GEF Evaluation Office

# GEF IMPACT EVALUATION

**Applying Quasi-Experimental Methods to Evaluate the Social Impacts of Protected Areas**

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# EXECUTIVE SUMMARY

## BACKGROUND

Protected areas are an important component of the biodiversity portfolio of the Global Environment Facility (GEF). Support for protected areas (e.g., national parks, reserves) represents the largest share of the portfolio. As the financial mechanism for the Convention on Biological Diversity (CBD), the GEF is, in terms of funding and other enabling activities, arguably the most important global catalyst for establishing and managing protected areas. Protected areas remain the most widely used conservation tool in developing countries, with more than $6.5 billion in annual global expenditures (Emerton et al., 2006). However, conservation and development experts are currently engaged in a lively debate regarding the impacts of protected areas on local communities, and policymakers are focused on the social dimensions of protected areas. For example, the CBD's Programme of Work on Protected Areas includes goals to avoid and mitigate negative impacts of protected areas on local communities (CBD, 2002).

At the same time, the GEF is currently engaged in linking its work in biodiversity conservation with human development issues. As noted by the GEF CEO (12 June 2007, Paris[1]), "it is more important than ever for the biodiversity community to elevate its discourse and to reinforce the relevance of biodiversity conservation to sustainable economic development in the 21st Century." In the context of these developments, the GEF Evaluation Office is examining approaches to evaluate the effects on local communities of protected areas supported by the GEF. This report presents the findings from a research project funded by the GEF Evaluation Office to contribute to this effort. The study

[1] From the article '2010 Biodiversity Indicators Partnership Launched in Support for the UN's Convention on Biological Diversity' available at http://www.twentyten.net/news/bip2010Launch.aspx.

applies a quasi-experimental approach to evaluate the socio-economic impacts of Costa Rica's renowned protected area network, in which the GEF has invested for many years.

## ANALYSIS

We use a quasi-experimental approach to provide rigorous estimates of the social impacts of protected areas. We seek to answer the question "what is the effect of this protected area on economic outcomes within neighboring communities?" To tackle this question, one must isolate the effects of other variables on the economic outcomes in local communities affected by protected areas. This in turn requires that we establish the counterfactual: "what would have happened if this protected area had not been established?" Matching methods, the particular quasi-experimental approach that we use in this study, provide one way to find suitable comparisons for communities affected by protection, thus establishing the counterfactual.

We apply the quasi-experimental approach to measure the impacts of Costa Rican protected areas established before 1980 on changes in socioeconomic outcomes between 1973 and 2000. We use matching methods to identify suitable counterfactuals for protected census segments in order to control for the overt bias from nonrandom placement of protection. We match each segment affected by protection with similar unprotected segments based on relevant pre-protection variables[2] that affect the likelihood of protection as well as changes in socioeconomic outcomes. We also estimate the spatial spillover effects of protection on unprotected segments located near protected areas, and we assess the sensitivity of the results to various changes in the sample or matching specification.

[2] We match on the following control variables: segment area, forest area (before any protected areas were established, i.e. 1960), "road-less volume", which is a measure of remoteness, agricultural land use capacity, and distance to nearest major city.

## MAIN FINDINGS

We find no evidence that protected areas in Costa Rica have had harmful impacts on the livelihoods of local communities – on the contrary, we find that protection has had *positive* effects on socioeconomic outcomes. The establishment of protected areas led to a lower poverty index[3] in local communities affected by protection. We find also that protection led to better outcomes in terms of condition of houses, slum conditions, and access to water supply, but we find no significant differences in measures of access to electricity or telephones.

Furthermore, we find that conventional evaluation methods (a difference in means test, or Ordinary Least Squares regression) produced biased estimates when applied to our sample. In contrast to the results above, those conventional methods erroneously implied that protection had negative impacts on the livelihoods of local communities. These findings suggest that conventional methods that fail to control for confounding factors or outcome baselines can lead to inaccurate estimates[4]. Our study demonstrates the key advantages of applying an impact evaluation approach to identify suitable counterfactuals for measuring the social impacts of protected areas.

[3] See main report for a detailed description of the poverty index.

[4] Technically, the Ordinary Least Squares regression controls for confounders and baselines. However, a regression imposes a parametric form on the relationship between protection and outcome, thus leading to bias when the parametric form is incorrect and the covariate distributions for key pre-protection variables are substantially different for protected and unprotected segments (see Technical Appendix).

# INTRODUCTION

Global efforts to reduce tropical deforestation rely heavily on the establishment of protected areas (MA 2005). One of the most controversial debates in conservation policy centers on the effect of these protected areas on local people and economies. This debate is particularly contentious with regard to developing nations where terrestrial protected area networks have rapidly expanded since the 1970s and where alleviating widespread rural poverty is a paramount concern. Moreover, the debate has intensified recently as policymakers seek to design schemes to reduce emissions from deforestation and degradation (REDD) in developing nations.

Although most studies of terrestrial protected areas focus on the environmental impacts of protection, conservation scientists and practitioners now recognize that the socio-economic impacts must also be considered (Balmford et al., 2005; Adams et al. 2004). In 2002, the Convention on Biological Diversity's Programme of Work on Protected Areas adopted a resolution to document the impacts arising from protected areas, particularly for local communities, in order to avoid and mitigate negative impacts (CBD, 2002). The 2003 World Congress on Protected Areas proclaimed that "that protected area management strives to reduce, and in no way exacerbates, poverty" (WPC, 2004).

People who live near terrestrial protected areas may experience positive or negative impacts as a result of protected area management. Protected areas may have negative impacts on local communities by restricting land use, or they may have positive impacts by creating economic opportunities for local communities (e.g. ecotourism). A credible study of the net effects of protected areas on the welfare of neighboring communities would include the following four elements: 1) objectively measurable indicators of human welfare at an appropriate scale of analysis (e.g., households, census tracts, villages, or regions); 2) observations of these indicators before and after the establishment of the protected area, or if no baseline observations are available, some other control for the initial state and trend of the indicators; 3) observations of these indicators from both treated units (i.e., areas known to be potentially affected by protected areas) and control units (i.e., areas similar to treated units in economic potential but known to be not affected, or less affected, by protected areas); and 4) observations of baseline characteristics that affect both where protected areas are located and how the selected indicators of human welfare change over time (e.g., land productivity). The last element refers to confounding factors that can bias the estimate of the protected areas' impacts; for example, if protected areas are located on less productive lands, a simple comparison of growth between communities near and far from protected areas may erroneously suggest protection is detrimental to economic growth when, in fact, growth differences arise from inherent land productivity differences. To date, no study with all of these elements has been published.

Most studies that attempt to estimate the net impact of protected areas (see Ferraro, 2002) focus on a single protected area and are based on attitudinal surveys, case study narratives, ex ante predictions based on historical use patterns and author assumptions, or ex post analyses that often prove little more than rural people near protected areas are poor (Scherl et al., 2004; Agrawal and Redford ,2006; Wilkie et al., 2006). As noted by Coad et al. (2008), these studies do not directly measure the impact of protected areas on poverty, wealth or other variables that might indicate an individual or community's wellbeing, nor do they use data from before and after a protected area has been established or allow for sufficient time after establishment to see an effect. Furthermore, with the exception of two county-level regional analyses in the United States (Duffy-Denno, 1998; Lewis et al., 2002),[5] previous analyses suffer from an inability to identify the effects of protected areas separate from confounding factors that co-vary with protected area establishment. As noted

[5] These two studies find no effect of protected areas on wage or employment indicators, but they also lack some data on pre-establishment conditions.

by Wilkie et al. (2006), "[t]o ascertain with confidence the influence of establishing and managing protected areas on the welfare of local people it is vital that conservation and social scientists conduct rigorous, controlled studies."

We conduct a rigorous, controlled study to estimate the causal impact of national protected area systems in Costa Rica. We combine quantitative indicators of community welfare, pre-park and post-park data, and matching methods that allow us to select control communities that are observationally similar to communities near protected areas. By 2000, more than 1 million hectares of land in Costa Rica had been assigned to legal protection. We address the question, "How different would socio-economic outcomes have been in neighboring communities in the absence of these protected areas?"

## DATA AND METHODS

We focus our analysis on protected forest ecosystems, which make up the vast majority of the protected area systems in Costa Rica. We wish to control for biophysical and socio-economic covariates that affect both changes in social welfare and the location of protected areas. We establish this control through matching methods, which are increasingly used as one way to establish cause-effect relationships using non-experimental data (Imbens, 2004). Matching works by contrasting differences in socio-economic outcomes among communities heavily affected by protected areas (treated) with outcomes among communities that are less affected by protected areas (controls), but which were similar in terms of the observed baseline covariates. The goal of matching is to make the covariate distributions of treated and control observations similar (called covariate balancing), thereby removing observable sources of bias. Thus matching mimics random assignment through the *ex post* construction of a control group (see Technical Appendix for more on matching methods).

We construct a spatial database that overlays indicators of social welfare, boundaries of protected areas, data on forest cover, and measures of intrinsic land productivity and accessibility to roads and cities (see Technical Appendix). The unit of analysis is the census segment (*segmento censal*), which is the smallest spatial unit for which we have comparable socioeconomic data over time from 1973 to 2000 (see Technical Appendix). Our sample comprises 17,071 census segments surveyed in 2000.

## OUTCOMES

Socio-economic outcomes are measured as the change between 1973 and 2000 in the following variables: (1) Poverty index: a multidimensional index of poverty obtained by using principal components analysis (PCA) to determine the best linear combination of a set of variables. We follow Cavatassi et al (2004) in the method and variables used to calculate this poverty index for Costa Rica; (2) Infrastructure services: Proportion of households living in slum areas; (3) Assets: Proportions of households (a) without a telephone; (b) with houses in bad condition; (c) without electricity; (d) without water supply. By contrasting changes in outcome indicators, rather than the post-protection measures only, we also control for unobservable, but temporally invariant, differences in outcomes between treated and control segments. As shown by Smith and Todd (2005), such 'difference-in-difference' estimators are more robust than traditional cross-section matching estimators.

## TREATMENT

The treatment is defined as "more than 20% of segment protected before 1980". We test the sensitivity of this restriction by using other thresholds of the proportion of segment protected to define the treatment group (see Technical Appendix). We focus on protected areas established before 1980 in order to allow 20 years or more for the impacts of the protected area to be experienced by local residents (see Technical Appendix for list of protected areas included in this study). To prevent bias from using controls affected by protection, we trim the sample in two ways: first, we exclude control segments with forest that received protection after 1980 (423 segments), and second, we also exclude control segments that received protection below the 20% threshold level before 1980.

## CONTROL VARIABLES

Based on our knowledge of the history of protected areas and patterns of economic growth in rural Costa Rica, we match treated and control segments based on the following variables: area of the segment under forest in 1960, land use productivity (based on climate, soil and slope), roadless volume in 1969 (a measure of accessibility to, and fragmentation from, transportation infrastructure; Watts et al, 2007), distance from segment's centroid to the nearest major city, and a baseline measure of the relevant outcome indicator (measured in 1973). Detailed descriptions and descriptive statistics of the control variables are provided in the Technical Appendix.

# RESULTS

We present estimates of the effects of protection on socioeconomic outcomes in Table 1. In the first two rows, we present matching estimates, and in the second two rows we present estimates based on more conventional methods as a comparison. Note that all the indicators are 'bads' e.g. percent of houses in bad condition, or percent of households lacking water supply. Thus, a negative sign indicates that protection had a positive effect on the change in the outcome indicator (i.e. protection reduced or alleviated poverty).

The first row of Table 1 presents the impact estimates from the matching approach: it reports the differences in mean change over time (1973-2000) in indicators between treated and matched control segments. Consider the estimated impact of protection on the poverty index in the first column of Table 1. Note that the poverty index in protected segments increased slightly from 8.089 to 8.418 between 1973 and 2000. However, the estimate in Table 1 implies that protection caused the treated segments' poverty index to increase, on average, by 3.251 points less than would have occurred in the absence of protection ($p<0.01$). Therefore, in the matched control segments (segments 'very similar' to the protected segments but which were not protected) the poverty index increased on average by 3.58 points between 1973 and 2000. In other words, protection alleviated poverty, because although poverty increased slightly in the protected segments the situation would have been worse if those segments had not been protected. The second row presents an estimate based on matching that uses calipers to improve covariate balance. Calipers define a tolerance level for judging the quality of the matches: if a treated segment does not have a match within the caliper (i.e., available controls are not good matches), it is eliminated from the sample. Fifteen to eighteen percent of the treated segments are dropped from the sample when we apply the calipers. For the poverty index variable, the estimate with calipers still implies that treated segments would have had smaller change in the poverty index (about 1.941 points lower) in the absence of protection ($p<0.01$).

The findings on other outcomes tell a similar story. In columns 2-6 of Table 1, the estimates imply that protection either reduced poverty in the protected segments (measured in terms of percent of houses in slums or in bad condition and percent of households without water supply) or had no significant effect (when poverty is measured in terms of access to telephone or access to electricity). There is no evidence that protection had negative impacts on the outcomes.

How significant are these impacts of protected areas on the outcomes? The ratio of the effect on the change in poverty index to the standard deviation of the matched control group is 0.319. According to Cohen's (1988) definition of 'effect size', this is a 'small' to 'medium' effect of protection on the change in poverty index (Cohen defines 'small' and 'medium' effects as ratios of 0.2 and 0.5 respectively).

Another way to put these results into perspective is to look at the number of households affected by protection. Consider the matching estimate for the outcome 'percent of households without water supply' (row 1, column 6): this estimate implies that the reduction in the number of households without water supply in the protected segments between 1973 and 2000 would have been lower by 6.429 percentage points in the absence of protection. This in turn implies that the percentage of households without water supply in the protected segments in 2000 would have been 6.429 percentage points higher in the absence of protection. There are, on average, 46.549 households in each of the 399 protected segments. Therefore, the matching estimate implies that 1,088 households in these local communities around protected areas would not have had access to water supply in 2000 in the absence of protection.

In the third and fourth row we present estimates based on more conventional methods in the conservation science literature. The third row reports estimates from an Ordinary Least Squares (OLS) regression of the outcomes on the binary protection variable, controlling for the same set of variables that we used in matching. With the exception of the estimate in column 6, the OLS estimates indicate that protection had harmful effects on the socioeconomic outcomes. For example, the estimate in the first column suggests that, controlling for the relevant variables, protection caused the poverty index to increase, on average, by an additional 2.068 points more than would have occurred in the absence of protection ($p<0.01$). These results indicate that even when key variables are controlled for, a conventional OLS regression fails to account for the selection bias that arises from non-random placement of protected areas. Note that in these regressions we control for confounders and baselines. However, a regression imposes a parametric form on the relationship between protection and outcome, thus causing bias when the parametric form is incorrect and the covariate distributions for treated and control segments are substantially different (on the other hand, matching is non-parametric, and as shown in the Technical Appendix, matching substantially reduces the differences in covariate distributions between the treated and control groups).

The fourth column replicates conventional methods which fail to control for confounding factors or outcome baselines, by doing a simple test of mean differences in the post-protection outcomes. In contrast to the estimates based on matching, all these estimates imply that protection caused significantly large negative impacts on the affected communities. For example, the estimate for the poverty index, the estimate implies that protection led to an average increase in the poverty index of 9.170 (a large 'effect size') higher in protected segments compared to unprotected segments.

The dramatic differences between the estimates based on matching (first two rows) and the estimates based on methods conventionally used to evaluate protected area effectiveness (rows three and four) suggest that the conventional methods can lead to substantially inaccurate estimates.

TABLE 1. EFFECT OF PROTECTION ON SOCIO-ECONOMIC OUTCOMES

| | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| **Outcome** | **Poverty index** | **Percent of houses in bad condition** | **Percent of houses in slums** | **Percent of households without telephone** | **Percent of households without electricity** | **Percent of households without water supply** |
| *Matching Estimates (Effect of protection on change in outcome 1973-2000)* | | | | | | |
| ***Covariate Matching – Mahalanobis*** | -3.251*** | -6.429*** | -2.142** | -1.032 | -1.731 | -5.856*** |
| | (0.973) | (2.189) | (1.064) | (2.051) | (3.697) | (1.652) |
| ***Covariate Matching – Mahalanobis with calipers*** | -1.941*** | -4.714** | -1.976** | -1.782 | 2.155 | -4.201*** |
| | (0.543) | (1.489) | (0.795) | (1.709) | (2.772) | (1.212) |
| ***[N outside calipers]*** | [65] | [72] | [63] | [57] | [60] | [63] |
| *Replicating Conventional Methods (Effect of protection on change in outcome 1973-2000)* | | | | | | |
| ***Ordinary Least Squares^*** | 2.068*** | 2.364*** | 0.621* | 11.243*** | 7.354*** | -2.622** |
| | (0.403) | (0.818) | (0.347) | (1.462) | (2.347) | (1.022) |
| *Replicating Conventional Methods (Effect of protection on post-protection outcome measured in 2000)* | | | | | | |
| ***Difference in Means†*** | 9.170*** | 6.114*** | 0.695** | 29.085*** | 19.270*** | 4.352*** |
| N treated | 399 | 399 | 399 | 399 | 399 | 399 |
| (N available controls) | (15988) | (15988) | (15988) | (15988) | (15988) | (15988) |

**^** An Ordinary Least Squares model regresses the outcome on protection while controlling for key covariates.
*†* A t-test is applied to evaluate the difference in means of post-protection outcomes between treated and control segments.
*‡* Standard errors in parenthesis under estimate.
**¤** Calipers restrict matches to units within 1 standard deviation of each covariate.
*** significant at 1%; ** significant at 5%; * significant at 10%

# DISCUSSION

## IMPLICATIONS OF THE FINDINGS

We find no evidence that Costa Rica's protected areas have had a net negative effect on local populations. In fact, we find the opposite: the evidence suggests that, if anything, protected areas have had a net positive effect on indicators of local social welfare. In contrast, estimates based on methods that fail to account for the non-random assignment of protected areas suggest the opposite relationship: protection has negative effects on social welfare.

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 near the protected area to promote ecotourism. Third, some conservation programs[6] 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). These results suggest that such interventions may have improved the livelihoods of local communities.

The absence of a net negative effect is remarkable given that we find in a previous study that protection has indeed resulted in reduced deforestation (Andam et al., forthcoming). Thus there have been opportunity costs incurred from protection, which suggests that other economic activities, such as tourism, or infrastructure investments associated with protected areas have helped to offset these costs. Costa Rica, however, has had relatively stable governments over the

[6] 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.

last few decades and has made substantial investments in their protected area system. Thus whether our results would hold for other nations is an open question[7]. This type of analysis should be repeated in other nations, including as treatments a variety of forest governance regimes (e.g., indigenous reserves). Our study thus highlights the need for cooperation between groups collecting spatially explicit poverty data, protected area data, and land-use -land-cover data[8].

## STUDY LIMITATIONS

We acknowledge that although we use a variety of indicators that are correlated with local well-being, they do not capture all aspects of well-being (e.g., hard-to-measure aspects such as "feeling in control of one's life" or "ability to maintain cultural traditions"). A second limitation of this study is the scale of observation: the data are only available at the census segment level and therefore we can only observe average outcomes at this aggregated level. Protected areas may have had 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

[7] Note that our results do not call into question the widely held belief that many of the benefits of biodiversity protection are enjoyed by residents far from protected areas, while many of the costs are incurred by local people (Balmford and Whitten, 2003), and thus transfers from wealthy to poor nations are needed to achieve conservation goals in poor nations.

[8] For example, the UNEP-WCMC Vision 2020 project, which seeks to expand the World Database on Protected Areas (WDPA) to cover socio-economic issues as well as develop indicators related to protected areas and social impacts.

relocate to census segments that are farther away from protected areas, and the effects of protection on these people cannot be detected without individual-level, panel data collected pre- and post-protection.

Although we cannot fully observe distributional impacts of protection, we conduct some analyses to explore this issue. If we assume that migration tends to be local (displaced or adversely affected residents move to nearby unprotected segments after protected areas are established), then we would expect these nearby segments to have worse socioeconomic outcomes, on average, than segments that are farther away from protected areas. However, we do not find this expected result when we apply the matching methods to measure the spillover effects of unprotected segments located close to protected areas (see Technical Appendix). On the contrary, we find that, with the exception of one outcome, protection has either no effect or small positive effects on nearby unprotected segments. We also test for differences in the change in population between 1973 and 2000 between protected and unprotected segments (not reported here), and we find no significant differences. While these tests do not completely rule out the possibility of adverse effects from protection on some subgroups of the local communities, they do suggest that our main findings (that protection led to positive changes in socioeconomic outcomes in protected segments) cannot be explained by a significant local migration out of the protected segments to nearby unprotected areas.

## CONCLUSION

We apply a quasi-experimental approach to provide rigorous estimates of the social impacts of protected areas in Costa Rica. We address the question "what is the effect of this protected area on economic outcomes within neighboring communities?" by using matching methods to identify suitable comparisons for affected communities. We find that Costa Rican protected areas have had a net positive impact on economic outcomes within neighboring communities.

Our research approach represents a major advance to estimate causal impacts of protected areas on local welfare and makes an important contribution to strengthening the evidence base in conservation policy (Ferraro and Pattanayak, 2006; Sutherland et al. ,2004). In principle, our approach to evaluating impacts could be applied to any measures of well-being and thus future collaborative evaluations among anthropologists, economists, and local people would be fruitful. Furthermore, future studies can use similar methods to explore how impacts vary conditional on observable covariates (e.g., how do impacts vary with the degree of baseline poverty?). Particularly interesting covariates to examine include variables capturing the degree of local participation in management decisions and benefit-sharing (i.e., does more participation lead to greater socio-economic benefits?), as well as the management status of the protected area (e.g., IUCN categories).

# TECHNICAL APPENDIX

## DATA

We use socioeconomic data from the Instituto Nacional de Estadistica y Censos (INEC). We use geographically referenced data that are available at the census segment level for 1973 and 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).

We develop a dataset of the census segments from the last census in 2000 by overlaying the GIS data layers for these segments with the GIS data layers for biophysical and infrastructure variables. We use areal interpolation techniques (Reibel, 2007) to disaggregate data from the census segments surveyed in 1973 to the level of the segments surveyed in 2000. Although there are 17,261 census segments in the GIS map, the final dataset consists of 17,071 segments, because we exclude segments for which there are data errors or for which there are no census data[9]. On average, a segment population is 109 in 1973 and 221 in 2000. The census segments have a mean area of about 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

[9] The excluded segments were not 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.

presented in Table A1. The treatment is defined as "more than 20% of segment protected before 1980". Costa Rica's protected areas are illustrated in Figure 1.

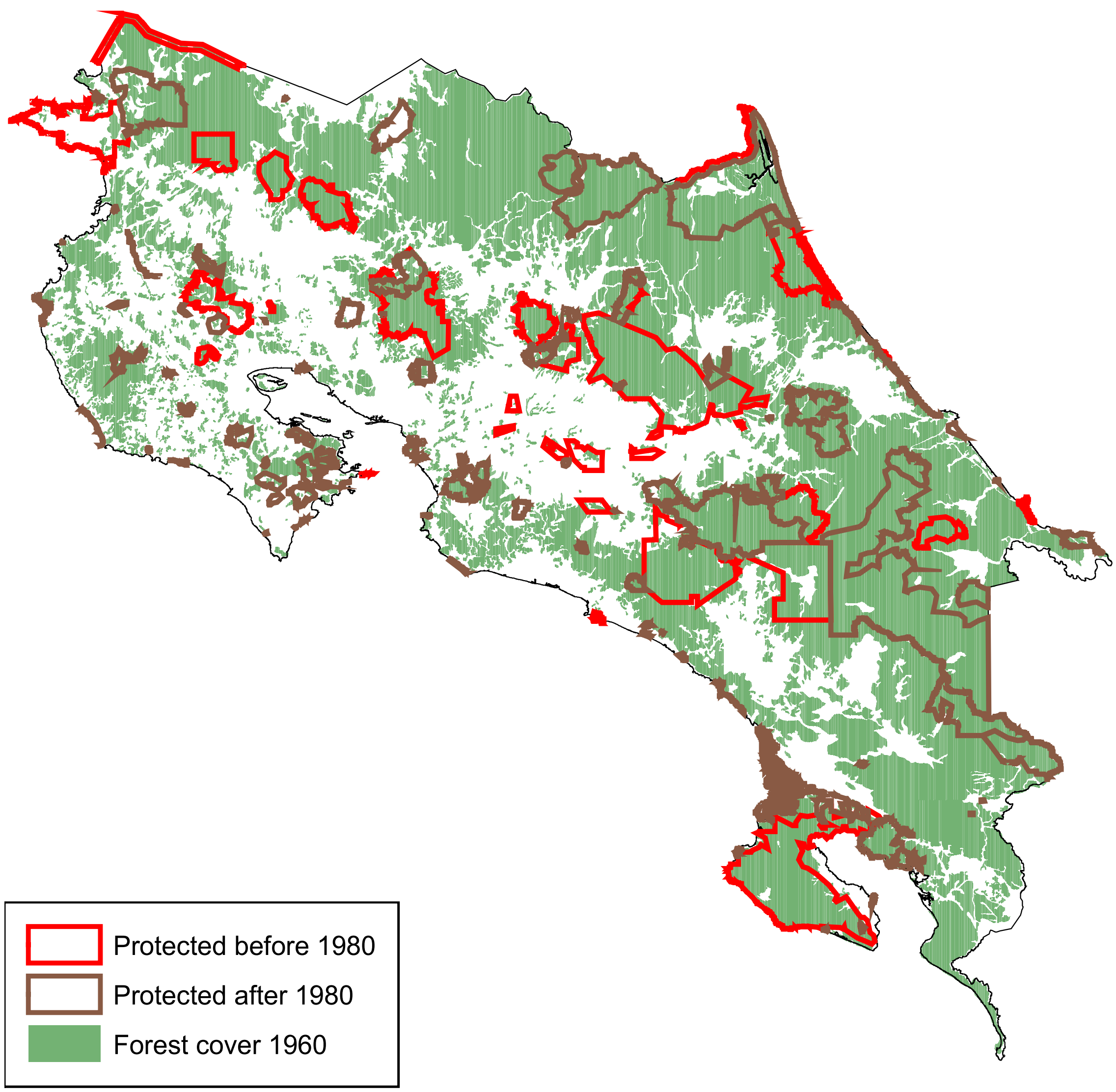


Figure 1. Costa Rica's Protected Areas

The pre-1980 protected areas whose effects are captured in this study include Biological Reserves (Cordillera Volcanica Central, Golfo Dulce, Grecia, Los Santos, Rio Macho, Taboga), forest reserves (Pacuare-Matina, Zona de Emergencia Volcan Arenal), a National Monument (Guayabo), National Parks (Barra Honda, Braulio Carrillo, Cahuita, Chirripo, Corcovado, Juan Castro Blanco, Manuel Antonio, Palo Verde, Rincon De La Vieja, Santa Rosa, Tortuguero, Volcan Iraza, Volcan Poas, Volcan Tenorio, Volcan Turrialba), Protected Zones (Arenal-Monterverde, Caraigres, Cerro Atenas, Cerros de Escazu, Ceros de la Carpintera, El Rodeo, Miravalles, Rio Grande, Rio Tiribi, Tenorio) and a Wildlife Refuge (Corredor Fronterizo).

In testing for the effect of protection, we 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. We 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), we define the following set of covariates:

- *Forest area:* We 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, we 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). We 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 volume 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, we use Costa Rica's *land use capacity classes*, which are determined by slope, soil characteristics, life-zones, risk of flooding, dry period, fog, and wind influences. We 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), we 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.

We 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[10] on the set of covariates. When we 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 we 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 we 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).

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[10] we obtain a binary treatment variable as follows: Treatment=1 if more than 20 percent of the protected area is protected and Treatment=0 otherwise.

TABLE A1. DESCRIPTIVE STATISTICS

| Variable | Description | Mean | Standard Deviation | Range |
|---|---|---|---|---|
| | ***Biophysical variables*** | | | |
| Area | Total land area covered by the segment in $km^2$ | 2.909 | 1.270 | 0.001 – 736 |
| Forest area | Total forest area in the segment in 1960 in $km^2$ | 1.711 | 11.229 | 0 – 708.707 |
| Road-less volume ($km^3$) | The sum of the product of area and distance to nearest road (1969) for every square of length 100m within the segment | 40.870 | 343.011 | 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 | 36.868 | 37.833 | 0.041 – 206.950 |
| Land use capacity classes I, II, and III (%) | Percent of segment area under the land classes I, II, and III, measured in $km^2$<br><br>Class I: Agricultural Production – annual crops;<br><br>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. | 33.400 | 44.400 | 0 – 100 |
| Land use capacity class IV (%) | Percent of segment area under the land class IV, measured in $km^2$<br><br>Class IV: Moderately suitable for agricultural production; permanent or semi-permanent crops such as fruit trees, sugar cane, coffee, ornamental plants, etc. | 40.100 | 46.500 | 0 – 100 |

TABLE A1. DESCRIPTIVE STATISTICS

| Variable | Description | Mean | Standard Deviation | Range |
|---|---|---|---|---|
| Land use capacity classes V, VI, and VII (%) | Percent of segment area under the land classes V, VI, and VII, measured in $km^2$<br><br>Class V: Strong limitations for agriculture; forestry or pastureland<br><br>Class VI: Strong limiting factors on agricultural production; land is only suitable for forest plantations or natural forest management<br><br>Class VII: Strong limiting factors on agricultural production; land is only suitable for forest plantations or natural forest management | 19.800 | 37.000 | 0 – 100 |
| Land use capacity classes VIII and IX (%) | Percent of segment area under the land classes VIII and IX, measured in $km^2$<br><br>Class VIII: Land is suitable only for watershed protection<br><br>Class IX: Land is suitable only for protection | 6.400 | 21.700 | 0 – 100 |
| Distance to forest | Distance from the centroid of the segment to the closest forest in 1960, measured in km | 2.209 | 2.050 | 0 – 9.045 |
| 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 |

TABLE A1. DESCRIPTIVE STATISTICS

| Variable | Description | Mean | Standard Deviation | Range |
|---|---|---|---|---|
| | ***Socioeconomic variables*** | | | |
| Poverty index (1973) | Multidimensional index of poverty derived from a linear combination of a set of key socioeconomic variables (see Technical Appendix for detailed description) | -0.028 | 10.033 | -25.594 – 26.995 |
| Poverty index (2000) | Multidimensional index of poverty derived from a linear combination of a set of key socioeconomic variables (see Technical Appendix for detailed description) | -0.086 | 8.190 | -14.102 – 65.911 |
| Condition of house (1973) | Percent of houses in the segment in bad condition | 15.010 | 12.669 | 0 – 86.441 |
| Condition of house (2000) | Percent of houses in the segment in bad condition | 10.851 | 12.252 | 0 – 100 |
| Slum conditions( 1973) | Percent of houses in the segment in slums | 1.926 | 4.463 | 0 – 60.368 |
| Slum conditions (2000) | Percent of houses in the segment in slums | 1.387 | 5.897 | 0 – 97.059 |
| Telephone access (1973) | Percent of households in the segment without telephone access | 93.961 | 14.119 | 0 – 100 |
| Telephone access (2000) | Percent of households in the segment without telephone access | 49.988 | 32.453 | 0 – 100 |
| Electricity access (1973) | Percent of households in the segment without electricity | 41.630 | 37.894 | 0 – 100 |
| Electricity access (2000) | Percent of households in the segment without electricity | 24.368 | 40.434 | 0 – 100 |
| Water supply | Percent of households in the segment without water supply | 34.990 | 34.472 | 0 – 100 |

TABLE A1. DESCRIPTIVE STATISTICS

| **Variable** | **Description** | **Mean** | **Standard Deviation** | **Range** |
|---|---|---|---|---|
| (1973) | | | | |
| Water supply (2000) | Percent of households in the segment without water supply | 4.156 | 13.624 | 0 – 100 |
| Population (1973) | Population of segment | 108.63 | 117.955 | 0 – 1893 |
| Population (2000) | Population of segment | 221.263 | 85.938 | 1 – 2318 |

In statistical jargon, the impact of a program (in the context of this study the program is protection) is the Average Treatment Effect on the Treated (ATT). The methods of matching provide one way to estimate the ATT when protection is influenced by observable characteristics of the community (e.g. income) and the analyst wishes to make as few parametric assumptions as possible about the underlying structural model that relates protection to the outcomes. 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 (i.e., protection is independent of forest cover change for similar land units), then the treatment effect can be measured without bias.

Mathematically, the key assumption is: $E[Y(0)\,|\,X,T=1]=E[Y(0)\,|\,X,T=0]=E[Y(0)\,|\,X]$ and $E[Y(1)\,|\,X,T=1]=E[Y(1)\,|\,X,T=0]=E[Y(1)\,|\,X]$, where $Y_i(1)$ is the socioeconomic outcome when census segment *i* is protected, $Y_i(0)$ is the outcome when segment *i* is unprotected, T is treatment (T=1 if protected), and X is the set of pretreatment characteristics on which segments are matched. This assumption, called the conditional independence assumption (CIA), implies that participation in the project depends solely on a set of observable characteristics (*X*), and that we can observe the variables which simultaneously affect both participation and outcomes. For identification purposes, we also need one other assumption: $c < P(T=1\,|\,X=x) < 1-c$ for $c > 0$. In other words, if all segments with a given vector of covariates were protected, there would be no observations on similar unprotected segments, and therefore, no suitable comparison group.

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 (Rubin, 1980; Rosenbaum and Rubin, 1983; Imbens, 2004). The impact of the program is then estimated as the average difference in the outcomes for each program participant from a weighted average of outcomes in each similar comparison group participant from the matched sample. Matching methods differ in the selection of the matched comparison and in how these weighted average differences in outcomes are constructed.

To select a matching method, the key consideration is to ensure that the matched target and comparison groups have similar pre-program characteristics. Therefore, the recommended approach is to use a variety of matching methods and select the one that gives the best covariate balance between matched target and comparison groups (Ho et al., 2007). We tried a variety of matching methods and selected the one that gave us the best covariate balance: covariate matching that uses the Mahalanobis distance metric to identify matches that are similar to the protected segments. We match with and without calipers. Matching was done in R (Sekhon, 2007).

In Table A2, we assess the differences between treated and control segments, before and after matching[11]. The third column presents mean covariate values for segments with protected areas (treated) and the fourth column presents mean covariate values for segments without protected areas (control). The fifth and sixth columns of Table A2 present two measures of the differences in the covariate distributions between treated and control segments: the difference in means and the average distance between the two empirical quantile functions (values greater than 0 indicate deviations between the groups in some part of the empirical distribution). The seventh and eighth columns present the median and maximum differences between the two empirical quantile functions (values greater than 0 indicate deviations between the groups in some part of

[11] For the matching covariates in the first six rows, which are used for the analyses on all the outcome variables, we report the differences for the matching model with poverty index as the outcome and as one of the baseline matching covariates. The differences are similar in the other matching models. For all other matching covariates that were used in only once in each model (that is, the baseline measures of socioeconomic indicators in 1973) we report the differences for that model.

the empirical distribution), and the last column presents the mean difference in the empirical cumulative distribution (to compare relative balance across the covariate dimensions).

Before matching, the protected segments are very different from the control segments: whereas protected segments had an average of about 15 sq. km of forest in 1960, control segments had on average less than 1 sq. km of forest.  Protected segments also had smaller percentages of their land under agriculturally productive lands, greater baseline road-less volume (less accessible forest), and are farther from major cities. Such characteristics tend to increase poverty and lower economic growth.  Furthermore, all the baseline socioeconomic outcomes suggest that protected segments are poorer in 1973 (except percent of houses in slums, which is about even). As described in the Data section of this Technical Appendix, a probit model that regresses a binary variable for protection on the covariates indicates that these covariates indeed influence the probability of protection.  As Table A2 shows, matching substantially improves covariate balance: the measures of differences in the fifth to ninth columns all reduce after matching, in some cases quite dramatically.

TABLE A2.COVARIATE BALANCE

| Variable | Sample | Mean Value Protected Segments | Mean Value Control Segments* | Diff in Mean Value | Mean eQQ Diff** | Median eQQ Diff** | Max eQQ Diff** | Mean eCDF Diff^ |
|---|---|---|---|---|---|---|---|---|
| *Forest Area in 1960 ($km^2$)* | Unmatched | 15.816 | 0.827 | 14.989 | 14.902 | 5.158 | 335 | 0.476 |
| | Matched | 15.816 | 10.422 | 5.394 | 5.218 | 1.427 | 335 | 0.051 |
| *High Productivity Land ▫ (percent of segment area)* | Unmatched | 4.830 | 34.735 | -29.905 | 0.299 | 0 | 1.000 | 0.285 |
| | Matched | 4.830 | 8.229 | -3.399 | 0.033 | 0 | 0.257 | 0.087 |
| *Medium Productivity Land (percent of segment area)* | Unmatched | 16.007 | 41.709 | -25.702 | 0.257 | 0 | 0.931 | 0.236 |
| | Matched | 16.007 | 17.507 | -1.500 | 0.016 | 0 | 0.141 | 0.019 |
| *Medium-Low Productivity Land (percent of segment area)* | Unmatched | 23.059 | 19.073 | 3.986 | 0.104 | 0.011 | 0.420 | 0.127 |
| | Matched | 23.059 | 24.284 | -1.225 | 0.033 | 0.007 | 0.131 | 0.035 |
| *Road-less Volume ($km^3$)* | Unmatched | 319.040 | 20.902 | 298.138 | 292.31 | 38.678 | 9707.2 | 0.339 |
| | Matched | 319.040 | 214.470 | 104.570 | 103.35 | 11.902 | 9707.2 | 0.060 |
| *Distance to City (km)* | Unmatched | 53.836 | 35.294 | 18.542 | 18.488 | 16.208 | 56.730 | 0.150 |
| | Matched | 53.836 | 55.582 | -1.746 | 4.795 | 3.496 | 21.948 | 0.035 |
| *Poverty Index in 1973* | Unmatched | 8.089 | -0.642 | 8.731 | 8.749 | 8.588 | 20.379 | 0.262 |
| | Matched | 8.089 | 8.912 | -0.823 | 0.964 | 0.399 | 4.093 | 0.037 |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| *Percent of houses in bad condition in 1973* | Unmatched | 18.342 | 15.019 | 3.323 | 3.502 | 3.161 | 26.251 | 0.081 |
| | Matched | 18.342 | 18.086 | 0.256 | 0.743 | 0.529 | 7.327 | 0.014 |
| *Percent of houses in slums in 1973* | Unmatched | 1.454 | 1.942 | -0.488 | 0.602 | 0.008 | 39.152 | 0.018 |
| | Matched | 1.454 | 1.396 | 0.058 | 0.146 | 0.004 | 3.483 | 0.027 |
| *Percent of houses without telephones in 1973* | Unmatched | 99.304 | 93.602 | 5.702 | 5.813 | 0.532 | 86.692 | 0.199 |
| | Matched | 99.304 | 99.337 | -0.033 | 0.053 | 0 | 3.046 | 0.005 |
| *Percent of houses without electricity in 1973* | Unmatched | 69.351 | 39.359 | 29.992 | 29.968 | 29.134 | 58.563 | 0.259 |
| | Matched | 69.351 | 70.347 | -0.996 | 2.584 | 1.752 | 9.030 | 0.035 |
| *Percent of houses without access to water supply in 1973* | Unmatched | 59.370 | 32.899 | 26.471 | 26.435 | 30.553 | 41.427 | 0.262 |
| | Matched | 59.370 | 61.724 | -2.354 | 2.657 | 1.500 | 8.912 | 0.029 |

¤ Low productivity land is the omitted category.

* Weighted means for matched controls.

** Mean/Median/Maximum Raw eQQ = mean/median/maximum difference in the empirical quantile-quantile plot of treatment and control groups on the scale in which the variable is measured.

^ Mean eCDF = mean differences in empirical cumulative distribution functions

## SPILLOVER EFFECTS

We estimate the spillover effects of protection onto neighboring unprotected segments. In this analysis, we define the treatment group as segments with more than 20 percent of their 10-km buffer protected before 1980. We take a number of precautions to ensure that we reduce potential bias in the estimation of local spillover effects. First, we 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, we exclude segments whose buffers received more than 10% protection before 1980[12]. Third, to reduce the potential bias from spillover effects of protection after 1980, we 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, we 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, we estimate a Probit selection model for 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 we therefore exclude from the set of matching covariates for this analysis.

[12] 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).

The estimates of spillover effects of protected areas on socioeconomic outcomes in neighboring unprotected segments are presented in Table A3. With the exception of the access to electricity outcome, we find that protection has either no effect or small poverty-alleviating effects in these unprotected segments.

TABLE A3. ESTIMATES OF THE SPILLOVER EFFECT OF PROTECTION ON SOCIOECONOMIC OUTCOMES IN NEIGHBORING UNPROTECTED SEGMENTS

| | **1** | **2** | **3** | **4** | **5** | **6** |
|---|---|---|---|---|---|---|
| **Outcome** | **Poverty index** | **Percent of houses in bad condition** | **Percent of houses in slums** | **Percent of households without telephone** | **Percent of households without electricity** | **Percent of households without water supply** |
| *Matching Estimates (Effect of protection on change in outcome 1973-2000)* | | | | | | |
| ***Covariate Matching – Mahalanobis*** | 0.134 | -1.241* | -0.282 | -0.621 | 10.071*** | -0.725* |
| | (0.258) | (0.673) | (0.257) | (1.165) | (1.903) | (0.416) |
| ***Covariate Matching – Mahalanobis with calipers*** | 0.147 | -1.373** | -0.223 | -0.654 | 10.101*** | -0.589 |
| | (0.252) | (0.665) | (0.252) | (1.161) | (1.894) | (0.390) |
| ***[N outside calipers]*** | [5] | [8] | [7] | [10] | [5] | [5] |
| N treated | 786 | 786 | 786 | 786 | 786 | 786 |
| (N available controls) | (11782) | (11782) | (11782) | (11782) | (11782) | (11782) |

♯ Standard errors in parenthesis under estimate.
▫ Calipers restrict matches to units within 1 standard deviation of each covariate.
*** significant at 1%; ** significant at 5%; * significant at 10%

## SENSITIVITY TO TREATMENT THRESHOLD

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 matching estimates have similar signs, magnitudes, and statistical significance levels. The matching estimate of the effect of protection on the change in poverty index lies between -1.28 and -2.89 and all estimates are significant ($p<=0.011$)[13]. All the other estimates are similar to the estimates in Table 1: for the outcome 'percent of houses in bad condition', the estimates lie between -3.81 and -5.41 ($p<0.01$); for 'percent of houses in slums' the estimates lie between -1.65 and -1.97 ($p<0.10$ with calipers and $p<0.05$ without calipers); for 'percent of households without telephone, -1.34 to 1.82 (not statistically different from zero); for 'percent of households without electricity, -2.37 to 1.89 (not statistically different from zero); and for 'percent of households without water supply', -3.78 to -6.10 ($p<0.01$). Therefore, we maintain the findings from Table 1 and conclude that the matching estimates do not indicate any harmful effects of protection on the socioeconomic outcomes. If protection had any effects on socioeconomic outcomes, these effects are small and beneficial.

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[13] Recall that in Table 1, the estimates are -1.94 and -3.25 with and without calipers respectively and both are significant ($p<0.01$).

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