Forest Conservation and Slippage: Evidence from Mexico's National Payments for Ecosystem Services Program

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ABSTRACT. *We investigate a Mexican federal program that compensates landowners for forest protection. We use matched controls from the program applicant pool to establish counterfactual deforestation rates. Deforestation was reduced by 50% in enrolled parcels, but expected average clearing rates without the program were low (0.8% per year), suggesting modest total avoided deforestation benefits. We test for two types of slippage: increased deforestation on other property belonging to program recipients and increased deforestation within markets where there are high levels of program participation. We find evidence of both, with substitution impacts reducing program effectiveness in common properties by about 4% on average.* (JEL O13, Q24)

I. INTRODUCTION

Payments for ecosystem services (PES) programs are an important form of incentive-based conservation worldwide (Landell-Mills and Porras 2002; Wunder 2007; Pagiola and Platais 2007; Gutman and Davidson 2007; Pfaff et al. 2007; Engel, Pagiola, and Wunder 2008; Jack, Kousky, and Sims 2008). These schemes, particularly payments for forest conservation in developing countries, are likely to expand dramatically under proposed international agreements to reduce carbon emissions from deforestation and degradation ("REDD" agreements, see IUCN 2009; UNFCCC 2009). However, there is little rigorous empirical evidence about the environmental effectiveness of existing PES programs (Pattanayak, Wunder, and Ferraro 2010).

In the context of reducing deforestation or forest degradation, the goal of PES programs is to induce additional forest conservation ("additionality" or "avoided deforestation") by raising the returns to forested land (see, e.g., Ferraro and Simpson 2002; Bond et al. 2009; Pagiola and Zhang 2010). Although this logic is theoretically sound, there is concern that PES programs may not generate additional environmental benefits. One concern is that programs are not effective at inducing additional forest conservation because they are paying landowners who would have kept land in forest even in the absence of payments (e.g., see de Janvry and Sadoulet 2006; Alix-Garcia, de Janvry, and Sadoulet. 2008). A second possibility is "slippage" (also "leakage" or "negative spillovers"): even if forest conservation programs do induce additional conservation on enrolled lands, these benefits may be undermined by new deforestation in other locations (for previous work on negative environmental spillovers in the context of land conservation see Berck and Bentley 1997; Wu 2000, 2005; Wu, Zilberman, and Babcock 2001; Chomitz 2002; Lichtenberg 2004; Wear and Murray 2004; Murray, McCarl, and Lee 2004; Murray, Sohngen, and Ross 2007; Fraser and Waschik 2005; Roberts and Bucholtz 2005, 2006; Gan and McCarl 2007; Robalino 2007; Plantinga and Richards 2008; Lichtenberg and Smith-Ramirez 2011).

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In this paper, we study program effectiveness and slippage for an early cohort of Mexico's national payments for hydrological services program (*Pago por Servicios Ambientales-Hidrológico*, or PSAH), which pays landowners to maintain forest cover. Between 2003 and 2009, approximately 2.27 million ha of land were entered into Mexico's program of payments for ecosystem services (Shapiro 2010), making it one of the largest PES programs in the world next to the U.S. Conservation Reserve Program, Costa Rica's PSA program, and China's Sloped Land Conversion Program. Mexico's program focuses on the conservation of existing forest, which is likely to be a feature of future programs under international climate agreements (CONAFOR 2009). Under the PSAH program, five-year renewable contracts are signed with both individual and communal landowners.[1] Landowners may enroll a portion of their property and must maintain existing forest cover within that parcel, but can make changes to land use in other parts of their property. Payment rates are fixed and for the 2004 cohort we study were set at 400 pesos/ha (approximately $36/ha) for cloud forest and 300 pesos/ha (approximately $27/ha) for other forest. Payments are made annually, and verification of forest cover through satellite image analysis or ground visits is conducted annually on approximately half of all enrolled properties (McAfee and Shapiro 2010). Areas where deforestation is detected within the enrolled property are removed from the program and payments are reduced proportionally.

We seek to contribute to the existing literature evaluating national PES programs (Muñoz-Piña et al. 2008; Alix-Garcia, de Janvry, and Sadoulet 2005, 2008; Alix-Garcia et al. 2008; Corbera and Brown 2008; Sills et al. 2007, 2008; Robalino and Pfaff 2011a; Arriagada et al. 2008, 2010; Pfaff, Robalino, and Sánchez-Azofeifa 2008; Robalino et al. 2007; Sánchez-Azofeifa et al. 2007) in two ways. First, we use parcel-level data and draw control properties from the applicant pool to construct a plausible counterfactual comparison group. Most previous studies on payments for ecosystem services have not measured impacts in comparison to controls or else use controls that may differ significantly from program recipients (see review by Pattanayak, Wunder, and Ferraro 2010). By using properties from the applicant pool, we ensure that control properties are similar with respect to key unobservable attributes including selection into treatment and the institutional capacity to apply. Rather than use all applicants, we use matching to preprocess the data and select an appropriate control group.[2] Controls are chosen using exact matching on region and tenure type (private property vs. common property) and covariate matching on the enrolled parcel's area, slope, elevation, baseline forest type, prior deforestation rates, municipal population density, degree of marginality, and access to markets. We then estimate impacts using Abadie and Imbens's (2002) bias-adjusted matching estimator and Tobit regression.

We find that the program has had small to moderate impacts on deforestation between 2003 and 2006. Specifically, the estimates indicate that the program decreased the average percentage area cleared by 1.2 percentage points. Given the baseline average percentage area deforested of 2.4% (0.8% per year) among the matched controls, this amounts to a decrease in deforestation rates in enrolled properties of approximately 50% and suggests that PES programs can be effective in changing deforestation behavior. However, the relatively low deforestation rate among matched controls means that the overall avoided deforestation impacts are modest because much of the enrolled area would likely have remained in forest even in the absence of payments.

Second, we model and test for slippage ("leakage") of deforestation to other areas.

[1] Communal lands include *ejidos,* which are federally recognized common property holdings with land tenure and governance rights granted to a set number of heads of households, and *communidades,* which are indigenous lands. Within these areas, land is allocated to both communal and private uses. Land enrolled in the program in the relevant years is generally communal land.

[2] Preprocessing follows Ho et al. (2007); other analyses of conservation program effectiveness using matching include those by Pfaff, Robalino, and Sánchez-Azofeifa (2008), Andam et al. (2008, 2010), Costello, Gaines, and Lynham (2008), and Ferraro, McIntosh, and Ospina (2007).

Following a debate about slippage effects of the U.S. Conservation Reserve Program (Wu 2000, 2005; Roberts and Bucholtz 2005, 2006), we discuss two possible types of slippage: substitution effects and output price effects. In the context of forest-conservation payments, a substitution slippage effect occurs when a landowner who removes one parcel of land from production (enrolling it in the program) shifts the planned production to another parcel within his landholdings. Credit constraints are discussed as one of a variety of common market imperfections in developing countries that might lead to such substitution slippage. Our analysis uses a simple household land allocation framework to illustrate that substitution slippage could occur as a result of households being credit-constrained. We test for substitution slippage by comparing deforestation rates in the nonenrolled portions of enrolled properties to those of the matched control properties—a common sense approach but one that has not been possible in previous studies given data constraints. We find evidence for substitution slippage, with the sign and magnitude of the effects varying by the degree of marginality. In poor *ejidos,* we find increased deforestation, lending support to the credit constraints hypothesis and raising concerns that slippage could erase program impacts. In wealthier *ejidos,* substitution slippage is actually negative, complementing the program's avoided deforestation impacts. On average, we find that the substitution slippage effect reduces avoided deforestation within common properties from 1.22 to 1.17 percentage points, or about 4% of the program impact.

An output price slippage effect occurs if the removal of multiple parcels of land from production or the introduction of payments alters market prices, and these changes in turn induce additional deforestation. Whether or not these changes will be spatially close to enrolled lands depends on the size of the relevant markets: we expect to see effects where there is sufficient overall enrollment to move prices and where markets are localized enough to concentrate those price effects. We test for output price slippage by comparing deforestation on un-enrolled land in areas of high and low total enrollment. We use the connectivity of transportation infrastructure (surrounding road density) to proxy for the degree of market integration. We find evidence consistent with output price slippage from the program, although the potential endogeneity of market-level enrollment limits the conclusiveness of this test.

II. EVALUATING THE ENVIRONMENTAL EFFECTIVENESS OF PES: PREVIOUS LITERATURE

Primary Impacts

Previous literature addressing the environmental effectiveness of national PES programs indicates small or modest benefits of these programs. Much of this literature focuses on the U.S. Conservation Reserve Program, one of the earliest national-scale programs that directly incentivized individual landowners for conservation actions (Sullivan et al. 2004; Feng et al. 2005; Lubowski et al. 2006). Using a structural model and county-level data, Goodwin and Smith (2003) find significant reductions in soil erosion as a result of the U.S. Conservation Reserve Program. Previous studies on China's Sloped Land Conversion Program (Xu et al. 2004, 2005; Uchida, Xu, and Rozelle 2005) also indicate that programs in China have achieved significant soil conservation benefits on the basis of modeling using household surveys on participant behavior and targeting criteria.

Few previous studies directly evaluate effectiveness using a comparison group of properties to estimate what would have happened in the absence of the program (Pattanayak, Wunder, and Ferraro 2010). To date, the only evaluations of national PES programs using direct control group comparisons that we are aware of are from Costa Rica (Sills et al. 2007, 2008; Arriagada et al. 2008; Pfaff, Robalino, and Sánchez-Azofeifa 2008; Robalino et al. 2007; Sánchez-Azofeifa et al. 2007). These studies generally find little or no impact, possibly because the overall rate of deforestation slowed in Costa Rica around the same time that the program was introduced. An exception is new work by Arriagada et al. (2010), which indicates significant avoided deforestation impacts of Costa Rica's program in the

Sarapiquí region, where deforestation rates were generally higher. Mexico, in contrast, continues to experience significant rates of deforestation (FAO 2005). The small number of studies that discuss Mexico's national PES program (Muñoz-Piña et al. 2008; Alix-Garcia, de Janvry, and Sadoulet. 2005, 2008; Alix-Garcia et al. 2008; Corbera and Brown 2008; McAfee and Shapiro 2010; Shapiro 2010) describe important debates about and changes in targeting strategy but do not directly estimate program effectiveness by measuring deforestation and using matched controls. Clearly, additional research is needed that measures impacts relative to a realistic counterfactual. Other ongoing work in Mexico in this vein includes a recent paper by Honey-Roses, Baylis, and Ramirez (2011) that uses spatial matching to evaluate the effectiveness of payments coupled with protected area designation near a monarch butterfly reserve. The paper finds that these conservation measures resulted in additional protection of 3% to 16% of high-quality forest habitat and 0% to 2.5% of lower-quality forest.

Slippage Effects

The possibility for secondary effects poses a serious problem for efforts to conserve forests or other land (Berck and Bentley 1997; Wu 2000; Wu, Zilberman, and Babcock 2001; Chomitz 2002; Murray, McCarl, and Lee 2004; Murray, Sohngen, and Ross 2007; Wear and Murray 2004; Fraser and Waschik 2005; Gan and McCarl 2007; Robalino 2007; Plantinga and Richards 2008). Previous work on the environmental benefits of the U.S. Conservation Reserve Program has indicated that these may be undermined by slippage of production to other areas (see debate between Wu 2000, 2005; Roberts and Bucholtz 2005, 2006), increased production on environmentally vulnerable land (Lichtenberg and Smith-Ramirez 2011), or increased environmental damage through intensified use of pesticides or fertilizers (Lichtenberg 2004). More optimistically, benefits may be enhanced if programs cause persistent changes in producer behavior (Roberts and Lubowski 2007).

Previous empirical literature on slippage is limited by data constraints and the context in which it has been studied. As noted by Roberts and Bucholtz (2005), substitution slippage effects are unlikely to occur in developed country settings because markets should function well to reallocate resources, making the returns to each parcel within a landholding independent of production choices on others. However, as we illustrate in the next section, in a developing country setting, substitution may be an important slippage channel because of rigidities in land, credit, or labor markets. Indeed, Uchida, Rozelle, and Xu (2009) find evidence of labor market effects in China due to the national PES program (Sloped Land Conversion Program). Considering deforestation more generally, Zwane (2007) suggests that increases in income in Peru may relax credit constraints, leading to increases in deforestation. In addition, Robalino and Pfaff (2012) find evidence for spatial spillovers in deforestation decisions in Costa Rica as a result of strategic interactions unlikely to exist in the context of well-developed markets. Output price slippage should occur in both developed and developing countries but will be observable only where markets are small enough to localize price changes. A developing country case such as Mexico, where high transportation costs due to poor infrastructure create localized markets in some areas, therefore provides an interesting setting to test for output price effects.

III. ECONOMIC FRAMEWORK

In order to illustrate how a conservation payments scheme may lead to slippage effects, we describe a simple model of household production and land allocation.[3] In the majority of the forest-holding communities in Mexico, deforestation is due to the incremental loss of forest from expansion of agricultural or pastoral activities (Alix-Garcia, de Janvry, and Sadoulet 2005; Alix-Garcia 2007); our model therefore focuses on the relative returns to these uses.

In our framework, households have endowments of land and capital. We assume that the

[3] The setup of an input purchase is similar to that of Guirkinger and Boucher (2008), which models how agricultural productivity depends on credit constraints.

total area of land that is managed by a household is fixed at T.[4] Households seek to maximize consumption, which is a function of the total value of their production. Households choose how much land to allocate to two types of production; the amount of land devoted to agriculture is denoted as a and to forest f, so that: $T = a + f$. Agricultural production is a decreasing returns to scale function of land in agriculture, a, and the quantity of a variable input, n (representing seeds, fertilizer, etc.), which households must purchase[5]:

$$y^a = y^a(a, n). \tag{1}$$

In order to purchase this input, households can either use their endowments (existing liquid wealth) or borrow. Forest production is a constant returns to scale function of the amount of land in forest:

$$y^f = y^f(f). \tag{2}$$

Households obtain utility from consuming these products in amounts c^a and c^f. They might also choose to consume some other tradable product: c^z. A well-behaved household utility function is defined over these three products: $u(c^a, c^f, c^z)$, with $u_i > 0$, $u_{ii} < 0$, where subscripts indicate derivatives with respect to the ith argument. Households may also sell or buy these products in nearby markets, for prices p^a or p^f or $p^z = 1$, respectively, for the agricultural good, forest good, and other tradable product.[6] We assume competitive markets, so individual producers take these prices as exogenous.

[4] The assumption of a land constraint is reasonable for areas where most land is formally or informally claimed, while a labor constraint might be most important in frontier situations (e.g., Pagiola and Holden 2001). Most land in Mexico is owned by communities or private property owners and does not constitute frontier land, so we focus on the land constraint.

[5] Assume that if either argument of the production function is equal to zero, then production is equal to zero.

[6] The relative prices of key agriculture and timber goods may vary significantly between regions and ecosystem types depending on the goods in question; we keep the model simple but allow for this heterogeneity in the empirical analysis.

Household Allocation of Land Subject to Credit Constraints

Households start with an endowment of liquid wealth, denoted K. They can spend some or all of this endowment on the variable input, n. They also have the option of borrowing a sum B from the bank to purchase more of the variable input and thus increase production; we also assume that they can consume leftover borrowed funds or capital. The cost of borrowing is r. At the end of each period, the household must pay back $(1 + r)B$ when agricultural products are harvested.

Borrowing has an upper bound because households have a limited amount of collateral to back their loans. We assume that this collateral is represented by the total value of their land (they cannot borrow against their cash endowment). In Mexico, the size of land parcels, both inside and outside of common properties, is strongly correlated with wealth (Finan, de Janvry, and Sadoulet 2005), and historically in the literature, land is thought of as a constraint to accessing credit (Eswaran and Kotwal 1986). We think that it is therefore reasonable to consider land-related wealth to be the largest constraint to borrowing, and describe the borrowing constraint as

$$B \le p^l T, \tag{3}$$

where p^l is the per unit value of land. Land within *ejidos* and *communidades* cannot easily be sold to outsiders, so access to credit for these landowners may in reality be more restricted. We test for implications consistent with this assumption in the empirical section.

At the start of the period, households can purchase productive inputs (n). Their purchase is limited by the amount of working capital ("cash-in-advance") that they have:

$$p^n n \le K + B. \tag{4}$$

Consumption is limited by total production plus endowments, which gives the constraint

$$p^a c^a + p^f c^f + c^z \le p^a y^a - p^n n + p^f y^f + K - rB. \tag{5}$$

Assuming that utility depends only upon consumption, households seek to maximize consumption subject to the working capital and borrowing constraint. The problem is to maximize utility subject to [1]–[4]. This is a standard agricultural household model with a credit constraint. By definition, the land constraint always binds. However, the borrowing and capital constraints may or may not. We discuss the implication of these conditions for households that have at least some land in agriculture.[7] Depending on their cash endowments, there are three types of households: those who do not choose to borrow at all, those who borrow but are unconstrained in the amount, and those who borrow and are constrained by credit limitations.

Nonborrowing households will chose only consumption of each good, the allocation of land, and the amount of the variable input. For these households, the optimal choice of land use is the one that equates the marginal product of agricultural land with the marginal product of forest land, where optimal input purchase is simply a function of price and production parameters (the standard result). Unconstrained borrowers do borrow in order to purchase inputs but are not limited by their endowments, therefore their optimal purchase of the input is a function of prices and the interest rate. Because the interest rate adds to the costs of production, these households will have less land in agriculture than those who do not borrow at all. Finally, constrained borrowers are limited by their endowments and will purchase fewer inputs. Comparing across the three types of households, and assuming the same land endowment, T, but varying cash endowments, K, we expect that less land will be put into agriculture relative to forestry as cash endowments decrease[8]: $a^{\mathrm{nb}} > a^{\mathrm{ub}} > a^{\mathrm{cb}}$.

Introducing PES Payments

We introduce payments for forest services into this setting in a stylized manner, but one that captures the key idea that PES changes the relative returns to land use and relaxes credit constraints. Each household can put land in the program, denoted as S, so that $T = a + f + S$, and for each unit of land in the program, the household earns a per unit payment, p^s. This payment, $p^s S$, comes to the household as cash that the household can then use to buy additional inputs or can consume. Once land is enrolled in the program, it cannot be used for forest production.[9] This is simply a modification of the land constraint, and the optimization problem proceeds as before.

Among nonborrowing households, if $p^s = p^f y_1^f$, then the optimal allocation of land to agriculture and choice of n is the same as in the no-policy case. When $p^s > p^f y_1^f$, nonborrowing households will choose to enroll some land previously in agriculture. This reflects the intended effect of the program, which is to induce additional forest land use by raising its returns.[10] For unconstrained borrowing households, the program has an additional effect of reducing the amount of interest that they need to pay on borrowed money, so these households may even require less compensation in order to be induced to enroll.[11] However, it is also possible that this decrease in the cost of agricultural production could cause them to increase their land in agriculture.

For constrained borrowers, the program changes both the price ratio of forestry relative to agriculture and the cash-in-advance constraint. Higher payments will tend to de-

[7] Corner solutions may exist in this model. It may, for example, be optimal to have all land in forest. In this case the PES program would have no impact on land allocation, and the payment would simply be a transfer.

[8] Where nb = nonborrowing households; ub = unconstrained borrowers; cb = constrained borrowers.

[9] This matches the reality of the contracts for the PSAH, which prohibit extractive activities or grazing in the enrolled land. The assumption that households can put as much land as they want in the program is a simplification, because in reality they can only enroll land that was already in forest use. There is also a maximum limit on the number of hectares that can be enrolled, but it effectively binds only for very large properties.

[10] In reality, forest takes time to regenerate, so the goal of the program is to induce additional land to *remain* in forest. We could think of this as corresponding to a dynamic framework where the price of agriculture relative to forest is increasing over time, so more land is being put in agriculture each period overall.

[11] An interesting implication that unfortunately we cannot test without household data but certainly deserves investigation. See Alix-Garcia, Shapiro, and Sims (2010) for the full derivation of this result. Note also that it is possible that for households who were just on the margin of borrowing, the cash availability from the program could push them from being unconstrained borrowers to being nonborrowers, which could increase land in agriculture.

crease the amount of land in agriculture but will also increase available cash. The net impact on land use will depend on how these terms grow relative to each other as p^s increases. As the difference between program payments and returns to forestry increases, the increase in relative returns to forested land will dominate and land in agriculture will decrease. However, if the program just barely compensates for the opportunity cost of forest production, it will lead to an increase in the amount of land in agriculture for credit-constrained households.

In summary, this framework illustrates that individual households operating under no constraints will always reduce their land in agriculture (increase their land in forest) if the relative prices are high enough to induce enrollment in the PES program. Households that are unconstrained borrowers may increase their land in agriculture if the decrease in their interest payment costs is large enough. Finally, constrained households are likely to increase their agricultural production and reduce forest area. Therefore, we expect to observe substitution slippage where households require borrowing in order to purchase agricultural inputs and/or are credit-constrained.

Output Price Slippage Effects

Potential slippage through price effects requires consideration of the aggregate market effects of the program. Output price effects could happen through either supply or demand channels. On the supply side, within our framework, the total number of households is N, and they fall into the three categories as above, depending on their endowments of K. Then total land used for agricultural production within a given geographic area is a function of the number of households in each category. This implies that the supply of agricultural goods, in addition to depending upon land and prices, also depends upon the number of nonborrowing, unconstrained, and constrained borrowing households. The standard price spillover effect is assumed to occur from a reduction in supply due to an increase in the price of agriculture as additional land is put in forest use.[12] Our model implies that it would be possible to see either an increase or a decrease in the supply of agriculture, depending on how many households fall into each category of borrowers above.[13] Therefore supply-side output price slippage effects in this case could be either positive or negative. The magnitude of these effects should depend on the elasticity of the corresponding demand (e.g., as demonstrated by Chomitz 2002).

Previous work has focused mainly on supply-side effects. In a developing country context where baseline incomes are low, program payments may also significantly shift demand. Increases in income as a result of the program may increase consumption of land-intensive goods and therefore demand for resource intensive products.[14] Our framework implies this, since all demand functions, c^i, must depend upon income and prices. Total market demand is the sum of all of the individual demand functions, so increases in household income as a result of program enrollment could lead to increases in demand for consumption goods. If these goods are agricultural, this will raise the marginal product of agriculture,[15] and households will tend to shift their allocation of land toward agriculture.

Since output price effects are mediated through markets, they are likely to be stronger

[12] Fraser and Waschik (2005) use a computable general equilibrium model to simulate policies for agricultural land retirement in Australia and find that this type of slippage may significantly reduce conservation benefits. A recent working paper by Dyer (2011) uses an agent-based general equilibrium model to analyze how leakage from a PES program may differentially affect small and large landholders.

[13] We focus on changes in demand for the agricultural good, but if enough households enroll forest in the program rather than use it for forest production, this would increase the price of forested goods. In general equilibrium and with an unlimited program budget, we would expect that the price of forest goods would rise until the marginal returns to forest land were equal to the price of the payments with some land in each use.

[14] See Alix-Garcia et al. (2011) for an example of this effect in the context of poverty alleviation programs. However, other forces may counter this trend. Wealth may also increase demand for forest products (Foster and Rosenzweig 2003; Pfaff, Chaudhuri, and Nye 2004), facilitate the development of stronger institutional mechanisms for conservation, or increase investment in greener technologies.

[15] Evidence from evaluations of Progresa suggests that this is the case (Gertler, Martinez, and Rubio-Codina 2006).

in areas that have high transportation costs and thus localized markets. Within the context of our model, an isolated market is one in which few households participate. A lower population will decrease the elasticities of supply and demand and concentrate the impact, possibly leading to a large local price effect and hence a large local program spillover.

IV. PROGRAM AND DEFORESTATION DATA

We evaluate direct impacts and slippage for an early cohort (2004) of Mexico's program. Evaluation of the PSAH presents the standard identification problem: one does not know how recipients would have behaved had they not received payments. In order to construct a plausible counterfactual for the 2004 cohort, we rely on a comparison to controls drawn from the applicant pool. We combine two types of nonrecipients as controls: applicants from 2004 that were rejected on the basis of administrative or geographic details, and recipients of payments in a future (2006) round of the program. Figure 1 shows the location of the participants and controls, indicating that controls and recipients have similar spatial distribution throughout the country. We use matching to select controls that are closest in terms of observable characteristics and geographic region. We then estimate impacts using Abadie and Imbens's (2002) bias-adjusted matching estimator or regression to correct for remaining observable characteristics.

Control Groups and Selection

A key advantage of using controls drawn from the applicant pool is that the owners of these parcels have demonstrated their desire to enroll in the program, revealing that their opportunity costs are sufficiently low to motivate application. A main criticism of previous research is possible selection bias due to the lower opportunity costs of enrolled parcels compared to controls. However, multiple characteristics may still differ between the groups and, if they are correlated with selection into the program and deforestation, could potentially bias estimates. In our case, these differences could be driven by the reasons for rejection of the 2004 cohort or differences in application criteria for the 2006 cohort.

Considering the rejected applicants, we note that there are three main reasons for rejection in 2004: being located outside of the "eligible zones," failing to meet the minimum forest cover requirement (80%), or having incomplete paperwork. More than 60% of the applicants in our sample were rejected for the first two reasons, that is, selection on observables. The eligible zones are determined by geographic characteristics, specifically whether properties were located upstream from significant population centers in a watershed with a high to medium degree of water scarcity (Shapiro 2010). To account for this selection, we match on or control for appropriate geographic characteristics as described below. The third reason for rejection is potentially more problematic: missing paperwork could reflect lower institutional capacity that is not directly observable and might be correlated with deforestation. To minimize this problem we limit our analysis to applicants that have sent in geo-referenced property boundaries and have already passed through a first round of screening, ensuring a reasonable level of capacity. We also match on municipal poverty levels and tenure type, which may correlate with institutional capacity.

Considering the future enrollees, we argue that they constitute a reasonable control group because 2004 was an early year of the program and because we can control for the few differences in the requirements for eligibility between 2004 and 2006.[16] Anecdotal and case study evidence (Shapiro 2010) suggests that the main reason for delaying application was that this new program had not been well publicized in 2004 and 2005 and so many landowners simply did not know about the

[16] There are three potentially substantial differences, which we take into account by matching or including appropriate control variables. The 2006 cohort had a smaller minimum area for enrollment (20 ha vs. 50 ha) and a smaller maximum (3,000 ha vs. 4,000 ha); we match on size of enrolled property. The 2006 cohort specifically prioritized indigenous and poor communities; we match on degree of marginality. The 2006 cohort required 50% rather than 80% baseline forest cover; we match on baseline proportion of enrolled area in two types of forest.

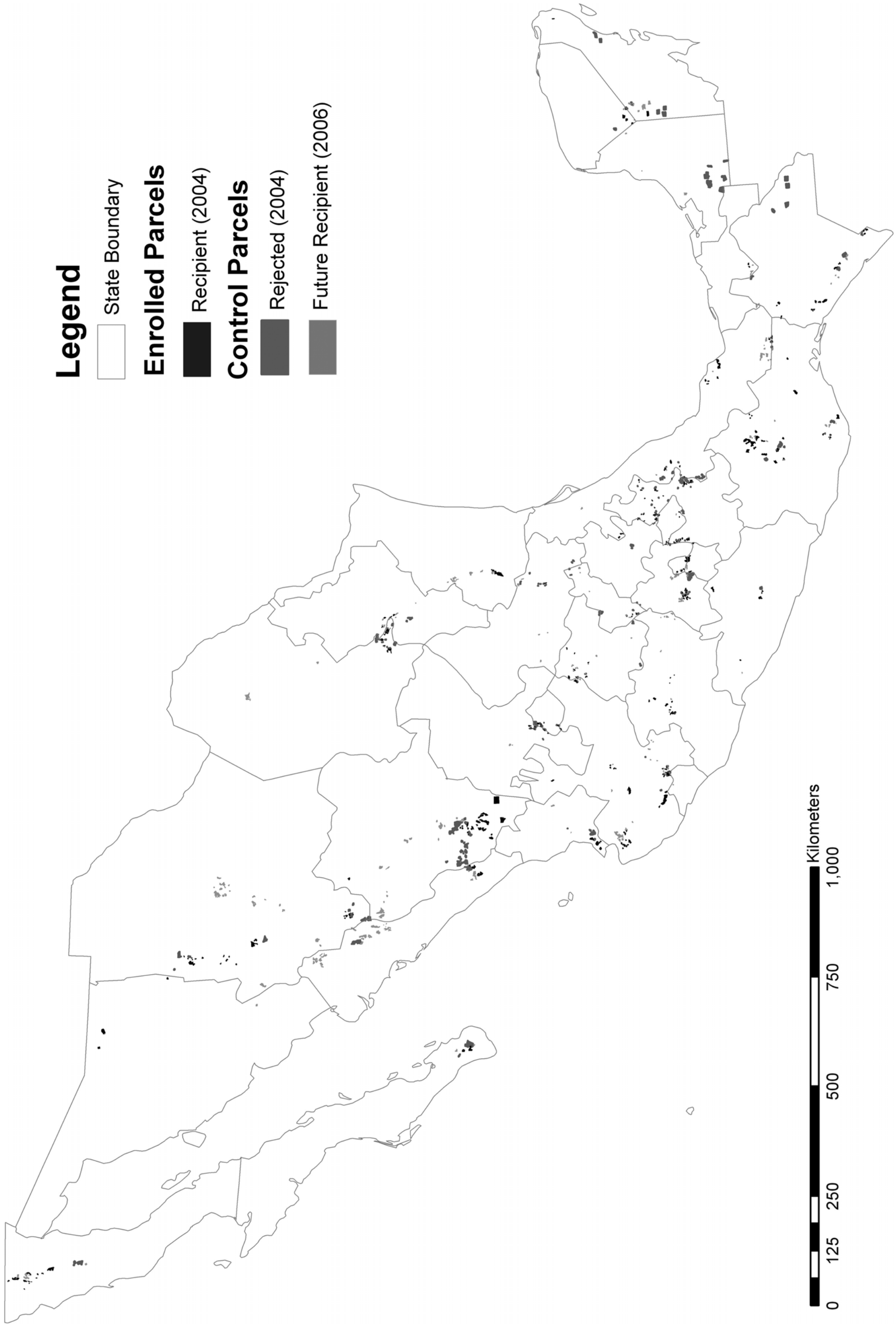

FIGURE 1
Location of Recipients and Control Properties

Source: Authors' calculations using program information from CONAFOR.

program.[17] If they did know about the program and planned to enroll, that would most likely bias our analysis toward finding no effect. In order to enroll in the program, households must have existing forest cover, so households who plan to enroll in the future would be less likely to deforest if they anticipate future enrollment.

Deforestation Measures

The deforestation data we use comes from Mexico's annual Monitoreo Forestal de Mexico (Mexican Forest Monitoring) program. To measure direct impacts, we calculate deforestation within the boundaries of the parcels enrolled in the PSAH program or submitted to the PSAH program between 2000 and 2003 (baseline deforestation) and between 2003 and 2006 (after program implementation). In order to measure localized spillovers, we calculate deforestation in the same periods for other areas within the same properties but outside the enrolled parcels or in surrounding buffer areas, as explained below in the section on slippage tests.

The classification of deforestation in the Monitoreo is based on changes in normalized difference vegetation index (NDVI) values across years.[18] Average NDVI for each year is measured using composites of multiple MODIS satellite images taken in the dry season. MODIS images are temporally dense, with repeated pictures every 2 weeks, but spatially coarse, with 250 m pixels. Deforestation is indicated by a decrease in average NDVI between years outside of the range of the normal seasonal vegetative cycles ("phenology"). The NDVI ranges and phenology for each ecosystem and vegetation type in Mexico were calibrated by CONAFOR using information from on-the-ground field data from the National Forest Inventory[19] and by matching the coarse resolution MODIS images with finer resolution images from the Landsat (30 m pixels) and SPOT (10 m pixels) satellites. As with all deforestation indicators based on NDVI, the data may contain measurement error due to weather shocks.[20] The weather cannot be influenced by participants but could be correlated with regional enrollment levels. We therefore include regional matching or controls in all analyses, as well as controls for slope, elevation, and type of forest. Since the mandate of the Monitoreo Forestal was to detect deforestation and not afforestation, our dependent variable is censored. We correct for this potential censoring problem by using Tobit regressions throughout the data analysis.[21]

The substantial advantage of this dataset is that it provides wall-to-wall coverage of the entire country over the appropriate time period. Previous analyses of PES programs in Mexico have not been able to directly measure deforestation on enrolled properties across the country, so the use of this data represents a significant improvement over existing research. A limitation of the data, however, is its resolution: the 250 m pixels correspond to approximately 6 ha of area. This does not, however, imply that areas of deforestation smaller than 6 ha cannot be detected. The NDVI is a continuous measure, so any clearing or degradation within the pixel area would decrease NDVI. Deforesta-

[17] If this assumption is correct, there should be no significant differences in deforestation behavior between the rejected applicants and future enrollees. To test this we used future enrollees as a "pseudo-treatment" group and checked whether the estimated causal effect of the program was in fact zero (following Heckman, Ichimura, and Todd 1997). We used both regression and matching estimators to assess the differences in impact. There were no significant effects in the overall treatment effect or on the effect on the probability of treatment, although the deforestation among deforesters in the 2006 enrollees was slightly higher than among those rejected in 2004. There was no significant effect of pseudo-treatment on the regression estimates of the best 90% matches.

[18] Details of the methodology are available online (CONAFOR 2011) and in a working paper and presentation constructed for the Food and Agriculture Organization (Meneses Tovar 2009a, 2009b).

[19] Sampling teams take photos and detailed measurements of tree sizes at each point; more than 22,000 geo-referenced sampling plots were established between 2004 and 2007 (de Jong, Gutiérrez, and Alanís de la Rosa 2008). Phenology was calibrated for nine vegetation types (CONAFOR 2011).

[20] NDVI is based on the "greenness" of vegetation and increases with high rainfall. The Monitoreo uses data from dry seasons in Mexico in order to limit this problem.

[21] Practically speaking, afforestation is unlikely to be a major concern, because Mexico was not a net afforester during this period; FAO's 2005 Global Forest Resources Assessment places Mexico in 13th place in the world in terms of net forest loss over the period 2000–2005.

tion of 2 ha would likely be detected by CONAFOR's method, and even smaller areas could be picked up if exposed soil remained after clearing.[22] The main limitation, therefore, is not that we cannot detect small areas of clearing, but that we do not know exactly where the deforestation is located within each 6 ha pixel. Given that the minimum land area required to enroll in the program in 2004 was 50 ha and the average area enrolled in our dataset is 700 ha, we feel that the scale of the error relative to the scale of the properties is appropriate.

V. DIRECT IMPACTS: ESTIMATION STRATEGY AND RESULTS

To estimate direct impacts, we first preprocess the data using matching and then employ matching or Tobit estimators. As shown in Table 1, a comparison between all recipient parcels from the 2004 PSAH cohort and all potential control parcels indicates moderate differences in characteristics across these two groups. Recipients are found in areas of somewhat higher slope and elevation and have lower proportions of semideciduous forest and more selva (tropical wet forest). They also tend to be in places with somewhat higher levels of road density, population density, and poverty. The overall average percentage area deforested for the nonrecipients is 2.4% (or 0.8% per year) and for the recipients is 1.4% (or 0.46% per year).

Preprocessing Using Matching

Following Ho et al. (2007), we use matching to "preprocess" the data in order to select the best controls from among the applicant pool. We match parcels based upon area enrolled, slope, elevation, proportion of the enrolled area with semideciduous forest, proportion of enrolled area with selva (tropical wet forest), baseline deforestation between 2000 and 2003, road density, municipal population density, and municipal poverty. We also require exact matching on region (see Table 2) and tenure type (communal vs. private property). We use only one match (the nearest neighbor), and matching is conducted with replacement.[23]

We restrict the estimation sample to enrolled parcels that have reasonably good matches, as defined by taking the best 90% of matches.[24] The bottom half of Table 1 shows the balance of observable characteristics for the best 90% matches, and Table 2 shows balance within each region. The balance across groups with respect to observable characteristics is reasonable overall and within region.[25] On average, there are no significant differences in control characteristics between recipient and nonrecipient groups in the matched subsample, although in Region 3 there remains a difference in average road density between the two groups.

Matching Estimator

The matching estimator is based on the concept of "potential outcomes." In particular, for each property $i = 1, \ldots, N$, we suppose that we can observe deforestation if the landholder does not receive the program ($Y_i(0)$) and if the landholder does receive the program ($Y_i(1)$). Only one of these two events is actually realized: W_i indicates the actual state of property i ($W_i = 1$ if the property participates in the program and zero otherwise). We are interested in the difference between these two outcomes: $\tau = E[Y_i(1) - Y_i(0)]$; specifically the average treatment effect among the treated. We use one match (the nearest neighbor) in calculating the effect

[22] Electronic interview with V. Radeloff, 2011, Department of Forestry and Wildlife Ecology, University of Wisconsin–Madison.

[23] There are several metrics that can be applied to establish the nearest neighbor within the sample; we use both the Mahalonobis metric and the inverse sample standard errors. The estimator calculates the average difference between the predicted outcomes for properties that are in and out of the program.

[24] This is similar to restricting a sample according to propensity score values, where the standard in the literature is to eliminate the 10% most and least likely to be receiving a program (Crump et al. 2009). We also restricted the sample to the best 80% matches, with no significant differences in results.

[25] Balance is also good within tenure type; there are no significant differences between covariates except that area enrolled is somewhat larger for the recipient group within private properties (t-statistic = 1.91).

TABLE 1
Summary Statistics for Recipients and Nonrecipients

Variable	Recipients[a]	Nonrecipients[a]	Test for Difference[b]
Full Sample			
Enrolled area (km^2)	7.04	9.35	2.26
ln(Average slope)	2.44	2.33	2.12
Average elevation (km)	2.09	1.87	3.40
Proportion enrolled area semideciduous forest	0.20	0.32	4.03
Proportion enrolled area selva	0.33	0.26	2.39
ln(Road density)	6.64	6.48	3.36
ln(Population density)	3.52	3.16	3.55
Municipal marginality index	−0.14	−0.26	1.79
Proportion with deforestation	0.22	0.23	0.17
Percent deforestation	1.41	2.36	1.99
Percent deforestation $\mid$ deforestation >0	6.30	10.28	2.27
Observations	352	462	
Best 90% of Matches[c]			
Enrolled area (km^2)	6.70	6.00	0.86
ln(Average slope)	2.48	2.53	1.12
Average elevation (km)	2.17	2.21	0.71
Proportion enrolled area semideciduous forest	0.19	0.17	0.73
Proportion enrolled area selva	0.30	0.31	0.09
ln(Road density)	6.67	6.73	1.32
ln(Population density)	3.47	3.58	1.07
Municipal marginality index	−0.14	−0.18	0.50
Proportion with deforestation	0.21	0.30	2.67
Percent deforestation	1.08	2.37	2.97
Percent deforestation $\mid$ deforestation >0	5.20	7.89	1.84
Observations	317	316	

Note: ln(Average slope) is the log of average slope of the enrolled area in degrees. ln(Road density) is the log of the length of roads in kilometers within a buffer of radius 50 km around each property. Proportion enrolled area in semideciduous forest and selva (tropical wet forest) are calculated from INEGI's Series III land use layer 2002. ln(Population density) is the log of municipal population density (people per square kilometer) from the 2000 census. Municipal marginality index is from CONAPO (2000), and ranges from −2 to 2, with larger numbers indicating greater poverty. Proportion with deforestation is the fraction of parcels with any deforestation within the enrolled area (2003–2006); Percent deforestation is the percent of parcel area that is deforested (2003–2006); Percent deforestation $\mid$ deforestation >0 is the percent of parcel area that is deforested among those with any deforestation (2003–2006). Deforestation data is from CONAFOR's Monitoreo Forestal de Mexico (CONAFOR 2011).

[a] Recipients refers to parcels enrolled in the 2004 PSAH cohort. Nonrecipients are parcels of rejected applicants from 2004 PSAH or future recipients of the program (2006 PSAH).

[b] Test for difference gives the *t*-statistic for difference in means.

[c] Best 90% matches calculated using the Mahalonobis metric. Matches are based on exact matching on region and tenure type (private property vs. common property) and the following covariates: ln(Area enrolled), ln(Slope), Elevation, Proportion enrolled area semideciduous forest, Proportion enrolled area selva, ln(Road density), ln(Municipal population density), Municipal marginality index, and Percent baseline deforestation (2000–2003).

$$\hat{\tau} = \frac{1}{N_1} \sum_{i:W_i=1}^{N} (Y_i - \hat{Y}_i(0)),$$

where

$$\hat{Y}_i(0) = Y_j \text{ if } W_i = 1,$$

with j indicating the observation in the control group that is closest to the treated observation i.

Because matches in finite samples can never be exact, we use the adjustment proposed by Abadie and Imbens (2002) and described by Abadie et al. (2004). The adjustment begins by estimating deforestation only for the control group using a simple ordinary least squares regression. The "missing outcome" is then estimated by taking the deforestation level of the closest match plus the difference between predicted deforestation for

TABLE 2
Summary Statistics for Matched Sample by Region (Best 90% of Matches)

Variable	Recipients	Nonrecipients	Test for Difference
Region 1: Northwest (Baja California N and S, Chihuahua, Coahuila, Durango, Sinaloa, Sonora)			
Enrolled area (km^2)	10.58	9.89	0.26
Proportion common property	0.49	0.48	0.08
ln(Average slope)	2.54	2.50	0.40
Average elevation (km)	2.34	2.32	0.27
Proportion enrolled area semideciduous forest	0.15	0.12	0.57
Proportion enrolled area selva	0.30	0.26	0.48
ln(Road density)	6.01	6.10	1.22
ln(Population density)	2.34	2.47	0.72
Municipal marginality index	−0.54	−0.57	0.14
Observations	68	67	
Region 2: Northeast (Aguascalientes, Guanajuato, Hidalgo, Nayarit, N. Leon, Queretaro, San Luis Potosi, Tamaulipas, Zacatecas)			
Enrolled area (km^2)	3.99	4.39	0.34
Proportion common property	0.42	0.42	0.00
ln(Average slope)	2.39	2.53	1.60
Average elevation (km)	2.24	2.24	0.01
Proportion enrolled area semideciduous forest	0.68	0.66	0.23
Proportion enrolled area selva	0.09	0.11	0.40
ln(Road density)	6.71	6.72	0.13
ln(Population density)	3.21	3.28	0.35
Municipal marginality index	−0.52	−0.46	0.50
Observations	48	48	
Region 3: Central (Colima, DF, Jalisco, Michoacan, Morelos, Mexico, Puebla, Tlaxcala)			
Enrolled area (km^2)	4.60	4.08	0.44
Proportion common property	0.76	0.76	0.00
ln(Average slope)	2.42	2.44	0.42
Average elevation (km)	2.50	2.61	1.52
Proportion enrolled area semideciduous forest	0.09	0.07	0.65
Proportion enrolled area selva	0.29	0.28	0.07
ln(Road density)	7.11	7.23	2.04
ln(Population density)	4.20	4.37	1.07
Municipal marginality index	−0.27	−0.32	0.63
Observations	121	121	
Region 4: South (Campecha, Chiapas, Guerrero, Oaxaca, Quintana Roo, Tabasco, Veracruz, Yucatan)			
Enrolled area (km^2)	8.20	6.61	1.37
Proportion common property	0.84	0.84	0.00
ln(Average slope)	2.56	2.68	0.97
Average elevation (km)	1.48	1.49	0.09
Proportion enrolled area semideciduous forest	0.09	0.08	0.43
Proportion enrolled area selva	0.46	0.49	0.44
ln(Road density)	6.54	6.52	0.24
ln(Population density)	3.50	3.51	0.11
Municipal marginality index	0.61	0.53	0.76
Observations	80	80	

Note: Variable definitions and matching strategy are equivalent to those in Table 1.

TABLE 3
Estimates of Program Impact on Deforestation: Matching Estimator (ATT)

	Mahalonobis Metric			Inverse Sample Standard Errors		
Dependent Variable	% Deforested (1)	Deforest (0/1) (2)	% Deforested | Deforest > 0 (3)	% Deforested (4)	Deforest (0/1) (5)	% Deforested | Deforest > 0 (6)
Full Sample						
Treatment effect	− 1.03*	− 0.09**	− 6.72**	− 1.03*	− 0.08*	− 6.87***
	(0.60)	(0.04)	(3.37)	(0.58)	(0.04)	(2.50)
Observations	814	814	185	814	81	185
Best 90% Matches						
Treatment effect	− 1.10***	− 0.10***	− 4.44***	− 1.57***	− 0.11***	− 4.62***
	(0.35)	(0.03)	(1.60)	(0.44)	(0.03)	(1.77)
Observations	633	633	161	633	633	161

Note: Standard errors in parentheses. Estimations from differences between nearest neighbors adjusted for remaining differences in observables (following Abadie and Imbens 2002). The dependent variables are the percent of enrolled property deforested (2003–2006); whether or not a property had any deforestation (2003–2006), and the percent of deforestation conditional on having positive deforestation (2003–2006). Columns (1)–(3) use matches based on the Mahalonobis metric, and Columns (4)–(6) use matches based on inverse sample standard errors. Matches are based on exact matching on region and tenure type (private property vs. common property) and the following covariates: ln(Area enrolled), ln(Slope), Elevation, Proportion enrolled area semideciduous forest, Proportion enrolled area selva, ln(Road density), ln(Municipal population density), Municipal marginality index, and Percent baseline deforestation (2000–2003).
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

the treated match and its best (closest) match, where the prediction is based on the coefficients estimated using the control group in the first step.

Regression Estimation

In order to understand how program impacts vary with respect to observable characteristics, we also estimate effects using regression on the best 90% matches. We postulate a relationship between deforestation and program participation as follows:

$$d_i = \propto + \tau W_i + \beta' X + u_i,$$

where the dependent variable is the percentage of total area deforested in the enrolled parcel between 2003 and 2006. This is a function of the indicator for receiving payments (W) plus geographic and socioeconomic control variables (X) that might be correlated with both deforestation and the probability of enrollment.[26] Because the dependent variable is censored, the equation is estimated using a Tobit.[27]

Results: Program Impact on Enrolled Land

Table 3 shows the estimated program impacts applying the matching estimator with bias adjustment. The upper panel of the table illustrates estimates based on the full sample, and the lower panel shows the estimates for a sample of the data restricted to the 90% best matches. Columns (1)–(3) use the Mahalonobis metric to obtain the matches, and Columns (4)–(6) use the inverse of sample standard errors. In order to understand how censoring may affect the estimates, Columns (2) and (5) show the impact of the program on the probability of deforestation, and Columns (3) and (6) on deforestation among those in the sample with positive forest loss ("deforesters"). The results from the matched

[26] As robustness checks, we have used total area deforested and ln(1 + area deforested) as dependent variables. In all cases we find similar results.

[27] We have run all estimations using OLS as a robustness check. Almost uniformly, we find the same significance pattern as in the Tobit. The OLS estimates are of similar magnitude to the Tobit marginal effects on deforestation conditional on positive deforestation. In other words, they are smaller in absolute value than the Tobit point estimates. This suggests the Tobit performs a necessary function in correcting for censoring.

sample suggest that the program significantly reduced the probability of deforestation by 0.10 to 0.11. Given that the probability of any deforestation in the matched control properties is 0.30, this represents an approximately 33% to 37% reduction in the probability of deforestation. We find that the program reduced the percentage of area deforested among all properties (without adjusting for censoring) by 1.1 to 1.6 percentage points and among deforesters by 4.4 to 4.6 percentage points.

Table 4 presents partial results from Tobit regression postmatching, where the regressions include the full set of matching covariates. The estimates similarly suggest that the program has significantly reduced deforestation. Calculating the mean marginal effects on expected percent area deforested (bottom of Table 4, Column (1)), the postmatching regression indicates that the program has reduced percent area deforested by 1.19 percentage points. Compared to the mean percent deforested for matched control properties, which is 2.4%, this suggests an approximate deforestation reduction of 50%. This indicates that the program was effective in reducing deforestation, but that the possible total avoided deforestation impacts are modest due to the low expected deforestation rates without the program. On the one hand, 2.4% across three years or 0.8% per year is twice as high as the average annual rates for the country as a whole between 2000 and 2005 of 0.4% per year (FAO 2005). On the other hand, the reduction is low in an absolute sense and suggests that most of the land enrolled would have remained in forest even without the program. Clearly, there is room for significant improvement in targeting to generate more avoided deforestation benefits.

Understanding possible heterogeneity in these impacts is important for the design of future policies, particularly in order to target the program in ways that mitigates potential trade-offs between environmental effectiveness and helping poor landowners.[28] We first consider heterogeneity across regions (Column (2)). The sum of the coefficients from the recipient variable and the recipient variable interacted with region is significantly different from zero at the 1% level in Region 4 (south), and significantly different from zero at the 10% level in Region 2 (northeast). This suggests that the program was effective in reducing deforestation in these regions. The estimated mean marginal effects are -1.81 percentage points for the south and -1.68, respectively. Note that the standard errors for each region are large, so the treatment effects are not statistically significantly different from each other across regions. There is no significant difference in effectiveness for common properties versus private properties (Column (3)), although the sign of the coefficient suggests larger impacts in common properties. Column (4) includes an interaction term between road density, which is a proxy for market access, and receipt of payments. There is no significant variation in impact across this covariate.

The last column introduces an interaction between the program and municipal poverty, measured by CONAPO's marginality index (the index ranges from -2 to 2, with larger numbers indicating greater poverty[29]). The estimates indicate decreasing program effectiveness with increasing poverty. The mean marginal effects are mapped out across poverty levels in Figure 2.[30] We see that most of the program's avoided deforestation benefits were in areas where poverty was classified as very low to medium. The marginal effects are not significant above a poverty level of around 0.5, which corresponds to CONAPO's classification of "high" and "very high" poverty levels. Approximately 25% of the sample falls

[28] Several previous studies have indicated heterogeneous effects of conservation policies by market access, using measures of roads or distance to cities, and land quality (e.g., Pfaff et al. 2009; Pfaff et al. 2007; Ferraro, Hanauer, and Sims 2011).

[29] The index is a continuous measure and was created using a principal components analysis based on seven variables from the 2000 census, including illiteracy rates, dwelling characteristics, and proportion of the population working in the primary sector.

[30] Figure 2 shows mean marginal effects on expected percent area deforested by poverty rate. Based on regression in Table 4, Column (5). Marginality index by municipality from CONAPO (2000). Numbers correspond to poverty grades as follows: very low (< -1.3), low (-1.3 to -0.7), medium (-0.7 to -0.1), high (-0.1 to 1), very high (>1).

TABLE 4
Estimates of Program Impact on Deforestation: Postmatching Tobit Regression

	(1)	(2)	(3)	(4)	(5)
Tobit Coefficients					
Recipient	− 5.19*** (1.47)	− 2.90 (3.52)	− 2.05 (3.52)	− 11.97 (15.75)	− 5.21*** (1.50)
Recipient × Region 2 (northeast)		− 4.41 (5.52)			
Recipient × Region 3 (central)		− 0.76 (4.14)			
Recipient × Region 4 (south)		− 4.98 (4.42)			
Recipient × Common property			− 3.74 (3.84)		
Recipient × ln(Road density)				0.98 (2.32)	
Recipient × Municipal poverty index					2.69* (1.61)
Observations	633	633	633	633	633
Pseudo *R*-squared	0.0637	0.0650	0.0643	0.0488	0.0503
Average Marginal Effects on Expected Deforestation					
Recipient	− 1.19*** (0.34)	− 0.67 (0.81)	− 0.47 (0.80)	− 2.74 (3.61)	− 1.19*** (0.35)
Recipient × Region 2		− 1.01 (1.27)			
Recipient × Region 4		− 1.14 (1.01)			
Recipient × Common property			− 0.86 (0.88)		
Recipient × Municipal poverty index					0.62* (0.37)

Note: Standard errors in parentheses. Regressions on the best 90% of matches. Dependent variable is the percent of total area deforested within recipient or control parcels between 2003 and 2006. All regressions also include controls for ln(Area enrolled), ln(Slope), Elevation, Proportion enrolled area semideciduous forest, Proportion enrolled area selva, ln(Road density), ln(Municipal population density), Municipal marginality index. Percent baseline deforestation (2000–2003), region, and tenure type (private property vs. common property).
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

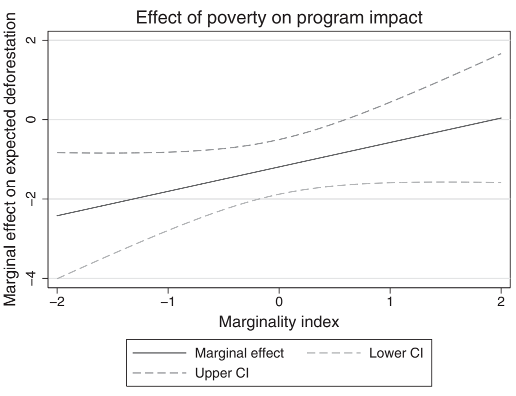

FIGURE 2
Heterogeneity in Program Impacts by Poverty

in municipalities with poverty levels higher than this.

In sum, the results indicate that the program has had a modest direct avoided deforestation impact, with larger impacts occurring in relatively wealthier municipalities. However, as mentioned previously, these direct impacts could be undermined by slippage of deforestation through changes in recipient behaviors on un-enrolled land within the same properties or through changes in prices that induce marketwide effects.

VI. SLIPPAGE: EMPIRICAL TESTS AND RESULTS

Substitution Slippage Tests

Substitution effects, by definition, occur within the landholdings of the owner who enrolls in the program. We therefore compare deforestation rates for treated and control groups on other property that is owned by the same landholder or close to the program parcel but is not enrolled (or submitted to be enrolled). We separate our analysis between private and common properties. For common properties, we know the boundaries of the community, so our primary tests of substitution slippage are based on calculating deforestation within the community but outside of the parcel enrolled (or seeking to be enrolled) in the PES program.[31] For private properties, individual property boundaries are not available. As a proxy, we calculate deforestation within 1 km and 5 km buffers around each privately enrolled parcel, under the assumption that other private landholdings are contiguous with the enrolled property.[32] Separating the analysis between private and common properties also makes sense as we might theoretically expect different effects either due to differences in market rigidities or collective action issues within the common properties.

Results: Substitution Slippage

Table 5 shows results of the tests for substitution slippage. Column (1) of Table 5 indicates that we find no significant substitution slippage effects on average. However, if we allow substitution slippage effects to vary by poverty rates, we do see evidence that substitution slippage effects are significantly larger as poverty increases (Columns (2) and (5)). Figure 3[33] graphs the mean marginal effects of the program on the percentage area deforested within common properties but outside of enrolled or submitted parcels, by the marginality index. At very high levels of poverty (1 to 2), the results indicate a 0.9 to 1.8 percentage point increase in deforestation on other common property lands. At low levels of poverty, substitution slippage is negative, indicating decreased deforestation within the rest of the common property—or "positive" spillovers. For example, at very low poverty (-1.3 to -2), the estimated marginal effects indicate an approximate 1.2 to 1.8 percentage point decrease in deforestation. The confidence intervals indicate that substitution slippage is statistically significant in very high and very low poverty common properties.

Comparison of these results to the estimates of direct program impact yields two insights. First, the magnitudes of the substitution slippage effects are large enough to warrant concern and further exploration. In order to compare the magnitudes of the direct effects to the slippage effects, we must account for the different average sizes of the enrolled *ejido* parcels ($\sim$800 ha) vs. the area of these *ejidos* outside of the enrolled parcels ($\sim$7,000 ha). Using our estimates from Table 4, Column (5), we calculated the expected change in area deforested for each *ejido*. We then calculate the expected change in area deforested due to substitution slippage using the estimation in Table 5, Column (2). Differencing the two gives us the net program impact for each *ejido,* that is, reduced deforestation by the program adjusted for leakage. Averaging across the net marginal impacts and converting back to percentage areas, we find a reduction in avoided deforestation impacts from 1.22 to 1.17 percentage points, or around 4% of the effect.[34] However, as implied by Figure 3, there is considerable heterogeneity in these results: in the poorest quartile of *ejidos,* the slippage effect swamps program impacts, increasing deforestation as a whole, while in the wealthiest quartile, slippage and the program complement each other.

The second insight is that we observe significant spillover deforestation where we did

[31] We also construct buffers of 1 and 5 km around the enrolled common property parcels, just in case community members also control land that is nearby but not within the official *ejido* boundary.

[32] The lack of data on private property boundaries adds noise to our efforts to measure substitution slippage. Abstracting from slippage effects, this would add attenuation bias, which would result in us underestimating the true substitution slippage from private properties, but the sign should be correct. If immediate neighbors are interacting in more complicated ways related to deforestation (e.g., Robalino and Pfaff 2010), then the sign of the bias is not clear.

[33] Figure 3 shows mean marginal effects on expected substitution slippage (percent area deforested within common properties but outside of enrolled or submitted parcel). Based on regressions in Table 5, Column (2). Marginality index by municipality from CONAPO (2000). Numbers correspond to poverty grades as follows: very low (< -1.3), low (-1.3 to -0.7), medium (-0.7 to -0.1), high (-0.1 to 1), very high (>1).

[34] These numbers are obtained by matching two separate samples: one from the impact estimation and one from the slippage estimation. Since these samples do not correspond exactly, these numbers are calculated over the subset of properties that end up as 90% best matches in both estimations.

TABLE 5
Substitution Slippage Tests

	Within Common Properties			Within 1 km Buffer		Within 5 km Buffer	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Common Properties							
Recipient	− 0.188 (0.82)	0.027 (0.80)	1.829 (0.94)**	− 1.268 (0.69)	− 1.009 (0.69)	− 1.009 (0.69)	− 0.182 (0.25)
Recipient × Municipal poverty		2.443* (0.95)			1.998* (0.79)		0.270 (0.28)
Recipient × ln(1 + Commercial banks)			− 2.73 (0.79)***				
Pseudo *R*-squared	0.026	0.031	0.048	0.034	0.037	0.039	0.039
Observations	359	359	343	425	425	425	425
Private Properties							
Recipient				− 1.351 (1.47)	− 0.995 (1.58)	0.411 (0.40)	0.248 (0.41)
Recipient × Municipal poverty					1.036 (1.61)		− 0.586 (0.44)
Pseudo *R*-squared				0.087	0.087	0.115	0.118
Observations				209	209	209	209

Note: Table shows Tobit coefficients with standard errors in parentheses. Dependent variable is the percent of total area deforested on nearby land outside of the enrolled or control parcels but inside *ejido* boundaries, inside a 1 km buffer, or inside a 5 km buffer (2003–2006). All regressions also include controls for ln(Area unenrolled in *ejido*/buffer) ln(Area enrolled), ln(Slope), Elevation, Proportion enrolled area semideciduous forest, Proportion enrolled area selva, ln(Road density), ln(Municipal population density), Municipal marginality index, Percent baseline deforestation (2000–2003), region, and tenure type (private property vs. common property). Note that the data on commercial banks is missing in several municipalities, thus accounting for the difference in sample sizes between Column (3) and Columns (1) and (2).
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

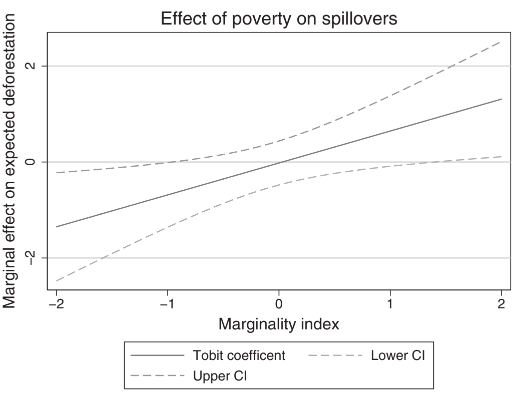

FIGURE 3
Within Common Property Substitution Slippage Effects by Poverty

not observe significant direct avoided deforestation impacts of the program: in poor municipalities. This lends support to the credit constraints hypothesis, because we observe slippage where the program does not appear to be reducing production but could have relaxed credit constraints. These results are consistent with a credit constraints hypothesis, even in wealthy municipalities where the slippage effect complements the program impact. Although not part of our model, it may simply be that common property owners in lower-poverty municipalities have different outside options; they may be able to invest in microenterprises or intensification rather than in agricultural expansion when their credit constraints are relaxed. Sorting out these mechanisms requires household-level survey data, an asset that we hope to collect in future research.

In the absence of more precise data on credit constraints, we conduct a simple exploration using data on the number of commercial banks in a municipality in 2003, available publicly from the Instituto Nacional de Estadística y Geografía (INEGI). The correlation between bank presence and poverty is -0.51, suggesting a lack of commercial credit supply in poor municipalities. In lieu of the interaction with poverty, we include an interaction with commercial banks while controlling for poverty (Table 5, Column (3)). The mean marginal slippage effect for program participation in a municipality with no commercial banks (the median value) is 0.66 percentage points and significant at the 5% level. A 1% increase from the mean in banks (which occurs at the 75th percentile of the data) decreases the amount of slippage to an insignificant number. Since it is highly likely that producers seek financing in places other than commercial banks, demand-side factors are likely at play as well, and since the bank data has a significant number of zeros and missing information, we take these results as merely suggestive. In addition, although our model focuses on credit constraints, we would like to reiterate the possibility that a variety of market frictions might lead to slippage effects, many of which might be correlated with the presence of commercial banks, including labor and land market imperfections.

An analysis of deforestation in 1 km and 5 km buffers around each private property (Table 5, bottom panel) does not indicate significant substitution slippage effects. This may be due to differences in behavior across property types; we expect that private landowners would be less likely to be credit-constrained. However, since the buffers around the enrolled private properties are a noisy measure of lands that might be used by private landowners, we may not have captured the true magnitude of the private-property substitution effect. Ideally, we would have information on the boundaries of individual properties enrolled in the program: future program managers should be encouraged to collect this information as part of the application process.

Output Price Slippage Tests

In order to examine potential slippage through output price effects, we calculate the area of land enrolled in the PSAH within a 50 km radius of each property in our sample. This gives us a proxy for the degree of the reduction in the supply of land and/or the magnitude of the total payments going to a particular area. All else equal, where there are more surrounding properties enrolled in the program, we expect price effects to be larger and to see a greater response in land use change. The ability to observe this effect, however, depends on markets being sufficiently localized to prevent price changes from being distributed through the entire national market. We expect that output price effects would only be observable both where program intensity is high and where markets are relatively localized due to having low access to infrastructure. We use road density (length of roads within a buffer of radius 50 km) to proxy for market access. We follow others (e.g., Keller and Shiue 2008; Donaldson 2010; Gollin and Rogerson 2010) in using the connectivity of transportation networks as a proxy for market access.

Results: Output Price Slippage

Table 6 shows regressions of deforestation in the buffer zones of 1 km and 5 km around all properties as a function of enrollment in the surrounding 50 km radius and other controls.[35] We leave out within-*ejido* measurements, as the results of the previous section suggest that impacts there may be confounded with substitution effects. We measure surrounding enrollment in the 50 km radius using both the total area enrolled and binary measures of high or low surrounding enrollment (defined by being in the top 10% and top 20% of area enrolled: 140 km^2 and 120 km^2 enrolled, respectively). We choose the binary measurement with the idea that price effects may be detectable only where there are very large amounts of the land in the program, that is, beyond some threshold. We find that having more surrounding area enrolled in the program is significantly related to changes in deforestation in all buffer zones and that the results vary with road density. As a robustness check, we also test whether this effect is different in recipient and nonrecipient commu-

[35] We also divided the sample between common and private properties in order to analyze the 1 km and 5 km buffers. This separation did not affect the results, so we present here the pooled sample for these buffers.

TABLE 6
Output Price Slippage Tests

	1 km Buffer			5 km Buffer		
	(1)	(2)	(3)	(4)	(5)	(6)
Tobit Coefficients						
Area enrolled[a]	0.008*			0.017**		
	(0.00)			(0.01)		
Enrolled × Road density	−0.001*			−0.003**		
	(0.00)			(0.00)		
Top 20% area enrolled		8.388			4.305	
		(7.85)			(2.89)	
Top 20% × Road density		−1.341			−0.626	
		(1.20)			(0.44)	
Top 10% area enrolled			1.940**			4.315***
			(0.71)			(1.17)
Top 10% × Road density			−0.287**			−0.629***
			(0.11)			(0.18)
Pseudo R-squared	0.114	0.117	0.115	0.075	0.076	0.078
Observations	814	814	814	814	814	814
Average Marginal Effects of High Enrollment on Expected Deforestation						
At 10% road density	−0.01	0.24	0.88*	0.23**	0.39	1.11***
	(0.15)	(0.42)	(0.50)	(0.09)	(0.04)	(0.30)
At 90% road density	−0.43**	−0.49	−0.78	−0.25	−0.17	−0.50
	(0.17)	(0.45)	(0.52)	(0.10)**	(0.27)	(0.32)

Note: Standard errors in parentheses. Dependent variable is the percent of total area deforested on nearby land outside of the enrolled or control parcels but inside a 1 km buffer, or inside a 5 km buffer (2003–2006). All regressions also include controls for ln(Area enrolled), ln(Slope), Elevation, Proportion enrolled area semideciduous forest, Proportion enrolled area selva, ln(Road density), ln(Municipal population density), Municipal marginality index, Percent baseline deforestation (2000–2003), region, and tenure type (private property vs. common property), and total area in buffer. Marginal effects for Columns (1) and (4) are calculated using a 1 standard deviation increase in enrolled area within a 50 km buffer.

[a] Area enrolled = km^2 of land enrolled in PSAH within a 50 km buffer.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

nities, and fail to reject the null hypothesis that they are the same.

The bottom panel of Table 6 shows marginal effects calculated at the 10th and 90th percentiles of road density. When the area enrolled in the program enters linearly, there appear to be decreases in deforestation at high road density and no effect at low road density. Using a dummy variable for the top 20%—those properties with high intensity of enrollment—shows a similar pattern: increases in deforestation at low road density and decreases at high density with increases in program intensity. Moving the cutoff line to the 10% of properties with the highest intensity of enrollment shows the same pattern, with significance only around properties in very remote areas. The magnitudes imply that being in the high-enrollment category and low–road density areas increases deforestation in the 5 km buffers by 1.1 percentage points. Compared to the mean deforestation rates of 1.3% in low-enrollment areas, this suggests that price slippage could also be large enough to swamp program effects in some areas. In order to help sort through the nonlinearity of these effects, we also estimate an ordinary least squares linear probability model on all the covariates of the regressions in Table 6, without program intensity or the interaction of program intensity with road density. We then extract the residuals from this estimation and use them as a dependent variable in two separate nonparametric regressions on pro-

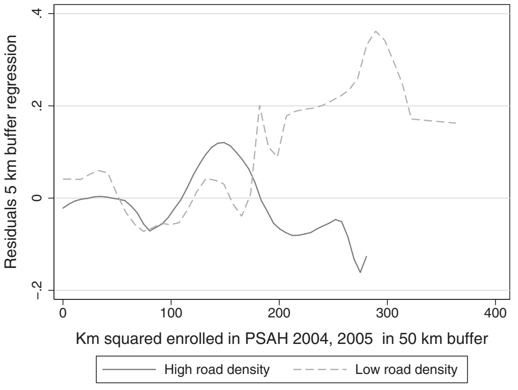

FIGURE 4
Output Price Slippage Effects by Road Density

gram intensity. The first regression uses the residuals from properties with high road density (greater than 25th percentile) and second uses those from properties with low road density (less than 25th percentile).[36] The results of the two are shown in Figure 4.[37] This figure reveals an interesting pattern: beyond an enrollment of about 180 km^2, increases in enrollment in low road density areas are correlated with increased deforestation, while similar increases in program intensity in good-infrastructure areas result in lower deforestation.

We caution that this test for output price spillovers is not conclusive, as it is possible that high enrollment of area in the program is correlated with some other unobserved characteristic of a region that also increases deforestation. Table 7 shows summary statistics for all the properties, with the sample separated into those with high and low surrounding enrollment (highest 10% vs. other 90%). From this table we observe that the two samples are similar in important deforestation determinants like slope, elevation, and road density, but their regional distribution is somewhat different, with more of the properties in the "high surrounding enrollment" group coming from Region 4 (the south).

VII. CONCLUSION

We analyze the impact of Mexico's payments for hydrological services program using recipients enrolled in an early cohort of the program. We find that the program has, on average, significantly reduced deforestation rates compared to what would have happened otherwise, but likely generated modest total avoided deforestation. We also find considerable heterogeneity in these effects; the program seems to be more effective in generating avoided deforestation where poverty is lower, and in the southern and northeastern states of Mexico. Future work should seek to better understand the mechanisms driving heterogeneity in program effectiveness across regions and poverty levels and to understand how program impacts have changed over time. Given that 2004 was one of the early years of implementation, and that later cohorts increased targeting to areas at higher risk of deforestation, future cohorts are likely to demonstrate greater avoided deforestation impacts. Our results also suggest that there may be trade-offs between increasing avoided deforestation and poverty alleviation, because larger impacts were found in less-poor areas. However, it is important to note that this trade-off is observed only among those who have currently applied for the program. It is possible that poverty alleviation goals might be achieved simultaneously with avoided deforestation if targeting criteria were to include deforestation risk (Alix-Garcia, de Janvry, and Sadoulet. 2008).

With respect to slippage, our results indicate possible deforestation spillovers through both price and substitution mechanisms. Substitution mechanisms that increase deforestation appear to be at work only in poor common properties. This is consistent with the notion that substitution might occur where there are significant market rigidities. Future growth of incentive-based forest conservation initiatives are expected to occur mainly in de-

[36] We use the 25th and 75th percentiles (rather than 10th and 90th as above, in order to guarantee a sufficient number of observations to conduct the nonparametric estimations.

[37] Nonparametric regressions of residuals on program intensity for properties with high road density (greater than 25th percentile) and low road density (less than 25th percentile). Residuals are from an OLS linear probability model of deforestation with the covariates from Table 6, excluding program intensity and the interaction of program intensity with road density. Figure shows results using a local polynomial estimator. Lowess estimation gives similar results.

TABLE 7
Summary Statistics by PSAH Enrollment in Surrounding 50 km

Variable	Lowest 90% Surrounding Enrollment	Highest 10% Surrounding Enrollment	Test for Difference[a]
Enrolled area (km^2)	7.81	12.36	2.86
Proportion common properties	0.59	0.83	4.42
ln(Average slope)	2.37	2.47	1.24
Average elevation (km)	1.98	1.92	.59
Proportion enrolled area semideciduous forest	0.28	0.21	1.51
Proportion enrolled area selva	0.28	0.43	3.46
ln(Road density)	6.55	6.58	0.40
Municipal poverty index	−0.21	−0.13	0.86
Ln(Population density)	3.30	3.43	0.84
Proportion in Region 2	0.21	0.05	3.57
Proportion in Region 3	0.31	0.18	2.69
Proportion in Region 4	0.20	0.51	6.61
Observations	724	93	

[a] Test for difference gives the *t*-statistic for difference in means. Other variables are the same as defined in Table 1.

veloping countries, where credit constraints are common, markets are more localized due to poor infrastructure, and incomes are significantly lower. Therefore, future work should seek to better understand the specific mechanism driving within-property spillover effects in developing country situations. Price mechanisms are likely to be ubiquitous across both developed and developing countries but can be detected only when enrollment levels are high and markets are localized. We find that deforestation is more likely when more land in the surrounding 50 km radius has been enrolled in the PSAH program and transportation networks are limited. Since we cannot differentiate between a price spillover caused by demand-side wealth effects versus one caused by a supply-side reduction in land available to agriculture, this is also an area deserving of future research.

Finally, the results indicate the importance of regional accounting for the inclusion of PES in schemes to reduce carbon emissions from deforestation and forest degradation. Given that spillovers may occur as program payments loosen credit constraints, one possibility for limiting this type of displacement is to use a contract that forbids land conversion in the entire area of the common property, rather than for a subset of land (as suggested by Schwarze, Niles, and Olander 2002). We do not think this is easily justified, as it would fundamentally change the nature of the contracts, would require higher payments, and would likely have significant negative livelihood impacts in poor communities reliant on subsistence production. This policy option would also still not address the issue of spillovers through output price effects beyond property boundaries. Rather, it is more important to ensure that REDD designers embed PES programs in larger national systems that track overall deforestation at a regional or national scale (e.g., the "national inventory approach" to REDD suggested by Plantinga and Richards 2008) instead of using a project-based accounting system. This would ensure that all spillovers at the country level—which are not likely to be unique to Mexico's case—would be accounted for.

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