Department of Economics- FEA/USP

Unveiling the Dynamic Impact of Protected Areas: An Event Study Analysis to Assess Conservation Effectiveness

Thiago Fonseca Morello Ramalho da Silva
Paula Carvalho Pereda
Ana Carolina M. Pessôa
Liana O. Anderson

Working Paper Series Nº 2024-02

DEPARTMENT OF ECONOMICS, FEA-USP
WORKING PAPER Nº 2024-02

Unveiling the Dynamic Impact of Protected Areas: An Event Study Analysis to Assess Conservation Effectiveness

Thiago Fonseca Morello Ramalho da Silva (fonseca.morello@ufabc.edu.br)
Paula Carvalho Pereda (pereda@usp.br)
Ana Carolina M. Pessôa (acmoreirapessoa@gmail.com)
Liana O. Anderson (liana.anderson@cemaden.gov.br)

Abstract:

Previous studies estimating the effect of the creation of protected areas (PAs) on forest conservation suffer from biases due to staggered protection and to unobservable drivers of protection's effectiveness. We address these biases by using a cohort-time refined effect estimator in an event study with Amazon Basin data from 2003 to 2020. Which also unveils meaningful dynamic patterns that remained so far hidden in previous papers' aggregate effects. Our findings show that PAs' effects on deforestation and fires were at least halved by the aforementioned biases, being also deflated in 13% and inflated in 16%, respectively, by the failure to control for concomitant and synergistic anti-deforestation policies. We also found strong evidence of forward-looking behaviour by deforesters, with deforestation becoming larger inside protected land two years before protection. This suggests that local agents rush to deforest after learning that the likelihood of being sanctioned will rise with protection. A gradual increase of the effect with the ageing of PAs confirmed that enforcing protection is subject to learning. Also notably, effects were heterogeneous. Whereas both moderately and severely restricted PAs avoided fires, only severely restricted avoided deforestation. In addition, whereas neither national nor subnational conservation unit PAs have reduced deforestation, national units reduced fires but subnational increased them. Indigenous lands reduced deforestation and fires. Results urge policymakers to plan the creation of PAs not merely seeking to change the tenure of land but mainly to align expectations of deforesters to national conservation goals.

Keywords: differences-in-differences, staggered treatment, event study, matching, protected areas, deforestation.

JEL Codes: C21, Q58.

Unveiling the Dynamic Impact of Protected Areas: An Event Study Analysis to Assess Conservation Effectiveness

Thiago Fonseca Morello Ramalho da Silva[1], Paula Carvalho Pereda[2], Ana Carolina M. Pessôa[3] and Liana O. Anderson[4].

Abstract

Previous studies estimating the effect of the creation of protected areas (PAs) on forest conservation suffer from biases due to staggered protection and to unobservable drivers of protection's effectiveness. We address these biases by using a cohort-time refined effect estimator in an event study with Amazon Basin data from 2003 to 2020. Which also unveils meaningful dynamic patterns that remained so far hidden in previous papers' aggregate effects. Our findings show that PAs' effects on deforestation and fires were at least halved by the aforementioned biases, being also deflated in 13% and inflated in 16%, respectively, by the failure to control for concomitant and syne

……….rgistic anti-deforestation policies. We also found strong evidence of forward-looking behaviour by deforesters, with deforestation becoming larger inside protected land two years before protection. This suggests that local agents rush to deforest after learning that the likelihood of being sanctioned will rise with protection. A gradual increase of the effect with the ageing of PAs confirmed that enforcing protection is subject to learning. Also notably, effects were heterogeneous. Whereas both moderately and severely restricted PAs avoided fires, only severely restricted avoided deforestation. In addition, whereas neither national nor subnational conservation unit PAs have reduced deforestation, national units reduced fires but subnational increased them. Indigenous lands reduced deforestation and fires. Results urge policymakers to plan the creation of PAs not merely seeking to change the tenure of land but mainly to align expectations of deforesters to national conservation goals.

Keywords: differences-in-differences, staggered treatment, event study, matching, protected areas, deforestation.

JEL Codes: C21, Q58.

[1] Corresponding author. Federal University of ABC, Brazil, and Land, Environment, Economics and Policy Institute, University of Exeter, UK. fonseca.morello@ufabc.edu.br;

[2] Department of Economics, University of São Paulo; pereda@usp.br;

[3] Amazon Environmental Research Institute (IPAM), Brazil; acmoreirapessoa@gmail.com;

[4] National Center for Monitoring and Early Warning of Natural Disasters (Cemaden), Brazil; liana.anderson@cemaden.gov.br

1 Introduction

Protected areas (PAs) have been repeatedly attested to be effective in conserving natural capital, especially highly ecologically valuable ecosystems such as forests and wetlands (Sze et al., 2022, Shi et al., 2020, Herrera et al., 2019, Wendland et al., 2015, Barnes et al., 2023). They have been shown to avoid deforestation, fires, and related carbon emissions, increase bird diversity, and reduce poverty (Barnes et al., 2023, Sims, 2010, Ferraro and Hanauer, 2014). The extension of protected land has expanded globally by 92% since the 1990s, now embracing 15.4% of Earth's land (Kuempel et al., 2018, Persson et al., 2021). Despite the abundance of PA studies, there are two reasons why new investigations are needed. First, from the policy planning perspective, whether the cost of protection, measured as forgone income from primary activities, is outweighed by ecological benefit, is an empirical question which is highly dependent on local and time-variant factors (Persson et al., 2021, Lima and Peralta, 2017).

Secondly, the methods so far adopted in the estimation of protected areas' (PAs') effect are biased by staggered creation of PAs over time (across multiple cohorts) and by unobservable drivers of PAs' effectiveness. What may lead to a distorted allocation of public funds for such policy and competing policies. Most studies seek to mitigate only the bias from non-random selection of sites for protection by relying on matching on observable covariates (Arriagada et al., 2016). This approach does not effectively address biases arising from influential non-observables. Factors, such as concomitant changes in environmental policy, or local characteristics, are not adequately accounted for. This is particularly relevant given that enforcement of deforestation prohibitions not coinciding with PAs has intensified from 2004 to 2014 in the Amazon (Assunção et al., 2020, Hargrave and Kis-Katos, 2013, Börner et al., 2015). One potential solution is to explore, after matching, ("within") variation across time with a differences-and-differences (DiD) approach, thus avoiding unobservable geographical variation sources and explicitly controlling for observed policy changes. This approach, which is rarely adopted (exceptions being Shi et al. 2020 and Keles et al., 2023), is limited by a second source of bias, the "negative weights" attached automatically to PA cohorts by standard DiD estimators, which aggregate all cohorts together, irrespective of their potentially heterogeneous effects (Goodman-Bacon, 2019, Callaway and Sant'Anna, 2021). Consequently, the causal interpretation of the treatment effect parameter may be compromised.

To address the aforementioned inaccuracies, this paper proposes a new methodological procedure to estimate the effect of PAs. It consists in, after the commonly adopted matching approach, applying Callaway and Sant'Anna's (2021) cohort-refined DiD estimator to unveil, with an event study, cohorts violating the parallel trends assumption. By removing these cohorts (hereafter also called "groups"), the treatment effect estimate obtained is both causal and accurate. By incorporating event study and cohort-refined DiD estimation to analysis, we innovatively expand the toolbox of PAs' effect identification. Furthermore, the challenge of measuring non-PA anti-deforestation policy efforts is addressed by leveraging publicly available proxies. At last, protection performance is measured in terms of two types of forest disturbance, deforestation and fires, the latter a source of forest degradation.

Research has so far largely overlooked the dynamic nature of protection's effect, especially delays and anticipations of changes in outcomes relative to the beginning of protection. This important dimension is pioneeringly made visible in this study by introducing a novel econometric technique that enables the consideration of non-immediate effects in the planning of PAs. This aspect holds great importance as the mere creation of PAs alone is insufficient to ensure effectiveness. Systematic enforcement, including on-field patrolling, is needed (Afriyie et al., 2021, Kuempel et al., 2018, Geldman et al., 2015). The performance of enforcement is dynamic for being contingent on several factors, such as (i) the underlying drivers of the decision to pursue forbidden activities, including deforestation and burning, such as agricultural prices (Assunção et al., 2015, Hargrave and Kis-Katos, 2013), (ii) the enforcement budget available (Kuempel et al., 2018, Jachman, 2008, Silva et al., 2019), and (iii) the process of learning how to enforce protection in the particular social-biophysical context of each PA (Geldman et al. 2015, Afriyie et al., 2021, Kuempel et al., 2018).

Therefore, despite being so far presented as instantaneous by econometric studies, protection's effect is dynamic as both the threats facing PAs and the capacity to withstand them oscillate over time and may affect different cohorts differently. The knowledge about this dynamics, which is available in scattered form across PA studies not necessarily relying on econometrics, is used for the first time in this paper to inform estimation and interpretation of protection's effect.

Our findings reveal significant biases arising from (i) unobservable heterogeneity not addressed by matching, which deflated effect on deforestation in 73%, (ii) staggered protection, which at least halved the effect on both deforestation and fires, (iii) non-parallel trends, which deflated in 39% the effect on fires and (iv) concurrent policy changes, which deflated the effect on deforestation in 13% and inflated the effect on fires in 16%. After removing these biases, protection proved doubtlessly effective. Additionally, it was particularly noteworthy the strong evidence of an increase in deforestation occurring two years before PA creation, which is consistent with forward-looking behaviour by illegal deforesters. These agents, anticipating that the probability of being sanctioned for illegal deforestation will rise in the post-protection period, "rush" to deforest in the pre-protection period (a behaviour evidenced by Temudo, 2012, and Pedlowsky et al., 1999).

Additionally, we observed heterogeneous effects across PA types, both aggregating or not across cohorts. Conservation units, which are managed either by national or subnational governments and do not necessarily ban farming, experienced more deforestation than unprotected land in six years of the pre-protection period, including the aforementioned rise two years before protection. Such type of event occurred only once in indigenous lands, whose utilization is constrained to traditional peoples' practices. Importantly, the event arose approximately when the lengthy process of indigenous lands' creation generally starts and was reverted in the subsequent year to a deforestation level below that of unprotected lands. Which may be another evidence of forward-looking behaviour, with an initial forest rush aborted after learning that governmental presence had already increased locally. Consistently with the specific dynamic patterns of the different PA types, only indigenous lands presented an unambiguously aggregate negative impact on deforestation. These lands also inhibited fires, which was also true for conservation units, despite subnational ones, where fires were more frequent than in unprotected land. Severely restrictive protected areas were more effective in avoiding the two types of forest disturbance. A final dynamic pattern worth mentioning is the gradual increase in deforestation and fire inhibition effect across PA's lifetime, confirming that enforcement is subjected to gains from learning.

Our research thus makes significant contributions to the literature evaluating the impact of PAs (e.g., Pfaff et al., 2015, Herrera et al., 2019, Wendland et al., 2015, Shi et al., 2020, Keles et al., 2023). We address critical sources of bias that have not been comprehensively considered in previous studies measuring PAs' effects. Specifically, we update the standard methodology with recent discoveries about the inaccuracies introduced by an homogeneizing aggregation of heterogeneous treatment cohorts (Goodman-Bacon, 2019, Roth, 2022, Callaway and Sant'Anna, 2021). The resort to Callaway and Sant'Anna's (2021) cohort-refined estimator not only mitigate biases, but also reveals dynamic patterns that were hidden in the aggregate effects reported by previous studies. These patterns are both consistent with a forward-looking theory of deforesters' behaviour, as we demonstrate, and highly relevant for planning PAs' implementation. They shed light on the evolution of protection's influence on deforestation. To the best of our knowledge, no other research has empirically investigated delays and anticipations associated with the creation of PAs[5].

The next section summarizes extant knowledge about the dynamics of protection's effect, presenting a theoretical model demonstrating that forward-looking behaviour is a microfoundation of protection's effect dynamics. Methods follow and results are then presented. They are confronted with previous studies in the discussion section. A short conclusion section closes the paper.

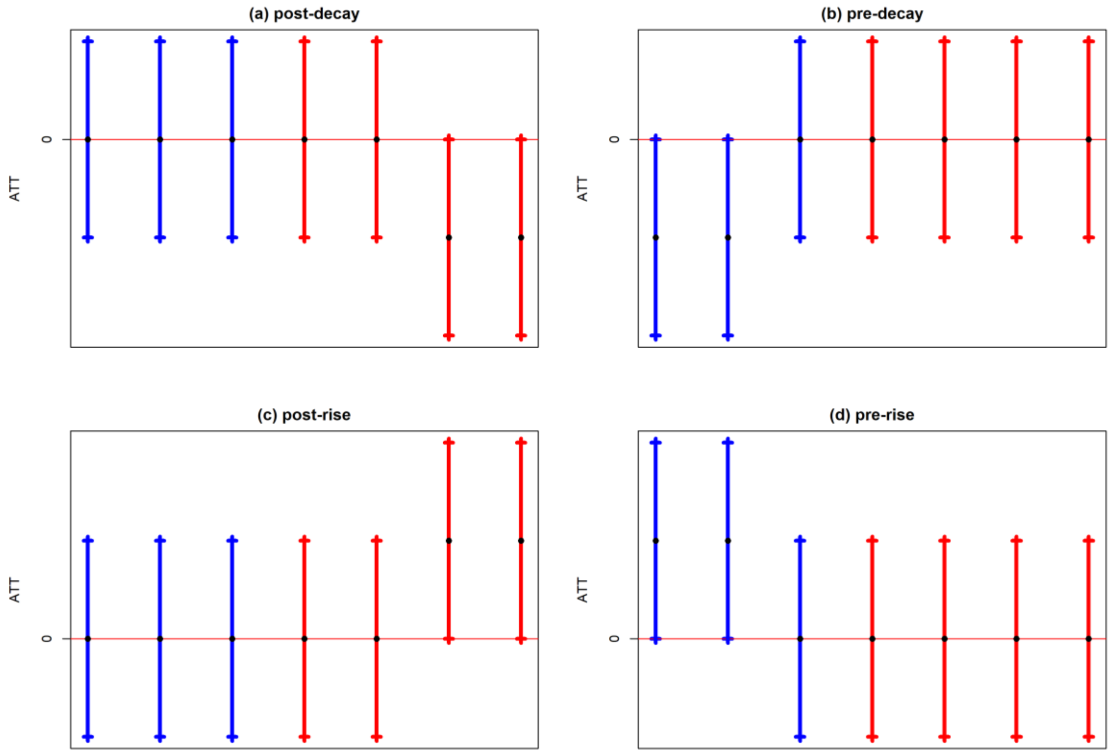
[5] Despite, perhaps, Keles et al. (2023), but with the important difference that authors' treatment is not the creation of PAs, but their downgrading, downsizing or degazettement.

2 Literature and theory

2.1 Literature review on the dynamics of protection's effect

The knowledge in the extant literature about the temporal patterns of protections' effect may be summarized into four types of dynamics, combining two dichotomous dimensions, namely: (1) timing relative to protection outset, i.e., either (1.a) pre-protection or (1.b) post-protection and (2) direction of effect, which is either (2.a) positive or (2.b) negative (figure 1).

Figure 1 Four types of dynamic effects, post-protection decay (a), pre-protection decay (b), post-protection rise (c) and pre-protection rise (d).



The study of the dynamics of protection's effect is challenged by two main sources of bias. First, untreated pixels are not all of them comparable to the treated. Second, standard aggregation of cohorts of treated pixels may automatically attach negative weights to some cohorts. These issues prevent the observation, in the raw data, of the types of dynamics here defined. Besides, these are "ideal types" which are less likely to be observed in pure form rather than mixed. Nevertheless, as detailed in the next paragraphs, it is coherent with previous studies to believe that the four types of effect dynamics may manifest as part of the process through which protected areas inhibit deforestation.

Inspired in literature, this paper relied on a simple conceptualization of the deforestation-protection nexus. It involves two agents, the policy-makers creating and enforcing PAs and the deforesters, who are standardly assumed to rationally balance agricultural profitability and financial gains from land appropriation and losses from sanctions (in line with Angelsen, 1999, Hargrave and Kis-Katos, 2013, Börner et al., 2015). It is further assumed that deforesters are forward-looking, predicting both agricultural prices and the future likelihood of sanction (similarly as in Pfaff et al., 2007). Then it is coherent to conclude that they optimally select the period to deforest as the one yielding the best balance, i.e., the largest expected agricultural income net of expected sanction cost. This cost, or, more precisely, sanctioning likelihood, should be larger in protected land because deforestation is generally forbidden and tenure ambiguity, which could prevent or delay sanctioning, is not an issue. Therefore, the link between protection and deforestation is in that the latter counter-incentivizes the former. This also apply to fires, which are strongly associated with deforestation (Morello et al., 2019).

Now turning to specific types of dynamics, a negative post-protection effect refers to the absence of effect in the first year of protection and the presence of a negative effect in subsequent years. This dynamics could be attributed to the gradual improvement of PA enforcement performance, as staff takes time to learn how to optimize patrolling in the specific set of biophysical and social conditions faced, which, according to Geldman et al. (2015), is in line with management theory (see also Afriyie et al., 2021). Also, PAs performance was found to improve over time (Geldman et al., 2015, Paiva et al., 2015). Deforesters may take advantage of initial enforcement caveats to keep their activity. Even if it may sound unreasonable the act of continuing to deforest a land which, after protection, became doubtlessly a property of the State, this impression falls apart when evidence on land speculation inside PAs and on the gains it generates, are brought to the fore (see Klinger and Mack, 2020, Bowman et al., 2021 and Carrero et al., 2022).

A positive delayed effect may result from lower enforcement inside rather than outside protected areas, which pushes deforestation towards PAs. This dynamics is even more likely if the budget invested in PAs is mainly used for their establishment (e.g., to indemnify expropriations), whereas the budget invested outside of PAs flows mainly to enforcement (Kuempel et al., 2018, Nolte et al., 2013). Moreover, budget managers may implicitly assume that protected land is less exposed to threats than unprotected, with enforcement prioritizing the latter (as noticed by Kuempel et al., 2018). Another reason, which is driven by the political cycle, is the loss of credibility of particular PAs, including those that are at risk of being degazetted or downsized (Keles et al., 2023, Kingler and Mack, 2020, Carrero et al., 2022). This tenure ambiguity may be more profitable to deforesters than the unambiguity of particular unprotected public lands. For instance, Carrero et al. (2022, figure 3), found fractions of self-declared private properties overlapping with protected areas that were larger than those overlapping with agrarian settlements and military areas. Local land users may also increase deforestation and other forms of natural resource degradation inside

PAs whose creation defied their interests, as a form of contestation (Debelo, 2012, Holmes, 2014[6]).

Now turning to changes occurring before protection, the literature is much less informative about them. Anticipated response of deforesters, or other resource users, to the restrictions imposed by protection, are infrequently mentioned, despite being fully consistent with the assumption of forward-looking agents. A drop in deforestation before protection, i.e., a negative pre-protection effect, may be motivated by deforesters revising their expectations of enforcement upwards after learning that a land area is to be protected. Indeed, governmental presence increases right since anthropological and ecological studies start being undertaken as means to inform the creation decision[7]. Keles et al. (2023, fig.7) indeed found negative ex-ante effects of protection in particular Amazonian locations (such as Pará state). Pre-protection effects may be also positive. The future protection of a land parcel could trigger its deforestation in the present, since protection increases the likelihood of sanction but not agricultural (or land) value. A first example is the "forest rush" induced by the prospect of creating a new PA in Guinea-Bissau, which led local traditional people to believe their land rights would be revoked (Temudo, 2012). They reacted in advance by resorting to many strategies to secure forest land, such as thinning forest canopy to plant market-value trees and replacing forest with orchards. Protest slashing-and-burning took place in a more advanced (and heated) stage of protection contestation (Temudo, 2012). A second example, reported by Pedlowsky et al. (1999), is the "rush for land" in the Brazilian state of Rondônia, triggered by the announcement of conservation units' creation, which was slowly implemented. A third example of an anticipated response to PA creation that (could have) raised environmental degradation is found in Baragwhanath and Bayi (2020). The authors make clear that contestation of indigenous lands, including invasion by non-indigenous resource users and deforesters, is possible up until the fourth and final phase of the creation process, which takes ten years and half in average to be achieved (FUNAI, 2023).

2.2 Theory

2.2.1 Assumptions

A dynamic-stochastic general equilibrium model was developed as means to generate the four types of dynamics from solid theoretical foundations. A standard RBC model was expanded to include deforestation as an investment on land, an asset which competed with fixed capital for savings. The household (HH), the sole asset owner in the economy, maximized the standard CRRA instantaneous utility function below (which is adopted by Lucas, 1999 and King, Plosser and Rebelo, 1988):

$$u(c_t, l_t^s) = \frac{c_t^{(1-\eta)\mu}(1 - l_t^s)^{(1-\eta)(1-\mu)}}{1 - \eta}$$

Where consumption and hours laboured are denoted by c_t and l_t, total time available is normalized to one, relative risk aversion coefficient is η and utility's consumption elasticity is μ. All quantity

[6] In the case study of Holmes (2014), peasants set fires near the borders of a PA as means to contest it.
[7] Conservation units and indigenous lands go through, respectively, two and five stages involving State presence, to be legally created (Brazil, 9985/2000 and 1775/1996, FUNAI, 2023). During the pre-creation assessment studies, agricultural, extractive and other activities may be forbidden and non-indigenous people re-settled outside (Brazil, 9985/2000 and 1775/1996).

variables are specified in per capita terms, population growth is ignored and the price of goods is the numerárie.

The budget constraint has, on the income side, the earnings from labouring ($w_t l_t$), from renting capital ($r_t k_t$) and land ($s_t a_t$), augmented by cash transfer received (t_t). Expenditures comprise, besides consumption, investment (i_t) and the total cost of deforestation, denoted as $(m_0 + \sigma.\theta.\alpha_{PA}\gamma_{PA,t})d_t$. That is:

$$w_t l_t^s + r_t k_t^s + s_t a_t^s + t_t = c_t + i_t + (m_0 + \sigma.\theta.\alpha_{PA}\gamma_{PA,t})d_t$$

The deforestation cost unfolds, firstly, into a direct cost, $m_0 d_t$, which covers the inputs required by the operation. Secondly, there is the expected fine. It is assumed that any positive level of deforestation inside a PA is liable for a penalty of θ monetary units per hectare, which, due to imperfect enforcement, has probability σ of being imposed. Deforestation outside PAs is free of penalty. The remaining components of the expected fine α_{PA} and $\gamma_{PA,t}$, come from the following calculation referring to the extent of deforestation occurring in protected areas, $d_{PA,t}$:

$$\mathrm{d}_{PA,t} = \frac{\mathrm{d}_{PA,t}}{\mathrm{d}_t}\mathrm{d}_t = \alpha_{PA,t}\gamma_{PA,t}\mathrm{d}_t$$

Where $\gamma_{PA,t}$ is the percentage of total forestland that is protected $\left(\frac{\mathrm{F}_{PA,t}}{\mathrm{F}_t}\right)$ and $\alpha_{PA,t}$ is a constant of proportionality connecting the shares of protected area in deforestation and in total forest area, i.e., $\frac{\mathrm{D}_{PA,t}}{\mathrm{D}_t} / \frac{\mathrm{F}_{PA,t}}{\mathrm{F}_t} = \alpha_{PA,t}$ (capital letters are used for non-percapita totals; it is considered that ratios involving them are equal to ratio involving their per-capita values). For simplicity, $\alpha_{PA,t}$ is assumed fixed and exogenous in simulations.

The land suitable to be used as a production factor, a_t, grows with deforestation and shrinks with physical depreciation at a rate ψ, that is:

$$a_t^s = (1-\psi)a_{t-1}^s + d_{t-1}$$

Incorporating the law of motion of capital, the HH problem is:

$$\max_{\{c_t,l_t,i_t,d_t,k_t,a_t\}} \mathrm{E}_0 \left\{ \sum_{t=1}^{T} \beta^t \left[\frac{c_t^{(1-\eta)\mu}(1-l_t^s)^{(1-\eta)(1-\mu)}}{1-\eta} + \lambda_{bc,t}\left[w_t l_t^s + r_t k_t^s + s_t a_t^s + t_t - c_t - i_t - (m_0 + \sigma.\theta.\alpha_{PA}\gamma_{PA,t})d_t\right] + \lambda_{K,t}[(1-\delta)k_{t-1}^s + i_{t-1} - k_t^s] + \lambda_{A,t}[(1-\psi)a_{t-1}^s + d_{t-1} - a_t^s] \right] \right\}$$

The representative firm operates a Cobb-Douglas function including capital, labour and land, thus solving the following problem:

$$\max\{K_{gt}^d, L_{gt}^d, A_t^d\} \left\{ J_t {K_t^d}^{\alpha} {L_t^d}^{\beta} {A_t^d}^{1-\alpha-\beta} - r_t K_t^d - w_t L_t^d - s_t A_t^d \right\}$$

Where J_t is the total factor productivity (TFP), a stochastic variable, and factor demands are K, L and A. With N denoting population level, factors' and good's markets clear:

$$K_t^d = k_t^s.N, L_t^d = l_t^s.N, A_t^d = a_t^s.N$$

$${K_t^d}^{\alpha} {L_t^d}^{\beta} {A_t^d}^{1-\alpha-\beta} = (c_t + k_{t+1} - (1-\delta)k_t + m_0 d_t)N$$

Government budget, which does not contain consumption nor taxation, is always balanced, that is:

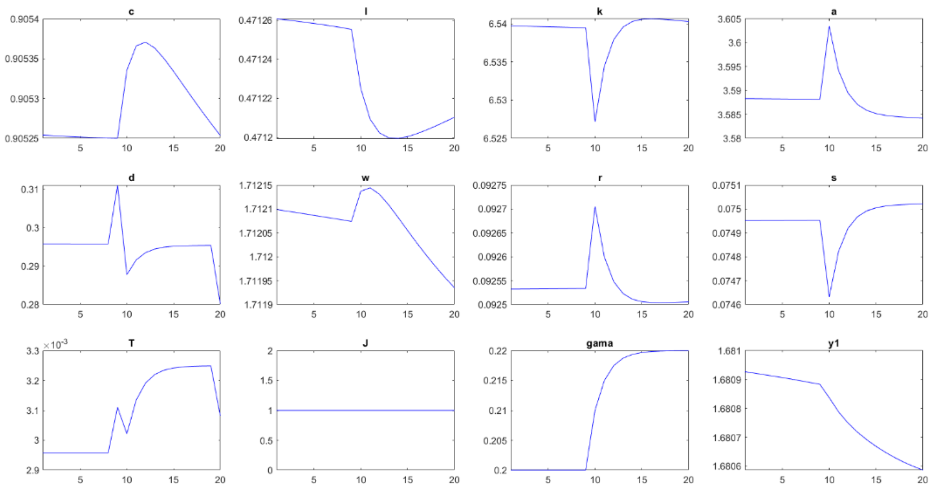
$$t_t.N = \left(\sigma.\theta.\alpha_{PA}\gamma_{PA,t}\mathrm{d}_t\right).N$$

2.2.2 Simulations

The steady state of the model was calibrated to a set of parameters meant to be as general as possible – data sources are found in appendix 3, which also contains the equations of the dynamic system. By abstracting TFP shocks, we simulated the effect of PA creation with a deterministic shock on the extent of land protected, $\gamma_{PA,t}$. The shock was fully known by the forward-looking household. With 20 periods of simulation and a one percentage point shock introduced from the 10^{th} to the 20^{th} period, the main result was the "forest rush" effect of an increase in deforestation in the 9^{th} period, with a decrease from then on (figure 2). This response can be interpreted as a way to smooth out the level of land, a valuable asset, across time. Thus both the pre-protection rise and post-protection decay dynamic effects were confirmed to be consistent with forward-looking behaviour. The economy does not return to the pre-shock steady state, but to a new steady state of lower consumption and output, which is reasonable since endogenous HH income was reduced (despite exogenous income was increased through a greater cash transfer).

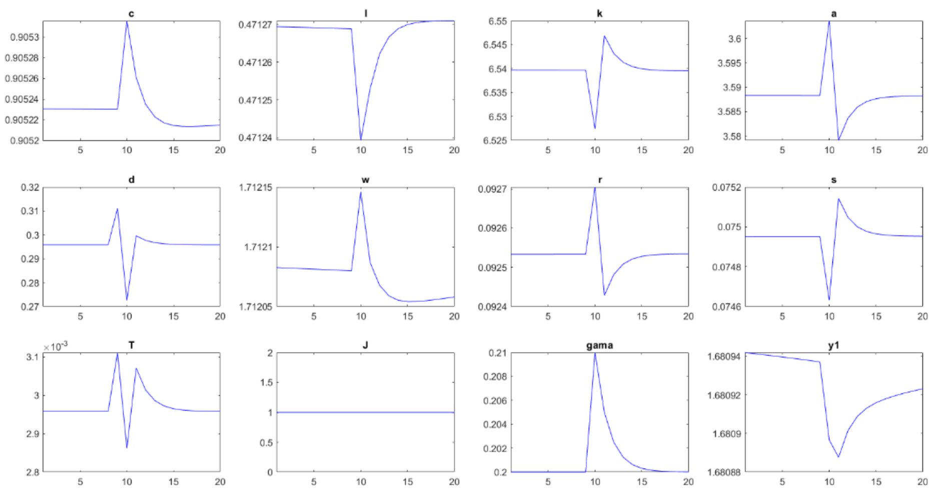
A second experiment applied a pulse shock in the share of forest protected and there were two differences. First, deforestation increased above the steady state level one period after the shock, a post-protection positive effect, which lasted eight periods (figure 3). In the previous simulation, deforestation remained below the steady state in all post-shock periods. This shows that if the HH expects PA creation to be reverted (pulse shock case), it will keep deforestation above the pre-creation level temporarily, while fine probability decays (which is line with the empirical evidence about PA erasure compiled by Keles et al., 2023). The second difference, which was expected, was the return of deforestation and the economy to the steady state, given the pulse nature of the shock – judging from percent differences based on pre-shock levels with a four decimal digit precision.

Figure 2 Permanent shock (impulse-response functions)



Note: “gama” is the shocked variable, “T” is cash transfer and y1 is the GDP per capita.

Figure 3 Pulse shock (impulse-response functions)



2.2.3 ATT deflation and inflation due to concurrent policies

Besides protection, other concurrent environmental policies curbing deforestation and fires take place in the Amazon. Intensification of forest law enforcement in non-protected government owned-lands is a key example. A crucial point is that, even not taking place in PAs, they may spill-over to PAs, also through a expectation formation mechanism, due to increased perception of state presence. Failure to control for these policies may either inflate or deflate the effect of PAs:

1. There is deflation if non-PA policies reduce forest disturbance more intensively outside rather than inside PAs (figure 4, chart 2). I.e., if lowering disturbance in the control group in a larger magnitude (after controlling, ATT should increase in absolute magnitude).

Putting alternatively, in this case other policies and protection are forces acting upon pixels with different treatment statuses;

2. There is inflation if non-PA policies decrease forest disturbance more intensively inside rather than outside PAs (that is, the indirect spill-over effect must be larger than the direct effect; figure 4, chart 3). I.e., when they diminish disturbance in the treated group in a larger magnitude (after controlling, ATT should decrease). In this case, protection and other policies both act upon treated pixels (they are forces that add up to each other).

Figure 4 Deflation and inflation by non-PA policies (control = black, treated = grey)

3 Empirical method and data

3.1 Identification strategy

The goal is to estimate the causal effect of protected areas (PAs) on deforestation, which is given by coefficient β in the equation below. The binary variable taking value one if the i-th pixel is protected in the t-th year, and null value otherwise, is denoted as "PA" and covariates are subsumed to vector X. The dependent variables are also binary and indicate whether deforestation or fires were detected inside the pixel.

$$\text{Deforestation}_{it} \,/\, \text{Fires}_{it} = \gamma + \beta PA_{it} + X_{it}\Gamma + a_i + \lambda_t + u_{it},\ i = 1,\ldots,N,\ t = 2003,\ldots,2020$$

Three main identification challenges are faced, (i) self-selection of the i-th site to be protected, (ii) potential confounding factors from omitted concurrent changes and (iii) staggered creation of PAs over time, which may lead to heterogeneous effects. To mitigate associated biases, matching was used in the first step to increase balance and the common extent of support between treated and untreated (control) observations. Secondly, we implement the group-time differences-in-differences approach developed by Callaway and Sant'Anna (2021) using covariates and fixed effects to estimate the average treatment effect on the treated (ATT). This two-step approach allows us to deal with self-selection on covariates and time-invariant unobservables, as well as to accurately calculate the average effect of PAs by appropriately accounting for group (cohort) heterogeneities.

One-to-one covariate matching on Mahalanobis distance (d_{ij}) was pursued with replacement, as imprecisely represented by the equation below, with Z being a covariate vector with the same variables of X and some more (Morgan and Winship, 2007, chap.4, StataCorp, 2013).

$$PA_i = \alpha + Z_i\Pi + e_i,\ i = 1,\ldots,N,\ t = 2003$$

$$d_{ij} = \{(Z_1 - Z_0)' V_{NxN}^{-1} (Z_1 - Z_0)\}^{\frac{1}{2}}$$

In which the covariate values for treated and control groups are denoted by Z_1 and Z_0, respectively, and V is Z's sample variance-covariance matrix.

Matching was performed using data from the first year of the dataset, 2003, in order to minimize the contamination of untreated pixels by the treated. The treated group consisted in all pixels protected in some year of the analysis period whereas the control group contained only the never-protected. Since the covariate vectors for deforestation and fires differed, given that only in the latter case deforestation was included, matching was separately implemented for each dependent variable. Based on the matching approach, we removed (i) controls not sufficiently comparable to the treated and (ii) treated pixels that could not find sufficiently comparable controls. The exclusion of treated observations relied on a one standard deviation (SD) caliper for each and all covariates (similar as in Arriagada et al., 2016 and Wendland et al., 2015)[8].

After restricting the sample to comparable pixels, we proceeded with the DiD-based ATT estimand developed by Callaway and Sant'Anna (2021) which was based on the outcome regression specification. The group-time estimates were aggregated at exposure-length level, in order for an event study to be carried out as means to pre-test the parallel trends assumption ensuring identification. Aggregation at whole-sample level generated an overall effect which was compared with standard DiD estimates, revealing the size of the bias due to the negative weights attached to group-time estimates automatically by standard DiD – all aggregations were based in Callaway and Sant'Anna's formulas (2021).

As means to ensure that the parallel trends assumption was met, a sufficient condition for causal effect identification, it was necessary to exclude groups causing violations. These are hereafter referred to as "critical groups", and understood as those whose group-time ATTs both belonged to significant pre-treatment exposure lengths and were, themselves, significant. These exclusions were step-wisely implemented, whenever a previous round of group removal was not enough to

[8] A half SD caliper was also considered as an alternative (and more rigorous) option. But since the matching quality gain it brought per unit of observation excluded was substantially smaller than the one yielded by the one SD caliper, only results generated by the latter are reported. Additionally, the sample size reduction the half SD caliper entailed was great enough to prevent generation of the group-time estimates.

drive all pre-treatment effects null[9]. The event study estimates, more precisely, the significance of pre-treatment effects, re-generated at each round, guided the operation.

Two-way fixed-effects DiD regressions (DID-FE) were also run as baseline for calculating the "staggered protection" bias, which is due to cohort-level heterogeneous ATTs being aggregated in an overall effect measure (which could lead to the negative weights bias referred to by Goodman-Bacon, 2018).

The robustness of the "critical groups" approach to group selection was assessed by comparing the associated overall ATTs with those generated by an alternative group selection approach based on Goodman-Bacon's (2018) decomposition. It revealed the weights in the standard two-way fixed-effects estimates of each binary comparison between never-treated and a specific cohort group, showing which cohorts were the top five in weight – these comparisons, in which strictly the never treated are taken as untreated units, were focussed in consistency with the matching convention of including only never-treated pixels in the control group. Three matched subsamples were the object of the robustness test: (i) whole Amazon Basin, (ii) only the Brazilian fraction of the Basin, without institutional covariates and (iii) Brazilian fraction with institutional covariates. In all these three, the top five cohorts in weight represented at least 66% of the total weight[10], which is a major share of the variation identifying ATT. Even with Goodman-Bacon's (2018) decomposition implemented separately in each subsample vs. dependent variable combination, it pointed, in all of them, to the same top five cohorts, namely, 2005, 2006, 2008, 2009 and 2016. Considering only these cohorts, Callaway and Sant'Anna's (2021) estimator was then ran for all six combinations.

3.2 Data

3.2.1 Covariates

Eight "subsamples" were analyzed, all of them at the geographical scale of 25 km^2 pixels and at the annual time scale from 2003 to 2020. The first sample covered the entire Amazon Basin, delimited accordingly with hydrological and ecological criteria (see Eva and Huber, 2005). It overlaps, at least partially, the territories of nine South-American countries, with Brazil occupying about 60% of the whole region. The second sample contained solely the Brazilian portion of the Basin (hereafter referred to as "Brazilian Amazon" for simplicity[11]). It was the only part of the Amazon Basin for which environmental policy data was available as means to explicitly control

[9] At most three rounds were required in all cases, with fires requiring mostly two rounds (five of the eight subsamples considered) and deforestation requiring mostly three rounds (four of the eight subsamples).
[10] This share was above 75% for four of the six combinations.
[11] We highlight that the fraction of the Amazonian Basin falling in the Brazilian territory does not coincide with the two more commonly adopted geographical delimitations of the Brazilian Amazon, which are either of ecological or legal nature (being termed "Brazilian Amazon biome" and "Legal Brazilian Amazon").

for concurrent policy changes. Abusing the meaning of "sample", what is here referred to as the third "subsample", also captured only Brazil, but included institutional covariates proxying non-protection policies implemented simultaneously with protection. In order to measure the effect of specific types of PAs, a common practice in the literature (Herrera et al., 2019, Amin et al., 2019), five additional subsamples included only treated pixels belonging to a specific PA type. Whereas the first two types corresponded to conservation units, either managed by national or subnational governments, the third type corresponded to indigenous lands. The last two subsamples also referred to conservation units, but grouped according with two levels of severity of protection constraints. First, units permitting only indirect resource use (where only ecological management and tourism are allowed), and those permitting direct use, i.e., extraction and (limited) removal of vegetation cover by inhabitants. These five different territories may exhibit specific protection effect dynamics given their particular constraints to natural resource exploitation and land usage, as well as the different agencies responsible for their management (Amin et al.,2019, Qin et al.,2023, Carrero et al.,2022).

The covariates based on which pixels were matched (vector "Z") belonged to three classes: (1) meteorological (temperature, precipitation and maximum cumulative water deficit), (2) land use and land cover (extent of farming, of forest and other natural landscapes, forest fragmentation and, in the case of fires, deforestation of primary and secondary vegetation), and (3) land profitability (distance to roads, rivers, populated areas and urban zones, population, terrain's elevation and slope and soil quality). All these variables were geoprocessed and aggregated to pixel-year level.

The post-matching DID estimation included the time-variant subset of the matching variables, X_{it}, in order to compensate for the static nature of matching (in line with Goodman-Bacon's (2018) statement that time-variant covariates attenuate staggered treatment bias). In addition, one of the "subsamples" contained three institutional variables explicitly controlling for environmental policy changes. These variables were municipal expenditure on environmental governance, area of properties embargoed due to illegal deforestation, and distance to the nearest environmental police headquarters (FINBRA, 2023, IBAMA, 2023a and 2023b). The first two variables were available only at the municipal level, and since all the three variables were time-invariant, they were interacted with a time trend to prevent elimination by the fixed-effects estimator - the three institutional covariates were available only for Brazil.

3.2.2 Sample reduction

The population variable exhibited great discrepancy between protected and non-protected pixels, with a large standard deviation in the second group (coefficient of variation = 16). Because of that, outlier pixels in population were eliminated from analysis before matching (which reduced fourfold the population's variable coefficient of variation). These pixels, whose population level was above the 99th percentile of the whole dataset (1,297 inhabitants/25 km^2 by 2003), were either urban or considerably closer to urban zones - 20% of them were at zero distance from urban towns, a percentage which was of 0.1% for non-outlier pixels; in addition, distance to urban towns was, among outlier pixels, statistically smaller in average (p-value < 0.01%). Outlier population pixels were thus unlikely to give place to deforestation, so that keeping them could contribute to an underestimation of the treatment effect.

Before matching, and in accordance with Callaway and Sant'Anna (2021, footnote 2), pixels treated before the second year of analysis (2004) were dropped, along with outlier pixels (details provided in section 3.2.2 below) – thus ensuring that all treated pixels were observed also in their pre-treatment state.

4 Results

4.1 Main effects[12]

Tables 1 and 2 show the average treatment effect on the treated (ATT), estimated by multiple approaches (columns (1) to (7)), for deforestation and fires. Starting with the former, in the matched subsamples[13], three violations of parallel trends assumption, in the form of significant pre-treatment effects, were observed in the event studies. These occurred at exposure lengths of -15, -9 and -2 years, the first two displaying significant negative effects and the last one showing a positive effect (Appendix 2, figure A.2.1.1) - lag -9 was not significant in the unmatched sample. To address the issue, we excluded the critical groups, namely 2006, 2013, 2016 and 2019, thus ensuring parallel trends.

In the unmatched sample, the overall ATT was of -0.0236, while in the matched sample, with and without the 1 SD caliper, it was larger in absolute magnitude, of -0.0294 and -0.0278 (table 1). But in the case in which the parallel trends assumption was met, i.e., without the critical groups, the ATT was of -0.025, showing that failure to meet the assumption was biasing upwards in 11%, in absolute value terms, the estimate (table 5). This last estimate was over twice as large, in module, as those with DiD-FE regressions, revealing that the negative weights bias, coupled with non-parallel trends, diminished the absolute size of the ATT (table 1).

[Main effects: fires] Fires were similarly subjected to parallel trends violations (in lags -11,-10, -6, -4, -1), which biased ATT downwards in 39% (Tables 2 and 5). Both the failure to match and

[12] Results based on the half SD caliper are omitted. The results reported are based on the 1 SD caliper, which achieved a satisfactory balance between matching quality and sample size (see Appendix 2).

[13] An assessment of matching quality is provided in Appendix 1.

the lack of a post-matching analysis deflated ATT, with non-staggered post-matching deflating further (table 5).

[Confounding policies] With the institutional variables that were available only for Brazil, 13% larger and 16% smaller ATTs were estimated for deforestation and fires, respectively (tables 2 and 5), compared with a Brazilian subsample without institutional covariates. Therefore, concurrent non-PA policies decreased deforestation more largely outside PAs, whereas they decreased fires more intensely inside PAs.

[Heterogeneity] Regarding ATT heterogeneity, only indigenous lands and a specific type of conservation unit, the most severely restrictive one (indirect use) were effective in preventing deforestation. Indigenous lands were slightly more effective, with an estimate closer to that for whole-PAs' effect than severely restrictive conservation units. A different pattern was observed for fires, which were blocked by indigenous lands and national conservation units. But subnational units unexpectedly presented a higher internal fire frequency than unprotected land. Units differing on degree of protection stringency were all effective, but again the most restrictive were most effective.

Table 1 Effect of PAs on deforestation using several approaches: DiD-FE and group-time estimates

	(1)	**(2)**	**(3)**	**Group-time**			
	Matching only	**DiD**	**DiD-FE**	**(4)**	**(5)**	**(6)**	**(7)**
				Unmatched, all groups	**Matched, no caliper, all groups**	**Matched, 1 SD caliper, all groups**	**Matched, 1 SD caliper, only non sig.pre-treat.groups**
Average treatment effect on the treated (ATT)	-0.0067***	-0.0124***	-0.0124***	-0.0236*	-0.0294*	-0.0278*	-0.025*
	(0.0013)	[0.0017]	[0.0016]	[0.0019]	[0.003]	[0.0032]	[0.0037]
N	594,702	594,702	594,702	2,235,996	725,724	594,702	415,080
N clusters	NA	33,039	33,039	124,222	40,318	33,039	23,060

Table 2 Effect of PAs on fire using different approaches: DiD-FE and group-time estimates

	(1)	**(2)**	**(3)**	**Group-time**

	Matching only	DiD	DiD-FE	(4)	(5)	(6)	(7)
				Unmatched, all groups	**Matched, no caliper, all groups**	**Matched, 1 SD caliper, all groups**	**Matched, 1 SD caliper, only non sig.pre-treat.groups**
Average treatment effect on the treated (ATT)	-0.0575***	-0.0052***	-0.0052***	-0.0153***	-0.0360***	-0.0369***	-0.0601***
	[0.0008]	[0.0012]	[0.0011]	[0.0014]	[0.0026]	[0.00291]	[0.0073]
N	592,380	592,380	592,380	2,235,996	726,048	592,380	209,628
N clusters	NA	32,910	32,910	124,222	40,336	32,910	11,646

Table 3 Effect of PAs on deforestation: Brazilian Amazon and PA-types' samples, group-time estimates after exclusion of critical groups

	(1) All protected areas, without institutional covariates, Amazon Basin	(2) All protected areas, without institutional covariates, Brazilian Amazon	(3) All protected areas, with institutional covariates, Brazilian Amazon	(4) Only indigenous lands, Amazon Basin	(5) Only subnational conservation units, Amazon Basin	(6) Only national conservation units, Amazon Basin	(7) Only indirect conservation units, Amazon Basin	(8) Only direct conservation units, Amazon Basin
ATT	-0.025*	-0.0279***	-0.0321***	-0.0243***	0.0022	-0.0113	-0.0227*	-0.0028
	[0.0037]	[0.0068]	[0.0053]	[0.0066]	[0.0095]	[0.0071]	[0.0093]	[0.0059]
N	415,080	145,224	241,074	106,830	57,762	88,038	84,366	141,948
N clusters	23,060	8,068	13,393	5,935	3,209	4,891	4,687	7,886

Table 4 Effect of PAs on fire: Brazilian Amazon and PAs types' samples, group-time estimates after exclusion of critical groups

	(1) All protected areas, without institutional covariates, Amazon Basin	(2) All protected areas, without institutional covariates, Brazilian Amazon	(3) All protected areas, with institutional covariates, Brazilian Amazon	(4) Only indigenous lands, Amazon Basin	(5) Only subnational conservation units, Amazon Basin	(6) Only national conservation units, Amazon Basin	(7) Only indirect conservation units, Amazon Basin	(8) Only direct conservation units, Amazon Basin
ATT	-0.0601***	-0.0624***	-0.0538***	-0.0352***	0.0323***	-0.0552***	-0.0499***	-0.0318***
	[0.0073]	[0.0096]	[0.0065]	[0.0049]	[0.0076]	[0.0065]	[0.0053]	[0.0067]
N	209,628	201,546	201,546	119,052	89,028	99,414	107,802	203,994
N clusters	11,646	148,914	201,546	6,614	4,946	5,523	5,989	11,333

Table 5 Four biases in naïve estimation (relative [and absolute] calculation)

	Deforestation	Fires
"Matching alone" bias	-73 % [-1.84%]	-4 % [-0.26%]
Staggered protection bias	-50 % [-1.26%]	-91 % [-5.49%]
Unparalleled trends bias	11 % [0.28%]	-39 % [-2.32%]
Concurrent policy bias	-13 % [-0.42%]	16 % [0.86%]

Note: relative bias is calculated as biased/unbiased – 1, that is, as the percentage in which biased absolute estimate exceeds the unbiased absolute estimate. Consistently, absolute bias was calculated as biased – unbiased.

4.2 Robustness test

Regarding deforestation, robustness was achieved both in sign and magnitude of estimates, the latter differing in no more than 14%. This is shown in table 6, which compares critical cohort exclusion with the inclusion of top-five cohorts in the weights obtained as part of Goodman-Bacon's (2018) decomposition. Nevertheless, in the case of fires (table 7), robustness restricted to estimates' sign, due to magnitude discrepancies of at least 40%, which suggested inflation of effect's size. Therefore, it is cautious to expect, in practice, lower effects on fires than those shown in the previous tables.

Furthermore, the direction of change in effects after controlling for concurrent policies was also robust, for the two dependent variables, with increase and decrease of the effects on deforestation and fires, respectively. But the smaller size of change, for the two outcomes, is also noteworthy.

Table 6 Robustness test, deforestation

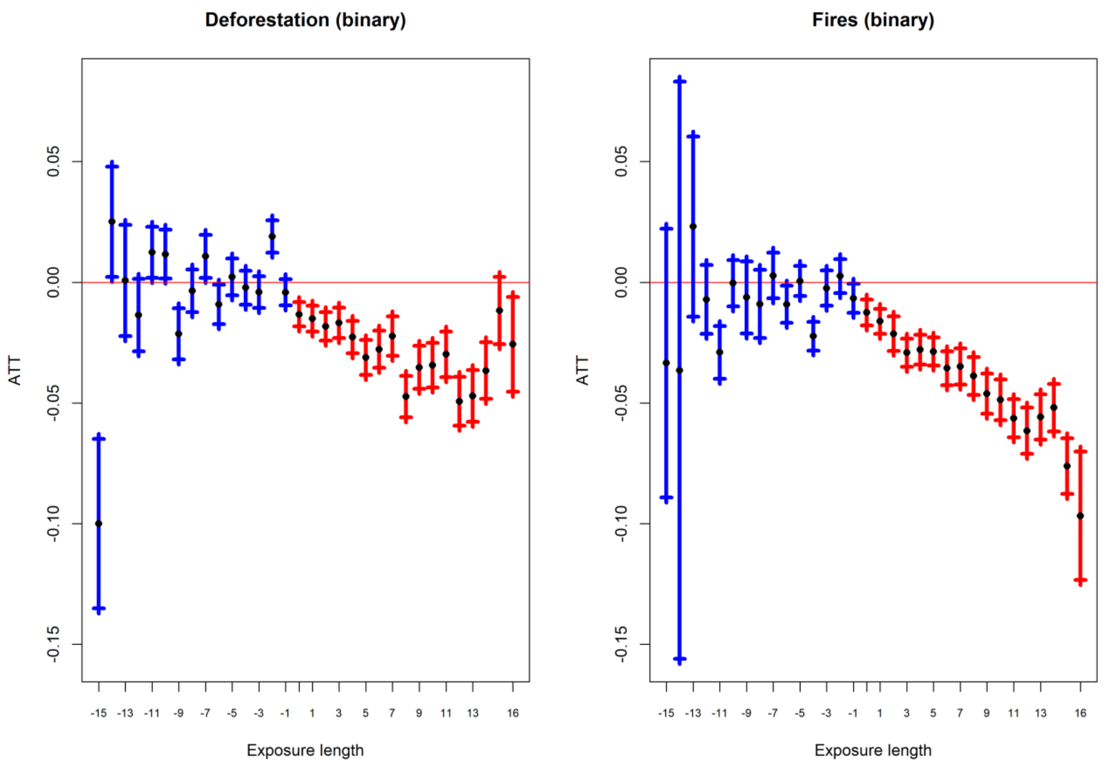
	(1)	(2)		(3)	(4)		(5)	(6)	
	All PAs			Only Brazilian PAs			Only Brazilian PAs with inst. var.		
	Critical groups	Top-five weights (rob.)	Percent diff [(2)/(1) -1]	Critical groups	Top-five weights (rob.)	Percent diff [(4)/(3) - 1]	Critical groups	Top-five weights (rob.)	Percent diff [(6)/(5) - 1]
ATT	-0.025*	-0.0255***	2%	-0.0279***	-0.0319***	14%	-0.0321***	-0.0342**	7%
	[0.0037]	[0.0037]		[0.0068]	[0.0045]		[0.0053]	[0.0046]	
N	415,080	431,550		145,224	349,776		241,074	349,776	
N clusters	23,060	23,975		8,068	19,432		13,393	19,432	

Table 7 Robustness test, fires

	(1)	(2)		(3)	(4)		(5)	(6)	
	All PAs			Only Brazilian PAs			Only Brazilian PAs with inst. var.		
	Critical groups	**Top-five weights (rob.)**	**Percent diff [(2)/(1) -1]**	**Critical groups**	**Top-five weights (rob.)**	**Percent diff [(4)/(3) -1]**	**Critical groups**	**Top-five weights (rob.)**	**Percent diff [(6)/(5) -1]**
ATT	-0.0601***	-0.0273***	-55%	-0.0624***	-0.0338***	-46%	-0.0538***	-0.0321***	-40%
	[0.0073]	[0.0030]		[0.0096]	[0.0039]		[0.0065]	[0.0042]	
N	209,628	429,750		148,914	348,138		201,546	348,138	
N clusters	11,646	23,875		8,273	19,341		11,197	19,341	

4.3 Dynamic effects

Figure 5 Event Study, whole 1 SD caliper sample, all groups



In this section we provide further information about the significant pre and post-treatment effects, interpreting them as manifestations of the four "ideal types" of effect dynamics depicted in figure 1. Only systematic effects are examined, i.e., those whose significance was observed in more than one "subsample", namely: (i) all PA types, (ii) only indigenous lands, (iii and iv) only subnational or national conservation units, (v and vi) only Brazil with or without institutional covariates. The event studies here described, which contain all groups, without any attempt to rule out significant pre-treatment effects, are found in figure 5 and in appendix 2.

[Positive ex-post at lag -2] A noteworthy finding is the positive pre-protection effect on deforestation observed at lag -2 in all five samples, except for the one involving only indigenous lands (figure 5; Appendix 2, figures A.2.1.1, A.2.2.1, to A.2.3.1). This effect can be attributed to the group treated in 2006. Its deforestation level in 2004 was larger than unprotected pixels. The group's pixels were evenly distributed between subnational and national conservation units in Brazil and most of them belonged to "direct-use" units, which are more permissive regarding resource extraction and land usage (Nolte et al., 2013). Importantly, this positive pre-treatment effect counterbalanced the negative pre-treatment effect of the 2009 group which was also captured into lag -2's effect.

[Positive ex-post at lag -10 in TIs] Positive and negative pre-treatment effects on deforestation at lags -10 and -9, respectively, were observed for the case of indigenous lands and in the Brazilian sample with institutional covariates. Focussing on indigenous lands, the two effects were due to the group treated in 2016. It must be highlighted that even with the effects observed many years before creation, they were still within the time span that indigenous lands take to be created (FUNAI, 2023)[14]. This suggests that these effects may be evidence of deforesters' forward-looking behaviour. The initially perceived gain, ten years before protection, from rushing to harvest forest resources and claim land, may disappear after one year as deforesters learn that governmental presence truly increased in the zone that is to be protected.

[Systematic fire effects] Negative pre-protection effects on fires four years and eleven years before protection were systematically observed across all (1-SD-caliper-matched) sub-samples (except, for the pre-effect at lag -4, for subnational conservation units). Whereas the pre-effect at lag -4 had its origin in Brazilian national conservation units and indigenous lands, the one at lag -11 also occurred in subnational conservation units. The cohorts associated with these pre-treatment effects were 2008, 2009 and 2016, for the case of lag -4, and 2016 for lag -11 (judging for the most recurrent critical group in each case).

[CUs more negative pre-treat effects] Another peculiarity of conservation units' event studies for deforestation is six positive pre-treatment effects, considering both national and subnational units (at lags -13, -7, -5, -3, -2, -1), whereas only one positive pre-treatment effect was observed in indigenous lands (at lag -10). This is another evidence that conservation units are more prone to experiencing rises in deforestation prior to protection. A similar, albeit weaker, pattern was

[14] The average duration of the creation process was of 10.5 years among the 127 Brazilian indigenous lands whose initial and final phases of creation dates were both available and consistent – meaning, by consistency, the initial date coming before the final date.

observed for fires. Whereas conservation units presented two or three positive pre-treatment effects, indigenous lands presented only one. A related result is that the lack of overall significance of subnational PAs against deforestation was due, in the sample without critical groups, to the significant inhibition effect up to the fifth year after creation being counterbalanced by a "stimulation effect", i.e., a larger inner deforestation, seven years and also ten to twelve years after creation. The same was observed for fires, whose level was larger inside subnational units than in unprotected land, with positive post-protection effects observed in leads 2, 8, 9, 11, 13, 14.

[Post-treatment patterns] Regarding post-treatment effects on deforestation, two prominent patterns emerge. Firstly, a two-year delay in the impact was observed only in indigenous lands. This could be attributed to enforcement not increasing immediately after the creation of indigenous lands (BenYishay et al. 2017). Secondly, a (approximately gradual) effect magnification was observed in all six subsamples (appendix 3, figures A.2.1.1, A.2.2.1, up to A.2.6.1, but except for A2.4.1). It is an evidence that enforcement staff take time to learn how to improve their performance. Gradual magnification was also true for fires, except in the case of subnational units, where fires were more frequent than in unprotected land. Such pattern may be both evidence of "learning-by-enforcing" and, relatedly, of reduced deforestation, which is a main purpose of fire usage. A delayed decrease was also true in indigenous land, but at one year after protection.

5 Discussion

A methodological contribution was made in this study by devising and applying a novel causal inference approach to estimate the impact of protected areas' on deforestation, which was robust to self-selection of sites for protection, to the staggered nature of protection, to unobservable drivers of protection and to confounders capturing concurrent environmental policies. The proposed analytical framework includes two key components, which are new to the literature branch assessing PAs' effect. First, cohort-time refined effect estimates. Second, an event study examination of effect's dynamics across protection length. It was demonstrated the need to remove some cohorts in order to ensure identification by the means of the parallel trends assumption, something ignored so far in the specific literature at the cost of a considerable bias, as here evidenced. These exclusions refined the variation found in the observational dataset available, isolating its causal component. Besides ensuring identification, the approach unveiled important dynamic patterns in the effect, including a deforestation above the unprotected level at two years before protection and a progressively magnified decrease after protection, the latter also the case for fires. Furthermore, specific dynamics were observed by type of PA, with conservation units being more exposed to pre-protection rises in deforestation and fires, while indigenous lands experienced a delayed post-protection decrease in deforestation, but not in fires.

The different effects of the different PA types, detected in the present paper, align with previous research in the field. A larger effect on deforestation was estimated by Nelson and Chomitz (2011, table 7) for indigenous lands, but, conversely, Amin et al. (2019), estimated conservation units to have a bigger effect. Diverging from the two studies and also from this paper, Herrera et al. (2010) estimated equivalent effects for the two PA types. But the greatest opposition to this paper's results, in which indigenous lands had the first and second largest inhibition effect on deforestation and fires, respectively, comes from BenYishay et al. (2017), who found a null effect of such PA type[15]. The divergence may be due to three differences with the analysis here conducted. First, BenYishay et al's. (2017) estimates relied strictly on before-and-after variation, as their sample contained only indigenous lands. In contrast, in this paper and in the majority of studies measuring deforestation inhibition by indigenous' lands - which all found a significantly negative effect -, the control group is made of non-PAs (Nelson and Chomitz, 2011, Qin et al., 2023, Herrera et al., 2019, Amin et al., 2019). This is an issue because indigenous people generally already inhabit the land whose property right they claim. Therefore, pressure on forest resources after recognition should not change considerably, exactly as BenYishay et al. (2017) found. Secondly, the author's measure of deforestation is a proxy that does not directly captures forest suppression, differing from the metric adopted here and in most of the literature. Third, despite that authors have also relied on matching, their period of analysis started eight years before the one adopted in this paper. To finish, the delayed impact of indigenous lands on deforestation, here uncovered, may be a reason why the authors, by ignoring effect dynamics, failed to attest the effectiveness of such change.

Pixels of different treatment statuses were impacted differently by concurrent policies in the cases of deforestation and fires (unprotected and protected, respectively, being more impacted). But in both cases the impact was negative, i.e., concurrent policies reduced the specific forest disturbance. What finds parallel in previous studies. Many of them have demonstrated the effectiveness of the Brazilian deforestation control program from 2004 to 2014, which involved not only the creation of PAs, but also rationing of agricultural credit to illegal deforesters and increasing on-site and remote monitoring and sanctioning (Assunção et al., 2020, Hargrave and Kis-Katos, 2013, Börner et al., 2015). Nevertheless, despite some studies measuring the PA effect mentioning, *en passant*, these concomitant interventions, none have explicitly controlled for them in their empirical analyses. A rather indirect approach, of breaking down analysis in pre and post-2004 sub-periods, was followed by Pfaff et al. (2015). This, despite automatically eliminating confounders in the pre-2004 period, fails to deliver a bias-free estimate reflecting the post-2004 sub-period, which is the most policy-relevant phase, given the substantial change in the incentives

[15] This explanation is in direct opposition to what is argued by Nelson and Chomitz (2011) regarding fires at the Latin American and Caribbean level.

to deforestation triggered by the enhanced policy (Börner et al., 2015). Nevertheless, Pfaff et al.'s (2015) and this paper's results converge for deforestation, but not for fires. The authors found a slightly lower effect in the post-2004 sub-period and here, similarly, a smaller effect on deforestation was detected without controlling for the non-PA policies strengthened after 2004. But a larger effect was found for fires, a discrepancy with Pfaff et al., (2015) which resides in two particularities of this paper. First that non-PA policies were explicitly controlled for. Second, the analysis period begun four years later and ended twelve years after. Additionally, BenYishay et al. (2017) found no influence of post-2004 policy strengthening, after interacting a 2004 binary variable with indigenous land legalization (a measure of the stage of completion of indigenous lands' creation), at odds with the results in this paper, which may be attributed to the differences between this and authors' studies, as described in the previous paragraph.

Despite not assessed by previous studies, the PA effect dynamics found in this paper aligns with results and arguments from other papers. For instance, the enhancement of the effect on deforestation and fires along the post-protection period is both in line with studies of PA enforcement arguing that such activity is subject to learning and also with the few empirical results showing that the effect increases along protection time (Geldman et al. 2015, Afriyie et al., 2021, West et al., 2022, fig.5, Duncanson et al., 2023). For another side, the post-protection rise in fires inside subnational PAs could be due to enforcement being reduced some years after creation, in line with studies pointing that protection is only effective under diligent monitoring and sanctioning (Lima and Peralta, 2017, p.810, Kuempel et al., 2018, Afriyie et al., 2021).

Regarding pre-protection effects, conservation units sometimes undergo a conflicting process of creation, with contestation from local actors (Brito, 2010, p.63, Temudo, 2012, Pedlowski et al., 2013). This could explain the six positive pre-protection effects on deforestation that conservation units were exposed to, the most notorious of them occurring two years before creation. The significance of such pre-treatment effect was unequivocal and persistent even after elimination of some groups, being a robust finding of this paper which has no parallel in the literature so far. Fires were also subject to (a few) positive pre-protection effects. The policy relevance of these findings is clear: policymakers should be aware that the creation of conservation units induces a "forest rush" two years before its legal completion, so that enforcement in the zone to be legally protected must be increased in advance as a preventative measure.

A leap in deforestation was observed by about the moment that the legal process of indigenous land establishment is started, which is of 10.5 years before completion. This suggests a potential rush to appropriate land and forest resources before prohibition. This is in line with Baragwhanath and Bayi (2020) result that only areas where indigenous property has been fully legally recognized can reduce deforestation. But, diverging from authors' results, the leap was followed, in the ninth year before full recognition of indigenous rights, by a fall in deforestation, probably due to the increased presence of the State during the early phase of PA creation. This is an indication that the mere possibility of indigenous property recognition may change the behavior of forward-looking deforesters.

6 Concluding remarks

The results achieved show that PAs' effects estimates from previous studies are likely to be biased due to unobservable drivers of protection effectiveness, uniform aggregation of PA cohorts with heterogeneous effects, non-parallel trends and failure to control for simultaneous non-protection policy. We showed that the parallel trends assumption is powerful enough to avoid these biases, together with explicit policy covariates, provided that cohorts are appropriately selected. This last task, which has been so far ignored in PA literature, must become a standard practice, the same way that matching is.

The non-robustness of the magnitudes of fires' effects to the "critical groups" selection approach shows that consistent justification of criteria is needed, as well as an assessment of robustness. A related implication is that different PA cohorts may have different histories of deforestation inhibition, being more and less effective at different stages of their lifetime, another reason for avoiding aggregations that treats them as homogeneous.

The policy implications of the findings are noteworthy. The effect dynamics must be accounted for in the cost-benefit analysis informing decisions about creating new protected areas. They may make a difference depending on the social discount rate adopted. Importantly, policy-makers should also be aware that publicizing the information that a site will be protected may lead to an increase in forest disturbance, as forward-looking deforesters anticipate losing access to forest resources. This possibility proved strong enough in regards to conservation units capacity to inhibit deforestation, outweighing any perceived increases in enforcement during the creation process.

Emphasis should be placed on the "forest rush" effect observed two years before the creation of conservation units. It is a warning that PA creation should not be seen solely as a legal process of changing the tenure status of a geographical zone, but, more broadly, as means to align the expectations of forward-looking deforesters with governmental conservation goals. That means signalling that sanction probability will not only increase after creation, but immediately, thus leaving no time for rushing to exploit resources.

Acklowledgements

The research was funded by the Brazilian National Council for Scientific and Technological Development (CNPq) [grants 350303/2021-5, 304967/2021-01, 314473/2020-3, 140877/2018-5, 140379/2018-5, 444418/2018-0], São Paulo State Research Support Foundation (FAPESP) [grants

2021/07660-2, 2020/16457-3, 2020/15230-5, 2020/08916], and by the Coordination for the Improvement of Higher Education Personnel (CAPES) [Finance Code 001].

References

Afriyie, J. O., Asare, M. O., Osei-Mensah, J., & Hejcmanová, P. A. V. L. A. (2021). Evaluation of long-term law enforcement monitoring in a West African protected area. Oryx, 55(5), 732-738.

Amin, A., Choumert-Nkolo, J., Combes, J. L., Motel, P. C., Kéré, E. N., Ongono-Olinga, J. G., & Schwartz, S. (2019). Neighborhood effects in the Brazilian Amazônia: Protected areas and deforestation. Journal of Environmental Economics and Management, 93, 272-288.

Angelsen, A. (1999). Agricultural expansion and deforestation: modelling the impact of population, market forces and property rights. Journal of development economics, 58(1), 185-218.

Arriagada, R. A., Echeverria, C. M., & Moya, D. E. (2016). Creating protected areas on public lands: is there room for additional conservation?. PloS one, 11(2), e0148094.

Assunção, J., Gandour, C., & Rocha, R. (2015). Deforestation slowdown in the Brazilian Amazon: prices or policies?. Environment and Development Economics, 20(6), 697-722.

Assunção, J., Gandour, C., Rocha, R., & Rocha, R. (2020). The effect of rural credit on deforestation: Evidence from the Brazilian Amazon. The Economic Journal, 130(626), 290-330.

Baragwanath K, Bayi E. (2020) Collective property rights reduce deforestation in the Brazilian Amazon. Proceedings of the National Academy of Sciences. 2020 Aug 25;117(34):20495-502.

Barnes, A. E., Davies, J. G., Martay, B., Boersch-Supan, P. H., Harris, S. J., Noble, D. G., ... & Robinson, R. A. (2023). Rare and declining bird species benefit most from designating protected areas for conservation in the UK. Nature Ecology & Evolution, 7(1), 92-101.

BenYishay, A., Heuser, S., Runfola, D., & Trichler, R. (2017). Indigenous land rights and deforestation: Evidence from the Brazilian Amazon. Journal of Environmental Economics and Management, 86, 29-47.

Börner J, Marinho E, Wunder S. Mixing carrots and sticks to conserve forests in the Brazilian Amazon: a spatial probabilistic modeling approach. PloS one. 2015 Feb 4;10(2):e0116846.

Bowman, K. W., Dale, S. A., Dhanani, S., Nehru, J., & Rabishaw, B. T. (2021). Environmental degradation of indigenous protected areas of the Amazon as a slow onset event. Current Opinion in Environmental Sustainability, 50, 260-271.

Brito, D. M. C. (2010) Conflitos socioambientais na gestão de unidades de Conservação: o caso da reserva biológica do lago Piratuba/AP. 375f. PhD Dissertation, Federal University of Amapá state, Brazil.

Callaway, B., & Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. Journal of Econometrics, 225(2), 200-230.

Carrero, G. C., Walker, R. T., Simmons, C. S., & Fearnside, P. M. (2022). Land grabbing in the Brazilian Amazon: Stealing public land with government approval. Land Use Policy, 120, 106133.

Debelo, A. R. (2012). Contesting views on a protected area conservation and development in Ethiopia. Social sciences, 1(1), 24-46.

Duncanson, L., Liang, M., Leitold, V., Armston, J., Krishna Moorthy, S. M., Dubayah, R., ... & Zvoleff, A. (2023). The effectiveness of global protected areas for climate change mitigation. Nature Communications, 14(1), 2908.

Eva, H.D., Huber, O., 2005. A proposal for defining the geographical boundaries of Amazonia. Luxembourg: Office for Official Publications of the European Communities, Luxembourg.

Ferraro, P. J., & Hanauer, M. M. (2014). Quantifying causal mechanisms to determine how protected areas affect poverty through changes in ecosystem services and infrastructure. Proceedings of the national academy of sciences, 111(11), 4332-4337.

FINBRA (2023) Municipal budget accounting data, Brazilian National Treasury, period: 2003-2020, variable: expediture by function (environmental management). Available at: https://www.tesourotransparente.gov.br/publicacoes/finbra-dados-contabeis-dos-municipios-1989-a-2012/2012/26

FUNAI (2023) Brazilian National Indigenous Foundation. Digital map of indigenous lands with dates of achievement of milestones of the indigenous land creation process. Available at: https://geoserver.funai.gov.br/geoserver/web/?0

Geldmann, J., Coad, L., Barnes, M., Craigie, I. D., Hockings, M., Knights, K., ... & Burgess, N. D. (2015). Changes in protected area management effectiveness over time: A global analysis. Biological Conservation, 191, 692-699.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. Journal of Econometrics, 225(2), 254-277.

Hargrave, J., & Kis-Katos, K. (2013). Economic causes of deforestation in the Brazilian Amazon: a panel data analysis for the 2000s. Environmental and Resource Economics, 54, 471-494.

Herrera, D., Pfaff, A., & Robalino, J. (2019). Impacts of protected areas vary with the level of government: Comparing avoided deforestation across agencies in the Brazilian Amazon. Proceedings of the National Academy of Sciences, 116(30), 14916-14925.

Holmes, G. (2014). Defining the forest, defending the forest: Political ecology, territoriality, and resistance to a protected area in the Dominican Republic. Geoforum, 53, 1-10.

IBAMA (2023) Brazilian National Institute of Environment and Natural Resources (environmental police). Database of environmental sanctions and embargoes. Land embargoed due to deforestation Variable: land area embargoed. Available at: https://servicos.ibama.gov.br/ctf/publico/areasembargadas/ConsultaPublicaAreasEmbargadas.php

IBAMA (2023) Brazilian National Institute of Environment and Natural Resources (environmental police). Geographical coordinates of headquarters. Available at: https://dadosabertos.ibama.gov.br/dataset/unidades-ibama/resource/93829d42-5811-47c3-8ea6-7d3912cc84b6

Jachmann, H. (2008). Monitoring law-enforcement performance in nine protected areas in Ghana. Biological conservation, 141(1), 89-99.

Keles, D., Pfaff, A., & Mascia, M. B. (2023). Does the Selective Erasure of Protected Areas Raise Deforestation in the Brazilian Amazon?. Journal of the Association of Environmental and Resource Economists, 10(4), 1121-1147.

Klingler, M., & Mack, P. (2020). Post-frontier governance up in smoke? Free-for-all frontier imaginations encourage illegal deforestation and appropriation of public lands in the Brazilian Amazon. Journal of Land Use Science, 15(2-3), 424-438.

Kuempel, C. D., Adams, V. M., Possingham, H. P., & Bode, M. (2018). Bigger or better: the relative benefits of protected area network expansion and enforcement for the conservation of an exploited species. Conservation Letters, 11(3), e12433.

Lima, D. M., & Peralta, N. (2017). Developing Sustainability in the Brazilian Amazon. Journal of Latin American Studies, 49(4), 799-827.

Morgan, S. L., & Winship, C. (2007). Counterfactuals and causal inference. Cambridge University Press. First Edition.

Morello, T. F., Ramos, R. M., Anderson, L. O., Owen, N., Rosan, T. M., & Steil, L. (2020). Predicting fires for policy making: Improving accuracy of fire brigade allocation in the Brazilian Amazon. Ecological Economics, 169, 106501.

Nelson, A., Chomitz, K.M., 2011. Effectiveness of Strict vs. Multiple Use Protected Areas in Reducing Tropical Forest Fires: A Global Analysis Using Matching Methods. PLoS ONE 6, e22722. https://doi.org/10.1371/journal.pone.0022722

Nolte, C., Agrawal, A., 2013. Linking Management Effectiveness Indicators to Observed Effects of Protected Areas on Fire Occurrence in the Amazon Rainforest: Management Effectiveness and Fire. Conservation Biology 27, 155–165. https://doi.org/10.1111/j.1523-1739.2012.01930.x

Paiva, R.J.O., Brites, R.S., Machado, R.B., 2015. The Role of Protected Areas in the Avoidance of Anthropogenic Conversion in a High Pressure Region: A Matching Method Analysis in the Core Region of the Brazilian Cerrado. PLOS ONE 10, e0132582. https://doi.org/10.1371/journal.pone.0132582

Pedlowski, M., Dale, V., & Matricardi, E. (1999). A criação de áreas protegidas e os limites da conservação ambiental em Rondônia. Ambiente & sociedade, 93-107.

Persson, J., Ford, S., Keophoxay, A., Mertz, O., Nielsen, J. Ø., Vongvisouk, T., & Zörner, M. (2021). Large differences in livelihood responses and outcomes to increased conservation enforcement in a protected area. Human Ecology, 49, 597-616.

Pfaff, A., Robalino, J., Herrera, D., & Sandoval, C. (2015). Protected areas' impacts on Brazilian Amazon deforestation: examining conservation–development interactions to inform planning. PloS one, 10(7), e0129460.

Pfaff, A., Robalino, J., Walker, R., Aldrich, S., Caldas, M., Reis, E., ... & Kirby, K. (2007). Road investments, spatial spillovers, and deforestation in the Brazilian Amazon. Journal of regional Science, 47(1), 109-123.

Qin, Y., Xiao, X., Liu, F., de Sa e Silva, F., Shimabukuro, Y., Arai, E., Fearnside, P.M., 2023. Forest conservation in Indigenous territories and protected areas in the Brazilian Amazon. Nat Sustain 1–11. https://doi.org/10.1038/s41893-022-01018-z

Roth, J. (2022). Pretest with caution: Event-study estimates after testing for parallel trends. American Economic Review: Insights, 4(3), 305-322.

Shi, H., Li, X., Liu, Xiaoping, Wang, S., Liu, Xiaojuan, Zhang, H., Tang, D., Li, T., 2020. Global protected areas boost the carbon sequestration capacity: Evidences from econometric causal analysis. Science of The Total Environment 715, 137001. https://doi.org/10.1016/j.scitotenv.2020.137001

Silva, J. M. C., de Castro Dias, T. C. A., da Cunha, A. C., & Cunha, H. F. A. (2019). Public spending in federal protected areas in Brazil. Land use policy, 86, 158-164.

Sims, K.R.E., 2010. Conservation and development: Evidence from Thai protected areas. Journal of Environmental Economics and Management 60, 94–114. https://doi.org/10.1016/j.jeem.2010.05.003

StataCorp Stata 13. Manual (in PDF). Entry: “teffects”. College Station, TX (2013)

Sze, J.S., Carrasco, L.R., Childs, D., Edwards, D.P., 2022. Reduced deforestation and degradation in Indigenous Lands pan-tropically. Nat Sustain 5, 123–130. https://doi.org/10.1038/s41893-021-00815-2

Temudo MP. “The White Men Bought the Forests” Conservation and Contestation in Guinea-Bissau, Western Africa. Conservation and Society. 2012 Jan 1;10(4):354-66.

Wendland, K.J., Baumann, M., Lewis, D.J., Sieber, A., Radeloff, V.C., 2015. Protected Area Effectiveness in European Russia: A Postmatching Panel Data Analysis. Land Economics 91, 149–168. https://doi.org/10.3368/le.91.1.149

West, T.A.P., Caviglia-Harris, J.L., Martins, F.S.R.V., Silva, D.E., Börner, J., 2022. Potential conservation gains from improved protected area management in the Brazilian Amazon. Biological Conservation 269, 109526. https://doi.org/10.1016/j.biocon.2022.109526

Appendix 1 Matching quality, all PAs

A.1 Deforestation

In the first stage of analysis, a one-to-one covariate matching with replacement on the Mahalanobis distance metric was pursued. It induced a clear improvement in the level of covariate balance, as compared with the matched sample. A slight further improvement was achieved with the introduction of the 1 SD caliper, but a more restrictive caliper, of half SD, brought no improvement (Table A.1.1, figures A.1.1 to A.1.4).

Table A.1.1 Matching sample sizes and percentage of covariates whose balance was "of concern" or "bad"

Matching	Treated	Control	Total	% reduction	%concern	%bad
Before matching	33,469	90,753	124,222	0%	22	35
No caliper	33,469	6,849	40,318	-68%	5	0
1 SD Caliper	26,755	6,284	33,039	-73%	0	0
0.5 SD Caliper	14,973	4,627	19,600	-84%	0	0

Figure A.1.1 Common support graph, non-caliper matching, before matching (left) and after matching (right)

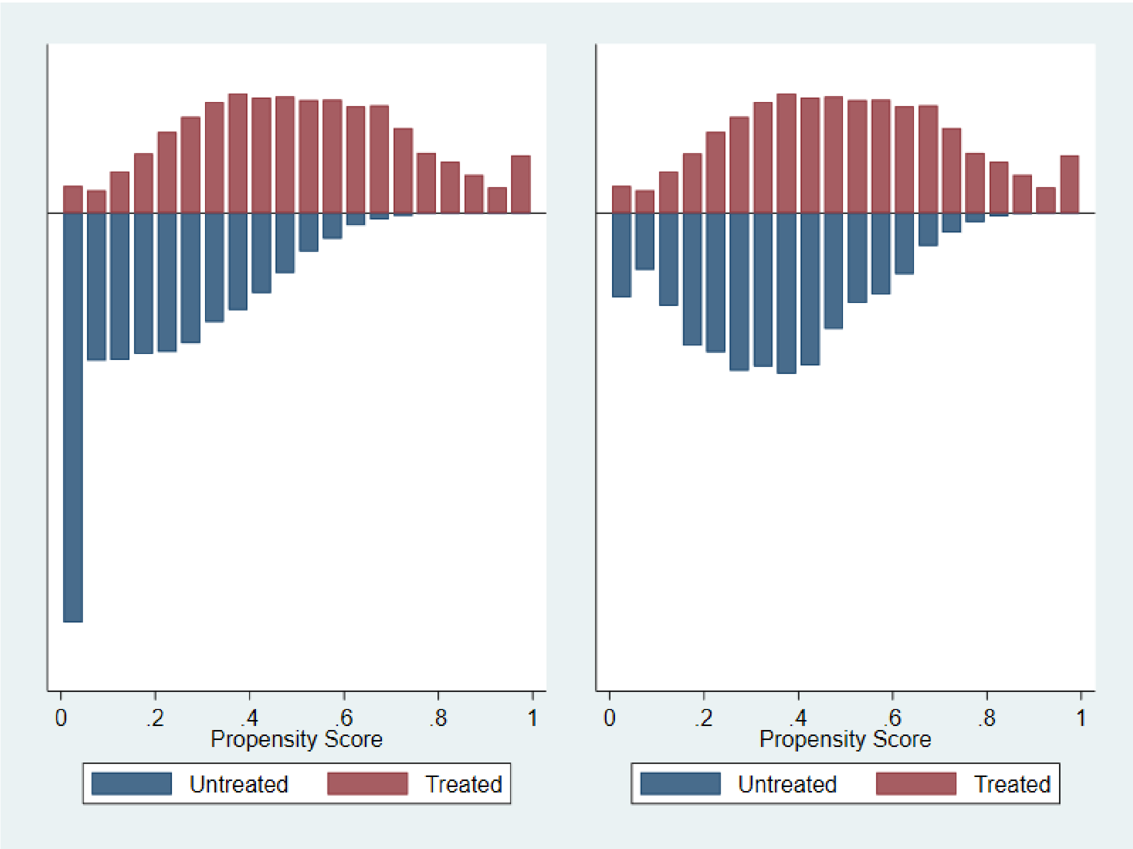


Figure A.1.2 Common support graph, 1SD-caliper matching, before matching (left) and after matching (right)

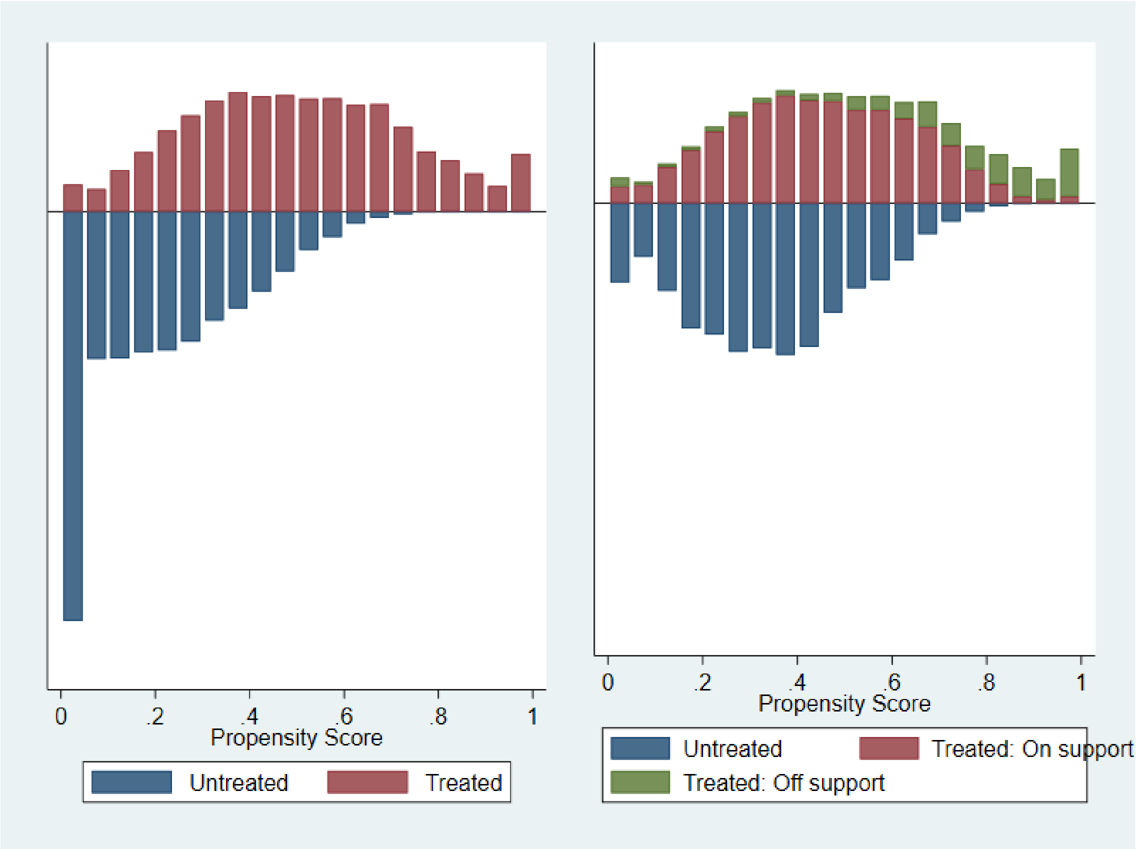


Figure A.1.3 Balance graph, non-caliper matching

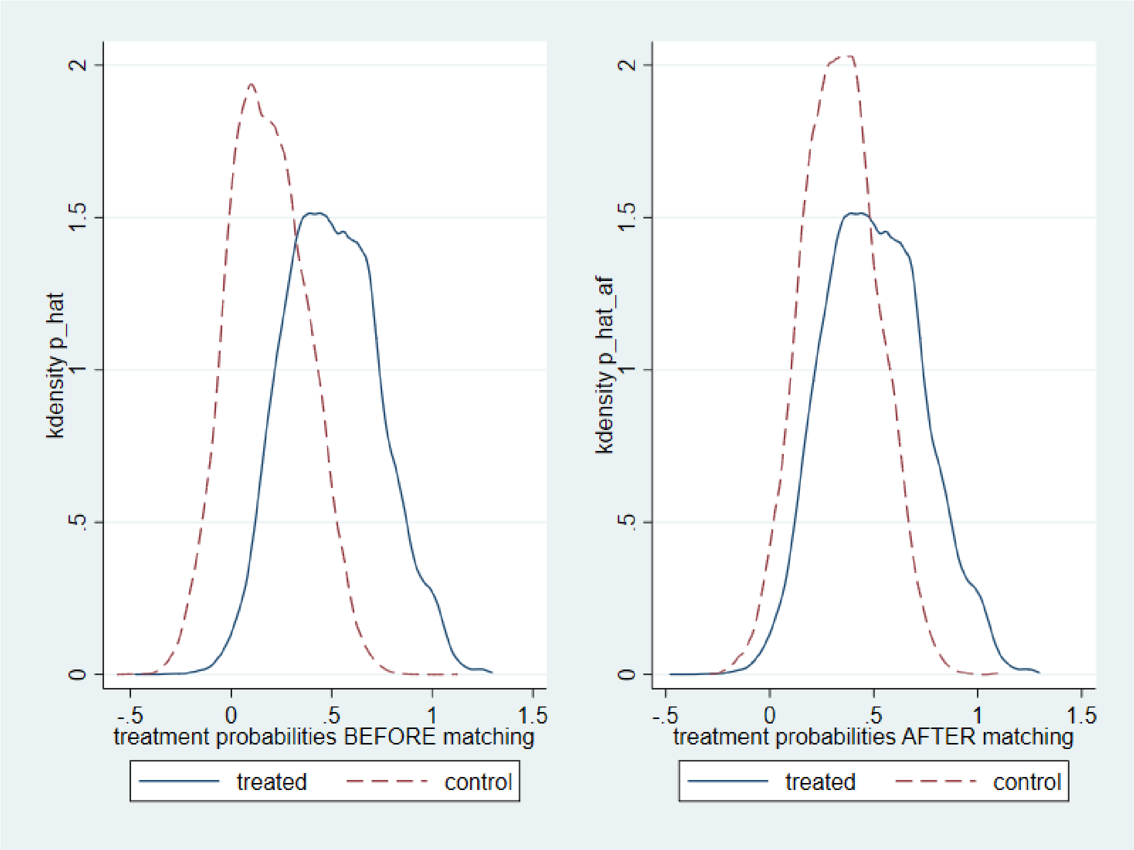
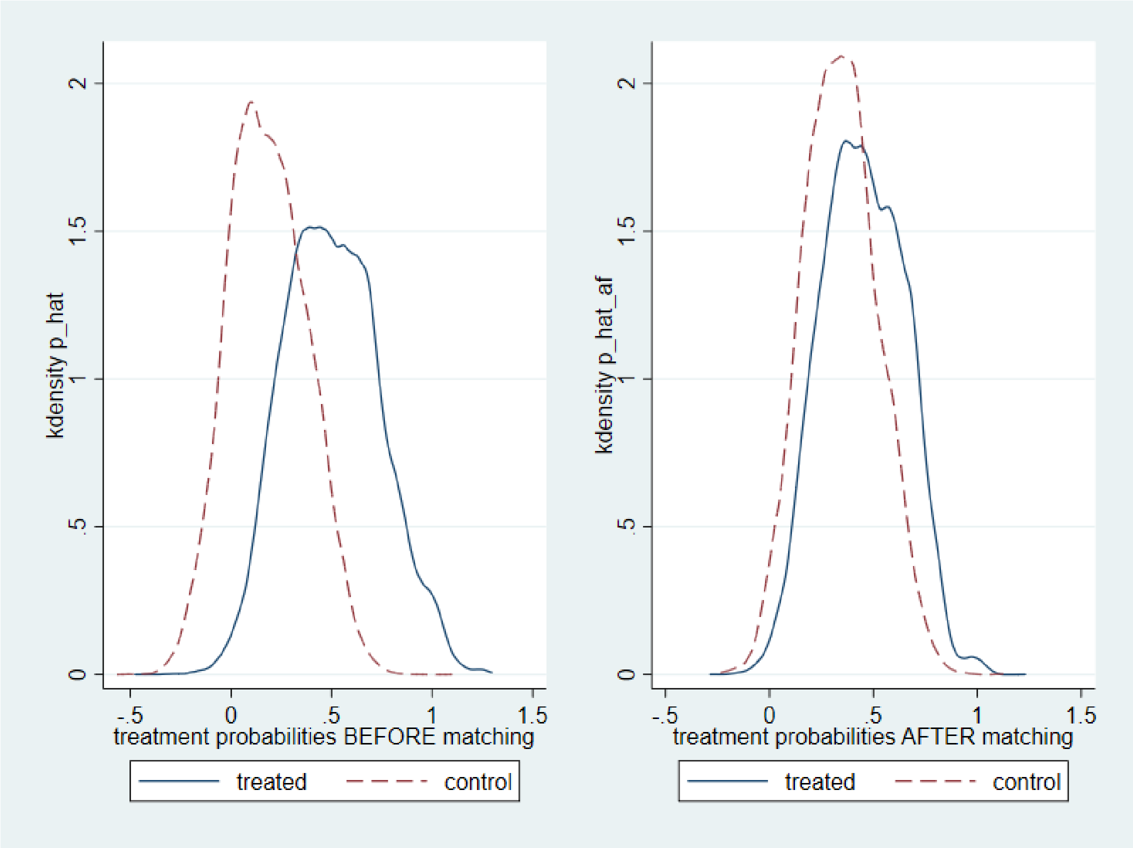


Figure A.1.4 Balance graph, 1SD-caliper matching



A.2 Fires

The covariate set used for matching in the case of fires was the same as in the case of deforestation, except for two additional variables, primary and secondary deforestation. Because of that small difference, nearly the same matching quality results were achieved (visually, i.e., in graphical terms, the results seem to be exactly equal; see graphs A.1.5 to A.1.8 below).

Table A.1.2 Matching sample sizes and percentage of covariates whose balance was "of concern" or "bad"

Matching	Treated	Control	Total	% redux	%concern	%bad
Before matching	33,469	90,753	124,222	0%	21	37
No caliper	33,469	6,867	40,336	-68%	6	0
1 SD Caliper	26,648	6,262	32,910	-74%	0	1
0.5 SD Caliper	14,774	4,522	19,296	-84%	0	0

Figure A.1.5 Common support graph, non-caliper matching, before matching (left) and after matching (right)

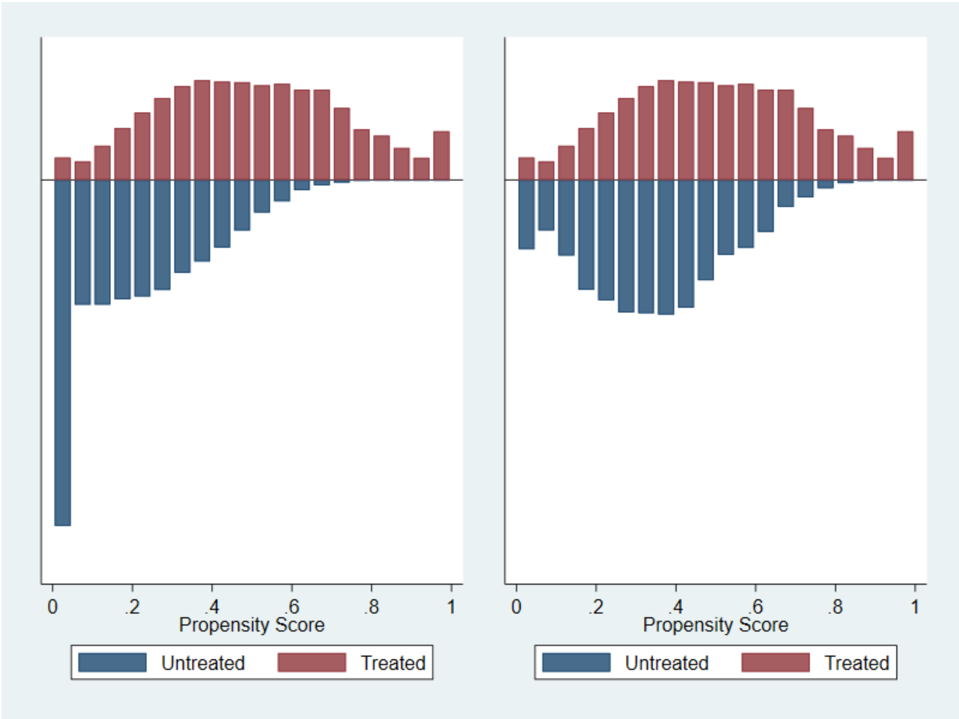


Figure A.1.6 Common support graph, 1SD-caliper matching, before matching (left) and after matching (right)

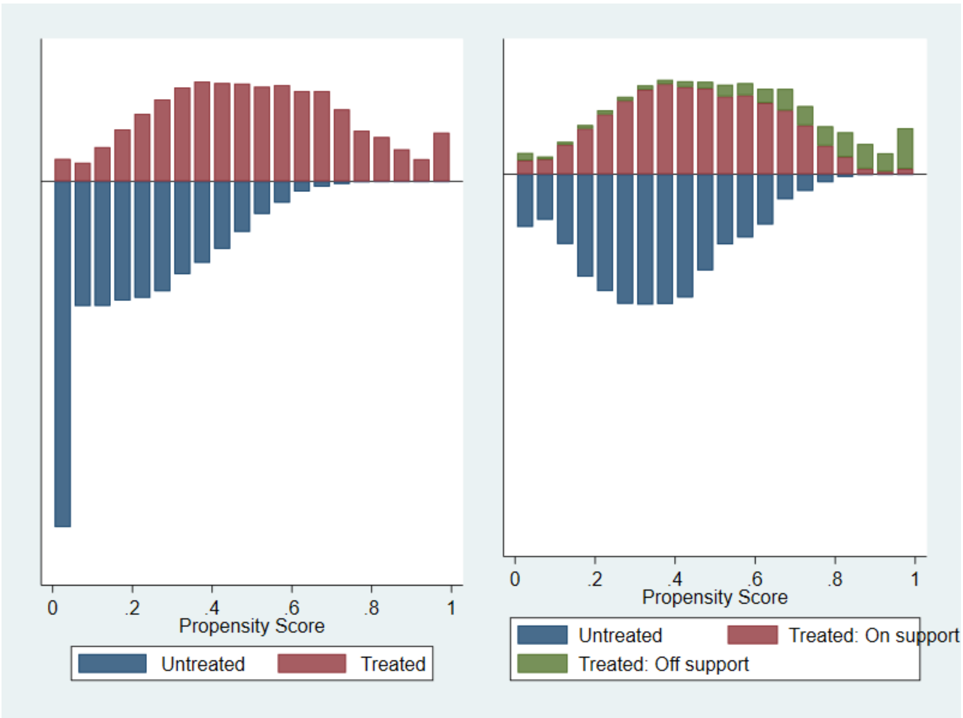


Figure A.1.7 Balance graph, non-caliper matching, before matching (left) and after matching (right)

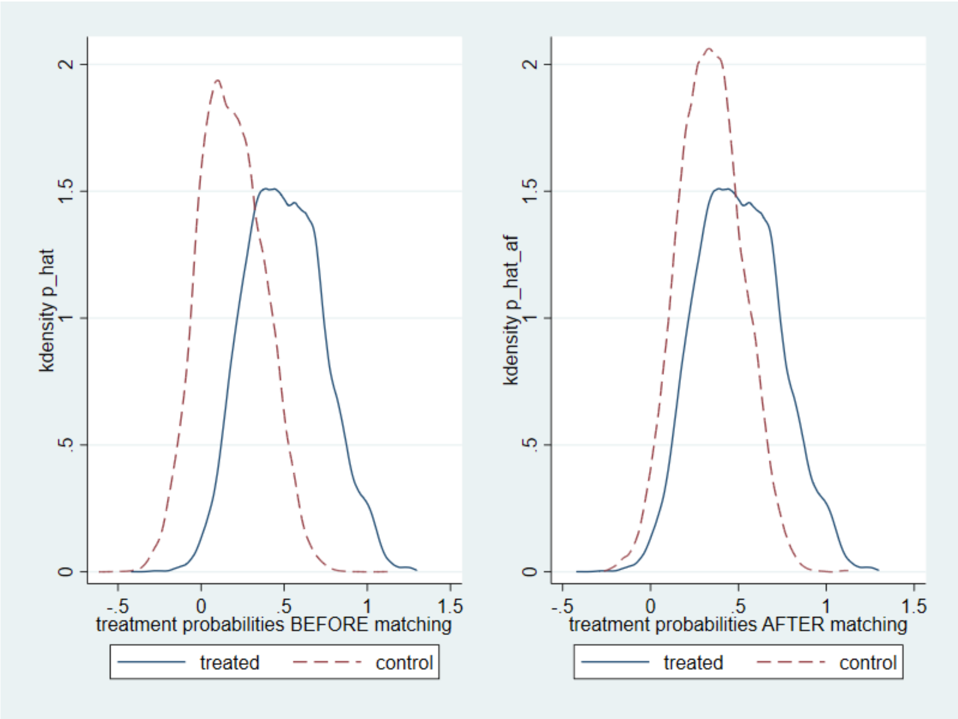
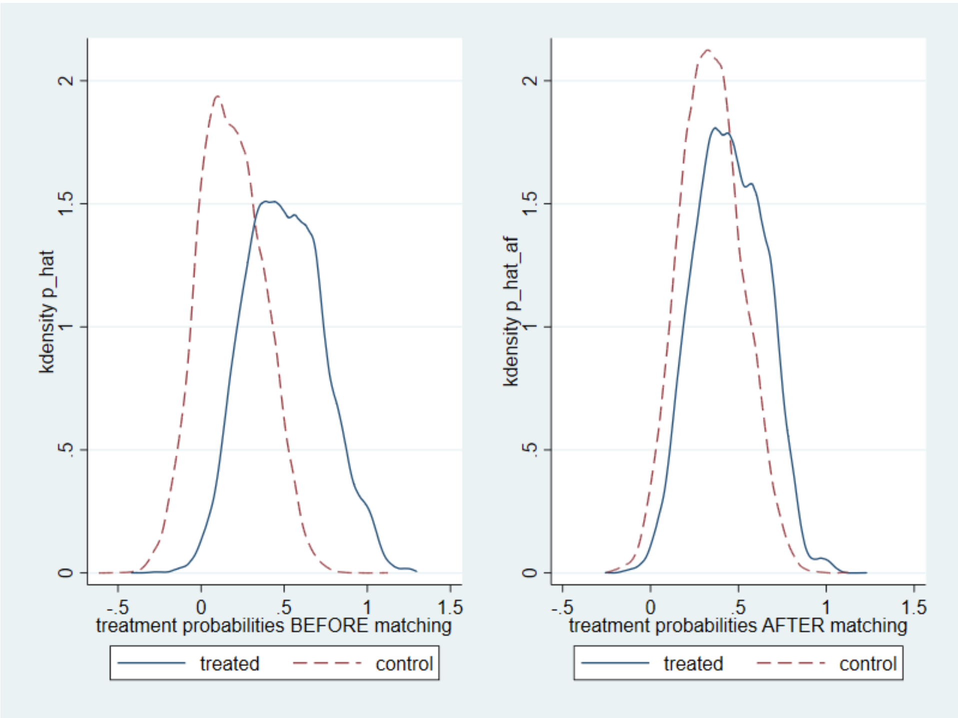


Figure A.1.8 Balance graph, 1SD-caliper matching, before matching (left) and after matching (right)



Appendix 2 Event study plots

A.2.1 Whole 1-SD caliper sample

A.2.1.1 All groups

Figure A.2.1.1 Event Study for deforestation, whole 1 SD caliper sample, all groups (blue = pre-treatment, red = post-treatment)

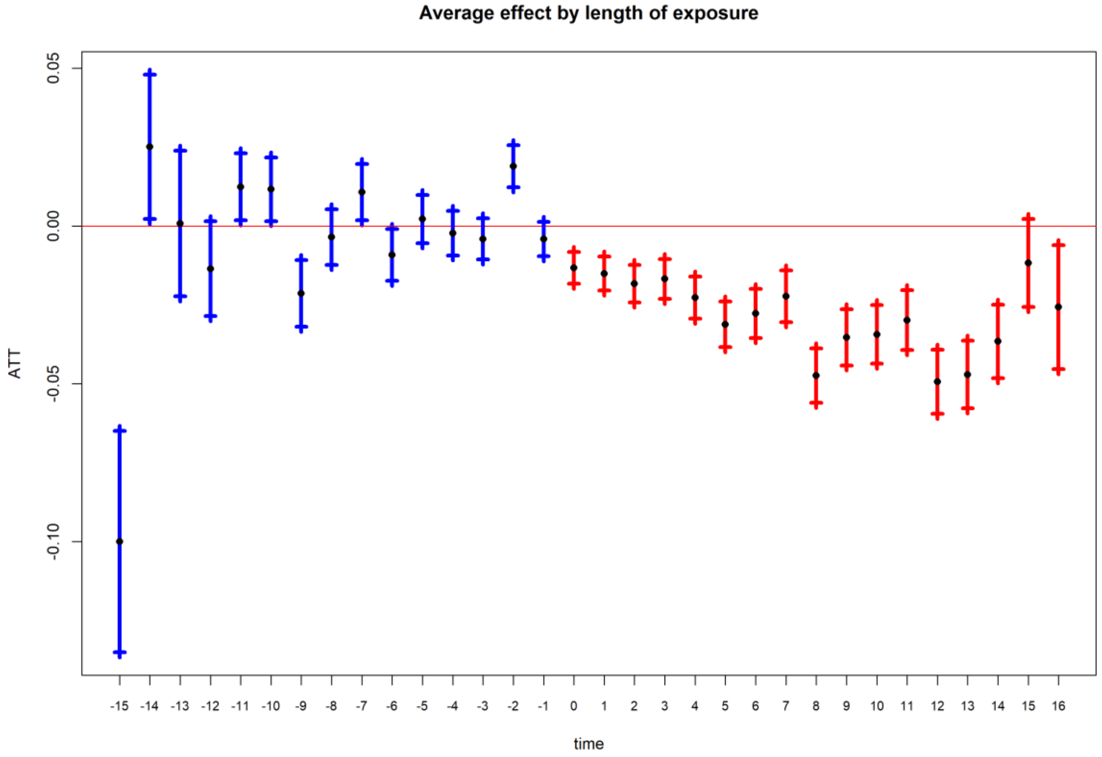
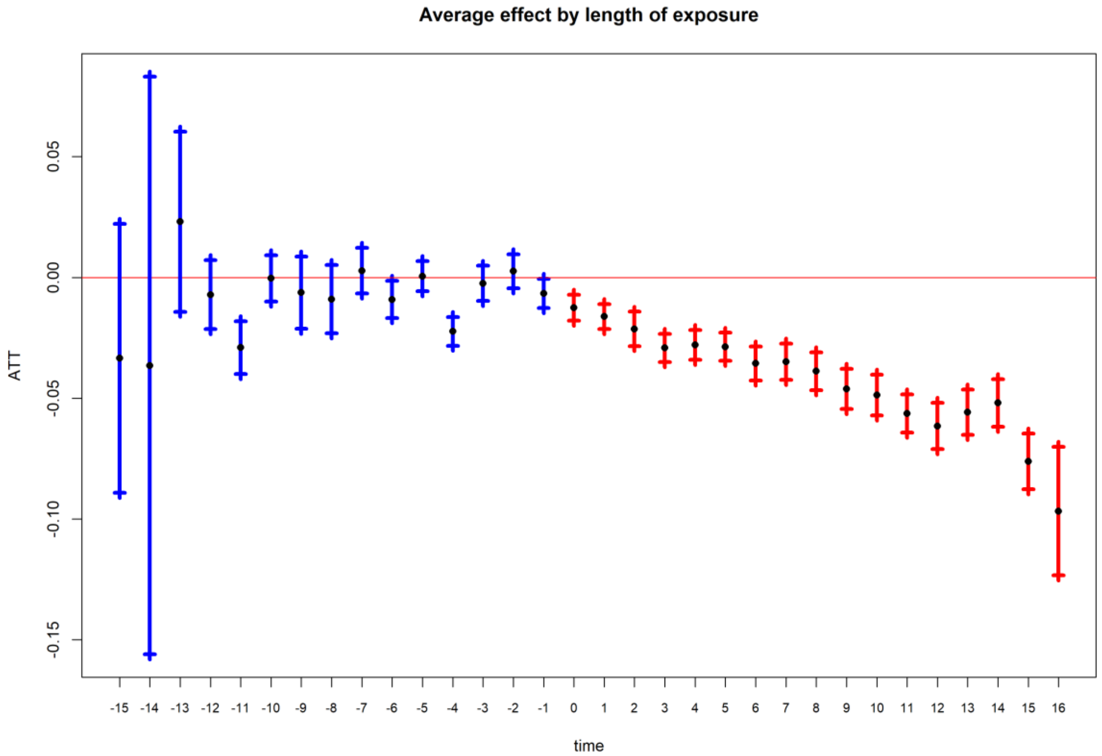


Figure A.2.1.2 Event Study for fires, whole 1 SD caliper sample, all groups (blue = pre-treatment, red = post-treatment)



A.2.1.2 Without critical groups

Figure A.2.1.3 Event Study for deforestation, whole 1 SD caliper sample, without critical groups

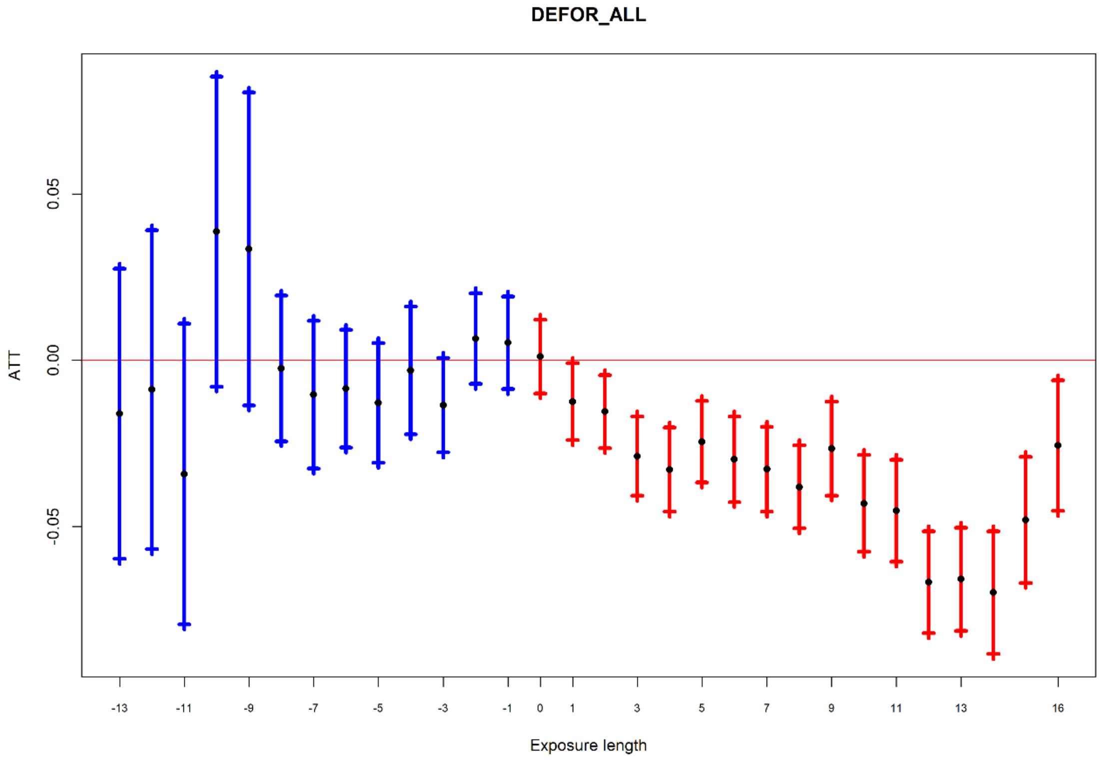
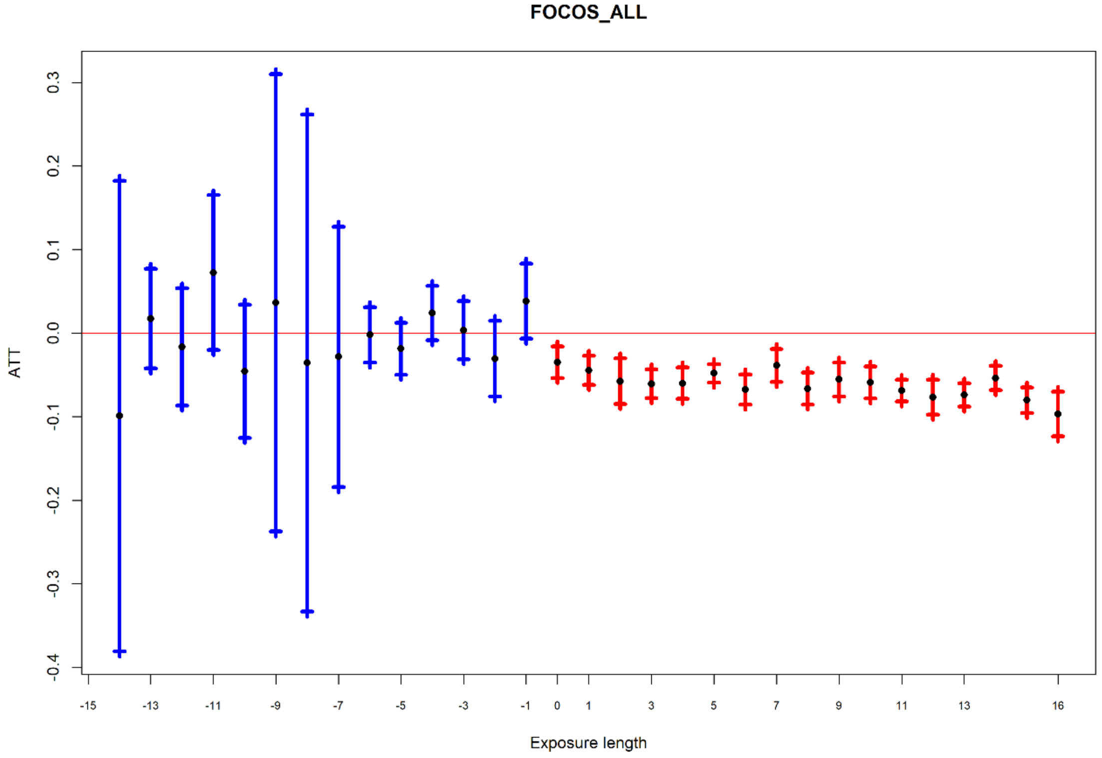


Figure A.2.1.4 Event Study for fires, whole 1 SD caliper sample, without critical groups



A.2.2 Brazil-only sample (with institutional covariates)

A.2.2.1 All groups

Figure A.2.2.1 Event Study for deforestation, Brazil-only sample with institutional variables, all groups

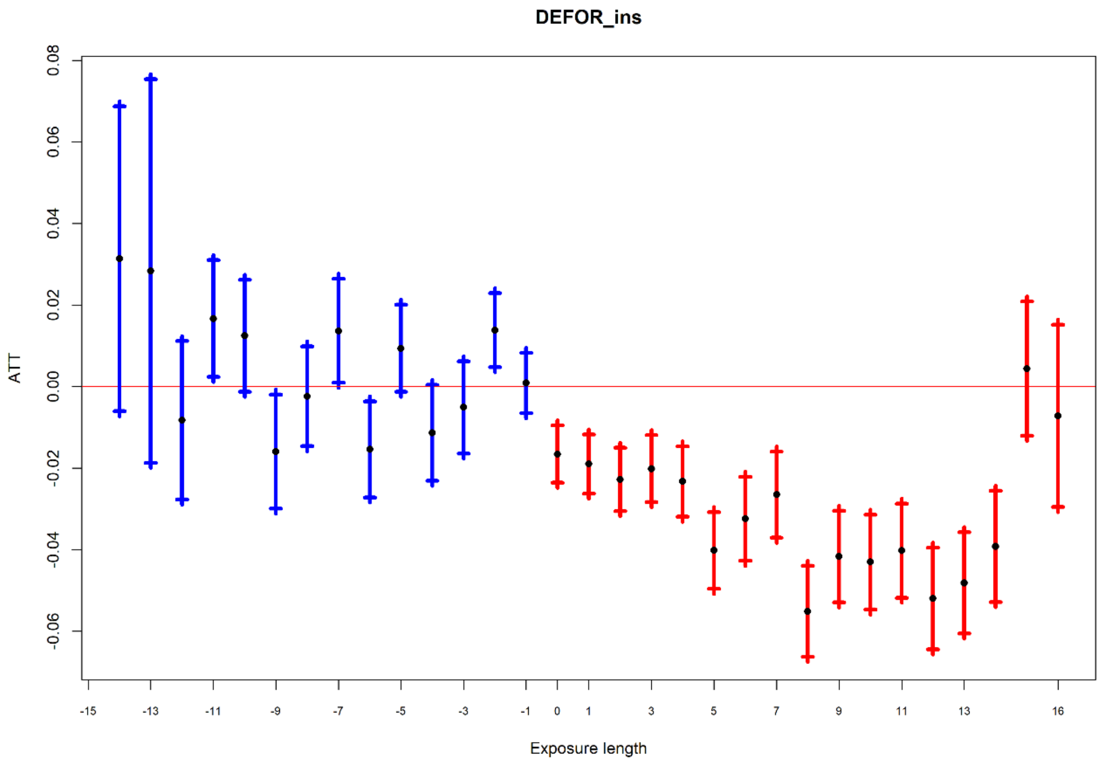
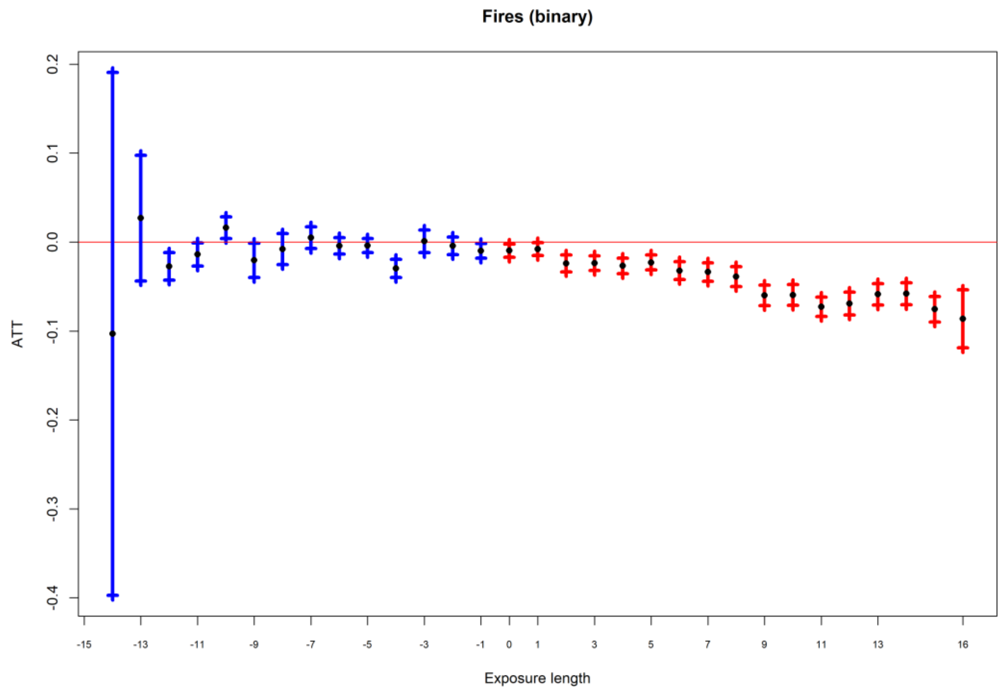


Figure A.2.2.2 Event Study for fires, Brazil-only sample with institutional variables, all groups



A.2.2.2 Without critical groups

Figure A.2.2.3 Event Study for deforestation, Brazil-only sample with institutional variables, without critical groups

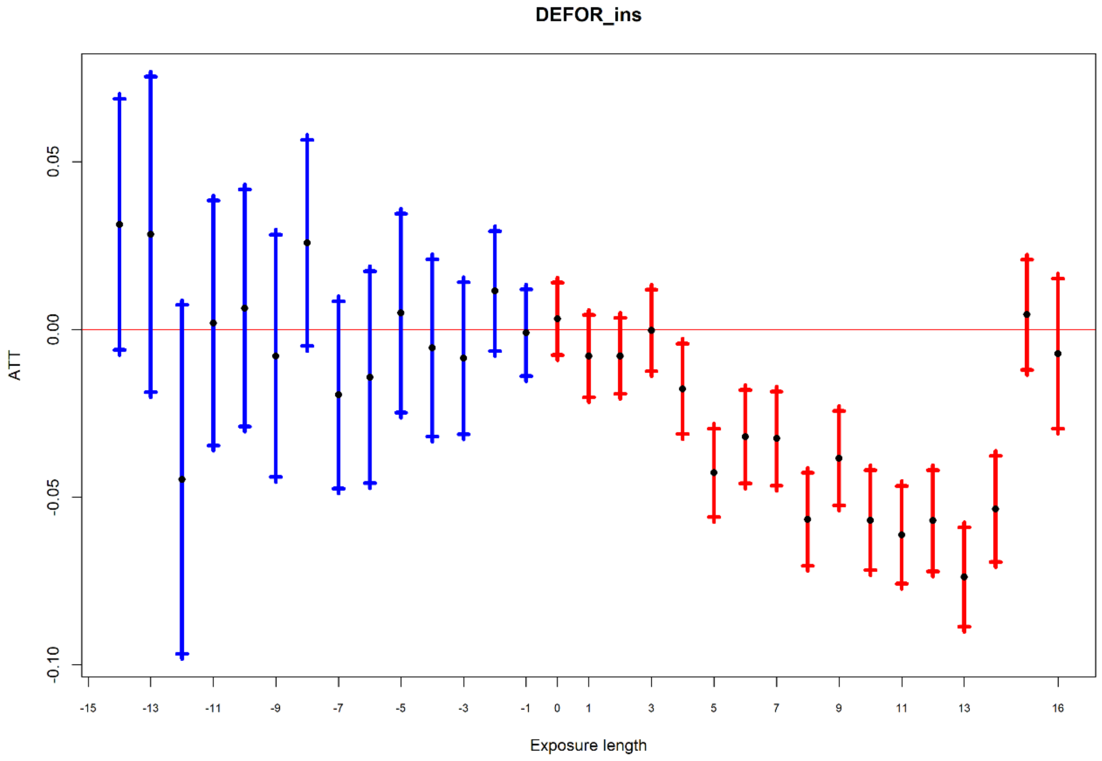
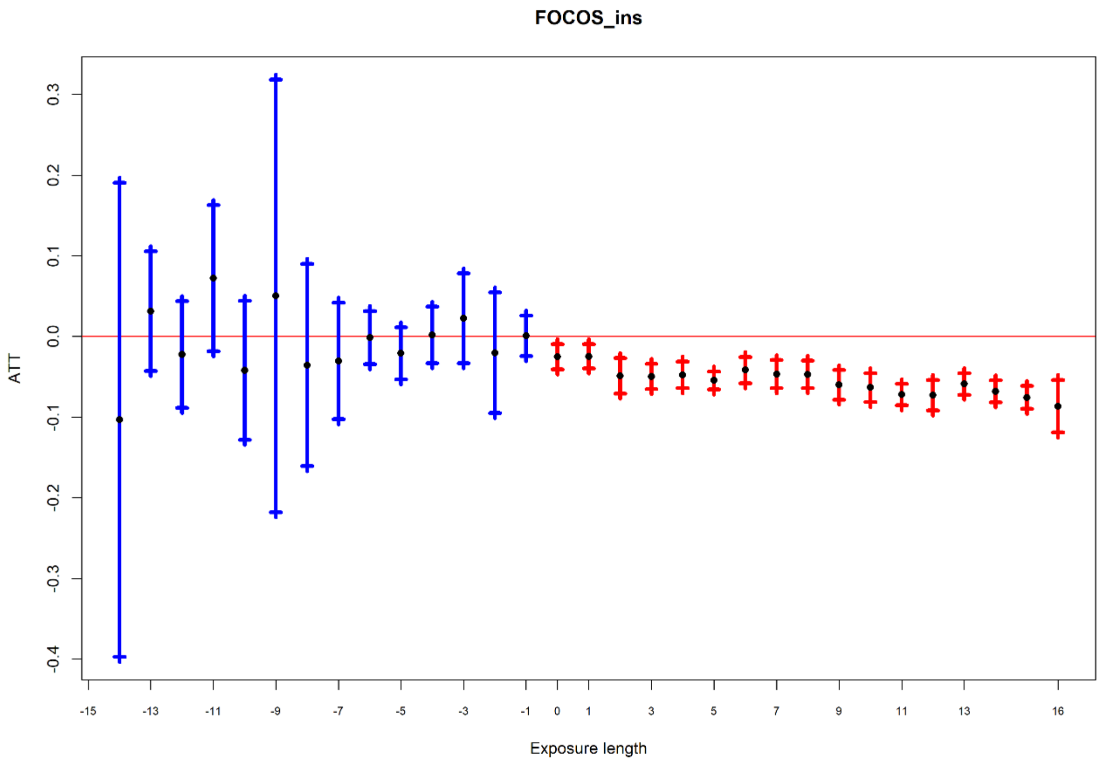


Figure A.2.2.4 Event Study for fires, Brazil-only sample with institutional variables, without critical groups



A.2.3 Brazil-only sample (without institutional covariates)

A.2.3.1 All groups

Figure A.2.3.1 Event Study for deforestation, Brazil-only sample without institutional variables, all groups

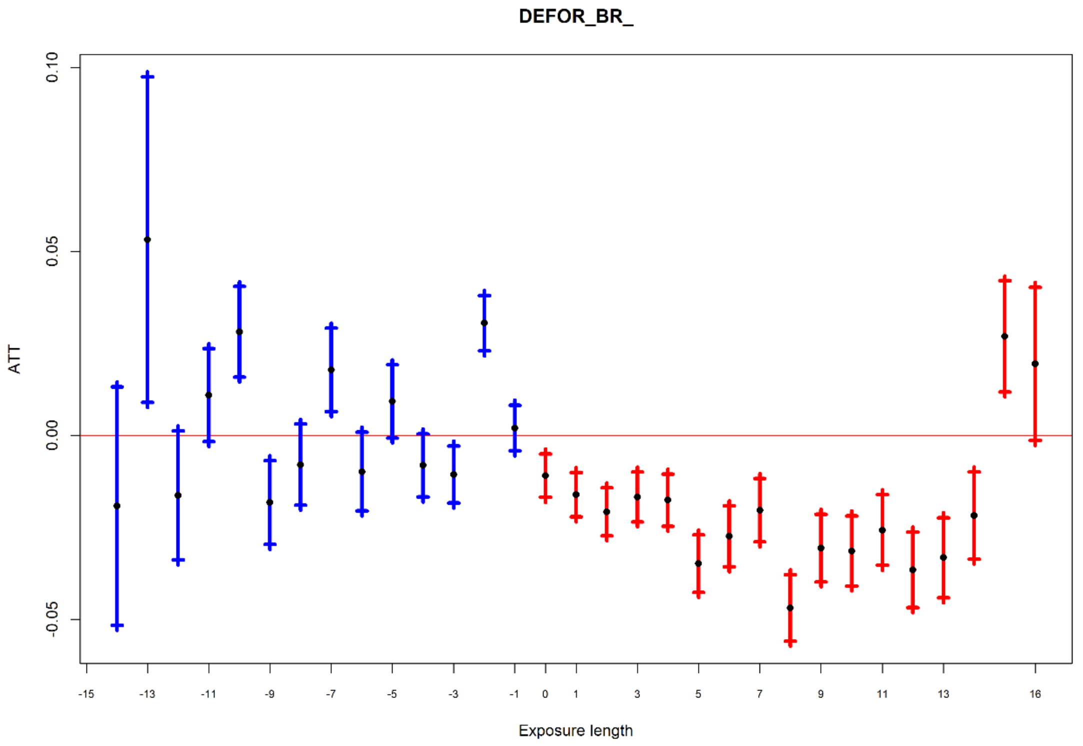
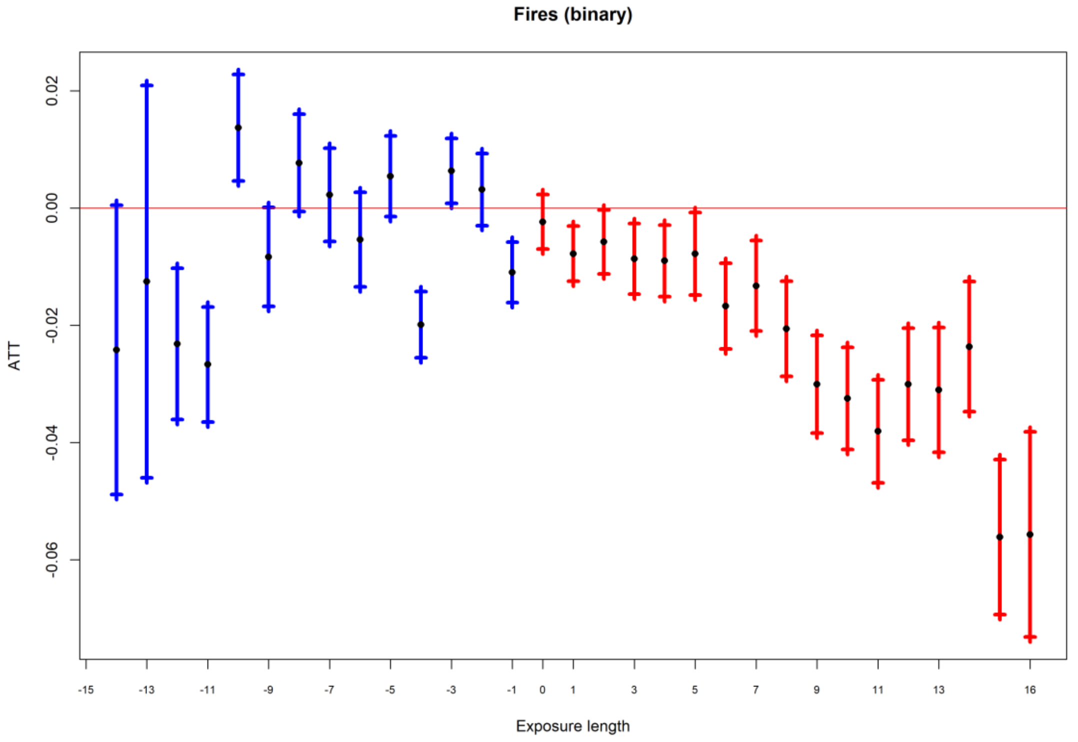


Figure A.2.3.2 Event Study for fires, Brazil-only sample without institutional variables, all groups



A.2.3.2 Without critical groups

Figure A.2.3.3 Event Study for deforestation, Brazil-only sample without institutional variables, without critical groups

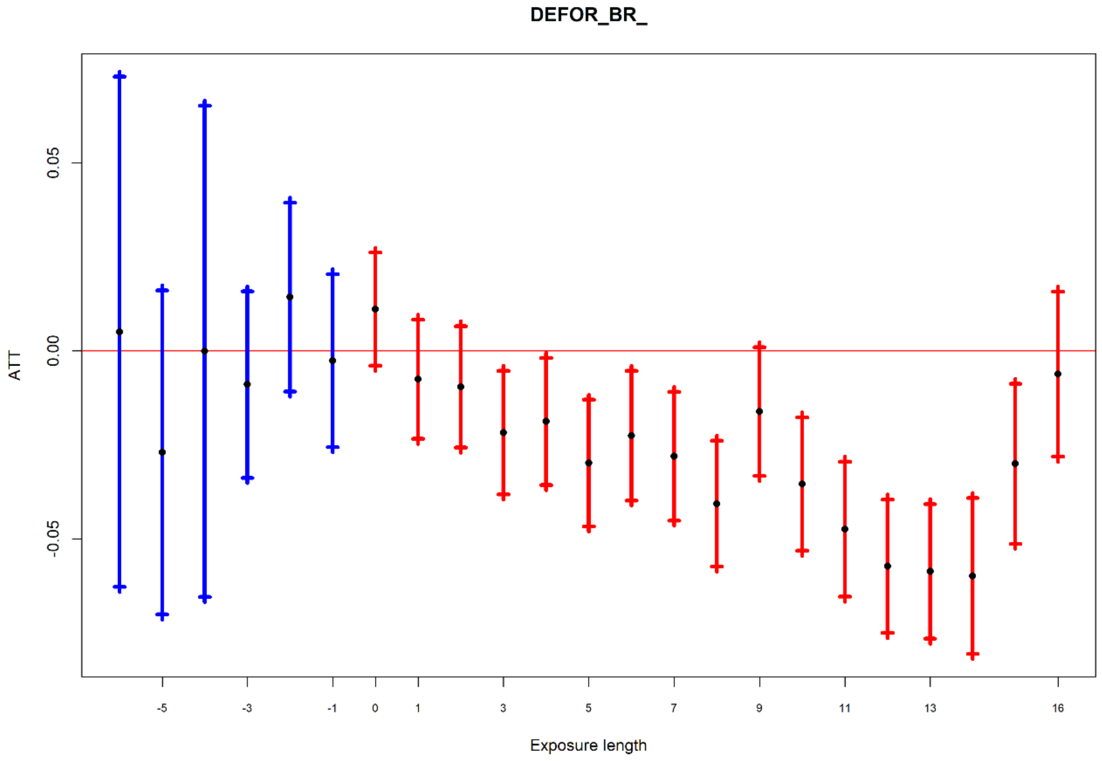
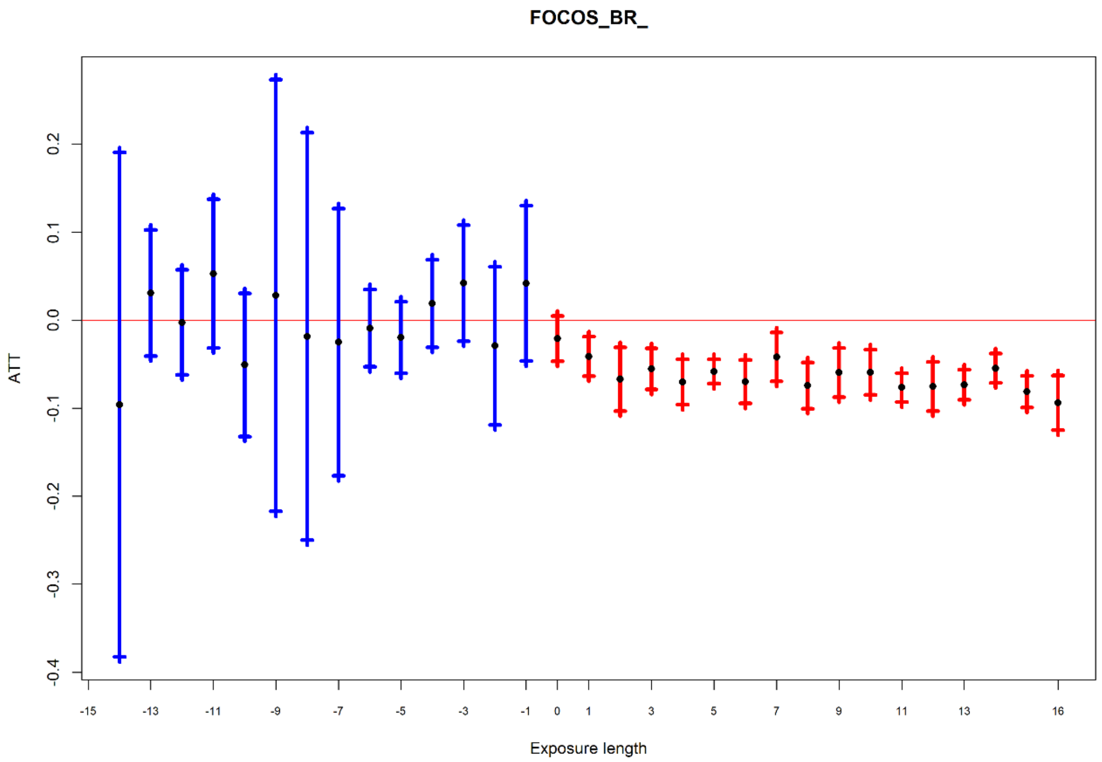


Figure A.2.3.4 Event Study for fires, Brazil-only sample without institutional variables, without critical groups



A.2.4 Subnational conservation units

A.2.4.1 All groups

Figure A.2.4.1 Event Study for deforestation, Subnational conservation units, all groups

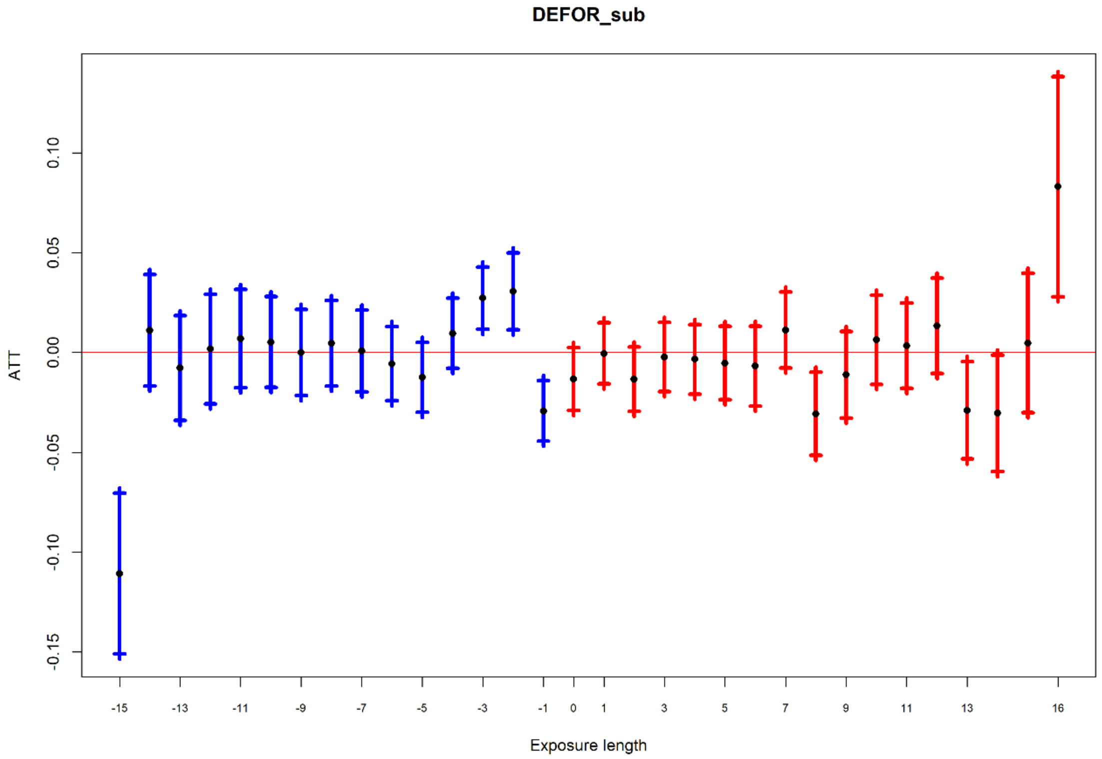
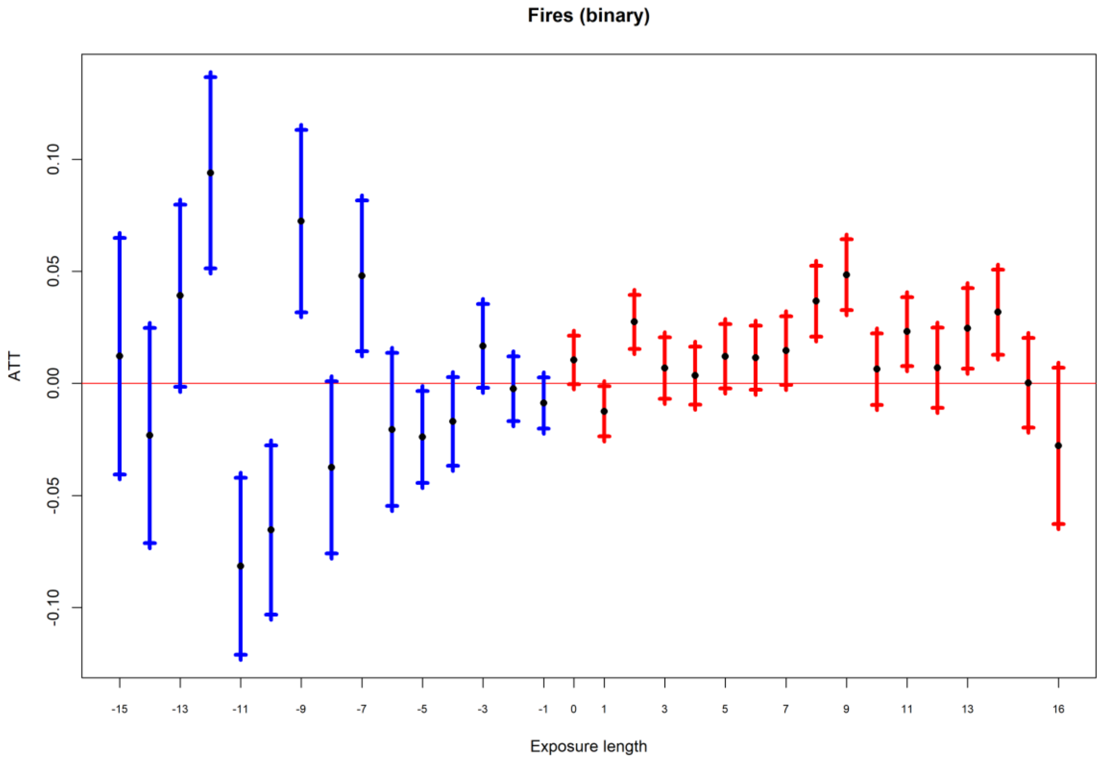


Figure A.2.4.2 Event Study for fires, Subnational conservation units, all groups



A.2.4.2 Without critical groups

Figure A.2.4.3 Event Study for deforestation, Subnational conservation units, without critical groups

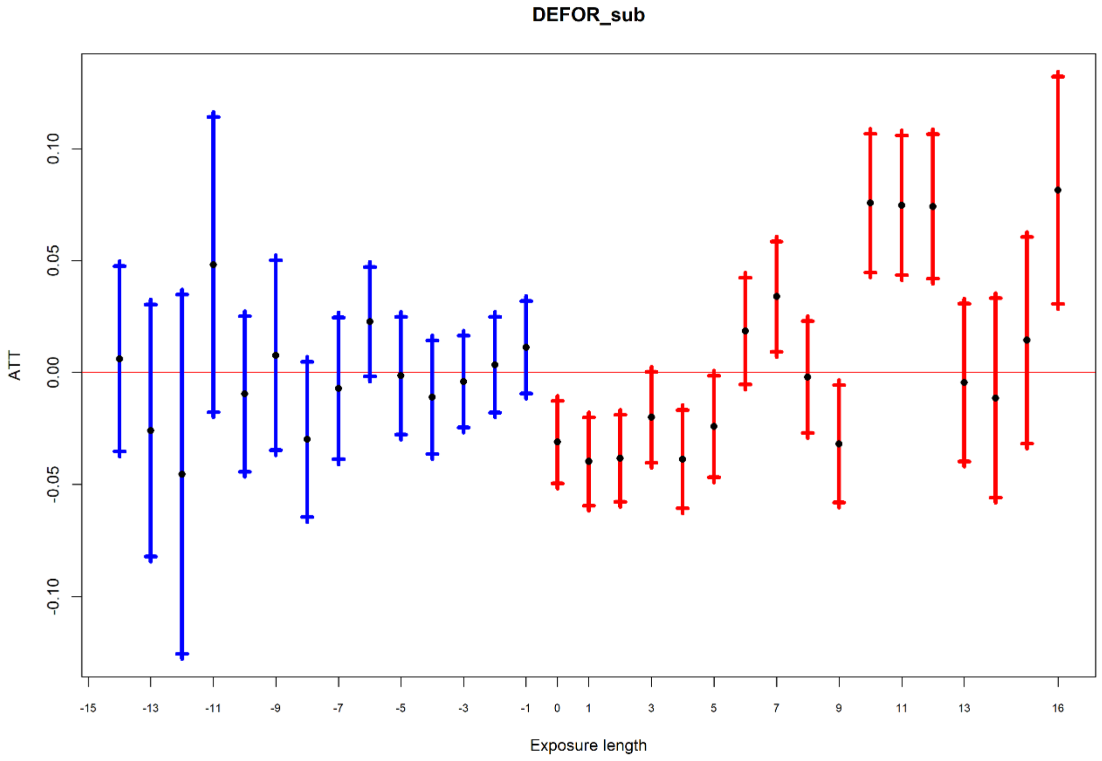
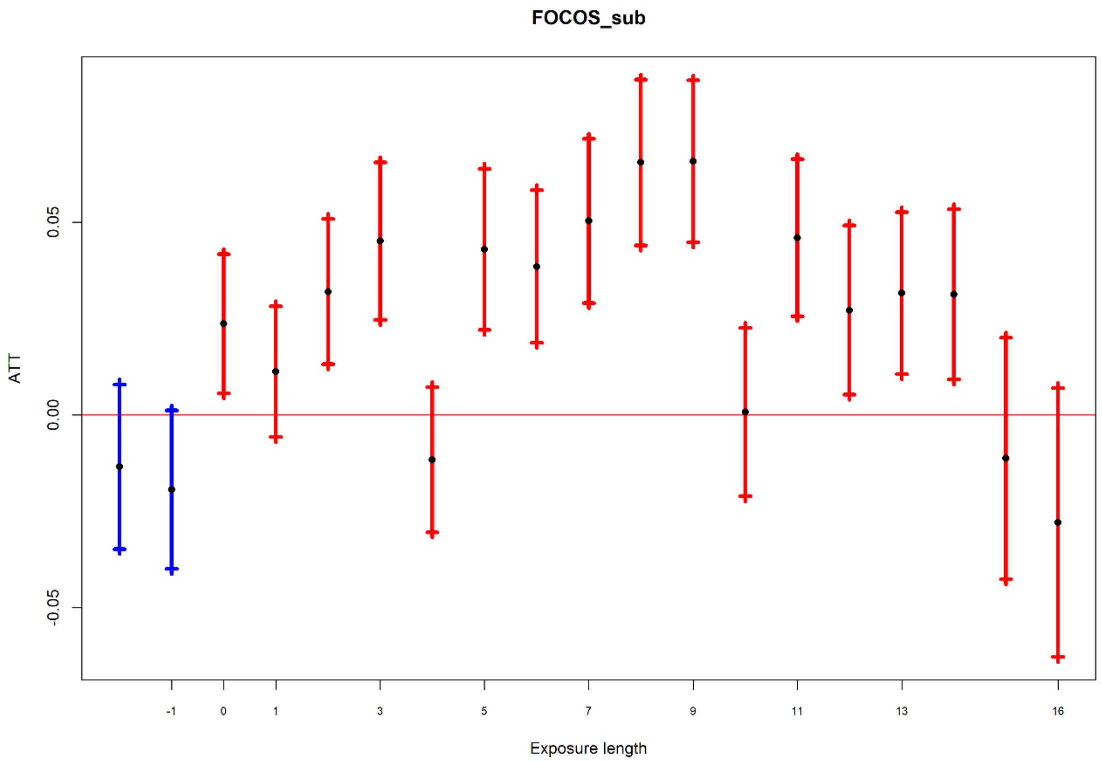


Figure A.2.4.4 Event Study for fires, Subnational conservation units, without critical groups



A.2.5 National conservation units

A.2.5.1 All groups

Figure A.2.5.1 Event Study for deforestation, National conservation units, all groups

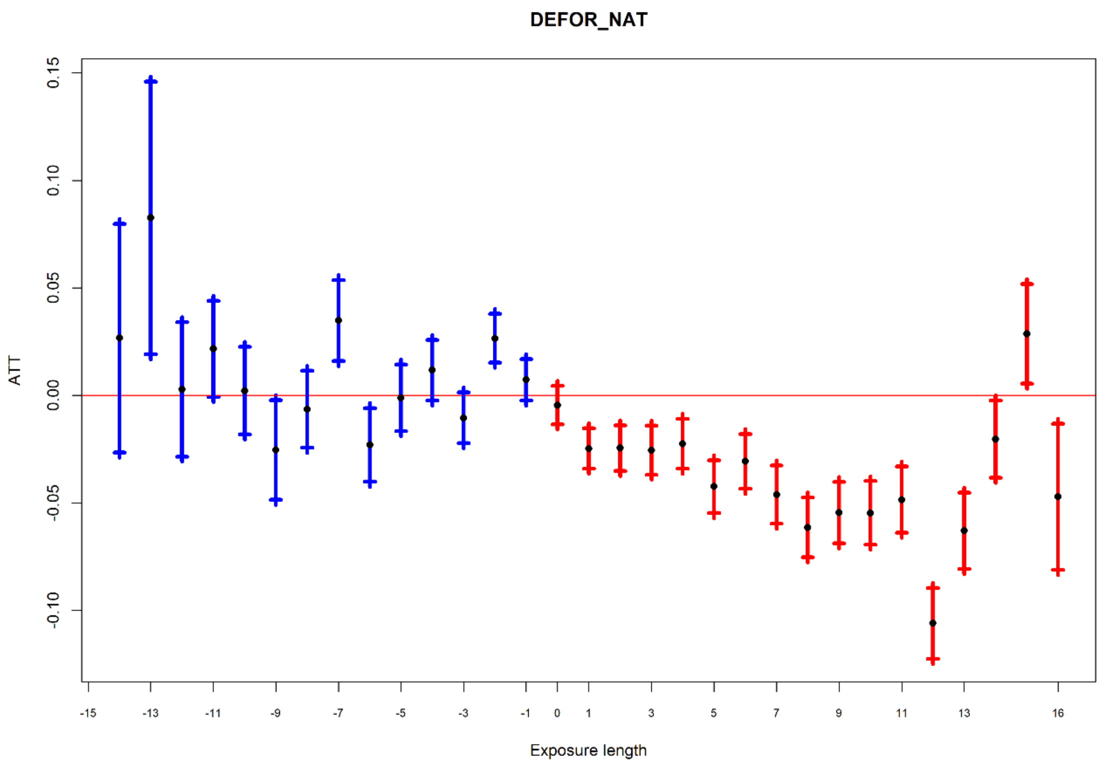
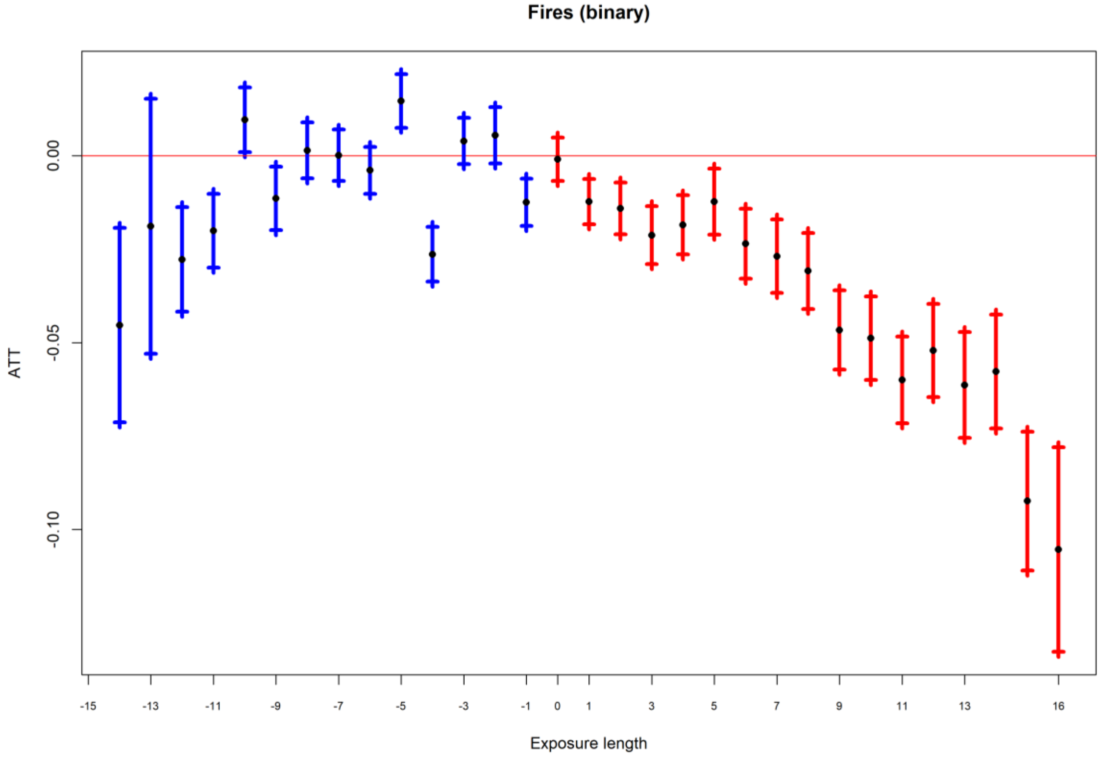
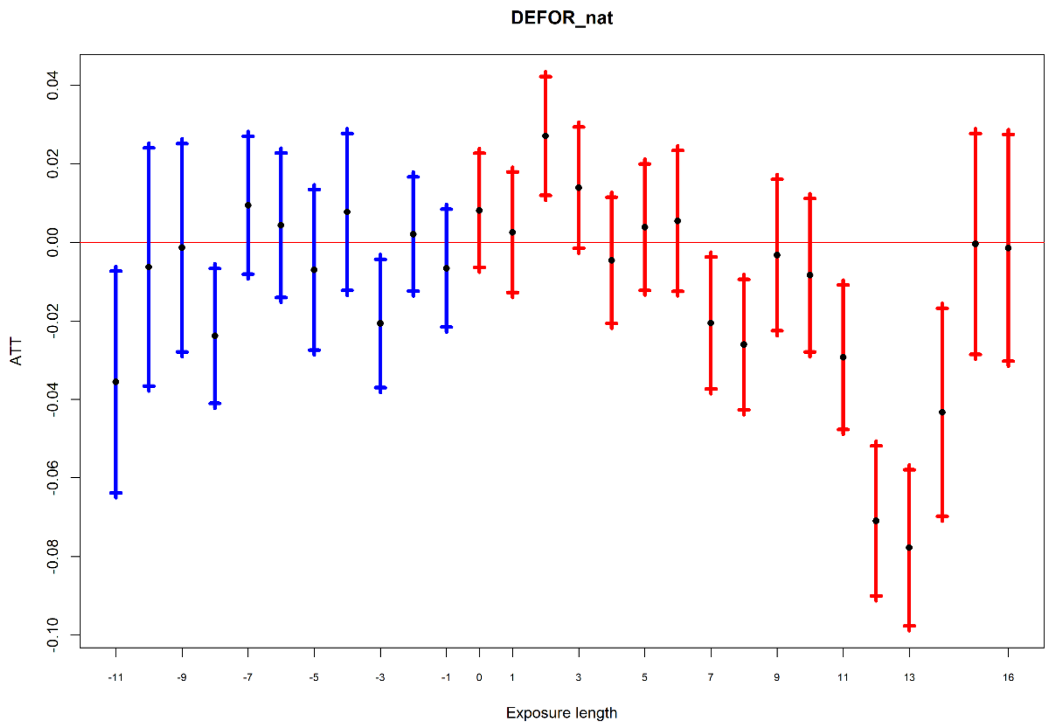


Figure A.2.5.2 Event Study for fires, National conservation units, all groups



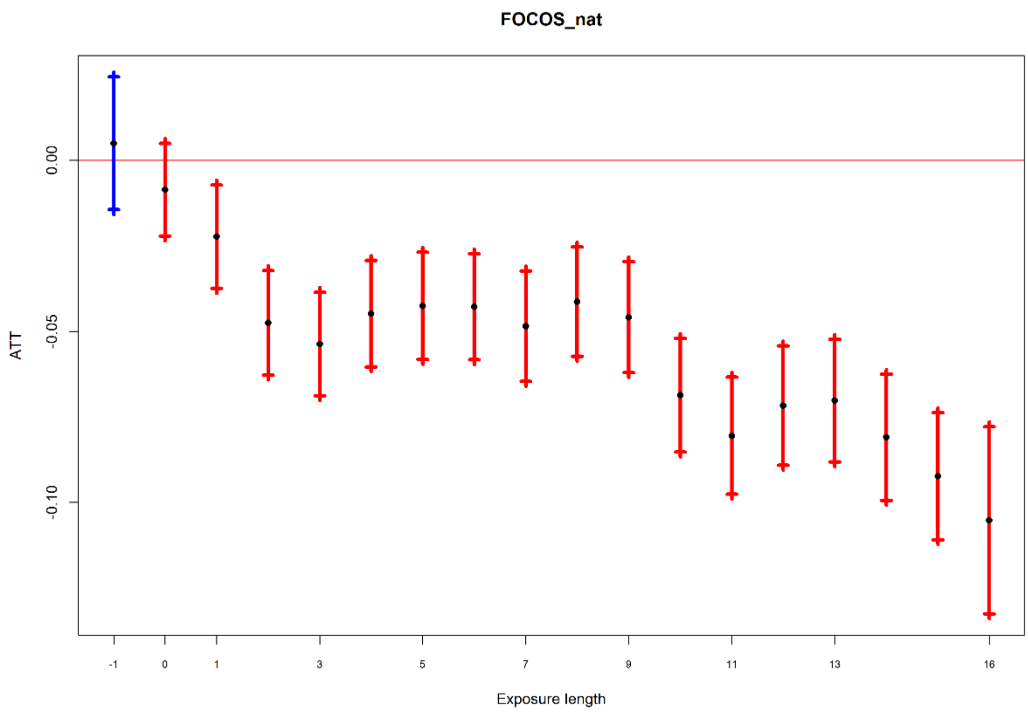
A.2.5.2 Without critical groups

Figure A.2.5.3 Event Study for deforestation, National conservation units, without critical groups



OBS: not all critical groups were excluded because only one group would have remained, which was considered to lead to a non-reliable (too specific) overall ATT. That is why significant pre-treatment effects remained.

Figure A.2.5.4 Event Study for fires, National conservation units, without critical groups



A.2.6 Indigenous lands

A.2.6.1 All groups

Figure A.2.6.1 Event Study for deforestation, Indigenous lands, all groups

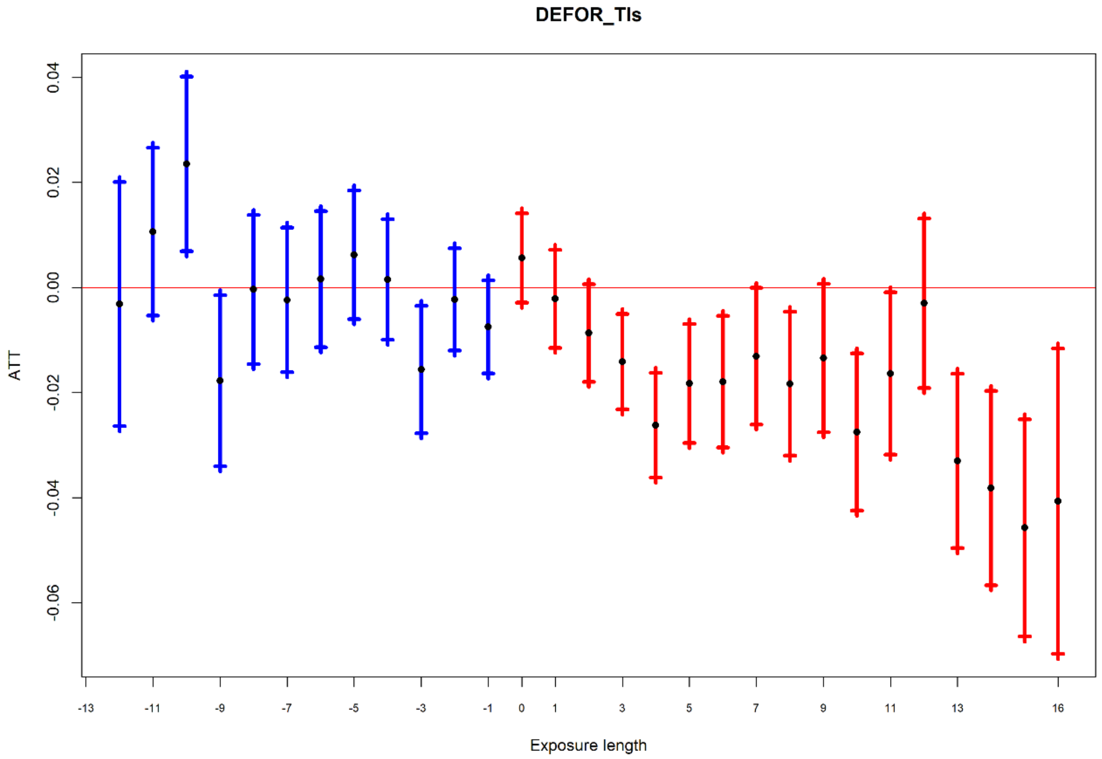
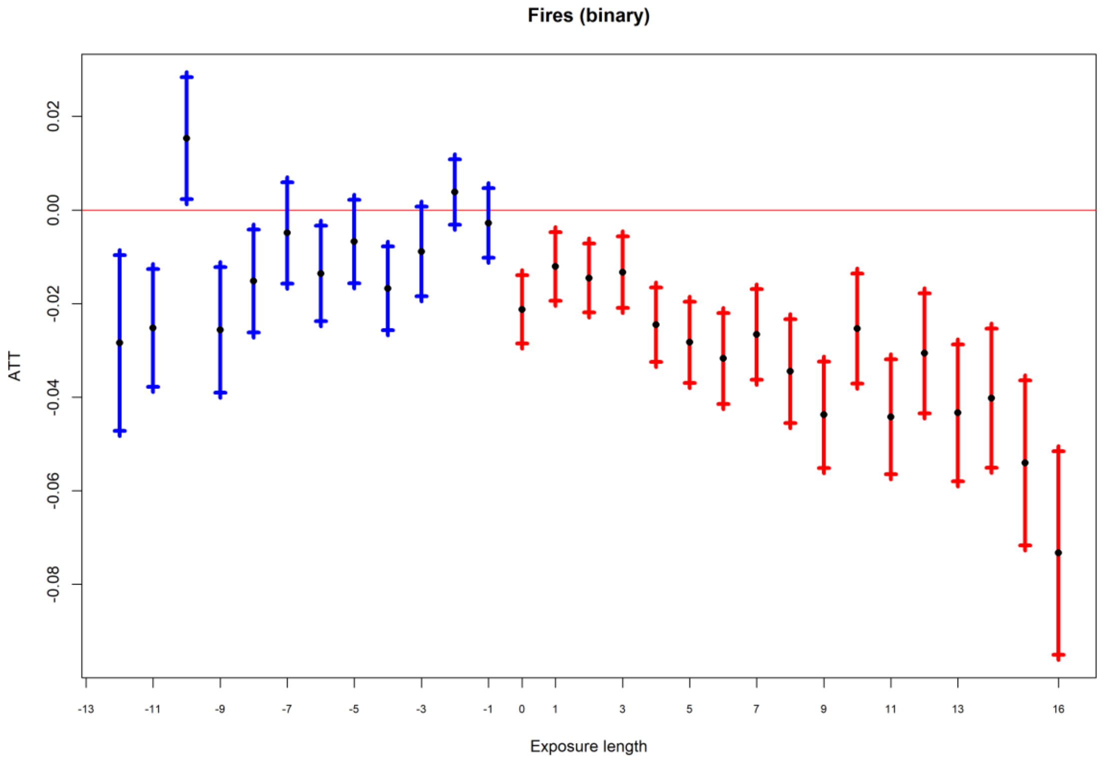


Figure A.2.6.2 Event Study for fires, Indigenous lands, all groups



A.2.6.2 Without critical groups

Figure A.2.6.3 Event Study for deforestation, Indigenous lands, without critical groups

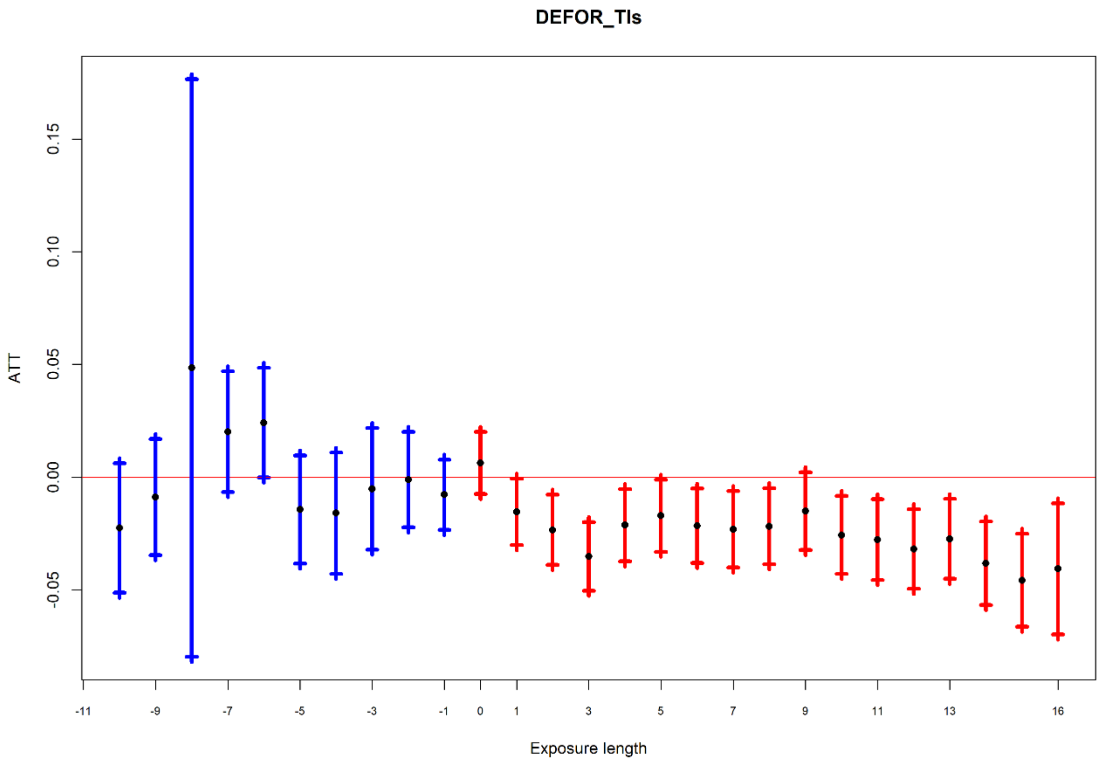
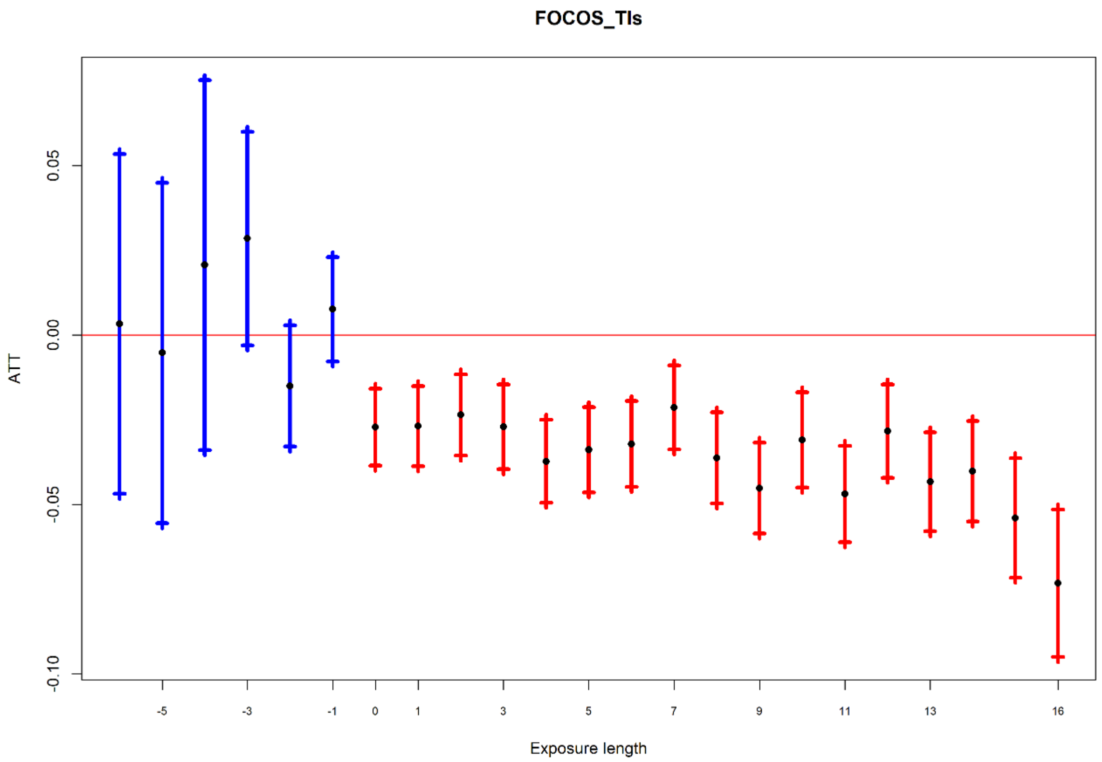


Figure A.2.6.4 Event Study for fires, Indigenous lands, without critical groups



A.2.7 Indirect use conservation units

A.2.7.1 All groups

Figure A.2.7.1 Event Study for deforestation, indirect conservation units, all groups

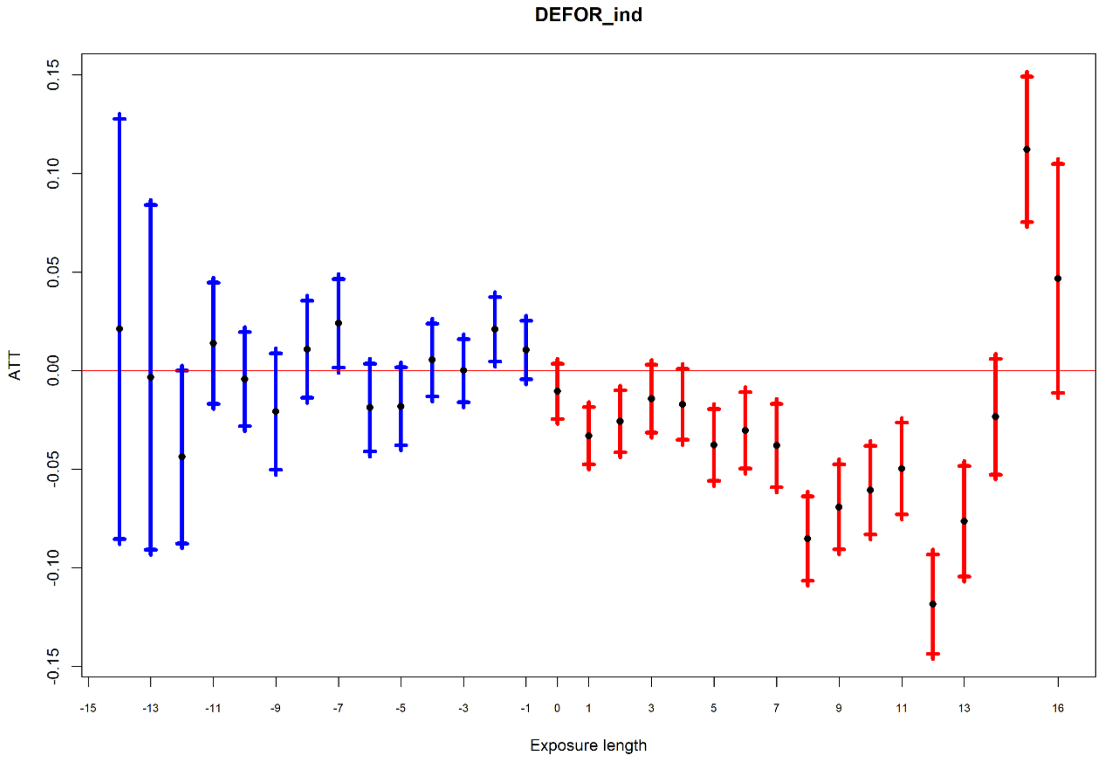
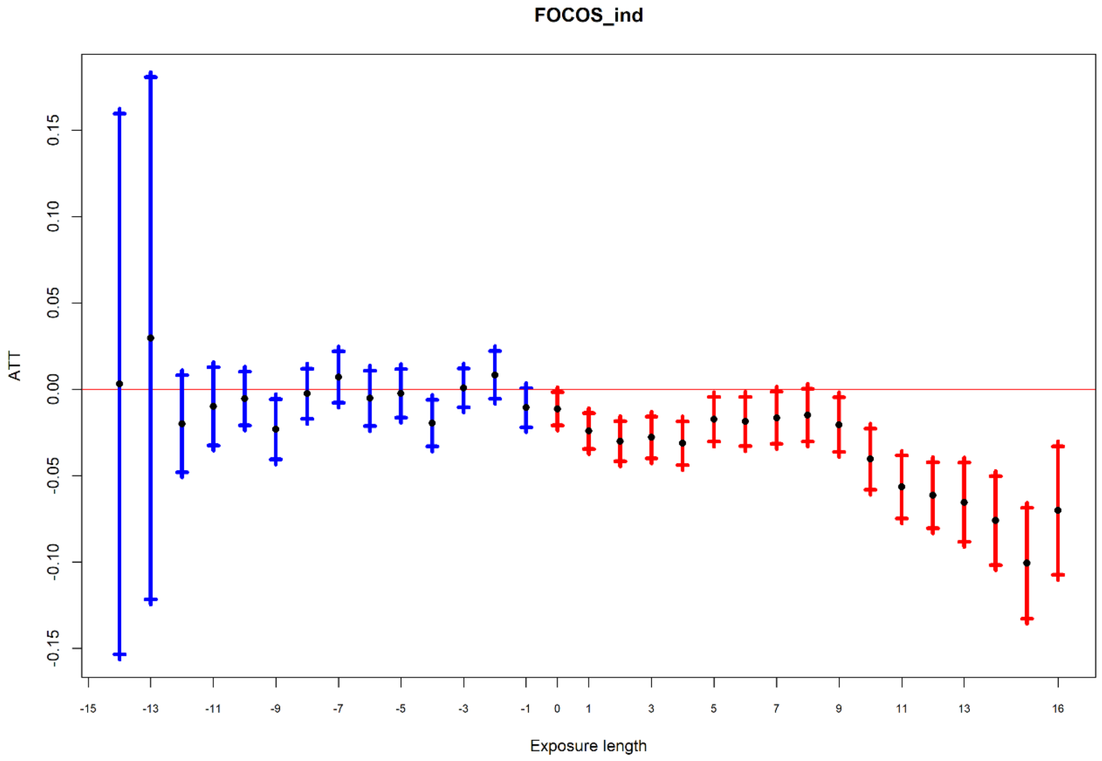


Figure A.2.7.2 Event Study for fires, indirect conservation units, all groups



A.2.7.2 Without critical groups

Figure A.2.7.3 Event Study for deforestation, indirect conservation units, without critical groups

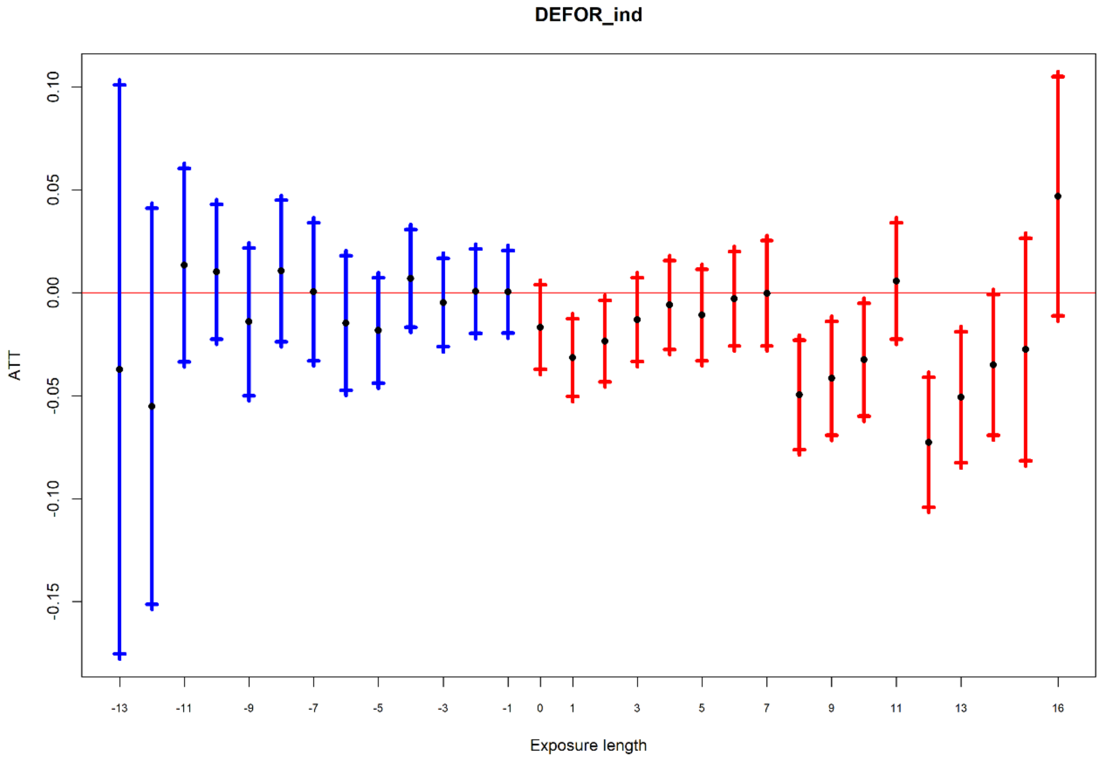
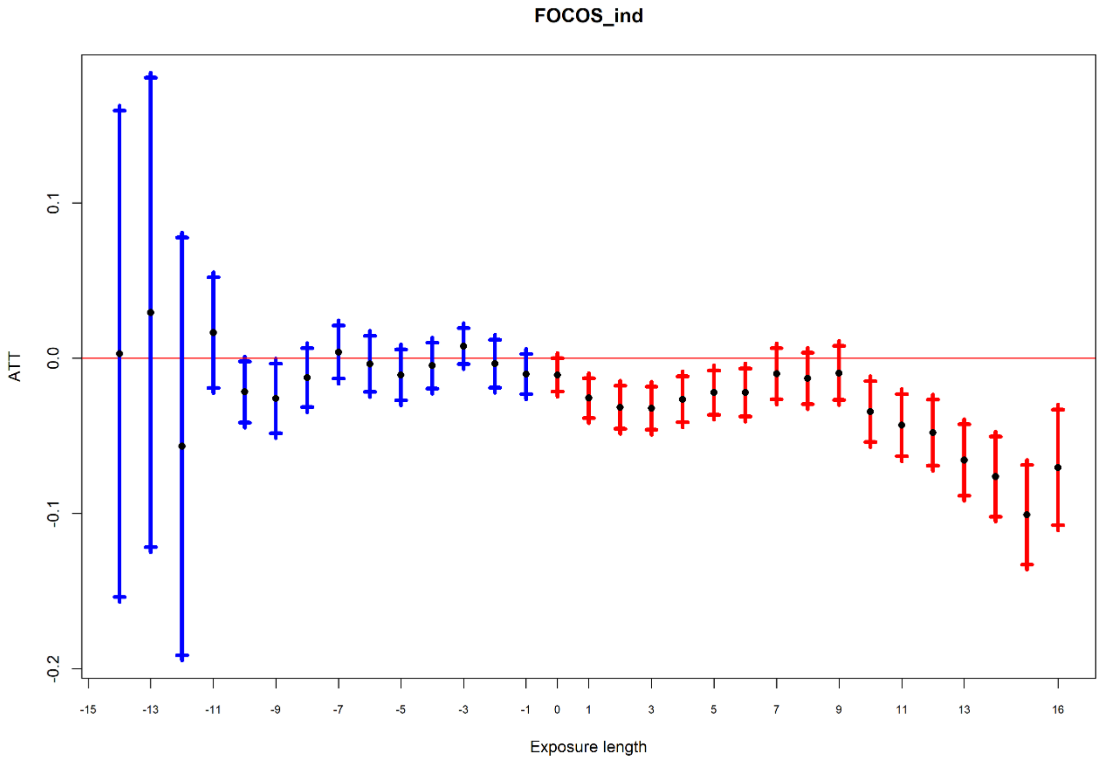


Figure A.2.7.4 Event Study for fires, indirect conservation units, without critical groups



A.2.8 Direct use conservation units

A.2.8.1 All groups

Figure A.2.8.1 Event Study for deforestation, indirect conservation units, all groups

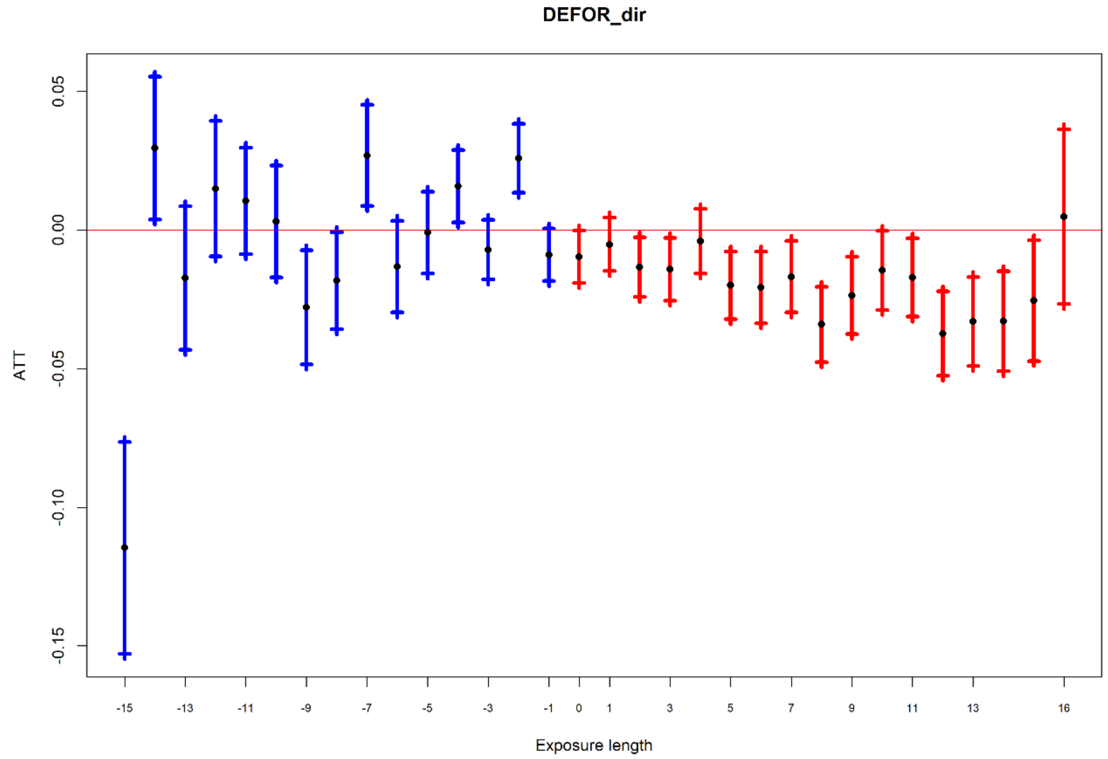
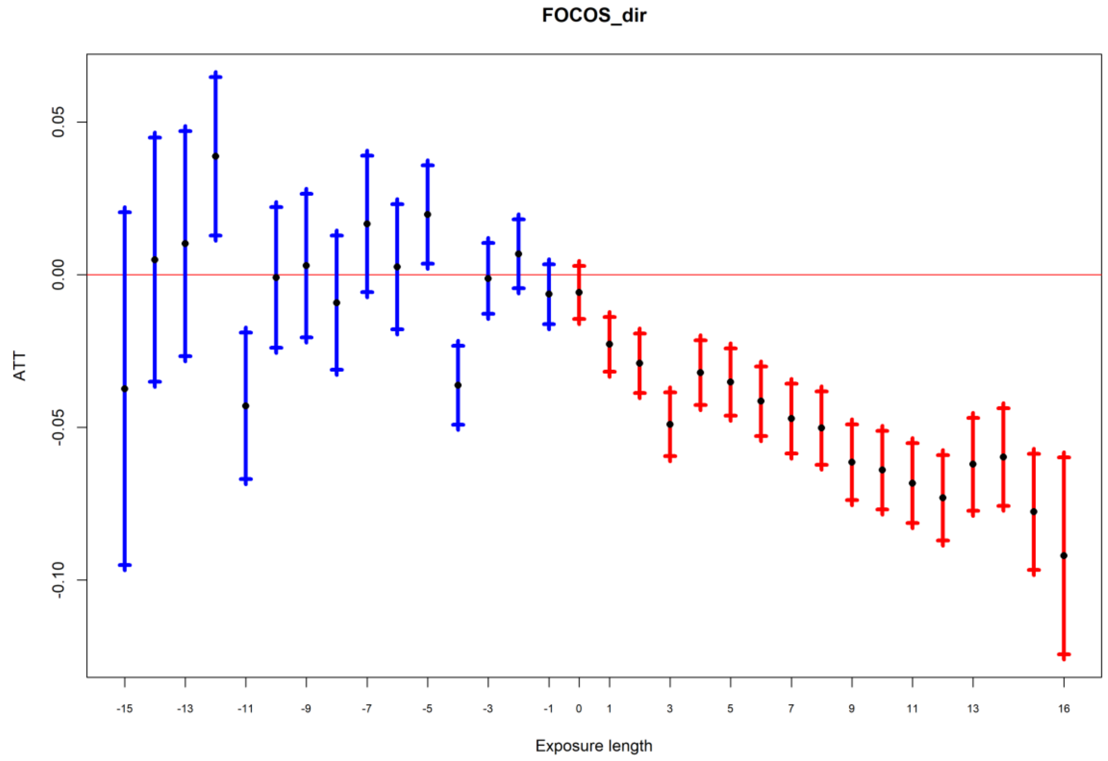


Figure A.2.8.2 Event Study for fires, indirect conservation units, all groups



A.2.8.2 Without critical groups

Figure A.2.8.3 Event Study for deforestation, direct conservation units, without critical groups

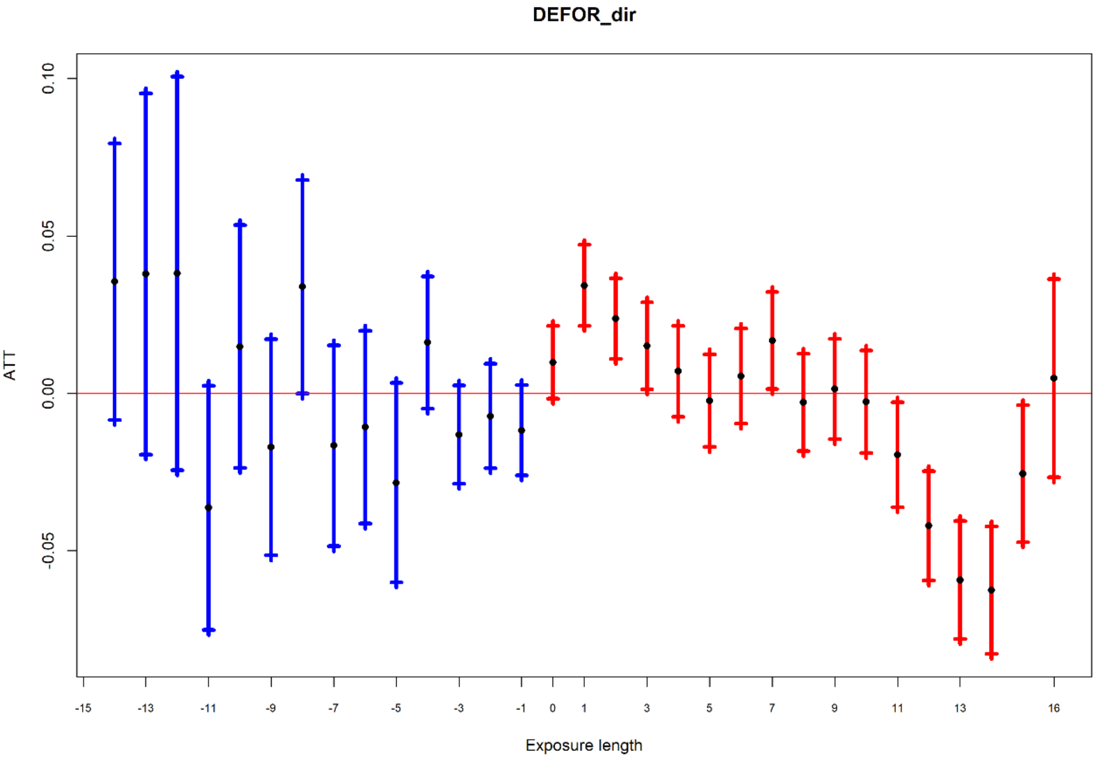
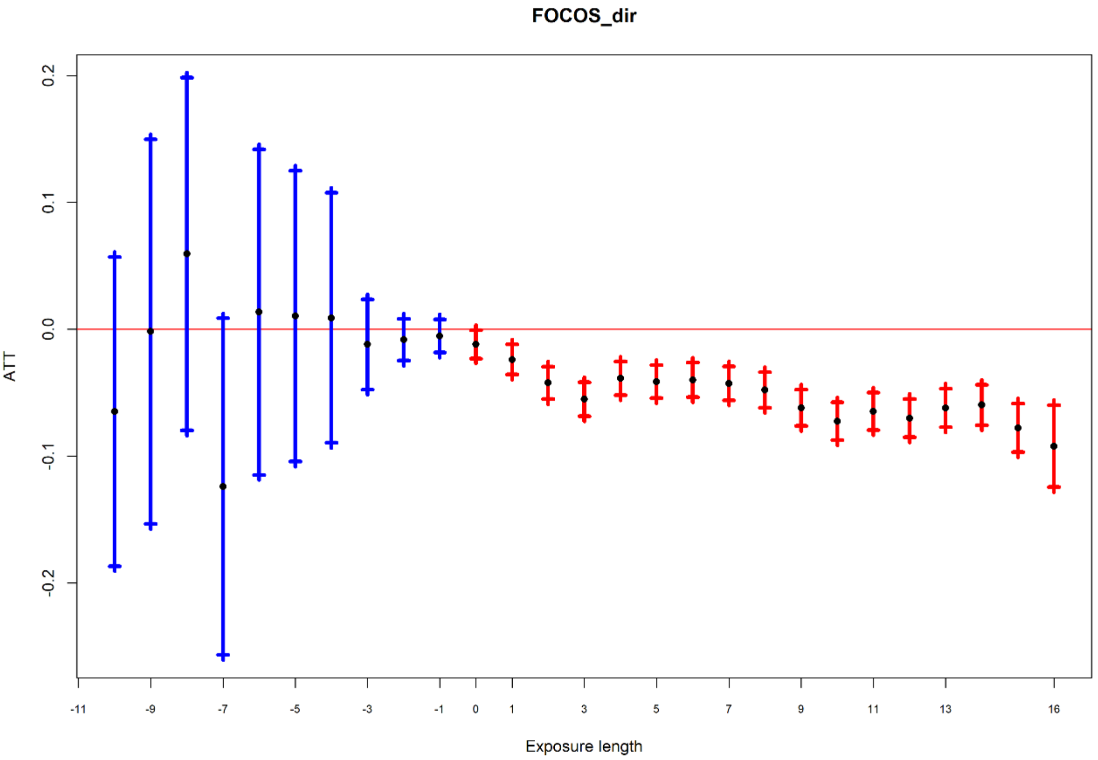


Figure A.2.8.4 Event Study for fires, direct conservation units, without critical groups



Appendix 3 The DSGE model

Table A.4 Parameters assumed in the simulations

Parameter	Name	Assumed level	Source
μ	Marginal utility of consumption	0.5	Zhang and Zhang (2020)
η	CRRA coefficient	2	Costa-Jr and Cintado (2018, table 3), Lucas (1999) and Klima et al. (2019)
β	Discount factor	0.99	Klima et al. (2019), Annicchiarico et al.(2012) and Palma and Portugal (2014).
δ	Capital depreciation rate	0.0824	Carvalho and Castro (2017)
ψ	Land depreciation rate	0.0824	Assumed by authors
α	Power of capital in production function	0.36	Costa-jr & Cintado (2018, table 3)
β_prod	Power of labor in production function	0.48	Assumed by authors
σ	Probability of deforestation detection	0.1	Assumed by authors
θ	Fine rate (applied to deforestation in PAs)	0.5	Assumed by authors
α_PA	Ratio of the shares of PA in deforestation and in forestland	1	Assumed by authors
ρ_J	AR(1) coefficient of TFP	0.95	Costa Jr. and Cintado (2018) and approximation of Miao (2014, page 474).
σ_J	Standard error of the TFP shock	0.01	Idem
ρ_γ_0	AR(1) intercept of protected share of forest	0.1	Assumed by authors
ρ_γ_1	AR(1) coefficient of protected share of forest	0.5	Assumed by authors
m0	Cost of deforestation (per hectare)	0.8	Assumed by authors

The dynamic system of the DSGE model is found below. It was simulated in Dynare®.

$$c_t = \frac{\mu}{1-\mu}(1-l_t^s)w_t (1)$$

$$w_t{}^{(1-\eta)\mu-1}(1-l_t^s)^{-\eta} = \beta \mathrm{E}_0\left[w_{t+1}{}^{(1-\eta)\mu-1}(1-l_{t+1}^s)^{-\eta}(r_{t+1}+1-\delta)\right](2)$$

$$w_t{}^{(1-\eta)\mu-1}(1-l_t^s)^{-\eta}\left(m_0+\sigma.\theta.\alpha_{PA}\gamma_{PA,t}\right) \\ = \beta \mathrm{E}_0\left[w_{t+1}{}^{(1-\eta)\mu-1}(1-l_{t+1}^s)^{-\eta}\left[s_{t+1}+(1-\psi)\left(m_0+\sigma.\theta.\alpha_{PA}\gamma_{PA,t+1}\right)\right]\right] (3)$$

$$k_t^s = (1-\delta)k_{t-1}^s + w_{t-1}l_{t-1}^s + r_{t-1}k_{t-1}^s + s_{t-1}a_{t-1}^s + \pi_{t-1} + t_{t-1} - \left(m_0+\sigma.\theta.\alpha_{PA}\gamma_{PA,t-1}\right)d_{t-1} \\ - c_{t-1}(4)$$

$$a_t^s = (1-\psi)a_{t-1}^s + d_{t-1}(5)$$

$$\alpha J_t \left(\frac{k_t^s}{l_t^s}\right)^{\alpha-1} \left(\frac{a_t^s}{l_t^s}\right)^{1-\alpha-\beta\_prod} = r_t (6)$$

$$\beta\_prod J_t \left(\frac{k_t^s}{l_t^s}\right)^{\alpha} \left(\frac{a_t^s}{l_t^s}\right)^{1-\alpha-\beta\_prod} = w_t (7)$$

$$(1-\alpha-\beta\_prod) J_t \left(\frac{k_t^s}{l_t^s}\right)^{\alpha} \left(\frac{a_t^s}{l_t^s}\right)^{-\alpha-\beta\_prod} = s_t \ (8)$$

$$\sigma.\theta.\alpha_{PA}\gamma_{PA} = t_t (9)$$

$$\gamma_{PA,t} = \rho_{\gamma,0} + \rho_{\gamma,1}\gamma_{PA,t-1} (11)$$

$$\log(J_t) = \rho_J \log(J_{t-1}) + \varepsilon_{J,t}; \ \ \varepsilon_{J,t} \sim iid\left(0, {\sigma_J}^2\right) \ (12)$$