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Clearing to Claim: Perverse Incentives of Land Regularization in the Brazilian Amazon

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Clearing to Claim: The Perverse Incentives of Land Regularization in the Brazilian Amazon*

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Abstract

Secure property rights are often promoted as a foundation for forest conservation, but the process of acquiring title can itself cause deforestation. We study this process in the Brazilian Amazon with a new panel of 1.09 million georeferenced properties and two decades of satellite-measured forest loss. We compare two federal titling programs that ran in the same region and years. One grants title for productive use at the time of inspection, the other for occupation before a fixed date in the past. Under the first, deforestation rises over the four years before title and peaks around the title year; under the second, it does not. In the self-declared environmental registry, landholders also redraw boundaries to fall just below a size threshold in conservation obligations, while verified cadastres show no such bunching. In both cases, landholders reshape the land, or the record of it, to meet the criteria the state uses to assign rights and obligations.

JEL codes: Q15, Q23, Q28, O13, H26.

Keywords: property rights, land titling, deforestation, Forest Code, bunching, difference-in-differences.

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1 Introduction

How should governments assign property rights to land when one goal is conservation? Formal title is among the most widely promoted policies in development, valued as a foundation for investment, credit, and, increasingly, environmental stewardship (de Soto, 2000; Deininger and Feder, 2009). A large empirical literature studies what happens to land use after rights are assigned, and its findings split by the form the rights take. Collective and customary formalization has generally protected forest (Blackman et al., 2017; Baragwanath and Bayi, 2020; Wren-Lewis et al., 2020), while individual titling has left deforestation unchanged or increased it (Liscow, 2013; Lipscomb and Prabakaran, 2020; Probst et al., 2020). The split reflects two opposing forces. Security lengthens horizons, so an owner who can appropriate the future returns to standing forest has less reason to exploit it quickly (Demsetz, 1967; Bohn and Deacon, 2000). Security also raises the returns to developing the land, which pushes use toward conversion (Besley, 1995; Goldstein and Udry, 2008; Galiani and Schargrodsky, 2010; Liscow, 2013). Which force dominates depends on institutional design.¹

This paper asks a prior question: how the rules for acquiring a right shape land use before the right is granted. Frontier land is rarely titled in a single step. Occupants spend years in a regularization process, and the state must decide what evidence establishes a valid claim and how costly a claim is to file. Whatever evidence it accepts becomes something claimants have reason to produce. In Brazil, as in many developing countries, the legal standard for converting occupation into title is demonstrated *productive use* of the land, and on a forested frontier the cheapest and most visible way to demonstrate productive use is to clear. Deforestation then becomes the evidence on which title is granted. We refer to this as clearing to claim.

Using title to reward land use is an old policy, and its incentives are well understood in theory. The 1862 U.S. Homestead Act granted 160 acres to settlers who lived on and cultivated a claim for five years. Allen (1991, 2019) argues that homesteading allowed the government to establish property rights on a frontier it could not otherwise police, at the cost of settlement that came earlier than the land’s value alone would justify, and Allen and Leonard (2021) show that the way land was acquired left lasting marks on land use. A century later, Brazil’s Estatuto da Terra (Law 4.504/1964) made productive use the basis both for claims to public land and for protecting private land from expropriation, and applied that standard to the largest tropical forest on Earth. Alston et al. (1999) model how this standard turns clearing into a signal of possession on the Amazon frontier, for squatters claiming land and for owners defending it against expropriation, and Angelsen (1999) shows formally that when clearing establishes a claim, the expectation of title raises the return to clearing early. Both predict that landholders will clear in anticipation of title. The evidence for this prediction is largely correlational (Alston et al., 2000), and the question of

¹BenYishay et al. (2017) find no average effect of indigenous demarcation in Brazil, and Pacheco and Meyer (2022) document that tenure–deforestation relationships vary sharply across socio-environmental contexts.

timing, whether landholders clear *because* title is coming, remains open. Answering it requires knowing when each claim was resolved, and observing comparable claimants for whom clearing earns nothing.

We provide that evidence and show that the design of the titling process determines whether clearing to claim occurs. We study Brazil’s Legal Amazon, where the federal government has run two land-titling programs with different eligibility rules. Under the first, title follows an on-site inspection that assesses productive use at the time of the visit.² This standard is forward-looking and makes clearing before the inspection privately optimal. Under the second, title is granted for occupation before a fixed date in the past, so current clearing cannot strengthen a claim.³ Both programs operated in the same region and period, were administered by the same agency, and ended in the same formal title. Comparing them lets us distinguish the productive-use requirement from selection into titling, frontier-wide shocks, and administrative cycles as explanations for clearing before title.

To make this comparison we build the first property-level panel of land tenure in the Brazilian Amazon. It harmonizes 1.09 million georeferenced parcels from four land registries and links each one to more than two decades of annual satellite measures of primary-forest loss.⁴ We exploit staggered titling dates with the [Callaway and Sant’Anna \(2021\)](#) difference-in-differences estimator, allowing for anticipation, and use the backward-looking program as a placebo.

Under the forward-looking program, deforestation rises over the four years before title, peaks in the title year and the year after, and returns to zero within five years. Estimates in the years just before the anticipation window are close to zero, consistent with parallel trends. The response is largest where title is most valuable, in high-obligation Amazon forest and among later titling cohorts. Applying the same estimator to the backward-looking program, we find no increase in clearing before title. This contrast is the paper’s central result. [Section 6.2](#) and [Appendix C](#) show that the null is precisely estimated and that the recorded titling dates are informative.

The cost of filing a claim matters for the same reason. When asserting a claim becomes cheap and is not verified, more occupants enter the regularization process, including many whose best route to secure possession is to establish facts on the ground before anyone else does. Digital land registries, now promoted worldwide as a scalable route to tenure security, lower the cost of claiming in exactly this way. We study Brazil’s version, a self-declared environmental registry created by the 2012 revision of the Forest Code, the law that sets conservation obligations on rural property. Holders draw their own boundaries online, the record becomes active without

²The program is INCRA’s georeferenced titling system, SIGEF. *Instituto Nacional de Colonização e Reforma Agrária* (INCRA) is Brazil’s federal land-reform agency; its *Sistema de Gestão Fundiária* (SIGEF) titles occupants of federal land.

³The second program is Terra Legal (Law 11.952/2009), which required current cultivation of the land but conditioned eligibility on occupation before December 2004.

⁴The four registries are SIGEF, the National Rural Cadastre (SNCI), Terra Legal, and the self-declared Rural Environmental Registry (CAR), administered through SICAR. [Section 4](#) and [Appendix A](#) describe the harmonization pipeline.

inspection, and registration is a step on the path to title. On undesignated federal land, where a registered polygon is often the only claim a holder ever makes, clearing rises in the years before registration by roughly five times the pre-title increase under the forward-looking program, and the increase appears even among parcels that never receive title.

Self-declaration also affects how holders report the size of their properties. In Brazil, a property’s size in fiscal modules determines how it is treated across land and environmental law, and four fiscal modules is the most consequential cutoff. Below it, holders are exempt from restoring forest cleared before 2008, register through a simplified procedure, qualify for family-farm credit, and can obtain title on the strength of their own declaration without a prior inspection. Above it, holders of Amazon forest land must restore their reserve to 80 percent of the property. The registry does not check declared sizes, so a holder can move below the threshold by redrawing a boundary. We find a large excess mass of declared parcels just below four fiscal modules. Because our linkage shows self-declared polygons drawn inside titled parent properties, we can also observe individual parents re-registered as several sub-threshold parcels. The excess mass appears only in the self-declared registry, and the titled registries, subject to the same rule, show essentially none. The comparison points to self-declaration as the source of the manipulation.

The paper contributes to the economics of property rights in two ways. First, existing quasi-experimental work estimates the effects of tenure security once it is granted (Blackman et al., 2017; BenYishay et al., 2017; Baragwanath and Bayi, 2020; Probst et al., 2020; Lipscomb and Prabakaran, 2020; Goldstein and Udry, 2008; Galiani and Schargrodsky, 2010). We provide, to our knowledge, the first causal evidence of the anticipatory clearing predicted by Alston et al. (1999), identified from staggered title dates with a backward-looking program as a placebo.⁵ The result also matters for measurement. When landholders respond before title arrives, designs that compare land use before and after the title date absorb part of the response into the baseline. They understate the clearing induced by the titling process and can make title itself appear to reduce deforestation. Second, we help reconcile the divided evidence on formalization (de Soto, 2000; Field, 2005, 2007; Galiani and Schargrodsky, 2010; Deininger and Feder, 2009). Whether titling protects forest or accelerates its conversion depends on how the program is designed. Programs that reward demonstrated use generate clearing on the path to title, and programs that tie eligibility to past behavior do not. We also document a new form of threshold manipulation, the subdivision of parcels on paper to qualify for lighter regulatory treatment, and observe it directly through cross-registry linkage, which is unusual in the bunching literature (Saez, 2010b; Chetty et al., 2011a; Kleven and Waseem, 2013). Its concentration in the self-declared registry is consistent with evidence that self-reported regulatory data are distorted when the regulated party controls the report (Duflo et al., 2013; Ghanem and Zhang, 2014).

⁵Alston et al. (2000) document the correlation between clearing and titling on the 1980s–1990s frontier, and Bohn and Deacon (2000) give cross-country evidence that ownership risk accelerates extraction. We isolate the forward-looking, strategic component with modern event-study methods and a placebo.

These findings bear directly on current policy. Brazil’s self-declared registry was named the first Digital Public Good for climate at COP30, and several countries have pledged to formalize rights over hundreds of millions of hectares, much of it in settings where occupation precedes title and productive use is the customary evidence of possession. The benefits of secure tenure, once reached, are well documented, and nothing in our results weighs against pursuing it. Our results show that the path to secure tenure carries environmental costs of its own, and that those costs depend on how the path is designed. Tying eligibility to occupation at a past date removes the incentive to clear for title, while verifying what claimants declare determines whether cheap registration extends secure rights or opens new loopholes.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting, and Section 3 presents a simple model of clearing to claim. Section 4 describes the data, Section 5 the empirical strategy, and Section 6 the results. Section 7 concludes.

2 Institutional Background

2.1 Land and Forest in the Brazilian Amazon

The Brazilian Legal Amazon⁶ spans roughly five million square kilometers, about ten times the area of California, and contains the world’s largest continuous tropical forest (Nobre et al., 2016; Lovejoy and Nobre, 2018). By 2020 approximately 800,000 km², or 17 percent of the region, had been cleared. Deforestation slowed sharply between 2004 and 2012 under the federal Action Plan for the Prevention and Control of Deforestation in the Legal Amazon (PPCDAm) and accelerated again after 2018 (Souza-Rodrigues, 2019; Ferrante and Fearnside, 2020).

Federal policy long encouraged settlement of this frontier. From the colonization programs of the military regime in the 1970s through the settlement initiatives of the federal land-reform agency (INCRA) in the 1980s and 1990s, the government granted rights to settlers who demonstrated occupation and productive use of the land (Alston et al., 1999, 1996). The result was a frontier of overlapping claims and informal occupation, in which contested title contributed directly to land-related violence (Fetzer and Marden, 2017). Conservation rules on private land were largely unenforced until PPCDAm began in 2004 (Souza-Rodrigues, 2019).

2.2 From Occupation to Title

Today an occupant of public land typically moves through three stages. The occupant first registers a self-declared claim in the Rural Environmental Registry (CAR, described in Section 2.5), later seeks legal recognition through one of the federal titling programs, and finally obtains a certified

⁶The official designation *Amazônia Legal* encompasses all seven states of Brazil’s North Region (Acre, Amapá, Amazonas, Pará, Rondônia, Roraima, and Tocantins), together with most of Mato Grosso and the western part of Maranhão.

property right. At each stage cleared land is worth more than standing forest. Cleared and registered parcels on public land are openly advertised for sale, sometimes with the registration, the georeferencing paperwork, and even outstanding environmental embargoes included, and the paperwork alone is priced at roughly \$2,000–\$3,000 per thousand hectares.

Figure 1 shows how these stages spread across the region between 2014 and 2020. The top row maps the expansion of deforestation, the middle row the expansion of titled private land in the three verified registries (SIGEF, Terra Legal, and SNCI, described below), and the bottom row the spread of CAR registrations. Titles and registrations expanded over the same frontier where deforestation was spreading, mostly along the Arc of Deforestation. Registration spread much faster and farther than title and reached large areas where ownership remains unresolved. Much of the frontier has therefore passed through an intermediate stage, claimed through low-cost registration and recognized in law only later, if at all.

Figure 2 shows the same sequence at the parcel level on the Altamira frontier of eastern Pará. The left panel dates the first forest loss at each location and the right panel the year each parcel was titled. Much of the land was cleared years before it was titled, so titles appear over a landscape that had already been converted. The question for this paper is how much of that earlier clearing the titling process itself induced.

2.3 Titling by Current Productive Use: SIGEF

The main route to title for occupants of undesignated federal land runs through INCRA’s georeferenced land-management system, the *Sistema de Gestão Fundiária* (SIGEF). Title follows an inspection in which an INCRA officer assesses whether the occupant meets the legal criteria for occupation and productive use. Under the Estatuto da Terra and later regularization law, productive use is the basis of the claim, and the extent of agricultural development, typically land cleared for pasture or crops, is the evidence on which title is granted. Because of INCRA’s administrative backlog, several years can pass between entering the process and the inspection. Throughout that period the occupant knows that the agricultural footprint visible on the day of the visit will shape the outcome. Section 3 formalizes the incentive this creates.

2.4 Titling by Past Occupation: Terra Legal

Terra Legal (Law 11.952/2009) regularized occupations of federal land in the Legal Amazon through the same agency. Applicants had to be farming the land when they applied, a requirement the law calls *cultura efetiva*, but the requirement that determined eligibility looked backward. Applicants had to show continuous occupation of the parcel since before December 1, 2004, documented through historical satellite imagery, utility records, or witness testimony.⁷ Because that

⁷Law 11.952/2009, Art. 5. Law 13.465/2017 later moved the occupation cutoff to July 22, 2008. Both dates precede every titling year in our sample (2013–2019), so under either rule current clearing could not establish

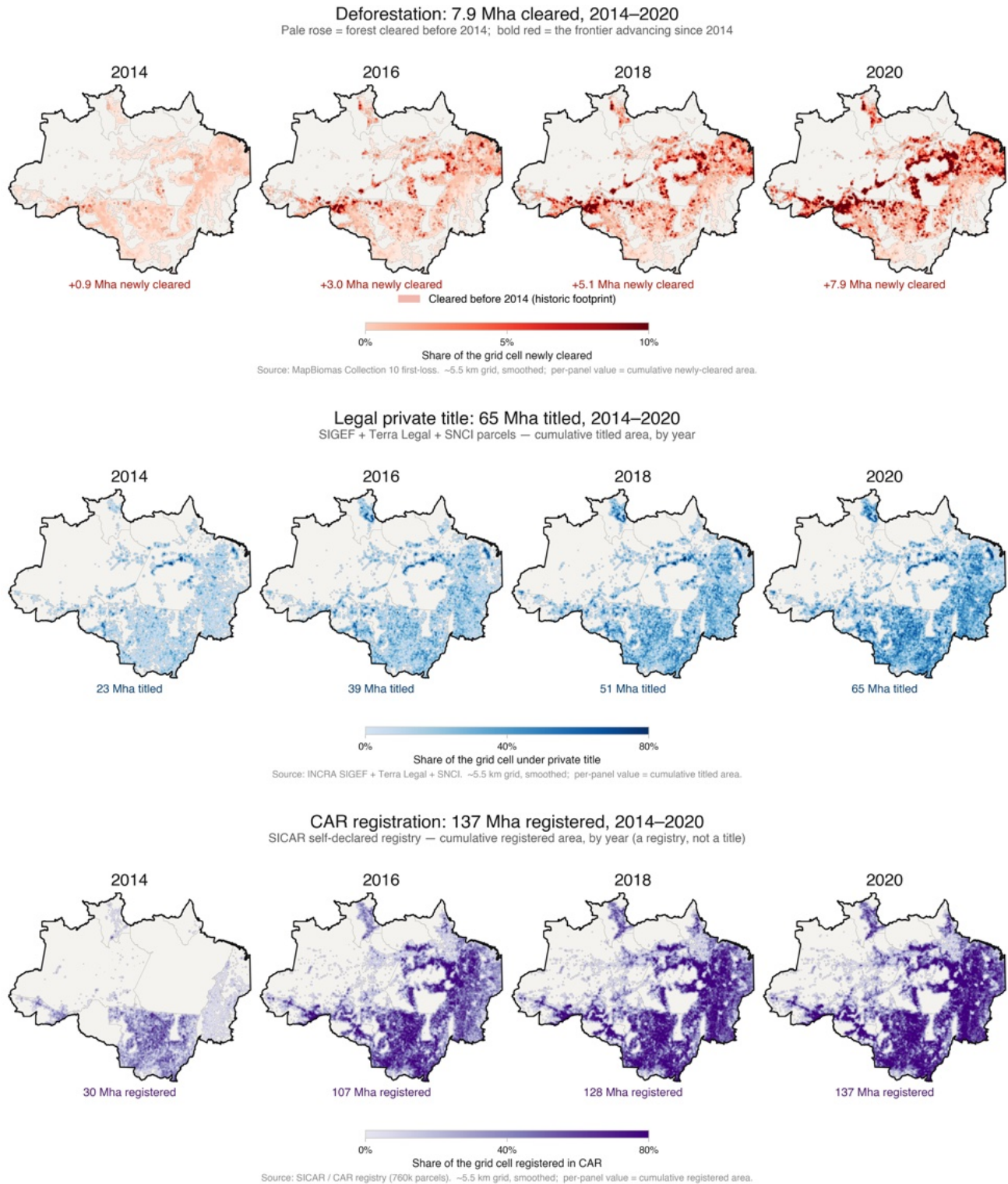


Figure 1: The Expansion of Deforestation, Formal Titles, and CAR Registration in the Legal Amazon

Notes: Each row shows the cumulative expansion of the respective layer in 2014, 2016, 2018, and 2020. Top row: native-forest loss from MapBiomass (clearing since 2014 in red, over the pre-2014 footprint in pale rose). Middle row: share of land under verified private title (SIGEF, Terra Legal, and SNCI). Bottom row: self-declared CAR (SICAR) registration. Each map reports the share of approximately 5.5 km grid cells covered by the respective layer, smoothed for visualization.

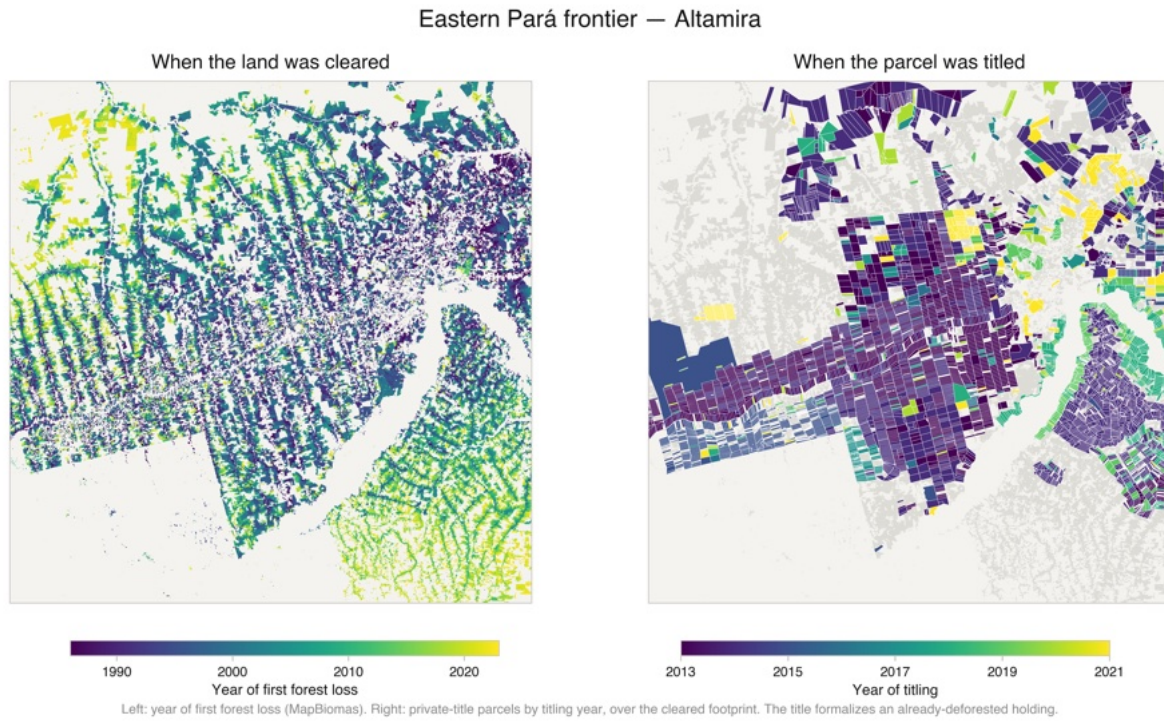


Figure 2: Clearing Precedes Titling: The Altamira Frontier in Eastern Pará

Notes: The left panel shows the year of first forest loss from MapBiomas. The right panel shows the year parcels were titled under the federal land cadastres. Titles generally appear after clearing has already occurred, illustrating the sequence from land clearing to formal property rights.

date had already passed when the program began, clearing after enrollment could not establish the occupation on which title depended. The program admitted claims of up to 15 fiscal modules (1,500 hectares), but most of its parcels are small, and in our data the mean Terra Legal parcel is about 90 hectares, just over one fiscal module (Table 2).

2.5 The 2012 Forest Code

The Forest Code (Law 12.651/2012) sets conservation obligations for all rural properties. Its 2012 revision changed the cost of claiming land and created a size threshold in those obligations.

CAR and the cost of claiming. The revision created the Rural Environmental Registry (*Cadastro Ambiental Rural*, CAR), a mandatory registry for all rural properties. To register, an owner or occupant draws the property boundary on a satellite-image interface, uploads documents identifying the holder and the claimed ownership or possession, and submits the record online.⁸ The record becomes active on submission. No field inspection takes place and nothing is checked against the land cadastre at the time of registration, and the subsequent review by state environmental agencies has been slow and incomplete. Registration confers no property right, but it has become part of the path to title, and since 2023 federal regularization explicitly requires an active CAR record (Decree 11.688/2023). On undesignated land a registered polygon is also widely treated as evidence of a claim in its own right. Entering the regularization process now costs little more than a taxpayer number, internet access, a few documents, and the ability to draw a polygon. This matters most on the roughly 60–70 million hectares of undesignated federal land in the Legal Amazon, where no one holds formal title.

The four-fiscal-module threshold. Property size, measured in fiscal modules (FM), determines many of a landholder’s obligations under Brazilian land and environmental law, and the most consequential cutoff is four FM.⁹ Below four FM, Article 67 waives the obligation to restore Legal Reserve cleared before July 2008, so the holder’s reserve is whatever native vegetation remained on that date. Above it, a holder on Amazon forest land must restore the reserve to 80 percent of the property. For a property that was largely cleared before 2008, crossing the threshold therefore changes its restoration liability by as much as 80 percent of its area. Properties below four FM also register in CAR through a simplified procedure and face lighter riparian restoration eligibility.

⁸Law 12.651/2012, Art. 29, §1, and Decree 7.830/2012. Art. 29, §2 states that registration is not a title and does not recognize ownership or possession.

⁹The fiscal module is a municipality-specific area unit calibrated to the minimum viable farm size, ranging from roughly 5 to 110 hectares. The provisions are Article 67 of the Forest Code (restoration exemption); Article 3, sole paragraph, and Decree 7.830/2012, Art. 8 (simplified registration); Article 61-A (riparian restoration); Law 11.326/2006 (family-farm status); and Law 11.952/2009, Art. 13 (regularization by declaration). [Soares-Filho et al. \(2014\)](#) estimate that the 2012 revision’s amnesty provisions erased roughly 58 percent of Brazil’s pre-existing restoration debt. Appendix A.3 details the legal rules and our pixel-level implementation.

rules, and the same cutoff governs eligibility for family-farm credit and for land regularization without a prior inspection. The threshold thus creates a notch (Kleven and Waseem, 2013), in which a small reduction in declared size produces a discrete fall in obligations.

In principle, the relevant size is the property’s area in July 2008, before any subdivision.¹⁰ In practice this is hard to enforce. CAR does not compare a declared polygon with earlier boundaries, and on undesignated public land there is no earlier boundary to compare it with. A holder can therefore move below the threshold by redrawing the property boundary, at essentially no cost. Holders in SIGEF and SNCI, whose boundaries are set through INCRA’s titling process, do not have this option. Appendix A.1 situates the registries within Brazil’s broader cadastral architecture.

3 Conceptual Framework

We organize the empirical work around a simple model of a frontier occupant who decides how much forest to clear while waiting for a title inspection.

An occupant holds a claim of area s_i that will be inspected in period T_i . In each period t the occupant clears d_{it} hectares at a convex cost $c(d_{it})$, and each cleared hectare yields agricultural rents r_i in every later period. Under a productive-use standard, the probability that the inspection grants title, $p(D_{iT_i}, s_i)$, increases with cumulative clearing at the time of the inspection, $D_{iT_i} = \sum_{t \leq T_i} d_{it}$. Title is worth V_i to the occupant, reflecting secure future rents, access to credit, and protection from eviction. The occupant chooses the path of clearing to maximize the discounted value of rents net of clearing costs, plus $\delta^{T_i} p(D_{iT_i}, s_i) V_i$, where δ is the discount factor. In any period before the inspection, optimal clearing satisfies

$$c'(d_{it}^*) = \underbrace{r_i \frac{\delta}{1-\delta}}_{\text{agronomic return}} + \underbrace{\delta^{T_i-t} p'(D_{iT_i}^*, s_i) V_i}_{\text{strategic return}} \quad (1)$$

where p' is the derivative of p with respect to cumulative clearing. The agronomic return, the discounted rents from an additional cleared hectare, is earned under any titling rule and is the same in every period. The strategic return is the increase in the expected value of title. A hectare cleared in any period before the inspection raises the probability of title by the same amount, but the payoff arrives only at T_i , so the strategic return is discounted by δ^{T_i-t} and rises as the inspection approaches. Because costs are convex, clearing rises with it. Once the claim is resolved the strategic return is zero, and clearing falls back to the level justified by agricultural rents alone. Uncertainty about the inspection date smooths this rise without removing it. The model yields four predictions.

¹⁰Forest Code, Article 12, §1, and Article 67.

Prediction 1 (Anticipation). *Under a productive-use standard, clearing rises as the inspection approaches and falls after the claim is resolved. Under a backward-looking rule, in which eligibility depends on occupation before a past date, p does not depend on current clearing, the strategic term is zero, and clearing shows no anticipatory rise.*

Terra Legal’s requirement that applicants be farming the land does not change this prediction. For most parcels it was verified by the applicant’s declaration, and any occupant already farming the land met it without further clearing.

Prediction 2 (Value). *Anticipatory clearing increases with the value of title V_i and with agricultural rents r_i . It should be larger in high-value frontier forest and in periods when formal title is worth more.*

Prediction 3 (Entry). *Lowering the cost of entering the claiming process draws in occupants who would not otherwise have entered, each of whom has an incentive to clear before the claim is assessed, so aggregate anticipatory clearing rises. Where registration itself confers de facto protection, as on undesignated public land, part of the value of a claim is realized at registration. Clearing should then rise ahead of the registration date, and it should appear even among claimants who never obtain title.*

Prediction 4 (Notch). *When obligations fall discretely below a size threshold and declared size can be changed at negligible cost, declared sizes bunch just below the threshold. Bunching should be absent in registries where boundaries are set through a titling process rather than drawn by the holder.*

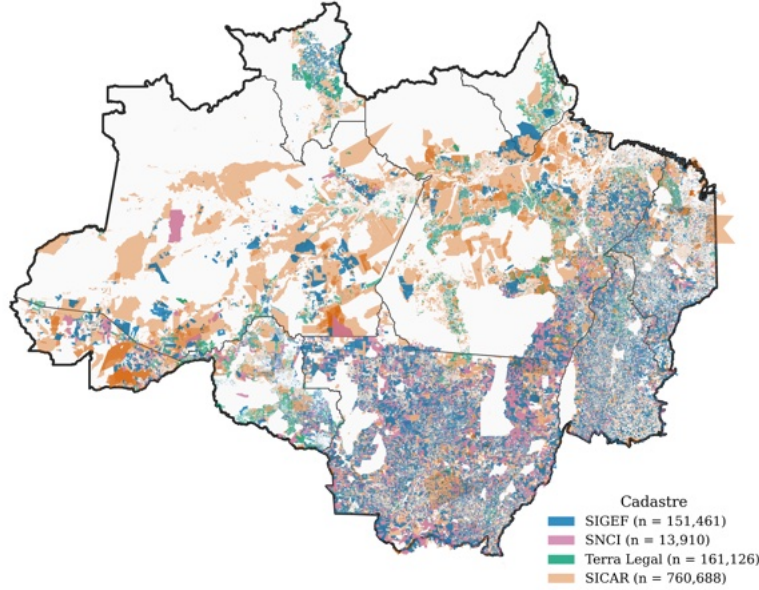
The threshold bundles several benefits (Section 2.5). One of them, the Article 67 restoration exemption, has value only for holders whose forest cover in 2008 was below the required reserve. If it is the main motive, bunching should be concentrated among parcels that were already largely cleared by 2008.

4 Data

Our analysis rests on a new property-level panel for the Legal Amazon. It combines four parcel-level land registries, three of titled land (SIGEF, SNCI, and Terra Legal) and one of self-declared claims (SICAR, the database behind CAR), and links each parcel to annual primary-forest loss. Figure 3 maps the combined set of parcels. We need all four registries because the paper’s three empirical exercises have different data requirements. The event studies require titling and registration dates, the bunching analysis requires the distribution of declared sizes within each registry, and the subdivision accounting requires linking self-declared polygons to the titled parcels that contain them. This section summarizes the data, and Appendix A describes the harmonization in full.

Cadastre overlay (Legal Amazon)

Titled private (SIGEF, SNCI, Terra Legal) + self-declared registry (SICAR)
n = 1,087,185 parcels total



Sources: INCRA SIGEF/SNCI; Camara et al. (2023) + IMAFLORA for Terra Legal; Brazilian Forest Service for SICAR. Harmonised by author.

Figure 3: Property Polygons Across the Four Land Registries

Notes: The 1.09 million parcel polygons in the harmonized database, by registry of origin (SIGEF, SNCI, Terra Legal, SICAR), Legal Amazon. Registry-specific maps are reported in Appendix A.

4.1 Land Registries

We build on the harmonization framework of [Camara et al. \(2023\)](#) and [Shinde \(2022\)](#). A common processing pipeline validates and repairs geometries, standardizes attributes, resolves duplicates, and reconciles parcels across registries, so that the same landholding can be followed through successive stages of registration and titling. The resulting database contains 1.09 million georeferenced parcels.

SIGEF, obtained directly from INCRA, contains 151,461 certified parcels titled between 2013 and 2021 (median 2018). Each record includes the parcel polygon, the titling date, and the parcel’s area. SIGEF parcels form the treated group in the main event study.

SNCI, INCRA’s legacy rural cadastre, predates SIGEF. Because historical SNCI records are not consistently georeferenced, we reconstruct representative parcel geometries following [Shinde \(2022\)](#). The sample contains 13,910 parcels titled between 2004 and 2018 (median 2012), which are larger and older than SIGEF parcels. Because they were titled under the legacy system rather than through SIGEF, we use them as never-treated controls in the SIGEF design.

Terra Legal combines two sources. We begin with the compilation of [Camara et al. \(2023\)](#), which covers the Amazon biome, and extend it with a May 2026 download from INCRA’s Terra Legal registry that adds post-2019 records and parcels in the Legal Amazon outside the biome. Parcels that appear in both sources are reconciled by geometric matching (Appendix A). The

database contains 161,126 Terra Legal parcels, of which 96,188 have a valid titling date, concentrated between 2013 and 2019 (median 2014). The parcels with titling dates form the treated group in the placebo design, and Terra Legal parcels also serve as a second never-treated control group in the SIGEF design.¹¹

SICAR, maintained by the Brazilian Forest Service, is the database of CAR registrations and by far the largest registry, with 760,688 self-declared parcels registered between 2014 and 2020 (median 2016). The public portal releases parcel geometries but not bulk registration dates, so we merge the polygon layer with parcel-level registration dates and attributes assembled by [Shinde \(2022\)](#). Because registration is self-declared and confers no property right, SICAR records claims as they are first made, before any formal title.

Cross-registry linkage and parent-child matching. Because the database keeps each registry’s records rather than merging overlapping polygons into a single layer, we can link records that describe the same landholding. This lets us follow properties from self-declared registration to formal title, and identify titled properties that were later re-registered under new boundaries. The linkage is what allows us to observe subdivision directly, rather than infer it from the distribution of declared sizes.

We assign each SICAR polygon to a titled parent if at least 50 percent of its area falls inside a SIGEF, SNCI, or Terra Legal parcel. Federal settlement projects are excluded, because each collective polygon contains many family lots and would generate mechanical matches. Of the 748,129 SICAR polygons that pass geometry checks, 192,135 (25.7 percent) are linked to a titled parent and 555,994 (74.3 percent) are *orphans* with no titled parent (Table 1). The distinction between linked and orphan parcels, together with the order of registration and titling, underlies both the subdivision accounting and the SICAR event studies.

Table 1: SICAR-to-Titled-Parent Linkage in the Legal Amazon

	SICAR children	Share of linked SICAR
Linked to SIGEF parent	100,859	52.5%
Linked to Terra Legal parent	68,359	35.6%
Linked to SNCI parent	22,917	11.9%
Any titled parent	192,135	100.0%
Orphan (no titled parent)	555,994	
SICAR rows in linkage table (N)	748,129	
SICAR polygons in raw extract	760,688	

Notes: A SICAR polygon is assigned to a titled parent if $\geq 50\%$ of its area falls inside a SIGEF, SNCI, or Terra Legal polygon. Settlement projects (*projetos de assentamento*) are excluded from parent matching because each collective polygon contains many family lots. The 12,559 SICAR polygons in the raw extract that do not appear in the linkage table fail geometry-validity or topology checks.

¹¹Parcels without titling dates enter only as never-treated controls in the SIGEF specification.

4.2 Forest Loss

Our primary outcome is annual primary-forest loss as a percentage of the parcel’s 1985 primary-forest stock. We compute it by intersecting each parcel polygon with the annual 30-meter land-cover maps of MapBiomass Collection 8 (Souza-Rodrigues, 2019). The baseline excludes secondary regrowth, so that changes in regrowth are not mistaken for clearing.

4.3 Covariates

Fiscal modules and Legal Reserve requirements. We take municipal fiscal-module (FM) values from INCRA’s official 2013 table, which gives the size of the fiscal module in hectares for each of Brazil’s 5,569 municipalities. Each property is assigned the FM value of the municipality containing its centroid, and its size in fiscal modules is its area divided by that value. This measure determines the property’s position relative to the four-FM threshold described in Section 2.5.

To measure Legal Reserve (LR) obligations, we construct a new 30-meter raster of the legally applicable LR requirement across the Legal Amazon. Within the Amazon biome, requirements come from the legal-zone classification of Camara et al. (2023). Outside the biome but within the Legal Amazon, we classify native vegetation in the IBGE 1:250,000 vegetation map into the Forest Code’s 20, 35, and 80 percent categories. Public lands and protected territories are added from official layers. Each pixel is assigned one of five requirements (20, 35, 50, 80, or 100 percent), and we use the raster in the heterogeneity analysis (Appendix A.3).

Arc of Deforestation. Following standard practice, we classify Pará, Mato Grosso, Rondônia, and Maranhão as the Arc of Deforestation.

Baseline depletion. For each parcel we compute the share of its 1985 forest cleared by 2004, the year PPCDAm began, and include the parcel’s within-sample decile of this share as a covariate to control for pre-existing trajectories.

Land designation. We classify a SICAR parcel as on undesignated federal land if at least 50 percent of its area falls within undesignated polygons of the national public-forest registry (CNFP). This applies to 56,470 parcels, or 8.5 percent of the SICAR panel.

4.4 Summary Statistics

Table 2 reports summary statistics by registry. The registries differ markedly in parcel size. SIGEF parcels average 404 hectares and SICAR parcels 230 hectares, SNCI parcels are much larger (2,251 hectares) because they include legacy estates, and Terra Legal parcels are the smallest (90 hectares, just over one fiscal module).¹² Between 69 and 80 percent of parcels in each registry lie in the Arc of Deforestation. Cumulative clearing before 2004 is highest among SICAR and Terra Legal

¹²Panel A covers all parcels in each raw extract, including the Terra Legal parcels without a titling date. Panel B covers the parcels in the event-study panels.

parcels (about 22 percent of the 1985 stock) and lowest among SIGEF and SNCI parcels (about 16 percent).

Table 2: Summary Statistics by Land Registry

	SIGEF (1)	SNCI (2)	Terra Legal (3)	SICAR (4)
<i>Panel A: Property characteristics (raw extract)</i>				
Area (ha), winsorized 1/99 mean	404.1	2,251.3	89.7	230.2
standard deviation	(734.7)	(3,455.6)	(172.2)	(608.4)
Area (fiscal modules), winsorized mean	5.20	28.18	1.28	3.02
standard deviation	(9.47)	(42.86)	(2.44)	(7.88)
Share in Arc of Deforestation (%)	74.0	80.4	69.4	75.0
<i>Panel B: 1985 baseline forest (DiD-panel parcels)</i>				
1985 primary forest cover (km ²), mean	3.05	14.56	0.78	3.08
median	0.59	4.08	0.39	0.44
Cumulative primary loss by 2004 (% of 1985), mean	16.5	15.5	22.2	21.9
median	4.5	4.0	14.3	12.2
<i>Panel C: Timing</i>				
Title / registration year, range	2013–2021	2004–2018	2013–2019	2014–2020
Title / registration year, median	2018	2012	2014	2016
<i>Panel D: Sample sizes</i>				
Polygons in raw extract	151,461	13,910	161,126	760,688
Parcels in DiD panel (post QC)	133,317	13,275	88,401	664,896

Notes: Standard deviations in parentheses. Areas winsorized at the 1st/99th percentiles within registry. Forest cover computed from MapBiomass Collection 8 over each polygon. The Arc of Deforestation comprises Pará, Mato Grosso, Rondônia, and Maranhão. “Parcels in DiD panel” removes parcels with missing baseline forest, missing covariates, or invalid geometry; the Terra Legal panel additionally drops parcels without a titling date.

5 Empirical Strategy

5.1 Staggered Difference-in-Differences with Anticipation

We estimate the effect of titling on clearing with the staggered difference-in-differences estimator of [Callaway and Sant’Anna \(2021\)](#). Let G_i denote the year parcel i is titled (its cohort), with $G_i = 0$ for parcels that are never treated, and let $Y_{it}(g)$ and $Y_{it}(\infty)$ denote potential outcomes when the parcel is first treated in year g and when it is never treated. The group-time average treatment effect is

$$ATT(g, t) = \mathbb{E}[Y_{it}(g) - Y_{it}(\infty) \mid G_i = g], \quad (2)$$

which we aggregate to event-study coefficients at event time $e = t - g$, weighting cohorts by size.

Because the mechanism operates before the title date, we allow for anticipation of $L = 4$

years, which moves the base period to $e = -5$. The coefficients at $e = -4, \dots, -1$ then measure anticipatory clearing, and those at $e = -8, \dots, -6$ test for parallel trends before the anticipation window opens. In the preferred specification the controls are never-treated parcels from SNCI and Terra Legal, which avoids comparisons between treated cohorts, and a robustness specification uses not-yet-treated SIGEF cohorts instead. Within each cohort and year we use the doubly robust estimator, which combines inverse-probability weighting with regression adjustment, conditioning on log area, fiscal-module category, baseline-depletion decile, and an Arc of Deforestation indicator. Standard errors come from a multiplier bootstrap with 1,000 replications, clustered by parcel, and event-study figures show simultaneous 95 percent confidence bands.

5.2 Identification and the Terra Legal Placebo

The identifying assumption is that, conditional on covariates, titled and control parcels would have followed parallel clearing paths in the absence of titling, apart from the four years of anticipation that the design allows. Two kinds of violation are most plausible. Parcels that enter titling may differ systematically from controls, for example because occupants who seek title are already expanding production. Titling dates may also coincide with shocks that raise clearing on their own, such as frontier booms, changes in enforcement, or waves of administrative activity at INCRA. Flat coefficients before the anticipation window address the first concern but not the second.

The Terra Legal program addresses both. It titled occupants of federal land in the same region and years, through the same agency, but its eligibility rule turned on occupation before a fixed date in the past (Section 2.4). Selection into titling, frontier-wide shocks, and administrative cycles would affect Terra Legal parcels as well. If any of them produced the pre-title rise under SIGEF, the same rise should appear before Terra Legal titles. The productive-use requirement, by contrast, predicts no rise (Prediction 1). We therefore apply the same estimator, with the same anticipation window and covariates, to Terra Legal parcels with titling dates, using SNCI parcels as controls.

Two features of the comparison limit what it can show. Terra Legal parcels are smaller and more depleted than SIGEF parcels (Table 2), so the comparison concerns the shape of the pre-title profile rather than the level of the estimates. And a null result is informative only if the design could have detected a rise and the titling dates are accurate. Appendix C examines both, showing that the anticipation-window estimates are precise and that a placebo-in-time exercise recovers the recorded title year.

5.3 Registration Events

The SICAR analyses use the same estimator with registration in place of titling. The treated group consists of SICAR parcels, and each parcel’s cohort is its registration year. Our main specification

focuses on parcels on undesignated federal land, where registration is most likely to be a claim, and uses never-treated titled parcels as controls. Because registrants on undesignated land differ from titled parcels, we also report specifications that use not-yet-registered SICAR parcels as controls (Appendix D). Those controls are themselves future claimants who may be clearing ahead of their own registration, so this comparison is conservative.

5.4 Bunching at Four Fiscal Modules

To measure excess registrations just below four fiscal modules we follow the bunching literature (Saez, 2010a; Chetty et al., 2011b; Kleven and Waseem, 2013; Bertanha et al., 2023). Let $f(z)$ be the empirical density of declared property sizes in fiscal modules. We fit a flexible polynomial to binned counts in a window around the cutoff $z^* = 4$, excluding a narrow band around z^* , and use the fitted counterfactual $\hat{f}_0(z)$ to estimate the excess mass

$$\hat{B} = \int_{z^* - \underline{\Delta}}^{z^*} [f(z) - \hat{f}_0(z)] dz. \quad (3)$$

We report $\hat{B}$ in levels, as excess registrations, and relative to the counterfactual mass. Inference is by nonparametric bootstrap over parcels. We complement the polynomial baseline with slope-constrained bounds following Bertanha et al. (2023) and a density-discontinuity test (McCrary, 2008), and we examine sensitivity to bin width, polynomial degree, the excluded band, and the estimation window (Appendix E).

5.5 Subdivision Accounting

The bunching estimator measures how the declared size distribution is distorted, and the registry linkage lets us observe subdivisions directly. When SICAR polygons lie inside the boundary of a titled parent, the pair is a re-registration of titled land, so counting such pairs requires no statistical inference. For each titled parent above four fiscal modules with at least one SICAR child below the threshold, we compute the Legal Reserve obligation implied by the parent’s size and by the children’s declared sizes, and sum the difference across pairs and by municipality. Because the calculation requires a titled parent, it is a lower bound. It omits claims that were registered as small fragments from the outset and subdivision on undesignated land.

Finally, to examine whether the threshold affects land use and not only registration, we estimate regression-discontinuity and difference-in-discontinuity designs at four fiscal modules, comparing the discontinuity in deforestation before and after 2012. Appendix F reports the design and results.

6 Results

6.1 Clearing before Title under the Productive-Use Standard

Figure 4 plots annual primary-forest loss on SIGEF parcels by year relative to title. Clearing rises in the years before title and falls after it. Figure 5 and Table 3 report the event-study estimates.

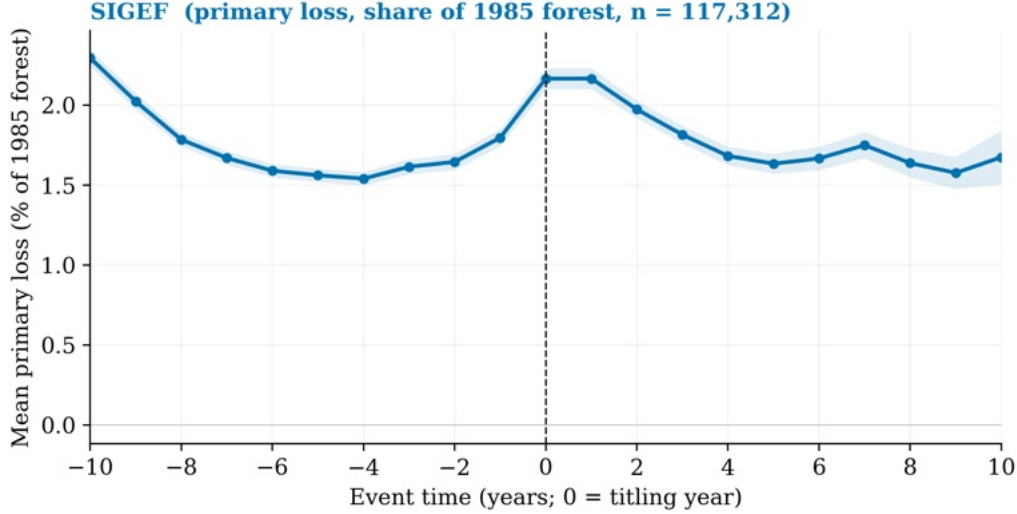


Figure 4: Primary-Forest Loss Around SIGEF Titling: Raw Pattern

Notes: Within-parcel annual primary-forest loss (% of 1985 baseline stock) by year relative to SIGEF titling, pooled across titling cohorts.

In the preferred specification (Column 2), the coefficients at $e = -7$ and $e = -6$ are small and statistically indistinguishable from zero, so clearing on SIGEF parcels and controls moved together in the years just before the anticipation window. The coefficient at $e = -8$ is negative and significant (-0.053), which indicates some relative increase in clearing on SIGEF parcels between eight and seven years before title. In the specification with not-yet-treated controls (Column 3) this coefficient is smaller (-0.018) and only marginally significant.

Within the anticipation window the estimates rise from 0.019 percentage points of the 1985 forest stock at $e = -4$ to 0.062 at $e = -1$, and all four are significant. They average about 0.04 percentage points a year, and summed over the window, excess clearing before title amounts to about 0.16 percent of the 1985 stock. The largest effects come in the title year and the year after (0.145 and 0.152). Clearing stays elevated for three years after title, which may reflect both the completion of clearing begun before the inspection and the higher return to investment that title brings (Probst et al., 2020; Liscow, 2013), and falls to zero by $e = 5$. The two-way fixed-effects specification in Column 1 sets $e = -1$ as its reference year. Because that year is itself a year of anticipatory clearing, the specification produces negative pre-period coefficients and understates the effects, which is why the design allows for anticipation.

Table 3: Dynamic Treatment Effects SIGEF title with Anticipation Window

	Outcome: Primary forest loss (% of 1985 baseline)		
	(1) Baseline	(2) Preferred	(3) Robustness
<i>Pre-anticipation placebo (parallel-trends test)</i>			
Event time $e = -8$	-0.139*** (0.013)	-0.053*** (0.011)	-0.018* (0.011)
Event time $e = -7$	-0.113*** (0.012)	-0.011 (0.009)	0.012 (0.010)
Event time $e = -6$	-0.115*** (0.012)	-0.006 (0.009)	0.001 (0.009)
Event time $e = -5$	-0.085*** (0.011)	0.000 ref.	0.000 ref.
<i>Anticipation window (treatment effects allowed)</i>			
Event time $e = -4$	-0.066*** (0.011)	0.019** (0.008)	0.020** (0.009)
Event time $e = -3$	-0.034*** (0.011)	0.047*** (0.010)	0.047*** (0.010)
Event time $e = -2$	-0.037*** (0.010)	0.031*** (0.011)	0.028*** (0.011)
Event time $e = -1$	0.000 ref.	0.062*** (0.012)	0.062*** (0.012)
<i>Post-titling effects</i>			
Event time $e = 0$	0.119*** (0.012)	0.145*** (0.012)	0.146*** (0.014)
Event time $e = 1$	0.127*** (0.013)	0.152*** (0.014)	0.153*** (0.014)
Event time $e = 2$	0.068*** (0.013)	0.118*** (0.013)	0.121*** (0.014)
Event time $e = 3$	0.010 (0.012)	0.123*** (0.012)	0.124*** (0.013)
Event time $e = 4$	-0.029** (0.013)	0.042*** (0.013)	0.042*** (0.013)
Event time $e = 5$	-0.067*** (0.013)	-0.025* (0.014)	-0.025** (0.013)
Event time $e = 6$	-0.063*** (0.015)	-0.020 (0.016)	-0.020 (0.015)
Event time $e = 7$	-0.042*** (0.016)	0.025 (0.017)	0.025 (0.017)
Anticipation window	0	4 periods	4 periods
Control group	SNCI + TL never-treated	SNCI + TL never-treated	Not-yet-treated
Unit fixed effects	Yes	—	—
Time fixed effects	Yes	—	—
Year $\times$ baseline-decile FE	Yes	—	—
UF $\times$ year FE	Yes	—	—
Covariates (LR%, FM, baseline q)	No	Yes	Yes
Estimator	TWFE event study	Callaway–Sant’Anna (DR)	Callaway–Sant’Anna (DR)
Inference	Cluster bootstrap	Cluster bootstrap	Cluster bootstrap
Cluster level	Parcel	Parcel	Parcel
Number of clusters	232,042	232,042	232,042
Number of observations	6,961,260	6,961,260	6,961,260
Bootstrap replications	1000	1000	1000
Random seed	2006	2006	2006

Notes: Dynamic treatment effects of SIGEF titling on within-parcel primary forest loss (% of 1985 baseline forest stock). The treated cohort is SIGEF parcels (anchor = titling year). Column (1) is a TWFE event study with parcel, year $\times$ baseline-decile, and UF $\times$ year fixed effects, omitting $e = -1$. Columns (2) and (3) follow Callaway–Sant’Anna with a four-period anticipation window (`anticipation = 4`, `base_period = ‘universal’`); the omitted base period is therefore $e = -5$. The preferred specification uses SNCI and Terra Legal parcels as never-treated controls; the robustness specification uses not-yet-treated SIGEF cohorts as controls. The estimation sample comprises 232,042 parcels (6,961,260 parcel-year observations) after dropping rows with missing covariates: 133,200 treated SIGEF parcels (titling cohorts 2013–2021) and 98,842 never-treated parcels (13,214 SNCI and 85,628 Terra Legal). Column (2) draws its comparison units from the 98,842 never-treated parcels only; column (3) additionally uses not-yet-titled SIGEF cohorts as controls, contributing comparison observations in the years before their own anticipation window opens (own event time $e < -4$). Standard errors are cluster-robust at the parcel level via multiplier bootstrap. The top block reports placebo coefficients in years $e = -8..-5$ before the anticipation window opens (a parallel-trends test: these should be small and statistically indistinguishable from zero in the CS columns). The middle block reports anticipatory clearing in the four years preceding titling; the bottom block reports post-titling effects. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

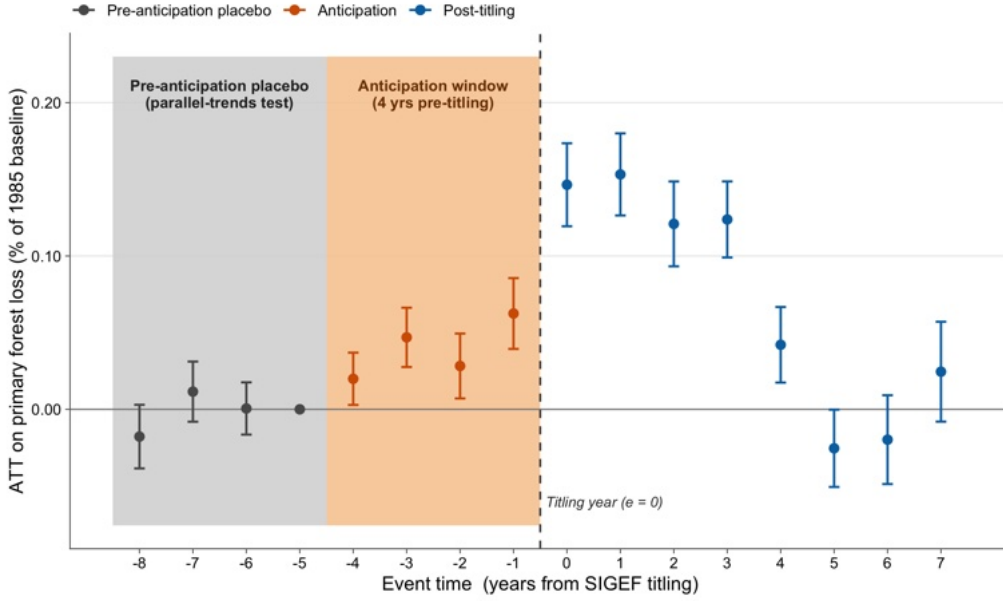


Figure 5: Event Study: SIGEF Titling and Anticipatory Deforestation

Notes: Callaway–Sant’Anna event study for primary-forest loss (% of 1985 baseline). Treated: SIGEF parcels (cohort = title year). Controls: never-treated SINC and Terra Legal parcels (Column 2 of Table 3). The version with not-yet-treated controls (Column 3) is in Figure B.1. Anticipation window $L = 4$, base period $e = -5$, doubly robust estimator with covariates. Bands are simultaneous 95% confidence intervals from a 1,000-replication multiplier bootstrap clustered by parcel.

Heterogeneity. Prediction 2 implies that anticipatory clearing should be larger where title is worth more. Figure 6 shows that the pre-title rise is concentrated among parcels in Amazon forest facing the 80 percent Legal Reserve requirement, with smaller effects in lower-obligation cerrado areas. These parcels have more standing forest, and on them the gain from demonstrating productive use is plausibly largest. Appendix B shows that anticipation is also strongest among later titling cohorts and among the smallest parcels. The rise among later cohorts coincides with the tightening of land governance in the Amazon after 2008, including municipal blacklists and stronger monitoring under PPCDAm, which plausibly raised the value of formal title.

6.2 The Terra Legal Placebo

Under Terra Legal’s backward-looking eligibility rule, Prediction 1 implies that clearing should not rise before title. Figure 7 shows the raw pattern and Figure 8 the event study.

Terra Legal parcels show no rise in clearing before title. Their clearing instead declines slowly relative to controls through the pre-title years, with no increase in the anticipation window and no peak at the title date. Because the decline begins in the far pre-period, we do not interpret the Terra Legal coefficients as causal effects. The most likely explanation is composition. Eligibility required occupation before 2004, which selected parcels that had already cleared much of their forest (Table 2), and their clearing rates fell as the remaining stock shrank. What matters for

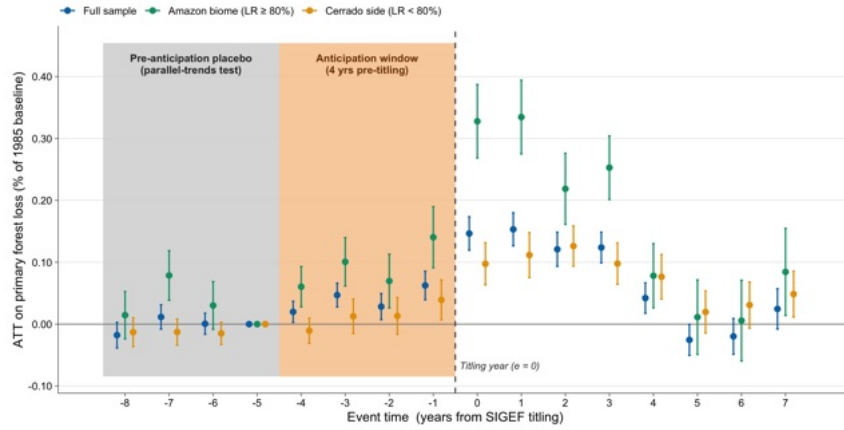
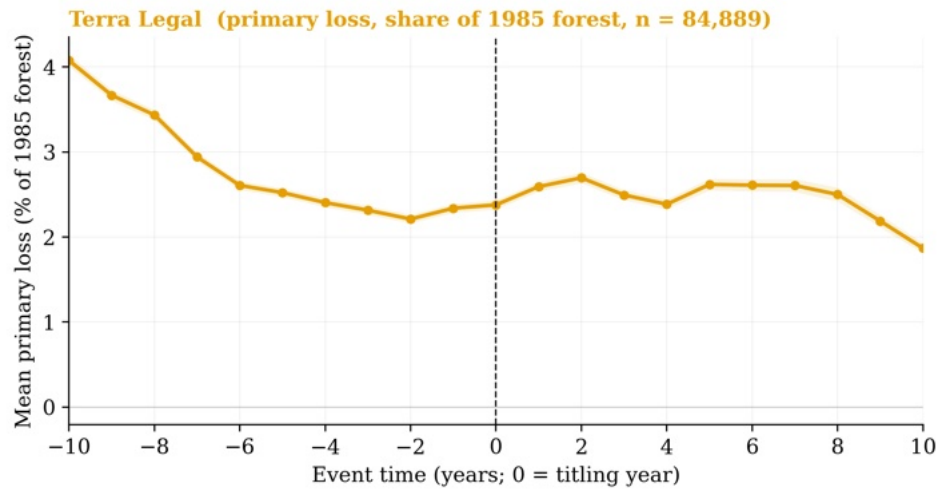


Figure 6: Heterogeneity in Anticipatory Clearing

Notes: Callaway–Sant’Anna event studies as in Figure 5, estimated separately for parcels facing the 80% Amazon-forest Legal Reserve requirement and for lower-obligation (cerrado) parcels. Heterogeneity by cohort and by parcel size is reported in Appendix B.



Note: Terra Legal parcels (federal land regularization, 2009-onward). Event time = parcel titling year.

Figure 7: Primary-Forest Loss Around Terra Legal Titling: Raw Pattern

Notes: Within-parcel annual primary-forest loss (% of 1985 baseline) by year relative to Terra Legal titling, pooled across cohorts. Eligibility required proof of occupation before December 2004, so current clearing could not strengthen a claim.

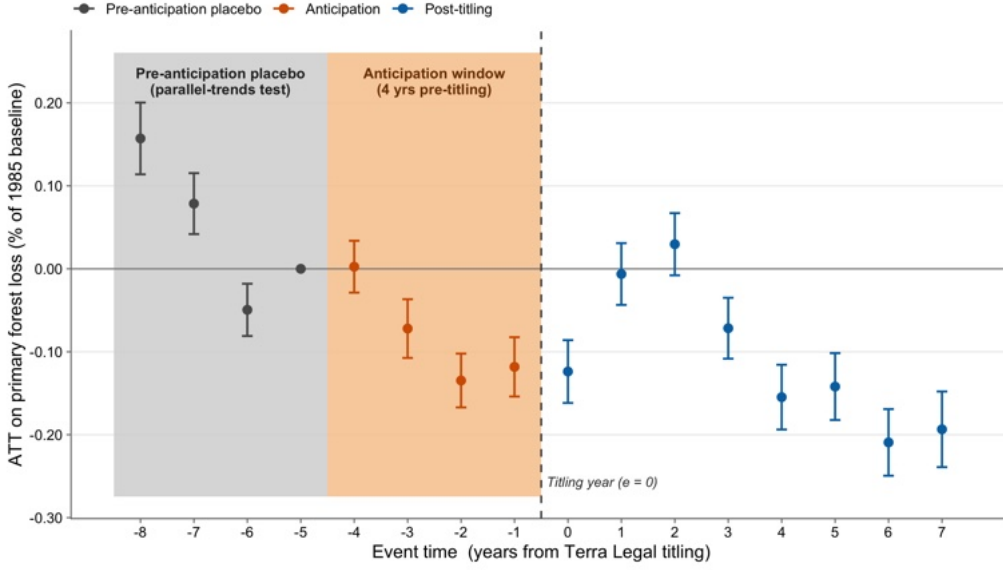


Figure 8: Placebo Event Study: Terra Legal Shows No Anticipatory Clearing

Notes: Callaway–Sant’Anna event study for Terra Legal parcels with titling dates (anticipation $L = 4$, base period $e = -5$, doubly robust estimator with covariates). Section 5.2 describes the control group. The SNCI-control version and the full coefficient table are in Appendix C.

the design is the shape of the profile, and nothing in the Terra Legal estimates resembles the rise toward the title date seen under SIGEF.¹³

Because the two programs operated under the same conditions (Section 5.2), the contrast makes it unlikely that selection into titling, frontier-wide shocks, or administrative cycles explain the SIGEF pattern. It points instead to the difference in eligibility rules, and in particular to whether current clearing could strengthen a claim. Appendix C shows that the null is informative. The anticipation-window estimates are precise enough to reject positive anticipatory effects, and re-anchoring the event study on placebo title dates two to four years before the true date shows that the modest post-titling increase in clearing appears only at the true date, so the recorded dates carry information.

6.3 Clearing before Registration

Prediction 3 concerns registration. We focus on SICAR parcels on undesignated federal land, which has no formal owner and where a registered polygon is often the first and only claim. Figure 9 presents the event study around registration, with titled never-treated parcels as controls.

Clearing rises sharply before registration. The anticipation-window estimates increase from

¹³Prior work on Terra Legal studies behavior after title. Probst et al. (2020) find that titles issued in 2011–2016 increased clearing among small and medium holders, consistent with secure rights raising the return to agricultural investment (Liscow, 2013), and Lipscomb and Prabakaran (2020) find no average effect in county-level data through 2012. Our placebo concerns whether clearing is used to earn title, which Terra Legal’s eligibility rule does not reward. The Benin land-formalization experiment, which also did not condition rights on land use, likewise finds no anticipatory clearing (Wren-Lewis et al., 2020).

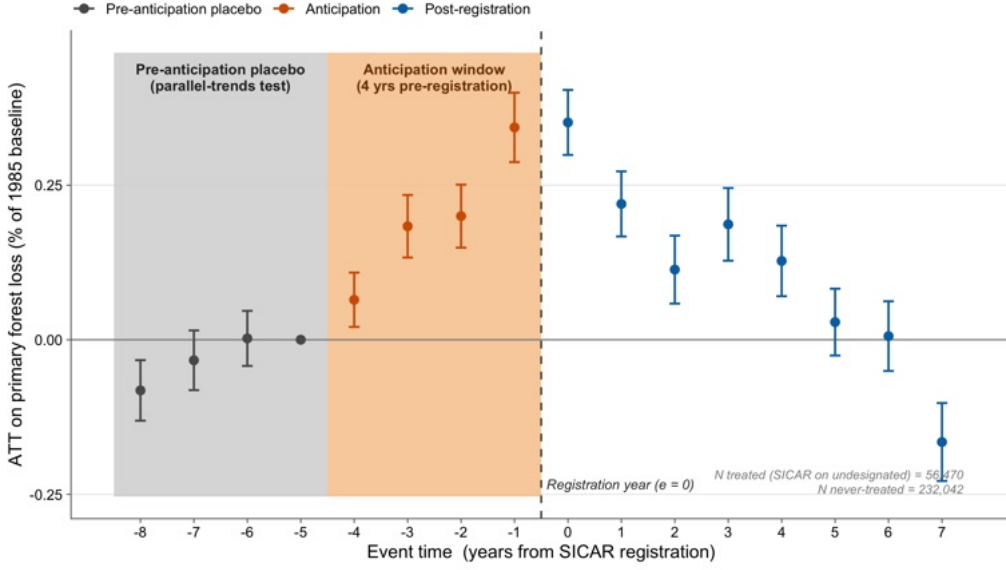


Figure 9: Event Study: CAR Registration on Undesignated Federal Land

Notes: Callaway–Sant’Anna event study for SICAR parcels with at least 50% of their area on undesignated federal land (CNFP), anchored at the registration year. Controls: titled never-treated parcels. Anticipation $L = 4$, doubly robust estimator. The full table, the pooled estimate for all SICAR parcels, and specifications with within-SICAR controls are in Appendix D.

0.065 percentage points of the 1985 stock at $e = -4$ to 0.343 at $e = -1$ and average about 0.20 a year, roughly five times the SIGEF average. Clearing peaks in the registration year (0.351) and remains elevated for four years before returning to zero. As under SIGEF, the coefficients at $e = -7$ and $e = -6$ are close to zero, while the coefficient at $e = -8$ is negative and significant (-0.082). The rise survives the more demanding comparison with not-yet-registered SICAR parcels as controls (Appendix D).

Figure 10 splits registrants on undesignated land by their later path to title. The rise before registration is largest among orphan parcels, which never obtain a SIGEF, SNCI, or Terra Legal title, where the estimates climb from 0.032 at $e = -4$ to 0.388 at $e = -1$. For these registrants the CAR polygon is the only claim they make, so their clearing cannot be anticipation of a later title. It is consistent with occupants establishing a visible footprint before they file a claim, and before a rival does.

The response per parcel is much larger than under SIGEF. Cheaper entry alone would not produce this, because the occupants it draws in are those for whom a claim is worth less (Section 3). The difference more likely reflects who registers on undesignated land. That land is mostly unlocated public forest, so registrants have more forest available to clear, and because no one holds title, they face competition from rival claimants that raises the value of establishing a footprint early. The incentive matters for the region because of the area involved, the roughly 60–70 million hectares of undesignated federal land described in Section 2.5. Cheap registration extended the clearing-to-claim incentive from the titling queue to this land.

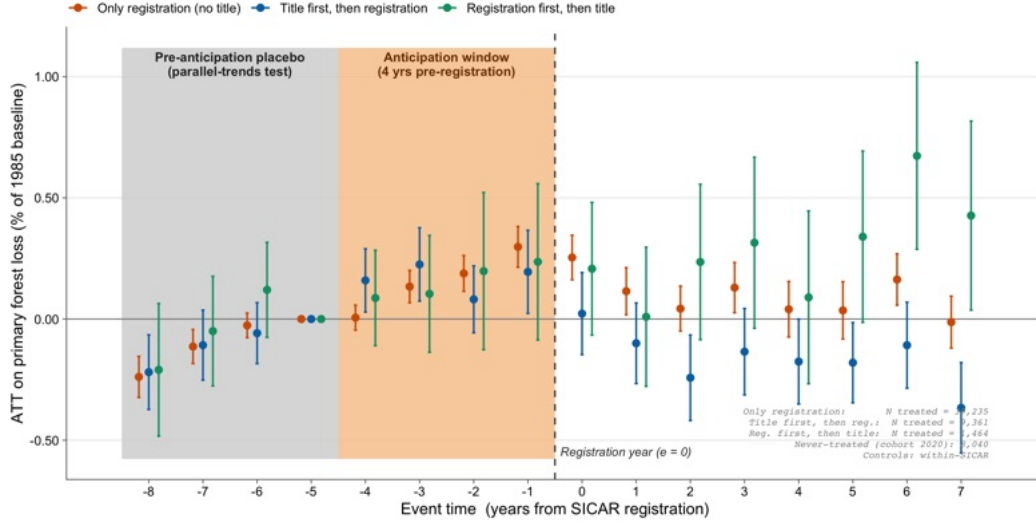


Figure 10: CAR Registration on Undesignated Land, by Path to Title

Notes: Event studies as in Figure 9, estimated separately for SICAR parcels on undesignated land that never obtain title (orphans), that were titled before registering, and that registered before being titled, using the linkage of Table 1. Coefficients are in Table D.2.

6.4 Bunching and Subdivision at Four Fiscal Modules

Prediction 4 concerns declared size. Figure 11 plots the distribution of declared property sizes around four fiscal modules in the Legal Amazon, with the fitted counterfactual density.

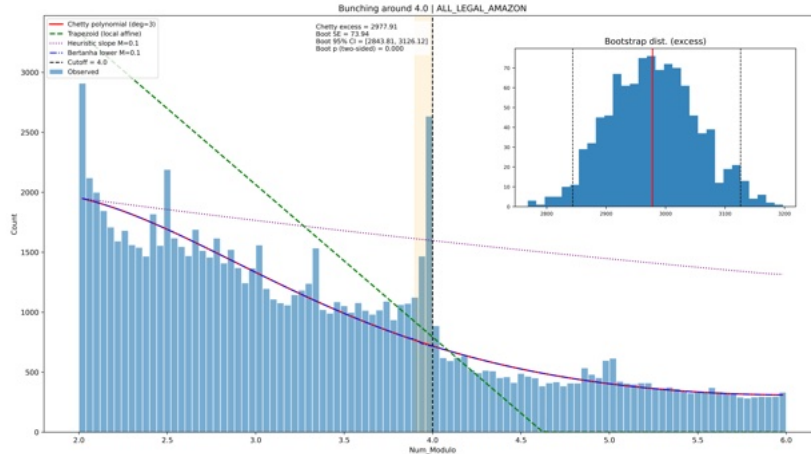


Figure 11: Bunching at the Four-Fiscal-Module Threshold: Pooled Legal Amazon

Notes: Histogram of declared property sizes (fiscal modules), Legal Amazon. The solid line is the counterfactual density, fitted excluding a band around the four-FM threshold (dashed vertical line). Shaded bins show the excess mass below the threshold, where obligations fall (Section 2.5). Estimator details and sensitivity analysis are in Appendix E.

There is a large excess mass just below the threshold. The pooled estimate implies 2,978 excess registrations, 133 percent more than the counterfactual density predicts in the bunching window, and the bootstrap confidence interval excludes zero. Estimated separately by state (Table 4), the

excess mass is significant in every state. In relative terms it is largest in Amazonas, Rondônia, Acre, Amapá, and Pará, and smallest in Mato Grosso and Tocantins.

Table 4: Bunching at the Four-FM Threshold: State-by-State Estimates

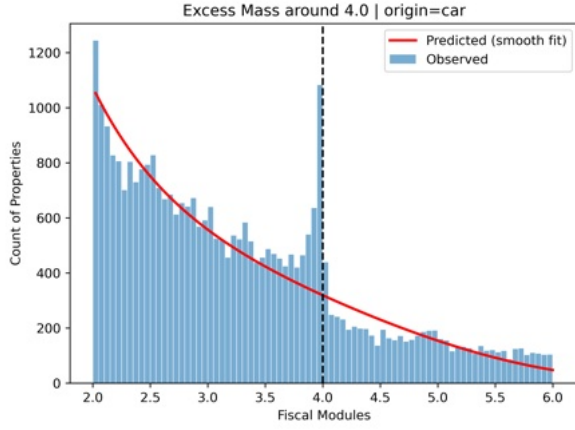
State	Parcels (N)	Excess registrations	% above predicted
Acre (AC)	37,353	1,145.5***	247.7%
Amapá (AP)	6,762	1,011.2***	235.8%
Amazonas (AM)	56,363	10,147.2***	343.2%
Maranhão (MA)	114,246	3,295.7***	59.8%
Mato Grosso (MT)	146,096	45.4***	17.6%
Pará (PA)	224,253	16,048.6***	215.5%
Rondônia (RO)	85,919	7,107.7***	337.1%
Roraima (RR)	10,533	789.3***	109.2%
Tocantins (TO)	79,004	894.9***	14.6%
Legal Amazon (sum of state estimates)	760,656	48,245.3***	127.7%

Notes: Excess-mass estimates from the bunching specification of Section 5.4, applied separately by state to the SICAR declared-FM distribution. The bottom row sums the state-level estimates and differs by construction from the single pooled-sample estimate reported in the text (Figure 11). “% above predicted” is the normalized excess mass $\times 100$. Significance from a density-discontinuity test at the 4-FM cutoff: *** $p < 0.01$ in all states. PA, MT, RO, and MA constitute the Arc of Deforestation.

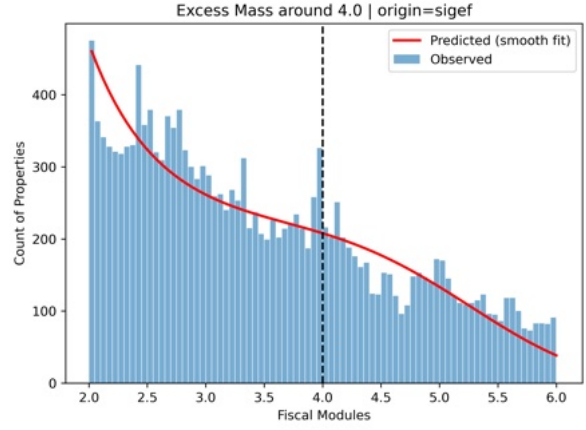
Bunching by registry. Figure 12 repeats the analysis by registry. The excess mass appears in SICAR, where holders draw their own boundaries, and is small or absent in SIGEF, SNCI, and Terra Legal, where boundaries are set through INCRA’s titling process. The rules tied to the threshold apply to properties in all four registries, so the difference points to self-declaration as the source of the manipulation. It is the land-registry counterpart of distorted self-reported pollution data (Duflo et al., 2013; Ghanem and Zhang, 2014).

Observing subdivisions directly. The registry linkage lets us observe the manipulation itself. Figure 13 shows an example from Rurópolis, Pará, where a titled property above the threshold was re-registered in CAR as three plots, each just below four fiscal modules. Because the CAR polygons lie inside an independently georeferenced titled boundary, such pairs are re-registrations of titled land and can be counted without statistical inference.

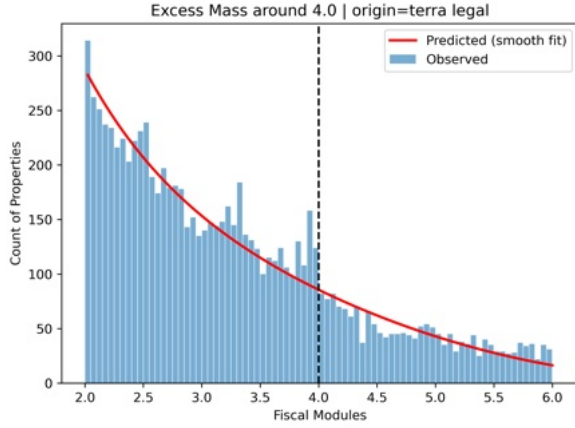
Summing across all titled parents above four fiscal modules with children below it, the Legal Reserve obligation at stake is at least 3.2 million hectares, about the area of Maryland (Figure 14). Mato Grosso, Pará, and Amazonas account for about 70 percent of the total, which equals about 18 percent of the Legal Reserve the titled parents would owe as whole properties (Appendix E). Although the law computes the reserve on a property’s size before subdivision, the registry records these children as small properties. The total is a lower bound in two senses. It counts only subdivisions of georeferenced titled parcels, omitting claims fragmented at first registration and



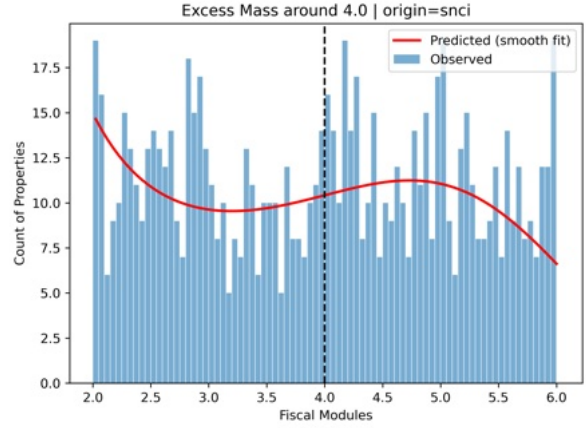
(a) CAR/SICAR (self-declared)



(b) SIGEF



(c) Terra Legal



(d) SNCI

Figure 12: Bunching by Registry Origin

Notes: Declared-size distributions around the four-FM threshold by registry. Only the self-declared CAR/SICAR registry shows excess mass below the threshold.

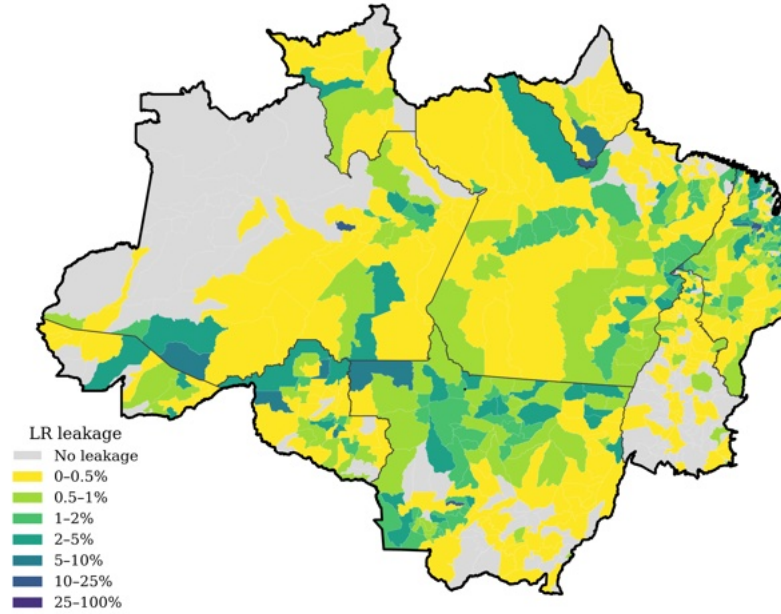


Figure 13: Subdivision in Practice: Rurópolis, Pará

Notes: A titled parent parcel above the four-FM threshold (left) and the self-declared SICAR polygons registered within its boundaries (right), each below four FM. Each child falls below the threshold and so is recorded as eligible for the lighter treatment described in Section 2.5. No cross-check between registries flagged the re-registration.

fragmentation on undesignated land, and it captures only the Legal Reserve component of the lighter treatment below the threshold.

Legal Reserve leakage by municipality



Source: author's calculation. Leakage = restoration obligation lost when subdivided parents push children below 4 FM (Art.\,67 amnesty).

Figure 14: Legal Reserve Obligation at Stake in Subdivision, by Municipality

Notes: Each Legal Amazon municipality is shaded by the share of its Legal Reserve obligation at stake when titled parents are re-registered as SICAR children below the four-FM threshold. For each titled parent in SIGEF, SNCI, or Terra Legal we compute the LR area required at the parent's size and biome, recompute the requirement over its SICAR children at their declared sizes, and aggregate the difference to the municipality. Grey municipalities have no titled parents linked to subdividing children in our sample. High shares concentrate along the southern Arc, in Rondônia, southern Amazonas, and the Mato Grosso-Pará frontier, where the titled-property base is largest. Source: authors' calculations from the subdivision linkage of Section 5.5.

Effects on land use. Appendix F estimates regression-discontinuity and difference-in-discontinuity designs at four fiscal modules. Properties just above the threshold clear less than those just below it, by 3.4 percentage points a year, and the gap appears only after 2012. Because declared size is manipulated, these estimates describe the realized difference between the two sides of the threshold rather than a causal effect at the cutoff. They are consistent with the most conversion-minded landholders having moved below the threshold on paper, leaving more compliant landholders above it.

7 Conclusion

This paper studies how the rules for acquiring a property right shape land use before the right is granted. Using a new property-level panel of 1.09 million parcels in the Brazilian Amazon, we

find that where title rewards demonstrated productive use, clearing rises over the four years before title and peaks around the title year. Where eligibility depends instead on occupation before a fixed date in the past, clearing does not rise before title. When registering a claim became cheap and unverified, clearing before registration on undesignated federal land was roughly five times the pre-title increase under the productive-use standard, and it appeared even among claimants who never obtained title. When obligations depended on a self-declared property size, declared sizes bunched just below the threshold, and titled properties were re-registered as smaller parcels with at least 3.2 million hectares of Legal Reserve obligation at stake.

The results bear on two questions in the economics of property rights. The first is why studies of formalization reach such different conclusions. Whether titling protects forest or accelerates its conversion depends in part on what a program rewards on the way to title. Programs that condition title on current land use induce clearing before title, while programs that condition it on past occupation do not. The second concerns measurement. Studies that compare land use before and after the title date treat the pre-title years as a baseline. Where landholders clear in anticipation, that baseline is inflated, so these designs understate the clearing induced by the titling process and can make title itself appear to reduce deforestation. Competition for title also dissipates part of the rents that secure tenure creates. On a tropical frontier the dissipation takes the form of forest loss, whose costs in carbon and biodiversity fall largely on others, so the social cost of the competition exceeds its private cost.

For policy, the results point to design choices that remove the incentive at its source rather than relying on enforcement. Tying eligibility to occupation before a past date removes the return to clearing for title, and archived satellite imagery now makes historical occupation cheap to verify. Because forest is lost for as long as claimants wait under a productive-use standard, delay in processing claims is itself costly. Digital registries lower the cost of claiming, and unless verification keeps pace they also lower the cost of false claims. Cross-checking declared boundaries against existing cadastres and remotely sensed land cover is now technically routine ([Assunção et al., 2023](#)). Obligations that change discretely at a declared size invite manipulation, which smoother schedules, or schedules tied to verified attributes, would avoid. These choices matter beyond Brazil. Its registry is a template for systems elsewhere, and the signatories of the COP30 Land Tenure Pledge are formalizing rights over large areas where occupation precedes title and productive use is the customary evidence of possession.

Our analysis has several limitations. The estimates identify how clearing responds to the regularization process, but they do not amount to a full welfare accounting. In particular, we cannot fully separate additional clearing from clearing brought forward in time. Post-title effects turn only slightly negative within our horizon, far too little to offset the earlier increase, but longer panels are needed to settle the question, and even pure retiming is costly when carbon damages rise over time. The estimates are also partial equilibrium. Titling one parcel may push claimants toward other land, and the value of qualifying for lighter treatment below the size threshold may

be capitalized into land prices. Finally, we do not observe whether land acquired through clearing converges to the land use of properties with established tenure, or whether the way land was acquired has lasting effects, as [Allen and Leonard \(2021\)](#) find for the American West. Answering these questions will require longer panels linked to data on credit, land transactions, and prices.

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Online Appendix

Clearing to Claim: The Perverse Incentives of Land Regularization in the Brazilian Amazon

A Institutional Background and Data Construction

A.1 Cadastre versus Land Registration in Brazil

Figure [A.1](#) shows the institutional hierarchy of Brazil’s land cadastres and distinguishes tenure registries from the environmental registry. On the tenure side, records range from high-certainty registries (INCRA’s SIGEF and SNCI, titled Terra Legal parcels, Quilombola territories, and homologated Indigenous Lands) to transitional layers such as non-homologated Indigenous Lands, untitled Terra Legal areas, undesignated public lands, and unallocated public forests (CNFP Type B). CAR, recorded in SICAR, sits apart. It monitors compliance with the Forest Code’s Permanent Preservation Areas and Legal Reserves but confers no property right. The ordering reflects legal certainty, spatial validation, and enforceability, following technical work on Brazilian land governance ([Reydon et al., 2018](#); [Freitas et al., 2018](#); [Reydon et al., 2015](#); [Shinde, 2022](#)).

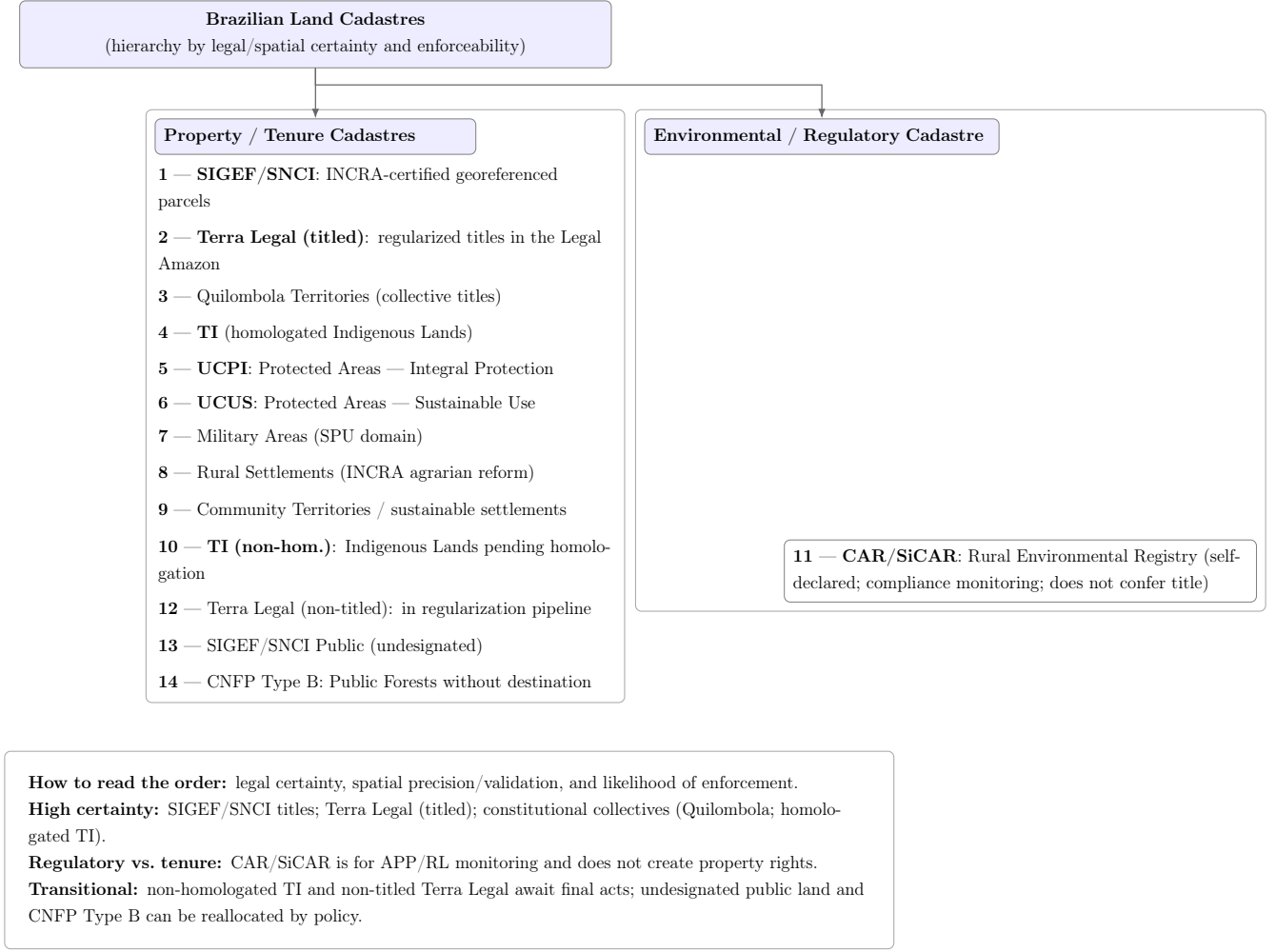


Figure A.1: Hierarchy of Brazilian land cadastres by legal certainty, spatial validation, and enforceability (Reydon et al., 2015, 2018; Freitas et al., 2018; Reydon et al., 2020; Shinde, 2022).

A.2 Registries and Harmonization

Brazil’s land administration is divided among institutions created for different legal purposes: formal titling (SIGEF and SNCI), federal land regularization (Terra Legal), environmental registration (SiCAR), and public-land administration (CNFP). The registries differ in spatial resolution, geometry quality, identifier structure, temporal coverage, and legal status, no single registry covers all rural land claims, and the same landholding often appears in several registries with different geometries, identifiers, or histories. We integrate the original registry releases through a common pipeline that builds on Camara et al. (2023) and Shinde (2022). It validates and repairs geometries, corrects topology, reconciles identifiers, standardizes attributes, detects duplicates, harmonizes dates, and matches parcels across registries. Rather than overlaying polygons, the pipeline reconciles the different administrative records of the same property, so that individual landholdings can be followed through the stages of regularization. The resulting database contains 1,087,185 georeferenced parcel polygons covering titled private land, self-declared claims,

and public-land designations. Figure 3 shows the combined set of parcels, and Figures A.2–A.6 show the individual layers.

Public lands (CNFP). The National Registry of Public Forests (*Cadastro Nacional de Florestas Públicas*, CNFP) is the official spatial inventory of federally administered public lands. We retain four mutually exclusive designations: undesignated public land, sustainable-use forests, strict-protection forests, and military lands, comprising 9,866 polygons (Figure A.2). These layers separate titled private land from claims on public land and identify SICAR registrations on undesignated federal land.

SIGEF. SIGEF certification requires georeferenced surveying to INCRA’s standards and administrative verification, which makes it the highest-quality cadastral layer for private land. Our extract, obtained directly from INCRA, contains 151,461 certified parcels titled between 2013 and 2021 (median 2018), each with a parcel geometry, titling date, area, fiscal-module classification, and Forest Code attributes (Figure A.3).

SNCI. SNCI records were created before systematic georeferenced certification and require spatial reconstruction. We obtain the registry from INCRA and reconstruct representative parcel geometries following Shinde (2022). The harmonized layer contains 13,910 titled parcels (2004–2018, median 2012), concentrated in older colonization regions and larger than SIGEF parcels (Figure A.4).

Terra Legal. We begin with the compilation of Camara et al. (2023), which covers the Amazon biome through 2019, and extend it with a May 2026 download from INCRA’s Terra Legal registry that adds post-2019 records and parcels in Legal Amazon states outside the biome. Duplicate records are reconciled through geometric matching, identifier reconciliation, and attribute validation. The unified layer contains 161,126 parcels, of which 96,188 have a valid titling date (2013–2019, median 2014) (Figure A.5).

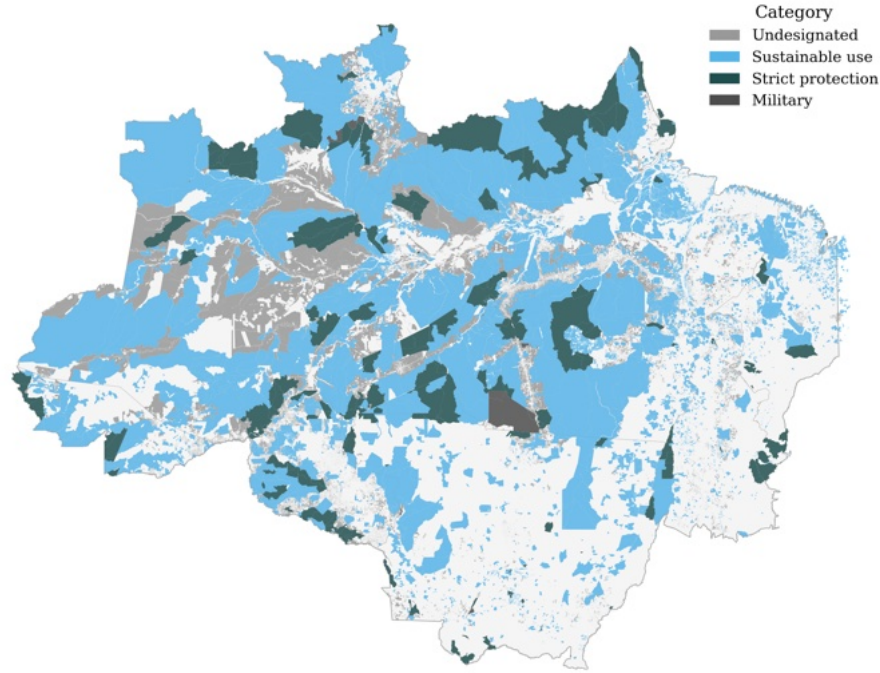
SICAR. SICAR is the national database of CAR registrations and the largest layer in our data. The public registry provides parcel geometries but not bulk registration dates, so we merge the polygon layer with registration dates and attributes assembled by Shinde (2022). The harmonized layer contains 760,688 self-declared parcels registered between 2014 and 2020 (median 2016) (Figure A.6). Because registrations are not verified when they are made, they overlap substantially with titled properties and public lands, which makes reconciliation across registries essential.

Cross-registry linkage. The database keeps each registry’s records rather than merging overlapping polygons into a single ownership layer. Linking records by identifiers, dates, attributes,

and boundaries allows us to follow properties from self-declared registration to title, to identify titled land that was registered again, and to measure subdivision directly. Section 4 and Table 1 describe the parent-child matching used in the subdivision analysis.

Public lands

Four CNFP categories, n = 9,866



Source: CNFP (Cadastro Nacional de Florestas Públicas), Brazilian Forest Service.

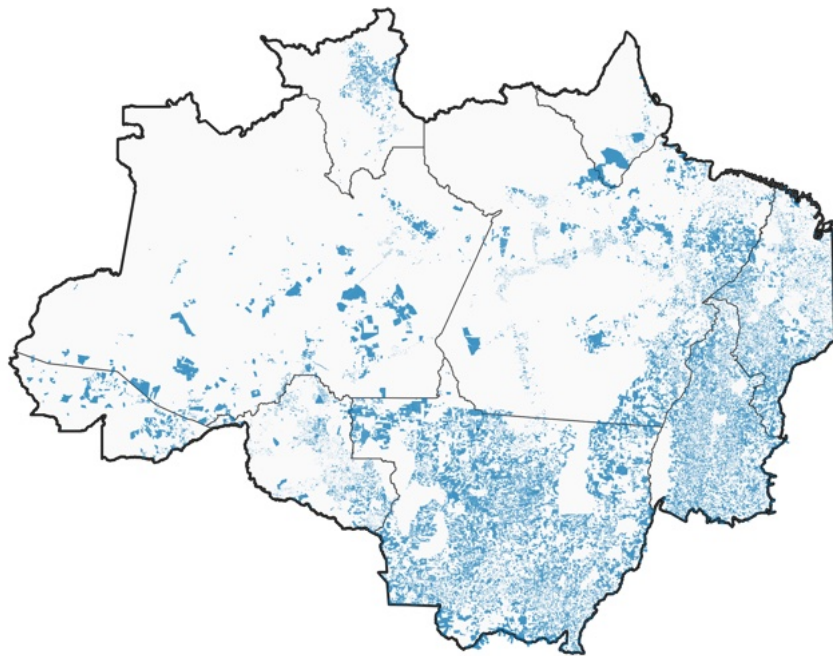
Figure A.2: Public Lands in the Legal Amazon (CNFP)

A.3 Fiscal Modules and the Legal Reserve Raster

The fiscal module is an area unit defined by INCRA in hectares. Its value varies across municipalities because it is meant to reflect local agricultural conditions, predominant production systems, and income potential, so one fiscal module ranges from a few hectares in some municipalities to more than one hundred in others. We take the official value for each of Brazil's 5,569 municipalities from INCRA's 2013 table. Each property in our database is assigned to the municipality containing its centroid, which gives every property a single fiscal-module value even when it crosses municipal boundaries, and its size in fiscal modules is its area in hectares divided by that value. Size in fiscal modules matters because the Forest Code and related laws set several thresholds in these units, the most important of which for this paper is the four-fiscal-module threshold described in Section 2.5. Below it, Article 67 exempts holders from restoring Legal Reserve cleared before July 2008, and their reserve is the native vegetation that remained at that date. Above it, the holder must restore the reserve to the share required by Article 12.

SIGEF parcels

Titled private land
n = 151,461

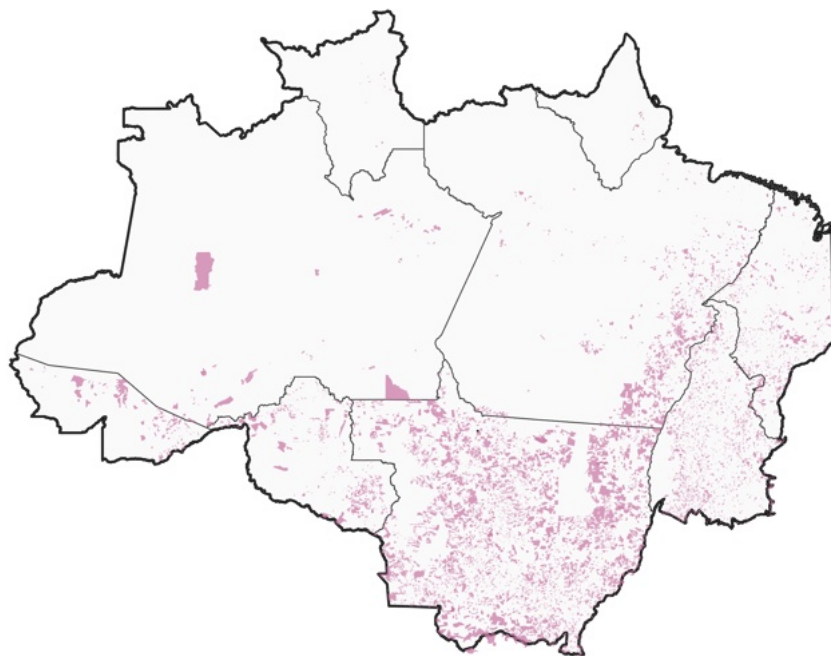


Source: INCRA, Sistema de Gestao Fundiaria (SIGEF), retrieved 2023.

Figure A.3: SIGEF Titled Parcels, retrieved in 2026

SNCI parcels

Titled private land
n = 13,910

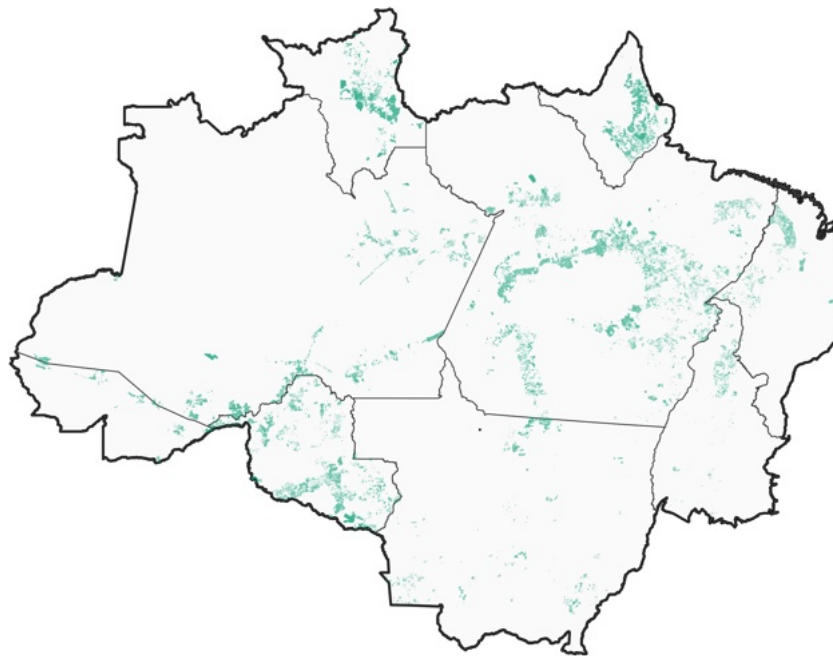


Source: INCRA, Sistema Nacional de Cadastro de Imóveis Rurais (SNCI), retrieved 2023.

Figure A.4: SNCI Titled Parcels, retrieved in 2026

Terra Legal parcels

Titled private land (smallholder)
n = 161,126

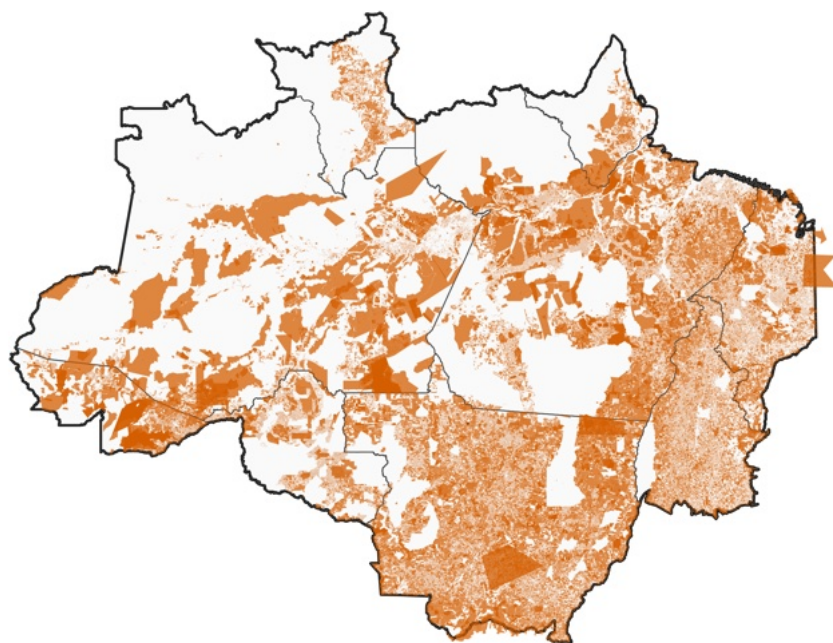


Source: Camara et al. (2023) and IMAFLORA, harmonised by author.

Figure A.5: Terra Legal Parcels, retrieved in 2026

SICAR parcels

Self-declared registry, not a title (overlaps title and public layers)
n = 760,688

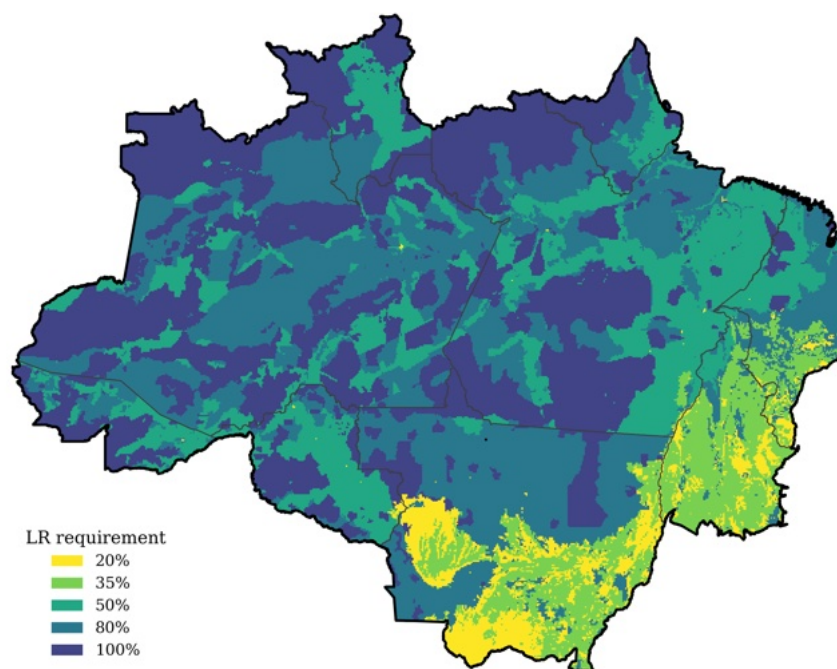


Source: SICAR (Cadastro Ambiental Rural), Brazilian Forest Service.

Figure A.6: SICAR Self-Declared Parcels, retrieved in 2021

To assign each property its applicable Legal Reserve requirement, we construct a 30-meter raster of requirements across the Legal Amazon. Property geometries come from CAR, SIGEF, SNCI, Terra Legal, and rural settlements, municipal boundaries and the Amazon biome boundary from the Brazilian Institute of Geography and Statistics (IBGE). Within the Amazon biome, requirements are assigned using the classification of [Camara et al. \(2023\)](#), which identifies the legal zones associated with 80, 50, and 100 percent requirements. Outside the biome but within the Legal Amazon, where that classification does not apply, we use the IBGE 1:250,000 vegetation map to classify native vegetation into the categories of Article 12(I), assigning 80 percent to forest formations, 35 percent to cerrado, and 20 percent to campos gerais. We validate the raster against municipal and state rules governing Legal Reserve requirements on private land. The result assigns each 30-meter pixel one of five requirements (20, 35, 50, 80, or 100 percent), with higher requirements in forested frontier regions and lower requirements in non-forest and consolidated agricultural zones (Figure A.7).

Legal Reserve requirement (Forest Code raster)



Source: constructed by the author from Camara et al. (2023), IBGE vegetation classes, and Forest Code (Lei 12.651/2012). Native 30 m, displayed at ~5

Figure A.7: Legal Reserve Requirement Raster for the Legal Amazon

Notes: Authors' construction using the 2012 Brazilian Forest Code (rules, mandates and legal documents), INCRA's 2013 municipal fiscal-module table, IBGE municipal boundaries and vegetation maps, SIGEF/SNCI, Terra Legal and Rural Settlements public-land layers, and the Legal Reserve zoning dataset of [Camara et al. \(2023\)](#). The resulting product is a 30-m pixel-level map of the applicable Legal Reserve requirement across the entire Legal Amazon.

B SIGEF: Additional Results

This appendix reports the SIGEF event study with not-yet-treated controls and the heterogeneity results summarized in Section 6.1.

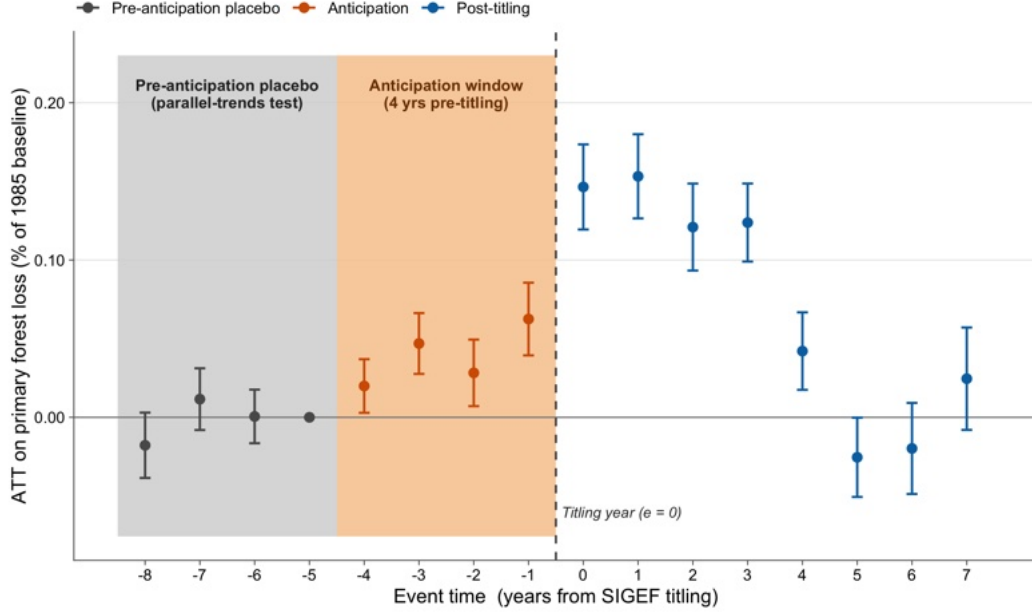


Figure B.1: SIGEF Event Study with Not-Yet-Treated Controls

Notes: As Figure 5, using not-yet-treated SIGEF cohorts as controls (Column 3 of Table 3).

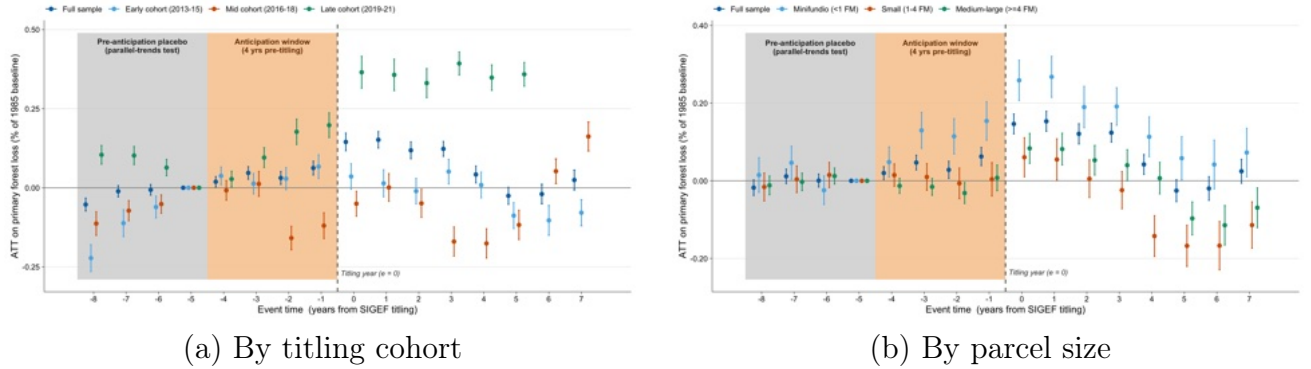


Figure B.2: Anticipatory Clearing by Cohort and Size

Notes: CS-DiD event studies by subgroup. Later titling cohorts exhibit the strongest anticipation, consistent with growing expectations of formalization; anticipatory clearing is strongest among smallholder (minifundio) parcels.

Table B.1: Dynamic Treatment Effects of SIGEF Titling — Frontier Heterogeneity

	Outcome: <i>Primary forest loss (% of 1985 baseline)</i>		
	(1) Full sample	(2) Arc states PA, MT, RO, MA	(3) Non-Arc states AM, AC, TO, RR, AP
<i>Pre-anticipation placebo (parallel-trends test)</i>			
Event time $e = -8$	-0.018* (0.010)	0.009 (0.012)	-0.133*** (0.018)
Event time $e = -7$	0.012 (0.010)	0.036*** (0.012)	-0.082*** (0.016)
Event time $e = -6$	0.001 (0.009)	0.017 (0.011)	-0.060*** (0.014)
Event time $e = -5$	0.000 ref.	0.000 ref.	0.000 ref.
<i>Anticipation window (treatment effects allowed)</i>			
Event time $e = -4$	0.020** (0.008)	0.017* (0.011)	0.023 (0.015)
Event time $e = -3$	0.047*** (0.009)	0.064*** (0.012)	-0.007 (0.016)
Event time $e = -2$	0.028*** (0.010)	0.044*** (0.013)	-0.010 (0.017)
Event time $e = -1$	0.062*** (0.011)	0.076*** (0.014)	0.028 (0.018)
<i>Post-titling effects</i>			
Event time $e = 0$	0.146*** (0.013)	0.189*** (0.016)	0.032 (0.020)
Event time $e = 1$	0.153*** (0.014)	0.189*** (0.016)	0.062*** (0.019)
Event time $e = 2$	0.121*** (0.013)	0.131*** (0.015)	0.101*** (0.021)
Event time $e = 3$	0.124*** (0.013)	0.134*** (0.014)	0.095*** (0.018)
Event time $e = 4$	0.042*** (0.013)	0.036** (0.015)	0.062*** (0.021)
Event time $e = 5$	-0.025* (0.014)	-0.035** (0.016)	0.008 (0.024)
Event time $e = 6$	-0.020 (0.015)	-0.034* (0.019)	0.027 (0.026)
Event time $e = 7$	0.025 (0.018)	0.022 (0.019)	0.033 (0.025)
Anticipation window	4 periods	4 periods	4 periods
Control group	Not-yet-treated	Not-yet-treated	Not-yet-treated
Covariates (LR%, FM, baseline q)	Yes	Yes	Yes
Estimator	Callaway–Sant’Anna (DR)	Callaway–Sant’Anna (DR)	Callaway–Sant’Anna (DR)
Inference	Cluster bootstrap	Cluster bootstrap	Cluster bootstrap
Cluster level	Parcel	Parcel	Parcel
Number of clusters	232,042	198,105	133,011
Number of observations	6,961,260	5,943,150	3,990,330
Bootstrap replications	1000	1000	1000
Random seed	2006	2006	2006

Notes: Dynamic treatment effects of SIGEF titling on within-parcel primary forest loss (% of 1985 baseline forest stock), estimated separately for the full sample and for two frontier sub-samples. Arc-of-deforestation states are Pará, Mato Grosso, Rondônia, and Maranhão (UF codes 15, 51, 11, 21); Non-Arc states are Amazonas, Acre, Tocantins, Roraima, and Amapá (codes 13, 12, 17, 14, 16). All three columns use the Callaway–Sant’Anna doubly-robust estimator with not-yet-treated SIGEF cohorts (and SSCI + Terra Legal never-treated parcels) as controls, a four-period anticipation window (`anticipation = 4`, `base_period = “universal”`; omitted base period is $e = -5$), parcel-level cluster-robust standard errors via multiplier bootstrap, and time-invariant covariates (Legal Reserve requirement, fiscal modules, baseline-forest decile). The top block reports placebo coefficients in years $e = -8..-5$ (parallel-trends test); the middle block reports anticipatory clearing in the four years preceding titling; the bottom block reports post-titling effects. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: Dynamic Treatment Effects of SIGEF Titling — Within-Arc State Heterogeneity

	Outcome: <i>Primary forest loss (% of 1985 baseline)</i>			
	(1) PA Pará	(2) MT Mato Grosso	(3) RO Rondônia	(4) MA Maranhão
<i>Pre-anticipation placebo (parallel-trends test)</i>				
Event time $e = -8$	0.119*** (0.027)	-0.059*** (0.018)	-0.020 (0.032)	0.043** (0.022)
Event time $e = -7$	0.038 (0.025)	0.021 (0.019)	-0.012 (0.032)	0.082*** (0.022)
Event time $e = -6$	0.024 (0.022)	0.021 (0.017)	-0.049* (0.025)	0.041** (0.020)
Event time $e = -5$	0.000 ref.	0.000 ref.	0.000 ref.	0.000 ref.
<i>Anticipation window (treatment effects allowed)</i>				
Event time $e = -4$	0.009 (0.022)	0.018 (0.016)	0.012 (0.029)	0.029 (0.020)
Event time $e = -3$	0.003 (0.024)	0.051*** (0.017)	0.245*** (0.042)	0.050** (0.021)
Event time $e = -2$	-0.041* (0.024)	0.003 (0.019)	0.372*** (0.049)	0.040* (0.024)
Event time $e = -1$	-0.030 (0.024)	0.017 (0.019)	0.359*** (0.044)	0.160*** (0.029)
<i>Post-titling effects</i>				
Event time $e = 0$	0.063** (0.029)	0.069*** (0.023)	0.776*** (0.066)	0.266*** (0.031)
Event time $e = 1$	0.025 (0.028)	0.085*** (0.023)	0.817*** (0.065)	0.245*** (0.034)
Event time $e = 2$	-0.010 (0.031)	0.044** (0.019)	0.681*** (0.053)	0.172*** (0.030)
Event time $e = 3$	-0.034 (0.027)	0.119*** (0.021)	0.530*** (0.043)	0.124*** (0.026)
Event time $e = 4$	-0.048* (0.029)	0.010 (0.020)	0.365*** (0.045)	0.007 (0.029)
Event time $e = 5$	-0.109*** (0.029)	-0.012 (0.023)	0.179*** (0.039)	-0.116*** (0.031)
Event time $e = 6$	-0.091** (0.041)	-0.007 (0.024)	0.181*** (0.051)	-0.139*** (0.036)
Event time $e = 7$	-0.111*** (0.040)	0.129*** (0.028)	0.165*** (0.055)	-0.139*** (0.041)
Anticipation window	4 periods	4 periods	4 periods	4 periods
Control group	Not-yet-treated	Not-yet-treated	Not-yet-treated	Not-yet-treated
Estimator	CS (DR)	CS (DR)	CS (DR)	CS (DR)
Cluster level	Parcel	Parcel	Parcel	Parcel
Number of clusters	118,823	145,505	110,469	119,602
Number of observations	3,564,690	4,365,150	3,314,070	3,588,060
Bootstrap replications	1000	1000	1000	1000
Random seed	2006	2006	2006	2006

Notes: Within-Arc state-by-state CS effects of SIGEF titling on primary forest loss (% of 1985 baseline). Each column restricts treated parcels to one of the four Arc-of-deforestation states (Pará, Mato Grosso, Rondônia, Maranhão), with all SNCI + Terra Legal never-treated parcels retained as the control pool. MT (col. 2) accounts for nearly half of all SIGEF parcels; the comparison shows whether the aggregate Arc result is driven by MT or by all four states. Same anticipation $L=4$ / base period $e = -5$ spec, parcel-clustered bootstrap SEs. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: Dynamic Treatment Effects of SIGEF Titling — Legal Reserve Heterogeneity

	Outcome: <i>Primary forest loss (% of 1985 baseline)</i>		
	(1) Full sample	(2) Amazon biome LR $\geq$ 80%	(3) Cerrado side LR < 80%
<i>Pre-anticipation placebo (parallel-trends test)</i>			
Event time $e = -8$	-0.018* (0.011)	0.014 (0.019)	-0.013 (0.012)
Event time $e = -7$	0.012 (0.010)	0.079*** (0.020)	-0.013 (0.011)
Event time $e = -6$	0.001 (0.009)	0.030 (0.020)	-0.015 (0.009)
Event time $e = -5$	0.000 ref.	0.000 ref.	0.000 ref.
<i>Anticipation window (treatment effects allowed)</i>			
Event time $e = -4$	0.020** (0.009)	0.060*** (0.017)	-0.011 (0.010)
Event time $e = -3$	0.047*** (0.010)	0.101*** (0.020)	0.013 (0.014)
Event time $e = -2$	0.028*** (0.011)	0.070*** (0.022)	0.013 (0.015)
Event time $e = -1$	0.062*** (0.012)	0.140*** (0.025)	0.039** (0.016)
<i>Post-titling effects</i>			
Event time $e = 0$	0.146*** (0.014)	0.328*** (0.030)	0.097*** (0.017)
Event time $e = 1$	0.153*** (0.014)	0.335*** (0.030)	0.112*** (0.018)
Event time $e = 2$	0.121*** (0.014)	0.219*** (0.029)	0.126*** (0.017)
Event time $e = 3$	0.124*** (0.013)	0.253*** (0.026)	0.098*** (0.017)
Event time $e = 4$	0.042*** (0.013)	0.078*** (0.026)	0.077*** (0.018)
Event time $e = 5$	-0.025** (0.013)	0.011 (0.031)	0.020 (0.017)
Event time $e = 6$	-0.020 (0.015)	0.006 (0.033)	0.031 (0.019)
Event time $e = 7$	0.025 (0.017)	0.084** (0.036)	0.049** (0.019)
Anticipation window	4 periods	4 periods	4 periods
Control group	Not-yet-treated	Not-yet-treated	Not-yet-treated
Covariates (LR%, FM, baseline q)	Yes	Yes	Yes
Estimator	Callaway-Sant'Anna (DR)	Callaway-Sant'Anna (DR)	Callaway-Sant'Anna (DR)
Inference	Cluster bootstrap	Cluster bootstrap	Cluster bootstrap
Cluster level	Parcel	Parcel	Parcel
Number of clusters	232,042	75,700	156,342
Number of observations	6,961,260	2,271,000	4,690,260
Bootstrap replications	1000	1000	1000
Random seed	2006	2006	2006

Notes: Dynamic treatment effects of SIGEF titling on within-parcel primary forest loss (% of 1985 baseline forest stock), estimated separately by the parcel's Forest-Code-mandated Legal Reserve obligation. Column (2) restricts to parcels with LR $\geq$ 80% (deep Amazon biome, highest reserve burden); column (3) restricts to parcels with LR < 80% (Cerrado / Cerrado-Amazon transition: LR = 20, 35, or 50%). All three columns use the Callaway-Sant'Anna doubly-robust estimator with not-yet-treated SIGEF cohorts (and SNCI + Terra Legal never-treated parcels) as controls, a four-period anticipation window (`anticipation = 4`, `base_period = "universal"`; omitted base period is $e = -5$), parcel-level cluster-robust standard errors via multiplier bootstrap, and time-invariant covariates (LR%, fiscal modules, baseline-forest decile). Top block: placebo coefficients in $e = -8..-5$ (parallel-trends test). Middle block: anticipatory clearing in the four years preceding titling. Bottom block: post-titling effects. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: Dynamic Treatment Effects of SIGEF Titling — Parcel-Size Heterogeneity

	Outcome: <i>Primary forest loss (% of 1985 baseline)</i>			
	(1) Full	(2) Minifundio < 1 FM	(3) Small 1–4 FM	(4) Medium-large ≥ 4 FM
<i>Pre-anticipation placebo (parallel-trends test)</i>				
Event time $e = -8$	-0.018* (0.010)	0.015 (0.023)	-0.016 (0.018)	-0.012 (0.012)
Event time $e = -7$	0.012 (0.010)	0.046** (0.022)	0.004 (0.017)	-0.003 (0.012)
Event time $e = -6$	0.001 (0.009)	-0.024 (0.019)	0.015 (0.016)	0.012 (0.010)
Event time $e = -5$	0.000 ref.	0.000 ref.	0.000 ref.	0.000 ref.
<i>Anticipation window (treatment effects allowed)</i>				
Event time $e = -4$	0.020** (0.008)	0.048** (0.020)	0.015 (0.015)	-0.013 (0.010)
Event time $e = -3$	0.047*** (0.010)	0.130*** (0.024)	0.010 (0.017)	-0.016 (0.011)
Event time $e = -2$	0.028** (0.011)	0.115*** (0.023)	-0.007 (0.020)	-0.031** (0.014)
Event time $e = -1$	0.062*** (0.012)	0.154*** (0.025)	0.004 (0.022)	0.008 (0.017)
<i>Post-titling effects</i>				
Event time $e = 0$	0.146*** (0.013)	0.259*** (0.026)	0.061** (0.026)	0.083*** (0.020)
Event time $e = 1$	0.153*** (0.013)	0.268*** (0.027)	0.055** (0.027)	0.082*** (0.021)
Event time $e = 2$	0.121*** (0.013)	0.190*** (0.027)	0.005 (0.025)	0.053*** (0.019)
Event time $e = 3$	0.124*** (0.012)	0.191*** (0.025)	-0.024 (0.024)	0.040** (0.020)
Event time $e = 4$	0.042*** (0.013)	0.113*** (0.026)	-0.142*** (0.027)	0.007 (0.021)
Event time $e = 5$	-0.025* (0.014)	0.058** (0.028)	-0.168*** (0.027)	-0.097*** (0.022)
Event time $e = 6$	-0.020 (0.016)	0.042 (0.032)	-0.167*** (0.032)	-0.114*** (0.026)
Event time $e = 7$	0.025 (0.016)	0.073** (0.032)	-0.114*** (0.031)	-0.069*** (0.026)
Anticipation window	4 periods	4 periods	4 periods	4 periods
Control group	Not-yet-treated	Not-yet-treated	Not-yet-treated	Not-yet-treated
Estimator	CS (DR)	CS (DR)	CS (DR)	CS (DR)
Cluster level	Parcel	Parcel	Parcel	Parcel
Number of clusters	232,042	94,789	76,360	60,893
Number of observations	6,961,260	2,843,670	2,290,800	1,826,790
Bootstrap replications	1000	1000	1000	1000
Random seed	2006	2006	2006	2006

Notes: Dynamic CS effects of SIGEF titling on primary forest loss (% of 1985 baseline), split by parcel size in fiscal modules (FM). The 4-FM cliff is the Lei 12.651/2012 Art. 67 amnesty threshold (properties ≤ 4 FM are exempt from Legal Reserve restoration); the 1-FM cliff is the administrative-simplicity / family-farmer-credit threshold. If anticipation is driven by amnesty-positioning, the 1–4 FM bin (col. 3) should display the largest pre-titling spike. Same spec as the main table (anticipation = 4, base period $e = -5$, not-yet-treated controls, parcel-clustered bootstrap SEs). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: Dynamic Treatment Effects of SIGEF Titling — Cohort Heterogeneity

	Outcome: <i>Primary forest loss (% of 1985 baseline)</i>			
	(1) Full	(2) Early 2013–15	(3) Mid 2016–18	(4) Late 2019–21
<i>Pre-anticipation placebo (parallel-trends test)</i>				
Event time $e = -8$	-0.053*** (0.010)	-0.222*** (0.022)	-0.113*** (0.019)	0.104*** (0.015)
Event time $e = -7$	-0.011 (0.009)	-0.112*** (0.022)	-0.072*** (0.016)	0.102*** (0.015)
Event time $e = -6$	-0.006 (0.009)	-0.061*** (0.018)	-0.052*** (0.015)	0.064*** (0.013)
Event time $e = -5$	0.000 ref.	0.000 ref.	0.000 ref.	0.000 ref.
<i>Anticipation window (treatment effects allowed)</i>				
Event time $e = -4$	0.019** (0.008)	0.038*** (0.014)	-0.008 (0.016)	0.028** (0.013)
Event time $e = -3$	0.047*** (0.010)	0.013 (0.016)	0.012 (0.020)	0.096*** (0.016)
Event time $e = -2$	0.031*** (0.010)	0.029 (0.018)	-0.159*** (0.019)	0.177*** (0.020)
Event time $e = -1$	0.062*** (0.011)	0.067*** (0.019)	-0.120*** (0.021)	0.198*** (0.020)
<i>Post-titling effects</i>				
Event time $e = 0$	0.145*** (0.014)	0.036* (0.021)	-0.050** (0.020)	0.365*** (0.026)
Event time $e = 1$	0.152*** (0.013)	0.014 (0.022)	0.001 (0.022)	0.357*** (0.025)
Event time $e = 2$	0.118*** (0.014)	-0.011 (0.021)	-0.049** (0.023)	0.331*** (0.023)
Event time $e = 3$	0.123*** (0.012)	0.051** (0.020)	-0.170*** (0.024)	0.393*** (0.019)
Event time $e = 4$	0.042*** (0.014)	0.009 (0.021)	-0.176*** (0.024)	0.348*** (0.021)
Event time $e = 5$	-0.025* (0.014)	-0.088*** (0.021)	-0.117*** (0.024)	0.359*** (0.019)
Event time $e = 6$	-0.020 (0.016)	-0.103*** (0.024)	0.052*** (0.020)	—
Event time $e = 7$	0.025 (0.016)	-0.079*** (0.021)	0.162*** (0.023)	—
Anticipation window	4 periods	4 periods	4 periods	4 periods
Control group	Never-treated	Never-treated	Never-treated	Never-treated
Estimator	CS (DR)	CS (DR)	CS (DR)	CS (DR)
Cluster level	Parcel	Parcel	Parcel	Parcel
Number of clusters	232,042	135,116	140,689	153,921
Number of observations	6,961,260	4,053,480	4,220,670	4,617,630
Bootstrap replications	1000	1000	1000	1000
Random seed	2006	2006	2006	2006

Notes: Dynamic CS effects of SIGEF titling on primary forest loss (% of 1985 baseline), split by titling-year cohort. Treated cohorts retained: early (2013–15), mid (2016–18), late (2019–21). Each subsample uses SNCI + Terra Legal parcels as never-treated controls (cleaner cohort comparison than not-yet-treated). The late cohort can only contribute to event times up to $e \approx 3$ ($2021 + 3 = 2024$), so blank cells in col. (4) reflect data availability not estimation issues. Same anticipation/base-period spec, parcel-clustered bootstrap SEs. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Terra Legal Placebo: Specification and Robustness

Under SIGEF, eligibility depends on productive use at the time of titling, which creates an incentive to clear before title. Terra Legal instead granted title for occupation before a fixed date in the past. If the rise in clearing before SIGEF titles reflected selection, administrative timing, or frontier-wide trends rather than the productive-use requirement, Terra Legal should show a similar rise, and it does not. Because this null result carries identifying weight, it must withstand the two problems that can make a null uninformative. The design might lack the power to detect a rise, or the recorded titling dates might be measured with so much error that a response would not line up with them. Section C.2 shows that the null is precisely estimated. Section C.3 uses placebo titling dates to show that the recorded dates carry information, because the modest post-titling response documented for Terra Legal by Probst et al. (2020) appears at the true title date and at no other.

C.1 Specification

All exercises use the Callaway and Sant’Anna (2021) estimator of the main analysis. Let G_i denote the title year of parcel i , t the calendar year, and $e = t - G_i$ event time. For each e , the estimator recovers

$$ATT(e) = \mathbb{E}[Y_{it}(1) - Y_{it}(0) \mid t - G_i = e],$$

where Y_{it} is annual primary-forest loss. The exercises in Section C.3 use the doubly robust estimator with not-yet-treated controls, a universal base period, parcel-clustered bootstrap standard errors, and the covariates of the main analysis. They set the anticipation window to $L = 0$, so that the pre-title dynamics are visible directly. If parcels cleared land to strengthen their claims, the estimates would show the SIGEF pattern, a rise toward the title date peaking at or just before $e = 0$. Terra Legal instead shows a gently declining profile through the anticipation window, followed by a modest short-run increase after titling, consistent with the effect of tenure security on smallholder clearing documented by Probst et al. (2020).

C.2 Precision

In the pooled specification with SSCI controls (Table C.1), the anticipation-window estimates are -0.075 , -0.190 , -0.238 , and -0.191 percentage points of the 1985 forest stock at $e = -4, \dots, -1$, with standard errors between 0.020 and 0.031. The 95 percent confidence interval lies below zero at every horizon in the window, so the data reject a positive anticipatory effect of any size, and in particular effects of the size estimated under SIGEF (0.019 to 0.062 in the anticipation window and 0.145 in the title year; Table 3). The estimator, controls, and outcome are the same as in the SIGEF design, which does detect a rise in the same region and years. Because the far pre-period is

not flat (Section 6.2), we do not interpret the negative estimates as causal effects. The identifying content is the shape of the profile, and nothing in the anticipation window rises toward the title date.

C.3 Placebo in Time

If the recorded titling dates were only loosely related to when claims were resolved, a rise before title could be smeared out, and the null would reflect poor data rather than absent incentives. To test this, we re-estimate the event study around dates known to be wrong. For every Terra Legal parcel we assign a placebo title year

$$G_i^P = G_i - K, \quad K \in \{2, 3, 4\},$$

and estimate the full event study around each placebo date. If the recorded dates carried no information, the true date would look no different from a date two to four years earlier. If they mark a real event, any response to titling should appear at the true date and nowhere else. For horizons of up to $K - 1$ years after a placebo date, every observation predates the true title, so no genuine titling response can contaminate the placebo estimates at those horizons. For this reason the summary in Figure C.2 focuses on $e = 0$ and $e = +1$.

Figure C.1 presents the event studies. Around each placebo date the estimates continue their gradual pre-existing decline, with neither a rise into the date nor a jump after it. Only the true title date is followed by a short-run increase in clearing.

Figure C.2 summarizes the result. At the true title date the estimate at $e = +1$ is positive and significant, reflecting the modest post-titling increase in clearing documented by Probst et al. (2020), while every placebo date produces estimates that are indistinguishable from zero or negative. The one response present in these data appears at the recorded title year, which shows that the titling dates are informative and that a rise before title, had there been one, would have been visible.

Figure C.3 overlays the true and placebo event studies on a common event-time axis. None of the placebo series rises before its date, and all continue the same smooth pre-existing path.

C.4 Additional Tables

Re-anchoring Terra Legal to a fake title date produces no anticipation ramp, no jump

Event study (CS-DR, $L=0$) around each anchor. Peach = anticipation window; dashed = the anchor ($e=0$ just right of it). Placebo anchors sit entirely in the true pre-title era.

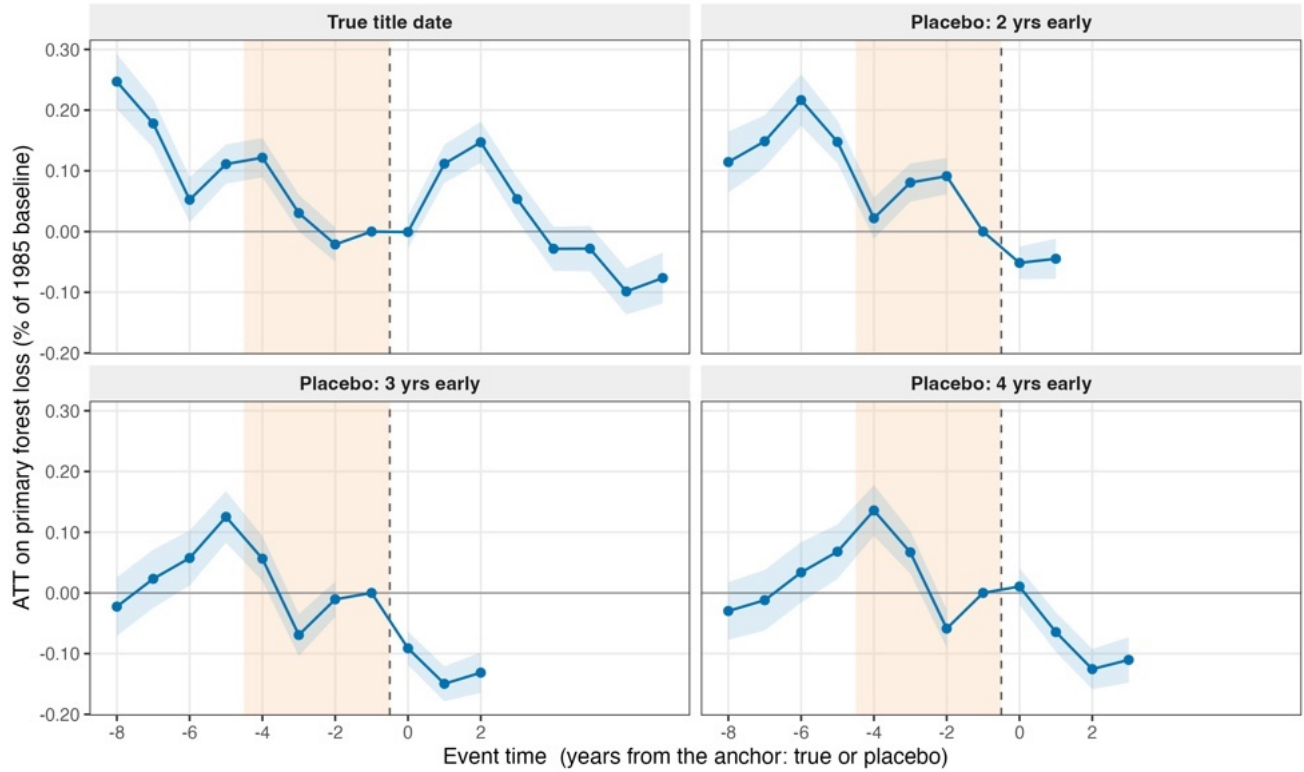


Figure C.1: Placebo in Time: Event Studies Around True and Fake Title Dates

Notes: CS-DR event studies ($L = 0$) for Terra Legal parcels anchored at the true title year and at placebo title years set two, three, and four years earlier. The shaded band marks the four-year anticipation window relative to each anchor. Placebo anchors generate neither an anticipatory ramp nor a jump at the anchor; only the true title date is followed by a short-run post-titling increase.

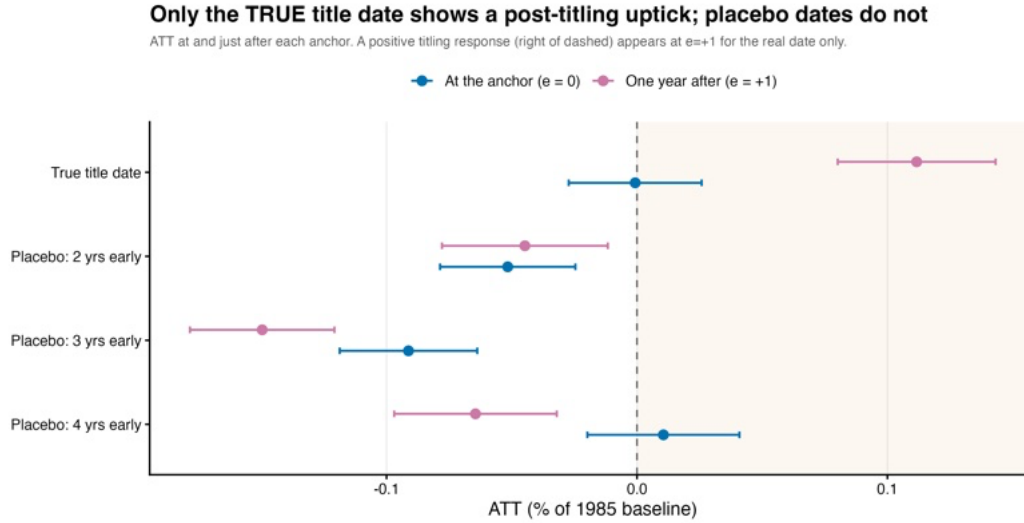


Figure C.2: Response at the Anchor: True versus Placebo Title Dates

Notes: Estimated effects at the anchor ($e = 0$) and one year later ($e = +1$) for the true title date and each placebo date. At these horizons all placebo-anchor observations predate the true title, so no genuine titling response can contaminate the placebo estimates. Only the true title date exhibits a positive post-titling response.

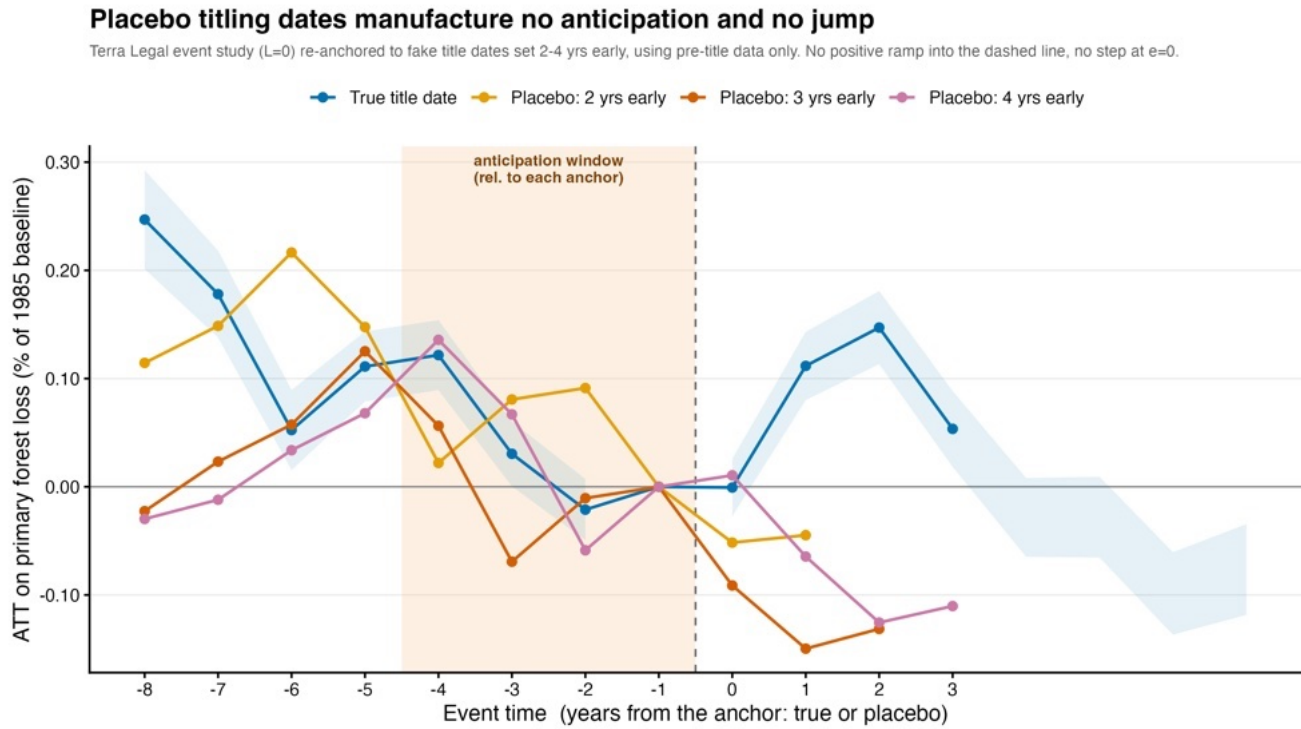


Figure C.3: Overlay of True and Placebo Event Studies

Notes: The four event studies of Figure C.1 on a common event-time axis, each re-centered on its own anchor. The placebo anchors generate neither anticipation nor discontinuities.

Table C.1: Terra Legal Pooled — SSCI Never-Treated Controls (SIGEF excluded)

Outcome: <i>Primary forest loss (% of 1985 baseline)</i>	
<i>Pre-anticipation placebo (parallel-trends test)</i>	
Event time $e = -8$	0.206*** (0.057)
Event time $e = -7$	0.135*** (0.042)
Event time $e = -6$	0.015 (0.033)
Event time $e = -5$	0.000 ref.
<i>Anticipation window (treatment effects allowed)</i>	
Event time $e = -4$	-0.075*** (0.020)
Event time $e = -3$	-0.190*** (0.024)
Event time $e = -2$	-0.238*** (0.027)
Event time $e = -1$	-0.191*** (0.031)
<i>Post-titling effects</i>	
Event time $e = 0$	-0.211*** (0.037)
Event time $e = 1$	-0.090** (0.041)
Event time $e = 2$	-0.018 (0.034)
Event time $e = 3$	-0.089*** (0.032)
Event time $e = 4$	-0.201*** (0.038)
Event time $e = 5$	-0.179*** (0.044)
Event time $e = 6$	-0.206*** (0.038)
Event time $e = 7$	-0.210*** (0.045)
Anticipation window	4 periods
Control group	Never-treated SSCI only (SIGEF excluded)
Estimator	Callaway-Sant'Anna (DR)
Cluster level	Parcel
Number of clusters	98,842
Number of observations	2,965,260
Bootstrap replications	1000
Random seed	2006

Notes: Robustness for the Terra Legal main DiD using only SSCI never-treated parcels as controls (SIGEF dropped). The treated cohort is all Terra Legal parcels with titling dates (cohorts 2013–2019). Same Callaway-Sant'Anna doubly-robust spec with anticipation $L = 4$ and base period $e = -5$; parcel-clustered bootstrap SEs. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2: Terra Legal — Frontier Heterogeneity

	Outcome: <i>Primary forest loss (% of 1985 baseline)</i>		
	(1) Full sample	(2) Arc states PA, MT, RO, MA	(3) Non-Arc states AM, AC, TO, RR, AP
<i>Pre-anticipation placebo (parallel-trends test)</i>			
Event time $e = -8$	0.157*** (0.021)	0.326*** (0.027)	-0.177*** (0.027)
Event time $e = -7$	0.079*** (0.018)	0.163*** (0.025)	-0.072*** (0.022)
Event time $e = -6$	-0.049*** (0.016)	-0.010 (0.021)	-0.098*** (0.021)
Event time $e = -5$	0.000 ref.	0.000 ref.	0.000 ref.
<i>Anticipation window (treatment effects allowed)</i>			
Event time $e = -4$	0.003 (0.015)	-0.065*** (0.020)	0.159*** (0.025)
Event time $e = -3$	-0.072*** (0.016)	-0.111*** (0.021)	0.025 (0.024)
Event time $e = -2$	-0.135*** (0.017)	-0.203*** (0.022)	0.007 (0.022)
Event time $e = -1$	-0.118*** (0.018)	-0.289*** (0.022)	0.230*** (0.027)
<i>Post-titling effects</i>			
Event time $e = 0$	-0.124*** (0.018)	-0.218*** (0.024)	0.072*** (0.026)
Event time $e = 1$	-0.006 (0.019)	-0.153*** (0.024)	0.280*** (0.028)
Event time $e = 2$	0.030 (0.020)	-0.206*** (0.024)	0.480*** (0.034)
Event time $e = 3$	-0.072*** (0.019)	-0.227*** (0.022)	0.230*** (0.029)
Event time $e = 4$	-0.155*** (0.018)	-0.294*** (0.024)	0.118*** (0.028)
Event time $e = 5$	-0.142*** (0.019)	-0.235*** (0.027)	0.043 (0.029)
Event time $e = 6$	-0.209*** (0.019)	-0.325*** (0.026)	0.022 (0.029)
Event time $e = 7$	-0.193*** (0.021)	-0.307*** (0.026)	0.047 (0.033)
Anticipation window	4 periods	4 periods	4 periods
Control group	Not-yet-treated	Not-yet-treated	Not-yet-treated
Estimator	CS (DR)	CS (DR)	CS (DR)
Cluster level	Parcel	Parcel	Parcel
Number of clusters	232,042	202,440	176,016
Number of observations	6,961,260	6,073,200	5,280,480
Bootstrap replications	1000	1000	1000
Random seed	2006	2006	2006

Notes: Dynamic CS effects of Terra Legal titling on primary forest loss (% of 1985 baseline), split by frontier zone. Arc-of-deforestation states are PA, MT, RO, MA; Non-Arc states are AM, AC, TO, RR, AP. Same anticipation $L=4$ / base period $e = -5$ spec, not-yet-treated controls (with SIGEF+SNCI never-treated as the control pool), parcel-clustered bootstrap SEs. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

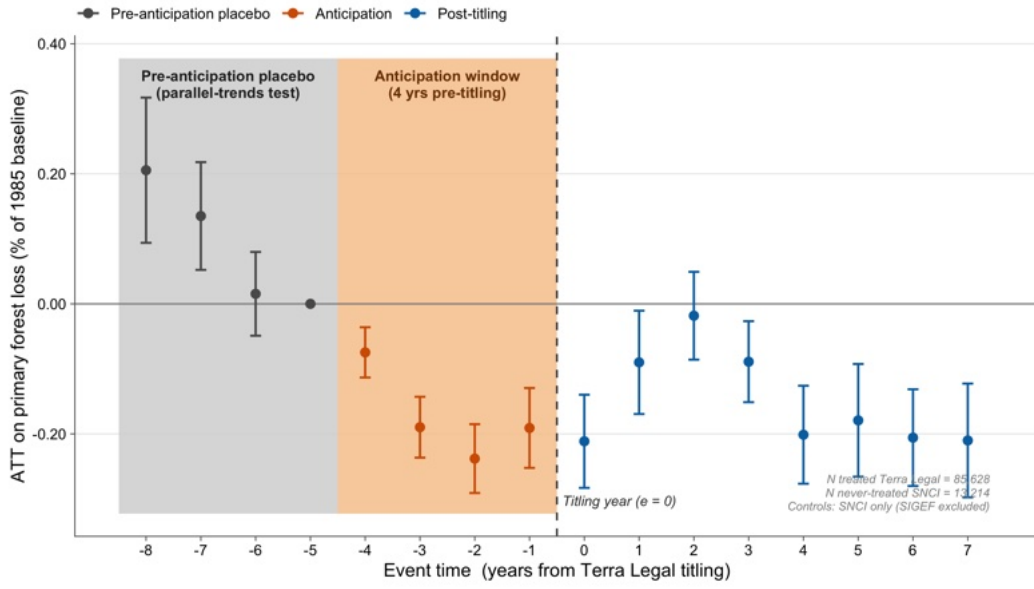


Figure C.4: Terra Legal Event Study with SNCI Controls

Notes: CS-DiD event study for Terra Legal titled parcels with SNCI controls, anticipation $L = 4$, base period $e = -5$. Pre-period ATTs decline rather than staying flat, indicating that parallel trends is not cleanly satisfied for the levels of the Terra Legal ATTs; the relevant design content is the *absence* of any pre-title ramp-up (see Section 6.2).

D CAR Registration: Additional Results

This appendix reports the SICAR estimates behind Section 6.3. The specifications differ in sample and control group in ways that matter for interpretation.

Pooling all SICAR registrations dilutes the response. Most registrations are made by holders of land that is already privately held, for whom registration stakes no new claim. The pooled estimate against titled controls (Table D.3) accordingly shows anticipation coefficients that are a fraction of those on undesignated land (Table D.1), and because its far pre-period is not flat, we treat it as descriptive. The response is concentrated on undesignated public land, where claims are contestable, as Prediction 3 implies.

Table D.2 decomposes the undesignated-land estimates by later path to title and underlies Figure 10 in the main text. The corresponding decomposition for all SICAR parcels (Figure D.2, Table D.4) also shows clearing before registration among orphan parcels, but its far pre-period coefficients are large and positive, so we rely on the undesignated-land decomposition.

Within-SICAR controls are a demanding benchmark. Parcels not yet registered are themselves future claimants who may be clearing ahead of their own registration, so using them as controls removes part of the response and pushes post-registration coefficients below zero, as in the pooled within-SICAR estimates (Figure D.1, Table D.5). The rise before registration on undesignated land nonetheless survives this comparison, with rising and significant effects from $e = -3$ to $e = -1$ (Table D.6), so it does not depend on the comparison with titled controls.

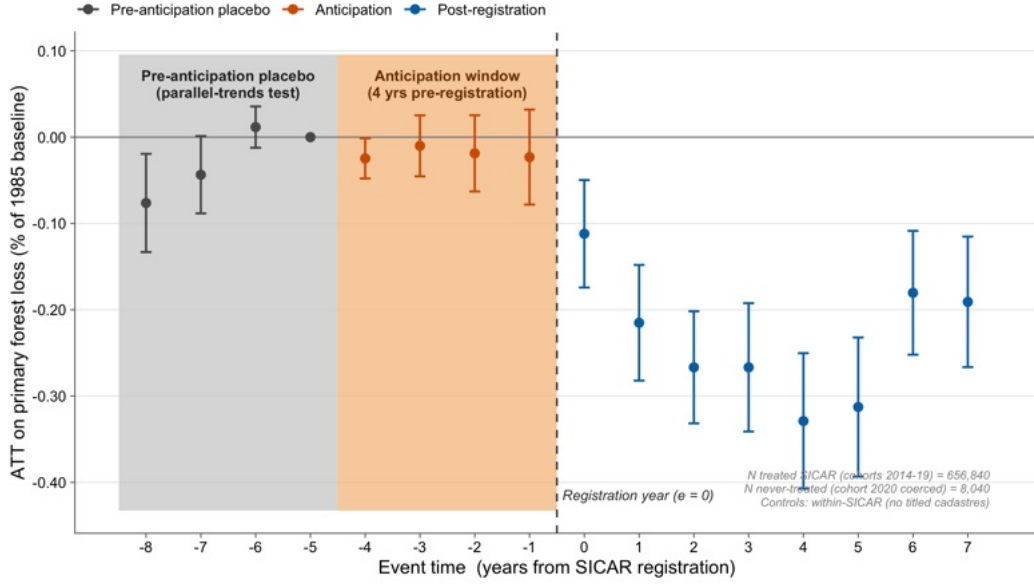


Figure D.1: Pooled SICAR Event Study

Notes: SICAR entrants versus never-treated (2020-cohort) SICAR parcels, all land designations pooled. Controls are within-SICAR and the treated sample is much larger than the undesignated subsample of Figure 9. Anticipation-window coefficients are near zero and post-registration coefficients negative because the not-yet-registered controls are themselves future claimants clearing ahead of their own registration; see the discussion at the start of this section. The informative within-SICAR comparison is the undesignated-land specification, where the anticipatory ramp survives this conservative benchmark.

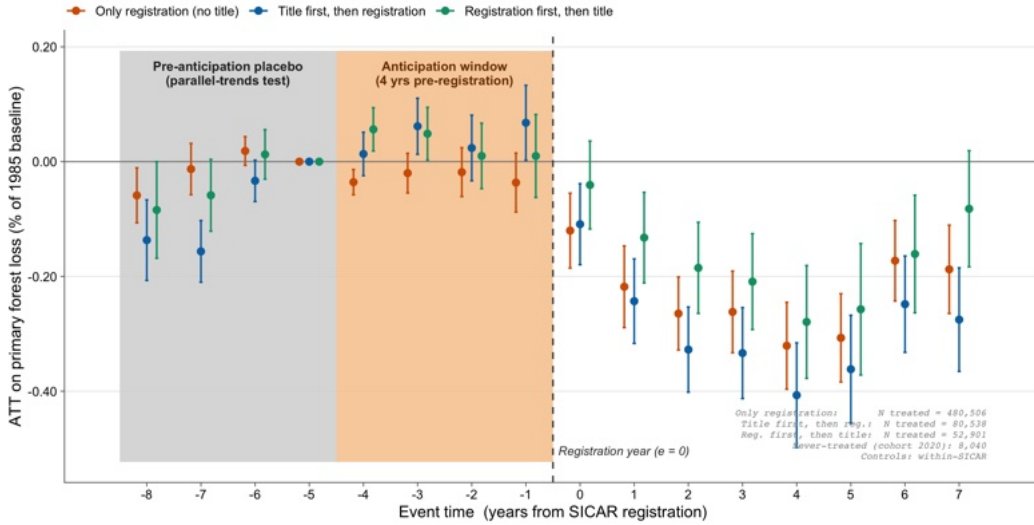


Figure D.2: SICAR Sub-Group Decomposition by Title Pathway

Notes: Event studies by sub-group: only-registration (orphan), title-first-then-registration, and registration-first-then-title, defined from the linkage of Table 1. The orphan and title-first groups show the clearest pre-registration clearing; the registration-first group is smaller and estimated less precisely.

Table D.1: SICAR on Undesignated Land (Pooled) — Dynamic Treatment Effects

Outcome: <i>Primary forest loss (% of 1985 baseline)</i>	
<i>Pre-anticipation placebo (parallel-trends test)</i>	
Event time $e = -8$	-0.082*** (0.025)
Event time $e = -7$	-0.033 (0.025)
Event time $e = -6$	0.002 (0.023)
Event time $e = -5$	0.000 ref.
<i>Anticipation window (treatment effects allowed)</i>	
Event time $e = -4$	0.065*** (0.022)
Event time $e = -3$	0.183*** (0.026)
Event time $e = -2$	0.200*** (0.026)
Event time $e = -1$	0.343*** (0.029)
<i>Post-registration effects</i>	
Event time $e = 0$	0.351*** (0.027)
Event time $e = 1$	0.220*** (0.027)
Event time $e = 2$	0.113*** (0.028)
Event time $e = 3$	0.187*** (0.030)
Event time $e = 4$	0.127*** (0.029)
Event time $e = 5$	0.029 (0.028)
Event time $e = 6$	0.006 (0.029)
Event time $e = 7$	-0.165*** (0.032)
Anticipation window	4 periods
Sample restriction	$\geq 50\%$ area in CNFP undesignated polygons
Control group	Never-treated SIGEF/SNCI/TL
Estimator	Callaway-Sant’Anna (DR)
Cluster level	Parcel
Number of clusters	288,512
Number of observations	8,655,360
Bootstrap replications	1000
Random seed	2006

Notes: Same spec as Table D.3, restricted to SICAR parcels whose geometry sits at least 50% inside CNFP undesignated public-land polygons. This isolates the “orphan claim on undesignated land” sub-population most central to the claim-to-clear hypothesis. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.2: SICAR on Undesignated Land — Sub-group Heterogeneity

	Outcome: <i>Primary forest loss (% of 1985 baseline)</i>		
	(1) Only registration (no title)	(2) Title first, then registration	(3) Registration first, then title
<i>Pre-anticipation placebo (parallel-trends test)</i>			
Event time $e = -8$	-0.087*** (0.032)	-0.046 (0.075)	0.027 (0.129)
Event time $e = -7$	-0.025 (0.027)	-0.024 (0.071)	0.078 (0.107)
Event time $e = -6$	0.010 (0.024)	-0.014 (0.057)	0.183* (0.103)
Event time $e = -5$	0.000 ref.	0.000 ref.	0.000 ref.
<i>Anticipation window (treatment effects allowed)</i>			
Event time $e = -4$	0.032 (0.026)	0.181*** (0.062)	0.091 (0.104)
Event time $e = -3$	0.182*** (0.029)	0.276*** (0.071)	0.136 (0.114)
Event time $e = -2$	0.240*** (0.029)	0.151** (0.070)	0.225 (0.159)
Event time $e = -1$	0.388*** (0.036)	0.324*** (0.075)	0.305** (0.146)
<i>Post-registration effects</i>			
Event time $e = 0$	0.412*** (0.033)	0.212*** (0.073)	0.355** (0.138)
Event time $e = 1$	0.284*** (0.032)	0.092 (0.081)	0.159 (0.145)
Event time $e = 2$	0.184*** (0.031)	-0.083 (0.070)	0.397** (0.159)
Event time $e = 3$	0.244*** (0.032)	-0.025 (0.077)	0.494*** (0.184)
Event time $e = 4$	0.165*** (0.033)	-0.039 (0.073)	0.354** (0.164)
Event time $e = 5$	0.079** (0.031)	-0.147** (0.073)	0.508*** (0.167)
Event time $e = 6$	0.050 (0.034)	-0.262*** (0.075)	0.611*** (0.193)
Event time $e = 7$	-0.110*** (0.037)	-0.488*** (0.085)	0.345* (0.202)
Anticipation window	4 periods	4 periods	4 periods
Sample restriction	$\geq 50\%$ area in CNFP undesignated polygons		
Control group	Never-treated	Never-treated	Never-treated
Estimator	CS (DR)	CS (DR)	CS (DR)
Cluster level	Parcel	Parcel	Parcel
Number of treated parcels	39,669	9,478	1,464
Number of never-treated controls	232,042	232,042	232,042
Number of observations	8,151,330	7,245,600	7,005,180
Bootstrap replications	1000	1000	1000
Random seed	2006	2006	2006

Notes: Same sub-group split as Table D.4, restricted to SICAR parcels with $\geq 50\%$ area inside CNFP undesignated polygons. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.3: SICAR Pooled — Dynamic Treatment Effects with Anticipation Window

Outcome: <i>Primary forest loss (% of 1985 baseline)</i>	
<i>Pre-anticipation placebo (parallel-trends test)</i>	
Event time $e = -8$	0.110*** (0.008)
Event time $e = -7$	0.084*** (0.007)
Event time $e = -6$	0.050*** (0.005)
Event time $e = -5$	0.000 ref.
<i>Anticipation window (treatment effects allowed)</i>	
Event time $e = -4$	0.008* (0.005)
Event time $e = -3$	0.050*** (0.006)
Event time $e = -2$	0.048*** (0.007)
Event time $e = -1$	0.072*** (0.007)
<i>Post-registration effects</i>	
Event time $e = 0$	0.037*** (0.007)
Event time $e = 1$	-0.054*** (0.007)
Event time $e = 2$	-0.133*** (0.007)
Event time $e = 3$	-0.147*** (0.007)
Event time $e = 4$	-0.181*** (0.008)
Event time $e = 5$	-0.237*** (0.008)
Event time $e = 6$	-0.238*** (0.008)
Event time $e = 7$	-0.270*** (0.009)
Anticipation window	4 periods
Control group	Never-treated SIGEF/SNCI/TL
Estimator	Callaway–Sant’Anna (DR)
Cluster level	Parcel
Number of clusters	896,922
Number of observations	26,907,660
Bootstrap replications	1000
Random seed	2006

Notes: Dynamic CS effects of SICAR self-registration on within-parcel primary forest loss (% of 1985 baseline forest stock). Treated cohort: all SICAR parcels (anchor = registration year, 2014–2020); controls: not-yet-registered SICAR cohorts at each (g,t). Same anticipation $L=4$, base period $e = -5$ spec, parcel-clustered bootstrap SEs. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.4: SICAR — Sub-group Heterogeneity (Untitled / Titled / Registered)

	Outcome: <i>Primary forest loss (% of 1985 baseline)</i>		
	(1) Only registration (no title)	(2) Title first, then registration	(3) Registration first, then title
<i>Pre-anticipation placebo (parallel-trends test)</i>			
Event time $e = -8$	0.135*** (0.007)	0.009 (0.023)	0.103*** (0.020)
Event time $e = -7$	0.120*** (0.007)	-0.066*** (0.018)	0.082*** (0.019)
Event time $e = -6$	0.059*** (0.006)	-0.013 (0.014)	0.058*** (0.016)
Event time $e = -5$	0.000 ref.	0.000 ref.	0.000 ref.
<i>Anticipation window (treatment effects allowed)</i>			
Event time $e = -4$	0.000 (0.006)	0.047*** (0.016)	0.061*** (0.014)
Event time $e = -3$	0.045*** (0.007)	0.112*** (0.018)	0.078*** (0.016)
Event time $e = -2$	0.055*** (0.007)	0.077*** (0.018)	0.048*** (0.017)
Event time $e = -1$	0.064*** (0.008)	0.156*** (0.021)	0.072*** (0.019)
<i>Post-registration effects</i>			
Event time $e = 0$	0.037*** (0.009)	0.027 (0.020)	0.073*** (0.018)
Event time $e = 1$	-0.048*** (0.008)	-0.094*** (0.019)	-0.001 (0.017)
Event time $e = 2$	-0.122*** (0.008)	-0.211*** (0.018)	-0.065*** (0.018)
Event time $e = 3$	-0.135*** (0.009)	-0.244*** (0.022)	-0.076*** (0.018)
Event time $e = 4$	-0.170*** (0.009)	-0.280*** (0.021)	-0.098*** (0.019)
Event time $e = 5$	-0.231*** (0.008)	-0.314*** (0.023)	-0.116*** (0.021)
Event time $e = 6$	-0.231*** (0.009)	-0.312*** (0.022)	-0.153*** (0.021)
Event time $e = 7$	-0.262*** (0.010)	-0.355*** (0.020)	-0.166*** (0.021)
Anticipation window	4 periods	4 periods	4 periods
Control group	Never-treated	Never-treated	Never-treated
Estimator	CS (DR)	CS (DR)	CS (DR)
Cluster level	Parcel	Parcel	Parcel
Number of treated parcels	486,747	81,804	53,022
Number of never-treated controls	232,042	232,042	232,042
Number of observations	21,563,670	9,415,380	8,551,920
Bootstrap replications	1000	1000	1000
Random seed	2006	2006	2006

Notes: Dynamic CS effects of SICAR registration on primary forest loss (% of 1985 baseline), split by relative timing of SICAR registration vs. any titled-cadastre overlap: (1) Only registration, no title = SICAR has no titled parent (“orphan”); (2) Title first, then registration = parent cadastre was titled before SICAR registration; (3) Registration first, then title = SICAR was registered before any parent title. Each sub-group keeps its treated SICAR parcels and the same pool of never-treated SIGEF + SNCI + Terra Legal controls. The “only registration” and “registration first” groups are the natural claim-to-clear test: SICAR predates any title. Same anticipation $L=4$ / base period $e = -5$ spec, parcel-clustered bootstrap SEs. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.5: SICAR Pooled — Not-Yet-Registered SICAR Controls

Outcome: <i>Primary forest loss (% of 1985 baseline)</i>	
<i>Pre-anticipation placebo (parallel-trends test)</i>	
Event time $e = -8$	-0.076*** (0.029)
Event time $e = -7$	-0.044* (0.023)
Event time $e = -6$	0.012 (0.012)
Event time $e = -5$	0.000 ref.
<i>Anticipation window (treatment effects allowed)</i>	
Event time $e = -4$	-0.025** (0.012)
Event time $e = -3$	-0.010 (0.018)
Event time $e = -2$	-0.019 (0.023)
Event time $e = -1$	-0.023 (0.028)
<i>Post-registration effects</i>	
Event time $e = 0$	-0.112*** (0.032)
Event time $e = 1$	-0.215*** (0.034)
Event time $e = 2$	-0.267*** (0.033)
Event time $e = 3$	-0.267*** (0.038)
Event time $e = 4$	-0.329*** (0.040)
Event time $e = 5$	-0.313*** (0.041)
Event time $e = 6$	-0.180*** (0.037)
Event time $e = 7$	-0.191*** (0.039)
Anticipation window	4 periods
Control group	Within-SICAR (cohort 2020 as never-treated)
Estimator	Callaway–Sant’Anna (DR)
Cluster level	Parcel
Number of clusters	664,880
Number of observations	19,946,400
Bootstrap replications	1000
Random seed	2006

Notes: Same SICAR pooled spec as Table D.3 but restricted to the SICAR universe (no titled-cadastre controls). To enable estimation across the full $e = -8.. + 7$ window with anticipation $L=4$, the latest SICAR cohort (2020, ~8k parcels) is coerced as never-treated for inference; treated cohorts span 2014–2019. Same parcel-clustered bootstrap SEs. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.6: SICAR on Undesignated Land — Not-Yet-Registered SICAR Controls

Outcome: <i>Primary forest loss (% of 1985 baseline)</i>	
<i>Pre-anticipation placebo</i>	
Event time $e = -8$	-0.240*** (0.039)
Event time $e = -7$	-0.124*** (0.034)
Event time $e = -6$	-0.037 (0.025)
Event time $e = -5$	0.000 ref.
<i>Anticipation window</i>	
Event time $e = -4$	0.040 (0.025)
Event time $e = -3$	0.135*** (0.030)
Event time $e = -2$	0.148*** (0.032)
Event time $e = -1$	0.250*** (0.043)
<i>Post-registration effects</i>	
Event time $e = 0$	0.193*** (0.046)
Event time $e = 1$	0.052 (0.047)
Event time $e = 2$	-0.026 (0.047)
Event time $e = 3$	0.071 (0.055)
Event time $e = 4$	-0.007 (0.052)
Event time $e = 5$	-0.026 (0.059)
Event time $e = 6$	0.118** (0.047)
Event time $e = 7$	-0.072 (0.052)
Anticipation window	4 periods
Sample restriction	$\geq 50\%$ area in CNFP undesignated polygons
Control group	Within-SICAR (cohort 2020 as never-treated)
Estimator	Callaway–Sant’Anna (DR)
Number of clusters	63,925
Number of observations	1,917,750
Bootstrap replications	1000
Random seed	2006

Notes: SICAR-on-undesignated pooled with within-SICAR controls (latest cohort 2020 coerced as never-treated to identify the full $e = -8.. + 7$ window under anticipation $L=4$). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E Bunching and Subdivision: Details

E.1 Running Variable, Sample, and Bins

The running variable is declared property size in fiscal modules, $x \equiv \text{FM}$, with cutoff $c = 4$. The baseline estimation window is $[x_{\min}, x_{\max}] = [2, 6]$, excluding a symmetric donut $[c - h, c + h]$ with $h = 0.10$ FM from the baseline fit. The running variable is binned on a regular grid of width $w = 0.04$ (robust to alternatives; see below). Let $\mathcal{B}$ index bin midpoints $x_b \in [x_{\min}, x_{\max}]$ and define the left-donut set $\mathcal{L} = \{b : x_b \in [c - h, c)\}$.

E.2 Chetty-Style Smooth Counterfactual (Baseline)

Following [Chetty et al. \(2011b\)](#) and [Bertanha et al. \(2023\)](#), we form histogram counts y_b and estimate a smooth counterfactual for log-counts using only bins outside the donut:

$$\log(y_b + 1) = \beta_0 + s(x_b) + \varepsilon_b, \quad b \in \mathcal{B} \setminus \mathcal{L},$$

where $s(\cdot)$ is a flexible polynomial or spline basis (baseline: cubic polynomial; robustness: degrees 2–4 and natural cubic splines). Fitted counts are $\hat{\mu}_b = \exp\{\hat{\beta}_0 + \hat{s}(x_b)\} - 1$ for all bins. The excess mass and its normalized version are

$$\widehat{EM} = \sum_{b \in \mathcal{L}} (y_b - \hat{\mu}_b), \quad \widehat{EM\%} = \frac{\sum_{b \in \mathcal{L}} (y_b - \hat{\mu}_b)}{\sum_{b \in \mathcal{L}} \hat{\mu}_b} \times 100.$$

E.3 Inference: Nonparametric Bootstrap

We resample properties with replacement; for each replication $r = 1, \dots, B$ we re-bin, re-estimate the baseline outside the donut, and recompute $EM^{*(r)}$, reporting the bootstrap standard error, percentile 95% CI, and a two-sided percentile p -value for $H_0 : EM = 0$ (computed as $2 \min\{P_{\text{boot}}(EM^* \leq 0), P_{\text{boot}}(EM^* \geq 0)\}$). The baseline uses $B = 400$ replications; final inference uses larger B where indicated. For comparison we also report the analytic Poisson-type standard error $\sqrt{\sum_{b \in \mathcal{L}} \hat{\mu}_b}$, but we favor bootstrap inference since it captures sampling variation from both the empirical distribution and the baseline fit.

E.4 Robustness and Alternative Baselines

- **Trapezoid / local-affine baseline.** A local affine baseline using the average of $n = 3$ neighbor bins on each side of the excluded band, as a low-dimensional benchmark.
- **Slope clipping.** A per-bin heuristic limiting changes in $\log(\hat{\mu}_b)$ to $\pm M \cdot \Delta x$, useful for visualization and rough sensitivity.

- **Formal slope bounds.** A discrete analogue of the Bertanha et al. (2023) bounds: we minimize (convex program) the predicted mass inside the donut subject to bounds $\pm M \cdot \Delta x$ on adjacent differences in log-predicted mass, holding values outside the donut at the Chetty baseline; this yields a lower bound on baseline predictions inside the donut (upper bounds via multi-start numerical maximization, with solver diagnostics reported).

From the slope-bound solutions we record, for each M , the predicted in-donut mass $\hat{\mu}_{\text{in}}^B(M)$ and implied excess $\widehat{EM}^B(M) = \sum_{b \in \mathcal{L}} y_b - \hat{\mu}_{\text{in}}^B(M)$.

E.5 Sensitivity

We report sensitivity of $\widehat{EM}$ to (i) bin width $w \in \{0.03, 0.04, 0.05\}$; (ii) polynomial degree (2–4) and spline alternatives; (iii) donut half-width h and estimation window; and (iv) restriction to titled parcels and other subpopulations, summarizing min/max $\widehat{EM}$ and $\widehat{EM}^\%$ across the grid.

E.6 McCrary Density-Discontinuity Test

As a complementary manipulation check we estimate the log-density jump at $c = 4$ with bias-corrected one-sided local polynomials (McCrary, 2008), reporting $\log \hat{f}_-(c) - \log \hat{f}_+(c)$ with robust standard errors across a bandwidth grid. Figure E.1 shows the spatial distribution of SICAR parcels around the threshold.

SICAR parcels by fiscal-module class

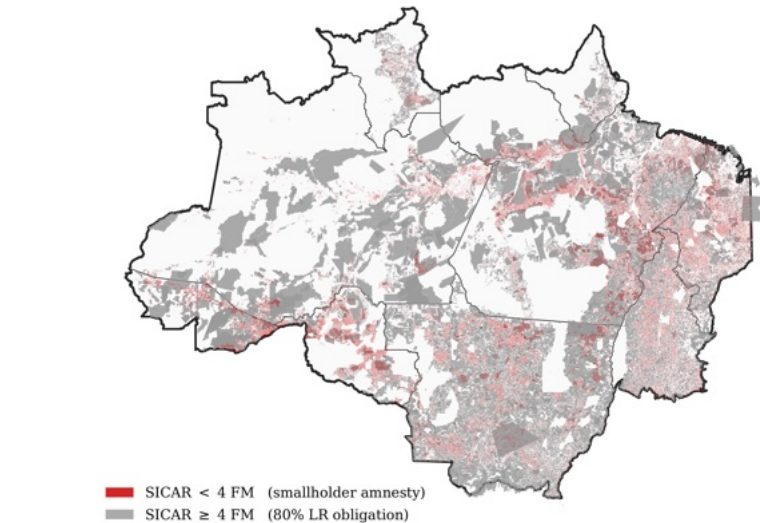


Figure E.1: SICAR Parcels Around the 4-FM Threshold

Notes: SICAR claims below 4 fiscal modules (red) and above the threshold (grey), Legal Amazon. Sub-threshold parcels cluster along active agricultural frontiers.

E.7 Legal Reserve at Stake by State

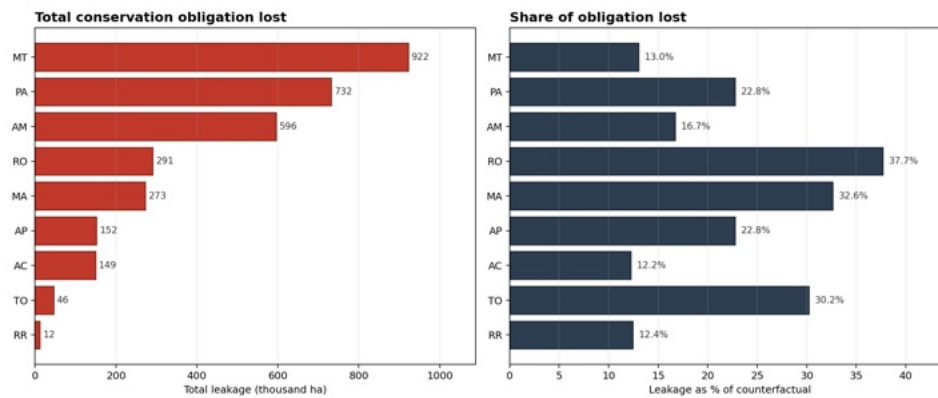


Figure E.2: Conservation Obligation Erased by Subdivision, by State

Notes: State decomposition of the Legal Reserve obligation at stake (at least 3.2 million hectares) when titled parents are re-registered as SICAR children below four fiscal modules. Mato Grosso, Pará, and Amazonas account for about 70% of the total, which equals about 18% of the Legal Reserve the titled parents would owe as whole properties.

F Land-Use Effects at Four Fiscal Modules

Design. The regression-discontinuity design uses declared property size in fiscal modules as the running variable, with the cutoff at four fiscal modules. The outcome is the annual deforestation rate averaged over the period after 2012. We estimate local linear regressions with triangular kernel weights, report several bandwidths, and control for baseline depletion and the Arc indicator. Because the rules tied to the threshold became operational with the 2012 Forest Code and CAR, we also estimate a difference-in-discontinuity design that compares the discontinuity before and after 2012:

$$\hat{\tau}_{\text{DiDisc}} = \left[\lim_{z \downarrow z^*} \hat{m}_{\text{post}}(z) - \lim_{z \uparrow z^*} \hat{m}_{\text{post}}(z) \right] - \left[\lim_{z \downarrow z^*} \hat{m}_{\text{pre}}(z) - \lim_{z \uparrow z^*} \hat{m}_{\text{pre}}(z) \right], \quad (\text{F.1})$$

where $\hat{m}_{\text{pre}}$ and $\hat{m}_{\text{post}}$ are conditional expectation functions estimated for 2005–2011 and 2013–2022. The difference-in-discontinuity removes any discontinuity at the threshold that predates the Code. Given the manipulation of declared size documented in Section 6.4, we interpret these estimates as describing the realized difference between the two sides of the threshold under sorting, not as causal effects at the cutoff.

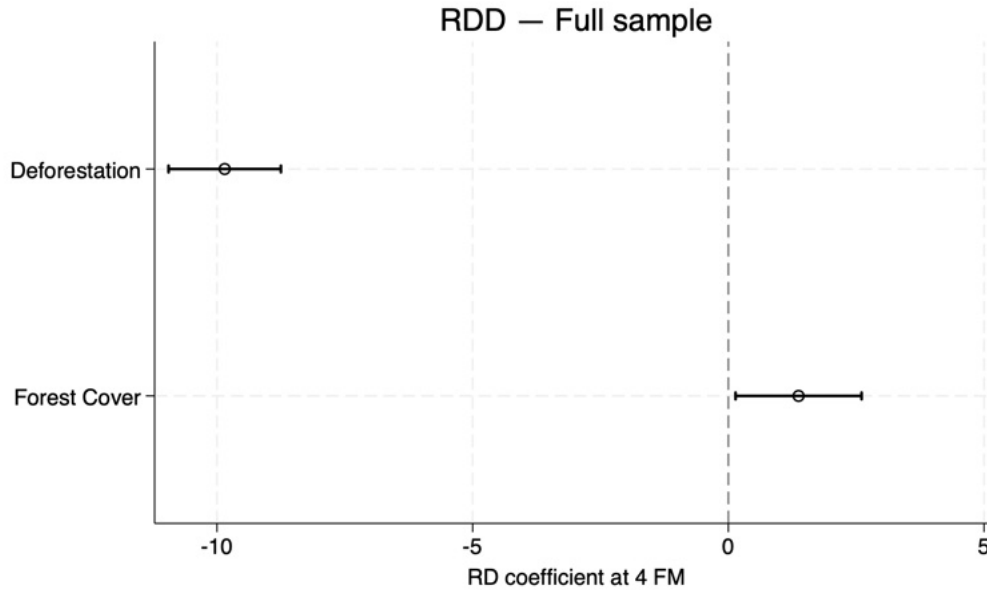


Figure F.1: RDD: Legal Reserve Obligation and Deforestation at the 4-FM Threshold

Notes: Local linear RD at 4 FM. Outcome: annual deforestation rate (primary forest loss as a fraction of remaining forest, averaged 2013–2022). Points are binned means; lines are local linear fits.

Results. Annual deforestation is 3.4 percentage points lower just above the cutoff, and the difference is significant at the 1 percent level. The difference-in-discontinuity estimates show that the gap emerged with the 2012 Code. The discontinuity before 2012 is not significant, while

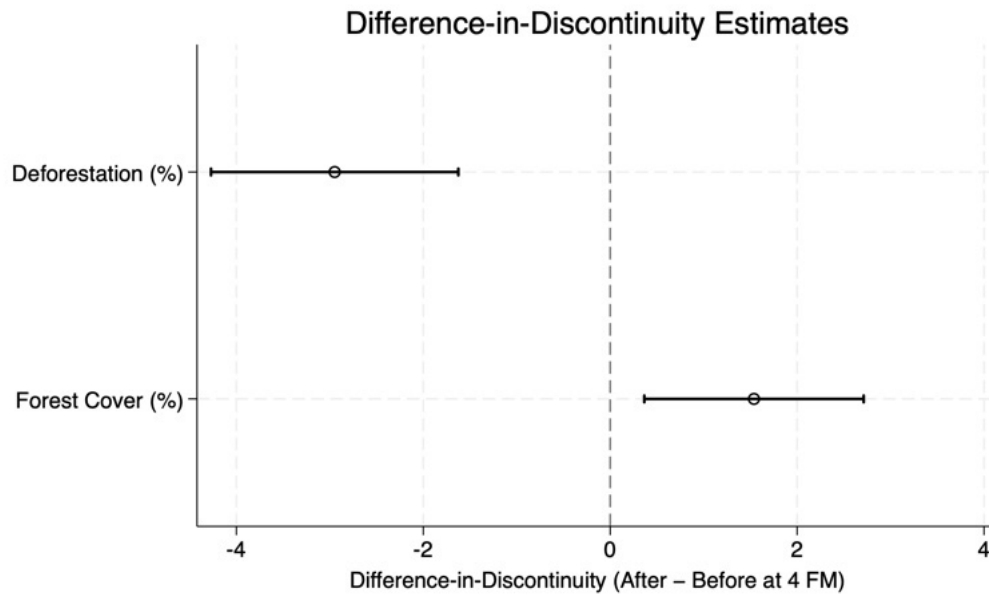


Figure F.2: Difference-in-Discontinuity: The 2012 Forest Code at 4 FM

Notes: RD discontinuities in deforestation rates before (2005–2011) and after (2013–2022) the 2012 Forest Code. The pre-Code discontinuity is small and statistically indistinguishable from zero; the post-Code discontinuity is 1.8–3.4 pp.

the discontinuity after 2012 ranges from 1.8 to 3.4 percentage points. The gap is larger in Arc states, reaching 5.1 percentage points in Rondônia. Because landholders who expected to clear had reason to move below the threshold on paper, properties remaining above it are likely to be more compliant on average, and the estimates should be read with that selection in mind. Together with the bunching and subdivision evidence, they suggest that sorting around the threshold weakened the Forest Code’s effect on forest cover.