Changing The Deforestation Impacts of Ecopayments: evolution (2000-2005) in Costa Rica's PSA program

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Abstract

Costa Rica's PSA (Pagos por Servicios Ambientales) environmental services payments, started in 1997, were the pioneers. Broadly cited, they have led to numerous suggestions that others emulate the PSA approach. Yet the PSA program has itself evolved over time. Following earlier work (Sanchez et al. 2007 and Pfaff et al. 2008 on PSA 1997-2000), we can evaluate here whether a change in implementation changes impacts on deforestation. Examining the PSA forest-protection contracts during 2000 and 2005, we find that less than 5 in 1000 (about 0.4%) parcels enrolled in the program would have been deforested annually without payments. To first order, this matches the 1997-2000 findings of low deforestation impact and may be explained by.low agricultural returns relative to those in ecotourism as well as by other conservation policies including the forestry law of 1996. However, there are differences in results which are instructive. First, the overall impact is in fact slightly higher than in 1997-2000; despite net reforestation, more deforestation took place and thus the PSA had a bit more land-use change to prevent. More important for upcoming policies such as global carbon payments, the shifts in PSA implementation eliminated the bias of the PSA payments towards lands that are relatively unprofitable and thus unlikely to be cleared even without payments. Thus we can see that even within the same country for the same basic policy idea, the details of implementation do matter. In this and other settings significant potential gains can be realized by increased targeting.

Keywords payments, environmental services, deforestation, Costa Rica

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1. Introduction

The Program for Environmental Services (Pagos por Servicios Ambientales, or PSA) in Costa Rica was one of the first initiatives in a developing country to compensate the provision of environmental services. The PSA has inspired others to try such payments strategies (see discussion in Chomitz et al. 1998, Ferraro 2001, Miranda et al. 2003, Pagiola 2002, Rojas and Aylward 2003, Sierra and Russman 2006, and Zbinden and Lee 2005). For others designing new programs and for Costa Rica istelf, the first few stages of the PSA also permit analysis and learning about how such programs can be improved.

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.

Yet affecting deforestation directly need not be the only objective of payments. They could compensate or reward landowners for decisions already taken, for instance sharing the surplus from the environmental benefits others reap. In Costa Rica, for example, with socioeconomic benefits for small and medium farmers as one objective of both the implementing agency (FONAFIFO 2008) and of course the farmers themselves, payments may well play this role. As discussed further below, given other policies which constrained farmers' land usage, these payments may function mostly as compensation.

This paper's focus, however, is the direct impact of the PSA upon deforestation. The major motivation for this focus is the widespread belief that payments in Costa Rica lowered deforestation (whatever their other goals). The PSA's forest protection contracts that we study make up more than 88% of the land enrolled (FONAFIFO 2008). Whether such payments reduce deforestation depends on how they affect landowner's land use decisions. Put more directly, signing a PSA forest protection contract does not assure a significant impact on deforestation. If contracts go to parcels that would not have been deforested anyway, such a program would have no impact at all upon deforestation. Our particular focus in this paper is whether a shift in implementation helps to raise impact. As Costa Rica is out front and learning from its own early efforts, others can learn too.

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, we find that about 0.4%, i.e. closer to 1 in 250, of parcels were saved. To first order, we find similarly very low payment impacts on deforestation in both periods.

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.

[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 in the Results discussion below we also note consistent 5x5km grid results for this later time period.

In fact, despite the first-order similarity of the impact results for Costa Rica still the difference between the 1997-2000 and 2000-2005 findings conveys the importance of the background socioeconomic context. The processes driving deforestation constrain what payments can do. Here, specifically, while during 2000-2005 there was net national reforestation there was more gross deforestation, i.e. a bit more for payments to prevent.

Our second main result concerns a different change relative to PSA in 1997-2000, specifically the acknowledged shift towards some targeting of PSA starting after 2000. . Targeting can not change that there was little deforestation to prevent in Costa Rica but, of global interest, it can affect the allocation of payments and can change policy impact.

In 1997-2000, that landowners volunteered parcels into the program seem to have strongly biased enrollment towards land that would not have been cleared without PSA (see Pfaff et al. 2008). This resulted in the PSA's impact being only one third as large as national average deforestation rates. Thus, randomly spreading PSA around the landscape would have had a higher impact. For 2000-2005, statistically that is effectively what occurred. PSA impact is about the same as or even a bit higher than the national average deforestation outside of the program area (i.e., 0.3%). Thus, while efficiency or impact does not appear to have been the goal[2], the shift in targeting of PSA increased efficiency. This suggests that other shifts which aim to increase efficiency could achieve even more.

Efficiency- or impact-oriented shifts could include an intentional focus on where payments are most likely to change land use and forest outcomes. This could involve higher payments where the agricultural opportunity costs of land left in forest are larger. We note that while such targeting and differentiation could raise impact, it might also shift the distribution of payments towards less poor landowners by focusing on those with the higher opportunity costs (and higher threats of clearing) and paying them higher rates. Generally, as noted above there could be various goals for such a program and thinking of all the possible goals, across stakeholders and in light of other policies, is important. This theme is taken up in the Discussion section below, after measuring impact on forest.

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.

[2] Targeting post-2000 started with a focus on proximity to protected areas or biological corridors, with the implicit goal seemingly to consider the level of expected gains in ecoservices if the payments saved forest.

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.

Using geographic information systems, 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 first units of observation.

We use forest cover maps from 2000 and 2005 from satellite pictures. This allows us to know if a location had forest in 2000 and, if so, if the forest was cleared by 2005.

We note that such data do not identify forest thinning or clearing under canopy. If such forest degradation without observed deforestation takes place more outside (or inside) of PSA locations, then we will under- (or over-) estimate PSA's complete impact on forest.

We focus on deforestation and thus on the PSA contracts for forest conservation. Thus our analysis is of 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

We use matching techniques to address the bias arising from the 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 addition, 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.

It is also important to acknowledge the role of these efforts in the Costa Rican political context. It has been argued that the PSA program was instrumental on reducing the opposition to the implementation of more stringent conservation laws that might have actually reduced deforestation significantly. However, even if this is true, one cannot attribute the effectiveness of the Costa Rican government on reducing deforestation rates solely to the PSA program as has been implemented. Any PSA system will not be successful in reducing deforestation without eliminating environmentally perverse government incentives, without a good and strong environmental framework, and without a long term national objective that goes beyond governments, congresses and political parties as in the Costa Rican context. THIS BASIC IDEA IS IN THE INTRO, AND HERE PERHAPS COULD LAY OUT SOME DETAILS OF THE FOREST LAW IDEA AND THE TAKINGS WITHOUT COMPENSATION IDEA AND THE PACKAGING OF THE POLICIES (I THINK PASSED TOGETHER - PACKAGE IN ACTUALITY?)

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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
Grids (5x5km)			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. 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

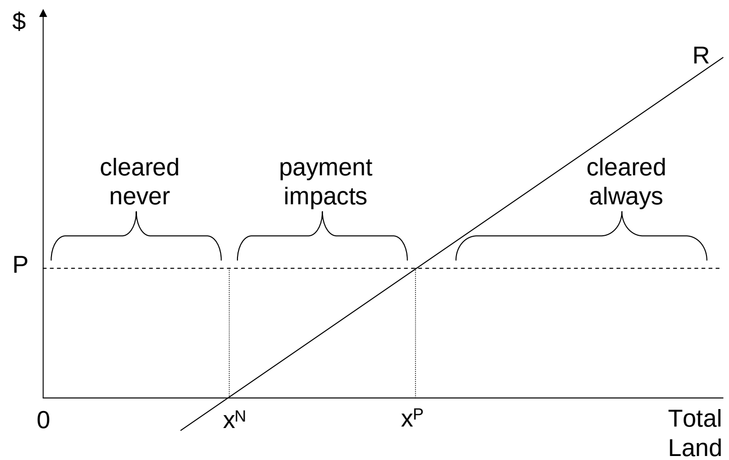


Figure 2

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

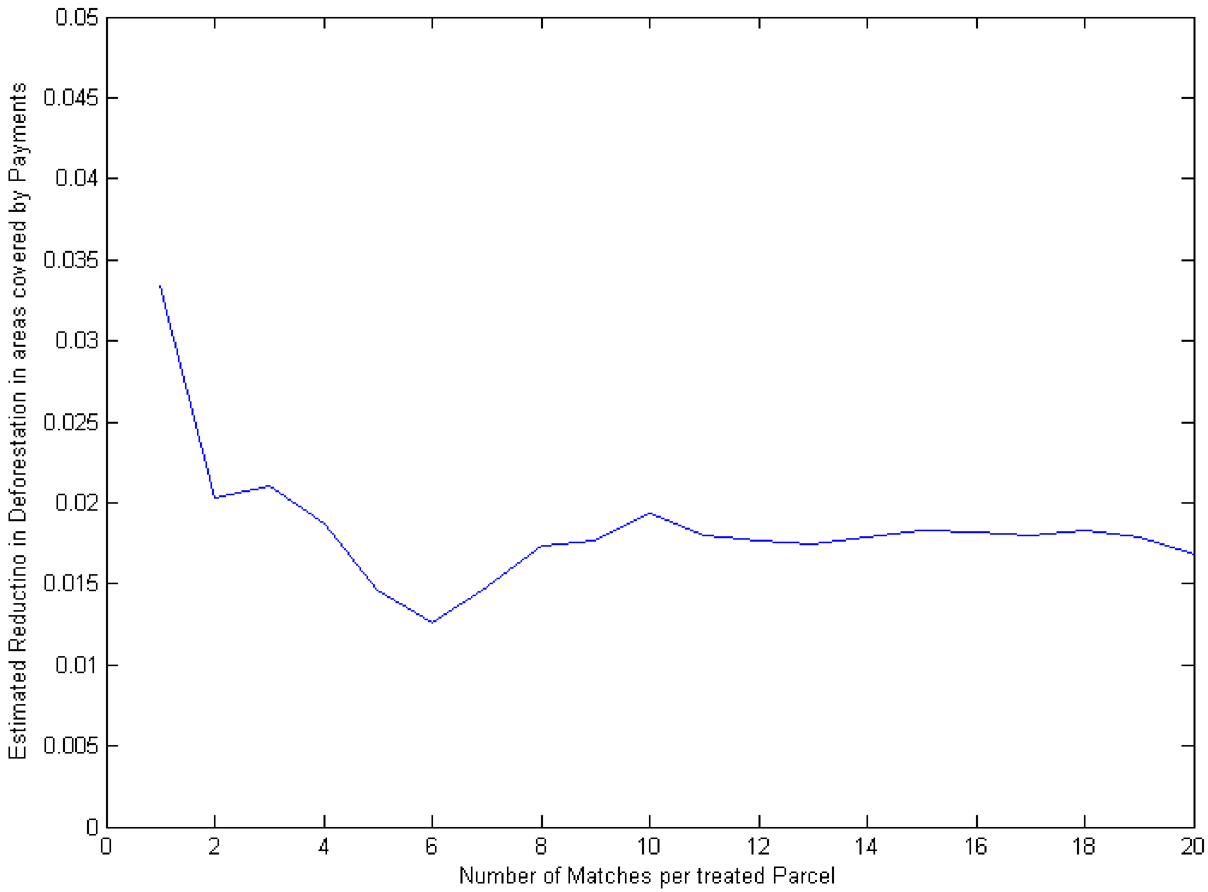
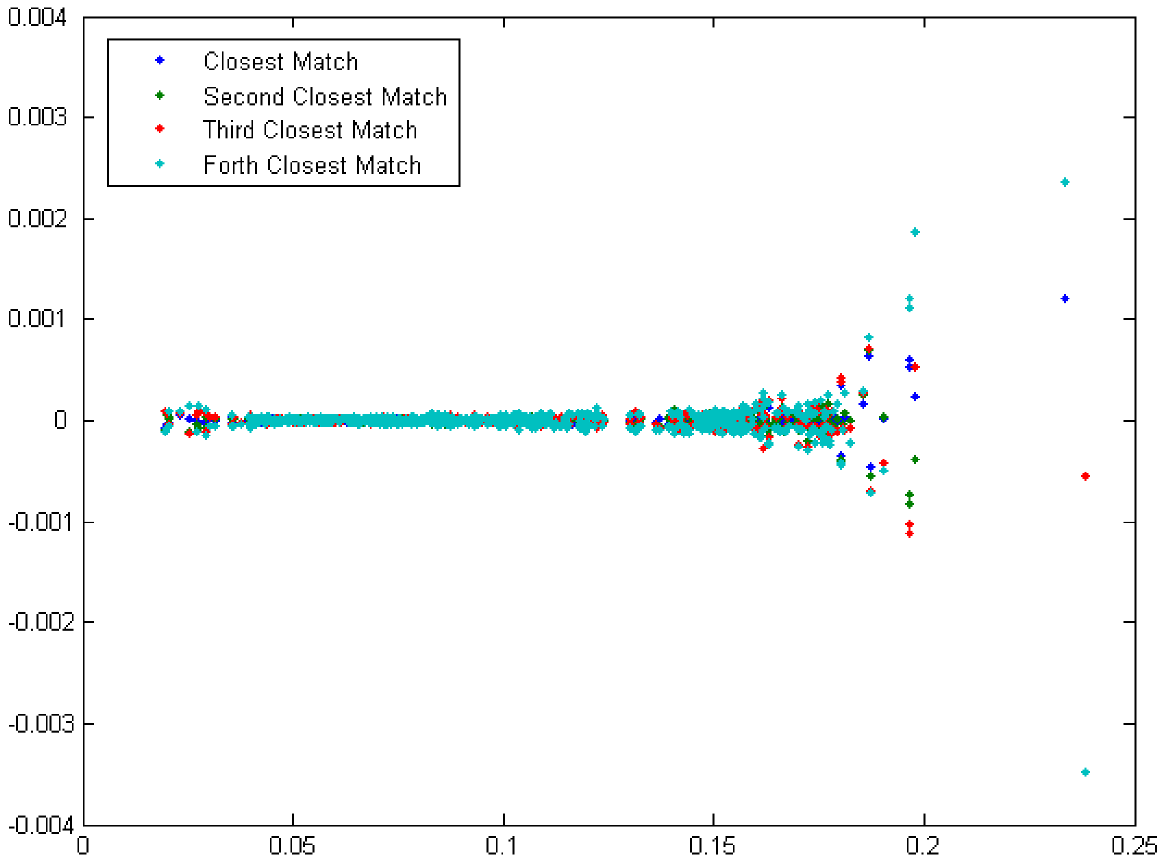


Figure 3

Differences between treated and matched untreated observations



Appendix

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.