THREE ESSAYS IN DEVELOPMENT AND ENVIRONMENTAL ECONOMICS

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To Hora, for his unconditional love and support.

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Contents

Introduction

The general aim of this dissertation is to understand how policies and interventions affect development, focusing on two recent initiatives in which there is still limited rigorous empirical evidence. First, I explore the impacts of environmental conditional cash transfer programs, namely Payments for Ecosystem Services (PES), which pay landowners to adopt land-use practices such as protecting forests. Although PES programs have become increasingly popular in recent years and have been seen as potential tools to achieve both environmental conservation and poverty reduction, their impacts are still not well understood. Second, I evaluate the impacts of affirmative action policies that seek to promote gender equality in political representation. Multiple countries have introduced some sort of gender quota for public elections, but surprisingly there are few quantitative evaluations, conducted in developing countries, that explore the causal effects of these policies on development.

The analysis presented in this dissertation is divided in two levels. The first level seeks to expand our understanding of how individuals react to the policies mentioned above and how policy design can be improved to maximize positive impacts. The assessment of PES programs explores how households' respond to these monetary incentives and proposes a strategy to increase their environmental and poverty reduction effectiveness. The second level of the analysis pays special attention to how leaders affect the development process by taking strategic decisions on the use of local resources. In particular, I explore how women politicians affect public expenditure choices that promote development. Moreover, I study how community leaders decide to allocate PES funds and how these decisions affect households' cooperation decisions. The next paragraphs present the motivation, objectives, findings, and main contributions of each of the chapters in this dissertation.

The first chapter analyzes whether monetary incentives modify cooperative behavior in activities that have been traditionally uncompensated. I analyze whether the introduction of PES in Mexican common property communities changes cooperation by exploring changes in households' participation and intensity of work in forest conservation activities, which for a long time have been done without compensation but that are now increasingly incentivized under PES. In addition, I explore changes in cooperation in non-forest community activities that remain uncompensated. The identification of the effects comes from comparing changes in behavior over time between households that belong to communities that applied to the PES program and were accepted, and those that belong to communities that applied to the program but were rejected. Parametric and non-parametric techniques on panel data are used to improved causal inference. Findings indicate that cash incentives increase cooperation in activities that are compensated, but that the framing of the incentive plays an important role in explaining cooperation in activities that remain uncompensated. As long as agents are exposed to sanctions resulting from deviant behavior and their actions are visible, lump-sum transfers without specific work conditionalities can be more effective than conditional payments to promote cooperation. This is the first study presenting rigorous empirical evidence on the impacts of PES programs on local level institutions and cooperation. Moreover, it contributes to the behavioral economics literature by presenting evidence that is based on real-life behavior.

The second chapter, which is co-authored with Prof. Jennifer Alix-Garcia and Prof. Katharine Sims, evaluates the impacts of PES on both environmental protection and poverty reduction. Despite the increased popularity of PES initiatives there are concerns about whether they can effectively generate avoided deforestation, and whether they can help the poor by providing cash incentives or harm them by restricting access to forest resources or new agricultural land. We estimate environmental impacts for the 2004-2009 Mexican PES program cohorts using annual measures of land cover from 2003-2011 and national program data. We evaluate wealth and investment impacts from a nationally-representative subsample of the 2008 cohort using survey data from 2007-2011. By using panel data on both program recipients and similar rejected applicants, we are able to control for possible omitted variables that are time invariant as well as for time trends affecting both groups. This paper contributes to the existing literature by providing new evidence across multiple years on the environmental effectiveness of a national PES program, which is currently only available for Costa Rica. Moreover, the wealth impacts of avoided deforestation programs have not been evaluated directly or compared to environmental impacts yet. Finally, we consider how the heterogeneity in impacts across spatial characteristics determines possible tradeoffs between the dual goals. We find that the program reduces the expected loss of land cover by 40-50 percent without affecting household wealth. Environmental gains are higher where poverty is low while household gains are higher where deforestation risk is low. This illustrates the difficulty of meeting multiple policy goals with one single tool.

The third chapter studies the impacts of increasing female representation in Bolivian municipal councils on public policy choices and welfare outcomes. In recent years, the increase in the number of women involved in politics has catalyzed some active debate about whether increasing female representation actually affects policy determination. In 1999 Bolivia implemented a gender quota system that required political parties to construct their lists of candidates for municipal councillors with at least 30% of women. Moreover, the law established a specific order for men and women in these lists. Municipal governments in Bolivia are interesting units of analysis since they enjoy significant independence from the national government and are the sole providers of important services, such as health, education, and basic infrastructure, among others. I compiled a detailed panel dataset with information on municipalities' expenditures, revenues, and multiple development indicators that spans from 2000 to 2010. This information has been complemented with detailed electoral data on the list of candidates, their identity, and the number of votes received by each party. An innovative identification strategy is proposed, which is based on a regression discontinuity design (RDD) that is applied to systems of proportional representation. More specifically, I exploit the pre-determined position of women in the list of candidates together with small differences in the distribution of votes that lead some parties to gain certain amounts of seats which guarantee the presence of a woman in the council. Findings indicate that municipalities with women councillors devote more resources to social investments and, in particular, they prioritize education, health, and environmental protection expenditures giving less attention to infrastructure investments. The impacts of female representation appear only some years after the elections and there is weak evidence on the links between changes in public policy choices and final outcomes. Besides the novel RDD strategy, this paper contributes to the literature by providing additional evidence from a developing country. So far, the only available studies on this topic in a developing country context come from India. Moreover, by looking at final outcomes, this chapter makes an attempt to respond to broader questions about public policy effectiveness and the relationship between gender equality and development.

Chapter 1

Cash for cooperation? Payments for Ecosystem Services and common property management in Mexico

1.1 Introduction

Traditional economic theory assumes that individuals are selfish and rational, and argues that higher monetary incentives will inevitably lead to more effort or higher performance. In recent years, a growing literature has argued that different non-pecuniary motives may shape human behavior and interact with economic incentives in unexpected ways (Akerlof 1980, Selten 1990, Rabin 1993, Ostrom 1998, Benabou & Tirole 2006). Unexpected behaviors in response to monetary incentives are often observed in activities that are traditionally unpaid, such as donations, contributions to charity, or community service (Gneezy & Rustichini 2000, Heyman & Ariely 2004, Ariely et al. 2009, Carpenter & Myers 2010). While there has been considerable experimental work analyzing these issues, studies exploring real-world situations are still rare. This study uses data collected in a non-laboratory setting to explore whether introducing monetary incentives for forest conservation, a traditionally

I am very grateful to Jennifer Alix-Garcia, Volker Radeloff, Elizabeth Shapiro, and Katharine Sims for giving me the opportunity to collaborate with them, giving me helpful feedback, and making much of this work possible. I am also very thankful to Laura Schechter, Brad Barham, students in the AAE Research Colloquium and JL research group meetings. I am specially grateful to the Graduate School at the University of Wisconsin, the International Initiative for Impact Evaluation (3ie), and the National Science Foundation (NSF) for financial support. Thanks to participants at the BREAD-IDEAS summer school in development economics, IZA summer school in labor economics, Development Economics Workshop at UW-Madison, Midwest International Development Conference, AERE summer meeting, and AAEA annual meeting for very helpful comments.

unpaid activity in the setting of study, modify cooperative behavior within common property communities. Moreover, it explores whether the framing of the incentive has a differential impact on behavior.

I exploit a unique situation currently happening in Mexico, where approximately 80 percent of the forest land is managed by common property communities (Bray et al. 2005), and where also one of the largest Payments for Ecosystem Services (PES) programs is currently being implemented (Muñoz Piña et al. 2008). In recent years, PES programs have become increasingly popular as mechanisms to mitigate climate change by providing landowners with cash or in-kind incentives in exchange for changing their land-use practices to provide an environmental service (Wunder et al. 2008*a*). One unique feature of communal PES funds in Mexico is that they are increasingly being used to promote work in forest conservation activities, which have historically been uncompensated and driven by social norms of cooperation. The literature so far has given little attention to the possibility that PES might change the logic of collective action (Kerr et al. 2011, Vatn 2010), harming or encouraging cooperation both in activities that become paid due to PES and those that remain unpaid. Mexico also provides an ideal setting in which to study the behavioral effects of incentive design, since there is heterogeneity in the distributional arrangements adopted by community leaders after a PES contract.

The paper begins by modeling a principal-agent framework, where leaders decide the optimal allocation rule for PES income anticipating households' behavior, and households choose the extent of their cooperation based on the incentive scheme chosen by the principal. I distinguish two types of distributional arrangements: an equal distribution of funds among all members without specific work conditionalities, and the provision of wages to members that work in some specific forest conservation activities. Assuming non-individualistic utility functions, the model shows that certain monetary incentives change the enforcement of social norms of cooperation making free riding behavior more or less acceptable. This hypothesis is based on empirical studies that suggest that providing economic incentives to people for obeying social norms may actually weaken norm enforcement (Fehr & Falk 2002). Model predictions indicate that, as long as households are exposed to sanctions and their actions are visible, transfers without clear work conditionalities reinforce social norms of cooperation and increase the time allocated to all types of community work. In contrast, wages weaken social norms of cooperation by signalling the market value of community labor, leading households to reallocate time only to paid work.

To test model predictions, I use community and household-level data collected from accepted and matched rejected applicants to the Payments for Hydrological Services program (PSAH) in Mexico. This novel dataset is one of the first large-scale datasets available to study the impacts of a PES program. Moreover, it has been complemented with qualitative case studies that help clarify the reasons underlying behavior. To analyze the impacts of monetary incentives on cooperation, I exploit variation over time in community access to PSAH and quantify effects both in the decision and intensity of work in community activities. Two types of community work are distinguished: one related to forest conservation, which is incentivized under PSAH, and non-forest work that remains unpaid but benefits the entire community, such as building and maintaining communal infrastructure. Using data from matched rejected applicants as the counterfactual case, allows to control for key unobservable characteristics that may simultaneously influence program enrollment and collective action at the community level. This empirical strategy constitutes an improvement over recent impact evaluations of PES programs around the world, which have used non-applicants as their control group (Uchida et al. 2009, Pfaff et al. 2008, Sanchez-Azofeifa et al. 2007*a*, Arriagada 2008). To the best of my knowledge, this is also the first study testing the impacts of PES on cooperative behavior within common property communities.

To analyze the impact of different incentive schemes on cooperation, I exploit variation in distributional arrangements across communities that participate in the PSAH. I start by analyzing how different communities determine the internal distribution of PES benefits. Then, I employ parametric and non-parametric approaches to account for the endogeneity in distributional decisions. More specifically, inspired by work that looks at the impacts of microfinance on certain groups of the population (Banerjee et al. 2010), nearest neighbor covariate matching is used to select communities in the control group that would have adopted a similar distributional rule in case they had benefited from PSAH. Results are robust to using instrumental variables estimation and instrumenting the distributional rule at the community level with the distributional rule observed in other communities within the same state but excluding the closest geographical neighbors to reduce the possibility of spillover effects. They are also robust, to predicting the distributional rule in a first step using a parametric estimation and bootstrapping standard errors. Heterogeneity in impacts across the visibility of actions is evaluated taking into account how labor-intensive and observable are the outcomes or products of different types of forest conservation activities. Finally, we consider land-use rights or membership status within the community as an exogenous measure of exposure to sanctions.

Findings indicate that monetary incentives increase cooperation in forest conservation activities, both in the intensive and extensive margins. More specifically, there is an increase of approximately 20% in both participation rates and number of days worked for households with land-use rights. In spite of these positive effects, monetary incentives on average don't have positive spillover effects on community work that remains unpaid. The results on incentive design are robust and consistent across both parametric and non-parametric methodologies and show that, as long as agents are exposed to sanctions and their actions are visible, transfers without clear work conditionalities are more effective than wages to increase the intensity of work not only in the forest, but in other unpaid community activities.

Full redistribution increases the days worked in the forest and in other unpaid community activities by approximately 50%. Finally, the empirical evidence shows that the increase in cooperation leads to small reductions in households' participation in own production activites, but there are no differential effects when considering the framing of the incentive.

Findings from this study enrich the current literature in two ways. First, within the behavioral economics field, they reinforce the idea that when behavior is driven by non-pecuniary motives monetary incentives could have unexpected effects. Therefore, understanding how incentive design interacts with behavior is essential. Second, within the environmental economics field, I provide evidence of how recent strategies that promote the conservation of natural resources in the marketplace can change collective action within common property communities. Future financial flows from developed to developing countries for REDD programs, within wich PES are key tools, are predicted to be close to US $30 billion a year (UN-REDD-Programme 2012). The results of this paper are relevant for the efficiency of PES programs, as they suggest that in contexts with strong institutions payments without specific work conditionalities could be more effective to promote forest conservation. Moreover, they are relevant for the sustainability of common property communities, particularly when the provision of public goods depends on households providing free labor to the community, and indicate that PES could be important tools to reinforce or weaken cooperation.

The paper is organized as follows. The next section discusses in detail how the paper fits into the current literature and the main contributions. In section 3 and 4, an overview of the PSAH Program and description of the data is presented. Section 5 explains the theoretical framework and testable predictions. Section 6 discusses the empirical approach and results, and section 8 presents the conclusions and main policy implications.

1.2 Relation to existing literature

Following Titmuss (1971) argument that individuals are more willing to donate blood voluntarily than when they are offered a monetary compensation, a growing literature has argued that different non-pecuniary motives may shape human behavior and interact with cash incentives in unexpected ways. A first group of studies discusses, both theoretically and empirically, the crowding-out effect of intrinsic motivation by extrinsic incentives. The main argument is that individuals undertake certain activities because they derive satisfaction from doing them; therefore, monetary compensation may reduce the effort or the time devoted to them (e.g Deci 1971, Lepper et al. 1973, Andreoni 1990, Frey & Oberholzer-Gee 1997, Frey & Jegen 2001). Many of the authors within this literature have used reduced-form models assuming a direct negative link between incentives and motivation. As Benabou & Tirole (2006) argue, a more discriminatory analysis is needed, as it is difficult to always assume that incentives will crowd-out motivation, and there are many examples, particularly in the labor literature, showing that incentives do work (Prendergast 1999, Lazear 2000).

A second group of studies highlights the importance of social norms as drivers of behavior. Within this literature, several authors have pushed forward the construction of a new behavioral theory where individuals are boundedly rational, there is moral behavior, and concerns about social approval (Akerlof 1980, Selten 1990, Rabin 1993, Ostrom 1998, Lindbeck et al. 1999, Benabou & Tirole 2006). There are multiple social norms that may interact with economic incentives in unexpected ways. Some empirical studies discuss norms of reciprocity under a principal-agent framework and show that if the agent perceives the actions of the principal as kind then she values the pay-off positively. On the contrary, if the actions are perceived as hostile then she values the pay-off negatively (Fehr et al. 1997, Fehr & Falk 2002). Other authors discuss concerns of social reputation or self-image. In this case, individual behavior should follow closely what society rewards or defines as appropriate (Fehr & Gachter 2000, Ariely et al. 2009, Carpenter & Myers 2010). Then, if monetary incentives reduce the social rewards attributed to a specific activity[1], reducing the effort you put into it could be a strategy to preserve one's reputation.

The present paper finds itself in the second group of studies proposing that social norms drive behavior. This theory of behavior is particularly important for collective action problems (Ostrom 1998, Vatn 2009) and characterizes natural resource management in many rural areas of the developing world (Baland & Platteau 1996). The main argument behind our theoretical approach is that cash incentives change the enforcement of social norms by making free riding more or less acceptable. A survey of the literature presented by Fehr & Falk (2002) suggests that rewarding people monetarily for obeying social norms may weaken norm enforcement and lead to the gradual erosion of norm-guided behavior. More specifically, Gneezy & Rustichini (2000) show that introducing a monetary fine for late-coming parents at day-care centers increases the number of late-coming parents. In this case, the fine changes the rules of behavior making it more acceptable to leave your child beyond the official collection time. Finally, Fuster & Meier (2010) present laboratory experimental evidence, based on public goods games, and show that free riders are punished less harshly when incentives are in place, which in fact leads to reductions in the average contribution to public goods.

This study also explores how incentive design affects cooperative behavior. Within this literature, several authors have showed that the nature of incentives is important. For example, Heyman & Ariely (2004) propose that in situations that are framed as social, such as helping someone to move, monetary incentives diminish the perceptions of the interaction as social, reducing the help provided when compared to in-kind incentives. The framing

[1] For example, by providing a monetary compensation for blood donation, then the act of donating may not be as socially valuable as before. It is actually possible that those that donate could be seen as interested only in money.

of incentives has also been discussed within contract theory literature. Some studies have shown that, in the presence of reciprocally fair actors, implicit contracts work better than explicit contracts in promoting higher levels of cooperation (Fehr & Schimdt 2000, Fehr & Gachter 2002).[2] A closely similar finding is presented by Rand et al. (2009) who show that reward outperforms punishment in repeated public goods games. The work at hand is most closely related to the contract literature since two types of monetary incentives are compared: lump-sum transfers, which could be conceived as implicit contracts, and wages, which are more closely related to explicit contracts.

In contrast to the studies reviewed before, most of which have used laboratory or field experimental evidence, this study looks at real-life behavior. Although experiments have the advantage of offering a clean identification of the effects, there is considerable debate around the external validity of these findings. This skepticism is based upon two observations. First, populations studied (usually college students in laboratory experiments) might behave very differently from the populations of interest. Second, even if populations are very similar, as it is the case of field experiments, the behavior of people in games can differ from their real-life behavior. In this sense, by using data collected in a non-game non-lab setting, the results in this paper constitute an improvement in terms of external validity. It is also important to mention that in most experimental evidence incentives are exogenously given and set by the researcher. In this paper, I exploit the unique situation in Mexico where the incentive design emerges organically from the relationship between community leaders and households. Although analyzing this principal-agent interaction is very interesting itself, I do acknowledge it may come at the cost of a less clean identification strategy, and this is why any inference needs to be based on multiple robustness checks.

[2] In the explicit contract the principals explicitly conditioned a fine on the agent's deviation from a desired effort level. In the implicit contract they promised a bonus after the effort was observed. The promise was not binding and it was just considered cheap talk.

Within the environmental economics literature, and to the best of my knowledge, this study is the first to formally test the effects of PES on cooperation decisions in common property management. Given the limited availability of household and community-level data from beneficiaries and non-beneficiaries of PES programs, we still know very little about the socioeconomic impacts of these programs. There is an emerging literature, mostly using data from the China's Sloping Lands Conversion Program that shows that, by releasing credit constraints, cash incentives promote more off-farm labor (Groom et al. 2007, Uchida et al. 2009). In this particular paper, the focus is on the specific labor decision of cooperating in community work. In terms of the distributional arrangements of PES funds, there are a few studies that discuss how the distribution of these incentives within communities influences households' perceptions about benefits and their use of environmental resources. Sommerville et al. (2010) use qualitative data and semi-structured interviews in Madagascar and look at changes in attitudes and behavior related to forest use using information from households living in communities that participate in a PES program. Their main finding is that changes in behavior were not caused by higher monetary incentives, but by more intensive monitoring and punishment. Vatn (2010) is probably the first one to discuss the possible effects that PES programs can have on cooperative behavior, by changing households' logic from doing what is considered appropriate to what is individually best to do. The author highlights the importance of the institutional context and the framing of the incentives, but unfortunately does not use any particular data to support his argument.

A recent study by Kerr et al. (2011) suggests the possibility that incentive payments coming from PES might influence collective action to manage common property. To evaluate this, authors run experiments in communities participating in a PES program, both in Mexico and Tanzania[3], and conclude that providing cash incentives raises participation

[3]In their experiments in Mexico, they provide some randomly selected households living in communities participating in a PES program three different treatments. In the first one, a payment was never offered or mentioned; in the second one, an individual cash payment was offered for their cooperation; in the third one, a payment to support the village was offered for each participant.

where people are otherwise uninterested, but that participation is always high when social norms about cooperation are strong. This study differs from theirs in three ways. First, although Kerr et al. (2011) is looking at real-life behavior, there is an important element about principal-agent interaction missing. In particular, in their study the researcher sets the incentive and interacts with households directly; moreover, the payment is offered for a single day of work. In real-life, community leaders are the ones who design the incentive and cooperation is repetitive. There are reasons to believe that households might behave differently when confronted by an outsider and also when cooperation is only related to a single day of work. A second distinction is that Kerr et al. (2011) only consider participation decisions, while this study looks at both the participation decision and the intensity of work. Analyzing both outcomes is important in contexts where community work has a long history, since it might be difficult for households to shirk completely, therefore only changes in the intensity of their participation are expected. Finally, the present study looks not only at how incentives change participation in activities where monetary compensation is provided but, most important of all, how they affect cooperation in activities that remain unpaid. As suggested theoretically by Holmstrom & Milgrom (1991), when individuals are confronted with multiple tasks but incentives are given only for some of them, we might observe a re-allocation of labor, particularly when performance in unpaid tasks is difficult to measure.

1.3 The Payments for Hydrological Services Program and common property communities in Mexico

The Payments for Hydrological Services program (PSAH) was first implemented by the National Forestry Commission (CONAFOR) in 2003 with the objective of increasing the production of hydrological services by promoting forest conservation.[4] For this, five year

[4]Hydrological services coming from forest protection are all those benefits that forests can bring to hydrological resources, such as regulating the hydrological regime, maintaining and improving water quality,

renewable contracts are signed both with individual and communal landowners. To set payments, forests are classified according to their importance for aquifers and watersheds. During the period studied (2008-2011), annual payments of US$ 27 per hectare of forest enrolled and US$ 36 for cloud forests were given[5]. The minimum amount of land required to enroll is 20 hectares and the maximum is 3000 hectares. Participants need to maintain forest cover and monitoring is conducted by satellite image analysis and ground visits[6]. To maintain forest cover, participants are encouraged to perform some forest conservation activities, such as constructing firebreaks, doing forest patrols, or constructing fences to avoid cattle coming into the forest. The program does not impose specific requirements on the types and intensity of forest conservation activities, which results in significant heterogeneity across beneficiaries in the effort they put into these activities.

The Mexican PSAH program is currently one of the largest PES schemes in the world. Between 2003 and 2011, approximately 2.7 million hectares of forestland were entered into the program and more than US$450 million of federal funds were distributed to 3,979 communal or smallholder private property participants (CONAFOR 2012). In 2008, approximately 45% of PSAH program recipients were common property communities, including "ejidos", which are federally recognized common property holdings with land tenure and governance rights granted to a set number of households, and "comunidades", which are indigenous lands. For this reason, the program is not only unique in terms of its poverty reduction potential[7], but offers the opportunity of examining how cash incentives for forest conservation

controlling erosion and sedimentation, reduction in soil salinity, etc.

[5]Payment rates were originally based on approximate calculations of the average opportunity cost of land conversion from forest to maize crops. They have been updated taking into account inflation (Shapiro & Castillo 2012).

[6]Landowners are removed from the program or payments are reduced if there are signs of deforestation in the enrolled land; moreover, payments are reduced if there is forest loss caused by natural causes, such as fires or pests (Muñoz Piña et al. 2008)

[7]PES programs in many countries benefit private landowners which are not necessarily at the bottom of the income distribution. In the Mexican case, communal property allows very poor households to access these benefits. According to data presented by CONAFOR, in 2006, 78% of payments went to forests owned by people living in population centres with high or very high marginalization rates. Moreover, according to data from the National Institute of Statistics, in 2004, 31% of the PSAH recipients had incomes below the

interact with common property management decisions. Program payments are given at the community level and there are no conditions on how communities should allocate funds at the local level. The only condition is that the forest cover needs to remain unchanged and communities should perform the forest conservation activities they propose to do when they enroll in the program.

Mexican common property communities resulted from a land reform that extended from the end of the 1910 Revolution until the early 1990s. During this time, an area equivalent to half the country was redistributed to peasants organized in communities, most commonly known as ejidos (Assies 2008). Ejidos are composed of two different kinds of property rights over land: private parcels and commons. Private land is mostly used for agricultural activities, while the commons are mainly dedicated to pasture and forest. Many people who are not full ejido members live within these communities, usually descendants of the original members who are denied membership rights by the legal restriction on inheritance to only one child. Non-members do not formally have voting rights or land, but in practice they often farm on lands ceded by others or illegally taken from the commons (Alix-Garcia et al. 2013). Authority in Mexican common property communities is well defined and divided into three bodies. The first one is the "asamblea", which is the principal decision-making body and where all members participate and vote. The second one is the "comisariado", which is the executive body and is composed by a president, a secretary, and a treasurer. The third one is the "consejo de vigilancia", which monitors the activities of the comisariado.

In most Mexican common property communities there is an old tradition of performing community work, which consists of non-paid activities that benefit all (VanWey et al. 2005)[8]. Some examples of community work are cleaning roads, painting schools, or building

extreme poverty line.

[8]Uncompensated community work receives different names in different regions, some of the most used names for these activities are: tequio, faena, fajina, fatiga, etc.

communal infrastructure. For communities that possess large amounts of forest land, forest conservation activities are usually important components of community work. In general, participation seems to depend on what community rules dictate about households' participation in community work and their enforcement. Over time, community work has proved to be very important for the provision of many public goods and for the subsistence of communities (Mutersbaugh 2002).

1.4 Data

I use primary household and community-level data collected in 4 different regions of Mexico between June and August of 2011. The data collected is part of a larger project that evaluates the environmental and socioeconomic impacts of PSAH.[9] The survey covers both beneficiaries and non-beneficiaries from the 2008 PSAH cohort. A stratified random sampling strategy was applied both by region and land-use rights. In a first step, four regions were selected (north, center, south west, and south east) based on dominant ecosystem type and socio-economic groupings. Within each region, several Landsat footprints (areas of 180X180 sq km) were randomly selected out of the subset of footprints with sufficient satellite imaginery available to measure past land-use change.[10] Within the geographical areas covered by these footprints, we identified all communities that applied to the program in 2008 and were accepted (treatment group), and matched them to communities that also applied to the program in 2008 but were rejected (control group).[11] During sampling, priority was then given to communities that had multiple good matches among the controls and vice versa within each region. Reasons for program rejection are mostly based on ob-

[9]For more details, please refer to Alix-Garcia et al. (2013)

[10]A footprint is a picture or an image of a certain area or portion of the earth taken by the Landsat satellites as they continuously pass around the earth.

[11]Nearest neighbor covariate matching was conducted applying the Mahalanobis metric within region and on the basis of the following covariates: distance to the nearest locality with population greater than 5000, elevation, slope, the area of the property submitted to be enrolled, the density of roads within a 50 km buffer, the average locality poverty level in 2005, and the percentage of submitted forest in coniferous forest, oak forest, cloud forest, upland and lowland rainforest.

servable characteristics. According to program data, some of the most important reasons are having less than the required minimum forest cover (50%)[12], limited funds from the program (35%)[13], being outside of the eligible zone (6%) and missing documentation (9%). In a second step, surveyors further stratified the sample within common property communities by land-use rights. Based on lists provided by program officers or community leaders, surveyors randomly selected 5 households with full land-use rights ("members") and 5 without them ("non-members"). The final sample is composed of 1056 households (557 beneficiaries and 499 non-beneficiaries) distributed over 111 common property communities[14]. Figure 1 and Table 1.1 show how the sample is distributed across regions and the footprints selected.

Both household and community surveys are quite comprehensive. In order to have baseline measurements, surveys included recall questions from 2007, which is the year previous to program implementation. No reference to the program was made when asking questions from the past in order to reduce potential recall bias. By having information from two different points in time (i.e. 2007 and 2011) we are able to construct a panel data set. In most cases, the household head responded to the survey [15]. Most questions are related to household-level information, such as household assets, access to land, and production de-

[12]The minimum forest cover required in 2008 was 50% (Sims et al. 2014)

[13]The criterion used by the program to select properties when funds are restricted is based on the applicants' score. This score is constructed taking into account the forest cover, whether the property is located in a natural protected area, the level of exploitation of aquifers, the index of risk of deforestation, the level of poverty and indigenous population in the municipality where the property is located, whether the applicant is a woman, among others. When multiple applicants have the same score, priority is given to common properties and then to those located in municipalities of high social interest as defined by the Secretary of Social Development, among the most important criteria (Alix-Garcia et al. 2013)

[14]This is a subsample of the total number of households surveyed. The total sample includes private landowners, which are excluded from this analysis. We also drop households for whom we do not know their land-use rights status. It is important to mention, that despite having a land-use rights stratification at the community level, the final number of households in the sample is not divisible by 10 and the number of members and non-members is not exactly the same. There are two explanations. First, in some small communities there were less than 10 households in total. Second, some communities only had households with land-use rights.

[15]When the household head was not present, surveyors tried finding him or her in the field or in the forest, or went back later during the day or the week to the house. If this was not possible, the survey was applied to the partner. When none of them was available, the survey was responded by an adult member in the household.

cisions, among the most important. We also collected detailed individual-level information about education, migration, and employment decisions. Questions related to community work and participation in forest conservation activities were asked at the household level. For forest work we have data for both years, for non-forest unpaid community work we only have data for 2011. The community survey was applied to a group of community leaders and included questions about community characteristics as well as questions about decisions related to the use and distribution of PSAH funds. The community survey also included questions about the number of activities and the number of households that participate in community and forest conservation activities.

1.5 Theoretical framework

To provide intuition about the possible mechanisms driving behavior, the relationship between community leaders and households is modelled within a single period principal-agent framework[16]. The model is specific to the context of the study, but borrows ideas from multiple models in the literature, such as those looking at the relationship between incentives, social norms, and behavior (Akerlof 1980, Lindbeck et al. 1999, Huck et al. 2001, Benabou & Tirole 2006); collective action within common property management (Baland & Platteau 1996); and leader-household interaction within Mexican common property communities (Alix-Garcia et al. 2005).

1.5.1 Community leaders' problem

In every period leaders observe the total funds available to the community. I assume the community only gets funds from the PES program, which promote the increase in forest conservation activities, such as opening or maintaining firebreaks, constructing fences to avoid cattle coming into the forest, and doing forest patrols, among others. Therefore, total

[16]To simplify things and given that we don't have empirical data to test dynamics, we do not construct a dynamic model. When multiple periods are considered, risk considerations will need to be included.

community income is given by $p^f F$, where p^f is the program payment per hectare of forest land and F is the total area of forest in the community. For simplicity, all forest is assumed to be enrolled in the program. Therefore, I do not model the decision on how much land to enroll, but focus on how PES funds are allocated. Within the period, leaders also determine the total number of days of community work $L^t(p^f F, Z_t)$ required for the well-functioning of the community. Community work involves both forest and non-forest activities; this is why this decision depends not only on the total payment received from the program ($p^f F$) but also on other community characteristics (Z_t)[17].

Since leaders need to encourage forest conservation activities, they can choose from two strategies when deciding how to allocate PES funds. The first is to divide equally the payment among households, making an implicit agreement of cooperation but without imposing specific work conditionalities. The second is to pay a daily wage to households that work in some pre-determined forest conservation activities. Choosing a mixed of these two strategies is possible[18]; therefore, the main decision leaders confront involves choosing the proportion of funds that will be distributed directly to households as lump-sum transfers (γ) or, inversely, the proportion of program's income that will be used to provide wages ($1 - \gamma$).

Leaders spend all funds available in a given period, this means there are no savings considerations in the model. Given the proportion of funds distributed directly as lump-sum transfers (γ), the total number of community work required (L^t), the funds available ($p^f F$),

[17] These could be not only geographical characteristics that affect the need for more or less work, such as the weather, slope, total area, but also social characteristics, such as how cooperative is the community or how ambitious are leaders.

[18] This is a simplified description of leaders' problem, but follows what the majority of communities surveyed in our sample do. I rule out the possibility of investing in public goods or of leaders keeping the money for themselves. As explained before, authority in Mexican communities is well structured and composed by different groups that continuously monitor each others' work; therefore, the assumption is that leaders' decisions cannot be driven by private interests. Moreover, most public investments, if any, that use PES funds, take place in the first years of the program; therefore, given that we have data from communities already in their third year of implementation we don't see these types of investments often.

the local wage (w), which is assumed to be fixed[19], and the number of households in the community (N), leaders calculate the amount they can transfer to each household (B), the maximum number of working days that can be paid (L^p_{max}) and those that will have to be done unpaid (L^u_{max}):

$$B = \frac{\gamma p^f F}{N} \tag{1.1}$$

$$L^p_{max} = \frac{(1-\gamma)p^f F}{w} \tag{1.2}$$

$$L^u_{max} = L^t(p^f F, Z_t) - L^p_{max} \tag{1.3}$$

Given this information, leaders need to choose the optimal proportion of funds that will be distributed (γ) in order to maximize community's net benefits. Benefits are represented by a function g, which is increasing in the total number of days worked, both paid and unpaid, by households in the community ($g(\sum_{i=1}^N l_i^u + \sum_{i=1}^N l_i^p)$)[20]. The allocation decision also has some costs. First, there are monitoring costs $C(\theta_i, \alpha, \gamma)$, which are household specific, and are a decreasing function of a household's level of exposure to sanctions ($\frac{\partial C}{\partial \theta_i} < 0$), they are also decreasing in the visibility of work ($\frac{\partial C}{\partial \alpha} < 0$) and increasing with the proportion distributed ($\frac{\partial C}{\partial \gamma} > 0$). Second, there are costs of designing explicit labor contracts $D(\gamma, Z)$, such as deciding which activities are going to be paid and who is going to do the work. These costs are a decreasing function of the proportion of funds distributed directly ($\frac{\partial D}{\partial \gamma} < 0$) and are a decreasing function of a community characteristic (Z_c) that measures the level of information, assistance, or capacity leaders have to design explicit labor contracts ($\frac{\partial D}{\partial Z_c} < 0$). To summarize, the community's net benefit function is given by:

[19] The data supports this assumption given that the reference value for wages paid in FCA is given by the wage paid in agricultural work. According to the data, in 2011 the average daily wage paid for both agricultural and forest work was 10 US$

[20] We can think of this function as mapping the intensity of work into some kind of welfare outcome, for example, better environmental quality or improved access to basic services.

$$\Phi = g(\sum_{i=1}^{N} l_i^u + \sum_{i=1}^{N} l_i^p) - D(\gamma, Z_c) - \sum_{i=1}^{N} C_i(\theta_i, \gamma) \tag{1.4}$$

To calculate benefits, leaders must anticipate households' labor reaction functions[21]. They know that paid labor decisions are determined by the maximum number of paid days that they can offer per household as well as some households characteristics, such as their outside options $l_i^p(\frac{L_{max}^p}{N}, X_i)$. The number of unpaid days are determined by the maximum number of unpaid days per household established by leaders ($\frac{L_{max}^u}{N}$). In addition, they depend on how exposed to sanctions, resulting from deviant behavior with respect to the maximum number of unpaid activities required, households perceive to be (θ_i). In this setting, we can think of sanctions as either material costs (e.g. fine) or social costs (e.g. stigma or shame) resulting from deviant behavior. Unpaid work also depends on how visible the activities are (α), the quantity of the transfer that they receive (B), and other household characteristics X_i. This means $l_i^u(\frac{L_{max}^u}{N}, \theta_i, \alpha, B)$, where θ_i and α are parameters distributed between zero and one. Leaders know that unpaid labor is increasing when households feel more exposed to sanctions ($\frac{\partial l_i^u}{\partial \theta_i} > 0$), when their actions are more visible ($\frac{\partial l_i^u}{\partial \alpha} > 0$), and when they receive monetary incentives to cooperate ($\frac{\partial l_i^u}{\partial B} > 0$).

The leaders' maximization problem, after replacing all the information presented above,

[21] This assumes leaders have full information about households' behavior. Moreover, we assume households cannot individually manipulate the distributional rule choice except through their labor supply decisions. Although households participate in community meetings and have the right to vote for some important community decisions, there is heterogeneity in their involvement and participation in these meetings. In general, leaders seem to be the ones proposing and making the most important decisions for the community. Fieldwork has shown that many households living in communities that participate in the PSAH program are not familiar with it. Approximately, only 50% of households in these communities are aware of the program, and the majority don't know the details about payment size or how the program operates. Moreover, households don't have direct contact with outside organizations. Within this context, the common practice is for leaders to decide how to allocate program funds, then they present their proposal in the asamblea, and usually most households accept it.

is:

$$\max_{\gamma} \quad g[\sum_{i=1}^{N} l_i^u(\frac{L_t(p^f F, Z_t)}{N} - \frac{(1-\gamma)p^f F}{wN}, \theta_i, \alpha, \frac{\gamma p^f F}{N}, X_i) + \sum_{i=1}^{N} l_i^p(\frac{(1-\gamma)p^f F}{wN}, X_i)]$$

$$-D(\gamma, Z_c) - \sum_{i=1}^{N} C_i(\gamma, \theta_i, \alpha) \quad (1.5)$$

$$s.t. \quad 0 \leq \gamma \leq 1$$

With no binding constraints, the first order conditions (FOC) are:

$$\sum_{i=1}^{N} \frac{\partial g}{\partial l_i^u} \frac{\partial l_i^u}{\partial \gamma} - \frac{\partial D}{\partial \gamma} = \sum_{i=1}^{N} \frac{\partial g}{\partial l_i^p} \frac{\partial l_i^p}{\partial \gamma} + \sum_{i=1}^{N} \frac{\partial C_i}{\partial \gamma} \quad (1.6)$$

The FOC indicate that leaders will choose to distribute the proportion of income that will allow them to equalize the marginal benefits of increasing distribution, which are derived from promoting unpaid community work and reducing contract design costs, with the marginal costs, which come from the reduction in labor devoted to paid activities and the increase in monitoring costs needed to sustain the cooperation agreement. Solving equation (1.6) gives the optimal distributional rule:

$$\gamma^* = \gamma(p^f, F, w, N, Z, \Theta, \alpha, X) \quad (1.7)$$

where Z aggregates all community characteristics that affect both costs and labor decisions, and X and Θ are summary measures of households' characteristics and their level of exposure to sanctions in the community. If constraints are binding, it is easy to see that those that decide to distribute all funds ($\gamma = 1$) are those for whom benefits from distribution are larger than costs, probably because the probability that households will deviate from the required activities is low and/or because it is too costly or difficult to design an schedule of payments or contracts. As opposed, communities that choose not to distribute any of the funds ($\gamma = 0$), are those whose costs exceed the benefits of distributing. These are probably communities where households are not exposed to sanctions, actions are not very visible, and therefore monitoring costs are very high.

1.5.2 Households' problem

A household i decides how much time to allocate to paid community activities (l_i^p), unpaid community work (l_i^u), and own production activities (l_i^o) to maximize its net benefit. The total endowment of time is given by T and no leisure is assumed to exist. Participating in any type of work entails a cost of $c_o(l_i^o)$, $c_p(l_i^p)$, and $c_u(l_i^u)$. Cost functions are convex in the amount of time allocated to each activity ($c_j'(l_i^j) > 0,\ c_j''(l_i^j) > 0$ and $j = \{o, p, u\}$).[22] Paid community activities yield a monetary reward w, which is fixed and determined at the community level based on local labor market conditions. Households get benefits from aggregate community work, and this is represented by a concave function $g(\sum_{i=1}^{N} l_i^p + \sum_{i=1}^{N} l_i^u)$ on the amount of time allocated, by all N households, to paid and unpaid community work ($g'(.) > 0,\ g''(.) < 0$). The function of benefits from community work $g(.)$ is the same that leaders observe, but here we assume there is heterogeneity in how much households can gain from these benefits. This is captured by a variable a_i, distributed between 0 and 1, that scales up or down the benefit function $g(.)$[23].

Households can also get benefits from own production activities. We can think broadly of own production activities as work that is done in agriculture, livestock activities, or off-farm employment. Households can sell their production for a unit price of p_i, which varies across households and can be considered a measure of outside wage or opportunity costs. B is the amount of lump-sum transfers the household receives from community leaders, and it is given by equation (1.1); therefore, it is increasing in the share of PES funds that leaders

[22]The convexity assumption for the costs functions is usually used the in literature, such as Benabou & Tirole (2006), Holmstrom & Milgrom (1991), Carpenter & Myers (2010)

[23]For example, if work in forest protection results in increased water supply or water quality, then the value of a_i will be higher for households working in agriculture when compared to those working off the farm.

distribute ($\frac{\partial B}{\partial \gamma} > 0$), but decreasing with the population size ($\frac{\partial B}{\partial N} < 0$).

I assume the community has a cooperation social norm that indicates that households should perform all unpaid community work required by leaders, and that leaders distribute the work equally among all households ($l_i^u = \frac{L_{max}^u}{N}$). Deviations from this norm negatively affect households' benefits and, from now on, they will be called sanctions. As mentioned before, sanctions can be either material or social costs. There is heterogeneity in the level of sanctions households experience and this is represented by a function $v_i(\theta_i, \alpha, B)$, which is increasing in the level of exposure to sanctions ($\frac{\partial v_i}{\partial \theta_i} > 0$), the visibility of actions ($\frac{\partial v_i}{\partial \alpha} > 0$), and the amount of lump-sum transfers received ($\frac{\partial v_i}{\partial B} > 0$)[24].

The sanction function is a central element in the model and it is inspired by models that propose a non-individualistic perspective of utility. In these models social sanctions enter the utility function to help enforce social norms or codes of behavior (Akerlof 1980, Lindbeck et al. 1999). In this model, there are two extra elements. First, I assume there is heterogeneity in households' exposure to sanctions. Second, I assume an explicit relation between the level of sanctions and different types of monetary incentives. The basic intuition is that when leaders increase the transfers provided to households, and given that they involve an implicit agreement of cooperation, leaders not only gain more power to sanction deviators but households may feel more embarrased if they deviate. Inversely, when leaders reduce B and offer more days of paid work, they are signalling the market value for community labor, which could reduce not only households' incentives to perform community activities without compensation, but also leaders' capacity to punish deviators.

[24]This closely follows the model of Huck et al. (2001), where the reference point is given by the team's optimum effort and disutility of norm deviation depends on other workers' average effort. Here I assume the reference point is the maximum established by leaders and disutility is determined by the level of exposure to sanctions, the visibility of actions, and monetary incentives.

Formally, the household maximization problem can be represented as follows:

$$\max_{l_i^p, l_i^u} \{p_i(T - l_i^p - l_i^u) - c_o(T - l_i^p - l_i^u) + wl_i^p - c_p(l_i^p) - c_u(l_i^u) + B$$

$$+a_i g(\sum_{i=1}^{N} l_i^p + \sum_{i=1}^{N} l_i^u) - v_i(\theta_i, \alpha, B)(\frac{L_{max}^u}{N} - l_i^u)\} \quad (1.8)$$

$$s.t. \quad 0 \leq l_i^p \leq L_{max}^p - \sum_{j=1}^{N-1} l_j^p$$

$$0 \leq l_i^u \leq L_{max}^u - \sum_{j=1}^{N-1} l_j^u$$

$$L_{max}^p = \frac{(1-\gamma)p^f F}{w}$$

$$L_{max}^u = L^t(p^f F, N_t) - \frac{(1-\gamma)p^f F}{w}$$

$$B = \frac{\gamma p^f F}{N}$$

The equilibrium conditions for the interior solutions are:

$$l_i^p : \quad w + a_i \frac{\partial g}{\partial l_i^p} + \frac{\partial c_o}{\partial l_i^p} = p_i + \frac{\partial c_p}{\partial l_i^p} \quad (1.9)$$

$$l_i^u : \quad a_i \frac{\partial g}{\partial l_i^u} + v_i + \frac{\partial c_o}{\partial l_i^u} = p_i + \frac{\partial c_u}{\partial l_i^c} \quad (1.10)$$

The previous conditions show that the optimal amount of labor allocated to both paid and unpaid community work is the one that equates the marginal benefits to the marginal costs of participation. Equation (1.10) shows that the marginal benefits from doing unpaid community work are derived not only from the benefits of increasing the availability of the public good or the reduction in the cost of time allocated to own-production activities, but households also benefit from the reduction in sanctions that result from increasing unpaid work. Corner solutions arise when either marginal benefits are greater ($l_i^{u,p} = L_{max}^{u,p} - \sum_{j=1}^{N-1} l_j^{u,p}$) or lower ($l_i^{u,p} = 0$) than marginal costs. More specifically, there are three types of households in equilibrium: non-cooperators, for whom the marginal costs of participation exceed the marginal benefits; unconstrained cooperators, for whom equalities (1.9) and (1.10) hold; and constrained cooperators, for whom the marginal benefits exceed the marginal costs of participation but are limited by the maximum number of working days determined by leaders.

To get testable predictions simple functional forms are assumed to solve the households' problem. For the labor costs, I assume convex functions ($c(l_i^j) = \frac{(l_i^j)^2}{2} \quad where\ j = \{o, p, u\}$). For the community work benefit, a linear function that aggregates both total paid and unpaid community work is assumed ($g(\sum_{i=1}^N l_i^p + \sum_{i=1}^N l_i^u) = \sum_{i=1}^N l_i^p + \sum_{i=1}^N l_i^u$). Finally, for the sanction function I assume the following form: $v_i = (1 + B)\theta_i\alpha$. The intuition of this function is simple. Lump-sum transfers (B) increase the level of sanctions, but in the absence of transfers households may still be exposed to them, depending on their exposure to sanctions (θ_i) and visibility of their actions (α).

The solutions to the maximization problem are given by:

$$l_i^{p*} = \frac{T - p_i + a_i + 2w}{3} - \frac{\theta_i\alpha}{3}(1 + \frac{\gamma p^f F}{N}) \tag{1.11}$$

$$l_i^{u*} = \frac{T - p_i + a_i - w}{3} + \frac{2\theta_i\alpha}{3}(1 + \frac{\gamma p^f F}{N}) \tag{1.12}$$

$$l_i^{o*} = \frac{T + 2p_i - 2a_i - w}{3} - \frac{\theta_i\alpha}{3}(1 + \frac{\gamma p^f F}{N}) \tag{1.13}$$

1.5.3 Main predictions about cooperative behavior in Mexican communities

Based on the theoretical model presented above, this section presents testable predictions for the impacts of payments for forest conservation within Mexican common property communities.

Prediction 1: *Cash incentives for forest conservation increase the time allocated to work in forest conservation activities.*

Cash incentives increase the total number of community activities required ($\frac{\partial L^t}{\partial P^f F} > 0$) and, depending on how leaders distribute these funds, there will be an increase either in the number of paid or unpaid days of work. If leaders distribute a high proportion of PSAH funds as lump-sum transfers (i.e. choose a high value of γ), households' sanctions from deviation increase ($\frac{\partial v_i}{\partial \gamma} > 0$). Moreover, since the number of unpaid activities increases, this promotes a further increase in the number of days worked in unpaid activities. Overall, the increase in the proportion distributed increases the number of days of unpaid community work ($\frac{\partial l_i^{u*}}{\partial \gamma} > 0$), and forest work is included in this category. Inversely, if leaders use most of the PSAH funds to pay wages for days worked in the forest (i.e. choose a low value of γ), sanctions for deviators decrease and households have more incentives to deviate and reduce the time allocated to unpaid community work. However, leaders also increase the amount of paid community activities available ($\frac{\partial \frac{L^p_{max}}{N}}{\partial \gamma} < 0$), which incentivizes households to devote more time to paid activities, as long as their outside option given them less benefits. Therefore, providing wages will also increase the amount of work in the forest ($\frac{\partial l_i^{p*}}{\partial \gamma} < 0$).

Prediction 2: *As long as some community activities remain unpaid, the higher the proportion of funds that are distributed as lump-sum transfers, the higher the amount of time that households allocate to all types of unpaid community work.*

Given that when leaders provide lump-sum transfers (high value of γ) they do not specify the activities that should be done, and that transfers increase sanctions, households' best response is to allocate more time to all types of unpaid community work required. Therefore, we should expect to see not only a higher intensity of work in forest conservation activities but also in other unpaid community activities. As opposed, since reducing the value of γ increases the amount of paid work available, then households' best strategy is to reallocate their time to paid activities. This effect is even amplified by the fact that a lower γ reduces sanctions from deviations making it easier for households to free-ride from work that remains unpaid.

Prediction 3: *To the extent that households are exposed to sanctions and their actions are visible, providing lump-sum transfers will have the desired effect both on forest work and on other types of unpaid community work.*

When households are more exposed to punishment (higher value of θ_i) their deviations are more costly, therefore they allocate more time to all types of unpaid community work ($\frac{\partial^2 l_i^{u*}}{\partial\gamma\partial\theta_i} > 0$). Similarly, when actions are more visible (higher value of α) any deviation generates higher sanctions, therefore the best response is to increase the time allocated to unpaid community work to match the level required by leaders ($\frac{\partial^2 l_i^{u*}}{\partial\gamma\partial\alpha} > 0$).

Prediction 4: *Cash incentives reduce the time allocated to own production activities, regardless of how they are framed. However, to the extent that there are some remaining unpaid activities and households are exposed to sanctions, lump-sum transfers will lead to a stronger reduction in time allocated to own production activities.*

When leaders decide to allocate funds as wages and increase the time allocated to paid community work, then households reduce the time allocated to own production activities $\frac{\partial l_i^{o*}}{\partial w} < 0$ as long as the wage is at least as high as the price they could get from their own production. Similarly, if leaders decide to allocate funds as lump-sum transfers then households allocate more time to all unpaid community work and reduce the time they devote to own production activities $\frac{\partial l_i^{o*}}{\partial B} < 0$. Following prediction 3, as long as there are more unpaid than paid activities, and given that households face more sanctions from deviation, the higher intensity of work in contexts with lump-sum transfers implies less work in own production activities. Note that this result is driven by the lack of leisure in the model.

1.6 Empirical analysis

The empirical analysis is divided into three subsections. First, some descriptive statistics and suggestive evidence is presented. Second, I formally explore the impact of cash incentives, on average, on cooperative behavior. Third, I evaluate whether there are differential impacts based on the framing of the incentive. As stated in the theoretical predictions, two types of community work are distinguished. The first, is forest conservation work, which is incentivized under PES, such as constructing fire breaks, doing forest patrols, reforestation, etc. The second, is work related to non-forest activities that remain unpaid, such as cleaning roads, building communal infrastructure, etc. I also look at work in own production activities, such as agriculture or off-farm employment.

For both types of community work (forest and non-forest), participation decisions and the intensity of participation are considered. The second outcome is measured by the average number of days per year that each male adult member in the household devotes to these activities[25]. For own production activities, data about participation and intensity of work of the head of the household is used[26]. Given that for work in forest conservation activities

[25]Male adult members are defined as those that are between 14 and 65 years old in 2007.

[26]I focus on head of households that were between 18 to 72 years old in the baseline. This is based on sample statistics about the approximate average age of entry and exit from the labor force. In addition, the lower

(FCA)and in own production activities information is available for both 2007 and 2011, but for other unpaid community work there is only data for 2011, different identification strategies are used and explained next.

To capture exposure to sanctions the sample for estimation is divided between households with land-use rights, from now on called "members", and those without these rights, called "non-members".[27] Land-use rights can be seen as an exogenous and appropriate measure of exposure to sanctions given that, in some cases, sanctions for not complying with community rules and labor could go as far as losing your rights. Based on field work, it is also evident that members feel very proud of their position within the community and therefore might feel more pressure to comply with leaders' requirements and to provide a good example for others. It is important to mention that, even if non-members may not be as exposed to sanctions as members, they could receive lump-sum transfers. Approximately 25% of communities providing lump-sum transfers indicate they distribute money also to non-members. Also, within the sample of beneficiaries, 35% of members indicate they received transfers and 13% of non-members report getting them.

1.6.1 Description of the context and suggestive evidence

Table 1.2 shows that community characteristics are well balanced across treatment and control groups.[28] The average area of communities in the sample is 8,080 hectares. The average population is approximately 2,000 people, but there is significant dispersion with the largest community having 40,000 people and the smallest 11. Communities are, on average, 30 Km.

bound is the majority of age in Mexico and also when most young people finish high school. The upper bound is the value reported by OECD for males as the average age of exit from the labor market (72.2) (OECD 2011).

[27]Separating the samples of members and non-members for estimation is not only useful for analysing exposure to sanctions, but it is also recommended by a Chow test that confirms that the coefficients of covariates in all regressions are different across both groups of households.

[28]The table reports both, the t-statistic and the normalized difference statistic. The second statistic is the difference in averages by treatment status scaled by the square root of the sum of the variances. This is a scale-free measure of the differences in distribution. Imbens & Wooldridge (2009) suggest as a rule of thumb one quarter.

away from large localities, and many of them are poor, as measured by an average asset based community wealth index that takes into account households' assets[29]. In terms of the composition of the population, few women have land-use rights (18%), approximately 60% of members have less than primary education, and the majority work in agriculture (77%). Forest is one of the main assets for most communities in the sample. The average hectares of forest per capita is 3.6. The average number of hectares of forest enrolled by beneficiary communities is 1,030. Assuming no costs of program implementation, the program per household payments are approximately 657 US$ per year, which is more than 6 times the monthly minimum wage in Mexico.[30]

Table 1.3 shows that household characteristics are also very well balanced. The final sample for estimation is obtained after pre-matching households to make sure they are as similar as possible in terms of their baseline cooperation decisions[31]. The average household is a family of 5. Almost 80% of household heads know how to read and write, but only 23% have more than primary education. Approximately, 50% of the sample indicate they speak an indigenous language. In terms of employment, the majority of household members(81%) work on the farm. Community work seems to be important, approximately, 54% of households participated in forest conservation activities in the baseline, where they worked on average 8 days per year.

[29] The index ranges from -2.46 to 4.10 and was constructed using household-level data and Principal Components Analysis (PCA). It takes into account different household assets (e.g. TV, stove, phone, car) and dwelling characteristics (e.g. material of floors and walls).

[30] The mean per household payment is 7,695 pesos. This was calculated assuming a household size of 5, taking into account the annual payment each community receives from the PSAH program and excluding the payments they give for technical support. The final amount was converted to US dollars using the exchange rate reported for the 15th of July of 2011 (11.72 pesos/ US$). The monthly minimum wage was calculated taking into account the daily minimum wage reported by CONASAMI. The average daily minimum wage in 2011 for the whole country was 58.1 pesos. Assuming there are 20 working days within a month, the monthly minimum wage is 1,161 pesos. Using the previous exchange rate, this is equivalent to 99 US$

[31] More specifically, households are matched based on their baseline participation decisions and number of days worked in forest conservation activities (FCA), and the average participation in FCA at the community level. I use Mahalanobis metric, matched exactly by region, and trim the sample based on the distance obtained after matching and keep those below the 95th percentile.

Figure 1.2 explores both the labor impacts of cash incentives on average and the differential effects resulting from the framing of the incentive. Cash incentives, on average, seem to increase the number of days worked in FCA but only for members. Moreover, members living in communities where lump-sum transfers are given work more days over time when compared to those in communities where wages are provided. For non-members we don't see differences between the two types of incentives. The theory indicates that lump-sum transfers reduce households' deviation from the required unpaid community work. I explore this by looking both at work in the forest that remains unpaid and other non-forest unpaid activities. Figure 1.3 plots changes in the proportion of unpaid FCA done in the community where households indicate they participated[32]. Over time, those receiving transfers, and particularly members, participate significantly more in all unpaid forest work, indicating that transfers could be reducing the incentives to deviate. Figure 1.4 shows the possible spillover effect to non-forest unpaid work. Again, members in communities with transfers work significantly more days on non-forest unpaid community work.

1.6.2 Average impacts of monetary incentives on cooperation

To look at the average impacts of cash incentives on work in forest activities, identification comes from time variation in community access to PES. This means, I compare changes over time in cooperation decisions between households that live in beneficiary and non-beneficiary communities. To do this, the following regression is estimated:

$$Y_{ijt} = \beta_0 + \beta_1 T_t + \beta_2 P_j + \beta_3 (T_t * P_j) + \varepsilon_{ijt} \tag{1.14}$$

where Y_{ijt} is the cooperation outcome of interest for household i, living in community j, in time t. T_t is a dummy variable that takes the value of 1 in the year 2011 and 0 in 2007,

[32]This variable is constructed given that we asked for a finite but very exhaustive list of activities both to households and leaders, and we know in which of those activities leaders offered wages.

and P_j is a dummy variable that takes the value of 1 if the household lives in a community that participates in the program and 0 otherwise. The coefficient β_3 is the parameter of interest, the Difference-in-Difference (DID) estimator. β_3 is expected to be positive. Since standard errors may be correlated among households living in the same community, we allow them to be clustered at the community level.

The estimator presented in equation 1.14 is an intent to treat estimator. The identification strategy relies on the fact that living in a beneficiary community implies a greater exposure to monetary compensation in exchange for community work done in the forest. Table 1.4 shows that households in treatment areas significantly increased their participation in paid activities by 52% and more than doubled the number of days worked in paid activities. There is also an increase of more than 70% in the number of different FCA done that were paid.

Although the DID approach eliminates unobservable time-invariant characteristics, there are two potential weaknesses remaining. First, I cannot control for unobserved temporal individual-specific or community-specific components that may influence treatment and that may also influence the outcome. Second, some macro effects can have differential impacts across treatment and control groups.[33] The sampling technique helps, to some extent, to reduce problems of unobservable characteristics driving the decision to enter the program that could also affect cooperation over time. This is because the control group was selected from all those communities that applied to the program in 2008 but were rejected. As it was mentioned before, the reasons of rejection are mostly based on observable characteristics, some of which were used in the matching performed before sampling communities in the control group.

[33]For example, if households in beneficiary communities and non-beneficiary communities have some (possibly unknown) characteristic that make them react differently to shocks.

To further improve identification, a DID matching approach is used, which consists in using a subsample of treated observations together with their best matches to estimate equation 1.14. This strategy helps eliminate not only time-invariant unobservable variables but also time-variant factors that have parallel trends (Blundell & Costa-Dias 2002). After checking the balance of the sample, households are matched based on their baseline participation and number of days worked in forest conservation activities, as well as a baseline community measure of average participation in forest work. As Table 1.3 shows, there are no significant differences in baseline cooperation decisions after matching and the final sample is well balanced.

Table 1.5 presents the estimation results for equation 1.14. Given that the variable of number of days worked is skewed, I apply the inverse hyperbolic sine transformation as suggested by Burbidge et al. (1988*b*)[34]. Linear probability models for the participation decisions are estimated, as they impose less parametric restrictions and marginal average effects should be fairly close to binary outcome models (Wooldridge 2002). Robustness checks are performed using probit models and results are fairly similar. The results in columns (1) to (4) show that cash incentives increase cooperation in forest conservation activities approximately by 20%, both in the intensive and extensive margins, but we only observe significant effects for member households.

To look at the impact of cash incentives on community work that remains unpaid, two alternative strategies are used. First, I explore changes over time in the proportion of total forest unpaid activities done in the community in which households indicate they participated. We can think of this as a measure of the deviation from the total number of unpaid forest activities required by community leaders[35]. A regression similar to equation

[34]Unlike the traditional log transformation, the inverse hyperbolic sine transformation is defined at zero and can be interpreted in the same standard way as a logarithm dependent variable.

[35]Looking at the total number of days worked in the forest without payment does not give much information for our analysis since, for communities providing lump-sum transfers, this number is always higher than

1.14 is estimated. Based on the theoretical framework, we would expect to see no impacts of cash incentives, on average, on this proportion given that households living in communities where a higher proportion of the funds are distributed would participate in most unpaid community work, but those in communities where wages are provided reallocate their time to paid activities. The results in columns (5) and (6) of Table 1.5 confirm this prediction showing no significant effects for both members and non-members.

The second approach is to look at work in non-forest unpaid community work. Since there is only information for 2011, the identification strategy relies on cross-sectional comparisons between beneficiaries and non-beneficiaries. The estimated regression is the following:

$$Y_{ijs} = \beta_0 + \beta_1 P_j + \alpha X_i + \gamma V_j + \alpha_s + \varepsilon_{ijs} \tag{1.15}$$

Where, Y_{ij} is the cooperation outcome of interest for household i in community j in state s. P_j is a dummy variable that takes the value of 1 when community j participates in the PSAH program, and X_i and V_j are household and community characteristics, respectively. α_s are state fixed effects and ε_{ijs} are clustered are the community level. The coefficient of interest is β_1 and measures the differences in cooperation outcomes between households that live in beneficiary and non-beneficiary communities. No impacts of cash incentives on other unpaid community work are expected either given that households' time allocation decisions could go in opposite directions depending on how the incentive is framed. As with unpaid forest activities, results show that cash incentives don't have positive spillover effects on other unpaid community work (Table 1.6).

It is important to acknowledge the limitations of this last approach when compared to the DID estimation. The most important one is that there might be baseline differences in cooperation that we don't observe and affect our results. Using a matched subsample is

for those providing wages. The proportion, however, seems more adequate to capture deviant behavior.

useful for this analysis, as long as cooperation decisions in FCA and in other types of community work are correlated in the baseline. If this is true, then matching based on baseline levels of cooperation in FCA helps to reduce concerns that households might be too different in terms of their baseline cooperation in other types of community work. Under this assumption, the first difference estimator should be almost as valid as the DID estimator. Given that there are similar conclusions for unpaid community work, both in and outside the forest, the assumption mentioned above is not unrealistic.

Following model predictions, the next step is to look at own production activities. For this, I estimate again equation 1.14 but the outcome variable is participation or number of days worked by the head of household in her primary activity. There are small and significant decreases in participation (5%) but not in the number of days. This negative effect is only significant for members and it is consistent with the increase we observe in the levels of cooperation for these types of households (Table 1.7).

1.6.3 Impact of different incentive schemes on cooperation

To analyze the impacts of incentive design, I examined heterogeneity in impacts across distributional rules adopted by community leaders related to the use of PES funds. The immediate approach to do this would be to use the sample of households living in communities that participate in the program and estimate the following regression:

$$Y_{ijst} = \beta_0 + \beta_1 T_t + \beta_2 D_j + \beta_3 (T_t * D_j) + \beta_4 V_j + \beta_5 u_j + \alpha_s + \varepsilon_{ijst} \qquad (1.16)$$

where Y_{ijt} is the labor outcome of interest for household i, living in community j, in time t. T_t is a dummy variable capturing time that takes the value of 1 in the year 2011 and 0 in 2007, and D_j is the proportion of program funds that are given directly to households as lump-sum transfers in community j[36]. V_j is a vector of community characteristics the affect the adoption of a distributional rule, and u_j is the area of forest per capita, which we include as a proxy measure for per capita payments. α_s are state fixed effects. The coefficient β_3 is the parameter of interest and is expected to be positive, particularly for community work that remains unpaid. As before, errors are assumed to be clustered at the community level.

Since the distributional rule adopted by leaders is a function of the level of cooperation, and households' level of cooperation is also a function of the distributional rule, the estimation of equation (1.16) would result in inconsistent and biased estimates of β_3. To solve this simultaneity problem, I implement a strategy that exploits the full-sample of program applicants. More specifically, I compare households in the treatment group receiving a particular type of incentive (i.e. wages or lump-sum transfers) with households living in communities that do not participate in the program but that otherwise their communities would have implemented a similar distributional arrangement.[37] To do this, I start by identifying the community characteristics that determine a given distributional arrangement. Then, given that the sample of communities participating in the program is small, I use nearest neighbor covariate matching to predict the distributional rule that communities in the control group would have adopted in case they had been accepted in the program.[38]

After completing this process, the following regression is estimated:

[36]We use the proportion distributed instead of a dummy variable for whether the community distributes lump-sum transfers or not, because within communities providing lump-sum transfers there is heterogeneity in the proportion of funds distributed. The average proportion in 0.75. Results are robust to using a binary variable for the distributional rule.

[37]This strategy has been used by other authors, such as Banerjee et al. (2010), to predict and compare the behavior of similar types of individuals in treatment and control groups.

[38]Matching is done exactly by region. Results are robust to using a two-step estimation process, where we first predict the distributional rule with a linear regression and then use the predicted values in the second step.

$$Y_{ijst} = \beta_0 + \beta_1 T_t + \beta_2 P_j + \beta_3 \hat{D}_j + \beta_4 (T_t * P_j) + \beta_5 (T_t * \hat{D}_j) + \beta_6 (P_j * \hat{D}_j)$$

$$+\beta_7 (T_t * P_j * \hat{D}_j) + \delta V_j + \eta u_j + \alpha_s + \varepsilon_{ijst} \qquad (1.17)$$

where $\hat{D}_j$ is the predicted proportion of program funds that are distributed directly to households in community j. V_j are community characteristics that affect the adoption of the distributional rule, u_j is the area of forest per capita, and α_s are state fixed effects. β_7 is the parameter of interest and is expected to be positive, particularly for forest work that remains unpaid. For non-forest unpaid work, I follow a similar strategy but use cross-sectional data.

Table 1.8 summarizes the differences in medians for multiple community characteristics[39]. Community characteristics that are usually discussed in the collective action literature are included, such as group size, income inequality, and the existence of rules (Baland & Platteau 1996, Ostrom 1990). To reduce dimensionality, I focus on population density instead of looking at total area and population size separately. We can see that a lower population density, low elevation and educational levels, small number of households with land-use rights, and a higher intensity of work in the forest in the baseline characterize communities that provide lump-sum transfers. There are also pronounced differences in the number of women that have land-use rights and in program per capita payments; communities providing transfers have fewer women and payments are 13 times higher than those with wages. As expected, a similar pattern is observed when we look at differences in area of forest per capita; communities with transfers enrolled 16 times the forest per capita than those with wages. Since we don't observe the payments per capita for communities in the control group,

[39]We focus on the median instead of the mean due to the small sample size and also because the distribution of several variables was skewed

we use area of forest per capita as a proxy of payments in the regression. Table 1.9 compares household characteristics after predicting the distributional rule in the control group. The sample is well balanced and there are only significant differences in household wealth that need to be controlled for during estimation.

Columns (1) to (4) in Table 1.10 report results for work in forest conservation activities. A higher redistribution of funds through lump-sum transfers increases participation and intensity of work but the impact is only significant for member households. Full redistribution increases participation by 63% and the number of days worked by 50%. There are two possible explanations for the increase in the intensity of work. The first is that households in communities where lump-sum transfers are given receive a larger amount of money for their work. We rule out this possibility by controlling for the area of forest per capita in all regressions, which should be a good proxy of program per capita payments. The second possibility is that not all forest work is paid in communities that provide wages. Therefore, it is possible that households reallocate their labor to paid activities and this is why we would expect to see a higher intensity of work in communities with lump-sum transfers.

In columns (5) and (6) of Table 1.10 this second hypothesis is tested. In particular, I look at the impact of the proportion distributed on deviations from unpaid forest work. The results are consistent with model predictions, indicating that transfers reduce deviations but only significantly for households with land-use rights. Full redistribution of funds through transfers increases the proportion of forest unpaid activities done over time in 92%. For work in non-forest unpaid activities, we also see a higher number of days worked in members, but effects are not as strong as before. On average, full redistribution increases the number of days work by 50% (Table 1.11).

Table 1.12 explores the impacts of the distributional rule on work in own production activities. Overall, results suggest that the framing of the incentive does not have any differential effect on behavior. This result suggests the possibility that households are using their leisure time to work in the forest and in other community activities and, therefore, the higher levels of cooperation observed in cases with higher redistribution of funds as transfers are not harming their own production activities.

Finally, I explore the impacts of the framing of the incentive on more and less visible activities. For this, I focus on two FCA that the PSAH strongly promotes: maintaining and constructing firebreaks and doing forest patrols. The main argument is that work in firebreaks is more visible, since it is usually done in large groups of people and the outcome is observable. On the contrary, forest patrols are usually done in small groups of people and the outcome is not observable. Table 1.13 shows that lump-sum transfers significantly increase the number of days worked only for members and only for activities that are visible. These results are not only consistent with model predictions but help to reduce any concerns that may arise from using self-reported data and the fact that households might have incentives to overstate their cooperation both for visible and non-visible activities. They also help to reduce concerns about recall data, as we would expect to see a similar bias across all types of activities.

1.7 Robustness checks

Results are robust to multiple tests and alternative specifications. First, they are robust to estimating a binary treatment for the distributional rule (i.e. wages versus lump-sum transfers) instead of a continuous treatment. Second, they are robust to estimating probit models instead of linear probability models for participation decisions. Third, they are robust to dividing the sample between indigenous and non-indigenous communities, which could be used as an alternative measure of exposure to sanctions, assuming households in indigenous communities are more exposed to sanctions and rules. Fourth, given that bootstrap does not work well with non-parametric methods, a two-step estimation method is implemented. In a first step, the distributional rule is predicted using a linear regression; in the second step, this prediction is introduced in the regression. The whole process is bootstrapped to correct standard errors. Results are also robust to drawing a random sample of members of equal size to the sample of non-members and estimating regressions again. This test helps to reduce any concerns that the non-significant results observed for non-members are related to their smaller sample. Finally, results are robust to using an instrumental variables (IV) estimator that uses the distributional rule of neighboring communities.[40]

1.8 Conclusions

Using households and community level data from accepted and rejected applicants to the Mexican Payments for Hydrological Services Program (PSAH), one of the largest PES programs in the world, this study contributes to the emerging literature on the labor impacts of PES programs, and exploits a unique setting to analyze whether monetary compensation modifies cooperative behavior in activities that, for a long time, have been unpaid. So far, the PES literature has given little attention to the possibility that although payments might increase work in forest conservation activities, they might also change the logic of collective action harming or encouraging cooperation in activities that remain unpaid. This paper shows that the framing of the incentive can have an important role in explaining behavior.

Findings indicate that monetary incentives increase work, both in the intensive and extensive margins, in forest conservation activities; however, effects are only significant for households with land-use rights. Our theoretical framework suggests that, to the extent

[40]More specifically, the proportion of program funds that are distributed as lump-sum transfers in communities within a given state but that are not so close neighbors is used as an IV. Excluding the closest neighbors reduces the possibility of having spillover effects. Some of the results mentioned above are reported in the appendix. The rest can be requested from the author.

that some community work remains unpaid, households that are more exposed to sanctions resulting from deviant behavior and whose actions are more visible, will increase their cooperation in all unpaid activities when they receive lump-sum transfers. In contrast, those that receive wages for specific forest conservation activities might reallocate their labor to paid work. The evidence presented here shows that transfers increase the intensity of work both in unpaid forest and non-forest work, but only for households with land-use rights and when activities are visible. There is no evidence that the increase in work in community activities is harming households' own production activities.

These findings highlight the importance of understanding how incentive design interacts with behavior in contexts where non-economic motivations play an important role. Moreover, they confirm that recent and popular strategies that promote the conservation of natural resources in the marketplace, such as PES, can change collective action within common property communities and should be further studied given the important implications that they can have, both on environmental and welfare outcomes. Some avenues for future research include analyzing the efficiency of our results. So far, we have focused only on labor outcomes; however, one important question is whether the increase in cooperation we observe is correlated with better forest conservation or improved public services provision. For this, data about changes in forest cover could be used. Moreover, detailed data about the types of unpaid community work performed as well as the related outcomes would be needed.

Before concluding it is important to acknowledge some of the limitations in this study. First, by using recall data there is the risk that estimated coefficients could be biased. Recall bias could arise from two different sources. First, if beneficiary households remember better past information given that they know when the program started. Second, if beneficiary households remember better because they have experienced a big change in income or labor as a result of the program. Since we did not give any reference about the program when asking the recall questions, plus the fact that many households don't actually know about the program since treatment is given at the community level, I believe that if there are any memory errors, they are probably not systematic and could actually bias coefficients towards zero. Moreover, if it is actually the case that people do remember better past information due to the significant change in labor they experienced, this is actually the impact of interest. Given the differences in our results looking at multiple types of forest conservation activities, concerns of systematic recall bias should be reduced. Future work will involve surveying treatment and control groups over time to better understand not only impacts during program implementation, but also once the program is over.

Another possible limitation is the assumption that the prices households get for their own production are exogenous to program implementation. By doing this, I exclude the possibility that income and labor market effects resulting from PES could change local prices. We show in a separate study that there are no large increases in consumption in beneficiary households that would lead to changes in local prices (Alix-Garcia et al. 2013). In addition, given that this is a conservation and not a reforestation program, large changes in agricultural practices that could affect prices are not expected. Moreover, own production activities could include also work done outside of the community that should not be sensitive to local price changes, if any. Given that there might be some hidden heterogeneity in the impacts of PES on prices, detailed data about prices is currently being collected to analyse this issue more carefully in the near future. Finally, I have not explored in detail all the labor choices that could emerge both at the household level and within the household resulting from program implementation. This is a topic that needs to be further explored in the PES literature, but lies outside the scope of this paper and is left for future work.

Table 1.1: **Sample size**

	Beneficiary communities	Beneficiary households	Non-beneficiary communities	Non-Beneficiary households
Region 1 (North)	13	130	14	131
Region 2 (center)	15	141	14	127
Region 3 (South West)	15	150	13	127
Region 4 (South East)	15	136	12	114
Total	58	557	53	499

Note: Region 1 includes the states of Chihuahua, Durango, and Sinaloa. Region 2 includes Guanajuato, Michoacan, Nayarit, Queretaro, and San Luis Potosi. Region 3 has Chiapas, Guerrero, and Oaxaca. Region 4 includes the states of Campeche, Quintana Roo, and Yucatan

Table 1.2: **Community characteristics**

	Benef	Non-benef	Diff.	Norm. Diff.
Total population	2455.053	1958.462	496.591	0.074
Total area	6955.972	8999.044	-2043.072	-0.108
Distance locality $\geq$ 5000 people (km)	31.960	29.901	2.059	0.080
Elevation (mt)	1563.654	1488.136	75.518	0.050
Average wealth index 2007	-0.104	0.089	-0.193	-0.105
Indigenous in sample	0.484	0.474	0.010	0.016
Members with less than primary education	0.544	0.649	-0.104	-0.246
Members that are women	0.202	0.159	0.043	0.198
Members working agriculture 2007	0.726	0.817	-0.091	-0.099
Members migrated past 4 years	0.145	0.179	-0.034	-0.061
Number of members	383.086	172.226	210.860	0.245
Area of forest per capita	4.320	2.907	1.413	0.179
Observations	58	53		

Note: * $p<0.10$ ** $p<0.05$ *** $p<0.01$. All information related to ejidatarios (i.e. households with land-use rights) refers to the proportion of people within that group that have the specified characteristic. The area of forest refers to that one enrolled (beneficiaries) or that one that could have been enrolled (non-beneficiaries) in the PSAH program. The wealth index is an average of community households' indices. This index was calculated taking into account household assets and access to basic services and using principal component analysis. The range of the wealth index goes from -2.46 to 4.10.

Table 1.3: **Household characteristics**

	Benef	Non-benef	Diff.	Norm. Diff.
Household size	4.905	4.593	0.312	0.095
Wealth Index 2007	-0.151	-0.194	0.042	0.016
Speaks indigenous language	0.506	0.513	-0.007	-0.010
Distance to locality $\geq$ 5000 people	32.175	31.046	1.129	0.043
Knows how to read and write	0.820	0.798	0.022	0.039
Male head of household	0.874	0.879	-0.005	-0.010
Age head of household	48.233	49.014	-0.781	-0.037
No education	0.175	0.175	-0.000	-0.000
More than primary education	0.258	0.203	0.055	0.093
Agricultural employment 2007	0.673	0.719	-0.046	-0.071
Participated in FCA 2007	0.549	0.533	0.016	0.023
Days worked in FCA 2007	8.871	6.948	1.923	0.077
Observations	517	364		

Note: * $p<0.10$ ** $p<0.05$ *** $p<0.01$. Wealth index ranges from -3.7 to 6.4. Statistics are reported on previously matched sample. Matching is performed on household participation decisions and number of days worked in FMA in 2007, and average community participation in 2007. FCA are forest conservation activities.

Table 1.4: **Changes in payments for forest conservation activities**

Dep. var.:	Number paid activities	Participation paid activities	Number paid days
Benef	0.381**	0.049	1.130
	(0.146)	(0.054)	(1.245)
Year	0.117	0.056	2.727**
	(0.091)	(0.040)	(1.358)
Year*Benef	0.496***	0.153***	6.353***
	(0.144)	(0.049)	(2.242)
Baseline mean	0.712	0.293	4.194
Baseline Std. Dev.	1.446	0.455	13.701
N	1752	1752	1752

Note: * p<0.10 ** p<0.05 *** p<0.01. Standard errors, reported in parenthesis, are robust and clustered at the community level.

Table 1.5: **Work in forest conservation activities**

Dep. var.:	Participation		Log number of days		Prop. unpaid done	
	Member	Non-mem	Member	Non-mem	Member	Non-mem
	(1)	(2)	(3)	(4)	(5)	(6)
Benef	0.050	-0.043	0.149	-0.025	-0.001	-0.028
	(0.068)	(0.064)	(0.096)	(0.100)	(0.037)	(0.046)
Year	0.106**	0.171***	0.181***	0.182**	0.037	0.051
	(0.050)	(0.055)	(0.065)	(0.074)	(0.033)	(0.043)
Year*Benef	0.107*	0.036	0.176**	0.152	0.024	-0.019
	(0.061)	(0.064)	(0.085)	(0.099)	(0.040)	(0.050)
N	1126	636	1126	636	1105	622
Baseline mean	0.565	0.511	8.034	8.279	0.288	0.241

Note: * p<0.10 ** p<0.05 *** p<0.01. Robust standard errors clustered at the community level. For participation we estimate a linear probability model. The baseline mean refers to participation rates and number of days worked. Members are households with land-use rights. Prop. unpaid done is the proportion of unpaid FCA done at the community level in which households participated.

Table 1.6: **Work in non-forest unpaid activities**

Dep. var:	Participation		Log number of days	
	Member	Non-member	Member	Non-member
Benef	-0.023	-0.012	0.044	-0.058
	(0.042)	(0.064)	(0.066)	(0.087)
N	538	308	528	306
Controls mean	0.730	0.713	5.587	5.468

Note: * p<0.10 ** p<0.05 *** p<0.01. Robust standard errors clustered at the community level. Controls include: Population density, number of ejidatarios, proportion of ejidatarios that are women, area of forest per capita, average of days worked in FMA in the community in 2007, household wealth index 2007, and state fixed effects. Members are households with land-use rights. The baseline mean refers to participation rates and number of days worked.

Table 1.7: **Work in own production activities**

Dep. var.:	Participation		Log of number of days	
	Member	Non-member	Member	Non-member
Benef	0.037	0.024	0.028	0.019
	(0.028)	(0.050)	(0.035)	(0.054)
Year	0.035***	0.040**	-0.034	-0.014
	(0.011)	(0.016)	(0.027)	(0.021)
Year*Benef	-0.044***	-0.035	-0.036	-0.033
	(0.015)	(0.026)	(0.034)	(0.040)
N	1085	620	1070	615
Baseline mean	0.900	0.864	5.108	4.787

Note: * p<0.10 ** p<0.05 *** p<0.01. Robust standard errors clustered at the community level. Other controls include: Population density, number of members, proportion of members that are women area of forest per capita, average of days worked in FCA in the community in 2007, household wealth index 2007, and state fixed effects. Sample considers information of head of households between 22-76 years old in 2011. Members are households with land-use rights.

Table 1.8: **Community characteristics by distributional rule**

	Transfers (Median)	Wages (Median)	Difference
Population density	0.06	0.30	-0.24**
Distance locality $\geq$ 5000 people (km)	30.92	29.26	1.66
Elevation (m)	134.86	2455.49	-2320.63***
Average wealth index 2007	-0.26	-0.12	-0.14
Variance wealth index 2007	1.55	2.29	-0.74
Indigenous in sample	0.90	0.25	0.65
Members with less than primary education	0.73	0.42	0.31
Members that are women	0.07	0.25	-0.18***
Members working agriculture 2007	0.68	0.74	-0.06
Members that migrated past 4 years	0.06	0.05	0.01
Number of members	24.50	126.00	-101.50**
Days worked in FCA 2007	17.72	7.47	10.25**
Participation in FCA 2007	0.70	0.50	0.20
Number of FCA 2007	2.55	1.45	1.10
Rules for forest use 2007	0.00	0.00	0.00
PSAH per capita payments	6989.35	506.39	6482.96***
Area of forest per capita	4.07	0.26	3.81***
Observations	20	38	

Note: * p<0.10 ** p<0.05 *** p<0.01. The difference in medians is tested using a k-sample median test. The range of the wealth index goes from -2.46 to 2.82. FCA are forest conservation activities. Members are those with land-use rights.

Table 1.9: **Household characteristics by predicted distributional rule**

	Transfers			Wages		
	Benef	Non-benef	N.Diff.	Benef	Non-benef	N.Diff.
Household size	4.614	4.500	0.038	5.056	4.608	0.132
Wealth Index 2007	-0.469	0.311	-0.360	0.013	-0.451	0.177
Indigenous language	0.619	0.533	0.123	0.447	0.415	0.046
Distance locality $\geq$ 5000 people	32.812	30.431	0.181	31.846	27.626	0.170
Knows how to read and write	0.801	0.806	-0.009	0.830	0.784	0.083
Male head of household	0.938	0.952	-0.043	0.842	0.872	-0.060
Age head of household	46.108	49.387	-0.148	49.332	48.838	0.024
No education	0.184	0.164	0.037	0.170	0.182	-0.022
More than primary education	0.167	0.131	0.070	0.306	0.238	0.109
Agricultural employment 2007	0.776	0.780	-0.006	0.619	0.771	0.237
Number rooms in house 2007	1.411	1.613	-0.147	2.124	1.932	0.112
Participated in FCA 2007	0.648	0.661	-0.020	0.499	0.574	-0.108
Days worked in FCA 2007	11.701	8.820	0.103	7.411	8.933	-0.058
Observations	176	62		341	148	

Note: Wealth index ranges from -3.7 to 6.4. Households belong to communities that were previously matched on population density, elevation, proportion of members that are women, total number of members, days worked in FCA in 2007, and area of forest per capita. N. diff. is the normalized difference. The rule of thumb for the normalized difference is 0.25 (Imbens and Wooldridge, 2009)

Table 1.10: **Work in forest conservation activities**

Dep. var.:	Participation		Log number of days		Prop. unpaid done	
	Member	Non-mem	Member	Non-mem	Member	Non-mem
	(1)	(2)	(3)	(4)	(5)	(6)
Year	0.096	0.229**	0.128	0.155	0.057	0.065
	(0.075)	(0.093)	(0.114)	(0.108)	(0.059)	(0.052)
Benef	-0.003	-0.071	0.020	-0.058	0.013	-0.032
	(0.071)	(0.103)	(0.091)	(0.153)	(0.047)	(0.062)
$Pro\hat{p}.dist.$	0.228**	-0.008	0.285*	0.047	0.125	0.003
	(0.103)	(0.174)	(0.162)	(0.279)	(0.080)	(0.161)
Year*Benef	0.080	-0.042	0.152	0.152	-0.032	-0.054
	(0.088)	(0.101)	(0.133)	(0.135)	(0.065)	(0.060)
Year*$Pro\hat{p}.dist.$	-0.268**	0.092	-0.270	0.170	-0.146	-0.102
	(0.116)	(0.274)	(0.167)	(0.457)	(0.117)	(0.169)
Benef*$Pro\hat{p}.dist.$	-0.171	-0.023	-0.265	0.108	-0.157*	0.006
	(0.124)	(0.218)	(0.192)	(0.353)	(0.079)	(0.182)
Year*Benef*$P.\hat{d}ist.$	0.361**	-0.031	0.511**	-0.086	0.261**	0.155
	(0.146)	(0.287)	(0.228)	(0.486)	(0.130)	(0.176)
N	914	488	914	488	905	477
Baseline mean	0.565	0.511	8.034	8.279	0.288	0.241

Note: * p<0.10 ** p<0.05 *** p<0.01. Robust standard errors clustered at the community level. Controls include: Population density, number of ejidatarios, proportion of ejidatarios that are women, area of forest per capita, average of days worked in FMA in the community in 2007, household wealth 2007, and state fixed effects. Members are households with land-use rights. Baseline means refer to participation rates and number of days worked.

Table 1.11: **Work in non-forest unpaid activities**

Dep. variable:	**Participation**		**Log number of days**	
	Member	**Non-member**	**Member**	**Non-member**
$\hat{Prop.dist.}$	-0.105	0.081	-0.007	0.147
	(0.072)	(0.087)	(0.151)	(0.164)
Benef	-0.102**	-0.100	-0.047	-0.203**
	(0.046)	(0.091)	(0.084)	(0.089)
Benef*$P.\hat{dist}.$	0.099	0.073	0.239*	0.264
	(0.072)	(0.118)	(0.128)	(0.203)
N	456	243	446	241
Control mean	0.730	0.712	5.633	5.322

Note: * p<0.10 ** p<0.05 *** p<0.01. Robust standard errors clustered at the community level. Controls include: Population density, number of members, proportion of members that are women, area of forest per capita, average of days worked in FCA in the community in 2007, household wealth index 2007, and state fixed effects. Members are households with land-use rights. Control means refer to participation rates and number of days worked.

Table 1.12: **Work in own production activities**

Dep.var.:	Participation		Log number of days	
	Member	Non-member	Member	Non-member
Benef	0.119***	-0.104**	0.112**	-0.126**
	(0.042)	(0.046)	(0.050)	(0.052)
Year	0.047**	0.022	-0.029	-0.038
	(0.018)	(0.019)	(0.042)	(0.034)
$\hat{Prop.dist.}$	0.048	-0.287***	-0.051	-0.353***
	(0.071)	(0.104)	(0.095)	(0.115)
Year*Benef	-0.056**	-0.001	-0.056	-0.002
	(0.022)	(0.033)	(0.052)	(0.056)
Year*$\hat{Prop.dist.}$	0.010	0.039	0.107	0.108*
	(0.048)	(0.053)	(0.071)	(0.061)
Benef*$\hat{Prop.dist.}$	-0.116*	0.277***	-0.036	0.300***
	(0.066)	(0.101)	(0.089)	(0.109)
Year*Benef*$P.\hat{dist}.$	-0.002	-0.088	-0.052	-0.104
	(0.051)	(0.066)	(0.081)	(0.083)
N	879	482	866	477
Baseline mean	0.900	0.864	5.108	4.787

Note: * p<0.10 ** p<0.05 *** p<0.01. Robust standard errors clustered at the community level. Other controls include: Population density, number of members, proportion of members that are women area of forest per capita, average of days worked in FCA in the community in 2007, household wealth index 2007, and state fixed effects. Sample considers information of head of households between 22-76 years old in 2011. Members are households with land-use rights.

Table 1.13: **Work in forest activities (visible vs. less visible)**

Dep. variable:	Log number of days worked			
	Visible activity		Not visible activity	
	Member	Non-member	Member	Non-member
Year	0.036	-0.000	-0.010	-0.010
	(0.051)	(0.063)	(0.030)	(0.059)
Benef	0.016	0.109	0.039	-0.045
	(0.065)	(0.078)	(0.040)	(0.087)
$\hat{Prop.dist.}$	0.033	0.239	-0.025	-0.022
	(0.108)	(0.170)	(0.079)	(0.118)
Year*Benef	0.070	0.134*	0.046	0.087
	(0.065)	(0.079)	(0.047)	(0.069)
Year*$\hat{Prop.dist.}$	-0.063	0.055	0.100	0.002
	(0.106)	(0.289)	(0.102)	(0.063)
Benef*$\hat{Prop.dist.}$	-0.247**	-0.360*	0.124	0.044
	(0.119)	(0.188)	(0.084)	(0.157)
Year*Benef*$\hat{Prop.dist.}$	0.403**	0.041	0.051	0.052
	(0.165)	(0.317)	(0.128)	(0.097)
N	914	488	914	488

Note: * p<0.10 ** p<0.05 *** p<0.01. Robust standard errors clustered at the community level. Other controls include: Population density, number of members, proportion of members that are women, area of forest per capita, average of days worked in FCA in the community in 2007, household wealth 2007, and state fixed effects. Members are households with land-use rights. Visible activities correspond to construction and maintenance of firebreaks, not so visible activities correspond to forest patrols.

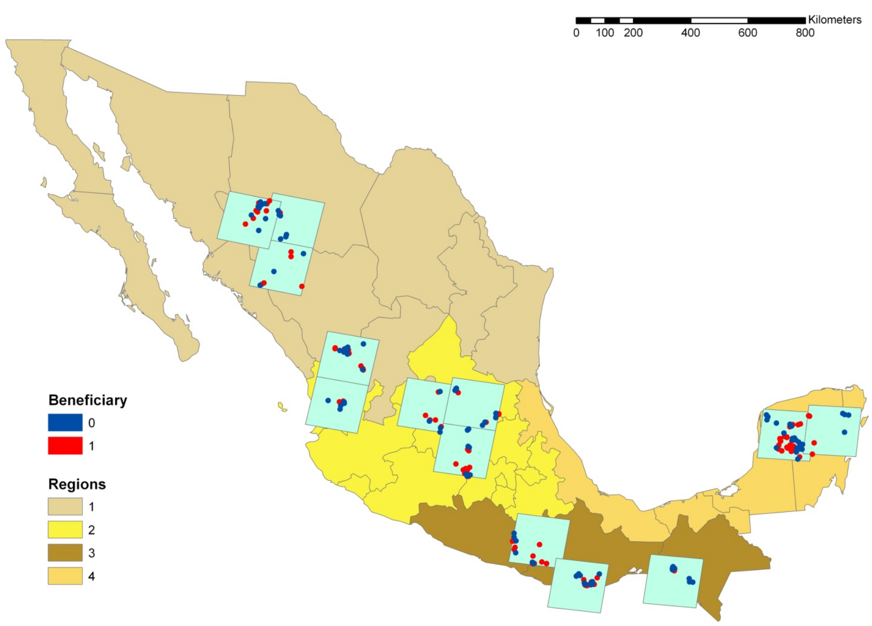

Figure 1.1: Centroid points for each property surveyed and footprints selected

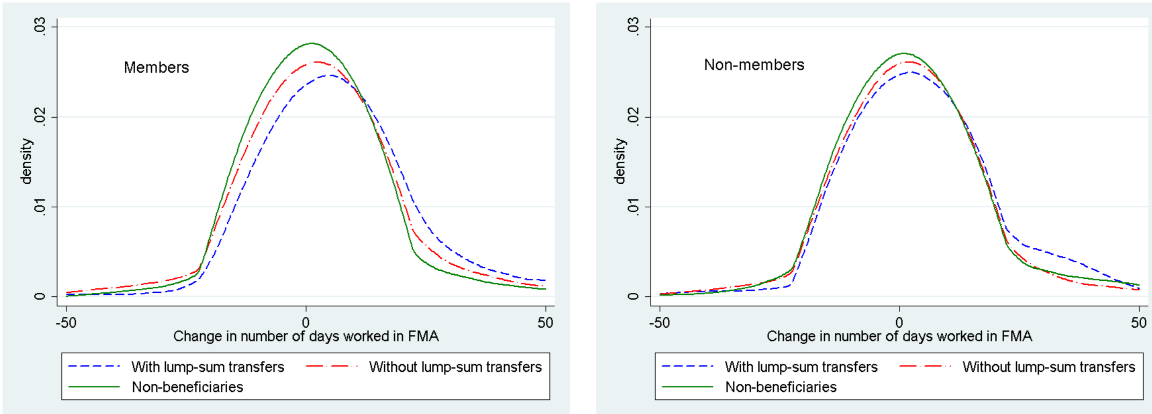

Figure 1.2: Changes in number of days worked in forest conservation activities 2011-2007

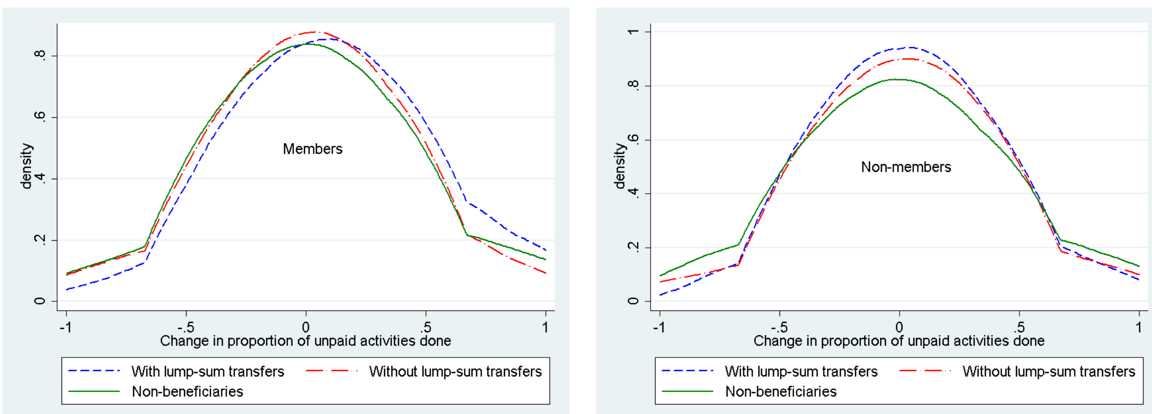

Figure 1.3: Changes in proportion of unpaid forest activities done by households 2011-2007

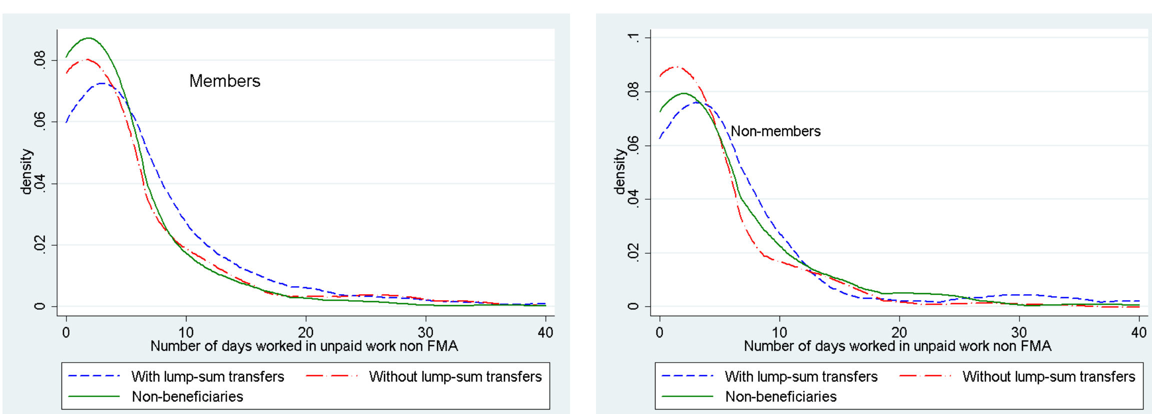

Figure 1.4: Days worked in non-forest unpaid activities 2011

Chapter 2

Only one tree from each seed? Environmental effectiveness and poverty alleviation in Mexico's Payments for Ecosystem Services Program

2.1 Introduction

Between 2000 and 2010, 13 million hectares of the world's forests, an area the size of Nicaragua, were converted every year to other uses (FAO 2010). Land-use change leads to losses of biodiversity and water quality, and is the second largest source of global CO2 emissions contributing to climate change (Pachauri 2008). The empirical evaluation of potential carbon emissions reduction policies (Martin et al. 2011, Li et al. 2013, Davis et al. 2013) is increasingly important given the expected negative social impacts of climate change

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(Schlenker et al. 2004, Tol 2009, Dell et al. 2012). Among the suite of options, policies for reducing emissions from deforestation and forest degradation, or "REDD" have been a centerpiece of international climate change negotiations (Stern 2008, for Conservation of Nature 2009, on Climate Change 2009). Future financial flows for REDD, mainly from developed to developing countries, are predicted to be close to US $30 billion a year (UN-REDD-Programme 2012). To reach REDD goals, many countries will employ conditional cash transfers to landowners, or "payments for ecosystem service" (PES). These programs are designed to increase the private returns to forest and thus reduce the difference between private and social values of forest. Mexico, Costa Rica, Ecuador and Brazil have already established payments for avoided deforestation programs while other countries are experimenting with them (Jindal et al. 2008, Wunder & Wertz-Kanounnikoff 2009, UN-REDD-Programme 2012). Although the primary goal of these programs is to reduce deforestation, program managers often face pressure to use them for poverty alleviation, particularly in developing countries (Landell-Mills & Porras 2002, Wunder et al. 2008*b*, Turpie et al. 2008, Lipper 2009). Given the strong global correlation between forest cover and poverty, PES programs appear at first glance to be an easy "win-win" policy solution for REDD. However, despite their popularity, rigorous empirical evidence on the impacts of payments for avoided deforestation on environmental and economic outcomes is extremely limited (Pattanayak et al. 2010, Alix-Garcia & Wolff 2014).

In this paper, we use two novel data sets to evaluate the land cover and wealth impacts of a national-scale environmental payments program and to examine the tradeoffs between environmental and social objectives. The program we study is Mexico's Payments for Hydrological Services Program (PSAH), a federal program which pays private or communal landowners to maintain forest cover under five year contracts. We estimate environmental impacts for the 2004-2009 program cohorts using annual measures of land cover from 2003-2011 and national program data. We evaluate household impacts for a nationally-representative subsample of the 2008 cohort using survey data from 2007 and 2011. In each case, panel data on both program beneficiaries and matched rejected applicants allows us to control for possible omitted variables that are time invariant as well as for time trends affecting both groups. The validity of the estimation strategy relies on the assumption that trends in beneficiary and non-beneficiary groups would have been the same in the absence of the program. Similar pre-program trends for beneficiaries and rejected applicants make this a plausible assumption.

We find that the program has reduced expected land cover loss from deforestation or degradation by 40-51 percent compared to what would have occurred in the program's absence. These results are robust to different specifications, including using different subsets of rejected applicants to establish counterfactual time trends. We also find positive but not statistically significant changes in household consumption and investment outcomes for beneficiaries in communal properties and for private properties. Calculations of minimum detectable effect sizes indicate that we can rule out substantial negative impacts of the program.

To understand potential tradeoffs between the program's environmental effectiveness and poverty alleviation potential, we construct a simple rents-based model of deforestation risk based on targeting characteristics. Consistent with this model, we find that avoided deforestation impacts could be increased by targeting more funds to land with high geographic risk of deforestation, but this would generally reduce participation by the poor. We do find some scope to increase participation of the poor by targeting more funds to communal properties, where deforestation risk is high and households are poor. Also consistent with theory, the household data show greater socioeconomic impacts where deforestation risk and thus expected opportunity costs are lower. Finally, our survey data indicate that high participation cost due to increased forest management activities limits the surplus gained by households and reduces the poverty alleviation potential of the program.

These results make three contributions to the literature. First, we add to limited existing evidence on the environmental effectiveness of large-scale avoided deforestation programs. Between 2003 and 2011, the Mexican National Forestry Commission (CONAFOR) allocated approximately 450 million USD to enroll more than 2.6 million hectares of land in the program, making it one of the largest PES in the world. Over the period from 2000-2010, nearly 195 thousand hectares of forest were lost in Mexico every year, making it one of the top 10 deforesters in the world (FAO 2010). Mexico's experience thus provides a case study which may be valuable for other countries contemplating similar policies. To date, research on avoided deforestation at the national level and across multiple years has only been conducted for Costa Rica's program (Sánchez-Azofeifa et al. 2007*b*, Arriagada et al. 2012, Pfaff et al. 2013, Robalino & Pfaff 2013). Rigorous retrospective evidence about the environmental effects of Mexico's program is limited to evidence from the Monarca reserve (Honey-Rosés et al. 2011) and the 2004 PSAH cohort (Alix-Garcia et al. 2012).

We advance the literature on PES impacts in Mexico and more generally by using panel data on both beneficiary and rejected applicants. An important criticism of previous evaluations is that apparent effectiveness may be driven by unobservable differences lowering participation costs such as low land quality or landowner skills (Pattanayak et al. 2010). By using the behavior of matched rejected applicants over time to establish the counterfactual, we ensure that all landowners have revealed their desire to enroll in the program and that their expected participation costs are low enough to motivate application. To our knowledge, the only previous paper to use rejected applicants from a national PES was our cross-sectional evaluation of the 2004 PSAH cohort on deforestation between 2003-2006 (Alix-Garcia et al. 2012). Here we evaluate environmental impacts for six cohorts (2004-2009) using panel data from 2003-2011. This allows us to estimate avoided deforestation impacts from regressions with location or parcel fixed-effects, better addressing potential selection issues, and also confirming that the earliest cohorts were not unique in their impact.

Our second contribution is to simultaneously evaluate the impacts of environmental conditional cash transfers on household wealth. Although PES programs are generally voluntary, it is possible that household risk aversion or lack of financial literacy could lead to average losses in wealth. In addition, where programs enroll communal property, as Mexico's does, community members without full land rights or who use communal land intensively may be harmed by restrictions on access to land (Hawkins 2011, Bulte et al. 2008, Pfaff et al. 2007, Zilberman et al. 2008). To date, predictions of household impacts have been made from inferences based upon the cross-sectional composition of the participant group, rather than actual measurement of changes in wealth outcomes as a result of participation (Uchida et al. 2007, Pfaff et al. 2007, Rios & Pagiola 2010, Gauvin et al. 2010, Jayachandran 2013). Simultaneous evaluation of environmental and social impacts is difficult due to the vastly different spatial scales of data needed and the fact that PES beneficiaries are located in remote or isolated areas which are costly to reach and survey. Although we do not find evidence for significant household impacts, we are able to rule out large negative impacts on household consumption or investment outcomes, even for households in communal properties without full land rights. This evidence is important to global policy debates about the potential negative impacts of PES programs.

Our third contribution is to investigate whether heterogeneity in program impacts results in tradeoffs between environmental efficacy and poverty alleviation. Previous research on PES has pointed out the potential theoretical irreconcilability between cost-effective avoided deforestation and poverty reduction if the forest at greatest risk is not owned by the poorest households (Pagiola et al. 2005, Pfaff et al. 2007, Alix-Garcia et al. 2008, Bulte et al. 2008, Jack et al. 2008, Zilberman et al. 2008, Leimona et al. 2009, Pattanayak et al. 2010, Pfaff & Robalino 2012, Pfaff et al. 2013). However, previous research simulates tradeoffs based on baseline profiles of beneficiaries (Uchida et al. 2009, Wünscher et al. 2008, Gauvin et al. 2010) whereas our assessment is based on heterogeneity in measured impacts. Our results indicate mainly tradeoffs between environmental protection and poverty alleviation. This suggests that policymakers implementing REDD should not be asked to meet multiple social goals with a single policy tool.

The paper proceeds as follows. Section 1 provides program background, an overview of our empirical strategy and data sources, and details of the pre-matching of data. Section 2 presents the analysis of land cover impacts and Section 3 the analysis of household impacts. Section 4 investigates tradeoffs between environmental and socioeconomic impacts, beginning with a conceptual framework and then testing for results consistent with this framework. Section 5 concludes.

2.2 Program background and data

2.2.1 Program background

Mexico's federal Payments for Hydrological Services program began in 2003 and gives annual payments to landowners to maintain forest cover under five-year contracts. The primary goal of the program is to protect forests in order to improve their "hydrological services", which include improved water quality, reduced erosion and sedimentation, and reduced flood hazards (Martínez et al. 2009, Bruijnzeel 2004). Forest cover also provides important benefits for carbon sequestration, and Mexico has promoted the PSAH program as part of their national strategy for reducing carbon emissions from deforestation and forest degradation (De Jong et al. 2008). Social goals, including the maintenance of rural income and poverty reduction, are secondary, but have been made explicit by prioritizing funding for municipalities with a high degree of poverty or high percentage indigenous population (Muñoz-Piña et al. 2008, Sims et al. 2013).

Both private and communal property landowners are eligible for the program. More than half of the program participants live in communally held and governed structures, including "ejidos", which are federally recognized common property holdings with land tenure and governance rights granted to a set number of households, and "comunidades", which are indigenous lands.[1]. Ejidos are composed of two different kinds of property rights over land: private parcels and commons. Forest land is usually located in the commons. Under the PSAH program, both private and communal landowners may choose to enroll forested parcels containing all or a portion of their property. Once accepted, beneficiaries must maintain forest cover within the enrolled parcel, but are allowed to change land cover in other parts of their property. Satellite image analysis and/or ground visits are used to randomly verify forest cover on enrolled parcels (Muñoz-Piña et al. 2008, Wunder et al. 2008*b*).[2] If CONAFOR finds deforestation due to intentional changes such as logging or conversion to agriculture or pasture, these parcels are removed from the program and payments stop, whereas if forest loss is due to natural causes such as fire or pests, payments are reduced (Muñoz-Piña et al. 2008).

We study the impacts of the program on beneficiaries entering in the 2004-2009 cohorts. Table 2.1 shows the annual payment rates, total area enrolled, and the number of parcels accepted and rejected for these cohorts. Payments correspond to approximately $36 USD per hectare for cloud forest and $27 USD per hectare for other forest types. The initial rates were based on estimates of the per hectare opportunity cost of growing maize (Muñoz Piña et al. 2008) and have since been adjusted to match inflation. Our survey data indicates that

[1] The Mexican ejidos and comunidades resulted from the land reform that extended from 1917 to 1992. Land reform redistributed an area equivalent to half the country with most changes during the 1930s and 1940s (Sanderson 1984, Assies 2008)

[2] External offices, usually NGOs or private consultants, provide technical assistance to beneficiaries and are in charge of monitoring program implementation in the field and reporting progress to CONAFOR on a regular basis. In addition, program officers may visit the enrolled parcels randomly or visit those areas where there seems to be evidence of change in forest cover.

these payments are significant in relation to income. On average, annual per capita payments for households in common properties are approximately $130 USD, which is greater than 1 month of work at minimum wage. For private property households, the average per household payments are approximately $3050 USD per year, which is 12% of household income, given the estimated income brackets of the private property households.[3]

2.2.2 Overview of the analysis

We use two novel data sets to evaluate environmental and socioeconomic impacts of PSAH. Figure 2.1 shows the overall structure and timing of our data. To evaluate environmental outcomes, we collected annual data on land cover from 2003-2011. Program enrollment over time is based upon spatial data on the boundaries of accepted and rejected parcels from each program cohort. To evaluate household outcomes, we designed and conducted a community and household survey covering a subset of beneficiary and rejected applicants from the 2008 cohort. The survey was conducted in 2011 and includes recall questions establishing a baseline for the year 2007.

As described in the following two sections, we identify program impacts from panel regressions comparing changes over time in outcomes for program beneficiaries and rejected applicants. Before estimating impacts, we pre-match both data sets to ensure covariate

[3]The mean per capita payment in common property communities is 1,539 pesos. This was calculated taking into account the annual payment each community receives from the PSAH program excluding payments for technical support. This number is a lower bound as it includes the total population in the community, including children and older adults. The final amount was converted to US dollars using the exchange rate reported for the 15th of July of 2011 (11.72 pesos/ USD). The monthly minimum wage was calculated taking into account the daily minimum wage reported by CONASAMI. The average daily minimum wage in 2011 for the whole country was 58.1 pesos. Assuming 20 working days within a month, the monthly minimum wage is 1,161 pesos. Using the previous exchange rate, this is equivalent to 99 USD. For private households, the mean payment per year is 35,777 pesos. Given the exchange rate, this is equivalent to 3,053 US Dollars. Since the survey does not have information about households' income, we use income data coming for the National Income and Expenditures survey (ENIGH), collected by INEGI in 2010, and assume that private households in our sample are located in the upper 3 deciles of the income distribution. According to the ENIGH, the average quarterly income for the upper 3 deciles is 72,398 pesos, so average annual income aprox. 289,593 pesos. Therefore the PSAH payments represent 12% of this total annual income.

overlap, particularly for characteristics that might influence selection into the program and program outcomes. A strong body of research indicates that matching methods can improve covariate overlap and reduce potential bias in regression analysis by ensuring that treatment and control groups are as similar as possible (Dehejia & Wahba 1999, Ho et al. 2007, Stuart 2010). By eliminating as potential controls parcels which do not share the same observable covariates as program beneficiaries, we generate a more plausible estimation of what beneficiaries would have done in the absence of the program. After matching, impacts are identified from regression models with location or household-level fixed effects, thus controlling for possible time-invariant unobservable differences between accepted and rejected parcels or households. The validity of both the environmental and the wealth estimations relies on the assumption that trends in beneficiary and non-beneficiary groups would have been the same in the absence of the program. As shown below, our data indicate similar pre-program trends for beneficiaries and rejected applicants, making this a plausible maintained assumption. In the next two sections, we provide the details of the data construction as well as our pre-matching strategies for each dataset.

2.2.3 Environmental Outcomes and National Program Data

Land cover

To assess the program's impacts on land cover, we use the average dry season normalized difference vegetation index (NDVI) in each year from 2003-2011. NDVI measures the "greenness" of vegetation based on the reflectance signatures of leafy vegetation (NASA 2012). Deforestation or significant forest degradation is indicated by a decrease in average annual NDVI. We use the dry season NDVI measures (February 15 - April 15) because they are less likely to be influenced by rainfall and because it is easier to differentiate agriculture from forest during the dry season. We use composites of MODIS data from the Aqua and Terra satellites, which provide weekly data covering all of Mexico at a pixel resolution of

250m x 250m (aprox. 6 hectares). Although the data used in this paper was newly constructed for this project, similar methodology has been previously established and field-tested by the Mexican National Forestry Commission (CONAFOR 2011, Meneses-Tovar 2009*a*,*b*). Economists have also relied on NDVI decreases to measure deforestation in previous research in both developed and developing countries (Foster & Rosenzweig 2003, Mansfield et al. 2005, Burgess et al. 2011). We use a continuous measure of NDVI in order to pick up possible small areas of loss or degradation within pixels. Finally, we control for possible variability in NDVI due to rainfall by including measures based on monthly rainfall from NOAA (NCEP CPC Mexico daily gridded realtime precipitation, .25 x .25 degrees resolution).

National program data and points sample

Spatial information on the boundaries of beneficiaries and rejected parcels was collected from the Mexican National Forestry Commission. Figure 2.2 shows the boundaries of parcels applying between 2004-2009 and indicates that the program is truly national in extent, with both accepted and rejected applicants spanning the country.

In seeking to evaluate the land cover impacts of the program using the national program data, we face two data construction issues. First, renewals and reapplications create spatial overlap between parcels from different cohorts. To deal with this, the unit of observation for the land cover analysis is a fixed location or "point". Using fixed locations allows us to code the status of each point in each year. Due to the timing of the program, we code beneficiary status according to whether a point entered the program in the previous calendar year. As is indicated by the timeline in Figure 2.1, applications to the program happen on an annual cycle that is offset from our outcomes: eligibility and selection rules are announced each year in January-February, applications are developed and processed March-June, decisions are published in July-August, and payments are started by the end of the year. Since NDVI is measured from Feb-April, our estimations use the enrollment decisions from the prior calendar year.

The second issue is that land use outcomes are likely to be spatially correlated due to similarity in geographies or influence of neighbors (Robalino & Pfaff 2012). We therefore do not use all possible fixed locations but instead create a random sample of fixed points. These are chosen at a density of aprox. 1 point per square km from within the boundaries of all forested areas submitted for application in the 2004-2009 cohorts.[4] Given the resolution of the NDVI data, this means that we sample aprox. 1 out of every 16 available NDVI pixels. The points are spaced randomly (not in a grid) and we also cluster standard errors of estimates by parcel in case spatial correlation remains.[5] We define parcel as the first non-overlapping area of land submitted by an individual landowner; on average there are 7.9 points per parcel (parcel average size is aprox. 800 ha or 8 sq km). The full sample of applicant points contains 17,307 locations which were beneficiaries in at least one cohort and 18,456 locations which were never beneficiaries. For each of these points, we collect a series of fixed geographic covariates related to the eligibility and selection rules for the program (Table 2.2). We calculate the NDVI level for each point from 2003-2011 giving us a balanced panel with 9 years of outcome data for each location.

Program selection and pre-matching of the points sample

Before estimating impacts, we pre-match data with the goal of reducing possible bias by ensuring covariate overlap between beneficiary and rejected applicant locations (Ho et al. 2007, Imbens & Wooldridge 2008, Stuart 2010). The characteristics we match on are covariates which determined selection into the program and could influence deforestation outcomes

[4]We use only points that were forested according to both the INEGI Series III land use layer (circa 2002) and having NDVI greater than 30 in regions 1-3 and 60 in region 4. Since our analysis covers the 2004-2009 cohorts, we randomly sample within those cohorts. We exclude any points which enrolled in the 2003 cohort and control for recipient status of any points which later became beneficiaries in 2010.

[5]We check robustness to clustering by municipality and the results do not change. On average there are 46.6 points per municipality.

(see description of program evolution in Sims et al. (2014)). From 2004-2005, eligible land was required to be upstream from urban centers or inside priority mountains areas, to be above overexploited aquifers, and to have > 80% forest cover. Within applicants, priority was given to those with more forest cover. From 2006-2009, the eligible zones were expanded to larger portions of the country and eligible parcels were required to have > 50% forest cover. To select applicants, a system was introduced which gave priority on the basis of predicted deforestation risk from Mexico's National Ecology Institute (INECC)[6] , water scarcity, location in protected areas or priority mountain areas, and location in a high poverty or majority indigenous municipality. As can be seen in Table 1, a substantial number of applicants were rejected each year, with the percentage of rejected applicants growing over time.

Table 2.2 shows summary statistics for points within accepted and rejected parcels. In addition, to understand how program applicants relate to Mexico as a whole, we also show summary statistics for a random sample of pre-program forested locations across all of Mexico. We calculate the normalized differences in means between all forested areas and beneficiary points and between beneficiary points and rejected applicant points (columns 5 and 6). The normalized difference is the difference in means between the treated and control groups divided by the standard deviation of the treated group and is the most commonly accepted diagnostic used to assess covariate balance (Rosenbaum & Rubin 1985, Stuart 2010). The statistics indicate that the program beneficiaries are fairly representative of all forested land in Mexico. The mean risk of deforestation for beneficiary points, according to INECC's index, is 2.48, compared to the national average of 2.85, indicating that the program did reach areas of moderate deforestation risk. However, the fact that the mean risk of deforestation for beneficiary points is only half of the maximum (INECC's scale is from 1-5) indicates scope to better target the program. Since INECC's deforestation index is not available for all points, we also create our own risk of deforestation index using only

[6] "Index of Economic Pressure to Deforest / Risk of Deforestation" version 1. Methodology available at http://www.inecc.gob.mx/irdef-eng.

never enrolled points and geographic determinants of deforestation.[7] Our index suggests that beneficiaries were at slightly higher risk of deforestation than the national average, but the mean difference is only 0.09 standard deviations. The summary statistics in Table 2.2 also confirm the selection criteria discussed above. Beneficiary applicants had higher initial NDVI, were somewhat closer to urban areas and had a slightly higher risk of deforestation than rejected applicants. Enrolled land was also more likely to be in communal properties and in majority indigenous municipalities.

Given these differences, we pre-match points using covariate matching in order to ensure similarity of beneficiary and rejected locations.[8] We require exact matches within geographic region, land tenure type (common property vs. other[9]), and year of first application; the other covariates used are NDVI in 2003, forest type (cloud forest vs. not), overlapping with an overexploited aquifer, the degree of water scarcity, being inside one of the priority mountains, being inside a protected area, slope, elevation, distance to the nearest locality with population greater than 5000, baseline municipal poverty, and being in a municipality with majority indigenous population. We match nearest neighbors on the basis of the Mahalanobis metric and with replacement and drop any repeated points before the regression analysis so that each control point appears only once in the final dataset.

[7]The risk index is constructed using GIS layers indicating areas of "suspected deforestation" across Mexico for the years 2004-2009 and 2011 (Forest Monitoring). Using the untreated applicant points, we regress suspected deforestation on elevation and slope categories, vegetation type categories, and the natural log of the distance to the nearest city. The coefficients from this regression are then used to predict the probability of deforestation for all the points in the sample. Because the distribution of this probability is skewed, we use ln(100x probability of deforestation) for our index. The low probability of deforestation in most of the country leads to a large number of negative values using this index.

[8]We also attempted propensity score matching to improve covariate balance. We found that it resulted in larger reductions in normalized difference for baseline NDVI but this came at a cost of substantially worse balance on several other covariates which are potentially important drivers of deforestation (results available from authors). These differences reflect the fact that Mahalanobis matching essentially treats covariates as equally important when looking for "good" matches while propensity score matching does not (Stuart 2010).

[9]Other types of beneficiaries include private landowners that apply to the program either individually or in groups or associations.

The 4th column of Table 2.2 shows summary statistics for the matched rejected points and Figure 2.3 shows the changes in normalized differences and covariate distributions due to matching. We find that matching improves the balance across nearly all covariates, with some cost coming from an increase in differences by municipal poverty and water scarcity. Post-matching, none of the normalized differences are greater than .15 standard deviations, which is below the suggested rule of thumb of .25 standard deviations (Rubin 2001, Imbens & Wooldridge 2008). Since averages can obscure possible underlying lack of overlap in the covariate distributions, we also examine the full distributions across beneficiary categories for continuous covariates and find that matching reduces these differences as well (Figure 2.3).

2.2.4 Household outcomes and survey sample

Household data: Survey Sample

To assess the socioeconomic impacts of Mexico's program, we designed and conducted a nationally representative community and household survey. The survey was conducted in 2011 and covered beneficiary and non-beneficiary applicants from the 2008 PSAH cohort (Figure 2.4). To establish baseline measurements, surveys included recall questions about assets and investments in 2007, the year prior to program implementation.[10]

We used a stratified regional sampling strategy covering the four regions of the country (see Figure 2.4). Within each region, we randomly selected 3-4 areas corresponding to the boundaries of Landsat satellite footprints (areas 180 x 180 sq km) and obtained the set of 2008 applicants within each area[11]. Before sending surveyors into the field, we pre-matched

[10] We acknowledge that using recall data could potentially bias our results if memory failure is systematically different between beneficiary and non-beneficiary households. If measurement error is equal between the two groups, then it should attenuate our results towards zero. In implementing the survey we attempted to reduce the concern of differential recall bias by avoiding the use of reference points related to the program. Moreover, we focus on outcomes that are easy to recall, such as household assets or education.

[11] Analysis of this sub-sample of Landsat data (30m x 30 m pixels) is not complete. Footprints were chosen

2008 program beneficiaries and rejected applicants in order to ensure similarity between treated and control parcels. Pre-survey matching used exact matching within region and tenure type and one-to-one covariate matching on distance to the nearest major locality, elevation, slope, the area of the parcel, road density, the average locality poverty level, and forest type. Parcels without good matches were eliminated and survey priority was given to landowners with multiple good matches.

The final household survey sample is composed of 118 private households (61 beneficiaries and 57 non-beneficiaries) and 1096 households in common property communities (590 beneficiaries and 506 non-beneficiaries) distributed over 116 communities. Table 2.3 indicates the breakdown of surveyed households in each region and Figure 2.4 shows the locations of the surveyed landowners. The surveyed properties are similar in regional distribution and forest type to those enrolled in the program in 2008 (results available from authors).

In the field, survey enumerators further stratified the sample by land rights status within communal properties. Based on lists provided by community leaders, surveyors randomly selected five households with full land-use rights and voting power ("ejidatarios" or "members") and five without ("non-ejidatarios" or "non-members").[12] Within the ejidos, non-members are usually descendants of the original members who are denied membership rights by the legal restriction on inheritance to only one child. Although they do not have formal land ownership, they often farm within the ejido, sometimes on commons land. We might therefore expect differential impacts on individuals with and without full membership rights to land. In the present paper we test for average impacts on households and for differences between members and non-members, but detailed modeling and analysis of possible

randomly from within the set containing multiple images across time. Some last minute adjustments in the sample were made due to security concerns: two footprints were swapped for nearby ones and two were added to increase sampling possibilities.

[12]Members of comunidades, in which all members have full rights, are grouped with ejidatarios.

internal community dynamics are beyond the scope of this paper (see however ongoing analysis by Yanez-Pagans (2014)).

Household outcomes

We examine impacts on outcomes indicating both short and long term wealth effects: food consumption, purchase of durables, household improvements, and productive investments. All estimations compare differences over time in outcomes between beneficiaries and non-beneficiaries except those on food consumption, which use cross-sectional variation (recall questions were not asked about food consumption).

Durables purchases and housing characteristics are aggregated by price to reduce dimensionality.[13] The durables index includes the following assets: television, refrigerator, computer, car, stove, phone, and cell phone. The housing index includes wall and floor construction materials and number of rooms. The prices used for weighting are based on data provided by households, from consumer agencies in Mexico, and estimates of the values of housing characteristics. The 2007 prices are used to construct the indices for 2011 in all cases.

For agricultural and human capital investments, we use as outcome variables: the number of cattle and number of small animals owned by the household, the presence or absence of investment in livestock infrastructure, agricultural inputs, and agricultural equipment, and school attendance for all household children in schooling age.[14] For food consumption, we construct an index using prices reported by households and whether or not they purchased a

[13] We also experimented with two other indices common to the development literature: the principal components analysis (PCA) on ordered data, which gives more weight to observations which provide more information about the variation in the data, and an inverse proportion index, which gives greater weight to assets which are relatively rare, like cars and computers, and less to more common assets, like televisions. Results for these two other methods are available upon request and are generally consistent with the price index results.

[14] For all binary variables, we estimate linear probability models, though the results are consistent with estimates obtained using a probit.

particular food item in the past month. The food items of interest are tortillas, cheese, milk, beef, pork, beans, tomatoes, and bread. Since food estimations are cross-sectional, they also include a series of covariates to control for observable differences across beneficiaries and non-beneficiaries. For those variables that have skewed distributions, such as the durables, housing, and food indices, as well as the number of cattle and small animals, we apply an inverse hyperbolic sine transformation (Burbidge et al. 1988*a*).[15]

Tables 2.4 and 2.5 shows baseline summary statistics for surveyed beneficiary and non-beneficiary households with complete information on the relevant covariates. Since households in common property communities are substantially poorer and less well educated on average than private property households, we analyze them separately.[16] In general, the samples of both communal and private property households have good balance on baseline covariates, with none of the normalized differences greater than .25 standard deviations (Tables 2.4 and 2.5). However, one potential balance concern is that although we matched on the basis of geographic covariates prior to surveying landowners, household level covariates were not observable at this step. For common property households, we therefore use a second round of matching based upon pre-program participation in forest conservation activities, which are a key determinant of the opportunity costs of participating in the program (Table 2.4).[17]

[15] Unlike traditional log transformation, the inverse hyperbolic sine transformation is defined at zero and can be interpreted in the same way as a logarithm dependent variable.

[16] Chow tests confirm that the coefficients of covariates in all regressions are different across both groups of households.

[17] Forest conservation activities include: constructing or maintaining firebreaks, constructing fences to avoid cattle entering into the forest, doing forest patrols, reforestation, soil conservation activities, and pest control, among others. We only pre-match households in common property communities as we don't have baseline data on cooperation for private landowners. The matched sample uses 1:1 covariate matching with replacement on the Mahalanobis metric. Households are matched exactly by region based on their baseline cooperation levels in forest conservation activities. Our results are also robust to using the full sample.

2.3 Avoided deforestation: Estimation and results

2.3.1 Main estimation strategy

We estimate panel regressions with point level fixed effects on the matched subsample using the following specification:

$$MNDVI_{ipst} = \beta Beneficiary_{ipst} + \delta_1 ln(dry_{it}) + \delta_2 ln(grow_{it}) + \delta_3 sdrain_{it} + \delta_4 hurricane_{it} + \alpha_{st} + \alpha_i + \varepsilon_{ipst} \quad (2.1)$$

where MNDVI is the mean dry season NDVI value for point i in parcel p, state s, and year t. The variable beneficiary is equal to 1 if the point was enrolled in the program in the previous year's cohort; β is the average program impact.[18]. Several variables are included to control for rainfall: ln(dry) is the natural logarithm of dry season rainfall across the entire sample. Similarly, ln(grow) uses rainfall in the other months prior to the dry season. To control for extreme weather events, particularly hurricanes, we also include the standard deviation of rainfall across the year, and a dummy variable for being in the top 10th percentile of rainfall during the hurricane season (October/November). State-year fixed effects (α_{st}) control for possible economic shocks to states in each year and point-level fixed effects (αi) control for unobservable fixed land characteristics. Standard errors are clustered at the parcel level to account for spatial and serial correlation. Clustering at the municipal level has little effect on the standard errors and does not change the significance of the estimates.

Our main effects are shown in Table 2.6. The first column indicates the simple preferred specification which estimates program impact as a difference in NDVI levels; the second estimates impact as a difference in trend and the third allows program impact to vary with

[18]We introduce the lag to take into account the timing of the applications versus the timing of the NDVI measurements (see Section 2.2.3)

additional years in the program. All specifications indicate a positive and significant impact of the program on land cover (an F-test indicates that the coefficients in column 3 are jointly significant at the 5% level.)

To interpret magnitudes, we compare coefficients to counterfactual trends of NDVI loss for matched controls and all non-recipient data.[19] The bottom row in Table 5 indicates the effect size of our estimates as a percentage of the expected five-year (contract lifetime) loss of NDVI. Our simple preferred specification (column 1) indicates that the program offsets approximately 40-51% of the expected NDVI loss. The second column indicates that the program changes the downward trend in NDVI by 0.0265 per year, or 28-36% of the expected trend. The third specification indicates that if we allow the effect to increase with additional years in the program, we offset 54-69% of the expected loss. We note that these results are similar to the results found in Alix-Garcia et al. (2012), confirming that results from the 2004 cohort are generalizable to the broader program. Yet, it is important to also note that our estimates indicate that the program does not completely stop loss of land cover.

Including point-level fixed effects gives conservative estimates of program impact by accounting for all fixed omitted variables at the point level. However, it does not take advantage of any within-parcel variation which may provide more information about the history of deforestation for a given landowner. Columns 4-6 therefore include fixed effects at the level of the submitted parcels and with point level control variables, including NDVI in 2003 as a measure of baseline forest cover and running the estimation on mean NDVI from 2004 to 2011. This indicates similar but somewhat higher program estimates, with effect sizes ranging from 28% to 93%.

[19] Among matched controls, we find an average annual loss of NDVI between 2004 and 2011 of -0.0731 with point fixed effects and -0.0748 using parcel fixed effects. Using all initially forested points, we find a trend of -0.0935 with point fixed effects and -0.1250 with parcel fixed effects

2.3.2 Parallel trends test and robustness checks

Identification in the empirical approach above comes from differences over time in within-beneficiary versus within rejected-applicant land cover. The validity of the estimates thus rests on the assumption that in the absence of the program, the expected trends in these two groups after controlling for fixed characteristics, state-year trends and rainfall, would have been parallel. Although we cannot test this assumption directly, we can assess whether time trends were parallel prior to enrollment. Columns 1 and 2 of Table 2.7 test for differential pre-program trends by interacting future beneficiary status with the time trend on data for years prior to enrollment or application to the program. We do not find significant differences in the mean NDVI trend across time for future beneficiaries, compared to future rejected applicants.[20] The estimated coefficient is negative, indicating a possibly larger deforestation rate among future beneficiaries, which is consistent with attempts to target the program to parcels at higher risk of deforestation and would bias our estimates of program impact downward.

A second way to test the robustness of our results is to use different groups of rejected applicants. As indicated in Figure 2.1, there are three main reasons for rejection: 1) having all the qualifications but being rejected for lack of funding due to program budget constraints (∼ 40% of sample), 2) failing to meet the geographic requirements, such as being outside the eligible zones or having less forest cover than is required (∼ 30%), 3) lacking complete paperwork or necessary documentation (∼30%). Columns 3-7 show robustness checks using different subsets of the data in order to address the concern that results might be sensitive to which set of rejected applicants are used to establish counterfactual trends. Column 3 uses only points which were beneficiaries at some time between 2004-2010. Column 4 uses

[20] We also tested for parallel trends using interactions between each year and future beneficiary status prior to enrollment or application to the program. While there were significant differences between future beneficiaries and rejected applicants for some pre-program years using the full sample, all differences were small and not statistically significant using the matched sample (results available upon request).

as controls only parcels which met all the requirements but did not receive payments due to lack of funding ("approved but unfunded"). Column 5 uses only those points within eligible zones that did not meet requirements, while column 6 restricts controls to all points within the eligible zones. Column 7 restricts controls to those which had high NDVI at baseline, indicating higher levels of forest cover. Column 8 uses as controls parcels which were rejected from the program only once. All specifications include the same set of controls as column 1 of Table 2.6. In all cases the results are robust to using these different subsets of controls and all estimates are larger than our baseline specification using all matched controls.

Finally, in columns 9 and 10 we show estimates using the full sample of unmatched points rather than the matched sample. The results remain positive but are smaller and not statistically significant with point fixed effects; they are smaller but significant when we use the full sample and parcel fixed effects. The smaller coefficient estimate for the entire sample is consistent with the fact that CONAFOR has selected into the program parcels at higher risk for deforestation, so that when one compares rejected parcels that are more similar, as we do by restricting the sample to appropriate matches, the estimated program effect is larger.

2.3.3 Dynamic behavior and reapplication

Possible dynamic behavior by program beneficiaries raises two potential issues. The first is that our effects are estimated using variation coming from both entry into and exit from the program. We might therefore be concerned that our estimates are driven by large behavioral changes on exit. A second, policy relevant concern is that although the program may have real short term impacts, those program effects might not be permanent if landowners choose to deforest after they exit the program.

To respond to these concerns, we recode the beneficiary variable to be equal to 1 for all years after the first year of acceptance. This treats beneficiary status as "permanent", limiting the identifying variation to that which comes from entry into the program and including all post-entry behavior in the impact estimate. As shown in the first column of Table 2.8, the result remains positive and significant, although slightly smaller than our baseline specification in Table 2.6, column (1). However, this result is not very surprising given that only two of the cohorts - 2004 and 2005 - have time to exit the program before 2011. Of these, around 35 percent reapply and more than half of those re-applicants renew their contracts. We therefore also test for differential permanent impacts within the two cohorts that can potentially exit the program (column 2) and observe a positive, but statistically insignificant difference, indicating that the permanent positive effect of the program still holds among cohorts that can exit the program.

Although we are limited by the data in our ability to explore dynamic impacts of the program, the next two columns of Table 2.8 test whether or not the permanent impact of the program varies for those that attempt to reapply (column 3), or for those that reapply and are accepted (column 4). None of these interactions is statistically different from zero, but the direction of the coefficients suggests that long term forest conservation may require long term contracts. This is not surprising, since if the purpose of the program is to prevent behavior that would have otherwise occurred, we should expect reversion in the absence of payments. An additional piece of evidence suggesting that the program may work partly by delaying deforestation is that rejected applicants deforest more after being rejected (column 5). This does not bias our estimates since both groups would engage in similar anticipatory deferred deforestation behavior until they know their enrollment status, but it is a potential mechanism for the result. Clearly, future work needs to explore whether program beneficiaries permanently shift livelihood strategies or whether the effects are undone once payments stop.

2.4 Socioeconomic impacts: Estimation strategy and results

2.4.1 Estimation strategy

We estimate program effects on durable purchase (and loss), household improvements, and productive investment using a household fixed effects model:

$$A_{ipt} = \beta Beneficiary_{pt} + \alpha_i + \alpha_t + \varepsilon_{ipt} \tag{2.2}$$

Where A_{ipt} represents outcomes for household i in community p at time t and $Beneficiary_{pt}$ measures program enrollment, which equals zero for all households in 2007 and one for households that live in a community that participates in the program in 2011 or private landowners that are recipients in 2011. The estimation includes both household (α_i) and time (α_t) fixed effects. Standard errors are robust and clustered at the community level. For private households, the errors are simply heteroskedastic robust, and the p subscript is superfluous. Given the stratified sample design, we estimate weighted regressions, where weights are given by the inverse of the probability of selection in each region.

2.4.2 Average impacts

Table 2.9 gives estimates of program impact on consumption goods. Panel a shows common property impacts and panel b impacts for private properties. We observe that none of the simple treatment effects is statistically significant, although all are positive for households in common properties. For households in private properties, the coefficient on the durables index is positive while the other two are very close to zero[21]. The magnitudes are also quite

[21] Given that we are testing multiple outcomes, the most conservative statistical significance level to use would be $\alpha/8$, or $p < .00625$ (8 outcomes; Bonferroni method). Only the heterogeneous treatment effects across poverty with the durables and housing index (for households in common properties), and with investment in agricultural inputs (for private landowners) are robust to this conservative correction (results available

small ranging from 0 to 4 percent increases from baseline levels of the index, or from 0 to less than 0.10 standard deviations. Considering within-community heterogeneity, there are no significant differences in consumption impacts for members versus non-members although the estimated marginal effects for members are positive and are positive or close to zero for non-members. The minimum detectable effects within common property communities range from .008 (about 0.04 standard deviations) for the housing index to 0.07 (0.17 standard deviations) for the durables index.[22] This suggests that, while the data is noisy, were there a program impact of a magnitude greater than 0.2 standard deviations, our sample would be likely to detect it at conventional significance levels. For private properties, the minimum detectable effect sizes are larger ranging from 0.01 standard deviations in the case of the housing index to 0.32 standard deviations for the durables index but still do not indicate that a substantial downside to the program likely occurred.

Table 2.10 shows average effects on investments in agricultural/pastoral production and education. In the common properties, we observe a positive and marginally significant increase in the number of cattle (5%). We do not see sizeable or significant changes in the likelihood of investing in livestock infrastructure, agricultural inputs or agricultural equipment. However, we do observe a marginally significant increase (around 13 percent relative to baseline) in the probability of attending school for children between the ages of 15 and 17 years old. We also see positive but insignificant increases in school attendance for children between 12-14 years old and 18-22 years old. There are no significant impacts for private households, although the coefficient on schooling implies a 6 percent increase relative to baseline in the probability of attending school for students between 12-22 years.[23]

upon request). They are also robust to using a p-value Sidak correction. Heterogeneous treatment effects are reported in the next section.

[22] The minimum detectable effect is the smallest true difference that has a good chance of being found to be statistically significant given the sample size. For these calculations, we assume a statistical power of 80 percent and consider the standard two-tailed test of statistical significance at 0.05.

[23] Given that the sample size for private households is smaller we do not disaggregate by age group in this case as it is done for households living in common property communities.

2.4.3 Parallel trends and robustness

As in the environmental analysis, the identification of impacts here depends upon the assumption that in the absence of the program, trends in beneficiary and non-beneficiary households would have been the same. We cannot directly test this assumption using our survey data, however, the Mexican government generates a poverty index for each locality in Mexico every 5 to 10 years. Using the data from 2000 and 2005, we test whether the change in this index is different across beneficiary and non-beneficiary localities. For common property localities, the t statistic for the difference in poverty change is 1.33, with a normalized difference of .056. For private property localities, the t statistic for the difference in poverty change is 1.08, with a normalized difference of .14. In both cases we fail to reject the null hypothesis that the trends in poverty just prior to program application were the same across beneficiary and non-beneficiary localities.

In addition, our results are robust to including all observations regardless of whether they have missing information in some covariates or to estimating impacts using the full sample of observations without matching on forest conservation activities. They are also robust to using a continuous treatment variable based on the per household program payments, instead of using a binary treatment variable.

In summary, the household results suggest that the program appears to have done no harm to households. When we consider within-community heterogeneity, we observe no significant differences in impacts for members versus non-members, indicating that the program is also not harming households without full land-use rights. However, the non-significant and small average impacts we observe may obscure important heterogeneity on other targeting dimensions or could indicate that there is little surplus to be gained once opportunity costs and participation costs are accounted for. These possibilities are explored theoretically and empirically in the next section.

2.5 Heterogeneity, tradeoffs, and policy implications

The previous two sections indicate that Mexico's PSAH is effective in reducing the rate of deforestation without harm to local livelihoods. At the same time, the low expected rates of deforestation for enrolled parcels suggest that targeting can be improved. Opportunities for managers to increase the environmental effectiveness of the program depend on whether there is systematic heterogeneity in avoided deforestation impacts that can be better exploited. The framework below highlights potential sources of heterogeneity and explores tradeoffs between avoided deforestation and poverty alleviation goals.

2.5.1 Framework

In order to illustrate the targeting problem faced by the program managers implementing payments for avoided deforestation, we discuss a simple rent driven model of land use (see e.g. Chomitz & Gray (1996), Samuelson (1983), Pfaff (1999), Robalino (2007), Angelsen (2008), Pagiola & Zhang (2010), Alix-Garcia et al. (2012)) . Assume that there is a set of landholders (indexed by i) who vary in multiple land characteristics (indexed by j) which may be used for program targeting, such as slope, altitude, ecosystem type, or distance to city. These characteristics may affect the returns to land uses either through productivity or cost. They may be correlated with each other and need not be spatially contiguous, but must have monotonic effects on rent.

Figure 2.5 shows a graphical representation of rent as a function of agricultural or forest land use (r_a or r_f) for one targeting characteristic (q_j), holding all else equal. Assuming each landholder chooses to allocate his land according to the highest rent, the value of that characteristic where landowners would initially be indifferent to either use is $q_j = b_0$ (the point at which agricultural rents equal forest rents). All else equal, landowners with values of q_j less than b^0 would choose agricultural use and those greater than b^0 forest use. As the returns to agricultural use rise over time and r_a shifts up, this boundary point will move to the right.[24] In the absence of any policy intervention, we expect landowners with values of q_j between b^0 and b^1 to deforest and change their land use to agriculture; these are the parcels "at risk" of deforestation.

PES programs are designed to offset this expected increase in agricultural rents by offering a corresponding increase in forest rents. We assume that the regulator must offer a fixed payment (this corresponds to the structure of Mexico's program as well as most existing PES schemes) leading to a parallel shift of r_f. However, regulators may also target the program to land with certain characteristics by using either geographic eligibility zones or a priority system (both were used by CONAFOR in the PSAH program). As shown in Figure 2.5, to achieve full avoided deforestation at least budgetary expenditure,[25] the regulator should choose a payment amount greater than or equal to the change in the agricultural rents (Δr_a) and should target parcels with q_j between b^0 to b^1. In partial equilibrium, the rent curve for forest would increase to r_f^{PESopt} and the indifference point between agriculture and forest would remain at b^0.

[24]Forest loss and degradation in Mexico are due to both human-induced change, primarily the expansion of agricultural or pastoral activities and logging, and to natural causes including fires, pests, disease, drought and storm damage (Deininger & Minten 1999, Alix-Garcia & de Janvry Elizabeth Sadoulet 2005, Bray & Klepeis 2005, Alix-Garcia 2007, Díaz-Gallegos et al. 2010). We prefer this model for simplicity but note that it emphasizes the agricultural and pastoral drivers of deforestation. Higher returns to agriculture may be due to population growth, high global agricultural commodity prices, or an increase in consumption of land intensive goods as the population gets richer (Alix-Garcia et al. 2012). Where illegal logging or natural causes of deforestation are significant, community decisions to protect forests may be also explained by the benefits generated by forest (including timber or non-timber forest products or local erosion control) relative to the costs of patrolling and maintaining the forest.

[25]Note that an efficient PES program would maximize environmental net benefits; these benefits might depend on land quality so full avoided deforestation might not be economically efficient. For simplicity, we assume uniform environmental benefits across land quality and focus on the cost-effectiveness of the program. Note however that cost-effectiveness cannot be assessed simply by comparing budgetary outlays to amount of deforestation avoided. The true costs of the program should include the administrative and transactions costs of running and participating in the program, and any distortionary effects of raising the program revenue on top of the opportunity costs implied by our diagram.

From this we see that the key to gaining high environmental effectiveness at least cost is to enroll only the parcels at risk of deforestation. To do this we need both adequate payments and targeting to high risk parcels. If payments are set too low, the program will not attract land at high risk of forest loss (e.g. the forest rent curve shifts up to r_f^{PESlow}, the agriculture-forest boundary shifts to b^{PESlow}, and avoided deforestation is only between b^{PESlow} and b^1). If eligibility is too broad (e.g. targeting landowners between b^0 and b^z), then only a small fraction of the payments generate behavioral change.[26]

This framework also demonstrates that environment-poverty tradeoffs in terms of who receives payments are determined by the underlying correlation between wealth and the characteristics determining land rents. If wealth is positively correlated with land value, then the land at highest risk of deforestation (i.e. b^0 to b^1) is likely to be owned by those in the middle of the wealth range. Thus if regulators target effectively, the poorest landowners will be excluded from participation. Opportunities to improve both environmental effectiveness and participation by the poor exist only if poverty is not well-correlated with the risk of deforestation and the regulator can prioritize poor households within the set of at risk parcels.

The framework also implies a likely tradeoff between household gains from the program and environmental effectiveness. Expected household surplus is the returns to forest minus opportunity costs, application/transaction costs, and participation costs. As apparent from Figure 5, the difference between the returns to forest use with PES in place and the forgone

[26]Note that this model is consistent with previous empirical and theoretical research suggesting heterogeneity in PES impacts across space. Arriagada et al. (2012) find larger avoided deforestation impacts of Costa Rica's PES program in the Osa region, where threats to forest are high. Wünscher et al. (2008) simulation shows that the avoided deforestation benefits of PES in Costa Rica could be increased by targeting based on landowners' participation costs, with higher payments to attract those with larger costs. Consistent with this, Pfaff et al. (2011) find that efforts to better target Costa Rica's PES payments starting in 2000 did improve avoided deforestation impacts from 2000-2005. Alix-Garcia et al. (2012) find more avoided deforestation where baseline poverty rates are lower and Honey-Rosés et al. (2011) find larger impacts of PES in protecting high quality habitat in the Monarca reserve.

returns to agriculture ($r_f^{PESopt} r_{a1}$) increases as rents decrease. (Where there is no risk of deforestation, this difference is the full amount of the payment.) Thus we should expect greater socioeconomic impacts of the program where the risk of deforestation is lower. However, if rents are indeed negatively correlated with wealth, PES should be progressive within the set of households that do receive payments.

In summary, there are three key predictions that emerge from this framework:

- Environmental effectiveness will be higher where the returns to agriculture/pasture production are higher, i.e., where geographic deforestation risk is higher.
- If the correlation between agricultural/pasture rents and poverty is negative and the program is targeted to high risk areas, then middle income landowners will enroll the largest amount of land, the poorest will be excluded, and program effectiveness will be higher among wealthier landholders.
- In this scenario, there will be greater wealth impacts in areas where deforestation risk is lowest, but within properties enrolled, relatively poorer beneficiaries will gain more wealth.

In the next sections we test for empirical findings consistent with these predictions.

2.5.2 Environmental effectiveness versus inclusion of the poor

This section tests the predictions that environmental effectiveness will be higher where deforestation risk is higher and where beneficiaries are wealthier. The estimations here use our baseline specification and sample, and interact proxies for deforestation risk and poverty with the beneficiary variable. Results are presented in Table 2.11, the first column of which shows the baseline estimation (repeated from Table 2.6) for comparison purposes. The second column shows interactions of the beneficiary term with distance from the nearest town, and with slope. In both cases, the results indicate that the program is more effective where deforestation risk is higher in points of lower slope, closer to cities. The third column shows an interaction term with an exogenous deforestation risk index which combines several characteristics (see footnote #7). As predicted by our framework, the interaction of this term with beneficiary status indicates that the program is much more effective where our measure of deforestation risk is higher. In particular, at the 80th percentile of deforestation risk, the program impact is .47 (se .09), increasing program effectiveness to nearly 70 percent, compared to the -0.07 and statistically insignificant impact for a point in the 20th percentile of deforestation risk.

In terms of poverty, we find significantly less avoided deforestation at higher levels of baseline municipal poverty (Table 2.11, column 4), suggesting that there is no easy strategy to increase avoided deforestation and enroll more poor.[27] However, when we break beneficiaries down into common property versus private and other types of beneficiaries, we find that the program is most effective in the common properties. This suggests a possible win-win solution by targeting to common property beneficiaries, who are generally poorer than private property landowners: the average municipal poverty index for the common property points in our sample is .326, relative to -.067 in the private properties. Enrolled common property lands tend to be of lower quality than privately owned land along the dimension of slope (12.6 v 10.1), but are generally found at similar distance (around 33 km) to the nearest large locality, and have similar predicted deforestation risk.[28] Thus the differences in effectiveness are unlikely to be driven by differences in land quality or distance costs.

[27] We also test for and find no significant heterogeneity in effectiveness by municipalities with majority indigenous status a metric often associated with poverty in Mexico. We test for heterogeneity by availability of water and being in an overexploited aquifer. We find no significant differences in avoided deforestation by overexploited aquifer status but we do find significantly less avoided deforestation with higher water availability (coefficient = -0.0016, standard error 0.0004). Water availability is positively correlated with more poverty (corr=0.44) so additional targeting to low water availability areas in order to increased avoided deforestation or hydrological benefits again implies a likely tradeoff with poverty reduction goals.

[28] Both have risk of 2.4 according to the INECC index; common properties have risk of -0.98 and private properties a risk of -0.95 according to our index.

We speculate that they may be explained by available opportunities not captured by the geographic risk characteristics. Within common properties, households tend to be more dependent on agriculture (in our survey over 80% of common property households report participating in agriculture in 2007 compared to around 50% of private households), implying fewer off-farm labor opportunities. Given the role of agriculture as a driver of deforestation in Mexico, these results further support the claim that the program is more effective in areas of higher deforestation risk.

2.5.3 Poverty alleviation

This subsection examines variation in the impacts on household wealth measures by deforestation risk and baseline poverty. To explore heterogeneity by risk of deforestation, we create a metric of deforestation risk using the predictions from the environmental section, and divide our sample into "high" (above the median), and "low" (below median) deforestation risk.[29] To examine variation in impacts by baseline poverty, we divide the sum of the housing and durables indices in 2007 at the median, and include an indicator for "below median" as the proxy for poverty.

Tables 2.12 and 2.13 show the results for the two types of interactions. For households in common properties, the deforestation risk estimates are generally consistent with the theory presented above. Where deforestation risk is high, we observe less program impact on the purchase of durables and housing improvements, although the interaction term is only statistically different from zero for durables (Table 2.12). The results imply that for low risk properties, the durables index is about 7 percent higher relative to baseline, due to the program, while there is no positive impact for high risk properties. With respect to investment outcomes, we observe that investment in the education of students 18-22 years old is lower for individuals living in high deforestation risk beneficiary properties (Table 2.13),

[29] In order to create a parallel index for the socioeconomic analysis, the coefficients used to create the deforestation risk index used in Table 10 are applied to the parcel level covariates.

with marginal significance. For private households, we find marginally significant negative effects of deforestation risk on the housing index, but no other significant heterogeneity (Table 2.12). The results on small animals, livestock infrastructure and agriculture investment do have signs consistent with theory, but are not statistically different from zero (Table 2.13).

Recall that our model also suggests that if deforestation risk is negatively correlated with wealth, then one should observe greater wealth effects among relatively poorer beneficiaries of the program. Consistent with this, we observe positive and significant interaction terms for the durables and housing indices in common properties (Table 2.12; a 12% increase in the durables index, and 1% in the housing index). For poor private households, there is also a significant increase in the durables index (10%) and a small increase in the housing index (0.4%). With regards to investment, results are mixed. For common properties, we see significantly greater investment in higher education (students 18-22 years), agricultural equipment, and agricultural inputs (Table 2.13). For private properties, there may be more investment in multiple agricultural outcomes by those below median wealth, but we also see significantly less investment in agricultural inputs (Table 2.13). Together, the results suggest that available surplus from the program is more likely to be used for consumption than investment by poorer households.

2.5.4 Participation costs

The surplus expected by landowners from an avoided deforestation program should be the difference between the payments and the opportunity costs of foregone deforestation, minus transaction and implementation costs. Part of the explanation for small household impacts could be that there is simply little surplus to be gained by beneficiaries. This seems counterintuitive since the predicted risk of deforestation among beneficiaries and annual deforestation rates in Mexico during this period imply that landowners would have deforested only about 1.5 hectares out of every 100, and thus that the opportunity costs of avoided deforestation are likely to be small.[30] Yet it may be explained if the payment size is relatively small compared to the transaction costs or forest maintenance costs of participating.

Survey participants were asked questions regarding the application process for the program, including the time spent to apply and payments to intermediaries, and regarding the implementation costs in terms of forest conservation activities. We find that application costs are relatively small as a fraction of the overall payments across five years, constituting a ratio of approximately .006 and .016 of total payments to common and private property beneficiaries, respectively. However, a surprising result from the survey data is that program implementation costs are considerable compared to payments. The most important household level costs of the program are related to labor engaged in forest conservation activities. Community leaders in beneficiary common properties report on average a greater number of worker days per year spent in fire prevention (+66 days), pest control (+17 days), and forest patrols (+142 days) compared to non-beneficiary common properties. Valuing all labor (both paid and unpaid) at the minimum wage, we estimate that the median ratio of the cost of additional labor in beneficiary communities relative to the amount of the payments is 0.84.[31] Private households also report more days spent in fire prevention (+38 days), pest

[30] From 2000-2010 the estimated annual rate of forest loss in Mexico was approximately 0.295% (FAO 2010). Over 5 years this is 1.5 %; Table 2.2 indicates that beneficiaries had a similar risk of deforestation to the country as a whole.

[31] The mean annual payment for common property communities, excluding payments given for technical assistance, is 352,567 pesos. This is equivalent to 30,082 US Dollars. These calculations subtract labor which could have been generated by other CONAFOR programs also operating at the community level. We note that the estimates of labor changes induced by the program in common properties are smaller if we use data reported by the households themselves. For all households in common properties, we find that on average the program induces a change of 4.4 additional days of labor in forest conservation (relative to the changes in labor in non-beneficiary communities). For non-member households, the program induces 6 additional days of forest labor and for member households, the program induces 3.6 additional days of forest labor. Valued at the minimum wage, 4.4 days of labor is worth about 255 pesos, which amounts to only 16 percent of the estimated mean per capita payment (assuming the total payment is divided evenly among members). We think this difference may be explained by a skewed distribution of forest conservation activities among householdsthe system of rotating responsibilities for community activities means that some households will disproportionately contribute to forest conservation in any given year but might not have been surveyed in the household sample. Also, in many communities the payments were used to hire labor for the extra activities and some of the labor may have come from outside of the community.

control (+4 days), and forest patrols (+76 days). In private households, the median ratio of additional labor costs to payments is 1.1. These high ratios suggest that the program may just cover the additional costs of forest protection, particularly against longer-term threats to forest health or from illegal logging.[32] This means that payments may be helpful for environmental effectiveness but again suggests the difficulty of using them to boost poverty alleviation.

2.6 Conclusion

Our analysis indicates that Mexico's Payments for Hydrological Services program has succeeded in significantly reducing expected land cover loss. This justifies some optimism about the potential for payments for ecosystem services to maintain environmental quality. But our findings also suggest the need to be cautious with respect to whether PES can generate significant poverty alleviation. Although program beneficiaries did gain in wealth while participating in the program, we find little evidence that this change can be attributed to the program.

In addition, our study highlights the likely tradeoffs in targeting in order to improve results on either dimension. Specifically, the poor in Mexico presently possess a significant amount of forest, but that forest is not necessarily at the greatest risk because it is often further from markets or of lower quality. Our results suggest that the greatest avoided deforestation impacts were gained where poverty was low while significant household gains occurred where risk of deforestation was low. To the extent that this relationship holds globally, REDD programs generally must confront this tradeoff. In Mexico's case, one opportunity for win-win targeting was created by the fact that common property forest holdings can generate significant avoided deforestation and have generally poorer households. Also,

[32] These ratios may also be overestimates if households value their labor at less than the minimum wage. For private households these labor costs tend to be paid labor extracted directly from payments, whereas for common properties they often represent voluntary service within the forest.

the program did appear to boost consumption more for poorer households, suggesting a progressive distribution within those who received payments. Careful analysis of risk of deforestation and ownership distributions in other countries may allow REDD designers to find similar opportunities, but they are likely to be limited.

Our analysis draws attention to several interesting avenues for future research. These include important questions about post-program behavior by landowners, within-community political economy, and whether there are significant program spillovers on deforestation or household behavior. Previous work (Alix-Garcia et al. 2012) finds preliminary evidence for deforestation leakage in an early cohort of the program; similar leakage in later years would reduce the environmental effectiveness of the program. On the other hand, increased knowledge of the program throughout Mexico may have induced landowners to maintain forest in order to keep the option of enrolling in the future. Finally, our analysis of changes in forest conservation activities suggests that the costs of participating in PES programs may be large compared to opportunity costs. Most analysis of PES and estimates of the costs of REDD focus on opportunity costs of forgone production, yet our household data suggests these may be smaller than the costs of forest maintenance in the Mexican case. Additional work is needed to better understand short and long run costs of participation as well as possible gains to communities from investments in forest resources or increased human capital.

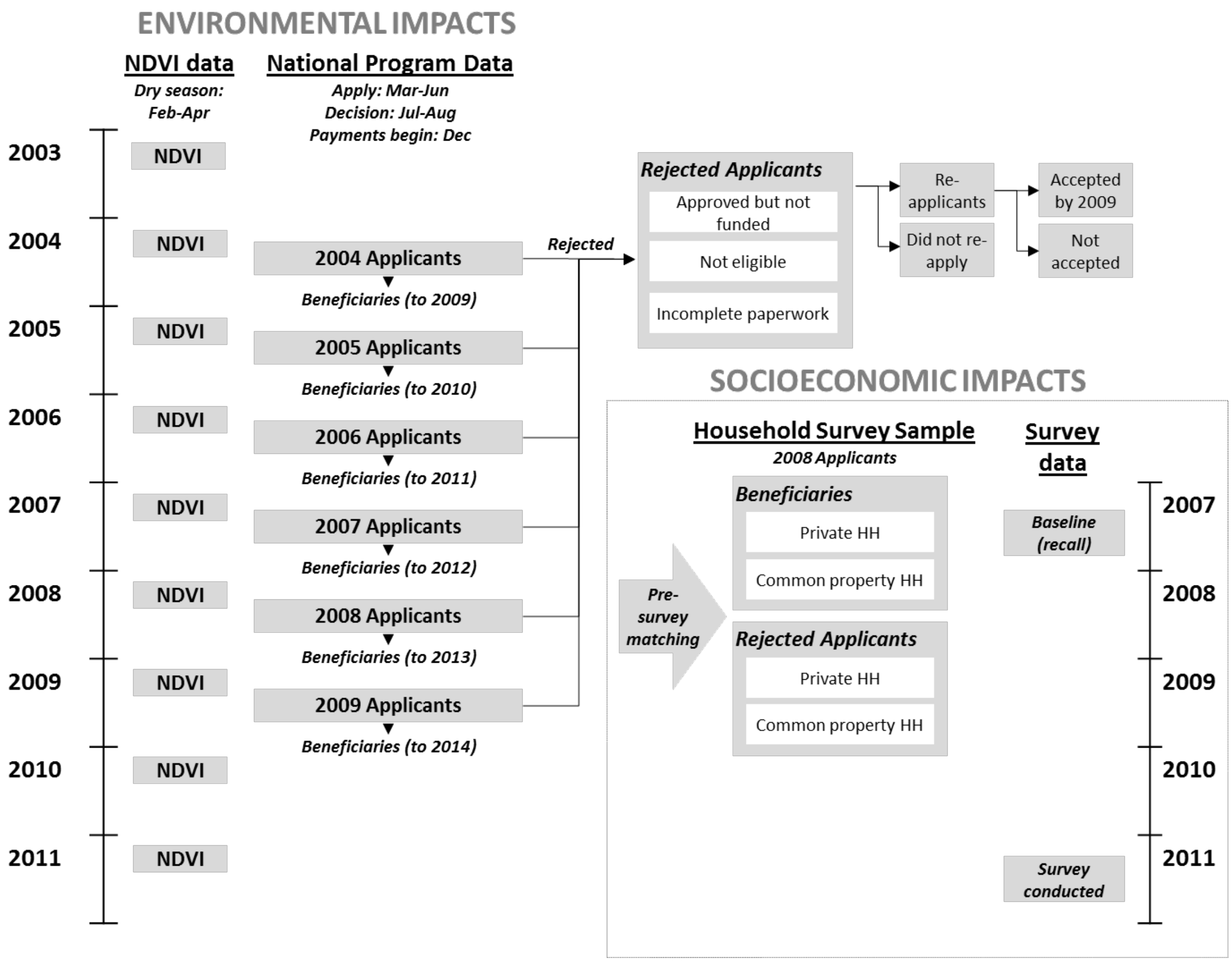

Figure 2.1: Schematic of data collection

Table 2.1: Payment rates and program applications 2004-2009 PSAH cohorts

	2004	2005	2006	2007	2008	2009
Payment rates						
Rate per hectare cloud forest (Mexican pesos)	400	400	413.70	429.85	447.02	465.80
Rate per hectare for other forest types (Mexican pesos)	300	300	316.35	328.71	341.84	356.20
Rate per hectare of oak forest (Mexican pesos)					394.43	411.00
Number and area of applicants						
Number beneficiary parcels	352	257	241	816	727	410
Area enrolled parcels (hectares)	178676	338045	127016	545577	324155	320196
Number rejected parcels	209	226	380	889	2032	925
Area rejected parcels (hectares)	256154	212402	492151	878132	985468	634333
Percentage of parcels that apply for re-enrollment after 5 years (based on point sample)	32.7	37.7	NA	NA	NA	NA
Percentage of parcels that successfully renew after 5 years	20.0	20.0	NA	NA	NA	NA

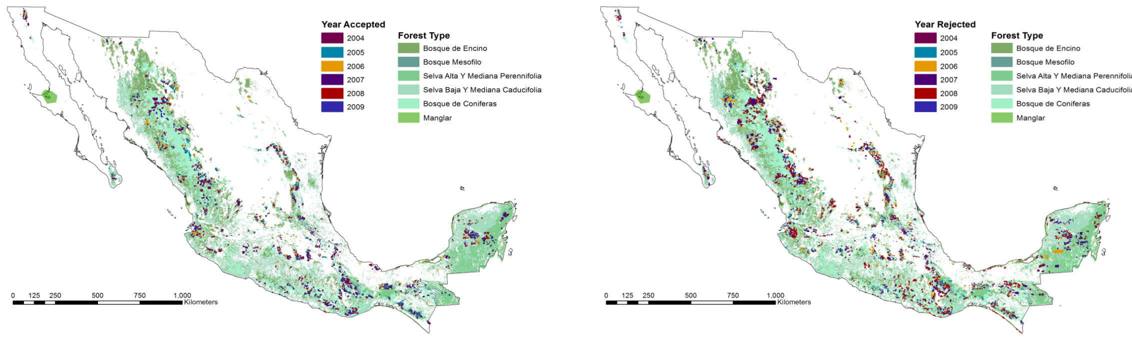

Figure 2.2: Beneficiaries (left) and Rejected Applicants (right) to PSAH, 2004-2009

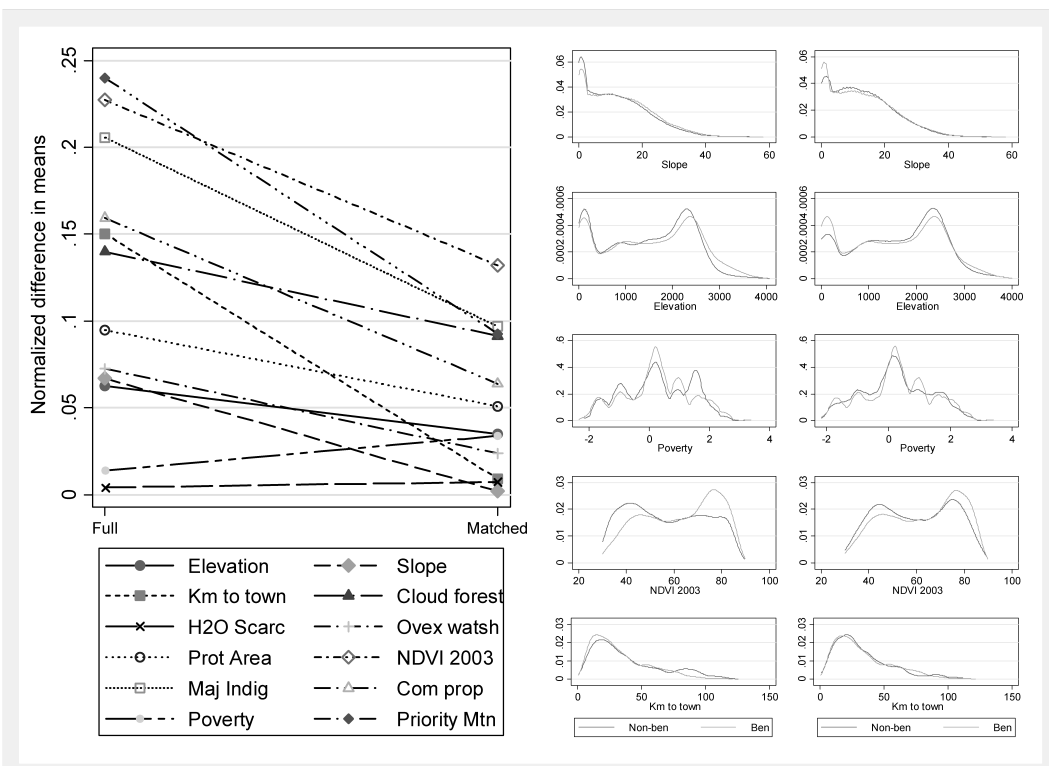

Figure 2.3: Changes in normalized differences and distributions after matching

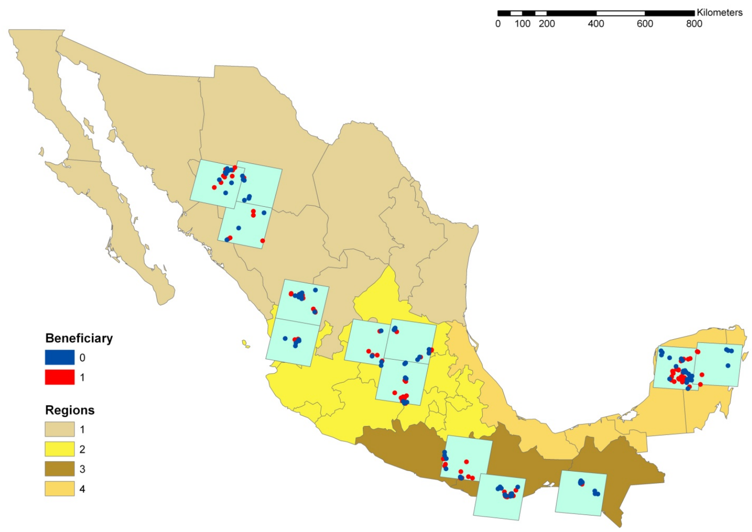

Figure 2.4: Survey sample and survey regions

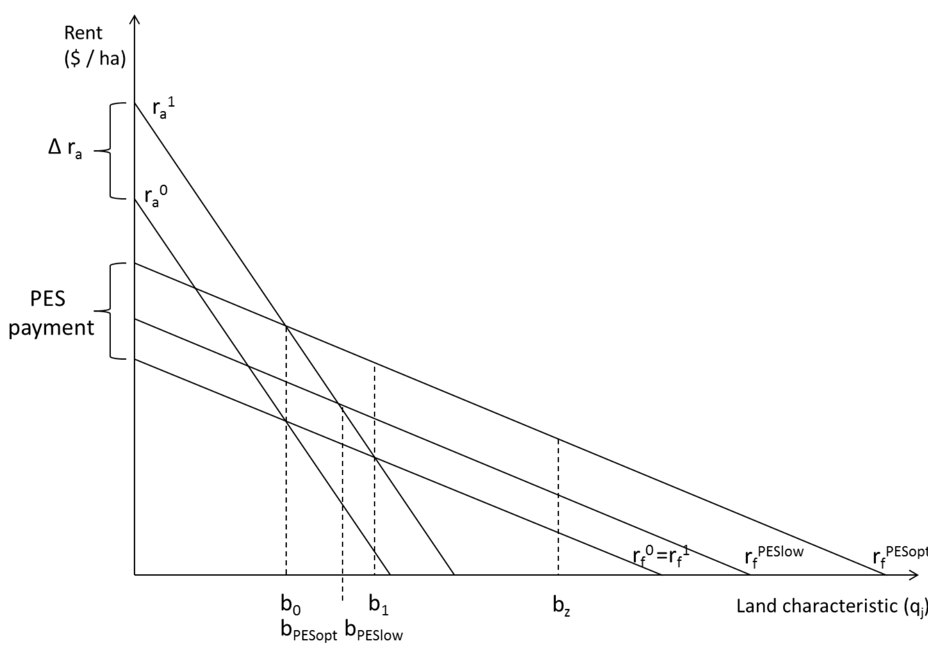

Figure 2.5: Economic framework: rent model of PES

Table 2.2: Summary statistics: points within applicant boundaries and other forested points

	All forested areas (1)	Benef (2)	All rejected applicants (3)	Matched rejected applicants (4)	Norm. diff. (1)v(2)	Norm. diff. (2)v(3)	Norm. diff. (2)v(4)
Slope (deg)	10.27	12.24	11.32	12.27	-0.14	0.07	0.00
	(9.52)	(9.93)	(9.64)	(9.55)			
Elevation (m)	1160.50	1520.47	1436.00	1567.50	-0.27	0.06	-0.03
	(866.68)	(981.13)	(921.10)	(930.00)			
Km to nearest town	38.11	33.52	38.76	33.24	0.13	-0.15	0.01
	(27.23)	(22.11)	(27.00)	(22.06)			
Municipal poverty	0.239	0.287	0.27	0.23	-0.03	0.01	0.03
	(1.02)	(1.12)	(1.13)	(1.09)			
Common property (0/1)	0.604	0.880	0.80	0.85	-0.47	0.16	0.06
	(0.49)	(0.33)	(0.40)	(0.36)			
Overexploited aquifer (0/1)	0.074	0.158	0.12	0.15	-0.19	0.07	0.02
	(0.26)	(0.37)	(0.33)	(0.35)			
Water scarcity (0-8)	7.18	6.849	6.86	6.83	0.16	0.00	0.01
	(1.31)	(1.67)	(1.53)	(1.61)			
Priority mountain (0/1)	0.068	0.245	0.12	0.19	-0.36	0.24	0.09
	(0.25)	(0.430)	(0.32)	(0.39)			
Protected area (0/1)	0.071	0.123	0.08	0.10	-0.13	0.09	0.05
	(0.26)	(0.33)	(0.28)	(0.30)			
Majority indigenous (0/1)	0.25	0.39	0.25	0.32	-0.21	0.21	0.10
	(0.43)	(0.49)	(0.44)	(0.47)			
Cloud forest (0/1)	0.03	0.09	0.04	0.06	-0.18	0.14	0.09
	(0.17)	(0.29)	(0.20)	(0.23)			
NDVI in 2003 (0-100)	55.45	62.30	57.18	59.39	-0.30	0.23	0.13
	(16.39)	(15.63)	(16.24)	(15.56)			
INE deforestation risk index	2.85	2.48	2.40	2.47	0.19	0.04	0.00
	(1.39)	(1.34)	(1.30)	(1.29)			
Geographic risk index	-1.08	-0.95	-1.12	-1.05	-0.09	0.12	0.04
	(1.07)	(1.00)	(1.10)	(1.09)			
Observations	44,104	17,307	18,456	4,489			

Matches are found using 1:1 covariate matching with replacement on the Mahalanobis metric. Exact matches are required within region, tenure type, and application year. Other matched covariates are slope, elevation, municipal poverty, distance to nearest locality with population greater than 5000, cloud forest, overexploited aquifer, degree of water scarcity, priority mountain, protected natural area, and municipality with majority indigenous population. Normalized difference is the difference in average covariate values, normalized by the standard deviation (Imbens and Wooldridge 2009).

Table 2.3: Sample size of survey and distribution by property type and region

Regions	Common properties			Private properties		
	Benef	Non-Benef	Total	Benef	Non-Benef	Total
1. North	137	136	273	15	14	29
2. Center	160	129	289	15	15	30
3. Southwest	149	128	277	16	15	31
4. Southeast	144	113	257	15	13	28
Total households	590	506	1096	61	57	118
Total properties	61	55	116	61	57	118

Regions are shown in Figure

Table 2.4: Summary statistics: beneficiary and non-beneficiary households
Common properties

Variables	Full sample			Matched sample		
	Benef	Non-Benef	Norm. Diff.	Benef	Non-Benef	Norm. Diff.
Food index 2011 (100s pesos)	2.122	2.040	0.066	2.134	2.014	0.096
Durables index 2007 (10000s pesos)	1.849	1.631	0.067	1.838	1.430	0.130
Housing index 2007 (10000s pesos)	9.958	10.057	-0.016	9.939	9.845	0.015
# cattle 2007	2.986	4.565	-0.087	2.819	4.810	-0.102
# small animals 2007	8.341	6.437	0.060	8.453	6.655	0.054
Livestock infrastructure 2007 (0/1)	0.125	0.113	0.028	0.122	0.107	0.034
Agricultural inputs 2007 (0/1)	0.695	0.626	0.102	0.697	0.663	0.051
Agricultural equipment 2007 (0/1)	0.178	0.198	-0.036	0.175	0.230	-0.096
Children attend school 2007 (0/1)	0.706	0.722	-0.024	0.709	0.747	-0.060
Elevation (m)	1602.26	1471.43	0.089	1593.35	1590.29	0.002
Slope (deg)	9.309	9.908	-0.058	9.501	10.454	-0.090
Distance locality>=5000 people (km)	32.240	30.194	0.079	32.379	30.537	0.069
Municipal poverty 2005	0.724	0.776	-0.037	0.743	0.770	-0.018
Area of parcel enrolled (ha)	1020.86	1241.28	-0.165	1039.82	1253.16	-0.161
Member of community	0.654	0.656	-0.003	0.642	0.636	0.009
Household size	4.897	4.597	0.091	4.918	4.607	0.095
Days worked FCA 2007	17.551	7.493	0.203	8.890	6.997	0.076
Participated in FCA 2007	0.581	0.409	0.247	0.549	0.505	0.062

The full sample considers all those observation that have complete information in all covariates used in the analysis. FCA are forest conservation activities. The matched sample uses 1:1 covariate matching with replacement on the Mahalanobis metric. Households are matched exactly by region based on their baseline cooperation levels in FCA. In addition, we exclude observations that have missing information in any of the covariates used in the analysis. The food index is constructed using households reported prices and considering the consumption of tortillas, milk, beef, pork, cheese, bread, tomato, and beans. Durables and housing indices are aggregates of assets (television, refrigerator, computer, stove, car, phone, and cellphone) and housing characteristics (floor, walls, number of rooms) valued at 2007 prices. Livestock infrastructure, agricultural inputs, and equipment in 2007 are binary variables indicating if the household had such expenditures in that year. Children enrolled in school consider those that are between 12 and 22 years old in 2011. The municipal poverty measure is the 2005 marginality index constructed by CONAPO, which considers multiple dimensions: education, access to basic services, employment, and population. The range of this poverty index goes from -1.28 to 3.25 for the full sample. Normalized difference is the difference in average covariate values, normalized by the standard deviation (Imbens and Wooldridge 2009).

Table 2.5: Summary statistics: beneficiary and non-beneficiary households
Private properties

Variables	Full sample		
	Benef	Non-Benef	Norm. Diff.
Food index 2011 (100s pesos)	2.479	2.532	-0.048
Durables index 2007 (10000s pesos)	4.738	4.275	0.126
Housing index 2007 (10000s pesos)	16.790	14.911	0.142
# cattle 2007	19.583	23.241	-0.053
# small animals 2007	31.417	12.000	0.096
Livestock infrastructure 2007 (0/1)	0.183	0.222	-0.068
Agricultural inputs 2007 (0/1)	0.317	0.463	-0.213
Agricultural equipment 2007 (0/1)	0.167	0.167	0.000
Children attend school 2007 (0/1)	0.911	0.841	0.149
Elevation (m)	1288.99	1271.38	0.014
Slope (deg)	9.110	9.721	-0.062
Distance locality>=5000 people (km)	26.75	29.23	-0.102
Municipal poverty 2005	0.917	0.659	0.175
Area of parcel enrolled (ha)	103.79	107.98	-0.037
Household size	4.333	3.926	0.139
Observations	60	54	

The full sample considers all those observation that have complete information in all covariates used in the analysis. The explanation of covariates presented in the table can be found in note of the previous table. Normalized difference is the difference in average covariate values, normalized by the standard deviation (Imbens and Wooldridge 2009).

Table 2.6: Impacts of PSAH on forest cover: main effects

	Dependent variable = mean dry season NDVI					
	Change in levels	Change in trend	Change in levels, years in program	Change in levels	Change in trend	Change in levels, years in program
Beneficiary	0.1863***		0.1396	0.2455***		0.1745**
	(0.0721)		(0.0880)	(0.0737)		(0.0874)
Beneficiary * time		0.0265**			0.0352***	
		(0.0127)			(0.0125)	
Beneficiary* years in program			0.0223			0.0344
			(0.0341)			(0.0347)
State-year FE	Y	Y	Y	Y	Y	Y
Rainfall controls	Y	Y	Y	Y	Y	Y
Point FE	Y	Y	Y			
Parcel FE				Y	Y	Y
N total	196164	196164	196164	174368	174368	174368
N points	21796	21796	21796	21796	21796	21796
N parcels	3495	3495	3495	3495	3495	3495
R2	0.49	0.49	0.49	0.68	0.68	0.68
Effect size as percent of 5 year counterfactual trend	40-51%	28-36%	54-69%	39-66%	28-47%	55-93%

* p< .10 ** p < .05 *** p < .01 Point or parcel-level fixed effects model (equation 1). Robust standard errors clustered at the parcel level in parentheses. Dependent variable is mean dry season NDVI (ranges from 0 to 100). Regressions use data from the 21796 points within program beneficiaries (N=17,307) and matched rejected applicants (N=4,489) (Table 2 columns 2 and 4). Regressions 1-3 use NDVI outcomes from 2003-2011. Regressions 3-6 use NDVI outcomes from 2004-2011 and include NDVI 2003 and other point-level covariates shown in Table 2 as controls.

Table 2.7: Parallel trend test and robustness checks

	Mahalanobis matched subsample								Fulld sample	
	Parallel trend test	Parallel trend test	Only treated parcels	Controls: approved but unfunded	Controls: elegible zones, not qualified	Controls: in elegible zones	Controls: high NDVI only	Controls: rejected and never reapplied	Baseline specification	
Future beneficiary *time	-0.0246	-0.0147								
	(0.0396)	(0.0664)								
Beneficiary			0.2466***	0.2253***	0.2104***	0.2161***	0.2010**	0.1961***	0.0629	0.1602**
			(0.0831)	(0.0763)	(0.0763)	(0.0726)	(0.0835)	(0.0745)	(0.0787)	(0.0799)
State-year FE	Y	Y	Y	Y	Y	Y	Y	Y		
Rainfall controls	Y	Y	Y	Y	Y	Y	Y	Y		
Point FE	Y		Y	Y	Y	Y	Y	Y	Y	
Parcel FE		Y								Y
N	84662	62866	155763	173178	174402	188073	134802	184653	321867	286104
R2	0.45	0.68	0.49	0.49	0.49	0.49	0.45	0.49	0.48	0.67

* p< .10 ** p < .05 *** p < .01 Robust standard errors clustered at the parcel level in parentheses. . Dependent variable is mean dry season NDVI ranges from 0 to 100). Columns (1)-(8) use subsets of the matched data and columns (9) and (10) use the full sample of points. Columns (1) and (2) use only observations for years prior to enrollment or application to the program. Column (3) uses only controls and column (4) only treated parcels. Columns (5)-(8) include all treated parcels and varying subsets of controls.

Table 2.8: Longer term program impacts on land cover

	Dependent variable = mean dry season NDVI				
	(1)	(2)	(3)	(4)	(5)
Permanent beneficiary dummy	0.1775**	0.1522*	0.1520*	0.1521*	
	(0.0759)	(0.0837)	(0.0837)	(0.0837)	
Permanent beneficiary dummy		0.1042	0.1703	0.1698	
* 2004 or 2005 cohort		(0.1555)	(0.1647)	(0.1647)	
Permanent beneficiary dummy			-0.1836	-0.3093	
*2004 or 2005 cohort			(0.1957)	(0.2486)	
*Reapply in 2009 or 2010					
Permanent beneficiary dummy				0.2287	
*2004 or 2005 cohort*Successfully				(0.3802)	
renew in 2009 or 2010 After application * time					-0.0781**
					(0.0338)

* p< .10 ** p < .05 *** p < .01 Point-level fixed effects model (equation 1). Robust standard errors clustered at the parcel level in parentheses, unless otherwise stated. Dependent variable is mean dry season NDVI (ranges from 0 to 100). Regressions use data from program beneficiaries and matched rejected applicants; matching as described in footnote of Table 2. Columns (1)-(4) use the full matched sample, whereas column (5) uses only the matched controls.

Table 2.9: Average impacts of PSAH on consumption: food, durables, housing

	Food index	Durables index	Housing index
a. Common properties			
Simple treatment			
Beneficiary	0.028	0.032	0.000
	(0.019)	(0.025)	(0.003)
Treatment by tenure class			
Beneficiary	0.015	0.016	0.001
	(0.024)	(0.026)	(0.004)
Beneficiary x member	0.020	0.024	-0.001
	(0.028)	(0.028)	(0.003)
Marginal effect (for members)	0.035	0.040	0.000
Base mean	2.014	1.673	9.901
Base standard deviation	0.870	2.263	4.417
Minimum detectable effect	0.053	0.070	0.008
N	922	1844	1844
a. Private properties			
Beneficiary	-0.009	0.022	0.001
	(0.027)	(0.043)	(0.001)
Base mean	2.532	4.519	15.900
Base standard deviation	0.846	2.601	9.544
Minimum detectable effect	-0.076	0.120	0.003
N	114	228	228

* p< .10 ** p < .05 *** p < .01 Dependent variables are measured in 2007 pesos and are transformed using the inverse hyperbolic sine. Durables and housing index estimates based on household fixed-effects model (equation 2). The food index column reports cross sectional regressions with 2011 data. The food index regressions also include: ln(distance to nearest city), household size, municipal poverty in 2005, if the household has a member with full land rights, and the mean elevation of the parcel. The food index is constructed using households reported prices and considering the consumption of tortillas, milk, beef, pork, cheese, bread, tomato, and beans. Durables and housing index regressions are aggregates of assets (television, refrigerator, computer, stove, car, phone, cell phone) and housing improvements (floor, walls, number of rooms) valued at 2007 prices. Standard errors are clustered at the property level for common properties and are heteroskedastic robust for private properties. Base means and standard deviations are for the variables in levels. The minimum detectable effect considers a power level of 0.8 and significance level of 0.05.

Table 2.10: Average impacts of PSAH on productive and human capital investment

	Agricultural investment					Educational investment		
	# Cattle	# Small animals	Livestock infrast.	Agricultural inputs	Agricultural equipment	Students 12-14 yrs.	Students 15-17 yrs.	Students 18 - 22 yrs.
a. Common properties								
Beneficiary	0.054**	-0.026	0.034	-0.007	-0.016	0.033	0.103*	0.044
	(0.022)	(0.038)	(0.030)	(0.025)	(0.020)	(0.038)	(0.060)	(0.057)
Base mean	3.627	7.723	0.116	0.683	0.197	0.946	0.810	0.526
Base std dev	13.110	23.457	0.320	0.465	0.398	0.226	0.393	0.500
Minimum detectable effect	0.062	-0.106	0.084	-0.070	-0.056	0.106	0.168	0.160
N	1844	1844	1844	1844	1844	597	676	979
b. Private properties								Student 12-22 yrs.
Beneficiary	0.055	0.006	0.093	0.105	0.045			0.057
	(0.086)	(0.121)	(0.071)	(0.069)	(0.063)			(0.075)
Base mean	21.316	22.219	0.202	0.386	0.167			0.880
Base std dev	48.579	145.791	0.403	0.489	0.374			0.327
Minimum detectable effect	0.241	0.338	0.199	0.193	0.177			0.210
N	228	228	228	228	228			201

* $p< .10$ ** $p < .05$ *** $p < .01$ Dependent variables are in levels, with the exception of the number of cattle and small animals, which are transformed using the inverse hyperbolic sine. Estimates include household fixed effects and standard errors clustered at the property level for common properties, and are heteroskedastic robust for private properties. Livestock infrastructure, agricultural inputs and equipment are all binary variables that take the value of 1 when the household invested in this category and zero otherwise. Regressions reported for the binary variables are linear probability models. The base mean and standard deviation are from variables in levels. The minimum detectable effect considers a power level of 0.8 and significance level of 0.05. The age of children corresponds to 2011.

Table 2.11: Heterogeneous effects on NDVI across deforestation risk and poverty measures

	Dependent variable = mean dry season NDVI				
	(1)	(2)	(3)	(4)	(5)
Beneficiary	0.1863***	0.6300***	0.4771***	0.2418***	-0.2476
	(0.0721)	(0.2273)	(0.0957)	(0.0716)	(0.1565)
Beneficiary x ln(slope)		-0.1014			
		(0.0807)			
Beneficiary x km to large locality		-0.0076***			
		(0.0029)			
Beneficiary x deforestation risk			0.2907***		
			(0.0584)		
Beneficiary x municipal poverty				-0.2423***	
				(0.0456)	
Beneficiary x common property					0.4983***
					(0.1734)
State-year FE	Y	Y	Y	Y	Y
Rainfall controls	Y	Y	Y	Y	Y
Point FE	Y	Y	Y	Y	Y
N	196164	196164	196164	196164	196164
R2	0.48	0.48	0.48	0.48	0.48

* p< .10 ** p < .05 *** p < .01 Point-level fixed effects model (equation 1). Robust standard errors clustered at the parcel level in parentheses, unless otherwise stated. Dependent variable is mean dry season NDVI (ranges from 0 to 100). Regressions use data from program beneficiaries and matched rejected applicants; matching as described in footnote of Table 2.2.

Table 2.12: Heterogeneous effects on consumption by deforestation risk and baseline poverty

	Food index	Durables index	Housing index
a. Common properties			
High risk of deforestation			
Beneficiary	0.019	0.074**	0.004
	(0.030)	(0.037)	(0.004)
Beneficiary x high risk	0.020	-0.079**	-0.006
	(0.039)	(0.040)	(0.005)
High poverty			
Beneficiary	0.019	-0.031	-0.005*
	(0.019)	(0.025)	(0.003)
Beneficiary x poor	0.021	0.123***	0.011***
	(0.027)	(0.041)	(0.003)
N	922	1844	1844
a. Private properties			
High risk of deforestation			
Beneficiary	0.017	0.023	0.005*
	(0.041)	(0.046)	(0.002)
Beneficiary x high risk	-0.051	-0.001	-0.005*
	(0.054)	(0.052)	(0.002)
High poverty			
Beneficiary	0.005	-0.024	-0.001
	(0.030)	(0.040)	(0.001)
Beneficiary x poor	-0.038	0.096*	0.004**
	(0.055)	(0.057)	(0.002)
N	114	228	228

* p< .10 ** p < .05 *** p < .01 All indices are constructed as described in footnote to Table 2.9 and transformed using the inverse hyperbolic sine.

Table 2.13: Heterogeneous effects on productive and human capital investment

	Agricultural investment					Educational investment		
	# Cattle	# Small animals	Livestock infrast.	Agricultural inputs	Agricultural equipment	Students 12-14 yrs.	Students 15-17 yrs.	Students 18 - 22 yrs.
a. Common properties								
High risk of deforestation								
Beneficiary	0.052*	-0.034	0.034	-0.001	-0.049*	-0.004	0.090	0.079
	(0.028)	(0.048)	(0.037)	(0.033)	(0.027)	(0.047)	(0.072)	(0.066)
Beneficiary x	0.003	0.015	0.000	-0.012	0.061**	0.053	-0.021	-0.114*
high risk	(0.032)	(0.050)	(0.038)	(0.033)	(0.031)	(0.047)	(0.069)	(0.068)
High poverty								
Beneficiary	0.050*	-0.014	0.019	-0.040	-0.045	0.068*	0.131*	-0.003
	(0.026)	(0.046)	(0.034)	(0.028)	(0.028)	(0.039)	(0.068)	(0.061)
Beneficiary x	0.009	-0.024	0.028	0.066*	0.057*	-0.069	-0.058	0.115*
poor	(0.031)	(0.050)	(0.031)	(0.036)	(0.032)	(0.044)	(0.068)	(0.064)
N	1844	1844	1844	1844	1844	597	676	979
b. Private properties								Student 12-22 yrs.
High risk of deforestation								
Beneficiary	0.182	0.029	0.141	0.113*	0.041			0.043
	(0.131)	(0.109)	(0.087)	(0.059)	(0.068)			(0.091)
Beneficiary x	-0.189	-0.034	-0.072	-0.013	0.006			0.017
high risk	(0.139)	(0.080)	(0.094)	(0.085)	(0.075)			(0.082)
High poverty								
Beneficiary	0.012	-0.005	0.072	0.233***	0.007			0.098
	(0.117)	(0.147)	(0.083)	(0.087)	(0.079)			(0.079)
Beneficiary x	0.090	0.022	0.045	-0.268***	0.079			-0.077
poor	(0.125)	(0.102)	(0.096)	(0.097)	(0.083)			(0.078)
N	228	228	228	228	228			201

* p< .10 ** p < .05 *** p < .01 Dependent variables and regressions as described in footnote to Table 2.10; with additional interaction terms.

Chapter 3

Women politicians, public expenditure decisions, and development outcomes in Bolivia

3.1 Introduction

In the past decades, many countries have amended their political systems to set aside positions to groups that are considered to be disadvantaged (Duflo 2005). Among these groups, women have received special attention. The increase in the number of women involved in politics has catalyzed some active debate. Most people would agree about the importance of giving equal opportunities to both men and women in their access to power positions; however, there persists a discussion about whether increasing female representation actually affects policy determination and welfare outcomes. This paper evaluates the impact of increasing female representation in Bolivian municipal councils on public expenditure decisions. In addition, it evaluates whether changes in public policy choices translate into better/worst welfare outcomes.

Theoretically, there is no clear answer as to whether having more women in political positions will have an effect on policy choices. It is possible that electoral incentives en-

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courage elected candidates to act accordingly to the interests of voters rather than follow their own interests. In these cases, public policy choices should just follow the median voter equilibrium (Downs 1957). However, in settings with weak electoral incentives, it is possible that the politicians' preferences and identity affect policy decisions (Besley & Coate 1997). Several studies have shown that men and women have different preferences. At the household level, there is evidence of differences in the effects of income in the hands of men and women, particularly women seem to be more concerned about the health and nutrition of girls in the house (Duflo 2000). At the policy level, women seem to be more concerned about social policy issues, they seem to support more child-related expenditures, and favor redistribution (Clots-Figueras 2011).

Earlier studies, using cross-sectional comparisons, conclude that women politicians do impact policy design. Thomas (1990) shows that U.S. states with higher female representation introduce and pass more bills responding to issues related to women, children, and families. Besley & Case (2000) find that the fractions of women in state houses are highly correlated with a state's workers' compensation policies. Since female representation may just be a reflection of the political preferences of the electorate, which may also drive policy determination, more recent papers have been more concerned about causality. Chattopadhyay & Duflo (2004) exploit the random assignment of a women's reservation policy in India. They show that reserving one-third of the seats for women in village councils of West Bengal and Rajasthan leads to more investment in infrastructure that is related to women. Svaleryd (2009) exploits fixed effects models and uses changes in the number of seats devoted for women in Swedish municipalities and shows that the greater share of women in these local-level positions increases expenditure on childcare. Rehavi (2007) and Clots-Figueras (2011) take advantage of close elections results between men and women to show that having more women in state legislatures increases public welfare expenditure in the United States and India, respectively.[1]

This study advances the literature in three different ways. First, by exploring the case of Bolivia, it helps in our understanding of the impacts of political representation policies in low income countries, which are still characterized by strong differences in gender roles that could affect women's political involvement. So far, the literature in this topic coming from a developing country has been limited to the case of India (Chattopadhyay & Duflo 2004, Clots-Figueras 2011). Second, no connections have been made yet between changes in expenditure choices and actual welfare outcomes. This analysis helps to respond to broader questions about public policy effectiveness and the relationship between gender equality and development. Finally, as opposed to previous studies that have used quasi-experimental designs and evaluate situations where two parties or two candidates are competing in an electoral race (Rehavi 2007, Clots-Figueras 2011), this paper proposes an innovative regression discontinuity design (RDD) that is useful to evaluate the impacts of gender quotas in political settings with multiple parties and proportional systems of representation.

Municipal governments in Bolivia are interesting units of analysis since they enjoy a significant independence from the national government and are the sole providers of important services, such as health, education, and basic infrastructure, among others. In 1999, a gender quota law was passed in the country that established that a certain proportion of the list of candidates for municipal councils, submitted by each political party prior to elections, needed to be women. Furthermore, the law proposed a specific order for men and women in these lists. Between 1998 and 2005 the number of women holding a seat in municipal coun-

[1] There is also a literature on gender quotas emerging from the political science field. This literature has been mostly focused on understading the impacts of quotas on female representation and the institutional and political system characteristics that help to increase women's political representation (e.g. Caul (2001), Tripp & Kang (2008)). In addition, there is a literature in economics looking at the impacts on gender quotas in enterprises. Adams & Ferreira (2009) show that female directors have a significant impact of board inputs and firm outcomes. Overall, they show that gender-diverse boards increase monitoring, but that mandating gender quotas in boards could harm well-governed firms in which additional monitoring is counterproductive.

cils more than tripled, although there is still heterogeneity in female representation across municipal councils. Since 2005 Bolivian municipalities have also experienced an important increase in their revenues given the nationalization of the hydrocarbon sector, which resulted in the redistribution of hydrocarbon taxes to municipalities. The evidence seems to suggest that women politicians have prioritized social investments when using these additional resources.

To conduct the empirical analysis, I compiled a comprehensive panel data on municipal expenditure allocations, revenues, and development indicators at the municipal level from 2000 to 2010. I combine this data with electoral information about the list of candidates and the distribution of votes observed during the elections of 2004. The RDD exploits the fact that women have been, in most cases, located in the third position in the list of candidates, therefore when a political party shifts from winning 2 seats to gaining 3 seats in the council at least one woman will be participating in the council. Given small margins of difference in the distribution of votes, which can be considered random, the identification strategy comes from comparing changes in public policy choices and development outcomes over time between municipalities that are are just above relevant thresholds for a shift in the seat allocation and municipalities that are just below these thresholds. This notion of finding thresholds where a seat change happens is inspired by recent work from (Folke 2013), who looks at the impacts of party representation on public policy choices.

Results indicate that women politicians prioritize social expenditures. More specifically, I find that social per capita expenditure is four times higher than the baseline level of expenditure observed before elections in municipal councils where at least one woman holds a seat. When looking at the composition of social expenditure, I observe that women politicians prioritize education, health, and environmental protection expenditures and give less attention to investments in public infrastructure. I estimate both the average impacts of female representation during the period of 2005 to 2008[2] and also test for differential time trends. Results show that the impacts of female representation appear only some years after elections. There is weak empirical evidence of links between changes in public policy choices emerging from higher female representation and the evolution of health and education indicators over time. This could be a result of outcomes lagging expenditure changes or of limited information on the actual use of public funds and more specific indicators.

The paper is structured as follows. In the next section, I provide some country background information about the role on municipal councils, the system of proportional representation that characterizes seat assignment in councils, and the gender quota law. In section 3, I explain the multiple data sources and discuss some suggestive evidence. In section 4, I explain the identification strategy and, in section 5, I present results and robustness checks. In section 6, the main conclusions and policy implications are discussed.

3.2 Country background

Bolivia is one of the poorest countries in South America, and also one of the most ethnically diverse. In 2011, 45% of the population was living below the poverty line (UDAPE 2012). In 2009, 52% of the population identified themselves as part of one of the 36 indigenous groups recognized by the constitution (INE 2009). This section provides information about the most important features of the Bolivian political system, it discusses the role of municipalities and municipal councils, how local elections operate, and provides details of the gender quota system.

[2] As it will be explained in Section 3, public expenditure data only spans the period from 2000 to 2008

3.2.1 Political system and the role of municipal councils

Bolivia's political system operates at 3 different geographical levels: the central government, the regional government, with 9 regions, and the municipal government, with 327 municipalities. After a decentralization law was passed in the country in 1994, municipalities became very important actors in local development. They became responsible for managing, preserving, and renewing health, education, basic services, and roads infrastructure. They also support social assistance programs, various human development and cultural projects, and environmental programs. Most municipal income comes from the national government (aprox. 85%). More specifically, national tax revenues and oil taxes are redistributed to municipalities based on their population.[3] In addition, they receive funds from the national level coming from the Highly Indebted Poor Country (HIPC) initiative. Municipalities may also raise their own resources through the collection of local taxes, and they can get loans, transfers, and credits from international sources (Zambrana 2008).

There are three main actors at the municipal level: the mayor, which is the executive authority, the municipal council, which is the legislative body at the local level, and civil society organizations, which are in charge of social control[4]. Over the years, municipal councils have proven to be very important for policy determination. They are the ones in charge of revising and accepting all projects proposed by the mayor to be implemented at the local level. Moreover, they monitor the use of local resources and have the power to remove a mayor from office if needed. [5]

[3] For more details about national tax revenue redistribution, please refer Law 1551, Article 20. For more details about oil tax redistribution, please read Decreto Supremo No 28421, Article 2.

[4] These are better known as "Organizaciones Territoriales de Base" (OTB) and they are composed by different neighborhood associations. OTBs are represented in the municipal government by the "Comite de Vigilancia", which is in charge of monitoring how municipal resources are invested.

[5] For more details on the role of municipal councils, please read Law 2028, Article 12.

3.2.2 Municipal elections and local politics

Since 1999, municipal councillors are elected for a period of five years and they can be re-elected once (OEP-PNUD 2012). 90 days before the election day, parties are required to submit their list of candidates for municipal councillors. Once votes are cast, council seats are assigned to each party following a system of proportional representation based on the d'Hondt method. This means that the total votes cast for each party are divided, first by 1, then by 2, then 3, right up to the total number of seats to be allocated for the municipality. The resulting quotients are ranked and seats are allocated in a consecutive order to parties whose quotients are on the top of the rank. Table 3.2 presents a numeric example where party A wins 3 seats, party B wins 1 seat, and party C wins 1 seat. The total number of seats in a municipal council is proportional to the population. All municipalities with less than 50.000 inhabitants have 5 seats. For each additional 50.000 people, municipalities gain 2 extra seats in the council up to a total of 11 seats.[6]

List of candidates for municipal councillors are closed. This means that once the list is submitted it cannot be changed but only under some special circumstances, such as when the candidate dies, resigns, or is disqualified.[7] Lists are published once in local newspapers; however, during the election day the ballot only shows the names of political parties and not the names of candidates. Therefore, people can only vote for a political party but not for a specific candidate.

Multiple political parties participate in municipal elections. In 2004, citizen and indigenous groups were allowed to run in the elections. Until this year only traditional parties were allowed to participate. Besides the 16 traditional political parties that were present

[6] All municipalities that are capitals of a department or region have 11 seats regardless of their population.

[7] All changes need to be made prior to elections. Candidates can be disqualified if they don't satisfy certain conditions, such as: minimum age, citizenship, home address in the municipality, etc. For more details, please read Law 1984, Article 113.

in the 1999 elections, 347 citizen groups and 52 indigenous groups participated in the 2004 elections (OEP-PNUD 2012). On average, the number of political parties per municipality in the 2004 elections was 8, the maximum was 22 and the minimum was 1.

Once the municipal council has been elected, members need to meet regularly every week and their sessions are open to the public. Municipal councils make decisions based on a voting system, where every member has the same weight, and decisions are accepted with 2/3 of the votes.

3.2.3 Gender quotas

In 1999, Bolivia passed an electoral law that introduced a gender quota system.[8] More specifically, the law committed political parties to put in their list of candidates for municipal councillors at least 30% of women. The law also established certain ordering for the first three positions in the list, which required some alternating (not perfect) between men and women. According to the law, configurations such as: 1)Man 2) Man 3) Woman or 1)Woman 2) Woman 3) Man were acceptable. As it can be seen in Figure 3.1, most of the party lists in 1999 placed women in the third place (70%). The law also established that for every principal candidate in the list there should be a substitute. Besides the requested ordering for the first three positions in the list, the 30% quota was applied to either substitute or principal candidates, which resulted in most women being located in substitute positions (ACOBOL 2009).

In 2004, citizen and indigenous groups were allowed to run in the elections. For these incoming groups a 50% gender quota and perfect alternating order was required in their lists.[9] As Figure 3.1 shows, although there was an important increase in the number of lists that had women in the second position (32%), still the majority of parties in 2004 placed

[8]For more details, please read Law 1984, Article No. 112, Part 2
[9]More details can be found in Law 2771, Article No. 9.

women in the third position (57%). Overall, gender quotas seem to have been effective at increasing female representation in municipal councils over time. In 1999 the total number of women in municipal councils was 229, more than twice the number observed in 1995. In 2004 the number of women councillors increased to 343. Despite the absolute increase in the number of women in power positions, female representation is still low and was just below 20% of total councillors in 2004. As Figure 3.2 shows, there is also significant heterogeneity in female representation across space. This is a result not only of how parties construct their lists but, as it will be explained in the next section, of how votes are distributed and the number of seats parties gain after an election.

In this study, I focus on women's political participation as a results of the elections of 2004. This decision is driven mostly by the fact that public expenditure data is missing before 2000 for many of the municipalities in the sample. By focusing on 2004, I am able to evaluate both post and pre-election trends. Data from the elections of 1999 is used to conduct some robustness checks.

3.3 Data and suggestive evidence

This study combines electoral, administrative, demographic, and development data for a sample of 327 municipalities. Data has been collected from multiple sources and spans the period from 2000 to 2010. Data from candidates, elected officials, and the distribution of votes across parties, from elections in 1999 and 2004, comes from the National Electoral Court. Yearly administrative data about municipal public expenditures and revenues comes from the Ministry of Finance and the Unit of Economic and Social Policy Analysis (UDAPE). Expenditure data spans the period from 2000 to 2008 and the revenue data goes from 2000 to 2010. Population data comes from the 2001 census and projected population data, from 2002 to 2010, comes from the National Statistics Institute (INE). Annual development indicators, both for health and education, come from UDAPE and are available for the period of 2000 to 2010. Other municipal-level data about geographic characteristics and access to basic services comes from INE and the Federation of Municipal Associations of Bolivia (FAM).

Table 3.1 reports some baseline descriptive statistics. Municipalities differ widely, but most of them are small, rural, and poor. Columns (1) to (5) report statistics for the full sample of municipalities. The average population size is approximately 26000 people, but the range goes from 236 to more than 1 million. On average, 19% of the population in these municipalities lived in urban areas, and 64% lived below the extreme poverty line in 2001. The comparison of municipalities with and without women councillors based on the 2004 election results, shows significant differences. Municipalities with women are more urbanized, have significantly higher levels of education, lower levels of poverty, are located in less mountainous regions, and have less indigenous population, among the most important differences. It is also evident that the number of seats won by the party with the most votes is higher in municipalities with women versus those that do not. Columns (6) to (8) report statistics for the subsample of municipalities that have only 5 seats in the council, which accounts for 83% of all municipalities in the country. As it will be explained in the next section, I focus on this subsample of municipalities for the empirical analysis. This not only facilitates the identification strategy but, as it can be seen in Table 3.1, municipalities within these subsample seem to be also more homogeneous across multiple dimensions. For example, differences in the size of the population, urbanization rates, and the percentage of indigenous people between municipalities that have women councillors and those that do not are no longer significant in municipalities that have 5 seats.

Figure 3.4 shows the evolution of per capita public expenditure in municipalities with and without women, based on the results of the 2004 elections. The evidence shows that trends in per capita public expenditure prior to the 2004 elections was similar between municipalities with and without women councillors. There is also evidence that a few years after the elections municipalities with women councillors experience a higher increase in per capita social expenditure. There is no evidence of differential trends in non-social per capita expenditure after the election year.

There could be two possible confounding effects affecting the trends in Figure 3.4. First, it is possible that differential trends in population changes over time could be driving results. The panel on the left in Figure 3.5 helps to rule out this concern. Second, there could be differential trends in changes in municipal income. This second point is particularly important given that Bolivia went through an important hydrocarbons nationalization process that brought new sources of income to municipalities since 2005. As it can be seen in the panel on the right Figure 3.5, municipalities with more women seem to have gained more from hydrocarbon taxes. This piece of information is important and will be taken into account in the empirical analysis.

3.4 Identification strategy

Estimating the causal impacts of having women in municipal councils on public expenditure decisions raises an important identification challenge. To illustrate this, assume that the baseline empirical specification tested is:

$$y_i = \beta_0 + \beta_1 W_i + \beta_2 X_i + \epsilon_i \tag{3.1}$$

Where y_i is the social or non-social per capita public expenditure in municipality i, W_i is an indicator variable that takes the value of one if at least one seat in the council is held by a woman, X_i are some observable characteristics of municipality i that might affect public expenditure decisions. If I were to estimate equation (3.1) as it is, the coefficient β_1

would be biased as a result of unobservable municipality characteristics that might affect simultaneously female representation and government expenditure. For example, municipalities that have more women councillors may be more progressive and this might also directly affect the priority that is given to certain types of expenditures. The possible simultaneous effect of voter preferences on expenditure outcomes and female representation seems to be less problematic in this setting, given that people vote for a party and not for a specific candidate. Nonetheless, it could still be the case that voters prefer parties that have more women in their lists.

To solve this endogeneity issue, I exploit a regression discontinuity design (RDD), similar in motivation to the close-elections approach (Clots-Figueras 2011), but that is relevant for systems of proportional representation. More specifically, I exploit the fact that, given that the majority of parties have constructed their lists placing women in the third position, the probability of having at least one woman in the council increases when a party gains three seats in an election. The causal effects of women's representation are then estimated by comparing outcomes across municipalities where a party barely received or did not receive a third seat. The main identification assumption is that the third seat is randomly allocated when we are sufficiently close to thresholds where this third seat change happens. This RDD strategy is inspired in recent work by Folke (2013), who looks at the impacts of party representation on public policy choices. In contrast to Folke (2013), who focuses on all possible thresholds where a seat change could happen for a given party, here I focus on a very specific change - when the party with the highest number of votes jumps from having two to having three seats in the council.

To implement this empirical approach, I focus on municipalities that have a total of 5 seats in the council, which constitute 83 % of all municipalities in the country. I then follow three steps. First, I simulate all possible configurations for the distribution of votes across parties and identify situations (i.e. "critical ties" or "thresholds") where due to a very small margin of difference in votes the party with the most votes could jump from having two to having three seats. Second, using information on the number of votes, I calculate the distance to these thresholds and determine which municipalities are above or below them. Finally, I define what being close to a threshold means.

Given that I focus on municipalities with 5 seats total, only the first 5 parties with the most votes in a municipality will be relevant for the analysis [10]. Parties are indexed by $p = \{1, 2, 3, 4, 5\}$ and the number of votes for party p is denoted by v_p. I further assume that the number of votes for each party within a given municipality can be perfectly ranked and that $v_1 > v_2 > v_3 > v_4 > v_5$[11]. Given that the number of seats that a party can gain is affected by the distribution of votes among all parties, the identification of relevant thresholds requires considering all possible vote share configurations that could lead to the party with the highest number of votes ($p = 1$) to gain three seats in the council. It is important to mention that party 1 could gain three seats by winning not only the third position, but also by winning the fourth or the fifth positions.

Based on the d' Hondt method, I identify all possible "critical ties" that could emerge when allocating the third, fourth, and fifth position in the council, respectively (please refer to the Appendix for more details). As mentioned before, I focus on situations where the party with most votes could jump from having two to having three seats. For example, Table B1 in the Appendix shows that once the first two seats have been allocated to party 1, this party could be (closely) competing with party 2 for the third position. Table B2 shows that two critical ties could arise in the competition for the fourth seat. In option A, party 1 is competing with party 2 for the fourth seat. In option B, party 1 is competing

[10] On average, municipalities have 7 parties, and the range goes from 3 to 22

[11] When looking at the data, in none of the municipalities there was a tie between two or more parties in the number of votes

with party 3. A similar logic follows for the fifth position. Table 3.3 summarizes all possible critical ties for each position and shows the conditions that need to hold in every case.[12]

Having identified all multiple ties or thresholds, the distance to these thresholds is defined as:

$$\lambda_j = v_1/3 - Q_j \tag{3.2}$$

Where Q_j is the relevant quotient against which the third quotient of party 1 is being compared to. When λ_j is positive, this means that party 1 won the tie and obtained three seats. When λ_j is negative, party 1 lost the tie and did not get the three seats. Table 3.3 summarizes all possible values of λ_j. Given that party 1 could lose a tie for the third position but still have chances to win the fourth or fifth positions, the maximum value of λ_j will indicate whether the party finally won a third seat or not.

To determine how far or close from the threshold the first party is, I take into account the total number of votes received by the five parties with the most votes in the municipality:

$$Z_i = \frac{Max(\lambda_1, \lambda_2, ..., \lambda_7)}{\sum_{i=1}^{5} v_i} \tag{3.3}$$

Therefore, Z_i is the running variable of interest and summarizes the distance to any of the relevant thresholds. By construction, the cut-off point is zero. In municipalities with positive values of Z_i the party with the most votes gains at least three seats. In those with

[12]This notion of finding critical ties could be also represented graphically with a multi-party map. For more details on the construction of these maps please visit http://www.geometricvoting.org.uk/htablec1.htm. Figure B1 in the Appendix shows an example, extracted from Folke (2013), for the distribution of three seats between three parties as a function of their votes shares. The shaded areas inside the triangle are those that are slightly above and below a critical tie. As mentioned before, Folke (2013) is interested in all possible seat changes, in my case however the shaded area of interest will be only the one that is closest to the right vertex of the triangle (where party 1 jumps from having two seats to having three seats).

negative values no party holds more than 2 seats in the council. As it can be seen in the panel of the left of Figure 3.6, it is true that for positive values of Z_i the probability of having a party with three seats in the council jumps sharply from zero to one. Given the position of women in the list of candidates, it is also true that the probability of observing at least one woman in the council increases above the threshold (panel on the right).

An important assumption for this RDD design to hold is that political parties should not be able to manipulate the final vote share to be above the thresholds. As Lee (2008) argues in the context of close election results, although parties may manipulate the overall vote share prior to elections with their campaigns, any small difference on the day of the election cannot be manipulated and can be considered as good as random. The way the vote count is done in Bolivian municipal elections reduces the concerns of having electoral fraud. Vote count is done manually at every electoral table. Citizens are selected randomly to be electoral judges during the election day and help with the vote count. Furthermore, the vote counting process is open to the public, so in most cases and, particularly in situations where parties could be in a close race, multiple observers are present to make sure that votes are counted correctly (CNE 2004). One final concern could be that municipalities where parties win only 2 seats could have different list of candidates than municipalities with 3 seats. As it is shown in Figure 3.3 there are no differences in list construction between these two types of municipalities.

As opposed to a sharp RDD, where the probability of treatment jumps from zero to one in the cutoff, here we have a fuzzy RDD given that the probability of having women in the council does not jump perfectly but is increasing above the cut-off. The presence of cross-overs (observations below the threshold that are treated) is explained by the fact that some parties, particularly citizen and indigenous groups, have placed women in the second position in their lists. Given this fuzzy design, the estimation of treatment effects is done with a two-stage least square IV approach:

$$W_i = \beta_0 + \beta_1 T_i + \beta_2 X_i + f(Z_i) + \varepsilon_i \tag{3.4}$$

$$Y_i = \gamma_0 + \gamma_1 W_i + \gamma_2 X_i + g(Z_i) + \mu_i \tag{3.5}$$

Where W_i is a binary variable that takes the value of one when at least one woman holds a seat in municipality i's council. Y_i is the per capita social or non-social public expenditure. T_i, the treatment variable, is used as an instrumental variable in this setting and takes the value of one when the running variable Z_i is above the cut-off (i.e.is positive) and zero otherwise. X_i is a vector of municipality characteristics that may affect the presence of women in the council and also public expenditure decisions. $f(.)$ and $g(.)$ are polynomial functions of the running variable that help to reduce residual variation. To estimate the effects on government expenditures of female representation I use panel data from 260 municipalities during the period 2000-2008.

As Lee & Lemieux (2010) indicate, including fixed effects is unnecessary for identification in a RDD and introduces further restrictions in the context of panel data. As the authors suggest, I estimate the system of equations (3.4) and (3.5)with a pool sample. More specifically, given that I focus on the 2004 elections, I pool the sample for all years after the election (2005 to 2008) and cluster standard errors at the municipality level to take into account within-municipality correlation of the errors over time. To exploit the structure of the panel data, I introduce lagged values of the dependent variables as baseline covariates. Moreover, I introduce time specific dummies to capture differential effects over time.

3.5 Results

3.5.1 Social versus non-social expenditure

Table 3.4 presents the average impacts of having women in municipal councils on social and non-social per capita expenditure during the period of 2005 to 2008. Social expenditure considers categories such as: health, education, social protection, and basic infrastructure (water and sanitation), among the most important. Non-social expenditure considers categories such as: road infrastructure, productive investments, security, and services.

Columns (1) and (4) present OLS estimates and show that in municipalities with women councillors social per capita expenditure more than doubles during the period of 2005 to 2008 while there are no significant impacts on non-social per capita expenditure. Columns (2) and (5) report the instrumental variables estimations, where the treatment variable T, which takes the value of one when the running variable is above the cut-off and zero otherwise, is used as an instrument for whether the council has a woman or not. The IV results show no significant impacts of women councillors on social or non-social per capita expenditure. As it can be seen in columns (3) and (6), the variable T is a very good predictor of the presence of women in municipal councils.

Based on the evidence presented in Figure 3.4, it seems plausible that the effects of women councillors on public expenditure decisions could take some time to appear. Table 3.5 presents the OLS and IV estimations for models that introduce differential time effects. Results indicate that two years after the 2004 elections statistically significant differences in social per capita expenditure exist between municipalities with women councillors and those without them. Results are robust across both OLS and IV regressions and show sizeable impacts. As column (2) shows, social per capita expenditure in 2007 and 2008 is more than 4 times the baseline per capita expenditure in municipalities with female representation. Column (5) reports no significant differences in time trends for non-social per capita expenditure, although for 2005 municipalities with women seem to invest also more in this category.

3.5.2 Categories of social expenditure

Following UDAPE's classification, I estimate separate regressions for multiple categories of social expenditure, namely: education, health, social protection, environment, infrastructure, and recreation. The social protection category considers all expenditures devoted to protect vulnerable populations, such as elderly people, children, and women. Some examples, are payment for the functioning of shelters for orphans or shelters for women that are victims of violence. The environmental category considers expenditures devoted to waste treatment and garbage disposal, environmental education programs, protected areas and reforestation programs, among others. The infrastructure category considers investments in water and sanitation. Finally, the recreation category considers investments in activities that promote culture and protect heritage, such as museums, libraries, etc.

Tables 3.6 and 3.7 present the OLS and IV results looking at different categories of social expenditure. The evidence suggests that women councillors mostly prioritize investments in education, health, and environmental protection, as there are sustained increases over multiple years in these categories. For education, the per capita expenditure is approximately 4 and 5 times higher than the baseline expenditure for the years 2007 and 2008, respectively. For health, the per capita expenditure is 6 and 5 times higher than the baseline for 2007 and 2008, respectively. For environment, the magnitudes are 2.5 and 4 times the baseline expenditure for 2007 and 2008. There are also increases in some years in social protection and recreational expenditure, and no impacts are observed in infrastructure expenditure.

3.5.3 Robustness checks

Multiple robustness checks are conducted. First, I introduce polynomial functions of the running variable in the regression to take into account the possibility of a non-linear relationship between the outcome and the running variable. As reported in Table 3.8, results are robust to including quadratic and cubic polynomials of the running variable. The point estimates and their statistical significance remains almost unchanged when including these polynomials. The only exception is the recreation expenditure, which is no longer statistically significant. Second, I estimate regressions with the subsample of observations that is closest to the cut-off point. More specifically, I restrict the sample to observations where the difference in votes between the two parties competing in any critical tie is no more than 5% of the total number of votes received by the five parties with the most votes. As it is shown in Table 3.9, statistically significant increases in total social, education, health, and social protection per capita expenditures are still observed for the year 2008 using this subsample of observations, but point estimates are larger. For the environmental per capita expenditure, results are robust and still observed for both 2006 and 2007.

One concern about the identification strategy could be that, even when the margin of difference in votes is small, by comparing municipalities where the party with the most votes won three seats versus those where it only obtained two seats, the effect of female representation could be confounded with the effect of having a party that holds majority in the council. Given that the outcome of interest is the allocation of public expenditure and not an outcome related to efficiency, a priori there are no reasons to believe that majority in a council will translate into different patterns of expenditure allocation. As a robustness check, I regress public expenditure decisions before the 2004 elections and look at the impacts of a party having three seats versus only holding two. As reported in Table 3.10, majority in the council did not affect expenditure decisions between 2000 and 2003.

3.5.4 Impacts on outcomes

One key question that emerges from the differential trends in public expenditure decisions observed over time between municipalities with and without women councillors is whether these differences have translated into better/worst welfare outcomes. Based on the results discussed in the previous section and also based on the availability of data, I analyze the evolution of education and health indicators from 2005 to 2010. Although there is also robust evidence of impacts on environmental expenditure, unfortunately there is not data about outcomes in this dimension. Columns (1) to (3) of Table 3.11 report results for education indicators, namely the primary enrollment rate, primary completion rate, and gender differences in the primary completion rate[13]. Columns (4) and (5) of Table 3.11 report results for health indicators, more specifically I focus on the coverage of the pentavalent vaccine[14] and the percentage of women that delivered their babies in a formal medical institution[15].

Overall, there are very few significant impacts of female representation on outcomes over time. There are some increases in the primary enrollment rate in the year 2007 and some reductions in the gender gap of the primary completion rate in 2006, which indicates that girls' completion rates get closer to those of boys. Despite the increase observed in education and health per capita expenditures in municipalities with higher female representation, there could be two explanations for the almost null impacts of these policy changes on outcomes. First, it could be that these are long terms outcomes and therefore we don't expect to see a big change in them in the short run. Second, it could be the case that the higher per capita expenditure observed both in health and education categories is going towards very specific

[13]Gender gaps are measured as the difference between the male average completion rate minus female completion rate.

[14]The pentavalent vaccine is a combination of five vaccines in one: diphtheria, thetanus, whooping cough, hepatitis B, and Haemophilus influenza type b (the bacteria that causes meningitis, otitis, and pneumonia). This vaccine has been promoted in developing countries for children under the age of 1 in order to reduce infant mortality.

[15]Formal institutions include not only hospital and clinics, but also small medical centers in rural areas. Delivery in a formal institution helps to reduce maternal and child mortality.

initiatives that are not captured by these outcomes.

3.6 Conclusions

This paper shows that female politicians influence public expenditure decisions and prioritize social investments. More specifically, by exploring the case of municipal councils in Bolivia, which have recently experienced an important increase in their revenues as a result of the nationalization of the hydrocarbon sector, I find that the presence of women in municipal councils leads to a significant and sizeable increase in social per capita expenditure. The analysis of the composition of social expenditure indicates that women councillors prioritize health, education, and environmental programs, and give less attention to investments in infrastructure. When I test for differential time trends, I observe that the impacts of female representation do not appear immediately after elections, but take some time to be seen. Given that for most women this is their first time holding power positions, these results could indicate that training and familiarity with administrative procedures is an important step to influence public policy choices.

Other authors have shown that the gender of the politician affects public policy choices; however, evidence from a developing country setting has been restricted to the case of India. By focusing on the case of Bolivia, one of the poorest countries in South America, this paper contributes to the understanding of the impacts of political representation policies in low income countries. Most importantly, as opposed to previous studies that evaluate situations where two parties or two candidates are competing in an electoral race, this paper proposes an innovative regression discontinuity design that is useful to evaluate the impacts of quotas in political settings with multiple parties and proportional systems of representation. In addition, none of the previous studies exploring the impacts of women on public expenditure choices have evaluated the effectiveness of public policy. In this paper, I show that despite the impacts of female representation on education and health expenditures, there is still no evidence of positive impacts on related outcomes. This could be a result of outcomes lagging expenditure changes or limited information on the actual use of public funds and more specific indicators.

Given the increased divergence in social investments observed across Bolivian municipalities in the last years, which could exacerbate inequality in the long run, the results from this study shed some light about some of the factors that might explain these differences. Although women's participation in local politics in Bolivia has increased in the past years, female representation is still low. More policies and initiatives need to be in place to make sure that women have equal access to local councils across space. Possible avenues for future research include understanding under what conditions can women be more effective in their participation and involvement in municipal councils. For example, the use of geographical data, about soil quality and agricultural practices, as it historically relates to social norms about gender (Alesina et al. 2013) could help to understand whether there is heterogeneity in the impacts of women across space based on the role women have in society. It will be also interesting to analyze whether certain characteristics of the politician or their party of affiliation facilitate their involvement or create differential impacts. A preliminary analysis suggest that belonging to the ruling party does not have any differential impact on expenditure, but that women belonging to an indigenous municipality are more concerned about environmental protection programs.

Table 3.1: Baseline municipality characteristics

	Full sample			Full sample		Restricted sample (5 seats)		
	Mean	sd	No women	With women	Diff.	No women	With women	Diff.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Elevation	2436.930	1508.330	2717.603	2321.502	396.101*	2666.614	2337.549	329.066
Slope	39.530	32.881	48.248	35.944	12.304**	47.830	35.221	12.609**
Agricultural potential	0.301	0.459	0.263	0.316	-0.053	0.274	0.267	0.007
Total population 2001	26453.635	93811.212	12843.137	32051.026	-19207.889	10357.036	9216.364	1140.672
Land area	3255.590	6588.986	2905.900	3399.402	-493.502	3061.677	3402.091	-340.414
Years of education 2001	5.115	1.669	4.520	5.360	-0.840***	4.544	5.097	-0.553**
% Indigenous population 2001	70.444	30.611	76.892	67.792	9.099*	76.307	68.586	7.721
% Female population 2001	0.485	0.029	0.488	0.483	0.005	0.487	0.479	0.008*
Life expectancy 2001	60.626	4.925	59.717	61.000	-1.283*	59.757	60.649	-0.892
Extreme poverty 2001	64.038	20.827	69.966	61.600	8.366***	69.980	64.962	5.018*
Urbanization rate 2001	0.190	0.282	0.110	0.223	-0.113***	0.103	0.143	-0.041
Prop. women councillors 2004	0.184	0.152	0.000	0.260	-0.260***	0.000	0.267	-0.267***
Highest no. seats won 2004	2.709	1.274	1.853	3.061	-1.208***	1.798	2.670	-0.873***
Observations	326	326	95	231	326	84	176	260

Note: * p<0.10 ** p<0.05 *** p<0.01. Differences tested with t-test statistics.

Table 3.2: Seat assigment example using d'Hondt method

	Party A	Party B	Party C	Party D	Party E
# votes	45	23	12	14	6
# votes/2	22.5	11.5	6	7	3
# votes/3	15	7.7	4	4.7	2
# votes/4	11.25	5.75	3	3.5	1.5
# votes/5	5	4.6	2.4	2.8	1.2
Party A	Position 1	Position 3	Position 4		
Party B	Position 2				
Party C	Position 5				

Table 3.3: Critical ties to win 3 seats

Position contested	Citical tie	Conditions required	α
Position 3	$v_1/3 = v_2$	$v_1/2 > v_2$	$\alpha_1 = v_1/3 - v_2$
Position 4-A	$v_1/3 = v_2/2$	$v_1/3 < v_2, v_2/2 > v_3, v_1/3 > v_3$	$\alpha_2 = v_1/3 - v_2/2$
Position 4-B	$v_1/3 = v_3$	$v_2/2 < v_3, v_1/2 > v_3, v_1/3 < v_2$	$\alpha_3 = v_1/3 - v_3$
Position 5-A	$v_1/3 = v_3$	$v_2/2 > v_3, v_2/3 < v_3, v_1/3 < v_2/2$	$\alpha_4 = v_1/3 - v_3$
Position 5-B	$v_1/3 = v_4$	$v_2/2 < v_4, v_1/2 > v_4, v_1/3 < v_3$	$\alpha_5 = v_1/3 - v_4$
Position 5-C	$v_1/3 = v_2/2$	$v_2/2 < v_3, v_2/2 > v_4, v_1/3 < v_3, v_1/3 > v_4$	$\alpha_6 = v_1/3 - v_2/2$
Position 5-D	$v_1/3 = v_2/3$	$v_1/3 > v_3, v_2/3 > v_3$	$\alpha_7 = v_1/3 - v_2/3$

Table 3.4: Social and Non-social per capita expenditure 2005-2008

	Social expenditure			Non-social expenditure		
	OLS	IV	First-stage	OLS	IV	First-stage
	(1)	(2)	(3)	(4)	(5)	(6)
Women	99.12***	199.03		12.24	97.66	
	(34.63)	(144.79)		(22.27)	(60.99)	
Z	-114.90	-345.43	0.06	-143.03	-339.55**	0.06
	(275.53)	(409.26)	(0.42)	(128.11)	((144.99)	(0.42)
Above threshold (T)			0.50***			0.49***
			(0.07)			(0.07)
Baseline expenditure	2.19***	2.17***	0.00	1.01***	0.98***	0.00
	(0.43)	(0.42)	(0.00)	(0.14)	(0.14)	(0.00)
IDH revenue	0.58***	0.58***	0.00	0.45***	0.44***	0.00
	(0.07)	(0.07)	(0.00)	(0.07)	(0.07)	(0.00)
Copart revenue	1.26**	1.28***	-0.00	0.95***	0.97***	-0.00
	(0.51)	(0.52)	(0.00)	(0.30)	(0.29)	(0.00)
N	984	984	984	984	984	984
Baseline mean	84.56	84.56		110.78	110.78	
Baseline std. dev.	87.82	87.82		116.29	116. 29	
Cragg-Donald F			152.71			151.47
Kleibergen-Paap F			55.84			55.18

Note: * p<0.10 ** p<0.05 *** p<0.01. Standard errors clustered at the municipality level. Revenue and expenditure variables are per capita. T takes the value of 1 when the running variable Z is above the cut-off (i.e. is positive).

Table 3.5: Social and Non-social per capita expenditure 2005-2008 (Time effects)

	Social expenditure			Non-social expenditure		
	OLS	IV	First-stage	OLS	IV	First-stage
	(1)	(2)	(3)	(4)	(5)	(6)
Women	-7.31	4.26		2.18	129.78**	
	(23.82)	(107.80)		(16.50)	(56.11)	
Year 2006*Women	44.39	70.09		22.33	-32.52	
	(40.71)	(89.73)		(24.17)	(58.51)	
Year 2007*Women	217.76***	360.08**		52.73*	34.15	
	(56.20)	(142.52)		(30.46)	(67.96)	
Year 2008*Women	163.99***	366.24**		-26.74	-75.53	
	(53.64)	(160.26)		(42.27)	(99.75)	
Year 2006	-18.69	-35.71	-0.01	-25.40	11.43	-0.00
	(40.94)	(48.41)	(0.01)	(23.41)	(37.02)	(0.01)
Year 2007	-22.32	-119.88*	-0.01	-35.28	-24.30	-0.00
	(74.99)	(69.79)	(0.02)	(41.09)	(43.09)	(0.02)
Year 2008	-156.17	-294.69*	-0.02	-121.93*	-91.65	-0.00
	(139.68)	(156.38)	(0.04)	(71.72)	(73.79)	(0.04)
Above threshold (T)			0.50***			0.50***
			(0.07)			(0.07)
Year 2006*T			-0.00			-0.00
			(0.00)			(0.00)
Year 2007*T			-0.00			-0.00
			(0.00)			(0.00)
Year 2008*T			-0.01			-0.01
			(0.01)			(0.02)
Z	-115.14	-354.47	0.06	-151.40	-374.85**	0.06
	(275.95)	(410.38)	(0.43)	(128.03)	(153.53)	(0.42)
N	984	984	984	984	984	984
Baseline mean	84.56	84.56		110.78	110.78	
Baseline std. dev.	87.82	87.82		116.29	116. 29	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Cragg-Donald F			36.73			36.16
Kleibergen-Paap F			13.41			13.12

Note: * p<0.10 ** p<0.05 *** p<0.01. Standard errors clustered at the municipality level. Other controls include baseline per capita expenditure (social and non-social, respectively), and municipal per capita revenues. Expenditure variables are per capita. T takes the value of 1 when z is positive. z is the running variable.

Table 3.6: Categories of social per capita expenditure 2005-2008 (OLS regressions)

	Edu	Health	Soc.Prot.	Envi	Infra	Recre
	(1)	(2)	(3)	(4)	(5)	(6)
Women	3.03	-8.85	1.88	-2.85	15.28*	-5.00
	(9.22)	(6.54)	(4.37)	(2.58)	(9.14)	(6.51)
Year 2006*Women	12.83	13.00	9.22	6.59**	-11.08	14.48
	(17.24)	(11.76)	(9.79)	(2.78)	(16.73)	(9.40)
Year 2007*Women	78.11***	40.16**	23.00	6.78*	21.87*	48.97***
	(27.95)	(17.14)	(15.18)	(3.47)	(12.62)	(12.57)
Year 2008*Women	55.12**	23.80	23.26*	4.32	0.22	58.88***
	(24.45)	(16.86)	(11.82)	(3.77)	(13.61)	(17.53)
Year 2006	-23.02	4.43	-10.84*	-6.70***	9.90	-1.29
	(20.46)	(6.93)	(5.64)	(2.21)	(15.34)	(5.16)
Year 2007	-29.97	19.22*	-13.99	-4.95**	-25.40	14.11
	(39.77)	(9.97)	(10.64)	(2.16)	(15.53)	(9.12)
Year 2008	-126.54*	14.52	-35.23*	-2.32	-49.34*	7.97
	(71.66)	(14.85)	(19.23)	(3.16)	(27.19)	(16.02)
N	984	984	984	984	984	984
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: * p<0.10 ** p<0.05 *** p<0.01. Standard errors clustered at the municipality level. Other controls include Z (running variable), the baseline per capita expenditure for each category, and municipal per capita revenues. Expenditure variables are per capita.

Table 3.7: Categories of social per capita expenditure 2005-2008 (IV regressions)

	Edu	Health	Soc.Prot.	Envi	Infra	Recre
	(1)	(2)	(3)	(4)	(5)	(6)
Women	4.22	33.60	24.76	-7.91	37.01	-33.74
	(38.09)	(31.48)	(20.77)	(5.05)	(28.99)	(29.60)
Year 2006*Women	-0.13	49.50	24.62	12.12**	-5.54	-8.99
	(34.40)	(36.11)	(33.05)	(5.41)	(30.57)	(19.68)
Year 2007*Women	123.21*	102.44*	42.81	20.07**	16.55	57.70*
	(64.25)	(52.35)	(45.39)	(9.29)	(29.06)	(35.05)
Year 2008*Women	146.69*	92.44*	84.29*	1.36	-13.24	59.34
	(79.94)	(51.46)	(43.85)	(6.92)	(30.39)	(53.39)
Year 2006	-13.96	-20.05	-21.03	-10.46**	6.23	14.85
	(19.82)	(19.54)	(14.81)	(4.21)	(21.44)	(15.18)
Year 2007	-60.72*	-23.60	-27.62	-13.98**	-21.77	8.95
	(34.83)	(28.11)	(21.03)	(5.91)	(23.10)	(20.37)
Year 2008	-188.74**	-32.73	-76.77**	-0.33	-40.10	9.00
	(81.83)	(30.08)	(32.52)	(5.67)	(25.30)	(38.17)
N	984	984	984	984	984	984
Baseline mean	31.15	18.26	3.82	4.88	14.84	10.45
Baseline std. dev.	35.10	28.01	5.90	13.01	17.34	10.35
Cragg-Donald F	38.32	38.01	38.29	34.91	38.12	38.20
Kleibergen-Paap F	14.15	13.84	13.87	12.65	13.87	14.05
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: * p<0.10 ** p<0.05 *** p<0.01. Standard errors clustered at the municipality level. Other controls include Z (running variable), the baseline per capita expenditure for each category, and municipal per capita revenues. Expenditure variables are per capita.

Table 3.8: Categories of social per capita expenditure 2005-2008
(IV regressions with polynomials of Z)

	Social (1)	Non-social (2)	Edu (3)	Health (4)	Soc.Prot. (5)	Envi (6)	Infra (7)	Recre (8)
Women	3.76	149.27**	23.63	56.46	42.39	-7.32	43.90	-84.81*
	(161.89)	(71.81)	(57.78)	(50.84)	(38.89)	(7.11)	(38.30)	(49.35)
Year 2006*Women	70.04	-32.46	-0.11	49.57	24.67	12.12**	-5.52	-9.17
	(40.75)	(45.96)	(34.44)	(36.15)	(33.10)	(5.41)	(30.57)	(19.69)
Year 2007*Women	359.95**	34.21	123.17*	102.50*	42.85	20.07**	16.57	57.50
	(142.56)	(67.99)	(64.27)	(52.40)	(45.45)	(9.28)	(29.05)	(35.00)
Year 2008*Women	366.04**	-75.38	146.68*	92.60*	84.39*	1.36	-13.20	58.89
	(160.30)	(99.71)	(79.89)	(51.54)	(43.82)	(6.92)	(30.39)	(53.44)
Year 2006	-35.14	11.48	-13.50	-19.90	-20.88	-10.44**	6.26	14.91
	(48.53)	(37.05)	(19.85)	(19.49)	(14.76)	(4.20)	(21.49)	(14.86)
Year 2007	-118.68*	-24.27	-59.80*	-23.34	-27.35	-13.94**	-21.72	9.22
	(69.77)	(42.88)	(34.67)	(28.11)	(21.14)	(5.91)	(23.02)	(20.41)
Year 2008	-292.44*	-91.57	-187.02**	-32.24	-76.27**	-0.25	-40.00	9.48
	(155.61)	(73.24)	(80.56)	(29.69)	(32.08)	(5.66)	(25.08)	(39.58)
Z	-486.70	-531.12	-326.23	-330.64	-291.78	-12.22	-166.27	535.99*
	(965.44)	(351.00)	(359.16)	(278.26)	(249.37)	(35.01)	(165.98)	(273.74)
Z^2	-3127.40	-722.02	-1967.15	-1285.44	-1019.01	-167.07	-90.93	1557.00
	(3503.96)	(1338.86)	(1393.16)	(996.24)	(824.77)	(123.54)	(630.20)	(998.83)
Z^3	7044.88	4409.19	7353.73	6178.06	4845.50	452.86	1201.09	-10786.06*
	(19618.26)	(7134.38)	(7094.25)	(5244.77)	(4730.94)	(670.70)	(3228.51)	(5538.30)
N	984	984	984	984	984	984	984	984
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: * p<0.10 ** p<0.05 *** p<0.01. Standard errors clustered at the municipality level. Other controls include baseline per capita expenditure for each category, respectively, and municipal per capita revenues. Expenditure variables are per capita.

Table 3.9: Categories of social per capita expenditure 2005-2008
(IV regressions sample close to threshold +/-0.05)

	Social	Non-Social	Edu	Health	Soc.Prot.	Envi	Infra	Recre
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Women	170.51	260.63**	55.47	41.12	46.56	-17.27	118.04*	41.14
	(178.97)	(119.02)	(76.24)	(63.14)	(50.73)	(11.86)	(62.72)	(39.10)
Year 2006*Women	12.26	-11.51	-49.62	90.50	39.65	24.13**	-64.68	-24.41
	(190.52)	(108.64)	(68.67)	(61.35)	(56.01)	(9.91)	(58.88)	(44.28)
Year 2007*Women	213.05	24.80	7.58	132.36	70.53	37.00**	-11.66	-16.42
	(257.78)	(106.75)	(117.11)	(81.82)	(83.20)	(16.39)	(48.53)	(42.16)
Year 2008*Women	486.29**	-59.10	229.28*	151.94*	87.16*	13.65	15.56	0.30
	(234.52)	(126.18)	(117.85)	(82.74)	(46.91)	(12.27)	(41.04)	(68.10)
Year 2006	16.53	17.28	19.33	-52.08	-26.63	-20.48**	57.26	27.57
	(161.74)	(86.51)	(57.90)	(39.08)	(30.99)	(8.82)	(51.16)	(43.81)
Year 2007	-20.88	-3.81	28.45	-54.55	-38.82	-27.55**	-3.99	51.11
	(196.26)	(72.59)	(90.68)	(52.52)	(55.68)	(11.89)	(36.96)	(37.51)
Year 2008	-457.74***	-107.96	-282.33***	-86.28	-90.16***	-9.28	-74.18***	38.94
	(156.68)	(77.30)	(86.76)	(55.83)	(33.00)	(11.06)	(27.63)	(58.68)
Z	-1780.14	-1339.17	-383.19	-808.51	-489.57	73.82	-1277.37	-10.56
	(1326.83)	(956.01)	(1319.98)	(1084.90)	(1025.19)	(197.67)	(1025.23)	(703.42)
N	484	484	484	484	484	484	484	484
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: * p<0.10 ** p<0.05 *** p<0.01. Standard errors clustered at the municipality level. Other controls include baseline per capita expenditure for each category, respectively, and municipal per capita revenues. Expenditure variables are per capita.

Table 3.10: Number of seats and social per capita expenditure 2000-2003

	Soc	Edu	Health	Soc.Prot.	Envi	Infra	Recre
3 seats 1999	-2.65	-3.84	-0.04	1.28	-1.96	-1.10	0.80
	(8.99)	(4.17)	(2.85)	(1.13)	(2.09)	(3.69)	(1.80)
Year 2001	-6.92	0.20	-2.75	0.92	-0.77	-2.78	-1.06
	(7.15)	(3.40)	(1.75)	(0.91)	(2.15)	(2.71)	(1.45)
Year 2002	-3.11	5.64	-2.15	-0.24	-1.09	-3.46	-1.07
	(8.00)	(4.43)	(1.79)	(0.64)	(2.11)	(2.54)	(1.88)
Year 2003	41.89***	12.96***	9.64***	3.17***	0.77	10.41***	5.58***
	(7.95)	(4.11)	(1.72)	(0.80)	(2.36)	(3.76)	(1.68)
Year 2001*3seats	3.40	2.55	0.31	-2.54*	2.05	-0.14	0.66
	(11.31)	(5.57)	(2.82)	(1.29)	(2.40)	(4.18)	(3.55)
Year 2002*3seats	19.28	11.86	1.07	0.05	2.76	5.14	-2.22
	(17.21)	(10.94)	(3.95)	(1.80)	(2.50)	(5.17)	(2.51)
Year 2003*3seats	15.54	10.56	4.14	-0.56	3.66	-2.34	-0.56
	(16.46)	(7.42)	(5.88)	(2.23)	(3.75)	(6.85)	(2.87)
Baseline expenditure	0.46***	0.41***	0.47***	0.09	0.03	0.20***	0.15**
	(0.09)	(0.11)	(0.10)	(0.06)	(0.03)	(0.06)	(0.06)
Copart income	0.03	0.01	0.01	0.00	0.00	0.01	0.01
	(0.04)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
N	887	887	887	887	887	887	887

Note: * p<0.10 ** p<0.05 *** p<0.01. Standard errors clustered at the municipality level. Income and expenditure variables are per capita.

Table 3.11: Women councillors, education, and health outcomes 2005-2010

	Primary enrollment (1)	Completion primary (2)	Comple.prim. gender gap (3)	Pentavalent Vaccine (4)	Delivery in institution (5)
Women	3.30	3.16	0.13	3.91	9.65
	(7.22)	(6.69)	(5.64)	(8.72)	(7.30)
Year 2006*Women	1.99	2.76	-16.04**	-0.49	-3.87
	(1.67)	(3.26)	(6.64)	(4.05)	(4.71)
Year 2007*Women	4.95*	5.84	-8.95	-1.49	-3.04
	(2.69)	(3.55)	(5.48)	(4.54)	(5.12)
Year 2008*Women	4.13	5.53	-9.06	-0.42	3.95
	(3.03)	(4.23)	(6.64)	(5.30)	(6.03)
Year 2009*Women	3.10	6.45	-9.77	8.79	7.62
	(3.39)	(4.47)	(6.70)	(7.56)	(6.44)
Year 2010*Women	2.54	0.87	-5.11	-4.91	21.79
	(3.78)	(4.28)	(6.16)	(7.17)	(19.66)
Year 2006	-5.12***	-3.81	9.08**	-3.58	3.62
	(1.55)	(2.48)	(4.34)	(2.92)	(3.61)
Year 2007	-10.50***	-9.28***	3.25	-5.50	3.70
	(3.01)	(3.22)	(4.12)	(3.90)	(4.00)
Year 2008	-16.38***	-10.69**	0.78	-12.09**	-0.36
	(4.81)	(4.69)	(4.40)	(5.99)	(4.86)
Year 2009	-17.32***	-8.72*	1.66	-18.58***	-3.28
	(4.95)	(4.68)	(4.70)	(6.44)	(4.71)
Year 2010	-21.09***	-8.76	-4.59	-15.01**	-6.28
	(6.27)	(5.66)	(4.36)	(7.38)	(9.38)
N	1476	1476	1476	1464	1464

Note: * p<0.10 ** p<0.05 *** p<0.01. Standard errors clustered at the municipality level. Other controls include Z (running variable), baseline indicators for each category, municipal per capita revenues. Expenditure variables are per capita.

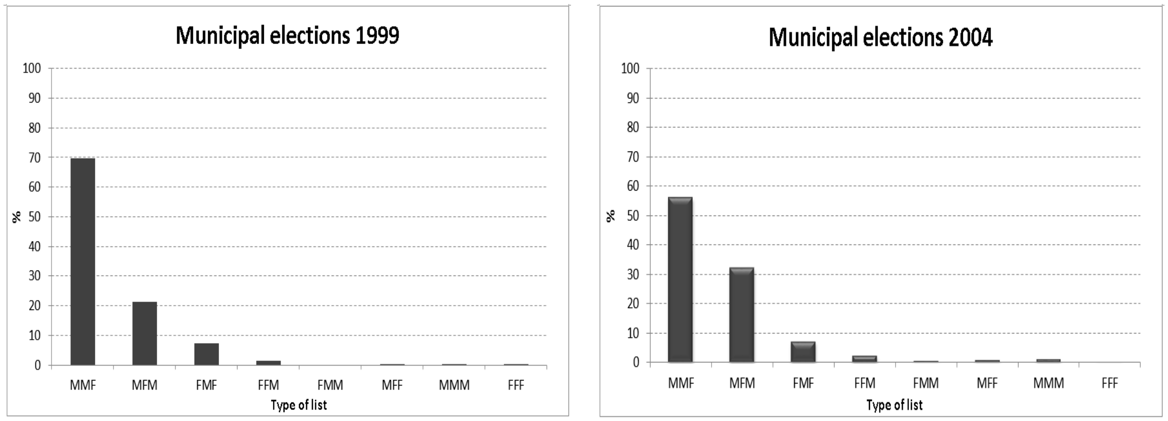

Figure 3.1: Types of list of candidates for municipal councillors

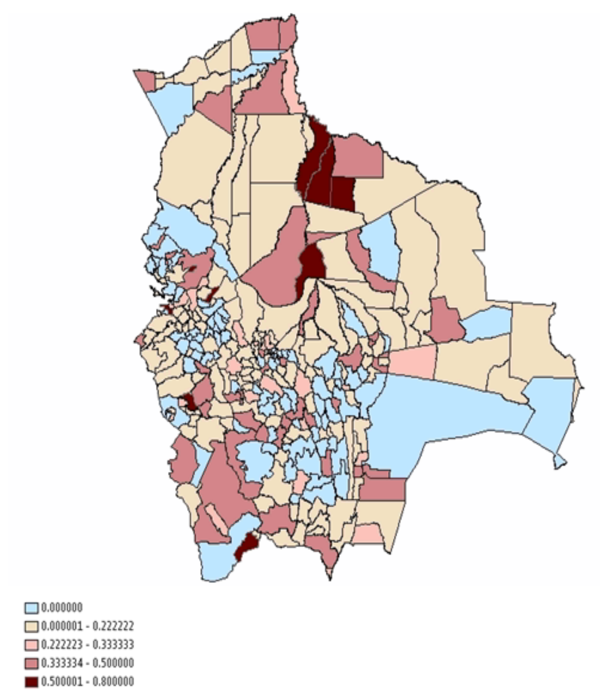

Figure 3.2: Proportion of women in municipal councils - 2004 elections

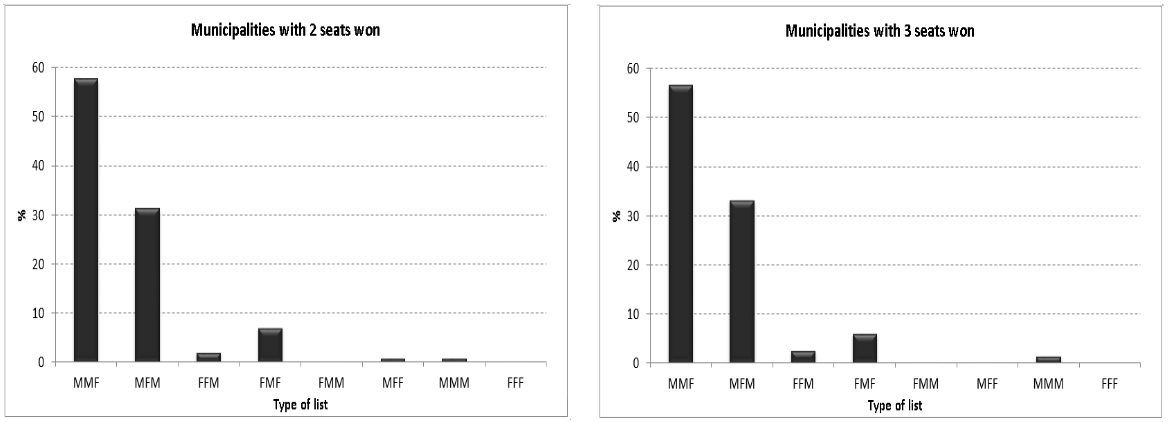

Figure 3.3: Types of list of candidates for municipal councillors

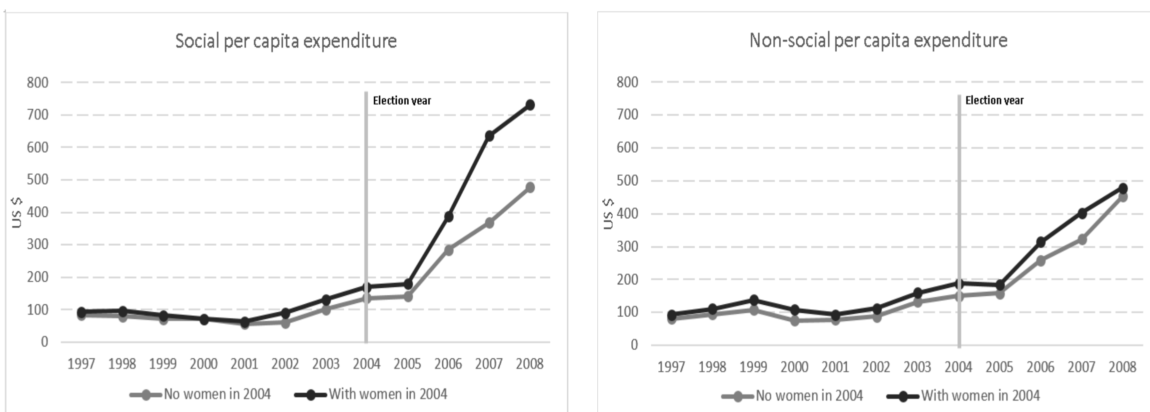

Figure 3.4: Evolution of per capita public expenditure in municipios with and without women councillors

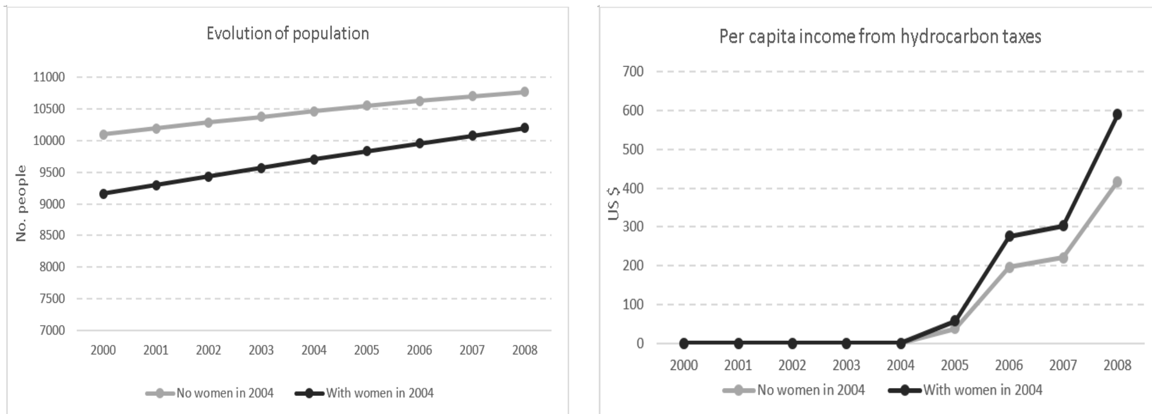

Figure 3.5: Evolution of potential confounders in municipios with and without women councillors

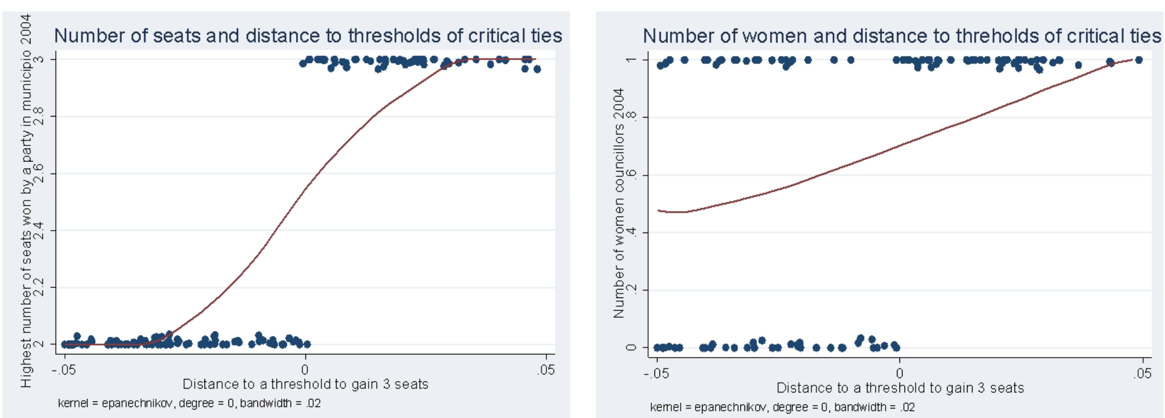

Figure 3.6: Evidence of discontinuity around the threshold

Appendix A

Table A1: **Distributional rule by region**

	Wages (%)	Transfers (%)	Proportion distributed
Region 1 (North)	92.31	7.69	0.05
Region 2 (center)	86.67	13.33	0.05
Region 3 (South West)	66.67	33.33	0.21
Region 4 (South East)	20.00	80.00	0.69
Total	65.52	34.48	0.26
Observations	38	20	58

Note: Uses only the sample of communities that participate in the program. Row percentages are reported in the first two columns. Prop.dist. is the proportion of PSAH funds distributed directly to households as lump-sum transfers.

Table A2: **Work in non-forest unpaid activities**

Dep. variable:	**Participation**		**Log number of days**	
	Member	**Non-member**	**Member**	**Non-member**
Benef*$P.\hat{dist}$.	0.060	0.082	1.191*	0.191
	(0.157)	(0.178)	(0.616)	(0.209)
N	456	243	446	241

Note: * p<0.10 ** p<0.05 *** p<0.01. Bootstrapped standard errors. Controls include: Population density, number of members, proportion of members that are women, area of forest per capita, average of days worked in FCA in the community in 2007, household wealth index 2007, and state fixed effects. Members are households with land-use rights. Control means refer to participation rates and number of days worked.

Table A3: **Work in forest conservation activities**

Dep. var.:	Participation		Log number of days		Prop. unpaid done	
	Member	Non-mem	Member	Non-mem	Member	Non-mem
	(1)	(2)	(3)	(4)	(5)	(6)
Year*Benef*$P.\hat{dist}$.	0.475**	-0.180	0.561*	-0.473	0.408*	-0.246
	(0.242)	(0.515)	(0.301)	(0.823)	(0.244)	(0.438)
N	914	488	914	488	905	477

Note: * p<0.10 ** p<0.05 *** p<0.01. Bootstrapped standard errors. Controls include: Population density, number of ejidatarios, proportion of ejidatarios that are women, area of forest per capita, average of days worked in FMA in the community in 2007, household wealth 2007, and state fixed effects. Members are households with land-use rights.

Table A4: **Work in forest activities (visible vs. less visible)**

Dep. variable:	Log number of days worked			
	Visible activity		Not visible activity	
	Member	Non-member	Member	Non-member
Year*Benef*$\hat{Prop.dist.}$	0.356*	-0.502	-0.188	0.132
	(0.207)	(0.492)	(0.201)	(0.142)
N	914	488	914	488

Note: * p<0.10 ** p<0.05 *** p<0.01. Bootstrapped standard errors. Other controls include: Population density, number of members, proportion of members that are women, area of forest per capita, average of days worked in FCA in the community in 2007, household wealth 2007, and state fixed effects. Members are households with land-use rights. Visible activities correspond to construction and maintenance of firebreaks, not so visible activities correspond to forest patrols.

Appendix B

Table B1: Critical tie for third position

	Party 1	Party 2	Party 3	Party 4	Party 5
# votes	v_1†	v_2*	v_3	v_4	v_5
# votes/2	$v_1/2$†	$v_2/2$	$v_3/2$	$v_4/2$	$v_5/2$
# votes/3	$v_1/3$*	$v_2/3$	$v_3/3$	$v_4/3$	$v_5/3$

Note: * are parties competing for the contested seat †are seats already allocated

Table B2: Critical ties for fourth position

Option A	Party 1	Party 2	Party 3	Party 4	Party 5
# votes	v_1†	v_2†	v_3	v_4	v_5
# votes/2	$v_1/2$†	$v_2/2$*	$v_3/2$	$v_4/2$	$v_5/2$
# votes/3	$v_1/3$*	$v_2/3$	$v_3/3$	$v_4/3$	$v_5/3$

Option B	Party 1	Party 2	Party 3	Party 4	Party 5
# votes	v_1†	v_2†	v_3*	v_4	v_5
# votes/2	$v_1/2$†	$v_2/2$	$v_3/2$	$v_4/2$	$v_5/2$
# votes/3	$v_1/3$*	$v_2/3$	$v_3/3$	$v_4/3$	$v_5/3$

Note: * are parties competing for the contested seat †are seats already allocated

Table B3: Critical ties for fifth position

Option A	Party 1	Party 2	Party 3	Party 4	Party 5
# votes	v_1†	v_2†	v_3*	v_4	v_5
# votes/2	$v_1/2$†	$v_2/2$†	$v_3/2$	$v_4/2$	$v_5/2$
# votes/3	$v_1/3$*	$v_2/3$	$v_3/3$	$v_4/3$	$v_5/3$

Option B	Party 1	Party 2	Party 3	Party 4	Party 5
# votes	v_1†	v_2†	v_3†	v_4*	v_5
# votes/2	$v_1/2$†	$v_2/2$	$v_3/2$	$v_4/2$	$v_5/2$
# votes/3	$v_1/3$*	$v_2/3$	$v_3/3$	$v_4/3$	$v_5/3$

Option C	Party 1	Party 2	Party 3	Party 4	Party 5
# votes	v_1†	v_2†	v_3†	v_4	v_5
# votes/2	$v_1/2$†	$v_2/2$*	$v_3/2$	$v_4/2$	$v_5/2$
# votes/3	$v_1/3$*	$v_2/3$	$v_3/3$	$v_4/3$	$v_5/3$

Option D	Party 1	Party 2	Party 3	Party 4	Party 5
# votes	v_1†	v_2†	v_3	v_4	v_5
# votes/2	$v_1/2$†	$v_2/2$ †	$v_3/2$	$v_4/2$	$v_5/2$
# votes/3	$v_1/3$*	$v_2/3$*	$v_3/3$	$v_4/3$	$v_5/3$

Note: * are parties competing for the contested seat †are seats already allocated

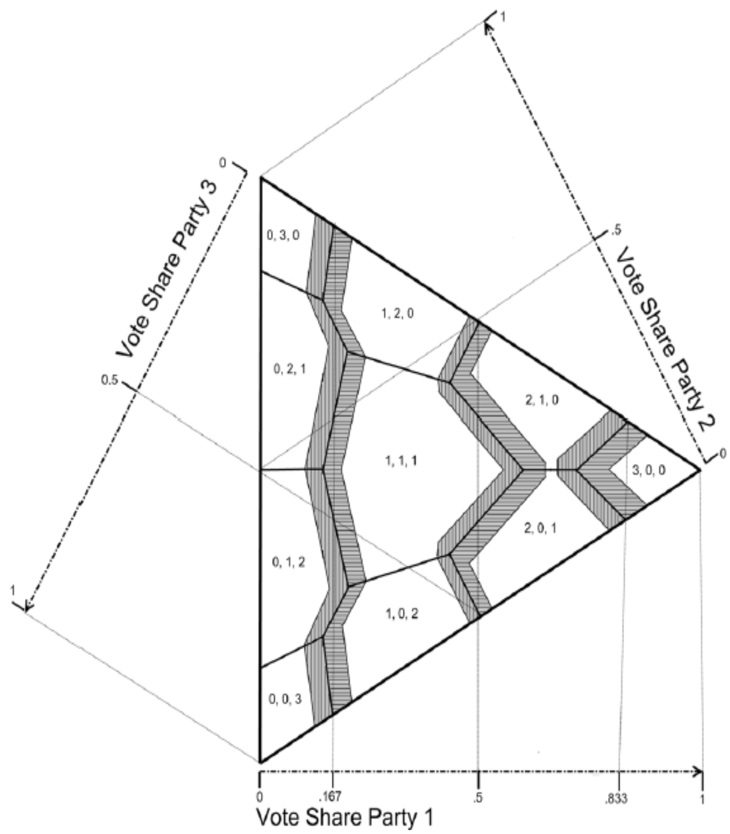

Figure B1: Party-map extracted from Folke (2013): Distribution of three seats between three parties as a function of their vote shares. The number of seats of each party is written within each contiguous "seat outcome" region in the order Party 1, Party 2 and Party 3. Regions defined as close to a threshold for Party 1 are marked in grey. The vertical lines indicate that Party 1 is close to gaining a seat, while the horizontal line indicates its being close to losing a seat. The seats are allocated using the Sainte-Lague method

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