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Essays on Behavioral Economics and Policy Design

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To my family

Table of Contents

"No one who achieves success does so without the help of others. The wise and confident acknowledge this help with gratitude." ***Alfred North Whitehead***

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Marcela Jaime

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Abstracts

This thesis consists of three self-contained chapters:

Chapter 1: Social norms and information diffusion in water-saving programs: Evidence from a randomized field experiment in Colombia

This paper investigates 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. We first evaluate both direct and spillover effects of the campaign. Then we investigate the role of social networks on information dissemination. Results indicate that social information and appeals to norm-based behavior shaped the behavior of households under study. Households directly targeted by the campaign reduced water use by 6.8% during the first year following the intervention. Most importantly, we find significant but short-term evidence of spillover effects: households that were not targeted by the campaign reduced water use by 5.8% 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 classification: C93, D03, L95, O12

Chapter 2: Does the water spill over? Spillover effects from a social information campaign

We investigate whether a social information campaign aimed at reducing water use causes a spillover effect on the use of electricity. On average, water use decreased by 6% for a treatment group for whom we conducted a social information campaign on their use of water, compared with that of a control group. We identify a further spillover effect on electricity usage among households that had efficient use of water before the campaign. The effect is sizeable; this group has almost 9% lower use of electricity after the campaign compared with the control group. We argue that this is consistent with a model of cognitive dissonance where, before the campaign, the individual held the belief that the moral costs/benefits of

consumption are not important despite being an efficient consumer. Due to the campaign, this belief is changed and there is a spillover effect on electricity use.

Key words: Social information, Spillover effects

JEL classification: C93, Q50

Chapter 3: Interactions between CAP agricultural and agri-environmental subsidies and their effects on the uptake of organic farming

In this article, we analyze the effects of the interactions of the two pillars of the European Union Common Agricultural Policy – market support and rural development – on farmers' uptake of organic farming practices. Special attention is given to the 2003 reform, which substantially altered the relative importance of the two types of support by decoupling direct agricultural payments from the production of a specific crop. In our empirical analysis, we study the case of Sweden, making use of the variation in the timing of farmers' decisions regarding participation in support programs. We estimate a dynamic non-linear unobserved effects probit model to take account of unobserved individual heterogeneity and state dependence. Our results indicate the existence of a negative effect of the market support system in place when organic farming techniques were adopted before the 2003 reform; however, this effect is reversed by the introduction of decoupling. Furthermore, the effects of support differ between certified and non-certified organic production: both pillars have significant effects on non-certified organic farming, whereas certified organic farming is exclusively driven by agro-environmental subsidies.

Key words: Common Agricultural Policy, Micro-analysis of farm firms, Panel data models, Subsidy decoupling

JEL classification: Q12, Q18, C23

Introduction

What explains how a household in Jericó, Colombia, and a small farmer in Skåne, Sweden, could be indirectly affected by policies that were not intended to change their behavior in a particular area? Why do unintended effects occur, and are changes in behavior synergistic or antagonistic with respect to the policies in place? Unintended effects of policies, either positive or negative, are often referred to as spillover effects. Spillover effects can be understood as externalities, general equilibrium effects, and interactions and behavioral effects that arise from interdependence between individual decisions, none of which are mediated by markets (Brock and Durlauf, 2001).

This thesis investigates spillover effects of monetary and non-monetary policy instruments for environment and natural resource management, in both developing and developed settings. All chapters share a common feature: the management of resources is characterized by the use of subsidies. Specifically, the thesis consists of three self-contained chapters on issues related to spillover effects of behavioral and policy interventions aimed at reducing negative incentives provided by consumption and production subsidies, and discusses the implications for environmental policy design. The first two chapters investigate spillover effects of a behavioral intervention aimed at incentivizing residential water savings in Colombia. Because there is limited scope for price reform and water prices are highly subsidized, the intervention relies on the use of non-monetary incentives (i.e., provision of social information). While the first chapter focuses on spillover effects due to social interactions within a domain, the second chapter identifies behavioral spillover across domains that are potentially driven by underlying personal motivations. The third paper focuses on monetary incentives, and investigates policy spillovers resulting from the interaction of subsidies in EU agriculture, before and after the introduction of the 2003 reform of the Common Agricultural Policy (CAP). The subsidies under study correspond to the main pillars of the CAP, which account for the monetary support devoted to agricultural and agri-environmental policy in the EU. Although there is consensus regarding the economic importance of having both subsidies in place, there is a concern that their interplay may undercut the adoption of sustainable practices, affecting environmental goals.

Background and motivation

The literature distinguishes between three types of spillover effects. The first type can be regarded as a *social interactions effect*, which takes place because the actions of a reference group could affect an individual's own preferences and behavior (Scheinkman, 2008). While the reference group is context dependent (e.g., family, neighbors, friends, colleagues, peers, etc.), the extent to which an individual's behavior is affected by the reference group depends exclusively on her degree of social connectedness (i.e., on the quality and/or number of connections she has with other individuals in her group). This implies that either being exposed to a large number of individuals (regardless of how closely connected an individual is to any of them) or being socially close to one or more individuals in the reference group is a sufficient condition for an individual to be potentially affected by the behavior of the group (Jackson, 2008).

Although past empirical studies downplayed the possibility of spillovers, there is now extensive evidence that policy interventions have spillover effects due to social interactions. Examples range from education and health (Miguel and Kremer, 2004) and retirement decisions (Duflo and Saez, 2003) to diffusion of agricultural technologies (Conley and Udri, 2010). In a policy intervention, individuals are randomly assigned between a treatment group and a control group. While the treatment group benefits from the intervention, the control group is regarded as an instrument for evaluating the performance of the intervention in a particular area, and thus it is not intended to be affected by the treatment, either directly or indirectly[1]. Because the targeted population is often a subset of the local economy (e.g., the village, neighborhood, municipality, etc.) and the intervention often targets a particular area, the presence of spillovers due to social interactions violates Rubin's (1986) "stable unit treatment value assumption" (SUTVA), which states that the experimental assignment of one individual has no effect on other individuals' potential outcomes. Consequently, if spillovers are not taken into account, the effectiveness of the treatment will be doubly miscalculated: while the effect on the treated is either underestimated or overestimated (depending on the direction of the effect), the effect on the untreated is unmeasured (Angeluci and Di Maro, 2010; Sinclair et al., 2012). This is because, in presence of spillovers, treatment and control individuals change their behavior simultaneously. Accounting for spillovers should thus involve comparing the treatment group with a control group that cannot be affected by the

[1] Because of ethical concerns, control individuals can also benefit afterward from the treatment, but, before the treatment, individuals in the control group are not supposed to be affected by the actions of the treated.

social setting. This can be done either by using multilevel designs in which treatments are randomly assigned to individuals and varying proportions of their neighbors (Sinclair et al., 2012) or by selecting nearby geographic areas with similar characteristics as controls (Fafchamps and Vicente, 2013).

The second type of spillover effect results from an individual's response to psychological processes dictating personal norms of behavior between domains, and thus can be seen as a *behavioral spillover*. Frey (1993) points out that correlation in behavior across domains is most likely to take place when individuals share similar types of inner motivations that affect behavior within each area. One of the core theories explaining these motivations is Festinger's (1957) theory of cognitive dissonance, which suggests that an individual has an inner drive to hold attitudes and beliefs that are in harmony and to avoid disharmony. This theory also points out that cognitive dissonance not only takes place when an individual realizes that her ideas or actions are inconsistent, but also when she is confronted by new information that conflicts with her existing beliefs. Thus, by being consistent between beliefs and behaviors, an individual reduces the disutility or discomfort she experiences when her behavior is not aligned across domains that, because of their perceived similarities, should be in harmony.

Festinger (1957) also distinguishes some instances of dissonance reduction, which have been incorporated in the economic analysis. One instance consists of changing either the behavior or the cognition; this implies that an individual will adopt a similar norm of behavior in the conflicting area. For instance, individuals reduce electricity use after being affected by traffic congestion charges (Kaida and Kaida 2015); individuals who recycle at home are more likely to use less packing waste while shopping (Thogersen, 1999); individuals also are willing to sort their own waste at home, even at a cost, in order to conform to a moral ideal of behavior (Czajkowski et al., 2014). Another instance of dissonance reduction consists of ignoring or denying any information that conflicts with existing beliefs. As Rabin (2002) exemplifies, increasing people's distaste for being immoral can increase the level of immoral activities in society. In either case, an individual's behavior in one area is translated to other areas. Unlike social interaction effects, behavioral spillovers take place internally. Hence, the magnitude of the effect will depend both on an individual's perception of the similarities between the domains of behavior and on the importance an individual attaches to conforming to her personal norm (Dolan and Galizzi, 2015; Thogersen, 2004). Behavioral spillovers have been investigated both in non-experimental studies (Thogersen, 1999; Thogersen and Olander,

2004) and experimental studies (Bednar et al., 2012; Benz and Meier, 2008). These studies evidence a series of spillover effects across a variety of domains, regardless of the approach.

The third type of effect arises when an individual is unintentionally affected by economic incentives that are imposed or granted by a third party. The third parties could be governments, companies or policy makers and the incentives could be, for instance, policy measures; thus, this effect can be regarded as a *policy spillover*. Although policy instruments are often designed to target an individual's behavior in a particular domain, because of the interactions between incentives, the scope of a policy instrument for incentivizing changes in behavior in a particular domain can be extended, synergistically or antagonistically, to other domains as well. The extent of the effect will thus depend on the relative importance of the conflicting areas in an individual's decision making. The interplay between agricultural and environmental policy can be understood as an example of policy spillover. Despite the increasing importance of improving the environmental performance of agriculture, policy instruments promoting intensified agriculture have resulted in negative environmental externalities. For instance, monetary support to agriculture has been associated with increased fertilizer usage (Lewandrowski et al., 1997) and reduced crop diversity (Tilman et al., 2002); other support programs could also result in transboundary pollution by shifting chemical usage from one country to another (Abler and Shortler, 1992). In other instances, because of more complex interactions with the natural environment, it is not clear whether the synergistic or the antagonistic effect will dominate (Just and Antle, 1990; Hediger and Lehmann, 2007). The presence of policy spillovers in agri-environmental policy can not only undermine or ameliorate the effect of the environmental policy, but also make it difficult to evaluate the environmental performance of agriculture (OECD, 2010). As Lichtenberg et al. (2010) point out, the recognition of these effects by agricultural economists has led to a reevaluation of some policies in place and has contributed to the understanding of the synergies and trade-offs involved. Overall, policy spillovers clearly have implications for policy design and cost-benefit analysis, as they affect both the effectiveness and cost of specific policy measures; failure to account for them increases the cost of meeting a particular environmental objective, making it less acceptable to the public and to policy makers.

Despite the underlying reasons for spillovers, their presence imposes a common consequence: spillover effects make it difficult to evaluate policies that were designed to target a particular behavioral domain or economic sector. Although spillover effects are often disregarded in empirical studies, their understanding and quantification have important potential for both policy design and cost-benefit analysis. In particular, while accounting for spillover effects due to social interaction enables us to estimate the real effect of a policy intervention on the area of interest, abandoning what Thogersen (1999) denominates “behavioral silos” will also enable us to determine whether policy interventions targeting behavior in one area could either reinforce or worsen behavior in other areas as well; this could give us a better understanding of the total effects of an intervention (i.e., the aggregated direct and indirect effects). Similarly, because coordinating policies across multiple jurisdictions and sectors is likely to increase administrative costs, compared to a situation in which policies are uncoordinated, accounting for policy spillovers could inform us about the potential benefits and costs of policy coordination, which is an invaluable input for policy formation.

Environmental and natural resource management, like other fields, is prone to spillover effects. Although this area relies on a broad set of policy instruments, including monetary and non-monetary incentives, the fact that an individual’s decisions regarding resource usage takes place in very complex contexts (e.g., socially, economically, politically and even psychologically) may reduce the efficiency of the policy instruments in place. For instance, although it is well known that subsidies threaten the sustainable use of natural resources, removing a subsidy is not always politically feasible. There is also evidence that providing monetary incentives may undermine individuals’ intrinsic motivations, giving rise to crowding-out effects, also known as “the hidden costs of rewards” (Frey, 2012). In contrast, non-monetary incentives, which are designed to crowd in intrinsic motivations, have demonstrated that is possible to enforce changes in behavior by providing moral rewards. Despite the important consequences these findings imply for policy design, concerns regarding the persistence of effects may undermine their potential as policy instruments. The scope of reduced efficiency of policy instruments and the fact that resource usage encompasses important social dynamics are fertile grounds that favor the study of spillover effects.

Chapter I of this thesis contributes to the literature on spillover effects due to social interactions in behavioral interventions. The paper investigates spillover effects of a social information campaign aimed at encouraging residential water savings in Colombia. Specifically, it evaluates whether households that were not targeted by the campaign, but knew of its existence, also decrease water use. The campaign was organized as a randomized field experiment, consisting of monthly delivery of consumption reports, including normative messages, for one year. Following the literature on spillover effects in program evaluation, we propose a methodology that allows a separation of direct and spillover effects of the information campaign. Then we investigate the role of social networks on 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. The results indicate that social information and appeals to norm-based behavior affected the behavior of households under study. Households directly targeted by the campaign reduced water use by 6.8% during the first year following the intervention. Wealthier households and high users of water decreased water use to a greater extent than poorer households and low users of water. Most importantly, we find evidence of spillover effects: households that were not targeted by the campaign reduced water use by 5.8% 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.

Overall, the findings demonstrate the potential of non-pecuniary incentives as a mechanism to influence water use in a developing country setting. Further, the effect was greatest among higher-income and high-usage households, two populations that impose more pressure on the resource. The findings also suggest that non-pecuniary incentives can be suitable and inexpensive instruments for shaping the behavior of an entire population in short-run interventions. However, spillover effects vanished after five months; therefore, long-run policy interventions will have a higher impact if the treatment is administered to the entire population.

While Chapter I focuses on individual behavior in a social setting, **Chapter II** focuses on the individual's search for internal consistency across consumption domains, and thus contributes to the literature on behavioral spillovers in environmentally responsible behavior. This paper investigates whether an information campaign aimed at encouraging residential water savings had spillover effects on electricity use. Although there is ample evidence that behavioral interventions can affect the consumption choices households make in areas such as water use and electricity separately, whether this behavioral intervention spills over to other consumption decisions is a question that remains answered. In 2013, we conducted a randomized field experiment in a Colombian town. We provided monthly consumption reports including normative messages to a treatment group for one year. During the same time period, we collected information about electricity use in the same households. We first investigate whether there is a direct spillover of the campaign itself (i.e., whether the information campaign on water use has an overall effect on the electricity use of households targeted by the campaign). Then we investigate whether the campaign operates through predetermined underlying motivations/attitudes giving rise to changes in water use (i.e., whether there are spillover effects for particular groups of households). The results indicate that, although we cannot distinguish an effect on electricity use for households receiving consumption reports, there is a positive spillover effect on electricity usage among households that had efficient use of water before the campaign. The effect is sizeable; this group has almost 9% lower use of electricity after the information campaign compared with the control group 11 months into the campaign. Interestingly, there are no observable differences between efficient and inefficient users of water with respect to stated reasons for saving water or regarding their perceptions of water scarcity. We argue that this is consistent with a model of cognitive dissonance in which, before the campaign, the individual held the belief that moral concerns about consumption are not important, despite being an efficient consumer. Due to the campaign, this belief is changed and there is a spillover effect on electricity use.

Chapter III contributes to the literature on policy spillovers resulting from the interplay of agricultural and agri-environmental incentives in EU agriculture. The paper analyzes the effects of the interactions of the two pillars of the European Union Common Agricultural Policy – market support and rural development – on farmers' uptake of organic farming practices. Although there is consensus that financial support under the pillars has been crucial for the viability of farming, there is the concern that intensive methods promoted under Pillar One may undercut the adoption of sustainable practices under Pillar Two. With the introduction of the CAP reform in 2003, the relative importance of both types of support was substantially altered because direct agricultural payments under Pillar One were decoupled from the production of a specific crop. The introduction of decoupling was expected to generate not only an increase in farmers' uptake of organic farming but also a significant change in the role of Pillar One as a driver of farmers' behavior. By using data from nine rounds of a balanced panel consisting of 394 Swedish producers during the period 2000-2008, we evaluate these hypotheses econometrically. The empirical strategy makes use of the variation in the timing of farmers' decisions regarding participation in support programs under the pillars. Moreover, by estimating dynamic non-linear unobserved effects probit models, we take account of unobserved individual heterogeneity and state dependence. Our results indicate the existence of a negative effect of the market support system in place when organic farming techniques were adopted before the 2003 reform; however, this effect is reversed by the introduction of decoupling. Furthermore, the extent to which Pillar One affects the uptake of organic farming also depends on market certification: certified farmers are not affected by the subsidies under Pillar One because they rely mainly on Pillar Two subsidies.

To summarize, this thesis investigates spillover effects that could take place in different spheres of environmental and natural resource management, in both developing and developed countries. It specifically analyzes three types of effects: spillover effects due to social interactions, behavioral spillovers and policy spillovers. The findings indicate that the studied interventions were affected by the three types of spillovers, and that the magnitude of the spillover effects was similar to that of the effects originally intended by the intervention. The results provide further evidence that a sole intervention could produce, and be affected by, more than one type of spillover effect. For instance, in the social information campaign implemented in Colombia, individuals' behavior was transmitted not only from one individual to another, but also within individuals across consumption domains. Moreover, the study of the interplay between agricultural and agri-environmental policy shows that it is possible to reverse the negative effects imposed by antagonistic policies that rely on monetary incentives. Thus, these findings contribute to the discussion on the importance of accounting for unintended effects of policies, as inputs not only for policy evaluation but also for the design of more cost-efficient interventions.

The findings are also expected to generate a discussion regarding the appropriateness of using non-monetary incentives as mechanisms for influencing individuals' behavior in developing countries. Moreover, the fact that these incentives affect individuals' behavior in areas other than those targeted by the policy, and that behavior can be transmitted from one individual to another, provides substantial evidence supporting the importance of using these incentives more frequently as policy instruments.

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Chapter I

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

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Abstract

This paper investigates 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. We first evaluate both direct and spillover effects of the campaign. Then we investigate the role of social networks on information dissemination. Results indicate that social information and appeals to norm-based behavior affected the behavior of households under study. Households directly targeted by the campaign reduced water use by 6.8% during the first year following the intervention. Most importantly, we find evidence of spillover effects: households that were not targeted by the campaign reduced water use by 5.8% 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 Classification: 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. 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). 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*)[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, 2012; Ferraro et al., 2011; Allcott, 2011).[2] There is also evidence on the effects of non-pecuniary incentives in other pro-environmental behaviors (see, e.g., Chong et al., 2013; 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 knew of its existence, also decrease 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,

[1] As Cialdini (2003) states: "*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).

[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.

[3] In contrast, information without a social comparison is not likely to achieve much savings (Smith and Visser, 2013; Ek and Söderholm, 2010; Campbell et al., 2004).

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 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

[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 socioeconomic 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.

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 discharge rates are very high and the corresponding cost of wastewater treatment is also very 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 climate variability could reduce water supply and, as a result, affect the energy supply, because the region relies heavily on hydropower.

[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 consumptions exceed 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.

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 fulfil 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 towards 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.

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 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.

[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.

In order to be able to make a direct comparison, 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. In the descriptive norm section, each household is compared to the mean and 25th percentile of its comparison group.[13] The injunctive norm section categorizes households as “Excellent,” “Average” or “Room to improve.” 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.

[Insert Figure 1 here]

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

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

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

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 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 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 Spillover effects

Due to network or other contextual effects, the impact of the intervention could 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 similar characteristics to Jericó, as an additional control. A random sample of 500 households was selected from the list of residential customers in this town.[14] 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).[15] Water provision in both towns is administered by public utilities that share the 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, Appendix A.

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 spillovers 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 likely to be 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 on 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 very informative for policy design.

[14]Users were randomly selected, as all the meters work perfectly according to the local water utility.
[15]At present, Támesis has 15,714 inhabitants, of which 6,397 live in the urban area.

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.[16] Table 1 presents the average pre- and post-treatment water use for the groups of 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 quite drastically over the year, but that the variation is similar across the different groups.

[Insert Table 1 here]

To begin with, we investigate the characteristics of the three different groups – targeted, untargeted and control – in the pre-treatment period. Tables A2-A4, in Appendix A, present the results of two procedures for testing the balance of both average water use and household characteristics in the 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).[17] 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.

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

[17] Normalized differences are 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. 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 relation 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.

Despite the fact that 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 significant different among groups;[18] 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) 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 latter captures the accessibility to the main focal point in the town.[19] 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.

[Insert Table 2 here]

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,

[18] 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.

[19] 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.

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 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. 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. Hence, estimates are regarded as baseline. The primary specification consists of the difference-in-differences estimator, in which water use is modelled as follows:

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

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

In a second stage, 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 both 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[20]. 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 are able to estimate

[20] 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).

spillover effects of the campaign, i.e., we can test whether households in Jericó that were not targeted by the campaign were indirectly treated, and therefore changed their water use. By doing so, we answer the main question of this paper. Second, by comparing targeted households in Jericó with control households in Támesis, we can estimate the "real" or *total effect* of the campaign on the average targeted household. In 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. Both spillovers and total effects of the campaign are captured by the parameter α in equation (1).

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

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

where: n_i is the demeaned measure of social connectedness[22] (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.

[21]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.

[22]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 often "solve" the multicollinearity problem in regression (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.

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%.[23] 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.

[Insert Table 3 and Figure 2 here]

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

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

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. 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 50th percentile of the distribution of each covariate as the cut-off.[24] Equation (1) is then estimated for each subsample. Estimation results suggest a great deal of heterogeneity, as shown in Table 4.

[Insert Table 4 here]

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. Moreover, ATEs are statistically significantly different, at the 1% level, from those of 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).

[24] 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.

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 significant after eleven months.

The monthly ATEs are displayed in Figure 3. Once again, estimates reveal a rapid and significant response to the treatment during the first and second months following the start of the experiment. 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.[25] 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.[26] Estimates of the total effects excluding April 2013 are displayed in columns (5)-(8) in Table 5.

[Insert Table 5 and figure 3 here]

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 steady and statistically significant, yet 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 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

[25]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 Appendix A.

[26]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.

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. 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.[27]

[Insert Table 6 and figure 4 here]

Our result suggests that untargeted households somehow 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 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

[27] 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.

doing 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 type 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. This is suitable if the objective of the intervention is 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 increase the stringency of the policies by either targeting all households or combining pecuniary and non-pecuniary incentives.

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.

[Insert Table 7 here]

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. Although most variables are statistically insignificant, 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).

[Insert Table 8 here]

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. This suggests that social networks did not 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. It is worth mentioning, 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 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. Because the spillover effects vanished after five months, long-run policy interventions will have a higher impact if the treatment is administered to the entire population.

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 attributed 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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List of Tables

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* Mean/std. Dev.	*Targeted* Norm. Diff.	*Untargeted* Mean/std. Dev.	*Untargeted* Norm. Diff.	*Control* 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)

Source: Own elaboration based on the *ex-post* data. Normalized differences are calculated with respect to the control. *Normalized differences exceed the threshold suggested by Wooldridge and Imbens (2009). Standard deviations in parentheses.

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). Cluster 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). Cluster 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). Cluster 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). Cluster standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 7. Reinforcement 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	-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

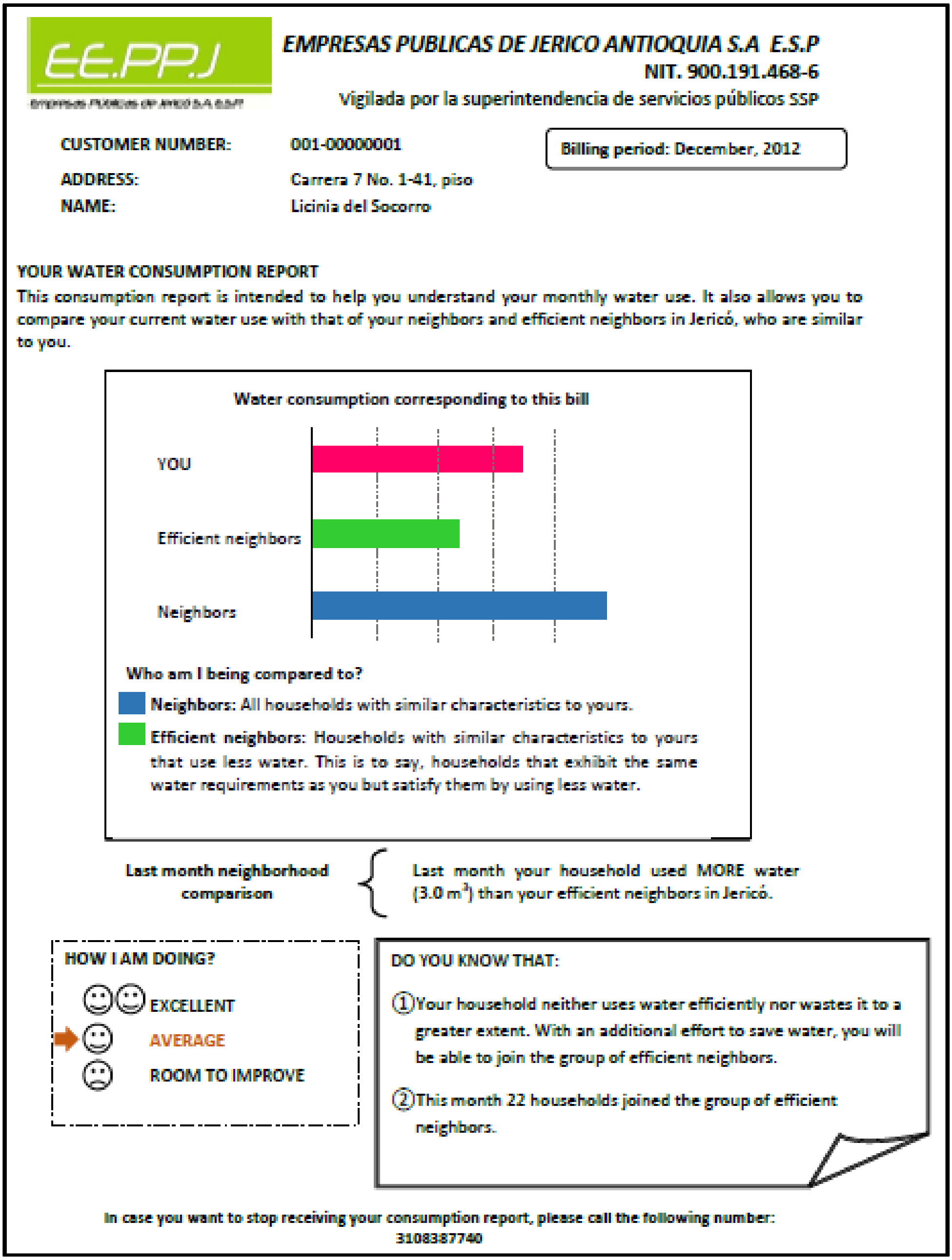
Note: This table shows estimates of the baseline specification capturing the heterogeneous treatment effects of the campaign for the matched sample of households with working meters. Estimates correspond to the period Dec. 2011 – Dec. 2013. April 2013 is excluded from all regressions. The dependent variable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

List of Figures



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Empresas Publicas de Jerico S.A E.S.P

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

Billing period: December, 2012

ADDRESS: Carrera 7 No. 1-41, piso

NAME: Licinia del Socorro

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:

①Your 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.

②This 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 1. Example of consumption report

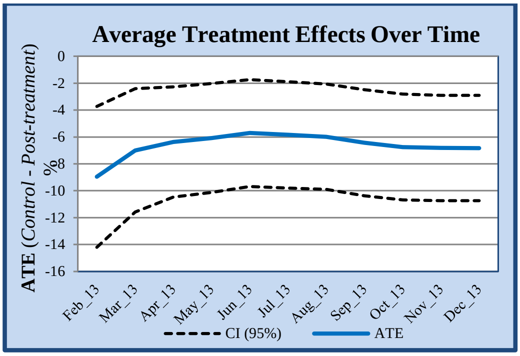


Figure 2. Monthly direct effects (Targeted vs. Untargeted)

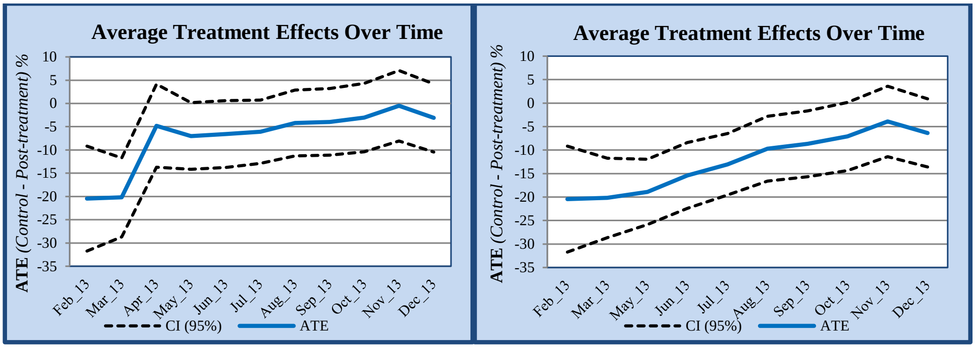


Figure 3. Monthly combined effects including (*left*) and excluding (*right*) April 2013 (Targeted vs. Control)

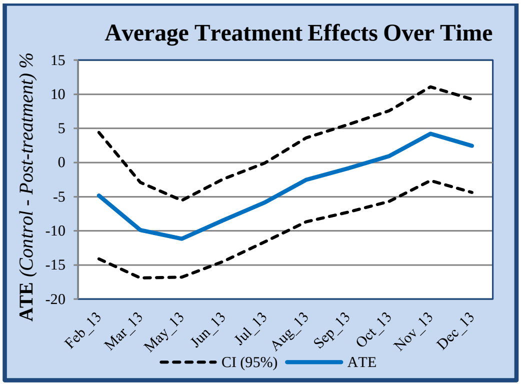


Figure 4. Monthly spillover effects (Untargeted vs. Control - Excluding April 2013)

Appendix A. Additional tables and figures

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.	- The 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 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 across households in the weak social norm treatment, compared with those in the control group. In contrast, the impact of strong social norms treatment can be detected after more than two years. - Evidence of waning: $ATE_{2008} > ATE_{2009}$.
Mizobuchi and Takeuchi (2012)	Electricity	- To examine the influence of economic and psychological factors on electricity conservation behavior in Matsuyama (Japan).	- 236 households were randomized into 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 decrease electricity use by 8.2%.
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 7onth.

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

Costa and Kahn (2013)	Electricity	- To evaluate the role of political ideology on the effectiveness of energy conservation nudges.	- The treatment consisted of the delivery of “home energy reports”, including neighbor comparisons and energy-savings tips. - Ideology is measured through affiliation to 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.
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 households in 2443 neighborhoods 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”.

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

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.
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 reduce 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 hot versus cold 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.

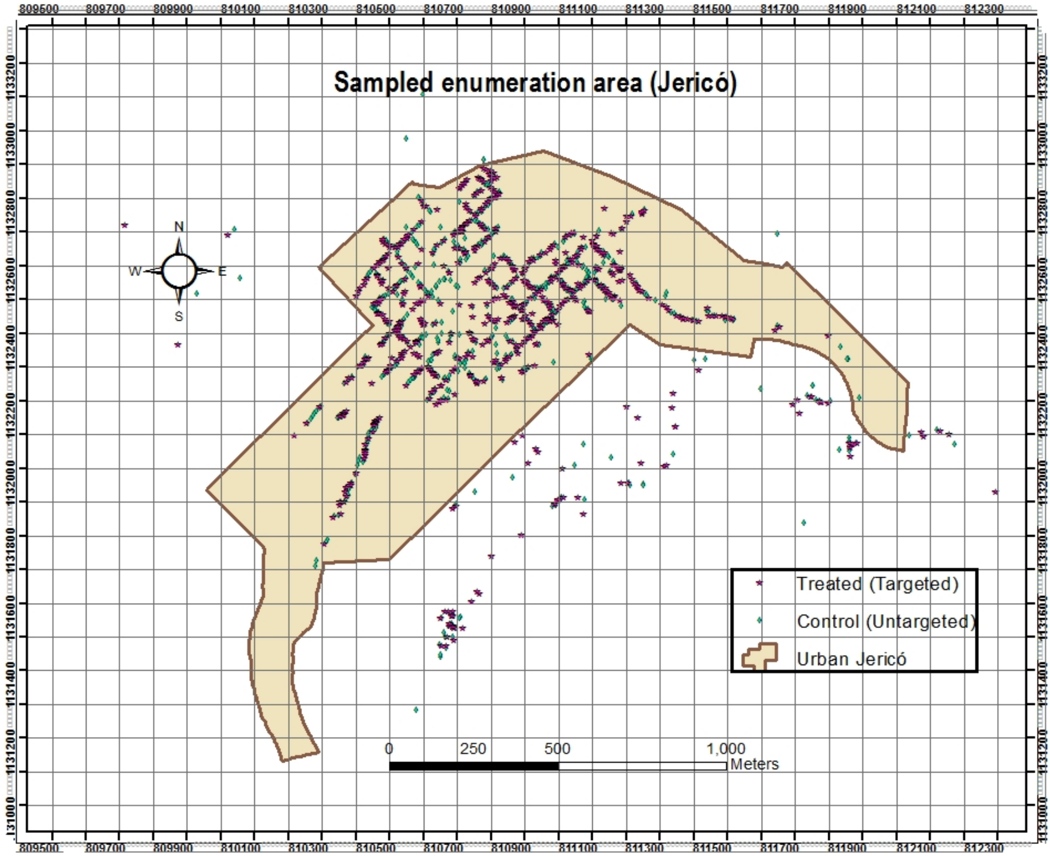


Figure A1. Spatial distribution of households participating in the experiment (Jericó)

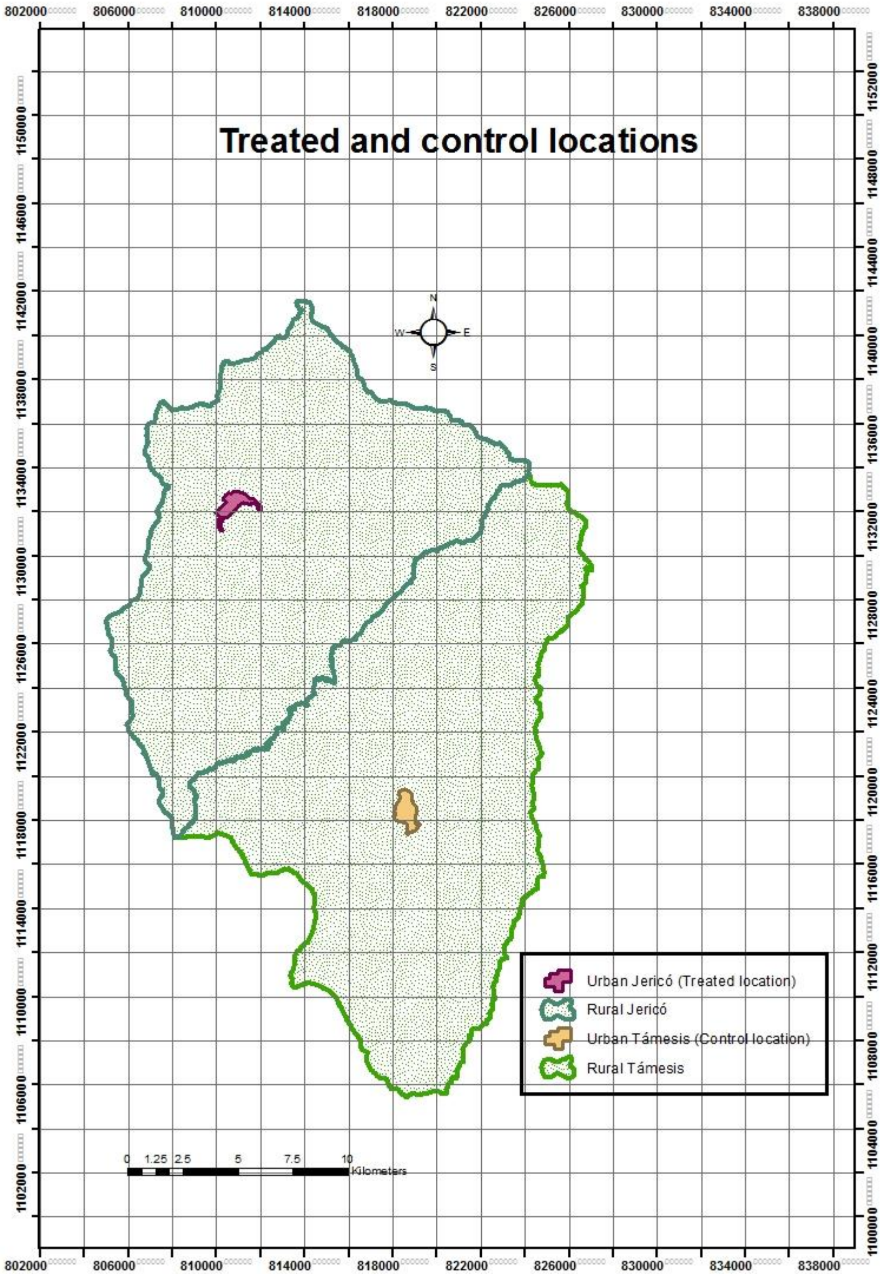


Figure A2. Spatial distribution of the treated and control towns

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	0.4759	655	656
				Socio-economics			
Gender	0.2551	0.2543	-0.0013	-0.0008	0.9730	639	645
Age	51.6	51.2	-0.0196	-0.4416	0.6202	639	645
Education (years)	8.26	7.94	-0.0485	-0.3265	0.2191	639	645
Household size	3.343	3.338	-0.0019	-0.0047	0.9618	639	645
Adult equivalent units	2.42	2.39	-0.0188	-0.0288	0.6296	655	656
Household income (COP/month)	468111	480577	0.0163	12466	0.6793	639	645
Owned house	0.571	0.569	-0.0032	-0.0022	0.9362	639	645
Rented house	0.351	0.358	0.0112	0.0076	0.7763	639	645
Family house	0.0798	0.0744	-0.0143	-0.0054	0.7174	639	645
				Dwelling			
House size (m^2)	62.43	60.78	-0.0280	-1.6550	0.4779	639	645
No. rooms	7.46	7.30	-0.0540	-0.1640	0.1707	639	645
Terrace	0.039	0.042	0.0098	0.0027	0.8037	639	645
Garden	0.19	0.22	0.0483	0.0277	0.2210	639	645
House (several floors)	0.17	0.19	0.0282	0.0154	0.4752	639	645
House (one floor)	0.74	0.73	-0.0286	-0.0178	0.4690	639	645
Apartment (building)	0.00939	0.00930	-0.0006	-0.0001	0.9870	639	645
Apartment (interior)	0.0313	0.0388	0.0287	0.0075	0.4677	639	645
House age	28.43	29.09	0.0287	0.6588	0.4677	639	645
No. years in dwelling	14.13	14.93	0.0360	0.8004	0.3615	639	645
No. months in dwelling (per year)	11.32	11.54	0.0716	0.2188*	0.0689	639	645
				Water infrastructure			
Dual flush toilets	0.1189	0.1163	-0.0047	-0.0027	0.9053	639	645
Water-saving showerheads	0.1002	0.107	0.0122	0.0068	0.7566	639	645
Water-saving sink and dishwasher	0.0767	0.0760	-0.0014	-0.0007	0.9715	639	645
Water-saving washing machine	0.0751	0.0589	-0.0458	-0.0162	0.2459	639	645
Water storage tank	0.5383	0.4992	-0.0526	-0.0391	0.1826	639	645
				Knowledge			
Average water bill (COP/month)	22313	22009	-0.0208	-304.7000	0.5986	634	643
Expensive water will	0.4049	0.3736	-0.0454	-0.0313	0.2613	610	613
Keep track water consumption	3.94	3.91	-0.0153	-0.0318	0.6996	634	644
				Social capital and Networks			
Time in county (years)	23.62	23.72	0.0060	0.0945	0.8791	633	643
No. organizations	1.00	0.93	-0.0349	-0.0718	0.3757	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)

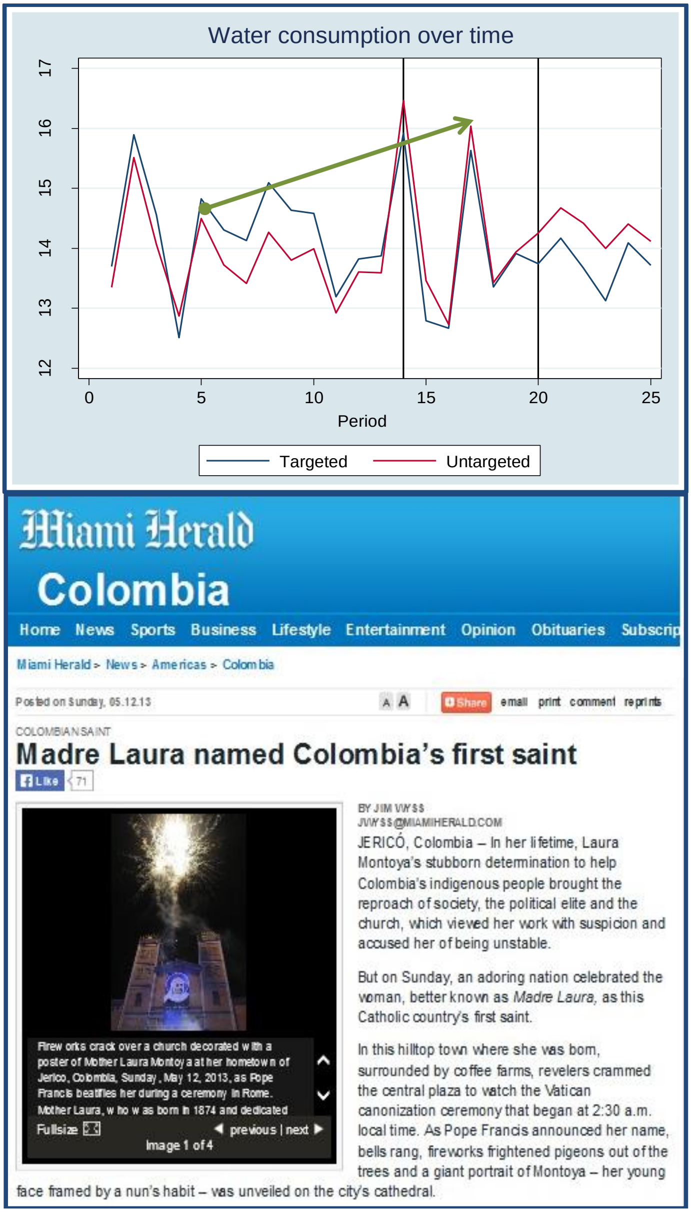
	Mean		Normalized	Difference	*p*-value	No. Obs.	
	Control	Targeted	difference	in means		Control	Targeted
				Water consumption			
Average consumption (m^3/month)	12.66	14.34	0.1311	1.683***	0.0016	500	656
				Socio-economics			
Gender	0.248	0.2543	0.0102	0.0063	0.8088	500	645
Age	48.54	51.2	0.1168	2.6550***	0.0054	500	645
Education (years)	7.16	7.94	0.1230	0.7804***	0.0035	500	645
Household size	3.25	3.34	0.0334	0.0840	0.4268	500	645
Adult equivalent units	2.38	2.39	0.0090	0.0134	0.8296	500	656
Household income (COP/month)	518908	480577	-0.0565	-38330	0.1920	500	645
Owned house	0.486	0.569	0.1170	0.0830***	0.0052	500	645
Rented house	0.412	0.3581	-0.0781	-0.0539*	0.0629	500	645
Family house	0.102	0.074	-0.0686	-0.0276*	0.0997	500	645
				Dwelling			
House size (m^2)	45.61	60.78	0.2724	15.17***	0.0000	500	645
No. rooms	6.73	7.30	0.1927	0.5717***	0.0000	500	645
Terrace	0.078	0.04186	-0.1073	-0.0361***	0.0092	500	645
Garden	0.228	0.2202	-0.0132	-0.0078	0.7535	500	645
House (several floors)	0.172	0.1907	0.0343	0.0187	0.4170	500	645
House (one floor)	0.768	0.7287	-0.0639	-0.0393	0.1298	500	645
Apartment (building)	0.014	0.0093	-0.0309	-0.0047	0.4572	500	645
Apartment (interior)	0.044	0.0387	-0.0186	-0.0052	0.6579	500	645
House age	27.89	29.09	0.0538	1.2070	0.2042	500	645
No. years in dwelling	14.69	14.93	0.0108	0.2399	0.7975	500	645
No. months in dwelling (per year)	11.86	11.54	-0.1463	-0.3236***	0.0007	500	645
				Water infrastructure			
Dual flush toilets	0.146	0.1163	-0.0530	-0.0297	0.2098	500	645
Water-saving showerheads	0.124	0.1070	-0.0312	-0.0170	0.4641	500	645
Water-saving sink and dishwasher	0.086	0.0759	-0.0217	-0.0100	0.6102	500	645
Water-saving washing machine	0.146	0.0589	-0.2008	-0.0871***	0.0000	500	645
Water storage tank	0.072	0.4946	0.5999	0.4226***	0.0000	500	645
				Knowledge			
Average water bill (COP/month)	13121	22009	0.5615	8887***	0.0000	500	643
Expensive water will	0.3612	0.3736	0.0182	0.0124	0.6736	479	613
Keep track water consumption	4.00	3.91	-0.0397	-0.0830	0.3456	500	644
				Social capital and Networks			
Time in county (years)	25.78	23.72	-0.1472	-2.0650***	0.0006	500	643
No. organizations	0.8120	0.9282	0.0625	0.1162	0.1408	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**	0.0144	500	655
				Socio-economics			
Gender	0.248	0.255	0.0115	0.0071	0.7848	500	639
Age	48.54	51.64	0.1376	3.0960***	0.0010	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	0.4157	500	639
Adult equivalent units	2.38	2.42	0.0268	0.0422	0.5249	500	655
Household income (COP/month)	518908	468111	-0.0825	-50797**	0.0549	500	639
Owned house	0.486	0.571	0.1202	0.0852***	0.0042	500	639
Rented house	0.412	0.351	-0.0892	-0.0615**	0.0338	500	639
Family house	0.102	0.080	-0.0545	-0.0222	0.1934	500	639
				Dwelling			
House size (m^2)	45.61	62.43	0.2961	16.82***	0.0000	500	639
No. rooms	6.73	7.46	0.2400	0.7357***	0.0000	500	639
Terrace	0.08	0.04	-0.1166	-0.0389***	0.0047	500	639
Garden	0.23	0.19	-0.0611	-0.0355	0.1452	500	639
House (several floors)	0.17	0.18	0.0061	0.0033	0.8851	500	639
House (one floor)	0.77	0.75	-0.0355	-0.0215	0.4018	500	639
Apartment (building)	0.01	0.01	-0.0303	-0.0046	0.4677	500	639
Apartment (interior)	0.04	0.03	-0.0471	-0.0127	0.2594	500	639
House age	27.89	28.43	0.0252	0.5483	0.5517	500	639
No. years in dwelling	14.69	14.13	-0.0257	-0.5604	0.5429	500	639
No. months in dwelling (per year)	11.86	11.32	-0.2038	-0.5423***	0.0000	500	639
				Water infrastructure			
Dual flush toilets	0.15	0.12	-0.0490	-0.0271	0.2459	500	639
Water-saving showerheads	0.12	0.10	-0.0472	-0.0238	0.2652	500	639
Water-saving sink and dishwasher	0.09	0.08	-0.0197	-0.0093	0.6452	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***	0.0000	500	639
				Knowledge			
Average water bill (COP/month)	13121	22313	0.5544	9192***	0.0000	500	634
Expensive water will	4.50	4.14	-0.0134	-0.3668	0.7504	500	634
Keep track water consumption	4.00	3.95	-0.0244	-0.0512	0.5632	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**	0.0215	500	630

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



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COLOMBIAN SAINT

Madre Laura named Colombia's first saint

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Fireworks 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

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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.

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

Chapter II

Does the Water Spill Over? Spillover Effects from a Social Information Campaign*

Fredrik Carlsson† and Mónica M. Jaime†,‡

Abstract

We investigate whether a social information campaign aimed at reducing water use causes a spillover effect on the use of electricity. On average, water use decreased by 6% for a treatment group for whom we conducted a social information campaign on their use of water, compared with that of a control group. We identify a further spillover effect on electricity usage among households that had efficient use of water before the campaign. The effect is sizeable; this group has almost 9% lower use of electricity after the campaign compared with the control group. We argue that this is consistent with a model of cognitive dissonance where, before the campaign, the individual held the belief that the moral costs/benefits of consumption are not important despite being an efficient consumer. Due to the campaign, this belief is changed and there is a spillover effect on electricity use.

Key words: Social information, Spillover effects

JEL classification: C93, Q50

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1. Introduction

There is now ample evidence that non-price conservation programs such as providing social information can affect the consumption choices households make in areas such as water use (Ferraro and Price, 2013; Ferraro et al., 2011) and energy use (Ito et al., 2014; Costa and Kahn, 2013; Ayres et al., 2013; Allcott, 2011). The provision of social information could, for instance, take the form of appeals to pro-social preferences or provision of social comparisons including normative messages. For example, in Allcott (2011), consumers were sent letters comparing their electricity use to that of their neighbors and were categorized based on the social approval of their actions; an average treatment effect of 2% was found. Similar effects were found in both Costa and Kahn (2013) and Ayres et al., (2013). In the water domain, Ferraro and Price (2013) show that, while pro-social messages decrease water use by nearly 3%, average treatment effects are larger when households are provided with normative messages (4.8%). There is also evidence that these types of interventions result in persistent changes in behavior, even after the treatment has ended. This suggests that cost-effectiveness assessments have underestimated the economic benefits of these programs (Allcott and Rogers, 2014; Bernedo et al., 2014).

Under the assumption that social information does affect behavior and consumption choices for a particular good, an interesting question is whether this provision of information spills over to other consumption decisions. There are, as we will discuss in detail, reasons that the consumption of other goods can increase or decrease due to the provision of social information. In this paper, we investigate whether an information campaign aimed at encouraging residential water savings had spillover effects on electricity use. In 2013, we conducted a randomized field experiment in a Colombian town. We provided monthly consumption reports including normative messages to a treatment group for one year. The average household targeted by the campaign decreased water use by 6.8% (Jaime, 2015). During the same time period, we collected information about electricity use in the same households. The goal was to ascertain whether the water-savings information campaign not only had an effect on water use, but also had a spillover effect on electricity use.

Why would an information campaign in one area affect behavior in another area? As discussed by Frey (1993), there could be motivation spillovers between different goods/areas, in particular if there are similar types of inner motivations that affect behavior for both goods, such as environmental or moral concerns. Whether the spillover is positive or negative, Frey argues, depends on a number of factors, such as the similarities between the goods, the motivations of the individuals and the strength of the social norms. Perhaps one of the most important reasons that environmentally friendly behavior in different domains is correlated is the desire to be consistent in beliefs and behaviors (Cialdini, 1984). According to the theory of cognitive dissonance, people wish to avoid holding contradictory beliefs, and suffer from being inconsistent (Festinger, 1957). A number of empirical studies on environmentally responsible behavior suggest that there is indeed a correlation in behavior across settings/goods (see, e.g., Kaida and Kaida, 2015; Thogersen, 2004; Thogersen and Olander, 2003; Thogersen 1999; Berger, 1997; Stern et al., 1986). For instance, individuals who changed their weekday travel mode due to the introduction of congestion charges also exhibited positive changes in resource and energy use (Kaida and Kaida, 2015). Similarly, Thogersen (1999) found that individuals who recycle at home were more likely to decrease packaging waste when shopping.

There is also a literature on measurement of pro-social behavior using experiments, which provides evidence that social preferences are partly stable over different domains (see, e.g., Blanco et al., 2011; Benz and Meier, 2008; Karlan, 2005) and over time (see, e.g., Carlsson et al., 2014; Volk et al., 2012; Brosig et al., 2007), although the extent of the stability varies among studies. However, another set of studies suggests that a variation of the strength of social preferences is to be expected due to moral licensing reasons (Monin and Miller, 2001). Moral licensing suggests that people receive an implicit license to behave selfishly in one setting by acting pro-socially in another setting. For example, Mazar and Zhong (2010) found that people became less altruistic after purchasing environmentally-friendly products than after purchasing conventional products. A study by Kouchacki (2011) showed that people were more willing to express prejudiced attitudes when their group members' past behavior had established non-prejudiced credentials.

There are two important things to point out. First, there are two distinct but related ways in which a spillover effect can occur. The first is direct spillover from the campaign itself. For example, raising environmental awareness in one area could raise awareness in other areas as well. The second is an indirect effect, working through predetermined underlying motivations/attitudes, which in turn gives rise to a change in behavior in the secondary area. Thus, the change in behavior in the secondary area will strictly depend on individuals' motivations/attitudes. We will not be able to isolate these effects from each other in a clean way in this study, but we can investigate the behavior in different groups. In particular, we can compare households with different characteristics before the start of the campaign. If the spillover effect is caused primarily by changes in behavior in the primary area, i.e., a direct effect, we would observe a spillover effect for the group of households receiving information in the primary area. A second point is that it is not clear whether we should expect a positive spillover effect (i.e., a reduction in energy usage) or a negative spillover effect (i.e., an increase in energy usage). As discussed, there are some reasons for expecting a positive spillover, for example, because of stability of social preferences across domains and the generalizability of norms. On the other hand, moral licensing suggests that there could be a negative spillover effect.

The rest of the paper is organized in the following way. In Section 2, we present a conceptual framework explaining spillovers across consumption domains and, in Section 3, we present the basics of the experimental design. In Section 4, we present the empirical strategy, Sections 5 and 6 present the results and a number of robustness checks. Section 7 concludes the paper.

2. Conceptual framework

We develop a simple conceptual framework with cognitive dissonance and social preferences. The purpose is to identify the channels through which an information campaign in one area can have a spillover effect in another area. In our model, an individual consumes two goods, x_1 and x_2. Apart from a direct positive utility of consuming the goods, the individual cares about her own consumption of these two goods compared with the consumption by other individuals. There is by now extensive evidence that people do care about their status or relative consumption; see, e.g., Clark and Senik (2010), Frank (1985a, b), Johansson-Stenman et al., (2002), and Solnick and Hemenway (1998). People also experience disutility by acting in ways that are at odds with their own identity (Akerlof and Kranton, 2000, 2005). Levitt and List (2007) distinguish between wealth and moral arguments of the utility function. In our specific case, we assume that there is a positive effect on utility that is caused by consuming less than others, because the consumption of the goods results in negative externalities. We assume an additive separable utility function of the following form:

$$U = u_1(x_1) + u_2(x_2) + \tau_1 S_1(x_1, \bar{x}_1) + \tau_2 S_2(x_2, \bar{x}_2), \quad (1)$$

where $\partial u_i/\partial x_i > 0$, $\partial^2 u_i/\partial x_i^2 < 0$, $\partial S_i/\partial x_i < 0$, and $\partial^2 S_i/\partial x_i^2 > 0$; the subscript *i* denotes goods (*i*=1,2). The function S_j represents the moral cost or benefit of the individual for good *i*, and the moral benefit is higher the less the individual consumes. For simplicity, we assume that the moral cost/benefit function depends only on the difference between one's own consumption and the average consumption in society, i.e., $x_i - \overline{x}_i$. That is to say, $S_i > 0$ if $x_i < \overline{x}_i$, and $S_i < 0$ if $x_i > \overline{x}_i$. The extent of concern for moral cost/benefit for good *i* is given by the parameter τ_i, where $\tau_i \geq 0$. Thus, the individual does not care about the externality per se, but cares only about how one's own contribution to the externality is related to others' contribution to the externality.

To begin with, let us look at the decision problem without any cognitive dissonance. The individual maximizes utility subject to the budget constraint $M = p_1 x_1 + p_2 x_2$, where p_i is the price of good *i*, and M is income. We do not see this as a game theoretic problem; instead, the individual takes the behavior of others as given, unaffected by the individual's own behavior. First order conditions are therefore:

$$\frac{\partial u_i(x_i)}{\partial x_i} + \tau_i \frac{\partial S_i(x_i, \overline{x}_i)}{\partial x_i} - \lambda p_i = 0; i = 1,2., \quad (2)$$

where λ is the Lagrange multiplier of the budget constraint. The moral concern, therefore, provides an incentive for the individual to reduce consumption. Note that, by assumption, there is no correlation in consumption between the two goods.[1] If we differentiate the indirect utility, V, function w.r.t. to τ_i we have by the envelope theorem that $\frac{dV}{d\tau_i} = S(x_i^*, \overline{x}_i)$, where x_i^* is the optimal consumption of good *i* in equilibrium. Thus, whether individual *j*'s maximized utility increases or decreases when τ_i increases depends on the value of the moral concern function. That is to say, indirect utility increases if $x_i < \overline{x}_i$, and decreases if $x_i > \overline{x}_i$.

We now introduce the possibility of cognitive dissonance. Our model is in line with the model in Oxoby (2003, 2004); for other applications in economics, see, e.g., Akerlof and Dickens (1982) and Rabin (1994). The individual has the possibility of assigning a weight, α, to the

[1] We want to emphasize that "no correlation in consumption" means that the utility from consuming one good is not affected by consuming a higher or lower amount of the other good. This does not rule out the possibility that both goods can be linked to some extent by external factors that are not directly linked to the utility maximization problem.

moral functions, where $\alpha \in [0,1]$. This weight is a measure of the extent of dissonance reduction (i.e., the effort the individual is willing to make in order to minimize the disutility experienced by holding contradictory beliefs regarding the consumption of both goods). We assume that changing toward being concerned with the moral consequences of the consumption is associated with a utility cost of $B > 0$. Because changing α is costly, an individual's choice of α can be seen as putting her into one of two regimes. Thus, an individual will choose either $\alpha = 0$ or $\alpha = 1$. The important assumption here is that this weight is the same for both moral functions, i.e., we assume that, if the individual *j* believes that moral concern is important for one good, it will be important for the other good as well. However, the marginal utility of moral concern can still be different for the two goods. The maximization problem then becomes:

$$Max\, U(x_1, x_2, \alpha) = u_1(x_1) + u_2(x_2) + \alpha[\tau_1 S_1(x_1, \bar{x}_1) + \tau_2 S_2(x_2, \bar{x}_2)] - \alpha B, \tag{3}$$

subject to the budget constraint $M = p_1 x_1 + p_2 x_2$. The first order conditions are:

$$\frac{\partial u_i(x_i)}{\partial x_i} + \tau_i \frac{\partial S_i(x_i, \bar{x}_i)}{\partial x_i} - \lambda p_i = 0 \text{ if } \alpha = 1; i = 1,2. \tag{4}$$

$$\frac{\partial u_i(x_i)}{\partial x_i} - \lambda p_i = 0 \text{ if } \alpha = 0; i = 1,2. \tag{5}$$

Next, we focus on exploring what would happen if the marginal utility of moral costs/benefits exogenously changes for one of the goods, i.e., if τ_i changes. Our assumption is that an intervention of providing information about a household's consumption (in particular, its consumption in relation to other households in its reference group) potentially affects the importance that an individual attaches to moral concerns. The intervention could affect an individual's utility through two channels. First, when an individual receives information about her household's consumption in relation to others, she gains new information about her status as a good environmental citizen. An individual might not attach as much importance to environmental behavior relative to others if she does not know what she and others are actually doing; with the intervention, she gains more information and thus potentially assign more weight to the moral concerns related to good *i*. The second channel through which the intervention could affect utility is related to cognitive dissonance. Because of the intervention, the individual is confronted with new information that could conflict with her existing beliefs regarding the consumption of both goods; therefore, the importance of moral concerns for one good could be transferred to the other good.

Let us denote the indirect utility function by $V_\lambda(x_1, x_2, \bar{x}_1, \bar{x}_2, \alpha)$. Furthermore, let us write the optimal levels of x_i for a given level of τ_i and α as $x_i^*(\tau_i, \alpha)$. To begin with, let us look at the value of τ_1,$\hat{\tau}_1$, where the individual is indifferent between $\alpha = 1$ and $\alpha = 0$. This is given by

$$u_1(x_1^*(\hat{\tau}_1, 1)) + u_2(x_2^*(\hat{\tau}_1, 1)) + \hat{\tau}_1 S_1(x_1^*(\hat{\tau}_1, 1), \bar{x}_1) + \tau_2 S_2(x_2^*(\hat{\tau}_1, 1), \bar{x}_2) - B = u(x_1^*(\hat{\tau}_1, 0)) + u(x_2^*(\hat{\tau}_1, 0)), \tag{6}$$

which can be written as:

$$u_1\left(x_1^*(\hat{\tau}_1, 0)\right) - u_1\left(\left(x_1^*(\hat{\tau}_1, 1)\right) + u_2\left(x_2^*(\hat{\tau}_1, 0)\right) - u_2\left(x_2^*(\hat{\tau}_1, 1)\right) = \hat{\tau}_1 S_1(x_1^*(\hat{\tau}_1, 1), \bar{x}_1) + \tau_2 S_2(x_2^*(\hat{\tau}_1, 1), \bar{x}_2) - B \tag{7}$$

Thus, an individual with consumption above the average for both goods will never be indifferent between $\alpha = 0$ and α =1, instead $\alpha = 0$ will always be preferred. This because $u_1\left(x_i^*(\hat{\tau}_1, 0)\right) > u_1\left(x_i^*(\hat{\tau}_1, 1)\right)$ and the right hand side is negative because $S_i < 0$ if $x_i > \bar{x}_i$. This also means that the optimal consumption of at least one good has to be sufficiently lower than the average consumption for the individual to be indifferent between α = 0 and α =1.

How will the choices of the goods change if τ_1 increases? This will depend on the value of α, and on whether the optimal consumption of x_1 was lower or higher than the average consumption before the intervention. We are particularly interested in uncovering the circumstances under which there is a spillover effect of the change in τ_1 on the optimal consumption of x_2 beyond a standard marginal substitution effect between x_1 and x_2.[2] Let τ'_1 denote the moral concern for good 1 as a result of the intervention ($\tau'_1 > \tau_1$). We can identify four exhaustive cases.

Case 1 ($\mathbf{S(x_1, \bar{x}_1) > 0}$, $\boldsymbol{\alpha}$ = 1): If an individual with $\tau'_1 > \tau_1$ prefers $\alpha = 1$ over $\alpha = 0$, and if $S_1(x_1, \bar{x}_1) > 0$ (i.e., if the person consumes below average in optimum), then she also prefers

[2] As we discuss in section 3, water and electricity use is indeed correlated. However, this is primarily due to the fact that the use of both goods is correlated with household size and income. The average expenditure shares of water and electricity in Colombia are 2.4% and 5%, respectively (Komives et al. 2005). This suggests that the income effect of a change in consumption will not be large.

$\alpha = 1$ for all $\tau''_1 > \tau'_1$. This follows from $\frac{dV_{\alpha=1}}{d\tau_i} = S_1(x_i, \bar{x}_i) > 0$. This means that the consumption of x_1 decreases when τ_1 increases. Consumption of x_2 should thus increase due to a standard substitution effect, but this is presumably small, and not what we mean by a spillover effect. Because the individual was already concerned with the moral consequences, and thus considered this when making the decision of x_2, there is no spillover effect in the sense of changes in moral concerns.

Case 2 ($S(x_1, \bar{x}_1) < 0, \alpha = 1$): In this case, the effect of an increase in τ_1 depends on the relative importance of moral benefits from x_2 and moral costs from x_1. If the utility of moral benefit from x_2 dominates, α will still be 1, and the consumption of x_1 will decrease. However, if avoiding moral costs from x_1 dominates, the individual will exert cognitive dissonance and α will change to zero. Because there is no longer moral concerns, the consumption of both x_1 and x_2 will increase. Thus, in this case, there is a negative spillover effect on the consumption of x_2 (increased consumption) due to the increase in τ_1.

Case 3 ($S(x_1, \bar{x}_1) < 0, \alpha = 0$): In this case, avoiding moral costs from x_1 is not important. An individual will exert cognitive dissonance by ignoring or denying any information that conflicts with existing beliefs. Thus, there is no change in behavior because α will still be zero.

Case 4 ($S(x_1, \bar{x}_1) > 0, \alpha = 0$): In this case, the effect of an increase in τ_1 depends on the relative importance of moral benefits from x_1 and moral costs from x_2. If the positive effect on utility from of x_1 dominates, α will still be 0, and there will be no change in the consumption of x_1. However, if avoiding moral costs from x_2 dominates, α will change to one, moral concerns for both goods will influence the decision, and there will be a decrease in consumption of both goods. Thus, there is a positive spillover effect on the consumption of x_2 (decreased consumption) due to the increase in τ_1. However, if avoiding negative moral concerns from x_2 is not a problem, α will still be 0, and there will be no change in the consumption of x_1.

Thus, what we have illustrated with the model is, first how cognitive dissonance concerning moral can affect the optimal consumption in a simple two-good model. More importantly, we have shown how cognitive dissonance can result in a spillover effect between the two goods.

There are two interesting instances in which this can occur. The first is when there is a negative spillover, which arises because an individual who is initially morally concerned consumes above the average for good x_1. Because of the increase in perceived importance of moral costs related to consuming good x_1, the individual experiences a negative utility from and exert cognitive dissonance by reducing the influence of moral concerns on his decisions, resulting in an increase in consumption of both goods.

The second instance results in a positive spillover because the individual is consuming below the average for good x_1, but the consumption of good x_2 is higher than the average because he initially is not concerned with the moral consequences. However, because of the increase in perceived importance on moral concerns related to consumption of good x_1, the individual decides to reduce cognitive dissonance, and as a result the moral concern about the impact of both goods will influence her decisions. Consumption will then decrease for both goods.

3. Experimental design

3.1 Description of the sample

The experiment was conducted in the town of Jericó, in Colombia. In this town, there were 2558 registered residential accounts with the local water utility. Of these households, 1311 participated in the experiment: 656 in the treatment group and 655 in the control group. Of the households that participated in the experimental study, we obtained records of monthly electricity consumption for 1012 households from the local electricity utility (502 receiving treatment and 510 in the control group).[3] Information on households' characteristics before and after the experiment was collected through a two-wave survey. The *ex-ante* survey took place in December 2012 and collected information regarding socio-economics, water and energy saving facilities, behavioral actions towards water/energy conservation, personal values and perceptions regarding water conservation, social norms, and social networks. The ex-post survey took place in April 2014, and included the same set of questions, but added some follow-up questions regarding the information campaign.

[3] Water and electricity are provided by two different utilities in Jericó. We obtained information about all electricity accounts, but we could only match 77.2% of these with the corresponding water account. To compare the characteristics of the observations included in the analysis with the ones we had to drop, we used two procedures: the standard difference in means and normalized differences. The latter is a scale-free measure of the difference in the distributions, suggested by Imbens and Wooldridge (2009). Although we find statistically significant differences in a few cases, the normalized differences are very small compared to the threshold value of 0.25. This suggests that there are no significant differences between the distributions of both groups that could affect the validity of our estimates. Results are available upon request from the authors.

3.2 The information campaign

The households participating in the experiment were randomly allocated between treatment and control groups. The treatment group received personalized consumption reports in connection with the monthly water bill, while the control group did not receive any reports. The reports were sent out every month, starting in January 2013, and ending in January 2014. Following Allcott (2011), the consumption reports had three main components. The first component contained information about water use, and households were compared to the mean and the 25th percentile of their comparison group.[4] In addition, they were provided with an injunctive categorization regarding their consumption level compared with other households: "Excellent", "Average" or "Room to Improve". These categories correspond to efficient, intermediate and inefficient use of water in the current month. The second component contained information about the environmental implications of being part of a specific category. Furthermore, they were provided information regarding the number of households that joined the most efficient group, also in the current month. Finally, the third component included an option for households to ask to stop receiving consumption feedback. Figure 1 provides an example of a consumption report. Further details regarding the experimental design can be found in Jaime (2015).

[Figure 1 here]

3.3 Baseline characteristics

In order to monitor water and electricity use throughout the year, the local water and electricity utilities gave us access to monthly consumption data from July 2012. Because consumption reports were first sent in January 2013, the months preceding the experiment are considered pre-treatment, while the period of February 2013 onward is regarded as post-treatment. Table 1 presents the average pre- and post-treatment water and electricity use for the treatment and control groups. As expected, there are no statistically significant differences between treatment and control households in terms of water use and electricity use before the start of the campaign (t-tests; *p*-values are 0.814 and 0.698, respectively). Furthermore, there

[4] The comparison groups were defined as "households with similar characteristics". In order to capture this, we used information on the household size and age composition of household members to compute adult equivalent units. Based on this, we divided households into three groups: small, medium and large households.

are no statistically significant differences with respect to household characteristics between the treatment and control group.

[Table 1 here]

Because both water and electricity use depends on household size, they are correlated with each other (the correlation coefficient is 0.34). However, we do not expect reductions or increases in water use to be directly related to electricity use to any large extent. First, due to the absence of seasonal variation in Colombia, there is no need for household heating. Second, the only appliance that directly links water and electricity use is the washing machine. Drying machines are not used, and most showers work with gas. The primary actions taken by households to reduce water use were: closing taps while brushing teeth, washing dishes and taking showers; watering the garden and plants at night; reusing water; and placing an object in the toilet tank. In contrast, to reduce electricity use, the primary actions taken by households were turning off lights and appliances when they are not in use, and unplugging appliances when leaving the house. A summary of the household's water and energy infrastructure at home, and the actions undertaken in order to save water and electricity, are shown in Tables A1-A2.

Table 2 presents households' characteristics and attitudes prior to the campaign for the subgroup of households with efficient, intermediate and inefficient use of water. Households were classified in either category based on the injunctive classification they were given in the first report (i.e., the only period in which water use could not have been affected by the campaign). Because water use is highly correlated across time (the piece-wise correlation is 0.83), households belonging to a given category most likely exhibited similar water use in the past. There are a few socio-economic characteristics that are different across the three groups. In particular, households with inefficient use of water have older household heads and live in larger houses. When it comes to motivations for saving water, however, there are essentially no observable differences between the three groups.[5]

[Table 2 here]

[5] Out of twelve comparisons, we find a statistically significant difference in only one case. This could very well be due to chance. If we would make a simple Bonferroni-correction (Benjamini and Hochberg, 1995) for multiple comparisons by multiplying the observed *p*-values with the number of comparisons, we would no longer observe any statistically significant difference in motivations for water savings across the three groups.

4. Empirical strategy

4.1 Homogeneous treatment effects

To begin with, we investigate whether the water use information campaign has an overall effect on electricity use of the households in our sample. Following Jaime (2015), we start by estimating the following difference-in-differences model:

$$y_{jt} = \delta T_j P_{jt} + \beta P_{jt} + \mu_t + v_j + \varepsilon_{jt}, \tag{8}$$

where y_{jt} denotes household *j*'s electricity use in period *t*, T_j is a treatment group indicator, P_{jt} is a post-treatment indicator, μ_t denotes month-by-year dummy variables, v_{jt} are household fixed effects, and ε_{jt} is the error term. Due to randomisation, the direct effect of the campaign is estimated the parameter δ. A negative estimate of δ would indicate that electricity use in treated households is lower than in the control households after the information campaign, and there would thus be a positive spillover effect of the information campaign. This equation is estimated by using the standard fixed effects estimator (OLS) and standard errors are clustered at the household level.

4.2 Heterogeneous treatment effects

We next investigate whether there are spillover effects for particular groups of households. To begin with, we focus on the households with different levels of water use. We do this in two ways.

First, we investigate whether there are differences in electricity use between households that had efficient and inefficient use of water before the start of the campaign. We do so for two reasons. First, according to Jaime (2015), the effect of the information campaign on water use was primarily among households that were high users of water prior to the campaign (i.e., households whose water use exceeded the 50^{th} percentile). Second, the simple theoretical model in Section 2 suggests that we could potentially observe a positive spillover effect on electricity usage in households that were at the onset efficient users of water (low use of water compared with others), and a negative spillover effect on electricity usage in households that were initially inefficient users of water (high use of water compared with others). For households that were efficient users of water before the campaign, our model predicts either no spillover (Case 1) or potentially a positive spillover (Case 4). For households that were inefficient users of water before the campaign, our model predicts either no spillover (Case 3) or potentially a negative spillover (Case 2).

We account for differences in pre-treatment water usage by dividing the households into three categories: efficient use, intermediate use, and inefficient use of water compared with their reference group. As previously mentioned, this corresponds to the injunctive categorization that households were given in the first report.[6] For each of these three groups, we estimate treatment effects of the information campaign on water and electricity use, respectively. This is done by estimating Equation (1) for each category, both for water and electricity.

As presented in the conceptual framework, a spillover effect explained by cognitive dissonance – positive or negative – will depend on the consumption of water and electricity before the campaign. Investigating the impact of cognitive dissonance on consumption requires comparing households that were efficient/inefficient in terms of both water and electricity use before the start of the campaign. In order to do so, we compute the injunctive classification that households would have received if they had been provided with feedback on electricity use. This allows us to divide households into three categories: efficient use, intermediate use and inefficient use of electricity compared with their reference group. We focus on households with efficient and inefficient use of water and/or electricity, because the effect for intermediate use is undetermined. We classify households into four exclusive cases: (1) inefficient water and electricity use, (2) inefficient water use and efficient electricity use, (3) efficient water use and inefficient electricity use and (4) efficient water and electricity use. For each of these groups, we estimate treatment effects of the information campaign for water and electricity use, respectively.

Finally, we also address heterogeneous treatment effects by distinguishing between households that increased or decreased their water use after the information campaign. A change in water use is evidence of a change in the household's moral concern, which is the main mechanism giving rise to spillovers based on our conceptual framework. We therefore compute the individual treatment effects on water use. This allows us to separate households

[6] For households in the treatment group, we use the injunctive classification they were given in the first report. Similarly, we use the injunctive classification that households in the control group would have received if they had been treated.

that decreased water use from those that increased water use or retained the same level. We then group treated households that decreased/increased water use with control households with the same water usage prior to the campaign.[7] We then estimate treatment effects of the campaign on electricity use for each of the six groups by means of the specification in equation (1).

5. Results

5.1 Homogenous treatment effects

To begin with, we investigate the average treatment effect on both water and electricity use. We estimate models for four time periods: 3, 6, 9 and 11 months after the campaign started. Estimates are based on the sample of households whose meters worked throughout the study period.[8] Focusing on this particular sample allows us to control for meter malfunctions that are unintended (e.g., leakages and stopped and reversed meters) and intended (e.g., covered meters that cannot be read), and also increases the reliability of our estimates. Results are presented in table 3.

[Table 3 here]

As reported in Jaime (2015), the information campaign had an overall effect on water use, and this effect lasted throughout the whole campaign. The average treatment effect on water use corresponds to about a 6.2% reduction in water use.[9] However, at the aggregate level, there is no indication of a positive or negative spillover effect on electricity use from the information campaign.[10] Thus, at the aggregate level, we do not find support for a positive or negative spillover effect.

[7] Households that did not change water use were excluded from the analysis. Although it would have been interesting to include them as a separate group, there were only four treated households that did not change their behavior, which makes it impossible to conduct any formal analysis.

[8] The water utility provided us with monthly information regarding meter's performance. This allowed us to identify meters that have always worked from those with permanent or temporal failure.

[9] Note that the average treatment effect on water use in this paper is not the same as the one in Jaime (2015) because here we only include a subset of all households: those households for which we also have information on their electricity use.

[10] The results are similar if we include all observations, i.e., even those without well-functioning meters. The treatment effect on water use is then around 6%, and there is still no evidence of spillover effects on electricity use. According to Jaime (2015), during April 2013, a well-defined shock affecting water use took place in Jericó. Results are also robust to the exclusion of this particular month.

5.2 Heterogeneous treatment effects

Let us now look at the treatment effects for the three categories of households based on their water use - compared with their reference group - before the treatment. The results are presented in Table 4 and Figure 2.

[Table 4 and Figure 2 here]

Let us begin with the differences in water use between treatment and control groups. Among the households with inefficient use of water before the information campaign, there is a sizeable and statistically significant difference between treatment and control groups: water use is 8.3% lower in the treatment group eleven months after the start of the campaign. Although the difference is slightly higher in the beginning of the time period, it remains large throughout the whole year, as shown in the lower panel in Figure 2. For households with an intermediate level of water use before the information campaign, there is no difference between the treatment and control groups. Finally, for households with efficient use of water before the campaign, there is some evidence of a treatment effect in the long run.

What about spillover effects on electricity use? For the first two groups - inefficient and intermediate users of water - there is no statistically significant difference between treatment and control groups, apart from one case. Thus, for these two groups, we can conclude that, on average, there is no spillover effect. However, for the third group - efficient water users - the difference in electricity use between treatment and control is negative and statistically significant. The difference is sizeable as well: electricity use is 9.1% lower in the water use treatment group 11 months after the start of the campaign. Although the effect is not statistically significant in the first two months after the start of the campaign, it remains large and significant throughout the whole year, as shown in the upper panel in Figure 2.

Thus, we have some evidence of a positive spillover effect of the water use information campaign on electricity use, positive in the sense of reduced use of electricity as well, but the effect is primarily among households with a low level of water use before the information campaign began. The average treatment effect on water for this group is, as we have seen, negative as well, although statistically significant only in the long run. Thus, the results suggest that there is a correlation in behavior between the two consumption areas and not only a direct effect of the information campaign per se.[11] The findings are consistent with our model with moral costs/benefits of consumption and cognitive dissonance. This model predicts that positive spillover will occur among households that have a low level of water consumption before the information campaign. However, our model only predicts a potential positive spillover effect among households with a low level of water consumption and a high level of electricity consumption. In order to test this, we next classify households on both water and electricity use before the campaign. Results are presented in Table 5 and Figure 3.

[Table 5 and Figure 3 here]

Let us begin with households with efficient water use before the campaign. For households that had inefficient electricity use, we find evidence of a positive spillover: treated households decrease electricity use by around 20% compared with control households. The effect is statistically insignificant in the first months after the start of the campaign, becoming significant after four months and remaining steady throughout the year. The positive spillover is in line with the predictions of our theoretical model: before the campaign, households were not concerned about the moral consequences of their consumption and had inefficient use of electricity. Because of the change in concern for moral costs/benefits, the households have an incentive to reduce electricity consumption. The effect of the campaign on water use is also negative although statistically insignificant. The evolution of the treatment effects throughout the campaign is presented in the mid-lower panel in Figure 3.

What about households with both efficient water and electricity usage? There is some evidence of a positive spillover for this type of household as well. The difference in electricity use between treatment and control is negative and statistically significant during the first three months after the start of the campaign. However, the difference then disappears. Note that households in this group reduce water use by about 15%. The effect on water use is negative but insignificant at the start of the campaign, but after that it is statistically significant. This group of households were efficient in both domains, but still reduced their use in both domains. The spillover effect is potentially due to a wish to be consistent in changes in behavior. Thus if a household reduces consumption in one area with a moral concern, it is likely to lead to changes in consumption in other areas with a similar moral concern.

[11] Results are robust to both the inclusion of all observations and the exclusion of April 2013.

Let us now look at the treatment effects for the subsample of households with both inefficient water use and electricity use before the start of the campaign. As can be seen in the left-upper panel in Table 6, there are no statistically significant differences in water and electricity use. This is consistent with our predictions from the model: households that are inefficient in both domains do not attach importance to moral costs/benefits of the consumption, and therefore disregard the information provided by the campaign.

Finally, we look at the treatment effects for the subsample of households with inefficient water use and efficient electricity use before the start of the campaign. Results are displayed in the right-upper panel in Table 6. In this group, there is no difference in water use between treatment and control, while electricity use decreases in the treatment group. The treatment effect is, however, only statistically significant from the fourth month onward, as shown in the middle-upper panel in Figure 3. These results are not consistent with our theoretical model, which predicts either no spillovers or a negative spillover for this group of households.

An alternative way to classify the households is based on whether they increased or decreased their use of water because of the information campaign. As explained in section 4, we compare treated households that increased/decreased water use with control households with similar water usage before the information campaign. In Table 6, the treatment effects on electricity use are estimated for the six different groups of households.

[Table 6 here]

First of all, there are no differences between treatment and control groups for households with inefficient use of water before the campaign, even when we allow for differences between those that increased and those that decreased their water use in the treatment group. By contrast, households that had intermediate use of water before the campaign exhibit important differences: while treated households that decreased their water use did not change electricity use compared with the control group, those that increased water use also increased electricity use by around 11.9%, compared with the control group. Nevertheless, the effect is only statistically significant at the end of the treatment.

The most interesting case is, of course, the households that already had efficient use of water before the campaign. Among these, the households that managed to further decrease water use due to the information campaign are most different from the control group: households in the treatment group that decreased water use after the campaign also decreased electricity use by 14.9% compared with the control group. Households in the treatment group that increased their water use during the campaign did not have a change in their use of electricity that was different from the control group.[12] Overall, these results suggest the existence of a positive spillover in the group of efficient households and some evidence of correlation in behavior among households that increased water usage in response to the information campaign.

6. Robustness checks

In this section, we estimate a set of models for households with an intermediate use of water and electricity. Furthermore, we investigate how sensitive our main results are to the classification of efficient and inefficient households.

6.1 Households with intermediate water and electricity use

The estimates in Tables 4 and 5 suggest that there could be a negative spillover effect on electricity use for this group of households. In order to examine this in more detail, we estimate treatment effects of the campaign on both water and electricity use when households are also classified based on electricity use before the campaign. Results are presented in Table A3. We find no evidence of a negative spillover effect.

6.2 Redefining the sample of efficient and inefficient households

During the campaign, households whose water use was higher than the 25th percentile but lower than the mean were categorized as “intermediate”. Households with water use below/above these cut-offs were categorized as efficient/inefficient. Although previous studies have used similar classification (Ayres et al, 2013; Allcott, 2011), the choice of the cut offs could be somewhat arbitrary. In order to investigate whether our results are sensitive to this classification, we estimate two additional sets of models. In the first set, households with water/electricity use lower than the 30th percentile are classified as efficient. Similarly, households with water/electricity use higher than the 70th percentile are classified as inefficient. In the second set, we use the percentiles 25th and 75th as cut-offs instead. We then estimate the treatment effects of the campaign in Table 5 using these two sets instead. Results are summarized in Tables A4-A6. As can be seen, results remain basically the same.

[12] Except for a few cases, results remain the same when including all observations and excluding April 2013.

7. Discussion

Does targeted social information in one area affect behavior in other areas? Are individuals who are affected by such information more or less likely to change their behavior in other areas not directly related to the information provided? These are the two broad questions we have addressed in this paper. There are reasons to expect that there is indeed a positive spillover from one area to another. In particular, we argue that a correlation between behaviors might not only be due to underlying differences but also due to cognitive dissonance, i.e., individuals strive to be consistent. What we find is some evidence of a positive spillover effect of the social information campaign, but only for a particular group of individuals. The information campaign on water use decreased water use for two groups of households: those with inefficient use of water before the information campaign and those with efficient use before the campaign. However, it is only for the households with efficient use of water that we observe a positive spillover effect on electricity. The effect is sizeable; this group has around 9% lower use of electricity compared with the control group 11 months into the information campaign. Interestingly, there are no observable differences between efficient and inefficient users of water with respect to stated reasons for saving water or regarding their perceptions of water scarcity. Thus, these cannot be the explanations for the difference in spillover effect of the campaign.

The types of households where we observe a positive spillover effect are those that, before the campaign, already had both efficient use of water and inefficient use of electricity. As for the households with efficient water and electricity use, we find a reinforcement effect of the campaign in the primary area. These findings are consistent with our model of moral concern and cognitive dissonance. This model predicts that a positive spillover effect is to be expected among households that, before the information campaign, already had an efficient level of water use. Among these households, the information campaign might have triggered an increased concern about moral and a desire to reduce dissonance by decreasing consumption of both water and electricity.

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List of tables

Table 1. Water and electricity use by comparison groups (m^3/month and kWh/month)

	Pre-treatment				*Post-treatment*			
	Water		Electricity		Water		Electricity	
	Treat.	Control	Treat.	Control	Treat.	Control	Treat.	Control
All households	14.67	14.54	119.88	121.74	13.78	14.50	118.68	121.13
	(9.18)	(9.28)	(71.59)	(80.13)	(8.56)	(9.32)	(73.62)	(81.21)
Small households	11.52	9.16	97.91	88.27	10.66	9.67	97.43	89.23
	(10.76)	(7.74)	(64.40)	(64.45)	(8.68)	(7.91)	(62.22)	(64.69)
Medium households	14.82	15.37	122.30	127.57	14.04	15.19	120.88	125.90
	(9.36)	(10.45)	(71.60)	(80.08)	(9.32)	(10.01)	(75.59)	(78.41)
Large households	21.92	21.51	153.23	158.84	19.55	21.35	152.55	164.24
	(13.72)	(11.38)	(115.04)	(129.53)	(11.12)	(13.79)	(129.86)	(149.75)

Source: Own calculations based on data from water and electricity utilities and *ex-ante* data. Pre-treatment corresponds to the period Jul. 2012 - Jan. 2013. Post-treatment corresponds to the period Feb. 2013 - Dec. 2013. Standard deviations in parentheses.

Table 2. Socio-economic characteristics and attitudes and perceptions regarding water scarcity

	Efficient users	Intermediate users	Inefficient users	*p-value* (eff-int)	*p-value* (eff-ineff)	*p-value* (ineff-eff)
			Socio-economics and dwelling characteristics			
Gender [*Household head*]	0.231	0.284	0.245	0.218	0.716	0.289
	(0.423)	(0.452)	(0.431)			
Age [*Household head*]	50.38	50.70	54.08	0.843	0.014	0.011
	(16.65)	(16.33)	(15.67)			
Education [*No. years – Househ. head*]	7.83	8.29	7.99	0.312	0.727	0.440
	(4.89)	(4.47)	(4.88)			
Household size [*No. family members*]	3.34	3.33	3.31	0.956	0.857	0.891
	(1.87)	(1.82)	(1.72)			
Household income [*1000 COP/Month*]	494	520	509	0.637	0.785	0.834
	(560)	(547)	(621)			
No. rooms	7.11	7.39	7.76	0.203	0.003	0.051
	(2.31)	(2.20)	(2.37)			
House age [*No. years*]	29.90	27.53	30.68	0.138	0.617	0.019
	(16.80)	(15.98)	(16.50)			
			Motivations for saving water			
Civic duty [%]	0.243	0.236	0.235	0.865	0.843	0.982
	(0.429)	(0.424)	(0.424)			
Important [%]	0.480	0.449	0.440	0.524	0.392	0.828
	(0.500)	(0.497)	(0.496)			
Pay less [%]	0.179	0.224	0.262	0.255	0.037	0.289
	(0.384)	(0.417)	(0.440)			
Social esteem [%]	0.069	0.057	0.057	0.599	0.588	0.992
	(0.254)	(0.232)	(0.232)			
			Perceptions regarding water scarcity			
Water scarcity [*At present - %*]	0.071	0.031	0.082	0.054	0.671	0.009
	(0.258)	(0.174)	(0.274)			
Water scarcity [*Future - %*]	0.763	0.741	0.754	0.602	0.813	0.725
	(0.425)	(0.438)	(0.431)			
			Water and electricity use			
Keep track water use [%]	0.497	0.536	0.542	0.426	0.336	0.882
	(0.501)	(0.500)	(0.499)			
Keep track electricity use [%]	0.503	0.525	0.530	0.657	0.500	0.896
	(0.501)	(0.500)	(0.5000)			
Keep track water and electricity [%]	0.486	0.525	0.527	0.424	0.376	0.954
	(0.501)	(0.500)	(0.500)			
No of obs.	173	263	332			

Note: Figures are based on the *ex-ante* survey and correspond to the subsample of households with continually working meters. Standard deviations in parentheses.

Table 3. Homogeneous treatment effects on water and electricity use

VARIABLES	*Water use* After 3 months	After 6 months	After 9 months	After 11 months	*Electricity use* After 3 months	After 6 months	After 9 months	After 11 months
Post × Treated	-5.694***	-4.898**	-5.914***	-6.157***	-0.613	0.446	0.329	0.264
	(2.139)	(2.114)	(2.126)	(2.126)	(1.802)	(1.889)	(2.030)	(2.098)
Post-treatment	-0.110	-0.542	-17.81***	-15.13***	-4.316***	-4.861***	2.368	-5.519***
	(2.112)	(2.143)	(2.202)	(1.948)	(1.482)	(1.538)	(1.788)	(1.909)
Constant	118.7***	119.9***	118.5***	118.2***	103.4***	103.5***	103.1***	103.6***
	(1.322)	(1.372)	(1.398)	(1.411)	(0.702)	(0.747)	(0.820)	(0.873)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	7,680	9,984	12,288	13,824	7,680	9,984	12,288	13,824
No. Households	768	768	768	768	768	768	768	768
R-squared	0.073	0.060	0.050	0.046	0.022	0.019	0.016	0.015

Note: This table shows estimates of the baseline specification capturing the homogeneous effects of the campaign for the subsample of households whose meters worked throughout the study period. Estimates correspond to the period Jul. 2012 - Dec. 2013. Cluster standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 4. Treatment effects on water use and electricity use for households classified according to water use before information campaign

A. Households with inefficient use of water ("Room to improve")

VARIABLES	*Water use*				*Electricity use*			
	After 3 months	After 6 months	After 9 months	After 11 months	After 3 months	After 6 months	After 9 months	After 11 months
Post × Treated	-9.708**	-8.318**	-8.836**	-8.349**	2.227	1.911	1.382	2.000
	(3.779)	(3.851)	(3.895)	(3.867)	(3.233)	(3.092)	(3.169)	(3.265)
Post-treatment	-12.47***	-33.34***	-41.43***	-42.62***	-7.679***	-2.504	0.930	5.272**
	(3.894)	(4.209)	(3.824)	(3.424)	(2.431)	(2.758)	(2.774)	(2.594)
Constant	178.9***	180.6***	178.5***	178.1***	119.2***	119.4***	118.9***	119.4***
	(2.350)	(2.454)	(2.505)	(2.521)	(1.175)	(1.208)	(1.279)	(1.351)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	3,320	4,316	5,312	5,976	3,320	4,316	5,312	5,976
No. Households	332	332	332	332	332	332	332	332
R-squared	0.141	0.113	0.095	0.088	0.028	0.027	0.023	0.024

B. Households with intermediate use of water ("Average")

VARIABLES	*Water use*				*Electricity use*			
	After 3 months	After 6 months	After 9 months	After 11 months	After 3 months	After 6 months	After 9 months	After 11 months
Post × Treated	-0.272	-0.604	-1.855	-2.452	1.363	4.760*	4.792	4.230
	(2.929)	(2.825)	(2.816)	(2.839)	(2.296)	(2.755)	(3.157)	(3.301)
Post-treatment	4.643*	-15.99***	-8.009***	-7.179***	-2.842	-5.598***	1.428	-5.508*
	(2.737)	(2.258)	(2.537)	(2.081)	(2.119)	(1.977)	(3.045)	(2.878)
Constant	89.68***	90.54***	89.48***	89.28***	95.42***	95.58***	95.17***	95.59***
	(1.292)	(1.334)	(1.330)	(1.341)	(1.133)	(1.223)	(1.375)	(1.462)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	2,630	3,419	4,208	4,734	2,630	3,419	4,208	4,734
No. Households	263	263	263	263	263	263	263	263
R-squared	0.058	0.049	0.041	0.038	0.025	0.021	0.018	0.016

C. Households with efficient use of water ("Excellent")

VARIABLES	*Water use*				*Electricity use*			
	After 3 months	After 6 months	After 9 months	After 11 months	After 3 months	After 6 months	After 9 months	After 11 months
Post × Treated	-6.631	-5.319	-7.017*	-8.168**	-8.811**	-8.820**	-8.395*	-9.089*
	(4.085)	(3.721)	(3.667)	(3.724)	(3.460)	(4.083)	(4.494)	(4.612)
Post-treatment	7.375*	3.343	12.80***	12.17***	-1.735	-4.666	6.561*	4.118
	(4.352)	(2.943)	(3.711)	(3.037)	(2.496)	(3.192)	(3.655)	(3.791)
Constant	47.45***	47.90***	47.34***	47.23***	85.07***	85.22***	84.85***	85.23***
	(1.836)	(1.790)	(1.815)	(1.841)	(1.293)	(1.475)	(1.700)	(1.837)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	1,730	2,249	2,768	3,114	1,730	2,249	2,768	3,114
No. Households	173	173	173	173	173	173	173	173
R-squared	0.047	0.037	0.036	0.039	0.041	0.031	0.024	0.022

Note: This table shows estimates of the baseline specification capturing the heterogeneous treatment effects of the campaign for the sample of households with electricity data and working meters. Estimates correspond to the period Jul. 2012 - Dec. 2013. Clustered standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5. Treatment effects on water and electricity use for households classified according to water and electricity use before the campaign

Inefficient use of water

	Inefficient water – Inefficient electricity				Inefficient water – Efficient electricity			
	Electricity use		*Water use*		*Electricity use*		*Water use*	
VARIABLES	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months
Post × Treated	4.047	3.897	-5.909	-4.122	-10.97*	-10.38*	-1.690	-3.050
	(4.750)	(4.985)	(5.962)	(5.568)	(5.592)	(5.963)	(7.979)	(8.947)
Post-treatment	-14.51***	-25.11***	-51.54***	-37.07***	21.68***	19.31***	-38.38***	-38.54***
	(3.740)	(4.736)	(4.885)	(4.688)	(5.440)	(5.959)	(9.572)	(9.186)
Constant	164.5***	164.5***	190.4***	187.8***	43.06***	43.06***	169.5***	167.2***
	(1.782)	(1.986)	(3.223)	(3.325)	(2.034)	(2.413)	(7.068)	(7.277)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	2,288	3,168	2,288	3,168	806	1,116	806	1,116
No. Households	176	176	176	176	62	62	62	62
R-squared	0.046	0.045	0.111	0.086	0.086	0.085	0.127	0.099

Efficient use of water

	Efficient water – Inefficient electricity				Efficient water – Efficient electricity			
	Electricity use		*Water use*		*Electricity use*		*Water use*	
VARIABLES	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months
Post × Treated	-17.68*	-22.13**	-5.639	-8.797	-8.817	-2.652	-14.16**	-16.37***
	(8.864)	(8.840)	(7.744)	(8.019)	(7.405)	(9.760)	(5.951)	(5.635)
Post-treatment	-3.184	-8.927	12.71*	18.10**	13.48*	16.11**	16.19***	26.41***
	(7.933)	(7.228)	(6.688)	(7.130)	(6.793)	(6.664)	(5.897)	(7.459)
Constant	151.6***	151.6***	54.12***	53.37***	38.32***	38.32***	42.20***	41.61***
	(3.365)	(3.889)	(4.549)	(4.459)	(2.082)	(3.051)	(3.118)	(3.370)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	598	828	598	828	767	1,062	767	1,062
No. Households	46	46	46	46	59	59	59	59
R-squared	0.086	0.099	0.066	0.061	0.036	0.042	0.049	0.065

Note: This table shows estimates of the baseline specification capturing the heterogeneous effects of the campaign for the subsample of households whose meters worked throughout the study period. Estimates correspond to the period Jul. 2012 - Dec. 2013. Cluster standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6. Treatment effects on electricity use for households classified according to water use before the campaign and change in water use during campaign

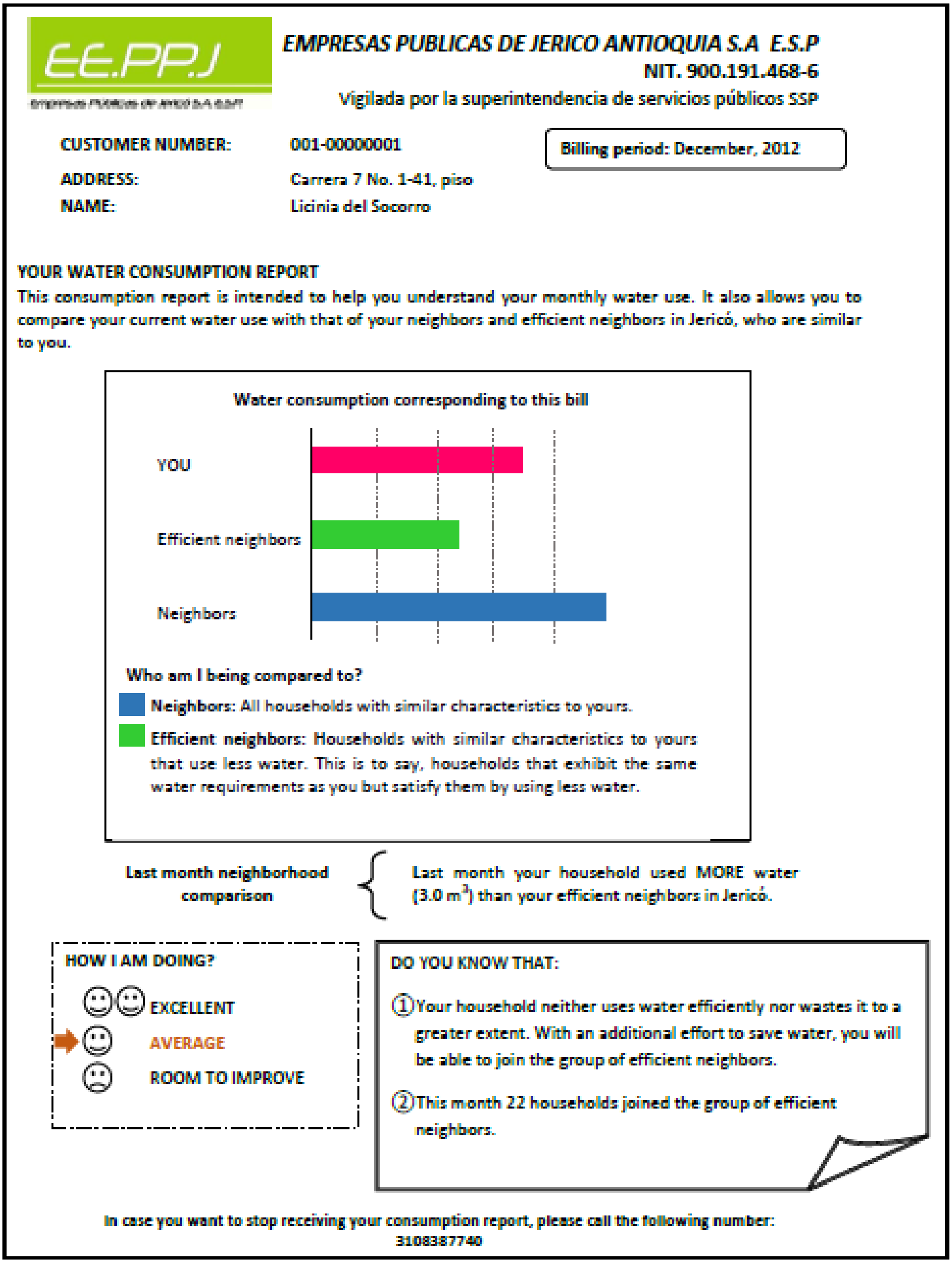
	A. Households with efficient use of water ("Excellent")							
VARIABLES	*Decreased water use*				*Increased water use*			
	After 3 months	After 6 months	After 9 months	After 11 months	After 3 months	After 6 months	After 9 months	After 11 months
Post × Treated	-13.66***	-13.26***	-13.62**	-14.82**	-1.996	-2.574	-1.049	-1.032
	(4.035)	(5.022)	(5.867)	(6.031)	(3.990)	(4.480)	(4.893)	(5.184)
Post-treatment	1.575	6.607	5.347	3.689	-1.065	3.946	7.589*	2.997
	(3.499)	(4.653)	(3.739)	(3.732)	(3.579)	(4.531)	(3.969)	(3.986)
Constant	82.39***	82.53***	82.18***	82.54***	84.66***	84.81***	84.44***	84.82***
	(1.465)	(1.683)	(1.920)	(2.055)	(1.557)	(1.782)	(1.969)	(2.117)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	1,360	1,768	2,176	2,448	1,210	1,573	1,936	2,178
No. Households	136	136	136	136	121	121	121	121
R-squared	0.057	0.040	0.032	0.031	0.032	0.032	0.035	0.034

	B. Households with intermediate use of water ("Average")							
VARIABLES	*Decreased water use*				*Increased water use*			
	After 3 months	After 6 months	After 9 months	After 11 months	After 3 months	After 6 months	After 9 months	After 11 months
Post × Treated	0.631	0.683	-0.802	-1.619	2.085	9.990***	12.12***	11.88**
	(2.776)	(2.911)	(3.123)	(3.216)	(2.963)	(3.642)	(4.551)	(4.899)
Post-treatment	-3.943*	-3.965**	1.133	-5.690*	-1.124	-5.960***	3.993	-3.922
	(2.213)	(1.875)	(3.083)	(2.903)	(2.298)	(1.976)	(3.361)	(3.179)
Constant	95.75***	95.92***	95.51***	95.93***	97.91***	98.09***	97.66***	98.10***
	(1.229)	(1.291)	(1.382)	(1.448)	(1.406)	(1.515)	(1.718)	(1.836)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	2,040	2,652	3,264	3,672	1,860	2,418	2,976	3,348
No. Households	204	204	204	204	186	186	186	186
R-squared	0.031	0.030	0.029	0.028	0.022	0.026	0.029	0.025

	A. Households with inefficient use of water ("Room to improve")							
VARIABLES	*Decreased water use*				*Increased water use*			
	After 3 months	After 6 months	After 9 months	After 11 months	After 3 months	After 6 months	After 9 months	After 11 months
Post × Treated	3.388	0.831	-0.305	-0.0737	-0.297	4.258	5.048	6.506
	(3.345)	(3.195)	(3.298)	(3.393)	(4.323)	(4.135)	(4.187)	(4.383)
Post-treatment	-8.613***	-5.164**	0.0877	-4.800*	-6.469***	-7.008***	2.344	-10.10***
	(2.374)	(2.599)	(2.824)	(2.638)	(2.427)	(2.616)	(3.039)	(3.556)
Constant	119.5***	119.7***	119.2***	119.7***	118.1***	118.3***	117.8***	118.3***
	(1.295)	(1.318)	(1.389)	(1.462)	(1.522)	(1.538)	(1.601)	(1.688)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	2,800	3,640	4,480	5,040	2,190	2,847	3,504	3,942
No. Households	280	280	280	280	219	219	219	219
R-squared	0.031	0.030	0.025	0.028	0.017	0.020	0.019	0.020

Note: This table shows estimates of the baseline specification capturing the heterogeneous treatment effects of the campaign for the sample of households with electricity data and working meters. Estimates correspond to the period Jul. 2012 - Dec. 2013. Cluster standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

List of figures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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 1. Example of a consumption report

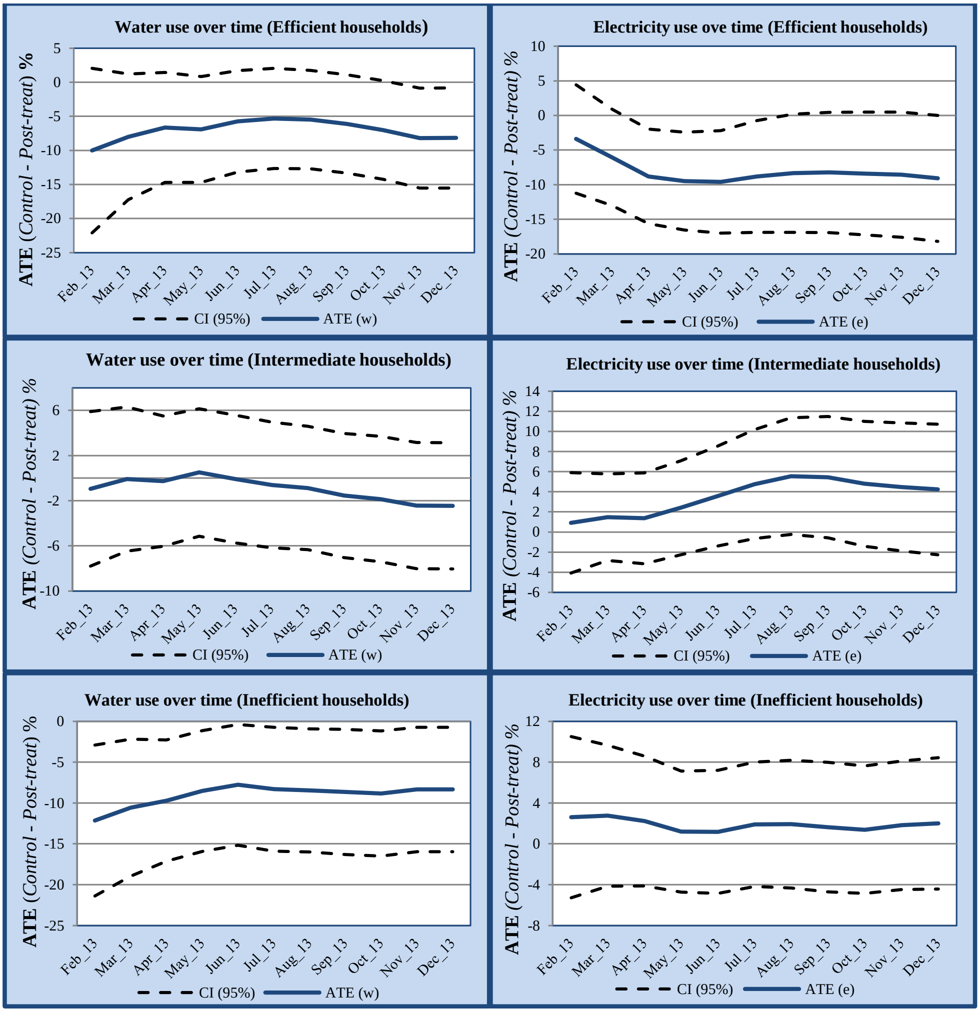

Figure 2. Average treatment effects for water and electricity use for households classified according to water use before information campaign

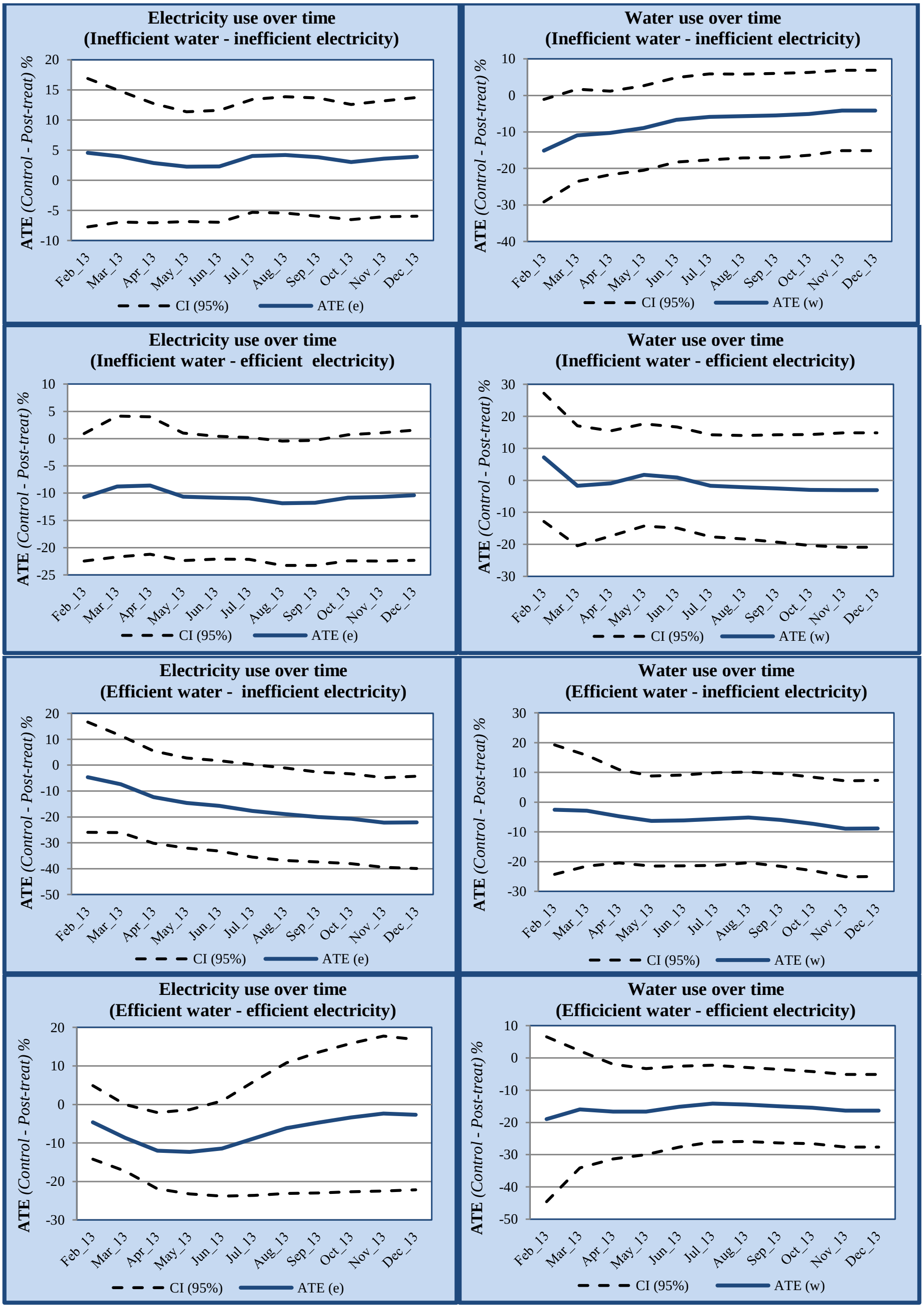

Figure 3. Average treatment effects for water and electricity use for households classified according to water and electricity use before the campaign

Appendix A. Additional tables and figures

Table A1. Water and electricity-saving infrastructure (*Share of households*)

	Treatment	Control	All
	Water-savings infrastructure		
Dual flush toilets	0.095	0.095	0.095
	(0.294)	(0.294)	(0.293)
Water-saving taps	0.072	0.084	0.078
	(0.259)	(0.278)	(0.269)
Water-saving showerheads	0.082	0.074	0.078
	(0.275)	(0.262)	(0.269)
Water-saving washing machine	0.093	0.098	0.095
	(0.290)	(0.297)	(0.293)
Rain-water collector tank	0.530	0.522	0.526
	(0.500)	(0.500)	(0.500)
	Electricity-savings infrastructure		
Other energy-saving appliances	0.097	0.108	0.102
	(0.296)	(0.311)	(0.303)
Energy-saving light bulbs	0.662	0.643	0.653
	(0.474)	(0.480)	(0.476)
No. Obs.	389	379	768

Note: Figures are based on the *ex-ante* survey and correspond to the subsample of households with working meters. Standard deviations in parentheses.

Table A2. Actions to save water and electricity (*Share of households*)

	Treatment	Control	All
	Water-savings actions		
Close the tap while brushing my teeth	0.951	0.971	0.961
	(0.216)	(0.168)	(0.194)
Close the tap while I soap up in the shower	0.951	0.955	0.953
	(0.216)	(0.207)	(0.212)
Close the tap while washing the dishes	0.964	0.966	0.965
	(0.187)	(0.182)	(0.184)
Close the tap while I do laundry	0.941	0.921	0.931
	(0.236)	(0.270)	(0.254)
Watering the garden and plants at night	0.411	0.435	0.423
	(0.493)	(0.496)	(0.494)
Reuse water and/or collecting rainwater	0.409	0.414	0.411
	(0.492)	(0.493)	(0.492)
Element in the toilet tank	0.092	0.133	0.112
	(0.315)	(0.340)	(0.328)
	Electricity-saving actions		
Turn off the light when leaving the room/house	0.969	0.966	0.967
	(0.173)	(0.182)	(0.178)
Turn off appliances when not being directly used	0.954	0.953	0.953
	(0.210)	(0.213)	(0.212)
Unplug appliances when not in use	0.848	0.844	0.846
	(0.359)	(0.363)	(0.361)
No. Obs.	389	379	768

Note: Figures are based on the *ex-ante* survey and correspond to the subsample of households with working meters. Standard deviations in parentheses.

Table A3. Treatment effects on water and electricity use for households with an intermediate use of water or electricity

Panel A. Intermediate use of water

VARIABLES	Inefficient electricity use				Intermediate electricity use				Efficient electricity use			
	Electricity use		*Water use*		*Electricity use*		*Water use*		*Electricity use*		*Water use*	
	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months
Post × Treated	6.770	4.238	3.316	1.922	2.364	0.885	-3.511	-6.291*	5.599	8.967	-4.915	-5.486
	(5.907)	(6.418)	(6.040)	(5.678)	(2.920)	(3.005)	(3.216)	(3.466)	(4.669)	(7.968)	(5.621)	(5.617)
Post-treatment	-6.785	-16.01***	-11.29**	-7.553	-0.577	-2.921	-5.190	-4.546*	15.83**	14.46**	-9.534*	0.775
	(7.056)	(5.911)	(5.041)	(5.134)	(2.299)	(2.210)	(3.258)	(2.705)	(7.150)	(5.584)	(5.202)	(8.731)
Constant	147.0***	147.0***	98.41***	97.04***	78.23***	78.23***	86.39***	85.19***	40.58***	40.59***	85.21***	84.02***
	(2.424)	(2.652)	(2.642)	(2.560)	(1.137)	(1.246)	(1.573)	(1.638)	(2.907)	(4.139)	(3.182)	(3.333)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	1,248	1,728	1,248	1,728	1,469	2,034	1,469	2,034	702	972	702	972
No. Households	96	96	96	96	113	113	113	113	54	54	54	54
R-squared	0.037	0.037	0.039	0.031	0.035	0.028	0.075	0.060	0.081	0.061	0.089	0.064

Panel B. Intermediate use of electricity

VARIABLES	Inefficient water use				Intermediate water use				Efficient water use			
	Electricity use		*Water use*		*Electricity use*		*Water use*		*Electricity use*		*Water use*	
	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months
Post × Treated	8.046	7.839	-16.76***	-18.84***	2.364	0.885	-3.511	-6.291*	-0.276	-1.408	3.462	0.866
	(4.893)	(5.242)	(5.842)	(6.564)	(2.920)	(3.005)	(3.216)	(3.466)	(6.449)	(6.126)	(5.134)	(5.151)
Post-treatment	-3.290	-0.951	-31.02***	-34.93***	-0.577	-2.921	-5.190	-4.546*	5.855	-0.412	2.950	5.715
	(4.077)	(4.364)	(7.194)	(7.227)	(2.299)	(2.210)	(3.258)	(2.705)	(7.861)	(4.418)	(3.816)	(3.921)
Constant	85.28***	85.28***	169.4***	167.0***	78.23***	78.23***	86.39***	85.19***	81.04***	81.05***	48.64***	47.97***
	(1.783)	(1.870)	(4.204)	(4.273)	(1.137)	(1.246)	(1.573)	(1.638)	(2.226)	(2.503)	(2.016)	(2.108)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	1,222	1,692	1,222	1,692	1,469	2,034	1,469	2,034	884	1,224	884	1,224
No. Households	94	94	94	94	113	113	113	113	68	68	68	68
R-squared	0.043	0.036	0.133	0.116	0.035	0.028	0.075	0.060	0.036	0.033	0.043	0.038

Note: This table shows estimates of the baseline specification capturing the heterogeneous effects of the campaign for the subsample of households whose meters worked throughout the study period. Estimates correspond to the period Jul. 2012 - Dec. 2013. Cluster standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table A4. Heterogeneous treatment effects on water and electricity for households classified according to water use before information campaign; robustness check with respect to definition of efficient and inefficient households

A. Efficient water use

VARIABLES	Percentiles 30th and 70th				Percentiles 25th and 75th			
	Water use		*Electricity use*		*Water use*		*Electricity use*	
	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months
Post × Treated	-4.278	-6.530**	-5.616*	-5.086	-5.319	-8.168**	-8.820**	-9.089*
	(2.960)	(3.047)	(3.215)	(3.809)	(3.721)	(3.724)	(4.083)	(4.612)
Post-treatment	1.927	10.47***	-4.524*	2.830	3.343	12.17***	-4.666	4.118
	(2.422)	(2.539)	(2.372)	(3.147)	(2.943)	(3.037)	(3.192)	(3.791)
Constant	50.81***	50.10***	85.75***	85.76***	47.90***	47.23***	85.22***	85.23***
	(1.469)	(1.515)	(1.278)	(1.652)	(1.790)	(1.841)	(1.475)	(1.837)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	3,042	4,212	3,042	4,212	2,249	3,114	2,249	3,114
No. Households	234	234	234	234	173	173	173	173
R-squared	0.037	0.038	0.028	0.020	0.037	0.039	0.031	0.022

Panel B. Inefficient water use

VARIABLES	Percentiles 30th and 70th				Percentiles 25th and 75th			
	Water use		*Electricity use*		*Water use*		*Electricity use*	
	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months
Post × Treated	-11.55**	-12.01***	2.076	2.082	-13.42**	-14.27***	1.207	1.433
	(4.495)	(4.576)	(3.525)	(3.735)	(5.232)	(5.221)	(4.016)	(4.273)
Post-treatment	-53.45***	-40.87***	-6.747**	-11.92***	-57.83***	-44.87***	-6.297**	-12.05***
	(4.439)	(4.188)	(2.904)	(3.635)	(5.179)	(4.908)	(3.170)	(4.161)
Constant	199.3***	196.5***	126.1***	126.1***	211.9***	209.0***	129.9***	129.9***
	(2.896)	(2.976)	(1.419)	(1.593)	(3.341)	(3.411)	(1.577)	(1.773)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	3,445	4,770	3,445	4,770	2,834	3,924	2,834	3,924
No. Households	265	265	265	265	218	218	218	218
R-squared	0.129	0.102	0.032	0.028	0.147	0.120	0.035	0.030

Note: This table shows estimates of the baseline specification capturing the heterogeneous effects of the campaign for the subsample of households whose meters worked throughout the study period. Estimates correspond to the period Jul. 2012 - Dec. 2013. Cluster standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A5. Treatment effects on water and electricity use for households classified according to water and electricity use before the campaign; robustness check percentiles 30th and 70th

	A. Inefficient use of water							
	Inefficient water – Inefficient electricity				Inefficient water – Efficient electricity			
	Electricity use		*Water use*		*Electricity use*		*Water use*	
VARIABLES	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months
Post × Treated	3.941	3.555	-4.788	-4.673	-10.43*	-10.46*	-4.426	-8.589
	(6.590)	(6.956)	(7.814)	(7.298)	(5.665)	(6.189)	(8.992)	(10.26)
Post-treatment	-15.32***	-29.61***	-36.67***	-42.13***	19.89***	21.89***	-54.51***	-50.25***
	(5.682)	(6.718)	(8.575)	(6.640)	(5.836)	(6.496)	(11.09)	(12.22)
Constant	185.2***	185.2***	208.5***	205.6***	45.97***	45.97***	196.8***	194.0***
	(2.471)	(2.787)	(4.335)	(4.492)	(2.370)	(2.647)	(7.961)	(8.257)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	1,547	2,142	1,547	2,142	663	918	663	918
No. Households	119	119	119	119	51	51	51	51
R-squared	0.050	0.047	0.123	0.095	0.074	0.076	0.166	0.135

	B. Efficient use of water							
	Efficient water – Inefficient electricity				Efficient water – Efficient electricity			
	Electricity use		*Water use*		*Electricity use*		*Water use*	
VARIABLES	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months
Post × Treated	-13.40	-15.39*	-5.709	-6.174	-4.314	1.499	-7.950*	-10.28**
	(8.635)	(9.001)	(8.413)	(8.687)	(4.956)	(7.318)	(4.378)	(4.281)
Post-treatment	9.573	-9.675	18.08*	17.29**	12.53**	13.37***	8.816**	26.04***
	(11.08)	(8.227)	(10.27)	(7.586)	(5.793)	(4.999)	(4.411)	(5.869)
Constant	165.4***	165.4***	52.67***	51.94***	43.95***	43.96***	47.13***	46.47***
	(3.394)	(3.999)	(4.110)	(3.953)	(1.889)	(2.847)	(2.411)	(2.584)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	611	846	611	846	1,248	1,728	1,248	1,728
No. Households	47	47	47	47	96	96	96	96
R-squared	0.073	0.067	0.059	0.051	0.024	0.032	0.039	0.059

Note: This table shows estimates of the baseline specification capturing the heterogeneous effects of the campaign for the subsample of households whose meters worked throughout the study period. Estimates correspond to the period Jul. 2012 - Dec. 2013. Cluster standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A6. Treatment effects on water and electricity use for households classified according to water and electricity use before the campaign; robustness check percentiles 25th and 75th

A. Inefficient use of water

	Inefficient water – Inefficient electricity				Inefficient water – Efficient electricity			
	Electricity use		*Water use*		*Electricity use*		*Water use*	
VARIABLES	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months
Post × Treated	2.501	2.340	-3.106	-4.424	-13.35*	-13.28	-0.241	-6.055
	(8.371)	(8.516)	(9.630)	(8.758)	(7.290)	(7.863)	(12.44)	(14.08)
Post-treatment	-17.13**	-26.79***	-51.46***	-52.76***	23.23***	19.64***	-61.77***	-69.76***
	(6.989)	(7.381)	(10.23)	(9.323)	(8.139)	(6.789)	(15.24)	(15.05)
Constant	197.6***	197.6***	221.8***	218.7***	39.97***	39.98***	219.8***	216.7***
	(2.876)	(3.147)	(5.192)	(5.266)	(3.115)	(3.504)	(11.17)	(11.62)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	1,183	1,638	1,183	1,638	429	594	429	594
No. Households	91	91	91	91	33	33	33	33
R-squared	0.059	0.058	0.152	0.127	0.104	0.108	0.188	0.151

B. Efficient use of water

	Efficient water – Inefficient electricity				Efficient water – Efficient electricity			
	Electricity use		*Water use*		*Electricity use*		*Water use*	
VARIABLES	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months	After 6 months	After 11 months
Post × Treated	-23.36	-24.10*	-2.908	-3.240	-8.817	-2.652	-14.16**	-16.37***
	(14.12)	(13.72)	(7.807)	(7.972)	(7.405)	(9.760)	(5.951)	(5.635)
Post-treatment	-10.95	-10.27	18.25**	12.43	13.48*	16.11**	16.19***	26.41***
	(13.57)	(11.21)	(7.981)	(10.88)	(6.793)	(6.664)	(5.897)	(7.459)
Constant	179.2***	179.2***	50.35***	49.65***	38.32***	38.32***	42.20***	41.61***
	(5.533)	(6.400)	(6.004)	(6.467)	(2.082)	(3.051)	(3.118)	(3.370)
Month-by-year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Obs.	325	450	325	450	767	1,062	767	1,062
No. Households	25	25	25	25	59	59	59	59
R-squared	0.134	0.129	0.118	0.104	0.036	0.042	0.049	0.065

Note: This table shows estimates of the baseline specification capturing the heterogeneous effects of the campaign for the subsample of households whose meters worked throughout the study period. Estimates correspond to the period Jul. 2012 - Dec. 2013. Cluster standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Chapter III

Interactions between CAP Agricultural and Agri-Environmental Subsidies and Their Effects on the Uptake of Organic Farming*

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Abstract

In this article, we analyze the effects of the interactions of the two pillars of the European Union Common Agricultural Policy – market support and rural development – on farmers' uptake of organic farming practices. Special attention is given to the 2003 reform, which substantially altered the relative importance of the two types of support by decoupling direct agricultural payments from the production of a specific crop. In our empirical analysis, we study the case of Sweden, making use of the variation in the timing of farmers' decisions regarding participation in support programs. We estimate a dynamic non-linear unobserved effects probit model to take account of unobserved individual heterogeneity and state dependence. Our results indicate the existence of a negative effect of the market support system in place when organic farming techniques were adopted before the 2003 reform; however, this effect is reversed by the introduction of decoupling. Furthermore, the effects of support differ between certified and non-certified organic production: both pillars have significant effects on non-certified organic farming, whereas certified organic farming is exclusively driven by agro-environmental subsidies.

Key words: Common Agricultural Policy, micro-analysis of farm firms, panel data models, subsidy decoupling

JEL classification: Q12, Q18, C23.

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1. Introduction

Policy instruments under the Common Agricultural Policy (CAP) are based on two main sources of financial support: market support and rural development, or Pillar One and Pillar Two. Pillar One consists of a series of subsidies available to nearly all farmers. The subsidies are aimed at improving the farmers' competitiveness while providing them with a steady income. In contrast, Pillar Two comprises a set of selective subsidies intended to support rural development and broader environmental goals, in which farmers' participation is also voluntary. Support under the pillars is non-exclusive, meaning that farmers could receive support from both pillars provided they meet the eligibility requirements for each pillar (EC 2010).

The CAP pillars have defined the orientation of agricultural and agri-environmental policy in the European Union (EU) over the years. In particular, direct payments linked to production and export and market subsidies have been at the core of market support, whereas Less Favored Areas (LFA)[1] allowances and environmental subsidies linked to participation in agri-environmental schemes have been key elements of rural development. Despite some reforms aimed at promoting more sustainable agriculture, Pillar One has remained the dominant part of the CAP, not only in the share of the EU budget but also in the hectares of farmland benefiting from it (Gay et al. 2005).

Though there is consensus that financial support under the pillars has been crucial for the viability of farming, concerns about agricultural production are increasingly juxtaposed against concerns for the environment (see, e.g., Lewandrowski et al. 1997). Agricultural production contributes significantly to many high-profile environmental problems. Intensive methods promoted under Pillar One may thus undercut the adoption of sustainable practices under Pillar Two. These methods also undercut reforms designed to advance the international and EU agenda of minimizing the environmental impacts of agriculture by phasing out environmentally harmful subsidies (Steenblik 2002).

In this article, we therefore investigate the effects of agricultural and agri-environmental policy on farmers' uptake of organic farming in Sweden – the country with the second largest share of organic agricultural land in Europe (EC 2010). Special attention is given to

[1] In less-favored areas, agricultural production is more difficult because of natural handicaps, e.g. difficult climatic conditions, steep slopes in mountain areas, or low soil productivity (EU 2003).

organic farming for three reasons. First, it is one of the agri-environmental indicators recognized by both the EU and the OECD (EC 2006; OECD 2001). Second, it is the only measure within Pillar Two that is extensively promoted throughout the EU. Third, it is regarded as an element of strategic investment in Sweden (MAFF 2000; MAFF 2008).

The main focus of the present article is the effects of the 2003 CAP reform, which, along with a set of other measures (EC 2004), substantially altered both the relative importance and the strength of the pillars by decoupling direct payments from the production of a specific crop. The reform intended to increase the uptake of organic farming in two ways. First, with the removal of the link between eligibility for Pillar One subsidies and choice of crops, producers acquired more freedom to choose crops that could be profitable when produced organically. Second, subsidies became independent from the level of production, implying that organic farmers (who were restricted by lower yields due to organic production standards) no longer had to accept reduced support through Pillar One (EC 2010). Hence, the decoupling was expected to generate not only an increase in farmers' uptake of organic farming but also a significant change in the role of Pillar One as a driver of farmers' behavior.

Previous studies have analyzed different dimensions of the CAP 2003 reform. Examples include the distributional and wealth effects of decoupled single farm payments (Femenia et al. 2010; Ciaian et al. 2012), the effects of decoupling on farm investment and output (Sckokai and Moro 2009), farmers' risk attitudes (Koundouri et al. 2009), disinvestment and farm exit (Kazukauskas et al. 2013), land market participation (O'Neil and Hanrahan 2012), farm fertilizer and pesticide expenditure (Jaraite and Kazukauskas 2012), and environmental consequences (Schmid and Sinabell 2007; Schmid et al. 2007). Similar outcomes have been analyzed in contexts outside the EU (Serra et al. 2006; Ahearn et al. 2006; Weber and Key 2012).

There is also a substantial body of research analyzing farmers' uptake of organic farming (D'Souza et al. 1993; Cary and Wilkinson 1997; Burton et al. 1999; De Cock 2005; Genius et al. 2006; Chouinard et al. 2008; Rezbanfar et al. 2011; Mzoughi 2011). The literature suggests a series of stylized facts: on average, organic farmers are younger, later entrants to agriculture; they are better educated, run smaller enterprises and are more concerned about environmental issues than conventional counterparts. More recent studies have focused on the effects of location, agglomeration, learning, and social networks on the spatial distribution of organic farming (Moschitz and Stolze 2009; Schmidtner et al. 2012; Allaire et al. 2012). The literature has also emphasized that farmers' reluctance to switch from and the reversal to conventional agriculture are major concerns (EC 2010).

Finally, a number of studies argue that, although farmers respond to both structural factors and economic incentives, their permanence in organic agriculture largely depends on financial support (Pietola and Lansink 2001; Rigby et al. 2001; Musshoff and Hirschauer 2008; Lapple and Donnellan 2009). This finding is particularly important in the EU, where CAP support is particularly important for the viability of farming, despite concerns regarding the economic feasibility of granting subsidies as the sector expands (Padel et al. 1999; Offermann et al. 2009; Stolze and Lampkin 2009).

Although considerable progress has been made in understanding the factors explaining the uptake of organic farming, some important questions remain. The most important of these concerns the interaction between the pillars. We hypothesize that the uptake of organic farming depends not only on the total amount of subsidies but also on how subsidies are distributed between the pillars. Furthermore, the introduction of decoupling gives us a unique opportunity to assess the effects of this interaction on farmers' decisions in response to changes in the relative importance of the pillars. This article is the first attempt to address both issues. To this end, we develop a conceptual framework to analyze the interactions between the pillars of the CAP and the uptake of organic farming. Our framework provides the basis for empirical investigation of these interactions.

We use micro farm-level data from the Farm Accountancy Data Network (FADN) to analyze farmers' uptake of organic farming before and after the reform. Specifically, we utilize nine rounds of a balanced panel consisting of 394 Swedish producers during the period 2000-2008. Our empirical strategy exploits the variation in the timing of farmers' decisions regarding voluntary participation in Pillar One and Pillar Two programs. Moreover, by estimating dynamic non-linear unobserved effects probit models, we are able to capture the extent to which the magnitudes of the subsidies under both pillars determine farmers' participation during the period under study, while taking into account individual heterogeneity and state dependence, i.e., farmers who are organic today are more likely to remain organic in the future due to management changes and production and income losses associated with the transition to organic farming. This approach has become very important, not only for testing theories but also in policy evaluation (Wooldridge 2010).

Thus, our paper provides evidence on the environmental effects of agricultural support to production. In particular, we show that the effect of Pillar One on the uptake of organic farming depends on the structure of the payments. Decoupling has significantly changed the role that Pillar One plays in shaping farmers' decisions. Due to the lower opportunity costs of adopting environmentally friendly farming practices, the negative effects of Pillar One on the uptake of organic farming have been substantially reduced since the reform. Furthermore, the extent to which Pillar One affects the uptake of organic farming also depends on market certification: certified farmers are not affected by the subsidies under Pillar One because they mainly rely on Pillar Two subsidies.

The article is organized as follows: The next section sets out the theoretical model illustrating the CAP support measures before and after the 2003 reform. The empirical strategy is described in the subsequent section, followed by a description of the data used to set up the study case. Next, we discuss the policy implications of the main results and conduct a number of robustness tests. The final section presents the main conclusions and some policy recommendations.

2. The pillars of the CAP and the 2003 reform in Sweden

Farmers' production decisions can be described as a two-stage process. First, they decide what to grow and then whether to produce conventionally or organically. The two (complementary) CAP pillars employ a series of policy instruments, including incentives to conventional production and incentives to organic production (see figure A1, Appendix A). To facilitate the presentation of these policies and the changes resulting from the 2003 reform, let us introduce some notation. Let A_{ij} and y_{ij} be the total acreage and average yield (Quintals/Ha) of farm *i* devoted to growing crop j.[2] We assume that y_{ij} depends on the use of inputs a_i and x_{ij} according to the function $y_{ij} = f(a_i, x_{ij})$, where x_{ij} is the quantity of polluting input used by crop j and a_i is a constant associated with farm *i*. In line with empirical evidence (see, e.g., Seufert et al. 2012), we assume that the average yield of organic

[2] Throughout these lines, the unit of analysis is the *farm*, defined as a specific production location. This should be differentiated from *farming unit*, which may consist of several production locations (agricultural business).

farming is lower than the average yield under conventional farming, i.e., $f(a_i, x_{ij} > 0) > f(a_i, x_{ij} = 0)$.

We can characterize the support to production of crop *j* under Pillar One as a *target price,* whereby the farmers are entitled to at least the target price $p_j{}^s$, and a *program yield*, whereby the farmers are entitled to direct payments $p_j{}^d$ that are coupled to the production that exceeds the target yield $y_j{}^p$. In addition to Pillar One subsidies, farmers who divert a fraction of their land to organic farming are entitled to a Pillar Two subsidy of g per hectare of land during the commitment period.[3] Before 2003, equal annual subsidies were allocated to uncertified and certified farmers, yet only certified farmers could market their products as organic, i.e., display the KRAV logo (MAFF 2000).

Let $p_j{}^m$ denote the market price of output *j*, $p_j{}^* = \max\{p_j{}^m, p_j{}^s\}$ and π_{ij}^{NO} the farm i's profit when producing crop j non-organically.[4] Based on the previous definitions, π_{ij}^{NO} can be represented as:

$$\pi_{ij}^{NO} = A_{ij}\left[p_j{}^* y_{ij}\left(x_{ij} > 0\right) + p_j{}^d \max\{y_{ij}(x_{ij} > 0) - y_j{}^p, 0\} - vx_{ij} - c_i\right], \qquad (1)$$

where the first two terms on the right-hand side correspond to the profits from production (including the support provided by the *target price* and *target yield* under Pillar One), net of the costs of the polluting input vx_{ij} and the cost per hectare c_i of any other inputs embodied in a_i. Furthermore, let π_{ij}^{O} denote the farm i's profit when producing non-certified organic. If λ_j denotes the fraction of land diverted to organic production of crop *j*, π_{ij}^{O} can be represented as:

$$\pi_{ij}^{O} = A_{ij}\left[\left[1-\lambda_j\right]\left[p_j{}^* y_{ij}\left(x_{ij} > 0\right) + p_j{}^d \max\left\{y_{ij}\left(x_{ij} > 0\right) - y_j{}^p, 0\right\} - vx_{ij} - c_i\right] + \lambda_j\left[p_j{}^* y_{ij}\left(x_{ij} = 0\right) + p_j{}^d \max\{y_{ij}(x_{ij} = 0) - y_j{}^p, 0\} - c_i + g\right]\right], \qquad (2)$$

where the first term corresponds to the payment for the "conventional" fraction of the production $\left[1-\lambda_j\right]$ and the second term corresponds to the payment for the non-certified

[3] Compensation for organic production is only paid for cultivation of arable land, on the basis of the units of livestock, and in the form of additional aid for mown meadows, pasture, and green fodder crops. Farmers initiating organic farming in year 2000 signed a six-year contract; from 2001 onward, contracts required a five-year commitment.

[4] Because price support is becoming less important within the CAP, in most cases $p_s \cong p_m$.

organic fraction of the production λ_j. As mentioned, subsidies under Pillar One are available for all types of farming. Thus, although organic farmers are entitled to all forms of support under Pillar One, the fact that the yields of organic farming are lower implies a reduced total subsidy that might be compensated by the government subsidy g and the savings related to the purchase of polluting inputs.

Finally, let us look at the profit for certified organic production. Let ${p_j}^c$ denote the market price of certified organic production of crop *j*. We know that ${p_j}^c > {p_j}^*$.[5] Market certification requires the total acreage of the farm devoted to crop *j* to be organic (KRAV 2007), which leads to profits π_{ij}^C:

$$\pi_{ij}^C = A_{ij}\left[{p_j}^c y_{ij}\left(x_{ij} = 0\right) + {p_j}^d \max\{y_{ij}(x_{ij} = 0) - {y_j}^p, 0\} - c_i + g\right] - \delta_i, \qquad (3)$$

where δ_i represents the annual costs of organic certification.[6] Note that the optimal fraction of land diverted to organic farming of crop *j* can be characterized as:

$$\lambda_j = \begin{cases} 1 \; if \; \pi_{ij}^C > \max\{\pi_{ij}^O, \pi_{ij}^{NO}\} \\ ]0,1[\; if \; \pi_i^O > \max\{\pi_{ij}^C, \pi_{ij}^{NO}\}. \\ 0 \; if \; \pi_i^{NO} > \max\{\pi_{ij}^C, \pi_{ij}^O\} \end{cases}$$

Thus, farmers produce crop *j* organically as long as $\pi_{ij}^{NO} < \max\{\pi_{ij}^C, \pi_{ij}^O\}$. Let us assume that $\pi_{ij}^C > \pi_{ij}^O$. The net gains of certified organic farming are given by the difference between equations (3) and (1):

$$\pi_{ij}^C - \pi_{ij}^{NO} = A_{ij}\left[\left[{p_j}^c y_{ij}\left(x_{ij} = 0\right) - {p_j}^* y_i(x_i > 0) + {p_j}^d\left[\max\left\{y_{ij}\left(x_{ij} = 0\right) - {y_j}^p, 0\right\} - \max\{y_{ij}(x_{ij} > 0) - {y_j}^p, 0\}\right] - c_i\right] + g + v x_{ij}\right] - \delta_i. \qquad (4)$$

That is, the difference in profits is given by the reduced support under Pillar One plus the government subsidy g and reduced costs of polluting input vx_{ij}, net of certification costs δ_i.

[5] The premium consumers pay for organic products in Sweden is about 30%. This figure has remained constant over the years (SCB 2005).

[6] The annual costs for KRAV certification consist of the license fee and the certification fee. The former entitles the farmer to use the KRAV label, whereas the latter pays the certification body for its certification services.

By analogy, if $\pi_{ij}^{O} > \pi_{ij}^{C}$, the net gains of non-certified organic farming are given by the difference between equations (2) and (1):

$$\pi_{ij}^{O} - \pi_{ij}^{NO} = A_{ij}\lambda_j\big[p_j{}^*\big[y_{ij}\big(x_{ij} = 0\big) - y_{ij}\big(x_{ij} > 0\big)\big] + p_j{}^d\big[\max\{y_{ij}\big(x_{ij} = 0\big) - y_j{}^p, 0\} - \max\{y_{ij}\big(x_{ij} > 0\big) - y_j{}^p, 0\}\big] + g + vx_{ij}\big], \quad (5)$$

where the difference in profits is given by the reduced support under Pillar One for the fraction of land diverted to organic farming plus the government subsidy and reduced costs of polluting input. Hence, from equations (4) and (5), we can hypothesize the existence of a negative effect of Pillar One subsidies on the uptake of organic farming. The fact that the financial support under Pillar One increases as yield increases and that the yield of organic farming is lower than the yield under conventional production implies that the higher the subsidies under Pillar One, the higher the incentives to intensify production and, thus, the higher the support under Pillar Two needed to persuade farmers to go organic.

Pillar One also negatively affects the uptake of organic farming through the choice of crops. To see this, assume that, in the first stage, farmers can choose between growing crop 1 and 2. Also assume that, if farmers decide to grow crop 1, the most profitable production method is conventional production. On the contrary, the most profitable production method for crop 2 is certified organic. Because equations (1) – (3) hold for each crop j =1, 2, it is easy to see that farmers would choose to produce crop 2 if $\pi_{i2}^{C} - \pi_{i1}^{NO} > 0$. If the support under Pillar One is crop-specific, this difference (and hence the uptake of organic farming) decreases when $p_1{}^s$ and $p_1{}^d$ increase or when $y_1{}^p$ decreases. A similar argument applies to $\pi_{i2}^{O} - \pi_{i1}^{NO}$.

2.1 The CAP reform in 2003

The 2003 reform involved three main components: decoupling most direct aid payments from production of specific crops, compulsory cross-compliance, and modulation. The latter two measures are not explicitly included in the analysis as they affect both conventional and organic farming equally. The rationale behind each component is to promote more sustainable agriculture by strengthening the criteria under which farmers can access market support.

The core element of the reform, however, was the introduction of the *Single Payment Scheme* (SPS), which is independent of production (EC 2004).[7] The regulation established that EU-15 Member States (hereinafter MS) using the normal direct payments regimen could introduce the SPS from January 1, 2005 and had to do so by January 2007.[8] In Sweden, the implementation of the above policies started on 1 January 2005.

Furthermore, along with the 2003 CAP reform, Sweden reformed support under the rural development plan (2007-2013), dividing organic farming subsidies into two categories: (1) certified organic production and (2) sustainable agricultural production adapted to organic production systems. Although these categories are equivalent to certified and non-certified organic farming, the rationale behind the separation of payments is to further incentivize farmers' participation in certified production. For further details regarding the amount of payments following the reform, see MAFF (2008).

In terms of the model above, the profits for conventional and non-certified and certified organic farming after the reform can be represented as:

$$\hat{\pi}_{ij}^{NO} = A_{ij}\left[{p_j}^m y_{ij}\left(x_{ij} > 0\right) + \mathrm{h} - v x_{ij} - c_i\right], \tag{6}$$

$$\hat{\pi}_{ij}^{O} = A_{ij}\left[\left[1 - \lambda_j\right]\left[{p_j}^m y_{ij}\left(x_{ij} > 0\right) + \mathrm{h} - v x_{ij} - c_i\right] + \lambda_j\left[{p_j}^m y_{ij}\left(x_{ij} = 0\right) + \mathrm{h} - c_i + \hat{g}\right]\right], \tag{7}$$

$$\hat{\pi}_{ij}^{C} = A_{ij}\left[{p_j}^c y_{ij}\left(x_{ij} = 0\right) + h - c_i + g^C\right] - \delta_i \tag{8}$$

where h is the decoupled subsidy per hectare, and g^C and $\hat{g}$ the subsidy per hectare diverted to organic farming and certified organic farming after the 2003 reform. As before, farmers participate in organic farming as long as $\hat{\pi}_{ij}^{NO} < \max\{\hat{\pi}_{ij}^{C}, \hat{\pi}_{ij}^{O}\}$. If $\hat{\pi}_{ij}^{C} > \hat{\pi}_{ij}^{O}$, the net gains of certified organic farming can be represented as:

[7] The remaining components of the reform included the introduction of both *compulsory cross-compliance* (requiring farmers to comply with certain agricultural and environmental standards in return for direct payments under the SPS) and *compulsory modulation* (implying a gradual reduction of all direct payments with the aim of financing the additional rural development measures announced in the reform). These measures are not explicitly included in the analysis as they affect both conventional and organic farming.

[8] In contrast, new MS have the option of continuing to use a system called a "single area payment scheme" or introducing the new SPS at any time.

$$\hat{\pi}_{ij}^{C} - \hat{\pi}_{ij}^{NO} = A_i \left[\left[p_j{}^c y_i(x_i = 0) - p_j{}^m y_i(x_i > 0) \right] + g^C + v x_i \right] - \delta_i \qquad (9)$$

Similarly, if $\hat{\pi}_{ij}^{O} > \hat{\pi}_{ij}^{C}$, the net gains of non-certified organic farming are given by

$$\hat{\pi}_{ij}^{O} - \hat{\pi}_{ij}^{NO} = A_{ij} \lambda_j \left[p_j{}^m \left[y_{ij}(x_{ij} = 0) - y_{ij}(x_{ij} > 0) \right] + \hat{g} + v x_{ij} \right] \qquad (10)$$

Comparing equations (4) and (9) and equations (5) and (10), it is straightforward that the introduction of decoupled subsidies should have increased participation in organic programs.[9] Because the support measures under both pillars are non-exclusive, the decoupled subsidy per hectare h does actually increase the profits from organic production, as seen in equations (7) and (8). Moreover, because the support under Pillar One is not crop specific after the reform, the decoupled subsidies have no effect on the choice of crop. Furthermore, the increased subsidy to certified organic production (i.e., $g^C > \hat{g}$) should have – *ceteris paribus* – led to a larger fraction of conventional farmers becoming fully organic. Hence, it is expected that the reform had a larger effect on the uptake of certified organic farming than on the uptake of non-certified organic farming.

In line with the model above, in the following sections we test three hypotheses regarding the interaction between the two CAP pillars:

H_1: *Before decoupling, total subsidies under Pillar One have a negative effect on the uptake of organic farming, while the reverse holds for subsidies under Pillar Two.*

H_2: *After decoupling, total subsidies under Pillar One have a positive effect on the uptake of organic farming as subsidies are no longer tied to yield/crop*.

H_3: *The increased subsidies to certified organic production have had a larger effect on the uptake of certified vs. uncertified organic farming.*

3. Empirical strategy

To identify a causal relationship between the CAP subsidies and the uptake of organic agriculture, we exploit the variation in the timing of farmers' decisions regarding participation

[9] Decoupling has caused other structural changes, for instance, entrance or exit of farming enterprises (see e.g., Kazukauskas et al. 2013). Entry and exit can have positive environmental effects if entrants are more environmentally friendly that those firms that exit the market. These impacts are outside the scope of the paper but could benefit from further research.

in Pillar One and Pillar Two programs. Specifically, while farmers' choices regarding Pillar One are made one year before the subsidies are paid, participation in and payment for Pillar Two programs occur in the same year; thus, farmers' choices regarding Pillars One and Two take place in periods t-1 and t, respectively (EC 2004). These differences allow us to separately identify the effect of each subsidy.

In addition, participation in organic farming exhibits two main features. On the one hand, shifting from conventional to organic agriculture requires not only substantial management changes but also production and income losses (Seufert et al. 2012).[10] This implies that a farmer who is organic today is more likely to remain organic in the future (i.e., there is state dependence in organic farming). On the other hand, farmers may have entered organic farming before the observation period, and therefore the history of the stochastic process driving its dynamics cannot be observed (i.e., there is an initial conditions problem). The second feature has special relevance to Sweden because organic farming support commenced there in the early 1990s, but it was not until the year 2000 that the EU started including farmers' participation in organic farming in its statistical records.

The combination of these features introduces some difficulties into the analysis. On the one hand, state dependence violates the assumption of strict exogeneity. At the same time, because the initial period is often correlated with unobserved individual characteristics, estimates may be biased and inconsistent and erroneous inferences drawn regarding the significance and magnitude of state dependence (Heckman 1981(a); Heckman 1981(b); Hsiao 2003).

We propose to estimate a dynamic non-linear unobserved effects probit model. Specifically, we utilize the conditional maximum likelihood estimator suggested by Wooldridge (2005), which takes care of unobserved individual heterogeneity while addressing the above-mentioned problems. Based on this approach, the farmer's current state regarding participation in organic agriculture is modeled as a function of participation in the previous period and unobserved individual characteristics. The model for the observed dependent variable can be written as follows:

[10] Yield losses reach about 34% of the agricultural production when conventional and organic systems are most comparable. However, losses can be lower depending on system and site characteristics.

$$P(y_{it} = 1 | y_{i,t-1}, \boldsymbol{z}_{it}, \alpha_i) = \Phi(\rho y_{i,t-1} + \boldsymbol{z}_{it}\boldsymbol{\gamma} + \alpha_i);\ i = 1, \dots, N; t = 2, \dots, T\ , \qquad (11)$$

where y_{it} is a dummy variable indicating farmer i's participation in organic farming in period t, $y_{i,t-1}$ is farmer i's participation status in period t-1, $\boldsymbol{z}_{it}$ is a vector of observed covariates at both the individual level (e.g., production inputs, land tenure, land use, soil quality, and CAP subsidies) and the county/harvest region level (e.g., share of organic farmers and potential productivity) that are strictly exogenous, conditional on the unobserved effect, α_i.[11] Moreover, ρ is a parameter representing the degree of state dependence, γ is a vector of parameters to be estimated, and Φ is the normal cumulative density function.

The effect of decoupling and the subsequent change in organic farming support are incorporated into the dynamic probit model as follows:

$$\begin{aligned} P(y_{it} = 1 | y_{i,t-1}, \boldsymbol{z}_i, x_{i,t-1}, w_{i,t}, \omega_t, \alpha_i) \\ = \Phi\Big(\rho y_{i,t-1} + \boldsymbol{z}_{it}\boldsymbol{\gamma} + \beta_{P1} x_{i,t-1} + \beta_{P2} w_{i,t} + \sum_T \delta_{P1t}(DP * \omega_t * x_{i,t-1}) \\ + \textstyle\sum_T \delta_{P2t}(DP * \omega_t * w_{i,t}) + \omega_t + \alpha_i\Big); \qquad (12) \end{aligned}$$

$$i = 1, \dots, N; t = 2, \dots, T; DP = \begin{cases} 1\ if\ t = T-3, \dots, T \\ 0\ if\ t = 2, \dots, T-4 \end{cases}$$

where $x_{i,t-1}$ and $w_{i,t}$ denote Pillar One and Pillar Two subsidies in periods t-1 and t, respectively.[12] Note that, while Pillar One subsidies are lagged, Pillar Two subsidies are

[11]Wooldridge (2005) approximates the distribution of α_i conditional on the initial period value and exogenous variables. This is done by assuming that $a_i \mid y_{i0}, z_i \sim$ Normal $(\varsigma_0 + \varsigma_1 y_{i0} + \boldsymbol{z_i}\varsigma_2, \sigma_a^2)$, where: $\alpha_i = \varsigma_0 + \varsigma_1 y_{i0} + \boldsymbol{z_i}\varsigma_2 + a_i$, with $z_i = (z_{i1},\dots,z_{iT})$ representing the observed history of the time-varying covariates ($\forall t$=2,…,T), and a_i is another unobservable individual-specific heterogeneity term that is uncorrelated with the initial observation.

[12]One might doubt the exogeneity of CAP subsidies. However, we believe that the potential endogeneity likely affects only Pillar Two. Our argument is that, given the universal eligibility of farmers for the subsidies in Pillar One, these payments do not directly prevent participation in organic farming (this could indirectly happen through yield and choice of crops, as mentioned in the theoretical model, but we account for them in the regressions as explanatory variables). Moreover, farmers' decisions regarding participation in Pillar One and Pillar Two take place at different moments in time. Because Pillar One is exogenous to the decision of becoming organic, we can also estimate the effects of decoupling, as it mainly affects Pillar One. Thus, the potential endogeneity of Pillar Two subsidies does not prevent us from estimating a casual effect of Pillar One subsidies on the uptake of organic farming or the effects of decoupling. With regard to Pillar Two, we acknowledge that they are potentially endogenous, but this endogeneity is minor and it would not affect our ability to identify the effects of Pillar Two. We make two arguments here.(1) As in the case of Pillar One, almost all support measures under Pillar Two are universal (e.g., both conventional and organic lands in Sweden are eligible). This also holds in the case of the agri-environmental schemes. (2) Information from the

contemporaneous in this specification so as to reflect the time at which farmers' decisions are made. The effect of decoupling is captured by $\sum_T \delta_{P1t}(DP * \omega_t * x_{i,t-1})$, an interaction term between Pillar One subsidies and the dummy variable corresponding to year *t* in the post-reform period. Similarly, the effect of the change in organic farming support is captured by $\sum_T \delta_{P2t}(DP * \omega_t * w_{i,t})$, an interaction term between Pillar Two subsidies and the dummy variable corresponding to year *t* in the post-reform period. Finally, ω_t is a year dummy. We are particularly interested in the sign and significance of the vectors δ_{p1t} and δ_{p2t} as they give us the effects of Pillar One and Two subsidies on the probability of being organic in the years succeeding the reform. That is to say, δ_{p1j} and δ_{p2j} capture the effects of the reform in the uptake of organic farming in year *t* compared with the baseline year. Based on the theoretical model, we expect β_{P2}>0, β_{P1}<0 (pre reform), β_{P1}≥0 (post reform), and δ_{P2j}>0 and δ_{P1j} >0, at least for some *t* (post reform).

Finally, a consistent estimator of the partial effects averaged at a distribution of the unobserved heterogeneity (Hereinafter APEs) is given by:

$$N^{-1}\sum_{i=1}^{N}\Phi\left(z_t\hat{\gamma}_a + \hat{\rho}_a y_{i,t-1} + \hat{\alpha}_{a0} + \hat{\alpha}_{a1} y_{i0} + z_i\hat{\alpha}_{a2}\right), \qquad (13)$$

where the 'a' subscript denotes the original parameter multiplied by the factor $(1+\sigma_a^2)^{-1/2}$, and $\hat{\gamma}, \hat{\rho}, \hat{\alpha}_0, \hat{\alpha}_1, \hat{\alpha}_2$ and $\hat{\sigma}_a^2$ are the maximum likelihood estimates. The proposed model explains not only the determinants of farmers' participation in organic farming but also the economic importance of state dependence in organic agriculture before and after the 2003 CAP reform. Moreover, the fact that the model accounts for unobserved effects enhances its importance not only for testing theories but also in policy evaluation (Wooldridge 2010).

4. Data

In this study, we use data from FADN, which is an instrument for evaluating income of agricultural holdings and the impacts of the CAP. It consists of an annual survey carried out by the MS of the EU, and includes two modules: physical/structural and economic/financial data. The survey provides representative data along three dimensions: region, economic size,

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Yearbook of Agricultural Statistics shows that organic farming payments account, on average, for 22% of the environmental subsidies (ranging from 9% in some counties to 39% in others). Moreover, we observe that they follow the same dynamics over time. This suggests that endogeneity is not very problematic because the non-organic component of the environmental subsidies is quite large (YAS 2000-2009). The robustness tests provided in the paper allow us to evaluate to what extent our results could be affected by this issue.

and type of farming (FADN 2010).[13] Specifically, we use nine rounds of farm-level data from a balanced panel consisting of 394 Swedish farmers during the period 2000-2008. The panel is a subset of a pooled dataset covering roughly 960 farmers.[14] This information is combined with county-level and harvest region data from the Yearbook of Agricultural Statistics (YAS) for the corresponding period.

Our definition of an organic farmer follows the EU classification, i.e., a farmer who devotes at least some of her total utilized agricultural area (UAA) to organic production. Nevertheless, the available information allows for a more detailed classification: non-organic, partially organic and fully organic farmers.[15] Table 1 presents the distribution of farmers by organic farming status over the study period.

[Insert table 1 here]

As shown in table 1, only 8.6% of the farmers in the panel were organic throughout the period 2000-2008, while 24% shifted between conventional and organic production during that time period. The percentages for the farmers in the pooled dataset were 4.1% and 26.4%, respectively. The data also indicates that about 12% of the panel farmers participate in the program for less than five years, suggesting that they may drop out organic farming before the contract ends. These figures are consistent with EC (2010), which states that, each year, a sizeable share (8-10%) of the producers leave the organic sector and revert to conventional agriculture, and vice versa.

Farmers in disadvantaged areas seem more likely to engage in organic production compared with those in more productive areas. Table 1 also shows that the share of conventional producers decreases as we move from normal to LFA. Nonetheless, farmers in less favored

[13] The annual sample covers approximately 80,000 holdings, representing a population of about 5 million farms in the MS-25. It comprises approximately 90% of the total utilized agricultural area, and accounts for about 90% of the total agricultural production in the EU. In the Swedish case, an average of 956 farmers are surveyed each year, representing a population of 30,440 holdings.

[14] Due to the sampling procedure, farmers are only surveyed in some years. Consequently, we can observe only about 40% of them during the study period. In spite of the availability of the pooled data, restricting the analysis to the balanced panel allows us to account for individual heterogeneity.

[15] Following the regulations EEC 2092/1991 and EC 834/2007, we define *partially organic* as a farmer who is converting to organic production (i.e., she is currently within the first two years after signing the contract) or applies both organic and other production methods (i.e., she combines both methods after completing the conversion period). In contrast, *fully organic farming* consists of farmers devoting their total UAA to organic production. Farmers in this category are more likely to be certified (EC, 1991; EC 2007).

mountain areas seem less likely to participate in organic farming than those in less favored non-mountain areas. This implies that farmers with average productivity have more incentives to engage in organic farming than those with either high or low productivity.

Although the figures above point to a dynamic pattern of entry and exit in organic farming, they do not show whether entry and exit follow a temporal pattern. Table 2 accounts for a temporal dimension by separating the flow of farmers converting to and reverting from organic agriculture during the study period. This separation provides some interesting findings. First, while most farmers entered as partially organic prior to the reform, entry as fully organic was the dominant pattern after the reform. Second, exit from organic production prior to the reform was mainly driven by partially organic farmers, whereas fully organic producers were most likely to exit after the reform. Finally, the data suggest that, overall, the dynamics of entry and exit slow down during the post-reform period (2005-2008) compared with the pre-reform period (2000-2003).

[Insert table 2 here]

Because the rate of certification among fully organic farmers cannot be observed from the data, the available information is unable to inform farmers' participation in the organic market. Figure 1 illustrates the relative importance of the share of hectares under the EU and KRAV schemes by using county-level data (YAS 2000-2009). While the share of organically farmed hectares is similar across the country, certified organic farming (measured as share of KRAV hectares) appears more important in the central and southern parts of the country. Figure 2 shows the spatial distribution of organic farmers. As can be seen, before the reform there was a concentration of organic farmers in the northern part of the country. This distribution became more uniform after the reform.

[Insert figures 1 and 2 here]

The organic market in Sweden has also evolved substantially during period under study, as shown in table 3. The awareness of consumers regarding organic products has been critical in the development of the market, and this has been accompanied by the greater availability of organic products in both specialized and unspecialized retail chains (EC 2010). This is demonstrated by increases in organic food expenditure per capita from 36.2% in the pre-reform period to 47.2% in the post-reform period.

[Insert table 3 here]

Due to the impossibility of disaggregating organic farming subsidies from the farm-level data, we use environmental subsidies, observed at the farm level,[16] as a proxy for organic farming subsidies. Similarly, both direct and SPS payments are proxied by Pillar One subsidies.[17] Table 4 shows the relative importance of the magnitudes of Pillar One and Pillar Two subsidies. First, fully organic farmers receive more subsidies per hectare than their partial and conventional counterparts. Second, Pillar One subsidies are higher for conventional farmers than for organic farmers, whereas partial and fully organic producers benefit from both Pillar One and Pillar Two subsidies equally. Third, the amount of Pillar One subsidies has increased in the post-reform period for all farmers, whereas the amount of Pillar Two subsidies appears to have decreased. Lastly, environmental subsidies are higher for fully organic producers, whereas LFA subsidies are larger in the sub-groups of conventional and partially organic farmers.

[Insert table 4 here]

Table 5 summarizes the main variables characterizing agricultural production in both organic and conventional holdings. The figures suggest important differences that are worth mentioning. First, organic farmers appear to be more labor and capital intensive than conventional farmers. This likely reflects the fact that, in absence of chemical inputs and fertilizers on the farm, organic producers have to implement alternative management practices that require more labor (EC 2010). Although farm assets ownership is similar for organic and conventional farmers, there are differences in ownership of animals, which is more prevalent in organic farms. Second, the average size of organic farms is larger. These size differences may be due to differences in farm specialization, along with the fact that larger farms may sometimes be necessary to compensate for higher production costs in the organic sector (EC

[16] Pillar Two subsidies include: (1) environment subsidies, (2) LFA subsidies, and (3) other rural development payments. Organic farming payments account for approximately 70.4% of the environmental subsidies in Sweden (Cantore et al. 2011).

[17] Pillar One subsidies comprise: (1) total subsidies for crops, (2) total subsidies for livestock, and (3) direct/decoupled payments. Direct income support (e.g., direct and SPS payments) accounted for nearly 80% of Pillar One expenditures in the EU during the period (2000-2008) (EC 2011).

2010). Third, irrigation seems to be unimportant in either group. Due to agro-ecological conditions in Sweden, farming cannot benefit from the adoption of irrigation techniques, compared to MS in Mediterranean areas. Fourth, conventional farms seem to operate a larger proportion of their agricultural land compared to organic farmers. This suggests either that conventional farmers devote less time to off-farm jobs or that a share of the land where organic production takes place is rented to other organic producers.

The figures also show that organic farmers tend to be specialized in milk, whereas conventional farmers are specialized in field crops. Differences can be explained by the difficulties/opportunity costs of carrying out organic production. According to EC (2000), permanent pastures for milk production are often eligible for agri-environment organic payments, and it is less risky to convert such pastures to organic than to convert other types of crops; this explanation is in line with the figures. A minority of farmers specializes in production systems other than field crops and milk, but there are no important differences between conventional and organic farms.

[Insert table 5 here]

Because direct payments were linked to the production of a specific crop prior to the reform, farmers' decisions depended not only on the market prices but also on the amount of the direct payment associated with a particular crop. The payments consisted of a fixed rate per metric ton, and thus more productive farmers were entitled to higher subsidies. In order to account for the effect of productivity on farmer behavior, we generate a measure of potential productivity, *potential cereal yield*. This variable was generated by using a three-stage procedure. First, we computed the absolute deviation of the observed individual yields with respect to the cereal yield of each harvest region. Then farmers were assigned to one of the 68 harvest regions based on a minimum distance criterion. Finally, each farmer was assigned the yield of her corresponding region. When information was unavailable, the yield of the corresponding county was assigned.[18] As shown in table 5, organic farmers exhibit slightly lower potential cereal yields, as expected.

[18]We also generated a measure of potential milk yields, defined as the average observed yield at county level. However, because this measure did not exhibit enough variation among counties, it was excluded from the analysis.

Finally, organic producers seem to be more likely to be located near each other (i.e., they tend to be concentrated in some counties). This implies that potential benefits from agglomeration may be a driver of the decision to adopt organic farming techniques.

5. Results and discussion

Table 6 summarizes the estimation results of the dynamic non-linear unobserved effects probit model in equation (12). Table 7 then shows the Average Partial Effects (APEs), our quantities of interest, defined as the partial effects averaged across the distribution of the unobserved heterogeneity. Because N is large and T is small in all specifications, we use panel data bootstrap (i.e., resampling all time periods from the cross-sectional units) for both standard errors and inference, as suggested by Wooldridge (2010).[19] We also estimate the main specification by means of the Arellano and Bond estimator. This estimate is regarded as baseline because its coefficients can be directly compared with the APE from non-linear models (Wooldridge 2010).

[Insert tables 6 and 7 here]

5.1 Interactions between the pillars before decoupling

Column (1) in tables 6 and 7 shows the determinants of the farmer's participation in organic farming from 2000 to 2003. As can be seen, before decoupling, Pillar One had a negative and significant effect on the probability of being organic, yet its APE is not statistically significant. In contrast, Pillar Two had no significant impact on the farmers' decisions. Despite these differences, the sign of both coefficients suggests the existence of trade-offs: while environmental subsidies increased participation in organic farming, the market support system in place before the 2003 reforms decreased it. Overall, the results clearly indicate that, prior to the 2003 CAP reform, Pillar One increased the opportunity costs of organic production.

Other factors explaining the farmers' participation decisions during this period are the farm size and the share of organic farmers in their corresponding counties. In both cases, the APEs are positive and statistically significant, and their magnitudes are 0.08% and 52.3%, respectively.

[19] Because we are dealing with dynamic non-linear models, the use of standard statistics such as the Hausman test could be misleading.

5.2 Interactions between the pillars after decoupling

Similarly, column (2) in tables 6 and 7 displays the determinants of participation in organic farming in the period 2005-2008.[20] As expected, there are substantial changes in the strength of the effect of the pillars on farmers' decisions around the time of decoupling.

One interesting finding is that – consistent with our hypothesis 2 – the estimated coefficient of Pillar One becomes positive and statistically significant, yet its APE remains insignificant. This result has several implications. First, it suggests that Pillar One no longer has a negative effect on the uptake of organic farming, and that decoupling has ameliorated the trade-offs farmers face when making production choices. Second, even though this coefficient and that of the pre-reform period are statistically significant only at the 10% level, their difference is statistically significant at the 1% level, indicating that the change in the role of Pillar One after the reform was considerably high. Third, the findings are in line with those in column (3) in table 7, which summarizes the determinants of the farmer's participation during the study period (2000-2008). Overall, this result corroborates that the introduction of decoupling has allowed farmers to respond to market signals while enjoying financial security, decreasing the opportunity costs of organic production.

Another interesting finding emerges when evaluating the role of Pillar Two. Both the estimated coefficient and its APE are highly statistically significant, suggesting that environmental subsidies have played an important role in the years following the reform. Specifically, a EUR 100/ha increase in environmental subsidies generates an increase in the probability of being organic by 14.2%.

This result also brings a series of implications. On the one hand, the difference of the estimated coefficient with that of the pre-reform is statistically significant at the 1% level, indicating that the change in the importance of Pillar Two after the reform was considerably high. On the other hand, the numbers are also in line with those in column (3) in table 7, suggesting that the increase in the relative importance of Pillar Two compared with Pillar One

[20] The results are robust to the inclusion of year 2004, which is when farmers started to adapt to the reform. Nevertheless, because decoupling officially took place beginning on January 1, 2005, we restrict the estimates to this period in order to match the regulation.

has provided farmers with the economic incentives needed to shift from conventional to organic agriculture.[21]

A third major finding relates to the effect of decoupling on farmers' participation in organic farming. Although the interaction terms suggest that decoupling has not generated an increase in the uptake of organic agriculture, the subsequent change in the rural development policy increased farmers' participation by 4.3% in 2008 compared to 2001 (the baseline year). Because the change in the rural development policy in Sweden was not introduced until 2007, this finding indicates that farmers are more responsive to policies perceived to directly affect their economic incentives than to those that may generate indirect effects in several domains, as is the case of decoupling. Altogether, the APEs in columns (2) – (3) partially support our second hypothesis. However, although Pillar One no longer has a negative effect on the uptake of organic farming, the introduction of decoupling has not generated enough incentives to increase farmers' participation in organic farming.

As a final point, another factor explaining farmers' participation decisions during this period is the share of organic farmers in their corresponding counties. Specifically, a one percentage point increase in the share of farmers in a given county increases the uptake of organic farming by 46.6%.

5.3 Effects of decoupling on certified and non-certified organic farming

The results above suggest that agglomeration effects play an important role for farmers' participation in organic farming, which is acknowledged in a number of other studies (Moschitz and Stolze 2009; Schmidtner et al. 2012; Allaire et al. 2012). Furthermore, our figures indicate that, due to the specific features of certified and non-certified organic agriculture, it is most likely that certified farmers tend to be concentrated in some counties, while non-certified farmers are uniformly distributed across the country. If this is the case, the 2003 CAP reform might have generated different effects on certified and non-certified organic producers.

[21] In order to account for the opportunity costs of being organic, besides controlling for suitable covariates, our estimates include farmers receiving no payments from Pillar Two. Data indicates that about 4% of the farms under study have never received Pillar Two subsidies, whereas 71% of them have always received positive amounts. Because this could potentially generate mechanical correlation with the dependent variable, we follow two strategies. First, we exclude from the analysis farmers that have never received Pillar Two subsidies. Second, we restrict our analysis to the subsample of farmers that have always received Pillar Two subsidies. Our results remain fundamentally unchanged in either strategy. Results are available upon request.

In order to account for such differences while examining the determinants of farmers' participation in certified and non-certified organic farming, we divide the data into two subsamples using the 50th percentile of the distribution of the average share of KRAV hectares in each county as a cut-off. Because the percentage of KRAV hectares in a given county can be used as a proxy for organic market development, the resulting "low share" and "high share" subsamples are associated with non-certified and certified organic farming, respectively.

The estimated parameters for both subsamples are displayed in columns (4) – (5) in tables 6 and 7. Participation in non-certified organic farming is explained by the standard determinants in the empirical literature (e.g., labor, capital, and soil quality), yet these variables appear unimportant for explaining participation in certified organic agriculture. Furthermore, there is no evidence of differentiated effects of monetary support: Pillar Two has a positive and significant effect in both subsamples, whereas Pillar One appears not to affect farmers' participation status in either subsample. Their corresponding APEs are not statistically significant.

The most important finding, however, is the effects of the 2003 CAP reform. The introduction of decoupling increased the probability of being non-certified organic by 2.7% in year 2005 compared to the baseline year. Similarly, the change in organic farming support increased the probability of being certified organic by 9.2% over the baseline year. The latter finding supports our third hypothesis that the increased subsidies to certified organic production have had a larger effect on the uptake of certified versus uncertified organic farming.

5.4 Differentiated effects of CAP subsidies

The extent to which Pillar One may affect the uptake of organic farming depends on the amount of subsidies to which farmers are entitled. Table 8 presents the APEs of the CAP subsidies at different percentiles of the average subsidy distribution. Columns (1) – (2) summarize the APEs corresponding to the CML model for the pre- and post-reform periods. As expected, farmers who received larger subsidies under Pillar One prior to the reform exhibited a lower probability of participation in organic agriculture than those who received lower amounts. For instance, a 100 EUR/ha increase in Pillar One subsidies decreases the probability of being organic by 1.9% among farmers in the 95th percentile, whereas there is no detectable change among farmers in the 5th percentile. The reverse holds for farmers who received larger Pillar Two subsidies after the reform.

The APEs for the period 2000-2008 are presented in column (3). As can be seen, farmers who received larger Pillar Two subsidies are more likely to engage in organic farming than those who received lower amounts. For example, a 100 EUR/ha increase in environmental subsidies increases the probability of being organic by 4.1% and 6.5% among farmers in the 5th and 95th percentile, respectively. In contrast, farmers' choices were unaffected by Pillar One regardless of the distribution of the subsidies. These findings are in line with those in columns (1) – (2) and provide strong support for the first two hypotheses of this study, namely that subsidies under Pillar One had a negative effect on the adoption of organic farming before decoupling; however, this effect has been reversed by the introduction of decoupling because subsidies are no longer tied to yield. Similarly, subsidies under Pillar Two have a positive effect on the adoption of organic farming.

[Insert table 8 here]

The differentiated effects of CAP support on the subsamples of non-certified and certified organic farming are summarized in columns (4) – (5). The results indicate that farmers' decisions regarding participation in non-certified organic farming are mainly driven by Pillar One. Specifically, a 100 EUR/ha increase in Pillar One subsidies decreases the probability of non-certified organic farming by 1.3% among farmers in the 95th percentile of its distribution, while changes in Pillar Two subsidies appear not to affect their behavior. In contrast, there is no evidence of differentiated effects of CAP subsidies for the subsample of certified organic producers.

5.5 Effects of decoupling across production systems

So far, we have assumed that there is a common policy effect of the reform across production systems. This may not be the case, as the cost of converting to organic farming substantially differs among specialization areas (EC 2010). In order to account for potential differentiated effects of the 2003 CAP reform across production systems, we estimate our main specification for the subsample of farmers specialized in field crops and milk. This subsample not only covers 75% of the farmers under study but also accounts for the differences in the costs of converting to organic farming. Although a more detailed analysis would be desirable, the number of observations in the remaining categories was not enough for estimating the CML model.

[Insert table 9 here]

Results are presented in table 9. Estimated coefficients indicate that decoupling has affected these sectors differently. We find evidence of a positive yet mild effect on the uptake of organic farming in the subsample of farmers specialized in milk. However, decoupling had no effect on farmers specialized in field crops. This difference could be explained by the fact that the field crop sector remains under "partial decoupling" after the reform (i.e., the policy instruments have been less stringent in this particular sector).[22] In contrast, the change in Pillar Two support has contributed to shaping farmers' incentives in the desired direction (though, during the first years after the reform, the transition from the dairy premium to the single payment scheme in the milk sector may have produced a negative effect on the uptake of organic farming by milk producers).

Our findings also indicate that the magnitude of Pillar Two subsidies appears to be more important for farmers specialized in field crops. This is not unexpected, as the production process of field crops relies heavily on polluting inputs and, hence, environmental subsidies play a major role in ameliorating income losses due to conversion to organic farming. This result is in line with those in tables 6 and 7, in spite of the fact that the corresponding APEs are not statistically significant.

5.6 State dependence in organic farming

To conclude, this section sheds light on the economic importance of state dependence in organic farming after correcting for the initial conditions problem. To this end, we compute the predicted probabilities of being organic in the current year given that the farmer was or was not organic in the previous year. The difference between these probabilities gives us the magnitude of state dependence in the corresponding year (Wooldridge 2005). Estimates are displayed in table 10.

[22] According to EC (2003), MS may retain 25% of the cereals/oilseeds/proteins component of the single payment scheme or, alternatively, up to 40% of the supplementary durum wheat aid in order to continue the existing coupled per hectare payments up to those percentage levels.

As expected, farmers who were organic in the past exhibit higher probabilities than their conventional counterparts of being organic at present. To take a concrete example, the estimated probabilities averaged over the distribution of the unobserved heterogeneity during the study period are 0.3691 for organic farmers and 0.1280 for conventional farmers. Thus, state dependence accounts for 24.1% of the probability of being organic during the period 2000-2008, which is non-negligible. This figure coincides with that in column (3) in table 7.

[Insert table 10 here]

Two points are worth noting. First, even after controlling for unobserved effects, the coefficient of the lagged dependent variable is highly statistically significant. Second, the initial conditions are also highly significant in all specifications. Both facts suggest that there is substantial correlation between the unobserved heterogeneity and the initial conditions, which underlines the importance of correcting for the initial conditions in dynamic nonlinear models (Wooldridge 2005). From a policy perspective, it is worth mentioning that despite the 2003 CAP reform, the magnitude of state dependence remains the same. This finding should be taken into account when designing policies aimed at promoting participation in organic farming.

6. Robustness checks

In this section, we develop a series of robustness checks to verify the validity of the results to the incorporation of the demand side of organic farming, the use of farm subsidies at a higher geographical level (e.g., country level versus individual level) and the implementation of an alternative policy effect identification strategy, in which we compare relatively similar farms across the borders that operate in slightly different policy environments.

6.1 Accounting for the changes on the organic food demand side

As indicated by the descriptive statistics (table 3), the organic market in Sweden has evolved substantially during the period under study. Though the effects of the evolution of organic farming should be captured in our model through the control for yearly dummies, we also develop a robustness check incorporating in our model two proxies of organic food demand: (1) organic food expenditure per capita, and (2) number of organic importers from third countries. The first variable accounts for the demand for organic food irrespective of its origin, while the second accounts for the demand for organic products that are not supplied by local producers. These variables are only observed at country-level per year. Results including these variables are presented in Table B1, Appendix B. Though the results indicate that the farmers have positively responded to market signals during the period under study (e.g., an increase in the demand for organic products has been accompanied by an increment in the uptake of organic farming), our results regarding the effects of the CAP Pillars remain. As expected, this confirms that the growth in the demand for organic products was already captured in our previous specification through the year dummies.

6.2 CAP subsidies and farm production choices

One might think that variations in Pillar One and Pillar Two subsidies at the farm level can be driven by changes in farm production choices. If this is the case, we will observe a change in farm structure across time, and the policy effect cannot be identified. Although our data evidences the absence of structural changes in farm management during the period under study, we estimate our main specification by using the average rate of subsidy per year at country level as a robustness test. This estimation strategy sheds light on the role of CAP support on the uptake of organic farming at the cost of a significant loss in variation across farmers and time. Results are displayed in Table B2, Appendix B. As can be observed, our results remain robust regarding the use of the yearly subsidies, with the exception that Pillar Two subsidies became insignificant, which is expected due to the lower variation in the data.

6.3 Quasi-experimental approach comparing different countries

As a final robustness check we implement a quasi-experimental approach to identify the policy effects. This is done by comparing relatively similar farms across borders, which operate in slightly different policy environments. We therefore estimate a Difference-in-Differences (DD) model using farm level data for Sweden, the Netherlands, and Finland. Among the three countries, Sweden implemented decoupling in 2005, whilst Netherlands and Finland did not implement it until 2006. Such time lag results in a pipeline design which allows us to use the farms in the Netherlands and Finland as counterfactuals for farms in Sweden and to evaluate the impact of decoupling on the uptake of organic farming. Our main specification follows that of Kazukauskas et al (2013):

$$P\left(y_{it} = 1 \middle| D_i, y_{i,t-1}, \boldsymbol{z}_{it}, \boldsymbol{x}_i, a_i, \tau_t, \omega_j\right) = \Phi\left(kD_i + \rho Y_{05} + \beta(D_i * Y_{05}) + \boldsymbol{z}_{it}\boldsymbol{\gamma} + \boldsymbol{x}_i\boldsymbol{\varphi} + \tau_t + \omega_j + a_i\right)$$

where D_i is the treatment dummy, which equals 1 if a farm is in Sweden and 0 otherwise; Y_{05} is a dummy variable for the post-decoupling period (i.e., 2005 onward); $D_i * Y_{05}$ is the interaction term between the treatment and post-decoupling period; $\boldsymbol{z}_{it}$ is a vector of time-varying variables; $\boldsymbol{x}_i$ is a vector of time-invariant variables; τ_t is a vector of year dummies; ω_j is a vector of country dummies; and a_i accounts for individual-specific unobserved characteristics. The effect of decoupling on the uptake of organic farming is captured by the parameter β. The model is estimated by means of a dynamic probit model, which controls for potential correlation between the random effect and exogenous variable by using either the history or the mean of the time-varying variables. We also estimate alternative specifications using random effects probit models that also account for individual heterogeneity by using the mean of the time-varying variables. A summary of the main results is presented in Table B3, Appendix B. As expected, results indicate that decoupling has not generated an increase in the uptake of organic farming in Sweden when compared to that in either Finland or the Netherlands.

Most importantly, because the introduction of decoupling has mainly affected the role of Pillar One subsidies, we augment the previous specification in an attempt to evaluate the change in the role of Pillar One subsidies as follows:

$$\begin{aligned} P\big(y_{it} = 1 \big| D_i, y_{i,t-1}, \boldsymbol{z}_{it}, \boldsymbol{x}_i, a_i, \tau_t, \omega_j\big) \\ = \Phi\big(kD_i + \rho Y_{05} + \beta(D_i * Y_{05}) + \delta_0 P1_{i,t-1} + \delta_1\big(P1_{i,t-1} * Y_{05}\big) \\ + \delta_2\big(P1_{i,t-1} * D_i\big) + \delta_3\big(P1_{i,t-1} * D_i * Y_{05}\big) + \boldsymbol{z}_{it}\boldsymbol{\gamma} + \boldsymbol{x}_i\boldsymbol{\varphi} + \tau_t + \omega_j + a_i\big), \end{aligned}$$

where $P1_{i,t-1}$ are the Pillar One subsidies, $P1_{i,t-1} * Y_{05}$ is the interaction term between Pillar One subsidies and the post-decoupling period, $P1_{i,t-1} * D_i$ is the interaction term between Pillar One subsidies and the treatment dummy, and $P1_{i,t-1} * D_i * Y_{05}$ is the interaction term among Pillar One subsidies, the treatment dummy and the post-decoupling dummy. In this specification, the parameter of interest is δ_3. Results are displayed in Table B4, Appendix B. The estimated coefficients of the interaction terms indicate that, while Pillar One subsidies negatively affect the uptake of organic farming in Sweden, in the post-decoupling period, market support no longer has an effect on the uptake of organic farming. The figures show that, although farmers receiving larger amounts of Pillar One subsidies were not affected to a greater extent by the introduction of decoupling, the reform has ameliorated the negative effect of market support on the uptake of agri-environmental measures.

Overall, results above are in line with those in columns 1-3 in Tables 6-7, which correspond to the main specification of the paper. Hence, the main results of the paper are robust to the use of this alternative specification.

7. Conclusions

This study has attempted to shed light on the potentially negative interaction effects of agricultural and agri-environmental policies on farmers' uptake of programs aimed at improving environmental outcomes. In particular, we analyzed the effects of the subsidies under Pillar One on the uptake of organic farming before and after the 2003 CAP reform. We hypothesized that, before the reform, the support under Pillar One had a negative effect on the uptake of organic farming, and that this effect was reversed to become a positive effect due to decoupling.

Our results support our hypothesis. Our estimates indicate the existence of a negative effect of Pillar One subsidies on the uptake of organic farming before decoupling: while environmental subsidies increase participation in organic farming, market support as then designed had the opposite effect. The fact that the structure of the payments under Pillar One affects the achievement of environmental goals has important implications for policy design and cost-benefit analysis. The negative effects of Pillar One on the uptake of organic farming affect both the effectiveness and cost of the Common Agricultural Policy; failure to account for them increases the cost of meeting environmental goals, making it less acceptable to the public and to policymakers.

Our results were confirmed by a series of robustness checks. These checks included incorporating the demand side of organic farming, using farm subsidies at a higher geographical level and implementing an alternative strategy to identify policy effects. This enables us to draw a number of general conclusions. First, although decoupling has not increased farmers' uptake of organic farming, it has contributed to alleviating the negative effects of market support. This is supported by the estimated coefficients for the pre-reform and post-reform periods in Sweden. In both cases, there is evidence of substantial changes in the relative importance of the Pillars: while farmers' choices regarding participation in organic agriculture were mainly driven by Pillar One before the reform, Pillar Two is the core determinant of behavior after the reform. Second, the results indicate that the change in organic farming support, along with decoupling, has generated an increase in the uptake of organic farming by 4.3% in year 2008 compared to the baseline year.

Regarding the effects of market certification, we acknowledge that a caveat of our analysis concerns the lack of individual information regarding participation in KRAV. Unfortunately, such information is difficult to obtain because, as a private certifier, KRAV has no duty to disclose information regarding the identity of its clients. We proxy the effects of market certification through information available at the county level. On the whole, however, we believe that our twofold approach (i.e., theoretical and empirical) clearly demonstrates the existence of differentiated effects of the CAP support on the uptake of certified and non-certified organic farming. Although the CAP subsidies appear insignificant to explain farmers' decisions in both subsamples, the reform has significantly contributed to attenuate the undesirable incentives of coupled payments in non-certified organic production while incentivizing farmers' participation in certified organic farming. In particular, the probability of being non-certified organic increased by 2.7% in the year 2005 compared to the baseline year, whereas farmers' participation in certified organic farming increased by 9.2% in the year 2008 compared to the baseline year.

There are other factors explaining the uptake of organic farming as well. First, state dependence matters in organic farming, implying that farmers' current behavior depends on past observed behavior as well as on unobserved effects. The identified factors shed light on important features that should be considered when designing policies aimed at promoting participation in organic farming, especially when determining the target population.

Second, in line with previous studies (e.g., Schmidtner et al. 2012; Allaire et al. 2012), we find that agglomeration effects are important drivers of farmers' participation decisions. This implies that farmers located in close proximity to each other exhibit similar choice behavior. This may arise due to communication between farmers, which can, for example, raise awareness, reduce information costs or change preferences. In addition, there may be unobserved favorable geographical and economic conditions at play, which influence adoption and are correlated over space. Unfortunately, our data only allows capturing agglomeration effects across farmers located in the same county since we do not know the specific geographical location of each firm. Fully accounting for the effects of spatial dependence would be a very interesting area for further research, provided further spatial information becomes available.

Finally, our study has mainly focused on the effects of CAP policy support. However, as described in the introduction, other studies point to the role of alternative drivers behind the uptake of organic farming, such as non-pecuniary motivations and the provision of information.

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List of tables

Table 1. Percentage of Farmers by Organic Farming Status and Type of Agricultural Area

Farming status	*Panel* (*2000-2008*)				*Pooling* (*2000-2008*)			
	All	Normal areas	LFA non-mountain	LFA mountain	All	Normal areas	LFA non-mountain	LFA mountain
Never organic	67.0	77.7	60.0	39.8	69.6	80.2	64.5	48.2
Once organic	5.1	2.6	8.7	2.6	4.8	3.9	4.7	8.3
Twice organic	2.0	2.5	1.3	2.6	3.0	2.6	2.8	4.9
3 to 5 times organic	8.1	5.2	9.3	18.7	10.6	6.0	14.7	20.8
6 to 8 times organic	9.1	6.2	8.6	26.6	8.0	4.9	8.4	15.1
Always organic	8.6	5.7	12.1	9.7	4.1	2.4	5.0	2.7
Obs.	3546	1783	1424	339	8638	4079	3573	986

Source: Own elaboration based on FADN data.

Table 2. Dynamics of Organic Farming

	2001	2002	2003	2004	2005	2006	2007	2008
Organic farmer (*t*)	14.4	17.1	20.9	21.7	24.7	24.2	19.9	20.2
Organic farmer (*t-1*)	20.9	14.4	17.1	20.9	21.7	24.7	24.2	19.9
Entering organic farming								
i. Conventional - partially	2.0	2.5	1.5	0.8	0.3	0	0	0.3
ii. Conventional – fully	0	1.0	3.0	1.0	3.5	1.5	1.0	1.0
Exiting organic farmer								
i. Partially - conventional	8.6	0.8	0.8	0	0.3	0.3	1.0	0.8
ii. Fully – conventional	0	0	0	1.0	0.5	1.8	4.3	0.3

Source: Own elaboration based on FADN data. Figures correspond to the balanced panel and are expressed in percentages (N=394).

Table 3. Organic Market in Sweden (2000-2008)

Year	Retail organic food sales[a] (Million EUR)	Org. food expenditure per capita [a,b] (EUR)	No. organic Producers[c]	No. organic Importers[c]
2000	177	19.9	3626	105
2001	322	36.2	5268	121
2002	401	44.8	3665	241
2003	396	44.1	3562	250
2004	396	43.9	4726	213
2005	403	44.6	3019	124
2006	347	38.1	5623	135
2007	431	46.9	2848	130
2008	547	59.1	3686	146

Source: [a] The world of organic agriculture, statistics and emerging trends (2000-2010). [b] Statistiska Centralbyrån. [c] Eurostat. (Base year=2000).

Table 4. Evolution of CAP Subsidies per Hectare [EUR/ha and %]

	Conventional		*Partially organic*		*Fully organic*	
	Pre Reform	Post reform	Pre reform	Post reform	Pre reform	Post reform
Total (EUR)	281	295	369	376	429	422
Pillar one (*% total*)	73.2	79.2	53.0	64.5	48.7	56.5
Pillar two (*% total*)	23.9	20.1	44.3	35.1	49.2	40.7
Agri-environment pay. (*% Pillar two*)	79.6	65.2	86.6	65.0	83.2	75.0
Less-favored areas (*% Pillar two*)	20.4	30.1	13.4	29.5	16.8	19.4

Source: Own elaboration based on FADN data. Figures correspond to the balanced panel (N=394) and are deflated (2000=100). Note: (a) Total subsidies exclude subsidies on investments and are expressed in EUR/ha; (b) Pillar One and Two figures might not sum up to 100% as subsidies for intermediate consumption, subsidies for external factors, subsidies under Art. 68, and other subsidies are not included; (c) Pillar One includes total subsidies for both crops and livestock and decoupled payments.

Table 5. Descriptive Statistics of Major Variables

Variable	*Panel* (*2000-2008*)		*Pooling* (*2000-2008*)	
	Conventional	Organic	Conventional	Organic
	Farm-level data			
Total labor (AWU)	1.40	1.99	1.34	1.61
	(0.92)	(1.30)	(1.19)	(1.18)
Total capital (EUR)	547926	642851	466511	496478
	(504317)	(547260)	(477422)	(491103)
Total UUA (Ha)	90.2	136.4	80.8	113.4
	(89.2)	(104.5)	(89.9)	(116.3)
Irrigation status	0.0472	0.0310	0.0398	0.0207
	(0.2122)	(0.1735)	(0.1956)	(0.1423)
Land owner occupation (%)	0.6374	0.5125	0.6362	0.5193
	(0.3301)	(0.3581)	(0.3496)	(0.3361)
Land rented (%)	0.3626	0.4875	0.3635	0.4805
	(0.3299)	(0.3581)	(0.3494)	(0.3360)
Specialist field crops	0.5198	0.2550	0.5001	0.2479
	(0.4997)	(0.4361)	(0.5000)	(0.4319)
Specialist milk	0.3204	0.5176	0.2727	0.3327
	(0.4667)	(0.5000)	(0.4454)	(0.4713)
Specialist grazing	0.0393	0.0836	0.0602	0.2362
	(0.1944)	(0.2770)	(0.2380)	(0.4248)
Specialist granivores	0.0137	0.0136	0.0211	0.0110
	(0.1165)	(0.1159)	(0.1436)	(0.1045)
Specialist mixed	0.1068	0.1302	0.1352	0.1673
	(0.3089)	(0.3368)	(0.3420)	(0.3733)
	County-level/Harvest region data			
Organic farmers (%)	0.1911	0.2809	0.1947	0.2740
	(0.1164)	(0.1312)	(0.1187)	(0.1275)
Potential yield cereals (Quintals/Ha)	9.78	9.72	-	-
	(1.167)	(1.073)		
Potential yield milk (Kg/cow)	7659	7428	-	-
	(970)	(857)		

Source: Own elaboration based on FADN data and the Yearbook of Agricultural Statistics (1999-2008). Standard deviations in parentheses. Figures in EUR are deflated (2000=100).

Table 6. Determinants of Organic Farming

Variable	*Pre reform* (1)	*Post reform* (2)	*Panel* (3)	*Non-certified* (4)	*Certified* (5)
Organic farmer (*t*-1)	1.088**	2.611***	2.045***	2.306***	2.091***
	(0.477)	(0.400)	(0.174)	(0.289)	(0.238)
Labor	-0.409	0.954*	0.0718	0.696*	-0.0577
	(0.519)	(0.542)	(0.197)	(0.380)	(0.245)
Capital	0.0156	-0.00289	-0.0122	-0.199**	0.0765
	(0.173)	(0.0898)	(0.0405)	(0.0825)	(0.0512)
Size	0.0174*	-0.00552	0.00384	0.000434	0.00542*
	(0.00958)	(0.00992)	(0.00265)	(0.00643)	(0.00297)
Irrigation	1.022	-1.297*	0.241	-	-
	(0.653)	(0.719)	(0.335)		
Land owner occup.	1.338	0.728	0.0743	-	-
	(1.465)	(1.395)	(0.545)		
Potential yield (*t*-1)	0.0130	-0.201	-0.0607	0.0198	-0.152
	(0.153)	(0.184)	(0.0798)	(0.107)	(0.110)
LFA mountain	0.238	-0.209	-0.0173	1.208*	-0.827
	(1.136)	(0.619)	(0.659)	(0.734)	(0.973)
LFA non mountain	0.188	0.737*	0.214	0.410	-0.0924
	(0.509)	(0.404)	(0.262)	(0.377)	(0.329)
Environmental subsidies	0.302	3.051***	0.637***	0.519*	0.468*
	(0.419)	(0.988)	(0.208)	(0.294)	(0.276)
Pillar 1 subsidies (*t-1*)	-0.520*	0.441*	-0.128	-0.236	-0.148
	(0.301)	(0.268)	(0.118)	(0.156)	(0.168)
Pillar1*2005	-	-	0.0786	0.454**	-0.144
			(0.155)	(0.227)	(0.240)
Pillar1*2006	-	-	-0.0599	0.115	-0.165
			(0.170)	(0.237)	(0.324)
Pillar1*2007	-	-	0.0377	-0.155	0.248
			(0.162)	(0.256)	(0.214)
Pillar1*2008	-	-	0.0547	0.371	-0.132
			(0.151)	(0.274)	(0.218)
Pillar2*2005	-	-	0.0504	0.315	0.455
			(0.352)	(0.501)	(0.544)
Pillar2*2006	-	-	-0.165	-0.420	0.634
			(0.322)	(0.398)	(0.696)
Pillar2*2007	-	-	0.512	0.777	0.557
			(0.367)	(0.580)	(0.535)
Pillar2*2008	-	-	0.645*	0.836	1.171**
			(0.379)	(0.681)	(0.549)
Share org. farmers	11.31***	9.984**	7.140***	-	-
	(3.936)	(4.545)	(1.190)		
Organic (t_o)	3.513**	2.028*	1.513***	1.496**	1.224***
	(1.416)	(1.136)	(0.364)	(0.590)	(0.464)
Constant	-7.749**	-2.304	-3.716***	-4.182***	-2.227*
	(3.173)	(2.016)	(1.206)	(1.556)	(1.315)
Log $\hat{\sigma}_a$	1.281	-0.381	-0.0338	-1.202	-0.344
	(0.785)	(1.121)	(0.347)	(0.925)	(0.546)
Log-likelihood	-166.2	-118.7	-404.9	-169.2	-213.9
Specialization dummies	Yes	Yes	Yes	Yes	Yes
Year dummy indicators	Yes	Yes	Yes	Yes	Yes
Obs.	1,182	1,182	3,152	1,648	1,504

Note: This table shows estimates from equation (2). Columns (1) – (3) correspond to the baseline model during the pre-reform period, the post-reform period, and the study period, respectively. Columns (5) and (6) correspond to the baseline model for the low and high shares of KRAV hectares. The dependent variable is participation in organic farming (dichotomous). Pillar *i* * year is the interaction between the lagged pillar *i*=1,2 subsidies and the year dummies following the reform. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Average Partial Effects for the Estimated Models

Variable	*Baseline*	*Pre reform* (1)	*Post reform* (2)	*Panel* (3)	*Non-certified* (4)	*Certified* (5)
Org. farmer (*t*-1)	0.404*** (0.0677)	0.0599 (0.0554)	0.300** (0.147)	0.241*** (0.0583)	0.241*** (0.0854)	0.268*** (0.0923)
Labor	0.0225 (0.0154)	-0.0189 (0.0295)	0.0445 (0.0275)	0.00478 (0.0143)	0.0169 (0.0150)	-0.00632 (0.0165)
Capital	0.000305 (0.00222)	0.000719 (0.00856)	-0.000135 (0.00338)	-0.000809 (0.00231)	-0.0110** (0.00492)	0.00657 (0.00412)
Size	0.000694 (0.000470)	0.000804* (0.000447)	-0.000258 (0.000365)	0.000256 (0.000193)	0.000132 (0.000226)	0.000415** (0.000199)
Irrigation	-0.0302 (0.0230)	0.0519 (0.0321)	-0.0589 (0.0447)	0.0167 (0.0299)	-	-
Land owner occup.	0.000830 (0.0308)	0.0619 (0.0710)	0.0340 (0.0467)	0.00495 (0.0411)	-	-
Wheat yield (*t*-1)	-0.00103 (0.0123)	0.00059 (0.00813)	-0.0094 (0.00902)	-0.00405 (0.00606)	0.00228 (0.00773)	-0.0113 (0.00918)
LFA mountain	-	0.0113 (0.0566)	-0.0096 (0.0334)	-0.00115 (0.0657)	0.152 (0.123)	-0.0500 (0.0735)
LFA non mountain	-	0.00871 (0.0323)	0.0349 (0.0217)	0.0143 (0.0232)	0.0457 (0.0358)	0.00553 (0.0295)
Environ. subsidies	0.0823*** (0.0298)	0.0140 (0.0238)	0.142*** (0.0503)	0.0425** (0.0170)	0.0340 (0.0270)	0.0349 (0.0293)
Pillar 1 subs. (*t*-1)	-0.0288 (0.0187)	-0.0240 (0.0153)	0.0206 (0.0136)	-0.00851 (0.00798)	-0.0154 (0.00966)	-0.0117 (0.0144)
Pillar1*2005	0.00972 (0.0157)	-	-	0.00524 (0.0118)	0.0268** (0.0111)	-0.0124 (0.0233)
Pillar1*2006	0.00713 (0.0165)	-	-	-0.00399 (0.0113)	0.00744 (0.0139)	-0.0155 (0.0176)
Pillar1*2007	0.0179 (0.0198)	-	-	0.00251 (0.0119)	-0.00628 (0.0207)	0.0180 (0.0289)
Pillar1*2008	0.0174 (0.0193)	-	-	0.00364 (0.0105)	0.0231 (0.0162)	-0.0108 (0.0222)
Pillar2*2005	-0.00218 (0.0156)	-	-	0.00336 (0.0223)	0.0252 (0.0315)	0.0393 (0.0446)
Pillar2*2006	-0.0289 (0.0186)	-	-	-0.0110 (0.0180)	-0.0229 (0.0191)	0.0552 (0.0359)
Pillar2*2007	-0.0128 (0.0234)	-	-	0.0341 (0.0210)	0.0476 (0.0380)	0.0442 (0.0412)
Pillar2*2008	0.00247 (0.0344)	-	-	0.0429* (0.0225)	0.0432 (0.0348)	0.0924** (0.0394)
Share org. farmers	0.883*** (0.149)	0.523** (0.222)	0.466** (0.230)	0.476*** (0.0979)	-	-
Organic (t_o)	-	0.276*** (0.0717)	0.190 (0.128)	0.145*** (0.0548)	0.144* (0.0739)	0.124* (0.0634)
Obs.	2,758	1,182	1,182	3,152	1,648	1,504

Note: This table presents a summary of the marginal effects from the estimated models. Column (1) corresponds to the linear estimation of the baseline model during the study period (Arellano & Bond estimator). Columns (2) – (4) correspond to the non-linear estimation of the baseline model during the pre-reform period, the post-reform period, and the study period, respectively (Wooldridge CML model). Columns (5) and (6) correspond to the non-linear estimation of the baseline model including both the low and high shares of KRAV hectares (Wooldridge CML model). Bootstrapped standard errors in parentheses (Replications=500). *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Average Partial Effects of CAP Subsidies (2000-2008)

Percentile	*Pre reform* (1)		*Post reform* (2)		*Panel* (3)		*Non-certified* (4)		*Certified* (5)	
	Pillar 1	Pillar 2	Pillar 1	Pillar 2	Pillar 1	Pillar 2	Pillar 1	Pillar 2	Pillar 1	Pillar 2
5th	-0.0273	0.0135	-0.00879	0.0410**	0.0202	0.115***	-0.0165	0.0315	-0.0121	0.0334
	(0.0217)	(0.0222)	(0.00859)	(0.0170)	(0.0134)	(0.0217)	(0.0111)	(0.0238)	(0.0158)	(0.0293)
25th	-0.0255	0.0138	-0.00866	0.0431**	0.0206	0.158***	-0.0160	0.0332	-0.0118	0.0349
	(0.0177)	(0.0231)	(0.00829)	(0.0183)	(0.0136)	(0.0325)	(0.0105)	(0.0261)	(0.0149)	(0.0311)
50th	-0.0240	0.0141	-0.00853	0.0455**	0.0212	0.219***	-0.0154	0.0354	-0.0116	0.0364
	(0.0151)	(0.0241)	(0.00803)	(0.0199)	(0.0140)	(0.0717)	(0.00973)	(0.0297)	(0.0143)	(0.0337)
75th	-0.0227*	0.0145	-0.00838	0.0498**	0.0219	0.336*	-0.0149	0.0382	-0.0114	0.0396
	(0.0130)	(0.0263)	(0.00777)	(0.0233)	(0.0150)	(0.177)	(0.00920)	(0.0355)	(0.0136)	(0.0418)
95th	-0.0193**	0.0160	-0.00793	0.0651*	0.0281	0.463***	-0.0132*	0.0482	-0.0107	0.0484
	(0.00912)	(0.0371)	(0.00725)	(0.0385)	(0.0284)	(0.116)	(0.00791)	(0.0626)	(0.0124)	(0.0639)
Obs.	1,182		1,182		3,152		1,648		1,504	

Note: This table shows the average partial effects of both Pillar One and Pillar Two subsidies at different percentiles of the average subsidy distribution during the pre-reform period, the post-reform period, and the study period, respectively. Bootstrapped standard errors in parentheses (Replications =500). *** p<0.01, ** p<0.05, * p<0.1.

Table 9. Determinants of Organic Farming by Farm Specialization

Variable	Field crops				Milk			
	Pre reform (1)	*Post reform* (2)	*Panel* (3)	*APEs* (4)	*Pre reform* (5)	*Post reform* (6)	*Panel* (7)	*APEs* (8)
Organic farmer (*t*-1)	2.071***	10.82***	2.018***	0.144	0.819	2.375***	2.461***	0.407***
	(0.654)	(3.938)	(0.387)	(0.136)	(0.679)	(0.469)	(0.213)	(0.0815)
Labor	0.247	-1.966	0.105	0.00386	0.653	0.240	0.286	0.0195
	(0.238)	(1.294)	(0.263)	(0.0188)	(0.607)	(0.327)	(0.259)	(0.0218)
Capital	-0.397	-0.530***	-0.0959*	-0.00354	0.332	-0.0269	-0.0150	0.000195
	(0.450)	(0.205)	(0.0583)	(0.00460)	(0.362)	(0.0842)	(0.0661)	(0.00548)
Size	0.00223	0.0651***	0.00434	0.000160	0.0163*	0.00138	0.000829	0.000305
	(0.00268)	(0.0176)	(0.00298)	(0.000241)	(0.00930)	(0.00568)	(0.00534)	(0.000380)
Potential yield (*t*-1)	-0.0782	-0.399	-0.195	-0.00721	0.220	-0.497	0.0595	0.00377
	(0.177)	(1.072)	(0.223)	(0.0188)	(0.216)	(0.305)	(0.0766)	(0.00910)
LFA mountain	-	-	-	-	-0.410	-1.299	0.510	0.0266
					(2.056)	(1.482)	(0.671)	(0.137)
LFA non mountain	-0.534	-15.41***	-0.558	-0.0191	0.594	-0.595	0.768**	0.0288
	(0.647)	(5.518)	(0.618)	(0.0321)	(1.133)	(0.781)	(0.378)	(0.0546)
Environmental subsidies	0.484	33.12***	2.327**	0.0859	0.871	3.772**	0.317	0.0274
	(0.636)	(10.56)	(1.103)	(0.0825)	(0.646)	(1.895)	(0.246)	(0.0294)
Pillar 1 subsidies (*t-1*)	0.569	4.170	-0.213	-0.00786	-0.211	0.523	-0.0222	-0.00370
	(0.887)	(3.633)	(0.576)	(0.0354)	(0.430)	(0.406)	(0.183)	(0.0156)
Pillar1*2005	-	-	0.525	0.0194	-	-	-0.100	-0.0109
			(0.777)	(0.0476)			(0.218)	(0.0266)
Pillar1*2006	-	-	-0.147	-0.00539	-	-	-0.213	-0.0165
			(0.971)	(0.0632)			(0.239)	(0.0167)
Pillar1*2007	-	-	0.693	0.0256	-	-	-0.536**	-0.0366
			(1.189)	(0.0699)			(0.231)	(0.0266)
Pillar1*2008	-	-	0.496	0.0183	-	-	-0.0527	-0.00625
			(1.005)	(0.0460)			(0.254)	(0.0190)
Pillar2*2005	-	-	0.0583	0.00215	-	-	0.0995	0.0147
			(1.400)	(0.0800)			(0.539)	(0.0385)
Pillar2*2006	-	-	0.917	0.0338	-	-	-0.311	-0.0285
			(1.755)	(0.0782)			(0.565)	(0.0394)
Pillar2*2007	-	-	1.214	0.0448	-	-	1.106*	0.0770
			(1.788)	(0.0884)			(0.565)	(0.0550)
Pillar2*2008	-	-	-0.505	-0.0186	-	-	0.0664	0.00709
			(1.461)	(0.0679)			(0.612)	(0.0374)
Organic (t_o)	1.141*	20.10***	1.500*	0.0825	4.696**	2.917*	0.939***	0.0864
	(0.649)	(5.741)	(0.908)	(0.134)	(1.836)	(1.634)	(0.296)	(0.0977)
Constant	-1.962	-10.57	-1.243	-	-14.08***	1.554	-5.942***	-
	(1.794)	(12.21)	(2.762)		(5.129)	(2.659)	(1.184)	
Log $\hat{\sigma}_a$	-13.87	4.307***	0.0386	-	2.072***	1.051	-12.60	-
	(439.8)	(0.420)	(0.973)		(0.713)	(0.912)	(33.49)	
Log-likelihood	-30.57	-29.92	-82.40	-	-81.32	-91.33	-181.45	-
Year dummy indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	372	525	1,028	1,028	528	646	1,340	1,340

Note: This table shows estimates from equation (2). Columns (1) - (3) correspond to the baseline model for the subsample of farmers specialized in field crops. Columns (5) - (7) correspond to the baseline model for the subsample of farmers specialized in milk production. Columns (4) and (8) correspond to the APEs of the estimated models (Bootstrapped standard errors in parentheses. Replications=500). The dependent variable is participation in organic farming (dichotomous). Pillar *i* * year is the interaction between the lagged pillar *i*=1,2 subsidies and the year dummies following the reform. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 10. State Dependence in Organic Farming (2000-2008)

Year	Organic (*t-1*)	Conventional (*t-1*)	Difference
2001	0.238***	0.0632***	0.174***
	(0.0421)	(0.0184)	(0.0433)
2002	0.379***	0.123***	0.257***
	(0.0682)	(0.0195)	(0.0705)
2003	0.438***	0.155***	0.284***
	(0.0652)	(0.0233)	(0.0705)
2004	0.410***	0.139***	0.271***
	(0.0625)	(0.0225)	(0.0671)
2005	0.457***	0.175***	0.281***
	(0.0663)	(0.0237)	(0.0730)
2006	0.400***	0.137***	0.264***
	(0.0571)	(0.0262)	(0.0665)
2007	0.288***	0.103***	0.185***
	(0.0424)	(0.0213)	(0.0465)
2008	0.343***	0.129***	0.214***
	(0.0481)	(0.0205)	(0.0529)

Note: This table shows the estimated probabilities of being organic in the current period given that the farmer is or is not organic in the previous period. Bootstrapped standard errors in parentheses (replications=500). *** p<0.01, ** p<0.05, * p<0.1.

List of figures

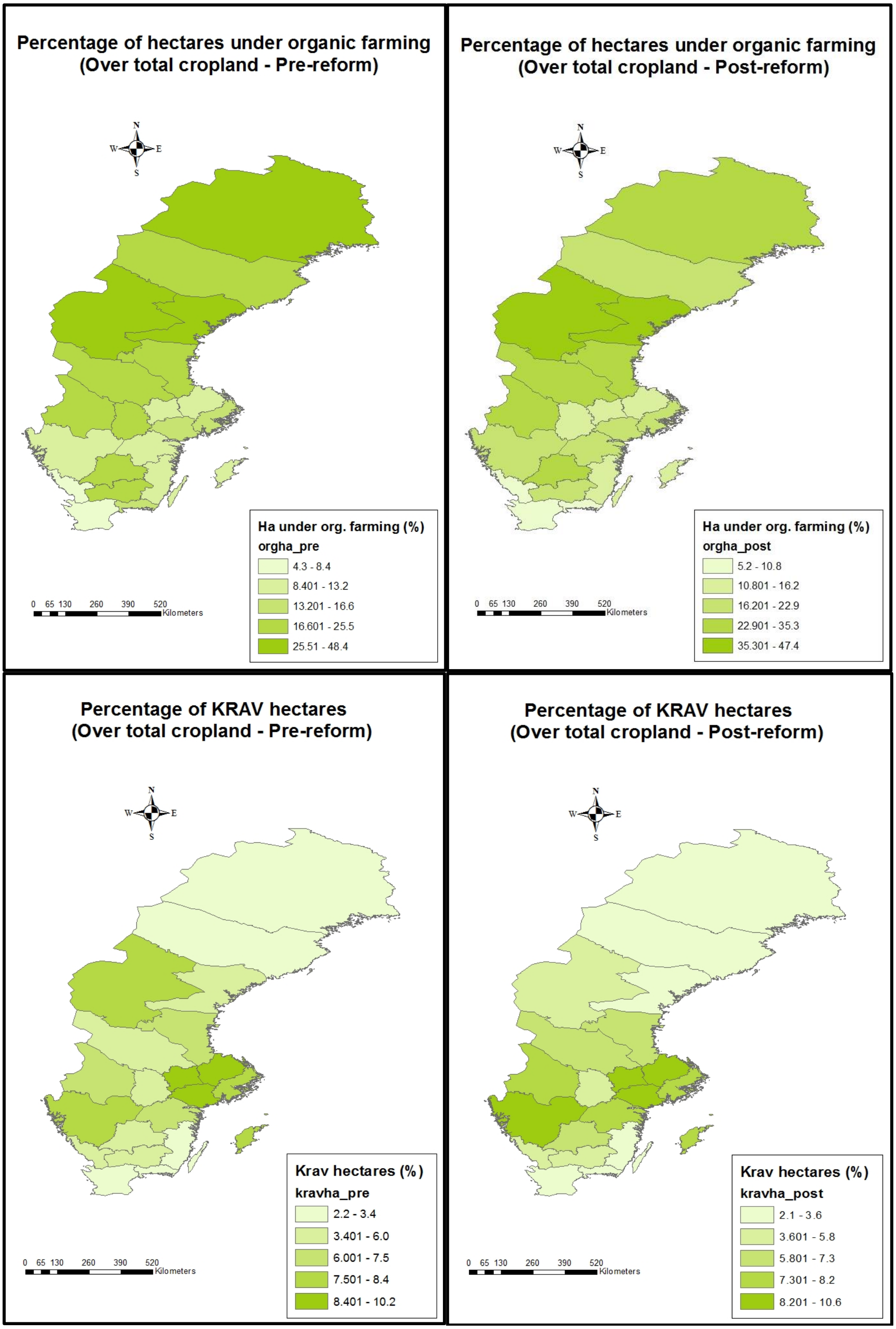

Figure 1. Spatial distribution of the share of hectares under non-certified and certified organic farming

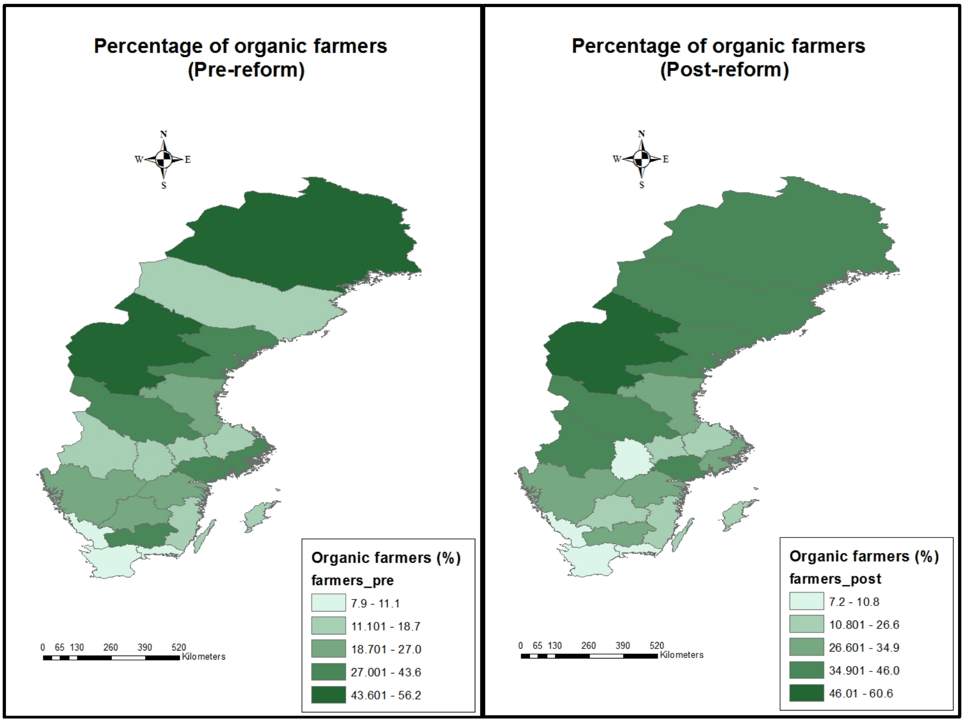

Figure 2. Spatial distribution of organic farmers

Appendix A. Agri-environmental Support in Sweden

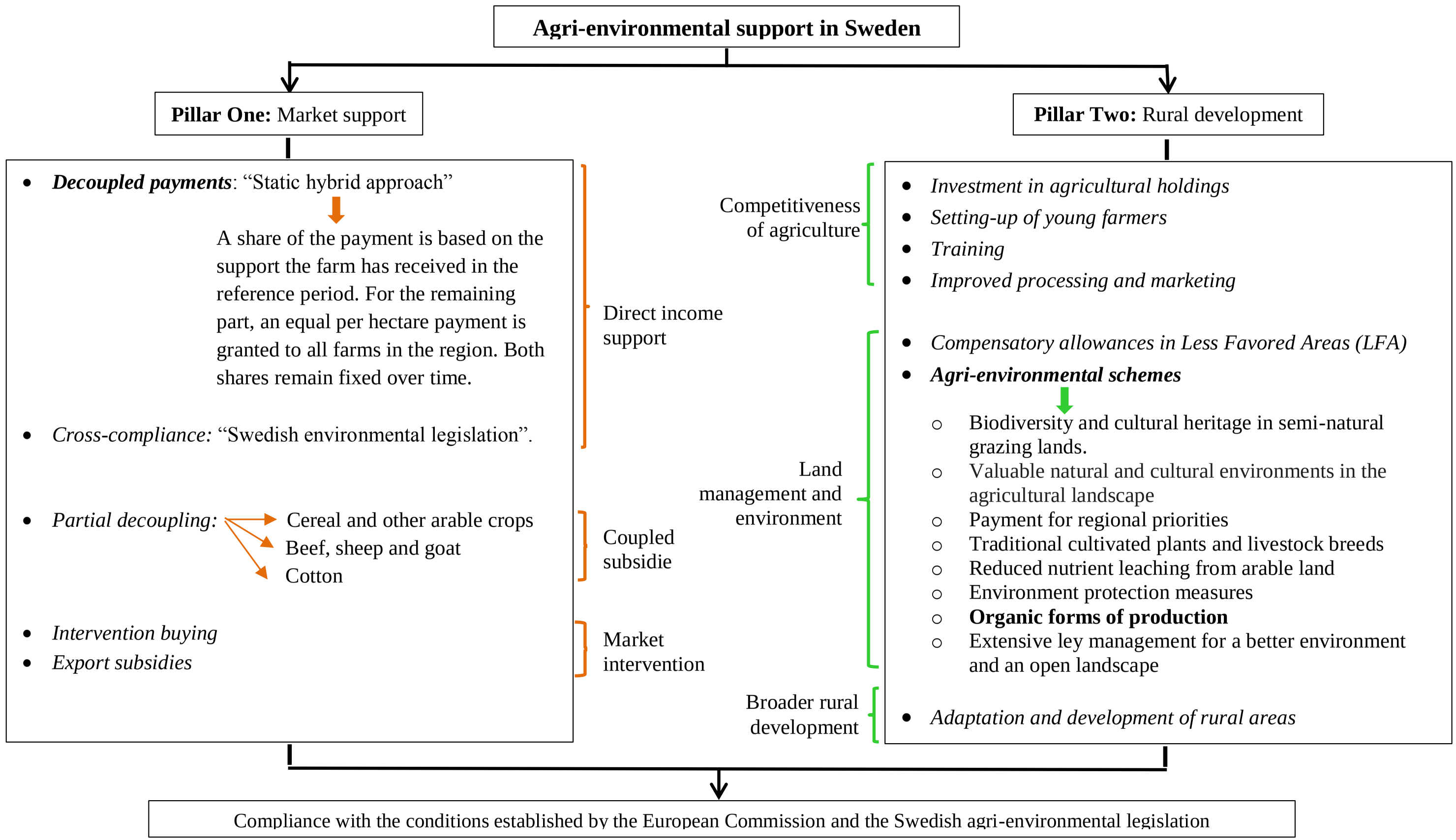

Figure A1. Pillar One and Two support measures in Sweden

Appendix B. Additional tables and figures

Table B1. Determinants of Organic Farming (Organic Food Demand)

Variable	Expend. on organic food per capita			No. organic importer operators		
	Pre reform (1)	*Post reform* (2)	*Panel* (3)	*Pre reform* (4)	*Post reform* (5)	*Panel* (6)
Organic farmer (*t*-1)	1.103**	2.647***	1.791***	1.078**	2.666***	1.937***
	(0.454)	(0.402)	(0.155)	(0.443)	(0.398)	(0.163)
Labor	-0.434	0.990*	0.0794	-0.429	1.032*	0.0875
	(0.512)	(0.537)	(0.186)	(0.515)	(0.551)	(0.187)
Capital	0.0241	0.00484	-0.0520	0.0254	-0.000338	-0.0220
	(0.173)	(0.0813)	(0.0376)	(0.175)	(0.0891)	(0.0369)
Size	0.0171*	-0.00650	0.00386	0.0171*	-0.00564	0.00358
	(0.00964)	(0.00951)	(0.00247)	(0.00959)	(0.00991)	(0.00249)
Irrigation	0.990	-1.050	0.210	0.997	-1.194*	0.218
	(0.648)	(0.678)	(0.324)	(0.651)	(0.710)	(0.320)
Land owner occup.	1.237	0.818	0.0559	1.260	0.721	0.0519
	(1.459)	(1.334)	(0.534)	(1.459)	(1.383)	(0.532)
Potential yield (*t*-1)	0.0944	1.296	0.0283	0.0991	0.879	-0.00809
	(0.189)	(0.981)	(0.0760)	(0.191)	(0.824)	(0.0749)
LFA mountain	-0.639	0.0181	-0.476	-0.686	0.111	-0.339
	(1.594)	(0.993)	(0.912)	(1.598)	(1.063)	(0.885)
LFA non mountain	0.255	0.710*	0.427	0.250	0.792*	0.460*
	(0.534)	(0.401)	(0.277)	(0.534)	(0.424)	(0.269)
Environmental subsidies	0.288	2.858***	0.739***	0.321	3.065***	0.699***
	(0.392)	(0.921)	(0.182)	(0.399)	(0.987)	(0.180)
Pillar 1 subsidies (*t-1*)	-0.495*	0.466*	-0.238**	-0.509*	0.477*	-0.131
	(0.293)	(0.264)	(0.0997)	(0.291)	(0.267)	(0.101)
Share org. farmers	11.60***	13.23***	7.530***	11.06***	9.488**	7.723***
	(3.739)	(4.334)	(0.984)	(3.743)	(3.845)	(0.983)
Organic food demand	0.0729*	0.0479*	0.0147	0.00510*	0.0476**	0.00436***
	(0.0387)	(0.0252)	(0.00938)	(0.00271)	(0.0219)	(0.00121)
Organic (t_o)	3.531**	1.834	1.600***	3.577***	2.068*	1.498***
	(1.400)	(1.124)	(0.363)	(1.352)	(1.142)	(0.353)
Constant	-10.85**	-4.411*	-12.08***	-8.952**	-9.112**	-12.20***
	(4.823)	(2.576)	(3.843)	(4.474)	(3.982)	(3.706)
Log $\hat{\sigma}_a$	1.280*	-0.695	-0.0711	1.306*	-0.425	-0.195
	(0.767)	(1.359)	(0.334)	(0.731)	(1.136)	(0.352)
Log-likelihood	-165.5	-118.3	-416.6	-165.4	-117.4	-411.2
Specialization dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy indicators	No	No	No	No	No	No
Obs.	1,182	1,182	3,152	1,182	1,182	3,152

Note: This table shows estimates from equation (2). Columns (1) – (3) correspond to the baseline model during the pre-reform period, the post-reform period, and the study period, respectively. The dependent variable is participation in organic farming (dichotomous). Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B2. Determinants of Organic Farming (Country-level Subsidies)

Variable	*Pre reform* (1)	*Post reform* (2)	*Panel* (3)
Organic farmer (*t*-1)	1.400***	2.644***	1.982***
	(0.373)	(0.432)	(0.167)
Labor	-0.498	1.166**	0.0597
	(0.438)	(0.473)	(0.182)
Capital	-0.0568	0.0295	-0.0202
	(0.126)	(0.0693)	(0.0365)
Size	0.0145*	-0.00973	0.00397
	(0.00757)	(0.00920)	(0.00253)
Irrigation	1.024*	-1.007*	0.244
	(0.572)	(0.610)	(0.334)
Land owner occup.	0.921	0.989	0.170
	(1.362)	(1.308)	(0.537)
Potential yield (*t*-1)	0.119	1.162	-0.00565
	(0.161)	(0.878)	(0.0768)
LFA mountain	-0.653	0.902	0.0276
	(1.384)	(0.943)	(0.985)
LFA non mountain	0.448	0.505	0.412
	(0.435)	(0.319)	(0.285)
Environmental subsidies	-0.389	6.165	0.464
	(1.879)	(7.124)	(0.798)
Pillar 1 subsidies (*t-1*)	-3.777**	4.410***	-3.234***
	(1.715)	(1.682)	(1.124)
Pillar1*post-decoupling	-	-	1.374
			(1.383)
Post-decoupling	-	-	-2.488
			(2.962)
Share org. farmers	10.66***	10.81***	6.923***
	(3.358)	(4.041)	(1.093)
Organic (t_o)	3.859***	1.815	2.384***
	(0.970)	(1.217)	(0.416)
Constant	2.483	-17.37**	0.578
	(5.244)	(8.093)	(4.431)
Log $\hat{\sigma}_a$	1.219**	-1.022	0.503
	(0.481)	(1.778)	(0.315)
Log-likelihood	-197.8	-139.2	-463.5
Specialization dummies	Yes	Yes	Yes
Obs.	1,182	1,182	3,152

Note: This table shows estimates from equation (2). Columns (1) – (3) correspond to the baseline model during the pre-reform period, the post-reform period, and the study period, respectively. The dependent variable is participation in organic farming (dichotomous). Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B3. Diff-in-diff Estimates (Decoupling)

Variable	*Dynamic Probit*		*Probit Model*
	(1)	(2)	(3)
Treatment × Yr05	-0.0717	-0.0774	0.0896
	(0.241)	(0.242)	(0.215)
Treatment	-0.0258	0.248	-0.278
	(0.959)	(0.727)	(0.675)
Yr05	0.0487	0.0563	0.146
	(0.278)	(0.280)	(0.254)
Log $\hat{\sigma}_a$	1.467***	1.643***	2.749***
	(0.215)	(0.147)	(0.0777)
Log-likelihood	-641.4	-656.7	-912.5
Controls	Yes	Yes	Yes
Specialization dummies	Yes	Yes	Yes
Country dummy indicators	Yes	Yes	Yes
Year dummy indicators	Yes	Yes	Yes
Mean covariates	No	Yes	Yes
History covariates	Yes	No	No
Obs.	9,976	9,976	9,976

Note: This table shows estimates from equation (13). Columns (1) – (2) correspond to Wooldridge's CML model, including the history of the time-varying covariates. Columns (3) – (4) correspond to Wooldridge's CML model, including the mean of the time-varying covariates. Columns (5) – (6) correspond to a Probit model, including the mean of the time-varying covariates. The dependent variable is participation in organic farming (dichotomous). Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table B4. Diff-in-diff Estimates (Pillar One Subsidies)

Variable	*Dynamic Probit*		*Probit Model*
	(1)	(2)	(3)
Treatment × Pillar1×Yr05	0.0397	0.0299	0.0656
	(0.115)	(0.119)	(0.113)
Treatment × Yr05	-0.0933	-0.0923	-0.0284
	(0.391)	(0.405)	(0.414)
Treatment × Pillar1	-0.215*	-0.218**	-0.131
	(0.115)	(0.109)	(0.134)
Pillar1 × Yr05	-0.00648	-0.00543	0.0272
	(0.0539)	(0.0595)	(0.0601)
Treatment	0.554	0.331	-0.297
	(0.906)	(0.775)	(1.010)
Yr05	0.0695	0.0832	0.0941
	(0.342)	(0.356)	(0.381)
Pillar1	0.0175	0.0279*	0.0400
	(0.0510)	(0.0157)	(0.0802)
Log $\hat{\sigma}_a$	0.948***	1.540***	3.295***
	(0.315)	(0.184)	(0.100)
Log-likelihood	-598.6	-642.7	-870.5
Controls	Yes	Yes	Yes
Specialization dummies	Yes	Yes	Yes
Country dummy indicators	Yes	Yes	Yes
Year dummy indicators	Yes	Yes	Yes
History covariates	Yes	No	No
Mean covariates	No	Yes	Yes
Obs.	9,976	9,976	9,976

Note: This table shows estimates from equation (14). Columns (1)‒(2) correspond to Wooldridge's CML model including the history and the mean of the time-varying covariates, respectively. Column (3) corresponds to a Probit model, including the mean of the time-varying covariates. The dependent variable is participation in organic farming (dichotomous). Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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