Efficient pricing and inefficient households: Water use and intrahousehold externalities*

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Abstract

When consumption generates negative externalities, raising the price to reflect the social cost of consumption is the preferred policy solution. In some cases – for example, household water and electricity use – consumption is susceptible to a second externality problem: each individual enjoys the private benefits of consumption but shares the costs with other household members, leading to overconsumption even from the household's viewpoint. We test the prediction that intrahousehold inefficiency dampens price sensitivity in the context of water use in Zambia, combining billing records, randomized price variation, and lab-experimental measures of intrahousehold efficiency. We find that households with above-median efficiency have a short-run price elasticity of -0.49, while those with below-median efficiency have an elasticity of -0.17. These results suggest that the required Pigouvian price when own and others' consumption is difficult to observe, yet usage is billed at the household level, will need to be set to correct both the environmental and the intrahousehold externalities. Alternative policies such as price incentives targeted toward the primary water-using household members or access to real-time data on household water consumption (which would improve the enforceability of intrahousehold agreements) could also be useful.

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1 Introduction

The standard policy prescription for managing consumption externalities is to change prices to internalize the externality (through a Pigouvian tax, for example). The effectiveness of the pricing approach depends on consumers observing and responding to the price. However, when "the consumer" is a group of individuals, often a household, a second externality problem may arise: each individual enjoys the private benefits of consumption but shares the costs with other members of the group, which means that households might overconsume even relative to their first best. The empirical implication is that households in which individuals fail to internalize the payoffs of other members of the household are less price sensitive. The price insensitivity of these households will drive up the required Pigouvian price.

We test the relationship between the degree to which individuals within the household internalize the utility of other members of the household – which we refer to as intrahousehold efficiency – and price sensitivity in the context of water usage in Livingstone, Zambia. We combine monthly billing data for 1,282 customers of the regional water utility with lab-experimental measures of intrahousehold cooperation and a randomized intervention that generates exogenous variation in prices. We separately survey both members of married couples that receive monthly water bills reflecting household consumption. As part of the survey, each respondent plays a modified dictator game with his or her spouse, which provides an incentive compatible measure of intrahousehold efficiency. In addition, randomly selected households are given a financial incentive to conserve water. If household water use, as measured by the monthly bill, falls below a specified consumption target (that we set at roughly 30% below its recent consumption), the household is entered into a lottery with 1 in 20 odds of a substantial payout. This financial incentive to reduce consumption is akin to raising the price of water over some range of consumption. The incentive treatment is explained to the couple together in one-third of the treated households, and to the man or woman alone in each of the other thirds. We follow households for three to nine months after treatment and observe both consumption and bill payment behavior.

In a simple household model, adapted from a non-cooperative household decision framework, individuals within the household make water use decisions taking others' decisions as given. Because the individual retains the full private benefits of consumption while sharing the costs (via the price paid by the household) with the rest of the household, consumption will tend to be too high. We introduce heterogeneity across individuals and households by allowing each decision maker to internalize some share of others' payoffs. In the limit, each member of the household fully internalizes the payoffs to others, and the household and

individual optimum converge. The model, along with our empirical set up, is agnostic about whether individuals internalize each other's payoffs because of altruism or monitoring and enforcement. Our set up generates three key predictions that we test with our data. First, we predict that more efficient households will be more price sensitive. Second, a change in individual-level prices within the home will result in a greater consumption response at the household level if the price change is directed to the individual who is not otherwise the residual claimant on the bill and, third, if it is directed to the large water user in the household.

We observe an average decrease in water use of 6.2 percent in response to the financial incentive to reduce consumption, which corresponds to a short run price elasticity of around -0.33. Consistent with the predictions from our model, the average response is driven almost entirely by more efficient households. Couples that share more with each other in the dictator game — our proxy for intrahousehold efficiency — reduce their water use more in response to the incentive treatment. We also observe heterogeneity based on which member of the couple receives the price shock. Specifically, the incentive treatment targeted at the woman in the household leads to reductions in water use that are twice as large as the other sub-treatments, though effects across incentive sub-treatments are not significantly different from one another. This greater sensitivity to a change in price targeted to the woman is consistent with our household model given two features of our sample: on average, the man is the residual claimant on the water bill and the woman uses more water than her husband. We test the last two predictions directly and observe that the individual price incentive is more effective if is directed to the individual who is not otherwise the residual claimant on the water bill and to the individual who uses more water. These two characteristics are highly correlated and, when tested together, residual claimant status is a more important driver of the effect.

Families in Livingstone devote a relatively large share of their incomes to water: an average monthly bill in our study setting is around USD 10. As a result, the incentive for households in our sample to conserve water, even absent our intervention, is strong relative to that in many developed countries where water or energy expenditures are a relatively small share of income.[1] Two features of the billing environment interfere with this incentive and may also affect households' response to our price incentive treatment. First, water is priced on an increasing block tariff, which results in a poor understanding of water prices and how consumption decisions affect the bill. We elicit price perceptions using a carefully framed procedure that elicits beliefs over quantities that are salient to water use decisions. While the

[1] In spite of the substantial cost, households in our sample consume roughly half the monthly average of US households.

median response is close to the truth, we observe a wide variance in beliefs across respondents and wide confidence intervals around individual beliefs. All households that receive the price incentive also receive information about the actual price of water. In addition, a sub-sample of household receive information about prices but no incentive to conserve. Second, a lack of trust in the water provider or a misunderstanding of the billing process might undermine the association between consumption and the monthly bill. We introduce a cross-cutting "provider credibility" treatment that explains how bills are generated. Neither the price information nor the credibility treatment result in measurable impacts on water use, even when prior beliefs about prices or the provider are taken into consideration.

The study links two previously unconnected strands of literature. First, the household economics literature studies deviations from the unitary model of the household, where even non-unitary households (i.e. different preferences or payoffs across members of the household) may be efficient if members of the household internalize each others preferences or are able to contract over bargaining surplus (Browning and Chiappori 1998; Lundberg and Pollak 2008). However, the presence of intrahousehold externalities, limited commitment or asymmetric information can result in Pareto inefficiencies (Lundberg and Pollak 1994; Bloch and Rao 2002; Chen 2013). Most of the existing empirical literature on non-unitary decision-making has failed to reject Pareto efficiency, with exceptions primarily associated with production or income pooling (e.g., Udry 1996; Duflo and Udry 2004; Ashraf 2009). Our focus is on consumption decisions, where evidence of non-cooperative outcomes are relatively scarce (e.g. Bobonis 2009; Attanasio and Lechene 2014), though the individual consumption of water or electricity may be considerably more difficult to monitor than other types of consumption. Our design is based on heterogeneity in in the degree of intrahousehold frictions, which has been addressed in only a limited number of studies in the intrahousehold literature (Hoel 2015; Angelucci and Garlick 2016, Angelucci and Garlick (2016)).

Second, the literature on corrective pricing in environmental economics highlights a number of reasons that consumers may fail to respond to prices, including misperceptions of price (Ito 2014; McRae and Meeks 2016), a lack of information about the price (Jessoe and Rapson 2014; Kahn and Wolak 2013), or a lack of salience (Allcott 2011). The incentive mis-alignment within the household that we study resembles the incentive problem between landlords and tenants, which leads to over-consumption of electricity (Levinson and Niemann 2004) and underinvestment in efficient durables (Myers 2015).

Methodologically, our approach has parallels in other studies that collect experimental evidence on household and other small group decisions around efficient resource use (Ostrom 2006; Velez et al. 2009; Mani 2011; Kebede et al. 2013). Few, however, have linked these measures directly to decisions outside of the experimental game. In this way, our proposed

approach resembles that of Karlan (2005) and others, where experimental measures in the field are used to explain heterogeneity in real world decisions.

The paper proceeds as follows. The next section presents a simple model of water use in the household. Section 3 describes the experimental design and implementation. Section 4 presents the results, and Section 5 concludes.

2 Water use within the household

We describe a stylized model of individual water use within the household to generate predictions about price sensitivity at the household level. The household's total water consumption W is the sum of the consumption decisions w_i of each individual $i = A, B$, which we will take to be husband and wife. The breakdown in efficiency within the household arises similarly to other resource extraction problems, in the sense that each individual makes a consumption decision taking her spouse's decision as given. The resulting equilibrium conditions do not necessarily equate the price of water to each individual's marginal benefit.

2.1 Set up

The water utility observes the household's total water consumption $W = w_A + w_B$ and charges the household pW, where p is the average marginal price of water for the household. Individuals observe their own consumption w_i and that of their spouse w_{-i}. Individual bargaining weights $\lambda_i > 0$ determine ex post division of the income Y that remains after the household pays the water bill: $\lambda_i(Y - pW)$. Bargaining weights sum to one ($\lambda_A + \lambda_B = 1$) and aggregate water expenditure cannot exceed income ($Y \geq pW$).[2] A cooperative household chooses aggregate consumption to equate the weighted marginal benefit and the price.

2.2 Individual best response

We allow for non-cooperative decision making, where i chooses w_i taking w_{-i} as given. Each individual receives utility from water use and disutility from paying for water: $v_i = f(w_i/\gamma_i) + \lambda_i(Y - pW)$ where $f' > 0$ and $f'' < 0$. The parameter γ_i measures heterogeneity across individuals in their utility from water consumption (or disutility from conserving); those with higher γ_i consume more.

Individuals may also internalize some share α_i of their spouse's utility, with person i's utility function $u_i = v_i + \alpha_i v_{-i}$. Person i chooses w_i to satisfy the first order condition:

[2]Note that households have an additional margin of adjustment in the timing and completeness of bill payment. We assume for the moment that bills are paid in full and on-time.

$$f'(w_i^*/\gamma_i)/\gamma_i = p(\lambda_i + \alpha_i\lambda_{-i}). \quad (1)$$

For $\lambda_i = 1$ or $\alpha_i = 1$, person i fully internalizes the household's cost of water consumption. Equation (1) shows that w_i^* is decreasing in p, λ_i, and $\alpha_i\lambda_{-i}$ (given $f' > 0$ and $f'' < 0$), and increasing in γ_i. Intuitively, i will use less if she is the residual claimant on any money saved, or if she is altruistic toward the savings that will accrue to her spouse, and will use more if she has greater utility from water use.

Note that this set up abstracts from enforcement, which may, along with altruism, lead to variation across individuals and households in α_i. Unlike the consumption of other private goods, where the household can maintain efficiency by dividing up income ex ante and enforcing consumption out of the individual account, water consumption is difficult to monitor. Even ex post, if i can only observe own consumption with some error ϵ, then she can only infer w_{-i} from the total bill with error: $\hat{w}_i = W - (w_i + \epsilon)$.[3]

2.3 Effect of a price change

Our experimental treatments are designed to make water use more costly to the household, effectively increasing the price. From equation (1), an increase in the price decreases individual water use. Water use is observed at the household level, so the testable prediction is: $\frac{\partial W}{\partial p} < 0$, i.e., the demand curve is downward-sloping.

Our focus, though, is on how this price sensitivity differs based on the inner workings of the household. At the individual level, $\left|\frac{\partial w_i}{\partial p}\right|$ is increasing in how much he or she internalizes his or her spouse, α_i. This is easy to see from equation (1), where α_i enters the righthand side multiplicatively with p. The testable predictions, denoting the average α_i in the household as $\overline{\alpha}$, are as follows.

Result: $\frac{\partial^2 W}{\partial p \partial \overline{\alpha}} < 0$. In other words, more efficient households are more price sensitive.

In our experiment, we give financial rewards for a reduction in household consumption to a specific individual, which one can think of as adding manipulating a person-specific component of the price, which we denote P_i. The individual utility function then becomes $v_i = f(w_i/\gamma_i) + \lambda_i(Y - pW) - P_iW$. The effect of such a manipulation depends on who the heavier user of water is, or γ_i.

Result: $\frac{\partial^2 W}{\partial P_i \partial \gamma_i} < 0$. Household consumption is more sensitive to a person-specific price targeted at a more intensive water user. This can be seen by via the revised first order

[3]Water use also differs from other private consumption to the extent that individuals care about externalities beyond the household.

condition, in which P_i and γ_i enter multiplicatively.

$$f'(w_i^*/\gamma_i) = \gamma_i(p(\lambda_i + \alpha_i\lambda_{-i}) + P_i) \tag{2}$$

Result: $\frac{\partial^2 W}{\partial P_i \partial \lambda_i} > 0$. Household consumption is less sensitive to a person-specific price aimed at someone who has more residual claimancy (higher λ_i) in the household. This is because this person consumes less to begin with, all else equal, so their responsiveness (in levels) to a further price change applied just to himself or herself is lower.

3 Experimental design and data

An empirical test of our simple household model requires three inputs: (1) a measure of aggregate household water consumption W, (2) exogenous variation in water prices p and P_i, and (3) measures of λ_i, γ_i and α_i. We describe how we operationalize each of these in turn. Our experimental design is summarized in Figure 1.

3.1 Water use

We partnered with the private regulated utility, the Southern Water and Sewerage Company, that provides piped water to residents in Livingstone, Zambia. Households are billed based on monthly meter readings, and charged according to an increasing block tariff. Our main outcome measure is household water use per month. We obtain monthly billing records, based on meter readings, for January 2012 through September 2016. For each household in our sample, we create a panel that extends four months after treatment and 20 months before treatment, ensuring that observations for all households cover a two year window, regardless of when they were surveyed. Water use is measured in cubic meters based on physical water meter readings collected monthly between the 20th and 25th of each calendar month. We keep only successful meter readings (i.e. drop the months in which meter readings were estimated or a meter was reported as broken or disconnected). We log transform the consumption outcome variable, which drops a small number of zero reading months (which are likely billing errors in any case).

The tariff schedule for 2015, when our study took place, is shown in Appendix figure A.1. The (weighted) average price in our sample is 4.9 Kwacha, or around 0.5 USD, per cubic meter. Average household consumption is around 19 cubic meters per month, around half of typical US household consumption, resulting in monthly consumption charges around 100 Kwacha or 10 USD per month. While we do not have household level monthly income

or expenditure measures for our sample, monthly expenditure data for urban households in Zambia suggest that households may spend around 5 percent of monthly income on water.

In addition to the measure of consumption, we estimate the impacts of the intervention on other customer outcomes including payment behavior and missing meter readings. Our outcome measures and other relevant statistics related to the monthly bill are shown in the top panel of Table 1, which also tests for balance across the treatments that we discuss next.

3.2 Price shock

Randomly varying water prices is infeasible. Instead, we affect the household's experienced water price through two experimental treatments. These treatments are implemented in conjunction with a household survey, run between May and December 2015, that we describe in detail in the next sub-section.

3.2.1 Incentive treatment

Households in the *incentive treatment* are provided with a monetary incentive to reduce water use. In the months following treatment, households are entered into a lottery for 300 Kwacha (~30USD) for reductions in billed consumption, relative to an account-specific reference period. Conditional on qualifying for the lottery, households had a 1 in 20 chance of winning. To qualify for the lottery, households had to reduce their consumption by at least 30% relative to their average water usage in a two-month reference window, which resulted in a mean reduction target of 5.8 (median 4.95) cubic meters. The reference window was updated twice over the course of roughly eight months of fieldwork. The first reference period (March-April 2015) was used for households surveyed in May-early August; this was updated using June-July billing data and applied to households surveyed through early October.[4]

In the notation of the household model, the price incentive treatment adds a term to the indirect utility function: $v_i = f(w_i/\gamma_i) + \lambda_i(Y - pW + R \times \mathbf{1}(W \leq \bar{W}))$, where R is the expected value of the lottery payout. With 1 in 20 odds, the expected value of the lottery incentive is around 1.5 USD or 15 Kwacha. The average consumption charge to households in our sample is around 100 Kwacha / month and the average price associated with the reference window is 5.1 Kwacha / cubic meter. With a target reduction of 5.8 cubic meters, expected value of the (net) price shock is 2.06 Kwacha per cubic meter or a roughly 40 percent increase in the average price.[5]

[4]In September, we expanded the sample; new households had July-August 2015 as their reference period. This sample was used from late September through December, when fieldwork was completed.

[5]Note that this calculation accounts for the increasing block tariff which causes the average price to fall from 5.1 Kwacha per cubic meter to 4.6 Kwacha / cu m. We therefore calculate the "net" price shock after

In our main price incentive treatment, both spouses learn about the lottery, and know that information is provided to both. In this case, the intervention is analogous to an increase in p. We also introduce variants on the price incentive treatment that provide only one of the spouses with information about the lottery and its outcome. These individual sub-treatments move the payoff from the lottery to outside of the λ_i term: $v_i = f(w_i/\gamma_i) + \lambda_i(Y - pW) + R \times \mathbf{1}(W \leq \bar{W})$, so are analogous to an increase in P_i in the model. This increases i's unilateral payoff from water savings, which has the greatest effect on overall household consumption if $\gamma_i > \gamma_{-i}$ and if $\lambda_i < \lambda_{-i}$. In other words, the relative effect of the individual lottery is largest if it is targeted to the spouse with the greatest contribution to aggregate usage or to the spouse with the lowest returns to conservation in the absence of the incentive. Of course, spouses could share the information with their spouse or the spouse could find out out about it, but the relative claim that the person-specific lottery enrollment is closer to a person-specific price than is the joint lottery treatment.

3.2.2 Price information

We also leverage the fact that most households are unaware of their marginal price to generate additional variation in prices. As part of the survey, we elicit price beliefs for both spouses then, in a price information treatment, provide accurate information about water prices. All lottery treatment households are also in the price information treatment, as shown in Figure 1. A challenge in communicating price information to households is that the units of consumption are difficult to map on to consumption. We both elicit price beliefs and provide price information in units of usage rather than the units used by the utility for billing purposes. Specifically, the survey asked "*suppose you wanted to save 20 Kwacha from your monthly bill; then, by how many minutes would your household as a whole have to reduce the use of the tap each day?*"[6] Treated households received information that cutting

accounting for this mechanical reduction in the average price associated with lower consumption.

[6]The question text included clarifications that we meant running the tap at a normal rate, as they would for daily activities like washing their hands, and also that we were asking them to think about the minutes that the tap was running during the various activities they did, and not the overall time spent doing chores. If the respondent said they did not know and could not provide an estimate, the questions was repeated once and they were given a second chance to respond. If they were still unable to answer the question, we asked them about a series of narrowing intervals, e.g. less than 20, 20-40, 40-60, more than 60, and conditional on the first interval, we asked about 5 minute intervals within that first interval, and then re-asked the main price belief elicitation question. 81% of men and 83% of women answered the question the first time it was asked, and an additional 10% of respondents answered the question in the second or third attempt. We then asked them about the highest and the lowest that they thought the number of minutes could be, and then asked them again for a best guess, giving them a chance to revise their previous answer if they wanted. The price belief elicitation question was asked after a series of questions on their own and their spouse's water use, as we found during piloting that thinking about water intensive chores beforehand made it easier for respondents to understand the question. The price elicitation module was piloted with almost

back by 20 minutes per day would save the average household 20 Kwacha on their monthly bill.

The effect of the price information depends on prior beliefs about water prices. Specifically, if households fully update, then the "price treatment" associated with the information is just the difference between the true price and the prior. Thus, individuals with a prior below the true price receive a positive price shock and vice versa. We categorize individuals into beliefs above and below the price information, and construct a household level measure that equals one if either spouse underestimated the price. The price information intervention also serves to remove variation in price beliefs, which allows us to calculate price elasticities associated with the price incentive treatment.

3.3 Intrahousehold measures

We measure λ_i and α_i through a household survey conducted separately (and simultaneously) for the husband and wife. A series of questions documents water use, bill payment responsibilities (λ_i) and intrahousehold cooperation, enforcement and altruism. In addition, we conduct modified dictator games between spouses as part of the survey visit. The game was couched as part of the survey that needed to be conducted separately with each spouse. Hence both spouses played the game concurrently and in private and the game was run by a trained surveyor who was of the same gender as the respondent.

The respondent is asked to pick one of two sealed envelopes and open it; the envelope contains either Kwacha 20 or Kwacha 30, and respondents only learns the value of their own draw, not the distribution. The surveyor explains that the money is theirs to keep and that they will be asked to make decisions using this money, but that they are under no expectation to share the amount. The respondent is then asked how much money they would send to their spouse, and separately, how much money they would send to a water conservation NGO. Before asking for their responses, the surveyor informs them that in each case, any money the respondent chooses to send will be doubled by the experimenter. The surveyor clarifies that the two decisions are mutually exclusive since the recipient of any money sent will be randomly selected after the respondent has made their decisions, with equal probability on each outcome, and that the respondent cannot influence which recipient is chosen at the end. The random endowments as well as the random selection of recipient ensures that respondents can conceal their own earnings from their spouse and, thus, her actions are based more closely on her own preferences rather than concern about retribution from her spouse. If the respondent chooses to send some money, and the spouse

300 households, who were then excluded from our sample.

is randomly chosen as the recipient, the spouse will know how much money was sent (since both spouses played the same game), but not how much money the respondent started with and hence how much the respondent chose to keep for herself. Similarly, the respondent can also choose to send nothing to the spouse and claim that the NGO was chosen as the final recipient. The surveyor explains these aspects of the game to the respondent and asks questions during the explanation to check for respondent comprehension, so that respondents know what information can and cannot be hidden from their spouse.

The game provides a revealed preference measure of α_i. The more the respondent sends to her spouse, the more she cares about his financial resources. The game simultaneously measures enforcement-based income sharing within the household, which may or may not carry over to water use; if a respondent expects that she can get back the amount she shared with her spouse, for example, she would share more. . In this sense, sharing in the game might be predictive of the primary water user being the residual claimant on savings from water conservation, or λ_i. For the analysis, we use survey questions to measure residual claimancy over the water bill specifically, with the idea that the household may arrive at different arrangements for more or less monitorable consumption goods. Specifically, both spouses are asked whose income is used to pay the water bill and who physically pays the bill (which is done in person in this context). If those answers match, that person is labeled as the residual claimant. If they do not, then follow up questions ask about how much discretion the one making the physical payment has over savings on the bill.

Finally, in addition to survey questions on water use and intrahousehold decision making, the survey collects information on demographics and socioeconomic status, as well as attitudes toward the water utility. In a cross-cutting "provider credibility" treatment, households were offered assurance that water bills are based on actual water use, to address concerns that both the lottery and price information treatments will be ineffective if households do not believe that bills reflect consumption.

3.4 Sample construction and summary statistics

3.4.1 Sample construction

Our sampling takes the universe of metered household accounts as provided by the water utility (SWSC) and imposes some restrictions based first on billing data, and then based on a short screening exercise that was conducted in the field. Using the panel of billing data for metered households as of April 2015, we excluded households that did not have a working meter for at least 3 out of the 4 preceding months. We also excluded households that use no water (i.e. are billed for zero cu.m.) in more than half of the preceding 4

months. Households that had very low variation in usage over the preceding four months were considered to have possibly tampered with the meter or have a meter reader who is not giving accurate reports to SWSC and are hence also excluded based on the following criteria: if the coefficient of variation in this period was less than 0.05, or if the quantity reported was identical for 3 or more months. Households with consistently low usage were also excluded as they would be least able to adjust their water consumption in response to a price shock; we drop households if their usage was on the lowest price tier (less than 6 cubic meters) for more than 2 of the preceding 4 months. Households whose median water usage in the preceding four months was above the 99th percentile were also dropped as they could also have had malfunctioning meters, or may not be as responsive to price shocks, and may also have been significantly more difficult to survey (because they were presumably very wealthy households or firms mislabeled as residential customers by SWSC). Finally we drop households with an extremely high outstanding balance with the water utility, or households that are owed a significant amount of money by SWSC, defined as 6 times or 4 times their median bill in the preceding four months respectively. This yields a total of roughly 8,500 households that we target for an in-person screening (roughly 87% of the initial sample).

Households were visited by a surveyor to collect data on characteristics we could not observe in the billing data, that we use for screening. Specifically, we require that the meter not be shared with other households, that the primary bill payer be married (or cohabiting) and that both spouses live at that address, and that the household was in residence for at least the 4 month period prior to April 2015 that was used to implement the restrictions in the billing data described above. We also exclude households planning to move in the following 6 months. We also drop households where the husband might be a migrant worker, or where an elderly couple lived at the address but did not actually pay their own bill. Our surveyors attempted to screen each household 3 times; any adult member of the household could be given the screening questionnaire. In total, around 6,000 households were successfully screened, of which 35% met all our screening criteria. We include all screened households in our analysis to improve the precision of our estimates.

Households that met the screening criteria were informed about the survey. We scheduled a second visit with the primary bill payer and his/her spouse, emphasizing that we needed both of them to be present for the full survey. We also informed respondents they would be compensated 40 Kwacha (~ 4 USD) for participating in the survey. At the scheduled time and date, a pair of surveyors (always a woman and a man) visited the screened household for a full survey. After a few preliminary demographic questions, husbands and wives were separated and surveyed individually in different rooms of the house. Enumerators elicited water price beliefs, asked for perceptions of own and family members' water usage, and

conducted the modified dictator's game described previously, to measure the respondent's altruism towards their spouse and water conservation issues. After finishing their individual questionnaires, both surveyors and respondents met back together in a common room for the last survey questions, and to receive the price information.

3.4.2 Sample statistics

Table 1 summarizes characteristics of the sample, including elicited price beliefs and attitudes toward the water provider, and tests for balance between the treatment (price incentive) and control groups. The top panel shows statistics from the water bill, and so we report means and standard deviations for the households that were screened out of the survey but are included in our analysis. The middle panel shows household characteristics gathered through the survey. Around half of the sample owns their own home and the average household size is close to six. Around 16 percent employ a maid. Our attempt to explain intrahousehold decision making around water use based on spousal preferences clearly simplifies the dynamic in a home with an average of four additional members, plus – in some cases – a maid. The majority of both husbands and wives in the sample have regular income, though regular income is more common among male respondents than female respondents. Price beliefs are reasonably accurate. In over 60 percent of households, at least one of the two respondents underestimated the price. Distrust of the service provider is high: in over 40 percent of households, both spouses say that high bills are the fault of the provider, i.e. not because of high consumption.

3.4.3 Intrahousehold measures

Our preferred measure of intrahousehold cooperation comes from the respondent's incentive compatible decision of how much to share with his or her spouse in the dictator game. We summarize the measure in the bottom panel of Table 1. Husbands send a larger fraction of their endowment to their wives than wives do to their husbands. Both spouses send a smaller share of their endowment to the water NGO than to their spouse. Table 1 also reports other water use measures associated with our theoretical predictions. In around 30 percent of households, the wife says she is the residual claimant on the bill. In over 85 percent of households, she does more water intensive activities than her husband. Thus, in the typical household, the man is the residual claimant (higher λ_i) and the woman is the larger water user (higher γ_i).

Of course, measures of intrahousehold efficiency may be correlated with other household characteristics that also affect water use and price sensitivity. Table 2 shows correlations

between three dictator game outcomes – the share of the endowment sent by the husband, by the wife, and whether the average share sent was above median (our main measure of intrahousehold efficiency) – and individual and household characteristics. First, the share of endowment sent to the spouse is positively correlated with the share sent to the water NGO. While this may indicate that individuals who are more altruistic in general are also more altruistic toward their spouses, it may also indicate some experimenter demand effect or confusion about the game. Second, the social desirability index score is not significantly correlated with our measure of intrahousehold efficiency, nor is an indicator for which spouse is the residual claimant on savings on the water bill. We will use both the NGO decision and the social desirability index as controls in robustness checks. Third, household assets are positively correlated and household size is negatively correlated with the dictator game measure at the household level. The household asset measure proxies for wealth, and we would expect richer households to be less price sensitive while more efficient households are predicted to be more price sensitive; this correlation is unlikely to drive a positive relationship between price sensitivity and intrahousehold efficiency. Larger households may be less price sensitive than smaller households for reasons similar to the externality problem that we model between spouses. Thus, while household size may partially drive the effect that we estimate, the underlying intrahousehold failure may be similar to what is measured by the dictator game. Of course, these relationships do not represent an exhaustive list of observable (and unobservable) characteristics that may affect the heterogeneity in price sensitivity that we estimate. Rather than trying to rule out an exhaustive list of observables, we rely on multiple predictions from our household model, all of which suffer from independent omitted variable concerns but all of which are unlikely to be simultaneously supported because of some omitted variable.

4 Results

4.1 Predictions

The experimental design and data collection described in the previous section generates the following empirical predictions:

1. The incentive treatments decrease water consumption.

2. The magnitude of the incentive treatment effects are increasing in intrahousehold efficiency, measured by $\bar{\alpha}$.

3. The individual-specific incentive treatments are more effective if they (a) go to the individual who consumes the most water in the household and (b) go to the individual who is not otherwise the residual claimant on the water bill in the household.

While our main focus is on the incentive treatments and intrahousehold efficiency, we also test some additional predictions:

1. Price information decreases water consumption among households that underestimate the price.

2. The incentive treatment decreases consumption more for households that are more trusting that their bill reflects their usage.

3. The incentive treatment decreases consumption more when paired with the provider credibility treatment.

4.2 Identification

We implement a randomized intervention, with monthly outcome data before and after the intervention. A standard difference in differences regression identifies the treatment effect:

$$y_{it} = \beta_1 treatedhh_i + \beta_2 post_{it} + \beta_3 treatedhh_i \times post_{it} + \epsilon_{it} \tag{3}$$

where $treatedhh_i$ is a binary indicator for assignment to one of the treatment groups and $post_{it}$ is a time-varying indicator that turns on for household i in the month after the survey. Note that $post_{it}$ varies across households and not just over time because the survey and treatment were rolled out over time. β_3 identifies the differential change in the outcome among treated household after the survey. Also note that even though a household was only eligible for the lottery for its consumption in the first full billing cycle after the survey date, we set $post_{it}$ equal to 1 as of the survey date because it is possible the intervention had immediate effects. In our preferred specification, we omit the month in which the survey occurred since it is only partially treated.[7]

To improve precision, we include time and household fixed effects in our preferred estimates. We also include screened households in the analysis to contribute to the identification of time fixed effects and the coefficient on $post_{it}$. For households that were screened but never surveyed, we use the date they would have been surveyed to define $post_{it}$, according to the survey schedule, had they not been screened out. All specifications therefore include

[7]Note that because the billing cycle starts on the 20th of each month, our definition of month corresponds to the billing cycle, i.e. July runs from June 21 to July 20.

an indicator for whether the household was in the surveyed sample, which we define as $surveyed_{it} \equiv surveyedhh_i \times post_{it}$. We show two different specifications. In one we allow for an effect of being surveyed so included $surveyed_{it}$ and in the other we assume this away (since we cannot reject that it is 0). We also define $treat_{it} \equiv treatedhh_i \times post_{it}$, where all treated households are also in the survey sample. We estimate:

$$y_{it} = \beta_1 treat_{it} + \beta_2 surveyed_{it} + \beta_3 post_{it} + \tau_t + \eta_i + \epsilon_{it} \tag{4}$$

where τ are month-year fixed effects and η_i are household fixed effects. In the presence of household fixed effects, β_1 identifies the treatment effect of interest, and β_2 captures any independent impact of being surveyed. We allow for arbitrary within household correlation in water use over time by clustering standard errors at the household level. Because gaps in the panel are associated with meter disconnections and other meter reading issues, we add a time-varying indicator to the month after a missing observation to control for the fact that these months may record only a partial month of consumption. Our main predictions involve heterogeneity in the response to treatment by household type, so we interact both $treat_{it}$ and $surveyed_{it}$ with relevant household characteristics.

We also plot average water use by month across treatment conditions and our preferred measure of intrahousehold efficiency in the months leading up to the survey (Appendix Figure A.2). Overall, we observe parallel trends by incentive treatment and intrahousehold efficiency.

4.3 Average treatment effects

Table 3 reports average treatment effects of the incentive treatment and the price information and provider credibility treatments. Column 1 report the standard difference in difference results from estimating equation (3), without household or time fixed effects. Columns 2 and 3 add household and month-year fixed effects, respectively. Average consumption across the treatment arms are not significantly different from the pool of surveyed control households. However, surveyed households consume less, on average, than do households that were screened out of the sample (the coefficient on surveyed).

Our main coefficient of interest is on Incentive x Post. We observe a statistically significant 6.2 to 6.7 percent decrease in monthly consumption in response to the lottery treatment, consistent with prediction 1: The incentive treatments decrease average water use. The implied short run price elasticity is -0.328 (column 3).[8] We observe no significant average effect

[8]We calculate the average price elasticity of demand using the coefficient on the incentive treatment as a measure of $\partial ln(q)/\partial p$, which we multiply by the average price. Our calculated short run price elasticity of

from the other treatments or from being surveyed (the coefficient on Surveyed x Post). Going forward, we focus on the specification shown in Column (3), which includes household and month-year fixed effects. We define the treatment variables as time varying and report results from estimating equation (4).

While we observe no average effect of the other treatments, the effects of the price information and provider credibility treatments should depend on respondent beliefs about water prices and the correspondence between water use and bills. Thus, we supplement the analysis in Table 3 with specifications that (a) interact the price information treatment with an indicator for whether the husband and wife underestimated the price, on average, and (b) interact the provider credibility treatment with attitudes toward the water provider. Both should lead to reductions in water use. Table 4 shows that these treatments are weak, even after taking heterogeneous responses into consideration.

4.4 Intrahousehold heterogeneity

The results shown in Table 5 breaks the lottery treatment out by the recipient. Recalling the patterns of water use and residual claimant status shown in the bottom panel of Table 1, prediction 3 implies that the lottery directed to the wife should have a larger effect, given that in the average household she is the larger water user and is not the residual claimant on reductions in the water bill. The pattern of coefficients in Table 5 confirm that the most effective lottery sub-treatment is directed toward the wife. We lack the precision, however, to reject that the effect of the wife lottery is statistically different from either of the other two sub-treatments. We test the third prediction more directly below.

Table 6 shows the main test of the predictions of our intrahousehold measure. We estimate the effect of the lottery treatments on household water use, and allow the effects to vary with the average amount shared in the dictator game (column 1) or the amount shared by each spouse (column 2). Consistent with prediction 2, column 1 shows a larger reduction from households that share above the median on average, with the total effect among these more efficient households resulting in a 10 percent decrease in water use. The average short run price elasticity among households with below-median dictator game contributions is -0.17 while the total effect for above-median households implies an elasticity of -0.49. Column 2 shows weaker effects when looking at the donation decisions of either spouse; the overall amount of cooperation appears to matter more when average α within the household is used to measure intrahousehold efficiency.

demand is slightly below the mean found in the literature reviewed by Dalhuisen et al. (2003) and in line with the short run elasticities summarized in Worthington and Hoffman (2008). In the literature, the long run elasticity is generally shown to be larger than the short run elasticity.

Our conceptual framework also clarifies that the effect of the individual lottery sub-treatments should depend on the existing incentives to conserve water, resulting in our third empirical prediction. If the lottery recipient is not otherwise the residual claimant on water savings, then the effect of the lottery is relatively large. In addition, if there is a disparity in water user's ability to affect the total amount consumed by the household, then directing the lottery to the larger user will also be more effective. We construct measures that equal one if the individual lottery went to the residual claimant or the larger water user, both based on the self-reports of the individual lottery recipient. Interacting these measures with an individual lottery indicator provides a test of prediction four that averages across individual lotteries. Table 7 shows the overall effect of the individual lotteries in column 1, which is consistent with the results shown in Table 3. Column 2 includes the interaction with the non-residual claimant indictor, which is significantly larger than the effect of the individual lottery if directed to the residual claimant. Column 3 shows the interaction with the bigger water user indicator, which is negative but statistically insignificant. The total effect on the larger user is a statistically significant decrease of 7.4 percent. When we combine these two measures, noting that the wife is often both the non-residual claimant and the bigger user, we see that the residual claimant status appears to be the more important of these channels.

4.5 Robustness checks

4.5.1 Measurement

We test for alternative interpretations of the heterogeneity we measure. Specifically, we repeat our main table of intrahousehold heterogeneity, controlling for our measure of overall altruism as measured by donations to the water NGO. This measure of giving to a water related NGO also acts as a measure of response to experimenter demand. To address social desirability bias more directly, we also add controls for a social desirability index constructed off of household responses to a modified version of the Marlow and Crowne (**?**) questionnaire. Each type of control is interacted with the full set of treatment indicators. Table 8 summarizes the results. Column 1 shows results with the NGO donation control, which absorbs some of the variation in our experimental measure of intrahousehold efficiency. Column 2 adds controls for the social desirability index. Column 3 includes both types of controls. Adding in either of these controls lowers the significance of the main results. The control for the NGO donation is a strong test of intrahousehold cooperation since it isolates the variation in intrahousehold cooperation above and beyond more general levels of cooperation that may also manifest within the home. Qualitative, the results point estimates remain similar to our main results.

5 Conclusion

We generate new evidence on the importance of intrahousehold decision making for consumption externalities that extend beyond the household. We combine water billing data with experimental measures of intrahousehold efficiency via a dictator game played between spouses and with an exogenous incentive for water conservation offered to a random subset of study households. Consistent with a simple model of individual water consumption that allows for non-cooperative intrahousehold decision making, we show that households in which spouses are less likely to internalize each others' financial gains (as measured by the dictator game) are also less responsive to the increase in the cost of consumption delivered by the conservation incentive. Also consistent with our model, targeting the conservation incentive to the individual within the household who otherwise has the least incentive to conserve water leads to greater overall response. Together, these results show that households that are internally less efficient are also less price sensitive.

Our main result relies on heterogeneity in a measured characteristic across households, so is susceptible to omitted variable concerns (i.e. some other factor may be correlated both with our measure of intrahousehold efficiency and with price sensitivity). While we cannot fully rule out this type of concern, we address it in three ways. First, we test multiple predictions that rely both on measured differences across household and design based variation in the payoffs to individuals within the household. While any one of these tests may be affected by some omitted variable, they are less likely to all be driven by the same unmeasured heterogeneity across households. Second, we rely on experimental measures of intrahousehold efficiency, which are more likely to accurately pick up the underlying characteristic of interest. Third, we measure a number of controls that allow us to perform more or less strict analyses of the variation allowed to influence household level price sensitivity.

These findings have implications for the efficiency of pricing policies designed to correct consumption externalities. If households are internally inefficient, then the price needed to achieve an efficient quantity of consumption will be higher than if individuals within the household internalize the cost that their consumption imposes on other household members (via the shared monthly bill). This deadweight loss associated with the intrahousehold decision process has not, to our knowledge, been discussed in the substantial literature on the use of corrective taxes in the face of existing distortions. While pricing water at the individual level may be infeasible, other interventions, such as more real-time information about consumption or technologies that lower the cost of conservation to the individual, may be able to overcome some of the intrahousehold frictions that we measure.

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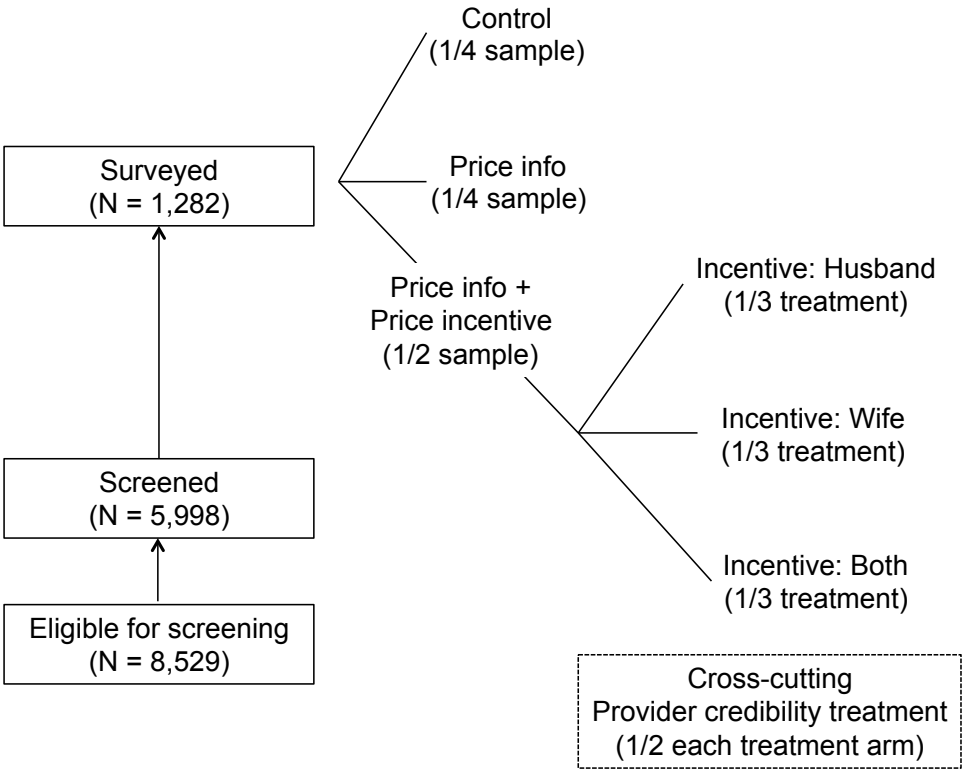


Figure 1: Experimental design

Table 1: Sample statistics & Balance

	Screened only (1)	No incentive (2)	Price incentive (3)	P-val (2)=(3) (4)
Quantity consumed	20.940	18.995	18.247	0.239
	(14.525)	(12.097)	(10.515)	
Any payment	0.738	0.764	0.769	0.566
	(0.195)	(0.166)	(0.166)	
Missing meter reading	0.137	0.100	0.112	0.210
	(0.188)	(0.157)	(0.170)	
Total monthly bill	99.848	91.294	85.667	0.123
	(88.152)	(69.039)	(60.963)	
Households	5312	664	618	
Owns home		0.512	0.495	0.546
		(0.500)	(0.500)	
People in HH		5.860	5.888	0.822
		(2.286)	(2.218)	
HH has maid		0.169	0.149	0.333
		(0.375)	(0.356)	
Rooms in HH		3.529	3.553	0.751
		(1.264)	(1.444)	
H Salaried or self-employed		0.774	0.778	0.856
		(0.418)	(0.416)	
W Salaried or self-employed		0.598	0.607	0.745
		(0.491)	(0.489)	
Either underestimated price		0.619	0.637	0.551
		(0.486)	(0.481)	
Both blame high bill on SWSC		0.440	0.414	0.356
		(0.497)	(0.493)	
Share sent to spouse by husband		0.702	0.690	0.398
		(0.269)	(0.254)	
Share sent to spouse by wife		0.520	0.513	0.597
		(0.262)	(0.260)	
Share sent to NGO by husband		0.312	0.303	0.522
		(0.253)	(0.232)	
Share sent to NGO by wife		0.275	0.276	0.923
		(0.222)	(0.220)	
Wife says she is residual claimant on the water bill		0.307	0.316	0.749
		(0.462)	(0.465)	
Wife does more water activities than husband		0.870	0.864	0.736
		(0.336)	(0.343)	
Households		664	618	

Notes: Pre-treatment means for all households (top panel), household-level statistics for surveyed households only (second panel), and measures of dictator game decisions and survey responses for surveyed households only (bottom panel). The quantity consumed is measured in cubic meters per month. H and W refer to husband and wife. The share sent to the spouse is measured as a fraction of the respondent's endowment.

Table 2: Correlates of dictator game decisions

	Husband share sent (1)	Wife share sent (2)	Sent above median (3)
H: Share NGO	0.183***	0.022	0.185***
	(0.030)	(0.030)	(0.058)
W: Share NGO	-0.010	0.163***	0.185***
	(0.033)	(0.033)	(0.063)
H: SDB score	0.052**	0.008	0.081*
	(0.022)	(0.021)	(0.041)
W: SDB score	0.019	-0.025	-0.013
	(0.021)	(0.020)	(0.040)
H: Residual claimant	-0.020	0.022	0.016
	(0.017)	(0.016)	(0.032)
W: Residual claimant	0.000	-0.012	-0.017
	(0.015)	(0.015)	(0.029)
HH assets	0.005**	0.015***	0.024***
	(0.003)	(0.003)	(0.005)
Household size	-0.004	-0.006**	-0.012**
	(0.003)	(0.003)	(0.006)

Notes: The share sent to the spouse and share sent to the NGO are measured as a fraction of the respondent's endowment. The SDB score measures social desirability bias using an adapted Crowne-Marlowe (1964) instrument. H and W refer to husband and wife.

Table 3: Average treatment effects

	log (quantity) (1)	log (quantity) (2)	log (quantity) (3)
Price incentive treatment	-0.013		
	[0.041]		
Price info treatment	0.007		
	[0.047]		
SWSC credibility treatment	-0.011		
	[0.033]		
Surveyed	-0.085**		
	[0.038]		
Surveyed x Post	0.050*	0.025	0.014
	[0.029]	[0.028]	[0.027]
Incentive x Post	-0.067**	-0.062**	-0.063**
	[0.030]	[0.029]	[0.029]
Price info x Post	-0.019	-0.011	-0.008
	[0.034]	[0.033]	[0.033]
SWSC credibility x Post	0.026	0.025	0.024
	[0.026]	[0.025]	[0.024]
HH FE		x	x
Month-Year FE			x
Observations (HH)	6,594	6,594	6,594
Observations (HH-months)	129,900	129,900	129,900

Notes: The panel begins 20 months (billing cycles) prior to the month of the survey and ends 4 billing cycles after the survey. Since treatment was provided at the survey, we use the recorded survey date to define the treatment variables. The HH assignment indicator is consistent across the panel, while the *Post-survey* indicator switches from 0 to 1 in the first full billing cycle after the date the household was surveyed; observations for billing cycles that contain the survey date are dropped. All households in the price incentive treatment also received the price information treatment. Standard errors are clustered at the at the household level.

Table 4: Price beliefs and provider trust

	log (quantity) (1)	log (quantity) (2)
Price info treatment	-0.012	
	[0.055]	
Price info treatment x Either spouse underestimated price	-0.020	
	[0.073]	
Provider credibility treatment		0.004
		[0.033]
Provider credibility x Distrust billing		0.046
		[0.049]
Total effect=0 (p)	0.498	0.157
HH FE	x	x
Month-year FE	x	x
Observations (HH)	6,337	6,594
Observations (HH-months)	124,827	129,900

Notes: Regressions include the time-varying survey indicator and the post indicator interacted with the heterogeneity variables. The bottom panel reports p-values for tests that the total effect is equal to zero. Standard errors are clustered at the at the household level.

Table 5: Average treatment effects: Price incentive sub-treatments

	log (quantity) (1)	log (quantity) (2)
Couple incentive	-0.039	-0.050
	[0.042]	[0.041]
Husband incentive	-0.047	-0.043
	[0.038]	[0.037]
Wife incentive	-0.098**	-0.095**
	[0.039]	[0.037]
HH FE	x	x
Month-Year FE		x
Observations (HH)	6,594	6,594
Observations (HH-months)	129,900	129,900

Notes: Regressions include the time-varying survey indicator and the post indicator. Standard errors are clustered at the at the household level.

Table 6: Heterogeneity in household efficiency

	log (quantity) (1)	log (quantity) (2)
Price incentive treatment	-0.034	-0.058*
	[0.032]	[0.033]
Price incentive treatment x Shared above median on average	-0.071	
	[0.050]	
Price incentive treatment x Husband shared above median		-0.022
		[0.051]
Price incentive treatment x Wife shared above median		-0.003
		[0.058]
Total effect=0 (p)	0.006	
Total effect husband=0		0.062
Total effect wife=0		0.272
HH FE	x	x
Month-year FE	x	x
Observations (HH)	6,587	6,587
Observations (HH-months)	129,776	129,776

Notes: Regressions include the time-varying survey indicator and the post indicator interacted with the heterogeneity variables. The bottom panel reports p-values for tests that the total effect is equal to zero. Standard errors are clustered at the at the household level.

Table 7: Heterogeneity in household cooperation: Individual responses

	log (quantity) (1)	log (quantity) (2)	log (quantity) (3)	log (quantity) (4)
Individual price incentive	-0.061**	-0.018	-0.048	-0.020
	[0.026]	[0.033]	[0.034]	[0.036]
Price incentive to non-residual claimant		-0.091**		-0.093**
		[0.043]		[0.046]
Price incentive to big user			-0.026	0.006
			[0.043]	[0.046]
Total effect claimant=0		0.002		0.017
Total effect user=0			0.026	0.754
HH FE	x	x	x	x
Month-year FE	x	x	x	x
Observations (HH)	6,594	6,594	6,594	6,594
Observations (HH-months)	129,900	129,900	129,900	129,900

Notes: Regressions include the time-varying survey indicator and the post indicator interacted with the heterogeneity variables. The bottom panel reports p-values for tests that the total effects are equal to zero. Standard errors are clustered at the at the household level.

Table 8: Robustness checks: Measurement

	log (quantity) (1)	log (quantity) (2)	log (quantity) (3)
Price incentive treatment	-0.005	-0.003	0.026
	[0.039]	[0.084]	[0.088]
Incentive treatment x Shared above median	-0.066	-0.070	-0.065
	[0.050]	[0.050]	[0.050]
Total effect=0 (p)	0.134	0.409	0.674
HH FE	x	x	x
Month-year FE	x	x	x
NGO share interactions	x		x
SDB score interactions		x	x
Observations (HH)	6,587	6,587	6,587
Observations (HH-months)	129,776	129,776	129,776

Notes: Regressions add controls for overall altruism and social desirability bias, and interacts them with the time-varying survey indicator and the post indicator. The bottom panel reports p-values for tests that the total effect is equal to zero. Standard errors are clustered at the at the household level.

Appendix

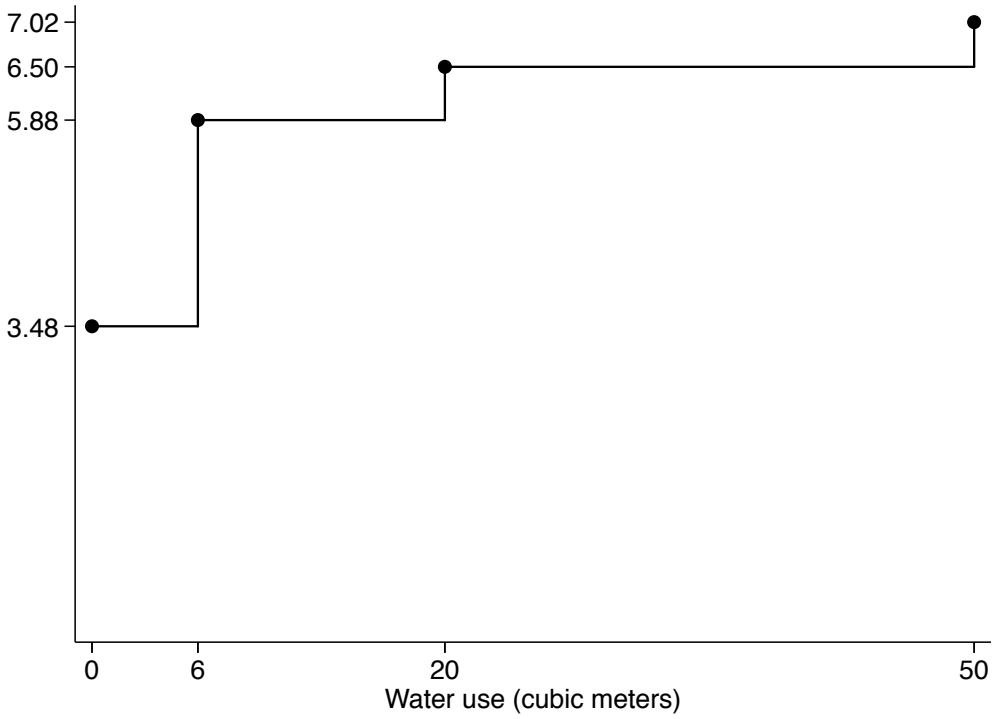


Figure A.1: 2015 tariff schedule

Notes: Water tariff for all residential customers in 2015. The marginal price, shown in Kwacha, is increasing in consumption, measured in cubic meters.

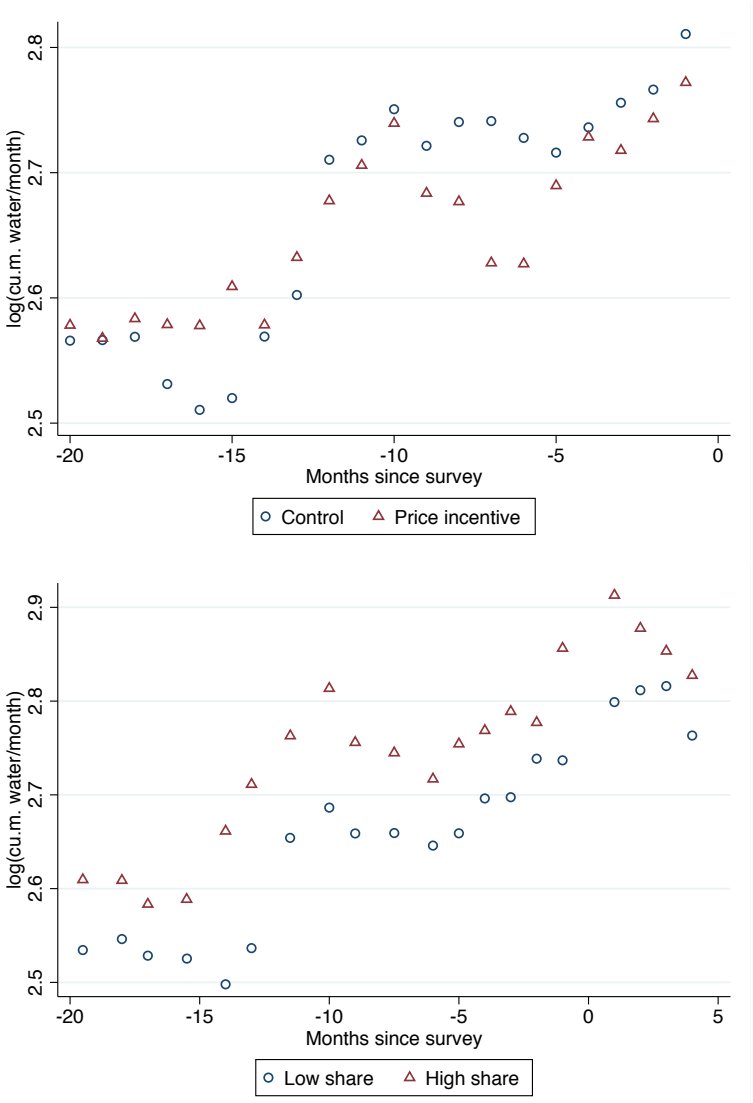


Figure A.2: Pre-treatment water use

Notes: Average water use, in log(cubic meters), for each month prior to the survey. The top figure shows monthly averages for the control and price incentive groups and the bottom figure shows monthly averages for households that contribute below and above the median in the dictator game.