Only One Tree from Each Seed? Environmental Effectiveness and Poverty Alleviation in Mexico's Payments for Ecosystem Services Program

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Environmental conditional cash transfers are popular but their impacts are not well understood. We evaluate land cover and wealth impacts of a federal program that pays landowners for protecting forest. Panel data for program beneficiaries and rejected applicants allows us to control for fixed differences and time trends affecting both groups. We find the program reduces the expected land cover loss by 40-51 percent and generates small but positive poverty alleviation. Environmental gains are higher where poverty is low while household gains are higher where deforestation risk is low, illustrating the difficulty of meeting multiple policy goals with one tool. **(JEL: Q28, Q56, Q58, I38, O13)**

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Between 2000 and 2010, 130,000 square km of the world's forests, an area roughly the size of Nicaragua, were converted every year to other uses (Food and Agriculture Organization of the United Nations 2010). Land-use change leads to losses of biodiversity and water quality and is the second largest source of global CO_2 emissions contributing to climate change (Intergovernmental Panel on Climate Change 2007). The empirical evaluation of potential carbon emissions reduction policies (Martin, De Preux, and Warner 2011, Li, Linn, and Spiller 2013, Davis, Fuchs, and Gertler 2014) is increasingly important given the expected negative social impacts of climate change (Schlenker, Hanemann, and Fisher 2005, Tol 2009, Dell, Jones, and Olken 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, International Union for Conservation of Nature 2009, United Nations 2009). Future financial flows for REDD, mainly from developed to developing countries, are predicted to be close to US $30 billion a year (United Nations REDD Programme 2011).

To reach REDD goals, many countries will employ voluntary conditional cash transfers to landowners who maintain forest cover. These "payments for ecosystem services" (PES) 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, Swallow, and Kerr 2008, Wunder and Wertz-Kanounnikoff 2009, United Nations REDD Programme 2011). 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 (e.g. Landell-Mills and Porras 2002, Wunder, Engel, and Pagiola 2008, Turpie, Marais, and Blignaut 2008, Lipper et al. 2009). PES programs can provide a steady stream of income in areas

where livelihoods are risky, and given the strong global correlation between forest cover and poverty, they appear at first glance to be an attractive "win-win" policy solution. However, despite their popularity, rigorous empirical evidence on the impacts of payments for avoided deforestation on both environmental and economic outcomes is extremely limited (Pattanayak, Wunder, and Ferraro 2010, Miteva, Pattanayak, and Ferraro 2012, Alix-Garcia and Wolff 2014). Simultaneous evaluation of impacts is difficult due to the vastly different spatial scales of data needed and the fact that many PES beneficiaries are located in remote or isolated areas which are costly to reach and survey.

In this paper, we use multiple novel data sets to evaluate the land cover and poverty reduction impacts of a national-scale environmental payments program and the tradeoffs between those objectives. The program we study is Mexico's Payments for Hydrological Services Program (PSAH), a federal program that pays private or communal landowners to maintain forest cover under five year contracts. We estimate environmental impacts for the 2004-2009 program cohorts using land cover data from 2000-2012 and national program data. We evaluate socioeconomic impacts at the household level using survey data from 2007-2011 for a nationally-representative subsample of the 2008 cohort and at the locality level using changes in the poverty index from 2000-2010. In each case, panel data for 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 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 multiple specifications, including

using different subsets of rejected applicants to establish counterfactual time trends. We find that the program has enrolled land with a similar degree of poverty as the national distribution and that on average the program has slightly reduced poverty at the locality level (~.05 standard deviations). We do not find significant impacts on average household consumption or investment, but the data show significant positive effects for poor households. In addition, minimum detectable effect sizes rule out substantial negative impacts on consumption or investment, even for the households most likely to be negatively affected by the program. Together the results suggest small but positive poverty alleviation impacts.

Given that most proposed REDD programs are in nations where poverty alleviation is a high priority, we also seek to understand whether it is possible to increase the program's environmental effectiveness without decreasing participation by the poor. Although the upside for households is small on average, the fact that the program provides a steady stream of payments and has positive impacts for poor households suggests that it is important to consider how changes in targeting would impact participation. To do so, we construct a simple rent-based model of deforestation risk based on targeting characteristics and test for heterogeneous impacts consistent with this model. The data show that avoided deforestation impacts could indeed be increased by targeting more funds to land with higher geographic risk of deforestation, but that this would reduce participation by the poor. Also consistent with theory, the household data show greater socioeconomic impacts where deforestation risk and thus expected opportunity costs are lower. However, there is some scope to improve environmental effectiveness and increase participation of the poor by targeting more funds to communal properties, where deforestation risk is high and households are poorer.

These results make three contributions to the literature. First, we add to the 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 (or 26,000 sq. km) of land in the program, making it one of the largest PES programs in the world. Mexico was also one of the top 10 deforesters in the world between 2000-2010, losing nearly 195,000 hectares of forest every year (FAO 2010). Mexico's experience 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, Arriagada et al. 2012, Robalino and Pfaff 2013, Pfaff et al. 2014). Rigorous retrospective evidence about the environmental effects of Mexico's program is limited to the Monarca reserve (Honey-Roses, Baylis, and Ramirez 2011) and the 2004 PSAH cohort (Alix-Garcia, Shapiro, and Sims. 2012).

We advance the literature on the environmental impacts of PES in Mexico and more generally by using panel data on both beneficiary and rejected applicants. A key concern with evaluations of PES is that apparent effectiveness may be driven by unobservable differences in participation costs such as low land quality or landowner skills (Pattanayak, Wunder, and Ferraro 2010, Miteva, Pattanayak, and Ferraro 2012). Lack of deforestation may thus be attributed to these omitted characteristics rather than participation in the program. By using the behavior of matched rejected applicants over time to establish the counterfactual, we ensure that all landowners have revealed both their desire to enroll in the program and low expected participation costs. To our knowledge, the only previous paper to use rejected applicants from a national PES program was our cross-sectional evaluation of the 2004 PSAH cohort on deforestation between 2003-2006 (Alix-Garcia, Shapiro, and Sims 2012). Here we evaluate environmental impacts for six cohorts (2004-2009) using annual data from 2003-2011 and exploiting variation in enrollment across time as well as space. This allows us to estimate avoided

deforestation impacts from regressions with location or parcel fixed-effects, better addressing potential selection issues and confirming that the program did significantly reduce deforestation.

Our second contribution is to simultaneously evaluate the impacts of environmental conditional cash transfers on household and locality wealth indicators. Although PES programs are generally voluntary and thus likely to benefit households, it is possible that 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, households without full land rights or who are intensive users of communal land may be harmed by access restrictions (Pfaff et al. 2007, Bulte et al. 2008, Zilberman, Lipper, and McCarthy 2008, Hawkins 2011). There is essentially no previous literature testing for household impacts of national PES programs that uses rejected applicants (Miteva, Pattanayak, and Ferraro 2012, Alix-Garcia and Wolff 2014). To date, household impacts have been studied using predictions based on the cross-sectional composition of the participant group or by comparing outcomes over time between participants and a control group composed of non-participants from the general population (Pfaff et al. 2007, Uchida et al. 2007, Rios and Pagiola 2010, Gauvin et al. 2010, Jayachandran 2013). Our results thus provide important evidence that PES can result in neutral or small positive impacts for households.

Our third contribution is to investigate whether heterogeneity in program impacts implies tradeoffs between environmental efficacy and poverty alleviation potential. Previous research on PES has pointed out the theoretical irreconcilability between cost-effective avoided deforestation and poverty reduction if the forest at greatest risk is not owned by the poorest households (Pagiola, Arcenas, and Platais 2005, Pfaff et al. 2007, Alix-Garcia, De Janvry, Sadoulet 2008, Bulte et al. 2008, Jack, Kousky, and Sims 2008, Zilberman, Lipper, and McCarthy 2008, Leimona and Joshi 2009, Pattanayak, Wunder, and Ferraro 2010, Pfaff and Robalino 2012,

Pfaff et al. 2013). However, previous empirical studies simulate tradeoffs based on baseline profiles of beneficiaries (Uchida, Rozelle, and Xu 2009, Wünscher, Engel, and Wunder 2008, Gauvin et al. 2010) whereas our assessment includes measured household impacts. Our results indicate mainly tradeoffs between environmental protection and poverty alleviation. This suggests that PES should be considered more of a "win-neutral" strategy for environment and development rather than a "win-win." Policymakers promoting REDD should be realistic and not expect implementing agencies to meet multiple social goals with a single policy tool.

The paper proceeds as follows. Section I provides program background, an overview of our empirical strategy and data sources, and details of the pre-matching of data. Section II presents the analysis of land cover impacts and section III the analysis of household impacts. Section IV investigates tradeoffs between environmental and socioeconomic impacts and section V concludes.

I. Program Background and Data

A. Program background

Mexico's PSAH 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 sustain 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 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 of indigenous population (Muñoz-Piña et al. 2008, Sims et al. 2014).

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 communal 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, Engel, and Pagiola 2008).[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 of PSAH. Table 1 shows the annual payment rates, total area enrolled,

[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 reallocation occurring 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 forest cover change.

and the number of parcels accepted and rejected for these cohorts. Yearly 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 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 percent of household income, given the estimated income brackets of the private property households.[3]

TABLE 1 HERE

B. Overview of analysis

We use multiple novel data sets to evaluate environmental and socioeconomic impacts of PSAH. Figure 1 shows the overall structure and timing of our data. To evaluate environmental outcomes, we use annual data on land cover from 2003-2011 (collected by us) and forest cover data from 2000 and 2012 (from Hansen et al. 2013). Program enrollment over time is based upon spatial data on the boundaries of accepted and rejected parcels from each program cohort. To

[3] The mean program per capita payment in common property communities is 1,539 pesos. This number is a lower bound as it excludes any payments for technical support and includes the total population in the community, including children and older adults. Amounts were converted to US dollars using the exchange rate reported for the 15th of July of 2011 (11.72 pesos/ USD). The daily minimum wage reported by CONASAMI in 2011 is 58.1 pesos and we assume 20 working days within a month. For private households, the mean payment per year is 35,777 pesos. 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 annual income for the upper 3 deciles is ~ 289,593 pesos.

evaluate household socioeconomic outcomes, we use data from a survey covering a subset of beneficiary and rejected applicants from the 2008 cohort. The survey was designed and conducted by us in 2011 and includes recall questions establishing a baseline for the year 2007. To evaluate socioeconomic outcomes at the locality level, we examine changes in the poverty index published by the Mexican government in 2000 and 2010.

FIGURE 1 HERE

As described in the following two sections, identification relies on panel regressions comparing changes over time in outcomes for locations or households receiving the program to those which applied but were rejected. Before estimating impacts, we pre-match both data sets to ensure covariate overlap, particularly for characteristics that might influence both 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 and Wahba, 1999, 2002, Ho et al. 2007, Stuart 2010). By eliminating as potential controls locations or households which do not share the same observable covariates as program beneficiaries, we generate a more plausible estimate 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 discussed in the following section, our data indicate similar pre-program trends for beneficiaries and rejected applicants, making this assumption plausible. In addition, for the environmental analysis, we are also able to exploit variation in enrollment across time due to having multiple cohorts enter the program in different

years. In the next two sections, we provide the details of the data construction including units of analysis and pre-matching strategies.

C. 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 causes a decrease in average annual NDVI. We use 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 (~6 hectares). Although the data used in this paper was newly constructed for this project, a similar methodology has been previously established and field-tested by the Mexican National Forestry Commission (CONAFOR 2011, Meneses-Tovar 2009a,b). Economists have also relied on NDVI decreases to measure deforestation in previous research in both developed and developing countries (Foster and Rosenzweig 2003, Mansfield et al. 2005, Burgess et al. 2012). The continuous measure of NDVI has the advantage of picking up areas of loss smaller than 6 hectares, as well as 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, 0.25 x 0.25 degrees resolution). To corroborate our results, we also use changes in forest cover at the locality level from a new analysis of global forest change using Landsat satellite images (30 m x 30 m resolution) between 2000 and 2012 (Hansen et al. 2013). This represents one of the first uses of this new dataset for the analysis of public policy.

National Program Data and Points Sample. Spatial information on the boundaries of accepted and rejected parcels was collected from the Mexican National Forestry Commission. Figure 2 shows the boundaries of parcels applying between 2004 and 2009 and indicates that the program is truly national in extent (rejected applicants are shown in Appendix Figure A1 and have similar spatial coverage).

In seeking to evaluate the land cover impacts of the program using the national program data, we face two data construction issues. First, reapplications and renewals 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. As is indicated by the timeline in Figure 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, we code beneficiary status according to whether a point entered the program in the previous calendar year.

FIGURE 2 HERE

The second issue is that land use outcomes are likely to be spatially correlated due to similarity in geographies or influence of neighbors (Robalino and 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 ~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

[4] We use only points that were forested according to both the INEGI Series III land use layer (circa 2002) and had baseline 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

we sample ~1 out of every 16 available NDVI pixels. The points are spaced randomly (not in a grid) and we 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 ~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 (Table 2). 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). 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.

TABLE 2 HERE

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 and Wooldridge 2009, Stuart 2010). The matching characteristics are covariates which determined selection into the program and could influence deforestation outcomes. Broadly, 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 percent 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

points which enrolled in the 2003 cohort and control for recipient status of any points which 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.

required to have > 50 percent 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 availability, location in protected areas or priority mountain areas, and location in a high poverty or majority indigenous municipality (see description of program evolution in Sims et al. 2014). 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.

We pre-match points that were beneficiaries in any year with points that applied but were never beneficiaries using covariate matching with the Mahalanobis metric.[7] We require exact matches within geographic region, land tenure type (common property vs. other[8]), 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 availability, 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 with replacement and drop any repeated points before the regression analysis so that each control point appears only once in the final dataset.

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[6] "Index of Economic Pressure to Deforest / Risk of Deforestation" version 1. Methodology at http://www.inecc.gob.mx/irdef-eng.) Scale of 1-5.

[7] 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).

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

Table 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. Since INECC's deforestation index is not available for all points, we create our own risk of deforestation index using only never enrolled points and geographic determinants of deforestation.[9] 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 square root of the sum of variances for both treated and control groups, and is the most commonly accepted diagnostic used to assess covariate balance (Rosenbaum and Rubin 1985, Stuart 2010).

The statistics indicate that the program beneficiaries are fairly representative of all forested land in Mexico. A frequent concern with PES programs is that they will attract areas with the lowest risk of deforestation. In this case the beneficiaries have, on average, similar risk to a randomly drawn point: 0.19 standard deviations less according to INECC's index and 0.09 standard deviations more according to our index. This indicates that the program did reach areas of moderate deforestation risk but also that there is scope to target the program to higher risk of deforestation areas. The summary statistics for the municipal poverty index for all forested areas compared to beneficiaries show that the

[9] 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, which ranges from -4 to 1.5. The low probability of deforestation in most of the country leads to a large number of negative values using this index.

program reached areas with a similar degree of poverty to the country as a whole. The summary statistics in columns 2, 3 and 5 also confirm the selection criteria discussed above. Compared to rejected applicants, beneficiary locations had higher initial NDVI, were somewhat closer to urban areas and had a slightly higher risk of deforestation according to INECC and our risk index. Enrolled land was also more likely to be in communal properties and in majority indigenous municipalities.

The 4th column of Table 2 shows summary statistics for the matched rejected points and Figure 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 in municipal poverty and water availability. 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 and Wooldridge 2009). 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 3). Comparing the sums of each of the normalized differences in Table 2 (last row, columns 5-7), we see that using just rejected applicants reduces the difference from 2.85 to 1.59 and matching further reduces this difference to 0.66. This illustrates that about half of the improved covariate balance comes from using rejected applicants and about half from matching, suggesting the usefulness of both steps in attempting to obtain unbiased estimates.

FIGURE 3 HERE

D. Socioeconomic outcomes and household 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 1). 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 four regions of the country (see Appendix Figure A2). 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 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, 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 114 private households (60 beneficiaries and 54 non-beneficiaries) and 1096 households in common property communities (590 beneficiaries and 506 non-beneficiaries) distributed

[10] Using recall data could potentially bias our results if memory failure is systematically different between beneficiary and non-beneficiary households. 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 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.

over 116 communities.[12] Appendix Table A1 indicates the breakdown of surveyed households in each region. The surveyed properties are similar in regional distribution and forest type to those enrolled in the program in 2008 (results available from authors).

The survey was also stratified by land rights status within communal properties. Surveyors randomly selected five households with full land-use rights and voting power ("ejidatarios" or “members”) and five without ("non-ejidatarios" or “non-members”).[13] 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. We test for average impacts on households and for differences between members and non-members, but detailed modeling and analysis of possible internal community dynamics are beyond the scope of this paper (see however ongoing analysis by Yañez-Pagans 2013).

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.

[12] We use households with complete information to ensure comparability across regressions. These households are not systematically different than the households with missing information on one or more variables and results are similar if we use all available data.

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

Durables purchases and housing characteristics are aggregated by price.[14] 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.

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 school-age household children.[15] For food consumption, we construct an index using prices reported by households and whether or not they purchased a 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

[14] We checked robustness with two other indices common to the development literature: 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.

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

of cattle and small animals, we apply an inverse hyperbolic sine transformation (Burbidge, Magee, and Robb 1988).[16]

Table 3 shows baseline summary statistics for surveyed beneficiary and non-beneficiary households. Since households in common property communities are substantially poorer and less well educated on average than private property households, we analyze them separately.[17] 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 (Table 3). 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 3).[18]

TABLE 3 HERE

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

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

[18] 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.

E. Locality level data on forest cover and socioeconomic outcomes

In order to corroborate our results with respect to new global forest change data from Hansen et al.'s (2013) analysis and to examine broader social impacts, we also analyze program impacts at the locality level across all of Mexico. A locality in Mexico is the smallest administrative unit, similar to a village or census tract.[19] For each locality we calculate the percent forest cover change from 2000-2012 (as a percent of land area; positive if a locality gained forest, negative if lost forest) and the change in the poverty index from 2000-2010. The locality poverty index is from CONAPO, Mexico's population agency, and is based on a weighted average of indicators including rates of literacy, primary schooling, availability of potable water, sanitation and electricity, and housing characteristics. We re-normalize each year's index values to have mean zero and standard deviation one, so changes can be interpreted in terms of standard deviations away from the national mean. To measure changes in program enrollment at the locality level, we calculate the share of land area that was enrolled in the program between 2000 and 2010, the share of land area that applied for the program but was never accepted, and the share of land area times the number of years each area was in the program.

II. Avoided Deforestation: Estimation and Results

A. Main estimation strategy

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

[19] We assign land area to each locality using the method of Thiessen polygons previously developed in Alix-Garcia, et al. (2013). Since locality points vary somewhat from year to year, we use the locations from 1995 (N=105,648) and calculate area weighted means of indicators using all available points from other years.

(1) $MNDVI_{ipst} = \beta beneficiary_{it} + \delta' \boldsymbol{rainfall_{it}} + \alpha_{st} + \alpha_i + \varepsilon_{ipst}$

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.[20] To control for rainfall and hurricanes ($\boldsymbol{rainfall_{it}}$) we include the natural logarithms of dry season rainfall and of rainfall in the other months prior to the dry season. To control for 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 (α_i) 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 4. 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 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 percent level.)

TABLE 4 HERE

To interpret magnitudes, we compare coefficients to counterfactual trends of NDVI loss for matched controls and all non-recipient data. The bottom row in Table 4 indicates the effect size of our estimates as a percentage of the expected

[20] We introduce the lag to take into account the timing of the applications versus the timing of the NDVI measurements (see Section I.c).

five-year (contract lifetime) loss of NDVI. Our simple preferred specification (column 1) indicates that the program offsets approximately 40-51 percent 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 percent 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 percent of the expected loss. We note that these results are similar to those found in Alix-Garcia, Shapiro, and Sims (2012), confirming that the findings 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 program estimates, with effect sizes ranging from 28 to 93 percent.

B. Parallel trends test and robustness checks

Identification in the analysis 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 5 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.[21] 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.

TABLE 5 HERE

A second way to test the robustness of our results is to use different groups of rejected applicants. As indicated in Figure 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 percent 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 percent), 3) lacking complete paperwork or necessary documentation (~30 percent). Columns 3-8 of Table 5 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. One concern of this type is that applicants who are rejected but reapply might be more likely to hold off on deforesting (biasing results towards zero). Column 3 uses only points that were beneficiaries at some time between 2004 and 2010. Column 4 uses as controls only parcels that 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

[21] 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).

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 4. The results are robust to using these different subsets of controls.

In addition, we check robustness to using the full sample and using a permanent beneficiary status to ensure results are not driven by exit. Columns 9 and 10 of Table 5 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. In columns 11 and 12, we respond to a concern that our estimates might be driven by deforestation upon exit of the program rather than avoided deforestation upon entry. 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. The result remains positive and significant (column (11)), although slightly smaller than our baseline specification in Table 4, column (1). This result is not very surprising given that only the 2004 and 2005 cohorts can exit the program before 2011 and within these two cohorts, approximately 35 percent reapply and 20 percent successfully renew their contracts. We also test for differential permanent impacts for these two cohorts (column 12) and observe a positive, but not statistically significant difference, indicating that the positive effect of the program still holds on average for cohorts

that can exit the program. For further exploration of exit and possible dynamic impacts of the program, see Appendix B.

C. Locality-level deforestation impacts

Finally, we also estimate program impacts at the locality level using a first differences regression:

(2) $$\Delta Y_{is} = \beta_0 + \beta_1 \Delta SharePSAH_{is} + \delta' \boldsymbol{X}_i + \alpha_s + \varepsilon_{is}$$

where ΔY_{is} is the change in forest cover from 2000 to 2012 for locality *i* in state *s*, $\Delta SharePSAH_{is}$ is the share of the locality enrolled in the program between 2000 and 2010, $\boldsymbol{X}_i$ is a vector of locality level controls (see Table 4 notes), and α_s is a state fixed effect. The results (Table 6) again indicate significant reduction in deforestation for localities with a higher share of area enrolled in the program. As shown in column 2, localities with a higher share of rejected areas did not see a significant increase in forest cover during this period, indicating that just applying for the program had no significant effect. Column 3 shows similar results when we account for the length of time each parcel within the locality was enrolled.[22] To interpret magnitudes, we note that during this time period, the average loss of forest cover across all localities was -1.2 percent of land area. The effect sizes are calculated using the average share enrolled, conditional on having any beneficiaries (0.23) and suggest that at the locality level, on average the program offset approximately 19-22 percent of the expected loss trend.

TABLE 6 HERE

[22] These results are robust to a variety of specifications, including: a) using hectares deforested instead of percent, and controlling for the area of the locality; b) using only localities with more than 10 percent in forest cover; c) dropping the largest and smallest 5 percent of localities; d) including squared terms for baseline forest cover and baseline poverty.

III. Socioeconomic Impacts: Estimation Strategy and Results

A. Estimation strategy

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

(3) $$A_{ipt} = \beta_1 beneficiary_{pt} + \alpha_i + \alpha_t + \varepsilon_{ipt}$$

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 for 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.

B. Impacts on household consumption

Table 7 gives estimates of program impact on consumption goods for common property and private property households. We observe that none of the simple treatment effects are 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.[23] The magnitudes are also quite small – ranging from 0 to 3.2 percent

[23] Given that we are testing multiple outcomes, the most conservative statistical significance level to use would be α/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

increases. Considering possible impacts on members versus non-members, we find no significant differences in consumption impacts, although the estimated marginal effects for non-members, for whom we might be most concerned about negative impacts, are positive for all three indices.

TABLE 7 HERE

For common properties, the minimum detectable effects within common property communities range from 0.8 percent for the housing index to 7 percent for the durables index.[24] This suggests that, while the data is noisy, were there a program impact with greater magnitude than this our sample would be likely to detect it at conventional significance levels. For private properties, the minimum detectable effects range from 0.3 percent to 12 percent and again do not indicate that a substantial downside to the program likely occurred.

C. Parallel trends and robustness

As in the environmental analysis, the identification of household socioeconomic impacts for the 2008 cohort 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, but we test it indirectly using pre-trends in the locality poverty index data from 2000 to 2005. We examine 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

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

[24] The minimum detectable effect is the smallest true effect 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 a two-tailed test of significance at p=0.05.

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 the survey sample from the 2008 cohort. In addition, our results are robust to including all observations in each regression regardless of whether they have missing information for some outcomes or covariates and 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.

D. Locality-level poverty index impacts

Table 8 shows program impacts on the locality level poverty index. Our dependent variable is the change in locality poverty from 2000-2010, and we use the same regression specification as equation 2. The regressions include controls for both the pre-trend from 1990-2000 and the 2000 level. We find significant decreases in the poverty index for localities with a greater share enrolled, and no changes for those with a greater share of area which applied but was rejected. We also find significant decreases in the poverty index when we account for the number of years each parcel within the locality was enrolled. Considering the effect if the whole locality received payments is a reduction of .17 standard deviations and the average share for localities with the program was 0.23, this implies an average reduction in the poverty index of 0.04 – 0.05 standard deviations. Thus, the results at the national level suggest that the program did have positive impacts on wealth but that the average effect sizes are quite small.[25]

TABLE 8 HERE

[25] These results are robust to the same specifications given in footnote 22.

E. Impacts on household investment and program participation costs

In this section we explore two possible explanations for the apparently small socioeconomic impacts. 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. Although the results in section II indicate that the program substantially reduced the expected deforestation trend, they also indicate that the average opportunity costs of participation should be small because expected deforestation rates are low overall. Two additional possibilities are then that households are using the money for investment rather than consumption, or that the participation costs of the program are high compared to the payments. We find evidence consistent with both possibilities.

Appendix Table A2 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 (5.4 percent). We do not see sizeable or significant changes in the likelihood of investing in livestock infrastructure, agricultural inputs, agricultural equipment, or schooling attendance for age groups 12-14 or 18-22. However, we do observe a marginally significant increase (approximately 13 percent relative to baseline) in the probability of attending school for children between the ages of 15 and 17 years old. There are no significant impacts on private household investment, although all signs are positive.

To determine whether transaction or implementation costs explain the small impacts, we conduct an accounting exercise using questions from the community leaders' survey about 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 transaction 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, program implementation costs are considerable compared to payments. The most important household 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.[26] Private households also report more days spent in fire prevention (+38 days), pest 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.[27] This means that payments may be helpful for environmental effectiveness but provides a likely explanation for why average wealth impacts are small.

[26] 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 households. According to this data, the program induces on average a change of 4.4 additional days of labor in forest conservation (relative to the changes in labor in non-beneficiary communities). Valued at the minimum wage, this is worth about 255 pesos, which amounts to only 16 percent of the estimated mean per capita payment (see footnote 3). We think this difference may be explained by a skewed distribution of forest conservation activities among households and a system of rotating responsibilities for community activities.

[27] 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.

IV. Heterogeneity, Tradeoffs, and Policy Implications

The previous two sections indicate that on average Mexico's PSAH is effective in reducing the rate of deforestation and has small positive impacts on 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.

A. Framework

In order to illustrate the targeting problem faced by program managers implementing payments for avoided deforestation, we discuss a simple rent driven model of land use (see e.g. Chomitz and Gray 1996, Samuelson 1983, Pfaff 1999, Robalino 2007, Angelsen 2010, Pagiola 2011, Alix-Garcia, Shapiro, and Sims 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 use 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 4 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. For example, returns to agriculture may be higher than returns to forest for land which is of low slope, but decrease rapidly as slope increases. This decrease is slower for forest since slope does not decrease forest productivity as rapidly as it does agricultural productivity. The classic statement of this model, using distance to city for q_j, can be found in Von Thunen (1966). 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.[28] 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.

FIGURE 4 HERE

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 4, to achieve full avoided

[28] 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 and Minten 1999, 2002, Alix-Garcia, de Janvry, and Sadoulet 2005, Bray and Klepeis 2005, Alix-Garcia 2007, Díaz-Gallegos, Mas, Velázquez 2009). 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, Shapiro, and Sims 2013) 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.

deforestation at least budgetary expenditure,[29] 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.

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.[30]

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[29] 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. Yet cost-effectiveness cannot be assessed simply by comparing budgetary outlays to amount of deforestation avoided: the true costs of the program include the administrative and transactions costs of running and participating in the program, and any distortionary effects of raising the program revenue in addition to the opportunity costs implied by our diagram.

[30] 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, Engel, and Wunder (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. (2014) find that efforts to better target Costa Rica's PES payments starting in 2000 did improve avoided deforestation impacts from 2000-2005. Alix-Garcia, Shapiro, and Sims (2012) find more avoided deforestation where baseline poverty rates are lower and Honey-Roses, Baylis, and Ramirez. (2011) find larger impacts of PES in protecting high quality habitat in the Monarca reserve.

This framework also demonstrates that tradeoffs between environmental effectiveness and participation of the poor 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 perfectly 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 is apparent from Figure 4, the difference between the returns to forest use with PES in place and the forgone returns to agriculture ($r_f^{PESopt} - r_a^1$) increases as rents decrease. Thus we should expect greater socioeconomic impacts of the program where the risk of deforestation is lower. In addition, if rents are indeed positively 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 if payments are targeted where geographic deforestation risk is higher.
- If the correlation between land rents and wealth is positive and the program is targeted to high deforestation 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. There will be greater wealth impacts in areas where

deforestation risk is lowest, but within the set of properties enrolled, relatively poorer beneficiaries will gain more wealth.

- Opportunities to make the program more pro-poor without compromising environmental effectiveness exist only if it is possible to re-target funds to poorer households with similar or higher risk of deforestation.

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

B. 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. All interacted covariates are demeaned, so we can interpret the beneficiary variable as the impact on the average location in the sample. As shown in Table 9, the results indicate that the program is indeed more effective where deforestation risk is higher – in locations with lower slope, closer to cities, or with higher levels of our deforestation risk index (columns 1 and 2). For example, 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 not statistically significant impact for a point in the 20th percentile of deforestation risk.

TABLE 9 HERE

In terms of poverty, we find significantly less avoided deforestation at higher levels of baseline municipal poverty (Table 9, column 3), suggesting the main tradeoff between targeting to the poor and improving environmental effectiveness.[31] However, the interaction with common property beneficiaries

[31] 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

(column 4) indicates the program is most effective in the common properties. This result is maintained even when also including the interactions with geographic deforestation risk and municipal poverty (column 5). It suggests some scope for a 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. Within common properties, households also tend to be more dependent on agriculture (in our survey over 80 percent of common property households report participating in agriculture in 2007 compared to around 50 percent of private households), implying fewer off-farm labor opportunities and a potentially greater importance of the steady-stream of payments provided by PES.

C. Poverty alleviation

Recall that our model suggests that if deforestation risk is negatively correlated with wealth, there should be greater wealth effects among relatively poorer beneficiaries of the program due to lower opportunity costs. Yet poor households may also benefit more simply because of a higher marginal utility of income. To explore heterogeneity by risk of deforestation, we create a metric of deforestation risk using the predictions from the environmental section to divide our sample into "high" (above the median), and "low" (below median) deforestation risk.[32] To examine variation in impacts by baseline poverty, we divide

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.

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

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.

The evidence in Table 10 is consistent with both hypotheses. For households in common properties, the deforestation risk interaction estimates (Table 10, columns 1 and 4) with consumption indicators are generally consistent with the theory presented above, although the interaction term is only significantly different from zero for durables. Private property households at high risk have marginally significantly lower impact for the housing index and no differences in the durables index. Considering the interactions with high poverty, we also observe positive and significant differences in consumption impact for both common property and private property households who are below median poverty in the baseline (Table 10, columns 2 and 5). For poor common properties the magnitudes indicate a 12.3 percent increase in the durables index, and 1.1 percent increase in the housing index; for poor private households, there is also a marginally significant increase in the durables index (9.6 percent) and a significant increase in the housing index (0.4 percent). There are no significant results for the food index for either common or private properties.

TABLE 10 HERE

In Columns (3) and (6) of Table 10 we introduce interactions for both risk of deforestation and poverty simultaneously. Coefficients are very similar although we lose some precision, which is evidence of the correlation of risk and poverty. More specifically, we see that the larger impacts for households that are poorer remain statistically significant in common property communities. For private households, the impacts for poor households only remain significant for the durables index, but the results with risk of deforestation are still significant.

Given that the risk index is based only on geographic variables, and therefore proxies for land productivity, these results may indicate that we observe greater

impacts for the poor both because they have lower opportunity cost as well as higher marginal utility of income.

With respect to investment outcomes for common property households, we observe almost no heterogeneity across deforestation risk, with the exception of increases in agricultural equipment for higher deforestation risk properties (Appendix Table A3). For private households, the results on cattle, small animals, livestock infrastructure and agriculture investment do have signs consistent with theory, but are not statistically different from zero (Appendix Table A3). Within common properties, poorer households have marginally significantly higher investment in agricultural inputs, equipment, and schooling, while the results for private properties are inconclusive (Appendix Table A3).

Together, these results again indicate tradeoffs between the possible poverty alleviation potential suggested by greater gains for the poor and the lower impacts for those at higher risk of deforestation. To the extent that the program could be targeted to poorer households with the same or higher risk of deforestation that would make the program more pro-poor; we explore this possibility in the next section.

D. Targeting policy thought experiment

To quantify the possibilities for improvements in targeting to generate environmental effectiveness or make the program more pro-poor, we use the point estimates from Table 9 to conduct four thought experiments, shown in Table 11. All of these experiments involve replacing 30 percent of the enrolled points with 30 percent of unenrolled points. This simulates what would happen if CONAFOR kept the same eligibility zones and criteria for the program but changed the

prioritization of deforestation risk, poverty, or common property ownership.[33] We find that increased targeting on our geographic risk of deforestation index doubles the NDVI impact but also substantially decreases the municipal poverty index, from 0.294 to 0.160. Increased targeting to the poorest recipients boosts the municipal poverty index to 1.08 but eliminates any environmental impact. Attempting to increase both deforestation and poverty impact simultaneously by including the poorest of the high risk deforestation candidates and excluding the richest of the low deforestation risk candidates greatly increases the municipal poverty index of enrolled properties, but results in a very similar environmental impact because there are not sufficient numbers of poor properties among those at high risk of deforestation to simultaneously increase the impact along both metrics. However, this exercise does reveal the small improvement possible by prioritizing common property applicants: the NDVI impact is boosted from 0.216 to 0.272 with a small increase in the municipal poverty index.

TABLE 11 HERE

V. 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 as a mechanism for maintaining environmental quality. Yet our findings also suggest the need to be cautious with respect to whether PES can generate significant poverty alleviation: our results suggest that overall PES is more of a "win-neutral" than "win-win" strategy for environment and development.

[33] This exercise assumes the same applicant pool and likely gives upper bounds to targeting changes, since the regulator cannot target at the point-level.

In addition, our study highlights the likely tradeoffs inherent in trying to improve environmental effectiveness. 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 targeting to increase avoided deforestation will generally decrease the potential for poverty alleviation by reducing participation of poor households, who stand to gain the most from the program. To the extent that this relationship holds globally, REDD program design must confront this tradeoff. In Mexico's case, one opportunity for win-win targeting is created by the fact that the program generates significant avoided deforestation in common properties, which also have poorer households. Careful analysis of risk of deforestation and ownership distributions in other countries may allow REDD designers to find similar opportunities.

Our analysis draws attention to several interesting avenues for future research. These include important questions about program spillovers on deforestation or household behavior, post-program behavior by landowners, and within-community political economy. Previous work (Alix-Garcia, Shapiro, and Sims 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 more forest in order to keep the option of enrolling in the future. This general equilibrium effect would mean that our results understate the true effectiveness of the program. We may also be concerned that although the program has real short term impacts, landowners will revert to deforestation when payments end. Previous research from the U.S. Conservation Reserve Program suggests that PES can have positive enduring impacts (Roberts and Lubowski 2007), but not enough time has passed in Mexico to test well for long term impacts.

Finally, our survey data indicate that a likely explanation for the small average socioeconomic impacts is high participation costs due to increased forest management and maintenance by beneficiaries. Most analyses of PES and estimates of the costs of REDD focus on opportunity costs of forgone production, yet our household data suggests these may be small compared to the costs of active forest maintenance. Additional work is needed to better understand short and long run costs of participation as well as possible long term gains to communities from investments in forest resources or increased human capital.

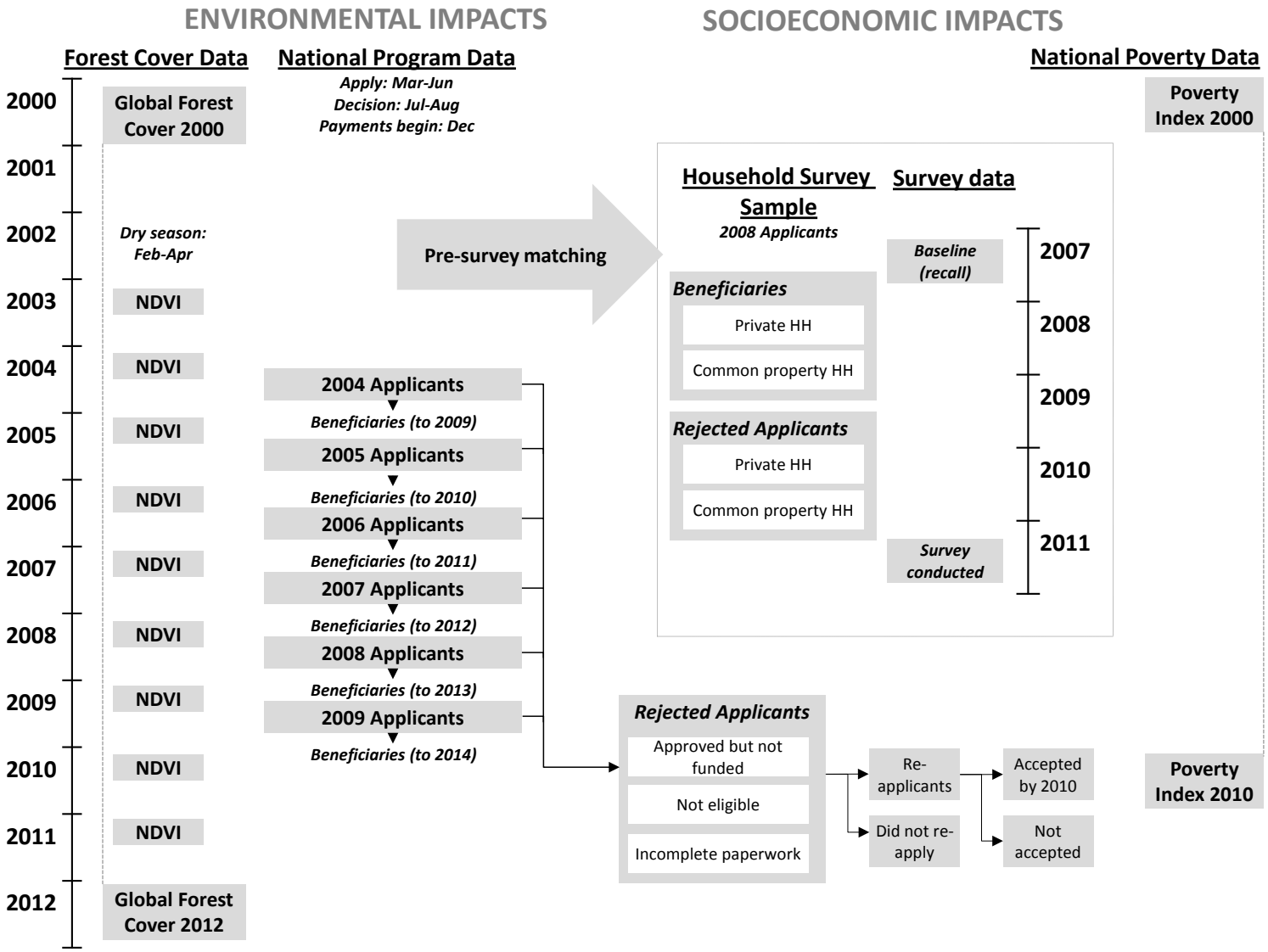

FIGURE 1. SCHEMATIC OF DATA COLLECTION

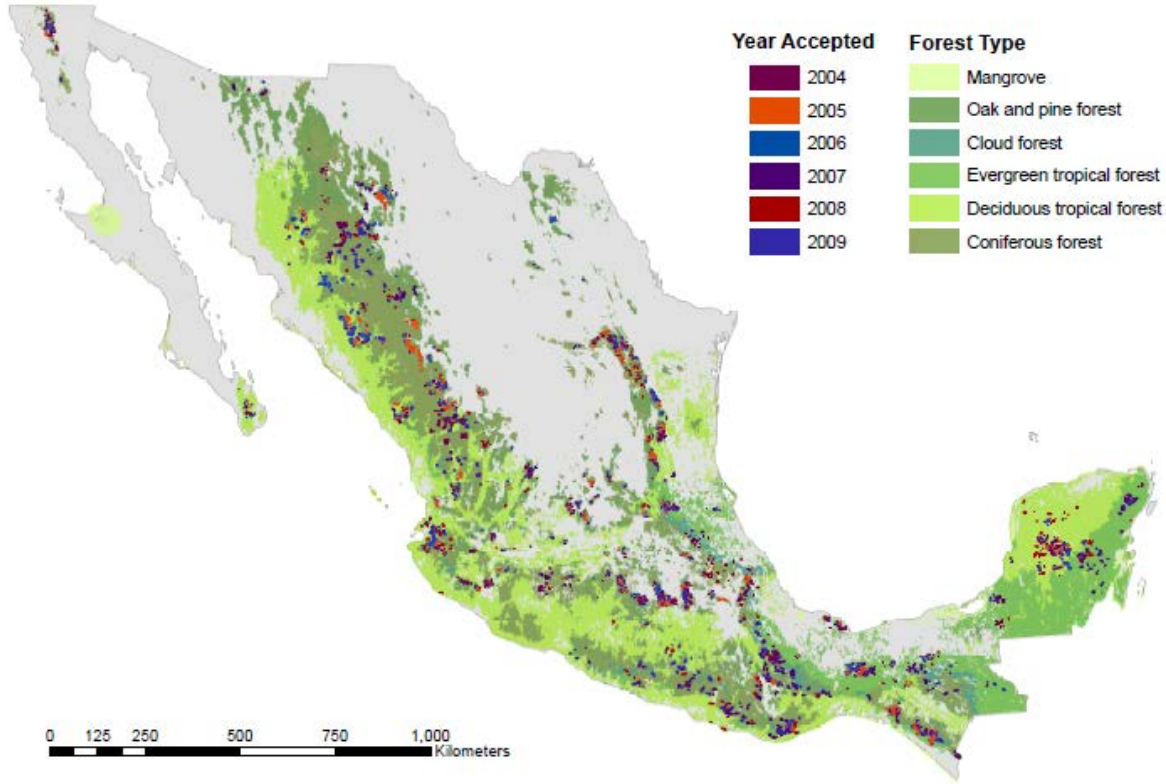

FIGURE 2. PSAH BENEFICIARIES, 2004-2009

Notes: Data on program applicants from CONAFOR. Forest types from the INEGI Series III land use layer (circa 2002).

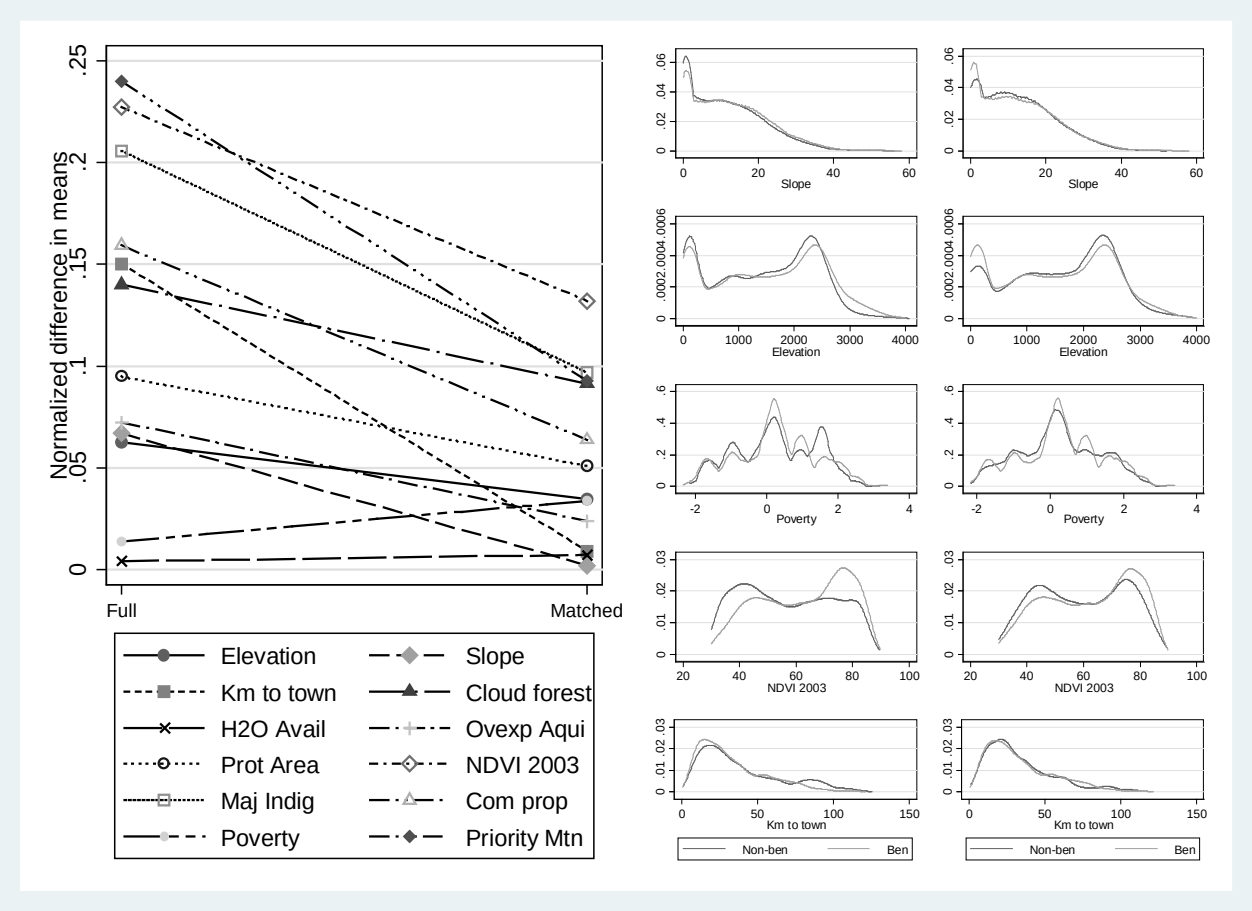

FIGURE 3. CHANGES IN NORMALIZED DIFFERENCES AND DISTRIBUTIONS AFTER MATCHING

Notes: Left side of figure shows normalized difference in means between beneficiary and non-beneficiary points before and after covariate matching. Right side of figure shows covariate distributions for beneficiary and non-beneficiary points before (left) and after (right) covariate matching.

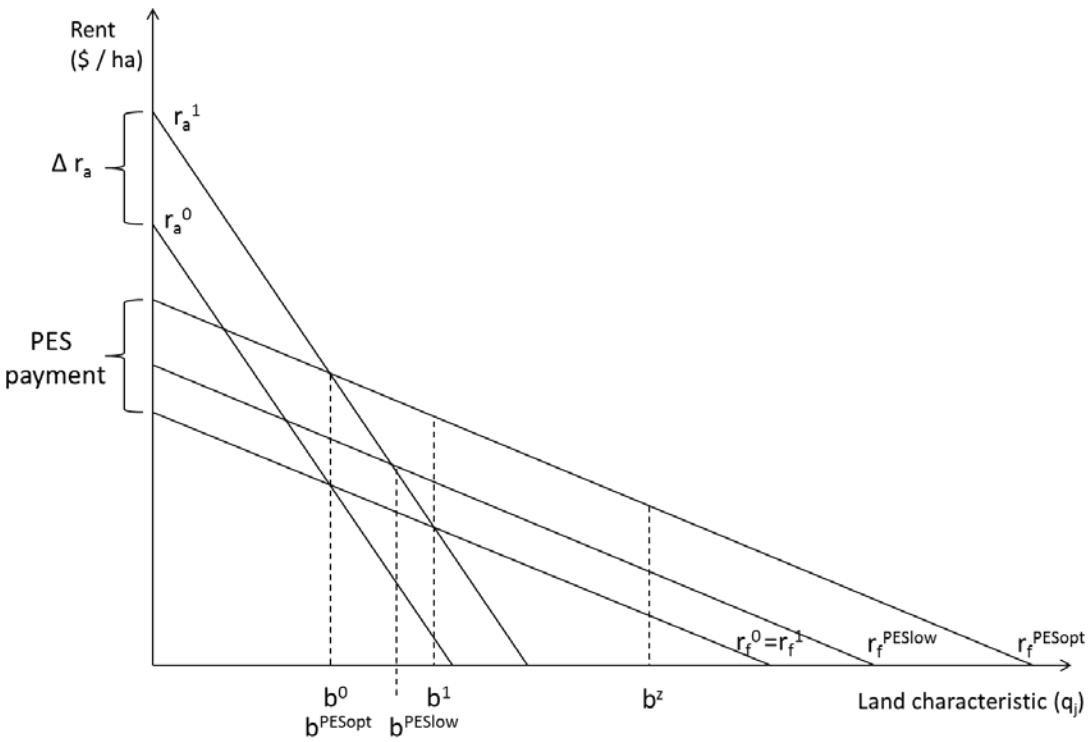

FIGURE 4: ECONOMIC FRAMEWORK: RENT MODEL OF PES

Notes: Graphical rent model described in Section IV.A. Y-axis indicates rents from agricultural (r_a) or forested (r_f) land. X-axis shows a land characteristic which affects rent and is used for targeting.

TABLE 1 - PAYMENT RATES AND PROGRAM APPLICATIONS FOR THE 2004-2009 PSAH COHORTS

	2004	2005	2006	2007	2008	2009
PANEL A. Payment rates						
Rate per hectare-year cloud forest (Mexican pesos)	400	400	413.70	429.85	447.02	465.80
Rate per hectare-year for other forest types (Mexican pesos)	300	300	316.35	328.71	341.84	356.20
					394.43 for oak forest	411.00 for oak forest
PANEL B. 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

TABLE 2 - SUMMARY STATISTICS: POINTS WITHIN APPLICANT BOUNDARIES AND OTHER FORESTED POINTS

	All forested areas	Beneficiaries	All rejected applicants	Matched rejected applicants	Normalized difference (1) v (2)	Normalized difference (2) v (3)	Normalized difference (2) v (4)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Slope (deg)	10.3	12.2	11.3	12.3	-0.14	0.07	0.00
	(9.5)	(9.9)	(9.6)	(9.6)			
Elevation (m)	1161	1520	1436	1568	-0.27	0.06	-0.03
	(867)	(981)	(921)	(930)			
Km to nearest town	38.1	33.5	38.8	33.2	0.13	-0.15	0.01
	(27.2)	(22.1)	(27.0)	(22.1)			
Municipal poverty	0.24	0.29	0.27	0.23	-0.03	0.01	0.03
	(1.02)	(1.12)	(1.13)	(1.09)			
Common property (0/1)	0.60	0.88	0.80	0.85	-0.47	0.16	0.06
	(0.49)	(0.33)	(0.40)	(0.36)			
Overexploited aquifer (0/1)	0.07	0.16	0.12	0.15	-0.19	0.07	0.02
	(0.26)	(0.37)	(0.33)	(0.35)			
Water availability (0-8)	7.18	6.85	6.86	6.83	0.16	0.00	0.01
	(1.31)	(1.67)	(1.53)	(1.61)			
Priority mountain (0/1)	0.07	0.25	0.12	0.19	-0.36	0.24	0.09
	(0.25)	(0.43)	(0.32)	(0.39)			
Protected area (0/1)	0.07	0.12	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)	55.5	62.3	57.2	59.4	-0.30	0.23	0.13
	(16.4)	(15.6)	(16.2)	(15.6)			
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 / Sum Nd	44,104	17,307	18,456	4,489	2.85	1.59	0.66

Notes: 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, divided by the square root of the sum of variances for both groups (Imbens and Wooldridge 2009). The last row in columns 5-7 gives the sum of the normalized differences across all the covariates.

TABLE 3-SUMMARY STATISTICS: BENEFICIARY AND NON-BENEFICIARY HOUSEHOLDS

	Common properties						Private properties		
	Full sample			Matched sample					
	Beneficiary	Non-Beneficiary	Norm. Diff.	Beneficiary	Non-Beneficiary	Norm. Diff.	Beneficiary	Non-Beneficiary	Norm. Diff.
Food index 2011 (100s pesos)	2.12	2.04	0.07	2.13	2.01	0.10	2.48	2.53	-0.05
Durables index 2007 (10000s pesos)	1.85	1.63	0.07	1.84	1.43	0.13	4.74	4.28	0.12
Housing index 2007 (10000s pesos)	9.96	10.06	-0.02	9.94	9.85	0.02	16.79	14.91	0.14
# cattle 2007	2.99	4.57	-0.09	2.82	4.81	-0.10	19.58	23.24	-0.05
# small animals 2007	8.34	6.44	0.06	8.45	6.66	0.05	31.42	12.00	0.10
Elevation (m)	1602	1471	0.09	1593	1590	0.002	1289	1271	0.01
Slope (deg)	9.31	9.91	-0.06	9.50	10.45	-0.09	9.11	9.72	-0.06
Distance locality>=5000	32.24	30.19	0.08	32.38	30.54	0.07	26.75	29.23	-0.10
Municipal poverty 2005	0.72	0.78	-0.04	0.74	0.77	-0.02	0.92	0.66	0.18
Area of parcel enrolled (ha)	1021	1241	-0.17	1040	1253	-0.16	103.8	108.0	-0.04
Household size	4.90	4.60	0.09	4.92	4.61	0.10	4.33	3.93	0.14
Member of community	0.65	0.66	-0.003	0.64	0.64	0.01	NA	NA	NA
Days worked FCA 2007	17.55	7.49	0.20	8.89	7.00	0.08	NA	NA	NA
Participated in FCA 2007	0.58	0.41	0.25	0.55	0.51	0.06	NA	NA	NA
Number of observations	590	506		548	374		60	54	

Notes: The full sample considers all those observations 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, divided by the square root of the sum of variances for both groups (Imbens and Wooldridge 2009).

TABLE 4 - IMPACTS OF PSAH ON NDVI

Dependent variable:	**Annual Mean Dry Season NDVI (Points data)**					
	Change in levels	**Change in trend**	**Change in levels, years in program**	**Change in levels**	**Change in trend**	**Change in levels, years in program**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
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)
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
R^2	0.49	0.49	0.49	0.68	0.68	0.68
Effect size (percent of 5 year trend)	40-51	28-36	54-69	39-66	28-47	55-93

Notes: Columns 1-6: Point or parcel-level fixed effects model (equation 1). Columns 1-6 all include state-year fixed effects and rainfall controls. 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. The effect sizes use counterfactual trends of NDVI loss: 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.

*** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level.

TABLE 5 - PARALLEL TREND TEST AND ROBUSTNESS CHECKS

Panel A	Dependent variable = Annual Mean Dry Season NDVI					
	Mahalanobis matched subsample					
	Parallel trend test	**Parallel trend test**	**Only treated parcels**	**Controls: approved but unfunded**	**Controls: in eligible zones, not qualified**	**Controls: in eligible zones**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Future beneficiary*time	-0.0246	-0.0147				
	(0.0396)	(0.0664)				
Beneficiary			0.2466***	0.2253***	0.2104***	0.2161***
			(0.0831)	(0.0763)	(0.0763)	(0.0726)
Point FE	Y		Y	Y	Y	Y
Parcel FE		Y				
N	84662	62866	155763	173178	174402	188073
R^2	0.45	0.68	0.49	0.49	0.49	0.49

Panel B	Dependent variable = Annual Mean Dry Season NDVI					
	Mahalanobis matched subsample		Full sample		Mahalanobis matched subsample	
	Controls: high NDVI only	**Controls: rejected and never reapplied**	**Baseline specification**		**Permanent beneficiary**	**Permanent beneficiary**
	(7)	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
Beneficiary	0.2010**	0.1961***	0.0629	0.1602**		
	(0.0835)	(0.0745)	(0.0787)	(0.0799)		
Permanent beneficiary dummy					0.1775**	0.1522*
					(0.0759)	(0.0837)
Permanent beneficiary dummy * 2004/2005 cohort						0.1042
						(0.1555)
Point FE	Y	Y	Y		Y	Y
Parcel FE				Y		
N	134802	184653	321867	286104	196164	196164
R^2	.45	0.49	0.48	0.67	0.48	0.48

Notes: Robust standard errors clustered at the parcel level in parentheses. . Dependent variable is mean dry season NDVI ranges from 0 to 100). All estimations include rainfall controls and state-year fixed effects. 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. Columns (11) and (12) use all matched data.

*** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level.

TABLE 6 - IMPACTS OF PSAH ON LOCALITY LEVEL DEFORESTATION: FIRST DIFFERENCE ESTIMATION

Dependent variable:	**Percent Forest Cover Change (Locality data)**		
	(1)	**(2)**	**(3)**
Share area beneficiary	1.1062***	1.0992***	
	(0.2879)	(0.2875)	
Share area rejected		0.1496	
		(0.2840)	
Share area beneficiary* years paid			0.2842***
			(0.0690)
State FE	Y	Y	Y
N localities	105648	105648	105648
R^2	0.273	0.273	0.273
Effect size (percent)	19-22	19-22	24-28

Notes: First differences model at the locality level. Robust standard errors, clustered at the municipality level in parentheses. Regression includes state fixed effects and controls for: locality poverty index in 2000, change in poverty index from 1990-2000; population density in 2000, distance to road, distance to nearest locality with population > 5000; distance to major city; average slope; average elevation; percent forest in 2000 (National Forest Inventory and Hansen et al. 2013); municipal poverty in 2000; degree of water scarcity; overexploited watershed status; whether or not municipality is majority indigenous. The effect sizes use counterfactual cover change trends among all localities (-1.192 percent) and untreated localities with > .01 share rejected areas (-1.373 percent).

*** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level.

TABLE 7 - IMPACTS OF PSAH ON HOUSEHOLD CONSUMPTION

	Common property households			Private property households		
Dependent variable:	**Food index (1)**	**Durables index (2)**	**Housing index (3)**	**Food index (4)**	**Durables index (5)**	**Housing index (6)**
PANEL A. Simple treatment						
Beneficiary	0.028	0.032	0.000	-0.009	0.022	0.001
	(0.019)	(0.025)	(0.003)	(0.027)	(0.043)	(0.001)
PANEL B. Treatment by tenure class						
Beneficiary	0.028	0.031	0.000			
	(0.019)	(0.024)	(0.003)			
Beneficiary x non-member	-0.020	-0.024	0.001			
	(0.028)	(0.028)	(0.003)			
Marginal effect (for non-members)	0.015	0.016	0.001			
Base mean	2.014	1.673	9.901	2.532	4.519	15.900
Base standard deviation	0.870	2.263	4.417	0.846	2.601	9.544
Minimum detectable effect	0.053	0.070	0.008	0.076	0.120	0.003
N	922	1844	1844	114	228	228

Notes: Columns 1-6: 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. Beneficiary x member interaction term uses demeaned covariates.

*** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level.

TABLE 8 - IMPACTS OF PSAH ON LOCALITY POVERTY: FIRST DIFFERENCES ESTIMATION

Dependent variable:	**Change in poverty index**		
	(1)	**(2)**	**(3)**
Share area beneficiary	-0.173***	-0.172***	
	(0.048)	(0.048)	
Share area rejected		-0.014	
		(0.047)	
Share area beneficiary*years paid			-0.043***
			(0.012)
Marginal effect at mean share	-0.041	-0.040	-0.051
	105648	105648	105648

Notes: Columns 7-9: First differences model at the locality level. Robust standard errors, clustered at the municipality level in parentheses. Regressions include state fixed effects and controls for: locality poverty index in 2000, change in poverty index from 1990-2000; population density in 2000, distance to road, distance to nearest locality with population > 5000; distance to major city; average slope; average elevation; percent forest in 2000 (National Forest Inventory and Hansen et al. 2013); municipal poverty in 2000; degree of water scarcity; overexploited watershed status; whether or not municipality is majority indigenous.

*** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level.

TABLE 9 - IMPACTS ON NDVI ACROSS DEFORESTATION RISK AND POVERTY MEASURES

	Dependent variable: mean dry season NDVI				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Beneficiary	0.1572**	0.1939***	0.1749**	0.1878***	0.1844**
	(0.0750)	(0.0724)	(0.0718)	(0.0719)	(0.0720)
Beneficiary x ln(slope)	-0.1014				
	(0.0807)				
Beneficiary x km to large locality	-0.0076***				
	(0.0029)				
Beneficiary x deforestation risk		0.2904***			0.2491***
		(0.0583)			(0.0569)
Beneficiary x municipal poverty			-0.2423***		-0.2145***
			(0.0456)		(0.0440)
Beneficiary x common property				0.4983***	0.5908***
				(0.1734)	(0.1711)
N	196164	196164	196164	196164	196164
R^2	0.48	0.48	0.48	0.48	0.49

Notes: Point-level fixed effects model with state-year dummies and rainfall controls (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). Variables interacted with beneficiary status are demeaned. Regressions use data from program beneficiaries and matched rejected applicants; matching as described in footnote of Table 2.

*** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level.

TABLE 10 - IMPACTS ON CONSUMPTION BY DEFORESTATION RISK AND BASELINE POVERTY

	Durables index			Housing index		
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A. Common properties						
Beneficiary	0.035	0.031	0.033	0.000	0.000	0.000
	(0.024)	(0.024)	(0.024)	(0.003)	(0.003)	(0.003)
Beneficiary x	-0.079**		-0.052	-0.006		-0.004
high deforestation risk	(0.040)		(0.037)	(0.005)		(0.004)
Beneficiary x poor		0.123***	0.110***		0.011***	0.010***
		(0.041)	(0.038)		(0.003)	(0.003)
N	1844	1844	1844	1844	1844	1844
PANEL B. Private properties						
Beneficiary	0.022	0.024	0.021	0.002*	0.001	0.002*
	(0.040)	(0.042)	(0.040)	(0.001)	(0.001)	(0.001)
Beneficiary x	-0.001		0.013	-0.005*		-0.004*
high deforestation risk	(0.052)		(0.053)	(0.002)		(0.002)
Beneficiary x poor		0.096*	0.097		0.004**	0.004**
		(0.057)	(0.059)		(0.002)	(0.002)
N	228	228	228	228	228	228

Notes: All indices are constructed as described in footnote to Table 7 and transformed using the inverse hyperbolic sine. Interaction terms use demeaned covariates.

*** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level.

TABLE 11 - SIMULATED TRADEOFFS IN TARGETING

"Enrolled" non-recipients	**"Disenrolled" recipients**	**NDVI impact**	**Municipal poverty index of recipients**
Actual program	Actual program	0.216	0.294
Highest deforestation risk points	Lowest deforestation risk points	0.442	0.160
Poorest	Wealthiest	0.005	1.080
Poorest of high deforestation risk points	Wealthiest of low deforestation risk points	0.245	0.712
Common properties	Private properties	0.272	0.337

Notes: Simulations use coefficients from Table 9 column 5. We limit the points analyzed to those within eligible zones. In each scenario we simulate the impact of "enrolling" the untreated points and "disenrolling" an equivalent proportion of the treated points.

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APPENDIX PART A: ADDITIONAL FIGURES AND TABLES REFERRED TO IN TEXT

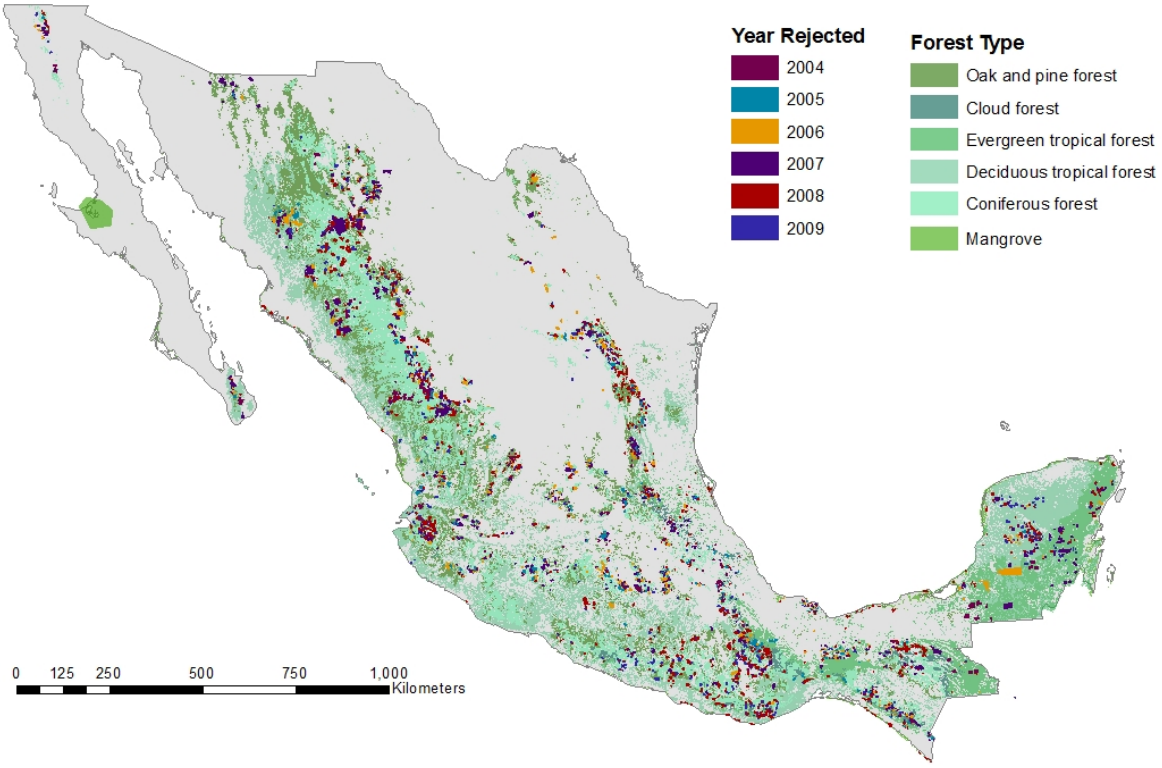

FIGURE A1. REJECTED PSAH APPLICANTS, 2004-2009

Notes: Data on program applicants from CONAFOR. Forest types from the INEGI Series III land use layer (circa 2002).

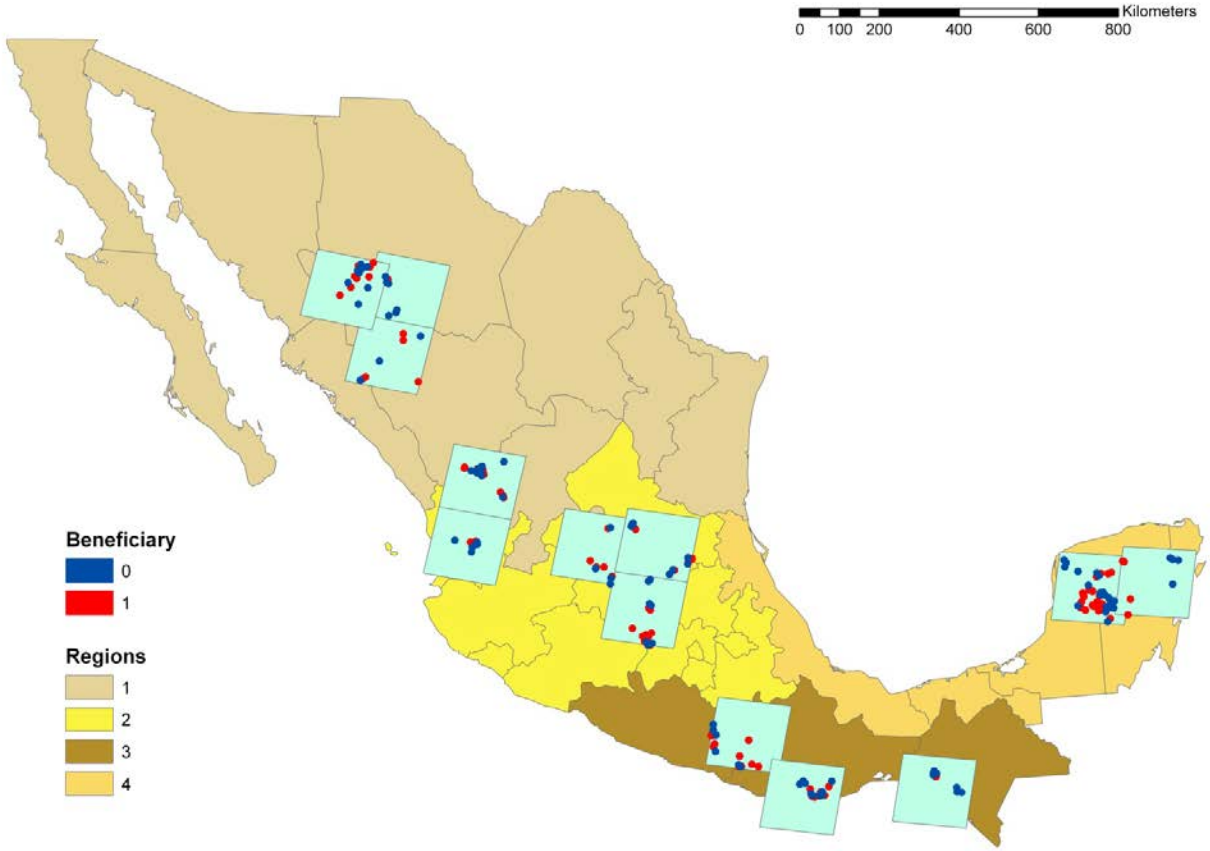

FIGURE A2. SURVEY SAMPLE AND SURVEY REGIONS

Notes: Centroid points of properties surveyed (summer 2011). Total number of properties surveyed = 234. Red points are properties that participated in the PSAH program. Blue points are matched properties that applied to the program but were rejected. Region 1 = North, Region 2 = Center, Region 3 = Southwest, Region 4 = Southeast.

TABLE A1 - SAMPLE SIZE OF SURVEY AND DISTRIBUTION BY PROPERTY TYPE AND REGION

Regions	Households in common properties			Private landowners		
	Beneficiaries	Non-Beneficiaries	Total	Beneficiaries	Non-Beneficiaries	Total
1. North	137	136	273	15	12	27
2. Center	160	129	289	15	15	30
3. Southwest	149	128	277	16	14	30
4. Southeast	144	113	257	14	13	27
Total households	**590**	**506**	**1096**	**60**	**54**	**114**
Total properties	**61**	**55**	**116**	**60**	**54**	**114**

Notes: Regions as shown in Figure A2.

TABLE A2 - AVERAGE IMPACTS OF PSAH ON PRODUCTIVE AND HUMAN CAPITAL INVESTMENT

	Agricultural investment					Educational investment		
	# Cattle	**# Small animals**	**Livestock infrast.**	**Agricultural inputs**	**Agricultural equipment**	**Student 12-14 yrs**	**Student 15-17 yrs**	**Student 18-22 rs**
PANEL 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
PANEL 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

Notes: 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.

*** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level.

TABLE A3 - EFFECTS ON PRODUCTIVE AND HUMAN CAPITAL INVESTMENT BY POVERTY AND DEFORESTATION RISK

	Agricultural investment					Educational investment		
	# Cattle	**# Small animals**	**Livestock infrast.**	**Agricultural inputs**	**Agricultural equipment**	**Student 12-14 yrs**	**Student 15-17 yrs**	**Student 18-22 yrs**
a. Common properties								
High risk of deforestation								
Beneficiary	0.054**	-0.027	0.034	-0.006	-0.019	0.033	0.103*	0.049
	(0.022)	(0.038)	(0.030)	(0.025)	(0.020)	(0.038)	(0.060)	(0.057)
Beneficiary x high risk	0.003	0.015	0.000	-0.012	0.061**	0.047	0.000	-0.112
	(0.032)	(0.050)	(0.038)	(0.033)	(0.031)	(0.049)	(0.070)	(0.070)
High poverty								
Beneficiary	0.054**	-0.026	0.033	-0.007	-0.017	0.036	0.104*	0.050
	(0.022)	(0.038)	(0.030)	(0.024)	(0.020)	(0.038)	(0.060)	(0.057)
Beneficiary x poor	0.009	-0.024	0.028	0.066*	0.057*	-0.069	-0.058	0.115*
	(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	0.088	0.012	0.106	0.107*	0.044			0.055
	(0.090)	(0.116)	(0.070)	(0.062)	(0.060)			(0.075)
Beneficiary x high risk	-0.189	-0.034	-0.072	-0.013	0.006			0.014
	(0.139)	(0.080)	(0.094)	(0.085)	(0.075)			(0.083)
High poverty								
Beneficiary	0.056	0.006	0.094	0.101	0.046			0.056
	(0.085)	(0.120)	(0.071)	(0.067)	(0.063)			(0.075)
Beneficiary x poor	0.090	0.022	0.045	-0.268***	0.079			-0.077
	(0.125)	(0.102)	(0.096)	(0.097)	(0.083)			(0.078)
N	228	228	228	228	228			201

Notes: Dependent variables and regressions as described in footnote to Table A2; with additional interaction terms. Interactions use demeaned covariates.

*** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level.