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Social Norms and Information Diffusion in Water-Saving Programs

Evidence from a Randomized Field Experiment in Colombia

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Social Norms and Information Diffusion in Water-Saving Programs: Evidence from a Randomized Field Experiment in Colombia

Mónica Marcela Jaime Torres and Fredrik Carlsson

Abstract

This paper investigates direct and spillover effects of a social information campaign aimed at encouraging residential water savings in Colombia. The campaign was organized as a randomized field experiment, consisting of monthly delivery of consumption reports, including normative messages, for one year. Results indicate that social information and appeals to norm-based behavior reduce water use by up to 6.8 percent in households directly targeted by the campaign. In addition, we find evidence of spillover effects: households that were not targeted by the campaign reduced water use by 5.8 percent in the first six months following the intervention. Nevertheless, neither direct nor spillover effects can be attributed to social networks for any of our chosen proxies of social and geographic proximity.

Key Words: peer effects, social norms, randomized evaluation, water utilities

JEL Codes: C93, D03, L95, O12

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1. Introduction

Recently, there has been a growing trend of employing social information, i.e., information about others' behavior, to influence individuals' own decisions. The basic idea is that individuals will conform to the behavior of others, for example, through social norms. As Lindbeck (1997) points out, both economic incentives and social norms give rise to purposeful or rational behavior: while economic incentives imply material rewards, social norms imply social rewards.[1] A series of randomized field experiments aiming at water and energy conservation suggests that the provision of both descriptive and injunctive messages can affect individuals' behavior by reducing water and electricity use (Bernedo et al. 2014; Allcott and Rogers 2014; Ito et al. 2014; Ferraro and Price 2013; Costa and Kahn 2013; Ayres et al. 2013; Smith and Visser 2013; Mizobuchi and Takeuchi 2013; Ferraro et al. 2011; Allcott 2011).[2] There is also evidence on the effects of non-pecuniary incentives on other pro-environmental behaviors (see, e.g.,

* Corresponding author: Mónica Marcela Jaime Torres, Department of Economics, University of Gothenburg, Sweden, marcela.jaime@economics.gu.se, School of Management and Business and Research Nucleus on Environmental and Natural Resource Economics (NENRE), University of Concepción, Chile, mjaime@udec.cl.

Fredrik Carlsson, University of Gothenburg, fredrik.carlsson@economics.gu.se

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[1] Once a norm is internalized in an individual's own value system, her behavior in accordance with or against the norm will also result in feelings of self-respect or guilt (Elster 1989; Young 2008).

[2] An overview of the main features of the experimental design and the main results of these information campaigns is presented in Table A1, Appendix A.

Chong et al. 2015; Gupta 2011). This suggests that behavioral policies could produce similar effects as classical price interventions (Allcott and Mullainathan 2010).[3]

In this paper, we investigate spillover effects of a social information campaign aimed at encouraging residential water savings in a Colombian town. Specifically, we are interested in evaluating whether households that were not targeted by the campaign, but potentially knew of its existence, also decreased water use. The campaign was organized as a randomized field experiment, and it was implemented in partnership with the local water utility. In this town, both the local government and the water utility, which is state-owned, consider it important to incentivize residential water savings.[4]

This paper extends previous research in three respects. First, despite the extensive evidence on the effects of norm-based messages on households' resource usage, existing literature has focused exclusively on direct effects. Following the literature on spillover effects in program evaluation (Fafchamps and Vicente 2013; Godlonton and Thornton 2013; Godlonton and Thornton 2012; Dickinson and Pattanayak 2011; Conley and Udry 2010; Duflo and Saez 2003), we propose a methodology that allows a separation of direct and spillover effects of the information campaign. We then investigate the role of social networks in information dissemination. In particular, we evaluate whether both direct and spillover effects are stronger for households that are socially connected with those directly targeted by the campaign. This is, therefore, the first attempt to evaluate both spillover effects and network effects in social campaigns aimed at promoting water/energy conservation.

Second, most of the studies have been conducted in developed countries; the only exception of which we are aware is Smith and Visser (2013) in South Africa. It is possible, perhaps even likely, that the effect of social information is context and institution specific. In particular, in a developing country, households will be relatively poor, and trust in institutions is

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3 In contrast, information without a social comparison is not likely to achieve much saving (Smith and Visser 2013; Ek and Söderholm 2010).

4 The water sector in Colombia is regulated by the Public Residential Services Law of 1994. According to this law, water policy, among other things, aims at protecting the poor through a cross-subsidies scheme in the form of area-based tariffs. Specifically, dwellings are classified into six socio-economic strata. Residential users belonging to the high-income class (strata 5-6), as well as industrial and commercial customers, pay a surcharge corresponding to 20% of their water and sewage bill. The money from the surcharge is then used to subsidize the basic consumption of users belonging to the lower-income class (strata 1-3). Subsidies are limited to covering up to 50%, 40% and 15% of the average service cost in strata 1, 2 and 3, respectively (Gomez-Lobo and Contreras 2003). Since its establishment in 1994, this policy has been used and refined by successive governments.

lower than in more developed countries (Knack and Keefer 1997). Furthermore, for political reasons, reform of water pricing is often difficult. Water is often subsidized in order to support poor households. However, in many cases, subsidy schemes affect all households, which could result in overconsumption.

Third, unlike previous studies, we also collect detailed household information through an *ex-ante* and *ex-post* survey. This enables us to investigate the heterogeneity of the treatment effects and shed some light on the underlying mechanisms. Understanding this heterogeneity is important not only for improving the cost-effectiveness of behavioral interventions, but also for policy design and decision making.

The rest of the paper is organized as follows. Section 2 presents the experimental design. The empirical strategy is presented in Section 3. In Section 4, the main results are discussed. Finally, Section 5 provides the main conclusions and policy recommendations.

2. Experimental Design

2.1 Context

The randomized field experiment took place in the town of Jericó, a small town situated in the southwestern region of Antioquia in Colombia. All households in the town receive water subsidies. Moreover, water-saving infrastructure is limited, individuals do not consider water scarcity a problem, and water usage in the town is very high (Cortés 2012).[5] However, both the local water utility EPJ (Empresas Públicas de Jericó) and the municipality of Jericó are concerned with encouraging households to save water.

According to EPJ, there are several reasons for this concern.[6] First, most residential water use is subsidized by the block pricing system. Second, the tariff reflects neither administration, maintenance and supply costs nor the value of investments to provide the service.[7] Third, water

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[5] Information provided by the water utility reveals that 50% of the households belonging to the lower income stratum exhibit overconsumption (i.e., their monthly water consumption exceeds 20 m^3). These figures are 38.3% and 39.5% for households in strata 2 and 3, respectively.

[6] The following reasons were cited in a personal interview with the EPJ manager, which took place in April 2013 in the EPJ headquarters in Jericó.

[7] At the time of the interview, an increase in the tariff was under discussion, but the public was unaware of this. The new tariff was adopted after this experiment was complete. It still will not cover the full cost of providing water.

discharge rates are very high and the corresponding cost of wastewater treatment is also high. Fourth, since EPJ is running a deficit, the municipality has to provide additional funds to the utility; consequently, the provision of other municipal services could be affected by the high water use. Finally, there are concerns that increased weather variability due to climate change could reduce water supply and, as a result, affect the energy supply, because the region relies heavily on hydropower.

2.2 Sampling and Household Data

According to the current EPJ records, there are 2,558 residential customers in Jericó. We include all active urban residential accounts whose meters fulfill the technical requirements,[8] which means that there are in total 1,857 households in our sample.

Before the implementation of the experiment, we conducted a survey in December 2012 to collect information at the household level. The survey included questions on socio-economic characteristics, water-saving facilities, behavioral actions toward water/energy conservation, personal values and perceptions regarding water conservation, social norms, and social networks.[9] The surveys were conducted via personal interviews in the respondents' homes. In total, 1,548 households were contacted and 1,311 households participated in the survey.[10] The response rate was thus nearly 85%.[11] We also conducted an *ex-post* survey in April 2014. It consisted of an extended version of the *ex-ante* survey, in which additional questions, aimed at identifying household networks and their characteristics, were introduced.

[8] The manager of EPJ informed us that some meters suffer from technical problems and will be replaced in the coming months. After analyzing their performance in the five months preceding the campaign, we defined all meters working perfectly for a period of at least three months as technically suitable. This criterion allows us to control for potential intentional manipulations by consumers.

[9] The survey implementation was carried out with the technical and logistical support of EPJ, Normal School of Jericó, and National University of Colombia, Campus Medellín.

[10] Although the households under study were previously identified, there were some difficulties in the field affecting the number of households to be interviewed. First, addresses were either repeated or non-existent in 232 cases. Second, 50 houses were uninhabited. Third, 19 residences are utilized for recreational purposes. Fourth, eight dwellings were either demolished, under construction or being remodeled.

[11] Non-responses are explained by two main reasons. First, individuals were on vacation at the time of the survey implementation. This accounts for 23.1% of non-responses. Second, individuals refused to answer the survey. This is the main reason for non-responses.

2.3 The Information Campaign

Interviewed households were randomly allocated to either a treatment group (also called the targeted group or the campaign subjects) or an untargeted group, with 656 households in the treatment group and 655 in the untargeted group. In the treatment group, households received personalized consumption reports, including a message appealing to both descriptive and injunctive norms. This report was received monthly with the water bill, for one year, starting in January 2013. The information contained in the reports was based on the billed water consumption of the corresponding month. The untargeted group received no reports or other messages, but members of that group were likely to know that some people in the community were receiving such information. An additional control group in a neighboring town was unlikely to know anything about the information campaign.

The experimental design closely follows the design of previous experiments (Ferraro and Price 2013; Costa and Kahn 2013; Ayres et al. 2013; Allcott 2011). The only difference is the definition of neighbors, which in our case are defined as "households with similar characteristics in terms of water needs." In order to capture households' similarities, we use information regarding household size and age distribution of its members so as to normalize household size into Adult Equivalent Units (AEU).[12] Based on this distribution, which ranges from 1 to 9.4, households were divided into three comparison groups: (1) Small ($1 \leq AEU < 2$), (2) medium ($2 \leq AEU < 5$), and (3) large ($AEU \geq 5$). Monthly water consumption in the reports is also expressed in AEU. This classification not only accounts for differences in household composition but also for economies of scale in water consumption within households (Haughton and Khandker 2009). This differs from previous studies, which compared houses with similar size and heating type.

Following Allcott (2011), the consumption reports had three components. The first is the *Social Comparison Component*, including descriptive and injunctive norms.[13] In the descriptive norm section, each household is compared to the mean and 25th percentile of its comparison group.[14] The injunctive norm section categorizes households as "Excellent," "Average" or

12 We use the following scale: $AEU = 1 + 0.7^{*}(N_{adults} - 1) + 0.5^{*}N_{children[6\text{-}18)} + 0.3^{*}N_{children(<6)}$

13 Cialdini (2003) suggests that the extent to which social information affects behavior depends not only on the information regarding what others do (i.e., descriptive messages) but also on whether approval of certain behavior is transmitted (i.e., injunctive messages).

14 In Allcott (2011), a household comparison group consisted of approximately 100 geographically-proximate households with similar characteristics, including square footage and heating type.

"Room to improve."[15] The second is the *Information Component*, in which households are given a detailed explanation of the environmental implications of being in a specific category. Furthermore, it provides information regarding the number of households joining the most efficient group in the current month. Finally, the third is the *Opting-out Component*, in which households are given the option to stop receiving consumption feedback. This one-treatment design is equivalent to the strict social norms treatment in Ferraro and Price (2013). Figure 1 provides an example of a consumption report, translated from Spanish.

2.4 Mechanisms of Effects

To conceptualize the channels through which the campaign operates, we assume a model in the spirit of both Levitt and List (2007) and Ferraro and Price (2013). Individuals experience moral utility from saving water because this contributes to ameliorating the negative external effects of overconsumption of water. This moral utility also depends on whether an individual behaves according to the notion of an acceptable level of water use in society (if such a notion exists), and on the extent to which an individual's actions are observed by others. We further assume that, even if an individual's own actions are unobserved, her utility will be affected by the knowledge that the actions of others have been observed, which raises the possibility that her own actions might be observed someday. We also assume that this effect on moral utility will be greater in so far as individuals are socially connected with those whose actions have been observed. This can be due to either environmental and status concerns (see, e.g., Schnellenbach 2012; Young 2008) or expectations regarding the observability of the individual's own actions in the future.

Because the provision of social information creates/reinforces the notion of an acceptable level of water use, households receiving consumption reports are more likely to experience moral payoffs, compared with those that do not receive such reports. Moreover, by receiving consumption reports, households realize their actions are being observed. Therefore, we would expect a reduction in average water use of households in the treatment group, compared with

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[15] Cialdini (2003) states that "Descriptive norms are relatively easy to accommodate because they are based in the raw behavior of individuals. In contrast, injunctive norms are based in an understanding of the moral rules of society; hence they required more cognitive assessment in order to operate successfully. As a result, one might expect that the impact of injunctive normative information would be mediated through cognitive assessments of the quality or persuasiveness of the normative information" (op. cit., page 4).

those in either the untargeted group (in the same town) or the additional control group (in a different town).

Similarly, by learning about the existence of the consumption reports, an individual who was not targeted by the campaign could become aware of the importance of saving water. Moreover, by knowing that the actions of others have been observed, an individual could also come to expect that her own actions may be observed in the future. Therefore, we would expect a reduction in average water use of households that, despite not being targeted by the campaign, find out about the consumption reports, compared with households that, because they are in another town, are not likely to find out that the campaign existed.

Finally, an individual socially linked to people whose actions are being observed by the campaign subjects is more likely to experience larger moral payoffs. This is because her current or future actions could be visible not only to the campaign subjects but also to individuals in her network. Not saving water could then result in a reduction in utility. Hence, we would expect a further reduction in average water use of households (either directly targeted by the campaign or not targeted but aware of the campaign) that are socially linked to treated households, compared to households that are not socially linked.

2.5 Identifying Spillover Effects

Due to network or other contextual effects, the impact of the intervention could thus go beyond the group of households that receive consumption reports. This complicates the evaluation of the information campaign, as treatment and control groups are no longer separated (Abbring and Heckman 2007). In an attempt to account for spillover effects of this campaign, we include a neighboring town, Támesis, with characteristics similar to Jericó, as an additional control. A random sample of 500 households was selected from the list of residential customers in this town.[16] These households also responded to the *ex-ante* and *ex-post* surveys, and the local water utility, EPT (Empresas Públicas de Támesis) provided us with monthly consumption data. Jericó and Támesis are not only geographically close but they also exhibit similar characteristics in terms of topography, demographics and economic activity that make them comparable (PDM 2008-2011b).[17] Water provision in both towns is administered by public utilities that share the

[16] Users were randomly selected, as all the meters were working perfectly according to the local water utility.

[17] At present, Támesis has 15,714 inhabitants, of whom 6,397 live in the urban area.

same principles, charge similar tariffs and serve about the same number of users. The spatial distribution of both the households participating in the campaign, and the treated and control towns, are presented in Figures A1-A2 in the appendix.

In the analysis, we distinguish between treated and control towns (i.e., Jericó and Támesis, respectively). Additionally, treated households in Jericó are regarded as *targeted* whilst control households in Jericó are regarded as *untargeted*. Households in Támesis are regarded as *control*. This approach facilitates the analysis of spillover effects of the campaign in two different ways. First, the introduction of a clean control enables us to assess the presence of spillover effects. This is done by comparing individuals who are potentially aware of the consumption reports (i.e., untargeted households) with individuals who will never realize its existence (i.e., control households). Second, we can investigate the role of social networks in information dissemination. By identifying targeted households that are socially linked with either targeted or untargeted households, we are able to disentangle *diffusion effects* (i.e., spillovers resulting from communication between targeted and untargeted individuals) from *reinforcement effects* (i.e., spillovers resulting from communication among targeted individuals) (Fafchamps and Vicente 2013). Because this analysis sheds light on the role of social networks in the dissemination of information, it is also informative for policy design.

2.6 Data and Baseline Characteristics

The water utilities gave us access to monthly consumption data from December 2011 to December 2013. Because consumption reports were sent between January 2013 and January 2014, we have a number of pre- and post-treatment observations.[18] Table 1 presents the average pre- and post-treatment water use for the targeted, untargeted and control households. A household's average consumption ranges between 12.7-14.4 m^3/month and, as expected, water consumption is higher in households with a larger number of adult equivalents. It should also be mentioned that water consumption varies over the year, but that the variation is similar across the different groups.

To begin with, we investigate the characteristics of the targeted, untargeted and control households in the pre-treatment period. Tables A2-A4, in the appendix, present the results of two procedures for testing the balance of both average water use and household characteristics in the

[18] Following Allcott (2011), any meter read more than 30 days after the first reports were delivered are considered post-treatment.

pre-treatment period. The first test consists of the standard difference in means. This is followed by the normalized differences suggested by Imbens and Wooldridge (2009), which are defined as the difference in averages by treatment status, scaled by the square root of the sum of the variances, as a scale-free measure of the difference in distributions.[19] As a rule of thumb, if normalized differences exceed 0.25, not only are the sample distributions different, but linear regression methods tend to be sensitive to the chosen specification. This approach is particularly important in this experiment because randomization took place at an individual rather than town level.

When comparing the targeted and untargeted households in Jericó, there is no evidence of statistically significant differences between the two groups. However, there are statistically significant differences between households in Jericó (both for targeted and untargeted households) and households in the control town. Specifically, the average water consumption of targeted and untargeted households differs from that of households in the control group. Although the differences are statistically significant, the normalized differences are small (0.13 and 0.10, respectively). Moreover, some characteristics regarding dwellings and water infrastructure in the house are also statistically significantly different among groups;[20] however, normalized differences exceed the threshold in only a few cases. Consequently, it will be important to take these differences into account in the econometric analysis.

2.7 Measures of Social Networks

Following Fafchamps and Vicente (2013), we assume that there are two channels that could explain information dissemination: *geographic proximity* and *social proximity*. Using information from the two surveys, we generate four measures of geographic proximity: (1) average distance to targeted households, (2) distance to the nearest targeted household, (3)

[19] Specifically: $\Delta_x = \frac{\overline{X_1} - \overline{X_0}}{\sqrt{S_0^2 + S_1^2}}$, where, for w=0,1, $S_w^2 = \sum_{i:W_i=w}(X_i - \overline{X_w})^2/(N_w - 1)$ is the sample variance of X_i, in the subsample with treatment $W_i=w$. According to Imbens and Wooldridge (2009), the reason for focusing on the normalized difference rather than on the t-statistic comes from their relationship to the sample size. For instance, while quadrupling the sample size leads, in expectation, to a doubling of the t-statistic, increasing the sample size does not systematically affect the normalized difference.

[20] Targeted households in Jericó seem to be wealthier than households in the control group in Támesis, as they inhabit their own houses, live in bigger houses and have water-saving equipment such as water storage tanks and water-saving watering machines. A similar pattern is observed when comparing untargeted households in Jericó with households in the control town.

number of targeted households within a radius of 10 to 50 m, and (4) distance to the main square. The first three measures are intended to capture the likelihood of discussing everyday issues with targeted neighbors, and the fourth captures the accessibility to the main focal point in the town.[21] These variables are summarized in the upper panel in Table 2.

Households are located, on average, within 10 m of the nearest household that was targeted by the campaign. The number of targeted neighbors located within a radius of 10 to 50 m ranges from 1.3 to 10.4 households. This implies that the likelihood of knowing a household that was targeted by the campaign is high. Moreover, households are located, on average, within 400 m of the main square, implying that they can easily access one of the main places where social interactions take place. It is worth mentioning that normalized differences do not exceed the threshold of 0.25 except in one case: the average distance to treated households.

Social proximity is proxied by the share of households that are members of the same churches (Godlonton and Thornton 2012), have children in the same schools, and participate in the same civic associations (e.g., board of neighbors, cash transfer programs, and environmental, youth and elderly associations). These variables are intended to capture the interactions of targeted households with other households that share common interests. One may assume that co-members not only talk to each other more frequently but also discuss personal matters. These variables are summarized in the lower panel in Table 2. The shares of church and school co-members are, on average, 31% and 4% of the households targeted by the campaign, respectively. However, the normalized difference corresponding to the share of church co-members also exceeds the threshold of 0.25. Moreover, participation in civic organizations is rather low, as, on average, households participate in less than one organization. To summarize, because the number of participants in the campaign is rather large, in this study we could not rely on measures of kinship and chatting, as in Fafchamps and Vicente (2013).

3. Empirical Strategy

The empirical strategy is based on reduced form specifications. The estimand of interest is the Average Treatment Effect (ATE) in the population of households participating in the experiment. The ATE is the expected effect of the treatment on a randomly drawn person from

[21] As in other Colombian towns, most social interactions take place in the main square. Because the cathedral and most restaurants, supermarkets and shops are located in its vicinity, this place is regarded by the inhabitants of Jericó as their main meeting point.

the population and is defined as α = E[$y_{it}^1 - y_{it}^0$], where y_{it}^1 and y_{it}^0 are the potential outcomes for household *i*'s water use at time *t* if the household was targeted or was not targeted by the campaign, respectively (Wooldridge 2010; Blundell and Costa 2009). Because households were given the possibility of opting out, the treatment group is defined as those sent the consumption reports or those actively opting out. Nevertheless, because only four households opted out, the treatment group can still be regarded as a random draw of the population. We are interested in three main effects: (1) homogeneous treatment effects assuming no spillovers, (2) homogeneous treatment effects accounting for spillovers, and (3) heterogeneous treatment effects due to social networks.

3.1 Homogeneous Treatment Effects Assuming No Spillovers

To begin with, we are interested in evaluating the direct effect of the campaign under the assumption of no spillovers. This gives us the change in water use of the average household that was targeted by the campaign when spillover effects are ruled out by assumption. The primary specification consists of the difference-in-differences estimator, in which water use is given by:

$$y_{it} = \alpha T_i P_{it} + \beta P_{it} + \mu_t + v_i + \varepsilon_{it}, \qquad (1)$$

where: y_{it} denotes household *i*'s water use in period *t*; T_i is a treatment status indicator that is equal to 1 if the household was targeted by the campaign, and 0 otherwise; P_{it} is a post-treatment indicator that is equal to 1 from February 2013 onward, and 0 otherwise; μ_t denotes month-by-year dummy variables; v_i are household fixed effects; and ε_{it} is the error term. Due to randomization, the direct effect of the campaign is consistently estimated by the parameter α. This equation is estimated by using a standard fixed effects estimator (OLS) and standard errors are clustered at the household level. Because spillovers are ruled out by assumption, this specification exclusively compares targeted and untargeted households.

3.2 Homogeneous Treatment Effects Accounting for Spillover Effects

Next, we focus on evaluating spillover effects of the campaign. The treatment effect can be decomposed into a direct and indirect effect. The direct effect stems from the treatment itself, whereas the indirect effect could be induced by factors unrelated to the campaign (Fafchamps and Vicente 2013). Because the sample of targeted and untargeted households does not allow us to account for such effects, we now need to use the households in the control town as well. Because households in these towns differ in terms of observable characteristics, we identify a "matched" control group in Támesis that is similar to the group of targeted/untargeted households in Jericó in terms of the core characteristics explaining water use. This control group is then utilized for estimating spillover effects by means of the difference-in-difference estimator in Equation (1). The identification strategy follows the procedure described by Imbens and Wooldridge (2009). In the first stage, using data from the *ex-ante* survey, we estimate propensity scores for each household using a probit model. After dropping the observations that fall outside the common support, households are matched on the basis of the propensity scores.[22] Equation (1) is then estimated on the matched sample by means of weighted regressions, in which control observations are weighted based on the number of times they were included as matches.

This procedure allows us to identify two different but important effects. First, by comparing untargeted households in Jericó with control households in Támesis, we estimate *spillover effects* of the campaign, i.e., we test whether households in Jericó that were not targeted by the campaign were indirectly treated, and therefore changed their water use. Second, by comparing targeted households in Jericó with control households in Támesis, we estimate the *total effect* of the campaign on the average targeted household. In the absence of spillovers, this effect should coincide with that in the previous section. Hence, the comparison of targeted households in Jericó with control households in Támesis can be used as a robustness check of the effects of the campaign.

3.3 Heterogeneous Effects Due to Social Networks: Reinforcement and Diffusion Effects

Finally, we are interested in evaluating the role of social networks in the dissemination of the information provided by the campaign.[23] If information is mainly disseminated through social networks, the ATEs will be stronger on households that are more closely linked to targeted households. Fafchamps and Vicente (2013) distinguish two types of effects: *reinforcement* and *diffusion* effects. The first occurs when targeted households are close to each other in a social or geographical sense, i.e., the treatment effect is strengthened because targeted households are socially connected. The second occurs when untargeted households are socially close to targeted

[22] We use a nearest neighbor 1-to-4 with replacement and a caliper of 0.01 as the matching method. While the nearest neighbor method imputes the missing potential outcome for each subject by using an average of the outcomes of similar subjects receiving the treatment, the caliper specifies the maximum distance at which two observations are a potential match (Abadie et al. 2004).

[23] Spillovers cannot necessarily be attributed to social networks. For instance, individuals visiting the water utility or the payment places could unintentionally find out about the reports.

households, i.e., information is disseminated from targeted to untargeted individuals. The specification to be estimated augments Equation (1) as follows:

$$y_{it} = \theta n_i T_i P_{it} + \alpha T_i P_{it} + \beta P_{it} + \mu_t + v_i + \varepsilon_{it}, \quad (2)$$

where n_i is the demeaned measure of social connectedness[24] (i.e., social or geographic proximity). The parameter of interest is θ, which measures the extent to which social networks affect household behavior, while the ATE is still captured by α.

4. Results

4.1 Homogeneous Treatment Effects Assuming No Spillovers

We begin by analyzing the direct effects of the campaign. Estimates corresponding to the primary specification given by Equation (1) are summarized in Table 3. Columns (1)-(4) evaluate the effect of the campaign for the whole group of households participating in the experiment, whereas Columns (5)-(8) restrict the analysis to the subsample of households whose meters worked perfectly during the study period. Because water consumption was normalized by dividing it by the average post-treatment control group consumption and multiplying by 100, estimated parameters capturing the ATEs can be interpreted as percentages of change (Allcott 2011).

The campaign has a positive and statistically significant effect on residential water savings. In particular, targeted households decreased their water use by 4.6%, compared with untargeted households in Jericó, during the first six months after the start of the experiment. After 11 months, the effect was 5.4%.[25] Our findings are consistent with those of Ferraro and Price (2013), who found a reduction in water use of about 4.8% in their strong social norm treatment.

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[24] The measures of social connectedness are demeaned as follows: $n_i = \tilde{n}_i - \frac{1}{N}\sum_{j=1}^{N} \tilde{n}_i$, where N is the total sample size and $\tilde{n}_i$ is a given measure of social/geographic proximity. By demeaning the covariates before forming interactions, we reduce the problem with multicollinearity (Wooldridge 2010). Also, by demeaning the variables, the parameters α and θ can be interpreted as the ATE and the differentiated effect of the ATE due to social networks, respectively. To facilitate the interpretation of the results, measures of distance are defined as the negative of the distance from household i to the place/household of interest.

[25] The ATEs are also robust to model specification: the effect of the campaign remains the same without controlling for seasonality.

Because the selection criterion for participating in the experiment included households whose meters worked in at least three out of the five months preceding the campaign, it is likely that some meters stopped functioning in a particular month. If this happens, the water utility charges the household the observed average consumption during the previous six months; in that case, changes in behavior of those households cannot be identified. Consequently, estimates including the entire group of participants can be interpreted as the lower bound of the ATE. Once the sample is restricted to households whose meters always worked (i.e., 73% of households), reduction in water use reaches 5.8% and 6.8%, 6 and 11 months after having been sent the first reports. Because information on the performance of meters comprises both unintended malfunction of meters (e.g., leakages, stopped and reversed meters) and intended malfunctions (e.g., covered meters that cannot be read), the assessment of the effects of the campaign will be more reliable when focusing on working meters. Therefore, the remaining analysis will be based on this subsample.

Figure 2 displays the monthly evolution of the ATE during the treatment period. There is an immediate effect of the treatment. Water use decreases by 8.9% in the first month following the experiment. From the second month onward, reductions in water use are, on average, 6.8%. Monthly ATEs are statistically significant at the 5% level in all periods.

Although the design of the campaign does not allow us to test the specific channels through which it operates, we can still investigate the extent to which households with different characteristics responded to the treatment. Following Ludwig et al. (2011), we identify a set of policy moderators (i.e., a set of characteristics that may influence the policy impact of the campaign). These characteristics are grouped into three categories: policy design, scope for water-savings, and *ex-ante* beliefs about one's own water use relative to neighbors. We divide our sample into two subsamples, using the 50^{th} percentile of the distribution of each covariate as the cut-off.[26] Equation (1) is then estimated for each subsample. Estimation results suggest a great deal of heterogeneity, as shown in Table 4.

The first panel in Table 4 summarizes household responses in the category *policy design*. The treatment effect is significant only for high users of water prior to the campaign and high-income households. These groups decrease water use by 10.3% and 10.1%, respectively.

[26] We follow this approach because our primary interest is to analyze the behavior of a group of individuals with similar characteristics over time. Moreover, although the randomization did not take place at the covariate level, the characteristics under study are balanced in their corresponding subsamples.

Moreover, ATEs are statistically significantly different, at the 1% level, from the reduction among low users and poor households, based on *t*-tests. This result is particularly important because both high users and high-income households put more pressure on the resource, and it also implies that non-pecuniary incentives can affect water use without hurting the poor. Findings are consistent with those in Allcott (2011), Ferraro and Price (2003), and Ferraro and Miranda (2013).

The second panel in Table 4 presents the heterogeneous responses in the category *scope for water-savings*. Households with older dwellings reduced their water use to a greater extent, compared with those in new dwellings. The reduction in water use in this group is 6.2%. This is statistically significantly different, at the 1% level, from the reduction among households in new dwellings. Although this may appear counter-intuitive, households in new dwellings could be less sensitive to the reports because their houses are already equipped with water-saving appliances, and their members may think that they are saving water already. The third panel in Table 4 displays the results for the category *ex-ante beliefs regarding the social norm*. As expected, households that prior to the campaign believed they were using less water than their neighbors (but, in fact, were using more) decreased water use to a greater extent than those who believed they were using more water. The ATE for households that initially believed they were using less water than their neighbors is 11.9%. The ATEs for households that initially believed they were using more water than their neighbors is 0.10%. Differences in treatment effects are also statistically significant, at the 1% level.

Overall, our findings suggest that, as in developed countries, behavioral interventions are suitable mechanisms to influence the behavior of households in developing countries. This is particularly important for the management of natural resources in developing countries, where price reforms are difficult to implement and trust in local institutions is low.

4.2 Homogeneous Treatment Effects Accounting for Spillover Effects

We now relax the assumption of no spillover effects of the information campaign. Consequently, the group of untargeted households is no longer a suitable control. We begin by analyzing the total effect of the campaign on the group of targeted households. Results are presented in Table 5. Columns (1)-(4) correspond to the primary specification in Equation (1) for the matched sample of households with working meters. By using the alternative control group in Támesis, we can identify a treatment effect only during the first six months following the start of the campaign. Although the effect is statistically significant only at the 10% level, its magnitude is fairly close to that of the direct effects in the previous section (6.1% vs. 5.8%). The effect is no longer statistically significant after eleven months.

The monthly ATEs are displayed in Figure 3. Once again, estimates reveal an immediate and significant response to the treatment during the first and second months. However, the effect disappears in the third month but is back again in the sixth month. This jump may suggest that an unexpected event affecting water use took place in this particular month. The manager of the water utility informed us that indeed a particular event took place in the town in April 2013.[27] This explains why the campaign did not generate an effect in this month. Even though our primary specification includes month-by-year dummies, both targeted and untargeted households were equally affected by the shock due to randomization, and, therefore, its overall effect on water use appears to cancel out when confining the analysis to the households in Jericó. Consequently, this particular month is removed from the analysis.[28] Estimates of the total effects excluding April 2013 are displayed in Columns (5)-(8) in Table 5.

After removing this month, we see that the average household participating in the experiment reduced water use by 13% and 6.3% the first six and eleven months after the start of the experiment. Although the ATE after eleven months is fairly close to that in the previous section of this paper (6.4% vs. 6.8%), the effects of the campaign after six months differ to a greater extent. Specifically, total effects in Table 5 are significantly larger than direct effects in Table 3. Figures reach 13% and 5.8%, respectively. This may indicate that the campaign had a larger impact in its early phase than what we find if we compare targeted and untargeted households. The bottom panel of Figure 3 displays the monthly ATEs, excluding April 2013. We can now observe that the effects are statistically significant, although decreasing over time.

We now focus on the spillover effects of the campaign. Table 6 presents the estimated effect of the campaign on the group of households in Jericó that did not receive consumption

[27] In March 2013, the Vatican announced the beatification of the first Colombian saint who, coincidentally, was originally from Jericó. The ceremony took place in Rome at the beginning of May 2013 and it was transmitted to the inhabitants of Jericó from the main square. During April 2013, hundreds of tourists visited the town and a large number of households rented out their rooms, as the touristic infrastructure in the county is quite limited. During this period, water use was significantly greater than that of the same month in the previous year, as shown in Figure A3 in the appendix.

[28] We also exclude April 2013 from the estimate of the homogeneous effects assuming no spillovers. The effect of the campaign after 6 and 11 months reaches 5.8% and 6.9%, respectively. Therefore, results are robust to the exclusion of this particular month.

reports. There is evidence that households not targeted by the campaign also decreased water use during the first six months after the start of the experiment. Specifically, the average untargeted household reduced water use by 5.8% compared to households in the control group in Támesis. This effect is statistically significant at the 0.05% level, and the magnitude and duration are not negligible. As far as we know, this is the first empirical evidence of spillover effects of social information campaigns aimed at water/energy conservation.

Monthly ATEs in Figure 4 indicate that, unlike targeted households, households not targeted by the campaign but subject to spillover effects take some time before responding to the treatment, which is to be expected. In particular, the first significant change is observed after two months dating from the start of the campaign; it reaches its maximum after three months and then starts to decrease. The effect vanishes from August 2013 onward.[29]

Our results suggest that untargeted households are affected by the campaign and change their behavior accordingly. There are two possible explanations. First, by becoming aware of the campaign, untargeted households also develop the notion of an acceptable level of water use; individuals became environmentally concerned and therefore reduce water use. Second, by knowing that other households have been treated, untargeted households updated their beliefs regarding the likelihood of being treated in the near future. Untargeted households subject to spillover effects respond by decreasing water use so as to hear good news if they receive consumption reports in the future. Although we are unable to identify the underlying mechanism giving rise to spillovers, the fact that the effect is short-lasting points toward the second explanation. Because untargeted households did not receive consumption reports in the subsequent periods, it is most likely that they revised their beliefs once again, stopping their efforts to decrease water use.

Findings also corroborate the idea that the early effect of the campaign on the average targeted household was larger than we initially thought. For the sample of households participating in the experiment, the ATE after six months can be calculated by adding the homogeneous treatment effect in the absence of spillovers and with spillover effects. By doing

[29] Results are also robust to the inclusion of April 2013. Although the estimates corresponding to the first six months following the campaign do not present statistically significant differences between untargeted households in Jericó and control households in Támesis, the monthly ATEs reveal that untargeted households decreased their water use by 9% in the second month following the campaign. This effect is captured when using the samples of all households and households with working meters.

so, the ATE reaches 11.6%, which is very close to the homogeneous treatment effect when accounting for spillovers (i.e., 11.6% vs. 13%).

To summarize, the analysis of spillovers in water use has a policy implication: as with other types of interventions (Fafchamps and Vicente 2013; Godlonton and Thornton 2012; Conley and Udry 2010), it is possible to influence the behavior of an entire population by targeting only a share of households. However, the spillover effect lasts only a few months. Thus, targeting a group of the population would be appropriate if the objective of the intervention was to reduce water use during short periods (e.g., droughts). However, if the objective is to promote permanent behavioral changes in the entire population, policy makers should target all households.

4.3 Heterogeneous Effects Due to Social Networks: Reinforcement and Diffusion Effects

As a final point, we are interested in evaluating whether the direct and spillover effects of the campaign can be ascribed to social networks. Table 7 presents the reinforcement effects following the specification in Equation (2). The regression models include measures of both geographic and social proximity as proxies of social connectedness.

We begin by evaluating the role of geographic proximity. As can be seen in the left panel of Table 7, there is weak evidence of reinforcement effects. Most variables are statistically insignificant, but the share of treated households within a radius of 10 meters appears to have a negative effect on water savings. Although this finding is in line with that of Godlonton and Thornton (2013), it is significant only at the 10% level; hence, it has to be interpreted with caution. Results evaluating the role of social proximity are displayed in the right panel of Table 7. None of our proxies of social proximity are statistically significant, confirming the notion of the absence of reinforcement effects. Overall, results suggest that the effect of the campaign was mainly driven by receiving the consumption reports and, to a lesser extent, by external factors not directly linked with the campaign. This result is not surprising because the campaign was not as visible as, for example, the one in Fafchamps and Vicente (2013).

We conclude our analysis with the evaluation of diffusion effects, which are presented in Table 8. Because spillover effects are observed only within the first six months of the experiment, regressions are restricted to the period December 2011 - July 2013. Surprisingly, results indicate that untargeted households socially connected with targeted households did not change their behavior to a greater extent than those not socially connected. Thus, we find no support for a finding that social networks play an important role in disseminating the information provided by the campaign. This implies that finding out that the reports exist, i.e., knowing that other households were targeted by the campaign, was sufficient to influence the behavior of the untargeted households. Note, however, that the estimated ATEs are increased after controlling for social networks.

Because the information provided by the campaign was not public, we expected that social networks could play a major role in explaining information dissemination from targeted to untargeted households. One possible explanation for the unexpected finding is that, as previously mentioned, spillovers are not necessarily attributed to social networks. For instance, untargeted individuals visiting the water utility or the payment places could find out about the reports from individuals who were targeted by the campaign. Another explanation is that social networks still play an important role through channels other than those we have explored so far. Thus, further investigation regarding the effects of alternative measures of social connectedness is still needed.

5. Conclusions

In this paper, we have analyzed the direct and spillover effects of an information campaign aimed at encouraging residential water savings in a small Colombian town. The campaign was organized as a randomized field experiment, consisting of monthly delivery of consumption reports including normative messages during one year. We first evaluated both the direct and spillover effects of the campaign. This was followed by an investigation into the effects of social networks on information dissemination among households in the town. This allowed us to disentangle reinforcement and diffusion effects.

Results show that social information and appeals to norm-based behavior reduced water use. Specifically, homogeneous treatment effects assuming no spillovers reveal that targeted households decreased water use by 6.8% during the eleven months following the start of the campaign. This finding is not only consistent with the notion that moral payoffs can influence consumption decisions but also demonstrates the potential of non-pecuniary incentives as a mechanism to influence water use in a setting of a developing country. In addition, the heterogeneous treatment effects show that wealthier households and high users of water decreased water use to a greater extent than did poorer households and low users of water. This finding is highly policy relevant because, while the rationale for subsidies is to benefit the poor, wealthier groups are those putting more pressure on the resource.

Results corresponding to the total effects of the campaign accounting for spillovers are also highly policy relevant. The estimated ATEs six months after the start of the campaign are significantly larger than those resulting when assuming no spillovers. This suggests that the campaign may have a larger impact in its early phase. Households not targeted by the campaign reduced water use by 5.8% in the first six months following the experiment. To the best of our knowledge, this finding is the first piece of evidence of the presence of spillover effects in social information campaigns aimed at water/energy conservation, which is a major contribution to the growing literature in this field. This suggests that non-pecuniary incentives can be suitable and inexpensive instruments for shaping the behavior of an entire population in short-run interventions.

However, we find no evidence of either reinforcement or diffusion effects. There are two possible explanations. On the one hand, spillovers may not necessarily be attributable to social networks. Thus, finding out about the reports rather than being socially connected is sufficient to influence the behavior of households. On the other hand, social networks still could play an important role through channels other than those we have explored so far. Unlike Fafchamps and Vicente (2013), we did not identify kinship and chatting as channels for spillover effects, possibly because we used a rather large sample. Notwithstanding these concerns, and given the magnitude and significance of the spillover effects, further investigation regarding the effects of alternative measures of social connectedness is still needed.

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Tables and Figures

Table 1. Water consumption by comparison groups (m^3/month)

Adult equivalent units (AEU)	*Pre-treatment*			*Post-treatment*		
	Targeted	Untargeted	Control	Targeted	Untargeted	Control
Small households (1 ≤ AEU < 2)	11.75	8.94	10.28	11.07	9.42	9.37
	(9.28)	(6.49)	(9.08)	(8.42)	(6.82)	(7.98)
Medium households (2 ≤ AEU < 5)	14.50	14.87	12.84	13.98	14.85	12.01
	(7.99)	(8.96)	(8.92)	(8.11)	(8.88)	(7.97)
Large households (AEU ≥ 5)	20.72	20.36	15.74	18.64	20.89	17.92
	(12.75)	(10.23)	(11.98)	(10.42)	(12.83)	(13.76)
All households	14.36	14.00	12.66	13.72	14.13	12.01
	(8.77)	(9.09)	(9.22)	(8.47)	(9.30)	(8.62)
No. Obs.	656	655	500	656	655	500

Source: Own elaboration based on both EPJ and EPT records, and *ex-ante* data. Pre-treatment corresponds to the period Dec. 2011 – Jan. 2013. Similarly, post-treatment corresponds to the period Feb. 2013 – Dec. 2013. Standard deviations in parentheses.

Table 2. Measures of social networks

Variable	*Targeted*		*Untargeted*		*Control*
	Mean/std. Dev.	Norm. Diff.	Mean/std. Dev.	Norm. Diff.	Mean/std. Dev.
			Geographic proximity		
No. Treated (*r*=10m)	1.29	-0.121	1.35	-0.090	1.52
	(1.28)		(1.26)		(1.43)
No. Treated (*r*=20m)	2.91	-0.078	2.97	-0.057	3.16
	(2.14)		(2.15)		(2.36)
No. Treated (*r*=30m)	4.89	-0.030	4.98	-0.010	5.02
	(3.03)		(3.02)		(3.16)
No. Treated (*r*=40m)	7.39	-0.019	7.56	0.010	7.50
	(4.28)		(4.22)		(4.18)
No. Treated (*r*=50m)	10.29	0.000	10.57	0.035	10.30
	(5.59)		(5.63)		(5.31)
Average distance to targeted [*meters*]	512.2	0.465*	508.3	0.438*	407.5
	(178.5)		(186.9)		(89.0)
Distance to nearest targeted [*meters*]	12.50	0.141	13.39	0.128	8.50
	(26.80)		(36.96)		(8.25)
Distance to main square [*meters*]	386.2	-0.169	374.1	-0.199	443.1
	(245.4)		(253.3)		(225.8)
			Social proximity		
Share co-members (church)	0.309	-0.581*	0.321	-0.563*	0.558
	(0.218)		(0.217)		(0.272)
Share co-members (school)	0.036	-0.099	0.040	-0.056	0.044
	(0.051)		(0.053)		(0.056)
No. Associations	0.502	0.043	0.496	0.037	0.456
	(0.748)		(0.776)		(0.746)

Note: Normalized differences are calculated with respect to the control. Standard deviations in parentheses. *Normalized differences exceed the threshold suggested by Wooldridge and Imbens (2009).

Table 3. Homogeneous treatment effects (Targeted vs. Untargeted)

	All households				*Working meters*			
VARIABLES	After 6 months		After 11 months		After 6 months		After 11 months	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Treated	-4.605**	-4.605**	-5.447***	-5.447***	-5.844***	-5.844***	-6.822***	-6.822***
	(1.997)	(1.998)	(1.944)	(1.945)	(2.015)	(2.016)	(1.999)	(2.000)
Post-treatment	-0.221	-13.39***	0.898	-15.89***	-1.821	-14.39***	-0.550	-14.22***
	(1.505)	(1.977)	(1.471)	(1.985)	(1.421)	(2.097)	(1.418)	(1.831)
Constant	101.5***	115.9***	100.4***	114.6***	106.6***	120.4***	105.4***	119.1***
	(0.300)	(1.215)	(0.428)	(1.208)	(0.302)	(1.333)	(0.440)	(1.329)
Month-by-year	No	Yes	No	Yes	No	Yes	No	Yes
No. Obs.	26,220	26,220	32,775	32,775	19,120	19,120	23,900	23,900
No. Households	1,311	1,311	1,311	1,311	956	956	956	956
R-squared	0.002	0.029	0.002	0.024	0.005	0.045	0.005	0.038

Note: This table shows estimates of the baseline specification capturing the homogeneous treatment effects of the campaign. Estimates correspond to the period Dec. 2011 – Dec. 2013. Columns (1)-(4) correspond to the standard diff-in-diff estimator including all households. Columns (5)-(8) correspond to the diff-in-diff estimator for the sample of working meters. The dependent variable is monthly water use (% change w.r.t. control group). Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Heterogeneous treatment effects (Targeted vs. Untargeted)

VARIABLES	*Policy Design*				*Scope for water-savings*				*Ex-ante beliefs*		
	Low-users	High-users	Low-inc.	High-inc.	New-dwell	Old-dwell	Owned	Non-own	Below	Average	Above
Post*Treated	-3.252	-10.27***	-3.826	-10.13***	-5.740	-6.717**	-4.247*	-10.43***	-11.89***	-6.086***	-0.103
	(1.991)	(3.182)	(2.969)	(2.837)	(3.842)	(2.684)	(2.372)	(3.507)	(4.470)	(2.298)	(10.06)
Post-treatment	-6.733***	-20.38***	-10.09***	-14.12***	-18.74***	-15.99***	-17.75***	-9.064***	-6.177	-15.90***	-19.06***
	(2.058)	(2.833)	(3.348)	(3.052)	(3.520)	(2.608)	(2.400)	(2.921)	(4.628)	(2.218)	(6.339)
Constant	68.63***	161.7***	117.7***	118.0***	111.4***	123.1***	121.3***	115.7***	106.9***	121.9***	137.0***
	(1.556)	(2.053)	(2.057)	(1.726)	(2.372)	(1.978)	(1.731)	(2.117)	(2.433)	(1.684)	(5.105)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	10,950	12,950	11,800	11,175	6,331	10,825	13,600	9,969	5,750	16,400	1,250
No. Households	438	518	472	447	254	434	544	399	230	656	50
R-squared	0.042	0.055	0.034	0.045	0.035	0.049	0.046	0.033	0.046	0.038	0.063

Note: This table shows estimates of the baseline specification capturing the heterogeneous effects of the campaign. Estimates correspond to the period December 2011– Dec. 2013 and include targeted and untargeted households whose meters always worked. The dependent variable is monthly water use (% change w.r.t. untargeted group). Clustered standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5. Homogeneous treatment effects (Targeted vs. Control)

	All periods				*Excluding April 2013*			
VARIABLES	After 6 months		After 11 months		After 6 months		After 11 months	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Treated	-6.071*	-6.071*	-3.111	-3.111	-13.04***	-13.04***	-6.351*	-6.351*
	(3.455)	(3.456)	(3.735)	(3.737)	(3.333)	(3.335)	(3.697)	(3.699)
Post-treatment	-2.060	0.118	-4.586	-12.84***	1.234	1.523	-3.157	-4.240
	(3.037)	(3.437)	(3.344)	(3.618)	(2.906)	(2.366)	(3.299)	(3.641)
Constant	109.1***	108.6***	111.1***	110.6***	109.1***	108.6***	111.1***	110.6***
	(0.563)	(2.143)	(0.899)	(2.122)	(0.475)	(2.177)	(0.841)	(2.138)
Month-by-year	No	Yes	No	Yes	No	Yes	No	Yes
No. Obs.	17,440	17,440	21,800	21,800	16,568	16,568	20,928	20,928
No. Households	872	872	872	872	872	872	872	872
R-squared	0.003	0.030	0.004	0.032	0.005	0.034	0.004	0.034

Note: This table shows estimates of the baseline specification capturing the homogeneous treatment effects of the campaign for the group of matched households with working meters. Columns (1)-(4) include all periods, whereas Columns (5)-(8) exclude April 2013. Estimates correspond to the period Dec. 2011 – Dec. 2013. The dependent variable is monthly water use (% change w.r.t. control group). Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6. Homogeneous treatment effects (Untargeted vs. Control)

VARIABLES	After 6 months		After 11 months	
	(1)	(2)	(3)	(4)
Post*Treated	-5.836**	-5.836**	2.451	2.451
	(2.937)	(2.938)	(3.480)	(3.482)
Post-treatment	-0.290	-1.419	-5.088*	-4.948
	(2.416)	(3.098)	(3.024)	(3.258)
Constant	104.2***	103.9***	106.1***	105.8***
	(0.413)	(2.307)	(0.793)	(2.183)
Month-by-year	No	Yes	No	Yes
No. Obs.	16,473	16,473	20,808	20,808
No. Households	867	867	867	867
R-squared	0.001	0.027	0.002	0.027

Note: This table shows estimates of the baseline specification capturing the homogeneous treatment effects of the campaign for the matched sample of households with working meters. Estimates correspond to the period Dec. 2011 – Dec. 2013. April 2013 is excluded from all regressions. The dependent variable is monthly water use (% change w.r.t. control group). Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Reinforcement effects

VARIABLES	*Geographic proximity* Average distance	Distance to nearest	Distance square	Treated (10m)	Treated (20m)	Treated (30m)	Treated (40m)	Treated (50m)	*Social proximity* Church co-memb	School co-memb	No.org
Post*Treated	-9.280**	-6.336*	-6.363*	-6.452*	-6.384*	-6.354*	-6.399*	-6.398*	-7.074*	-6.131*	-6.588
	(4.149)	(3.772)	(3.633)	(3.707)	(3.677)	(3.733)	(3.723)	(3.740)	(3.982)	(3.669)	(4.033)
Post*Treated*n	0.0170	-0.0931	-0.0150	4.074*	2.296	0.598	0.230	0.0864	15.18	58.27	1.010
	(0.0302)	(0.350)	(0.0131)	(2.410)	(1.451)	(1.012)	(0.714)	(0.605)	(13.27)	(62.94)	(8.324)
Post-treatment*n	-0.0367	0.0435	0.00442	-2.773	-1.523	-0.258	-0.336	-0.275	-11.31	-33.63	1.584
	(0.0294)	(0.348)	(0.0119)	(2.017)	(1.240)	(0.847)	(0.609)	(0.531)	(10.97)	(54.35)	(8.048)
Post-treatment	0.548	-4.332	-2.429	-1.432	-4.081	-14.57***	-14.56***	-14.53***	-3.109	-4.401	-4.048
	(4.380)	(3.631)	(3.494)	(4.175)	(3.635)	(3.626)	(3.614)	(3.634)	(3.763)	(3.652)	(3.967)
Constant	110.6***	110.6***	110.6***	110.6***	110.6***	110.6***	110.6***	110.6***	110.6***	110.6***	110.6***
	(2.144)	(2.137)	(2.139)	(2.145)	(2.144)	(2.137)	(2.136)	(2.135)	(2.136)	(2.133)	(2.136)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	20,928	20,928	20,928	20,928	20,928	20,928	20,928	20,928	20,928	20,928	20,928
No. Households	872	872	872	872	872	872	872	872	872	872	872
R-squared	0.035	0.034	0.034	0.035	0.034	0.034	0.034	0.034	0.034	0.034	0.034

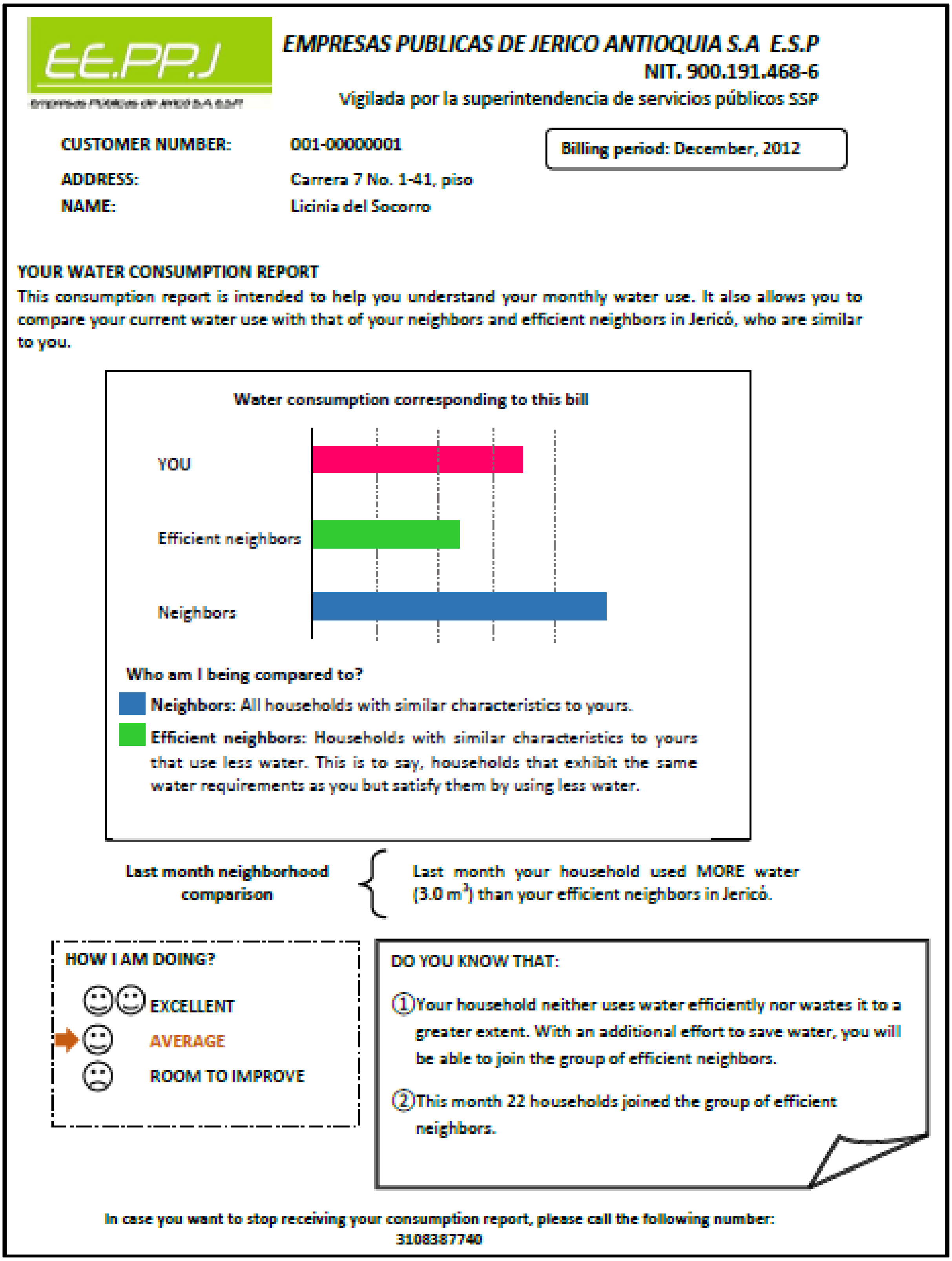
Note: This table shows estimates of the baseline specification capturing the heterogeneous treatment effects of the campaign for the matched sample of households with working meters. Estimates correspond to the period Dec. 2011 – Dec. 2013. April 2013 is excluded from all regressions. The dependent variable is monthly water use (% change w.r.t. control group). Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8. Diffusion effects

				Geographic proximity						*Social proximity*	
VARIABLES	Average distance	Distance to nearest	Distance square	Treated (10m)	Treated (20m)	Treated (30m)	Treated (40m)	Treated (50m)	Church co-memb	School co-memb	No.org
Post*Treated	-8.073**	-5.997*	-6.664**	-6.210**	-6.013**	-5.878**	-5.877**	-5.972**	-6.337**	-5.926**	-5.315*
	(3.534)	(3.137)	(2.891)	(2.948)	(2.941)	(2.951)	(2.951)	(2.989)	(3.206)	(2.931)	(2.969)
Post*Treated*n	0.0397	0.0551	-0.00737	1.730	0.833	0.0844	0.232	0.368	3.776	-1.423	0.265
	(0.0291)	(0.323)	(0.0121)	(1.980)	(1.294)	(0.930)	(0.669)	(0.545)	(11.66)	(48.70)	(4.614)
Post-treatment*n	-0.0363	-0.0549	0.0113	-2.371	-1.246	-0.176	-0.210	-0.204	-4.717	27.24	-3.702
	(0.0283)	(0.323)	(0.0106)	(1.631)	(1.113)	(0.770)	(0.569)	(0.475)	(8.676)	(40.76)	(4.229)
Post-treatment	0.942	0.0604	-0.770	-1.089	0.0392	-1.382	-1.379	0.00461	-1.001	-1.271	-1.868
	(3.468)	(2.159)	(3.206)	(3.112)	(2.117)	(3.092)	(3.100)	(2.112)	(3.279)	(3.124)	(3.080)
Constant	103.9***	103.9***	103.9***	103.9***	103.9***	103.9***	103.9***	103.9***	103.9***	103.9***	103.9***
	(2.309)	(2.307)	(2.310)	(2.308)	(2.307)	(2.307)	(2.307)	(2.306)	(2.305)	(2.308)	(2.307)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	16,473	16,473	16,473	16,473	16,473	16,473	16,473	16,473	16,473	16,473	16,473
No. Households	867	867	867	867	867	867	867	867	867	867	867
R-squared	0.028	0.027	0.027	0.028	0.028	0.027	0.027	0.027	0.027	0.027	0.028

Note: This table shows estimates of the baseline specification capturing the heterogeneous treatment effects of the campaign for the matched sample of households with working meters. Estimates correspond to the period Dec. 2011 – Jul. 2013. April 2013 is excluded from all regressions. The dependent variable is monthly water use (% change w.r.t. control group). Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 1. Example of consumption report



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EMPRESAS PUBLICAS DE JERICO ANTIOQUIA S.A E.S.P
NIT. 900.191.468-6
Vigilada por la superintendencia de servicios públicos SSP

CUSTOMER NUMBER: 001-00000001
ADDRESS: Carrera 7 No. 1-41, piso
NAME: Licinia del Socorro

Billing period: December, 2012

YOUR WATER CONSUMPTION REPORT

This consumption report is intended to help you understand your monthly water use. It also allows you to compare your current water use with that of your neighbors and efficient neighbors in Jericó, who are similar to you.

Water consumption corresponding to this bill

YOU
Efficient neighbors
Neighbors

Who am I being compared to?

Neighbors: All households with similar characteristics to yours.

Efficient neighbors: Households with similar characteristics to yours that use less water. This is to say, households that exhibit the same water requirements as you but satisfy them by using less water.

Last month neighborhood comparison { Last month your household used MORE water (3.0 m^3) than your efficient neighbors in Jericó.

HOW I AM DOING?

EXCELLENT
AVERAGE
ROOM TO IMPROVE

DO YOU KNOW THAT:

① Your household neither uses water efficiently nor wastes it to a greater extent. With an additional effort to save water, you will be able to join the group of efficient neighbors.

② This month 22 households joined the group of efficient neighbors.

In case you want to stop receiving your consumption report, please call the following number: 3108387740

Figure 2. Monthly direct effects (Targeted vs. Untargeted)

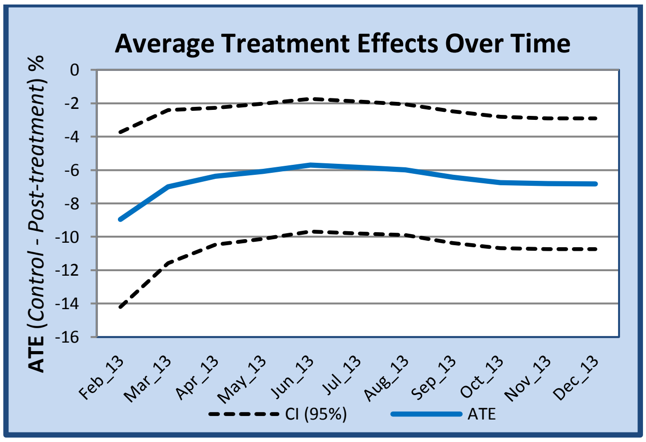


Figure 3. Monthly combined effects including (left) and excluding (right) April 2013 (Targeted vs. Control)

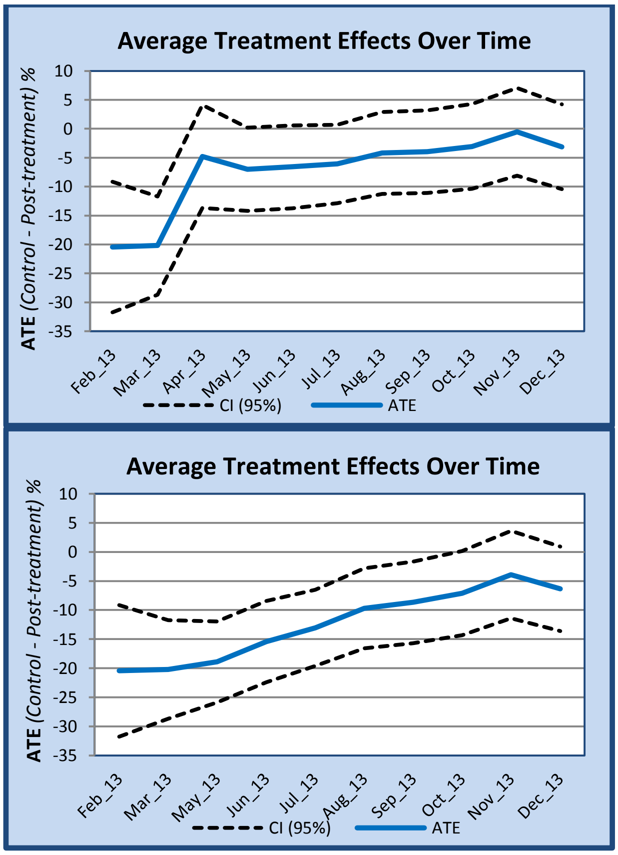
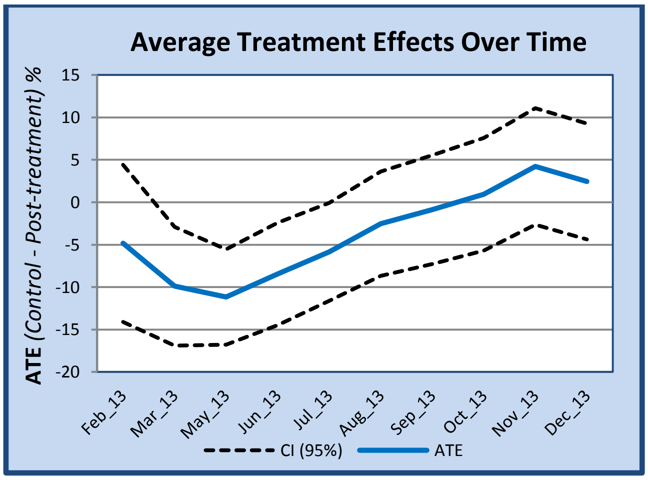


Figure 4. Monthly spillover effects (Untargeted vs. Control - Excluding April 2013)



Appendix.

Table A1. Overview of social information campaigns aimed at water/energy conservation

Author	Type	Objective	Experimental design	Main results
Allcott (2011)	Electricity	- To conduct an impact evaluation of the most notable non-price energy conservation program in the U.S.	- Treatment consisted of the delivery of energy report letters, including conservation tips and descriptive and injunctive social norms. - About 600,000 households across the US were divided into one of 17 experimental populations. - Reports were first sent during Spring 2008, for an indefinite period.	- The average household receiving the "Home Energy Report" decreases electricity use by about 3%. - Average Treatment Effects (ATEs) are equivalent to an 11-20% short-run price increase or a 5% long-run price increase. - After 2 years of treatment, there is no evidence of a decline in ATEs.
Ferraro et al. (2011)	Water	- To analyze how different norm-based strategies influence long-run patterns of residential water use in Metropolitan Atlanta (Georgia).	- Follow-up of the households that participated in Ferraro and Price (2013) experiment, 2 years after its implementation. - Analysis of post-treatment residential water demand, over the period 2007-2009.	- Within a year, there are no significant differences in water use between the weak social norm treatment and the control group. In contrast, there is still a treatment effect after more than two years in the strong social norms treatment group. - Evidence of waning: $ATE_{2008} > ATE_{2009}$.
Mizobuchi and Takeuchi (2013)	Electricity	- To examine the influence of economic and psychological factors on electricity conservation behavior in Matsuyama (Japan).	- 236 households in 2 treatments: monetary rewards and monetary rewards with comparative feedback. Households were aware of their participation in the experiment. - Duration: 8 weeks.	- The ATE for households in the reward treatment is 5.9%. In contrast, households that were provided with both economic rewards and comparative feedback decreased electricity use by 8.2%.

Table A1. Overview of social information campaigns aimed at water/energy conservation (Continued)

Ferraro and Price (2013)	Water	- To examine the effect of norm-based messages on residential water demand in Metropolitan Atlanta (Georgia).	- 3 treatments: technical advice, weak social norm and strict social norm. - One-shot experiment: letters were sent out on week 21 (2007). - 100,000 households participated in the experiment.	- The ATEs for the technical advice, weak social norm and strict social norm treatments are 1%, 2.8% and 4.8%, respectively. The latter corresponds to a price increase of 12-15%. - The effectiveness of the normative messages started to wane after the fourth month.
Costa and Kahn (2013)	Electricity	- To evaluate the role of political ideology on the effectiveness of energy conservation nudges.	- Treatment: delivery of "home energy reports", including neighbor comparisons and energy-savings tips. - Ideology is measured through affiliation with political parties, donation to environmental organizations and purchase of green energy. - About 85,000 households participated in the experiment. - Reports were sent between March 14 and May 9, 2008.	- Democratic households reduce consumption by 2.4%, while Republican households decrease consumption by 1.7%. - Households that opted out of the experiment were more likely to be both high consumers of energy and political conservatives. - There are no differential responses between political liberals and conservatives to the normative message included in the first report.

Table A1. Overview of social information campaigns aimed at water/energy conservation (Continued)

Ayres et al. (2013)	Electricity and natural gas	- To evaluate two natural field experiments, providing normative messages on households' usage of electricity and natural gas.	- Two experiments: SMUD (electricity) and PSE (electricity and gas). - In SMUD, reports were delivered to high users monthly and to low users quarterly. In PSE, the frequency of the report was randomized. - About 84,000 households participated in each experiment. - Duration: 1 year (SMUD) and 7 months (PSE).	- In the SMUD experiment, ATEs are 2.3% (high users) and 1.5% (low users). In contrast, there are no significant differences between households receiving monthly or quarterly reports. The average household in the PSE experiment reduced energy use by 1.2%. - Evidence of waning. - Opting out rates are 2% (SMUD) and 1% (PSE).
Smith and Visser (2013)	Water	- To assess the scope of behavioral interventions on households' water use in Cape Town.	- 8 treatments, including social norms and the salience of household's own consumption, salience of water-savings actions and salience of the scrutiny of a household's actions. - About 400,000 participated in the experiment. - The first informational inserts were sent in November 2012.	- Water savings corresponded to 1% of total water use in the town. - The ATEs are larger when households are compared with both neighbors and efficient neighbors. - This result holds in both the "month-period insert" and "year-period insert".
Bernedo et al. (2014)	Water	- To study the longer-term impacts of a one-time behavioral nudge aimed at promoting residential water savings during a drought in the U.S.	- Follow up of the households that participated in the experiments by Ferraro and Price (2013) and Ferraro et al (2011), 6 year after its implementation.	- The treatment effect declines by about 50% one year after the letters were sent. However, from this year onward, the ATE remains steady in both magnitude and statistical significance. - Persistence of treatment effects suggests the presence of longer-lived adjustments to either habits or water-savings infrastructure at home.

Table A1. Overview of social information campaigns aimed at water/energy conservation (Continued)

Allcott and Rogers (2014)	Electricity	- To estimate the long-run effects of the major non-price energy conservation program in the U.S.	- Follow up of the households targeted by Opower experiments in Allcott (2011), 5 years after its implementation. - Treated households were randomly assigned to have treatment either discontinued after about 2 years or continued indefinitely.	- Households in the discontinuation treatment reduced energy use by 2%. However, the ATEs decay at a rate of about 10-20% per year. - Households do not habituate fully even after 2 years of treatment: ATEs in the third through fifth years are 50-60% stronger if the intervention is continued instead of discontinued.
Ito et al. (2014)	Electricity	- To evaluate the effect of intrinsic and extrinsic motivations on energy conservation in peak demand hours.	- Two treatments: moral suasion and monetary incentives. Households in the former received a message requesting voluntary energy conservation with no monetary incentives. In contrast, households in the latter received high electricity prices during peak demand hours. - The interventions were repeated so as to analyze immediate versus later decision making among the groups.	- The moral suasion group decreases electricity use by 8% during the first treatment days. However, the effect disappears after few days. - The monetary incentives group decreases electricity use by 17%. The effect is persistent not only during repeated interventions but also after treatment discontinuation. - There is evidence of habit formation for the monetary treatment only.

Figure A1. Spatial distribution of households participating in the experiment (Jericó)

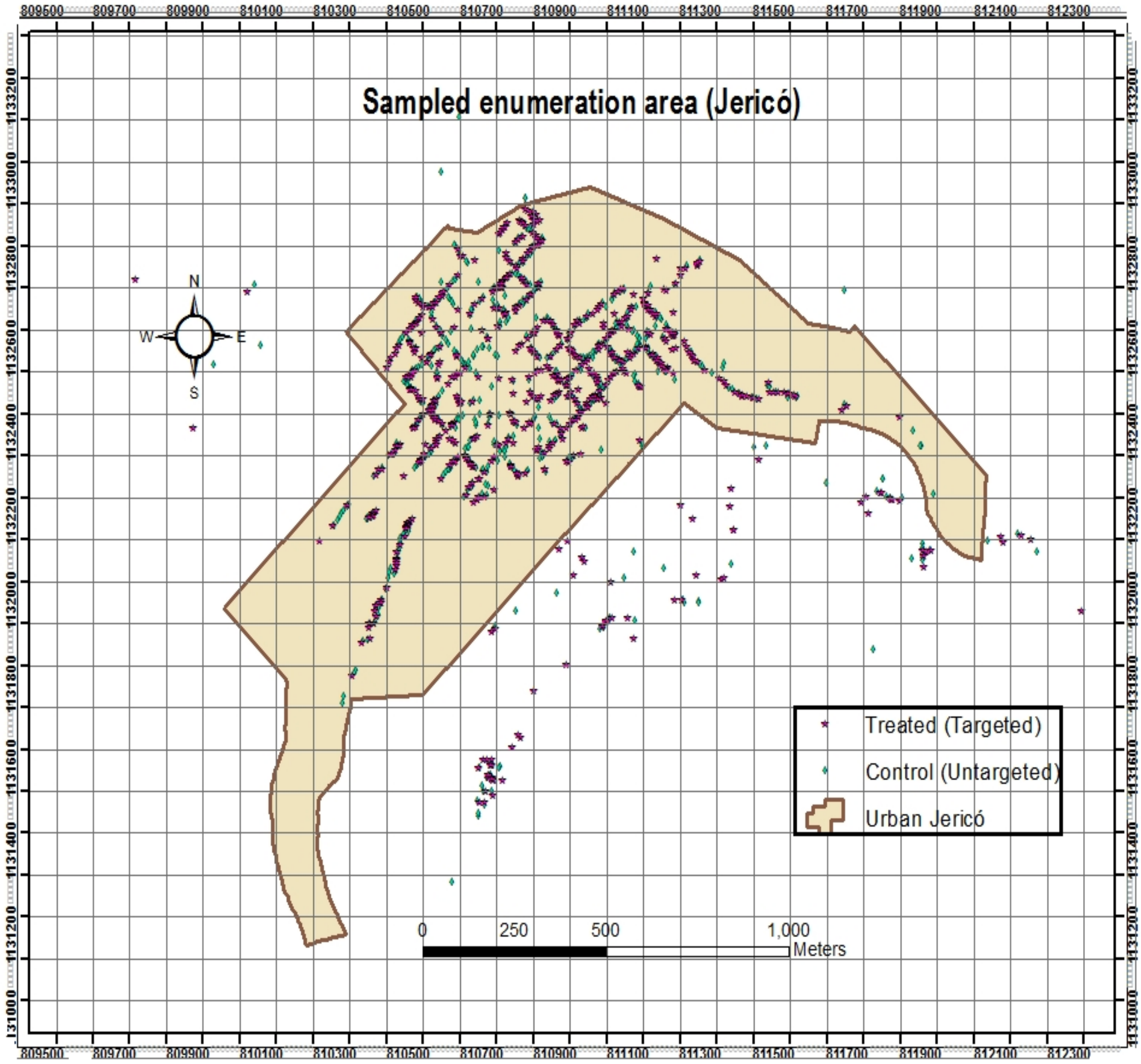


Figure A2. Spatial distribution of the treated and control towns

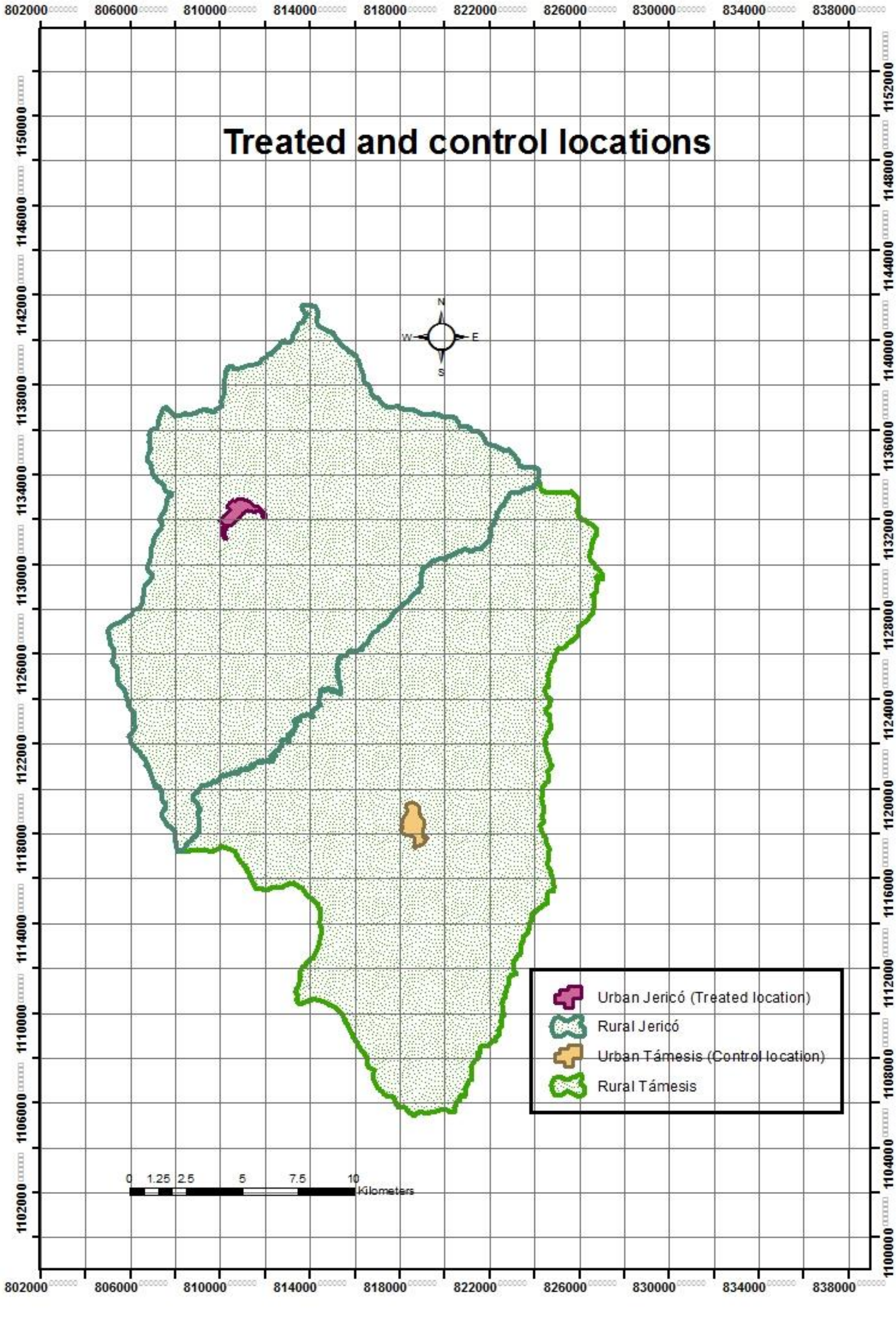


Table A2. Difference in means (targeted vs. untargeted – pre-treatment)

	Mean		Normalized	Difference	*p*-	No. Obs.	
	Untargeted	Targeted	difference	in means	value	Untargeted	Targeted
				Water consumption			
Average consumption (m^3/month)	13.99	14.34	0.0278	0.3518	0.476	655	656
				Socio-economics			
Gender	0.255	0.254	-0.0013	-0.0008	0.973	639	645
Age	51.6	51.2	-0.0196	-0.4416	0.620	639	645
Education (years)	8.26	7.94	-0.0485	-0.3265	0.219	639	645
Household size	3.343	3.338	-0.0019	-0.0047	0.962	639	645
Adult equivalent units	2.42	2.39	-0.0188	-0.0288	0.630	655	656
Household income (COP/month)	468111	480577	0.0163	12466	0.679	639	645
Owned house	0.571	0.569	-0.0032	-0.0022	0.936	639	645
Rented house	0.351	0.358	0.0112	0.0076	0.776	639	645
Family house	0.0798	0.0744	-0.0143	-0.0054	0.717	639	645
				Dwelling			
House size (m^2)	62.43	60.78	-0.0280	-1.6550	0.478	639	645
No. rooms	7.46	7.30	-0.0540	-0.1640	0.171	639	645
Terrace	0.039	0.042	0.0098	0.0027	0.804	639	645
Garden	0.19	0.22	0.0483	0.0277	0.221	639	645
House (several floors)	0.17	0.19	0.0282	0.0154	0.475	639	645
House (one floor)	0.74	0.73	-0.0286	-0.0178	0.469	639	645
Apartment (building)	0.0095	0.0093	-0.0006	-0.0001	0.987	639	645
Apartment (interior)	0.0313	0.0388	0.0287	0.0075	0.468	639	645
House age	28.43	29.09	0.0287	0.6588	0.468	639	645
No. years in dwelling	14.13	14.93	0.0360	0.8004	0.362	639	645
No. months in dwelling (per year)	11.32	11.54	0.0716	0.2188*	0.069	639	645
				Water infrastructure			
Dual flush toilets	0.119	0.116	-0.0047	-0.0027	0.905	639	645
Water-saving showerheads	0.100	0.107	0.0122	0.0068	0.757	639	645
Water-saving sink and dishwasher	0.077	0.076	-0.0014	-0.0007	0.972	639	645
Water-saving washing machine	0.075	0.059	-0.0458	-0.0162	0.246	639	645
Water storage tank	0.538	0.499	-0.0526	-0.0391	0.183	639	645
				Knowledge			
Average water bill (COP/month)	22313	22009	-0.0208	-304.7000	0.599	634	643
Expensive water will	0.405	0.374	-0.0454	-0.0313	0.261	610	613
Keep track water consumption	3.94	3.91	-0.0153	-0.0318	0.699	634	644
				Social capital and Networks			
Time in county (years)	23.62	23.72	0.0060	0.0945	0.879	633	643
No. organizations	1.00	0.93	-0.0349	-0.0718	0.376	630	655

Source: Own elaboration. *** p<0.01, ** p<0.05, * p<0.1. COP refers to Colombian peso. 1 US$ = 1847.91 COP (21-05-2013).

Table A3. Difference in means (targeted vs. control – pre-treatment)

	Mean		Normalized difference	Difference in means	*p*-value	No. Obs.	
	Control	Targeted				Control	Targeted
			Water consumption				
Average consumption (m^3/month)	12.66	14.34	0.1311	1.683***	0.002	500	656
			Socio-economics				
Gender	0.248	0.254	0.0102	0.0063	0.809	500	645
Age	48.54	51.2	0.1168	2.6550***	0.005	500	645
Education (years)	7.16	7.94	0.1230	0.7804***	0.004	500	645
Household size	3.25	3.34	0.0334	0.0840	0.427	500	645
Adult equivalent units	2.38	2.39	0.0090	0.0134	0.830	500	656
Household income (COP/month)	518908	480577	-0.0565	-38330	0.192	500	645
Owned house	0.486	0.569	0.1170	0.0830***	0.005	500	645
Rented house	0.412	0.3581	-0.0781	-0.0539*	0.063	500	645
Family house	0.102	0.074	-0.0686	-0.0276*	0.099	500	645
			Dwelling				
House size (m^2)	45.61	60.78	0.2724	15.17***	0.000	500	645
No. rooms	6.73	7.30	0.1927	0.5717***	0.000	500	645
Terrace	0.078	0.04186	-0.1073	-0.0361***	0.009	500	645
Garden	0.228	0.2202	-0.0132	-0.0078	0.754	500	645
House (several floors)	0.172	0.1907	0.0343	0.0187	0.417	500	645
House (one floor)	0.768	0.7287	-0.0639	-0.0393	0.130	500	645
Apartment (building)	0.014	0.0093	-0.0309	-0.0047	0.457	500	645
Apartment (interior)	0.044	0.0387	-0.0186	-0.0052	0.658	500	645
House age	27.89	29.09	0.0538	1.2070	0.204	500	645
No. years in dwelling	14.69	14.93	0.0108	0.2399	0.798	500	645
No. months in dwelling (per year)	11.86	11.54	-0.1463	-0.3236***	0.001	500	645
			Water infrastructure				
Dual flush toilets	0.146	0.116	-0.0530	-0.0297	0.210	500	645
Water-saving showerheads	0.124	0.107	-0.0312	-0.0170	0.464	500	645
Water-saving sink and dishwasher	0.086	0.076	-0.0217	-0.0100	0.610	500	645
Water-saving washing machine	0.146	0.059	-0.2008	-0.0871***	0.000	500	645
Water storage tank	0.072	0.495	0.5999	0.4226***	0.000	500	645
			Knowledge				
Average water bill (COP/month)	13121	22009	0.5615	8887***	0.000	500	643
Expensive water will	0.361	0.374	0.0182	0.0124	0.674	479	613
Keep track water consumption	4.00	3.91	-0.0397	-0.0830	0.346	500	644
			Social capital and Networks				
Time in county (years)	25.78	23.72	-0.1472	-2.0650***	0.001	500	643
No. organizations	0.8120	0.9282	0.0625	0.1162	0.141	500	655

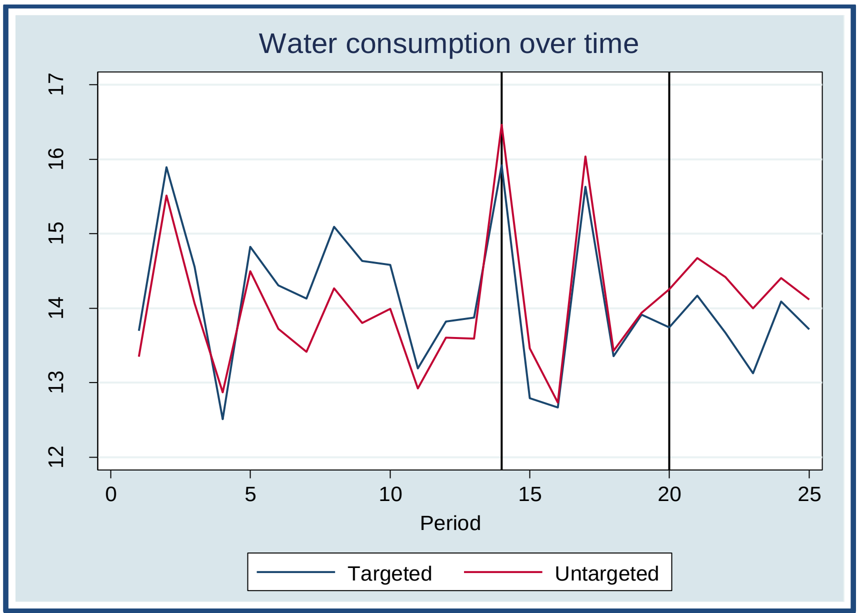
Source: Own elaboration. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A4. Difference in means (untargeted vs. control – pre-treatment)

	Mean		Normalized	Difference	*p*-	No. Obs.	
	Control	Untargeted	difference	in means	value	Control	Untargeted
				Water consumption			
Average consumption (m^3/month)	12.66	13.99	0.1023	1.331**	0.014	500	655
				Socio-economics			
Gender	0.248	0.255	0.0115	0.0071	0.785	500	639
Age	48.54	51.64	0.1376	3.0960***	0.001	500	639
Education (years)	7.16	8.26	0.1699	1.1070***	0.0001	500	639
Household size	3.25	3.34	0.0343	0.0887	0.416	500	639
Adult equivalent units	2.38	2.42	0.0268	0.0422	0.525	500	655
Household income (COP/month)	518908	468111	-0.0825	-50797**	0.055	500	639
Owned house	0.486	0.571	0.1202	0.0852***	0.004	500	639
Rented house	0.412	0.351	-0.0892	-0.0615**	0.034	500	639
Family house	0.102	0.080	-0.0545	-0.0222	0.193	500	639
				Dwelling			
House size (m^2)	45.61	62.43	0.2961	16.82***	0.000	500	639
No. rooms	6.73	7.46	0.2400	0.7357***	0.000	500	639
Terrace	0.08	0.04	-0.1166	-0.0389***	0.005	500	639
Garden	0.23	0.19	-0.0611	-0.0355	0.145	500	639
House (several floors)	0.17	0.18	0.0061	0.0033	0.885	500	639
House (one floor)	0.77	0.75	-0.0355	-0.0215	0.402	500	639
Apartment (building)	0.01	0.01	-0.0303	-0.0046	0.468	500	639
Apartment (interior)	0.04	0.03	-0.0471	-0.0127	0.259	500	639
House age	27.89	28.43	0.0252	0.5483	0.552	500	639
No. years in dwelling	14.69	14.13	-0.0257	-0.5604	0.543	500	639
No. months in dwelling (per year)	11.86	11.32	-0.2038	-0.5423***	0.000	500	639
				Water infrastructure			
Dual flush toilets	0.15	0.12	-0.0490	-0.0271	0.246	500	639
Water-saving showerheads	0.12	0.10	-0.0472	-0.0238	0.265	500	639
Water-saving sink and dishwasher	0.09	0.08	-0.0197	-0.0093	0.645	500	639
Water-saving washing machine	0.15	0.08	-0.1587	-0.0709***	0.0001	500	639
Water storage tank	0.07	0.54	0.6106	0.4643***	0.000	500	639
				Knowledge			
Average water bill (COP/month)	13121	22313	0.5544	9192***	0.000	500	634
Expensive water will	4.50	4.14	-0.0134	-0.3668	0.750	500	634
Keep track water consumption	4.00	3.95	-0.0244	-0.0512	0.563	500	634
				Social capital and Networks			
Time in county (years)	25.78	23.62	-0.1567	-2.1590***	0.0002	500	633
No. organizations	0.81	1.00	0.0982	0.1880**	0.022	500	630

Source: Own elaboration. *** p<0.01, ** p<0.05, * p<0.1.

Figure A3. Average water consumption in April 2012 vs. April 2013 in Jericó (top) and extract of a newspaper (bottom)



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COLOMBIAN SAINT

Madre Laura named Colombia's first saint

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Firew orks crack over a church decorated with a poster of Mother Laura Montoya at her hometown of Jerico, Colombia, Sunday, May 12, 2013, as Pope Francis beatifies her during a ceremony in Rome. Mother Laura, who was born in 1874 and dedicated

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Image 1 of 4

BY JIM WYSS
JWYSS@MIAMIHERALD.COM

JERICÓ, Colombia – In her lifetime, Laura Montoya's stubborn determination to help Colombia's indigenous people brought the reproach of society, the political elite and the church, which viewed her work with suspicion and accused her of being unstable.

But on Sunday, an adoring nation celebrated the woman, better known as *Madre Laura*, as this Catholic country's first saint.

In this hilltop town where she was born, surrounded by coffee farms, revelers crammed the central plaza to watch the Vatican canonization ceremony that began at 2:30 a.m. local time. As Pope Francis announced her name, bells rang, fireworks frightened pigeons out of the trees and a giant portrait of Montoya – her young face framed by a nun's habit – was unveiled on the city's cathedral.