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Essays on the Evaluation of Land Use Policy: The Effects of Regulatory Protection on Land Use and Social Welfare

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ESSAYS ON THE EVALUATION OF LAND USE POLICY: THE EFFECTS OF REGULATORY PROTECTION ON LAND USE AND SOCIAL WELFARE

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ESSAYS ON THE EVALUATION OF LAND USE POLICY: THE EFFECTS OF REGULATORY PROTECTION ON LAND USE AND SOCIAL WELFARE

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In loving memory of my father

Kwesi Akwansah Andam

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SUMMARY

Research Questions

Societies frequently implement land use policies to regulate resource extraction (e.g. national parks or payments for environmental services) or to regulate development (e.g. zoning in urban areas or road building prohibitions). However, two important policy questions remain unresolved. First, how effective are land use regulations? Second, how do land use regulations affect economic conditions?

Three issues complicate the evaluation of the effects of land use policies: (1) overt bias may lead to incorrect estimates of a policy's effects where policy implementation is nonrandom (selection on observables); (2) the policy may affect outcomes in neighboring unregulated lands (spatial spillovers); and (3) Unobservable differences between regulated and unregulated lands may lead to bias in the evaluation (hidden bias). Previous evaluations of land use policies fail to address these sources of bias simultaneously.

In this dissertation, I develop an approach that jointly accounts for these complications. I apply the approach to evaluate the effects of Costa Rica's protected areas on land use and socioeconomic outcomes from 1960 to 2000. Specifically, I address three questions: (1) What is the effect of protected areas on deforestation inside and outside protected areas? (2) What is the effect of protected areas on reforestation inside and outside protected areas? (3) What is the effect of protected areas on socioeconomic outcomes in communities around protected areas?

Methodology

I use matching methods to identify unprotected lands that are similar to protected lands in terms of characteristics that jointly affect both the likelihood of protection and the land use or poverty outcomes. Matching methods ensure that any remaining differences in outcomes between protected and unprotected lands can be attributed to the effect of protection policies. In addition, I measure spillover effects by using the matching procedure to find suitable lands to compare with unprotected lands located near protected areas.

Main Findings

(1) Protection resulted in a relatively small amount of avoided deforestation (about 10% or less of the forest protected or between 46,929 ha and 106,889 ha);

(2) Protection resulted in reforestation of about 20% (between 10,388 ha and 15,124 ha) of the non-forest areas that were protected;

(3) Protection had little effect on land use outside protected areas, most likely because, as noted above, protected areas had only small effects on reducing deforestation or increasing reforestation.

(4) There is little evidence that protected areas had harmful impacts on the livelihoods of local communities – on the contrary, I find that protection had small positive effects on socioeconomic outcomes.

(5) The evaluation methods traditionally used for evaluating protected areas are biased. In contrast to the results listed above, those conventional methods overestimated the amount of avoided deforestation from protection by a factor of three or more, and those methods erroneously implied that protection had negative impacts on the livelihoods of local communities.

Policy Implications

(1) Although global expenditures on protected areas are about $6.5 billion, little is known about the returns on these investments. This study indicates that protected area effectiveness can be substantially weakened by targeting of protection towards lands that are not threatened with conversion in the absence of protection.

(2) The results have significant implications for climate change policy debates on how (and if) developing countries should be allowed to generate greenhouse gas emissions credits for avoided deforestation. Measuring avoided deforestation correctly is a key component in setting appropriate baselines for such an emissions credit program. Avoided deforestation is not directly observable and thus the act of protection generates the credits. In such a system, there is a strong potential for actors to claim avoided deforestation where there is none.

(3) Policymakers should give careful consideration to current proposals to compensate communities living in or around protected areas. Such proposals assume that

protected areas are harmful for those communities, but my results suggest that protection may not have harmful effects on socioeconomic outcomes.

CHAPTER 1: MEASURING AVOIDED DEFORESTATION FROM PROTECTED AREAS

Abstract

Protected areas are the most widely used strategy for reducing deforestation. However, previous efforts to estimate the effectiveness of protected areas have failed to achieve a basic precept of program evaluation: establishing the counterfactual. To know the amount of deforestation that has been avoided, one must estimate the amount of deforestation that would have occurred in the absence of protection. I demonstrate how matching estimators can be used to estimate how much deforestation would have occurred in and around protected areas.

I apply these methods to estimate avoided deforestation from Costa Rica's world renowned protected area system between 1960 and 1997. Protection resulted in a relatively small amount of avoided deforestation (about 10% or less of the forest protected). Furthermore, the methods traditionally used in conservation science 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.

Introduction

In the last decade, the need to subject programs designed to protect biodiversity to more rigorous assessments has become increasingly clear (see Ferraro and Pattanayak 2006, and references therein). The *Policy Responses* volume of the Millennium Ecosystem Assessment (2005) describes the immature state of knowledge about program effectiveness with its statement that: “Few well-designed empirical analyses assess even the most common biodiversity conservation measures.”

Protected area evaluations must: (1) control bias generated from the nonrandom nature of policy or program implementation; (2) detect and control for effects of protected areas on unprotected lands (spatial spillovers); and (3) assess the sensitivity of the results to hidden bias. These characteristics are generally absent in the conservation science literature, leading to inconclusive findings about program effectiveness (Stern et al., 2001; Vanclay, 2001). In fact, most studies do not even include just two of the three characteristics.

I implement an analysis that includes all three components in the context of the most popular policy for protecting biodiversity and ecosystem service flows: protected areas (Millennium Ecosystem Assessment, 2005). Such 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 high-income to low-income nations. The most common policy

for reducing deforestation is the establishment of protected areas and other land use restrictions.

However, setting appropriate baselines for an emission credit program is complicated because "avoided deforestation" is a counterfactual event that cannot be observed. Analysts must construct the counterfactual – the deforestation that would have occurred if an area of forest were not protected – from observations or theory.

I demonstrate how to construct a counterfactual and apply these methods to estimate avoided deforestation in Costa Rica between 1960 and 1997 as a result of establishing protected areas. Costa Rica has one of the most widely lauded protected areas systems (Pfaff & Sanchez, 2004; Sanchez-Azofeifa, Daily, Pfaff, & Busch, 2003) 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. I answer the question, "How much more forest would have been cleared in the absence of the protected areas?"

I find that traditional methods used in the conservation science literature overestimate protection's effectiveness by a factor of three or more: only 10% or less of the forest area protected between 1960 and 1997 can be classified as avoided deforestation. The results are robust to alternative specifications and measures, as well as to unobservable confounders that affect both protection and deforestation.

In the next section, I review the relevant literature and explain in more detail the major methodological issues in the evaluation of protected areas. Then, I describe the methods, data, and results before concluding.

Previous Research

Determinants of Protected Area Location

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. 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 & Sussman, 1990; Hunter & Yonzon, 1993; Pauchard & Villarroel, 2002; Pressey, 1995). Similarly in Costa Rica, empirical studies have found that protected areas are located largely in areas unsuitable for agriculture (Cornell, 2000; Helmer, 2000; Powell, Barborak, & Mario Rodriguez, 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, Redford, & Sanderson, 1998; Evans, 1999).

Estimated Effects of Protected Areas on Deforestation

In a review of 49 protected area assessments, Naughton-Treves et al. (2005) find that 13 examine deforestation only in the protected areas. Of the 36 that compare deforestation inside and outside protected areas, 32 find lower deforestation rates inside protected areas. For example, Bruner et al. (2001) find that expert-reported land clearing rates were lower inside protected areas than within a 10-km surrounding belt. Such assessments would be valid if protection was randomly assigned across the landscape and

spatial spillovers were absent. But protection is definitively a non-random process. The few assessments that formally control for other covariates known to affect deforestation either use a small set of covariates (Cornell, 2000; Mas, 2005), which can exacerbate the bias in avoided deforestation estimates (Heckman, Ichimura, & Todd, 1997), or a highly parametric, regression-based approach (Chomitz & Gray, 1996; Cornell, 2000; Cropper, Puri, & Griffiths, 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).

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) 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, Southworth, & Tucker, 2002).

Two types of spatial dependence can occur in land cover change: (1) land use in one area affects the likelihood of land cover change in neighboring areas (spatial lag); and (2) Spatially correlated unobservable characteristics that influence land use (spatial error).

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 (Armsworth, 2006; Berck & Bentley, 1997; Murray, McCarl, & Lee, 2002; Quigley & Swoboda, 2004; Wu, 2000). 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 (Langholz, Lassoie, & Schelhas, 2000) or the failure to develop local market infrastructure, slowing the exploitation of forested lands in the surrounding areas. Note that I 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 can bias estimates of avoided deforestation in two ways. First, using the surrounding unprotected lands as controls could bias estimates of the effect of protection (Stern et al., 2001; Vanclay, 2001), unless spillover effects are stripped from the estimated counterfactuals. Second, the evaluation must incorporate the effect of protection on land use outcomes outside protected areas in the estimate of the net effect of protection.

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 protected lands. 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.

Methods

Matching Methods

In evaluation, we want to estimate the Average Treatment Effect on the Treated (ATT), which is the about of avoided deforestation from protected areas. If protection were allocated randomly across land units, then one could do what most studies have done: simply compare deforestation in protected and unprotected lands, because the expected forest cover change in the absence of protection is identical for protected and unprotected lands. However, because decisions to protect land are determined by observable characteristics, protected and unprotected lands differ in characteristics that may also affect forest cover change after protection.

Matching methods 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 (Imbens, 2004; P. R. Rosenbaum & Rubin, 1983; Rubin, 1980). 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 the analysis includes all of the determinants that jointly affect both selection into protection and deforestation. 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.[1]

Based on recent studies, I estimate the ATT using three matching estimators (Abadie & Imbens, 2006a; Frolich, 2004): (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.[2]

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[1] Mathematically, the assumption implies $E[Y(0) \mid X, T=1] = E[Y(0) \mid X, T=0] = E[Y(0) \mid X]$ and $E[Y(1) \mid X, T=1] = E[Y(1) \mid X, T=0] = E[Y(1) \mid X]$, where $Y_i(1)$ is the deforestation when land plot *i* is protected (Y = 1 if plot is deforested), $Y_i(0)$ is the deforestation when land plot *i* is unprotected, T is treatment (T=1 if protected), and X is the set of pretreatment characteristics on which units are matched. For identification purposes, I also need one other assumption: $c < P(T=1 \mid X=x) < 1-c$ for $c > 0$. If all land units with a given vector of covariates were protected, there would be no observations on similar unprotected land units.

[2] With the exception of the kernel matching which was done in Stata v.9 (Leuven & Sianesi, 2003), matching was done in R (Sekhon, 2007). I also used a nearest-neighbor propensity score matching estimator, but given the results from this estimators were similar to those presented in Table 2, I do not present these results.

The nearest-neighbor matching is with replacement and I resolve the mean-variance tradeoff in the match quality by using two nearest neighbors; the counterfactual outcome is the average among these two.[3] Based on recent work that demonstrates that bootstrapping standard errors is invalid with non-smooth, nearest-neighbor estimators (Abadie & Imbens, 2006b), I use Abadie and Imbens' variance formula (2006a). For the kernel matching estimator, I use a bandwidth of 0.06 and I bootstrap the standard errors (999 replications).

In the covariate matching estimators, I 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, I implement caliper matching in the context of the bias-adjusted, nearest-neighbor Mahalanobis matching estimator (Smith & Todd, 2001). The calipers are defined as 0.5 standard deviations of each matching covariate. For the propensity score estimator, I enforce a common support restriction. I conduct balancing tests for all the matching estimators. The balancing tests compare the means of the matching covariates for matched and control groups using a t-test.

Testing for Sensitivity to Hidden Bias

Although I take great care to ensure that the conditional independence assumption is satisfied, non-experimental analyses are always susceptible to hidden biases. To

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

determine how strongly an unmeasured confounding variable must affect selection into the treatment to undermine the conclusions, I use the bounds recommended by Rosenbaum (2002) (see also Diprete and Gangl (2004)). Although there are other sensitivity tests available (Ichino, Mealli, & Nannicini, 2006), Rosenbaum's bounds are relatively free of parametric assumptions and provide a single, easily interpretable measure of the way in which unobservable covariates could affect the analysis.

If the probability of unit *j* being selected 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 implies 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. I 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. This statistic was 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.

Study Site and Data

Study Site

Costa Rica has a current 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 established more than 150 protected areas.

Data

I test the effect of protection on the amount of deforestation that occurred between 1960 and 1997. In the treatment group, I include national parks, biological reserves, forest reserves, protected zones, and wildlife reserves. I exclude lands controlled by indigenous people, because they are subject to a different legal and land use regime. For similar reasons, I exclude a small number of government designated wetlands.

Protected areas are established over time and thus in the matching process, I want to ensure that the time-varying covariate data (see Table 1.1 below) are reasonable approximations to the time period in which a protected area was established.[4] Thus I break the analysis up into two cohorts: protected areas established before 1980 and between 1985 and 1997. I do not believe the data permit an estimation of the effect of protection between 1980 and 1984 without further assumptions, but I include these years in robustness checks of the results.

I restrict the first treatment cohort to the 42 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

[4] For example, for the 1960 to 1980 treatment, I 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.

established in the late 1960s and the 1970s, but few in the early 1980s. In the Results section, I 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.

Forest cover across the country is measured from a combination of aerial photographs acquired between 1955 and 1960 (called the 1960 dataset) and from 1997 Landsat Thematic Mapper satellite images (Landsat data also exist for 1986 and 2000). 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 soil, climate, and 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).

I draw a random sample of 20,000 land plots that were forested in 1960. Each plot has an area of 3 hectares. This unit is the minimum mappable unit, or pixel, and thus the 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 plot per 1.5 sq km of forest cover[5]. In addition to units from indigenous reserves and wetlands, I 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[6]. Nine protected areas established before 1980 are not represented in the 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.[7]

In the matching analysis, I am interested in controlling for factors that jointly affect land use and the likelihood that a plot is selected for protection. Based on anecdotes 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)), I select variables that capture accessibility of the plot (distance to forest edges, distance to roads

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[5] To check the accuracy of the random sampling process, I confirmed that there were no significant differences between the 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).

[6] The following pre-1980 protected areas are represented in the 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.

[7] 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.

and slope) and land use opportunities (a function of the plot's production potential and distance to roads and major markets). See Table 1.1 for summary statistics. The core set of covariates are as follows:

- *Distance to roads*: Roads make forests more accessible to deforestation agents, and ease the transportation of agricultural produce or logs from cleared land (Helmer, 2000; Sader & Joyce, 1988; Veldkamp, Weitz, Staritsky, & Huising, 1992). I 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 (Chaves-Esquivel & Rosero-Bixby, 2001; Rosero-Bixby & Palloni, 1998). I 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*: Mild slopes, fertile soils, and humid life zones make land more productive for agriculture and therefore make deforestation more likely (Chaves-Esquivel & Rosero-Bixby, 2001; Rosero-Bixby & Palloni, 1998; Sader & Joyce, 1988; Sanchez-Azofeifa & Harriss, 2001; Veldkamp et al., 1992). I 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*: Proximity to agricultural markets is a key explanatory variable in deforestation (Barbier & Burgess, 2001; Kaimowitz &

Angelsen, 1998). Therefore, following Pfaff and Sanchez (2004), I 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, the core set of covariates are consistently found to affect deforestation. The causal effects of other covariates like population density and other socioeconomic characteristics (e.g., poverty, education) are less agreed upon. Nevertheless, I define an extended set of covariates that includes the core set plus the following:

- *Distance to railroads and river transportation network*. In addition to the measure of distance to roads, I 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). Railways and rivers may have affected accessibility of forests for deforestation and the ease of transportation of forest products.
- *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 (Chaves-Esquivel & Rosero-Bixby, 2001; Rosero-Bixby & Palloni, 1998). As with all of the measures below, I measure population density at district-level (*distrito*)[8] from the 1973 census (a mid-point in the main period of protection activity).

[8] 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. I use information collected by the FAO on district splits over time (Cavatassi, Davis, & Lipper, 2004) to re-

- *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.
- *District-level proportion of adults educated beyond the secondary level*: Education increases residents' opportunities for off-farm employment, which can reduce deforestation pressure (Mulley & Unruh, 2004).
- *District-level proportion of households using fuel-wood for cooking*: Fuel-wood use is a proxy for the use of forest resources by district residents, which would affect deforestation.
- *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 that these variables also affect the designation of protected areas, I model the selection process directly using the data. I use a probit model that regresses a binary variable for protection on the core and extended sets of covariates. The most influential variables are land-use capacity classes. Holding other relevant factors constant, less productive plots are more likely to be selected for protection. In addition, less accessible plots (plots farther from forest clearings and roads) are more likely to be

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 I re-aggregate the new district and all parent districts into one unit. The final dataset therefore has 398 "districts".

protected, as are plots in larger districts with lower population densities, a greater proportion of immigrants, and a greater proportion of educated citizens.

Table 1.1. 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	.001	.026	0 – 1

Table 1.1. Descriptive Statistics

Name	Description	Mean	Standard Dev.	Range
	crops			
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 production; permanent or semi-permanent crops such as fruit trees, sugar cane, coffee, ornamental plants, etc.	.125	.330	0 – 1
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

Results

Selection on Observables

I begin by ignoring spatial interactions and addressing only the bias due to selection on observables. 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 1.2 presents the treatment effect estimates using the matching estimators, as well as more traditional estimation methods in the conservation science literature. The results in Table 1.2 are based on the 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 I can choose matches for the protected plots. In Naughton-Treves *et al.*'s (2005) review of 20 published studies that analyze 49 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 1.2 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.

Table 1.2. 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]
DIM: controls within 10km of PA, include plots deforested pre-protection {N treated} [N available controls]	-0.453 {1996} [5480]	-0.380 {290} [3164]	-0.497 {1996} [4956]	-0.475 {1996} [5314]
Baseline Reference Estimate	-0.392	-0.261	-0.392	-0.392
Covariate matching – Inverse variance ‡	-0.045* (0.024)	-0.067 (0.018)	-0.113 (0.031)	-0.110 (0.028)
Covariate Matching – Mahalanobis	-0.049** (0.023)	-0.061 (0.018)	-0.111 (0.029)	-0.115 (0.027)
Covariate Matching – Mahalanobis with calipers ▫ ***[N outside calipers]***	-0.056 (0.016) [237]	-0.061 (0.016) [43]	-0.124 (0.019) [411]	-0.129 (0.018) [320]
Propensity score matching – Kernel [N off common support]	-0.048 (0.009) [0]	-0.075 (0.011) [0]	-0.134 (0.014) [117]	-0.123 (0.012) [74]
N treated (N available controls)	2711 (12572)	557 (4724)	2711 (10371)	2711 (11078)

* Significant at 10%; ** Significant at 5%; All other coefficients significant at 1%

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

‡ Standard errors for matching estimates are in parenthesis under estimate.

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

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, such as Bruner et al. (2001) restrict the control group to a 10-km unprotected zone around each protected area. 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 some analyses of this type do not, as we did, exclude lands deforested before protection. Such "post-protection-only" analyses suffer from even more bias because (1) deforestation may take place before protection is implemented and (2) 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. (2001)). As indicated in the fourth row of Table 1.2, this type of analysis implies that 45% of protected plots would have been deforested had they not been protected.

The fifth 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, I 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 the core covariate set. Because I have no digitized observations of forest cover prior to 1960, I make the assumption that all of the pixels were previously forested at some point in the past. Avoided deforestation is estimated to be 39% of the protected areas protected before 1980.

The sixth through ninth 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 matched 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 I 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 the matches may be imperfect in another way that biases the treatment effects away from zero.

To put Table 1.2's estimates into perspective, consider that 483,000 ha of forest were protected between 1960 and 1980. Thus the first row estimate implies that in 1997, 429,000 ha of this protected forest still had forest cover because of protection. The second, third and fourth row estimates imply 158,000 to 189,000 ha of avoided deforestation. The matching estimators imply only 22,000 to 27,000 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 1.2, I 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 threats.

Given that post-1980 protection led to avoided deforestation, I replicate, in the third column of Table 1.2, 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 I 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 near protected lands (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 44,000 to 65,000 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, I 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, I propose an alternative approach that directly adjusts the sample to incorporate the treatment effects from post-1985 protection[9].

I estimate that post-1985 protection led to avoided deforestation of 6.5% (average of matching estimates in the second column). In the sample, this percentage corresponds

[9] In the 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.

to 36 plots. I thus randomly select thirty-six plots that were protected between 1986 and 1997, and were not deforested within that period, and I change their status from "forest" to "deforested" in 1997. I 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 1.2 and are similar to those in the third column.

I also calculated treatment effects using the extended covariate set. The treatment effects from the matching estimators are similar to results in Table 1.2 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).

Spatial Interactions and Matching Estimators

As noted in an earlier discussion, 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. I therefore use matching estimators to test for spatial spillovers.

To begin, I define the treatment group as unprotected plots that are within two kilometers of the boundary of protected areas created before 1980, and I 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, I attempt to avoid estimation bias due to spillovers from post-1980 protected areas by estimating spatial spillovers from 1960 to 1986 instead of 1960-1997 as was used to estimate the direct effects of protection. For the latter analysis, I am able to identify and exclude control units that could have been affected by post-1980 protection (columns 3 and 4 of Table 1.2). However, for the spillover analysis, I have no way of defining the extent of potential spillovers from post-1980 protection. Therefore, I 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 1.3. In the first column, I 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, I 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. I find no evidence of substantial spillover effects with either traditional methods or the matching methods. For both time periods, I also test for spillovers in subsequent intervals (2-4 km, 4-6 km, 6-8 km) and I do not find treatment effects that are significantly different from zero at the 1% level.

Table 1.3. Spatial Spillover Effect of Protection on Deforestation

Outcome	Deforestation 1986	Deforestation 1997
Treatment group	**Unprotected units within 2-km of pre-1980 Protected Areas**	**Unprotected units within 2-km of 1985-1996 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**
Outcome for treated units only	-0.628	-0.879
Difference in Means[†]	-0.168	-0.026[⊥]
Covariate matching – Inverse variance[‡]	-0.039[⊥] (0.022)	0.001[⊥] (0.023)
Covariate Matching – Mahalanobis	-0.043[⊥] (0.022)	0.001[⊥] (0.022)
Covariate Matching – Mahalanobis with calipers[▫] ***[N outside calipers]***	-0.045** (0.020) [53]	0.0005[⊥] (0.020) [30]
Propensity score matching – Kernel ***[N off common support]***	-0.116 (0.016) [4]	-0.028* (0.017) [0]
N treated (N available controls)	1060 (9849)	430 (4294)

[⊥] Not significant at 10%; * Significant at 10%; ** Significant at 5%; All other coefficients significant at 1%

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

[‡] Standard errors for matching estimates are in parenthesis under estimate.

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

The results suggest that spatial spillovers from protected areas are either absent or positive but small. Given that I estimated small treatment effects of protected areas, the lack of evidence for negative spillover effects from protection is not surprising. The 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 I do not detect substantial spillover effects on deforestation on neighboring unprotected lands arising from the establishment of protected areas between 1960 and 1996, I conclude that the estimates in Table 1.2 reflects the full effect of protected areas both within and outside protected areas. Had I found evidence of such spillovers, I 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 the 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. I 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 the matching methods, I 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 I assume that the treatment effect of protection between 1980 and 1984 lies somewhere between the

estimates for pre-1980 and post-1985 protection, then an estimate of avoided deforestation for 1980-1984 would lie within the range of 14,465 ha to 16,875 ha. Therefore, the best estimate of avoided deforestation between 1960 and 1997 from all protected areas is between 49,186 ha and 111,356 ha.

Sensitivity to Hidden Bias

I follow Rosenbaum (2002) to determine how strongly an unmeasured confounding variable must affect selection into the treatment to undermine the 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 1.4 indicates that the estimated negative treatment effect of protection, 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 I were to exclude from the sample plots protected after 1980, I 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.

I can use the same methods to examine the degree to which unobserved bias causes us to underestimate the effect of protection (in absolute value). I construct 99% confidence intervals for the 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 1.2. The upper bound of the interval is -0.230.

Thus the 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 1.4. 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**
1	<0.001	<0.001	<0.001
1.5	<0.001	<0.001	<0.001
2	<0.001	<0.001	<0.001
2.5	<0.001	<0.001	<0.001
3	<0.001	<0.001	<0.001
3.5	0.075	0.044	<0.001
4	0.844	0.766	<0.001

Other Robustness Checks

I conduct additional robustness checks to examine the sensitivity of the treatment estimates to the composition of the sample, the matching specifications, and I am able to confirm that the estimated treatment effects are robust. I 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 1.2.

Moreover, the matching estimates are always smaller than their corresponding estimates obtained using the traditional estimation methods. Therefore, the robustness checks support the 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:* I estimate treatment effects without excluding indigenous reserves and wetlands from the sample;
- *Excluding protected areas established in 1985 from 1986-1997 analysis:* I 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*[10] *without adjustment:* In the main analysis, I excluded 1980-1984 protected areas because I believed that 1960 forest data were too old for matching these parcels. Here, I include them and estimate the effects of pre-1984 protection on deforestation between 1960 and 1997.
- *Maintaining protected areas established between 1980 and 1984 with adjustment:* I repeat the robustness check above with one modification. I 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, I 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:* I vary the number of nearest neighbors that are matched with treatment plots from 1 to 10.
- *Varying the kernel bandwidth:* I estimate kernel-based propensity score matching with kernel bandwidths 0.01 and 0.11.
- *Matching without bias-correction:* I compare the matching estimates to matching estimates without Abadie and Imbens' (2006a) post-matching, bias correction.

[10] In the sample, 1,545 plots were protected between 1980 and 1984. The following protected areas established between 1980 and 1984 are represented in the 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*: I replace the land-use capacity categories with measures of slope and Holdridge (1967) life-zones, as used in other deforestation studies in Costa Rica.

I 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. I obtain a wider range of avoided deforestation estimates at this coarser scale compared to the results from the pixel-level analysis. Some of the matching estimates suggest that there was no significant avoided deforestation from protection while others detect some avoided deforestation. If I assume that the covariate matching estimator using calipers is the highest quality estimate, then the 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).

Conclusion

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. Protected areas are the most widely used policy tool for biodiversity protection. In addition, protected areas play a key role in current climate change policy debates. Measuring avoided deforestation in the absence of formal protection is difficult because avoided deforestation is a counterfactual event. Moreover, the potential for positive and negative spillovers onto unprotected ecosystems further complicates the evaluation of protected area effectiveness.

I 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 49,000 ha and 111,000 ha. 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).

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.[11]

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 protect lands that were inexpensive to protect 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 (De Camino, O., Arias, & Perez, 2000).

One should also remember that this analysis is retrospective. The future role of Costa Rica's protected areas in affecting land use may be different from the past (but

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[11] 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 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.

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 smaller than previously estimated.

CHAPTER 2: MEASURING REFORESTATION FROM PROTECTED AREAS

Abstract

Protected areas have long been the principal means for achieving biodiversity conservation goals. Although their main aim is to protect existing biodiversity, the restoration of biodiversity through reforestation is also increasingly becoming an important conservation goal. However, very few assessments of protected areas focus on reforestation as a measure of protected area effectiveness. Measuring reforestation (or other outcomes) from protective measures is complicated because reforestation is a counterfactual event. By ignoring the nonrandomized nature of protected area establishment and the spatial spillovers that can result from their establishment, past empirical estimates of reforestation fail to properly estimate the counterfactual vegetation cover.

I demonstrate how matching estimators can be used to estimate reforestation in and around protected areas. I apply the methods to estimate reforestation from Costa Rica's world renowned protected area system between 1960 and 1997. Protection resulted in the reforestation of about 20% of the non-forest areas that were protected. Furthermore, the methods traditionally used in conservation science overestimate the amount of reforestation.

Introduction

The protection of existing biodiversity has long been the primary goal of conservation policies. However, given the significant loss of global biodiversity in recent decades, the restoration of damaged ecosystems is increasingly becoming as important as the protection of existing biodiversity (Young, 2000). Therefore, while the protection of existing forests is the primary goal of protected areas, reforestation of cleared forests is another important measure of the effectiveness of protected areas. Reforestation may lead to the recovery of near-extinct species. Reforestation also increases carbon sequestration and, by so doing, reduces greenhouse gas emissions (Silver, Ostertag, & Lugo, 2000). Restored forests can also reduce pressure on primary forests, which tend to possess higher biodiversity value, by providing alternatives for loggers and other forest users. In this Chapter, I measure the effects of protected areas on reforestation.

Reforestation is rarely used as an indicator of protected area effectiveness. In most assessments of protected areas, deforestation is the outcome measure (Naughton-Treves et al., 2005). However, in some protected area studies a measure of reforestation is implicit, because most studies do not differentiate between gross deforestation, which is the amount of forest that is cleared over time, and net deforestation, which is the amount of forest cleared less reforestation. A few researchers have focused explicitly on reforestation rates as a measure of protected area effectiveness (Helmer, 2000; Triantakonstantis, Kollias, & Kalivas, 2006). Helmer (2000) compares reforestation inside and outside Costa Rica's protected areas, and finds more reforestation in protected areas than on unprotected lands. Helmer uses spatial sampling to minimize spatial

autocorrelation, but does not correct for selection bias or spatial interactions between protected and unprotected areas. Measuring reforestation from protective measures is complicated because reforestation is a counterfactual event. By ignoring the nonrandomized nature of protected area establishment and the spatial spillovers that can result from their establishment, past empirical estimates of reforestation fail to properly estimate the counterfactual vegetation cover.

I apply the methodology developed in Chapter 1 to construct suitable counterfactuals and to estimate reforestation in Costa Rica between 1960 and 1997 as a result of establishing protected areas. Some lands in Costa Rica were deforested *before* they were placed under protected area status. This may have been driven in part by preemptive clearing, where landowners cleared trees on their lands to prevent their land being placed under protection. It is also possible that some significant forest clearing occurred even after legislation had been passed to establish a protected area, if enforcement of the restrictions was delayed. Therefore, the rate of reforestation since protected area establishment provides an additional measure of the effectiveness of Costa Rican protected areas. This analysis, in combination with the estimates of avoided deforestation in Chapter 1, provides a broad picture of the effects of protected areas on land cover change in Costa Rica.

Data

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).

I select a random sample of 20,000 land plots that were not covered by forest in 1960. Each plot has an area of 3 hectares. This unit is the minimum mappable unit, or pixel, and thus the outcome variable is binary: a plot is either not forested or reforested (reforested = 80%+ canopy cover). Thus the dependent variable is a categorical outcome that determines whether a land plot has been reforested by 1997 or not. All aspects of the analysis, including rules for excluding plots, are otherwise the same as those used in the avoided deforestation analysis (Chapter 1). The final dataset comprises 15,913 land plots. By 1986, 3,325 of these plots had been reforested, and by 1997, 3,238 plots were reforested. The total number of plots placed under protection before 1980 is 820, of which 393 were reforested by 1986 and 406 were reforested by 1997. Detailed statistics on the characteristics of the sample are provided in Table 2.1.

The matching covariates are the same as those used described in Chapter 1. The core set of covariates are distance to roads, distance to closest forest edge, land use capacity class, and distance to nearest major city. The extended set of covariates adds the following: distance to railroads and river transportation network, district-level population

density, district-level proportion of immigrants, district-level proportion of adults educated beyond the “secundaria” level, district-level proportion of households using fuel-wood, and area of district. Detailed descriptions of the matching covariates are provided in Chapter 1.

I use the same set of matching covariates as in Chapter 1 because the factors that determine deforestation rates are likely to be the same factors that determine reforestation rates. Indeed, researchers who use net deforestation as a dependent variable make this assumption, because net deforestation includes reforestation. Helmer (2000) found that in Costa Rica, many of the drivers of deforestation, such as soil fertility, distance to roads, slope, and elevation, are also important determinants of reforestation rates.

Table 2.1. Descriptive Statistics

Name	**Description**	**Mean**	**Standard Dev.**	**Range**
Reforestation 1960-1997	Coded 1 if plot was reforested between 1960 and 1997, 0 otherwise	0.210	0.407	0 – 1
Reforestation 1960-1986	Coded 1 if plot was reforested between 1960 and 1986, 0 otherwise	0.216	0.412	0 – 1
Reforestation 1986-1997	Coded 1 if plot was reforested between 1986 and 1997, 0 otherwise (non-forested plots in 1986 only)	0.050	0.218	0 – 1
Protection before 1980	Coded 1 if plot is in a protected area created before 1980, 0 otherwise	0.053	0.224	0 – 1
Protection 1980-1984	Coded 1 if plot is in a protected area created between 1980 and 1984, 0 otherwise	0.002	0.049	0 – 1
Protection 1985-1996	Coded 1 if plot is in a protected area created between 1985 and 1996, 0 otherwise	0.045	0.207	0 – 1
Distance to forest 1960	Distance to closest forest in 1960, measured in km	1.307	1.387	3.5×10^{-5} – 10.235

Table 2.1. Descriptive Statistics

Name	Description	Mean	Standard Dev.	Range
Distance to forest 1986	Distance to closest forest in 1986, measured in km (units under forest in 1986 only)	0.885	1.178	4.3×10^{-5} – 10.235
Distance to road 1969	Distance to nearest road in 1969, measured in km	9.654	10.514	2.6×10^{-4} – 64.536
Distance to railroads and river transportation 1969	Distance to nearest railroad or river transportation in 1969, measured in km	32.641	23.511	3.0×10^{-4} – 103.996
Distance to national road 1991	Distance to nearest national road in 1991, measured in km	2.437	3.409	0.144 – 42.988
Distance to major city	Distance to closest major city (Limon, Puntarenas, or San Jose), measured in km	65.460	39.166	0.439 – 216.993
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	0.008	0.089	0 – 1
Class II	Suitable for agricultural production requiring special land and crop management practices such as water conservation, fertilization, irrigation, etc.	0.152	0.359	0 – 1
Class III	Suitable for agricultural production requiring special land and crop management practices such as water conservation, fertilization, irrigation, etc.	0.160	0.366	0 – 1
Class IV	Moderately suitable for agricultural production; permanent or semi-permanent crops such as fruit trees, sugar cane, coffee, ornamental plants, etc.	0.229	0.420	0 – 1
Class V	Strong limitations for agriculture; forestry or pastureland	0.014	0.118	0 – 1
Class VI	Strong limiting factors on agricultural production; land is only suitable for forest plantations or natural forest management	0.127	0.333	0 – 1
Class VII	Strong limiting factors on agricultural production; land is only suitable for forest plantations or natural forest	0.160	0.367	0 – 1

Table 2.1. Descriptive Statistics

Name	**Description**	**Mean**	**Standard Dev.**	**Range**
	management			
Class VIII	Land is suitable only for watershed protection	0.053	0.223	0 – 1
Class IX	Land is suitable only for protection	0.088	0.283	0 – 1
District area	Area of district in which land plot is located, measured in square km	402.266	445.681	0.548 – 2408.735
Population density	Population density of district in which land plot is located, measured as number of people per square km (1973)	70.635	399.518	0.253 – 11963.43
Percentage of immigrants	Number of people born outside their canton of residence (1973)	0.358	0.224	0.014 – 0.913
Percentage of adults with secondary-level education	Percentage of adults with secundaria or universitaria level education (1973)	0.068	0.057	0.007 - 0.498
Fuel-wood use	Percentage of households using fuel-wood for cooking (1973)	0.745	0.227	0.004 - 1

Analysis and Results

I use matching methods (see detailed explanation of methods in Chapter 1) to find valid counterfactuals for protected units, thereby ensuring that any differences in reforestation rates can be attributed to protection status. I estimate the effect of protection on reforestation inside and outside of protected areas.

Results are presented in Tables 2.2 and 2.3. The treatment effect estimates from matching with the core covariates range from 0.189 to 0.229 for pre-1980 protection (i.e., 19% - 23% of additional reforestation)[12]. This corresponds to between 6,961 ha and 8,592 ha of land reforested between 1960 and 1997 as a result of protection before 1980.

[12] The matching estimates with the extended covariate set range from .179 to .284, and as with the estimates in Table 2, the matching estimates are smaller than the estimates with the traditional empirical methods.

I apply the Rosenbaum (2002) test for sensitivity to hidden bias[13], and find that these estimates for pre-1980 protection are robust to unobserved hidden bias (first two columns in Table 2.4). I also test for the effect of post-1985 protection and obtain matching estimates range from 0.05 to 0.087, or between 3,222 ha and 5,606 ha of additional reforestation. However, when I test for sensitivity to hidden bias I find that these estimates for post-1985 protection are not robust to unobserved hidden bias (Table 2.4, column 3).

If I assume that the treatment effect of protection between 1980 and 1984 lies somewhere between the estimates for pre-1980 and post-1985 protection, then an estimate of reforestation from protection for 1980-1984 would lie within the range of 205 ha to 926 ha. I detected no spatial spillovers of reforestation onto neighboring unprotected lands as a result of protection (Table 2.3).

Therefore, ignoring any potential hidden bias, the best estimate of reforestation between 1960 and 1997 from all protected areas is between 10,388 ha and 15,124 ha. In the presence of hidden bias that might affect the estimates for post-1980 protection, the best estimates would be limited to a range of 6,961 ha to 8,592 ha of reforestation from all protected areas.

[13] See Chapter 1 for a description of the method.

Table 2.2. Effect of Protection on Reforestation

	1	2	3
Treatment group	**Protected pre-1980**	**Protected 1985-1996**[14]	**Protected pre-1980**
Control group	**Unprotected pre-1980**	**Never protected**	**Never protected**
Outcome in protected areas only	0.495	0.125	0.495
Difference in Means†	0.307 (0.018)	0.081 (0.016)	0.319 (0.018)
Covariate matching – Inverse variance	0.189 (0.032)	0.050 (0.016)	0.228 (0.033)
Covariate Matching – Mahalanobis	0.189 (0.032)	0.050 (0.016)	0.216 (0.033)
Covariate Matching – Mahalanobis with calipers [N outside calipers]	0.203 (0.031) [69]	0.050 (0.016) [0]	0.229 (0.030) [93]
Propensity score matching – Kernel [N off common support]	0.206 (0.020) [0]	0.087† (0.074) [368]	0.234 (0.020) [0]
N treated (N available controls)	820 (15093)	425 (11937)	820 (14598)

* Significant at 10%; ** Significant at 5%; All other coefficients significant at 1%

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

‡ Standard errors for matching estimates are in parenthesis under estimate.

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

[14] I obtained this treated group by selecting a random sample of 1000 pixels in protected areas established between 1985 and 1996 (the final size of the group is 425 because I exclude pixels located in indigenous reserves, wetlands, clouds or problems areas, and pixels reforested by 1986).

Table 2.3. Spatial Spillover Effect of Protection on Reforestation

	1	2
Outcome	**Reforestation 1986**	**Reforestation 1997**
Treatment group	**Unprotected units within 2-km of pre-1980 Protected Areas**	**Unprotected units within 2-km of 1985-1996 Protected Areas**[15]
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**
Outcome for treated units only	0.228	0.051
Difference in Means†	0.041 (0.013)	0.008† (0.009)
Covariate matching – Inverse variance	0.016† (0.018)	0.002† (0.009)
Covariate Matching – Mahalanobis	0.013† (0.018)	0.002† (0.009)
Covariate Matching – Mahalanobis with calipers [N outside calipers]	0.009† (0.017) [53]	0.002† (0.009) [2]
Propensity score matching – Kernel [N off common support]	0.016† (0.013) [3]	0.008† (0.009) [0]
N treated (N available controls)	1093 (13858)	704 (11166)

* Significant at 10%; ** Significant at 5%; All other coefficients significant at 1%

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

‡ Standard errors for matching estimates are in parenthesis under estimate.

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

[15] I obtained this treated group by selecting a random sample of 1000 pixels located within 2 km of protected areas established between 1985 and 1996 (the final size of the group is 704 because I exclude pixels located in indigenous reserves, wetlands, clouds or problems areas, and pixels reforested by 1986).

Table 2.4. 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**
1	<0.001	<0.001	0.997
1.5	<0.001	<0.001	1
2	<0.001	<0.001	1
2.5	0.047	0.044	1
3	0.619	0.604	1
3.5	0.975	0.973	1
4	1	1	1

CHAPTER 3: EVALUATING THE EFFECTS OF PROTECTED AREAS ON SOCIOECONOMIC OUTCOMES

Abstract

The potential impact of protected areas on the livelihoods of local communities is currently one of the most important (and controversial) issues among practitioners in the fields of biodiversity conservation and poverty alleviation. Although there is widespread concern about the potentially harmful consequences of establishing protected areas, obtaining empirical measures of the socioeconomic impacts on local communities has proven difficult because of the difficulty in establishing the counterfactual.

I demonstrate how matching methods can be used to establish such a counterfactual, and I use this approach to measure the effects of Costa Rican protected areas established before 1980 on four key socioeconomic indicators in 2000 – employment, access to electricity, access to telephones, and access to computers. I estimate the effects of protected areas on socioeconomic outcomes within protected census segments as well as within unprotected segments that are located close to protected areas. I find no evidence that protected areas had harmful impacts on the livelihoods of local communities. On the contrary, I find that protection had small positive effects on socioeconomic outcomes.

Introduction

One of the most important policy questions in the fields of biodiversity conservation and poverty alleviation is the impact of protected areas on the livelihoods of communities living in or around protected areas. By many measures, protected areas are the most widely used tool for *in situ* conservation of biodiversity (Millennium Ecosystem Assessment, 2005). Most conservationists consider the establishment of protected areas to be a key strategy for global preservation of biodiversity (Brandon et al., 1998; Terborgh & Van Shaik, 1997). However, several researchers have called for a more careful consideration of the socioeconomic costs of establishing protected areas, arguing that protected areas should not be established if they reduce the social welfare of local communities (Cernea, 2006). This view is reflected in the World Parks Congress (2004) declaration, which states, "many costs of protected areas are borne locally – particularly by poor communities" (World Parks Congress, 2004).

Although this issue has important policy implications (e.g., some proposals have been made for compensating local communities after the establishment of protected areas), a key unresolved question is, "do protected areas have negative impacts on the livelihoods of people living in or near protected areas?" This question has not been addressed empirically (Schmidt-Soltau & Brockington, 2004; Wilkie et al., 2006), although there has been some theoretical research on this issue (Robalino, 2007). According to Wilkie *et al.* (2006), two of the complications that make an empirical assessment difficult are the lack of baseline data on economic indicators prior to protected area establishment, and the lack of valid counterfactuals for people affected by

protected area establishment. Margaret Buck Holland (2006, personal communication) and Wilkie *et al.* (2006) are addressing some of these challenges in ongoing research.

I apply the methods described in Chapter 1 to measure the effects of protected areas on social welfare in Costa Rica. I use matching methods to identify suitable counterfactuals for protected census segments in order to control for the overt bias from nonrandom placement of protection. I match segments affected by protection with unprotected segments based on relevant pre-protection variables that affect the likelihood of protection as well as differences in socioeconomic outcomes. I also estimate the spatial spillover effects of nearby protection on unprotected census segments, and I assess the sensitivity of the results to various changes in the sample or matching specification. In the next section, I review the background literature on this issue, after which I describe the data, methods, results, and sensitivity analysis before I conclude.

Background

A key challenge of modern society is to balance social welfare and environmental conservation (Sanderson & Redford 2003; Adams *et al.* 2004). One view of economic development and the environment is based on the Kuznets curve. Simon Kuznets's (1955) original theory posits that as societies develop, inequality will continue to grow to a critical point, after which inequality declines. Early economists cited an "environmental Kuznets curve" to argue that as societies develop, environmental degradation will grow worse up to a critical point, after which it starts to reduce as more resources are devoted to solving environmental problems that arise from economic

growth. Although this view has been criticized for leading to irreversible environmental failures in developing countries (Sanderson & Redford, 2003), the theory holds for removable pollution or other reversible environmental problems.

An alternative view of environment and development, called Sustainable Development, emerged late in the 20th century (Adams, 2001). Sustainable development is the integration of economic development, environmental conservation, and social equity. However, in practice, applying sustainable development principles to protected areas and other conservation programs has not yielded the expected benefits in terms of social welfare and the protection of biodiversity.

The debate over the effects of protected areas on social welfare has its roots in the debate described above (economic development versus environmental conservation). The earliest protected areas focused on restricting all access to forests (Colchester, 2004). However, since the 1980s conservation groups have sought to apply the principles of sustainable development to the management of protected areas, and to reduce the adverse effects of protected areas on local forest users. The most common examples of these efforts are Integrated Conservation and Development Projects (ICDPs), which seek to reduce the strain of protected areas on local forest users. However, like other sustainable development programs, ICDPs seem to have failed to achieve their goals of improving social welfare while protecting the environment (Wunder, 2001).

According to Sanderson and Redford (2003), the difficulties in implementing "sustainable development" have led to a renewed focus among development practitioners, seeking to improve social welfare without sufficient regard for

environmental consequences. Among conservationists, the failure of "sustainable development" has led to calls for a return to protection of biodiversity as a goal in itself (Brandon et al., 1998; MacKinnon, 1997; Terborgh & Van Shaik, 1997; Van Shaik & Kramer, 1997), including the establishment of strict protected areas that restrict access to forests. However, social advocates argue that restricting access to forests reduces the welfare of local people (Cernea, 2006; Colchester, 2004). In general, the current debates lack empirical evidence on the effects of protected areas on social welfare (Schmidt-Soltau & Brockington, 2004; Wilkie et al., 2006).

Data

I obtain data on socioeconomic variables from the Instituto Nacional de Estadistica y Censos (INEC). Geographically referenced data are available at the district level for 1973, 1986, and 2000, and at the census segment level for 2000. The Earth Observation Systems Laboratory of the University of Alberta, Canada, provided the GIS data layers for forest cover, protected areas, and the locations of major cities. 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). 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).

I develop a dataset of the census segments surveyed in 2000[16] by overlaying the GIS data layers for these segments with the GIS data layers for biophysical and infrastructure variables. Although there are 17,261 census segments in the GIS map, the final dataset consists of 17,254 segments, because I exclude 12 segments for which there are no census data[17]. On average, a segment consists of 60 households and 220 people. The census segments have a mean area of 3-km^2, and the area of a segment varies from 0.001-km^2 in urban areas to more than 700-km^2 in less populated rural areas. Descriptive statistics for the segments in the sample are presented in Table 3.1.

I measure socioeconomic outcomes using three variables that have been used in the literature as poverty indicators for Costa Rica (Cavatassi et al., 2004; ITCR, 2004; Rosero-Bixby & Palloni, 1998; World Bank, 1997, 2000), and I include a fourth outcome measuring access to computers. The outcomes are:

- *Employment*: the percentage of employed adults (age 15 and older);
- *Access to electricity:* As a measure of infrastructure service provision and income, I measure the percentage of households with electricity;
- *Access to telephones:* the percentage of households with telephones;
- *Access to computers:* the percentage of households with computers.

[16] Note that 2000 is the only year for which census segment boundaries are available in GIS. I originally intended to use segment-level socioeconomic variables from 1973 as matching covariates. However, there are no 1973 census data at the same geographic scale as the census segments which were surveyed in 2000, because the 1973 segments were split up over time to create the smaller segments in 2000. In addition, GIS maps of census boundaries are only available for 2000. Hard copy maps of earlier census segment boundaries exist, but extensive digitization would be required to create GIS layers for these segments (more than 4000 segments in 1973). I have attempted to digitize the census boundaries from earlier years, but these GIS layers are not yet available due to delays in digitizing.

[17] The excluded segments may not have been surveyed because there are no residents within those segments. Some of the excluded segments represent protected areas or wetlands or are located within protected areas or wetlands.

In testing for the effect of protection on these outcomes, I match segments based on variables that jointly affect the socioeconomic outcomes in the segment and the likelihood that the land within a segment is protected. I seek variables that capture the expected benefits and costs of protecting the land from the perspective of Costa Rican officials (in terms of amount of forest protected, land use opportunities that would be forgone if the land were protected, and accessibility). These variables also affect socioeconomic outcomes because they affect agricultural production, market access, and infrastructure service provision. Based on anecdotes of the history of Costa Rica's protected areas and the literature on variables affecting land use decisions (especially the review of Kaimowitz and Angelsen (1998) I define the following core set of covariates:

- *Segment area:* Smaller segments are more densely populated and thus less likely to be placed under protection.
- *Forest area:* I include a measure of the area of the segment under forest in 1960, which is the earliest measure of forest cover prior to the establishment of protected areas. Forest area is likely to be highly correlated with the likelihood of protected area location. It is also likely to affect socioeconomic outcomes. For instance, segments with more forest cover may offer more opportunities for exploiting forest products.
- *"Road-less volume"*: Road-less volume is a metric developed by Watts et al. (2007) to measure accessibility to transportation infrastructure. Road-less volume provides a better way of capturing this effect than measures such as road density

or the distance from each segment to the nearest road, because such measures only reflect accessibility at the larger segment scale. In contrast, road-less volume measures the accessibility of each plot of land and aggregates this measure to the segment level. Furthermore, road-less volume simultaneously measures the extent to which roads have penetrated a segment as well as the extent to which roads have penetrated adjacent segments. First, I calculate the road-less volume for each square of length 100m across the country (road-less volume = distance from center of the square to nearest road * area of the square). I then add the road-less volumes for all squares within a segment to obtain the total road-less volume for the segment. Road-less volume may have opposing effects on the likelihood of protection. On the one hand, remote lands may be considered less threatened by deforestation and therefore may be more likely candidates for protection. Thus, segments with larger road-less volumes may be more likely to be protected. On the other hand, protected areas that are created for ecotourism may be located near roads to make those parks more accessible, implying that segments with smaller road-less volumes would be protected. Road-less volume also affects socioeconomic outcomes by affecting access to forest, agricultural lands, and markets.

- *Land use capacity*: To capture the land use opportunities in each segment, I 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. I measure the total area under each land use capacity class for each segment.

Productive lands are less likely to be placed under production, and higher agricultural productivity may lead to better social welfare.

- *Distance to nearest major city:* Following Pfaff and Sanchez (2004), I measure the distance from the centroid of the segment to one of three major cities, Limon, Puntarenas, and San Jose. Segments closer to the capital, San Jose, and other major cities may be seen as less remote and therefore less likely to attract protection. On the other hand, protected area restrictions may be easier to enforce in areas closer to major cities, making those areas more likely candidates for protection. The farther a segment is from a major city, the lower the expected socioeconomic outcomes.

In addition to the main analysis, I conduct a sensitivity analysis to determine whether the results change when we define an alternative set of matching covariates. For that analysis, I define an extended set of covariates that adds the following covariates to the core set of matching covariates. The effects of these variables on land use decisions (and therefore protected area placement and social welfare) are less agreed upon in the literature.

- *Distance to railroads and river transportation network.* As an additional measure of remoteness, I measure the distance from the centroid of each segment to a railroad in 1969 or a river that was part of the river transportation network in 1969.

- *District-level[18] 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 (Chaves-Esquivel & Rosero-Bixby, 2001; Rosero-Bixby & Palloni, 1998). Therefore, I include this variable as a measure of the likelihood of protection. As with all of the measures below, I measure population density at district-level (*distrito*)[19] from the 1973 census (a mid-point in the main period of protection activity).
- *District-level proportion of immigrants*: Harrison (1991) and Rosero-Bixby and Palloni (1998) find correlations between the percentage of immigrants and land use.
- *District-level proportion of adults educated beyond the secondary level*: Education increases residents' opportunities for off-farm employment, which can reduce deforestation pressure (Mulley & Unruh, 2004).
- *District-level proportion of households using fuel-wood for cooking*: Fuel-wood use is a proxy for the use of forest resources by district residents, which would affect deforestation.

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[18] As noted in a previous footnote, I originally intended to use segment-level socioeconomic variables from 1973 as matching covariates. However, there are no 1973 census data at the same geographic scale as the census segments which were surveyed in 2000, because the 1973 segments were split up over time to create the smaller segments in 2000. In addition, GIS maps of census boundaries are only available for 2000. Hard copy maps of earlier census segment boundaries exist, but extensive digitization would be required to create GIS layers for these segments (more than 4000 segments in 1973).

[19] 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".

- *Size (area) of district*: District area is negatively correlated with administrative capacity and economic growth, which might influence deforestation and protected area placement.

I test the effects of these variables on the likelihood of protection by modeling the selection decision using a probit regression of the binary treatment variable[20] on the set of covariates. When I exclude segment area from the model, area of forest has the largest effect on the likelihood of protection. Segments with more forest area in 1960 are significantly more likely to be protected, holding other factors (except segment area) constant. When I control for segment area, the coefficient on forest area becomes much smaller and less significant and the sign changes to negative. This implies that part of the effect of forest area on the likelihood of protection is driven by the size of the segment itself. Also, segments with less productive lands, segments that are farther from major cities, and segments with larger areas, are all more likely to be protected. On the other hand, all else being equal, segments with larger road-less volume are less likely to be protected. However, when I exclude area of segment and area of forest from the selection equation, segments with larger road-less volume are more likely to be protected. These effects of road-less volume on protected area placement imply that (1) large forests in large segments that have not been penetrated by roads are more likely to be protected, but (2) holding the forest and segment areas constant, lands are also more likely to be protected if they are easily accessible (to tourists, for example). When I test the selection

[20] I obtain a binary treatment variable as follows: Treatment=1 if more than 20 percent of the protected area is protected and Treatment=0 otherwise. Further details are provided in the Methods and Results sections.

model on the extended set of covariates, I find that segments that are farther from railways and rivers are more likely to be protected, as are segments in districts with lower population densities and a larger proportion of households using fuel-wood.

Table 3.1. Descriptive statistics

Variable	Description	Mean	Standard Deviation	Range
Core Matching covariates				
Area	Total land area covered by the segment in km^2	2.947	12.600	0.001 – 736
Forest area	Total forest area in the segment in 1960 in km^2	1.732	11.229	0 – 708.707
Road-less volume (km^3)	Calculated as the product of area and distance to nearest road (1969) for every square of length 100m within the segment, and summed for all squares in the segment	43.117	344.621	0 – 25433.470
Distance to major city	Distance from centroid of the segment to closest major city (Limon, Puntarenas, or San Jose), measured in km	37.045	37.757	0.041 – 206.950
Land use capacity classes I, II, and III	Area under the land classes I, II, and III, measured in km^2 Class I: Agricultural Production – annual crops; Class II: Suitable for agricultural production requiring special land and crop management practices such as water conservation, fertilization, irrigation, etc.; Class III: Suitable for agricultural production requiring special land and crop management practices such as water conservation, fertilization, irrigation, etc.	0.623	2.250	0 – 49.300
Land use capacity class IV	Area under the land class IV, measured in km^2 Class IV: Moderately suitable for	0.503	2.401	0 – 108.000

Table 3.1. Descriptive statistics

Variable	Description	Mean	Standard Deviation	Range
	agricultural production; permanent or semi-permanent crops such as fruit trees, sugar cane, coffee, ornamental plants, etc.			
Land use capacity classes V, VI, and VII	Area under the land classes V, VI, and VII, measured in km^2 Class V: Strong limitations for agriculture; forestry or pastureland Class VI: Strong limiting factors on agricultural production; land is only suitable for forest plantations or natural forest management Class VII: Strong limiting factors on agricultural production; land is only suitable for forest plantations or natural forest management	0.950	4.147	0 – 124.000
Land use capacity classes VIII and IX (reference group)	Area under the land classes VIII and IX, measured in km^2 Class VIII: Land is suitable only for watershed protection Class IX: Land is suitable only for protection	0.845	10.500	0 – 734.000
Other matching covariates				
Distance to forest	Distance from the centroid of the segment to the closest forest in 1960, measured in km	2.198	2.048	0 – 9.045
Distance to railroads and river transportation	Distance from the centroid of the segment to the nearest railroad or river transportation in 1969 in km	37.352	18.723	0.001 – 104.070
District-level fuel-wood use (1973)	Proportion of households using fuel-wood for cooking in 1973 in the district within which the segment is located	0.470	0.317	0.001 – 1
District-level immigrants (1973)	Proportion of residents born outside their canton of residence in 1973 in the district within which the segment is located	0.377	0.201	0.014 – 0.913
District-level education (1973)	Proportion of adults with secundaria or universitaria level education in 1973 in the district within which the	0.143	0.111	0.007 – 0.619

Table 3.1. Descriptive statistics

Variable	Description	Mean	Standard Deviation	Range
	segment is located			
District area (1973)	Area of the district (1973) within which the segment is located, measured in km^2	246.162	454.936	0.548 – 2408.735
District-level population density (1973)	Population density in 1973 of the district within which the segment is located, measured in number of people per km^2	1190.537	2314.777	0.253 – 11963.430
		Socioeconomic outcomes		
Employment	Percentage of employed residents between the ages of 15 and 70 years	0.499	0.097	0 – 1
Access to electricity	Percentage of households in the segment with electricity	0.946	0.166	0 – 1
Access to telephones	Percentage of households in the segment with telephones	0.497	0.325	0 –1
Access to computers	Percentage of households in the segment with computers	0.129	0.160	0 – 1
		Protection		
Proportion of segment protected before 1980	Proportion of the segment area that was protected before 1980	0.015	0.100	0 – 1
Proportion of segment protected after 1980	Proportion of the segment area that was protected after 1980	0.010	0.082	0 – 1
Proportion of buffer protected before 1980	Proportion of the land within 10-km of the segment protected before 1980	0.065	0.084	0 – 0.937
Proportion of buffer protected after1980	Proportion of the land within 10-km of the segment protected after 1980	0.017	0.054	0 – 0.854

Methods

I test for the effects of protection before 1980 within a segment on the socioeconomic outcomes in 2000 for individuals and households within that segment. I define a binary treatment variable by categorizing segments as “protected” if at least 20 percent of the area in the segment was protected before 1980. I chose this somewhat conservative threshold of 20 percent to ensure that the proportion of the segment that is protected is sizeable enough for any effects of protection to be detected in the analysis, while at the same time, ensuring that the treatment group is a representative sample of all segments that received some protection. With this threshold, the treatment group includes 404 segments, which represents more than 65 percent of all segments with any protection[21]. I also exclude 423 segments that received protection after 1980. To further reduce bias from using unsuitable counterfactuals, I exclude all segments with any protection at all before 1980 from the set of controls (that is, I exclude controls with more than 0 percent but less than 20 percent of their land area protected from the analysis). I use the core set of covariates for the analysis. A summary of the analysis using the extended set of covariates is presented in the Sensitivity Analysis section.

I use matching methods (see detailed explanation of matching methods in Chapter 1) to find valid counterfactuals for the protected segments. I estimate treatment effects using four matching estimators: (1) nearest-neighbor covariate matching estimator with Mahalanobis weighting and calipers of 1 standard deviation for each covariate; (2) A

[21] I test the sensitivity of this restriction by using other thresholds of the proportion of segment protected to define the treatment group. The results are presented in the Sensitivity Analysis section.

covariate matching estimator with weights generated from a search using a genetic matching algorithm (Sekhon, 2007); (3) nearest-neighbor covariate matching estimator with inverse variance weighting and exact matching on a dummy variable coded 1 if the segment had forest cover in 1960 and 0 otherwise; and (4) kernel (Gaussian) propensity score matching estimator with common support enforced. Since the socioeconomic dependent variables are proportions, the homoskedasticity assumption may be violated, and therefore I estimate robust standard errors for the covariate matching estimators. For the propensity score matching estimator, I bootstrap the standard errors with 999 replications.

I also tried covariate matching estimators without the caliper restriction (inverse variance weighting and Mahalanobis weighting). However, when I conduct balancing tests for the matching estimators (comparing means of matching covariates for matched and control groups using a t-test), I find that there is severe imbalance on key matching covariates for these covariate matching estimators without calipers. For example, after matching, the mean forest area in 1960 for the protected segments is about 10-km^2 more than the mean forest area in 1960 for their matched unprotected segments (about 100 percent more forest cover in the protected segments) when I use covariate matching without calipers. With calipers, the difference is only 1-km^2. Therefore, I estimate the treatment effects with the covariate matching with calipers, thus ensuring that the protected segments and their matched controls are reasonably similar in terms of the matching covariates.

Results

The treatment effect estimates are presented in Table 3.2. A positive treatment effect indicates that protection improves the socioeconomic outcome, while a negative treatment effect indicates that protection makes the socioeconomic outcome worse.

Before using matching methods, I estimate the effects of protection using two traditional estimation methods – tests for differences in means and Ordinary Least Squares (OLS) regressions – so that I can compare these estimates with the matching estimates. In the first row of Table 3.2 I present estimates from t-tests of the difference in means between the treated and control groups. With the exception of unemployment, these naïve estimates all indicate that socioeconomic outcomes are worse in the treatment group compared with the controls. The second row of estimates in Table 3.2 is from OLS regressions of the outcomes on the treatment dummy variable and the matching covariates. These OLS estimates also indicate that, holding other factors constant, protection has negative impacts on all the socioeconomic outcomes, except employment.

The matching estimates, however, imply radically different effects of protection on the socioeconomic outcomes:

Employment: The matching estimates imply that protection increases employment by 1 or 2 percentage points, but none of these matching estimates is significantly different from zero. Thus, I conclude that there is no significant difference between protected and unprotected segments in terms of employment.

Electricity: The covariate matching estimates indicate that protection improves access to electricity by 5 or 6 percentage points. These estimates imply that about 768 to 921

households[22] had access to electricity because they were located in protected segments. The kernel matching estimate indicates that protection reduces access to electricity by about 14 percentage points. However, the balancing test results indicate that there is severe imbalance between the protected and unprotected segments when I use this propensity score matching estimator, and the imbalance is generally in directions that would make it appear that protection has a negative effect on the socioeconomic outcomes. For example, with kernel matching, the matched protected segments have more than 7 times more forest cover in 1960 and more than 6 times more road-less volume than their unprotected matches. In contrast, for the covariate matching using the genetic algorithm, the protected segments have less than 1.5 times more forest area and road-less volume than their unprotected matches. Therefore, based on the covariate matching estimates I maintain the conclusion that protection improved access to electricity by 5 or 6 percentage points.

Telephones: The covariate matching estimates are all positive but insignificant, indicating that protection has little effect on access to telephones, or at most, a small positive effect. Again, the kernel estimate, which has very poor balance on the matching covariates, indicates that protection has a negative effect of about 26 percentage points.

Computers: I find little effect of protection on this outcome. None of the covariate matching estimates is greater in magnitude than 1 percentage point or significantly different from zero. Here too, the kernel matching estimate indicates a negative effect of

[22] I obtain this estimate by multiplying the total number of households in the matched protected segments by the treatment effect estimate.

about 9 percentage points, but as noted above, this estimator has very poor balance on the key covariates.

Thus, the matching results indicate that, as a result of protected areas established before 1980, access to electricity in protected census segments was higher than in unprotected segments by 5 or 6 percentage points (between 768 and 921 households) by the year 2000, and I conclude that this protection had little effect or small positive effects on employment, access to telephones and access to computers in 2000.

Table 3.2. Estimates of the effect of protection on socioeconomic outcomes.

	1	**3**	**4**	**5**
Outcome (proportions)	**Employed adults**	**Households with electricity**	**Households with telephone**	**Households with computer**
Difference in Means†	0.012 (<0.001)	-0.168 (<0.001)	-0.290 (<0.001)	-0.099 (<0.001)
OLS	0.019 (<0.001)	-0.059 (<0.001)	-0.124 (<0.001)	-0.053 (<0.001)
Covariate Matching – Mahalanobis with calipers [N outside calipers]	0.015 (0.045) [119]	0.054 (<0.001) [120]	0.017 (0.185) [120]	0.001 (0.866) [120]
Covariate Matching – Genetic algorithm	0.011 (0.367)	0.058 (0.009)	0.004 (0.835)	-0.007 (0.311)
Covariate matching – exact matching on presence of forest in 1960	0.018 (0.072)	0.064 (0.001)	0.006 (0.699)	-0.004 (0.449)
Propensity score matching – Kernel [N off common support]	0.011 (>0.200) [9]	-0.141 (<0.001) [9]	-0.259 (<0.001) [9]	-0.090 (<0.001) [9]
N treated (N available controls)	403 (16534)	404 (16539)	404 (16539)	404 (16539)

† A t-test is used to evaluate the difference in means between treated and control segments.

‡ p-values for matching estimates are in parenthesis under estimate.

▫ Calipers restrict matches to units within 1 standard deviation of each covariate.

Sensitivity Analysis

Spillover Effects of Protection onto Neighboring Unprotected Segments

I estimate the spillover effects of protection on socioeconomic outcomes within neighboring unprotected segments. In this analysis, I define the treatment group as segments with more than 20 percent of their 10-km buffer protected before 1980. I take a number of precautions to ensure that I reduce potential bias in the estimation of local spillover effects. First, I exclude all segments that have received protection before or after 1980 (1119 segments). Second, to reduce the potential bias due to the impact of spillovers among the controls, I exclude segments whose buffers received more than 10% protection before 1980[23]. Third, to reduce the potential bias from spillover effects of protection after 1980, I exclude 490 segments whose 10-km buffers received more than 10% protection after 1980.

There are 12,332 segments which were not covered by any forest in 1960. However, if a forest is located close to these non-forest segments, this factor may determine whether a protected area is located near to these non-forest segments. In other words, even though the segment itself has zero forest area, its proximity to a forest may affect the likelihood of being in the treatment group for this analysis. Therefore, I include the *distance to forest 1960* to the set of matching covariates. This covariate is measured as the distance from the centroid of each segment to the nearest forest in 1960. To confirm that this covariate is indeed relevant, I estimate a Probit selection model for

[23] Unlike the first analysis, I do not exclude segments below the 10 percent threshold because this restriction would exclude more than 80 percent of the potential controls (12,375 segments).

this treatment. Segments that are closer to forests are more likely to be included in the treatment group. All the other variables are significant, except area of segment, which I therefore exclude from the set of matching covariates for this analysis.

The estimates of spillover effects of protected areas on socioeconomic outcomes in neighboring unprotected segments are presented in Table 3.3. All the matching estimates are negative, except two estimates for computer access, which are both smaller in magnitude than 1 percentage point and not significantly different from zero. The results imply that protection has small positive effects on socioeconomic outcomes in these neighboring unprotected segments. Therefore, any potential bias in the matching estimates in Table 3.2 due to the spillover effects I detect in Table 3.3 would not overturn the qualitative conclusions. If I corrected for these spillover effects, I would still find that protection has zero or beneficial effects on the socioeconomic outcomes within the protected segments.

To test this conclusion, I repeat the analysis in Table 3.2, but exclude all control segments whose buffers held more than 10 percent protection before 1980 (819 segments). I find that the conclusions, based on the results in Table 3.2, are robust to this additional restriction on the selection of control segments. The results, presented in Table 3.4, show that the matching estimates are similar to the estimates in Table 3.2.

Table 3.3. Estimates of the spillover effect of protection on socioeconomic outcomes. Treatment: at least 20 percent of 10-km buffer of segment protected before 1980

	1	**3**	**4**	**5**
Outcome (proportion)	**Unemployed adults**	**Households lacking electricity**	**Households lacking telephone**	**Households lacking computer**
Difference in Means †	0.029 (<0.001)	0.019 (0.001)	0.003 (0.800)	-0.011 (0.076)
OLS	0.027 (<0.001)	0.008 (0.063)	0.024 (0.006)	0.010 (0.047)
Covariate Matching – Mahalanobis with calipers [N outside calipers]	0.011 (0.017) [13]	0.003 (0.367) [13]	0.030 (0.009) [13]	-0.004 (0.643) [13]
Covariate Matching – Genetic algorithm	0.015 (0.001)	0.007 (0.191)	0.041 (<0.001)	0.023 (0.003)
Propensity score matching – Kernel [N off common support]	0.030 (<0.001) [0]	0.018 (<0.001) [0]	0.017 (<0.100) [0]	-0.002 (>0.100) [0]
N treated (N available controls)	796 (11877)	796 (11885)	796 (11885)	796 (11885)

† A t-test is used to evaluate the difference in means between treated and control segments.
‡ p-values for matching estimates are in parenthesis under estimate.
▫ Calipers restrict matches to units within 1 standard deviation of each covariate.

Table 3.4: Controlling for spillover effects

(Exclude control segments with more than 10% of 10-km buffer protected before 1980)	**1**	**3**	**4**	**5**
Outcome (proportions)	**Employed adults**	**Households with electricity**	**Households with telephone**	**Households with computer**
Difference in Means †	0.022 (<0.001)	-0.162 (<0.001)	-0.268 (<0.001)	-0.097 (<0.001)
OLS	0.024 (<0.001)	-0.062 (<0.001)	-0.130 (<0.001)	-0.059 (<0.001)
Covariate Matching – Mahalanobis with calipers [N outside calipers]	0.016 (0.018) [112]	0.064 (0.001) [113]	0.010 (0.497) [113]	-0.002 (0.695) [113]
Covariate Matching – Genetic algorithm	0.012 (0.336)	0.056 (0.013)	0.009 (0.638)	0.002 (0.810)
Covariate matching – exact matching on presence of forest in 1960	0.018 (0.072)	0.064 (0.001)	0.006 (0.699)	-0.004 (0.449)
Propensity score matching – Kernel [N off common support]	0.022 (<0.001) [9]	-0.139 (<0.001) [9]	-0.237 (<0.001) [9]	-0.086 (<0.001) [9]
N treated (N available controls)	403 (12371)	404 (1)	404 (16539)	404 (16539)

† A t-test is used to evaluate the difference in means between treated and control segments.
‡ p-values for matching estimates are in parenthesis under estimate.
▫ Calipers restrict matches to units within 1 standard deviation of each covariate.

Sensitivity to Selection of Matching Covariates

I repeat the analysis in Table 3.2, using the extended set of covariates described in the Data section, and the qualitative conclusions remain unchanged. Here too, there are some differences between the results with traditional estimation methods and the matching estimates. For example, the OLS estimate for access to electricity is -0.057 and significant at 1 percent, implying that protection causes harmful outcomes in access to electricity. On the other hand, the covariate matching estimates for access to electricity are 0.081 (p=) with Mahalanobis matching with calipers and 0.043 (p=0.088) with genetic algorithm matching. The covariate matching estimates for the other outcomes are either positive and significant or not significantly different from zero at 1%.

Sensitivity to Treatment Threshold Specification

In the main analysis, I consider segments to be "protected" if at least 20 percent of the area in the segment was protected before 1980. With this threshold, the treatment group includes more than 65 percent of all segments with any protection. To test the sensitivity of the results to the level of the threshold for selecting the treatment group, I repeat the analysis with less restrictive thresholds. I define treatment groups comprising segments with at least 10 percent protected (this results in a treatment group made up of about 71 percent of all segments with some protection) and at least 1 percent protected (this results in a treatment group of about 88 percent of all segments with some protection).

The qualitative conclusions do not change when I conduct this analysis. The covariate matching estimates for employment remain are positive, less than 1 percentage point, and not significantly different from zero, the estimates for electricity are positive and larger in magnitude than the estimates in Table 3.2 (the largest estimate indicates that protection increases access to electricity by about 10 percentage points), the estimates for telephone access lie between zero and 4 percentage points, and the estimates for effects on computer access are less than 1 percentage point and no significantly different from zero. I also use alternative thresholds of 15 percent and 10 percent for the spillover analysis.[24] The spillover effect estimates are 0.02 or 0.03 for employment, 0.01 or less for electricity, between 0.06 and 0.09 for telephone access, and 0.02 or less for computer access. None of the spillover matching estimates is negative and significantly different from zero.

Therefore, based on the sensitivity analysis, I maintain the findings from Table 3.2 and conclude that the matching estimates do not indicate any harmful effects of protection on the socioeconomic outcomes. I find that if protection had any effects on socioeconomic outcomes, these effects are small and beneficial.

Conclusion

The question I seek to address in this paper is, "what is the effect of protected areas on social welfare?" The answer to this question has important implications for

[24] I do not use a threshold less than 10 percent for the spillover analysis because this would drastically limit the number of available controls. At 1 percent, nearly 60 percent of the entire sample would be included in the treated group.

conservation policy and the development of poverty alleviation policies and programs. However, an empirical answer to this question has not been forthcoming because of the complications involved in establishing a counterfactual for protected areas. I address this issue by using matching methods to establish counterfactuals for census segments with protected areas, and matching these protected segments with similar unprotected segments based on pre-protection characteristics that affect the likelihood of protection as well as socioeconomic outcomes. I use the methods to measure the effects of protection on socioeconomic outcomes in protected segments. I also measure the spillover effects of protection on socioeconomic outcomes in unprotected segments that are located close to protected areas.

The results indicate that protected areas do not reduce social welfare. On the contrary, I find that protected areas may have small positive effects on some socioeconomic outcomes. For example, I find that protecting at least 20 percent of a census segment before 1980 increased the percentage of households with electricity by about 5 or 6 percentage points or more within the segment by the year 2000. In the sample used for this study, this finding implies that more than 700 additional households had access to electricity because of the establishment of protected areas. I do not find significant effects of protected areas on employment, access to telephones, or access to computers within protected segments. I find that protection has small positive spillover effects on socioeconomic outcomes within unprotected segments located close to protected areas. These beneficial spillover effects imply that the estimates of the direct

effects of protection on protected segments may only be a lower bound of the entire positive effect of protection on protected segments.

However, note that in this study, I measure average effects at the census level, and so I am unable to detect any effects of protection on socioeconomic outcomes at smaller scales (e.g. household level). Protected areas may have some adverse effects on subgroups of the community, and these effects may not be observable at the census tract level. For example, if protected areas cause shifts in economic activities from agriculture to ecotourism, as seems to be the case in Costa Rica, farmers may be adversely affected while the tourism industry experiences growth. Theoretical models indicate that the establishment of protected areas leads to higher land rents and lower agricultural wages, which can lead to changes in income distribution (Robalino, 2007). Distributional consequences such as these are not addressed in an analysis at the census segment level. Furthermore, after protected areas are established, displaced residents and subgroups of the community who are adversely affected may relocate to census segments that are farther away from protected areas. The effects of protection on these people would not be detected in this study.

In spite of these limitations, this analysis of the socioeconomic effects of protected areas is the most rigorous attempt to date – previous assessments of these effects have been based on findings that communities living in or near protected areas tend to be poorer than other communities. However, as I show in this study, once suitable counterfactuals have been identified to compare with communities living in or

near protected areas, the average effect of protected areas on socioeconomic outcomes is zero or slightly positive.

How do protected areas lead to beneficial socioeconomic outcomes? There are a few possible explanations for these findings. First, protection may lead to the growth of an ecotourism industry that creates better economic opportunities for communities living in or near protected areas. Second, since tourism is Costa Rica's main source of foreign exchange, the establishment of a protected area may have led to an increase in government provision of infrastructure services (e.g. electricity, telephones) near the protected area to promote ecotourism. Third, some conservation programs[25] have sought to reduce the deforestation pressure on protected areas by investing in communities living in or near protected areas (e.g. by promoting income-generating activities that do not degrade forests). Although there is little evidence that such projects reduced the pressure on forests, these results suggest that such interventions may have improved the livelihoods of local communities.

[25] For example, a project called the Amistad Conservation and Development Initiative (AMISCONDE), worked with local farmers around protected areas to improve agricultural practices from 1991-1997. This project was implemented by Conservation International and various partners.

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VITA

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