Global Environment Facility

Evaluation Office

PROTECTED AREAS AND AVOIDED DEFORESTATION: A STATISTICAL EVALUATION

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DRAFT FINAL REPORT

AUGUST 2007

TABLE OF CONTENTS

1. EXECUTIVE SUMMARY

1.1 BACKGROUND

This report presents the findings from a research project funded by the Evaluation Office of the Global Environment Facility (GEF). Our objective was to develop a quasi-experimental methodology for evaluating the effectiveness of protected areas (e.g., national parks, reserves) in reducing deforestation, and to apply the methodology in a country that has received GEF funds.

Protected areas are an important component of the GEF's biodiversity portfolio and are central to the "avoided deforestation" debate in climate change policy. However, it is difficult to estimate the returns from investments in protected areas in terms of avoided deforestation. Measuring avoided deforestation from protective measures is difficult because avoided deforestation is a counterfactual event and thus cannot be observed directly. By ignoring the nonrandomized nature of protected area establishment and the spatial spillovers that can result from their establishment, past empirical estimates of avoided deforestation fail to properly estimate the counterfactual vegetation cover. We demonstrate how matching estimators can be used to estimate avoided deforestation in and around protected areas. These same methods can be used to evaluate the effects of protected areas on reforestation and on human welfare around protected areas, as well as the impacts of other land use policies such as payments for environmental services or road building prohibitions.

1.2 RESEARCH DESIGN

We apply our matching estimator methodology to estimate the effects of protection on avoided deforestation inside and outside Costa Rica's world-renowned protected area system. We split the analysis into three sections: (1) evaluation of the effectiveness of all protected areas, (2) evaluation of the effectiveness of GEF-funded protected areas[i], (3) comparison of the effectiveness of GEF-funded and non GEF-funded protected areas. To conduct this analysis, we develop a dataset that includes historical information on forest cover, protection status, and biophysical, infrastructure, and socio-economic characteristics of the landscape. These latter characteristics affect both the likelihood that a land plot would be protected and the probability that the plot would be deforested. Thus they are potential confounding variables that can mask the effect of protection on deforestation. Matching analysis provides a way to control for these potential confounders by ensuring that protected plots are only compared to unprotected plots that are similar in their observable characteristics.

1.3 MAIN FINDINGS

Our evaluation of all protected areas indicates that protection resulted in a relatively small amount of avoided deforestation of about 10% (about 111,000 ha) or less of the forest protected between 1960 and 1997. In our evaluation of GEF-funded protected areas, which

[i] The GEF funded activities in two protected areas, Corcovado national park and La Amistad national park between 1993 and 1998 through a project called the Biodiversity Conservation and Sustainable Development in the Amistad and Osa Conservation Areas project (GEF Project ID: 364).

received funding between 1993 and 1998, we estimate that protection resulted in about 8% (about 19,000 ha) or less avoided deforestation between 1986 and 1997. In the period 1997-2005, protection of these same areas resulted in 11% (about 25,000 ha) or less avoided deforestation. We find that the GEF-funded protected areas reduced deforestation by a little more than other protected areas: between 2 and 7 percentage points for 1986-1997 and at most 2 percentage points for 1997-2005. We find our results robust to alternative specifications and measures, as well as to unobservable confounders that affect both protection and deforestation.

Note, however, that we are not explicitly modeling the effect of funding levels, but rather the effect of protection. Thus we cannot completely isolate the effects of GEF funding from the effects of other investments made into these same protected areas. If the Costa Rican government viewed GEF funds as a substitute for its own, it may have funded these areas at levels lower than it would have in the absence of GEF funds (thus making GEF funding look less effective in our analysis). Alternatively, the GEF may have simply invested its funds into protected areas that were already successful in the absence of GEF funds (thus making GEF funding look more effective in our analysis). Furthermore, we are looking only at one aspect of conservation outcomes: avoided deforestation. GEF investments may have affected other outcomes that are not measured directly in this evaluation.

Our analysis also indicates that methods traditionally used in conservation science to estimate protected area effects on land cover change overestimate the amount of avoided deforestation by a factor of three or more. The reasons for this overestimation have implications for the use of protected areas in biodiversity conservation and climate change policies and are discussed in the report's conclusion.

2. INTRODUCTION

2.1 MOTIVATION

In the last decade, conservation scientists and practitioners have become increasingly strident in their demand for more rigorous assessments of policies and programs designed to protect biodiversity and ecosystem services (see Ferraro and Pattanayak 2006, and references therein). The immature state of knowledge about the effectiveness of different programs and policies is most clearly observed in the recent publication of the *Millennium Ecosystem Assessment* (2005). While the biological chapters are rife with data and empirical studies, the *Policy Responses* volume lists as one of its "Main Findings" the following: "Few well-designed empirical analyses assess even the most common biodiversity conservation measures."

The most common policy for protecting biodiversity and reducing tropical deforestation is the establishment of protected areas (e.g., national parks, reserves, etc.). Every year, about $6.5 billion is spent on more than 100,000 protected areas around the world. However, it is difficult to estimate the returns from these investments in protected areas in terms of avoided deforestation.

Protected areas also play a key role in the recent high-profile debate over whether developing nations should be allowed to generate greenhouse gas emission credits from "avoided deforestation." Proponents claim such credits offer a win-win opportunity: (1) they create incentives for reducing deforestation, which is a leading source of greenhouse gas emissions from developing countries; and (2) they transfer wealth from wealthy to less wealthy nations. The most common policy for reducing deforestation is the establishment of protected areas and other land use restrictions. Poor nations that incur costs to establish these protected areas wish to earn credits for the global environmental services that these areas provide. Setting appropriate baselines, however, is complicated because "avoided deforestation" is a counterfactual event and cannot be observed.

2.2 KEY ISSUES

An analysis of the effectiveness of any policy or program designed to protect ecosystems and their concomitant services must include at least the following three characteristics: (1) control of overt bias generated from the nonrandom nature of policy or program implementation (selection on observables); (2) detection and control for spatial spillovers; and (3) an assessment of sensitivity of the results to hidden bias (unobservable heterogeneity). These characteristics, however, are generally absent in the conservation science literature and this absence leads to inconclusive findings about program effectiveness (e.g., Stern *et al.* 2001; Vanclay 2001). In fact, we are unable to find an analysis that includes just two of the three characteristics.

2.3 RESEARCH DESIGN

To address the challenges described above, analysts must construct the counterfactual – the deforestation that would have occurred if an area of forest were not protected – from observations or theory. We demonstrate how such a counterfactual can be constructed and apply our methods to estimate avoided deforestation in Costa Rica between 1960 and 2005 as a result of establishing protected areas. Costa Rica has one of the most widely lauded protected areas systems (Sanchez-Azofeifa et al. 2003; Pfaff & Sanchez-Azofeifa 2004) and is a leader in the debate to have "avoided deforestation" credits recognized by the Kyoto Protocol. Between 1960 and 1997, Costa Ricans cleared more than one million hectares of forest and protected about 900,000 hectares of forest. We answer the question, "How much more forest would have been cleared in the absence of the protected areas?"

We split the analysis into three sections: (1) we estimate the avoided deforestation between 1960 and 1997 from all protected areas; (2) we estimate avoided deforestation in the periods 1986-1997 and 1997-2005 from two protected areas where activities were funded by the GEF in the 1990s, Corcovado national park and La Amistad national park; (3) we compare avoided deforestation from these GEF-funded protected areas with avoided deforestation from protected areas that did not receive GEF funding.

3. BACKGROUND

3.1 DETERMINANTS OF PROTECTED AREA LOCATION

According to the Millennium Ecosystem Assessment (2005, p. 130), "many protected areas were specifically chosen because they were not suitable for human use." Empirical studies from various countries support this assertion (Green and Sussman 1990; Hunter & Yonzon 1993; Pressey 1995; Scott *et al.* 2001; Pauchard and Villarroel 2002). Similarly in Costa Rica, empirical studies have found that protected areas are located largely in areas unsuitable for agriculture (Powell *et al.* 2000; Cornell 2000; Helmer 2000; Sanchez-Azofeifa *et al.* 2003).

Others have argued that protected areas were preferentially established in areas where there was the least political opposition (Brandon *et al.* 1998; Evans 1999). Anecdotes and formal analyses thus suggest that, for political and economic reasons, governments may establish protected areas on lands that are not likely to be cleared in the absence of protection.

3.2 SPATIAL COMPLEXITY IN EVALUATING PROTECTED AREAS

Spatial interactions, such as spillovers or spatially correlated errors, are common in land use models (Anselin 2002). Rosero-Bixby and Palloni (1998), Kerr *et al.* (*unpublished study*), and Pfaff and Robalino (*unpublished study*) find spatial dependence in deforestation across landscapes in Costa Rica. Similar findings have been made in deforestation studies in Cameroon (Mertens & Lambin 2000) and Honduras (Munroe *et al.* 2002).

In general, there are two ways to characterize spatial dependence in land cover change: (1) *Spatial lag* occurs when land use in one area affects the likelihood of land cover change in neighboring areas; and (2) *Spatial error correlation* occurs when unobservable characteristics that influence land use are spatially correlated.

A specific type of spatial lag that is relevant for this study is a spillover from regulatory protection onto unregulated lands (other common terms for spillovers are “slippage”, “leakage”, “displacement”, and “enhancement”). Several theoretical models and empirical studies have shown that land use regulations can affect land use on unregulated lands (Berck & Bentley 1997; Wu 2000; Murray *et al.* 2002; Quigley & Swoboda 2004; Armsworth 2006). Spillovers can be negative: displacement of agricultural pressures, exploitation to meet the demands of protected area tourists, or preemptive clearing by landowners near protected areas to prevent future government expropriation for protected areas. Spillovers can also be positive: the establishment of private reserves near protected areas (see Langholz (2000) for a review of such reserves in Costa Rica) or the failure to develop local market infrastructure, which is critical for facilitating the exploitation of forested lands in the reserve's surrounding areas. Note that we focus on local "neighborhood" spillovers rather than more distant spillover effects in other regions or sectors of the economy. The latter are most appropriately studied in a computable general equilibrium model.

Local spillover effects should be addressed when estimating avoided deforestation for two reasons. First, in the presence of spillovers, using the surrounding unprotected lands as controls could bias estimates of the effect of protection (Stern et al. 2001; Vanclay 2001). Spillover effects must be stripped from the estimated counterfactuals. Second, spillovers imply that the effect of protection extends beyond the boundaries of protected areas and thus must be incorporated in an estimate of the net effect of protection.

Note that if both spatial lag and spatial error correlation exist, the evaluation is pulled in two opposing directions. The presence of spatial lag calls for selecting controls that are <u>not</u> neighbors of treated units, as explained above. However, spatial error correlation implies unobserved characteristics (e.g. weather patterns, socioeconomic conditions) that determine the likelihood of deforestation are similar on neighboring lands. Thus the presence of spatial error correlation calls for selecting controls that are neighbors of treated units.

3.3 ESTIMATED EFFECTS OF PROTECTED AREAS ON DEFORESTATION

In a review of forty-nine protected area assessments, Naughton-Treves *et al.* (2005) find that thirteen assessments examine deforestation only in the protected areas. Thirty-six others compare deforestation inside and outside protected areas, and thirty-two of them find lower deforestation rates inside protected areas. For example, Bruner *et al.* (2001) assess protected area effectiveness by comparing (expert-reported) land clearing rates inside protected areas with the rates within a 10-km surrounding belt. They find lower rates inside protected areas and conclude that protected areas are thus effective.

Such assessments, however, are valid only if protection was randomly assigned across the landscape and spatial spillovers were absent. As noted in the previous section, protection is definitively a non-random process. Only a few assessments have formally controlled for other covariates known to affect deforestation, but they either use a small set of covariates (Cornell 2000; Mas 2005), which can exacerbate the bias in avoided deforestation estimates (Heckman *et al.* 1997), or a highly parametric, regression-based approach (Chomitz & Gray 1996; Cornell 2000; Cropper *et al.* 2001; Deininger & Minten 2002; Mas 2005), which is prone to specification bias. Moreover, no analysis has tested the sensitivity of results to hidden bias that may not have been removed by conditioning on observable covariates (see Methods), nor has any addressed the potential confounding caused by spatial spillovers (see next section).

4. METHODS

4.1 MATCHING METHODS

In statistical jargon, avoided deforestation from protected areas is the Average Treatment Effect on the Treated (ATT). If protection is allocated randomly across land units, we can do what most studies have done: estimate the counterfactual simply by using the status of unprotected units because the expected forest cover change in the absence of protection is identical for protected and unprotected lands. However, as noted above, decisions to protect land are determined by observable characteristics. Thus protected and unprotected lands, on average, differ in characteristics that may also affect forest cover change after protection.

The methods of matching provide one way to assess the effect of protection when protection is influenced by observable characteristics and the analyst wishes to make as few parametric assumptions as possible about the underlying structural model that relates protection to deforestation. Matching works by, *ex post*, identifying a comparison group that is "very similar" to the treatment group with only one key difference: the comparison group did not participate in the program of interest (Rubin 1980; Rosenbaum & Rubin 1983; Imbens 2004). Matching mimics random assignment through the *ex post* construction of a control group. If the researcher can select observable characteristics so that any two land units with the same value for these characteristics will display homogenous responses to the treatment, then the treatment effect can be measured without bias.

Measuring the ATT without bias requires that, given a vector of covariates, the non-treated outcomes are what the treated outcomes would have been had they not been treated (i.e., protection is independent of forest cover change for "similar" land units). This "conditional independence assumption" requires that selection into treatment occurs only on observable characteristics. Hence an unbiased estimator requires that we have included all of the determinants of the selection problem that also affect outcomes. Arguably one can satisfy this requirement in the case of protected areas because the land units themselves exert no idiosyncratic influence. Thus the problem is only one of *eligibility* and not one of self-selection.[i]

Based on recent studies, we estimate the ATT using four matching estimators (Frolich 2004; Abadie and Imbens 2006a): (1) nearest-neighbor covariate matching estimator with an inverse variance weighting matrix to account for the difference in scale of the covariates; (2) nearest-neighbor covariate matching estimator with Mahalanobis weighting; and (3) kernel (Gaussian) propensity score matching estimator.[ii]

The nearest-neighbor matching is with replacement and we resolve the mean-variance tradeoff in the match quality by using two nearest neighbors; the counterfactual outcome is the average among these two.[iii] Based on recent work that demonstrates that bootstrapping standard errors is invalid with non-smooth, nearest-neighbor estimators (Abadie and Imbens 2006b), we

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[ii] With the exception of the kernel matching (done in Stata v.9; Leuven and Sianesi 2003), matching was done in R (Sekhon, 2006). We also used a nearest-neighbor propensity score matching estimator, but given the results from this estimators were similar to those presented in Table 2, we do not present these results.

[iii] Given our large sample size, we do not need to use more than two neighbors as is often done in other nearest-neighbor matching analyses (e.g., Abadie and Imbens 2006a; McIntosh 2007). We varied the number of neighbors from one to ten and the ATT estimate changes very little.

use Abadie and Imbens' variance formula (2006a). For the kernel matching estimator, we use a bandwidth of 0.06 and we bootstrap the standard errors (999 replications).

In our covariate matching estimators, we use Abadie and Imbens' (2006a) post-matching bias-correction procedure that asymptotically removes the conditional bias term in finite samples As an additional form of quality control, we implement caliper matching in the context of our bias-adjusted, nearest-neighbor Mahalanobis matching estimator (Smith and Todd 2005). The calipers are defined as 0.5 standard deviations of each matching covariate. For the propensity score estimator, we enforce a common support. We conduct balancing tests for all our matching estimators. The balancing tests compare the means of the matching covariates for matched and control groups using a t-test.

4.2 DETECTING HIDDEN BIAS

Although we take great care to ensure that the conditional independence assumption is satisfied, non-experimental analyses are always susceptible to hidden biases. To determine how strongly an unmeasured confounding variable must affect selection into the treatment to undermine our conclusions, we use the bounds recommended by Rosenbaum (2002; see also Diprete and Gangl 2004). Although there are other sensitivity tests available (e.g., Ichino et al. 2006), Rosenbaum's bounds are relatively free of parametric assumptions and provide a single, easily interpretable measure of the way in which unobservable covariates enter.

If the probability of agent *j* selecting into the treatment is π_j, the odds are then $\frac{\pi_j}{1-\pi_j}$. The log odds can be modeled as a generalized function of a vector of controls x_j and a linear unobserved term, so $\log(\frac{\pi_j}{1-\pi_j}) = \kappa(x_j) + \gamma u_j$, where u_j is an unobserved covariate scaled so that $0 \le u_j \le 1$. Take a set of paired observations where one of each pair was treated and one was not, and identical observable covariates within pairs. In a randomized experiment or in a study free of bias, $\gamma = 0$. Thus under the null hypothesis of no treatment effect, the probability that the treated outcome is higher equals 0.5. The possibility that u_j is correlated with the outcome means that the mean difference between treated and control units may contain bias.

The odds ratio between unit *j* which receives the treatment and the matched control outcome *k* is: $\frac{\pi_j(1-\pi_k)}{\pi_k(1-\pi_j)} = \exp\{\gamma(u_j - u_k)\}$. Because of the bounds on u_j, a given value of γ constrains the degree to which the difference between selection probabilities can be a result of hidden bias. Defining $\Gamma = e^{\gamma}$, setting $\gamma = 0$ and $\Gamma = 1$ implies that no hidden bias exists, and hence is equivalent to the usual regression assumptions. Increasing values of Γ imply an increasingly important role for unobservables in the selection decision. The differences in outcomes between the treatment and control are calculated and ranked. We contrast outcomes using matched plots from the kernel propensity score matching estimator. A Wilcoxon's signed rank statistic is then used to compare the sums of the ranks of the pairs in which the treatment was higher than the control (calculated using Stata code 'rbounds'; Gangl 2004).

The intuitive interpretation of the statistic for different levels of Γ is that matched plots may differ in their odds of being protected by a factor of Γ as a result of hidden bias. The higher the level of Γ to which the difference remains significantly different from zero, the stronger the relationship is between treatment and differences in deforestation. Note that the assumed unobserved covariate is a strong confounder: one that not only affects selection but also determines whether deforestation is more likely for the treatment units or their matched controls.

5. DATA

5.1 STUDY SITE

Costa Rica has a population of 4.45 million and a land area of 51,100 sq km. Costa Rica has experienced high rates of deforestation since the beginning of the 20th century, driven mainly by the expansion of cattle grazing and coffee and banana production. During the 1960s and 1970s, the country had one of the top five deforestation rates in the world (FAO 1990). Since the mid-1960s, the government has designated more than 150 protected areas.

5.2 DATA SOURCES

Forest cover across the country is measured from a combination of aerial photographs acquired between 1955 and 1960 (called the 1960 dataset) and from Landsat Thematic Mapper satellite images for 1986, 1997, 2000, and 2005. GIS data layers for forest cover, protected areas, and locations of major cities were provided by the Earth Observation Systems Laboratory of the University of Alberta, Canada. Other GIS data layers include a map of land use capacity based on exogenous factors (soil, climate, topography) from the Instituto Tecnologico de Costa Rica (ITCR 2004), and socioeconomic data from the Instituto Nacional de Estadistica y Censos (INEC). GIS layers for transportation roads, railroads, and the river transportation network were digitized by Margaret Buck Holland from hard copy maps of 1969 and a 1991 road layer (map source: Instituto Geográfico Nacional (IGN) of the Ministerio Obras Publicas y Transporte (MOPT) of Costa Rica).

5.4 MATCHING VARIABLES

In our matching analysis, we are interested in controlling for factors that jointly affect land use and the likelihood that a plot is selected for protection. Based on our knowledge of the history of Costa Rica's protected areas, as well as the literature on tropical deforestation (especially the review of Kaimowitz and Angelsen 1998), we select variables that capture accessibility of the plot (distance to forest edges, distance to roads and slope) and land use opportunities (a function of the plot's production potential and distance to roads and major markets). See Table 1 for summary statistics. Our core set of covariates are as follows:

- *Distance to roads*: In Costa Rica, roads make forests more accessible to deforestation agents, and ease the transportation of agricultural produce or logs from cleared land (Sader & Joyce 1988; Veldkamp *et al.* 1992; Helmer 2000). We measure the distance from each plot to a road in 1969 (to a road in 1991 for the 1985-1997 analysis).

- *Distance to the forest edge:* Proximity to forest edges increases accessibility and the likelihood of deforestation (Rosero-Bixby & Palloni 1998; Chaves-Esquivel & Rosero-Bixby 2001). We measure the distance between a land plot and the nearest cleared plot from the 1960 forest cover map (from the 1986 map for the 1985-1997 analysis).

- *Land use capacity*: In Costa Rica, forests are generally cleared for crops and pasture. Therefore, the variables that make land more productive for agriculture also tend to make the land more likely to be deforested. Mild slopes, fertile soils, and humid life zones make deforestation more likely (Sader & Joyce 1988; Veldkamp *et al.* 1992; Rosero-Bixby & Palloni 1998; Chaves-Esquivel & Rosero-Bixby 2001; Sanchez-Azofeifa & Harriss 2001). We use Costa Rica's *land use capacity classes*, which are determined by slope, soil characteristics, life-zones, risk of flooding, dry period, fog, and wind influences.

- *Distance to nearest major city*: The distance between a plot of land and the nearest agricultural markets has been identified as a key explanatory variable in surveys of the deforestation literature (Kaimowitz & Angelsen 1998; Barbier & Burgess 2001). Therefore, following Pfaff and Robalino (*unpublished study*), we include a measure of distance to one of three major cities, Limon, Puntarenas, and San Jose.

In Kaimowitz and Angelsen's (1998) review of deforestation studies, our core set of covariates are consistently found to causally affect deforestation. The causal effects of other covariates like population density and other socioeconomic characteristics (e.g., poverty, education) are less agreed upon. Nevertheless, we define an extended set of covariates that includes our core set plus the following:

- *Distance to railroads and river transportation network*. In addition to our measure of distance to roads, we also create a data layer that measures the distance from each plot to a railroad (1969) or a river that is part of the river transportation network (1969).

- *District-level population density*: Harrison (1991) finds strong correlations in Costa Rica between the population density in a canton and the level of deforestation, and this correlation has been confirmed in other studies for smaller land areas in Costa Rica (Rosero-Bixby & Palloni 1998; Chaves-Esquivel & Rosero-Bixby 2001). As with all of the measures below, we measure population density at district-level (*distrito*)[iv] from the 1973 census which is a mid-point in the main period of protection activity. (These measures are obtained from the 1984 census for the 1985-1997 analysis.)

- *District-level proportion of immigrants*: Harrison (1991) and Rosero-Bixby and Palloni (1998) find correlations between the percentage of immigrants and the level of deforestation.

[iv] Geographic boundaries for the 437 districts in 2000 are defined in a GIS data layer. The number of districts increased between 1973 and 2000 because some districts were split up to form smaller districts. We use information collected by the FAO on district splits over time (Cavatassi et al. 2004) to re-aggregate new districts to their 1973 parent districts. In a few cases, a new district is created from more than one parent district, in which case we re-aggregate the new district and all parent districts into one unit. The final dataset therefore has 398 “districts”.

- *District-level proportion of adults educated beyond the secondary level*: We use this variable as a measure of residents' opportunities for off-farm employment, which can reduce deforestation pressure (Mulley and Unruh 2004).
- *District-level proportion of households using fuel-wood for cooking*: We use this variable as a measure of the use of forest resources by district residents.
- *Size (area) of district*: District area is (negatively) correlated with administrative capacity and economic growth, which might influence deforestation and protected area placement.

To confirm the narrative and empirical evidence that our set of covariates known to affect deforestation also affects the designation of protected areas, we model the selection process directly using our data. We estimate marginal effects from a probit model that regresses deforestation on the core and extended sets of covariates. The most influential variables are the land-use capacity classes. Holding other relevant factors constant, the less productive the land is, the more likely the plot will be selected for protection. The other variables are also correlated with protection decisions, but their influence is not as large: less accessible plots (in terms of distance from forest clearings and roads) are more likely to be protected, as are plots in larger districts with lower population densities, a greater proportion of immigrants, and a greater proportion of educated citizens.

6. ANALYSIS AND RESULTS

6.1 AVOIDED DEFORESTATION FROM ALL PROTECTED AREAS

Protected areas are established over time and thus in the matching process, we want to ensure that the time-varying covariate data (see Table 1 below) are reasonable approximations to the period in which a protected area was established.[v] Thus, we break our analysis up into two cohorts: protected areas established before 1980 and between 1985 and 1997[vi].

We restrict the first treatment cohort to the forty-two protected areas established before 1980 for two reasons. First, this restriction allows more than fifteen years for a treatment effect to be observed. Second, a relatively large number of protected areas were established in the late 1960s and the 1970s, but few in the early 1980s. In the analysis below, we present results that allow matches with any unprotected plot, results that exclude plots protected after 1980, and results that adjust post-1980 protected plots for the treatment effects of post-1985 protection.

We draw a random sample of 20,000 land plots that were forested in 1960. Each plot has an area of 3 ha. This unit is the minimum mappable unit, or pixel, and thus our outcome variable is binary: a plot is either forested or deforested (forested = 80%+ canopy cover). The total forest cover in Costa Rica in 1960 is 30,357 sq km. Therefore, the dataset includes approximately one

[v] For example, for the 1960 to 1980 treatment, we obtain socio-economic data from the 1973 census data, which may be a reasonable proxy for conditions during the 1960s and 1970s, but not necessarily for later years.

[vi] We believe our data allow us to estimate the effects of protection more accurately within these periods than between 1980 and 1984. However, we include the 1980-1984 period in robustness checks of our results.

plot per 1.5 sq km of forest cover[vii]. We exclude lands controlled by indigenous people as treated or control units because they are subject to a different legal and land use regime. For similar reasons, we exclude a small number of government-designated wetlands. In addition to units from indigenous reserves and wetlands, we exclude the following units from the sample: 804 plots that were located in areas where GIS specialists suspected that incorrect forest cover classification may have occurred; 879 plots that were located in areas covered with clouds or shadows in Landsat images; and fifty-nine plots that did not align well with district areas because of errors in GIS programming.

The final dataset comprises 15,283 land plots. These plots include 2711 protected plots from thirty-three protected areas[viii]. Nine protected areas established before 1980 are not represented in our sample: five are islands that are not covered by the 1960 forest cover layer, and four are small protected areas that were not captured by the random sampling process because they are small.[ix]

[vii] To check the accuracy of the random sampling process, we confirmed that there were no significant differences between our sample of land plots and the population (entire land area) in terms of important characteristics (forest cover change, protected status, type of protection, and proportion under each land capacity class).

[viii] The following pre-1980 protected areas are represented in our sample. Biological Reserves: Alberto Manuel Brenes and Hitoy Cerere; Forest Reserves: Cordillera Volcanica Central, Golfo Dulce, Grecia, Los Santos, Rio Macho, and Taboga; Monumento Nacional: Guayabo; National Parks: Barra Honda, Braulio Carrillo, Cahuita, Chirripo, Corcovado, Juan Castro Blanco, Palo Verde, Rincon De La Vieja, Santa Rosa, Tortuguero, Volcan Iraza, Volcan Poas, Volcan Tenorio, and Volcan Turrialba; Protected Zones: Arenal-Monterverde, Caraigres, Cerro Atenas, Cerros de Escazu, Ceros de la Carpintera, El Rodeo, Miravalles, Rio Grande, and Tenorio; Wildlife Refuge: Corredor Fronterizo.

[ix] Two are small forest reserves, Pacuare-Matina and Zona de Energencia Volcan Arenal, one is the smallest national park, Manuel Antonio, and the last is a small protected zone around Rio Tiribi.

Table 1. Descriptive Statistics

Name	Description	Mean	Standard Dev.	Range
Deforestation 1960-1997	Coded 1 if forest was cleared between 1960 and 1997, 0 otherwise	.374	.484	0 – 1
Deforestation 1960-1986	Coded 1 if forest was cleared between 1960 and 1986, 0 otherwise	.369	.483	0 – 1
Deforestation 1986-1997	Coded 1 if forest was cleared between 1986 and 1997, 0 otherwise (units under forest in 1986 only)	.084	.277	0 – 1
Protection before 1980	Coded 1 if plot is in a protected area created before 1980, 0 otherwise	.171	.377	0 – 1
Protection 1980-1984	Coded 1 if plot is in a protected area created between 1980 and 1984, 0 otherwise	.085	.278	0 – 1
Protection 1985-1996	Coded 1 if plot is in a protected area created between 1985 and 1996, 0 otherwise	.061	.240	0 – 1
Distance to edge of forest 1960	Distance to closest clearing in 1960, measured in km	2.550	2.616	7.7×10^{5} – 17.675
Distance to edge of forest 1986	Distance to closest clearing in 1986, measured in km (units under forest in 1986 only)	11.515	1.293	.042 – 12.358
Distance to road 1969	Distance to nearest road in 1969, measured in km	18.260	12.935	0.004 – 63.641
Distance to railroads and river transportation 1969	Distance to nearest railroad or river transportation in 1969, measured in km	28.367	21.623	0.001 – 103.70
Distance to local road 1991	Distance to nearest local road in 1991, measured in km	5.026	5.354	4.8×10^{4} – 38.719
Distance to national road 1991	Distance to nearest national road in 1991, measured in km	7.381	7.084	2.3×10^{4} – 38.527
Distance to major city	Distance to closest major city (Limon, Puntarenas, or San Jose), measured in km	78.346	38.778	4.595 – 212.277
Land use capacity classes:	Dummy variables coded 1 if plot is inside a land class or classes, and 0 otherwise.			
Class I	Agricultural Production – annual crops	.001	.026	0 – 1
Class II	Suitable for agricultural production requiring special land and crop management practices such as water conservation, fertilization, irrigation, etc.	.033	.179	0 – 1
Class III	Suitable for agricultural production requiring special land and crop management practices such as water conservation, fertilization, irrigation, etc.	.088	.283	0 – 1
Class IV	Moderately suitable for agricultural	.125	.330	0 – 1

Name	Description	Mean	Standard Dev.	Range
	production; permanent or semi-permanent crops such as fruit trees, sugar cane, coffee, ornamental plants, etc.			
Class V	Strong limitations for agriculture; forestry or pastureland	.016	.127	0 – 1
Class VI	Strong limiting factors on agricultural production; land is only suitable for forest plantations or natural forest management	.169	.375	0 – 1
Class VII	Strong limiting factors on agricultural production; land is only suitable for forest plantations or natural forest management	.151	.358	0 – 1
Class VIII	Land is suitable only for watershed protection	.031	.173	0 – 1
Class IX	Land is suitable only for protection	.385	.487	0 – 1
District area	Area of district in which land plot is located, measured in square km	834.000	710.000	2.161 – 2410.000
Population density	Population density of district in which land plot is located, measured as number of people per square km (1973)	15.638	53.906	.886 – 3671.928
Percentage of immigrants	Number of people born outside their canton of residence (1973)	.458	.221	.014 – .913
Percentage of adults with secondary-level education	Percentage of adults with secundaria or universitaria level education (1973)	.055	.051	.007 – .335
Fuel-wood use	Percentage of households using fuel-wood for cooking (1973)	.740	.254	.088 – .994

We begin by ignoring spatial interactions and focusing only on the selection on observables problem. Recall that avoided deforestation is the difference between the change in forest cover (Y=1 if deforested) from 1960 to 1997 on protected plots and the change in forest cover in the same period on matched unprotected plots. Table 2 presents the treatment effect estimates using our matching estimators, as well as more traditional estimation methods in the conservation science literature. The results in Table 2 are based on our core set of covariates (see previous section). Note that negative treatment effects indicate that protection results in less deforestation than there would have been otherwise; i.e., avoided deforestation.

The first column of results places no constraints on the set of unprotected plots from which we can chooses matches for our protected plots. In Naughton-Treves *et al.*'s (2005) review of twenty published studies that analyze forty-nine protected areas, 27% of the analyses examine change in land cover only in the protected area to infer the protected area's effectiveness. Such studies implicitly assume that the counterfactual is 100% deforestation. The first row in Table 2 replicates this type of analysis. This grossly naïve treatment effect estimate suggests that 89% of the plots protected before 1980 would have been deforested by 1997 in the absence of protection.

The second row replicates the kind of analysis completed by the remaining protected area evaluations reviewed by Naughton-Treves et al.: deforestation on protected units is compared to deforestation on unprotected units, without controlling for any other covariates. This naïve treatment effect estimate implies that 36% of the protected plots would have been deforested by 1997 had they not been protected before 1980.

Some of the traditional "inside-outside" analyses restrict the control group to a 10-km unprotected zone around each protected area (e.g., Bruner *et al.* 2002). The third row replicates this type of analysis and generates a slightly smaller treatment effect: 33% of protected plots would have been deforested had they not been protected. Note that this estimate is lower than what would have been derived if we allowed our sample to include plots already deforested in 1960. Such "post-protection-only" analyses are obviously biased because deforestation may take place before protection is implemented and protection is much less likely to be assigned to deforested plots. However, such analyses can be found in the published literature (e.g., Bruner et al. 2002). Conducting this analysis with our Costa Rica data implies that 45% of protected plots would have been deforested had they not been protected.

The fourth row represents a treatment effect derived from a baseline reference, which is the most commonly suggested way of measuring avoided deforestation in climate change negotiations. This method first regresses deforestation in a period on observable characteristics. The estimated equation is then used to predict in the next period the expected deforestation probability for each forested parcel. The difference between the predicted and the actual deforestation rates for an area is the estimated avoided deforestation. Thus, for this analysis, we draw a new random sample of 20,000 pixels (with and without forest cover) and estimate a probit equation of deforestation for the period before 1960 using our core covariate set. Because we have no digitized observations of forest cover before 1960, we assume that all of our pixels were previously forested at some point in the past. Avoided deforestation is estimated to be 39% of the protected areas protected before 1980.

Table 2. Effect of Protection on Deforestation: Core Covariate Set

	1	**2**	**3A**	**3B**
Treatment group	**Protected pre-1980**	**Protected 1985-1996**	**Pre-1980 protected**	**Pre-1980 protected**
Control group	**Unprotected pre-1980**	**Never protected**	**Never protected**	**Unprotected pre-1980, with adjustment for post-1980 protection**
Outcome in treatment group only	-0.888	-0.968	-0.888	-0.888
Difference in Means†	-0.355	-0.112	-0.438	-0.419
Difference in Means: controls within 10km of protected area [N available controls]	-0.326 [4507]	-0.097 [2130]	-.0375 [3866]	-0.359 [4201]
Baseline Reference Estimate	-0.392	-0.261		
Covariate matching – Inverse variance‡	-0.045 (0.065)	-0.067 (<0.001)	-0.113 (<0.001)	-0.110 (<0.001)
Covariate Matching – Mahalanobis	-0.049 (0.034)	-0.061 (<0.001)	-0.111 (<0.001)	-0.115 (<0.001)
Covariate Matching – Mahalanobis with calipers▫ [N outside calipers]	-0.056 (<0.001) [237]	-0.061 (<0.001) [43]	-0.124 (<0.001) [411]	-0.129 (<0.001) [320]
Propensity score matching – Kernel [N off common support]	-0.048 (<0.001) [0]	-0.075 (<0.001) [0]	-0.134 (<0.001) [117]	-0.123 (<0.001) [74]
N treated (N available controls)	2711 (12572)	557 (4724)	2711 (10371)	2711 (11078)

† A Chi-squared test is used to evaluate the difference in means between protected and unprotected units.

‡ Numbers in parenthesis under matching estimates represent p-values.

▫ Calipers restrict matches to units within 0.5 standard deviations of each covariate.

The fifth through eighth rows present the treatment effect estimates from the matching estimators. All imply that about 5% of protected plots would have been deforested by 1997 in the absence of protection, but not all are significant at the 1% level. These dramatically different estimates imply that the traditional methods used to evaluate protected area effectiveness do not fully remove the sources of bias.

Note that although matching substantially improved the covariate balance between treated and control plots, some imbalance remains: protected plots are slightly farther from the forest frontier and from transportation infrastructure than their match counterparts. Given these two covariates are negatively correlated with deforestation, the matching estimates may still be biased away from zero (i.e., they are too large). Moreover, protection occurred over time between 1960 and 1980, but we only observe forest cover in 1960. At any point in time, deforested parcels are much less likely to be protected than forested parcels, and thus our matches may be imperfect in another way that biases the treatment effects away from zero.

To put Table 2's estimates into perspective, consider that 483,339 ha of forest were protected between 1960 and 1980. Thus the first row estimate implies that formal protection resulted in 429,205 ha of avoided deforestation. The second, third and fourth row estimates imply 157,569 to 189,469 ha of avoided deforestation. The matching estimators imply only 21,750 to 27,067 ha of avoided deforestation.

One could reasonably argue, however, that land plots protected between 1980 and 1997 are not valid counterfactuals if protection after 1980 had a protective effect. In the second column of Table 2, we present estimates that corroborate this argument. Based on the matching estimators, 6 - 7% of the protected forested plots between 1985 and 1997 would have been deforested by 1997 had they not been protected. Note that the differences among the matching and traditional estimates are not as dramatic as in the first column. The smaller differences, combined with the knowledge that deforestation rates were low across the nation between 1986 and 1997, suggest better targeting of protected areas post-1985 in terms of deforestation threat.

Given that post-1980 protection led to avoided deforestation, we replicate, in the third column of Table 2, the estimates of avoided deforestation for pre-1980 protection after excluding from the sample all plots that were protected after 1980. Note that post-1980 protected areas are typically located near the pre-1980 protected areas. Therefore, the sample we obtain after excluding post-1980 protected plots is similar to a sample that would be obtained after some form of "spatial sampling" to exclude counterfactuals that are located close to treated units (see Mertens and Lambin 2000; Munroe *et al.* 2002).

The treatment effects in the third column are larger than the estimates in the first column, but the matching estimators still generate avoided deforestation estimates that are much smaller than those generated by traditional methods. The matching estimators imply avoided deforestation estimates of 43,651 to 64,767 ha. The larger treatment effect under the post-1980 exclusion is consistent with two interpretations: (1) protection after 1980 had a protective effect and thus using post-1980 protected plots as counterfactuals for pre-1980 protection biases the treatment effect toward zero; or (2) plot characteristics are spatially correlated and thus the quality of the matches declines when post-1980 plots are excluded from the sample.

To explore the second interpretation, we examine the covariate balance between matched control and treated units in the analyses of the first and third columns. In the third column's analysis, balance is slightly worse for covariates that favor protection for protected units, but not substantially so. As a robustness check, and to demonstrate how one might address a situation in which balancing becomes substantially worse with spatial sampling, we propose an alternative approach that directly adjusts the sample to incorporate the treatment effects from post-1985 protection[x].

We estimate that post-1985 protection led to avoided deforestation of 6.525% (average of matching estimates in the second column). In our sample, this percentage corresponds to 36 plots. We thus randomly select thirty-six plots that were protected between 1986 and 1997, and were not deforested within that period, and we change their status from "forest" to "deforested" in 1997. We then estimate the treatment effect of pre-1980 protection, maintaining the control units that were protected between 1985 and 1996. The results from this adjusted analysis are presented in the fourth column of Table 2 and are similar to those in the third column.

We also calculated treatment effects using the extended covariate set. The treatment effects from the fully specified probit model and from the matching estimator are similar to

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[x] In our sample, 557 plots were protected between 1985 and 1997. These plots are located in the following protected areas established between 1985 and 1997. Biological Reserves: Cerro las Vueltas and Lomas de Barbudal; Forest Reserve: Rio Pacuare; National Parks: Arenal, Guanacaste, and Piedras Blancas; Protected Zones: Acuiferos Guacimo y Pococi, Cuenca del Rio Banano, Cuenca del Rio Siquirres, Cuenca Rio Abangares, Cuenca Rio Tuis, Montes de Oro, Nosara, Peninsula de Nicoya, Rio Toro, Tivives, and Tortuguero; Wildlife Refuges: Aguabuena, Bahia Junquillal, Barra del Colorado, Bosque Alegre, Bosque Nacional Diria, Camaronal, Fernando Castro Cervantes, Gandoca-Manzanillo, Golfito, Hacienda Copano, La Marta, Limoncito, Mata Redonda, Penas Blancas, and Rancho La Merced.

results in Table 2 and are thus not reported in a table. The covariate matching estimator estimates range from -0.044 to 0.146, and the kernel matching estimates range from -0.096 to -0.205. The latter matches, however, show much worse balance than in the covariate matching on coefficients that bias the treatment effect up in absolute value (i.e., land use capacity, distance to transportation infrastructure).

6.2 AVOIDED DEFORESTATION FROM GEF-FUNDED PROTECTED AREAS

Between 1993 and 1998, the GEF provided funding for activities in Corcovado and La Amistad national parks through the Biodiversity Conservation and Sustainable Development in the Amistad and Osa Conservation Areas project (GEF Project ID 364)[xi]. We estimate the effect of these two protected areas on deforestation between the periods 1986-1997 and 1997-2005. This analysis answers the question, "how much more deforestation would have occurred in the periods 1986-1997 and 1997-2005 if the two protected areas had not been established?"

For this analysis, we draw random samples of 4,000 plots within the GEF-funded protected areas (2,000 plots each within Corcovado and La Amistad national parks), 10,000 plots outside Corcovado national park but within the Osa peninsula, and 20,000 plots outside all protected areas established before 1980. We use the criteria described in the previous section to exclude some plots from the analysis and the final dataset consists of 50,533 plots.

[xi] The project was implemented by UNDP and executed by SINAC. The project cost $8 million, and its timeline was 1993-1998. Project activities included improving administrative capacity, repairs to access roads, and the establishment of protected area borders.

The results of this analysis are presented in Table 3. The estimates in the first three rows are obtained by applying the traditional estimation methods described in the previous section, and the interpretation of the results follows the interpretation presented in the previous section for the results in Table 2. Negative treatment effects indicate that protection results in less deforestation than there would have been otherwise; i.e., avoided deforestation.

In the first column, we restrict the controls to plots that were not protected before 1985. Inverse variance covariate matching in row 4 indicates that about 5% of the plots under the GEF-funded protected areas would have been deforested between 1986 and 1997 if they had not been protected. However, the Mahalanobis covariate matching estimate in row 5 implies 23% avoided deforestation. However, when we ensure quality control using calipers in row 6, more than half of the treatment group is excluded from the analysis and the Mahalanobis estimate is less than 1% avoided deforestation. Therefore, our best estimates of the effect of the two protected areas on deforestation 1986-1997 are 5% (row 4) and 8.5% (row 7).

We measure the effects of protection on deforestation 1997-2005 in column 3. The matching estimates in rows 4 and 5 imply that there was no significant difference in deforestation between the protected plots and their matched unprotected lands. However, when we enforce further restrictions on the quality of matches with calipers (row 6) or by enforcing common support (row 7), we find significant effects of protection. The highest estimate in row 7 indicates that there was 11% avoided deforestation between 1997 and 2005 from the two GEF-funded protected areas.

Table 3. Effect of Protection on Deforestation in GEF-Funded Protected Areas: Core Covariate Set

	1986-1997		1997-2005	
	1	**2**	**3**	**4**
Treatment group	**GEF-Funded Protected Areas**	**GEF-Funded Protected Areas**	**GEF-Funded Protected Areas**	**GEF-Funded Protected Areas**
Control group	**Unprotected pre-1985**	**Never protected**	**Unprotected pre-1985**	**Never protected**
Outcome in treatment group only	-0.996	-0.996	-0.998	-0.998
Difference in Means†	-0.140	-0.151	-0.133	-0.152
Difference in Means: controls within 10km of protected area [N available controls]	-0.119	-0.143	-0.124	-0.158
Covariate matching – Inverse variance‡	-0.054 (0.051)	-0.056 (0.054)	-0.011 (0.676)	-0.001 (0.980)
Covariate Matching – Mahalanobis	-0.229 (<0.001)	-0.232 (<0.001)	0.023 (0.402)	0.004 (0.910)
Covariate Matching – Mahalanobis with calipers▫ ***[N outside calipers]***	-0.004 (0.091) [2922]	-0.005 (0.057) [2984]	-0.039 (<0.001) [2902]	-0.053 (<0.001) [3063]
Propensity score matching – Kernel [N off common support]	-0.085 (<0.005) [1412]	-0.084 (<0.005) [1851]	-0.107 (<0.001) [1790]	-0.118 (0.021) [665]
N treated (N available controls)	3488 (6042)	3488 (5583)	3490 (5971)	3490 (5039)

† A Chi-squared test is used to evaluate the difference in means between protected and unprotected units.

‡ Numbers in parenthesis under matching estimates represent p-values.

▫ Calipers restrict matches to units within 0.5 standard deviations of each covariate

In the previous section, we note that post-1985 protection had some effects on deforestation rates and that this may affect the validity of using controls protected post-1985. Therefore, in the second and fourth columns of Table 3, we replicate the analysis in the first and third columns, excluding controls protected post-1985. We obtain results that are similar to the results we obtained when we did not place this restriction on the selection of controls. Note that for all our analyses in this section, even the highest matching estimates are slightly lower than the estimates using the traditional methods of comparing protected and unprotected lands (rows 2 and 3).

The total forest areas under the two GEF-funded protected areas are 230,689 ha and 230,898 ha in 1986 and 1997 respectively. Thus, our matching estimates imply that between 12,457 ha and 19,609 ha of forest in the period 1986-1997 and at most 25,399 ha of forest in 1997 were not deforested because they were under protection in these two parks.

We also estimate the treatment effects for these GEF-funded areas using the extended covariate set. The results are similar to the results presented in Table 3. The matching estimates lie within a range of 3% to 30% for the period 1986-1997 and 0% and 11% for the period 1997-2005. However, as noted in the previous section, balancing is worse for the extended covariate analysis compared with the core covariate analysis.

6.3 COMPARING GEF-FUNDED AND NON GEF-FUNDED PROTECTED AREAS

We apply our matching methods to estimate the differences in avoided deforestation from the GEF-funded protected areas (described in the previous section) and other protected areas that did not receive GEF funding. For this analysis, we obtain control units by drawing a random sample of 20,000 plots within non-GEF funded protected areas. In this analysis, our goal is to determine whether GEF-funded protected areas contributed differently to avoided deforestation than other protected areas.

The results are presented in Table 4. In the first column, the matching estimates in rows 2 and 3 imply that GEF-funded protected areas resulted in 2 to 7 percentage points more avoided deforestation than non-GEF-funded protected areas for the period 1986-1997. The results in the second column imply that there is little or no difference in the avoided deforestation from the two groups of protected areas for the period 1997-2005. Only one of the matching estimates in the second column is significantly different from zero, and that estimate in row 4 implies only about 3% difference in avoided deforestation between GEF-funded and non GEF-funded protected areas. Given that 230,898 ha of forest in 1997 were under the GEF-funded protected areas, this estimate implies that at most, GEF-funded protected areas contributed 7,000 ha more avoided deforestation than non GEF-funded protected areas.

When we match based on the extended set of covariates, we obtain similar results. The avoided deforestation estimates lie between 3% and 8% for 1986-1997 and between 2% and 5% for 1997-2005 and only one of the matching estimates is significantly different from zero at the 1% level.

Table 4. Effect of Protection on Deforestation in GEF-Funded versus Non GEF-Funded Protected Areas: Core Covariate Set

	1	**2**
	1986-1997	**1997-2005**
Treatment group	**GEF-Funded Protected Areas**	**GEF-Funded Protected Areas**
Control group	**Other Protected Areas created before 1983**	**Other Protected Areas created before 1983**
Difference in Means †	-0.033	-0.021
Covariate matching – Inverse variance ‡	-0.030 (0.094)	-0.016 (0.229)
Covariate Matching – Mahalanobis	-0.043 (0.022)	-0.017 (0.157)
Covariate Matching – Mahalanobis with calipers ▫ ***[N outside calipers]***	-0.074 (<0.001) [1265]	-0.027 (0.003) [1265]
Propensity score matching – Kernel ***[N off common support]***	-0.020 (<0.025) [358]	-0.001 (>0.400) [739]
N treated (N available controls)	3488 (7406)	3490 (7430)

† A Chi-squared test is used to evaluate the difference in means between protected and unprotected units.

‡ Numbers in parenthesis under matching estimates represent p-values.

▫ Calipers restrict matches to units within 0.5 standard deviations of each covariate

6.3 SPATIAL INTERACTIONS AND MATCHING ESTIMATORS

As noted in Section 2, land use regulations may generate spillovers into untreated land plots in the neighborhoods around protected areas. Highly parametric, traditional spatial econometric models (e.g., a probit with spatial lag) risk a specification bias when controlling for such spillovers. Moreover, generating a transparent estimate of the average spillover effect is not easily done through interpretation of the spatial lagged coefficient. We therefore use matching estimators to test for spatial spillovers.

To begin, we define the treatment group as unprotected plots that are within two kilometers of the boundary of protected areas created before 1980, and we define the control group as unprotected plots that are more than two kilometers away from the protected areas. A negative treatment effect implies a positive spillover: a positive spillover occurs when plots near protected areas experience less deforestation.

For the analysis of spatial spillovers from pre-1980 protected areas, we wish to avoid any potential estimation bias due to spillovers from post-1980 protected areas. We do this by estimating spatial spillovers within the 1960-1986 period instead of the 1960-1997 period that we used to estimate the direct effects of protection. For the latter analysis, we are able to identify and exclude control units that could have been affected by post-1980 protection (columns 3 and 4 of Table 2). However, for the spillover analysis, we have no way of defining the extent of potential spillovers from post-1980 protection. Therefore, we use the earliest available measure of deforestation after 1980 (1986) as the outcome for this analysis.

The estimates of spatial spillover effects are presented in Table 5. In the first column, we test for spatial spillover effects of protection on deforestation between 1960 and 1986. The estimates from the traditional methods in the first two rows indicate positive spillover effects, but the matching estimates are ambiguous. With the exception of the kernel estimate, the matching estimates imply that plots within 2-km of protected areas established before 1980 experienced about 4% less deforestation than plots more than 2-km away from protected areas. Only the kernel estimate is sizeable and significant at the 1% level. However, the covariate balancing using this estimator is worse on variables that would bias the estimate away from zero.

In the second column, we test for spillover effects on deforestation between 1986 and 1997, defining treatment as location within 2-km of protected areas created between 1985 and 1996. We find no evidence of substantial spillover effects with the matching methods. For both time periods, we also test for spillovers in subsequent intervals (2-4 km, 4-6 km, 6-8 km) and we do not find treatment effects that are significantly different from zero at the 1% level.

In the third and fourth columns, we test for spillover effects from the GEF-funded protected areas on deforestation between 1986-1997 and 1997-2005. The treatment is defined as location within 2-km of the GEF-funded protected areas. To avoid bias from spillovers from other protected areas, we exclude from the control group all plots within 2-km of protected areas established before 1996 for the 1986-1997 analysis (before 2004 for the 1997-2005 analysis). For both periods, three of the four matching estimators indicate that there are no significant spillover effects from the GEF-funded protected areas. The only matching estimate that indicates some negative spillover effects is the Mahalonobis matching with calipers in column 5.

Table 5. Spatial Spillover Effect of Protection on Deforestation

	1960-1986	1986-1997	1986-1997	1997-2005
Treatment group	Unprotected units within 2-km of pre-1980 Protected Areas	Unprotected units within 2-km of 1985-1996 Protected Areas	Unprotected units within 2-km of GEF-funded Protected Areas	Unprotected units within 2-km of GEF-funded Protected Areas
Control group	Unprotected units more than 2-km away from Pre-1980 Protected Areas	Unprotected units more than 2-km away from 1985-1996 Protected Areas	Unprotected units more than 2-km away from Pre-1996 Protected Areas	Unprotected units more than 2-km away from Pre-2004 Protected Areas
Outcome for treated units only	-0.628	-0.879	-0.786	-0.854
Difference in Means†	-0.168	-0.026	0.053	-0.029
Covariate matching – Inverse variance‡	-0.039 (0.076)	0.001 (0.965)	0.110 (0.169)	-0.034 (0.690)
Covariate Matching – Mahalanobis	-0.043 (0.052)	0.001 (0.950)	0.099 (0.234)	-0.032 (0.718)
Covariate Matching – Mahalanobis with calipers▫ *[N outside calipers]*	-0.045 (0.028) [53]	0.0005 (0.981) [30]	0.120 (0.042) [25]	0.136 (0.006) [30]
Propensity score matching – Kernel [N off common support]	-0.116 (<0.001) [4]	-0.028 (0.095) [0]	0.054 (0.379) [0]	-0.024 (0.675) [1]
N treated (N available controls)	1060 (9849)	430 (4294)	42 (4713)	48 (3854)

† A Chi-squared test is used to evaluate the difference in means between protected and unprotected units.

‡ Numbers in parenthesis under matching estimates represent p-values.

▫ Calipers restrict matches to units within 0.5 standard deviations of each covariate

However, for this estimate, the calipers excluded about 60% of the treated group from the analysis, and therefore this may not be a representative estimate of spillover effects from the two protected areas. We conclude that there are no substantial spillover effects from the GEF-funded protected areas.

Our results suggest that spatial spillovers from protected areas are either absent or positive but small. Given that we estimated small treatment effects of protected areas, the lack of evidence for negative spillover effects from protection is not surprising. Our selection models and balancing tests suggest that there would be low deforestation pressure on protected lands, implying that protection would lead to little or no displacement of deforestation pressure onto neighboring unprotected lands.

Because we do not detect substantial spillover effects on deforestation on neighboring unprotected lands arising from the establishment of protected areas between 1960 and 1996, we conclude that the estimates in Table 2 reflects the full effect of protected areas both within and outside protected areas. Had we found evidence of such spillovers, we would resort to the spatial sampling and sample adjustment methods used in the previous section to control for post-1980 treatment effects in the pre-1980 estimates.

Thus our best estimate of avoided deforestation between 1960 and 1997 *within* and *outside* protected areas established before 1980 is between 5% and 15% of the area protected. These values correspond to avoided deforestation between 24,167 ha and 72,501 ha. We can also provide an estimate of avoided deforestation from protected areas established post-1980. Between 1980 and 1984, 244,168 ha of forest were placed under protection (based on 1986 forest map), and another 175,906 ha of forest were protected between 1985 and 1996. Using our matching methods, we estimate that the treatment effect for protected areas established between 1985 and 1996 is between 6% and 7%, which corresponds to avoided deforestation between 10,554 ha and 12,313 ha. If we assume that the treatment effect of protection between 1980 and 1984 lies somewhere between our estimates for pre-1980 and post-1985 protection, then our estimate of avoided deforestation for 1980-1984 would lie within the range of 14,465 ha to 16,875 ha. **Therefore, our best estimate of avoided deforestation between 1960 and 1997 from all protected areas is between 49,186 ha and 111,356 ha.**

Similarly, since we do not find substantial spillover effects from GEF-funded protected areas for both 1986-1997 and 1997-2005, we conclude that the estimates in Table 3 represent the total effect of protection on deforestation inside and outside the GEF-funded protected areas. **Therefore, our best estimate of avoided deforestation between 1986 and 1997 from GEF-funded protected areas is between 12,457 ha and 19,609 ha. For 1997-2005, our best estimate is that GEF-funded protected areas resulted in less than 25,399 ha of avoided deforestation.**

6.4 ANALYSES OF SENSITIVITY TO HIDDEN BIAS

We follow Rosenbaum (2002) to determine how strongly an unmeasured confounding variable must affect selection into the treatment to undermine our conclusions. Recall that the assumed unobserved covariate is a strong confounder: one that not only affects selection but also determines whether deforestation is more likely for the treatment or the matched control units.

The first column in Table 6 indicates that the estimated negative treatment effect of protection between 1960 and 1997 for all protected areas, using the core covariates, remains significantly negative even in the presence of moderate unobserved bias. The results imply that if an unobserved covariate caused the odds ratio of protection to differ between protected and unprotected plots by a factor of as much as 3, the 99% confidence interval would still exclude zero. The second column indicates that the estimated treatment effect, using the extended covariate set, is also robust to unobserved hidden bias. If we were to exclude from the sample plots protected after 1980, we obtain similar qualitative conclusions. The third column indicates that the estimated treatment effect of protection between 1985 and 1996 is robust to substantial unobserved hidden bias. The fourth and fifth columns indicate that the results for the effects of GEF-funded protected areas between 1986 and 1997 are also robust to unobserved hidden bias (the results for 1997-2005 GEF-funded protection are similar and so are not presented here).

We can use the same methods to examine the degree to which unobserved bias causes us to <u>underestimate</u> the effect of protection (in absolute value). We construct 99% confidence intervals for our estimate under varying degrees of unobserved bias. Even if an unobserved covariate causes the odds ratio of protection to differ between protected and unprotected plots by a factor of 4, the 99% confidence interval would still exclude the naïve treatment effect estimates from the first three rows of Table 2. The upper bound of the interval is -0.230.

Thus our conclusions are robust to hidden bias: (1) protection led to avoided deforestation, but (2) the level of avoided deforestation is much less than what empirical methods commonly used in the conservation science literature would estimate.

Table 6. Rosenbaum critical p-values for treatment effects. Test of the null of zero effect.

	Protected area treatment effect				
Γ	**Protection pre-1980: Core covariate set**	**Protection pre-1980: Extended covariate set**	**Protection 1985-1996**	**GEF-Funded Protection (1986-1997)**	**GEF-Funded vs. Non GEF-Funded Protection (1986-1997**
1	<0.001	<0.001	<0.001	<0.001	<0.001
1.5	<0.001	<0.001	<0.001	<0.001	<0.001
2	<0.001	<0.001	<0.001	<0.001	<0.001
2.5	<0.001	<0.001	<0.001	<0.001	<0.001
3	<0.001	<0.001	<0.001	<0.001	<0.001
3.5	0.075	0.044	<0.001	<0.001	<0.001
4	0.844	0.766	<0.001	<0.001	<0.001

6.5 OTHER ROBUSTNESS CHECKS

We conduct additional robustness checks to examine the sensitivity of the treatment estimates to the composition of the sample the matching specifications, and we are able to confirm that the estimated treatment effects are robust. We experiment with various sample compositions and matching specifications (see list below). The matching estimates of avoided deforestation from pre-1980 protection always lie between 5% and 22% (core and extended covariate sets). This range is similar to the range of estimates from the main analysis in Table 2.

Moreover, the matching estimates are always smaller than their corresponding estimates obtained using the traditional estimation methods. Therefore, the robustness checks support our qualitative conclusion that the traditional methods consistently over-estimate the avoided deforestation from Costa Rican protected areas. The robustness checks are described briefly below.

- *Maintaining indigenous reserves and wetlands:* We estimate treatment effects without excluding indigenous reserves and wetlands from the sample;
- *Excluding protected areas established in 1985 from 1986-1997 analysis:* We estimate the treatment effects of protection on deforestation between 1986 and 1997, using protection between 1986 and 1996 as the treatment instead of protection between 1985 and 1996;

- *Maintaining protected areas established between 1980 and 1984*[xii] *without adjustment:* In the main analysis, we excluded 1980-1984 protected areas because we believed that 1960 forest data were too old for matching these parcels. Here, we include them and estimate the effects of pre-1980 protection on deforestation between 1960 and 1997.

- *Maintaining protected areas established between 1980 and 1984 with adjustment:* We repeat the robustness check above with one modification. We assume that 1980-1984 protected areas and 1985-1996 protected areas have similar treatment effects. Then, based on the estimated treatment effect of protected areas created between 1985 and 1996, we adjust the deforestation outcome in 1997 for units that were protected between 1980 and 1984. The adjustment procedure is similar to the one described in the Results section for plots protected between 1985 and 1996.

- *Varying the number of nearest neighbors:* We vary the number of nearest neighbors that are matched with treatment plots from 1 to 10.

- *Varying the kernel bandwidth:* We estimate kernel-based propensity score matching with kernel bandwidths 0.01 and 0.11.

- *Matching without bias-correction:* We compare our matching estimates to matching estimates without Abadie and Imbens' (2006a) post-matching, bias correction.

[xii] In our sample, 1,545 plots were protected between 1980 and 1984. The following protected areas established between 1980 and 1984 are represented in our sample. National Parks: Barbilla, Carara, and Parque Internacional la Amistad; Protected Zones: Cerro Narra, Cerros de Turrubares, El Chayote, La Selva, Las Tablas, and Rio Navarro y Rio Sombrero; Wildlife Refuge: Cano Negro.

- *Matching with alternative measures of land use capacities*: We replace our land-use capacity categories with measures of slope and Holdridge (1967) Life Zones, as used in other deforestation studies in Costa Rica (e.g., Pfaff and Robalino).

We also estimate the ATT at the larger unit of *distritos* (administrative districts), in which the outcome variable is the area of forest in 1960 that was deforested by 1997. Treatment is defined as 5% or more of the district under protection before 1980. The matching covariates, measured at the district level, are: area of forest in 1960, district area, road density in 1969, density of railroad and river transportation network in 1969, average distance from major cities, percentage of district in each land use capacity class, population density, percentage of population with secondary education, percentage of population that are immigrants, percentage of population that uses firewood. We obtain a wider range of avoided deforestation estimates at this coarser scale compared to the results from the pixel-level analysis. Some of our matching estimates suggest that there was no significant avoided deforestation from protection while others detect some avoided deforestation. If we assume that the covariate matching estimator using calipers is the highest quality estimate, then our district-level avoided deforestation estimates are similar to those generated at the pixel-level (e.g., about 25,000 ha with the core covariate set).

6.6 EFFECT OF PROTECTION ON REFORESTATION

As a demonstration of how this evaluation approach can be used to measure other outcomes that the GEF might be interested in, we use the methods of the previous four sections to estimate the effect of protection on reforestation inside and outside of all Costa Rican protected areas. Since this is a demonstration only, we do not repeat this analysis for the subset of GEF-funded protected areas.

We draw a new sample of 20,000 land plots that were *deforested* in 1960 and the outcome is "reforested by 1997." All aspects of the analysis, including rules for excluding plots, are otherwise the same as those used in the avoided deforestation analysis. The final dataset comprises 15,913 land plots.

The treatment effect estimates from matching with the core and extended covariate sets are similar, and range from 0.179 to 0.284 for pre-1980 protection (i.e., 18% - 28% of additional reforestation). This corresponds to between 6593 ha and 10,460 ha of land reforested between 1960 and 1997 as a result of protection before 1980. For post-1985 protection, matching estimates range from 0.05 to 0.087, or between 3222 ha and 5606 ha of additional reforestation. We detected no spatial reforestation spillovers in neighboring lands as a result of protection.

7. CONCLUSION

7.1 DISCUSSION OF RESULTS

Empirical assessments of the role protected areas play in land use patterns are central to policies related to ecosystem protection and the provision of ecosystem services. In particular, protected areas play key roles in current climate change and biodiversity policy debates. Measuring avoided deforestation and reforestation that would not have taken place in the absence of formal protection is difficult because they are counterfactual events. Moreover, the potential for positive and negative spillovers onto unprotected ecosystems further complicates the evaluation of protected area effectiveness.

We find that only about ten percent or less of the Costa Rican forest protected between 1960 and 1997 would have been deforested in the absence of protection: between 49,186 ha and 111,356 ha. Another 8415 ha to 16,066 ha of deforested land recovered its forest cover because of protection. Our analysis also suggests that, on average, spillover effects are small and can be ignored (if they exist, they appear to be positive; i.e., protection may lead to small amounts of avoided deforestation in neighborhoods near protected areas).

In our evaluation of GEF-funded protected areas, which received funding between 1993 and 1998, we estimate that protection resulted in 5% to 8% (12,457 ha and 19,609 ha) avoided deforestation between 1986 and 1997. In the period 1997-2005, protection of these same areas resulted in 11% (25,399 ha) or less avoided deforestation. We find that the GEF-funded protected areas reduced deforestation by a little more than other protected areas: between 2 and 7 percentage points for 1986-1997 and at most 2 percentage points for 1997-2005. We find our results robust to alternative specifications and measures, as well as to unobservable confounders that affect both protection and deforestation.

Note, however, that we are not explicitly modeling the effect of funding levels, but rather the effect of protection. Thus we cannot completely isolate the effects of GEF funding from the effects of other investments made into these same protected areas. If the Costa Rican government viewed GEF funds as a substitute for its own, it may have funded these areas at levels lower than it would have in the absence of GEF funds (thus making GEF funding look less effective in our analysis). Alternatively, the GEF may have simply invested its funds into protected areas that were already successful in the absence of GEF funds (thus making GEF funding look more effective in our analysis). Furthermore, we are looking only at one aspect of conservation outcomes: avoided deforestation. GEF investments may have affected other outcomes that are not measured directly in this evaluation.

The limited effectiveness of protected areas in changing land use patterns in Costa Rica stems from administrative targeting of protection towards forests for which private agents had few incentives to deforest. In other words, the Costa Rican government chose to protect lands that were generally low in economic and political cost. This pattern highlights an important complication in proposals to allow nations to generate avoided deforestation credits: asymmetric information between the suppliers and the certification agents. Avoided deforestation is an unobservable event and a nation may have better information than outsiders about where deforestation is will likely take place. A cap-and-trade system that allows nations to set their own caps provides strategic incentives for nations to take advantage of this private information when setting their caps. Regardless of the source of emissions, such incentives are a problem in any system that allows each nation to set its own cap, (e.g., a nation may be aware that its industrial base is declining because of broader economic conditions). However, the private information about deforestation risk is arguably better than the information about future economic conditions that will affect a nation's other sources of greenhouse gas emissions.[xiii]

[xiii] The potential for asymmetric information to reduce additionality is even higher in proposals to allow avoided deforestation credits to be sold in offset arrangements, where polluters in a capped system are allowed to trade with

Although poor targeting clearly contributed to the low levels of avoided deforestation from protection, there are other potential contributors. Costa Rican policymakers in the 1960s and 1970s may have expected deforestation pressures to continue unabated into the 1980s and 1990s. They may have thus decided to gazette lands that were inexpensive to gazette in the 1960s and 1970s (i.e., low pressure) in order to create a bulwark against deforestation pressures after 1980. However, structural readjustment in the mid-1980s lead to a cessation of agricultural subsidies, which, when combined with growth of the manufacturing and service sectors, greatly reduced deforestation pressures (DeCamino *et al.* 2000).

One should also remember that our analysis is retrospective. The future role of Costa Rica's protected areas in affecting land use may be different from the past (but such a difference would require a fundamental change in the historical deforestation processes). Moreover, protected areas are designated for reasons other than preventing deforestation. For example, forests may be protected to generate opportunities for tourism, to restrict hunting, to protect rural livelihoods associated with low-level extractive activities, or to raise environmental awareness among citizens and firms. Thus one should not necessarily infer that Costa Rica's protected area network has generated few benefits simply because the gains in terms of avoided deforestation were small.

polluters in an uncapped system. Here it is the act of protection that generates the credits and thus the incentives to claim avoided deforestation where none exists is even stronger.

7.1 EVALUATING OTHER GEF INVESTMENTS

As we note in the Introduction, one of the "Main Findings" of the *Millennium Ecosystem Assessment* (2005) is that there are few well-designed empirical evaluations of biodiversity conservation policies. The methodology used in this evaluation of protected area effectiveness can be used to improve impact evaluations of GEF investments in projects such as payments for environmental services, ecotourism projects, and community forest management. Note that while we focus on the utility of this approach for measuring impacts of biodiversity programs, these methods can be used in impact evaluations of GEF projects in other focal areas such as climate change and international waters. This evaluation approach is particularly useful for measuring the impact of GEF interventions with two characteristics: (1) implementation is assigned to different geographic areas in a nonrandom manner; and (2) the project may result in spatial spillover effects on neighboring areas. The key to implementing this evaluation approach successfully is to establish valid counterfactuals that measure the outcomes that would have occurred without the GEF intervention. This requires the collection of data in non-project areas in addition to data collection in project areas. The main data requirements for applying this methodology include measures of outcomes or indicators before and after the implementation of the project, and measures of important characteristics that potentially influence outcomes in both project and non-project locations.

This evaluation approach may be used to assess different outcomes of a conservation program. For example, although the main outcome measure we considered in this study is avoided deforestation, we demonstrate how this methodology can be easily adapted to evaluate reforestation from protected areas. Once suitable counterfactuals have been identified by matching project areas with non-project areas, the impact of the policy or project can be obtained by measuring differences in outcomes, as we have demonstrated in this study.

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