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Does titling private property spur or stem deforestation? Evidence from Bolivia*

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Abstract

Over the past quarter century, policymakers have spent billions of dollars on initiatives aimed at providing formal legal title to landholders in the Global South. But evaluations of these initiatives have focused on socioeconomic outcomes, not environmental ones. We use official property-level cadastral records, fine-scale satellite data on land use, and heterogeneity-robust difference-in-difference estimators to analyze the effect of titling rural private properties on forest loss in Bolivia, a country with both a long-standing land titling initiative and exceptionally high deforestation rates. We find that the adverse effect of titling on forest cover was modest but nontrivial in the short term and potentially more substantial in the medium term. On the average property, titling boosted forest loss by 23–29 hectares in the ten years after titles were awarded, a 7–8 percent increase over what would have occurred otherwise. Moreover, the magnitude of this effect increased monotonically during the 10-year event window. Results for other land uses suggest that agriculture was at least as important as pasture as a driver of forest loss associated with titling. Finally, we find that titling had particularly large deforestation effects on properties hosting large commercial farms and those in Santa Cruz department.

Keywords: Land regularization, land formalization, tenure security, property rights, forest loss, land use change, staggered difference-in-differences, heterogeneity robust, Bolivia

JEL Codes: Q15, Q23, Q56, C18

1 Introduction

Over the past 25 years, national governments, multilateral development banks, and other stakeholders have spent at least US $2.5 billion to provide formal legal title to landholders in the Global South (Tseng et al., 2021). The principal goals have been to boost productivity, incomes, and other socioeconomic outcomes (Holland and Diop, 2022). A considerable literature evaluates the effects of land titling programs on these outcomes. However, far less attention has been devoted to the effects of land titling on environmental outcomes, particularly deforestation (Busch and Ferretti-Gallon, 2023; Tseng et al., 2021; Börner et al., 2020), which remains a pressing global problem (Goldman et al., 2025). As discussed in the next section, both theory and empirical evidence suggest that land titling programs have countervailing effects on deforestation and that the net effect is an empirical matter.

Bolivia offers an important opportunity to help fill this gap in the evidence. Since 2000, the country has ranked among the world's top ten with the most net forest loss (FAO, 2010; FAO, 2020). And during this same period, and for several decades leading up to it, the Bolivian government, with funding and technical support from international cooperation organizations, led a high-profile effort to title rural properties. Yet to our knowledge, the effects of this effort on forest loss have not been rigorously examined.

We use official property-level cadastral records, fine-scale satellite data on land use, and heterogeneity-robust difference-in-difference estimators to analyze the effect on forest loss of rural land titling in Bolivia. Our sample consists of private properties classified as 'medium' and 'enterprise', which accounted for more than one-third of the country's forest loss during our 1986–2023 study period. We find that on these properties, the adverse effect of titling on forest cover was modest but nontrivial in the short term and potentially more substantial in the medium term. On the average property, titling boosted forest loss by 23–29 hectares in the ten years after title was awarded, a 7–8 percent increase over what would have occurred otherwise. Moreover, on the average property, the magnitude of this effect increased monotonically during the 10-year event window. Regression results for other land uses suggest that agriculture was at least as important as pasture as a driver of forest loss associated with titling. Finally, we find that titling had particularly large deforestation effects on properties hosting large commercial farms and those in the department of Santa Cruz.

Our study makes three contributions to the literature. First, it adds to the somewhat thin set of counterfactual studies that examine the effect on deforestation of titling private properties. Second, to our knowledge, it is the first counterfactual evaluation of the effect on forest loss of Bolivia's longstanding rural titling initiative. Finally, it identifies both the specific land use changes associated with titling private properties, and the characteristics of the properties on which these changes occur, information that can help policy makers target and tailor efforts to minimize deforestation associated with land titling.

The remainder of this paper is organized as follows. The next section briefly reviews the theoretical and empirical literature on the link between deforestation and titling private properties. The third section provides background on deforestation, agrarian reform, and the legal and regulatory systems governing land titling and land use in Bolivia. The fourth section presents our empirical strategy and the fifth section describes our sample and data. The sixth section summarizes our results and presents robustness checks. And the last section offers a discussion of the limitations of our study, the relevance of our findings, the causal mechanisms and dynamics that underpin them, and their policy implications.

2 Literature

Economic theory suggests that titling and other interventions that strengthen land tenure security on private property have an ambiguous overall impact on deforestation as a result of two countervailing effects known in the literature as the 'investment effect,' which spurs deforestation, and the 'conservation effect,' which stems it (Liscow, 2013). The starting point for both effects is the notion that titling a property reduces the land manager's risk that at some point in the future, the property will either be expropriated by the state or 'grabbed' by private sector agents. The investment effect obtains when that reduced risk increases the long-run returns the landowner expects to obtain from agriculture, pasture and other cleared land uses relative to the returns from forested land uses from, for example, harvesting timber and non-timber and forest products (Farzin, 1984). The conservation effect obtains when the reduced risk has the opposite effect (Mendelsohn, 1994; Barbier and Burgess, 2001). Which effect dominates depends critically on place-based factors such as the capital requirements and input prices associated with each land use (Farzin, 1984).

In addition to the conservation and investment effects, the theoretical and empirical literature has identified several other mechanisms for the effect of land titling on deforestation, which also have countervailing impacts. On one hand, titling can spur deforestation by providing land managers with additional collateral that in turn enhances their access to the credit needed to finance land use change (Feder and Feeny, 1991). On the other hand, titling can stem deforestation by enabling land managers to participate in payments for environmental services (PES) programs, reducing emissions from deforestation and degradation plus (REDD+) initiatives, and other conservation schemes for which formal land rights typically are a prerequisite (Jones et al., 2017). Finally, titling can promote conservation by weakening incentives to clear land in order to lay claim to it (Place and Otsuka, 2001; Brasselle et al., 2002).

Given all of these countervailing effects, the net effect of titling private properties on deforestation is an empirical matter[1]. Counterfactual evidence on that net effect is thin and mixed. Several studies find that titling and/or other land tenure security interventions exacerbate deforestation on private property. Probst et al. (2020) use property-level panel data and two-way fixed effects regressions to identify the effect of a large-scale land-titling program on deforestation in the Brazilian Amazon. They find that the program increased deforestation on small and medium private properties, most likely because it boosted the expected returns to agriculture and ranching. Liscow (2013) relies on cross-sectional property-level

[1]A separate literature examines the effect of titling communal properties on deforestation, see e.g., Baragwanath and Bayi (2020), Blackman et al. (2017), and Vélez et al. (2020)

data (for both private and communal properties) along with instrumental variable models to identify the effect on deforestation of Nicaragua's Sandinista agrarian reform during the 1980s. He finds that titling associated with this reform exacerbated deforestation, and attributes that result to both increases in the expected long run return to cleared land uses and to enhanced access to credit. Walker (2021) uses cross-sectional property-level data along with propensity score matching to examine the effect on deforestation of a large-scale titling campaign in Panama. She finds that the campaign was associated with forest loss and hypothesizes that that effect was due to speculative deforestation by land holders seeking to obtain title. Finally, Abman and Carney (2020) use administrative unit-level panel data along with two-way fixed effects models to explore the impact of holding formal land titles on deforestation in Vietnam. They find that title was positively correlated with deforestation.

Other counterfactual studies have found that titling private properties either stems or has no discernable effect on deforestation. Relying on a randomized controlled trial, Wren-Lewis et al. (2020) find that a program in Benin that combined titling with community forest management reduced deforestation. Holland et al. (2017) use a mixed-methods approach to assess the impact of a 'forest-friendly' titling program on lands surrounding the Cuyabeno Reserve in Ecuador. They find that titling complemented by land use change restrictions reduced deforestation by more than one-third, while titling without such restrictions had no discernable effect. Finally, Lipscomb and Prabakaran (2020) use county-level panel data along with two-way fixed effects models to explore the effect of the same titling program in the Brazilian Amazon examined by Probst et al. (2020). They are not able to discern an effect.

3 Background

Here we provide background on deforestation, agrarian reform, and the legal and regulatory systems governing land titling and land use in Bolivia.

3.1 Forests

Bolivia's forests comprise 51 million hectares and cover more than half of the country's territory. More than two-thirds of these forests are primary and more than three-quarters are located in the lowland region in the northern and eastern parts of the country (FAO, 2020).

Although rates of deforestation in Bolivia were relatively low until the 1980s, they have risen dramatically since then. The country has lost 3–4 percent of its forest cover since 1990 (FAO, 2025). Since 2000, Bolivia has ranked among the ten countries in the world with the highest annual net forest loss (FAO, 2010; FAO, 2020; FAO, 2025). In 2024, it lost 1.48 million hectares of primary forest, more than any other country in the world except Brazil.

The three leading proximate causes of deforestation in lowland Bolivia have been, in order of importance, mechanized agriculture, cattle ranching, and small-scale agriculture (Müller et al., 2013). These proximate causes have, in turn, been driven by four interrelated factors: first, migration from highlands to lowlands, actively encouraged by the state starting as early as the 1940s and more recently propelled by ill-defined land tenure and economic opportunities related to agriculture and ranching in the lowlands; second, a dramatic expansion of commercial agriculture due partly to the 2000–2014 Latin American commodity boom; third, the accelerating exploitation of mineral and energy resources, including gold, natural gas, oil, and hydroelectricity; and finally the completion of a series major infrastructure projects, including roads, power plants, and agricultural processing facilities (Wanderly et al., 2018).

3.2 Agrarian reform

Bolivia's current land tenure system is a product more than six decades of agrarian reform that can be divided into three broad phases, each of which had different effects in the lowlands than in the highlands (Fundación ARU-INRA, 2025).

Phase I: 1953-1995. The Agrarian Reform Law (Decree Law 3464) of 1953 marked the beginning of the first phase. It aimed to abolish forced labor, redistribute land held in large quasi-feudal estates to the laborers they employed, and promote rural development. The National Institute of Agrarian Reform (*Instituto Nacional de Reforma Agraria*, INRA) was established in 1958 to manage these reforms.

Over next three decades, substantive agrarian reform mainly occurred in the Andean highlands where 25 million hectares of land was redistributed to more than 630,000 households mostly as micro-landholdings (Killeen, 2024; Fundación ARU-INRA, 2025). In the lowlands, reform played out differently: 25 to 30 million hectares of land were redistributed, mostly to local economic elites as medium and large landholdings (Fundación ARU-INRA, 2025). By the late 1980s, the slow pace of the reforms and their inconsistent implementation across geographies led to growing dissatisfaction with INRA and to intermittent civil unrest.

Phase II: 1996-2005. The second phase of agrarian reform began with the passage of the New Agrarian Reform Law of 1996 (Law 1715), which reflected then ascendent neoliberal thinking. It aimed to modernize the land tenure system, improve the functioning of private land markets, and enhance administrative efficiency. To those ends, it established a more systematic process for defining, registering, and titling properties, set a 10-year deadline for completing a comprehensive national land registry, and established a framework for formally titling indigenous communal lands, which were principally located in the lowlands.

Phase III: 2006-present. The third phase of Bolivia's agrarian reform began with the 2006 election of the left-leaning Morales administration and the subsequent passage of the Redirection of Agrarian Reform Law of 2006 (Law 3545) that prioritized titling indigenous communal lands, expanded the state's power to expropriate landholdings deemed unproductive, and extended by seven years the deadline for completing the national land registry. This law quickly spurred political opposition in lowland departments dominated by commercial agriculture. That opposition in turn, blunted the law's implementation there (Killeen, 2024; Fundación ARU-INRA, 2025). The 2006 law was followed three years later the by the new Constitution of 2009, which strengthened the legal basis for recognizing the land rights of both indigenous communities and indigenous migrants to the lowlands, set a (nonretroactive) cap of 5,000 hectares on the size private landholdings, and reaffirmed that small properties are indivisible, inalienable, and exempt from agricultural property taxes.

3.3 Legal and regulatory systems

The laws and regulations summarized above established three categories of private property mainly defined by their size: small ($<$ 500 hectares), medium (500–2500 hectares), and agricultural enterprises (2500–5000 hectares, hereafter simply "enterprises")[2]. For the reasons discussed in Section 5, our analysis focuses on medium and enterprise properties.

Medium and enterprise properties have been a significant contributor to forest loss in Bolivia. In 1985, 49 percent of the average sample property was forested (Table 1). During our 38-year study period, the average medium and enterprise property lost 36 percent of its forest land. Collectively, this loss comprised 35 percent of all deforestation in Bolivia during our study period.

Three processes have been used to award title to private properties. The first, Integrated Regularization with Legal Cadaster (CAT-SAN), typically entails government-led cadastral sweeps that define, register, and title all properties in a defined geographic area. The second is Simple Land Regularization (SAN-SIM), which entails either landowners initiating the titling process, or the state initiating it in response to land conflict. The third is Regularization of Indigenous Communal Lands (CAT-TCO), which entails partitioning overlapping claims by indigenous communities and private properties.

Rules governing the conditions under which the state can expropriate private property

[2] These laws and regulations also established two categories of communal property defined by the ethnicity of their inhabitants: Native Peasant Titled Territories (originally called Original Community Lands), which are managed by indigenous groups, and Communal Properties, which are not.

have played an important role in land tilting in Bolivia and its link to land use change. First established during the early days of Bolivia's agrarian reform as a means of redistributing land held by large estates, these rules effectively require that those seeking to obtain a new title or retain an existing one demonstrate that their land fulfills an Economic-Social Function (*Función Económico-Social*, FES). In forested areas, FES is typically demonstrated by clearing at least some forest to harvest timber and/or expand agriculture or pasture (Killeen, 2024; Müller et al., 2014).[3] Hence, FES requirements have created powerful incentives for deforestation. These requirements do not apply to small properties ($<$ 500 hectares). INRA is supposed to monitor compliance with FES requirements every two years.

4 Empirical strategy

Our data are at the level of the properties (cadastral polygons). Because property boundaries are defined only when formal title is awarded, our regression sample consists only of ever-titled units: it does not include any never-titled units. As discussed below, this feature of our sample helps to control for confounders correlated with selection into the treatment.

Our identification strategy exploits temporal variation in the award of title due in part to the staggered rollout of INRA's land titling campaigns across municipalities (second-level administrative units), which is discussed below.

To identify the cumulative effect of titling, we compare outcomes on already-titled properties with those on not-yet-titled properties during a 10-year event window following the award of title. We use a 10-year window because, as discussed in the next section, our data do not accommodate a longer one: 73 percent of the properties in our sample were awarded title within 10 years of the last year of our panel. More specifically, we estimate generalized

[3] The criteria for FES have changed over time. Originally, landowners needed to demonstrate that they were using land for agriculture, commercial forestry, or raising livestock. The Forest Law of 1996, aimed to mitigate the resulting incentives for deforestation by broadening FES criteria to include biodiversity conservation, research, and ecotourism. However, in practice, the easiest and most certain way to demonstrate FES remained clearing forest for agriculture, commercial forestry, and livestock (Müller et al., 2014). In any case, the Redirection of Agrarian Reform Law of 2006 (Law 3545) reestablished the original criteria for FES on the grounds that the relaxed criteria could be used to justify large landholdings.

difference-in-differences (two-way fixed effects) models of the form

$$y_{itl} = \alpha_i + \lambda_t + \delta_l title_{it=1,2,\ldots,10} + \sigma_l title_{it=10,11,\ldots,n} + X_i' t \beta_l + \varepsilon_{itl} \qquad (l = 1, 2, \ldots, n) \quad (1)$$

where i indexes properties, t indexes years, l indexes land uses, y is the number of hectares in a given land use, α are property fixed effects, *lambda* are year fixed effects, *title* is a binary dummy that indicates whether the property was titled, X is a vector of time-varying control variables, δ, σ and β are parameters or vectors of parameters to be estimated, and ε is an error term. The property fixed effects control for unobserved time-invariant property heterogeneity (due to for example, altitude, slope and other geophysical factors) and the year fixed effects control for unobserved temporal heterogeneity affecting all properties (due to for example, changes in national land rights policies and international agricultural commodity prices). The estimated parameter δ is the average treatment effect on the treated (ATT) during the 10-year event window. It can be interpreted as the average number of hectares of land use l gained or lost during that period as a result of the award of title.

In addition to Equation (1), we explore the dynamic single-year effects of titling by estimating event study models of the form

$$y_{itl} = \alpha_i + \lambda_t + \sum_{j=-5}^{-1} \theta_{lj} title\ year\ j_{it} + \sum_{k=0}^{10} \omega_{lk} title\ year\ k_{it} + X_{it}' \beta_l \qquad (l = 1, 2, \ldots, n) \quad (2)$$

where j indexes single-year temporal leads, k indexes single-year temporal lags, *title year j* is an indicator of whether property i was titled in the jth year before year t, *title year k* is an indicator of whether property i was titled in the kth year after year t, θ's are treatment lead parameters to be estimated, and ω's are treatment lag parameters to be estimated. The estimated parameter θ_{lj} can be interpreted as the average number of hectares of land use l gained or lost due to the award of title in the jth year before titling and the estimated parameter ω_{lj} can be interpreted as the average number of hectares of land use l gained or lost due to the award of title in the kth year after titling.

Finally, to explore treatment effect heterogeneity, we estimate models that include interactions effects with the form

$$\begin{aligned} y_{itl} = \alpha_i + \lambda_t + \delta_l title_{it=1,2,...,10} + \sigma_l title_{it=10,11,...,n} + \varphi title_{it=1,2,...,10} Z_i + \\ \gamma_l title_{it=10,11,...,n} Z_i + X_i' t \beta_l + \varepsilon_{itl} \qquad (l = 1, 2, \ldots, n) \end{aligned} \tag{3}$$

where Z is a vector of time invariant property characteristics, and φ and γ are parameters to be estimated.

We estimate Equations (1-3) using both ordinary least squares (OLS) and three estimators discussed below that are robust to heterogenous treatment effects. We cluster standard errors at the property-level.

These three sets of models (Equations 1-3) require four identifying assumptions. The first is conditional parallel trends, which in our case requires that had properties not been titled, average land use outcomes for treated properties conditioned on property fixed effects, year fixed effects, and the time-varying covariates in X would have evolved in parallel to those of the not-yet-treated properties. The second identifying assumption is no anticipation, which in our case requires that land managers' land use decisions prior to titling were not affected by anticipation of titling. The third assumption is the stable unit treatment value assumption (SUTVA), which in our case requires that land managers' land use decisions were not affected by the award of title to other land managers. Although these three assumptions cannot be directly tested, as discussed below, we conduct robustness checks that reflect on their plausibility.

The final identifying assumption for generalized difference-in-difference models like ours is that treatment effects are homogenous across cohorts. Recent research has shown that absent homogeneity, generalized difference-in-differences OLS models can generate biased treatment effects estimates (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfoeuille, 2023).[4] In our case, homogeneity requires that the effect of titling on land use be the

[4]More specifically recent research has established that the treatment effect generated by a generalized

same for early titled properties and late titled ones. The plausibility of that assumption is open to question since our 38-year panel spans significant changes in socio-economic and political conditions, as well as properties hundreds of kilometers apart. Therefore, to address this threat to identification, we use three estimators designed to be robust to heterogenous treatment effects: Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfoeuille (2020) and Sun and Abraham (2021). Intuition for these estimators and the advantages and disadvantages of each are summarized in Roth et al. (2023) and Wang et al. (2024).

Finally, as noted above, because our regression sample includes only ever-treated units, it controls for both observed and unobserved confounding factors correlated with selection into the treatment (Wang et al., 2024). By construction, only properties that have selected into the treatment, and that therefore share unobserved properties correlated with that selection, are included in the sample.

5 Sample and data

Here we discuss our regression sample, define the variables used in our regression analysis, and present descriptive statistics.

5.1 Sample

As noted above, our sample comprises medium and enterprise properties, and not small ones. There are three reasons. First, medium and enterprise properties contain the large majority of forest land in private properties: 65 percent in 1985, the year before the first

difference-in-difference OLS estimator can be expressed as a weighted average of treatment effects derived from multiple simple two-by-two difference-in-difference sub-experiments in which the treatment group consists of units exposed to the treatment in a given period, and the control group consists of units exposed in other periods (de Chaisemartin and D'Haultfoeuille, 2023). Ideally, these sub-experiments entail only 'clean' comparisons between treated units exposed in a given period and control units that have yet to be exposed (later treated). However, when treatment effects differ across groups or over time, the OLS estimator can make 'forbidden' comparisons with control units that already have been exposed (earlier treated). Forbidden comparisons can result in negative weights being assigned to treatment effects from certain sub-experiments, which in, turn can bias overall treatment effect estimates (Goodman-Bacon, 2021).

year of our panel. Second, as discussed above, medium and enterprise properties are quite different from small properties not only in terms of size, but also in terms of institutional and regulatory factors that likely moderate the effect of titling on deforestation: medium and enterprise properties typically host commercial farms while small properties typically host subsistence farms; medium and enterprise properties can be used for collateral and can be sold in whole or part, while small ones cannot; and medium and enterprise properties must demonstrate that their land fulfills an Economic-Social Function (FES) and must pay agricultural property taxes, while small properties do not have to do either of those things. And finally, medium and enterprise properties, and not small ones, are the logical target of policy efforts to stem forest loss simply because it is much more cost effective to engage with roughly 8,000 medium and enterprise properties averaging 1,800 hectares versus roughly 111,000 lowland small properties averaging 42 hectares.

As of 2025, 7,775 properties in Bolivia were classified as either medium or enterprise. Of these properties, the year title was awarded is available for 7,298 (94 percent), which comprise our regression sample. The lion's share of these properties are located in the eastern departments of Beni, Santa Cruz, Chuquisaca, and Tarija (Figure 1). Collectively, they account for 12 percent of the country's territory and in 1985, the year before the first year of our study period, they contained 11 percent of its forestlands (Table 1).

[Insert Table 1 and Figure 1 here]

Our panel spans 1986–2023, the 38 years for which our land use outcome data are available. Hence, our regression sample comprises (7,298 properties $\times$ 38 years =) 277,324 property-year observations.

5.2 Variables

5.2.1 Land use and land cover

Derived from Landsat satellite images, our annual land use and land cover data have a spatial resolution of 30m×30m and span the years 1986–2023 (MapBiomas Bolivia, 2025). They include six broad classes and 19 subclasses. The broad classes are forest formation, nonforest natural formation, farming, nonvegetated areas, waterbodies, and not observed. We use these data to define four property-level dependent variables: *forest* is the number of hectares of land in a property assigned to the forest formation class; *pasture* is the number of hectares assigned to the pasture subclass; *agriculture* is the number of hectares assigned to the agriculture subclass; and *agriculture+pasture* is the number of hectares assigned to the agriculture or pasture subclasses (Table 2).[5] In addition, we use these data to define a moderator variable, *pasture 1985*, which is the number of hectares of pasture in 1985.

[Insert Table 2 here]

5.2.2 Cadastral

The boundaries of properties, their characteristics at the time of title, and the date of title are derived from INRA's comprehensive national cadaster (INRA, 2025). We use this cadaster to define three property-level independent variables of interest: *title*, a dummy that indicates whether the property was titled in year t; *title year j*, a lead dummy that indicates whether the property was titled j years after t; and *title year k*, a lag dummy that indicates whether the property was titled k years before t (Table 2). In addition, we use the cadastral data to generate several moderator binary dummies, all defined at the time title was awarded: *male owners* indicates whether the property had only male owners; *cattle* indicates whether cattle was deemed the main economic activity on the property; *legal entity* indicates whether

[5]The data include three farming subclasses: agriculture, pasture, and mosaic, a mixture of forests and cleared land uses. We do not include mosaic in our analysis because it is rare in our data. The average property in our sample comprised 1,788.31 hectares of which only 24.71 hectares (1 percent) were classified as mosaic.

it was owned by a legal entity (versus family); *enterprise* indicates whether it was designated as an agricultural enterprise; *medium* indicates whether it was designated as such; and *early title* indicates whether title was awarded before 2010.

In addition to these variables used in the regression analysis, we use the cadastral data to define several variables that are not included as regressors, but for which we report descriptive statistics. The variable *size* is the geographic extent of the property in hectares; and three dummies, *CAT-SAN*, *SAN-SIM*, and *SAN-TCO* identify properties titled via a cadastral sweep, a more ad hoc process, or via a process of partitioning communal and private property.

5.2.3 Weather

We use two time-varying weather variables as controls, both derived from Copernicus satellite data (C3S, 2017). The variable *rainfall* is average annual precipitation in meters, and the variable *temperature* is average annual temperature in °C.

5.3 Descriptive statistics

The average sample property comprises 1,788 hectares, and the median property comprises 1,207 hectares (Table 2). As noted above, our sample consists of properties that INRA designates as medium and enterprises, both defined principally by their size: 250–2500 hectares for medium properties and 2501–5000 hectares for agricultural enterprises (Figure 2). However, properties can be designated as medium or enterprises based on other criteria.[6]

[Insert Figure 2 here]

Over our entire study period, the average property comprised 810 hectares of forest, 116 hectares of agriculture, and 113 hectares of pasture (Table 2). However, land use and land

[6] For example, properties smaller than 250 hectares are included in the enterprise category when they feature commercial (versus subsistence) operations such as collective processing mills, or storage and distribution facilities. And properties larger than 5000 hectares are included in the enterprises category when titled prior to the promulgation of the 5000 hectare cap in 2009.

cover in our sample properties changed significantly during this period (Figure 3). The total area with forest fell from 6.95 million hectares in 1985 to 4.45 million hectares in 2023, a 36 percent decline, while the total area with agriculture rose from 0.14 million hectares in 1985 to 1.68 million hectares in 2023, a 1,080 percent increase, and finally area with pasture rose from 0.31 million hectares in 1985 to 2.44 million hectares in 2023, a 691 percent increase. Our econometric analysis aims to determine how much of these changes in land use are attributable to the award of title.

[Insert Figure 3 here]

On average, 24 percent of the sample properties had a title in any given year (Table 2). The first titles in our sample were awarded in 1999 and the last in 2020 (Figure 4). The lion's share of titles—87 percent—were awarded after 2009. Titling peaked between 2014 and 2019, when more than 600 titles were awarded each year. As noted above, 73 percent of the properties in our sample were awarded title within 10 years of 2023, the last year of our panel.

[Insert Figure 4 here]

The spatiotemporal distribution of titling illustrates clustering that corresponds to government-led cadastral sweeps (Figures 1 and 5). A total of 17 percent of the properties were titled through the CAT-SAN process, 67 percent through the SAN-SIM process, and 16 percent through the SAN-TCO process (Table 2).

[Insert Figure 5 here]

On average, 56 percent of the properties had only male owners in the titling year, 70 percent featured cattle as the main economic activity, 68 percent were designated medium properties, 32 percent as enterprises, and 8 percent as legal entities (Table 2).

6 Results

In general, we find that the award of title spurred a modest but nontrivial reduction in *forest* in the 10 years after it was awarded, matched by an increase in *agriculture+pasture* of similar magnitude. In addition, we find that these effects increased monotonically during our ten-year event window, and that the effect of titling on *forest* was moderated by a variety of property characteristics.

6.1 Cumulative ten-year effects

Although for completeness, we report the OLS estimates, our discussion focuses on our preferred heterogeneity-robust estimates. For our cumulative effects models (Equation 1), the heterogeneity-robust estimates are roughly half the magnitude of the OLS estimates (Table 3; Figure 6).

[Insert Table 3 and Figure 6 here]

The heterogeneity-robust estimates indicate that titling reduced forested area by 23–29 hectares on the average sample property during a 10-year event window (Table 3 and Figure 6). These losses represent 7–8 percent of the total forest lost on the average sample properties during our 1986–2023 study period (350 hectares; Table 1) and imply that titling was responsible for a total of 167,000–208,000 additional hectares of forest loss in Bolivia during this time.

Effects on each of the land uses associated with farming—agriculture and pasture—are consistently positive but less significant and robust. The reason is that not surprisingly, each of these effects is smaller in magnitude than that for forest, while standard errors are on par with those for forest. As a result, confidence intervals for the agriculture and pasture treatment effects are more likely to include zero. In general, the treatment effect estimates for agriculture have roughly the same magnitude as those from pasture, suggesting that on average, titling spurs about as much agriculture as it does pasture.

Finally, titling has a robust positive effect on a combination of the two main farming land uses—agriculture and pasture. The magnitude of this effect is on par with that for forest. Our heterogeneity robust estimates indicate that titling increases land devoted to both agriculture and pasture by 15–23 hectares. These increases represent 5–8 percent of the growth in these land uses on the average sample property during our study period (512 hectares) and imply that titling was responsible for a total of roughly 107,000–164,000 additional hectares of agriculture and pasture.

6.2 Dynamic single-year effects

Results from our event study models (Equation 2) indicate that the absolute value of the magnitudes of the single-year effects of titling on *forest*, *agriculture* and *agriculture+pasture* grow larger over time in a more or less monotonic fashion (Figure 7 and Tables S1, S2, and S4). For *pasture*, the magnitude of the effect increases and then decreases over time (Figure 7 and Table S3). Depending on the estimator used, the peak effect occurs in years 5–9.

[Insert Figure 7 here]

6.3 Effect heterogeneity

To explore treatment effect heterogeneity, we rely on our OLS estimator because our heterogeneity robust estimators only report treatment effect estimates and not estimates of coefficients for ancillary covariates such as interaction terms (Equation 3). As noted above, the magnitude of our OLS estimates of the cumulative 10-year effects from Equation 1 generally exceed those for our preferred heterogeneity robust estimates (Table 3 and Figure 6) so the same may be true for the estimated marginal effects of moderator covariates in Equation 3 (Table 4 fourth to last row). We aim to control for this upward bias by normalizing estimated marginal effects by the OLS ATT from Equation 1 (Table 4 second to last row).

We focus on the heterogenous effect of titling on *forest* (Table 4). Heterogenous effects on agriculture and pasture are reported in the Appendix (Tables S5 and S6). In addition, we focus on the marginal effects of moderating covariates in multiple regressions, each with a single interaction term (Table 4 Models 1–9). Marginal effects from a single regression that includes all moderating covariates are similar (Table 4 Model 10).

[Insert Table 4 and Figure 8 here]

Results indicate that the average effect of titling differs across subgroups of properties defined by our moderating covariates (Table 4, Figure 8). They suggest that titling had the biggest effects on forest loss in subgroups of properties classified as enterprises, those classified as legal entities, and those located in Santa Cruz department. Among the 2,352 properties classified as enterprises, titling spurred an average of 211 hectares of forest loss per property in the 10 years after the award of title, a 315 percent increase compared to the full-sample ATT of 51 hectares of forest loss. Among the 552 properties classified as a legal entities, titling resulted in an average of 215 hectares of forest loss, a 322 percent increase. Among the 346 properties classified as both an enterprise and a legal entity, titling was associated with an average of 341 hectares of forest loss, a 570 percent increase. Among the 4,090 properties in Santa Cruz department, titling spurred an average of 165 additional hectares of forest loss, a 224 percent increase. And finally, among the 1,155 properties in Santa Cruz department classified as an enterprise, titling spurred an average of 506 additional hectares of forest loss, a 1,155 percent increase.

As for the other moderating property characteristics, among the 953 properties that received title before 2010, titling was associated with an average of 111 additional hectares of forest loss, a 118 percent increase compared to the full-sample ATT. And among the 5,106 properties that had cattle as their main economic activity when title was awarded, titling spurred an additional 61 hectares of forest loss, a 21 percent increase.

Interestingly, in one subgroup, the award of title reduced forest loss compared to the full-sample ATT. Among the 3,938 properties that included at least some pasture when title was awarded, titling resulted in just 34 hectares of forest loss per property, a 33 percent reduction compared to the full-sample ATT. Finally, among the 2,271 properties in Beni department, titling actually reduced forest loss by 126 hectares per property. Both of these heterogenous effects likely reflect the same phenomenon: in Beni, where properties feature relatively little forest land and have long been dominated by pasture, titling increases rather than decreases forest land (Table 1).

As for the effects of titling on agriculture and pasture, in each subgroup, as in the full sample, not surprisingly the combined effects of titling on these two land uses have more or less the same magnitude and opposite sign of the effect on forest (Figure 8; Tables S4 and S5). The more interesting results concern the differential effects of titling on agriculture and pasture in several subgroups. We find that titling spurs far more agriculture than pasture on properties classified as legal entities, those that are both enterprises and legal entities, those that were titled before 2010, and those in Santa Cruz. The opposite is true—titling spurs more pasture than agriculture—for properties where the main activity was cattle at the time-of-title, and those that featured at least some pasture at the time of title. Finally, in Beni department, titling spurred pasture but actually led to a reduction in agriculture.

6.4 Robustness

Here we report results of tests and sensitivity analyses that shed light on the plausibility of the key identifying assumptions discussed in Section 4. The results are consistent with the no anticipation assumption and provide mixed support for conditional parallel trends and SUTVA. In addition, a placebo test suggests our results are not an artefact of random variation

6.4.1 Conditional parallel trends

The conditional parallel trends assumption is untestable because we cannot observe outcome trends for treated properties absent treatment. Historically, a common approach to assessing the validity of this assumption has been to check whether outcome trends during pre-treatment periods for treatment and control groups are parallel. In an event study framework, that entails checking the statistical significance of coefficients on single-year lead dummy variables. For *forest*, the land use in which we're mainly interested, for two of our three preferred heterogeneity-robust estimators— Callaway and Sant'Anna (2021) and de Chaisemartin and D'Haultfoeuille (2020)—these tests do not reject the null hypothesis of parallel pre-trends (Figure 7 and Table S1).[7]

However, recent literature has demonstrated that this approach to assessing parallel trends tends to under-reject the null hypothesis (Roth et al., 2023). To address that concern, Rambachan and Roth (2023) propose a sensitivity analysis that tests the robustness of estimated treatment effects to increasingly large violations of the parallel trends assumption. More specifically, it first computes an $\bar{M}$ statistic that indicates the magnitude of the violation in the post-treatment period normalized by the largest observed violation in pre-treatment periods. It then generates a set of robust 95 percent treatment effect confidence intervals, each corresponding to a larger $\bar{M}$. Hence, a confidence interval that does not include zero for an $\bar{M}$ equal to one implies that the treatment effect is robust to violations of the parallel trends assumption in the post-treatment period as large as the largest violation observed in in the pretreatment period.

We apply the Rambachan and Roth (2023) procedure (using the HonestDID command in Stata 17) both to the simple unweighted 10-year average of single-year post-treatment effects for forest, and to individual single-year post treatment effects for forest. In each case, we rely on the Callaway and Sant'Anna (2021) estimator and allow for five pre-treatment periods. The 10-year average of single-year treatment effect estimates is statistically significant for an $\bar{M} = 0.5$ but not for $\bar{M} = 1$, implying that it is robust to violations of parallel trends assumption as large as half the size of the largest violation observed in five pre-treatment

[7]As for *agriculture* and *pasture*: In the Callaway and Sant'Anna (2021) and de Chaisemartin and D'Haultfoeuille (2020) regressions explaining agriculture, none of the single-year pre-trend coefficients are significant (Figure 7 and Table S2). However, in the de Chaisemartin and D'Haultfoeuille (2020) regression explaining pasture, coefficients are positive and significant for pre-treatment years 4 and 5 (Figure 7 and Table S3).

periods (Figure 9). However, results for individual single-year treatment effects corresponding to relative years nine and ten are more robust. They are statistically significant for $\bar{M}$ equal to one and 1.5 respectively (Figure 10).

[Insert Figure 9 and 10 here]

6.4.2 No anticipation

To elucidate whether titling has anticipatory effects, we conduct joint F-tests on the estimated coefficients for the pre-treatment leads in our event study model (Equation 2). For all three of our preferred heterogeneity robust estimators, these coefficients are not jointly significant (Table 5), suggesting that anticipatory effects are not a concern.

[Insert Table 5 here]

6.4.3 Stable unit treatment value assumption

To shed light on the plausibility of SUTVA, we estimate our cumulative effect model (Equation 3) using a subsample in which spillover should be attenuated: those properties that do not have any neighbors that received a title during the 10-year event window after their own title was awarded. Neighbors are defined as properties whose borders lie within 100 meters of each other. The 'neighborless' subsample comprises 1,035 properties that do not have any neighbors, and 118 properties that do have neighbors, but none that were awarded title during the 10 year event window (Figure S1). All told, the subsample comprises (1,035 properties $\times$ 38 years =) 39,330 property-year observations.

Treatment effects estimated with this subsample are broadly consistent with, albeit somewhat smaller than, those from the full sample (Table 6). For forest, two of the three preferred heterogeneity robust treatment effect estimates are statistically significant. They range from 16–18 hectares compared to 23–29 hectares for the full sample, a 31–36 percent difference. For agriculture, all three heterogeneity robust estimators are significant and range from 8–13 hectares compared to 8–14 hectares for the full sample, a 2-8 percent difference. For pasture, none of the estimates using the neighborless sample are significantly different, the same result we obtain using the full sample. And finally, for agriculture+pasture, estimates from two of the three heterogeneity robust estimators are significant ranging from 13-14 hectares compared to 15-22 hectares using the full sample, a 14-39 percent difference. These results suggest that spillover may bias our treatment effect estimates upwards, and that these estimates therefore may best be considered an upper bound.

[Insert Table 6 here]

6.4.4 Placebo test

Finally, to shed light on whether our estimated treatment effects are artefacts of random variation in our data rather than true causal effects, we conduct an unrestricted mixed placebo test for difference-in-differences, which entails three steps (Chen et al., 2025). First, we create 500 'fake' data sets. For each one, we designate as treated (i.e., titled) a random sample of all 277,324 property-years in our regression sample, that is, we randomly assign placebo treatments both over time ('in-time placebo') and across properties ('in-space placebo'). The total number of placebo treatments remains the same as in our real data: each sample property is treated during one year of our panel. Second, we use the Callaway and Sant'Anna (2021) procedure to estimate an average placebo treatment effect for the forest outcome for each new data set (Equation 1). Finally, we check whether our actual treatment effect estimate (Table 2) falls outside the distribution of the placebo treatment effects. Having done that, our actual treatment effect estimate, represented by a solid vertical line, falls well outside the distribution of placebo treatment effects, indicating that it is not an artefact of random variation (Figure 11).

[Insert Figure 11 here]

7 Discussion

Here, we discuss the limitations of our study, the relevance of our findings, the causal mechanisms and dynamics that underpin them, and their policy implications.

7.1 Limitations

Our analysis has at least three limitations. First, our robustness checks provide only mixed support for two identifying assumptions. Sensitivity analysis indicates that (for at least one estimator, Callaway and Sant'Anna (2021)), our main results are only robust to violations of the parallel trends assumption as large as half the size of the largest violation observed in pre-treatment years. In addition, while results generated using a neighborless sample are broadly consistent with our main results, they suggest spillover may bias our treatment effect estimates upwards. Second, even though our preferred generalized difference-in-difference estimators control for some types of unobserved heterogeneity, we are not able to rule out bias due to time-varying confounding factors. Finally, although our heterogeneity analysis provides suggestive evidence, we are not able to definitively identify mechanisms that explain our results.

7.2 Relevance

At the national-level, the forest loss caused by titling medium and enterprise properties is a significant contributor to deforestation in Bolivia, but by no means a leading one. As noted above, our estimates imply that all told, titling medium and enterprise private property in Bolivia caused 167,000–208,000 hectares of forest loss over and above the loss that would have occurred otherwise during our 1986–2023 study period. This area comprises 2–3 percent of total forest loss in Bolivia during this period attributable to all causes on all types of properties (7,403,626 hectares see Table 1) (MapBiomas Bolivia, 2025). However, in certain departments, the forest loss caused by titling medium and enterprise properties is more important. As noted in Section 6, our heterogeneity analysis indicates that in Santa Cruz department, the marginal effect of titling on forest loss is more than twice as large as the average effect for all departments.

7.3 Mechanisms

Although we are not able to definitively identify the causal mechanisms for the effect of titling on forest loss, two sets of results shed light on them. First, our estimates of the cumulative effect of titling (Equation 1) indicate that in our full sample of properties, agriculture was at least as important as pasture as a driver of forest loss. On its face, this finding seems to contradict studies that have identified cattle ranching as the leading driver of forest loss in South America (De Sy et al., 2015). But it is important to keep in mind that our findings do not suggest that pasture is not a key driver of forest loss in the Bolivian lowlands, only that it is not necessarily the most important driver of forest loss associated with land titling.

Second, our heterogeneity analysis indicates that farms classified as commercial (versus subsistence) and that are located in Santa Cruz department drive the effect of titling on forest loss. We find that this effect is more than three times larger on properties classified as enterprises than on the average property, almost four times larger on properties classified as legal entities, more than two time larger in Santa Cruz department, and more than 10 time larger on properties classified as enterprises in Santa Cruz department (Table 4; Figure 8). These effects swamp those for other subgroups. Relatedly, we find that in Santa Cruz, titling spurred far more agriculture than pasture.

Taken together, these two sets of results suggest that at least to some extent, the mechanism for the effect of titling on forest loss entails the expansion agriculture on large commercial farms in Santa Cruz. Why would titling have a particularly strong effect on the expansion of agriculture (versus pasture) on properties hosting large commercial farms?

The theoretical and empirical literature summarized in Section 3 offers two potential explanations. One has to do with the so-called investment effect of land titling—the hypothesis that by reducing the risk that land will be expropriated or grabbed, land titling boosts the returns land managers expect to obtain from cleared land uses relative to forest. This effect could boost the expected returns to agriculture by more than the expected returns to pasture if investments in agriculture were less liquid than those in pasture and therefore harder to recoup in the event of expropriation or land grabbing. That would certainly seem to be the case for investments in commercial agriculture such as irrigation, silos, and other fixed structures versus at least one key investment in pasture—livestock—which can generally be sold and/or moved.

A second possible reason that titling has a particularly strong effect on agriculture has to do with credit. As noted in Section 3, the award of title enables managers of medium and enterprise properties (but not small properties) to use their land as collateral, which in turn should improve their access to formal credit. Improved access to credit would spur more investment in commercial agriculture than pasture if agriculture entailed lumpier investments (e.g., irrigation, silos) than investments in pasture (e.g., livestock) and/or earned higher returns.

7.4 Dynamic effects

As noted above, results from our event study models (Equation 2) indicate that the single-year effects of titling on forest and agriculture grow larger over time (Figure 7 and Tables S1 and S2). The implication is that the adverse effects titling on forests that we have described here may continue to grow during at least some of the years after the end of our 10-year event window.

At least two factors may explain why our treatment effects increase in magnitude over time. First, the hypothesized mechanisms for the effect of titling on forest loss discussed in Section 2—the investment effect and the credit effect—likely operate with a lag. In the case of the investment effect, it may take years for managers to adjust their expectations about the returns to competing land uses. In the case of the credit effect, it may take years for managers to acquire credit and then use it to buy or rent the inputs needed for land use change. Given these lags, one would not expect titling to have large effects on forest loss immediately after title is awarded.

Second, the effect of titling on forest loss may be self-reinforcing. Land managers invest in land use change because they expect agriculture and pasture to generate a positive net return. That may take several years. But if and when these investment generate profits, they can be used to finance additional land use change. In other words, land use change in one period may facilitate additional land use change in later periods.

7.5 Policy implications

Our finding that titling spurs significant forest loss on medium and enterprise properties in Bolivia does not imply that titling these properties has been ill-advised. Titling can have a variety of important socioeconomic benefits including on agricultural productivity, income, and women's empowerment (Tseng et al., 2021). These benefits would need to be measured and then taken into account to assess the overall net benefits of titling. Also, even leaving aside the issue of such pecuniary benefits, a normative argument could be made for titling as a basic right.

Having said that, the principal policy implication of our results is that there is scope for the stakeholders involved in rural land titling, including governments, land managers, and civil society, to reduce forest loss associated with titling medium and enterprise properties. Notwithstanding the rapid forest loss on such properties over the past three decades, considerable forest remains: 32 percent of the average sample property was forested in 2023, the last year of our study period, and collectively this forestland comprised almost 4.5 million hectares. Forest loss on medium and enterprise properties could be stemmed by for example, strengthening monitoring and enforcement of land use change restrictions, providing incentives for forest conservation via PES programs, and mitigating perverse incentives generated by subsidies to cleared land uses and regulations that encourage land managers to clear forest to demonstrate they are using land productively. Our finding that the effect of titling on forest loss is particularly strong on properties hosting large farms engaged in commercial agriculture and on those in Santa Cruz department, can help policymakers target and tailor such policies to enhance their cost effectiveness.

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Tables

Table 1: Total area, forest, and medium and enterprise properties, by department

	Beni	Santa Cruz	Tarija	Chuquisaca	Others	Total
Area						
Total area (ha)	21,904,222	41,098,672	4,021,262	5,496,368	44,719,366	117,239,889
Forest						
Area 1985 (ha)	9,985,480	33,022,098	2,895,600	2,958,513	16,327,579	65,189,270
Loss 1985–2023 (ha)	517,579	5,823,758	221,746	114,686	725,857	7,403,626
Loss 1985–2023 (%)	5.18	17.64	7.66	3.88	4.45	11.36
Medium and enterprise properties						
Number	2,260	4,106	334	282	316	7,298
Total area / total area dept. or nation (%)	29.73	15.86	10.63	7.42	0.70	12.09
Avg. forest 1985 (%)	15.23	76.78	95.62	95.29	65.99	49.37
Total area forest / total area forest in dept. or nation	9.93	15.16	14.12	13.13	1.27	10.74
Avg. forest loss per property 1985–2023 (ha)	29.67	573.23	208.90	83.47	128.92	350.07
Avg. forest loss per property 1985–2023 (%)	6.76	47.03	17.07	6.06	19.68	36.49
Contribution to total forest loss 1985–2023 (%)	12.96	40.42	31.47	20.52	5.61	34.51

Table 2: Variables

Variable	Description	Units	Source	Scale	Years	Mean[a]	SD[a]
OUTCOME							
forest	land use = forest	ha	MapBiomas Bolivia (2025)	30m	1999–2020	809.843	1,393.041
agriculture	land use = agriculture	ha	MapBiomas Bolivia (2025)	30m	1999–2020	115.983	428.017
pasture	land use = pasture	ha	MapBiomas Bolivia (2025)	30m	1999–2020	112.643	313.308
TREATMENT							
title cumulative	formal legal title in year t	0/1	INRA (2025)	2m	1999–2020	0.238	0.426
title year j	formal legal title awarded in t+j	0/1	INRA (2025)	2m	1999–2020	NA	NA
title year k	formal legal title awarded in t+k	0/1	INRA (2025)	2m	1999–2020	NA	NA
CONTROL							
rainfall	annual rainfall	m	C3S (2017)	1km	1985–2023	0.127	0.038
temperature	average annual temperature	°C	C3S (2017)	1km	1985–2023	24.013	2.552
MODERATORS							
medium	official designation	0/1	INRA (2025)	2m	1999–2020	0.678	0.467
enterprise	official designation	0/1	INRA (2025)	2m	1999–2020	0.322	0.467
cattle	cattle ranching main activity	0/1	INRA (2025)	2m	1999–2020	0.700	0.458
pasture 1985	has pasture in 1985 > 0	0/1	MapBiomas Bolivia (2025)	30m	1985–2023	0.540	0.498
male owners	owners all male	0/1	INRA (2025)	2m	1999–2020	0.557	0.497
legal entity	owners include legal entity	0/1	INRA (2025)	2m	1999–2020	0.076	0.264
early title	title awarded before 2009	0/1	INRA (2025)	2m	1999–2020	0.131	0.337
OTHER							
size	geographic extent	ha	INRA (2025)	2m	1999–2020	1,788.314	2,088.332
CAT-SAN	cadastral sweep process	0/1	INRA (2025)	2m	1999–2020	0.171	0.376
SAN-SIM	ad hoc process	0/1	INRA (2025)	2m	1999–2020	0.672	0.470
SAN-TCO	private vs communal lands process	0/1	INRA (2025)	2m	1999–2020	0.158	0.364

[a] n = (7,298 properties × 38 years =) 277,324 property-year observations.

Table 3: Cumulative 10-year effects of titling on land uses; two-way fixed effects regressions; dependent variable is the number of hectares of a specified land use in year t = 1986–2023; treatment is award of formal title (s.e.)

	OLS	C&S	S&A	D&D
FOREST				
title	-50.92***	-28.56***	-26.57***	-22.85***
	(8.12)	(8.47)	(5.67)	(5.16)
Percent avg. Δ	14.54	8.16	7.59	-6.53
Total Δ	-371,614.16	-208,430.88	-193,907.86	-166,759.30
AGRICULTURE				
title	30.38***	10.00	14.36**	7.84*
	(6.64)	(5.25)	(4.58)	(3.76)
Percent avg. Δ	14.17	4.66	6.70	3.66
Total Δ	221,713.24	72,980.00	104,799.28	57,223.62
PASTURE				
title	8.586*	12.47	1.514	6.76
	(3.75)	(6.46)	(3.079)	(3.79)
Percent avg. Δ	2.89	4.20	0.51	2.27
Total Δ	62,660.63	91,006.06	11,049.17	49,349.48
AGRICULTURE + PASTURE				
title	38.97***	22.47**	15.87**	14.60**
	(8.20)	(8.34)	(5.74)	(5.17)
Percent avg. Δ	7.62	4.39	3.10	2.85
Total Δ	284,403.06	163,986.06	115,819.26	106,550.80

All models include: n = (7,298 properties × 38 years =) 277,324 property-year observations; two time-varying control variables—rainfall and temperature; both property and year fixed effects; and standard errors that are clustered at the property-level. C&S = Callaway and Sant'Anna (2021); D&D = de Chaisemartin and D'Haultfoeuille (2020); OLS = ordinary least squares; S&J = Sun and Abraham (2021). Average Δ 1986–2023: forest = -350.17 ha; agriculture = 214.41 ha; pasture = 297.19 ha; agriculture + pasture = 511.6. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Treatment effect heterogeneity; two-way fixed effects OLS regressions; dependent variable is the number of hectares of forest in year t = 1986–2023; treatment is award of formal title (s.e.)

	(1) *cattle*	(2) *pasture*	(3) *legal entity*	(4) *enterprise*	(5) *enterprise×legal entity*	(6) *early title*	(7) *beni*	(8) *santa cruz*	(9) *enterprise×santa cruz*	(10) *all interactions*
title	-29.19***	-68.98***	-35.22***	17.57**	-35.65***	-13.11**	-106.4***	88.80***	39.58***	201.7***
	(10.60)	(9.826)	(8.301)	(7.182)	(8.109)	(6.211)	(8.799)	(7.212)	(6.834)	(14.31)
title×cattle	-32.20***									-175.4***
	(10.38)									(12.47)
title×pasture		34.87***								40.03***
		(10.42)								(9.670)
title×legal entity			-179.7***							-74.94***
			(28.62)							(20.08)
title×enterprise				-229.0***						-10.77
				(14.96)						(8.540)
title×enterprise×legal entity					-305.6***					40.47
					(42.37)					(46.72)
title×early title						-98.01***				-69.00***
						(20.25)				(17.85)
title×beni							232.3***			92.17***
							(7.198)			(11.85)
title×santa cruz								-253.8***		-117.1***
								(9.004)		(9.720)
title×enterprise×santa cruz									-545.2***	-463.3***
									(25.69)	(27.40)
Marginal effect (ha)	-61.39***	-34.10***	-214.96***	-211.47***	-341.22***	-111.12***	125.96***	-165.03***	-505.60***	
	(8.89)	(9.49)	(27.91)	(15.51)	(41.83)	(18.97)	(8.17)	(10.38)	(25.47)	
Marginal effect (% ATT)[a]	20.57	-33.02	322.17	315.31	570.14	118.23	-347.37	224.11	892.96	
Properties in subgroup (no.)[b]	5106	3938	552	2352	346	953	2271	4090	1155	7298

[a]Marginal effect as a percentage of OLS average treatment effect on treated (-50.92 ha).
[b]Number of properties for which dummy variable = 1, e.g., cattle = 1.
All models include: n = (7,298 properties × 38 years =) 277,324 property-year observations; two time-varying control variables—rainfall and temperature; both property and year fixed effects; and standard errors that are clustered at the property-level. OLS = ordinary least squares.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 5: No anticipation assumption: joint F-test for statistical significance of single-year lead dummy variables (title year_{-5} − title year_{-1}); two-way fixed effects regressions; dependent variable is the number of hectares of forest in year t = 1986–2023; treatment is award of formal title (s.e.)

Estimator	F-Statistic	P-value
C&S	8.18	0.147
D&D	5.39	0.370
OLS	2.94	0.012
S&A	5.25	0.385

All models include: n = (7,298 properties × 38 years =) 277,324 property-year observations; two time-varying control variables—rainfall and temperature; both property and year fixed effects; and standard errors that are clustered at the property-level. C&S = Callaway and Sant'Anna (2021); D&D = de Chaisemartin and D'Haultfoeuille (2020); OLS = ordinary least squares; S&J = Sun and Abraham (2021).

Table 6: Treatment effect estimates using a 'neighborless' subsample: Cumulative 10-year effects of titling on land uses; two-way fixed effects regressions; dependent variable is the number of hectares of a specified land use in year t = 1986–2023; treatment is award of formal title (s.e.)

	OLS	C&S	S&A	D&D
FOREST				
title	-14.52	-18.18**	-9.235	-15.76**
	(11.46)	(8.845)	(7.267)	(6.878)
AGRICULTURE				
title	10.17**	13.18***	7.982***	9.717***
	(4.362)	(5.072)	(2.887)	(3.717)
PASTURE				
title	-3.559	0.533	-2.385	2.855
	(7.670)	(4.972)	(5.110)	(3.528)
AGRICULTURE + PASTURE				
title	6.613	13.71*	5.597	12.57**
	(9.521)	(8.288)	(6.255)	(5.991)

All models include 1,035 properties that do not have any neighbors that received a title during the 10-year event window after their own title was awarded and n = (1,035 properties $\times$ 38 years =) 39,330 property-year observations; two time-varying control variables—rainfall and temperature; both property and year fixed effects; and standard errors that are clustered at the property-level. C&S = Callaway and Sant'Anna (2021); D&D = de Chaisemartin and D'Haultfoeuille (2020); OLS = ordinary least squares; S&J = Sun and Abraham (2021). *** p<0.01, ** p<0.05, * p<0.1

Figures

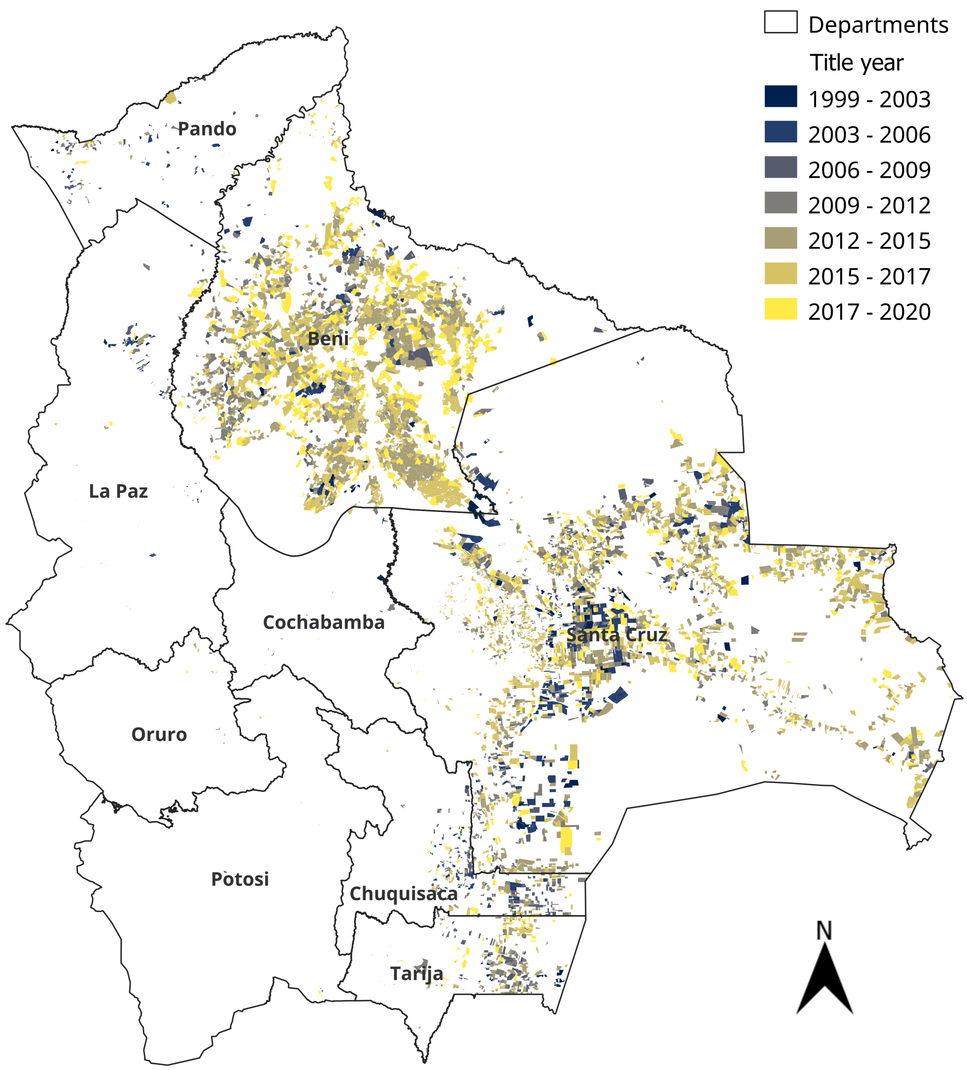

Figure 1: Sample properties, by year title awarded

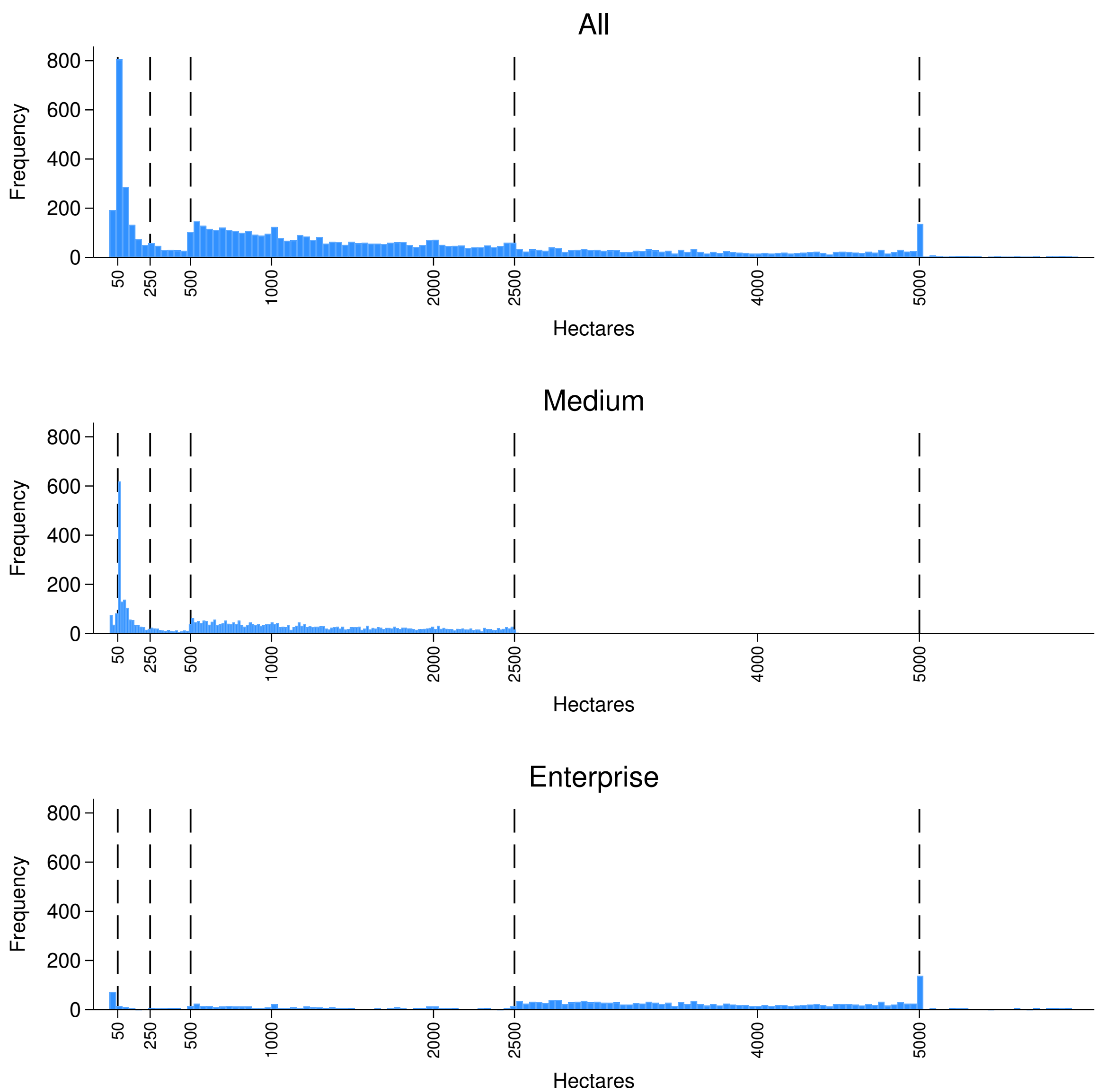

Figure 2: Sample properties, by category and size

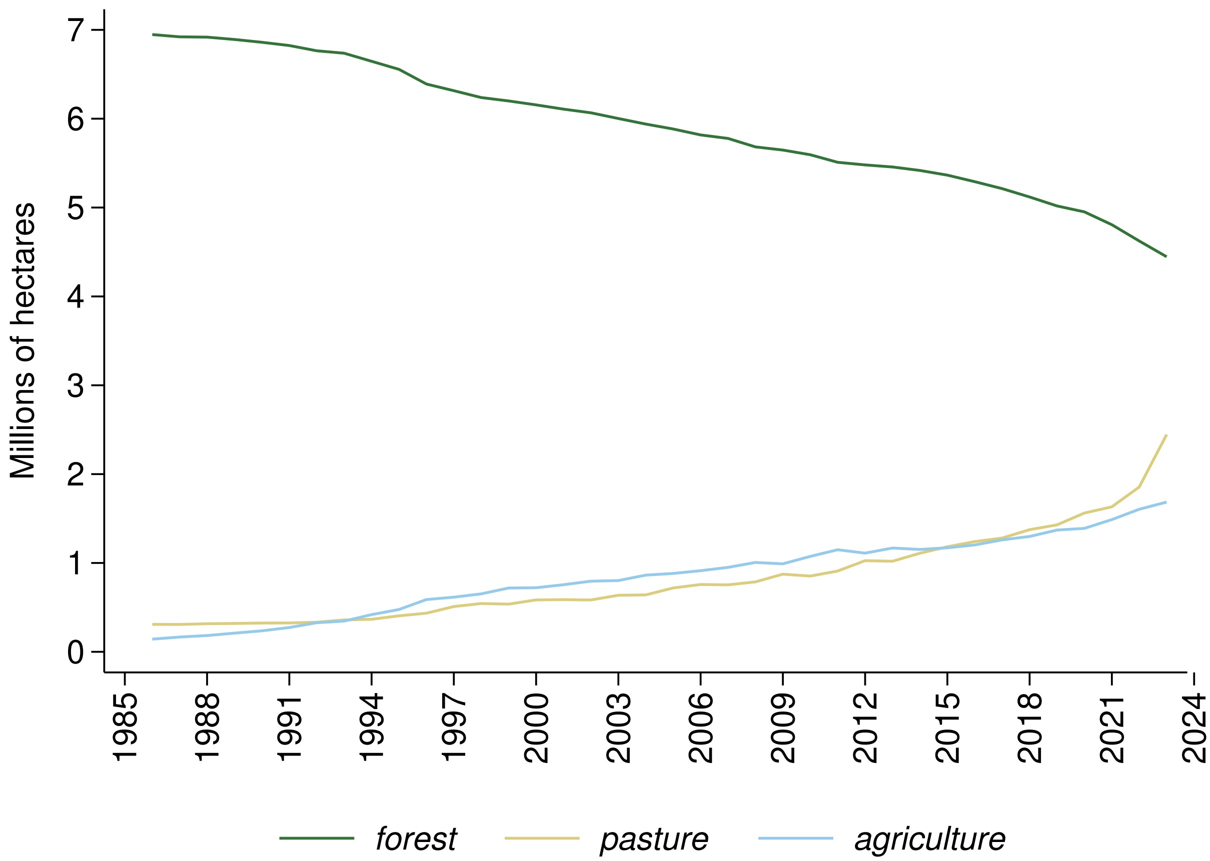

Figure 3: Land uses in sample properties

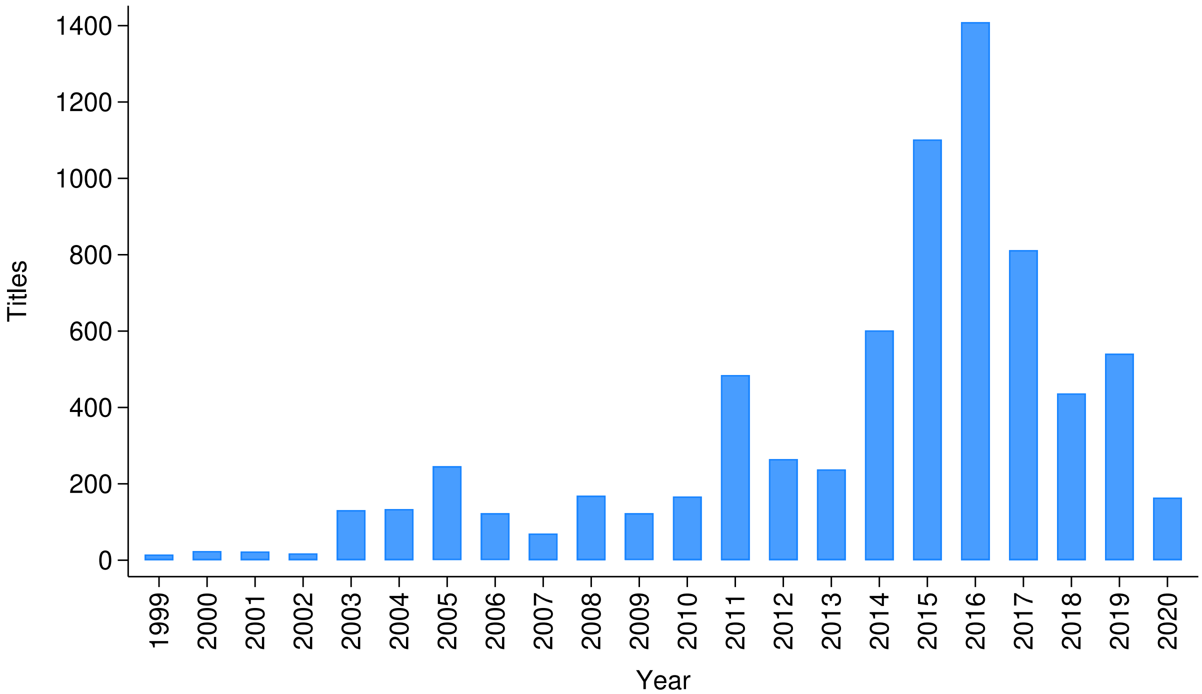

Figure 4: Titles awarded by year

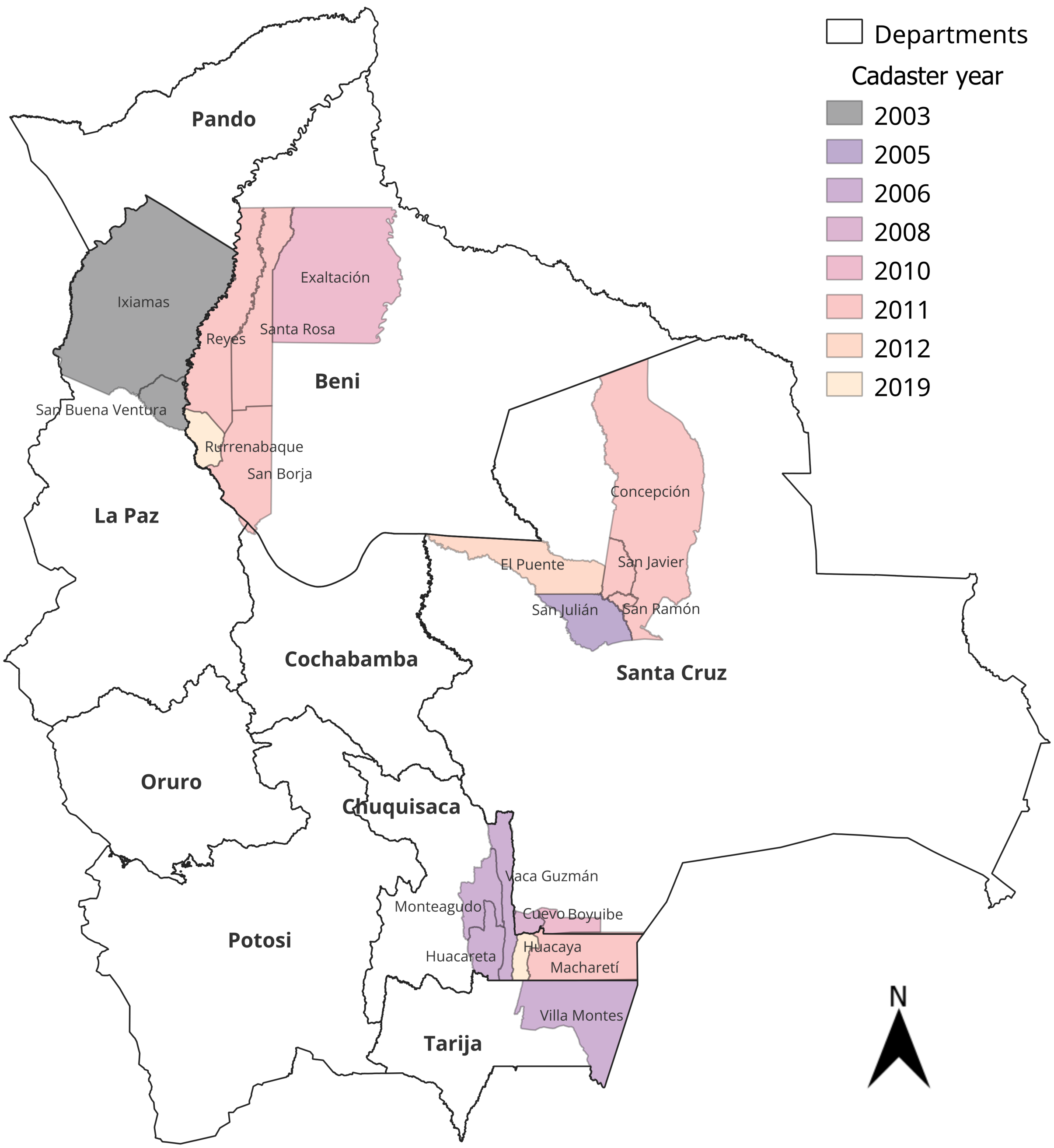

Figure 5: Cadastral sweeps

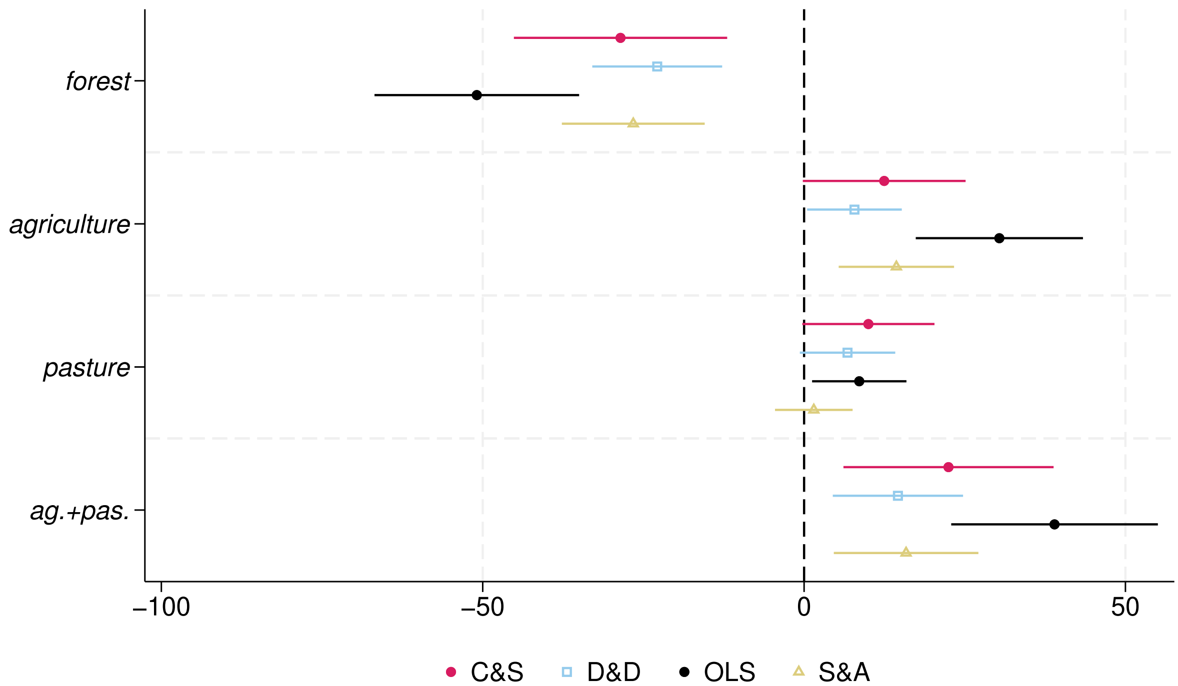

Figure 6: Cumulative 10-year effect of *title* on land uses

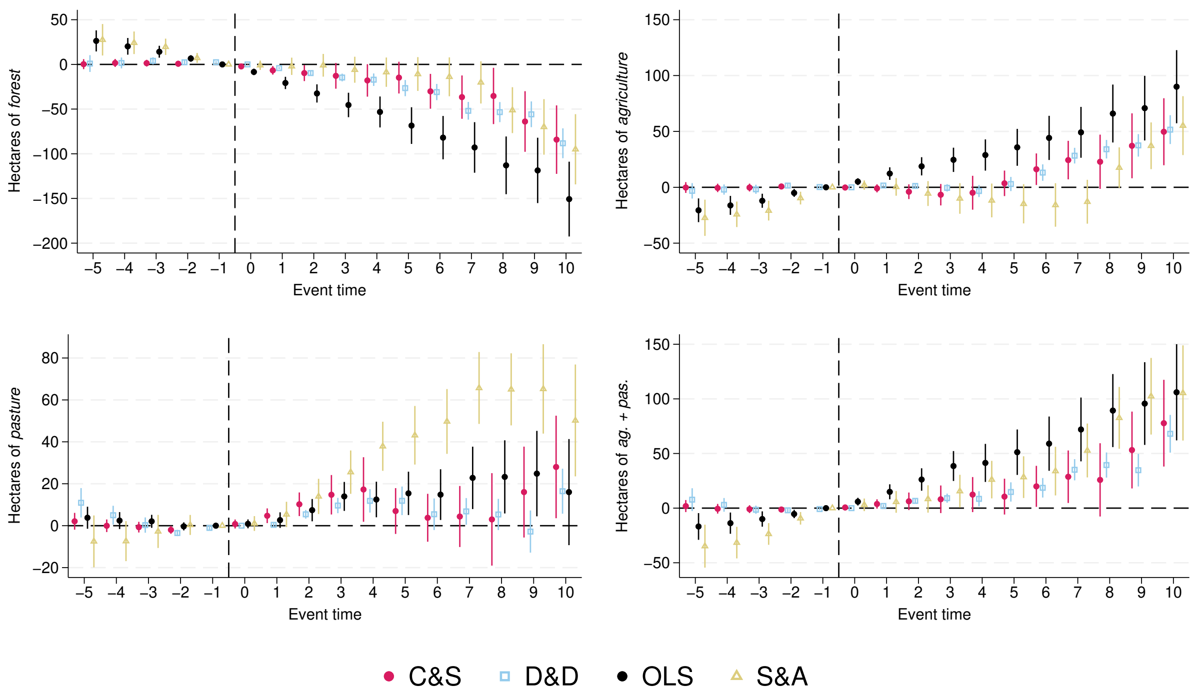

Figure 7: Dynamic effects of titling on land uses: Two-way fixed effects regressions; dependent variable is the number of hectares of forest, agriculture and pasture in year t = 1986–2023; treatment is award of formal title

Notes: All models include: n = (7,298 properties × 38 years =) 277,324 property-year observations; two time-varying control variables—*rainfall* and *temperature*; both property and year fixed effects; and standard errors that are clustered at the property-level. Symbol is point estimate and whiskers are 95 percent confidence intervals. C&S = Callaway and Sant'Anna (2021); D&D = de Chaisemartin and D'Haultfoeuille (2020); OLS = ordinary least squares; S&A = Sun and Abraham (2021).

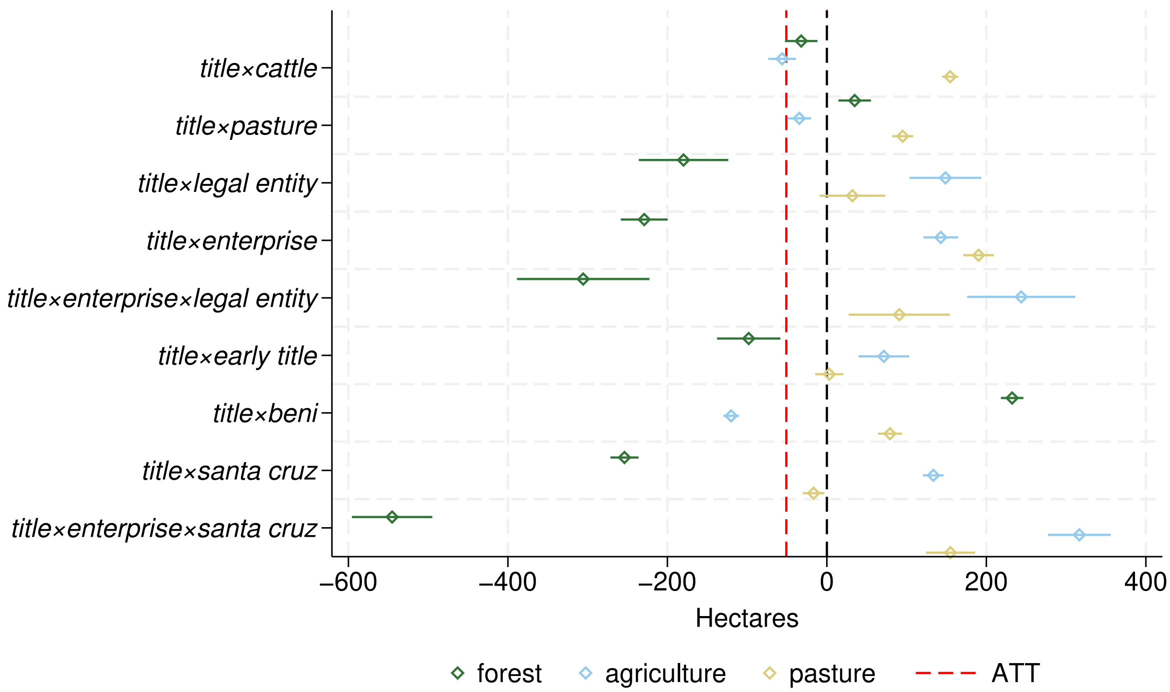

Figure 8: Treatment effect heterogeneity; marginal effects from two-way fixed effects OLS regressions; dependent variable is the number of hectares of forest, agriculture, or pasture in year t = 1986–2023; treatment is award of formal title

Notes: All models include n = (7,298 properties × 38 years =) 277,324 property-year observations; two time-varying control variables—*rainfall* and *temperature*; both property and year fixed effects; and standard errors that are clustered at the property-level. Symbol is point estimate and whiskers are 95 percent confidence intervals. ATT = average treatment effect on treated for forest; OLS = ordinary least squares.

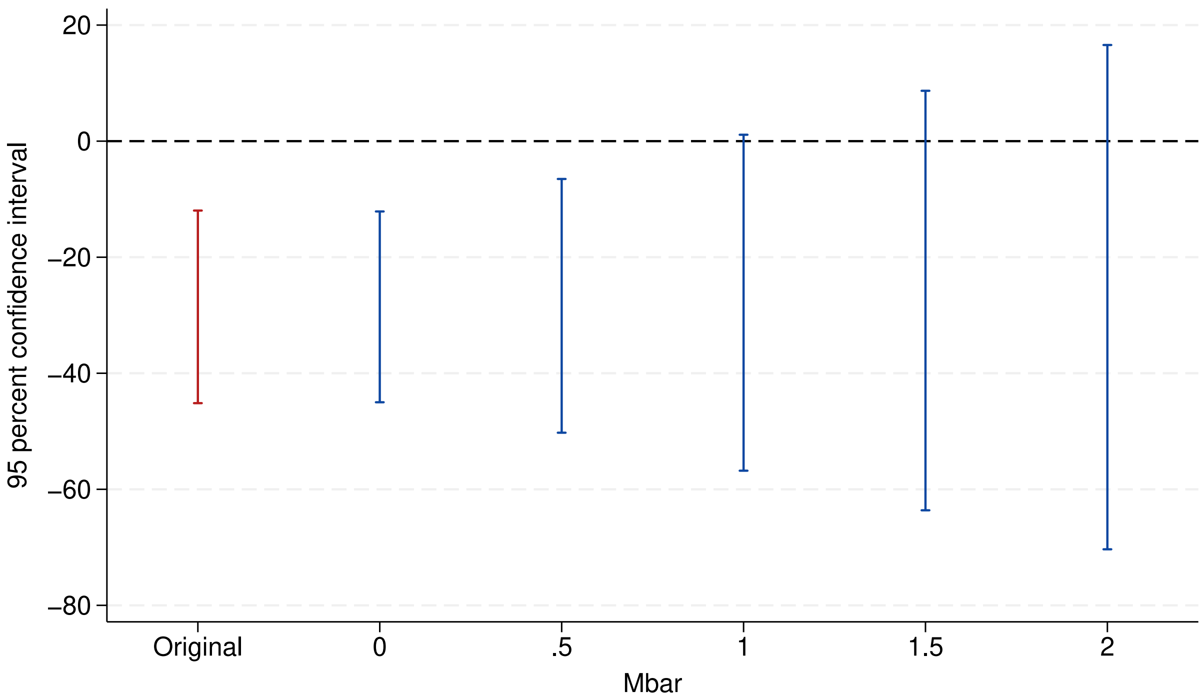

Figure 9: Sensitivity analysis: Robustness of estimated treatment effects to violations of parallel trends assumption (Rambachan and Roth, 2023).

Notes: The treatment effect is an unweighted 10-year average of single-year treatment effects generated using Callaway and Sant'Anna (2021) estimators. $\bar{M}$ indicates the magnitude of the violation of parallel trends in the post-treatment period normalized by the largest violation observed five pre-treatment periods. The red bar is the original conventional 95 percent confidence interval and blue bars are robust confidence intervals for each $\bar{M}$.

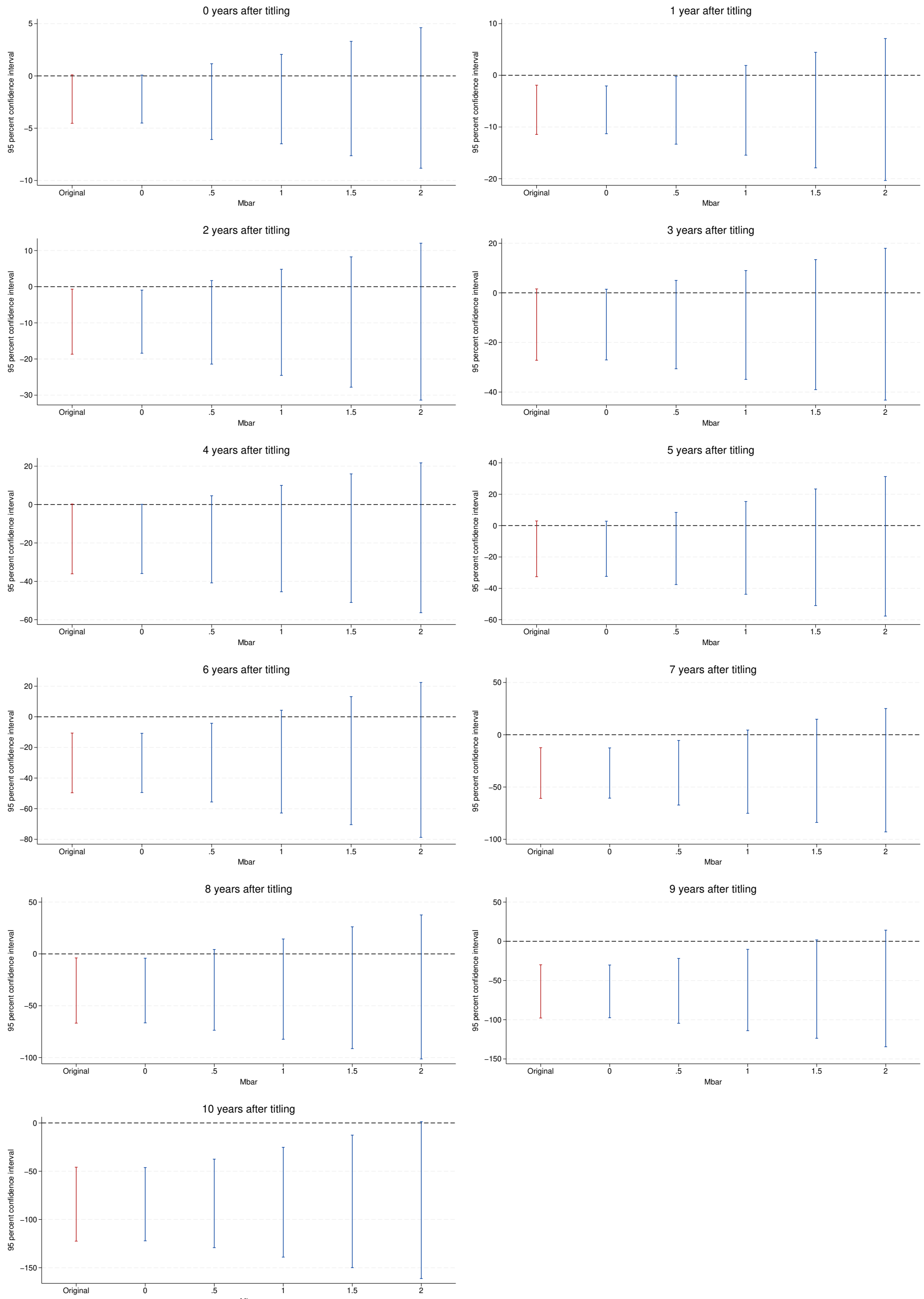

Figure 10: Sensitivity analysis: robustness of estimated single-year treatment effects to violations of parallel trends assumption (Rambachan and Roth, 2023).

Notes: Treatment effects are generated using Callaway and Sant'Anna (2021) estimators. $\bar{M}$ indicates the magnitude of the violation of parallel trends in the post-treatment period normalized by the largest violation observed five pre-treatment periods. The red bar is the original conventional 95 percent confidence interval and blue bars are robust confidence intervals for each $\bar{M}$.

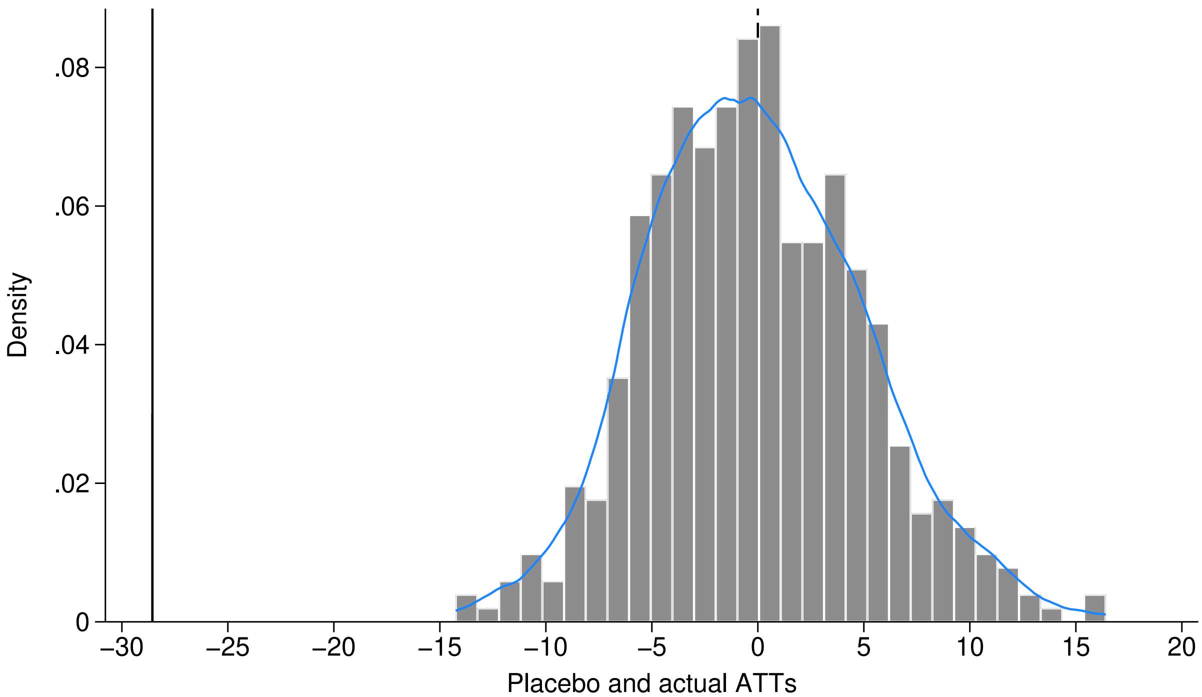

Figure 11: Unrestricted mixed placebo test for staggered difference-in-differences using Callaway and Sant'Anna (2021) estimator.

Notes: The histogram comprises cumulative average treatment effect on treated (ATTs) for the forest outcome for 500 'fake' data sets in which placebo treatments are randomly assigned both over time and across properties. The solid blue line is the kernel density estimate and the solid black line is the estimated ATT using real data.

Appendix

Table S1: Dynamic effects of titling on *forest*; two-way fixed effects regressions; dependent variable is the number of hectares of forest in year t = 1986–2023; treatment is award of formal title (s.e.)

	(1) C&S	(2) D&D	(3) OLS	(4) S&A
title year -5	0.278	0.953	26.31***	27.56**
	(2.852)	(9.384)	(6.014)	(8.971)
title year -4	1.627	1.634	20.20***	24.21***
	(2.283)	(6.072)	(4.764)	(6.444)
title year -3	1.382	3.958	14.16***	19.68***
	(1.907)	(4.007)	(3.395)	(4.665)
title year -2	0.755	2.416	6.649***	7.220*
	(1.131)	(2.426)	(1.872)	(2.836)
title year 0	-2.215	0	-8.398***	-0.947
	(1.181)	(.)	(1.840)	(2.675)
title year 1	-6.674**	-4.156***	-20.77***	-2.148
	(2.430)	(1.162)	(3.505)	(4.927)
title year 2	-9.675*	-9.688***	-32.47***	-1.011
	(4.588)	(2.359)	(5.191)	(6.489)
title year 3	-12.80	-14.65***	-45.39***	-5.961
	(7.339)	(4.313)	(6.970)	(7.419)
title year 4	-17.92	-17.11*	-53.22***	-8.693
	(9.275)	(6.966)	(8.826)	(8.356)
title year 5	-14.78	-26.52**	-68.46***	-10.77
	(9.061)	(9.043)	(10.47)	(9.643)
title year 6	-30.11**	-30.94***	-81.96***	-13.87
	(9.942)	(9.063)	(12.37)	(11.10)
title year 7	-36.61**	-51.92***	-92.91***	-20.16
	(12.37)	(9.906)	(14.43)	(12.05)
title year 8	-35.32*	-53.37***	-112.9***	-51.18***
	(16.05)	(11.22)	(16.47)	(13.09)
title year 9	-63.88***	-55.83***	-118.6***	-69.94***
	(17.31)	(14.43)	(18.66)	(15.85)
title year 10	-84.15***	-88.24***	-150.7***	-94.98***
	(19.54)	(16.72)	(21.34)	(20.04)
No. properties	7298	7298	7298	7298
No. property-years	277324	277324	277324	277324
R-squared			0.96	

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

All models include: n = (7,298 properties × 38 years =) 277,324 property-year observations; two time-varying control variables—rainfall and temperature; both property and year fixed effects; and standard errors that are clustered at the property-level. C&S = Callaway and Sant'Anna (2021); D&D = de Chaisemartin and D'Haultfoeuille (2023); OLS = ordinary least squares; S&J = Sun and Abraham (2021).

Table S2: Dynamic effects of titling on *agriculture*; two-way fixed effects regressions; dependent variable is the number of hectares of agriculture in year t = 1986–2023; treatment is award of formal title (s.e.)

	(1) C&S	(2) D&D	(3) OLS	(4) S&A
title year -5	-0.226	-3.190	-20.57***	-27.33***
	(2.460)	(6.920)	(5.423)	(8.264)
title year -4	-0.546	-2.044	-16.21***	-24.20***
	(1.913)	(4.565)	(4.327)	(5.824)
title year -3	-0.239	-1.765	-12.06***	-20.91***
	(1.782)	(4.066)	(3.198)	(4.515)
title year -2	0.718	1.519	-5.039**	-9.679***
	(1.166)	(1.928)	(1.795)	(2.898)
title year 0	-0.192	0	5.044**	1.983
	(1.013)	(.)	(1.561)	(2.125)
title year 1	-0.956	1.558	12.28***	0.344
	(1.844)	(1.061)	(2.865)	(4.021)
title year 2	-3.968	1.232	18.77***	-5.594
	(3.363)	(1.902)	(4.160)	(5.676)
title year 3	-6.646	-0.498	24.58***	-10.01
	(4.900)	(3.271)	(5.528)	(6.983)
title year 4	-4.873	-3.320	28.90***	-11.79
	(7.731)	(5.410)	(7.130)	(7.723)
title year 5	3.547	2.926	35.76***	-14.78
	(5.819)	(6.339)	(8.424)	(8.946)
title year 6	16.16*	13.16	44.19***	-15.83
	(7.211)	(7.391)	(10.08)	(9.932)
title year 7	24.35**	28.30***	49.20***	-12.94
	(8.794)	(7.015)	(11.61)	(10.01)
title year 8	22.85	34.08***	66.02***	17.59
	(12.37)	(8.286)	(13.22)	(9.252)
title year 9	37.14*	37.53***	70.85***	37.16***
	(14.83)	(10.05)	(14.77)	(10.67)
title year 10	49.72**	51.60***	90.03***	55.20***
	(15.29)	(12.91)	(16.70)	(13.44)
No. properties	7298	7298	7298	7298
No. property-years	277324	277324	277324	277324
R-squared			0.71	

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

All models include: n = (7,298 properties × 38 years =) 277,324 property-year observations; two time-varying control variables—rainfall and temperature; both property and year fixed effects; and standard errors that are clustered at the property-level. C&S = Callaway and Sant'Anna (2021); D&D = de Chaisemartin and D'Haultfoeuille (2023); OLS = ordinary least squares; S&J = Sun and Abraham (2021).

Table S3: Dynamic effects of titling on *pasture*; two-way fixed effects regressions; dependent variable is the number of hectares of pasture in year t = 1986–2023; treatment is award of formal title (s.e.)

	(1) C&S	(2) D&D	(3) OLS	(4) S&A
title year -5	2.109	10.94	3.798	-7.480
	(2.041)	(7.011)	(2.682)	(6.312)
title year -4	-0.0317	5.012	2.442	-7.374
	(1.531)	(4.482)	(2.067)	(4.864)
title year -3	-0.697	0.293	2.089	-2.736
	(1.292)	(3.638)	(1.581)	(4.022)
title year -2	-1.983*	-3.491*	-0.317	0.357
	(0.963)	(1.494)	(0.980)	(2.431)
title year 0	0.808	0	0.881	1.041
	(1.096)	(.)	(1.076)	(1.801)
title year 1	4.748**	0.410	2.666	5.378
	(1.785)	(1.264)	(1.896)	(3.082)
title year 2	10.23***	5.434**	7.419**	13.96**
	(2.872)	(2.054)	(2.712)	(4.284)
title year 3	14.76**	9.625**	13.97***	25.55***
	(4.798)	(3.655)	(3.522)	(5.272)
title year 4	17.27*	11.85*	12.50**	37.89***
	(7.853)	(5.585)	(4.330)	(5.977)
title year 5	6.997	11.87	15.43**	43.14***
	(5.577)	(6.812)	(5.274)	(7.130)
title year 6	3.777	5.464	14.82*	49.73***
	(5.826)	(7.529)	(6.169)	(7.880)
title year 7	4.377	6.821	22.82**	65.70***
	(7.428)	(6.350)	(7.596)	(8.742)
title year 8	3.007	5.359	23.28**	65.04***
	(11.25)	(7.438)	(8.902)	(8.771)
title year 9	16.05	-2.806	24.85*	65.26***
	(11.06)	(10.05)	(10.40)	(10.88)
title year 10	28.03*	16.46	16.02	50.21***
	(12.48)	(10.73)	(12.91)	(13.61)
No. properties	7298	7298	7298	7298
No. property-years	277324	277324	277324	277324
R-squared			0.65	

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

All models include: n = (7,298 properties × 38 years =) 277,324 property-year observations; two time-varying control variables—rainfall and temperature; both property and year fixed effects; and standard errors that are clustered at the property-level. C&S = Callaway and Sant'Anna (2021); D&D = de Chaisemartin and D'Haultfoeuille (2023); OLS = ordinary least squares; S&J = Sun and Abraham (2021).

Table S4: Dynamic effects of titling on *agriculture+pasture*; two-way fixed effects regressions; dependent variable is the number of hectares of agriculture and pasture in year t = 1986–2023; treatment is award of formal title (s.e.)

	(1) C&S	(2) D&D	(3) OLS	(4) S&A
title year -5	1.884	7.745	-16.78**	-34.81***
	(2.803)	(10.46)	(6.231)	(10.02)
title year -4	-0.578	2.968	-13.76**	-31.57***
	(2.261)	(6.261)	(4.957)	(7.365)
title year -3	-0.937	-1.473	-9.970**	-23.64***
	(1.841)	(3.740)	(3.573)	(5.061)
title year -2	-1.265	-1.972	-5.355**	-9.322**
	(1.066)	(2.189)	(1.940)	(2.921)
title year 0	0.616	0	5.926**	3.024
	(1.066)	(.)	(1.801)	(2.935)
title year 1	3.792	1.968	14.95***	5.722
	(2.176)	(1.270)	(3.501)	(5.078)
title year 2	6.260	6.666**	26.19***	8.368
	(4.091)	(2.413)	(5.232)	(6.388)
title year 3	8.114	9.127*	38.55***	15.53*
	(6.444)	(4.162)	(6.981)	(7.643)
title year 4	12.40	8.528	41.40***	26.10**
	(8.177)	(6.950)	(8.882)	(8.756)
title year 5	10.54	14.79	51.19***	28.37**
	(8.356)	(8.812)	(10.62)	(9.738)
title year 6	19.94*	18.62*	59.01***	33.90**
	(9.572)	(8.911)	(12.66)	(11.36)
title year 7	28.73*	35.12***	72.02***	52.76***
	(12.26)	(9.681)	(14.89)	(12.57)
title year 8	25.86	39.44***	89.30***	82.63***
	(17.11)	(11.52)	(17.05)	(14.42)
title year 9	53.19**	34.73*	95.70***	102.4***
	(17.95)	(14.90)	(19.31)	(17.87)
title year 10	77.75***	68.06***	106.1***	105.4***
	(20.25)	(17.27)	(22.44)	(22.22)
No. properties	7298	7298	7298	7298
No. property-years	277324	277324	277324	277324
R-squared			0.69	

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

All models include: n = (7,298 properties × 38 years =) 277,324 property-year observations; two time-varying control variables—rainfall and temperature; both property and year fixed effects; and standard errors that are clustered at the property-level. C&S = Callaway and Sant'Anna (2021); D&D = de Chaisemartin and D'Haultfoeuille (2023); OLS = ordinary least squares; S&J = Sun and Abraham (2021).

Table S5: Treatment effect heterogeneity; two-way fixed effects OLS regressions; dependent variable is the number of hectares of agriculture in year t = 1986–2023; treatment is award of formal title (s.e.)

	(1) *cattle*	(2) *pasture*	(3) *legal entity*	(4) *enterprise*	(5) *enterprise×legal entity*	(6) *early title*	(7) *beni*	(8) *santa cruz*	(9) *enterprise×santa cruz*	(10) *cattle*
title	68.98***	48.29***	17.40***	-12.26**	18.21***	2.841	59.07***	-43.24***	-22.10***	-52.37***
	(9.379)	(7.588)	(6.693)	(5.706)	(6.557)	(5.129)	(7.120)	(5.733)	(5.548)	(11.39)
title×cattle	-56.10***									6.817
	(8.868)									(10.60)
title×pasture		-34.60***								-27.63***
		(7.487)								(7.547)
title×legal entity			148.7***							26.68*
			(22.93)							(15.38)
title×enterprise				142.8***						10.15*
				(11.06)						(5.950)
title×enterprise×legal entity					243.6***					29.01
					(34.58)					(38.82)
title×early title						71.42***				51.99***
						(16.21)				(15.18)
title×beni							-120.0***			-16.17*
							(4.958)			(8.820)
title×santa cruz								133.5***		45.96***
								(6.596)		(7.516)
title×enterprise×santa cruz									316.6***	267.9***
									(20.00)	(21.30)
Marginal effect (ha)	12.88*	13.69*	166.05***	130.53***	261.85***	74.26***	-60.97***	90.30***	294.45***	
	(6.95)	(7.66)	(22.62)	(12.23)	(34.36)	(15.26)	(6.37)	(8.36)	(20.38)	
Marginal effect (% ATT)[a]	-57.61	-54.93	446.55	329.62	761.85	144.43	-300.67	197.22	869.17	
Properties in subgroup (no.)[b]	5106	3938	552	2352	346	953	2271	4090	1155	7298

[a]Marginal effect as a percentage of OLS average treatment effect on treated (30.38 ha).
[b]Number of properties for which dummy variable = 1, e.g., cattle = 1.
All models include: n = (7,298 properties × 38 years =) 277,324 property-year observations; two time-varying control variables—rainfall and temperature; both property and year fixed effects; and standard errors that are clustered at the property-level. OLS = ordinary least squares.
* p<0.10, ** p<0.05, *** p<0.01

Table S6: Treatment effect heterogeneity; two-way fixed effects OLS regressions; dependent variable is the number of hectares of forest in year t = 1986–2023; treatment is award of formal title (s.e.)

	(1) *cattle*	(2) *pasture*	(3) *legal entity*	(4) *enterprise*	(5) *enterprise×legal entity*	(6) *early title*	(7) *beni*	(8) *santa cruz*	(9) *enterprise×santa cruz*	(10) *cattle*
title	-97.07***	-40.42***	5.788	-48.47***	4.048	7.454*	-10.28**	17.64***	-17.15***	-240.3***
	(4.870)	(4.955)	(4.143)	(4.245)	(4.022)	(3.977)	(4.407)	(4.599)	(3.845)	(10.42)
title×cattle	154.6***									147.1***
	(5.157)									(7.531)
title×pasture		95.05***								58.78***
		(6.695)								(6.159)
title×legal entity			32.03							49.64***
			(21.06)							(13.73)
title×enterprise				190.2***						155.4***
				(9.814)						(10.35)
title×enterprise×legal entity					90.78***					-25.04
					(32.40)					(37.72)
title×early title						3.014				29.31***
						(8.992)				(8.790)
title×beni							79.07***			48.43***
							(7.681)			(7.322)
title×santa cruz								-16.73**		75.76***
								(6.907)		(6.040)
title×enterprise×santa cruz									155.0***	23.19
									(15.79)	(20.67)
Marginal effect (ha)	57.51***	54.63***	37.81*	141.75***	94.82***	10.47	68.79***	0.91	137.85***	
	(4.38)	(5.16)	(19.65)	(8.50)	(31.14)	(7.80)	(6.57)	(5.47)	(14.62)	
Marginal effect (% ATT)[a]	569.83	536.31	340.43	1551.01	1004.44	21.92	701.21	-89.38	1505.56	
Properties in subgroup (no.)[b]	5106	3938	552	2352	346	953	2271	4090	1155	7298

[a]Marginal effect as a percentage of OLS average treatment effect on treated (8.58 ha).
[b]Number of properties for which dummy variable = 1, e.g., cattle = 1.
All models include: n = (7,298 properties × 38 years =) 277,324 property-year observations; two time-varying control variables—rainfall and temperature; both property and year fixed effects; and standard errors that are clustered at the property-level. OLS = ordinary least squares.
* p<0.10, ** p<0.05, *** p<0.01