Civil Conflict Reduced the Impact of Colombia's Protected Areas

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Abstract

Protected areas are fundamental to reaching the goals laid out as a part of the recent Sustainable Development Goals. Recognition of the importance of protected areas from policy makers, practitioners, and academics alike has led to increased interest in evaluating the effectiveness of this conservation instrument. Colombia is a particularly interesting setting in which to study the effects of protected areas. Colombia is one of the most biodiverse countries in the world and has a large network of protected areas. Importantly, during the decades that the protected area network was being developed, Colombia also saw the unique and marked rise of guerrilla activity (domestic terrorism). In addition to negative social outcomes, guerrilla activity also has led to increases in deforestation; and although evidence shows that Colombia's protected areas have achieved modest reductions in deforestation, further evidence suggests that protected areas have facilitated guerrilla activities. We use quasi-experimental methods to (1) estimate the average impact of Colombia's protected areas established prior to 2002 on forest cover (1990-2010) and poverty (1993-2005), and (2) decompose those average effects into those that are attributable to protected areas' effect of guerrilla activities (mechanism effect) and those that are net the mechanism effects (the direct effect). We find that, on average, protected areas had no statistically significant impact of either outcome. However, we find that, had protected areas not also increased guerrilla activities, reductions in deforestation would have been over twice as large and statistically significant, and the poverty reducing effects would have been over three times as large.

Keywords: mediation, causal inference, matching, guerrilla, conflict, FARC, Latin America, terrorism

1 Introduction

Protected areas have played a prominent role in the preservation of terrestrial resources, ecosystem services, and biodiversity. Given the ambitious objectives of the recently formulated Sustainable Development Goals (United Nations, 2015), the proliferation of protected areas is likely to continue, especially in developing nations. Poverty advocates have long voiced concerns that protected areas have the potential to adversely impact local communities - communities that tend to already be marginalized. Indeed, some studies that investigate the spatial heterogeneity of protected areas impacts find the potential for poverty exacerbation in some areas (Ferraro et al., 2011; Ferraro and Hanauer, 2011; Hanauer and Canavire-Bacarreza, 2015). The potential for these high opportunity costs of establishing protected areas highlights the importance of understanding the environmental return to these investments. Concerns over the potential for negative socioeconomic impacts in tandem with the SDGs have spurred the call for better understanding of the impacts of the policies used to achieve the goals: it is important to understand to what extent a policy has been effective prior to further expansion. Such calls for evidence on the environmental impacts of protected areas based on high quality impact evaluations have been met by researchers in recent years. A growing body of literature focuses on applying quasi-experimental methods to estimate the amount of deforestation that has been avoided due to the establishment of protected areas. However, such evidence from Latin America is still relatively sparse (published evidence exists for Bolivia (Hanauer and Canavire-Bacarreza, 2015), Brazil (e.g., Pfaff et al. (2014)), Costa Rica (Andam et al., 2008), Peru (Miranda et al., 2016), and there is even less empirical evidence on the mechanisms through which protected areas achieve (or do not achieve) environmental protection.

Colombia is a particularly interesting setting in which to study the effects of protected areas. It is one of the most biodiverse countries in the world and has a large network of protected areas. Colombia also has experienced one of the longest periods of civil conflict in the modern era. The violent conflict between armed guerrilla groups, paramilitaries, and the government, dates back nearly a century to political uprisings from agrarian communities (Palau 2006). In the decades since, the Revolutionary Armed Forces of Colombia (FARC) distinguished itself as the primary guerrilla organization and evolved to using drug cultivation and trafficking, kidnapping, and other criminal activities to fund and push their agenda (Palau 2006). In addition to hundreds of thousands of

casualties, these activities and conflicts have had negative socioeconomic and environmental consequences (e.g., Vargas (2003); Fergusson et al. (2014)). Importantly, during the decades that the protected area network was being developed, Colombia also saw the unique and marked rise of guerrilla activity. The objective of this research is to examine the causal relationships between protected areas, guerrilla violence, deforestation, and poverty. We draw from recent studies that find Colombia's protected area system lead to increased guerrilla violence in their vicinity (Canavire-Bacarreza et al., 2018) and that civil conflicts lead to deforestation (Fergusson et al., 2014). Taken together, these results raise the concerns that protected areas might not be achieving their environmental charter if they, at the same time, promote activities that are deleterious to those goals. Given that guerrilla activities can also have socioeconomic impacts, this further raises the concern regarding the impacts of Colombia's protected areas on surrounding communities.

We begin by estimating the average impact of Colombia's protected areas established prior to 2002 on forest cover (1990 to 2010) and poverty (1993 to 2005). We find no significant impact on either outcome, on average. We then extend this analysis using advanced quasi-experimental methods developed by (Flores and Flores-Lagunes, 2011) and (Ferraro and Hanauer, 2014b) to estimate how Columbia's protected areas affected forest cover and poverty through their effects of guerrilla activities. In doing so we are able to quantify the direct and mechanism effects of protected areas on those outcomes. The direct effects are of particular interest because they allow us to quantify what the effects of Columbia's protected areas would have been, had they not also affected guerrilla activities. We find that, had protected not affected guerrilla activities, they would have reduced deforestation by over twice as much (1.8 percentage points,) and reduced poverty by over three times as much. Given the recent peace accord, our results should provide valuable information to policy makers about how future protected areas might perform.

2 Methods

2.1 Estimands

2.1.1 Average Treatment Effect on the Treated (ATT)

In order to understand the mechanism and direct effects of protected areas we must first estimate the relevant average effect. For this portion of the analysis we are interested in the average treatment effect on the treated (ATT).[1] Practically this means we must answer the question, "what would forest cover and poverty in protected municipalities have been had they been directly affected by a protected area?"[2] This counterfactual question is captured by the ATT expressed in terms of potential outcomes:

$$ATT = E[Y(1)|D = 1] - E[Y(0)|D = 1], \tag{1}$$

where $Y(1)$ and $Y(0)$ represent the potential outcomes under "treatment" or "control," respectively, and D is a binary variable that denotes exposure to treatment or control (1 or 0, respectively). In our context, exposure to treatment implies exposure to protection and the ATT only considers the potential outcomes for those units (municipalities) exposed to treatment. The first term in equation (1) is the expected outcome for treated units, which we observe. The second term is the expected outcome for treated units, had they not been treated. This term is unobservable and represents the counterfactual that we must estimate. The concept of estimating the average impact of protection on our outcomes is captured by the directed acyclic graph (DAG) in panel (a) of Figure 1. Each variable is linked by a single headed arrow, which defines the direction of a causal relationship. The DAG in Figure 1a also shows that in order to estimate the ATT from (1), we must also condition on confounding variables that jointly determine protection and our outcomes.

[1] The ATT only requires the estimation of counterfactual outcomes for those units exposed to the intervention. This is appropriate in our study because certain areas, like cities, would never be exposed to a protected area. Thus we have no interest in estimating what would have happened in these areas had they been exposed to protection.

[2] See Canavire-Bacarreza et al. (2018) for a discussion of how this counterfactual question relates to local spillovers.

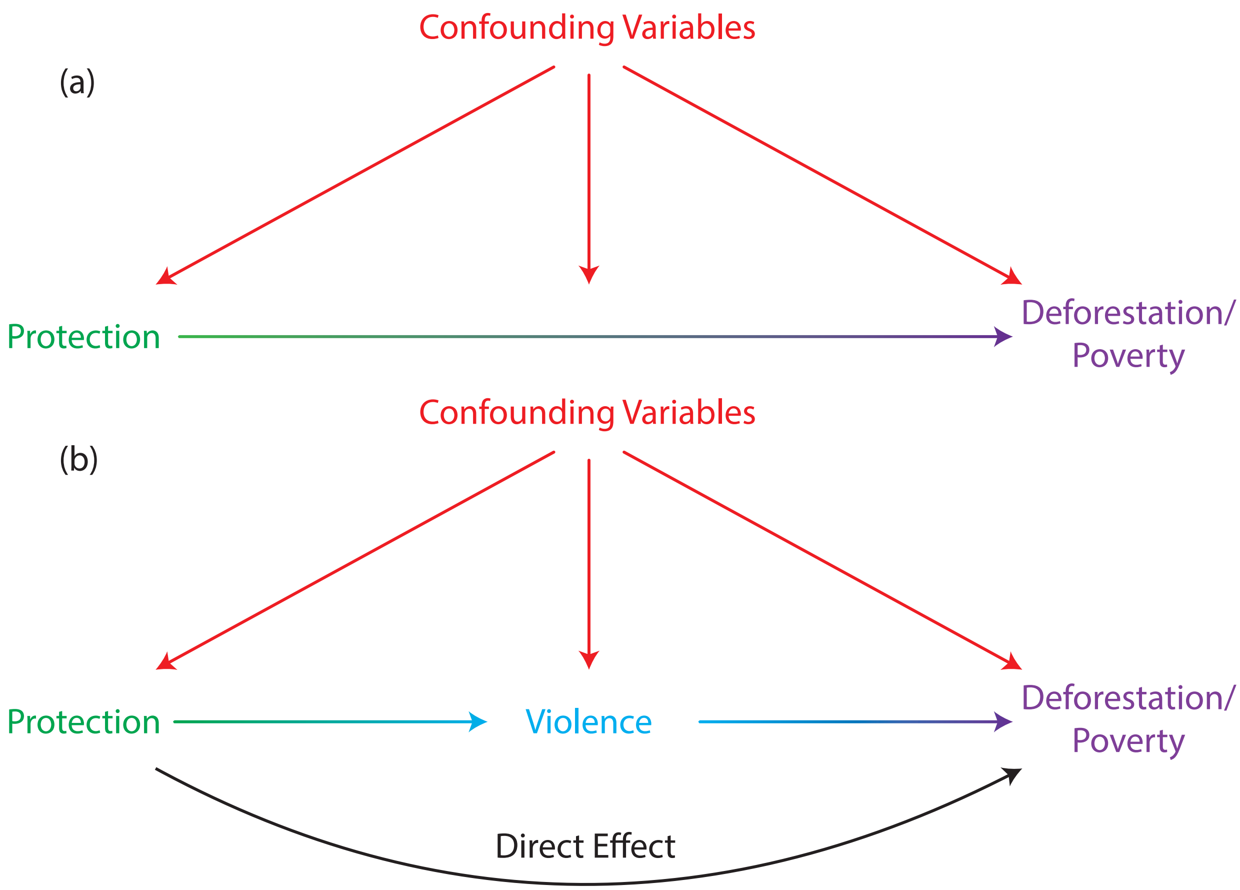


Figure 1: DAG showing ATT and other pathways.

2.1.2 Direct and Mechanism Effects

To understand how protected areas would have performed in the absence of guerrilla activities we must first understand how those areas affected forest cover and poverty through their effect on guerrilla activities. This means that in order to estimate the direct effects of protection, we must also estimate the mechanism effects. Thus we are interested in two related estimands, which decompose the ATT- the mechanism average treatment effect on the treated ($MATT$) and the net average treatment effect on the treated ($NATT$ or direct effect). This decomposition of the ATT is captured b the DAG in panel (b) of Figure 1. The elaborated causal pathway from protection to the outcomes, through violence captures the mechanism effect ($MATT$ in our framework). Similarly, the effect of protection on our outcomes that is not transmitted through violence captures the direct or net effect or protection ($NATT$ in our framework).

To identify these estimands we need additional concepts and assumptions. Take S as a post-treatment mechanism that is measured at an intermediate period between administration of treatment and measurement of outcome. Because, by definition, S is affected by treatment and thus must be handled in a manner similar to the outcome of interest (Y). Therefore, similar to 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 means that there are four potential outcomes we must consider for each unit: $(Y_i(1), Y_i(0), S_i(1), S_i(0))$.

These compound potential outcomes capture the following: (1) $Y_i(1, S_i(1))$, when the unit and mechanism are affected by treatment (equivalent to total effect of treatment $Y_i(1)$); (2) $Y_i(1, S_i(0))$, when the unit is affected by treatment but the mechanism is not (the mechanism is "blocked" (Flores and Flores-Lagunes, 2011)); (3) $Y_i(0, S_i(0))$, when neither the unit nor the mechanism is affected by treatment (equivalent to $Y_i(0)$), and; (4) $Y_i(0, S_i(1))$, when treatment affects the unit, but not the mechanism. 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 casual mechanism and net effects, we use the principal strata framework developed by Frangakis and Rubin (2002) (see also, Rubin (2004); Mealli and Rubin (2003)). Defining a principal stratum is similar to the concept of matching individuals (or groups of individuals) based on similar potential outcomes in a standard

quasi-experimental setting. Two units from different treatments (e.g., protected, unprotected) share a principal stratum if they share potential mechanism outcomes (formally a principal stratum is defined where $\{S(0) = s_0, S(1) = s_1\}$.

The $MATT$ and $NATT$ require that we estimate outcomes for treated units, had the mechanism been blocked (not affected the outcome).

The $MATT$ can be written

$$MATT = E\left\{E\left[Y_i(1, S_i(1)) - Y(1, S_i(0)) | S_i(0) = s_0, S_i(1) = s_1, X_i = x, D_i = 1\right]\right\}. \tag{2}$$

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 isolating the only source of variation in (2) to be the effect on outcomes due to a change in the mechanism (via blocking the effect of the mechanism on the outcome in the second term of (2)). Conversely, the $NATT$ isolates the effect on outcomes due to a change in treatment

$$NATT = E\left\{E\left[Y_i(1, S_i(0)) - Y_i(0, S_i(0)) | S_i(0) = s_0, S_i(1) = s_1, X_i = x, D_i = 1\right]\right\}, \tag{3}$$

holding S at untreated levels and thus identifying the direct effect of the treatment (i.e., net the effect of the mechanism). Estimation of $NATT$ requires that we estimate the same (key) counterfactual as for the $MATT$ (the first term in 3) and then subtract from that the full counterfactual for treated units (outcomes for treated units had the not been treated, the second term in 3). An advantage of the $MATT$ and $NATT$ is that they decompose the ATT such that $ATT = MATT + NATT$. This decomposition states that the ATT 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 directly to the effect of treatment, the $NATT$ (see Figure 1b). Therefore, once either $MATT$ or $NATT$ is estimated the complementary estimate falls out of the difference with ATT.

2.2 Estimation Strategy

Estimation of either $MATT$ or $NATT$ is confounded by the fact that the key counterfactual, $Y_i(1, S_i(0))$ is typically unobservable. We use a multistage approach to estimate the key counterfactual, and thus the $MATT$ and $NATT$. We use the same matching process as Canavire-Bacarreza et al. (2018) in the first step. Once we have our quasi-experimental sample, we use a two-step method suggested by Flores and Flores-Lagunes (2011) and Ferraro and Hanauer (2014b) to estimate to estimate outcomes for protected municipalities had violence followed similar patterns observed in matched unprotected municipalities, which is the key counterfactual.

2.2.1 First Stage: Matching

Following Canavire-Bacarreza et al. (2018), we use one-to-one genetic matching with replacement and post-match bias-adjustment (Abadie et al., 2004; Abadie and Imbens, 2006) to match control units to treated units. This approach serves two purposes. First, it serves to satisfy the mean independence assumption

Assumption 1. $E[Y_i(0) \mid X_i, D_i = 1] = E[Y_i(0) \mid X_i, D_i = 0]$,

which states that, once we condition on the appropriate confounders (X), the observable outcomes for control units, on average, can be used as counterfactuals for the treated units, had they not been treated. Thus this allows us to estimate the ATT. If we further invoke the assumption that X sufficiently captures the mechanism process, then the matched pairs (treated and control units) can be assumed to lie within the same principle strata. Formally we assume (mechanism isolation)

Assumption 2. $E[S_i(0) \mid X_i, D_i = 1] = E[S_i(0) \mid X_i, D_i = 0]$.

In other words, we assume that, conditional on X, protection is not assigned based on expectations that guerrilla activities will be different under treatment and control conditions.

Thus the first-stage matching also serves to provide 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. This 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 1 for a description of the covariates used for matching (also see Canavire-Bacarreza et al. (2018)).

2.2.2 Second Stage: Estimate the Influence of Mechanisms

We follow Ferraro and Hanauer (2014b) to impute the key counterfactual. In addition to assumptions 1 and 2 we assume 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 (Flores and Flores-Lagunes, 2011).

Assumption 3. *Suppose*

$$E\left[Y_i(1, S_i(1))|S_i(1), X_i = x, D_i = 1\right] = a_1 + b_1 S_i(1) + c_1 X_i, \tag{4}$$

then,

$$E\left[Y_i(1, S_i(0))|S_i(1), X_i = x, D_i = 1\right] = \overbrace{a_1 + b_1 \underbrace{S_i(0)}_{\substack{\textit{Observed} \\ \textit{matched} \\ \textit{control} \\ \textit{values}}} + c_1 X_i}^{\textit{Coefficients from (4)}}. \tag{5}$$

Assumption 3 implies that the marginal effect of the mechanism is constant across treatment arms. In (4) and (5) of assumption 3, b_1 represents the marginal effect of the mechanism S. The key counterfactual $(\widehat{Y}(1, \widehat{S}(0)))$ can be estimated by evaluating (5), which uses the coefficients from (4), setting $S_i(0) = E[S_i(0) \mid D = 1] = [\widehat{S}_i(0) \mid D = 1]$ which, according to equation 3, is equal to the observed control mechanism values within the common principal stratum of each treated unit.

Empirical estimation of the key counterfactual involves regression imputation similar to post-match regression bias adjustment suggested by Abadie and Imbens (2006) and Abadie et al. (2004). We first run a regression of observed outcomes on covariate and mechanism values for treated units ((4)). 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 (5)) and the matched control unit mechanism outcomes (where in (5) $S_i(0) = E[S_i(0) \mid D = 1] = S_i^{obs}(0)$ and $S_i^{obs}(0)$ is the observed mechanism outcome of each treated units respective matched control). Replacing the first term in (2), the empirical form for $MATT$ becomes

$$MATT = E\underbrace{\left\{E\left[Y_i^{obs}(1)|S_i^{obs}(1) = s_1, X_i = x, D = 1\right]\right\}}_{\text{Observed Protected Values}} - \underbrace{E\left[f_1(S_i(0), X_i)\right]}_{\text{Estimated in (5)}}. \tag{6}$$

Similarly, the empirical form of $NATT$ becomes

$$NATT = \underbrace{E\left[f_1(S_i(0), X_i)\right]}_{\text{Estimated in (5)}} - \underbrace{E\left\{E\left[Y_i^{obs}(0)|S_i^{obs}(0) = s_0, X_i = x, D = 1\right]\right\}}_{\text{Observed Unprotected Values}}, \tag{7}$$

where $f_i(S_i(0), X_i)$ in (6) and (7) is equal to $E\left[Y_i(1, S_i(0)) \mid S_i(1), X_i = x, D = 1\right]$ from (5).

2.3 Empirical Counterfactual Estimator

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

$$S_{i:D=0} = X_{i:D=0}\beta_{1C} + \epsilon \tag{8}$$

where $S_{i:D=0}$ and $X_{i:D=0}$ represent the observed mechanism and baseline covariate values, respectively, of matched *unprotected* municipalities. The coefficients from (8) are then used to impute counterfactual mechanism outcomes for each mechanism

$$\left[\widehat{S}_i(0)|D = 1\right] = X_{i:D=1}\widehat{\beta}_{1C} \tag{9}$$

where $X_{i:D=1}$ are the observed covariate values of the *protected* municipalities. Observed and counterfactual mechanism values for the protected municipalities can be seen in Tables 4 and 5. The imputed counterfactual mechanism values from (8) are then used to calculate the counterfactual of interest: the outcomes for protected units, had protection not affected mechanisms $(\widehat{Y}_i(1, S_i(0)))$.

To estimate this counterfactual value we first estimate the influence of covariates and mechanism on outcomes using the protected municipalities

$$Y_{i:D=1} = X_{i:D=1}\beta_{1D} + S_{i:D=1}\beta_{2D} + \epsilon \tag{10}$$

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

$$\widetilde{Y}_{i:D=1} = X_{i:D=1}\widehat{\beta}_{1D} + \widehat{S}_i\widehat{\beta}_{2D} \tag{11}$$

where $\widehat{S}_i = \left[\widehat{S}_i(0)|D=1\right]$ are the counterfactual mechanism values from (9), thus $\widetilde{Y}_{i:D=1} = \widehat{Y}_i(1, S_i(0))$. $MATT$ is calculated by subtracting the mean of the fitted values ($\widetilde{Y}_{i:D=1}$) from mean of the observed protected tract outcomes ($Y_i(1, S(1)) = Y_i(1)$). Similarly, the $NATT$ is calculated by subtracting the counterfactual outcomes for treated units (the full counterfactual estimated for the ATT) from the mean of the fitted values ($\widetilde{Y}_{i:D=1}$).

2.4 Precision Estimates

To calculate the precision of our $NATT$ and $MATT$ estimates we base our standard error estimator on the heteroskedasticity robust matching-based estimator suggested by (Abadie and Imbens, 2006).[3] 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 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

$$\widehat{\sigma}^2_{i:D=0}\left(X_i\right) = \left(Y_i - Y_l\right)^2/2, \tag{12}$$

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

$$\widehat{V}_{D=0} = \sum_{N_{D=0}} \lambda_i^2 \cdot \widehat{\sigma}_i^2\left(X_i\right), \tag{13}$$

where $\lambda_i = \#C_i/N_{D=0}$, 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 individual-level variance for protected units is based on unit level deviations from the estimated $NATT$ and $MATT$. We estimate variances

[3] A function that estimates the standard errors outlined in this section was programmed in R 3.2.1 and is available from the authors upon request.

$$\widehat{\sigma}^2_{i:D=1}\left(X_i\right) = \left(\widehat{Y}_i(1, \widehat{S}(0)) - \widehat{Y}_i - \widehat{NATT}\right)^2, \tag{14}$$

and

$$\widehat{\sigma}^2_{i:D=1}\left(X_i\right) = \left(Y_i - \widehat{Y}_i(1, \widehat{S}(0)) - \widehat{MATT}\right)^2. \tag{15}$$

for the $NATT$ and $MATT$, respectively.

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

$$\widehat{V}_{D=1} = \frac{1}{N^2_{D=1}} \sum_{N_{D=1}} \widehat{\sigma}^2_{i:D=1}\left(X_i\right). \tag{16}$$

for each estimand.

By combining each of these treated unit-level variance estimates (14 and 15) with the control unit-level variance estimate (13), we calculate the final standard errors (for each estimand separately) as

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

3 Data

Our data comprise socioeconomic and biophysical characteristics compiled from multiple sources (see Canavire-Bacarreza et al. (2018) for details) meant to capture those characteristics that jointly determine where protected areas were established, guerrilla violence, forest cover, and poverty. We use all IUCN categories reported in the latest update to identify all national-level protected areas. All geographic information systems (GIS) calculations are made in ArcMap 10.x.

3.1 Unit of Analysis

Environmental and socioeconomic outcomes are not necessarily measured at the same geographic scale. However, we use municipalities as our unit of observation so as to remain maintain constant

units of analysis across environmental and socioeconomic outcomes, which are defined by political boundaries. Our baseline census comes from 1993, at which time there were 1,056 municipalities with an average area of 1,042 km^2 (range 1.42 - 66,002km^2).

3.2 Treatment

We use the creation of the National System of Natural Protected Areas in 1998 to define the baseline for our analyses. The logic is that, even though many protected areas were technically established previously, lack of recognition and funding meant parks established prior to 1998 were paper parks (see Canavire-Bacarreza et al. (2018) for further details). 152 national protected areas (covering 11% of Colombia) were established prior to 2002 (with baseline being 1998). To remain consistent with Canavire-Bacarreza et al. (2018) and others (e.g., (Andam et al., 2010a; Canavire-Bacarezza and Hanauer, 2013; Ferraro and Hanauer, 2014a)) we define a municipalitiy as "protected"' if at least 10% of its area is occupied by a protected area.[4] In order to avoid comparison units that are marginally protected, we drop any municipalitiy with protection in the range of [1%, 10%)). Our final set of data includes 909 municipalities, 115 of which are considered protected.

3.3 Outcomes

Forest Cover- Forest coverage is measured at the 30m (pixel) level for 1990, 2000, 2005, and 2010. We are interested in how forest cover has changed between 1990 (pre-baseline) and 2010. To assign forest cover values at the unit-level, we use GIS to calculate the proportion of each municipality covered by forest in each period.

Poverty-Colombia's standard measure of poverty is captured by the Unsatisfied Basic Needs Index (UBNI), which assess the number of basic household necessities that are unmet. Specifically, UNBI measures the percentage of households in each municipality for which more than two out of six components deemed necessary remain unmet. Our baseline measure of UBNI comes from the 1993 population census and the terminal measure comes from the 2005 census.

[4](Canavire-Bacarreza et al., 2018), who estimate the impact of protection on violence in Colombia, test the sensitivity of their results to changes in the protection threshold. There results do not change, qualitatively, when they change the threshold.

3.4 Mechanism

We use the same data as Canavire-Bacarreza et al. (2018) to measure yearly violence between 1993 and 2014. Following that paper, and others from the literature, a violent act committed by FARC is defined as the sum of explosive terrorist attacks, arsonist terrorist attacks, private property assaults, entity terrorist attacks, political terrorist attacks, route blockings, armed contact, ambushes, harassing, population incursions, land piracy, and illegal road blockings within a municipality (Camacho and Rodriguez, 2013; Lemus, 2014; Rodriguez and Sanchez, 2012).

By definition, a causal mechanism is an intermediate outcome that is affected by treatment and, in turn, affects the primary outcome. Therefore, we are interested in measuring how violence changed from baseline (1998) to an intermediate terminal year. In our estimation we use several terminal years (2002, 2003, and 2004) because we have the luxury of yearly measurements of violence, and to ensure that our results are not sensitive to choice of terminal year. We choose 2004 as our latest terminal year because our most recent poverty measure is based on the 2005 census and we want to ensure that our mechanism is measured at an intermediate period. We also create an "average mechanism" variable that captures the average change in violent attacks between baseline and all three terminal years. This compound mechanism variable should help to capture the average direct and mechanism effects across our study period.

3.5 Covariates of Interest

In addition to conditioning on baseline (or pre-baseline) measures of all outcomes and mechanisms, we are interested in a set of covariates (X from our Methods section) that capture the determinants of guerrilla violence, forest cover (or deforestation), poverty, and how protected areas are sited. Previous studies from Latin America (e.g., Andam et al. (2008); Canavire-Bacarezza and Hanauer (2013)) show that protected areas tend to adhere to the so-called "high and far" or "rocks and ice" bias (Joppa and Pfaff, 2009). Practically, this means that protected areas have tended to be sited far from cities on land that is unsuitable for agriculture and other economic activities; in other words, on low-opportunity-cost land. These same characteristics are also tend to be highly correlated with forest cover and poverty in Latin American and Caribbean countries. Thus, we choose a set of covariates that capture these characteristics prior to the establishment of protected

areas. For a brief description and summary statistics for each covariate, see Table 1. For a detailed description of, and rational for, each covariate see Canavire-Bacarreza et al. (2018).

Table 1: Municipality-level summary statistics for conditioning covariates by protection status.

Covariate	Source	Status	Mean	St. Dev.	Min	Max
Guerrilla attacks 1993	CEDE	Protected	1.391	2.713	0	21
		Unprotected	0.759	2.574	0	45
Guerrilla attacks 1994	CEDE	Protected	1.643	3.143	0	21
		Unprotected	0.926	2.705	0	39
Guerrilla attacks 1995	CEDE	Protected	1.565	3.369	0	25
		Unprotected	0.729	2.409	0	40
Guerrilla attacks 1996	CEDE	Protected	1.783	3.060	0	21
		Unprotected	0.802	2.634	0	36
Guerrilla attacks 1997	CEDE	Protected	2.026	3.912	0	26
		Unprotected	0.855	2.592	0	39
Guerrilla attacks 1998	CEDE	Protected	1.313	2.010	0	10
		Unprotected	0.763	2.795	0	63
Poverty (UBNI) 1993	CEDE	Protected	56.026	20.485	19.265	100.000
		Unprotected	53.441	18.380	9.154	100.000
Population density (pop/km) 1993	CEDE	Protected	38.987	43.312	0.315	238.875
		Unprotected	104.399	438.299	0.167	11,324.060
Distance to city (km)	CEDE	Protected	90.436	61.327	0.000	313.270
		Unprotected	77.486	52.868	0.000	360.770
Distance to market (km)	CEDE	Protected	79.123	82.832	0.000	552.749
		Unprotected	52.730	55.028	0.000	662.080
Average rainfall (cm)	IDEAM	Protected	171.188	117.483	38.611	738.460
		Unprotected	151.266	83.175	20.505	793.190
Percent forest cover 1990	IDEAM	Protected	44.717	27.998	1.738	97.066
		Unprotected	21.784	22.008	0.000	97.977
Average slope (degrees)	NASA	Protected	11.826	6.222	0.096	26.020
		Unprotected	9.528	6.565	0.065	25.650
Average elevation (m)	NASA	Protected	1,121.887	878.420	2	2,975
		Unprotected	1,210.724	1,235.700	2	3,087

Notes: 115 protected and 794 unprotected municipalities (marginally protected municipalities excluded from calculations)
CEDE: Centro de Estudios Sobre Desarrollo Economico
IDEAM: Instituto de Hidrologia, Meteorologia y Estudios Ambientales

4 Results

4.1 First-Stage Matching and ATT

In the first step of our estimation strategy we use genetic matching to: (1) estimate the ATT (under Assumption 1), and (2) create a matched set of units that will allow us to estimate direct ($NATT$) and mechanism ($MATT$) effects (under Assumptions 2 and 3). We use one-to-one nearest neighbor genetic matching (Diamond and Sekhon, 2013) with replacement and post-match bias adjustment

(Abadie et al., 2004; Abadie and Imbens, 2006). We choose the genetic matching algorithm to follow Canavire-Bacarreza et al. (2018), and because it provides the best post-match balance across the relevant covariates of all nearest-neighbor algorithms (e.g., Mahalanobis, inverse, propensity score, etc.). See Table 2 for balance results. Table 3 provides results from various estimators of the average effect of protected areas on forest cover and poverty.

Table 2: Balance results for the primary genetic matching specification.

Covariate	Status	Mean Protected	Mean Unprotected	Difference in Means	Normalized Dif in Means	Mean eQQ Difference	Percent Improvement
Guerrilla Attacks	Unmatched	1.391	0.759	0.632	0.169	0.791	
1993	Matched	1.391	1.217	0.174	0.044	0.313	72.5%
Guerrilla Attacks	Unmatched	1.643	0.926	0.718	0.173	0.826	
1994	Matched	1.643	1.626	0.017	0.004	0.330	97.6%
Guerrilla Attacks	Unmatched	1.565	0.729	0.836	0.202	0.896	
1995	Matched	1.565	1.435	0.130	0.028	0.235	84.4%
Guerrilla Attacks	Unmatched	1.783	0.802	0.980	0.243	1.130	
1996	Matched	1.783	1.565	0.217	0.043	0.670	77.8%
Guerrilla Attacks	Unmatched	2.026	0.855	1.171	0.250	1.200	
1997	Matched	2.026	1.461	0.565	0.121	0.565	51.7%
Guerrilla Attacks	Unmatched	1.313	0.763	0.550	0.160	1.070	
1998	Matched	1.313	1.104	0.209	0.077	0.226	62.0%
Poverty 1993	Unmatched	56.026	53.441	2.585	0.094	3.219	
(UBNI)	Matched	56.026	56.526	−0.500	0.017	1.652	80.7%
Distance to	Unmatched	90.436	77.486	12.950	0.160	13.241	
Capital (km)	Matched	90.436	84.648	5.788	0.069	11.179	55.3%
Rainfall (cm)	Unmatched	171.188	151.266	19.921	0.138	19.808	
	Matched	171.188	176.671	−5.483	0.035	13.330	72.5%
Average Slope	Unmatched	11.826	9.528	2.299	0.254	2.287	
(degrees)	Matched	11.826	11.307	0.520	0.058	0.710	77.4%
Distance to	Unmatched	79.123	52.730	26.392	0.265	25.887	
Market (km)	Matched	79.123	72.684	6.439	0.053	12.924	75.6%
Illiteracy 1993	Unmatched	83.512	85.347	−1.834	0.144	2.250	
	Matched	83.512	84.175	−0.662	0.046	2.204	63.9%
Percent Forest	Unmatched	44.717	21.784	22.933	0.644	22.804	
Cover 1990	Matched	44.717	42.909	1.809	0.046	3.203	92.1%
Population	Unmatched	38.987	104.399	−65.412	0.149	138.843	
Density 1993	Matched	38.987	48.357	−9.370	0.109	10.713	85.7%
Average	Unmatched	1121.887	1210.724	−88.837	0.059	278.209	
Elevation (m)	Matched	1121.887	1189.939	−68.052	0.056	125.478	23.4%

Notes: Mean eQQ difference is the mean difference in the empirical quantile-quantile distributions for protected and unprotected municipalities.
Normalized difference in means: $\frac{\overline{X}_1 - \overline{X}_0}{\sqrt{S_1^2 + S_0^2}}$, where the subscripts denote protection status, $\overline{X}$ is the sample mean, and S^2 is the sample variance.
Percent improvement calculates the improvement in mean covariate balance after matching.

4.1.1 Outcome: Forest Cover

An estimate from a simple difference in means of 2010 forest cover between protected and (all) unprotected municipalities implies that protected areas prevented over 19 percentage points of deforestation (first two rows of Table 3). However, as is often noted in the literature (e.g., Joppa and Pfaff (2009); Andam et al. (2008)), such a naive estimator is fraught with selection bias due to the non-random nature of protected area establishment. The simple difference in means can be biased because of state dependency (protected municipalities started with higher levels of forest cover) and fundamental failure to control for the selection process.

The bottom two rows of Table 3 present results for the primary matching specification. By comparison, it is clear that positive bias is present in the naive estimator. Genetic matching results in point estimates that suggest protected areas can be attributed with increasing forest cover by 0.83 percentage points compared to similar, unprotected areas. In other words, protected areas prevented under 1 percentage point of deforestation that would have occurred in their absence. This point estimate is quite low in comparison to other Latin American and Caribbean countries (e.g., Andam et al. (2008); Hanauer and Canavire-Bacarreza (2015); Miranda et al. (2016)) and is statistically insignificant. Taken in isolation, these results imply that Colombia's protected areas were ineffective in achieving their environmental goals.

Table 3: Results from Primary and Ancillary Analyses. For each method $Y(D=1)$ and $Y(D=0)$ represent the average observed and counterfactual measures of poverty for protected municipalities, respectively. Each "Effect"' column is calculated as $Y(D=1)-Y(D=0)$.

	Forest Cover			Poverty		
Method	$Y(D=1)$	$Y(D=0)$	Effect	$Y(D=1)$	$Y(D=0)$	Effect
Naïve Difference	35.297	15.758	19.539***	45.430	43.593	1.873
in Means	[121]	[802]	{1.248e-12}	[121]	[802]	{0.405}
Regression on Raw	35.297	34.435	0.862	45.430	45.136	0.2933
Data	[121]	[802]	(0.719)	[121]	[802]	(1.105)
Post-Match Frequency	35.297	34.867	0.431	45.430	45.567	-0.137
Weighted Regression	[121]	[94]	(1.344)	[121]	[94]	(1.768)
Genetic Matching	35.297	34.540	0.843	45.430	45.145	-0.285
	[121]	[121]	(1.515)	[121]	[121]	(1.905)

*, **, *** indicate that treatment effect estimates are different from zero at 10%, 5%, and 1% levels, respectively.
[Number of observations]
(Standard errors)
{P-value}

4.1.2 Outcome: Poverty

The simple difference in means of 2005 poverty between protected and (all) unprotected municipalities implies that protected areas increased the proportion of households considered as poor by 1.87 percentage points (first two rows of Table 3). Although this result is statistically insignificant, it flies in opposition to results from other countries, in which protected areas have resulted in positive socioeconomic outcomes (e.g., Andam et al. (2010b); Canavire-Bacarezza and Hanauer (2013)). After controlling for the selection process, and the fact that poverty was higher in protected municipalities at baseline, we find insignificant point estimates that protected areas decreased poverty

4.2 Direct and Mechanism Effects

Our primary goal is to estimate the impacts of protected areas on forest cover and poverty, had they not also increased guerrilla activities. We use the $NATT$ to conceptualize and then estimate these direct effects. In doing so, we also estimate the mechanism effects of protection through changes in guerrilla activities on our respective outcomes. We use the $MATT$ to conceptualize and estimate these mechanism effects.

Figure 2 demonstrates the concepts and estimates associate withe both the $NATT$ and $MATT$ for both forest cover and poverty. All calculations in Figure 2 are based on using the average change in number of guerrilla attacks between 1998 (baseline) and 2004 as the mechanism outcome (see Tables 4 and 5 for full results). The center, blue, elaborated causal pathway shows how protection affected guerrilla violence (i), the marginal effect of these changes on the respective outcomes (ii), and how, taken together, these translate into effects on 2010 forest cover and 2005 poverty.

As in Canavire-Bacarreza et al. (2018) we find that protected areas significantly increased the average number of attacks in protected municipalities by nearly two per year (i). We find that the marginal effect of these attacks on forest cover is -0.502 and significant ($p < 0.01$). In other words, each additional guerrilla attack increased deforestation by approximately 0.5 percentage points. Taken together, this implies that, because protection increased guerrilla activities, deforestation was nearly 1 percentage point higher in protected municipalities than it would have been (the $MATT$). In terms of socioeconomic outcomes, the point estimate from our results implies that violence serves to increase poverty (ii) and thus poverty was higher in protected census tracts

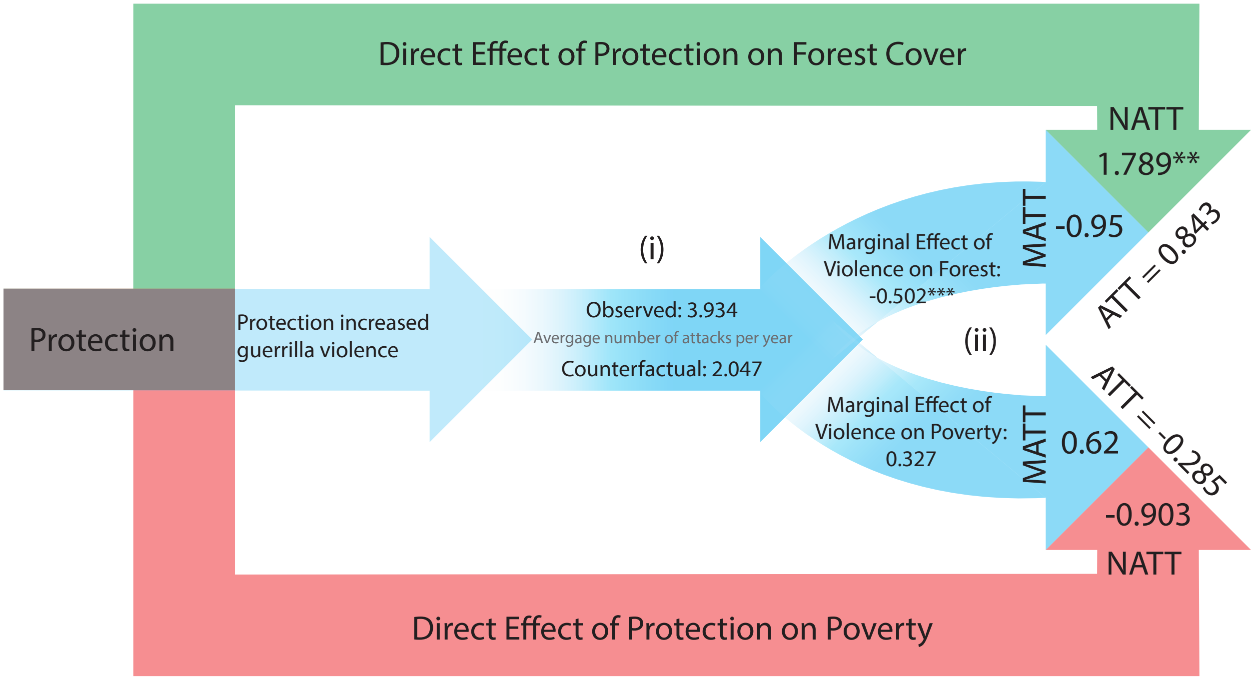


Figure 2: Mechanism and direct pathway estimations using change in average (2002-2004) number of guerrilla attacks as mechanism measure. The extended mechanism pathway shows the observed and counterfactual violence values (mechanism) and the marginal effect of violence on the respective outcomes.

due to the increased guerrilla attacks caused by protection. However, none of these estimates are statistically significant.

The outside pathways (top and bottom) in Figure 2 depict the direct effect of protected areas on the respective outcomes, as measured by the $NATT$. Interestingly, the estimated direct effect of protection on forest cover implies that, had protected areas not affected guerrilla activities, protected areas would have decreased deforestation by 1.79 percentage points. This estimate is statistically significant ($p < 0.05$), more than double the ATT, and more in-line with estimates from other countries in the region (e.g., Bolivia (Hanauer and Canavire-Bacarreza, 2015)). The point estimate for the direct effect of protection on poverty implies poverty alleviating effects of protected areas, and is over three times larger (in absolute terms) than the ATT, but still statistically insignificant.

Full $NATT$ and $MATT$ results for forest cover and poverty can be found in Tables 4 and 5, respectively. For each terminal year of the mechanism outcome the tables show: (i) number of

Table 4: Forest cover mechanism estimation results.

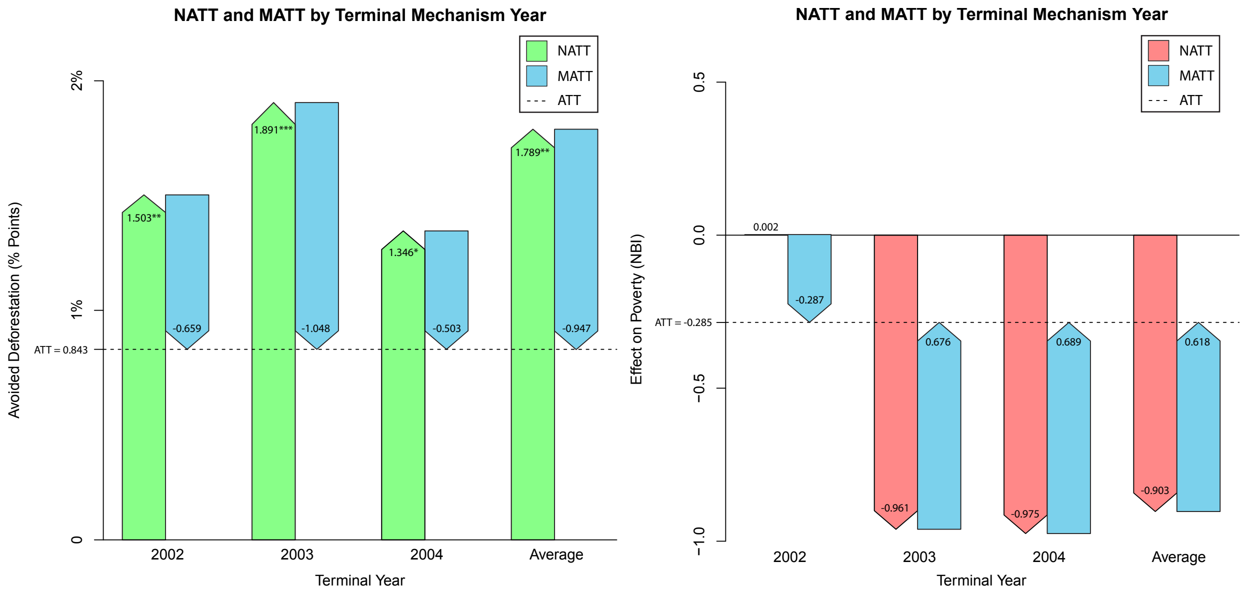
	Outcome: Percentage Forest Cover 2010: Primary $ATT = 0.843$						
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)
Mechanism	Affected Units	Observed Mechanism	Counterfactual Mechanism	Mechanism Coefficient	$MATT$	$NATT$	$LNATT$
Δ Attacks 1998-2002	81	2.950	1.793	-0.570*** [0.167]	-0.659 (0.981)	1.503** (0.760)	0.706 {-4.128,4.709}
Δ Attacks 1998-2003	89	4.686	2.215	-0.424*** [0.103]	-1.048 (0.961)	1.891*** (0.787)	1.797 {-0.934,4.074}
Δ Attacks 1998-2004	83	4.165	2.132	-0.247*** [0.090]	-0.503 (0.944)	1.346* (0.794)	1.789 {-0.797,4.699}
Mean Δ Attacks 1998-2004	103	3.934	2.047	-0.502*** [0.127]	-0.947 (0.962)	1.789** (0.792)	4.329*** {1.226,7.109}

***,**,* represent significance at the 1%, 5% and 10% levels, respectively.
(Heteroskedasticity robust standard errors)
[Standard errors]

Table 5: Poverty mechanism estimation results.

	Outcome: Poverty (NBI) 2005: Primary $ATT = -0.285$						
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)
Mechanism	Affected Units	Observed Mechanism	Counterfactual Mechanism	Mechanism Coefficient	$MATT$	$NATT$	$LNATT$
Δ Attacks 1998-2002	81	2.950	1.793	-0.248 [0.297]	-0.287 (1.460)	0.002 (1.063)	-1.566 {-6.354,3.619}
Δ Attacks 1998-2003	89	4.686	2.215	0.274 [0.187]	0.676 (1.445)	-0.961 (1.050)	-1.636 {-7.956,6.946}
Δ Attacks 1998-2004	83	4.165	2.132	0.339** [0.155]	0.689 (1.445)	-0.975 (1.029)	-2.669 {-7.011,1.388}
Mean Δ Attacks 1998-2004	103	3.934	2.047	0.327 [0.229]	0.618 (1.442)	-0.903 (1.042)	-2.520 {-7.712,3.289}

***,**,* represent significance at the 1%, 5% and 10% levels, respectively.
(Heteroskedasticity robust standard errors)
[Standard errors]



(a) Direct (green) and mechanism (blue) effects of protection on forest cover.

(b) Direct (red) and mechanism effects (blue) of protection on poverty.

Figure 3: Depicts the average (ATT), direct ($NATT$), and mechanism ($MATT$) effects of protection on forest cover (a) and poverty (b) by terminal year of the violence mechanism.

protected municipalities affected by violence during that period, (ii) the observed average number of guerrilla attacks, (iii) the estimated counterfactual average number of guerrilla attacks, (iv) the estimated marginal effect of violence on outcomes, (v) the estimated $NATT$, (vi) the estimated $MATT$, and (vii) the estimated local $NATT$ (see Robustness section).

Figure 3 depicts these full results. In both panels ((a) and (b)), the columns represent the magnitude of the respective effects, the arrows on the columns represent the direction of the effect, the dashed line marks the ATT, and the blue column represents the mechanism effect. Panel (a) shows that, regardless of terminal-mechanism year, all the direct effects of protection are larger (in absolute terms) than the ATT and statistically significant. In most cases, the results imply that reductions in deforestation would have been twice as large in protected municipalities, had protected areas not affected guerrilla activities. Similarly, excepting terminal year 2002, the poverty reducing impacts of protection would have been three times as large, had protected areas not affect guerrilla activities (none of the poverty results are statistically significant).

5 Robustness

5.1 *ATT*

To ensure that our primary ATT estimates are not solely a function of our choice of estimators (genetic matching), we we estimate the average impacts of protected areas on forest cover and poverty using two other regression-based estimators. The main results from these estimators can be found in Table 3 and the full coefficient estimates are presented in Table 6. Results from a regression on the full data set (all unprotected municipalities included) and a post-match frequency weighted (the number of times an unprotected municipality is used as a match for a protected municipality) regression are qualitatively and quantitatively similar to the primary specification. Similar results for the poverty outcome are only slightly different from the primary estimator. In both cases, the point estimates remain statistically insignificant.

5.2 Local Net Average Treatment Effect on the Treated

Estimation of the $MATT$ and $NATT$ requires the imputation of counterfactual mechanism values which are, by definition, unobserved. Our data provide a unique opportunity to estimate the causal effects of protection net of violence under less restrictive assumptions than those used in the main analyses. We exploit the fact that some protected municipalities are observed in the absence of the violence mechanism. For this subset of the data $S_i(1) = S_i(0)$ by definition. In other words, the potential mechanism value for protected units that did not incur violence is 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[5]

$$LNATT = E\{E[Y_i(1, S_i(0)) - Y_i(0, S_i(0)) | S_i(1) = S_i(0) = s_0]\}, \tag{17}$$

for the subset of data in the principal stratum$\{S_i(1) = S_i(0) = s_0\}$ (Flores and Flores-Lagunes,

[5]This framework follows directly from the framework for the local net average treatment effect ($LNATE$) established by Flores and Flores-Lagunes (2011).

2011). The fact that we observe protected municipalities that were not affected by violence means that we can take the simple difference in these protected municipalities 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. Flores and Flores-Lagunes (2011) 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.

Column (vii) in Tables 4 and 5 present the $LNATT$ estimates for forest cover and poverty, respectively. The bracketed numbers under the point estimates are the 95% bootstrapped confidence intervals. The $LNATT$ estimates for forest cover are roughly similar to the $NATT$ estimates across terminal years. The 2002 terminal-year estimates are slightly lower and the $LNATT$ when we average the mechanism across all years is larger and statistically significant. All $LNATT$ results for the poverty outcome are larger, in absolute terms, but remain statistically insignificant. We believe that the similarity between the two estimates provides evidence that the assumptions and methods employed in the main analyses are providing unbiased estimates of the respective direct and mechanism effects.

Table 6: Results from regressions using full dataset and post-match data for each outcome.

	Full Data		*Post Match*	
	% Forest 2010	NBI 2005	% Forest 2010	NBI 2005
	(1)	(2)	(3)	(4)
Protected	0.861	0.293	0.431	−0.137
	(0.719)	(1.105)	(1.344)	(1.768)
% Forest 1993	0.819***	−0.031	0.889***	−0.088*
	(0.012)	(0.019)	(0.037)	(0.049)
Slope	−0.042	0.063	−0.284*	0.033
	(0.043)	(0.067)	(0.152)	(0.200)
Elevation	0.0001	−0.0004	0.0005	0.00002
	(0.0002)	(0.0003)	(0.001)	(0.001)
Dist. Market	0.023***	0.008	0.021*	0.002
	(0.005)	(0.008)	(0.012)	(0.016)
Dist. Capital	−0.006	0.023***	−0.020	0.048***
	(0.005)	(0.007)	(0.013)	(0.018)
Population Density 1993	−0.0001	−0.0003	−0.024	−0.012
	(0.001)	(0.001)	(0.017)	(0.022)
Average Rainfall	0.007**	−0.011**	−0.002	−0.006
	(0.003)	(0.004)	(0.007)	(0.009)
NBI 1993	0.001	0.850***	−0.056	0.883***
	(0.015)	(0.023)	(0.050)	(0.066)
Attacks 1998	−0.127	−0.106	−0.190	0.071
	(0.086)	(0.132)	(0.383)	(0.503)
Constant	−3.699***	−1.764	3.281	−2.923
	(1.022)	(1.572)	(4.390)	(5.774)
Observations	923	923	215	215
R^2	0.898	0.701	0.872	0.658
Adjusted R^2	0.897	0.698	0.865	0.641
Residual Std. Error	6.923	10.646	10.356	13.621
F Statistic	803.554***	213.657***	138.397***	39.203***
Note:				*p<0.1; **p<0.05; ***p<0.01

6 Conclusions

Protected areas are created to conserve biodiversity and ecosystem services. However, it has been shown that they can also have unintended consequences. In certain circumstances protected areas have had negative socioeconomic impacts on surrounding communities (e.g., Hanauer and Canavire-Bacarreza (2015); Ferraro et al. (2011)). In Columbia, specifically, protected areas have been shown to harbor guerrilla groups, such as the FARC. In this study we assess the degree to which such unintended consequences can affect the chartered goals of protected areas.

We begin by estimating the impact that Columbia's protected area system had on forest cover between 1990 and 2010, and on poverty between 1993 and 2005. We find that, on average, protected areas had essentially no impact on either outcome. These findings are particularly out of character for the region on the environmental side. All high-quality evidence from Latin American and Caribbean countries shows that protected areas have significantly reduced deforestation. Given that previous studies showed that Columbia's protected areas increased guerrilla activities (Canavire-Bacarreza et al., 2018), and that guerrilla activities increase deforestation (Fergusson et al., 2014), we ask how deforestation and poverty might have been different in protected areas, had they not also increased guerrilla activities.

We build off findings, and the quasi-experimental approach, of Canavire-Bacarreza et al. (2018) to estimate the direct and mechanism effects of Columbia's protected areas on forest cover and poverty through their effects on guerrilla violence. The direct (or net) effects allow us to answer the question of what forest cover and poverty would have been in areas affected by protected areas, had protected areas not induced increased guerrilla activities? Our results indicate that protected areas would have been approximately twice as effective in reducing deforestation (statistically significant) and had three times the poverty reducing effect (statistically insignificant), had they not also increased guerrilla activities.

Our study and results are important for several reasons. It is the first study to use an innovative quasi-experimental design to estimate direct effects of protected areas. Therefore, we provide a framework for application of such estimators (treatment and precision) to other geographic areas and policies. These results should also be of interest to Colombian policy makers in the current political climate. Future environmental policy will ideally be guided by evidence on past performance.

However, under the recently signed peace accord, our estimates of the direct effects of protected areas are likely more indicative than average effect estimates of how protected areas will perform in the future.

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