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Nava Ashraf
James Berry
Jesse M. Shapiro

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

Nava Ashraf
Harvard Business School

James Berry
Massachusetts Institute of Technology

Jesse M. Shapiro*
University of Chicago and NBER

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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, either by targeting distribution to high-use households (a screening effect), or by stimulating use psychologically through a sunk-cost effect. We develop a methodology for separating these two effects. We implement the methodology in a field experiment in Zambia using door-to-door marketing of a home water purification solution. We find that higher prices screen out those who use the product less. By contrast, we find no consistent evidence of sunk-cost effects.

JEL classification: C93, D12, L11, L31

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

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1 Introduction

Non-profit approaches to the distribution of health products in developing countries are often grouped into "social marketing" and "public health" categories, with the former emphasizing retail sales and the latter emphasizing free distribution through health clinics. Advocates of the public health approach often object to the use of prices to mediate distribution. 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 is commonly interpreted to mean that higher prices cause greater product use through a *sunk-cost effect* (Thaler, 1980; Eyster, 2002). An equally plausible interpretation, however, is a *screening effect*: that higher prices skew the composition of buyers towards households with a greater propensity to use the product (Roy, 1951; Oster, 1995).

Each of these effects is of broader economic interest—the former as a central prediction of psychology and economics, and the latter as an implication of the allocative role of prices. Isolating them may also help to clarify the terms of the ongoing policy debate over product pricing. However, the two effects are intrinsically unidentified in standard observational data: both imply that as prices rise, buyers use more. Evidence on the sunk-cost hypothesis has therefore been confined largely to hypothetical choices and a single, small-scale field experiment (Arkes and Blumer, 1985). Clean evidence that higher product prices select households with a greater likelihood of using the product is similarly limited.

In this paper, we present evidence on the effect of prices on product use from a large-scale field experiment in Zambia involving Clorin, an inexpensive, socially marketed disinfectant. Our experimental design allows us to separately identify screening and sunk-cost effects, and our setting allows us to measure product use objectively, without relying solely on household self-reports. We find strong evidence for screening effects: households with a greater willingness to pay for Clorin are also those most likely to use Clorin in their drinking water. By contrast, we find no evidence for sunk-cost effects, and only weak evidence for a modified version of the sunk-cost hypothesis suggested by practitioners.

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, in Zambia, Clorin 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.

In the paper, we lay out a simple model of Clorin use. Households differ in the costs and benefits of using Clorin in their drinking water, about which they are partially informed at the time of purchase. Households base their purchase decisions on the expected value (benefits net of costs) of using Clorin and on the offer price, and agree to buy when the expected value of using Clorin in drinking water is high. The model therefore predicts a screening effect: the higher is the offer price, the more the set of buyers is selected on their expected value from using Clorin, and hence the more likely are buyers to use Clorin in their drinking water.

We allow for psychological effects of prices by adopting Eyster's (2002) framework, which assumes a desire for consistency. In this framework, households pay a psychological cost if they purchase Clorin and do not use it in their drinking water. The magnitude of the psychological cost depends on how much better off the household would have been had it not bought Clorin; i.e., on the amount paid for Clorin. Households can avoid that cost by using Clorin in their drinking water, thus validating their decision to purchase Clorin in the first place. The model therefore predicts a sunk-cost effect: for a given offer price, greater transaction prices result in a stronger desire to rationalize one's purchase decision, and hence greater use in drinking water.

Under the assumptions of the model, then, our two-stage pricing design solves the core identification problem, allowing us to test separately for screening and sunk-cost effects. Varying the offer price for a given transaction price allows us to test for a screening effect of prices on the mix of buyers, holding constant the psychic cost of a failure to use Clorin in drinking water. Varying the transaction price for a given offer price then tests for a sunk-cost effect of prices on drinking-water use, holding constant the selection of buyers.

We find strong evidence for screening effects: holding constant the transaction price, the households who agree to a higher offer price are (statistically and economically) more likely to use Clorin in their drinking water at follow-up. That is, higher willingness-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. These findings indicate that households have substantial information about their use propensities that is not available directly to the econometrician, and that this information plays an important role in their purchase decisions.

Turning to sunk-cost effects, we find no evidence that households paying a higher transaction price are more likely to use Clorin in their drinking water, and some of our point estimates even suggest the opposite. This is true even among households displaying the sunk-cost effect in hypothetical choice scenarios. Our confidence intervals are tight enough to rule out effects of roughly the same order of magnitude as the point estimates of the screening effect that we estimate. Moreover, although Clorin is a relatively inexpensive product, the variation in prices we induce in our experiment is sufficient to generate a substantial effect on purchase probabilities, suggesting that our failure to find sunk-cost effects may not be due to small stakes. Hence, our findings do not support the model's prediction of a sunk-cost effect. In response to practitioner suggestions, we also test the hypothesis that paying something results in more use than paying nothing. Again, we cannot rule out the null of no effect, although in this case the sign, magnitude, and cross-household variation in point estimates are at least consistent with the hypothesized relationship.

On the whole, then, our results imply, at best, a limited role for sunk-cost effects in the domain of health product use, while providing strong support for the hypothesis that households have private information about their use propensities that is reflected in willingness-to-pay.

Our theoretical discussion suggests two caveats regarding the interpretation of these results. First, we test for sunk-cost effects operating through the mechanism proposed in Eyster (2002). Though Eyster's model is the most fully articulated single-agent theory of sunk-cost effects of which we are aware, reasonable alternatives exist with possibly different empirical implications. We discuss the robustness of our conclusions to alternative theories of sunk costs when we present the model. Second, the interpretation of our results depends on assumptions about what happens to Clorin that is purchased but not used in drinking water. The weight of the evidence indicates that Clorin not used in drinking water is used for household cleaning (in place of bleach or detergent), but our data are not definitive. The model clarifies how our results would be affected given alternative assumptions about how Clorin is used when it is not used in drinking water.

Our paper makes several contributions to the existing literature. Methodologically, we implement the first field experiment to identify both screening and sunk-cost effects.[1] 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, Arkes and Blumer's (1985) design does not attempt to identify the screening effect, and Karlan and Zinman's (2006) design does not attempt to identify sunk-cost effects.[2]

Substantively, we show that households base a health product purchase decision on private information about their propensity to use the product. In addition to its more direct relevance to the pricing of health products in developing countries, this finding contributes to ongoing efforts to study the role of private information in health care (e.g., Finkelstein and McGarry, 2006) and other domains (Karlan and Zinman, 2006). We also fail to find consistent evidence for sunk-cost effects in only the second, and by far the largest, field experiment on sunk-cost effects to date.[3] Ours is

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

[2] In Karlan and Zinman's (2006) design, the discounted interest rate (analogous to our transaction price) directly affects households' marginal incentives to default (through moral hazard or repayment burden), whereas in our context the transaction price is purely sunk.

[3] Eyster's (2002) review identifies Arkes and Blumer's (1985) as the only field study of sunk-cost effects to date. 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, 2007).

the first field study of sunk costs to include a treatment in which participants paid nothing for the product,[4] and the first to explicitly connect hypothetical choice responses and other measures of psychological propensity to objectively measured field behaviors.[5]

Beyond its implications for social science, our study informs an important set of public policy issues, from the pricing of health products in developing countries in particular (Kremer and Miguel, 2007)[6] to non-profit pricing strategy more generally.[7] Clorin and related "point-of-use" water purification systems hold promise as tools for addressing the lack of clean water facing over one billion people (USAID, 2006; Thevos et al, 2002-2003; Kremer et al, 2006). As with many health inputs, these tools rely on household behavior to produce desirable health outcomes (Grossman, 1972), implying that models of product use are likely to play an important role in the design and implementation of policies relating to water purification.[8]

Several hurdles remain, however, in deriving firm policy conclusions from our findings. First, our data have the power to test the effects of pricing on drinking-water use of Clorin, not on the incidence of water-borne illness, the ultimate outcome of interest. However, strong extant evidence shows 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). Second, Clorin is a relatively inexpensive health product, whereas much of the controversy surrounding social marketing has centered on more expensive products such as insecticide-treated mosquito nets. Using an inexpensive product serves to minimize income effects and hence to permit cleaner tests of the effects of interest. However,

[4] A number of existing papers explore the special role of zero prices, but none focuses on the effects on post-purchase use. See, for example, Shampanier, Mazar and Ariely (2007), Thornton (forthcoming), and Karlan and List (2007). 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).

[5] In this sense, our study also contributes to a growing literature connecting laboratory and survey responses to incentivized choices in markets (Fehr and Goette, 2007; Karlan, 2005; Ashraf, Karlan, and Yin, 2006).

[6] 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.

[7] 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).

[8] The determinants of product use also play an important role in many industrial organization contexts. For example, utilization is of intrinsic public policy interest in the market for energy-intensive consumer durables (Hausman, 1979) and advertiser-supported media (Kalita and Ducoffe, 1995; Petrin, 2003). Our methods may be useful in identifying the relationship between pricing and utilization in such markets.

we must leave to future work the question of how our conclusions generalize to more expensive products. Indeed, since our original writing, Cohen and Dupas (2007) have applied a version of our experimental design in the context of insecticide-treated mosquito nets, with some substantive differences in conclusions. Finally, and most broadly, we focus here on two effects of pricing on use, but others, such as quality signalling (Milgrom and Roberts, 1986) and access to retail distribution networks, may also play a role in determining optimal pricing policy.

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 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.[9] Chemically, Clorin is sodium hypochlorite bleach, which can be mixed with water stored in the household in order to kill water-borne pathogens, and thus prevent the contraction of water-borne illnesses that are especially dangerous to young children. Because many households in Zambia obtain their water from sources that are not properly chlorinated, and because Clorin is less expensive than boiling water or other alternative methods of disinfection, it has been a popular product since its launch in 1998 (Olembo et al, 2004).

Clorin is marketed by the bottle (see figure 1), and a single bottle is sufficient to disinfect up to 1,000 liters of water (about one month's water supply for a family of six). Clorin is sold widely in both retail outlets (for about 800-1,000 Kw) and health clinics (for about 500 Kw). 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.[10] The fact that Clorin is a relatively inexpensive product limits

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

[10] 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).

the possibility that wealth effects contaminate our estimates (see also section 7).

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 also 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.

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. (We provide additional tests for informational effects in section 7).

3 Conceptual Framework

In this section, we present a simple model of Clorin purchase and use, allowing for sunk-cost effects using Eyster's (2002) framework, in which households have a taste for rationalizing their past actions. The model clarifies the interpretation of our results and its dependence on our key assumptions. At the end of the section, we discuss how our results would generalize to alternative models of sunk-cost psychology.

3.1 Material Payoffs

We will consider household behavior in some number of periods indexed by t. In each period t, each household i must decide whether to purchase Clorin and, if so, whether to use it in their drinking water. In period t, each household i can decide to purchase Clorin for an exogenous, possibly household-specific offer price $p_{it} \in (0, R]$, where $R > 0$ is the retail price of Clorin. If the household decides to buy Clorin, it may receive an exogenous discount, so that it will only have to pay the transaction price $\tilde{p}_{it} \in [0, p_{it}]$. We assume that households do not anticipate the possibility of a discount, or, equivalently, that they consider the probability of a discount after purchase to be negligible.

We experimentally manipulate the prices faced by households in a particular period t'. For simplicity, we will assume that in all previous and subsequent periods, Clorin retails at R, and there are no discounts. That is, we assume that, in any period $t \neq t'$, $p_{it} = \tilde{p}_{it} = R$. In period t', however, prices $p_{it'}$ and $\tilde{p}_{it'}$ are randomly assigned across households in a manner specified in section 4 below. We will also assume for simplicity that the utility from using Clorin in a given period is independent of its use in previous or future periods.[11]

We will think of a period as approximately one month, about the time it takes to use up a bottle of Clorin in drinking water for a family of six. We assume that Clorin cannot be stored across periods. Chemically, Clorin is storable, but in practice the vast majority of households in our sample appear to have exhausted the Clorin we sold them within the six weeks of our follow-up period. This suggests that it is reasonable to focus on within-period use. Implicitly, we are thereby assuming that if a household buys Clorin in some period and does not use it in drinking water, it is exhausted in some other way. Field interviews we conducted after our study suggest a plausible account is that households use Clorin to undertake household chores such as bleaching sheets, washing vegetables, and cleaning toilets (see section 4.5).[12]

In each period, each household makes two decisions. First, the household must decide whether to buy Clorin. Then, if the household has purchased Clorin, it must decide whether to use Clorin in its drinking water. We let $b_{it} \in \{0, 1\}$ be an indicator for whether household i buys Clorin in period t. We let $d_{it} \in \{0, 1\}$ denote whether household i puts Clorin in its drinking water in

[11] Together, these assumptions mean that when we consider the effect of an increase in the offer price $p_{it'}$, we ignore cross-channel substitution (households buying from our door-to-door marketers who would otherwise have bought in a store) and intertemporal substitution (households buying from us who would otherwise have bought at a later time). The demand elasticities we estimate in our experiment are therefore unlikely to generalize (quantitatively) to a permanent, market-wide change in the retail price R.

[12] If our data are misleading, and in fact many households store and defer use of Clorin in drinking water, this force would reduce the power of our experimental tests to detect sunk-cost effects. In the model below, sunk-cost effects arise because households view it as a mistake to have purchased Clorin when they do not use it in their drinking water. In a model with storability, sunk-cost effects of the sort we test for would instead require that households view it as a mistake to have purchased Clorin when they do not use it in their drinking water in the period immediately following purchase.

period t, with $d_{it} = 0$ whenever $b_{it} = 0$. We assume these decisions are made sequentially, so that the decision to purchase is fixed when the household chooses whether to use Clorin in its drinking water. Consistent with the discussion above, we can think of $d_{it} = 0$ as performing household chores (other than purifying drinking water), which, as seems reasonable, we assume is possible with or without Clorin.

To apply Eyster's (2002) framework, we need to specify both material payoffs and utility, the latter incorporating the psychological desire to rationalize past choices. We begin by specifying the material payoff function, which we normalize so that the payoff from not buying Clorin is 0.

Households differ in the benefits and costs of using Clorin in their drinking water. We will write the net material payoff (in Kwacha) to household i from buying Clorin and using it in its drinking water in period t as $v_i + \varepsilon_{it} - \tilde{p}_{it}$. Here, v_i captures factors that are constant over time for a given household and are known at the time of purchase, and ε_{it} captures time-varying shocks unknown at the time of purchase.

Formally, we assume that v_i is distributed i.i.d. across households, that ε_{it} is distributed i.i.d across households and time periods, and that v_i and ε_{it} are independent of one another and of offer and transaction prices p_{it} and $\tilde{p}_{it}$ (the last condition being maintained by experimental randomization). Consistent with the interpretation of ε_{it} as an unanticipated shock, we will normalize $E(\varepsilon_{it}) = 0$. To simplify exposition, we assume that both v_i and ε_{it} are distributed according to (possibly different) differentiable CDFs with full support on the real line, and that v_i has finite mean.

The terms v_i and ε_{it} are general enough to accommodate a range of factors. For example, Clorin is used when the household obtains water from its local source, and hence its use requires attention from the female head of household at that time. In some households, the female head may have many other chores to complete when obtaining water, making it hard to put Clorin in at the right time (low v_i), whereas in others the female head may have few other responsibilities coincident with obtaining water (high v_i). In addition, for a given household, variation in the household's day-to-day needs may lead to higher (low ε_{it}) or lower (high ε_{it}) demands on the female head's attention when she obtains water, affecting the incentive to use Clorin.[13]

[13] Many other interpretations are possible. For example, households may differ in their general level of concern about water-borne illness, inducing variation in v_i. After purchase, some households may hear about an especially

To complete the specification of payoffs we must specify the payoff to buying Clorin and then not using it in drinking water, using it instead for household cleaning. In that case, the benefit of Clorin is the market value of the standard household cleaners whose use is offset by Clorin. That interpretation suggests a small return from using Clorin for non-drinking-water purposes. For example, data from a 2006 survey of retail prices show that the sodium hypochlorite in a bottle of Clorin can be obtained from Jik, a more concentrated household bleach, for about 300 Kw.[14] Moreover, our field experience suggests that households find products like Jik to be more effective cleaners, suggesting that 300 Kw of Jik may deliver even more than one Clorin bottle's worth of cleaning power. Given the small benefit to Clorin not used in drinking water, we assume for simplicity that the material payoff to buying Clorin and not using it in drinking water is $-\tilde{p}_{it}$, implying that no household would buy Clorin if it were not possible to use it in drinking water. Although that assumption is extreme and unlikely to hold exactly for all households, it seems likely to be a reasonable approximation on average, based both on the calculations above and on our conversations with female heads of household.[15]

To summarize, we can specify an (*ex-post*) material payoff function $u(b_{it}, d_{it}; \tilde{p}_{it}, v_i, \varepsilon_{it})$ that relates choices to payoffs as a function of household characteristics and the transaction price:

$$\begin{aligned} u(0,0;\tilde{p}_{it}, v_i, \varepsilon_{it}) &= 0 \\ u(1,0;\tilde{p}_{it}, v_i, \varepsilon_{it}) &= -\tilde{p}_{it} \\ u(1,1;\tilde{p}_{it}, v_i, \varepsilon_{it}) &= v_i + \varepsilon_{it} - \tilde{p}_{it}. \end{aligned}$$

(Recall that $d_{it} = 0$ whenever $b_{it} = 0$, so $u(0,1;\tilde{p}_{it}, v_i, \varepsilon_{it})$ is undefined.)

bad local incident of child diarrhea, leading to a high ε_{it}. Or they may hear that diarrhea episodes have been rare recently, leading to a low ε_{it}.

[14] As of the 2006 retailer survey, a 750ml container of Jik, consisting of 3.5% sodium hypochlorite, retailed for a median of 7,000 Kw. A 250ml bottle of Clorin, consisting of 0.5% sodium hypochlorite, retailed for a median of 800 Kw. The amount of sodium hypochlorite in a container of Jik is therefore equivalent to the amount found in about 21 bottles of Clorin, so 333 Kw of Jik buys as much sodium hypochlorite as one bottle (800 Kw) of Clorin. Note that this calculation ignores the convenience of Clorin's smaller size (and hence lower price per sales unit). Recall, however, that a week's supply of cooking oil for a household costs about 4,800 Kw, suggesting that 7,000 Kw is not a prohibitive outlay.

[15] If the assumption were to fail, then the sunk-cost effects we derive below would operate only over the range of transaction prices $\tilde{p}_{it}$ *above* the cost savings from using Clorin as a household cleaner.

3.2 Utility and Regret

To allow for psychological effects of prices, we will suppose that households have a taste for consistency, i.e. for taking actions in the present that rationalize their past choices. Following Eyster (2002), we implement this assumption by positing that realized household utility $U(b_{it}, d_{it}; \tilde{p}_{it}, v_i, \varepsilon_{it})$ depends both on *ex-post* material payoff and on a regret function:

$$U(b_{it}, d_{it}; \tilde{p}_{it}, v_i, \varepsilon_{it}) \equiv u(b_{it}, d_{it}; \tilde{p}_{it}, v_i, \varepsilon_{it}) - \rho r(b_{it}, d_{it}; \tilde{p}_{it}, v_i, \varepsilon_{it}) \tag{1}$$

where $r()$ denotes regret and $\rho \geq 0$ indexes the importance of regret in the household's utility function.

We refer the reader to Eyster (2002) for a more careful exposition of the regret function. Formally, it is defined as follows

$$r(b_{it}, d_{it}; \tilde{p}_{it}, v_i, \varepsilon_{it}) \equiv u\left(\tilde{b}(d_{it}; \tilde{p}_{it}, v_i, \varepsilon_{it}), d_{it}; \tilde{p}_{it}, v_i, \varepsilon_{it}\right) - u(b_{it}, d_{it}; \tilde{p}_{it}, v_i, \varepsilon_{it}) \tag{2}$$

where

$$\tilde{b}(d_{it}; \tilde{p}_{it}, v_i, \varepsilon_{it}) \equiv \arg\max_b \{u(b, d_{it}; \tilde{p}_{it}, v_i, \varepsilon_{it})\}.$$

Informally, given an action pair, regret is how much better the household's material payoff would be if it could re-choose its first stage action taking its second stage action, and the realization of ε_{it}, as given.[16] Households prefer to avoid regret, i.e. to choose actions that limit the harm done by their past choices given their current ones.

The definition in (2) implies that

$$\begin{aligned} r(0,0; \tilde{p}_{it}, v_i, \varepsilon_{it}) &= 0 \\ r(1,0; \tilde{p}_{it}, v_i, \varepsilon_{it}) &= \tilde{p}_{it} \\ r(1,1; \tilde{p}_{it}, v_i, \varepsilon_{it}) &= 0. \end{aligned} \tag{3}$$

[16] Note that, as we have defined it, regret depends on the realized transaction price $\tilde{p}_{it}$, which is technically not observed by non-purchasing households. However, because non-purchasing households do not make a use decision, and because households are unaware of the possibility of a discount at the time of purchase, this is purely a notational simplification, with no implications for behavior.

That is, regret is experienced only when the household buys Clorin but does not use it in its drinking water, and is felt in proportion to the amount spent on Clorin.

In the model, then, if a household knew for sure that it would not use Clorin to purify its drinking water, it would not buy Clorin. Households buy Clorin because they may use it in drinking water, but if circumstances are such that Clorin is not used in drinking water, the household regrets its purchase, and the regret experienced is greater the more the household paid for Clorin. Households may therefore be willing to use Clorin in drinking water to avoid regret, i.e. to rationalize the past decision to buy.

3.3 Choice and Identification

We can now specify how the household chooses whether to purchase Clorin and, if so, whether to use it in drinking water. Beginning with the use decision, if the household has purchased Clorin, the use decision is given by $d^*_{it}(\tilde{p}_{it}, v_i, \varepsilon_{it})$, where

$$d^*_{it}(\tilde{p}_{it}, v_i, \varepsilon_{it}) \equiv \arg\max_d U(1, d; \tilde{p}_{it}, v_i, \varepsilon_{it}).$$

This, in turn, implies that

$$\begin{aligned} d^*_{it}(\tilde{p}_{it}, v_i, \varepsilon_{it}) &= 1 \\ &\Longleftrightarrow \\ v_i + \varepsilon_{it} &\geq -\rho\tilde{p}_{it}. \end{aligned}$$

That is, the household will use Clorin in its drinking water if the net benefit of doing so exceeds the regret associated with not doing so. (Note that we have assumed for simplicity that ties are broken in favor of use.)

When households are deciding whether to purchase Clorin in the first place, they do not anticipate the discount, and do not know the realization of the time-varying shock ε_{it}. Therefore we write the expected-utility-maximizing purchase decision as $b^*_{it}(p_{it}, v_i)$, with

$$b^*_{it}(p_{it}, v_i) \equiv \arg\max_b EU(b, d^*_{it}(p_{it}, v_i, \varepsilon_{it}); p_{it}, v_i, \varepsilon_{it})$$

where the expectation is taken over the distribution of the shocks ε_{it}. It follows that

$$
\begin{array}{rcl}
b^*_{it}(p_{it}, v_i) & = & 1 \\
& \Longleftrightarrow & \\
E\left[\max\left(v_i + \varepsilon_{it}, -\rho p_{it}\right) \mid v_i, p_{it}\right] & \geq & p_{it}
\end{array}
$$

where we adopt the arbitrary rule that ties are broken in favor of purchasing.

Note that the household conditions its decision on the (known) household-specific benefit parameter v_i and on the offer price p_{it}. In particular, there exists a real-valued cutoff $v^*(p_{it})$, strictly increasing in p_{it}, such that the household purchases if and only if $v_i \geq v^*(p_{it})$. The proof of this result is straightforward; economically, it follows from the fact that a higher v_i increases the anticipated benefit from buying Clorin and a higher p_i increases the anticipated cost (both financially and psychologically).

Empirically, we do not observe the choice functions $b^*_{it}(p_{it}, v_i)$ and $d^*_{it}(\tilde{p}_{it}, v_i, \varepsilon_{it})$, but rather the empirical frequencies of different choices as a function of the prices p_{it} and $\tilde{p}_{it}$. To develop the model's empirical predictions, then, it will be helpful to write out these probabilities:

$$
\begin{aligned}
\Pr\left(b^*_{it}(p_{it}, v_i) = 1 \mid p_{it}, \tilde{p}_{it}\right) &= \Pr\left(v_i \geq v^*(p_{it})\right) \\
\Pr\left(d^*_{it}(\tilde{p}_{it}, v_i, \varepsilon_{it}) = 1 \mid p_{it}, \tilde{p}_{it}\right) &= \Pr\left(v_i + \varepsilon_{it} \geq -\rho\tilde{p}_{it} \mid v_i \geq v^*(p_{it})\right)
\end{aligned}
$$

where the second probability conditions on the decision to purchase Clorin.

In a non-experimental period $t \neq t'$, $p_{it} = \tilde{p}_{it} = R$, so there is no price variation and hence no comparative statics to test. (The model does imply restrictions on the relationship between prices in the experimental period and use in non-experimental period, which we explore in appendix A.)

In the experimental period t', by contrast, the above equations have two important empirical implications about the relationship between prices and use.[17]

- The first, which we call the *screening effect*, is that, conditional on purchase, the probability

[17] A third, more obvious, implication is that the greater is the price of Clorin, the fewer households will purchase it. Note that this result comes both from the traditional substitution effect, and from the fact that a higher price implies greater anticipated regret in the case in which Clorin is not used in drinking water.

of use is higher the greater is $p_{it'}$. This is because a higher $p_{it'}$ imposes a stricter (higher) cutoff $v^*(p_{it'})$ on anticipated benefits for buyers.

- The second, which we call the *sunk-cost effect*, is that, conditional on purchase, the probability of use is increasing in the transaction price $\tilde{p}_{it'}$ whenever $\rho > 0$. This is because a greater transaction price implies a greater desire to rationalize the purchase decision (or, equivalently, greater regret from not using Clorin in drinking water).

Observe that these two effects cannot be distinguished if transaction prices cannot vary independently of offer prices. If $p_{it'} = \tilde{p}_{it'}$ for all i in some period t, then a finding that higher prices causes more drinking water use conditional on purchase would be consistent with the presence of either the sunk-cost effect ($\rho > 0$) or the screening effect (heterogeneity in v_i), or both.[18] Hence, the two-stage pricing design solves an identification problem that would be present in data with only a single price, even if that price were suitably exogenous.

It is worth noting that, although Eyster's model is the most fully articulated single-agent theory of sunk-cost effects of which we are aware, reasonable alternatives exist with possibly different empirical implications. For example, if sunk-cost effects were driven by a desire to justify the *ex ante* wisdom of one's decision, rather than *ex post* wisdom of one's decision as in Eyster's framework, it is possible that the offer price, rather than the transaction price, would influence use behavior.[19] Such effects would confound our tests.[20] On the other hand, Thaler's (1980) prospect-theoretic justification for sunk-cost effects hinges on a desire to avoid a feeling of loss experienced when one

[18] Note that the psychology of regret is also relevant for the magnitude of the screening effect, as the parameter ρ partially determines the degree of price sensitivity in the purchase decision.

[19] Sunk-cost effects could also operate through a desire to justify the purchase to another member of the household, rather than to oneself (Prendergast and Stole, 1996). In that case, whether the offer or transaction price would influence use behavior through the sunk-cost mechanism would depend on the informational conditions in the household. Because only the offer price is known to the buyer at the time of the purchase decision, a fully informed household member would judge the intelligence of the decision based on the offer price. However, because only the transaction price is actually implemented, it may be more observable to other members of the household than the offer price. In that case, higher transaction prices would lead to greater use due to a desire to justify the purchase decision to other household members, so our tests for sunk-cost effects would be valid.

[20] Another possible confound arises if use depends not on the transaction price itself but on the discount, i.e. the difference between the offer and transaction prices. We find no evidence in our data of 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. If instead psychological effects operate through the absolute (as opposed to relative) size of the discount (offer price minus transaction price), the resulting model is collinear with those we estimate, and is therefore 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 sunk-cost effect. If greater discounts tend to decrease use, our estimates will understate the screening effect and overstate the sunk-cost effect.

does not realize consumption gains from a past purchase. In such a model transaction prices likely would mediate the effect, suggesting that our tests may be valid under mechanisms other than the one we model explicitly.

4 Experimental and Survey Design

Our main study consisted of a baseline survey, a randomized door-to-door marketing intervention approximately two weeks later, and a follow-up survey approximately two weeks after the intervention. We also conducted a second, longer-term follow-up survey, and a small-scale interview study (on a different sample) to assess non-drinking-water uses of Clorin.[21]

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").[22] 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.[23] 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

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

[22] 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.

[23] 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.

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.[24] 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 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,[25] 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).[26] 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

[24] 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.

[25] 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.

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

(such as light and heat) than measurement of total chlorine.[27] 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.[28]

It is worth noting that chlorination and Clorin use in drinking water are not identical concepts, even though they are closely related. 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 detectable 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]

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[27] Using total chlorine in place of free chlorine in our analysis results in stronger evidence of a screening effect and no evidence of a sunk-cost effect.

[28] 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.

[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 B 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

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 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.[34] 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.[35] 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.[36] After that, the marketing session ended.

To make the transaction price as psychologically salient as possible, 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. Showing the amount of the transaction price on the face of the coupon (see appendix figure 1) and requiring participants to write the transaction price on a receipt also served this purpose. We expected these measures

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 B).

[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] As we report in section 7, conditional on the offer price we find no evidence that household purchase decisions were related to the transaction price, consistent with the intent of our design.

[36] 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. 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.

to maximize the power of our tests for sunk-cost effects.[37]

To minimize inference about the market price of Clorin based the offer and transaction prices, the marketing script explicitly told respondents that Clorin was available in retail outlets for around 800 Kw.[38] To minimize inference about the quality of the Clorin bottles on offer, marketers introduced themselves as official representatives of SFH, the highly credible organization that produces, distributes, and markets Clorin throughout Zambia.[39]

We also took steps 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 size of the discount was chosen randomly, but every household received a discount of at least 100 Kw. We offered a discount to every household to avoid disappointing the respondents, and to ensure that every household was exposed to the coupon (in case of any advertising effects of the coupon itself).[40] 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

[37] 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. 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. We did not ask respondents to recall the amount of the transaction price if they paid a positive price.

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

[39] 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, 2007).

[40] This design choice represents a potentially important departure from Arkes and Blumer's (1985) design, in which identification of sunk-cost effects relies in part on comparing those who received a discount to those who did not.

were assigned a transaction price of 0 (a discount of 700 Kw), and 10 percent were assigned to a transaction price of 100, 200, 300, 400, 500, and 600 Kw (discounts of 600, 500, 400, 300, 200, and 100 Kw, respectively).

We assigned the offer and transaction prices randomly prior to the marketing outings, so that every household was assigned an offer price and a transaction price, even if we were unable to reach the household during marketing. The randomization was fully stratified by compound, with every compound receiving (up to integer constraints) the exact same mix of offer and transaction prices.[41] 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.[42]

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.[43] (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.) Our key results are fully robust to controls for baseline use.

[41] We made an effort to reach all sampled 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. As a further check on possible social effects of our price manipulations, we have verified that a household's purchase and use decisions are uncorrelated with the offer and transaction prices assigned to the closest neighboring household (results not shown).

[42] We conducted these balancing tests, separately by compound, on the sample of households surveyed in the baseline. We could not conduct analogous tests for the balance of transaction prices on the sample of households buying Clorin from us in the marketing phase, because we could not predict which households would be reached for our marketing intervention, or which households would purchase Clorin.

[43] 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 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.[44] We re-interviewed households approximately two weeks after the marketing intervention, but actual lags varied due to logistical factors.[45] 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.

The follow-up interview consisted of several sections. First, we repeated a handful of demographic questions from the baseline survey, as a check on the identity of the respondents.[46] 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 sunk-cost psychology. 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,

[44] 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.

[45] 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.

[46] 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.)

most likely because wealthier households tended to be in more developed sections of the compounds and were therefore easier to locate.[47] In the follow-up survey, attrition was still related to observables, though less so than in the marketing phase. Most importantly, at neither stage is the offer or transaction price 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.

4.5 Additional Survey Data

We will test for screening and sunk-cost effects by relating use measured at follow-up to offer and transaction prices. In order for this to constitute a valid test of the model, the follow-up period must correspond to the experimental period t'. This means that the Clorin we sold during the marketing intervention was in use at the time of the follow-up. To check this, we asked our surveyors at follow-up 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. In addition to confirming our expectations regarding the rate of exhaustion of Clorin, this evidence serves to mitigate concerns about inter-household transfer or resale of bottles.[48]

The model also assumes that Clorin not used in drinking water is used in some other way, rather than being stored for later use. We can bring some evidence to bear on this issue. We fielded a second follow-up survey so that, if we did find evidence of sunk-cost effects, we would be able to study whether they persisted after households had exhausted the bottle we sold them. Interviews occurred approximately six weeks after the marketing intervention, although actual lags varied due to logistical factors.[49] Consistent with the model's assumptions, surveyor inventories conducted at

[47] 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.

[48] In the short survey we describe below, only 6 percent of households report ever giving Clorin away.

[49] We used a survey instrument similar to that from the first follow-up. 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. (As in the first follow-up, we attempted to contact only those households that had been successfully

second follow-up showed that in over 80 percent of households the bottle we sold was either absent (76 percent of households) or empty (6 percent).

In our discussion of the model, we interpret a household that buys Clorin and does not use Clorin in its drinking water as having used Clorin for household chores. For additional evidence on the plausibility of this assumption, we turn to evidence from a series of in-depth interviews we conducted on a convenience sample of 49 Clorin-using female heads of household from four compounds in Lusaka, over six days in January and February 2008.[50] From pilot interviews we identified a set of chores in which Clorin was sometimes used. For each chore, an interviewer discussed with the respondent how she did the chore and whether she used Clorin at any point. If the respondent indicated that she used Clorin in doing the chore, the interviewer asked her to demonstrate how much Clorin she used (with a Clorin bottle filled with water). The interviewer then measured the amount with a measuring cup. Interviewees report substantial non-drinking water use of Clorin. Some 61 percent of households report using Clorin for purposes other than drinking water purification (96 percent report using it in drinking water). The most common reported alternative use was washing clothes, followed by cleaning toilets. According to our measurement exercise, these uses often involve substantial amounts of Clorin. Accounting for the relative frequency of different types of uses, we estimate that, among the respondents we interviewed, the average household devotes 38 percent of Clorin by volume to non-drinking-water uses.[51] This finding suggests that our interpretation of non-drinking-water use as use in household chores is at least quantitatively plausible.

5 Evidence on Screening Effects

In this section, we test for a screening effect: namely, that the higher is the offer price, the greater is the propensity to use Clorin in drinking water among those who buy. The model predicts such

contacted during the marketing intervention.)

[50] Interviewers collected data on age and years of schooling to test comparability with ever-users of Clorin in our baseline survey. Means of the two variables are broadly comparable between the two samples, with the interview survey sample somewhat more educated than the baseline sample. We are grateful to Michael Kremer for suggesting that we conduct these interviews to learn more about alternative uses of Clorin.

[51] In our original follow-up survey, about one-fifth of households report using Clorin for non-drinking-water purposes, substantially below the figure we found in our interviews. A plausible account of the discrepancy is that we took greater care in the in-depth interviews to list comprehensively the chores in which Clorin might be used.

effects because households have information about their likelihood of using Clorin in drinking water, and they use this information in making their purchase decisions. Hence, households that are less likely to use Clorin in drinking water are willing to pay less to buy it, and are therefore less common in the pool of buyers the greater is the offer price.

A prerequisite for such an effect is that 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.[52] The model implies that an increase of 100 Kw in the offer price would result in a (highly statistically significant) 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.[53] 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.

As a first test for the screening effect, 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. Consistent with a screening effect, 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 raising the offer prices on the propensity to use Clorin in drinking water among buyers. We estimate linear probability models relating the probability of use (both self-reported and measured) to offer prices, with transaction price fixed effects in all specifications to control for any sunk-cost effects.[54] 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,

[52] 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 B).

[53] 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.

[54] Probit models of use yield nearly identical estimates. Our results are also robust to allowing for interactions between offer and transaction prices in affecting Clorin use. See appendix B for details.

corresponding to an economically nontrivial usage elasticity (at the mean price and usage) of 0.3 to 0.4.

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. The second is that higher prices select buyers with a greater *unobservable* (to the econometrician) propensity to use. In addition to its intrinsic relevance to the economics of private information, separating these two mechanisms is potentially 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.

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.[55] These demographics are of the sort that might be available in a detailed household census. 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.

Note that, in contrast to a typical randomization-based regression, asking how including observables affects the coefficient of interest in table 3 is not a test of the validity of our randomization. This is because the regressions in table 3 are run *conditional* on purchase, rather than unconditionally, so that we are asking how correlated are observables with offer prices *conditional* on the decision to buy. Recall, however, that a nearly marginally significant relationship between offer price and baseline use is visible in the balancing tests in appendix table 2. We have therefore confirmed (results not shown) that screening effects are comparable in magnitude and statistical significance to those in panel A of table 3 when we include the entire range of baseline characteristics in the model, including baseline use. 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. (We have also re-estimated our screening model dropping households that

[55] 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.

had Clorin at home as of the baseline, and find, if anything, stronger evidence of screening effects on the restricted sample. See appendix A for details.)

The findings in table 3 imply that a household's willingness-to-pay is informative about its propensity to use Clorin, 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 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 (results not shown). In contrast to concerns that pricing leads to distribution to the "richest of the poor," we do not find that buyers at higher prices are wealthier or more educated.

This result may of course be sensitive to our choice of a relatively inexpensive product. Using two crude proxies for a household's potential for health gains from drinking water use—the number of children below age 5 and a dummy for whether the female head of household is pregnant—we find no evidence that those with greater potential for health gains have a greater willingness-to-pay for Clorin. Again, this result must be taken with caution, as our survey was not designed optimally to measure the propensity for health gains from drinking water use.

6 Evidence on Sunk-Cost Effects

In this section, we use variation in the transaction price to test for a sunk-cost effect. Sunk-cost effects arise in our model because using Clorin in drinking water rationalizes the household's decision to buy Clorin. Because the psychic cost of failing to rationalize the purchase decision is greater the more costly was the purchase, the model predicts that the likelihood of use (conditional on purchase) is rising in the transaction price, holding the offer price constant.

Figure 4 graphs the relationship between transaction prices and use at follow-up, holding constant the offer price using fixed effects. The figure shows no consistent evidence that paying more for Clorin increases use.

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) suggests 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:[56]

Suppose you bought a bottle of juice for 1,000 Kw. When you start to drink it, you

[56] 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.

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.[57] Consistent with existing evidence, we find that households in our sample do display sunk-cost effects in their responses to 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.[58]

Panel A of table 4 follows the model in testing for an effect of the transaction price on the likelihood of Clorin use at follow-up (both self-reported and measured), holding the offer price constant. 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 that we report in table 3.[59]

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.

Overall, then, our data do not provide evidence of sunk-cost effects as predicted by the model. We also designed our randomization to test a secondary hypothesis, suggested to us by NGO personnel, namely that paying something results in more drinking-water use than paying nothing.

[57] 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.

[58] 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.

[59] 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).

In panel B of table 4, we estimate models paralleling those in panel A, using a dummy for whether the household paid a positive transaction price as our key independent variable. As in panel A, we cannot rule out the null of no effect of the act of paying on drinking-water use. However, in contrast to panel A, the point estimates in panel B are large and positive, and in general larger among sunk-cost than non-sunk-cost households. Therefore, while our data show no evidence of an effect of an act of paying, they are at least consistent with such an effect, suggesting the need for further research.[60]

7 Robustness and Interpretation

Below, we use several pieces of evidence from our study to test the validity of the maintained assumptions of our model.

Effect of transaction price on purchase decisions. It is crucial to the interpretation of our results 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).[61]

Income effects of transaction prices. If paying more for Clorin reduced household wealth significantly, this could in principle attenuate the sunk-cost effect (though not the screening effect). As a simple test for this possibility, we have tested for sunk-cost effects among those in our sample with above-median wealth (as proxied by durables ownership). Even among this group, there is no

[60] 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). In our second follow-up survey, we also find some evidence of an effect of the act of paying on Clorin use, using a somewhat more precise measure of chlorination (results not shown).

[61] 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. 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.

evidence of a sunk-cost effect, providing further evidence that attenuation due to income effects is unlikely to be a major confound. 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.

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, then our estimates could be confounded. 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.[62] Moreover, controlling for this index in our main specifications leaves our key conclusions unchanged (see appendix B).

A related possibility is that households' beliefs about Clorin prices were impacted by our experimental 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.[63]

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

[62] 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 on our aggregate quality index. We do find some evidence that higher transaction prices (somewhat counterintuitively) *worsen* attitudes towards Clorin on the sample of never-users, but this result is only marginally statistically significant ($p = 0.089$).

[63] 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.

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 agree with the statement (results not shown).

8 Conclusions

In this paper, we report evidence from a field experiment in Lusaka, Zambia, designed to test the effect of prices on the use of Clorin, a socially marketed drinking water disinfectant. Our experimental design permits us to separately test for two effects of prices: a screening effect (higher prices change the mix of buyers), and a sunk-cost effect (higher prices induce greater use among those who buy). We find strong evidence of screening effects: raising the price of Clorin attracts buyers with a significantly greater propensity to use Clorin in their drinking water, indicating a positive correlation between willingness-to-pay and the propensity to use Clorin in drinking water. By contrast, we do not find evidence for sunk-cost effects: holding constant the distribution of willingness-to-pay, raising the price at which a household transacts does not affect its propensity to use Clorin in drinking water. Our results thus imply, at best, a limited role for sunk-cost effects in the domain of health product use, while providing strong support for the hypothesis that households have private information about their use propensities that is reflected in willingness-to-pay.

Our findings therefore cast doubt on justifications for health product pricing based on sunk-cost effects, while suggesting a possible role for prices as an allocative tool. As we have stressed, however, these implications must be taken with caution. They depend, in part, on the health (and other) consequences of non-drinking-water uses of Clorin, which to our knowledge have not been adequately explored in the medical literature. Moreover, 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 evaluating the role of these channels remains an important area for future research.

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A Appendix: Intertemporal Implications of the Model

In the model, realized transaction prices $\tilde{p}_{it'}$ in period t' will not affect purchase or use behavior in any period $t \neq t'$: sunk-cost effects are localized to the period in which the transaction occurs. The model does, however, have implications for the relationship between offer prices $p_{it'}$ conditional on purchase in period t' and purchase and use behavior in periods $t \neq t'$. In this appendix we briefly summarize those implications. We also provide some empirical tests using data from our baseline survey, reported in appendix table 4. The baseline period can be thought of as a reasonable approximation to period $t'-1$, in that any household owning Clorin as of the baseline survey would have purchased it from a retailer (or health clinic).[64] It is worth noting, however, that we did not design our study to test these intertemporal implications, so the evidence reported below should be thought of as preliminary.

Begin by considering the relationship between purchase behavior in the experiment and purchase behavior in non-experimental periods. The model predicts that households purchasing in period t' are more likely to buy at other periods $t \neq t'$, the greater is the offer price $p_{it'}$ in period t'. To see this, observe that, for households that purchase in the experimental period t', we can write the following for any non-experimental period $t \neq t'$:

$$\Pr\left(b_{it}^{*}\left(R, v_i\right) = 1 \mid b_{it'}^{*}\left(p_{it'}, v_i\right) = 1, R, p_{it'}\right) = \Pr\left(v_i \geq v^{*}\left(R\right) \mid v_i \geq v^{*}\left(p_{it'}\right)\right)$$

with $v^{*}\left(R\right) \geq v^{*}\left(p_{it'}\right)$. Because $v^{*}\left(\right)$ is increasing in $p_{it'}$, it follows directly that, conditional on purchase in period t', the probability of purchase in a period $t \neq t'$ is greater the greater is $p_{it'}$. Intuitively, the higher is a household's demonstrated willingness to pay in period t', the greater is its likelihood of purchasing at the retail price in other periods. We present a test of this implication in column (1) of appendix table 4. We measure purchase at baseline with an indicator for whether our surveyor inventory indicates that the household had a non-empty Clorin bottle at the time of the baseline survey. We find a marginally statistically significant positive relationship between offer price and baseline purchase (conditional on purchase at the marketing stage), consistent with the model.

Consider next the relationship between purchase behavior in the experiment and use behavior in non-experimental periods. Perhaps somewhat surprisingly, the model predicts that, conditional on purchase in the experiment and in a non-experimental period, the experimental offer price will be unrelated to use in non-experimental periods. Formally, consider the expression for use conditional on purchase in both the experimental period t' and some non-experimental period $t \neq t'$:

$$\Pr\left(d_{it}^{*}\left(R, v_i, \varepsilon_{it}\right) = 1 \mid b_{it'}^{*}\left(p_{it'}, v_i\right) = 1, R, p_{it'}\right) = \Pr\left(v_i + \varepsilon_{it} \geq -\rho R \mid v_i \geq v^{*}\left(R\right) \wedge v_i \geq v^{*}\left(p_{it'}\right)\right)$$

That is, the set of households purchasing at both time t and time t' are those for whom both $v_i \geq v^{*}\left(R\right)$ and $v_i \geq v^{*}\left(p_{it'}\right)$. However, because $R \geq p_{it'}$, $v^{*}\left(R\right) \geq v^{*}\left(p_{it'}\right)$, so that the two

[64] It is also possible, in principle, to use data from our second follow-up survey to approximate purchase and use behavior in period $t'+1$. In practice, however, some households still had the Clorin they purchased from us as of the second follow-up. (Removing those households from the data is problematic, because the fact that they did not exhaust all their Clorin is endogenous to their use behavior.) Moreover, 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), suggesting further need for caution in interpreting experimental treatment effects from the second follow-up survey. Nevertheless, we have conducted tests parallel to those in appendix table 4 using data from our second follow-up. With the exception of the first implication (that offer price will be related to purchase behavior at the second follow-up conditional on purchase at the marketing stage), the conclusions are identical.

conditions collapse to a single one:

$$\begin{aligned}\Pr\left(d_{it}^{*}\left(R, v_i, \varepsilon_{it}\right)=1 \mid b_{it'}^{*}\left(p_{it'}, v_i\right)=1, R, p_{it'}\right) &= \Pr\left(v_i+\varepsilon_{it} \geq -\rho R \mid v_i \geq v^{*}(R)\right) \\ &= \Pr\left(d_{it}^{*}\left(R, v_i, \varepsilon_{it}\right)=1 \mid R\right)\end{aligned}$$

Therefore we predict no relationship between offer price at time t' and use behavior at time $t \neq t'$. Intuitively, this result comes about because anyone willing to pay R should also be willing to pay $p_{it'} \leq R$, so that offer prices in the experiment convey no new information about the composition of households purchasing in non-experimental periods.[65] Columns (2) and (3) of appendix table 4 present tests of this implication. Among those who had Clorin in their households as of the baseline (our proxy for purchase at time $t'-1$) and who purchased Clorin in the marketing stage (time t'), there is no relationship between offer price and self-reported or measured use at baseline, consistent with the model. The coefficients are small, negative, and statistically insignificant.

Finally, consider the reverse question of how purchase behavior in non-experimental periods affects our predictions for use in the experimental period. Following a logic parallel to the preceding argument, it is straightforward to show that, conditional on purchase in a non-experimental period, there should be no relationship between offer price in the experimental period and use in the experimental period, holding constant the transaction price. That is, the screening effect should be absent for households that purchase in non-experimental periods. In equations, this is because:

$$\begin{aligned}\Pr\left(d_{it'}^{*}\left(\tilde{p}_{it'}, v_i, \varepsilon_{it}\right)=1 \mid p_{it'}, \tilde{p}_{it'}, b_{it}^{*}\left(R, v_i\right)=1\right) &= \Pr\left(v_i+\varepsilon_{it} \geq -\rho \tilde{p}_{it'} \mid v_i \geq v^{*}(R) \wedge v_i \geq v^{*}\left(p_{it'}\right)\right) \\ &= \Pr\left(v_i+\varepsilon_{it} \geq -\rho \tilde{p}_{it'} \mid v_i \geq v^{*}(R)\right).\end{aligned}$$

By contrast, households that did not purchase in non-experimental periods will still display the screening effect, although with a different magnitude than the overall population:

$$\Pr\left(d_{it'}^{*}\left(\tilde{p}_{it'}, v_i, \varepsilon_{it}\right)=1 \mid p_{it'}, \tilde{p}_{it'}, b_{it}^{*}\left(R, v_i\right)=0\right)=\Pr\left(v_i+\varepsilon_{it} \geq -\rho \tilde{p}_{it'} \mid v^{*}\left(p_{it'}\right)<v_i \leq v^{*}(R)\right).$$

We test these implications in specifications reported in columns (4) through (7) of appendix table 4. Consistent with the model, among those who purchased at the marketing stage and had Clorin at baseline, there is a statistically insignificant relationship between offer price and use at follow-up. (We note, however, that due to small sample size our confidence intervals cannot rule out nontrivial effects.) By contrast, among those who purchased at the marketing stage but did not have Clorin in the home as of the baseline, there is a positive and statistically significant relationship between offer price and use.

B Appendix: Additional Robustness Checks

Appendix table 5 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 5 checks the robustness of our results to using a probit

[65] Our model assumes a constant willingness-to-pay over time. A model with time-varying shocks to c_i that are known in advance of purchase to the household could yield a positive relationship between use in non-experimental periods and experimental offer price. However, given that the non-experimental periods we study are close in time to the experimental period, constant willingness-to-pay over time may be a reasonable approximation.

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 5 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 5, 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 5, 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 sunk-cost effects. Including this control does not meaningfully change any of our key coefficients.

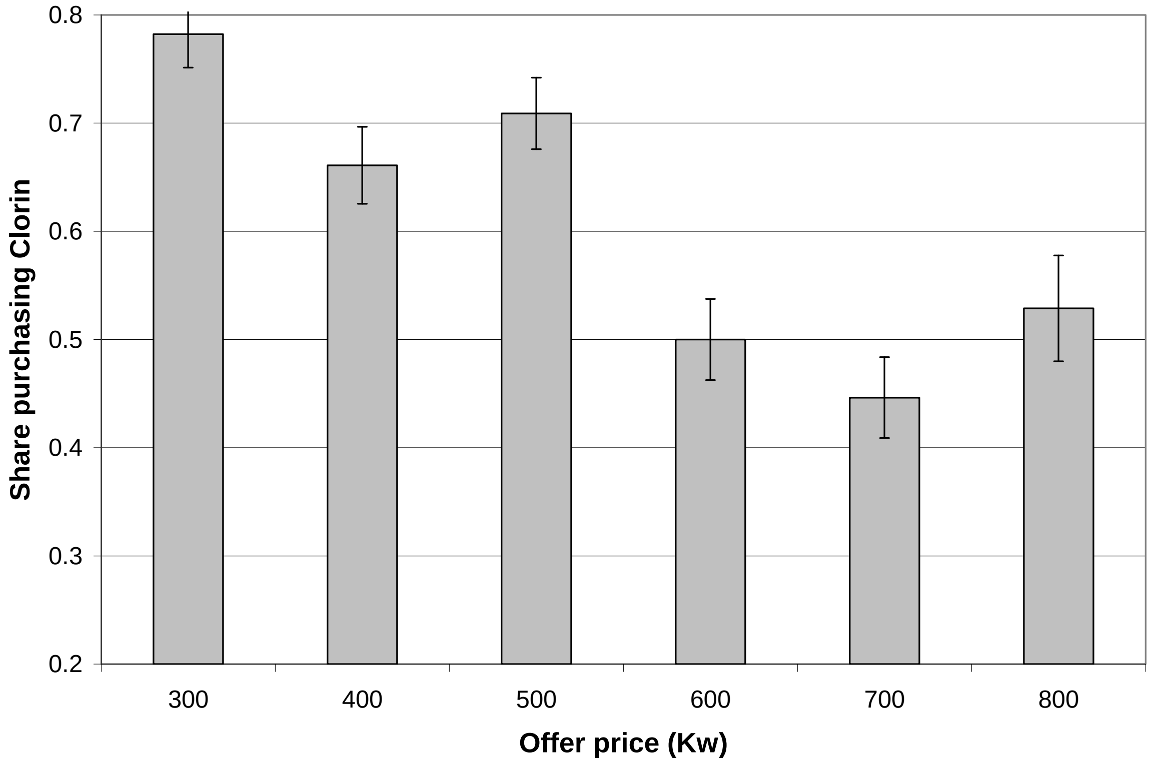
Specification (6) of appendix table 5 presents a set of ordered probit models, using as a dependent measure the amount of free chlorine in the household's drinking water as of the follow-up survey. The direction and statistical significance of the ordered probit parameters are comparable to those of 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 5 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.[66] 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 dependent variable is not in the same units as the coefficients in our main specification.)

[66] 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.

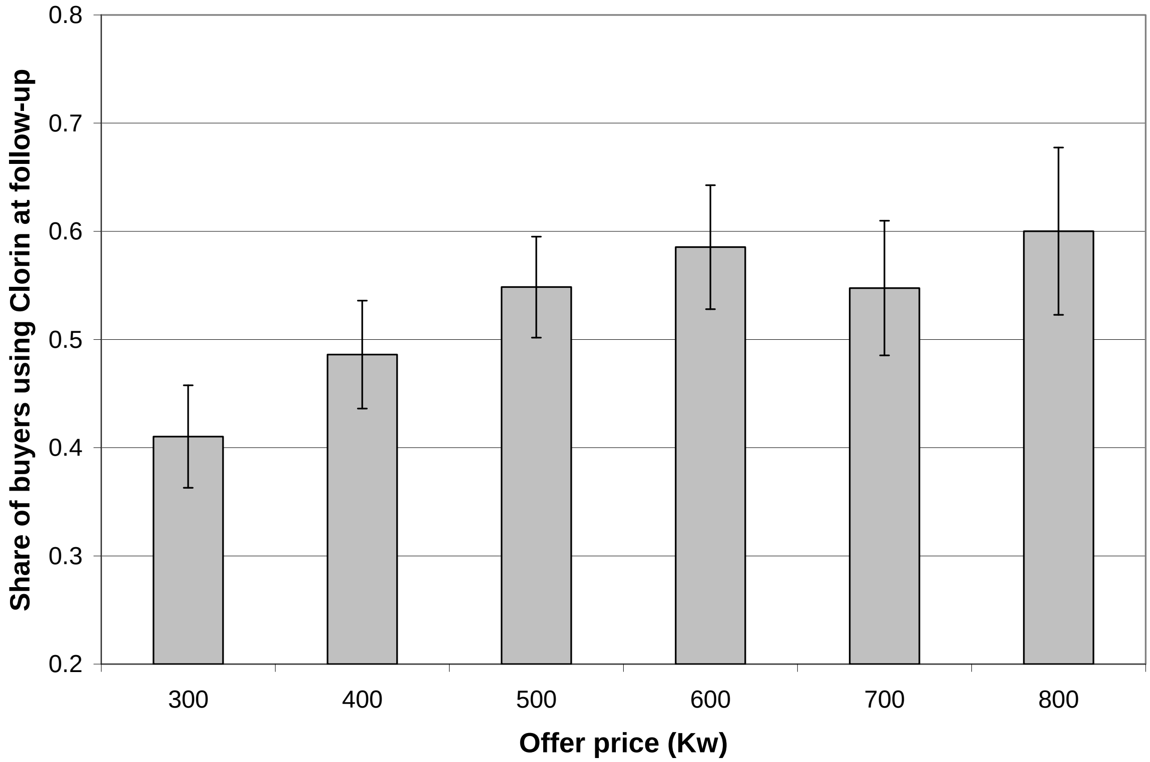
Figure 1 *A bottle of Clorin*

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



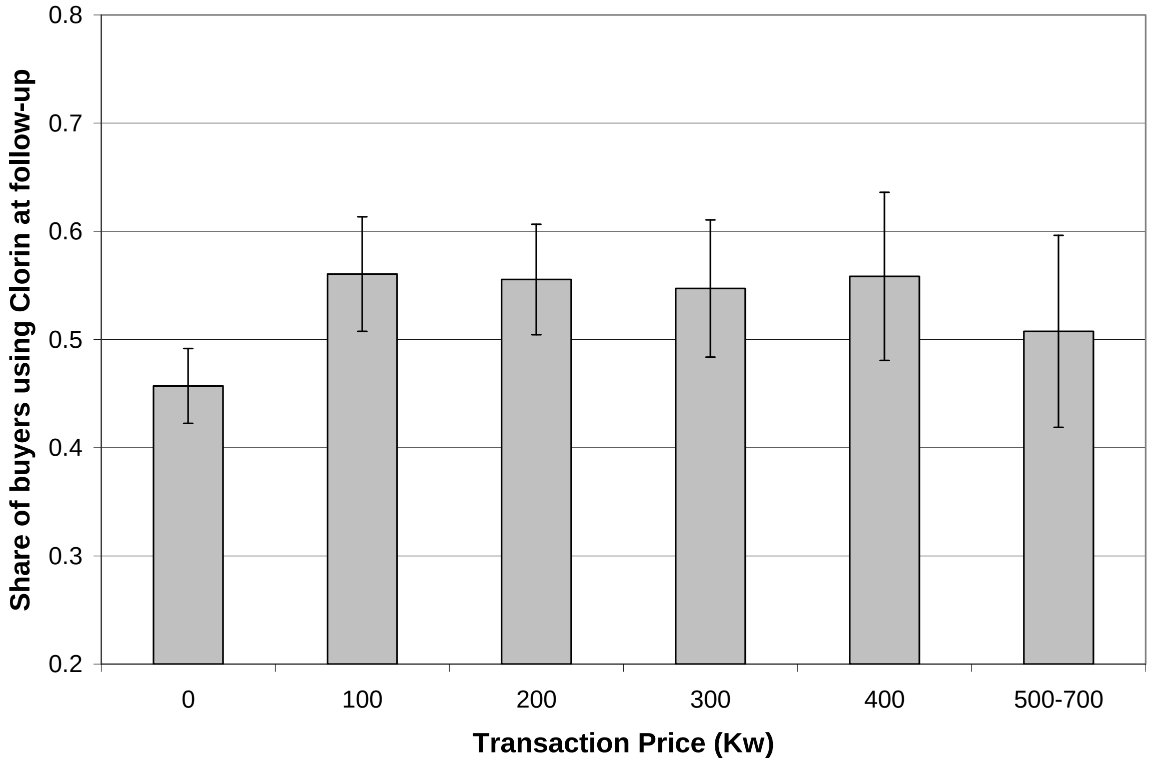
Notes: Figure shows share of households purchasing Clorin in door-to-door marketing intervention, at different offer prices (in Zambian Kwacha). Error bars reflect ±1 standard error.

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.

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)
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 *Evidence on screening effects*

Panel A: Screening on subsequent use of Clorin

	(1A)	(2A)
Dependent variable	Water currently treated with Clorin? (follow-up; self-reported)	Drinking water contains free chlorine? (follow-up; measured)
Offer price	0.0373	0.0321
(100 Kw)	(0.0149)	(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

	(1B)	(2B)
Dependent variable	Water currently treated with Clorin? (follow-up; self-reported)	Drinking water contains free chlorine? (follow-up; measured)
Offer price	0.0327	0.0293
(100 Kw)	(0.0150)	(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 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 *Evidence on sunk-cost effects*

Panel A: Tests for sunk-cost effect

	(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: Tests for effect of act of paying

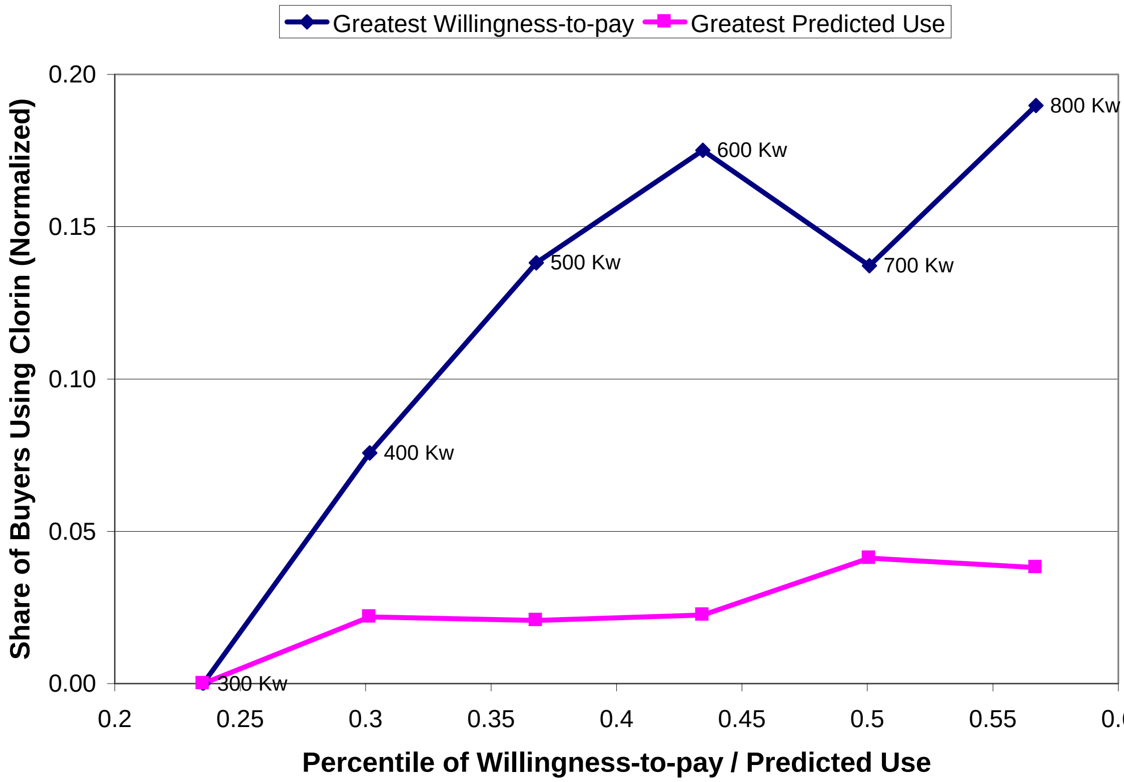
	(1B)	(2B)	(3B)	(4B)	(5B)	(6B)
Dependent variable	Water currently treated with Clorin? (follow-up; self-reported)			Drinking water contains free chlorine? (follow-up; measured)		
Sample	All	Sunk-cost household?		All	Sunk-cost household?	
		Yes	No		Yes	No
Transaction price > 0	0.0565	0.1840	0.0372	0.0318	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 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.

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 *Predicting use: willingness-to-pay vs. demographics*



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

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

Appendix Table 4 *Intertemporal implications of the model*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable	Have Clorin in household? (baseline; inventory)	Water currently treated with Clorin? (baseline; self-reported)	Drinking water contains free chlorine? (baseline; measured)	Water currently treated with Clorin? (follow-up; self-reported)		Drinking water contains free chlorine? (follow-up; measured)	
Sample: Clorin in home at baseline?	All	Yes	Yes	Yes	No	Yes	No
Offer price (100 Kw)	0.0182 (0.0104)	-0.0067 (0.0245)	-0.0093 (0.0265)	-0.0289 (0.0288)	0.0512 (0.0169)	-0.0046 (0.0294)	0.0386 (0.0171)
No. of obs.	614	133	133	117	429	117	425

Notes: Standard errors in parentheses. See appendix A for details. Sample consists of households who purchased Clorin at marketing stage.

Appendix Table 5 *Additional robustness checks*

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

Notes: Standard errors in parentheses. See appendix B 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).