Can Information Reduce Nonpayment for Public Utilities? Experimental Evidence from South Africa*

Andrea Szabó
Department of Economics
University of Houston
aszabo2@uh.edu

Gergely Ujhelyi
Department of Economics
University of Houston
gujhelyi@uh.edu

April 21, 2014

Abstract

Nonpayment for public utilities is an important constraint to expanding service access in developing countries. As a potential policy response, this study implements and evaluates a randomized water education campaign in a low income peri-urban area in South Africa. We estimate substantial treatment effects: on the order of a 30% increase in payments over a three-month period. Surprisingly, these effects are not driven by an increase in households' knowledge. We consider various possible explanations, and argue that the intervention likely had "nudging" effects on households. Our findings have important implications for understanding energy conservation and other public information campaigns.

*We are grateful for the cooperation of Odi Water Services, in particular to Sipho Nkosi and his dedicated team for implementing the education program, and to Pieter Avenant for giving us access to the billing data. We also thank the Water Research Commission of South Africa for supporting our project, and Patrick Chiroro and his team at Impact Research International for implementing our surveys. We thank Hunt Allcott, Paul Ferraro, and our colleagues at the University of Houston for useful comments. All views expressed here are the authors' and do not necessarily represent those of any of the organizations involved. Detailed supplementary materials, including education brochures and survey questionnaires, are available on the authors' websites.

1 Introduction

Improving people's access to basic utilities like electricity, water, or phone service is viewed as a key challenge in many developing countries. However, consumers' ability or willingness to pay for services can be an important constraint to investment in infrastructure. For example, the difficulty to collect unpaid bills has been cited as a major obstacle to improving electricity provision in India (Ahluwalia, 2002), the former Soviet Union (Lampietti et al., 2007), and Colombia (McRae, 2013). In South Africa, nonpayment presents a major problem for local governments and prevents the efficient use of the existing infrastructure for electricity, water, and sanitation (Republic of South Africa, 2011).[1]

The most obvious response to nonpayment, denied service, is often not feasible in developing countries. Such actions could go against social perceptions of fairness and erode citizens' trust in local governments, resulting in even more nonpayment or even civil unrest. In South Africa, the expansion of the water and sanitation infrastructure to poor black localities took place after the fall of Apartheid, and access to these services, codified in the constitution, is viewed as a requirement for human dignity. Even when social norms are less obvious, nonpayment can be caused by consumer dissatisfaction with service delivery or a lack of trust in the provider. In this case, punishing nonpayment by denying service could lower trust even further and would be highly counterproductive.[2]

In such settings, utilities' response to nonpayment requires a delicate balancing act between various costly strategies. When consumers simply refuse to pay, have no individual meters or there are widespread informal connections, utilities may not undertake any enforcement action (e.g., World Bank, 1999). In our setting, informal connections are virtually nonexistent, consumers have individual meters, and consumption is highly price elastic (Szabó, 2013). Here, the water provider has purchased and installed restriction devices that limit the flow to a bare minimum for households with large outstanding balances (about a third of the population). In many cases these households will continue not paying, and may simply leave the taps open, perhaps with a container underneath to collect water. Such limited enforcement strategies are costly to the provider, lead to waste, and often do little to incentivize payment.

In this paper, we explore an alternative strategy to reduce nonpayment: providing information. An important reason behind nonpayment may be a lack of familiarity with the consumption process. Households may not understand the billing system or the quantities

[1] By nonpayment, we mean a failure to pay the billed amount. This is different from a policy of providing free services (e.g., to the poor), under which consumers do not *have* to pay.

[2] In a related context, a lack of trust in government has been recognized as an important cause of tax evasion (see Slemrod (2007) for a survey).

of the service they consume in their everyday activities. This lack of information can lead to the poor management of household water consumption, and to high monthly bills that the household finds difficult to pay.[3] Even if households are able to pay, they may feel less compelled to do so if they do not understand why the bill is so high. Since water is a complicated good to purchase, providing households information may be an effective tool to reduce nonpayment.

To study this question, we implemented a randomized water education campaign in a collection of low income peri-urban townships in South Africa. Education officers visited a treatment group of 500 households to give them accessible information about various aspects of the water consumption process, including the water meter, the bill, and the amount of water used by various everyday activities. We evaluate the program combining administrative billing data with in-depth survey information. Compared to a control group, we find that treated households were more likely to pay their bill and made higher payments. We estimate that, over a three-month period, our treatment reduced the fraction of consumers making no payments by 4-5 percentage points and increased total payments by about 30%.

Our data allows us to test several possible mechanisms behind our findings. A natural possibility is that information allowed households to better manage their water consumption and thus their household finances. As a result, households could save enough money to pay their bills. Although we find no change in average water consumption, treated households do report an increase in water conservation activities, and we observe a reduction in water usage among the highest consumers. These patterns are consistent with households rationally substituting high-consumption activities with low-consumption ones. Surprisingly, however, we find no evidence that treated households' information increased on average. This is true across a variety of measures: compared to control households, treated households are no more likely to understand water quantities, know how much water they consume, or be able to read their water bill. For example, even after our treatment less than 12% of households are able to tell their consumption from their bill. We provide extensive evidence, using both survey data and households' GPS locations, that this is not due to information spillovers between the control and treatment groups. We also do not find evidence suggesting a lack of information sharing within households that would prevent us from accurately measuring changes in consumer knowledge. Finally, although we do see some increase in information among the less educated and the poor, the reduction in nonpayment takes place primarily among the more educated and the rich. Thus, heterogenous information effects across groups are also unlikely to explain the payment results.

[3] Previous research in other contexts has highlighted the suboptimal financial decisions caused by a lack of information. See, e.g., Cole et al. (2013) on insurance purchases by poor Indian households.

An alternative to information as an explanation for increased payments is a set of "psychological effects" suggested by various literatures. In the US, conservation campaigns often attempt to "nudge" consumers to conserve electricity or water by comparing their consumption to those of their neighbors, or by including various pictograms on their bills (see, e.g., Allcott (2011) and Ferraro and Price (2013)). Even though we consciously tried to minimize any perception of social pressure associated with our information campaign, it is possible that households' perceived our education visits as similar nudges. Another possibility, suggested by the literature on tax evasion (e.g., Slemrod, 2007) is that education visits improved the provider's public perception, leading to an increased willingness to pay. Consumers may have reciprocated the providers' education efforts by paying more. While we cannot identify one particular psychological effect, we do show that the education visits did not simply act as reminders for households to pay their bills. In particular, taking advantage of the fact that we observe the entire population of consumers, we are able to show that our detailed surveys on water usage and bill payment did not affect consumers' behavior. The education visits conducted by the provider's officers were crucial for increasing payments.

Our paper is most closely related to a recent economics literature studying conservation campaigns for electricity and water (Reiss and White, 2008; Allcott, 2011; Ayres et al., 2013; Ferraro and Price, 2013; Ferraro and Miranda, 2013; Jessoe and Rapson, 2013; Allcott and Rogers, 2014).[4] Our work differs from these in four key ways. First, we focus on nonpayment, which has not been studied in the literature but is a key issue in many developing countries. Second, to our knowledge, this is the first paper to evaluate a randomized water education campaign in a developing country, where the lack of information is known to be a problem and where small improvements in water use could have large impacts on household welfare. Third, while most campaigns in the literature are designed to generate psychological effects (e.g., through appeals to social norms), we explicitly focus on providing information.[5] Fourth, the campaigns analyzed previously tended to be highly impersonal interventions, such as letters sent in the mail, delivering a narrow message. By contrast, we analyze an in-depth education campaign involving household visits by our education officers which allows us to communicate detailed information to the households.

We are not aware of any experimental study on nonpayment for public utilities or other services. The closest to this topic is the literature on tax evasion, which includes several

[4] A related literature in marketing and psychology is reviewed in Abrahamse et al. (2005).

[5] In the context of previous studies, information on basic ways to save water or electricity is thought to be widely available in the population. Therefore even interventions that include tips for conservation (e.g., take showers instead of baths) are viewed as pro-social appeals rather than giving consumers new information (Allcott, 2011; Ferraro and Price, 2013). One study focused on information provision is Jessoe and Rapson (2013) who analyze the provision of real-time feedback on household electricity use.

recent experiments (Slemrod (2007) and Hallsworth et al. (2014) provide references). A major issue in this literature is misreporting and the failure to declare one's income. This is conceptually distinct from the problem of nonpayment, where a consumer has already received a bill, and the two are likely to involve different calculations by the individual (for example, evasion requires weighing the probability of an audit, while nonpayment occurs in a setting where the individual's debt is common knowledge). Nonpayment is also easier to study empirically because information on the true amount owed already exists, while this typically has to be estimated to measure misreporting. A recent paper by Hallsworth et al. (2014) is the first to study the nonpayment, as opposed to the misreporting, of taxes. Like the conservation literature, they focus on pro-social appeals in letters sent out to taxpayers in the UK and find that the resulting psychological effects achieve large reductions in tax nonpayment.

More generally, our paper also relates to recent studies of information provision as a policy tool in various contexts ranging from providing water quality information (Madajewicz et al., 2007; Jalan and Somanathan, 2008; Bennear and Olmstead, 2008), through mitigating misleading advertising (Glaeser and Ujhelyi, 2010), to improving households' financial decisions (Duflo and Saez, 2003; Cole et al., 2011; Chetty and Saez, 2013). Our results suggest that apart from a direct increase in knowledge, information campaigns can affect behavior through other channels as well.

2 Research setting and design

2.1 Research setting

We conducted our research in cooperation with Odi Water, a small public water provider serving a group of "townships" (low income suburbs / villages) located approximately an hour's drive North of Pretoria (Figure 1 in the Appendix). The area has a well-functioning water infrastructure developed in the mid 1990s as part of government efforts to develop black neighborhoods after Apartheid. The provider is owned and managed by the local government which also reviews and sets the price schedule annually (in July).[6]

On the supply side, the water market operates much as it does in developed countries. All households have modern individual water meters on their property; the meter is read every month and the household receives a bill in the mail (showing amount used, current charges, as well as any previous balance); payment options available include paying at one of the many supermarkets, paying at the provider's office, paying at the bank, or paying on-

[6] Szabó (2013) provides further details on the setting as well as on the administrative data used here.

line. On the demand side, however, the market exhibits several anomalies. Many consumers apparently waste water - for example, it is not uncommon to see garden taps left open, with or without an overflowing bucket underneath. Households also appear to use water on some luxuries, such as washing their cars at home, or irrigating a flowerbed or lawn in the dry season. As a result, households often accumulate large bills that they have difficulty paying. In our data, the average household's monthly water bill is around 7% of its income, and its overdue balance is 9 times as large.[7] Most consumers pay their bills infrequently. In the 3 months preceding our treatment, about a quarter of the households in our sample did not pay their bill, and only 15% paid every month. Payments that do occur are often in round figures, unrelated to the consumer's last bill or outstanding balance. Total payments over the same 3-month period were in multiples of 100 Rand for half of the households that made any payments (see Figure 2 in the Appendix). Since consumers typically pay at large supermarkets, banks, or the provider's office, round figures cannot be explained by a lack of small change but likely reflect households' attempt to budget for water in the face of large outstanding balances.

Unpaid balances accrue interest, and the provider restricts the water supply of the worst offenders. This is done by installing a flow limiter that reduces water flow to a bare minimum. Restricted households are charged an additional fee for this device.

Clearly, waste and nonpayment are costly both to the households and to the water utility. Why do these behaviors arise? Based on Odi Water's experience, as well as our own visits in the field, households' lack of understanding regarding water consumption is a major cause. The issue is not the availability of information: indeed, information is widely available in a format that most consumers from Western countries would consider standard (water meter on the property, detailed monthly bills, a customer service department to answer questions). In our baseline survey described below, 99.5% of households knew where their water meter was located,[8] 97.8% understood the basic operation of the meter (that numbers on the dial would increase when water was being used), and 95.7% stated that they regularly receive water bills. The issue is also not that consumers simply do not care about water. In our sample, close to 40% of respondents stated recently talking to neighbors or friends about water use. Instead, the primary issue appears to be that consumers have trouble understanding the information that is presented to them. For example, over 80% of consumers were unable to tell their consumption from their water bill. In general, households exhibited very little

[7] By comparison, the average US household spends less than 0.5% of its income on water (American Water Works Association, 1999).

[8] By comparison, in one North-American study, 11.3% of respondents did not know whether they had one or two water meters (American Water Works Association, 1999). This exemplifies the higher salience of water related issues in Southern Africa.

familiarity with the meaning of the numbers on the meter and the units in which their water consumption was being measured. When asked to guess how much water their household used, only 8 households (1%) stated their consumption in kiloliters, the units of measurement used by the provider (1 kl = 1000 liters ≈ 264 gallons). While in principle household could have multiplied their consumption by 1000 and responded accurately in liters, it is clear that this did not happen. Among those answering in liters, 98% gave numbers lower than the median consumption of 12,000 liters, and 61% gave numbers less than or equal to 1000 liters. There is also a lack of knowledge about the consumption process, e.g., how much water is used in various everyday activities. In a quiz, we asked households to compare pairs of activities in terms of their water usage. In each pair, one activity used at least twice as much water as the other. Only 14% of respondents ranked each pair correctly, and 45% ranked less than half of them correctly.

At least anecdotally, this lack of information is a major impediment to households' ability to manage their water consumption and make sure they can afford what they use. Consumers often complain about the size of their water bill and about not understanding why it is so high. While some households might *choose* to consume excessive amounts of water, this is clearly not the case for most.[9]

2.2 Hypotheses

To study whether information can improve efficiency and reduce nonpayment in this market, we designed and implemented a water education campaign. As described in detail in Section 2.3 below, we provided information to households on various aspects of the consumption process (water meter, bill, ways to conserve water) in an accessible manner through individual household visits. If this treatment is effective at increasing information and improving efficiency, we expect to see an improvement in household's ability to manage their consumption. We should see an increase in households' knowledge (as measured in our follow-up survey), an increase in conservation practices, and a higher propensity to make payments. With regards to quantity of water consumed, the prediction is ambiguous. Increased information might lead to less waste, which will tend to reduce consumption, but this in turn could lead to increased consumption in other activities (for example, upon learning how much water baths use compared to showers, a household could substitute taking baths with taking showers). We show this point formally in the Appendix.

[9] In fact, some consumers have started to voluntarily request that the provider install restriction devices on their service to help them better manage their consumption.

H1: Information effect. The information campaign should result in increased consumer knowledge, increased use of conservation practices, and more payments. The effect on consumption is ambiguous.

Given the emphasis on psychological motives behind conservation in the existing literature, another possibility presents itself. Our education program could exert an influence on payment and consumption through at least three psychological or "nudging" channels. First, the education visit may remind a consumer of his outstanding bill or make his water choices more salient. Allcott and Rogers (2014) analyze such reminder effects in inducing electricity savings in US conservation campaigns. Relatedly, Jessoe and Rapson (2013) find that increasing the salience of electricity usage induces households to conserve more energy. Zwane et al. (2011), Karlan et al. (2012) and Stango and Zinman (2014) provide evidence on the role of reminders in other contexts.

Second, the consumer could feel compelled to pay his bill or reduce his water usage because the visit might suggest to him that this is what he "should" do, either by highlighting social norms or increasing perceived scrutiny by the provider. For example, Allcott (2011) and Ferraro and Price (2013) show that social pressure is an effective tool to induce conservation in the US. Our intervention may have had a similar effect even though, as described above, we went to great lengths to ensure that the education visits focus on transmitting neutral information, rather than prescriptive messages on how consumers should behave.

Third, the consumer might appreciate the provider's efforts in reaching out to the households, and might make more payments to reciprocate this. In the tax avoidance literature, emotions like reciprocity and trust towards the government are thought to be relevant determinants of payments (see Bazart and Bonein (2014) and the references therein).

All these possibilities, which we will refer to as "psychological effects," can lead to more conservation and payments without changing households' information. As above, conservation in some activities may lead to substitution towards other water using activities, so the prediction on consumption is again ambiguous.

H2: Psychological effects. The information campaign should not change consumer knowledge, but should increase the use of conservation practices and lead to more payments. The effect on consumption is ambiguous.

In general, the distinction between information effects and psychological effects is not entirely clear-cut in the literature. Any campaign may provide both information and psychological nudges - for example, telling households about their neighbors' consumption, as in US campaigns, simultaneously conveys information and appeals to social norms. One contribution of this paper is to use a research design that allows us to isolate the information channel because we explicitly measure households' knowledge.[10] In addition, we are able to directly address the salience / reminder channel by testing for survey effects, as described in Section 4.4 below.

2.3 Water education program

In an attempt to improve households' information, we designed and implemented an in-depth water education program in cooperation with Odi Water officials. The program consisted of household visits by Odi Water education officers trained by us specifically for this project. Visits were conducted in November and December 2012, and each visit lasted between 30 minutes to 1 hour.

During the visit, the officers gave the households 5 brochures containing information on specific aspects of water usage: reading the water meter, understanding the bill, detecting and fixing leaks, tips on conserving water indoors, tips on conserving water outdoors. They explained the contents of the each brochure to the household, highlighting specific points agreed upon during our training session. We designed and wrote the brochures ourselves, with feedback from Odi Water's marketing department, drawing from water information campaigns developed for primary school students in South Africa, as well as public information campaigns in the US. (Copies of the brochures are available on the authors' websites.) All information in the brochures was presented in an accessible and reader-friendly manner (colors, pictures, examples). For example, one section of the brochure on indoor water conservation explained how to save water with every toilet flush. "Step 1: Use a large soft drink bottle (or several small ones). Fill it partially with pebbles. Fill the rest of the way with water. Step 2: Close the lid tightly and place it in the tank. If it floats or moves around, go back and add more pebbles. Make sure that the bottle doesn't obstruct the flushing mechanism." These instructions were accompanied by a picture of someone filling a plastic bottle, and another one showing the bottle sitting in the toilet tank. The water bill brochure showed a picture of a water bill, highlighting and explaining the most important pieces of information shown on the bill (last month's usage, amount due, outstanding balance, etc.). Brochures were available both in English and the main local language (Setswana), and the education officers conducted the visits in the households' preferred language. Feedback from the experiment suggested that households were delighted with the information campaign.

[10] By "information" we will mean knowledge of facts as measured in our surveys. This includes understanding of water quantities, one's bill and consumption, the price of water, and which everyday activities use the most water. Our findings regarding changes of information refer to the 12 measures we use to capture these (see Section 3.4 below for details).

Compared to previous interventions analyzed in the literature, our treatment had three distinguishing features. First, it was an in-depth education campaign. Most programs in the literature consist of simple interventions like a letter sent out to households or a flyer included with their monthly bill. Our officers personally visited each household and provided them with extensive information on various features of the consumption process. Second, our treatment was explicitly focused on information provision, and we deliberately tried to minimize the social pressure component as much as possible. Our education materials used descriptive rather than prescriptive language. For example, they described the various ways available for households to pay their water bill but did not say "you should pay your bills." The education officers were also trained to provide information only and not tell households what they should or should not do. Third, we conducted our campaign in a developing country setting where the lack of information is known to be a serious issue.

2.4 Sampling, implementation, and data

Sampling. In February 2012, we randomly selected 500 treatment and 500 control households to participate in the project. We used the population of residential water consumers of Odi Water, excluding commercial users. We excluded consumers using more than 300 kl (or 25 times the average). These accounts, comprising 0.3% of the population, are likely associated with unreported commercial activities or major leaks. We also excluded consumers whose account was less than a year old to ensure that participating households would all be experienced in using the local water infrastructure, paying the provider's bills, etc. Participating households were selected via stratified random sampling, with stratification based on administrative information available at the time. This included water consumption (quartiles of the population),[11] registered "indigent" status providing discounted water pricing (yes/no),[12] whether the consumer was restricted (yes/no), and whether the consumer had made a payment on his water bill during the previous year - resulting in 32 strata.

Implementation. A baseline survey was administered to participating households in March - April 2012. This baseline survey collected household characteristics, as well as detailed information regarding households' knowledge about water consumption. Our survey used an independent local survey company with extensive field experience in the area, and the surveyors were young people living in neighboring communities. They introduced themselves as working for researchers at the University of Houston who were interested in

[11] 1-6 kl, 7-10 kl, 11-16 kl, and above 16 kl.

[12] Subject to an income threshold, households can register with the municipality as "indigent" to receive discounted pricing on various public services. In the case of water, indigent households receive the first 6 kl free of charge.

gathering information about water needs and water usage in the area. Most locals are quite social, and the typical interview would feel like a conversation about the topics in the survey rather than a formal Q&A session. When training our surveyors, we particularly encouraged this approach for questions designed to measure households' knowledge. We wanted to make sure that respondents would feel at ease telling us what they knew and did not know, rather than feel that they were taking a test. The goal was to present these question as if they were part of a "fun guessing game."

The education program took place in November - December 2012: Odi Water education officers visited the 500 treatment households. Finally, a follow-up survey was administered to all participating households in February 2013. Note that the water price schedule, reviewed by the local government every July, is fixed throughout the intervention and the followup survey. In our regressions below, all payment and consumption data corresponds to the same price schedule.

Throughout the project our unit of analysis is the household. This makes sense because water is consumed, and paid for, jointly by all members of the household, and both consumption and payment is measured at the household level. It was also logistically infeasible to target our treatment to specific individuals within the household.[13]

Missing data. Due to logistical difficulties and funding issues, we only managed to gather baseline survey data for 803 households. (Note that administrative baseline data, including data on consumption, payment, restriction and indigent status is available for the entire sample.) For regressions where we control for baseline survey characteristics, simply dropping observations with missing baseline data would result in potentially biased estimates as we would be analyzing a potentially imbalanced sample. Instead, we deal with missing baseline information by imputation, as is done in the medical literature on randomized controlled trials (White and Thompson, 2005).[14] Specifically, for categorical variables, we create an additional "missing" category, while for continuous variables, we replace missing values with their means, and create an additional indicator that takes a value of 1 for these observations, and 0 otherwise. While this procedure might lead to biased coefficients for the control variables, it will not bias the estimated treatment effects.

[13] For both the surveys and the treatment, households were identified based on their billing information, which included the name (last name and first initial) and address the account was under. Surveyors and education officers were instructed to look for the person whose name was on the bill. If that person was not home, they were to talk to an adult member of his or her household (and revisit if such a person was not available either). Targeting specific individuals would have required collecting personal information to identify those individuals. This would have raised human subjects concerns and would have made respondents less willing to participate.

[14] See Fairlie and Robinson (2013) for a recent example from economics. Our results are robust to using controls from the follow-up instead of the baseline.

During the education visits and the follow-up survey, 8 of our participating 1000 households could not be reached after multiple attempts or refused. Furthermore, an examination of Odi Water records revealed a name change on the account of 26 households during our study period. We exclude these households from the analysis, and restrict our attention to the remaining 966 households, implying a low attrition rate of 3.4%.[15]

Sample characteristics. Table 1 shows means and standard deviations of various observables in our treatment and control groups. Not surprisingly, given the fine level at which we were able to stratify, the two groups are fully balanced on observables.

3 Specification and results

3.1 Specification

Given our randomized treatment, we can estimate treatment effects consistently from the following simple regression:

$$y_i = \beta_0 + \beta_1 T_i + \varepsilon_i, \tag{1}$$

where y_i is the outcome of interest for household i and T_i is an indicator equal to 1 for treated households. To increase the precision of the estimates, we sometimes include in (1) indicators for the strata used in sampling, the baseline value of the outcome y, and various demographic controls.

3.2 Payment

We begin by studying changes in consumers' payment behavior caused by the treatment. The first row of Table 2 compares households' total payment (in logs) in the three months following the treatment between the treatment and control groups. In column (1) we only control for total payment in the three months prior to the treatment and find that, following the treatment, the average treated household paid 32% more than the average control household. The magnitude of this estimate drops to 25-26% but remains robustly significant when including indicators for the sampling strata (column 2), a variety of socio-economic characteristics from the baseline survey (column 3), and average monthly consumption in the three months before the treatment (column 4). As we show below, we do not see an increase in consumption in response to the treatment. Thus, the increased payment we find is not explained by households simply using more water.

[15] Out of these 966, we have baseline data for 776 households (80%). We impute missing baseline data as described above. Of course, missing follow-up data is never imputed, so the number of observations in some regressions is less than 966 due to missing variables.

Table 1: Testing the balance of observables across groups

	Control	Treatment	Difference
Consumption (kl)	15.001	16.969	1.967
	(0.628)	(1.333)	(1.473)
Payment (Rand)	278.450	242.509	-35.941
	(18.337)	(15.941)	(24.297)
Payment (yes/no)	0.566	0.515	-0.050
	(0.023)	(0.023)	(0.032)
Restricted	0.294	0.292	-0.003
	(0.021)	(0.021)	(0.029)
Indigent	0.286	0.298	0.012
	(0.021)	(0.021)	(0.029)
Baseline survey	0.812	0.795	-0.017
	(0.018)	(0.018)	(0.026)
Informal shacks	0.123	0.129	0.006
	(0.017)	(0.017)	(0.024)
Employed hh members	1.048	0.996	-0.052
	(0.032)	(0.030)	(0.044)
HH size	4.338	4.481	0.143
	(0.078)	(0.094)	(0.122)
No formal schooling	0.010	0.005	-0.005
	(0.005)	(0.004)	(0.006)
Some primary school	0.010	0.010	-0.000
	(0.005)	(0.005)	(0.007)
Primary school	0.065	0.088	0.023
	(0.013)	(0.014)	(0.019)
Some high school	0.217	0.202	-0.016
	(0.021)	(0.020)	(0.029)
High school	0.434	0.432	-0.003
	(0.025)	(0.025)	(0.036)
Some higher educ.	0.165	0.152	-0.013
	(0.019)	(0.018)	(0.026)
Higher education	0.098	0.111	0.013
	(0.015)	(0.016)	(0.022)
Hot water	0.691	0.641	-0.050
	(0.023)	(0.024)	(0.034)
Owns car	0.369	0.364	-0.005
	(0.025)	(0.025)	(0.035)
Owns fridge	0.977	0.982	0.005
	(0.008)	(0.007)	(0.010)
Income (Rand)	7,056.548	6,736.557	-319.990
	(236.554)	(226.010)	(327.167)
N. sampled neighbors	1.134	1.251	0.117
	(0.050)	(0.054)	(0.074)
Has treated neighbor	0.466	0.444	-0.022
	(0.023)	(0.023)	(0.032)

Notes: The table presents the means of various observables in the treatment and control groups as well as their difference, with standard errors in parentheses. ’Consumption’ is average consumption in the 3 months prior to the treatment. ’Payment (Rand)’ is the household’s total payment during this time, and ’Payment (yes/no)’ is 1 if the household has made a payment. ’Baseline survey’ is 1 if we have baseline survey information on the household. ’Informal shacks’ is 1 if there are informal shacks on the property. ’Hot water’ is 1 if the household has hot running water. ’N. sampled neighbors’ is the number of households included in the sample in a 100 meter radius, and ’Has treated neighbor’ is 1 if one of these households is in the treatment group. In the third column ***, **, * denotes statistical significance at the 1, 5, and 10 percent level, respectively.

Table 2: Payment amount

Dependent variable	Mean and std. dev.	(1)	(2)	(3)	(4)
January - March	3.211	0.320**	0.251*	0.266**	0.262**
total payment	(3.021)	(0.136)	(0.129)	(0.130)	(0.129)
January payment	1.793	0.247*	0.214	0.229*	0.241*
	(2.543)	(0.136)	(0.135)	(0.136)	(0.138)
February payment	1.833	0.282**	0.246*	0.221	0.222
	(2.537)	(0.137)	(0.137)	(0.138)	(0.139)
March payment	1.959	0.180	0.128	0.126	0.115
	(2.624)	(0.141)	(0.137)	(0.138)	(0.138)
Number of observations		966	966	966	947
Strata indicators		No	Yes	Yes	Yes
Demographic controls		No	No	Yes	Yes
Pre-treatment consumption		No	No	No	Yes

Notes: Each cell presents the estimated treatment effect from a different regression. The first column gives the dependent variable, and columns (1)-(4) correspond to different specifications. All payment and consumption measures are in logs. Each specification includes average monthly payment during the 3 months prior to the treatment. 'Demographic controls' are the number of children, teenagers, adults in the household, number of employed members, education of respondent, household income, and whether the household has hot running water, owns a car, or owns a refrigerator. 'Pre-treatment consumption' is average consumption during the 3 months prior to the treatment. Robust standard errors in parentheses. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

Table 3: Payment propensity

Dependent variable	Mean and std. dev.	(1)	(2)	(3)	(4)
January - March	0.545	0.050**	0.038*	0.041*	0.040*
payment (0/1)	(0.498)	(0.024)	(0.022)	(0.022)	(0.022)
January payment (0/1)	0.342	0.045*	0.038	0.040	0.043*
	(0.474)	(0.026)	(0.026)	(0.026)	(0.026)
February payment (0/1)	0.355	0.055**	0.048*	0.043*	0.043
	(0.479)	(0.026)	(0.026)	(0.026)	(0.026)
March payment (0/1)	0.367	0.034	0.024	0.023	0.021
	(0.482)	(0.027)	(0.026)	(0.026)	(0.026)
Number of observations		966	966	966	947
Strata indicators		No	Yes	Yes	Yes
Demographic controls		No	No	Yes	Yes
Pre-treatment consumption		No	No	No	Yes

Notes: Each cell presents the estimated treatment effect from a different regression. The first column gives the dependent variable: 1 if the household made a payment over the given period, 0 otherwise. Columns (1)-(4) correspond to different specifications. Each specification controls for whether the household made a payment during the 3 months prior to the treatment. 'Demographic controls' are the number of children, teenagers, adults in the household, number of employed members, education of respondent, household income, and whether the household has hot running water, owns a car, or owns a refrigerator. 'Pre-treatment consumption' is average consumption during the 3 months prior to the treatment (in logs). Robust standard errors in parentheses. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

The remaining rows of the table indicate that the increase in monthly payments is short lived: the effect remains similar in the first two months but falls by almost a half and becomes insignificant by the third month. It is important to note that the effects are not reversed. In principle, our treatment could have encouraged households to simply move their payments forward in time, leading to lower payments in later periods. We do not find evidence of this in the data. Thus, in our sample, the provider appears to have achieved a one-time, net 25-30% increase in payments as a result of the information campaign.

Did the extra payment come only from households paying more, or did households' propensity to pay increase as well? Table 3 shows that the treatment increased the fraction of households making at least one payment in the three months following the treatment by 4-5 percentage points (relative to a mean of 54.5%). This effect is again driven by the two months immediately following the treatment. Table 4 shows that treated households also made significantly more payments, with a small increase of around 0.1 extra payment relative to a mean of 1.1 over the three-month period.

Since payments are bounded below by 0, estimation methods that take into account such censoring may provide more precise results. In Table 5, we estimate treatment effects on payment amounts using Tobit regressions. These give somewhat larger marginal effects than

Table 4: Payment frequency (3 months)

	(1)	(2)	(3)	(4)
Treatment	0.097**	0.088*	0.092**	0.092**
	(0.048)	(0.046)	(0.046)	(0.047)
Number of observations	966	966	966	947
Strata indicators	No	Yes	Yes	Yes
Demographic controls	No	No	Yes	Yes
Pre-treatment cosumption	No	No	No	Yes

Notes: Each column corresponds to a different regression. The dependent variable is the number of payments made by the household in the 3 months following the treatment (mean = 1.064, std. dev. = 1.136). Each specification controls for the number of payments the household made during the 3 months prior to the treatment. 'Demographic controls' are the number of children, teenagers, adults in the household, number of employed members, education of respondent, household income, and whether the household has hot running water, owns a car, or owns a refrigerator. 'Pre-treatment consumption' is average consumption during the 3 months prior to the treatment (in logs). Robust standard errors in parentheses. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

those presented above. For example, we estimate a 26% increase on 3-month payments due to our treatment among those who make positive payments, and a much larger unconditional effect of 37% (reflecting the fact that some households switched from 0 to positive payments). Estimating the effects on the propensity to make payments with Probit instead of OLS also yields larger point estimates.

The above treatment effects could be underestimated if the treatment induced some households to register as indigent and receive a free water allowance. In the Appendix, we show that there is no evidence that our treatment had an effect on households' registered indigent status.

3.3 Consumption

This section studies the effects of our treatment on consumption. Although our predictions regarding consumption are ambiguous, it is an interesting outcome to investigate for a number of reasons. First, we need to make sure that any changes in payment behavior are not mechanically due to changes in consumption. Second, especially in southern Africa, water is a scarce commodity and conservation is likely to yield large positive externalities.

In the first row of Table 6, the dependent variable is the log of average household consumption in the three months following the treatment. The following rows look at each of these months separately, and columns (1-3) successively add various controls. The absence

Table 5: Treatment effects on payment amount and propensity: Tobit and Probit estimates

Period	Treatment effect on payment (1)	Treatment effect on payment | payment > 0 (2)	Treatment effect on payment propensity (3)
Jan-March	0.367**	0.258**	0.082**
	(0.171)	(0.120)	(0.038)
January	0.228*	0.192*	0.055*
	(0.128)	(0.108)	(0.032)
February	0.247*	0.202*	0.064**
	(0.133)	(0.109)	(0.033)
March	0.200	0.159	0.045
	(0.142)	(0.113)	(0.033)

*Notes:*Marginal effects of the treatment indicator from Tobit (columns 1 and 2) and Probit (column 3) regressions. The first column gives the period of the dependent variable. In columns (1) and (2), the dependent variable is log payment over the given period. Column (1) presents unconditional marginal effects, and column (2) marginal effects conditional on positive payments (from the same regression). In column (3) the dependent variable is an indicator equal to 1 if the household made a payment over the given period and 0 otherwise. Columns (1) and (2) control for total payment in the 3 months before the treatment, and column (3) controls for whether a payment was made during this period (marginal effects are evaluated at the means of the controls). Robust standard errors in parentheses. N = 966. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

Table 6: Consumption

Dependent variable	Mean and std. dev.	(1)	(2)	(3)
January - March	2.470	0.010	-0.016	-0.014
average consumption	(0.671)	(0.031)	(0.032)	(0.033)
January consumption	2.095	-0.049	-0.068	-0.065
	(0.978)	(0.058)	(0.060)	(0.060)
February consumption	2.632	0.021	-0.016	-0.014
	(0.688)	(0.018)	(0.024)	(0.024)
March consumption	1.700	-0.059	-0.052	-0.051
	(1.478)	(0.096)	(0.095)	(0.096)
Number of observations		947	947	947
Strata indicators		No	Yes	Yes
Demographic controls		No	No	Yes

Notes: Each cell presents the estimated treatment effect from a different regression. The first column gives the dependent variable: log consumption over the given period. Columns (1)-(4) correspond to different specifications. Each specification controls for average consumption during the 3 months prior to the treatment (in logs). 'Demographic controls' are the number of children, teenagers, adults in the household, number of employed members, education of respondent, household income, and whether the household has hot running water, owns a car, or owns a refrigerator. Robust standard errors in parentheses. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

of an average treatment effect can never be rejected. In Table 7 we break up the sample into consumption quartiles. This shows some evidence that, in response to the treatment, low consumers increased their consumption while high consumers reduced it. In particular, we find a significant reduction of 9.5% among the highest consumers in the sample.

These patterns are consistent with both pure information and pure psychological effects. They may indicate substitution between various water using activities coupled with increased conservation, particularly among the highest users. To investigate this further, we look at households' self-reported conservation behavior.

Table 7: Treatment effects on consumption by consumption quartile

	First quartile	Second quartile	Third quartile	Fourth quartile
	Less than 7 kl	7 - 11 kl	12 - 19 kl	More than 19 kl
Treatment	0.083	0.043	-0.023	-0.095*
	(0.069)	(0.063)	(0.056)	(0.052)
N	227	246	245	229

Notes: Each column regresses average consumption in the 3 months following the treatment on a treatment indicator on a different consumption quartile of the sample. Robust standard errors in parentheses. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

Table 8: Effect of treatment on conservation

Dep. var	Mean and std. dev.	Treatment effects (1)	(2)	(3)	(4)
Use rainwater	0.013	-0.006	-0.007	-0.005	-0.007
	(0.115)	(0.007)	(0.008)	(0.007)	(0.007)
Reuse water	0.291	0.013	0.014	0.015	0.012
	(0.455)	(0.029)	(0.029)	(0.029)	(0.029)
Repair leaks	0.415	0.104***	0.103***	0.104***	0.103***
	(0.493)	(0.032)	(0.031)	(0.031)	(0.032)
Conserve with laundry	0.291	0.084***	0.083***	0.083***	0.082***
	(0.455)	(0.029)	(0.029)	(0.029)	(0.029)
Conserve with irrigation	0.252	-0.040	-0.040	-0.040	-0.041
	(0.434)	(0.028)	(0.028)	(0.028)	(0.028)
Number of actions	1.361	0.167**	0.166**	0.166**	0.155**
	(1.108)	(0.071)	(0.070)	(0.070)	(0.071)
No action	0.224	-0.033	-0.033	-0.034	-0.030
	(0.417)	(0.027)	(0.026)	(0.026)	(0.027)
Strata indicators		No	Yes	Yes	Yes
Baseline dep. var.		No	No	Yes	Yes
Demographic controls		No	No	No	Yes

Notes: Each cell presents the estimated treatment effect from a different regression. The first column gives the dependent variable. Except for the last two, these are dummies for whether the respondent reported having taken the action to conserve water. 'Number of actions' is the number of actions the household reported. 'No action' is a dummy equal to 1 if the household did not report taking any action. Columns (1-4) correspond to different specifications. 'Demographic controls' are the number of children, teenagers, adults in the household, number of employed members, education of respondent, household income, and whether the household has hot running water, owns a car, or owns a refrigerator. Robust standard errors in parentheses. N = 965. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

Our surveys asked whether the household recently took actions to conserve water, listing several possibilities. Table 8 looks for treatment effects in these answers. Treated households are 10 percentage points more likely to report fixing leaks around the house and 8.5 percentage points more likely to report conserving water during laundry ("Use washing machine less / use fuller loads"). Treated households also report taking more actions than control households, although the fraction of households taking no action does not differ significantly between the two groups. This suggests that the treatment primarily increased conservation on the intensive margin, among households already taking steps to conserve water.

These findings on conservation are again consistent with both pure information and pure psychological effects. Households may have learned new information that helped them use water more efficiently, leading to conservation (information effect). On the other hand, households could also simply follow the conservation practices mentioned in our education materials even if they did not fully understand why these are useful (psychological effect).[16]

3.4 Information

We saw that our treatment raised households' propensity to pay their water bill and increased their self-reported conservation activities. What is the mechanism behind these effects? An important element of our research design is that we are able to look at households' information directly, and evaluate the extent to which change in knowledge is responsible for the treatment effects we found above.

Our information campaign focused on three key areas of the water consumption process: (1) Understanding the meter; (2) Understanding the bill; (3) Understanding water quantities used in everyday activities. Separate sections in our surveys were designed to measure each of these areas. As described in Section 2.4, we took steps to ensure that respondents did not feel like they were being tested and felt comfortable telling our surveyors what they knew and did not know.

The meter. As described in Section 2.1, although households know where their meter is located and understand what it is for, there is a lack of understanding about what the numbers mean and the units in which water is measured. Our information campaign significantly raised households' familiarity with the concept of a kiloliter, increasing by 4 percentage points the number of households who gave us their estimated water usage in kiloliters (first row of Table 9). However, many households seem to have become familiar with the word "kiloliter" without learning what it means. Over 60% of those who answered in kiloliter after the treatment gave unrealistic numbers of several hundred or even thousands of kiloliters. Again, responses in liters were too low, with 90% giving numbers less than 1000 liters. When asked how many liters a kiloliter represented, only 3 respondents gave the correct answer. Others didn't know or were off by a factor of 10, with no significant difference between treatment and control. Households' learning about how water consumption is measured was superficial.

The bill. A large majority of households state that they understand their water bill: only 6% view the bill as "almost impossible to understand." Our information campaign may have decreased this further, although the effect is not significant (Table 9). However, stating that the bill is understood does not mean that the respondent actually understands it. In the

[16] A third possibility is that households could lie about taking conservation actions to satisfy perceived social expectations. While we cannot rule this out definitively, we find it reassuring that there are no significant differences in reports of using rainwater or reusing household water in Table 8. Neither of these practices was mentioned in our education campaign. Households report more conservation activities in those areas that were explicitly covered in the campaign.

Table 9: Effect of treatment on information

Dep. var	Mean and std. dev.	N	Treatment effects (1)	(2)	(3)	(4)
Response in kl	0.104	953	0.038*	0.038*	0.037*	0.036*
	(0.305)		(0.020)	(0.020)	(0.020)	(0.020)
Bill hard to understand	0.062	952	-0.024	-0.024	-0.023	-0.026
	(0.241)		(0.016)	(0.016)	(0.016)	(0.016)
Reads consumption from bill	0.397	731	0.036	0.042	0.043	0.046
	(0.490)		(0.036)	(0.037)	(0.037)	(0.037)
Consumption accurate	0.114	731	0.037	0.037	0.038	0.034
	(0.317)		(0.023)	(0.023)	(0.023)	(0.023)
Tariff in ballpark	0.045	820	-0.017	-0.017	-0.016	-0.017
	(0.208)		(0.014)	(0.014)	(0.015)	(0.015)
Tariff error	70.591	396	-21.967	-5.935	-6.141	-3.922
	(171.806)		(17.540)	(9.637)	(9.256)	(9.217)
Increasing tariff	0.699	964	-0.023	-0.021	-0.023	-0.020
	(0.459)		(0.030)	(0.030)	(0.030)	(0.030)
N. correct answers	2.485	965	0.057	0.056	0.059	0.046
	(1.002)		(0.064)	(0.064)	(0.064)	(0.065)
Q1 correct	0.459	966	-0.006	-0.006	-0.005	-0.012
	(0.499)		(0.032)	(0.032)	(0.032)	(0.032)
Q2 correct	0.726	966	0.052*	0.053*	0.050*	0.051*
	(0.446)		(0.029)	(0.029)	(0.028)	(0.029)
Q3 correct	0.604	965	-0.005	-0.005	-0.005	-0.013
	(0.489)		(0.032)	(0.032)	(0.032)	(0.032)
Q4 correct	0.697	966	0.015	0.013	0.017	0.019
	(0.460)		(0.030)	(0.030)	(0.030)	(0.030)
Strata indicators			No	Yes	Yes	Yes
Baseline dep. var.			No	No	Yes	Yes
Demographic controls			No	No	No	Yes

Notes: Each cell presents the estimated treatment effect from a different regression. The first column gives the dependent variable. 'Response in kl' is 1 if the respondent's guess about their consumption is stated in kiloliters. 'Reads consumption from bill' is 1 if the respondent was able to find a water bill and reads out their consumption from the bill. 'Consumption accurate' is 1 if this number matches any consumption in the administrative data from the prior 6 months. 'Tariff in ballpark' is 1 if the respondent's guess about the kiloliter price is between 5-25 Rand. 'Tariff error' is max(0,the respondent's guess about kiloliter price - 25). 'Increasing tariff' is 1 if the respondent understands that the tariff schedule is increasing. The last 5 rows are the number of correct answers to the quiz and indicators for whether individual questions were answered correctly. 'Please take a guess: Do you think more water is used... Q1. by the baths/showers your household takes during the month OR by washing your clothes during the month. Q2. if you fill 2 two-liter bottles of soda with water OR if you flush the toilet once. Q3. if you use the outside hose for 10 minutes OR if you do one load of laundry. Q4. if you open the tap for 1 minute OR with the water a person drinks in a day.' Columns (1-4) correspond to different specifications. 'Demographic controls' are the number of children, teenagers, adults in the household, number of employed members, education of respondent, household income, and whether the household has hot running water, owns a car, or owns a refrigerator. Robust standard errors in parentheses. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

follow-up survey, with the water bill in their hands, 60% of respondents admit to not being able to tell their consumption from the bill, and another 28% read out an incorrect number from the bill. Overall, less than 12% of households are able to tell their consumption from the bill.[17] There was no significant difference between treatment and control (Table 9).

Households also do not know how much water costs. In the follow-up, less than 5% of households gave numbers in the ballpark of the true kiloliter price. These are the households who state prices between 5 and 25 Rand (the true kiloliter price is between 10 and 21, depending on consumption). About half of the remaining households say that they don't know the price, and the other half report prices that are much higher – the mean answer is 95 Rand. There was no difference between treatment and control either in the fraction of households whose answers were in the ballpark of the true price, or in how far off reported prices were from realistic values (Table 9). There was no difference in knowing the fact that the price schedule is increasing, i.e., that an additional kiloliter costs more when consumption is high than when it is low (Table 9).[18] Households' learning about their water bill was at best superficial.

Quantities of water used. We used a quiz to measure households' understanding of quantities of water used in various everyday activities. Four questions asked households to guess which of two activities used more water (e.g., using the outside hose for 10 minutes, or doing one load of laundry). In each pair, one activity typically uses at least twice as much water as the other. We based these questions on materials used in a South African primary school program. Like all other questions, these were read to the respondent by the surveyor, and we trained our surveyors to present the questions as a fun guessing exercise rather than as a test. The average number of correct answers is 2.5 for both the baseline and the follow-up survey, and the distribution of the number of correct responses is also very similar. There were no differences between treatment and control (Table 9).

Looking at the fraction of correct answers to individual questions, we only find a significant effect for one of them, regarding the amount of water used when flushing the toilet.[19] This turns out to be a relevant dimension as the toilet is typically a major source of indoor water consumption. In a study of US and Canadian cities, the toilet was found to be the largest single source of indoor water use, responsible for 26.7% of consumption (American Water Works Association, 1999). Our treatment had a modest effect, raising the fraction

[17] We cannot be entirely sure about exactly which bill the respondent was looking at. 11.4% of respondents stated a number that corresponded to *any* bill the household received in the 6 months prior to the survey. This likely overestimates the fraction of households giving correct answers.

[18] To measure this, we asked households to imagine flushing the toilet 1000 times and to guess whether the last flush would cost them more or less than the first.

[19] "Do you think more water is used (a) if you fill 2 two-liter bottles of soda with water, or (b) if you flush the toilet once?" The correct answer is (b) for all toilet tanks used in this area.

of correct answers by 5%, relative to a mean of 73% (Table 9). Overall, households' understanding of quantities of water used did not increase much in response to the information campaign.

Taken together, our education treatment had at best a modest effect on households' knowledge. Surprisingly, we do not see an increase in average information that would explain the reduction in nonpayment achieved by our education campaign.

4 Possible explanations

4.1 Spillovers

A potential concern with any information treatment is the possibility of spillovers. Treated households could talk to their neighbors about what they have learned, or they could give them the information brochures. Even if the treatment was effective at increasing households' information, such spillovers could result in no difference in information between the treatment and control groups. Could this be responsible for the lack of information effects we found above?

Note first that in most cases, we did not simply find the information of treatment and control groups to be similar, but also that they were both similarly low both before and after the treatment (post-treatment means are given in Table 9). While spillover effects from our treatment could potentially explain the first of these patterns, they are unlikely to account for the second. If the treatment had increased information and there were spillovers, we would expect to find increased knowledge in both the treatment and control groups.

To formally test whether spillovers were present in our intervention, we collected data to identify individuals who would be most likely to be exposed to information spillovers. First, our survey collected information on whether the respondent had talked to his neighbors or friends about water in the previous 6 months. If there were information spillovers, these would likely be present among the 39% who reported talking about water with others. Second, we collected each household's GPS coordinates and thus know their location relative to other households.[20] Information spillovers could occur between neighbors, and we can capture this by creating an indicator for whether a household has other treated households nearby.

Let $Exposure$ represent one of the above proxies for exposure to information spillovers.

[20] Although each property has a street address used for mail delivery, there is no official map of our study area that would contain these addresses. House numbers often follow each-other in surprising orders. Thus, GPS coordinates are the only way to map these households.

We estimate

$$Y_i = \beta_0 + \beta_1 Treat_i + \beta_2 Exposure_i + \beta_3 Exposure_i \times Treat_i + \varepsilon_i,$$

where, Y_i is one of our measures of respondent i's knowledge. If the treatment did have an effect on Y_i, but large spillovers caused us to find no effect, then we expect to find $\beta_1 > 0$ (treatment effect among those not exposed to spillovers) and $\beta_2 > 0$ (spillover effect in the control group). By contrast, if the treatment was indeed ineffective, we expect $\beta_1 = \beta_2 = 0$.

In Table 10, our measure of exposure is $Talks$, which takes a value of 1 if the respondent talked to neighbors about water in the previous 6 months. Our dependent variables are the main information measures that we found to be unaffected by our treatment. The Table also presents an F-test and the corresponding p-value for the hypothesis that $\beta_1 = \beta_2 = 0$ (no spillovers). For 5 out of 8 variables, the hypothesis of no spillovers is not rejected. In the remaining 3 columns (1, 7, and 8), the coefficients on $Talks$ is negative: if anything, individuals who talk to others have less information. In Column (6), we find a positive and significant β_1 but the point estimate on $Talks$ remains insignificant and negative. This suggests that the treatment may have been relatively more effective in improving the respondents' quiz scores among individuals who do not talk about water.[21] In none of these regressions does the evidence support the idea that the treatment raised information but was accompanied by large information spillovers ($\beta_1 > 0$, $\beta_2 > 0$).

Table 11 presents corresponding regressions using GPS coordinates to identify a household's neighbors, and using the treatment status of a household's neighbors to capture potential exposure to information spillovers. The variable $Treated\ neighbors$ takes a value of 1 if there is one or more treated household in a 100 meter radius around the respondent. 45% of the households in our study have such a neighbor, and the number of treated neighbors ranges between 0 and 4. The results in Table 11 also reject the idea that spillover effects could explain the lack of information effects found above. We do not find any support for the hypothesis that $\beta_1 > 0$ and $\beta_2 > 0$. In some cases, having neighbors in the treatment group is associated with significantly worse information.

[21] This does not appear to be because these individuals had a lower level of knowledge to start with. In fact, individuals with $Talks_i = 0$ had a slightly higher average score at baseline (2.55 vs. 2.52, the difference is not statistically significant). Instead, individuals who talk less about water with others may have been more attentive during the education visit.

Table 10: Checking for spillover effects I: Talking to neighbors

	(1) Response in kl	(2) Bill hard to understand	(3) Reads consumption from bill	(4) Consumption accurate	(5) Tariff in ballpark	(6) Tariff error	(7) Increasing tariff	(8) N. correct answers
Treatment	0.040	-0.022	0.041	0.015	-0.020	-30.092	0.006	0.138*
	(0.026)	(0.021)	(0.049)	(0.031)	(0.018)	(21.937)	(0.036)	(0.082)
Talks	-0.028	-0.015	-0.042	-0.009	0.001	-7.799	-0.085*	-0.101
	(0.025)	(0.025)	(0.052)	(0.031)	(0.023)	(31.669)	(0.044)	(0.092)
Talks x Treatment	0.002	-0.006	-0.008	0.043	0.008	27.315	-0.063	-0.179
	(0.039)	(0.031)	(0.073)	(0.048)	(0.030)	(34.665)	(0.062)	(0.133)
N	947	946	726	726	815	391	958	959
F test (Treatment, Talks)	2.999	0.577	1.207	0.278	0.812	1.317	2.371	3.315
p value	0.050	0.562	0.300	0.757	0.445	0.269	0.094	0.037
Mean dep. var.	0.104	0.062	0.397	0.114	0.045	70.591	0.699	2.485

Notes: Each column lists the coefficient estimates from a different regression (apart from the constant). The column headings give the dependent variable. 'Response in kl' is 1 if the respondent's guess about their consumption is stated in kiloliters. 'Reads consumption from bill' is 1 if the respondent was able to find a water bill and reads out their consumption from the bill. 'Consumption accurate' is 1 if this number matches any consumption in the administrative data from the prior 6 months. 'Tariff in ballpark' is 1 if the respondent's guess about the kiloliter price is between 5-25 Rand. 'Tariff error' is max(0,the respondent's guess about kiloliter price - 25). 'Increasing tariff' is 1 if the respondent understands that the tariff schedule is increasing. 'N. correct answers' is the number of correct answers in our quiz. 'Talks' is 1 if the repondent answered Yes to 'In the previous 6 months have you talked to friends or neighbors about the way you use or save water?' Robust standard errors in parentheses. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

Table 11: Checking for spillover effects II: Treated neighbors

	(1) Response in kl	(2) Bill hard to understand	(3) Reads consumption from bill	(4) Consumption accurate	(5) Tariff in ballpark	(6) Tariff error	(7) Increasing tariff	(8) N. correct answers
Treatment	0.021	-0.045**	0.008	0.028	0.020	-13.413	-0.079*	0.030
	(0.027)	(0.021)	(0.049)	(0.032)	(0.020)	(9.537)	(0.041)	(0.088)
Treated neighbors	-0.023	-0.026	-0.098*	-0.020	0.037	32.376	-0.015	0.070
	(0.025)	(0.024)	(0.051)	(0.031)	(0.023)	(39.460)	(0.042)	(0.088)
Treated neighbors x Treatment	0.037	0.046	0.058	0.018	-0.082***	-20.982	0.127**	0.064
	(0.040)	(0.031)	(0.073)	(0.047)	(0.028)	(40.964)	(0.059)	(0.130)
N	953	952	731	731	820	396	964	965
F test (Treatment, Treated neighbors)	1.416	2.190	2.708	1.154	1.418	1.487	2.100	0.319
p value	0.243	0.112	0.067	0.316	0.243	0.227	0.123	0.727

Notes: Each column lists the coefficient estimates from a different regression (apart from the constant). The column headings give the dependent variable. 'Response in kl' is 1 if the respondent's guess about their consumption is stated in kiloliters. 'Reads consumption from bill' is 1 if the respondent was able to find a water bill and reads out their consumption from the bill. 'Consumption accurate' is 1 if this number matches any consumption in the administrative data from the prior 6 months. 'Tariff in ballpark' is 1 if the respondent's guess about the kiloliter price is between 5-25 Rand. 'Tariff error' is max(0,the respondent's guess about kiloliter price - 25). 'Increasing tariff' is 1 if the respondent understands that the tariff schedule is increasing. 'N. correct answers' is the number of correct answers in our quiz. 'Treated neighbors' is 1 if the repondent has a neighbor in the treatment group (in a 100 meter radius). Robust standard errors in parentheses. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

4.2 Information sharing within the household

Another possible explanation for the lack of a measured information effect is that information may not be shared within the household. As described in Section 2.4, it makes sense to consider the household as the unit of analysis since consumption and payment are measured at the household level. However, this raises the possibility that surveyed individuals within the household are different from treated individuals. To fix ideas, suppose that the education officers met with the wife, who is responsible for paying the water bill, and the treatment successfully increased her knowledge. Suppose this information channel explains the findings above. We may still measure no treatment effect on information if our surveyors in the follow-up survey talked to the husband *and* the wife failed to share her information with him.[22]

We perform two further tests to assess the possibility that information sharing within the household might be an important factor in explaining the findings above. First, based on the respondent's age and gender, we identify households where the same respondent is likely to have answered the baseline and the follow-up survey. If the same person answered both surveys, it is more likely that (s)he was also home during the education visit. Under the information story, these households should show the biggest increase in knowledge relative to the control group. We have 28 such households in the control and 25 in the treatment group. Including this indicator and its interaction with treatment status yields a significant interaction in only one case, but with the wrong sign (Table 12). Relative to the control group, treated households where the same person was home during both surveys do not have significantly more information than others.

Our second test is based on the idea that if information sharing within the household is a major factor, we would expect treatment effects to diminish as households get larger. This is both because information sharing within the household becomes harder in a larger household, and because a larger household makes it more likely that the education officers and the surveyors met with different members of the household. As before, the interaction of household size with treatment status is only significant in one regression, but with the wrong sign (Table 13). Relative to the control group, smaller treated households do not have more information than larger households.

Based on these measures, we do not see any evidence to suggest that our finding of no treatment effects on information is due to the lack of information sharing with households.

[22] In some sense, this explanation also falls under psychological effects (H2 in Section 2.2). If the treated individual does not share her information within the household but, e.g., simply tells her husband and children to change their water consumption habits, then the households' behavior as a whole changed due to a "social pressure" exerted by the wife. If by contrast she had explained to her family what she had learned, then this would have been reflected in the follow-up survey.

Table 12: Information sharing within the household I: Same respondent

	(1) Response in kl	(2) Bill hard to understand	(3) Reads consumption from bill	(4) Consumption accurate	(5) Tariff in ballpark	(6) Tariff error	(7) Increasing tariff	(8) N. correct answers
Treatment	0.042*	-0.033*	0.057	0.049*	-0.028*	-29.914	-0.024	0.090
	(0.023)	(0.018)	(0.042)	(0.027)	(0.017)	(21.584)	(0.034)	(0.075)
Same respondent	-0.010	0.067	0.244**	0.175*	-0.021	-15.463	-0.142	0.072
	(0.051)	(0.068)	(0.106)	(0.093)	(0.042)	(28.969)	(0.097)	(0.179)
Same respondent x Treatment	0.136	0.027	-0.277*	-0.087	0.034	12.192	0.013	-0.146
	(0.104)	(0.100)	(0.160)	(0.137)	(0.062)	(37.602)	(0.141)	(0.295)
N	763	763	578	578	666	327	774	774
F test (Treatment | Same respondent)	3.116	0.004	2.037	0.082	0.008	0.331	0.007	0.038
p value	0.078	0.948	0.154	0.774	0.927	0.565	0.933	0.845

Notes: Each column lists the coefficient estimates from a different regression (apart from the constant). The column headings give the dependent variable. 'Response in kl' is 1 if the respondent's guess about their consumption is stated in kiloliters. 'Reads consumption from bill' is 1 if the respondent was able to find a water bill and reads out their consumption from the bill. 'Consumption accurate' is 1 if this number matches any consumption in the administrative data from the prior 6 months. 'Tariff in ballpark' is 1 if the respondent's guess about the kiloliter price is between 5-25 Rand. 'Tariff error' is max(0,the respondent's guess about kiloliter price - 25). 'Increasing tariff' is 1 if the respondent understands that the tariff schedule is increasing. 'N. correct answers' is the number of correct answers in our quiz. 'Same respondent' is 1 the followup respondent's gender and age match the baseline respondent's gender and age, age+1, or age+2. Robust standard errors in parentheses. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

Table 13: Information sharing within the household II: HH size

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Response in kl	Bill hard to understand	Reads consumption from bill	Consumption accurate	Tariff in ballpark	Tariff error	Increasing tariff	N. correct answers
Treatment	0.063	0.010	0.001	0.000	-0.061*	-13.067	0.075	0.293*
	(0.040)	(0.034)	(0.090)	(0.058)	(0.033)	(40.686)	(0.071)	(0.165)
HH size	-0.004	0.015**	0.000	-0.017***	-0.003	3.970	0.002	0.060***
	(0.005)	(0.007)	(0.013)	(0.006)	(0.005)	(5.902)	(0.010)	(0.023)
HH size x Treatment	-0.006	-0.008	0.008	0.008	0.010	-1.971	-0.023	-0.054
	(0.007)	(0.008)	(0.019)	(0.011)	(0.007)	(6.783)	(0.015)	(0.036)
N	950	949	730	730	817	395	961	962
Treatment at mean HH size	0.038	-0.024	0.034	0.035	-0.016	-21.739	-0.025	0.055
	(0.020)	(0.016)	(0.036)	(0.023)	(0.015)	(17.531)	(0.030)	(0.065)

Notes: Each column lists the coefficient estimates from a different regression (apart from the constant). The column headings give the dependent variable. 'Response in kl' is 1 if the respondent's guess about their consumption is stated in kiloliters. 'Reads consumption from bill' is 1 if the respondent was able to find a water bill and reads out their consumption from the bill. 'Consumption accurate' is 1 if this number matches any consumption in the administrative data from the prior 6 months. 'Tariff in ballpark' is 1 if the respondent's guess about the kiloliter price is between 5-25 Rand. 'Tariff error' is max(0,the respondent's guess about kiloliter price - 25). 'Increasing tariff' is 1 if the respondent understands that the tariff schedule is increasing. 'N. correct answers' is the number of correct answers in our quiz. 'HH size' is the number of individuals in the household. Robust standard errors in parentheses. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

4.3 Heterogenous treatment effects

Another way to reconcile our findings with information effects is the possibility of heterogenous treatment effects across subgroups. For example, suppose that our education campaign raised knowledge among the less educated but not among the highly educated (who were more knowledgeable to begin with) and that the less educated made large payments as a result. Then as long as behavior was sufficiently responsive to information in this subgroup, the lack of an average treatment effect on information can be consistent with the large effects on nonpayment that we found.

To investigate this possibility, we focus on five dimensions of heterogeneity: restricted status at baseline, indigent status at baseline, water consumption before the treatment, the respondent's education, and household income. The first three of these variables were used in our stratified sampling procedure because they are natural candidates for determinants of households' ability or willingness to respond to our treatment. For example, restricted households or those consuming low amounts of water may not be able to adjust their consumption by much, and indigent households may find it more difficult to increase their payments. We add income and education because they are obvious dimensions of heterogeneity, especially for an information campaign. To maximize our sample size, we use income and education measures from the follow-up survey. We measure education by whether the respondent completed high school (the share of such respondents is 58% in the control and 57% in the treatment group). In the Appendix, we show that there are no significant differences in pre-treatment consumption or payment between the control and treatment group in any of these subgroups.

Table 14 studies the heterogeneity of treatment effects on payment and consumption. Each panel interacts our treatment indicator with one of the five variables mentioned above. In each case, a test of heterogenous treatment effects is equivalent to asking whether the interaction term is statistically significant. We find evidence of heterogenous treatment effects on nonpayment for two variables, education and income. In Panel D, more educated households significantly increased both their payment amount and their propensity to pay, while less educated households did not change their behavior in response to the treatment. Similarly, in Panel E, higher income households were more likely to pay and paid more. The increase in amount paid becomes statistically significant at a household income of 6300 Rand, which is just below the median of 6600 Rand. The increase in payment propensity becomes significant at 7800 Rand. This makes sense: our treatment increased payments among those who are more able to pay. For consumption, we confirm the heterogeneity by amount consumed found earlier (Panel C, column (4)). The effect of the treatment is significantly positive below 6 kl (log average consumption = 1.8) and negative for consumption exceeding

Table 14: Heterogenous treatment effects on payment and consumption

	(1) Payment amount	(2) Payment propensity	(3) Payment frequency	(4) Consumption
Panel A: Restricted				
Treatment	0.227	0.035	0.075	0.029
	(0.158)	(0.027)	(0.058)	(0.036)
Interaction	0.294	0.045	0.071	-0.066
	(0.308)	(0.053)	(0.101)	(0.070)
Restricted	-0.728***	-0.131***	-0.214***	0.001
	(0.226)	(0.039)	(0.076)	(0.048)
N	966	966	966	947
Panel B: Indigent				
Treatment	0.334**	0.043	0.078	0.016
	(0.159)	(0.027)	(0.057)	(0.037)
Interaction	-0.046	0.023	0.065	-0.021
	(0.309)	(0.055)	(0.106)	(0.067)
Indigent	-0.063	-0.028	-0.046	0.041
	(0.231)	(0.041)	(0.079)	(0.045)
N	966	966	966	947
Panel C: Pre Consumption				
Treatment	0.740	0.097	0.222	0.270*
	(0.514)	(0.090)	(0.159)	(0.138)
Interaction	-0.173	-0.020	-0.051	-0.103**
	(0.192)	(0.033)	(0.061)	(0.051)
Pre Consumption	0.426***	0.051**	0.121**	0.694***
	(0.146)	(0.024)	(0.048)	(0.031)
N	947	947	947	947
Panel D: Education				
Treatment	-0.010	0.001	0.050	0.002
	(0.199)	(0.035)	(0.073)	(0.046)
Interaction	0.586**	0.085*	0.092	0.013
	(0.274)	(0.048)	(0.097)	(0.062)
Education	-0.243	-0.039	0.037	0.030
	(0.202)	(0.035)	(0.073)	(0.041)
N	960	960	960	941
Panel E: Income				
Treatment	0.038	-0.007	0.056	0.043
	(0.210)	(0.037)	(0.073)	(0.048)
Interaction	0.032*	0.006*	0.005	-0.003
	(0.019)	(0.003)	(0.007)	(0.004)
Income	0.004	-0.000	0.008	0.009***
	(0.013)	(0.002)	(0.005)	(0.003)
N	857	857	857	844

Notes: Panels A-E investigate heterogenous treatment effects by different grouping variables. 'Restricted' is 1 if the consumer was restricted at baseline. 'Indigent' is 1 if the consumer was registered as indigent at baseline. 'Pre Consumption' is average consumption in the 3 months before the treatment (in logs). 'Education' is 1 if the follow-up respondent has completed high school and 0 otherwise. Income is total household income in 1000 Rand at follow-up. The columns in each panel correspond to separate regressions. The column headings give the dependent variable. 'Payment amount' is total payment in the 3 months following the treatment in logs; 'Payment propensity' is 1 if the household made a payment during this period, and 'Payment frequency' is the number of payments made. 'Consumption' is average consumption in the 3 months following the treatment (in logs). All regressions control for the value of the dependent variable during the 3 months prior to the treatment. Robust standard errors in parentheses. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

30 kl (log average consumption = 3.4).

Table 15 adds the same interactions, one at a time, to the information regressions. Did the groups driving the treatment effects for nonpayment experience an improvement in our information measures? The answer seems to be 'no.' In Panel E, we do not see any heterogeneity by income, while in Panel D, it is the *less* educated who show some evidence of increased knowledge. In column (1), the less educated are more likely to respond in kiloliters, and in column (4), they show an increased ability to tell their consumption from the bill. In addition, we find evidence that the treatment increased the ability of indigent households to tell their consumption from their bill (Panel B, columns (3) and (4)), and we also see some improvement in the information of low consumers (Panel C). However, as we saw in Table 14, these groups were not driving the payment results.

Overall, while our treatment shows some impact on the information of specific groups, this change in information is unlikely to explain the reduction in nonpayment.

4.4 Psychological effects

Section 2.2 describes various psychological effects that may be consistent with our findings of increased payments and no change in information. These include the increased salience of unpaid bills, social pressure and scrutiny, and reciprocity towards the provider. While our data does not permit us to identify all of these channels separately, we can test for one particular psychological channel: salience effects similar to those discussed by Zwane et al. (2011), Karlan et al. (2012) and others.

Consider the possibility that the education visit acted as a reminder for the household about any outstanding bills. Indeed, spending 1/2-1 hour talking to the education officers is likely to have made water consumption in general more salient, and increased payments could have been a response to this. This would have relevant policy implications, as it may imply that the involvement of utility employees may not be crucial, and simply sending reminders in the mail could have similar effects.

Our data allows us to address this because our *surveys* also increased the salience of water consumption and unpaid bills. Our surveys inquired at length about households' conservation and payment behavior, including whether they had ever missed a payment. We also asked respondents to find their water bill and read out their consumption. By contrast, our education officers were explicitly trained not to check households' bills or whether they had paid, and not to collect any kind of information during the visits. Thus, if the primary effect of the education visits was to increase salience, we expect to find a similar and possibly larger effect from our surveys.

Table 15: Heterogenous treatment effects on information

	(1) Response in kl	(2) Bill hard to understand	(3) Reads consumption from bill	(4) Consumption accurate	(5) Tariff in ballpark	(6) Tariff error	(7) Increasing tariff	(8) N. correct answers
Panel A: Restricted								
Treatment	0.031	-0.018	0.017	0.035	-0.020	-24.676	-0.033	0.079
	(0.024)	(0.019)	(0.043)	(0.028)	(0.015)	(22.039)	(0.036)	(0.077)
Interaction	0.025	-0.019	0.066	0.005	0.011	19.581	0.035	-0.075
	(0.043)	(0.033)	(0.079)	(0.052)	(0.037)	(24.942)	(0.064)	(0.142)
Restricted	-0.019	-0.002	-0.060	-0.003	0.032	-45.110**	0.018	-0.033
	(0.027)	(0.026)	(0.055)	(0.034)	(0.027)	(22.319)	(0.045)	(0.097)
N	953	952	731	731	820	396	964	965
Panel B: Indigent								
Treatment	0.042*	-0.038**	-0.018	0.008	-0.028	-2.831	0.007	0.070
	(0.023)	(0.018)	(0.043)	(0.026)	(0.018)	(8.385)	(0.035)	(0.076)
Interaction	-0.011	0.049	0.186**	0.099*	0.037	-79.015	-0.101	-0.047
	(0.043)	(0.035)	(0.079)	(0.056)	(0.030)	(67.470)	(0.065)	(0.143)
Indigent	0.005	-0.020	-0.099*	0.010	-0.029	77.600	0.045	0.082
	(0.029)	(0.025)	(0.054)	(0.035)	(0.022)	(66.399)	(0.045)	(0.097)
N	953	952	731	731	820	396	964	965
Panel C: Pre Consumption								
Treatment	0.105*	-0.140**	0.302**	0.061	0.012	29.809	-0.019	-0.136
	(0.059)	(0.056)	(0.126)	(0.067)	(0.047)	(24.112)	(0.102)	(0.211)
Interaction	-0.025	0.045**	-0.104**	-0.009	-0.011	-21.277*	-0.000	0.075
	(0.024)	(0.019)	(0.048)	(0.024)	(0.018)	(11.038)	(0.039)	(0.079)
Pre Consumption	0.045***	-0.053***	0.098***	-0.008	0.009	16.547*	-0.024	-0.057
	(0.017)	(0.015)	(0.035)	(0.016)	(0.015)	(9.518)	(0.030)	(0.059)
N	934	933	719	719	801	382	945	946
Panel D: Education								
Treatment	0.102***	-0.028	0.087	0.085**	-0.026	-54.035	-0.051	-0.021
	(0.033)	(0.027)	(0.056)	(0.037)	(0.021)	(37.991)	(0.043)	(0.097)
Interaction	-0.115***	0.006	-0.098	-0.085*	0.016	57.549	0.045	0.122
	(0.041)	(0.033)	(0.073)	(0.048)	(0.029)	(39.549)	(0.059)	(0.130)
Education	0.006	-0.038	-0.108**	0.019	0.000	-46.624	-0.089**	-0.047
	(0.026)	(0.025)	(0.052)	(0.031)	(0.023)	(38.102)	(0.041)	(0.088)
N	948	946	728	728	816	393	958	959
Panel E: Income								
Treatment	0.052	-0.021	0.037	0.083*	0.006	-23.625	-0.040	0.036
	(0.035)	(0.022)	(0.056)	(0.044)	(0.033)	(14.859)	(0.045)	(0.095)
Interaction	-0.001	0.001	-0.000	-0.005	-0.001	0.896	0.003	0.001
	(0.003)	(0.002)	(0.005)	(0.004)	(0.004)	(0.783)	(0.003)	(0.007)
Income	0.000	-0.001	0.004	0.001	0.006*	-0.970*	0.007***	0.014***
	(0.002)	(0.001)	(0.004)	(0.003)	(0.003)	(0.567)	(0.002)	(0.005)
N	846	843	657	657	735	351	855	856

Notes: Panels A-E investigate heterogenous treatment effects by different grouping variables. 'Restricted' is 1 if the consumer was restricted at baseline. 'Indigent' is 1 if the consumer was registered as indigent at baseline. 'Pre Consumption' is average consumption in the 3 months before the treatment (in logs). 'Education' is 1 if the follow-up respondent has completed high school and 0 otherwise. Income is total household income in 1000 Rand at follow-up. The columns in each panel correspond to separate regressions. The column headings give the dependent variable. 'Response in kl' is 1 if the respondent's guess about their consumption is stated in kiloliters. 'Reads consumption from bill' is 1 if the respondent was able to find a water bill and reads out their consumption from the bill. 'Consumption accurate' is 1 if this number matches any consumption in the administrative data from the prior 6 months. 'Tariff in ballpark' is 1 if the respondent's guess about the kiloliter price is between 5-25 Rand. 'Tariff error' is max(0,the respondent's guess about kiloliter price - 25). 'Increasing tariff' is 1 if the respondent understands that the tariff schedule is increasing. 'N. correct answers' is the number of correct answers in our quiz. Robust standard errors in parentheses. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

Table 16: Survey effects

Dep. var:	Jan - March total payment (1)	Jan - March payment (0/1) (2)	Jan - March payment frequency (3)	Jan - March avg. consumption (4)
Panel A				
Control	0.010	0.005	0.010	-0.002
	(0.131)	(0.022)	(0.047)	(0.030)
N	985	985	985	962
Panel B				
Treatment	0.259**	0.043*	0.098**	0.010
	(0.127)	(0.022)	(0.045)	(0.031)
N	988	988	988	970

Notes: Panel A estimates survey effects by comparing the control group in our study (Control = 1) to 500 randomly selected households who did not participate in our study. Panel B compares our treatment group to this "new control group." Each column corresponds to a different dependent variable, and every regression controls for sampling strata indicators and the pre-treatment value of the dependent variable. Robust standard errors in parentheses. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

Because we have access to administrative data for the entire population, we can directly test for such survey effects. We randomly select a "new control group" of 500 households who did not participate in our study in any way (using the same stratification procedure as for participating households). In Table 16, Panel A, we compare these households to our actual control group. Thus, the variable $Control$ takes the value of 1 if a household was surveyed (but not treated) in our study, and 0 if it did not participate. Because of random sampling, the coefficient on $Control$ consistently estimates the change in behavior caused by our two surveys only. We find no effect for either payment or consumption. By comparison, Panel B compares the new group and our treatment group. As expected, the results are numerically similar to those found earlier.

Being selected to participate in our study and being surveyed did not affect behavior; the education visits did. This makes it unlikely that the effect of the education visits operated primarily by increasing the salience of households' unpaid bills.

5 Conclusion

We implemented and evaluated an information campaign as a potential response to nonpayment for water in South African townships. Our education visits had a substantial impact, reducing the fraction of households making no payments by 4-5 percentage points and increasing the amount of payments by approximately 30% over a three-month period. We find no effect on average consumption, but treated households report an increase in conservation practices, and we find some evidence of a reduction in quantities consumed for the highest consumers.

Surprisingly, these effects do not appear to be due to increased knowledge. On average, treated households are no more likely to understand quantities of water used or their water bill than households in the control group. Although we see some increase in information among the less educated and the poor, the reduction in nonpayment is not driven by these changes. Instead, the findings suggest a psychological explanation, where households are "nudged" by perceived social pressure or to reciprocate the provider's efforts by paying more. Consistent with this, we show that involvement of the provider's employees - as opposed to the visit of our independent surveyors - was crucial to achieve the increase in payments. These findings show that public information campaigns may generate unintended consequences, including psychological responses, that can impact their effectiveness.

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A Appendix: The effect of information on consumption

Consider a consumer allocating his income y between two water-using activities, w_1 and w_2 (e.g., number of showers taken and number of car washes), and a third good x. Assume his utility is given by $u(w_1, w_2) + x$, where u is increasing in both arguments and concave. Normalize the units so that the water-using activities use w_1 and αw_2 kiloliters of water, respectively, where α is a constant. If the price of x is 1 and the price of each kiloliter of water is p (ignoring block pricing for simplicity), the consumer's problem is

$$\max_{w_1, w_2, x} u(w_1, w_2) + x \quad \text{s.t.} \quad y = x + p(w_1 + \alpha w_2),$$

and the first order conditions are $\frac{\partial u}{\partial w_1} = p$, $\frac{\partial u}{\partial w_2} = \alpha p$, and $x = y - p(w_1 + \alpha w_2)$.

Suppose that the consumer is misinformed about α, and thinks that it is smaller than it actually is. E.g., a consumer might think that washing the car once uses only as much water as taking a shower, or $\alpha = 1$, while in fact α is closer to 10. How would this consumer react if he learned the true value of α? We can answer this question by looking at the comparative statics of the above problem with respect to α. The total amount of water used by the consumer can be written as $w_1(\alpha) + \overline{\alpha} w_2(\alpha)$, where $\overline{\alpha}$ is the truth, and $w(\alpha)$ is the consumer's choice given his guess about α. It is straightforward to show that the change in total water consumption is proportional to

$$\frac{\partial [w_1(\alpha) + \overline{\alpha} w_2(\alpha)]}{\partial \alpha} \sim \overline{\alpha} u_{11} - u_{12}.$$

When the two water using activities are complements ($u_{12} > 0$), this is negative. Learning that an activity uses more water than the consumer thought leads to reduced water use. However, when the activities are sufficiently strong substitutes ($u_{12} << 0$), total water use can increase. The reason is that the consumer lowers w_2 but increases w_1. For example, he may switch to taking more showers and fewer baths, and total water use can rise.

B Appendix: Additional tables and figures

Table 17: Treatment effects on indigent status

	(1)	(2)	(3)	(4)
Treatment	0.013	0.002	0.002	0.004
	(0.030)	(0.010)	(0.010)	(0.010)
Strata indicators	No	Yes	Yes	Yes
Baseline dep. var.	No	No	Yes	Yes
Demographic controls	No	No	No	Yes

Notes: : Each cell presents the estimated treatment effect on indigent status from a different regression. The dependent variable is an indicator for indigent status in January (the month following the treatment), mean = 0.318. Columns (1-4) correspond to different specifications. 'Demographic controls' are the number of children, teenagers, adults in the household, number of employed members, education of respondent, household income, and whether the household has hot running water, owns a car, or owns a refrigerator. Robust standard errors in parentheses. N = 966. ***, **, * denote significance at 1, 5, and 10 percent, respectively.

Table 18: Average consumption across subgroups before the treatment

	Control	Treatment	Difference	p-value
Non-restricted	2.524	2.526	0.002	0.98
Restricted	2.557	2.537	-0.02	0.83
Non-indigent	2.541	2.539	-0.002	0.97
Indigent	2.516	2.506	-0.009	0.91
Low consumption	1.989	1.923	-0.066	0.14
High consumption	3.09	3.12	0.03	0.48
Low education	2.505	2.518	0.013	0.86
High education	2.557	2.534	-0.023	0.72
Low income	2.484	2.494	0.01	0.89
High income	2.579	2.611	0.032	0.66

Notes: The table presents log average consumption in the 3 months before the treatment in the subgroups used for the heterogenous treatment effects analysis. Reported values are the means in each group, the difference between control and treatment, and the p-value for a t-test of zero difference. For consumption and income, 'Low' and 'High' refer to below-median and above-median, respectively. For education, they refer to whether the respondent completed high school.

Table 19: Average payment across subgroups before the treatment

	Control	Treatment	Difference	p-value
Non-restricted	3.68	3.468	-0.212	0.35
Restricted	2.397	1.892	-0.505	0.14
Non-indigent	3.703	3.41	-0.293	0.2
Indigent	2.302	2.061	-0.241	0.45
Low consumption	2.705	2.392	-0.313	0.22
High consumption	3.976	3.609	-0.368	0.19
Low education	3.125	2.782	-0.342	0.25
High education	3.435	3.181	-0.254	0.32
Low income	3.274	2.771	-0.503	0.08
High income	3.5	3.387	-0.113	0.7

Notes: The table presents log average payment in the 3 months before the treatment in the subgroups used for the heterogenous treatment effects analysis. Reported values are the means in each group, the difference between control and treatment, and the p-value for a t-test of zero difference. For consumption and income, 'Low' and 'High' refer to below-median and above-median, respectively. For education, they refer to whether the respondent completed high school.

Figure 1: Sample area

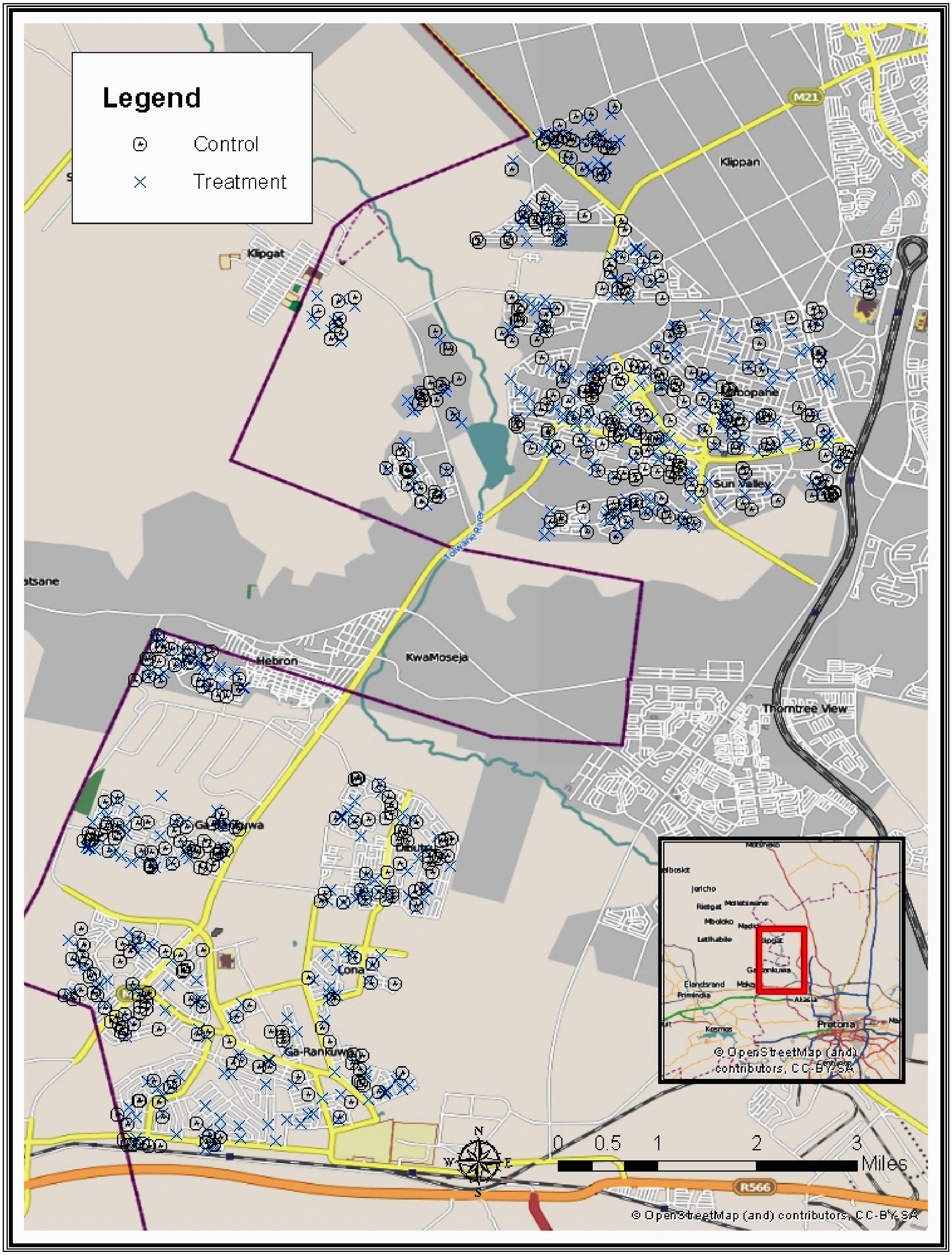

Figure 2: Distribution of payments (August 2012)