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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, to increase bird diversity, and to 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. Firstly, 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 our region of study, the Amazon Basin (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, 2021, 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 aggregate 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, and also in terms of a highly damaging form of natural resource exploitation, illegal artisanal mining.

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, fires and mining, (iii) non-parallel trends, whose biases ranged from a 39% deflation to a 11% inflation and (iv) concurrent policy changes, which deflated the effect on deforestation in 13% and inflated the effect on fires and mining in 16% and 25%, respectively. 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 utilisation 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 and mining, which was also true for conservation units, except for 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, but not mining. A final dynamic pattern worth mentioning is the gradual intensification of the inhibition of deforestation, fire and mining, 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 a homogeneous aggregation of heterogeneous treatment cohorts (Goodman-Bacon, 2021, 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 model of deforesters' behaviour we developed 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

[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.

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.

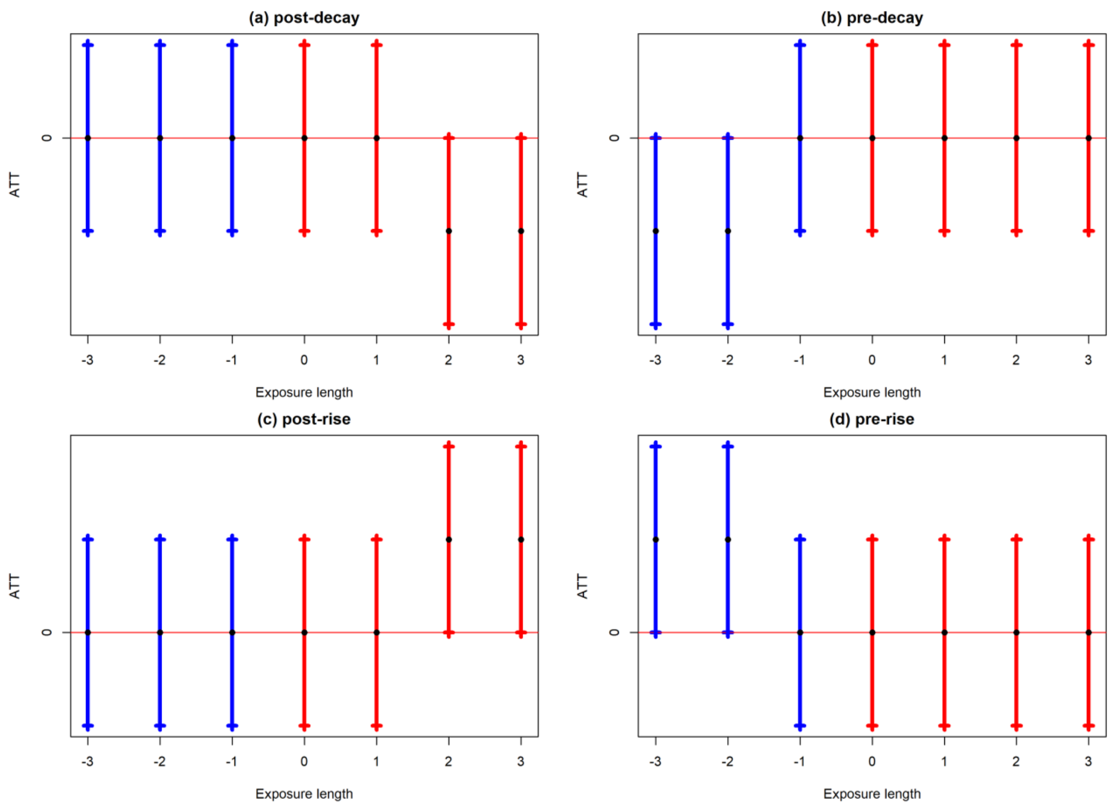
2 Literature and theory

In this section we stablish the empirical and theoretical foundations of the dynamics of PAs' effects. We start with a taxonomy of dynamics and demonstrate its theoretical consistency with a forward-looking behaviour model. Then evidence on effects' dynamics collected by previous studies is presented.

2.1 Theory

The available knowledge about the temporal patterns of protections' effect may be summarized into four types of dynamics, combining two 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 four types of dynamics are consistent with basic economics. To demonstrate that, we now present and simulate a theoretical model whose main microfoundation is forward-looking expectations formed by the representative resource-extracting household. For simplicity, we focus on one type of extraction - or, more precisely, suppression of - forest resources, deforestation, since the other forms considered in the paper, fires and mining, are associated with deforestation[6]. The model is essentially one of intertemporal consumption decision in which households' savings can be only accumulated in the form of land. Following the classical Ricardian analysis, land is available in different qualities, or "grades", which differ in the gross per-hectare return yielded.

Owned land can be only expanded via deforestation and for this a right to deforest must be purchased by the current market price. This is the first component of deforestation's cost, which is referred to as "endogenous price". Its main function is introducing (perfect) competition for land in the model, thus leading to the equalisation of net return across different land grades (another crucial foundation of Ricardos' analysis; Blaug, 1997). The second component, referred to as "exogenous price", is policy-based, corresponding to the expected sanction the household is continuously exposed to, due to legal and illegal deforestation rights exchanged in the market. More precisely, rights are issued either officially by government, or illegally, by pioneer land grabbers and both are purchased by the household.

Creation of PAs is understood strictly as an increase in the exogenous price of low-quality land, since, in practice, it consists in a (permanent and local) rise of expected sanction on illegal resource appropriation, which generally takes place where agriculture is less profitable. The assumptions here presented are formalised in what follows.

2.1.1 Assumptions

The representative household (HH) maximises the instantaneous CRRA utility function below, with c_t denoting contemporaneous consumption and η the relative risk aversion coefficient (η > 0).

$$u(C_t) = \frac{C_t^{(1-\eta)}}{1-\eta}$$

The budget constraint has, on the income side, the gross earnings from investment on land, $\pi(A_{i,t})$. Expenditures comprise consumption and deforestation cost. The latter unfolds into the endogenous market-based price, $p_{i,t}$, and into the exogenous policy-based price, m_{it}. That is:

$$\sum_{i=1}^{N} (\mathrm{p}_{i,t} + \mathrm{m}_{i,t}).\mathrm{D}_{i,t} + \mathrm{C}_t = \sum_{i=1}^{N} \pi_i(\mathrm{A}_{i,t})$$

The gross return function is quadratic with a single interior maximum, "Amax":

$$\pi_i(\mathrm{A}_{i,t}) = \delta_i \left(Amax.\mathrm{A}_{i,t} - \frac{{\mathrm{A}_{i,t}}^2}{2} \right), i = 1, \dots, N$$

The larger gross return yielded by land of higher quality is captured with a greater δ_i. Deforested land is accumulated, growing with deforestation and, for simplicity, is not subject to depreciation:

$$\mathrm{A}_{i,t} = \mathrm{A}_{i,t-1} + \mathrm{D}_{i,t-1}, i = 1, \dots, N$$

[6] What is evidenced, for the case of fires, by Aragão and Shimabukuro (2010), with a 81% rate of increased deforestation pixel also exhibiting increased fire frequency. For the case mining, see Asner and Tupayachi (2017).

Compiling all expressions and equations, the HH problem is:

$$\max_{\{C_t,\{D_{i,t},A_{i,t}\},i=1,\ldots,N\}} E_0 \left\{ \sum_{t=0}^{\infty} \beta^t \left[\frac{\mathrm{C}_t^{\;1-\eta}}{1-\eta} + \sum_{i=1}^{N} \lambda_{i,t} \left(\mathrm{A}_{i,t-1} + \mathrm{D}_{i,t-1} - \mathrm{A}_{i,t} \right) \right. \right.$$

$$\left. \left. + \lambda_{BC,t} \left[\sum_{i=1}^{N} \pi_i \left(\mathrm{A}_{i,t} \right) - \sum_{i=1}^{N} \left(\mathrm{p}_{i,t} + \mathrm{m}_{i,t} \right) . \mathrm{D}_{i,t} - \mathrm{C}_t \right] \right] \right\}$$

The representative issuer of deforestation rights must incur a cost of taking control of land, which involves building of (unpaved or paved) roads and minimal infrastructure. It maximises profit in a perfectly competitive market for rights:

$$Max_{\{\mathrm{D}_{i,t}^{S}\}} \left\{ \mathrm{p}_{i,t} \mathrm{D}_{i,t}^{S} - C\left(\mathrm{D}_{i,t}^{S} \right) \right\}$$

Total cost is assumed as cubic, as standard in microeconomics and, consequently, marginal cost is quadratic. The rights' market clearing condition, which determines the endogenous price, is:

$$\mathrm{D}_{i,t}^{S}\left(\mathrm{p}_{i,t} \right) = \mathrm{D}_{i,t}$$

2.1.2 Simulations

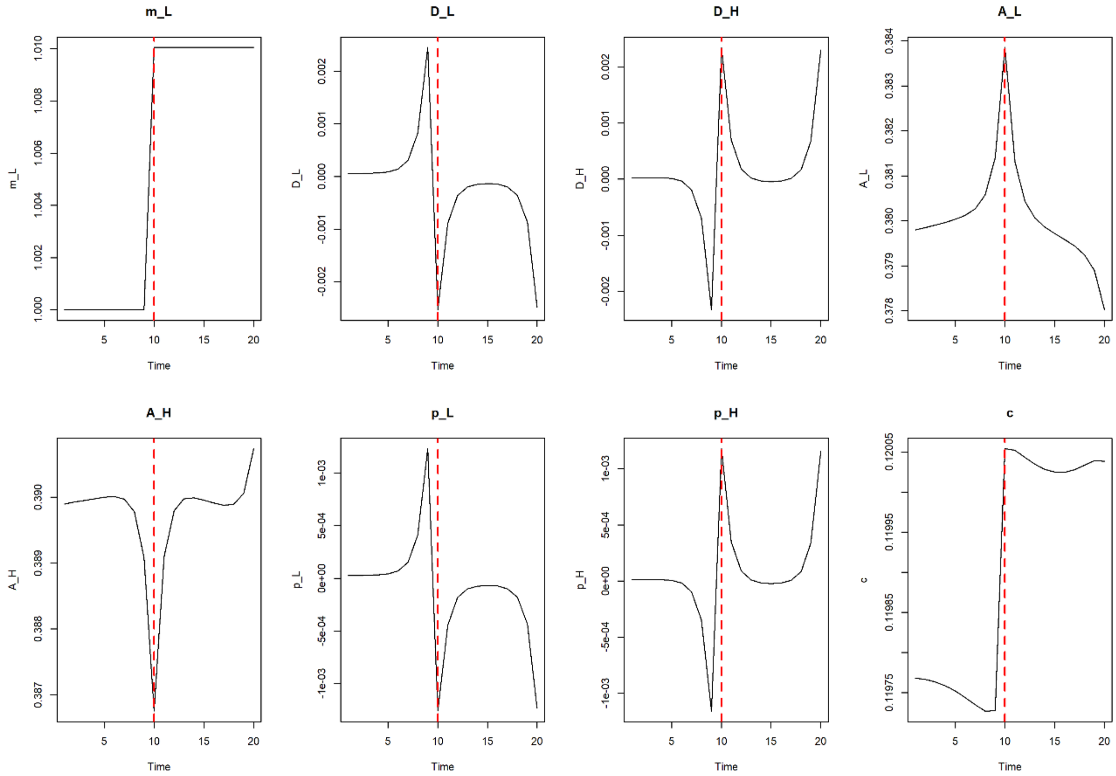
The steady state of the model was calibrated to a set of parameters meant to be as general as possible – data sources are found in appendix 4, which also contains the equations of the dynamic system. For simplicity, only two land grades were assumed, low quality or i = L, and high quality or i = H. The model's internal consistency was evaluated by conceiving the exogenous price components as stochastic shocks unexpected to the household. A near-negligible correlation between the shocks m_R and m_{NR}, of 0.1%, was assumed. Besides the confirmation of consistency, relevant responses to the shocks were observed, namely:

- Deforestation of a specific land grade responded negatively to the exogenous component of its own price and positively to the exogenous component of the other grade's price (different land grades were substitutes);
- The endogenous component of deforestation price worked as a self-correction mechanism decreasing after a positive shock to the exogenous component, thus re-stablishing the long-run equilibrium;
- Consumption increased with a positive shock to the exogenous price component, which is in accordance with the "return-on-savings" mechanism behind intertemporal consumption choice (i.e., with an unexpected fall in the return of assets, it becomes less attractive to save).

Now, to simulate PA creation, it was introduced a shock to low-quality land that was both fully expected and durable, lasting from half of the period on, i.e., on t = 10 since a time horizon of twenty instants was assumed (Figure 2). The exogenous price of high-quality land was kept unchanged. The forest rush effect was doubtless. It was followed by a three-stage trajectory, which started with a smooth increase, proceeding to stagnation and then ending with smooth decrease. At the end, deforestation inside PAs was smaller, uncovering a post-decay effect. Importantly, high-quality-land-deforestation followed the exactly opposite trajectory, what is another indication that crowding-out of deforestation is a potential side-effect of PA creation. Consumption fell gradually before the shock, attesting that consumption smoothing was at play, rising sharply afterwards, again because of the decreased return-on-savings. Interestingly, a slightly larger consumption level was achieved. The reason for this is that, without capital accumulation, only land accumulation, savings are fully converted in land. The forest rush, by prematurely increasing deforestation, expanded land, what increased future income, enabling consumption to increase. The endogenous price of low-quality land followed own deforestation, which is expected as it was demand for deforestation that responded to the shock (and not supply of deforestation shocks).

The two dynamic effects lacking, pre-fall and post-rise, were also generated by the model, but with an expected shock on exogenous price of high-quality land. The reasons were analogously the same as in the shock to low-quality land price. The former was due to the rush to deforest outside PAs, which meant allocating HH budget with priority to such locations, with not much resources left for deforesting inside. Now post-rise occurred as substitution of high-quality for low-quality land deforestation - the two can be also observed in Figure 2, by mentally switching all variables indexes from “L” to “H” and vice-versa.

Figure 2 Perfect foresight simulation, low-quality land exogenous price (m_L) shocked at t = 10



2.2 Evidence

2.2.1 PAs' effects dynamics

Besides theoretically sound, the four types of effect dynamics have also being observed by previous investigations about the process through which protected areas inhibit detrimental resource extraction. Starting with a negative post-protection effect means the absence of effect in the first year of protection and the presence of a negative effect in subsequent years. This dynamic type could be attributed to the gradual improvement of PA enforcement, as staff takes time to learn how to optimise patrolling in the specific set of biophysical and social conditions faced, what, 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). Resource extractors may take advantage of these initial enforcement caveats to keep their activity.

A post-protection rise in deforestation may result from relatively weaker enforcement inside rather than outside protected areas, which pushes deforestation towards PAs, as shown by the theoretical model. 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 lands are 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[7]).

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 negative pre-protection effect may be motivated by extractors 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[8]. Keles et al. (2023, fig.7) indeed found negative ex-ante effects of protection in particular Amazonian locations (such as Pará state). That would be captured, in the theoretical model, by a positive and permanent shock in m_L representing not creation itself, but the outset of the process of creation, what would anticipate the decay in deforestation in low-quality land.

Pre-protection effects may be also positive. The future protection of a land parcel could trigger its deforestation in the present, through the increased sanction likelihood mechanism explored in the theoretical model. 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, a process that was slowly implemented. A third example of an anticipated response to PA creation that (could have) raised environmental degradation is found

[7] In the case study of Holmes (2014), peasants set fires near the borders of a PA as means to contest it.

[8] 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).

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, in the Brazilian case (FUNAI, 2023).

2.2.2 Confounder policies

Since we seek, besides detecting PAs' effects dynamics, to estimate an aggregate effect across treatment exposure length, there is need to worry about another source of bias observed in the literature analysing our outcome variables. This is the implementation, in the Amazon, of other concurrent environmental policies affecting deforestation, fires and mining. Intensification of the enforcement of laws constraining these activities in non-protected government owned-lands is a key example which, in the theoretical model, is captured by m_H (Assunção et al., 2020, Morello et al., 2020, Damonte, 2018). Another example is stronger enforcement inside PAs, which, albeit also captured by m_L, is an intervention that differs from the one we focus, which is the creation of PAs (Geldman et al. 2015). Failure to control for these policies, which, for not consisting in PA creation, work as confunders, may either inflate or deflate the effect of PAs. More precisely:

1. There is deflation if confounder policies reduce forest disturbance more intensively outside rather than inside PAs (figure 3, 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 confounder 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 3, 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 3 **Deflation and inflation by confounder policies (control = black, treated = grey)**

3 Empirical method and data

3.1 Identification strategy

Our empirical goal is double, both testing for the presence of the four types of dynamics and accurately estimating the overall effect of PAs, i.e., the effect aggregated across the length of exposure to protection. The main barriers we face to proceed are two sources of bias. First, untreated pixels are not all of them comparable to the treated. Second, with cohorts of pixels defined in terms of length of exposure to protection, aggregating them in a standard way could automatically attach negative weights to some cohorts. To mitigate these biases, we adopt an identification strategy. It estimates the effect of PAs, which is represented by β in the equation below. The associated binary variable, "PA", takes value one if the i-th pixel is protected in the t-th year, and null value otherwise. Covariates are subsumed to vector X. The dependent variable, Y, is a generic environmental outcome.

$$Y_{it} = \gamma + \beta PA_{it} + X_{it}\Gamma + a_i + \lambda_t + u_{it},\ i = 1,\ldots,N,\ t = 2003,\ldots,2020$$

Three main identification challenges are faced, (i) self-selection of the i-th site to be protected, (ii) staggered creation of PAs over time, which may lead to heterogeneous effects, and, (iii) potential confounding factors from omitted concurrent changes. 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 minimise 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, from one side, and fires and mining, from another side, differed, given that only in the latter case deforestation was included, matching was separately implemented for each set of dependent variables. 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)[9].

After restricting the sample to comparable pixels, we proceeded with the DiD estimator 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. Further aggregation, across all exposure lengths, generated the overall effect estimate. But before computing it, we excluded groups violating the parallel trends assumption. These are hereafter referred to as "critical groups", and understood as those with significant group-time ATTs belonging to a pre-treatment exposure length, that, for its turn, was significant. These exclusions were step-wisely implemented, whenever a previous round of group removal was not enough to drive all pre-treatment effects null[10]. The event study

[9] 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.

[10] 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). Mining was an exception as in the subsample with indigenous lands and institutional covariates, four rounds were required. Still for such outcome variable, in the high quality of management subsample, three rounds were needed and, in all other subsamples, at most two rounds.

estimates, more precisely, the significance of pre-treatment effects, re-generated at each round, guided the operation.

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 (2021) 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 our 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[11], which is a major share of the variation identifying ATT. Even with Goodman-Bacon's (2021) 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 Outcome variables

Three are the outcomes based on which effectiveness of protection is assessed. First, suppression of primary and secondary natural vegetation, i.e., pristine and regeneration, respectively, the most common dependent variable in empirical PA studies. We also look to fires as an indicator of forest degradation, which, despite apparently less ecologically impactful, is being attested, by a growing body of research, as at least as damaging as deforestation (Qin et al., 2019, Barlow et al., 2016, Matricardi et al., 2020). The third outcome is a highly damaging form of resource extraction, artisanal mining of surface or near-surface mineral deposits, which consists mainly in goldmining (Teixeira et al., 2021, Moreno-Louzada and Menezes-Filho, 2023). Indeed, at least in Brazil, a substantial part of gold deposits are located inside or near PAs (Rizzotto et al., 2022), as attested by sanctioned offenses data from the Brazilian conservation unit authority (ICMBIO, 2024).

3.2.2 Subsamples and covariates

Ten "subsamples" were analysed, 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

[11] This share was above 75% for four of the six combinations.

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[12]). It was the only part of the Amazon Basin for which data was available to control for confounder policies. Remote-sensing mining data was also only available for Brazil. Abusing the meaning of "sample", what is here referred to as the third "subsample", also captured only Brazil, but included institutional covariates proxying non-PA-creation policies implemented simultaneously with creation. 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. All specific types of PAs we consider may exhibit particular 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 eighth subsample was an imposition of the limited availability of data about quality of management of PAs. The institution in charge of conservation units (ICMBIO) surveys units annually and, based on that, generates a five level index, which was aggregated in two levels, low-to-medium and high management quality (ICMBIO, 2024). The data available did not covered all units, as some did not fill the survey form and others could not be found in the original dataset, due to the lack of, or inconsistency in, the few variables available for unit retrieval. Only 30% of the units in our sample could be included in analysis. Only the latest survey year, 2022, was considered.

The final subsample comprised only pixels at 20 km from natural gold deposits. The locations of these deposits, informed by the Brazilian Geological Service (SGB, 2024), were used to select pixels where goldmining activity could take place. More precisely, pixels with at least five percent of their area within 20 km of the deposits were allocated to a subsample hereafter referred to as "gold reserve pixels". Pursuing analysis within this subsample avoided an

[12] 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").

overestimation bias due to the possibility of artisanal mining being less likely inside PAs because of a lack of mineral reserves.

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. With fires and mining as dependent variables, two extra covariates were included, the extents of deforestation of primary and secondary vegetation.

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 (2021) statement that time-variant covariates attenuate staggered treatment bias. In addition, one of the "subsamples" contained four institutional variables explicitly controlling for confounder policies. 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.

3.2.3 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 (what 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– thus ensuring that all treated pixels were observed also in their pre-treatment state.

3.2.4 Artisanal mining

The mining dependent variable was retrieved from Mapbiomas (2024), being originally generated from satellite imagery. It captured the land area occupied by artisanal mining of gold and other minerals ("garimpo") and was available only for the Brazilian portion of the Amazon Basin. The data was converted to binary variables indicating whether either artisanal mining, in general, or goldmining, specifically, occurred in each pixel-year. The analysis of mining was ran both within the subsample of pixels at 20 km from gold deposits and with the whole sample, as means for assessing estimates' robustness; in the former case only the goldmining dependent variable was part of analysis. Other subsamples were also considered, namely, indigenous lands, and conservation units permitting either direct or indirect resource usage.

4 Results

4.1 Main effects[13]

Tables 1 to 3 show the average treatment effect on the treated (ATT), estimated by multiple approaches (columns (1) to (7)), for deforestation, fires and mining. Starting with deforestation, in the matched subsamples[14], 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 8). This last estimate was over twice as large, in absolute value, 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).

Fires were similarly subjected to parallel trends violations (in lags -11,-10, -6, -4, -1), which biased ATT downwards in 39% (Tables 2 and 8). Both the failure to match and the lack of a post-matching analysis deflated ATT, with non-staggered post-matching deflating further (table 2). Similar findings were obtained for mining, whose estimates here mentioned refer only to

[13] 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).

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

indigenous lands, due to the nullity of the effects, and to the impossibility of estimating some of the group-time effects, in the subsample with all PAs (see table A.3 in the appendix). Significant pre-trends occurred at seven of the twelve exposure lengths (namely, -11, -10, -9,-8,-6,-5,-4), with failure to address this issue deflating the effect in 25%. The bias from not conducting a postmatching analysis was smaller, of 1%, but the main bias was not addressing the staggered nature of PAs, which deflated the effect in 90% (tables 3 and 8).

With the institutional variables that were available only for Brazil, 13% larger, 16% smaller and 25% smaller ATTs were estimated for deforestation, fires and mining, respectively, compared with a Brazilian subsample without institutional covariates (table 8). Therefore, concurrent non-PA policies decreased deforestation more largely outside PAs, whereas they decreased fires and mining more intensely inside PAs.

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. Different patterns were observed for fires and mining, which were blocked by indigenous lands and national conservation units. Subnational units unexpectedly presented a higher internal fire frequency than unprotected land, but, expectedly, diminished mining in a smaller magnitude – given the less limited resources available for management and enforcement at the national, rather than state, level (Herrera et al., 2019). Units differing on degree of protection stringency were all effective, but again the most restrictive were most effective, except for mining, for which the opposite was true.

There was no evidence that areas with higher quality of management avoided a larger extent of deforestation or fires; in fact, non-effectiveness prevailed, irrespective of how good management was. Such irrelevance of management quality was only reinforced by the mining results, which showed that both low-to-medium and high quality PAs diminished such form of resource exploitation (table 7).

Table 1 Effect of PAs on deforestation using several approaches: matching sample and post-matching DID, 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: matching sample and postmatching DID, 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 artisanal mining using several approaches: matching sample and postmatching DID, DiD-FE and group-time estimates, Brazilian indigenous PAs only

	(1)	(2)	(3)	(4)
	Matching only	DID-FE	Group-time, matched 1 SD, all groups	Group-time, matched 1 SD, only non-sig. pre-treat. groups
ATT	-0.045***	-0.00437*	-0.034***	-0.045***
SE	[0.0013]	[0.0019]	[0.0046]	[0.0064]
N	168,264	168,264	168,264	91,296
Clusters	9,348	9,348	9,348	5,072

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All protected areas, without institu-tional covariates, Amazon Basin	All protected areas, without institu-tional covariates, Brazilian Amazon	All protected areas, with institutional covariates, Brazilian Amazon	Only indigenous lands, Amazon Basin	Only subnational conservation units, Amazon Basin	Only national conservation units, Amazon Basin	Only indirect conservation units, Amazon Basin	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 5 Effect of PAs on fire: Brazilian Amazon and PAs types' samples, group-time estimates after exclusion of critical groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All protected areas, without institu-tional covariates, Amazon Basin	**All protected areas, without institu-tional covariates, Brazilian Amazon**	**All protected areas, with institu-tional covariates, Brazilian Amazon**	**Only indigenous lands, Amazon Basin**	**Only subnational conservation units, Amazon Basin**	**Only national conservation units, Amazon Basin**	**Only indirect conservation units, Amazon Basin**	**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 6 Effect of PAs on mining: PAs types' samples, Brazil, group-time estimates after exclusion of critical groups

	(1)	(2)	(3)	(4)	(5)	(6)
	Indigenous lands, without institutional covariates	**Indigenous lands, with institutional covariates**	**Only subnational conservation units, Brazil**	**Only national conservation units, Brazil**	**Only indirect conservation units, Brazil**	**Only direct conservation units, Brazil**
ATT	-0.0448481***	-.0360017***	-0.0312038***	-.0793828***	-.0498855***	-0.0542048***
SE	[0.0064]	[0.00799]	[0.0087]	[0.0087]	[0.00976]	[0.0062]
N	91296	81,612	99,648	83,556	75,978	175,896
Clusters	5072	4,534	5,536	4,642	4,221	9,772

Table 7 Effect of Brazilian PAs of medium-to-low and high quality of management: group-time estimates after exclusion of critical groups

	Deforestation		Fires		Mining	
	High quality	Low-to-medium quality	High quality	Low-to-medium quality	High quality	Low-to-medium quality
ATT	0.0024	0.0653**	-0.0266+	-0.06837***	-0.0321*	-0.0684***
SE	[0.0147]	[0.0216]	[0.0147]	[0.0079]	[0.0149]	[0.0079]
N	61,578	217,746	64,998	217,098	37,062	198,360
Clusters	3,421	12,097	3,611	12,061	2,059	11,020

Note: management quality was measured by the authority in charge of Brazilian conservation units, based on a multidimensional indicator developed by the own authority and based on questionnaires responded by the staff of the areas (ICMBIO, 2024). Not all PAs were evaluated.

Table 8 Four biases in naïve estimation (relative [and absolute] calculation)

	Deforestation	Fires	Artisanal mining
"Matching alone" bias	-73 % [-1.84%]	-4 % [-0.26%]	1 % [0.04%]
Staggered protection bias	-50 % [-1.26%]	-91 % [-5.49%]	-90 % [-4.05%]
Unparalleled trends bias	11 % [0.28%]	-39 % [-2.32%]	-25 % [-1.1%]
Concurrent policy bias	-13 % [-0.42%]	16 % [0.86%]	25 % [0.88%]

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 abs(biased) – abs(unbiased), with "abs" standing for absolute value. Artisanal mining numbers refer to indigenous lands only.

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 9, which compares critical cohort exclusion with the inclusion of top-five cohorts in the weights obtained as part of Goodman-Bacon's (2021) decomposition. Nevertheless, in the case of fires (table 10), robustness was restricted to estimates' sign, due to 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. The same is true regarding mining, whose estimates differed not only in size, but also in significance if based on the groups selected with the robustness test criterion (Table 11).

Furthermore, the direction of change in effects after controlling for concurrent policies was also robust for the deforestation and fires, but not for mining. In all three cases, the magnitude of change was smaller in the robustness test.

Table 9 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%	-0.028***	-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 10 Robustness test, fires

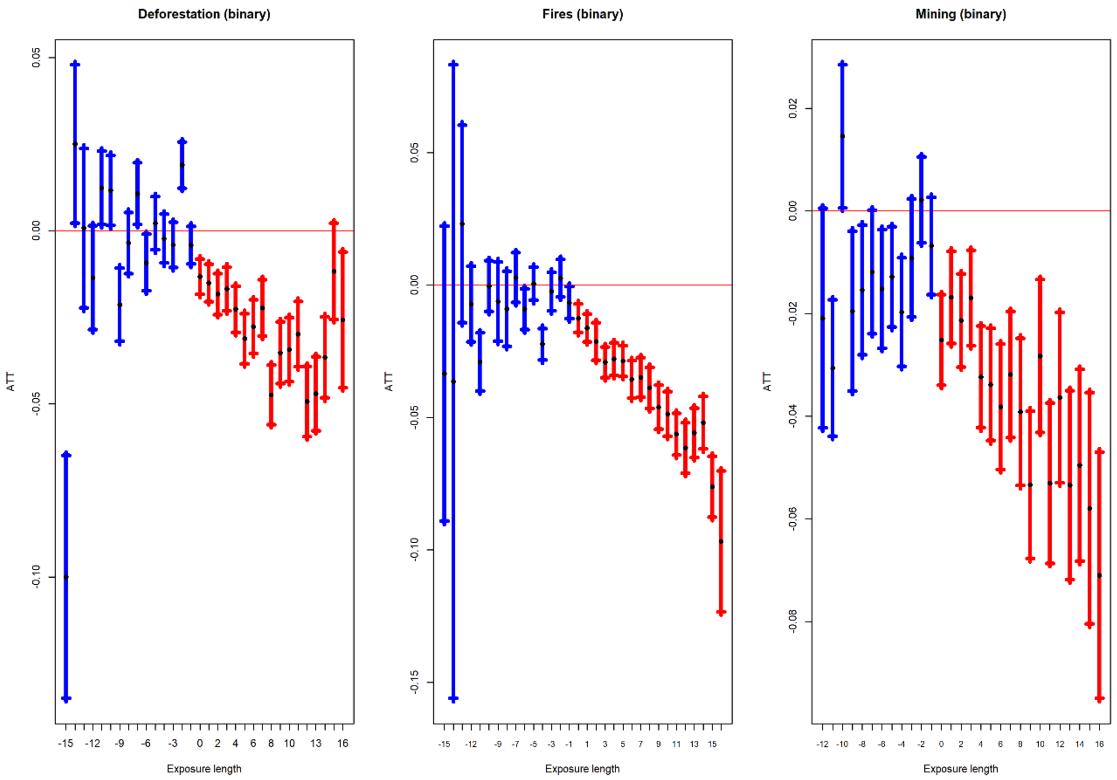
	(1)	(2)		(3)	(4)		(5)	(6)	
	All PAs			Only Brazilian PAs			Only Brazilian PAs with inst. var.		
	Critical groups	**Top-five weights (rob.)**	**Percent diff [(2)/(1) -1]**	**Critical groups**	**Top-five weights (rob.)**	**Percent diff [(4)/(3) -1]**	**Critical groups**	**Top-five weights (rob.)**	**Percent 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	

Table 11 Robustness test, mining

	(1)	(2)		(3)	(4)	
	Brazilian indigenous lands			Brazilian indigenous 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]**
ATT	-0.045***	-0.01	-78%	-0.0360017***	-0.016+	44%
	[0.0064]	[0.0069]		[0.0079]	[0.00871]	
N	91,296	98,100		81,612	97,164	
N clusters	5,072	5,450		4,534	5,398	

4.3 Dynamic effects

Figure 4 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 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) indigenous lands, (iii and iv) subnational or national conservation units, (v and vi) Brazil with or without institutional covariates. The event studies here described, which contain all groups, without any attempt to address significant pre-treatment effects, are found in figure 4 and in appendix 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 4; 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 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)[15]. 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.

Negative pre-protection effects on fires four years and eleven years before protection were systematically observed across all 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).

Another peculiarity of conservation units' event studies for deforestation was the 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 observed for fires. Whereas conservation units presented two or three positive pre-treatment effects, indigenous lands presented only one. The converse was seen for mining, in which case significant pre-treatment effects were more numerous among indigenous PAs[16]. Also, a negative pre-protection effect four years before protection was observed for mining.

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

[15] 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.

[16] Seven significant pre-treatment effects against at most three for specific types of conservation units; national units are an exception as they had almost the same number of significant effects of indigenous PAs.

post-protection effects observed in leads 2, 8, 9, 11, 13 and 14. All post-treatment effects, up to sixteen years after creation were significantly negative in the case of mining.

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 takes 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. It was also observed for mining, in indigenous lands and conservation units' subsamples. 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.

To confirm and better understand the pre-rise in deforestation, leads of the time-variant treatment variable were added to a two-way fixed effects model, as seen below:

$$y_{i,t} = \beta_0 + \delta d\_PA_{i,t} + \sum_{j=1}^{L} \alpha_j d\_PA_{i,t+j} + \beta_1 x_{i,t} + a_i + u_{i,t}$$

Up to six leads were considered as this was the level of a proxy for the duration of the PA creation process[17]. The most consistent patterns revealed by results were the positive second lead and the negative sixth lead (table 12). Which means that deforestation, fires and mining decreased six years before creation of conservation units, which is when the average unit started being created. It also means that, importantly, the three outcomes rose two years before creation, which is another evidence of the forest rush.

[17] Since creation time was not a public information, we relied on a proxy, the average number of years separating the start, by the competing authority, of the bureaucratic process leading to creation, and creation itself, a proxy for creation time. This is inexact because creation may have started before the bureaucratic process. The average of a sample of 15 conservation units was 5.13 years.

Table 12 Treatment lead tests, FE regressions

	Deforestation			Fires			Mining		
	All PAs	**Subnational conservation units**	**National conservation units**	**All PAs**	**Subnational conservation units**	**National conservation units**	**All PAs (Brazil)**	**Subnational conservation units (Brazil)**	**National conservation units (Brazil)**
Negative leads	3		6	6	6	6	6	6	6
Positive leads	2	2	2,4	2	2, 5	2		2,3	2,5
F-stat	126.76	133.81	189.49	281.37	68.14	161.28	8.26	101.93	148.39
p-value	<0.01%	<0.01%	<0.01%	<0.01%	<0.01%	<0.01%	0.2195	<0.01%	<0.01%
N	594,702	143,298	256,266	592,380	141,696	255,978	473,940	111,330	204,282
Clusters	33,039	7,961	14,237	32,910	7,872	14,221	26,330	6,185	11,349

4.4 Further robustness tests

The robustness of matching was assessed with an alternative approach. It selected controlled and treated pixels as those within 50 or 100 km of PAs' boundaries, but, respectively, either outside or inside a PA. Distances were calculated in order to accommodate the time variation of pixel-to-boundary distance, due to the staggered nature of protection. As the result, matching-based effects on deforestation proved non-robust in terms of sign, which was positive in the robustness test and without controlling for institutional factors (appendix 3, tables A.4 and A.5). When controlling, sign was robust, but ATTs' magnitudes were up to 88% larger. For the case of fires, estimates' sign proved robust, but the magnitude did not, with distance-based ATTs systematically smaller in up to 75%. Nevertheless, since spatial proximity does not ensure protected and unprotected pixels are satisfactorily comparable, these discrepancies should be taken as indication that deforestation effects' signs may be heterogeneous in the spatial dimension, and that both deforestations' and fires' effects magnitudes are spatially heterogeneous.

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 introduced by 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 and mining. Furthermore, specific dynamics were observed by type of PA, with conservation units being more exposed to pre-protection rises in deforestation and fires, but not in mining.

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. (2019) estimated equivalent effects for the two PA types. But the greatest opposition to this paper's results, in which indigenous lands had either the first or second largest inhibition effect on deforestation, fires and mining, comes from BenYishay et al. (2017), who found a null effect of such PA type[18]. 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.

The substantial biases due to confounder policies is an indirect evidence that these polices considerably altered outcome variables. 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

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

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 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 or mining. 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 and mining, 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 legalisation (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 available 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., 1999). 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 recognised 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.

That PAs effectively avoided mining is not at odds with the literature, despite the recent growth of the activity inside these areas (Moreno-Louzada and Menezes-Filho, 2023, Asner and Tupayachi, 2017). The mechanism is the same as for deforestation and fires. As in the theoretical model, the higher likelihood of sanction within PAs counterbalances the incentive from the presence of natural reserves. But that is only true where enforcement is systematically present, which is not the case for all PAs (Asner and Tupayachi, 2017, Weisse and Naughton-Treves, 2016). Therefore, our results suggest, indirectly, that enforcement of Brazilian PAs has been enough to contain, or at least mitigate, artisanal mining. This is remarkable, given the attractiveness of the activity in the region and its negative environmental, and also social, consequences (Teixeira et al., 2021, Asner and Tupayachi, 2017, Weisse and Naughton-Treves, 2016).

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 already is.

The non-robustness of the magnitudes of fires' and mining's 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 damage inhibition, being more and less effective at different stages of their lifetime, another reason for avoiding aggregations that treats them as homogeneous.

It is noteworthy that, despite PAs' effect on mining have proved more robust, this damage source, differently from deforestation and fires, is subject to the natural barrier of absence of mineral reserves. Thus, instead of a proof of effectiveness of institutional protection, this can be merely evidence of effectiveness of “natural protection” and thus of non-additionality of PAs against mining.

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 resource extractors with governmental conservation goals. That means signalling that sanction probability will not only increase after creation, but immediately, thus leaving no time for a resource exploitation rush.

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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)

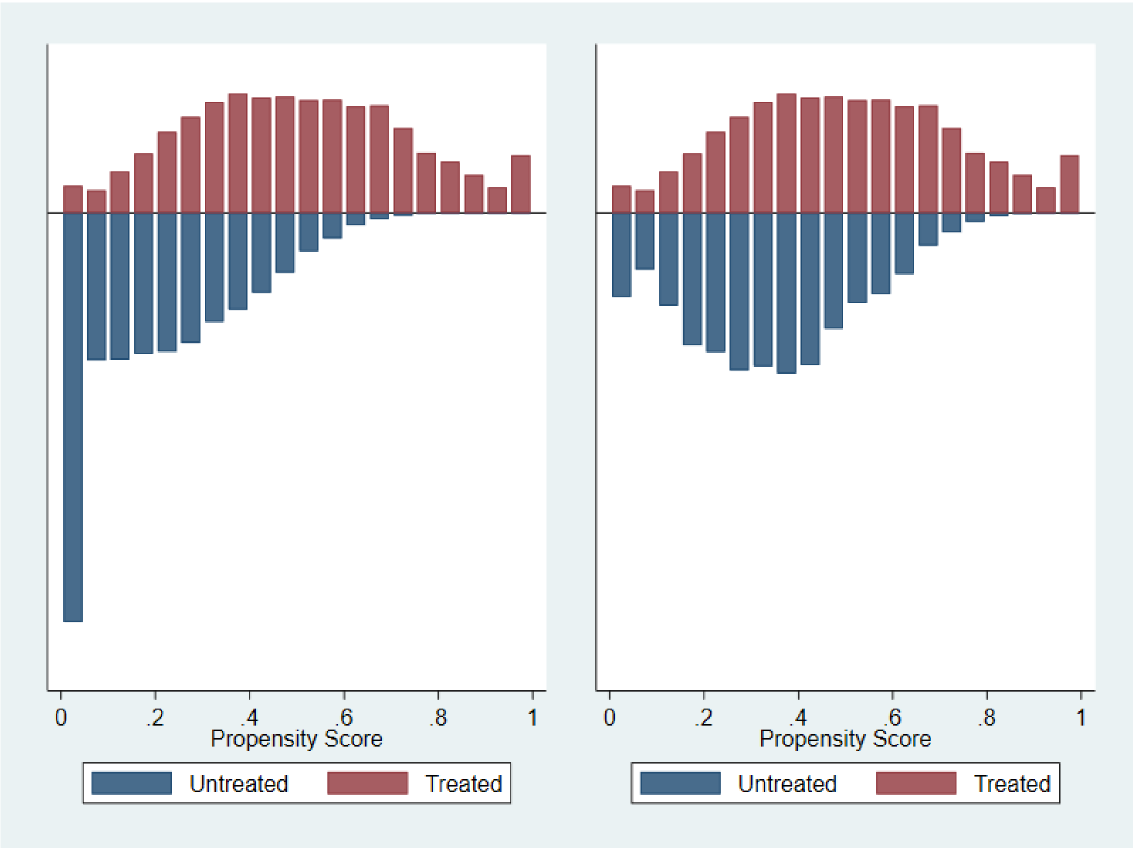


Figure A.1.2 Common support graph, 1SD-caliper matching, before matching (left) and after matching (right)

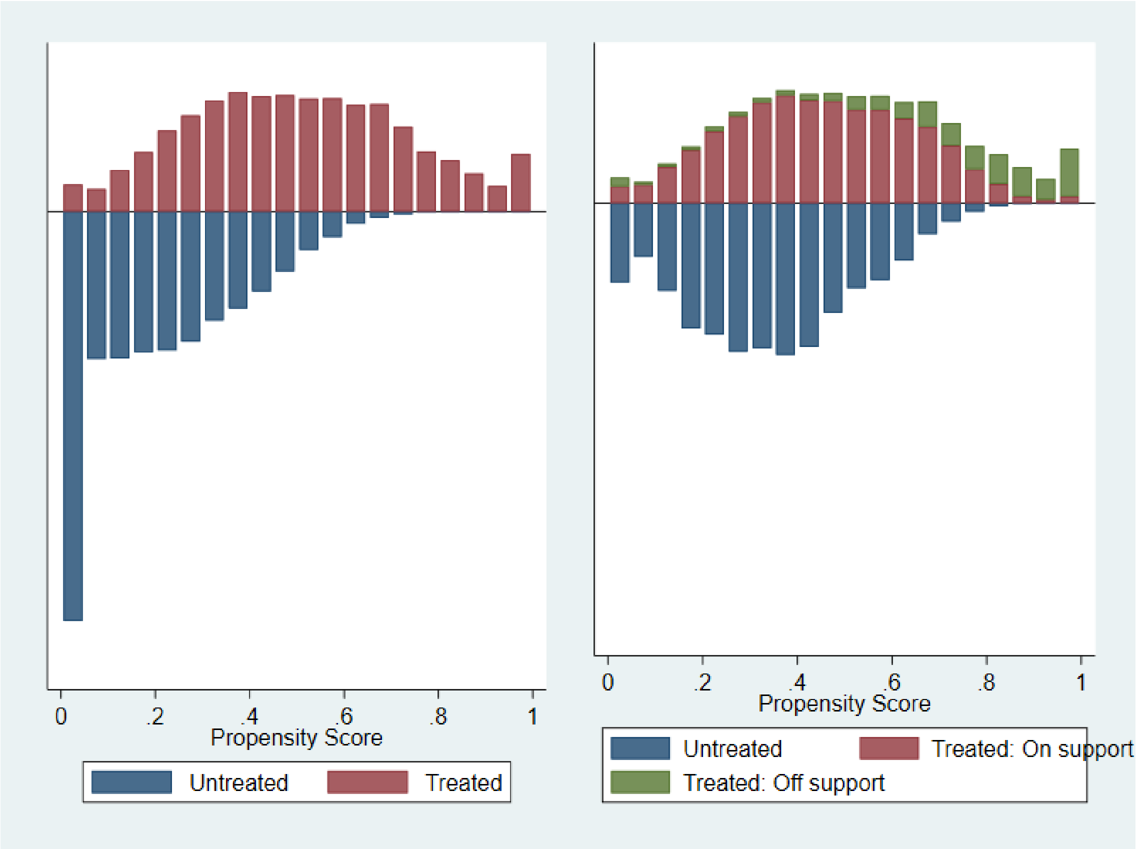


Figure A.1.3 Balance graph, non-caliper matching

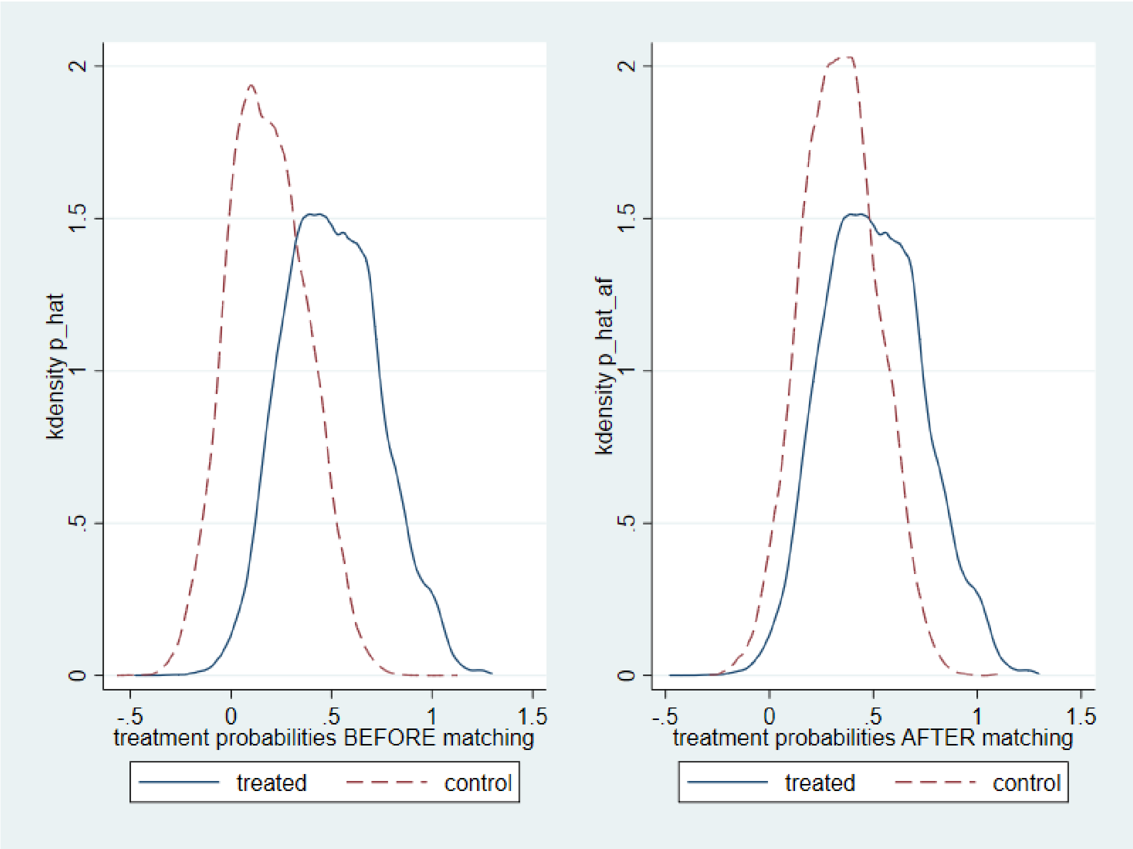
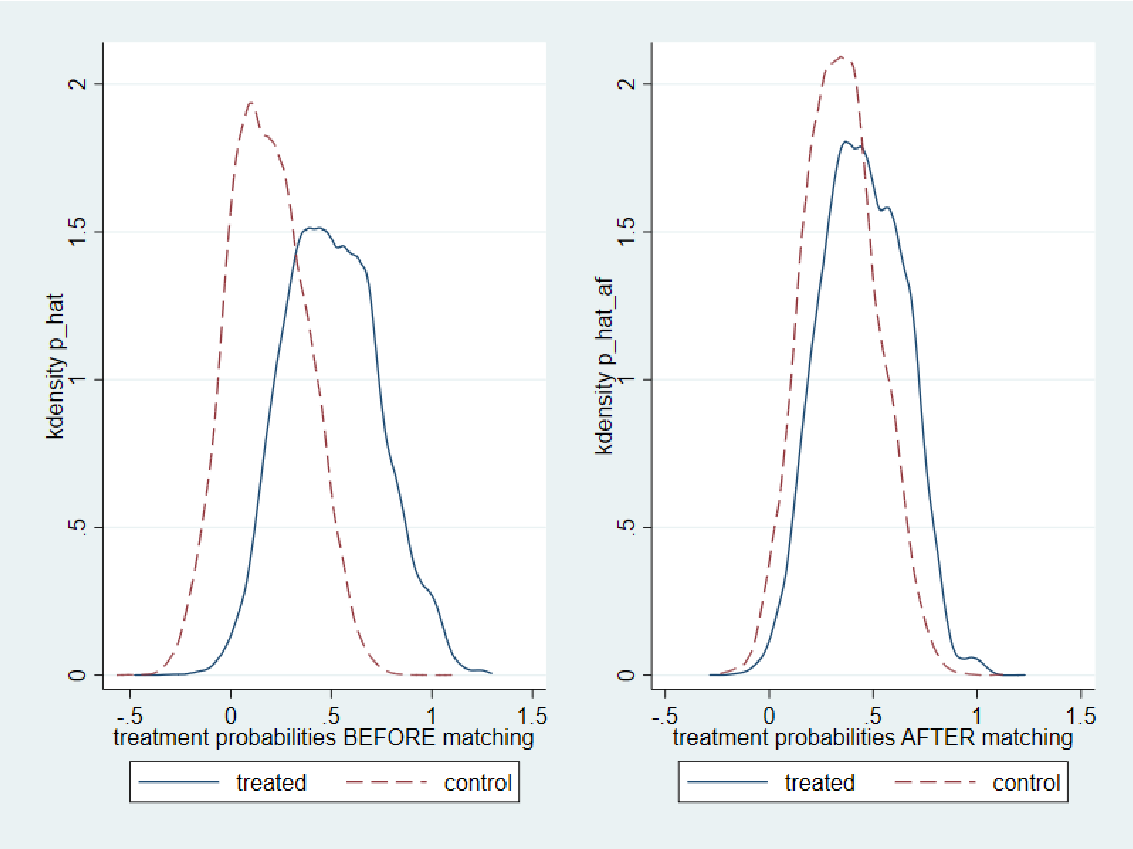


Figure A.1.4 Balance graph, 1SD-caliper matching



A.2 Fires and mining

The covariate set used for matching in the case of fires and mining 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)

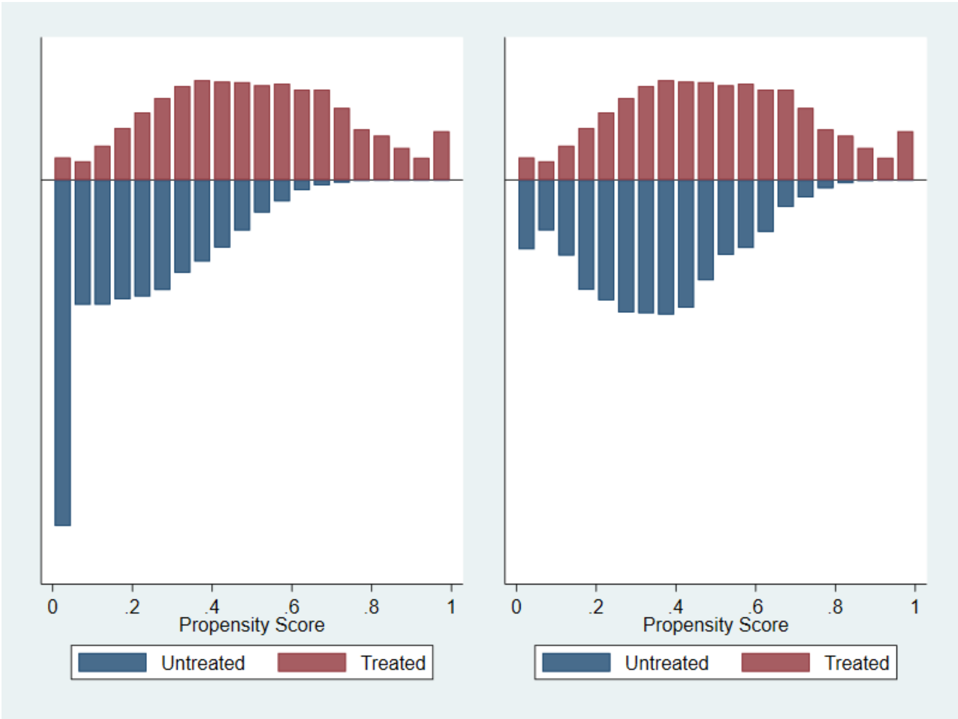


Figure A.1.6 Common support graph, 1SD-caliper matching, before matching (left) and after matching (right)

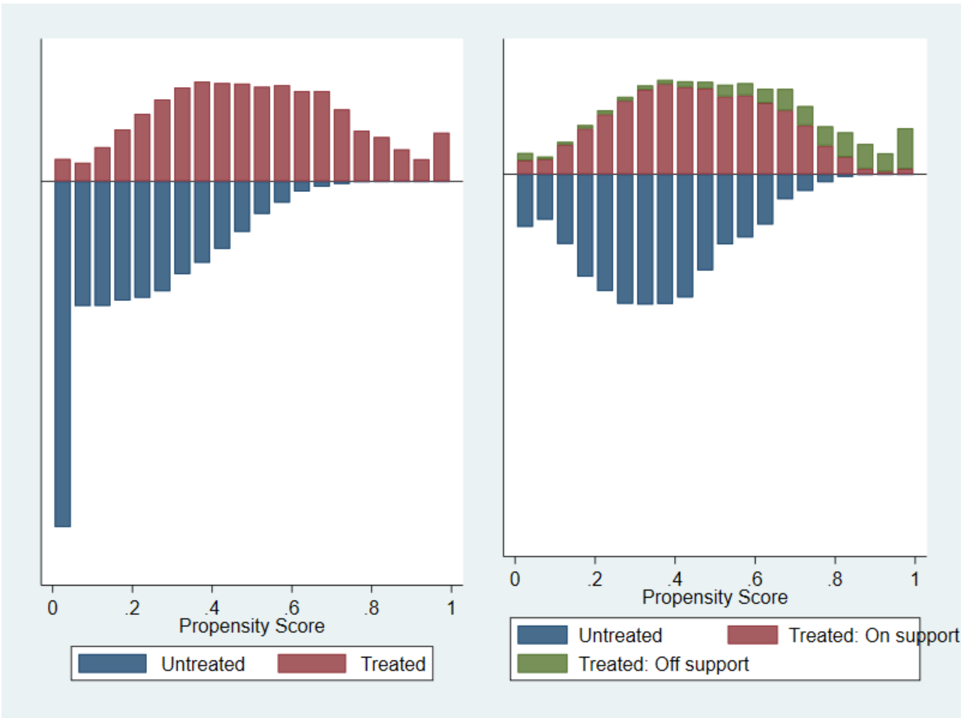


Figure A.1.7 Balance graph, non-caliper matching, before matching (left) and after matching (right)

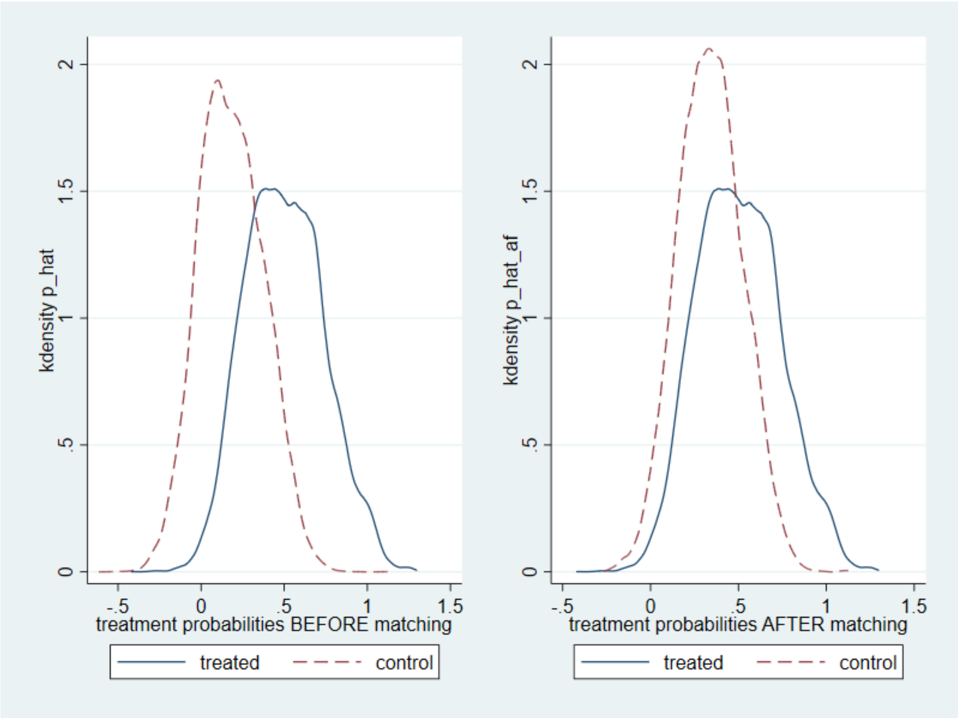
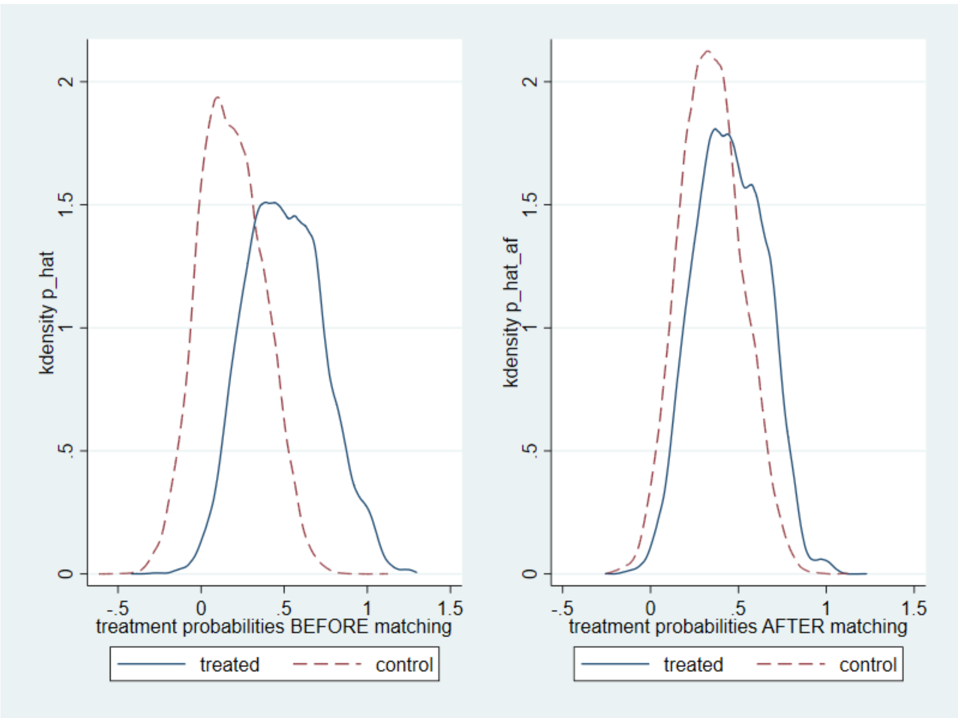


Figure A.1.8 Balance graph, 1SD-caliper matching, before matching (left) and after matching (right)



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)

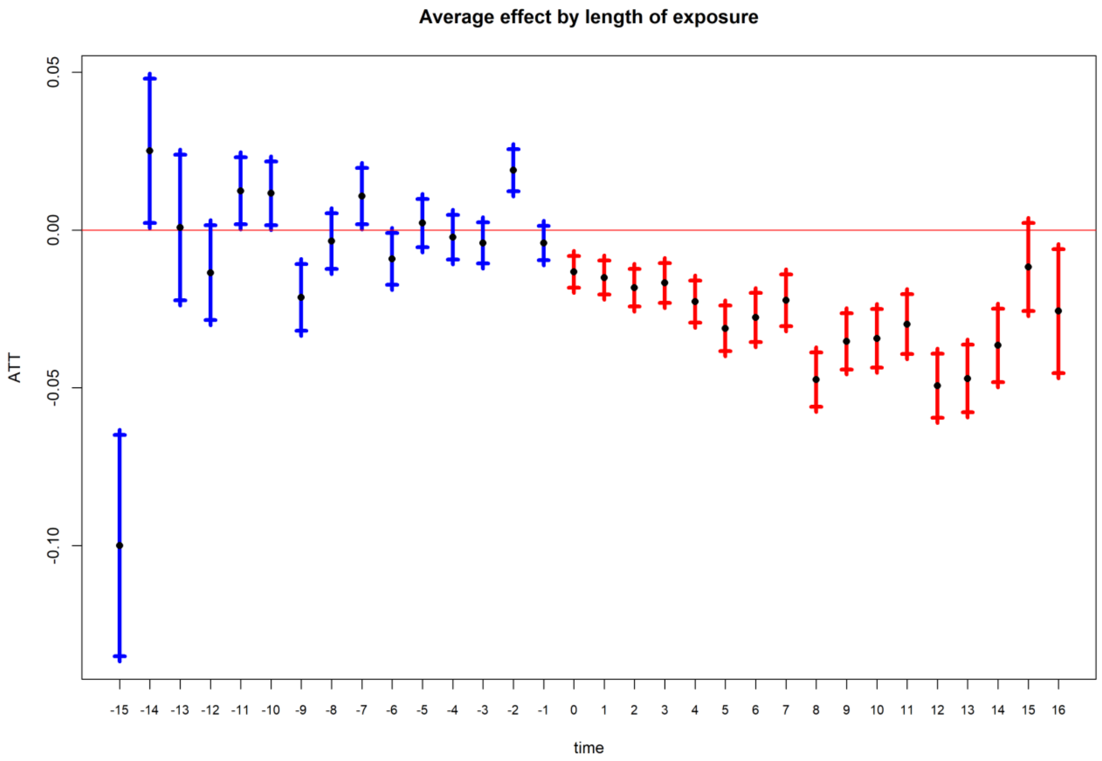
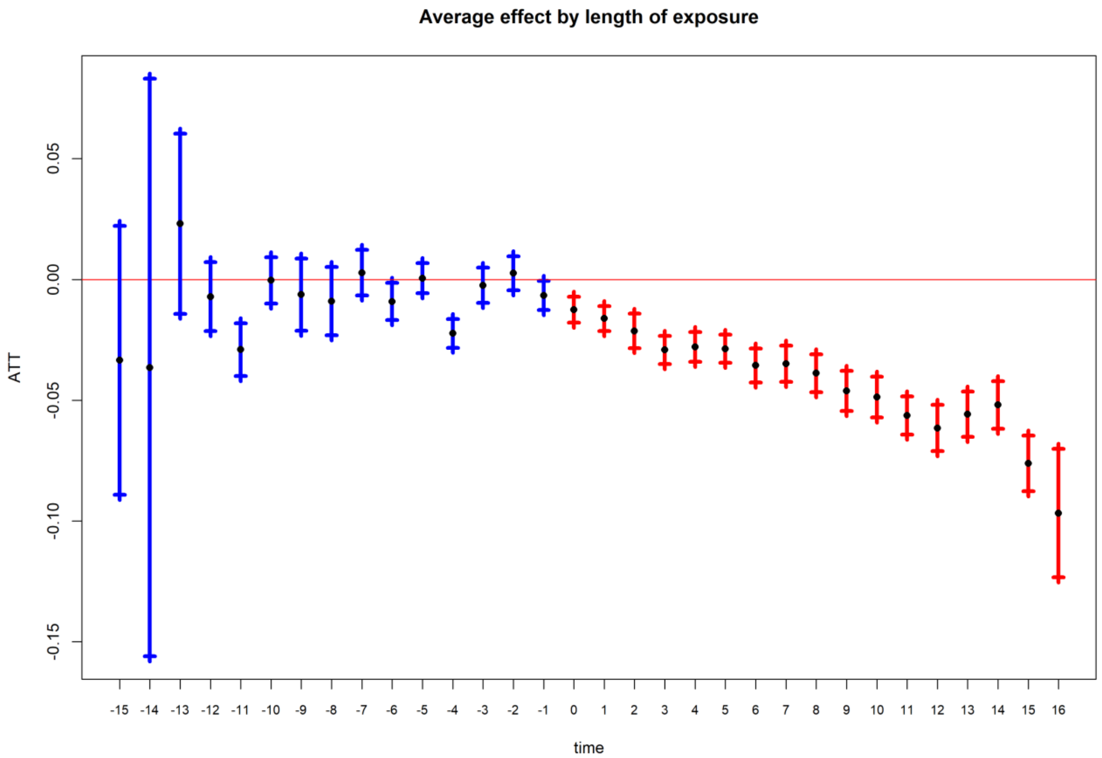


Figure A.2.1.2 Event Study for fires, whole 1 SD caliper sample, all groups (blue = pre-treatment, red = post-treatment)



A.2.1.2 Without critical groups

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

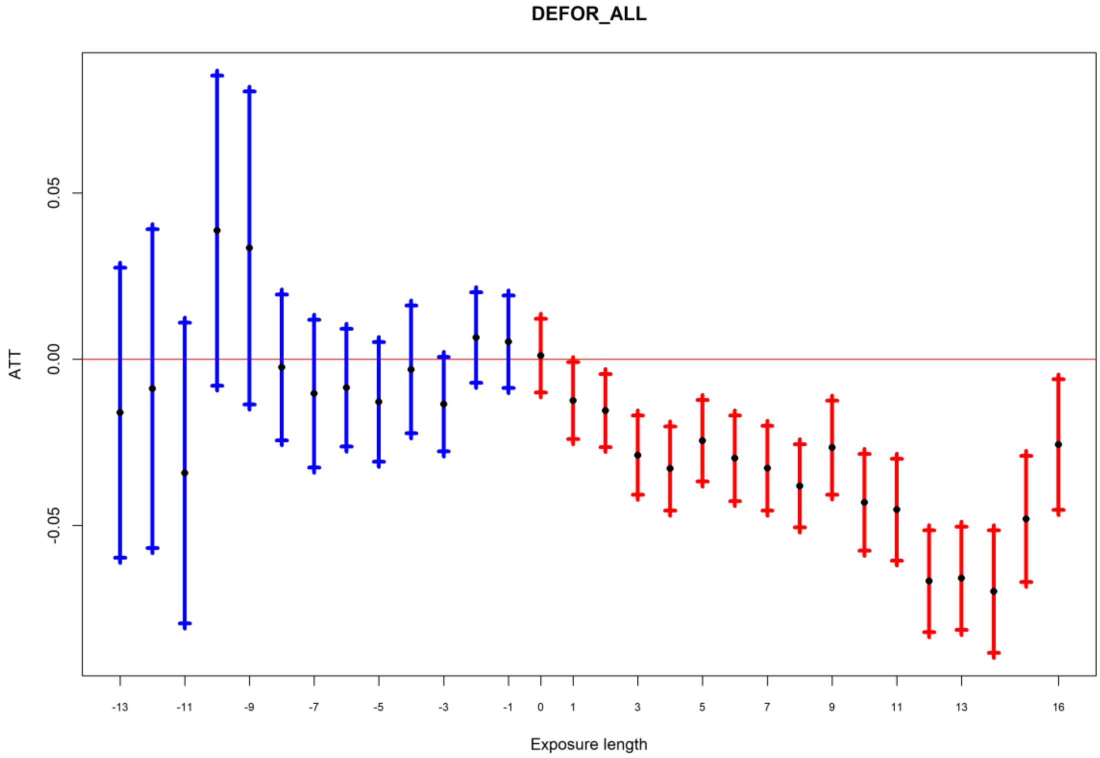
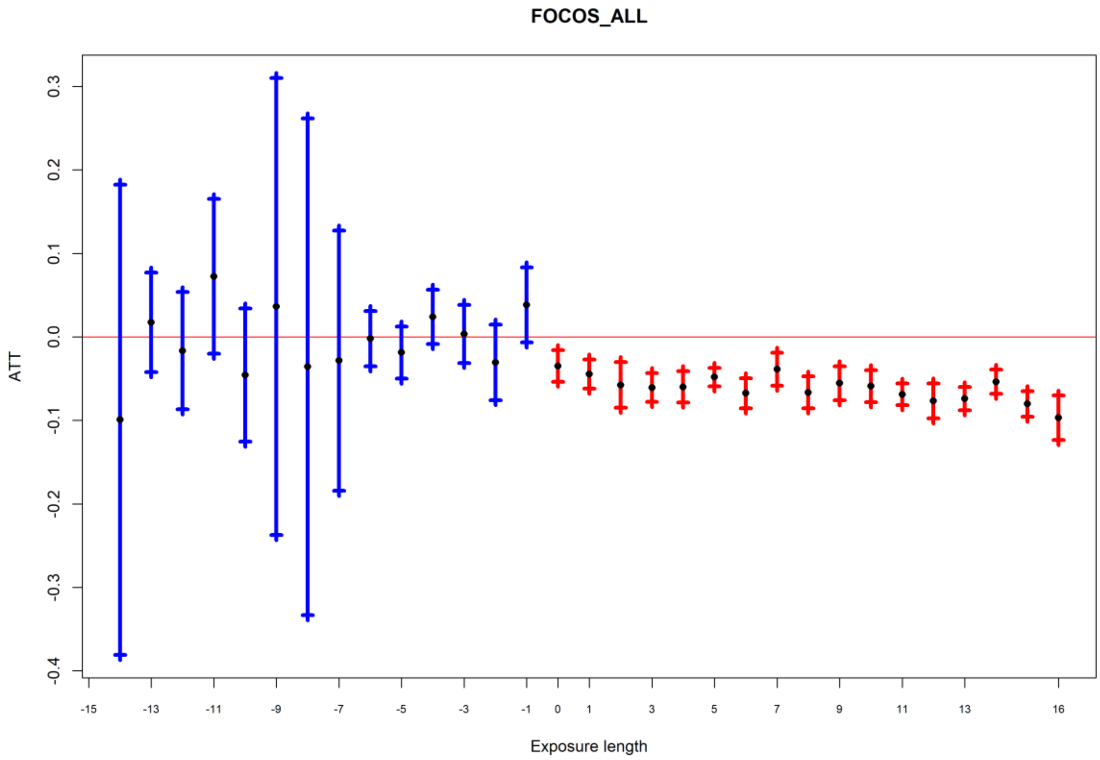


Figure A.2.1.4 Event Study for fires, whole 1 SD caliper sample, without critical groups



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

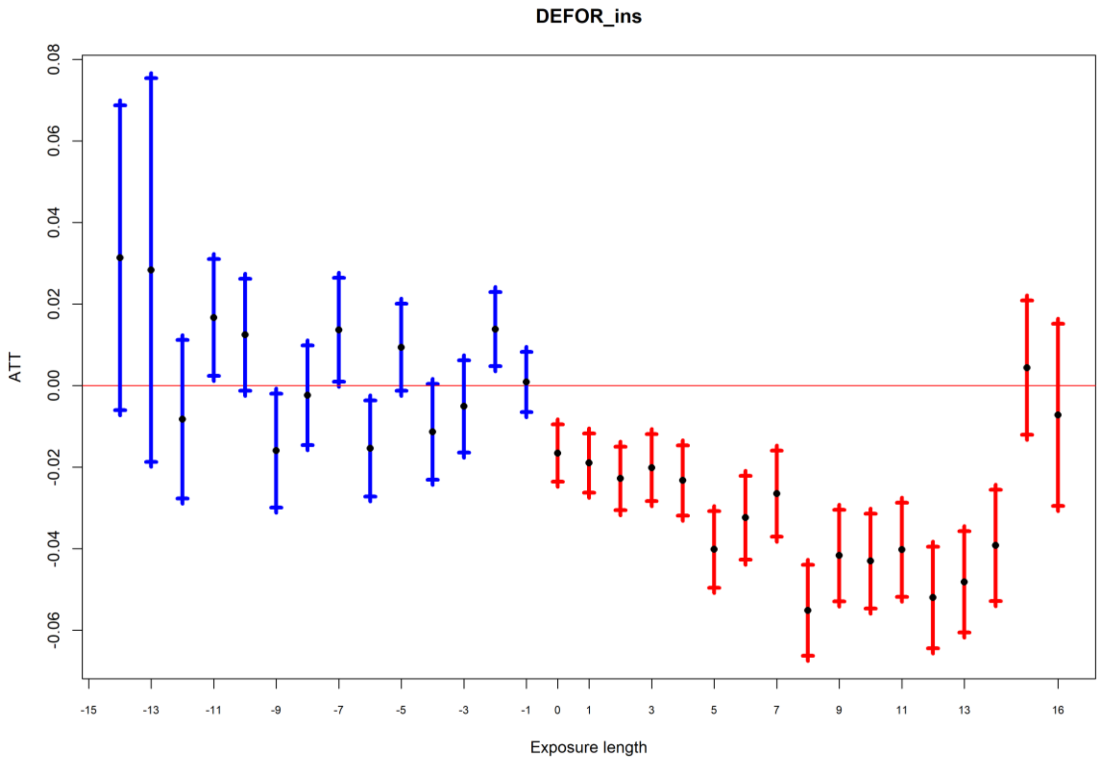


Figure A.2.2.2 Event Study for fires, Brazil-only sample with institutional variables, all groups

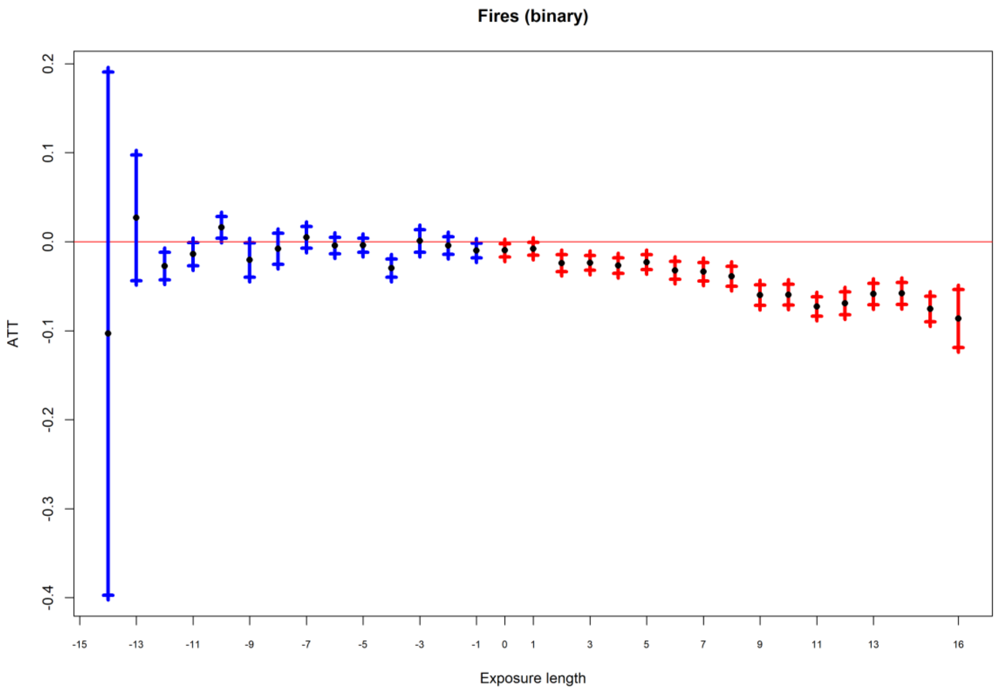
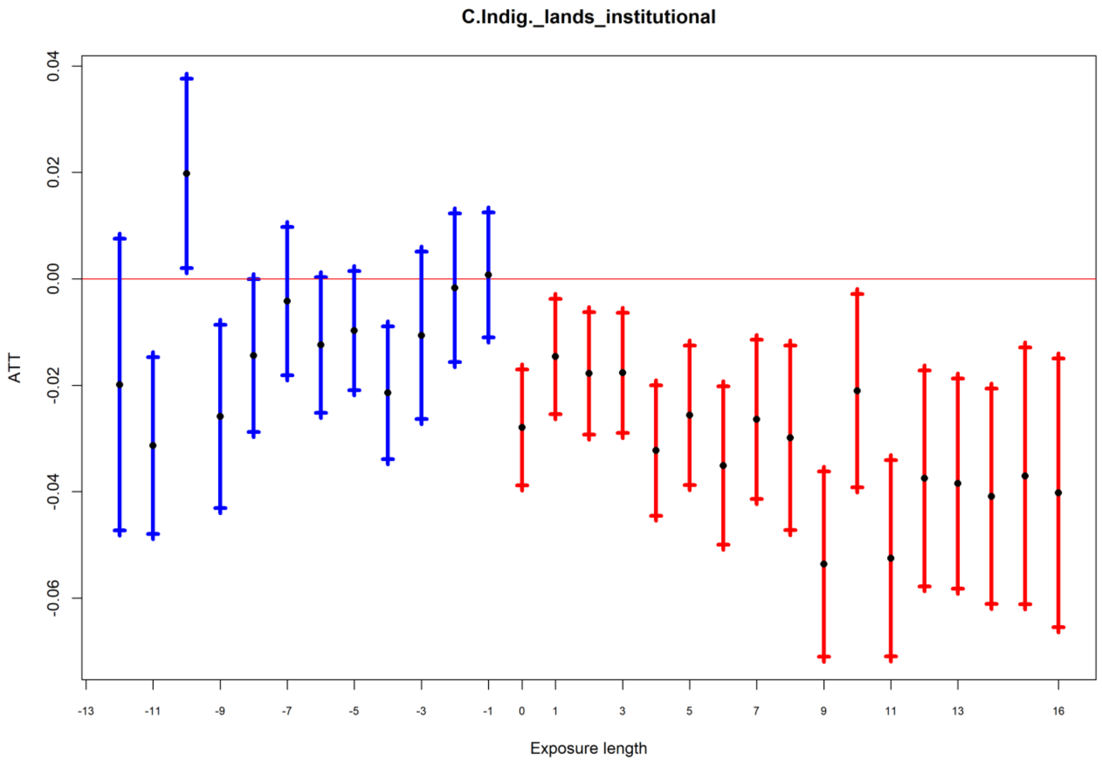


Figure A.2.2.3 Event Study for mining, Indigenous lands subsample with institutional variables, all groups



Note: due to the nullity of PAs' effect in the subsample with all Brazilian PAs, this plot refers to the Brazilian indigenous lands subsample, where the effect was significant.

<u>A.2.2.2 Without critical groups</u>

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

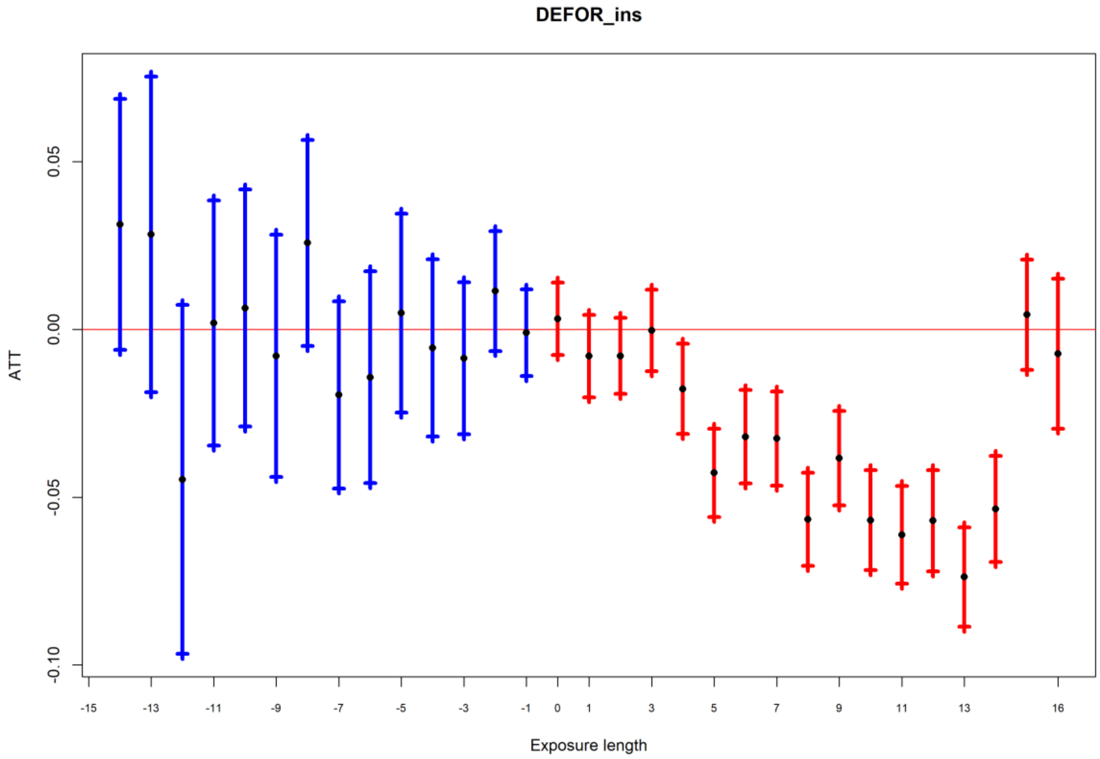


Figure A.2.2.5 Event Study for fires, Brazil-only sample with institutional variables, without critical groups

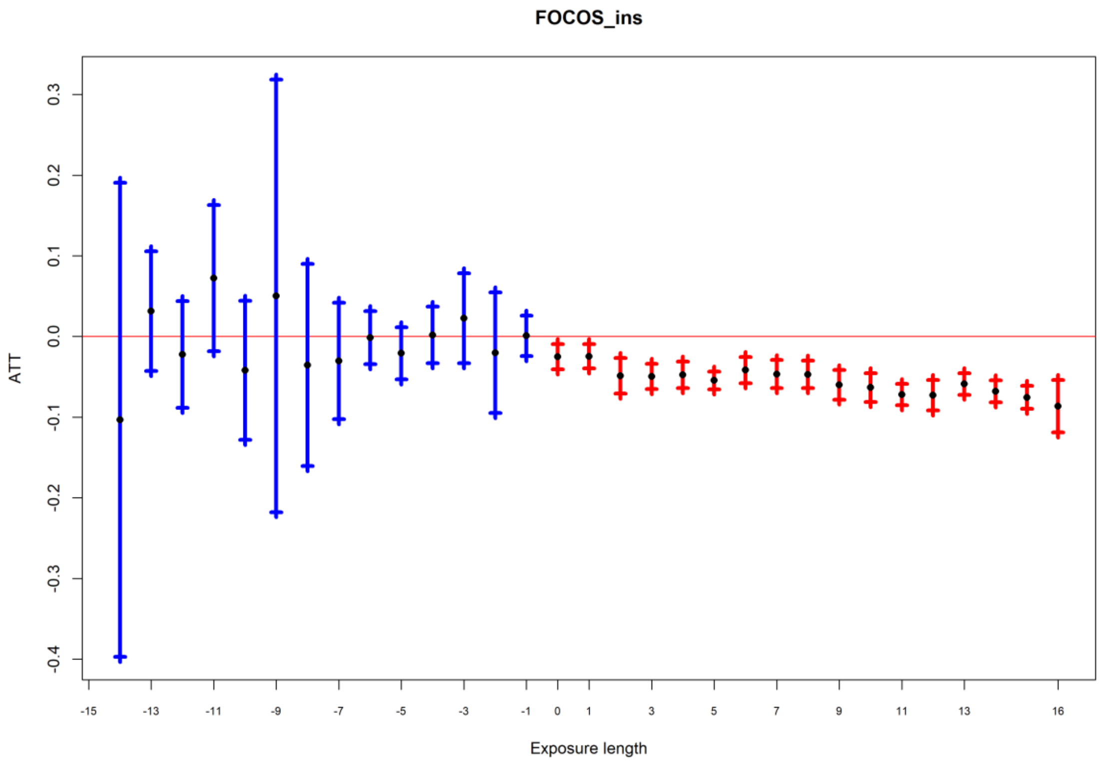
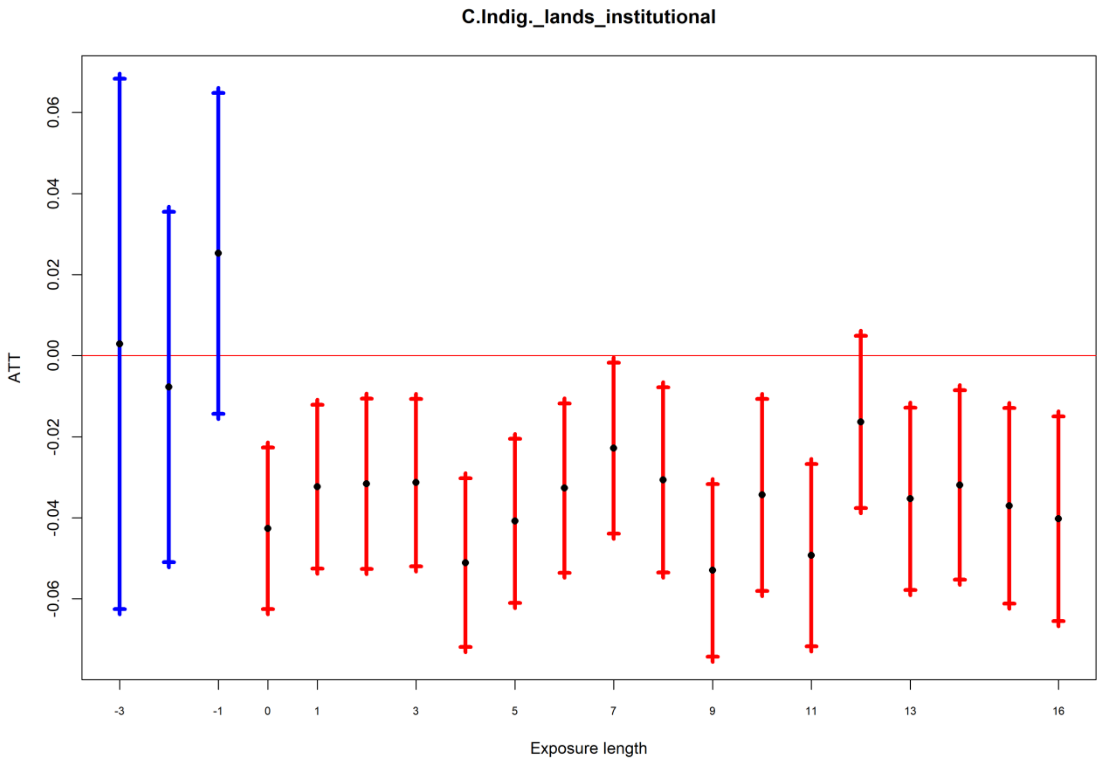


Figure A.2.2.6 Event Study for mining, Brazil-only sample with institutional variables, without critical groups



Note: due to the nullity of PAs' effect in the subsample with all Brazilian PAs, this plot refers to the Brazilian indigenous lands subsample, where the effect was significant.

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

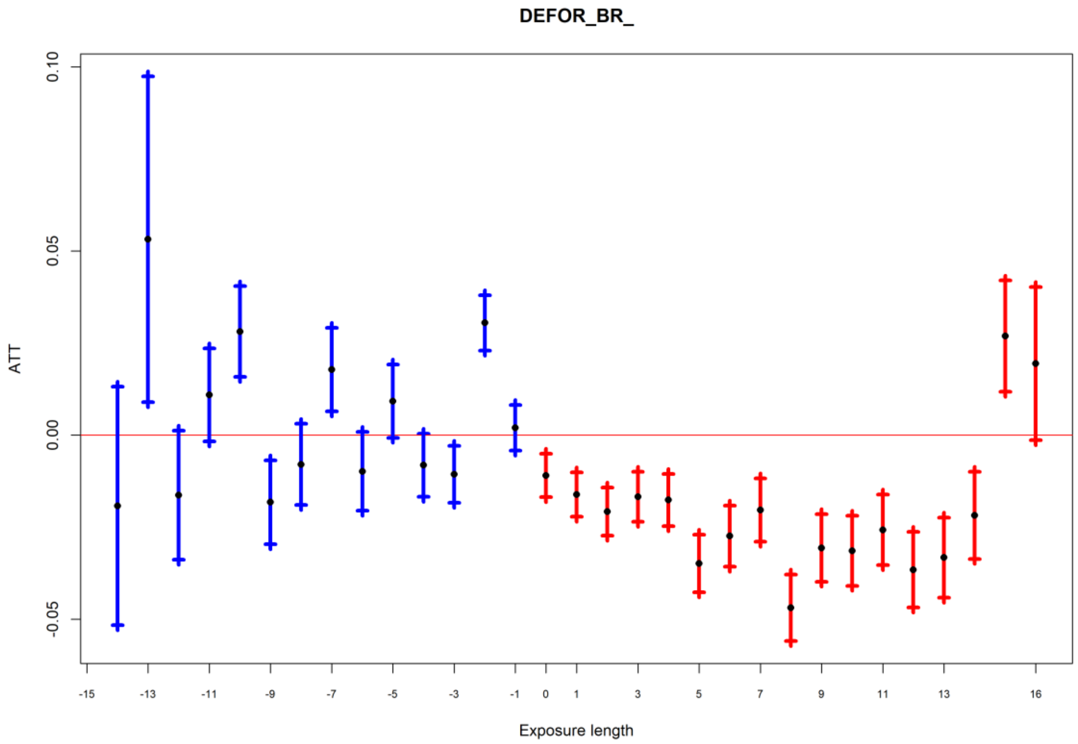


Figure A.2.3.2 Event Study for fires, Brazil-only sample without institutional variables, all groups

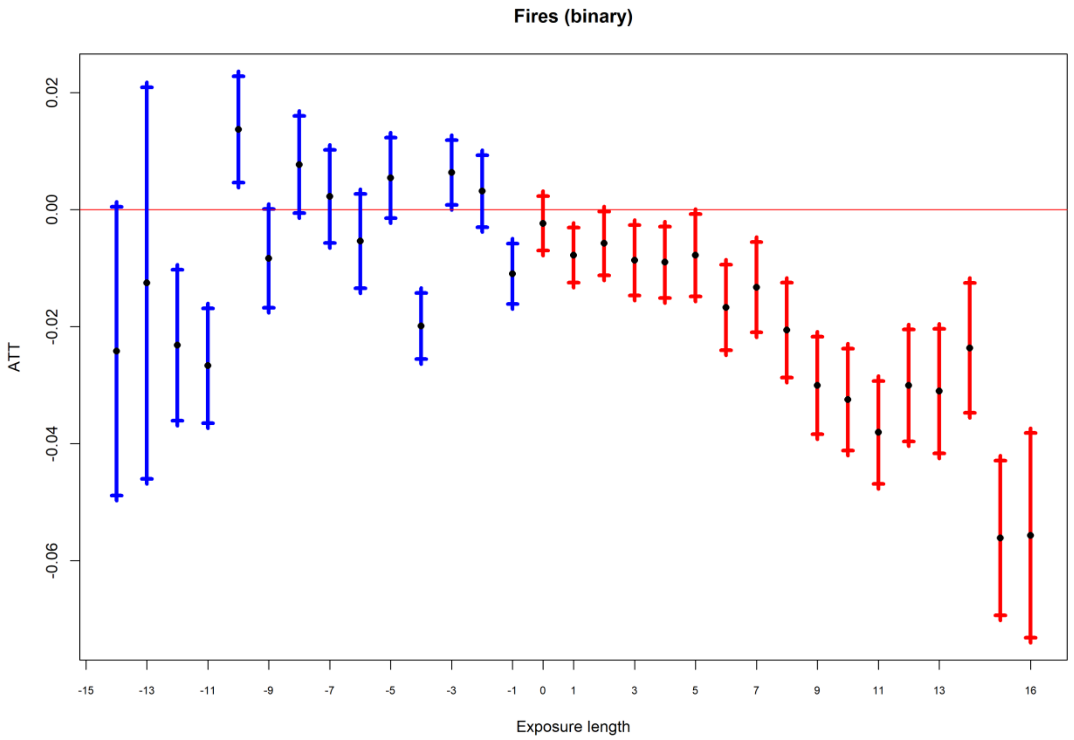
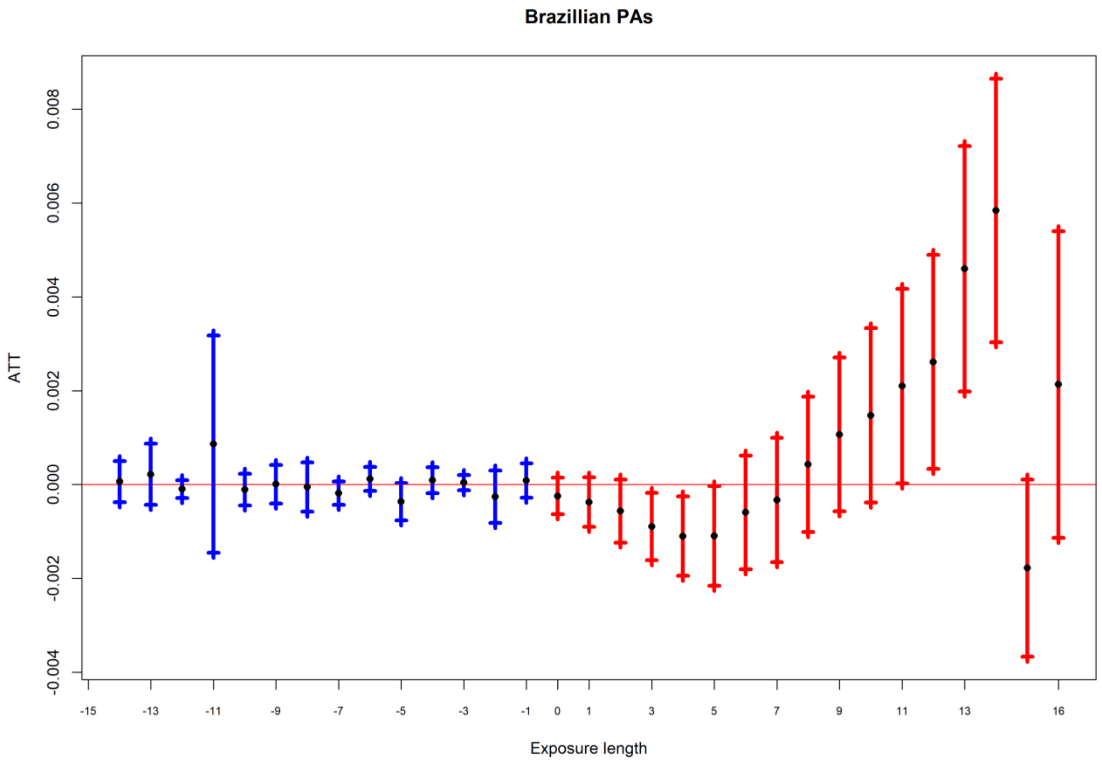


Figure A.2.3.3 Event Study for mining, Brazil-only sample without institutional variables, all groups



A.2.3.2 Without critical groups

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

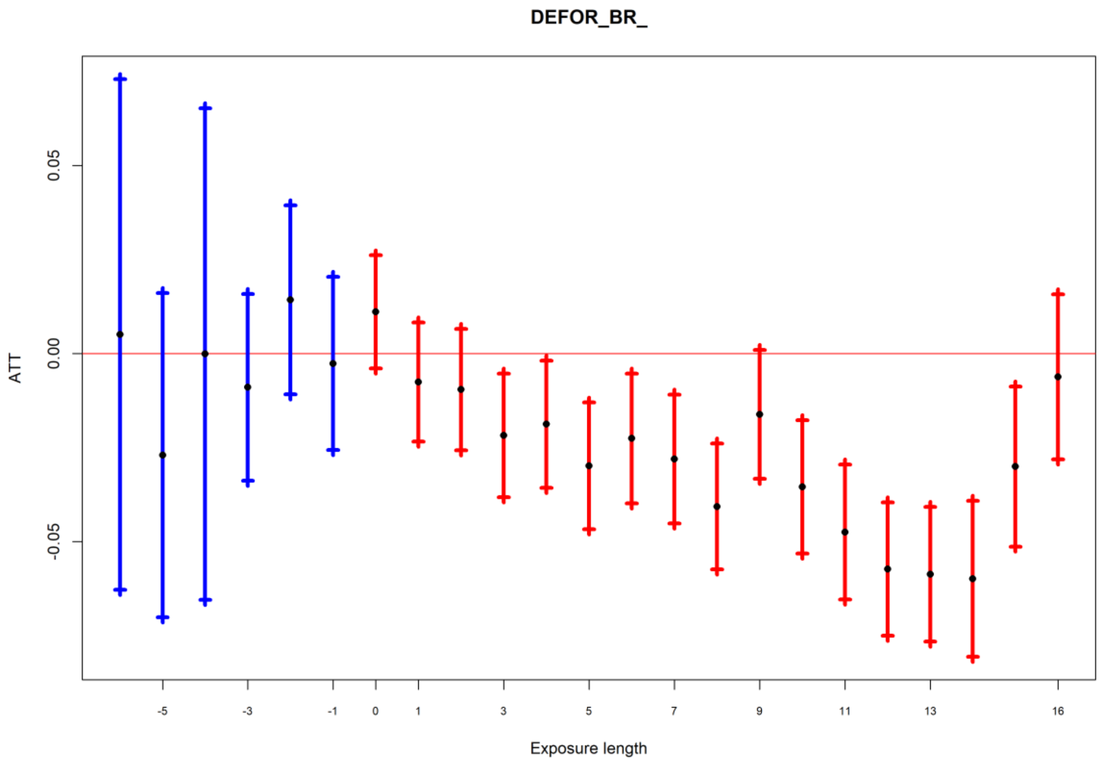


Figure A.2.3.5 Event Study for fires, Brazil-only sample without institutional variables, without critical groups

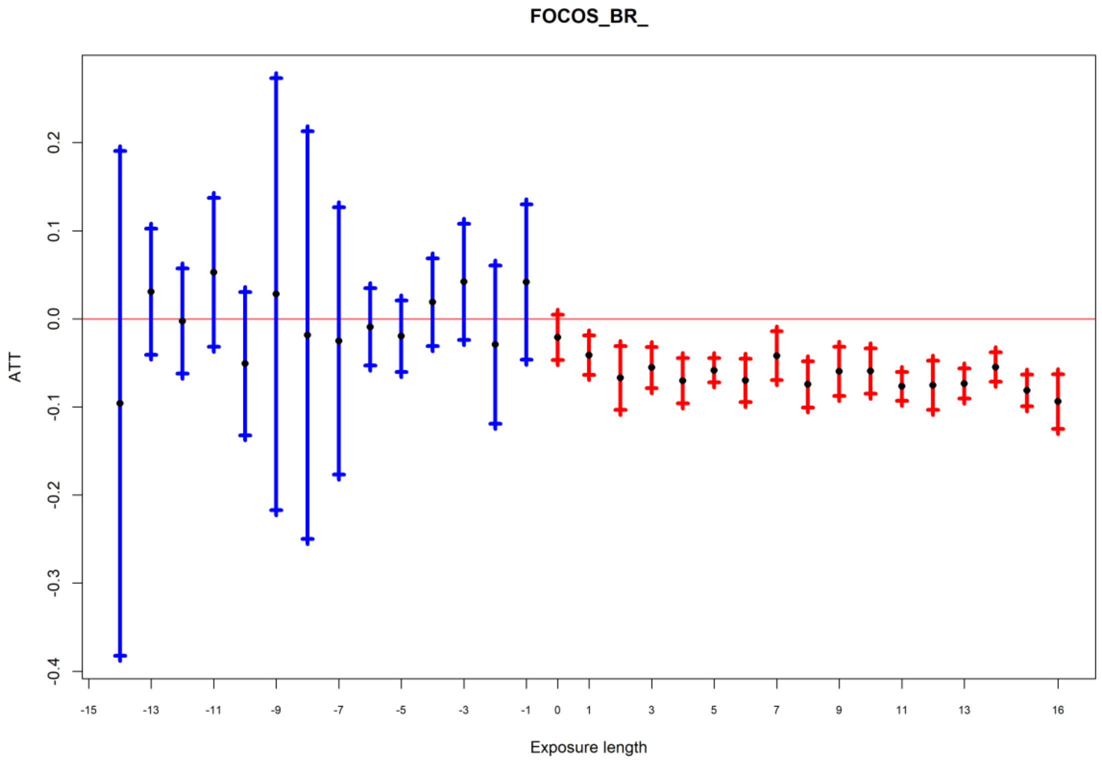
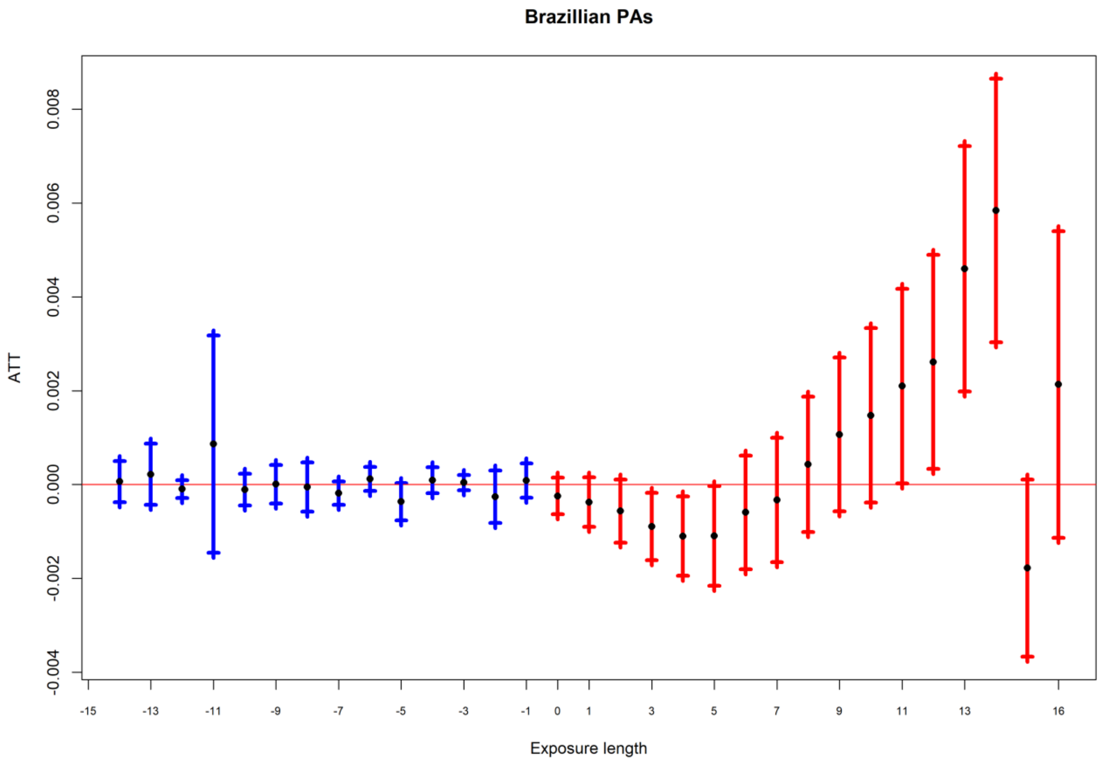


Figure A.2.3.6 Event Study for mining, Brazil-only sample without institutional variables, without critical groups



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

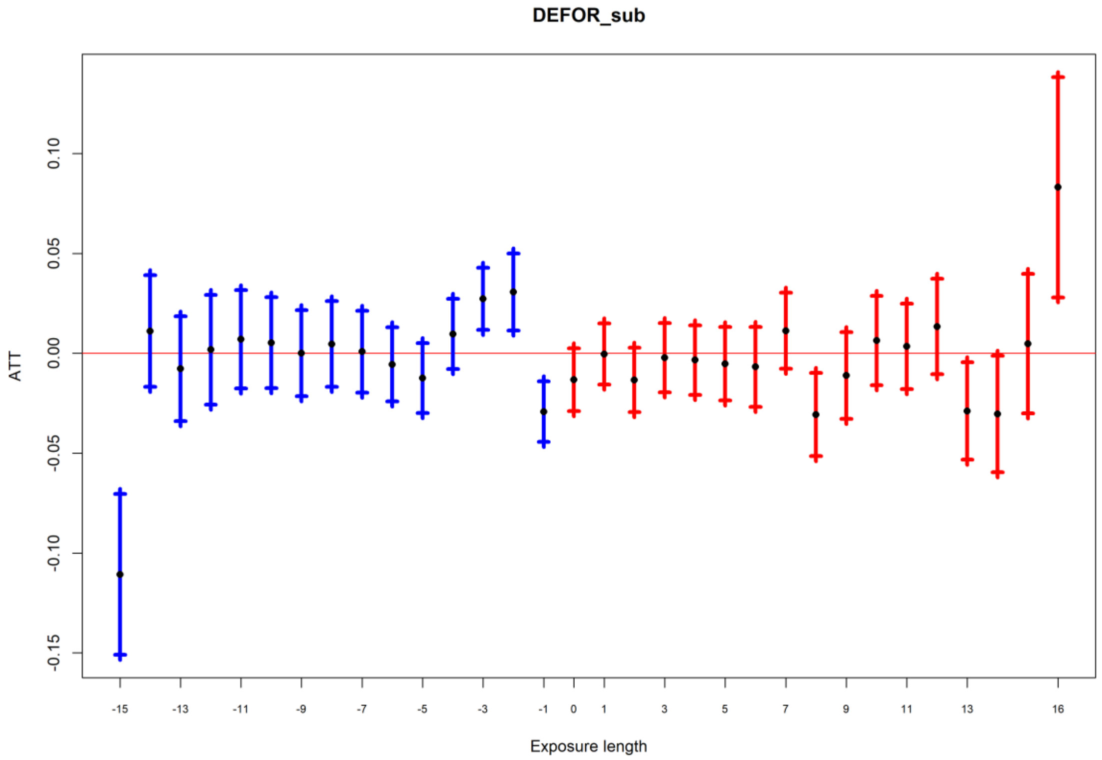


Figure A.2.4.2 Event Study for fires, Subnational conservation units, all groups

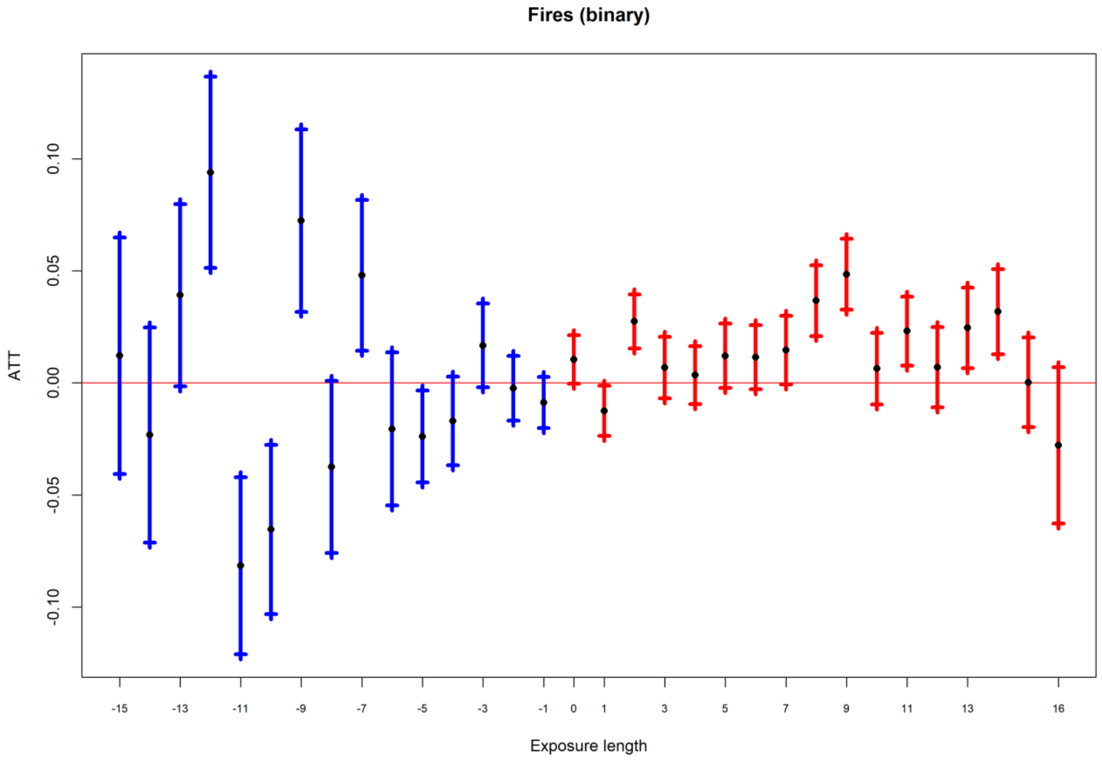
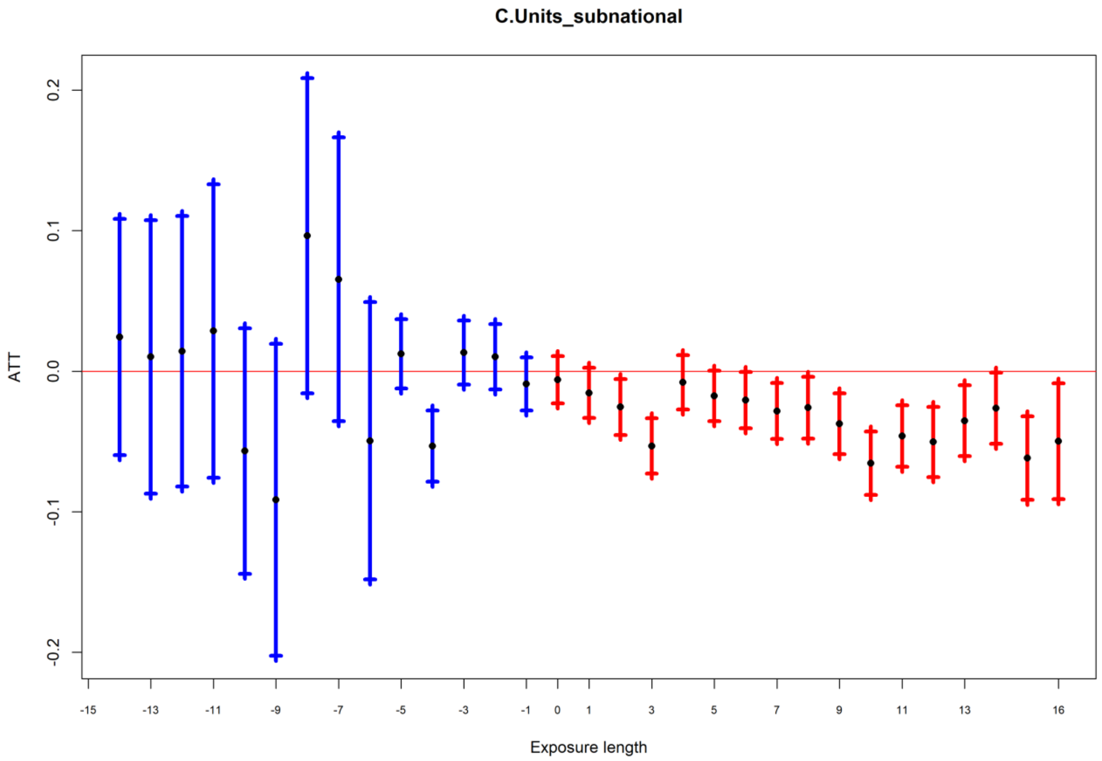


Figure A.2.4.3 Event Study for mining, Subnational conservation units, all groups



A.2.4.2 Without critical groups

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

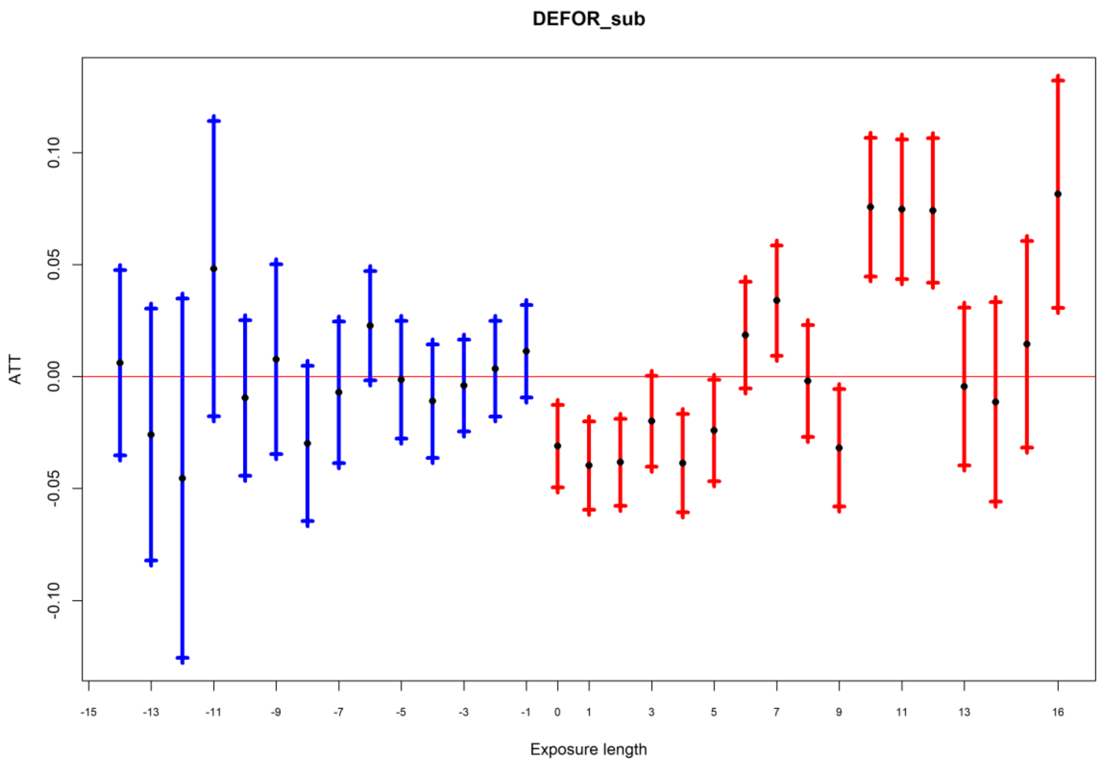


Figure A.2.4.5 Event Study for fires, Subnational conservation units, without critical groups

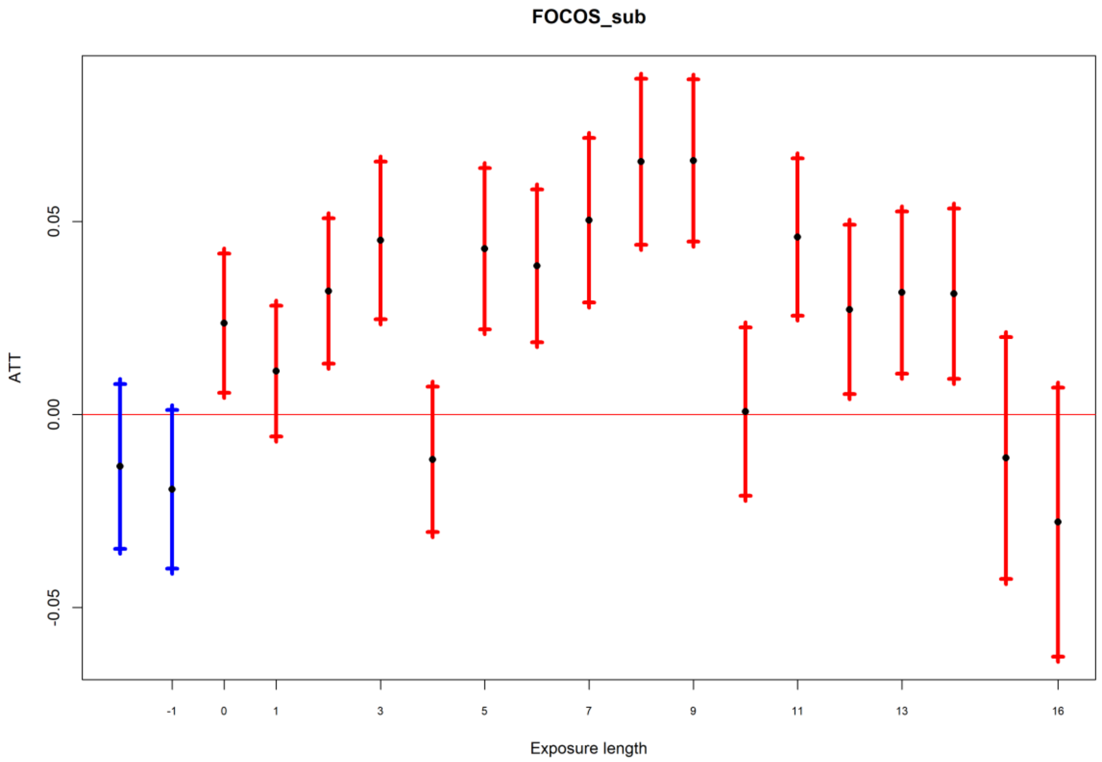
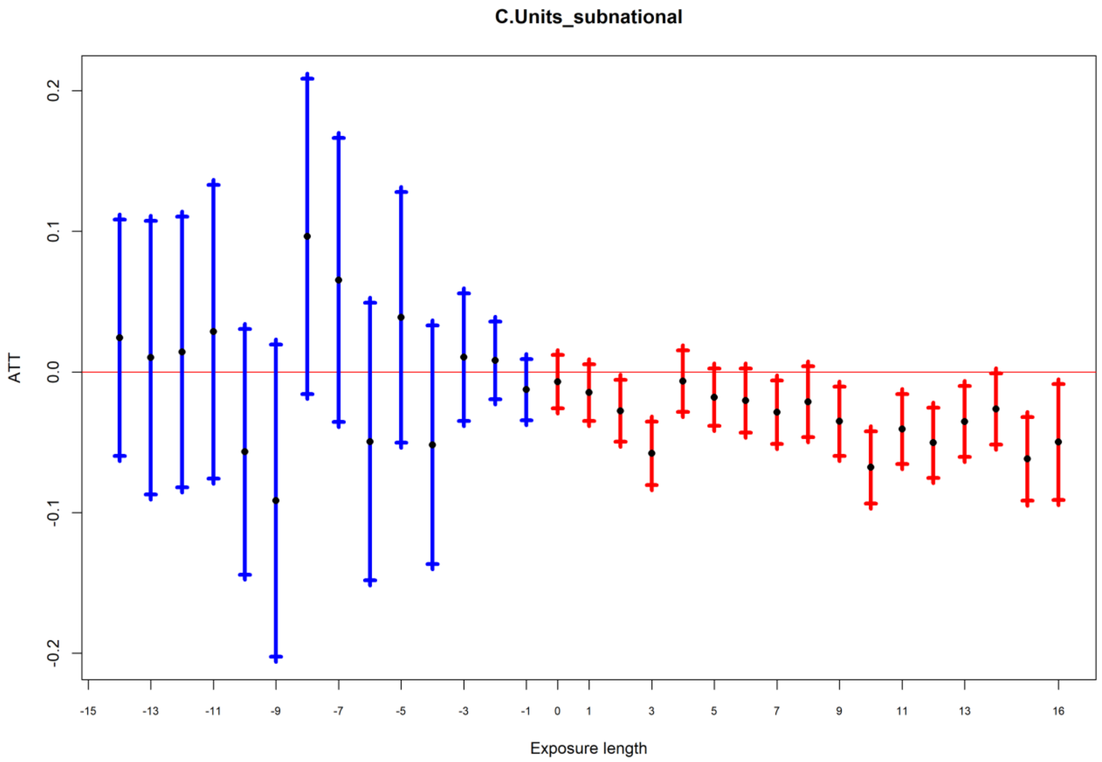


Figure A.2.4.6 Event Study for mining, Subnational conservation units, without critical groups



A.2.5 National conservation units

<u>A.2.5.1 All groups</u>

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

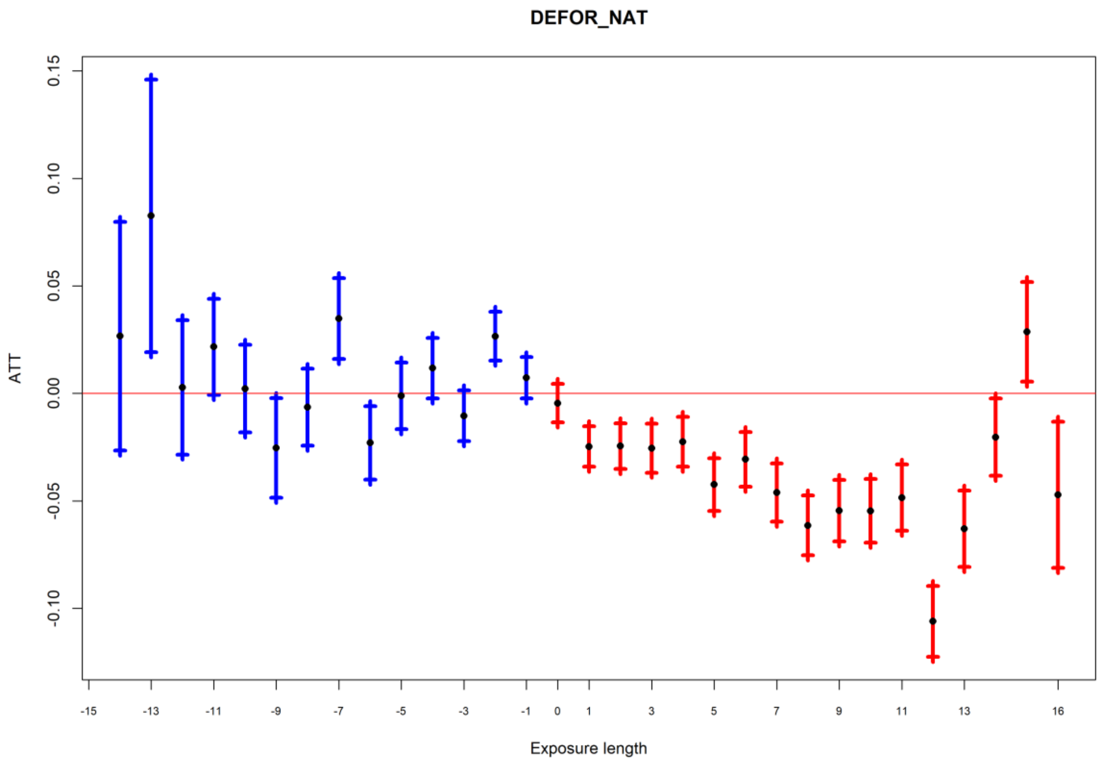


Figure A.2.5.2 Event Study for fires, National conservation units, all groups

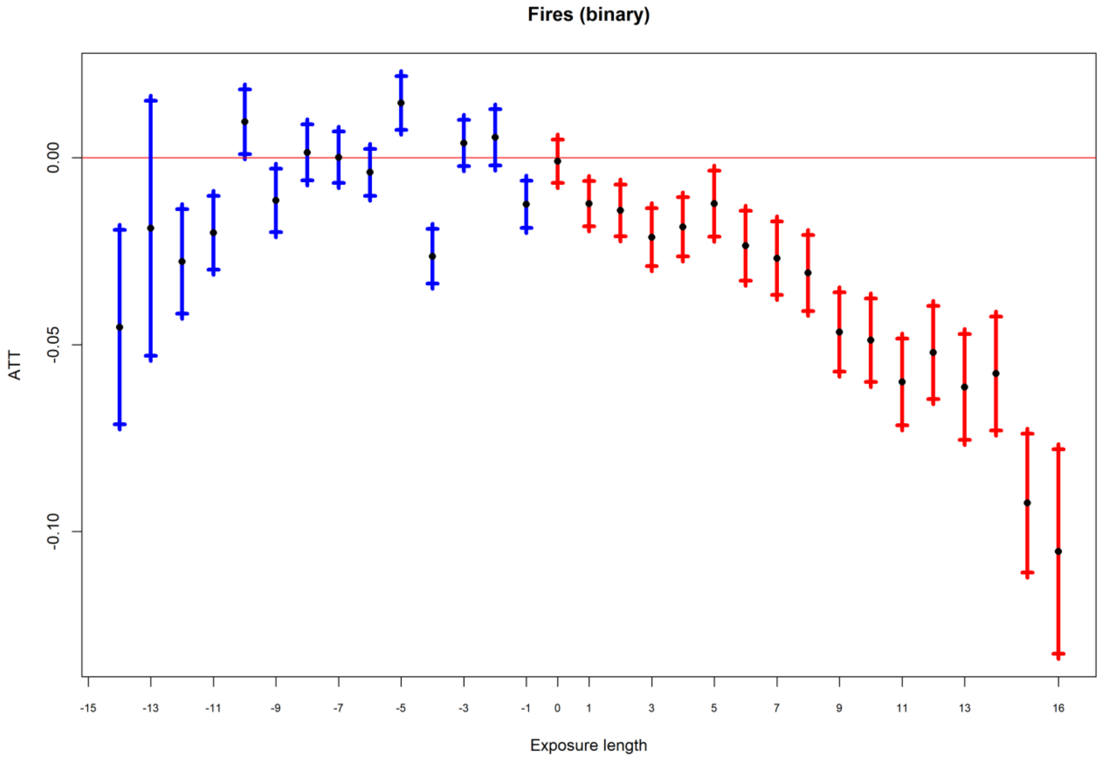
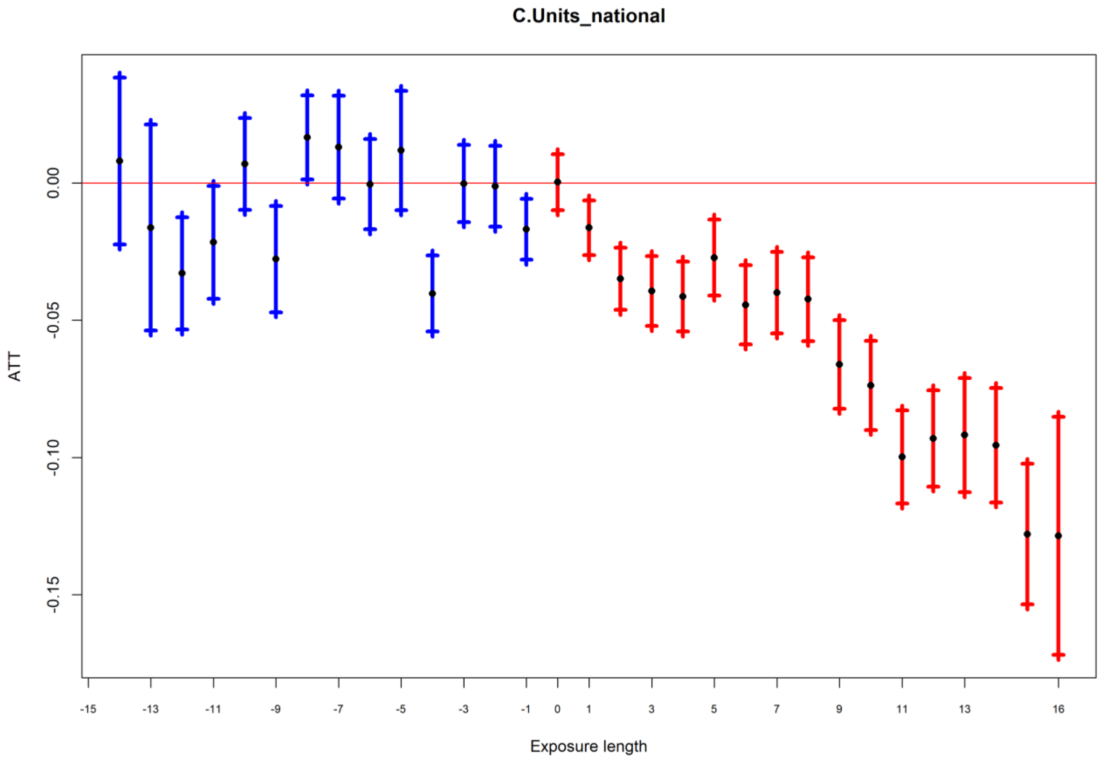
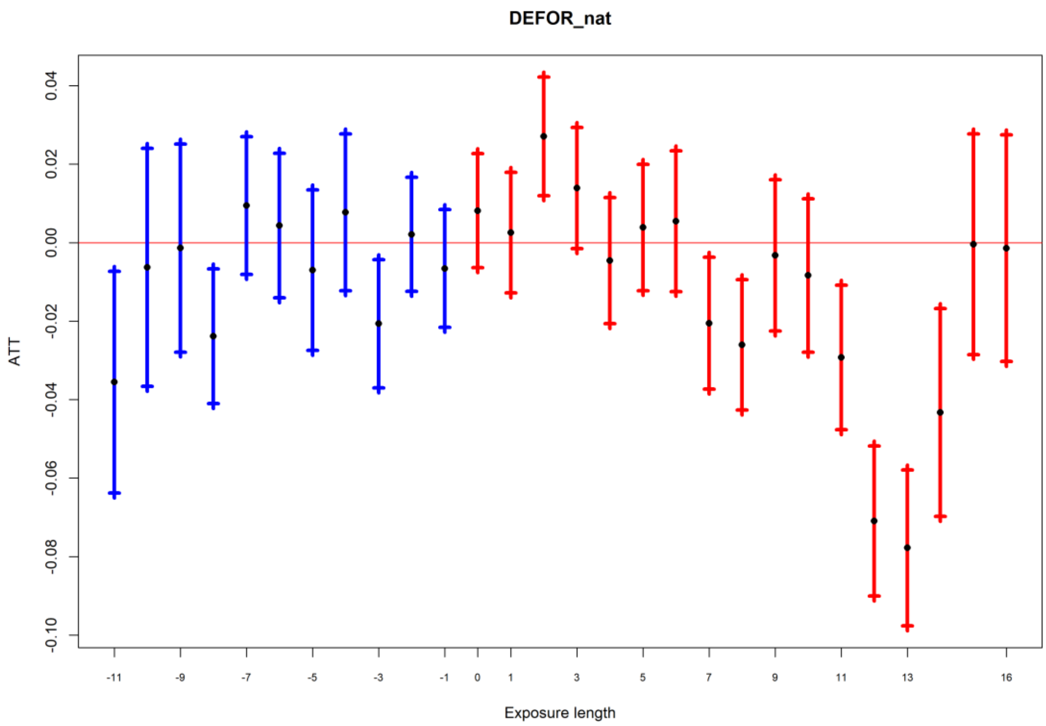


Figure A.2.5.3 Event Study for mining, National conservation units, all groups



A.2.5.2 Without critical groups

Figure A.2.5.4 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.2.5.5 Event Study for fires, National conservation units, without critical groups

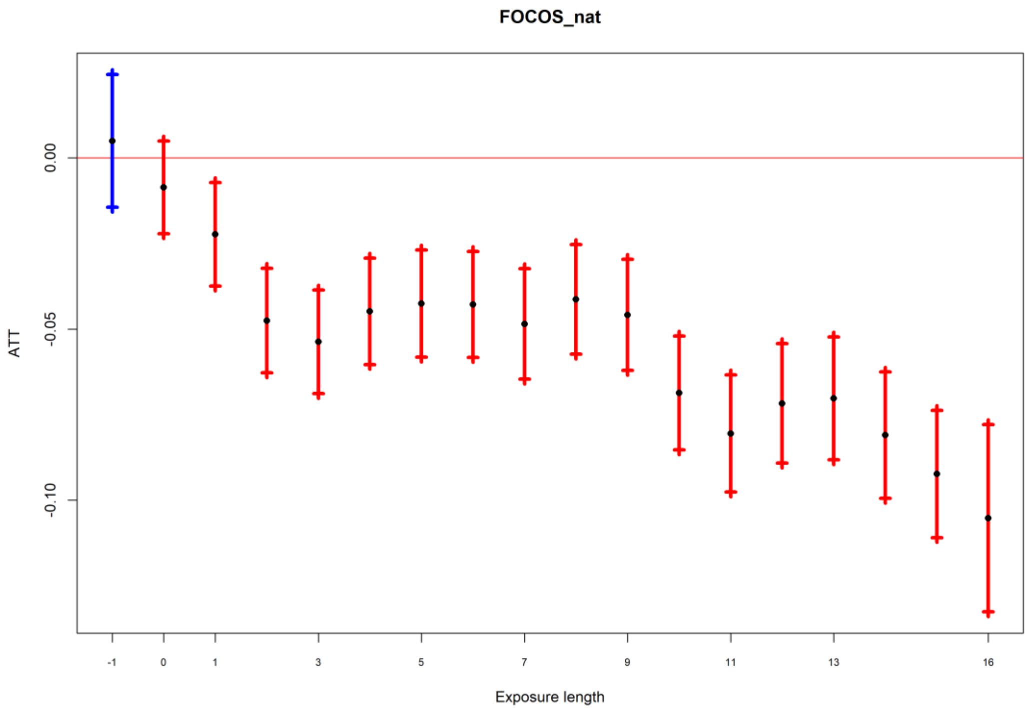
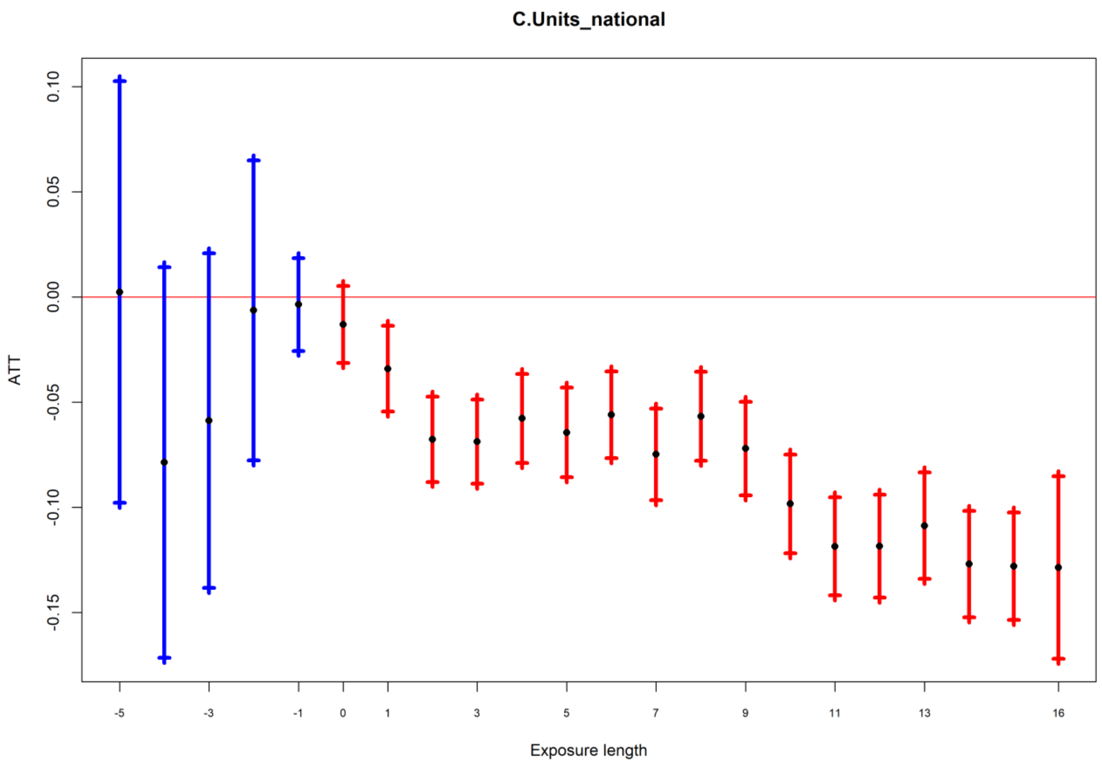


Figure A.2.5.6 Event Study for mining, National conservation units, without critical groups



A.2.6 Indigenous lands

A.2.6.1 All groups

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

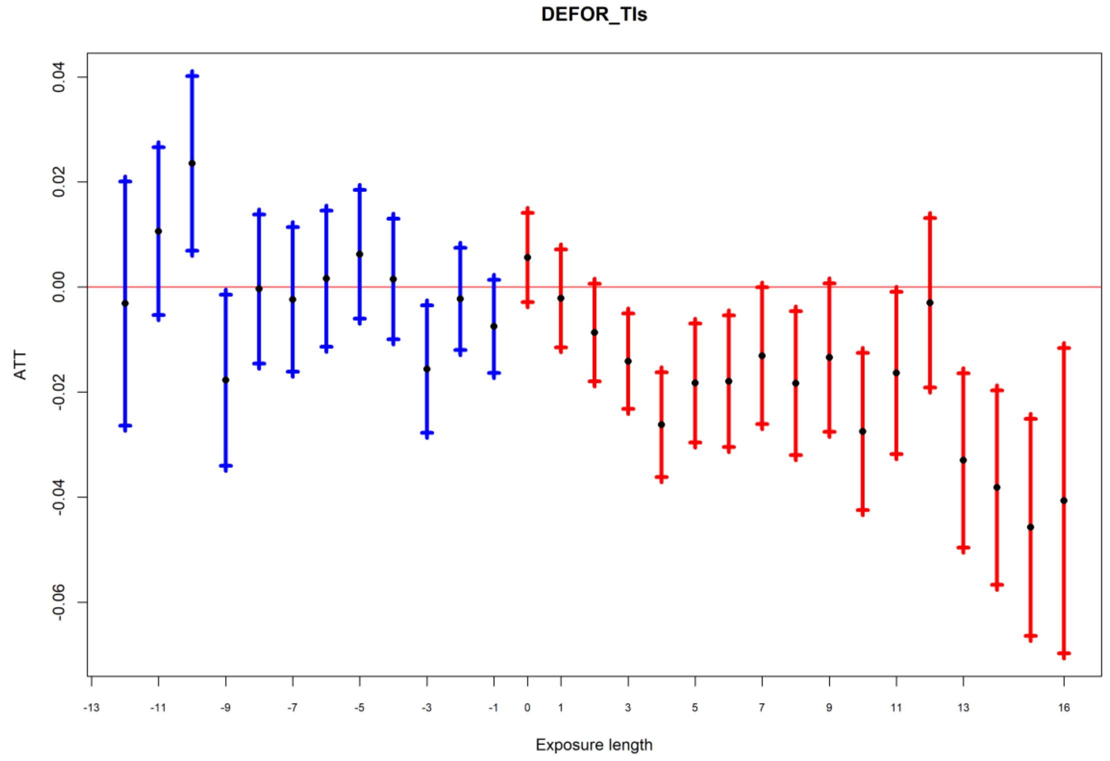


Figure A.2.6.2 Event Study for fires, Indigenous lands, all groups

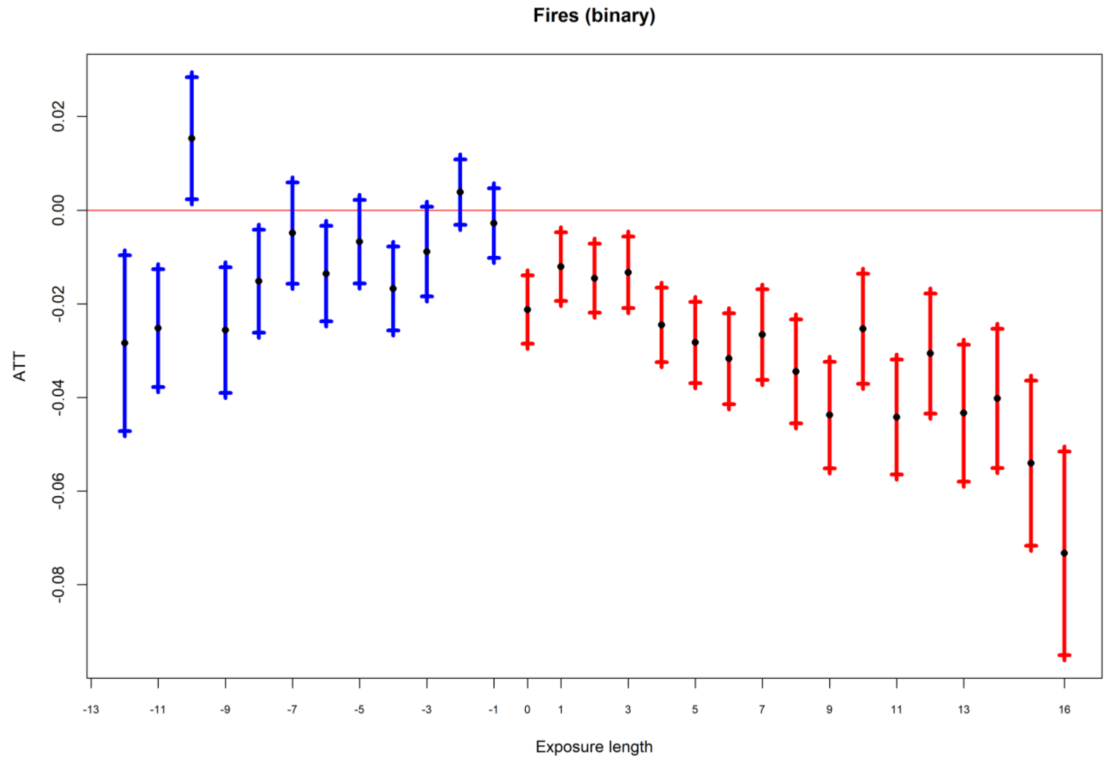
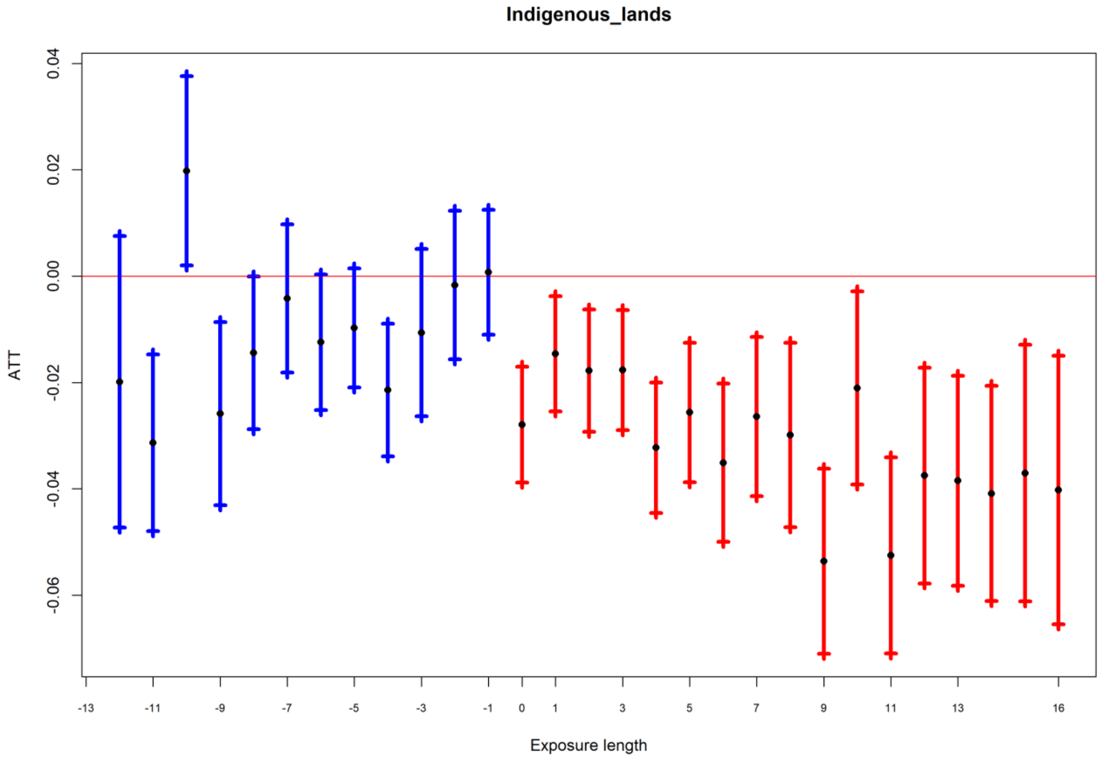


Figure A.2.6.3 Event Study for mining, Indigenous lands, all groups



A.2.6.2 Without critical groups

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

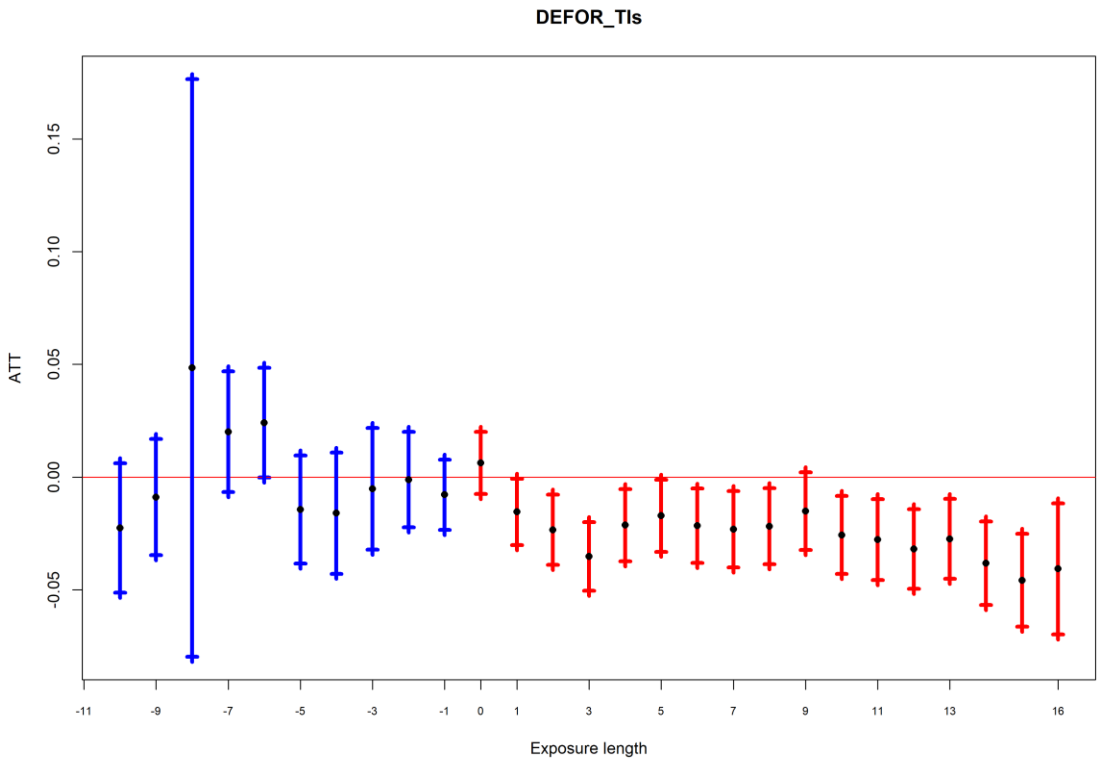


Figure A.2.6.5 Event Study for fires, Indigenous lands, without critical groups

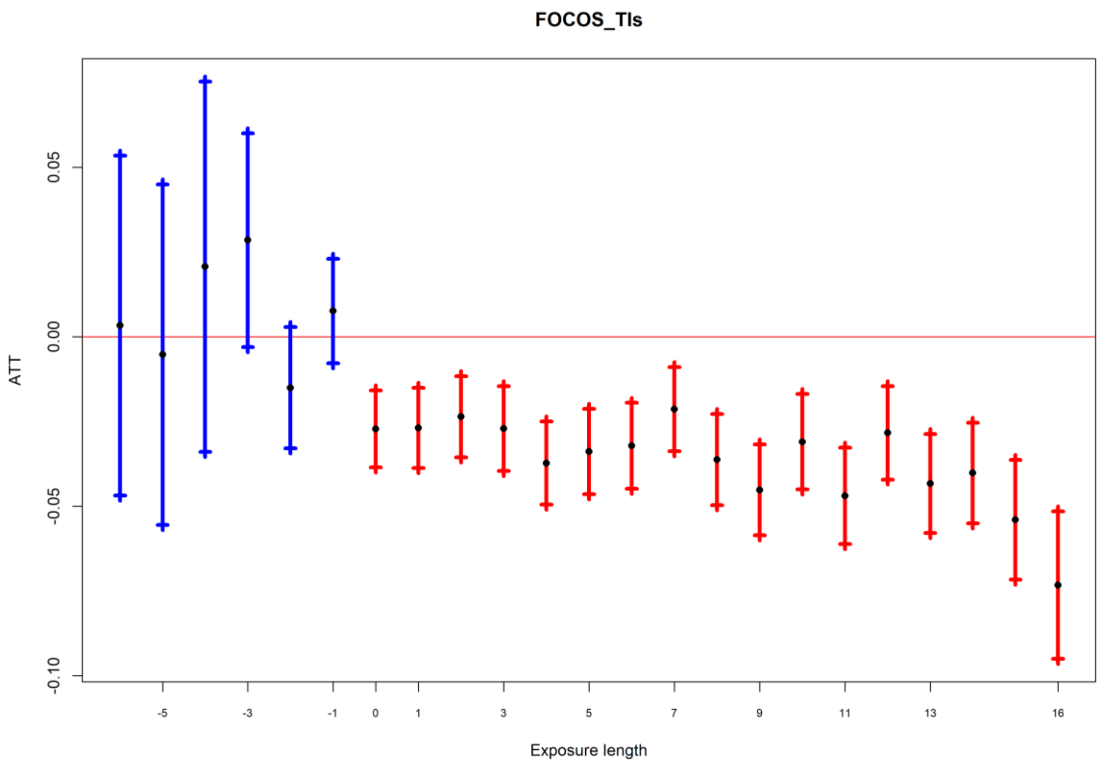
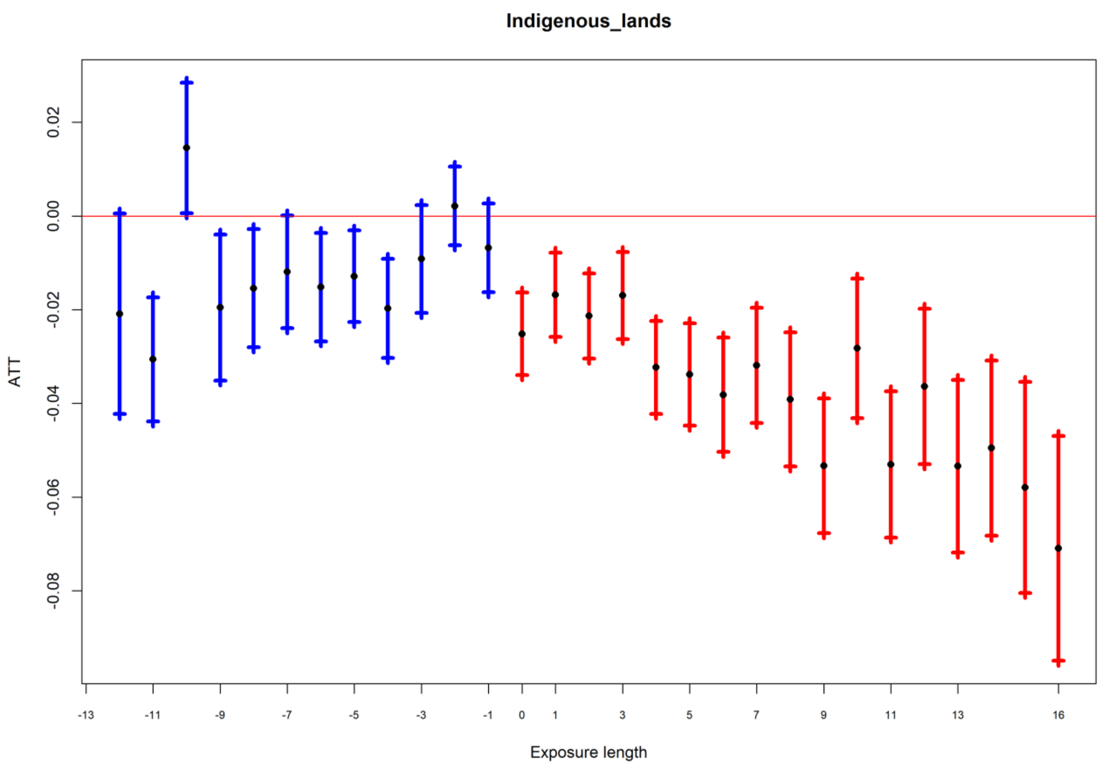


Figure A.2.6.6 Event Study for mining, Indigenous lands, without critical groups



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

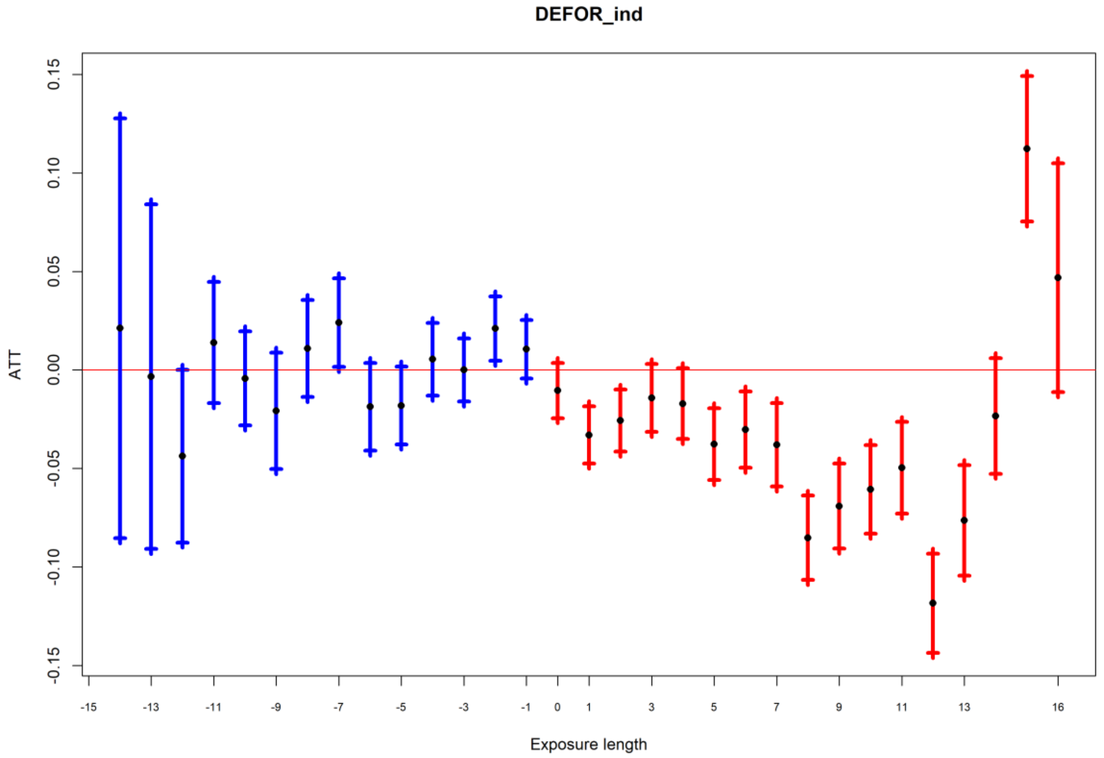


Figure A.2.7.2 Event Study for fires, indirect conservation units, all groups

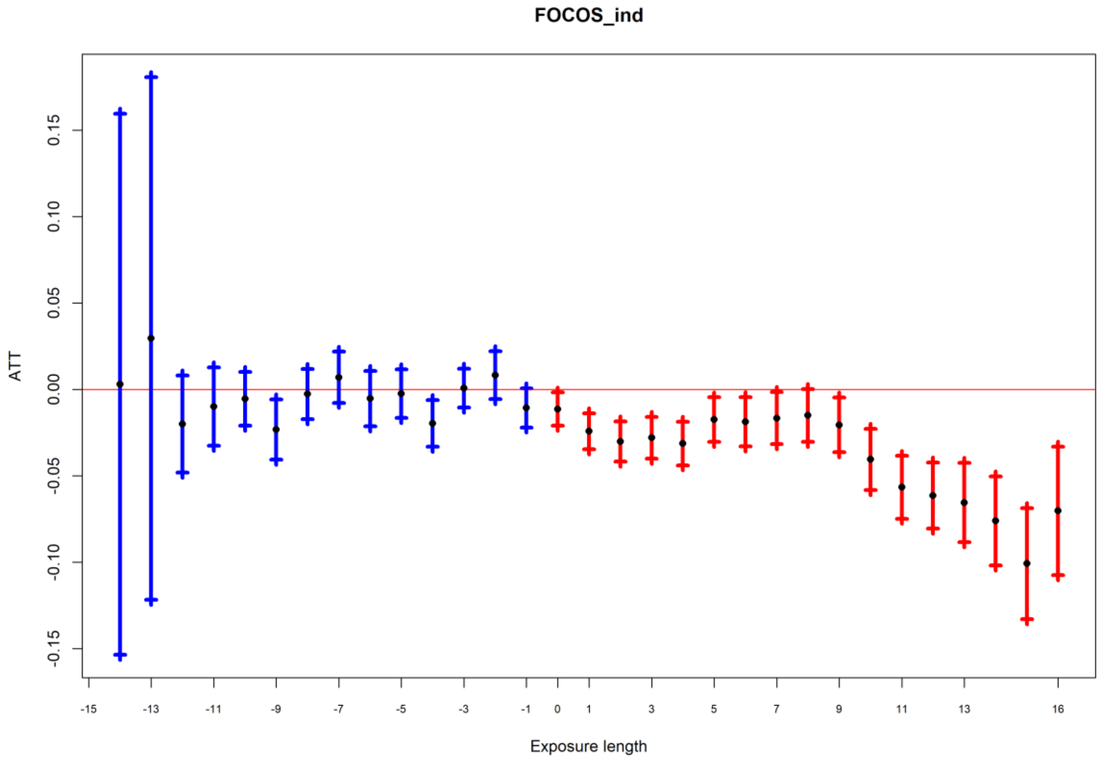
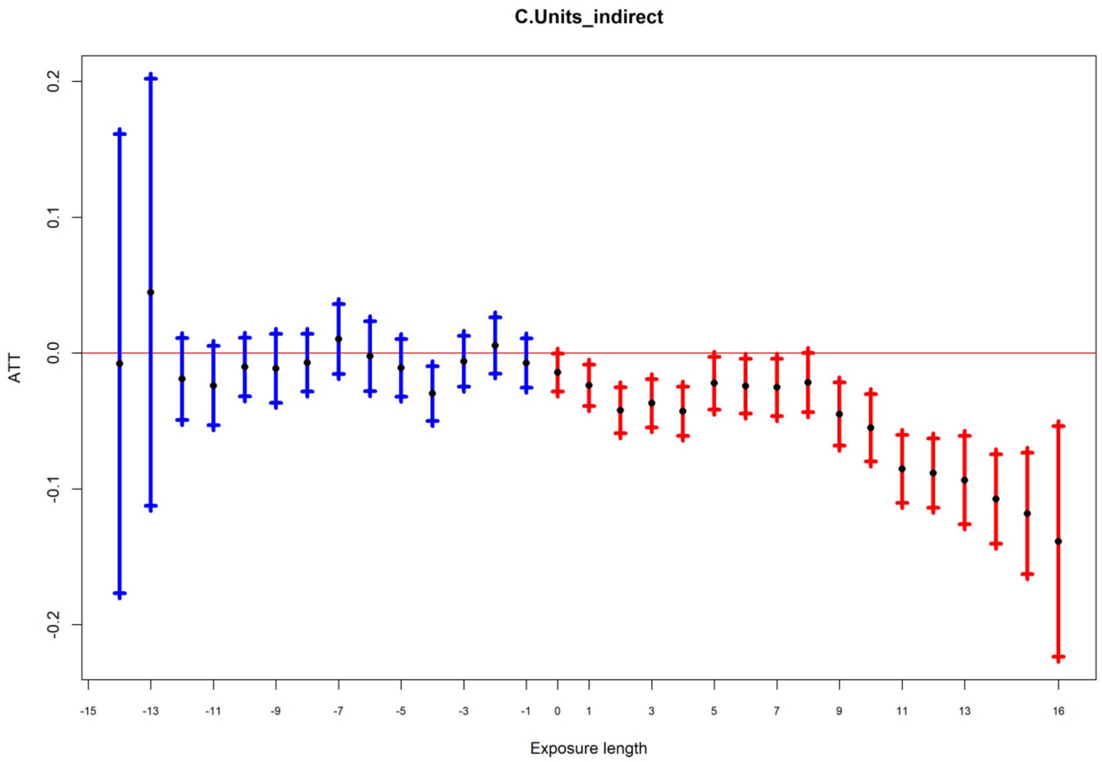


Figure A.2.7.3 Event Study for mining, indirect conservation units, all groups



A.2.7.2 Without critical groups

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

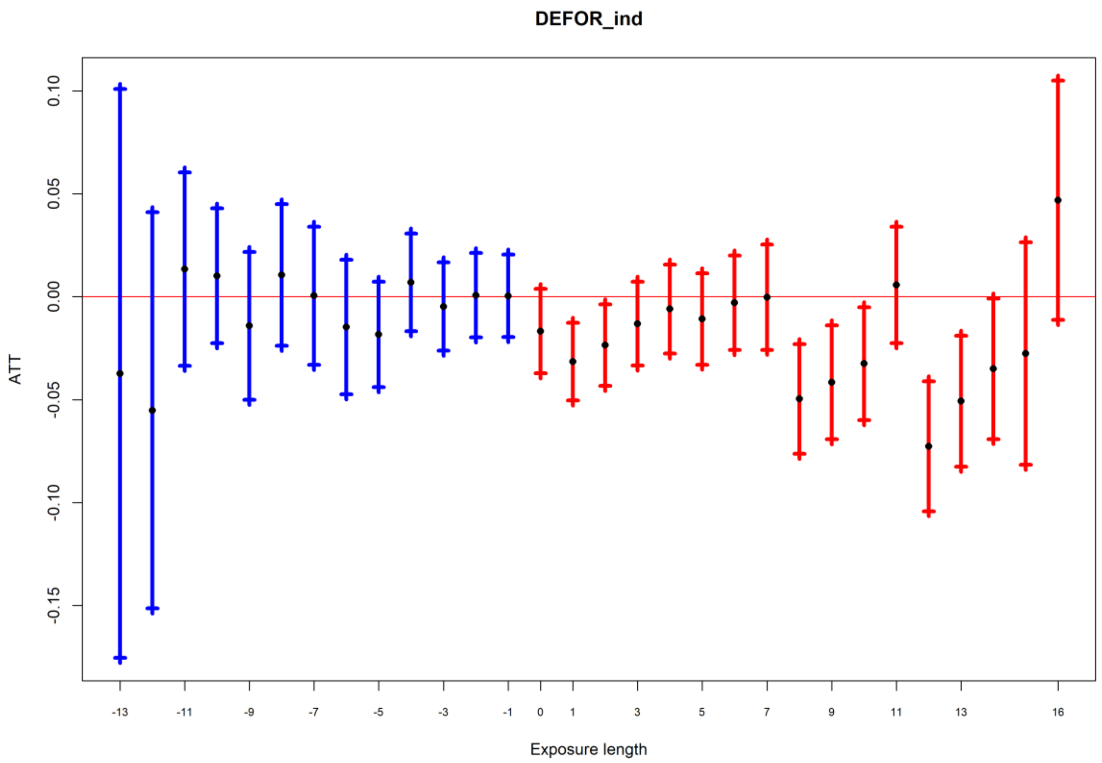


Figure A.2.7.5 Event Study for fires, indirect conservation units, without critical groups

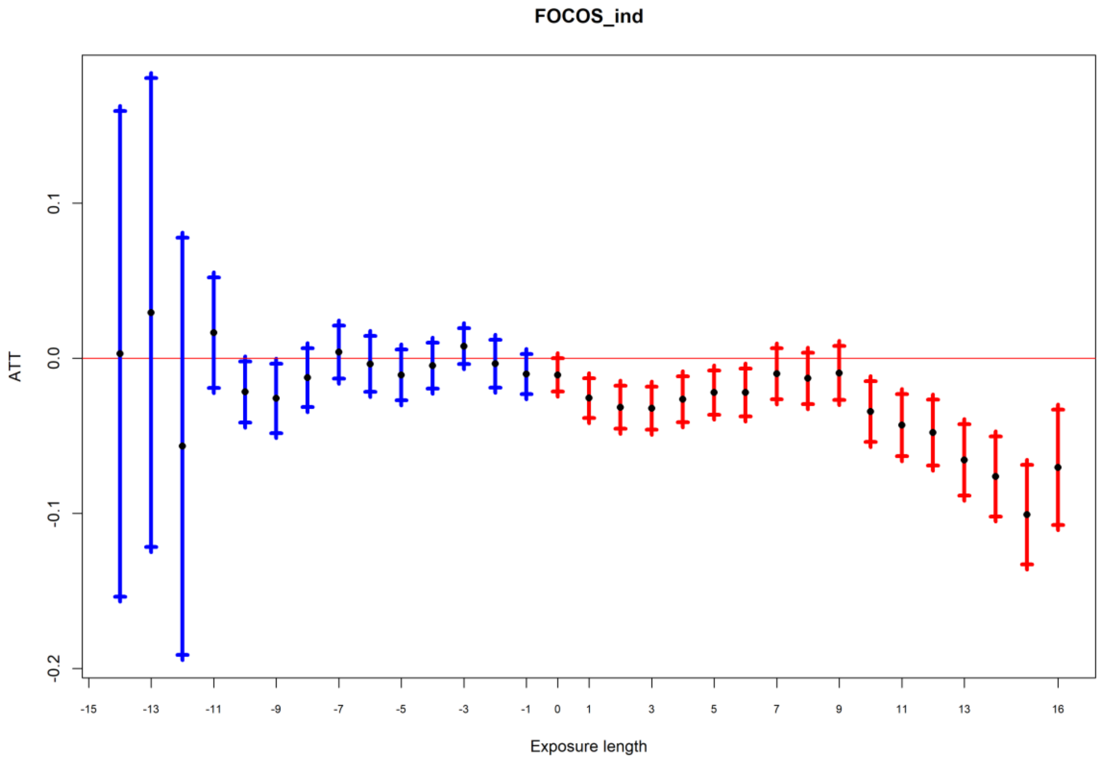
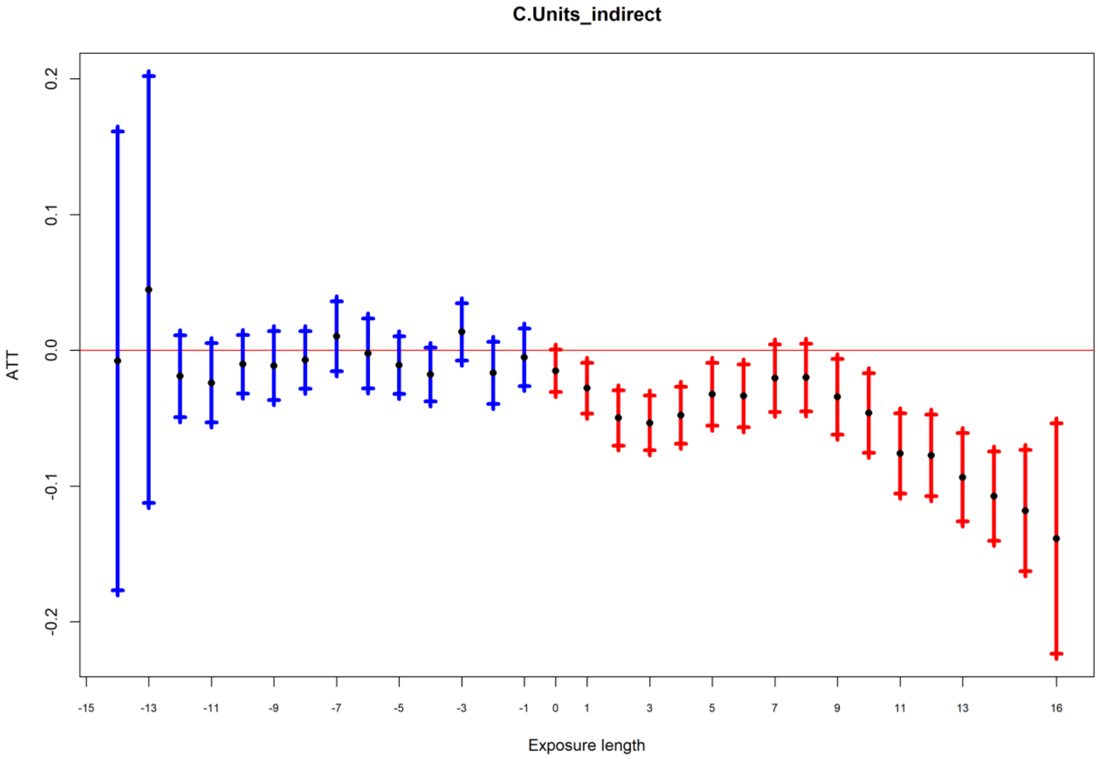


Figure A.2.7.6 Event Study for mining, indirect conservation units, without critical groups



A.2.8 Direct use conservation units

<u>A.2.8.1 All groups</u>

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

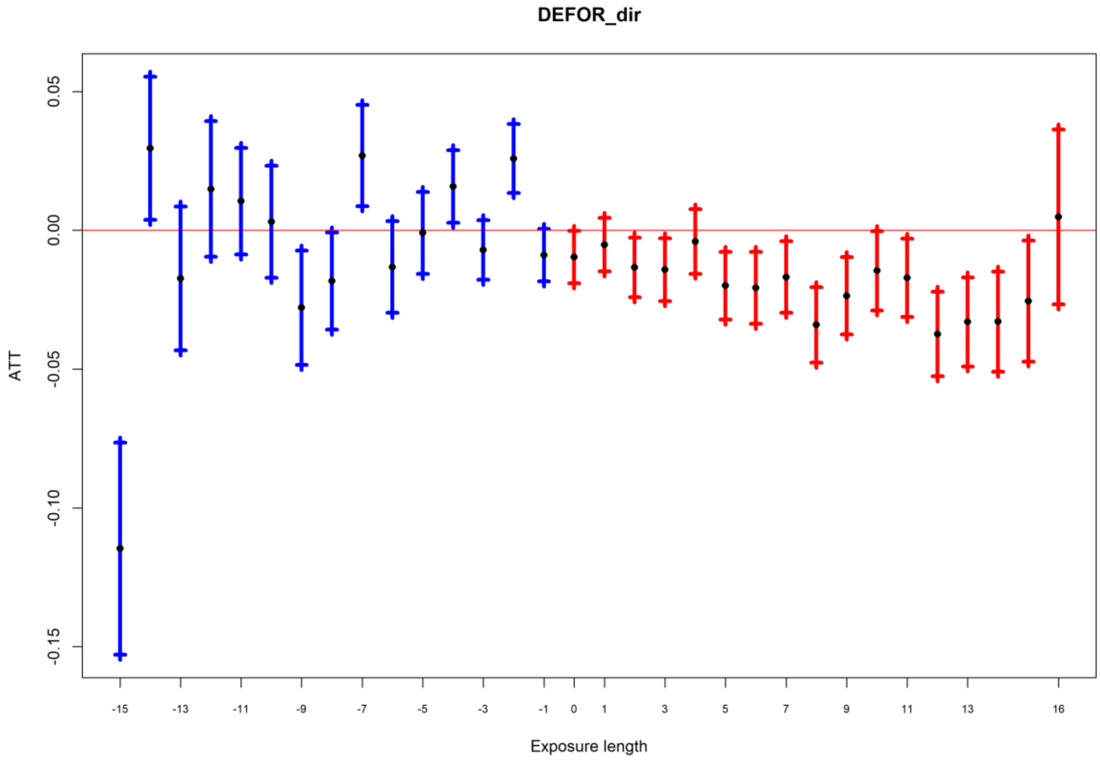


Figure A.2.8.2 Event Study for fires, indirect conservation units, all groups

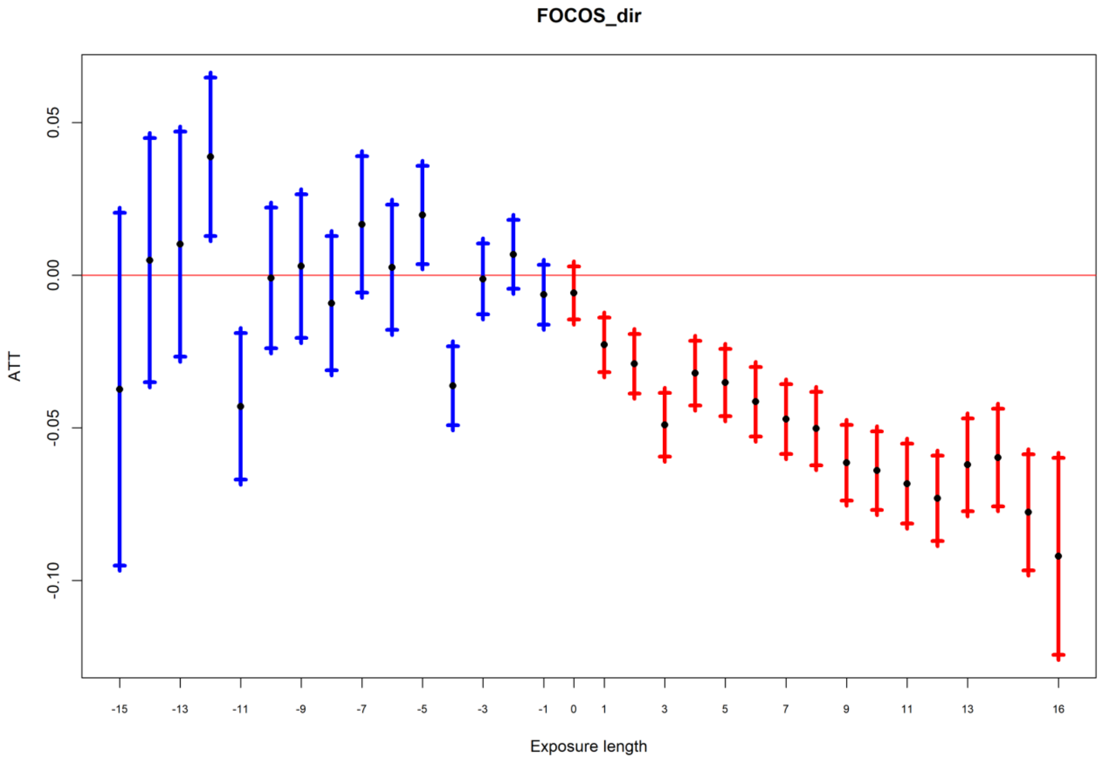
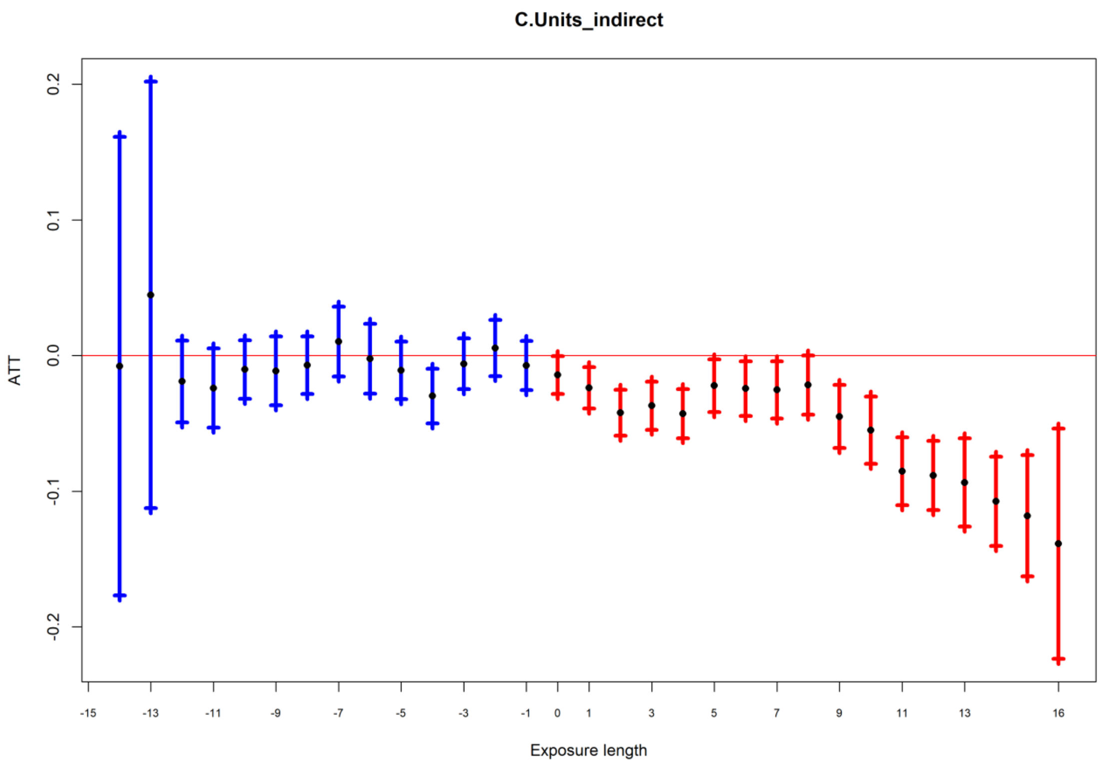


Figure A.2.8.3 Event Study for mining, indirect conservation units, all groups



A.2.8.2 Without critical groups

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

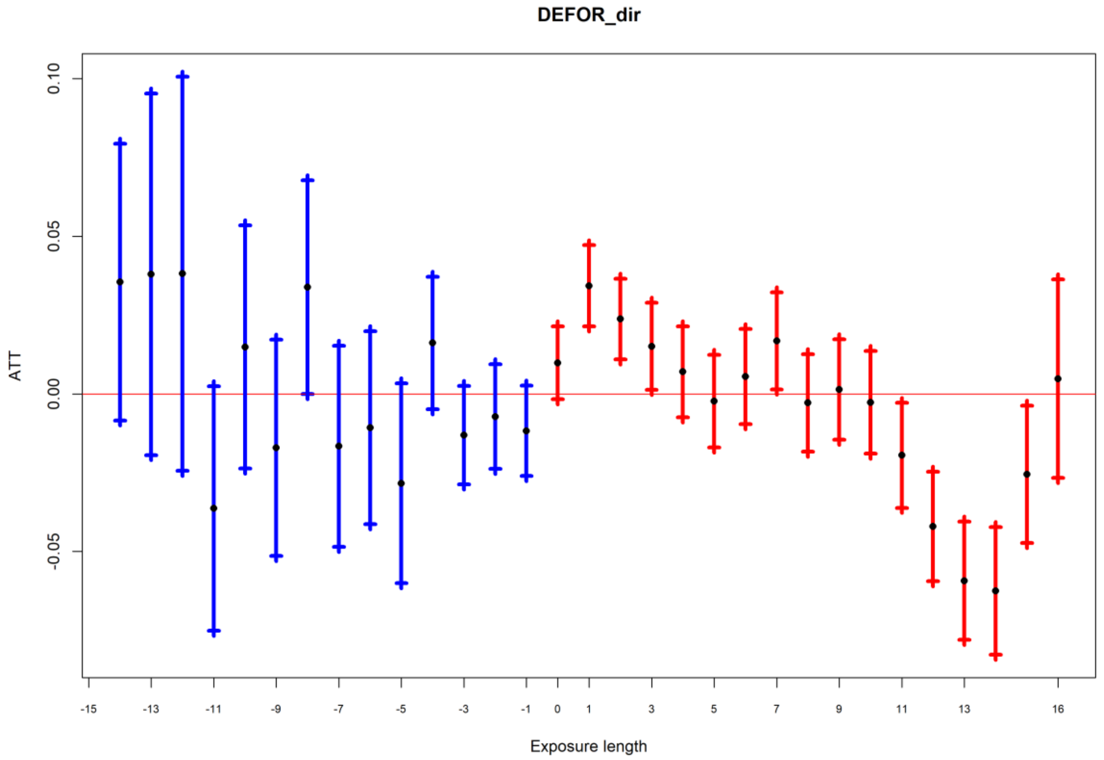


Figure A.2.8.5 Event Study for fires, direct conservation units, without critical groups

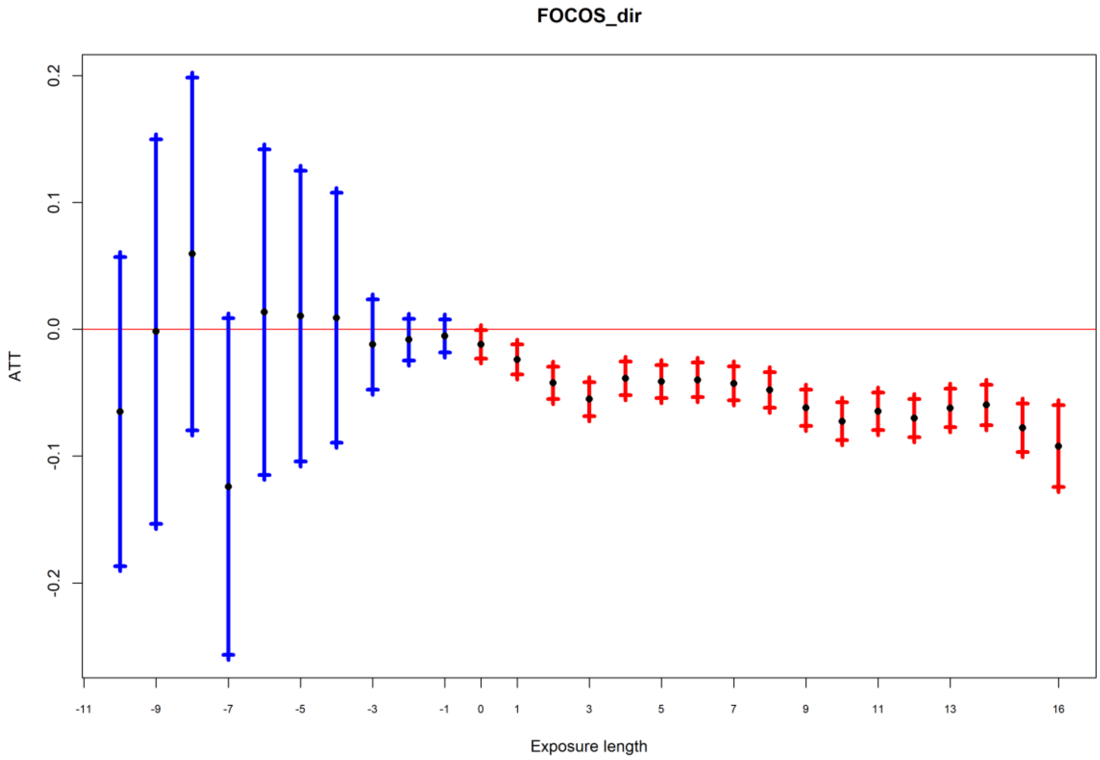
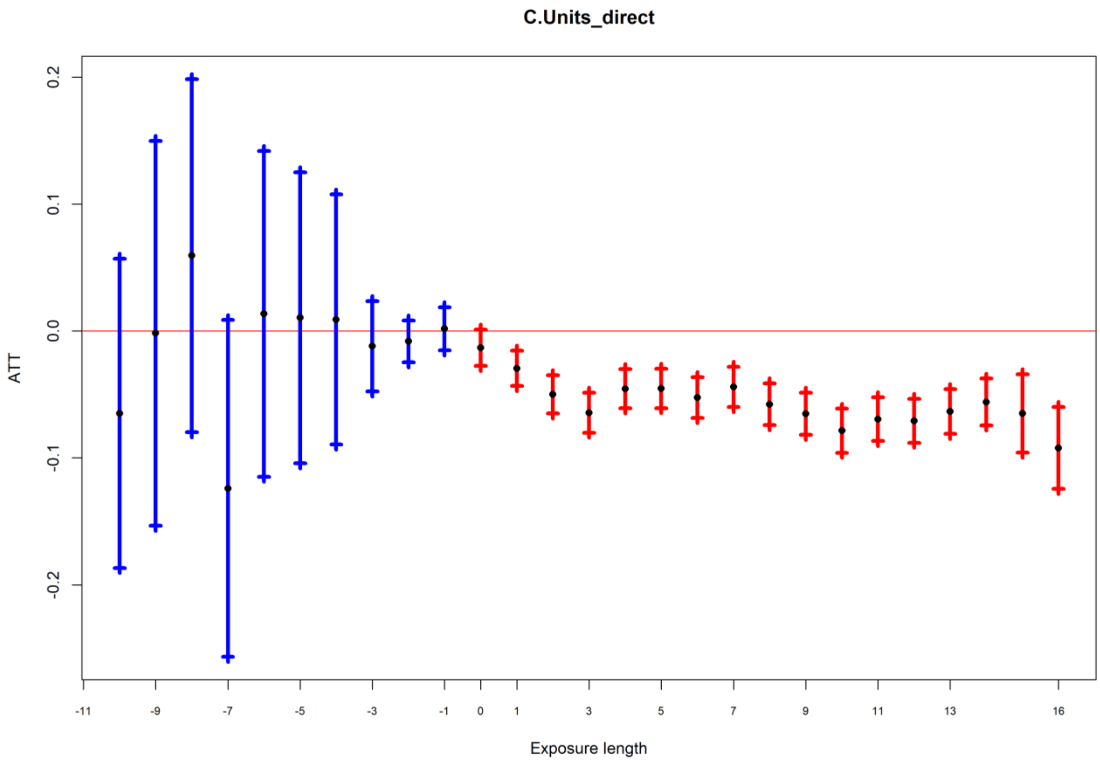


Figure A.2.8.6 Event Study for mining, direct conservation units, without critical groups



Appendix 3 Additional tables

Table A.3 Effect of PAs on mining, Brazilian PAs, alternative dependent variables (whole artisanal mining or gold mining) and subsamples (near gold reserves or not)

	Brazilian PAs	Brazilian PAs, Y = gold (only)	Brazilian PAs within 20km of gold reserves, Y = gold (only)
ATT	0.0005488	0.0005811	0.0950574
SE	[.0005432]	[.0005056]	[.1941719]
N	473,940	473,940	55,260
Clusters	26,330	26,330	3,070

Table A.4 Robustness test based on 50km and 100km internal and external buffers from PAs' boundaries: deforestation

	All PAs, 50 km buffered	All PAs instt, 50 km buffered	All PAs, 100 km buffered	All PAs instt, 100 km buffered
ATT	.0047424 ***	-0.0029307***	.0052005 ***	-.0030422 ***
SE	[.0001126]	[0.0001174]	[.0001014]	[0.000093]
N	1,488,731	990,848	1,703,583	1,174,506
Clusters	74,884	47,886	92,681	63,507

Table A.5 Robustness test based on 50km and 100km internal and external buffers from PAs' boundaries: fires

	All PAs, 50 km buffered	All PAs instt, 50 km buffered	All PAs, 100 km buffered	All PAs instt, 100 km buffered
ATT	-.013563 ***	-.025101***	-.0148688 ***	-0.0231495
SE	[.0028774]	[.0037408]	[.0024932]	[0.0031783]
N	1,559,166	990,848	1,789,979	1,254,632
Clusters	78,063	47,886	97,337	67,894

Appendix 4 The DSGE model

Table A.6 Parameters assumed in the simulations

Parameter	Name	Assumed level	Source
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_L	Gross return coefficient, low-quality land	0.5	Assumed by authors
δ_H	Gross return coefficient, high-quality land	1	Assumed by authors
Amax	Optimal accumulated area level	0.4	Assumed by authors
α_1	Coefficient of quantity in the deforestation right supply function	0.5	Assumed by authors
α_2	Coefficient of squared quantity in the deforestation right supply function	1	Assumed by authors

The dynamic system of the DSGE model is found below for i = L, H. It was simulated in Dynare®.

$$\mathrm{C}_t{}^{-\eta}\left(\mathrm{p}_{i,t}+\mathrm{m}_{i,t}\right)=\beta\mathrm{E}_0\left\{\mathrm{C}_{t+1}{}^{-\eta}\left(\frac{d}{d\mathrm{A}_{i,t+1}}\pi_i\left(\mathrm{A}_{i,t+1}\right)+\mathrm{p}_{i,t+1}+\mathrm{m}_{i,t+1}\right)\right\}\ (1)$$

$$\mathrm{A}_{i,t}=\mathrm{A}_{i,t-1}+\mathrm{D}_{i,t-1}\ (2)$$

$$\sum_{i=1}^{N}(\mathrm{p}_{i,t}+\mathrm{m}_{i,t}).\,\mathrm{D}_{i,t}+\mathrm{C}_t=\sum_{i=1}^{N}\pi_i\left(\mathrm{A}_{i,t}\right)\ (3)$$

$$\mathrm{D}_t^S=\frac{-\mathrm{a}_2+\sqrt{\mathrm{a}_2^2-4\mathrm{a}_1(\mathrm{a}_3-\mathrm{p}_t)}}{2\mathrm{a}_1}\ (4)$$

$$\pi_i\left(\mathrm{A}_{i,t}\right)=\delta_i\left(Amax.\,\mathrm{A}_{i,t}-\frac{\mathrm{A}_{i,t}{}^2}{2}\right)\ (5)$$

$$\frac{d}{d\mathrm{A}_{i,t}}\pi_i\left(\mathrm{A}_{i,t}\right)=\delta_i\left(Amax-\mathrm{A}_{i,t}\right)\ (6)$$

$$\log\left(\mathrm{m}_{i,t}\right)=\mathrm{u}_{i,t}\ (7)$$