Conservation impact evaluation using remotely sensed data

Alberto Garcia and Robert Heilmayr

October 1, 2021

Abstract

Conservation scientists are increasingly measuring the impacts of conservation interventions by applying quasiexperimental impact evaluation to remotely sensed panel data on land use change. However, these applications come with new challenges. Using Monte Carlo simulations and analytical proofs, we demonstrate that many of the panel econometric models employed for conservation impact evaluation are biased - the significance, magnitude and even direction of estimated effects from many studies are likely incorrect. These errors threaten to undermine the evidence base that underpins conservation policy adoption and design. We review this burgeoning literature and develop guidance for the design of econometric models quantifying conservation policy effectiveness.

1 Introduction

Policymakers often need to understand the causal impacts of conservation interventions. Do marine protected areas stop unsustainable harvesting of fish? Can payments for ecosystem services encourage lasting reforestation? While randomized experiments are the gold standard for the identification of causal relationships (Edwards et al. 2020; Jayachandran et al. 2017), conservation often poses questions that are prohibitively expensive, unethical or impossible to pursue through experimentation. In such settings, a growing portfolio of statistical techniques enable researchers to draw causal conclusions using observational data (Larsen, Meng, and Kendall 2019). Increasingly, these econometric approaches to impact evaluation are being used to disentangle the causal relationships that underpin conservation decisionmaking (Butsic, Lewis, et al. 2017; Baylis et al. 2016; Williams, Balmford, and Wilcove 2020). These econometric methods can generate estimates of an intervention's impact by comparing observed outcomes to a statistical counterfactual of what would have happened in the absence of an intervention (Ferraro 2009; Meyfroidt 2016; Ferraro, Sanchirico, and Smith 2019; Ribas et al. 2020).

Importantly, these econometric methods account for the non-random assignment of interventions that often confound identification of causal relationships in conservation. For example, a low rate of deforestation within a remote protected area may reflect the protected area's effectiveness, or it may be indicative of the remote location's poor suitability for agricultural development (Andam et al. 2008; Pfaff et al. 2009). To build valid counterfactuals in the face of non-random selection, researchers frequently build upon spatial, panel data to observe changes in outcomes across treated and control units after the adoption of an intervention (Blackman 2013). When a rigorous research design is applied to panel data, observational studies can yield conclusions that are comparable to what a researcher would discover if they were able to run a randomized experiment (Ferraro and Miranda 2017).

One important development that has enabled the proliferation of conservation impact evaluation is the increasing prevalence of remotely sensed datasets detailing conservation outcomes through time (Blackman 2013; Jones and Lewis 2015). For example, NASA's Landsat archive can be used to generate consistent information on land use spanning the entirety of the world since the 1970s (Hansen and Loveland 2012). As a result, a scientist hoping to quantify the impacts of a land use policy adopted decades ago can assemble data for treated and control units that span both pre- and post-implementation periods (Jain 2020).

However, many commonly used measures of land use change have structural differences from the data used

1

in traditional, linear panel models. For example, deforestation is often measured using data with a similar structure to the Global Forest Change product produced by Hansen et al. (2013). When converted to a panel structure, these data yield binary observations detailing the first year in which each 30 by 30m pixel was deforested. Importantly, the data do not allow for the detection of reforestation timing and, as a result, are unable to detect repeated deforestation events in the same location. To date, insufficient attention has been paid to how such binary, irreversible outcome data may affect the performance of standard econometric tools.

Here, we use a combination of analytical proofs and Monte Carlo simulations to demonstrate that many econometric analyses of deforestation are likely biased - significance, magnitude and even direction of estimated effects might be incorrect. While we focus on deforestation, many of our results apply to any setting with binary, irreversible data. The resulting biases arise even when researchers follow common guidance to adopt "rigorous" research designs with valid counterfactuals (Blackman 2013; Jones and Lewis 2015). Our main result shows that two-way fixed effects regressions with pixel unit fixed effects cannot identify the desired treatment effect parameter. Papers published in both conservation science and economics journals as recently as 2021 use this problematic specification to recover treatment effect estimates. To help guide future impact evaluations, we identify multiple ways in which this bias can be reduced or even eliminated. We then explore non-random selection that arises due to irreversibility in the deforestation setting and how this feature of the data may lead to bias. Finally, we reflect on the econometric benefits that emerge when researchers are able to match their model structure to the relevant scale of the deforestation process.

2 Measuring avoided deforestation using panel methods

We focus on the case in which a researcher would like to quantify the impact an intervention has had on deforestation. We assume that the intervention has clearly defined boundaries (e.g., a protected area, certified concession, or indigenous territory), and that the researcher has access to spatially explicit observations of forest cover and forest loss spanning the periods before and after the intervention was adopted. This general setting describes a broad array of studies that apply panel methods to remotely sensed data (Table 1).

Table 1: Econometric model structures used in avoided deforestation impact evaluations

Paper	Panel Method	Unit of analysis	Unit fixed effects level
Alix-Garcia and Gibbs 2017	TWFE	binary point/pixel	pixel
Alix-Garcia et al 2018	TWFE	binary point/pixel	pixel
Anderson et al. 2018	matched DID	binary point/pixel	NA
Araujo et al. 2009	TWFE using instrument	state	state
Arriagada et al. 2012	matched DID	farm	NA
Baehr et al. 2021	TWFE	binary pixel/grid cell	pixel
Baylis et al. 2012	DID	grid cell	NA
BenYishay et al. 2017	TWFE	grid cell	grid cell
Blackman 2015	matched unit FE model	binary point/pixel	county
Blackman and Villalobos 2020	matched TWFE	forest magagement unit	forest magagement unit
Blackman et al. 2017	TWFE	community	community
Blackman et al. 2018	matched TWFE	forest magagement unit	forest magagement unit
Busch et al. 2015	matched TWFE	grid cell	grid cell
Bustic et al. 2017	TWFE	binary point/pixel	pixel
Carlson et al. 2018	matched TWFE	plantation	plantation
Heilmayr and Lambin 2016	matched DID	property	NA
Herrera et al. 2019	matched regression	binary point/pixel	NA
Holland et al. 2017	matched TWFE	landowner parcel	landowner parcel
Jones and Lewis 2015 (1)	matched TWFE	binary point/pixel	pixel
-2	matched TWFE	household parcel	household parcel
Jones et al. 2017	matched TWFE	household	household
Koch et al. 2018	matched DID	municipality	NA
Nolte et al. 2017	DID	NA	deforestation
Panlasigui et al. 2018	TWFE	binary point/pixel	pixel
Pfaff 1999	regression	county	NA
Sanchez-Azofeifa et al 2007	regression	grid cells	NA
Shah and Baylis 2015	DID	grid cell	NA
Sims and Alix-Garcia 2017	TWFE	locality	locality
Tabor et al. 2017	TWFE	fokontany	fokontany
Wendland et al. 2015	matched TWFE	binary point/pixel	pixel

In each of the studies detailed in Table 1, the researcher's goal is to measure the impact that a specific policy had on deforestation within treated units, also known as the *average treatment effect on the treated (ATT)*. The *ATT* estimates the difference between the average deforestation rate of treated units with treatment, and the average deforestation rate of treated units without treatment. The fundamental challenge is that, for every treated unit, the researcher is unable to observe the value that the outcome would have taken in the absence of treatment (Holland 1986). In our case, this means that the researcher cannot observe the deforestation that would have occurred in treated units had they not received treatment. Figure 1 displays this problem in the context of a simulated conservation intervention that reduced deforestation rates in treated areas — the landscape is depicted as observed by the researcher at the end of the observation period, including the unobservable counterfactual of what would have happened if the conservation intervention had not been adopted. Note that in untreated areas, there is no counterfactual deforestation, since no intervention ever took place.
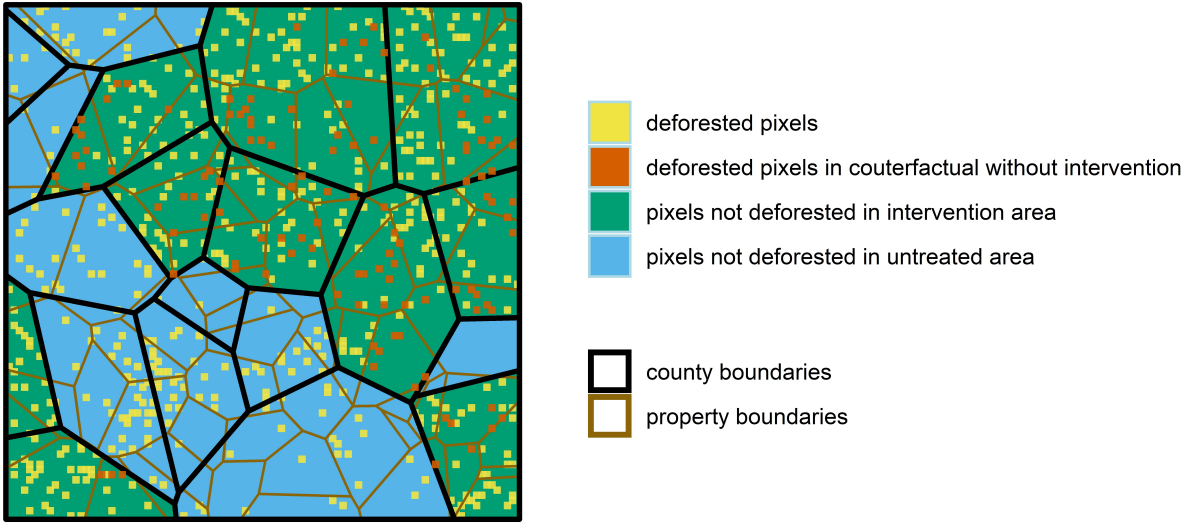

Figure 1: A map of a simulated landscape depicting patterns of deforestation under an effective conservation intervention, as well as counterfactual deforestation illustrating what would have happened in the absence of the intervention.

2.1 Modeling the decision to deforest

We model deforestation (y_{ivt}) as a binary choice by a landowner to clear a small plot of land i within their larger property v at time t. The decision to deforest depends upon a latent variable (y^*_{ivt}) that represents the returns from the plot of land in its cleared state $(V^{cleared}_{ivt})$ relative to the returns from its forested state $(V^{uncleared}_{ivt})$, such that:

$$y^*_{ivt} = V^{cleared}_{ivt} - V^{uncleared}_{ivt} \tag{1}$$

$$y_{ivt} = \begin{cases} 1 & \text{if } y^*_{ivt} > 0 \\ 0 & \text{otherwise} \end{cases} \tag{2}$$

This generic clearing rule underpins a broad class of more specific static and dynamic models that have been used to explore the determinants of deforestation (e.g., Pfaff 1999; Kerr, Pfaff, and Sanchez 2003; Pfaff and Sanchez-Azofeifa 2004).

However, this basic model makes an assumption that the decision to deforest is reversible. In reality, a number of characteristics of both the process of deforestation, as well as the methods used to detect deforestation in individual plots, complicate this assumption. First, the goal of many conservation interventions is to prevent the loss of mature forests that may take decades, if not centuries, to regrow. In such cases, deforestation itself may be considered irreversible in human time scales, focusing the researchers' attention upon the first instance in which a plot is deforested. Even when deforestation of secondary forests is an object of interest, constraints imposed by remotely sensed datasets may force empirical researchers to treat deforestation as irreversible. Gradual processes of reforestation are inherently harder to identify than abrupt losses of forest cover (Hansen et al. 2013). In addition, determining the precise year in which the extended process of forest regrowth began is currently an active area of research for the remote sensing community, and often requires many years of post-regrowth observations. As a result, commonly used deforestation datasets such as the Global Forest Change product often only identify the first year in which a pixel was cleared (Hansen et al. 2013). Whether desired, or due to technical limitations, the resulting inability to observe repeated deforestation means that deforestation is, in effect, an irreversible process in most conservation impact evaluations In response to this irreversibility, Jones and Lewis (2015) and Alix-Garcia and Gibbs (2017) have suggested that deforested pixels should be dropped in the periods after they are first cleared. We follow this guidance, further modifying our binary deforestation variable:

$$
y_{ivt}^o = \begin{cases} 1 & y_{ivt}^* > 0 \text{ and } y_{iv\tau}^* \leq 0 \text{ for all } \tau < t \\ 0 & y_{ivt}^* \leq 0 \text{ and } y_{iv\tau}^* \leq 0 \text{ for all } \tau < t \\ NAN & otherwise \end{cases} \tag{3}
$$

2.2 Estimating the ATT: difference-in-differences and two-way fixed effects estimators

The remainder of our paper focuses on two methods commonly used to estimate the ATT in conservation intervention settings with panel data: Difference-in-Differences (DID) and Two-way Fixed Effects (TWFE) regression models (Blackman 2013; Jones and Lewis 2015). Our parameter of interest, the ATT, is the average effect of the conservation intervention on treated pixels. Let $y_{ivt}(1)$ and $y_{ivt}(0)$ denote the potential outcomes of pixel i in property v in time t with and without the treatment, respectively. In addition, let t_0 denote the year that the intervention was implemented and let D_i represent a dummy indicating whether pixel i is ever treated. The ATT can now be expressed at:

$$
ATT = E[y_{ivt}(1) - y_{ivt}(0)|t \geq t_0, D_i = 1]
$$

The DID and TWFE methods have become so popular in part, because the researcher does not need random assignment of treatment to generate convincing estimates of a program's impact on avoided deforestation. Instead, the researcher must make a common trends assumption, under which we evaluate both methods.

Assumption 1: (Common trends)

$$
E[y_{ivt}(0)|t \geq t_0, D_i = 1] - E[y_{ivt}(0)|t < t_0, D_i = 1] = E[y_{ivt}(0)|t \geq t_0, D_i = 0] - E[y_{ivt}(0)|t < t_0, D_i = 0]
$$

Assumption 1 requires that deforestation rates in the intervention area and untreated units would have experienced the same average change in the outcome (trend) in the absence of the intervention. Put another way, this means that pixels in treated and untreated areas would have experienced the same change in their probability of deforestation across the two periods had no intervention ocurred. While fundamentally untestable, ensuring that deforestation rates in the intervention area and the control area followed parallel trajectories prior to the date of the intervention can give creedence to this assumption (Butsic, Lewis, et al. 2017).

We also evaluate the DID and TWFE models under the following stable unit treatment value assumption (SUTVA)

Assumption 2: (SUTVA)

$$\forall d \in \{0,1\}: \text{ if } D_i = d \text{ and } t \geq t_0, \text{ then } y_{ivt}(d) = y_{ivt}$$

Assumption 2 requires that the potential outcomes for pixel i, $y_{ivt}(1)$ and $y_{ivt}(0)$, do not depend on the treatment status of any other pixel. There also cannot exist unobserved versions of treatment that may affect the potential outcomes.

The typical DID regression model includes a dummy variable equal to one for units in the treatment group, a dummy variable equal to one for observations in the period after the intervention, and their interaction.

Regression 1: (DID regression) Let β_{DID} denote the coefficient of the interaction between D_i and an indicator for whether the intervention has been implemented in time t, $\mathbb{1}\{t \geq t_0\}$, in the following (population) OLS regression:

$$y_{ivt}^o = \alpha_0 + \alpha_1 D_i + \alpha_2 \mathbb{1}\{t \geq t_0\} + \beta_{DID} \text{ x } D_i \mathbb{1}\{t \geq t_0\} + \epsilon_{it} \tag{4}$$

Conceptually, the DID estimator calculates the treatment effect as the difference between the differences of the treated and untreated observations before and after treatment (Butsic, Lewis, et al. 2017).

$$\beta_{DID} = E[y_{ivt}^o | t \geq t_0, D_i = 1] - E[y_{ivt}^o | t < t_0, D_i = 1] - (E[y_{ivt}^o | t \geq t_0, D_i = 0] - E[y_{ivt}^o | t < t_0, D_i = 0])$$

When the y_{ivt}^os are i.i.d., it is straightforward to show under Assumption 1 and 2,

$$\beta_{DID} = ATT$$

Often, however, the researcher wants to estimate the ATT in a setting that does not fit the two-group, two-period case covered by DID models. In such cases, TWFE regressions are frequently used to apply DID methods to multiple groups or treatment periods. This amounts to estimating a regression that controls for unit and time fixed effects, which control for unobservable confounding variables that may vary across units or through time.

Regression 2: (TWFE regression) Let β_{TWFE} denote the coefficient of the interaction between D_i and $\mathbb{1}\{t \geq t_0\}$ in the following (population) OLS regression:

$$y_{ivt}^o = \alpha + \beta_{TWFE} \text{ x } D_i \mathbb{1}\{t \geq t_0\} + \lambda_t + \gamma_i + \epsilon_{it} \tag{5}$$

Here λ_t and γ_i represent the year and pixel fixed effects, respectively.

In the case of two groups and two time periods, the TWFE regression typically gives an estimate equivalent to that of the DID model (Wooldridge 2010). With this in mind, many researchers, including those in the conservation impact evaluation literature, have used the TWFE model as a "generalized DID" that can be estimated in settings in which units undergo treatment in more than two distinct time periods (Table 1). For example, a researcher may use a TWFE regression model to examine the effectiveness of a network of protected areas where the protected areas were created at different times, or a payment for ecosystem services (PES) program that enrolled properties in annual cohorts.

3 Pixel level, TWFE models fail to estimate the ATT

Despite widespread use of pixel level analyses of deforestation, the application of TWFE models to a binary, irreversible process such as deforestation yields a biased estimate of the ATT. Specifically, the proof contained in Appendix 10.1 shows that, in the two-group, two-period case, the coefficient of interest from the TWFE model (β_{TWFE}) estimates the post-treatment difference in outcomes (single difference), rather than the desired ATT:

$$\beta_{TWFE} = ATT + \underbrace{E[y_{it}(0)|t < t_0, D_i = 1] - E[y_{it}(0)|t < t_0, D_i = 0]}_{\text{pre-treatment difference in deforestation rates}}$$

Regression 2 thus forgoes the benefits that panel methods provide and, if the treated area has a different baseline deforestation rate than the control, will represent a biased estimate of the intervention's impact. Many conservation interventions are specifically targeted towards locations with either low opportunity costs for conservation, or high threats of conversion. As a result, it is likely that many conservation impact evaluations will have treatment and control units that experienced different pre-treatment deforestation rates. It is important to note that this bias could even lead to changes in the estimated treatment effect's sign, in addition to errors in the effect's magnitude and significance.

3.1 Alternative construction of y_{ivt}^o cannot recover ATT

Although dropping previously deforested pixels from the panel introduces bias into the TWFE estimate of the ATT, keeping observations in the panel after initial deforestation (i.e. using y^{ivt} *as the outcome variable in place of y^o{ivt}*) introduces its own problems. The ATT as defined in Section 2.2 is an estimate of the impact of an intervention on the frequency of deforestation events (i.e. the decision to clear). Keeping the deforested pixel in the panel beyond the first period in which it was observed as deforested would incorrectly imply that it has actively been deforested in each subsequent time period, when in fact, no new deforestation event or clearing decision has occurred. This is intuitively problematic, because the deforestation rate in each period would be monotonically increasing by construction, which is not necessarily the case. We show that pixel-level regression models do not drop deforested pixels in subsequent periods do indeed incur severe bias if used to estimate the ATT as it is defined here (Appendix 10.2).

Alternately, some researchers opt to keep deforested pixels in the panel, and choose to estimate an intervention's impact on deforested area, rather than on deforestation rates. However, when reforestation timing is unaccounted for, as is the case with most data products using a binary measure of deforestation, this is not possible. Rather than measuring deforested area at any given time, this outcome variable measures the stock of ever-deforested area through the current time period. It is unclear that this is a relevant parameter of interest in many cases and should not be interpreted as either a deforestation rate or the deforested area in a given time period. See (Appendix 10.2) for further discussion on this point.

4 Monte Carlo simulations to compare alternative model performance

The rapid growth of the conservation impact evaluation literature has resulted in a diversity of alternate model structures that could yield more accurate estimates of the effectiveness of different interventions (Table 1). To explore the relative performance of these different models, we employ a series of Monte Carlo simulations. Specifically, we randomly generate synthetic landscapes with known policy effectiveness and analyze the performance of different econometric models in estimating the policy's known impact.

4.1 Data generating process

Each of our simulated landscapes consists of administrative units that are either untreated ($D = 0$) or are assigned to a conservation treatment ($D = 1$). We observe deforestation in two, even-length periods, a pre-treatment ($t < t_0$) and a post-treatment ($t \geq t_0$) period.

We follow Equation (1) and model these binary deforestation events as a function of each pixel's unobservable value along the continous, latent variable (y_{ivt}^*) indicating the return to clearing pixel i, in property v, in year t:

$$y_{ivt}^* = V_{ivt}^{cleared} - V_{ivt}^{uncleared}$$
$$= \beta_0 + \beta_1 D_i + \beta_{2,0}(1 - D_i)\mathbb{1}\{t \geq t_0\} + (\beta_{2,1} + \beta_3)D_i\mathbb{1}\{t \geq t_0\} + \alpha_i + u_{it} + \rho_v$$

That is, the returns to deforestation evolve over the two time periods ($\mathbb{1}\{t \geq t_0\}$), and differ across the control ($D_i = 0$) and treated pixels ($D_i = 1$). In addition, we assume that the value of deforestation is influenced by time-invariant random disturbances at the scale of individual pixels ($\alpha_i \sim N(0, \sigma_a^2)$) or properties

$(\rho_v \sim N(0, \sigma_p^2))$, as well as time-varying, pixel-scale disturbances $(u_{it} \sim N(0, \sigma_u^2))$. These disturbances can represent a variety of spatial and temporal processes including, for example, the biophysical characteristics of a location, or the preferences of a property owner.

4.2   Assumed parameter values

For the remainder of the paper, we explore a guiding example that has been parameterized to represent an impactful intervention in a high deforestation setting. Conservation interventions often have annual treatment effects smaller than a 1 percentage point reduction in the annual deforestation rate (e.g. Robalino and Pfaff 2013; Jones, Holland, Naughton-Treves, et al. 2017). These modest reductions in the annual deforestation rate, however, can amount to large landscape-scale effects. For example, Alix-Garcia et al. (2018) find that environmental land registration in Brazil's Amazonian states of Mato Grosso and Para reduced the annual deforestation rate by an average of 0.5 percentage points, which has resulted in an overall reduction in deforestation of 10%.

The initial simulated landscape has the following characteristics: a pre-treatment deforestation rate of 2% in the control area; a pre-treatment deforestation rate of 5% in the intervention area; a decrease in the deforestation rate of 0.5 percentage points between the first and second period in the absence of treatment; and an average reduction of 1 percentage point in the deforestation rate in treated units due to the intervention ($ATT = -0.01$). We assume that $\sigma_u = 0.5$. Finally, we begin by assuming away time invariant pixel ($\sigma_a = 0$) and property-level disturbances ($\sigma_p = 0$) but relax this assumption in Section 6.1.1. Note that Assumptions 1 and 2 are satisfied by construction. The derivations detailing the mapping from the landscape characteristics to the corresponding parameters in y_{ivt}^* can be found in Appendix 10.3.

4.3   Evaluation criteria

We compare econometric models using a combination of estimate bias, root mean squared error (RMSE), and coverage probability. Using our Monte Carlo simulations, we calculate estimate bias as the difference between each model's mean estimate of the ATT and the known ATT parameter. RMSE describes the distribution of estimates around the ATT. Coverage probability is defined as the proportion of simulations in which the true ATT lies within the simulation's 95% confidence interval (CI). As such, we would expect the ATT to lie within this CI 95% of the time, however, factors such as the bias of the estimates, their distribution, and treatment of standard errors may impact coverage.

5   Alternative specifications can yield unbiased estimates of the ATT

TWFE models have risen to prominence due to their flexibility in applying DID methods to settings with multiple groups and variation in treatment timing. However, TWFE models with pixel fixed effects are not a viable approach to estimate the ATT in deforestation impact evaluations. Column 1 of Figure 2 shows the bias associated with Regression 2. In our guiding example, the ex-post single difference is 0.02 (the ATT plus the post-treatment group difference in deforestation rates), when the true ATT is equal to -0.01. This means that a positive bias of 0.03 results from the use of this regression model. However, we show that multiple alternate model specifications enable researchers to generate unbiased estimates of the ATT. We describe three straightforward solutions to this challenge below.

5.1   Traditional difference-in-differences model

In the two-group, two-period case the typical DID model (Regression 1) is an unbiased estimator of the ATT, as shown in columns 2-5 of Figure 2 (The typical DID is equivalent to including treatment fixed effects). However, researchers often want to use TWFE models because of their flexibility in situations that do not fall under the typical DID setup. Therefore, researchers should be aware of the trade-offs using aggregated units of analysis and fixed effects when using TWFE models for deforestation impact evaluation.

5.2 Spatially aggregated fixed effects

One can also use fixed effects at the level of a spatially aggregated unit rather than the pixel level to resolve bias associated with TWFE regressions with pixel fixed effects. For simplicity, we assume the researcher can choose between three levels at which to aggregate the data: grid cell, county, and property. Grid cells are uniform grids layered over the study area. Counties are heterogeneous administrative units at which we assign the treatment. Lastly, properties are smaller spatial units that lie within one or more counties. Pixel level TWFE models with spatially aggregated unit fixed effects are all in the form of Regression 3.

Regression 3: (pixel level TWFE regression with spatially aggregated unit fixed effects) Let $\beta_{FE,j}$ denote the coefficient of the interaction between D_i and $\mathbb{1}\{t \geq t_0\}$ in the following (population) OLS regression:

$$y^o_{ivt} = \alpha + \beta_{FE,j} \text{ x } D_i \mathbb{1}\{t \geq t_0\} + \lambda_t + \gamma_j + \epsilon_{it} \tag{6}$$

, where λ_t denotes year fixed effects and γ_j denotes one of grid (g), county (c), or property (v) fixed effects ($j = \{g, c, v\}$).

Columns 6-8 of Figure 2 show that pixel level TWFE regressions are unbiased estimators of the ATT when grid, county, or property fixed effects are used rather than pixel fixed effects. We also see that, in the absence of property level perturbations (i.e. $\sigma_p = 0$ in the DGP), all three models provide similar estimates and estimate distributions.
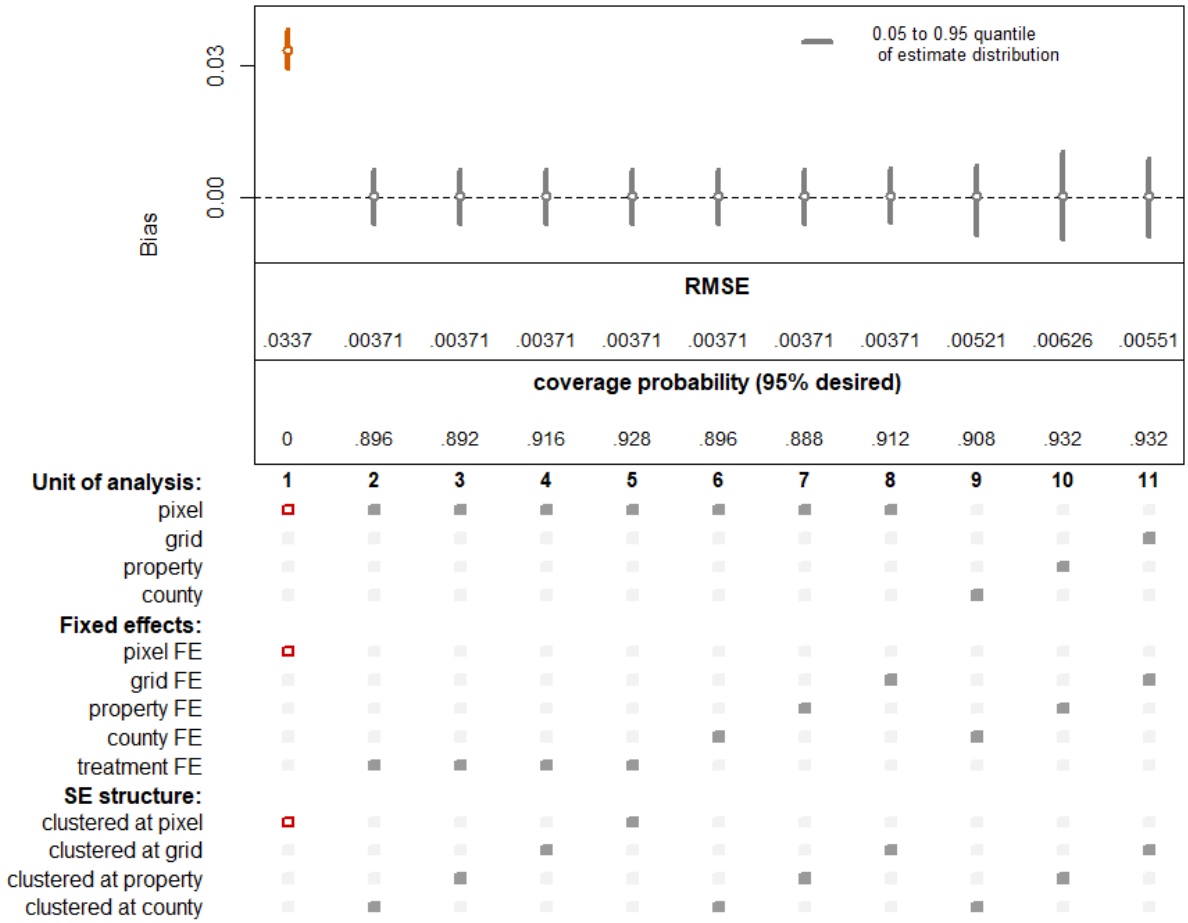

Figure 2: bias, distribution, and coverage of all models with clustered standard errors.

5.3 Spatially aggregated units of analysis

Another potential solution to the bias associated with TWFE models is for researchers to aggregate multiple pixel level observations into larger units of analysis. The researcher must now calculate the deforestation rate in each time period. While we detail the tradeoffs of various deforestation rate calculations in the appendix (Appendix 10.4), the following results are based on Equation (7) which is arguably the most commonly used formula in the literature (e.g. Carlson et al. 2018; Busch, Ferretti-Gallon, et al. 2015).

$$z_{jt} = \frac{F_{j,t-1} - F_{j,t}}{F_{j,t-1}}$$

, where $F_{j,t}$ and $F_{j,t-1}$ are the forest cover in unit j at times t and $t-1$, respectively. Unit j represents one of the three aggregated units, the grid cell (g), county (c), or property (v).

Regression 4: (grid/county/property level regression) Let β_j denote the coefficient of the interaction between D_j and $\mathbb{1}\{t \geq t_0\}$ in the following (population) OLS regression, where $j = \{g, c, v\}$:

$$z_{jt} = \alpha + \beta_j \text{ x } D_j \mathbb{1}\{t \geq t_0\} + \lambda_t + \gamma_j + \epsilon_{jt} \tag{7}$$

, where λ_t denotes year fixed effects and γ_j denotes one of grid, county, or property fixed effects. Note that in these regressions, the level of unit fixed effects matches the unit of analysis. The treatment variable $D_j = \frac{1}{N_j} \sum_{i=1}^{N_j} D_i$, is the average treatment value amongst all pixels in unit j.

Columns 9-11 of Figure 2 show that the bias of the estimates does not vary dramatically across different levels of aggregation, however, the distributions are varied. In particular, the property-level model had the highest RMSE of unbiased specifications due to the heterogeneity in property sizes across the landscape. At the same time, relative to the pixel level models with spatially aggregated unit fixed effects, the coverage probability of these models is closer to the desired 95%.

–>

–> –> –> –> –>

6 The value of linking model structure to the spatial process of deforestation

In the absence of pixel-level (u_{it}) and property-level (ρ_v) disturbances to the latent value of deforestation (y_{ivt}^*), aggregating either fixed effects or units of observation at a variety of spatial scales can generate relatively unbiased estimates of the ATT (Figure 2). However, the process of land use decisionmaking may frequently depend upon such fine-grained, spatial disturbances. For example, a landowner may have preferences for a specific land cover, or may have access to unique resources that alter their returns to deforestation. In the presence of such disturbances, it is possible that incorporating fixed effects at the level of land use decision-making could improve model performance.

To evaluate the impact of property-level fixed effects, we re-run our Monte Carlo simulations while steadily increasing the standard deviation of the property-level disturbances (σ_p) to the returns to deforestation (Figure 3). These results highlight that econometric models whose structure matches the process by which land use decisions are made, can generate estimates of the ATT with less bias, lower RMSE, and more accurate coverage. Although researchers often use a binary point, pixel, or grid cell as the unit of analysis and scale at which fixed effects are estimated (Table 1), this approach to aggregation may lead to biased results if the land use change process operates at a different scale.

6.1 Choosing an appropriate scale for aggregation

6.1.1 Matching grid cell resolution to scale of heterogeneity may reduce bias

Although aggregation at the scale of individual decision-making may be preferred in many research settings, researchers often do not have access to the geospatial boundaries of individual properties. In these cases, a grid cell or alternative administrative unit that captures spatial heterogeneity is likely preferable to pixel
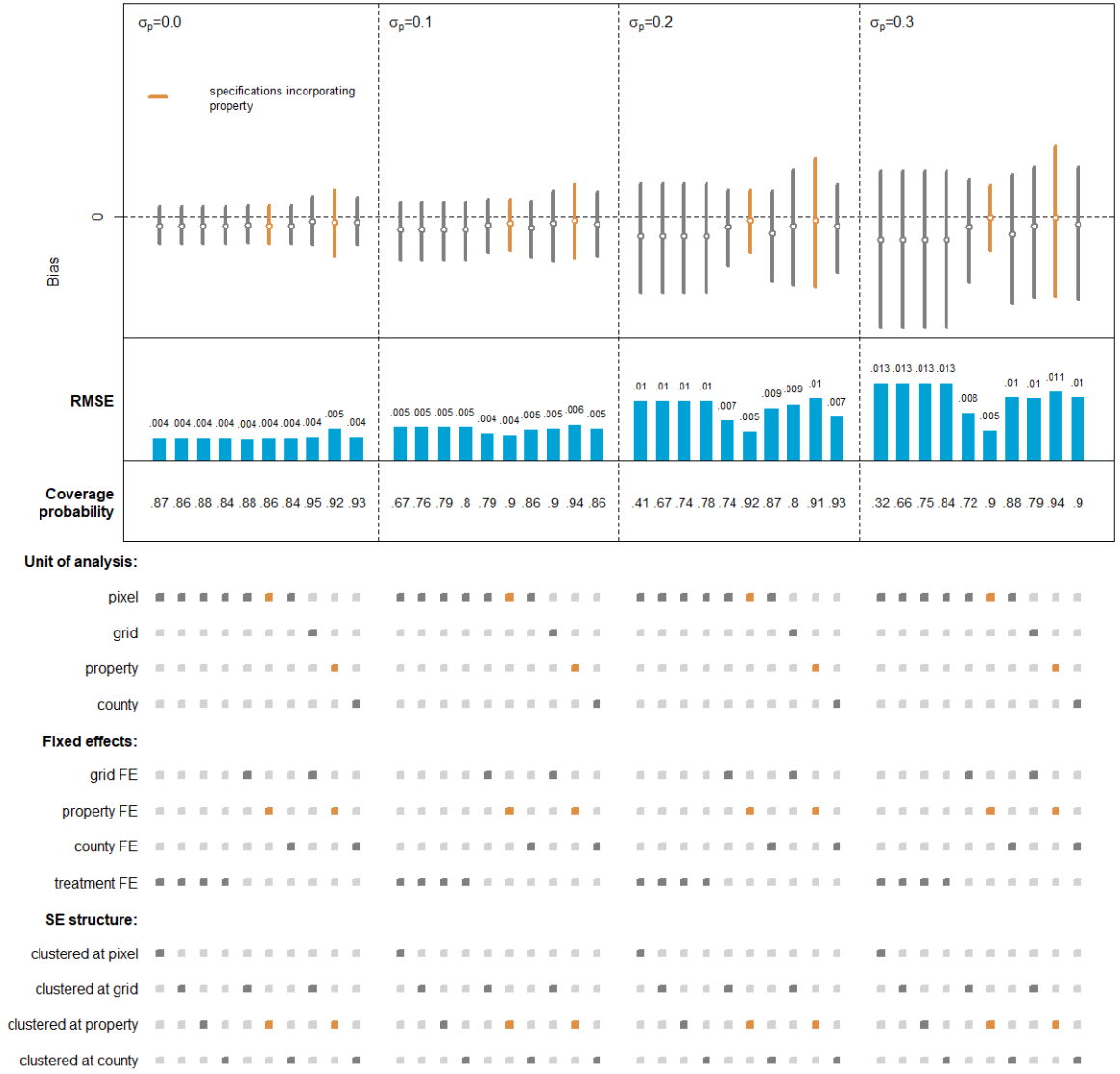
Figure 3: As property-level disturbances increase (individual panels moving left to right), models that incorporate property boundaries into their structure tend to outperform models with different levels of spatial aggregation.

level DID models for purposes of both estimation and inference. The scale of this grid cell or alternative unit relative to the process of land use change is important. Grid cell size plays a role in both estimation and inference, particularly when property level unobservables are present.

We now vary the grid cell resolution used in our monte carlo simulations to understand the importance of scale when conducting conservation impact evaluations. Table 2 helps us to get a sense of relative scale within our synthetic landscapes for this section. We assume pixels are comparable to Landsat resolution (30 m), and so the average property in our landscape is 81 hectares. This average property area is equivalent to a 30 x 30 pixel grid cell in our setting.

Table 2: Grid cell resolution and comparable area

resolution (pixels)	comparable resolution (m)	area (hectares)
3	90	0.81
5	150	2.25
10	300	9.00
20	600	36.00
30	900	81.00
50	1500	225.00
100	3000	900.00

Figure 4 shows how bias, coverage probability, and RMSE depend on grid size in pixel-level TWFE regressions with grid unit fixed effects (Regression 3, where j represents grid cell j). Grid cells near to or slightly larger than the size of the average property tend to perform better than undersized or particularly large grid cells. We find that too high of resolution (too small of grid cells) analyses suffer from potentially severe bias, and low resolution (too large of grid cells) analyses, while largely unbiased, suffer from imprecise estimation and inference.

The top panel of Figure 4 shows that even in the absence of property-level unobservables ($\sigma_p = 0$), too small of grids result in biased estimates of the ATT. The introduction of property unobservables exacerbates the issue with small grid cells, causing undersized grids to result in upward bias much more severe than when property unobservables play a lesser role. This results from the fact that small grid cells or pixels do not match the scale at which the deforestation process operates. Larger grid cells account for the spatial autocorrelation introduced within property boundaries by the property unobservables, while pixels and smaller grid cells do not. In the case when property unobservables are extreme ($\sigma_p = 0.25$), we see that bias is decreasing in grid cell size and is minimized at an point where grid cells are approaching the size of the larger properties in the landscape. When property unobservables are present, RMSE and coverage probability stabilize at the point where grid size approaches the scale of the average property.
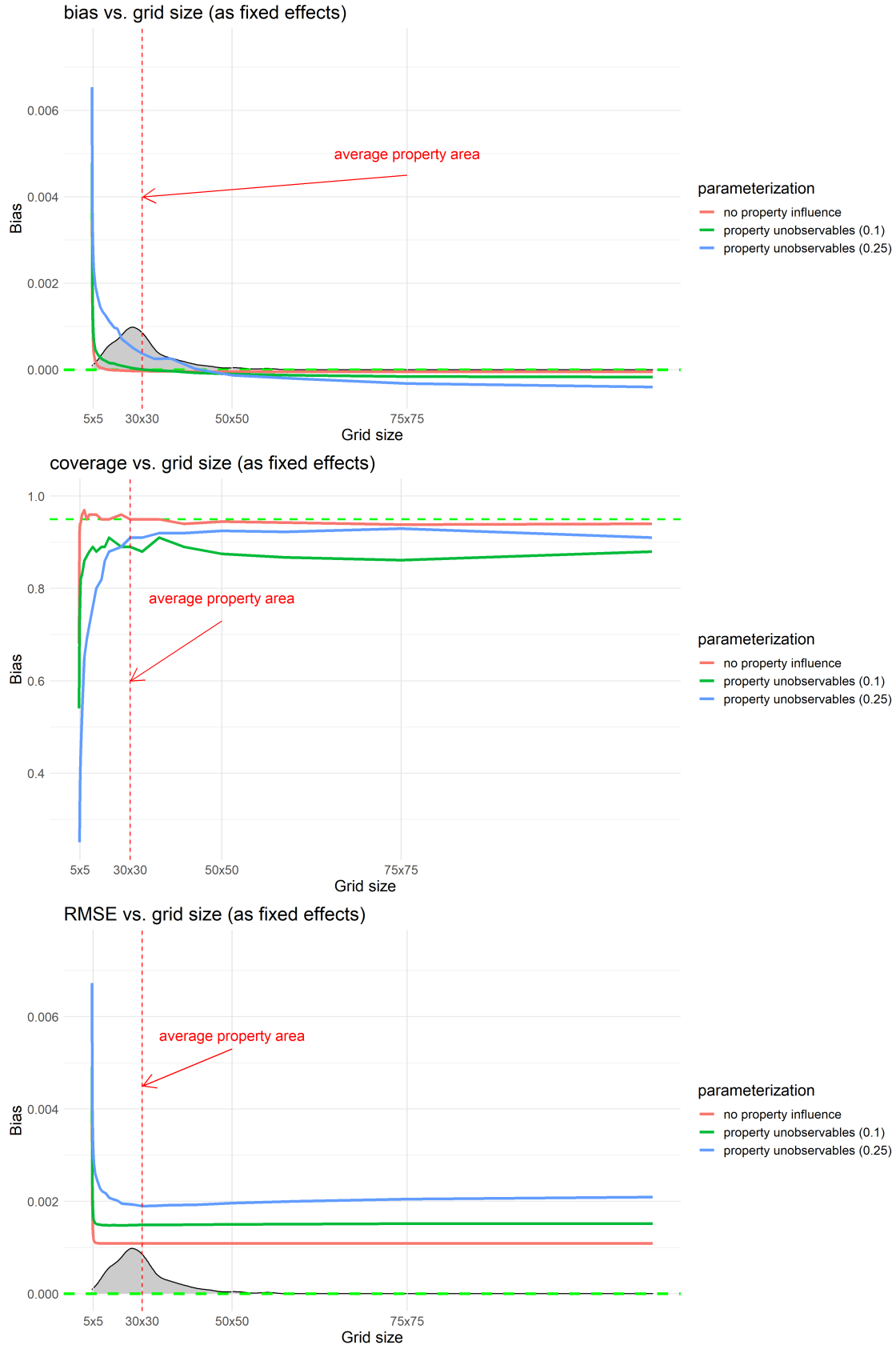
Figure 4: Regression 3 with grid unit fixed effects; bias, coverage, and rmse depend on grid size when property level unobservables are present. Note: x-axis begins at 2x2 grid cells to ensure grid cells are not simply pixels.
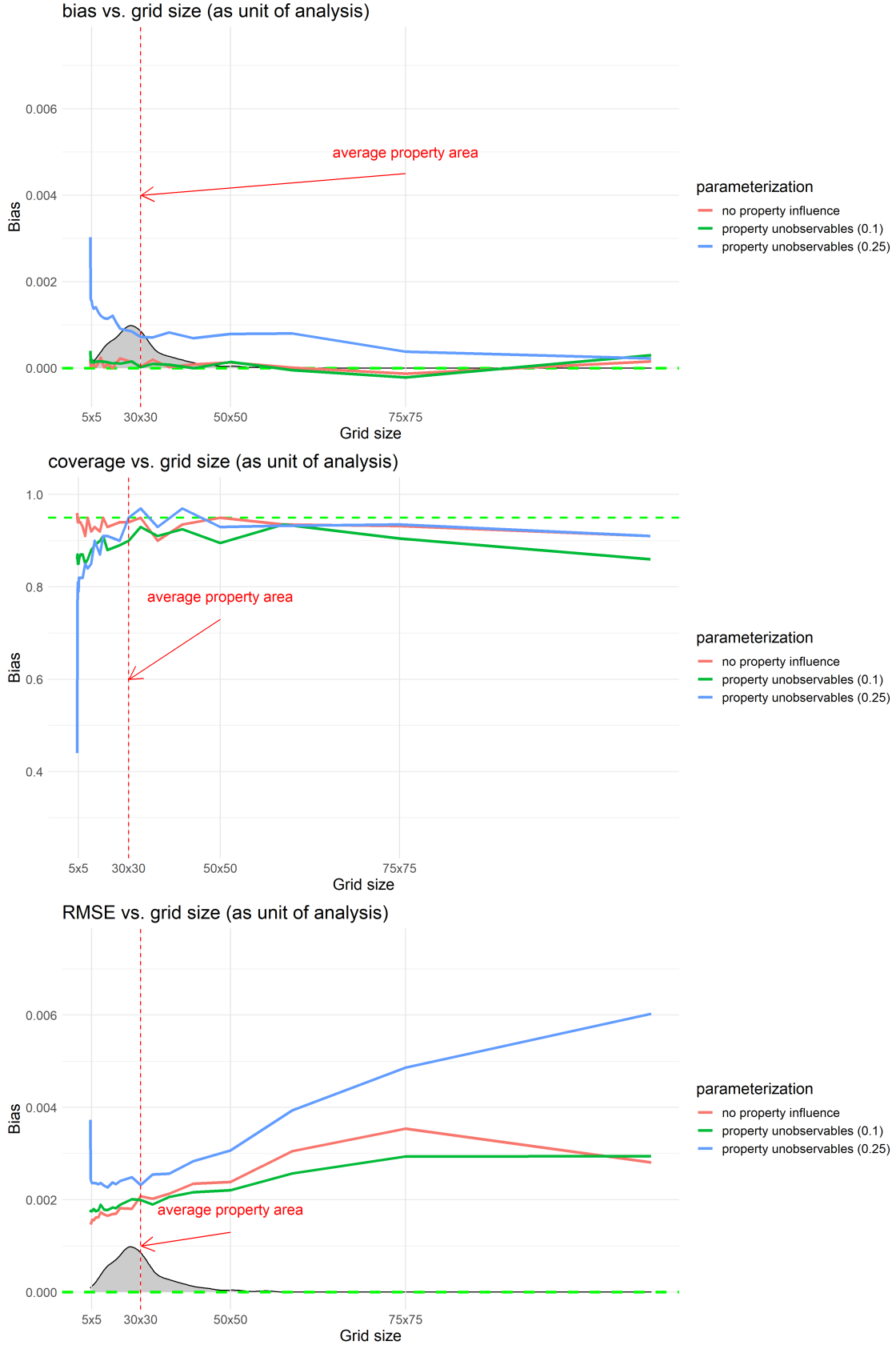
Figure 5: Regression 4 with grid as unit of analysis; bias, coverage, and rmse depend on grid size when property level unobservables are present. Note: x-axis begins at 2x2 grid cells to ensure grid cells are not simply pixels.

Figure 5 shows how bias, coverage probability, and RMSE depend on grid size in grid-level TWFE regressions (Regression 4, where j represents grid cell j). Again, undersized or particularly large grid cells suffer in terms of bias, coverage probability, and RMSE relative to grid cells near the scale of the average property. In the presence of significant heterogeneity at the property scale ($\sigma_p = 0.25$), undersized grids suffer from severe bias. This manifests in poor coverage probability and RMSE for these undersized grids as well. As grid cells become particularly large, all parameterizations begin to suffer in tems of RMSE and coverage, making the use of very low resolution grid cells a potential concern in terms of precision and inference.

The findings in this section demonstrate the dangers of conducting the analysis at a scale that does not match that which drives landscape heterogeneity or the deforestation process. Using grid cells or pixels at too small of scale is likely to result in biased estimates. If using the grid cell as the unit of analysis rather than simply as the level of unit fixed effects, low resolution grid cells are likely to result in imprecise estimates of the ATT and undesireable inference.

6.2 Weighting by area recovers landscape scale estimates

Authors frequently choose to use a pixel or grid cell as their preferred unit of analysis for purposes of interpretation. The coefficient of interest from a specification using the pixel as the unit of analysis can be interpreted as the average effect of the intervention across the landscape. In contrast, if a property is used, the coefficient should be interpreted as the effect of the intervention on the characteristic property in the sample. In order to obtain a landscape-scale interpretation, researchers should weight the regression by the area of the unit of analysis (i.e. property).
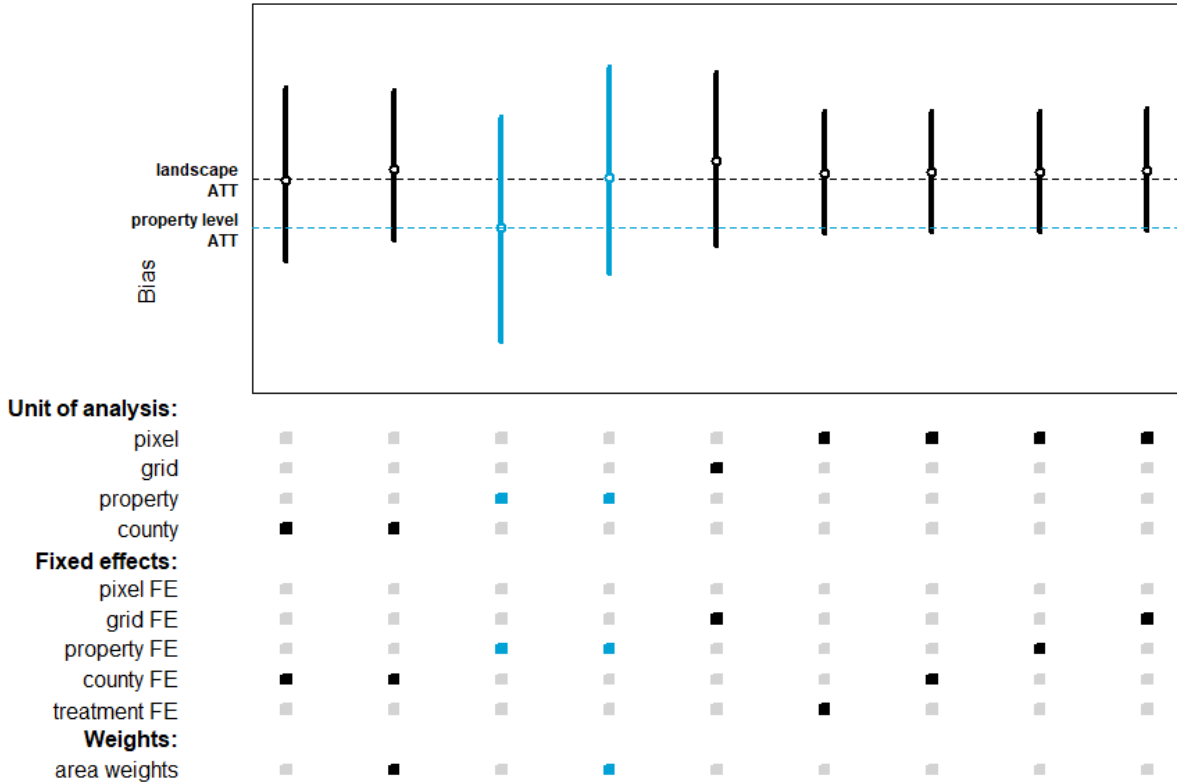

Figure 6: weighting recovers landscape scale interpretation

The use of area weights is likely to be most important when the ATT for the characteristic property differs from the ATT across the landscape. We now consider the case when treatment effects are correlated with

property size. The treatment effect now varies across properties, and properties with greater areas experience treatment effects of a lower magnitude than smaller properties. The ATT across the landscape is still -0.01. However, because the treatment is more impactful in properties of smaller size, the treatment effect for the average property is less than -0.01, which is the landscape ATT.

Figure 6 shows the ATT at both the property and landscape scale. The property-level TWFE regression identifies the ATT relative to the characteristic property when area weights are not used and the landscape scale ATT when they are used. Researchers should use these area weights when they are interested in the impact of the intervention across the landscape. In cases where the researcher is interested in how an intervention affects incentives at the property level, weighting may not be neccessary.

7 Extension to multiple groups and time periods

The typical DID regression applies to settings with two-groups and two-periods, however, many times researchers use TWFE regressions to exploit variation across groups of units that receive treatment at different times. Recent work has show that in these cases, TWFE regressions identify a weighted average of all possible two-group/two-period DID estimators in the data (Goodman-Bacon 2021). Further, when estimating the ATT, some weights on each group-time treatment effect parameter may actually be negative, which could lead, for instance, to a negative regression coefficient while all the treatment effect parameters are positive (Chaisemartin and D'Haultfœuille 2020). Newly developed estimators (e.g., Callaway and Sant'Anna 2020; Gardner 2021) seek to address these weighting issues by individually calculating two-period, two-group DID parameters before aggregating them to a summary measure of the overall ATT.

One might conclude that these new estimators solve the issues with pixel level TWFE regressions that we present in Section @ref(twfe_fail), since some of these estimators involve separately calculating all two period/two-group treatment effects. We consider the estimators developed in this burgeoning literature in order to show that the issue with pixel unit fixed effects can still plague researchers' estimates even with this new suite of methods.

Here, we introduce a setting in which groups of units receive treatment at different times. We consider three groups: an early group, a late group, and a never-treated group, where the early and late groups undergo treatment in years three and four, respectively. Each group experiences differing pre-treatment deforestation rates (7%, 4%, and 2% for the early, late, and never-treated groups, respectively) and no time trend. The on-impact ATT is -0.02 for both treated groups. Common trends is satisfied by construction, and we do not introduce any dynamic effects. Figure 7 shows the observed deforestation rates ($E[y^o_{ivt}]$) from one iteration of our simulation in this setting.
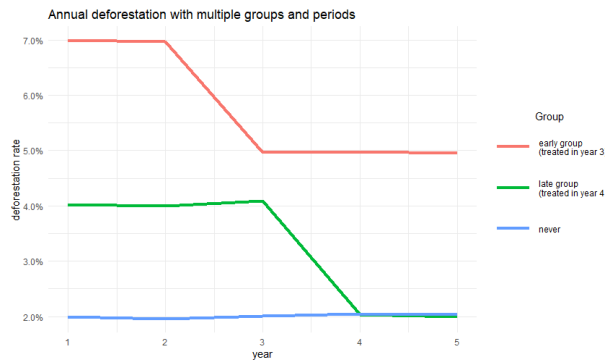

Figure 7: Observed deforestation in simulated example with multiple groups and periods.

The left panel of Figure 8 shows that the estimators developed in Callaway and Sant'Anna (2020) and Gardner (2021) suffer from similar bias to TWFE regressions with pixel unit fixed effects if the pixel is used as the unit of analysis. All methods yield a treatment effect greater than or equal to 0 in all post-treatment periods, reflecting the fact that pre-treatment period deforestation rates are unaccounted for by the estimators. This

is particularly clear in the Callaway and Sant'Anna (2020) estimator in which pre-treatment periods are all precisely zero, indicating that the estimator could only compute treatment effects using pixels that survived until the end of the observation period. The right panel of Figure 8 shows that this bias is eliminated when one uses an aggregated unit of analysis with binary treatment (e.g., county).
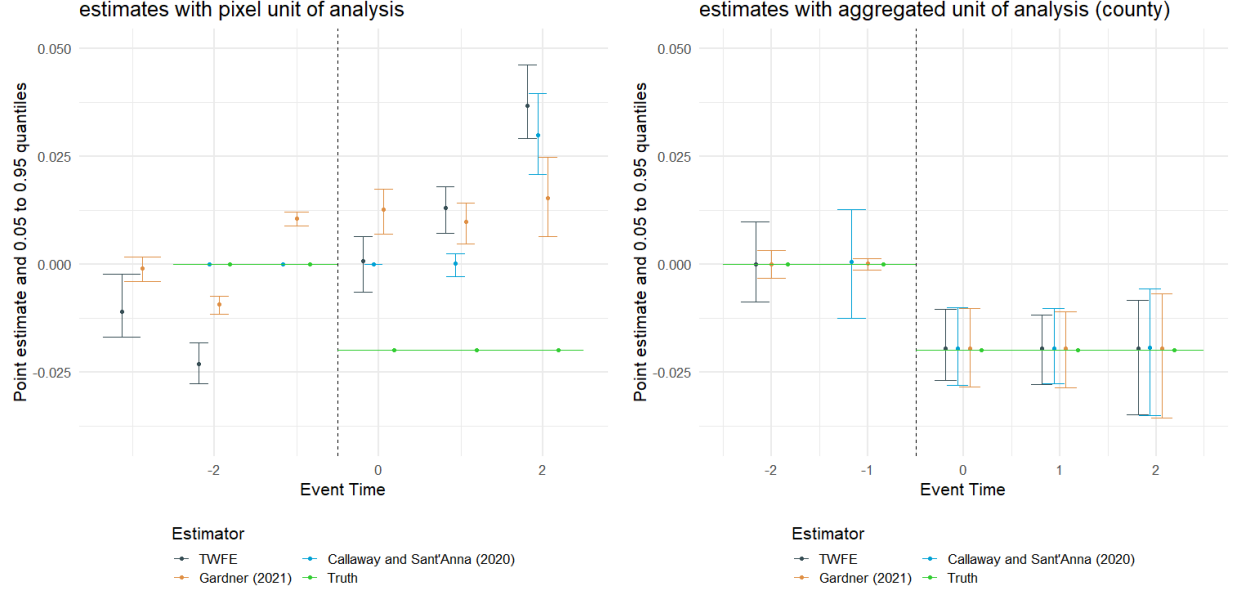

Figure 8: New estimators, similar to TWFE regressions with pixel unit fixed effects, cannot identify ATT with pixel as unit of analysis

7.1 Evidence of weighting concerns in the TWFE estimator

We now examine the performance of the Callaway and Sant'Anna (2020) and Gardner (2021) estimators relative to TWFE regressions when treatment effects vary across time and across groups. We again work with an early, late and untreated group. Figure 9 shows deforestation rates in each of the three groups through time.
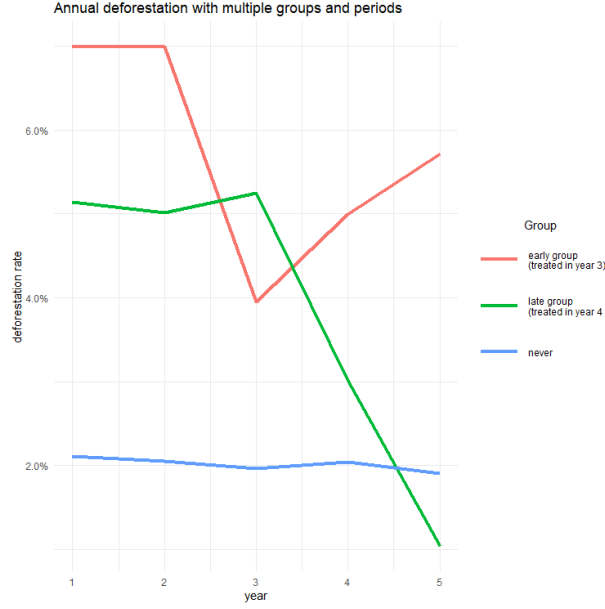
Figure 9: Observed deforestation in simulated example when treatment effects vary across groups and through time

Figure 10 shows the event study estimates produced by each of the three estimators as well as the "truth" for both pixel and county-level analyses. Again, none of the estimators yield the ATT with pixel-level analyses. In the county-level estimates, we see that the Callaway and Sant'Anna (2020) and Gardner (2021) estimators slightly outperform the TWFE estimator. This is evidence of the weighting that has become a concern with TWFE estimators in these type of settings. While each estimator produces a weighted average of group-time treatment effects, the weights do not neccessarily correspond to each groups representation in the sample for a given event-time treatment effect as do the weights in the alternative estimators.
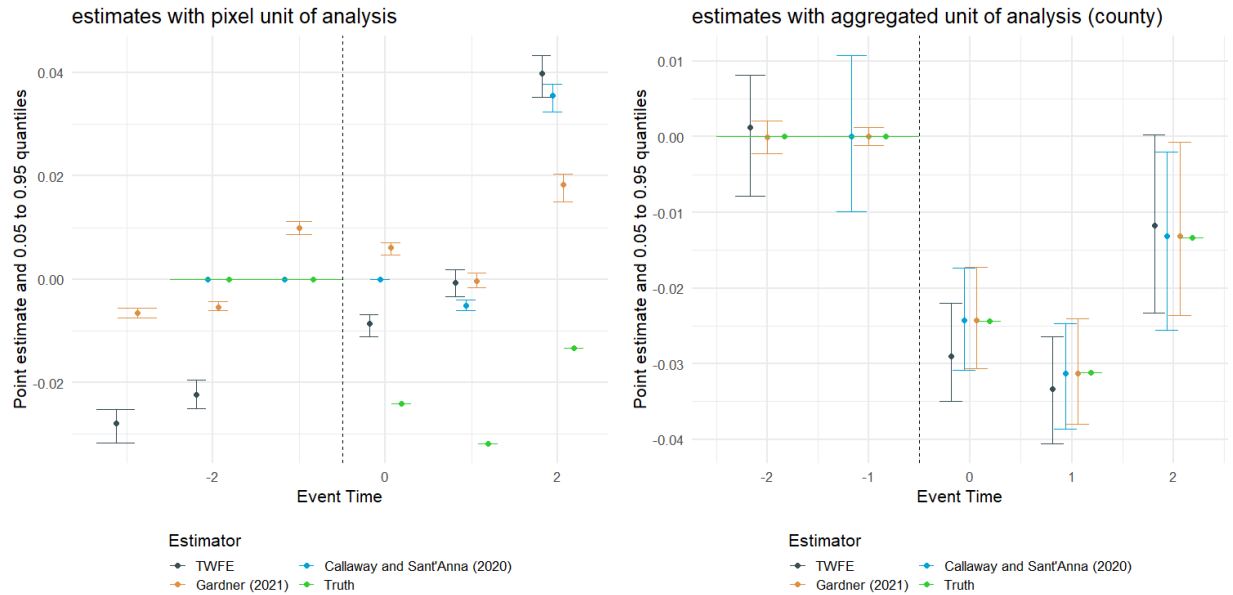

Figure 10: TWFE regressions suffer from weighting concerns when treatment effects vary across groups and through time

8 Conclusions

Despite past guidance to the contrary, models that seek to estimate a policy's impact on an irreversible, binary outcome are unable to yield unbiased estimates of the ATT. This is due to the fact that when pixels are dropped after they first become deforested, TWFE models cannot identify off of points deforested in the pre-treatment periods. This novel finding casts doubt on the reliability of a number of estimates in the avoided deforestation literature.

In order to avoid the bias introduced by pixel-level, TWFE models of deforestation, researchers can either (a) aggregate the level at which they estimate fixed effects; or (b) aggregate their unit of analysis. The optimal scale of spatial aggregation under either of these approaches depends upon the process by which land use decisions are made. Specifically, bias can arise when the model specification diverges from the scale at which land use change is determined. Ultimately, context plays a role in what is feasible, and researchers should make clear the limits to their impact evaluation strategy.

We do not integrate many aspects of the growing literature that explores how remotely sensed data may need special consideration when used in impact evaluation. Aspects of the collection process such as satellite sensor characteristics, atmospheric conditions, and image processing may influence the structure of remotely sensed data products (Jain 2020; Alix-Garcia and Millimet 2021). Of particular concern is the possibility of non-classical measurement error, which can lead to biased estimates of the ATT (Wooldridge 2010). Alix-Garcia and Millimet (2021) propose a solution for the case of a remotely sensed binary outcome in which misclassification is non-classical. As the proliferation of research relying on satellite data continues, researchers will need consider the intricacies of particular satellite data sources in order to substantiate causal claims.

Without clear guidance on key econometric decisions, a wide variety of quasi-experimental methods have been used to estimate the impact of policy interventions on deforestation. We show that a number of studies use specifications that suffer from estimation bias and imprecise inference. The observations made here may apply to a wider audience beyond the set of researchers investigating the impacts of conservation interventions on deforestation. Any evaluation of interventions implemented at a spatial scale may benefit from this discussion. These results further apply to many settings in which the outcome represents an irreversible binary event. For example, studies addressing technology adoption as an outcome may fall prey to the same issues we describe with a point level analysis. Moving forward, it is imperative that researchers use methods that minimize bias and allow inference at expected levels of confidence. Misleading results may lead policymakers to avoid impactful policy designs or adopt policies that exacerbate environmental damages.

9 References

Alix-Garcia, Jennifer, and Holly K. Gibbs. 2017. "Forest Conservation Effects of Brazil's Zero Deforestation Cattle Agreements Undermined by Leakage." *Global Environmental Change* 47 (November): 201–17. https://doi.org/10.1016/j.gloenvcha.2017.08.009.

Alix-Garcia, Jennifer, and Holly K. Gibbs. 2017. "Forest Conservation Effects of Brazil's Zero Deforestation Cattle Agreements Undermined by Leakage." *Global Environmental Change* 47 (November): 201–17. https://doi.org/10.1016/j.gloenvcha.2017.08.009.

Alix-Garcia, Jennifer, and Daniel L Millimet. 2021. "Remotely Incorrect? Accounting for Nonclassical Measurement in Satellite Data on Deforestation," 56.

Alix-Garcia, Jennifer, Lisa L. Rausch, Jessica L'Roe, Holly K. Gibbs, and Jacob Munger. 2018. "Avoided

Deforestation Linked to Environmental Registration of Properties in the Brazilian Amazon: Environmental Registration in the Amazon." *Conservation Letters* 11 (3): e12414. https://doi.org/10.1111/conl.12414.

Alix-Garcia, Jennifer, Lisa L. Rausch, Jessica L'Roe, Holly K. Gibbs, and Jacob Munger. 2018. "Avoided Deforestation Linked to Environmental Registration of Properties in the Brazilian Amazon: Environmental Registration in the Amazon." *Conservation Letters* 11 (3): e12414. https://doi.org/10.1111/conl.12414.

Andam, K. S, P. J Ferraro, A. Pfaff, G. A Sanchez-Azofeifa, and J. A Robalino. 2008. "Measuring the Effectiveness of Protected Area Networks in Reducing Deforestation." *Proceedings of the National Academy of Sciences* 105 (42): 16089.

Anderson, Christa M., Gregory P. Asner, William Llactayo, and Eric F. Lambin. 2018. "Overlapping Land Allocations Reduce Deforestation in Peru." *Land Use Policy* 79 (December): 174–78. https://doi.org/10.1016/j.landusepol.2018.08.002.

Araujo, Claudio, Catherine Araujo Bonjean, Jean-Louis Combes, Pascale Combes Motel, and Eustaquio J. Reis. 2009. "Property Rights and Deforestation in the Brazilian Amazon." *Ecological Economics* 68 (8-9): 2461–8. https://doi.org/10.1016/j.ecolecon.2008.12.015.

Arriagada, R. A., P. J. Ferraro, E. O. Sills, S. K. Pattanayak, and S. Cordero-Sancho. 2012. "Do Payments for Environmental Services Affect Forest Cover? A Farm-Level Evaluation from Costa Rica." *Land Economics* 88 (2): 382–99. https://doi.org/10.3368/le.88.2.382.

Baehr, Christian, Ariel BenYishay, and Bradley Parks. 2021. "Linking Local Infrastructure Development and Deforestation: Evidence from Satellite and Administrative Data." *Journal of the Association of Environmental and Resource Economists* 8 (2): 375–409. https://doi.org/10.1086/712800.

Baylis, Kathy, Jordi Honey, and M Isabel Ramírez. 2012. "Conserving Forests: Mandates, Management or Money?" *Selected Paper Prepared for Presentation at the Agricultural & Applied Economics Association's 2012 AAEA Annual Meeting*, August, 17.

Baylis, Kathy, Jordi Honey-Rosés, Jan Börner, Esteve Corbera, Driss Ezzine-de-Blas, Paul J. Ferraro, Renaud Lapeyre, U. Martin Persson, Alex Pfaff, and Sven Wunder. 2016. "Mainstreaming Impact Evaluation in Nature Conservation." *Conservation Letters* 9 (1): 58–64. https://doi.org/10.1111/conl.12180.

BenYishay, Ariel, Silke Heuser, Daniel Runfola, and Rachel Trichler. 2017. "Indigenous Land Rights and Deforestation: Evidence from the Brazilian Amazon." *Journal of Environmental Economics and Management* 86 (November): 29–47. https://doi.org/10.1016/j.jeem.2017.07.008.

Blackman, Allen. 2013. "Evaluating Forest Conservation Policies in Developing Countries Using Remote Sensing Data: An Introduction and Practical Guide." *Forest Policy and Economics* 34 (September): 1–16. https://doi.org/10.1016/j.forpol.2013.04.006.

Blackman, Allen, Leonardo Corral, Eirivelthon Santos Lima, and Gregory P. Asner. 2017. "Titling Indigenous Communities Protects Forests in the Peruvian Amazon." *Proceedings of the National Academy of Sciences* 114 (16): 4123–8. https://doi.org/10.1073/pnas.1603290114.

Blackman, Allen, Leonard Goff, and Marisol Rivera Planter. 2018. "Does Eco-Certification Stem Tropical Deforestation? Forest Stewardship Council Certification in Mexico." *Journal of Environmental Economics and Management* 89 (May): 306–33. https://doi.org/10.1016/j.jeem.2018.04.005.

Busch, Jonah, Kalifi Ferretti-Gallon, Jens Engelmann, Max Wright, Kemen G. Austin, Fred Stolle, Svetlana Turubanova, et al. 2015. "Reductions in Emissions from Deforestation from Indonesia's Moratorium on New Oil Palm, Timber, and Logging Concessions." *Proceedings of the National Academy of Sciences* 112 (5): 1328–33. https://doi.org/10.1073/pnas.1412514112.

Busch, Jonah, Kalifi Ferretti-Gallon, Jens Engelmann, Max Wright, Kemen G. Austin, Fred Stolle, Svetlana Turubanova, et al. 2015. "Reductions in Emissions from Deforestation from Indonesia's Moratorium on New Oil Palm, Timber, and Logging Concessions." *Proceedings of the National Academy of Sciences* 112 (5): 1328–33. https://doi.org/10.1073/pnas.1412514112.

Butsic, Van, David J. Lewis, Volker C. Radeloff, Matthias Baumann, and Tobias Kuemmerle. 2017. "Quasi-Experimental Methods Enable Stronger Inferences from Observational Data in Ecology." *Basic and Applied Ecology* 19 (March): 1–10. https://doi.org/10.1016/j.baae.2017.01.005.

Butsic, Van, Catalina Munteanu, Patrick Griffiths, Jan Knorn, Volker C. Radeloff, Juraj Lieskovský, Daniel Mueller, and Tobias Kuemmerle. 2017. "The Effect of Protected Areas on Forest Disturbance in the Carpathian Mountains 1985-2010: Carpathian Protected Areas." *Conservation Biology* 31 (3): 570–80. https://doi.org/10.1111/cobi.12835.

Callaway, Brantly, and Pedro H. C. Sant'Anna. 2020. "Difference-in-Differences with Multiple Time Periods." *Journal of Econometrics*, December, S0304407620303948. https://doi.org/10.1016/j.jeconom.2020.12.001.

Carlson, Kimberly M., Robert Heilmayr, Holly K. Gibbs, Praveen Noojipady, David N. Burns, Douglas C. Morton, Nathalie F. Walker, Gary D. Paoli, and Claire Kremen. 2018. "Effect of Oil Palm Sustainability Certification on Deforestation and Fire in Indonesia." *Proceedings of the National Academy of Sciences* 115 (1): 121–26. https://doi.org/10.1073/pnas.1704728114.

Chaisemartin, Clément de, and Xavier D'Haultfœuille. 2020. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review* 110 (9): 2964–96. https://doi.org/10.1257/aer.20181169.

Edwards, Ryan B, Walter P Falcon, Gracia Hadiwidjaja, Matthew M Higgins, Rosamond L Naylor, and Sudarno Sumarto. 2020. "Fight Fire with Finance: A Randomized Field Experiment to Curtail Land-Clearing Fire in Indonesia," 62.

Ferraro, Paul J. 2009. "Counterfactual Thinking and Impact Evaluation in Environmental Policy." *New Directions for Evaluation* 2009 (122): 75–84. https://doi.org/10.1002/ev.297.

Ferraro, Paul J., and Juan José Miranda. 2017. "Panel Data Designs and Estimators as Substitutes for Randomized Controlled Trials in the Evaluation of Public Programs." *Journal of the Association of Environmental and Resource Economists* 4 (1): 281–317. https://doi.org/10.1086/689868.

Ferraro, Paul J., James N. Sanchirico, and Martin D. Smith. 2019. "Causal Inference in Coupled Human and Natural Systems." *Proceedings of the National Academy of Sciences* 116 (12): 5311–8. https://doi.org/10.1073/pnas.1805563115.

Gardner, John. 2021. "Two-Stage Differences in Differences." *Working Paper*, April, 34.

Goodman-Bacon, Andrew. 2021. "Difference-in-Differences with Variation in Treatment Timing." *Journal of Econometrics*, June, S0304407621001445. https://doi.org/10.1016/j.jeconom.2021.03.014.

Hansen, Matthew C., and Thomas R. Loveland. 2012. "A Review of Large Area Monitoring of Land Cover Change Using Landsat Data." *Remote Sensing of Environment*, Landsat Legacy Special Issue, 122 (July): 66–74. https://doi.org/10.1016/j.rse.2011.08.024.

Hansen, M. C., P. V. Potapov, R. Moore, M. Hancher, S. A. Turubanova, A. Tyukavina, D. Thau, et al. 2013. "High-Resolution Global Maps of 21st-Century Forest Cover Change." *Science* 342 (6160): 850–53. https://doi.org/10.1126/science.1244693.

Heilmayr, Robert, and Eric F. Lambin. 2016. "Impacts of Nonstate, Market-Driven Governance on Chilean Forests." *Proceedings of the National Academy of Sciences* 113 (11): 2910–5. https://doi.org/10.1073/pnas.1600394113.

Herrera, Diego, Alexander Pfaff, and Juan Robalino. 2019. "Impacts of Protected Areas Vary with the Level of Government: Comparing Avoided Deforestation Across Agencies in the Brazilian Amazon." *Proceedings of the National Academy of Sciences* 116 (30): 14916–25. https://doi.org/10.1073/pnas.1802877116.

Holland, Margaret B., Kelly W. Jones, Lisa Naughton-Treves, José-Luis Freire, Manuel Morales, and Luis Suárez. 2017. "Titling Land to Conserve Forests: The Case of Cuyabeno Reserve in Ecuador." *Global Environmental Change* 44 (May): 27–38. https://doi.org/10.1016/j.gloenvcha.2017.02.004.

Holland, Paul W. 1986. "Statistics and Causal Inference." *Journal of the American Statistical Association* 81 (396): 945–60. https://doi.org/10.1080/01621459.1986.10478354.

Jain, Meha. 2020. "The Benefits and Pitfalls of Using Satellite Data for Causal Inference." *Review of Environmental Economics and Policy* 14 (1): 157–69. https://doi.org/10.1093/reep/rez023.

Jayachandran, Seema, Joost de Laat, Eric F. Lambin, Charlotte Y. Stanton, Robin Audy, and Nancy E. Thomas. 2017. "Cash for Carbon: A Randomized Trial of Payments for Ecosystem Services to Reduce Deforestation." *Science* 357 (6348): 267–73. https://doi.org/10.1126/science.aan0568.

Jones, Kelly W., Margaret B. Holland, Lisa Naughton-Treves, Manuel Morales, Luis Suarez, and Kayla Keenan. 2017. "Forest Conservation Incentives and Deforestation in the Ecuadorian Amazon." *Environmental Conservation* 44 (1): 56–65. https://doi.org/10.1017/S0376892916000308.

Jones, Kelly W., Margaret B. Holland, Lisa Naughton-Treves, Manuel Morales, Luis Suarez, and Kayla Keenan. 2017. "Forest Conservation Incentives and Deforestation in the Ecuadorian Amazon." *Environmental Conservation* 44 (1): 56–65. https://doi.org/10.1017/S0376892916000308.

Jones, Kelly W., and David J. Lewis. 2015. "Estimating the Counterfactual Impact of Conservation Programs on Land Cover Outcomes: The Role of Matching and Panel Regression Techniques." *PLOS ONE* 10 (10): e0141380. https://doi.org/10.1371/journal.pone.0141380.

Kerr, Suzi, Alexander S P Pfaff, and Arturo Sanchez. 2003. "Development and Deforestation: Evidence from Costa Rica," 19.

Koch, Nicolas. 2018. "Agricultural Productivity and Forest Conservation: Evidence from the Brazilian Amazon," 22.

Larsen, Ashley E., Kyle Meng, and Bruce E. Kendall. 2019. "Causal Analysis in ControlImpact Ecological Studies with Observational Data." *Methods in Ecology and Evolution* 10 (7): 924–34. https://doi.org/10.1111/2041-210X.13190.

Meyfroidt, Patrick. 2016. "Approaches and Terminology for Causal Analysis in Land Systems Science." *Journal of Land Use Science* 11 (5): 501–22. https://doi.org/10.1080/1747423X.2015.1117530.

Nolte, Christoph, Beatriz Gobbi, Van Butsic, and Eric F Lambin. 2017. "Decentralized Land Use Zoning Reduces Large-Scale Deforestation in a Major Agricultural Frontier." *Ecological Economics*, 11.

Panlasigui, Stephanie. 2018. "Impacts of Certification, Uncertified Concessions, and Protected Areas on Forest Loss in Cameroon, 2000 to 2013." *Biological Conservation*, 7.

Pfaff, Alexander, Juan Robalino, G. Arturo Sanchez-Azofeifa, Kwaw S. Andam, and Paul J. Ferraro. 2009. "Park Location Affects Forest Protection: Land Characteristics Cause Differences in Park Impacts Across Costa Rica." *The B.E. Journal of Economic Analysis & Policy* 9 (2).

Pfaff, Alexander S. P. 1999. "What Drives Deforestation in the Brazilian Amazon?" *Journal of Environmental Economics and Management* 37 (1): 26–43. https://doi.org/10.1006/jeem.1998.1056.

Pfaff, Alexander S. P., and G. Arturo Sanchez-Azofeifa. 2004. "Deforestation Pressure and Biological Reserve Planning: A Conceptual Approach and an Illustrative Application for Costa Rica." *Resource and Energy Economics* 26 (2): 237–54. https://doi.org/10.1016/j.reseneeco.2003.11.009.

Puyravaud, Jean-Philippe. 2003. "Standardizing the Calculation of the Annual Rate of Deforestation." *Forest Ecology and Management* 177 (1-3): 593–96. https://doi.org/10.1016/S0378-1127(02)00335-3.

Ribas, Luiz Guilherme dos Santos, Robert L. Pressey, Rafael Loyola, and Luis Mauricio Bini. 2020. "A Global Comparative Analysis of Impact Evaluation Methods in Estimating the Effectiveness of Protected Areas." *Biological Conservation* 246 (June): 108595. https://doi.org/10.1016/j.biocon.2020.108595.

Robalino, J., and A. Pfaff. 2013. "Ecopayments and Deforestation in Costa Rica: A Nationwide Analysis of PSA's Initial Years." *Land Economics* 89 (3): 432–48. https://doi.org/10.3368/le.89.3.432.

Ruggiero, Patricia G. C., Jean Paul Metzger, Leandro Reverberi Tambosi, and Elizabeth Nichols. 2019. "Payment for Ecosystem Services Programs in the Brazilian Atlantic Forest: Effective but Not Enough." *Land Use Policy* 82 (March): 283–91. https://doi.org/10.1016/j.landusepol.2018.11.054.

Sánchez-Azofeifa, G. Arturo, Alexander Pfaff, Juan Andres Robalino, and Judson P. Boomhower. 2007. "Costa Rica's Payment for Environmental Services Program: Intention, Implementation, and Impact." *Conservation Biology* 21 (5): 1165–73. https://doi.org/10.1111/j.1523-1739.2007.00751.x.

Shah, Payal, and Kathy Baylis. 2015. "Evaluating Heterogeneous Conservation Effects of Forest Protection in Indonesia." Edited by Dafeng Hui. *PLOS ONE* 10 (6): e0124872. https://doi.org/10.1371/journal.pone.0124872.

Sims, Katharine R. E., and Jennifer M. Alix-Garcia. 2017. "Parks Versus PES: Evaluating Direct and Incentive-Based Land Conservation in Mexico." *Journal of Environmental Economics and Management* 86 (November): 8–28. https://doi.org/10.1016/j.jeem.2016.11.010.

Tabor, Karyn, Kelly W. Jones, Jennifer Hewson, Andriambolantsoa Rasolohery, Andoniaina Rambeloson, Tokihenintsoa Andrianjohaninarivo, and Celia A. Harvey. 2017. "Evaluating the Effectiveness of Conservation and Development Investments in Reducing Deforestation and Fires in Ankeniheny-Zahemena Corridor, Madagascar." Edited by Mauro Villarini. *PLOS ONE* 12 (12): e0190119. https://doi.org/10.1371/journal.pone.0190119.

Wendland, K. J., M. Baumann, D. J. Lewis, A. Sieber, and V. C. Radeloff. 2015. "Protected Area Effectiveness in European Russia: A Postmatching Panel Data Analysis." *Land Economics* 91 (1): 149–68. https://doi.org/10.3368/le.91.1.149.

Williams, David R., Andrew Balmford, and David S. Wilcove. 2020. "The Past and Future Role of Conservation Science in Saving Biodiversity." *Conservation Letters* n/a (n/a): e12720. https://doi.org/10.1111/conl.12720.

Wooldridge, Jeffrey M. 2010. "Econometric Analysis of Cross Section and Panel Data, Second Edition."

10 Appendix

10.1 proof showing pixel level TWFE regression models with pixel fixed effects do not identify ATT

In settings with a binary and unrepeatable outcome variable, the commonly used pixel level TWFE model yields the post-treatment difference in outcomes (single difference), rather than the desired ATT.

proof:

Consider a two-period setting ($t = 1, 2$) with multiple pixels indexed by i. We observe y_{it}, the realized deforestation occurring in each pixel in each time period. Some units are exposed to a policy treatment ($D_i = 1$) in the second time period ($t_0 = 2$). Using the potential outcomes framework, we consider the potential outcomes for each observation as $y_{i,2}(D_i)$. In this notation, the treatment effect for unit i can be defined as

$$\tau_i = y_{i,2}(1) - y_{i,2}(0)$$

In many deforestation maps generated through remote sensing, deforestation is represented as a binary indicator. Furthermore, deforestation is only observed once for a given location since these data products typically do not monitor the timing of reforestation. Given these constraints, deforestation is typically (Alix-Garcia and Gibbs (2017); Jones and Lewis (2015)) represented as a binary, unrepeatable variable taking the following values:

$$y_{it} = \begin{cases} 0 & \text{the pixel has never been deforested} \\ 1 & \text{the pixel was deforested in year } t \\ NAN & \text{the pixel was deforested in a year } < t \end{cases}$$

The traditional two-way fixed effects model seeks to estimate this effect using the following regression specification:

$$y_{it} = \alpha + \beta_{TWFE} \text{ x } D_i \mathbb{1}\{t \geq t_0\} + \gamma_i + \eta_{t=2} + u_{it}, \text{ for } t = 1, 2$$

Taking into account the data structure of y_{it} and our two-period case, we are left with:

$$y_{i1} = \alpha + \gamma_i + u_{i1}$$

and

$$y_{i2} = \begin{cases} \alpha + \beta_{TWFE} \text{ x } D_i + \gamma_i + \eta_{t=2} + u_{i2} & y_{i1} = 0 \\ NAN & y_{i1} \neq 0 \end{cases}$$

First differencing,

$$y_{i2} - y_{i1} = \begin{cases} (\alpha + \beta_{TWFE} \text{ x } D_i + \gamma_i + \eta_{t=2} + u_{i2}) - (\alpha + \gamma_i + u_{i1}) & y_{i1} = 0 \\ NAN & y_{i1} \neq 0 \end{cases}$$

Focusing on the first case, where $y_{i1} = 0$

$$\begin{aligned} y_{i2} - y_{i1} &= (\alpha + \beta_{TWFE} \text{ x } D_i + \gamma_i + \eta_{t=2} + u_{i2}) - (\alpha + \gamma_i + u_{i1}) \\ &= \beta_{TWFE} \text{ x } D_i + \eta_{t=2} + \Delta u_i \end{aligned}$$

The general expression can be restated as:

$$y_{i2} - y_{i1} = \begin{cases} \beta_{TWFE} \text{ x } D_i + \eta_{t=2} + \Delta u_i & y_{i1} = 0 \\ NAN & y_{i1} \neq 0 \end{cases}$$

With binary treatment (D_i), $\hat{\beta}_{TWFE}$, the regression's estimate of β_{TWFE} can be expressed as the double difference in mean outcomes across treated / untreated units, and across the two time periods:

$$\hat{\beta} = \frac{1}{n_{i:D_i=1}} \sum_{i:D_i=1} y_{i2} - \frac{1}{n_{i:D_i=1}} \sum_{i:D_i=1} y_{i1} - \left(\frac{1}{n_{i:D_i=0}} \sum_{i:D_i=0} y_{i2} - \frac{1}{n_{i:D_i=0}} \sum_{i:D_i=0} y_{i1} \right)$$

However, this is only valid when $y_{i1} = 0$. As a result, we can restate as:

$$\hat{\beta}_{TWFE} = \frac{1}{n_{i:D_i=1}} \sum_{i:D_i=1} y_{i2} - 0 - \left(\frac{1}{n_{i:D_i=0}} \sum_{i:D_i=0} y_{i2} - 0 \right)$$

Applying the potential outcomes notation to indicate whether we see the treated or untreated outcome:

$$\hat{\beta}_{TWFE} = \frac{1}{n_{i:D_i=1}} \sum_{i:D_i=1} y_{i2}(1) - \frac{1}{n_{i:D_i=0}} \sum_{i:D_i=0} y_{i2}(0)$$

Adding and subtracting $\frac{1}{n_{i:D_i=1}} \sum_{i:D_i=1} y_{i2}(0)$ gives:

$$\hat{\beta}_{TWFE} = \frac{1}{n_{i:D_i=1}} \sum_{i:D_i=1} y_{i2}(1) - y_{i2}(0)$$
$$+ \frac{1}{n_{i:D_i=0}} \sum_{i:D_i=1} y_{i2}(0) - \frac{1}{n_{i:D_i=0}} \sum_{i:D_i=0} y_{i2}(0)$$

And finally, taking the expectation gives:

$$E[\hat{\beta}_{TWFE}] = ATT + Diff$$
$$\beta_{TWFE} = ATT + Diff$$
$$\blacksquare$$

10.2 Keeping pixels in periods after they are first deforested is not a viable solution

Remotely sensed metrics of deforestation at the pixel level are often subject to the dynamics of forest disturbance and regrowth. After a deforestation event occurs, the deforested area is unlikely to revert to forest cover within the study period, as it takes several years for forest to regenerate to a detectable level. Further, many data products do not allow for the monitoring of forest regrowth. In the panel therefore, it is likely that in the periods after a pixel is first realized as deforested, subsequent observations of the pixel will also observe the pixel as deforested.

The logic for dropping binary pixels after they first become deforested is as follows. A forested pixel switches from its assigned value of 0 to a value of 1 following a discrete deforestation event. Keeping the deforested pixel in the panel beyond the first period in which it was observed as deforested may imply that it has actively been deforested in each subsequent time period. In fact, no new deforestation event has ocurred, but the area simply remains deforested from the prior event. These pixels, therefore, contribute positively towards the deforestation rate in each period they are left in the panel. As such, the coefficient cannot recover the ATT.

10.2.1 Analytical expression of bias in two-period two-group case when pixels are not dropped from the panel

The DID estimand is

$$E[y_{it}|t \geq t_0, D_i = 1] - E[y_{it}|t < t_0, D_i = 1] - (E[y_{it}|t \geq t_0, D_i = 0] - E[y_{it}|t < t_0, D_i = 0])$$
$$= P(y_{it} = 1|t \geq t_0, D_i = 1) - P(y_{it} = 1|t < t_0, D_i = 1) -$$
$$(P(y_{it} = 1|t \geq t_0, D_i = 0) - P(y_{it} = 1|t < t_0, D_i = 0))$$

In the case of an irreversible binary outcome, the first and third terms can be reexpressed, giving

$$P(y_{it} = 1|t \geq t_0, D_i = 1) \cup P(y_{it} = 1|t < t_0, D_i = 1) - P(y_{it} = 1|t < t_0, D_i = 1) -$$
$$(P(y_{it} = 1|t \geq t_0, D_i = 0) \cup P(y_{it} = 1|t < t_0, D_i = 0) - P(y_{it} = 1|t < t_0, D_i = 0))$$
$$= P(y_{it} = 1|t \geq t_0, D_i = 1) + P(y_{it} = 1|t < t_0, D_i = 1) - P(y_{it} = 1|t \geq t_0, D_i = 1) \cap P(y_{it} = 1|t < t_0, D_i = 1) -$$
$$P(y_{it} = 1|t < t_0, D_i = 1) -$$
$$(P(y_{it} = 1|t \geq t_0, D_i = 0) + P(y_{it} = 1|t < t_0, D_i = 0) - P(y_{it} = 1|t \geq t_0, D_i = 0) \cap P(y_{it} = 1|t < t_0, D_i = 0)$$

10.2.2 Monte Carlo evidence

Figure 11 demonstrates the bias incurred from keeping deforested pixels in the panel after they are first realized as deforested in the context of our guiding example. Pixels that were deforested prior to the implementation of the policy continued to contribute to the deforestation rate in the post period in both the treatment and control groups. Dropping the pixels in the periods after they are first observed as deforested eliminates this bias in the DID model, as seen in Figure 11.

DID dropping pixels mean: -0.0018 RMSE: 0.00096
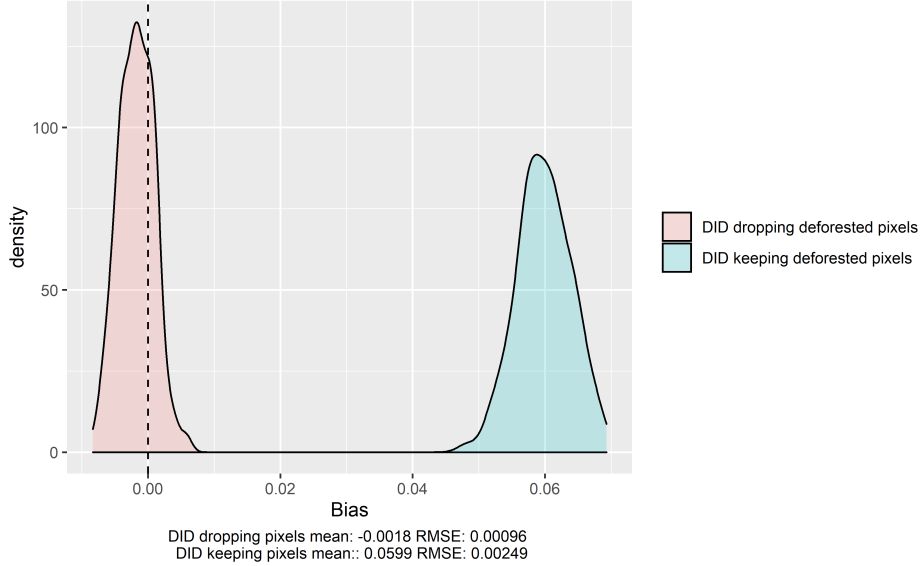DID keeping pixels mean:: 0.0599 RMSE: 0.00249

Figure 11: Distribution of DID estimates leaving deforested pixels in the panel and of DID estimates dropping deforested pixels. Note that leaving deforested pixels in the panel incurs severe bias.

10.3 Initial Monte Carlo parameter to β coefficient mapping

The following five parameters and their definitions inform the simulation parameterizations.

$$baseline_0 = E[y_{it}(0)|t < t_0, D_i = 0]$$
$$baseline_1 = E[y_{it}(0)|t < t_0, D_i = 1]$$
$$trend_0 = E[y_{it}(0)|t \geq t_0, D_i = 0] - E[y_{it}(0)|t < t_0, D_i = 0]$$
$$trend_1 = E[y_{it}(0)|t \geq t_0, D_i = 1] - E[y_{it}(0)|t < t_0, D_i = 1]$$
$$ATT = E[y_{it}(1) - y_{it}(0)|t \geq t_0, D_i = 1]$$

Note the following constraints on the parameters:

$$E[y_{it}(0)|t \geq t_0, D_i = 0] \geq 0$$
$$E[y_{it}(1)|t \geq t_0, D_i = 1] \geq 0$$

The parameters can be expressed as follows:

$$
\begin{aligned}
ATT =& E[y_{it}(1) - y_{it}(0)|t \ge t_0, D_i = 1] \\
=& E[y_{it}(1)|t \ge t_0, D_i = 1] - E[y_{it}(0)|t \ge t_0, D_i = 1] \\
=& P(y_{it}(1) = 1|t \ge t_0, D_i = 1) - P(y_{it}(0) = 1|t \ge t_0, D_i = 1) \\
=& P(y_{it}^*(1) > 0|t \ge t_0, D_i = 1) - P(y_{it}^*(0) > 0|t \ge t_0, D_i = 1) \\
=& P(\beta_0 + \beta_1 + \beta_{2,1} + \beta_3 + \alpha_i + u_{it} > 0) - P(\beta_0 + \beta_1 + \beta_{2,1} + \alpha_i + u_{it} > 0) \\
=& P(-\alpha_i - u_{it} < \beta_0 + \beta_1 + \beta_{2,1} + \beta_3) - P(-\alpha_i - u_{it} < \beta_0 + \beta_1 + \beta_{2,1}) \\
=& F(\beta_0 + \beta_1 + \beta_{2,1} + \beta_3) - F(\beta_0 + \beta_1 + \beta_{2,1})
\end{aligned}
$$

$$
\begin{aligned}
trend_0 =& E[y_{it}(0)|t \ge t_0, D_i = 0] - E[y_{it}(0)|t < t_0, D_i = 0] \\
=& P(y_{it}(0) = 1|t \ge t_0, D_i = 0) - P(y_{it}(0) = 1|t < t_0, D_i = 0) \\
=& P(y_{it}^*(0) > 0|t \ge t_0, D_i = 0|y_{it}^*(0) < 0|t < t_0, D_i = 0) - P(y_{it}^*(0) > 0|t < t_0, D_i = 0) \\
=& \frac{(1 - P(y_{it}^*(0) > 0|t < t_0, D_i = 0))P(y_{it}^*(0) > 0|t \ge t_0, D_i = 0)}{(1 - P(y_{it}^*(0) > 0|t < t_0, D_i = 0))} - P(y_{it}^*(0) > 0|t < t_0, D_i = 0) \\
=& P(-\alpha_i - u_{it} < \beta_0 + \beta_{2,0}) - P(-\alpha_i - u_{it} < \beta_0) \\
=& F(\beta_0 + \beta_{2,0}) - F(\beta_0)
\end{aligned}
$$

$$
\begin{aligned}
trend_1 =& E[y_{it}(0)|t \ge t_0, D_i = 1] - E[y_{it}(0)|t < t_0, D_i = 1] \\
=& P(y_{it}(0) = 1|t \ge t_0, D_i = 1) - P(y_{it}(0) = 1|t < t_0, D_i = 1) \\
=& P(y_{it}^*(0) > 0|t \ge t_0, D_i = 1 \cap y_{it}^*(0) < 0|t < t_0, D_i = 1) - P(y_{it}^*(0) > 0|t < t_0, D_i = 1) \\
=& P(-\alpha_i - u_{it} < \beta_0 + \beta_1 + \beta_{2,1}) - P(-\alpha_i - u_{it} < \beta_0 + \beta_1) \\
=& F(\beta_0 + \beta_1 + \beta_{2,1}) - F(\beta_0 + \beta_1)
\end{aligned}
$$

$$
\begin{aligned}
baseline_0 =& E[y_{it}(0)|t < t_0, D_i = 0] \\
=& P(y_{it}(0) = 1|t < t_0, D_i = 0) \\
=& P(y_{it}^*(0) > 0|t < t_0, D_i = 0) \\
=& P(-\alpha_i - u_{it} < \beta_0) \\
=& F(\beta_0)
\end{aligned}
$$

$$
\begin{aligned}
baseline_1 =& E[y_{it}(0)|t < t_0, D_i = 1] \\
=& P(y_{it}(0) = 1|t < t_0, D_i = 1) \\
=& P(y_{it}^*(0) > 0|t < t_0, D_i = 1) \\
=& P(-\alpha_i - u_{it} < \beta_0 + \beta_1) \\
=& F(\beta_0 + \beta_1)
\end{aligned}
$$

, Where $F()$ is the CDF of a $N(0, \sigma_a^2 + \sigma_u^2 + \sigma_p^2)$

Now solving for the β coefficients:

solving for β_0

$$baseline_0 = F(\beta_0)$$
$$\Leftrightarrow$$
$$\beta_0 = F^{-1}(baseline_0)$$

solving for β_1

$$baseline_1 = F(\beta_0 + \beta_1)$$
$$\Leftrightarrow$$
$$\beta_1 = F^{-1}(baseline_1) - \beta_0$$

solving for $\beta_{2,0}$

$$trend = F(\beta_0 + \beta_{2,0}) - F(\beta_0)$$
$$\Leftrightarrow$$
$$trend + baseline_0 = F(\beta_0 + \beta_{2,0})$$
$$\Leftrightarrow$$
$$F^{-1}(trend + baseline_0) = \beta_0 + \beta_{2,0}$$
$$\Leftrightarrow$$
$$\beta_{2,0} = F^{-1}(trend + baseline_0) - \beta_0$$

solving for $\beta_{2,1}$

$$trend = F(\beta_0 + \beta_1 + \beta_{2,1}) - F(\beta_0 + \beta_1)$$
$$\Leftrightarrow$$
$$trend + baseline_1 = F(\beta_0 + \beta_1 + \beta_{2,1})$$
$$\Leftrightarrow$$
$$F^{-1}(trend + baseline_1) = \beta_0 + +\beta_1 + \beta_{2,1}$$
$$\Leftrightarrow$$
$$\beta_{2,1} = F^{-1}(trend + baseline_1) - \beta_0 - \beta_1$$

solving for β_3

$$ATT = F(\beta_0 + \beta_1 + \beta_{2,1} + \beta_3) - F(\beta_0 + \beta_1 + \beta_{2,1})$$
$$\Leftrightarrow$$
$$ATT + F(\beta_0 + \beta_1 + \beta_{2,1}) = F(\beta_0 + \beta_1 + \beta_{2,1} + \beta_3)$$
$$\Leftrightarrow$$
$$F^{-1}(ATT + F(\beta_0 + \beta_1 + \beta_{2,1})) = \beta_0 + \beta_1 + \beta_{2,1} + \beta_3$$
$$\Leftrightarrow$$
$$\beta_3 = F^{-1}(ATT + F(\beta_0 + \beta_1 + \beta_{2,1})) - (\beta_0 + \beta_1 + \beta_{2,1})$$

with hetergeneous treatment effects

$$ATT = E(\beta_0 + \beta_1 + \beta_{2,1} + \beta_3) - E(\beta_0 + \beta_1 + \beta_{2,1})$$
$$= P(-\alpha_i - u_{it} - \beta_3 < \beta_0 + \beta_1 + \beta_{2,1} + \mu) - P(-\alpha_i - u_{it} < \beta_0 + \beta_1 + \beta_{2,1})$$
$$= G(\beta_0 + \beta_1 + \beta_{2,1} + \mu) - F(\beta_0 + \beta_1 + \beta_{2,1})$$

, where $\beta_3 \sim N(\mu, \sigma_{te}^2)$ and $G()$ is the CDF of a $N(0, \sigma_a^2 + \sigma_u^2 + \sigma_p^2 + \sigma_{te}^2)$ and

$$ATT = G(\beta_0 + \beta_1 + \beta_{2,1}) - F(\beta_0 + \beta_1 + \beta_{2,1})$$
$$\Leftrightarrow$$
$$ATT + F(\beta_0 + \beta_1 + \beta_{2,1}) = G(\beta_0 + \beta_1 + \beta_{2,1} + \mu)$$
$$\Leftrightarrow$$
$$G^{-1}(ATT + F(\beta_0 + \beta_1 + \beta_{2,1})) = \beta_0 + \beta_1 + \beta_{2,1} + \mu$$
$$\Leftrightarrow$$
$$\mu = G^{-1}(ATT + F(\beta_0 + \beta_1 + \beta_{2,1})) - (\beta_0 + \beta_1 + \beta_{2,1})$$

10.4 Calculating deforestation rates

Upon choosing an aggregated unit of analysis, the researcher must compute the deforestaion rate. This varies thoughout the literature, and many authors do not explicitly define the formula used. Different names are used to describe the calculation of the annual deforestation rate, which generates further confusion (Puyravaud 2003). We test the performance of three common deforestation rate formulas in the literature.

One common formula to calculate annual deforestation rates is

$$\text{Outcome 1)} = \frac{F_{i,t-1} - F_{it}}{F_{i,t-1}} \tag{8}$$

, where F_{it} and $F_{i,t-1}$ are the forest cover at times t and $t-1$, respectively. This calculation is used consistently in the literature (e.g. Carlson et al. 2018; Busch, Ferretti-Gallon, et al. 2015), and is arguably the most widely used formula. Some authors have also calculated the deforestation rate in relation to the initial observed level of forest cover, replacing F_{it} with F_{i0}, the baseline forest cover, in equation (1). This gives outcome 2):

$$\text{Outcome 2)} = \frac{F_{i0} - F_{it}}{F_{i0}} \tag{9}$$

Lastly, we consider a formula derived from the Compound Interest Law that has also been used in recent studies (e.g. Ruggiero et al. 2019; Puyravaud 2003). Outcome 3) is given by:

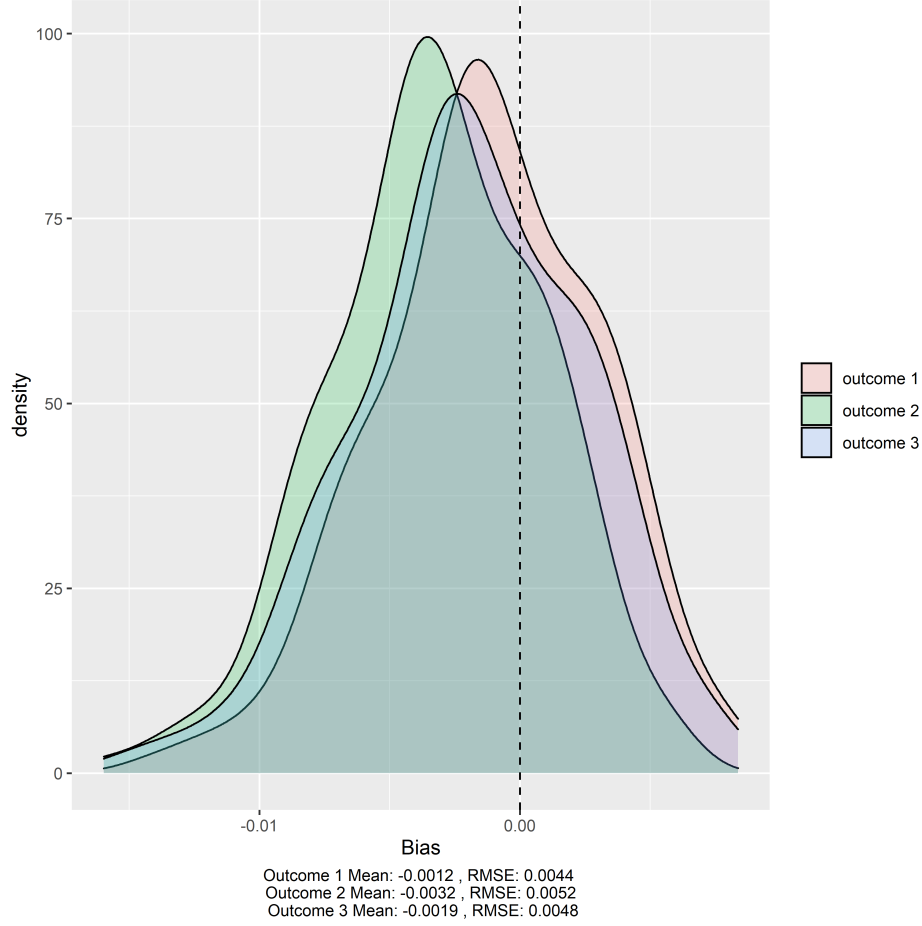$$\text{Outcome 3)} = ln(F_{i,t-1}/F_{it}) \tag{10}$$

Figure 12: Distribution of estimates produced by different outcome variable formulae

Figure 12 demonstrates that outcome 1 results in the least bias in our guiding example. The other outcomes result in relatively greater bias, although this difference seems minimal in our setting. RMSE is also comparable across the three outcomes.

We express concern surrounding the use of the initial observed baseline forest cover in the calculation of deforestation rates, as in formula (2). As the rate is calculated in periods further away from the baseline, the deforestation rate will be increasingly high, even if the rate has not changed over time. We demonstrate this issue in Figure 13 by examining how bias behaves as a function of the length of study period. Formula (2) is indeed the least robust to changes in the length of study period. We set $trend = 0$ and $ATT = 0$ to show a basic case where the intervention had no effect and deforestation rates in the treated and untreated group are stable though time.

Regardless of authors' choice of formula, we advise that this formula be explicitly stated in the paper. This will help to avoid confusion as to which formula was used and help researchers understand which methods are the standard within the literature. Moving forward in our paper, all specifications using aggregated data use outcome 1. In our guiding example, it resulted in the least bias and lowest RMSE, and it is currently the most common deforestation rate calculation used in the literature.

Here, we show that outcome formula 2) is problematic in our context. We examine the performance of the three outcomes in an example where $ATT = 0$ and $trend = 0$. This simple exercise allows us to gauge how the outcomes behave through time. As we see below, outcome 2) is the least robust to changes in the study period length, while outcomes 1) and 2) behave nearly identically.
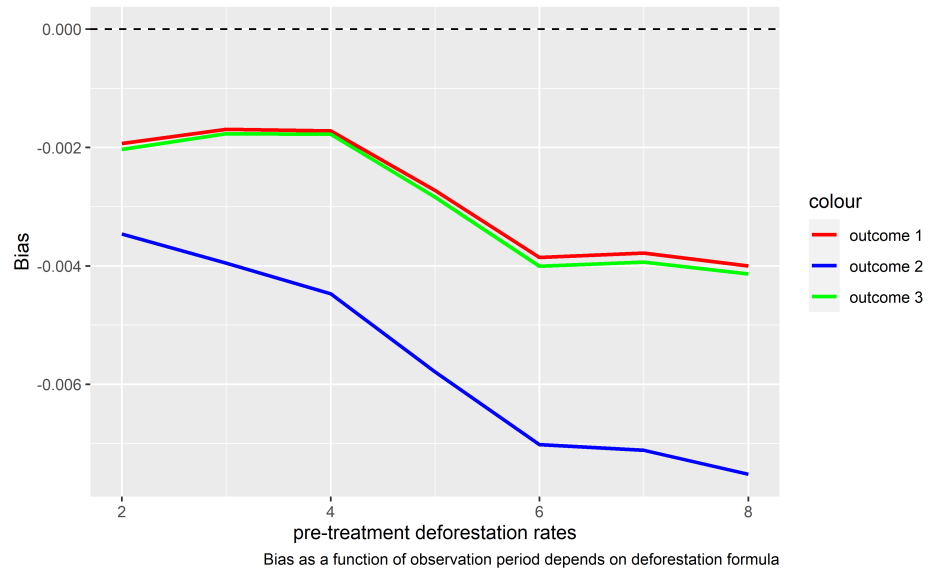
Bias as a function of observation period depends on deforestation formula

Figure 13: Bias as a function of study period length with ATT =0 and no time trend