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Can Higher Prices Stimulate Product Use?
Evidence from a Field Experiment in Zambia

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Abstract

The controversy over whether and how much to charge for health products in the developing world rests, in part, on whether higher prices can increase use. We test this hypothesis in a field experiment in Zambia using door-to-door marketing of a home water purification solution. Our methodology separates the *screening* effect of prices (charging more changes the mix of buyers) from the *psychological* effect of prices (charging more stimulates greater use for a given buyer). We find that higher prices screen out those who use the product less. The amount paid does not have a psychological effect on use, but there is some evidence that the act of paying increases use. We use our data to estimate an economic model of product use, simulate counterfactuals, and develop tentative implications for pricing policy.

JEL classification: C93, D12, L11, L31

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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).[1] In general, 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).

The latter argument—that higher prices promote use—is of particular relevance in the area of health, since use is an essential prerequisite to reaping the benefits of most health products. From the standpoint of economic theory, there are two reasons why charging more for a product could lead to greater use. First, consistent with the economic theory of program targeting, higher prices could screen out those who do not plan to use the product (Oster, 1995). Second, following psychological theory on the "sunk-cost" effect, paying more for something may actually encourage subsequent use (Thaler, 1980).[2] Separating these two effects—which we will refer to, respectively, as the *screening* and *psychological* effects of prices—is important for pricing policy. In particular, while the screening effect can only cause use *among buyers* to increase with prices, the psychological effect can, in principle, cause *total use* to increase with the price. As a result, the psychological effect may be a relevant consideration for policy even if resources are unlimited and waste is not costly.

Despite their importance both to the economic theory of product pricing and to policy issues,

[1] 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).

[2] A third possibility is that prices convey information about the quality of the product (Milgrom and Roberts, 1986). We do not list this mechanism above, despite its potential relevance to pricing policy, because (as we argue in greater detail below) it is not likely to be a major force in our study, which focuses on a relatively well-known product.

separating these two effects has been hampered by the fact that both predict that as prices rise, buyers will use more. As a result, evidence on sunk-cost effects has largely been confined to hypothetical choices and a single, small-scale field experiment (Arkes and Blumer, 1985). Moreover, despite the fact that empirical selection into behaviors plays a central role in a wide range of economic models since Roy (1951), clean evidence for selection based on prices has been relatively scarce, with recent field experiments finding only weak evidence for selection (Karlan and Zinman, 2006).

In this paper, we present evidence from a large-scale field experiment in Lusaka, Zambia involving Clorin, an inexpensive, socially marketed disinfectant. Our experimental design allows us to separately identify the screening and psychological effects of prices, and our setting allows us to measure product use objectively, without relying solely on household self-reports. In contrast to previous studies, we find strong evidence for screening effects. Moreover, we find that prices are a useful screening tool even when rich data on household characteristics are available. Turning to psychological effects, we find no evidence that, conditional on paying a price, higher prices induce greater product use, though our findings are consistent with an effect of the act of paying itself. After presenting our findings, we estimate an explicit economic model of product use and use it to develop tentative implications for pricing policy.

Clorin is well-suited to the goals of our study. It is a chlorine bleach solution used to kill pathogens in household drinking water, and thus reduce the incidence of water-borne illnesses (Quick et al, 2002). Its chemical composition makes it detectable by test strips similar to those used in backyard pools, which permits us to avoid the pitfalls of relying solely on household self-reports of use. Moreover, it is a well-known, widely used product with an established retail market, which serves to limit the informational role of prices, a potential confound to the effects of interest. Finally, it is inexpensive, so that income effects (another potential confound) are relatively unlikely.

Our main experimental intervention was a door-to-door sale of Clorin to about 1,000 households in Lusaka. Each participating household was offered a single bottle of Clorin for a one-time only, randomly chosen *offer price*, which was at or below the prevailing retail price. Households that agreed to purchase at the offer price received an unanticipated, randomly chosen discount, thus allowing us to vary the *transaction price* separately from the offer price. About two weeks 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.

Under a set of conditions that we lay out explicitly (and defend) in the paper, this two-stage pricing design allows us to separately estimate the screening and psychological effects of prices on Clorin use. Varying the offer price for a given transaction price allows us to identify the screening effect of prices on the mix of buyers. Varying the transaction price for a given offer price then identifies the psychological effect of prices, holding constant the selection of buyers.

We find strong evidence for screening effects: holding constant the transaction price, the likelihood of use is (statistically and economically) significantly higher among those who agree to a larger offer price. That is, higher willigness-to-pay for Clorin is associated with a greater propensity to use. This holds true even when we condition on a range of household characteristics, suggesting that the component of willingness-to-pay that is uncorrelated with observables is nevertheless highly predictive of Clorin use. In addition, some simple calculations suggest that willingness-to-pay is more predictive of use than an optimal linear combination of household characteristics observable as of the baseline survey.

Our evidence on the psychological effect of prices is more mixed. We find no evidence that the amount of the transaction price influences Clorin use, even among households displaying the sunk-cost effect in hypothetical choice scenarios. Our evidence is, however, consistent with the hypothesis that paying something results in more use than paying nothing. Although this latter finding is not consistently statistically significant, it is stronger among households displaying the sunk-cost effect in hypothetical choices and persists in a longer-term follow-up survey, both of which suggest the possibility of a real effect.

To allow us to make tentative statements about the policy implications of our data, we estimate an explicit economic model of Clorin purchase and use, in which we allow for both screening and psychological effects. If our (statistically insignificant) point estimate of the psychological effect of the act of paying on use is correct, our model implies that the use-maximizing price is positive but small, and that a nontrivial price can be charged with no loss in use relative to giving Clorin away for free. Even absent psychological effects, our model implies that the screening effects we estimate are large enough to matter for policy: to maintain the current level of Clorin use in the *absence* of screening effects, the NGO would need to reduce the price of Clorin by roughly one-half. We stress, however, that these conclusions may be sensitive to the parametric assumptions of our structural model.

A more general caveat is that our model predicts Clorin use, which is a means to an end (health) rather than an end in itself. While we cannot measure health outcomes directly, there is strong extant evidence that home water purification solutions like Clorin can reduce the incidence of water-borne illnesses (Quick et al, 2002), even in populations in which the use of alternative methods (such as boiling) is reasonably common (Quick et al, 1999). Understanding the determinants of Clorin use is therefore likely to be an important part of designing policy to reduce the incidence of water-borne illnesses.

Our paper makes several contributions to the existing literature. The first is an experimental methodology for separating the screening and psychological effects of prices.[3] Our two-stage pricing design is a close cousin both to Arkes and Blumer's (1985) study of the sunk-cost effect in the use of theater tickets, and to Karlan and Zinman's (2006) study of adverse selection and moral hazard in the South African loan market. However, some important differences are worth noting. Arkes and Blumer's (1985) design does not identify either the screening effect or the effect of a zero transaction price.[4] Karlan and Zinman's (2006) study is closer in employing a two-stage pricing design, but in a financial market (rather than product market) context. In addition, in Karlan and Zinman's (2006) study the discounted interest rate (analogous to our transaction price) affects households' marginal incentives to default (through moral hazard or repayment burden), whereas in our context the transaction price is purely sunk, and can therefore affect product use only through psychological channels such as the sunk-cost effect.[5]

The second main contribution of our study is to show direct evidence that changing the price of a product affects the selection of buyers. While this proposition is in some sense uncontroversial, the

[3] See Harrison and List (2004) for a review of field experiments in economics more generally.

[4] Because the finding of sunk-cost effects in Arkes and Blumer's (1985) relies on comparing those who received a discount to those who did not, their design also partially confounds the effect of receiving a discount with the effect of the transaction price. By contrast, all households participating in our experiment receive at least some discount, so that none of our estimates can be interpreted as the effect of receiving a discount.

[5] In principle, variation in the transaction price could also affect use through strategic channels (Prendergast and Stole, 1996), if the purchaser of Clorin feels the need to justify the purchase price to other members of the household (Ashraf, 2005). We provide a crude test of this explanation in the paper, and find that it is partially consistent with our evidence.

most closely related existing study (Karlan and Zinman, 2006) finds only weak evidence for selection. More importantly, our data show that much of the selection induced by prices is on unobservables—that is, on dimensions of heterogeneity not related to observable household characteristics. This is not an obvious implication of economic theory, and it may have important ramifications for public policy. In particular, this finding suggests that allocation mechanisms (e.g., prices) that exploit the endogenous sorting of households may outperform approaches that allocate goods and services based solely on measurable household characteristics.

We also contribute to the study of the psychological effects of prices on product use.[6] While evidence from hypothetical choices supports the sunk-cost premise (Thaler, 1980; Arkes and Blumer, 1985), evidence from incentivized laboratory behaviors is more mixed (Friedman et al, 2004). Eyster's (2002) review identifies only one existing field experiment on sunk costs (Arkes and Blumer, 1985), making our study only the second and by far the largest field experiment on sunk-cost effects. Ours is also the first field study of sunk costs to include a treatment in which participants paid nothing for the product,[7] and the first to explicitly connect hypothetical choice responses and other measures of psychological propensity to objectively measured field behaviors.[8] Our findings may also suggest a need to incorporate a distinction between the amount paid and the act of paying into the psychology of sunk costs.

Beyond its implications for social science, our study informs an important set of public policy issues. First, it relates to a growing literature on the use of "point-of-use" water purification systems, which hold promise as a tool for addressing the lack of clean water facing over one billion people (USAID, 2006; Thevos et al, 2002-2003; Kremer et al, 2006). Second, our finding that higher prices help to target distribution to households with a greater propensity to use the product may serve as a partial counterpoint to existing studies (e.g., Kremer and Miguel, forthcoming) showing undesirable negative effects of prices on the take-up of health products.[9] Our evidence is likely

[6] More generally, our evidence contributes to existing research on the psychology of product pricing (see, e.g., Gourville and Soman, 2002; Shiv, Carmon and Ariely, 2005).

[7] 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).

[8] In this sense, 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).

[9] In this sense, 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).

to have special relevance in contexts in which the effectiveness of health care or health products depends on household behavior (Grossman, 1972).[10]

The determinants of product use play an important role in many industrial organization contexts.[11] For example, utilization is of intrinsic public policy interest in the market for energy-intensive consumer durables (Hausman, 1979). Product use directly affects profits in sectors such as entertainment and media, where advertisers may care not only about the number of subscribers but also the intensity with which they use the product (Kalita and Ducoffe, 1995; Petrin ,2003).[12] Our methods may be useful in identifying the relationship between pricing and utilization in such markets.

The remainder of the paper is organized as follows. Section 2 provides background information on our experimental setting. Section 3 lays out a formal model of Clorin use and discusses the conditions needed for identification. Section 4 describes the design of our surveys and door-to-door marketing experiment. Sections 5 and 6 present our findings on the effect of price changes on product purchase and use. Section 7 describes a series of robustness checks on our key conditions. Section 8 discusses estimation of a model of Clorin use and implications for pricing policy. Section 9 concludes.

2 Experimental Setting: Zambia, Safe Water, and Clorin

Clorin is a water purification solution that is marketed in Zambia by the Society for Family Health (SFH), a local affiliate of Population Services International (PSI), an international non-profit organization.[13] 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

[10] Though there have been some studies of the effectiveness of prices in encouraging product use in social marketing contexts, existing research typically takes a non-experimental approach (Meekers, 1997; Maxwell et al, 2006). 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.

[11] In this sense, our study also relates to a growing literature on "behavioral industrial organization" which seeks to integrate insights from psychology into the study of firm-consumer interactions (DellaVigna and Malmendier, 2004; Gabaix and Laibson, 2004; Spiegler, 2004 and 2005; Eliaz and Spiegler, 2004; Heidhues and Köszegi, 2004; Bertrand et al, 2005).

[12] Free newspapers, for example, typically sell advertising space at a significantly lower price per copy "sold" than more traditional papers, a phenomenon that seems likely to be related to the rate of readership (Blair and Romano, 1993).

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

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).[14] These prices are modest by Zambian standards; for comparison, in Lusaka, a week's supply of cooking oil for a family of six costs about 4,800 Kw.[15] The fact that Clorin is a relatively inexpensive product limits the possibility that wealth effects contaminate our estimates.[16]

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, and over 99 percent mention Clorin when asked which water purification solutions they have heard of. Informal interviews and focus groups further suggest high levels of awareness of Clorin prices. These facts, combined with additional precautions described 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

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

[15] As of June 1, 2006, 800 Kw was equivalent to about \$0.25 US. Average monthly urban household income in Zambia in 2002-2003 was 790,652 Kw (United Nations Economic Commission for Africa, 2006).

[16] See section 7 of the analysis for additional checks on the possibility of contamination from wealth effects.

psychological effects that are the focus of our study.[17]

3 Conceptual Framework

In this section, we present a simple and fairly general model of households' decisions regarding the purchase and use of Clorin. The model will serve to illustrate our key hypotheses, and to make clear how our experimental design allows us to test them separately.

We assume that each household i has a willingness-to-pay WTP_i for a bottle of Clorin. The parameter WTP_i is distributed in the population according to a cdf $F()$ on an interval $[0, v]$. At a given price p, a household will buy Clorin if and only if $WTP_i \geq p$, so that the probability of purchase can be written as

$$\Pr(buy_i \mid p) = 1 - F(p). \tag{1}$$

This equation embeds our first, and most obvious hypothesis, namely that fewer households will purchase Clorin the higher is its price.

If a household buys Clorin, its utility U_i from using it is given by

$$U_i = \alpha + \beta WTP_i + h(p) - \varepsilon_i. \tag{2}$$

Here, α is an intercept, and β captures the relationship between a household's willingness-to-pay and its propensity to use Clorin. Incorporating WTP_i into the utility of use in this way can be thought of as a reduced-form of a model in which use intentions impact the decision of whether or not to purchase Clorin.[18] When $\beta > 0$, higher-willingness-to-pay households are more likely to use Clorin if they receive it, resulting in a screening effect of prices.

The function $h(p)$ captures the psychological effect of prices on product use. The sunk-cost hypothesis holds that $h(p)$ is increasing in p. Later, we will test separately for an effect of the act of paying ($h(p)$ "jumps" discontinuously from $p = 0$ to $p > 0$) and an effect of the amount paid

[17] In section 7, we report direct evidence that participants' attitudes toward Clorin and beliefs about Clorin's market price did not change in response to the prices we charged.

[18] See Hausman (1979), Dubin and McFadden (1984), Mannering and Winston (1985), and Petrin (2003) for examples of more structured approaches to models of purchase and use.

($h(p)$ is increasing in p for $p > 0$).[19]

We assume an outside option of 0, so that a household uses Clorin if and only if $U_i \geq 0$. We assume that ε_i is an iid shock independent of WTP_i, distributed according to cdf $G()$. This means that the probability of using Clorin for a given household i is given by

$$\Pr(use_i \mid buy_i, WTP_i) = G(\alpha + \beta WTP_i + h(p)). \tag{3}$$

Because the households that buy Clorin at a given price p must have willingness-to-pay that exceeds p, average use among buyers is given by

$$\Pr(use_i \mid buy_i, p) = E[G(\alpha + \beta WTP_i + h(p)) \mid WTP_i \geq p]. \tag{4}$$

Equation (4) illustrates the two ways in which changing the price of Clorin could affect the share of buyers who use the product. First, higher prices may increase use directly, through the psychological effects modeled in the function $h(p)$. Second, higher prices will induce low-willingness-to-pay households to select out of the pool of buyers, which, if $\beta > 0$, results in higher average use among the households that continue to buy.

With exogenous price variation and data on WTP_i for each household, it would be straightforward to separate these two effects by directly estimating equation (3). Differences in use among households with different willingness-to-pay who purchase at the same price would identify the screening parameter β, and differences in households with the same willingness-to-pay, but paying different prices, would identify the psychological effect function $h(p)$. In a typical market setting, however, data on each household's willingness-to-pay are not readily available; rather, willingness-to-pay must be inferred from purchase decisions. This means that to estimate the effect of WTP_i on use, the researcher must look at how the rate of use among buyers changes as prices vary. But this same price variation must also be used to identify the psychological effects of prices on product use. As a result, the screening and psychological effects are not separately identified in observational data on prices and product use.

[19] Note that we assume that prices do not influence use through an income or wealth effect. As we argue in section 2 above, Clorin is sufficiently inexpensive that such effects should be small. See also section 7 for additional evidence that income effects do not play an important role in our analysis.

This fundamental identification problem motivates our experimental design (described in more detail in section 4.3 below). In our experiment, we first offer to sell a bottle of Clorin for a (randomly assigned) offer price η. If a household agrees to purchase at that price, we then (randomly) assign a discount, so that we can vary the transaction price τ separately from the offer price. Under some conditions, separate variation in η and τ make it possible to recover both β and the function $h(\bullet)$ over the range of experimental variation. In particular, we suppose (i) that the offer price η (and not the transaction price) governs a household's decision to buy Clorin, (ii) that the psychological impetus to use Clorin depends only on the price τ at which the household ultimately transacts, and (iii) that separating the offer and transaction prices (as opposed to charging a single price) does not itself induce a change in use behavior.

Under conditions (i) to (iii) we can rewrite equation (4) as follows:

$$\Pr(use_i \mid buy_i, \eta, \tau) = E\left[G\left(\alpha + \beta WTP_i + h(\tau)\right) \mid WTP_i \geq \eta\right]. \tag{5}$$

This equation illustrates that variation in the offer price η (for a given transaction price τ) will identify the relationship between use and willingness-to-pay (captured in β), and that variation in the transaction price τ (for a given offer price η) will identify the psychological effect $h(\bullet)$.

In section 4.4 below, we discuss the aspects of our design that serve to maintain the plausibility of our key conditions (as well as the implicit assumption that experimental participants do not learn about the product or its price from the prices in the experiment). In section 7, which follows our analysis, we present the results of a series of falsification tests for these conditions.

4 Experimental and Survey Design

Our study consisted of a baseline survey, a randomized door-to-door marketing intervention approximately two weeks later, a follow-up survey approximately two weeks after the intervention, and a second, longer-term follow-up survey.[20]

[20] Our three survey instruments, and our marketing script, are available as a supplemental appendix to this paper.

4.1 Baseline Survey Procedures and Sample Selection

We fielded our baseline survey to $1,260$ households in Lusaka, Zambia in May, 2006. To select households, we first selected five low-income peri-urban areas ("compounds").[21] Because we wanted to sample a population whose water source had limited chlorination (to maximize the health benefits of Clorin), 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. 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.[22] To minimize our influence on participants' behavior, our baseline survey instrument informed participants that we might return for a follow-up interview, but it did not specify the time or nature of that interview, nor did it state that such an interview was certain to occur.

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. 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.

Appendix table 1 compares average demographic characteristics of the households in our baseline sample to Lusaka residents sampled in the 2001 Demographic and Health Survey (DHS) of Zambia.[23] The characteristics are broadly comparable between the two samples. Because we interviewed the female head of household, our respondents tend to be slightly older and more likely

[21] 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.

[22] 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.

[23] 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.

to be married than the DHS respondents. The households in our baseline sample also have slightly lower levels of durables ownership than those in the DHS data, probably because of our insistence on sampling low-income compounds without access to the main Lusaka water line.

4.2 Measuring Clorin Use and Water Chlorination

Our primary survey measure of Clorin use is the household's (yes or no) response to whether its stored drinking water is currently treated with Clorin. We complement this subjective measure with an objective estimate of the chemical concentration of chlorine in the household's drinking water. In the last part of the interview, the surveyor put a small amount of household drinking water (usually stored in a large plastic jug) 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,[24] which tests for both free chlorine radicals (chlorine available to kill pathogens) and total chlorine (free chlorine plus chloramines, a by-product of chlorine combining with organic compounds).[25] We focus on free chlorine, because our own experimentation, as well as conversations with the manufacturer, suggest that the free chlorine measurement is more reliable and less sensitive to variation in test conditions (such as light and heat) than measurement of total chlorine.[26] The test strip identifies seven possible concentrations of free chlorine: 0, 0.1, 0.2, 0.5, 1, 2.5, and 5 parts per million.[27]

It is worth noting that chlorination and Clorin use are not identical concepts, even though they are closely related.[28] A household could have chlorine in its water without using Clorin: water from some taps is (often inconsistently) chlorinated. And, if a household's drinking water is highly contaminated to start out, then it is possible to use a low dose of Clorin without leaving any de-

[24] 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.

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

[26] Using total chlorine in place of free chlorine in our analysis results in stronger evidence of a screening effect, and less evidence of a psychological effect.

[27] For reference, 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.

[28] It is also important to stress that when we refer to Clorin use we mean use *in drinking water*. In our follow-up survey, about one-fifth of households report using Clorin for non-drinking-water purposes (mostly for washing clothes). We focus on use in drinking water because it is the primary way in which households obtain health benefits from Clorin.

tectable free chlorine residual in the water. Nevertheless, as expected, measured chlorination is highly related to self-reported use of Clorin, and a Pearson χ^2 test definitively rejects the independence of the two distributions ($p - value < 0.001$). 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. Indeed, levels of free chlorine of 2.5 and 5 parts per million are only found in households that report that their water is treated with Clorin. In order to limit sensitivity to these rare outliers, we follow Parker et al. (2006) in using in our analysis a binary measure of the presence of free chlorine (free chlorine levels of 0.1 parts per million or greater).[29] In the baseline survey, 41 percent of the households have at least 0.1 parts per million of free chlorine in their water.

4.3 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.[30] 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.[31] Marketers identified themselves as representatives of SFH.

After making contact with the female head of household, the marketers followed a written script.[32] 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 1 for exact proportions.) The marketing

[29] Our substantive conclusions are unchanged (estimates are identical in direction and statistical significance) when we instead estimate ordered probit models using the level of free chlorine as the dependent variable. See appendix A for details.

[30] Marketers were paid on a fixed rate per day worked.

[31] 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.

[32] In principle, marketers' tone or body language could have differed with the offer price, confounding our estimates of treatment effects. During training exercises, and during a small number of supervised transactions, we observed no indications of variation in body language or tone related to offer prices. Marketers commonly did not look at the offer price before beginning the script. All our key results are robust to marketer fixed effects, and our data show no evidence of differential treatment effects by marketer (see appendix A).

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.

If the respondent agreed to buy at the initial offer price, the marketer informed her that she might be eligible for an additional discount.[33] The respondent was given a sealed envelope, which contained a coupon offering a one-time discounted price 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, wrote the amount of the transaction price on a receipt, and signed it.[34] After that, the marketing session ended. Use of a receipt allowed us to check that the marketers had complied with the instructions, and provided an additional incentive for them to do so.[35]

The size of the discount was chosen randomly, but every household received a discount of at least 100 Kw, allowing us to hold constant any effect of receipt of a discount on use. 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 1 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,

[33] 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. Our findings are robust to excluding households that requested a return visit due to a lack of cash on hand.

[34] None of the participants who were prepared to pay the initial offer price subsequently refused to buy at the discounted transaction price.

[35] Hand-checking of these receipts confirmed that different receipts from the same marketer were in different handwriting, providing further evidence of the integrity of the marketing process. In four cases, the marketer transacted at a price other than the one we specified due to human error, and in one case the offer price was incorrect. In these cases, we will use the intended prices rather than the actual prices for the purposes of our analysis, to ensure that these errors do not contaminate our findings. We note, however, that this choice does not meaningfully affect our results.

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.[36] At the time of randomization we used an F-test to verify that observable characteristics were balanced across treatments, and, in a few cases, re-randomized when this was not the case.[37]

Appendix table 2 presents regressions of treatment conditions on a range of household characteristics measured in the baseline survey, with specifications that parallel our analysis of price effects. In all cases, an F-test of the restriction that all covariates enter with a coefficient of zero fails to reject at any conventional significance level, and the coefficients are generally individually statistically insignificant. Two exceptions are worth noting. First, among households reached during marketing, baseline self-reported Clorin use is almost marginally statistically significantly related to the offer price ($p = 0.103$). Second, among those who purchased Clorin in the marketing phase, there is a statistically significant relationship between the transaction price and the chemical presence of free chlorine in the baseline ($p = 0.020$), although the relationship with self-reported use is insignificant and has the opposite sign. (A dummy for whether the household paid a positive transaction price is positively but not statistically significantly related to either self-reported Clorin use or measured chlorination.) We pay special attention in our analysis to the effects of controls for baseline chlorination levels and Clorin use because of the possible lack of balance on these variables.[38]

[36] We made an effort to reach all households in a given compound within a short period, so as to minimize communication between households about the price randomization. Debriefing interviews after a pilot experiment suggest that communication about the discounts was rare. See section 7 for further evidence that our price manipulations did not have social spillover effects.

[37] We conducted these balancing tests, separately by compound, on the sample of households surveyed in the baseline. We could not conduct analogous tests for Clorin buyers, because we could not predict which households would be reached for our marketing intervention, or which households would purchase Clorin.

[38] In the full sample, using soap after using the toilet (self-reported) is marginally statistically significantly negatively related to the offer price ($p = 0.054$). Among buyers, an indicator for the female head of household having attended school is marginally statistically significantly positively related to the positive price condition ($p = 0.083$), though our measure of years of schooling is marginally significantly negatively related to the positive price condition ($p = 0.087$), suggesting no consistently signed relationship with schooling levels.

4.4 Interpretation of Marketing Intervention

As we discuss in section 3 above, in order to use variation in offer and transaction prices to separately identify the screening and psychological effects of prices, we require a few key conditions. First, we require that households' purchase decisions are based only on the offer price, and not on the transaction price (condition (i) of section 3). Because we put the coupons in sealed envelopes and did not tell the marketers what value was on each household's coupon, our procedures prevented marketers from communicating (directly or indirectly) the transaction price to households.

The second important condition is that any psychological effects of prices on use work through the transaction price, rather than the agreed-upon offer price, or the discount between the offer and transaction prices (condition (ii) of section 3). 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 appendix figure 1). Finally, asking respondents to write the final transaction price on a receipt and sign it served to highlight the transaction price.

The next condition is that participants behave in response to the two-price structure as they would in response to a more standard offer with a single price (condition (iii) of section 3). We attempted to make the two-price structure seem as natural as possible. 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 giveaways) are not unheard of for products like Clorin, and participants seemed to accept this explanation. After we explained that the initial offer price was a promotional price, participants rarely questioned the reason for the discounted transaction price.

The final key condition is that variation in the offer and transaction prices does not affect households' beliefs about the market price of Clorin, the quality of the Clorin we offered, or the quality of Clorin in general. To minimize inference about prices, the marketing script explicitly told respondents that Clorin was available in retail outlets for around 800 Kw.[39] Marketers introduced themselves as official representatives of SFH, the highly credible organization that produces, distributes, and markets Clorin throughout Zambia. This served to reduce the likelihood that participants would feel that the Clorin being offered was atypical or sub-par.[40] Finally, because (as noted in section 2 above) nearly all respondents had heard of Clorin and the vast majority had used it at some point, we can expect comparatively limited price-based inference about the quality of Clorin in general.

These design elements serve to maintain the plausibility of the conditions necessary for identification. In section 7, we present additional evidence on the plausibility of these conditions in our context.

4.5 Follow-up Survey

For our follow-up survey, we sent the original survey teams to find and re-interview the households that we successfully reached for the marketing intervention.[41] We re-interviewed households approximately two weeks after the marketing intervention, but actual lags varied due to logistical factors.[42] We chose the timing of this survey to fall in the middle of the period during which households would be using the bottle of Clorin we sold them.[43]

The follow-up interview consisted of several sections. First, we repeated a handful of demo-

[39] Early pilot interviews suggested that most people in Lusaka are well aware of these prices.

[40] Because surveyors introduced themselves as carrying out a health survey for a researcher at Harvard University, having marketers identify themselves as representatives of SFH also provides greater confidence that behavior in response to the marketing intervention is not driven by the belief that the experimental participants are "being watched" (Levitt and List, 2006).

[41] 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. Note, however, that we interviewed households reached in our marketing intervention whether or not they purchased Clorin from our marketing team.

[42] 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. This occurred in 58 cases, and our findings are not substantively different when we restrict attention to the cases in which we successfully reinterviewed the original respondent.

[43] To confirm this expectation, we 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 the 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. Among households in which the surveyors identified the bottle we sold, in the vast majority of cases (nearly 80 percent) the bottle was partly, but not completely, full.

graphic questions from the baseline survey, as a check on the identity of the respondents.[44] 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. 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 (out of the 1,004 households that were successfully reached during the marketing phase). Appendix table 3 presents some evidence on the determinants of attrition. In the marketing phase, we were more successful in reaching households that owned a larger share of the set of durables goods (car, radio, television) that we asked about, most likely because wealthier households tended to be in more developed sections of the compounds and were therefore easier to locate.[45] In the follow-up survey, attrition was still related to observables, though less so than in the marketing phase. Most importantly, at no stage are the offer and transaction prices related to the likelihood of attrition. This provides some reason to believe that our experimental results are not confounded by differential sample selection across treatment conditions.

4.6 Second Follow-up Survey

We fielded a second follow-up survey to allow us to study longer-term effects of transaction prices on Clorin use *after* households had exhausted the bottle we sold them.[46] Interviews occurred ap-

[44] Among the 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 0.94 - 0.97. (The demographic characteristics are inconsistent between the baseline and follow-up surveys in 8 percent of cases.)

[45] 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. 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.

[46] Surveyors' inventory of the bottles we sold (marked with an "X") showed that, as of the second follow-up survey, in the vast majority of households the bottle we sold was either absent (76 percent of households) or empty (6

proximately six weeks after the marketing intervention, although actual lags varied due to logistical factors. We used a survey instrument similar to that from the first follow-up. 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.[47]

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.[48] An analysis of survey attrition shows a marginally statistically significant negative relationship between offer prices and the likelihood of contact in the second follow-up survey (results not shown). 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 of offer prices. We note, however, that our analysis of this survey centers primarily on variation in transaction prices, which are not statistically significantly related to attrition rates (see appendix table 3).

5 Evidence on Screening Effects of Offer Prices

In this section, we report our findings on the extent to which raising the offer price screens out households with lower propensity to use Clorin. As a prelude to that analysis, we first ask whether offer prices affect purchasing behavior. Figure 2 shows the effect of offer price on the propensity to buy Clorin during our door-to-door intervention. The figure shows a downward-sloping relationship between offer price and the share purchasing Clorin, with nearly 80 percent of respondents buying Clorin at 300 Kw and only about 50 percent buying at 800 Kw. Column (1) of table 2 presents an estimate of a linear probability model of demand as a function of the offer price.[49] The model implies that an increase of 100 Kw in the offer price would result in a (highly statistically significant)

percent), confirming our expectation that most households would no longer be using the bottle we sold them by the time of the second survey.

[47] We did not test source water (in addition to drinking water) for the first follow-up survey because we were unable to get a sufficient number of test strips in time.

[48] As in the first follow-up, we attempted to contact only those households that had been successfully contacted during the marketing intervention.

[49] 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. Estimated marginal effects from probit models are virtually identical to those reported in table 2 (see appendix A).

7 percentage point reduction in the probability of purchase, corresponding to an economically nontrivial price elasticity (evaluated at the mean offer price and purchase probability) of about -0.6.[50] Columns (2) and (3) of table 2 show that the results in column (1) are robust to adding baseline controls, and to restricting to households reached in the follow-up survey, respectively.

Having established that higher offer prices reduce the number of buyers, we turn next to the first substantive implication of our model, namely that, as offer prices rise, use among buyers should also rise (that is, $\beta > 0$). Figure 3 shows coefficients from a regression of self-reported use among buyers on dummies for offer price, controlling for transaction price fixed effects to hold constant any psychological effects.[51] The figure shows an upward-sloping relationship between offer price and the likelihood of use among buyers.

Table 3 presents more parametric estimates of the effect of offer prices on Clorin use among buyers. We estimate linear probability models relating the probability of use (both self-reported and measured) to offer prices,[52] with transaction price fixed effects in all specifications to control for psychological effects.[53] The regressions in panel A of the table show that, conditional on transaction price, an increase of 100 Kw in the offer price leads to a statistically significant 3 to 4 percentage point increase in Clorin use among buyers, corresponding to an economically nontrivial usage elasticity (at the mean price and usage) of 0.3 to 0.4. Put differently, our estimates imply that, due to screening effects alone, 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 more than 25 percentage points.

In principle, there are two possible reasons for our finding that higher offer prices lead to greater use among buyers. The first is that higher offer prices select buyers whose *observable* characteristics—education, wealth, etc.—are predictive of Clorin use.[54] The second is that higher

[50] 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 a free bottle of Clorin.

[51] We have also conducted a less structured exercise in which we compute, for each transaction price, the use at each offer price relative to use at 300 Kw, and then average these differences across transaction prices. This is similar to the fixed effects approach, but does not impose separability on the effects of offer and transaction prices. The picture that results from this alternative approach shows similar patterns to those in figure 3.

[52] Probit models of use yield nearly identical estimates (see appendix A).

[53] See appendix A for evidence on the robustness of our results to allowing for interactions between offer and transaction prices in affecting Clorin use.

[54] Note that this possibility is *not* inconsistent with the validity of our experimental randomization, because these statements relate to the relationship between prices and observables *conditional* on purchase, rather than uncondi-

prices select buyers with a greater *unobservable* (to the econometrician) propensity to use. Separating these two mechanisms is important for policy, because if the screening effect is driven largely by selection on observables, the targeting effects of pricing may be largely achievable through programs that target distribution of Clorin based on observable demographics. By contrast, if the screening effect comes mostly from unobservable propensities, prices may be helpful in targeting distribution to likely users even when household demographics are available.[55]

In panel B of table 3, we test between these explanations, by re-estimating the specifications of panel B, but including as covariates a vector of household demographic characteristics measured as of our baseline survey.[56] These demographics are of the sort that might be available in a detailed household census, and so could in principle be used to target distribution of Clorin. The coefficients on offer price in panel B are only about 10 percent smaller than those in panel A, indicating that the vast majority of the screening effect we estimate is driven by higher prices selecting buyers with a relatively high unobservable propensity to use Clorin.[57]

These findings imply that willingness-to-pay contains useful information about use propensities, over and above what is available in a vector of household demographics. A related (but different) question is whether demographics are more or less predictive of use than willingness-to-pay. This is similar to asking whether a model relating use to demographics has a higher or lower R^2 than a model relating use to willingness-to-pay. In practice, however, because we do not observe willingness-to-pay directly, comparing the fit of these two models using R^2 is not possible. An alternative approach, which we adopt, is to ask whether a hypothetical distribution of Clorin in which it is given to the households with the highest predicted use (based on demographics) achieves more or less use among recipients than an equivalently selective pricing scheme (i.e., a pricing scheme

tionally.

[55] Separating observable and unobservable sources of heterogeneity may also be relevant to empirical models of selection, which frequently posit important dimensions of heterogeneity that are observable to economic agents but not to the econometrician (Heckman, 2001).

[56] Note that 9 respondents refused to answer one or more demographic survey questions. To verify that the slight difference in sample composition between panels A and B does not explain the difference in coefficients, we have re-estimated the specifications in panel A of table 3, excluding the 9 observations with missing values of one or more demographic characteristics, and find virtually identical results, as expected.

[57] Screening effects are also comparable in magnitude and statistical significance to those in panel A of table 3 when we include the entire range of baseline characteristics (as in appendix table 2) in the model. We use the more restricted set of characteristics in the table to more accurately represent the types of household data that might plausibly be available to marketers of Clorin. As a further robustness check, we have also re-estimated our screening model, dropping either households that report using Clorin as of the baseline survey, or households that had some Clorin at home as of the baseline. Our results are, if anything, stronger on this restricted sample.

that distributes Clorin to an equal number of households).

To implement this comparison, we first estimate a linear probability model relating use among buyers to our set of household demographics. From this model, we obtain a predicted use $\widehat{use}_i$ for each household i. Let $x(\eta)$ be the percent of households buying at offer price η predicted by the demand model in column (1) of table 2. After ranking households by predicted use $\widehat{use}_i$, we calculate, for each offer price η, the share of buyers reporting use at follow-up among the households in the top $x(\eta)$ percent by predicted use. This allows us to compare the top households by willingness-to-pay and the top households by predicted use at the same percentiles of the respective distributions. For example, at an offer price of 300 Kw, our demand model predicts that 76 percent of households will buy. We therefore compute reported Clorin use among the top 76 percent of buyers, ranked according to predicted use. Appendix figure 2 shows the resulting usage rates by offer price, normalized relative to the rate at 300 Kw. As the figure shows, the data exhibit significantly more slope with respect to willingness-to-pay than with respect to household demographics. The difference in observed use between buyers at 800 Kw and buyers at 300 Kw is more than four times larger than the analogous difference between households categorized by predicted use.

Our data also allow us to study directly how the observable characteristics of buyers change with the offer price, which we do in appendix B. We find some evidence that higher offer prices are more likely to attract baseline users, although these effects are statistically insignificant and smaller in magnitude than the screening effects in table 3. We find little evidence that buyers at higher prices are wealthier or more educated, suggesting that, at least in this context (and price range) higher prices do not target distribution to the "richest of the poor." Finally, consistent with Kremer and Miguel (forthcoming), we do not find evidence that higher prices selectively attract households with the greatest potential for health gains from Clorin, although we note that our measure of potential health gains is crude.

6 Evidence on Psychological Effects of Transaction Prices

In this section, we use variation in the transaction price to test the second substantive implication of the model in section 3, namely that the price has a direct psychological effect on Clorin use. As noted in section 3, we will be interested both in whether the amount paid for Clorin influences use (whether the function $h(\bullet)$ is increasing), and in whether the act of paying itself influences use (whether the function $h(\bullet)$ has a discontinuity at zero). As a first pass at testing these hypotheses, figure 4 graphs the relationship between transaction prices and use at follow-up, holding constant the offer price using fixed effects.[58] The figure shows no consistent evidence that paying more for Clorin increases use, although it is consistent with the hypothesis that those who paid something for Clorin used it more than those who paid nothing.

To test these hypotheses more formally, in table 4 we estimate regression models relating the probability of Clorin use at follow-up to the transaction price, including offer price fixed effects to control for differences in the composition of buyers at different prices. Because our analysis of balance (in section 4) does not rule out the possibility that transaction prices are statistically related to baseline use, we include a full set of baseline controls in all models.

In addition to testing for an effect of transaction prices on use, our data also allow us to relate any effects we find to a crude measure of the household's psychological propensities. At the end of our follow-up survey, we included a series of hypothetical choices designed to mirror the types of questions frequently used to elicit sunk-cost effects in the existing literature:[59]

> 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.[60] Consistent with existing evidence, we find that households in our sample do display sunk-cost effects in their responses to

[58] We have also conducted a less structured exercise in which we compute, for each offer price, the use at each transaction price relative to use at 0 Kw, and then average these differences across offer prices. The picture that results from this alternative approach shows similar patterns to those in figure 4.

[59] We placed these questions at the end of the survey in case these questions revealed anything about the study's hypotheses. Note that, in contrast to the most typical hypothetical-choice studies of sunk-cost effects, we employ a within-subject, rather than between-subject design. We chose this approach because it allows us to more cleanly classify households into "sunk-cost" and "non-sunk-cost" groups.

[60] 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.

these questions, with over 20 percent of respondents 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.[61]

Panel A of table 4 follows existing literature on sunk-cost effects in testing for an effect of the transaction price on the likelihood of Clorin use at follow-up (both self-reported and measured). Consistent with figure 4, there is no evidence of such an effect. Our point estimates (in specifications 1A and 4A) indicate an effect of transaction price on use that is small in magnitude and inconsistently signed. Our confidence intervals allow us to rule out positive effects on the probability of use greater than 3.6 percentage points (self-reported use) or 1.9 percentage points (measured use) per 100 Kw. These intervals rule out sunk-cost effects equal in size to the point estimates of the screening effect we report in table 3.[62]

In specifications (2A) and (5A) of table 4, we focus specifically on households that display the sunk-cost effect in our hypothetical choice scenario, and again find statistically insignificant point estimates with no consistent sign. The differences in coefficients between sunk-cost and non-sunk-cost households are statistically insignificant and inconsistently signed, suggesting no clear relationship between hypothetical choice behavior and the observed response to transaction prices.

In addition to an effect of the amount paid, it could be that the act of paying itself influences use. This hypothesis was suggested to us by several NGO personnel, and it motivated our decision to include a large number of households with a zero transaction price in our randomization design. In panel B of table 4, we estimate models paralleling those in panel A, but using a dummy for whether the household paid a positive transaction price as our key independent variable.

In some contrast with panel A, the point estimates in panel B consistently show large and positive—though statistically insignificant—effects of paying a positive transaction price on Clorin use. The estimated effect of the act of paying is driven almost entirely by the subsample of households that display the sunk-cost effect in hypothetical choices, which is consistent with our *a priori* hypotheses about the possible psychological mechanism for an effect of the act of paying. However, while the differences between the sunk-cost and non-sunk-cost households are large and

[61] 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, and 32 percent who said they would finish drinking it at 5,000 Kw. The differences among these groups are all highly statistically significant in paired t-tests.

[62] A formal test of the equality of the causal and screening effects, incorporating the statistical uncertainty in both estimates, yields p-values of 0.233 (self-reported use) and 0.072 (measured use).

consistently signed, they are not statistically significant.

As a more direct test of the practitioner hypothesis that "when products are given away free, the recipient often does not value them or even use them" (PSI, 2006), we have also split the sample according to respondents' self-reported agreement with the statement that "I value something more if I paid for it." We find that the estimated effect of paying a positive transaction price on Clorin use is far larger among those who report strong agreement with the statement than those who do not, with the effect on self-reported use becoming statistically significant ($p = 0.046$) in the sample of those reporting strong agreement (results not shown).[63] Combined with our earlier evidence, this contrast is suggestive, though not conclusive evidence, of a possible psychological effect of the act of paying on Clorin use.

It may also be possible to learn about the psychological mechanisms at work by looking at how the act of paying affects use of bottles of Clorin other than the one purchased from our marketers. Data from our second follow-up survey allow us to do this, because most households had exhausted the Clorin we sold by the time of the second follow-up.[64] In appendix table 4 we show that, with our standard use measures, we continue to find nontrivial, but statistically insignificant, effects of the act of paying on Clorin use as of the second follow-up. When we improve the precision of our chemical measure of Clorin use by adjusting for the chlorination of the household's water source (public or private tap), we find a positive and statistically significant effect of the act of paying on measured use ($p = 0.038$). Because existing theories of sunk-cost effects (Thaler, 1980; Eyster, 2002) would predict that any effect of the transaction price applies only to the bottle purchased from our marketers,[65] our finding of possible long-term effects may be useful in further refining the psychology of sunk costs.

[63] We have also tested crudely for a possible non-psychological mechanism for the point estimate on paying a positive transaction price, namely strategic intra-household interactions. In particular, if the female head of household needs to convince other members of the household that a costly purchase of Clorin was justified (so as to maintain credibility as a decision-maker), she may be inclined to use it more than if she had received it for free (Prendergast and Stole, 1996; Ashraf, 2005). Consistent with this mechanism, we find that the estimated effect of paying a positive transaction price is greater for married than for unmarried respondents.

[64] Another way to test for such an effect is to compare households that are or are not still using the bottle we sold as of the first follow-up survey. This approach, however, would be confounded by the fact that households that have exhausted the Clorin we sold are (by definition) high-frequency users, which would create a bias due to selection on the outcome variable (use).

[65] In Thaler's (1980) model, this is because the "loss" of paying for a bottle of Clorin is experienced at the time the bottle is consumed, so that by the time a second bottle is being consumed the loss of paying for a previous bottle is no longer relevant. In Eyster's (2002) model, this is because one's ability to use Clorin now has nothing to do with one's decision to purchase a previous (and now exhausted) bottle of Clorin.

7 Robustness and Interpretation

As section 4.4 details, we took steps in designing our experiment to maintain the conditions needed for identification, as outlined in section 3. Below, we use several pieces of evidence from our study to further support the plausibility of these conditions.

Effect of transaction price on purchase decisions. 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. We can test for such a lapse by asking whether transaction prices affected demand, after controlling for the offer price. Estimates of a linear probability model of demand indicate that, after controlling for offer price, a household's final transaction price had no statistical effect on its propensity to purchase Clorin (results not shown).[66]

Effect of offer price on psychological propensity to use Clorin. Our identification relies on psychological effects being mediated by transaction prices, rather than by offer prices. Some evidence indicates that we succeeded in making the transaction price salient. In the follow-up survey, respondents were asked whether anyone had offered them Clorin for free in the last month.[67] Among households that, according to 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 recall error.

If 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. As an additional check on this issue, we have tested whether the screening effect is larger for the households that display the sunk-cost effect in hypothetical choices. Comparing sunk-cost and non-sunk-cost households, the differences in estimated screening effects are statistically insignificant and of the wrong sign. If our hypothetical

[66] 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 ($p < 0.001$). We have also conducted this test separately for each of the six marketers involved in our experiment. In no case is there a statistically significant negative effect of transaction price on purchase probability. In one case, there is a marginally statistically significant positive effect ($p = 0.095$) of transaction price on purchase probability, but such a finding is not surprising given that we execute six separate tests, and the direction of the effect is not consistent with the idea that household demand responded to the transaction price.

[67] The survey did not ask how much the respondent remembers paying for the bottle sold by our marketers.

choices proxy well for psychological propensities, then these tests provide evidence against the view that psychological effects contaminate our estimates of the screening effect.

Discount effects. A related issue is that participants may have responded psychologically to the discount relative to the offer price, rather than the transaction price itself. We have tested for a relationship between use and the relative size of the discount—that is, the difference between offer and transaction prices, divided by the offer price. We find no evidence for such a proportional discount effect on use (results not shown).

A more subtle issue arises if the absolute (as opposed to relative) size of the discount (offer price minus transaction price) has an independent effect on use, in addition to any effect of offer and transaction prices. Because the offer price, transaction price, and discount are collinear, such a model is not identified. If the effect of a greater discount is to increase use, then our estimates will tend to overstate the screening effect and to understate the psychological effect. If greater discounts tend to decrease use, our estimates will understate the screening effect and overstate the psychological effect. If, because of our efforts to make the transaction price salient, the discount does not exert an independent effect, then our estimates will consistently identify the underlying offer and transaction price effects, even in the linear case.

Informational effects of offer and transaction prices. If, despite our design precautions, 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 confound the effects we estimate. To 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 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 index that averages all three is also unaffected by our treatments.[68] Moreover, controlling for this index in our main specifications leaves our key conclusions unchanged (see appendix A).

A related possibility is that households' beliefs about Clorin prices were impacted by our ex-

[68] Among households that report never having used Clorin as of our baseline survey, who might be expected to know the least about the product, there is no evidence of an effect of offer price or of having a positive transaction price on our aggregate quality index. We do find some evidence that higher transaction prices (somewhat counterintuitively) *worsen* attitudes towards Clorin, but this result is only marginally statistically significant ($p = 0.089$).

perimental treatment. 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.[69]

Income effects of transaction prices. If paying more for Clorin reduced household wealth significantly, this could in principle attenuate the psychological 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 evidence of a psychological effect of the amount paid, providing further evidence that attenuation due to income effects is unlikely to be a major confound.[70]

Social learning and communication. Debriefing interviews in a pilot study indicated that participants did not communicate much about the discounts we offered. As a further check on possible social effects of our price manipulations, we have verified (results not shown) that a household's purchase and use decisions are uncorrelated with the offer and transaction prices assigned to the closest neighboring household.

Marginal cost fallacy. For the households in our survey, the marginal cost of using Clorin is determined by the market replacement price, not by the transaction price. However, it may be that households psychologically perceived the cost of using Clorin to be higher when their transaction price was higher, which could attenuate sunk-cost effects and explain our failure to find an effect of the amount paid. To assess this possibility, we included in our follow-up survey a question designed to get at a household's propensity to behave in this way. In particular, we asked respondents to evaluate the statement "When I buy something that is expensive, I try to use it sparingly," on an agree/disagree scale. Comparing households that did and did not agree strongly with this statement reveals no evidence that the effect of transaction price on use is higher for households that do not

[69] 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, 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.

[70] Relatedly, interaction regressions show no evidence that the effect of the transaction price differs with our proxy for household wealth. In addition to its relevance for the issue of income effects, this test also provides some (admittedly crude) evidence against the view that sunk-cost effects are present only when the amount at stake is large relative to the household's income.

agree with the statement (results not shown).

8 Implications for Pricing Policy

In this section, we draw tentative conclusions about pricing policy based on our data, by estimating a parameterized version of the model in section 3. The advantage of estimating an explicit economic model is that it allows us to make predictions about use at a wide range of prices, including those outside the range of our experimental data, as well as to study the separate role of the screening and psychological effects in determining the overall relationship between prices and use. The corresponding disadvantage is that the model's predictions may be sensitive to its parametric assumptions.

We assume, first, that the distribution of willingness-to-pay in the population is uniform on the interval $[0, v]$, which in turn implies that demand is a linear function of offer prices η:

$$\Pr\left(buy_i \mid \eta\right) = 1 - \frac{\eta}{v}. \tag{6}$$

Consistent with our earlier evidence, we further assume that there is no psychological effect of the amount paid for Clorin, but that the act of paying itself may influence use, so that the psychological effect function $h(\tau) = \delta I(\tau > 0)$ for a given transaction price τ. Finally, we allow use to depend on a full set of baseline characteristics X, and assume that the shock ε_i to the utility of using Clorin is distributed as type-II extreme value. Together, these assumptions imply that, among buyers, the rate of use can be written as

$$\Pr\left(use_i \mid buy_i, \eta, \tau\right) = E\left(\frac{\exp\left(\alpha + \beta WTP_i + \delta I\left(\tau > 0\right) + X\gamma\right)}{1 + \exp\left(\alpha + \beta WTP_i + \delta I\left(\tau > 0\right) + X\gamma\right)} \mid WTP_i \geq \eta\right). \tag{7}$$

It is straightforward to estimate equations (6) and (7) jointly via maximum likelihood. (For the purposes of our policy calculations, we will assume, counterfactually, that non-buyers do not use Clorin, as would be the case if our door-to-door sale were the only channel for Clorin distribution.)[71]

[71] We adopt this assumption because it provides a better approximation to the context in which a policymaker sets a market-wide price, so that, by definition, non-buyers cannot use the product. Of course, in our experiment, some non-buyers do use Clorin they obtained through other sources (retail outlets or health clinics). When we enrich our model to allow a constant rate of use among non-buyers, the model implies even more strongly that nontrivial

Table 5 presents our estimates of the parameters of this model, with use measured both by self-reports and by measured chlorination. In general, the parameters have the expected sign. The screening effect parameter β is positive, statistically significant with self-reported use, marginally statistically significant with measured chlorination, and large enough to generate important differences in use propensity as a function of willingness-to-pay (see appendix figure 3). The psychological effect of the act of paying, measured by δ, is positive and statistically insignificant. The maximum willingness to pay in the population, v, is approximately $1,400$ Kw, which is highly statistically significant.[72]

The first (blue) line in figure 5 presents predicted Clorin use, based on the model, at alternative prices. For each price p, we predict purchase and use using equations (6) and (7) above, with $\eta = \tau = p$. Because of the psychological effect of the act of paying, the model predicts that moving from free distribution to a price of 100 Kw induces an increase in usage of around 4 percentage points. Use declines monotonically thereafter. In other words, at the point estimate, the model implies that the use-maximizing price is 100 Kw. In addition, the point estimates from the baseline model imply that a price of over 500 Kw (the price in health clinics) could be charged with no loss in use relative to free distribution.

These conclusions are contingent on the point estimate of the psychological effect of the act of paying for Clorin, which is not precise enough to permit us to statistically reject equality between use at 0 Kw and use at 100 Kw (see appendix figure 4). The second (pink) line in figure 5 therefore adopts a more conservative approach, assuming that there is no psychological effect of the act of paying on use. (Formally, we impose that $\delta = 0$, and hold other parameter values constant.) In the scenario without psychological effects, the use-maximizing price is 0, because raising prices always reduces the number of buyers and, hence, the number of users.

Even absent a psychological effect, the screening effects we estimate have potentially important implications for policy. To highlight these, in the third (green) line we graph predicted use as a

prices can be charged with little loss in use relative to free distribution, partly because of the positive use rate among non-buyers. We assume zero use among non-buyers in our baseline calculations both to be conservative and to better approximate the policy context of interest.

[72] The confidence interval on the parameter v admits values as low as about $1,300$ Kw, which is just above the upper end of the retail price distribution. Given that buying from our marketers offered the added convenience of a door-to-door sale, the model appears to be making plausible forecasts about the persistence of demand at high, out-of-sample offer prices.

function of price, under the assumption that there is no screening effect. (Formally, we assume that the willingness-to-pay distribution among buyers is identical at all prices.) A comparison between the curves reveals that ignoring the empirical selection of buyers results in potentially inaccurate predictions about use. For example, at the prevailing retail price of 800 Kw, the no-screening curve predicts almost 50 percent less use than the model with screening effects. Relatedly, the model implies that, without screening effects, a policymaker would need to charge half as much for Clorin (less than 400 Kw) in order to get the same use as the model with screening effects predicts at 800 Kw. In other words, the model implies that screening effects permit the policymaker to achieve the same level of Clorin use with a smaller subsidy, a fact that could be important in a scenario with limited policy resources.

It is important to stress that, beyond the conditions we outline in section 3, moving from our experimental data to predictions about pricing policy requires two significant further assumptions. First, as figure 5 itself makes clear, our policy simulations make predictions about prices outside the range of our experimental data, most importantly about use behavior at offer prices below 300 Kw.[73] Second, we vary prices in the context of a one-time-only, door-to-door marketing exercise, whereas pricing policy concerns long-term, market-level parameters.[74] For these reasons, the calculations in figure 5 are best thought of as an illustration of the approximate economic significance of our estimates, rather than as an exact guide to the consequences of alternative pricing policies.[75]

[73] We have examined the robustness of our conclusions to several alternative assumptions about purchase and use behavior at low prices. First, we have recalculated predicted use assuming that household willingness-to-pay is "lumped" at 0 Kw (Ariely and Shampan'er, 2004; Kremer and Miguel, forthcoming), and obtain similar predictions to our baseline model. Second, we have estimated a model in which we allow the psychological effect of prices to differ with a household's willingness-to-pay, and find that this alternative model predicts a somewhat greater difference between use at 100 Kw and use at 0 Kw than our baseline model. Finally, to check that households with extremely low willingness-to-pay are not likely to display high Clorin use, we have verified that households that refuse to purchase Clorin from us at 300 Kw tend to have relatively negative attitudes regarding water purification solution.

[74] It is unclear how accounting for this difference would affect our conclusions regarding policy. On the one hand, the fact that part of the relationship we estimate between offer price and purchase probability is likely due to substitution away from retail purchases biases us towards finding too negative a relationship between price and use, relative to a market-wide experiment. On the other hand, the fact that our experiment is one-time-only means we ignore the effect of a permanent price change on the shadow cost of using Clorin, which tends to bias us towards finding too weak a relationship between price and use, relative to a permanent price change.

[75] A third implicit assumption is that the storage costs of Clorin are small. If they are not, then in the context of a long-run policy of free or inexpensive distribution, low-use households might not purchase (or accept) much Clorin, thus reducing the magnitude of the screening effect. We note, however, that about 20 percent of respondents in our follow-up survey report using Clorin for non-drinking-water purposes such as doing laundry. If such uses are important, then even households with low likelihood of using Clorin for drinking water might accept it for free (or purchase at a low price) on an ongoing basis, in which case the screening effects we estimate would again become relevant for determining the relationship between prices and use.

9 Conclusions

In this paper, we report evidence from a field experiment in Lusaka, Zambia, designed to estimate the effect of higher prices on the use of Clorin, a household water purification solution. We find strong evidence that higher prices selectively attract buyers with a greater propensity to use Clorin. We find no evidence that the amount paid has a psychological effect on use, although our evidence is consistent with a psychological effect of the act of paying. A simple economic model of Clorin use suggests that the effects we estimate could have important implications for the costs and benefits of alternative pricing strategies.

Beyond the context we study, our experimental methodology may be useful in identifying the role of prices in governing use of other household health products, such as insecticide-treated mosquito nets. Our findings may also suggest a need to revisit the psychology of sunk costs, with greater emphasis on the difference between paying something and paying nothing, and on longer-term effects of prices on use.

While our study focuses on two important channels through which pricing policy may influence the use of health products in developing countries, we abstract from several others, most notably the informational role of prices in the introduction of new goods, and the role of prices in permitting NGOs to access retail distribution networks. Carefully estimating the importance of these channels remains an important area for future research.

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A Appendix: Additional Robustness Checks

Appendix table 6 checks the robustness of our key results to a number of alternative specifications. For each alternative model, we show the effect of offer prices on purchase probabilities, the effect of offer price on use among buyers, and the effect of transaction price (or a dummy for a positive transaction price) on use among buyers. In specification (1), we reproduce the coefficients from our main tables for comparison.

Specification (2) of appendix table 6 checks the robustness of our results to using a probit model of purchase and use, rather than a linear probability model. The table reports the estimated marginal effects evaluated at the sample mean of the covariates. In all cases these estimates are very similar to those we obtain in our main specification.

Specification (3) of appendix table 6 includes dummy variables for the six marketers we employed to control for any marketer-specific effects on purchase or use. Because the assignment of marketers is statistically unrelated to the price treatments, including these controls does not meaningfully affect our results. As a further check on this issue, we have estimated models of demand and use in which we interact our price treatments with marketer fixed effects (results not shown). In every case, F-tests indicate that the marketer-price interactions are jointly statistically insignificant. Our key results also survive (though with greater standard errors due to reduced sample size) when we eliminate the data associated with each marketer, one marketer at a time (results not shown). Finally, our results are robust to controlling for the date at which the household was reached by our marketer (results not shown).

In specification (4) of appendix table 6, we check the robustness of our results to relaxing the assumption that the effects of offer and transaction prices do not interact in determining the probability of Clorin use. Specifically, we have re-estimated our key models of use, allowing the effect of offer price to differ freely by transaction price, and allowing the effect of transaction price to differ freely by offer price. By averaging the coefficients across these separate specifications, we can obtain an estimate of the average effect of offer and transaction prices that does not restrict the effect of one price to be independent of the other. The results are similar to those in the main specification.

In specification (5) of appendix table 6, we control explicitly for an index of the respondent's self-reported attitudes toward Clorin at follow-up (see section 7). Though this index could be endogenous to our treatment conditions, including the index allows us to check whether any informational effects of prices might be confounding our estimates of the screening and psychological effects. Including this control does not meaningfully change any of our key coefficients.

Specification (6) of appendix table 6 presents a set of ordered probit models, using as a dependent measure the amount of free chlorine in the household's drinking water as of the follow-up survey. The direction and statistical significance of the ordered probit parameters are comparable to those of the main specification. To permit a comparison of magnitudes, in square brackets we report the implied marginal effect of a change in the key independent variable on the likelihood of having at least some free chlorine. The implied marginal effects are quantitatively similar to those in the main specification.

Specification (7) of appendix table 6 presents a set of ordered probit models, using as a dependent measure an index of how recently the respondent reports putting Clorin in her household's drinking water.[76] The estimates are similar to those of the main model in direction and statistical significance. (A direct comparison of magnitudes is not possible because the underlying parameters of the ordered probit models are not in the same units as the coefficients in our main specification.)

[76] In order of recency, the categories are: one week ago or more, between 48 hours and one week, between 24 and 48 hours, between 12 and 24 hours, between 6 and 12 hours, and within the last 6 hours.

B Appendix: Effect of Offer Price on Buyer Characteristics

Appendix table 5 presents regressions of baseline characteristics on offer price among buyers. Unlike table 3, we do not include transaction price fixed effects in these models, because psychological effects of prices cannot have impacted households' baseline characteristics.

Baseline use. Panel A of table 5 addresses the extent to which higher offer prices target distribution to households with a greater likelihood of baseline use. We find some evidence that buyers at higher prices are more likely to use Clorin as of the baseline, but the estimates are much smaller than the comparable estimates from follow-up data in table 3, and are statistically insignificant. (Some alternative measures of baseline use, such as the self-reported recency of the last Clorin use, do show a statistically significant relationship with offer prices among buyers, in the expected direction.)[77]

Wealth and education. In panel B of appendix table 5, we examine the effect of higher prices on the wealth and education of the purchasing households, an issue of significant potential interest in program design. We find no evidence that buyers at higher offer prices are wealthier or more educated. For example, we find that an increase of 100 Kw in the offer price increases the average share of durables owned (a proxy for wealth) by purchasing households by 0.16 percentage points, or about one percent of a standard deviation.[78] 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.[79] Results are similar with years of schooling as a dependent variable.

Health benefits of Clorin. The health benefits of Clorin are greater for households with young children, elderly, or immune-compromised members, as these individuals are most likely to experience severe consequences from diarrhea and other water-borne illnesses (Murray and Lopez, 1996). In panel C of appendix table 5, we show that households willing to pay higher prices for Clorin are not systematically those with the greatest potential health benefits, at least as measured by two crude proxies, the number of children below age 5, and a dummy for whether the female head of household is pregnant.

[77] Note that, while the model in section 3 does predict higher rates of baseline use among households willing to pay higher offer prices, this prediction does not result from the screening effect. Rather, it comes from the fact that buyers at higher offer prices should be more likely to be willing to buy Clorin at its prevailing market price. The reason the screening effect is not relevant for the relationship between offer prices and baseline use among buyers is that, theoretically, *all* households willing to buy Clorin at the prevailing market price would be expected buy at the prices we offered in our study, so that changing the offer price does not change the distribution of willingness-to-pay among prior purchasers of Clorin, and therefore does not change expected use among prior buyers. The relationship between offer price and baseline use (among those purchasing in our marketing phase) could therefore be smaller (or larger) than that between offer price and follow-up use.

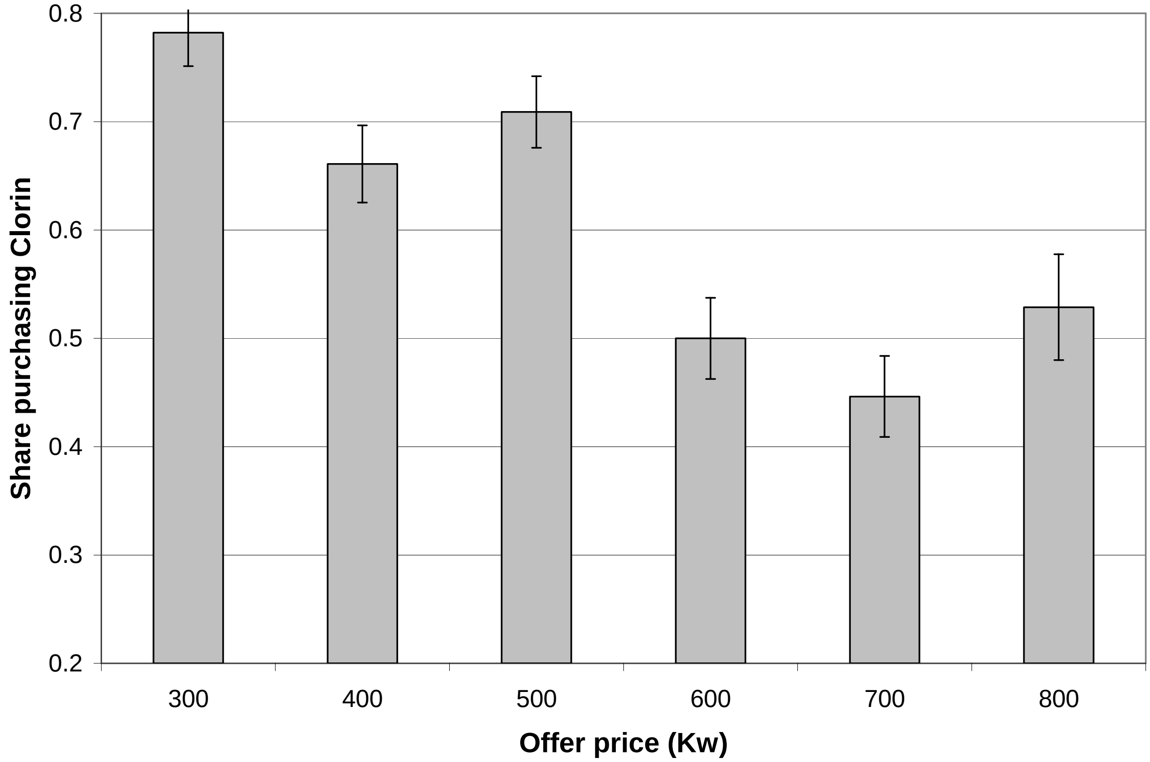
The same logic applies to use in our second, longer-term follow-up survey. This survey was done after most households had exhausted the Clorin we sold them, so that purchases were once again governed by whether the household's willingness-to-pay exceeds the market price. In fact, we find no statistically significant evidence of sorting on offer prices in this second survey, though we note that these results must be taken with caution in light of evidence that offer prices are somewhat correlated with attrition rates in the second survey (see subsection 4.6 above).

[78] 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. Filmer and Pritchett (2001) show that the first principal component of a set of durables ownership dummies performs as well or better than an expenditure-based measure in predicting children's school enrollments. In our data, the first principal component has a correlation of 0.99 with the share of durables owned.

[79] 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. Results are also similar (not shown) when we use durable goods prices from Zambia to construct a measure of the value of the household's durable assets.

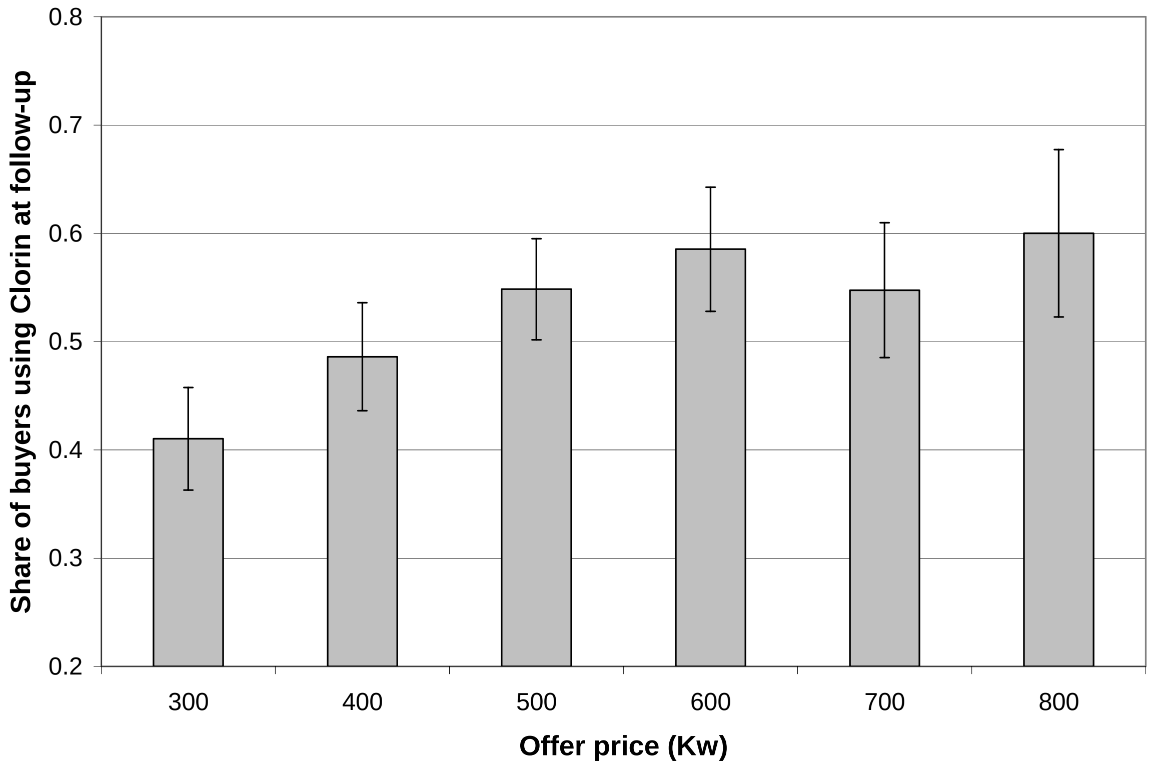
Figure 1 *A bottle of Clorin*

Figure 2 *The effect of offer price on purchase of Clorin*



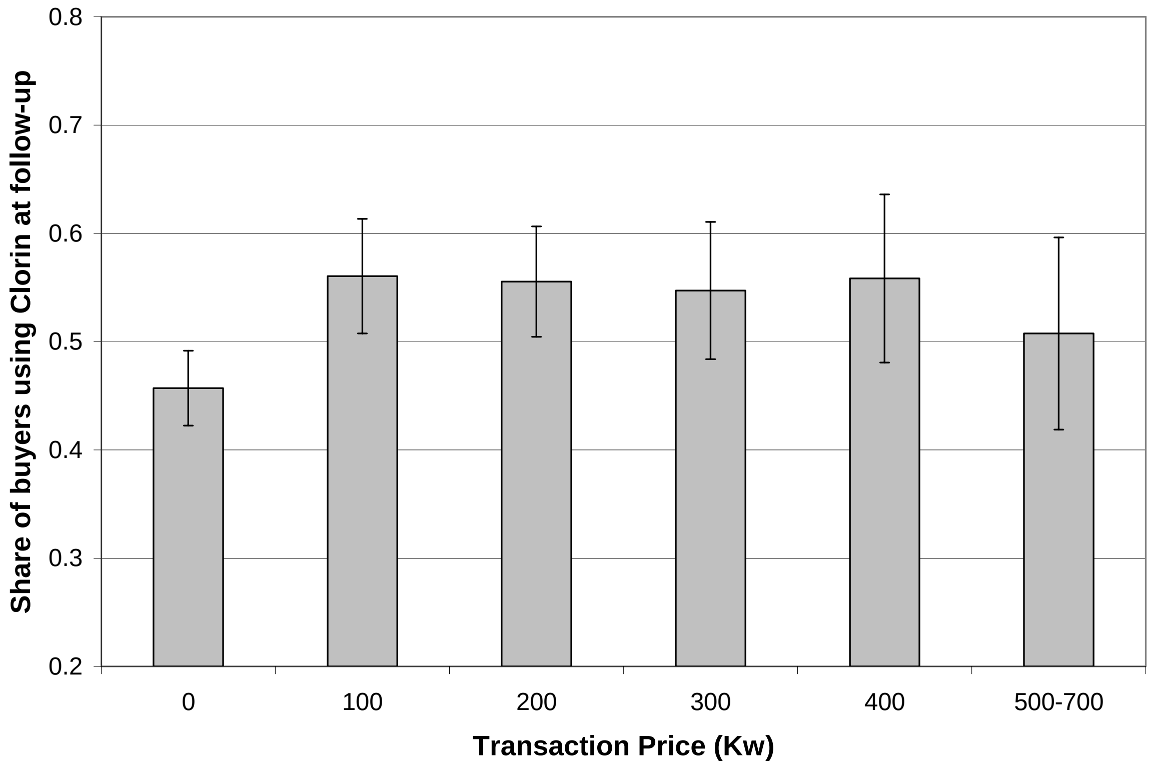
Notes: Figure shows share of households purchasing Clorin in door-to-door marketing intervention, at different offer prices (in Zambian Kwacha). Error bars reflect ±1 standard error.

Figure 3 *Usage rates of Clorin by offer price*



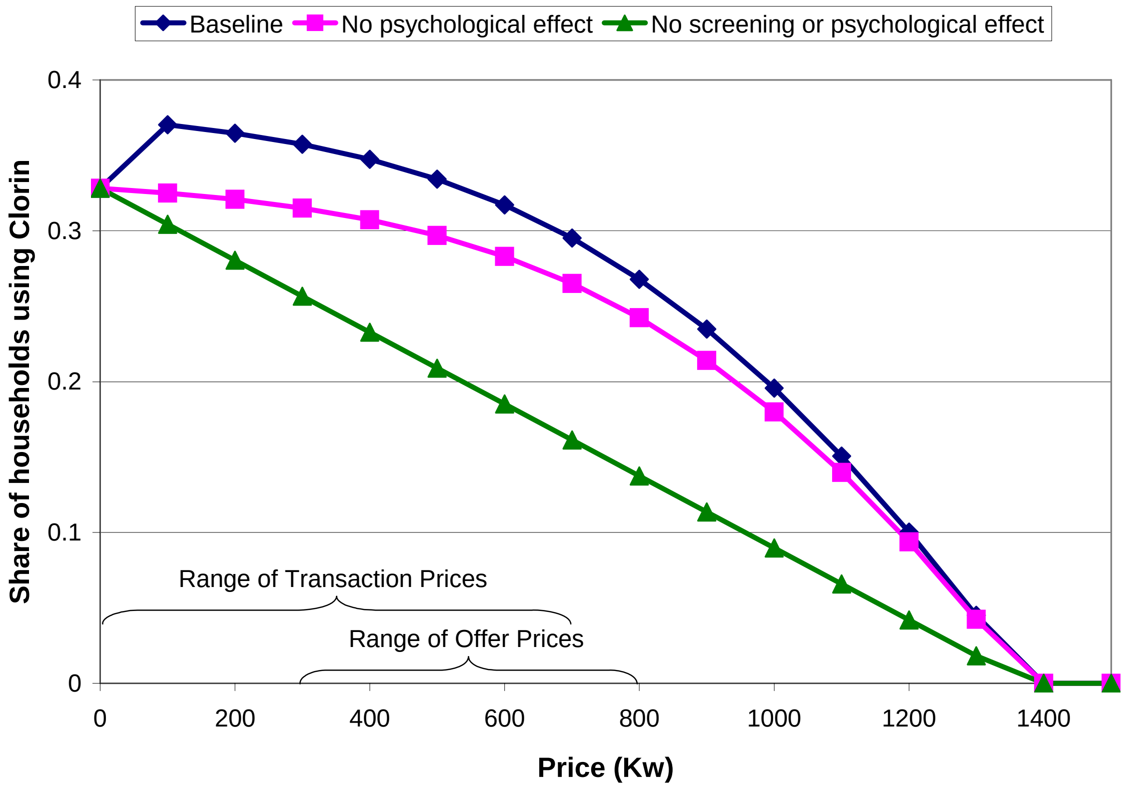
Notes: Figure shows coefficients from a regression of self-reported Clorin use at follow-up on dummies for offer price, with fixed effects for transaction price, for those households that purchased Clorin in our door-to-door marketing exercise. Coefficient on omitted category (offer price = 300 Kw) is normalized so that predicted share at sample mean of offer price dummies is equal to the observed share using Clorin. Error bars reflect ± 1 standard error.

Figure 4 *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 that purchased Clorin in our door-to-door marketing exercise. Coefficient on omitted category (transaction price = 0 Kw) is normalized so that predicted share at sample mean of transaction price dummies is equal to the observed share using Clorin. Cells with transaction price of 500, 600, and 700 Kw have been aggregated to improve precision. Error bars reflect ±1 standard error.

Figure 5 *Simulated effects of alternative pricing strategies*



Notes: Figure shows predictions based on estimated model as described in section 8, with parameter values as listed in column (1) of table 5. "No psychological effect" model assumes that usage rates are unaffected by transaction price. "No screening or psychological effect" model further assumes that the selection of buyers is identical at all prices.

Table 1 *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 226 households receiving an offer price of 300 Kw.

Table 2 *Estimates of the demand for Clorin*

Dependent variable: Household purchased Clorin (dummy)

	(1)	(2)	(3)
Sample	All	All	Follow-up
Offer price	-0.0664	-0.0653	-0.0708
(100 Kw)	(0.0093)	(0.0094)	(0.0099)
Transaction price			
(100 Kw)			
Constant	0.9640	0.9578	0.9892
	(0.0516)	(0.0520)	(0.0547)
Baseline controls?	NO	YES	NO
Sample mean of dependent variable	0.6116	0.6111	0.6135
Number of observations	1004	990	890

Notes: Standard errors 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 appendix table 2, standardized to have a sample mean of 0. Fourteen households are missing data on one or more baseline controls due to questionnaire refusals. Column (3) restricts the sample to respondents reached for the follow-up survey.

Table 3 *The effect of offer price on the usage rate 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
Sample mean of dependent variable	0.5147	0.5332
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
Sample mean of dependent variable	0.5140	0.5366
Number of observations	537	533

Notes: Standard errors in parentheses. Estimates are from linear probability models with fixed effects for transaction price, estimated on the sample of households that 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 appendix table 2. Nine households are missing data on one or more baseline demographics due to questionnaire refusals. We lack data on measured chlorination for 4 households due to a lack of stored drinking water for testing.

Table 4 *The effect of transaction price on the usage rate of buyers*

Panel A: Effect of amount paid

	(1A)	(2A)	(3A)	(4A)	(5A)	(6A)
Dependent variable	Water currently treated with Clorin? (follow-up; self-reported)			Drinking water contains free chlorine? (follow-up; measured)		
Sample	All	Sunk-cost household?		All	Sunk-cost household?	
		Yes	No		Yes	No
Transaction price	0.0097	0.0348	0.0042	-0.0071	-0.0106	-0.0079
(100 Kw)	(0.0133)	(0.0334)	(0.0149)	(0.0133)	(0.0330)	(0.0147)
Difference		*0.0306*			*-0.0027*	
(sunk-cost vs. non-sunk-cost)		*(0.0366)*			*(0.0361)*	
Offer price fixed effects?	YES	YES	YES	YES	YES	YES
Baseline controls?	YES	YES	YES	YES	YES	YES
Sample mean of dep. var.	0.5140	0.4336	0.5354	0.5366	0.4732	0.5534
No. of obs.	537	113	424	533	112	421

Panel B: Effect of act of paying

	(1B)	(2B)	(3B)	(4B)	(5B)	(6B)
Dependent variable	Water currently treated with Clorin? (follow-up; self-reported)			Drinking water contains free chlorine? (follow-up; measured)		
Sample	All	Sunk-cost household?		All	Sunk-cost household?	
		Yes	No		Yes	No
Transaction price > 0	0.0565	0.1840	0.0372	0.0318	0.0816	0.0240
	(0.0442)	(0.1030)	(0.0496)	(0.0440)	(0.1020)	(0.0493)
Difference		*0.1468*			*0.0576*	
(sunk-cost vs. non-sunk-cost)		*(0.1144)*			*(0.1133)*	
Offer price fixed effects?	YES	YES	YES	YES	YES	YES
Baseline controls?	YES	YES	YES	YES	YES	YES
Sample mean of dep. var.	0.5140	0.4336	0.5354	0.5366	0.4732	0.5534
No. of obs.	537	113	424	533	112	421

Notes: Standard errors in parentheses. Estimates are from linear probability models with fixed effects for offer price, estimated on the sample of households that 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 appendix table 2. We lack data on measured chlorination for 4 households due to a lack of stored drinking water for testing. Estimates for sunk-cost and non-sunk-cost households are from fully interacted models; estimates of the differences between the coefficients for these samples are from interactions between the relevant independent variable and a dummy for whether the household displays the sunk-cost effect in hypothetical choices.

Table 5 *Model parameters*

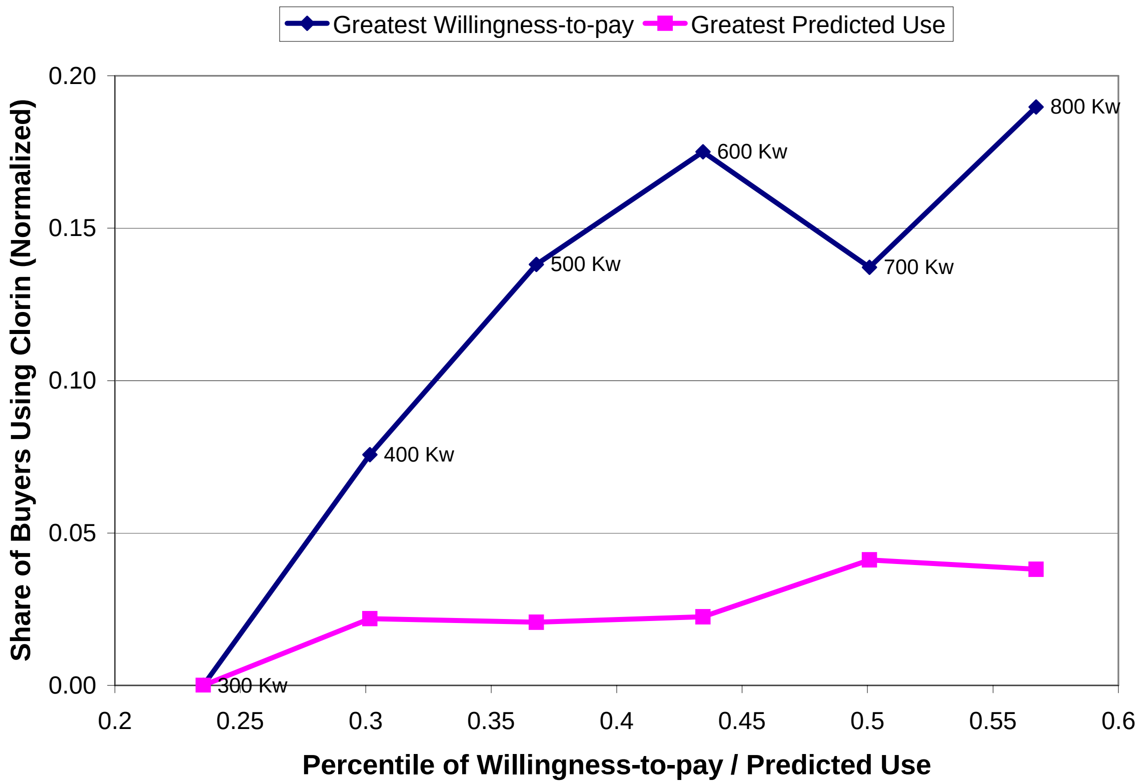
Parameter description	Parameter	(1) Water currently treated with Clorin? (follow-up; self-reported)	(2) Drinking water contains free chlorine? (follow-up; measured)
Intercept	α	-3.2864	-1.9916
		(1.5527)	(1.1989)
Effect of WTP on propensity to use	β	0.3333	0.2189
		(0.1605)	(0.1276)
Effect of positive price on propensity to use	δ	0.2755	0.1287
		(0.2223)	(0.2060)
Maximum WTP in population (in 100 Kw)	v	13.7660	13.7064
		(0.5557)	(0.5518)

Notes: Standard errors in parentheses. Table shows estimates of parameters of model in section 8, estimated by maximum likelihood.

Appendix Figure 1 *Sample coupon from door-to-door marketing*

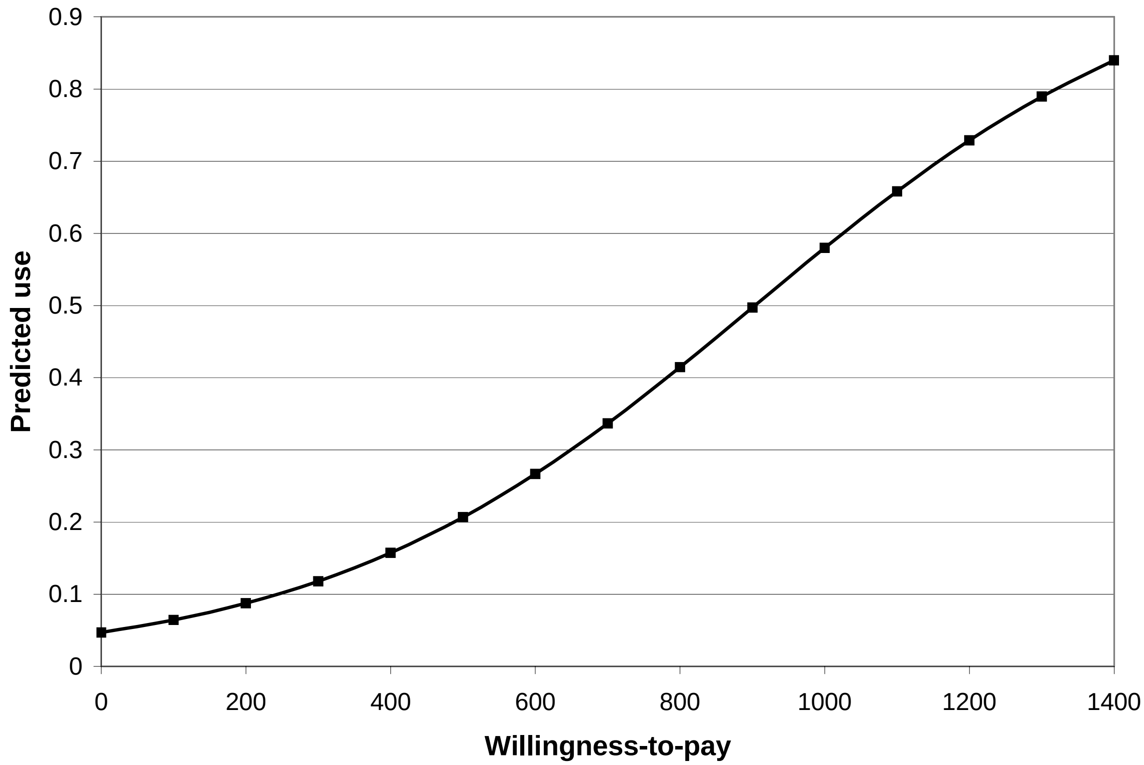
Notes: Figure shows a sample discount coupon from door-to-door marketing experiment. Coupon shows the final price at which the bottle transacted.

Appendix Figure 2 *Price-based targeting vs. demographics-based targeting*



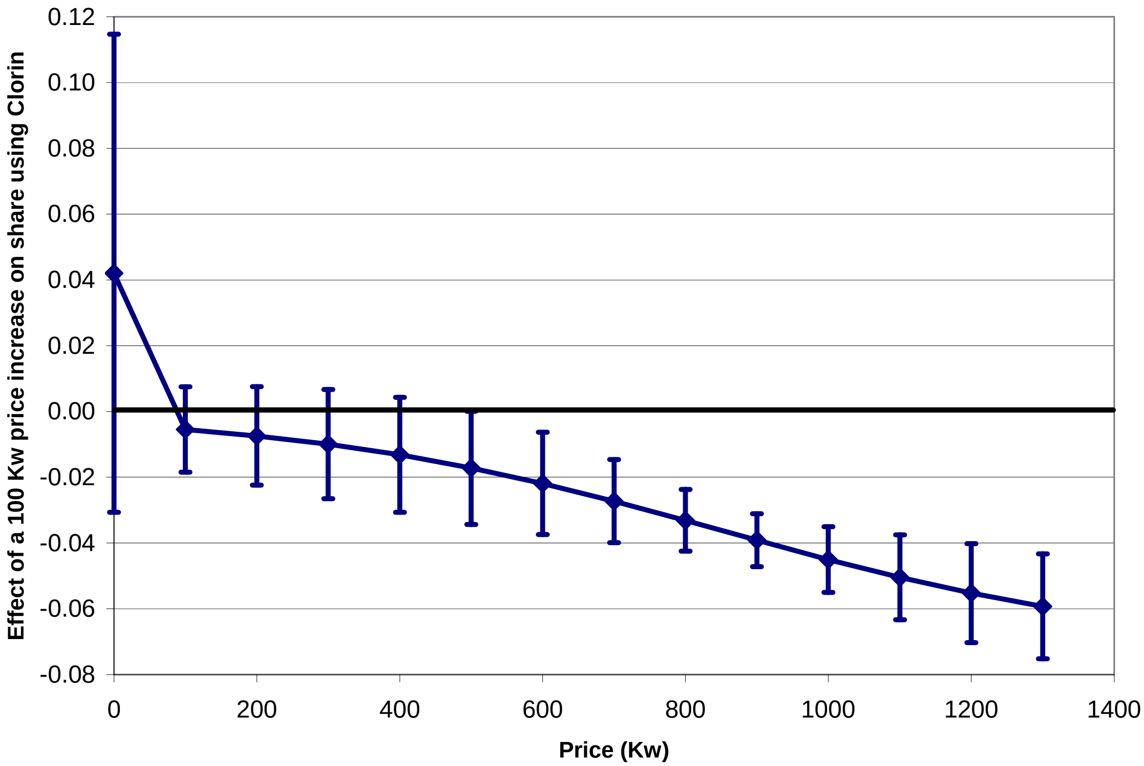
Notes: Top (blue) line shows share reporting Clorin use among buyers at or above each percentile of willingness-to-pay, with willingness-to-pay distribution based on estimated demand model from column (1) of table 2. Bottom (pink) line shows share reporting Clorin use among buyers at or above each percentile of predicted Clorin use, with predicted use determined through an OLS regression of self-reported use on baseline demographic characteristics (age, schooling, marital status, pregnancy, household composition, wealth, and locality fixed effects, as in appendix table 2). Share of use at lowest percentile is normalized to 0.

Appendix Figure 3 *Predicted use of Clorin as a function of willingness-to-pay*



Notes: Figure shows predicted use as a function of willingness-to-pay, based on model in section 8, with parameter values as listed in column (1) of table 5. Calculations assume a household with demographic characteristics equal to the sample mean paying a positive transaction price.

Appendix Figure 4 *Marginal effect of price increases on total Clorin use*



Notes: Figure shows predicted marginal change in total Clorin use at each initial price, based on estimated model as described in section 8, with parameter values as listed in column (1) of table 5. Standard errors computed using the delta method.

Appendix 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.

Appendix Table 2 *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; self-reported)	(0.1114)	(0.1250)	(0.1747)	(0.0525)
Drinking water contains free chlorine?	0.0764	0.0150	0.3300	0.0643
(baseline; measured)	(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)
Ever 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
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.

Appendix Table 3 *Determinants of sample attrition*

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	Marketing	Purchased Clorin		Purchased Clorin	
					Second	Second
Dependent variable	Marketing	Follow-up	Follow-up	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		-0.0024	
	(0.0068)		(0.0080)		(0.0094)	
Transaction price > 0				0.0325		-0.0494
				(0.0267)		(0.0313)
Water currently treated with Clorin?	0.0074	-0.0122	-0.0092	-0.0121	0.0035	0.0070
(baseline; self-reported)	(0.0302)	(0.0267)	(0.0338)	(0.0338)	(0.0397)	(0.0397)
Drinking water contains free chlorine?	0.0269	0.0152	-0.0057	-0.0057	0.0422	0.0446
(baseline; measured)	(0.0242)	(0.0214)	(0.0274)	(0.0273)	(0.0323)	(0.0321)
Use of soap before handling food	0.0355	-0.0131	0.0004	-0.0010	-0.0147	-0.0110
(index)	(0.0420)	(0.0371)	(0.0487)	(0.0487)	(0.0573)	(0.0572)
Use of soap after using toilet	-0.0268	-0.0069	-0.0231	-0.0220	-0.0379	-0.0390
(index)	(0.0434)	(0.0381)	(0.0496)	(0.0495)	(0.0583)	(0.0582)
Attitude toward water purification	0.0508	0.0965	0.0838	0.0852	-0.1077	-0.1059
(index)	(0.0613)	(0.0541)	(0.0690)	(0.0689)	(0.0812)	(0.0809)
Age in years	0.0016	0.0022	0.0034	0.0035	0.0025	0.0024
	(0.0013)	(0.0011)	(0.0015)	(0.0015)	(0.0017)	(0.0017)
Ever attended school?	-0.0063	-0.0224	-0.0035	-0.0068	0.1294	0.1362
	(0.0498)	(0.0438)	(0.0556)	(0.0557)	(0.0654)	(0.0654)
Years of completed schooling	-0.0052	0.0028	0.0078	0.0081	-0.0080	-0.0087
	(0.0051)	(0.0046)	(0.0059)	(0.0059)	(0.0069)	(0.0069)
Currently married?	0.0317	0.0214	0.0811	0.0789	-0.0161	-0.0138
	(0.0314)	(0.0283)	(0.0364)	(0.0364)	(0.0428)	(0.0427)
Currently pregnant?	-0.0085	0.0215	-0.0410	-0.0412	-0.0641	-0.0642
	(0.0369)	(0.0331)	(0.0430)	(0.0429)	(0.0505)	(0.0504)
Ever given birth to any children?	-0.0447	-0.0033	-0.0242	-0.0215	-0.0063	-0.0088
	(0.0490)	(0.0441)	(0.0563)	(0.0563)	(0.0663)	(0.0661)
No. of children in household under age 5	0.0133	0.0044	-0.0105	-0.0112	0.0070	0.0086
	(0.0145)	(0.0130)	(0.0178)	(0.0178)	(0.0209)	(0.0209)
No. of people in household	0.0074	0.0109	0.0121	0.0120	0.0016	0.0014
	(0.0053)	(0.0046)	(0.0058)	(0.0058)	(0.0068)	(0.0068)
Share of durables owned	0.1763	0.0638	0.0020	0.0033	0.2080	0.2079
	(0.0784)	(0.0697)	(0.0870)	(0.0869)	(0.1023)	(0.1021)
Locality fixed effects?	YES	YES	YES	YES	YES	YES
Fixed effects for offer price?	NO	NO	YES	YES	YES	YES
Fixed effects for transaction price?	NO	YES	NO	NO	NO	NO
F-test that control coefficients are 0	2.05	1.61	1.50	1.50	1.26	1.27
p-value of F-test	0.0060	0.0512	0.0837	0.0833	0.2118	0.2020
Number of observations	1244	990	605	605	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.

Appendix Table 4 *Longer-term effects of the act of paying on Clorin use*

	(1)	(2)	(3)
Dependent variable		Drinking water contains:	
	Water currently treated with Clorin?	any free chlorine?	more free chlorine than source water?
	(2nd follow-up; self-reported)	(2nd follow-up; measured)	
Transaction price > 0	0.0582	0.0361	0.0852
	(0.0420)	(0.0445)	(0.0409)
Offer price fixed effects?	YES	YES	YES
Baseline controls?	YES	YES	YES
Mean of dep. var.	0.3300	0.3893	0.2984
Number of obs.	506	506	506

Notes: Standard errors in parentheses. Estimates are from linear probability models estimated on the sample of households that 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 appendix table 2.

Appendix Table 5 *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
Panel B: Screening on household wealth and education			
(3)	Share of durables owned	0.0016 (0.0043)	614
(4)	Years of completed schooling (index)	0.0701 (0.0926)	613
Panel C: Screening on health benefits of Clorin			
(5)	Number of children under age 5	0.0129 (0.0203)	614
(6)	Respondent is pregnant?	0.0009 (0.0078)	614

Notes: Standard errors in parentheses. Estimates are from OLS regressions estimated on the sample of households that purchased Clorin in the door-to-door marketing intervention. All dependent variables measured as of the baseline survey.

Appendix Table 6 *Additional robustness checks*

	Specification	Effect on purchase: Offer price (100 Kw)	*Use measure*	Effect on use: Offer price (100 Kw)	Transaction price (100 Kw)	Transaction price > 0
(1)	**Main tables**	-0.0664	*Self-reported*	0.0373	0.0097	0.0565
		(0.0093)		(0.0149)	(0.0133)	(0.0442)
			Measured	0.0321	-0.0071	0.0318
				(0.0150)	(0.0133)	(0.0440)
(2)	**Probit**	-0.0678	*Self-reported*	0.0377	0.0111	0.0657
		(0.0097)		(0.0151)	(0.0142)	(0.0469)
			Measured	0.0325	-0.0077	0.0368
				(0.0152)	(0.0143)	(0.0472)
(3)	**Marketer fixed effects**	-0.0633	*Self-reported*	0.0351	0.0103	0.0610
		(0.0092)		(0.0151)	(0.0135)	(0.0446)
			Measured	0.0301	-0.0099	0.0276
				(0.0150)	(0.0132)	(0.0440)
(4)	**Average treatment effects**	—	*Self-reported*	0.0381	0.0129	0.0497
				(0.0148)	(0.0143)	(0.0464)
			Measured	0.0324	-0.0108	0.0176
				(0.0150)	(0.0148)	(0.0469)
(5)	**Controlling for quality assessments**	-0.0706	*Self-reported*	0.0341	0.0144	0.0695
		(0.0098)		(0.0144)	(0.0130)	(0.0431)
			Measured	0.0311	-0.0057	0.0357
				(0.0149)	(0.0133)	(0.0440)
(6)	**Ordered probit on free chlorine**	Underlying parameter		0.0701	-0.0171	0.0881
		(Standard error)		(0.0336)	(0.0314)	(0.1048)
		[Implied marginal effect]		[0.0273]	[-0.0064]	[0.0328]
(7)	**Ordered probit on recency**	Underlying parameter		0.0695	-0.0121	0.0969
		(Standard error)		(0.0319)	(0.0294)	(0.0977)

Notes: Standard errors in parentheses. See appendix A for details. Effect of offer price on purchase estimated on sample of households reached during marketing. Effect of offer price on use estimated on sample of households that purchased Clorin during door-to-door marketing intervention, in a specification that includes transaction price fixed effects. Effect of transaction price (and positive transaction price) on use estimated on sample of households that purchased Clorin during door-to-door marketing intervention, in a specification that includes offer price fixed effects and baseline controls (baseline Clorin usage and water chlorination, general health behaviors and attitudes, household demographics, and locality fixed effects, as in appendix table 2).