use.

adopt new technologies.[34] An even simpler specification considers whether households at "high-intensity" treatment springs are more likely to use WaterGuard, whatever their own treatment status. This regression is shown in column 1. Being at a high-intensity spring more than doubles the probability that a household will use WaterGuard (a 7.8 percentage point increase) at follow-up compared to the baseline adoption rate of 6%. Interestingly, the effect of being at a high-intensity spring does not differ based on a household's own treatment status. However, we find no evidence that the intensity of treatment affects take-up after directly controlling for a household's close connections to the treatment group (columns 2-6).

Turning our preferred specification, in which we control for the intensity of treatment and own treatment status and focus instead on the nature of a household's own characteristics, we find the higher the proportion of close contacts who were in the treatment group, the more likely the household is to use WaterGuard. If all of a household's close contacts are in the treatment group, the probability of WaterGuard usage increases by 8.6 percentage points.[35] While this is small compared to the increase in take-up among treatment households in response to receipt of free WaterGuard, it implies that households who did not themselves receive WaterGuard are over twice as likely to chlorinate their water if all of their close contacts had a supply of free WaterGuard. Interestingly, we do not find that the effect of close connections to treatment households differ significantly for treatment or comparison households (column 3).

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[34] In column 7 we report results from an alternate specification that uses the number of close contacts, and the number of close contacts who were members of the treatment group, rather than the proportion specification we prefer.

[35] A number of households do in fact have a fully saturated network in the sense that all of their close contacts among study households were members of the treatment group. Of the 1104 households who had at least one close contact among the other study households, 13% had the proportion in the treatment group equal to one. Among these, not quite half had only one close contact, and several had as many as 5 or 6 close contacts to the treatment group. Only 16% of households with at least one close contact among study households had a value of zero for the proportion in the treatment group, and the distribution of proportions in the treatment group included all possible values given the variation in the number of close contacts in general and the number of treatment households at each spring, with proportions equal to 1/3, 1/2, and 2/3 each accounting for roughly 10% of the sample.

In column 4 we compare the effect of close contacts to the WaterGuard product generated by the experiment with the effect of close contacts to households who had previously used the product, which may be endogenous if a household's own use is jointly determined with the decisions of their contacts. Interestingly, we can reject equality of the experimental and observational effects for households who were themselves members of the comparison group (p-value 0.02), but the sign on the observational measure is counterintuitive, with more prior contacts reducing the chance a household would report using WaterGuard at follow-up. While we lack the power to reject equality for treatment households, the magnitudes of the effects are as would be expected, with the observational measure overstated relative to the experimental measure.

Neither distant contacts (column 5) nor second-degree close contacts (column 6) seem to be particularly influential in the take-up decision; we can reject the equality of the effects of such contacts and close contacts with p-values of 0.02 and 0.04, respectively. This contrasts with Kremer and Miguel's (2007) finding that Granovetter's (1973) "weak links" are important means of learning about deworming drugs in neighboring part of Kenya.[36] Given the high rate of non-mutual relationships in our data, we also explored various definitions of closeness, categorizing relationships based on agreement between the two households and on the maximum frequency of conversation reported by either household. None of these variations appear to be as relevant as the respondent's own assessment of her relationship with another household (results not shown).

Having established that close connections are the salient channel for social network effects, in Table 8 we investigate whether contacts with certain types of community members are more influential than others. We find no evidence that family members are driving the social network effects (column 2), but relationships with members of the same tribe do seem to carry particular weight (column 3). Since tribe determines mother-tongue, this would be consistent with conversation

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[36] We categorize another household as an acquaintance if the respondent reports knowing them but speaking to them once per week or less.

being an important means by which information about the product diffuses. While we don't see major differences in conversation frequency between households who are or are not members of the same tribe, it is certainly possible that the nature of these conversations differs, making it easier for information to flow between members of the same tribe.

Self-identified community leaders are also differentially influential (column 4), as seems natural since they have displayed a willingness to participate in community improvement in some other sphere as well. Importantly, the social network effects do not stem from sociable people per se (column 5), but rather specifically from those who serve their community.

We also explored the importance of contacts to households who were likely to have a particular interest in spreading the word about WaterGuard, such as those who had previously engaged in WaterGuard conversations with the respondent household (column 6) and those who reported a recent local cholera outbreak (column 7). Though the magnitude of these effects is not as large as that from contacts with members of the same tribe and community leaders, only about 10% of relationships are with households who could be identified as concerned along one of these dimensions so it is possible that we lack the power to estimate these effects.

5.4 Mechanisms by which networks effects change behavior

The WaterGuard technology may diffuse in social networks either mechanistically, via gift-giving, or through learning and peer effects. Either of these mechanisms could potentially be reinforced if the intervention changed the structure of networks for some reason, as well, though as mentioned above we find no evidence of that this occurred. We are able to examine each of these diffusion paths and conclude that both the mechanistic and learning effects are at work.

Since network effects do not seem to differ depending on the household's own treatment status, it appears that positive reinforcement is one way in which social networks increase take-up, encouraging households who had free WaterGuard to use it. Additionally, another possibility is that comparison households are encouraged to purchase WaterGuard for themselves after hearing about it from a close connection in the treatment group. Of the 64 comparison households whose reported treating their drinking water with chlorine at follow-up, 50 (78%) said that they had purchased WaterGuard in the past six months, while only 16% of treatment households who reported chlorinating their water at follow-up also reported purchasing WaterGuard (results based on verified chlorination are very similar).

Alternatively, the chlorine in the water of comparison households could have come directly from a treatment household as a gift. In the follow-up survey round 7% of comparison households reported receiving some WaterGuard from another community member in the past six months. This is likely a lower bound on the number of comparison households that did actually receive WaterGuard as a gift from a treatment household, since among a subset of comparison households that were asked both generally about other members of their community and specifically about each other sample household, 4% of households reported being given WaterGuard based on the specific questions but did not recall being given WaterGuard when asked about other members of their community in general. From treatment households' reports of what they did with the WaterGuard they received as part of the intervention, we know that sharing was common with almost half of the households in the treatment group giving at least one bottle away. Treatment households were approximately equally likely to share WaterGuard with others who lived inside and outside their compound, and conditional on sharing, the average gift was two bottles.

In summary, of the 64 comparison households who reported treating their drinking water with chlorine at follow-up, 50 had purchased WaterGuard in the past 6 months and 21 reported being given WaterGuard (11 households both purchased and received WaterGuard).

6. Discussion and conclusion

The provision of free chlorine dramatically increases the number of households who treat their home water, leading to major reductions in home water contamination and diarrhea among young children. However, data from coupon distribution shows that only a small fraction of households purchase the chlorine product (WaterGuard) after the intervention, despite deep discounts. Why is demand for WaterGuard so price-elastic when the benefits of use are so high? It seems unlikely that hidden costs in terms of distance to the purchase point could explain these trends when so many households make regular trips to market centers where the shops were located, leaving other hidden costs such as taste as the most plausible explanations.

Nonetheless, even if only a few households adopt the product at the market price, with enough social learning widespread take-up might be achievable. Encouragingly, study households were more likely to talk with other community members about WaterGuard after the intervention, but ultimately we do not find strong evidence that these conversations translated into significantly higher take-up rates. Consistent with the low demand observed at positive prices, we find only a moderate amount of social spillovers; these seem to have affected both treatment and comparison households. Though our social network effects are not very precisely estimated, close connections to members of the treatment group seem to have increased the probability that a household would adopt the WaterGuard technology. These network effects pale in comparison to the effect of assignment to treatment, but among comparison households the increase is economically relevant. While we are generally pessimistic about the potential for network spillovers to meaningfully increase take-up in the absence of a broader intervention, the finding that community leaders are particularly influential does identify a promising distribution channel in this area that could be more cost-effective than less targeted forms of social marketing.

A point-of-use chlorine technology is more effective at reducing home water contamination than spring protection, a source water quality improvement. Yet while households who are given the product for free are quite likely to use it, and experience major health benefits as a result, the willingness to pay for the chlorine technology is extremely low. This discrepancy between the effectiveness and the willingness to pay for two alternate water quality improvements has important implications for policy makers when setting funding priorities. In particular, it seems unlikely that the significant benefits from WaterGuard use can be realized in a market environment in which consumers have such low valuation of the good, and ongoing subsidies will likely be necessary to achieve sustained widespread adoption.

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Figure 1: Rural Water Project (RWP) Timeline 2004-2007

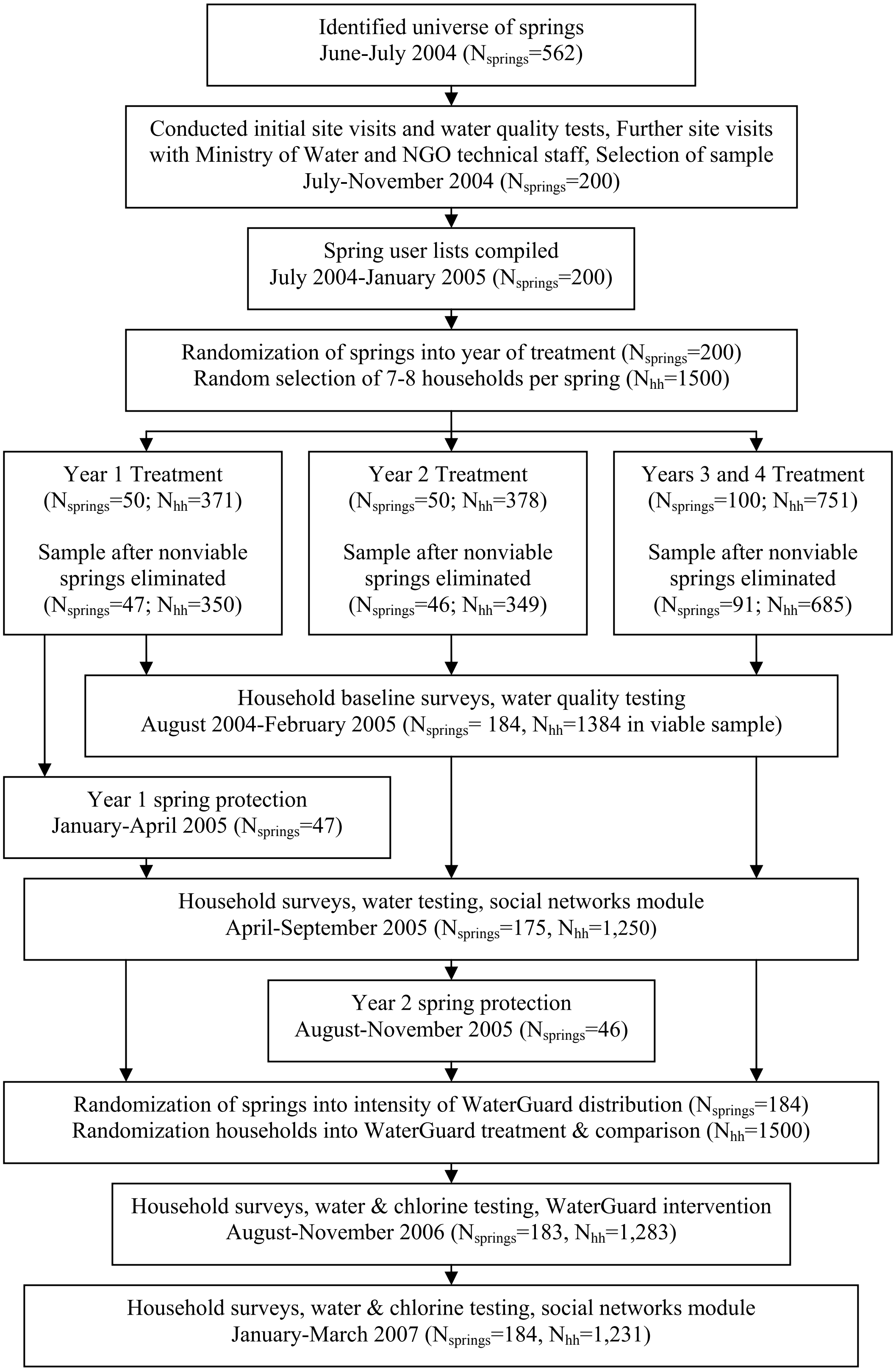


Figure 2: Baseline and Post-Intervention ln(Home water *E. coli* MPN)

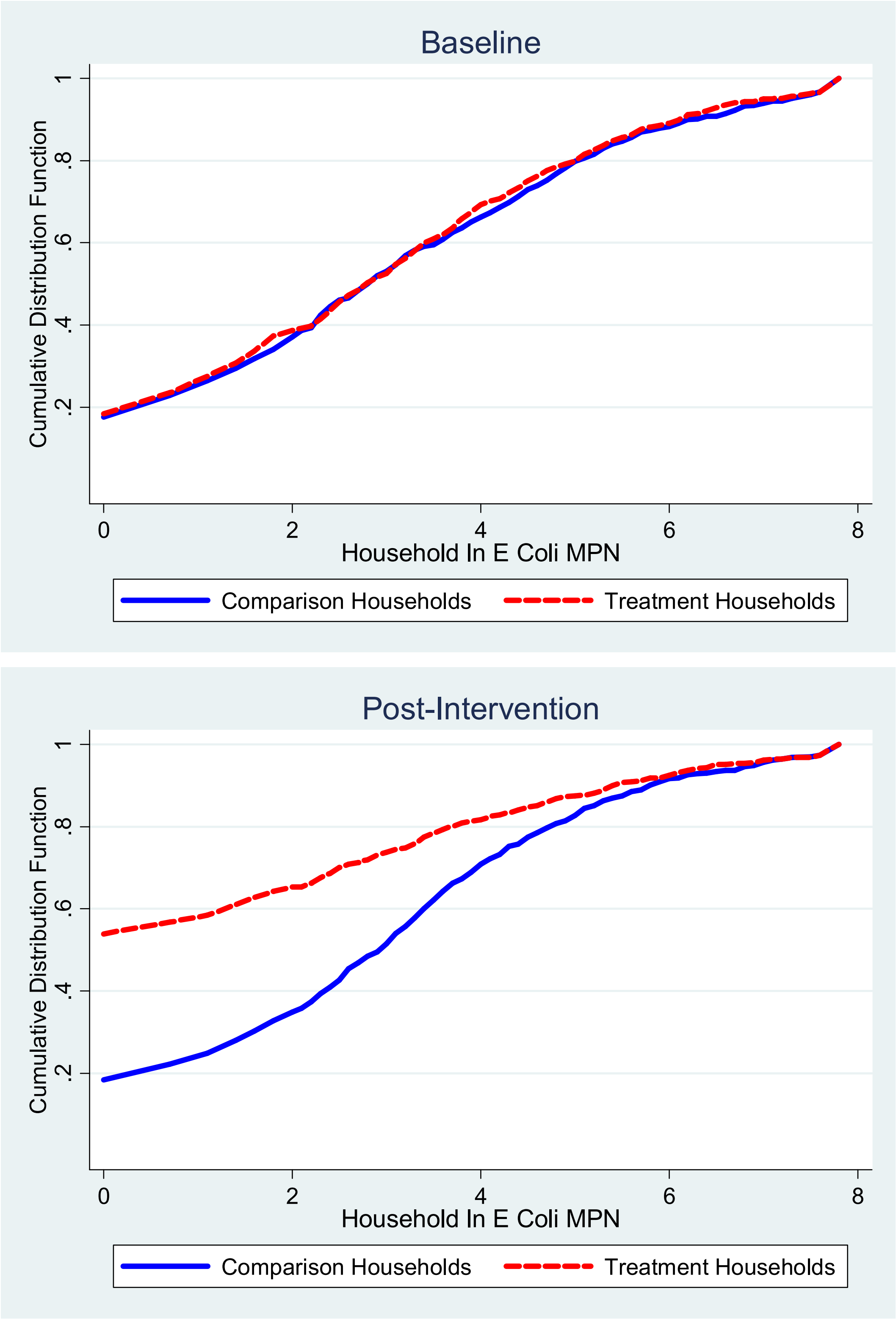
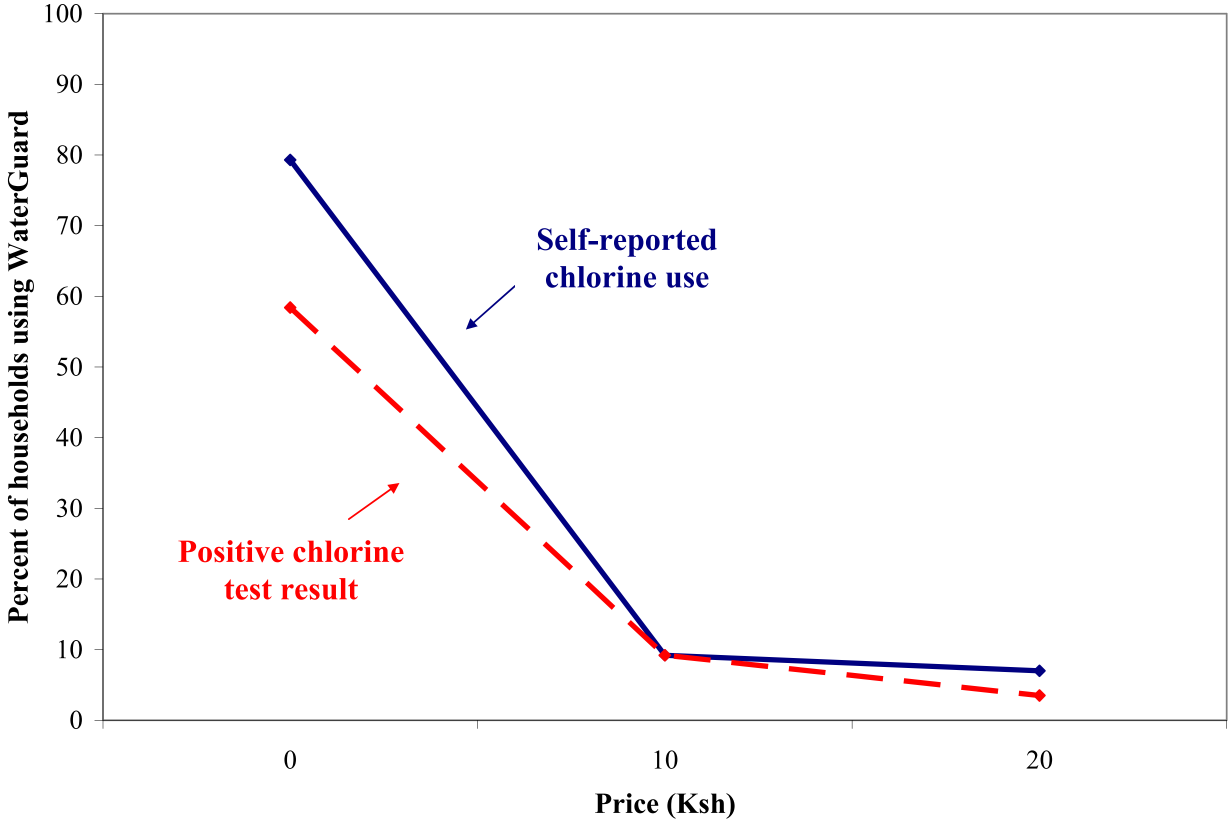


Figure 3: Demand for WaterGuard



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).

Table 1: Baseline descriptive statistics

	Treatment (WaterGuard) Mean (s.d.)	Obs.	Comparison Mean (s.d)	Obs.	Treatment – Comparison (s.e)
Panel A: Household summary statistics					
Household's "assigned" spring protected by IPA(a)	0.50	670	0.50	664	0.00
	(0.5)		(0.50)		(0.04)
Ln. *E. coli* MPN (CFU/ 100 ml)	3.24	668	3.22	659	0.02
	(2.17)		(2.17)		(0.12)
Water is high quality (*E. coli* MPN $\leq$ 1)	0.14	668	0.14	659	0.00
	(0.34)		(0.35)		(0.02)
Water is high or moderate quality (*E. coli* MPN <100)	0.73	668	0.75	659	-0.02
	(0.44)		(0.43)		(0.02)
Water is poor quality (*E. coli* MPN 100-1000)	0.20	668	0.18	659	0.03
	(0.40)		(0.38)		(0.02)
Water is very poor quality (*E. coli* $\geq$ 1000)	0.07	668	0.07	659	-0.01
	(0.25)		(0.26)		(0.01)
Walking distance to closest water source (minutes)	8.62	664	8.12	659	0.50
	(8.01)		(7.46)		(0.40)
Respondent years of education	5.66	667	5.71	663	-0.06
	(3.62)		(3.61)		(0.20)
Children under age 12 in the compound	4.05	670	4.03	664	0.02
	(2.42)		(2.54)		(0.14)
Children under age 3 in the compound	1.43	670	1.41	664	0.02
	(1.39)		(1.28)		(0.08)
Iron roof indicator	0.70	648	0.70	640	0.00
	(0.46)		(0.46)		(0.03)
Household has a pit latrine	0.86	669	0.87	662	-0.01
	(0.35)		(0.34)		(0.02)
Respondent reported cholera in community in past 2 years(a)	0.14	673	0.09	645	0.04
	(0.34)		(0.29)		(0.02)**
Respondent had heard of WaterGuard(b)	0.73	614	0.73	610	0.00
	(0.44)		(0.44)		(0.03)
Water in the home treated with WaterGuard, self-report(b)	0.08	610	0.07	610	0.01
	(0.27)		(0.25)		(0.02)
Water storage container in home was covered	0.92	611	0.91	607	0.01
	(0.27)		(0.28)		(0.02)
Yesterday's drinking water was boiled indicator	0.25	668	0.29	656	-0.04
	(0.43)		(0.45)		(0.03)
Respondent diarrhea prevention knowledge score	3.06	670	3.22	664	-0.17
	(2.09)		(2.25)		(0.13)
Respondent said "dirty water" causes diarrhea	0.68	670	0.68	664	0.00
	(0.47)		(0.47)		(0.03)
Household has soap in the home	0.92	669	0.89	663	0.03
	(0.27)		(0.31)		(0.02)*
Respondent's number of close contacts(b)	4.06	611	3.87	612	0.20
	(1.90)		(1.96)		(0.12)*
Number of close contacts to respondent(b)	3.59	681	3.49	691	0.10

	(0.06)		(0.06)		(0.11)
Panel B: Child demographics and health[(a)]					
Child age (years)	1.83	908	1.79	859	0.04
	(0.99)		(0.99)		(0.04)
Child male (=1)	0.49	893	0.51	845	-0.02
	(0.50)		(0.50)		(0.03)
Child had diarrhea in past week indicator[(c)]	0.22	884	0.18	842	0.04
	(0.42)		(0.38)		(0.02)**
Child had diarrhea in past week indicator, first observation[(d)]	0.25	897	0.24	852	0.01
	(0.43)		(0.43)		(0.02)
Child weight (kg)	10.77	824	10.58	771	0.20
	(3.53)		(3.24)		(0.16)
Child height (cm)	78.48	815	78.46	767	0.20
	(11.81)		(11.65)		(0.16)

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

Household data are from the 2004 survey, except where noted. Child-level data are from the 2006 survey and are restricted to those age 3 and under.

Household survey respondent is the mother of the youngest child in the compound (or the youngest adult woman available).

(a): At the time of the WaterGuard intervention in the third (2006) survey round.

(b): Because of changes in survey design, responses to these questions are not available for the first (2004) round of data collection and are instead taken from the second (2005) round.

(c): Diarrhea is defined as three or more “looser than normal” stools per day.

(d): Using the first available diarrhea data for children age 3 and under in survey round 3 (2006).

Table 2: WaterGuard distribution take-up impacts

	Dependent variable: Water treated with chlorine					
	Self-reported chlorine use			Positive chlorine test result		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment (WaterGuard) indicator	0.691	0.696	0.689	0.515	0.526	0.572
	(0.032)***	(0.024)***	(0.066)***	(0.034)***	(0.025)***	(0.074)***
Protected spring indicator	0.049	0.051	0.052			
	(0.026)*	(0.025)**	(0.024)**			
Interactions with treatment indicator:						
Protected spring indicator	0.010					
	(0.040)					
Baseline ln(spring water *E. coli* MPN)		0.001			-0.004	
		(0.008)			(0.010)	
Baseline latrine density			0.001			0.108
			(0.178)			(0.204)
Baseline diarrhea prevention score			-0.004			-0.003
			(0.013)			(0.015)
Baseline knowledge of safe water			0.008			0.017
			(0.056)			(0.060)
Baseline boiled water yesterday indicator			0.099			0.065
			(0.052)*			(0.062)
Baseline mother's years of education			-0.004			-0.003
			(0.007)			(0.008)
Baseline number of children under 3			-0.009			0.030
			(0.028)			(0.029)
Baseline number of children under 3 with diarrhea			0.028			-0.108
			(0.084)			0.005
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Survey rounds	2-4	2-4	2-4	3-4	3-4	3-4
R^2	0.57	0.57	0.59	0.52	0.51	0.53
Observations (spring clusters)	3784	3760	3416	2563	2547	2243
	(184)	(183)	(184)	(184)	(184)	(184)
Number of households	1413	1405	1215	1406	1398	1209
Mean (s.d.) of dependent variable prior to intervention	0.06	0.06	0.06	0.02	0.02	0.02
	(0.24)	(0.24)	(0.24)	(0.14)	(0.14)	(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. Due to changes in survey design, self-reported water treatment status is not available for the first survey round. Chlorine tests were only conducted during the final two survey rounds. 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.

Baseline values of all variables are from the third (2006) survey round in which the intervention took place. In columns 3 and 6 baseline number of children (under age 12), baseline number of children with diarrhea, baseline iron roof and baseline iron roof density within spring community are included as additional control variables. Baseline spring water quality, latrine density, diarrhea prevention score, mother's education, number of children, number of children under 3, number of children with diarrhea, number of children under 3 with diarrhea, and iron roof density are de-meaned.
Survey round and month fixed effects included in all regressions but not reported. When interactions are included, baseline variables are interacted with survey round in addition to interactions with treatment (WaterGuard) indicator. These coefficients not reported in the table.

Table 3: WaterGuard distribution household water quality impacts

	Dependent variable:							
	ln(Home water *E. coli* MPN)				*E. coli* in home water (binary indicator)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Water treated with chlorine, self-report$^{(a)}$				-1.936				-0.529
				(0.235)***				(0.044)***
Treatment (WaterGuard) indicator	-1.370	-1.236	-1.303		-0.372	-0.359	-0.295	
	(0.190)***	(0.144)***	(0.412)***		(0.036)***	(0.027)***	(0.085)***	
Protected spring indicator	-0.314	-0.311	-0.280		-0.001	0.000	0.000	
	(0.133)**	(0.135)**	(0.136)**		(0.021)	(0.021)	(0.022)	
Interactions with treatment indicator:								
Protected spring indicator	0.209				0.018			
	(0.219)				(0.042)			
Baseline ln(spring water *E. coli* MPN)		-0.144				-0.012		
		(0.053)***				(0.010)		
Baseline latrine density			0.197				0.093	
			(0.979)				(0.169)	
Baseline diarrhea prevention score			0.084				0.028	
			(0.098)				(0.017)	
Baseline knowledge of safe water			-0.102				-0.091	
			(0.343)				(0.076)	
Baseline boiled water yesterday			0.043				-0.041	
			(0.331)				(0.072)	
Baseline mother's years of education			0.003				0.008	
			(0.046)				(0.009)	
Baseline num. of children under 3			0.142				-0.006	
			(0.197)				(0.035)	
Baseline num. of children under 3 with diarrhea			0.950				0.080	
			(0.507)*				(0.097)	
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey rounds	1-4	1-4	1-4	3-4	1-4	1-4	1-4	3-4
R^2	0.09	0.09	0.10	--	0.14	0.14	0.15	0.24
Observations (spring clusters)	5117	5091	4625	3320	5117	5091	4625	3320
	(184)	(183)	(184)	(184)	(184)	(183)	(184)	(184)
Number of households	1414	1406	1215	1195	1414	1406	1215	1195
Mean (s.d.) of dependent variable prior to intervention	2.99	2.99	2.99	2.99	0.18	0.18	0.18	0.18
	(2.21)	(2.21)	(2.21)	(2.21)	(0.38)	(0.38)	(0.38)	(0.38)

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. MPN stands for "most probable number" coliform forming units (CFU) per 100ml.

Baseline values of all variables are from the third (2006) survey round in which the intervention took place. In columns 4 and 8 baseline number of children (under age 12), baseline number of children with diarrhea, baseline iron roof and baseline iron roof density within spring community are included as additional control variables. Baseline spring water quality, latrine density, diarrhea prevention score, mother's education, number of children, number of children under 3, number of children with diarrhea, number of children under 3 with diarrhea, and iron roof density are de-meaned.
Survey round and month fixed effects included in all regressions but not reported. When interactions are included, baseline variables are interacted with survey round in addition to interactions with treatment (WaterGuard) indicator. These coefficients not reported in the table.
(a): Instrumented with WaterGuard treatment indicator variable, number of close contacts in treatment group, and protected spring indicator.

Table 4: Health outcomes for children age three or younger at time of intervention

	Dependent variable: Diarrhea in past week				
	(1)	(2) Probit	(3)	(4)	(5)
Treatment (WaterGuard) indicator	-0.012	-0.045	-0.073	-0.076	-0.121
	(0.018)	(0.025)*	(0.031)**	(0.031)**	(0.038)***
Protected spring indicator	-0.050	-0.060	-0.049	-0.050	-0.064
	(0.016)***	(0.031) *	(0.034)	(0.034)	(0.034)*
Interactions with treatment indicator:					
Protected spring indicator					0.089
					(0.040)**
Child fixed effects	No	No	Yes	Yes	Yes
Treatment group fixed effects	No	Yes	No	No	No
Month of year controls	No	Yes	Yes	Yes	Yes
Gender-age controls	No	No	No	Yes	Yes
R^2	0.00	-	0.49	0.49	0.49
Child-year observations (spring clusters)	5103	5102	5102	5102	4950
	(184)	(184)	(184)	(184)	(184)
Number of children			2121	2121	2011
Mean (s.d.) of the dependent variable in the comparison group in survey rounds 3-4	0.20 (0.40)	0.20 (0.40)	0.20 (0.40)	0.20 (0.40)	0.20 (0.40)

Notes: Column 2 estimated using probit (marginal effects presented), columns 1 and 3-5 estimated using OLS. Huber-White robust standard errors (clustered at the spring level) are presented, significantly different than zero at * 90% ** 95% *** 99% confidence. Sample restricted to children age three or younger at the time of the intervention. Diarrhea defined as three or more “looser than normal” stools within 24 hours at any time in the past week. The gender-age controls include linear and quadratic current age (by month), and these terms interacted with a gender indicator. Baseline values of all variables are from the third (2006) survey round in which the intervention took place. Columns 2-5 also contain a survey round control.

Table 5: Discrete choice models (conditional and mixed logit) of WaterGuard usage

	WaterGuard usage based on self-reports & redeemed coupons				
	(1)	(2)	(3)	(4)	(5)
WaterGuard indicator	0.424 (0.071)***	0.378 (0.083)***	0.395 (0.073)***	-0.529 (0.183)***	
Mixed logit – Mean (normal):					1.473 (0.122)***
Mixed logit – Std. dev. (normal):					1.129 (0.172)***
Price (0, 10, or 20 Ksh per bottle)	-0.190 (0.006)***	-0.190 (0.006)***	-0.189 (0.007)***	-0.188 (0.006)***	
Mixed logit – Mean (restricted triangular):					-0.390 (0.019)***
Mixed logit – Sample Std. dev. (restricted triangular):					0.090
WaterGuard indicator * Protected spring indicator		0.085 (0.080)			
WaterGuard indicator * Children under age 3			-0.007 (0.060)		
WaterGuard indicator * Children age 3-12			0.150 (0.045)***		
Price * Children under age 3			-0.009 (0.006)		
Price * Children age 3-12			-0.006 (0.004)		
WaterGuard indicator * Baseline latrine ownership				0.585 (0.152)***	
WaterGuard indicator * Baseline knowledge of safe water				0.321 (0.111)***	
WaterGuard indicator * Baseline mother's years of education				-0.001 (0.012)	
Log likelihood at convergence	-2096	-2095	-1984	-1956	-1763
Number of observations	12684	12684	12090	11922	12684
Number of households	1451	1451	1366	1347	1451
Mean willingness to pay for WaterGuard (Ksh)	2.23	--	--	--	4.57

Notes: Conditional logit model in columns 1-4 and mixed logit model in column 5 (grouped by choice situations, one per household in each survey round). Significantly different than zero at * 90% ** 95% *** 99% confidence. Each observation is a unique pair between a household and one of the two water treatment options in the choice set: use WaterGuard or not. The WaterGuard indicator variable, which captures the benefits of using the product, is equal to one for the option of using WaterGuard and zero otherwise. The price of WaterGuard varies according to the experimental design – zero for treatment households in the post-intervention survey round, 10 for the subset of treatment households who received coupons during the 12 months in which they could be redeemed, and 20 for all households prior to the intervention and control households after the intervention; the price of not using WaterGuard is always zero. The dependent variable is equal to one for the water treatment option chosen by the household; for prices of zero and 20 this is based on the household's self-report of WaterGuard usage and for the price of 10 is based on whether or not the household redeemed the monthly coupon according to shopkeepers' records. Data are from the final three survey rounds (2005, 2006, and 2007) and shopkeepers' records for the 12 months in which coupons could be redeemed. In column 4, additional control variables are latrine density within spring community, hygiene knowledge score, number of children under 12 living in the home, home has iron roof indicator, iron roof density within spring community, and the boiled water yesterday indicator (all measured at baseline), interacted with the WaterGuard indicator. Mean willingness to pay for WaterGuard is the ratio of the coefficients on the WaterGuard indicator and price variable in column 1 and is calculated at the household level using the conditional means of the random coefficients in column 5.

Table 6: Changes in conversation patterns and contacts following WaterGuard distribution

	Panel A Topic and frequency of conversation, as reported by respondent household				Panel B Respondent household named non-respondent household as:	
	WaterGuard		Children's health			
	Many times (1)	Ever (2)	Many times (3)	Ever (4)	Close contact (5)	Close or distant contact (6)
Treatment (WaterGuard) indicator for respondent household in pair	0.074 (0.018)***	0.197 (0.028)***	0.025 (0.022)	0.060 (0.025)**	0.004 (0.021)	0.021 (0.013)*
Treatment indicator for non-respondent household in pair	0.046 (0.014)***	0.126 (0.020)***	0.006 (0.018)	0.032 (0.021)	0.006 (0.018)	0.020 (0.012)*
Interaction of respondent and non-respondent households' treatment indicators	0.027 (0.023)	-0.017 (0.031)	0.021 (0.028)	0.017 (0.030)	0.035 (0.026)	0.003 (0.014)
R^2	0.03	0.06	0.01	0.02	0.01	0.01
Household pair observations (spring clusters)	6557 (183)	6557 (183)	6531 (183)	6531 (183)	7220 (184)	7220 (184)
Mean (s.d.) of the dependent variable in survey round 2	0.04 (0.19)	0.10 (0.30)	0.25 (0.43)	0.51 (0.50)	0.61 (0.49)	0.86 (0.35)

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. In Panel A, the dependent variable is a binary indicator equal to 1 if the respondent household reported conversing on the given topic at the given frequency with the household in question (each respondent was asked about each of the other study households at their spring); in Panel B, the dependent variable is a binary indicator equal to 1 if the respondent household reported having a relationship of the given type with the household in question . Data are from the fourth survey round. Columns 1-4 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. Columns 5 and 6 include a control for whether or not the respondent reported having the given type of relationship with the household in question during the second survey round. Data on conversation patterns and relationships are only available for the second and fourth survey rounds.

Table 7: Social networks & WaterGuard take-up

	Dependent variable: Water treated with chlorine, self-report						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment (WaterGuard) indicator	0.712	0.688	0.706	0.687	0.682	0.679	0.689
	(0.035)***	(0.029)***	(0.046)***	(0.047)***	(0.030)***	(0.030)***	(0.030)***
High-intensity treatment indicator	0.078	-0.005	-0.005	-0.005	0.025	0.034	-0.008
	(0.034)**	(0.036)	(0.036)	(0.036)	(0.044)	(0.049)	(0.043)
Interaction of treatment and high-intensity indicators	-0.074						
	(0.051)						
Interactions of baseline network characteristics with post-intervention indicator:							
Proportion of close contacts in treatment group (received free WaterGuard) [(a)]		0.086	0.103	0.111	0.067	0.060	
		(0.046)*	(0.053)*	(0.054)**	(0.049)	(0.051)	
Number of close contacts among study households at spring		0.010	0.01	0.009	0.021	0.008	-0.002
		(0.006)	(0.006)	(0.006)	(0.009)**	(0.008)	(0.009)
Treatment (WaterGuard) indicator * Proportion of close contacts in treatment group			-0.037	-0.043			
			(0.070)	(0.070)			
Proportion of close contacts who had previously used WaterGuard				-0.119			
				(0.065)*			
Treatment indicator * Proportion of close contacts who had previously used WaterGuard				0.232			
				(0.093)***			
Proportion of distant contacts in treatment group[(b)]					-0.050		
					(0.041)		
Proportion of 2nd degree close contacts in treatment group[(c)]						-0.055	
						(0.048)	
Number of close contacts in treatment group							0.022
							(0.015)
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey rounds	2-4	2-4	2-4	2-4	2-4	2-4	2-4
R^2	0.57	0.57	0.57	0.57	0.57	0.57	0.57
Observations (spring clusters)	3784	3443	3443	3443	3443	3443	3443
	(184)	(184)	(184)	(184)	(184)	(184)	(184)
Number of households	1413	1223	1223	1223	1223	1223	1223
Mean (s.d.) of dependent variable pre-intervention	0.06	0.06	0.06	0.06	0.06	0.06	0.06
	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)	(0.24)

Notes: Estimated using OLS with household fixed effects. Huber-White robust standard errors (clustered at the spring level) are presented, significantly different than zero at * 90% ** 95% *** 99%confidence. 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. Columns 2-6 and 8 also include an indicator variable for households who have zero contacts interacted with the post-intervention indicator. In columns 5 and 6 there are also equivalent indicators for households who have no distant or 2nd degree close contacts, as well as controls for the number of distant or 2nd degree close contacts.

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

(b): Distant contacts are defined as households with whom the respondent reports talking once a week or less.

(c): 2nd degree close contacts are the close contacts of close contacts (not including the original close contacts or the respondent’s household itself).

Table 8: Social networks, relationship types, & WaterGuard take-up

Baseline proportion of different types of close contacts who are members of the treatment group interacted with the post-intervention indicator:	Dependent variable: Water treated with chlorine, self-report						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment (WaterGuard) indicator	0.688	0.687	0.691	0.691	0.689	0.686	0.69
	(0.029)***	(0.029)***	(0.029)***	(0.029)***	(0.029)***	(0.029)***	(0.028)***
All close contacts(a)	0.086	0.068	-0.007	0.042	0.102	0.079	0.078
	(0.046)*	(0.058)	(0.065)	(0.053)	(0.064)	(0.048)	(0.047)
Family members		0.029					
		(0.050)					
Same-tribe			0.121				
			(0.056)**				
Community leaders(b)				0.077			
				(0.046)*			
Socially well-connected(c)					-0.023		
					(0.056)		
Previously discussed WaterGuard						0.039	
						(0.067)	
Exposed to cholera(d)							0.040
							(0.050)
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey rounds	2-4	2-4	2-4	2-4	2-4	2-4	2-4
R^2	0.57	0.57	0.57	0.57	0.57	0.57	0.57
Observations	3443	3443	3443	3443	3443	3443	3443
(spring clusters)	(184)	(184)	(184)	(184)	(184)	(184)	(184)
Number of households	1223	1223	1223	1223	1223	1223	1223
Percentage of relationship pairs of given type	60%	59%	21%	36%	57%	9%	10%
Mean (s.d.) of the dependent variable prior to the intervention	0.06 (0.24)	0.06 (0.24)	0.06 (0.24)	0.06 (0.24)	0.06 (0.24)	0.06 (0.24)	0.06 (0.24)

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 are from the fourth survey round. Additional control variables not shown but included in all columns are an indicator variable for springs at which six households were given free WaterGuard (the "high-intensity" treatment), 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. All columns also include indicator variables for zero close contacts and zero contacts of a particular type interacted with the post-intervention indicator. Column 1 replicates column 2 of Table 6.

(a): Close contacts are defined as those in which the respondent reports talking to another household 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.
(c): Households are defined as well-connected socially based on the number of other households at their spring who report being close contacts with the household in question. When 4 or more other households report being close contacts with a given household, that household is considered well-connected socially. The median number of other households that report being close contacts is 4.
(d): In the third survey round, each household was asked if their community had been affected by cholera in the past two years. There is a surprising lack of consensus regarding cholera exposure within spring communities, but we use each contact's self-report since this is likely what governs their level of concern regarding cholera.