

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

By NAVA ASHRAF, JAMES BERRY, AND JESSE M. SHAPIRO*

The controversy over 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 evidence of economically important screening effects. By contrast, we find no consistent evidence of sunk-cost effects. (JEL C93, D12, I11, M31, O12)

Nonprofit 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,”¹ and that fees ensure that goods only reach “the richest of the poor.”² Advocates of pricing counter that “when products are given away free, the recipient often does not value them or even use them.”³

The latter argument is commonly interpreted to mean that higher prices cause greater product use through a *sunk-cost effect* (Richard Thaler 1980; Erik Eyster 2002). An equally plausible

* Ashraf: Harvard Business School, Baker Library 443, Boston, MA 02163 (e-mail: nashraf@hbs.edu); Berry: Department of Economics, Cornell University, 486 Uris Hall, Ithaca, NY 14853 (e-mail: jimberry@cornell.edu); Shapiro: University of Chicago Booth School of Business, 5807 South Woodlawn Avenue, Chicago, IL 60637 (e-mail: jesse.shapiro@chicagobooth.edu). We are grateful to Gary Becker, Stefano DellaVigna, Dave Donaldson, Erik Eyster, Matthew Gentzkow, Jerry Green, Ali Hortaçsu, Emir Kamenica, Dean Karlan, Larry Katz, Michael Kremer, Stephen Leider, Steve Levitt, John List, Kevin M. Murphy, Sharon Oster, Amil Petrin, Matt Rabin, Mark Rosenzweig, Peter Rossi, Al Roth, Philipp Schnabl, Andrei Shleifer, Richard Thaler, Jean Tirole, Tom Wilkening, Jonathan Zinman, and seminar participants at the Harvard Business School, the University of Chicago, the Massachusetts Institute of Technology, the London School of Economics, the Paris-Jourdan Sciences Economiques, the Institut d’Economie Industrielle, Toulouse, the UQAM/CIPREE Conference on Development Economics, Yale University, Washington University in St. Louis, and Princeton University for helpful comments and Rob Quick at the Centers for Disease Control for his guidance on the technical aspects of water testing and treatment. We wish to thank Steve Chapman, Research Director of Population Services International D.C., for his support, and the Society for Family Health in Zambia for coordinating the fieldwork, particularly Richard Harrison and T. Kusanthan, as well as Cynde Robinson, Esnea Mlewa, Muza Mupotola, Nicholas Shiliya, Brian McKenna, and Sheena Carey de Beauvoir. Marie-Hélène Cloutier provided outstanding assistance with our in-depth interviews on alternative uses of Clorin, and Kelsey Jack provided numerous insights based on her field experience. Emily Oster provided tabulations of demographic characteristics from the DHS. Michael Kremer suggested conducting in-depth interviews to learn more about alternative uses of Clorin. We gratefully acknowledge financial support from the Division of Faculty Research and Development at Harvard Business School, the George and Obie Schultz fund at the Massachusetts Institute of Technology, and the Neubauer Family Faculty Fellowship at the University of Chicago Booth School of Business.

¹ Benn, Hilary. 2006. “Meeting Our Promises in Poor Countries.” Speech, London School of Hygiene and Tropical Medicine, London, June 15, 2006. <http://webarchive.nationalarchives.gov.uk/+/http://www.dfid.gov.uk/Media-Room/Speeches-and-articles/2006-to-do/Meeting-our-promises-in-poor-countries/>.

² McNeil, Donald G. Jr. 2005. “A Program to Fight Malaria in Africa Draws Questions.” *New York Times*, June 11.

³ Population Services International (PSI). 2006. “What is Social Marketing?” http://www.psi.org/resources/pubs/what_is_sm.html (accessed September 4, 2006).

interpretation, however, is a *screening effect*: that higher prices skew the composition of buyers towards households with a greater propensity to use the product (A. D. Roy 1951; Sharon M. 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 (Hal R. Arkes and Catherine 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 field experiment in Zambia involving Clorin, an inexpensive, socially marketed drinking-water 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 waterborne illnesses (Robert 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 above zero and 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 outline a simple economic model of Clorin use. Households that purchase Clorin may use it either in their drinking water, or for non-drinking water purposes such as household cleaning. A screening effect arises if the households willing to pay the most for Clorin are also those with the greatest propensity to use it in their drinking water. In that case, the higher is the offer price, the more the set of buyers is skewed towards those with greater willingness-to-pay, and hence the more likely are buyers to use Clorin in their drinking water.

We also allow for prices to affect use through a sunk-cost effect. A sunk-cost effect arises if higher transaction prices induce a greater psychological cost of failing to use Clorin in drinking water. In that case, a higher transaction price results in greater use in drinking water. Such effects could operate through loss-aversion (Thaler 1980), regret over mistaken purchases (Eyster 2002), a greater feeling of psychological commitment to a more expensive product (PSI 2006), or some other psychological mechanism.

Our two-stage pricing design permits 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 Clorin in drinking water about two weeks after purchase. This finding holds 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 hypothesis of a positive 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. In section VI we develop tentative implications of our findings for pricing policy. We show that for a given level of health externalities from Clorin use in drinking water, the screening effects we find reduce the optimal subsidy to Clorin by approximately one-half, relative to a case with no screening effects. We also discuss how our results might vary depending on the type of product in question, and in particular discuss ways to reconcile our findings with those of Jessica Cohen and Pascaline Dupas (2010), who deploy a related methodology to study the effect of prices on purchase and use of insecticide-treated mosquito nets in Kenya.

These calculations indicate that our screening effect estimates are large enough to be economically important. We stress, however, that taking policy implications from our findings requires a number of strong assumptions, which we review in Sections I and VI. In particular, while we argue that the screening effect is best interpreted as evidence of substitution towards drinking water uses and away from non-drinking water uses, we cannot rule out that some of the effect we find is due to intertemporal substitution towards earlier and away from later use. We discuss evidence on this possibility, along with other robustness checks, in Section V. There we show that estimated screening effects as of a second, longer-term follow-up are consistent in magnitude with our main specifications but are too imprecisely estimated to be conclusive.

Our paper makes several contributions to the existing literature. Methodologically, we implement the first field experiment to identify both screening and sunk-cost effects.⁴ Our two-stage

⁴ See Glenn W. Harrison and John A. List (2004) for a review of field experiments in economics more generally.

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 Dean Karlan and Jonathan Zinman's (2009) 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 (2009) design does not attempt to identify sunk-cost effects.⁵

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., Amy Finkelstein and Kathleen McGarry 2006) and other domains (Karlan and Zinman 2009). 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.⁶ Ours is the first field study of sunk costs to include a treatment in which participants paid nothing for the product,⁷ and the first to explicitly connect hypothetical choice responses and other measures of psychological propensity to objectively measured field behaviors.⁸

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 (Michael Kremer and Edward Miguel 2007)⁹ to nonprofit pricing strategy more generally.¹⁰ 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 (Angelica K. Thevos et al. 2002; Kremer et al. 2009).¹¹ As with many health inputs, these tools rely on household behavior to produce desirable health outcomes (Michael 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.¹²

The remainder of the paper is organized as follows. Section I provides background information on our experimental setting and outlines our hypotheses and key assumptions. Section II describes the design of our surveys and door-to-door marketing experiment. Sections III and

⁵ In Karlan and Zinman's (2009) 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.

⁶ Eyster's (2002) review identifies Arkes and Blumer's (1985) experiment ($N = 60$) 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 (Daniel Friedman et al. 2007).

⁷ A number of existing papers explore the special role of zero prices, but none focuses on the effects on postpurchase use. See, for example, Kristina Shampanier, Nina Mazar, and Dan Ariely (2007), Rebecca Thornton (2008), and Karlan and List (2007). More generally, our evidence contributes to existing research on the psychology of product pricing (see, e.g., John Gourville and Dilip Soman 2002; Baba Shiv, Ziv Carmon, and Dan Ariely 2005).

⁸ In this sense, our study also contributes to a growing literature connecting laboratory and survey responses to incentivized choices in markets (Ernst Fehr and Lorenz Goette 2007; Karlan 2005; Ashraf, Karlan, and Wesley Yin 2006).

⁹ Though there have been some studies of the effectiveness of prices in encouraging product use in social marketing contexts, existing research typically takes a nonexperimental approach (Dominique Meekers 1997; C.A. Maxwell et al. 2006). An exception is Jennie I. Litvack and Claude 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.

¹⁰ In this sense, our paper relates to the economics of pricing in nonprofit industries in general (Joseph P. Newhouse 1970; Cheryl A. Casper 1979; Sharon M. Oster 1995; Richard Steinberg and Burton A. Weisbrod 1998; Oster, Charles M. Gray, and Charles Weinberg 2003), and in social marketing organizations in particular (Philip Kotler and Eduardo L. Roberto 1989; Jere R. Behrman 1989).

¹¹ See also: United States Agency for International Aid. 2006. World Water Day. http://www.usaid.gov/locations/sub-saharan_africa/features/worldwaterday06.html (accessed September 22, 2006).

¹² 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 (Jerry A. Hausman 1979) and advertiser-supported media (Jukti Kumar Kalita and Robert H. Ducoffe 1995; Amil Petrin 2003). Our methods may be useful in identifying the relationship between pricing and utilization in such markets.



FIGURE 1. A BOTTLE OF CLORIN

IV present our findings on the effect of price changes on product purchase and use. Section V describes a series of robustness checks on our key conditions. Section VI develops tentative implications for pricing policy. Section VII concludes.

I. Experimental Setting and Hypotheses

Clorin is a water purification solution that is marketed in Zambia by the Society for Family Health (SFH), a local affiliate of Population Services International (PSI), an international non-profit organization.¹³ Chemically, Clorin is sodium hypochlorite bleach. It is sold by the bottle (see Figure 1), and is widely available in both retail outlets (for about 800-1,000 Zambian kwacha) and health clinics (for about 500 Kw). Clorin has been a popular product since its launch in 1998 (Lynnette Olembo et al. 2004).

Clorin is mainly intended for use as a drinking water disinfectant. When mixed with water stored in the household, Clorin kills waterborne pathogens, preventing the contraction of waterborne illnesses that are especially dangerous to young children. Many households in Zambia obtain their water from sources that are not properly chlorinated and are therefore at risk of waterborne illnesses. Alternative methods such as boiling are more expensive than Clorin per unit of water and may also be more costly in terms of time. 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). To use Clorin in drinking water, the person treating the water (usually the female head of household) must measure an appropriate dose of Clorin using the bottlecap, stir it into the water storage vessel (usually a large plastic jug), and let the vessel stand for 30 minutes before drinking the water.

Despite its advantages, not all households who buy Clorin once continue to buy it, and not all households who buy Clorin use it in their drinking water. In market research, past users of Clorin

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

identify the price, the smell and taste of Clorin, and a belief that their drinking water is currently safe as primary reasons for not currently using Clorin (Olembo et al. 2004). In our own preliminary field discussions, women often mentioned being busy or distracted after gathering water as a reason for buying Clorin and not using it in their drinking water.

Clorin that is not used in drinking water is primarily used as a household cleaner. In a small sample of in-depth interviews we conducted after our original study concluded, we found that 61 percent of Clorin-using households report using Clorin for purposes other than drinking water purification (96 percent report using it in drinking water). The most common such use was washing clothes, followed by cleaning toilets. We estimate that the average interviewee devotes 38 percent of Clorin by volume to non-drinking water uses. Here, Clorin serves as a substitute for common household bleach, which we estimate is somewhat cheaper than Clorin per unit of sodium hypochlorite.

We will use (subjective and objective) indicators of drinking-water use of Clorin at a two-week follow-up to test the effect of a change in the price of Clorin on the allocation of Clorin to drinking-water and non-drinking water uses. We will consider two mechanisms for such an effect. The first is a change in the composition of buyers. As the price rises, buyers are selected from higher and higher portions of the distribution of willingness-to-pay. If buyers with higher willingness-to-pay are more likely to use Clorin in drinking water, then we expect to see an increase in drinking-water use in response to an increase in price. Such an effect would arise, for example, if households are similar in their valuation of Clorin as a cleaner but are heterogeneous in their valuation of Clorin as a drinking-water disinfectant.

The second is a sunk-cost effect. Some psychological models of consumer behavior, such as loss aversion and aversion to regret (Thaler 1980; Eyster 2002), predict that households who have paid more for a product are more likely to use it, holding constant any effect on selection. These effects arise because a greater purchase price leads to a greater feeling of loss or regret upon a failure to use the product. Such effects would operate in our setting if, for example, households regard household cleaning as a low value use of Clorin, so that using it as a cleaner rather than in drinking water induces a feeling of regret over the initial purchase.

Because only the offer price affects the decision to buy and only the transaction price affects the regret experienced from non-drinking water use, we make the following testable predictions:

- *Screening effect*: Conditional on purchase, the probability of drinking-water use is increasing in the offer price.
- *Sunk-cost effect*: Conditional on purchase, the probability of drinking-water use is increasing in the transaction price.

In the online Appendix we illustrate these predictions formally in a model of Clorin purchase and use based on Eyster (2002).

Our ability to cleanly test these hypotheses rests on several economic assumptions. First, only the offer price affects purchase decisions, and only the transaction price induces sunk-cost effects. We maintain the former condition as part of our experimental design and discuss direct evidence for it in Section V. The latter condition is emphasized by our design and is an implication of our formal model. We discuss implications of alternative frameworks in the online Appendix.

Second, willingness-to-pay is correlated with the type of use, rather than its timing. This is important because we measure use only at a point in time, rather than in a continuous audit, so our measurement strategy has the potential to confuse delayed use with non-drinking water use. In a second follow-up conducted at a six-week horizon, we find that most households had exhausted the bottle of Clorin we sold, indicating relatively limited scope for intertemporal substitution in use. We discuss additional evidence on intertemporal substitution in Section V.

Third, we ignore informational effects of prices (Paul Milgrom and John Roberts 1986). 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 features of our design, 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 V.

Fourth, we ignore wealth effects of prices. The price of Clorin is 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.¹⁴ The fact that Clorin is a relatively inexpensive product limits the possibility that wealth effects contaminate our estimates, and we discuss additional evidence against an income-effect interpretation in Section V.

II. 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.¹⁵

A. 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 periurban areas ("compounds").¹⁶ 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 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.¹⁷

The survey interview was divided into several sections. First, we asked for a variety of basic demographic information, such as age, marital status, schooling levels, fertility history, household composition, and ownership of various durable goods (as a proxy for wealth or income). We then asked a range of questions about media exposure, malaria knowledge, and behaviors related to malaria prevention. These questions served to make the purpose of our study less transparent to the interviewee. Finally, we asked several sets of questions related to water use practices,

¹⁴ As of June 1, 2006, 800 Kw was equivalent to about USD \$0.25. Average monthly urban household income in Zambia in 2002–2003 was 790,652 Kw. United Nations Economic Commission for Africa (UNECA), 2006. "The Statistical Estimation of Poverty Duration and Transition in Zambia." Addis Ababa, Ethiopia: Economic Commission for Africa. http://www.uneca.org/eca_programmes/srdc/sa/publications/statistical-estimation.pdf (accessed September 29, 2006).

¹⁵ Our three survey instruments, and our marketing script, are available as a supplemental Appendix to this paper.

¹⁶ Within the five compounds we chose, we sampled ten 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.

¹⁷ 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.

diarrhea, soap use, attitudes toward and use of water purification techniques, access to water sources, and detailed questions on the use of Clorin.

Table A1, in the Appendix, 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 (<http://www.measuredhs.com/>). 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.

B. 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,¹⁸ which tests for both free chlorine radicals (chlorine available to kill pathogens) and total chlorine (free chlorine plus chloramines, a byproduct of chlorine combining with organic compounds).¹⁹ We focus on free chlorine because our own experimentation, as well as conversations with the manufacturer, suggest that the free chlorine measurement is more reliable and less sensitive to variation in test conditions (such as light and heat) than measurement of total chlorine.²⁰ 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.²¹

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 Amy A. 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

¹⁸ 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.

¹⁹ See chapters 13 and 14 of Barbara A. Hauser (2002) for more information on chlorine chemistry and chlorine testing.

²⁰ 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.

²¹ For reference, US 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.

TABLE 1—DISTRIBUTION OF OFFER AND TRANSACTION PRICES

	Offer price (Kw)						Total
	300	400	500	600	700	800	
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 (39.82)	90 (39.65)	90 (39.65)	90 (39.65)	90 (39.65)	50 (39.68)	500 (39.68)
100	67 (29.65)	45 (19.82)	34 (14.98)	27 (11.89)	22 (9.69)	10 (7.94)	205 (16.27)
200	69 (30.53)	46 (20.26)	34 (14.98)	27 (11.89)	23 (10.13)	11 (8.73)	210 (16.67)
300	—	46 (20.26)	34 (14.98)	28 (12.33)	23 (10.13)	11 (8.73)	142 (11.27)
400	—	—	35 (15.42)	27 (11.89)	23 (10.13)	11 (8.73)	96 (7.62)
500	—	—	—	28 (12.33)	23 (10.13)	11 (8.73)	62 (4.92)
600	—	—	—	—	23 (10.13)	11 (8.73)	34 (2.7)
700	—	—	—	—	—	11 (8.73)	11 (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.

per million or greater).²² In the baseline survey, 41 percent of the households have at least 0.1 parts per million of free chlorine in their water.

C. 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.²³ 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.²⁴

After making contact with the female head of household, the marketers followed a written script.²⁵ 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

²² 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 for details.

²³ Marketers were paid on a fixed rate per day worked.

²⁴ If the marketers found a house but there was no one home, they returned at least three times on two different days to try to contact the original respondent. If someone was home but it was not the female head of household named in the baseline survey, they made an appointment to return when the female head would be home.

²⁵ 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

of 800 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.²⁶ The respondent was given a sealed envelope, which contained a coupon offering a one-time discounted price on the bottle of Clorin.²⁷ 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.²⁸ 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.²⁹ 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 Figure A1) and requiring participants to write the transaction price on a receipt also served this purpose. We expected these measures to maximize the power of our tests for sunk-cost effects.³⁰

To minimize inference about the market price of Clorin based on the offer and transaction prices, the marketing script explicitly told respondents that Clorin was available in retail outlets for around 800 Kw.³¹ 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.³²

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

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).

²⁶ 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.

²⁷ None of the participants who were prepared to pay the initial offer price subsequently refused to buy at the discounted transaction price.

²⁸ As we report in Section V, 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.

²⁹ 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.

³⁰ 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 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 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.

³¹ Early pilot interviews suggested that most people in Lusaka are well aware of these prices.

³² 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” (Steven D. Levitt and List 2007).

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).³³ 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 percent discount, and thus had a transaction price of zero. For each offer price, we split the remaining 60 percent of households evenly among the set of transaction prices that were above zero but at least 100 Kw below the offer price. (See Table 1 for details.) So, for example, among households that were offered Clorin for 700 Kw, 40 percent were assigned a transaction price of 0 (a discount of 700 Kw), and 10 percent were assigned to a transaction price of 100, 200, 300, 400, 500, and 600 Kw (discounts of 600, 500, 400, 300, 200, and 100 Kw, 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.³⁴ At the time of randomization we used an *F*-test to verify that observable characteristics were balanced across treatments and, in a few cases, rerandomized when this was not the case.³⁵

Table A2 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.³⁶ (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.) As we show in our main tables, our key results are fully robust to controls for baseline use.

³³ 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.

³⁴ 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).

³⁵ 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.

³⁶ 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.

D. Follow-up Survey

For our follow-up survey, we sent the original survey teams to find and reinterview the households that we successfully reached for the marketing intervention.³⁷ We reinterviewed households approximately two weeks after the marketing intervention, but actual lags varied due to logistical factors.³⁸ 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.³⁹ 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). This attrition rate is high in comparison with other longitudinal studies in developing countries (Harold Alderman et al. 2001). Among households for which we obtained a cause of noncontact, the majority (68 percent) had either moved away or were temporarily away from home, with the remainder refusing interview. Duncan Thomas, Elizabeth Frankenberg, and James P. Smith (2001) report a number of steps to minimize attrition from moving, including identifying at baseline a set of individuals who are likely to know the respondent's whereabouts in the future, tracking "local" movers at the initial survey phase, and tracking more distant movers in a second survey phase. We did not implement these steps because we did not anticipate such high attrition over such a short time frame.

In the online Appendix, we show that attrition is somewhat related to covariates but is statistically unrelated to the price treatments. Consequently, David S. Lee (2009) bounds on treatment effects for contacted households are tight. Joel L. Horowitz and Charles F. Manski (2000) bounds on treatment effects for the overall population (including the uncontacted households) are much wider. An analysis of how the screening effect varies with the difficulty of contacting the household shows no reason to expect a reversal of our finding of positive screening effects in the overall population. The analysis does, however, suggest some caution regarding the power of our sunk-cost tests in the overall population.

³⁷ 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.

³⁸ 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 was permitted to accept another female adult household member as an interviewee, noting 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. When no alternate respondent was available, the surveyor recorded a recontact failure and was instructed to note, if possible, a reason for the failure.

³⁹ 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.)

E. Additional Survey Data

Our tests assume that the Clorin we sold during the marketing intervention was generally 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 interhousehold transfer or resale of bottles.⁴⁰

Our tests also assume 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.⁴¹ Consistent with the model’s assumptions, surveyor inventories conducted at 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).

To evaluate the importance of non-drinking water uses of Clorin, we conducted a series of in-depth interviews on a convenience sample of 49 Clorin-using female heads of household from four compounds in Lusaka, over six days in January and February 2008.⁴² 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.⁴³

III. 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.

⁴⁰ In the short survey we describe below, only 6 percent of households report ever giving Clorin away.

⁴¹ 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 contacted during the marketing intervention.)

⁴² 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.

⁴³ 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.

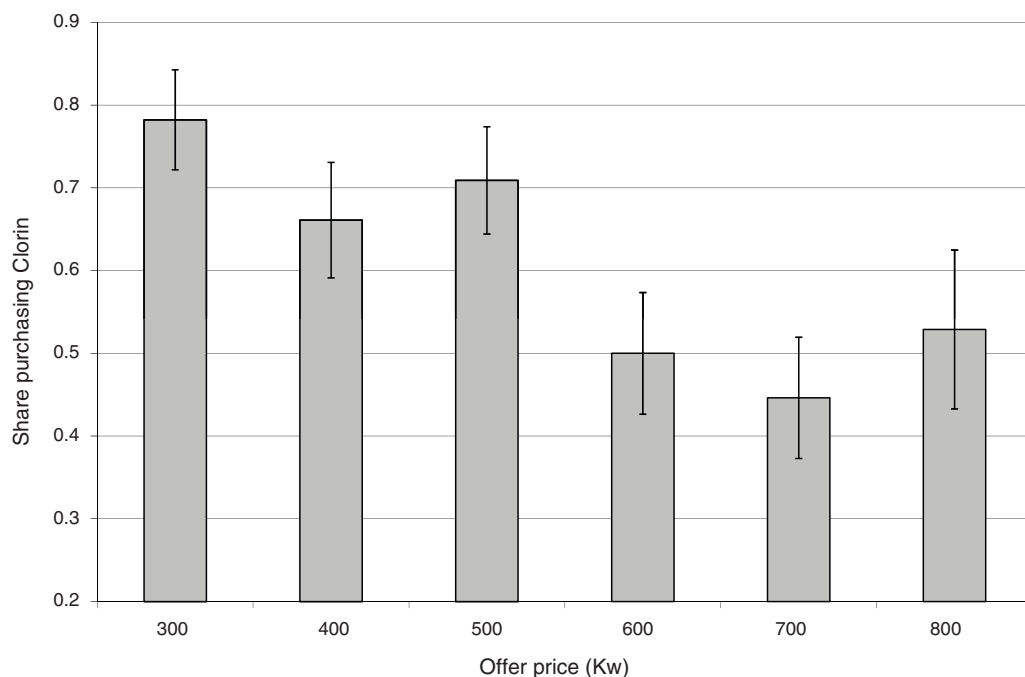


FIGURE 2. THE EFFECT OF OFFER PRICE ON PURCHASE OF CLORIN

Notes: Figure shows share of households purchasing Clorin in door-to-door marketing intervention, at different offer prices (in Zambian Kwacha). Error bars reflect ± 1.96 standard errors.

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.⁴⁴ The model implies that an increase of 100 Kw in the offer price would result in a (highly statistically significant) reduction in purchases of about 7 percentage points, or about 11 percent of the sample mean purchase probability.⁴⁵ 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.

⁴⁴ 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).

⁴⁵ 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.

TABLE 2— ESTIMATES OF THE DEMAND FOR CLORIN
 Dependent variable: Household purchased Clorin (dummy)

Sample	All (1)	All (2)	Follow-up (3)
Offer price (100 Kw)	-0.0664 (0.0093)	-0.0653 (0.0094)	-0.0708 (0.0099)
Constant	0.9640 (0.0516)	0.9578 (0.0520)	0.9892 (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 Table A2, 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 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.⁴⁶ Columns 1 and 3 show that, conditional on transaction price, an increase of 100 Kw in the offer price leads to a statistically significant increase in Clorin use among buyers of 3 to 4 percentage points, or 6 to 7 percent of mean usage.

As columns 2 and 4 show, controlling for households’ baseline characteristics tends, if anything, to strengthen these conclusions.⁴⁷ In other words, the positive selection visible in columns 1 and 3 is largely attributable to sorting on characteristics other than those we measure in our baseline controls. (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, because the sample is selected based on the endogenous decision to buy Clorin.)

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 baseline controls. 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

⁴⁶ 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 for details.

⁴⁷ Note that nine 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 reestimated the specifications in panel A of Table 3, excluding the nine observations with missing values of one or more demographic characteristics, and find virtually identical results, as expected.

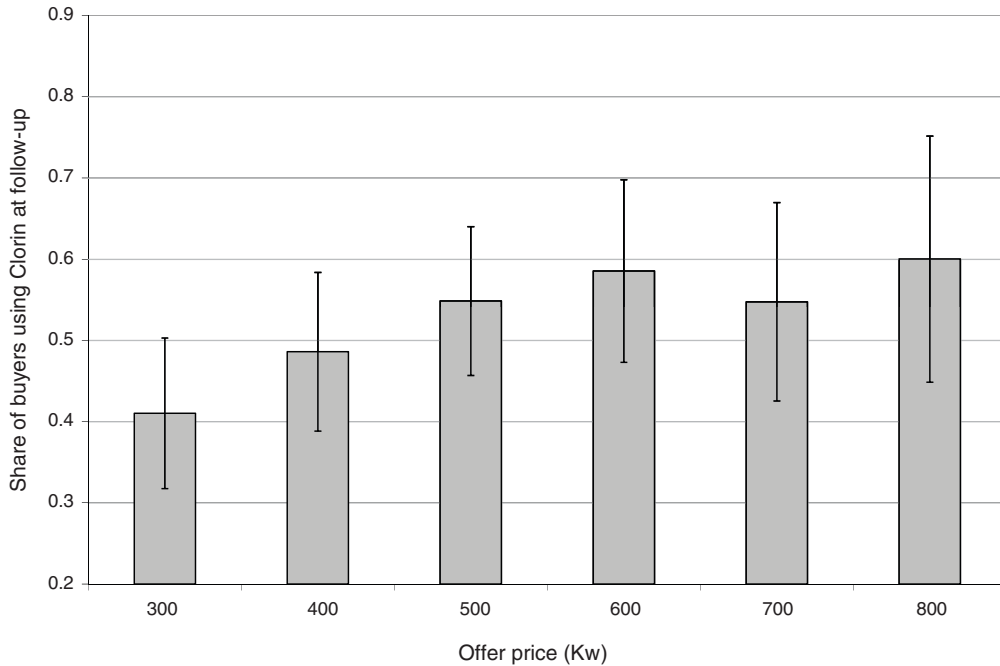


FIGURE 3. USAGE RATES OF CLORIN BY OFFER PRICE

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 ± 1.96 standard errors.

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. Figure A2 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 twice as large as 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 five 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.

TABLE 3—EVIDENCE ON SCREENING EFFECTS

Dependent variable	Water currently treated with Clorin? (follow-up; self-reported)		Drinking water contains free chlorine? (follow-up; measured)	
	(1)	(2)	(3)	(4)
Offer price (100 Kw)	0.0373 (0.0149)	0.0388 (0.0150)	0.0321 (0.0150)	0.0397 (0.0149)
Transaction price fixed effects?	Yes	Yes	Yes	Yes
Baseline controls?	No	Yes	No	Yes
Sample mean of dependent variable	0.5147	0.5140	0.5332	0.5366
Number of observations	546	537	542	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 controls” includes baseline Clorin usage and water chlorination, general health behaviors and attitudes, household demographics, and locality fixed effects, as in Table A2. 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.

IV. Evidence on Sunk-Cost Effects

In this section, we use variation in the transaction price to test for a sunk-cost effect.

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 II) 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:⁴⁸

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

Participants were able to answer yes or no and could provide additional comments if they liked. After this question, we asked two similar follow-up questions of all participants, one for the case of a 5,000 Kw bottle of juice, and one for the case of a 500 Kw bottle.⁴⁹ Consistent with

⁴⁸ 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.

⁴⁹ 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?”

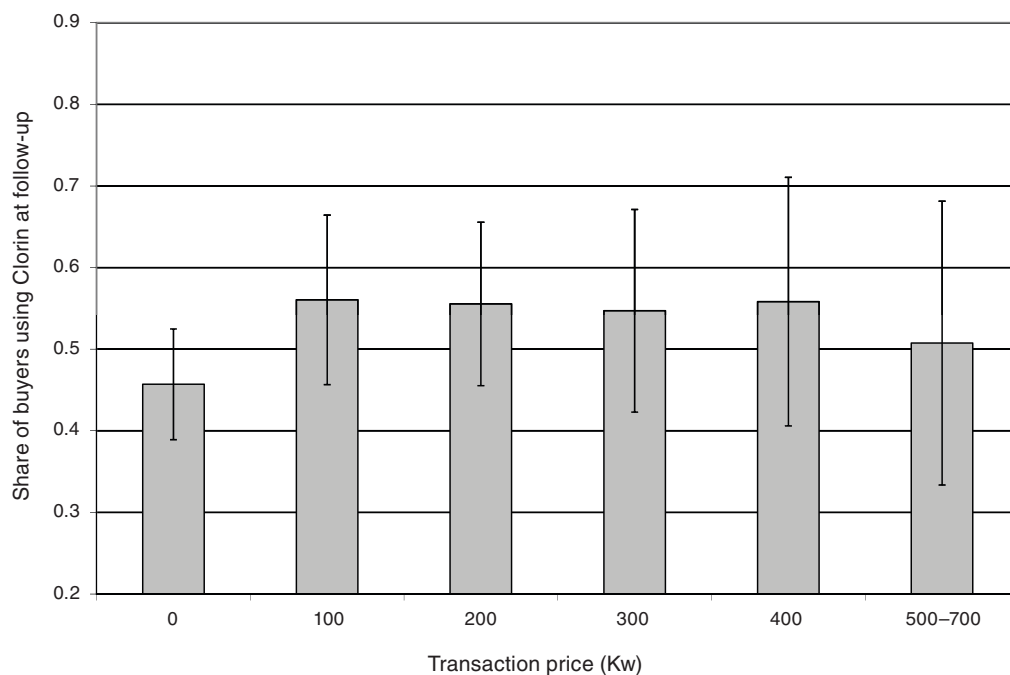


FIGURE 4. USAGE RATES OF CLORIN BY TRANSACTION PRICE

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 ± 1.96 standard errors.

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.⁵⁰

Panel A of Table 4 tests 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 3.⁵¹

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.

⁵⁰ 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.

⁵¹ 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).

TABLE 4—EVIDENCE ON SUNK-COST EFFECTS

<i>Panel A: Tests for sunk-cost effect</i>						
Dependent variable	Water currently treated with Clorin? (follow-up; self-reported)			Drinking water contains free chlorine? (follow-up; measured)		
	Sunk-cost household?			Sunk-cost household?		
Sample	All	Yes	No	All	Yes	No
	(1A)	(2A)	(3A)	(4A)	(5A)	(6A)
Transaction price (100 Kw)	0.0097 (0.0133)	0.0348 (0.0334)	0.0042 (0.0149)	-0.0071 (0.0133)	-0.0106 (0.0330)	-0.0079 (0.0147)
<i>Difference</i> (sunk cost versus non-sunk cost)		0.0306 (0.0366)			-0.0027 (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
Observations	537	113	424	533	112	421
<i>Panel B: Tests for effect of act of paying</i>						
Dependent variable	Water currently treated with Clorin? (follow-up; self-reported)			Drinking water contains free chlorine? (follow-up; measured)		
	Sunk-cost household?			Sunk-cost household?		
Sample	All	Yes	No	All	Yes	No
	(1B)	(2B)	(3B)	(4B)	(5B)	(6B)
Transaction price > 0	0.0565 (0.0442)	0.1840 (0.1030)	0.0372 (0.0496)	0.0318 (0.0440)	0.0816 (0.1020)	0.0240 (0.0493)
<i>Difference</i> (sunk cost versus non-sunk cost)		0.1468 (0.1144)			0.0576 (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
Observations	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 Table A2. 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.

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: that paying something results in more drinking-water use than paying nothing. 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.⁵²

V. 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).⁵³

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 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.—Quality inference from prices could confound our tests. Most plausibly, if households perceive higher prices to be evidence of greater efficacy, higher prices could induce greater use in drinking water, resulting in overestimates of both screening and sunk-cost effects.

To test for such effects, 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

⁵² 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).

⁵³ 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.

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.⁵⁴ Moreover, controlling for this index in our main specifications leaves our key conclusions unchanged (see Appendix).

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.⁵⁵

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

Heterogeneity in timing versus composition of use.—An alternative interpretation of the screening effect we estimate is that higher willingness-to-pay is associated with *earlier*, rather than greater, use in drinking water. On that view, households with higher willingness-to-pay should exhaust their bottle of Clorin sooner, and the screening effect should be weaker the later we measure Clorin use, eventually becoming negative (as lower willingness-to-pay households use up the Clorin whose use they delayed).

We test these predictions in columns 1 through 3 of Table 5 using data from our second follow-up survey.⁵⁶ Column 1 shows that the likelihood of having exhausted the bottle as of the second follow-up is statistically unrelated to the offer price. Columns 2 and 3 show that among those who have not exhausted the bottle we sold as of the second follow-up, there is no evidence of a reversal of the screening effect.⁵⁷ The coefficients we estimate are positive and somewhat larger than those in our baseline estimates, though the standard errors are large due to small sample size.⁵⁸

⁵⁴ 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 ($p = 0.089$).

⁵⁵ 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.

⁵⁶ Note that 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 a need for caution in interpreting the estimates in Table 5.

⁵⁷ We conduct these tests on the sample of households who have not exhausted the bottle we sold because for households that have exhausted that bottle, the purchase decision is governed not by the offer price but by the retail price of Clorin. We show in the online Appendix that this implies sharply different predictions about the relationship between use and offer price for such households.

⁵⁸ Any heterogeneity in the timing of use correlated with willingness-to-pay must therefore arise prior to the six-week follow-up. Although by definition we cannot measure use between our first and second follow-up surveys, survey logistics mean that some households were reached sooner after purchase than others. We have estimated screening effects separately for households reached with above- and below-median delay between purchase and follow-up (results

TABLE 5—HETEROGENEITY IN TIMING VERSUS COMPOSITION OF USE

Dependent variable	Bottle exhausted?	Water currently treated with Clorin?	Drinking water contains free chlorine?	Use Clorin for non-drinking water purposes	
Survey Measure type	Second follow-up Measured	Second follow-up Self-reported	Second follow-up Measured	Follow-up Self-reported	Follow-up Self-reported
Sample	All (1)	Not exhausted (2)	Not exhausted (3)	All (4)	Exhausted (5)
Offer price (100 Kw)	-0.0071 (0.0125)	0.0401 (0.0301)	0.0430 (0.0297)	-0.0136 (0.0132)	-0.0278 (0.0164)
Transaction price fixed effects?	Yes	Yes	Yes	Yes	Yes
Sample mean of dep. var.	0.8162	0.4427	0.4122	0.2385	0.2550
Observations	468	131	131	520	349

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

Columns 4 and 5 of Table 5 test directly for heterogeneity in the composition of use by relating offer price to the household's answer to the follow-up survey question "Do you use Clorin for any purpose other than purification of drinking water?"⁵⁹ Column 4 of Table 5 shows an economically nontrivial but statistically insignificant negative effect: each 100 Kw increase in the offer price reduces the likelihood of non-drinking water use by 1.4 percentage points (as against a sample mean of 24 percentage points). Column 5 shows a larger and marginally significant effect for those who had exhausted the bottle we sold before the second follow up (for whom the follow-up survey question is most likely to reference use of the bottle we sold).

To summarize, Table 5 shows no evidence that the rate of use is related to willingness-to-pay, and some suggestive evidence of a direct connection between willingness-to-pay and the composition of use. However, we stress that our evidence on the relationship between willingness-to-pay and the composition of use is imprecise and not conclusive, and that we do not have the statistical power to rule out some relationship between willingness-to-pay and the timing of use.

VI. Implications for Pricing Policy

The economic rationale for subsidizing Clorin rests on health externalities generated by Clorin use. These, in turn, depend on how Clorin is used. In this section we present a simple model, following Cohen and Dupas (forthcoming), to illustrate how our estimates of the screening effect influence the optimal subsidy to Clorin.

Suppose that every bottle of Clorin purchased produces a health externality that is not internalized by the buyer. The externality is produced only if Clorin is used in drinking water. Finally, suppose that Clorin is produced at constant marginal cost and that utility is quasilinear in money.

Under these assumptions, the optimal Pigouvian (Arthur C. Pigou 1952) subsidy to Clorin is equal to the size of the health externality created by the *marginal* bottle of Clorin purchased at the subsidized price. The health externality is proportional to the share of the marginal bottle

not shown). We find positive point estimates for both groups, and if anything we find that the screening effect is more pronounced for households visited later.

⁵⁹ Note that this question is poorly worded in that it does not specify a time frame. (By contrast, our question about drinking-water use is specific to the present.) Also, unlike drinking-water use, we have no "objective" (non-self-reported) measure of non-drinking water use.

that is used in drinking water. It is the marginal bottle that matters, because it is that bottle that is purchased if the subsidy is increased. When the screening effect is present, the marginal buyer is less likely to use in drinking water than the average buyer, resulting in a lower optimal subsidy than would obtain if the marginal buyer were identical to the average buyer.

Imagine a market with price p . An analyst with data on the share of households $b(p)$ who buy at that price and the share of buyers $s(p)$ who use Clorin in drinking water cannot directly measure the use behavior of the marginal buyer. Rather, $s(p)$ measures the usage behavior of the average buyer, which the screening effect predicts is above that of the marginal buyer.

Our price experiment makes it possible to compare the use behavior of the marginal buyer to that of the average buyer. This, in turn, tells us, for a given level of externality, how much the optimal subsidy would change from incorporating the presence of heterogeneity among households.

In particular, the ratio of the drinking-water use rate of the marginal buyer to that of the average buyer tells us how much the optimal subsidy is reduced by incorporating screening effects. This ratio, denoted by λ , is given by

$$\lambda = 1 - \frac{s'(p)/s(p)}{-b'(p)/b(p)}.$$

When the screening effect holds, average use is rising in the market price, so $s'(p) > 0$ and $\lambda < 1$.

The ratio λ can be computed directly from our estimates. The numerator is the screening coefficient we estimate in Table 3 normalized by the average rate of drinking-water use. The denominator is the price coefficient we estimate in Table 2 normalized by the mean purchase rate.

At our point estimates, λ is 0.33 or 0.45 for self-reported and measured use, respectively. These values can be interpreted to mean that the optimal subsidy is half as large as it would be absent the screening effect, i.e., if marginal and average use were identical. Put differently, the magnitude of the per-drinking-water-use externality needed to justify a given subsidy is more than twice as large as it would be if we ignored the screening effect. We stress, however, that these estimates involve nonlinear combinations of our parameters with nontrivial resulting statistical uncertainty: bootstrapped standard errors are 0.29 and 0.28 for the two values, respectively.

Several comments are in order regarding the applicability of these estimates outside the experimental context. First, they are intrinsically local to the price variation we induce in the experiment. Hence, they are relevant to evaluating the optimality of the existing level of subsidy, but not necessarily to determining the globally optimal subsidy over the full range of possibilities. Most important, we do not include a treatment with free distribution in our experiment. Although the screening effect we estimate appears to be approximately monotonic over the range of experimental offer prices, there is no guarantee that the selection of buyers at the margin would behave similarly at prices at or nearer to zero.

Second, the experiment induced variation in door-to-door prices, with prices at retail stores and health clinics held constant. This means that some of the additional buyers we attract by lowering prices are households who otherwise would have bought Clorin in retail stores and health clinics. The effect of prices on the selection of buyers could therefore be different in an experiment in which prices are varied at the market level, across all channels. We show in the online Appendix, however, that our screening effects are, if anything, stronger among households that were not using Clorin as of our baseline survey, which might be regarded as the households most likely to substitute from retail or other channels.

Third, the price variation induced in the experiment is short run. If we experimentally varied long-run prices, the changes in composition of uses we identify would be attributable to two sources: changes in the composition of households that choose to buy Clorin (the screening effect), and changes in how existing buyers choose to use the Clorin they buy. The latter effect

arises because the shadow cost of acquiring Clorin in the future is pegged to the future market price. In our study this is held constant, so our estimates isolate the first channel. In a change in policy, the second channel could also be important, and our data do not speak to the direction or magnitude of the shadow cost effect.

Fourth, we focus on only one behavioral channel—substitution between uses—by which the effect of the marginal bottle of Clorin could differ from the effect of the average bottle sold. The presence of additional substitutes or complements to Clorin would further complicate this picture. For example, if households regard Clorin as a substitute for boiling water or other treatment methods, then a complete analysis of the health externality of the marginal bottle of Clorin requires estimates of the extent to which that bottle crowds out these other treatment methods. Although Clorin has been shown to reduce the incidence of waterborne illnesses (Quick et al. 2002) even in populations in which the use of other treatment methods is reasonably common (Quick et al. 1999), an ideal analysis of the optimal subsidy to Clorin would include a direct estimate of the health effect of the marginal bottle sold, thus incorporating all behavioral responses to a greater subsidy.

The analysis above applies to pricing of Clorin. A more challenging question is how our results apply to the pricing of other health inputs sold by NGOs in the developing world. Although our study contains no direct evidence on these questions, it is possible to identify some key factors that differ across products and could affect the way pricing affects the selection of buyers.

The first factor is the up-front cost of the product. Clorin is relatively inexpensive, which is convenient because it mitigates concerns about income effects confounding our findings. However, this factor may also increase the degree to which households select on use intentions, because households do not differ much in whether they have the cash on hand with which to buy Clorin. For a product with greater up-front cost, heterogeneity in willingness-to-pay may result from heterogeneity in liquidity or credit constraints more than heterogeneity in use intentions. If these two factors are not highly correlated, selection effects of the kind we see would be weaker for products with greater up-front costs.

The second factor is the availability of nonhealth uses of the product. We interpret our findings as evidence that the likelihood that a household uses Clorin for non-drinking water (i.e., household cleaning) purposes is related to its willingness-to-pay. Such factors would clearly be less important for a product with fewer nonhealth uses. If nonhealth uses arise because households are uninformed about a product's health uses, such effects might also diminish over time as information improves, in which case screening effects would be mitigated as the market matures.

This second factor may help to explain differences between our findings and those of Cohen and Dupas (2010), who use a two-stage pricing design to study the effect of prices on the purchase and use of insecticide-treated mosquito nets in Kenya. Consistent with our results, Cohen and Dupas (2010) find little evidence of sunk-cost effects. However, they also find no evidence for screening effects, despite strong evidence that higher prices reduce purchases, and they estimate that λ is approximately 1. Although these differences could be attributable to differences in context or methodology, an economically richer account is that, relative to Clorin, insecticide-treated nets are less amenable to nonhealth uses. While there have been accounts of nets being used for nonhealth purposes such as fishing (see, e.g., William Easterly 2006), Cohen and Dupas (2010) find no evidence for such uses: in their study the most common causes of nonuse are waiting for another net to wear out and waiting for the birth of a child.

VII. 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. 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 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. In particular, we find that the difference in use propensities between the marginal and average user of Clorin is substantial enough, at our point estimates, to have important implications for pricing policy. As we have stressed, however, these conclusions rest on strong assumptions about the generalizability of our findings outside the experimental context.

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. One potentially important factor is the role of price as a carrier of information. For a product newly introduced on the market, higher prices might, in equilibrium, signal higher quality or efficacy (Milgrom and Roberts 1986). In this case, higher prices could increase (or change) use through a higher perception of quality. An NGO interested in fostering high levels of use might, in such an environment, attempt to charge high prices initially to highly informed segments of the population, to allow other segments to observe high willingness-to-pay among the informed. Of course, this proposal is speculative, and we are not aware of a complete theoretical analysis of optimal Pigouvian prices in the presence of uncertainty over product quality. Carefully evaluating the role of prices as a carrier of information therefore remains an important area for future research.

APPENDIX: ADDITIONAL ROBUSTNESS CHECKS

Table A3 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 on use among buyers. In specification (1), we reproduce the coefficients from our main tables for comparison.

Specification (2) of Table A3 checks the robustness of our results to using a probit model of purchase and use, rather than a linear probability model. The table reports the estimated marginal effects evaluated at the sample mean of the covariates. In all cases these estimates are very similar to those we obtain in our main specification.

Specification (3) of Table A3 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 Table A3, 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 reestimated our key models of use, allowing the effect of



FIGURE A1. SAMPLE COUPON FROM DOOR-TO-DOOR MARKETING

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

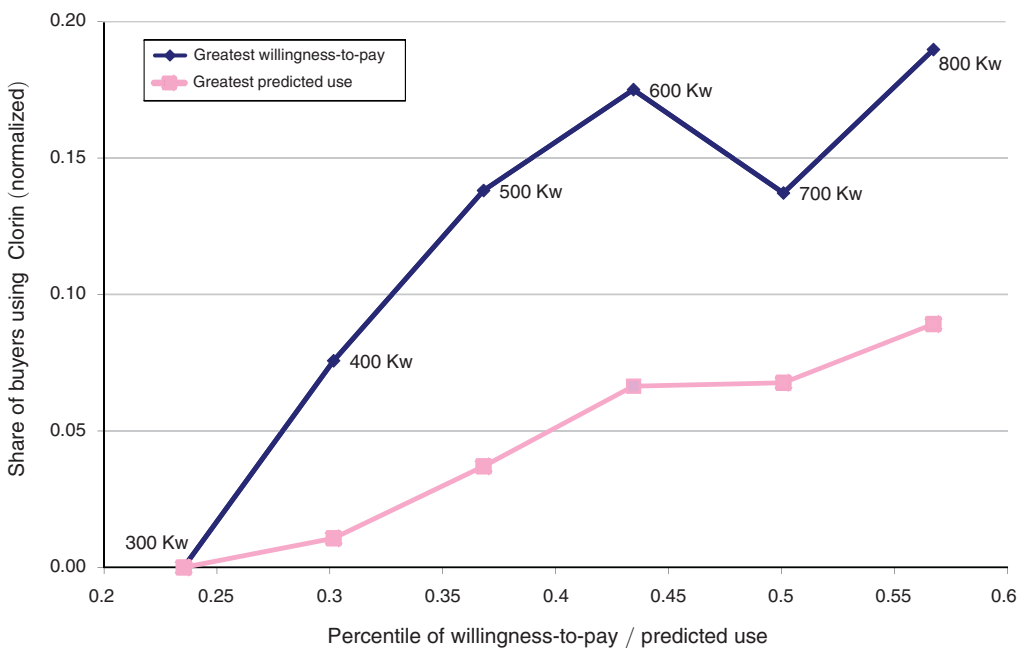


FIGURE A2. PREDICTING USE: WILLINGNESS-TO-PAY VERSUS DEMOGRAPHICS

Notes: Top (dark) 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 (lighter) 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 controls (baseline Clorin usage and water chlorination, general health behaviors and attitudes, household demographics, and locality fixed effects, as in Table A2). Share of use at lowest percentile is normalized to 0.

TABLE A1—DEMOGRAPHIC CHARACTERISTICS OF THE BASELINE SAMPLE

Source Sample	Baseline survey All (1)	Baseline survey Ages 15–49 (2)	DHS Ages 15–49 (3)
Age	32.8257 (0.3130)	30.1593 (0.2254)	27.1425 (0.2948)
Years of completed schooling	6.6418 (0.1013)	7.0285 (0.1013)	7.2379 (0.1209)
Married?	0.8000 (0.0113)	0.8327 (0.0111)	0.5642 (0.0170)
Currently pregnant?	0.1143 (0.0090)	0.1254 (0.0099)	0.0754 (0.0091)
Total number of living children	3.1867 (0.0630)	2.9484 (0.0614)	2.1932 (0.0791)
Number of children in household under age 5	0.9619 (0.0245)	0.9875 (0.0253)	1.1767 (0.0365)
Household owns a radio?	0.5540 (0.0140)	0.5721 (0.0148)	0.6266 (0.0166)
Household owns a television?	0.4992 (0.0141)	0.5151 (0.0149)	0.5501 (0.0171)
Household owns a refrigerator?	0.1905 (0.0111)	0.1940 (0.0118)	0.2686 (0.0152)
Household owns a bicycle?	0.1000 (0.0085)	0.1077 (0.0092)	0.1213 (0.0112)
Household owns a motorcycle?	0.0008 (0.0008)	0.0009 (0.0009)	0.0012 (0.0012)
Household owns a car?	0.0230 (0.0042)	0.0258 (0.0047)	0.0836 (0.0095)
Number of observations	1,260	1,124	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.

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 Table A3, we control explicitly for an index of the respondent's self-reported attitudes toward Clorin at follow-up (see Section V). 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 Table A3 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.

TABLE A2—TESTING THE BALANCE OF OBSERVABLES ACROSS TREATMENT CONDITIONS

Sample <i>Dependent variable</i>	All <i>Offer price</i> (1)	Marketing <i>Offer price</i> (2)	Purchased Clorin <i>Transaction price</i> (3) <i>Transaction price > 0</i> (4)	
Water currently treated with Clorin? (baseline; self-reported)	0.1474 (0.1114)	0.2040 (0.1250)	-0.1171 (0.1747)	0.0668 (0.0525)
Drinking water contains free chlorine? (baseline; measured)	0.0764 (0.0892)	0.0150 (0.1003)	0.3300 (0.1412)	0.0643 (0.0425)
Use of soap before handling food (index)	0.0032 (0.1546)	-0.0881 (0.1735)	0.2281 (0.2519)	0.0860 (0.0757)
Use of soap after using toilet (index)	-0.3067 (0.1593)	-0.1992 (0.1782)	0.0863 (0.2564)	-0.0192 (0.0771)
Attitude toward water purification (index)	-0.0828 (0.2258)	-0.3628 (0.2531)	0.5490 (0.3564)	0.0645 (0.1071)
Age in years	0.0032 (0.0046)	0.0023 (0.0052)	-0.0002 (0.0076)	-0.0010 (0.0023)
Ever attended school?	-0.0986 (0.1830)	-0.1235 (0.2050)	0.2510 (0.2874)	0.1501 (0.0864)
Years of completed schooling	0.0097 (0.0189)	0.0187 (0.0215)	-0.0352 (0.0305)	-0.0157 (0.0092)
Currently married?	0.0870 (0.1160)	0.0381 (0.1327)	-0.1274 (0.1881)	0.0416 (0.0565)
Currently pregnant?	-0.0118 (0.1355)	0.0768 (0.1550)	-0.0400 (0.2222)	-0.0037 (0.0668)
Ever given birth to any children?	-0.1571 (0.1806)	-0.1471 (0.2065)	0.2126 (0.2913)	-0.0410 (0.0876)
No. of children in household under age 5	0.0474 (0.0536)	0.0596 (0.0609)	0.0904 (0.0918)	0.0381 (0.0276)
No. of people in household	-0.0196 (0.0193)	-0.0106 (0.0214)	-0.0377 (0.0298)	-0.0042 (0.0090)
Share of durables owned	0.1286 (0.2885)	0.0603 (0.3265)	0.2612 (0.4499)	0.0100 (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
<i>F</i> -test that all coefficients are 0	0.64	0.64	0.90	0.93
<i>p</i> -value of <i>F</i> -test	0.8719	0.8686	0.5802	0.5395
Number of observations	1,244	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.

Specification (7) of Table A3 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.⁶⁰ 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.)

⁶⁰ 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.

TABLE A3—ADDITIONAL ROBUSTNESS CHECKS

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

Notes: Standard errors in parentheses. See Appendix 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 Table A2).

REFERENCES

- Alderman, Harold, Jere R. Behrman, Hans-Peter Kohler, John A. Maluccio, and Susan Cotts Watkins. 2001. "Attrition in Longitudinal Household Survey Data: Some Tests for Three Developing-Country Samples." *Demographic Research*, 5(4): 79–124.
- Arkes, Hal R., and Catherine Blumer. 1985. "The Psychology of Sunk Cost." *Organizational Behavior and Human Decision Processes*, 35(1): 124–40.
- Ashraf, Nava, Dean Karlan, and Wesley Yin. 2006. "Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines." *Quarterly Journal of Economics*, 121(2): 635–72.
- Behrman, Jere R. 1989. "The Simple Analytics of Contraceptive Social Marketing." *World Development*, 17(10): 1499–1521.
- Casper, Cheryl A. 1979. "Pricing Policy for Library Services." *Journal of the American Society for Information Science*, 30(5): 304–309.
- Cohen, Jessica, and Pascaline Dupas. 2010. "Free Distribution or Cost-Sharing? Evidence from a Randomized Malaria Prevention Experiment." *Quarterly Journal of Economics*, 125(1): 1–45.
- Easterly, William. 2006. *The White Man's Burden: Why the West's Efforts to Aid the Rest Have Done So Much Ill and So Little Good*. New York: Penguin.
- Eyster, Erik. 2002. "Rationalizing the Past: A Taste for Consistency." <http://www.nuffield.ox.ac.uk/Users/Eyster/papers/rationalize.pdf>.
- Fehr, Ernst, and Lorenz Goette. 2007. "Do Workers Work More If Wages Are High? Evidence from a Randomized Field Experiment." *American Economic Review*, 97(1): 298–317.

- Finkelstein, Amy, and Kathleen McGarry. 2006. "Multiple Dimensions of Private Information: Evidence from the Long-Term Care Insurance Market." *American Economic Review*, 96(4): 938–58.
- Friedman, Daniel, Kai Pommerenke, Rajan Lukose, Garrett Milam, and Bernardo A. Huberman. 2007. "Searching for the Sunk Cost Fallacy." *Experimental Economics*, 10(1): 79–104.
- Gourville, John, and Dilip Soman. 2002. "Pricing and the Psychology of Consumption." *Harvard Business Review*, 80(9): 90–96.
- Grossman, Michael. 1972. "On the Concept of Health Capital and the Demand for Health." *Journal of Political Economy*, 80(2): 223–55.
- Harrison, Glenn W., and John A. List. 2004. "Field Experiments." *Journal of Economic Literature*, 42(4): 1009–55.
- Hauser, Barbara A. 2002. *Drinking Water Chemistry: A Laboratory Manual*. Boca Raton, FL: CRC Press.
- Hausman, Jerry A. 1979. "Individual Discount Rates and the Purchase and Utilization of Energy-Using Durables." *Bell Journal of Economics*, 10(1): 33–54.
- Horowitz, Joel L., and Charles F. Manski. 2000. "Nonparametric Analysis of Randomized Experiments with Missing Covariate and Outcome Data." *Journal of the American Statistical Association*, 95(449): 77–84.
- Kalita, Jukti Kumar, and Robert H. Ducoffe. 1995. "A Simultaneous-Equation Analysis of Pricing, Circulation, and Advertising Revenue for Leading Consumer Magazines." *Journal of Media Economics*, 8(4): 1–16.
- Karlan, Dean S. 2005. "Using Experimental Economics to Measure Social Capital and Predict Financial Decisions." *American Economic Review*, 95(5): 1688–99.
- Karlan, Dean, and John A. List. 2007. "Does Price Matter in Charitable Giving? Evidence from a Large-Scale Natural Field Experiment." *American Economic Review*, 97(5): 1774–93.
- Karlan, Dean, and Jonathan Zinman. 2009. "Observing Unobservables: Identifying Information Asymmetries with a Consumer Credit Field Experiment." *Econometrica*, 77(6): 1993–2008.
- Kotler, Philip, and Eduardo L. Roberto. 1989. *Social Marketing: Strategies for Changing Public Behavior*. New York: Free Press.
- Kremer, Michael, and Edward Miguel. 2007. "The Illusion of Sustainability." *Quarterly Journal of Economics*, 122(3): 1007–65.
- Kremer, Michael, Jessica Leino, Edward Miguel, and Alix Peterson Zwane. 2009. "Spring Cleaning: Rural Water Impacts, Valuations, and Institutions." http://www.economics.harvard.edu/faculty/Kremer/files/SIP_2009.03.08-EM-CLEAN.pdf.
- Lee, David S. 2009. "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects." *Review of Economic Studies*, 76(3): 1071–1102.
- Levitt, Steven D., and John A. List. 2007. "What Do Laboratory Experiments Measuring Social Preferences Reveal About the Real World?" *Journal of Economic Perspectives*, 21(2): 153–74.
- Litvack, Jennie I., and Claude Bodart. 1993. "User Fees Plus Quality Equals Improved Access to Health Care: Results of a Field Experiment in Cameroon." *Social Science & Medicine*, 37(3): 369–83.
- Maxwell, C. A., R. T. Rwegoshora, S. M. Magesa, and C. F. Curtis. 2006. "Comparison of Coverage with Insecticide-Treated Nets in a Tanzanian Town and Villages Where Nets and Insecticides Are Either Marketed or Provided Free of Charge." *Malaria Journal*, 5(44): 1–6.
- Meekers, Dominique. 1997. "The Implications of Free and Commercial Distribution for Condom Use: Evidence from Cameroon." Population Services International Research Division Working Paper 9.
- Milgrom, Paul, and John Roberts. 1986. "Price and Advertising Signals of Product Quality." *Journal of Political Economy*, 94(4): 796–821.
- Newhouse, Joseph P. 1970. "Toward a Theory of Nonprofit Institutions: An Economic Model of a Hospital." *American Economic Review*, 60(1): 64–74.
- Olembo, Lynnette, Fred A. D. Kaona, Mary Tuba, and Gilbert Burnham. 2004. "Safe Water Systems: An Evaluation of the Zambia CLORIN Program." <http://www.ehproject.org/pdf/others/zambia%20report%20format.pdf>.
- Oster, Sharon M. 1995. *Strategic Management for Nonprofit Organizations: Theory and Cases*. New York: Oxford University Press.
- Oster, Sharon M., Charles M. Gray, and Charles Weinberg. 2003. "Pricing in the Nonprofit Sector." In *Effective Economic Decision-Making by Nonprofit Organizations*, ed. Dennis R. Young, 27–46. New York: Foundation Center.
- Parker, Amy A., R. Stephenson, P. L. Riley, S. Ombeki, C. Komolleh, L. Sibley, and Robert Quick. 2006. "Sustained High Levels of Stored Drinking Water Treatment and Retention of Hand-Washing Knowledge in Rural Kenyan Households Following a Clinic-Based Intervention." *Epidemiology and Infection*, 134(5): 1029–36.

- Petrin, Amil.** 2003. "Consistent Policy Analysis When Some Heterogeneity is Unobserved." Unpublished.
- Pigou, Arthur Cecil.** 1952. *The Economics of Warfare*. London: MacMillan and Co.
- Quick, Robert, Kimura, Angelica Thevos, Mathias Tembo, I. Shamputa, L. Hutwagner, and Eric Mintz.** 2002. "Diarrhea Prevention through Household-Level Water Disinfection and Safe Storage in Zambia." *American Journal of Tropical Medicine and Hygiene*, 66(5): 584–9.
- Quick, Robert, L. Venczel, Eric Mintz, L. Soletto, J. Aparicio, M. Gironaz, L. Hutwagner, et al.** 1999. "Diarrhea Prevention in Bolivia through Point-Of-Use Water Treatment and Safe Storage: a Promising New Strategy." *Epidemiology and Infection*, 122(1): 83–90.
- Roy, A. D.** 1951. "Some Thoughts on the Distribution of Earnings." *Oxford Economic Papers*, 3(2): 135–46.
- Shampanier, Kristina, Nina Mazar, and Dan Ariely.** 2007. "Zero as a Special Price: The True Value of Free Products." *Marketing Science*, 26(6): 742–57.
- Shiv, Baba, Ziv Carmon, and Dan Ariely.** 2005. "Placebo Effects of Marketing Actions: Consumers May Get What They Pay for." *Journal of Marketing Research*, 42(4): 383–93.
- Steinberg, Richard, and Burton A. Weisbrod.** 1998. "Pricing and Rationing by Nonprofit Organizations with Distributional Objectives." *To Profit or Not to Profit: The Commercial Transformation of the Non-profit Sector*, ed. Burton A. Weisbrod, 64–82. Cambridge, UK: Cambridge University Press.
- Thaler, Richard.** 1980. "Toward a Positive Theory of Consumer Choice." *Journal of Economic Behavior & Organization*, 1(1): 39–60.
- Thevos, Angelica, Sonja J. Olsen, Josefa M. Rangel, Fred A. D. Kaona, Mathias Tembo, and Robert Quick.** 2002. "Social Marketing and Motivational Interviewing as Community Interventions for Safe Water Behaviors: Follow-Up Surveys in Zambia." *International Quarterly of Community Health Education*, 21(1): 51–65.
- Thomas, Duncan, Elizabeth Frankenberg, and James P. Smith.** 2001. "Lost But Not Forgotten: Attrition and Follow-Up in the Indonesia Family Life Survey." *Journal of Human Resources*, 36(3): 556–92.
- Thornton, Rebecca L.** 2008. "The Demand for, and Impact of, Learning HIV Status." *American Economic Review*, 98(5): 1829–63.