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Choosing and Using Safe Water Technologies: Evidence from a Field Experiment in Kenya

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Choosing and Using Safe Water Technologies:
Evidence from a Field Experiment in Kenya

by

Jill Emily Luoto

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Committee in charge:

Professor Elisabeth Sadoulet, Chair
Professor Ethan Ligon
Professor David I. Levine

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Abstract

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This dissertation examines the decision-making of poor rural Kenyan households with respect to the adoption of point-of-use (POU) safe water technologies designed to expand access to safe drinking water in the developing world. Low-cost POU products such as chlorine and filters substantially reduce diarrhea, which kills two million children in poor countries each year. Nevertheless, POU products remain little used in many parts of the developing world, even when they are widely available at subsidized prices. This dissertation presents results from a six-month field experiment conducted in rural western Kenya that provided all participating households exposure to a variety of free POU products. The design of this study allows me to compare competing safe water products as well as to explore the primary factors that determine consumer preferences for water treatment.

In chapter 1 I consider relative consumer preferences for, and the use of, three competing POU products to understand the role of product design in adoption. My study cycled 400 households through three successive, randomly ordered two-month trials of three competing POU products. I find that households' stated preferences for products often deviate from their revealed usage behaviors. I find suggestive evidence that a product's market value plays a role in stated, but not revealed, preference. In particular, the cheapest of the three products, a liquid chlorine product branded as WaterGuard, was consistently used at the highest rates by households. Nonetheless, when households were asked to choose a six-month supply of one of the three products as a parting gift at the end of our study when all households had experienced all products, WaterGuard was chosen at the lowest rates. This divergence of stated and revealed preferences could have important implications for the scalability of all three POU products: If households will use what they won't choose (in a market setting) and vice versa, reducing disease and achieving market scalability may be two distinct problems to solve. Of course, these findings ignore the role of price, an issue we consider in chapter 3.

In chapter 2, I consider the common decision-making barriers to the adoption of any safe water product or behavior. I hypothesize that incomplete information and behavioral biases may constrain a household's decision to use a POU technology. To test these hypotheses, households were randomly assigned to receive the results of water quality tests, as well as marketing messages designed to appeal to well-known decision-making heuristics. Sharing water quality information increased water treatment by 8-13 percentage points, representing a 12-23% increase over base values. Social marketing messages that harnessed findings from behavioral economics increased water treatment by an additional 9-11 percentage points in total. In particular, framing safe water products as both increasing health and avoiding disease (not just increasing health) increased usage on the order of 4-6 percentage points. This finding is consistent with a story that loss aversion will spur greater action. A public commitment to treat water regularly had similar results, and even larger effects at households that showed "present-biased" responses to hypothetical questions about future payoffs.

Chapter 3 is split into two main parts. In the first part, I explicitly model and quantify the role of experience in changing households' valuations for POU technologies. I find that experience with a safe water product can increase households' stated willingness to pay for a safe water product, suggesting that these private health products are experience goods and offering potential insight into ways to increase demand for these products. The second part of chapter 3 considers various anomalies from the collected Kenyan data that were unexpected at the outset. I find that households are more likely to adopt a safe water technology when they receive an unannounced visit from a survey enumerator during a product trial, suggesting strong Hawthorne effects as well as a potential channel for social pressure to increase adoption. I also find that relative product preferences are strongly influenced by the order in which a household experienced a product, and these ordering effects do not fully dissipate with market experience. This finding does not support the neoclassical assumption of stable and innate preferences behind the rational model.

In total, this dissertation aims to better understand how households form preferences over safe water products as well as choose to adopt safe water behaviors. Its findings can offer promising avenues for incremental improvements in the market for safe water (and other) technologies. More broadly, it suggests that economic models of decision-making that fail to account for the accompanying psychology behind a decision can often fall short.

Dedication

To my parents, Randi and Dennis, who are the reasons I got here.
And to my husband and best friend, Peter, who made it all worthwhile.

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Chapter 1

Choose and Use

Point-of-use (POU) safe water technologies offer perhaps the greatest immediate potential to expand access to safe drinking water in the developing world. More than one billion people lack access to potable drinking water worldwide (WHO 2004). Inadequate access to safe water is a primary cause of the estimated two million child deaths from diarrhea that occur each year in poor countries (Zwane and Kremer 2007), making it the world's second[1] leading cause of under-five mortality (Gerlin 2006). Making water 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). Such problems have led to the development of many low-cost technologies that improve drinking water at the household, or point of use. Such POU measures range from solar disinfectant practices, filters and UV irradiation devices (where electricity is available), to disinfectants such as chlorine solution and tablets, as well as flocculant/disinfectant mixtures.

A number of randomized controlled studies have shown that such low-cost POU drinking water treatment measures can substantially reduce diarrheal incidence (Clasen, Roberts, Rabie, Schmidt and Cairncross 2006). Nevertheless, adoption rates of POU technologies remain low in many parts of the world, even when widely available. Although generally found to be microbiologically effective at cleaning drinking water and therein reducing disease, many of these findings are based on short-term and intensive field studies with frequent interactions between study staff and participating households. Questions as to the long-term acceptability and sustainability of POU technologies by the poor in more natural settings remain largely unanswered (Arnold and Colford 2007). The research frontier is now shifting from documenting the efficacy of each technology to understanding the consumer's decisions to adopt and use any safe water technology.

In economics, much recent work has focused on the appropriate role of charging positive prices for these technologies. Ashraf, Berry and Shapiro (2007) find that charging higher prices screens out non-users of a chlorine product in urban Zambia, while Kremer, Miguel, Mullainathan, Null and Zwane (2009) argue that charging any positive price drastically cuts demand for a chlorine product in Busia, Kenya. Both studies restrict themselves to consider the role of price for just one of many POU products, namely, a liquid chlorine product. Although there is not yet a complete consensus on the appropriate role of charging positive prices for this (and other) technologies,

[1]Excluding neonatal causes.

some argue to consider providing POU (and other preventative) technologies for free. Because POU adoption may include positive externalities due to reductions in the disease environment, freely providing such products may lead to societal benefits that outweigh their private market costs (Holla and Kremer 2009).

Yet similar to many other promising technologies such as condoms and bednets, the benefits of POU technologies accrue only if individuals make a private decision to use them. Moreover, many POU technologies are characterized by a relatively infrequent purchase decision but a repeated usage decision every time water is collected. Thus, overcoming budget constraints may be a necessary, but is not a sufficient, condition for POU technologies to lessen diarrhea's impact. While cost remains an obstacle, a larger obstacle appears to be achieving change in people's daily water-related behaviors (Zwane and Kremer 2007, Luby, Mendoza, Keswick, Chiller and Hoekstra 2008).

Whether a distribution model for POU technologies remains reliant on private markets or moves to one of free provision, understanding the factors that influence consumer preference and valuation for, and adoption of, POU technologies is important if they are to achieve large reductions in disease incidence as well as any sort of scalability among the poorest. Yet to our knowledge, there has been just one previous, non-experimental attempt to understand these questions. Sobsey, Stauber, Casanova, Brown and Elliott (2008) rank several available POU products targeted at low-income populations on the basis of their potential for scaled distribution and sustained adoption. However, their ranking is based on reviews of multiple other published materials; Lantagne, Meierhofer, Allgood, McGuigan and Quick (2009) critique the approach. At the same time, there has been little to no effort thus far to understand the factors that determine consumer preferences for POU products outside of controlled experimental settings.

This dissertation presents results from a six-month field experiment conducted in rural western Kenya that provided all participating households exposure to a variety of free POU products. The design of our study allows us to compare competing POU products as well as to explore the primary influences that determine consumer preferences for water treatment. We believe the comparatively relaxed data collection schedule of our study allows us to create knowledge that can be transferable outside of controlled experimental settings.

The rest of this chapter proceeds as follows. We next describe the study design and setting as well as introduce the three included POU products. In section 2 we describe the collected data. Section 3 presents results on product usage and consumer preferences, and section 4 discusses our findings and concludes.

1.1 Study Design Overview

1.1.1 Setting

We partnered with the non-governmental organization (NGO) CARE-Kenya to carry out a field study from July 2008 to February 2009 of 400 randomly selected households in 28 villages located within the Nyawita sublocation of Nyanza province, rural western Kenya. This part of Kenya is among Kenya's poorest regions and was chosen due to the area's seasonal reliance for drinking water on turbid earthpans, which are naturally occurring pools of surface water that often dry up between rainy seasons. Drinking water conditions vary tremendously throughout the year in this part of Kenya, but rainwater collection and reliance on public taps are higher quality and favored options when available.[2] Other available types of water sources include the Yala river that borders one side of Nyawita and the various earthpans that dot the landscape.

At the baseline interview, in July and August 2008, 86.5% of water stored by households (from rain water, tap water, earthpan water, and river water) tested positive for *E. coli*, an indicator of fecal contamination. WHO international drinking water standards recognize the presence of any *E. coli* in drinking water to constitute a nonzero risk of waterborne disease (WHO 1997). This period comprises the tail end of the long rainy season, when one might expect better than average water quality if households practice rainwater harvesting. Despite this, baseline rates of water contamination were high.

Rates of reported diarrheal prevalence were similarly high at baseline, when 42% (169 of 400) of homes reported that a child under five had diarrhea in the preceding two weeks. Such high baseline diarrheal prevalence was matched by a seemingly high rate of concern: A majority (55%) of respondents freely named diarrhea in their list of the three most problematic diseases affecting their district. Despite this, households do not appear to put much effort into diarrhea prevention: Just 18% (71 of 400) of households reported consistently boiling their drinking water (despite 58% of households being able to name "boil drinking water" as a method of diarrhea prevention[3]), and only 7% of homes (29 of 400) reported that their current drinking water was treated by another POU method, despite the fact that 98% of homes had heard of at least one POU method. POU treatment was verified (by a positive chlorine test) in just 6 of 400 homes (1.5%). In sum, there appears to be a missing link between concern for diarrhea and taking action to prevent it.

Such a pattern is not unique to Nyawita. Kremer et al. (2009) similarly find low usage of a chlorine product despite high product awareness in Busia, Kenya. However, in Busia, springs are a primary water source type for households, while in Nyawita there are effectively no springs, and the majority of households reported having rain or tap water on hand during the baseline interviews. Despite the different types of sources, baseline household water quality is comparably poor across studies.[4]

1.1.2 Experimental Design

Our field experiment began at the tail end of the long rainy season in July-August 2008 when enumerators visited 400 randomly selected compounds (a collection of households; Luo tradition allows for polygamous marriages) across the 28 villages comprising Nyawita. The sole selection criterion for inclusion in the study was the presence of a child under five in the compound (a census of Nyawita was first conducted to identify all eligible households). Enumerators requested to speak

[2]Rainfall patterns in Nyawita follow a bimodal distribution, with monthly average peaks of ~160 mm occurring in April and August, respectively. May-August constitutes a moderately dry period with monthly rains of ~100 mm in June. From September onward, precipitation drops steeply, with monthly average rains of less than 40 mm in January (Kenya Agricultural Research Institute (KARI) n.d.).

[3]This question was borrowed from Kremer, Leino, Miguel and Zwane (2007) and asked as an open response. Enumerators prompted respondents three times without suggesting answers.

[4]Rates of households with contaminated stored drinking water supplies at baseline are 86% and 86.5% in the two studies. Table 1.1 has more information on baseline water collection practices in Nyawita.

with the mother of the youngest child in the compound to conduct a baseline interview of present water and hygiene knowledge and behaviors, as well as prior exposure to any POU technologies. Enumerators then gave an educational script about the dangers of unsafe drinking water followed by detailed presentations on three different POU measures in randomized order: a liquid chlorine product branded as WaterGuard, Procter & Gamble's flocculant-disinfectant powder branded as Pur, and porous ceramic filters.

At the end of the baseline interviews, respondents were randomly assigned one of the three POU technologies for a two-month trial. Two months later, all households were revisited and asked about their updated preferences for the POU products and the quality of stored, untreated and treated water was tested in order to verify product usage. Respondents then were cycled through (in random fashion) one of the remaining POU products for a new two-month trial. This process was repeated until every participant had the opportunity to experience all 3 POU products, each for two months, in random order, for a total of six months of exposure to safe water products.

In between each two-month product cycle, a randomly selected subset of 100 households was subjected to an unannounced, 5-minute "spot check." These spot checks were intended to observe usage patterns at lengths of product exposure less than the full two-month cycles, as well as to check on POU product performance. It is worth noting that the majority of households had an uninterrupted two months to reveal their usage and preference patterns, as compared to the more frequent enumerator visits conducted in other published studies of POU product impacts on diarrhea morbidity. A complete time-line of data collection activities can be found in Figure 1.1. The final exit surveys were conducted in January and February, 2009, at the peak of the long dry season, for the 370 households that completed the study.[5]

1.1.3 The Three POU Products

All of the included POU products have been tested in numerous randomized controlled field trials and shown to significantly reduce contamination in drinking water in a variety of settings (Clasen et al. 2006). The randomized order of product assignments was achieved by printing the product assignments directly into surveys and preassigning surveys to households. Compliance by enumerators was easy to observe since they had to carry the assigned product and associated supplies to an interview. At all follow-up interviews, enumerators would collect leftover supplies of the previously assigned product while distributing the new product's supplies. All products were distributed along with covered buckets and taps in order to enable safe storage, which helps to minimize the chances for recontamination of treated water within the household. Brief introductions to the three included POU products of WaterGuard, Pur and a filter follow.

WaterGuard

The US 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

[5] This results in an overall retention rate of 92.5%. More details on attrition during the study can be found in section 1.2 when we describe the collected data.

SWS involves three components. One, contaminated water is treated with a sodium hypochlorite solution (marketed as WaterGuard in Kenya). 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 most 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 that of children less than five years old (Luby, Agboatwalla, Raza, Sobel, Mintz, Baier, Rahbar, Qureshi, Hassan, Ghouri, Hoekstra and Gangarosa 2001, Crump, Otieno, Slutsker, Keswick, Rosen, Hoekstra, Vulule and Luby 2004, Quick, Kimura, Thevos, Tembo, Shamputa, Hutwagner and Mintz 2002, CDC 2006, Makutsa, Nzaku, Ogutu, Barasa, Ombeki, Mwaki and Quick 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, Bartram, Lochery and Wegelin 2001).

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

WaterGuard is currently available in Nyawita and commercially distributed by the U.S.-based non-profit organization Population Services International (PSI) at a subsidized price of 20 Kenyan shillings (Ksh) (about ~$0.25 USD in July 2009) for one bottle, which should last a typical household for 1-2 months.

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, Parra, Boisson and Collin 2005, Lantagne 2001). However, the efficacy of filters can be lessened in settings with turbid source waters because it slows the filtration process (Brown and Sobsey 2006).

There are currently many different styles of ceramic filters designed to treat water at the household level. For this study, we used Stefani silver-coated ceramic "candle"-shaped water filters (Sterilaqua, manufactured by Cerâmica Stéfani of São Paolo, Brazil). These filters are distributed through a number of retailers in Kenya, though are not currently available in Nyawita. We produced a low-cost version of the filter device by employing locally sourced materials. 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 then 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, namely, filling it with water. However, the filtration rate of 1-2 L/hr can be slow, and further declines with the head loss of a dropping water level in the upper bucket, and varies with the turbidity level of the feed water. By comparison, the recommended wait-time for treatment using WaterGuard is 30 minutes for 20 L, while the Pur treatment process requires roughly 30 minutes for 10 L.

Pur

Manufactured by Procter & Gamble, Pur is a flocculant-disinfectant powder produced in single-use sachets that cleans 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, Mendoz, Lopez, Alvarez, Hoekstra, Keswick and Luby 2006). Pur is particularly effective at cleaning turbid water: Its flocculant powder is capable of turning brown water clear.

The use of Pur involves considerably more steps than the other two POU measures. It functions by adding one sachet of its mix to a bucket containing 10 L of water and then stirring the water briskly for 5 minutes. Next, 5 more minutes of waiting time are needed to allow the water's impurities to settle. Then, the water should be filtered using a cotton cloth into a separate storage vessel and left to set for 20 minutes until it is clean. Finally, the residual impurities from the filtration process need to be properly disposed of.

Pur is also commercially distributed by PSI in Nyawita. One sachet of Pur, which treats 10 L of water or about a two-day supply for a typical household, is sold locally at a subsidized price of 7-9 Ksh, depending on the local vendor (about $0.09-$0.11 USD in July 2009).

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

Product Comparisons

While the three included POU technologies all serve the same purpose of delivering safe drinking water at the household, we hypothesized that their different characteristics of usage would cause experience with them to affect consumer preferences differentially. Although our study does not allow us to be able to test for the marginal impact on usage for particular characteristics of each product, we can gain suggestive evidence of the factors that matter to consumers on a daily basis.

WaterGuard was the POU product most familiar to the respondents in our study at baseline. This could diminish the value of hands-on experience with WaterGuard relative to the other products if households do not have much to learn about WaterGuard's benefits. On the other hand, from a consumer's perspective, WaterGuard is arguably the easiest and quickest to implement.

Filters do not strongly alter the taste of the water unlike the other two products that include chemical treatment. Furthermore, filters are a durable good and this could benefit their initial perceived value. However, filters can be slow. If consumers face tight time constraints or have high discount rates, this could put the filter at a disadvantage during product trials and experience with the filter could negatively affect consumer valuations for it.

The comparatively complex preparation of Pur could put it at a disadvantage relative to the other POU technologies. If households find the labor costs involved in the preparation of Pur inconvenient and not worth the benefits of clean drinking water that it delivers, Pur may lose value through experience. Pur might initially impress consumers because it is capable of turning cloudy water clear, making it a visually attractive option in a setting such as ours with turbid source waters. However, if the value of this attribute loses significance over time, consumer valuations of Pur could further deteriorate with experience.

1.2 Data Description and Summary Statistics

1.2.1 Data Collection Procedures and Measuring Product Usage

At each household visit, enumerators performed a variety of tests to measure water quality and product usage. More details about water testing procedures can be found in Appendix A.1. At the baseline visit, samples of water were drawn from a household's stored supply of drinking water and tested for fecal contamination via the presence of *E. coli*. (If a household reported their drinking water as chemically treated such as with WaterGuard or Pur, a chlorine test also was performed and results recorded.) At all subsequent visits including spot checks, households gave self-reports of usage, and chlorine tests were performed at households assigned WaterGuard or Pur. At filter households, enumerators recorded their own observations about usage. Because the included products treat at most 20 L of water at one time, yet trips to collect drinking water often collect more than this amount, a common practice among respondent households was to have greater supplies of water on hand than just treated drinking water. This enabled the collection of both untreated (pre-treated) and treated (post-treated) stored supplies of drinking water. All samples were tested for *E. coli*.[6]

A primary outcome of interest for this dissertation will be measures of usage of the safe water products. In Section 1.4 we will also offer approximate expected health and cost effectiveness estimates due to free POU product provision, but our study did not expressly collect measures of health. Rather, we rely on findings from numerous existing field studies from the epidemiological and public health fields that have shown that (1) POU measures are effective at cleaning drinking water and thereby reducing diarrheal incidence (Clasen et al. 2006); (2) clean drinking water reduces diarrheal incidence significantly (Zwane and Kremer 2007); and (3) reductions in diarrheal episodes lead to positive health outcomes, particularly for children under five (Jones, Steketee, Black, Bhutta, Morris and the Bellagio Child Survival Study Group 2003).[7]

We construct several different definitions of product usage, each with its own advantages and disadvantages. All conclusions presented here are robust to the various definitions of use. Where

[6]Fewer samples were collected when pre-treated or post-treated supplies were unavailable.

[7]Besides death, risks of diarrheal illness include dehydration and malnutrition, which in turn can lead to diarrhea in a negative cycle. One estimate is that 25% of the growth differential between children in developing and developed countries is due to diarrhea ((Black, Brown and Becker 1984) as cited in (Mirza, Caulfield, Black and Macharla 1997)). Furthermore, diarrheal episodes can imply missed days of school or work for the ill and their caretakers.

appropriate we will attempt to distinguish between POU product usage and other behaviors that product assignments could affect such as water collection. First, we rely on self-reports of product usage. Self-reports should be comparable across all three products, but are likely to overestimate actual usage due to courtesy bias. Another definition labels a household a "user" if its sample of treated drinking water meets some threshold level of *E. coli* contamination. We consider levels of zero, and less than 10, coliform forming units (CFU) of *E. coli* per 100 mL of water.[8] Zero *E. coli* is the WHO internationally recommended level for no risk of contracting illness from drinking water, and *E. coli* contamination levels less than 10 CFU/100 mL qualify as "low risk" according to WHO guidelines (WHO 1997). However, such definitions fail to take into account the quality of the corresponding pre-treated drinking water, and they arguably incorporate product efficacy into their definitions, independent of household behavior. Despite these potential drawbacks, we can compare these measures to "counterfactual" measures of usage by considering the contamination levels of corresponding untreated (pre-treated) samples to check if it is POU *usage* that is the behavior affected by product assignments. This can also provide suggestive evidence if product assignments affect water collection behaviors. A final definition of "usage" is an indicator for whether a household's treated water tested negative for contamination and pre-treated water tested positive for contamination. This is a clear indication of (competent) usage, but excludes those households for which we lack both pre-treated and post-treated samples and furthermore classifies "incompetent" users as nonusers.

In addition to these dichotomous measures of product "usage," we construct two continuous measures of usage. One is the natural log of the actual count (Most Probable Number, or MPN) of *E. coli* CFU/100 mL in a household's "drinking water" (we define "drinking water" to be a household's treated water if present or, if only untreated water is on hand at the time of the interview, we label it as the household's drinking water).[9] The second continuous measure of usage calculates the *change* in the natural log *E. coli* between a household's treated and pre-treated water. For those households with treated water on hand but no pre-treated water, we use predicted values for the quality of pre-treated water. Specifically, we impute the pre-treated water's Ln *E. coli* based on out-of-sample predictions from a model of untreated Ln *E. coli* on a series of village, survey wave, and source type dummies and their interactions for those households with both pre-treated and treated water on hand. For those households with only pre-treated water on hand, we assign a difference of zero. For these two continuous measures, smaller (more negative) values imply greater usage and/or cleaning performance.

1.2.2 Data Description and Tests of Randomizations

1.2.2.1 Household Summary Statistics

Table 1.1 contains baseline summary statistics of households included in the study. Most households (53%) rely on farming as their main income source, and just 18% of respondents report an education level beyond primary. 70% of households report having a latrine or toilet structure at

[8]For convenience, throughout the paper we consider an *E. coli* measurement of <1 CFU/100 mL to be zero. Our tests for *E. coli* are not able to detect *E. coli* contamination levels below <1 CFU/100 mL, but this satisfies WHO drinking water quality guidelines.

[9]For cases with zero *E. coli* counts, we substitute -1 for their log values in order not to drop these observations.

baseline and 63% have an iron roof (with the remainder being thatch). Households report feeling liquidity constrained, with 59% reporting it to be "very difficult" or "impossible" to get 500 Ksh (~\$6.25 in July 2009) by tomorrow. Average household size is about 6 people, and 89% of respondents are female.[10] The average household spends 30 minutes per trip to collect water.

At baseline, the vast majority of households had heard of WaterGuard (98%) and Pur (89%), while there is some knowledge of filters (36%). Rates of previous usage of the products is much lower, at 45% for WaterGuard, 40% for Pur and <1% for filters. Similarly, reported rates of previous purchases of any of the products is somewhat low at 40%, 18% and 0% for WaterGuard, Pur, and the filter, respectively.

1.2.2.2 Water Quality Summary Statistics

Table 1.2 contains information about household water quality at baseline. 86.5% of household stored water samples taken during the baseline survey tested positive for *E. coli* and therefore posed a nonzero risk for contracting waterborne illness. According to WHO guidelines on the relative risk for waterborne illness posed by different levels of *E. coli* coliform forming units, the median household at baseline had drinking water contamination levels that put them at intermediate risk for contracting illness.

Table 1.4 contains information about water quality by source type. Surface waters (earthpan and river) have significantly more *E. coli* (means of 716 and 595 CFU/ 100 ml, respectively) than harvested rainwater and standpipe (tap) water (means of 231 and 248 CFU/100 ml, respectively; difference statistically significant at $p<0.001$). The median is substantially lower than the mean for *E. coli* in rainwater and tap water because of right skewness. Table 1.4 shows that surface waters tend to be of lower quality, but no source type has such high quality water as to pose no risk of causing illness.

A household's decision for where to collect water is likely to be a function of expected collection time, and distance and expected water quality from the various available sources. All of these decision variables in turn will depend on the season. Accordingly, Table 1.3 reveals a complex pattern of water collection behaviors by households over the course of the study. At the baseline survey, conducted during the rainy period of July-August 2008, 49% of households reported relying on lower-quality surface water (river, earthpan, wells or springs) sources when asked for their "main" drinking water source. However, when the same respondents were asked in the same survey where they had collected their currently stored water, only 15% listed surface water sources, and over half the respondents listed rainwater (as compared to 8% who reported rainwater as their "main" source).

As the study progressed through the local climate cycle, rainwater harvesting increased to 68% of the respondents in Follow Up wave 1 (conducted in September-October 2008) and then began to decline with the onset of the dry season, as only 30% of the households reported collecting rainwater in the final Follow Up wave during the dry season of January-February 2009. Meanwhile, surface water sources for current drinking water increased in prevalence to 30% of households by the final survey wave.

The type and quality of source from which a household collects its water is potentially highly

[10] In 11% of households, a male respondent was interviewed if no adult female was available.

related to its decision whether to use a safe water product, but may also be endogenous to product assignment. We will try to account for source water quality and source type when comparing performance and usage of the included POU products.

1.2.2.3 Tests of Balance Across Randomized Product Asssignments

We do not present comparisons of equality across all three product assignments. Rather, we summarize by saying that for a total of 55 baseline household descriptive variables compared across the randomized first product assigned, 54 of the 55 (98%) baseline descriptive variables are balanced (p-value > .1 for F-test of equality of means). Furthermore, all baseline variables describing a household's water quality and collection habits balance. We therefore feel confident that this randomization was effective. Table A.1 in the Appendix contains a list of the variables that do not balance across all randomized treatments in the study.

1.2.2.4 Sample Size and Attrition

To detect differences in rates of product usage of 10 percentage points with 80% power and 95% confidence required a sample size of approximately 100 households per product-trial for a total of 300 households. We sampled 133 households per product-trial to allow for attrition that might occur over the seven months of study.

Over the total 8 months duration 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.[11] By far the most common reason for a household to drop out of the study was migration to an urban area, and therefore our results are most representative of a persistently rural popluation. Attrition does not appear related to a household's assigned product or other randomized treatment assignments.[12]

1.3 Results

1.3.1 Base Impacts of POU Product Provision on Usage and Water Quality

Identification of the impact of product type on usage relies on the randomization of product assignments, which were orthogonal to seasonal effects. We therefore begin by presenting nonparametric mean comparisons to identify the effects of product assignments on usage.

Mean rates of POU product usage vary by the definition of product "use," but all measures show improvements over baseline measures of water quality. Table 1.5 summarizes usage results across user definitions and products. All rates of usage in Table 1.5 are from water samples taken approximately two months after a product has been assigned to a household (i.e., excluding spot checks). Standard errors take into account the repeated observations of households at subsequent visits.

[11] Figure 1.1 contains actual household counts for each round.

[12] Chi-squared test p-value is .16 on a probit regression that predicts dropout as a function of all treatment assignments; estimation not shown. Other randomized treatments are dicussed in chapter 2.

As expected, self-reported usage is highest, with 72% of households self-reporting current use of their POU product two months after receiving it across all follow-up survey rounds and products. Combining all study waves, households are more likely to self-report usage of WaterGuard (77%) and the filter (75%), relative to Pur (62%; p<0.001 on three-way Wald test of equality; column 4 of Table 1.5). The difference in self-reported usage between WaterGuard and the filter is not statistically significant (p=0.50).

Figure 1.2 tries to gauge the reliability of self-reported usage by comparing water quality among those self-reported users whose usage can be confirmed (via a positive chlorine test among households with WaterGuard or Pur) with those households whose usage cannot be similarly confirmed (i.e., households assigned WaterGuard or Pur who self-report usage but have a negative chlorine test). Figure 1.2 excludes households assigned a filter (since no chlorine tests were performed at these household visits) and plots nonparametric locally weighted regressions of a household's "drinking water" log *E. coli* as a function of its untreated water's log *E. coli*. The short dashed line presents results for the confirmed "non-using" households, those who fail to self-report usage. Their drinking water quality approximately follows a 1-1 relation to the quality of their untreated water, consistent with non self-reporting users not using their POU products. Meanwhile, the solid line has results for the subset of "confirmed users," those with a positive chlorine test. Their drinking water quality remains quite high (with low *E. coli*) despite increasing levels of contamination in untreated water - the POU products (WaterGuard and Pur) are effectively cleaning water and self-reports are capturing true usage of them. Finally, the long dashed line has results for the "unconfirmed users" - those who self-report usage yet have a negative chlorine test. Their drinking water quality lies between these two bounds, although slightly closer to that of the "confirmed users." Figure 1.2 is suggestive that self-reports are a reasonably reliable measure of usage, although upwardly biased.

WaterGuard dominates all objective measures of usage. Across all water sources and all survey waves, column 1 of Table 1.5 shows that 51% of WaterGuard households had stored treated drinking water samples with no detectable *E. coli* (*E. coli* concentrations <1 CFU/100 mL). These same households, when provided Pur, had *E. coli* concentrations <1 CFU/100 mL 33% of the time (p<0.001 on test of equality with WaterGuard), and when provided filters, 39% of the time (p<0.001 for Wald test of equality with WaterGuard and p=0.14 for test of equality with Pur). A similar pattern exists when usage is defined as households having "low" *E. coli* (concentrations <10 CFU/100 mL (column 2)), when usage is defined as households having contaminated pre-treated water and uncontaminated treated water (column 3), and the continuous measure of usage for a household's drinking water's log *E. coli* (column 5). In column 6 we consider product-specific definitions of usage. For WaterGuard and Pur, usage is defined as a positive chlorine test, and WaterGuard continues to dominate (44% versus 31%, p-value=0.0001 on two-way test of equality). For the filter, usage is defined as the enumerator observing filter usage. Although this column suggests the filter is being used at much greater rates (76%), this definition of usage is likely upwardly biased for the filter (in hindsight enumerators should have verified product usage by looking inside of filters; in practice they only checked whether the filter was present and appeared in use). Meanwhile, it is likely downward biased for WaterGuard and Pur due to the dissipation of chlorine over time. Thus, we hesitate to draw direct comparisons for this definition of usage across products.

WaterGuard's superior performance arguably could be a combination of greater usage and higher product efficacy. In an attempt to separate out these two factors, we briefly consider usage among two different subsets. One, the "confirmed users" of all products - those households that have either a positive chlorine test (with WaterGuard or Pur) or observed filter usage. And two, the subset of self-reported users of the products. Although both constitute nonrandom subsamples (with sample sizes of 165, 117 and 288 for "confirmed users," and 291, 235 and 284 for self-reported users of WaterGuard, Pur and the filter, respectively), each is drawn from the same larger group of 400 households and arguably such findings can be informative. In such an exercise, among "confirmed users," 75% of WaterGuard households have no detectable *E. coli* in their treated water, while 62% of Pur and 49% of Filter households do (p=0.000 on three-way Wald test of equality; results not shown). For the subset of self-reported users, corresponding numbers are 63% for WaterGuard, and 49% for both Pur and filter households (p=0.000 on three-way Wald test of equality; results not shown). This could suggest that WaterGuard is not only being used at greater rates, but it is also outperforming the other two products.

It is possible that households respond to their product assignment with differential water collection practices, and perhaps this allows WaterGuard to appear to have greater cleaning performance. To check for this, we compare the quality of pre-treated water as well as the difference in quality between pre-treated and post-treated water by product. Table 1.6 contains the results of such an exercise. Column 1 offers suggestive evidence that households collect higher quality water in response to receiving the filter as opposed to either of the other two products: 36% of filter observations had pre-treated water quality with "low" *E. coli* (MPN <10 CFU/100 mL), while for WaterGuard the corresponding rate was 30% and it was 28% for Pur (p-value=0.021 on three-way Wald test for equality). A similar pattern remains when we consider a continuous measure of quality by looking at the natural log of *E. coli* MPN in a household's pre-treated water in column 2, although we cannot reject the null hypothesis of three-way equality across produts (p-value = .16). However, these findings do not suggest that WaterGuard's overall superior performance is due to *it* benefitting from cleaner input water.

Although it does not appear that households collected cleaner water in response to receiving WaterGuard, it is still possible that households adjust the *types* of sources from which they collect water in response to their assigned product. We initially hypothesized that Pur might be more attractive than WaterGuard to households reliant on lower quality source waters due to its ability to turn turbid water clear. To test if households respond to Pur with differential collection practices, we estimate a multinomial logit model of the following form:

$$SourceType_{ipt} = \alpha_p + \alpha_t + \varepsilon_{ipt} \tag{1.1}$$

where the set of choices for values of the dependent variable includes tap water, rain water, earthpan water or "other" source type, α_p are product fixed effects and α_t are survey wave fixed effects for household i. Errors are clustered at the village level to allow for correlated source type dependence by village. We will further test whether households respond to Pur by changing their water collection behaviors by estimating:

$$Y_{ipt} = \alpha_p + \alpha_t + \varepsilon_{ipt} \tag{1.2}$$

where Y_{ipt} is either an indicator for household i visiting a "low quality" source (earthpan, river, or "other" surface water source) in response to receiving product p at time t, or Y_{ipt} is the number of minutes a household reports spending collecting water. Again, we cluster error terms at the village level.

Results of estimation of equations 1.1 and 1.2 are presented in Table 1.7. Columns 1-4 present marginal effects for selecting each source type (tap water, rain water, earthpan water, and "other" surface water) from multinomial logit estimation of equation 1.1. A Chi-square test rejects the null hypothesis of equality across product assignments with p=0.0001, suggesting that household choice of source *type* responds to its assigned product. Column 5 contains marginal effects of product assignment from logit estimation of equation 1.2 where the dependent variable is an indicator for households reporting a low quality source (river Yala, earthpan or other surface water source). Again, there is evidence that households respond to receipt of Pur by visiting lower quality source types: a Chi-square test rejects the null of equality between Pur and WaterGuard assignments with p=0.02, and a test of three-way equality across all products has p=0.054. However, column 6 contains results from OLS estimation of equation 1.2 with a dependent variable of average round-trip water collection times. An F-test fails to reject a null hypothesis of equality between the three products (p=0.448). Also, Wilcoxon signed rank tests fail to reject null hypotheses of equality between median turbidity levels and median *E. coli* levels of pre-treated water among WaterGuard and Pur households (not shown). In sum, even if households respond to receipt of Pur by visiting a lower quality *type* of source, it does not appear that this results in any time savings on their part nor significantly lower pre-treated water quality. This somewhat puzzling result is likely due to the lower overall usage rates of Pur relative to the other two products (among the subsets of self-reported users of WaterGuard and Pur, a similar Wilcoxon signed rank test of median pre-treated water *E. coli* levels between the two products rejects a null of equality with p=0.014).

Meanwhile, a simple difference in the rates of pre-treated and treated water samples having "low" *E. coli* (differencing column 1 of Table 1.6 from column 2 of Table 1.5) would continue to suggest that WaterGuard is the product that is used at the highest rates. Column 3 of Table 1.6 tells a similar story; it calculates the *change* in the natural log *E. coli* between a household's treated and pre-treated water. WaterGuard has a greater log redution in *E. coli* concentrations than the other two products (p-value of 0.016 on three-way Wald test).

1.3.1.1 Sustained Usage

Because of the small size of our enumerator team relative to the sample size and frequency of visits, each set of household visits was staggered over several weeks. This, combined with the spot-check visits made to a subset of households during each product rotation, enables us to examine the relationship between time exposure to products and various measures of product usage. In Figure 1.3, we present results of a nonparametric locally-weighted regression of the fraction of homes having treated and untreated water with E. coli concentrations <1 CFU/100 mL, across all three products tested and all three product cycles. Thus, seasonality and relative product performance do not affect results.

The probability of treated water E. coli concentrations <1 CFU/100 mL were highest immediately after the household received the POU product, at over 60%. This probability drops steadily over the first month of exposure to the product, and then stabilizes at approximately 40% at the follow-up surveys. Meanwhile, the probability of non-detection of E. coli in untreated water fluctuates between 9% and 18%. At any given point in the studied time period, treated water is at least

2.9 times more likely to exhibit E. coli <1 CFU/100 mL than untreated water. (However, these differences are statistically significant only at their means and not along their entire distributions, because of the design of our initial power calculations. We therefore interpret with caution.)

Figure 1.3 can be interpreted as either good news or bad. The good news is that the subset of households that learn they like using their POU products appears to stabilize by around the second month of exposure, and an overall usage rate of 40% or more (if the measure of no detectable *E. coli* is too conservative) across three products is quite high compared to baseline values. The bad news is that within the first month there appears to be significant drop-out of users as households either learn they do not like treating their water or tire of the behavior. Troublingly, Arnold and Colford (2007) find that reductions in diarrhea risk decline over time in a systematic meta-analysis, with study durations ranging from 10 to over 80 weeks. The maximum duration with a product that we report here is about 10 weeks. Thus, further deteriorations in usage are possible when products are no longer provided for free outside of an experimental setting.

1.3.2 Product choices

While the higher usage rates of WaterGuard relative to the other two products would suggest a revealed preference for it, households' stated preferences largely disagree. Figure 1.4 presents households' self-reported product preferences at baseline and at each of the subsequent two-month follow-up surveys as they accumulate experience with the products.

Filters were reported most preferred at both baseline (45% of homes) and after trying each product (44%). The fraction of households identifying Pur as their preferred choice remained fairly constant over the first four months of the study, with 16% at baseline, 17% at Follow Up 1, and 20% at Follow Up 2. The fraction increased to 35% at the final visit. Meanwhile, 34% of households identified WaterGuard as their preferred choice at baseline, but only 21% at the final visit. This decline occurs in spite of the higher usage rates of WaterGuard relative to the other two products.

Our baseline survey was conducted at the end of the short rainy season while the final survey was conducted in the middle of the long dry season. Thus, any changes in preferences from baseline to the exit survey confound experience with the shifting seasons. Despite this, usage rates of WaterGuard are consistently higher than the other two products yet at no point does WaterGuard finish atop the list as the most preferred product.

Although the value of stated preference measures is debated among economists (see a comparison of stated and revealed preference measures in Wardman (1988)), in this case stated preferences may be very important if they best represent the purchase decisions a consumer may make in a market setting when deciding between competing POU products. While both our measures of usage and stated preference necessarily ignore price (and WaterGuard is the cheapest of the three POU products included), this could be discouraging news for WaterGuard's market viability that depends on repurchase due to its consumable nature.[13]

On the other hand, of the three included products, WaterGuard's consistently higher usage rates combined with its lowest production costs would suggest that *if* a policy of free provision of POU products were to be enacted, it would be most likely realize the greatest impact at lowest cost with

[13]In chapter 3 we look at households' willingness to pay for the three POU products in more detail.

WaterGuard. However, we hesitate to extrapolate our findings based on a short-term study on just 400 households in one area of Kenya to such a widescale policy recommendation, and caution that further tests of consumer preferences in other locales are warranted.

1.3.2.1 Choice Experiment

In effort to check the reliability of our stated preference measures of product preference, at the final household visit after all households had experienced each product in turn, we offered each their choice of a filter, 100 sachets of Pur, or three bottles of WaterGuard. Importantly, the Pur option included two buckets, one with a cover and tap and a second for the preparation process of Pur, and the WaterGuard option included a single safe storage bucket with a cover and a tap. The quantities of WaterGuard and Pur provided homes with access to improved drinking water for approximately 6 months.

We also offered households an outside health good, soap, in case they did not care for any of the POU products. Bars of soap are a commonly purchased item among these households and the same bar of soap is often used for washing dishes, bathing, and cleaning. We therefore anticipated it to hold value among respondents. The results of this choice are presented in Figure 1.5 and highly correlate with our stated preference measures at this final wave (set of correlations between stated most preferred product and chosen product at exit is $\rho \in \{.64, .79, .73\}$).

In this final choice, Pur did even better compared to WaterGuard, coming in a close second to filters at 40% versus 44% (we fail to reject a null hypothesis that they were chosen at the same rate with p = 0.36), while WaterGuard fared even worse than with the stated preference measures, with less than 15% of households choosing it from among the products and a two-month supply of soap.

It is important to note that we equalized the total access to safe water across the three products in giving out approximately 6 months' worth of supplies of Pur and of WaterGuard. However, this approximate equalization of days meant the market values of the three POU choices were markedly different, with WaterGuard worth approximately $0.80 plus one bucket compared to Pur's value of $9.33 plus two buckets and the (used) filter's value of approximately $10-12. The filter would typically treat well over six months worth of water (barring breakage).

1.3.2.2 Relation between usage and product choices

Filters are the only product where there appears to be a strong relationship between users and choosers of the filter. Table 1.8 presents results comparing previous usage of the three products by their final product choice in the choice experiment at the final survey wave when all households are "fully experienced" consumers who have had access to all three products. Columns 7-9 compare usage rates of the filter across households that chose the filter as their final parting gift and those that did not. Usage of the filter is higher among choosers of the filter across all three definitions of usage at 95% confidence or greater based on t-tests. Meanwhile, in columns 1-6 we fail to reject the null hypothesis of equal usage rates across choosers of WaterGuard and Pur for all definitions of usage. It is perhaps not surprising that choosers of Pur at the final exit survey were not found to be using Pur when previously assigned to it if they had Pur during a rainier season with less turbid waters. However, it may be somewhat discouraging to see this divergence between choice and usage of WaterGuard. We discuss the final choice results in greater detail in the next section.

1.4 Discussion and Conclusions

Despite its superior performance and usage rates, WaterGuard is not the product most preferred or most frequently selected by households in our study. Conversely, filters are not the most effective of the products, but they are the most popular. Several households referred to filters as "water points" in the local language.

The objectionable taste and odor of chlorinated water is sometimes identified as a limit on adoption of chlorine-based products such as WaterGuard and Pur, and this would normally be a reasonable explanation for the popularity of the filters. Yet chlorine odor and taste did not prevent WaterGuard from being used at greater rates than the other two products in the study.

We asked households to explain their reasons for naming a product as their most preferred, and 67% of those that preferred the filter did so because it was "easiest to use" (across all waves), while less than 4% cited the filter's superior taste and odor. At the same time, of those households that ranked WaterGuard or Pur as their *least* preferred product at a household visit, 20% of the time objectionable odor and taste was listed as a reason, while 27% of the time difficulty-of-use was mentioned, 24% of the time a failure to remove turbidity, and 20% of the time the duration of the treatment process was mentioned.

By including buckets with each of the product offerings in the choice experiment, we attempted to control for the advantage that the filter units possess as a durable good (as opposed to the consumables, WaterGuard and Pur). Even so, the highest number of households selected the filter (although we cannot reject equality between the rates at which filters and Pur were chosen). This result could suggest that their aspirational characteristics may exceed those of the chemical products, even when WaterGuard and Pur are offered with buckets. Another important caveat with respect to the final choice is that many users likely recognized that with proper care, a filter unit would function for far longer than the six month supply offered with Pur and WaterGuard. On the one hand, 52% of the 163 households that chose the filter listed as one of their reasons: "It will last." On the other hand, 90% of filter-choosing households also listed "easiest to use" as a reason for their choice.

We initially hypothesized that the increase in preference for Pur (at the expense of WaterGuard) between baseline and exit could be a function of changing seasonal water supply conditions. Specifically, rainwater harvesting declined as the study progressed out of the rainy season into a dry period, and, thus, there was an increasing reliance on the turbid waters for which Pur was designed. Indeed, the fraction of households reporting their stored water to be from a low quality source (earthpan or the Yala River or other surface sources) did increase from 15% at baseline to 30% at exit ($p<0.0001$ on test of equality). Meanwhile, rainwater declined from 54% to 29% over this period (see Table 1.3). Moreover, Table 1.7 suggests that product assignment affects household choice of source type.

We now suspect a combination of factors bearing on Pur's relative popularity at exit, including the changing water supply conditions, Pur's second bucket, a social desirability bias that prevented households from choosing the outside option of soap, and an awareness among the study population of the local pricing of Pur and WaterGuard. Both products are available in commercial retail outlets in Bondo town, and the market price of the Pur and WaterGuard products offered in the choice experiment are approximately \$9.33 and \$0.80, respectively. It is difficult to measure how the higher market value of both Pur and its second bucket (perhaps coupled with the possibility of resale) led respondents to choose Pur. Meanwhile, though we produced the filter units from locally available buckets, the filter elements themselves are available only in Kisumu (some 60 km away), and, thus, information on the filter price was not available to study participants. Instead, they would have to estimate product resell values themselves.

Impact on Health

If we combine our findings with those from other studies, we can roughly estimate expected heatlh effects from the free provision of WaterGuard (the cheapest to provide and the product used at the highest rates). In particular, our baseline survey found that 46% of households reported a child less than 5 having diarrhea in the previous two weeks. This translates into approximately 6.9 diarrheal episodes per child-year,[14] a number close to that found by Kremer et al. (2007) in their baseline survey in Busia, Kenya. We assume an overall average rate of usage of 51% of freely provided WaterGuard (approximate mean rate of households with no detectable *E. coli* across all two-month follow-up surveys with WaterGuard). Given these figures and an average reduction in diarrheal incidence from use of a POU product of 40% (Clasen et al. 2006),[15] the base effects of free provision would be to avert 2.7 diarrheal cases per household-year (averaged across users and non-users of freely provided POU products). Making the strong assumption that reductions in diarrheal incidence lead to proportional decreases in child mortality, this translates into 3.1 child deaths averted per 1000 households per year.[16] Using a standard conversion from mortality and morbidity to DALYs, this amounts to 100 DALYs averted (from both mortality and morbidity averted) per 1000 households per year. Our estimate of the cost effectiveness of the intervention would be \$52.26 per 1000 household-years.[17] These estimates are far below traditional figures used to calculate cost-effectiveness of health interventions (World Bank (1993) uses a rule of thumb that \$150 per DALY averted is "extremely cost effective"), and do not include medical costs saved (for both households and governments or NGOs), nor the time savings of avoiding illness. Of course, we stress that our study was not designed to calculate health effects and these estimates are for illustrative purposes only.

[14] The average number of under-5s per household is 1.9 in our baseline survey.

[15] This figure is consistent with the 33% lower rates of self-reported diarrhea we find when comparing self-reported users versus self-reported non-users of products in our study. We use the estimated effectiveness of 40% to avoid the endogenous nature of our own estimates; the estimate of 40% is approximately the average estimated effectiveness from Clasen et al. (2006) across the three included POU products.

[16] We borrow the estimate from Kremer et al. (2007) (footnote 19) that 1.16 child deaths are averted for every 1000 diarrhea cases averted.

[17] Using the estimate from Clasen (2008) that it costs \$4.10 per household per year to provide WaterGuard (Table 2.5b).

Conclusions

This is the first study of which we are aware to compare preference and usage of competing POU products that target the developing world poor. Our focus on product adoption reflects our concern that many microbiologically effective POU products have had difficulty being adopted by large numbers of users. POU product dissemination at scale to the poor will not occur until we better understand the preferences and behaviors of the at-risk populations.

Like most field experimental results, the external validity of our findings is subject to question. Towards this end, we are in the process of replicating our study in the urban slums of Dhaka, Bangladesh. Primary water sources in Dhaka's slums are municipal taps, and levels of contamination are very high among participant households. In our Dhaka study, we have a different mix of POU products but similar tests of relative consumer preference and usage. We hope that similarly rigorous investigations will occur in other regions, using other study designs, examining longer time periods, and testing other products.

Figure 1.1: Time-line of Data Collection Activities

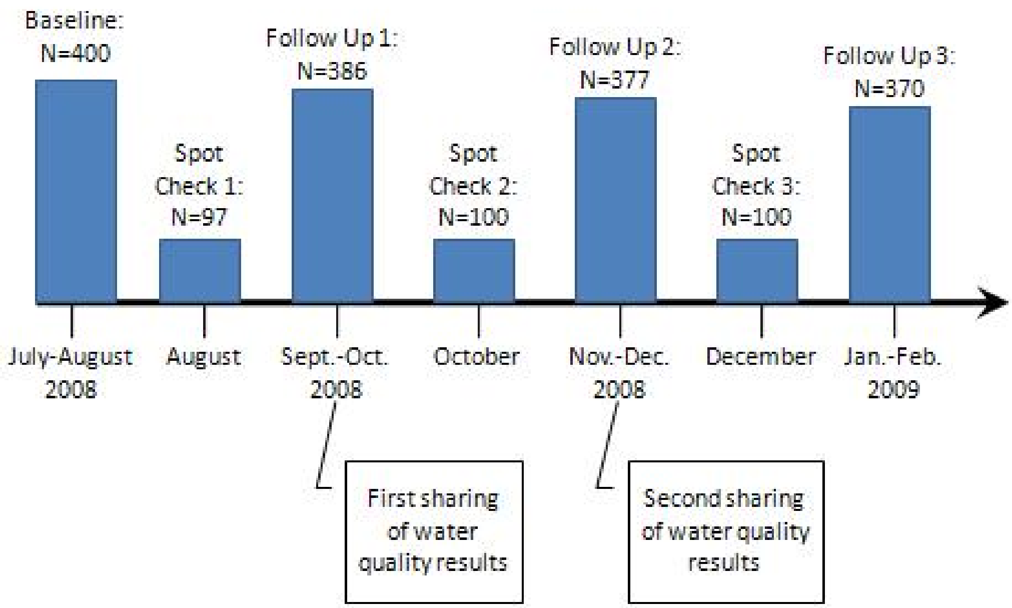
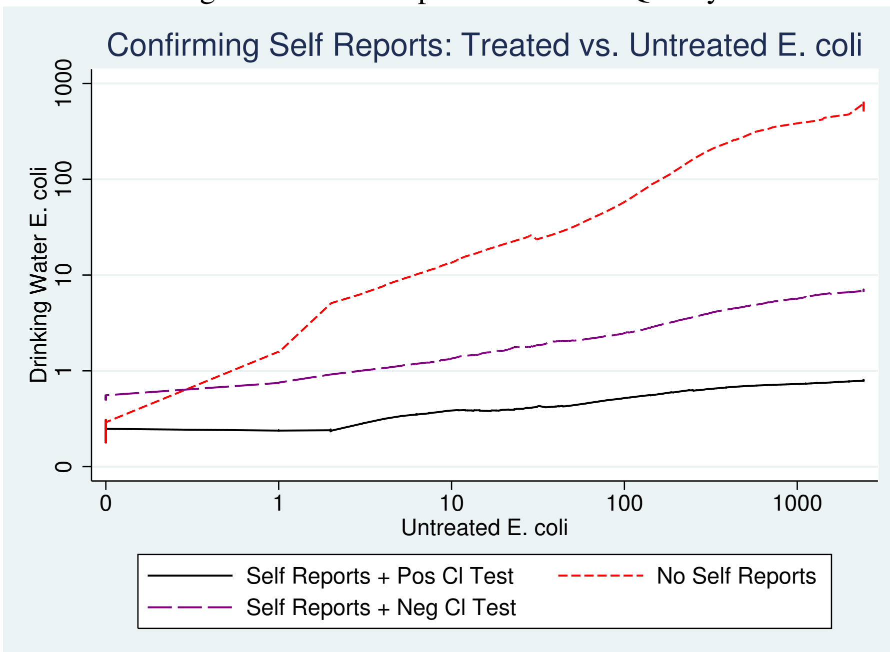
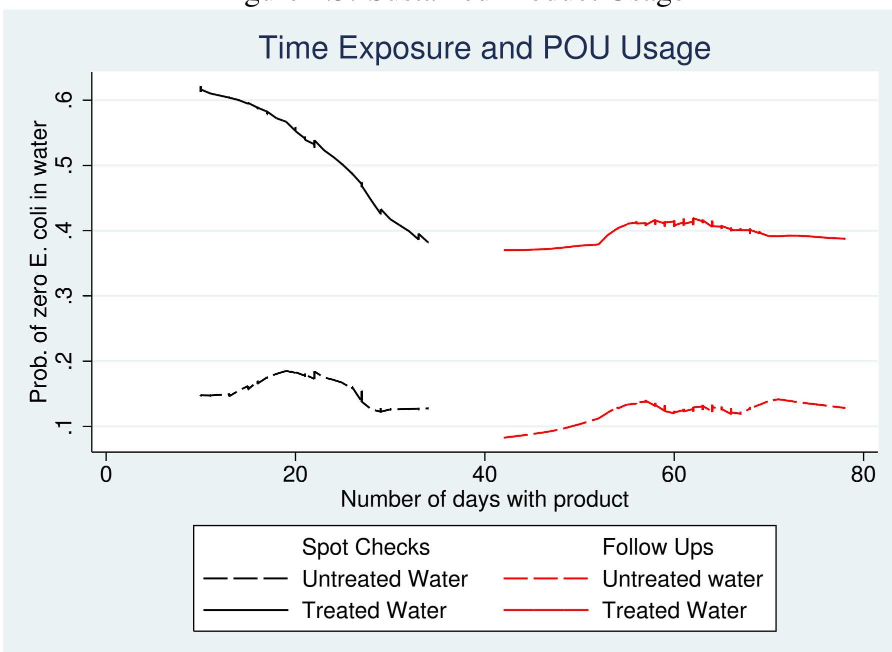


Figure 1.2: Self Reports and Water Quality



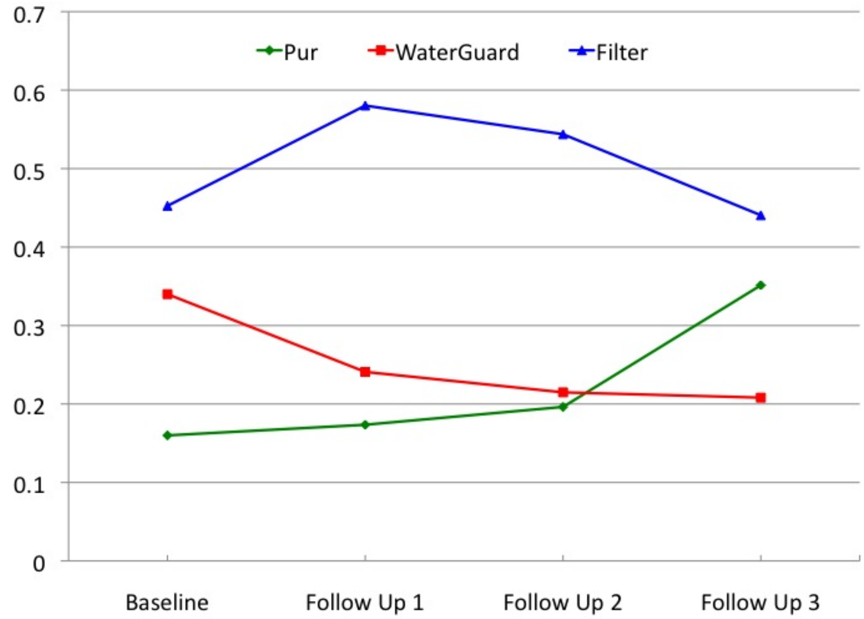
Nonparametric locally weighted (lowess) regression of the log *E. coli* in households' "drinking" water (treated if present, else untreated) on untreated household water quality, separately for confirmed users (households that self-report usage of WaterGuard or Pur and have positive chlorine test), unconfirmed users (households that self-report usage of WaterGuard or Pur and have negative chlorine test) and confirmed non-users (households that fail to self-report use of WaterGuard or Pur). Log scales on axes. Filter observations excluded. Bandwidth=.8.

Figure 1.3: Sustained Product Usage



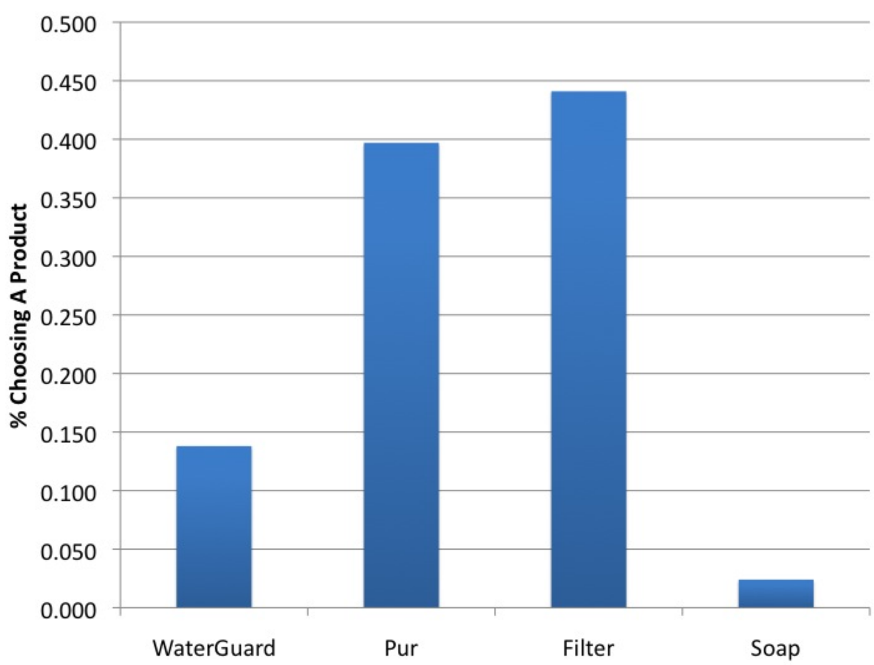
Nonparametric locally weighted (lowess) regression of fraction of households with treated water having no detectable E. coli as a function of the number of days of exposure with a POU product. Averaged over all survey waves and all products. Spot checks and follow-up surveys presented separately; the gap between them is due to the lack of data around that length of exposure. Bandwidth=0.8.

Figure 1.4: Product Preferences by Survey Wave



Share of households reporting each product as their most preferred at each survey wave.

Figure 1.5: Final Product Choices



Final survey wave choice experiment results.

Table 1.1: Summary Baseline Means

Baseline Water and Hygiene	**Obs.**	**Mean**	**S.D.**
Soap present in home during interview	400	0.463	0.499
HH reports child < 5 had diarrhea in past two weeks	400	0.423	0.495
HH reports death of a child	400	0.335	0.473
Roundtrip water collection time (minutes)	400	29.9	27.153
Baseline Respondent/HH 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
No. of additional "occasional" drinkers from HH's pot	400	2.825	3.082
Some secondary education or above	400	0.183	0.387
Illiterate adult respondent	400	0.113	0.316
HH reports farming as main income source	400	0.525	0.500
HH prefers 50 Ksh today vs. 100 Ksh in 1 week	400	0.318	0.466
Baseline Wealth Indicators			
Iron roof indicator	400	0.625	0.485
HH has a latrine or toilet structure	400	0.700	0.459
HH owns a radio	400	0.825	0.380
Liquidity constrained	400	0.588	0.493
Baseline POU Knowledge and Experience			
HH has heard of WaterGuard	400	0.983	0.131
HH has heard of Pur	400	0.893	0.310
HH has heard of filter	400	0.360	0.481
HH has used WaterGuard previously	400	0.450	0.498
HH has used Pur previously	400	0.405	0.492
HH has used filter previously	400	0.008	0.086
HH has purchased WaterGuard previously	400	0.403	0.491
HH has purchased Pur previously	400	0.175	0.380
HH reports always boiling their water	400	0.178	0.383

Liquidity constrained households are defined as finding it "very difficult" or "impossible" to get 500 Kenyan shillings (~$6.25 as of July 2009) by tomorrow. No households reported having previously purchased a filter at baseline.

Table 1.2: Baseline Water Quality in the Household

Baseline Water Quality Variables	**Obs**	**Mean**	**S.D.**
E. coli MPN (Count of *E. coli* CFU/100 mL in stored water)	377	155.705	413.148
"Zero Risk" (No *E. coli* in stored water)	377	0.135	0.342
"Low Risk" (*E. coli* MPN < 10 CFU/100 mL)	377	0.403	0.491
"Intermediate Risk" (10 CFU/100 mL < *E. coli* MPN < 100 CFU/100 mL)	377	0.679	0.467
"High Risk" (*E. coli* > 100 CFU/100 mL)	377	0.186	0.389
"Very High Risk" (*E. coli* > 1000 CFU/100 mL)	377	0.053	0.224

Includes only baseline observations. 23 households had no stored drinking water on hand at baseline. These are averages across all source water types.

Table 1.3: Water Collection Sources by Survey Wave

Panel A

Response to: What is your main drinking water source during current season?

	Baseline	Follow Up 1	Follow Up 2	Follow Up 3
% Tap	0.43	0.42	0.43	0.47
N	(173)	(158)	(160)	(173)
% Rainwater	0.08	0.19	0.19	0.06
N	(31)	(70)	(70)	(23)
% Earthpan	0.36	0.30	0.27	0.31
N	(144)	(113)	(99)	(114)
% Other	0.13	0.12	0.13	0.16
N	(52)	(45)	(48)	(60)

Panel B

Response to: Where did you collect the water currently stored in your household?

	Baseline	Follow Up 1	Follow Up 2	Follow Up 3
% Tap	0.30	0.21	0.33	0.41
N	(121)	(78)	(121)	(150)
% Rainwater	0.54	0.68	0.43	0.30
N	(217)	(255)	(161)	(108)
% Earthpan	0.11	0.08	0.18	0.18
N	(45)	(30)	(66)	(66)
% Other	0.04	0.06	0.08	0.12
N	(17)	(22)	(29)	(46)

“Other” source type includes the river Yala, wells, springs, and boreholes. “Surface water” in the text refers to the sum of “other” and earthpan.

Table 1.4: Untreated Water Quality by Source Type

	Obs	**Mean**	**Median**
Tap	290	248.2	23.1
		(31.5)	
Rainwater	456	230.8	11
		(26.7)	
Earthpan	149	715.6	230
		(73.2)	
Other	82	594.5	152.5
		(90.0)	

Data are averaged over all survey waves.
Excludes spot check observations.

Table 1.6: Differential Water Collection Practices by Product?

	(1)	(2)	(3)
	U<10	Ln *E. coli U*	Δ Ln *E. coli*
WaterGuard	0.295	3.300	-2.386
	(0.02)	(0.16)	(0.08)
Pur	0.282	3.471	-1.977
	(0.02)	(0.15)	(0.08)
Filter	0.363	3.105	-1.791
	(0.02)	(0.15)	(0.07)
Observations	1133	977	1077

Baseline and spot checks omitted. Standard errors in parentheses clustered at household. Column 1 indicates share of observations with pre-treated water having "low" *E. coli* of MPN <10 CFU/100 mL. Column 2 is a continuous measure of the (natural) log of *E. coli* for households' pre-treated water, and smaller values imply higher quality water; column 3 calculates the difference between the natural log of *E. coli* in a household's pre-treated water and the natural log of *E. coli* in its post-treated water (more details in text). More negative values imply superior cleaning performance in this column.

Table 1.5: Usage Rates Across User Definitions and Products

	(1)	(2)	(3)	(4)	(5)	(6)
	Zero *E. coli*	T<10	T=0, U>0	Self-Report	Ln *E. coli*	Product-Specific
Baseline	0.128	0.380		0.073	2.719	0.015
	(0.02)	(0.02)		(0.01)	(0.12)	(0.01)
WaterGuard	0.508	0.652	0.547	0.774	0.908	0.439
	(0.03)	(0.02)	(0.03)	(0.02)	(0.14)	(0.03)
Pur	0.334	0.468	0.429	0.618	1.474	0.308
	(0.02)	(0.03)	(0.03)	(0.02)	(0.14)	(0.02)
Filter	0.387	0.589	0.414	0.753	1.335	0.764
	(0.03)	(0.03)	(0.03)	(0.02)	(0.14)	(0.02)
All Products	0.410	0.569	0.463	0.715	1.241	0.503
	(0.01)	(0.02)	(0.02)	(0.01)	(0.09)	(0.01)
Post-Baseline Obs	1133	1133	730	1133	1077	1133

Spot checks omitted. Standard errors in parentheses clustered at household level. Column 1 defines usage as treated water with no detectable E. coli; column 2 indicates share of homes with treated water E. coli MPN < 10 CFU/100 mL. Column 3 restricts sample to those households that have both untreated and treated samples on hand, and defines usage as untreated water testing positive for E. coli and treated water testing negative. Column 4 defines usage as self-reporting treatment: current water is treated, treatment was in the past 7 days, and household reports use of POU product every time water is collected. Column 5 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 this definition. Column 6 contains product-specific definitions of usage: for WaterGuard and Pur, it shows the share of households with a positive chlorine test, for filter households, it shows the share of households where the enumerator recorded observed filter usage. More details in text.

Table 1.7: Water Source Types by Product

	(1)	(2)	(3)	(4)	(5)	(6)
	Source Type				Low Quality	Time Collect
	Tap Water	**Rain Water**	**Earthpan Water**	**Other Surface Water**		(Minutes)
WaterGuard	-0.084	0.135	-0.064	0.013	-0.062	-6.597
	(0.03)***	(0.05)***	(0.03)**	(0.02)	(0.03)*	(1.33)***
Pur	-0.104	0.094	-0.032	0.042	-0.004	-6.045
	(0.03)***	(0.04)**	(0.03)	(0.02)**	(0.03)	(1.82)***
Filter	-0.120	0.135	-0.041	0.026	-0.025	-4.932
	(0.03)***	(0.04)***	(0.03)	(0.03)	(0.04)	(1.84)**
Survey Wave Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1517	1517	1517	1517	1517	1517

Excludes spot checks. Standard errors in parentheses clustered at village. Columns 1-4 present marginal effects from multinomial logit regression of source type on assigned product (equation 1.1 in text). Column 5 presents marginal effects results from logit estimation of households collecting water from a "low quality" source (earthpan, river, or other surface source) on assigned product. Column 6 presents results of OLS estimation of the number of minutes spent collecting water as function of assigned product.

Table 1.8: Choice and Usage Correlate?

WaterGuard	(1)	(2)	(3)
	Pos. Cl Test	Zero *E. coli*	Self-Report
First Choice	0.451	0.549	0.804
S.E.	(0.03)	(0.03)	(0.02)
N	51	51	51
Not First Choice	0.439	0.498	0.771
S.E.	(0.07)	(0.07)	(0.06)
N	319	319	319
Pur	(4)	(5)	(6)
	Pos. Cl Test	Zero *E. coli*	Self-Report
First Choice	0.286	0.313	0.633
S.E.	(0.04)	(0.04)	(0.04)
N	147	147	147
Not First Choice	0.318	0.345	0.610
S.E.	(0.03)	(0.03)	(0.03)
N	223	223	223
Filter	(7)	(8)	(9)
	Obs. Usage	Zero *E. coli*	Self-Report
First Choice	0.828	0.454	0.847
S.E.	(0.03)	(0.04)	(0.03)
N	163	163	163
Not First Choice	0.715	0.338	0.681
S.E.	(0.03)	(0.03)	(0.03)
N	207	207	207

Results from final survey wave only comparing previous observed usage of each product between those that chose that a given product as parting gift and those that did not. Columns 1-3 contain results on previous usage performance for WaterGuard, averaged across all survey waves. Column 1 defines usage as a positive chlorine test with WaterGuard; column 2 indicates share of households with zero E. coli in treated water during WaterGuard trial; column 3 looks at self-reported usage. Columns 4-6 repeat these definitions of usage for Pur observations, and columns 7-9 repeat them again for the filter, with the exception that column 7 defines usage as enumerator observed filter usage.

Chapter 2

Information and Persuasion: Achieving Safe Water Behaviors in Kenya

The previous chapter considered the role of product design in encouraging adoption of, and preference for, POU technologies. Although we find significant differences in usage rates across products, we never approach 100% usage of any of the three POU products. It is possible that no product will achieve widespread adoption without first addressing the common decision-making barriers to the adoption of any POU product or behavior. All POU technologies are designed to deliver safe drinking water, yet all necessitate some level of effort and initiative on the part of households. If households do not have a clear understanding of the reasons why to use a POU technology, or if they simply find POU technologies uninteresting or unappealing, adoption may remain low for any product, even if given away for free.

This chapter considers the common decision-making barriers to the adoption of *any* POU product or behavior. We abstract away from issues of relative product performance and preferences to consider informational and behavioral constraints that all POU technologies have in common. In particular, we hypothesized that individuals may (1) lack complete information about the quality of their drinking water and its link with diarrhea; and (2) make decisions using commonly utilized heuristics instead of solving complex decision problems as perfectly rational economic agents.

Our experiment tested hypothesis (1) by providing randomly selected households with the results of water quality tests. Providing information about water quality has increased the likelihood of households adopting safe water behaviors in other settings (Madajewicz, Pfaff, van Geen, Graziano, Hussein, Momotaj, Sylvi and Ahsan 2007, Jalan and Somanathan 2008). Our test of the role of information is unique in that we allow for the possibility that the provision of information is not only expanding people's information sets (hypothesis (1)), but is adding salience to a problem that is already somewhat understood (hypothesis (2)). We attempt to disentangle these two channels by testing if the type of information provided matters: Some households were provided results from common water source collection points, while others were provided both source water results and results from their own private in-home stored water supplies. From a policy perspective, it is much more cost-effective if village-level information is sufficient to engender behavior change. Moreover, in a neoclassical world the additional personalized information should not matter if both tests show contamination; the provision of common source results should provide any missing information about available water quality. In practice, attention is a limited resource (DellaVigna 2009), and people often use an "availability heuristic" to weight personal experience more heavily in decisions involving a variety of self-protective behaviors (Simonsohn, Karsson, Loewenstein and Ariely 2008).[1] We therefore predicted that the personalized results may further increase usage by adding salience to this information.

To test hypothesis (2), our study randomly assigned marketing messages designed to appeal to well known psychological heuristics. There is a wide body of evidence that important, real world decisions can be affected by how those decisions are presented, even when the content of a choice set has not changed (Cialdini 1993, Bertrand, Karlan, Mullainathan, Shafir and Zinman 2009). Although some POU products have enjoyed extensive social marketing campaigns in many countries, the effectiveness of marketing campaigns that attempt to harness such behavioral biases in favor of the use of POU products has not been extensively explored.[2] More generally, while marketing can influence consumer demand for a product's purchase, there is less evidence that marketing can induce *behavior change* after a product has been purchased. Given the low rates of sustained adoption of POU products throughout much of the developing world, it is safe to conclude that a viable marketing campaign that successfully persuades individuals to change their behavior remains elusive.

This chapter will show that both information and marketing appeals increase POU usage rates beyond that achieved by their free distribution. In particular, the sharing of common (village level) source water quality information results in an 8-13 percentage point rise in rates of product use, representing a 12-24% increase over base values. The additional sharing of personalized (household level) water quality information does not further increase use. Marketing messages that were designed to appeal to findings from the psychology and persuasive communication literatures raised rates of water treatment by 16-32% in total. Messages that "framed" safe water technologies as increasing health *and* avoiding disease (not just increasing health) raised usage rates by 4-6 percentage points, or 8-18%. Marketing messages that had households publicly commit to water treatment realized similar results. The additive nature of our marketing messages makes their effect sizes of a scale that may be notable to policy makers and marketers alike, and deserves further field testing.

This chapter contributes to the empirical literature on the adoption of preventative health technologies as well as suggests possible avenues for improvements on a decision-making model that assumes full information and fully formed, consistent preferences. Our results also suggest promising means for measurable and incremental improvements in the market viability of these private health products. Many of these interventions are potentially cost-effective and necessitate only a rethinking of existing marketing strategies.

The rest of this chapter proceeds as follows. Section 2 describes the study design we implemented to test the roles of information and marketing appeals in achieving adoption of POU products. It then outlines the behavioral theories underlying the information and marketing ap-

[1] The availability heuristic can be explained intuitively as "the impact of seeing a house burning on the subjective probability of such accidents is probably greater than the impact of reading about a fire in the local paper" (p. 1127) (Tversky and Kahneman 1974).

[2] An exception is Kremer et al. (2009), who explore the ability of various intensive social marketing tactics to induce greater adoption of a chlorine product in Busia, Kenya. More details comparing our study with theirs are given in section 2.1.2.

peals, and describes how they were implemented. Section 3 describes the data collected. Section 4 presents results, and section 5 concludes.

2.1 Study Design Overview

2.1.1 Experimental Design

The design of our marketing and information tests were implemented orthogonally to the cyclical disbursement of products as described in Section 1.1.2 of Chapter 1.

At the baseline and all follow-up survey rounds, after the product introductions, respondents were exposed to a randomly assigned "framed" marketing message. The framed messages were implemented orthogonally to the order of product introductions, and were intended to test the ability of differently framed messages to influence adoption of any safe water behavior and not relative preferences between the three POU products. In particular, one half of households were randomly assigned to hear a "positively framed" message that emphasized only the gains from POU usage, while the other half of households were given a "contrast frame" that contrasted what one stands to lose from non-use with the gains from POU usage.

At the end of an interview after respondents were randomly assigned one of the three POU technologies for a new two month trial, orthogonal to the framing treatment and the newly assigned product, one-half of households was asked to verbally commit to use their assigned POU product.

The same marketing (framing and commitment) treatments were re-delivered at each two-month follow-up survey visit.

Both of these randomized marketing treatments were implemented at the household level and used modular algebra to ensure exact orthogonality across treatments; assigned marketing messages were printed directly into surveys and surveys were preassigned to households on a randomized basis.

In addition, our study shared information about water quality. Assignment to treatment for the information campaign was randomized at the village level to minimize any leakage of effects across households between survey rounds. The information treatments were similarly printed directly into personalized surveys and were administered as follows. At the first follow-up visit two months after the baseline interview, households in one third of villages were provided with information about the quality of their common source drinking water collection points. Households in a separate third of villages were provided information about the quality of their common source, as well as private household, drinking water supplies, based on tests performed two months previously during the baseline interview. A final third of households was not provided the results of any water quality tests during this visit. At the second follow-up visit four months after the baseline interview, villages were staggered into "full treatment" as follows. Households in villages that received only source water results two months before now also received results of water quality tests performed on their own stored supplies,[3] while the households that had not received any

[3] At this visit, water quality test results shared were from the two month mark of the study, unless tests showed contamination despite the household reporting use of their POU product. This was the case for 66 household visits. For these observations, households were provided with the water quality results from the baseline round and this was made clear to respondents. This was done to avoid biasing true users of the products against a product that was

information were now provided the results of source water quality tests. Information about water quality was communicated as either a "contaminated" or "not contaminated" result, i.e., there was no discussion about levels of contamination.

2.1.2 Persuasion Interventions

Marketing is traditionally used to influence consumer demand for a product's purchase. However, the ability of marketers to get POU products off the shelves of suppliers will not deliver any health benefits. Only after households make a private decision to use these products can health benefits accrue. Moreover, only with sustained usage can a successful business model develop for private health products of a consumable nature. This study explored the ability of utilizing well known psychological heuristics to affect household behavior with respect to actual product usage.

Our study is not the first to test the ability of marketing messages to affect behavior. Bertrand et al. (2009) find that mailed fliers that include randomized advertising content appealing to various psychological heuristics can affect the take-up of loans in South Africa. Agarwal and Ambrose (2008) find that randomly assigned direct mail solicitations influenced consumers' choices of financial contracts in the US home mortgage market. Dupas (2009) finds that the take-up of mosquito nets is more sensitive to price than to marketing in Kenya. However, all of these studies involve the charging of positive prices for the various services, and all consider the ability of marketing to affect a one-time behavior. In the context of POU safe water products where the decision to treat water is repeated, Kremer et al. (2009) also test the ability of intensive social marketing appeals to induce greater adoption of WaterGuard in Kenya. They find small positive effects but conclude that social marketing alone does not hold much promise of promoting widespread adoption. However, ours is the first study to expressly design and test various marketing appeals that attempt to harness behavioral anomalies to increase POU usage rates in tandem with free POU products. Furthermore, we have additional measures of usage that we believe avoid some of the problems with the measures in Kremer et al. (2009) (self-reports and chlorine residual).

The design of our study is such that outcomes are measured two months following treatment at a successive interview (or at shorter intervals for the subset of homes that received a spot check). We argue that this is a relatively stringent test of these persuasive and informational interventions, but it is also the correct test from the viewpoint of achieving medium-term behavioral change. If such tactics can be harnessed in a predictable way to encourage water treatment behavior change, then a potentially powerful tool could be at the disposal of marketers, producers, NGOs, and governments alike. The choices of what decision-making heuristics to test were drawn from the psychology literature on persuasive communication.

2.1.2.1 Framing

We test whether households are induced to use their POU products more when exposed to marketing messages that emphasize their benefits in terms of gaining health, or in terms of both avoiding

performing at less than 100% effectiveness (all products perform at greater than zero effectiveness, but it is possible that a product reduces only 99% of contamination instead of 100%, for example). All results presented in this paper are robust to the exclusion of these observations.

sickness and gaining health. There are competing hypotheses in the literature for whether "framing" POU adoption as a gain or a loss should bring about the larger response. Psychology research can suggest the superiority of emphasizing the gains from usage (Rothman, Martino, Bedell, Detweiler and Salovey 1999). Moreover, conventional wisdom among social marketers of POU technologies is to focus on the positive aspects of usage. Population Services International (PSI), an NGO that handles marketing and product distribution for both WaterGuard and Pur in Kenya as well as 20 other countries, has published a social marketing "best practices" manual that argues strongly in favor of a positively framed message. In it they write:

> "Branded campaigns need to be aspirational; that is, 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, p.28).

However, there is evidence in the persuasive communication literature that a "contrast" frame that first frames a problem (e.g., unsafe water) as a loss, and then presents a solution (e.g., POU products), with emphasis on the individual's ability to achieve the solution, in a positive frame, can be a powerful formula for inducing behavior change (Gass and Seiter 2007).

We test for such an effect in the context of POU adoption. Our priors on expected effects of this marketing treatment were in favor of the contrast frame inducing a greater response. By first reminding households of the costs of sickness, it is possible that such a framed message could appeal to loss aversion, wherein people tend to overweight losses relative to gains (Tversky and Kahneman 1981, Kahneman and Tversky 1979).[4] It is also consistent with 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). Finally, it could also be consistent with a model of incomplete information if the contrast framed message provided information that was not contained in the positively framed message. However, because both versions of the framed messages were delivered after a common educational script that discussed the importance of safe drinking water and the dangers of diarrheal disease to all households, it is not clear that the vivid messages meant to appeal to respondents' emotions contained differing levels of information.

To implement this randomization, at each survey round after enumerators introduced the POU products, households assigned to hear the positive frame were exposed to a marketing appeal that included images of happy, smiling children and a "clean" glass of water as the enumerator read to the respondent a few sentences about what they stood to gain from regular use of a safe water product. The other half of households were exposed to a marketing appeal that contrasted photographs of a sad, crying child and a visibly dirty glass of water next to a happy, smiling child with a "clean" glass of water. The corresponding verbal script read by survey enumerators began by emphasizing that the sad, crying child had diarrhea due to drinking contaminated water. It then became *exactly the same* as the positively framed message to emphasize what the respondent stood to gain from

[4] Figure 2.1 explains the intuition behind this argument.

regular use of a safe water product: clean water and a happy, smiling child.[5]

2.1.2.2 Consistency with Public Commitment

The "commitment consistency" psychological 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, Carnot, Beach and Young 1987). This effect is strongest when that commitment is made in front of others (Cialdini 1993), possibly by incorporating social pressure into its effects. There is also evidence that predicting one's own future behavior can influence that behavior (Cialdini 1993).

However, the ability of this psychological tenet to affect real world behavior remains inconclusive and may depend on the context. Greenwald et al. (1987) find positive effects on voting behavior, but Smith, Gerber and Orlich (2003) fail to find a similar effect. Closer to our setting, Kremer and Miguel (2007) find no effect of asking adolescent respondents in Kenya to commit to taking a deworming drug on subsequent adoption. Webb and Sheeran (2006) perform a meta-analysis of 47 randomized trials in psychology that test the ability of this heuristic to influence a wide range of behaviors. They find small to medium effects on behavior due to randomized interventions that alter one's intentions and conclude that the intention-behavior link is present, but confounded by a variety of other factors that moderate this link.

Our study tests the ability of harnessing such "commitment effects" to induce greater POU safe water product usage. At the end of each interview, as a new product was given to a household for a new two month trial, survey enumerators asked a randomly chosen half of participant households if they intended to use their assigned POU technology.[6] Then, enumerators asked these respondents to promise aloud to use their safe water product to keep their families healthy. This pledge was optional for the household, although in practice all respondents were willing to make it. These respondents were next asked to predict if they would be found to be using their safe water product two months later when the enumerator returned. At this point, these treatment households were given a photographic reminder of this commitment to hang in their homes. At the baseline visit, these photographic reminders were posters showing images of all three of the safe water products as well as images of happy, smiling mothers and children.[7] After the first two month trial with a product, these same households were given a second, personalized poster that showed images of the products as well as a photo of the respondent herself that had been taken by the enumerator at the end of the baseline interview two months prior.[8] The other "control" half of homes did not

[5]Translations of the verbal scripts and accompanying images for both frames can be found in Figures 2.2 and 2.3. The positive or "contrast" frame visual images were shown to respondents via the use of marketing 'flip charts' carried by the survey enumerators. We gratefully acknowledge input on parts of the positively framed verbal script from members of the Rural Water Project (RWP) in Busia, Kenya, as well as ideas from Meyerowitz and Chaiken (1987) and Block and Keller (1995).

[6]The same half of respondents was given this treatment at each visit to reinforce earlier treatments.

[7]Figure 2.4 shows the "commitment poster" delivered to treatment homes during the baseline visit. We thank Clair Null for this suggestion, and recognize that we potentially confound the psychological effects of this treatment with the effects of being reminded to treat one's water by these posters.

[8]Figure 2.5 shows an example of a personalized "commitment poster" delivered to treatment homes at the first follow-up visit. These posters were intended to strengthen any effects from this commitment treatment by drawing on the "availability heuristic." Similar personalized posters were delivered to the "control" half of households at the final exit interview as these became valuable commodities in the community.

receive any additional messages or reminders of this sort.

Our priors were that this psychological commitment treatment would induce greater product usage by altering respondents' preferences over the treatment decision. In particular, by increasing the utility associated with POU usage due to the psychological benefits of staying true to one's word (as well as being reminded of this promise by the posters), we hypothesized that treated respondents would self-identify as users and subsequently follow through with greater POU usage behavior.

2.1.3 Information Interventions

Another possible cause for low adoption rates of new technologies in developing countries is that individuals lack complete information. In the context of drinking water, being provided with information about household water quality can increase the likelihood of households adopting safe water behaviors. Madajewicz et al. (2007) find that informing rural Bangladeshi households about arsenic contamination levels in wells results in 60% of those with unsafe wells to switch, as compared to 14% of those with "safe" wells. Similarly, Jalan and Somanathan (2008) find that informing urban Indian households about the results of water quality tests results in an 11 percentage point increase in rates of adoption of safe water behaviors among households that had not been previously treating their water. However, Jalan and Somanathan (2008) rely on self-reported outcomes to measure the impact of information, which is likely upwardly biased: If households have been told their drinking water is contaminated, it may be socially difficult to respond that one is *not* doing anything to treat the water in response. Further, Jalan and Somanathan (2008) share information only on households' private stored supplies of drinking water. This design may not be cost effective as a policy approach, and the authors do not consider the possibility that the provision of water quality information adds salience instead of expanding people's information sets. Our study attempts to distinguish between these channels by sharing both common source, and personalized own, water quality information. Finally, their study did not provide free POU products in tandem with this information and they consider a relatively wealthy segment of an urban Indian population.[9] This allows the possibility that income effects will play a role in people's treatment decisions and ignores a large segment of the developing world that is extremely poor and lacks access to safe drinking water. Closer to our setting, Kremer et al. (2009) argue that lack of awareness might not be a constraint to POU adoption in nearby Busia, Kenya, as a majority of households in their study understood the health benefits of using WaterGuard. However, understanding the health benefits from water treatment (implying knowledge of the link between contaminated water and disease) addresses just one of two channels through which incomplete information may affect a household's POU usage decision. Households also may lack information about the quality of their available drinking water supplies. In our baseline survey, we found evidence that households may lack full information in both respects. 49% of households (194 of 400) failed to name "drink clean water" when asked ways to prevent diarrhea, and 42% of households (166 of 400) thought their drinking source was safe to drink without treatment despite 100% contamination rates among non-rain water catchment sources.

We test whether lack of awareness about water quality is influencing POU usage in our setting

[9] 26% of their respondents own a computer.

and if so, what type of information is necessary to increase POU usage rates. It is important to point out that due to the design and timing of our information sharing campaign, our information treatments test whether households lack information about their water quality (the second hypothesized channel for information to affect POU usage). We did not begin sharing information until the two-month follow-up survey round, yet the baseline survey round (and all subsequent survey rounds) included an educational script on the link between contaminated drinking water and diarrheal disease. Since all households received this script, we do not directly test for the impact of linking contaminated water and disease.

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 and no information, respectively. 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 may not be feasible to ask local governments to test the quality of the drinking water in every household's private stored supplies, and moreover to do so repeatedly to keep such information up to date. If, however, the sharing of source water quality results can induce as great a response, then a potentially more practical policy prescription exists.

Our ex ante 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 in the control condition.[10] Formally, we outline our hypotheses as follows. Consider a simple model with two possible states of the world in any single time period: healthy and sick. Let consumers experience health $h = \alpha$ in the sick state and $h = \alpha + \theta$ in the healthy state. Thus, the "gain" to health in the healthy state is θ where $\theta > 0$. Assume that prior to the baseline survey, consumers know their realized health in each state of the world from previous experience and hold priors on the probability of experiencing the healthy state, but due to incomplete information do not necessarily link the resulting states of the world in any period to their drinking water quality (i.e., they lack information that contaminated water leads to disease). Thus, prior to our baseline survey, assume in any period consumer i has expected utility given by:

$$E[U(h^i)] = \hat{p}_0^i(\alpha + \theta) + (1 - \hat{p}_0^i)\alpha \tag{2.1}$$

where $\hat{p}_0^i$ is consumer i's ex ante subjective belief about the probability of realizing the healthy state, and we assume $\hat{p}_0^i \sim F(p_0, \sigma^2_{p_0})$. In this setup there is no decision for consumers to make to affect their realized utility in any period; they take whatever fortune they are dealt in each period. After the baseline survey and its associated educational component about the link between contaminated water and disease as well as free POU product provision, consumers begin to relate their expected utility in any period to their decision to use a POU product (i.e., this becomes an argument in their expected utility). Consumers form new beliefs $\hat{p}_1^{qi}$, with $\hat{p}_1^{qi} \sim F(p_1^q, \sigma^2_{p_1^q})$, about the probability of realizing the healthy state in any period when using POU product q, where we assume $1 \geq p_1^q \geq p_0$ for all q. That is, on average consumers expect POU products to increase their chances of realizing the healthy state. Consumers will now use their POU product q if the expected utility from doing so is greater than that from non-use. We can write:

[10] We discuss how responses to a "safe" versus "contaminated" personalized water quality test may differ when we present results in section 2.3.1.

$$
\begin{aligned}
E[U(h^i|T^{qi}=1)] = &\quad \hat{p}_1^{qi}(\alpha+\theta)+(1-\hat{p}_1^{qi})(\alpha)-C^q \geq \\
E[U(h^i|T^{qi}=0)] = &\qquad \hat{p}_0^{i}(\alpha+\theta)+(1-\hat{p}_0^{i})(\alpha)
\end{aligned} \tag{2.2}
$$

where T^{qi} is the treatment decision by consumer i ($T^{qi} = 1$ or $T^{qi} = 0$), and C^q is the perceived "costs" of usage of product q due to the time and effort involved. In expectation this decision simplifies to using POU product q if and only if:

$$
(p_1^q - p_0)\theta \geq C^q \tag{2.3}
$$

The probability differential $p_1^q - p_0$ in equation (2.3) can represent consumers' average prior expectation about the health gain from use of POU product q, and a consumer will use a POU product if the expected relative gain from doing so outweighs any costs in effort from use.[11]

When consumers are provided information about source water quality two months following the baseline survey, any uncertainty they may have had about the safety, or lack thereof, of available water sources is effectively eliminated. We model this as a downward shift in mean beliefs about the probability of realizing the healthy state in the absence of POU treatment ($p_0^{source} < p_0$). This causes some households who did not satisfy (2.3) originally to now switch to usage of their POU product.

As explained in the introduction, we hypothesized that the provision of personalized water quality information might add salience to a household's decision and result in greater usage than the common source information if the two tests both show contamination. In our model we can think of this as further modifying consumers' expected utility over the sick state in the absence of POU usage. This salience effect can be thought of either as consumers further re-weighting the probability of the sick state in the absence of treatment disproportionately ($1 - \hat{p}_0^* \geq 1 - \hat{p}_0$), or by decreasing their anticipated utility in the sick state from α to $\alpha + \gamma$ (with $\gamma < 0$). If the provision of personalized water quality tests causes attention to focus on the bad outcome, instead of on the probability of its occurrence, this could increase its perceived "cost." Such a response has been hypothesized in other settings (Sunstein 2003). In either case, our ex ante prediction was that the personalized information would further increase POU product usage by adding "vividness effects." Empirically, we will make attempts to distinguish between our information treatments adding vividness and increasing knowledge.

2.2 Data Description and Summary Statistics

2.2.1 Data Description and Tests of Randomizations

Our study included many different types of randomizations, all implemented orthogonally to each other, with the exception of the village-level information randomizations. We did not anticipate interactive effects across the independently assigned randomizations, but in Table 2.1 we present

[11] Here we do not formally model the learning process consumers undergo to update beliefs on the quality of the POU products. Although expectations about product quality are sure to change following experience with them, this does not add to the intuition of the problem at hand. Chapter 3 has a short model of learning about product quality.

cell sizes for each combination of randomizations within each post-baseline wave. Average post-baseline cell size is 10.5 households that receive the same product (WaterGuard, filter or Pur), frame (positive or contrast), commitment treatment assignment (treat or control), and initial information treatment (zero, source, or source + own) in a given survey round. Six cells have the minimum cell size of seven households and fifteen cells have the maximum cell size of 13 households. Baseline cell sizes are larger since there was no information treatment or product assignment at this point in the study.

We do not present comparisons of equality across treatment categories in baseline descriptive statistics for each treatment individually due to the many points of randomization. Rather, we summarize by saying that for the 55 baseline household descriptive variables compared across the individual-level randomized treatment assignments of framing message received and commitment treatment received, nearly all balance. Specifically, 52 of the 55 (92%) baseline descriptive variables are balanced (p-value > .1 for t-test of equality of means) across frames; and 53 of 55 (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 marketing randomizations. We therefore feel confident that our marketing randomizations were effective.

The village-level information randomizations had more pre-treatment differences. 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 been provided 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 upwards bias that may result from staggering wealthier villages into the information treatment first are attenuated by the timing of this treatment. Furthermore, it is possible that such an imbalance across treatment groups will bias any estimated effects of information *downwards* if the information treatments operate primarily via the channel of expanding people's information sets and the wealthier, more educated villages had greater baseline awareness (although we do not see evidence of this when comparing baseline rates of knowledge). A list of the particular variables that do not balance for each treatment are in Table 2.8.[12]

2.3 Estimation Strategy and Results

2.3.1 Impact of Persuasion and Information Interventions

We first consider the persuasion randomizations independently to test whether each affects product usage. Later we will introduce multivariate regression techniques to control for confounding factors, but the nonparametric identification of effects is cleanest with the basic comparison of means (Freedman 2008). We therefore begin by combining all post-treatment waves of data (i.e., all waves after the baseline that are subject to being affected by our randomized treatments) and estimate the impact of treatment using univariate linear regression:[13]

[12]Results of all baseline comparisons are available upon request.

[13]Results are consistent if we estimate marginal probit effects for all 0/1 usage definitions. For ease of interpretation, we present results from linear probability regressions for all 0/1 usage outcomes.

$$Y_{ipt} = \alpha + \beta_M T_i^M + \varepsilon_{ipt} \tag{2.4}$$

where Y_{ipt} is a measure of usage of product p at time t by household i, and T_i^M is an indicator for either the framing treatment (T^F = 1 for "contrast" frame, 0 otherwise) or commitment treatment (T^C = 1 if assigned, 0 otherwise). Due to the randomized assignment to treatment for both manipulations, β_M should deliver unbiased estimates of their causal effects on product usage.

We cluster the error terms ε_{ipt} in equation 3.16 at the household to allow for correlated outcomes across survey waves for the same household. Results are consistent if we cluster disturbance terms at the village level to allow for correlation across households that may share a common water source, for example. Furthermore, likelihood ratio tests reject the null hypothesis of village random effects when we modify equation 3.16 to estimate a multi-level mixed-effects linear regression model that allows for village random effects in the error term. For ease of interpretation and due to the household-level assignment to treatment for the marketing interventions, we therefore present results from standard OLS estimation with robust standard errors clustered at the household.

In Table 2.7 we present results from ordered logit estimations of equation 3.16 with errors clustered at the village level to calculate the cumulative effects of our marketing interventions. To do this, we restrict the analysis to the final round of data on the 370 remaining participant households and count the sum total number of times a household was found to be using its free safe water products over the course of the past 6 months at each two-month follow up survey round. Since these results largely confirm those from OLS estimation of equation 3.16, we do not discuss them separately.

To estimate the effects of the information treatments, we must account for the fact that they were introduced in a staggered fashion over time and randomized at the level of villages. We therefore modify equation 3.16 to estimate their effects while controlling for confounding seasonality and other effects. Equation 2.5 presents an average treatment effect (ATE) estimator of all of our randomizations including the effects of the two types of water quality information shared (source and own results):

$$Y_{iptv} = \alpha_t + \alpha_p + \quad \beta S_{v,t-1} + \delta O_{v,t-1} + \theta F_{it-1} + \lambda C_{it-1} + \varepsilon_{iptv} \tag{2.5}$$

Y_{iptv} is a measure of usage of product p by household i at time t in village v. $S_{v,t-1}$ is an indicator variable that takes on a value of 1 if households in village v received information about source water quality at a previous visit to induce a response at time t. $O_{v,t-1}$ is another indicator variable that equals 1 if households in village v received information about their own private stored supplies *in addition to* source water quality results, $S_{v,t-1}$. Thus, in practice $O_{v,t-1}$ tests if the sharing of personalized water quality results affects POU usage *above and beyond* that realized by the sharing of common source water results. To separate out the effects of the other marketing treatments we include F_{it-1} as an indicator if household i received the contrast framing treatment in a previous wave, while C_{it-1} indicates that household i received the commitment treatment in a previous wave. We include survey wave fixed effects α_t to control for any common time-varying factors such as seasonality, and we include product fixed effects α_p to control for differential base rates of usage or cleaning performance across products. Note that equation 2.5 does not impose linearity since all explanatory variables are dummies, and thus the coefficients are estimates of the differences in conditional means of Y_{iptv}. The independent and randomized assignments to all treatments imply that each β, δ, θ, and λ coefficient in equation 2.5 is an unbiased estimate of the reduced form average treatment effect of that particular treatment on usage.

Due to the village-level assignment of the information treatments and the fact that our study site is comprised of just 28 villages, we are concerned about over-rejection of the null hypothesis of no effect from our information treatments due to intra-village correlation in outcomes and the relatively small number (28) of clusters (Duflo, Glennerster and Kremer 2006). Although the estimated intracluster correlation coefficient from estimation of equation 2.5 is small across the various user definitions ($\rho = .03$ is the maximum), in an effort to be conservative we estimate disturbance terms ε_{iptv} from equation 2.5 with a nonparametric cluster bootstrap procedure using village-level clusters due to the village-level assignment of the information treatments.

We will also estimate differential effects of the personalized water quality information for those households that receive a "safe" versus "contamined" personalized result by interacting the $O_{v,t-1}$ dummy from equation 2.5 with an indicator for whether a household received a "contaminated" personal water test result. We anticipate the effects of a "contaminated" personal test result to be more vivid, and hence induce greater POU usage, than a "safe" result. However, it is likely that personal water quality is endogenous to household behavior. We therefore will interpret such results with caution.

2.3.1.1 Framing Results

Table 2.2 contains results from estimation of equation 3.16 for the effects of our framed marketing messages on POU usage at all two-month follow-up rounds. Across the various definitions of product use, we find consistent and reasonably strong evidence that the "contrast frame" is more effective than the "positive only" frame at inducing product use. Although not always statistically significant, the magnitude and direction of the estimated effect sizes all suggest the superiority of contrasting what one stands to lose from nonuse with what one stands to gain from use over focusing solely on the potential gains. Moreover, the effect sizes are illuminating: Column 1 of Table 2.2 shows that contrast frame homes are nearly 6 percentage points more likely to have "safe" treated water at home (p-value of 0.06), representing a nearly 15% increase in usage over that of the "positive only" households. Column 5 shows that contrast frame households had an average *E. coli* count in their "drinking water" that was .43 log points lower than that found in positive frame households, which translates into 36% lower contamination levels.

We argue that the contrast frame is realizing an effect via product usage (i.e., treatment), and does not differentially affect other behaviors related to water collection or storage. Column 3 shows that among those households where both pre-treated and treated water samples were collected, households that received the contrast frame are nearly 8 percentage points more likely to have treated water with no contamination despite contaminated pre-treated water (p-value of .04). Column 4 shows that the share of respondents that self-report treatment is 5 percentage points greater among the contrast frame households (p-value of .07). Since it is not clear that the two framed messages would differentially affect any courtesy bias inflating these numbers, such differences are notable. Finally, we compare column 1 (2) with column 6 (7). While column 1 (2) shows that the contrast frame increased rates of *treated* water with zero (<10 CFU/100 mL) *E. coli,* there is no difference across frames in the corresponding rates of untreated water meeting this same quality threshold.

It is possible that the effects of this marketing treatment two months following its delivery are weakened by the passage of time. Figure 2.6 incorporates data from all spot check rounds to consider this possibility. Figure 2.6 graphs separate nonparametric plots of usage trends over the length of exposure with a product by framed marketing message and shows that the beneficial effect of the contrast frame appears to remain constant, despite a general trend of decreasing POU usage over time (consistent with the pattern seen in Figure 1.3 from Chapter 1). Figure 2.6 combines all spot checks and full survey rounds into one continuous measure of the number of days' exposure to a product, and combines all survey waves and all products, so seasonality and product performance do not affect these results.[14]

Although Figure 2.6's results are only suggestive, the finding that a simple marketing appeal that merely frames the POU usage decision in a new light may be able to affect behavior up to 11 weeks after it is given (the maximum time in Figure 2.6[15]) suggests that marketing could be one under-utilized avenue for increasing POU adoption rates. Since the psychology underlying our framing treatment would be effectively free to scale up, it could be one worthy of further investigation.

2.3.1.2 Commitment Results

Results from estimation of equation 3.16 for the commitment manipulation are listed in Table 2.3. Across the various definitions of product use, the commitment treatment consistently results in higher rates of usage, although these differences are not uniformly statistically significant. Column 1 shows that the commitment treatment is estimated to increase the likelihood of a household having zero *E. coli* contamination by 6 percentage points, or a nearly 15% rise in usage over control homes (p-value .06). Columns 2, 3 and 4 show that the estimated effect size from this treatment is not sensitive to the precise definition of usage: All estimates indicate that committing oneself to using a POU product increases rates of POU usage on the order of 5-8 percentage points. Although we suspected that the estimated treatment effect would be inflated by courtesy bias for rates of self-reported usage, results with this definition do not differ substantially from others (column 4). Column 5 in Table 2.3 suggests that commitment-treated households have slightly better drinking water quality, although this difference is not statistically significant.

Again, this marketing manipulation appears to have realized an effect via product usage and not through some other behavioral channel such as water collection habits. If we compare treated and untreated water samples across commitment treatment status by looking at columns 1 and 6 (2 and 7) in tandem, we see that the commitment treatment increases rates of *treated* water having no detectable (<10 CFU/100 mL) *E. coli* by 6 (5) percentage points (p-value .06 (.17)), but has no effect on the rates of untreated water that met this threshold (p-value .87 (.16)).

Our commitment marketing treatment is not a typical "commitment treatment" in the traditional vein of behavioral economics in that it does not formally restrict one's future self in order to ad-

[14] We do not graph corresponding confidence intervals since these differences across frames are statistically significant at their means (p-value .06) but not along their entire distributions, due to the design of our initial power calculations.

[15] Some households that were not reached on a first attempt for a follow-up interview were revisited at the end of a survey round, meaning some household obervations had more than two months with a product.

dress problems of time-inconsistent preferences (e.g., see Ashraf, Karlan and Yin (2006)); rather, it is a psychological commitment that we hypothesized may affect respondent's preferences over the treatment decision by increasing the utility associated with product usage due to the psychological benefits of staying true to one's word. However, POU technologies can be described as investment goods wherein the perceived "costs" of usage are incurred in the current period but the health benefits are not realized until later periods, and there is a wealth of evidence that people are often present-biased and overweight the present period relative to all future periods (DellaVigna 2009). It is easy to imagine that this is another hindrance to the widespread and sustained usage of POU technologies, particularly among present-biased households. Although our psychological commitment marketing treatment does not directly address problems of present-biased preferences, its design may be such that it causes respondents to adjust their *own* future planned behavior in response to receiving it. While we should be careful about estimating heterogeneous treatment effects on subgroups since doing so can compromise the benefits of randomization (Duflo et al. 2006), if doing so can potentially uncover underlying behavioral mechanisms driving the reduced form treatment effects we see (Deaton 2009), it could be a worthy exercise.

Our baseline survey tried to identify respondents with present-biased preferences by asking a hypothetical question about whether they would prefer to receive 50 Kenyan shillings (Ksh; about $0.70 in July 2009) today or 100 Ksh in one week. Although the credibility of such hypothetical questions is subject to question (see discussion of alternative interpretations of such questions in Ashraf et al. (2006)), it provides a crude approximation of those households we expect to benefit most from a (traditional) commitment device. 127 of 400 (32%) baseline respondents preferred 50 Ksh today, and these respondents are evenly distributed across the randomized commitment treatment (p-value of .24 on two-sided t-test of equality). We label those households that prefer 50 Ksh today "present-biased" (with an implied weekly discount rate of greater than 100%), while the other households we label "patient." Being "present-biased" does not appear related to a household's observable wealth or other characteristics. Across the same 55 baseline descriptive variables for which we tested the different randomizations in section 1.2 (excluding the indicator for being present-biased), we find that 51 of 54 (94%) are balanced (p-value of > .1 on two-sided t-test) across this categorization, including the rates of households that have soap in the home, report a child having diarrhea in the previous two weeks, own a radio, own an iron roof, report liquidity constraints, and have a latrine. We therefore believe that our definition of present-biased households is identifying the intended subgroup and we calculate the effects of the commitment treatment among patient and present-biased households separately.

Table 2.5 presents results. The average commitment treatment effect is much larger among present-biased households: Committing oneself to using a POU product leads to a 12 percentage point rise in the rate of households with no detectable *E. coli* in their treated water two months later, a 35% increase (column 2). Among the "patient" households that opted for 100 Ksh in one week, the estimated effect size is much smaller and insignificant (column 1). This same effect is seen when usage is defined as a continuous measure (the log of "drinking water" quality, columns 3 and 4). A parallel test for larger effects of our framing treatment among present-biased households does not show similar results (results not shown). These results suggest that the commitment treatment may at least partially operate by enabling present-biased households to address their present-biased preferences. Although we cannot say with certainty if this subpopulation of present-biased households suffer from time-inconsistent preferences or if they simply have very high discount rates on the water treatment decision (see ODonoghue and Rabin (1999)), additional field work is currently under way to distinguish between these two possibilities. In future work we will explore further any synergies between psychology's "commitment consistency" theory and behavioral problems of time-inconsistency.

2.3.1.3 Additive Marketing Effects

The similar magnitudes of the framing and commitment treatment effects on usage in Tables 2.2 and 2.3 is a surprise, and further analysis reveals that the two effects are additive. We designed these marketing treatments to have separate and distinct effects on behavior and accordingly their interaction has no effect. However, this means that if we compare outcomes across those households that received neither the commitment nor framing treatment (i.e., received only a positively framed message) with those that received both marketing treatments (i.e., received the contrast frame and the commitment treatment), the overall effect size due to marketing is quite large. Table 2.4 contains the results of such an exercise. It shows that the combined effects of these marketing treatments were to raise rates of product usage by 16-40% across all user definitions, or 16-32% across user definitions that do not exclude households that lack both treated and untreated water samples (column 3). Since these marketing treatments were implemented orthogonally to each other, comparing outcomes of those households that received neither treatment with those that received both restricts analysis to one half of the data.[16] The additive nature of the effects of these two psychological manipulations may suggest that a marketing strategy that harnesses many known behavioral anomalies in tandem to encourage use of POU products could realize large effects (although this may be an unsatisfying field test of behavioral economics from a theoretical point of view).

2.3.1.4 Information Sharing + All Effects Combined

Table 2.6 contains the results of estimation of equation 2.5. Table 2.6 suggests that the sharing of source water quality information significantly increases POU product usage and that the sharing of own water quality information does not encourage further usage. Column 1 shows that the percentage of households with zero *E. coli* in their treated water increased by nearly 10 percentage points (significant at the 5% level), or about a 24% increase over the mean base value across the three POU products,[17] in response to the provision of source water quality information. The additional sharing of own water quality results does not further increase usage. Column 2 presents differential effects of the personalized information between those households that received a "contaminated" versus "safe" test result. It suggests that the sharing of a "contaminated" personalized water test result does not spur greater usage and may even have deterrent effects, although the standard errors are too large to draw inference. Columns 3 and 4 present ATE results with alternate indicators of

[16] When we compare baseline descriptive statistics across these two subgroups, we again find that our randomizations were effective. Specifically, 51 of 55 (93%) baseline descriptive variables balance across these two subgroups.

[17] Mean "base" values cited in Table 2.6 are from the first two-month follow-up survey. We exclude all pre-treatment (baseline) data from this estimation since all treatments affect behavior only at points in time after the baseline interview. Results are not sensitive to the exclusion of baseline data nor the choice of "base" usage values.

usage, and both continue to suggest that the provision of source water quality information positively affects usage. Column 5 shows that the sharing of source water quality results leads to a statistically significant .6 log reduction in a household's drinking water *E. coli*, which translates into approximately a 49% reduction in contamination levels.[18]

Table 2.6 does not support our initial hypothesis that the personalized information would further increase usage by adding salience effects to the pure informational effect of sharing common source water quality information. Figure 2.7 further suggests that there is no benefit to the additional provision of personalized water quality information. It presents the relative average rates of usage (defined as having treated water *E. coli* MPN < 10 CFU/ 100 mL) across the three information groups as each was staggered into treatment. In the figure, the short dashed red and blue lines together constitute the effects of sharing "source" results, while the red lines alone constitute the sharing of own water quality results. Solid black lines represent having received no water quality information. The vertical lines in this figure mark the introduction of groups into a new information treatment category. This figure clearly demonstrates the ability of source water quality information to realize an effect equal to or greater than the sharing of own water quality information. Two months after the first sharing of water quality information, the homes that received only source results realize the biggest relative gains and are outperforming the source+own homes. Two months after this, when the final third of villages can react to being provided with source water quality results, we see that usage rates converge across the three groups. The additional sharing of personalized water quality information does not result in any higher usage above that achieved by the sharing of common source results.

Despite the failure of personalized information to realize distinct salience effects from the informational effects of the source water quality information, we find suggestive evidence that salience may still have played some role in increasing usage. Column 6 of Table 2.6 presents results of equation 2.5 where the dependent variable is an indicator that equals 1 if a household's *untreated* water had an *E. coli* MPN < 10 CFU/ 100 mL. Although outside the bounds of statistical significance with cluster bootstrapped standard errors, the magnitude of the estimated effect suggests that a portion of the source information "treatment" could be to encourage better selection of water sources, i.e., those that are less contaminated. This same definition of usage, when applied to a household's *treated* water, shows a statistically significant rise in usage of 13 percentage points (column 3). If the results in column 6 are taken as informative, it would imply that about half of the source information treatment is being exercised through improved collection practices (ignoring differences in levels of statistical significance). Yet the script that households received for the source water information treatment only specified that *all* of the source water in Nyawita had tested positive for contamination; it did not present results of relative levels of contamination across sources.[19] Thus, it was up to respondents themselves to infer which were the relatively cleaner sources. It appears possible that they may have done so. A related estimation suggests that households spend an average of about 6 additional minutes collecting water in response to learning

[18] All results in Table 2.6 are similar if we include village fixed effects. Results with village fixed effects are available upon request.

[19] The source water quality script did make the allowance that rain water may be free from contamination when collected. However, rates of rain water collection decreased over the course of the study as seasons progressed from rainy to dry (see Table 1.3 in Chapter 1). Also, the script emphasized that a good way to be certain one's water was safe was to use a POU product such as the one provided by our study.

source water results (Column 7 of Table 2.6). Furthermore, at each wave our survey asked whether respondents believed their drinking water to be "safe" without treatment when collected from their chosen source. At baseline, 42% of homes thought their source was "safe" without treatment. At the first follow-up survey two months later, just 7% of homes thought their source for drinking water was "safe." Although this latter statistic is surely biased downwards as households had just enjoyed two months with free POU products, because of the timing of this treatment, it means that 93% of households self-reported their source water to be unsafe *before* any households were provided the results of source water quality tests. The fact that the source water quality information nevertheless had such a large effect on behavior is somewhat puzzling. It is possible that this is still a pure information effect; if knowledge is a continuous variable, asking households a yes/no question about the safety of their source water may have failed to uncover any underlying uncertainty to their responses. However, it also could suggest that a portion of the effect may have been adding salience to a known problem versus uncovering an unknown one. From a policy perspective, it can be construed as good news that the sharing of village-level water quality tests can realize all the benefits of personalized information (in tandem with free POU products). However, if a part of this effect is due to salience, further reminders may be necessary to maintain such large effects over time. Future work should aim to further unravel any unintended salience effects from a pure information effect to better understand their respective roles in achieving safe water behaviors over the long term.

2.4 Discussion and Conclusions

Our experiment considered the roles of information and marketing in achieving behavior change in the use of free POU safe water technologies. We find positive and incremental effects from all of our treatments, as well as evidence that the marketing messages are additive in nature and not substitutes for each other. The information treatments raised rates of safe water product usage by 12-24% and the combined effects of the marketing treatments increased rates of usage by 16-32%. Although the individual effect sizes may be small, they are economically relevant to the extent they could be incorporated at little to no cost to existing marketing strategies for these products. Moreover, future work should further test the additivity of these heuristics, which could make them even more economically significant.

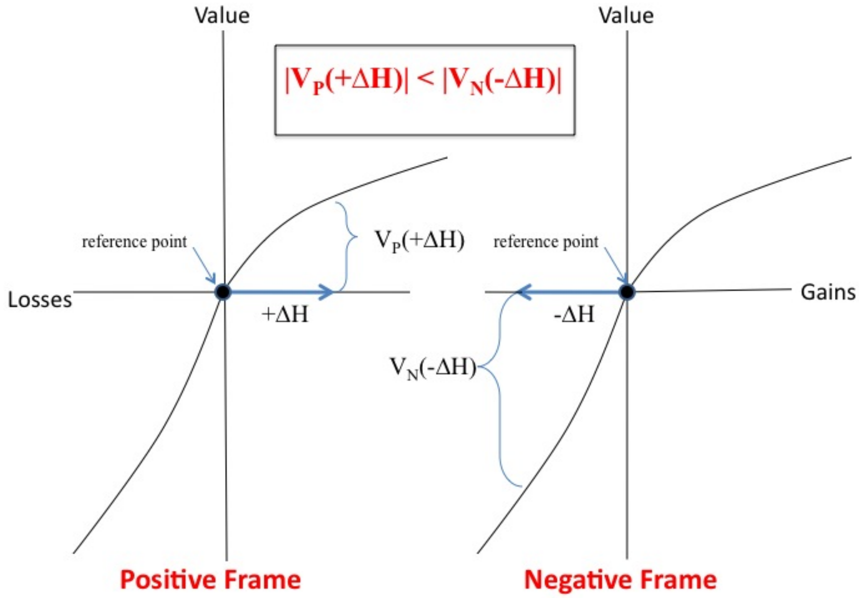
Our base-level marketing results are of a similar magnitude to the "short run" (~3 week) results found by Kremer et al. (2009) for their persuasion treatments to encourage use of WaterGuard in nearby Busia, Kenya, but larger than their "medium-run" results that found no effect of their persuasive appeals on usage. Possible explanations for the different findings include that our marketing messages appealed to different psychological heuristics, the two studies were in different areas with different source water types and quality, and we had three different POU products, all provided for free. Furthermore, their study's medium-run effects were estimated 3-6 months following treatment while the maximum amount of time elapsed to estimate our study's effects of the information and marketing campaigns was about 2.5 months (in any single survey wave). Our findings are also on par with those of Ashraf et al. (2007) for their estimates of the causal effect of pricing on usage of a chlorine product in urban Lusaka, Zambia.

Our results stand in contrast to those of Dupas (2009). She found no role for marketing in achieving take-up of mosquito nets in Kenya when charging positive prices for the nets. Although the behavior of preventing diarrhea differs from that of preventing malaria, the possibility arises that marketing can affect sustained behavior change even if it has no effect on willingess to pay for prevention. We will examine this question in more detail in Chapter 3. Of course, it is hard to generalize these conflicting results since we test different marketing interventions in different contexts. Our different findings highlight the heterogeneity in consumer take-up of health prevention measures.

In sum, our results can help to shed light on some of the informational and behavioral constraints to safe water treatment as well as promising avenues for incremental improvements in the market for POU technologies. Moreover, while providing POU technologies for free may be a good policy decision in some contexts, it is important to note that our results suggest that budgetary issues are not the only constraints preventing widespread and sustained adoption of POU technologies. Making the decision to treat water more interesting and salient with the use of good marketing messages, and making the reasons to use a safe water technology clear and vivid to households by providing information about water quality, could further help, and may be wise additions to any such policy.

Like most field experimental results, the external validity of our findings is subject to question. Towards this end, we are in the process of replicating our study in the urban slums of Dhaka, Bangladesh. Primary water sources in Dhaka's slums are municipal taps, and levels of contamination are very high among participant households. In our Dhaka study, we have a different mix of POU products but similar marketing and informational tests. If our findings from urban Bangladesh are found to confirm those from rural Kenya presented here, we will gain greater confidence in their external validity. Furthermore, by harnessing well known psychological heuristics in a predictable way in two very different settings using different technologies, our two studies could help to uncover new insights into people's decision-making processes more generally. Such findings could contribute to the growing behavioral economics literature that seeks to improve upon the standard economic model of behavior and thereby help develop new strategies to encourage the adoption of a variety of behaviors or technologies.

Figure 2.1: Hypothesized Framing Effects Due to Loss Aversion

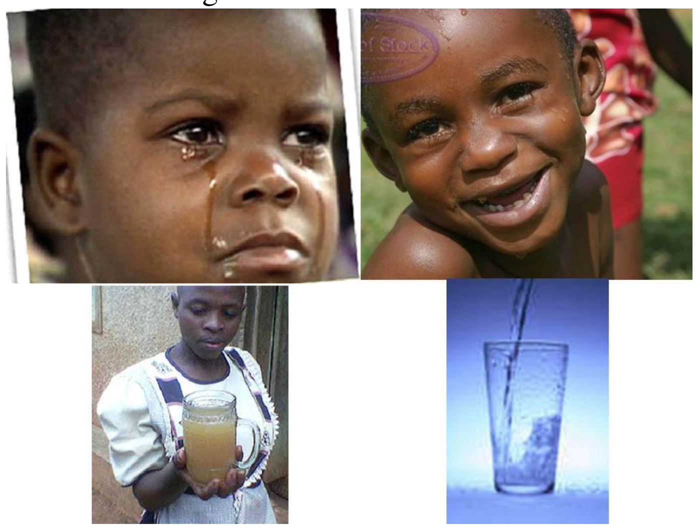


Hypothesized value function of Kahneman and Tversky (1979). ΔH represents the health gains from POU usage; $V_P(\Delta H)$ gives the perceived value of health gains under a positively framed message; $|V_N(\Delta H)|$ is the perceived value of avoiding health losses under a negative frame.

Figure 2.2: 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 2.3: Contrast 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.”

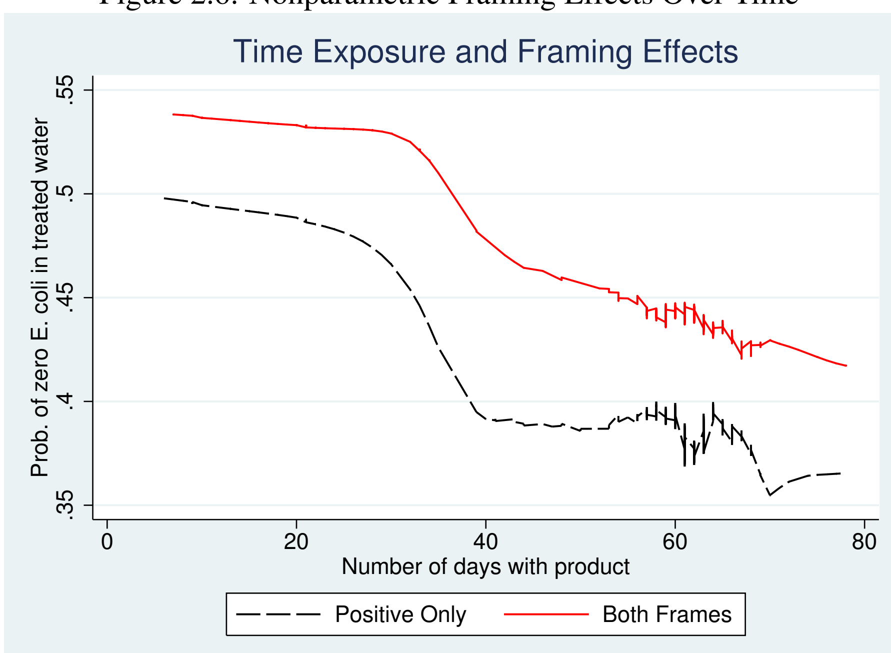
Figure 2.4: Baseline Generic Commitment Poster

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 by enumerators whose 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 2.5: Sample Personalized Commitment Poster

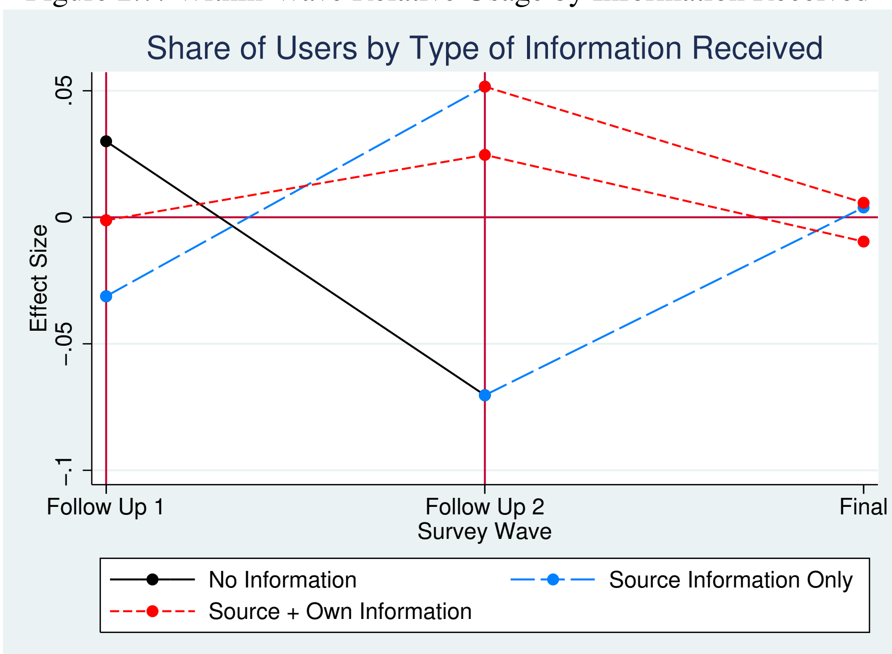
Sample "personalized" commitment poster distributed to households that received "commitment treatment" at follow-up 1 interview.

Figure 2.6: Nonparametric Framing Effects Over Time



Locally weighted (lowess) regression of POU usage indicator (defined as having zero *E. coli* in treated water) over the number of days with a product (count). The number of days with a product combines data from all spot check rounds with full follow-up survey rounds into one continous measure; there are more observations around 60 days. Bandwidth=1.

Figure 2.7: Within-Wave Relative Usage by Information Received



The two vertical lines represent introduction of new information treatments at follow-up 1 and follow-up 2 waves. “Effect Size” refers to relative share of total users within one wave from each of the three information treatment groups. Usage is defined as a household’s treated water having an *E. coli* MPN < 10 CFU/100 mL.

Table 2.1: Randomization Cell Sizes

			WaterGuard		**Pur**		**Filter**	
			Control	**Commit**	**Control**	**Commit**	**Control**	**Commit**
Follow-Up 1	**No Info**	**Positive**	11	11	11	12	10	13
		Contrast	9	13	12	9	10	12
	Source Info	**Positive**	8	12	12	10	13	8
		Contrast	12	8	8	13	10	9
	All Info	**Positive**	13	10	10	9	9	12
		Contrast	11	12	12	10	11	11
Follow-Up 2	**No Info**	**Positive**	11	12	10	13	11	11
		Contrast	10	12	9	13	11	9
	Source Info	**Positive**	12	10	13	8	7	12
		Contrast	10	8	12	8	7	13
	All Info	**Positive**	8	9	9	12	13	10
		Contrast	11	10	9	12	12	10
Follow-Up 3	**No Info**	**Positive**	10	13	11	11	11	12
		Contrast	10	7	10	12	8	13
	Source Info	**Positive**	13	8	7	12	12	10
		Contrast	7	13	10	7	12	8
	All Info	**Positive**	9	12	13	10	8	9
		Contrast	11	10	11	10	9	11

Post-baseline average cell size is 10.5 household observations. 15 cells have the maximum cell size of 13 households and 6 cells have the minimum cell size of 7 household observations.

Table 2.2: Usage Rates by Framing Treatment

	(1) T=0	(2) T<10	(3) T=0, U>0	(4) Self-Report	(5) Ln *E. coli*	(6) U=0	(7) U<10
1=received contrast frame	0.056	0.042	0.077	0.052	-0.431	-0.021	-0.009
	(0.03)*	(0.03)	(0.04)**	(0.03)*	(0.17)**	(0.02)	(0.03)
Constant	0.382	0.549	0.426	0.689	1.450	0.134	0.318
	(0.02)***	(0.02)***	(0.03)***	(0.02)***	(0.13)***	(0.01)***	(0.02)***
Observations	1133	1133	730	1133	1077	1133	1133
p-value Wald test	0.059	0.174	0.038	0.071	0.012	0.317	0.766

Standard errors in parentheses. *p<.10, **p<.05 ***p<.01

Results from OLS estimation of equation 3.16 for framing treatment. Baseline (pre-treatment) and spot checks omitted. Standard errors clustered at household level. Column 1 defines usage as treated water with no detectable *E. coli*; column 2 indicates share of homes with treated water *E. coli* MPN < 10 CFU/100 mL. Column 3 restricts sample to those households that have both treated and untreated samples on hand, and defines usage as untreated (pre-treated) water testing positive for *E. coli* and treated water testing negative. Column 4 defines usage as self-reporting treatment: current water is treated, treatment was in the past 7 days, and household reports use of POU product every time water is collected. Column 5 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 this definition. Columns 6 and 7 present results for untreated water quality using same definitions as columns 1 and 2 for treated water. Column 6 considers the share of households with untreated water having no detectable *E. coli*; column 7 indicates share of households with untreated water *E. coli* MPN < 10 CFU/100 mL.

Table 2.3: Usage Rates by Commitment Treatment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	T=0	T<10	T=0, U>0	Self-Report	Ln *E. coli*	U=0	U<10
1=received commitment	0.056	0.050	0.075	0.052	-0.190	-0.003	-0.042
	(0.03)*	(0.03)	(0.04)**	(0.03)*	(0.17)	(0.02)	(0.03)
Constant	0.381	0.544	0.425	0.689	1.337	0.125	0.335
	(0.02)***	(0.02)***	(0.03)***	(0.02)***	(0.12)***	(0.01)***	(0.02)***
Observations	1133	1133	730	1133	1077	1133	1133
p-value Wald test	0.059	0.105	0.042	0.076	0.269	0.874	0.161

Standard errors in parentheses. *p<.10, **p<.05 ***p<.01

Results from OLS estimation of equation 3.16 for commitment treatment. Baseline (pre-treatment) and spot checks omitted. Standard errors clustered at household level. Column definitions of usage are identical to those in Table 2.2.

Table 2.4: Usage Rates by Marketing Treatments Combined

	(1) T=0	(2) T<10	(3) T=0, U>0	(4) Self-Report	(5) Ln *E. coli*	(6) U=0	(7) U<10
1=received contrast frame & commitment	0.112	0.092	0.149	0.104	-0.620	-0.024	-0.051
	(0.04)***	0.04)**	(0.05)***	(0.04)**	(0.25)**	(0.03)	(0.04)
Constant	0.347	0.523	0.372	0.663	1.568	0.126	0.340
	(0.03)***	(0.03)***	(0.04)***	(0.03)***	(0.19)***	(0.02)***	(0.03)***
Observations	568	568	372	568	539	568	568
p-value Wald test	0.006	0.035	0.004	0.014	0.013	0.373	0.237

Standard errors in parentheses. *p<.10, **p<.05 ***p<.01

Results from OLS estimation of equation 3.16 for both framing and commitment treatments combined. Results include only those households that received neither or both marketing treatments. Baseline (pre-treatment) and spot checks omitted. Standard errors clustered at household level. Column definitions of usage are identical to those in Table 2.2.

Table 2.5: Mean Rates of Usage: "Patient" and "Present-Biased" Households

	(1)	(2)	(3)	(4)
	Zero *E. coli*	Zero *E. coli*	Ln *E. coli*	Ln *E. coli*
	Patient	Present-Biased	Patient	Present-Biased
Control Households	0.392	0.353	1.313	1.400
	(0.03)	(0.04)	(0.15)	(0.24)
Commitment Treated Households	0.418	0.475	1.324	0.811
	(0.03)	(0.04)	(0.15)	(0.19)
Difference	0.026	0.122**	0.011	-0.589*
	(0.04)	(0.05)	(0.21)	(0.30)
p-value Wald test	0.478	0.0225	0.958	0.0542
Observations	779	354	742	335

*p<.10, **p<.05, ***p<.01

Baseline (pre-treatment) and spot checks omitted. Standard errors in parentheses clustered at household level. Columns 1 and 2 define usage as treated water with no detectable *E. coli*; dependent variable in columns 3 and 4 is a continuous measure of usage that calculates the log of *E. coli* in "drinking water" (treated water if present, else pre-treated water). More negative values imply more intense usage with this definition. Odd numbered columns contain results across commitment treatment for "patient" homes; even numbered columns contain similar results for "present-biased" households, as defined by responses to hypothetical survey question about future payoffs. More details in text.

Table 2.6: All Treatments Combined

	(1) T=0	(2) T=0	(3) T<10	(4) Self-Report	(5) Ln *E. coli*	(6) U<10	(7) Collection Time (Minutes)
1=received source info	0.098	0.100	0.127	0.085	-0.601	0.074	6.15
	(0.05)**	(0.05)**	(0.05)**	(0.05)	(0.27)**	(0.05)	(2.93)**
1=received own info	-0.010	0.018	-0.015	-0.054	-0.114	-0.015	-1.64
	(0.04)	(0.05)	(0.04)	(0.04)	(0.20)	(0.03)	(2.10)
1=received own info & dirty		-0.041					
		(0.05)					
1=received contrast frame	0.054	0.054	0.031	0.052	-0.317	-0.015	0.49
	(0.03)**	(0.03)**	(0.03)	(0.03)**	(0.14)**	(0.03)	(1.50)
1=received commitment	0.039	0.038	0.036	0.051	-0.138	-0.024	-1.04
	(0.03)	(0.03)	(0.03)	(0.03)*	(0.14)	(0.03)	(1.50)
Mean Dep. Var	0.402	0.402	0.583	0.676	0.960	0.370	24.05
	(0.03)***	(0.03)***	(0.03)***	(0.03)***	(0.17)***	(0.03)***	(1.67)***
Observations	1430	1430	1430	1133	1357	1430	1132
No. Clusters	28	28	28	28	28	28	28

Nonparametric village cluster bootstrapped standard errors in parentheses. *p<.10, **p<.05 ***p<.01

Results from OLS estimation of equation 2.5 for all treatments combined. Baseline wave omitted. All models include survey wave and product fixed effects. Results are similar if village fixed effects are also included. Columns 1 and 2 define usage as treated water with no detectable *E. coli;* column 3 indicates share of household observations with treated water *E. coli* MPN < 10 CFU/100 mL. Column 4 defines usage as self-reporting treatment: current water is treated, treatment was in the past 7 days, and household reports use of POU product every time water is collected. Column 5 is a continuous measure of usage that calculates the log of *E. coli* in "drinking water" (treated water if present, else untreated water). More negative values imply more intense usage with this definition. Column 6 presents results for rates of untreated water with *E. coli* MPN < 10 CFU/100 mL. Column 7 presents results for round-trip water collection time in minutes. Columns 4 and 7 exclude observations from spot checks when these outcomes were not measured. .Mean of dependent variable gives base rate of usage across three products at the first two-month follow-up survey wave.

Table 2.7: Ordered Logit Results on Cumulative Usage, Marketing Treatments

	(1)	(2)	(3)	(4)	(5)	(6)
	T=0	T<10	T=0, U>0	Self-Report	U=0	U<10
Contrast Frame	0.344	0.224	0.292	0.302	-0.294	-0.053
	(0.215)	(0.188)	(0.233)	(0.175)*	(0.266)	(0.151)
Commitment Treatment	0.381	0.287	0.348	0.276	-0.008	-0.253
	(0.190)**	(0.188)	(0.197)*	(0.141)*	(0.213)	(0.212)
Both Marketing Treatments	0.749	0.512	0.648	0.567	-0.315	-0.300
	(0.244)***	(0.235)**	(0.320)**	(0.211)***	(0.379)	(0.248)

*p<.10, **p<.05, ***p<.01

Standard errors in parentheses. Results in log-odds from ordered logit estimates of cumulative effects of contrast framed message, commitment treatment, and both marketing treatments combined on usage. Estimations are on the final survey wave of 370 households for the contrast frame and commitment treatment. The estimations for both marketing treatments combined in the bottom row include only those households that received neither or both marketing treatments and have 185 total observations. Each column has a different outcome variable that counts the number of times a household was observed to be using its product (according to the differing definitions of use) across the 3 two-month follow-up survey waves (therefore possible values are 0, 1, 2, 3). Column 1 counts number of times a household's treated water had no detectable *E. coli*; column 2 counts the number of times a household had treated water *E. coli* MPN < 10 CFU/100 mL. Column 3 counts the number of times a household had untreated (pre-treated) water testing positive for *E. coli* and treated water testing negative. Column 4 counts the number of times a household self-reported treatment: current water is treated, treatment was in the past 7 days, and household reports use of POU product every time water is collected. Columns 5 and 6 present results for untreated water quality using same definitions as columns 1 and 2 for treated water. Column 5 counts the number of times a household had untreated water with no detectable *E. coli*; column 6 counts the number of times a household had untreated water *E. coli* MPN < 10 CFU/100 mL.

Table 2.8: Unbalanced Means Across Randomizations

Product Imbalances:	WaterGuard	Pur	Filter	p-value F-stat
% Respondents ever used WaterGuard	0.515	0.376	0.459	0.072
Frame Imbalances:	**Contrast**	**Positive Only**		**p-value t-test**
# of children < 5	1.815	2.025		0.021
# of children < 1	0.327	0.415		0.086
% HHs w/ child < 5 recent diarrhea	0.380	0.465		0.086
Commitment Imbalances:	**Control**	**Treat**		**p-value t-test**
% Respondents w/ only 1 spouse	0.675	0.755		0.077
% HHs w/ child < 5 recent diarrhea	0.465	0.380		0.086
Baseline Information Imbalances:	**Zero**	**Source**	**Source + Own**	**p-value F-stat**
% HHs w/ zero *E. coli*	0.059	0.040	0.269	0.000
% HHs w/ *E. coli* MPN < 10 CFU/100 mL	0.277	0.236	0.581	0.000
% Respondents w/ 2ndary education or above	0.128	0.271	0.172	0.083
Household size	6.221	5.952	5.522	0.096
% HHs w/ permanent roof	0.499	0.691	0.688	0.045
% HHs w/ latrine	0.695	0.572	0.792	0.010
% Respondents talk w/ neighbors about water	0.289	0.402	0.264	0.084
% Respondents report losing a child	0.426	0.352	0.231	0.008
% Respondents own a phone	0.481	0.687	0.610	0.008

**All water quality variables balance across information categories at Follow Up 1 when information treatments were first administered.*

Note: For each randomization, this is a complete list of those variables that do not balance of 55 considered.

Chapter 3

The Role of Experience and Other Findings on Safe Water Behaviors

The previous two chapters have considered the roles of product design, marketing, and information in achieving widespread adoption of safe water technologies. In this chapter we have two distinct points of focus. First, we look at the results of our collected willingness to pay data and explicitly consider the role that experience plays to encourage valuation for the different safe water technologies. Second, we present some of the unexpected findings from our field experiment in Kenya and consider what these can teach us more generally about how households form preferences over safe water products as well as choose to adopt safe water behaviors. We begin first by motivating and presenting the results of our willingness to pay exercise.

3.1 Experience and Willingness to Pay

Safe water technologies, like many other private health products such as mosquito nets and condoms, can be described as experience goods. Experience goods are characterized by people not knowing how much they like a good until they experience it (Nelson 1970). If a health product is an experience good and households must learn its benefits through use of it, poor households that have not been exposed to it previously may have good reason not to purchase it, as doing so risks that they will have paid the product's price only to discover that their valuation of it is less than what they paid. It seems plausible that resource-scarce poor households would be particularly averse to any form of buyer's remorse, and this could contribute to the failure of many potentially life-saving health products to achieve both commercial viability and widespread adoption in developing country markets.

This chapter begins by asking what role experience plays in the market for one type of health product, point-of-use safe water technologies. It develops a model and empirically tests whether POU technologies that are currently sold in the markets of many developing countries are experience goods whose valuation on the part of consumers changes following experience with them. If experience is found to increase consumers' valuations of one or all of these health products, and in particular, increase consumers' valuations to a level that allows for cost-recovery of their supply, it suggests that the commercial viability of these products may be within reach with appropriate marketing strategies. If, on the other hand, these products are found not to gain valuation through experience on the part of consumers, and their valuations do not allow for sufficient cost-recovery, it could imply that the policy goal of achieving widespread access to safe drinking water will be difficult to achieve by the private sector alone, at least on a commercially-viable basis.

3.1.1 Study Design

To calculate the value of experience we conducted a willingness to pay study in conjunction with the cyclical disbursement of products as described in section 1.1.2 of Chapter 1, and orthogonally to the marketing and informational treatments of Chapter 2 section 2.1.1.

At the baseline interview, after the product introductions, we collected willingness to pay (WTP) information in a closed-ended double-bounded dichotomous choice contingent valuation (CV) exercise. WTP information was collected over all products from a respondent only after all three POU products had been introduced and before a household knew which product it would be assigned for a two-month trial. The order of product introductions and WTP questions was randomized across households to minimize any ordering effects. A more complete discussion of the WTP solicitation format used, as well as scripts, can be found in Appendix A.2.

Two months later when all households were revisited to cycle them through new product trials, updated WTP data were gathered in identical fashion. This process was repeated at each two-month follow-up survey as households accumulated experience with the various safe water technologies.

The collected WTP information provide a cardinal measure of consumer valuations for the POU products. Although stated preference WTP measures are often biased upwards (Murphy, Allen, Stevens and Weatherhead 2003), the *change* in WTP following experience can provide insight into the value of experience, beyond the ordinal measures we collected with respect to consumer preferences over the technologies as well as usage of them.

3.1.2 Literature Review

There is some disagreement in the literature about what effect "experience" with a product will have on consumer valuations for it. Crocker and Shogren (1991) hypothesize that respondents in a CV study will overstate their WTP for an unknown commodity because they value the information brought about by better understanding how the commodity enters their utility function. They test this "preference learning" hypothesis in the laboratory on undergraduate students with respect to self-insurance and self-protection goods in a market for risk-reduction after they endow the students with \$10, and they find results generally favorable to their hypothesis. Likewise, Shogren, List and Hayes (2000) find a negative "preference learning" effect that causes the high price premia paid for new food products in lab valuation exercises to disappear once respondents have experienced the new product. These studies argue that a respondent's valuation for an unknown good in a CV exercise will include two components: the consumption value of the good, and the information value of learning one's preferences for the good (Grossman, Kihlstrom and Mirman 1977, Crocker and Shogren 1991, Shogren et al. 2000). Thus, after learning about the good, valuations for it will decrease. Hoehn and Randall (1987), meanwhile, hypothesize that initial WTP estimates will always be understated for an unknown commodity due to a "value formation problem" brought about by time and resource constraints unique to the CV choice context. For example, survey enumerators may be under pressure to collect as many observations as possible under a limited research budget, causing them to limit the time devoted to explaining fully a proposed valuation question to each respondent. Alternatively, it is possible that the respondent does not devote her full resources towards the formation of a value in the CV context. Unlike Crocker and Shogren (1991), Hoehn and Randall (1987) assume respondents know how an unknown commodity enters their utility function, and under this assumption they show that both imperfect communication and incomplete optimization will cause WTP estimates to be understated for an unknown commodity relative to a value measure derived at under ideal circumstances.

Similar to Crocker and Shogren (1991), we hypothesized that consumers may not know how a new POU product enters their utility function, but we argue in this paper that "experience" with a POU product need not always lead to lower valuations on the part of consumers. Rather, the value of information (experience) may be positive or negative depending on which of two factors dominates: the value of learning how the good fits into a consumer's preference set, which should diminish following experience; and the value of learning about the benefits of safe water, which we hypothesize is either zero or positively related to experience with a POU safe water product. Furthermore, we believe that any information value related to "preference learning" is of limited importance for our setting. The difficulties associated with introducing new health products into developing country markets seem to support the stance that poor consumers are less likely to take into account the value of learning how a new product fits into their preference set. This is likely because our study population operates under much more stringent budget constraints than undergraduate students in the US, which could make them much less open to product experimentation. Finally, it seems entirely likely that the respondents in our study may initially underestimate the value of having a POU product in their homes if they do not have a clear understanding of the benefits of safe drinking water until after experiencing it through the use of a POU product. Our findings in chapter 2 of a large effect on usage of safe water products in response to being provided information about water quality supports this argument.

Our study is close in aim to that of Jalan, Somanathan and Chaudhuri (2003), who hypothesize that lack of awareness about the adverse health effects of dirty drinking water plays a significant role in keeping demand for water purification systems low among urban households in Delhi, India. They proxy "awareness" by the schooling levels of household members, recent experience with diarrhea in the household, and exposure to mass media outlets, and estimate the effects of "awareness" on WTP while controlling for wealth. Their estimated "awareness" effects on WTP are similar in magnitude to the effects of wealth. However, they do not have direct measures of WTP from a CV survey and instead construct measures of WTP based on estimated costs of different POU technologies, household characteristics and household water purification behaviors. We hypothesize that direct, hands-on experience with a POU product is another means to increase consumer "awareness" about the benefits of clean drinking water. We are interested in calculating the value of this hands-on experience and the next section outlines our strategy to do this.

3.1.3 Model

For many consumer products, it is the consumer's initial estimate of utility for the product that determines the purchase decision, but this is often based on imperfect information about the quality of the good as well as the utility associated with it. After using the good and learning its "experienced" quality, a consumer's posterior estimate of utility for the good will take into account this updated estimate of quality. Thus, if a consumer's utility estimates for the product change following experience, this implies that initial product prices could turn away some consumers who otherwise would gain positive utility from having the good.

In this chapter we combine models by Grossman et al. (1977) and Rauh and Seccia (2006) to estimate an endogenous learning-by-doing model of health production wherein households learn the true quality of, and therefore their true valuations for, the POU products through experience with them. We will measure the value of experience with these POU technologies as the change in willingness to pay responses following experience with a technology. To calculate a consumer's willingness to pay for a technology, we use a closed-ended double-bounded dichotomous choice CV survey designed to elicit people's valuation of a POU product, and compare this valuation before and after a consumer experiences a randomly assigned product. If peoples' valuations for a POU product change following experience with it, it implies that the product is an experience good. Depending on the direction and magnitude of the effect of experience on people's valuations, this information could be useful in the design of marketing strategies for the product. In particular, if experience with a product has a beneficial effect on people's valuation of it, then free samples might be a good marketing strategy for WaterGuard and Pur, while a return policy might work for the filters. On the other hand, if consumers learn they do not like a technology following experience with it, it spells trouble for the technology's market viability.

Formally, consider a representative consumer who lives for $T > 0$ periods and produces health h_t in each of those periods. Assume that diarrhea and other waterborne diseases are the only forms of ill-health that the consumer need consider and that the only action she can take to prevent such negative health experiences is to use her assigned POU product q, where $q \in \{W, P, F\}$, corresponding to WaterGuard, Pur, and the filter, respectively. Due to variations in environmental factors as well as the fact that drinking contaminated water is not the only route through which one contracts diarrhea, the health signal observed by the consumer in any given period from use (or nonuse) of POU product q will vary. Accordingly, we follow Grossman et al. (1977) and assume that realized health in period t, h_t, is related to use of POU product q at time t by the linear equation:[1]

$$h_t = \alpha + \theta^q \chi_t^q + \epsilon_t \tag{3.1}$$

where χ_t^q is an indicator of use of POU product q in period t, α represents "average" health over time in the absence of the use of any POU product, and ϵ_t is an iid normal random variable that allows for random variations in health independent of POU product use or nonuse. In the absence of using any POU product at time t, the consumer experiences health $\alpha + \epsilon_t$. We assume the consumer knows her history of health experiences without use of a POU product, and thus knows

[1] We assume POU product use at time t affects health in the same period. A more appropriate description might be that POU use at time $t-1$ helps to produce health at time t due to the investment good nature of POU products. This simplification is made to reduce the computational burden and should not affect the intuition of the problem.

α and the variance of ϵ_t, which we assume to be 1.

Thus, θ^q represents the contribution of use of POU product q on the consumer's realized health and thereby represents the "quality" of POU product q. We assume the consumer does not know θ^q at the start of the first period but has beliefs about the true value of θ^q for each POU product q that she updates at the end of each period as she accumulates experience with it.[2]

We assume the consumer derives utility in each period from consumption of market goods denoted $\vec{x}$, her health experienced in that period, h_t, and any costs associated with expended effort in the use of POU product q, β^q.[3] The consumer's utility function is assumed to be additively separable in these inputs and can be written:

$$\bar{U}_t = w(\vec{x}_t) + h_t - \beta^q \chi_t^q \tag{3.2}$$

In each period the consumer purchases at most one POU product q subject to her budget constraint:

$$\vec{x_t} + p^q \chi_t^q = y \tag{3.3}$$

where the prices of market goods in $\vec{x}_t$ have been normalized to 1, p^q is the relative price of POU product q, and y denotes the consumer's income. (For simplicity we hold all prices and income constant across time periods.) The consumer's budget constraint specifies that her decisions to purchase and use a POU product are one and the same. This does not allow for a situation in which she purchases POU product q and then fails to use it. The reason for our conjoining these two decisions into one will become clear later on. Additionally, because in this setting the POU products q are assigned to the consumer and she has at most one POU product at a time, only one p^q enters her budget constraint at a time.

To keep the model tractable, we follow Rauh and Seccia (2006) and restrict the consumer to live for two periods (T = 2) and we allow for only two possible quality levels for θ^q, θ_L^q and θ_H^q, with 0 < θ_L^q <θ_H^q, corresponding to low and high quality, respectively. Assume the true quality of product q is given by θ_H^q, but due to the uncertainty of the health signal observed as well as the consumer's lack of familiarity with the product and its intended benefits, ex ante the consumer is unsure if the quality is θ_L^q or θ_H^q. Assume the consumer's initial priors about product quality are evenly split across the two possible quality levels; i.e., the consumer believes $\theta^q = \theta_L^q$ and $\theta^q = \theta_H^q$ with equal probability, and thus the consumer has initial beliefs $\bar{\theta}^q = \frac{1}{2}[\theta_L^q + \theta_H^q]$. Also assume that $\bar{\theta}^q \geq \beta^q$. This assumption is made only to keep the problem interesting. If this inequality failed to hold, she would never choose to purchase and use POU product q. Finally, assume a minimum overlap in possible quality signals such that $\theta_L^q + 1 > \theta_H^q - 1$.

In the first period, the consumer decides whether to use her POU product or not. This decision affects her health (and therefore utility) in period 1, but also affects her utility in period 2 as she observes her first period health and uses this information to update her beliefs about θ^q in the

[2]If the consumer never uses a product in a period and sets $\chi_t^q = 0$, there is no further information with which to update her beliefs about product quality in period $t+1$ and thus no learning has taken place. In practice, this happened in just 30 of over 1100 observations (2.5%) of household behavior.

[3]We do not model the disutility associated with POU product use as an unknown varaible in the consumer's problem for two reasons. One is to keep the model simple. Two, we assume the costs of effort with a POU product can be learned with certainty upon first use, while the benefits of such usage may not reveal themselves fully after first use due to random health shocks (as capture by ϵ_t).

second period. If the consumer used her POU product q in the first period, the probability that the consumer believes $\theta^q = \theta^q_H$ at the start of the second period is given by:

$$\rho(h_1, \chi^q_1|\theta^q_L, \theta^q_H) = \frac{f_H}{f_H + f_L} \tag{3.4}$$

where$f_H \equiv f(h_1 - \alpha - \theta^q_H \chi^q_1)$ and$f_L \equiv f(h_1 - \alpha - \theta^q_L \chi^q_1)$ represent conditional densities based on her newly perceived probabilities that $\theta^q = \theta^q_L$ and $\theta^q = \theta^q_H$ (Rauh and Seccia 2006). If the consumer chooses $\chi^q_1 = 0$, she does not learn any information about θ^q and her beliefs on product quality remain $\bar{\theta}^q$. The consumer's problem is to choose $\{\chi^q_1(\theta^q_L, \theta^q_H), \vec{x}_1\}$ and $\{\chi^q_2(\theta^q_L, \theta^q_H), \vec{x}_2\}$ to maximize her prior expectation of $\bar{U}_1 + \bar{U}_2$.

Starting with the second period, the consumer's decides whether to use her POU product or not as well as her consumption of market goods in $\vec{x}$ subject to her budget constraint and based on her beliefs about product quality. These beliefs in turn are conditional on the information about product quality at her disposal at the start of period 2. Let this expectation be denoted by $E[\theta^q|I_2]$, where I_2 is the consumer's information set at the start of period 2 and includes information about h_1 and θ^q_1.

Thus, in the second period the consumer maximizes her expected value of 3.2 subject to 3.3. If the consumer sets $\chi^q_2 = 0$, she will spend all of her available income on market goods in $\vec{x}$ and we can therefore insert her budget constraint directly into her expected utility, given by:

$$E[\bar{U}(\chi^q_2 = 0)|\theta^q_L, \theta^q_H] = w(y) + \alpha \tag{3.5}$$

If instead the consumer chooses $\chi^q_2 = 1$, she will have expected utility:

$$E[\bar{U}(\chi^q_2 = 1)|\theta^q_L, \theta^q_H] = w(y - p^q) + \alpha + E[\theta^q|I_2] - \beta^q \tag{3.6}$$

Thus, the consumer will choose to use product q in the second period if and only if:

$$E[\bar{U}^1|\theta^q_L, \theta^q_H, \chi^q_2 = 1] \geq E[\bar{U}^0|\theta^q_L, \theta^q_H, \chi^q_2 = 0] \iff w(y - p^q) + \alpha + E[\theta^q|I_2] - \beta^q \geq w(y) \tag{3.7}$$

where $E(\theta^q|I_2) = \rho\theta^q_H + (1 - \rho)\theta^q_L$ and $\bar{U}^1$ ($\bar{U}^0$) denotes the state of having (not having) the good. That is, the consumer will use POU product q in the second period if and only if her expected utility from using the product is greater than that from not using it. This expected utility depends on the consumer's expectations about product quality $E(\theta^q|I_2)$, which in turn are directly related to her first period decision for χ^q_1 as well as her first period realized health signal, h_1. Note that if the consumer chooses $\chi^q_1 = 1$ and then draws a large and negative ϵ_1, both $\rho < \frac{1}{2}$ and $\rho\theta^q_H + (1 - \rho)\theta^q_L < \beta^q$ are possible consequences. In such a scenario, the consumer will drop out and not try her POU product in the second period as she sees no benefit in doing so based on her first period's experience. The phenomenon that many consumers begin to use a POU product but drop out over time is thus explained in this model as being due to drawing a large, negative ϵ_1 after choosing $\chi^q_1 = 1$. In practice, we collected suggestive evidence of this by asking the consumer if she has recently experienced diarrhea and compare her response with her WTP and usage decisions.

In the first period, the consumer chooses χ^q_1 and x_1 to maximize:

$$w(x_1) + h(\chi^q_1|\theta^q_L, \theta^q_H) = w(x_1) + \alpha + \bar{\theta}^q\chi^q_1 - \beta^q\chi^q_1 + E(V_2|I_1) \tag{3.8}$$

where we introduce the term $E(V_2|I_1)$, which is meant to capture the consumer's present discounted expected value of the information about product quality from her first period experience that she can carry over to the next period. Because the consumer's first period actions will provide information to her about product quality that she can use to inform her decisions in the second period, we allow for the possibility that the consumer will be forward-thinking and therefore allow for this term to enter her first period problem. This allows for the consumer's choice of χ_1^q to be related to her desire to improve upon her estimate of product quality $\bar{\theta}^q$ in the second period.

Intuitively, the value of this information is the value (measured in utility terms) that the consumer places on the amount of learning about product quality she expects to achieve by choosing $\chi_1^q = 1$. We do not specify an exact valuation function for how the consumer calculates $E(V_2|I_1)$, but we do allow for it to enter the consumer's first period problem. If the consumer does not take the value of learning into account in the first period and acts as a myopic consumer, $E(V_2|I_1)$ will equal zero. This is the outcome we hypothesize in section 3.1.2. If she does take the value of learning about θ^q into account in her first period decision, this term will be nonzero but unobserved and in the error term of an empirical specification.

If the consumer chooses $\chi_1^q = 0$ in the first period, her expected utility is given by $w(y) + \alpha$. If the consumer chooses $\chi_1^q = 1$ instead, her expected utility is $w(y - p^q) + \alpha + \bar{\theta}^q - \beta^q + E(V_2|I_1)$. Thus, the consumer will choose to use product q in the first period if and only if:

$$w(y - p^q) + \bar{\theta}^q - \beta^q + E(V_2|I_1) \geq w(y) \tag{3.9}$$

Now, this outline of the consumer's learning process thus far assumes that each POU product q has a well-defined and stable price, p^q. However, in our setting we are collecting WTP estimates for each POU product and we are interested in how these WTP estimates change over time. Thus we link our model above with the consumer's WTP in the following manner. We assume that the consumer only has a positive WTP for POU product q when her expected utility from using the product is greater than her expected utility from not using it. This means that instead of the consumer's purchase and usage decisions being one and the same, the consumer's WTP decision and her decision regarding product usage are linked as one joint decision. Thus, when the consumer is asked her WTP about product q at time 1, she will calculate the p_1^q that satisfies:

$$w(y - p_1^q) + \bar{\theta}^q - \beta^q + E(V_2|I_1) = w(y) \tag{3.10}$$

where $0 < p_1^q < y$. If no such feasible p_1^q exists for the consumer to satisfy 3.10, the consumer's WTP is zero at $t = 1$ and we expect the consumer to be more likely not to use her assigned POU product during the product trial.

In the second period, the consumer's WTP for product q is the p_2^q that satisfies:

$$w(y - p_2^q) + E(\theta^q|I_2) - \beta^q = w(y) \tag{3.11}$$

where $0 < p_2^q < y$. Again, if no such feasible p_2^q exists, the consumer's WTP will be zero at $t = 2$.

We are interested in how her WTP changes over time as she learns more about θ^q, and thus want to calculate the difference $p_1^q - p_2^q$. We therefore model her WTP as measured by her stated "prices" in terms of her expenditure function as follows:

$$
\begin{aligned}
WTP_t \equiv\ & p_t^q = e(1, 0{\times}E[\theta_t^q|I_t, \theta_L^q, \theta_H^q], 0{\times}E(V_2|I_1){\times}T, \bar{U}_t^0) \\
& -e(1, 1{\times}E[\theta_t^q|I_t, \theta_L^q, \theta_H^q], 1{\times}E(V_2|I_1){\times}T, \bar{U}_t^0) \\
& = y - e(1, 1{\times}E[\theta_t^q|I_t, \theta_L^q, \theta_H^q], 1{\times}E(V_2|I_1){\times}T, \bar{U}_t^0)
\end{aligned} \tag{3.12}
$$

where $e(\cdot)$ denotes the consumer's expenditure function, the first argument of her expenditure function is the normalized prices of market goods in $\vec{x}$, and the consumer's expectations about product quality $E[\theta_t^q|I_t, \theta_L^q, \theta_H^q]$ only enter the consumer's expenditure function in the state of having the product. Finally, T is an indicator variable that takes on the value of 1 only when $t = 1$ (the first period), and thus the value of information $E(V_2|I_1)$ only enters the consumer's expenditure function in the first period and in the state of having the good. This term as well as the one measuring expectations of product quality are the ones that we expect to change over time as a consumer learns about product quality. Finally, $\bar{U}_t^0$ represents the consumer's utility at time t in the state of not having the good (and thus $p_t^q = 0$ and $\chi_t^q = 0$).

We want to estimate:

$$
\begin{aligned}
WTP \equiv\ & p_2^q - p_1^q \\
=\ & [y - e(1, 1{\times}E[\theta_2^q|I_2, \theta_L^q, \theta_H^q], 1{\times}E(V_2|I_1){\times}0, \bar{U}_2^0)] \\
-\ & [y - e(1, 1{\times}E[\theta_1^q|I_1, \theta_L^q, \theta_H^q], 1{\times}E(V_2|I_1){\times}1, \bar{U}_1^0)] \\
=\ & e(1, 1{\times}E[\theta_1^q|I_1, \theta_L^q, \theta_H^q], 1{\times}E(V_2|I_1){\times}1, \bar{U}_1^0) \\
-\ & e(1, 1{\times}E[\theta_2^q|I_2, \theta_L^q, \theta_H^q], 1{\times}E(V_2|I_1){\times}0, \bar{U}_2^0)
\end{aligned} \tag{3.13}
$$

That is, we want to estimate the change in the consumer's WTP as she updates her beliefs about θ^q based on her first period experience and as the value of preference learning $E(V_2|I_1)$ decreases. As she accumulates experience with POU product q in the first period, the change in her WTP effectively captures the change in her expectations about product quality across time periods, $E(\theta^q|I_2) - \bar{\theta}^q$, minus the amount that she initially anticipated valuing learning about this change, $E(V_2|I_1)$. The relative magnitudes of these two factors (that we expect to be countervailing) will determine whether her experience with POU product q causes her valuation of it to increase or decrease.

3.1.4 Data Description and Estimation Strategy

We collected WTP data from all respondents for all three included POU products at four successive survey waves (baseline, and three consecutive follow-up surveys). WTP responses were collected in a closed-ended double-bounded dichotomous choice contingent valuation format, and the order of WTP questions by product was randomized over households. Starting bid prices P_{qit} for the products were also randomized across households. Depending on the response to a starting bid for a product q, a second bid price was asked such that $P_{qit}^H > P_{qit}$ if the response was "yes," and $P_{qit}^L < P_{qit}$ if the first response was "no." There are thus four possible observed response patterns for each product q at time t: yes/yes, no/no, yes/no, and no/yes. We analyze these data using interval regression to allow for the bounded nature of our WTP estimates.

The randomized starting bid prices for each product were chosen such that the highest possible starting bid price was set equal to the product's prevailing market price. This was done in anticipation that prior to our baseline survey a majority of households in the study area would be familiar with WaterGuard and Pur, as well as with their prevailing market prices, and would therefore never accept a price above these thresholds. Indeed, 98% and 89% of baseline respondents had heard of WaterGuard and Pur, respectively, while 39% and 17% reported previously having purchased the products at baseline (see Table 1.1). A similar strategy was used successfully by Ashraf et al. (2007) in their study of the role of charging positive prices for a chlorine product in urban Zambia. In our case, this strategy backfired in that it resulted in a troublingly high acceptance rate for the initial bid prices of WaterGuard and Pur through the first three waves of the study. In hindsight, we believe that the subsidized market prices of WaterGuard and Pur are already so low that it may have felt embarrassing for households to say "no" to such low prices.[4] Panel A of Figure 3.1 and Panel A of Figure 3.2 show the relatively inelastic demand curves for WaterGuard and Pur from these waves, respectively. This problem was detected in time to raise bid prices for the final survey wave. Panel B in these Figures show the share of households willing to pay the higher starting bid prices for WaterGuard and Pur in the final wave (at which point all households are "fully experienced" consumers). Due to the inelastic demand curves generated by the first three rounds of data for WaterGuard and Pur as well as the likelihood that these data are inflated upwards due to courtesy bias, we will focus on results for the filter when looking at the role of experience on WTP. Figure 3.3 shows that the filter initial bid prices were sufficiently high to generate variation in the responses to our WTP questions.

To estimate the change in WTP for the filter, we begin by estimating a WTP distribution over the population and look at how that changes following experience with the filter. Equation 3.12 specifies the consumer's WTP for product q to be a function of income, prices of other market goods, and value of and estimated quality associated with having the good, which is itself a function of experience. However, our identification of the effect of experience cannot rely on estimated product quality in practice, as this is an unobserved variable. Rather, it will come through the change in people's WTP following experience with q. To disentangle the effects of experience from any general time effects, we will include the WTP responses of households that have and have not yet experienced the filter in a double-difference framework; by comparing the change in WTP of those households that have experienced the filter with the change in WTP of those households that have not yet experienced it, we arguably are able to isolate any effects due to experience from general time effects given the randomized order of filter assignments to households.

Figure 3.3 plots the share of households willing to pay a given initial bid price for the filter from the first two rounds (thus, there should be no relative product preferences affecting these results). It compares the baseline WTP for all households against two subgroups from the first follow-up survey two months later: the WTP for the filter among those households whose first assigned product was WaterGuard or Pur, and the WTP for the filter among those households that had just experienced the filter. Figure 3.3 suggests that WTP for the filter rises with experience. In particular, it increases among all households following experience with any safe water technology (a general time effect), but that it increases even more for those households that had just experienced the filter

[4]Pilot WTP surveys were conducted in a nearby village and in 15 observations we were not able to foresee this problem.

for the previous two months. Such a pattern could be explained by all households learning the value of safe water with POU experience, and filter households also learning exactly how the filter fits into their preference set and therefore valuing it more. Of course, Figure 3.3 does not take into account other changes between survey rounds such as changes in income. We therefore below present an estimator of WTP that attempts to control for such countervailing factors.

To estimate the role of experience on WTP for the filter as in Equation 3.13, we begin by including only the first two waves of WTP data when each household has experienced exactly one product and relative product preferences are not a factor. We use interval regression to account for the bounded nature of our collected WTP data and estimate the double-difference:

$$WTP_{ivqt} = \alpha + \delta Log(Price)_{ivqt} + \beta_1 T_t + \beta_2 Fl_{ivqt} + \beta_3 (T_t \times Fl_{ivqt}) + \vec{X}'_{ivt}\vec{\pi} + \varepsilon_{ivqt} \quad (3.14)$$

where WTP_{ivqt} represents the stated interval willingness to pay of household i in village v at time t for product q (the filter in this case), and $Log(Price)_{ivqt}$ controls for the log of the initial bid price offered to household i at time t for product q. T_t is an indicator that takes on the value of 0 for the baseline WTP responses, and 1 for WTP responses given at the first follow-up survey round after each household has experienced exactly one POU product. Fl_{ivqt} is another indicator that equals 1 if household i was assigned the filter during the first product trial. Thus, β_2 should equal 0 if we randomized first product assignments correctly, and β_3 on the interaction of "filter first" households with the time dummy should capture the differential change in WTP for the "filter first" households from other households yet to experience the filter. The $\vec{\pi}$ will capture changes in variables over time that could confound the effect of experience. In practice, this will include indicators for positive and negative changes in income for households between surveys in order to control for income changes over time affecting WTP. ε_{ivqt} is the error term that we will cluster at the village level.

From chapter 2 we saw that our marketing and information treatments positively affected usage of the safe water products. We can test how these treatments, as well as other correlates, affected households' WTP for the filter by including all waves of data and estimating:

$$\begin{aligned} WTP_{ivpt} &= \alpha + \delta Log(Price)_{ivpt} + \gamma_1 S_{v,t-1} + \gamma_2 O_{v,t-1} + \gamma_3 F_{i,t-1} + \gamma_4 C_{i,t-1} \quad (3.15) \\ &\quad + \theta_v + \theta_t + \beta(\theta_t \times Fl_{ivqt}) + \vec{X}'_{iv}\vec{\lambda} + \vec{X}'_{ivt}\vec{\pi} + \varepsilon_{ivpt} \end{aligned}$$

where variables are as defined before, but now $S_{v,t-1}$ is an indicator variable that takes on a value of 1 if households in village v received information about source water quality at a previous visit to induce a response at time t, while $O_{v,t-1}$ is another indicator variable that equals 1 if households in village v received information about their own private stored supplies *in addition to* source water quality results, $S_{v,t-1}$. F_{it-1} indicates if household i received the contrast framing treatment in a previous wave, and C_{it-1} indicates that household i received the commitment treatment in a previous wave. We include survey wave fixed effects θ_t to control for any common time-varying factors, and village fixed effects θ_v to control for time-invariant differential village water source types and quality as well as village wealth characteristics. We estimate a vector of covariates $\vec{\lambda}$ to test how WTP varies with household characteristics that economic theory would suggest are important, as well as a vector $\vec{\pi}$ to capture changes in variables over time that could confound the effect of experience with a safe water product. Here, $\vec{X}_{ivt}$ will include indicators for whether a household reports a recent episode of diarrhea in addition to indicators for positive and negative income changes between surveys. Finally, we continue to estimate the effect of experience on WTP by including Fl_{ivqt}, but now it indicates if a household has previously experienced the filter prior to stating its WTP for the filter at time t (as opposed to having just experienced the filter in the previous round). We can estimate similar versions of equation 3.15 for WaterGuard and Pur but using only the final wave of data when bid prices were raised.

3.1.5 WTP Results and Conclusions

Table 3.1 contains results of estimation of equations 3.14 and 3.15 for the filter. In column 1 we restrict ourselves to consider just the first two survey waves, before and after each household has experienced exactly one randomly assigned product. This is the most clean test of the role of experience on WTP for the filter since it does not allow relative valuations between products. The "just experienced filter dummy" (Fl_{ivqt}) leads to a 31% increase in WTP for the filter, suggesting a large premium for experience and matching the picture in Figure 3.3. This experience premium remains when we consider all rounds of WTP data, although becomes somewhat smaller: having previously experienced the filter increases WTP by 25% on average across all waves. In both columns we see significant anchoring effects as WTP *rises* with starting initial bid prices. This result is suggestive that our stated WTP measures are inflated upwards even for the filter. Moreover, this suggests strong anchoring effects on WTP responses that do not fully dissipate with experience (if we interact survey wave dummies with the log of the starting bid price we get negative coefficients that do not fully offset these positive initial anchoring effects; results not shown).[5]

In column 2 we see the results of the marketing and information treatments on WTP for the filter (equation 3.15). We see that WTP rises in response to the provision of source water quality information, but there is no additional premium in response to the personalized information. This is consistent with the findings from chapter 2 wherein source water quality information significantly increased usage of the POU products but the personalized information did not further increase usage. However, unlike our findings on usage, we do not see that our marketing treatments have any significant effect on WTP for the filter. Despite this, we can see interesting correlations of household characteristics with WTP that often agree with economic theory. Households that have previously lost a child have significantly higher willingness to pay for the filter, possibly suggesting heightened aversion to future forms of sickness. Households that report having a toilet structure at baseline (usually a latrine) also have higher WTP, which could be consistent with these households having both greater capacity to pay as well as greater concern and/or awareness of general health and hygiene. Also, we find a positive correlation between households' baseline knowledge of the number of ways to prevent diarrhea and subsequent WTP for the filter. We find that households that report themselves to be liquidity constrained (measured here as answering it would be "very difficult" or "impossible" to secure 500 Kenyan shillings (Ksh; ~$6.25 in July 2009) in cash within 24 hours) have significantly lower WTP, as do households we label "impatient" due to their baseline

[5] In the second part of this chapter we consider in greater detail other anomalies in households' decisions and preferences over POU technologies.

hypothetical preference for 50 Ksh today versus 100 Ksh in one week (see chapter 2). This latter finding is consistent with our hypothesis that households with tight time constraints might have lower preference for the filter due to its slow filtration rate.

Table 3.2 contains similar tests of the correlates of WTP for WaterGuard and Pur. Due to the inelastic demand curves for WaterGuard and Pur generated by the first three rounds of data (Figures 3.1 and 3.2), we restrict ourselves to the final wave of data when bid prices were raised. Note this means that all households are equally experienced across all three POU products. We see that the initial bid price has the expected negative effect on WTP for both products when prices were raised sufficiently. We also see positive effects from having just experienced each product, although these findings are not statistically significant. We do not see large effects from our marketing treatments on WTP at the final wave for WaterGuard or Pur. We also see that similar to findings for the filter in Table 3.1, liquidity constrained households report lower WTP. Finally, we see some interesting differences between the WTP responses for WaterGuard and Pur. Namely, households with toilets report statistically significantly higher WTP for Pur, but lower WTP for WaterGuard, although the WaterGuard results are not significant. Also, households that report a recent episode of diarrhea during the last product trial have significantly higher WTP for Pur (36%), but not WaterGuard. This could be indicative of households trusting the efficacy of Pur more as the dry season set in during this wave: At the final exit wave when all households had experienced all products, 51% of households named Pur as the product that cleaned their water the best, while just 10% named WaterGuard (30% named the filter and 9% said all three products cleaned equally well).

The overall findings that our stated WTP estimates are likely inflated upwards on an absolute scale is discouraging; a reliable estimate of demand for POU products in this market would be valuable information towards expanding the number of households with safe water in this region. However, the findings in Table 3.1 and Figure 3.3 that suggest that experience with a filter can increase the relative demand for it is encouraging. Moreover, a 25% increase in average WTP following hands-on experience is quite large. If such an experience premium does not dissipate, and up front costs for a filter's purchase could be minimized, this could suggest a promising avenue for creative ways to market filters in rural Kenya.

3.2 Further Findings on Usage and Preferences

In the second part of this chapter, we explore other facets of the collected data from Kenya that are of general interest to understand more about how households make decisions to treat their drinking water and how they form preferences over POU products.

3.2.1 Hawthorne Effects

A central question in much of public health is how to get households to adopt healthy behaviors at minimal expense. POU products have gained attention in recent years as an effective and low-cost solution to the problem of rapidly expanding access to safe drinking water (Harris 2005). However, one reason POU products constitute an inexpensive policy solution is because they are characterized by transferring the burden of water treatment away from public agencies and into the hands of private individuals; moreover, they currently rely on private markets as the primary suppliers. This means relying on private behaviors to solve the problem, and yet we do not know much about how to achieve these behaviors (Luby et al. 2008, Zwane and Kremer 2007).

During each of the three product rotations, our study performed unannounced short "spot check" visits to a randomly selected subset of villages. The purpose of these spot checks was to get a glimpse of product usage at lengths of product exposure less than the full two-month cycles. Although these visits were no greater than five minutes in length and were intended only to collect water samples to verify usage of the products, we see evidence of their realizing large Hawthorne effects: At subsequent follow-up visits, usage is substantially higher among the subset of households that had received a spot check during that product trial. Figure 3.4 graphs separate nonparametric localized plots of usage trends over the length of exposure with a product by whether a household received a spot check prior to that follow-up survey. Seasonality and relative product performance do not affect the findings of Figure 3.4, which combines all survey waves (and hence spot checks) and all products into one measure. A similar plot of the share of households with no detectable E. coli in their pre-treated water does not show any differences across groups (results not shown), suggesting households respond in large numbers to the unannounced spot check visit by using their POU products.

Because Figure 3.4 does not account for differences in products or time, we further disentangle the effects of the spot checks on usage from other countervailing effects with the following estimation:

$$Y_{iptv} = \beta SC_{vt} + \delta_t + \delta_p + \delta_i + \varepsilon_{iptv} \tag{3.16}$$

where Y_{iptv} is some measure of usage of the safe water product p at time t by household i in village v, and SC_{vt} indicates if village v received a spot check prior to survey wave t. The δ_t and δ_p are survey wave and product fixed effects. In some estimations we will also include household fixed effects, δ_i, to estimate the within-household change in behavior due to the spot checks.[6] We cluster the error term ε_{ivpt} at the village level to account for the assignment of randomized spot checks at the village level. Due to the randomized assignment of villages to receive spot checks, β should deliver an unbiased estimate of the (inadvertent) average effect of spot checks on usage.

Table 3.3 contains the results of estimation of equation 3.16 for various definitions of usage. Column 1 defines usage as a household having either a positive chlorine test (if assigned WaterGuard or Pur) or the enumerator observing filter usage at a two-month follow-up visit. It shows that the subset of households that received a spot check prior to a full round of follow-up visits were 8 percentage points (significant at 1%) more likely to be found to be using their POU product than those households that did not receive a spot check. The impact of spot checks increases to 12 percentage points if we include household fixed effects with this definition (column 2), suggesting a 20% increase in usage in response to receiving a spot check. Results are similar if usage is instead defined as a household having no detectable E. coli in its supply of treated water (columns 3 and 4). A parallel test of the effects of receiving a spot check on untreated water quality shows no effect (column 7), suggesting that households respond to a spot check by *using* their safe water product and not through some other channel such as changing their water collection habits. Columns 5 and

[6]Results are very similar if we instead estimate village fixed effects.

6 present results for self-reported usage and show similar, although somewhat smaller, effects due to the spot checks.

From a policy perspective, the finding that a household visit can have such large effects on subsequent adoption could be discouraging news as such visits would be prohibitively expensive on a large scale. However, these findings do raise interesting questions about what benefits there may be from social pressure and other channels to achieve the behavior of widespread water treatment. The possibility that social networks can be an influential mechanism through which greater water treatment behaviors can be achieved arises. More research looking into this channel as a means to expand access to safe water may be warranted.

3.2.2 Recency and Primacy

Much of economic theory posits that individuals have innate, stable preferences (McFadden 2001). Rational theory postulates that consumers seek to maximize their utility subject to these preferences and any outside constraints. While the standard model does allow for experience to help agents learn their true underlying preferences, these preferences are assumed to be pre-determined. However, there is a wealth of evidence from the psychology, marketing and behavioral economics literatures that documents regular and predictable deviations from this rational model of consumer behavior (DellaVigna 2009). Two such commonly documented deviations from the rational model are primacy and recency effects on decision-making, wherein the order in which a consumer experiences a good can affect her preference over it. That is, in many scenarios the first or most recently experienced item or good in a sequence remains the most salient in a consumer's mind and can therefore affect her choices over the goods.

Whether deviations such as primacy or recency effects reflect temporary processing errors on the part of individuals, or a more fundamental failure of the assumptions of innate and stable preferences behind the rational model, remains a subject of much debate among economists and psychologists alike (e.g., seeMcFadden (2001)). Furthermore, there is some evidence that market experience may reduce instances of deviations from the rational model (List 2003).

In our study, at the baseline and each successive two-month follow-up survey wave, households were asked which product was their most preferred.[7] Because the order of product assignments was randomized across individuals and survey waves, we can test for the existence of primacy and recency effects affecting consumers' relative stated preferences over the products, as well as whether these effects dissipate over time as households gain experience with each successive product. After the first follow-up survey wave, each household had experienced exactly one (randomly assigned) product. After the final exit survey wave, all households were "equally treated" in the sense that all had experienced each of the three products for two months apiece. At this point in the study, households only differ by the *order* in which they experienced each product.

Table 3.4 looks for the existence of primacy or recency effects on households' stated product preferences over time. If household preferences are stable and pre-determined, we would expect to see no effects on preferences due to ordering. That is, after the first product trial we would expect

[7] The translated survey question is, "Which of these products do you like the best?" At baseline, this question was asked after households had been introduced to all three POU products; at follow-up surveys, this question was asked after enumerators reintroduced the products a household was yet to experience.

that (averaged over households):

$$H_0^1 : Pr(U_j^1|T^1 = j > U_{-j}^1|T^1 = j) = Pr(U_j^1|T^1 \neq j > U_{-j}^1|T^1 \neq j), \forall j \tag{3.17}$$

where U_j^1 signifies utility from product $j \in \{W, P, F\}$ for WaterGuard, Pur and the filter, respectively, at time $t = 1$, the first follow-up wave, and T^1 signifies the product assigned during survey wave $t = 1$. That is, the probability of product j being most preferred among those households that have experienced it is equal to the probability of j being most preferred among households that are yet to experience it. Of course, hypothesis H_0^1 in equation 3.17 does not allow for households to learn their true preferences with market experience, and furthermore cannot disentangle any primacy from recency effects since the first product experienced is also the most recent.

To disentangle primacy from recency effects as well as allow for experience to mitigate either ordering effect on household preferences, we can also test whether at the final round the following are true (again averaged over households):

$$H_0^2 : Pr(U_j^3|T^1 = j > U_{-j}^3|T^1 = j) = Pr(U_j^3|T^1 \neq j > U_{-j}^3|T^1 \neq j), \forall j \tag{3.18}$$

$$H_0^3 : Pr(U_j^3|T^3 = j > U_{-j}^3|T^3 = j) = Pr(U_j^3|T^3 \neq j > U_{-j}^3|T^3 \neq j), \forall j \tag{3.19}$$

Here, U_j^3 signifies utility from product $j \in \{W, P, F\}$ for WaterGuard, Pur and the filter, respectively, at the final survey wave (time $t = 3$), when all households have experienced all products, and T^t signifies the product assigned during survey wave $t \in \{0, 1, 2, 3\}$. Hypothesis H_0^2 in equation 3.18 tests for the existence of primacy effects at the final exit wave, while hypothesis H_0^3 in equation 3.19 tests for the existence of recency effects at this wave.

Table 3.4 calculates the share of households preferring each product by their first, or most recent, assigned product. In Panel A of this Table we see the results of equation 3.17 at the first follow-up survey wave. The first product a household has just experienced significantly raises its likelihood of being most preferred at the two-month mark of the study. Although we cannot disentangle primacy from recency effects at this point in the study, and furthermore market experience is not equal across households, the combined effects result in 84% (for the filter) to nearly 500% (for Pur) increases in the likelihood that a product is named as the most preferred if a product is the one experienced by a household (when comparing against that product's likelihood of being most preferred among those who are yet to experience it). F-tests reject the null hypothesis H_0^1 of no combined primacy and recency effects at the two-month mark of the study for all products.

Panel B tests hypothesis H_0^2 in equation 3.18 and looks for primacy effects affecting the final product preferences of households after they have experienced all products. We do not see any evidence that the first product a household experienced affects its final product preferences; we fail to reject H_0^2. However, this could suggest that households learn their true preferences through experience, or that the large primacy + recency effects we saw after the first product trial in Panel A were mostly due to recency effects.

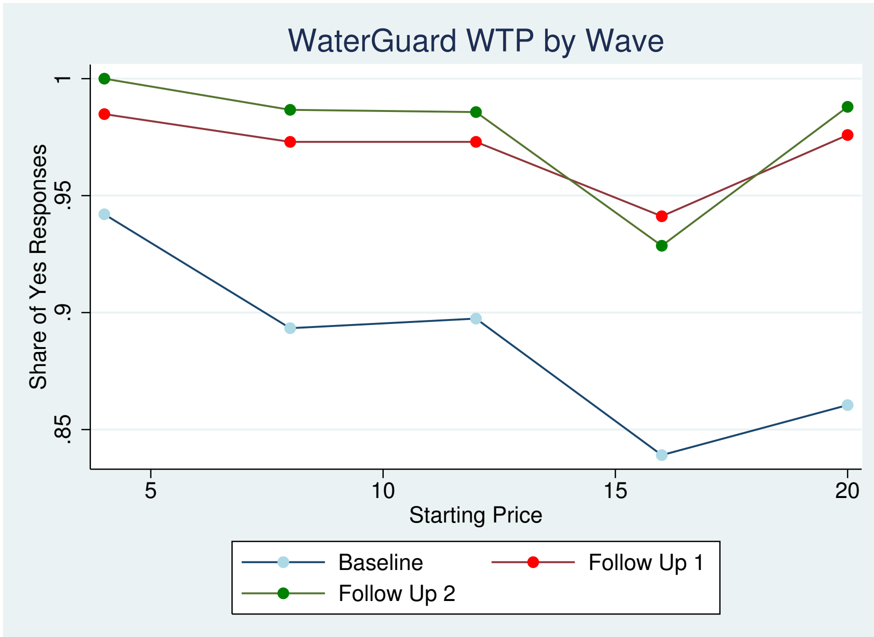
Towards disentangling these possibilities, Panel C of this Table also considers the final exit wave and tests hypothesis H_0^3 in equation 3.19 for the existence of recency effects. It shows that the most recent product a household has experienced significantly increases households' stated preferences for it, on the order of 40% (for the filter) to 86% (for WaterGuard). Although smaller than the initial primacy + recency effects affecting preferences after one product trial (Panel A), recency effects do not fully disappear despite all households having equal experience by the final exit wave (we reject H_0^3).

It is possible that households did not seriously consider the question about their most preferred product, and this hypothetical bias is inflating any estimate of recency effects. To test for this, we consider the parting gifts that households chose at the final exit wave, reflecting a revealed preference instead of their stated product preferences, when all households are "fully experienced" consumers. Although the correlation between households' final stated preferences and revealed preferences is quite high ($\rho = .59$), we allow for this possibility. Furthermore, as we discussed in Chapter 1, differences between stated and revealed product preferences at the final exit wave are also likely due to different market values of the three products as well as the second bucket accompanying the Pur and filter packages. Despite these differences, Figure 3.5 shows that recency effects do not disappear, even when choices are binding for households. If we compare the share of households choosing each product that experienced it last against those that experienced it earlier, we can reject the null hypothesis that they were chosen at the same rates for all three products (t-test p-values are .053 for WaterGuard, .004 for Pur, and .050 for the filter). Moreover, recency effects appear to dominate primacy effects on final gift choices when comparing choices across a first or last assigned product, although these differences are statistically significant only for Pur (p=.012 on two-sided t-test of primacy versus recency effects).

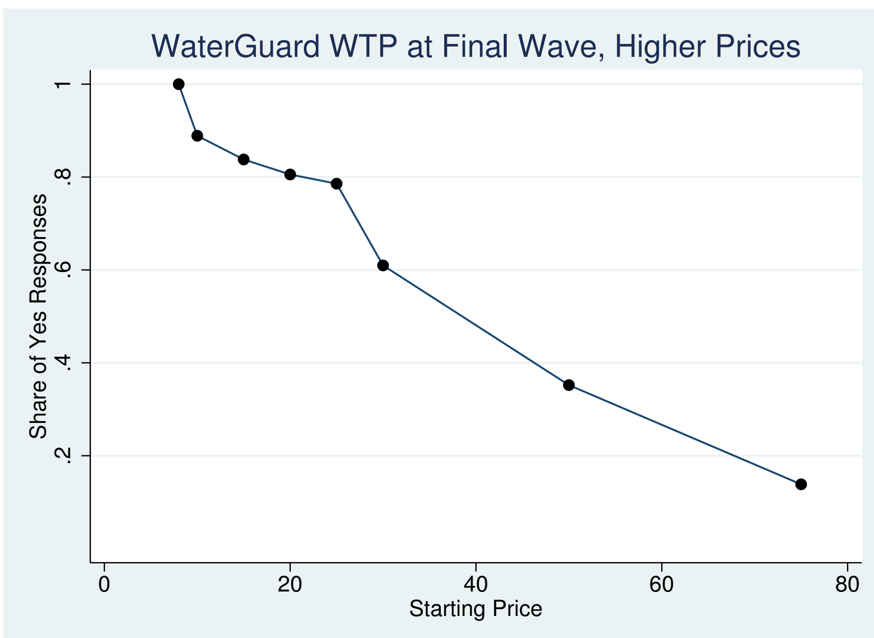
The finding of large and persistent recency effects on consumer choices of which goods they prefer (in a relative sense) does not support the rational model's assumptions of stable and innate preferences. There is really no way to explain this finding other than that these households are making decisions using a heuristic of choosing the most recently experienced product. It is difficult to say whether such a finding can affect the absolute decision of whether or not to adopt POU products more generally, since these findings are in relative terms between competing POU products. If these findings do extend to the overall adoption decision, it is easy to imagine that this heuristic could influence safe water behaviors negatively; if households use this heuristic to choose the same water treatment method today that they chose yesterday, and yesterday they failed to treat, they are likely to fail to treat today. Of course, this is a lofty extrapolation based on households' relative preferences; nonetheless, it is a troubling thought. On the other hand, if recency effects really do affect households' water treatment decisions more generally, such findings potentially could be harnessed in favor of POU water treatment with free samples and some incentive to begin use today.

Figure 3.1: WaterGuard Stated Demand Curves

Panel A:



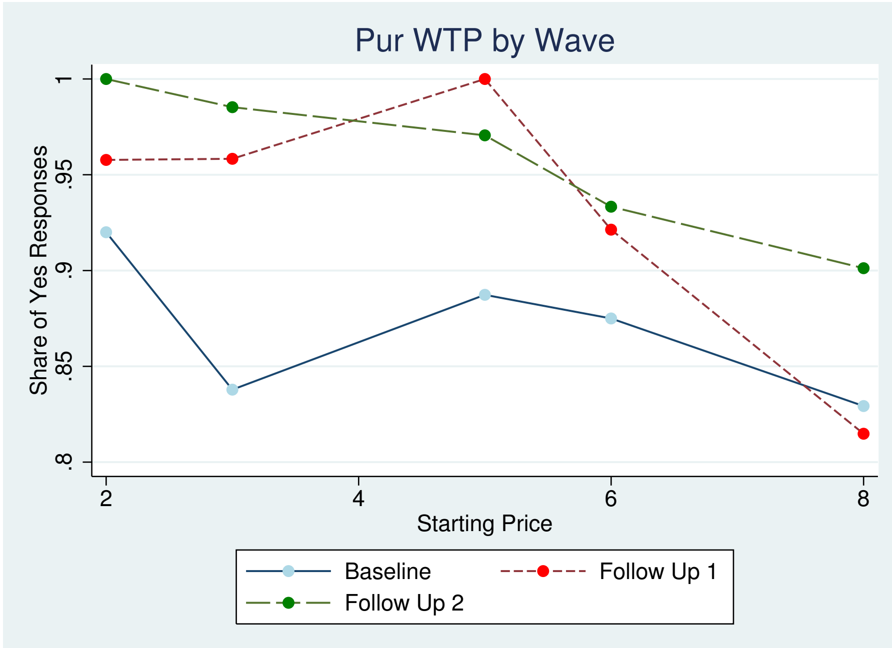
Panel B:



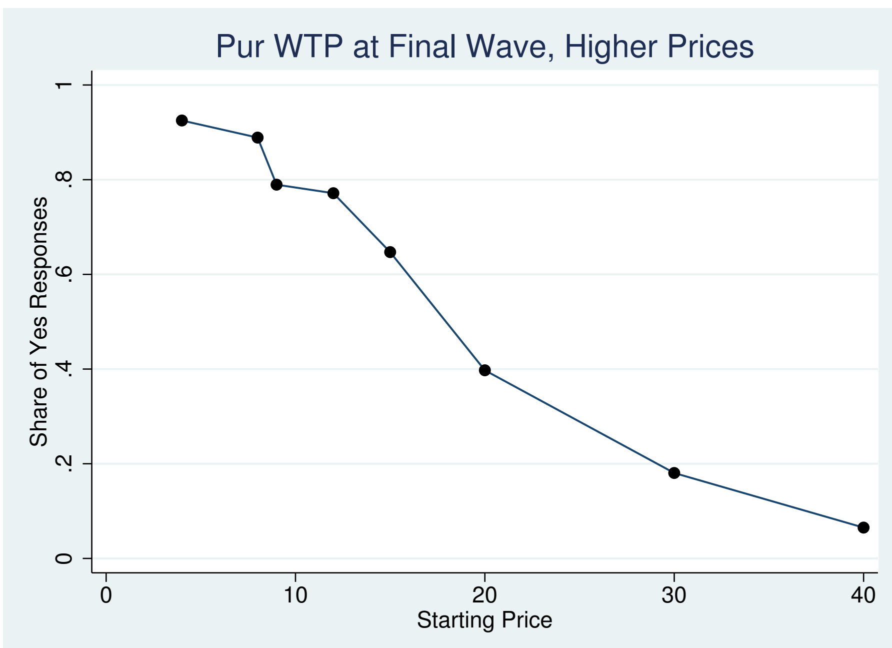
Panel A contains the share of households that respond "Yes" to initial bid prices for WaterGuard at baseline and first two follow-up surveys. Panel B contains similar data at final survey wave when initial bid prices were raised. All results are in terms of hypothetical WTP. Note vertical scales are different in Panels A and B. Market price of WaterGuard is 20 Kenyan shillings (~0.27 in July 2009) for one bottle, about a month supply.

Figure 3.2: Pur Stated Demand Curves

Panel A:



Panel B:



Panel A contains the share of households that respond "Yes" to initial bid prices for Pur at baseline and first two follow-up surveys. Panel B contains similar data at the final survey wave when initial bid prices were raised. All results are in terms of hypothetical WTP. Note vertical scales are different in Panels A and B. Market price of Pur is 7-10 Kenyan shillings (~$0.09-0.13 in July 2009) for one sachet, about a two-day supply.

Figure 3.3: Filter Stated Demand Curve and Experience

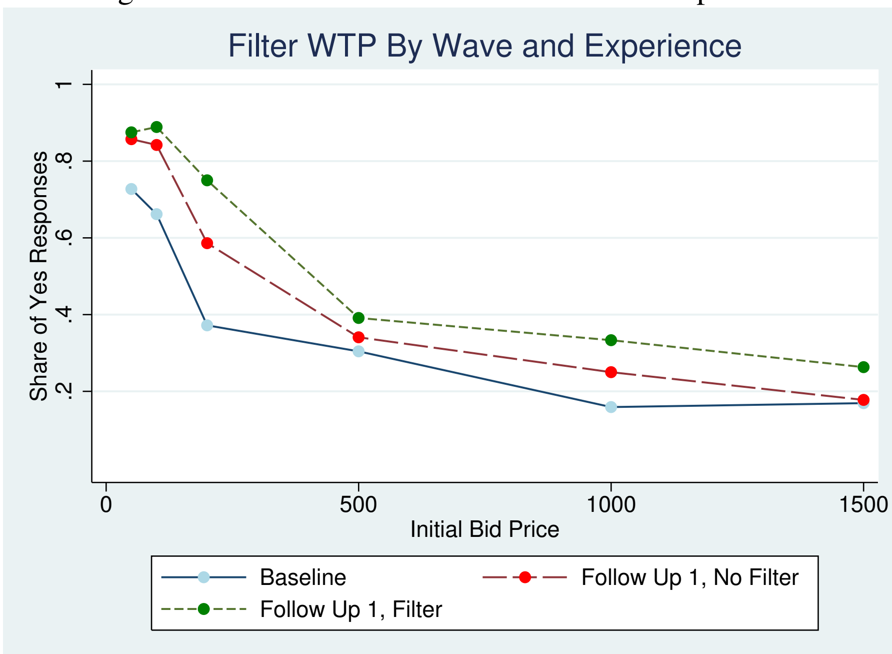
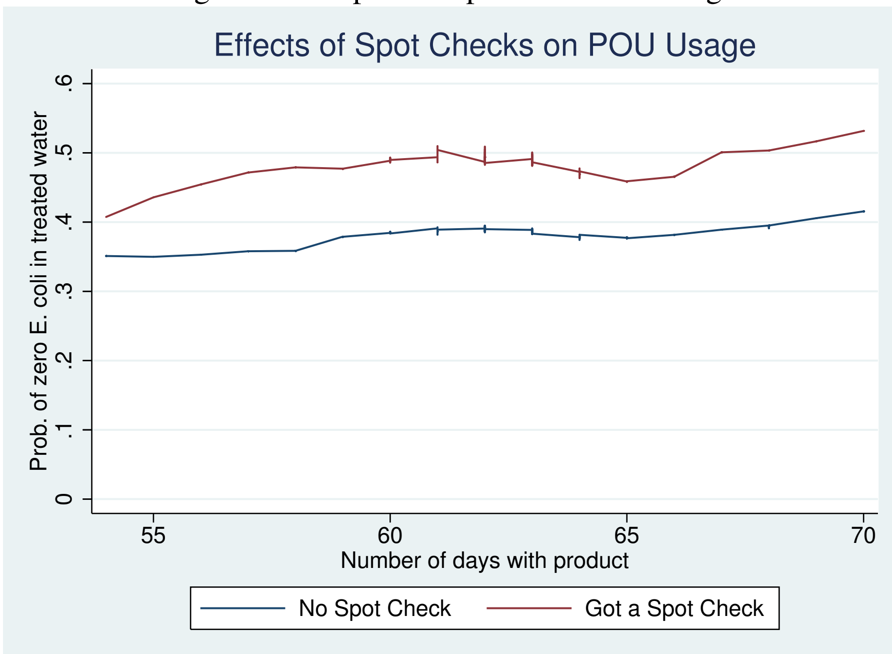


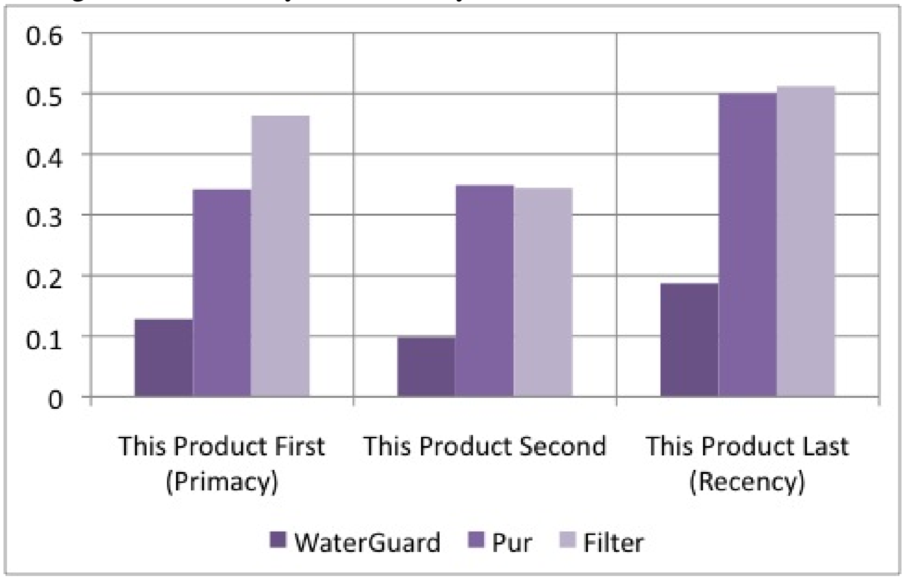
Figure plots share of households responding “yes” to a given initial bid price at baseline and first follow-up survey wave. At first follow-up, households are divided between those households that had just experienced the filter, and those that had first experience with WaterGuard or Pur.

Figure 3.4: Impact of Spot Checks on Usage



Nonparametric localized (lowess) regression of the share of households with no detectable E. coli in treated water on number of days with product, separately for those households that received a spot check during that product trial versus those that did not. Bandwidth=1. There are more data around 60-65 days.

Figure 3.5: Primacy and Recency Effects on Final Gift Choices



Final wave only (N=370). Plot of the share of households choosing each product as final parting gift, as a function of whether that product was experienced during the first, second or last product trial. Differences across first versus last experience are statistically significant for Pur (p-value on two-sided t-test = 0.012), but not the other two products. Differences across last versus earlier (first or second) product are statistically significant for all three products (p-value $\leq .05$).

Table 3.1: Filter Willngness to Pay

	(1) First 2 Waves	(2) All Waves
model		
Log of initial filter bid price	0.200	0.110
	(0.09)**	(0.07)*
Just experienced filter dummy	0.314	
	(0.18)*	
Negative income change dummy	0.371	-0.00863
	(0.42)	(0.25)
Positive income change dummy	-0.0436	0.0270
	(0.18)	(0.11)
Experienced filter previously dummy		0.246
		(0.14)*
Source information dummy		0.315
		(0.15)**
Own information dummy		-0.160
		(0.13)
Contrast frame dummy		-0.162
		(0.11)
Commitment dummy		0.170
		(0.12)
Toilet dummy		0.188
		(0.13)
Lost child indicator		0.388
		(0.13)***
Household size		0.125
		(0.07)*
Square of household size		-0.0114
		(0.00)***
Impatient dummy		-0.250
		(0.14)*
Liquidity constrained HH dummy		-0.655
		(0.13)***
Recent diarrhea dummy		-0.0235
		(0.12)
# of ways HH knows to prevent diarrhea at baseline		0.191
		(0.05)***
Observations	782	1527
Chi-squared	57.84	255.9

Standard errors in parentheses

* p<.10, ** p<.05, *** p<.01

Results from interval regression. Standard errors in parentheses clustered at village. Both columns include survey wave fixed effects. Column 1 contains results from estimation of equation 3.14 for the first two waves only. Column 2 contains results of equation 3.15 for all survey waves.

Table 3.2: WaterGuard and Pur Willingness to Pay, Final Wave Results

	(1) WaterGuard	(2) Pur
model		
Log of initial WaterGuard bid price	-0.374	
	(0.12)***	
Just experienced WaterGuard dummy	0.156	
	(0.15)	
Contrast frame dummy	0.103	-0.0881
	(0.14)	(0.12)
Commitment dummy	0.0943	0.0384
	(0.12)	(0.12)
Toilet dummy	-0.162	0.300
	(0.13)	(0.14)**
Lost child indicator	0.174	-0.0251
	(0.17)	(0.11)
Household size	0.0904	0.0772
	(0.08)	(0.09)
Square of household size	-0.00370	-0.00416
	(0.01)	(0.01)
Impatient dummy	0.0723	0.00824
	(0.15)	(0.12)
Liquidity constrained HH dummy	-0.282	-0.297
	(0.13)**	(0.10)***
Recent diarrhea dummy	-0.0842	0.361
	(0.16)	(0.18)**
# of ways HH knows to prevent diarrhea at BL	0.112	0.0698
	(0.05)**	(0.06)
Log of initial Pur bid price		-0.295
		(0.11)***
Just experienced Pur dummy		0.0823
		(0.13)
Observations	370	370
Chi-squared	62.77	34.48

Standard errors in parentheses

* p<.10, ** p<.05, *** p<.01

Results from interval regression. Standard errors in parentheses clustered at village. Both columns include only the final wave of data when initial bid prices were raised. Column 1 contains results for WaterGuard. Column 2 contains results for Pur.

Table 3.3: Impact of Spot Checks on Usage

	(1) Pos Cl or Enum. Obs.	(2) Pos Cl or Enum. Obs.	(3) T=0	(4) T=0	(5) Self-Report	(6) Self-Report	(7) U=0
1=received spot check prior	0.080	0.117	0.084	0.114	0.045	0.080	-0.016
to this round	(0.024)***	(0.032)***	(0.029)***	(0.034)***	(0.026)*	(0.034)**	(0.024)
Mean Dep. Var	0.565	0.561	0.128	0.129	0.718	0.716	0.128
	(0.023)***	(0.020)***	(0.023)***	(0.021)***	(0.025)***	(0.021)***	(0.023)***
Product FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	No	Yes	No	Yes	No	Yes	No
Observations	1133	1133	1533	1533	1133	1133	1533
No. Clusters	28	28	28	28	28	28	28

*p<.10, **p<.05 ***p<.01

Results from estimation of equation 3.16. Robust standard errors in parentheses clustered at village. Spot check observations of usage excluded from all columns. Columns 1 and 2 define usage as either a positive chlorine test (for WaterGuard and Pur) or enumerator observed filter usage and exclude baseline observations. Columns 3 and 4 define usage as treated water having no detectable E. coli in treated water and include baseline observations. Columns 5 and 6 define usage as self reporting treatment: current water is treated, treatment was in the past week, and household reports use of POU product each time water is collected. Baseline observations excluded from these columns. Column 7 considers the share of households with no detectable E. coli in their *untreated* water, and includes baseline observations. Base measures of usage are from the baseline round when possible; otherwise they are averages across all three products from the first follow-up survey round.

Table 3.4: Primacy and Recency Effects on Stated Product Preferences

Share Preferring	(1) Water-Guard	(2) Pur	(3) Filter
Panel A: Follow Up 1			
WaterGuard	**.469**	.085	.431
Pur	.133	**.391**	.477
Filter	.117	.047	**.836**
Total	.244	.171	.580
Primacy + Recency Effects:	.344***	.325***	.382***
% Change	+275%	+492%	+84%
Panel B: Follow Up 3 First Product Assigned			
WaterGuard	**.200**	.376	.424
Pur	.258	**.300**	.442
Filter	.168	.376	**.456**
Primacy Effect:	-.013	-.076	.023
% Change	-6%	-20%	+5%
Panel C: Follow Up 3 Last Product Assigned			
WaterGuard	**.301**	.268	.431
Pur	.194	**.468**	.339
Filter	.130	.317	**.553**
Recency Effect:	.139***	.176***	.168***
% Change	+86%	+60%	+40%

*p<.10, **p<.05 ***p<.01

Significance stars based on F-tests. Baseline and Follow Up 2 survey waves excluded. Panel A includes share preferring each product by first product assigned after first product trial. "Primacy + Recency" in this panel refers to combined effects of first product experienced on resulting preference for that product. Specifically, the percentage point increase in share preferring a product if that product was first (and also most recently) experienced versus not yet experienced. Panel B includes share preferring each product by first product assigned after final product trial when all households have experienced all products. Primacy in this panel refers to percentage point change from experiencing a product first versus later. Panel C presents share preferring each product by last (most recent) product assigned after final product trial when all households have experienced all products. Recency effect calculates percentage point increase in share preferring if last product experienced versus earlier. Stated product preference refers to household responses to survey question, "Which product do you like the best?"

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Appendix

Appendix A.1: Water Testing Procedures

We tested source waters, stored untreated water, and stored treated water for turbidity, *E. coli*, and free chlorine residual (in treated water samples in which either PUR or Waterguard were 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).

Appendix A.2: Willingness to Pay Script

The willingness to pay section of our survey came immediately after enumerators gave an educational script about the dangers of diarrhea and the importance of safe drinking water and then introduced and described the remaining new products to a household (those a household was yet to experience). After these product introductions, WTP questions were asked about all products to all households. Below is an English translation of the contingent valuation scenario described to households before asking their willingness to pay for the three technologies, as well as a sample of the WTP series of questions for one of the products (WaterGuard). In practice, the order of products asked about was randomized across households, as were the bid prices offered. These randomizations were printed directly into households' personalized surveys, and surveys were preassigned to households. To minimize the chances of courtesy bias inflating the share of "yes" responses, we allowed for any "yes, but ..." response to be coded as a "no." Moreover, any household that failed to respond they were "definitely sure" about their WTP response was coded as a "no." (In practice we still received a troublingly high acceptance rate for WaterGuard and Pur through the first three waves.)

ENUMERATOR SAY: Now I want to ask you whether your household would buy any of the safe water products I've described to you if they were going to be sold in your village. There is no right or wrong answer. We really want to know what you think. Some people say that they would not pay for a safe water product because they have more important things to spend their money on, or because they believe they can prevent diarrhea and other waterborne illnesses in other ways. For example, they may boil their drinking water. Other people say they would not buy a safe water product because they believe the diarrhea situation here is not so bad. However, if you purchase and use a safe water product correctly your family can be much more likely to avoid the pain and suffering of diarrhea.

You should keep these factors in mind when answering the following questions. Please answer as truthfully as possible.

I want you to imagine you are walking into a store with your household's money and must decide what to buy. You have many choices for how to spend your money at the store. For example, you can buy sugar, cooking fat, meat, soap, or tea. If you decide to buy one of these safe water products, it means that money cannot be use to buy other items in the store. Please think very hard about if your household would buy any of the safe water products I've described. There is no right or wrong answer.

PRODUCT 1:WATERGUARD

Q501. [ENUMERATOR HOLD UP WATERGUARD PRODUCT + PHOTOS:] Is your household willing to buy a bottle of WaterGuard, where one bottle is enough to make your household's drinking water safe to drink for about one month, here and now at a price of 12 Ksh? |_____|

1 YES
2 YES, BUT IF I CAN PAY IN INSTALLMENTS OVER TIME →SKIP TO Q503
3 YES, BUT I CANNOT PAY TODAY(PAY LATER) → SKIP TO Q503
4 NO→SKIP TO Q503
-99 DON'T KNOW→ SKIP TO Q503

<u>[IF "YES" (CODE 1) TO Q501:]</u>

Q502. Are you willing to pay 20 Ksh for one bottle? |_____| (SKIP TO Q504 AFTER ANY RESPONSE)

1 YES→ SKIP TO Q504
2 YES, BUT IF I CAN PAY IN INSTALLMENTS OVER TIME→ SKIP TO Q504
3 YES, BUT I CANNOT PAY TODAY (PAY LATER)→ SKIP TO Q504
4 NO → SKIP TO Q504
-99 DON'T KNOW→ SKIP TO Q504

<u>[IF ANY FORM OF "NO" (CODES 2, 3, 4, -99) TO Q501:]</u>

Q503. Are you willing to pay 6 Ksh for one bottle? |_____|

1 YES
2 YES, BUT IF I CAN PAY IN INSTALLMENTS OVER TIME
3 YES, BUT I CANNOT PAY TODAY
4 NO
-99 DON'T KNOW

Q504: Are you "definitely sure" or "probably sure" that you would (not) buy WaterGuard here and now at the price I have named? (REMIND RESPONDENT OF LAST PRICE NAMED.) |_____|

1 Definitely sure
2 Probably sure
-98 REFUSE TO ANSWER
-99 DON'T KNOW

Table A.1: Table of Unbalanced Baseline Means Across Treatments

	WaterGuard	Pur	Filter	p-value
Product Imbalances:				**F-stat**
% Respondents ever used WaterGuard	0.515	0.376	0.459	0.072
	Contrast	**Positive**		**p-value**
Frame Imbalances:		**Only**		**t-test**
# of children < 5	1.815	2.025		0.021
# of children < 1	0.327	0.415		0.086
% HHs w/ child < 5 recent diarrhea	0.380	0.465		0.086
	Control	**Treat**		**p-value**
Commitment Imbalances:				**t-test**
% Respondents w/ only 1 spouse	0.675	0.755		0.077
% HHs w/ child < 5 recent diarrhea	0.465	0.380		0.086
	Zero	**Source**	**Source**	**p-value**
Baseline Information Imbalances:			**+ Own**	**F-stat**
% HHs w/ zero *E. coli*	0.059	0.040	0.269	0.000
% HHs w/ *E. coli* MPN < 10 CFU/100 mL	0.277	0.236	0.581	0.000
% Respondents w/ 2ndary education or above	0.128	0.271	0.172	0.083
Household size	6.221	5.952	5.522	0.096
% HHs w/ permanent roof	0.499	0.691	0.688	0.045
% HHs w/ latrine	0.695	0.572	0.792	0.010
% Respondents talk w/ neighbors about water	0.289	0.402	0.264	0.084
% Respondents report losing a child	0.426	0.352	0.231	0.008
% Respondents own a phone	0.481	0.687	0.610	0.008

**All water quality variables balance across information categories at Follow Up 1*

List of those baseline descriptive variables (of a total of 55 considered) that do not balance (p-value $\leq .1$ on two-sided t-test or F-test) across randomized treatments of first product assigned, framing marketing treatment received, commitment marketing treatment, and initial information treatment. Means of variables for information treatments are at village level since assignment to information treatment category was at this level.