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As global efforts to protect ecosystems expand, the socioeconomic impact of protected areas on neighboring human communities continues to be a source of intense debate. The debate persists because previous studies do not directly measure socioeconomic outcomes and do not use appropriate comparison groups to account for potential confounders. We illustrate an approach using comprehensive national datasets and quasi-experimental matching methods. We estimate impacts of protected area systems on poverty in Costa Rica and Thailand and find that although communities near protected areas are indeed substantially poorer than national averages, an analysis based on comparison with appropriate controls does not support the hypothesis that these differences can be attributed to protected areas. In contrast, the results indicate that the net impact of ecosystem protection was to alleviate poverty.

conservation policy | poverty | empirical evaluation | protected areas | ecosystems

The effect of national parks and reserves on their human neighbors is arguably the most controversial debate in conservation policy (1–9). This debate is particularly contentious in developing nations and has intensified recently as these nations contemplate expanding and strengthening protected area systems under agreements to reduce carbon emissions from deforestation and degradation (REDD) (10). Because ecosystem protection limits agricultural development and exploitation of natural resources (11–14), opposition to protected areas is frequently driven by the assumption that they impose large economic costs and thus exacerbate local poverty (4, 15, 16). However, protected areas can also generate economic benefits by supplying ecosystem services, promoting tourism, and improving infrastructure in remote areas. Net impacts on poverty could thus be positive or negative (1, 2, 8, 17, 18). Recognizing this debate, the 2003 World Congress on Protected Areas' Durban Accord (page 4) urged society to commit "to protected area management that strives to reduce, and in no way exacerbates, poverty" (16).

Assessing empirically whether protected areas have achieved this goal of "do no harm" is difficult. Many studies document high poverty levels and negative community events that are associated with the establishment of protected areas (see references in refs. 19–21). However, these studies do not clearly demonstrate a causal link between protection and poverty because they fail to use direct measures of socioeconomic well-being and to control for confounding effects of geographic and baseline characteristics (5–7, 9, 20). Protected areas are frequently established in remote areas with high poverty rates and low-quality agricultural land (22). To judge whether protected areas are responsible for exacerbating poverty, the appropriate comparison must be between communities living in or near protected areas and communities with similar characteristics and trends that are not affected by protected areas (8, 18, 23).

We achieve this requisite comparison through a quasi-experimental design that improves on previous studies in four significant ways. First, we use poverty measures based on household-level surveys. Household-level data on tangible assets provide the most reliable comparative indicators of human welfare. Second, we analyze impacts at the local scale, which matches the scale at which protected areas are likely to affect communities (see ref. 24 for a discussion on importance of scale). Third, we employ matching methods to select appropriate control communities. These controls are used to answer the central research question: "How different would poverty have been in communities around protected areas in the absence of these areas?" We compare communities heavily affected by protected areas (treated) with similar communities that are less affected by protected areas (controls). Matched control communities are chosen to be similar to treated communities with respect to confounding baseline characteristics that may affect both the placement of protected areas and how poverty changes over time. Matching methods thus ensure that the impacts observed in this study are not due to broader trends in economic growth and poverty reduction, which would affect both treated and control communities. Fourth, our study estimates long-term system-wide impacts, rather than the impacts of a single protected area or small set of protected areas. We study impacts in Costa Rica and Thailand because they are biodiverse developing nations with reliable national statistics and were early adopters of protected area systems, yet they have quite different institutional, economic, and ecological histories.

Poverty Measures and Protected Areas

Our poverty measures are based on national census data of household characteristics and assets (see *Materials and Methods* and *SI Appendix*). In Costa Rica, we use a poverty index (25). In Thailand, we use the poverty headcount ratio, which is the share of the population with monthly household consumption below the poverty line (26). Larger values of both measures imply greater levels of poverty. The unit of analysis for Costa Rica is the census segment (tract), and for Thailand the subdistrict. The outcome of interest is poverty in 2000.

We focus on protected areas created 15 or more years before the poverty outcomes are measured to study longer-term impacts. The treated units are defined as segments and subdistricts with 10% or more of their areas protected by 1985 in Thailand and by 1980 in Costa Rica. We select a 10% threshold because it reflects the call by the fourth World Congress on National Parks and

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Protected Areas to protect 10% of each of the world's major biomes by 2000, and by the Conference of Parties to the Convention on Biological Diversity to conserve 10% of each of the world's ecoregions. The group of available controls, from which matched controls are selected, comprises segments or subdistricts with <1% of their areas protected before 2000.

Constructing Comparison Groups

Globally, the overlap between areas of high poverty and high biodiversity is large (27). In Costa Rica, the mean poverty index in treated (protected) segments was more than five points higher in 2000 than in control (unprotected) segments; a difference greater than one standard deviation. This large difference, however, does not necessarily reflect a causal relationship between poverty and conservation. Segments overlapping with protected areas were already among the poorest segments at baseline (Fig. 1).[3] These baseline differences are important because poverty at baseline and in 2000 are highly correlated ($r = 0.83$). Protected areas were also placed in areas with low geographic potential for economic growth. As shown by Fig. 2, treated subdistricts in Thailand were considerably steeper than control subdistricts. In both countries, treated areas had lower expected land productivity and at baseline were more forested and less accessible to roads and markets (Table S5 and Table S6 of *SI Appendix*).

Spatial overlap between protected areas and low economic potential is a global phenomenon (see references in refs. 11 and 22). A credible analysis must control for confounding baseline characteristics that affect both the placement of protected areas and changes in poverty. We identify potential confounders based on the history of protected area establishment and patterns of economic growth in rural Costa Rica and Thailand. These confounders include preprotection poverty, forest cover, land productivity, and access to transportation and market infrastructure (Table S1 and Table S2 of *SI Appendix*).[4]

To control for these confounders, we use matching methods with bias-adjustment for imperfect matching in finite samples (28). The goal of matching is to ensure the covariate distributions of treated and control units are similar (called covariate balancing), thereby removing observable sources of bias. Matching can be viewed as a way to make the treated and control covariate distributions look similar by reweighting the sample observations (e.g., control units that are poor matches receive a weight of zero). Matching thus mimics experimental design by *ex post* construction of a control group (see *Materials and Methods* for details). For both samples, the covariate balance improves dramatically after matching (Table S5 and Table S6 of *SI Appendix*).

Results

Fig. 3 presents impact estimates for both countries (Table S7 of *SI Appendix* presents estimates in tabular format). The first (dark gray) bar in each panel presents the differences in means of 2000 poverty measures between treated and untreated areas without controlling for baseline differences. The positive signs of the estimates seem to suggest that protection exacerbated poverty.

In contrast, the impact estimates based on matching to control for confounders (lighter bars) indicate that protection reduced poverty. The second bar in the left panel shows that the mean poverty index among Costa Rica's treated segments was ∼1.3 points lower than matched control segments. This estimate implies that ∼10% of the poverty reduction observed in treated segments over time is attributable to protected areas. The second bar in the right panel shows that the mean poverty headcount ratio among treated subdistricts in Thailand was 7.9 percentage points lower than matched control subdistricts. This value corresponds to ∼30% of the counterfactual poverty level, which is represented by the mean poverty headcount ratio for the matched control subdistricts. The third bars in each panel present estimates based on matching using calipers to improve covariate balance. Calipers define a tolerance level for judging the quality of the matches: if available controls are not good matches for a treated unit (i.e., there is no match within the caliper), the unit is eliminated from the sample [see *SI Appendix* (*Methods*) for details]. The estimated impacts on poverty are similar to the estimates generated without calipers.

Another way to indicate the relative magnitudes of the impacts is to normalize the results using effect sizes calculated by dividing the average treatment effect on the treated estimate by standard deviation of the matched control units. For Costa Rica, the estimated effect sizes on the poverty index from matching without and with calipers are −0.20 and −0.22, respectively. For Thailand, the estimated effect sizes on the headcount ratio from matching without and with calipers are −0.43 and −0.30.

Thus, although a simple comparison of mean differences in postprotection poverty suggests that protection exacerbated poverty, there is no evidence of such an impact conditional on baseline characteristics. In fact, the evidence suggests the opposite: protection contributed to poverty alleviation.

Robustness Checks

We conducted a series of robustness checks (see *SI Appendix* for details). As an alternative postmatching model to estimate treatment effects and control for imperfect covariate balance, we estimated postmatching, linear regressions using the matching covariates and extended sets of covariates (Table S10 and Table S11 of *SI Appendix*). We also changed the cut-off date to include all protected areas established before 2000 (Table S15 of *SI Appendix*) and changed the protection threshold defining treatment from 10% to 20% and 50% (Table S16 of *SI Appendix*). The estimated treatment effects are consistently negative and significantly different from zero.

One rival explanation for our results is that protected areas displace poor people into control segments or subdistricts, thereby making protection falsely appear to alleviate poverty. To test this hypothesis, we estimated the effect of protected areas on population (Table S12 of *SI Appendix*). The estimated effects of protection on population density and growth rates are small and statistically indistinguishable from zero ($P > 0.10$), which could be consistent with an emigration story only if the exodus of poor people were matched by a countervailing influx of wealthier people.

Another rival explanation is that protection had negative effects on poverty in nearby control segments or subdistricts. To assess this explanation, we re-estimated the treatment effects after excluding all control units within 10 km of a protected area—i.e., those that might be contaminated by spillovers. We also directly estimated local spillovers by matching control units located within 5 km of a protected area to control units farther away from protected areas. The results do not support the rival spillover explanation (Table S17 of *SI Appendix*): in contrast, the results suggest that if spillovers exist, they are positive, which implies that our estimates are biased toward zero rather than away from zero.

A third rival explanation is that in spite of our efforts to control for observable sources of bias, we may have omitted a confounding variable that is positively correlated with both protection and poverty reduction. Sensitivity analysis examines the degree to which uncertainty about hidden biases in the assignment of protection could alter our conclusions. We use Rosenbaum's (29)

[3] Most protected areas were created just before or well after 1973, and the 1973 census data allow for construction of a poverty index that is directly comparable to the 2000 index.

[4] Unlike in the Costa Rica case, but similar to the situation in many nations, baseline poverty data for small areas do not exist in Thailand. To control for the baseline state and trend of the poverty outcomes, we use a large set of fixed or pretreatment characteristics that, based on theory and practice, are believed to affect both poverty and protection. We also force the matches to be within the same district to control for unobservable district-level, time-invariant characteristics (see *SI Appendix* for details).

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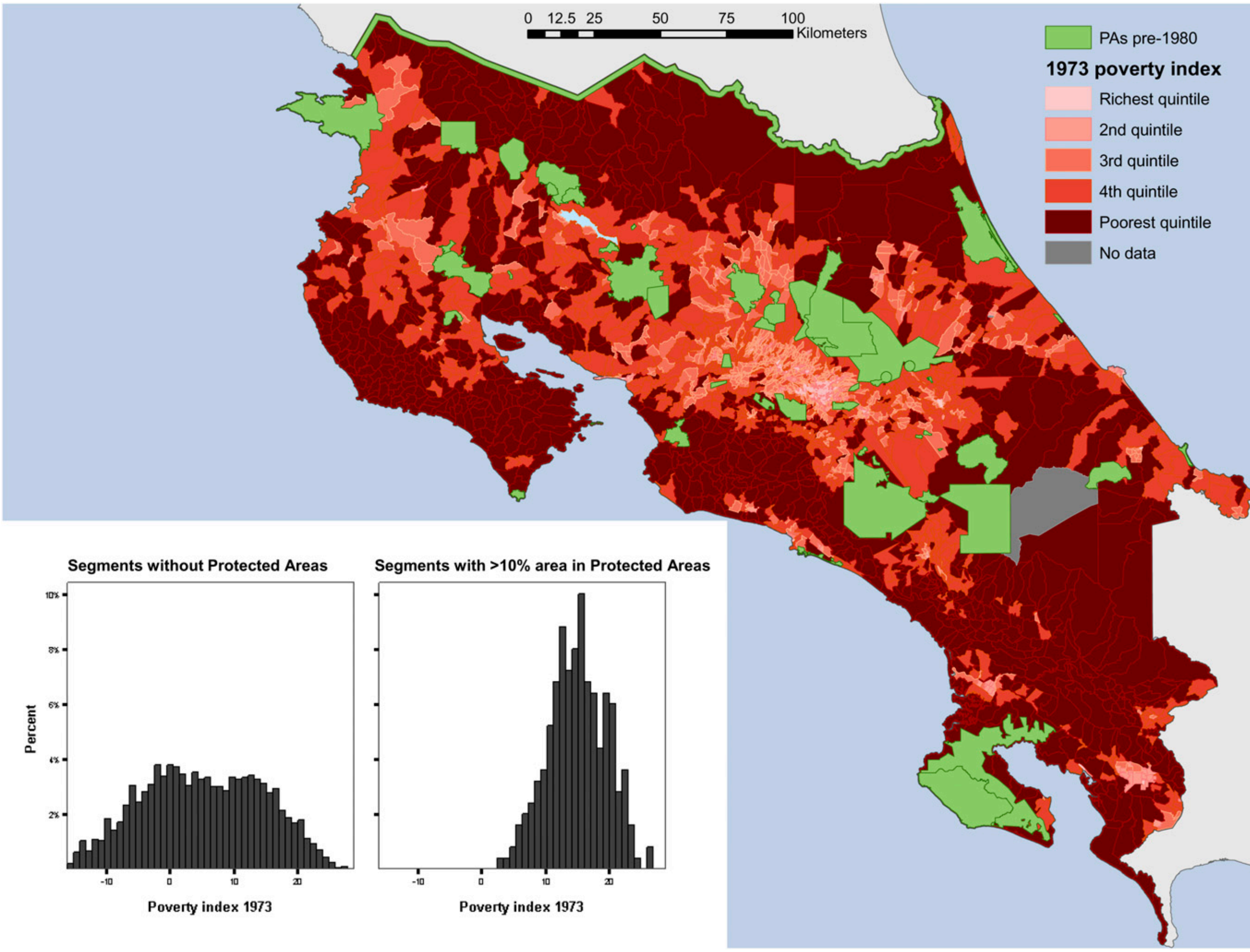

Fig. 1. Costa Rican protected areas established before 1980 were placed in census segments that had baseline poverty indices three times higher than segments without protected areas. The odds of a segment having >10% of its area protected before 1980 are >20 times higher for segments with above-average baseline poverty.

recommended sensitivity test (see *SI Appendix* for details). In both countries, our finding that protection did not exacerbate poverty could change only in the presence of a powerful unobserved confounder, strongly correlated with both protection and poverty alleviation (see Table S8 and Table S9 of *SI Appendix*).

Discussion

Many authors have noted a dearth of empirical evidence in conservation policy (e.g., refs. 23 and 30). Previous studies examine the environmental impacts of protected areas (11–13, 31–37), but one of the most contentious debates in conservation science and policy is the impact of protected areas on the human welfare of neighboring communities. The debate remains contentious because previous studies have failed to use direct measures of human welfare and empirical designs that estimate counterfactual outcomes: how would these communities have fared in the absence of protected areas? Estimating counterfactual poverty levels requires one to control for factors that jointly affect where protected areas are established and the local dynamics of economic growth and poverty. We demonstrate that such control can be obtained by combining available secondary data, which provide objective quantitative measures of poverty and confounders, with statistical methods designed to identify causal relationships. Our study highlights the need for cooperation between groups collecting spatially explicit data on poverty, protected areas, and land-use/land-cover change.[5]

Despite the differences in Costa Rica's and Thailand's institutions, economic development trajectories, and protected area system histories, we find no evidence that their protected areas systems have exacerbated poverty on average in neighboring communities. In fact, we find the opposite: if anything, protected areas have reduced poverty.[6] This result is remarkable given that previous studies have shown that protected area systems in these two nations have reduced deforestation (11, 39). These results thus support recent claims, based on an examination of World Bank project evaluations, that biodiversity conservation is not necessarily incompatible with development goals[7]

[5] For example, the UNEP-WCMC Vision 2020 project seeks to expand the World Database on Protected Areas to socioeconomic issues as well as develop indicators related to protected areas and social impacts.

[6] Our results, which focus on changes in poverty, 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 (38).

[7] Although our conclusion that protected areas reduce poverty appears to be consistent with the results of Wittemyer et al. (9), they use population growth as a proxy for socioeconomic benefits. In our study, population, whether measured as densities or as growth rates, was not significantly affected by protected areas in either nation (see *SI Appendix* for details). In Costa Rica, if one did not control for confounding factors in estimating the population impact of protection, one would have erroneously inferred that protection caused a significant population increase.

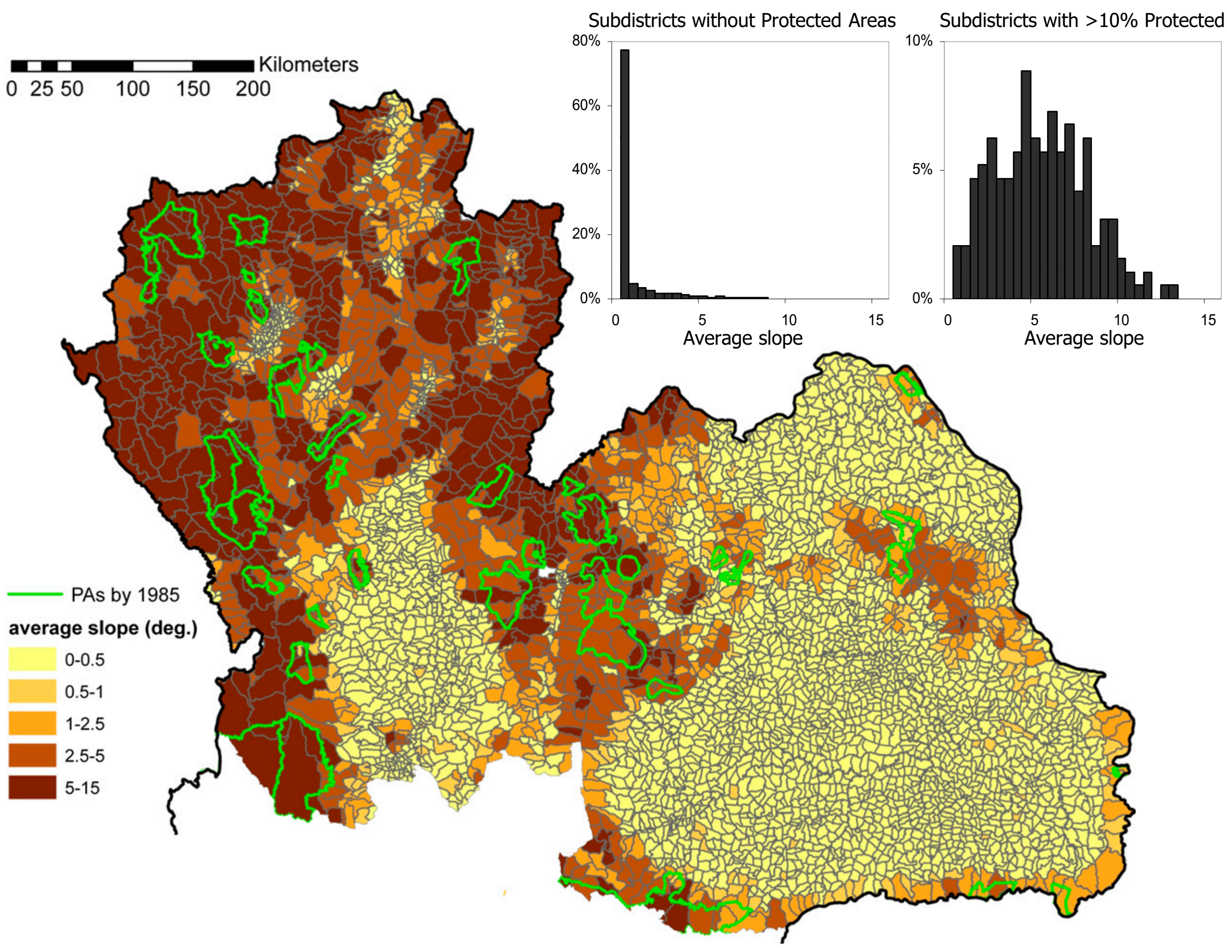

Fig. 2. Protected areas in North and Northeast Thailand established before 1985 were placed in subdistricts with land more than five times steeper than land in subdistricts without protected areas. Much of this targeting was designed to protect important upper watershed areas.

(40). Our results also suggest that protecting biodiversity can contribute to both environmental sustainability and poverty alleviation, two of the United Nations Millennium Development Goals (41, 42).

Several caveats should be emphasized. First, we measure the average net impact of protected areas on poverty. Our results do not imply that all segments, subdistricts or poor households experienced poverty alleviation from protected areas. Second, we measure the impact of protected areas over decades. Short-term impacts may differ. Third, our measures of poverty are based on a limited set of material dimensions and do not capture all dimensions of social welfare (43) (e.g., hard-to-measure aspects such as "feeling in control of one's life" or "ability to maintain cultural traditions"). Future collaborative evaluations among anthropologists and economists could explore other dimensions (44). Finally, our analysis does not elucidate the specific mechanisms through which protected areas may have reduced poverty. We speculate that benefits to local residents have included tourism business opportunities, investments in human and physical capital by national and international agents, and the maintenance of ecosystem services (39, 45, 46). Research to understand these mechanisms is a clear future priority.

Finally, Costa Rica and Thailand are not representative of all developing nations. They have both experienced rapid macroeconomic growth (47, 48), have had relatively stable political systems, have made substantial investments in their protected area systems, and have relatively successful eco-tourism sectors. Thus whether our results would hold for other nations is an open question. Our study can, and should be, replicated in other nations, as well as extended to include a variety of land governance regimes (e.g., indigenous reserves) and explorations of the ways in which impacts vary based on observable covariates and protected area management status (49). Only through multination replications and extensions can we obtain a global picture of the impacts of protected areas on human welfare.

Materials and Methods

For more details on data and methods, see *SI Appendix* and Tables S1–S4.

Data. For Costa Rica, we use data from the population and housing censuses conducted by the Instituto Nacional de Estadistica y Censos (INEC) in 1973 and 2000. Digitized GIS census segment boundaries for 1973 and 2000 were provided by the Cartography Department at INEC. GIS data layers for forest cover in 1960 (11), protected areas (source: National System of Conservation Area Office, Ministry of Environment and Energy, 2006), and the locations of major cities were provided by the Earth Observation Systems Laboratory, University of Alberta, Canada. Other GIS layers are land use capacity (source: Ministry of Agriculture) and roads digitized from hard copy maps for 1969 (source: Instituto Geográfico Nacional, Ministerio Obras Publicas y Transporte). The poverty index builds on recent efforts to develop a census-based poverty index for Costa Rica (25). Summary statistics of the data are presented in Table S1 of *SI Appendix*.

For Thailand, we use data from a poverty mapping analysis (26) which combines data from the Thai Socio-Economic Survey 2000, the Thai Population and Housing Census 2000, and the 1999 Village Survey. Instead of the poverty headcount ratio used in the main text, one could also use the poverty gap, which measures the mean distance between household consumption and the poverty line. Using the gap yields the same conclusions (Table S7 of *SI Appendix*). Protected area boundaries are from the IUCN

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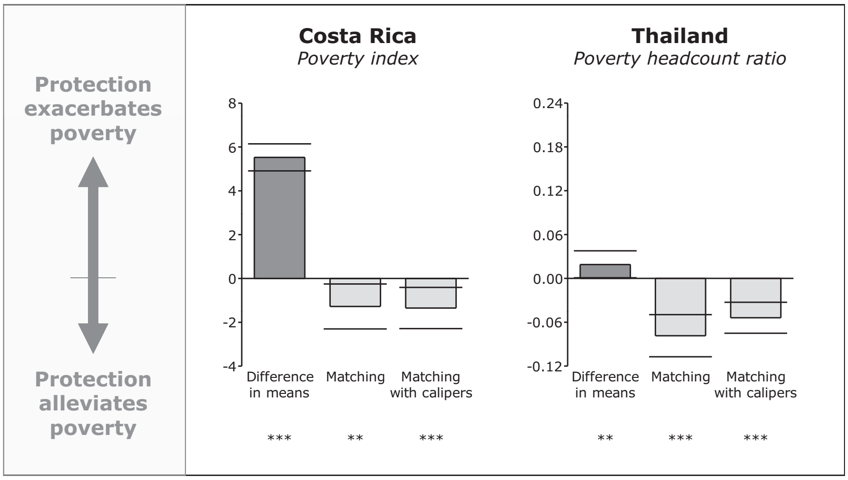

Fig. 3. Do protected area systems exacerbate poverty? Poverty rates in 2000 were, on average, higher near Costa Rica and Thailand protected areas, seemingly suggesting that protected area systems have exacerbated poverty (dark bars). However, estimates using matching methods to control for differences in baseline characteristics that affect both poverty and the location of protected areas indicate that protected areas have alleviated poverty (lighter bars). Bars refer to 95% confidence intervals. Standard errors for matching estimates were calculated using the robust variance formula in ref. 27. A *t* test is used to assess the difference in means between treated and control units. Asterisks refer to tests of the null hypothesis of zero impact (**, $P < 0.05$; ***, $P < 0.01$). Costa Rica sample: *N* treated = 249; *N* control = 4164; *N* treated dropped by calipers = 22. Thailand sample: *N* treated = 192; *N* control = 3479; *N* treated dropped by calipers = 48.

World Database of Protected Areas (Thailand country dataset supplied by ARCBC-ASEAN). Years of establishment for protected areas from this database were cross-checked with information from Thailand's Department of National Parks. Sources of geographic data and summary statistics are presented in Table S2 of *SI Appendix*.

The units of analysis, poverty measures, and covariates are described in the main text, and further details, including the methods for deriving the poverty measures and the motivation for selecting covariates, are provided in *SI Appendix*.

Methods. We use matching methods to estimate the effect of protected areas on poverty in communities near protected areas: the Average Treatment Effect on the Treated (see *SI Appendix* for details). Matching was done in R (50). We selected the matching method that produced the best covariate balance with each country sample (51). For Costa Rica, we chose covariate matching that uses the Mahalanobis distance metric. For Thailand, we chose nearest-neighbor propensity score matching with exact matching on district. All matching is one-to-one with replacement: each treated unit is matched to one control unit. Based on recent work that demonstrates that bootstrapping standard errors is invalid with nonsmooth, nearest-neighbor matching with replacement (52), we use Abadie and Imbens' variance formula whose asymptotic properties are well understood (28). We use the version that is robust to heteroskedasticity. We use a postmatching bias-correction procedure that asymptotically removes the conditional bias in finite samples (28). For caliper matching, we define the caliper as one standard deviation of each matching covariate.

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Supporting Information

Protected Areas Reduced Poverty in Costa Rica and Thailand

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SI Text

Data

Unit of observation. **In the Costa Rica analysis,** the unit of analysis is the census segment (*segmento censal*), which is akin to a census tract. It is the smallest census unit for which we have comparable census data in 1973 and 2000. Each census segment represents between forty to sixty households. Due to the increase in population and number of households between each census, the relative size and number of segments shifted considerably between both census years. Therefore, we faced the challenge of reconciling segment geography from the two periods. We overcome this challenge by using an areal interpolation technique known as areal weighting (1, 2). Using the TwoThemes extension developed for ArcView®, we aggregated 2000 census data to the 1973 segment boundaries, and disaggregated 1973 census data to the 2000 segment boundaries.

Areal interpolation assumes spatial homogeneity within the unit of analysis. We believe it is more accurate to assume spatial homogeneity for the 2000 census segments than to do so for the 1973 census segments because the 2000 segments are smaller in size. Thus our main dataset uses 1973 census segment boundaries, with all census data from 2000 aggregated to the census segment geography of the 1973 segments. Two segments were excluded because there were no 1973 census data for them[*]. We test the robustness of our estimates by repeating our analyses with the 1973 census data disaggregated to match the 2000 census segments (see 'Other Robustness Checks').

[*] We have anecdotal evidence from INEC that these two segments were not surveyed in 1973 because there were no residents within those segments at that time.

In the Thailand analysis, the unit of analysis is a subdistrict ("tambon"). In descending order of size, Thailand has administrative units of "province," "district," "subdistrict," and "village." The sample consists of subdistricts in the North and Northeast regions, where the majority of protected forest areas are located. We exclude subdistricts that are less than 10 km away from a major city (population > 100,000; all of these cities had been established by the 1960's). The average size of a subdistrict in the sample is 74 sq km; the average population is 5043.

Outcomes. The poverty measures used in the analysis are country-specific. The Costa Rica analysis uses a relative poverty measure in which census tracts are compared to each other, not an absolute standard like $1/day. The Thailand analysis uses an absolute standard based on consumption, which is the Thai government definition of the poverty line, rather than an international standard. Thus in both cases we use measures that are more akin to defining poverty as "a socially-specific concept, whereby the consumption needs for escaping poverty in a given society depend on what people generally consume in that society" (3). We believe this notion of poverty is the most policy-relevant notion for nations contemplating maintaining or expanding protected areas (in contrast to, for example, an international poverty line). For additional background on the measurement of poverty in general, see (3-5).

In the Costa Rica analysis, we analyze the effects of protection on a poverty index derived from data common to the 1973 and 2000 population and housing censuses. The poverty index was obtained by using principal components analysis (PCA). The first principal component, that which captures the most variance among the combination of factors, is used to construct the index: factor scores from the first component are used as weights for each variable, which are then combined into a single index score. Cavatassi et al. (6) used PCA in developing a time-variant poverty index for Costa Rica at the third administrative, or district, level. They selected PCA because it can focus solely on census data, is flexible for constructing an index of change over time, is relatively inexpensive and easy to calculate once the data are compiled, and has been used in several countries with results comparable to those of consumption-based welfare indicators (7, 8).

The 16 variables included in the poverty index are described in table S3. To the greatest extent possible, we use the same set of variables as Cavatassi et al. As noted in their report, the variables in their analysis have been found in other studies to be associated with poverty in Costa Rica. We adjust some variables to align them with the index of unsatisfied basic needs (UBN), part of Costa Rica's own national poverty mapping efforts. To make the indexes comparable over time, we follow Cavatassi et al. by pooling the data for 1973 and 2000 before applying the PCA to generate weights for estimating the poverty index.

Socioeconomic outcomes for Thailand are from a poverty mapping analysis by Healy and Jitsuchon (9), applying the poverty mapping methodology developed by Elbers et al. (10). In general, poverty mapping involves estimating poverty for small areas by combining data from household consumption/expenditure surveys, which are detailed but have limited coverage, and census surveys, which contain only basic information on household characteristics but have comprehensive geographic scope. In the Thai case, household income and consumption for the households in the 2000 Thai Socio-Economic Survey are modeled as a function of household characteristics and assets for which the Census contains data for 20% of all Thai households (9). These relationships are then used to predict household income and consumption for all households in the Census. By running simulations and aggregating across households, this method can generate precise estimates of poverty down to the subdistrict level. Poverty mapping techniques have been demonstrated with reasonable accuracy by comparison to known true small-area values (10, 11). Concerns about precision (see report by Banerjee et al. (12) and response by Lanjouw and Ravallion (13)) are not a major concern given that poverty is used here as the outcome variable.

The poverty measurements used in this analysis are the poverty headcount ratio and poverty gap, which are part of the Foster-Greer-Thorbecke (FGT) family of poverty measures (14). The poverty headcount ratio (FGT0) is the share of the population with consumption below the poverty line. The poverty gap (FGT1) modifies this measure by weighting for how far households' consumption falls below the poverty line.

Treatment. The treatment and control units are defined in the main text. The number of protected areas in the analysis is listed in table S4 by International Union for Conservation of Nature (IUCN) categories. Detailed descriptions of the historical process of establishing protected areas are provided elsewhere for both Costa Rica (15) and Thailand (16) and are therefore not repeated here.

Covariates. We control for covariates that could potentially confound the estimation of the effects of protection. We confirmed the narrative and empirical evidence that these variables also affect the designation of protected areas by modeling the selection process directly using our data and a probit model (regressing a dummy variable for treatment on the covariates).

For the Costa Rica analysis, we control for the following covariates in the matching analysis:

Proportion of segment under forest cover in 1960 area: This is the earliest measure of forest cover prior to the establishment of protected areas. *"Road-less volume"*: Road-less volume is a metric that measures accessibility to transportation infrastructure (17). The road-less volume for a census segment is obtained by multiplying the distance from center of each 100 square meter plot in the segment to nearest major road in 1969, and then summing for all plots within the segment. *Land use capacity*: We use Costa Rica's *land use capacity classes*, which are determined by slope, soil characteristics, life zones (18), risk of flooding, dry period, fog, and wind influences (19). The classes are defined in table S1. *Distance to nearest major city*: This variable is a measure of proximity to large agricultural markets. Following a similar Costa Rican study (20), we use as proxies for access to agricultural markets the three major cities: Limon, Puntarenas, and San Jose. *Baseline poverty index in 1973*: This index is derived as described above in section entitled "Outcomes."

The choice of variables **for the Thailand analysis** draws on qualitative research into the history of the process of designation for protected areas in Thailand (16). Areas in Thailand were more likely to be protected if they were important for national watershed protection, were further from high quality agricultural land, were forested on historical land use maps, and were further from mineral and timber resources (16). All of these factors are also likely to affect socioeconomic outcomes. Control variables were therefore chosen that could best proxy for these factors. The sources of these data are described in table S2. Fixed geographic controls include: *average and maximum slope and elevation*, *distance to Thai national boundary*, *distance to navigable river, distance to mineral deposits*, *eco-region*, *average temperature and rainfall*, and *upper watershed status*. Pre-treatment characteristics include historical *forest cover (measured in 1973)*, *distance to major and minor roads in 1962*, *distance to railroad line*, and *distance to* *major city* (these major cities were established in the 1960's). Unfortunately, data on population or poverty measures at the subdistrict level are not available for earlier time periods in Thailand (these are available at the district level but would be redundant with district level fixed effects). Because Thailand was primarily an agricultural country throughout this period, forest cover serves as the best available control for prior level of development. To control for unobservable differences in political/institutional characteristics or initial regional development, we use exact matching at the district level.

Methods

In statistical jargon, the socioeconomic effects of protected areas that we attempt to measure are 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 and the analyst wishes to make as few parametric assumptions as possible about the underlying structural model that relates protection to the socioeconomic outcomes (e.g., the poverty index). 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 (21-23). If the researcher can select observable characteristics so that any two census communities with the same value for these characteristics will display homogenous responses to the treatment (i.e., protection is independent of outcomes for similar communities), 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 outcome when community *i* is protected, $Y_i(0)$ is the outcome when community *i* is unprotected, T is treatment (T=1 if protected), and X is the set of pretreatment characteristics on which communities are matched. This is called the conditional independence assumption and its implication is that, conditional on X, the outcomes and treatment are independent. In the context of our analyses, this assumption implies that, after conditioning on a set of observable characteristics, poverty outcomes are independent of protected area assignment (as would be the case if protected areas were randomly assigned across the landscape). For identification purposes, we also need one other assumption called the overlap assumption: $c < P(T = 1 \mid X = x) < 1 - c$ for $c > 0$. This assumption implies that the conditional distributions of the treated and control units overlap for the vector of covariates X. This assumption is required for identification, because if all communities with a given vector of covariates were protected, there would be no observations on similar unprotected communities.

The matching methods used in the analysis are described in the main text. Table S5 presents the **covariate balancing results for Costa Rica** when matching without calipers. The table includes three measures of the differences in the covariate distributions between protected and unprotected segments: the difference in means, measures of the distance between the two empirical quantile functions (values greater than 0 indicate deviations between the groups in some part of the empirical distribution), and the mean difference in the empirical cumulative distribution (to compare relative balance across the covariate dimensions). If matching is effective, these measures should move dramatically towards zero (24). The measures in the fifth to ninth columns indeed move dramatically towards zero after matching (we present the matching method that yields the best covariate balance). Covariate balance is even more improved when matching with calipers, particularly on the road-less volume where the difference in mean values falls to 59.5 km^3 (full balancing results available from authors).

Table S6 presents the **covariate balancing results for Thailand** when matching without calipers. As in the Costa Rican case, matching substantially improves the covariate balance on all covariates. To save space, balancing results with calipers are not shown; balance improves with calipers, as expected.

Table S7 presents the impact estimates from Fig 3 in the main text in a more explicit tabular format. Table S7 also presents impact estimates using the poverty gap as the measure of poverty in Thailand. The poverty gap weights the poverty headcount by the distance separating the population from the poverty line. It therefore represents a measure of the amount of resources (cash transfers) that would be needed to eradicate poverty.

In the main text, we calculate relative impact measures. For Costa Rica, the 1973 mean poverty index for the 249 treatment segments is 15.050. In 2000, it is -1.588. Dividing the estimated treatment effect by the change in poverty index (16.64) implies that 7.7% of the poverty reduction observed in treated segments is estimated to be attributable to protected areas. For Thailand, we simply divide the estimate of the impact (0.079) by the mean poverty headcount ratio in the matched control subdistricts (0.282); this change corresponds to 28.0% of the counterfactual poverty level.

Sensitivity to Hidden Bias. To determine how strongly an unmeasured confounding variable must affect selection into the treatment to undermine our conclusions, we use the bounds recommended by Rosenbaum (25). Although there are other sensitivity tests available (e.g., (26)), Rosenbaum's bounds are relatively free of parametric assumptions and provide a single, easily interpretable measure of the way in which the unobservable covariate enters.

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

The odds ratio between unit *j* which receives the treatment and the matched control outcome *k* is: $\frac{\pi_j(1-\pi_k)}{\pi_k(1-\pi_j)} = \exp\{\gamma(u_j - u_k)\}$. Because of the bounds on u_j, a given value of γ constrains the degree to which the difference between selection probabilities can be a result of hidden bias. Defining $\Gamma = e^{\gamma}$, setting $\gamma = 0$ and $\Gamma = 1$ implies that no hidden bias exists, and hence is equivalent to the conditional independence assumption underlying the matching method analysis. 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. We contrast outcomes using matched units from the analysis with and without calipers. The Rosenbaum bounds test is then used to test the difference between the paired outcomes.

Rosenbaum bounds compute bounds on the significance level of the matching estimate as $\Gamma = e^{\gamma}$ changes values. The intuitive interpretation of the statistic for different levels of Γ is that matched units 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 post-treatment poverty. A study is considered highly sensitive to hidden bias if the conclusions change for $\Gamma = e^{\gamma}$ just barely larger than 1, and insensitive if the conclusions change only for large values of $\Gamma = e^{\gamma} > 1$ (25). Note that the assumed unobserved covariate is a strong confounder: an unobserved covariate, or set of them, that is a near perfect predictor of protected areas' effects on poverty, is closely associated with the spatial assignment of protection, and is uncorrelated with the other covariates for which we control in the analysis. Showing that a result is sensitive at a given level of Γ does not mean that this strong confounder exists and that protection has no impact.

Robustness Checks

Tables S8 and S9 present the results of the tests of sensitivity to hidden bias. The upper half of table S8 presents the significance level (critical p-values) of the **Costa Rica estimates** as Γ increases. The upper halves of tables S9a and S9b present the significance levels of the **Thailand estimates** as Γ increases. In both the Costa Rica and Thailand contexts, the assumed powerful unobserved confounder would only have to be weakly associated with protection to render our estimates insignificantly different from zero.

We also examine the Γ at which the 95% confidence interval would include an effect of protection on poverty of a "moderate" effect size of 0.5 (27), but in the opposite direction (i.e., protection exacerbates poverty). In other words, we determine the levels of Γ at which the confidence interval would include a positive ATT with an effect size of 0.5. To estimate upper bounds on the confidence intervals as Γ increases, we calculate Rosenbaum bounds using the Wilcoxon test statistic, which can then be used to calculate confidence intervals as Γ increases

(28, 29). The lower half of table S8 indicates that, **for the matched Costa Rica sample** constructed without calipers, Γ would have to be as large as 3.4 for the confidence interval to include a value that implies a moderate exacerbation of poverty from protection. The Γ value would have to be as large as 4.7 for the Costa Rica sample constructed with calipers. **For the Thailand data**, the lower half of tables S9a and S9b indicates that Γ would have to be as large as 6.8 and 7.2 for the poverty headcount and poverty gap outcomes respectively when matching with no calipers (as large as 5.5 and 5.4 when matching with calipers). Thus only a very large amount of hidden bias could have caused us to estimate that protection had a small role in alleviating poverty when, in fact, protection may have had a moderate impact on exacerbating it. The omitted confounder would have to be one that increases the odds that a unit has more than 10% of its area protected by more than three-fold in Costa Rica and more than five-fold in Thailand.

The conclusions in the main text are also robust to alternative ways to control for imperfect matching, changes in the sample composition, changes in the matching specifications, and changes in the scale of the analysis. The estimates reported in the main text are our best estimates of the effects of protection on poverty. The robustness checks described below are not intended to increase the accuracy of our estimates, but rather to determine if alternative analyses would give estimates that would overturn our conclusions. We find they would not: under no robustness check do we draw the conclusion that protected areas exacerbated poverty.

Post-matching Regressions. Successful matching makes treatment effect estimates less dependent on the specific post-matching statistical model (24). In the main text, we use Abadie and Imbens' post-matching bias-correction procedure to adjust for imperfect matching (30). An alternative approach is to run post-matching Ordinary Least Squares (OLS) regressions on the matched samples. We report only the marginal effect estimates because hypothesis testing is not the purpose of this analysis, but rather to confirm Fig 1's estimates (Table S7) are robust to alternative model specifications.

Table S10 presents post-matching regression estimates **for Costa Rica**. We use a weighted OLS model of the poverty index outcome on the covariates. Each post-matching regression estimate thus corresponds to a matching estimate in Table S7. The post-matching regression estimates in the second column of table S10 are similar to the matching estimates in the main text.

We test model dependence further by running regressions using a modified set of covariates (i.e. we match on the core set and regress on elements of the modified set of variables). For the regression, we replace the *proportion of segment under forest in 1960* with the *area under forest in 1960*, replace the *proportion of the segment under each land use class* with the *area of the segment under each land use classes*, replace the *road-less volume* with the *distance to nearest road* (the distance from the centroid of the segment to a road in 1969), and we control for the *segment area* and *population density in 1973*. We report these estimates in the third column of table S10. We find that the post-matching regression estimates continue to differ little from those reported in table S7.

The Thailand post-matching regression results are in table S11. For example, in the first column and first row of table S11, we run a weighted OLS model of the poverty headcount ratio (2000) on the full set of covariates using the matched dataset from the matching procedure in the second column and first row of table S7. The post-matching regression estimates in table S11 are similar to the matching estimates in table S7. Including district fixed effects (table S11, second row) also produces similar estimates although they are somewhat smaller in terms of magnitude.

Population Effects. One rival explanation for our observed results is that protected areas displaced poor people to other segments or subdistricts, thereby making protection appear to alleviate poverty. To assess this rival explanation, we estimated the effect of protected areas on population. table S12 reports the estimated effects of protection on population density and growth rates in Costa Rica (growth rate is the population in 2000 minus the population in 1973, which is then divided by the population in 1973), and the Thailand results from matching for protection's impact on population density. All population estimates are small and statistically indistinguishable from zero (p>0.10), with the exception of the estimate on population growth for Costa Rica (matching with no calipers) which is not a robust estimate[†].

Other robustness checks.

Changing the scale of the unit of observation (Costa Rica): Instead of using the aggregated segment boundaries (1973 census boundaries), we use the disaggregated segment boundaries (2000 census boundaries). The difference in these two scales is described earlier in the Data section. Using the 2000 census boundaries as the unit of analysis, there are 483 treated units and 16,249 controls. The mean difference in 2000 poverty index between treated and all control segments is 6.732 (stand. err. = 0.238; p<0.01). The estimates based on matching are -2.390 without calipers (stand. err. = 0.442; p<0.01) with calipers and -1.611 with calipers (stand. err. = 0.359; p<0.01). Thirty-seven treated segments are dropped using the calipers. See table S13 for

[†] When we improve balance by matching with calipers, the estimate decreases by more than 80 percent, and is no longer significantly different from zero (p<0.10); a post-matching regression estimates reduces the estimate by more than 60 percent.

covariate balance, and table S14 for tests for hidden bias for this analysis. Table S14 indicates that the estimates of the impact of protection for these Costa Rica census data are slightly more robust to potential hidden bias than the estimates reported in the main text. The lower half of table S14 shows that for the confidence interval for the estimate from these data to include a finding of moderate size that protection exacerbates poverty, Γ must be greater than 5.1.

Including protected areas established later (Costa Rica and Thailand): We estimate the treatment effects of protected areas established before 2000. Table S15 presents the estimates.

Varying the threshold of protection (Costa Rica and Thailand): We vary the threshold criterion for defining a treated unit from 20% to 50%. Tables S16a and 16b present the matching estimates, including the 10% threshold estimates from Table S7 and Fig 1 for reference.

Testing for the presence of spillovers into control units (Costa Rica and Thailand): Households in census tracts or subdistricts that are close to treated units might also be positively or negatively affected by protected areas. If such spillovers are negative, they can make it appear as though protection alleviates poverty. If they are positive, they can mask some of the impact of protection in treated units because poverty was also alleviated in some control units as a result of protected areas. To explore these possibilities, we take two approaches. Both approaches assume that if spillovers exist, they are a decreasing function of distance from protected area boundaries (i.e., the closer a unit is to the protected area, the more affected it would be by the protected area). The first approach removes from the control group any units that could be contaminated by spillovers. We re-estimate the treatment effects after excluding all control units within 10 km of a protected area. The results, in the first two columns of Table S17 ("exclusion check"), are similar to those in Table S7. We then directly estimated local spillovers by matching control units located within 5 km of a protected area to control units farther away from protected areas.

This second approach ("estimation check") aims to directly measure spillovers by comparing outcomes in control units "close" to protected areas with matched control units "far" from protected areas: in other words, we take the sample of control units and redefine treatment as having a protected area located within a specified distance from the unit but not in the unit itself. These results yield small values that are not statistically different from zero (columns 3 and 4 of Table S17). Based on the signs of the estimates (and estimates using a 2 km buffer, not reported here), the results indicate that to the extent that socioeconomic spillovers to surrounding communities exist, these spillovers are positive; i.e., control units near protected areas experience reduced poverty as a result of their proximity to protected areas. Thus, if spillovers are present, they are likely biasing our estimates towards zero, making it harder to detect a poverty alleviation effect and implying our estimates may somewhat underestimate the poverty reduction impacts of protection. Testing hypotheses in a separate regression framework by including a spatial lag measuring distance to protected area yields a similar conclusion.

Changing the set of control units (Costa Rica and Thailand): We vary the rule that control units must have less than 1% of their area overlapping with protected areas (results available upon request). Inferences regarding effects of protection on poverty and population do not change with these changes in the rule.

Table S1. Descriptive statistics for Costa Rica dataset (N = 4691).

Name	**Description**	**Mean**	**Standard deviation**	**Minimum Maximum**
Proportion of segment under forest in 1960	Total forest area in the segment in 1960 divided by total area of the segment	0.160	0.284	0.000 1.000
High Productivity Land (proportion)	Percent of segment 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.	0.288	0.404	0.000 1.000
Medium Productivity Land (proportion)	Percent of segment area under the land class IV, measured in km^2 Class IV: Moderately suitable for agricultural production; permanent or semi-permanent crops such as fruit trees, sugar cane, coffee, ornamental plants, etc.	0.421	0.454	0.000 1.000
Medium-low Productivity Land (proportion)	Percent of segment 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	0.209	0.354	0.000 1.000
Low Productivity Land (proportion)	Percent of segment 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	0.078	0.219	0.000 1.000
Protected before 1980 (proportion)	Proportion of the segment area that was protected before 1980	0.024	0.114	0.000 1.000
Segment area (km^2)	Total land area of segment (in square km)	10713.088	32455.426	4.660 862683.313
Roadless 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	162.224	911.764	0.000 33970.148
Distance to city (km)	Distance from centroid of the segment to closest major city (Limon, Puntarenas, or San Jose), measured in km	38.276	39.529	0.029 214.906
Distance to road	Distance from centroid of the segment to nearest road (1969) measured in km	4.520	8.328	0.000 62.656
Poverty index in 1973	Multidimensional index of poverty derived from a linear combination of a set of key socioeconomic variables (see Outcome section for detailed	6.462	9.556	-16.133 28.855

	description and table S3 for list of variables)			
Poverty index in 2000	Multidimensional index of poverty derived from a linear combination of a set of key socioeconomic variables (see Outcome section for detailed description and table S3 for list of variables)	-6.469	5.193	-16.390 23.290
Population in 1973	Total number of residents in segment in 1973	398.904	133.912	28 1527
Population in 2000	Total number of residents in segment in 2000	811.291	1255.309	1.000 26169
Population Density in 1973 (Persons per km^2)	Total number of residents in segment in 1973 divided by segment area	4.630	8.169	0.000 64.721
Population Density in 2000 (Persons per km^2)	Total number of residents in segment in 2000 divided by segment area	3.957	5.720	0.000 44.072
Population growth 1973-2000	Difference in segment population in 2000 and segment population in 1973 divided by segment population in 1973	1.098	3.765	-0.981 85.838

Table S2. Summary statistics and data sources for Thailand dataset (N = 3671).

Name	Description	Source	Mean	Sd	Min	Max
Poverty headcount ratio	Share of population with estimated consumption below poverty line	Healy and Jitsuchon 2007; data is measured for the year 2000	0.215	0.130	0.000	0.953
Poverty gap	Mean distance estimated consumption to poverty line		0.049	0.038	0.000	0.397
Population density	People per square km		105.9	103.7	0.321	3778
Protected	Coded as 1 if more than 10% in National Park or Wildlife Sanctuary by 1985	IUCN World Database on Protected Areas	0.052	0.223	0	1
Avg. slope	Average slope of land (deg)	NIMA's Digital Terrain Elevation Data GTOPO30/ USGS Global GIS (1999)	1.018	2.042	0.000	14.33
Avg. elevation	Average elevation in meters		217.5	161.2	18.35	1159
Maximum slope	Maximum slope (deg.)		4.050	6.990	0.000	46.99
Max. elevation	Maximum elevation in meters		319.3	316.4	21.075	2435
Distance to major city	Distance to nearest major city (pop > 100,000)	ESRI World Cities (2000)	85.59	44.51	10.05	222.6
Distance to major road ('62)	Distance to major road in km, 1962	digitized East Asia Road Map, U.S. Map Service (1964); data from 1962	5.26	6.22	0.002	76.16
Distance to any road ('62)	Distance to minor road, 1962 road map in km		10.42	9.577	0.002	88.08
Forest cover ('73)	Percent forest cover, 1973 from TRFIC/NASA satellite data	Tropical Rain Forest Info. Center / NASA Landsat Multi Spectral Scanner (MSS)	0.194	0.315	0.000	1.000
Distance to rail line	Distance to rail line (km)	Vector Map Level 0 / USGS Global GIS (1997)	55.05	45.76	0.015	222.1
Distance to major river (km)	Distance to major river (km) (flow accumulation > 5000)	USGS EROS Data Center, Hydro 1k dataset	21.61	16.61	0.010	97.82
Near watershed boundary	less than 1 km from boundary major watershed		0.461	0.499	0	1
Dist. mineral deposits	Distance to mineral deposits (km)	Mineral Resource Data System (MRDS)/ USGS Global GIS	119.46	84.73	1.371	376.4
Distance Thai border	Distance to Thai border in km	Vector Map Level 0 / USGS Global GIS (1997)	91.62	52.36	0.062	218.9
Temperature	Avg. monthly temperature (˚C)	Ministry of Transp. of Thailand / Marc Souris (IRD)	25.37	1.448	18.07	27.85
Rainfall	Avg. monthly rainfall (mm)		1064	225.3	375.8	2308
Ecoregion 2	Percent tropical and sub-tropical coniferous forest	WWF Conservation Science Program / USGS Global GIS	0.004	0.048	0.000	1.000
Ecoregion 3	Percent tropical and sub-tropical dry broadleaf forest		0.728	0.408	0.000	1.000
Northeast	1 = northeast region	Thai NSO classification	0.667	0.471	0	1

Table S3. Variables used to calculate poverty indexes for 1973 and 2000 (Costa Rica).

Variable	Description
Male	Percentage of men in total population
Dependency ratio	Dependency ratio (children aged < 15 and people aged >65 divided by remainder of the population)
Employed	Percentage of people who are employed compared with the economically active population
Illiterate	Percentage of illiterate population aged 10+
Adult primary or no education	Percentage of adult population (18+) with educational attainment of primary level or no formal education
No toilet	Percentage of dwellings without toilet
No hot water	Percentage of dwellings without access to hot water
Use coal or wood	Percentage of households that cook with charcoal or wood
Dirt floor	Percentage of dwellings with dirt floor
House in bad conditions	Percentage of dwellings in bad condition
No washing machine	Percentage of dwellings without washing machine
No electricity	Percentage of dwellings without electricity
No telephone	Percentage of dwellings without telephone
No refrigerator	Percentage of dwellings without refrigerator
No water system	Percentage of dwellings without connection to private or public water system
No sewage	Percentage of dwellings without indoor plumbing
Crowding	Percentage of dwellings with 3 or more occupants per bedroom

Table S4. Number of protected areas in the analysis, by IUCN Category.

	Costa Rica		Thailand	
IUCN Category	***Pre-1980***	***1981-2000***	***Pre-1985***	***1986-2000***
Ia/Ib	4	3	15	16
II	17	8	22	33
III	0	0	0	0
IV	1	66	0	0
V	0	0	0	0
VI	17	25	0	0

Table S5. Covariate balance: Matching without calipers (Costa Rica).

Covariate	Sample	Mean Value Treated Segments	Mean Value Control Segments*	Diff in Mean Values	Mean eQQ Diff**	Median eQQ Diff**	Max eQQ Diff**	Mean eCDF Diff^
Poverty Index in 1973	Unmatched	15.050	5.377	9.673	9.687	9.774	19.258	0.304
	Matched	15.050	15.237	-0.187	1.640	1.541	3.523	0.073
Proportion of segment under forest in 1960	Unmatched	0.523	0.117	0.406	0.405	0.455	0.769	0.451
	Matched	0.523	0.488	0.035	0.035	0.033	0.087	0.036
High Productivity Land (proportion)	Unmatched	0.092	0.304	-0.211	0.212	0.000	0.871	0.191
	Matched	0.092	0.120	-0.027	0.030	0.012	0.155	0.079
Medium Productivity Land (proportion)	Unmatched	0.209	0.453	-0244	0.245	0.073	0.799	0.211
	Matched	0.209	0.200	0.009	0.026	0.014	0.149	0.036
Medium-Low Productivity Land (proportion)¤	Unmatched	0.233	0.196	0.037	0.102	0.050	0.368	0.130
	Matched	0.233	0.243	-0.010	0.034	0.021	0.135	0.046
Roadless Volume (km^3)	Unmatched	1113.100	66.820	1046.28	1035.300	156.440	28799.000	0.363
	Matched	1113.100	681.500	431.6	440.880	25.083	28799.000	0.687
Distance to City (km)	Unmatched	58.530	34.865	23.665	23.624	23.107	42.112	0.196
	Matched	58.530	57.563	0.967	5.281	3.923	24.942	0.036

¤ Low productivity land is the omitted category.
* Values for matched controls are weighted means.
** 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. The mean difference is reported for categorical productivity variables.
^ Mean eCDF= mean differences in empirical cumulative distribution functions

Table S6. Covariate Balance: Matching without calipers (Thailand).

Covariate	Sample	Mean Value Treated Sub-districts	Mean Value Control Sub-districts	Diff in Mean Values	Mean eQQ Diff**	Median eQQ Diff**	Max eQQ Diff**	Mean eCDF Diff^
Average slope (degrees)	Unmatched	5.089	0.793	4.296	4.278	4.517	6.600	0.561
	Matched	5.089	4.750	0.339	0.534	0.588	1.010	0.056
Average elevation (m)	Unmatched	514.790	201.120	313.670	311.750	301.170	525.850	0.390
	Matched	514.790	470.920	43.870	50.603	42.872	121.040	0.057
Maximum slope (degrees)	Unmatched	19.233	3.213	16.020	15.935	17.304	22.304	0.567
	Matched	19.233	16.092	3.141	3.066	2.766	16.105	0.091
Maximum elevation (m)	Unmatched	1006.600	281.400	725.200	722.000	781.070	1034.900	0.435
	Matched	1006.600	922.130	84.470	93.961	84.217	627.010	0.058
Distance to Navigable River (km)	Unmatched	30.335	21.126	9.209	9.120	9.375	20.042	0.135
	Matched	30.335	29.841	0.494	3.255	2.527	31.207	0.042
Percent forest cover (1973)	Unmatched	0.704	0.165	0.538	0.537	0.627	0.871	0.508
	Matched	0.704	0.632	0.072	0.077	0.016	0.262	0.055
Distance to major city (km)	Unmatched	98.355	84.880	13.475	16.838	17.747	40.015	0.116
	Matched	98.355	96.515	1.840	8.786	7.285	30.275	0.068
Dist. major road pre-treatment (1962) (km)	Unmatched	17.780	10.012	7.768	7.583	6.368	23.548	0.174
	Matched	17.780	17.031	0.749	1.788	1.341	24.275	0.033
Dist. to any road pre-treatment (1962) (km)	Unmatched	12.414	4.862	7.552	7.408	6.406	28.958	0.238
	Matched	12.414	13.102	-0.688	1.602	0.917	15.479	0.033
Distance to Thai national border (km)	Unmatched	79.535	92.281	-12.746	12.811	11.185	32.806	0.068
	Matched	79.535	82.776	-3.241	8.135	5.288	47.451	0.034
Near watershed boundary	Unmatched	0.823	0.441	0.382	0.380	0.000	1.000	0.191
	Matched	0.823	0.849	-0.026	0.025	0.000	1.000	0.013
Distance to railroad	Unmatched	72.946	54.064	18.882	23.009	22.669	76.803	0.152
	Matched	72.946	66.076	6.870	9.350	8.236	23.004	0.060
Distance to mineral deposits	Unmatched	80.312	121.620	-41.308	41.837	33.885	142.780	0.145
	Matched	80.312	69.733	10.579	10.833	9.591	41.031	0.067
Avg. temperature	Unmatched	23.358	25.481	-2.123	2.109	2.235	3.397	0.362
	Matched	23.358	23.627	-0.269	0.322	0.304	0.791	0.056
Avg. rainfall	Unmatched	1023.500	1066.200	-42.700	57.826	34.839	705.160	0.056
	Matched	1023.500	1001.900	21.600	25.365	15.344	371.200	0.033
Pct ecoregion 2	Unmatched	0.029	0.003	0.026	0.025	0.000	0.714	0.046
	Matched	0.029	0.011	0.018	0.018	0.000	0.585	0.033
Pct ecoregion 3	Unmatched	0.571	0.736	-0.165	0.164	0.000	0.633	0.172
	Matched	0.571	0.604	-0.033	0.053	0.000	0.272	0.053
Northeast region	Unmatched	0.396	0.682	-0.286	0.286	0.000	1.000	0.143
	Matched	0.396	0.396	0.000	0.000	0.000	0.000	0.000

** 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. The mean difference is reported for categorical productivity variables.

^ Mean eCDF= mean differences in empirical cumulative distribution functions

Table S7. Estimated impacts of protected areas on poverty in 2000

Outcome	*Poverty Index (Costa Rica)* ^	*Poverty Headcount Ratio (Thailand)* #	*Poverty Gap (Thailand)* #
	Conventional Estimates		
Difference in Means †	5.526***	0.019**	0.007**
	(0.315)	(0.010)	(0.003)
	Matching Estimates		
Matching ‡	-1.278**	-0.079***	-0.024***
	(0.524)	(0.015)	(0.005)
Matching with calipers ‡¤	-1.349***	-0.054***	-0.017***
[N treated dropped by calipers]	(0.480)	(0.011)	(0.004)
	[22]	[48]	[48]
N treated	249	192	192
(N available controls)	(4164)	(3479)	(3479)

^ Average treatment effect on the treated of more than 10% of the segment protected before 1980.
# Average treatment effect on the treated of more than 10% of the sub-district protected before 1985.
† A t-test of the difference in means between treated and control segments.
‡ For Costa Rica, Mahalanobis covariate matching is used. For Thailand, nearest-neighbor propensity score matching with exact matching on district is used. Robust standard errors are in parenthesis under estimate.
¤ Calipers restrict matches to units within 1 standard deviation of each covariate.
*** and ** indicate significance at 1% and 5% levels, respectively.

Table S8. Tests for sensitivity to hidden bias: Critical p-values and upper bound confidence intervals for matching estimates (Costa Rica).

	1	2
	Poverty Index (Matching without calipers)	**Poverty Index (Matching with calipers)**
Γ	***Critical p-values for treatment effects*** [†]	
1.1	0.210	0.044
1.2		0.139
	Upper bound 95% confidence interval	
3.4	3.309	
3.7		3.112

[†] Test of the null of zero effect.

Table S9a. Tests for sensitivity to hidden bias: Critical p-values and upper bound confidence intervals for matching estimates (Thailand: Poverty headcount outcome).

	1	**2**
	Matching without calipers	**Matching with calipers**
Γ	***Critical p-values for treatment effects*** †	
1.1	0.001	0.005
1.2	0.007	0.016
1.3	0.023	0.041
1.4	0.060	0.087
1.5	0.125	0.156
	Upper bound 95% confidence interval	
6.8	0.090	
5.5		0.089

† Test of the null of zero effect.

Table S9b. Tests for sensitivity to hidden bias: critical p-values and upper bound confidence intervals for matching estimates (Thailand: Poverty gap outcome).

	1	**2**
	Matching without calipers	**Matching with calipers**
Γ	***Critical p-values for treatment effects*** †	
1.1	0.000	0.001
1.2	0.002	0.005
1.3	0.006	0.016
1.4	0.020	0.038
1.5	0.048	0.077
1.6	0.099	0.136
1.7	0.174	
	Upper bound 95% confidence interval	
7.2	0.032	
5.4		0.031

† Test of the null of zero effect.

Table S10. Post-matching weighted regression estimates: Estimated impacts of protected areas on poverty in 2000 (Costa Rica).

	1	2
Outcome	**Poverty Index^ (Costa Rica)**	**Poverty Index¤ (Costa Rica)**
Matching without calipers‡ (N=498)	-0.815	-0.858
Matching with calipers‡¤ (N=454)	-1.126	-0.974

^ Regression on matched covariates only
¤ Regression on modified set of covariates (see full description in SOM text)

Table S11. Post-matching weighted regression estimates with matching covariates: Estimated impacts of protected areas (Thailand).

	1	2
Outcome	**Poverty Headcount Ratio** # **(Thailand)**	**Poverty Gap** # **(Thailand)**
Post-matching estimates (no district fixed effects) ***(N† =394)***	-0.064	-0.020
Post-matching estimates (with district fixed effects) ***(N† =394)***	-0.045	-0.014

Regression on matched covariates.
† N reflects the number of treated observations available for matching. There are three instances of ties; weights are used to correct for the fact that these three treated observations appear more than once in the matched data set

Table S12. Estimated impacts of protected areas on population in 2000.

	1	2	3
Outcome	**Population Density^ (Costa Rica)**	**Population Growth^ (Costa Rica)**	**Population Density # (Thailand)**
	Conventional Estimate		
Difference in Means†	-4.278***	0.443*	-68.724***
	(0.374)	(0.250)	(7.602)
	Matching Estimates		
Matching without calipers‡	-0.175	-1.865***	-0.573
	(0.125)	(0.676)	(3.920)
Matching with calipers‡▫	-0.173	-0.363	-1.240
	(0.124)	(0.315)	(3.104)
[N treated dropped by calipers]	[21]	[28]	[48]
N treated	249	249	192
(N available controls)	(4164)	(4164)	(3479)

^ Average treatment effect on the treated of more than 10% of the segment protected before 1980. Population density is calculated in persons per square km (population density = total population / segment area in km). Population growth is calculated as the relative change in population between 1973 and 2000 (Population growth = (Population in 2000 – Population in 1973)/Population in 1973).
# Average treatment effect on the treated of more than 10% of the sub-district protected by 1985.
† A t-test of the difference in means between treated and control segments.
‡ For Costa Rica, covariate matching on the Mahalanobis distance metric is used. For Thailand, nearest neighbor propensity score matching with exact matching on district is used. Robust standard errors are in parenthesis under estimate (Abadie & Imbens).
▫ For Costa Rica and Thailand, calipers restrict matches to units within 1 standard deviation of each covariate.
***, **, * indicate significance at 1%, 5%, 10% respectively.

Table S13. Covariate balance: Poverty index outcome, matching without calipers, using disaggregated segment boundaries (Costa Rica).

Covariate	**Sample**	**Mean Value Treated Segments**	**Mean Value Control Segments***	**Diff in Mean Values**	**Mean eQQ Diff****	**Median eQQ Diff****	**Max eQQ Diff****	**Mean eCDF Diff^**
Poverty Index in 1973	Unmatched	13.641	6.573	7.068	7.079	6.469	18.353	0.274
	Matched	13.641	13.962	-0.321	0.495	0.403	1.959	0.024
Proportion of segment under forest in 1960	Unmatched	0.512	0.138	0.374	0.373	0.377	0.866	0.379
	Matched	0.512	0.512	0.000	0.010	0.005	0.062	0.012
High Productivity Land (proportion)	Unmatched	0.079	0.345	-0.266	0.267	0.000	0.989	0.252
	Matched	0.079	0.102	-0.023	0.023	0.000	0.186	0.028
Medium Productivity Land (proportion)	Unmatched	0.170	0.413	-0.243	0.243	0.000	0.912	0.222
	Matched	0.170	0.170	0.000	0.013	0.000	0.103	0.017
Medium-Low Productivity Land (proportion)¤	Unmatched	0.223	0.194	0.029	0.089	0.002	0.378	0.110
	Matched	0.223	0.209	0.014	0.020	0.000	0.116	0.030
Roadless Volume (km^3)	Unmatched	441.99	22.838	419.152	416.35	64.081	19641	0.347
	Matched	441.99	285.21	156.78	157.30	15.748	19641	0.053
Distance to City (km)	Unmatched	58.048	35.490	22.558	22.510	21.750	48.079	0.175
	Matched	58.048	56.477	1.571	5.182	3.398	24.281	0.034

¤ Low productivity land is the omitted category.
* Values for matched controls are weighted means.
** 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. The mean difference is reported for categorical productivity variables.
^ Mean eCDF= mean differences in empirical cumulative distribution functions

Table S14. Tests for sensitivity to hidden bias: Critical p-values and upper bound confidence intervals for matching estimates using disaggregated segment boundaries (Costa Rica).

	1	2
	Poverty Index (Matching without calipers)	**Poverty Index (Matching with calipers)**
Γ	***Critical p-values for treatment effects*** †	
1.1	0.004	0.111
1.2	0.035	
1.3	0.146	
	Upper bound 95% confidence interval	
5.1		4.226
6	4.520	

† Test of the null of zero effect.

Table S15. Estimated impacts of protected areas on poverty: all areas protected before 2000.

	1	2	3
Outcome	**Poverty Index^ (Costa Rica)**	**Poverty Headcount Ratio # (Thailand)**	**Poverty Gap # (Thailand)**
	Conventional Estimate		
Difference in Means†	5.873***	0.018***	0.008***
	(0.257)	(0.006)	(0.002)
	Matching Estimates		
Matching without calipers‡	-1.000**	-0.051***	-0.013***
	(0.442)	(0.013)	(0.005)
Matching with calipers‡¤	-1.158***	-0.045***	-0.012***
	(0.405)	(0.010)	(0.004)
[N treated dropped by calipers]	[35]	[70]	[70]
N treated	393	471	471
(N available controls)	(4164)	(3479)	(3479)

^ Average treatment effect on the treated of more than 10% of the segment protected by 2000.
# Average treatment effect on the treated of more than 10% of the subdistrict protected by 2000.
† A t-test of the difference in means between treated and control segments.
‡ For Costa Rica, covariate matching on the Mahalanobis distance metric is used. For Thailand, nearest neighbor propensity score matching with exact matching on district is used. Robust standard errors (Abadie & Imbens) are in parenthesis under estimate.
¤ Calipers restrict matches to units within 1 standard deviation of each covariate.
*** and ** indicate significance at 1% and 5%, respectively.

Table S16a. Varying thresholds of protection for defining treatment: Estimated impacts of protected areas on poverty in 2000, matching[ǂ] without calipers.

	1	2	3
Outcome	**Poverty Index**[^] **(Costa Rica)**	**Poverty Headcount Ratio**[#] **(Thailand)**	**Poverty Gap**[#] **(Thailand)**
Treatment: More than 10% protected	-1.278**	-0.079***	-0.024***
	(0.524)	(0.015)	(0.005)
{N treated}	{249}	{192}	{192}
Treatment: More than 20% protected	-1.513***	-0.078***	-0.022***
	(0.535)	(0.018)	(0.007)
{N treated}	{209}	{156}	{156}
Treatment: More than 50% protected	-1.600**	-0.076***	-0.020**
	(0.642)	(0.024)	(0.008)
{N treated}	{82}	{61}	{61}

^ Average treatment effect on the treated of more than 10%, 20%, or 50% of the segment protected before 1980.
# Average treatment effect on the treated of more than 10%, 20%, or 50% of the sub-district protected by 1985.
ǂ For Costa Rica, covariate matching on the Mahalanobis distance metric is used. For Thailand, nearest neighbor propensity score matching with exact matching on district is used. Robust standard errors are in parenthesis under estimate (Abadie & Imbens).
*** and ** indicate significance at 1% and 5%, respectively.

Table S16b. Varying thresholds of protection for defining treatment: Estimated impacts of protected areas on poverty in 2000, matching‡ with calipers¤.

	1	2	3
Outcome	**Poverty Index^ (Costa Rica)**	**Poverty Headcount Ratio^ (Thailand)**	**Poverty Gap^ (Thailand)**
Treatment: More than 10% protected	-1.349***	-0.054***	-0.017***
	(0.480)	(0.011)	(0.004)
[N dropped by calipers]	[22]	[48]	[48]
{N treated}	{249}	{192}	{192}
Treatment: More than 20% protected	-1.705***	-0.058***	-0.018**
	(0.516)	(0.014)	(0.005)
[N dropped by calipers]	[20]	[42]	[42]
{N treated}	{209}	{156}	{156}
Treatment: More than 50% protected	-1.438**	-0.059***	-0.017**
	(0.578)	(0.017)	(0.007)
[N dropped by calipers]	[11]	[24]	[24]
{N treated}	{82}	{61}	{61}

^ Average treatment effect on the treated of more than 10%, 20%, or 50% of the segment protected before 1980.
# Average treatment effect on the treated of more than 10%, 20%, or 50% of the sub-district protected by 1985.
‡ For Costa Rica, covariate matching on the Mahalanobis distance metric is used. For Thailand, nearest neighbor propensity score matching with exact matching on district is used. Robust standard errors are in parenthesis under estimate (Abadie & Imbens).
¤ Calipers restrict matches to units within 1 standard deviation of each covariate.
*** and ** indicate significance at 1% and 5%, respectively.

Table S17. Robustness checks for spillover effects

	Exclusion Check (Costa Rica) **Treatment is more than 10% protected and controls must be more than 10-km from any protected area by 2000**	**Exclusion Check (Thailand)** **Treatment is more than 10% protected and controls must be more than 10-km from any protected area by 2000**	**Estimation Check (Costa Rica)** **Treated are units within 5-km of protection. All units with more than 1% protection before 2000 dropped**	**Estimation Check (Thailand)** **Treated are units within 5-km of protection. All units with more than 1% protection before 2000 dropped**
Matching †	-1.232***	-0.0910***	-0.019	-0.0319
	(0.442)	(0.0160)	(0.095)	(0.0246)
Matching with caliper †¤	-1.504***	-0.0549***	0.011	-0.0348
	(0.367)	(0.0113)	(0.092)	(0.0244)
[N treated dropped by calipers]	[34]	[60]	[21]	[1]
N treated	249	192	1546	53
(N available controls)	(678)	(3149)	(2618)	(3426)

^ Outcome is poverty index for Costa Rica and poverty headcount ratio for Thailand.
† For Costa Rica, Mahalanobis covariate matching is used. Robust standard errors are in parenthesis under estimate of average treatment effect. For Thailand, nearest-neighbor propensity score matching with exact matching on district is used. Robust standard errors are in parenthesis under estimate.
¤ Calipers restrict matches to units within 1 standard deviation of each covariate.
*** and ** indicate significance at 1% and 5% levels, respectively.

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