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Forest law enforcement through district blacklisting in the Brazilian Amazon

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Abstract

Deforestation in the Brazilian Amazon has dropped substantially after a peak at over 27 thousand square kilometers in 2004. Starting in 2008, the Brazilian Ministry of the Environment has regularly published blacklists of critical districts with high annual forest loss. Farms in blacklisted districts face stricter registration and environmental licensing rules. In this paper, we quantify the impact of blacklisting on deforestation. We first use spatial matching techniques using a large set of covariates to identify appropriate control districts. We then explore the effect of blacklisting on change in deforestation in double difference regression analyses using panel data covering the period from 2002-2012. Several robustness checks are conducted including an analysis of field-based enforcement missions as a potential causal mechanism behind the effectiveness of the blacklist. We find that the blacklist has considerably reduced deforestation in the affected districts even after controlling for in situ enforcement activities.

Keywords: deforestation, impact evaluation, matching

JEL codes: Q32, Q15, Q38, Q5, H43

1. Introduction

Brazil stands out as one of the few countries in the world, where tropical deforestation rates have dropped over the past decade (Hansen et al. 2013). Emerging evidence from semi-experimental evaluation studies on the effectiveness of Brazil's post-2004 strategy to combat Amazon deforestation unambiguously suggests that environmental policy has come to play a major role in determining land use decisions in the region (J. Assunção et al. 2012; CEPAL-IPEA-GIZ 2011; Hargrave and Kis-Katos 2013). Apart from a substantial expansion of the region's protected area network, field-based law enforcement operations targeted to deforestation hot-spots by using improved remote sensing technologies have been among the major short-term success factors (Juliano Assunção et al. 2013a). Between late 2007 and early 2008, Brazil has introduced two additional measures to reinforce *in situ* enforcement action. Resolution 3.545 published in 2008 by the Brazilian Monetary Council (Conselho Monetário Nacional) limits credit access for farms that are non-compliant with the Brazilian Forest Code and establishes best-practice rules for offenders to re-access credit flow. Assuncao et al. (Juliano Assunção et al. 2013b), estimate that this measure has avoided 2700 square kilometers of deforestation between 2009 and 2011. The Presidential Decree 6.321 (December 2007) created the legal basis for a list of priority municipalities, henceforth districts, with outstanding historical deforestation rates. In "blacklisted" districts, stricter rules with regard to the authorization of forest clearing applied and defined administrative targets (see details below) had to be fulfilled to qualify for removal from the list.

Both decrees essentially operate as cross-compliance measures, where access to public services or administrative rights at farm or district level is made conditional on compliance with forest law. In this paper we apply semi-experimental evaluation techniques to gauge the role that district blacklisting has played in the overall contribution of Brazil's policy mix to combat Amazon forest loss. We find that, on average, blacklisted districts have experienced distinctly larger reductions in deforestation than comparable non-listed districts and produce evidence that this difference is partially a genuine effect of blacklisting.

The paper is structured as follows. Below, we describe key elements of the Brazilian blacklisting strategy. We also discuss the potential mechanisms and pathways through which blacklisting might have contributed to reducing deforestation beyond the combined effect of other policy instruments (theory of change). Section 2 summarizes our empirical strategy to estimate the effect of blacklisting on deforestation. Section 3 documents our data sources and section 4 presents main results and robustness checks. In section 5 we discuss potential caveats of our analysis in the context of the emerging literature evaluating conservation programs and section 6 provides conclusions and implications for conservation policy design.

History and impact logic of the Brazilian district blacklist

Decree 6.321, published in December 2007, clearly defines the objective of the blacklist as a strategy to monitor and control illegal deforestation and prevent land degradation. It states that the list is to be updated annually based on official deforestation statistics and specifies the complementary roles of IBAMA and the National Institute for Agrarian Reform (INCRA) in monitoring and registering landholdings in the blacklisted districts. Three criteria are put forward as being used (without further specification) to compose the blacklist, namely:

1. The total deforested area
2. The total deforested area of the preceding three years
3. The increase of deforestation of minimum three out of the past five years

Figure 1 schematically depicts how the blacklist has evolved since the publication of Decree 6.321.

[Figure 1. History of district blacklisting and blacklist criteria.
Positive numbers in parentheses depict additions to the blacklist. Negative numbers depict removals.]

In January 2008, the first blacklist was published covering 36 districts. Seven districts were added in each of the years 2009 and 2011. Only six districts were removed until 2012. Removal was conditioned on registering at least 80% of the eligible area (mostly privately claimed land) under the CAR. Moreover, annual deforestation had to be kept below 40 sqkm.

District blacklisting probably qualifies as the most innovative element in Brazil's multi-instrument conservation policy mix. To our knowledge no other country has yet applied a similar institutional cross-compliance mechanism in the forestry sector. The impact pathway of blacklisting is still unclear and very little research on blacklisting as a governance mechanism exists. Jacobs and Anechiarico (1992) argue that contractor blacklisting is a sensible and ethically justifiable strategy to protect government organizations from fraud. China has experimented with an environmental disclosure policy including the publication of lists of violators of environmental regulations. A recent study found that this blacklisting strategy has helped in engaging civil society stakeholders in environmental governance (Tan 2014). The study, however, concluded that effects on behavioral change have been limited due to the country's authoritarian structure. In 2010, a synthesis report by the Transparency and Accountability Initiative found that transparency and accountability policies have considerable potential to make a improve governance in sectors, such as public service delivery, natural resource governance, and donor aid (McGee and Gaventa 2010). Similar findings on public disclosure policies are

2. Empirical Strategy

The methodological challenge of evaluating the effect of the blacklist on deforestation in the blacklisted districts consists of identifying an appropriate counterfactual scenario of what would have happened in the absence of the blacklist (Khandker et al. 2010). From the previous section, we know that blacklisting was not random. Instead, regulators have used defined selection criteria that were all linked to historical deforestation. Regression Discontinuity Design (RDD) is a commonly used evaluation technique for interventions were the selection mechanism is known (Hahn et al. 2001). Unfortunately, the exact approach used to arrive at the published blacklists was never made public. Although past deforestation highly correlates with selection, it is not possible to reproduce the first list of 38 districts based on the three published selection criteria alone. We can thus only speculate, which other criteria could have played a role in composing the blacklist. Moreover, our sample of treated districts is too small for informative local linear regression analyses in an RDD.

A frequently used quasi-experimental evaluation technique in the presence of unknown selection mechanisms is matching (Andam et al. 2008; Gaveau et al. 2009; Ho et al. 2007; Honey-Rosés et al. 2011; Paul R. Rosenbaum and Rubin 1983). Matching relies on propensity scores or other distance measures that are derived from observed characteristics of treated and non-treated observations (here districts). Treated observations are paired with "similar" non-treated (or control) observations to reduce the bias in treatment effect estimations. A strong assumption of the matching estimator is unconfoundedness, i.e. one assumes that no other than the observed criteria were relevant in selecting districts into the blacklist. Moreover, matching requires that there is a considerable region of overlap in the distance measures or propensity scores of treated and non-treated observations of the sample. While we are able to control for a large number of potential selection criteria (see below), our sample of non-blacklisted districts is unlikely to be a satisfactory pool of potential controls, because most blacklisted districts have indeed been among the highest deforesting districts in the Brazilian Amazon region before the blacklist was enacted. Matching can, nonetheless, help us to identify an appropriate set of control observations and thus represents a sensible preprocessing step in our evaluation strategy (Ho et al. 2007).

Since the group of potential control districts is likely to exhibit lower pre-treatment levels in deforestation than the treated districts we will rely on the double difference method to ultimately estimate the treatment effect of blacklisting (Hargrave and Kis-Katos 2013; Khandker et al. 2010). A critical assumption of the double difference method is that treated and control observations exhibit parallel time trends in the outcome variable (time invariant heterogeneity). In other words, absent blacklisting, we assume that treated and control districts would have had the same change in deforestation over time even though they exhibit different absolute levels in forest loss. While we cannot test whether this assumption holds, it is possible to explore the implications of some forms of violations in robustness tests (see below).

Following (Jalan and Ravallion 1998), we derive the double difference estimator for our purpose as follows. Using log deforestation as outcome variable, the panel fixed effect can be written as:

$$\ln Def_{it} = B_{it}'\beta + X_{it}'\gamma + tZ_i'\delta + \alpha_i + \eta_t + u_{it} \qquad \text{Eq. (1)}$$

where B_{it} is treatment variable indicating whether the municipality i has been blacklisted in a given year t, X_{it} the vector for time-varying covariates, Z_i the vector for time-invariant covariates (or the so-called "initial conditions"), α_i is the municipality-specific fixed effect, η_t the year-specific treatment effect, and u_{it} the error term. Initial conditions are interacted with the time variable t. We are interested in the average treatment effect β.

Both fixed effect and first difference estimators can be used, but we proceed with the first difference estimator that is less prone to serial correlation (Verbeek 2012, pg. 349). Taking first differences, Eq. 1 becomes:

$$\Delta \ln Def_{it} = \Delta B_{it}'\beta + \Delta X_{it}'\gamma + Z_i'\delta + \Delta\eta_t + \Delta u_{it} \qquad \text{Eq. (2)}$$

where the municipality-specific fixed effect is canceled out and the initial conditions stay in the equation as time-invariant covariates ($\Delta t = 1$). Other than in (Jalan and Ravallion 1998), where a single time trend is assumed for all periods, we assume year-specific fixed effects.

Our timeframe of analysis covers all years between 2002 and 2012. Deforestation is measured over the period from August and July and we adjust all explanatory variables accordingly. Treatment indicators have to account for the fact that blacklists were released at different points in the year. The first list of 38 districts was published in February 2008. Hence, we set treatment B_{it} to 0.5 to represent the six months during which blacklisting could have affected deforestation in 2012. The second and third blacklists were published in April 2009 and May 2011 and the respective treatments are set to 0.25 and 0.17 (see Eq. 3 below). The 4th list was published in October 2012 and thus outside our analytical timeframe.

$$B_{it} = \begin{cases} (0,1) & \text{in 1st year of blacklisting} \\ 1 & \text{in 2nd and all subsequent years after blacklisting} \\ 0 & \text{otherwise} \end{cases} \qquad \text{Eq. (3)}$$

The treatment coefficient β measures the average change in deforestation due to blacklisting for all years after treatment. Hence, we initially assume a constant influence of blacklisting throughout the timeframe of analysis, but will later also analyze dynamic effects.

Confounding factors that could affect deforestation are considered in the covariates vectors X_{it} and Z_i of Eq. 2. Our choice of covariates is based on previous empirical work on tropical deforestation in the Amazon region and beyond (Aguiar et al. 2007; Andersen 1996; Araujo et al. 2009; Arima et al. 2007; Hargrave and Kis-Katos 2013; Kaimowitz and Angelsen 1998; Pfaff 1999). We broadly distinguish between (1) time invariant, (2) time varying, and (3) mechanisms with potential effects on our outcome variable (deforestation). Since clouds represent a significant source of measurement error in remotely sensed deforestation data, we include cloud cover in all regression analysis and report the respective coefficient estimates. Descriptive statistics of all variables used in regressions are reported in Table 2 below.

Among time invariant covariates, we consider various measures of deforestation and forest cover up until the beginning of our 2002-2012 time frame and control for district size and population density. Moreover, we control for farm characteristics, indicators of agricultural intensification, average land values, and average travel distance to district cities, which have shown to be important predictors of deforestation in previous studies (Angelsen and Kaimowitz 2001; Pfaff 1999).

Among time varying predictors, we consider GDP per capita, timber and soy prices (zero in districts without soy production) (see also, Hargrave and Kis-Katos 2013), and the area of settlements, protected areas, and indigenous territories in each district. All these tenure categories have been found to affect deforestation rates in previous studies (Ezzine-de-Blas et al. 2011; Soares-Filho et al. 2010). In addition, we control for political factors by introducing dummy variables indicating whether districts are governed by the Brazilian Workers Party (dominating political party at federal level during most of the studied time frame). As mechanism through which the blacklist could have affected deforestation we consider the number of field-based inspections by the environmental protection agency registered in each district per year.

The panel data models are implemented in R using the function “plm” from the “plm” package (Croissant and Millo 2008; R Core Team 2012). For post-matching regressions we run a placebo analysis to test whether results are simply an artifact of selection bias. Placebo tests are reported in Appendix (SI Table 1.1).

Details on the approaches used to analyze, dynamic treatment effects, spatial spillovers, and causal mechanism effects are provided in the respective subsections below.

3. Study area and data

Our study area is the Legal Brazilian Amazon, an area of approximately five million square kilometers that extends into nine Brazilian states. Figure 2 depicts the study area highlighting changes in average deforestation in blacklisted and non-blacklisted districts after the cut-off point in 2008, when the Decree 6.321 was enacted.

[Figure 2 here]

From Figure 2 it becomes clear that the blacklisted districts have experienced the largest reductions in average annual deforestation from the period 2003-2007 to the period 2008-2012. Large increases in average deforestation almost exclusively occurred in non-blacklisted districts, but many of the latter also experienced reductions in forest loss.

Table 1 summarizes the data sources used in this study. Table 2 presents descriptive statistics for all variables used in for the empirical analysis. The Brazilian Legal Amazon district database from the Brazilian Institute for Geography and Statistics (IBGE) covers 771 districts. To avoid bias introduced by districts with no or negligible forest cover, e.g. in the Amazon/Cerrado ecotone, we exclude 312 districts (none of which was blacklisted) by restricting the sample to districts with a minimum initial forest coverage of 10% in 2002.

[Tables 1 + 2 here]

4. Results

Descriptive analysis and baseline regressions

Figure 3 depicts average deforestation (left panel) and average year-to-year increase in forest loss for blacklisted and non-blacklisted districts during our study period. Average deforestation in blacklisted districts exhibits a much faster decrease than deforestation in untreated districts, but substantial decreases already occurred before the blacklist was enacted in 2008, for example between 2004 and 2005. The right panel of Figure 3 shows that average year-to-year percentage changes in deforestation were constantly lower in blacklisted than in control districts after 2006.

[Figure 3]

We, nonetheless, start our analysis with all observations in a series of baseline models using the specification in Eq. 2 and gradually adding covariate groups (Table 3).

[Table 3 here]

All models are balanced panels, but due to missing values in some time invariant variables (see Table 2) some observations are dropped in models (2-4). In model (3) we have to omit the year 2012 and model 4 discards both the years 2012 and 2002.

All four models yield similar results with large and highly significant average treatment effects. The two-sided Durbin Watson test for serial correlation indicates serial correlation only for model (1). Not shown in Table 3: Year effects are negative with the exception of 2008 and 2010 and among the time invariant covariates cumulated deforestation in 2002, tractor density, and land value per hectare are negatively associated with change in deforestation. Among time varying covariates, the timber price is negative and the settlement area positively associated with deforestation. Hargrave and Kis-Katos (Hargrave and Kis-Katos 2013) report similar results with regard to timber prices and argue that high value timber could boost long-term investment in forest and therefore contribute to lower deforestation.

Model 4 includes the lagged number of field inspections as a potential external mechanisms through which blacklisting could have affected deforestation. The coefficient is insignificant, but the role of field inspections as a causal mechanism will be further investigated below.

Post-matching regressions

As discussed above, regression models tend to be less prone to misspecification and selection bias when data is preprocessed using matching techniques. As matching covariates we use the official blacklisting criteria reported by the Brazilian authorities (Figure 1) and further include a large group of variables (including size and land use variables, economic conditions, and conservation policies). Matching is implemented in R using the “Matching” package (Sekhon 2011) and the Mahalanobis distance measure. A comparison of the covariate balance before and after matching is provided in Table SI 2.1 (Appendix). For almost all variables, the standard mean difference has greatly improved after matching. However, significant imbalances still exist and thus a simple comparison between the means of blacklisted and matched non-blacklisted groups would likely be biased. Figure SI 2.1 in the Appendix compares average year-to-year change in deforestation and average deforestation trends separately for blacklisted, matched non-blacklisted, and un-matched non-blacklisted districts. After matching, treated and control districts do exhibit very similar pre-blacklist deforestation trends, which makes us confident that the critical assumptions for our subsequent double difference regression are likely to hold.

We use the matched dataset to re-estimate baseline models (3) and (4) in Table 3, which we consider the most adequate specifications. Results are presented in Table 4. Note again that model (4) only considers the years from 2003-2011.

[Table 4 here]

Using the matched dataset, both models show improved goodness of fit. At the same time, all time invariant covariates cease to be significant. Among time varying covariates only timber prices (negative sign) and the indicator variable for the district mayor's term (positive) are significant in both models. Both models also now suggest the same average treatment effect, which corresponds to a 29% decrease in annual deforestation in blacklisted district as a result of blacklisting.

We test our main results using alternative matching techniques. We compare the results from our preferred matching approach to (1) a one-to-one matching on propensity scores, (2) a one-to-two matching on the Mahalanobis distance, and (3) a one-to-one matching on the Mahalanobis distance using only the official selection criteria to the blacklist. The blacklisting effect stays significant are slightly higher in size (Results not presented here). Our preferred one-to-one matching with replacement on the Mahalanobis distance with an extended set of covariates turns out to be the most conservative version to estimate the effect of blacklisting.

Dynamic treatment effects

As discussed previously, several blacklists were published over time and some districts were removed from the lists in the process. Laporte and Windmeijer (Laporte and Windmeijer 2005) show how delayed response to treatment can lead to substantial differences in treatment effects in the post-treatment periods. In this section we allow for dynamic treatment effects. We do not consider anticipation effects prior to treatment though, because the period lying between the publication of Decree 6,321 and the first blacklist was too short to have resulted in significant effects on deforestation as measured by the INPE-PRODES program (see data sources Table 1). To account for dynamic treatment effects we split the blacklisting dummy into several dummies as follows:

$$\Delta \ln Def_{it} = \Delta B_{it}'\beta_0 + \Delta B_{it+1}'\beta_1 + \Delta B_{it+2}'\beta_2 + \Delta B_{it+3}'\beta_3 + \Delta X_{it}'\gamma + Z_i'\delta + \Delta\eta_t + \Delta u_{it}$$

Eq. (4)

B_{it} is between 0 and 1 as for the year of blacklisting and zero for all subsequent years. The treatment variables B_{it+1}, B_{it+2} and B_{it+3} are set to one only in the first, second and third year after blacklisting respectively, for each blacklisted district. We thereby capture the effect of blacklisting over the years. The treatment coefficients β_0 to β_3 can be interpreted as the average effect of blacklisting on deforestation for the respective year after blacklisting. Results for models (3) and (4) are shown in Table 5. Model (3) suggest that blacklisting has significant effects on deforestation in all subsequent years. In model (4) only the coefficients for the second and third year after blacklisting are significant. For the first year after blacklisting in model (4), clustering standard errors at district level increases the p-value from 0.02 to 0.11. Overall, the dynamic treatment effect are rather stable over time, i.e. considering standard errors individual year effects are not significantly different from each other.

[Table 5 here]

Spatial spillover effects

Spatial spillover effects, such as leakage or deterrence, could bias our treatment effect estimation. In our sample, 129 out of the 408 non-blacklisted districts have had at least one blacklisted neighbor district. Leakage could take place if the blacklist encouraged deforestation agents to move to neighboring non-blacklisted districts. However, it is also possible that the fact of having a blacklisted neighbor district deters land users in non-blacklisted districts from deforesting. In the case of leakage from blacklisted to neighboring non-blacklisted districts we would overestimate the effect of blacklisting on deforestation, especially if these districts are part of our matched set of control districts. If deterrence effects of blacklisting were leading to more conservation in neighboring districts, we would underestimate the effect of blacklisting both in blacklisted districts and at the regional scale.

We account for spatial leakage effects from blacklisting by introducing a neighboring treatment effect as follows:

$$\Delta \ln Def_{it} = \Delta B_{it}'\beta + \Delta NB_{it}'\varphi + \Delta X_{it}'\gamma + Z_i'\delta + \Delta\eta_t + \Delta u_{it} \qquad \text{Eq. (5)}$$

Our main interest lies in the effect of blacklisting on neighboring districts that have not been blacklisted, φ. The neighbor effect NB_{it} is set equal to one when a district is not blacklisted and has at least one blacklisted neighbor and becomes zero otherwise. Consequently B_{it} and NB_{it} are mutually exclusive, i.e. they can only be jointly zero but not jointly one. Table 6 reports results for model specifications (3) and (4) as in the previous section for both the pre and the post-matching data sets.

[Table 6 here]

We find evidence pointing to a significant spillover effect on non-blacklisted neighbors of blacklisted districts in the unmatched sample (see left columns in Table 6). The negative sign of the newly introduced neighbor dummy variable suggests that blacklisting a district may have deterrence effects on deforestation also in neighboring non-blacklisted districts. The spillover effect, however, ceases to be significant when we run the same model with the matched data set (right columns in Table 6). The post-matching regression models do not capture the spillover effect, because the matched control group consists predominantly of direct neighbors of blacklisted districts. Most districts that are neither blacklisted nor have one or more blacklisted neighbors are dropped in the process of matching. This finding suggests that the treatment effects estimated in Tables 4 and 5 are probably biased, because matched control districts tend to be neighbors of blacklisted districts. The bias, however, leads us to under rather than overestimate the effect of blacklisting in blacklisted districts and the size of the coefficient for the "Neighbor blacklisted" variable in Table 6 gives us an indication of the size of the bias.

Blacklisting and field-based enforcement

Above we have produced evidence that the drop in deforestation after 2007 was much more pronounced in blacklisted districts than in other Amazonian districts. However, our analysis does not allow for conclusions with respect to the causal mechanism behind the effect of blacklisting on deforestation. In section 1 we have discussed potential impact channels or mechanism that could have played a role in reinforcing the effectiveness of blacklisting. One of these mechanisms is the practice of *in situ* field inspections that were shown to have played an important role in Brazil's efforts to reduce Amazon deforestation (Juliano Assunção et al. 2013a; Hargrave and Kis-Katos 2013).

While we have controlled for the number of field inspections in model (4), our estimator may still be biased if field inspections were actually affected by blacklisting (Paul R Rosenbaum 1984). To avoid this bias we need an empirical approach that allows us to determine (1) what the number of field inspections would have been in the absence of blacklisting and (2) what the effect of blacklisting would have been, had there not been any effect on field inspections. Based on a method proposed by Flores and Flores-Lagunes (Flores and Flores-Lagunes 2009), Ferraro and Hanauer (Ferraro and Hanauer 2014) have recently addressed similar questions in the context of protected areas. The isolated effect of a mechanism is called the mechanism average treatment effect (MATT). The remaining effect of blacklisting is called the net average treatment effect (NATT).

Beyond the assumptions made up to this point, two additional assumptions are necessary to estimate MATT and NATT: (1) Expectations that blacklisting will increase the density of field inspections in blacklisted districts have not influenced selection onto the list, and (2), changes in the number of field inspections have the same effect in districts where blacklisting has affected the number of field inspections and in districts were it has not. The second assumptions could theoretically be violated, for example, if field inspections in blacklisted districts would somehow have been of a different nature than inspections in other districts. Our data does not contain any information in that regard.

To gauge the potential mechanism effect of field inspections, we estimate the NATT with the mechanism effect blocked (i.e. holding field inspections at the counterfactual level). The difference between the overall average treatment effect measured above and the NATT is then the mechanism effect of field inspections.

The implementation involves three steps:

1. We restrict the sample to the 50 blacklisted districts and run Model 5 for the post-treatment period (2008-2011). This gives us a set of coefficients including the effect of lagged fines on deforestation. I.e., in order to avoid a potential reverse causality between deforestation and the number of issued fines, we use fines from the previous year (t-1).

2. Second, we set the number of field inspections in the blacklisted districts to counterfactual levels, i.e. the number of fines from the matched paired non-blacklisted districts to the blacklisted districts. All other variables keep their original values. With the new values and the point estimates from step (1), we predict the counterfactual deforestation level for the blacklisted districts. Under the assumptions made above, the counterfactual deforestation represents the level of deforestation had there been no change in field inspections as a result of blacklisting.

3. We re-estimate model (5) with the matched data set used in Table 4 and deforestation as well as fine levels modified as described in step (1) and (2) to arrive at the NATT of blacklisting.

This last step is done for both immediate treatment effect and the dynamic treatment effect model.

Results are reported in Table 7.

[Table 7 here]

As expected, the net treatment effect estimates for blacklisting in Table 7 are significant (with p=0.104 for the variable “blacklisted in t=0”) and smaller than the “gross” treatment effects in Tables 5 and 6. Due to the size of the standard errors, however, we cannot safely conclude that there is a significant post-treatment mechanism effect of field inspections that would bias our average treatment effect estimations. While we acknowledge the possibility of such an effect, we believe it is unlikely that it dominates in the overall effect of blacklisting.

5. Discussion

We have found a robust and strongly significant negative effect of district blacklisting on deforestation. As we discuss in the introduction, there are several potential pathways, through which we could theoretically explain this result. Given data limitations, we were only able to formally test for the role of field-based enforcement missions as a potential causal mechanism behind blacklist effectiveness. Inspections, however, turned out to be less important in explaining deforestation reductions in the blacklisted districts than we had expected. Administrative disincentives, reputational risk, and positive external support thus remain as potentially jointly effective causal mechanisms behind the effect of the Brazilian blacklist that could be explored in further research.

As potential rival explanation for our findings we have to consider the credit restriction imposed by the Brazilian Monetary Council in the same year, in which the first blacklist was published (Juliano Assunção et al. 2013b). Note however, that the credit policy covered the whole Brazilian Amazon biome, where also most of our matched control districts are located. Only 5 control districts (and 7 blacklisted districts) extend into the part of the Legal Brazilian Amazon that is not considered Amazon biome.

Like any quasi-experimental evaluation, our analysis remains prone to unobservable bias. One important potential source of bias would, however, lead us to under rather than overestimate the conservation effect of blacklisting. Since blacklisting is endogenously determined by deforestation, a naïve comparison of deforestation rates (see Figure 3) clearly suggests higher deforestation in blacklisted than in non-blacklisted districts. Due to limited common support, this bias could not be fully corrected for by matching, which is why we only rely on matching as a pre-processing technique (Ho et al. 2007).

On the other hand, we would indeed overestimate the negative effect of blacklisting on deforestation, had blacklisted districts exhibited a faster decrease in deforestation in the unobserved counterfactual scenario than the non-listed control districts (parallel time trend or time invariant heterogeneity assumption). A related common evaluation pitfall, termed "Ashenfelter's or pre-program dip", can occur if selection is affected by unusual pre-program changes in the outcome variable (Heckman and Smith 1999). In our case, a pre-blacklist peak in deforestation could hypothetically have resulted in a selection of districts that would have exhibited much faster decreases in deforestation - even in the absence of blacklisting - than any potential control district. While we cannot completely rule out such a phenomenon, we argue that it is unlikely to play a major role in explaining our findings. First, because we control for past increases in deforestation rates in our matching exercise and both pre and post-matching differences in the number of increases in deforestation in the period 2002-2007 are rather small. Second, because the blacklist was enacted five years after average deforestation had peaked in the blacklisted districts (see Figure 3). In the two years prior to the publication of the blacklist, deforestation trends had instead been remarkably similar in treated and control districts. And third, the blacklisted districts have been leading deforestation rankings even prior to our observation period. Hence, and as supported by our placebo treatment analysis (Table SI 1.1), the substantial drop in average forest loss in these districts after 2008 can hardly be attributed solely to normalization after an unusual peak.

We are thus confident that our analysis correctly identifies the blacklist as an environmental governance measure that made a substantial complementary contribution to bringing deforestation down in the Brazilian Amazon region.

6. Conclusions

In this study we have used a quasi-experimental evaluation design to gauge the potential contribution of district blacklisting to the drop in deforestation rates in the Brazilian Amazon. Blacklisting has been used in other environmental governance contexts (McGee and Gaventa 2010), but we are unaware of any attempt at quantifying the effect of blacklisting through counterfactual-based evaluation.

We find that the average effect of blacklisting on deforestation in blacklisted districts ranges between roughly 14-36%. Based on the average own treatment effect estimated by model (3) in Table 5, this corresponds to an absolute reduction in deforestation of roughly 4500 sqkm between 2008-2012. While this is less than the cumulated effects of improved field-based enforcement calculated by (Juliano Assunção et al. 2013a; Hargrave and Kis-Katos 2013), it is more than the amount of avoided deforestation (2700 sqkm) that (Juliano Assunção et al. 2013b) attribute to the credit restrictions that were enacted in the same year as the blacklist.

In other words, until 2012, the decision to bolster the Brazilian anti-deforestation campaign by district blacklisting has avoided almost 80% of one year's deforestation in the whole Brazilian Amazon, where annual deforestation rates have been fluctuating around 5600 sqkm since 2011.

At federal level, the incremental administrative costs of maintaining the blacklist have probably been low. However, the blacklist has reportedly induced a substantial amount of local level transaction costs and operational expenses by supporting NGO and state-level government organizations. Putting a price tag on the Brazilian blacklisting experience is thus not a straightforward exercise.

Given the scarce evidence on the effectiveness of transparency and accountability measures, our results should nonetheless encourage experimentation with blacklisting as a complementary forest conservation measure. Clearly, a country's administrative structure is likely to affect outcomes in significant ways. For example, Brazilian districts (i.e. municipalities) do not have environmental policy mandates as opposed to the more decentralized governance structure in other tropical forest countries, such as Indonesia (Luttrell et al. 2014). The effectiveness of the diverse potential impact channels of blacklisting may thus differ substantially depending on the ability of local stakeholders to organize towards the goal of being removed from a blacklist.

From a national government's point of view, including in the context of an international mechanism to Reducing Emissions from Deforestation and Degradation (REDD+), blacklisting would appear as a low-cost and no-regret option to increase compliance with forest law.

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Tables and Figures

Table 1: Data sources

Variable	Year(s)	Source
Blacklist additions and removals	2008-2012	Decree 6.321/2007 and Provision 28/2008, Provision 102, 203/2009, Provision 66,67,68/2010 , Provision 138, 139, 175/2011, Provision 187,322,323,324/2012 (Uniao)
Deforestation and clouds	2002-2012	INPE-PRODES (INPE-PRODES)
Municipality list and borders	2007	IBGE (IBGE)
Protected areas	2002-2012	IBAMA (IBAMA)
Indigenous areas	2002-2012	IBAMA (IBAMA)
Settlement areas	2002-2012	INCRA (INCRA)
Mayors' party affiliation	2002-2012	TSE (Eleitoral)
IPCA price deflator	2002-2012	IBGE (IBGE)
Soy prices	2002-2012	IBGE-PAM (IBGE-PAM)
Timber prices	2002-2012	IBGE-PEVS (IBGE-PEVS)
GDP	2002-2011	
Number of farms	2006	IBGE (IBGE)
Share of land owners	2006	IBGE Agricultural Census (Agropecuário 2006)
Land value per ha	2006	
Number of tractors	2006	
Cattle stocking rate	2006	
Population	2007	IBGE Demographic Census(IBGE 2000)
Average distance to district center		Nelson (Nelson 2008)
Field-based law enforcement inspections	2002-2010	Hargave and Kis-Katos (Hargrave and Kis-Katos 2013) and Börner et al. (submitted - this issue)

Table 2: Descriptive statistics of variables (2002-2012) used in empirical analyses

Variable	N	Mean	St. Dev.	Min	Max
Dependent					
ln deforestation	5,038	2.21	1.49	0.00	7.18
Treatment					
blacklisted	5,038	0.04	0.19	0.00	1.00
Correction for measurement errors					
ln clouds (sqkm)	5,038	1.88	2.70	0.00	10.89

Time invariant					
ln deforested area in 2002 (sqkm)	5,038	19.92	2.68	0	23.05
ln district total area (sqkm)	5,038	8.29	1.34	4.16	11.98
ln forest area in 2002 (sqkm)	5,038	7.42	1.84	3.37	11.92
ln area under farms in 2005 (ha)	4,950	11.4	1.36	4.73	14.19
ln population density in 2007 (persons/sqkm)	5,038	1.43	1.41	-2.36	7.05
ln farm density in 2005 (farms/sqkm)	4,950	-1.57	1.49	-6.13	2.51
ln share of small farms in 2005 (%)	4,950	-0.41	0.39	-3.8	-0.01
ln tractors in 2005(units per district)	4,950	0.12	0.23	0	2.18
ln stocking rate in 2004 (heads/ha of pasture land)	4,917	0.15	0.81	-6.95	3.46
ln share of land owners in 2005 (%)	4,950	4.23	0.45	1.5	4.61
ln land value in 2005 (BRL/ha)	4,928	6.83	0.8	4.38	8.92
ln average distance to district center (hours)	4,950	6.19	0.97	2.61	8.51
Time varying[1]					
GDP per capita (BRL/capita)	4,580	94.72	110.02	13.14	1,501.61
Soy price (BRL/ton)	5,038	0.001	0.003	0	0.02
Timber price (BRL/cubic meter)	5,038	0.97	0.9	0	9.6
ln indigenous area (skqm)	5,038	2.84	3.57	0	11.43
ln multiple use protected area (skqm)	5,038	2.76	3.51	0	10.77
ln strictly protected area (skqm)	5,038	1.46	2.91	0	10.5
ln settlement area (skqm)	5,038	4.91	2.68	0	10.21
party affiliation (binary)	5,038	0.11	0.3	0	1
Mechanisms					
ln field inspections in t-1 (Number)	4,122	1.55	1.38	0.00	6.32

[1]Monetary figures are in 2012 Brazilian Reais (BRL), 1 BRL corresponded to USD 0.56 on average in 2012 (www.oanda.com).

Table 3: Deforestation and blacklisted municipalities, full sample first difference regressions

	Dependent variable:			
	log of deforestation			
	(1)	(2)	(3)	(4)
Blacklisted	-0.821***	-0.576***	-0.597***	-0.540***
	(0.098)	(0.105)	(0.123)	(0.124)
Log of cloud area	0.031***	-0.029***	-0.022***	-0.022***
	(0.007)	(0.006)	(0.006)	(0.006)
Year effects	YES	YES	YES	YES
Time invariant covariates		YES	YES	YES
Time variant covariates			YES	YES
Number of field inspections				YES
Observations	5,038	4,460	4,014	3,568
Adjusted R^2	0.408	0.135	0.146	0.159
2-sided Durbin-Watson-Statistic	1.171	2.393	2.401	2.471
DW test p-value	<0.000	0.479	0.479	0.479

*Note:*The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Time invariant and variant covariates include first differences of the variables reported in Table 3. *,**,*** denote significance at the 10/5/1% level.

Table 4: The effect of blacklisting after matching, first difference regressions

	Dependent variable:	
	log of deforestation	
	(3)	(4)
Blacklisted	-0.308**	-0.301**
	(0.149)	(0.151)
Log of cloud area	0.008	0.008
	(0.013)	(0.013)

Year effects	YES	YES
Time invariant covariates	YES	YES
Time variant covariates	YES	YES
Number of fines		YES
Observations	900	800
Adjusted R^2	0.300	0.302
2-sided Durbin-Watson-Statistic	2.566	2.623
DW test p-value	0.479	0.479

Note: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Time invariant and variant covariates include first differences of the variables reported in Table 3. Observations are selected by a 1:1 closest neighbor matching on the Mahalanobis distance measure, with replacement. *,**,*** denote significance at the 10/5/1% level.

Table 5: Dynamic effects of blacklisting

	Dependent variable:	
	log of deforestation	
	(3)	(4)
blacklisted in t+0	-0.495*	-0.473
	(0.290)	(0.296)
blacklisted in t+1	-0.341**	-0.328**
	(0.140)	(0.143)
blacklisted in t+2	-0.586***	-0.555***
	(0.150)	(0.155)
blacklisted in t+3	-0.375**	-0.338
	(0.189)	(0.207)
Log of cloud area	-0.495*	-0.473
	(0.290)	(0.296)
Year effects	YES	YES
Time invariant covariates	YES	YES
Time variant covariates	YES	YES
Number of fines		YES
Observations	900	800
Adjusted R^2	0.308	0.309
2-sided Durbin-Watson-Statistic	2.566	2.621
DW test p-value	0.479	0.479

Note: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Time invariant and variant covariates include first differences of the variables reported in Table 3. Observations are selected by a 1:1 closest neighbor matching on the Mahalanobis distance measure, with replacement. *,**,*** denote significance at the 10/5/1% level.

Table 6: Spatial neighbor effects before and after matching

	Dependent variable:			
	log of deforestation			
	Before matching		After matching	
	(3)	(4)	(3)	(4)
Blacklisted	-0.696***	-0.635***	-0.375*	-0.368*
	(0.128)	(0.129)	(0.199)	(0.202)
Neighbor blacklisted	-0.229***	-0.214***	-0.091	-0.091
	(0.069)	(0.069)	(0.169)	(0.170)
Log of cloud area	-0.023***	-0.022***	0.007	0.007
	(0.006)	(0.006)	(0.013)	(0.013)
Year effects	YES	YES	YES	YES
Time invariant covariates	YES	YES	YES	YES
Time variant covariates	YES	YES	YES	YES
Neighborhood characteristics	YES	YES	YES	YES
Number of fines		YES		YES
Observations	4,014	3,568	900	800
Adjusted R^2	0.150	0.163	0.301	0.302
2-sided Durbin-Watson-Statistic	2.422	2.481	2.570	2.628
DW test p-value	0.479	0.479	0.479	0.479

Note: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Time invariant and variant covariates include first differences of the variables reported in Table 3. Observations are selected by a 1:1 closest neighbor matching on the Mahalanobis distance measure, with replacement. *,**,*** denote significance at the 10/5/1% level.

Table 7: Net average treatment effect of blacklisting

	Dependent variable:	
	log of deforestation	
	(5a)	(5b)
Blacklisted	-0.269*	
	(0.150)	
Blacklisted in t+0		-0.479
		(0.295)
Blacklisted in t+1		-0.294**
		(0.142)
Blacklisted in t+2		-0.533***
		(0.154)
Blacklisted in t+3		-0.311
		(0.205)
Log of clouds	0.009	0.011
	(0.013)	(0.013)
Year effects	YES	YES
Time invariant covariates	YES	YES
Time variant covariates	YES	YES
No. of fines at counterfactual level	YES	YES
Observations	800	800
Adjusted R^2	0.300	0.308
2-sided Durbin-Watson-Statistic	2.627	2.623
DW test p-value	0.479	0.479

Note: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Time invariant and variant covariates include first differences of the variables reported in Table 3. Observations are selected by a 1:1 closest neighbor matching on the Mahalanobis distance measure, with replacement. *,**,*** denote significance at the 10/5/1% level.

Figures

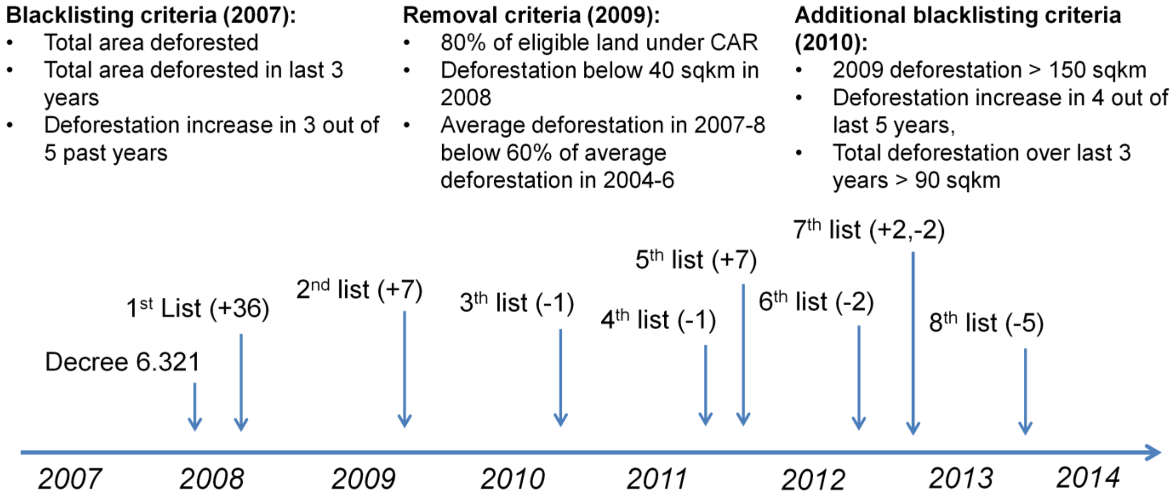

Figure 1: History of district blacklisting and blacklist criteria. Positive numbers in parentheses depict additions to the blacklist. Negative numbers depict removals.

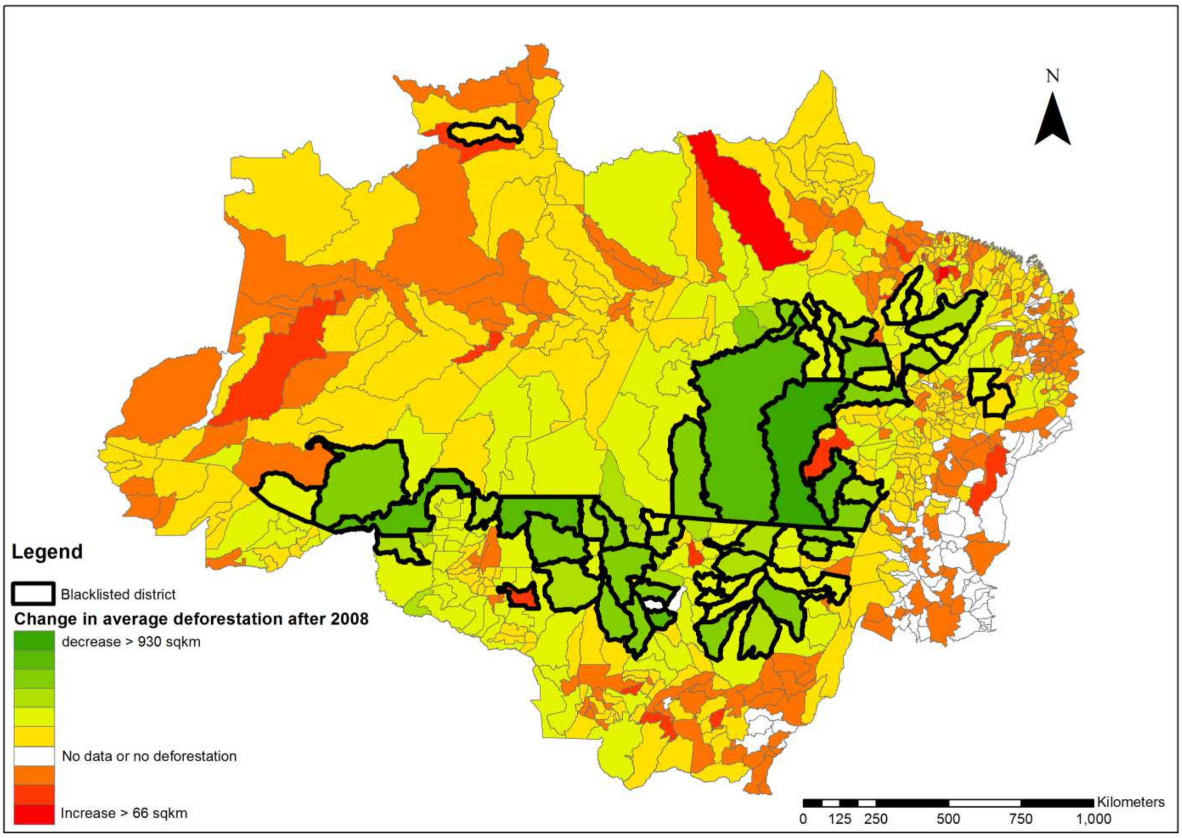

Figure 2: Change in average deforestation (sqkm) in blacklisted and non-blacklisted districts comparing the years 2008-2012 and 2003-2007. Together the districts shown in the map represent the Legal Brazilian Amazon.

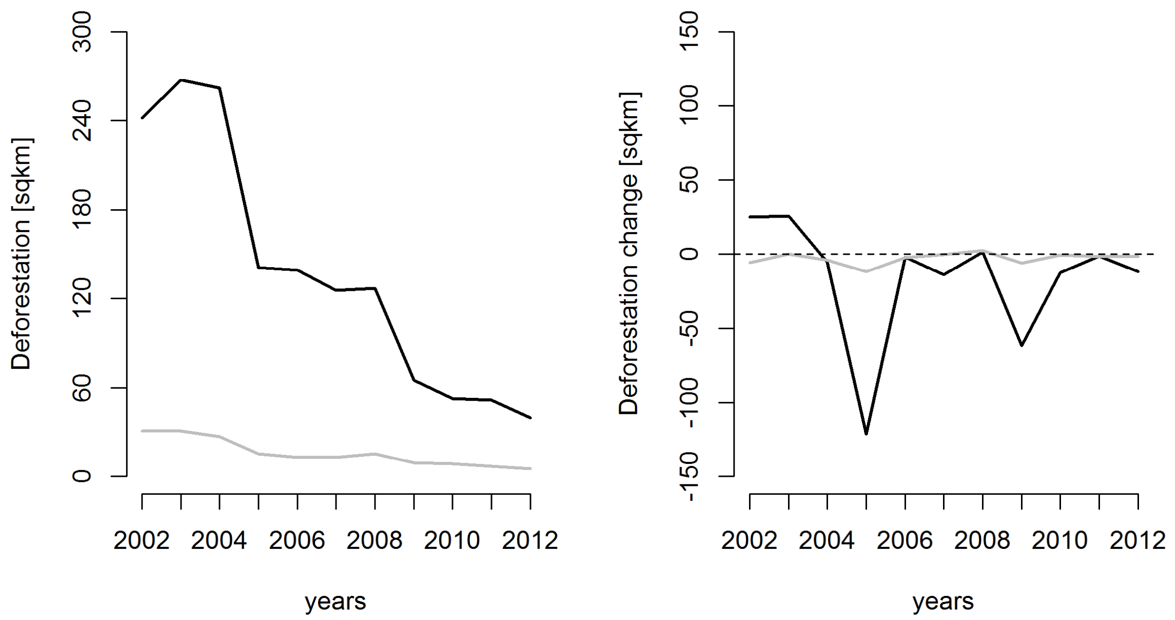

Figure 3: Deforestation trends in blacklisted (black lines) and non-blacklisted (grey lines) districts.

Appendix S1: Placebo regression

For placebo regressions we re-code the treatment variable as if blacklisting had started in 2006 as opposed to 2008. We then use models (3) and (4) with post-matching data to estimate the placebo treatment effect. As expected the placebo treatment variable is insignificant.

Table SI 1.1: Placebo post-matching first difference regressions

	Dependent variable:	
	log of deforestation	
	(3)	(4)
blacklisted (as if in t-2)	0.013	0.043
	(0.163)	(0.170)
Log of cloud area	0.007	0.008
	(0.013)	(0.013)
Year effects	YES	YES
Time invariant covariates	YES	YES
Time variant covariates	YES	YES
Number of fines		YES
Observations	900	800
Adjusted R^2	0.297	0.299
2-sided Durbin-Watson-Statistic	2.554	2.612
DW test p-value	0.479	0.479

Note: The table reports first difference estimates with the dependent variable being the change in the log of yearly newly deforested area. Standard errors, clustered at district level, are reported in parentheses. Time invariant and variant covariates include first differences of the variables reported in Table 3. Observations are selected by a 1:1 closest neighbor matching on the Mahalanobis distance measure, with replacement. *,**,*** denote significance at the 10/5/1% level.

Appendix S2

Table SI 2.1: Covariate balance before and after matching

Covariates	**Standardized differences in means**	
	Before matching	**After matching**
Official criteria		
total deforested areas 2007	1.189	0.759
deforestation 2005	0.992	0.741
deforestation 2006	0.899	0.695
deforestation 2007	0.746	0.575
def. Increase in past 5 yrs	0.784	0.261
Size and land use		
municip. area (mil. Km2)	0.384	0.276
% forest coverage 2002	0.840	0.559
% settlement area 2007	-3.811	-0.505
settlement area 2007 (km2)	0.325	-0.024
farm area	0.258	-0.135
popula. density (1000/km2)	0.268	-0.055
Economic and agricultural conditions		
GDP per capita 2005	-2.953	-0.559
GDP per capita 2006	-0.426	-0.321
GDP per capita 2007	1.160	0.609
No. farms per km2	0.128	0.133
% small farms	0.236	-0.142
distance to nearest city	0.209	-0.065
land value (in BRL/ha)	-0.929	-0.179
% farms w/ legal title	0.340	0.142
cattle stocking rate	0.321	0.047
No. tractors per farm	-0.315	0.091
Protected areas		
% indigeneous 2007	-1.434	-0.109
% strictly protected 2007	-0.640	-0.153
% multiple use 2007	0.255	0.205
indigeneous 2007 (km2)	0.081	0.031
strictly protected 2007 (km2)	0.036	-0.042
multiple use 2007 (km2)	0.101	0.202
Fines		
No. fines 2005	0.613	0.306
No. fines 2006	0.707	0.428
No. fines 2007	0.676	0.336

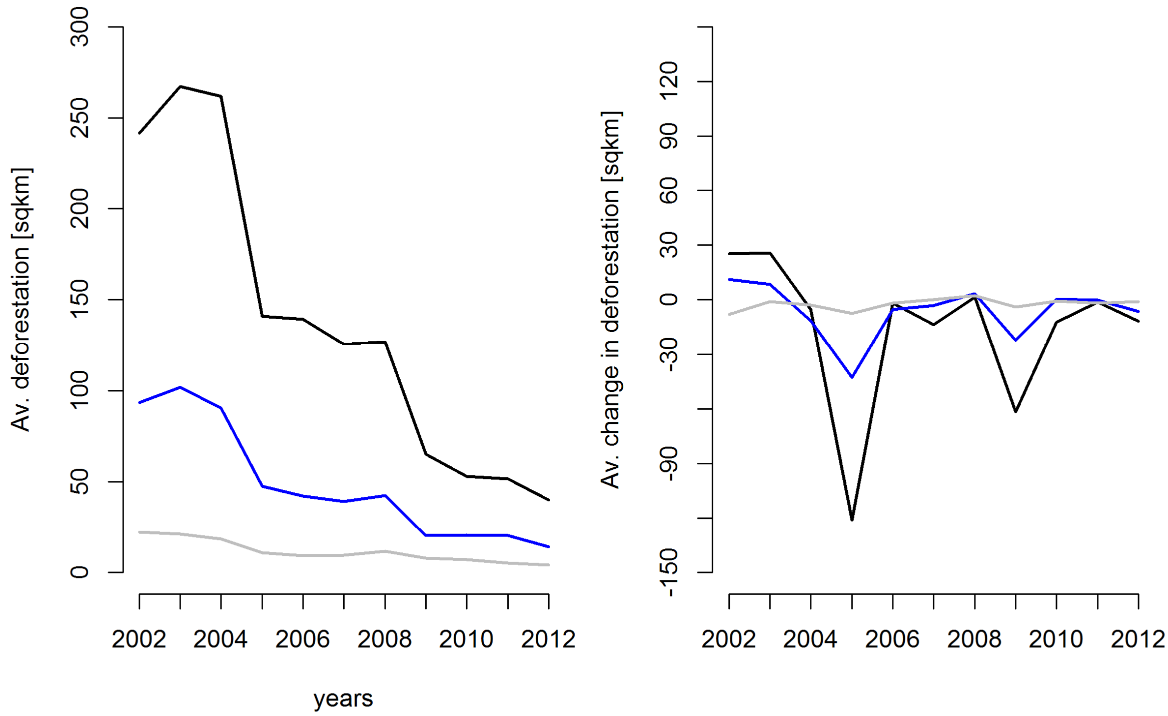

Figure SI 2.1: Average deforestation and change in deforestation after matching. Black lines represent blacklisted districts, blue lines represent matched control districts, and grey lines represent unmatched control districts.