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Land, Climate, and Policy: Essays on Land Use Change in Latin America

By

BRUNO PIMENTA

DISSERTATION

Submitted in partial satisfaction of the requirements for the degree of

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To my mother, Andrea ...

The memories and the love are always with me.

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ABSTRACT

Land, Climate, and Policy: Essays on Land Use Change in Latin America

Land use decisions lie at the intersection of climate, markets, and institutions. Policies designed to conserve forests or sequester carbon, markets created to govern commodity supply chains, and climatic shocks that alter the profitability of land management all shape how land is used, which in turn leads to environmental consequences. Yet these forces rarely operate in isolation, and their interactions often produce outcomes that are heterogeneous across agents or unintended by policymakers. This dissertation examines three such interactions in Latin America, using quasi-experimental methods to identify causal effects of climate shocks, market-based conservation policies, and land-use regulation on deforestation, land markets, and freshwater provision.

The first chapter studies how droughts differentially affect deforestation on public versus private lands in the Brazilian Amazon. Climate change has increased the frequency of drought events in the Amazon. Deforestation worsens local climate dynamics, and weather shocks can, in turn, influence land-use decisions. In the Brazilian Amazon, where land tenure is a mix of public and private holdings, landholders on public lands may lack incentives to manage forests sustainably. Using spatially matched comparisons within 0.5° grid cells, we find that during drought years, ranchers on public lands expand pasture area by 30% more than in baseline years, while private landholders show no significant response. We explore the mechanisms behind this difference and find that drought-induced pasture degradation leads to expansion in both tenure types. However, ranchers on public lands expand pasture by 20% more than those on private lands in response to similar degradation. Moreover, only public landholders continue to expand even after controlling for degradation, suggesting additional drivers, such as lower deforestation costs in drier, more flammable forests. These findings indicate that climate-induced droughts increase deforestation pressure on public forests, which store substantial carbon stocks and cover over two-thirds of the Amazon. This vulnerability complicates efforts to mitigate climate change. Policymakers should enhance monitoring of public lands during drought years

and improve enforcement of public property rights.

The second chapter examines the Amazon Soy Moratorium (ASM), a landmark private-sector initiative that restricted the purchase of soy grown on recently deforested land. Estimates suggest the moratorium prevented up to 1.8 million hectares of deforestation — a figure that, given the small scale of direct forest-to-soy conversion, could only be explained by an indirect effect: the moratorium discouraging cattle ranchers from clearing forest speculatively in anticipation of future land sales to soy producers. We investigate this mechanism by testing whether the moratorium disrupted the pasture-soy land market, using a difference-in-differences design that compares pasture-to-soy conversion on unregulated and regulated lands before and after the moratorium’s introduction. We find no evidence of increased conversion on unregulated land — and in fact observe a decline. While this null result reduces concerns about within-Amazon leakage, it also sits in tension with the large effects estimated in the literature: a moratorium strong enough to deter speculative clearing at scale should have generated detectable reallocation pressure on unregulated land. A key challenge is that the 2006–2007 soy price crash coincides with the moratorium’s introduction, preventing us from cleanly separating policy effects from the contemporaneous collapse in demand.

The third chapter turns to a different class of environmental policy: the promotion of tree cover expansion as a carbon sequestration strategy. While increasing forest area is frequently advocated as a cost-effective way to meet climate mitigation goals, the expansion of exotic monoculture industrial tree plantations (ITP) may disrupt local ecosystem services in ways overlooked by carbon accounting frameworks. We investigate the impact of these plantations on freshwater provision using watershed-level data and panel variation in land-cover change in Chile. Our results indicate a delayed but robust negative effect of ITP expansion on freshwater availability. A one percentage point increase in ITP conversion reduces annual normalized net outflow by approximately $0.011 \text{ Mm}^3/\text{year}$ per km^2 after 7 to 9 years — equivalent to roughly 7% of mean annual net outflow at observed expansion rates.

The three chapters contribute to a growing literature on the heterogeneous and some-

times perverse consequences of climate, institutions, and environmental policy for land use. They highlight the importance of accounting for institutional context, such as land tenure, when predicting behavioral responses to climate shocks; of scrutinizing the mechanisms behind estimated policy effects when direct channels are insufficient to explain their magnitude; and of considering trade-offs across ecosystem services when designing and evaluating conservation policy. As climate change intensifies and carbon mitigation becomes more urgent, understanding these interactions will be essential for designing effective environmental policies.

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Chapter 1

The Differential Responses of Farmers on Private and Public Lands to Droughts in the Brazilian Amazon

Bruno Pimenta, Matthew Reimer, and Kevin Novan

Abstract. *Climate change has increased the frequency of drought events in the Amazon. Deforestation worsens local climate dynamics, and weather shocks can, in turn, influence land-use decisions. In the Brazilian Amazon, where land tenure is a mix of public and private holdings, landholders on public lands may lack incentives to manage forests sustainably. This study examines how droughts differentially affect pasture expansion on public versus private lands. Using spatially matched comparisons within 0.5° grid cells, we find that during drought years, ranchers on public lands expand pasture area by 30% more than in baseline years, while private landholders show no significant response. We explore mechanisms behind this difference and find that drought-induced pasture degradation leads to expansion in both tenure types. However, ranchers on public lands expand pasture by 20% more than those on private lands in response to similar degradation. Moreover, only public landholders continue to expand even after controlling for degradation, suggesting additional drivers, such as lower deforestation costs in drier, more flammable forests. These findings indicate that climate-induced droughts increase deforestation pressure*

on public forests, which store substantial carbon stocks and cover over two-thirds of the Amazon. This vulnerability complicates efforts to mitigate climate change. Policymakers should enhance monitoring of public lands during drought years and improve enforcement of public property rights.

1.1 Introduction

Despite decades of regulation and improved monitoring, deforestation in the Amazon remains one of the most pressing global environmental challenges (Franco et al., 2025). Much of the ongoing deforestation is occurring on public land, where land grabbers and squatters regularly occupy and clear the land for productive use (e.g. cattle grazing). Previous research highlights weak institutions can contribute to the growth of deforestation on these public lands (Balboni et al., 2023; Mendelsohn, 1994). When land tenure is insecure, as it is for much of the publicly occupied land, the risk of losing ownership causes land holders to discount the returns from preserving the value of the land, creating weaker incentives to invest in costly conservation or restoration practices and stronger incentives to expand into forested territory when opportunities arise (Bohn & Deacon, 2000; Farzin, 1984). In this paper, we demonstrate that droughts, which have become more frequent and severe in the Amazon (Bottino et al., 2024; Ottoni et al., 2023), have the potential to amplify these incentives, leading to more rapid deforestation of public lands.

Understanding how land tenure may alter incentives to deforest is particularly relevant in the Brazilian Amazon, where the remaining forest is spread across a mosaic of public and private lands. Between 2019 and 2021, roughly half of the deforestation occurred on indigenous lands, units of conservation, and undesignated public forests (Moutinho et al., 2022). Commercial agriculture is the main driver of deforestation throughout the region, with pasture-fed cattle ranching alone accounting for 90% of deforestation (Mapbiomas, 2024; Tyukavina et al., 2017). Cattle ranching in the Amazon is characterized by extensive pasture management with little investment in irrigation and commercial inputs like fertilizer and lime (Feltran-Barbieri & Féres, 2021; Garrett et al., 2021). As a result, ranchers depend heavily on rainfall, making them particularly vulnerable to climate

shocks.

In the Amazon, climate shocks, and in particular droughts, have become far more frequent and intense. Several "once-in-a-century droughts" occurred in the last few decades and the 2023-2024 drought recorded the lowest levels of rainfall in 120 years of measurement (Clarke et al., 2024). These droughts not only degrade the productivity of land being used for agriculture or grazing, they also reduce the cost of clearing new land through burning, a common practice in the Amazon (Boucher et al., 2013). Both of these channels can put additional pressure on ranchers to deforest and expand into more productive land (Staal et al., 2020). Yet the extent to which land tenure mediates such responses has not been clarified theoretically or verified empirically in the literature.

To explore how ranchers' responses to droughts can differ based on land tenure, we first introduce a simple model of a rancher's dynamic decision to expand their pasture holdings into new, previously forested land. We first examine how pasture expansion rates differ across land tenure regimes in the absence of climate shocks. Our model incorporates two key differences between the incentives faced by ranchers on public versus private land. First, ranchers on public lands face a threat of eviction, weakening their incentive to expand their pasture holdings. Second, and working in the opposite direction, the fact that there is no opportunity cost to using more public land strengthens the incentive for ranchers on public lands to expand their pasture holdings. Thus, in the absence of drought shocks, the difference in the baseline rate of pasture expansion on private versus public lands is ambiguous.

Next, to understand how the rate of pasture expansion responds to climate shocks, we introduce two drought impacts in the model. First, droughts degrade the quality of existing pastureland, increasing the demand for new, non-degraded pastureland. Second, to capture the fact that it is easier to clear forested land when it is drier, we model drought shocks as reducing the cost of clearing new land for pasture. While our model illustrates that the drought-induced degradation of pasture will drive up pasture expansion rates equally across public and private lands, we find that the reduction in clearing costs drives a larger increase in pasture expansion on public lands. In sum, the findings from

our theoretical model suggest that droughts systematically amplify pasture expansion on public relative to private lands, even when baseline expansion rates differ, thereby highlighting how climate shocks and tenure insecurity interact to shape land-use dynamics.

To empirically examine whether droughts have a more pronounced impact on pasture expansion within public versus private lands, we use observed land-use behavior across public and private land in the Brazilian Amazon. We test whether droughts induce greater pasture expansion on public than on private lands by comparing their responses within the same spatial and climate context. Our approach exploits plausibly exogenous variation in the timing and severity of droughts across 0.5° grid cells in the Brazilian Amazon and contrasts land-use changes between public and private areas inside each cell. This within-cell, differences-in-differences framework controls for all time-invariant local characteristics and common shocks, isolating how land tenure mediates farmers' responses to droughts. In line with the conceptual model, we interpret stronger drought responses on public lands as evidence that droughts systematically amplify pressure for pasture expansion on public relative to private land.

Our empirical findings provide significant evidence that ranchers on public lands respond more to droughts: we find that during drought years, ranchers on public lands expand pasture area by 30% more than in baseline years, while private landholders' land use shows no significant difference. These findings are in line with the predictions from our theoretical model.

However, we find that these results also depend on the region's climate context. When we disaggregate the analysis by climatic conditions, a more nuanced pattern emerges. Ranchers on public lands consistently expand their pasture area during droughts, regardless of whether the region is dry or humid. In contrast, ranchers on private lands increase pasture expansion during droughts only in drier regions. This heterogeneity suggests that in areas where rainfed agriculture predominates, even private landholders may lack incentives to invest in sustainable land management, behaving more like their counterparts on public lands.

Investigating the possible mechanisms behind our main results, we find that droughts

affect pasture degradation, which in turn affects pasture expansion. Ranchers in public lands are more sensitive to pasture degradation than private land farmers. This finding provides evidence that public land ranchers are less inclined to invest in and restore their degraded pastureland, preferring instead to move to new land. We also find that farmers in public lands still react to droughts after controlling for pasture degradation, which is evidence that other mechanisms are at play. One potential mechanism is that the environmental deregulation after 2012 incentivizes farmers to claim more public land, and the reduction in clearing costs during droughts provides better opportunities to claim new land. While our data do not allow us to identify this potential mechanism unequivocally, we find supporting evidence consistent with land claims expanding during droughts in the post-2012 period.

This study contributes significantly to three branches of literature. A large body of research investigates the relationship between land tenure security and land use. Building on the classic argument by Hardin (1968), who demonstrated in his *Tragedy of the Commons* that open-access resources tend to be overexploited in the absence of well-defined property rights—so that, for open-access forests, land is quickly converted to agricultural use and little or no effort is made to regenerate or maintain it—subsequent works have examined how different forms of tenure influence land-use decisions. Mendelsohn (1994) proposed a model predicting that farmers with low tenure security would discount the future more heavily and adopt less sustainable practices.

The effects of land tenure security on deforestation, however, are theoretically ambiguous and depend on the nature of the extractive process. Better tenure security can provide incentives to invest in land and make it more productive; thus, if deforestation is capital-intensive, greater security may lead to higher investment and more deforestation. Conversely, if deforestation is labor-intensive and farmers in public lands view the land as a public good, they may lack incentives to invest and manage it sustainably, thereby increasing deforestation (Krishna et al., 2017; Walker et al., 2025).

Subsequent empirical works examined this relationship across different contexts, particularly in the Brazilian Amazon. Pacheco and Meyer (2022) show that private tenure

outperforms undesignated public lands in reducing deforestation, while protected public lands such as conservation units and Indigenous territories outperform private tenure. Similarly, Baragwanath and Bayi (2020) find that granting property rights to Indigenous communities leads to lower deforestation. Other studies explore specific policy and institutional changes: Probst et al. (2020) find that recipients of land titles under the *Terra Legal* program increased deforestation after titling, while Qin et al. (2023) document substantial reductions in deforestation following the demarcation of protected areas, particularly those under strict protection. Conversely, Keles et al. (2023) show that the erasure of protected areas increases deforestation when these areas had previously constrained agricultural expansion. Overall, the meta-analysis by Robinson et al. (2014) synthesizes these findings, confirming that protected public lands tend to yield better conservation outcomes than private lands, while private tenure generally outperforms undesignated public lands, especially in South America.

A related branch of literature investigates the relationship between agricultural efficiency and land use. Many studies investigate how increasing cattle stocking rates and restoring degraded pasture could help preserving and restoring native vegetation (Cohn et al., 2014; Feltran-Barbieri & Féres, 2021; Spera, 2017). We show that cattle intensification will remain a challenge as long as public land is readily available.

The literature on the impact of climate shocks on land-use decisions remains nascent, and theory suggests that the effects of droughts on deforestation through agricultural land-use change are complex and ambiguous (Desbureaux & Damania, 2018). On the one hand, climate shocks reduce agricultural yields, lowering the profitability of agriculture, raising the value of outside options, and thereby reducing deforestation pressure. On the other hand, drought-induced land degradation may lead farmers to seek new cultivating areas, increasing deforestation.

Empirical evidence on this relationship remains limited. A handful of studies examining droughts and deforestation in Africa—where subsistence agriculture dominates—find that droughts tend to increase deforestation (Desbureaux & Damania, 2018; Leblois, 2021; Vaglietti et al., 2022). In the Amazon, Staal et al. (2020) document a feedback mech-

anism between drought and deforestation, estimating that deforestation contributes to about 4% of the region’s observed drying. They also find that while deforestation responses to droughts vary across locations, deforestation increases by an average of 0.13% in drought years.

We connect these branches of literature by investigating how climate shocks affect land-use responses across different land tenures. To the best of our knowledge, it is the first study to explore how the economic incentives generated by the interplay of climate and the different land tenures may result in significantly different land use responses. The findings also inform policymakers to increase public lands’ monitoring during drought years and design mechanisms to better enforce public property rights.

1.2 Background

1.2.1 Land Tenure and Property Rights

A mosaic of public and private lands, the Brazilian Amazon has a long history of insecure land tenure in which land grabbers and squatters often occupy public land (Azevedo-Ramos et al., 2020). Public lands are mainly divided into conservation units, indigenous lands, and undesignated public forests. Some conservation units may allow sustainable agricultural production, but others strictly forbid economic activities. Undesignated public forests are not protected and are especially vulnerable to occupation.

The origins of Brazil’s land tenure system are deeply rooted in inequality. During the colonial period under Portuguese rule, large estates were granted to a small elite through a feudal system called *sesmarias*, while most peasants cultivated land informally (Mueller et al., 2011). When the *sesmarias* were abolished in 1823, no new rules immediately replaced them, creating a legal vacuum in which both subsistence farmers and powerful landowners invaded unclaimed public land. Conflicts were often resolved by force: as a 19th-century senator observed, “land conflicts were settled by the blunderbuss”¹ (Westin, 2020b).

In 1850, the *Lei de Terras* (Land Law) sought to impose order by requiring that land

¹Bacamarte, an old firearm.

acquisitions occur via purchase, ending free occupation (Westin, 2020b). While designed to curb arbitrary appropriation, the law consolidated land concentration by excluding small farmers without capital (Araujo et al., 2009). From then on, Brazil’s rural landscape combined a minority of secure large landholders with a large share of informal occupants, patterns that later shaped frontier expansion.

For centuries, the Amazon was sparsely populated and economically peripheral, apart from the rubber boom ² at the turn of the 20th century. As such, the land dynamics described above remained largely outside the Amazon. This changed under the military dictatorship (1964–1985), which promoted settlement as part of a national integration strategy. The federal government empowered the National Institute for Colonization and Agrarian Reform (INCRA) to manage colonization programs, appropriating large tracts of state lands along new highways such as the Transamazônica (L. Alston et al., 2000; Araujo et al., 2009; Carrero et al., 2022). Between 1970 and 1985, the population of Pará, a state at the center of Brazil’s agricultural frontier, nearly doubled, and farmland expanded rapidly, as both smallholders and large ranchers moved into the region (L. Alston et al., 2000; Garrett et al., 2021).

With the return to democracy, Brazil enacted a new Constitution in 1988, reshaping the legal framework for land and environment. The charter enshrined the *social function of property*, requiring land ownership to contribute to social welfare and environmental protection; reinforced protections for Indigenous territories; and mandated the creation of conservation units. Public lands were recognized as belonging to the Union or state governments to be allocated for collective purposes such as agrarian reform, conservation, or Indigenous recognition (Mueller, 2018).

At the same time, the Constitution institutionalized contradictions that persist. While prohibiting privatization of undesignated public lands, Article 191 allowed squatters to transform occupation into ownership via “productive use” (usufruct after one year and full title after five years), creating incentives to deforest in order to demonstrate cultivation

²The rubber boom, fueled by rising global demand for latex used in industrial goods such as tires, brought a temporary influx of migrants and capital to the region, but collapsed once Southeast Asian plantations outcompeted Amazonian extraction.

(Araujo et al., 2009).

Although official settlement programs distributed small plots, much occupation occurred informally on “vacant” or undesignated public lands (Araujo et al., 2009). A frontier logic took hold that deforestation itself could confer property rights: clearing forest and establishing pasture signaled “productive use” and strengthened claims—according to a squatter, “the best title is the biggest axe” (L. Alston et al., 2000). Thus, the 1988 charter was both a democratic breakthrough (expanding Indigenous and environmental rights) and a continuation of frontier dynamics where occupation and clearing could translate into claims.

Subsequent laws deepened these contradictions. The Terra Legal program (2009), one of the world’s largest titling initiatives, granted conditional rights to occupants of undesignated public lands who could prove cultivation prior to 2008. Despite safeguards, the program legitimized illegal occupation and spurred speculation (Carrero et al., 2022; Probst et al., 2020).

In 2012, Congress approved a major reform of the Forest Code. While presented as improving compliance and monitoring, it amnestied illegal deforestation prior to 2008 and introduced the self-declaratory Rural Environmental Registry (*Cadastro Ambiental Rural*, CAR) (Garrett et al., 2021; Mueller, 2018). Although CAR does not establish ownership, it has been used by land grabbers to bolster claims over invaded areas, especially when paired with deforestation to signal “productive use” (Carrero et al., 2022). Over the following decade, repeated amnesties and expansions in the legalizable size of claims further incentivized invasion and regularization expectations (Abdenur et al., 2020; Carrero et al., 2022). Nonetheless, the Brazilian Federal Police often arrests landgrabbers (Comunicação Social da Polícia Federal em Rondônia, 2024).

The Amazon today is a patchwork of tenure regimes: titled private farms and agrarian reform settlements, Indigenous lands, conservation units, and vast undesignated public forests—on the order of 100 million hectares (Azevedo-Ramos et al., 2020; Pacheco & Meyer, 2022). Clearing in undesignated lands is always illegal (Assunção et al., 2023). Yet these areas are disproportionately targeted by land grabbing and speculative occupation,

with many operators anticipating future regularization given the historical pattern of amnesties and titling (Carrero et al., 2022).

In sum, the trajectory of property rights in Brazil reveals a persistent tension between formal protections and informal practices. From colonial concentration to the 1988 Constitution and subsequent reforms, the Amazon faces the consequences of these contradictions: insecure tenure, state-sponsored colonization, and permissive or contradictory legislation converge to make public lands a vast agricultural frontier where land-grabbing is opportunistic, but property rights are insecure (L. Alston et al., 2000; Carrero et al., 2022; Mueller, 2018). As a result, deforestation in public lands is one of the most pressing problems in the Amazon. Between 2019 and 2021, 51% of the total deforestation occurred in indigenous lands, units of conservation, and undesignated public forests (Moutinho et al., 2022).

1.2.2 Droughts and Deforestation

The Amazon Basin is an exceptionally humid region that covers more than 40% of South America’s land area and has a mean annual precipitation of 2300mm (Fisch et al., 2006). Rainfall is not equally distributed across the Amazon, however. The Southern portion of the Amazon receives less than 2000mm of rainfall, while the Northwestern portion of the Amazon, in the bordering region of Brazil, Colombia, and Venezuela, receives over 3000mm of rainfall, and there is no dry season in this region (Fisch et al., 2006; Michot et al., 2019). Nevertheless, most of the Amazon has marked rainy and dry seasons (Marengo, 2004; NASA, 2021; Skidmore, 2023). The rainy season lasts from November to March, and the dry season from May to September, with August being the driest month (Fisch et al., 2006; NASA, 2021). April and October are considered transition months (Fisch et al., 2006). The rainforest acts as a water recycling system and pumps humidity back into the atmosphere (NASA, 2021). As a result, significant rainfall takes place even during the dry months (Bacellar, 2022).

Despite the region being naturally humid, climate change has increased the frequency and intensity of drought events in the Amazon. The 2023-2024 drought recorded the lowest level of rainfall in 120 years of measurement, and several “once-in-a-century droughts”

took place in the last few decades (Clarke et al., 2024). Different studies document increases in the duration of the dry season (Bottino et al., 2024). Droughts in the Amazon are also related to the El Niño Southern Oscillation (ENSO) phenomenon. The region suffered drier conditions during the ENSO years of 1983, 1994/1995, 1997/1998, 2005, 2010, 2015/2016 and 2023. Moreover, climate change is making extreme ENSO events more frequent (Skidmore, 2023). The rainforest is increasingly at risk of savannization as the region’s climate shifts to drier conditions (Bottino et al., 2024).

Local disturbances of the region’s ecosystem also affect rainfall patterns. Deforestation affects the region’s climate by altering the water cycle. Trees pump water from the soil into the air through their leaves, and this transpiration mechanism acts as an essential buffer during droughts (Staal et al., 2020). However, a feedback mechanism between climate and deforestation may exist, as the drivers of deforestation are also affected by climate shocks. Deforestation substantially increased in 2005, 2007, 2010, and 2015, and those years were marked by intense El Niño events, high air temperatures, or severe drought (Qin et al., 2023).

Droughts may affect land use change by lowering deforestation costs. In Brazil, fire is often used in the deforestation process. Fires do not occur naturally in the Amazon. As a humid rainforest, fires only occur during warm and dry conditions (Bottino et al., 2024). Deforestation usually occurs during the dry months, when the forest is easier to cut down and burn (Boucher et al., 2013). Droughts might amplify deforestation by making the vegetation more flammable and making it easier to use traditional slash-and-burn techniques. Furthermore, farmers often use fire to clear pasture of weeds, and dryer vegetation makes it easier for the fire to escape into nearby areas (Staal et al., 2020).

Another channel through which climate shocks may affect land use change is by affecting agricultural yields. Agriculture, a sector particularly vulnerable to climate shocks, relies on precipitation and temperature for production. Farmers may respond to droughts by altering land use, and the farmers’ circumstances may alleviate or increase deforestation pressure (Amacher et al., 2009; Balboni et al., 2023; Mendelsohn, 1994; Robinson et al., 2014). On the one hand, considering the farmer has access to outside options, climate

shocks reduce agricultural yields, thereby increasing the value of alternative options and alleviating deforestation pressure. On the other hand, farmers may attempt to expand their cropland or pasture to new areas to compensate for degradation caused by climate shocks, potentially leading to long-term land-use changes.

Pasture-fed cattle ranching is the agricultural activity that drives the most deforestation in the Brazilian Amazon. It is responsible for 90% of the total deforestation in the region (Mapbiomas, 2024). Cattle ranching in the Amazon is characterized by extensive pasture management. Investments, such as irrigation and commercial inputs, including fertilizers and lime, are uncommon (Garrett et al., 2021). These extensive practices lead to across-the-board pasture degradation due to inadequate land use and management of vegetation or livestock, which often results from overgrazing, lack of fertilization, and pest control (Feltran-Barbieri & Féres, 2021). Since reverting this degradation requires considerable investments, ranchers often move to newly deforested land (Garrett et al., 2021). As a result, ranchers depend heavily on rainfall, and this extensive production system leaves farmers particularly vulnerable to climate shocks.

1.3 Conceptual Model

Understanding how land tenure conditions shape ranchers’ responses to climate shocks requires a framework that explicitly incorporates the risk environment faced by farmers in the Amazon. The Brazilian Amazon is characterized by a mosaic of private and public lands, where the latter include conservation units, Indigenous territories, and vast expanses of undesignated public forests (UPFs). In these public lands, ranchers often occupy irregularly and face persistent legal and extralegal threats to their tenure security. Federal law prohibits the privatization of these forests, yet enforcement is uneven, and waves of amnesties and titling initiatives have blurred the boundaries between licit and illicit claims (Carrero et al., 2022).

To formalize these dynamics, we model a farmer’s land use as an asset-replacement problem. Farmers are assumed to graze cattle on a single parcel that degrades over time, and this parcel can be “replaced” by clearing a fresh plot of land. Each season, the farmer

observes the quality of the land, which is a function of time and drought spells, and then chooses to continue grazing on the current piece of land or to replace the land and clear a new plot.

1.3.1 Dynamics

Formally, let $d \in \{0, 1\}$ denote a farmer's binary decision to replace the land. Land quality $q \in [0, \bar{q}]$ is an endogenous state variable and is assumed to decrease over time. Land quality is negatively impacted by drought spells $z \in \{0, 1\}$, an exogenous state variable that follows a stationary Markov process. Land quality in period t can be expressed using the state equation:

$$q_t = \begin{cases} g(q_{t-1}, z_t) & \text{if } d_t = 0, \\ \bar{q} & \text{if } d_t = 1. \end{cases}$$

That is, current land quality q_t depends on last period's land quality and whether there is a drought spell in the current period. The condition $0 < g_q < 1$ ensures that land quality is decreasing over time, and $g(q, 1) < g(q, 0)$ implies land quality is negatively impacted by drought. If a farmer replaces the parcel, land quality "resets" to its maximum $\bar{q}$.

Let's consider a farmer's utility $u(q, d)$ as a function of grazing profits $\pi(q)$, which are assumed to be increasing and concave in land quality, and depends on replacement costs:

$$u(q, d) = \begin{cases} \pi(q) & \text{if } d = 0, \\ \pi(\bar{q}) - \rho - f(z) & \text{if } d = 1. \end{cases}$$

Here, ρ represents the rental rate (shadow price of land) and $f(z)$ represents the cost of clearing new land, assumed to decrease in drought spells: $f(1) < f(0)$. Each season, the farmer observes (q_t, z_t) and makes a land-use decision to maximize expected net present utility $V(q, z)$, represented with the Bellman equation:

$$V(q, z) = \max \left\{ u(q, 0) + \beta \mathbb{E}_{z'} V(g(q, z'), z'), u(\bar{q}, 1) + \beta \mathbb{E}_{z'} V(g(\bar{q}, z'), z') \right\},$$

where $\beta < 1$ denotes the discount factor. The Bellman equation says that the farmer must compare the benefit of waiting to replace the land with the benefit of replacing the land, where these benefits consist of present-day utility and the expected future benefits of all future land use.

Consider now a farmer's policy function $d^*(q, z)$, which describes their optimal land replacement decision, given a land quality of q and a drought spell of z . Define the "advantage-of-waiting" as:

$$\begin{aligned}\Delta(q, z) &= \underbrace{[\pi(q) + \beta \mathbb{E}_{z'} V(g(q, z'), z')]}_{\text{Wait}} - \underbrace{[\pi(\bar{q}) - \rho - f(z) + \beta \mathbb{E}_{z'} V(g(\bar{q}, z'), z')]}_{\text{Replace}} \\ &= \underbrace{\pi(q) - [\pi(\bar{q}) - \rho - f(z)]}_{\text{Present advantage of waiting}} - \underbrace{\beta [\mathbb{E}_{z'} V(g(\bar{q}, z'), z') - \mathbb{E}_{z'} V(g(q, z'), z')]}_{\text{Future disadvantage of waiting}}\end{aligned}$$

The advantage-of-waiting function sufficiently describes a farmer's policy function: as long as $\Delta(q, z) > 0$, i.e., the benefits to waiting are larger than the benefits of replacing, a farmer will wait to replace their land. This can be decomposed into a present advantage of waiting and a future disadvantage of waiting. The present advantage compares the benefits of the current degraded land $\pi(q)$ to those the would receive from undegraded land $\pi(\bar{q})$ net of clearing costs $\rho + f(z)$. For high values of land quality, the present advantage will be positive and will decrease over time as the land degrades. The future disadvantage of waiting is the relative future benefit of starting the next period with less degraded land if the farmer replaces the current land. Note that $V(g(\bar{q}, z'), z') \geq V(g(q, z'), z')$ since $\bar{q} \geq q$; thus, the future disadvantage of waiting is weakly positive and becomes greater over time as the land degrades. Thus, the present advantage of waiting must be large enough to offset the future disadvantage of waiting in order to delay replacing the land.

The policy function can be completely described by a threshold rule using the advantage-of-waiting function, $\Delta(q, z)$. Since $\pi(q)$ is assumed to be strictly increasing in land quality q and the cost of clearing $\rho + f(z)$ does not depend on land quality, then the value function $V(q, z)$ is strictly increasing in q , and thus, $V(g(q, z'), z')$ is strictly increasing in q . As a result, $\Delta(q, z)$ is strictly increasing in q and there exists a threshold land quality $q^*(z)$ such that we can define the farmer's policy function as:

$$d^*(q, z) = \begin{cases} 0 & \text{if } q > q^*(z) \quad (\text{Wait}), \\ 1 & \text{if } q \leq q^*(z) \quad (\text{Replace}), \end{cases}$$

The threshold $q^*(z)$ marks the value of the land quality below which a farmer chooses to replace their land. Higher values of $q^*(z)$ represent land replacement at younger ages and with less degradation, meaning a higher rate of pasture expansion for any given drought conditions z . Larger differences in $q^*(z)$ across drought conditions, $q^*(1) - q^*(0)$, represent a larger set of land quality values that are subject to replacement with a drought, thereby representing a larger pasture expansion in response to a drought.

1.3.2 Tenure differences and drought effects

We can now use this theoretical framework to depict the effect that droughts and land tenure, individually, have on a farmer's land-use choice, and, finally, use our model to predict how droughts and land tenure interactions affect land-use decisions.

Let us first analyze the effects of droughts on a generic farmer's decisions. Drought affects the farmer's calculus in two ways, as illustrated in Figure 1.1 (notice that the advantage-of-waiting curve, $\Delta(q, z)$, is upward sloping). First, it induces a "curve shift" by reducing the cost associated with replacing the current parcel of land with a new one. This can be represented by a downward shift in $\Delta(q, z)$ by $f(0) - f(1)$, and results in an increase in the quality threshold q^* . Thus, for land with quality $q(0) < q < q(1)$, a drought induces land replacement (i.e., pasture expansion) by decreasing the cost of replacement.

Second, drought induces a "state shift" by negatively affecting the quality of land itself, and thus, the profits associated with waiting. This can be seen in the movement from q_0 , the quality of land without drought ($z = 0$), to q_1 , the quality of land with drought ($z = 1$). Thus, any land quality that is pushed over the threshold $q^*(1)$ by droughts will be replaced by the farmer. Both mechanisms, therefore, lead to a reduction in the advantage of waiting and thus increase pasture expansion.

Let us now examine the impact of different land tenure systems on land use. We can adjust two parameters to differentiate public from private land use.

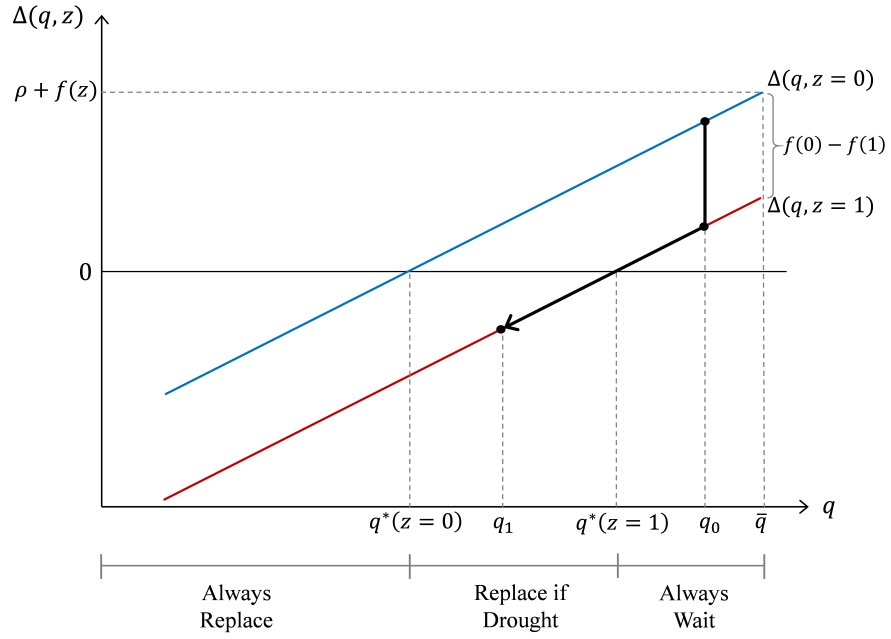


Figure 1.1: Impacts of droughts on land replacement

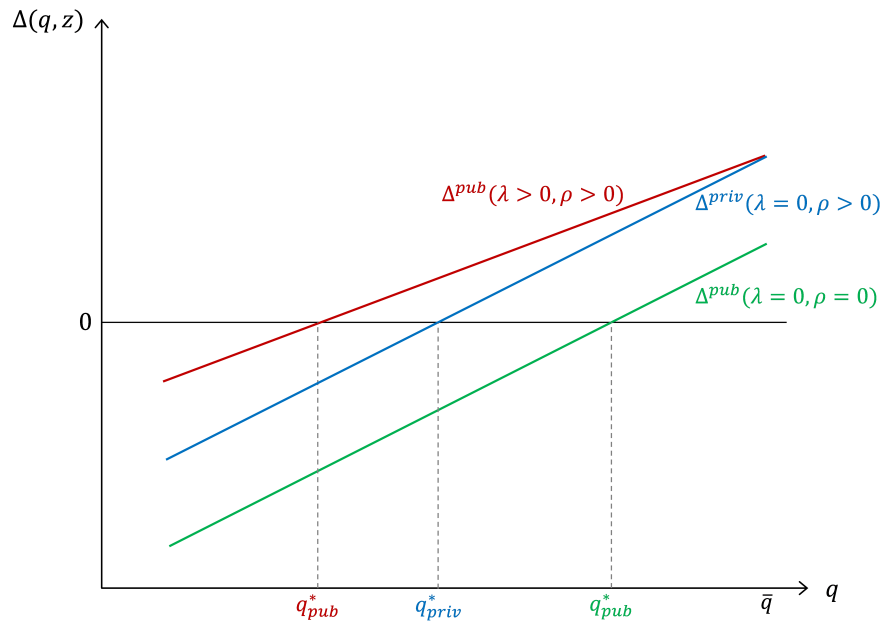


Figure 1.2: Private versus public land conversion

First, the parameter β represents the discount factor associated with a risk-free discount rate. Suppose a farmer on public land faces some probability of being evicted. Assuming that the probability of eviction is memoryless and is independent of land quality, then we can represent a public farmer's discount factor as $\beta(1 - \lambda)$, where $0 < \lambda < 1$ is the constant hazard of being evicted.

The motivation for the eviction hazard constant λ is rooted in Brazil's contemporary land-tenure realities. Farmers on public lands are subject to state enforcement risks such as fines, embargoes, and the confiscation of means of production (Moutinho & Azevedo-Ramos, 2023). They also face violent disputes with other claimants, which have historically resulted in threats, forced displacements, and even assassinations (Carrero et al., 2022; A. Sant'Anna & Young, 2010). Other documented sources of tenure insecurity on public lands include the risk of arrest by state law enforcement and the reduced marketability of properties with contested or fraudulent titles³.

Second, the parameter ρ represents the opportunity cost of converting forested land to pasture. For private land owners with scarce land, we can think of $\rho > 0$, either due to the shadow value of land that is already owned by the farmer, or the rental/purchase price of land on the private market. For public land users, expanding into public forest has no such opportunity cost, and thus, $\rho = 0$.

We are now in a position to conduct some comparative statics to see how land use differs across private and public land. The comparative statics for a given drought spell z are depicted in Figure 1.2. With a positive probability of being evicted, public farmers have a smaller discount factor. This has the effect of rotating $\Delta(q, z)$ up around the point at $\bar{q}$, illustrating that the advantage of waiting for public farmers is higher than for private farmers for all $q < \bar{q}$ because they discount more the future disadvantage of waiting. Thus, $q_{pub}^*(z) < q_{priv}^*(z)$. In other words, public farmers are less likely to incur a costly investment in land clearing if there is some probability that they will be evicted and unable to reap the rewards. Thus, the probability of eviction incentivizes public farmers

³The appendix provides news sources illustrating: (1) arrests of land grabbers by federal police, (2) the classification of properties with disputed or fraudulent titles as "distressed assets" in the land market, and (3) violent disputes among competing claimants.

to hold onto land longer than private farmers, allowing the land to degrade more. Private farmers, on the other hand, are more likely to replace land and expand pasture, all else equal.

In contrast, with no opportunity cost associated with land $\rho = 0$, public farmers have a constant lower advantage of waiting than private farmers, regardless of the land quality. This has the effect of shifting $\Delta(q, z)$ down in a parallel fashion. As a result, $q_{pub}^*(z) > q_{priv}^*(z)$: the relative ease of converting new land into pasture incentivizes public farmers to hold on to land for shorter periods of time and expand pasture at greater rates than private farmers. Overall, whether public farmers expand pasture at greater rates than private farmers depends on whether the “opportunity-cost-of-land” effect dominates the “probability-of-getting-evicted” effect.

Based on these results, we can finally analyze the differential drought effects across public and private farmers. Figure 1.3 depicts the comparative statics predictions, assuming that private and public farmers have the same baseline pasture conversion rates, $q_{pub}^*(0) = q_{priv}^*(0)$, for simplicity.

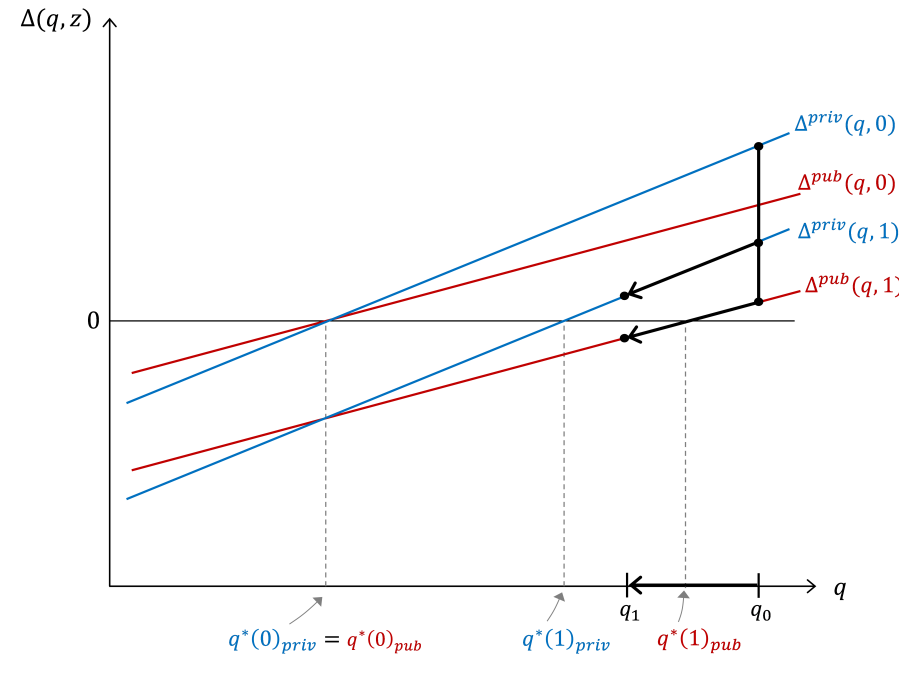


Figure 1.3: Drought effects: Public vs. Private

First, the drought-induced “curve-shift” effect from reducing the clearing cost of land

shifts the advantage-of-waiting curve down. This "curve-shift" effect is represented in 1.3 by the difference between $q^*(1)_{priv}$ and $q^*(0)_{priv}$ for private lands, and the difference between $q^*(1)_{pub}$ and $q^*(0)_{pub}$ for public lands. Since public and private farmers experience the same reduction in land-clearing costs, $\Delta(q, z)$ shifts down vertically by the same amount for both types of farmers. However, $\Delta(q, z)$ is flatter for public farmers since they discount the future disadvantage of waiting more than private farmers, resulting in a larger horizontal shift in $\Delta(q, z)$ for public farmers. Thus, $q^*(1)_{pub} - q^*(0)_{pub} > q^*(1)_{priv} - q^*(0)_{priv}$: more land will be replaced by public farmers than private farmers in response to the drought, regardless of the baseline pasture conversion rates. The reduction in deforestation costs during droughts matters relatively more for farmers on public lands, because the risk of eviction makes them reluctant to invest in costly clearing methods that might not yield returns. By reducing the investment cost, droughts shift the balance between the "opportunity-cost-of-land" effect and the "probability-of-getting-evicted" effect towards the former, and therefore, public land farmers react more to droughts.

Second, the "state-shift" effect reduces the quality of land itself. Land quality that is pushed over the threshold $q^*(1)$ by drought will be replaced by the farmer. This is depicted in Figure 1.3 by the movement from $q^*(0)$, the quality of land without the drought, to $q^*(1)$, the quality of land with the drought. As depicted, the drought induces enough degradation in land quality to justify replacing the land for the public farmer, but not the private farmer. Note that while the curve shift effect is determined by the slope of $\Delta(q, z)$, the size of the state-shift effect will be the same for both public and private farmers: any land quality q that lies within $q(0) - q(1)$ of the threshold $q^*(1)$ will be induced to being replaced. Thus, as long as droughts are assumed to affect land quality equally across land types, the "state-shift" effect will be the same.

Overall, the model underscores that differences in land tenure regimes critically mediate farmers' responses to drought. In the absence of climatic shocks, relative rates of pasture expansion on private versus public lands depend on whether the higher opportunity cost of waiting (which encourages more expansion by private farmers) outweighs the higher opportunity cost of conversion (which restrains it). Droughts, however, intro-

duce additional forces that shift this balance. The “curve-shift” effect, operating through reductions in clearing costs, induces a larger response on public lands, where tenure insecurity typically discourages costly long-term investments. By contrast, the “state-shift” effect, operating through reductions in land quality, generates symmetric responses across tenure types as long as drought-induced degradation is uniform. Taken together, these mechanisms imply that droughts systematically amplify pasture expansion on public relative to private lands, even when baseline expansion rates differ, thereby highlighting how climate shocks and tenure insecurity interact to shape land-use dynamics.

Our simple model highlights the interaction of land tenure and climate shocks in shaping land-use responses, but it omits other relevant dynamics. One important example is investment in pasture restoration. Because farmers on public lands face weaker incentives to undertake costly, long-term improvements, they are less likely to restore degraded pasture. A higher eviction hazard (λ) further discourages such investments, as the risk of losing access to the land reduces the expected returns from long-term restoration. When drought reduces the cost of clearing new land, these farmers may therefore favor expansion into previously uncleared areas rather than investing in restoration.

1.4 Empirical Framework

Our empirical strategy links the theoretical predictions of Section 1.3 to observed land-use behavior across public and private land. We test whether droughts induce greater pasture expansion on public than on private lands by comparing their responses within the same spatial and climate context. Our approach exploits plausibly exogenous variation in the timing and severity of droughts across 0.5° grid cells in the Brazilian Amazon and contrasts land-use changes between public and private areas inside each cell. This within-cell, differences-in-differences framework controls for all time-invariant local characteristics and common shocks, isolating how land tenure mediates farmers’ responses to droughts. In line with the conceptual model, we interpret stronger drought responses on public lands as evidence that droughts systematically amplify pressure for pasture expansion on public relative to private land.

1.4.1 Data

Our analysis combines high-resolution spatial data on land use, land tenure, and climate to examine how droughts affect pasture expansion across public and private land in the Brazilian Amazon. We merge annual land-cover classification from the MapBiomass Project (Souza et al., 2020) with land-tenure information from the Atlas of Brazilian Agriculture and drought indicators from the Standardized Precipitation-Evapotranspiration Index (SPEI). These datasets together provide consistent coverage of forest, pasture, and climatic conditions across both public and private lands from 2003 to 2021. The resulting grid-cell level panel allows us to track changes in pasture area and quality through time and to relate them to exogenous variation in drought conditions within the same local environment.

MapBiomass uses Landsat satellite images and machine learning algorithms to classify images into land uses, including pasture, based on variables such as spectral color bands and indices of vegetation moisture. For pasture predictions, MapBiomass only uses data from the wet season, as pasture is vulnerable to climate conditions. In the Amazon, this class may occur in recently deforested areas, even before farming activities have started. Areas of natural pasture are predominantly classified as grassland or wetland, separated from manmade pasture. MapBiomass also provides data on the quality of the pasture, which is classified as healthy, moderately degraded, or severely degraded based on normalized vegetation moisture information. The data is available at a refined spatial resolution of 30m x 30m.

The SPEI is a relative measure of drought based on the water balance of a reference period, indicating water surpluses (positive values) or deficits (negative values) based on monthly precipitation and potential evapotranspiration. It represents the number of standard deviations from the normally accumulated climatic water balance for the respective location and time of the year, and is available at the 0.5° level. Our analysis considers the average drought conditions from January to December. We also explore seasonal drought conditions when investigating the possible mechanisms of droughts and

differential pasture expansion.⁴

The Atlas of Brazilian Agriculture, created using a methodology described by Sparovek et al. (2019), integrates public datasets of land tenure from different sources. Therefore, it can be considered a snapshot of Brazilian land tenure as of 2019. It addresses any overlap between private and public land using a hierarchical model that classifies the land tenures based on a priority system.⁵ The Atlas of Brazilian Agriculture also provides information on property registration under the Rural Environmental Registry (CAR), which was instituted in 2012 by the Brazilian government to facilitate the environmental regulation of rural properties since many did not comply with Brazil’s forest legislation (Garrett et al., 2021). CAR consists of a self-declaratory registry where landowners input their farms’ geolocated boundaries into the system, as well as the location of the farm’s forest reserve. The Atlas of Brazilian Agriculture considers two types of land registered in CAR: “CAR premium” consists of properties that are likely to be legitimate private lands and have less than 5% of their area overlapped with other properties; “CAR poor,” in turn, are properties registered in CAR with more than 5% of their area overlapped with other properties. CAR-poor properties are likely to represent the illegitimate occupation of public lands, and thus, we removed such lands from our analysis to better distinguish between public and private lands. Robustness tests show that including those lands in our analysis does not change our results.

Table 1.1 presents descriptive statistics for the years 2003 to 2022. On average, cells experienced droughts 28% of the time, and 65% of the land area is considered public. On average, private lands have a higher percentage of pastureland than public lands, with the former having a 20% pasture saturation rate, compared to 12% for the latter. Figure 1.4 shows the average occurrence of droughts over our studied area, where we can observe a higher drought rate in the southern and western portions of the map.

⁴The SPEI index compares the drought conditions in a given number of months to the historical average drought conditions over those months. For instance, SPEI-3 May 2009 compares the drought conditions from March to May 2009 to the historical average drought conditions for those months. The primary drought variable we use in this paper is SPEI-12 December.

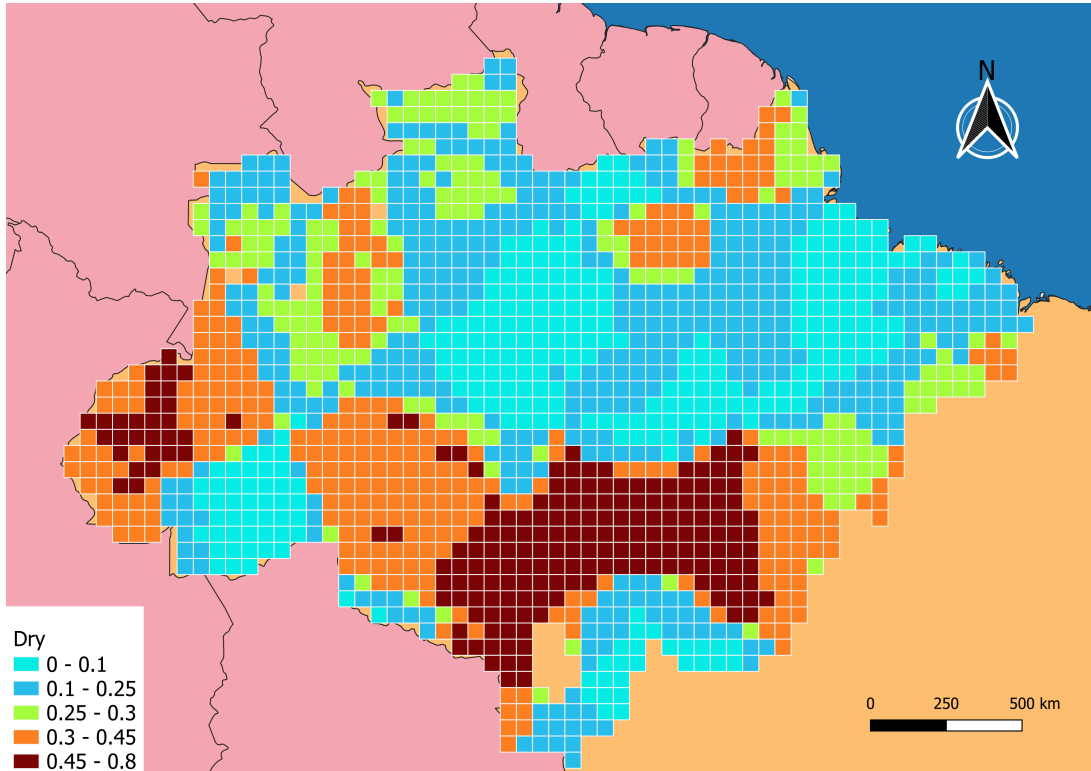
⁵For instance, private land under CAR has less priority than indigenous land because indigenous rights are established in the Brazilian Constitution.

Table 1.1: Basic Summary Statistics

	Mean	SD	Min	Max	N
Dry	0.2840	0.4509	0.00	1.00	50620
Share of Public Lands	0.6555	0.3259	0.00	1.00	50620
Pasture Growth (int)	0.0218	0.1100	-1.14	3.50	50600
Private Pasture Growth	0.0276	0.1214	-1.13	3.50	22580
Public Pasture Growth (int)	0.0171	0.0996	-1.14	1.85	28020
Pasture Growth (tot)	0.0349	0.1374	-1.13	4.36	50600
Public Pasture Growth (tot)	0.0408	0.1488	-1.13	4.36	28020
Pasture Saturation	0.1554	0.2379	0.00	1.00	50600
Private Pasture Saturation	0.1985	0.2457	0.00	1.00	22580
Public Pasture Saturation	0.1206	0.2254	0.00	1.00	28020
Pasture Growth From Forest	0.8690	0.2288	0.00	1.00	39885

Pasture Growth (int) corresponds to $\widehat{PGint}_{ts}$. Pasture Growth (tot) corresponds to $\widehat{PGint}_{tot}$

Figure 1.4: Average Drought Conditions from 2003 to 2022



1.4.2 Measuring Intensive and Extensive Pasture Growth

The primary goal of this paper is to investigate how ranchers on private lands react differently to drought than ranchers on public lands, particularly those who have already settled on public lands at the time of drought, as opposed to those who invade public lands in response to a drought. Unfortunately, we cannot observe the boundaries of public land farms since these ranchers are settled irregularly. Therefore, we cannot use public and private farms as our unit of analysis to distinguish between expansion and invasion of public land.

To address this issue, we use information on land tenure and land use to distinguish between intensive and extensive pasture growth. Intensive pasture growth measures the expansion of pasture on land adjacent to previously established pastureland, reflecting expansion by existing ranchers, while extensive pasture growth captures the creation of new pasture on previously unoccupied lands. Accordingly, for each 0.5° cell and year t , we classify growth in pastureland that intersects with pastureland in $t - 1$ as intensive and growth in pastureland that does not intersect with pastureland in $t - 1$ as extensive (Figure 1.5).

Using these definitions, we construct two aggregate measures of pasture growth for each tenure type: (1) *Intensive-pasture growth* (PG_{int}), which includes all new pasture on private lands and only extensive growth on public lands; and (2) *Total-pasture growth* (PG_{tot}), which includes all observed new pasture on private and public land. Assuming that all private lands are already settled and that extensive growth in public lands results from new ranchers invading unoccupied public lands, the measure PG_{int} captures the effect of droughts on already settled ranchers. In contrast, the measure PG_{tot} captures the total effect of droughts on pasture expansion, including the invasion of public land. We provide additional details on the interpretation these measures under different assumptions in the Appendix.

We can observe that pasture growth is larger on private lands when we consider just the observed intensive growth in public lands, but it is larger in public lands when we consider the total growth in those lands (Table 1.1). Figure 1.6, in turn, shows the

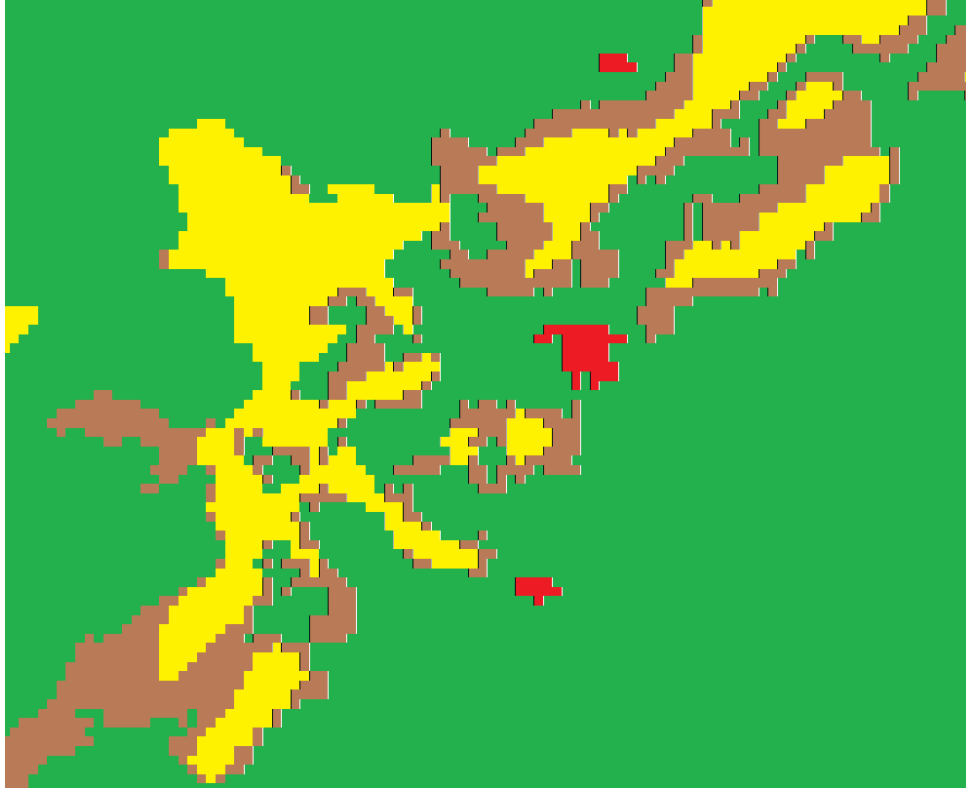


Figure 1.5: Observed Intensive and Extensive Pasture Growth. Yellow represents pasture in year $t - 1$, brown represents observed intensive pasture growth in year t , and red represents observed extensive pasture growth in year t .

average difference between public and private pasture growth rates over the studied period, considering only the observed intensive growth for public lands. Green cells observed more public land pasture growth, and pink cells observed more private land pasture growth. Generally, average private land pasture growth is more concentrated in the northern portion of the map, where average drought conditions were milder.

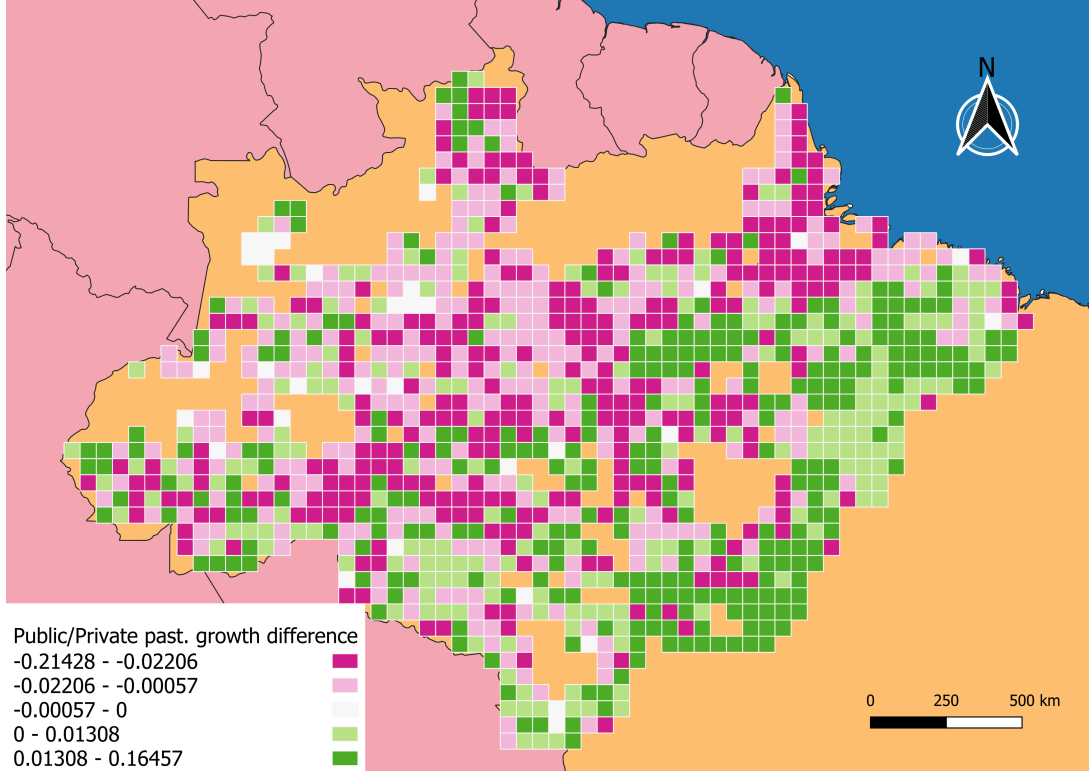
1.4.3 Estimation Strategy

We estimate the differential response of farmers to drought in public and private lands within the same cell using the following model:

$$y_{cit} = \beta_0 + \beta_1 Dry_{ct} + \beta_2 Priv_{ci} + \beta_3 Dry_{ct} \cdot Priv_{ci} + \gamma_c + \delta_t + \varepsilon_{cit}, \quad (1.1)$$

where y_{cit} denotes pasture growth in cell c , year t , and tenure i , Dry_{ct} is a dummy variable that equals to one if cell c experiences a drought in year t , and $Priv_{ci}$ is a dummy

Figure 1.6: Average Difference between Public and Private Pasture Growth Rates



Considering only observed intensive growth for public lands

variable that equals one if land tenure i in cell t is private. Cell- and year-fixed effects are represented by γ_c and δ_t , respectively. We measure pasture growth as the difference in logs of pastureland area in years t and $t - 1$ using either the intensive or total pasture-growth measures, PG_{int} and PG_{tot} . We define $Dry_{ct} = 1$ when a cell's SPEI is one standard deviation below its historical mean. Thus, we estimate whether drought shocks in year t induce pasture expansion in public and private lands, considering a cell's usual climate.

Our coefficients of interest are β_0 through β_3 . The constant β_0 captures pasture expansion on public land in non-drought years, while $\beta_0 + \beta_2$ captures pasture expansion on private land in non-drought years⁶. Thus, β_2 represents differential baseline pasture

⁶Because the constant and fixed effects do not have a unique solution, a normalizing constraint must be imposed. We follow standard practice and impose the constraint that the fixed effects sum to zero, so that the constant represents the average of all fixed effects rather than the effect for any particular reference cell or year.

Under this normalization, β_0 captures pasture expansion on public land in non-drought years evaluated at the average cell and year, while $\beta_0 + \beta_2$ captures the analogous quantity for private land. Importantly, the beta coefficients β_1 , β_2 , and β_3 are invariant to this normalization choice—they are recovered iden-

expansion between farmers on private and public land, providing a direct test of whether the “opportunity-cost-of-land” effect dominates the “probability-of-getting-evicted” effect from our conceptual model. Similarly, the coefficient β_1 indicates how farmers respond to drought years on public land, while $\beta_1 + \beta_3$ indicates how farmers respond to droughts on private lands. Thus, β_3 represents how farmers on private land respond differently to droughts than those on public land, providing a direct test of the main hypothesis from our conceptual model.

The cell fixed effects control for all factors common to private and public farms within the same cell. In essence, it allows us to compare how differently public farmers react to droughts compared to private farmers within the same cell.

We exploit quasi-experimental variation in droughts to causally identify the differential impact of droughts on public and private pasture expansion. To interpret our estimates as causal, there must be no time-varying unobserved variables correlated with the difference between public and private pasture expansion and droughts. This assumption would be violated if droughts are more (or less) likely to occur in cells with differential trends in public and private pasture expansion.

As we compare pasture growth across public and private lands within cells, we trimmed our sample to only include cells with more than one square kilometer of pasture in both public and private lands. In doing so, we avoid distortions from percentage changes in places with small baseline pastureland and ensure we are using cells with comparable public and private pastureland. Similarly, we only included cells with at least 10% of forest cover in 2001 in both land tenure types, ensuring that pastureland has not saturated an entire cell, thereby preventing pasture expansion. We cluster our standard errors to account for spatial autocorrelation, clustering neighboring cells into groups of four, equalling 100km by 100km squares. We also use Conley spatial standard errors as a robustness exercise, with the code made available by Hsiang (2010).

tically regardless of how the constant is treated—so our difference-in-differences estimates of interest remain fully interpretable.

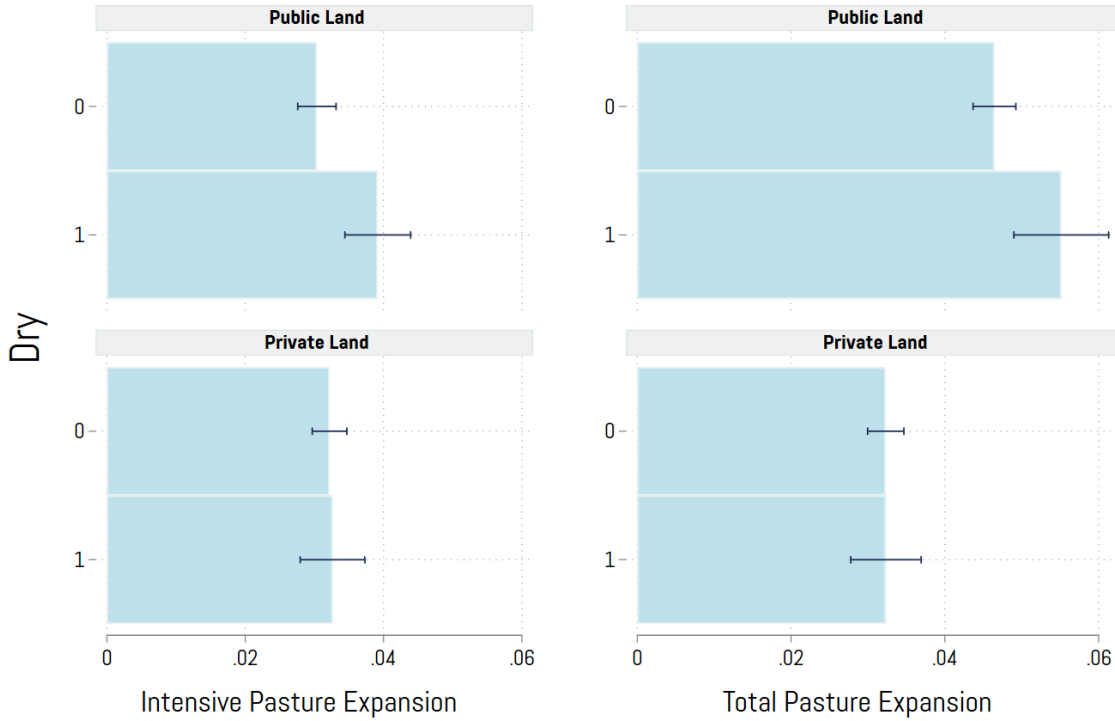
1.5 Results

Our main results are presented in Figure 1.7 and Table S1.2. We estimate that droughts increase the growth rate of intensive pasture expansion on public lands by 0.88 percentage points, an increase of roughly thirty percent over the baseline growth rate in non-drought years. In contrast, we find a small, insignificant increase of 0.04 percentage points in the growth rate of intensive pasture expansion on private lands in response to droughts. This 0.84 percentage-point difference between public and private land is statistically significant at the 1% significance level and robust across both measures of pasture growth (*PGint* and *PGtot*). The results derived from using *PGint* as our outcome variable predict a lower baseline expansion rate for public lands under no drought conditions and a larger response to dry conditions compared to the results using *PGtot*. Considering the intensive pasture expansion variable, the point estimates predict that public land farmers expand their pasture by 3.9% during droughts, compared to 3% in non-drought years. Farmers in private lands expand their pasture, on average, by 3.2% regardless of the dry conditions.⁷

Consistent with our conceptual model, these results provide evidence that farmers on public lands react more to droughts than those on private lands. However, as our conceptual model highlights, there are several possible explanations for this result, including fewer incentives to invest and manage the land sustainably, better opportunities to deforest and claim lands during droughts, or different socioeconomic characteristics across public land and private land farmers. We explore some of these mechanisms in the next section.

⁷As we discuss in the Appendix, some intensive growth on public lands may be mistakenly classified as extensive growth. In this case, the increase in pasture expansion on public lands using *PGint* is underestimated. Since expansion growth on public lands using *PGint* is larger than when using *PGtot*, it must be that both measures underestimate the pasture growth in public lands (See third line of Table S1.1). Further, since the effect of droughts on intensive expansion on public land is greater than the effect on private land, both measures also underestimate the difference in pasture expansion growth between public and private lands.

Figure 1.7: Droughts and pasture expansion



1.5.1 Mechanisms

1.5.1.1 Pasture Degradation

In this section, we explore some possible mechanisms for the observed difference in pasture expansion across public and private land farmers during drought years. First, we investigate the role of pasture degradation between droughts and farmers' land use response. Economic theory postulates that negative productivity shocks, such as droughts, increase the value of outside option activities and thus reduce investment in pastureland. However, in a context of an extensive production system with low capital input and abundant land, drought-induced pasture degradation may lead ranchers to look for new lands.

Our conceptual framework in Section 1.3 predicts that land degradation should trigger similar responses among farmers on both public and private lands. However, the model does not explicitly account for investment in pasture restoration. Since farmers on

public lands face weaker incentives to restore degraded pastures, they may respond more strongly to degradation by expanding into new areas instead. To examine these dynamics, we first analyze how droughts influence pasture degradation rates, and then assess how degradation, in turn, affects pasture expansion under each land tenure regime.

Table 1.2 shows the impacts of droughts on pasture degradation rate ⁸. Droughts increase the pasture degradation rate by 0.5 percentage points, with no different impacts across public and private land pastures. Pasture on private lands, on average, suffers 0.6 percentage points more degradation each year, which could be a result of private farms being older or public land ranchers abandoning the land faster. However, private land pasture could face more visible degradation if those farmers employ a pasture rotation system. In this case, other parts of their pasture area would also regenerate more. The second column of table 1.2 includes the pasture regeneration rate ⁹ as a control, and the results do not change significantly.

Next, we examine how pasture degradation influences pasture expansion across different land tenure types. Table 1.3 presents these results. For both the full sample and the public land subsample, the estimated effect of droughts on pasture expansion declines once we control for the degradation rate, suggesting that degradation operates as a mediating mechanism. In all samples, higher degradation rates are associated with greater

⁸The MapBiomass dataset includes data on pasture quality and classifies the pasture into three categories: good, medium, and degraded. We calculate the pasture degradation rate as

$$PDeg_{c,i,t} = \frac{p_{c,i,t}^{gm} + p_{c,i,t}^{gd} + p_{c,i,t}^{md}}{p_{c,i,t-1}^G + p_{c,i,t-1}^M}$$

where $p_{c,i,t}^{gm}$ is the pasture area classified as good in $t - 1$ and medium in t , $p_{c,i,t}^{gd}$ is the pasture area classified as good in $t - 1$ and degraded in t , $p_{c,i,t}^{md}$ is the pasture area classified as medium in $t - 1$ and degraded in t , $p_{c,i,t-1}^G$ is the total pasture area classified as good in $t - 1$, and $p_{c,i,t-1}^M$ is the total pasture area classified as medium in $t - 1$.

⁹Pasture regeneration rate is calculated analogously to the pasture degradation rate. It is calculated as

$$PReg_{c,i,t} = \frac{p_{c,i,t}^{mg} + p_{c,i,t}^{dg} + p_{c,i,t}^{dm}}{p_{c,i,t-1}^M + p_{c,i,t-1}^D}$$

where $p_{c,i,t}^{mg}$ is the pasture area classified as medium in $t - 1$ and good in t , $p_{c,i,t}^{dg}$ is the pasture area classified as degraded in $t - 1$ and good in t , $p_{c,i,t}^{dm}$ is the pasture area classified as degraded in $t - 1$ and medium in t , $p_{c,i,t-1}^M$ is the total pasture area classified as medium in $t - 1$, and $p_{c,i,t-1}^D$ is the total pasture area classified as degraded in $t - 1$.

Table 1.2: Droughts and pasture degradation

	Pasture degradation	
Dry	0.0048*** (0.0013)	0.0039*** (0.0012)
Private Land	0.0062*** (0.0011)	0.0061*** (0.0011)
Dry*Private Land	-0.0005 (0.0014)	-0.0009 (0.0014)
Constant	0.0417*** (0.0007)	0.0527*** (0.0010)
Observations	26814	26557
R ²	0.4670	0.5062
Past. Regeneration Controls	No	Yes

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

pasture expansion, indicating that ranchers tend to clear new areas when their existing pastures deteriorate.

This relationship is slightly stronger for public lands: a one-percentage-point increase in the degradation rate leads to an expansion of approximately 0.31% in pasture area on public lands, compared to 0.27% on private lands ¹⁰. These findings are consistent with the hypothesis that ranchers operating on public lands have weaker incentives to invest in pasture restoration and, consequently, rely more on expanding their grazing area when degradation occurs.

Finally, the effect of droughts on public intensive pasture expansion remains significant when we control for degradation, but it disappears when we consider the total pasture expansion. This result is evidence that there are still other mechanisms through which droughts affect intensive public pasture expansion, but other mechanisms likely affect extensive expansion in the opposite direction.

¹⁰This difference is statistically significant, as reported in Table S1.3 in the Appendix.

Table 1.3: Droughts, degradation and pasture expansion

Intensive Pasture Expansion						
	(1)	(2)	(3)	(4)	(5)	(6)
Dry	0.0046 (0.0030)	0.0032 (0.0029)	0.0092** (0.0036)	0.0074** (0.0034)	0.0001 (0.0030)	-0.0008 (0.0029)
Degrad. rate		0.3158*** (0.0259)		0.3159*** (0.0376)		0.2656*** (0.0387)
Total Pasture Expansion						
	(1)	(2)	(3)	(4)	(5)	(6)
Dry	0.0044 (0.0034)	0.0031 (0.0033)	0.0086* (0.0044)	0.0069 (0.0043)	0.0001 (0.0030)	-0.0008 (0.0029)
Degrad. rate		0.2707*** (0.0298)		0.3050*** (0.0418)		0.2656*** (0.0387)
Observations	26814	26814	13389	13389	13395	13395
Sample	Full	Full	Public	Public	Private	Private

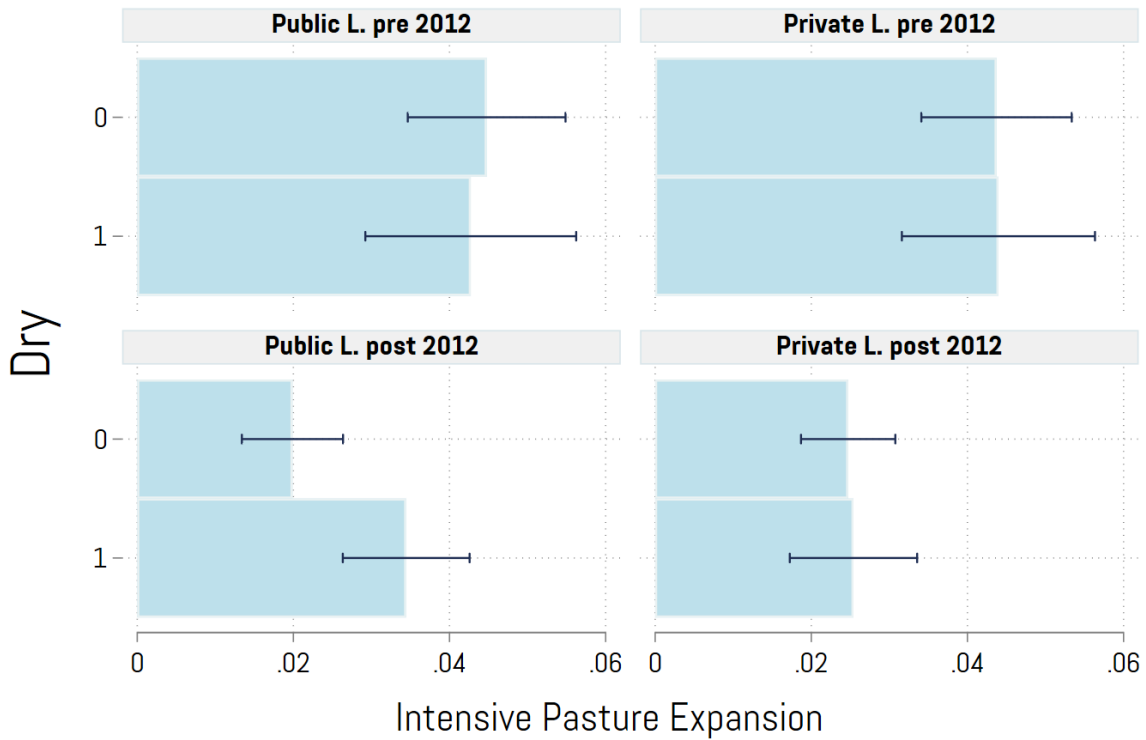
Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ **1.5.1.2 Loosening of environmental regulation**

In 2012, the Brazilian Congress enacted a new forest code that, among other measures, grandfathered in irregular properties in public lands established before 2008. A. A. Sant’Anna and Costa (2021) studied the effects of this loosening in environmental regulation and found evidence that this bail-out created incentives for farmers to violate the law in the expectation of receiving future amnesties. At the same time, Brazilian law makes it easier to acquire land tenure if the land is being used productively (L. J. Alston et al., 1999). Therefore, by making the vegetation more flammable and reducing the cost of deforestation, in the context of post-2012 loosening of environmental regulation, droughts may create the perfect opportunity for farmers in public lands to expand their productive area and their claim to the land.

We test the hypothesis that there was a break in public farmers' response to drought before and after 2012. Figure 1.8 shows the results of a regression that allows the impacts of droughts on intensive pasture expansion to differ before and after 2012. We observe that droughts have little to no effect on intensive public pasture expansion in the pre-2012 period, whereas it has a significant impact in the post-2012 period. Droughts seem to have no impact on private expansion in either period. These results are robust when we use total pasture expansion as our outcome variable. Figure S1.2 in the Appendix presents the total pasture expansion results.

Figure 1.8: Droughts and intensive pasture expansion pre and post-2012



These results are consistent with the hypothesis that the relaxation of environmental regulations after 2012 created stronger incentives for farmers on public lands to expand their properties, particularly by taking advantage of drier conditions to clear new areas. However, we cannot conclusively attribute the observed differences in responses

across periods solely to this policy change, as other factors also varied between the two periods—most notably, the frequency of droughts. The earlier period (2003–2011) experienced milder climatic conditions, with cells under drought only 17.5% of the time, whereas in the later period (2012–2022), drought frequency increased to 37%. Consequently, part of the observed shift could reflect differences in climatic variability rather than regulatory changes alone, or limited statistical power in the earlier period due to fewer drought events.

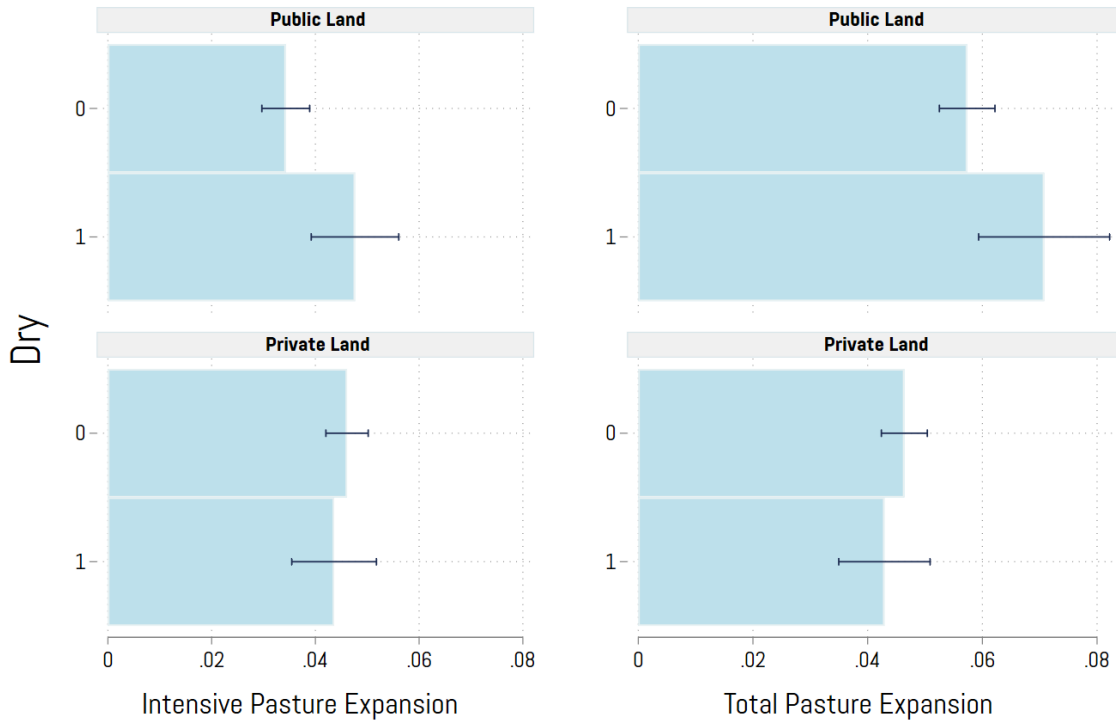
1.5.2 Spatial Heterogeneity by Humidity

We also examine whether the effect of droughts on pasture expansion differs between public and private lands across regions with distinct baseline climatic conditions. The drought variable, based on the Standardized Precipitation–Evapotranspiration Index (SPEI), measures how climate conditions deviate from the historical average for each location and month. In this section, we test whether the impact of droughts varies between relatively humid and drier regions within our sample. To characterize baseline climate conditions, we use the Climate Hazards Group InfraRed Precipitation with Stations (CHIRPS) dataset (Funk et al., 2015) to compute each cell’s average annual rainfall for the period 1981–2000. We then divide the sample into two groups—below- and above-median historical rainfall—where the median across all cells is 2,022 mm.

Using the same specification as in Equation 1.1, we analyze how farmers on public and private lands respond to droughts within each climatic subsample. Figure 1.9 reports the results for the more humid regions. The patterns closely resemble those observed in Figure 1.7, which includes the full sample. In humid areas, droughts lead to significant pasture expansion on public lands, both in terms of intensive and total expansion measures, while farmers on private lands show no discernible response to drought conditions.

The results for the drier subsample, shown in Figure 1.10, reveal a different pattern. Farmers on public lands continue to expand their pasture area during drought years; however, in these regions, private land farmers also become responsive to droughts. As a result, the difference in responses between public and private landholders is no longer statistically significant. One possible explanation is that the drier baseline conditions

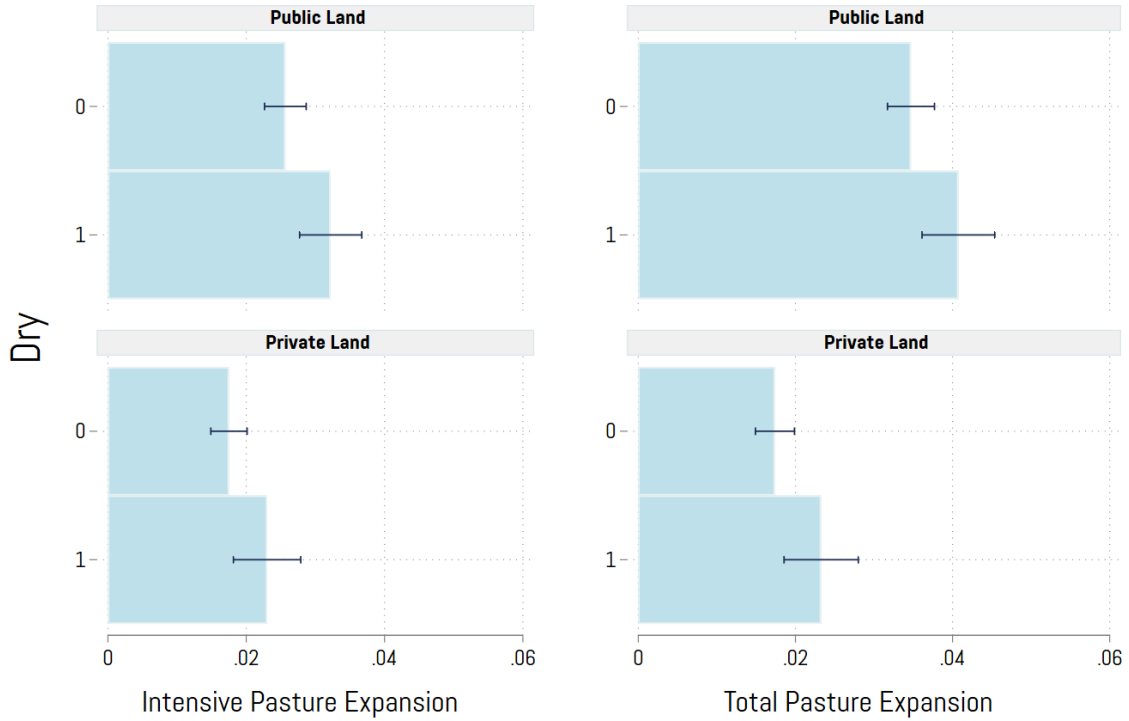
Figure 1.9: Droughts and pasture expansion in the more humid subsample



reduce incentives for private land ranchers to invest in pasture restoration. In predominantly rainfed systems, maintaining productivity under dry conditions can be costly, so even private landholders may find it more profitable to expand into new areas rather than invest in existing pastures.

Overall, the results indicate that most observed differences in private and public land ranchers' behavior come from the more humid subsample. While ranchers in public lands always respond to droughts, ranchers in private lands respond only in the drier subsample. This difference in private ranchers' behavior across the subsamples could indicate that higher rainfall levels give ranchers more incentives to invest in the land and restore degraded pastures. However, ranchers in public lands still react to droughts in the humid subsample, as the eviction possibility still discourages any investment in the land. The results indicate that the difference in responses to droughts of ranchers across both

Figure 1.10: Droughts and pasture expansion in the drier subsample



tenures also depends on the baseline climate conditions of the area.

1.5.3 Robustness Exercises

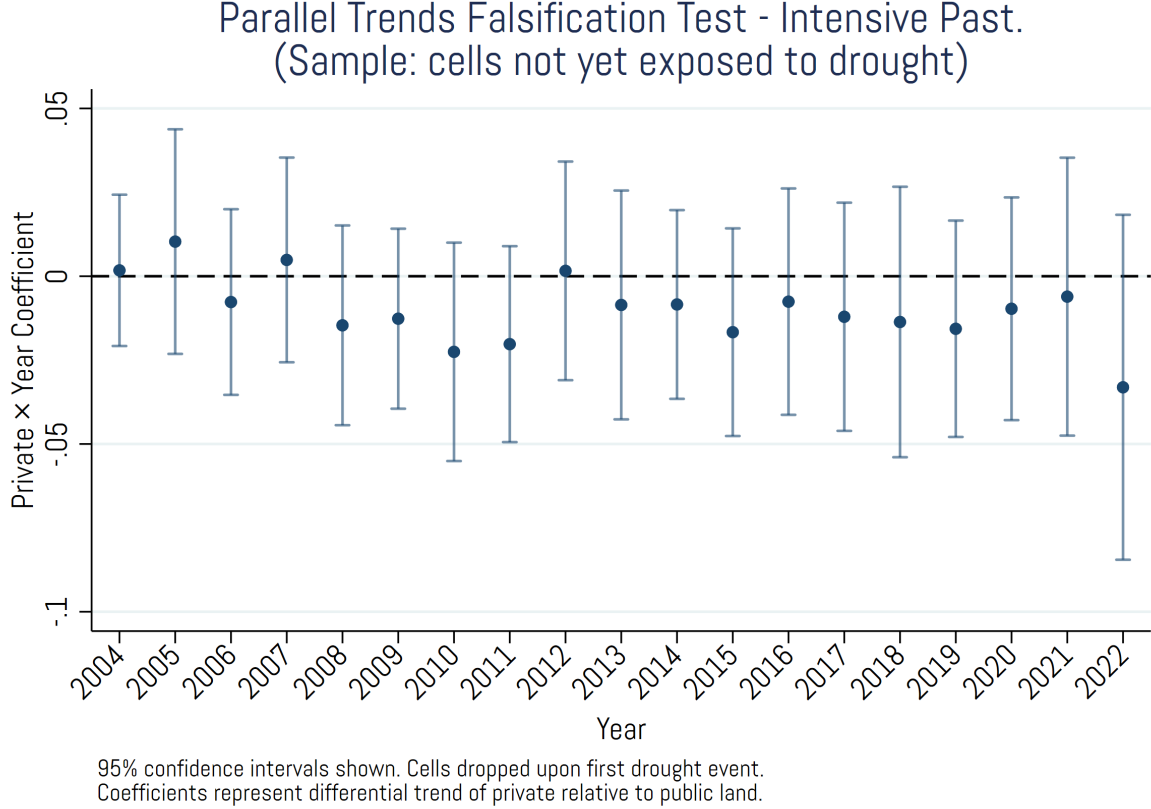
1.5.3.1 Falsification Test

We run a falsification test of our identifying assumption: absent droughts, public and private lands experience parallel growth trends. For this falsification test, we regress our pasture expansion variables on interactions of year fixed effects with a private dummy, controlling for year fixed effects and tenure, using a subsample of never-treated and yet-to-be-treated cells. We construct this subsample by dropping cells once they experience their first drought event. If pasture growth in public and private lands is not parallel absent droughts, we should observe nonzero effects of our interaction dummies.

Figure 1.11 shows the results of regressing the interaction dummies on intensive pasture growth on the never-treated and yet-to-be-treated subsample. These non-significant

results increase our confidence that our main results are not driven by factors other than droughts. The falsification test for total pasture growth also produced similar nonsignificant results, which are presented in Figure S1.1 in the Appendix.

Figure 1.11: Trends in intensive past. expansion across private and public lands in never-treated and yet-to-be treated subsample



1.5.3.2 Alternative Outcome Variable: Forest to Pasture Conversion

In this section, we test whether our results are robust using another variable as our outcome of interest. Specifically, we use the forest-to-pasture conversion as our alternative outcome variable. The MapBiomass dataset allows us to observe forested pixels in year $t - 1$ converted into pasture in year t . As before, we consider intensive forest-to-pasture conversion if the new pasture area is spatially connected with the pasture area in $t - 1$, and total forest-to-pasture conversion accounts for all observed forest-to-pasture conversion.

Table 1.4 presents the results of estimating equation 1.1 with our outcome of interest being forest to pasture conversion in square km. The results are similar for both the intensive and total forest-to-pasture conversion variables. Overall, the coefficients have the same sign as our pasture expansion specification but are measured with more noise. This noise could come from measurement errors derived from using remote sensing imagery for land use classification, as described by Alix-Garcia and Millimet (2023).

Table 1.4: Droughts and forest to pasture conversion

	Forest to Past. (Int.)	Forest to Past. (Tot.)
Dry	0.4996 (0.3272)	0.5046 (0.3727)
Private Land	3.6069*** (0.5680)	3.9726*** (0.6567)
Dry*Private Land	-0.6990* (0.4141)	-0.8734* (0.4864)
Constant	5.9229*** (0.2950)	6.8437*** (0.3390)
Observations	26814	26814
R ²	0.3723	0.3718

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

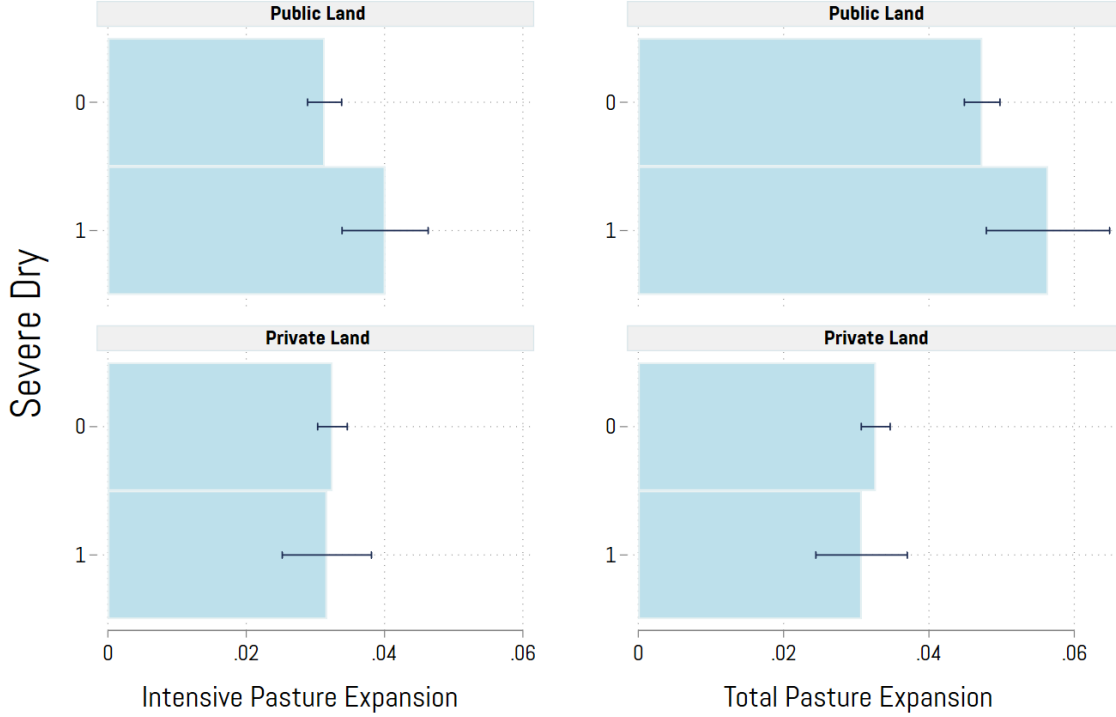
1.5.3.3 Alternative Independent Variable: Severe and Extreme Droughts

We also investigate whether our results are sensitive to drought severity. Our main specification defines a drought episode for a given cell c at time t if the SPEI index is below one standard deviation of its historical average for cell c at time t . In this section, we test the robustness of our results to this SPEI index threshold.

We define severe and extreme droughts as periods when the SPEI index falls below 1.5 and 2 standard deviations, respectively, from its historical average for cell c at time t . Overall, our findings are robust to these alternative drought definitions. Figures 1.12

and 1.13 display the results for severe and extreme droughts, respectively. Compared to the baseline results, the patterns remain largely consistent; however, Figure 1.13 shows a slight reduction in pasture expansion on private lands during extreme droughts. This difference, though, is small and not statistically significant.

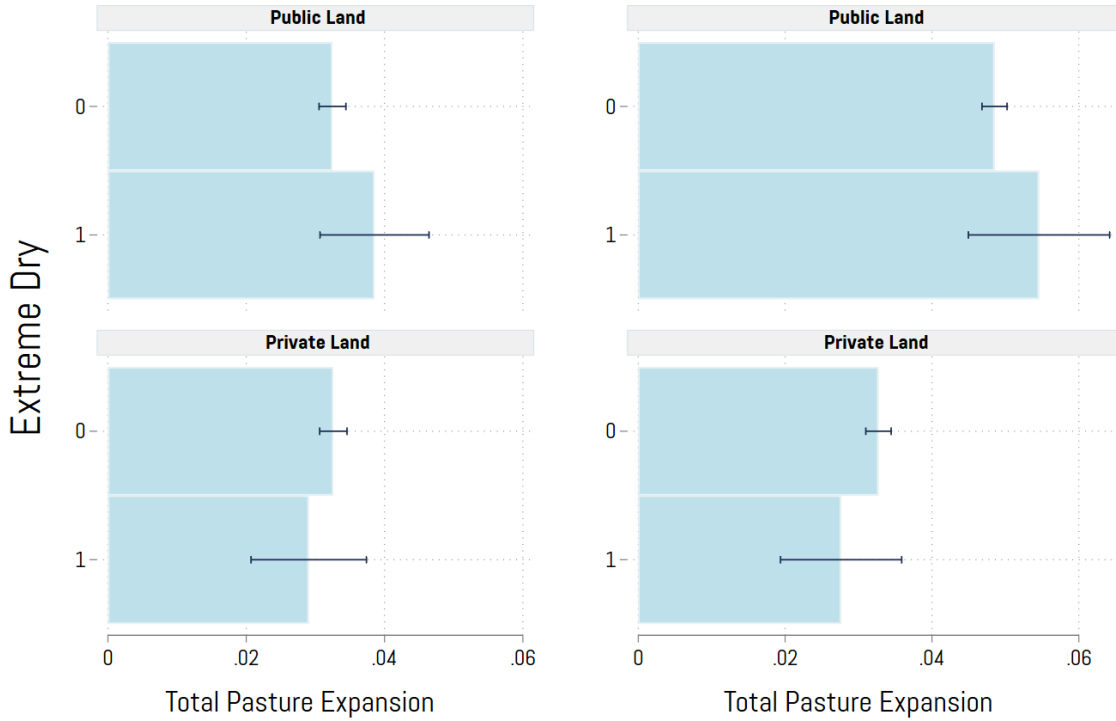
Figure 1.12: Severe Droughts and pasture expansion



1.5.3.4 Including CAR poor lands

In this section, we reintroduce the CAR poor lands into our data. As described in section 1.4.1, CAR poor lands are properties registered in CAR with more than 5% of their area overlapped with other properties. Since these properties are more likely to be invaded public lands, we introduce them in our data in the public lands category. The results, shown in Figure 1.14, remain consistent with those obtained when CAR poor lands are excluded, indicating that their inclusion does not significantly affect our findings.

Figure 1.13: Extreme Droughts and pasture expansion

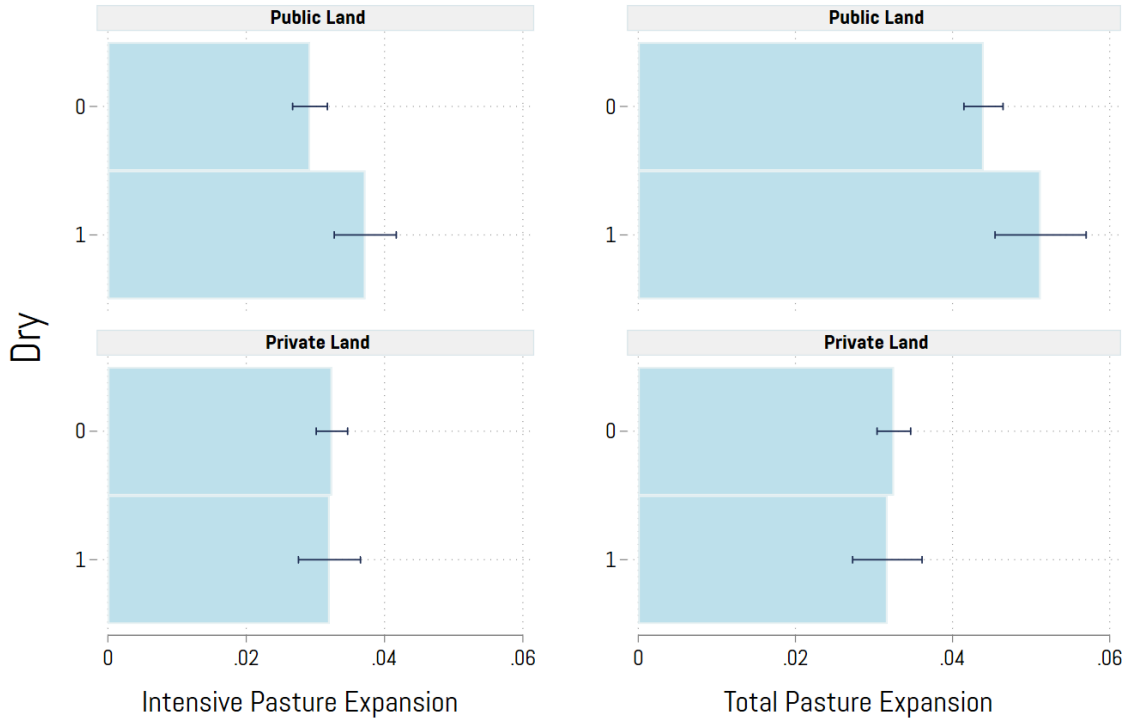


1.5.3.5 Fixed Effects

We also investigate whether our results are robust to different fixed effects specifications. Figure 1.15 shows the results of regressions that include all the combinations of cell and year-fixed effects for intensive pasture expansion. The fixed effects make the *Dry* coefficient more precisely estimated, but the *Dry*Private* interaction coefficient is stable over all combinations of fixed effects. Therefore, even if the basis effect of droughts on pasture expansion depends on the specification, the different response between private and public land farmers is robust in all specifications. These results are similar when considering the total pasture expansion, as shown in figure S1.3 in the Appendix.

We also investigate whether our results are robust to the recent DiD estimators. More specifically, we compare the de de Chaisemartin et al. (2022), henceforth CDiD, and two-way fixed effects estimators. Since our baseline model in equation 1.1 does not include unit

Figure 1.14: Droughts and pasture expansion including CAR poor lands



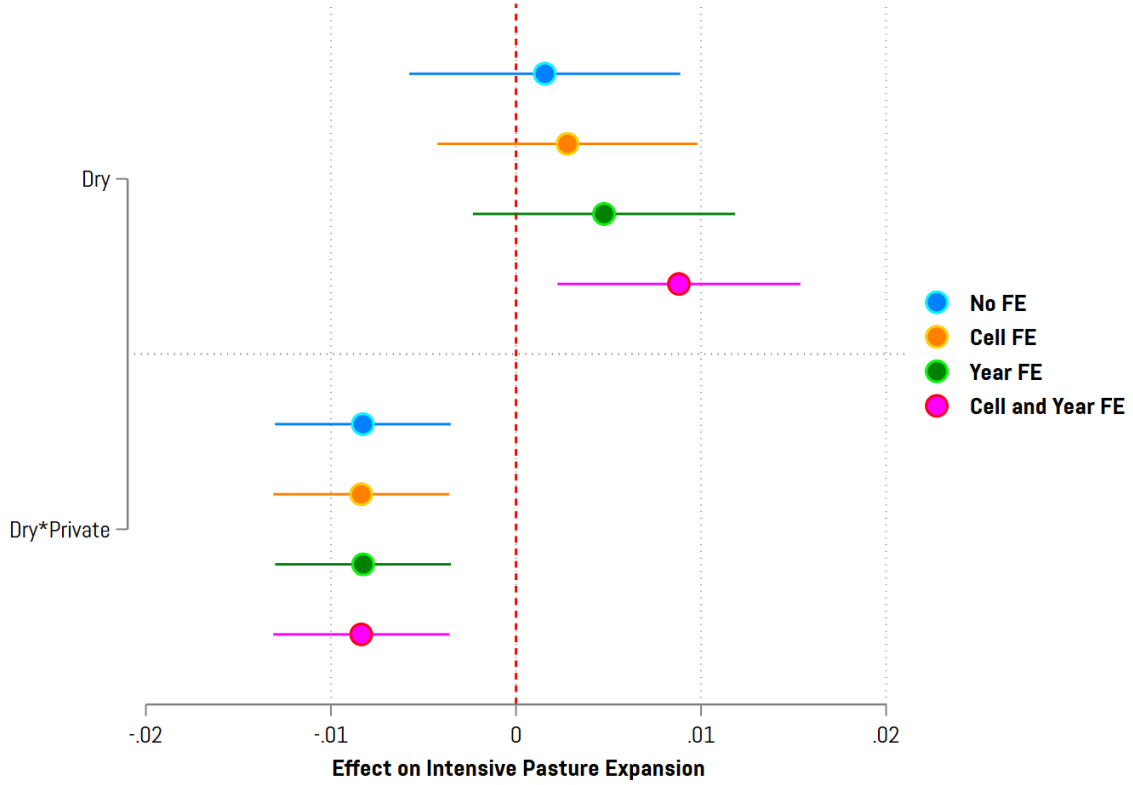
fixed effects (which would be tenure-by-cell fixed effects) and the recent DiD estimators, such as CDiD, always include unit fixed effects, we compare the CDiD estimator to the following two-way fixed effects model:

$$y_{cit} = \beta_0 + \beta_1 Dry_{ct} + \gamma_{ci} + \delta_t + \varepsilon_{cit} \quad (1.2)$$

where γ_{ci} is the unit (tenure-by-cell) fixed effect. We estimate the equation 1.2 and CDiD models for each private and public subsample.

The results are shown in table 1.5. Compared to the CDiD, the TWFE estimator underestimates the impacts of droughts on both private and public lands. However, the CDiD estimator still predicts a larger impact of droughts on public lands compared to private lands. The CDiD estimator predicts that droughts lead to a pasture expansion of 1.31% or 1.11% for the intensive and total expansion metrics used, respectively. The

Figure 1.15: Different fixed effects specifications, intensive expansion



same estimator predicts an expansion of 0.32% of pasture on private lands. It is worth noting, however, that compared to the CDiD, the TWFE estimator greatly underestimates the impacts of droughts on private lands (0.01% vs. 0.32%). Nonetheless, for both estimators, droughts' impact on private lands is not statistically different than zero at the 5% significance level. On the other hand, both estimators predict a positive and statistically significant impact of droughts on public lands pasture expansion, regardless of the metrics used.

1.5.3.6 Sample Trimming

We also investigate whether our results are driven by the specific thresholds in pasture and forest cover we use for trimming. In our original specification, we trim our sample to include cells in which both public and private lands have at least 1 km^2 of pasture in the previous year and at least 10% of forest cover in 2001. We find that changing the pasture thresholds to 3 km^2 and 5 km^2 , and the forest cover in 2001 to 15% and 20% do

Table 1.5: TWFE and de Chaisemartin et al., 2022 estimates

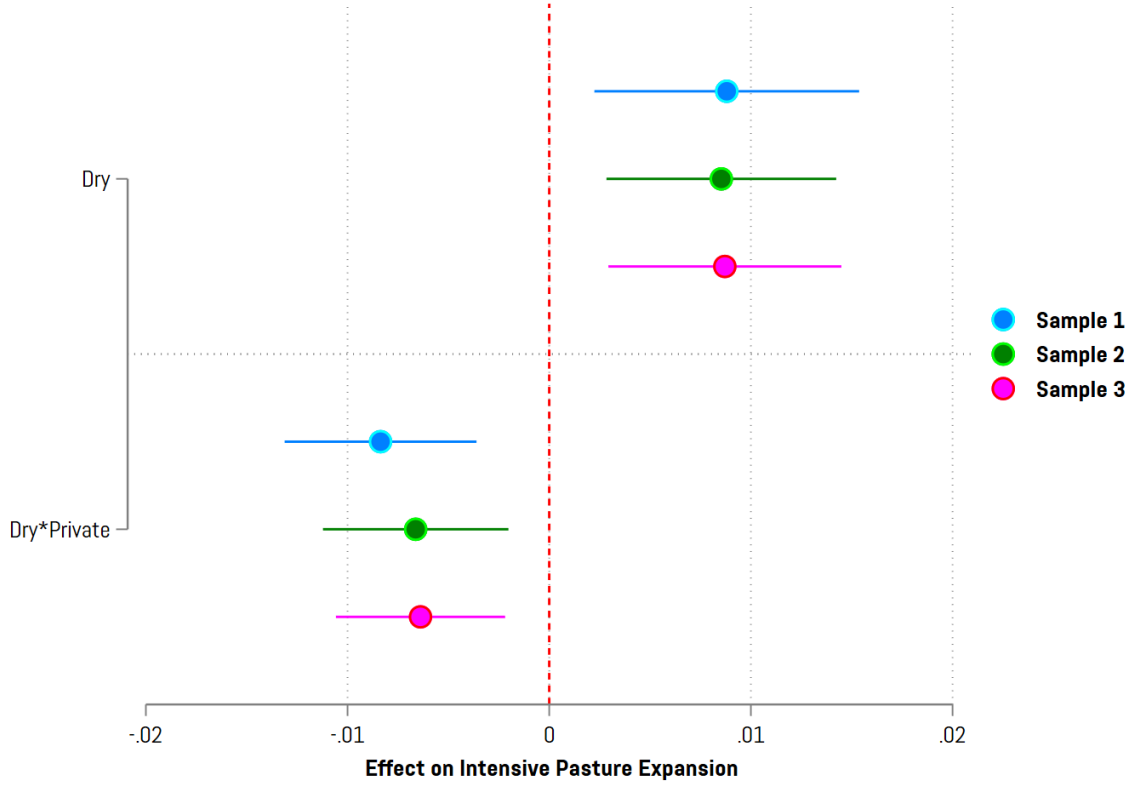
Intensive Pasture Expansion					
		Estimate	SE	LB CI	UB CI
Public	TWFE	0.0092	0.0036	0.0021	0.0162
	CDiD	0.0131	0.0042	0.0050	0.0213
Private	TWFE	0.0001	0.0030	-0.0059	0.0061
	CDiD	0.0032	0.0024	-0.0014	0.0078
Total Pasture Expansion					
		Estimate	SE	LB CI	UB CI
Public	TWFE	0.0086	0.0044	0.0000	0.0173
	CDiD	0.0111	0.0051	0.0010	0.0211
Private	TWFE	0.0001	0.0030	-0.0059	0.0061
	CDiD	0.0032	0.0030	-0.0026	0.0090

not change our results. Figure 1.16 shows the results for intensive pasture expansion. The results for total expansion, which are similar, are presented in figure S1.4 in the Appendix.

1.5.3.7 Conley Spatial Standard Errors and Weights

Considering that cells have different compositions of pasture and forests within their public and private lands, we want to compare how pasture growth responds to dry conditions in cells with a similar composition of pasture and forest across different land tenures. Here, we test whether giving greater weight to cells with similar public and private land cover compositions changes our results. Let $p_{pub} = \frac{pasture}{pasture+forest}$ in public lands, and $p_{priv} = \frac{pasture}{pasture+forest}$ in private lands. Our weights take the form $1 - |p_{pub} - p_{priv}|$, thus giving more weight to more balanced cells. Figure 1.17 shows that these weights do not change our results significantly. Figure 1.17 also shows that using Conley standard errors with a 200 km threshold does not impact our standard errors significantly. Figure S1.5 in the Appendix shows the same exercise for total expansion, with similar results.

Figure 1.16: Different trimming samples, intensive expansion

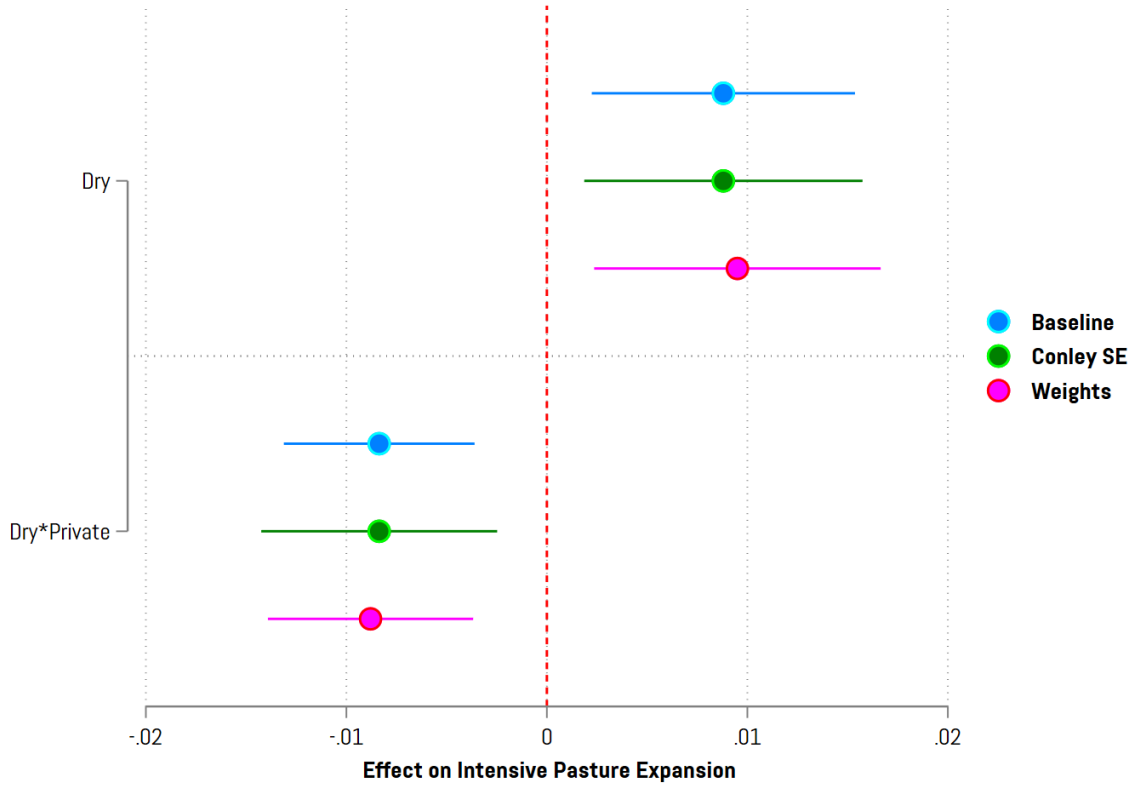


S1 corresponds to a sample in which both public and private lands within a cell have at least 1 km^2 of pasture in the previous year and at least 10% of forest cover in 2001. S2 corresponds to a sample with thresholds of 3 km^2 of pasture and 15% of forest cover in 2001. S3 corresponds to a sample with thresholds of 5 km^2 of pasture and 20% of forest cover in 2001.

1.6 Concluding Remarks

This paper investigates how farmers under different land tenure regimes respond to climate shocks. Ranchers in public lands expand their pastureland more during drought years than private land ranchers, consistent with the premise that higher eviction risk causes ranchers to discount the future and avoid investing in pasture restoration. By expanding their pastureland during droughts, farmers in public lands mitigate the immediate negative effects of climate stress by bringing forestland into use. However, the results show that these differences are largely driven by the more humid regions of the Amazon: ranchers in public lands consistently respond to droughts across all climate

Figure 1.17: Conley SE and weights, intensive expansion



zones, whereas private land ranchers do so only in the drier areas. This suggests that higher rainfall gives private landholders stronger incentives to invest in their properties and restore degraded pastures. Yet even in humid regions, ranchers in public lands still expand pasture rather than restore it, as eviction risk continues to discourage long-term investment. Investigating the mechanisms behind these results, we find that farmers in public lands respond more strongly to pasture degradation, reinforcing the theoretical argument that tenure insecurity amplifies short-term, extensification-based adaptation to drought.

We also find that the effects of droughts in public land pasture expansion remain positive and statistically significant even after controlling for pasture degradation, which is evidence that other mechanisms are at play. We investigate whether the environmental code modifications of 2012 gave farmers in public lands incentives to claim more land by expanding their productive area. We find that our results are driven by the post-2012

period. These findings are consistent with farmers in public lands using droughts as an opportunity to deforest and claim land after the environmental deregulation after 2012 and could help explain why the effect of droughts on public pasture expansion remains positive and statistically significant after controlling for pasture degradation. Nevertheless, we cannot undoubtedly attribute these post-2012 results to environmental deregulation since there are other important differences between these two periods. For instance, the pre-2012 period was characterized by milder climate conditions, whereas droughts were more frequent in the post-2012 period.

However, we must interpret these results with caution. We observe more pasture expansion in public lands during drought years, but our data does not allow us to conclude that these observed discrepancies are caused by land tenure. Farmers in public and private lands are inherently different from each other in socioeconomic terms. For instance, our results could be driven by different mitigation strategies by farmers of distinct income groups if farmers in public lands are poorer on average than those in private lands. Furthermore, our work focuses on droughts' short-term impacts across the different land tenure classes. Our results cannot tell us what the differences in deforestation across land tenure would be if the whole region became drier in the long term.

Our work presents new opportunities for future research. First, there is a need to better understand the differences between farmers in public and private lands at the farm level and how socioeconomic disparities could drive different responses to droughts. Second, future work should investigate the long-term effects of droughts on land-use decisions. A drier climate should induce land use conversion to drought-tolerant agricultural activities. However, differences in land tenure could induce diverse incentives for this long-term adaptation to a drier climate.

Overall, these results provide evidence that the increased occurrence of droughts under climate change expands the pressure on public land forests. Land use change driven by even short-term climate deviations can contribute to the erosion of the local environment. Since public lands cover more than two-thirds of the Amazon, the public forests represent a gigantic carbon stock, and their vulnerability makes it challenging to curb climate

change. Our results should guide policymakers to increase the monitoring of public lands during drought years, design mechanisms to better enforce public property rights, and impose a tax on extensive cattle ranching. However, they do not provide information that either land tenure would perform better in environmental measures under permanent drier conditions.

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Appendices

Appendix 1.1 — Measuring Intensive and Extensive Pasture Growth: Details

One practical challenge of this paper is that we do not observe the boundaries of public land farms since these ranchers are settled irregularly. To overcome this obstacle, we impose a set of assumptions that affect the interpretation of our empirical results.

To motivate this discussion, suppose we observe two ranchers, i and j . Rancher i is settled in private lands, and Rancher j in public lands. Suppose we also observe their land plots in years t and $t + 1$ and suppose the ranchers expand their pasture land from year t to $t + 1$. We call this growth in the ranchers' pasture *intensive* pasture growth. Ideally, we would like to compare both ranchers' intensive pasture growth.

Suppose now that we observe, close to rancher's j land, a newcomer rancher k in year $t + 1$. Rancher k was not settled in the area during year t and decided to create a new ranch on previously unoccupied public land in year $t + 1$. This pasture growth that comes from rancher k 's new activity we call *extensive* pasture growth.

The primary goal of this paper is to investigate how ranchers on public lands react differently to drought shocks compared to ranchers on private lands. It is important to note that this problem involves ranchers already settled in public lands when the drought hits, as farmer j ; it does not involve the problem of public land invasion by landgrabbers, as farmer k . As we cannot observe well-defined properties in public lands, we cannot directly observe ranchers j and k . That is, we cannot directly observe intensive and extensive growth in public lands. Therefore, we cannot use public and private farms as our unit of analysis and compare the land-use change across farms of different land tenures.

However, we observe land tenure, that is, whether a given area is public or private land, and we also observe land use. Let us define the observed intensive and extensive pasture growths using these two pieces of information. Define observed intensive growth in year t , $\widehat{IG}_t$, as the growth in pasture that intersects pastureland in year $t - 1$, and extensive growth in year t , $\widehat{EG}_t$, as the growth in pastureland that does not intersect with pastureland in year $t - 1$ (Figure 1.5).

Based on these definitions, let us define the following two estimators of intensive pasture growth:

$$\widehat{PGint}_{ts} = 1(s = pub) \cdot (\widehat{IG}_{ts}) + 1(s = priv) \cdot (\widehat{IG}_{ts} + \widehat{EG}_{ts}) \quad (S1.1)$$

$$\widehat{PGtot}_{ts} = 1(s = pub) \cdot (\widehat{IG}_{ts} + \widehat{EG}_{ts}) + 1(s = priv) \cdot (\widehat{IG}_{ts} + \widehat{EG}_{ts}) \quad (S1.2)$$

where s indicates if the land tenure is public or private.

The variable $\widehat{PGint}_{ts}$ considers only the observed intensive growth as the actual intensive growth in public lands. Assuming all private lands are already settled, all pasture growth in these lands is intensive growth. For private lands, both observed intensive and extensive growth are considered the actual intensive growth. The underlying assumption is that all observed extensive growth in public lands comes from new ranchers invading public lands.

Analogously, the variable $\widehat{PGtot}_{ts}$ considers that all observed growth is intensive for both land tenures. The underlying assumption in this case is that the number of ranchers in public lands is fixed, and there are no newcomers. The assumption for private lands is the same as before.

Finally, considering that we want to use $\widehat{PGint}_{ts}$ and $\widehat{PGtot}_{ts}$ to estimate the impacts of droughts on intensive public and private pasture growth, it is important to understand the implications for our estimations when the assumptions above are not met. More precisely, we would like to use these variables to estimate the impacts of droughts on the pasture growth difference between public and private lands. Keeping the assumption that all growth in private lands is intensive, we assume all growth in private lands is estimated correctly since these variables capture the same growth in private lands. Let us now consider three cases for public lands: all observed extensive growth is extensive, all observed extensive growth is intensive, and finally, some but not all observed extensive growth is intensive. Let us also consider b_{pub} , b_{priv} , and b_{EG} the effect of droughts on intensive public land pasture growth, private land pasture growth, and extensive public

land pasture growth, respectively. We analyze how changing these three assumptions affects the estimation of b_{pub} and $b_{priv} - b_{pub}$ if we use $\widehat{PGint_{ts}}$ and $\widehat{PGtot_{ts}}$ as our pasture growth variables.

Let us consider the first case. If all observed extensive growth is extensive, then the variable $\widehat{PGint_{ts}}$ precisely captures the intensive pasture growth on public lands. Therefore, b_{pub} and $b_{priv} - b_{pub}$ are precisely estimated since these different assumptions do not affect the estimation of b_{priv} .

However, in this case, $\widehat{PGtot_{ts}}$ includes as intensive growth in public lands some extensive growth. The implications for the estimation bias of b_{pub} depend on the signs of b_{pub} and b_{EG} , and the bias of $b_{priv} - b_{pub}$ depend on the signs of b_{pub} and b_{EG} , and also whether $b_{priv} > b_{pub}$ or $b_{priv} < b_{pub}$. Consider b_{pub}^* and $b_{priv-b_{pub}}^*$ the respective estimator of each coefficient. The bias in each one of these cases is summarized on the right-hand side of table S1.1. First, consider that both b_{pub} and b_{EG} are negative. Since $\widehat{PGtot_{ts}}$ adds the extensive growth to the intensive growth, both negative effects compound, and the negative effect of droughts on intensive public land pasture is overestimated, i.e., $b_{pub}^* < b_{pub} < 0$. In this case, if $b_{priv} > b_{pub}$, then $b_{priv} - b_{pub}^* > b_{priv} - b_{pub} > 0$ and our estimator overestimates the positive difference between private and public land pasture growth. However, if $b_{priv} < b_{pub}$, then $b_{priv} - b_{pub} < b_{priv} - b_{pub}^* < 0$, and our estimator underestimates the negative difference between private and public growth.

Consider now that b_{pub} is negative, but b_{EG} is positive. Since $\widehat{PGint_{ts}}$ includes extensive pasture growth in its composition, b_{EG} reduces b_{pub} negative effect and b_{pub}^* is underestimated. If $b_{priv} > b_{pub}$, then $b_{priv} - b_{pub} > b_{priv} - b_{pub}^* > 0$ and our estimator underestimates the positive difference between private and public land pasture growth. However, if $b_{priv} < b_{pub}$, then $b_{priv} - b_{pub}^* < b_{priv} - b_{pub} < 0$, and our estimator overestimates the negative difference between private and public growth.

We can apply the same logic to the other cases. If $b_{pub} > 0$ and $b_{EG} \leq 0$, b_{pub}^* is underestimated and $b_{priv} - b_{pub}^*$ is overestimated and underestimated if $b_{priv} > b_{pub}$ and $b_{priv} < b_{pub}$, respectively. If $b_{pub} > 0$ and $b_{EG} > 0$, then b_{pub}^* is overestimated. Now, $b_{priv}^* - b_{pub}^*$ will be underestimated if $b_{priv} > b_{pub}$ and overestimated if $b_{priv} < b_{pub}$.

Let us now consider the second case in which all observed extensive growth is actually intensive growth. Now, the variable $\widehat{PGtot}_{ts}$ precisely captures all the intensive growth, and the estimators derived from it should be unbiased. However, now $\widehat{PGint}_{ts}$ fails to consider the change in intensive pasture growth that we observe as extensive. Therefore, b_{pub}^* will underestimate b_{pub} regardless of the sign. The bias in this case is summarized on the left-hand side of table S1.1.

The bias of $b_{priv}^* - b_{pub}^*$ depends on the sign of b_{pub} and whether $b_{priv} > b_{pub}$ or $b_{priv} < b_{pub}$. If $b_{pub} < 0$ and $b_{priv} > b_{pub}$, then $0 < b_{priv} - b_{pub}^* < b_{priv} - b_{pub}$, and $b_{priv}^* - b_{pub}^*$ is underestimated. If $b_{priv} < b_{pub}$, then $b_{priv} - b_{pub}^* < b_{priv} - b_{pub} < 0$ and $b_{priv}^* - b_{pub}^*$ is overestimated. Analogously, $b_{priv}^* - b_{pub}^*$ will be overestimated if $b_{priv} < b_{pub}$ and underestimated if $b_{priv} > b_{pub}$, considering that $b_{pub} > 0$.

Finally, considering that a fraction, $0 < \alpha < 1$, of all observed extensive pasture growth in public lands is actually intensive growth, then both variables fail to capture the correct intensive growth. The same cases discussed before apply and the bias is summarized in table S1.1. In sum, the estimators derived from $\widehat{PGint}_{ts}$ and $\widehat{PGtot}_{ts}$ can both simultaneously underestimate or overestimate the actual effects b_{pub}^* and $b_{priv}^* - b_{pub}^*$, therefore, together they do not represent upper and lower bounds. The estimator derived from $\widehat{PGint}_{ts}$ will always underestimate b_{pub}^* if $\alpha > 0$, and the estimator derived from $\widehat{PGtot}_{ts}$ may overestimate or underestimate it depending on the sign of b_{EG} . The bias of $b_{priv}^* - b_{pub}^*$ depend on whether $b_{priv} < b_{pub}$, and on the sign of b_{EG} in the case of $\widehat{PGtot}_{ts}$. Ultimately, comparing the sign and magnitude of b_{pub}^* and $b_{priv}^* - b_{pub}^*$ derived from both variables helps us shed light on which case is more likely to be accurate.

Regardless of the assumptions, the variable $\widehat{PGtot}_{ts}$ captures the overall difference in pasture growth across public and private lands. It may shed light on how each type of tenure incentivizes land use conversion regardless of the type and number of farmers in each type of land. Even though this last variable may not perfectly capture how farmers settled in both types of land respond to climate shocks, it may still contribute to important policy implications.

Table S1.1: Intensive pasture growth estimators and estimation bias

$\widehat{PGint}$ if $\alpha > 0$						$\widehat{PGtot}$ if $\alpha < 1$			
b_{pub}^*			$b_{priv}^* - b_{pub}^*$			b_{pub}^*		$b_{priv}^* - b_{pub}^*$	
$b_{priv} > b_{pub}$			$b_{priv} < b_{pub}$			$b_{priv} > b_{pub}$		$b_{priv} < b_{pub}$	
b_{pub}	< 0	U-	U+	O-	b_{EG}	< 0	O-	O+	U-
						≥ 0	U-	U+	O-
b_{pub}	> 0	U+	O+	U-	b_{EG}	≤ 0	U+	O+	U-
						> 0	O+	U+	O-

U represents an underestimated coefficient and O represents an overestimated coefficient. The signs represent whether the coefficient is positive or negative, ie, U- represents an underestimation of a negative coefficient, and O+ represents an overestimation of a positive coefficient

Appendix 1.2 — Supplementary Tables

Table S1.2: Droughts and pasture expansion

	Intensive Past.	Total Past.
Dry	0.0088*** (0.0033)	0.0087** (0.0042)
Private Land	0.0018 (0.0021)	-0.0141*** (0.0017)
Dry*Private Land	-0.0084*** (0.0024)	-0.0087*** (0.0030)
Constant	0.0304*** (0.0014)	0.0464*** (0.0014)
Observations	26814	26814
R ²	0.2242	0.2296

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table S1.3: Droughts and pasture degradation

	Intensive Past.	Total Past.
Degrad. rate	0.3942*** (0.0309)	0.3420*** (0.0342)
Private Land	0.0049** (0.0023)	-0.0134*** (0.0022)
Degrad. rate*Priv. Land	-0.1605*** (0.0387)	-0.1089*** (0.0392)
Constant	0.0160*** (0.0016)	0.0342*** (0.0016)
Observations	26814	26814
R ²	0.2375	0.2380

Standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix 1.3 — Supplementary Figures

Figure S1.1: Trends in total past. expansion across private and public lands in never-treated and yet-to-be treated subsample

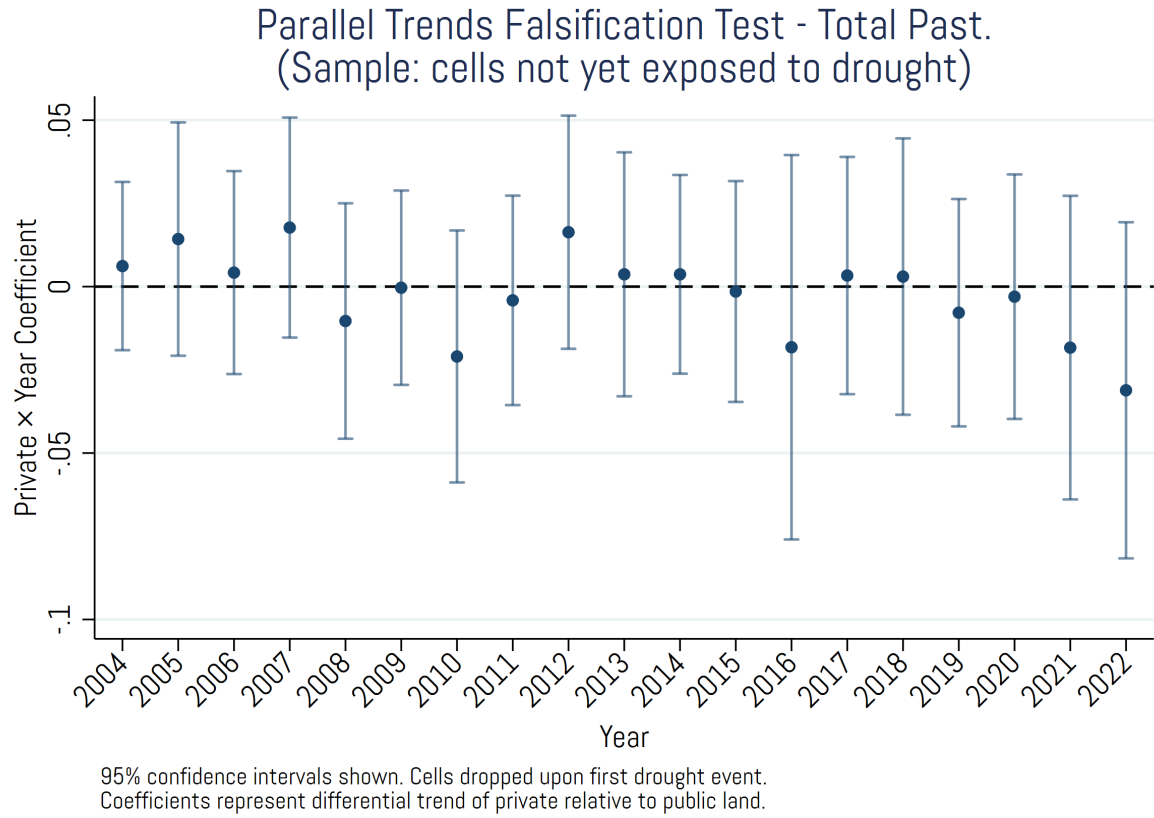


Figure S1.2: Droughts and total pasture expansion pre and post-2012

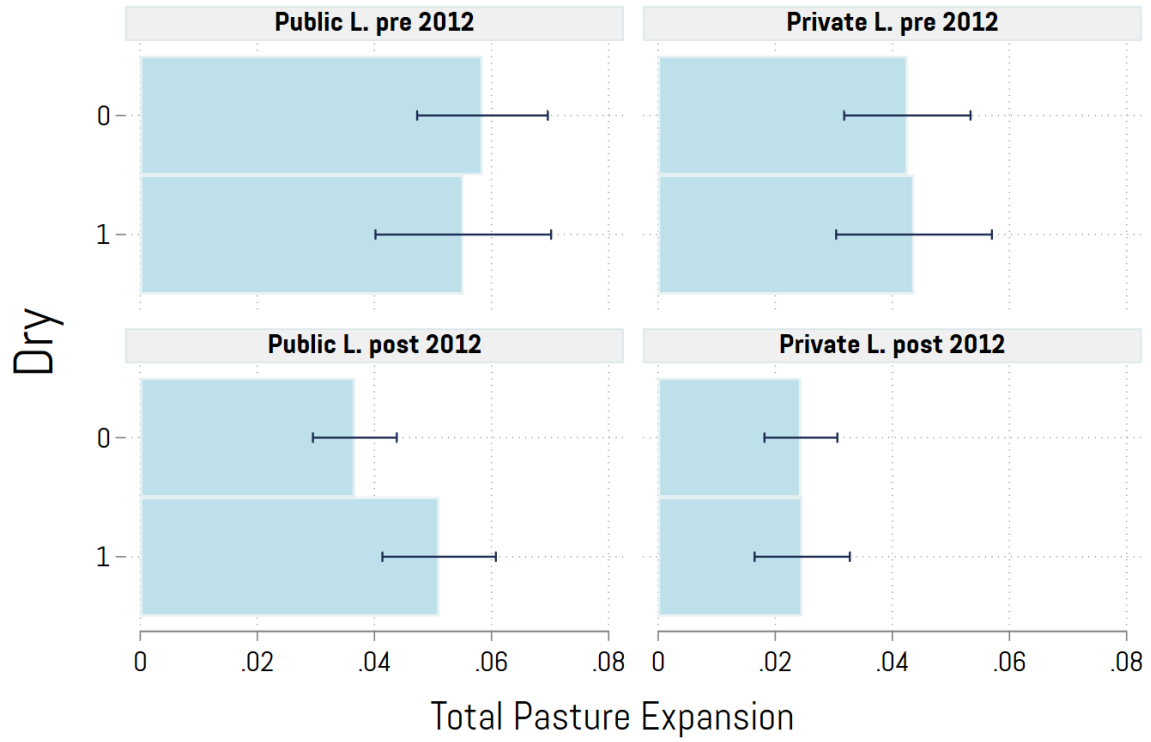


Figure S1.3: Different fixed effects specifications, total pasture expansion

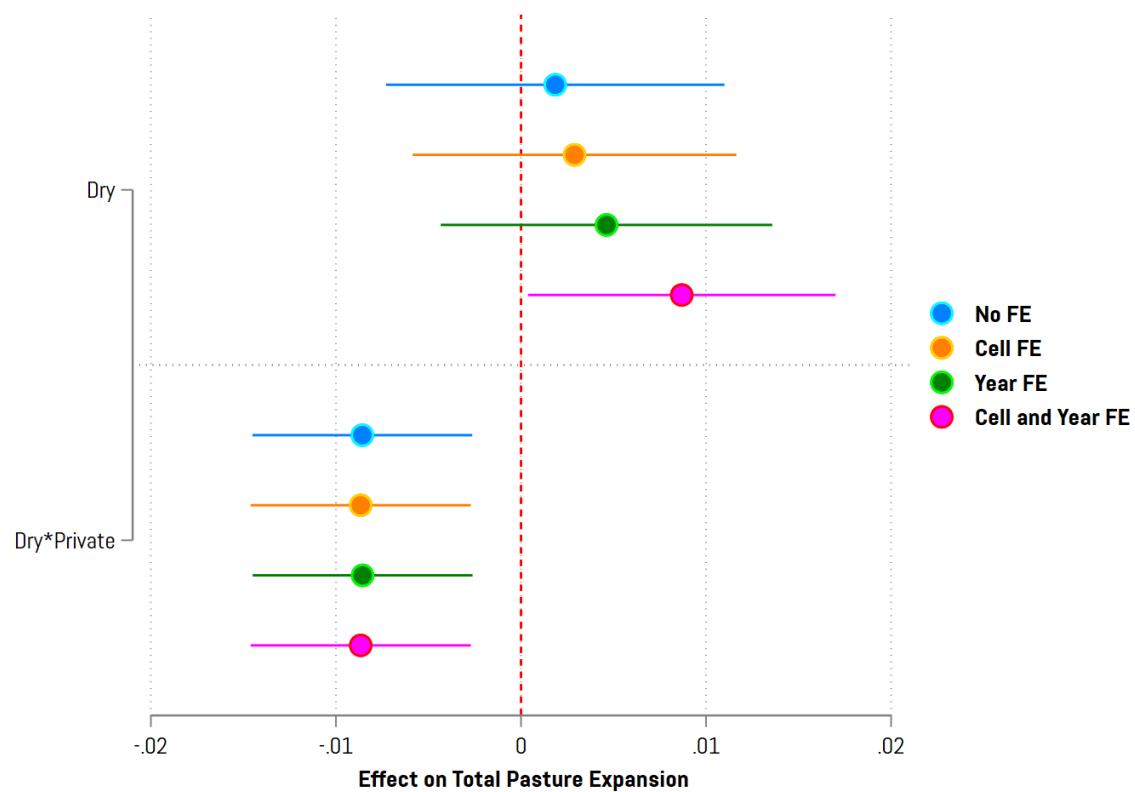
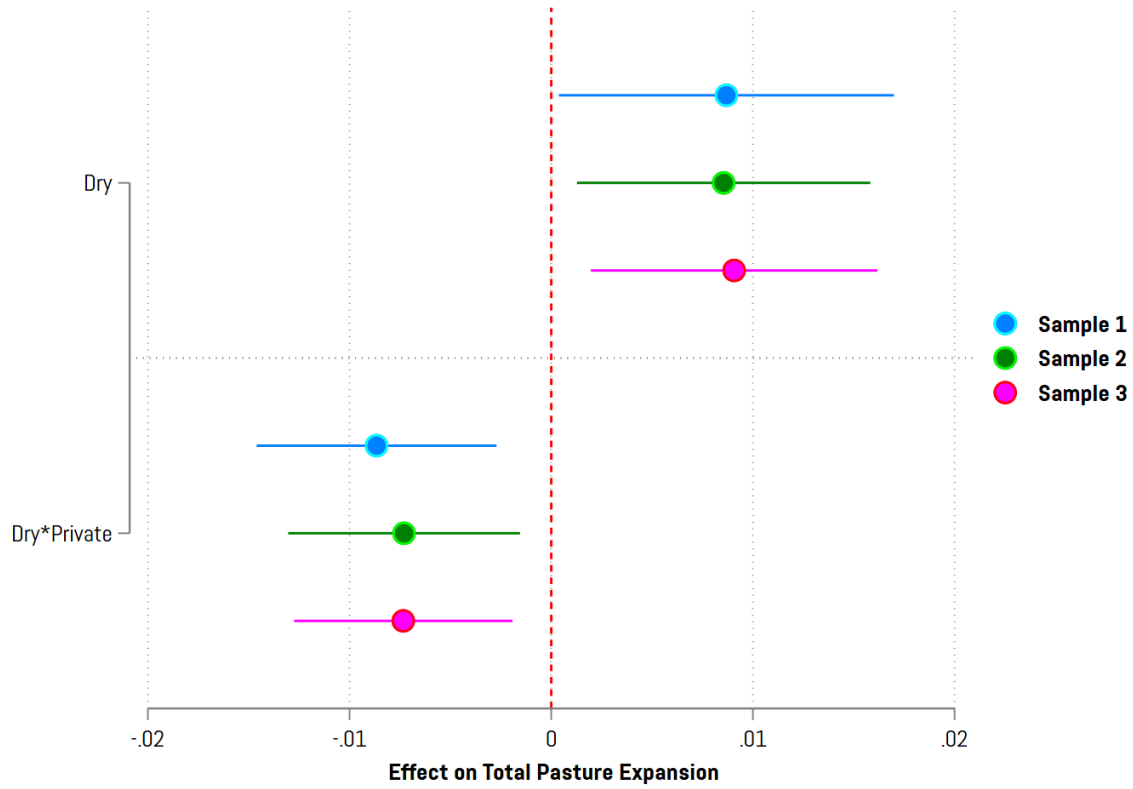
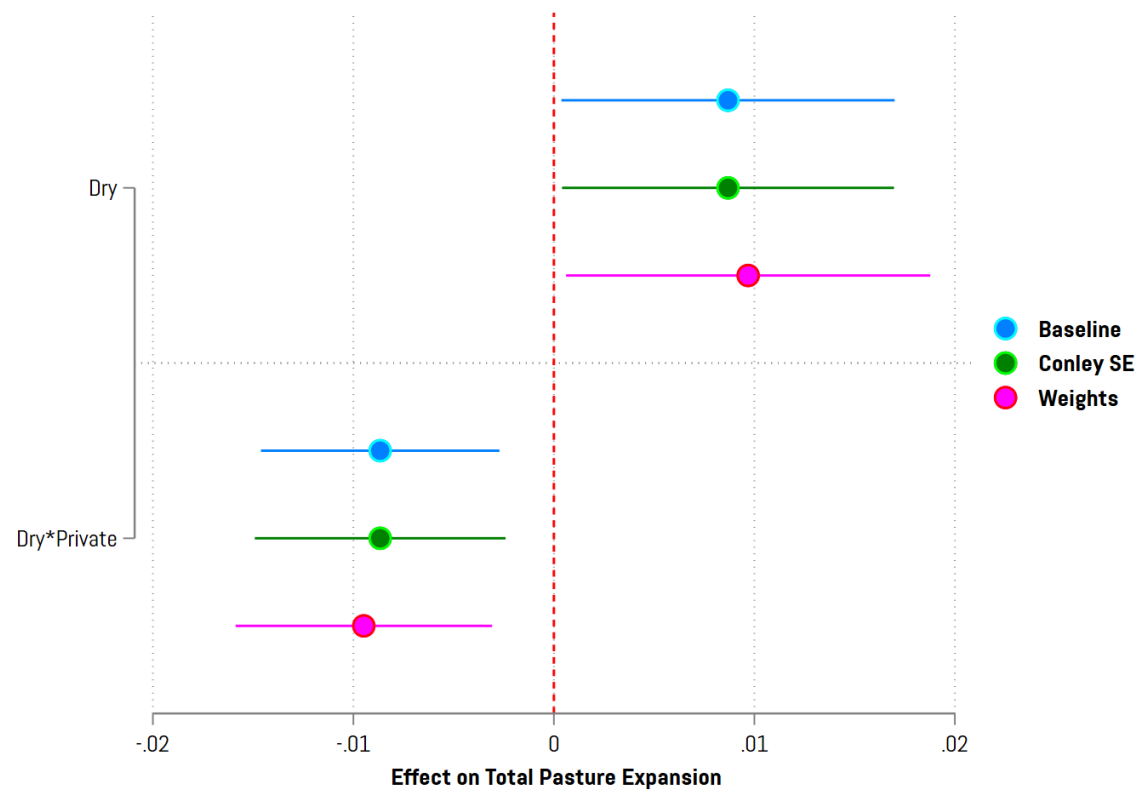


Figure S1.4: Different trimming samples, total expansion



S1 corresponds to a sample in which both public and private lands within a cell have at least 1 km^2 of pasture in the previous year and at least 10% of forest cover in 2001. S2 corresponds to a sample with thresholds of 3 km^2 of pasture and 15% of forest cover in 2001. S3 corresponds to a sample with thresholds of 5 km^2 of pasture and 20% of forest cover in 2001.

Figure S1.5: Conley SE and weights, total expansion



NEWS SOURCES

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Chapter 2

The Effects of the Amazon Soy Moratorium on Cattle Ranchers' Land Use Decisions

Bruno Pimenta

Abstract. *The Amazon Soy Moratorium (ASM) has been widely recognized as a successful private-sector initiative to curb deforestation. Estimates suggest the moratorium prevented up to 1.8 million hectares of deforestation — a figure that, given the small scale of direct forest-to-soy conversion, could only be explained by an indirect effect: the moratorium discouraging cattle ranchers from clearing forest speculatively in anticipation of future land sales to soy producers. We investigate this mechanism by testing whether the moratorium disrupted the pasture-soy land market, using a difference-in-differences design that compares pasture-to-soy conversion on unregulated and regulated lands before and after the moratorium's introduction. We find no evidence of increased conversion on unregulated land — and in fact observe a decline. While this null result reduces concerns about within-Amazon leakage, it also sits in tension with the large effects estimated in the literature: a moratorium strong enough to deter speculative clearing at scale should have generated detectable reallocation pressure on unregulated land. A key challenge is that the 2006–2007 soy price crash coincides with the moratorium's introduction, preventing us from*

cleanly separating policy effects from the contemporaneous demand collapse.

2.1 Introduction

In the 2000s, following the introduction of genetically engineered (GE) soy in Brazil, soybean cultivation rapidly expanded across the country, including the Amazon. A Greenpeace report linking Amazon deforestation to major commodity traders prompted soy producers and traders to sign an agreement banning the purchase of soy from newly deforested areas — an initiative that became known as the Amazon Soy Moratorium (ASM). Originally established in 2006, the moratorium’s signatories agreed not to buy soy from land deforested after July of that year. The moratorium was renewed periodically and, in 2016, extended indefinitely (Gollnow et al., 2018; Heilmayr, Rausch, et al., 2020). The ASM aimed to eliminate deforestation from the soy supply chain by restricting purchases to only those lands cleared before the cutoff date.

The ASM has been widely recognized — both in academic literature and media coverage — as a landmark private sector agreement that contributed to reducing deforestation, with several studies suggesting it effectively curtailed soy-driven forest loss in the Amazon (Gibbs et al., 2015; Heilmayr, Rausch, et al., 2020; Macedo et al., 2012; Zu Ermgassen et al., 2020). Nonetheless, its scope — limited to a single commodity (soy) and a single biome (the Amazon) — raised concerns about overall effectiveness and the potential for leakage (Gollnow et al., 2018; Macedo et al., 2012; Meyfroidt et al., 2014; Villoria et al., 2022). One commonly cited leakage channel is the displacement of soy expansion to the Cerrado, a neighboring biome not covered by the moratorium (Gollnow et al., 2018; Heilmayr, Rausch, et al., 2020; Macedo et al., 2012; Meyfroidt et al., 2014; Zu Ermgassen et al., 2020).

A second channel concerns cattle ranching — the Amazon’s most prominent driver of deforestation (Mapbiomas, 2024; Tyukavina et al., 2017). The land market in the region is deeply integrated: the vast majority of soy expansion has occurred on previously cleared pastureland (Carreira et al., 2024; Macedo et al., 2012; Meyfroidt et al., 2014; Richards, 2015), linking soybean farmers’ land-use decisions directly to those of cattle ranchers. One important implication is that cattle ranchers have historically cleared forest speculatively

— not only to expand grazing, but in anticipation of eventually selling pastureland to soybean producers (Heilmayr, Rausch, et al., 2020; J. Miranda et al., 2019).

By designating lands deforested after July 2006 as ineligible for soy production, the ASM may have altered cattle ranchers’ land-use calculus. To the extent that ranchers internalized this constraint, anticipating that newly cleared land could not be sold to soy farmers, the moratorium may have compounded its direct effect through an indirect channel: reducing deforestation by cattle ranchers who would otherwise have cleared forest in anticipation of future land sales. This, in turn, may have redirected ranchers toward intensifying cattle production on existing pastureland rather than expanding into forest. Alternatively, if soy expanded into unregulated pastures, ranchers may have deforested areas to replace grazing land, undermining the policy’s gains.

A study by Heilmayr, Rausch, et al. (2020) estimates that the ASM prevented 1.8 million hectares of deforestation over a decade. The study employs a triple-differences strategy, comparing the evolution of deforestation in soy-suitable and soy-unsuitable lands across the Amazon and the Cerrado¹, before and after the ASM’s implementation in the Amazon. We argue that the only way the ASM could have prevented deforestation at this scale is by preventing speculative clearings. Understanding why requires confronting a basic accounting fact. Direct forest-to-soy conversion in the Amazon represented only 16,000 hectares in 2005, after peaking at 68,000 hectares in 2003 (Mapbiomas, 2024). Even under the assumption that the moratorium prevented all such conversion, cumulative direct savings over a decade would be on the order of 160,000 hectares — less than 10% of Heilmayr, Rausch, et al. (2020)’s central estimate of 1.8 million hectares.² If the ASM’s estimated impact is real, most of it must have operated through indirect channels — specifically, by altering the land-use decisions of cattle ranchers, who account for the vast majority of deforestation in the Amazon. If so, it would suggest that incomplete policies can generate surprisingly wide effects when the markets they target are deeply integrated with adjacent ones.

¹A neighboring biome outside the ASM’s jurisdiction.

²Considering 2005’s estimate, which assumes direct soy deforestation would have remained constant. In fact, deforestation fell rapidly in the latter half of the 2000s for various reasons, as explained in Section 2.2, so keeping soy deforestation constant and attributing its effect to the ASM represents an upper bound

This study investigates whether the ASM disrupted the pasture-soy land market — the channel through which any large-scale effect on deforestation would have had to operate, given that direct forest-to-soy conversion accounted for only a small fraction of Amazon deforestation at the time of the moratorium’s introduction. We test whether the moratorium displaced soy expansion toward unregulated pastureland within the Amazon — a form of regulatory arbitrage distinct from the cross-biome displacement studied elsewhere — and find no evidence of such displacement. Rather than simply reducing leakage concerns, this null result sits in tension with the large deforestation reductions estimated in the literature: a moratorium strong enough to deter speculative clearing at scale should have generated detectable reallocation pressure on unregulated land. That we find no evidence of soy displacement toward unregulated land — and in fact observe a decline in conversion — makes the land market channel, and with it the scale of the estimated effects, harder to sustain. A key limitation is that the 2006–2007 soy price crash coincides with the moratorium’s introduction, and our design cannot cleanly separate the two, leaving open the possibility that policy-induced disruption is masked by the contemporaneous demand collapse.

To investigate these questions, we track land-use transitions at the pasture-plot level using MapBiomas satellite data, exploiting variation across pasture vintages — cohorts of land defined by the year they were first converted from forest to pasture. This vintage structure maps naturally onto the moratorium’s regulatory logic: plots cleared after July 2006 are *regulated* and ineligible for soy production under the ASM, while older plots are *unregulated* and freely convertible. To study whether the moratorium induced leakage by displacing soy expansion toward unregulated lands, we compare pasture-to-soy conversion rates in unregulated vintages before and after the moratorium’s implementation, controlling for vintage and age.

Our findings reveal that soy expansion was not displaced toward unregulated pastureland following the ASM. We find no evidence of an increase in cumulative pasture-to-soy conversion on unregulated cohorts, and the age-specific hazard rate of conversion actually declined following implementation. This decline is more consistent with a market-wide

contraction in soy demand than with policy-induced reallocation — a key empirical challenge is that the 2006–2007 soy price crash coincides almost exactly with the moratorium’s introduction, and our design cannot simultaneously absorb year-level shocks, making it difficult to cleanly attribute the leakage null to the moratorium itself. Nonetheless, this absence of leakage reduces concerns that the moratorium triggered compensatory deforestation through regulatory arbitrage within the Amazon.

Separately, a simple accounting exercise constrains where the moratorium’s estimated effects could have come from: since direct forest-to-soy conversion never exceeded 68,000 hectares annually and represented only 16,000 hectares by 2005, even perfect enforcement could not account for more than a fraction of the 1.8 million hectares of prevented deforestation estimated by Heilmayr, Rausch, et al. (2020). The bulk of that reduction must therefore have operated through cattle ranchers’ speculative clearing behavior — the indirect channel through which the moratorium’s large-scale effects would have occurred.

This study contributes to two branches of literature. One branch of literature examines the effectiveness of voluntary supply chain agreements in curbing deforestation. Kastens et al. (2017) finds that forest to soy conversion decreased from 9.4% of total deforestation to 3.9% in the state of Mato Grosso after the moratorium. Heilmayr, Rausch, et al. (2020) uses a triple differences framework to find the causal impacts of the ASM on deforestation. The study finds that deforestation fell more in soy-suitable regions of the Amazon than in less suitable regions, and that the difference between soy-suitable and unsuitable regions of the Cerrado was greater. The authors estimate that the ASM prevented 1.8 millions of hectares of deforestation—a large estimate, given that the direct forest-to-soy conversion never accounted for more than 5% of total deforestation (Mapbiomas, 2024). We contribute to this literature by providing a novel accounting decomposition that constrains the channels through which the ASM’s estimated impact could have operated — showing that the scale of effects documented by Heilmayr et al. (2020) could not have been achieved through direct soy restrictions alone, and must therefore have operated through cattle ranchers’ clearing behavior.

Another branch of literature examines leakage from zero-deforestation supply agree-

ments. Studies of cross-biome leakage find mixed evidence: Moffette and Gibbs (2021) and Heilmayr, Rausch, et al. (2020) find that soy and cattle production expanded in the Cerrado following the ASM and Cattle Agreements, while Macedo et al. (2012) finds no significant leakage and Villoria et al. (2022) estimates it offsets up to half the policy’s gains. A related strand investigates cattle displacement by soy within the Amazon: Arima et al. (2011) finds that soy expansion in established areas drove pasture into the frontier, though Richards (2015), Gollnow et al. (2018), and Heilmayr, Rausch, et al. (2020) show this displacement fell sharply after 2006. We contribute to both strands by studying a distinct form of leakage — regulatory arbitrage within the Amazon, where soy expansion may have shifted from regulated to unregulated parcels rather than leaving the region, and by examining whether the moratorium altered ranchers’ clearing and investment behavior on existing pasture.

This study is the first to examine how the ASM — a policy targeting a single commodity — affected an interconnected but unregulated sector: cattle ranching. By tracing how ranchers adjusted their land-use and investment decisions in response to soy restrictions, this paper sheds light on how sector-specific zero-deforestation agreements can achieve environmental goals by affecting interconnected sectors.

2.2 Background

The first half of the 2000s witnessed remarkable growth in soy production across the Brazilian Amazon. The legalization of genetically engineered (GE) seeds in Brazil in 2003 transformed the country’s soy sector, enabling cultivation and improving yields in previously unsuitable warm regions (Carreira et al., 2024). Concurrently, a commodity price boom driven by surging Chinese demand spurred rapid expansion in soy production (Carreira et al., 2024). As shown in Figure 2.1, planted area in the Amazon rose from 14 million hectares in 2000 to 25 million hectares in 2005, while production increased from 1 million to 5 million tons over the same period, according to the Municipal Agricultural Survey (*Pesquisa Agrícola Municipal* — PAM). The second half of the decade, however, brought a slowdown in soy expansion. Soy prices crashed in 2006 and 2007 (Macedo et al.,

2012), and cultivated area fell to 22 million hectares in 2007 — a 12% decline from its 2005 peak — before recovering steadily in subsequent years.

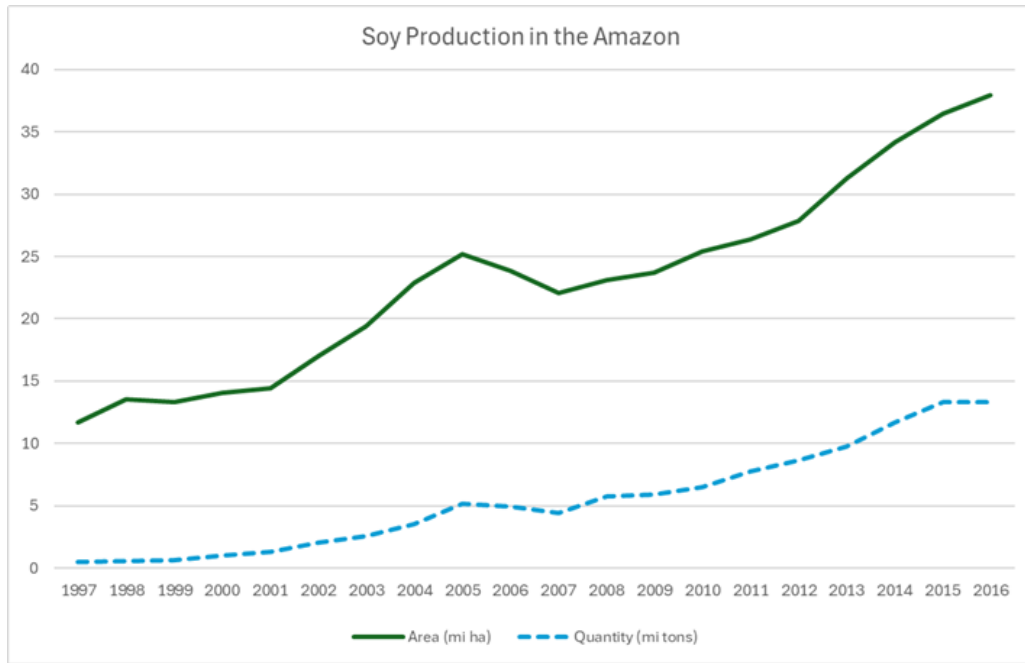


Figure 2.1: Soy Area and Quantity Produced — Municipal Agricultural Survey (PAM)

This expansion in soy cultivation came largely at the expense of low-productivity pastures (Macedo et al., 2012; Meyfroidt et al., 2014). While a fraction of soy expanded directly into forests during the first half of the 2000s, soy subsequently expanded almost exclusively over pastureland (Carreira et al., 2024; Macedo et al., 2012; Meyfroidt et al., 2014; Richards, 2015). Lower-value cattle ranching relocated to more remote, low-rent areas, while higher-value soy production concentrated in accessible, high-rent regions with established infrastructure and supply chains (Meyfroidt et al., 2014). Rising soy profitability increased the opportunity cost of holding land for cattle, creating incentives for ranchers to sell their properties and clear new land elsewhere (Macedo et al., 2012).

Cattle ranching has historically been the primary driver of deforestation in the Amazon. Figure 2.2 illustrates deforestation across the arc of deforestation by land use. Amazon deforestation remained high in the early 2000s, peaking in 2004, with pasture conversion as the dominant land-use change. MapBiomas data reveal no direct deforestation attributable to the soy sector; however, a small fraction is recorded under "Other Tempo-

rary Crops” (OTC), a category that may include soy plots that MapBiomass was unable to classify.

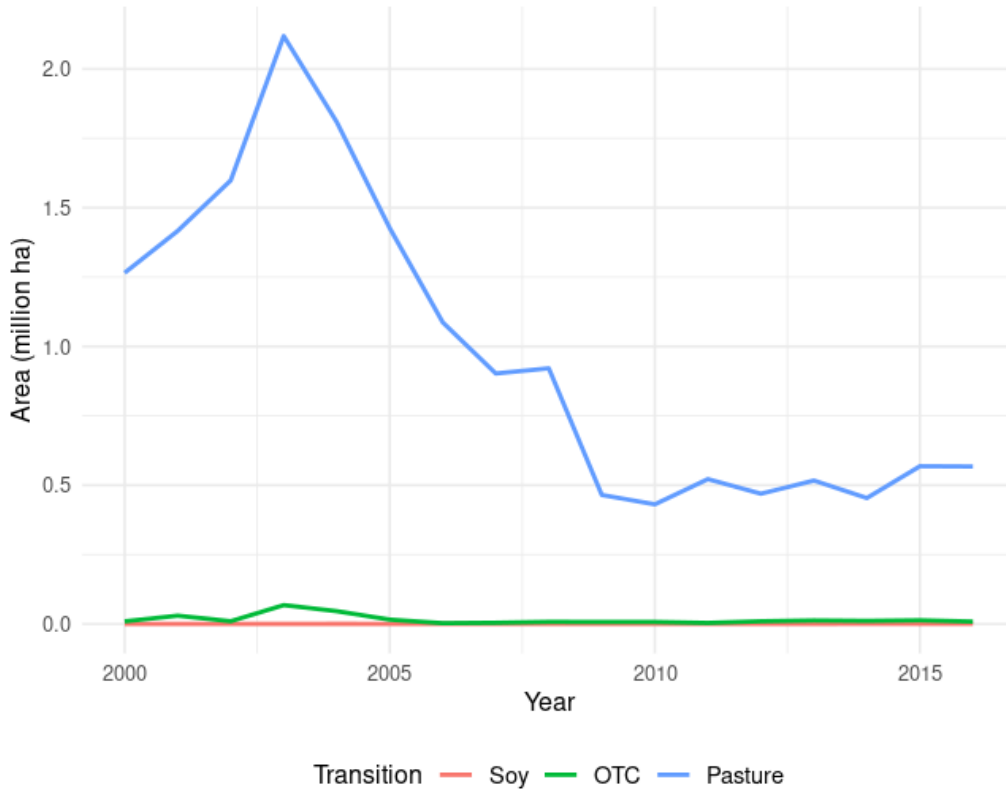


Figure 2.2: Forest Conversion by Land Use in the Amazon’s Arc of Deforestation — MapBiomass

The emergence of GE soy in the early 2000s nonetheless reshaped land markets and deforestation dynamics along the Amazon frontier (Macedo et al., 2012). Although most deforestation is attributed to pasture expansion, soy may have indirectly contributed by raising land values and displacing cattle ranching — an effect that remains difficult to quantify (Gollnow et al., 2018; Macedo et al., 2012; Meyfroidt et al., 2014). Carreira et al. (2024) provides evidence that deforestation accelerated more rapidly in municipalities with greater potential productivity gains from GE soy during the boom period. The scale of deforestation — which reached 27,000 km² in 2004 — its links to the agricultural sector, and its environmental consequences raised growing concerns in academia and among environmental advocates, including biodiversity loss and local-to-global climate impacts,

prompting responses from both policymakers and the private sector (Heilmayr, Rausch, et al., 2020; Meyfroidt et al., 2014; Richards, 2015).

In 2004, the Brazilian government launched the Action Plan for the Prevention and Control of Deforestation in the Legal Amazon (PPCDAm), which introduced coordinated action across government institutions and established novel procedures for monitoring, environmental control, and territorial management (Assunção et al., 2015). A particularly consequential measure was the introduction of the Real-Time System for Detection of Deforestation (DETER), which enabled remote-sensed monitoring with daily observations (Assunção et al., 2015; Macedo et al., 2012). In parallel, the federal government substantially expanded protected areas in the Amazon, increasing coverage by 520,000 km² between 2004 and 2009 (Assunção et al., 2015).

The period after 2007 marked the beginning of the PPCDAm’s second phase. New measures included sanctions and heightened monitoring for municipalities placed on a high-priority deforestation list, a decree raising penalties for deforestation infractions, and the conditioning of rural credit approval on borrowers’ demonstrated compliance with environmental regulations (Assunção et al., 2015; Heilmayr, Rausch, et al., 2020; Macedo et al., 2012).

Alongside these governmental efforts, the private sector launched its own initiatives to reduce deforestation in agricultural supply chains. In 2006, soy producers and traders — led by the Brazilian Association of Vegetable Oil Industries and the National Association of Grain Exporters — announced a ban on the purchase of soy from newly deforested areas in the Amazon biome, an initiative that became known as the Amazon Soy Moratorium (ASM) (Gollnow et al., 2018; Heilmayr, Rausch, et al., 2020; Macedo et al., 2012; Richards, 2015). The initiative emerged in response to a Greenpeace report linking Amazon deforestation to three major American commodity traders — Cargill, Bunge, and ADM — which together controlled 90% of Brazil’s soy trade (Gollnow et al., 2018; Heilmayr, Rausch, et al., 2020). The original agreement, lasting two years, prohibited soy production on lands cleared after 24 July 2006 (Heilmayr, Rausch, et al., 2020). It was renewed periodically until 2016, when it was extended indefinitely, with the cutoff date

later adjusted to July 2008 to align with revisions to Brazil’s Forest Code (Gollnow et al., 2018; Heilmayr, Rausch, et al., 2020) enacted in 2012. However, disputes over the moratorium’s legality and sustained opposition from the agribusiness sector ultimately led to its suspension in 2025 (USDA Foreign Agricultural Service, 2025).

The monitoring arm of the ASM is the Soy Working Group (*Grupo de Trabalho da Soja* — GTS), an association of industry representatives and prominent environmental organizations (Gollnow et al., 2018; Heilmayr, Rausch, et al., 2020). The GTS refined its methodology over time to identify soy farms in violation of the ASM. Early monitoring efforts were limited in scope: no non-compliant soy was detected in the initial years, and early assessments relied on regional sampling. From the 2009/10 crop year onward, monitoring expanded substantially through the combined use of satellite imagery and aerial flyovers. However, due to logistical constraints and the increasing expansion of soy in the Amazon, flyovers were discontinued after the 2012/13 crop year, after which monitoring relied exclusively on remote sensing (ABIOVE et al., 2022).

The success of its previous campaign in persuading soy producers to adhere to an anti-deforestation moratorium led Greenpeace to launch a new campaign, this time against cattle-ranching-led deforestation. The campaign was once again successful, and in 2009, 4 major meatpackers signed an agreement not to buy from producers in areas deforested after 2009 (Moffette & Gibbs, 2021). At the same time, federal prosecutors in Brazil filed lawsuits supporting the policy’s implementation, and in 2009, 21 lawsuits led to the signing of 140 legally binding terms of adjustment of conduct (Moffette and Gibbs (2021)). This set of sector agreements and legal contracts came to be known as “cattle agreements” (CA). One relevant critique of the CA is that, unlike crops such as soy, cattle are mobile and more difficult to track, and the CA applied only to properties sold directly to slaughterhouses. A commonly cited leakage channel involves cattle raised on deforested land being laundered through intermediary farms before reaching monitored slaughterhouses, effectively circumventing the supply chain restrictions (Alix-Garcia and Gibbs (2017)).

Following these combined efforts, deforestation rates fell sharply in the second half of

the 2000s, declining from 27,000 km² in 2004 to 4,500 km² in 2012 (Gollnow et al., 2018; Meyfroidt et al., 2014; Richards, 2015). This decline is broadly attributed to a combination of environmental policies, zero-deforestation supply-chain commitments, declining agricultural commodity prices, and appreciation of the Brazilian Real, which reduced the cost competitiveness of Brazilian soy exports (Gollnow et al., 2018; Macedo et al., 2012; Meyfroidt et al., 2014). A growing body of research has sought to disentangle the relative contribution of each factor. Assunção et al. (2023) find that increased monitoring reduced deforestation, while Assunção et al. (2019) document a similar effect from restrictive rural credit policies. Assunção and Rocha (2019) shows that sanctioning high-priority municipalities curbed forest loss, largely through heightened law enforcement, and Qin et al. (2023) finds that the expansion of protected areas played an independent role. On the market side, Assunção et al. (2015) finds that declining commodity prices were associated with reduced deforestation. Alix-Garcia and Gibbs (2017) finds that leakage significantly offsets the efficiency of the CA. Finally, Heilmayr, Rausch, et al. (2020) provides evidence that the Soy Moratorium itself contributed meaningfully to preventing deforestation.

The Soy Moratorium represents the first voluntary zero-deforestation supply-chain initiative implemented in a tropical region (Gollnow et al., 2018). It has attracted considerable scholarly attention for two reasons: first, as a corporate social responsibility initiative, it introduced private-sector actors into conservation policy in a novel way; and second, it has frequently been found effective in reducing soybean-related deforestation, suggesting its potential as a model for analogous initiatives across other commodities (Gollnow et al., 2018).

Concerns surrounding the moratorium included the risk of displacing pasture — and thus indirectly driving deforestation — and the potential leakage of soy production into other biomes, particularly the Cerrado, effects that remain difficult to quantify (Gollnow et al., 2018; Macedo et al., 2012; Meyfroidt et al., 2014). The observed decline in pasture-driven deforestation alongside continued growth in soy area, illustrated in Figures 2.1 and 2.2, partially mitigated these concerns, as soy expansion over pasture appeared increasingly decoupled from forest loss (Macedo et al., 2012; Meyfroidt et al.,

2014). Advances in pasture management and sanitary measures that boosted cattle sector productivity may have further reduced the pressure to clear additional land (Macedo et al., 2012).

The rebound in deforestation after 2012 renewed questions about the effectiveness of policies targeting forest loss, including the ASM. The GTS identified a growing number of non-compliant properties, reaching 64,316 hectares in 2018 — equivalent to 1.4% of all soy production and 1.2% of all deforestation in the Amazon biome (Gollnow et al., 2018; Heilmayr, Rausch, et al., 2020). Beyond the risk of leakage, loopholes in the agreement meant that non-compliant soy could still enter supply chains (Gollnow et al., 2018).

2.3 Conceptual Framework

To study how the Amazon Soy Moratorium (ASM) may have affected land conversion in the cattle-ranching sector, we develop a simple framework that tracks pasture plots by their *vintage*, defined as the year in which each plot was first converted from forest to pasture. Let cohort s denote the set of plots converted from forest to pasture in year s , and let t denote the current year. The age of a cohort in a given year t is denoted by $a = t - s$.

Absent any policy intervention, we assume that in each period a fraction of the area in a given cohort is converted from pasture to soy. Let $\theta_{Soy}(a)$ denote the cumulative proportion of cohort s that has been converted to soy as a function of its age a . We model this conversion process as

$$\theta_{Soy}(a) = \min\{1, f(a)\},$$

where $f : \mathbb{R}_+ \rightarrow \mathbb{R}_+$ is a generic conversion function satisfying

$$f(0) = 0 \quad \text{and} \quad f'(a) > 0.$$

Conversion to soy increases with pasture age, reflecting declining pasture productivity and rising incentives to switch land use over time. The function $f(a)$ captures the unconstrained propensity to convert pasture to soy as a function of age, while the $\min\{\cdot\}$ operator ensures that conversion is bounded above by full conversion.

Now consider the implementation of the Amazon Soy Moratorium (ASM). Let year T denote the cutoff date established by the policy. Pasture cohorts converted after year T are directly regulated by the ASM and therefore face explicit barriers to subsequent soy expansion. In contrast, cohorts converted prior to year T are unregulated and remain eligible for conversion to soy. Figure 2.3 illustrates the expected soy conversion paths of regulated and unregulated cohorts, normalized by age. Assuming the policy has teeth, regulated cohorts exhibit lower conversion rates than unregulated cohorts at any given age, reflecting the binding nature of the moratorium on post- T land. Under a perfectly enforced moratorium, no soy expansion would occur on regulated land.

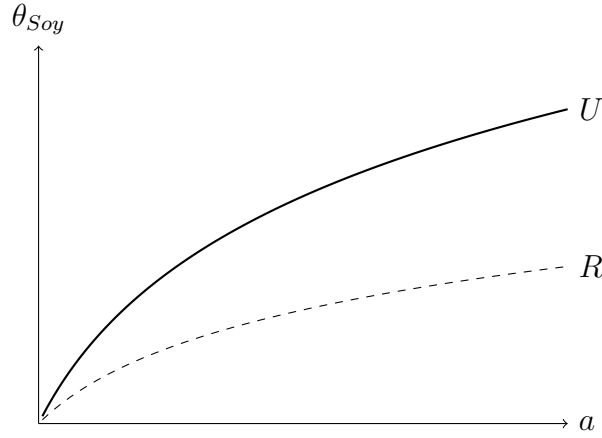


Figure 2.3: Expected conversion paths of regulated and unregulated cohorts

To motivate spillover effects onto unregulated land, consider an environment in which global demand for soy grows at a constant rate. For simplicity, we assume that soy productivity is constant over time, so that increases in total output must be met by a proportional expansion of the cultivated area. However, by restricting soy expansion on regulated cohorts, the ASM limits the supply of newly available land eligible for conversion. This restriction in the supply of eligible land creates excess demand for soy area, redirecting conversion pressure toward the closest available substitute — unregulated land. As a result, expansion pressure is displaced toward cohorts that remain unregulated—namely, those converted prior to year T . Consistent with this intuition, we assume that the implementation of the ASM induces a positive shock to pasture-to-soy conversion among unregulated cohorts ($s < T$) following the policy's introduction.

There are two conceptually distinct channels through which the ASM may affect pasture-to-soy conversion on unregulated land. First, the moratorium may induce an *immediate level shift* in conversion following its implementation, as producers who had planned to expand onto regulated land instead substitute toward unregulated alternatives. This one-time reallocation would generate a burst of conversion shortly after the policy's introduction, without permanently altering the subsequent conversion rate:

$$f_2(a) = f(a) + \gamma, \quad \gamma > 0.$$

Second, the moratorium may permanently increase the *rate* at which unregulated pasture is converted to soy, by durably raising the relative returns to converting unregulated land. If producers internalize the restriction on regulated land as a permanent feature of the market, unregulated pasture becomes persistently more valuable, sustaining a higher conversion rate over time:

$$f_1(a) = \alpha f(a), \quad \alpha > 1.$$

Both channels may operate simultaneously, with an initial scramble to reroute expansion followed by a lasting shift in conversion incentives. We therefore represent the post-ASM conversion function for unregulated cohorts as

$$f^*(a) = \alpha f(a) + \gamma, \quad \alpha > 1, \gamma > 0.$$

Figure 2.4 illustrates these mechanisms. Prior to the ASM, pasture in cohort s is progressively converted to soy, as shown in line 1. This trajectory also characterizes conversion dynamics in the absence of any moratorium-induced spillovers. If the ASM increases the annual conversion rate, conversion accelerates after the cutoff date, resulting in a steeper trajectory, as shown by line 2. If, instead, the ASM induces an immediate but transitory response, conversion exhibits a discrete upward shift at the time of implementation, as shown by line 3. Finally, if both channels operate simultaneously, conversion displays both a level shift and a steeper post-ASM trajectory, as depicted by line 4.

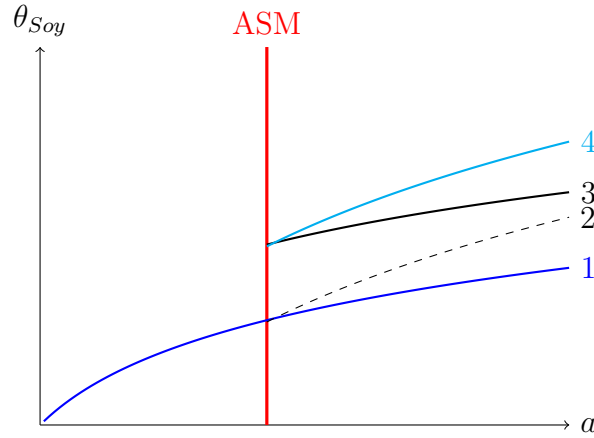


Figure 2.4: Possible paths of pasture to soy conversion on unregulated lands before and after the ASM

2.4 Empirical Strategy

Figures 2.5–2.7 illustrate how we construct the counterfactual pasture-to-soy conversion path for unregulated lands in the absence of the ASM. Figure 2.5 depicts the conversion paths of two unregulated cohorts in calendar time t . Because the cohorts enter pasture in different years, they are at different ages when the ASM is implemented, and thus their observed post-moratorium trajectories reflect both age-driven conversion dynamics and policy-induced shocks. This representation makes clear why direct comparisons across cohorts in calendar time are not informative: at any given date, cohorts differ mechanically in their underlying propensity to convert due to age effects. Figure 2.6 re-expresses the same trajectories using cohort age a as the running variable, aligning cohorts by their position along the conversion lifecycle. In this age-based representation, the older cohort can serve as a counterfactual for the younger cohort in the absence of the ASM, assuming that conversion dynamics are otherwise comparable across vintages. Finally, Figure 2.7 translates this comparison into event time, centered on the ASM cutoff. The difference between the observed post-ASM conversion path of the treated cohort and the counterfactual path implied by older, unregulated cohorts of the same age isolates the ASM’s spillover effect on pasture-to-soy conversion.

Our conceptual framework views pasture-to-soy conversion as a gradual process that unfolds as pasture ages. Absent policy intervention, older pastures are increasingly likely

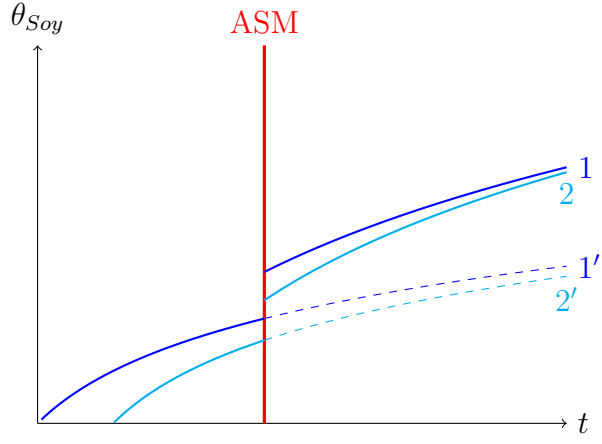
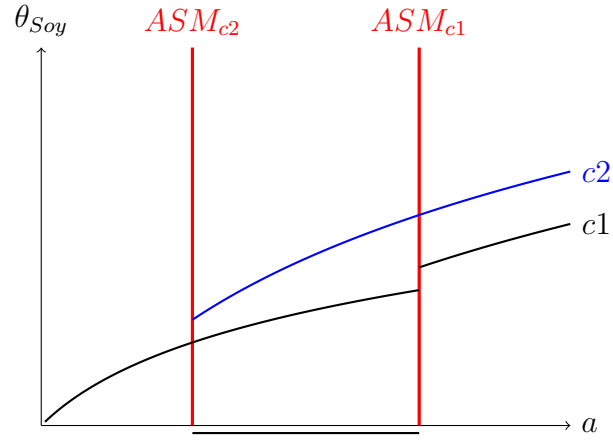


Figure 2.5: Conversion paths of two unregulated cohorts before and after the ASM across calendar time t



Cohort 2 used as counterfactual for Cohort 1

Figure 2.6: Conversion paths of two unregulated cohorts before and after the ASM across cohort age a

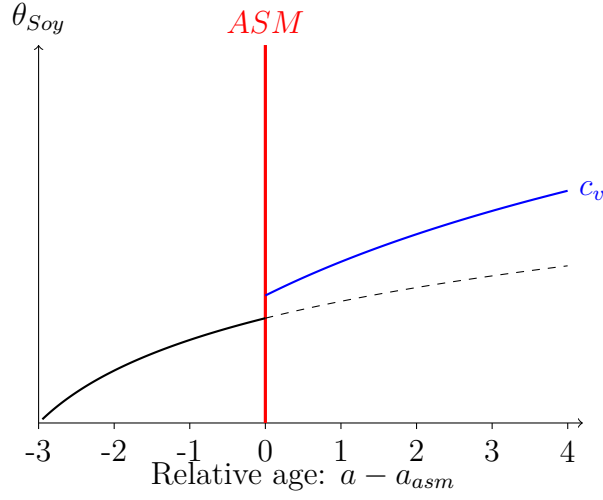


Figure 2.7: Conversion paths of two unregulated cohorts before and after the ASM across event time

to be converted to soy, reflecting declining pasture productivity and, therefore, rising opportunity costs of not converting. The Amazon Soy Moratorium does not restrict conversion on land cleared before the cutoff year, but it may nonetheless affect these unregulated cohorts by shifting soy expansion toward them.

To identify the spillover effects described in Section 2.3, we exploit the quasi-random variation in the timing of the ASM implementation relative to the ages of the unregulated vintages. We employ a modified event-study design with age fixed effects, which allows us to identify the dynamic effects of the ASM on pasture-to-soy conversion within comparable age groups. This empirical design directly reflects the intuition provided in Figure 2.7, which translates to the following specification:

$$y_{isa} = \lambda_s + \alpha_i + \gamma_a + \sum_{0 \leq r \leq \bar{T}} 1[D_{sa} = r] \cdot \beta_r + \theta X_{is} + \varepsilon_{isa} \quad (2.1)$$

where y_{isa} is the conversion outcome for municipality i and vintage s at age a ; $r = a - a_s^*$ indexes years since ASM implementation at a given age for vintage s ; λ_s are vintage fixed effects; α_i are municipality fixed effects; and γ_a are age fixed effects.

In this specification, the dummy $1[D_{sa} = r]$ equals one when vintage s has been exposed to the ASM for exactly r years at age a . In other words, it identifies observations that are r periods past the moratorium cutoff for that vintage. The β_r coefficients then

trace out the dynamic treatment effects as a function of time since exposure. Importantly, this specification allows us to identify the effects of the ASM, captured by β_r , while flexibly controlling for pasture age. As a result, the estimated event-time coefficients recover the gap between the observed and counterfactual paths depicted in Figure 2.6.

The age fixed effects γ_a absorb the common conversion lifecycle shared across cohorts, ensuring that the estimated treatment effects are not confounded by the mechanical increase in conversion rates as pasture ages — which would otherwise bias any comparison between younger treated cohorts and their post-ASM trajectories upward. The vintage fixed effects λ_s address a complementary concern: since older cohorts serve as counterfactuals for younger treated ones, any systematic difference in conversion levels across vintages — independent of age — would be misattributed to the moratorium. By absorbing these level differences, λ_s ensures that the identification comes from within-age, within-vintage variation rather than from secular trends correlated with cohort timing. The municipality fixed effects α_i control for time-invariant local characteristics, such as baseline economic conditions and climate.

Our unit of observation is the municipality–vintage–age cell. We aggregate pixel-level land-use data to the municipal level to construct age and vintage-specific measures of pasture-to-soy conversion. Spatially aggregating pixels mitigates measurement errors arising from misclassification in remote sensing data (Carreira et al., 2024). Specifically, for each municipality and pasture vintage, we compute two conversion outcomes.

First, we measure the cumulative probability of conversion to soy, defined as the share of pixels within a given vintage and municipality that have been converted to soy by age a . Second, we construct the age-specific hazard rate of conversion, defined as the share of pixels that transition from pasture to soy at age a among those that remain in pasture at age $a - 1$. Under this aggregation, y_{isa} denotes either the cumulative conversion probability or the hazard rate for municipality i , vintage s , and pasture age a . The cumulative probability indicates whether land from a given cohort is more likely to have been converted to soy by a given age than land from other cohorts. At the same time, the hazard rate captures the conditional probability that a pasture not yet converted at age

$a - 1$ transitions to soy at age a . The event-time coefficients β_r therefore capture how the municipality-level conversion profile of a given pasture cohort deviates from its post-ASM counterfactual in the absence of the moratorium.

We estimate Equation 2.1 using the imputation estimator proposed by Borusyak et al. (2024). Specifically, we first estimate the vintage and age fixed effects, λ_s and γ_a , using observations from vintages that are not yet affected by the moratorium at a given age. We then use these estimates to construct the counterfactual post-ASM outcomes that would have been observed in the absence of the moratorium. That is, information from not-yet-treated vintages at a given age is used to predict the potential outcomes of untreated vintages exposed to the ASM. The difference between observed and imputed outcomes yields the treatment effect for each unit and event time. These estimates are unbiased under two standard assumptions: parallel trends, which requires that, in the absence of the moratorium, observed controls fully characterize untreated potential outcomes; and no anticipation, which requires that producers do not adjust their outcomes prior to the moratorium. Unlike a standard two-way fixed-effects OLS estimator, this approach accommodates heterogeneous treatment effects across cohorts and event times, delivering unbiased estimates under such heterogeneity. Standard errors are clustered at the municipality-vintage level to account for serial correlation in outcomes within pasture cohorts over time.

The Borusyak et al. (2024) estimator modifies the classic event-study framework by excluding explicit estimates of pre-treatment coefficients and is therefore a “lags-only” estimator. Rather than normalizing treatment effects relative to the period immediately preceding treatment, as in a conventional event study, this approach uses the average of all pre-treatment periods as the reference. Consequently, the estimated post-treatment coefficients measure deviations from the entire pre-ASM trajectory rather than from a single baseline year.

Identification relies on a parallel trends assumption analogous to that in standard difference-in-differences designs: absent the moratorium, conversion outcomes for vintages exposed to the ASM would have followed the same age-specific trends as those for vintages

not yet treated. Under this assumption, predicted outcomes from yet-to-be-treated vintages at a given age serve as valid untreated counterfactuals for treated vintages, enabling the imputation procedure to recover unbiased dynamic treatment effects.

This modified event-study design measures how pasture conversion for a given vintage deviates from its untreated counterfactual trajectory over time after the policy’s implementation. The sequence of β_r coefficients captures the dynamic response to the moratorium, allowing conversion to adjust through discrete jumps, gradual changes, or both. A permanent, discrete increase in conversion would appear as a level shift that persists over time, whereas a change in the conversion rate would appear as a change in slope in the cumulative profile.

2.5 Data

Our study uses high-resolution land-use data to examine cattle ranchers’ land-use decisions before and after the ASM. We compiled data from the MapBiomas Collection 8 dataset (Mapbiomas, 2024). This dataset provides annual coverage of land-use information, including forest, pasture, and soy land cover. This detailed land-cover information allows us to identify when a pixel was first converted from forest to pasture and to track subsequent land-use changes over time. This data allows us to construct a panel containing land-use information for each pixel at each age. For example, a pixel converted from forest to pasture in 2002 (vintage 2002) will be one year old in 2003, two years old in 2004, and so on. We can then observe how pixels belonging to the same vintage and regulatory group respond to exogenous variations in exposure to the Soy Moratorium across ages.

MapBiomas uses Landsat satellite imagery and machine learning algorithms to classify images into various land-use categories. They use numerous variables extracted from the images to train the machine-learning model, including spectral color bands and vegetation moisture indices. Areas of natural pasture are predominantly classified as grassland or wetland, and are separated from man-made pasture. The final product consists of granular pixels (30m) classified into 31 possible categories, including pasture, soybeans, natural vegetation, urban areas, and waterways.

One feature of the MapBiomass dataset is that a lack of imagery in a given region can make it difficult to precisely identify a crop. The dataset contains a land use class called "other temporary crops" (OTC), which includes areas identified as temporary crops—those with cultivation cycles of less than one year—but where MapBiomass was unable to distinguish the specific crop type. As such, this class may encompass crops such as beans, corn, cotton, rice, and soy.

We further combine our land-use dataset with measures of soil suitability for soy and mechanized agriculture. Our soy suitability and mechanized agricultural suitability data are sourced from the FAO-GAEZ dataset, and Soares-Filho et al. (2014), respectively. The former dataset comprises soil suitability indices for various crops, including soybeans, at a low resolution (approximately 9 km). We selected the suitability data under the rainfed and high-input scenarios. Soares-Filho et al. (2014) utilizes topography and soil characteristics to develop detailed (60 m resolution) maps of each area's potential for mechanized agriculture, categorizing it as very suitable (2), moderately suitable (1), or unsuitable (0). We follow (Heilmayr, Rausch, et al., 2020) and categorize a pixel as suitable for soy if its FAO-GAEZ index is greater than or equal to 4000 and its mechanized agriculture index is nonzero.

Our study focuses on the arc of deforestation — the agricultural frontier along the southern edge of the Amazon, where soy expansion and cattle ranching exert the greatest pressure on the remaining forest Heilmayr, Rausch, et al. (2020). Formally, we define our study area as the portion of the Amazon biome within 300 km of the Cerrado border, following Heilmayr, Rausch, et al. (2020). Figure 2.8 shows land use across Brazil, with our study area highlighted. Our database includes pasture vintages from 2000 to 2015, with land-use data available for each vintage through 2022. Our resulting municipality–vintage–age panel includes 333 municipalities and 16 vintages. Given the 2006 cutoff date, we have 6 unregulated vintages (2000–2005). However, the 2005 vintage is excluded from the estimation because the imputation estimator requires at least one untreated period per vintage to construct a counterfactual, and the moratorium took effect the following year, leaving no such observations for that vintage. Our control group, therefore, comprises 5

unregulated vintages (2000–2004).

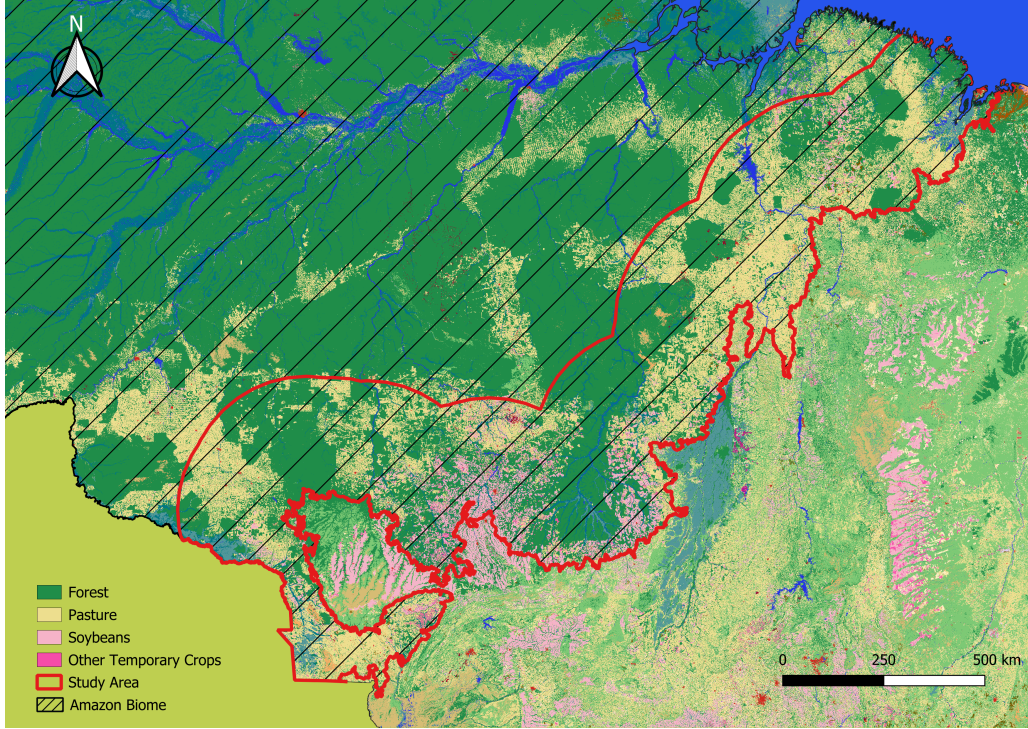


Figure 2.8: Land use in the Amazon in 2024 - MapBiomass

Figure 2.9 shows the size of each vintage from 2000 to 2010. Vintages from the first half of the decade are substantially larger, peaking in 2003, which reflects the historically high deforestation rates of that period. The subsequent decline in vintage size mirrors the broader slowdown in Amazon deforestation documented in Section 2.2, driven by the combination of policy interventions and shifting economic conditions.

Figure 2.10 displays the distribution of relative age among unregulated vintages (years 2000–2004) at the time of the ASM. The left-censored shape of the pre-moratorium bins reflects a mechanical feature of the data: since 2000 is the earliest vintage in our sample, only that cohort contributes observations at age -5 relative to the ASM, only vintages 2000 and 2001 contribute at age 4, and so on. As a result, the number of observations rises steeply from age -5 to age -1 , at which point all five unregulated vintages are represented and the distribution stabilizes. From age 1 onward, the count remains essentially flat, as each vintage contributes a full cohort of observations at every post-ASM age.

Figure 2.11 shows the evolution of land use by age for the unregulated and regulated

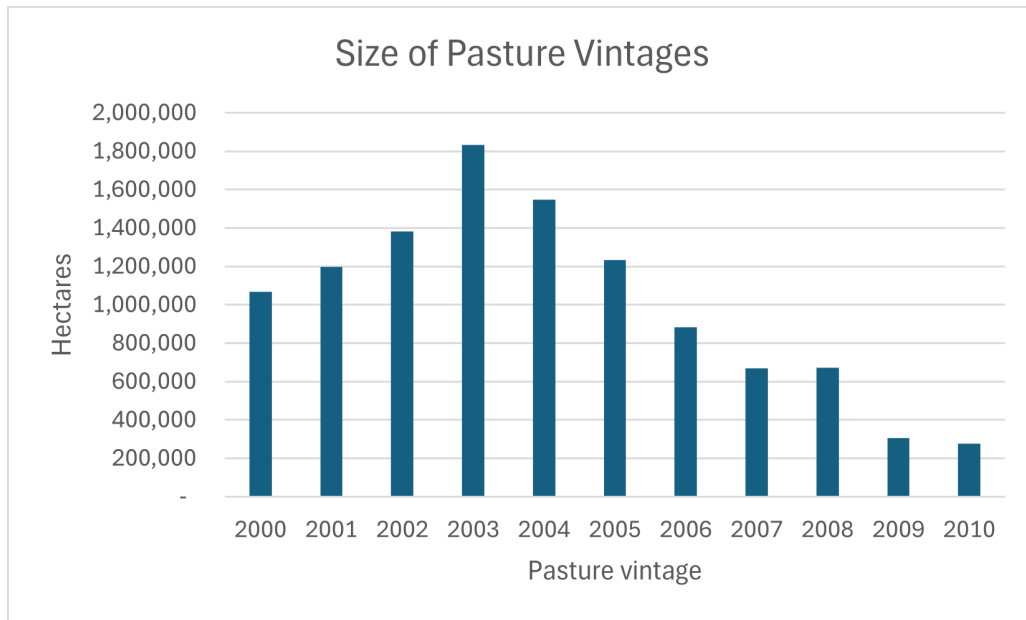


Figure 2.9: Forest-to-pasture conversion by year - Source: MapBiomass

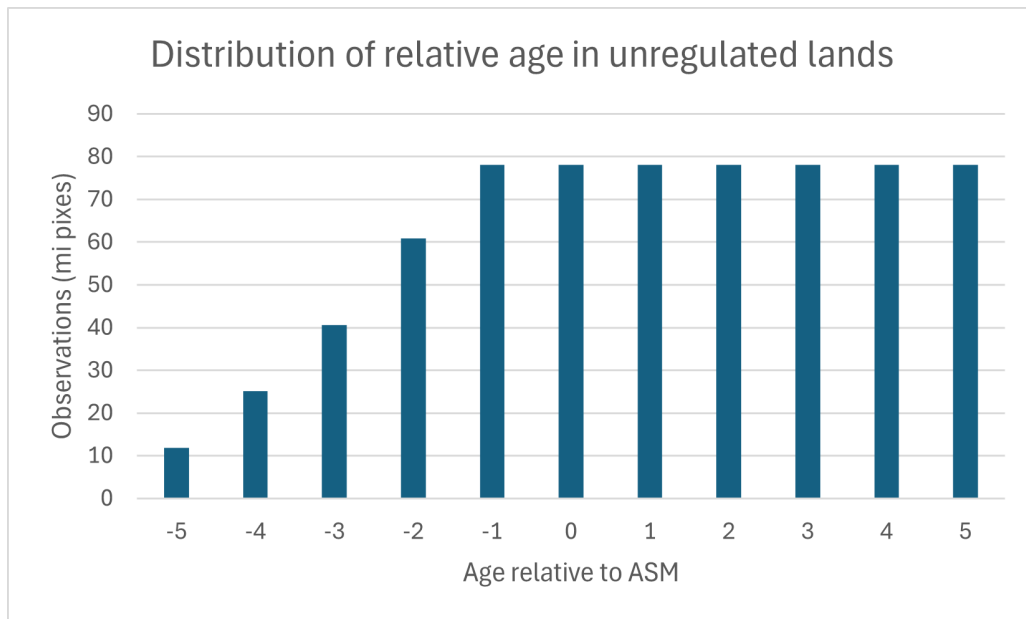


Figure 2.10: Age distribution of unregulated vintages relative to the ASM

vintages. The share of each vintage covered by soy, OTC, and forest increases steadily as vintages age across both groups. We observe a higher prevalence of soy in the unregulated group and greater forest reversion in the regulated group. Table 2.1 shows land use shares by regulation group. At early ages, regulated vintages already show substantially more forest reversion (11% vs 5.6%). Over all periods, pasture shares converge across groups (around 80%), despite unregulated vintages being older on average — indicating that regulated land does not simply remain as pasture but transitions differently. The most striking difference is in soy: unregulated vintages show more than twice the share of soy as regulated vintages (10.7% vs 4.3%), consistent with the moratorium redirecting agricultural expansion. Nonetheless, the non-negligible share of soy on regulated land indicates that the moratorium is not perfectly enforced, with some regulated pastures still transitioning to soy production.

Table 2.1: Land use shares by regulation group

	Early (age < 6)			All periods	
	Unregulated		Regulated	Unregulated	Regulated
	Before 2006	Total	Total		
Share					
Pasture	0.8976	0.8578	0.8525	0.7709	0.7978
Soy	0.0231	0.0419	0.0169	0.1067	0.0433
OTC	0.0348	0.0416	0.0189	0.0373	0.0296
Forest	0.0408	0.0563	0.1104	0.0805	0.1341

Table 2.2 shows summary statistics at the municipality-vintage-age level by regulation group. The two groups are similar in size (37,781 and 36,900 observations), and after aggregating at this level, the soy share gap narrows relative to Table 2.1 (4% vs 2.4%), reflecting within-municipality variation in conversion intensity. Regulated vintages retain a higher forest share (20.5% vs 16.9%). Most importantly, both groups exhibit nearly identical soy suitability and mechanization suitability conditions, supporting the identifying assumption that regulated and unregulated vintages face similar agricultural potential and that treatment assignment is not confounded by land quality. Summary statistics



Figure 2.11: Land use evolution by regulation group

restricted to early ages ($age < 6$) are reported in Table S2.1 in the appendix.

2.6 Results

Our main results are presented in Figures 2.12 and 2.13.

Because the Borusyak et al. (2024) estimator is a lags-only specification that does not estimate pre-treatment coefficients within the main regression, we recover pre-treatment dynamics using a separate regression estimated on untreated observations only, following the testing procedure in Borusyak et al. (2024). The reference group for these pre-treatment coefficients consists of all periods more than k periods prior to the event date, where k denotes the number of pre-treatment coefficients shown. In the main imputation regression, there is no single reference period: the counterfactual for each treated obser-

Table 2.2: Summary statistics at the municipality-vintage-age level by regulation group

Variable	N	Mean	Std. dev.	Min	Max
<i>Unregulated</i>					
Soy Share	37781	0.0406	0.1391	0	0.9709
Forest Share	37781	0.1688	0.1798	0	1
OTC Share	37781	0.0179	0.0528	0	1
Soy Suitability	37781	0.4064	0.4138	0	1
Mech. Ag. Suit.	37743	0.9208	0.4361	0	2
<i>Regulated</i>					
Soy Share	36900	0.0241	0.0769	0	0.9228
Forest Share	36900	0.2047	0.1934	0	1
OTC Share	36900	0.0152	0.0475	0	0.8966
Soy Suitability	36900	0.4042	0.4204	0	1
Mech. Ag. Suit.	36900	0.9209	0.4525	0	2

vation is constructed by imputing fixed effects estimated from all pre-treatment observations, effectively averaging across the full set of untreated observations as the reference. This separation of testing from estimation avoids the bias that arises in traditional leads-and-lags event-study regressions, where pre-trend coefficients are estimated jointly with treatment effects and can be contaminated by treatment-effect heterogeneity (Borusyak et al., 2024).

Figure 2.12 reports the dynamic effects of the moratorium on the cumulative probability of pasture-to-soy conversion among unregulated vintages. The pre-treatment coefficients are individually and jointly insignificant ($\chi^2(3) = 5.68$, $p = 0.128$), supporting the parallel trends assumption. Following the implementation of the moratorium, we detect no significant change in the cumulative probability of conversion. If the moratorium were binding and effective on regulated lands, one might expect some displacement of soy expansion toward unregulated vintages. However, we find no evidence of an increase in cumulative conversion in these cohorts.

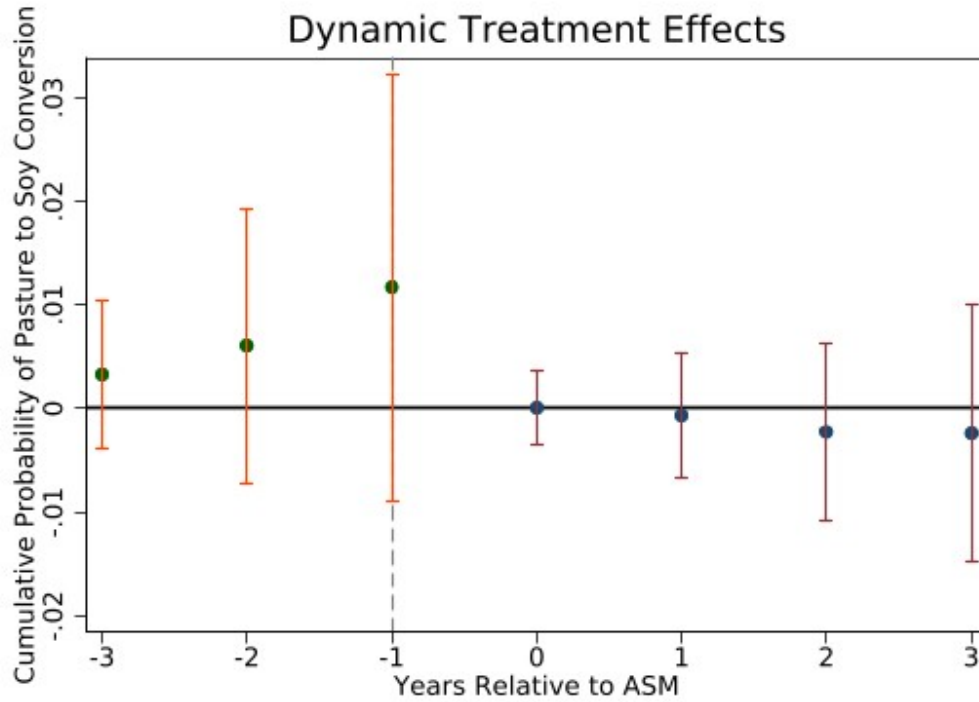


Figure 2.12: Unregulated vintages pasture to soy conversion before and after the ASM (cumulative probability)

Figure 2.13 presents the corresponding results for the age-specific hazard rate of pasture-to-soy conversion. As with the cumulative specification, the pre-treatment coefficients are individually and jointly insignificant ($\chi^2(2) = 5.58$, $p = 0.061$), supporting the parallel trends assumption. The post-moratorium hazard rates are negative and statistically significant. If the moratorium effectively constrained soy expansion on regulated land, one might expect some displacement toward unregulated pasture at the margin. Instead, we observe a decline in the conditional probability that remaining pasture transitions to soy following the ASM.

The difference between the cumulative and hazard results reflects the distinction between stock and flow outcomes. The cumulative probability captures the total share of land converted up to a given age and therefore incorporates substantial pre-moratorium conversion, which dilutes the post-ASM signal. In contrast, the hazard rate measures the marginal, age-specific probability of transition among remaining pasture, isolating new conversions after the moratorium. A post-ASM decline in the hazard rate thus indicates

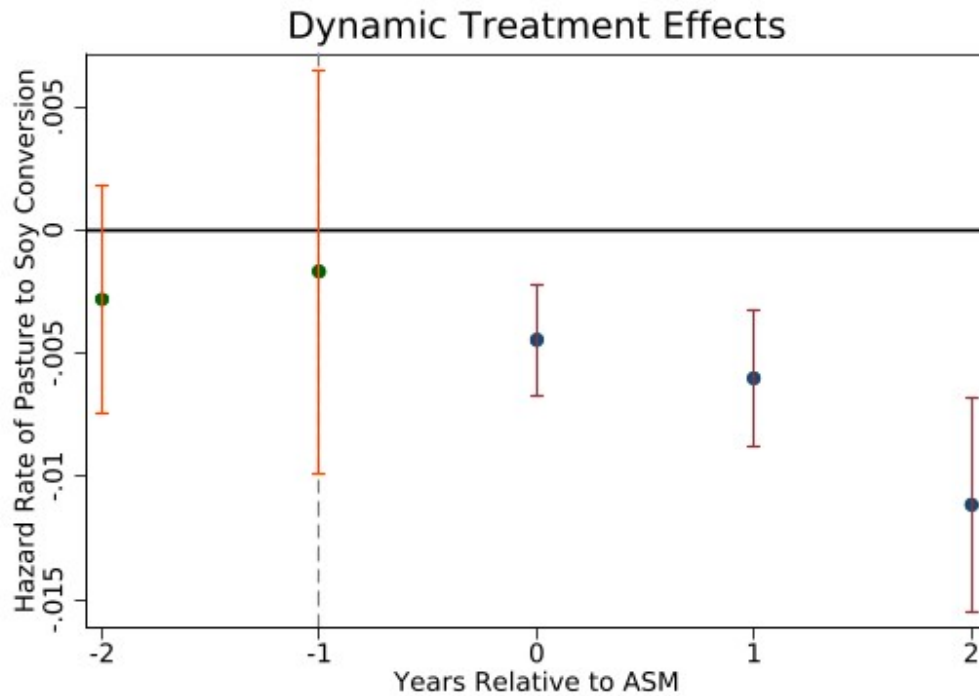


Figure 2.13: Unregulated vintages pasture to soy conversion before and after the ASM (hazard rate)

a reduction in new conversions that is not large enough — over the observed horizon — to overcome the pre-existing stock of converted land and generate a detectable shift in cumulative conversion.

These findings provide no evidence that soy expansion was reallocated from regulated to unregulated pasture following the ASM. If the moratorium had been fully binding on regulated land, a natural prediction would be some displacement toward older, unregulated vintages. We do not observe such a pattern. Instead, the decline in pasture-to-soy conversion among unregulated cohorts is more consistent with a market-wide contraction in soy demand — most plausibly the 2006–2007 price crash documented in Figure 2.1 — than with policy-induced reallocation.

An important limitation of our empirical design is that age fixed effects preclude the use of year fixed effects, thereby limiting our ability to directly control for contemporaneous shocks to soy conversion. The 2006–2007 price crash is a particularly salient concern: it coincides with the moratorium’s implementation, is large in magnitude, and would in-

dependently suppress conversion among both regulated and unregulated cohorts. This identification challenge is not unique to our setting — the contemporaneity of the price crash and the ASM complicates causal attribution more broadly in this literature. We intend to address this by controlling for regional soy prices in a subsequent version of this paper.

2.7 Concluding Remarks

This paper investigates how the Amazon Soy Moratorium, a policy targeting a single commodity, affected cattle ranchers' decisions by restricting land markets. Contrary to our expectations, under an effective moratorium, we observe no increase in pasture-to-soy conversion in areas not regulated by the moratorium after its implementation. This finding reduces concerns that the moratorium displaced ranchers and caused indirect deforestation. However, it raises questions about the channels through which the moratorium may have operated — particularly given that direct forest-to-soy conversion never exceeded 68,000 hectares annually and represented only 16,000 hectares by 2005 (Mapbiomas, 2024), meaning that even if the moratorium had perfectly blocked all such conversion, cumulative direct savings over a decade would account for less than 10% of Heilmayr, Rausch, et al. (2020)'s central estimate of 1.8 million hectares of prevented deforestation.

Our findings reveal no evidence that the moratorium displaced soy expansion onto unregulated pastureland within the Amazon, and in fact, observe a decline in pasture-to-soy conversion following implementation. Rather than simply reducing leakage concerns, this result sits in tension with the large deforestation reductions estimated in the literature. A simple accounting exercise sharpens the puzzle: since direct forest-to-soy conversion never exceeded 68,000 hectares annually and represented only 16,000 hectares by 2005, any large-scale effect of the moratorium would have had to operate through the land market — yet we find no detectable trace of such disruption. The most important caveat is that the 2006–2007 soy price crash coincides almost exactly with the moratorium's introduction, and our design cannot cleanly separate the two. Resolving this identification challenge, and thereby determining whether the moratorium genuinely disrupted the land

market or whether the estimated effects reflect the contemporaneous collapse in demand, remains the central open question.

In 2025, nearly two decades after the Amazon Soy Moratorium took effect, Brazil's agribusiness sector — an increasingly powerful political force — successfully pressured the government into suspending it (USDA Foreign Agricultural Service, 2025). That the industry sought to dismantle the moratorium suggests that, from the agribusiness perspective, the ASM was at least partially binding: a policy perceived as toothless would not have warranted the political effort to remove it.

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Appendices

Appendix 2 — Supplementary Tables

Table S2.1: Summary statistics at the municipality-vintage-age level by regulation group
- Age < 6

Variable	N	Mean	Std. dev.	Min	Max
<i>Unregulated (Before 2006)</i>					
Soy Share	4890	0.0099	0.0596	0	0.8860
Forest Share	4890	0.0868	0.1416	0	1
OTC Share	4890	0.0124	0.0523	0	0.6943
<i>Unregulated (Total)</i>					
Soy Share	9770	0.0128	0.0659	0	0.8860
Forest Share	9770	0.1142	0.1628	0	1
OTC Share	9770	0.0156	0.0573	0	0.6943
<i>Regulated (Total)</i>					
Soy Share	16060	0.0098	0.0512	0	0.8834
Forest Share	16060	0.1475	0.1732	0	1
OTC Share	16060	0.0146	0.0573	0	0.8966

Chapter 3

Unintended Consequences of Nature-Based Climate Mitigation: Exotic Natural Capital and Water Scarcity

Ignacio Oliva and Bruno Pimenta^{*}

Abstract.

Increasing tree cover area is often promoted as an effective policy to meet carbon mitigation goals. However, the expansion of exotic monoculture industrial tree plantations (ITP) can disrupt local ecosystem services. This paper studies the impact of these plantations on freshwater provision using watershed-level data and panel variation in land-cover change in Chile. Our results indicate a delayed but robust negative effect of ITP expansion on freshwater availability. A one percentage point increase in ITP conversion reduces annual normalized net outflow by approximately 0.011 Mm³/year per km² after 7 to 9 years — equivalent to roughly 7% of mean annual net outflow at observed expansion rates.

^{*} I contributed to the research design, data collection, data analysis, and the writing. This material is reproduced with permission from Ignacio Oliva.

3.1 Introduction

Over the past decades, afforestation initiatives have gained broad support from governments and civil society as a strategy to mitigate climate change and promote ecosystem restoration (IPCC, 2023b). Afforestation has the potential to increase carbon sequestration while halting environmental degradation, thereby restoring eroded and damaged ecosystems (Mirzabaev & Wuepper, 2023). However, the environmental impacts of afforestation depend on how it is carried out. Investing in tree plantations monocultures (ITP, for industrial tree plantations) has been promoted as a profitable and scalable strategy to expand forested areas. Nevertheless, this strategy might limit ecosystem services provision, particularly freshwater (Durán & Barbosa, 2019; Lewis et al., 2019).¹ Despite these concerns, countries that have disclosed their afforestation strategies have determined that 50% of the land to be planted (102.1 Mha) will be dedicated to commercial tree plantations (Lewis et al., 2019). Given the scale of current afforestation commitments, understanding their risks is crucial for climate policy design.

Two mechanisms are commonly cited in the literature as potential explanations for how ITPs may alter the water cycle in a given basin, thereby reducing the volume of freshwater available. First, ITPs are thought to consume more water than the average species to sustain higher growth rates (Scott, 2005). Second, they are associated with higher evapotranspiration rates (Scott, 2005; van Dijk & Keenan, 2007), the aggregate volume of water that evaporates from the soil and is transpired by trees. Together, these mechanisms are hypothesized to reduce the amount of water ultimately released into the watershed, potentially diminishing the level of surface water available. From an economic perspective, these hydrological changes reduce the freshwater supply, thereby affecting both economic production and welfare.

In this paper, we examine whether ITPs limit the provision of freshwater ecosystem services in Chile. Climate change is expected to reduce access to freshwater, with negative

¹Apart from ecosystem service provision disruption, several authors have shown evidence regarding ITP's limited carbon sequestration due to the short life cycle of ITP-derived products (gomez2024), supply chain direct emissions (Lewis et al., 2019), and the loss of other carbon-rich land covers (Heilmayr, Echeverría, & Lambin, 2020). We abstract from this discussion to focus exclusively on the effects ITPs have on a single ecosystem service: freshwater provision.

consequences for human welfare. According to the IPCC (2023a), half of the world’s population already faces severe water scarcity for at least part of the year — a challenge especially acute in regions of South America, including Chile. Furthermore, declining freshwater availability threatens key productive sectors, such as agriculture and industry, and increases the risk of waterborne diseases by limiting access to clean water. These dynamics pose an important risk: a mitigation strategy designed to slow climate change may unintentionally amplify water scarcity, one of its consequences.

To identify the effects of ITP expansion on freshwater availability, we set our study in Chile, a country that has doubled the area covered by ITPs since 1975, largely at the expense of agricultural land, shrubland, and native forest (Heilmayr et al., 2016). This sustained expansion makes Chile an ideal case study for the long-term hydrological effects of plantation forestry, and the policy stakes are high: Chile has pledged to expand tree plantations by 100,000 hectares as part of its Nationally Determined Contributions toward carbon neutrality by 2050 (Durán & Barbosa, 2019; Hoyos-Santillan et al., 2021).

We exploit historical land-cover variation from 1986 to 2015, measuring freshwater availability as net outflow — the difference between streamflow measured at a gauge station and upstream inflows from the corresponding drainage area — thereby isolating the hydrological contribution of each drainage area independently of upstream conditions. We estimate a three-year average panel model with subcatchment fixed effects, which absorb time-invariant determinants of both plantation location and hydrology. A residual identification concern is that the timing of ITP expansion may respond to time-varying local conditions, such as economic shocks or anticipated changes in land productivity, that also affect net outflow. We address this using a shift-share instrument that interacts baseline land suitability with changes in international pulp prices, ensuring that identified variation in expansion timing is driven by global price shocks rather than local conditions. We complement this with a long-difference approach that compares subcatchments over a 30-year horizon, using baseline terrain suitability as an instrument. In both specifications, we disaggregate ITP expansion by source land cover — distinguishing conversions from agriculture, native forest, and shrubland — to study whether the hydrological effect of

plantation expansion depends on what it replaces.

Our results consistently indicate a delayed but robust negative effect of ITP expansion on freshwater availability. A one percentage point increase in ITP conversion reduces annual normalized net outflow by approximately $0.011 \text{ Mm}^3/\text{year}$ per km^2 after 7 to 9 years — equivalent to roughly 7% of mean annual net outflow at observed expansion rates. The effect varies meaningfully by source land cover: conversions from agriculture and shrubland, which contribute relatively little to evapotranspiration, produce per-unit effects approximately three times the average, whereas conversions from native forest generate a more modest incremental impact. Long-difference estimates corroborate these findings over a 20-year horizon, with results consistent across two independently constructed land-cover datasets.

Our research makes three contributions to the current literature. First, we provide novel quasi-experimental evidence on the hydrological effects of ITP expansion, addressing identification challenges that have limited previous research in this area. A large body of literature has found suggestive correlations indicating a negative impact of ITPs on freshwater availability in Chile (Alvarez-Garreton et al., 2019; Esquivel et al., 2020; Lara et al., 2009, 2021; Martínez-Retureta et al., 2020). While valuable, this existing research does not address certain identification challenges. It implicitly assumes that tree-plantation allocation is exogenous to freshwater availability and lacks a counterfactual (i.e., what would have happened had a different land-cover been adopted). We address these challenges with two models that exploit quasi-random variation in land cover to study the long-term effects of ITP expansion.

Our second contribution is to extend current knowledge of the impacts of ITPs on ecosystem services. Heilmayr, Echeverría, and Lambin (2020) found that an ITP subsidy introduced in Chile in 1975 reduced biodiversity without increasing carbon storage. We complement this by documenting a previously unquantified cost: the reduction in freshwater provision. This matters not only as an ecosystem service in its own right, but also because freshwater availability is closely linked to welfare outcomes — previous research has found that ITP expansion has increased poverty by displacing more labor-

intensive agricultural activities (Anríquez et al., 2021; Figari, 2025), and water scarcity could compound these effects further.

3.2 Industrial tree plantations, water cycle and land cover change in Chile

Forests and shrublands retain water in their root systems, increasing soil moisture. This allows deep soil water reserves to recharge and gradually release water to watersheds. However, a monoculture plantation of exotic species can fail to maintain this biogeophysical cycle due to the species' characteristics. The first mechanism through which pines and eucalyptus interfere with freshwater provision is higher water consumption. Varieties used in ITPs adapt well to different soils and exhibit a higher growth rate than local species, consuming more water to sustain this growth (Scott, 2005). The second mechanism is evapotranspiration, the total amount of water that evaporates from the soil and transpires from plants within a given area. Pines and (especially) eucalyptus exhibit a higher evapotranspiration coefficient than alternative land covers in the country (Balocchi et al., 2023).

Taken together, higher water consumption and higher evapotranspiration imply that pine and eucalyptus plantations may consume and return to the atmosphere a larger share of available water than native vegetation. Several studies suggest that this can reduce deep soil moisture (Iroumé et al., 2021) and groundwater recharge (Scott & Lesch, 1997), with possible downstream implications for surface-water availability over longer time horizons. At the same time, there is little evidence that these plantations offset increased water use through enhanced local rainfall via “rainfall recycling”. In contrast to mature native forests, industrial timber plantations are typically harvested on relatively short rotations (around 20 years), which may limit their capacity to contribute meaningfully to rainfall generation through atmospheric feedbacks.

3.2.1 Industrial tree plantations' expansion in Chile

Pine and eucalyptus plantations in Chile began in the late 1800s in the country's central-south zone (Lara & Veblen, 1993). In 1938, the Chilean government implemented the

first law to promote private timber companies. This policy was further expanded during the 1960s. In 1974, the dictatorship introduced legislation (DL701) to boost the forestry industry. This law defined economic incentives for ITPs in “forestry-suitable land”, defined as all land that is not arable. It provided a 75% subsidy for afforestation costs, a land tax exemption for ITPs, and a range of additional subsidies for ITP management and pruning. This law also established economic incentives for replanting after logging and prevented the expropriation of land.

As a result, the forest industry expanded rapidly in the center and center-south zones of the country, between parallels 32 and 45 south. This area comprises 9 administrative regions (the country’s larger political subdivisions). In these regions, land covered by ITPs almost doubled between 1975 and 2020. This expansion took place at the expense of shrubland (31.0%), agricultural land (45.1%), and native forest (22.3%) (Heilmayr et al., 2016). Overall, the area of land covered by ITPs increased at a rate of 4.3% between 1986 and 2001, and at a rate of 2.2% between 2001 and 2011 (Heilmayr et al., 2016). In the first stage (1975 - 1990), this expansion occurred in areas near existing sawmills and roads. After 1990, plantations spread irregularly (Nahuelhual et al., 2012).

ITP expansion was higher in the Maule and Bío-Bío regions, where this industry first developed (Nahuelhual et al., 2012). Figure 3.1 shows the nine regions where the ITP industry has evolved, from Valparaíso to Los Lagos. Maule and Bío-Bío are highlighted in orange.

3.3 Empirical Strategy

To study the impact of ITP on freshwater resources, we set our unit of analysis to water-flow measurements collected at gauge stations within our study area. These water-flow measurements will depend on changes in land cover within the corresponding upstream *subcatchment* and on weather conditions. A *catchment* corresponds to the topographic land area in which all surface runoff and groundwater flow to a single downstream point — a gauge station in our setting. A *subcatchment* is the non-overlapping unit within this system, such that every grid cell in the study area belongs to a unique subcatchment.

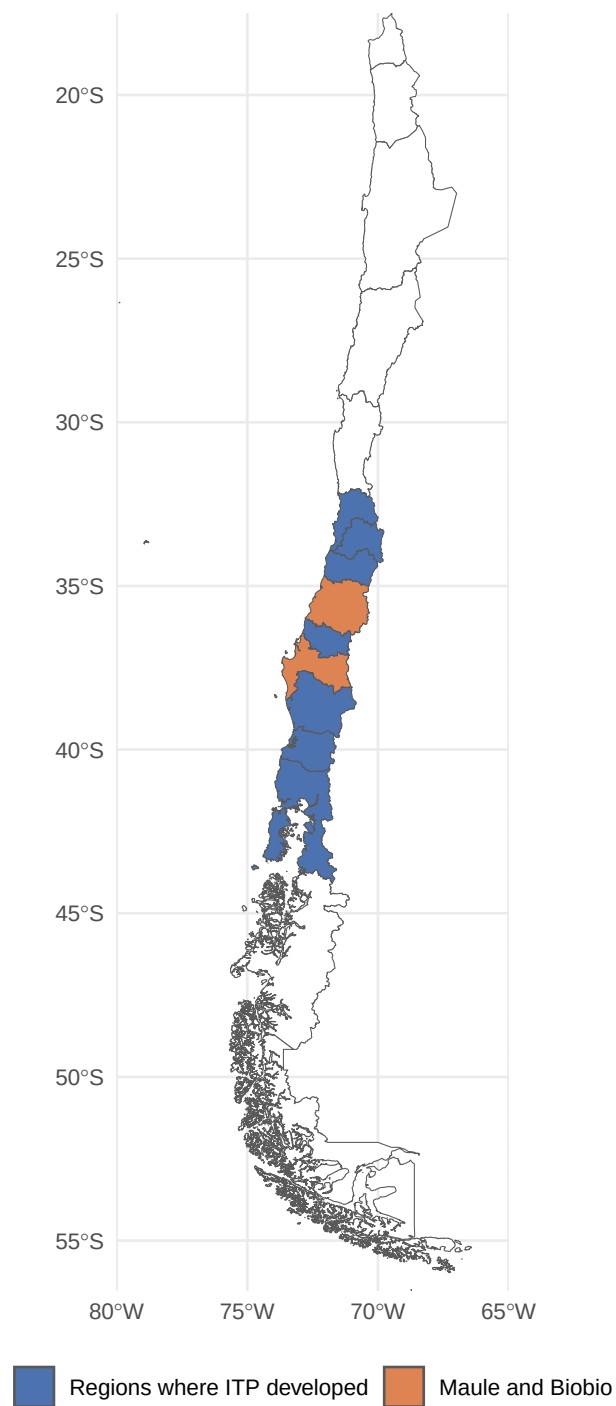


Figure 3.1: Regions where the industrial tree plantations industry developed

Regions where industrial tree plantations developed present better pine and eucalyptus planting conditions. A region is the biggest political division in the country.

Working at the subcatchment level ensures that each unit of land area is attributed to a single gauge station, preventing double-counting of land-cover changes across nested catchments. Because streamflow at any gauge still incorporates upstream contributions, we calculate the net outflow (the difference between outflow and inflow from the immediately upstream subcatchment) as our measure of freshwater availability. We finally define a *basin* as the largest catchment that may contain one or more subcatchments and is not nested within any other catchment.

3.3.1 Net Outflow Model for Subcatchments

We begin by outlining a simple water balance model, which provides the conceptual foundation for our empirical strategy. Our outcome of interest is net outflow — the difference between streamflow measured at a gauge station and upstream inflows, which serves as our measure of freshwater provision at the subcatchment level. Throughout our paper, we measure water outflow as the total annual volume recorded at a given gauge station, in millions of cubic meters ($Mm^3/year$). To derive this quantity, we start from the annual water balance of a basin:

$$\frac{\Delta S}{\Delta year} = P - ET - Q, \quad (3.1)$$

where $\frac{\Delta S}{\Delta year}$ denotes the change in water storage in a year, P is precipitation (total annual rainfall), ET is evapotranspiration, and Q represents surface runoff. Over an annual time horizon, changes in water storage are small relative to total flows. We therefore assume $\frac{\Delta S}{\Delta year} \approx 0$, which is standard in applied hydrology at this scale (Han et al., 2020).

Because our unit of analysis is a subcatchment associated with a streamflow gauge, it is useful to express runoff in terms of stream inflows and outflows. Let Q^{in} denote total upstream inflows into the subcatchment and Q^{out} the outflow. The water balance for a subcatchment can then be written as

$$0 = Q^{in} + P - ET - Q^{out}. \quad (3.2)$$

Rearranging this expression yields a relationship for net outflow:

$$Q^{out} - Q^{in} = P - ET \quad (3.3)$$

To take this to the data, we need to be more precise about what drives ET , and what other outflows are relevant at the subcatchment level. We decompose Q^{out} into two components: Q_{gauge}^{out} , which represents the outflow measured at the gauge station, and human consumption (U). We also decompose evapotranspiration into water-body evapotranspiration (ET^{water}), land-cover evapotranspiration (ET^l), and water infiltration (I), determined by soil conditions. We can now write our subcatchment net outflow model as:

$$Q_{gauge}^{out} - Q^{in} = P - ET^l - ET^{water} - I - U \quad (3.4)$$

Equation 3.4 provides the theoretical basis for our estimating equation, but its right-hand side contains terms we do not observe directly. We therefore express each component as a function of observable variables. Land-cover evapotranspiration ET^l depends on the extent and type of land cover — including ITP, agriculture, and other classes — and on temperature. Water-body evapotranspiration ET^{water} is similarly a function of temperature. Infiltration I reflects soil characteristics and aridity conditions, which we treat as time-invariant at the subcatchment level. Human consumption U is proxied by urban extent and the presence of dams and reservoirs, which can also directly alter outflow levels. Substituting these approximations into Equation 3.4 and absorbing time-invariant soil and aridity conditions into a subcatchment fixed effect ϕ_i yields the following estimable model:

$$y_{it} = \beta \mathbf{ITP}_{it} + \mathbf{W}'_{it}\eta + \mathbf{X}'_{it}\theta + \phi_i + \varepsilon_{it}. \quad (3.5)$$

In Equation 3.5, $y_{it} = Q_{gauge}^{out} - Q^{in}$ corresponds to the total annual net flow outcome in subcatchment (i), within basin b at year (t). $\mathbf{ITP}_{it}$ corresponds to conversions of other land covers to ITP. $\mathbf{W}'_{it}$ is a vector that includes total annual precipitation, and maximum and minimum temperature. $\mathbf{X}'_{it}$ is a vector that includes urban land cover and

the presence of dams and reservoirs. The subcatchment fixed effect ϕ_i absorbs the time-invariant soil and aridity conditions we approximated above, completing the mapping from the theoretical model to the estimating equation.

Subcatchments cover large areas, so only substantial cumulative ITP expansion is likely to produce a detectable signal in net outflow. Additionally, the relationship between land cover change and water outflow is slow-moving. This pushes us toward empirical strategies that emphasize long-run, large-scale variation rather than year-to-year fluctuations. We employ two complementary approaches: a three-year average panel, which reduces noise from water-gauge errors derived from the machine-learning algorithm while preserving within-unit variation over time, and long differences, which capture the cumulative effect of land-cover change over extended horizons. For each model, we estimate the effects of ITP expansion on the full annual period and during the summer season, when water flow levels are lower.

While the water balance model treats land cover as a stock — the total area under a given cover type determines ET continuously — we operationalize $\mathbf{ITP}_{it}$ in two ways that reflect different empirical strategies. In the panel specification, we use annual conversions to ITP, which captures the flow of new plantation area and is better suited to identifying short- to medium-run effects within subcatchments over time. In the long-differences specification, we use cumulative conversions, which approximate the stock of ITP area and are more appropriate for capturing the total accumulated effect over longer horizons. Both measures are grounded in the same theoretical relationship; they differ in how they partition the variation in plantation cover across time and units. As a robustness check, we also consider the share of the total subcatchment area covered by ITP, which provides a scale-invariant measure of plantation intensity that is directly comparable across subcatchments of different sizes.

3.3.2 Three-year average panel

With the estimating equation established, we now turn to implementation. A central challenge is that ITP expansion is not random — firms select plantation locations based on terrain, rainfall, and proximity to processing infrastructure, all of which may inde-

pendently affect net outflow. We address this endogeneity concern using a shift-share instrument, described below. We also face two measurement challenges: streamflow data contain gaps (which we address through imputation; see Section 3.4.1 for details), and the hydrological response to plantation expansion is slow. Averaging over three-year windows reduces the influence of imputed values and measurement noise in the streamflow data while preserving medium-run variation in land cover and plantation expansion. It also allows us to capture the delayed effect of new plantations on water outcomes. A three-year window is consistent with previous research that has detected reductions in runoff after 3 years of eucalyptus being planted (Scott & Lesch, 1997).

In experimental settings, peak water reduction from eucalyptus plantations is reached after 7–9 years (Scott, 2005; Scott & Lesch, 1997). To capture this delayed hydrological response, we include up to two lags of the treatment variable, allowing the effect of plantation expansion to accumulate over successive three-year periods.

Formally, for subcatchment i in period g , we compute three-year averages for all variables. Define the starting year of period g as $\tau_g = 1986 + 3(g - 1)$, $g = 1, 2, 3, \dots$. So $\tau_1 = 1986$, $\tau_2 = 1989$, $\tau_3 = 1992$, and so on. Then the three-year average is:

$$\bar{a}_{ig} = \frac{1}{3} \sum_{t=\tau_g}^{\tau_g+2} a_{it}$$

We normalize all land-cover variables to the area share to have intercomparable units of analysis. We also normalize net outflow by subcatchment area to ensure comparability across subcatchments of different sizes. Substituting into Equation 3.5 and applying this averaging yields:

$$\bar{y}_{ibg} = \sum_{s=0}^2 (\beta_s \mathbf{ITP}_{ibg-s}^{lc}) + \bar{\mathbf{W}}'_{ibg} \eta + \bar{\mathbf{X}}'_{ibg} \theta + \alpha S_i + \phi_i + \varepsilon_{ibg}. \quad (3.6)$$

Here $\bar{y}_{ibg}$ is area-normalized net outflow averaged over period g . $\mathbf{ITP}^{lc}$ is the conversion rate from source land cover lc (agriculture, native forest, shrubland, and total conversion)² to ITP, normalized by subcatchment area and averaged over period $g - s$. Estimating

²We define total ITP conversion as the sum of agriculture, native forest, and shrubland conversion.

separate regressions for each source cover allows us to test whether the hydrological effect of *ITP* expansion differs depending on what it replaces. $\bar{\mathbf{W}}_{ig}$ includes weather controls, and $\bar{\mathbf{X}}_{ig}$ includes urban land cover and dam and reservoir presence, as in Equation 3.5. The subcatchment fixed effect ϕ_i absorbs time-invariant soil and climatic conditions within subcatchment i .

The coefficient β_s captures the effect on area-normalized net outflow of a one percentage point increase in the share of subcatchment area converted from land cover lc to ITP, s periods prior, holding weather, infrastructure, terrain, and basin conditions constant.

A central identification concern is that plantation expansion is nonrandom and may respond to observed and unobserved determinants of hydrological outcomes. Since ITPs are rain-fed, firms are more likely to expand into areas with favorable rainfall and terrain, and locate near processing infrastructure — all of which may independently affect net outflow. The subcatchment fixed effect ϕ_i absorbs all time-invariant sources of this confounding, including persistent differences in rainfall, soil type, topography, and land suitability. To the extent that endogeneity arises from stable subcatchment characteristics, the most plausible channel in this setting, ϕ_i eliminates it. We additionally control for precipitation to account for year-to-year rainfall variation within subcatchments.

A residual concern is whether firms selected locations based on expected *future* hydrological conditions rather than current ones. This anticipation channel is likely limited. Anticipation bias would require investors in the 1980s and 1990s to have had access to reliable, spatially granular forecasts of long-run changes in subcatchment-level precipitation or outflows, which seems unlikely in this setting. Moreover, any time-invariant component of such forward-looking selection (e.g., firms consistently targeting subcatchments with favorable long-run water availability) is absorbed by ϕ_i . Still, even if forward-looking selection is unlikely, concerns may remain about time-varying unobservables—such as local economic shocks that simultaneously drive plantation expansion and affect water use. To address these residual sources of endogeneity, we instrument ITP expansion with a shift-share measure that interacts baseline expansion potential with changes in international pulp prices.

The exposure term is the share of subcatchment land with slopes between 12% and 30% that was not yet under plantation in 1986, capturing remaining suitable terrain available for expansion. We focus on this slope range — rather than the broader 12% to 60% range that defines agronomic suitability for tree plantations (Stolpe, 2011) — because moderately sloped terrain is more accessible for machinery and harvesting operations, making it the most responsive margin to price incentives. The shift is the change in FOB pulp export prices over time. The subcatchment fixed effect also controls for the overall share of land within the 12% to 30% slope range, isolating expansion potential from general terrain characteristics, and for distance to pulp mills operating before 1986, absorbing any direct effect of proximity to processing infrastructure on net outflow.

The relevance condition is that increases in pulp prices induce greater plantation expansion where more suitable remaining land is available. Identification therefore comes from within-subcatchment changes over time in exposure to common international pulp-price shocks, differentially scaled by baseline expansion potential. Because the exposure component of the instrument, the share of suitable land available for expansion in 1986, is fixed prior to the sample period, its level effect is absorbed by the subcatchment fixed effect. The first stage is thus identified by comparing how plantation expansion within the same subcatchment changes over time as international pulp prices fluctuate, with subcatchments that had greater remaining suitable land in 1986 responding more strongly to these common price shocks.

The exclusion restriction requires that, conditional on controls and the subcatchment fixed effect, international price changes affect net outflow only through induced plantation expansion. This is supported by the fact that FOB prices are set in international markets, where Chilean producers are price-takers, and that most of the cellulose produced is exported (Figure S3.1). Since Chile accounts for a small share of global pulp supply (see Figure S3.2), domestic hydrological conditions have no plausible effect on world prices. We find no statistically significant correlation between annual log changes in world pulp import prices and Chilean export quantities (see Figure S3.3). Together, the suitability share and the international price shock isolate the demand-driven component of ITP

expansion that is orthogonal to local determinants of net outflow.

Our specification includes subcatchment fixed effects ϕ_i but not period fixed effects. The identifying variation in the shift-share instrument comes from differential exposure across subcatchments to changes in international pulp prices over time. Subcatchment fixed effects absorb all time-invariant differences across units, including the baseline expansion-potential measure used in the instrument’s exposure component. Adding period fixed effects would additionally absorb the common time-series variation in pulp prices, leaving identification to rely only on differential responses across subcatchments within each period.

Given the relatively small panel dimension (128 subcatchments observed over 10 periods), introducing a full set of period fixed effects would absorb a substantial share of the remaining identifying variation in the instrument. We therefore rely on subcatchment fixed effects, together with controls for precipitation and other time-varying covariates, to account for the most plausible sources of confounding in this setting.

Our instrument uses lagged FOB prices $\Delta F\bar{O}B_{g-1}$ to instrument for contemporaneous ITP expansion $IT\bar{P}_{ig}^{lc}$, while lagged expansion terms $IT\bar{P}_{ig-s}^{lc}$ for $s > 0$ enter uninstrumented. Using lagged prices is a conservative choice that further insulates the instrument from any concern that current expansion responds to current outflow shocks.

We include lagged expansion terms directly, which requires a sequential exogeneity assumption: conditional on controls and fixed effects, past expansion is uncorrelated with the current structural outflow shock. This is plausible in our setting because planting is a long-horizon investment, and adjustment to short-run hydrological fluctuations is limited.

The main threat would be a persistent unobserved shock — such as a multi-year drought — that simultaneously reduces net outflow and discourages planting. We consider this unlikely given that planting decisions are made years in advance and respond to international market conditions rather than local water availability. A potential concern is that the “mega-drought” beginning in 2011 (Garreaud et al., 2020) could induce a correlation between lagged expansion and current outflow shocks. In our 3-year panel, this episode is confined to the terminal part of the sample, limiting its scope.

With the identification strategy established, we formally describe the instrument:

$$Z_{ig} = \Delta F\bar{O}B_{g-1} \times S_i^r,$$

where $\Delta F\bar{O}B_{g-1}$ is the lagged period change in FOB export prices and S_{ib}^r is the share of remaining suitable land in subcatchment i in 1986. The first stage regresses $I\bar{T}P_{ibg}^{lc}$ on Z_{ig} and the full set of controls in Equation 3.6, and the second stage replaces $I\bar{T}P_{ig}^{lc}$ with its fitted values. Standard errors are clustered at the subcatchment level to account for serial correlation in net outflow within subcatchments over time.

3.3.3 Long differences

We complement our three-year average panel with a long differences strategy (Burke & Emerick, 2016) to assess the long-term effects of ITP on net outflow. Averaging over extended windows further reduces the influence of imputed values and measurement noise in the streamflow data, and attenuates persistent pixel-level misclassification errors in the land-use data (Taylor & Druckenmiller, 2022). Beyond data quality, there is a substantive reason to look at longer horizons: hydrological responses to land-cover change unfold gradually as soil structure, infiltration capacity, and groundwater recharge adjust over time, so long differences are better suited to capturing cumulative effects than year-to-year variation. Finally, differencing between two distant periods directly purges time-invariant subcatchment heterogeneity.

Starting from Equation 3.5, we compute window averages over n years beginning in year s for each variable:

$$\bar{a}_i = \frac{1}{n} \sum_{t=s}^{s+n-1} a_{it}.$$

Taking the difference between end-window and start-window averages, the subcatchment fixed effect ϕ_i and terrain steepness S_i cancel by differencing since both are time-invariant. Letting α_0 denote the difference in common time trends and ν_i the error term, this yields:

$$\begin{aligned}\bar{y}_{i2} - \bar{y}_{i1} &= \alpha_0 + \beta(\overline{ITP}_{i2} - \overline{ITP}_{i1}) + (\overline{\mathbf{W}}'_{i2} - \overline{\mathbf{W}}'_{i1})\eta + (\overline{\mathbf{X}}'_{i2} - \overline{\mathbf{X}}'_{i1})\theta + \nu_i \\ \Delta\bar{y}_i &= \alpha_0 + \beta\Delta\overline{ITP}_i^{lc} + \eta\Delta\overline{\mathbf{W}}_i + \Delta\overline{\mathbf{X}}_i'\theta + \nu_i.\end{aligned}\tag{3.7}$$

In the long-differences specification, ITP_i^{lc} is defined as the cumulative area converted from land cover lc to ITP, normalized by subcatchment area. For example, if 1 percent of agricultural land is converted to ITP in year t and another 1 percent in year $t + 1$, cumulative conversion stands at 1 percent in the first period and 2 percent in the second, yielding a window average of 1.5 percent. Taking long differences in this variable, therefore, captures the change in the average cumulative plantation area across the two windows. As in the panel specification, we estimate separate regressions for each source land cover and complement the analysis with the ITP area share as a robustness check.

The coefficient β captures the long-run effect on area-normalized net outflow of a one percentage point increase in the cumulative share of subcatchment area converted from land cover lc to ITP between the start and end windows.

Since ITPs operate on approximately 20-year rotation cycles, differences spanning multiple rotations would capture expansion followed by harvest and replanting, introducing mean reversion in cumulative ITP conversion and understating the true treatment effect. We therefore use 10-year windows, equivalent to half a rotation cycle, which captures the accumulation phase without crossing into harvest.

We estimate Equation 3.7 by OLS. Because long differencing removes all time-invariant subcatchment heterogeneity, the primary endogeneity concern from the panel — firms selecting locations based on stable terrain, rainfall, and infrastructure characteristics — is resolved by construction. The error term ν_i captures the change in net outflow between windows, so only a confounder that predicts trends in freshwater flow, rather than its level, could bias the estimate. This would require firms to have anticipated and responded to multi-decade hydrological trends when making planting decisions in the 1980s and 1990s, which we consider implausible given the absence of reliable long-run forecasts and the fact that expansion was driven by international market conditions.

3.4 Data

We compute our main variables using the subcatchments defined by a gauge station. Gauge stations and their daily water-flow observations were obtained from publicly available records compiled in the CAMELS-CL dataset (Alvarez-Garreton et al., 2018). This dataset also contains polygons defining the catchments drained by each gauge station. Land-cover data are drawn from *CR2LUC*, a novel dataset comprising yearly panel data on a 0.01 square grid (Boisier et al., 2024). We finally obtain daily weather observations (precipitation and temperature) from the *CR2MET* dataset (Boisier, 2023). We summarize the sources of the variables we employ in this study in the Appendix (Table S3.1).

3.4.1 Dependent variable: Net outflow

We measure freshwater availability as the total net annual outflow recorded at a given gauge station, in millions of cubic meters ($Mm^3/year$). Net annual outflow is defined as the outflow at a given station minus the inflows recorded at immediately upstream stations. Outflow is the sum of daily water-flow observations at a station within a hydrological year (April through March), and inflow is the corresponding sum from direct upstream subcatchments. In short, net annual outflow captures the total volume of water measured at a given station in a year, net of the volume measured at direct upstream stations in the same year.

Daily streamflow records in Chile are incomplete for several reasons, including equipment failures, power outages, extreme weather events, and limited access to remote logger locations (Arriagada et al., 2021). To input missing data and obtain a complete panel, we impute missing daily observations using MissForest (Stekhoven & Bühlmann, 2012), a nonparametric iterative imputation algorithm. This algorithm was validated for daily streamflow reconstruction in central and southern Chile by Arriagada et al. (2021). To select the gauge stations we work with, we restrict our sample to stations with at least 75% valid observations between 1986 and 2015, yielding 128 stations. This threshold ensures sufficient data quality while retaining enough stations for analysis. Under this threshold, we impute 8.8% of daily observations across the 128 gauge stations in our sample. Further details of the imputation procedure are provided in the Appendix 3.2.3. In addition to

annual net outflow, we also compute summer inflow and outflow variables by summing water flow measurements during the summer months (November–February).

To construct the drainage map identifying upstream-downstream relationships between subcatchments, we define a subcatchment’s downstream neighbor as the closest subcatchment polygon within 1 kilometer of its gauge station. If no subcatchment falls within this radius, we assume the subcatchment has no downstream station in the sample. We manually reviewed and corrected 7 subcatchments exhibiting consistency problems.

We normalize net outflow by subcatchment area (km^2), yielding cumulative discharge per unit area ($Mm^3/year$ per km^2). Since subcatchments vary substantially in size — with areas ranging from 81 to 6785 km^2 in our sample — raw outflow figures are not directly comparable across units. Normalization accounts for the correlation between subcatchment size and annual net outflow.

3.4.2 Unit of measurement: subcatchments

We construct subcatchment polygons for the 128 gauge stations that comprise our sample. The CAMELS-CL database provides *catchment* polygons for each gauge station, where each polygon captures the total drainage area upstream of a gauge, including areas attributed to nested upstream gauges. We obtain our *subcatchments* by subtracting, from each catchment polygon, all smaller polygons whose area overlaps with it by at least 99%.

Altogether, the 128 subcatchments cover 58.1% of the study area, with higher concentration in the regions where the ITP industry has developed most — Maule, Ñuble, Bío-Bío, and Araucanía (see Figure 3.2 and Table S3.2). Subcatchments have a mean area of 986.9 km^2 .

3.4.3 Land cover variables

We calculate the area (km^2) of each land-cover class within each subcatchment by intersecting our subcatchment polygons with the *CR2LUC* dataset (Boisier et al., 2024). This dataset contains annual panel data (1950–2020) on land-cover fractions at 0.01 grid resolution. It identifies 48 land uses grouped into eight categories that we use in our study: agricultural land, forest, shrubland, natural grassland and tundra, built-up land, barren

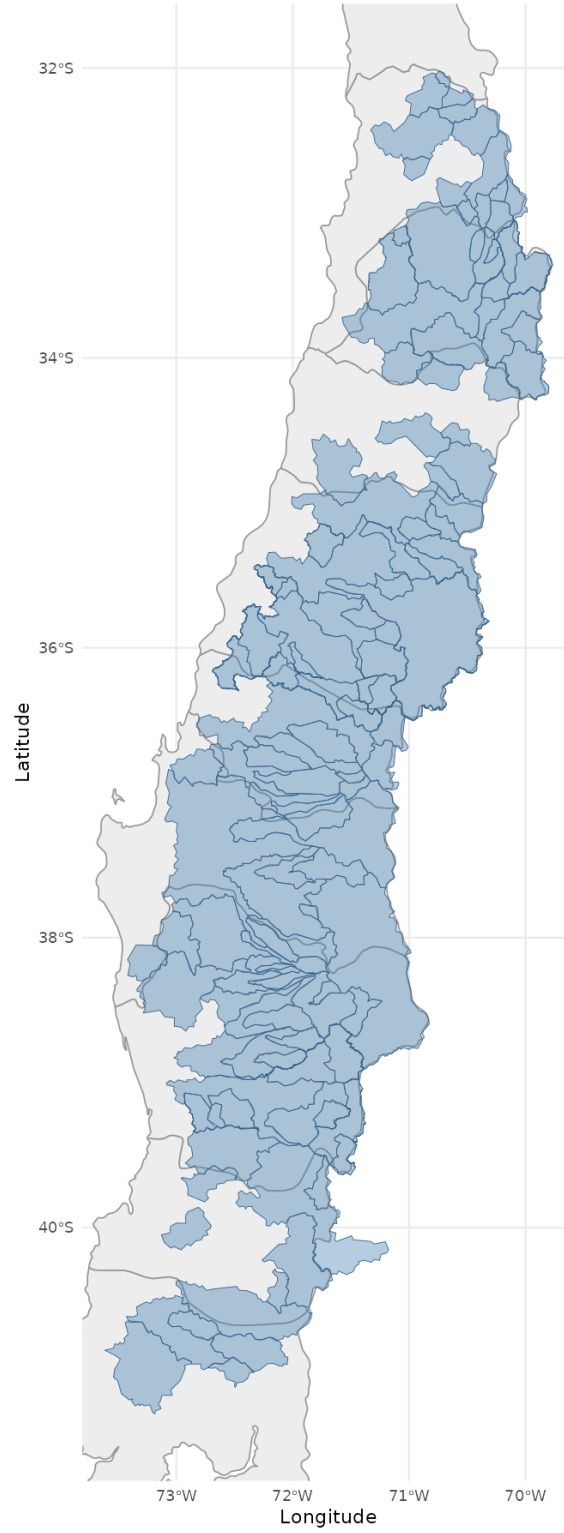


Figure 3.2: Map of subcatchments over administrative regions in the study area

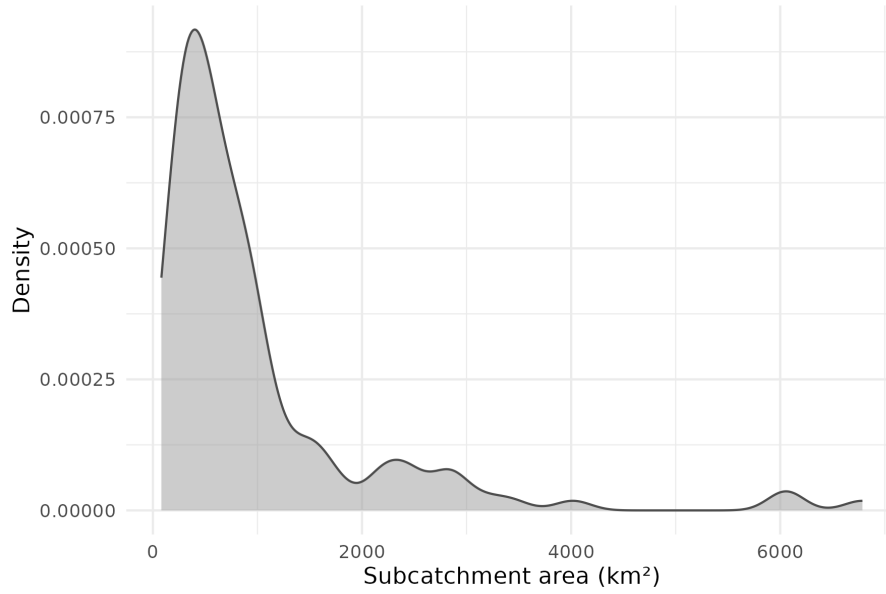


Figure 3.3: Subcatchments' area distribution. Sample of 128 subcatchments

land, water bodies and salt pans, and glacier and permanent snow. Forest is divided into two subcategories: native forest and tree plantations. This dataset was generated using a machine learning algorithm trained on satellite and historical data from multiple sources.

In addition to these annual *level* variables, we construct *conversion* variables that measure, for each land-cover class, how much area arrived from a different class. This allows us to ask not only whether ITP expanded, but also where that expansion originated: agricultural land, native forest, or shrubland. A net increase in plantation area may obscure distinct conversion histories across origin classes, which may have different effects on water resources.

For each consecutive year pair in the CR2LUC panel (1986–2015), we allocate observed changes in land-cover fractions across feasible origin–destination pairs using an iterative proportional fitting (IPF) algorithm. IPF finds the unique transition matrix that simultaneously satisfies the total area leaving each origin class and the total area arriving at each destination class, restricting flows to feasible pairs. The conversion variables are the cumulative sum of these annual flows over the comparison window.

To determine which conversions are feasible, we build a transition feasibility matrix using 30-m pixel rasters from Heilmayr (2024) for 1986, 2001, and 2011. We compute

pixel-level crosstabs for 1986–2001 and 2001–2011 and retain any transition that exceeds 0.05% of total pixel observations in either period. We additionally remove transitions that are ecologically implausible in our study region and add transitions involving grassland, which has no equivalent in Heilmayr’s classification. The resulting feasibility matrix is shown in Table S3.3.

We normalize all level and conversion variables by subcatchment area to obtain land-cover shares, allowing comparisons across subcatchments of different sizes.

3.4.4 Weather variables

We aggregate total annual and summer precipitation for each subcatchment on a hydrological-year basis using *CR2MET v2.5* gridded data at 0.05 resolution (Boisier, 2023). Daily precipitation is area-weighted across subcatchment pixels. We calculate annual and summer average maximum and minimum temperatures (°C) from the same grid by averaging all daily values within the hydrological year for each subcatchment pixel.

3.4.5 Averaging variables for 3-year panel and long differences

We collapse all variables into non-overlapping 3-year periods (1986–1988, 1989–1991, ..., 2013–2015). Precipitation, temperature, net outflow, and land-cover level variables are computed as the mean of annual values within each period. Likewise, we get averages for the starting and ending time windows of our long differences.

Land-cover conversion variables require more care. A simple difference in period means (e.g., plantation area in 1989–1991 minus plantation area in 1986–1988) measures the *net* change but can be zero even when large amounts of land were converted in both directions. We therefore construct *gross* conversion variables as the cumulative sum of annual IPF flows across all years between consecutive periods. These conversion variables measure the total area that moved from each origin class to each destination class, including flows that cancel out in the net difference.

Table 3.1: Land-cover shares across subcatchments and change between 1986–1988 and 2013–2015

	1986–1988	1998–2000	2013–2015	Δ
<i>Subcatchment characteristics (km²)</i>				
Subcatchment area	986.9 (1127.9)	986.9 (1127.9)	986.9 (1127.9)	
<i>Land cover (% of subcatchment area)</i>				
Agricultural	14.7 (15.9)	13.7 (15.1)	13.1 (14.8)	-1.6***
Native forest	36.7 (23.2)	34.8 (22.9)	33.7 (23.0)	-3***
Plantation	7.7 (11.7)	11.1 (15.3)	13.7 (17.8)	6***
Shrubland	14.3 (12.7)	13.9 (12.7)	13.0 (12.0)	-1.3***
Grassland	5.2 (6.4)	5.1 (6.4)	5.0 (6.4)	-0.3***
Built-up	0.5 (1.4)	0.5 (1.6)	0.8 (2.1)	0.3***
Barren	14.8 (23.3)	14.8 (23.3)	14.8 (23.3)	0
Water	1.5 (3.1)	1.5 (3.1)	1.4 (3.1)	-0.1***
Glacier	1.4 (3.7)	1.4 (3.7)	1.4 (3.7)	0

Standard deviations in parentheses. Δ is the difference between the 2013–2015 and 1986–1988 period means. Significance of Δ : * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Paired t -test against $H_0: \Delta = 0$.

3.4.6 Changes in net outflow and land-cover (1986 - 2015)

Table 3.1 shows that the share of land dedicated to tree plantations increased by 6% on average across the subcatchments between 1986 and 2015. Conversely, the largest net declines have been in native forest (−3.1%), agricultural land (−1.6%), and shrubland (−1.3%). Consistent with our assumptions, changes in glacier area are negligible.

Across the different periods, most of the increase in the area covered by tree plantations comes from native forest (51.5%), followed by agricultural land (26.3%) and shrubland (18.9%) (Table 3.2).³

³These values differ from Heilmayr et al. (2016)’s previous estimates, which determine that most of tree plantations were allocated on previous agricultural land. The reason for this discrepancy is that the CR2LUC database identifies a larger share of native forest at the expense of agricultural land. While this difference does not affect our main results, it has implications for our heterogeneity analysis, which we discuss in the Conclusion section.

Table 3.2: Industrial tree plantation expansion by previous land cover

Previous land cover	1989–91	1992–94	1995–97	1998–00	2001–03	2004–06	2007–09	2010–12	2013–15	Pooled
Agricultural	23.3	24.3	25.5	27.0	27.0	26.5	25.8	26.8	28.4	26.3
Native forest	55.1	54.4	53.4	52.3	51.4	50.9	50.8	49.9	48.5	51.5
Shrubland	18.3	18.1	17.9	17.6	18.3	19.1	20.0	19.9	19.6	18.9
Grassland	3.2	3.1	3.0	3.0	3.0	3.2	3.2	3.2	3.2	3.1
Barren	0.1	0.1	0.2	0.2	0.2	0.2	0.2	0.2	0.2	0.2
Net plantation change (km ²)	1026.5	997.5	950.3	817.4	586.2	764.0	908.8	845.8	732.3	7628.8

Columns report each prior land-cover class’s share of area converted to tree plantation in each three-year transition period. Shares sum to 100 within columns, subject to rounding. The Pooled column reports shares computed from aggregate conversions over the full 1989–2015 period. The final row reports the net change in plantation area (km²) per period.

Figure 3.4 shows the change in distribution of annual outflow and the most relevant land-cover variables in our model. The first panel shows that the weighted annual outflow distribution shifts leftward, indicating an overall reduction in water level at each sub-catchment’s outlet. Additionally, we observe an increase in tree plantations, accompanied by meaningful reductions in agricultural land and native forest.

Finally, Table 3.3 shows reductions in average precipitation across subcatchments and an increase in maximum and minimum temperatures. The main reduction identified is in annual precipitation, which decreased by 17.1% between the start and end of our sample period. Net outflows indicate small differences between inflows and outflows, suggesting that the effect of tree plantations on water flow may be limited, yet substantial in water storage over time. Nevertheless, we observe a 21.7% decrease in net outflow over the study period.

3.4.7 Land suitability and changes in cellulose prices

To obtain the share of suitable land that is not yet planted, we use a 30-m pixel raster derived from LANDSAT images (Heilmayr, 2024). For each pixel, we get the average slope using elevation data from the European Space Agency and Airbus (2022). We then subtract from the total area with a slope between 12% and 30% the area already covered by ITPs. Using the Heilmayr (2024) dataset yields higher precision in land coverage (compared to the $\sim 1 - km$ grid raster with fraction land cover from CR2LUC).

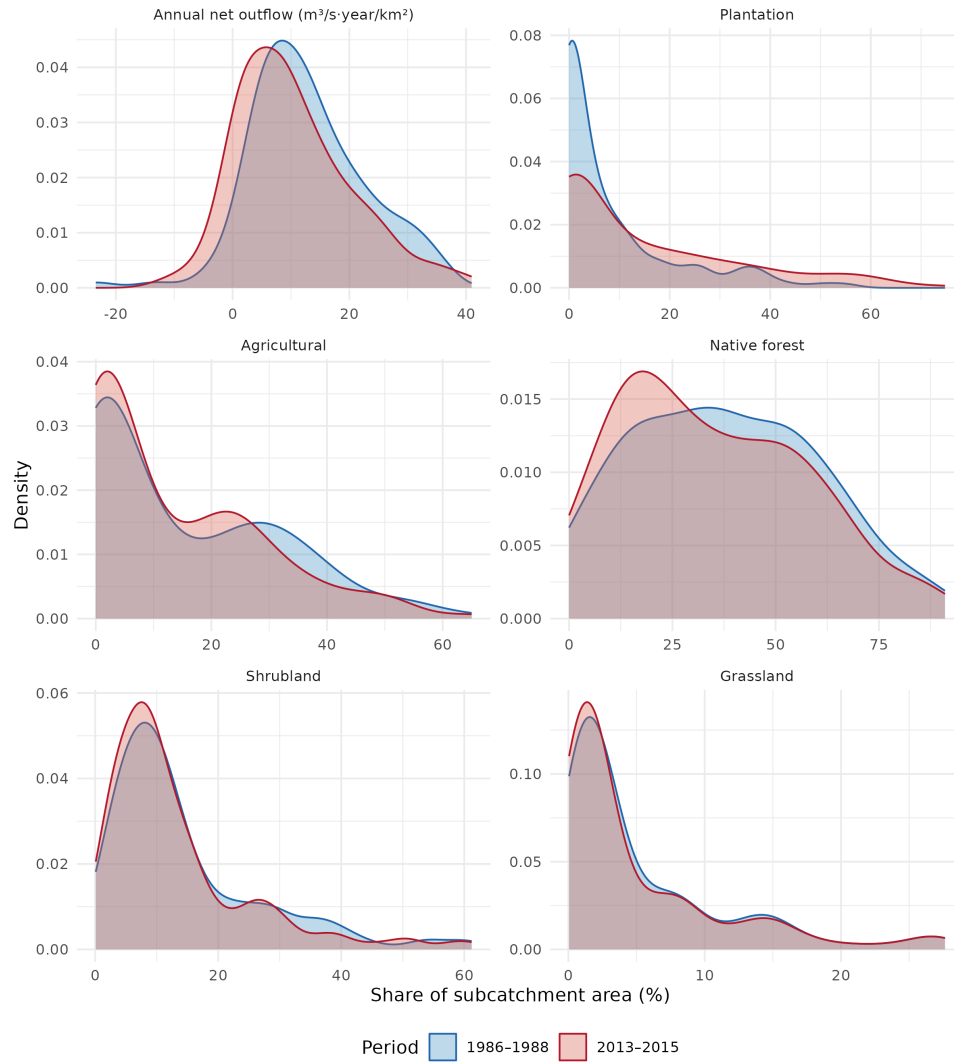


Figure 3.4: Changes in water outflow and land use share distribution 1986 - 2015

First panel shows annual outflow distribution normalized by subcatchment area. Remaining panels show changes in land-cover share for ITP (plantations) and relevant land-cover alternatives.

Table 3.3: Weather and net outflow across subcatchments and change between 1986–1988 and 2013–2015

	1986–1988	1998–2000	2013–2015	Δ
<i>Precipitation (mm)</i>				
Annual precipitation	1633.3 (730.8)	1356.7 (682.4)	1351.6 (743.9)	-281.8***
<i>Temperature (°C)</i>				
Max temperature, annual	15.7 (4.3)	15.7 (4.3)	16.2 (4.3)	0.6***
Min temperature, annual	4.9 (2.2)	4.7 (2.1)	5.0 (2.1)	0**
<i>Flow (Mm³/year per km²)</i>				
Annual net outflow	1.15 (0.86)	0.86 (0.77)	0.90 (0.86)	-0.29***

Standard deviations in parentheses. Δ is the difference between the 2013–2015 and 1986–1988 period means. Significance of Δ : * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Paired t -test against $H_0: \Delta = 0$.

To calculate changes in international cellulose prices, we use FOB export prices for chemical pulp available at INFOR’s (National Forestry Institute) forest export database.⁴ We calculate the annual volume-weighted average of quarterly FOB export unit values to get an annual prices, which we later average over three years. The price shift is defined as the log difference between prices in two periods.

3.5 Results

3.5.1 Panel Results

Table 3.4 presents OLS and shift-share IV estimates of Equation 3.6 for total ITP conversion,⁵ estimated separately for annual and summer net outflow. First-stage estimates, reported in the Appendix, confirm that the shift-share instrument is relevant across all specifications, with F-statistics above 10 in all cases. The results point to a delayed rather than immediate hydrological response to plantation expansion. We observe no statistically significant effect of contemporaneous ITP conversion on net outflow. We observe a

⁴<https://wef.infor.cl/>

⁵As previously described, we define total ITP conversion as the sum of agriculture, native forest, and shrubland conversion.

positive effect of the first lag in the OLS specification, but it is statistically significant only at the 10% level, and it is not robust to the inclusion of the IV. However, a consistently negative and statistically significant effect emerges at the second lag across both OLS and IV estimates — columns (1) and (2) of Table 3.4 — consistent with the 7–9 year horizon for peak water reduction documented in experimental settings (Scott, 2005; Scott & Lesch, 1997). Since the ITP conversion variable is expressed as a proportion of subcatchment area, the coefficient of -1.12 implies that a one percentage point increase in the share of subcatchment area converted to ITP reduces normalized net outflow by approximately $1.12/100 \approx 0.011 \text{ } Mm^3/year$ per km^2 after 7 to 9 years. At the observed average expansion rate of 6 percentage points over our study period, this implies a cumulative reduction of approximately $6 \times 0.011 \approx 0.066 \text{ } Mm^3/year$ per km^2 , equivalent to roughly 7% of mean annual net outflow.

We now disaggregate ITP expansion by source land cover. Table 3.5 presents results for conversion from agriculture to ITP. The pattern is qualitatively similar to the total conversion results: only the second lag is statistically significant and stable across OLS and IV specifications. The effect is, however, substantially larger in magnitude than the total conversion estimate. Using the IV coefficient of -4.04 , a one percentage point increase in the share of subcatchment area converted from agriculture to ITP reduces annual normalized net outflow by approximately $4.04/100 \approx 0.04 \text{ } Mm^3/year$ per km^2 after 7 to 9 years. Agriculture accounts for approximately 26% of ITP conversions in our sample, so this larger coefficient suggests that replacing agricultural land — which contributes relatively little to evapotranspiration compared to forest — with water-intensive eucalyptus plantations produces a proportionally stronger hydrological response.

Table 3.6 presents results for conversion from native forest to ITP. In contrast to the total conversion and agriculture-to-ITP models, we find no statistically significant effect at any lag in either the OLS or IV specifications. First-stage F-statistics, reported in the Appendix, confirm that the instrument remains relevant for this subsample, so the null result is not driven by a weak instrument. Point estimates for the second lag are negative, consistent in sign with the delayed hydrological response found elsewhere, but they are

Table 3.4: Effect of Total ITP Expansion on Net Outflow

	Normalized Net Outflow	
	OLS	IV
	(1)	(2)
Total ITP Conversion (%)	0.23 (0.79)	-0.73 (2.67)
Total ITP Conversion, lag 1	0.82* (0.44)	1.05 (0.78)
Total ITP Conversion, lag 2	-1.07*** (0.35)	-1.12*** (0.38)
Dams and reservoirs controls	Yes	Yes
Climate controls	Yes	Yes
Urban land-use controls	Yes	Yes
Gauge fixed effects	Yes	Yes
N	1,278	1,278
R ²	0.95502	0.95502

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered at the subcatchment level in parentheses.

small in magnitude and imprecisely estimated. This pattern is consistent with the higher baseline evapotranspiration of native forests: replacing them with eucalyptus plantations produces a more modest incremental change in water consumption compared to replacing low-vegetation agricultural land, and the effect may be too small to detect reliably given the variation in our sample. Notably, native forest accounts for approximately 53% of ITP conversions in our sample, so while the per-unit hydrological impact of this margin appears limited, its quantitative importance for the aggregate water balance warrants continued attention.

Table 3.7 presents results for conversion from shrubland to ITP. Shrubland accounts for approximately 18% of ITP conversions in our sample and contributes relatively little

Table 3.5: ITP Expansion on Former Agricultural Land and Net Outflow

	Normalized Net Outflow	
	OLS	IV
	(1)	(2)
Ag $\rightarrow$ ITP (%)	2.87	-2.23
	(1.84)	(10.28)
Ag $\rightarrow$ ITP, lag 1	1.43	2.80
	(0.91)	(2.94)
Ag $\rightarrow$ ITP, lag 2	-3.86***	-4.04***
	(0.78)	(0.91)
Dams and reservoirs controls	Yes	Yes
Climate controls	Yes	Yes
Urban land-use controls	Yes	Yes
Gauge fixed effects	Yes	Yes
N	1,278	1,278
R ²	0.95525	0.95518

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered at the subcatchment level in parentheses.

to evapotranspiration compared to native forest, so its replacement with water-intensive eucalyptus plantations is expected to produce a proportionally stronger hydrological response. Consistent with this, the annual second lag is negative, large, and stable across OLS and IV specifications — using the IV coefficient of -5.37 , a one percentage point increase in shrubland-to-ITP conversion reduces annual normalized net outflow by approximately $-5.37/100 \approx 0.054 \text{ } Mm^3/\text{year}$ per km^2 after 7 to 9 years, the largest per-unit effect across all source covers. A positive coefficient at lag 1 in the OLS specification loses statistical significance in the IV specification, which we attribute to noise arising from endogeneity bias, sampling variation, or a combination of both.

As a robustness check, Table 3.8 presents results using the share of ITP in total arable

Table 3.6: ITP Expansion on Former Native Forest and Net Outflow

	Normalized Net Outflow	
	OLS	IV
	(1)	(2)
Native forest $\rightarrow$ ITP (%)	0.55 (1.31)	-2.58 (5.12)
Native forest $\rightarrow$ ITP, lag 1	1.01 (0.67)	1.64 (1.23)
Native forest $\rightarrow$ ITP, lag 2	-0.68 (0.56)	-0.77 (0.58)
Dams and reservoirs controls	Yes	Yes
Climate controls	Yes	Yes
Urban land-use controls	Yes	Yes
Gauge fixed effects	Yes	Yes
N	1,278	1,278
R ²	0.95481	0.95481

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered at the subcatchment level in parentheses.

land — the sum of agriculture, native forest, shrubland, and ITP area — as the treatment variable, without lags. Since the ITP share is a slow-moving stock variable, including lags would introduce severe collinearity; we therefore estimate only the contemporaneous effect. The results are uniformly insignificant across both specifications. This null finding is consistent with our main results: the ITP share variable changes slowly across both units and time, so the contemporaneous specification lacks the precision to detect the delayed hydrological effect that emerges clearly in the conversion specifications. More importantly, the absence of any significant effect, conditional on our full set of controls, suggests that ITP changes are not systematically located in subcatchments with particular trends in water flow, further supporting our identification strategy.

Table 3.7: ITP Expansion on Former Shrubland and Net Outflow

	Normalized Net Outflow	
	OLS	IV
	(1)	(2)
Shrubland → ITP (%)	-2.06 (3.21)	-4.88 (12.31)
Shrubland → ITP, lag 1	4.02** (1.76)	4.89 (4.19)
Shrubland → ITP, lag 2	-5.28*** (1.44)	-5.37*** (1.56)
Dams and reservoirs controls	Yes	Yes
Climate controls	Yes	Yes
Urban land-use controls	Yes	Yes
Gauge fixed effects	Yes	Yes
N	1,278	1,278
R ²	0.95525	0.95523

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered at the subcatchment level in parentheses.

Overall, the panel results paint a consistent picture of a delayed but robust negative effect of ITP expansion on annual freshwater availability. Across all source land covers, the hydrological impact of plantation expansion materializes only after 7 to 9 years, consistent with experimental evidence on eucalyptus water consumption (Scott, 2005; Scott & Lesch, 1997). The magnitude of the effect varies meaningfully by source cover: conversions from shrubland and agriculture — land covers that contribute relatively little to evapotranspiration — produce the largest per-unit reductions in net outflow, while conversions from native forest, which already sustains high evapotranspiration, generate a smaller incremental effect. The robustness check using the ITP area share yields uniformly null results, consistent with the delayed nature of the hydrological response.

Table 3.8: ITP Area Share and Net Outflow: Robustness Check

	Normalized Net Outflow	
	OLS	IV
	(1)	(2)
ITP % (arable land)	0.16	-0.50
	(0.20)	(0.86)
Dams and reservoirs controls	Yes	Yes
Climate controls	Yes	Yes
Urban land-use controls	Yes	Yes
Gauge fixed effects	Yes	Yes
N	1,279	1,279
R ²	0.95497	0.95496

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered at the subcatchment level in parentheses.

3.5.2 Long-Difference Results

We now present the results of our long-difference estimations. We compute averages over two windows — 1986 to 1995 and 2006 to 2015 — yielding a cross-section in which observations are separated by approximately 20 years. This specification allows us to draw on two independent land-cover datasets. The CR2LUC dataset, used in the panel analysis, provides annual observations at 1 km resolution but does not directly record land-use transitions, necessitating the classification algorithm described in Section 3.4.3. The Heilmayr (2024) dataset offers finer resolution (30 meters) and records land-use changes directly, but is available only for 1986, 2001, and 2011; we therefore assign the 1986 values to the first window and the 2011 values to the second. The consistency of results across these two independently constructed datasets serves as an additional robustness check on our findings.

Table 3.9 presents estimates of Equation 3.7 using the share of ITP in arable land

as the treatment variable. The results are negative, statistically significant, and consistent across both datasets. Using the coefficient from the CR2LUC dataset, a one percentage point increase in ITP area share reduces annual normalized net outflow by approximately $1.29/100 = 0.013 \text{ } Mm^3/year$ per km^2 over the 20-year horizon, providing strikingly consistent evidence of the long-run hydrological impact compared to our panel estimates (0.011). At the observed average expansion rate of 6 percentage points over our study period, the CR2LUC estimate implies a cumulative reduction of approximately $6 \times 0.013 \approx 0.078 \text{ } Mm^3/year$ per km^2 over 20 years, equivalent to roughly 8% of mean annual net outflow.

Table 3.9: ITP Area Share and Net Outflow: Long Differences

	Dependent variable: Normalized net outflow	
	CR2LUC land use	Heilmayr (2024) land use
	(1)	(2)
ITP % (arable land)	-1.29*** (0.32)	-0.69*** (0.22)
Dams and reservoirs controls	Yes	Yes
Climate controls	Yes	Yes
Urban land-use controls	Yes	Yes
N	128	128
R ²	0.24506	0.23269

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered at the subcatchment level in parentheses.

Table 3.10 presents long-difference estimates for conversion from agriculture to ITP. Results are negative, statistically significant, and consistent in direction across both datasets. Using the CR2LUC estimate, a one percentage point increase in agriculture-to-ITP conversion reduces annual normalized net outflow by approximately $0.33/100 = 0.003 \text{ } Mm^3/year$ per km^2 over 20 years. The Heilmayr (2024) dataset yields a substantially larger estimate of $1.61/100 = 0.016 \text{ } Mm^3/year$ per km^2 .

This discrepancy likely stems from differences in land-cover classification and the scale at which land-cover change is captured. As shown in the transition summaries in Appendix 3.2.4, the Heilmayr (2024) dataset identifies a significantly larger decline in agricultural and pasture land (-52.6 km^2) compared to the more conservative agricultural category in CR2LUC (-19.2 km^2). Because the Heilmayr (2024) agricultural category includes pastures—which typically exhibit lower baseline evapotranspiration—the measured hydrological impact of replacing this land with high-consumption eucalyptus plantations is mechanically larger. Despite these differences in point estimates, we interpret the directional consistency across two independently constructed datasets as the key finding.

Table 3.10: ITP Expansion on Former Agricultural Land and Net Outflow: Long Differences

	Dependent variable: Normalized net outflow	
	CR2LUC land cover	Heilmayr (2024) land cover
	(1)	(2)
Ag $\rightarrow$ ITP (%)	-0.33*** (0.09)	-1.61*** (0.41)
Dams and reservoirs controls	Yes	Yes
Climate controls	Yes	Yes
Urban land-use controls	Yes	Yes
N	128	128
R ²	0.23805	0.25770

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered at the subcatchment level in parentheses.

Table 3.11 presents long-difference estimates for conversion from native forest to ITP. Consistent with the panel results, the long-difference estimates are largely insignificant, except for the marginally significant estimate from the CR2LUC dataset. This is consistent with the smaller and noisier per-unit effect of native forest conversion documented in the panel analysis — since native forest already sustains high evapotranspiration, the

incremental hydrological impact of replacing it with eucalyptus is modest.

Table 3.11: ITP Expansion on Former Native Forest and Net Outflow: Long Differences

	Dependent variable: Normalized net outflow	
	CR2LUC land use	Heilmayr (2024) land use
	(1)	(2)
Native forest $\rightarrow$ ITP (%)	-0.08*	-0.19
	(0.04)	(0.53)
Dams and reservoirs controls	Yes	Yes
Climate controls	Yes	Yes
Urban land-use controls	Yes	Yes
N	128	128
R ²	0.20859	0.19745

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered at the subcatchment level in parentheses.

Table 3.12 presents long-difference estimates for conversion from shrubland to ITP. The results are consistent with the agriculture-to-ITP long-differences findings: negative, statistically significant, and directionally consistent across both datasets and estimation methods. Using the CR2LUC IV estimate, a one percentage point increase in shrubland-to-ITP conversion reduces annual normalized net outflow by approximately $0.49/100 = 0.005 \text{ } Mm^3/year$ per km^2 over 20 years. As with the agriculture results, the Heilmayr (2024) dataset yields a substantially larger estimate of $2.03/100 = 0.02 \text{ } Mm^3/year$ per km^2 , which we attribute to differences in how the two datasets classify shrubland transitions rather than a substantive disagreement.

The long-difference results corroborate the main findings from the panel analysis. ITP expansion is associated with a robust negative effect on annual net outflow over a 20-year horizon, with consistent evidence across datasets and estimation methods for total ITP area share, as well as for agriculture-to-ITP and shrubland-to-ITP conversions. Native forest conversion is the exception, yielding insignificant results in both datasets,

Table 3.12: ITP Expansion on Former Shrubland and Net Outflow: Long Differences

	Dependent variable: Normalized net outflow	
	CR2LUC land use	Heilmayr (2024) land use
	(1)	(2)
Shrubland $\rightarrow$ ITP (%)	-0.49***	-2.03***
	(0.10)	(0.67)
Dams and reservoirs controls	Yes	Yes
Climate controls	Yes	Yes
Urban land-use controls	Yes	Yes
N	128	128
R ²	0.26262	0.22933

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered at the subcatchment level in parentheses.

which we attribute to the modest incremental hydrological impact of replacing already high-evapotranspiration vegetation with eucalyptus over long horizons. The directional consistency across two independently constructed land-cover datasets lends additional credibility to our overall findings.

3.6 Concluding Remarks

This paper studies the effect of industrial tree plantations on freshwater availability in Chile. Using a water balance model as our theoretical foundation, we estimate the impact of ITP expansion on net water outflow — the difference between gauge-measured outflows at the subcatchment level and upstream inflows— through two complementary empirical strategies: a three-year average panel with a shift-share instrument, and long differences. Our findings are particularly relevant in the context of Chile’s ongoing megadrought, which has reduced precipitation significantly since 2011 (Garreaud et al., 2020), making freshwater availability an increasingly critical concern.

Our results consistently indicate a delayed yet robust negative effect of ITP expansion

on freshwater availability. A one percentage point increase in ITP conversion reduces annual normalized net outflow by approximately $0.011 \text{ Mm}^3/\text{year}$ per km^2 after 7 to 9 years, equivalent to roughly 7% of mean annual net outflow at the observed rate of expansion over our study period. This effect is driven primarily by the slow-moving nature of hydrological responses to land-cover change, as soil structure, infiltration capacity, and groundwater recharge adjust gradually to new vegetation. Disaggregating by source land cover reveals meaningful heterogeneity: conversions from agriculture and shrubland — land covers that contribute relatively little to evapotranspiration — produce per-unit effects approximately three times the average, while conversions from native forest generate a more modest incremental impact, given its already high evapotranspiration. These findings are corroborated by our long-difference estimates, which confirm a persistent negative effect on net outflow over a 20-year horizon.

The source-cover heterogeneity has important implications for land-use policy. While replacing agriculture and shrubland with ITPs produces the highest hydrological costs, replacing native forest carries significant biodiversity costs that extend well beyond water availability. Policymakers and forestry planners face a difficult trade-off: no source cover is without consequence, and the choice of where to expand plantations involves balancing hydrological, ecological, and economic considerations simultaneously. These trade-offs are particularly acute given the scale of proposed ITP expansion in Chile: the industry has suggested extending plantations by $100,000 \text{ km}^2$ to meet carbon sequestration goals. Extrapolating our point estimates to this scale suggests dramatic reductions in freshwater availability across affected subcatchments, with consequences that would fall disproportionately on rural communities and agricultural producers that depend on these water sources.

More broadly, our paper documents the unintended local consequences of nature-based climate mitigation strategies. Basing carbon sequestration on the planting of fast-growing foreign species can exacerbate the impacts of climate change at the local level by reducing freshwater availability precisely when and where it is most needed.

Our analysis has several limitations that point to directions for future research. Our

sample is constrained to 128 subcatchments with imputed streamflow data, which limits our ability to include unit and time fixed effects simultaneously and reduces statistical power. Our linear specification identifies the marginal effect of ITP expansion at observed levels of plantation coverage, but the shape of the dose-response function beyond this range remains unknown — effects could differ substantially at the scale of expansion proposed by the industry. Additionally, while we control for precipitation and temperature, these controls may not fully capture the complex water dynamics that emerge during prolonged droughts such as the one Chile has experienced since 2010 (Garreaud et al., 2020). Replicating this analysis in other plantation forestry contexts — particularly in regions with richer hydrological data — would help establish the generalizability of our findings and allow for more precise identification strategies. Beyond the hydrological effects studied here, an important open question concerns the downstream welfare consequences of ITP-induced water loss for agricultural producers, rural communities, and ecosystems that depend on freshwater availability. Quantifying these welfare effects would provide a more complete picture of the true social cost of industrial plantation expansion.

Acknowledgments

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Appendices

Appendix 3.1 — Chemical pulp production and exports in Chile

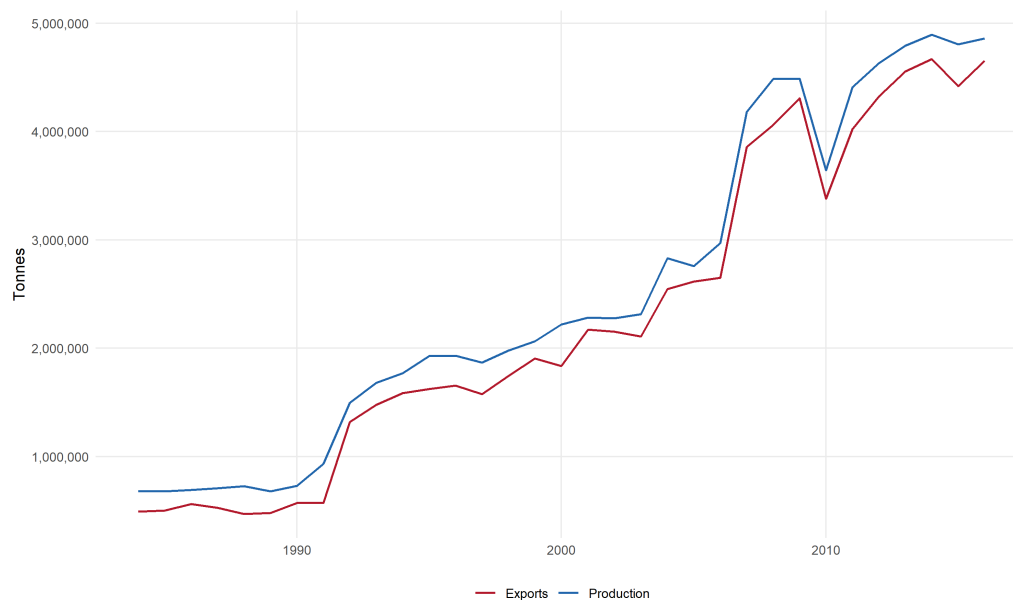


Figure S3.1: Volume of yearly chemical pulp production and exportation in Chile.

Notes: Values in tonnes of chemical pulp. Own elaboration based on FAOSTAT data.

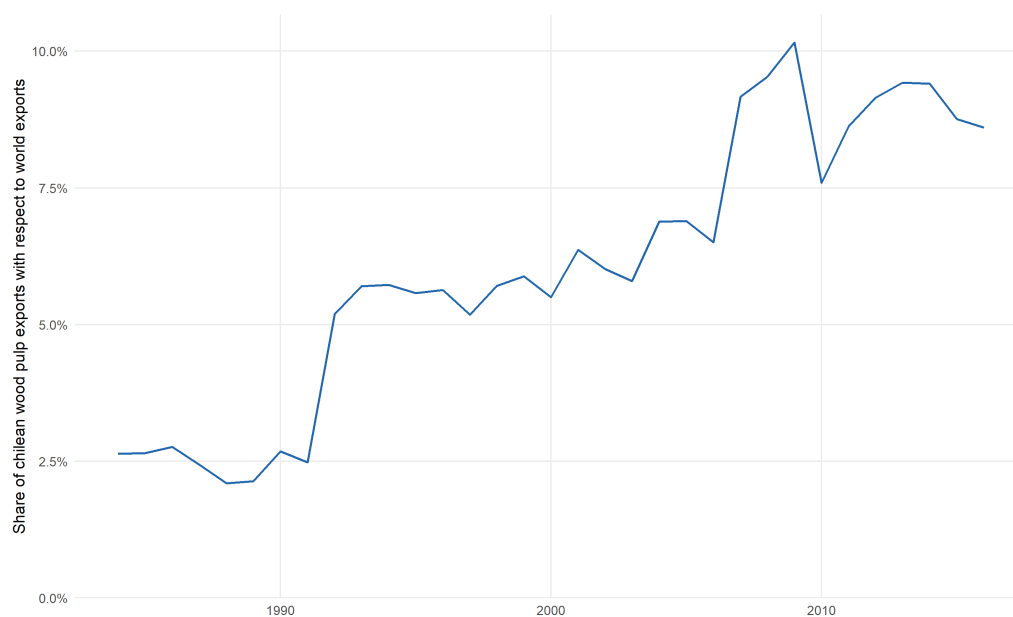


Figure S3.2: Share of Chilean chemical pulp exports with respect to global exports, 1985–2016

Notes: Share calculated as annual Chilean wood pulp export quantity divided by world export quantity. Own elaboration based on FAOSTAT data.

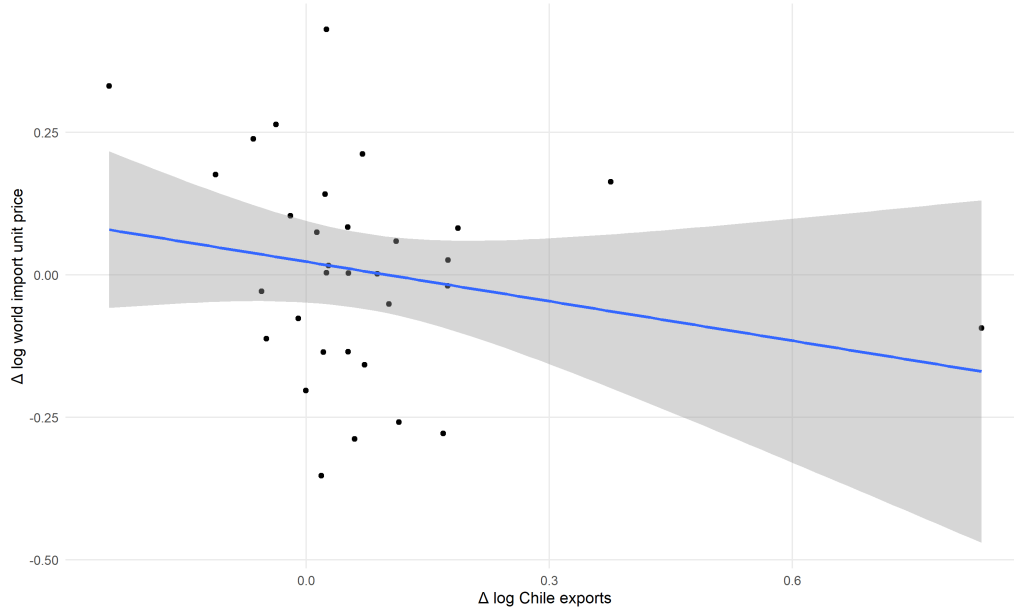


Figure S3.3: Annual Changes in Chilean Pulp Exports and World Import Prices, 1985–2016

Notes: Each observation is a year. The horizontal axis shows the annual log change in Chilean chemical pulp export quantity; the vertical axis shows the annual log change in the world pulp import unit value, constructed as total world import value divided by total import quantity (USD per tonne). The shaded band is the 95% confidence interval of a linear fit. The slope is negative but statistically not different from zero ($p = 0.23$). Source: Own elaboration based on FAOSTAT data.

Appendix 3.2 — Data Processing

3.2.1 — Main data sources

Table S3.1: Land cover, water, and weather data sources.

Variable	Unit	Resolution	Start	Freq	Dataset	Source
ITP cover	km ²	0.01° ($\approx$ 1-km grid)	1950	year	CR2LUC	(Boisier et al., 2024)
(Other) Land cover	km ²	0.01° ($\approx$ 1-km grid)	1950	year	CR2LUC	(Boisier et al., 2024)
Fresh water availability	m ³ /s	catchment	1979	day	CAMELS-CL	(Alvarez-Garreton et al., 2018)
Climate controls	-	0.05° (5-km grid)	1960	day	CR2MET v2.5	(Boisier, 2023)

3.2.2 — Subcatchments’ coverage

Table S3.2: Subcatchment coverage by Chilean region

Region	Area (km ²)	Subcatchments (km ²)	Coverage (%)
Valparaíso	16,504	6,990	42.4
Región Metropolitana de Santiago	16,077	14,662	91.2
Libertador General Bernardo O’Higgins	16,642	6,173	37.1
Maule	28,941	24,382	84.2
Ñuble	13,547	11,345	83.7
Bío-Bío	24,247	17,875	73.7
La Araucanía	33,949	29,341	86.4
Los Ríos	16,286	6,955	42.7
Los Lagos	47,477	6,370	13.4
Total	213,670	124,091	58.1

3.2.3 — Water records reconstruction using MissForest

Daily streamflow data for Chilean subcatchments are drawn from the CAMELS-CL dataset (Alvarez-Garreton et al., 2018), which compiles gauge records from the Dirección General de Aguas (DGA). Raw records contain substantial gaps due to sensor failures, maintenance periods, and reporting lapses (Arriagada et al., 2021). To obtain a balanced panel suitable for regression analysis, we reconstruct missing observations using the MissForest algorithm (Stekhoven & Bühlmann, 2012).

MissForest is a non-parametric imputation method that trains a separate random forest for each variable, using all other variables as predictors, and iterates until convergence. Unlike parametric methods, it imposes no distributional assumptions on the data and automatically captures non-linear relationships and interactions among stations (Stekhoven & Bühlmann, 2012). The algorithm is particularly well-suited to hydro-meteorological data, where missingness is irregular and the joint distribution across gauges is complex (Arriagada et al., 2021).

The validity of the imputation rests on the Missing at Random (MAR) assumption

(Rubin, 1976): the probability that an observation is missing depends only on observed data, not on the unobserved streamflow values. In the context of gauge records, this is plausible because missingness is typically driven by mechanical failures or administrative decisions that are unrelated to the flow measured at the time of dropout. However, the assumption weakens for heavily regulated rivers where operational decisions — in particular hydropeaking — introduce a stochastic component in discharge that is unrelated to climate and unobservable to neighboring gauges. Arriagada et al. (2021) document that MissForest performs satisfactorily to well for rivers with natural flow regimes, but fails to recover daily streamflow series at gauges subject to hydropeaking, where the algorithm cannot learn a stable relationship between stations. We include controls for dams and artificial water reservoirs in our main specification to account for this source of bias.

Before imputation, daily discharge is transformed as $\tilde{Q} = \log(1 + Q)$ to reduce its right skewness. This is a modeling choice motivated by the finding of Hong and Lynn (2020) that MissForest produces biased estimates for highly skewed variables, because the conditional mean predictions of a random forest cannot extrapolate beyond the range of observed values. The transformed series are arranged in a station-by-day matrix and passed to the MissForest routine. At each iteration, missing entries are replaced with predictions from the updated random forest trained on the current imputed matrix. The algorithm stops when the discrepancy between successive imputed matrices ceases to decrease — the iteration at which it first increases is taken as the convergence criterion. Predictions are then back-transformed to the original streamflow scale.

We run MissForest with 1,000 regression trees and a maximum of 100 iterations, matching the specification of Arriagada et al. (2021). Variables are imputed in parallel across 15 cores to reduce computation time. We retain stations with at least 75 percent of daily observations present over the study period, yielding 128 stations from the 295 gauges in the raw CAMELS-CL data. Of the 1,636,224 total station-day observations, 8.8% were missing prior to reconstruction and were imputed. Figure S3.4 shows a histogram with the distribution of gauge stations by completeness. Furthermore, we show in Figure S3.5 the distribution of missing observations across selected gauge stations.

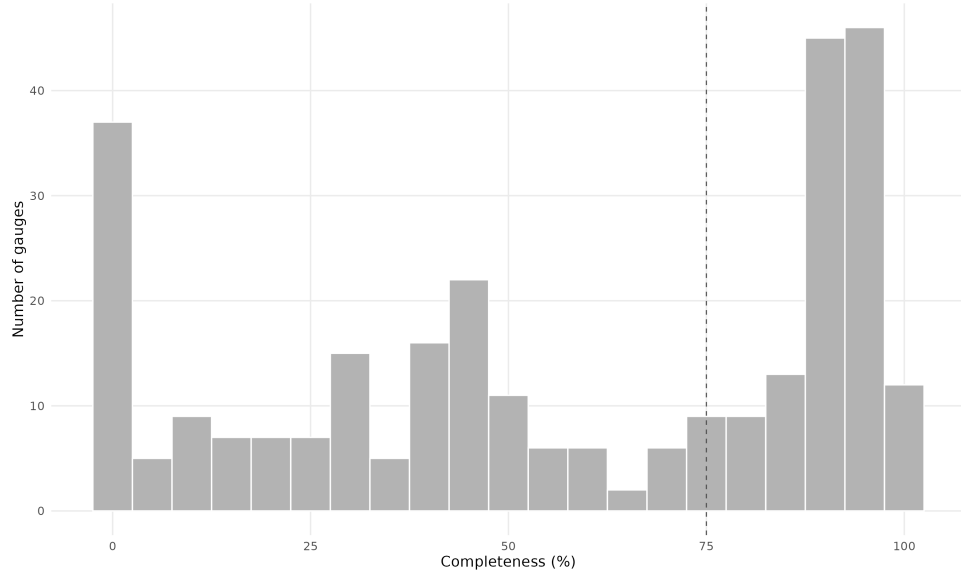


Figure S3.4: Histogram with distribution by completeness

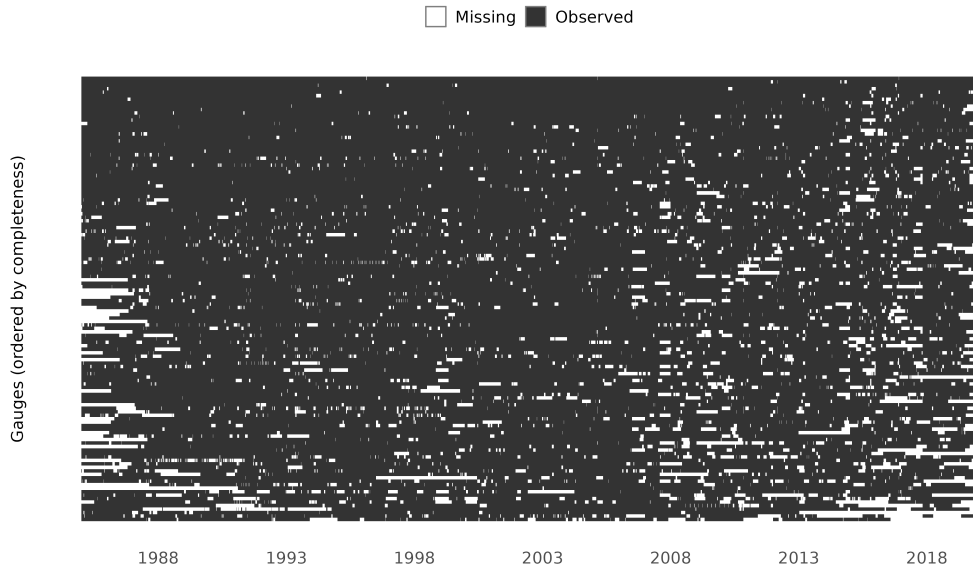


Figure S3.5: Distribution of missing observations in gauges that met the 75% of daily observations threshold. Each row represents a gauge station. Stations are sorted by completeness level.

We report the spatial results of our reconstruction in Figure S3.6. The map suggests that subcatchments with a high share of imputed values are evenly distributed across space.

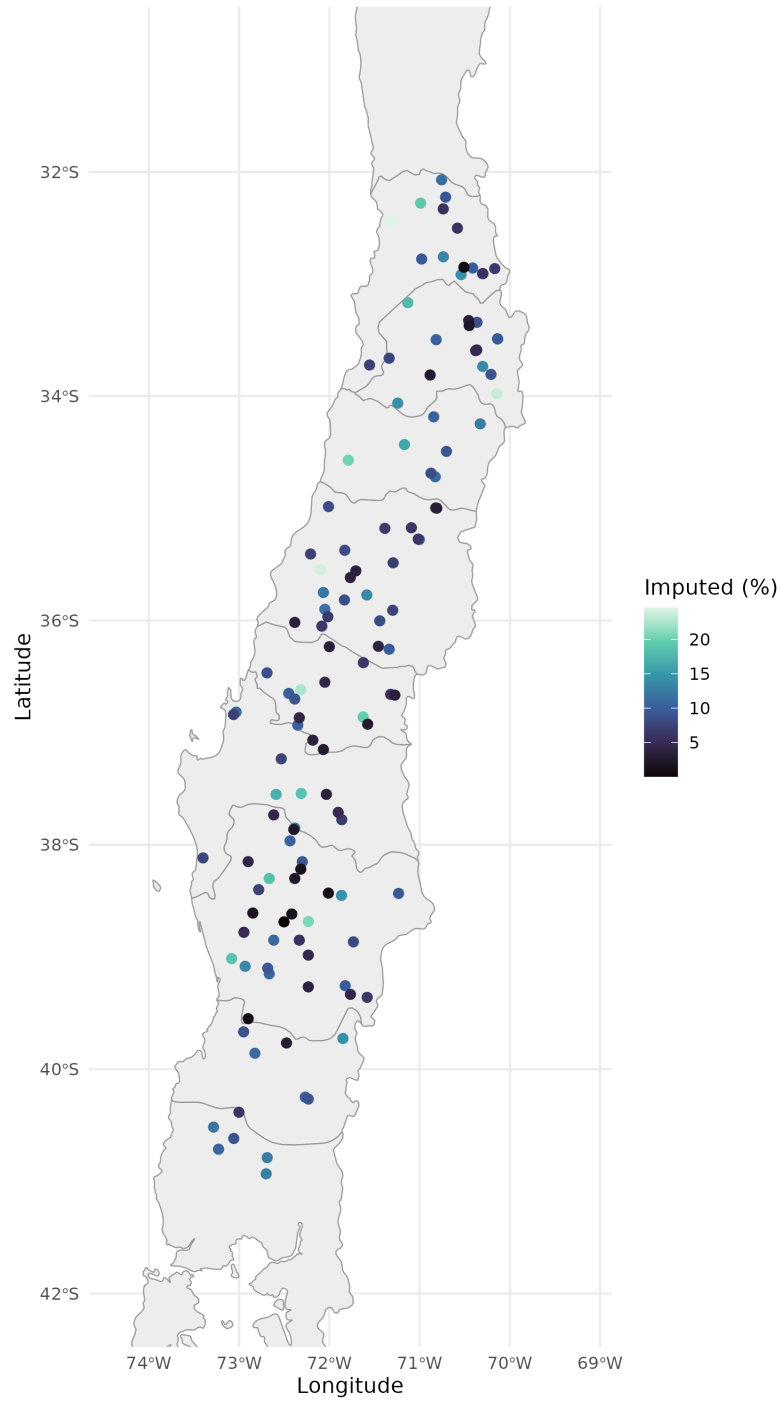


Figure S3.6: Map with the share of imputed observations on the 128 selected gauge stations.

To validate the reconstruction, we implement a holdout exercise. We randomly mask 20 percent of the observed values in the 20 most complete stations and re-run MissForest

on the resulting matrix using a lighter specification (100 trees, 10 iterations).

We evaluate reconstruction accuracy by masking a random 20% of observed values at the 20 most complete stations, re-running MissForest on the masked dataset, and comparing imputed values at masked positions to their true observed values. We report two goodness-of-fit statistics: the coefficient of determination (R^2) and percent bias (PBIAS), which measures the average tendency of imputed values to over- or underestimate observations (Moriasi et al., 2015). Across stations, mean $R^2 = 0.908$ and mean PBIAS = -3.23% ; both fall in the “very good” range by the thresholds in Moriasi et al. (2015).⁶ Figure S3.7 shows imputed against observed values for all masked station-days.

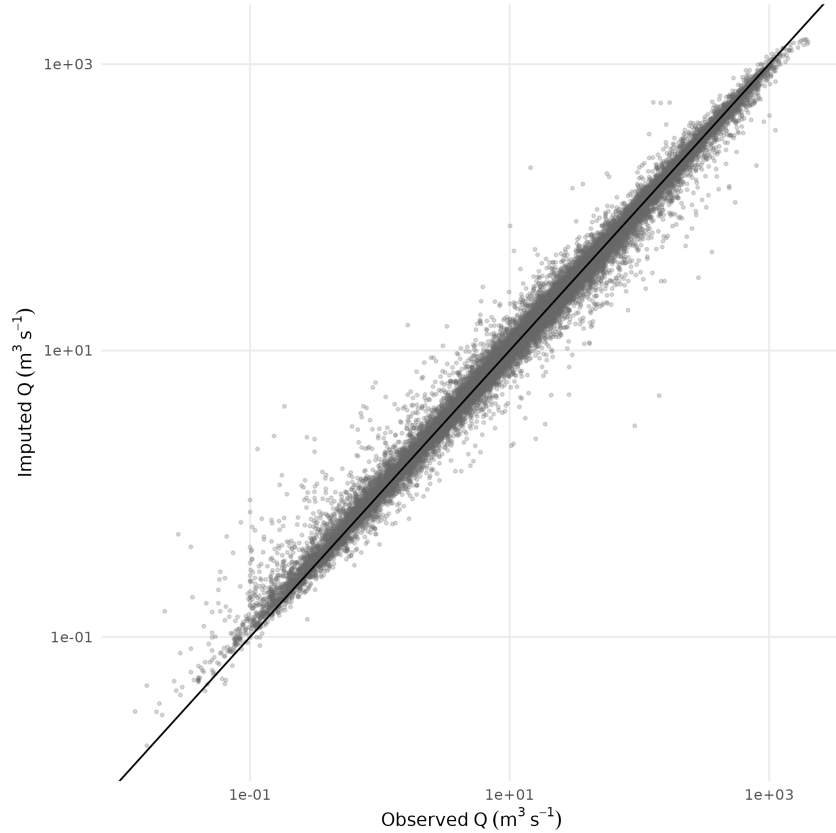


Figure S3.7: Holdout validation: imputed vs. observed streamflow.

Each point is a masked station-day ($N = 20$ stations, 20% of observed values masked). Axes are on a logarithmic scale; days with zero flow at either observed or imputed position are excluded from the plot. The solid line is the 1:1 reference. Mean $R^2 = 0.908$, mean PBIAS = -3.23% .

⁶These thresholds were developed for watershed model calibration. We apply them as a benchmark for imputation accuracy.

Finally, we present hydrographs for nine reconstructed gauge stations (Figure S3.8). Imputed observations are shown in red. Visually, the reconstructed segments align closely with the observed surrounding series, preserving the main temporal structure of stream-flow while avoiding obvious artifacts, such as discontinuous jumps or excessive smoothing.

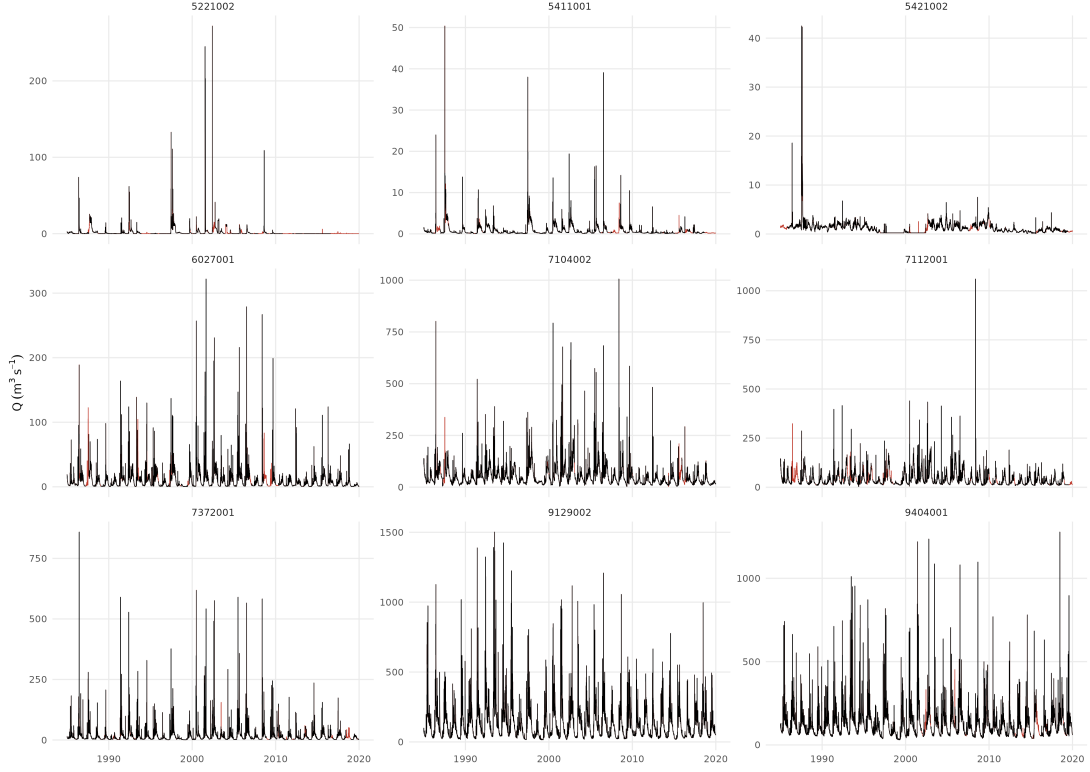


Figure S3.8: Reconstructed hydrographs for nine illustrative gauge stations.

Imputed observations are shown in red. The figure provides a visual check on the hydrological plausibility of the reconstructed series.

A3.2.4 — Land cover transition matrix

Table S3.3: Feasibility matrix for land-cover transitions

<i>From \ To</i>	Agricultural	Native Forest	Plantation	Shrubland	Grassland	Built-up	Barren	Water	Glacier
Agricultural	1	1	1	1	1	1	1	1	0
Native Forest	1	1	1	1	1	0	1	1	0
Plantation	1	1	1	1	1	0	0	0	0
Shrubland	1	1	1	1	1	1	1	0	1
Grassland	1	1	1	1	1	1	1	0	1
Built-up	0	0	0	0	0	1	0	0	0
Barren	1	1	1	1	1	1	1	1	1
Water	1	0	0	1	1	0	1	1	0
Glacier	0	1	0	0	0	0	1	0	1

Rows: origin class; columns: destination class. A value of 1 indicates a feasible transition; 0 indicates a transition blocked as physically or institutionally implausible. Feasible transitions are identified from 30-m satellite data (Heilmayr, 2024); losses are allocated proportionally across feasible destination classes only.

A3.2.5 — Summary statistics for historical satellite images (Heilmayr, 2024)

	1986	2001	2011	Δ
<i>Subcatchment characteristics (km²)</i>				
Subcatchment area	986.9 (1131.9)	986.9 (1131.9)	986.9 (1131.9)	
<i>Land cover (% of subcatchment area)</i>				
Forest (lcf_210)	26.1 (23.7)	22.7 (21.3)	21.9 (21.7)	-4.2***
Plantation (lcf_220)	3.6 (7.4)	9.7 (14.0)	12.2 (16.2)	8.6***
Shrub (lcf_300)	17.6 (16.5)	21.4 (15.8)	18.7 (15.7)	1.2
Pasture and ag (lcf_100)	26.6 (27.0)	20.3 (21.6)	21.7 (21.4)	-4.9***
Temp. bare soil (lcf_100)	0.0 (0.0)	0.5 (1.1)	0.0 (0.1)	0**
Snow/ice (lcf_800)	1.9 (4.0)	2.6 (5.8)	1.9 (4.5)	0
Perm. bare soil (lcf_600)	21.6 (30.2)	17.0 (23.0)	17.5 (25.8)	-4.1***
Water (lcf_700)	0.8 (2.9)	1.0 (2.9)	0.7 (2.9)	0
Urban (lcf_500)	0.3 (1.1)	0.6 (1.4)	0.5 (1.6)	0.3***

Table S3.4: Average land use share per land cover across subcatchments.

Data from 3 LANDSAT images processed by (Heilmayr, 2024). Land use labels correspond to the author's. In parentheses, CR2LUC equivalent. LANDSAT images do not identify grassland.

	1986	2001	2011	Δ
<i>Subcatchment characteristics (km²)</i>				
Subcatchment area	986.9 (1131.9)	986.9 (1131.9)	986.9 (1131.9)	
<i>Land cover (km²)</i>				
Forest (lcf_210)	240.1 (398.2)	208.2 (340.6)	200.7 (340.2)	-39.4***
Plantation (lcf_220)	43.2 (151.7)	106.2 (284.0)	125.5 (290.4)	82.3***
Shrub (lcf_300)	169.7 (260.2)	204.0 (269.5)	184.8 (270.6)	15.1
Pasture and ag (lcf_100)	296.4 (460.7)	228.7 (344.5)	243.8 (357.9)	-52.6***
Temp. bare soil (lcf_100)	0.0 (0.0)	6.7 (20.7)	0.3 (1.7)	0.3*
Snow/ice (lcf_800)	16.6 (43.3)	17.8 (41.7)	23.5 (101.9)	6.9
Perm. bare soil (lcf_600)	168.0 (407.3)	137.0 (362.4)	126.7 (257.0)	-41.3***
Water (lcf_700)	12.4 (47.6)	15.0 (49.3)	12.5 (47.5)	0.1
Urban (lcf_500)	5.1 (32.0)	10.0 (40.3)	9.1 (43.5)	3.9***

Table S3.5: Average land use surface per land cover across subcatchments.

Data from 3 LANDSAT images processed by (Heilmayr, 2024). Land use labels correspond to the author's. In parentheses, CR2LUC equivalent. LANDSAT images do not identify grassland.

Appendix 3.3 — First Stage IV Regressions: Panel

	ITP % (arable land)	Ag → ITP	Native forest → ITP	Shrubland → ITP	Total ITP Conversion
	(1)	(2)	(3)	(4)	(5)
Slope suitable area (12–30%)	0.3212***	0.0272***	0.0536***	0.0225***	0.1037***
× Lagged price change	(0.0465)	(0.0038)	(0.0085)	(0.0044)	(0.0134)
Outcome, lag 1		0.2890***	0.2098***	0.3179***	0.2645***
		(0.0457)	(0.0429)	(0.0930)	(0.0408)
Outcome, lag 2		-0.0511**	-0.0383*	-0.0394*	-0.0677***
		(0.0252)	(0.0199)	(0.0228)	(0.0229)
Dams and reservoirs controls	Yes	Yes	Yes	Yes	Yes
Climate controls	Yes	Yes	Yes	Yes	Yes
Urban land-use controls	Yes	Yes	Yes	Yes	Yes
Gauge fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,279	1,278	1,278	1,278	1,278
R ²	0.96967	0.94638	0.97294	0.94956	0.96400
F-test (1st stage)	70.882	48.925	85.337	50.345	93.331

* p < 0.10, ** p < 0.05, *** p < 0.01.

Standard errors clustered at the subcatchment level in parentheses.