Deforestation Impacts of Environmental Services Payments: the evolution (2000-2005) of the Costa Rican PSA program

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Abstract

Costa Rica's environmental services payments (Pagos por Servicios Ambientales or PSA) program started in 1997 and was the true pioneer in this area. It is broadly cited and led to numerous calls to emulate the approach in various ways. It has itself evolved over time, with acknowledged shifts in focus. To evaluate the impacts of changed implementation, following prior work on the 1997-2000 payments (Sanchez et al. 2007, Pfaff et al. 2008) we evaluate here the impact of the PSA forest protection contracts during 2000 and 2005. We find that less than 1 in 100 (about 0.4%) of the parcels enrolled in the program would have been deforested annually without payments, i.e. due to 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 found for 1997-2000. However, we find that shifts in implementation have eliminated the bias in PSA location towards places where PSA's impact on deforestation is even lower than on average plots. Thus, we show a conservation impact of the changes in how program parcels are chosen. Yet it would appear that there remain significant potential gains from increased targeting of areas with some deforestation pressure, including with payments that differ over space.

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1. Introduction

The payments for environmental services implemented in Costa Rica were one of the first initiatives in a developing country to compensate for provision of environmental services. Following Costa Rica's lead, other countries are trying to develop and implement similar strategies (see, e.g., Rosales 2003, Echavarria et al. 2004). The Costa Rican program both is an inspiring example and permits analysis and learning about how such programs could 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).

In principle, payments can induce environmental services provision while at the same time improving living standards in rural areas. A payment both shifts the incentives towards forest and, for owners who accept payments, provides returns above those from alternative non-forest land uses. These attributes have made payments a popular policy.

However, what payments actually achieve depends on how they affect landowner decisions relevant for forest cover. Put more directly, the signing of PSA forest protection contracts does not assure an impact on deforestation. If contracts go to parcels that would not have been deforested, such a program would have no impact at all upon deforestation.

To estimate the program's deforestation impact, we need to determine what would have been the deforestation rate if the payments had not been implemented. We can then compare 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. In expectation 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, there are good reasons to believe payments are not randomly located. Two examples of relevant influences make this clear. On one hand, the government can influence the selection of land into the program and might prefer to maximize its impact upon forest area by targeting deforestation threats and trying to block them (as discussed in Pfaff and Sanchez 2004). If so, then the average deforestation rate outside of the PSA will underestimate the program's impact. On the other hand, since the PSA involves the choice by landowners of whether to offer their land to the program, landowners with the objective of maximizing profits might only enroll land with very low (and even negative) potential profit in agriculture, i.e. lands that would be very unlikely to be deforested even if there were no payments. If so, PSA's impact could be very low and even could be zero.

Such influences on program enrollment make it challenging to correctly estimate the impact of the program. One useful strategy is 'matching', i.e. take the characteristics of parcels in PSA and find the most similar non-PSA parcels, so that their outcomes will provide the best possible guess at what would have happened on PSA lands without PSA. This strategy has been used to estimate the impact in Costa Rica not only of ecoservices

payments 1997-2000 (Pfaff et al 2008) but also of protected areas (Andam et al. 2008, Pfaff et al. 2007). Here we apply this matching approach to the PSA program after 2000.

There are two main motivations for examining payments again, for a later period. First, the number of such careful studies of impact, controlling for non-random location, is exceptionally small. Thus questions may exist about whether the details of the analysis drove prior results, for instance whether time period or scale of the data were influential.[1]

Second, we can go beyond confirmation of prior policy results to novel results, specifically examining for the first time whether a shift in policy did influence outcomes. The PSA program is acknowledged to have shifted its focus over time for parcel choice. While many variations upon such a policy are possible, as we will discuss further below, here we can examine whether an actual, intentional targeting effort affected PSA impacts.

Thus, we want to apply the state of the art in such evaluation, i.e. do matching, for the 2000-2005 period. We apply both propensity-score matching (Rosenbaum 1983) and covariate matching (Abadie and Imbens 2005). We find results of similar magnitudes and significance, with robustness to various matching choices, within these two approaches.

Specifically, to start we confirm the first-order conclusion about PSA 1997-2000 found in Pfaff et al. 2008. They found that forest conservation contracts prevented deforestation in just under 0.1% , i.e. less than 1 in 1000, of the parcels enrolled in PSA. For 2000-2005, here we find that 0.4%, i.e. closer to 1 in 250, of parcels were saved. To first order, clearly both results show very low impacts on deforestation from payments.

It is important to recognize that this does not mean all payments programs would have such low impact. Context is crucial and Costa Rica differs from other settings in terms of other policies that greatly lower deforestation, leaving the payments little to do. The difference between the 1997-2000 and 2000-2005 results actually conveys the point that the background socioeconomic processes driving deforestation powerfully bound what the payments can do. While during 2000-2005 there was net national reforestation, in fact there was a little more gross deforestation, i.e. a bit more for payments to prevent.

Our second main result concerns a change relative to the 1997-2000 PSA period. While targeting post-2000 can not change that there was little deforestation to prevent, one significant impact was to eliminate the bias in location resulting from policy design.

In 1997-2000, PSA's impact was only one third as large as national deforestation because, it seems, landowner choice biased enrollment towards land that would not have been cleared even without PSA. For 2000-2005, though, PSA impact is about the same as the deforestation outside of the program (i.e., 0.3%). Thus, while impact on forest cover

[1] We note, for instance, that Sanchez et al. 2007, using 5x5km gridded data, find in regression analysis that there is no statistically significant association between deforestation and density of parcels enrolled in PSA. This result is quite consistent with the very low impact results from matching discussed below. However, Pagiola 2008 suggests, after looking at multiple studies, that data scale and time period both could matter. Thus here we also note consistent preliminary 5x5km gridded data results for the 2000-20005 period too.

does not appear to have been the goal, the shift in targeting of PSA increased efficiency. This suggests that other shifts which aim to increase efficiency could achieve even more.

Below, Section 2 shows in a simple way how payments can affect deforestation and the challenges to correctly estimating impact. Section 3 describes the data. Section 4 discusses our empirical strategy, with results in Section 5. Finally, Section 6 concludes.

2. Land-Use Choice with Payment Option

The simplest model of a land owner maximizing returns is useful for communicating several issues that constrain payment impact and its estimation. Figure 1 orders land according to the profitability of clearing, i.e. profits in clearing minus profits in forest, with agriculture more favored to the right. Where the relative profits are greater than zero, the land is predicted to be deforested. With no payments, forest will remain within $[0, x^N]$ while the forest will be cleared from the rest of the land, i.e. to the right of x^N.

As ecopayments compete against non-forest land uses, landowners sign up for payments in $[0, x^P]$, where the payment is larger than other returns. Not all who apply would modify their behavior as a result of the payment. Those in the interval $[0, x^N]$ would not; with or without payments, their land will be forested. In contrast, the parcels in the interval $[x^N, x^P]$ would deforest in the absence of payments but not if being paid.

Program impact depends on the fraction of enrollment from $[x^N, x^P]$, denoted α. If α equals 1, i.e. only land from $[x^N, x^P]$ is enrolled, all payments prevent deforestation. Yet if α equals 0, i.e. only land from $[0, x^N]$ is enrolled, then payments have no effect.

We estimate α by finding non-PSA locations similar to the parcels in the program and computing deforestation rates for those places. The fraction of those places that was cleared is an estimate of α. If all were cleared, all were from $[x^N, x^P]$ and so α equals 1; put another way, then the PSA payments program would be said to be 100% efficient.

Note that not all of the parcels within $[0, x^P]$ will apply, as some land owners may not know about the PSA program and/or face excessively high application costs. Further, not all those who apply are guaranteed to be enrolled. The PSA may not have the funds.

Assuming that all of $[0, x^P]$ apply, which parcels are enrolled affects the accuracy of simple impact estimates. If $\alpha = 1$, i.e. targeting is good so all of $[x^N, x^P]$ is enrolled but nobody from $[0, x^N]$ is, then forest outside PSA will be in $[0, x^N]$. Those are not locations similar to the enrolled. None will be cleared, though all of the enrolled would have been, and thus α would be underestimated at zero even though all payments had impact. Should $\alpha = 0$ in fact, it would be overestimated, at one, though the payments made no difference.

Generally, accurate estimation of α requires that there be parcels outside the PSA program that are similar to those enrolled in PSA. We believe that this is the case, i.e. that both parcels which would be cleared without payments and parcels which would not can

be found both within the PSA program and outside. This is supported by the observations that while some owners did not know about the program, still PSA was oversubscribed.

3. Data

We use data obtained from three sources: first, geographic information about the spatial distribution of forest in 2000 and 2005; second, information about the PSA (payments for environmental services) program, obtained from FONAFIFO; and third, more geographic information from the Ministry of Transport and the Instituto Teconológico de Costa Rica.

We randomly drew 50,000 locations from across Costa Rica. On average, then, we have one such location per square kilometer. In this study, these locations represent parcels and will be our primary scale of analysis, i.e. our primary units of observation.

We use forest cover maps from 2000 and 2005. This allows us to determine if a location had forest in 2000 and, if so, if the same location was deforested or not by 2005. We focus on deforestation behavior and PSA contracts for forest conservation. Therefore our analysis only looks at areas with forest cover in 2000. Locations that were covered by forest (outside of national parks) represent 25.6% of the land cover in Costa Rica in 2000.

We have also obtained information from FONAFIFO on locations that received payments for environmental services during those years. Of the three types of payments, those that we focus on, the forest protection contracts, make up 92% of the total area in protection or reforestation or forest management contracts (FONAFIFO 2006).

For each location, we find the distances to the closest national road, the closest local road, the closest river the closest national park. We also find the distance from each location to the country's capital, San José and to the two main ports, Limón and Caldera. Additionally, we obtained spatial information about average annual precipitation, slope of the terrain and the cardinal direction in which the slope faces. These three characteristics are important for agricultural production. Finally, we classify each location by its life zone. These zones are based on Holdrich Life Zone criteria. We divided these life zones into good, medium and bad according to suitability for agriculture. Good Life Zones includes all humid (medium precipitation) areas, which have moderate temperatures. The Medium lifezones include very humid areas (high precipitation) in moderate to mountain elevations (and hence moderate temperatures). Bad 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 employ a division of Costa Rica into grids of 5km by 5km (as in Sanchez-Azofeifa et al 2007). For each grid cell, from forest in 2000 and in 2005 we calculate the deforestation rate for the period. We also calculated the fraction of the 2000 forest area that received forest conservation contracts during this same period.

4. Empirical Strategy

To estimate the program's deforestation impact, we need to determine what would have been the deforestation rate if the payments had not been implemented. We can then compare 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. In expectation 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, there are good reasons to believe payments are not randomly located. Two examples of relevant influences make this clear. On one hand, the government can influence the selection of land into the program and might prefer to maximize its impact upon forest area by targeting deforestation threats and trying to block them (as discussed in Pfaff and Sanchez 2004). If so, then the average deforestation rate outside of the PSA will underestimate the program's impact. On the other hand, since the PSA involves the choice by landowners of whether to offer their land to the program, landowners with the objective of maximizing profits might only enroll land with very low (and even negative) potential profit in agriculture, i.e. lands that would be very unlikely to be deforested even if there were no payments. If so, PSA's impact could be very low and even could be zero.

We use matching techniques to address bias arising from non-random allocation of payments across Costa Rica. The principle of matching is to find an adequate control group by pairing each treated observation with the most similar untreated observations; thus parcels enrolled in the program are compared to similar parcels outside the program.

For example, the payments may be biased towards low agricultural productivity. If this were the case, we would want to compare deforestation rates in low productivity agricultural areas outside the program with the deforestation observed within the PSA. Matching applies this principle using multiple characteristics to define plot similarity.

We must define specifically what 'similarity' is, using the parcel characteristics. One index used to define similarity is the Euclidean distance between the characteristics vectors after the variables have been normalized (Abadie and Imbens 2006). Another is the probability that the parcel would be enrolled in the PSA program (Rosenbaum 1983).

In the latter strategy, parcels in the program are compared to parcels outside the program with a similar probability of being enrolled. The probabilities used to do this are from by a probit model (Appendix A.1) 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 former, called covariate matching, gives the same weight to each characteristic. The latter, i.e. propensity score matching, weights each characteristic according to its effect on the likelihood of being treated.

Once similarity is defined, we choose a number of similar non-PSA observations to compared to each PSA observation. There is a tradeoff here. As the number increases, the variance of the estimator decreases as it is based on more data. However, bias rises

because more dissimilar observations are used. To check robustness with transparency, we present how the impact estimate varies as the number of matched observations rises.

We then determine if there is 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. "similarity" is small, then the estimate about the treatment effect for those treated observations might not be accurate. Therefore, we need to identify all the treated observations with enough control observations which are sufficiently similar, since we can accurately estimate the treatment effect only for those observations.

Explicitly determining in what conditions we have enough empirical information to estimate impacts is one of the main advantages of matching. If we do not impose this rule about similarity, we have exactly the same problem faced in analyses that ignore the non-randomness of the PSA location, that the treated and untreated groups are different. For imposing this rule, we test if the means of each covariate from the treated group and from the matched untreated group are statistically indistinguishable. In additionm we examine the difference of propensity scores between treated and untreated observations for each level of propensity score to check if particular subgroups of the groups differ.

Given a control group, we will estimate counterfactual deforestation and compare it with actual deforestation. We will run a regression using the protected and the matched unprotected points with a protection dummy and including other covariates expected to affect deforestation rates. The standard errors from such a regression are incorrect (even with bootstrapping, as per Abadie and Imbens 2006). Following Hill et al. 2003, we start to (but do not fully) address the issues with the standard errors by weighting unprotected observations using the number of times they are included as controls for protected points. For the covariate matching, Abadie and Imbens 2006 provide a consistent estimator of the standard errors. We simply apply that estimator when using that matching approach.

5. Results

The deforestation rate in areas without payments during 2000-2005 was 1.40%, which represents a 0.28% rate of annual deforestation. The naïve estimate of the impact of payments from looking at deforestation rates outside would be 1.40% (as in Table 1). After controlling for other variables that also affect deforestation, we find that estimated effect actually increases to 1.62%. Taking into account other controls, we find that the estimates do not change significantly and that the difference between specification 1 and specification 2 is even smaller. These results imply that deforestation would only have occurred within 0.32% (i.e. less than 1%) of the land enrolled into the program per year.

The Propensity Score Matching estimates are also similar to the naïve estimates. When using four matches per treated observation, we find that 2.00% 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 is saving 0.40% of the land enrolled per year. By adjusting by covariates after using matching to get the comparison group, the effect decreases for both specifications. However, the changes are very small in either case.

The Covariate Matching shows very similar estimates of impact. Again, the bias adjustment pushes the estimates downwards. However, the estimates of the impact remain significant and very small in magnitude. The estimated percentage of enrolled land saved per year ranges from 0.38% to 0.42% 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 increases. Figure 2 presents these additional robustness checks, all of which support the conclusions we give here.

We also examine how well matching has done in finding untreated observations that are similar to PSA observations. Are they more similar to the treated than the full set of untreated locations? Are they "the same" as the treated in the sense of variable means? Table 2 presents, for each covariate, tests of the mean between the treated and the control group. We see no evidence that after matching the covariates differ significantly between treated and control -- for all of the variables except one. The only variable that still seems ignorantly different is distance to local roads. However, the difference between the match untreated and treated is smaller than the difference between all untreated and the treated. In Figure 3, we also show 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.

6. 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 one percent of the land enrolled into the program would have been deforested in a given year if the payments have not been implemented. 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 to using another unit of analysis (5x5km grid cells).

This small impact on deforestation given the PSA resources expended (again this is consistent with Sanchez-Azofeifa et al 2007 and Pfaff et al 2008) 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. Also, though, the implementation strategies employed by FONAFIFO after 2000 appear, consciously or not, to have eliminated the bias in PSA location during 1997-2000, one which had lowered the impact on land use. Thus policy re-design can indeed affect outcomes, offering significant hope for many other settings in which payments may be tried and then adjusted to maximize impact.

This would appear to be necessary since, as noted, to first order almost none of the payments are actually preventing deforestation (in this context, i.e. not in all settings). That is not surprising when targeting was essentially not present (1997-2000) or focused (post-2000) not on impact but on the level of gains in ecoservices if there were an impact. Further, it is something that we believe could be changed by focusing on forest impacts.

One way to do so is to focus on where these payments would change the land use. That would mean trying to avoid places with negative agricultural returns, i.e. places that would be enrolled at any level of payment and would be in forest even without payment. Thus, payments would be above agricultural returns for locations that would be cleared.

This could go hand by hand with higher payments for areas where the opportunity costs of leaving land in forest are larger. Such targeting and differentiation could raise impact, as it could be the most cost effective way of changing land use in many locations.

However, if payments are larger, then for a fixed budget there are fewer of them. Further, if they are larger for those with higher returns, they may be transferring more to those who already have more resources. This could work against any distributional goals.

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Table 1:
Effect of Payments for Environmental Services on Reducing Deforestation
Far away from Payments of any type (1 Km)
Standard Errors in Parenthesis

	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

*covariates from specification 1, **covariates from specification 2, For PSM: Standard Errors consider repeated control observations. Annual deforestation rate = 1- (1-deforestation in 5 years)$^{0.2}$ and vice versa for the grid regression which was annualized before the regression

Table 2
Statistical Tests of Difference in Means between treated and matched controls of Covariates

Covariates	Means of Treated	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 Jose	102.7695	101.4960	0.48	114.3276
Distance to Caldera	12.1387	11.9523	0.32	12.2401
Distance to Limon	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.

Figure 1 – land-use choice with payment option

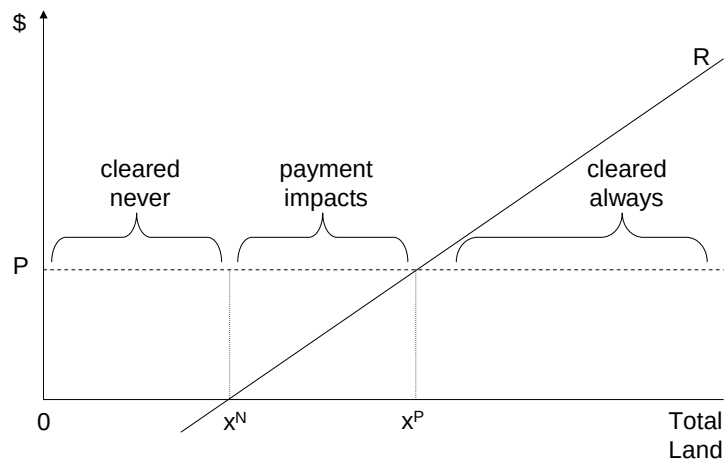


Figure 2

Estimates of Impact as the number of matches increases
using Propensity Score Matching (Spefication 2)

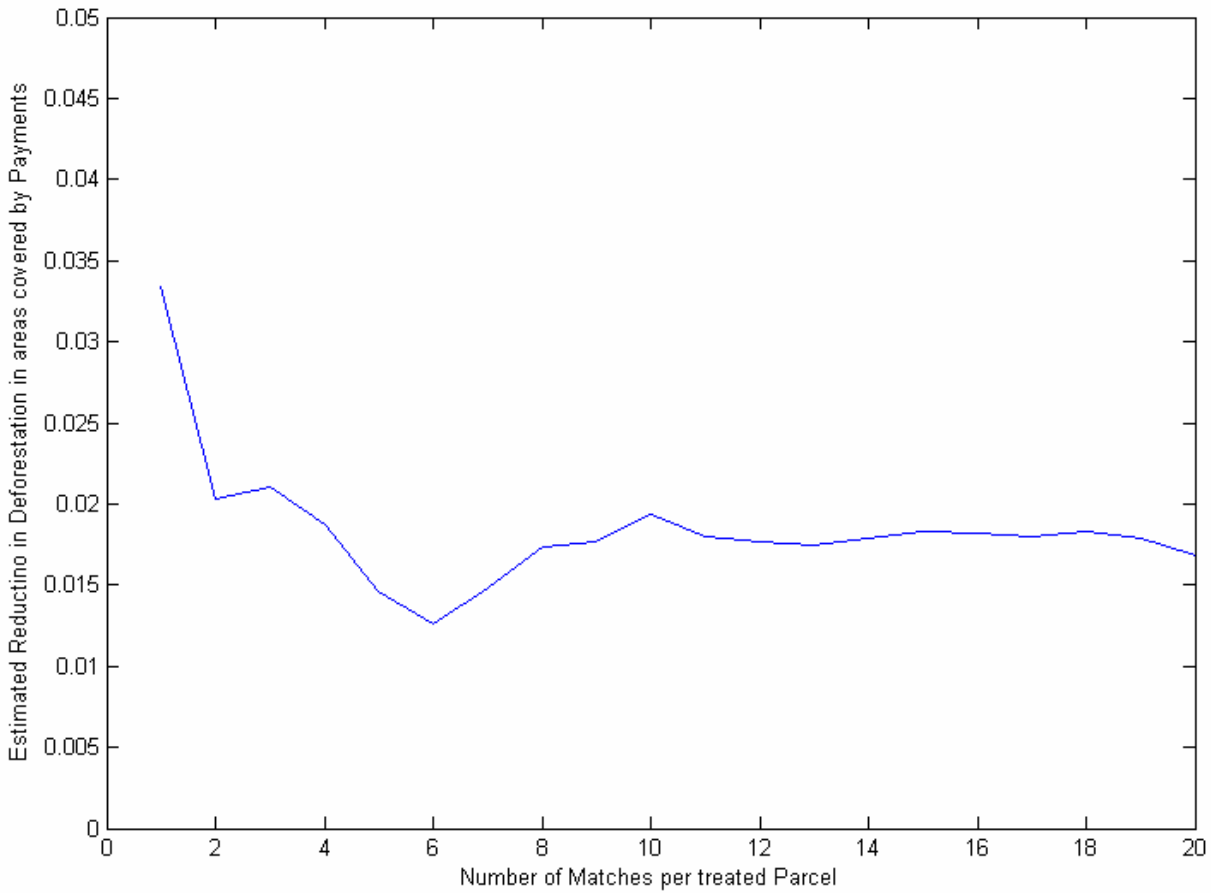
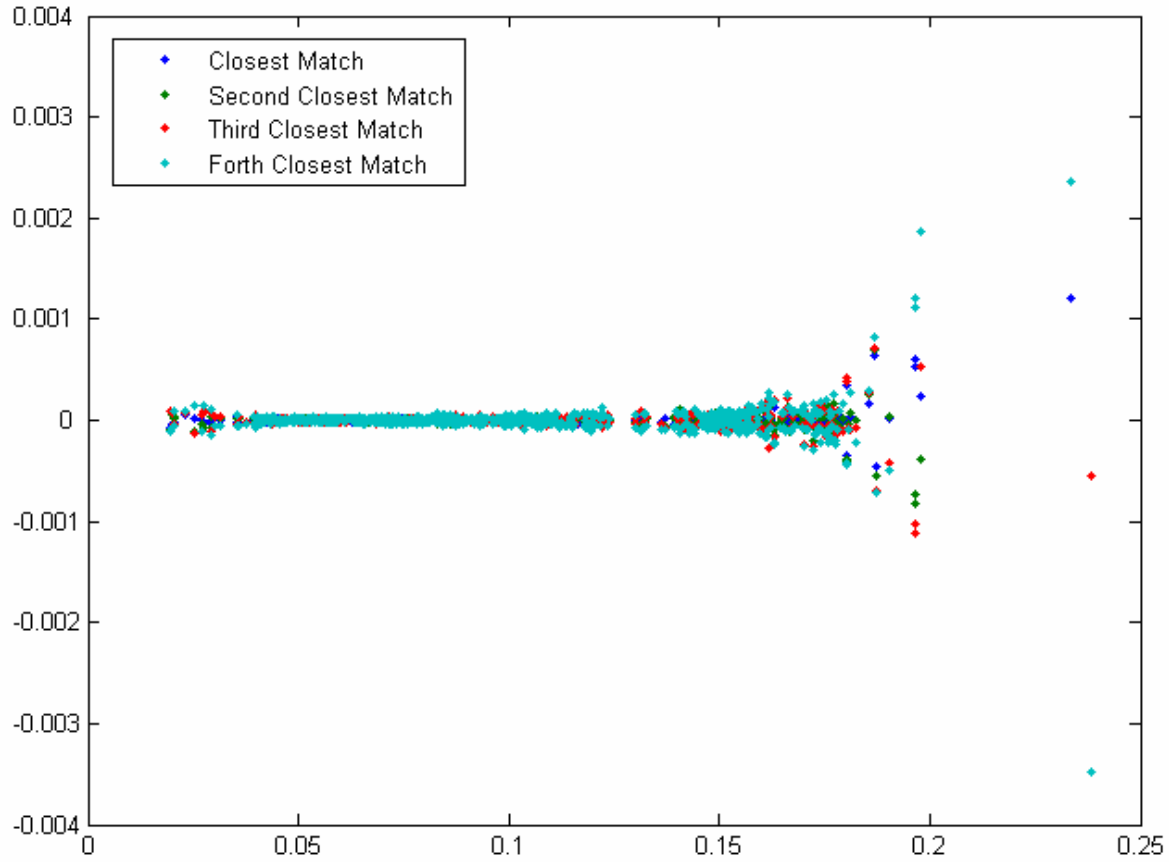


Figure 3

Differences between treated and matched untreated observations



Appendix

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Probit Maximum Likelihood Estimates
Dependent Variable =        Treatment
McFadden R-squared     =    0.0332
Estrella R-squared     =    0.0193
LR-ratio, 2*(Lu-Lr)    =  210.1854
LR p-value             =    0.0000
Log-Likelihood         = -3065.1123
# of iterations        =      7
Convergence criterion  =   7.2317714e-011
Nobs, Nvars            =  10944,     7
# of 0's, # of 1's     =  10019,   925
*****************************************************************
Variable       Coefficient      t-statistic    t-probability
LZG               0.119341         1.913999         0.055646
LZB               0.347256         7.441191         0.000000
DSJ              -0.009203        -6.622759         0.000000
DCA               0.070448         6.069397         0.000000
DLI               0.038809         5.338129         0.000000
SDA              -0.001798        -7.918853         0.000000
C                -1.915432       -16.832424         0.000000
```

Probit of Specification 2

```
Probit Maximum Likelihood Estimates
Dependent Variable =        Treatment
McFadden R-squared     =    0.0343
Estrella R-squared     =    0.0200
LR-ratio, 2*(Lu-Lr)    =  217.3305
LR p-value             =    0.0000
Log-Likelihood         = -3061.5397
# of iterations        =      7
Convergence criterion  =   1.1111858e-010
Nobs, Nvars            =  10944,    12
# of 0's, # of 1's     =  10019,   925
*****************************************************************
Variable       Coefficient      t-statistic    t-probability
LZG               0.105225         1.665383         0.095865
LZB               0.346623         6.848824         0.000000
DSJ              -0.010159        -6.985146         0.000000
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.