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Information and Persuasion

Achieving Safe Water Behaviors in Kenya

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Information and Persuasion: Achieving Safe Water Behaviors in Kenya

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Abstract

Convincing people to adopt preventive health behaviors consistently is difficult, yet many lives could be saved if we understood better how to do so. For example, low-cost point-of-use (POU) technologies such as chlorine and filters can substantially reduce diarrheal disease (Clasen et al. 2006). Nonetheless, they are not widely or consistently used anywhere in the developing world, even when widely available. We ran a randomized field study in Kenya in which households received free POU products to test the importance of informational and behavioral constraints on usage. Sharing information about local water quality increases water treatment by 7-10 percentage points (11-24%) above that achieved by providing free products. Persuasive social marketing messages that harness findings from behavioral economics increase water treatment by an additional 9-11 percentage points. These results suggest promising avenues for incremental improvements in encouraging water treatment (and possibly other preventive health) behaviors. However, repeated exposures may be necessary to sustain behavior change.

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I. Introduction

Inadequate access to safe water is a primary cause of the estimated 1.8 million child deaths from diarrhea that occur each year in poor countries (Boschi-Pinto, Velebit, and Shibuya 2008). Fortunately, a number of randomized controlled studies have shown that low-cost point-of-use (POU) safe water technologies such as chlorine or a filter can substantially reduce diarrheal incidence (Clasen et al. 2006). Unfortunately, adoption and regular use of POU technologies remains low among the global poor. Although affordability is a barrier, changing daily behavior appears to be at least as large an obstacle (Zwane & Kremer 2007; Luby et al. 2008).

This paper presents results from a field experiment conducted in rural western Kenya that provided all participating households with free POU products.[1] By examining usage in a

[1] A companion paper (Albert, Luoto, and Levine 2010) compares the popularity and usage of the several POU products. Because all the POU products massively improve the safety of drinking water, we focus here on the non-price barriers that limit their usage. The appropriate role of

situation in which purchasing the product is not a constraint, we can test potential informational and behavioral constraints on POU product usage. We hypothesized that many people lack complete information about the quality of their drinking water and its link with waterborne illness. We also hypothesized that heuristics emphasized by psychologists and behavioral economists (e.g., Cialdini 1993; Bertrand et al. 2009) can influence safe water behaviors. Specifically, we hypothesized that the framing of the decision to use a POU product might matter and that requesting a public commitment to use a POU product could increase regular usage.

We tested the role of information by providing randomly selected households with the results of water quality tests. Providing information about water quality has affected consumers' safe water behaviors in other settings (Madajewicz et al. 2007; Jalan & Somanathan 2008; Opar et al. 2007; Bennear et al. 2011; see Lucas et al. (2011) for a review). Our test of the role of information is unique in that some households received information on the quality of their source water (e.g., a nearby river) while other households received information about the quality of their source water *and* of the water they had stored in their own home. Water that is safe at the source, where it is collected, often does not lead to drinking water that is safe in the home due to recontamination between the source and the household (Wright, Gundry, and Conroy 2003). From a policy perspective, it is much more cost-effective if village-level information is sufficient to change behavior. At the same time, people often weight personal experience more heavily in decisions involving a variety of self-protective behaviors (Simonsohn et al. 2008). Perhaps for this reason, telling people about their own estimated risks often improves health behaviors more than telling people about estimated risks that apply more generally (Edwards et al. 2000). We

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charging for POU products has been explored by others (e.g., Kremer et al. (2009); Ashraf et al. (2007); Holla & Kremer (2009)) and we do not enter this debate here.

therefore predicted that sharing information on unsafe source and unsafe personal stored water would increase usage relative to sharing information solely on source water.

We tested the importance of framing by providing different participants with different messages. Some emphasized that POU products improved health while others emphasized both avoiding disease and improving health. This intervention was designed to test competing recommendations about effective framing in the psychological and behavior change literatures (see citations below). We tested the importance of public commitment by asking respondents if they would use the product daily. Again, this intervention builds on an extensive experimental literature in other settings (see below).

Both information and marketing appeals increase POU usage rates beyond that achieved by free distribution alone. In particular, sharing information that the community's water sources are contaminated increases product use 7-10 percentage points (11-24 percent). The additional sharing of information of a household's own water quality has no further effect on use.

The two marketing messages also raised usage of the point-of-use water treatment products. Specifically, messages that "framed" safe water technologies as both avoiding disease and improving health (as opposed to improving health alone) raised usage rates by 4-6 percentage points, or 6-15%. Marketing messages that asked consumers to publicly commit to water treatment had similar results. If these results generalize, a standard intervention that distributes POU devices for free could—by adopting these messages—save approximately 20 additional disability-adjusted life years (DALYs) per 1000 households with young children per year, at minimal increase in cost.

Our results also suggest promising avenues to explore for improvements in the market viability of health products distributed by the private sector. Many of these interventions are potentially cost-effective and necessitate only a rethinking of existing marketing strategies.

2. Study Design

2.1 Background

We partnered with CARE-Kenya to carry out a field study from July 2008 to February 2009 of 400 randomly selected compounds (a collection of households; Luo tradition allows for polygamous marriages) in 28 villages within the largely rural Nyawita sublocation of Nyanza province in western Kenya. This is among Kenya's poorest regions and was chosen due to its seasonal reliance on turbid earthpans - surface reservoirs that sometimes go dry between rainy seasons - for drinking water. Drinking water conditions vary considerably throughout the year, but most respondents prefer rainwater collection and public taps when available.[2] Other water sources include the Yala River bordering one side of Nyawita and the various earthpans that dot the landscape.

2.2 Experimental Design

Prior to the start of the study, CARE staff conducted a census of all compounds in the 28 villages and recorded which had a child under five, the sole criterion for inclusion in the study. From this list, 400 compounds were chosen by a random-number generator.

[2] Nyawita has rainy seasons near April and August (each with a monthly average near 160 mm), with moderate rain in the short dry season (averaging about 100 mm in June) and little rain in the long dry season (averaging less than 40 mm in January)(Kenya Agricultural Research Institute (KARI) n d).

In July-August 2008, our enumerators visited these compounds and asked to conduct a baseline interview with the mother of the youngest child. If that mother was not available, a mother of a child under five was selected. If no eligible mother was available, enumerators were allowed to substitute the father (11% of baseline interviews). In the rare instances that no one in the household was available or willing to participate in the study, a substitute compound was chosen on the basis of logistical convenience; due to the dispersed rural landscape, we simply chose the nearest neighboring compound with a child under five.

The baseline interview asked respondents about their current water and hygiene knowledge and behaviors, as well as prior exposure to any POU technologies. Enumerators then read an educational script about the dangers of unsafe drinking water, followed by detailed presentations on three POU products in randomized order: a liquid chlorine product branded as WaterGuard, Procter & Gamble's flocculant-disinfectant powder branded as PUR, and a gravity-driven porous ceramic filter. All three products have been shown to substantially reduce contamination in drinking water in a number of previous randomized trials (Clasen et al. 2006, 2005; Jain et al. 2010; Crump et al. 2004).[3]

After the product introductions, enumerators presented marketing messages with randomly assigned “frames.” Half of the households heard a “positively framed” message that emphasized the gains from POU usage while the other half were given a “contrast frame” that contrasted what one stands to lose from non-use with the gains from POU product usage. The framed messages were implemented orthogonally to the order of product introductions.

[3] See Appendix A for brief introductions to WaterGuard, PUR, and the filter. A companion paper, Albert et al. (2010), examines usage rates by product in greater detail.

At the end of the baseline interviews, respondents were randomly assigned one of the three POU technologies for a two-month trial. Each family received a covered bucket with tap along with their assigned product to minimize the chances for recontamination of treated water within the household. At this point, orthogonal to the framing treatment and the assigned product, enumerators asked one-half of respondents to commit verbally to the enumerator to use their assigned POU product for all of their drinking water.

Two months later, enumerators revisited all households to collect stored water (both treated and untreated, if available) to test for product usage. Enumerators then re-delivered the same marketing (framing and commitment) treatments as they distributed one of the remaining POU products for a new two-month trial. They also collected any leftover supplies of the previously assigned product. This process was repeated until every participant had experienced a two-month trial of each of the three POU products in random order.

In later survey waves (following the baseline), enumerators also shared information about water quality to a randomized subset of respondents. Assignment to treatment for the information campaign was randomized at the village level to minimize any leakage of effects across households between survey rounds. More details on the design of our information campaign can be found in Figure 1 and Table 1.

Our study's two-month product trials before measuring usage is longer than the exposure in many epidemiological studies on POU products (e.g., Crump et al. (2004) visit households weekly to document usage over a 20-week study period; Luby et al. (2001) visit 1, 3, 6 and 10 weeks after baseline; Quick et al. (2002) use weekly diarrheal surveillance and biweekly water testing; etc.). We believed that this would be enough time for households to fall into their "normal" (i.e., approximately long-term) behaviors. However, to observe usage at times participants could not anticipate and at lengths of product exposure less than the full two-month cycles, enumerators visited a randomly selected subset of 100 households for an unannounced, five-minute "spot check" in between each two-month product cycle. The final exit surveys were conducted in January and February 2009, at the peak of the long dry season. Figure 1 shows a time-line of the data collection.

3. Marketing and Information Interventions

We drew from the extensive decision-making literatures from economics, behavioral economics, and psychology to choose interventions that we felt were likely to affect POU usage.

Our use of behavioral economics heuristics to design marketing appeals joins a small but growing literature in economics. For example, Bertrand et al. (2009) finds that interventions drawn from behavioral economics have economically important effects on loan take-up in South Africa while Dupas (2009) finds smaller effects on purchase of insecticide-treated mosquito nets in Kenya.

These studies all examine the consumers *acquiring* goods or services, but health benefits also require consumers *using* the new products. Closer to our own study, Kremer et al. (2009) test the power of intensive social marketing appeals to increase adoption of WaterGuard in Kenya. The effects they find are positive, but too small to conclude that social marketing alone can lead to widespread purchase and use. Their research leaves open the question whether different social marketing approaches might have larger effects.

3.1 Framing

The literature offers competing hypotheses on whether framing POU adoption as a gain or a loss should bring about the larger response.

A subset of psychology research (e.g., Rothman et al. (1999)) suggests that a positive frame works best to change behavior. This view has become conventional wisdom among many social marketers. For example, the very large non-profit PSI (formerly Population Services International), the world's leader in social marketing, handles marketing and product distribution for both WaterGuard and PUR in Kenya and 20 other countries. Their manual on social-marketing best practices argues for positively framing messages:

> Consumers need to be inspired by the images and messages they see and hear and then aspire to create the same images in their homes. To create the aspiration, branded campaigns need to focus on the positive attributes of using the safe water solution. To get across the notion that the product can help protect children's health, campaigns must convey images of happy, healthy families that successfully use the product (PSI 2007).

However, due to loss aversion, gains relative to the status quo are often valued less than avoiding losses relative to the status quo (Tversky and Kahneman 1981; Kahneman and Tversky 1979). Moreover, some studies of behavior change argue for use of a "contrast" frame that first describes a problem (e.g., unsafe water) as a loss and then presents a solution (e.g., POU products), emphasizing the individual's ability to achieve the solution (Gass and Seiter 2007).

At each survey round, once the enumerators had introduced the POU products, they tested the role of framing by reading messages with either a positive or a contrast frame. Respondents in households assigned to the positive frame saw images of smiling children and a visibly clean glass of water as the enumerator read a script about what users stood to gain from regular use of a safe water product. The other respondents saw photographs of a crying child and a visibly dirty glass of water next to a smiling child with a visibly clean glass of water. The accompanying script began by emphasizing that the sad child had diarrhea from drinking contaminated water. It then became exactly the same as the positively framed message, emphasizing what the respondent stood to gain from regular use of a safe water product: clean water and a happy child.[4]

3.2 Consistency with Public Commitment

In psychology, the "commitment consistency" theory posits that people will go to great lengths to stay true to a commitment they have made in order to be—or appear to be—consistent (Greenwald et al. 1987; Cialdini 1993). This effect is strongest for commitments made in front of others (Cialdini 1993). There is also evidence that predicting one's own future behavior can influence that behavior (Cialdini 1993).

However, the strength of this commitment effect outside the laboratory remains inconclusive and may depend on the context.[5] Webb and Sheeran (2006) perform a meta-analysis of 47 randomized trials in psychology that test the ability of verbal commitments to influence a wide

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[4] Translations of the verbal scripts and accompanying images for both frames can be found in the online Appendix B. We gratefully acknowledge input on parts of the positively framed verbal script from members of the Rural Water Project (RWP) in Busia, Kenya and ideas from Meyerowitz & Chaiken (1987) and Block & Keller (1995).

[5] For example, Greenwald et al. (1987) find that a spoken commitment to vote has a positive effect on voting, but Smith et al. (2003) do not. Closer to our setting, Kremer & Miguel (2007) find no effect of adolescent respondents in Kenya committing to taking a deworming drug and Dupas (2009) finds no effect of a verbal commitment on subsequent purchases of mosquito nets.

range of behaviors. They find that a commitment increases participants' intentions to act, but (because not all intentions lead to action) has only small to medium effects on actions.

Some people who intend to act then forget to do so (Gollwitzer & Sheeran 2006). Thus, our commitment arm also included a reminder poster. Due to limited sample size, we were unable to test the effects of the reminder poster separately from the effects of the public commitment, so our estimated "commitment" effect also includes any increase from the reminder poster.

Enumerators implemented this treatment for a randomly chosen half of participant households. At the end of the interview after giving the household a new product, the enumerator first asked the respondent if she or he intended to use the assigned POU technology. The enumerator then asked the respondent to promise aloud to use the safe water product to keep their families healthy. This pledge was optional, but all respondents were willing to make it. The respondent was next asked to predict if she or he would be found to be using the safe water product two months later when the enumerator returned. At this point, the respondent was given a poster to hang in their homes as a reminder of the commitment. At the baseline visit, these photographic reminders were posters showing images of all three of the safe water products as well as images of smiling mothers and children. After the first two-month trial with a product, enumerators gave respondents a personalized poster showing images of the products as well as a photo of the respondent herself, taken by the enumerator two months earlier, at the end of the baseline interview. [6]

[6] Figure A.3 and A.4 in the online Appendix B shows the poster delivered to homes in the commitment treatment during the baseline visit and a personalized poster delivered at the first follow-up. We also delivered personalized posters to the "control" households at the final interview because the posters became valued in the community. We thank Clair Null for

The other respondents did not receive any additional messages or reminder posters.

3.3 Information on the Quality of Source and Own Water

Another possible cause for low adoption rates of household safe water technologies in developing countries is that people lack information on the hazards of contaminated water. Providing information about water contamination sometimes leads households to change how they collect or treat water (Madajewicz et al. 2007; Opar et al. 2007; Bennear et al. 2011; Jalan and Somanathan 2008). Most closely related to our study, Jalan and Somanathan (2008) find that informing urban Indian households about the safety of water stored in their homes increased self-reported safe water behaviors by 11-percentage-points among households that had not been treating their water. However, Davis et al. (2011) find that providing information increases self-reported treatment more than objectively measured treatment; thus, we collect both self-reported and objective measures of usage.

Further, Jalan and Somanathan (2008) share information only on households' stored supplies of drinking water, which may not be cost-effective to scale. Our study shares information on both the source water quality and the household's own water quality, so we can compare the relative effects of each.

At the first follow-up visit two months after the baseline interview, respondents in one-third (9) of the 28 villages were given detailed information about the quality of their water source (where they collect water), based on water quality tests. Respondents in another third (9) of the villages were given information about the quality of their source water and of their household's own drinking water, the latter having been collected at baseline in their own homes. A final third

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suggesting the poster.

(10) of the villages was not given the results of any water quality tests during this visit.[7] The source water treatment always communicated that the available source water was contaminated. The information on the household's own stored water communicated either a "safe" or "contaminated" result.[8]

[7] At the second follow-up visit, four months after the baseline interview, households that had received only source-water results two months before now also learned the quality of their stored water supplies, while the households that had not received any information were now told the quality of their source water. At this visit, water quality test results shared were from the two-month mark of the study, except for 66 homes whose tests showed contamination despite the household reporting use of their POU product. For these observations, households were given the water quality results from the baseline round. We made this clear to the respondents in order to avoid biasing true users of the products against a product that was performing at less than 100% effectiveness (all products perform at greater than zero effectiveness, but a product may reduce, for example, only 99% of contamination instead of 100%). Results are robust to the exclusion of these observations.

[8] In either case, the script emphasized that it was important to use the safe water product to ensure that one's water was safe. Also, ex ante, we anticipated the vast majority of baseline households to have contaminated drinking water (which was the case), but there were more "safe" results communicated for the "late" treatment groups that received information about the quality of their stored water from the first follow-up survey when all households had free safe water products. In section 6, we discuss in more detail how responses differ between a "safe" and "contaminated" personal water test result.

To our knowledge this is the first formal test of how households respond to information about source water quality in comparison to information about own water quality or no information at all. If information is a constraint to the adoption of safe water behaviors, then the type of information necessary to bring about individual-level behavior change matters. It is not feasible for merchants or local governments to test the quality of every household's stored drinking water repeatedly. But if sharing information on the quality of the source water can induce an equal response to sharing personalized information, it could be a more cost-effective way to increase point-of-use water treatment.

Our hypothesis for this set of randomizations was that the information about own water quality would spur greater POU product usage than the source water quality results as long as both types of information showed contamination, but that any information would induce greater POU usage than with no information.

4. Data Description and Summary Statistics

4.1 Water Collection and Measuring Product Usage

At each household visit, enumerators performed a variety of tests to measure water quality and product usage. They drew samples from a household's stored supply of drinking water, which we tested for fecal contamination, as indicated by the presence of *E. coli*. Our POU products can treat up to 20 liters of drinking water at a time, but villagers often collect more than that, so it was common to find both untreated and treated drinking water on hand in a household. In such cases, we drew samples from both. Enumerators also asked for self-reported POU product usage.

For the sake of tractability, we focus on three measures of product usage, although all results of the information and marketing interventions are robust to other measures. Our first indicator is self-reported product usage. Self-reports are comparable across all three products, but are likely to overestimate actual usage due to courtesy bias (Kremer et al. 2009; Jain et al. 2010). We also create an indicator for a household having no detectable *E. coli*; that is, less than 1 coliform forming unit [CFU] per 100 mL of water, a level which World Health Organization (WHO) guidelines categorize as indicating no risk of contracting illness from drinking water (WHO 1997). Our third definition of usage is a continuous measure: the natural log of the actual count (most probable number, or MPN) of *E. coli* (CFU/100 mL) in a household's drinking water (defined as a household's treated water if present; otherwise its untreated water). We code log(*E. coli*=0) as -1 and, for households with *E. coli* MPNs above the maximum detectable value of 2419.6, we substituted that maximum value. For this usage measure, smaller (or more negative) values imply greater usage.

Water treated with a POU product can have detectable *E. coli* if the product is not highly effective (usually a result of user error). Thus, all of the persuasion and information randomizations were implemented orthogonally to product assignments, so product efficacy should not affect these results. Additionally, we include product fixed effects in some models to control for differences in rates of product usage and of their effectiveness when used.

Conversely, if the source water is not contaminated and no recontamination occurs during household storage, then even untreated water may have no detectable *E. coli*. Thus, we compare measures of contamination in a household's treated drinking water to its pre-treated or source water.

4.2 Balanced Treatment Groups

Our study included many types of randomizations; all but the village-level information randomizations were implemented orthogonally to each other. The Appendix documents the overall effectiveness of our randomizations.[9] Additionally, while we did not anticipate interactive effects across the independently assigned randomizations, Appendix Table A1 presents cell sizes for each combination of randomizations (framing treatment, commitment treatment, information treatment, and assigned POU product) within each post-baseline wave.

4.3 Summary Statistics and Attrition

Table 2 presents baseline summary statistics of households included in the study. Most households (53%) rely on farming as their main income source and only 18% of respondents report an education level beyond primary. Average household size is about six people, and 89% of respondents are female.

Average water quality at baseline is poor. 86.5% of household stored water samples taken during the baseline survey (from rain water, tap water, earthpan water, and river water) tested positive for *E. coli*. The median household at baseline had drinking water contamination levels that WHO classes as "intermediate risk" for contracting illness. As these baseline measures were taken toward the end of the longer rainy season, when rainwater was plentiful, it is likely that, for most of the year, untreated water quality is even lower.

Rates of reported diarrheal prevalence were also high at baseline; 42% of respondents reported that a child under five years old had suffered an episode of diarrhea in the preceding two weeks. Such high baseline prevalence was matched by a high rate of reported concern; a majority (55%) of respondents freely named diarrhea in their list of the three most problematic

[9] The village-level information randomized treatments had some baseline differences in values that were mitigated by the time of the introduction of this treatment. We discuss this in detail in the Appendix.

diseases affecting their district. Yet only 18% of respondents reported consistently boiling their drinking water (although 58% of respondents named "boil drinking water" as a method of diarrhea prevention). Furthermore, only 7% reported that their current drinking water was treated by another POU method, despite the fact that 98% of respondents had heard of at least one POU method (WaterGuard). We could detect chlorine in only 1.5% of homes at baseline. In sum—and as others have found (Kremer et al. 2009)—there is a missing link between concern for diarrhea and taking action to prevent it.

Over the eight months of the study, 30 of the original 400 households dropped out, resulting in an overall retention rate of 92.5%. Between each successive full round of surveys, retention rates were 97%, 98% and 98%, respectively.[10] By far the most common reason for a household to drop out of the study was migration to an urban area; thus, our results are most representative of a persistently rural population. Attrition does not appear to be related to a household's assigned product or to any other randomized treatment assignment.[11]

5. Estimation Strategy

5.1 Effects of All Persuasion and Information Interventions

To assuage fears that any individual effects we cite are not due to unintended interactions amongst the large number of crossed experimental arms in our study, we begin by estimating the effects of our marketing and information interventions in tandem before breaking them down into their components. Equation 1 presents an average treatment effect (ATE) estimator of the

[10] Figure 1 contains household counts for each round.

[11] Chi-squared test p-value is .16 on a probit regression that predicts dropout as a function of all treatment assignments; estimation not shown.

effects of all of the randomized treatments:

$$1. \quad Y_{iqtv} = \alpha_t + \alpha_q + \beta S_{v,t-1} + \delta O_{v,t-1} + \theta F_{i,t-1} + \pi C_{i,t-1} + e_v + \varepsilon_{iqtv}.$$

Y_{iqtv} is a measure of usage of product *q* by household *i* at time *t* in village *v*. $S_{v,t-1}$ is an indicator variable that equals 1 if households in village *v* received information about source water quality during a previous visit. $O_{v,t-1}$ is an indicator variable that equals 1 if households in village *v* received information during a previous visit about their own private stored supplies in addition to source water quality results, $S_{v,t-1}$. Thus, $O_{v,t-1}$ tests if the sharing of personalized water quality results affects outcomes above and beyond that realized by the sharing of common source water results. $F_{i,t-1}$ indicates that household *i* received the contrast framing treatment in a previous wave while $C_{i,t-1}$ indicates that household *i* received the commitment treatment in a previous wave. The α_t are survey-wave fixed effects to account for the staggered introduction of the information treatments. The α_q are product fixed effects to control for products having different rates of usage or effectiveness when used. We also include village random effects e_v to control for any differential time-invariant village characteristics. Due to the village-level assignment of the information treatments, we estimate disturbance terms ε_{iqtv} from equation 1 clustered at the village.

Because of the relatively small number of villages (28), we are concerned about over-rejection of the null hypothesis of no effect from our information treatments due to intra-village correlation in outcomes (Duflo *et al.* 2006). We therefore also estimate Equation 1 using a wild cluster bootstrap to check the robustness of our results (Cameron, Gelbach, and Miller 2008).

To test for differential effects of the personalized water quality information between households that received a “safe” or a “contaminated” personalized result, we also interact the

$O_{v,t-1}$ dummy from Equation 1 with an indicator for whether a household's personalized water test showed that its stored water was contaminated. But since household water quality is endogenous to household behavior, we interpret such results with caution.

5.2 Marketing Interventions

To isolate the effects of our marketing interventions on usage, we combine all waves after the baseline and estimate the base impact of our marketing treatments using univariate linear regression:[12]

$$2. \quad Y_{iqt} = \alpha + \beta_M T_i^M + \varepsilon_{iqt}$$

where Y_{iqt} is a measure of usage of product q at time t by household i and T_i^M is an indicator for a marketing treatment—either the framing treatment ($T^F = 1$ for "contrast" frame, 0 otherwise) or the commitment treatment ($T^C = 1$ if assigned, 0 otherwise)—or is an indicator of receiving the "full" marketing treatment of both messages ($T^M = 1$ for "contrast" frame and commitment combined, 0 if a household received neither treatment). Due to the randomized assignment to treatment for both interventions, all β_M should deliver unbiased estimates of their causal effects on product usage. We cluster the error terms ε_{iqt} in Equation 2 at the household to allow for correlated outcomes across survey waves for the same household and due to the household-level randomization of the marketing treatments.

[12] Results are very similar from logistic models when usage is a dichotomous outcome. For ease of interpretation, we present linear probability models.

6. Results

6.1 Effects of Providing Free POU Products

Table 3 presents several measures of product usage, averaging across all products, treatments, and survey rounds. All measures show large increases in rates of water treatment relative to baseline. As expected, self-reported usage is highest, with 72% of households self-reporting current use of their POU product two months after receiving it.

6.2 Results from Information and Marketing Interventions Combined

Table 4 contains the results of estimating Equation 1 for the main effects of all of the randomized treatments. Sharing source water quality information significantly increases POU product usage but sharing own water quality information does not increase usage further. The percentage of households with zero *E. coli* in their treated water increases by nearly 10 percentage points (significant at the 1% level), or about a 24% increase over the mean base value across the three POU products,[13] in response to the provision of source water quality information (column 1). Although the estimates lack precision, Table 4 shows that the additional sharing of personalized water test results does not further increase use.

The contrast frame and the commitment intervention both have point estimates suggesting small increases in product usage, though the estimates are not always statistically significant. We will examine the effects of the marketing treatments further after first considering the effects of the information interventions in more detail.

[13] In Table 4, the mean "base" values are from the first two-month follow-up survey in order to differentiate the effects of free product provision from the effects of the information and marketing treatments.

There is no evidence that those who learned that their household's own water is contaminated increased usage more than those who were told that their household's water was safe (column 2).

Respondents were seven percentage points more likely to self-report usage after they learned that their source was contaminated, although this effect is only significant at the 10% level (column 3). Sharing information about source water quality leads to a statistically significant 0.6 log reduction in a household's drinking water *E. coli*, which translates into approximately a 49% reduction in contamination levels (column 4). Although the point estimates for the personalized information are not always precise, they do not suggest any further beneficial effect after users learn that their village-level sources are contaminated.

Our results do not substantively change when we re-estimate Equation 1 using a wild cluster bootstrap procedure to account for the relatively small number of village clusters (28). In particular, we can reject at the 5% level or above the null hypothesis of no effect from the source information treatments for the rates of households with no detectable *E. coli* and the reductions in a household's drinking water *E. coli*, but not for the rates of self-reported users (results available upon request).

In sum, Table 4 suggests that the provision of village-level information can realize large effects on behavior. However, we find suggestive evidence that this effect is larger at the intermediate spot-check visits than the full two-month follow-up survey rounds (results available upon request). Although our study lacks power to detect trends in usage, it raises the possibility that a one-time village-level information campaign may be insufficient to achieve behavior change over the long term and that repeated exposures may be necessary.

6.3 Marketing Effects

Although the individual marketing treatments realize small effects on usage when we control for all other randomized treatments (Table 4), the nonparametric identification of effects is cleanest with the basic comparison of means (Freedman 2008). Table 5 contains results from estimation of Equation 2 to compare usage rates across marketing treatments at all follow-up survey rounds.[14]

In Panel A, we take advantage of the randomized and orthogonal assignment of the two marketing messages to compare the usage rates of the quarter of the sample that received only a positively framed message and no commitment intervention with the usage rates of the quarter that received both the contrast frame and the commitment intervention. The combined effects of these marketing treatments increase product usage by a statistically significant 13-32% across all measures of usage at all two-month follow-up surveys.

We next disentangle this overall "marketing effect" into its components.

6.3.1 Framing

About half of the total "marketing effect" can be attributed to the "contrast frame" message (Table 5, panel B). Although the effect is not statistically significant for self-reported usage (column 2), the sign and magnitude suggest that contrasting what one stands to lose from nonuse with what one stands to gain from use is more effective than focusing solely on the potential gains. Moreover, the point estimates are fairly large: contrast-frame households are nearly six percentage points more likely to have uncontaminated treated water at home two months later (p-value of 0.06), a nearly 15% increase over usage in households with only a positive frame

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[14] Table 5 excludes all intermediate spot-check observations, since there is no evidence that the effects of our marketing interventions dissipate between intermediate spot checks and two-month follow-up survey rounds (results not shown).

(column 1). Similarly, contrast-frame households had an average *E. coli* count in their drinking water that was 0.43 log points lower than that found in positive-frame households, which translates into 36% lower contamination levels (column 3).

The contrast frame improves treated water quality by increasing POU product usage, not by improving the quality of source water collected. Specifically, the quality of untreated water (unlike treated water) did not differ by frame (see column 4).

Although the hypothesis motivating the contrast frame was that loss aversion would spur greater action, the results in Table 5 are consistent with other interpretations, such as a model of limited attention in which a reminder of sickness adds salience to the treatment decision by causing people to consider the full spectrum of possible outcomes (DellaVigna 2009).

6.3.2 Consistency with Public Commitment

The commitment treatment increases all measures of usage, although not always by a statistically significant amount (panel C of Table 5). The commitment treatment increases the likelihood of a household having no detectable *E. coli* by six percentage points, about 15% more than in non-commitment homes (column 1, significant at 10%). Although we suspected that the estimated treatment effect would be inflated by courtesy bias for rates of self-reported usage, results with this measure do not differ substantially (column 2, significant at 10%).

Again, the effect of commitment appears to be due to increases in product usage, not shifts to safer water sources. That is, there is no detectable effect of commitment on untreated water quality (column 4).

6.4 Robustness Checks

To adjust for any random differences in baseline characteristics that might affect our findings, we ran multivariate regressions. Results for tables A2 and A3 were very similar (appendix). Results for the marketing treatments are also similar if we cluster disturbance terms at the village level to allow for correlation across households. Finally, results are similar if we estimate the effects of treatment at the household level with an ordered probit analysis of the number of the three products each household used over the course of the six month study (Appendix table A4).

7. Discussion

Our experiment considered the roles of information and marketing messages in increasing usage of free POU safe water products. We find positive and incremental effects from all of our treatments, although suggestive evidence that the effects of our information treatments may dissipate between the intermediate spot checks and full follow-up survey rounds.

All of our effects are measured at a maximum length of time of two months and we therefore cannot say with certainty what effects we would see in the long run. Nonetheless, we are cautiously optimistic on a few fronts. One, our results could potentially be incorporated into preexisting distribution and marketing models for these products at little to no cost. Consumable POU products such as WaterGuard and PUR are currently packaged and sold in units that are not meant to last more than two months. Two, the finding that two marketing interventions appear additive suggests that a marketing strategy that harnesses multiple behavioral principles might realize large effects. Finally, we find no evidence that households react more to personalized risk information once village-level water quality information has already been provided. This result could be very important, given the excitement over the potential of information campaigns to improve water treatment behaviors (Madajewicz et al. 2007; Opar et al. 2007; Jalan and Somanathan 2008) yet the uncertainty of how best to do so (Bennear et al. 2011; Edwards et al. 2000; Lucas et al. 2011).

In this paper we focused on the impacts of the informational and marketing treatments on usage of safe water products. Our study collected self-reports on diarrheal episodes at each two-month survey wave, yet our study was never powered to find health effects (and we do not; results available upon request).[15]

If we combine our findings with those from previous studies, we can roughly estimate expected health effects from the free provision of these POU products as well as the additional health benefits from greater usage due to the marketing and information treatments. Under reasonable assumptions, free provision of POU products to 1000 households for one year *without* using our preferred marketing messages would avert over 2000 cases of diarrhea and save 2.4 lives, for a total savings of 79 disability-adjusted life years (DALYs).[16] Assuming a cost of

[15] Ours is not the first study that fails to find a link between improved access to health-producing goods and subsequent health outcomes (Devoto et al. 2011; Tarozzi et al. 2010).

[16] Our baseline survey suggests roughly 6.9 diarrheal episodes per year per child and our experiment finds that either the marketing or information treatments increases usage from roughly 40% to roughly 50% . Assuming usage of a POU reduces diarrheal incidence by 40% (Waddington et al. 2009), free provision without our interventions would avert 2.1 diarrheal cases per year per recipient household. Free provision with marketing or information increases this figure to 2.6 diarrheal cases averted per household-year. Following Kremer et al. (2007)(footnote 19), if 1000 averted diarrhea cases prevent 1.16 child deaths, then free provision of POU to 1000 households would save 2.4 lives without marketing and 3 lives with marketing. Using a standard conversion from mortality and morbidity to DALYs, free provision to 1000 households saves 79 DALYs without marketing or 98 with marketing.

$4.10 per household-year (Clasen, 2008 Table 2.5b), regular free chlorine provision would cost $41.81 per DALY, far below the $150 per DALY which was the World Bank's 1993 standard for "highly effective."

Adding either our information or our marketing treatments to the regular free chlorine provision would increase those savings by roughly 20%, to three lives and 98 DALYs. On the one hand, these estimates are very rough and may not generalize to other settings. On the other hand, the marketing messages are almost free and provision of community-level water quality information is inexpensive. Thus, taking advantage of these results could be a cost-effective means to improve health outcomes.

Our results stand in contrast to those of Kremer et al. (2009) and Dupas (2009). Kremer et al. (2009) found short-run effects from their persuasion treatments to encourage use of WaterGuard in nearby Busia, Kenya, but found this effect to dissipate over many months. Dupas (2009) did not find that marketing messages increased purchase of mosquito nets in Kenya. It is hard to understand the causes of our conflicting results because we test different interventions in different contexts. We do not wish to overstate the importance or generalizability of our results, yet we also think that our results show that marketing can matter.

Our findings highlight the heterogeneity in consumer take-up of health prevention measures and the need for further research on more products in more settings. More research is also needed to explore what (if any) combinations of messages and interventions can change behavior reliably and consistently.

Our estimated benefits are actually too low because we include neither the medical costs saved (for both households and provider governments or NGOs) nor the time saved by avoiding illness.

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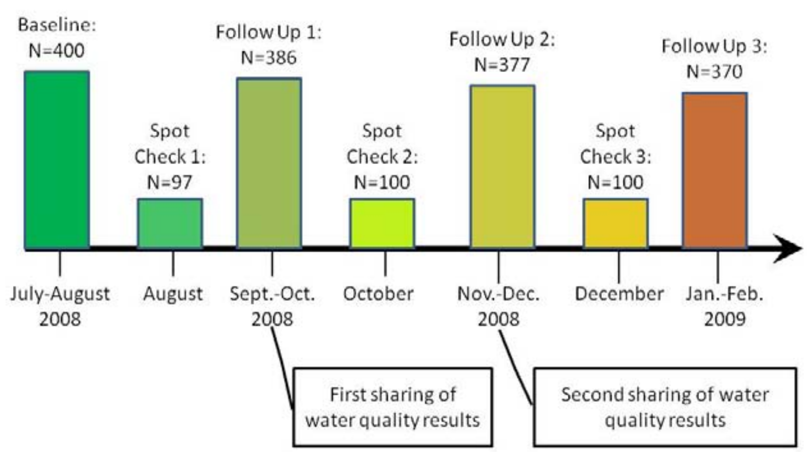
Figure 1: Timeline



Notes: More details on staggered introduction of information treatments in Table 1.

Table 1: Information-Sharing Design

	Baseline	Follow-Up 1 (2 months)	Follow-Up 2 (4 months)	Follow-Up 3 (6 months)
A. (10 villages)	No Information	No Information	Source Information	
B. (9 villages)	No Information	Source Information	Source + Own Information	
C. (9 villages)	No Information	Source + Own Information	Source + Own Information	

Timing of introduction of information interventions.

Table 2: Summary Baseline Means

Variable	Obs	Mean	s.d.
Baseline Water Quality Variables			
"Zero Risk" (No detectable *E. coli* in stored water)	377	0.135	0.342
"Low Risk" (E. coli MPN < 10 CFU/100 mL)	377	0.403	0.491
"Moderate Risk" (E. coli > 100 CFU/100 mL)	377	0.186	0.389
"High Risk" (E. coli > 1000 CFU/100 mL)	377	0.053	0.224
Baseline Respondent/Respondent Characteristics			
Female respondent	400	0.885	0.319
Married, with only 1 spouse	400	0.715	0.452
Household size	400	5.935	2.326
Some secondary education or above	400	0.183	0.387
Illiterate adult respondent	400	0.113	0.316
Respondent reports farming as main income source	400	0.525	0.500
Iron roof indicator	400	0.625	0.485
Water and Hygiene Knowledge and Behaviors			
Respondent reports child < 5 had diarrhea in past two weeks	400	0.423	0.495
Respondent lists diarrhea in top 3 diseases of concern	400	0.555	0.503
Current water source is tap water	400	0.303	0.460
Current water source is rain water	400	0.543	0.499
Current water source is earthpan	400	0.113	0.316
Current water source is river	400	0.025	0.156
Respondent has heard of WaterGuard	400	0.983	0.131
Respondent has heard of Pur	400	0.893	0.310
Respondent has heard of filter	400	0.360	0.481
Respondent has used WaterGuard	400	0.450	0.498
Respondent has used Pur	400	0.405	0.492
Respondent has used filter	400	0.008	0.086
Respondent names boiling as means of diarrhea prevention	400	0.580	0.494
Respondent reports always boiling their water	400	0.178	0.383
Respondent self-reports POU usage	400	0.073	0.260
Positive chlorine test	400	0.015	0.122
Roundtrip water collection time	400	29.900	27.153
Baseline: Respondent thinks source water is "safe" without treatment	400	0.415	0.493
Follow Up 1: Respondent thinks source water is "safe" without treatment	385	0.067	0.251

23 households had no stored drinking water on hand at baseline. Current water source is source for water stored at baseline interview.

Table 3: Base Usage Rates across Multiple Measures of Usage

	(1)	(2)	(3)
	No detectable *E. coli*	Self-reported usage	Ln *E. coli*
Baseline	0.128	0.073	2.719
	(0.020)	(0.001)	(0.120)
All Products	0.41	0.715	1.241
	(0.010)	(0.001)	(0.090)
Post-Baseline Obs.	1133	1133	1077

Notes: Spot checks omitted. Standard errors in parentheses clustered at household. Column 3 is a continuous measure of usage that calculates the natural log of *E. coli* in "drinking water" (treated water if present, else untreated water). Water with no detectable *E. coli* was assigned a ln(*E. coli*) value of -1. More negative values of ln(*E. coli*) imply higher rates of product usage.

Table 4: Results of Information and Marketing Treatments on POU Product Usage

	(1)	(2)	(3)	(4)
	Treated E. coli < 1	Treated E. coli < 1	Self-Report	Ln *E. coli*
1=received source info	0.098	0.100	0.071	-0.633
	(0.035)***	(0.035)***	(0.037)*	(0.175)***
1=received own info	-0.010	0.018	-0.036	-0.125
	(0.035)	(0.044)	(0.042)	(0.183)
1=received own info & "dirty"		-0.041		
		(0.058)		
1=received contrast frame	0.054	0.054	0.033	-0.316
	(0.032)*	(0.032)*	(0.025)	(0.176)*
1=received commitment	0.039	0.038	0.040	-0.131
	(0.028)	(0.027)	(0.022)*	(0.153)
Mean (Post Baseline) Dep. Var	0.412	0.412	0.642	0.844
	(0.05)	(0.05)	(0.05)	(0.23)
Spot Check Observations Included	Yes	Yes	No	Yes
Village Random Effects	Yes	Yes	Yes	Yes
Chi-squared	619.66	619.8	3970.31	184.84
Observations	1830	1830	1816	1357
No. Clusters	28	28	28	28

Standard errors in parentheses clustered at village. *p<.10, **p<.05 ***p<.01. Results from estimation of Equation 1 for effect of all randomized treatments combined. All models include survey wave and product dummies as well as village random effects. Columns 1 and 2 define usage as treated water with no detectable *E. coli*. Column 3 indicates share of households self-reporting treatment. Column 4 is a continuous measure of usage that calculates the natural log of *E. coli* in “drinking water” (treated water if present, else untreated water) and excludes baseline observations. Water with no detectable *E. coli* was assigned a ln(*E. coli*) value of -1. More negative values of ln(*E. coli*) imply higher rates of product usage. Mean of dependent variable gives base rate of usage across three products at first follow-up survey wave.

Table 5: Product Usage Rates by Marketing Treatment

	(1)	(2)	(3)	(4)
	Treated *E. coli* <1	Self-Report	Ln *E. coli*	Untreated *E. coli* < 1
Panel A: Both Marketing Treatments				
1=received contrast frame & commitment	0.112	0.089	-0.62	-0.024
	(0.04)***	(0.04)**	(0.25)**	(0.03)
Constant	0.347	0.663	1.568	0.126
	(0.03)***	(0.03)***	(0.19)***	(0.02)***
Observations	568	568	539	568
Panel B: Framing Treatment				
1=received contrast frame	0.056	0.040	-0.431	-0.021
	(0.03)*	(0.03)	(0.17)**	(0.02)
Constant	0.382	0.683	1.450	0.13
	(0.02)***	(0.02)***	(0.13)***	(0.01)***
Observations	1133	1133	1077	1133
p-value Wald test	0.059	0.166	0.012	0.317
Panel C: Commitment Treatment				
1=received commitment	0.056	0.050	-0.190	-0.003
	(0.03)*	(0.03)*	(0.17)	(0.02)
Constant	0.381	0.678	1.337	0.125
	(0.02)***	(0.02)***	(0.12)***	(0.01)***
Observations	1133	1133	1077	1133
p-value Wald test	0.059	0.084	0.269	0.874

Standard errors in parentheses clustered at household. *p<.10, **p<.05 ***p<.01. Results from OLS estimation of Equation 2 for both marketing treatments combined (panel A), framing (panel B), and commitment (panel C). Panel A includes only those households that received neither or both marketing treatments. Baseline (pre-treatment) and spot checks omitted. For all panels, column 1 defines usage as a household's treated water having no detectable *E. coli*, column 2 indicates share of households self-reporting treatment, and column 3 is a continuous measure of usage that calculates the natural log of *E. coli* in "drinking water" (treated water if present, else untreated water). Water with no detectable *E. coli* was assigned a ln(*E. coli*) value of -1. More negative values of ln(*E. coli*) imply higher rates of product usage. Column 4 presents results for the share of households with untreated (pre-treated) water having no detectable *E. coli*, similar to the definition for treated water in column 1.

Appendix A: The Three Included POU Products

WaterGuard

The U.S. Centers for Disease Control and Prevention (CDC), together with the Pan American Health Organization, developed the Safe Water System (SWS) in response to the need for an inexpensive and simple intervention that delivers clean drinking water to the poor in developing countries. The SWS involves three components. One, contaminated water is treated with a sodium-hypochlorite solution (marketed in Kenya as WaterGuard). Two, water should be stored in a proper manner to prevent recontamination. This generally means containers with a narrow mouth, lid, and spigot, so that people's hands do not come into direct contact with the water. Three, educational and behavior change techniques should be implemented to establish a link between contaminated water and disease and to encourage improved personal hygiene and water storage practices as well as regular treatment of water. The SWS arguably has been the most widely implemented POU measure in developing countries and the subject of the greatest number of randomized controlled studies to establish its efficacy in combating diarrheal illness. These studies largely agree on SWS's ability to reduce overall diarrheal incidence as well as the incidence in children less than five years old (S. Luby et al. 2001; Crump et al. 2004; R E Quick et al. 2002; Makutsa et al. 2001). Moreover, an overview study of the cost-effectiveness of various interventions found SWS to be the most cost-effective intervention aimed at improving water and sanitation (Hutton and Haller 2004). SWS is also found to be appropriate and effective in a variety of settings with a variety of source water qualities (Mintz et al. 2001).

To use WaterGuard: Add one capful of solution into 20 L of water (the standard jerrycan size). If the water is turbid, add two capfuls. Stir the water briefly and then let rest for 30 minutes before drinking.

In conjunction with a free bottle of WaterGuard, our study provided 20 L buckets with covers and taps. This was done to prevent recontamination and thereby make this product more directly comparable to the filter, which includes safe storage in its product design.

Ceramic Filters

A variety of field studies have documented the efficacy of ceramic water filters in reducing diarrheal incidence in a variety of developing country settings (Clasen et al. 2005; Lantagne 2001). However, the efficacy of filters has been found to be lessened in settings with turbid source waters, which slows the filtration process (Brown and Sobsey 2006).

There are currently many different styles of ceramic filter designed to treat water at the household level. For this study, we used Stefani ceramic candle-shaped water filters. The filter design consists of two 20 L buckets stacked one on top of the other. Untreated water is poured into the top bucket and gravity causes the water to flow through the Stefani porous ceramic filters into the bottom bucket, which then dispenses cleaned water through a tap. Thus, the use of a filter involves just one step for households—filling it with water.

Pur

Manufactured by Procter & Gamble, PUR is a flocculant-disinfectant powder produced in single-use sachets that clean 10 L of water at a time. Since its introduction in 2003, a growing number of field trials have documented its efficacy in cleaning water and reducing diarrheal morbidity in a variety of settings (Crump et al. 2004; Chiller et al. 2006). PUR is particularly effective at cleaning turbid water; its flocculant powder is capable of turning brown water clear.

Using PUR involves considerably more steps than using the other two POU measures. One needs to add one sachet to a bucket containing 10 L of water, stir the water briskly for five minutes, wait five more minutes to let the impurities settle, use a cotton cloth to filter the water into a separate storage vessel, let that set for 20 minutes until it is clean, and properly dispose of the residual impurities.

Together with a two-month supply of PUR, our study provided two buckets with covers—one with a tap for safe storage and one without a tap for the preparation process. Again, this was done to allow PUR homes to have safe storage and thereby make this product more directly comparable to the filter.

Water Testing Procedures

We tested (a) source waters, (b) stored untreated water, and (c) stored treated water for turbidity, *E. coli*, and free chlorine residual (in treated water samples in which either PUR or WaterGuard had been used). Turbidity testing was performed using a portable turbidimeter (Model 2100P, Hach Company, Loveland, CO). In heavily contaminated waters, *E. coli* measurement was conducted using Petrifilm *E. coli*/coliform count plates (3M, St. Paul, MN). In samples anticipated to have lower (<3000 CFU/100 ml) concentrations, we used the Colilert Quantitray-2000 assay (IDEXX Laboratories, Westbrook, ME). Free-chlorine residual was measured using othotolidine (OTO) test kits (ILP/Swimline, Edgewood, NY).

Balanced Treatment Groups

Of the 55 baseline household descriptive variables compared across each individual-level randomized treatment assignment of the first product assigned, the framing message received, and the commitment treatment received, 54 (98%) balance (p-value > .1 for F-test of equality of means) across first product assigned, 52 (92%) balance (p-value > .1 on t-test for equality of means) across frames, and 53 (96%) balance (p-value > .1 on t-test for equality of means) across commitment treatment status. Furthermore, all baseline variables describing a household's water quality and collection habits balance across the individual-level randomizations. We therefore feel confident that our individual-level randomizations were effective.

The village-level information randomizations had more pre-treatment differences (as expected with the much smaller N). In general, wealthier, more educated villages were assigned to receive the information treatments first. However, this treatment was implemented during the two-month follow-up survey round, after all households had had a free POU product for two months. At this follow-up survey round, all variables describing household water quality and product usage are balanced (p-value > .1) across information treatment groups. Arguably, any upward bias that may result from staggering wealthier villages into the information treatment first are attenuated by the timing of this treatment. We include village random effects in our estimations of the effects of information to adjust for the clustered nature of this intervention.

Table A1: Randomization Cell Sizes

			WaterGuard		Pur		Filter	
			No Commit	Commit	No Commit	Commit	No Commit	Commit
Follow-Up 1	No Info	Positive Frame	11	11	11	12	10	13
		Contrast Frame	9	13	12	9	10	12
	Source Info	Positive Frame	8	12	12	10	13	8
		Contrast Frame	12	8	8	13	10	9
	Source + Own Info	Positive Frame	13	10	10	9	9	12
		Contrast Frame	11	12	12	10	11	11
Follow-Up 2	No Info	Positive Frame	11	12	10	13	11	11
		Contrast Frame	10	12	9	13	11	9
	Source Info	Positive Frame	12	10	13	8	7	12
		Contrast Frame	10	8	12	8	7	13
	Source + Own Info	Positive Frame	8	9	9	12	13	10
		Contrast Frame	11	10	9	12	12	10
Follow-Up 3	No Info	Positive Frame	10	13	11	11	11	12
		Contrast Frame	10	7	10	12	8	13
	Source Info	Positive Frame	13	8	7	12	12	10
		Contrast Frame	7	13	10	7	12	8
	Source + Own Info	Positive Frame	9	12	13	10	8	9
		Contrast Frame	11	10	11	10	9	11

Baseline and spot checks omitted. Numbers indicate number of household-survey wave observations that received a given combination of treatments during a given survey wave. Information treatment categorizations are identified by a household's initial information group assignment due to the staggered introduction of this treatment (see Table 1).

Table A2: Marketing Multivariate Results with Baseline Controls

	Framing		Commitment		Both Marketing Treatments	
	(1)	(2)	(3)	(4)	(5)	(6)
	Treated *E. coli* < 1	Ln *E. coli*	Treated *E. coli* < 1	Ln *E. coli*	Treated *E. coli* < 1	Ln *E. coli*
1=received contrast frame	0.049	-0.416				
	(0.029)*	(0.169)**				
1=received commitment			0.056	-0.204		
			(0.029)*	(0.171)		
1=received contrast frame & commitment					0.109	-0.622
					(0.041)***	(0.250)**
Female dummy	-0.002	0.118	0.003	0.088	0.052	-0.351
	(0.051)	(0.255)	(0.049)	(0.248)	(0.060)	(0.323)
permanent roof indicator	0.005	0.090	0.007	0.076	-0.002	0.164
	(0.031)	(0.181)	(0.032)	(0.183)	(0.044)	(0.256)
Age	0.001	-0.008	0.001	-0.008	0.002	-0.019
	(0.001)	(0.007)	(0.001)	(0.007)	(0.002)	(0.009)**
Household size	-0.016	0.124	-0.016	0.114	0.014	-0.054
	(0.020)	(0.104)	(0.020)	(0.103)	(0.025)	(0.140)
Sq. household size	-0.000	-0.003	-0.000	-0.002	-0.002	0.006
	(0.001)	(0.006)	(0.001)	(0.006)	(0.001)	(0.007)
Constant	0.436	0.769	0.439	0.654	0.158	2.455
	(0.129)***	(0.660)	(0.124)***	(0.657)	(0.147)	(0.923)***
Observations	1133	1077	1133	1077	568	539

Standard errors in parentheses clustered at household. *p<.10, **p<.05 ***p<.01. Results from OLS multivariate estimation for framing (columns 1 and 2), commitment (columns 3 and 4), and both marketing treatments (columns 5 and 6) controlling for baseline characteristics. All models include a series of education dummies for the household head. Columns 5 and 6 include only those households that received neither or both marketing treatments. Baseline (pre-treatment) and spot checks omitted. Odd columns define usage as treated water with no detectable *E. coli*; even columns present a continuous measure of usage that calculates the natural log of *E. coli* in "drinking water" (treated water if present, else untreated water). More negative values imply more intense usage.

Table A3: All Treatments Combined with Baseline Controls

	(1)	(2)
	Treated *E. coli* < 1	Ln *E. coli*
1=received source info	0.084	-0.681
	(0.04)**	(0.25)**
1=received own info	-0.030	-0.105
	(0.06)	(0.25)
1=received contrast frame	0.052	-0.308
	(0.03)	(0.18)*
1=received commitment	0.034	-0.137
	(0.03)	(0.15)
Female dummy	-0.004	0.217
	(0.05)	(0.22)
Permanent roof indicator	-0.000	0.135
	(0.03)	(0.20)
Age	-0.001	-0.003
	(0.00)	(0.01)
Household size	-0.021	0.073
	(0.02)	(0.11)
Sq. household size	0.000	-0.002
	(0.00)	(0.01)
Mean dep. Var	0.412	0.844
	(0.05)	(0.23)
Village Fixed Effects	Yes	Yes
Observations	1425	1353
No. Clusters	28	28

Standard errors in parentheses clustered at village. *p<.10, **p<.05 ***p<.01. Both models include survey wave and product dummies as well as a series of education dummies for household head. Baseline (pre-treatment) observations omitted. Column 1 defines usage as treated water with no detectable *E. coli*; column 2 is a continuous measure of usage that calculates the natural log of *E. coli* in "drinking water" (treated water if present, else untreated water). More negative values imply more intense usage with log(*E. coli*). Mean of dependent variable gives base usage across three products at first follow-up survey wave.

Table A4: Ordered Logit Results on Cumulative Usage due to Marketing Treatments

	(1)	(2)	(3)
	Treated *E. coli* < 1	Self-Report	Untreated *E. coli* < 1
Contrast Frame	0.344	0.240	-0.294
	(0.21)	(0.15)	(0.27)
Commitment Treatment	0.381	0.257	-0.008
	(0.19)**	(.15)*	(0.21)
Both Marketing Treatments	0.749	0.502	-0.315
	(0.24)***	(0.18)***	(0.38)

Robust standard errors in parentheses clustered at village. *p<.10, **p<.05 ***p<.01. Results in log-odds from ordered logit estimates of cumulative effects of contrast framed message, commitment treatment, and both marketing treatments combined on usage at all two-month follow-up survey rounds. Estimations are on the final survey wave of 370 households for the contrast frame and commitment treatments and on only those households that received both or neither marketing treatments in the bottom row (185 observations). Definitions of usage for each column can be found in the notes accompanying Table 5.

Online Appendix B: Marketing Visual Aids

Figure A1: Positive Frame

A rough English translation of the corresponding verbal script read aloud to respondents with this set of images is: “By using one of these safe water products, you will be more likely to have clean, safe drinking water, which can help to keep your child[ren] happy and healthy. Use of a safe water product can make it more likely that your days will be healthy, when you can get your important tasks done. And, treating your water makes it more likely that your children will be healthy so they can grow, attend school, and learn. A safe water product can help you to achieve a healthier life. A healthier life is a happier life."

Figure A2: Contrast (Positive + Negative) Frame

A rough English translation of the corresponding verbal script read aloud to respondents with this set of images is: “Here is a picture of a sad, sick boy from drinking dirty water like we have around here. Here is a picture of a happy, healthy boy. His mother is doing many things to ensure he is having a healthy life and is happy. You also have the strength and the ability to bring such happiness to your children if you provide them with treated water. Use of a safe water product can make it more likely that your days will be healthy, when you can get your important tasks done. And, treating your water makes it more likely that your children will be healthy so they can grow, attend school, and learn. A safe water product can help you to achieve a healthier life. A healthier life is a happier life.”

Figure A3: Baseline Commitment Poster

Households assigned to receive the "commitment treatment" were given this poster at the end of the baseline visit. They were also read an additional verbal script whose approximate English translation is: "Before I leave, I would just like to ask you one more thing. You've told me that your child[ren]'s health is important to you and that your child has suffered diarrhea before. Do you want to avoid diarrhea in the future? (WAIT FOR RESPONSE) Do you believe treating your water is important to make it safe to drink? (WAIT FOR RESPONSE) Do you intend to use your safe water product every day for all your children's drinking water to keep them healthy? (WAIT FOR RESPONSE) Will you please say to me, "I will use this safe water product to keep my family's drinking water safe." Finally, as an additional way to remind you to treat your water with your safe water product every day, I'm hoping you will accept this small poster as a gift. Will you hang this poster on the wall in your home to remind you to treat your water every day? Thank you." ENUMERATOR GIVE POSTER TO RESPONDENT.

Figure A4: Sample Personalized Commitment & Reminder Poster

Sample "personalized" commitment poster distributed to households that received "commitment treatment" at follow-up 1 interview.