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Deforestation Impacts of Environmental Services Payments

Costa Rica's PSA Program 2000–2005

**Juan Robalino, Alexander Pfaff, G. Arturo Sánchez-Azofeifa,
Francisco Alpízar, Carlos León, and Carlos Manuel Rodríguez**



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Abstract

Costa Rica's environmental services payments program (Pagos por Servicios Ambientales, or PSA) started in 1997 and was the true pioneer in this area. It is broadly cited and has led to numerous calls for emulating its approach in various ways. It has itself evolved over time, with acknowledged shifts in focus. To measure the impacts of changed implementation, following earlier work on the 1997–2000 payments (Sánchez-Azofeifa et al. 2007; Pfaff et al. 2007), we evaluated the impact of the PSA forest protection contracts during 2000 and 2005. We found that less than 1 in 100 (about 0.4 percent) of the parcels enrolled in the program would have been deforested annually without payments, i.e., due to the net impact of the land returns in agriculture versus in ecotourism, as well as the effects of other conservation policies. This low return on investment is, to first order, the same as was seen for 1997–2000. However, we found that shifts in implementation have eliminated the bias in PSA location toward places where PSA's impact on deforestation was even lower than on average plots. Thus, we showed that the impact increased due to changes in how program parcels were chosen. However, significant potential gains can be realized by increased targeting of areas with some deforestation pressure, including with payments that differ over space.

Key Words: Forest, land, payment for environmental services, policy evaluation, conservation policies matching

JEL Classification: Q24, Q22, Q28, C21

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Introduction

The Program for Environmental Services (Pagos por Servicios Ambientales, or PSA) implemented in Costa Rica was one of the first initiatives in a developing country to focus on providing environmental services, particularly through compensation. After seeing the success of this initiative, other countries have implemented similar strategies. The Costa Rican program not only is the lead example but also permits analysis—and thus learning—of how this initiative can be improved (see Chomitz et al. 1998; Ferraro 2001; Miranda et al. 2003; Pagiola 2002; Rojas and Aylward 2003; Sierra and Russman 2006; Zbinden and Lee 2005).

Payments can promote and generate environmental services, while at the same time improving living standards in rural areas. They can create incentives for landowners to supply environmental services and can compensate those who produce these services at low cost. These

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attributes make such payments popular in policy and academic circles. What payments actually achieve depends on their impact on the actual generation of environmental services. Whether these payments affect the supply of environmental services or not is a matter of how they affect landowner decisions to avoid deforestation. Put more directly, signing a PSA forest protection contract does not assure a significant impact on deforestation. If all of these contracts went to parcels that would not have been deforested, then the program would have no effect at all on deforestation.

We estimated the magnitude of the effect of payments for environmental services on deforestation in Costa Rica between 2000 and 2005. We found that 0.4 percent—less than 1 in 100—of the parcels in the program would have been deforested in a given year if payments had not existed. This is statistically significantly different from zero, but small.

Correctly estimating the impact of this policy is challenging. If payments were distributed randomly, deforestation rates outside the land enrolled into the program would be a good estimate of the impact. However, the agency could perfectly target areas with a higher deforestation threat to increase the impact. This estimate could also be biased in the opposite direction if landowners only offered land in low deforestation threat areas.

To determine the direction of bias, one can estimate probabilities of deforestation using a model of deforestation prior to the implementation of the program (see, e.g., Pfaff et al. 2007). For the period 1997–2000, Pfaff et al. (2007) found that the deforestation rate outside payments was about 0.2 percent per year and showed that, based on a probability deforestation model for 1986–1997, areas enrolled in the program were less likely to be deforested than areas not in the program. These two pieces of evidence indicate that the forest contracts during that period saved less than 0.2 percent per year of the land enrolled in the PSA program.

An alternative approach would be to look at the characteristics of the parcels in the PSA program and on this basis seek appropriate comparison parcels whose outcomes without contracts would provide a better guess at what the PSA program prevents. This strategy is known as matching and has been used to measure the impact of payments for environmental services (see Pfaff et al. 2007). It was found that for the period 1997–2000, forest conservation contracts stopped deforestation in about 0.08 percent of the land enrolled in the program, which represented one-third of the deforestation rate outside the program. This result is consistent with the bound in Pfaff et al. (2007), but gives a specific number.

To determine the impact of payments during 2000–2005, we used matching techniques. We found an increase in the impact of the program, relative to 1997–2000. We found that the

percentage of land enrolled in the program that was actually saved annually was around 0.4 percent. This is the result of (and in this case, is similar to) the deforestation rate of all land outside the program (0.3 percent). The similarity implies that the shifts in PSA's targeting strategies appeared to eliminate the 1997–2000 bias in the location of the enrolled parcels. However, the effect is still small and perhaps could be increased if threats were targeted.

These results were generated using both covariate (Abadie and Imbens 2005) and propensity score (Rosenbaum 1983) approaches to matching. The two approaches found similar magnitude and significance, and results were robust to the number of untreated observations matched to program parcels and to specifications using different variables.

Finally, we also employed 5 km-by-5 km-grid cells as our units of observation to re-estimate the program's deforestation impacts. The results at this coarser scale are more consistent with the parcel level, and the estimate changed to 0.78 percent. Comparing this result to 1997–2000 (Sánchez-Azofeifa et al. 2007), again we see that, with background trends in rates of deforestation plus changes in targeting, the program's impact has increased. Further, more than 99 percent of the land enrolled into the program annually would not be deforested.

Better targeting of areas with high deforestation pressure is required to increase the impact of the program. Note that better targeting may need to go hand-in-hand with higher payments for areas where the opportunity costs of maintaining forest are greater. If these payments are implemented somewhere else, targeting and differentiated payments across regions will be required to obtain significant reduction in deforestation.

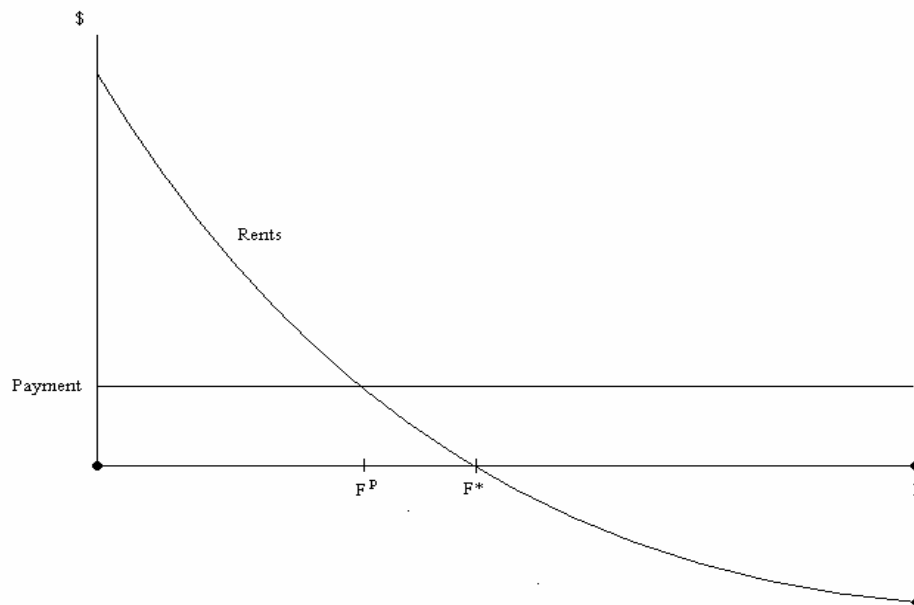
In the following section, we explain how payments affect deforestation rates. We describe the data in Section 2. In Section 3, we discuss the empirical strategies used. We show our results in Section 4. Finally, in section 5, we discuss our results.

1. Modeling Payments' Deforestation Impacts

The von Thunen model provides a simple but useful framework to consider how payments might be expected to affect deforestation. Rents are determined by the opportunity costs of keeping land in forest. In figure 1, forest land is ordered according to the rent it provides, from highest rents down to lowest. Where rents are greater than zero, the land will be deforested. Where rents are negative, land will not be deforested. In sum, in the absence of forest-linked payments, such as PSA and other such programs, deforestation will take place from 0 to F^* in the figure and forest will be maintained between F^* and L .

As implied above, payments for environmental services compete for influence in land use against the gains from various non-forest land uses. Landowners will decide to enroll their land in a forest-payments program only if the payment is larger than what they would receive otherwise. Landowners will want to enroll in the interval F^P to L .

Figure 1



However, often only a fraction of these lands will be accepted into the program. There are different reasons for why not all the land in the interval $[F^P, L]$ will be enrolled. Some landowners, even if they would benefit, might not apply either because they do not know about the program or because the application costs are too large, for example. Additionally, even if landowners apply, there is some probability that their land might be rejected, e.g., if the land is not an environmental priority or the program has run out of funds.

Of the parcels that might end up in the program, only those in the interval $[F^P, F^*]$ would modify their behavior as a result of the payments. They would be deforested in the absence of payments, but stay forested if enrolled. Thus, payment impact depends on the fraction of land enrolled that is from interval F^P to F^* . This fraction is denoted by α . If α equals 1, only land from the interval $[F^P, F^*]$ is enrolled and payments have an effect in every program parcel. If α equals 0, only land from $[F^*, L]$ is enrolled and the program has zero impact. Deforestation without the program would be the same.

We estimated α by finding locations outside the program that were similar to the parcels in the program and computing the deforestation rates for those places. If a large percentage of the places similar to enrolled parcels was deforested, payments efficiently alter forest outcomes. If only a small percentage of the comparison parcels was deforested, the program had a small impact given the resources expended.

Note that if the program enrolls all the land in the interval $[F^P, L]$, then it will not be possible to obtain the right control group, i.e., parcels that are effectively the same as those enrolled. However, we believe that some people do not apply for reasons that are not related to rents. Some simply do not know about the program or find the application or transaction costs too high. Additionally, parcels are rejected by the program because of limited funding or because they are not within the targeted environmental areas. These events create the possibility of estimating the counterfactual deforestation rate.

2. Data

We used data from three sources: 1) geographic information about the spatial distribution of forest in 2000 and 2005, 2) information about the PSA program obtained from FONAFIFO, and 3) more geographic information from the Ministry of Transport and the Instituto Tecnológico de Costa Rica.

Using geographic information systems, we randomly picked 50,000 locations across Costa Rica. On average, we had one location per square kilometer. In this study, these locations represented parcels and were our first units of observation.

We used forest cover maps from 2000 and 2005, which allowed us to determine whether a location had forest in 2000 and, if so, whether the same location had been deforested or not by 2005. We focused on deforestation behavior and PSA contracts for forest conservation. Therefore, our analysis only looked at areas with forest cover in 2000. Locations that were

covered by forest (outside of national parks) represented 25.6 percent of the land cover in Costa Rica in 2000.

Three types of payments were available: forest protection, reforestation, and forest management. Of these payments, forest protection contracts make up 92 percent of the total area enrolled in the program (FONAFIFO 2006). Therefore, we focused our analysis on forest protection contracts.

For each location, we marked the distances to the closest national road, the closest local road, the closest river, and the closest national park, as well as the distance from each location to Costa Rica's capital, San José, and to the two main ports, Limón and Caldera. Additionally, we noted the average annual precipitation, slope of the terrain, and the cardinal direction in which the slope faces—characteristics important for agricultural production. Finally, we classified each location by its life zone, based on Holdrich life-zone criteria. We divided these life zones into good, medium, and bad according to suitability for agriculture. Good life zones include all humid (medium precipitation) areas, which have moderate temperatures. The medium life zones include very humid areas (high precipitation) in moderate to mountain elevations (and thus moderate temperatures). Bad life zones include very humid areas with high temperatures, very dry hot areas, and rainy life zones, all of which are less productive.

To test robustness, we also divided Costa Rica into 5 km-by-5 km grids (following Sánchez-Azofeifa et al. 2007). For each grid cell, from forest cover in 2000 to its state in 2005, we calculated the deforestation rate for the period. We also calculated the fraction of the 2000 forest area that had forest conservation contracts during this same period.

3. Empirical Strategy

To estimate the program's impact on deforestation, we needed to determine the deforestation rate had payments not been implemented. We then compared the actual deforestation rate with the estimated counterfactual deforestation rate.

If payments were implemented randomly across all forest lands, we would only need to look at the deforestation rate outside the program for a good indicator of PSA's effect on clearing. The expectation would be that all other factors will cancel out and the only difference in deforestation inside the program and outside the program will be due to the contracts. However, the payments were not implemented randomly. Their location was driven by two forces: 1) landowners, who want to maximize profits, may only be willing to enroll land with low opportunity costs that would not have been deforested; and 2) the government, which

chooses among the offers what land will be enrolled into the program, may select land that will maximize environmental benefits or can maximize deforestation impact (as in Pfaff and Sánchez-Azofeifa 2004).

We used matching techniques to avoid bias from the non-random allocation of payments across Costa Rica. The principle of this technique is to find an adequate control group by matching each treated observation to the most similar untreated observations. Parcels enrolled in the program were compared to similar parcels outside the program.

For example, if payments were located only in low productivity agricultural areas, we would want to compare deforestation rates of low productivity agricultural areas in the program with deforestation rates of low productivity agricultural areas outside the program. Matching applies this principle to a multidimensional space.

First, we defined “similarity.” We used the characteristics vector of each parcel. One strategy for doing this is to use a distance between characteristics vectors (Abadie and Imbens 2006). Another strategy is using the probability of the parcels being enrolled in the program.

In the latter strategy, parcels in the program are compared to parcels outside the program with a similar probability of being enrolled. The probabilities are estimated using a probit model (see appendix A1 and A2) for being enrolled, with regressors being all the covariates of the treatment (Rosenbaum 1983). The basic difference between these two strategies is how the characteristics are weighted. The first, called covariate matching, gives the same weight to each characteristic. The second, propensity score matching, weights each characteristic according to its effect on the likelihood of being treated.

Once similarity was defined, we chose the number of untreated observations that were most similar when compared to each treated observation. There is a trade-off when defining the number of matches. When the number of matches increases, the variance of the estimator will decrease because it is based on more data. However, the bias will also increase because more dissimilar observations are matched to each treated observation. For robustness and transparency, we chose to show how the estimate of impact varies as the number of matched control observations for each treated point increases.

We then determined whether there was enough overlap between the treated and untreated observations. If, for a majority of the treated observations, the “distance” to their closest matches is large (i.e., their “similarity” is small), then the estimate for the treatment effect for the treated observations may not be accurate. Therefore, we needed to identify all the treated observations with enough control observations that are sufficiently similar.

We can accurately estimate the treatment effect only for these observations. Any inference about observations that do not satisfy these conditions could clearly be biased. Explicitly determining the characteristics of the groups where we have enough empirical information for inferences about the effects is one of the main advantages of matching techniques over other econometric standard analysis. Two strategies were followed to show whether this condition held. We tested whether the means of each covariate from the treated group and the matched untreated group were different statistically. We also graphed the difference of propensity scores between treated and untreated observations.

Given the adequate control group, we were able to estimate the counterfactual deforestation and compare it to the actual deforestation rate. We ran a regression using observations the treated and the matched untreated parcels with the treatment dummy to get the estimated effect plus other variables that affect deforestation rate.

The standard errors generated by the regression to statistically test significance of the results are incorrect. Some untreated observations are used multiple times when they serve as the best match for more than one treated observation, in which case the number of observations we actually had would be misrepresented. We addressed this problem by weighting units by the number of times that they actually appeared (as in Hill et al. 2003).

When we used propensity scoring to match points, the variance of the estimated propensity scores should also be taken into account in the variance of the estimator and, therefore, in hypothesis testing. Dehejia and Wahba (2002) used bootstrap methods to obtain an empirical distribution of the effect; however, this technique has been shown to fail for the nearest neighbor matching as these estimates are highly non-smooth (Abadie and Imbens 2006). Moreover, Abadie and Imbens 2006 provided a consistent estimator of the standard errors when using covariate matching, which we also chose to use.

4. Results

The deforestation rate in areas without payments during 2000–2005 was 1.40 percent, which represented a 0.28 percent rate of annual deforestation. The naïve estimate of the impact of payments from looking at deforestation rate outside the areas with payments would be 1.40 percent as seen in table 1. After controlling for other variables that also affect deforestation, we found that estimated effect actually increased to 1.62 percent. Taking into account other controls, we found that the estimates did not change significantly and that the difference between

specification 1 and specification 2 was even smaller. These results imply that deforestation would only have occurred within 0.32 percent of the land enrolled into the program per year.

The propensity score matching estimates were also similar to the naïve estimates. When using four matches per treated observation, we found that 2.00 percent of the land enrolled in the program would have been deforested in five years. At an annual deforestation rate, this implies that the effect of the program saved 0.40 percent of the land enrolled per year. By adjusting by covariates after using matching to get the comparison group, the effect decreased for both specifications. However, the changes were very small in either case.

Table 1. Effect of Payments for Environmental Services on Reducing Deforestation Far Away from Payments of Any Type (1 Km)

	No bias adjustment	No bias adjustment	Bias adjusted	Bias adjusted
	5-year effect (%)	Annual effect (%)	5-year effect (%)	Annual effect (%)
			Specification 1	
All data	-1.40 (-2.81)	-0.28	-1.62 (-3.25)	-0.32
PSM (n=4)*	-2.00 (-2.43)	-0.40	-1.66 (-2.02)	-0.33
CVM (n=4)*	-2.11 (-4.04)	-0.42	-2.09 (-4.01)	-0.42
			Specification 2	
All data	-1.40 (-2.81)	-0.28	-1.59 (-3.19)	-0.32
PSM (n=4)**	-2.32 (-2.68)	-0.46	-1.87 (-2.19)	-0.37
CVM (n=4)**	-1.89 (-3.41)	-0.38	-1.92 (-3.46)	-0.38
Grids (5x5 km)			Specification 1	
All data (OLS)	-4.37	-0.89 (-2.54)	-3.83	-0.79 (-3.64)

* Covariates from specification 1; ** covariates from specification 2; for PSM: standard errors consider repeated control observations.

Notes: Standard errors are in parentheses. Annual deforestation rate = $1 - (1 - \text{deforestation in 5 years})^{0.2}$, and vice versa for the grid regression, which was annualized before the regression.

The covariate matching showed similar estimates of impact. Again, the bias adjustment pushed the estimates downward. However, the estimates of the impact remained significant and small in magnitude. The estimated percentage of enrolled land saved per year ranged from 0.38 percent to 0.42 percent when using this approach.

We also examined how the estimates of program impact change as the number of untreated observations matched to each treated observation increased. Figure 2 presents these additional robustness checks, all of which support the conclusions we give here.

Figure 2. Estimates of Impact as the Number of Matches Increases Using Propensity Score Matching (Specification 2)

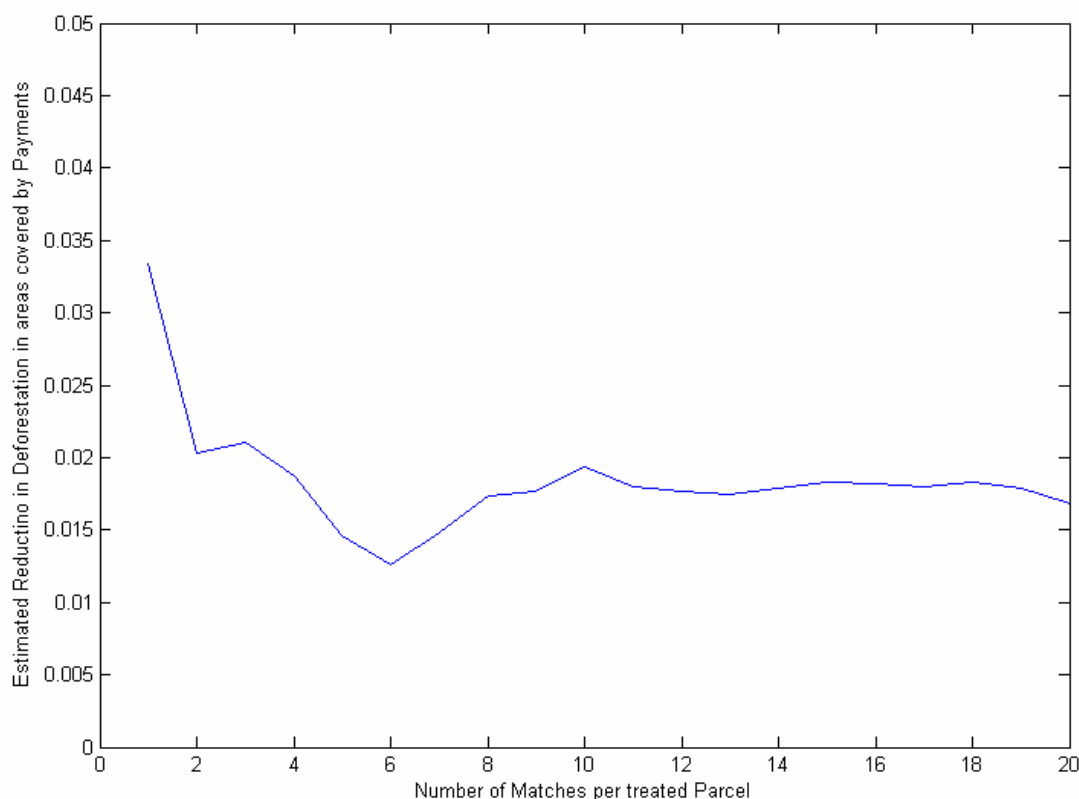


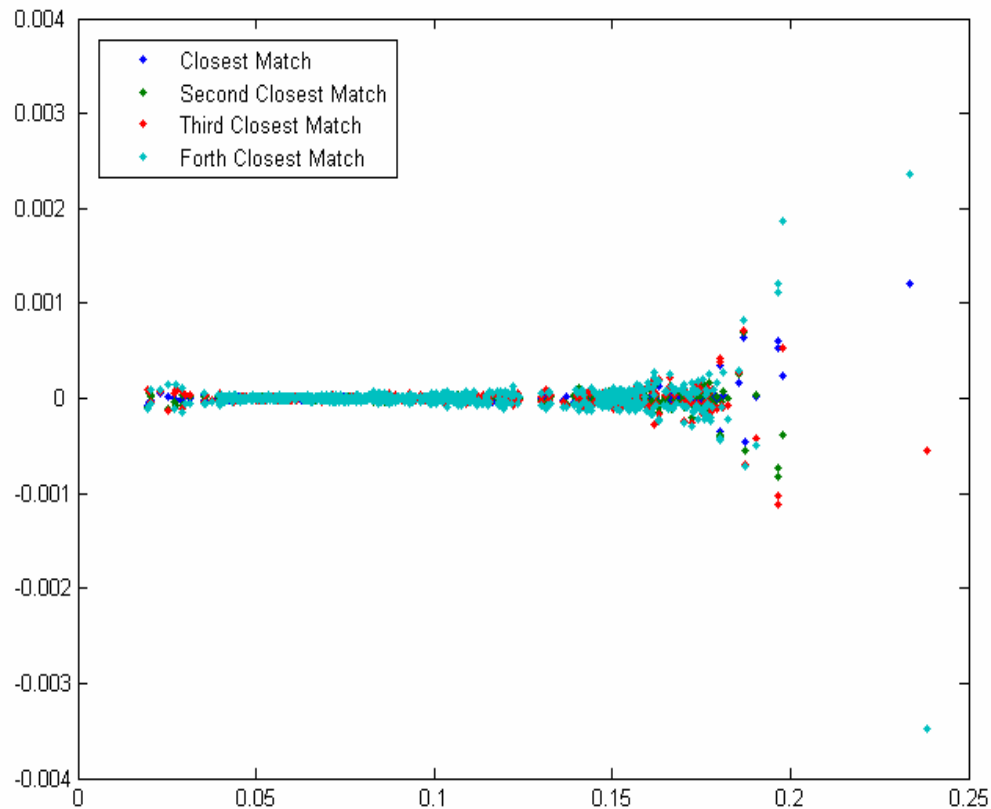
Table 2. Statistical Tests of Difference in Means between Treated and Matched Controls of Covariates

Covariates	Means of treated controls	Means of matched controls	P-value of test of difference in means	Means of all controls
Good life zone	0.2216	0.2362	0.34	0.3230
Bad life zone	0.6119	0.5897	0.21	0.4385
Distance to San José	102.7695	101.4960	0.48	114.3276
Distance to Caldera	12.1387	11.9523	0.32	12.2401
Distance to Limón	14.2676	14.4091	0.62	15.6083

Distance to local roads	3.5372	3.2338	0.02	3.3452
Distance to national roads	6.1158	5.9173	0.34	5.5252
Distance to national parks	5.1587	5.3264	0.40	5.4529
Distance to rivers	1.5877	1.5683	0.71	1.5982
Precipitation	3.5029	3.5048	0.95	3.3552
Slope	49.3546	42.7316	0.83	66.1945.

We looked at how well the matching procedures created groups of untreated observations that are similar to the treated observations by asking, first, if they were more similar to the treated than the full set of untreated, and second, if they were “the same” as the treated in the sense of statistical differences. Table 2 presents tests for each covariate of the difference in mean between the treated and the control group. We saw no evidence that, after matching, the covariates differed significantly between treated and control in the variables except one. The only variable that still seemed different was distance to local roads. However, the difference between the matching untreated and treated observations was smaller than the difference between all untreated and the treated. Figure 3 shows a scatter plot of the differences between the treated and the untreated matched observations. It can easily be seen that even in the extreme cases, the observations from the two groups are very similar except in whether they are treated.

Figure 3. Differences between Treated and Matched Untreated Observations



Finally, we noted that the estimated effects using grids were larger than the estimated effects from the rest of the estimates. The increase in the results when changing the scale of the unit of analysis may be due to the presence of interactions among landowners or spillovers (evidence in Robalino and Pfaff 2007). However, these were roughly the same, in the sense that the potential value from greater targeting once again appeared very high.

5. Conclusions

We estimated the deforestation impact of the payments for environmental services made within Costa Rica's PSA program between 2000 and 2005. We found that less than 1 in 100 (about 0.4 percent) of the parcels enrolled in the program would have been deforested annually without payments, i.e., due to the net impact of the land returns in agriculture versus in ecotourism, as well as the effects of other conservation policies. This result is to first order the same as in prior work for 1997–2000 PSA payments. It is also robust to using different matching approaches with pixel-level data and using another unit of analysis (5 km-by-5 km grid cells).

This small impact on deforestation, given the PSA resources expended, (again, this is consistent with Sánchez-Azofeifa et al. [2007] and Pfaff et al. [2007]) could well be the result of current and previous deforestation policies in Costa Rica that have significantly reduced deforestation in the entire country. Other factors, such as the reduction in the opportunity costs of deforestation and the increase in incentives of forest protection due to the booming ecotourism industry, could also have affected deforestation rates.

It is important, though, to mention that, while small, the impact has increased for 2000–2005, relative to estimates for 1997–2000. One important reason for this is that the background deforestation rate has increased, although the implementation strategies employed by FONAFIFO after 2000 appear (consciously or not) to have eliminated the bias in PSA location during 1997–2000 as well.

The higher rate of deforestation also appears to enhance the potential for the program to have greater impact, since there is deforestation to be avoided. However, to seize on this as a way to increase the impact per unit of resources spent, the agency will have to explicitly target areas with deforestation pressure. It may also need to increase some payments to enroll land that would have been cleared.

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Appendix

A1. Probit of Specification 1

Probit Maximum Likelihood Estimates	Variable	Coefficient	t -statistic	t-probability
Dependent variable = Treatment	LZG	0.119341	1.913999	0.055646
Log likelihood = -3065.1123	DSJ	-0.009203	-6.622759	0.000000
No. of observations: 10944	DCA	0.070448	6.069397	0.000000
No. of 0's, no. of 1's = 10019, 925	DLI	0.038809	5.338129	0.000000
	SDA	-0.001798	-7.918853	0.000000
	C	-1.915432	-16.832424	0.000000

A2. Probit of Specification 2

Probit Maximum Likelihood Estimates	Variable	Coefficient	t -statistic	t-probability
Dependent variable = Treatment	LZG	0.105225	1.665383	0.095865
Log likelihood = -3061.5397	LZB	0.346623	6.848824	0.000000
No. of observations: 10944	DSJ	-0.010159	-6.985146	0.000000
No. of 0's, no. of 1's = 10019, 925	DCA	0.077008	6.379139	0.000000
	DLI	0.042786	5.570062	0.000000
	DLR	-0.006803	-1.031783	0.302197
	DNR	0.008622	1.891651	0.058564
	DPA	-0.000692	-0.199027	0.842245
	DRI	-0.010185	-0.802073	0.422528
	ELE	-0.042155	-1.827106	0.067711
	SDA	-0.001807	-7.871110	0.000000
	C	-1.808663	-13.149480	0.000000

* Dummies left out medium life zones and flat land for cardinal direction of the slope.