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Can we increase the impacts from payments for ecosystem services? Impact rose over time in Costa Rica, yet spatial variation indicates more potential

Juan Robalino [a,*], Alexander Pfaff [b,*], Catalina Sandoval [c,1], G. Arturo Sanchez-Azofeifa [d]

[a] *Instituto de Investigaciones en Ciencias Económicas, Universidad de Costa Rica, Costa Rica*
[b] *Sanford School of Public Policy, Duke University, United States*
[c] *Departamento de Investigación, Banco Central de Costa Rica, Costa Rica*
[d] *Faculty of Science, University of Alberta, Canada*

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ABSTRACT

As programs with payments for ecosystem services (PES) have become more numerous, raising the need for and also the opportunity for rigorous evidence on their contributions, we examine shifts within Costa Rica's *Pagos por Servicios Ambientales* (PSA) program. The PSA was heralded from its initiation, despite demonstrations of low early impacts. We study shifts in impact over time across early periods and whether further adjustments could raise contributions. Looking over time, we find that PSA contracts signed for the 2000–2005 period had higher impacts than contracts for the program's initial time period, 1997–1999 found in previous research. Looking over space, we find that PSA payments have higher impacts for lower slopes and lower market distances. Linking these results, the rise in impact for 2000–2005 occurred alongside a shift in the targeting of PSA, which was along ecological dimensions (limiting effects of owners offering unprofitable lands). Yet the spatial variations in impacts we document suggest that explicitly targeting impact offers the potential to further raise PES impacts in Costa Rica, as well as in other nations.

1. Introduction

Payments for ecosystem services (PES) programs have risen in frequency across recent decades. Yet evidence suggests they have variable impacts, including small impacts (Sánchez-Azofeifa et al., 2007, Alix-Garcia et al., 2012, Arriagada et al., 2012, Robalino and Pfaff, 2013, Samii et al., 2014, Sims and Alix-Garcia, 2017, Börner et al., 2017, Cuenca et al., 2018, and Jayachandran et al., 2017[2]). While even a low annual impact per year could generate benefits above costs (Costedoat et al., 2012), nonetheless some guidance about where impact is higher could improve programs. We document a rise in impact, over time, as well as spatial variations in impacts which suggest that targeting impact could raise it further.

Payments that are conditional upon on keeping standing some forest that could have been deforested can in principle raise ecoservices while, at the same time, improving living standards. Transferring funds raises the popularity of the PES approach relative to, say, protected areas that often are implemented without compensation for the losses of productive private land uses. Such compensation to ecoservice providers can raise PES acceptance and adoption to socially efficient levels, while also affecting equity (via the sharing of surplus from private provision of services).

Unfortunately, past PES acceptance may have been relatively high because people were glad to be paid “money for nothing” (Ferraro and Pattanayak, 2006), i.e., to do exactly what they would have done in the absence of PES. It is not at all clear that most such incentives programs have shifted private land use toward forest. For instance, signing a forest-protection contract and fulfilling it would not lower deforestation if that parcel *would not have been deforested anyway*. Thus, since forest pressures and PES enrollments vary over time, as well as across space, we test whether the impacts of Costa Rica's famed PSA program also varied over time and across space.

* Corresponding authors.
E-mail addresses: Juan.Robalino@ucr.ac.cr (J. Robalino), alex.pfaff@duke.edu (A. Pfaff), sandovalac@bccr.fi.cr (C. Sandoval), arturo.sanchez@ualberta.ca (G.A. Sanchez-Azofeifa).

[1] The ideas expressed in this paper are those of the authors and not necessarily represent the view of the Central Bank of Costa Rica.

[2] Pattanayak et al., 2010, Samii et al., 2014, and Snilstveit et al., 2019 offer reviews. Samii et al. stress heterogeneous impacts. Snilstveit et al., 2019 emphasize limitations on PES evidence, due to the heterogeneity in the methods used.

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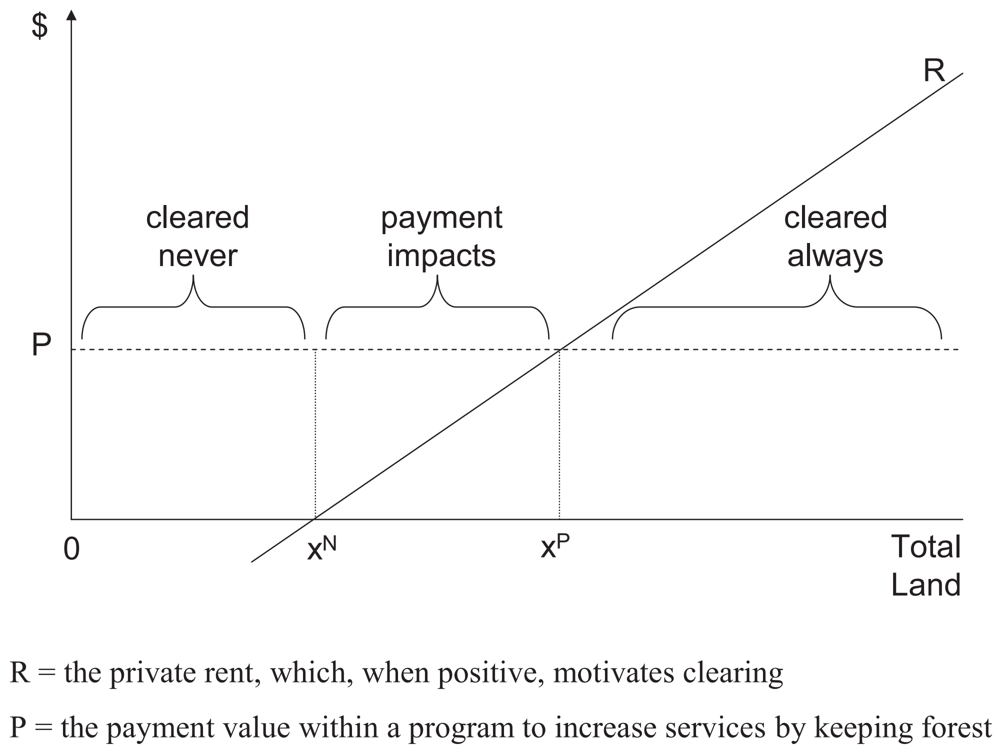

Fig. 1. Privately Optimal Land Use & Payment Impact By Location. R = the private rent, which, when positive, motivates clearing. P = the payment value within a program to increase services by keeping forest

Costa Rica's widely heralded *Pagos por Servicios Ambientales* (PSA) payments program was one of the first developing country initiatives to compensate the provision of environmental services.[3] It inspired other countries to implement PES, through not only pure imitation but also direct facilitation (see the discussions in Chomitz et al., 1998, Ferraro, 2001, Miranda et al., 2003, Pagiola, 2002, Rojas et al., 2003, Sierra and Russman, 2006, and Zbinden and Lee, 2005). Just as helpful, its early time periods permit analysis, and learning, about how impacts can vary. Rigorous impact estimates matter in light of the public claims that PSA lowered deforestation.[4]

We consider PSA's forest-protection contracts, which make up over 88% of PSA's lands during that period (FONAFIFO, 2006). Such contracts are signed only for lands that are in forest at the start of the contract. Contracts offer payments, on an annual basis, if land remains forested during the contract. Thus calculating impact requires that we estimate what the deforestation rate would have been, on the contracted lands, if the payments program had not existed. That is easy to estimate if the payments were distributed randomly, as unpaid land will be similar to paid land and thus unpaid outcomes offer a natural and likely a good estimate of what would have occurred on the paid lands without any payments. Yet there are strong reasons to expect that payments are not distributed randomly. One is that agencies can influence the selection of land into a program, so that their preferences could easily generate non-random distributions (Pfaff and Sánchez-Azofeifa, 2004). Another is that landowners have to volunteer for such programs. To maximize profit, they might be more likely to enroll parcels with low or negative agricultural profits. From the perspective of the agency, that means lands unlikely to be deforested, implying low impacts upon deforestation. Like other PES studies (Arriagada et al., 2012; Alix-Garcia et al., 2012; Robalino and Pfaff, 2013), we will use ‘matching’ in order to compare such selectively volunteered lands with similar lands.

Two questions about best designs for future PES programs, in Costa Rica and elsewhere, motivate a reexamination of the PSA program: looking over time, at PSA's initial adjustments, did PSA's average impact change in the short run? and looking across space, at the entire nation, underlying that average program impact how much did PSA impacts vary across the landscape? We care about such potential variations in PSA impacts for two reasons They can help to explain how early ecological re-targeting could have shifted the program's average forest impact. Also, they demonstrate, in practical terms, potential gains from programs' future targeting of impacts.

Comparing the PSA's 2nd and 1st time periods allows us to study the potential importance of differences in selection between the contracting periods. During the PSA's initial time period, selection was simply first-come-first-served. During the 2nd period, the government implemented environmental criteria for enrollment. Given previous research that focused on the deforestation impact during the 1st period (1997–1999, in Robalino and Pfaff, 2013), here we can compare how shifts affected impact for the same set of contracts. That can be done only for these two periods.

Theory supports targeting impacts. Basic land-use models predict varied impacts of well-enforced PES contracts – as observed in Costa Rica – since deforestation rates to be blocked by PES vary across the landscape. This implies that ecological targeting of PES could limit effects on enrollment from privately volunteering of low-profit land. It also suggests using matching to estimate impacts of 2000–2005 PSA payments, looking for unenrolled forest parcels as similar as possible to enrolled forests. We estimate average impact and, in a large spatially specific data set, split the sample in different ways into subsets that we predict should differ in impacts from PES.

Concerning change over the time periods, we find a higher PSA impact for 2000–2005. Then looking over space – matching for each subset – we find significant differences in impacts. Forest impacts from

[3] Benefits for farmers is one objective of the implementing agency (FONAFIFO, 2006) and, of course, the farmers. Given other policies which constrained farmers' land usage, these payments may function mostly as compensation.

[4] See, for instance, a highly public endorsement of those claims by Friedman (2009) in a New York Times Op-Ed.

PSA's 2nd period are higher for lands closer to cities and with lower slopes.

Below, Section 2 conveys a standard, simple conditional prediction for payment impact, along with background on Costa Rica's PSA program. Section 3 describes our data and empirical approach. Section 4 provides our results, then Section 5 summarizes and discusses implications.

2. Background & framework

2.1. Costa Rica's Pagos por Servicios Ambientales (PSA) ecoservices payments (PES) program

The legal bases for paying forested-land owners are the 1995 Environmental Law 7554, 1996 Forestry Law 7575, and 1998 Biodiversity Law. These payments officially are for specific services, i.e., allegedly are linked to values for climate change, water, biodiversity, and scenery. Yet all enrolled parcels are paid equally. PSA payments averaged US$22 to US$42/ha/year, as was calculated based on the returns from cattle ranching returns, a major competing land use.[5]

Conservation contracts require owners to protect existing primary or secondary forest for five years, paying US$210/ha in equal annual installments. Reforestation contracts require tree planting on abandoned cleared land and that resulting forest will be maintained for 15 years. Management contracts require a 'low-intensity logging' plan and ongoing ecoservices provision.

Fondo Nacional de Financiamiento Forestal (FONAFIFO), a public agency created under Forestry Law 7575, administers the PSA. Inspection responsibilities have rested with the Sistema Nacional de Areas de Conservacion (SINAC) and Ministerio del Ambiente y Energıa (MINAE). Funding was initially from a 15% tax on fossil fuels established under the Forestry Law. Article 69 stated that FONAFIFO was to receive one third of the revenue, which it rarely did. After that article was repealed in 2001, the Ley de Simplificacion y Eficiencia Tributaria assigned 3.5% of tax revenue to the program, yielding less funding in theory but more in practice (Camacho and Reyes, 2002). As of 2003, this provided an average of $6.4 million/year (Pagiola, 2002).

2.2. Simple model of payment impact

Fig. 1's simple conditional predictions frame our testing for the impacts of payments. Land is ordered by its profitability in cleared uses such as agriculture, as in the von Thunen model.

Where agricultural minus forest profit is positive, to the right of x^N, the land will be deforested. Naturally, if there are changes over time in the returns from agriculture, e.g., from increases or falls in relevant prices, then which land is expected to be cleared would shift with those shifts.

A payment P for ecoservices adds to the profits in forest, raising the hurdle for clearing. Thus, now private landowners sign up for payments in the range $[0, x^P]$, i.e., where P is larger than the relative profit earned from clearing. Forest beyond x^P is cleared. Those in $[0, x^N]$ would not modify their plans since they choose forest with payments or without. Those within $[x^N, x^P]$ are cleared without payments but not with payments, so payment impact depends on the fraction of enrollment that is from $[x^N, x^P]$. If it is 1, i.e., only land from $[x^N, x^P]$ is enrolled, all payments prevent deforestation. If it is 0, i.e., only land from $[0, x^N]$ is enrolled, then there are no impacts.

We estimate PSA impact using the deforestation observed for unpaid locations similar to paid lands. If all such similar controls were cleared, we would say that all payments saved forest (relying upon the similarity of the controls to figure out what fraction of PSA was in $[x^N, x^P]$).

As to whether we should expect that we will be able to come up with such useful controls that are similar yet outside PSA, they may exist if some landowners did not know about PSA or were turned away due to limits on funding. We stress that finding similar controls is critical here: if only $[x^N, x^P]$ enrolls, so non-PSA forest is not similar to PSA – no controls would be cleared, yet all PSA would have been cleared if not enrolled – then impact is 100%, yet seems to be 0%; conversely, if all of $[0, x^N]$ is enrolled, using $[x^N, x^P]$ as controls overestimates impact at 100%.

Very generally, an accurate estimation of the actual impacts of any such payments policy requires that some land parcels that are similar to the enrolled parcels exist outside the program. We believe that this is, in fact, the case in Costa Rica. Specifically, for both in and outside PSA, some of the land parcels would be cleared in the absence of payments while others would not be.

2.3. Related evidence on conservation impacts

Rigorous evidence concerning the impacts of conservation policies is growing, to some extent led by studies of protected areas. Despite strong location biases, due to private interests in producing on profitable lands (for a global analysis of protected areas see Joppa and Pfaff, 2009), on average protected areas have reduced deforestation (Andam et al., 2008 for Costa Rica, Joppa and Pfaff, 2010, Pfaff et al., 2015a for Brazil's Amazon, Muñoz Brenes et al., 2018 for Central America).

Yet, supporting our simple model above, there is clear evidence that impacts vary greatly across space – from well above the average for some sites (Pfaff et al. 2009 and 2015b) to zero. Blackman et al., 2015 finds 'paper parks' in Mexico, although a follow-up for post-2000 impacts (Pfaff et al., 2017) is relevant here in that the impacts of Mexican protected areas rose over time. Indicators of profitability – transport costs to markets, given distances to roads and to cities, and characteristics that affect costs and yields, like slope and soil quality – suggest protected areas in regions of higher deforestation pressure generate higher impact than those where profits are low.

Such factors can matter enough to overcome differences in, e.g., types of protected areas. Pfaff et al., 2013 for instance, for Acre State in the Brazilian Amazon, document a famous case in which protected areas with fewer restrictions go to locations with more people and more pressure even to the point that a more strictly protected area does not actually generate more conservation (supported by Nelson and Chomitz, 2011, with global evidence for fire frequency as their outcome, this has also been shown for Bolivia, Costa Rica, Indonesia, and Thailand in Ferraro et al., 2013).

Rigorous studies for PES are less abundant than in the literature for protected areas, yet they also feature heterogeneous results (Samii et al., 2014). Sánchez-Azofeifa et al., 2007 and Robalino and Pfaff, 2013 show very limited impact for PSA in Costa Rica, in its earliest years, given low average deforestation threat (noting a higher-threat region may have permitted more PES impact (Arriagada et al., 2012)). Mexico's PES also had an impact but its magnitude was limited by low baseline deforestation (see Alix-Garcia et al., 2012; Alix-Garcia et al., 2015). Also, in Brazil, there is evidence that payments have had limited impact on forest cover (Fiorini et al., 2020).

A payments program in Ecuador called Socio Bosque had a statistically significant effect in reducing deforestation (Cuenca et al., 2018). Interestingly, analyses suggest the magnitude of impact is larger for individual PES contracts than for collective contracts. Deforestation inside of PES contracts seem to be very similar between types of contracts, suggesting that the locations of private contracts differ, generating counterfactuals of significantly different deforestation rates.

For Uganda, Jayachandran et al., 2017 employed a randomized control trial in a very high deforestation context despite relatively low returns from production. There, a well-implemented, village-level intervention reduced deforestation inside enrolled villages – by over half, which is a much bigger effect in total magnitude than, say, halving

[5] US$8-125/ha/year depending on location, land type, and practices (Arroyo-Mora et al., 2005, Castro and Arias, 1998).

the deforestation rates in Mexico's PES. We note Jones et al., 2016 find a relatively high fraction of threat was blocked by Socio Bosque.

Many elements within PES program design influence effectiveness and explain how it varies globally (see for example, Wunscher and Engel, 2012; Engel, 2016; Börner et al., 2017; Wunder et al., 2020). One key issue mentioned in these analyses that could explain effectiveness is targeting (Wunscher and Engel, 2012; Engel, 2016; Börner et al., 2017; Wunder et al., 2020).

Given relatively limited impacts documented by some studies – due to design limitations or low baseline deforestation – it is not surprising that such literature has not focused a great deal upon spillovers such as spatial 'leakage', in which clearing that was discouraged on any enrolled parcel was in fact simply shifted to another parcel, negating on net any effect upon conservation (Pfaff and Robalino, 2017 discuss a number of mechanisms possibly relevant for PES spillovers). However, some spillovers do not require impact. Alpizar et al. 2015 and 2017 consider a form of "behavioral spillovers", in which those excluded from an incentive shift behaviors – despite zero changes in income or price – because they are upset at being excluded (based on past behaviors).

3. Data & empirical strategy

3.1. Data

Using GIS (geographic information systems), we randomly drew a set of 50,000 points, from across Costa Rica, on average one per square kilometer. These are our units of observation.

3.1.1. Deforestation

We obtained data on forest in 2000 and 2005 from the University of Alberta. The data were derived from satellite images (resolution $\leq 28 \times 28$ m). They allow us to see which points were deforested between 2000 and 2005. It is worth noting that we use "pixels", not parcels, since we do not observe the legal boundaries of private land holdings.

3.1.2. Payments

FONAFIFO provided sites for farms with 2000–2005payment contracts. Of the 3 types, forest-protection contracts make up 92% of the area (FONAFIFO, 2006). We focus on them and analyze forested lands, which in 2000 are 25.6% of all of the lands outside of the protected areas.

3.1.3. Covariates

From the Ministry of Transport and Instituto Teconológico de Costa Rica, for each point we find the distances to the closest national road, closest local road, closest river, and closest PA. We also get distances to the country's capital, San José, and two main ports, Limón and Caldera. We obtained the average annual precipitation, slope, and the direction in which the slope faces – all important for agriculture. Finally, we classified points by Holdridge Life Zone, then grouped them per suitability for agriculture: 'good' includes all humid (medium precipitation) areas with moderate temperatures; 'medium' is the very humid areas (high precipitation) areas in moderate to mountain elevations and hence moderate temperatures; and 'bad' includes the very humid areas with high temperatures, very dry hot areas and rainy life zones – all of which are less productive.

3.1.4. Final sample

We focus on land that was forested in 2000, dropping 27,146 observations without forest. We also must drop all parcels in other forms of protection. That accounts for 12,083 observations. Finally, we drop 803 observations that were non-forest-conservation PES, PES contracts without information about the year of implementation, PES contracts before 1997, and observations with missing values in the covariates. Thus, we are left with 9933 observations – which include 9171 observations outside PES, 556 observations in the 2000–2005 contracts, and 216 observations in the 1997–1999 contracts (including 10 observations that were in contracts during both periods).

3.2. Empirical strategy

We use matching techniques to address potential for bias that arises due to a non-random allocation of these payments across Costa Rica. The principle of matching is to find an adequate control group by comparing a treated observation with the most similar untreated observations. Should the voluntary enrollment lead PSA to lands biased toward low agricultural productivity, we would want to compare PSA with deforestation in lower productivity areas without payment.

Matching uses observed land characteristics to define similarity, albeit in different ways. For covariate matching (Abadie and Imbens, 2006), the index defining similarity is the Euclidean distance between the treated and untreated points' characteristics (after variables are normalized). For propensity-score matching (Rosenbaum and Rubin, 1983), the index is the estimated probability that the parcel would be enrolled in the PSA program as a function of its observable characteristics. One generates this estimated probability using a probit model for being enrolled (see appendix), with regressors being all the covariates of the treatment (Rosenbaum and Rubin, 1983). These strategies differ in how characteristics are weighted. Covariate matching gives the same weight to each one, while propensity-score matching weights characteristics by effects on the likelihood of being treated.

Once 'similarity' has been defined, within each matching approach, we must choose the number of most-similar-non-PSA observations that we will compare to each treated observation. There is a tradeoff. As the number rises, the variance of the estimator decreases given more data. However, bias rises, since now more dissimilar observations are used. To check robustness with transparency, we present how the impact estimate varies as the number of matches is increased.

As the number of untreated matches to each treated observation is increased, in particular we must always check again whether there is sufficient similarity between treated and untreated observations, since finding the 'most similar' controls does not guarantee finding 'very similar'. Matching is not accomplishing its goal if, for many treated observations, the "distance" to their closest matches is large, i.e., if 'similarity' remains limited despite having found the best matches. Should that be the case, the arguments to use matching versus regression would not have a basis. Thus, we need to identify which of the treated observations have sufficiently similar matches, as we should expect to improve estimation of PSA's treatment effect only using those observations. This has implications for the number of points used, not only the number of matches per treated but also the number of treated used, should we drop those with few or zero very similar matches.

Transparent documentation of whether sufficient data exist to estimate impacts well is an advantage of matching. Without documenting (non-) similarity and basing one's samples on that, the analyst faces exactly the same problem present in many analyses that ignore the non-random distribution of conservation policy locations, i.e., that treated and untreated groups could differ. Thus, for each covariate, we test whether the means of the treated and matched-untreated groups are statistically indistinguishable. In addition, to check similarity we examine the differences in propensity scores between treated and untreated observations, for each level of propensity score.

Finally, using the deforestation observed for a control group that we verified to be similar we generate an estimate of the deforestation rate which would have occurred on the treated lands had they not been treated. That can be compared to the observed deforestation on treated lands to estimate treatment impact. For that comparison, we run a regression with the treated and matched untreated (i.e., control) observations, with a treatment dummy, plus all of the covariates that are expected to affect deforestation (which helps to adjust for any bias from remaining differences in observed covariates across the treated and matched untreated groups, as matching is not perfect). For the covariate

Table 1
Descriptive statistics.

	(1)	(2)
	Non-PSA Forest in 2000	PSA Forest in 2000
Deforestation rate 2000–2005	0.0247	0*
Slope (%)	55	47
Distance to forest frontier (m)	193	304
Distance to national parks (m)	5475	5120
Distance to San José (m)	113,837	100,613
Distance to national roads (m)	3720	5209
Distance to local roads (m)	2337	3200
Distance to Caldera (m)	109,765	113,837
Distance to Limón (m)	175,671	152,386
Distance to rivers (m)	1450	1521
Precipitation (mm)	3269	3479
Elevation (m)	409	507
Life Zone Good (%)	37.15	19.42
Life Zone Medium (%)	24.96	20.50
Life Zone Bad (%)	37.89	60.07
PROVINCES		
San Jose (%)	9.73	10.61
Alajuela (%)	13.22	20.50
Cartago (%)	3.80	2.70
Heredia (%)	4.32	14.21
Guanacaste (%)	31.38	20.32
Puntarenas (%)	25.00	16.73
Limon (%)	12.55	14.93
#obs	9161	556

We suspect they were unpaid parcels owned by farmers with paid parcels as well. To not bias downward our already small impact estimates, we drop those points.
* The PSA polygons provided by FONAFIFO included 10 points that were cleared.

matching approach, Abadie and Imbens, 2006 provide a consistent estimator of the standard errors and we can simply apply that estimator when using that matching approach.

4. Results

Here we consider some descriptive statistics concerning treated and untreated observations, then different estimates of PSA's average impact in 2000–2005, all in in light of the discussion above. Our average impact estimate shows a rise over time in nationwide impact, relative to 1997–1999. We also show, though, that the average blends important variations in impact over the landscape.

4.1. Average 2000–2005 impact

4.1.1. Descriptive statistics

Table 1's initial entry conveys that during 2000–2005 the deforestation rate nationwide on forested parcels without payments was 2.47% (~0.5% per year). The second column is for PSA observations, which essentially all remained forested.[6] The simplest possible estimate of PSA's impact, then, is that across their five-year contracts the payments prevented that 2.47% clearing.

The rest of Table 1 conveys that, despite the possibility that a private desire to voluntarily enroll only low-profit lands biased PSA locations, such bias is not clear for the 2000–2005 PSA. For instance, while paid lands are farther from national roads, as well as from local roads and the port of Caldera, they have lower slope on average, plus are closer to San Jose and port of Limon.

[6] As in the note below Table 1, there were 10 points labeled as PSA yet deforested during 2000–2005. The clearing of parcels paid under Costa Rica's PSA would be surprising, given good enforcement, although of course possible. It might be, then, that 'PSA' polygons from FONAFIFO included parcels not enrolled but owned by a farmer with enrolled parcels. Our results are from analyses in which we dropped these apparently cleared points because, given our relatively low estimates of the impact from PSA, we preferred any bias to be upward, yielding an upper bound.

Table 2
Average PSA impact on 2000–2005 deforestation.

Method	PSA 00–05
Naïve	−0.0247***
OLS	−0.0244***
ATT estimates (bias adjusted)	
PSM, caliper 1% $n = 1$	−0.0299***
PSM, caliper 1% $n = 4$	−0.0246***
PSM, caliper 1% $n = 10$	−0.0245***
CM, n = 1	−0.0201***
CM, n = 4	−0.0203***
CM, n = 1 exact[1]	−0.0200***
CM, n = 4 exact[1]	−0.0202***
#obs (treated)	556
#obs	9717

ATT: average treatment effect, PSM: Propensity Score Matching, CM: Covariate Matching, "n":number of control observations matched to any given treated observation. "exact": the control observations are taken from the same province as the treated. Covariates used: distance to cities, distance to roads, distance to forest edge, distance to port, distance to rivers, distance to national parks, life zone, soil fertility, rain index, elevation, slope, province.
*** $p < 0.01$,

4.1.2. Average impact

Given the lack of a clear bias in parcel characteristics, we might not expect much bias in Table 1's simplest impact estimate of 2.47% avoided deforestation for the 5-year PSA contracts. Table 2 confirms that claim is robust, using a variety of estimates of average impact, nationwide, with matching and regressions to control, in different ways, for influences of land characteristics. If using just a regression to control for observed differences, we find essentially the same value.

Propensity-score matching estimates then either confirm this same value or, if anything, raise the estimated coefficient − although even the highest estimate is not significantly different. Further, we can see that this estimate is not sensitive to adding additional matches for each of the treated observations. That may not be surprising for such a small program − few treated points – since there should be sufficient variation in the untreated pool to find multiple similar matches.

Table 2's covariate-matching estimates are lower − at essentially 2% across a contract − yet not statistically significantly different. As noted, covariate and propensity-score matching use different weights in determining the 'most similar' untreated points for each treated observation. Combined with differences across pools in both directions, within Table 1, it is unsurprising that there would be small differences across these two approaches in matching's estimates of impact. Even these lower estimates are 80% of the simple estimate in Table 1, i.e., are much higher than, for instance, the common finding of less than 50% for protected areas (Joppa and Pfaff, 2010).

We also examine how well matching has done in finding untreated points that are similar to the treated or PSA observations. Table 3 presents, for each covariate, tests of treated-control differences for the first propensity-score-matching impact estimate in Table 2. Given Table 1, not surprisingly Table 3 provides no evidence that any of the covariates differed significantly.

4.2. Heterogeneous impacts over time

Table 4 joins Table 2 with some additional calculations in order to allow consideration of any shift over time in the forest impacts of Costa Rica's PSA program. Its first column copies an average impact estimate from Table 2, to compare with prior estimates of the 1997–1999 impacts

Table 3
Balances for Table 2's first matching estimate.

Covariate	Differences	Signif?	Covariate	Differences	Signif?
Distance to forest frontier (m)			**Slope (grades)**		
Initial difference	110.40	***	Initial difference	−7.98	**
Current difference $n = 1$	9.65		Current difference n = 1	3.60	
Distance to local roads (m)			**Elevation (m)**		
Initial difference	863.14	***	Initial difference	97.49	***
Current difference n = 1	101.68		Current difference n = 1	29.32	
Distance to national roads (m)			**Precipitation (mm)**		
Initial difference	1488.33	***	Initial difference	210.86	***
Current difference n = 1	−459.94	*	Current difference n = 1	23.02	
Distance to national parks (m)			**Province San José**		
Initial difference	−355.01		Initial difference	0.01	
Current difference n = 1	27.36		Current difference n = 1	0.01	
Distance to San José (m)			**Province Alajuela**		
Initial difference	−13,224.10	***	Initial difference	0.07	***
Current difference n = 1	209.11		Current difference n = 1	0.01	
Distance to Caldera (m)			**Province Cartago**		
Initial difference	4071.80		Initial difference	−0.01	
Current difference n = 1	3453.25		Current difference n = 1	−0.01	
Distance to Limón (m)			**Province Heredia**		
Initial difference	−23,285.80	***	Initial difference	0.10	***
Current difference n = 1	−5496.56		Current difference n = 1	−0.05	**
Distance to rivers (m)			**Province Guanacaste**		
Initial difference	71.21		Initial difference	−0.11	***
Current difference n = 1	−71.46		Current difference n = 1	−0.01	
Life Zone Good (%)			**Province Puntarenas**		
Initial difference	−0.18	***	Initial difference	−0.08	***
Current difference n = 1	−0.01		Current difference n = 1	0.03	
Life Zone Bad (%)			**Province Limón**		
Initial difference	0.22	***	Initial difference	0.02	
Current difference n = 1	−0.01		Current difference n = 1	0.02	

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

Table 4
Comparing "2000–2005 Impact" Across PSA-location Cohorts.

	(1)	(2)	(3)
	Table 2 (above)	new calculations	Difference[+] (1)–(2)
PSA-location Cohorts	2000–2005	1997–1999	
Deforestation Period	2000–2005	2000–2005	
Method			
Naïve	−0.0247***	−0.0247**	0.0000
OLS	−0.0244***	−0.0159***	−0.0084***
ATT estimates (bias adjusted)			
PSM, caliper 1% n = 1	−0.0299***	−0.0250***	−0.0049
PSM, caliper 1% n = 4	−0.0246***	−0.0228***	−0.0018
PSM, caliper 1% n = 10	−0.0245***	−0.0165***	−0.0080**
CM, n = 1	−0.0201***	−0.0092	−0.0109***
CM, n = 4	−0.0203***	−0.0130*	−0.0073***
CM, n = 1 exact	−0.0200***	−0.0091	−0.0108***
CM, n = 4 exact	−0.0202***	−0.0129*	0.0073***
#obs (treated)	556	216	
#obs	9717	9377	

ATT: average treatment effect, PSM: Propensity Score Matching, CM: Covariate Matching, "n": number of control observations matched to any given treated observation. "exact": the control observations are taken from the same province as the treated. Covariates used are: distance to cities, distance to roads, distance to forest edge, distance to port, distance to rivers, distance to national parks, life zone, soil fertility, rain index, elevation, slope, and province. [+] For PSM, a Chi2 test was used to compare the coefficients. For CM, a t-test of the mean differences between treated and CM matched controls (clustered standard errors were calculated using the matched controls IDs in case that they are used multiple times).
*** $p < 0.01$,
** $p < 0.05$,
* $p < 0.1$.

of the PSA. Those were, at most, one half of 1% over 3 years (Robalino and Pfaff, 2013). As Table 2's estimates range from 2% to 3%, over 5 years, the 2000–2005 impacts seem higher.

Table 4's second column then shows what the impacts would have been if the same sites as in the 1997–1999 PSA cohort were conserved while facing 2000–2005 deforestation pressures, as pressures matter for impacts. On the one hand, this estimate of potential 2000–2005 impact for PSA's 1997–1999 locations is never higher and sometimes is clearly lower than the actual 2000–2005 impacts. For all CM estimates, and for one PSM ($n = 10$), the differences in impact between the contracts implemented in 2000–2005 and 1997–1999 are above two percentage points and are statistically significant (see column 3 of Table 4). This suggests perhaps the adjustments in PSA locations over time, within the ecological re-targeting, raised impact by lowering the influences of voluntary enrollment of low-profit land. However, the estimated

Table 5
Heterogeneity of PSA's Impacts on 2000–2005 Deforestation: splitting the sample along important geographic dimensions.

	(1)		(2)	(3)		(4)
ATT estimates	Slope		Differences+	Distance to San José		Differences+
(bias adjusted)	high	low		high	Low	
	(1a)	(1b)	(1b)–(1a)	(2a)	(2b)	(2b)–(2a)
(1) PSM, caliper 1% n = 1	−0.0051	−0.0427*****, *	−0.0376***	−0.0113	−0.0311***	−0.0197
(2) PSM, caliper 1% n = 4	−0.0082***	−0.0368***	−0.0285***	−0.0107***	−0.0325***	−0.0218***
(3) PSM, caliper 1% n = 10	−0.0131***	−0.0356***	−0.0225***	−0.0093***	−0.0310***	−0.0217***
(4) CM n = 1	−0.0083	−0.0361***	−0.0278***	0.0000	−0.0337***	−0.0337***
(5) CM n = 4	−0.0075	−0.0339***	−0.0263***	−0.0012	−0.0327***	−0.0315***
(6) CM, n = 1 exact[1]	−0.0080	−0.0361***	−0.0281***	0.0000	−0.0337***	−0.0337***
(7) CM, n = 4 exact[1]	−0.0074	−0.0337***	−0.0263***	−0.0012	−0.0325***	−0.0312***
#obs (treated)	240	316		203	353	
#obs	4730	4987		4861	4856	
cutoff	14	14		11.61	11.61	

ATT: average treatment effect, PSM: Propensity Score Matching, CM: Covariate Matching, “n”: number of control observations matched to any given treated observation. “exact”: the control observations are taken from the same province as the treated. Covariates are distance to cities, distance to roads, distance to forest edge, distance to port, distance to rivers, distance to national parks, type of life zone, soil fertility index, rain index, elevation, slope and provinces. + For PSM, a t-test from a regression with the all the treated and the PSM matched controls with a dummy indicating the low sloped sample and interacted with the treatment variable and with the covariates was used. For CM a *t*-test of the mean of the differences between treated and the CM matched controls was used (including clustered standard errors using as grouping variable the matched controls IDs in case that they are used multiple times).

*** $p < 0.01$,

** $p < 0.05$,

* $p < 0.1$.

potential 2000–2005 impacts for PSA's 1997–1999 locations also, as in Table 4's first column, are higher than the 1997–1999 estimated impacts for the same locations. This suggest that a rise in average pressure over time[7] could have played a role (much as shown by Haruna et al., 2014 for Panama impacts over time).

4.3. Heterogeneous impacts over space (indicating the potential for future targeting of impacts)

Table 5 strongly supports the simple conditional prediction generated from Fig. 1, i.e., that PSA's impact could vary significantly as a function of where payments are in the landscape. For Table 5, the role of factors affecting profits in agriculture, as depicted in Fig. 1, is played by both the slope of the pixel in question and the distance of that pixel from the capital, San Jose. Higher slope and greater distance from such a major market should lower profitability, making it less likely that any forested land in those conditions would be cleared in the absence of payment.

The estimates from applying either matching approach to our subsets of the treated points support this conjecture. Table 5's propensity-score-matching estimates are 2–4 times as large for the higher profitability sites of PSA, with lower slopes and distances, as for the lower-profit sites (and as seen in columns 2 and 4, those differences are statistically significant). We also provide covariate-matching impact estimates, which for payment subsets with higher slopes are roughly 4 times as large (note the lower-profit estimates are insignificant given correct standard errors). For payment subsets by distance, again the low-profit areas show insignificant treatment effects, and further they are quite close to zero – making the differences across these subsets very clear.

[7] This is confirmed by nationwide satellite data. Net deforestation was actually negative for Costa Rica as a whole, however that is because more deforestation occurred than during 1997–2000 but was outweighed by reforestation.

5. Discussion

We estimated the deforestation impacts of the payments for environmental services (PES) made by Costa Rica's PSA program during 2000–2005. We found that about 2.5% of the land enrolled appeared to have been prevented from being deforested, over the life of the 5-year PSA contract. This result is quite robust to various approaches to controlling for observable land characteristics, including variations of different types upon both propensity-score and covariate matching efforts. Further, this estimate of annual impact is higher than found for the previous 1997–1999 contracts.

Yet we also demonstrate significant heterogeneity in PSA's impacts across the landscape, which suggests that considerably larger gains in average impact could be possible with targeting. Some subsets of the landscape appear to have generated no PSA impacts at all, while others have impacts well above the average. Our result is consistent with the model of land use we presented. It predicted that, with perfect enforcement, impact would vary with the private baseline clearing which, in turn, varies with the determinants of profitability such as slopes and market distances.

These results, rigorously documenting significant variations in the impact from payments, suggest the potential for increasing the impacts from ecosystem services payments via targeting. While of course other motivations will affect implementation, beyond maximizing local impact, these findings suggest understanding of varied private baseline pressures could inform selection. That said, any selective targeting could also have downsides (e.g., Alpizar et al., 2017a, 2017b find a potential for negative reactions by those excluded – depending on the rule utilized for exclusion, in particular if it is based upon prior behaviors). Nonetheless, heterogeneous payment impacts documented here motivate at least the consideration of any potential net benefits from targeting.

It is important to keep in mind that our matching results, making use of similar controls, relied upon an assumption of no unobserved factors being correlated with the use PES and the rate of deforestation. They could bias results. We argue that as in previous research (e.g., Andam et al., 2008; Herrera et al., 2019), the variables we use to match can significantly reduce bias. Further, the fact that during the time period we study the supply of land for PES was greater than the area which could be enrolled suggests we had sufficient parcels to find truly similar controls.

Declaration of Competing Interest

We report no conflict of interest.

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