Targeting in payments for forest restoration: evidence from Chile's Native Forest Law

Alberto Garcia*

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Abstract

Widespread landscape restoration has become an important part of global efforts to address the intertwined challenges posed by climate change, biodiversity loss, and rural poverty. Payments for ecosystem services are popular, and policymakers often target priority groups in an effort to achieve dual objectives of increased forest cover and social development. This approach has proven to create tradeoffs in payments for avoided deforestation, however, limited evidence exists on its viability in the restoration context. We evaluate the land cover impacts and targeting strategy of a Chilean federal program that pays landowners to restore their property with native species and prioritizes engagement of rural smallholders and indigenous communities. We find positive tree cover impacts amongst compliant properties, and we estimate that the program paid the average landowner \$45.31 USD per ton of carbon removed. Although prioritized social characteristics were negatively associated with compliance, administrators verified project completion, preventing nearly \$30 million USD in unconditional cash transfers. In contrast to many studies on avoided deforestation, complying smallholders in high poverty comunas generated the greatest tree cover gains per enrolled hectare. These findings illustrate that in contrast to payments for avoided deforestation, targeting for social development may enhance environmental effectiveness in payments for restoration.

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1 Introduction

In order to achieve the warming targets set by the IPCC, both emissions reductions and removals of carbon from the atmosphere will be necessary (Masson-Delmotte et al. 2018). Reforestation and forest restoration have been lauded as potentially near term, large scale and low-cost options to achieve these carbon removals (Busch et al. 2019). Global initiatives such as the Bonn Challenge, Trillion Trees Campaign, and UN Decade on Ecosystem Restoration highlight the enthusiasm for forest restoration as a climate solution (Chazdon and Brancalion 2019), however, there is limited evidence documenting the effectiveness of policies seeking to encourage restoration at large scales. Payments for ecosystem services (PES), which pay private landowners in exchange for providing environmental benefits, have been used extensively in the context of avoided deforestation (Börner et al. 2017), and are likely to play a major role as countries begin to scale up forest restoration in line with global commitments (Gichuki et al. 2019).

Political processes often lead policymakers to design programs aimed at multiple objectives (Tinbergen 1952). PES programs are no different, and are frequently designed to achieve social development goals in addition to environmental gains (Zilberman, Lipper, and Mccarthy 2008; Lipper et al. 2009; Pagiola, Rios, and Arcenas 2008). This is done by targeting payments toward landowners or communities with the greatest need for poverty alleviation (Wunder 2008). However, subsidies that reflect political processes have the potential to undermine environmental benefits (Jack, Kousky, and Sims 2008), and this strategy of targeting for social development has proven to create tradeoffs in payments for avoided deforestation programs (Alix-Garcia, Sims, and Yañez-Pagans 2015).

The effectiveness of payments for avoided deforestation is driven by the presence of deforestation risk (Alix-Garcia and Wolff 2014), but the drivers of effectiveness in payments for restoration is less clear. In the deforestation context, targeting payments toward households unlikely to deforest in the absence of payments generates relatively little additional forest cover relative to the no-payment scenario (Pfaff 1999; Cisneros et al. 2022). It is well documented that poverty is associated with lower levels of deforestation (Busch and Ferretti-Gallon 2017), and as a result of this broad finding, it has been shown that enrollment of landowners in high-poverty areas generates relatively fewer environmental benefits (Pfaff et al. 2007). Further, credit constrained and low-income landowners may be prone to leakage, meaning that they deforest unenrolled land upon enrolling part of the property (Jack and Cardona Santos 2017; Alix-Garcia, Shapiro, and Sims 2012).

While evidence detailing the pitfalls of targeting for “win-wins” in payments for avoided deforestation is widespread, there is a need for rigorous evidence in the restoration setting. Further, it is not clear that the same finding applies to payments for restoration. Private information about the costs to providing forest often make targeting difficult (Jack 2013), and the underlying correlation between targeted characteristics and costs of producing forest may be different in the deforestation and restoration cases. For example, relatively poor households may be unlikely to deforest in the absence of payments, but it may also be infeasible for them to produce new forest without payments due to credit constraints. In this scenario, payments may not generate any behavior change in the deforestation case, but lead landowners to begin providing forest in the restoration case. Second, non-compliance often plays a role in the restoration context. Landowners may enroll in a PES program but fail to complete contracted activities if, for example, landowners are uncertain about restoration costs and benefits (Oliva et al. 2020). If targeted characteristics are positively correlated with non-compliance, tension may arise between participation of priority groups and program effectiveness.

Chile's Native Forest Law provides us the perfect setting in which to explore the potential of payments for restoration to achieve low-cost carbon removals and whether targeting for social development undermined environmental effectiveness. The Native Forest Law provided subsidies for native forest restoration and explicitly sought co-benefits such as economic development of smallholder, indigenous, and rural communities. Program administrators did so through an annual competition for subsidies to support new projects aimed at either restoring existing native forest or planting new forests. Applications to the competition were split into two separate contests based on property size and landowner assets. They were then explicitly scored to determine priority, where the score is based not only project-specific characteristics, but also social characteristics that we show to have a clear positive association with poverty. Few PES programs explicitly score applications by social characteristics, giving us a unique look at the explicit targeting of certain priority characteristics.

In this paper, we use novel data to quantify the land cover impacts of Chile's Native Forest Law for ten cohorts (2009–2018) using annual data from 2000 to 2018. Temporally consistent annual landcover data detailing tree cover and other land classes such as grasslands and croplands provide a unique opportunity to evaluate restoration outcomes using panel difference-in-differences approaches. Using the estimates of program impact, we calculate the carbon impacts of the policy and the price paid to the average landowner per ton of carbon removed through natve forest restoration. We then evaluate the Native Forest Law's strategy to prioritize participation of marginalized landowners by examining treatment effect heterogeneity across contest, social score, and poverty.

A key concern with evaluations of programs with voluntary participation is that apparent effectiveness may actually be due to different participation costs (Jack and Jayachandran 2019). The typical enrollee to the Native Forest Law competition was already increasing forest cover prior to enrollment in the program. If we were to simply compare enrolled and unenrolled properties after enrollment, we would likely overestimate the effect of the subsidies, since enrollees were producing new forest cover even without payments. In order to alleviate concerns surrounding selection, we use pre-processing techniques to construct a set of control properties from a pool of all rural properties in the major forested regions of Chile. These matched control properties are much more comparable to enrollees than the typical unenrolled property based on a detailed set of land use and property characteristics. These characteristics include pre-program land-uses, which are likely to drive enrollment decisions. Difference-in-differences methods further allow us to control for fixed differences in land cover between enrollees and the control group as well as time trends affecting both groups. Using this strategy to estimate treatment effects for properties that dropped out after the first six months and never received payment yields no statistically or economically meaningful treatment effect, lending creedence to our estimates for program beneficiaries.

We find that the program increased tree cover on the properties of landowners who received payment, coming largely from land that was previously cropland or grassland. Enrolling the full property through the Native Forest Law contest led to a 0.21 % increase in the share of the property covered in forest on average. Event study estimates based on weaker identification assumptions show that dynamic effects play a significant role and that the program led to a more substantial increase in tree cover for cohorts observed for several years post-enrollment. Based on final program impacts for the earliest cohorts, the subsidy competition paid the average landowner an estimated $45.31 USD per ton of carbon. Several moderating factors make this a plausible carbon price in this setting. On one hand, this price assumes that later cohorts achieve similar treatment effects and that newly established forest matures. On the other hand, subsidized restoration in already standing native forest is not likely to be reflected in our satellite-derived measures of tree cover extent. Further, environmental benefits may be understated if landowners substitute away from plantation forest into native forest, which is not observable in our study.

Non-compliance in the Native Forest Law was high, as over two-thirds of applicants never received payment. Landowners with higher application social scores did comply at relatively lower rates, however, program administrators avoided unconditional payments by requiring verification of activity completion. This allowed the program to target priority characteristics without increasing the probability of unconditional cash transfers. We find that smallholders drive the main treatment effect, while larger, wealthier landowners in the other interested party contest provided no positive tree cover benefits at all. Increased comunity-level poverty was associated with greater tree cover impacts, for which the program's scoring mechanism was a good predictor. These findings are in contrast to the expectation in payments for avoided deforestation, where wealthier landowners with high deforestation risk often provide the greatest additionality.

Contributions. Our results make several contributions to the literature. First, we add to the limited existing evidence on the environmental effectiveness of large-scale native forest restoration programs. Between 2009 and 2019, the National Forest Corporation (CONAF) allocated approximately US $58 million to enroll more than 235 hectares of land through the Native Forest Law, making it one of the largest native forest restoration programs in the world. There are relatively few studies evaluating payments for forest restoration and even fewer quantifying the impacts of large-scale or national policies. Notable exceptions include Uchida, Xu, and Rozelle (2005), who examine land enrolled in China's Grain for Green program. While unable to examine land cover outcomes explicitly, they find that payments were relatively well targeted toward plots with low costs of conversion and high potential for erosion reduction. Heilmayr, Echeverría, and Lambin (2020) use econometric simulation to evaluate the land cover impacts of Chile's Decree Law 701, which subsidized monoculture plantations of exotic eucalyptus and pine. They find that additionality was quite low, and further, that native forest extent was reduced as landowners substituted toward plantation forest. España et al. (2022) is one of the few studies of which we are aware that quantifies land cover impacts of a national scale afforestation or reforestation policy using quasiexperimental methods. They quantify the effects of Chile's Decree Law 701 using difference-in-difference methods and find that the policy led to a 13% increase in forest plantations amongst subsidy participants.

Our second contribution is to highlight key differences between targeting in payments for reforestation in contrast to avoided deforestation. Several studies find that targeting for social development has come at the cost of environmental gains in payments for avoided deforestation. Alix-Garcia, Sims, and Yañez-Pagans (2015) find that the landowners for which PES most effectively reduced poverty provided the least avoided deforestation. In another study, Alix-Garcia, Shapiro, and Sims (2012) identify leakage amongst capital-constrained landowners in Mexico's National PES program, reducing program benefits in high-poverty areas. No study to our knowledge has addressed whether targeting for social development is costly in the reforestation or restoration setting. In this study, we identify a potential opportunity for win-wins in the restoration context, which to our knowledge, has not been documented before. Smallholders in high-poverty areas actually yielded greater tree cover gains than larger, wealthier landowners. Descriptive evidence suggests that this may be due to the fact that wealthier landowners were already expanding tree cover at relatively higher rates prior to program enrollment. This highlights that poor landowners may experience relatively greater obstacles to producing new forest in the restoration setting. Although prioritized characteristics were associated with increased non-compliance, non-complying landowners were never paid, avoiding unconditional cash transfers.

2 Background: Chile's Native Forest Law

Chile's long history of public policies supporting tree cover expansion provides an incredibly useful setting in which to measure the impacts of payments for restoration. Chile's decree law no. 701 (DL 701) is one of the world's longest operating afforestation subsidies, but mainly promoted even-aged mococulture plantations of eucalyptus and pine that had negative effects on biodiversity and native forest extent (Heilmayr, Echeverría, and Lambin 2020).

In an attempt to encourage the recovery and protection of native forests, Chile sought to pass the Ley de Recuperación del Bosque Nativo y Fomento Forestal (Native Forest Law) as a successor to DL701 (Clapp 1998). Initially expected in 1994, it became frozen in legislature before finally passing in 2008. In addition to protections for native forests, the law established an annual competition for grants to support private landowners in their efforts to manage, restore, or reforest their land using native species. Between 2009 and 2019, more than $58 million were allocated through these competitions for projects covering 235 thousand hectares. Much of this allocated funding has not been paid to landowners, however, as program follow-through is low.

The subsidy component of the law encourages three types of activities: 1) the regeneration, restoration or protection of native forests; 2) silvicultural activities aimed at restoring native forests for timber production purposes; and 3) silvicultural activities aimed at restoring native forests for non-timber production purposes. Of the 12,889 projects enrolled between 2009 and 2019, 10,912 (84.66%) restored native forest for the purposes of timber production. Examples of subsidized activities include thinning, enrichment planting (introduction of trees to degraded forest), and establishment of new forest via tree planting. Few estimates on the impacts of the Native Forest Law on land cover currently exist. CONAF estimated the carbon impacts of the Native Forest Law through 2018 as it relates to Chile's Nationally Determined Contribution (NDC) as part of the Paris Agreement. These estimates, however, assume that the carbon stored by every eligible subsidized hectare is the direct result of the law. These types of estimates ignore the concept of additionality, since some of this forest would likely exist even in the absence of the law.

By prioritizing native forest rather than monocultures of pine or eucalyptus, the law seeks to incentivize the preservation of biological diversity. Prioritizing carbon-plantings without consideration of other co-benefits may result in negligible biodiversity co-benefits (Bryan et al. 2016). In fact, DL701 resulted in the decline of native forest and biodiversity, landowners replaced native forest with plantation forest, which provide significantly less biodiversity than native forests in Chile (Heilmayr, Echeverría, and Lambin 2020).

The Native Forest Law prioritizes not only forest cover in line with Chile's NDC goal of managing 200,000 hectares of native forest, but also the participation of underrepresented groups. It is explicitly mentioned in the Native Forest Law seeks to meet the dual objectives of rural economic development in addition to protection of the environment. In the case of the Native Forest Law subsidy competition, this is done by prioritizing applications of landowners deemed to be of higher social priority. This is done in two main ways: 1) holding separate contests for smallholders and wealthier, larger properties; and 2) a scoring mechanism that gives higher scores to indigenous peoples, smaller properties, and those in rural regions. We further explore the ramifications of this targeting strategy in Section 5.

Upon having an application selected through the Native Forest Law's competition, landowners must clear two key administrative hurdles in order to receive payment. First, landowners must provide an updated native forest management plan, detailing the specific activities to be performed on the property, a timeline for activity completion, and a georeferrenced map of the property. Landowners generally have six months from the date of selection to provide the management plan or be dropped from the program. Many landowners drop out at this stage of the program, as only 37.05% of enrollees submit the management plan within the required window. The second hurdle is to have the project's completion verified by a third party. The timeframe for activities generally lasts multiple years, and landowners are not eligible to receive payment until a minimum of two years after enrollment. Conditional on submitting the management plan, the rate of payment is relatively high at 75.44%. Ultimately though, only 31.88% of projects enrolled between 2009 and 2019 have actually been paid out on. Program administrators have cited both capital constraints and a lack of labor supply as potential causes for such high rates of non-compliance. Submission of cartography with the management plan may also deter some landowners who struggle with the administrative hurdle itself.

3 Data and descriptive trends

3.1 Administrative data

We have obtained property boundaries for all rural properties in the major forested regions of Chile as of the year 2009. Data on the awarded properties are available through CONAF and reflect aspects of the property and projects such as project objective, project surface area, bonus amount, and whether a management plan was submitted within six months. Also included is each property's parcel indentifier, which is unique to each property within a comuna, Chile's level 3 administrative unit. We match the enrolled properties to their corresponding boundaries via this unique parcel identifier. In addition, we observe payment recipients, which are matched to the corresponding program application, indicating whether a project was successfully completed.

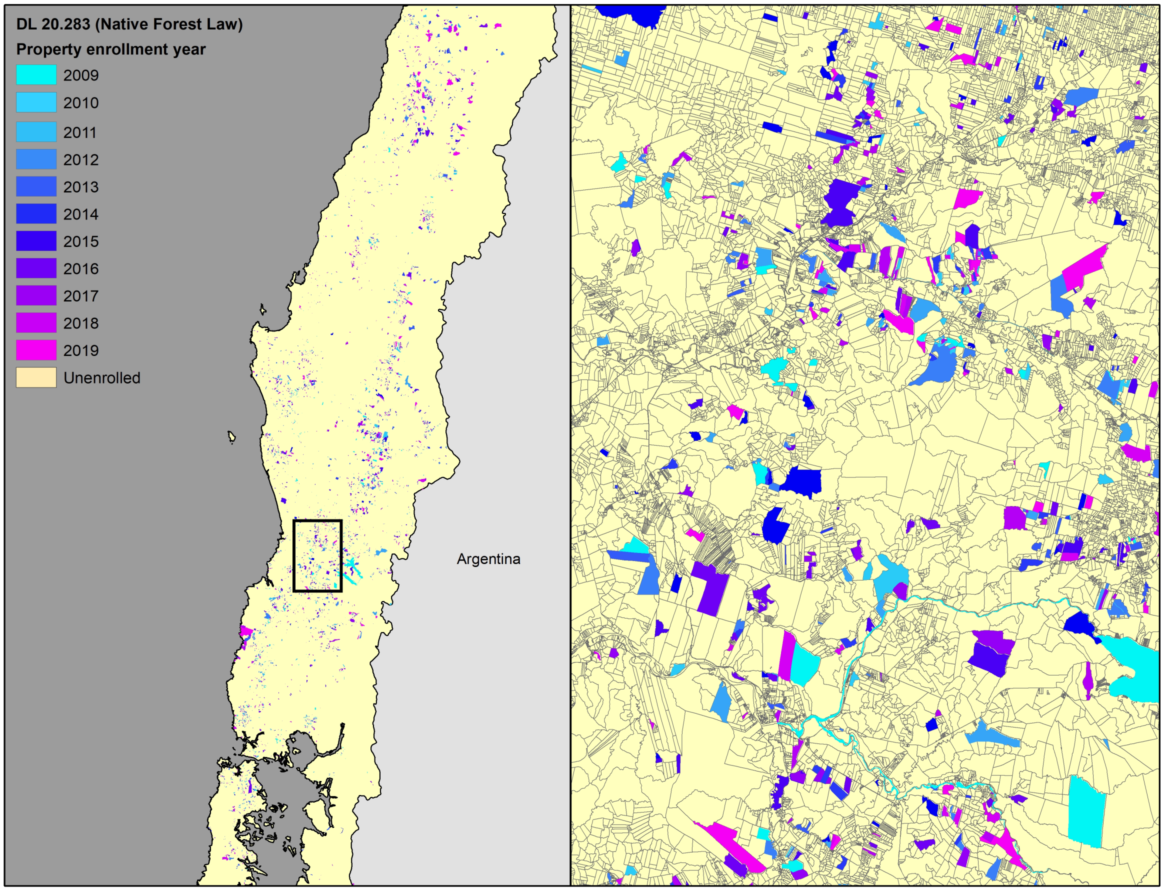

Figure 1: Properties enrolled through the Native Forest Law subsidy contest

3.2 Satellite data

In order to quantify environmental impacts of the Native Forest Law, we use annual measures of land cover. Our primary outcomes of interest come from annual Landsat (30m) resolution maps of land cover developed in Graesser et al. (2022). These maps classify pixels into one of the following classes for each year between 2000 and 2018: forest, crop, grassland, shrub, and bareground. These land cover maps provide a unique opportunity to examine restoration outcomes. While many studies use satellite derived measures of deforestation to generate annual panels of forest loss, very few studies leverage annual variation in specific land cover types outside of those focused on North America and Europe. Further, the Graesser et al. (2022) product was developed specifically to produce more consistent land cover estimates over long time periods and gradual change events such as restoration, making it the ideal data product for our analyses.

Second, Landsat resolution land-use classification maps of these regions in Chile developed in (Heilmayr, Echeverría, and Lambin 2020) allow us to distinguish the proportion of each property engaged in specific land uses prior to the existence of the Native Forest Law. Of particular interest is the distinction between plantation forest and native forest, which cannot be distinguished in the Graesser et al. (2022) product. In contrast to native forest, high levels of plantation forest may indicate greater ability to manage forest and undertake contracted activities. All of our satellite derived measures cover the extent of the major forested areas of Chile, representing the regions that contain the vast majority of Native Forest Law enrollees.

3.3 Descriptive statistics and trends

Summary statistics for enrolled properties are shown in Table 1. Most enrollees enroll less than 15% of their property through the competition, and typical enrollee properties already have quite a large area of native forest cover and some plantation forest prior to the existence of the Native Forest Law. Section 8.1 in the Appendix shows how descriptive statistics vary across subgroupings of the data such as contest type and compliance.

Table 1: Summary statistics for enrollees in Native Forest Law subsidy contest

Statistic	Mean	Median	Std. Deviation
Property size (ha)	201.116	45.120	514.733
Subsidized surface (ha)	15.460	5.840	39.632
Proportion of property subsidized	0.233	0.143	0.240
Bonus amount (UTM)	84.589	34	202.193
Received payment	0.319	0	0.466
Submitted management plan	0.371	0	0.483
Timber production objective	0.842	1	0.365
Received extensionist support	0.499	0	0.500
Pct. tree cover change (00-08)	0.074	0.001	1.365
Native forest	0.430	0.432	0.305
Plantation	0.151	0.026	1.223
Tree cover	0.683	0.774	0.302
Crop	0.024	0	0.102
Grassland	0.272	0.191	0.267
Dist. to native timber processing (km)	21.235	15.141	17.359
Dist. to any timber mill (km)	10.730	9.092	7.774

Figure 2 shows the average change in tree cover in the 5 years leading up to enrollees' application to the subsidy competition. The left panel shows the raw trends in tree cover for the typical property across both the smallholder and other interested party contests. The right panel shows the rate of tree cover change in the years leading up to enrollment across the two contests. In both contests, we see that tree cover was already increasing on the typical enrollee's property prior to enrollment. This is evident by the raw trends (left panel) and the positive rate of tree cover change (right panel) for all years immediately leading up to enrollment. The right panel of Figure 2 shows that the rates of tree cover gain across both contests was similar until the three years leading up to enrollment. At this point, the typical property in the other interested party contest increases the rate of tree cover gain. This means that by the time of application, landowners in the other interested party contest are already increasing forest cover at a rate nearly four times higher than enrollees in the smallholder contest. Smallholders on the other hand do not change their rate of tree cover gain at any point leading up to the enrollment year. This observation raises the question of whether landowners in the other interested party applied to the contest to subsidize tree cover expansion that they were already undertaking without any subsidization.

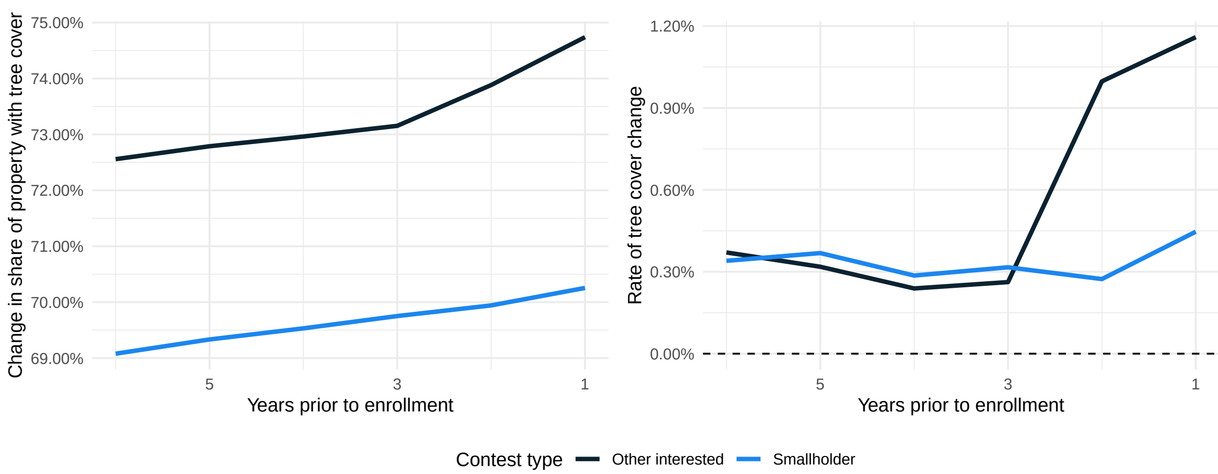

Figure 2: This figure shows rates of change by land cover type for enrollees in the years leading up to their application and enrollment in the Native Forest Law subsidy competition.

4 Program evaluation

4.1 Constructing a counterfactual

To quantify the environmental impacts of the Native Forest Law subsidy contest, we focus on landowners who complied with program requirements and received payment. As is the problem with many PES impact evaluations, enrollment is non-random. Landowners choose to enroll in the program and, in theory, have an opportunity cost equal to or lower than program payment. This means that the average enrollee likely has lower participation costs than the average unenrolled landowner. It is then ill-advised to simply use unenrolled properties as the counterfactual, since unobservable factors affecting enrollment could drive changes in forest cover outcomes, not enrollment.

In order to move toward a more convincing counterfactual, we first use matching as a pre-processing technique to generate a control group from amongst all unenrolled rural properties in the major forested regions of Chile. This should yield control properties with more similar opportunity costs to enrollees than amongst the general population. This approach is similar to many recent studies in the literature (e.g., Cisneros et al. 2022).

The covariates used for matching include environmental and economic characteristics likely to determine enrollment decisions and project performance. We include pre-enrollment property land-use including levels of native forest, plantation forest, and pasture. Landowners with similar levels of plantation forest, native forest, and other land uses on the property should face a similar decision about whether to enroll in a program involving native forest management. Other included covariates give a sense of a property's productive potential, remoteness, and timber market access. We also match on land cover pre-trends to help us build confidence in the common trends identification assumptions we make in the next section. In doing so, we use land cover trends leading up to the first Native Forest Law subsidy competition. This helps to avoid concerns of overfitting and should still allow us to see any major anicipatory land cover changes in our pre-trend analyses. Thus, seeing that pre-enrollment trends hold should lend further creedence to the matching process. Matches are made with replacement based on nearest neighbor propensity scores from a logit model. We include the two unenrolled nearest-neighbors for each program enrollee in the control group.

Prior to pre-processing, enrolled and unenrolled properties differ significantly. The typical enrollee had significantly less land engaged in pasture or agriculture and significantly more native forest already relative to the typical unenrolled property. Table 2 displays balance checks for all covariates used. The normalized mean difference and variance were reduced for nearly every covariate after the matching process. Figure 8 in the Appendix shows how comparability between selected covariate distributions drastically improved between treatment and control properties after matching. After processing, balance improved on every included covariate, and the normalized mean difference fell below the often-used threshold of 0.25 for every covariate.

One concern may be our decision to exclude rejected applicants from the control group. Given that these properties have revealed their intention to enroll in the program, it seems that they may have opportunity costs similar to program enrollees. However, the composition of the rejected applicants differs between smallholders and other interested parties depending on year, and the composition of the rejected applicant group is relatively unstable through time. Further, it would be difficult to make claims about the differences between the smallholders and other interested parties, it is much easier to be rejected in the other interested party contest for most contest years. We discuss the rejected applicants in more detail in Appendix 8.6.

Table 2: Covariate balance before and after matching

	Norm. mean diff.		Variance diff.	
Covariate	**Unmatched**	**Matched**	**Unmatched**	**Matched**
Tree cover trend (05-08)	0.01	0.00	5.66	4.17
Crop trend (05-08)	-0.06	-0.01	6.78	3.51
Grassland trend (05-08)	0.06	0.02	13.90	3.12
Native forest (2001)	-1.32	0.12	0.37	1.35
Plantation forest (2001)	0.09	0.04	2.31	1.48
Tree cover	-1.03	0.20	1.42	1.04
Grassland	-0.37	-0.13	0.06	1.25
Crop	-0.15	-0.09	0.04	0.35
Shrubs	-0.12	-0.06	0.77	0.77
Development	0.02	-0.03	292.44	0.10
Water	0.01	-0.02	17819.17	1.78
Slope	-1.04	0.04	0.34	1.57
Elevation	-1.12	-0.08	0.24	1.41
Lattitude	-0.15	-0.09	0.80	1.00
Area	-0.37	-0.10	0.15	1.93
Dist. to industry	-0.60	-0.01	0.53	1.98
Dist. to native specific industry	-0.07	-0.02	0.98	1.10

4.2 Main specification

We take advantage of our panel data setting and estimate the following equation to reveal the land cover impacts of the Native Forest Law subsidy contest:

$$outcome_{it} = \beta_0 + \beta_1 \times intensity_{it} + \gamma_i + \lambda_t + X_{it} + \epsilon_{it} \tag{1}$$

where $outcome_{it}$ represents the share of property i engaged in a specific land cover outcome in year t; $intensity_{it}$ represents the proportion of property i enrolled through the Native Forest Law subsidy contest in year t; and γ_i and λ_t represent property and year fixed effects, respectively. Property fixed effects (γ_i) control for unobserved time invariant characteristics such as landowner preferences. Year fixed effects (λ_t) control for time-varying shocks that are common across all properties such as changes in other environmental policies. Conditional on covariates and fixed effects, β_1 recovers the impact of enrollment in the Native Forest Law contest, conditional on compliance.

Because Equation 1 relies on property and year fixed effects, it falls under the umbrella of two-way fixed effects (TWFE) estimators. This literature has received ample attention in recent years, particularly in the case of binary treatment (i.e., whether a property enrolled) (Roth et al. 2022; Chaisemartin and D'Haultfœuille 2022). In this context, binary treatment would ignore the proportion of the property enrolled through the contest. Equation 1 is valuable in our context, because the median landowner enrolls less than 15% of their land in the program, with significant variation across properties (Table 1).

Importantly, $intensity_{it}$ represents a continuous treatment in the context of TWFE estimation. Callaway, Goodman-Bacon, and Sant'Anna (2021) decompose TWFE estimators when treatment is continuous and show that β_1 represents the weighted average change in outcomes from incremental changes in land enrollment across and within periods. Thus, my identification relies on the following assumption: properties that enrolled an additional increment of land in the Native Forest Law contest, must experience the same evolution in outcomes as properties that never enrolled the increment. We evaluate the plausibility of this common trends assumption based on both raw trends and an event study approach in Appendix 8.3.

4.3 Event study

The dynamics of payments for ecosystem services are important, and perhaps moreso in the restoration context. Tree cover is not established instantaneously, including in satellite-derived measures of tree cover, where a pixel is only classified as tree cover if it is greater than 30% canopy cover. Survivorship of trees is also key, as many planting initiatives have led to minimal long-term success (Coleman et al. 2021). These factors are echoed in the Native Forest Law payment scheme, where landowners are not even eligible to receive their payment in the first year of enrollment.

We use the estimator developed in Callaway and Sant'Anna (2020) to generate event study treatment effects. It is important to note that this estimator relies on binary treatment. While this cannot account for the fact that properties enroll only a selected proportion of a property, it provides robustness properties not true of equation (1). First, our event study estimates rely on a relatively weaker conditional common trends assumption. Common trends need only hold after conditioning on our detailed set of time-invariant pre-treatment characteristics. The Callaway and Sant'Anna (2020) estimator is also robust to general treatment effect heterogeneity, which, in severe cases, could flip the sign of our main specification's treatment effect estimates (Chaisemartin and D'Haultfœuille 2020).

4.4 Program evaluation results

Table 3 shows that using the matched control group, tree cover exands on the characteristic enrolled property after enrollment relative to the counterfactual. It also sees a decline in grassland and cropland. Since $intensity_{it}$ is the proportion of the property enrolled, β_1 can be interpreted as the impact of enrolling the average landowner's full property through the Native Forest Law competition. For the average property, enrolling the full property leads to a 0.63 percentage point (0.90%) increase in the share of the property with tree cover. However, since the average landowner enrolls 23% of their land (Table 1), the impact for the typical enrollee is closer to a 0.14 percentage point (.21%) increase. It also led to a 0.19 (6.55%) and 0.45 (1.79%) percentage point decline in the share of the typical enrollees' property with crop and grassland, respectively. Standard errors are clustered at the property, the level of the decision-making unit and at which treatment is assigned.

Table 3: Estimates of subsidy impact using matched control group

	Tree cover	Crop	Grassland
Intensity	0.00628**	-0.00188**	-0.0045*
	(0.00239)	(0.00076)	(0.00225)
Num.Obs.	183521	183521	183521
R2	0.970	0.899	0.958
Control group	matched 2-to-1	matched 2-to-1	matched 2-to-1
2008 mean	0.699	0.029	0.251

Standard errors are clustered at the property level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

In order to gauge the plausibility of the matched control group as a counterfactual, we consider a similar estimation strategy using program enrollees who never submitted a management plan. These non-compliant properties had applications selected through the competition, however, they failed to provide a management plan within the required six-month window following selection. Therefore, they were dropped from the program. Because these landowners dropped from the program within such a short time-frame and never submitted a management plan indicating exact project details, it is unlikely they engaged in sustained restoration activity. We use the same pre-processing techniques to generate a matched control group for these non-compliers and again estimate Equation 1. Table 4 shows that the estimated tree cover treatment effect is not only statistically insignificant, but also of no meaningful magnitude. Although there is a statistically significant effect for crop, the magnitude is very small. It may be the case that landowners in this group shifted away from crop cover in response to having their application selected, but never could establish tree cover in its place. Finding no meaningful effect amongst properties that engaged with the program but dropped out soon after enrollment lends credence to our main estimation strategy.

Figure 3 shows event study treatment effects based on the estimator developed in Callaway and Sant'Anna (2020). Similar to results based on equation 1, these estimates indicate that the Native Forest Law subsidies increased tree cover, while reducing crop and grassland. These graphs indicate that most of the increased

Table 4: Estimates of subsidy impact on non-compliers using matched control group

	Tree cover	Crop	Grassland
Intensity	6e-05	-1e-04**	5e-05
	(5e-05)	(4e-05)	(5e-05)
Num.Obs.	321309	321309	321309
R2	0.970	0.893	0.957
Control group	matched 2-to-1	matched 2-to-1	matched 2-to-1
2008 mean	0.699	0.023	0.258

Standard errors are clustered at the property level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

tree cover seems to have come from grassland conversion. Pre-treatment estimates in Figure 3 represent pseudo-ATTs, which are all indistinguishable from zero across the three land cover types. The conditional common trends assumption on which these estimates rely, thus, seems plausible. Our event study estimates are robust against concerns surrounding general treatment heterogeneity and do not rely on assumptions as stringent as those from our main specification, so seeing that these results are similar to those from equation 1 lends confidence to our main results. Similar to the main TWFE specification, estimates are based relative to both the control group and not-yet-treated properties. In the Appendix (Figure 11), we include similar results based on inclusion of only the never-treated control properties in the control group.

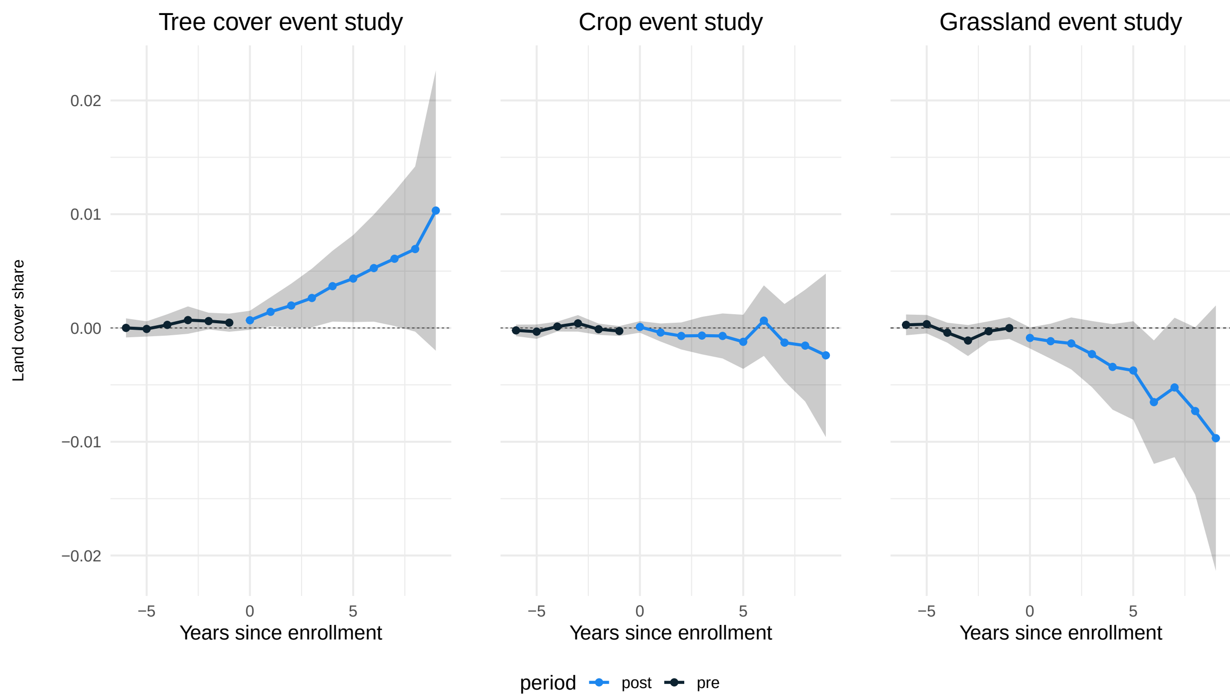

Figure 3: This figure shows event time treatment effects of the Native Forest Law subsidy contest for beneficiary properties. The three panels show event time treatment effects based on binary treatment for three land cover types: Tree cover, Crop, and Grassland.

Looking at Figure 3, the treatment effect has not leveled-off. This could be due to a couple different factors. First, landowners may still be establishing new tree cover 9 years after initial enrollment (10th year of treatment). In this case, the estimates from both equation 1 and the event study will underestimate the carbon and land cover impacts of the subsidies. However, it may also be the case that this sustained effect is due to the changing composition of cohorts in each event-time window. Appendix 8.5 shows that there is no clear systematic relationship between cohorts and treatment effect. We also present results of a “balanced” event study, proposed in Callaway and Sant’Anna (2020), in Figure 4. This balanced event study estimates event-time treatment effects for cohorts that are observed for at least 8 years post-enrollment (i.e., the 2009 and 2010 cohorts). The benefit of the balanced event study is that there is no change in treatment unit composition across event-time windows. The major cost of balancing is that fewer groups are used to compute these event-study-type estimands, which can lead to less informative inference (Callaway and Sant’Anna 2020).

As expected, both types of event study results do imply that the treatment effect is increasing over time, suggesting that the estimates based on equation 1 may underestimate the ultimate impact of the program.

We believe that the balanced event-study estimates represent a more informative path of tree cover impacts through time than those presented in Figure 3. As expected, there is no clear effect for the first few years following enrollment. The subsidized activities often take several years to complete, and newly planted trees will not be picked up instantaneously in the outcome variable. While we expect biomass accumulation to continue through time, tree cover extent is not likely to continue to be established infinitely far in the future as a direct result of subsidized projects, as suggested by Figure 3. We see in Figure 4 that balanced treatment effect estimates do increase through time but seem to level off 7 years post-enrollment. The final event-time estimate from the balanced event study suggests that enrollment led to a 0.70 percentage point (1.00%) increase in the share of the property with forest cover on average. If we assume that all of the forest cover gain ocurred in the subsidized proportion of the property, this effect is closer to 3.04 percentage points (4.34%), much greater than the effect suggested by results in Table 3.

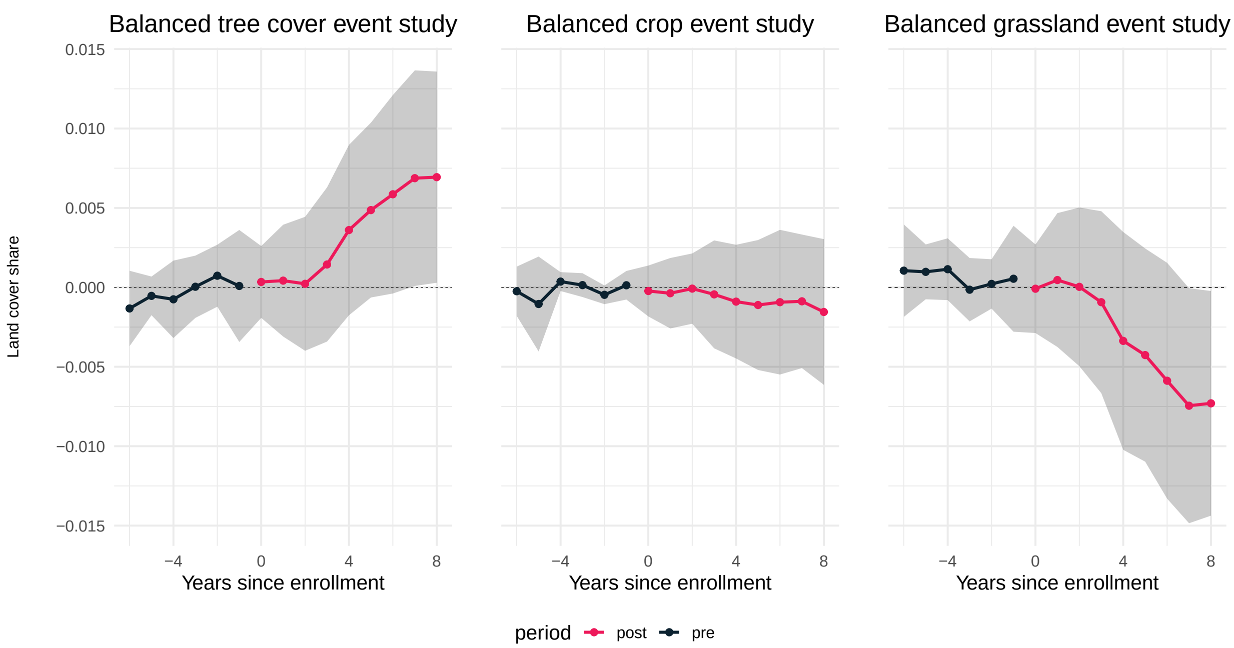

Figure 4: This figure shows balanced event time treatment effects of the Native Forest Law subsidy contest for beneficiary properties. Only estimates cohorts experiencing treatment for at least 8 years are included, so there is no change in the composition of treatment cohorts by event-time window. The three panels show event time treatment effects based on binary treatment for three land cover types: Tree cover, Crop, and Grassland.

4.5 Carbon impacts

Previous work identifies the potential for restoration to remove significant amounts of carbon from the atmosphere at a relatively low carbon price (e.g., Busch et al. 2019; Austin et al. 2020), however, there exists little evidence on the ex-post costs of real-world policy to achieve these removals. We present back of the envelope estimates of the carbon price achieved on the average property through the subsidy competition. We provide these estimates of the carbon price based on the final event-time estimate in our balanced event study.

We estimate the price of carbon removals achieved by the first two cohorts of the competition to be $45.31 per ton of carbon. This is our preferred estimate for the ultimate impacts of the subsidy payments, but it does rely on assumptions about future carbon accumulation. First, this assumes that later cohorts see dynamic effects similar to those experienced by the 2009 and 2010 cohorts. Second, we rely on assumptions about the carbon content of newly established native forest. The estimates of carbon content per hectare of native forest used in this calculation are based on mature native forests by region in Chile. While this number may overestimate the current carbon content in newly established native forest subsidized through the Native Forest Law, our method assumes that this subsidized forest will eventually mature and attain these levels of carbon content. One moderating factor is the fact that our estimates are unable to capture carbon benefits achieved through restoration of already standy forest, which is relatively common amongst subsidized projects. This factor lends some confidence that our estimate is not a significant underestimate of the true final cost.

5 Targeting for social development

5.1 The Native Forest Law's targeting strategy

In order to qualify for the smallholder contest, landowners must have assets and a property size below a set threshold for each region. The benefits of qualifying for the smallholder contest include 15% higher payments as well as increased odds of having an application selected through the contest. Landowners in the smallhoder contest are also significantly more likely to receive extensionist support. Extensionists often helped landowners understand the potential for different activities on the property and assisted landowners to complete and submit an application. In 2019, CONAF also began offering technical assistance to selected landowners through the smallholder contest. While the Native Forest Law used these separate contests to alleviate concerns that large corporations with significant assets would reap the rewards from the program, smallholder classification is still quite broad. In the typical region, properties up to 200 hectares can qualify for the smallholder contest. Figure 6 shows that while many applicants in the smallholder contest are truly landowners with small properties, many larger properties are able to enter the smallholder contest.

In addition to holding separate contests, the program used a scoring system in order to assign project funding priority within each contest. Projects were granted funding in descending order of score until the allocated funding had been assigned. This meant that projects sometimes went unfunded because of a low score, although no ex-ante cutoff existed. This was particularly common in the other interested parties contests, which were granted funding after the smallholders. In some years, a second smallholder contest was held, causing smallholders to go unfunded because of low scores. The scoring criteria include factors related both to landowner, property, and project characteristics. This score, although not always critical for smallholder applicants, provides insight to program administrators' preferences for project prioritization.

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$$score_i = social_i + project_i$$

where $social_i$ represents components of the score deemed to be of social importance by program designers; and $project_i$ represents components of the score representing project specific characteristics unrelated to the landowner themselves. The social score, $social_i$ and the project score, $project_i$ can be further broken down as follows:

$$social_i = \gamma_t VI + \beta_t VPS$$

$$project_i = \lambda_t VP + \psi_t VP$$

, where

- VI = social characteristics of interest, including property size (higher scores to smaller properties) and total subsidy amount (penalizing particularly large projects)
- VPS = other priority social characteristics, including indigenous status (higher scores to indigenous landowners or communities)
- VP = project characteristics
- VT = land characteristics

, and γ_t, β_t, λ_t, and ψ_t represent the weights given to each category in year t.

This score demonstrates the specific project characteristics prioritized by program administrators. While the score does not explicitly target high-poverty landowners, if the objective is to promote rural economic development and poverty alleviation, we are interested in whether the score actually helps administrators target low-income landowners. In order to understand whether the social score was a good predictor of poverty alleviation potential, we regress 2007 comuna level poverty on the social and project scores.

$$ln(ComunaPov_i) = \rho_0 + \rho_1 \times ln(social_i) + \rho_2 \times ln(project_i) + e_i \tag{2}$$

The results are shown in Table 5 and indicate that $social_i$ is associated with increased comuna-level poverty, holding $project_i$ constant. We also see that $project_i$ is associated with greater levels of poverty, perhaps indicative of less profitable alternate uses of the land in high-poverty comunas. Incuding region fixed effects in the model eliminates the clear association of the project score with poverty, however. It is encouraging to see that CONAF's generated social score is actually positively associated with poverty.

Table 5: Social score is associated with relatively higher comuna level poverty

Outcome var.	ln(ComunaPov)			
	(1)	(2)	(3)	(4)
ln(Social score)	0.09601***	0.14969***	0.07363***	0.19827***
	(0.01847)	(0.02249)	(0.01293)	(0.01679)
ln(Project score)	0.13323***	0.13470***	-0.01432	-0.00567
	(0.01367)	(0.01721)	(0.00907)	(0.01097)
Num.Obs.	12828	8750	12828	8750
R2	0.009	0.009	0.617	0.645
Subsample	Full sample	Smallholder	Full sample	Smallholder
Region FE	No	No	Yes	Yes

Standard errors are clustered at the property level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.2 The relationship between program impact and targeting

To answer the question of whether the Native Forest Law's strategy to target for social development undermined environmental impacts, we consider several variables that correspond to application prioritization and socioeconomic status. We first consider treatment effect heterogeneity based on the contest into which projects were admitted: the smallholder or other interested party contest. Table 6 shows that payments through the smallholder contest were more effective than those through the other interested party contest, and further, that payments to the average enrollee through the other interested party contest did not yield any positive tree cover impacts at all. Landowners in the other interested party contest were not eligible for the smallholder contest, either because they had assets exceeding the allowable threshold or a particularly large property (generally greater than 200 hectares). This means that the wealthiest and largest properties were the most cost-ineffective enrollees on average.

Table 6: Heterogeneous tree cover impacts by contest

Outcome var.	Tree Cover		Crop		Grassland	
	(1)	(2)	(3)	(4)	(5)	(6)
Intensity	0.00714**	-0.00758	-0.00163**	-0.00448	-0.00532**	0.00725
	(0.00267)	(0.00555)	(0.00072)	(0.00304)	(0.00243)	(0.0058)
Num.Obs.	133114	50407	133114	50407	133114	50407
Subsample	Smallholder	Other interested	Smallholder	Other interested	Smallholder	Other interested

Standard errors are clustered at the property level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

We next consider heterogeneity based on comuna-level poverty. Although poverty was not explicitly used by program administrators to target, higher values of $social_i$ are associated with higher comuna-level poverty (Table 7), and poverty alleviation is generally the primary goal of targeting payments to marginalized groups. We use the following regression to do so:

$$outcome_{it} = \alpha_0 + \alpha_1 \times intensity_{it} + \alpha_2 \times intensity_{it} \times ln(ComunaPov_i) + \gamma_i + \lambda_t + X_{it} + e_{it} \quad (3)$$

where $ComunaPov_i$ represents the percent of poverty in the Comuna where landowner i resides. Here, α_2 represents the parameter of interest, indicating the association of $ComunaPov_i$ with the treatment effect of the Native Forest Law subsidies. We see that projects located in comunas with higher rates of poverty actually yielded higher treatment effects. While we cannot make claims that increased poverty actually caused these increased treatment effects, it was predictive of increased treatment effects per enrolled hectare in the Native Forest Law subsidy competition, and thus, targeting these marginalized landowners did presumably improve program outcomes.

In order to better interpret the association of comuna-level poverty with program impacts, Figure 5 displays marginal effect plots for tree cover. These plots reveal how the treatment effect varies across values of comuna-level poverty. The left panel shows how the tree cover treatment effect varies across the percentage of the comuna in poverty, while the right shows how the treatment effect varies across values of its natural log. Again, we see that increased levels of comuna-level poverty are associated with greater program impacts on tree cover. The average percent of poverty within comunas of enrolled properties was 19.80% (blue line), above the country average of 16.11%. While the Native Forest Law subsidies have greater tree cover impacts with increased poverty, the subsidies are predicted to have non-positive effects on tree cover only within particularly low poverty comunas.

Table 7: Heterogeneous tree cover impacts by comuna-level poverty

Outcome var.	Tree Cover		Crop		Grassland	
	(1)	(2)	(3)	(4)	(5)	(6)
Intensity	-0.03326**	-0.03952**	0.00297	0.00393	0.03115*	0.03522*
	(0.01548)	(0.01747)	(0.00306)	(0.00313)	(0.01617)	(0.01783)
Intensity x ln(ComunaPov)	0.01631**	0.01952**	-0.00200	-0.00233*	-0.01471**	-0.01696**
	(0.00684)	(0.00790)	(0.00127)	(0.00134)	(0.00694)	(0.00789)
Subsample	Full sample	Smallholder	Full sample	Smallholder	Full sample	Smallholder

Standard errors are clustered at the property level.
* p < 0.1, ** p < 0.05, *** p < 0.01

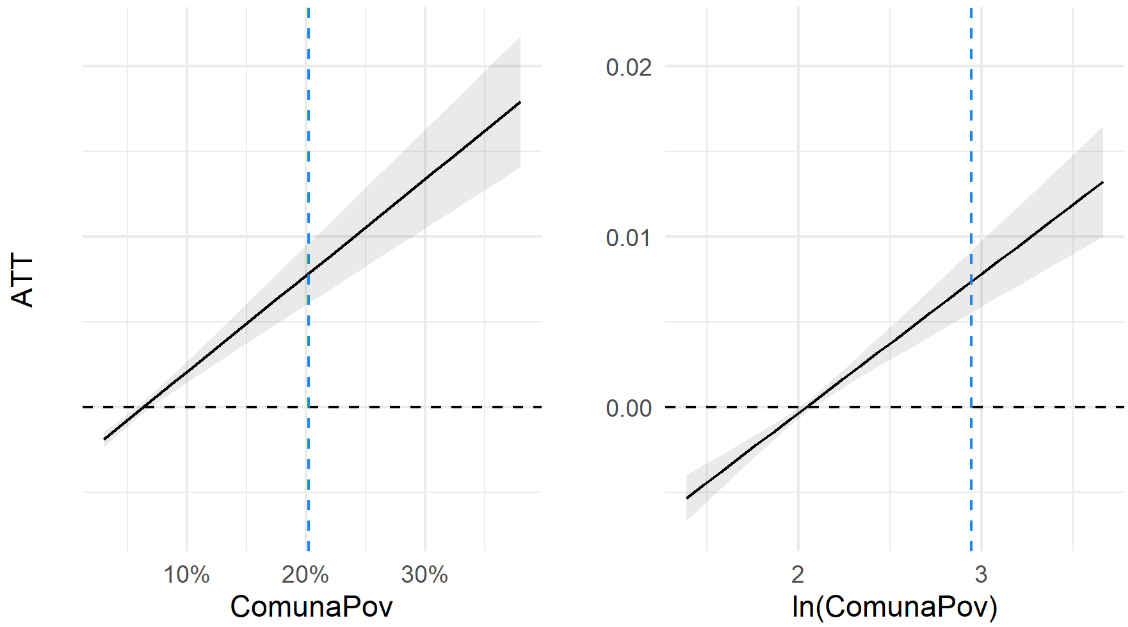

Figure 5: Marginal effects plots show how treatment effects vary across different values of comuna-level poverty. Increased comuna poverty is associated with increased treatment effects, although treatment effects are expected to be positive except within particularly low-poverty comunas. The vertical blue dashed line shows mean comuna-level poverty in each panel.

5.3 Understanding the role of non-compliance

Non-compliance in the Native Forest Law subsidy competition is high. There are multiple reasons why non-compliance may influence the effectiveness of program administrators' strategy to target for social development in payments for restoration programs. First, targeting payments toward groups that suffer from high levels of non-complance may be costly if compliance is not verified, as is often the case in low-income countries (Alix-Garcia and Wolff 2014). This is because targeting priority groups would increase the probability of unconditional cash transfers. As discussed, CONAF verified compliance in the Native Forest Law subsidy competition. By requiring both the submission of a management plan as well as third-party verification of activity completion to receive payment, CONAF saved significant resources by not paying landowners who never completed contracted activities. These compliance checks prevented payment of $28.73 million USD to landowners who never completed their planned forest restoration activities. As seen in Table 4, these projects led to no meaningful tree cover impacts, so compliance checks greatly improved overall cost-effectiveness of the program relative to the case where non-compliers were paid.

Second, if targeted characteristics are positively correlated with both program impacts and non-compliance, administrators may want to better understand how to improve compliance amongst these groups. In the Native Forest Law, nearly two-thirds of applicants fail to engage meaningfully with the program after having an application approved in the subsidy competition. These landowners showed interest in participating but ultimately chose to drop out or were unable to meet administrative hurdles. Non-compliers are inherently different than compliers, so even if priority characteristics are associated with increased non-compliance, we cannot claim that they would have produced the same tree cover benefits as similarly prioritized compliers.

Although CONAF avoided unconditional cash transfers and we cannot make claims about non-compliers' forgone impacts, the association of priority characteristics with compliance is informative for understanding the program's design. In order to examine how prioritization of social characteristics in the Native Forest Law scoring system was correlated with compliance, we use regressions of the following form:

$$complied_i = \psi_0 + \psi_1 \times ln(social_i) + \psi_2 \times ln(project_i) + X_i + u_i$$

where $complied_i$ is a dummy variable equal to 1 if landowner i followed through and received payment for successful project completion.

Table 8 shows the results of these regressions. Our coefficient of interest in these regressions is ψ_1, which captures the association of an increase in $social_i$ on compliance, holding the other parts of the project component constant. We see that higher values of $social_i$ are associated with a decreased probability of compliance in the smallholder contest, indicating that landowners who were prioritized by the program were less likely to comply. This association does not hold in the other interested party contest, however. That said, the smallholder contest saw increased levels of compliance on average, possibly due to higher payments and greater use of extensionists. Comuna-level poverty is not clearly associated with reduced compliance, holding $social_i$ and $project_i$ constant. Column 5 shows that extensionists were associated with large increases in compliance probability, perhaps indicating that some of the risk of non-compliance by priority groups can be mitigated.

The results of Table 8 suggest a few lessons for the design of payments for restoration programs where non-compliance is high. First, even when priority characteristics are associated with greater levels of non-compliance, administrators can still target these groups without sacrificing cost-effectiveness if compliance is checked. CONAF checked compliance in an inexpensive way, simply requiring submission of a management plan and verification that landowners completed the expected activities. This allowed them to target for social development without increasing unconditional cash transfers. Second, the most cost-effective enrollees conditional on compliance were also the enrollees least likely to follow-through. While untestable, if non-compliers would have provided environmental benefits comparable to similar compliers, efforts to engage and keep these landowners could improve overall program impacts without sacrificing effectiveness. Whether this is likely to be the case is left for future work.

Table 8: Social score is negatively associated with compliance

Outcome var.	Complied				
	(1)	(2)	(3)	(4)	(5)
ln(Social score)	-0.07505***	-0.07997***	0.00597	-0.08223***	
	(0.01985)	(0.02024)	(0.03622)	(0.02453)	
ln(Project score)	0.06191***	0.06305***	0.06491***	0.08496***	
	(0.01313)	(0.01340)	(0.01346)	(0.01683)	
Smallholder	0.11672***	0.11573***	0.41503***		0.18457***
	(0.01002)	(0.01023)	(0.10594)		(0.04907)
ln(ComunaPov)		0.00146	0.00345	0.00021	0.01620
		(0.01295)	(0.01297)	(0.01673)	(0.01630)
ln(Social score) x Smallholder			-0.12038***		
			(0.04229)		
ln(Subsidy amount)					0.01850***
					(0.00342)
Extensionist					0.12641***
					(0.00842)
ln(ComunaPov) x Smallholder					-0.02007
					(0.01677)
Num.Obs.	13294	12828	12828	8750	12821
R2	0.073	0.075	0.076	0.085	0.090
Subsample	Full sample	Full sample	Full sample	Smallholder	Full sample
Region FE	Yes	Yes	Yes	Yes	Yes

Standard errors are clustered at the property level.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

6 Discussion and conclusion

Prominent initiatives such as the Bonn Challenge, Trillion Trees Initiative, and UN Decade on Ecosystem Restoration hope to address the intertwined challenges of rural poverty, climate change and biodiversity loss through large-scale afforestation and reforestation. Initial national plans indicate that many countries will follow Chile's earlier model for tree cover expansion, relying heavily upon subsidies and plantation forests to achieve their commitments (Lewis et al. 2019). In light of the fact that this model may have negative impacts on native forest extent, biodiversity, and other outcomes, payments for native forest restoration may provide a more sustainable and socially beneficial path forward. Further, this may lead to increased additionality if alternative policies simply subsidize plantation forests that would have been planted anyways. In this paper, we find that payments through Chile's Native Forest Law led to native forest expansion and did so at a relatively low carbon price. These findings indicate that this model may a viable approach to designing payments for reforestation to achieve carbon removals.

Targeting for poverty alleviation in payments for avoided deforestation has often led to tradeoffs in terms of environmental efficacy (e.g., Alix-Garcia, Sims, and Yañez-Pagans 2015). However, the question of whether this strategy creates the same tradeoffs in payments for reforestation and restoration has gone unstudied. Land owned by smallholders may not suffer from deforestation risk in the absence of payments, but relatively poorer landowners may also not produce new forest without payments. This may be due to credit constraints that prevent smallholders from scaling up restoration activities. In Chile's Native Forest Law subsidy competition, we find that socially prioritized landowners generated the greatest environmental benefits conditional on compliance. Smallholders in high-poverty comunas actually saw the greatest tree cover gains per enrolled hectare. In contrast, paying larger, wealthier landowners to restore forests generated no additional tree cover or carbon benefits. While non-compliance was relatively higher amongst prioritized landowners, CONAF required project verification. This allowed program administrators to target these priority groups without increasing the probability of unconditional cash transfers, which would degrade cost-effectiveness. These findings indicate that targeting marginalized groups was not detrimental to environmental effectiveness, as has often been seen in payments for avoided deforestation, but was in fact beneficial.

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8 Appendix

8.1 Descriptive statistics

8.1.1 Property size distributions

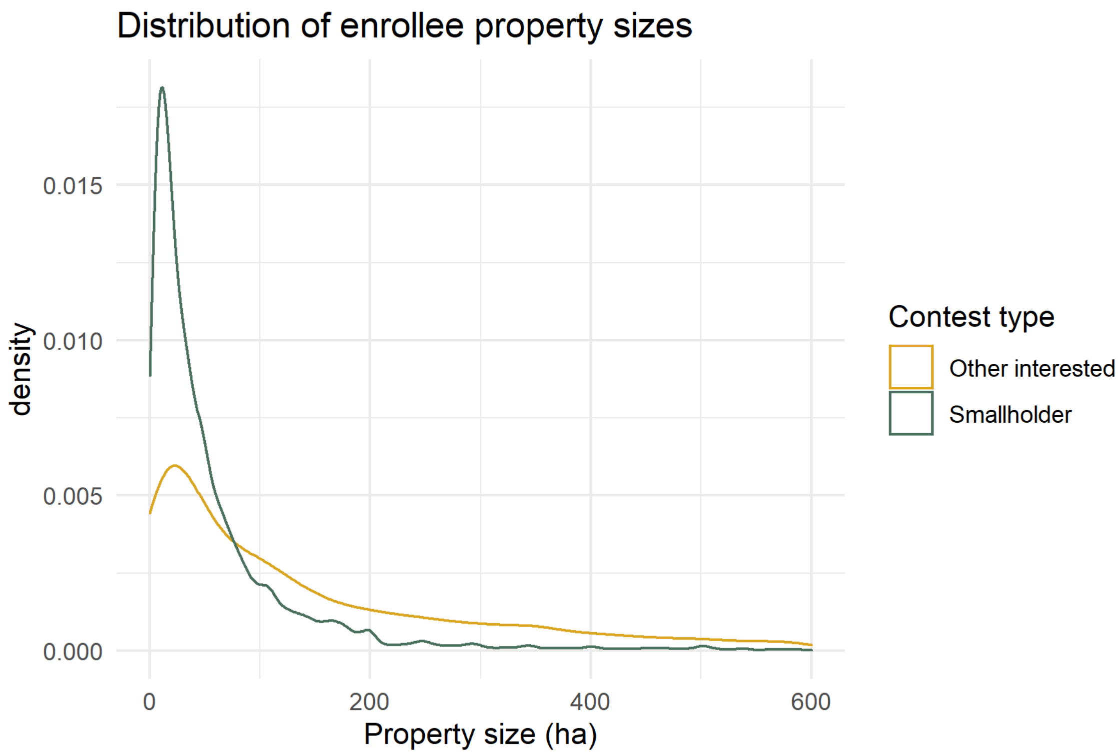

Figure 6: Distribution of property sizes amongst enrollees in both contests

8.1.2 Proportion of property subsidized

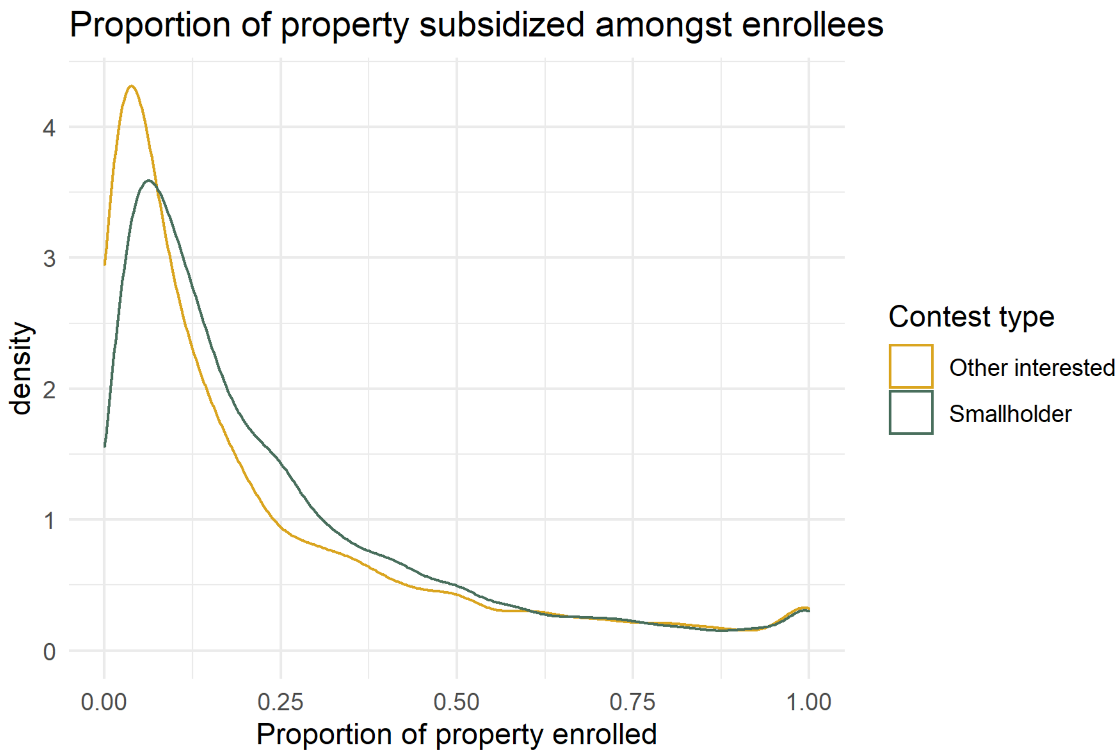

Figure 7: Distribution of proportion of property enrolled amongst enrollees in both contests

Table 9: differences in observables between paid and unpaid properties (limited to pre-2019 applicants)

Covariate	**Complier mean**	**Unpaid mean**	**T-test p value**
Award (UTM)	89.715	85.344	0.238
Bonus area (ha)	15.916	19.268	0.031
Property area (ha)	354.411	492.047	0.000
Extensionist	0.584	0.438	0.000
Timber production objective	0.888	0.866	0.019
Proportion forest	0.400	0.394	0.513
Proportion plantation	0.083	0.081	0.490
Proportion pasture	0.239	0.257	0.020
Proportion shrub	0.247	0.230	0.003
Mod-to-severe erosion	0.234	0.189	0.000
Slope	4.163	4.329	0.078
Elevation	462.335	411.319	0.000
Dist. to native timber industry (m)	18788.914	14800.298	0.000
Dist. to any timber industry (m)	11127.435	9064.145	0.000
Dist. to road (m)	3511.841	3714.166	0.079

8.1.3 Beneficiaries vs. non-compliers

Table 9 shows that landowners who follow-through differ from those who do not on a number of characteristics. This includes project and property characteristics. Notably, projects that do follow-through are likely to belong to properties with more plantation forest, but there is no notable difference in native forest. This perhaps indicates some prior experience managing or planting forests amongst compliers.

Table 10: differences in observables between properties enrolled in the smallholder vs. other interested parties contest)

Covariate	Smallholder mean	Other interested mean	T-test p value
Complier	0.351	0.241	0.000
Award (UTM)	64.527	131.264	0.000
Bonus area (ha)	11.883	28.780	0.000
Property area (ha)	132.841	1026.368	0.000
Extensionist	0.549	0.421	0.000
Timber production objective	0.866	0.888	0.016
Proportion forest	0.397	0.394	0.651
Proportion plantation	0.071	0.104	0.000
Proportion pasture	0.262	0.228	0.000
Proportion shrub	0.233	0.240	0.229
Mod-to-severe erosion	0.215	0.180	0.000
Slope	4.264	4.294	0.754
Elevation	413.225	459.626	0.000
Dist. to native timber industry	15480.370	17472.244	0.000
Dist. to any timber industry	9556.058	10155.446	0.004
Dist. to road	3840.739	3244.245	0.000

8.1.4 Smallholder vs. other interested party contest

Table 9 shows that landowners who participate in the smallholder contest differ from those in the other interested party contest.

8.2 Pre-processing

8.2.1 selected covariate distributions

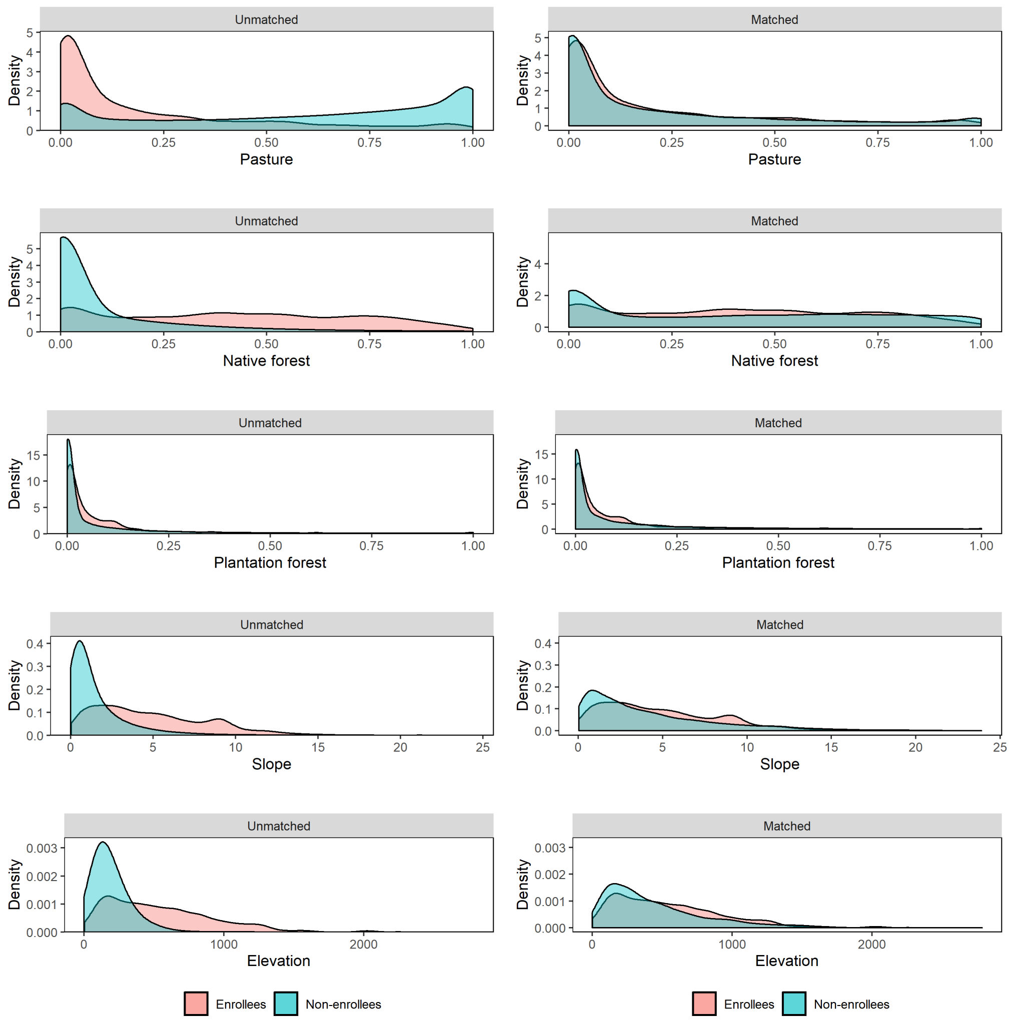

Figure 8: Covariate distributions prior to and after matching for control and enrolled complier treatment groups.

8.3 Common trends assumption

As mentioned in the main text, for β_1 from equation (1) to yield the causal effect of the program, we need to rely on a common trends assumption. In order to evaluate the plausibility of common trends in this setting, we examine the raw tree cover trends of the complying enrollees relative to the matched control group. Figure 9 shows that the matched group had slightly higher levels of tree cover, but the trends are comparable prior to the existence of the Native Forest Law. This is made particularly clear in the right panel, which adjusts for 2008 differences in tree cover to better evaluate the pre-trends prior to the implementation of the program in 2009.

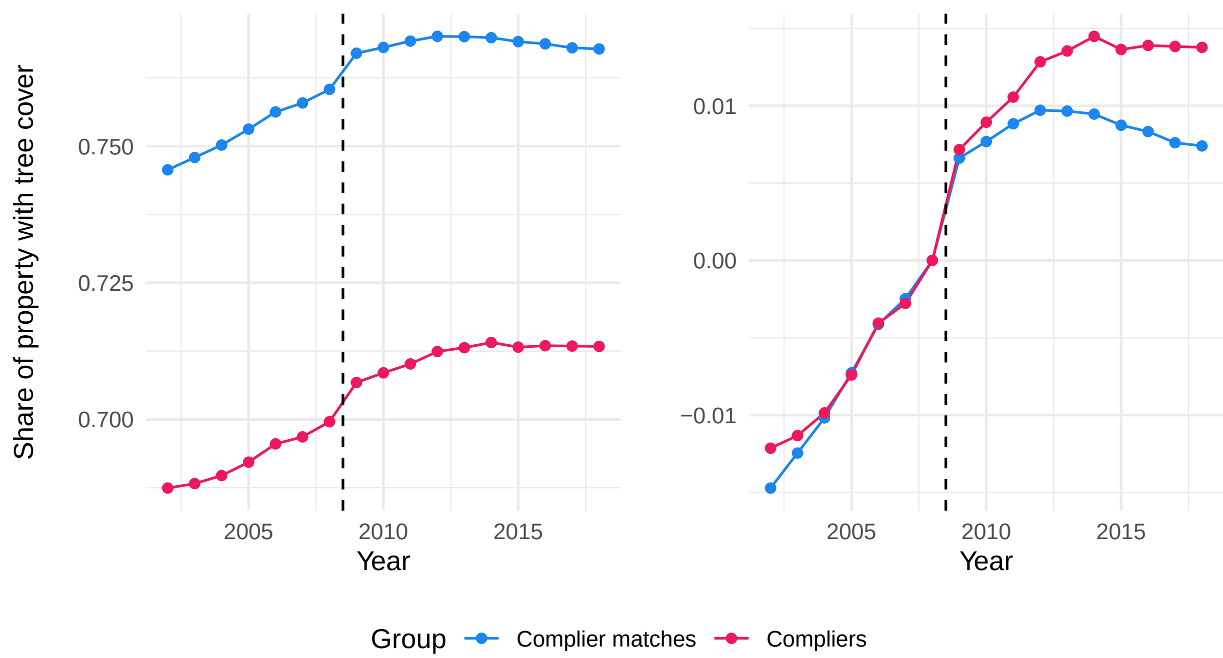

Figure 9: Raw trends in tree cover shares between enrolled properties and matched control properties. The right panel de-means the trends relative to 2008, the year prior to the first Native Forest Law subsidy contest.

We also use an event study to examine the plausibility of common trends. The event study accounts for staggered treatment in a way that examining raw trends cannot. No covariates are included, meaning that unconditional common trends are evaluated. Figure 10 displays pre-treatment pseudo-ATT estimates based on the event study.

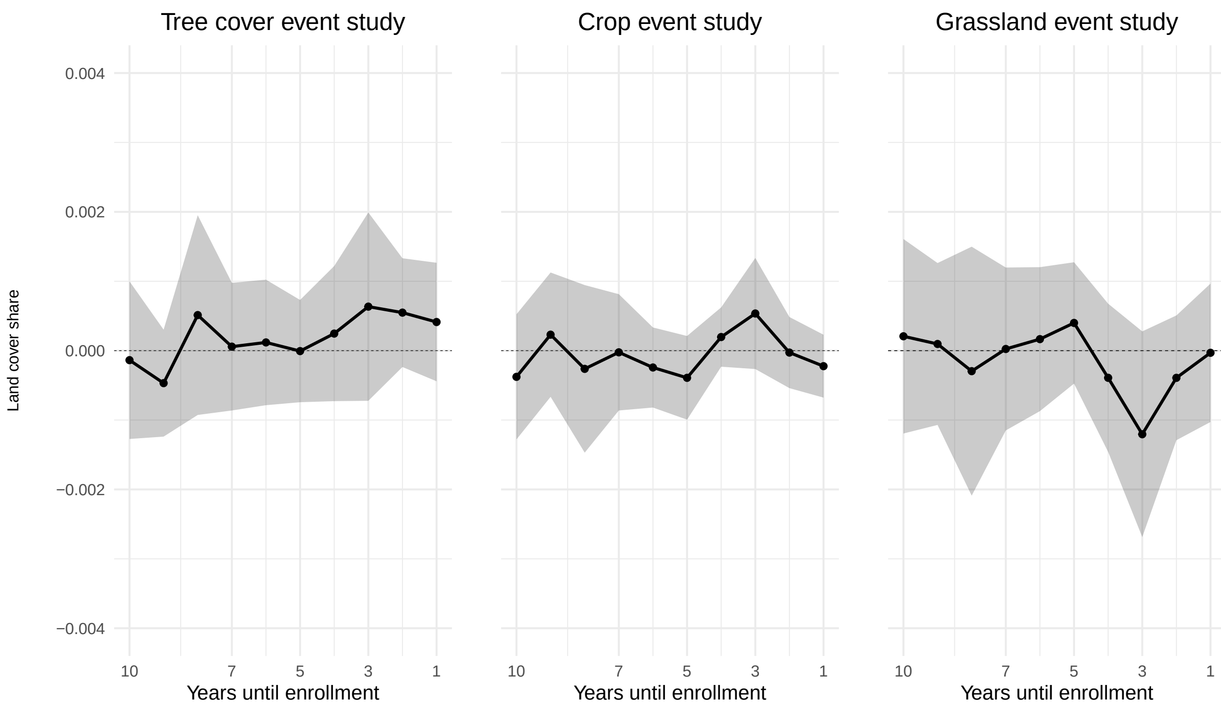

Figure 10: This figure shows pre-treatment event time treatment effects of the Native Forest Law subsidy contest for beneficiary properties based on unconditional common trends. The three panels show three land cover types: Tree cover, Crop, and Grassland.

8.4 Event-study estimates

8.4.1 Callaway and Sant'anna estimator

Recent papers have shown that the typical two-way fixed effects estimator may generate biased results in the presence of treatment effect heterogeneity (e.g., Goodman-Bacon 2018; Callaway and Sant'Anna 2020; Chaisemartin and D'Haultfœuille 2020). This could be particularly important in our case, given that there are over 150 cohort-time cells. The estimator proposed in Callaway and Sant'Anna (2020) computes each 2x2 cohort-time treatment effect ($ATT_{g,t}$) individually, before aggregating them with intuitive weights.

The estimand for each of the $ATT_{g,t}$s is as follows:

$$ATT_{g,t} = E[outcome_{it}(1) - outcome_{it}(0)|G_i = g, t \geq t_o]$$

Each $ATT_{g,t}$ then represents the treatment effect for cohort g in time t. To generate the $ATT_{g,t}$s, we first subset the data to only contain observations at time t and $g-1$, from units with either $G_i = g$ or that are in the control group. For example, for the $ATT_{2015,2019}$, we subset to only the 2015 cohort and control group for the years 2014 and 2019. Then using only the observations from this subset, we calculate $ATT_{g,t}$ using the doubly robust difference-in-differences estimator developed in Sant'Anna and Zhao (2020). This involves first estimating a propensity score using a logit model and allows for common trends to hold only after conditioning on pre-treatment covariates. With this method, we can identify the $ATT_{g,t}$s if either (but not necessarily both) the propensity score or outcome regression is correctly specified (Sant'Anna and Zhao 2020).

We focus on event study measures of the ATT. Within each event time window, we aggregate the $ATT_{g,t}$s with weights corresponding to group size.

$$ATT_{es}(e) = \sum_{g \in G} \mathbb{1}\{g + e \leq \mathrm{T}\} P(G_i = g | G_i + e \leq \mathrm{T}) ATT_{g,g+e}$$

This is the average effect of participating in the treatment e time periods after a characteristic property is enrolled in the program across all cohorts that are ever observed to have participated in the treatment for exactly e time periods. The year a property enrolls in the program is denoted by $e = 0$.

8.4.2 Balanced event study

Callaway and Sant'Anna (2020) discuss the fact that interpretation of $ATT_{es}(e)$ may be complicated by compositional changes through time. In our case, this may impact the interpretation of dynamic treatment effects if changes in the cohort composition through event time create the appearance of increasing effects through time. To determine whether this may affect interpretation of treatment effects through time, we also estimate $ATT_{bal}(e, e')$:

$$ATT_{bal}(e, e') = \sum_{g \in G} \mathbb{1}\{g + e' \leq \mathrm{T}\} P(G_i = g | G_i + e' \leq \mathrm{T}) ATT_{g,g+e}$$

The definition of $ATT_{bal}(e, e')$ is very similar to $ATT_{es}(e)$ except that it calculates the average group-time average treatment effect for units whose event time is equal to e and who are observed to participate in the treatment for at least e' periods. In our context, we set $e' = 8$, meaning that only properties observed at least 8 years after the initial enrollment year are included. Differences in $ATT_{bal}(e, e')$ across different values of e, therefore, cannot be due to differences in the composition of groups at different values of e (Callaway and Sant'Anna 2020).

8.4.3 Estimates based on only never-treated units

We explore whether exclusion of not yet treated cohorts changes our estimates. Figure 11 shows that using only the matched control group as the control yields comparable event study estimates.

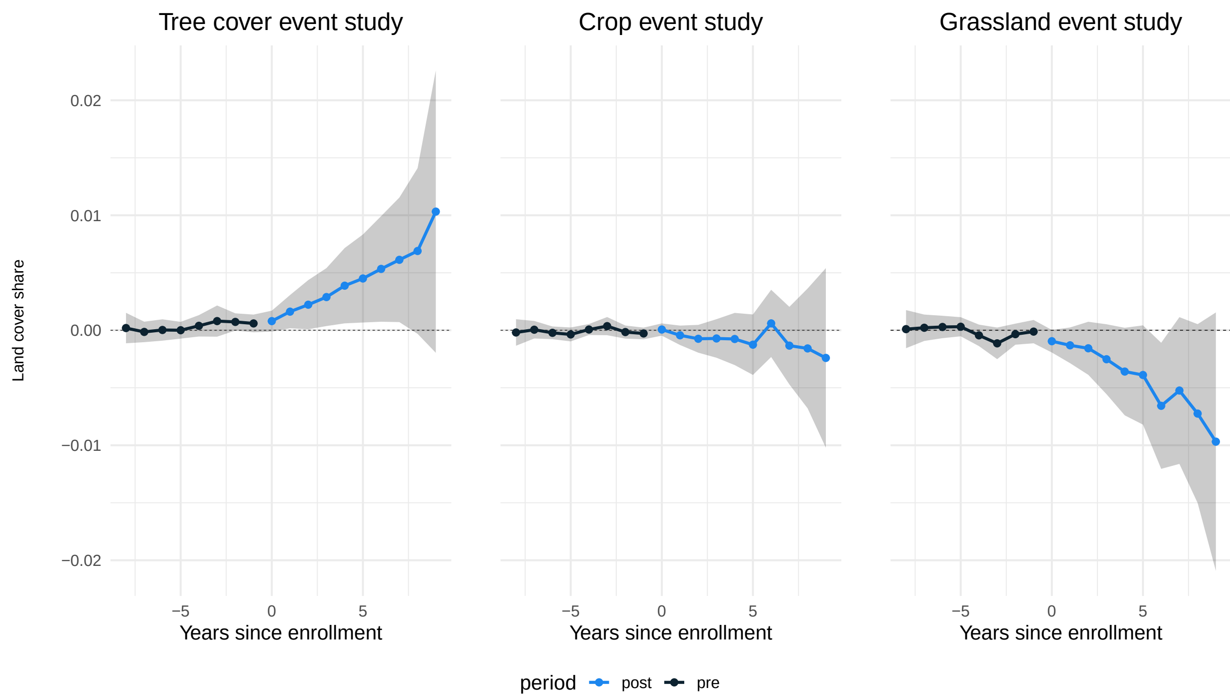

Figure 11: This figure shows event time treatment effects of the Native Forest Law subsidy contest for beneficiary properties including only the matched control group in the control group. The three panels show event time treatment effects based on binary treatment for three land cover types: Tree cover, Crop, and Grassland.

8.5 Cohort treatment effects

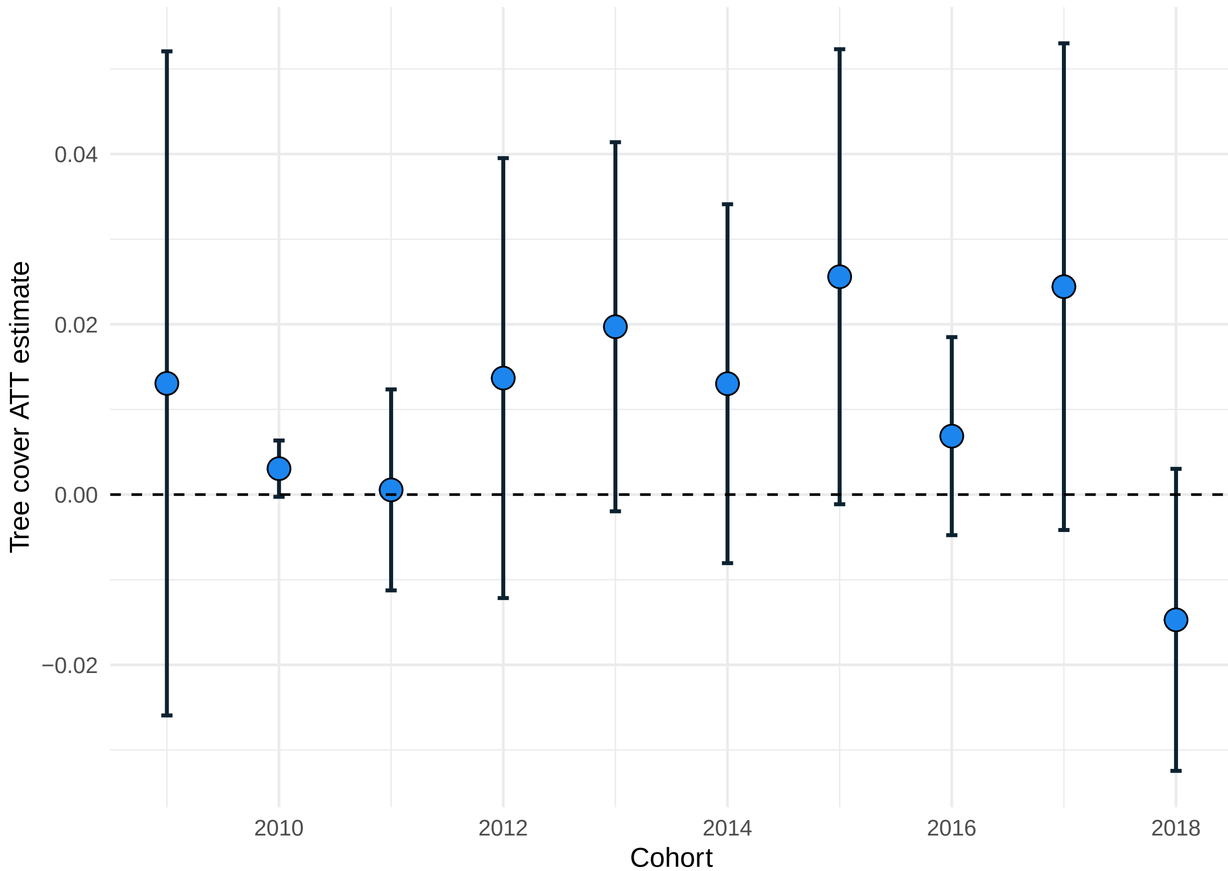

Figure 12: This figure shows tree cover treatment effects of the Native Forest Law subsidy contest for beneficiary properties based on equation 1, separated by cohort.

8.6 Rejected applicant histogram

During the annual Native Forest Law contest, applicants submit a management plan detailing the specifics of the project to be considered for an award. Judges score each application based on a number of criteria including the size of the property, project extent, specific activities to be performed, and the cost of the project. After scores are tallied, awards are dispersed in order of project score. Awards are given to the smallholder contest first, and subsequently to the other interested party contest. Thus, in years when the contest's funds run out, other interested parties generally go unfunded. In years in which the contest does not exceed the funding theshold for both groups, a second smallholder-only contest is held for any additional project applicants. These contests generate unawarded smallholders. Projects can become rejected either by scoring below the threshold that receives funding or because of unapproved proposed activities in the application itself. Thus there are two ways to get rejected.

Figure 13 shows the distribution of rejected applicants by contest across different contest years. One thing to note is that many of the rejected applicants are able to adjust their application, reapply, and enroll in subsequent years. We see that the two contests see different trends in the number of rejected applicants through time.

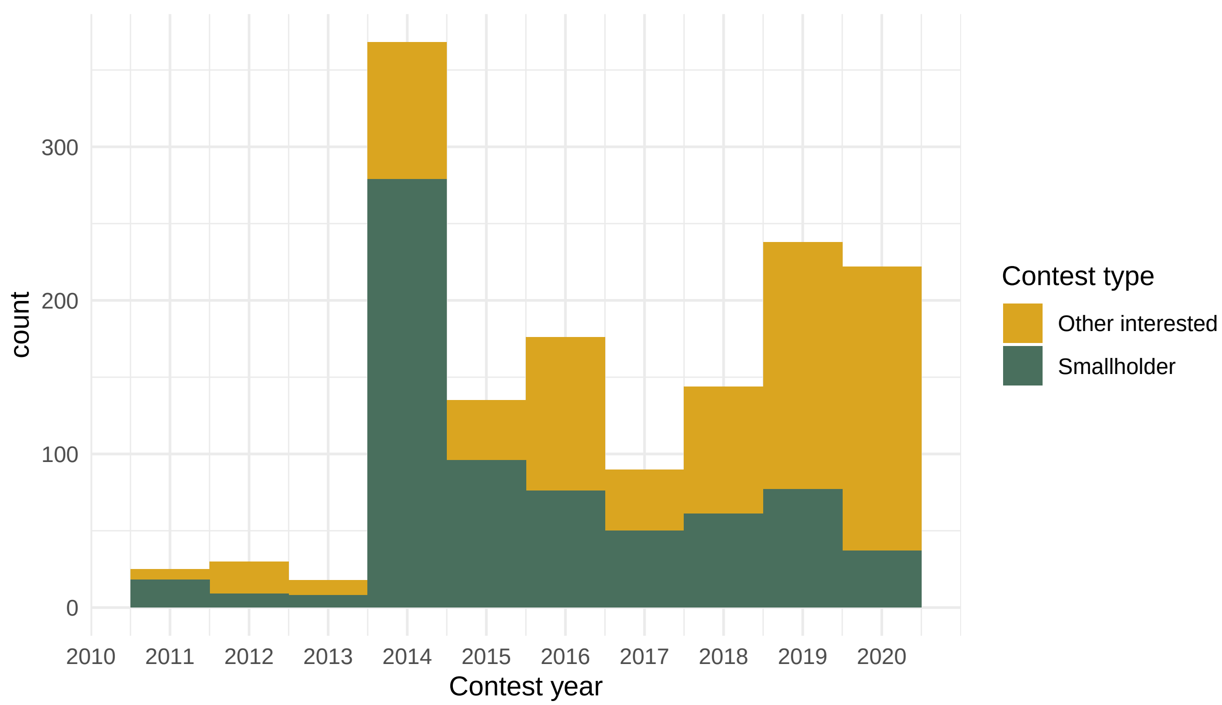

Figure 13: This figure shows rejected applicants by contest across years.