Can Higher Prices Stimulate Product Use?

Evidence from a Field Experiment in Zambia

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October 30, 2006

Abstract

The pricing of health products in the developing world has become a center of controversy among policymakers, with important implications for the efficient targeting of social programs more generally. A key issue in this debate is whether higher purchase prices lead to more intensive product use and, therefore, greater health benefits. We present results from an experiment in Lusaka, Zambia, designed to test whether charging more for a home water purification solution results in more use of the product. Our methodology separates the *screening* effect of prices (charging more changes the mix of buyers) from the *causal* effect of prices (charging more stimulates greater use for a given buyer). We find that higher prices screen out less intensive users of the product. High prices do not cause greater product use than low prices for a given buyer, but there is some evidence that the act of paying increases use. Our estimates imply that positive prices may be optimal even if maximizing use is the sole objective.

JEL classification: C93, D12, L11, L31

Keywords: chlorination, water-borne diseases, sunk cost effect, non-profit strategy, social marketing

*We are grateful to Gary Becker, Erik Eyster, Matthew Gentzkow, Emir Kamenica, Larry Katz, Michael Kremer, Steve Levitt, John List, Kevin M. Murphy, Sharon Oster, Andrei Shleifer, Richard Thaler, and seminar participants at the Harvard Business School and the University of Chicago for helpful comments and Rob Quick at the Centers for Disease Control for his guidance on the technical aspects of water testing and treatment. We wish to thank Steve Chapman, Research Director of Population Services International D.C., for his support, and the Society for Family Health in Zambia for coordinating the fieldwork, particularly Richard Harrison and T. Kusanthan, as well as Cynde Robinson, Esnea Mlewa, Muza Mupotola, Nicholas Shiliya, Brian McKenna, and Sheena Carey de Beauvoir. We gratefully acknowledge financial support from the Division of Faculty Research and Development at Harvard Business School. E-mail: nashraf@hbs.edu, jimberry@mit.edu, jmshapir@uchicago.edu.

1 Introduction

Economic approaches to the targeting of social programs emphasize schemes that make participation attractive to those with genuine need, and costly to others, so that individual choices endogenously generate an efficient allocation of social goods and services (Diamond and Mirrlees, 1978). Policy tools for achieving such favorable selection include eligibility criteria (Diamond and Mirrlees, 1978; Parsons, 1991), in-kind transfers (Nichols and Zeckhauser, 1982; Dye and Antle, 1986; Blackorby and Donaldson, 1988), and explicit pricing of public services (Vickrey, 1963; Oster, Gray, and Weinberg, 2003). In all of these cases, the optimal policy balances the value of the program to deserving individuals against the waste associated with distribution to those not genuinely in need.

This tension has become central to a controversy over the practice of charging money for the purchase of life-saving health products in the developing world. Critics of pricing argue that "charging people for basic health care...[is] unfair" (Benn, 2006), and that fees ensure that goods only reach "the richest of the poor" (McNeil, 2005). Advocates of pricing counter that "when products are given away free, the recipient often does not value them or even use them" (PSI, 2006).

Indeed, it is possible that charging more for a product will lead to greater or more intensive use. Such a pattern could arise for two reasons. First, consistent with the economic theory of program targeting, higher prices could screen out those who do not plan to use the product intensively, resulting in greater use per buyer (Oster, 1995). Second, following psychological evidence on the sunk cost effect (Thaler, 1980), paying more for something may actually encourage subsequent use, which could, in principle, increase total use in the population. Since use is an essential prerequisite to reaping the benefits of most health-promoting products, these hypotheses have important implications for health policy, and for understanding the role of pricing in the nonprofit and public sectors more generally.

Despite the importance of pricing strategy in these contexts, there has been surprisingly little systematic evidence on the role of prices in encouraging or discouraging product use. In this paper, we report the results of a field experiment in Lusaka, Zambia designed to test for the effect of pricing on the use of Clorin, an inexpensive, socially marketed disinfectant. Clorin is used to kill pathogens in household drinking water, and thus reduce the incidence of water-borne illnesses that can be especially dangerous to young children (Quick et al, 2002). We conducted door-to-door sales of Clorin, offering one bottle for a one-time-only, randomly chosen *offer price* to $1,004$ households. Households that agreed to purchase at the offer price received a randomly chosen discount, thus allowing us to vary the *transaction price* separately from the offer price. After the marketing intervention, we conducted a follow-up survey, in which we asked about Clorin use, and measured the chemical presence of Clorin in the household's stored water.

This two-stage pricing design allows us to separately estimate the different effects of prices on the intensity of product use. By varying the offer price for a given transaction price, we can estimate the effect of changes in the offer price on the mix of buyers for the product, or what we will call the *screening* effect of prices. By varying the transaction price for a given offer price, we are able to identify the effect of changes in the transaction price on a given household's propensity to use the product, or what we will call the *causal* effect of prices. Because we included in our randomization design a significant number of households that did not pay anything, our data allow us to further separate this causal effect into an effect of the amount paid, and an effect of the act of paying.

We find strong evidence that higher prices screen out less intensive users of the product. For a given transaction price, increasing the offer price by 10 percent results in a 3.6 percent increase in reported use among buyers in a follow-up survey. Similar patterns are present when we measure use by testing chemically for the presence of the product in the household's drinking water. These screening effects are present even when we condition on baseline demographics and Clorin use, indicating that the ability to target delivery based on observables would not eliminate the usefulness of prices as a screening tool. Moreover, raising the offer price does not increase the wealth of the average buyer, indicating that, in this context, higher prices do not deliver the product only to the "richest of the poor." However, we also find that higher prices do not help target delivery of Clorin to families with small children, who derive the greatest health benefits from clean water.

Turning to the causal effect of prices, we find no evidence that paying more for Clorin results in greater use of the product. This finding contrasts with previous evidence on the sunk cost effect, and arises despite the fact that the households in our sample display sunk cost effects in hypothetical choices. Our estimates are precise enough to rule out interesting magnitudes, indicating that this finding does not merely result from a lack of statistical power.

We do, however, find some evidence that the act of paying itself induces households to use Clorin more. While this effect is not robust statistically, it is stronger among households that report that they value something more if they pay for it, and among households displaying the sunk-cost effect in hypothetical choices.[1] Our findings are therefore consistent with a psychological link between household's use of a product and the act of paying, but show no evidence of such a link with the amount paid.

Evidence from a second, longer-term follow-up survey confirms that charging a positive transaction price may affect product use, although greater survey attrition than in the initial follow-up (and some evidence of selective attrition) means that this finding must be taken with caution. We find no evidence of screening effects in the longer-term follow-up data, which may imply that prices are especially effective at identifying households with an immediate need for the product.

We combine our estimates of the screening and causal effects of prices into a simple model that predicts product use as a function of price. The model indicates that an organization interested in maximizing product use would charge a positive price, even if revenues were not a concern. Because we find evidence of a causal effect of the act of paying, but not of the amount paid, the use-maximizing price is small. However, the model implies that higher prices (close to the prevailing retail price of Clorin) could be charged with little or no reduction in use relative to free distribution. These model-based implications suggest that understanding the screening and causal effects of prices is a critical input to resolving public policy debates over the appropriateness of user fees for access to social products and services.

Methodologically, our paper is most closely related to Karlan and Zinman's (2006) experimental study, which uses a two-stage pricing design to estimate the importance of adverse selection and moral hazard in loan demand and default in South Africa.[2] We use the two-stage pricing approach to answer a different set of policy questions, in a product market (rather than financial market) context. Moreover, because our discount offers were for one time only, they did not affect the marginal incentives of the households with respect to product use, so there is no analogue of the

[1] The effect is also stronger for married women than single women, which may suggest some role for an agency-based explanation (Prendergast and Stole, 1996; Ashraf, 2005).

[2] More broadly, our approach relates to recent research investigating the effectiveness of alternative marketing approaches in developing countries (Bertrand et al, 2005), and to field experiments more generally (Harrison and List, 2004).

moral hazard effect in our context. Arkes and Blumer's (1985) study of the sunk cost effect in the use of theater tickets is also related methodologically, but does not investigate the screening effects of prices, does not examine the effects of a zero transaction price, and deals with a market context fairly different from the distribution of health products in developing countries.

Topically, our paper relates to the economics of pricing in non-profit industries in general (Newhouse, 1970; Casper, 1979; Oster, 1995; Steinberg and Weisbrod, 1998; Oster, Gray, and Weinberg, 2003), and in social marketing organizations in particular (Kotler and Roberto, 1989; Behrman, 1989). Because many individual health outcomes depend on household behavior as well as on inputs from the health care sector (Grossman, 1972), our findings may have important implications for the role of prices even in areas with significant health externalities (Kremer and Miguel, forthcoming). Though there have been some studies of the effectiveness of prices in encouraging product use in social marketing contexts, existing research takes a largely non-experimental approach (Meekers, 1997).[3]

Our study also relates to the literature on the psychology of product pricing (Gourville and Soman, 2002; Shiv, Carmon and Ariely, 2005), and especially to research on the sunk cost effect. With the exception of Arkes and Blumer's (1985) study, all existing evidence on the sunk cost effect comes from hypothetical choices or laboratory experiments (Eyster, 2002; Friedman et al, 2004). Ours is only the second field experiment on the sunk cost effect that we are aware of, and it is the first to include a treatment in which participants paid nothing for the product.[4] Our finding that use does not vary with the amount paid is inconsistent with previous evidence on sunk-cost effects. However, our finding that the act of paying may itself impact use accords with the basic premise that sunk costs affect behavior. This contrast may suggest a need to revisit the psychology of sunk costs, with an emphasis on the distinction between the amount paid, and the act of paying.[5]

Lastly, the water disinfectant product that we use in our study is of intrinsic interest in light of

[3] An exception is Litvack and Bodart (1993), who study a natural experiment in which public health facilities in Cameroon adopted both user fees and improved quality of care. Because of the simultaneous adoption of these two policies, Litvack and Bodart's (1993) research design does not permit separate identification of the effect of fees on utilization.

[4] A number of existing papers explore the special role of zero prices, but none focuses on the effects on post-purchase use. See, for example, Ariely and Shampan'er (2004), Thornton (2006), and Karlan and List (2006).

[5] By providing the first experiment linking market behaviors to hypothetical-choice elicitations of sunk-cost effects, our study also contributes to a growing literature connecting laboratory and survey responses to incentivized choices in markets (Goette, Huffman, and Fehr, 2003; Karlan, 2005; Ashraf, Karlan, and Yin, 2006).

growing concerns about access to clean water among the world's poor. Almost 1.2 billion people worldwide lack access to clean water, and water-borne diseases kill some 3.1 million people each year (USAID, 2006). Because they do not require the construction of new infrastructure or pipelines, household products like Clorin are a promising tool for addressing this problem. Our research contributes to ongoing research on the adoption and usage of Clorin and similar products (Thevos et al, 2002; Kremer et al, 2006) by showing that pricing can form part of an effective strategy for encouraging product use.

The remainder of the paper is organized as follows. Section 2 describes the design of our surveys and door-to-door marketing experiment. Section 3 presents our findings on the effect of price changes on product purchase and use. Section 4 presents our findings on the longer-term effects of pricing on use. Section 5 develops the implications of our estimates for optimal pricing policy. Section 6 concludes.

2 Experimental and Survey Design

Our study consisted of a baseline survey, a randomized door-to-door marketing intervention approximately two weeks later, and a follow-up survey approximately two weeks after the intervention. (We also conducted a second, longer-term follow-up survey, which we discuss in section 4 below.)

In the baseline survey, we asked questions about basic demographics, health attitudes, and water treatment. We also tested participants' water for the presence of chlorine. In the marketing intervention, we offered to sell a single bottle of Clorin to each household at a randomly chosen offer price. Households that agreed to buy were then given a randomly chosen discount, thus allowing us to vary the transaction price separately from the offer price. In the follow-up survey, we again asked questions about water treatment and tested household water supplies. At the end of the survey, we asked several questions relating to potential psychological mechanisms for a causal effect of prices.

2.1 Background on Clorin

Clorin is a water purification solution that is marketed in Zambia by the Society for Family Health (SFH), a local affiliate of PSI, an international non-profit organization.[6] Chemically, Clorin is sodium hypochlorite bleach, which can be mixed with water stored in the household in order to kill water-borne pathogens, and thus prevent the contraction of water-borne illnesses that are especially dangerous to young children. Because many households in Zambia obtain their water from sources that are not properly chlorinated, and because Clorin is less expensive than boiling water or other alternative methods of disinfection, it has been a popular product since its launch in 1998 (Olembo et al, 2004).

Clorin is marketed by the bottle (see figure 1), and a single bottle is sufficient to disinfect up to 1,000 liters of water (about one month's water supply for a family of six). Clorin is sold widely in both retail outlets (for about 800-1,000 Kw) and health clinics (for about 500 Kw).[7] These prices are modest by Zambian standards; for comparison, in Lusaka, a bottle of Coca-Cola costs about 2000 Kw.[8] The fact that Clorin is a relatively inexpensive product limits the possibility that wealth effects contaminated our study of the effects of offer and transaction prices on Clorin use.[9]

In addition to the inherent importance of clean water for health in the developing world, we chose to use Clorin in our study for two practical reasons. First, Clorin use can be measured not only by household self-reports, but by chemical tests for the presence of chlorine in stored drinking water. These tests are themselves imperfect, because households' source water (i.e., water from taps) sometimes contain chlorine, and the levels of chlorination in source water vary considerably across space and over time. Despite these drawbacks, the objectivity of chemical tests creates the possibility of cross-validating households' subjective reports, an option that is not available with many health products (e.g., condoms).

Second, because Clorin has been widely marketed for several years, most households are familiar with the product and with its prevailing retail price. In our baseline survey (described below), nearly 80 percent of respondents report having used Clorin at some point, with over 99 percent mentioning

[6] See <http://www.psi.org/resources/pubs/clorin.html> for additional information.

[7] The recommended retail price of Clorin is 800 Kw.

[8] As of June 1, 2006, 800 Kw was equivalent to about $0.25 US. Average monthly urban income in Zambia in 2002-2003 was 790,652 Kw (UNECA, 2006).

[9] See subsection 3.4 of the analysis for additional checks on the possibility of contamination from wealth effects.

Clorin when asked which water purification solutions they have heard of. Our experience further suggests high levels of awareness of Clorin prices. We also took steps (detailed below) in our experiment to ensure that the households in our study had uniform, accurate information about prices in the existing market for Clorin. These facts, combined with additional precautions we describe below, serve to minimize the information participants could have gleaned from the prices we charged in our experiment. While informational effects of prices are relevant for policy, limiting their role allows us to more cleanly test for the screening and causal effects that are the focus of our study.[10]

2.2 Baseline Survey

We fielded our baseline survey to 1,260 households in Lusaka, Zambia in May, 2006. To select households, we first selected five peri-urban areas ("compounds"). Because we wanted to sample a population whose water source had limited chlorination (so that use of Clorin was likely to make a significant difference in pathogen levels in the drinking water), we avoided compounds close to the main water line in Lusaka. We also avoided compounds where we knew that NGOs were (or had recently been) distributing Clorin for free from door to door.[11] Our interviews focused on female heads of household, because prior experience (later confirmed by our baseline data) suggested that they play a central role in decision-making about purchases of Clorin, and are typically the household members responsible for putting Clorin in the water.[12]

The households we sampled seem to be fairly representative of the Lusaka population, despite the requirements we imposed in selecting compounds. Table 1 compares average demographic characteristics of the households in our baseline sample to Lusaka residents sampled in the 2001

[10] In subsection 3.4 of the analysis, we show directly that participants' attitudes toward Clorin and beliefs about Clorin's market price did not change in response to the prices we charged.

[11] Within the five compounds we chose, we sampled 10 randomly chosen standard enumeration areas (SEAs) for surveying. Within each SEA, we sampled one out of every five households until the target of 252 households was reached for the compound.

[12] Our surveyors worked in teams of four, each with a supervisor who organized the "one out of five" sampling within the SEA. Each day, during the baseline survey, the teams traveled to the compounds on their schedule. At each household, the surveyor asked to speak with the female head of household, and if there was no one home or the female head was unavailable, the surveyor returned later that day to complete the survey. If the female head of household could not be reached on that day, the house was skipped. After identifying the female head of household, the surveyor read a consent form and asked for permission to conduct a 30 minute interview regarding her health choices.

Demographic and Health Survey (DHS) of Zambia.[13] Though the DHS questionnaire does not allow us to restrict to female heads of household, in other respects the samples should be fairly comparable. In column (1) of the table, we present means of a set of demographic characteristics available in both our data and the DHS. In column (2), we restrict attention to households in which the female head is between ages 15 and 49, since these are the ages present in the DHS sample. In column (3), we report comparable means for the DHS data. Comparing columns (2) and (3) suggests that our sample is reasonably similar to the DHS sample. Our respondents tend to be slightly older and more likely to be married than the DHS respondents, most likely due to our insistence on speaking to the female head of household. The households in our baseline sample also have slightly lower levels of durables ownership than those in the DHS data. On the whole, however, the demographics line up fairly closely, suggesting that our findings may have applicability beyond the compounds we sampled.

The survey interview was divided into several sections. First, we asked for a variety of basic demographic information, such as age, marital status, schooling levels, fertility history, household composition, and ownership of various durable goods (as a proxy for wealth or income). We then asked a range of questions about media exposure, malaria knowledge and behaviors related to malaria prevention. In addition to serving as possible proxies for general health knowledge and attitudes, these questions served to make the purpose of our study less transparent to the interviewee. Finally, we asked several sets of questions related to water use practices, diarrhea, soap use, attitudes toward and use of water purification techniques, access to water sources, and detailed questions on the use of Clorin.

In the last section of the interview, the surveyor tested the household's drinking water for the presence of chlorine. Most households store their water in a large plastic jug. The surveyor put a small amount of stored water into a Styrofoam cup, and inserted a chemical test strip into the cup. After exposure to water, areas of the test strip change color based on chlorine concentrations in the water. We used the Sensafe Waterworks 2 test strip,[14] which tests for both free chlorine radicals

[13] We are grateful to Emily Oster for providing tabulations of demographic characteristics from the DHS. See <http://www.measuredhs.com/> for further details on the survey.

[14] The Sensafe Waterworks 2 test strip is Industrial Test Systems part number 480655. See <http://www.sensafe.com/> for corporate information and <http://www.sensafe.com/480655.php> for additional information about the test strip.

(chlorine available to kill pathogens) as well as total chlorine (free chlorine plus chloramines, a by-product of chlorine combining with organic compounds).[15] The test strip identifies seven possible concentrations of both free and total chlorine: 0, 0.1, 0.2, 0.5, 1, 2.5, and 5 parts per million. Our own experimentation, as well as conversations with the manufacturer, suggested that the free chlorine measurement was more reliable and less sensitive to variation in test conditions (such as light and heat). Moreover, the presence of free chlorine radicals is the more relevant metric of the safety of drinking water, because it measures the presence of chemical sanitizer that is available to kill pathogens.[16] We will therefore focus our attention on the measurement of free chlorine.

Measured chlorination is highly related to self-reported use of Clorin. Among the 21 percent of households that report that their water is currently treated with Clorin, more than 60 percent have at least some free chlorine, whereas this figure is below 40 percent for the households that report that their water is not currently treated with Clorin.[17] A Pearson χ^2 test definitively rejects the equality of the two distributions ($p - value < 0.0001$). Indeed, levels of free chlorine of 2.5 and 5 parts per million are only found in households who report that their water is treated with Clorin. In order to limit sensitivity to these rare outliers, we will use a binary measure of the presence of free chlorine (free chlorine levels of 0.1 parts per million or greater) in our analysis. In the baseline survey, 41 percent of the households have at least 0.1 parts per million of free chlorine in their water.[18]

Although the strong correspondence between self-reported water treatment and measured chlorination suggests that both measures capture meaningful variation in use of Clorin, neither measure is likely to be perfect. Self-reported water treatment may be subject to various recall biases, some of which could be affected by our experimental treatment. Measured chlorination avoids these pitfalls, but is affected by variation in ambient chlorination in water taps, and perhaps by variation in testing conditions and surveyor care. We will therefore use both self-reported and chemical mea-

[15] See chapters 13 and 14 of Hauser (2002) for more information on chlorine chemistry and chlorine testing.

[16] U.S. drinking water guidelines typically call for a minimum free chlorine residual of 0.2 parts per million and a *maximum* total chlorine concentration of 4 parts per million. (See <http://www.epa.gov/safewater/mcl.html>, <http://www.nps.gov/public_health/inter/faqs/faq_dw.htm#3>.) Note, however, that smaller amounts of free chlorine residual still afford some protection against contamination by pathogens.

[17] It is not surprising that a large fraction of households who report not using Clorin have some chlorine in their water. Many household water sources (i.e., taps) have some chlorination, with levels that vary across locations and over time.

[18] By contrast, fewer than 10 percent of sampled households have free chlorine concentrations of 0.2 parts per million or greater. Our cutoff therefore captures the richest degree of variation in the dependent measure.

sures in our analysis of the experimental results, with the goal of obtaining convergent evidence for the effects we report.

2.3 Door-to-Door Marketing Experiment

For our marketing experiment, we sent a team of six marketers out in May and June of 2006 to the 1,260 households from the baseline survey.[19] The marketing was designed to occur about two weeks after the household was surveyed for the baseline, but actual lag times varied due to variation in logistical factors such as the difficulty of contacting the original survey respondents.

The marketers were not the same people as the surveyors, and did not mention any connection with the surveyors. Specifically, whereas surveyors introduced themselves as carrying out a health survey for a researcher at Harvard University, the marketers introduced themselves as representatives of SFH, which produces, distributes, and markets Clorin throughout Zambia. This contrast serves two purposes. First, it provides greater confidence that behavior in response to the marketing intervention is not driven by the belief that the experimental participants are "being watched." Second, the SFH name carries significant credibility, which is useful in minimizing the risk that participants thought the discounted Clorin bottles must be somehow sub-par.

After making contact with the female head of household, the marketers followed a written script, which was personalized for the household. The marketer offered to sell a single bottle of Clorin for a one-time-only price. This initial offer price was chosen randomly, with 10 percent of households receiving an offer price of 800 Zambian Kwacha (Kw), and the remaining 90 percent split as evenly as possible among offer prices of 300, 400, 500, 600, and 700 Kw. (See table 2 for exact proportions.) The typical retail price of Clorin is between 800 and 1,000, so most households were receiving some price discount in addition to the added convenience of door-to-door purchasing. The marketing script for each household specified the initial offer price to be charged, allowing us to control the randomization directly, and ensuring that the marketers had no discretion in setting this price.

[19] The six marketers worked with one supervisor who coordinated the visits in the compound. They used hand-drawn maps prepared by the teams that conducted the baseline survey in order to find the households from the baseline. They also used information, collected in the baseline survey, about when the female head of household was most likely to be at home. If the marketers found a house but there was no one home, they returned at least three times on two different days to try to contact the original respondent. If someone was home but it was not the female head of household named in the baseline survey, they made an appointment to return when the female head would be home.

The marketing script explicitly told respondents that Clorin was available in retail outlets for around 800 Kw. Although early pilot interviews suggested that most people in Lusaka are well aware of these prices, we included this information in the script to guard against the possibility that households would infer something about the market price (or quality) of Clorin from the prices we offered in our experiment.

If the respondent agreed to buy at the initial offer price, the marketer informed her that she might be eligible for an additional discount.[20] The respondent was given a sealed envelope, which contained a coupon offering a one-time discount on the bottle of Clorin. Using a sealed envelope allowed us to control the amount of the discount, and to prevent the marketer from signaling the discount using body language or other cues. After the respondent opened the envelope, the respondent paid for the bottle of Clorin and the marketing session ended.[21]

When asked why they were offering Clorin at lower-than-normal prices, marketers explained that the price was part of a special promotion. They used the same explanation to account for the additional discount after the asking price was agreed upon. Door-to-door sales (and occasional giveaways) are not unheard of for products like Clorin, and participants seemed to accept this explanation. Indeed, after we explained that the initial offer price was a promotional price, participants rarely questioned the reason for the discounted transaction price.

Because we wanted variation in the transaction price (and not variation in the offer price) to identify any psychological effects of price on product use, we designed several features of the marketing procedure to make the transaction price salient. First, marketers were trained to offer the discount *before* the respondents went to retrieve the cash payment, so that the respondents would count out only the amount of money needed to pay the transaction price. Second, the coupon stated the final transaction price, rather than the amount of the discount (see figure 2). Finally, respondents were asked to write the final transaction price on a receipt and sign it.

[20] If the respondent agreed to buy at the initial offer price, but did not have the necessary cash on hand, the marketer offered to reschedule, and returned to complete the script at the arranged date and time.

[21] None of the respondents decided not to buy the bottle of Clorin after the discount was offered. In order to guarantee that the transaction took place at the specified price, the marketer asked each respondent to fill out a receipt, and included that along with the payment in the discount envelope marked with the household's unique survey identification number. This allowed us to check that the marketers had complied with the instructions. In four cases, the marketer transacted at a price other than the one we specified due to human error. In these cases, we will use the intended transaction price rather than the actual transaction price for the purposes of our analysis, to ensure that these errors do not contaminate our findings.

The size of the discount was chosen randomly, but every household received a discount of at least 100 Kw. We offered a discount to every household to avoid disappointing the respondents, and to ensure that every household was exposed to the coupon (in case of any advertising effects of the coupon itself). Because we hypothesized that paying even a small amount might be very different psychologically than paying nothing, we randomized the discounts so that, regardless of the offer price, 40 percent of households received a 100% discount, and thus had a transaction price of zero. For each offer price, we split the remaining 60 percent of households evenly among the set of transaction prices that were above zero but at least 100 Kw below the offer price. (See table 2 for details.) So, for example, among households that were offered Clorin for 700 Kw, 40 percent were assigned a transaction price of 0 (a discount of 700 Kw), and 10 percent were assigned to a transaction price of 100, 200, 300, 400, 500, and 600 Kw (discounts of 600, 500, 400, 300, 200, and 100 Kw, respectively).

We assigned the offer and transaction prices randomly prior to the marketing outings, so that every household was assigned an offer price and a transaction price, even if we were unable to reach the household during marketing. The randomization was fully stratified by compound, with every compound receiving (up to integer constraints) the exact same mix of offer and transaction prices. At the time of randomization we verified that observable characteristics were balanced across treatments, and, in a few cases, re-randomized when this was not the case.

Table 3 presents a test of the relationship between our treatment conditions and a range of household characteristics measured in the baseline survey. Column (1) shows a regression of the offer price (in units of 100 Kw) on measures of Clorin use and chlorination, health behaviors and health attitudes, and a range of household demographic characteristics. To separate the screening and causal effects of prices, our analysis of offer prices in section 3 will study the effect of variation in offer price that is not related to transaction price. To parallel this analysis, the specification in column (1) includes fixed effects for the transaction price. The regression shows no evidence of any contamination of the treatment: the regression coefficients are all individually insignificant, and an F-test of the null hypothesis that all coefficients are zero fails to reject at any conventional significance level.

The analysis in column (1) refers to the entire baseline sample (excluding households that refused to answer one or more demographic question). However, only 1,004 households, or 80 percent of the sample, were successfully reached during the marketing intervention, and we will not use data from the unreached households in our experimental analysis. Column (2) of table 3 therefore repeats the analysis of column (1), restricting attention to the sample of households reached by our door-to-door marketing. Again, the analysis shows good balance of observables with respect to the treatment. Baseline self-reported use is marginally statistically significantly related to the offer price, but none of the other covariates is significantly related to the treatment, and again the F-test fails to reject the null of no relationship between household characteristics and the offer price.

In column (3) of table 3, we conduct a parallel analysis of the covariates of the transaction price, conditional on the offer price. Because the transaction price was only relevant for those households who purchased Clorin, we restrict attention to that sample in this specification. There is a statistically significant relationship between the chemical presence of free chlorine in the baseline survey and the transaction price in this sample, but the relationship with self-reported use has the opposite sign, and no other covariate is statistically related to the treatment. The F-test of the restriction that all covariates enter with a coefficient of zero fails to reject the null. Overall, then, this table confirms the exogeneity of the treatment. However, because of the lack of balance with respect to pre-existing chlorination levels, we will pay special attention in our analysis to the effects of controls for baseline chlorination levels and Clorin use.

Some of our analysis will focus on the contrast between households that paid a positive transaction price for their bottle of Clorin and those who received the bottle for free. In column (4) of table 3, we use as the dependent variable an indicator for whether the household paid a positive price, again restricting attention to those who bought Clorin, and controlling for offer price. The statistically significant relationship with baseline chlorination levels is not present in this case, although both self-reported Clorin use and measured chlorination are positively (but not statistically significantly) related to the positive price condition. An indicator for the female head of household having attended school is marginally statistically significantly positively related to the positive price condition, but our measure of years of schooling is marginally significantly negatively related to the positive price condition, suggesting no systematic relationship to schooling levels. Overall, there is little evidence of a systematic bias, and the F-test again confirms that the set of covariates are jointly not significantly related to the treatment condition.

2.4 Follow-up Survey

For our follow-up survey, we sent the original survey teams (three teams of four, each with one supervisor) to find and re-interview the households that we successfully reached for the marketing intervention.[22] We re-interviewed households approximately two weeks after the marketing intervention, but actual lags varied due to logistical factors.[23]

The follow-up interview consisted of several sections. First, we repeated a handful of demographic questions from the baseline survey, as a check on the identity of the respondents.[24] Next, we asked a variety of questions about health knowledge and attitudes, and hygiene practices. We then asked a detailed set of questions about the household's use of Clorin, followed by questions about whether the household had been visited by marketers at any point in the past. This question served as an additional check on whether we had reached the correct household.[25] After concluding the questions on Clorin use, we tested the household's water, following the same procedure as in the baseline survey. Finally, once we had concluded measurement of Clorin use and chlorination, we asked several questions relating to the sunk cost effect and the idea that paying for something may lead one to value it more. We asked these questions at the end of the survey because we did not want households' answers to these questions to affect their responses about Clorin use.

We reached 890 households in the follow-up survey, representing about 89 percent of the $1,004$ households who were successfully reached during the marketing phase. Table 4 presents some

[22]Because they were not exposed to our marketing experiment, we did not attempt to interview the households that we did not reach during the door-to-door marketing.

[23]If the surveyors found a house but there was no one home, they returned at least three times to contact the original respondent. If someone was home but it was not the female head of household named in the baseline survey, they made an appointment to return when the female head would be home. In cases where it proved exceedingly difficult to reach the female head of household, the surveyor accepted another female adult household member as an interviewee, and noted this adjustment in the questionnaire.

[24]Among the 832 cases in which our records indicate that we successfully reinterviewed the original respondent, these demographic characteristics are strongly correlated between the baseline and follow-up surveys, with (highly statistically significant) correlation coefficients of 0.9 or more. (The demographic characteristics are inconsistent between the baseline and follow-up surveys in less than 9 percent of cases.) Our findings are not substantively different when we restrict attention to the cases in which we successfully reinterviewed the original respondent.

[25]We also asked our surveyors to identify the bottles of Clorin we had sold, which we had marked on the bottom with an "X." In nearly 80 percent of cases in which our records indicate that the household purchased Clorin, the surveyors were able to identify the marked bottle among the household's inventory of Clorin bottles.

evidence on the determinants of attrition. Because the experimental intervention did not begin until the marketing phase, attrition is more of a concern for the follow-up than for the marketing phase, but for completeness the table presents evidence on attrition at both stages.

Column (1) of table 4 shows estimates of a linear model of the probability that a household in the baseline survey was reached during the marketing phase. The two treatment conditions are statistically unrelated to attrition at this stage, which is unsurprising given that the experiment did not begin until the marketing stage. Some household characteristics do seem to play a role in attrition. For example, households that own a larger share of the set of durable goods (car, radio, television) that we asked about were statistically significantly more likely to be successfully reached during the door-to-door marketing. This most likely occurred because wealthier households tended to be in more developed sections of the compounds and were therefore easier to locate.[26] Households in the fifth locality we surveyed were also significantly more likely to be reached, probably because that compound had a more organized system of household addresses than the other compounds. An F-test indicates that the variables in the model are jointly statistically significantly related to the likelihood that the household was reached during the marketing phase, suggesting that attrition was not random.

In column (2) of table 4, we examine the relationship between contact in the follow-up survey and the initial offer price among households that were successfully reached for door-to-door marketing. To isolate variation in the offer price from variation in the transaction price, and to better parallel the analysis of the experimental findings, we include fixed effects for the transaction price in the model. As in column (1), we find no evidence that the offer price received by the household affects its likelihood of being reached in the follow-up survey, although we do find that demographic characteristics are somewhat predictive of attrition.

In column (3) of table 4, we conduct a parallel investigation of the relationship between attrition and the transaction price, controlling for the offer price. Here we restrict attention to households that purchased Clorin during the door-to-door marketing, because these are the households for whom the transaction price constitutes an experimental intervention. There is no statistical relationship between transaction price and the likelihood of contact in the follow-up. Moreover, in

[26] Wealthier households were also more likely to have address plates on their homes (rather than having their address written on the door or outside walls), which helped the survey team to locate the address.

this sample the F-test indicates that household characteristics are only marginally statistically significantly related to the probability of attrition.

Finally, in column (4) of table 4, we repeat the specification of column (3), replacing the continuous transaction price variable with an indicator for whether the transaction price was positive. As in column (3), we find that the probability of contact is unrelated to the treatment condition, and is only marginally statistically significantly related to the household's demographic characteristics.

The evidence in table 4 shows that, while the attrition process was not random, it does not appear to have been affected by the experimental intervention. This provides some reason to believe that our experimental results are not confounded by differential sample selection across treatment conditions.

3 Effects of Price on Purchase and Use of Clorin

We present the results of our experiment in three parts. First, we report our findings on the relationship between offer price and the probability of a household purchasing Clorin during the door-to-door marketing intervention. Next, we study the screening effect of offer prices on the composition of households that buy Clorin, with special emphasis on their propensity to use Clorin in their water. Finally, we examine the causal effect of transaction prices on a household's intensity of Clorin use.

The effect of an increase in the price of Clorin on use of the product depends on all three of these effects. A higher price reduces the number of households purchasing Clorin (the effect measured in subsection 3.1), which may depress Clorin use. However, this effect may be mitigated if some of the households excluded by the higher price would not use the product even if they bought it (the screening effect estimated in section 3.2). Finally, some households who continue to buy despite the higher price may use the product more intensively as a result of the price increase, say because of a sunk cost effect (the causal effect explored in section 3.3).

In section 5, we incorporate these three effects into a formal model of Clorin use, and develop implications for optimal pricing when the objective is maximizing use.

3.1 Effects of Price on Purchase of Clorin

In this subsection, we explore the effect of offer price on a household's propensity to purchase Clorin during the door-to-door marketing phase of our study. Though the finding that higher prices result in fewer purchases is not novel, it is an important "first stage" for the analysis of screening in subsection 3.2. In addition, as noted above, knowing how many potential purchasers are discouraged from buying by a price increase is a critical input to a complete understanding of how prices impact usage.

Figure 3 shows the effect of offer price on the propensity to buy Clorin during our door-to-door intervention. For each offer price, the figure reports the share of respondents purchasing Clorin at each offer price level, along with the number of observations in each cell.[27] The figure shows a clear, downward-sloping relationship between offer price and the share purchasing Clorin. The relationship is nearly monotonic, and suggests a strong price effect, with nearly 80 percent of respondents buying Clorin at 300 Kw, and only about 50 percent buying at 800 Kw.[28] The relationship is also highly statistically significant: a Pearson χ^2 test strongly rejects the null hypothesis of no relationship between offer price and purchase frequency ($p < 0.001$).

For a more parametric evaluation of the relationship between price and demand, we will consider the following simple model of demand for Clorin in our door-to-door marketing. Let WTP_i represent household i's willingness to pay for Clorin in our door-to-door marketing. It is natural to assume that $WTP_i \geq 0$ for all households i, i.e., that all households would have accepted the Clorin had we given it away for free. Moreover, because Clorin is available in retail stores, there is likely to be some upper bound v to households' willingness to pay, most likely equal to the highest prevailing retail price, possibly with some premium for the convenience of a door-to-door sale.

For simplicity, we will assume that WTP_i is distributed uniformly on the range $[0, v]$. Letting p_i be the offer price, we will suppose that a household purchases Clorin if and only if $WTP_i \geq p_i$; that is, if its willingness to pay exceeds the offer price. Letting b_i be an indicator for whether household

[27] Because we did not reach every baseline respondent during the marketing intervention, the distribution of observations differs somewhat from the randomization scheme in table 2. However, the distributions are quite close, with respondents allocated more or less evenly across prices from 300 to 700 Kw, and a smaller number at 800 Kw.

[28] The two price levels that violate monotonicity—500 Kw and 800 Kw—are, respectively, the health clinic price and current suggested retail price of Clorin. One explanation of the "spikes" in demand at these points is that these are salient prices that have special resonance with consumers (Anderson and Simester, 2003).

i purchased Clorin, we can then write

$$\Pr(b_i = 1) = \Pr(WTP_i \geq p_i) \quad (1)$$
$$= 1 - \frac{p_i}{v}.$$

This model is straightforward to estimate using ordinary least squares (OLS), regressing an indicator for Clorin purchase on the offer price.

Column (1) of table 5 presents an estimate of equation (1). The model implies that an increase of 100 Kw in the offer price would result in about a 7 percentage point reduction in the probability of purchase. This coefficient is highly statistically significant, and is economically nontrivial, corresponding to a price elasticity (evaluated at the mean offer price and purchase probability) of about -0.6.

The regression has a constant of about 0.96, indicating that the model predicts that 96 percent of households would accept a free Clorin giveaway delivered to their door. This estimate is statistically indistinguishable from unity, which is consistent with our *a priori* intuition that few households would turn down such a valuable gift.[29] Note that, because we did not offer Clorin for free as part of our study, this is a successful *out-of-sample* prediction of the model. The quality of this prediction serves to lessen concerns that the behavior of the households in our study was unusual or unrepresentative due to the prior surveying or other aspects of the experimental design (Levitt and List, 2006), and to increase confidence in our ability to make policy judgments about a range of prices not included in our study.[30]

The model also predicts that the maximum willingness to pay in the population is approximately 1,450 Kw, with a standard error of about 130 Kw. Our estimate of the maximum willingness to

[29] We have also estimated a specification that restricts the constant term to be unity. In this restricted model, we again find that an increase in offer price of 100 Kw reduces the purchase probability by a highly statistically significant 7 percentage points.

[30] Additional specification checks support the use of a simple linear demand model. First, we find that adding a quadratic term in offer price does not improve the model's fit, suggesting that, within the range of experimental variation, there are no detectable nonlinearities in demand. Second, the marginal effect of offer price on demand estimates from logit and probit demand models is virtually identical to those reported in table 5, and the in-sample predictions of the three models are essentially indistinguishable from one another. However, because logit and probit models assume that willingness to pay varies over the entire real line, they tend to predict that a significant number of households would turn Clorin down even if it were free. These findings suggest that, because of the restricted range of willingness to pay in our context, linear probability models may outperform logit and probit models in predicting demand out of sample.

pay is thus statistically indistinguishable from $1,200$ Kw, which is close to the 99th percentile of the retail price distribution. The model therefore appears to be making plausible, though perhaps optimistic, forecasts about the persistence of demand at high, out-of-sample offer prices.

Columns (2) through (4) of table 5 report various checks on the robustness of the results in column (1). In column (2), we re-estimate the model of column (1), but include a wide range of baseline variables as controls. Given the evidence in table 3 that the offer price is orthogonal to households' baseline characteristics, we would not expect the inclusion of these controls to affect our estimates, and indeed the coefficient on offer price in column (2) is nearly identical to that reported in column (1). In column (3), we include an alternative set of controls; namely, fixed effects for the particular individual who attempted to market Clorin to the household. Although we tried to maintain uniformity of procedures across marketers, some marketers may have been more effective than others. However, because the choice of marketer was independent of the offer price, these controls should improve the fit of the model without much change in the estimated price coefficient, which is indeed what we find. In column (4), we re-estimate the specification in column (1) on the sample of households that we reached in our follow-up survey, to check whether the demand curve we estimate is significantly different for this subsample. We again find a constant term close to unity and a coefficient of around 7 percentage points per 100 Kw, suggesting that the underlying distribution of willingness to pay is not very different for the group of households we successfully contacted for our follow-up survey.

It is crucial to our experimental design that households were not aware of their final transaction price when deciding whether to purchase Clorin from us. Because we put the coupons in sealed envelopes and did not tell the marketers what value was on each household's coupon, only a lapse in procedures could have allowed marketers to signal directly or indirectly to households what their transaction price would be. While we made every effort to design our study to guard against such a lapse, we can test for whether one occurred directly in our data by asking whether transaction prices affected demand, after controlling for the offer price. In column (5) of table 5, we estimate a model of demand that includes both offer and transaction prices. Consistent with our protocols, we find that, after controlling for offer price, a household's final transaction price had no statistical effect on its propensity to purchase Clorin. This lack of statistical significance is not due to a lack of power: an F-test definitively rejects the null hypothesis of equal effects of offer and transaction prices ($p < 0.001$). Our data therefore support the view that households had no information about the transaction price, so that, conditional on offer prices, transaction prices could not have affected the composition of the households purchasing Clorin.

The findings in this subsection indicate that raising the offer price by 100 Kw reduced demand by about 7 percentage points. In the next subsection, we analyze which households persisted in purchasing Clorin as we raised the offer price. In particular, we study whether higher offer prices were differentially attractive to households with a strong intention to use Clorin in the near future.

3.2 Estimates of the Screening Effect of Prices

In the previous subsection we showed that higher offer prices reduced demand for Clorin in our door-to-door marketing. In this subsection, we ask how the households who were willing to buy at high prices differ from those who were only willing to buy at low prices.

We first ask whether prices are a useful tool for targeting Clorin distribution to those households that are most likely to use the product. We find consistent evidence that households that agree to buy Clorin at higher prices are more likely to use the product than those who only agreed to buy at lower prices. Next, we examine whether prices allow targeting based on characteristics not otherwise observable to the distributor. We find that the pattern of more intensive use among households buying at higher prices holds even after we control for households' observable characteristics. In other words, we find that prices are a useful screening device even when it is possible to target distribution based on a wide range of household characteristics. Finally, we study some additional costs and benefits of pricing-based targeting. We show that higher prices do not crowd out the poorest households. However, we also find that higher prices do not help to target distribution to the households with the greatest possible health benefits from Clorin.

We turn first to the basic question of whether higher prices help to target distribution of Clorin to the households most likely to use it. Our primary measure of Clorin use will be a dummy for whether the respondent reports that her household's stored drinking water is currently treated with Clorin, as of the time of the follow-up survey. Denote this variable by u_i. As a simple discrete-choice model, we can suppose that each household has an average utility for using Clorin in the weeks following our marketing intervention. This utility is likely to be related to the household's willingness to pay WTP_i, and may also be affected by the transaction price τ_i. On each day, the household receives a shock to its utility for using Clorin, and it chooses to use Clorin on days when the overall utility (average utility plus day-specific shock) is positive. If utility is linear in willingness to pay and transaction price dummies, and if utility shocks are uniform, then we can write that

$$\Pr\left(u_i = 1 \mid WTP_i, \tau_i, b_i = 1\right) = \beta WTP_i + \sum_{j=0}^{7} \rho_j \left(\tau_i = j\right) \tag{2}$$

where ρ_j is a fixed effect for a transaction price of $100j$ Kw. (Note that this expression is conditional on $b_i = 1$; in other words, our analysis of use will focus on those households that purchased Clorin.)

Equation (2) is conditional on the household's willingness to pay, which we do not directly observe. However, we do observe the price at which the household agreed to purchase Clorin, which tells us the range on which its willingness to pay must lie. Given the distributional assumptions in the previous subsection, the expected willingness to pay given an offer price of p_i is linear in p_i, so that

$$\Pr\left(u_i = 1 \mid p_i, \tau_i, b_i = 1\right) = \tilde{\beta} p_i + \sum_{j=0}^{8} \tilde{\rho}_j \left(\tau_i = j\right). \tag{3}$$

Here, $\tilde{\beta}$ and $\tilde{\rho}_j$ are just transformations of the parameters in equation (2).[31]

All else equal, we would expect that the households planning to use Clorin regularly after the marketing intervention would be those most willing to pay for it, i.e. that $\tilde{\beta} > 0$. Note that it may also be the case that households with greater *ex ante* use (use prior to the marketing intervention) will have higher willingness to pay. However, if households' desire to use Clorin fluctuates over time, willingness to pay may be more related to Clorin use after the marketing intervention than before it. In the analysis that follows, we first test the hypothesis that greater willingness to pay is associated with more use in our follow-up survey. We then turn to an analysis of whether *ex ante* use (as measured in our baseline survey) is related to the offer price at which the household agreed to buy Clorin.

[31] Equation (3) allows for a fairly flexible relationship between transaction prices and usage, but it does not allow the screening effect $\tilde{\beta}$ to be different for households paying different transaction prices. As a robustness check, we have estimated model (3) separately by transaction price. The average effect of offer price across these groups is similar in size and statistical significance to the coefficients we report in table 6. Moreover, we cannot reject the restriction that the screening effect is identical across groups paying different transaction prices.

Specification (1A) of table 6 presents estimates of equation (3) using data on self-reported Clorin use from our follow-up survey. We find that, conditional on transaction price, an increase of 100 Kw in the offer price leads to a 3.7 percentage point increase in reported Clorin use among buyers. This coefficient is statistically significant ($p = 0.012$) and economically nontrivial, corresponding to a usage elasticity (at the mean price and usage) of about 0.36.[32] Put differently, our model implies that moving from a free giveaway to a sale at the common retail price of 800 Kw would increase the proportion of usage among purchasers by almost 30 percentage points. Provided that any causal effect of prices acts through the transaction price rather than the offer price, this increased usage comes entirely from the screening effect of prices on the composition of buyers.

Figure 4 presents these findings graphically. The figure shows coefficients from a regression of usage on dummies for offer price, controlling for transaction price fixed effects. We have normalized the y-intercept so that the average usage predicted by the fixed-effects model is equal to the observed average usage. The figure shows a clear, upward-sloping relationship between offer price and intensity of use among buyers, and suggests that the findings in table 6 occur robustly throughout the distribution of offer prices. The figure also does not reveal any striking nonlinearities, suggesting that our linear model may be a good approximation to the behavior of interest.

Because specification (1A) is based on respondents' self-reported use of Clorin, it is possible that our findings are driven partly by reporting bias, if those who bought at higher prices are merely those who like to appear as if they are using Clorin regularly. In specification (2A), we check for this possibility by using as a dependent measure an indicator for whether our chemical test strip showed evidence of free chlorine in the household's drinking water. Using the test strip measure, we find a comparable, though slightly lower, coefficient, indicating that an increase in price of 100 Kw increases by about 3.2 percentage points the share of buyers whose water is chlorinated. This coefficient is statistically significant ($p = 0.033$) and economically meaningful, corresponding to an elasticity of chlorination with respect to price of about 0.30.

Although we find that price is an effective screening mechanism for identifying households that will use Clorin intensively, it might be possible to achieve similar results by targeting using observable household characteristics, such as marital status, education, or household composition.

[32] Logit and probit models of use produced almost identical estimates of the marginal effect of higher prices on usage among buyers.

In other words, a targeted distribution system that uses information about household characteristics might be able to achieve the same composition of buyers as a price mechanism.

To test for this possibility, in panel B of table 6 we re-estimate model (3), but include a vector of household demographic characteristics. Note that these variables are not "controls" in the traditional sense, since we are interested in selection, rather than treatment, effects of prices. Rather, the inclusion of these variables allows us to ask whether higher prices select buyers that are more intensive users *given* their demographic characteristics. In other words, panel B asks whether high prices are a useful screening device even when rich information on household demographics is available. The coefficients in panel B are only slightly smaller than those in panel A, indicating that much of the sorting that we observe in the data is on characteristics that are not available in standard demographic surveys.

The results in panel C of table 6 provide some explanation as to why sorting on demographics does not account for much of the screening effect we estimate. In the models in this panel, we include self-reported Clorin use and estimated water chlorination, measured as of the baseline survey. Including these variables reduces the screening on self-reported usage somewhat, and increases very slightly the estimated screening on chlorination, but both coefficients remain large and statistically significant. In other words, even if we hold constant past usage of Clorin, higher prices select buyers who will use Clorin intensively in the future. Given this finding, it is not surprising that demographic characteristics do not account for much of the screening effect of prices.

There are various reasons why prices might have been an effective screening tool even conditional on information on baseline usage. For example, it could be that households maintain an average stock of Clorin sufficient for their needs, in which case door-to-door marketing may have been most likely to attract households that were anticipating a spike in their needs, rather than households that routinely use a lot of Clorin. Alternatively, it could be that our baseline measures of usage include some noise, and are therefore only imperfect proxies for a household's true usage frequency. In either case, our findings suggest that prices may be an effective screening mechanism even when information on prior use is available to the distributor.

In order to study the sorting underlying the results in table 6 more directly, in table 7 we study how the characteristics of buyers of Clorin change as the offer price increases. Panel A of the table presents estimates of models similar to equation (3), but with various measures of baseline usage as dependent variables. In specification (1), we find some evidence that buyers at higher prices were more likely to report Clorin use in the baseline, but the estimate is much smaller than the comparable estimates from follow-up data in table 6, and is statistically insignificant. Results are similar when we use measured chlorination, in specification (2) of table 7.

The lack of statistical precision in specifications (1) and (2) could mean that there was no sorting on pre-existing Clorin usage, or simply that the sorting on baseline usage was smaller than that on follow-up usage, and therefore more difficult to pick up statistically. To improve our precision, we attempt in specification (3) to aggregate information across several measures of Clorin use as of the baseline. In particular, we estimate a regression model that predicts self-reported Clorin use in the follow-up survey from self-reported Clorin use, measured chlorination, an index of the recency of the last Clorin use, and a measure of the number of bottles of Clorin found in the household, all measured as of the baseline survey. We then regress the fitted values from this model on the offer price, to see if higher offer prices are associated with greater predicted follow-up use. We find that raising the offer price by 100 Kw increases predicted usage among buyers by about 0.6 percentage points, which is statistically significant ($p = 0.037$) but economically much smaller than the screening on follow-up use that we report in table 6. Thus, while we do find some evidence of screening on baseline usage, this sorting is not quantitatively sufficient to explain most of the sorting on follow-up use that we estimate in table 6. This finding suggests that some of the screening value of prices may accrue from identifying households with a high immediate need for Clorin, as opposed to identifying households that consistently use the product intensively.

Although we find that prices are an effective means of targeting households that will use Clorin intensively, some of this screening could come at the cost of excluding the poorest households, or those with the least education. Since these are the households that may be most in need of the products that non-profit organizations distribute, excluding the very poor could be a significant disadvantage of higher prices. In panel B of table 7, we examine the effect of higher prices on the wealth and education of the purchasing households. In specification (6), we find that higher prices do not significantly increase the average wealth of the buying population, as proxied by a measure of the share of consumer durables owned by the household.[33] The coefficient is small, implying that an increase of 100 Kw in the offer price increases the average share of durables owned by purchasing households by 0.16 percentage points, or about one percent of a standard deviation. Moreover, our estimate is reasonably precise, with a confidence interval that rules out effects greater than six percent of a standard deviation per 100 Kw.[34]

Specification (7) of table 7 examines whether higher prices result in higher levels of education among the buying population. Again, we find little evidence that higher prices exclude those with lower human capital: an increase of 100 Kw increases the probability that a purchasing household's female head attended school by about 0.07 percentage points, which is both statistically insignificant and economically tiny. In specification (8), we report that an increase of 100 Kw in the offer price increases the female head of household's average years of schooling among buyers by a statistically insignificant 0.07 years, or about two percent of a standard deviation.

One potential benefit of pricing is that it may help to target distribution toward those households that stand to receive the greatest health improvements from use of Clorin. This especially relates to households with young children, who are the most likely to be severely harmed by diarrhea and other water-borne illnesses (Murray and Lopez, 1996). In panel C of table 7, we assess the effects of higher offer prices on the potential for health gains among the purchasing population. In specification (9), we estimate that raising the offer price has only a small and statistically insignificant effect on the average number of children below age 5 in purchasing households. The coefficient is fairly precisely estimated, with a confidence interval that rules out positive effects larger than about six percent of a standard deviation. In specifications (10) and (11), we report that two other markers of the potential for health benefits—a recent incident of child diarrhea or a pregnant female head of household—are not more prevalent among the buying population when the offer price is higher. The findings in panel C therefore suggest that price screening is not an especially good tool for identifying the households with large potential health benefits from Clorin. Of course, by the same logic these estimates also imply that low prices do not disproportionately

[33]See Morris et al (2000) for evidence that such assset-based measures can provide a good approximation to more sophisticated measures of household wealth in sub-Saharan Africa.

[34]This finding is not limited to the mean of the distribution: we find no evidence (results not reported) that higher prices reduce the share of the purchasing population with durables ownership in the bottom quartile of our sample, suggesting that even the very poor are not driven out by higher prices within the range of our price variation.

help to target households with small children.

Taken together, the findings in this subsection suggest that prices are an effective tool for targeting distribution towards intensive product users. The sorting induced by higher prices occurs along dimensions not related to observable demographics, and only partly related to pre-existing usage levels. Higher prices do not exclude the least well off, although they also do not help to target those with the greatest potential health benefits from Clorin.

3.3 Estimates of the Causal Effect of Prices

In the previous subsection, we used variation in the offer price to show that charging higher prices can help to identify the households most likely to use the product intensively. Another possible effect of higher prices is to induce greater use among purchasers, through a causal mechanism such as the sunk cost effect (Thaler, 1980). In this section, we use variation in the transaction price to estimate this causal effect of prices.

We begin, following the existing literature, by examining whether purchasers paying higher prices are more likely to use Clorin than those paying lower prices. In contrast to previous field experimental evidence on the sunk cost effect (Arkes and Blumer, 1985), we find no evidence of such a pattern. We then move beyond existing field evidence to consider whether the act of paying itself impacts usage. We do find some evidence of such an effect, though it is statistically somewhat weak. However, we find that the effect of the act of paying is greatest among households whose hypothetical choices exhibit a sunk cost effect, and those who report valuing something more if they pay for it, suggesting the possibility of a robust psychological mechanism. (We also consider, and find evidence consistent with, agency-based explanations for this effect.)

Effect of the amount paid. We turn first to the question of whether paying more for Clorin causes greater or more frequent use of the product. We will estimate a model of the following form:

$$\Pr\left(u_i = 1 \mid p_i, \tau_i, b_i = 1\right) = \tilde{\rho}\tau_i + \sum_{j=3}^{8} \tilde{\beta}_j \left(p_i = j\right). \tag{4}$$

Recall that u_i is an indicator for whether the household was using Clorin as of the follow-up survey, and that p_i and τ_i index the offer and transaction prices for household i, respectively. Each $\tilde{\beta}_j$ is a fixed effect for an offer price of $100j$ Kw, allowing us to control nonparametrically for the offer price.[35] Conditioning on $b_i = 1$ means that we only consider households that purchased Clorin during the door-to-door marketing; for other households, it does not make sense to estimate a causal effect of the transaction price.

In specification (1A) of table 8, we present estimates of model (4). We find that increasing the transaction price by 100 Kw increases the probability that the household reports using Clorin by less than two percentage points, which is statistically insignificant and economically smaller than the screening effects we identified in subsection 3.2. Specification (1B) adds our full set of baseline controls to the model, and finds a somewhat smaller coefficient on transaction price. Specifications (3A) and (3B), which use measured chlorination rather than self-reported use as a dependent variable, find a zero or even slightly negative effect of higher prices on the use of Clorin in the household's drinking water.

Our estimates of model (4) reveal no evidence of a causal effect of the amount on product usage, and these estimates are precise enough to rule out interesting magnitudes. For example, in specification (1B), the confidence interval rules out magnitudes above 3.6 percentage points per 100 Kw, about the size of the screening effects we presented in the previous subsection.

To further check whether our estimates are precise enough to rule out interesting magnitudes, we can ask whether our confidence intervals exclude the point estimates in Arkes and Blumer's (1985) study of the effect of discounts on the use of theater tickets, which constitutes the only other field experimental evidence on the sunk cost effect that we are aware of. Some simple calculations indicate that Arkes and Blumer's estimates translate into an elasticity of ticket use with respect to price of about 0.6.[36] Our estimates in specification (1B) of table 8 imply an elasticity of Clorin use with respect to transaction price of about 0.03, with a confidence interval that rules out elasticities

[35] Equation (4) allows for a fairly flexible relationship between offer prices and usage, but it does not allow the causal effect $\tilde{\rho}$ to be different for households agreeing to different offer prices. As a robustness check, we have estimated model (4) separately for households paying different offer prices. The average treatment effect across these groups is similar in size and statistical significance to the coefficients we report in table 8. We cannot reject the hypothesis that the effect of the transaction price is independent of the offer price.

[36] Arkes and Blumer (1985) randomly assigned discounts to individuals purchasing season tickets to the Ohio University Theater. Their study included three (approximately equally-weighted) treatments: those receiving no discount (and paying the full price of $15), those receiving a $2 discount (and paying $13), and those receiving a $7 discount (and paying $8). The average number of tickets used by these three groups was, respectively, 4.11, 3.32, and 3.29. Relative to the average price paid of about $12, the average discount involves a 36 percent reduction in price. And, relative to average utilization, the groups receiving a discount use an average of about 23 percent less. These values correspond to an elasticity of utilization with respect to price of about 0.6.

of about 0.11. We can therefore definitively reject the hypothesis that our participants exhibited an elasticity comparable to that in Arkes and Blumer's (1985) study ($p < 0.001$).

While a lack of power does not seem to explain our failure to find a sunk cost effect, it could be that the households in our study are simply psychologically different from the populations used in prior studies of the psychology of sunk cost. To test for this explanation, we included, at the end of our follow-up survey, a series of hypothetical choices designed to mirror the types of questions frequently used to elicit sunk cost effects in the existing literature. (We placed these questions at the end of the survey in case these questions revealed anything about the study's hypotheses.) In particular, we asked the following question:

> Suppose you bought a bottle of juice for 1,000 Kw. When you start to drink it, you realize you don't really like the taste. Would you finish drinking it?

Participants were able to answer yes or no, and could provide additional comments if they liked. After this question, we asked two similar follow-up questions of all participants, one for the case of a 5,000 Kw bottle of juice, and one for the case of a 500 Kw bottle.[37]

Consistent with existing evidence, we find that participants report that they would be more likely to finish a bottle if it cost more to purchase. Twelve percent of respondents reported that they would finish drinking the juice if it cost 500 Kw, as against 14 percent who said they would finish it had it cost 1,000 Kw. This difference, though economically small, is highly statistically significant (paired $t = -2.78$, $p = 0.006$). Nearly 33 percent of respondents said they would finish drinking the juice if it cost 5,000 Kw, which is both economically and statistically very different from the distribution of responses at 1,000 Kw (paired $t = -13.45$, $p < 0.001$). Indeed, we find that over 20 percent of respondents display the sunk cost effect, in the sense of reporting that they would finish the juice at 5,000 Kw but not at 1,000 Kw, or that they would finish it at 1,000 Kw but not 500 Kw. Moreover, as we report below, there is no effect of the amount paid on Clorin use even for this subsample of the population. Our evidence therefore suggests that our failure to find

[37] To isolate sunk cost effects from informational effects of prices, the follow-up questions emphasized that the juice in question was the same bottle of juice regardless of the price we specified. For example, the second question asked "Now suppose you actually had paid 5,000 Kw for that bottle of juice...Would you finish drinking the bottle?" Surveyors were instructed to emphasize the word *that*, thus stressing the fact that this question refers to the same bottle as in the question about 1,000 Kw.

an effect of the amount paid on use does not result from differences in the way our participants perceive sunk costs in hypothetical scenarios.

Effect of the act of paying. Because our randomization design ensured that a sizable fraction of the sample paid nothing for Clorin, we are able to test whether the act of paying itself impacts use, as suggested by some existing literature (e.g., Oster, Gray, and Weinberg, 2003). Figure 5 shows that our study does reveal some evidence for a discontinuity around a zero transaction price. Each bar reflects the coefficient on a dummy for the associated transaction price, in a regression model similar to equation (4) but with dummy variables in place of the linear term in transaction prices. The coefficients are normalized so that the average bar height is equal to the average usage in the population. In other words, each bar reports our estimate of how much Clorin a household would use given each transaction price, holding constant the offer price at which the household agreed to buy. We have combined the data for transaction prices of 500, 600, and 700 Kw, because the sample sizes in these cells are very small.

The graph shows that usage increases from a transaction price of zero to one of 100 Kw, and remains essentially constant thereafter, dipping slightly for a transaction price of 500-700 Kw. In specification (2A) of table 8, we test formally for an effect of paying for Clorin (i.e., an effect of a positive transaction price) on use measured as of the follow-up survey. We find that requiring a household to pay for Clorin results in a statistically significant increase in usage of about 9 percentage points. This effect is economically large, but as specification (2B) shows, it is not robust statistically to the inclusion of controls, although it remains economically important even in this specification. Moreover, the coefficient is positive and economically large, but not statistically significant, when we measure Clorin use by the degree of water chlorination (specifications 4A and 4B). We therefore find some support for the view that there is a discontinuity at zero, but not enough to statistically reject the null hypothesis of no transaction price effect.

Evidence on possible mechanisms. One approach to resolving this ambiguity in our findings is to examine the heterogeneity in the estimated effect of a positive transaction price on product use. In particular, because we asked respondents various questions relating to the sunk cost effect and other potential mechanisms for a causal effect of price, we can look directly at whether participants who report being subject to these mechanisms are those for whom the transaction price has the greatest effect on usage.

Specifications (1) and (2) of table 9 present estimates of the effect of a positive transaction price on Clorin usage, with the sample split according to whether the survey respondent displayed the sunk cost effect in the hypothetical choices described above. All specifications include our full set of baseline controls, as well as fixed effects for the offer price. Consistent with the hypothesis that the sunk cost effect plays some role in the effect of the act of paying, we do find that the effect is larger among those respondents (about one-fifth of the sample) who display the sunk cost effect. This is true when we measure usage either through self-reports or through our measurement of the chlorination of the household's drinking water. In both cases, however, our estimates are not precise enough to permit us to statistically distinguish the coefficients for the two subsamples.

The practitioner literature on pricing by nonprofits speaks of "buy-in" or "commitment" effects of prices, a concept that seems slightly different from the sunk cost effect and relates to the effect of a payment on the extent to which a household "values" a product. Because we did not know of a standard set of questions that we could use to measure a household's susceptibility to such an effect, we asked about it directly, querying households to strongly disagree, disagree somewhat, agree somewhat, or strongly agree with the statement that "I value something more if I paid for it." About half of the participants said that they strongly agree with this statement, and answers to this question were not strongly related to our measure of the sunk cost effect, suggesting a potentially different psychological phenomenon. As specification (3) of table 9 shows, among households who strongly agreed with this statement, there is a statistically significant and economically large effect of a positive transaction price on the propensity to report using Clorin. Among households that do not strongly agree with the statement, the effect of a positive transaction price is essentially zero. This contrast provides some support for a "valuation" mechanism. Note, however, that we cannot distinguish the two coefficients statistically, and that the difference in coefficients is much smaller when we measure usage with our chemical test for chlorination.

A final potential mechanism for an effect of transaction prices on product usage is an agency-type explanation in the spirit of Prendergast and Stole (1996) or Ashraf (2005). In particular, it may be that participants need to convince other members of the household that their purchases were justified, so as to maintain their credibility in future purchasing decisions. Such a dynamic could make it optimal to use a product more intensively if it was costly than if it was freely given. Although we do not have a perfect test of this mechanism, a simple approach is to compare respondents who make choices in the presence of other adults to those who do not. To implement this comparison, in specifications (5) and (6) of table 9, we split the sample according to whether the respondent is married. Consistent with the agency mechanism, the effect of a positive transaction price on self-reported usage for married respondents (about four-fifths of the sample) is a large and statistically significant 10 percentage points. By contrast, for unmarried respondents the effect is strongly negative, and statistically distinguishable from the coefficient for married respondents. A similar, though more muted, contrast is present when we measure usage with the chemical presence of free chlorine. Though this finding by no means conclusively demonstrates that the effect of the act of paying has an agency component, it is at least consistent with this hypothesis.

Though the estimates in table 9 refer to the effect of the act of paying on Clorin use, it is possible to conduct similar tests for the role of alternative psychological mechanisms in mediating the effect of the amount paid. Consistent with our earlier finding that the amount paid does not impact use, we do not find consistent evidence that the effect of the amount paid is greater in subsamples displaying the proposed psychological mechanisms (results not shown). For example, while we do find that the effect of amount paid on self-reported use is slightly larger (though still small and statistically insignificant) among households displaying the sunk-cost fallacy in hypothetical choices, this difference reverses in direction when we used measured chlorination as our dependent variable. Moreover, even for those who strongly agree that they value something more if they pay for it, increasing the payment amount has a statistically insignificant *negative* effect on subsequent use. Similar inconsistencies arise for the contrasts between married and unmarried respondents. Taken together, these estimates reinforce the finding that, conditional on paying, the amount paid does not influence the decision to use Clorin.

The evidence reported in this subsection shows that the act of paying for a product may have an influence on a household's decision to use it, especially among households that display the sunk cost effect in hypothetical choices, and among households that report valuing something more if they pay for it. By contrast, we find no evidence that, conditional on paying, the amount paid influences use. Thus, while our findings are consistent with the basic psychological premise that sunk costs can influence behavior, they contrast with previous evidence suggesting that larger sunk payments induce greater product use.[38] That this is so despite our finding that participants display sunk-cost effects in hypothetical choices is consistent with Friedman et al's (2004) finding that sunk cost effects are small in laboratory experiments with real financial incentives. Overall, our results may suggest a need to revisit the role of prices in the psychology of product use, with special emphasis on the act of paying, rather than the amount paid.

3.4 Robustness and Interpretation

As section 2 outlines, we took steps in designing our experiment to rule out effects of prices other than through the screening and causal mechanisms that we estimate above. It is possible, however, that the experimental variation in prices induced changes in Clorin use through mechanisms we have not modeled. In this subsection, we use several pieces of evidence from our study to test for the presence of alternative effects of prices, namely informational and income effects, as well as to test whether offer prices might have had psychological effects on use. In all cases, the tests reveal no evidence of these effects. Though not fully conclusive, these findings, together with the details of the experimental design, provide some reason to believe that our analysis identifies the parameters of interest.

Effects of treatment on beliefs about Clorin quality. By basing our study on Clorin, a well-known product with an established market, we sought to minimize the role of price-based inference. If, despite our design, higher offer or transaction prices were taken to be evidence that Clorin is a better product, and favorable beliefs induce more product use, this could have exacerbated either the screening effects or the causal effects we estimate. This would not affect our conclusion that there is no overall causal effect of prices, but it could mean that the screening effect or effect of positive prices is overstated. For a crude test for such an effect, we can take advantage of the presence in our follow-up survey of several measures of respondent attitudes toward Clorin. In particular, the survey asks the respondent (on an agree/disagree scale) whether water purification

[38] While our results are inconsistent with Arkes and Blumer's (1985) conclusions, the two sets of findings have common ground, in the sense that Arkes and Blumer find that, conditional on receiving a discount, the amount of the discount does not affect use of theater tickets. It is therefore possible that the effects they estimate result from the presence of a discount *per se*, rather than the dollar amount of the discount or sunk cost. This would be consistent in spirit with the sunk cost effect but, as with our results, would suggest a different set of hypotheses regarding its underlying psychological causes.

solution is easily available, whether it makes the water taste bad, and whether it is an effective way to prevent diarrhea. None of these scales is statistically significantly affected by either the offer or transaction price, and an aggregate index that includes all three is also unaffected by our treatments.

Effects of treatment on beliefs about market prices. We told participants the prevailing retail price of Clorin, and stressed to them that our offer to sell Clorin was for one-time only. These steps were intended to limit the possibility that participants would believe that lower offer or transaction prices reflected a decrease in the future marginal cost of acquiring Clorin, and change their usage in response. If such an effect occurred, it would tend to attenuate the screening and causal effects we estimate. To test for this confound, we asked Clorin buyers in the follow-up how much they usually pay for a bottle of Clorin, and we asked those who reported not buying Clorin how much they would expect to pay for a bottle. We find no effect of offer or transaction prices on participants' responses to these questions.[39]

Income effects. Because the amounts of money involved in our study were modest by Zambian standards, we would not expect income effects to be a major confound. However, if paying more for Clorin reduced household wealth, this could in principle attenuate the causal effect (though not the screening effect). As a simple test for this possibility, we have tested for an effect of transaction price on usage among those in our sample with above-median wealth (as proxied by durables ownership). Even among this group, there is no causal effect of prices, suggesting that attenuation due to income effects is unlikely to be a major confound.

Psychological effects of offer prices. By identifying the transaction price before participants counted their money, by printing the transaction price on the coupon, and by asking participants to write the transaction price on a receipt and sign it, we endeavored to make the transaction price salient to the households in our study. Some evidence indicates that we succeeded at least in making part of the transaction price memorable. In the follow-up survey, respondents were asked whether anyone had offered them Clorin for free in the last month. Among households that, according to

[39] Because not all respondents were asked how much they would expect to pay for Clorin in the future, it is possible that this measure understates the true effect of transaction prices on expectations. To address this concern, in the second follow-up survey (described in section 4 below), we asked all respondents how much they would expect to pay for a bottle of Clorin in the future. We again find no statistically significant relationship between responses to this question and the transaction price at which the household purchased Clorin.

our records, received a free bottle (zero transaction price), some 60 percent report having received a bottle for free, as against only 16 percent among those who did not receive a free bottle (transaction price above 0). The difference between these two groups is highly statistically significant, and the presence of some positive responses among those paying for Clorin seems plausibly attributable to survey noise.

If, despite our efforts to guard against this, participants still felt psychologically "committed" to the offer price, some of what we identify as a screening effect could come from sunk-cost type effects. Recall, however, that the evidence in table 7 indicates that some of the screening is on baseline characteristics, which cannot have been caused by sunk cost effects. As an additional check on whether some of the remaining screening effects could be due to psychological effects of prices, we have estimated the specifications in table 9, but contrasting the screening effect rather than the causal effect across different groups of participants. On the whole, we find no evidence that screening effects are larger in the households for which we would expect larger causal effects of prices. The coefficients are statistically indistinguishable and economically similar across groups, and the contrasts across groups have no consistent direction. Indeed, when we measure use with respondents' self reports, we find that if anything the screening effects are slightly smaller among households that display the sunk cost fallacy in a hypothetical choice scenario. If our hypothetical choices proxy well for psychological propensities, then these tests provide evidence against the view that psychological effects contaminate our estimates of the screening effect.

4 Longer-Term Effects of Prices on Clorin Use

The preceding estimates relate offer and transaction prices to Clorin use measured approximately two-weeks after our door-to-door marketing intervention. For some purposes, longer-term usage measures may also be of interest. For example, if those who agree to buy Clorin at high offer prices use more Clorin soon after their purchase, but not in the long run, this would suggest that prices are effective in identifying buyers with an immediate desire to use Clorin, as opposed to those with a persistently high propensity to use it. By contrast, if screening effects arise in both short-run and long-run follow-up data, this could indicate that high prices identify those with a constant, high-intensity need for Clorin.

Longer-term data may also be informative about the causal effect of transaction prices on Clorin usage. If higher transaction prices induce greater Clorin use in the long-term, this could indicate a fundamental shift in the household's attitude toward Clorin, rather than a temporary elevation in use due to the high cost associated with a particular bottle.

To shed some light on these issues, we conducted a second follow-up survey, with interviews occurring approximately six weeks after the marketing intervention. As in the first follow-up, we attempted to contact only those households who had been successfully contacted during the marketing intervention. We used a survey instrument similar to that from the first follow-up, with a few additional questions about willingness to pay for Clorin. In addition to testing each household's stored drinking water for chlorine, we were able in this survey wave to test the source (public or private tap connected to the main water line) from which each household obtained its water. This source water testing allows us to control for variation in chlorination due to factors other than a household's use of Clorin.

We successfully contacted approximately 80 percent of households for the second follow-up, significantly lower than the 89 percent recontact rate from the first follow-up. Moreover, an analysis of survey attrition reveals some (marginally statistically significant) evidence that households receiving higher offer prices were less likely to be interviewed in the second follow-up survey. We also find some (weaker) evidence of a relationship between transaction prices and attrition. These results suggest a need for caution in interpreting the findings from the second follow-up survey, as selective attrition could induce a bias in our estimates of experimental effects.

With that caveat in mind, we turn in table 10 to an analysis of the results from our second follow-up survey. Columns (1) through (3) of the table present results using self-reported Clorin use as the dependent measure; columns (4) through (6) repeats all specifications using the chemical presence of free chlorine as the dependent variable.

An important methodological difference between the first and second follow-up surveys is that in the second survey, we were able to measure the chlorination of the household's water *source*, in addition to the stored water within the home. In other words, our surveyors measured the chlorination of the public or private tap from which each household obtained its water, allowing us to adjust our estimates for variation in ambient chlorine levels. As a result, our estimates in columns (4) through (6) will use as a dependent measure a dummy for whether the household's stored water contained a *greater* amount of free chlorine than water from the household's water source. Using this augmented variable should improve the precision of our estimates of effects of price on chlorination levels.

In column (1) of table 10, we present evidence on the screening effect of prices, examining whether households that agree to buy at a higher offer price also report using more Clorin in the second follow-up survey. We find no evidence of such a relationship; if anything, it is slightly negative, though statistically indistinguishable from zero. Results are similar when we examine measured chlorination in column (4). Taken together, these findings suggest that higher offer prices did not screen out households with a low long-term propensity to use Clorin. This is consistent with our earlier finding that offer prices only screen slightly on baseline usage, and suggests that high offer prices were especially effective at selecting households with a high short-term need for Clorin.

In columns (2) and (5), we turn to an analysis of the causal effect of transaction prices. As before, we estimate a precise, small, and statistically insignificant effect of greater transaction prices on subsequent propensity to use Clorin. This finding stands in contrast to earlier evidence on the sunk cost effect (Arkes and Blumer, 1985).

In columns (3) and (6), we focus on the contrast between households that paid something for Clorin and those who paid nothing. Our point estimates are nontrivial, implying an effect of positive transaction prices on self-reported usage of around six percentage points and on measured chlorination of around 9 percentage points. Indeed, the effect on measured chlorination is statistically significant (in contrast to the results from the first follow-up), which may reflect the gain in statistical precision from the ability to correct for source chlorination. These results therefore support the possibility of longer-term effects of positive prices on intensity of use.

5 Implications for Optimal Pricing Policy

In section 3, we establish that charging higher prices for Clorin reduces demand for it, but helps to target distribution to those households that are most likely to use it. We also show that, conditional on this sorting, charging a positive price may induce greater use than giving Clorin away for free. Finally, we show that higher positive prices do not induce greater use than lower positive prices.

In this section, we combine these elements into a simple model of Clorin use that allows us to develop implications for pricing policy. This will allow us to use our estimates to simulate events that did not occur in our sample. While such an extrapolation should necessarily be taken with caution, the simulations in this section serve to highlight the potential value of our estimates for policymaking. They also allow us to evaluate whether the screening and causal effects of prices are large, relative to the negative effect of higher prices on the propensity to buy Clorin.

Formally, we will adopt a simple model of Clorin purchase and use that parallels the models we used for our empirical analysis. Specifically, we will assume that the probability of purchasing Clorin is given by

$$\Pr(b_i = 1) = 1 - \frac{p_i}{v} \tag{5}$$

where the slope parameter $\frac{1}{v}$ is estimated from our demand data in table 5.[40] We will assume that Clorin use among buyers of Clorin is determined by the following equation:

$$\Pr(u_i = 1 \mid b_i = 1) = \alpha + \beta p_i + \gamma (p_i > 0). \tag{6}$$

Here, β captures the screening effect of offer prices on self-reported use, estimated in column (1A) of table 6. The parameter γ captures the causal effect of positive transaction prices on subsequent use, and is estimated in column (2B) of table 8. Consistent with our empirical evidence, we will assume no causal effect of prices beyond the contrast between positive prices and free distribution. Finally, we will fit the intercept α to equalize average use in our sample with average predicted use according to model (6).

Note that, although we estimate β and γ using variation in offer and transaction prices, respec-

[40] For greater consistency with the model, we use estimates from a specification in which we restrict the constant term to be unity, but this assumption is immaterial to the findings in this section.

tively, for the purposes of our policy analysis we will assume that the Clorin distributor charges a single price p_i that serves as both the offer and transaction price. This is the first way in which our policy simulation makes inferences out-of-sample, in the sense of predicting behavior in cases not directly observed in our experiment.

The model in equations (5) and (6) also implicitly assumes that the parameters from our study can be used to estimate behavior at offer prices above 800 Kw and below 300 Kw, although our study did not include such prices. Despite the fact that the linear models we estimate provide a good in-sample fit to the behaviors we measure, caution must necessarily be taken in assuming that the same functional relationships hold outside the range of experimental variation. This caveat holds especially for the impact of offer prices below 300 Kw, because households that refused to purchase Clorin from us at 300 Kw were, by definition, excluded from our transaction price randomization. We therefore cannot be sure that these households would respond to variation in the transaction price in the same fashion as the households that are willing to pay 300 Kw or more for Clorin, for whom we have in-sample evidence of the effect of the act of paying and the amount paid on the propensity to use Clorin.

We will further assume for the purposes of our simulation that non-buyers of Clorin do not use the product. Because Clorin is widely available in retail stores in Lusaka, this assumption is counterfactual. However, it allows us to more nearly approximate what would happen if the market price of Clorin itself, rather than the price of Clorin through a door-to-door delivery, were to vary.

Using our estimates to predict the effects of a permanent change in the market price of Clorin requires two further caveats. First, our experiment varied the door-to-door price of Clorin without changing either the retail price or the future price of the product. This means that part of the effect of raising the price of Clorin on the probability of purchase comes from substitution away from purchases from retail outlets, or from future purchases. By contrast, a permanent, market-wide change in the price of Clorin would vary all prices simultaneously, which means that the only substitution effect in such a case would be from buying Clorin to not buying Clorin at all. Qualitatively, this means that the demand parameter we estimate in equation (5) is likely to overstate the effects of an increase in the market price of Clorin on Clorin sales, a bias that would lead us to understate the case for high prices.

The second, related, caveat is that a permanent change in the price of Clorin would lead to a reduction of current use, because the shadow price of replacing Clorin would rise as a result of the increase in future prices. Because we took care to offer a one-time purchase only, our experiment does not include such effects. This fact is useful for isolating screening and causal effects of prices, but also means that our results may omit a negative effect of future prices on current use that would be present in the case of an economy-wide price change. This could lead us to overstate the case for high prices, and would therefore tend to work against our overestimation of the effect of price on purchases.

Taken together, then, the likely effect of ignoring the permanence and breadth of an economy-wide price change on our estimates is ambiguous, because the two potential biases we outline above work in opposite directions. This suggests that caution is needed in extrapolating our results to counterfactual effects of policy changes. However, our estimates provide important information on the underlying economic principles at work in the market for Clorin, and aggregating them through a policy simulation allows us to demonstrate the economic significance of the screening and causal effects of prices, relative to the negative effect of higher prices on the propensity to buy Clorin.

Figure 6 presents the results of our simulation. The top line presents the results of the fully specified model. Consistent with our findings on the causal effect of prices, the model predicts that moving from free distribution to a price of 100 Kw induces an increase in usage of around 6.5 percentage points. Thereafter, use tends to decline with price, but this decline is relatively gradual because of the screening effects we estimate. In other words, although our demand estimates suggest that an increase in price of 100 Kw reduces purchases by 7 percentage points, our screening evidence indicates that many of those who do not purchase as a result of higher prices would not have used the product anyway. As a result of the screening effect, then, we find that usage with positive prices does not decline below the level of usage with free distribution until the price exceeds 700 Kw per bottle of Clorin.

Our model shows that the use-maximizing price of Clorin is around 100 or 200 Kw.[41] However, it also shows that use at 700 Kw is similar to use given free distribution. This implies that a

[41] To be more precise, the model implies that the use-maximizing price is 220 Kw, but predicted use at this price is essentially identical to predicted use at 100 Kw. The slight difference between the two results from the quadratic curvature implied by multiplying the linear models for demand and use in equations (5) and (6), respectively.

distributor with even an arbitrarily small concern for revenue (or ability to translate revenue into additional usage through advertising and other methods) would prefer to charge 700 Kw rather than distributing the product for free, a striking result given the prevailing retail price of around 800 Kw.

The second line in figure 6 highlights the role of screening by setting $\beta = 0$ while leaving all other parameters unchanged. In this case, only the causal effect of prices works against the effect of price on purchases. We therefore find that only prices between 100 and 200 Kw achieve use greater than or equal to use under free distribution; to justify higher prices rather than free distribution, a nontrivial concern for revenue would be necessary. The sharp differences between this case and our baseline case with screening highlight the quantitative importance of our screening results.

The final line in figure 6 "turns off" the causal effect of prices by setting $\gamma = 0$. In this case, the effect of price on use comes solely through the demand curve (5), and therefore shows a fairly steep, linear decline in use as prices are increased. Contrasting this case with our baseline case shows that the screening and causal effects of prices interact to generate important policy advice. In particular, when both screening and causal effects are operating, even prices close to the prevailing retail price can be charged with little or no loss in usage relative to free distribution. By contrast, charging such prices in the absence of the price effects we estimate would lead to 50 percent lower use than free distribution. This contrast suggests that understanding the screening and causal effects of prices on product use is a critical step in resolving the ongoing debate over the pricing of health products in the developing world.

6 Conclusions

Efficient social programs must target their benefits to the most needy recipients in order to maximize their impact. With benefits that come in-kind—in the form of products or services—a critical targeting issue is identifying households that will actually use the benefit in question. Our research shows that pricing can be an effective tool for identifying those households. In a field experiment in Lusaka, Zambia, we find that households agreeing to buy a water purification solution at higher prices are much more likely to use the product after purchasing it. We also find some evidence that the act of paying influences, over and above the selection effects on the composition of households receiving the product. However, we find that, conditional on paying, increases in the amount paid do not increase product usage. These findings have important implications for economics and psychology, and for both private- and public-sector industries in which product use is an important input to firms' objectives.

Simulations from a simple model suggest that our findings are quantitatively important for optimal pricing policy. Our model implies that prices close to the prevailing retail price can be charged with little or no reduction in usage relative to free distribution. While lower prices would lead to higher use, these findings imply that even an arbitrarily small concern for revenue generation (or an ability to translate greater revenues into greater product usage) would be sufficient to make free distribution suboptimal from the perspective of maximizing product usage. These policy simulations suggest that understanding the screening and causal effects of prices on product use is a crucial input to resolving the ongoing debate over the pricing of social programs, especially in the developing world.

Our findings also have broader implications about the psychological effects of prices on product use. Contrary to previous studies on the sunk cost effect, we find no evidence that higher prices lead to greater use. However, we do find some evidence that charging a positive price results in greater use than giving away the product for free, an effect that is especially strong for households that seem susceptible to the sunk cost effect and psychologically related mechanisms, as measured by attitudinal surveys. The finding that survey instruments can help to account for the heterogeneity in the effects of prices on product use may have strategic implications for organizations wishing to predict the effect of prices on usage among a population of consumers.

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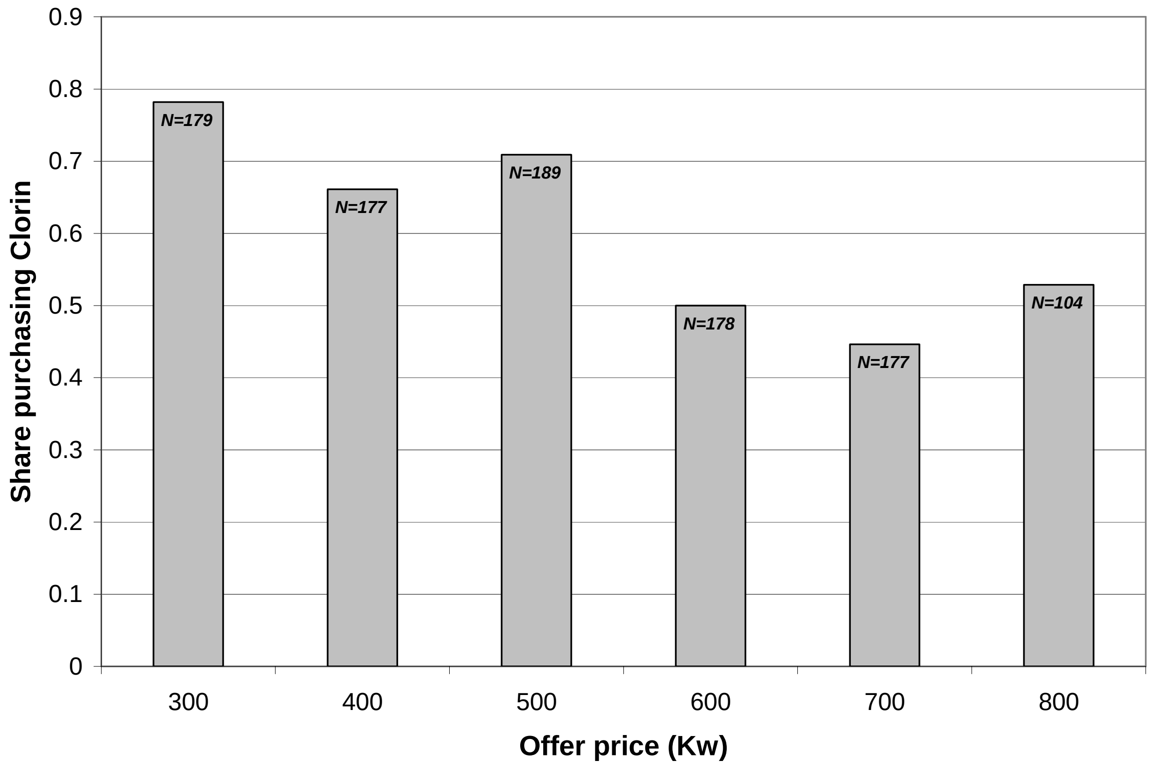
Figure 1 *A bottle of Clorin*

Figure 2 *Sample coupon from door-to-door marketing*

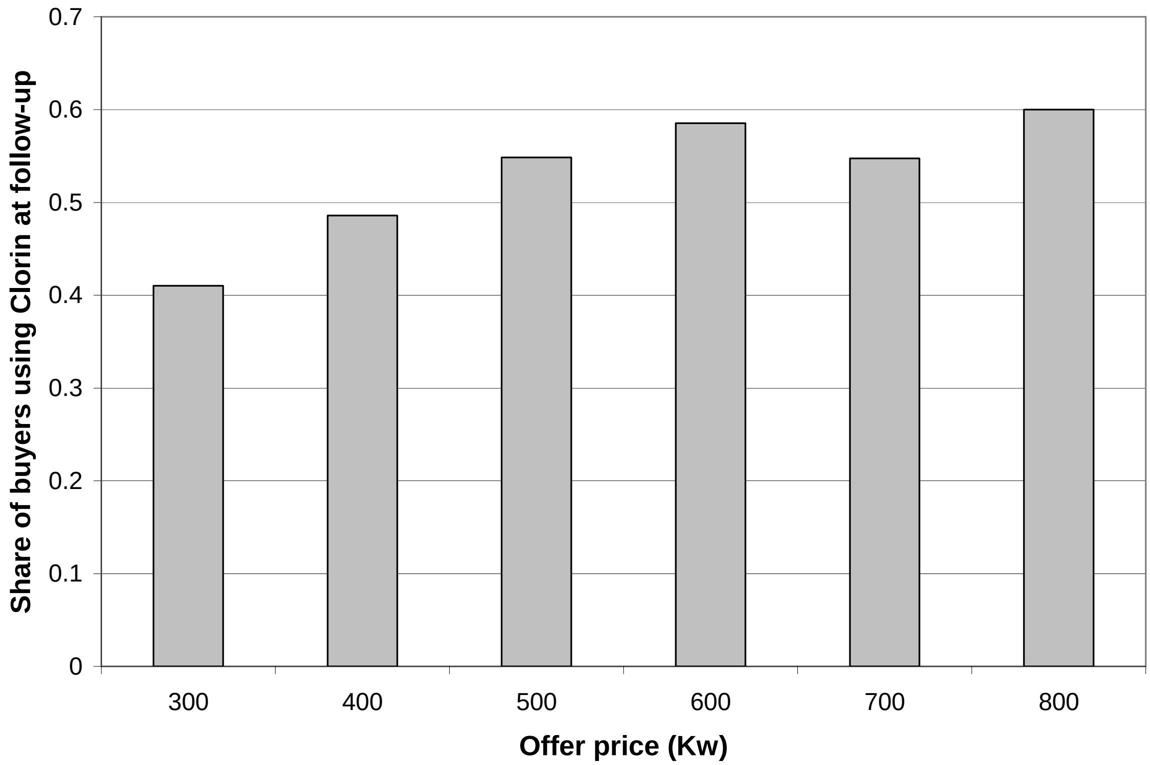
Notes: Figure shows a sample discount coupon from door-to-door marketing experiment. Coupon shows the final price at which the bottle transacted.

Figure 3 *The effect of offer price on purchase of Clorin*



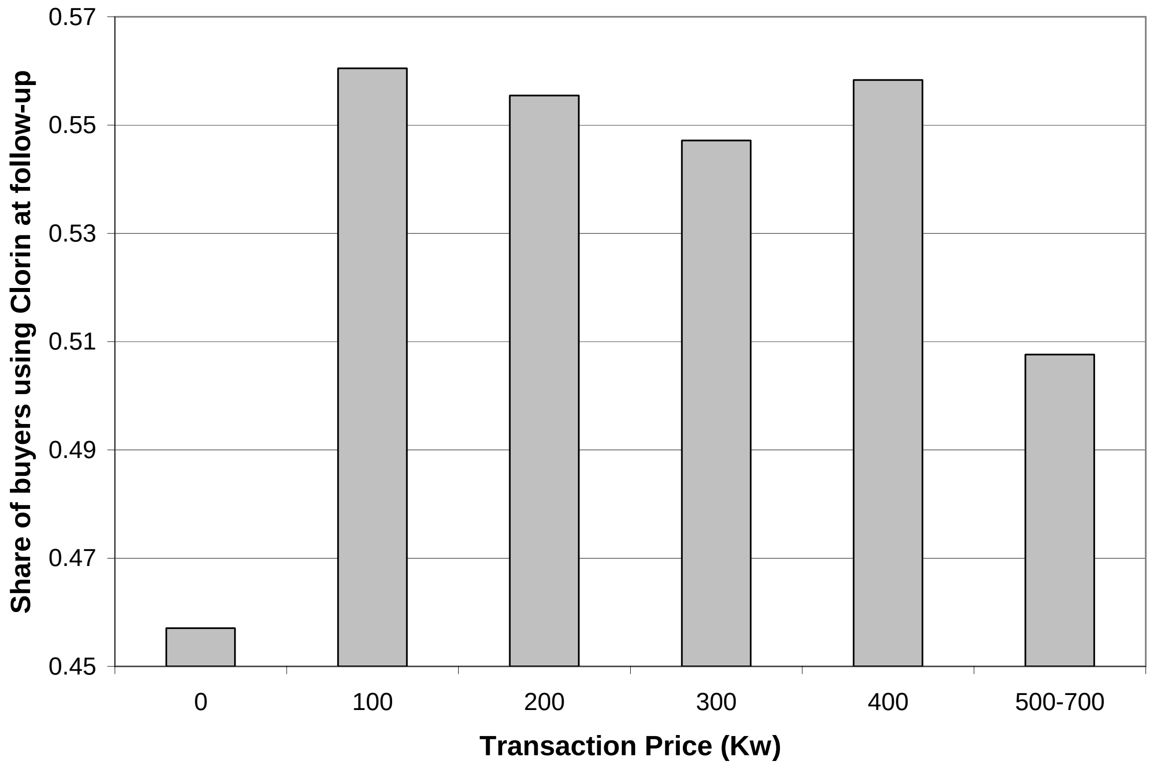
Notes: Figure shows share of households purchasing Clorin in door-to-door marketing intervention, at different offer prices (in Zambian Kwacha). Reported N indicates total number of households facing each offer price.

Figure 4 *Usage rates of Clorin by offer price*



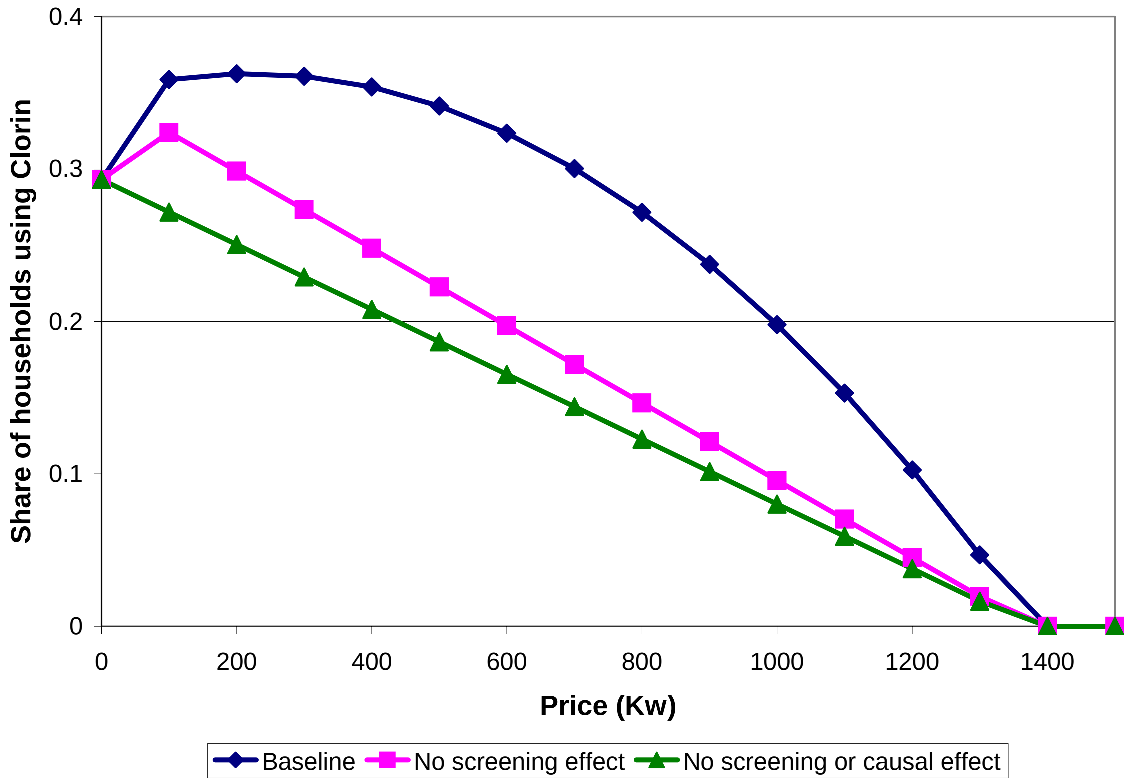
Notes: Figure shows coefficients from a regression of self-reported Clorin use at follow-up on dummies for offer price, with fixed effects for transaction price, for those households who purchased Clorin in our door-to-door marketing exercise. Coefficient on omitted category (offer price = 300 Kw) is normalized so that predicted share at mean offer price is equal to the overall share using Clorin.

Figure 5 *Usage rates of Clorin by transaction price*



Notes: Figure shows coefficients from a regression of self-reported Clorin use at follow-up on dummies for transaction price, with fixed effects for offer price, for those households who purchased Clorin in our door-to-door marketing exercise. Coefficient on omitted category (transaction price = 0 Kw) is normalized so that predicted share at mean transaction price is equal to the overall share using Clorin. Cells with transaction price of 500, 600, and 700 Kw have been aggregated to improve precision.

Figure 6 *Simulated effects of alternative pricing strategies*



Notes: Figure shows predictions based on estimated demand and usage models as described in section 5. "Baseline" model assumes that non-buyers do not use Clorin. "No screening effect" model assumes that usage rates are unaffected by offer price. "No screening or causal effect" model further assumes that transaction prices do not affect usage.

Table 1 *Demographic characteristics of the baseline sample*

	(1)	(2)	(3)
Source	Baseline survey	Baseline survey	DHS
Sample	All	Ages 15-49	Ages 15-49
Age	32.8257	30.1593	27.1425
	(0.3130)	(0.2254)	(0.2948)
Years of completed schooling	6.6418	7.0285	7.2379
	(0.1013)	(0.1013)	(0.1209)
Married?	0.8000	0.8327	0.5642
	(0.0113)	(0.0111)	(0.0170)
Currently pregnant?	0.1143	0.1254	0.0754
	(0.0090)	(0.0099)	(0.0091)
Total number of living children	3.1867	2.9484	2.1932
	(0.0630)	(0.0614)	(0.0791)
Number of children in household under age 5	0.9619	0.9875	1.1767
	(0.0245)	(0.0253)	(0.0365)
Household owns a radio?	0.5540	0.5721	0.6266
	(0.0140)	(0.0148)	(0.0166)
Household owns a television?	0.4992	0.5151	0.5501
	(0.0141)	(0.0149)	(0.0171)
Household owns a refrigerator?	0.1905	0.1940	0.2686
	(0.0111)	(0.0118)	(0.0152)
Household owns a bicycle?	0.1000	0.1077	0.1213
	(0.0085)	(0.0092)	(0.0112)
Household owns a motorcycle?	0.0008	0.0009	0.0012
	(0.0008)	(0.0009)	(0.0012)
Household owns a car?	0.0230	0.0258	0.0836
	(0.0042)	(0.0047)	(0.0095)
Number of observations	1260	1124	849

Notes: Table shows means of variables, with standard errors in parentheses. Columns (1) and (2) use data from our baseline survey. Column (3) uses data on Lusaka residents from the 2001 Demographic and Health Survey (DHS) of Zambia. Actual number of observations in columns (1) and (2) varies slightly across variables due to questionnaire refusals.

Table 2 *Distribution of offer and transaction prices*

	Offer Price (Kw)						
	300	400	500	600	700	800	Total
Number of participants	226	227	227	227	227	126	1260
(percent of all participants)	(17.94)	(18.02)	(18.02)	(18.02)	(18.02)	(10.00)	(100)
Transaction Price (Kw):							
0	90	90	90	90	90	50	500
	(39.82)	(39.65)	(39.65)	(39.65)	(39.65)	(39.68)	(39.68)
100	67	45	34	27	22	10	205
	(29.65)	(19.82)	(14.98)	(11.89)	(9.69)	(7.94)	(16.27)
200	69	46	34	27	23	11	210
	(30.53)	(20.26)	(14.98)	(11.89)	(10.13)	(8.73)	(16.67)
300	—	46	34	28	23	11	142
		(20.26)	(14.98)	(12.33)	(10.13)	(8.73)	(11.27)
400	—	—	35	27	23	11	96
			(15.42)	(11.89)	(10.13)	(8.73)	(7.62)
500	—	—	—	28	23	11	62
				(12.33)	(10.13)	(8.73)	(4.92)
600	—	—	—	—	23	11	34
					(10.13)	(8.73)	(2.7)
700	—	—	—	—	—	11	11
						(8.73)	(0.87)

Notes: The first section of the table shows the distribution of participants across offer prices, with percent of total in parentheses. The remaining rows show the distribution of transaction prices conditional on a given offer price, with conditional percentages in parentheses. For example, the cell listed under an offer price of 300 Kw and a transaction price of 200 Kw should be read to say that 69 households received an offer price of 300 Kw and a transaction price of 200 Kw, and that these 69 households represent 30.53 percent of the households receiving an offer price of 300 Kw.

Table 3 *Testing the balance of observables across treatment conditions*

	(1)	(2)	(3)	(4)
Sample	All	Marketing	Purchased Clorin	
Dependent variable	*Offer Price*	*Offer Price*	*Transaction Price*	*Transaction Price > 0*
Water currently treated with Clorin?	0.1474	0.2040	-0.1171	0.0668
(baseline)	(0.1114)	(0.1250)	(0.1747)	(0.0525)
Drinking water contains free chlorine?	0.0764	0.0150	0.3300	0.0643
(baseline)	(0.0892)	(0.1003)	(0.1412)	(0.0425)
Use of soap before handling food	0.0032	-0.0881	0.2281	0.0860
(index)	(0.1546)	(0.1735)	(0.2519)	(0.0757)
Use of soap after using toilet	-0.3067	-0.1992	0.0863	-0.0192
(index)	(0.1593)	(0.1782)	(0.2564)	(0.0771)
Attitude toward water purification	-0.0828	-0.3628	0.5490	0.0645
(index)	(0.2258)	(0.2531)	(0.3564)	(0.1071)
Age in years	0.0032	0.0023	-0.0002	-0.0010
	(0.0046)	(0.0052)	(0.0076)	(0.0023)
Ever attended school?	-0.0986	-0.1235	0.2510	0.1501
	(0.1830)	(0.2050)	(0.2874)	(0.0864)
Years of completed schooling	0.0097	0.0187	-0.0352	-0.0157
	(0.0189)	(0.0215)	(0.0305)	(0.0092)
Currently married?	0.0870	0.0381	-0.1274	0.0416
	(0.1160)	(0.1327)	(0.1881)	(0.0565)
Currently pregnant?	-0.0118	0.0768	-0.0400	-0.0037
	(0.1355)	(0.1550)	(0.2222)	(0.0668)
Every given birth to any children?	-0.1571	-0.1471	0.2126	-0.0410
	(0.1806)	(0.2065)	(0.2913)	(0.0876)
No. of children in household under age 5	0.0474	0.0596	0.0904	0.0381
	(0.0536)	(0.0609)	(0.0918)	(0.0276)
No. of people in household	-0.0196	-0.0106	-0.0377	-0.0042
	(0.0193)	(0.0214)	(0.0298)	(0.0090)
Share of durables owned	0.1286	0.0603	0.2612	0.0100
	(0.2885)	(0.3265)	(0.4499)	(0.1352)
Locality fixed effects?	YES	YES	YES	YES
Fixed effects for offer price?	NO	NO	YES	YES
Fixed effects for transaction price?	YES	YES	NO	NO
F-test that all coefficients are 0	0.64	0.64	0.90	0.93
p-value of F-test	0.8719	0.8686	0.5802	0.5395
R^2	0.1913	0.1840	0.1242	0.0337
Number of observations	1244	990	605	605

Notes: Standard errors in parentheses. "Marketing" refers to households reached for door-to-door marketing. All variables measured as of baseline survey. Transaction price fixed effects excluded from F-test in columns (1) and (2). Offer price fixed effects excluded from F-test in columns (3) and (4). Prices in units of 100 Kw.

Table 4 *Determinants of sample attrition*

	(1)	(2)	(3)	(4)
Sample	All	Marketing	Purchased Clorin	
Dependent variable	*Marketing*	*Follow-up*	*Follow-up*	*Follow-up*
Offer price (100 Kw)	0.0021	0.0022		
	(0.0073)	(0.0069)		
Transaction price (100 Kw)	-0.0031		0.0063	
	(0.0068)		(0.0080)	
Transaction price > 0				0.0325
				(0.0267)
Water currently treated with Clorin?	0.0074	-0.0122	-0.0092	-0.0121
(baseline)	(0.0302)	(0.0267)	(0.0338)	(0.0338)
Drinking water contains free chlorine?	0.0269	0.0152	-0.0057	-0.0057
(baseline)	(0.0242)	(0.0214)	(0.0274)	(0.0273)
Use of soap before handling food	0.0355	-0.0131	0.0004	-0.0010
(index)	(0.0420)	(0.0371)	(0.0487)	(0.0487)
Use of soap after using toilet	-0.0268	-0.0069	-0.0231	-0.0220
(index)	(0.0434)	(0.0381)	(0.0496)	(0.0495)
Attitude toward water purification	0.0508	0.0965	0.0838	0.0852
(index)	(0.0613)	(0.0541)	(0.0690)	(0.0689)
Age in years	0.0016	0.0022	0.0034	0.0035
	(0.0013)	(0.0011)	(0.0015)	(0.0015)
Ever attended school?	-0.0063	-0.0224	-0.0035	-0.0068
	(0.0498)	(0.0438)	(0.0556)	(0.0557)
Years of completed schooling	-0.0052	0.0028	0.0078	0.0081
	(0.0051)	(0.0046)	(0.0059)	(0.0059)
Currently married?	0.0317	0.0214	0.0811	0.0789
	(0.0314)	(0.0283)	(0.0364)	(0.0364)
Currently pregnant?	-0.0085	0.0215	-0.0410	-0.0412
	(0.0369)	(0.0331)	(0.0430)	(0.0429)
Every given birth to any children?	-0.0447	-0.0033	-0.0242	-0.0215
	(0.0490)	(0.0441)	(0.0563)	(0.0563)
No. of children in household under age 5	0.0133	0.0044	-0.0105	-0.0112
	(0.0145)	(0.0130)	(0.0178)	(0.0178)
No. of people in household	0.0074	0.0109	0.0121	0.0120
	(0.0053)	(0.0046)	(0.0058)	(0.0058)
Share of durables owned	0.1763	0.0638	0.0020	0.0033
	(0.0784)	(0.0697)	(0.0870)	(0.0869)
Locality fixed effects?	YES	YES	YES	YES
Fixed effects for offer price?	NO	NO	YES	YES
Fixed effects for transaction price?	NO	YES	NO	NO
F-test that all coefficients are 0	2.05	1.61	1.50	1.50
p-value of F-test	0.0060	0.0512	0.0837	0.0833
Number of observations	1244	990	605	605

Notes: Standard errors in parentheses. "Marketing" refers to households reached for door-to-door marketing. "Purchased Clorin" refers to households that purchased Clorin during door-to-door marketing. All variables measured as of baseline survey. Offer price and transaction price variables excluded from F-tests.

Table 5 *Estimates of the demand for Clorin*

Dependent variable: Household purchased Clorin (dummy)

	(1)	(2)	(3)	(4)	(5)
Sample	All	All	All	Follow-up	All
Offer price	-0.0664	-0.0653	-0.0656	-0.0708	-0.0704
(100 Kw)	(0.0093)	(0.0094)	(0.0091)	(0.0099)	(0.0097)
Transaction price					0.0128
(100 Kw)					(0.0089)
Constant	0.9640	—	—	0.9892	0.9646
	(0.0516)			(0.0547)	(0.0516)
Baseline controls?	NO	YES	NO	NO	NO
Fixed effects for marketer?	NO	NO	YES	NO	NO
R^2	0.0484	0.0733	0.0954	0.0548	0.0504
Number of observations	1004	990	1004	890	1004

Notes: Standard errors are in parentheses. Estimates are from linear probability models. "Baseline controls" includes baseline Clorin usage and water chlorination, general health behaviors and attitudes, household demographics, and locality fixed effects, as in table 3.

Table 6 *The effect of offer price on the usage rates of buyers*

Panel A: Screening on subsequent use of Clorin

Dependent variable	(1A) Water currently treated with Clorin? (follow-up; self-reported)	(2A) Drinking water contains free chlorine? (follow-up; measured)
Offer price (100 Kw)	0.0373 (0.0149)	0.0321 (0.0150)
Transaction price fixed effects?	YES	YES
R^2	0.0254	0.0191
Number of observations	546	542

Panel B: Screening conditional on baseline demographics

Dependent variable	(1B) Water currently treated with Clorin? (follow-up; self-reported)	(2B) Drinking water contains free chlorine? (follow-up; measured)
Offer price (100 Kw)	0.0327 (0.0150)	0.0293 (0.0149)
Transaction price fixed effects?	YES	YES
Baseline demographics?	YES	YES
R^2	0.0596	0.0827
Number of observations	537	533

Panel C: Screening conditional on baseline usage

Dependent variable	(1C) Water currently treated with Clorin? (follow-up; self-reported)	(2C) Drinking water contains free chlorine? (follow-up; measured)
Offer price (100 Kw)	0.0342 (0.0147)	0.0323 (0.0149)
Transaction price fixed effects?	YES	YES
Baseline usage?	YES	YES
R^2	0.0534	0.0362
Number of observations	546	542

Notes: Standard errors are in parentheses. Estimates are from linear probability models with fixed effects for transaction price, estimated on the sample of households who purchased Clorin in the door-to-door marketing intervention and who were reached for the follow-up survey. "Baseline demographics" includes measures of age, schooling, marital status, pregnancy, household composition, wealth, and locality fixed effects, as in table 3. "Baseline usage" includes baseline Clorin usage and water chlorination.

Table 7 *The effect of offer price on the baseline characteristics of buyers*

	Dependent variable	Effect of offer price (100Kw)	N
Panel A: Screening on baseline use of Clorin			
(1)	Water currently treated with Clorin? (self-reported)	0.0137 (0.0105)	614
(2)	Drinking water contains free chlorine? (measured)	0.0095 (0.0124)	614
(3)	Predicted self-reported use in follow-up	0.0056 (0.0027)	600
Panel B: Screening on household wealth and education			
(6)	Share of durables owned	0.0016 (0.0043)	614
(7)	Ever attended school?	0.0007 (0.0088)	614
(8)	Years of completed schooling (index)	0.0701 (0.0926)	613
Panel C: Screening on health benefits of Clorin			
(9)	Number of children under age 5	0.0129 (0.0203)	614
(10)	Any child under 5 had diarrhea in last two weeks?	0.0001 (0.0109)	480
(11)	Respondent is pregnant?	0.0009 (0.0078)	614

Notes: Standard errors in parentheses. Estimates are from OLS regressions with fixed effects for transaction price, estimated on the sample of households who purchased Clorin in the door-to-door marketing intervention. All dependent variables measured as of the baseline survey. Predicted self-reported use in follow-up is predicted by regressing use in follow-up survey on dummies for self-reported Clorin use and measured chlorination, as well as an index of the recency of the last use of Clorin and the number of bottles of Clorin in the household, all estimated as of the baseline survey.

Table 8 *The effect of transaction price on the usage rate of buyers*

Panel A: No controls

	(1A)	(2A)	(3A)	(4A)
Dependent variable	Water currently treated with Clorin? (follow-up; self-reported)		Drinking water contains free chlorine? (follow-up; measured)	
Transaction price (100 Kw)	0.0197 (0.0134)		0.0024 (0.0135)	
Transaction price > 0		0.0935 (0.0440)		0.0587 (0.0444)
Offer price fixed effects?	YES	YES	YES	YES
R^2	0.0211	0.0253	0.0086	0.0118
No. of observations	546	546	542	542

Panel B: Baseline controls

	(1B)	(2B)	(3B)	(4B)
Dependent variable	Water currently treated with Clorin? (follow-up; self-reported)		Drinking water contains free chlorine? (follow-up; measured)	
Transaction price (100 Kw)	0.0097 (0.0133)		-0.0071 (0.0133)	
Transaction price > 0		0.0565 (0.0442)		0.0318 (0.0440)
Offer price fixed effects?	YES	YES	YES	YES
Baseline controls?	YES	YES	YES	YES
R^2	0.1027	0.1046	0.1130	0.1134
No. of observations	537	537	533	533

Notes: Standard errors are in parentheses. Estimates are from linear probability models with fixed effects for offer price, estimated on the sample of households who purchased Clorin in the door-to-door marketing intervention and who were reached for the follow-up survey. "Baseline controls" includes baseline Clorin usage and water chlorination, general health behaviors and attitudes, household demographics, and locality fixed effects, as in table 3.

Table 9 *Mechanisms for the causal effect of prices*

Independent variable: Transaction price > 0

	Sample	Water currently treated with Clorin? (follow-up; self-reported)	*N*	Drinking water contains free chlorine? (follow-up; measured)	*N*
Does respondent display the sunk cost effect in hypothetical choices?					
(1)	Yes	0.1840	113	0.0816	112
		(0.1002)		(0.1027)	
(2)	No	0.0372	424	0.0240	421
		(0.0499)		(0.0492)	
Difference		0.1468		0.0576	
		(0.1144)		(0.1133)	
Does respondent strongly agree that "I value something more if I paid for it"?					
(3)	Yes	0.1263	286	0.0525	283
		(0.0625)		(0.0636)	
(4)	No	0.0125	251	0.0003	250
		(0.0656)		(0.0640)	
Difference		0.1138		0.0522	
		(0.0905)		(0.0903)	
Is respondent married?					
(5)	Yes	0.1020	444	0.0607	442
		(0.0484)		(0.0487)	
(6)	No	-0.1458	93	-0.1538	91
		(0.1152)		(0.1111)	
Difference		0.2479		0.2146	
		(0.1254)		(0.1252)	

Notes: Standard errors are in parentheses. Estimates are from linear probability models with fixed effects for offer price, estimated on the sample of households who purchased Clorin in the door-to-door marketing intervention, who were reached for the follow-up survey, and who fall into the specified categories. All specifications include controls for baseline Clorin usage and water chlorination, general health behaviors and attitudes, household demographics, and locality fixed effects, as in table 3.

Table 10 *Longer-term effects of prices on Clorin use*

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	Water currently treated with Clorin? (2nd follow-up; self-reported)			Drinking water contains more free chlorine than source water? (2nd follow-up; measured)		
Offer price	-0.0105			-0.0135		
(100 Kw)	(0.0145)			(0.0140)		
Transaction price		-0.0007			0.0109	
(100 Kw)		(0.0124)			(0.0121)	
Transaction price > 0			0.0582			0.0852
			(0.0420)			(0.0409)
Transaction price fixed effects?	YES	NO	NO	YES	NO	NO
Offer price fixed effects?	NO	YES	YES	NO	YES	YES
Baseline controls?	NO	YES	YES	NO	YES	YES
R^2	0.0146	0.1214	0.1249	0.0190	0.1136	0.1200
Number of observations	513	506	506	513	506	506

Notes: Standard errors are in parentheses. Estimates are from linear probability models estimated on the sample of households who purchased Clorin in the door-to-door marketing intervention and who were reached for the second follow-up survey. "Baseline controls" includes baseline Clorin usage and water chlorination, general health behaviors and attitudes, household demographics, and locality fixed effects, as in table 3.