Supporting Information

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SI Text

Data

For full details on data, see Andam et al. (1) [see Andam et al. (2) for details on forest cover data]. Digital layers of protected areas (source: National System of Conservation Area Office, Ministry of Environment and Energy, 2006) were provided by the Earth Observation Systems Laboratory, University of Alberta. Other geographic information system (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). Socioeconomic data come 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. Summary statistics of the data are presented in Table S7.

Areal Interpolation. The number of census tracts in Costa Rica increased from 4,694 (mean area 8.82 km^2) in 1973 to 17,239 (mean area 2.95 km^2) in 2000. In addition, the division of census tracts over time was not necessarily guided by the borders of the original census tracts. We use the method of areal interpolation (3) to rectify the spatial differences in census tracts during our study period, thereby allowing comparison of socioeconomic data across time. Our final unit of analysis is defined by the 2000 census tract border. Therefore, areal interpolation assigns baseline values to 2000 census tracts based on the proportion of overlap with the 1973 census tracts, assuming uniform spatial distribution. As a robustness check, we run the primary analyses defining the unit of interest, using the 1973 census tract boundaries. Under this formulation areal interpolation assigns outcome values to these units based on the proportion of overlap with the 2000 census tracts, assuming uniform distribution.

Poverty Index. An asset-based poverty index based on Cavatassi et al. (4) acts as our outcome of interest (2000) and as a control covariate (1973). The index is created using a principal components analysis that measures the relative influence of variables believed to affect poverty on a latent outcome (poverty). Using the estimated influences of each variable, and the observed values of the variables in each census tract, we estimate the relative poverty by census tract.

The variables used in the poverty index estimation are as follows (* indicates a percentage): men in total population*, families who cook with coal or wood*, families without washing machine*, families without refrigerator*, people who are used and get a salary as job remuneration*, illiterate population aged 12 or more*, household dwellings without connection to private or public water system*, household dwellings without sewers*, household dwellings without electricity*, household dwellings without telephone*, dwellings with earth floor*, dwellings in bad condition*, dwellings without bathroom*, dwellings without access to hot water*, dependency ratio, average number of occupants per bedroom, and average years of education per adult.

Forest Cover. We use changes in forest cover between 1960 and 1986 as our proxy to capture ecosystem services other than tourism. These data come from Andam et al. (2) and are measured at a 3-ha resolution. Forest cover (or lack thereof) on each 3-ha parcel is defined by an 80% threshold. In other words, if a parcel contains at least 80% canopy cover in a given time period, then it is considered forested. However, if canopy cover is less than 80%, the parcel is not considered forested.

Tourism Mechanism. Without a direct measure of tourism services we choose the presence of a park entrance as a proxy/correlate. The GIS data on park entrances were provided by Robalino and Villalobos-Fiatt (5). Based on knowledge of the protected area system, we add Cabo Blanco and Guayabo national parks to the list of protected areas with an entrance. The addition of these two protected areas increases the number of census tracts affected by a protected area with an entrance from 205 to 227. However, the estimated mechanism average treatment effect on the treated (*MATT*) is not affected significantly by this addition ($MATT = -1.4$ without inclusion).

There are incomplete tourism visitation data for the last 10 y, but, for five reasons, we do not believe they can be used to estimate mechanism effects: (*i*) We have no baseline tourism for any of the parks and before 2000 the data are patchy (e.g., only four parks have data from the 1980s and none have data from 1970s); (*ii*) even in the last 10 y, there are some years with zero reported tourists, including for large protected areas that regularly receive tens of thousands of tourists annually; (*iii*) even if we had complete data, it is not clear which single year should be used or whether some average over the years would be better; (*iv*) many parks without entrances are not reported at all and thus may not be truly zeros; and (*v*) there is no reason to believe there is a one-to-one mapping between the raw number of tourists and poverty reduction (i.e., the kind of tourists and tourism matters and our data on the origin and activities of the tourists are even worse than raw numbers; in other words, estimating the marginal effect of adding one more tourist is not theoretically justified).

Migration. As noted by Andam et al. (1), one of the potential rival explanations for the finding that protected areas reduced poverty is migration. The authors are concerned that the emigration, from protected census tracts to unprotected census tracts, of individuals most adversely affected by protection could lead to the false conclusion that protected areas were beneficial to surrounding populations. However, the authors find no statistical evidence that protection had an impact on population density. Thus, if poor people were leaving protected census tracts they must have been replaced by wealthier immigrants [for full results see Andam et al. (1)].

Despite the lack of evidence that migration is driving the estimated average treatment effect on the treated (*ATT*), there may be patterns of migration systematically linked to our mechanisms that could be accounting for our estimated *MATT*s (and therefore the *ATT*). Perhaps the most plausible scenario relates to the opportunities from tourism afforded by protected areas. (We thank an anonymous reviewer for raising this concern.) If protected areas spur immigration of individuals with higher propensities to gain from the ecotourism industry, then the observed poverty effects might not be accruing to the existing community members. Therefore, the assertion that protected areas made the poor better off would be an inaccurate interpretation of the results. If tourism opportunities led to significant in-migration of those poised to benefit from the opportunities, then we would expect to see differentially greater population densities in census tracts that were affected by a park entrance. However, when we match census tracts that were affected by a park entrance to unprotected census tracts, we find no statistical difference in population densities ($ATT = 0.015$, $P > 0.8$). Therefore, only a more complex pattern of migration, where immigration of wealthier individuals was offset by emigration of poor individuals, could account for the observed poverty impacts in both the ATT and the $MATT$.

Mechanism Theory and Concepts

Setup. In the study from which we draw our data (1), the estimand of interest is the average treatment effect on the treated, ATT: the effect of protection on poverty in the census tracts that were protected. Estimation of the ATT is akin to asking the question, "What would outcomes for protected census tracts have been had they not been protected?" Thus, we estimate the $MATT$. To estimate the causal mechanisms through which protected areas have affected poverty, we use an augmented potential outcomes framework [we follow the framework and much of the notation of Flores and Flores-Lagunes (6)]. The ATT can be defined as

$$ATT = E[Y_i(1) - Y_i(0)|T_i = 1], \qquad \textbf{[S1]}$$

where $Y_i(1)$ and $Y_i(0)$ are the potential outcomes for each unit $i \in N$ (e.g., census tracts) under treatment ($T = 1$) and control conditions ($T = 0$), respectively. In words, the ATT estimates the difference between observed outcomes, for treated units, and counterfactual outcomes (what the outcome would have been for those treated units, had they not been treated). The fact that counterfactual outcomes [$Y_i(0)$ $|T = 1$ in this case] are unobservable requires one to invoke the following assumption:

Assumption 1.

$$E[Y_i(0)|X_i, T_i = 1] = E[Y_i(0)|X_i, T_i = 0],$$

which states that, on average, the observed outcomes of untreated units represent the counterfactual outcomes for treated units (i.e., had the treated units not been treated) with similar values of X.

Principal strata. Identification of posttreatment mechanism effects requires additional concepts and assumptions. Suppose S is a posttreatment mechanism that is measured at an intermediate period between administration of treatment and measurement of outcome. (Note that the three mechanisms of interest are denoted formally as S_j, where $j = 1, 2, 3$. For ease of exposition throughout the majority of this discussion, the subscript is omitted.) Because, by definition, S is affected by treatment, it is not unconditionally independent of treatment (this is true under random assignment of treatment as well) and thus must be handled in a manner similar to the outcome of interest (Y). Therefore, as with Y, S has two potential outcomes $S_i(1)$ and $S_i(0)$ for each i, depending on assignment to treatment or control, respectively. This simply states that because mechanisms are affected by treatment, with the exception of some special cases, the mechanism outcome for each unit is dependent on the administered treatment. The implications, within the potential outcomes framework, are that four potential outcomes must now be considered for each unit $(Y_i(1), Y_i(0), S_i(1), S_i(0))$.

There are now four compound potential outcomes of interest for each i: (*i*) $Y_i(1, S_i(1))$, the outcome when the unit is exposed to the treatment and the mechanism is affected by the treatment, which represents the total effect of treatment and is equivalent to $Y_i(1)$ in the traditional potential outcomes framework; (*ii*) $Y_i(1, S_i(0))$, the outcome when the unit is exposed to the treatment but the mechanism is not affected by the treatment (in other words, the outcome the unit would experience were we to expose it to the treatment but hold the value of the mechanism at its no-treatment value; in the language of Flores and Flores-Lagunes, the mechanism is "blocked"); (*iii*) $Y_i(0, S_i(0))$, the outcome when the unit is not exposed to the treatment and the mechanism is not affected, which is equivalent to $Y_i(0)$ in the traditional potential outcomes framework (i.e., posttreatment mechanism is not affected in absence of treatment); and (*iv*) $Y_i(0, S_i(1))$, the outcome when the unit is not exposed to the treatment but the mechanism is affected as it would be if the unit were treated. In general, only $Y_i(1, S_i(1))$ and $Y_i(0, S_i(0))$ are observed in practice, leaving $Y_i(1, S_i(0))$ and $Y_i(0, S_i(1))$ as counterfactuals that require estimation.

To help conceptualize the joint potential outcomes and identify the causal mechanism effect, we follow Flores and Flores-Lagunes (6) by using the principal strata framework developed by Frangakis and Rubin (7) [also Rubin (8) and Mealli and Rubin (9)]. Defining a principal stratum is similar to the concept of matching units (or groups of units) based on similar potential outcomes in a standard quasi-experimental setting. Two units from different treatments (e.g., protected and unprotected) share a principal stratum if they share potential mechanism outcomes [formally a principal stratum is defined where $\{S(0) = s_0, S(1) = s_1\}$].

Our primary estimand, the $MATT$, requires that we estimate outcomes for treated units, had the mechanism been blocked (not affected the outcome). To find suitable counterfactual mechanism values for treated units (i.e., to identify units from disparate treatments but similar principal strata) we need the assumption of conditional mechanism isolation:

Assumption 2.

$$E[S_i(0)|X_i, T_i = 1] = E[S_i(0)|X_i, T_i = 0].$$

In other words, we assume that, conditional on X, protection is not assigned based on expectations that tourism, infrastructure, or ecosystem services will be different under treatment and control conditions.

If covariates X jointly determine selection into treatment, outcomes, and mechanism outcomes, then *Assumptions 1* and *2* allow for the identification of mechanism effects. Upon cursory examination, this may seem untenable. However, when one considers that the primary purpose of X is to control for the nonrandom process of selection into treatment and that treatment directly affects mechanisms, these sequential assumptions seem more reasonable.

Estimands. Our estimands follow directly from the framework for mechanism average treatment effects ($MATE$) and net average treatment effects ($NATE$) developed by Flores and Flores and Flores-Lagunes (6). A principal stratum is defined as $\{S(0) = s_0, S(1) = s_1\}$, which states that units located within a common principal stratum would have similar mechanism outcomes [s_0 had they been in the control group ($S(0)$) or s_1 had they been treated ($S(1)$), independent of actual treatment received]. The $MATT$ can be written

$$MATT = E\{E[Y_i(1, S_i(1)) - Y_i(1, S_i(0))|S_i(0) = s_0, S_i(1) = s_1, X_i = x, T_i = 1]\}. \qquad \textbf{[S2]}$$

To estimate the $MATT$ one must ask, "What would outcomes for the treated have been, had they remained treated but had treatment not affected the mechanism?" Estimation of the MATT answers this question by eliminating all sources of variation in the outcome other than that which is due to a change in the mechanism (via blocking the effect of the mechanism on the outcome in the second term of **[S2]**). A similar estimand of interest is the net average treatment effect on the treated ($NATT$), which isolates the effect on outcomes due to a change in treatment,

$$NATT = E\{E[Y_i(1, S_i(0)) - Y_i(0, S_i(0))|S_i(0) = s_0, S_i(1) = s_1, X_i = x, T_i = 1]\}, \qquad \textbf{[S3]}$$

holding S at untreated levels. An advantage of the $MATT$ and the $NATT$ is that they decompose the ATT such that $ATT = MATT +$

NATT. This decomposition states that the average treatment effect on the treated is equal to the proportion of the of treatment effect that is due to a change in the mechanism (induced by treatment), the *MATT*, and the proportion that is due to other mechanisms or solely to the effect of treatment, the *NATT* (Fig. 1*B*). Therefore, once either the *MATT* or *NATT* is estimated, the complementary estimate falls out of the difference with the *ATT*. [Satisfaction of *Assumption 2* is necessary for this identity to hold. Morgan and Winship (10) outline conditions under which $T \to Y$ can be estimated using a set of mechanisms (e.g., the set of mechanisms is exhaustive and isolated). However, one can measure the partial effect of $T \to Y$ using a nonexhaustive set of mechanisms, S (i.e., $S \to Y$), which leads to an estimate of *MATT*. In conjunction with *Assumption 1*, under which unbiased estimates of the *ATT* can be estimated, the remaining difference between *MATT* and *ATT* can be attributed to the mechanisms not included in S. The full decomposition can be written as $ATT = E[Y(1,S(1)) - Y(1,S(0))|T=1] + E[Y(1,S(0)) - Y(0,S(0))|T=1]$, given principal strata $\{S(0)=s_0, S(1)=s_1\}$.]

Estimation Strategy

Estimation of either the *MATT* or *NATT* is confounded by the fact that $Y_i(1,S_i(0))$ is typically unobservable. [In the case where a subgroup of treated units for which treatment did not affect mechanism values can be identified, Flores and Flores-Lagunes (6) develop an estimand for the local net average treatment effect (*LNATE*), which requires less restrictive assumptions. See *SI Text*, *Robustness* for an application of this methodology to our data.] We use matching in the first stage of the estimation to satisfy *Assumptions 1* and *2* (Fig. 1*B*). Postmatching, we follow methods suggested by Flores and Flores-Lagunes (6). We use mechanism data from the matched control units, and a simple assumption about the way in which mechanisms affect outcomes within principal strata, to impute outcomes for treated units had treatment not affected the mechanism variables: the counterfactual of interest.

First Stage: Matching. We use one-to-one Mahalanobis covariate matching with replacement and postmatch bias adjustment (11, 12) to match control units to treated units. This approach serves two purposes. First, it provides an estimate of the *ATT*, for comparison with the *MATT* and *NATT*, which offers comparability to previous studies from Costa Rica (1, 13, 14). Second, it provides a set of matched controls that, by *Assumptions 1* and *2*, are within the same principal strata as the treated units to which they are matched. The latter purpose implies that the mechanism outcomes of the matched controls can be assumed to be the value that would have been observed by their treated counterparts, had treatment not affected the mechanisms. See Table S7 for a description of the covariates used for matching [also see Andam et al. (1) for complete details].

Second Stage: Estimate the Influence of Mechanisms. Flores and Flores-Lagunes (6) suggest using a form of regression adjustment to impute outcomes for treated units had treatment not affected mechanisms. The necessary assumption for this approach (in addition to *Assumptions 1* and *2*) is that the mechanism has a similar effect on potential outcomes $Y_i(1,S_i(1))$ and $Y_i(1,S_i(0))$; i.e., their conditional expectation functions share the same functional form (6).

Assumption 3. *Suppose*

$$E[Y_i(1,S_i(1))|S_i(1),X_i=x,T_i=1] = a_1 + b_1 S_i(1) + c_1 X_i, \qquad \textbf{[S4]}$$

then

$$E[Y_i(1,S_i(0))|S_i(1),X_i=x,T_i=1] = \overbrace{a_1 + b_1 \underbrace{S_i(0)}_{\substack{\text{Observed}\\\text{matched}\\\text{control}\\\text{values}}} + c_1 X_i}^{\text{Coefficients from [S4]}}. \qquad \textbf{[S5]}$$

Assumption 3 implies that the marginal effect of a change in the mechanism outcome has the same effect, on average, on units for whom exposure to treatment affects the mechanism as it does on units for whom exposure to treatment does not affect the mechanism.

In **[S4]** and **[S5]** of *Assumption 3*, b_1 represents the effect on the outcome due to a change in the value of the mechanism S. The counterfactual of interest $(\hat{Y}(1,\hat{S}(0)))$ can be estimated by evaluating **[S5]**, which uses the coefficients from **[S4]**, setting $S_i(0) = E[S_i(0)|T=1] = [\widehat{S}_i(0)|T=1]$, which, according to Eq. **S3**, is equal to the observed control mechanism values within the common principal stratum of each treated unit.

Empirical estimation of the counterfactual of interest $(\widehat{Y}_i(1,\widehat{S}_i(0)))$ is conducted by first running a regression of observed outcomes on covariate and mechanism values for treated units as in **[S4]**. Using the coefficients from this regression (a_1, b_1, c_1), we impute $\widehat{Y}_i(1,\widehat{S}_i(0))$, using the same treated unit covariates (as in **[S4]**) and the matched control unit mechanism outcomes [where in **[S5]** $S_i(0) = E[S_i(0)|T=1] = S_i^{obs}(0)$ and $S_i^{obs}(0)$ is the observed mechanism outcome of each treated unit's respective matched control]. Replacing the first term in **[S2]**, the empirical form for the *MATT* becomes

$$MATT = \underbrace{E\{E[Y_i^{obs}(1)|S_i^{obs}(1)=s_1, X_i=x, T=1]\}}_{\text{Observed Protected Values}} - \underbrace{E[f_1(S_i(0),X_i)]}_{\text{Estimated in [S5]}}. \qquad \textbf{[S6]}$$

Similarly, the empirical form of the *NATT* becomes

$$NATT = \underbrace{E[f_1(S_i(0),X_i)]}_{\text{Estimated in [S5]}} - \underbrace{E\{E[Y_i^{obs}(0)|S_i^{obs}(0)=s_0, X_i=x, T=1]\}}_{\text{Observed Unprotected Values}}, \qquad \textbf{[S7]}$$

where $f_i(S_i(0),X_i)$ in **[S6]** and **[S7]** is equal to $E[Y_i(1,S_i(0))|S_i(1),X_i=x,T=1]$ from **[S5]**.

We again emphasize the intuition behind the counterfactual of interest, which can be used in the estimation of both the *MATT* and *NATT*. The regression imputation methods presented in **[S4]** and **[S5]** allow us to address the question, "What would the outcomes for treated units have been had their respective covariates $(X_i^{obs}|T=1)$ and influences of these covariates on outcomes (b_1) remained the same, but their mechanism had taken on the values that would have been observed had they not been treated $(S(0)|T=1)$?" We note that the difference between $S_i^{obs}(1)|T=1$ (the observed mechanism value of treated units) and $\hat{S}_i(0)|T=1$ (the estimated counterfactual values of treated units, had they not been treated) represents the unit-level causal effect of treatment on mechanism outcomes $(T \to S)$.

Bias-Adjusted Mechanism Outcomes. Abadie and Imbens (12) and Abadie et al. (11) suggest the use of postmatch regression bias adjustments in the estimation of the *ATT* to control for bias that remains from imperfect matching in finite samples. We apply a similar method in the estimation of our counterfactual mechanism outcomes.

Postmatch bias adjustment in estimation of the *ATT* is conducted by first running a regression of outcomes on matching covariates $Y_{T=0} = X_{T=0}\beta_C + \epsilon$. This regression estimates the impact (β_C) of the matching covariates on outcomes for the matched control sample. To impute the ATT counterfactual of interest, β_C is combined with the covariates from the treated units $X_{T=1}$ to estimate $\hat{Y}_{BA} = X_{T=1}\beta_C$: what treated unit outcomes would have been had their matching covariates had the same influence on outcomes as the control units. Note that if matching produces perfect balance across treated and matched control units, then a counterfactual based on the observed values of the matched control outcomes $(Y_{i:T=0})$ will be identical to those estimated from the regression bias adjustment procedure $(\hat{Y}_{i:BA})$.

The estimation of our counterfactual of interest in [**S6**] is a function of both b_1 from [**S4**] and $\hat{S}_i(0)\ |T=1$. By *Assumption 2*, we can use the mechanism outcomes of the matched controls as the counterfactual for treated units. However, if imbalance in the baseline mechanism covariates remains after matching, we may be concerned that counterfactual mechanism values will be biased. [If mechanism outcomes are state dependent, then imbalance is a concern. For instance, if, after matching, unprotected tracts have lower baseline roadless volume, on average, than protected tracts, change in roadless volume may be less (in absolute terms) in unprotected tracts, simply because they started with larger road networks.] We therefore estimate our counterfactual mechanism values

$$\left[\hat{S}_i(0)|T=1\right] = S^{obs}_{i:T=0} + \hat{\mu}_0(X_{i:T=1}) - \hat{\mu}_0(X_{i:T=0}), \qquad \textbf{[S8]}$$

where $\hat{\mu}_0$ represents the predicted values obtained from combining the coefficients from a control group regression of mechanism outcomes on covariates with the respective treated $(\hat{\mu}_0(X_{i:T=1}))$ or control group $(\hat{\mu}_0(X_{i:T=0}))$ covariates. This procedure estimates the influence of baseline covariates on mechanism outcomes for control units and uses these estimated values to impute what the mechanism outcomes would have been had the control units been treated.

SEs. To calculate the precision of our $MATT$ estimates we base our SE estimator on the heteroskedasticity robust matching-based estimator suggested by Abadie and Imbens (12). (A function that estimates the SEs outlined in this section was programmed in R 2.15.1 and is available from the authors upon request.) Our estimator is calculated in two stages to allow for heteroskedastic variances within and across treatment arms. The variance for control units is calculated using a within-treatment arm matching estimator. The Mahalanobis weighting matrix from the original matching process (used to create the matched sample) is used to find the nearest within-treatment arm (unprotected) neighbor to estimate unit-level variances,

$$\hat{\sigma}^2_{i:T=0}(X_i) = (Y_i - Y_l)^2/2, \qquad \textbf{[S9]}$$

where Y_l represents the outcome of the nearest neighbor to unit i. The treatment-level variance is then calculated as

$$\hat{V}_{T=0}\left(\widehat{MATT}\right) = \sum_{N_{T=0}} \lambda_i^2 \cdot \hat{\sigma}_i^2(X_i), \qquad \textbf{[S10]}$$

where $\lambda_i = \#C_i/N_{T=0}$, and $\#C_i$ is the number of times that control unit i occurs in the set (was used as a match in the original matching specification).

The unit-level variance for protected units is based on unit-level deviations from the estimated $MATT$:

$$\hat{\sigma}^2_{i:T=1}(X_i) = \left(Y_i - \hat{Y}_i\left(1, \hat{S}(0)\right) - \widehat{MATT}\right)^2. \qquad \textbf{[S11]}$$

These unit-level variances are then aggregated to calculate the treatment-level (protected) variance:

$$\hat{V}_{T=1}\left(\widehat{MATT}\right) = \frac{1}{N^2_{T=1}} \sum_{N_{T=1}} \hat{\sigma}^2_{i:T=1}(X_i). \qquad \textbf{[S12]}$$

The final $MATT$ SE estimate is therefore

$$\hat{\sigma}\left(\widehat{MATT}\right) = \sqrt{\left(\hat{V}_{T=0} + \hat{V}_{T=1}\right)}.$$

Empirical Estimation of *MATT*

We conduct two distinct analyses to estimate the $MATT$ of our mechanisms of interest. First, the mechanisms are considered separately and the procedure outlined in the preceding sections is performed for each mechanism. Second, the mechanisms are considered jointly in the estimation of each $MATT$ via inclusion of all mechanisms in [**S4**] and [**S5**]. All results presented in the main text stem from the joint estimation with mechanism bias adjustment (*Bias-Adjusted Mechanism Outcomes* subsection).

We begin by matching protected and unprotected census tracts, using one-to-one Mahalanobis covariate matching with replacement. The resulting matched set [identical to the sample used by Andam et al. (1)] comprises 483 protected and 489 unprotected tracts (six ties during the first-stage matching); the covariate balance is shown in Table S8. Using postmatch regression bias adjustment, the estimated ATT is −2.39, according to the poverty index. This result indicates that census tracts with at least 10% of their area occupied by a protected area before 1980 had differentially greater levels of poverty reduction (lower poverty index scores) between 1973 and 2000, on average, than comparable census tracts that remained unaffected by protected areas [see Andam et al. (1) for full details].

Counterfactual Mechanism Values. The counterfactual of interest necessitates estimation of mechanism outcomes for treated units, had protection not affected the mechanism. For each mechanism, estimation of the counterfactual entails a two-step process. First, we estimate a matched unprotected group regression

$$S_{i:T=0} = X_{i:T=0}\beta_{1C} + \epsilon, \qquad \textbf{[S13]}$$

where $S_{i:T=0}$ and $X_{i:T=0}$ represent the observed mechanism and baseline covariate values, respectively, of matched unprotected census tracts. The coefficients from [**S13**] are then used to impute counterfactual mechanism outcomes for each mechanism,

$$\left[\hat{S}_i(0)|T=1\right] = X_{i:T=1}\hat{\beta}_{1C}, \qquad \textbf{[S14]}$$

where $X_{i:T=1}$ are the observed covariate values of the protected census tracts (empirical analog to Eq. **S8**). Observed and counterfactual mechanism values for the protected census tracts are shown in Table S2. The imputed counterfactual mechanism values from [**S14**] are then used to calculate the counterfactual of interest: the outcomes for protected units, had protection not affected mechanisms $(\hat{Y}_i(1, \hat{S}_i(0)))$.

Columns *i* and *ii* of Table S2 list the observed and estimated counterfactual mechanism values for the protected census tracts (see Table S9 for estimates of counterfactual mechanism values when bias adjustment is not implemented). The counterfactual for our proxy for tourism is straightforward. Of the 227 census tracts that were impacted by a protected area with a park entrance, none would have a park entrance in the absence of protection. The estimated counterfactual for change in forest cover is significantly different from observed values as well. The average deforestation in protected census tracts between 1960 and 1986 was only 5.76%. We estimate that, had protection not affected deforestation, deforestation would have been ∼16.5%

(i.e., the combination of avoided deforestation and additional reforestation between 1960 and 1986 due to the establishment of protected areas was ~10.8%). Finally, we observe that there was greater infrastructure development (greater reduction in roadless volume) in protected census tracts between 1969 and 1991. However, the counterfactual measures of road networks are not significantly different from observed values.

Single-Mechanism Estimation. In the single-mechanism estimation the following procedure is run on each mechanism of interest independently. We first estimate the influence of covariates and mechanism on outcomes, using the protected census tracts

$$Y_{i:T=1} = X_{i:T=1}\beta_{1T} + S_{i:T=1}\beta_{2T} + \epsilon, \quad \textbf{[S15]}$$

where $Y_{i:T=1}$, $X_{i:T=1}$, and $S_{i:T=1}$ are the observed outcomes, matching covariates, and mechanism values for the protected census tracts, respectively. The counterfactual of interest is then estimated by obtaining the fitted values from

$$\tilde{Y}_{i:T=1} = X_{i:T=1}\hat{\beta}_{1T} + \hat{S}_i\hat{\beta}_{2T}, \quad \textbf{[S16]}$$

where $\hat{S}_i = [\hat{S}_i(0)\ |T=1]$ are the counterfactual mechanism values from **[S14]**, and thus $\tilde{Y}_{i:T=1} = \hat{Y}_i(1, S_i(0))$. *MATT* for each mechanism is calculated by subtracting the mean of the fitted values $(\tilde{Y}_{i:T=1})$ from mean of the observed protected tract outcomes $(Y_i(1, S(1)) = Y_i(1))$.

Joint-Mechanism Estimation. In the single-mechanism estimation strategy each mechanism is considered independently. However, the estimated impact (according to $\hat{\beta}_{2T}$) of a particular mechanism may be influenced by the inclusion or exclusion of additional mechanisms in [**S15**]. By including all of the mechanism variables in [**S15**] we allow the coefficients for each mechanism to reflect the presence of the other mechanisms. We therefore use the joint-mechanism estimation to generate our primary results. We note that because there is little correlation between our mechanism variables (Table S6), the results from single- and joint-mechanism estimations are nearly identical. For this reason we report only the results from the joint-mechanism analysis in the main text. For clarity we denote the park entrance, change in roadless volume, and forest cover mechanism variables as *E*, *R*, and *F*, respectively. The joint-mechanism estimation analog to [**S15**] is

$$Y_{i:T=1} = X_{i:T=1}\beta_{1T} + E_{i:T=1}\beta_{2T} + R_{i:T=1}\beta_{3T} + F_{i:T=1}\beta_{4T} + \epsilon, \quad \textbf{[S17]}$$

where all variables represent the observed values from the protected census tracts. The counterfactual of interest for each mechanism is estimated in a series of three imputations,

$$\tilde{Y}^{E}_{i:T=1} = X_{i:T=1}\hat{\beta}_{1T} + \underbrace{\hat{E}_i\hat{\beta}_{2T}}_{\text{Counter-factual}} + R_i\hat{\beta}_{3T} + F_i\hat{\beta}_{4T} \quad \textbf{[S18]}$$

$$\tilde{Y}^{R}_{i:T=1} = X_{i:T=1}\hat{\beta}_{1T} + E_i\hat{\beta}_{2T} + \underbrace{\hat{R}_i\hat{\beta}_{3T}}_{\text{Counter-factual}} + F_i\hat{\beta}_{4T} \quad \textbf{[S19]}$$

$$\tilde{Y}^{F}_{i:T=1} = X_{i:T=1}\hat{\beta}_{1T} + E_i\hat{\beta}_{2T} + R_i\hat{\beta}_{3T} + \underbrace{\hat{F}_i\hat{\beta}_{4T}}_{\text{Counter-factual}}, \quad \textbf{[S20]}$$

where $\widehat{E}_i$, $\hat{R}_i$, and $\hat{F}_i$ represent the imputed mechanism values from **[S14]** (i.e., $[\hat{S}_i(0)|T=1]$ for the respective mechanisms). Eqs. **S18–S20** show that the counterfactual of interest for each mechanism is estimated by substituting the imputed mechanism values (from **[S14]** for the mechanism of interest) into the respective equation, while leaving the covariates and complementary mechanism values at observed levels. (A function that performs this iterative process was written in R 2.15.1 and is available from the authors upon request.) For instance, the counterfactual of interest for change in roadless volume $(\tilde{Y}^{R}_{i:T=1})$ is calculated by plugging in the imputed counterfactual values for change in roadless volume $(\hat{R}_i)$ into the coefficients from **[S17]**, while leaving covariates $(X_{i:T=1})$ and mechanism values for park entrances (E_i) and change in forest cover (F_i) at the observed levels of protected census tract.

Results for the joint-mechanism estimation strategy are shown in columns *v* and *vi* of Table S2. We find that inclusion of all mechanisms in [**S15**] does change the estimated influence of each mechanism (compare with column *iii* of Table S2): The coefficient on the park entrance mechanism increases in absolute terms from −2.75 to −3.636 (indicating a relative increase in poverty reduction attributable to tourism), the coefficient on the roadless volume mechanism increases from 0.00098 to 0.0011 (indicating a relative increase in poverty reduction attributable to infrastructure development), and the coefficient on the forest cover mechanism increases from 2.139 to 4.873 (indicating a relative increase in poverty exacerbation attributable to forest cover changes).

Under the joint-mechanism estimation we find that the *MATT* for the park entrance mechanism increases, in absolute terms, to −1.738. This result implies that tourism associated with the establishment of protected areas accounts for approximately half of the estimated poverty reduction due to protection. Joint estimation also affects the *MATT* for the deforestation mechanism, which increases to 0.422. In other words, changes in forest cover from the establishment of protected areas have little impact on poverty (not statistically significant). Joint-mechanism estimation has a trivial effect on the *MATT* of roadless volume, which increases, in absolute terms, to −0.081.

Summary of Primary Identification Strategy

Given the number of specifications and the relative complexity of our econometric strategy, we now offer a descriptive exposition of our primary specification to highlight the intuition underlying the preceding sections. Each of the following subsections represents a stage in our overall strategy to identify the causal mechanism impacts. It is important to recall that the overarching goal of our study is to expand the analysis of Andam et al. (1) by elucidating and quantifying three potential channels through which Costa Rica's protected areas that were established before 1980 affected poverty in surrounding communities between 1973 and 2000.

Setup: Matching and Estimating the Average Treatment Effect on the Treated. Like Andam et al. (1) we begin our analysis by using matching methods to find comparable census tracts that were never affected by protection for the 493 census tracts that have at least 10% of their area covered by a protected area (established before 1980). We use the same one-to-one Mahalanobis matching as Andam et al. (1). It is important to note that the fundamental goal of matching is to control for observable differences in pretreatment confounding variables (covariates) by creating a matched set of treated (protected) and control (unprotected) units (census tracts) that have similar distributions across the key covariates (balance). By creating balance across covariates, matching ensures that the remaining variation in outcome is due solely to variation in treatment status. Andam et al. (1) choose the Mahalanobis weighting method because it provides the best balance (compared with alternative specifications) across baseline measures of poverty, agricultural suitability, infrastructure (roadless volume), and forest cover.

Using the matched set, we first estimate the average treatment effect on the treated (ATT). Estimating the ATT is akin to asking the question, "What would poverty in protected census tracts have been had protected areas not been established?" In our study [and in Andam et al. (1)], estimating the ATT is more appropriate than estimating the average treatment effect (ATE) (which is a weighted average of the ATT and the average treatment effect on the controls, ATC) because there are many census tracts throughout Costa Rica where protected areas would never be established. Therefore, estimating the effect of protection in such areas is inappropriate.

Following Andam et al. (1) we use postmatch regression bias adjustment (12) to calculate the $ATT = -2.39$. This estimate implies that average poverty index values would have been 2.39 points higher (i.e., greater poverty) in protected census tracts had the protected areas not been established. This translates to an effect size of 0.27, which is moderate.

Estimating the Impact of Protection on Mechanisms. To decompose the ATT into its constituent mechanism effects we first must estimate the impact that protection had on the respective mechanisms: (*i*) the establishment of a park entrance, (*ii*) change in roadless volume (1960–1991), and (*iii*) change in forest cover (1969–1986). Fundamentally, the impact that protection had on each mechanism is quantified by how much the mechanism values changed in protected census tracts due to the establishment of protected areas. Although we observe values for each of the mechanisms in the protected census tracts, we cannot observe what these values would have been had protected areas not been established. We estimate this counterfactual mechanism value for each protected census tract, using the observed mechanism value from the unprotected census tract to which each protected census tract is matched. The underlying assumption (*Assumption 2*) is that, after matching, the observed mechanism values of the matched unprotected census tracts, on average, represent the mechanism values that the protected census tracts would have experienced, had protected areas not been established. Thus, the difference between the observed mechanism values from the protected census tracts and their matched counterparts represents the causal impact of protected areas on the respective mechanisms (this is the difference between columns *i* and *ii* in Table S2).

Estimating the Marginal Effect of Mechanisms on Poverty. The previous step tells us how protection affected mechanisms. To complete the causal link between protection and poverty (through the mechanisms) we must understand how the mechanisms affect poverty. We use a regression-based method suggested by Flores and Flores-Lagunes (6) to estimate the impact of mechanisms on poverty. We begin by running a regression of poverty on all three mechanisms and all of the covariates used in the matching process, using only the protected census tracts. The key insight from this regression is that it provides the marginal effect of each mechanism on poverty for protected census tracts. In other words, the estimated coefficients on the mechanisms express by how much expected poverty would change in protected census tracts when the value of a mechanism changes by 1. We use these estimated coefficients to estimate what poverty in protected census tracts would have been had protected areas not affected the mechanisms (e.g., had no park entrances been established) by plugging the counterfactual mechanism values (from the previous step) into the estimated coefficients and repeating the process for each mechanism. For example, to estimate what poverty would have been in protected census tracts had protection not affected the establishment of roads, we plug the observed values of covariates, entrances, and change in forest cover back into the estimated coefficients along with the estimated counterfactual values for roadless volume. The fitted values from this process represent the level of poverty that would have been observed in protected census tracts had protection not affected the establishment of roads. This process is repeated for each mechanism.

Estimating the Mechanism Average Treatment Effect on the Treated. From the previous step we have an estimate of what poverty in protected census tracts would have been in the absence of the respective mechanisms. Thus, each of these estimates contains all of the impacts of protected areas on poverty, net the effects of the mechanism of interest (this is the key counterfactual). Therefore, we can estimate the impact of the mechanism itself by taking the difference between the ATT (which contains all of the impacts of protected areas on poverty) and the key counterfactual (which contains all of the impacts excepting the impact of the mechanism). The resulting difference is attributable to the impact of protected areas on poverty through the mechanism: the $MATT$. The difference between the ATT and the key counterfactual is calculated for each of the mechanisms to estimate the $MATT$ for each mechanism.

Robustness

Local *NATT*. The estimation of the $MATT$ and $NATT$ requires the imputation of counterfactual mechanism values that are, by definition, unobserved. Our data provide a unique opportunity to estimate the causal effects of the protection net of tourism under less restrictive assumptions than those used in the main analyses. We exploit the fact that some protected census tracts are observed in the absence of a park entrance mechanism. For this subset of the data $S_i(1)=S_i(0)$ by definition. In other words, the potential park entrance mechanism value for protected units that did not receive an entrance is the same under protection as it would have been in the absence of protection $(S_i(1)=S_i(0)=s_0)$. Therefore, we can identify this principal stratum $(\{S_i(1)=S_i(0)=s_0\})$ without invoking *Assumption 2*. In addition, we observe $Y_i(1,S_i(0))$ for this subset of the data and, therefore, do not need *Assumption 3* to impute the counterfactual of interest.

The local $NATT$ ($LNATT$) can be estimated [this framework follows directly from the framework for the $LNATE$ established by Flores and Flores-Lagunes (6)] as

$$LNATT = E\{E[Y_i(1,S_i(0)) - Y_i(0,S_i(0))|S_i(1)=S_i(0)=s_0]\}, \quad \textbf{[S21]}$$

for the subset of data in the principal stratum $\{S_i(1)=S_i(0)=s_0\}$ (6). The fact that we observe protected census tracts that were not affected by a park entrance means that we can take the simple difference in these protected tract outcomes $(Y_i(1,S_i(0)))$ and their matched controls $(Y_i(0,S_i(0)))$, both of which are observed in the data. We estimate the $LNATT$ for this subgroup to be −0.253. Flores and Flores-Lagunes (6) note that the $LNATT$ represents the local ATT ($LATT$) for this subgroup because there is no mechanism effect for this group, so $Y_i(1,(S_i(0))=Y_i(1)$. Therefore, under *Assumption 1*, $LNATT=LATT=E[Y_i(1)-Y_i(0)|X_i]$ for this subgroup.

We can make comparisons to the $MATT$ estimates, using the estimated $LNATT$ and an additional assumption of constant unit net effects (6):

Assumption 4.

$$Y_i(1,S_i(0)) - Y_i(0,S_i(0)) = C, \quad \textit{for all } i.$$

Under this assumption we can define $LNATT=NATT$ and therefore estimate $MATT=ATT-LNATT$. Using this framework, the estimate of park entrance $MATT$ (−2.14) is close to the estimate from the main analysis (−1.74). We believe that the similarity between the two estimates provides evidence that the assumptions and methods used in the main analyses are providing unbiased estimates of the respective mechanism effects.

Mechanism Analysis Using 1973 Census Tract Boundaries. As noted above, the number of census tracts in Costa Rica has changed over time. We choose to use the 2000 census boundaries because they offer comparability to previous results and greater precision on our mechanism estimates. However, to ensure that our results are not driven by our choice of unit boundary, we run all mechanism analyses using the 1973 census boundaries to define the unit of observation. The results from these analyses are shown in Table S3.

The results are very similar under this alternative unit specification (Table S3, column *vi*). Of the 249 protected census tracts, 135 were affected by a park with an entrance and the *MATT* on the tourism mechanism accounts for a similar proportion (compared with the primary analysis in Table S2, column *vi*) of the *ATT* ($MATT = -0.723$, $ATT = -1.27$). The results for the infrastructure mechanism are also similar to our primary analysis: Although increased road networks have poverty-alleviating effects, protected areas did not induce enough road development for this mechanism to play a significant role. The *MATT* estimate for change in forest cover is proportionately smaller than in the primary analysis but remains insignificant.

The results from the 1973 analysis are consistent with those from our primary analysis and provide further evidence that our estimates are not highly sensitive to specification.

Alternative Mechanism Estimation Strategies. The linear structural estimation model (LSEM) proposed by Baron and Kenny (15) has been a workhorse for estimating causal mechanism (mediation) effects in the political science literature. The method uses the results from multiple linear regressions to estimate direct (net) and indirect (mechanism) effects. We estimate the impacts of tourism, infrastructure, and forest cover change, using the LSEM to ensure that our results are not driven by our choice of estimation strategy.

Mechanism effect estimates from the LSEM stem from the results of three separate linear regressions,

$$Y_i = \alpha + \beta_1(T_i) + \epsilon_{i1} \qquad \textbf{[S22]}$$

$$S_i = \zeta + \beta_2(T_i) + \epsilon_{i2} \qquad \textbf{[S23]}$$

$$Y_i = \eta + \gamma(T_i) + \beta_3(S_i) + \epsilon_{i3}, \qquad \textbf{[S24]}$$

where Y_i, T_i, and S_i represent the observed values of poverty index in 2000, protection, and mechanism, respectively. β_1 from [**S22**] represents to the total effect (akin to the *ATT*), β_2 represents the effect of treatment on the mechanism (the initial causal pathway in Fig. 1*B* of the main text), and β_3 represents the impact of the mechanism on poverty (the second causal pathway in Fig. 1*B* of the main text). Baron and Kenny (15) suggest that the indirect effect (mechanism effect) is equal to $(\beta_2 \cdot \beta_3)$.

Unbiased estimates from the LSEM framework rely on the same assumptions described above: that outcomes and mechanisms are independent of treatment conditional on covariates. Therefore, we implement the same first-stage matching as described above before running the regressions, and we add all covariates to Eqs. **S23** and **S24**.

The results from the LSEM estimation are shown in Table S4, and we use the Mediation package (16) in R 2.15.1 to estimate *P* values. The total effect is estimated to be −1.7, which represents the *ATT* absent regression bias adjustment. The effects of each mechanism are made by recalculating Eqs. **S23** and **S24** for each mechanism, independently. The results from the LSEM estimation are quite similar to our primary results. Tourism that is associated with the establishment of protected areas remains the largest contributor to the reductions in poverty. Infrastructure development, again, does not have a significant impact on poverty. The point estimate on the change in forest cover mechanism is roughly similar to that of our primary results (0.53 compared with 0.42); however, according to the SE estimator suggested by Imai et al. (17), the LSEM estimate is highly significant ($P < 0.01$). In all LSEM mechanism estimates the SEs are much smaller than those from the primary analysis.

We believe that the similarities between the estimates from our primary analysis and those from the LSEM provide further evidence that our results are not sensitive to our estimation strategy. Further, our precision estimates are comparatively more conservative, meaning that we are less likely to draw unfounded inference from our results.

Sensitivity Analysis Using LSEM. *Assumptions 1* and *2* [similar to sequential ignorability by Imai et al. (17)] are required for identification of mechanism effects, but like any other assumption on ignorability they are untestable. However, Rosenbaum (18) suggested a method for testing the sensitivity of average treatment effect estimates to the presence of unobserved heterogeneity, i.e., violations of ignorability. Essentially, so-called Rosenbaum bounds estimate how strong an omitted confounder would have to be to prevent rejection of the null hypothesis of zero treatment effect.

Imai et al. (17) suggest sensitivity analysis under the LSEM framework in the spirit of the Rosenbaum bounds. Imai et al. (17) note that *Assumptions 1* and *2* imply independence of the LSEM regressions. Therefore, ϵ_2 should not be correlated with ϵ_3. The logic of Imai et al.'s (17) sensitivity test asks how much correlation between ϵ_2 and ϵ_3 (e.g., the importance of an omitted confounder) would be necessary to negate statistically significant mechanism effect estimates. The analysis imposes correlation in the error terms measured by the term ρ, where $-1 \le \rho \le 1$. Results that are negated at large values of ρ are robust to unobserved heterogeneity, whereas results that are negated at small values of ρ are not. We use the Mediation package to perform the sensitivity analysis suggested by Imai et al. (17).

Given the estimates from the LSEM approach were similar to our primary estimates, we believe this sensitivity analysis is informative. The analysis implies that the conclusion that tourism from protected area creation contributed to poverty reductions would be maintained unless an unobservable confounder had a negative correlation with expected poverty and where the government places park entrances of −0.21 or more negative.

The ρs on the tourism and change in forest cover mechanisms (last column of Table S4) are −0.21 and 0.27. We interpret these parameters as indicating that the estimated mechanism effects are moderately sensitive to unobserved heterogeneity. However, it should be noted that any measure on the sensitivity of an estimate to unobserved heterogeneity in no way implies the presence of heterogeneity.

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Table S1. Total visitation in 2007 to protected areas that were established before 1980

Protected area	Total visitation
PN Arenal†	58,829
PN B. Honda†	2,571
PN Cahuita Sector Playa Blanca	74,487
PN Cahuita Sector Pto. Vargas	23,781
PN Cahuita	98,268
PN Carara†	21,469
PN Carrillo†	14,480
PN Chirripo†	4,242
PN Corcovado†	28,225
PN Guayabo†	0
PN Juan Castro†	0
PN ML. Antonio†	241,193
PN Palo Verde†	6,612
PN Poas†	307,002
PN Rincon	44,597
PN Sta. Rosa†	30,231
PN Tenorio†	11,512
PN Tortuguero†	117,626
PN Turrialba†	16,846
RB Hitoy Cerere†	0
RNA Cabo Blanco†	6,967
Total	1,108,938

PN, National Park; RB, Biological Reserve; RNA, Absolute National Reserve.
†Protected area with entrance.

Table S2. Primary results for single- and joint-mechanism estimation

			Single mechanism		All mechanisms	
	i	*ii*	*iii*	*iv*	*v*	*vi*
Mechanism	Observed mechanism	Counterfactual mechanism	Mechanism coefficient	*MATT*	Mechanism coefficient	*MATT*
Park entrance	227	0	−2.75***	−1.327***	−3.636***	−1.738***
			[0.517]	(0.348)	[0.514]	(0.349)
Δ roadless volume	−203.9	−176.2	0.00098	−0.076	0.0011*	−0.081
			[0.00064]	(0.347)	[0.0006]	(0.339)
Δ forest cover	−0.0576	−0.165	2.139***	0.18	4.392***	0.422
			[1.016]	(0.347)	[1.037]	(0.342)

***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively. Terms in parentheses are heteroskedasticity robust SEs. Terms in brackets are SEs.

Table S3. 1973 mechanism results using mechanism bias adjustment

			Single mechanism		All mechanisms	
Mechanism	*i* Observed mechanism	*ii* Counterfactual mechanism	*iii* Mechanism coefficient	*iv* *MATT*	*v* Mechanism coefficient	*vi* *MATT*
Park entrance	135	0	−1.045	−0.566	−1.43***	−0.775*
			[0.502]	(0.439)	[0.483]	(0.428)
Δ roadless volume	−727,579	−674,147	2.69e-06***	−0.143	2.779e-06***	−0.148
			[3.68e-07]	(0.447)	[3.679e-07]	(0.447)
Δ forest cover	−0.067	−0.23	0.627	0.103	0.398	0.065
			[1.24]	(0.439)	[1.194]	(0.427)

***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively. Terms in parentheses are heteroskedasticity robust SEs. Terms in brackets are SEs.

Table S4. LSEM mechanism and sensitivity estimates

Component	Effect	β_2	β_3	ρ
Total effect	−1.7**	NA	NA	NA
	(0.51)			
Park entrance	−1.54***	0.458	−3.36***	−0.21
	[0.00]	0.021	0.51	
Δ roadless volume	−0.008	−6.37	0.001	0
	[0.73]	19.2	0.344	
Δ forest cover	0.536***	0.11***	4.863***	0.27
	[0.00]	0.015	0.7	

***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively. Terms in parentheses are SEs. Terms in brackets are *P* values.

Table S5. Additional infrastructure mechanisms

			All mechanisms	
Mechanism	*i* Observed mechanism	*ii* Counterfactual mechanism	*iii* Mechanism coefficient	*iv* *MATT*
No. schools	0.423	0.461	0.493*	−0.068
			[0.636]	(0.339)
No. clinics	0.002	0.025	−5.296	0.069
			[4.922]	(0.342)

***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively. Terms in parentheses are heteroskedasticity robust SEs. Terms in brackets are SEs.

Table S6. Correlation between mechanism variables

Mechanism	Park entrance	Δ roadless volume	Δ forest cover
Park entrance	1	−0.086	−0.003
Δ roadless volume	—	1	0.081
Δ forest cover	—	—	1

Table S7. Summary statistics for matching covariates and mechanism variables

Covariate	Description	Status	Mean	Median	SD	Range
Baseline poverty	Poverty index measured in 1973	Unprotected Protected	6.57 13.64	6.83 13.3	7.77 4.63	−16.09–26.94 2.25–25.03
Forest cover	% census tract occupied by forest in 1960	Unprotected Protected	0.139 0.512	0.00 0.595	0.299 0.382	0–1 0–1
% high productivity land	% census tract occupied by land use capacity I, II, or III land	Unprotected Protected	0.345 0.079	0 0	0.45 0.19	0–1 0–0.99
% medium-high productivity land	% census tract occupied by land use capacity IV land	Unprotected Protected	0.413 0.17	0 0	0.47 0.28	0–1 0–1
% medium-low productivity land	% census tract occupied by land use capacity VI, VII, or VIII land	Unprotected Protected	0.194 0.223	0 0.01	0.37 0.31	0–1 0–1
Distance to major city	Average distance (km) from each 3-ha land plot within a census tract to nearest major city: Limon, Puntarenas, or San Jose	Unprotected Protected	35.49 58.05	20.48 41.82	36.86 46.13	0.04–203.26 7.61–179.51
Roadless volume	The sum of the product of area and distance to nearest road (1969) for every 1-ha parcel within the census tract	Unprotected Protected	22.84 441.98	0.05 64.13	90.33 1,440.69	0–2,151.24 0–21,792.36
Mechanism variables						
Park entrance	Binary indicator equal to 1 if census tract has at least 10% of its area occupied by a protected area with a park entrance		0.0123	0	0.11	0–1
Δ roadless volume	Change in roadless volume between 1969 and 1991		−18.43	0	136.951	−9,327–54.87
Δ forest cover	% change in forest cover between 1960 and 1986 within each census tract		−0.066	0	0.273	−1–1

Table S8. Balance results for first-stage Mahalanobis one-to-one matching covariate matching with replacement

Covariate	Status	Mean protected	Mean unprotected	Difference in means	Normalized difference	Mean eQQ difference	% improve mean difference
Poverty index 1973	Unmatched	13.640	6.573	7.068	0.602	7.079	
	Matched	13.506	13.824	−0.317	0.025	0.495	95.51
% forest 1960	Unmatched	0.512	0.138	0.374	0.577	0.373	
	Matched	0.506	0.505	0.00039	0.001	0.010	99.89
% land use capacity 1, 2, 3	Unmatched	0.078	0.345	−0.267	0.384	0.267	
	Matched	0.077	0.100	−0.023	0.049	0.023	91.36
% land use capacity 4	Unmatched	0.170	0.413	−0.243	0.327	0.243	
	Matched	0.170	0.170	−0.00001	0.000	0.013	99.99
% land use capacity 5, 6, 7	Unmatched	0.222	0.193	0.0288	0.043	0.089	
	Matched	0.219	0.206	0.0137	0.023	0.020	52.35
Distance to major city	Unmatched	58.048	35.490	22.558	0.268	22.510	
	Matched	57.449	55.895	1.553	0.015	5.182	93.11
Roadless volume 1969	Unmatched	441.986	22.837	419.148	0.285	416.354	
	Matched	436.564	281.715	154.849	0.090	157.296	63.05

Table S9. Results for single- and joint-mechanism estimation without mechanism bias adjustment

			Single mechanism		All mechanisms	
Mechanism	*i* Observed mechanism	*ii* Counterfactual mechanism	*iii* Mechanism coefficient	*iv* *MATT*	*v* Mechanism coefficient	*vi* *MATT*
Park entrance	227	0	−2.75***	−1.327***	−3.636***	−1.738***
			[0.517]	(0.348)	[0.514]	(0.349)
Δ roadless volume	−203.9	−137.2	0.00098	−0.115	0.0011*	−0.127
			[0.00064]	(0.349)	[0.0006]	(0.342)
Δ forest cover	−0.0576	−0.175	2.139***	0.201	4.392***	0.465
			[1.016]	(0.346)	[1.037]	(0.342)

***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively. Terms in parentheses are heteroskedasticity robust SEs. Terms in brackets are SEs.