# Rural Perspectives in a Global Economy

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ABSTRACT

My dissertation is grounded in policy issues that affect rural economic growth in emerging markets. In a global economic framework, where growth and redistribution are par for the course, policies promoting rural development deserve particular attention. In the first two chapters, I explore how property rights establishment affects trade, deforestation and agricultural productivity in the Brazilian Amazon. My third chapter presents substantial evidence that crises propagate differently across rural versus urban areas in Latin America.

In my first chapter, "Do Property Rights Solve the Tragedy of Commons under Free Trade? Evidence from Brazil," I find that the titling scheme *Terra Legal* increases wood exports from the Amazon, contradictory to policy expectations of reducing deforestation in the short run. Following property rights establishment, wood exporting intensifies initially and peaks at the third quarter post programme rollout. Evidence further suggests that medium-sized farms, which obtain the rights cheaply, clear forest land to make way for livestock production. A 10% increase in the area registered by such farms in a municipality increases average export value of wood-based products (by 2.5%), as well as animal-based products (by 4.3%), per quarter, within a municipality. To estimate the effect precisely, I use confidential, municipal-level export data and control for municipal-product unobserved heterogeneity and quarter-year trend, relying on the scheme's phased rollout for identification. I also present a dynamic model of land allocation, which shows that while deforestation could increase in the transition to steady state following titling, the agent reduces deforestation in the long run and allocates more land to sustainable forestry in the new steady state. To the best of my knowledge, this paper is one of the first to explore the effect of property rights establishment on exports, both empirically and theoretically.

The results indicate that the effect from removing the fear of appropriation, which induces agricultural expansion at the expense of forests, dominates the effect from correcting the inefficiency of open access. In my second chapter, "Property Rights, Agricultural Productivity and Deforestation in the Brazilian Amazon," co-authored with Molly Lipscomb, we explore the agricultural channel more thoroughly. Property rights establishment may increase a farmer's investment in his land due to lower risk of expropriation, easier access to credit, and the ability to sell the land. Empirically, we show that the titling scheme does indeed increase credit access—both at the intensive and extensive margins; the smallest farms appear to be the main beneficiaries. A 1 percentage point (p.p.) increase in the area registered by small farms leads to a 30% in credit financing and a 5 % increase in the number of credit contracts for investment purposes. However, we find no evidence indicating that the small farms engage in deforestation; they decrease their temporary crop cultivation, but do not increase their permanent crop cultivation. Increasing the area registered by medium-sized farms that obtain their land cheaply leads to an increase in the area deforested, as well as an increase in the rate of deforestation, by 0.3% and 4.4%, respectively, in response to a 1 p.p increase in their area registered. Finally, removing the threat of expropriation, along with increased access to credit, results in a reduction of total area cultivated due to a decreased cultivation of temporary crop cultivation. However, the largest properties increase their overall cultivation, particularly of cash crops and cocoa, though we cannot attribute this to the credit channel.

My final chapter, "Asymmetric Effects of Crises in Urban vs. Rural Areas in Latin America: A Study Using Nightlights," more broadly assesses the impact of shocks in rural Latin America and the Caribbean. Using nightlights observed from space as a proxy for real GDP and population density maps, I construct a very precise measure of economic activity at five-square-kilometer grid level, a level of disaggregation never used before. To estimate the effect, I run separate regressions based on population density and cluster the errors at the country-level, using the pairs cluster bootstrap-t procedure to deal with the problem of too few clusters in my sample. Rural/semi-rural areas see their real income growth fall following systemic banking crises and currency crises, with the effect ranging between 0.5% to 1% based on the subsample and type of crisis. Notably, I fail to reject the null for their urban counterparts. These findings indicate that capital markets are not integrated by population at this disaggregate level and that the assumption of perfect risk-sharing is strongly violated.

JEL Classifications: R11, F18, O13, O18
Keywords: Property Rights, Trade, Development, Deforestation, Renewable Resources, Land-Use, Agriculture, Credit Markets, Nightlights

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# Contents

# Chapter 1

# Do Property Rights Solve the Tragedy of Commons under Free Trade? Evidence from Brazil

## 1.1 Introduction

The Amazon rainforest, located predominantly in Brazil, provides global ecosystem services, in the form of carbon sequestration, climate control and biodiversity. However, just in the last four decades, almost 20% of the Amazon Rainforest has been destroyed, driven by world demand for timber and domestic demand for arable land.[1] The absence of property rights hastens the inexorable march towards extinction by threatening the forest's renewability.

An instinctive solution to stem the tide of deforestation is the establishment of property rights. Compelling agents to internalize the opportunity cost of forest exploitation should reduce the over-harvesting that puts the "tragedy" in the commons. In addition, reducing the artificial comparative advantage created by the absence of property rights in free trade, should decrease forest exploitation even further and

[1] Wallace, Scott. "Last of the Amazon." *National Geographic* January 2007. <http://ngm.nationalgeographic.com/2007/01/amazon-rain-forest/wallace-text>.

reduce the volume of wood-based exports. I call this the 'Tragedy of the commons' reversal. However, obtaining land rights increases security and can serve as a collateral for loans, leading to agricultural expansion at the expense of the forests, a 'productivity' boost that happens at the expense of forests. Given these two opposing forces, it is unclear how Brazil's latest titling scheme, *Terra Legal*, affects Brazilian wood exports and the Amazon rainforest at large.

This paper provides empirical evidence that between 2009-2012, Brazil's latest titling scheme *Terra Legal*, dismayingly *increases* wood-based exports from the Amazon, and consequently, deforestation, at least in the short run. To the best of my knowledge, this is one of the first empirical results on the impact of a public-to-private transfer of land rights on wood-based exports. To estimate the effect precisely, I use confidential,[2] municipal-level export data and control for municipal-product unobserved heterogeneity in wood exports, in addition to quarter-year fixed effects.

The property rights intervention, which started in July 2009, is different from previous titling efforts in Brazil because it took titling to the squatters. This resulted in a phased rollout over time because officials travelled to the sites to inspect the properties, as well as to advertise and increase uptake.[3] This feature, in addition to the pricing scheme, made titling free for many squatters; small farms below a certain size threshold[4] obtained the property for free, while medium properties paid some discounted price; only the large properties paid the full market price for the title.[5] The "catch" is the requirement that land owners maintain forests on 80% of

[2]The data at the municipal-HS6-level is restricted as of mid-2014.

[3]However, it was not strictly phased in that agents could still register their property at any time if they were willing to travel to the field office.

[4]The size threshold varied by municipality, based on population and proximity to cities.

[5]However, it is not clear how the government determined which the market price or the discounted price that the land owners must pay, nor do I have information on how much they paid.

their property, the so-called "Legal Reserve," or risk losing the title, though evidence shows that agents probably do not adhere to this.

Relying on the scheme's phased rollout, exogenous to exports, for identification, I find that bringing *Terra Legal* to a municipality increases its quarterly export value by roughly 48% on average.[6] I also find that the effect of on-site rollout intensifies for the first few months and peaks at the third quarter post-rollout, before petering out. These results indicate that the 'productivity boost' dominates the 'commons reversal,' at least in the short run. I also perform a similar analysis with agricultural commodities, but find that the post-rollout effect is insignificant. The responsiveness of wood-based exports, but not agricultural ones, to titling is reasonable, particularly if land clearing is the first of many steps in the agricultural production process. An agent can immediately cut down the trees to make way for alternate land uses, but increasing agricultural output requires more time and effort.

Given that trees need to mature for at least 25-30 years before they are ready for harvestation, sustainable forestry and diversification might be more feasible for large properties sprawling many square kilometres, which can take advantage of economies of scale; these are also the likely exporters. Meanwhile, intensifying agriculture in a small plot of land is the best option for a small farmer to increase his revenue. Thus, it is reasonable to expect the effect of rollout on wood harvesting and exports to vary over the size of the property.

Indeed, I find that the medium properties appear to be the real culprits behind the increase in wood exports. A 10% increase in registered area by medium-sized

[6] Following my conversation with the officials, I can state with a reasonable degree of confidence that *Terra Legal* did not prioritize municipalities with high deforestation rates. Determining the regions that *Terra Legal* can title every month is a complex process involving lengthy discussions with other federal agencies; these regions also span many municipalities.

farms leads to a 2% hike in average wood export value within a municipality beyond the post-rollout effect, whereas that same increase in overall area registered decreases average quarterly wood export value within a municipality by approximately 2%. The "medium size" effect persists for agricultural commodities as well. A 10% increase in the area registered by medium-sized farms increases overall agricultural export value by roughly 2.5% per quarter for a municipal-product pair on average, and animal-based export value by a little bit over 4%.

Interestingly, there is no effect of large farms on wood-based exports. Because these properties experience the most scrutiny, these land owners might not be willing to risk losing their property for violating the 80% legal reserve, and are thus more circumspect in their wood harvesting and more likely to engage in sustainable forest management. However, the medium-sized farms that obtain their land rights cheaply are sized "just right" (or just wrong); they are small enough to engage in illegal land clearing and go unnoticed, but large enough to access the world market and reap the benefits.

I also find that there is no effect of rollout or registrations on plant-based exports, a reflection of the subsistence farming that is prevalent in the Amazon, particularly by small farms. However, this finding does not rule out an effect on crops all together. In fact, in the second chapter of my thesis, my co-author and I find that the cultivation of cash crops increase in response to an increase in registered area by the largest farms because agents have more access to credit.

Apart from showing what happens to the wood following deforestation, an interesting question in and of itself given that the total export value of Brazilian wood-based products amounted to roughly $8.7 billion USD in the year 2010 alone,[7] the use of

[7]$8,726,608,770 USD to be exact: From World Integrated Trade Solution (http://wits.

wood-based exports as the outcome variable, rather than deforestation, has several advantages. First, it allows me to estimate the effect more precisely. While satellite maps of deforestation have improved markedly and can provide precise measures of how much land has been deforested throughout the year, the annual deforestation amounts is a composite, determined by aggregating those images. This leads to measurement error, particularly if cloud cover is high during certain time periods, logging occurs in the dead of the night or trees are cherry-picked rather than clear cut. Alternatively, the export customs data will pick up any wood product that leaves the country, and it is available every month, rather once a year. Moreover, an increase in wood-based exports, implies the production of wood-based products increases, which is possible only if the inputs increase as well, i.e. more wood is harvested. Secondarily, the availability of the export data at the monthly level improves my identification because the phased rollout occurs more gradually, and exports are less likely to be correlated with rollout than deforestation. This also allows me to estimate the heterogenous effect of rollout over time.

In addition to my main empirical findings, I also contribute to the rich theoretical literature on property rights, trade and land use by considering a switch in the property rights regime, an area which has not been explored much. I extend the dynamic model presented in Hartwick et al. (2001), in which an agent in a small open economy chooses how much land to allocate to sustainable forestry and agriculture, to the second-best scenario where property rights do no exist, modeled as a threat of appropriation a la Besley (1995). I show that while the agent allocates more land to forests in the long run following property rights establishment, deforestation can

worldbank.org/CountryProfile/en/Country/BRA/Year/2010/TradeFlow/Export/Partner/all/Product/44-49_Wood).

nevertheless increase in the transition to the steady state, depending on parameters.

## 1.2 Literature Review

To put my research on wood exports from the Brazilian Amazon in context, I provide a brief overview on the three most relevant strands of literature. The first is the literature on trade in renewable resources, well-documented in Bulte and Barbier (2005). Mostly theoretical, works in this field examine the impact of trade in renewable resources under different property rights regime. In this paper, I turn that idea on its head and instead examine the consequence to trade following a shift in property rights ownership. A handful of papers within this literature also study the aspect of conflicting land use and how the terrestrial renewable resource uses land which competes with other production, such as agriculture, and sustainable living; this paper provides empirical evidence to this discussion. Finally, a strand of development literature examines property rights and squatting more generally; I contribute to this literature by considering the trade aspect of a domestic policy implementation.

The singular conclusion of the literature on trade in renewable resources is that the effects of free trade on welfare are ambiguous, depending on initial endowments, preferences, the types of production function and so forth; most importantly, they also vary based on whether the resource-rich country has optimally managed resources, open access or something in between. Where trade is welfare-improving under optimal management, it could be detrimental under its polar opposite—open access. Taking into account the positive externalities provided by the resource can lead to ambiguity even under optimal management (Barbier and Rauscher, 1994; Barbier and Schulz, 1997). The short-term gains from trade in open access might dissipate over time as the resource becomes extinguished, leaving the resource-rich country worse off in the long run, regardless of whether the country is a small open economy or large (Brander and Taylor, 1997a,b, 1998).

Trade could also take place in the "wrong" direction; i.e. a country that does not have true comparative advantage in the resource-intensive product could end up exporting it because lack of property rights gives it *apparent* comparative advantage Chichilnisky (1994) She shows that if two countries are identical but for their property rights regime, i.e. one has clearly defined property rights, while one has none, then the country that has no property rights will export to the country that does and will be worse off in the long run because of resource exploitation. Karp et al. (2001) extends this to imperfect property rights in both large economies; trade still flows from the country with a higher degree of open access.

Recognizing that certain economic activity can result in resource destruction, some theoretical models incorporate the land conversion issue and resource loss more generally. Skonhoft (1998) finds that the social planner will allocate less land for the resource habitat in the long run if the alternative uses for the land is more profitable. Barbier and Burgess (1997) develop a model that determines the optimal allocation of land use between forestation and agriculture that depends on the degree to which agents internalize the positive externalities of forests. Bulte and Horan (2003) study habitat conversion in a open access scenario and find that multiple equilibria exist that depend on the extent of the spillover.

The papers that add trade to this specific feature of land conversion or habitat destruction generally do so in a open access framework. Jinji (2006) tweaks Brander and Taylor's small open economy model to account for unique features of a terrestrial resource stock; by allowing the carrying capacity of the stock to vary, the resource-rich small open economy could see its stock increase following trade liberalization. Smulders et al. (2004) extend Brander and Taylor's 2x2x2 large open economy model by adding a third sector, agriculture, the major source of habitat destruction and find that trade can also intensify habitat destruction though the agricultural channel. The one exception is Hartwick et al. (2001), which model the land allocation decision of a social planner dynamically within a small open economy. They first present their findings for the baseline model when land conversion is irreversible and then extend it to a case where the land is cleared and then allowed to recover. My theoretical model extends the baseline model presented in Hartwick et al. (2001) to account for lack of property rights over land.

Proponents of trade liberalization argue that the establishment of the property rights itself might be a consequence of trade, i.e. they endogenize the property rights establishment. Free trade increases the value of renewable resources over time; this coupled with faster resource implementation under open access might galvanize governments to grant property rights to protect their assets (Copeland and Taylor, 2009). Their theoretical model shows that an economy with the right combination of properties, such as a high time preference and fast growing resource stock, can make the transition into optimal resource management following trade liberalization. Hotte et al. (2000) model another aspect of endogeneity: a firm's decision to enforce its property rights over renewable resources. If the cost of enforcement exceeds the resource value, enforcement is not profitable, resulting in de facto open access despite well-defined property rights. They analyze trade liberalization in this context and find that switching from open access in autarky to enforcement under free trade might lead to resource conservation, but welfare loss because the more valuable resource demands more costly enforcement.

Collectively summing up these theoretical predictions, exports of wood and wood byproducts should fall after closing the open-access inefficiency, the forest stock should recover, and resource-rich Brazil would be better off under optimal management when trading. I show empirically that a property rights regime change increases resource extraction and exports, contrary to theoretical predictions. This paper fills a much needed gap in the literature, where empirical tests are rare due to lack of data. Notable exceptions include Taylor (2011), who empirically attributes the virtual extinction of the buffalos in the 1800s to trade, open access, and a key technological improvement in Britain that increased demand for buffalos. Bulte and van Kooten (1999) provide evidence that the ivory trade ban was more effective in conserving the African elephant in Zambia than was free trade.

Papers in the development literature have also addressed property rights and squatting. Besley (1995) develops a theoretical model that shows that having a land title allows individuals to invest in their land because it gives them security and freedom from expropriation; he provides empirical evidence from Ghana to back it up. Mendelsohn (1994) presents two theoretical model of squatters; one shows that rent dissipates if property rights are excessively defended, while the other shows that even relatively low threats of appropriation can discourage investment in long-term assets such as forestry. My model incorporates features of the Besley model in the agricultural sector.

Previous research has examined Brazil's property rights problems as well. Alston et al. (1999) use game theory to model rural conflict involving squatters in Brazil; they also provide empirical evidence using state-level data from 1988-1995 and find that the Brazil's land reform policy might incentivize agents to engage in more violence. Araujo et al. (2009) model deforestation as a way to secure property rights and also incorporate violent grabbing. Empirically, they find that insecure land rights could have contributed to higher deforestation rates in the Brazilian Amazon between 1988-2000. In this paper, I use a better dataset at a more disaggregate level, as well as a newer titling scheme that varies in the ease of titling as well as pricing. By considering wood exports rather than deforestation directly, I am also able to use a more precise estimation strategy.

## 1.3 A Dynamic Model of Competing Land Use

I extend the small open economy model presented in Hartwick et al. (2001) to a second-best world with a lack of property rights on land, which can support forests or crops. I show that following property rights establishment, more land is allocated to sustainable forestry in the long run, the typical "tragedy of the commons" correction effect. However, the model also predicts that the rate of deforestation and exports could increase in the short-run, based on the initial conditions and the timing of the property rights introduction.

### 1.3.1 Framework

Given that the focus of this paper is the establishment of property rights within a trading economy, I assume for simplicity that the small open economy in this model is already in free trade. Each period, a representative squatter, endowed with one unit of land, decides how much land to clear in order to produce the agricultural good $A$, the price of which is normalized to one. Clearing land results in timber $T$, a byproduct that can be sold at world price $p$. The squatter engages in sustainable forestry on land that is not utilized for agriculture in order to produce the forestry good $F$, priced at $P_F$ in the world market. Note that the forestry good $F$ that is sustainably harvested is inherently distinct from the byproduct $T$. One can think of the forestry good as a composite good that includes the nut, sap etc., in addition to the physical wood.[8]

The production functions for the forestry product $F$ and the agricultural good $A$ are given by two strictly concave functions, $f(L)$ and $g(1-L)$ respectively, where the only input is the forestland $L$ or the agricultural land $1-L$. The cost of clearing $R$ hectares of land for agriculture is given by the function $C(R)$, which is strictly increasing and strictly convex for $R > 0$.

I make the following key assumptions in the model. First, land is abundant and equals $N$, which is a magnitude greater than the economy's static population $n$. However, the representative squatter can manage only one unit of land in any given period. Thus, the economy's "productive" land that results in any output $n$ is less than the total endowment of land $N$.[9] Second, most of the squatter's land contains forests initially, i.e. $L_0$ is close to 1 at time $t = 0$; if the land is fully covered in forests, the marginal revenue of agricultural production will be so high that the squatter will rapidly clear the land for cultivation. Third, land clearing is irreversible; once the land is converted to agriculture, it remains so indefinitely. I find that this has indeed been the case historically in Brazil.[10] Fourth, the small open economy exports the wood products $T$ and $F$ in free trade; given the initial assumption regarding the

[8]The two similar products could also be differentiated by quality; the sustainably harvested timber may be of higher quality due to careful selection and better extraction techniques than the timber that is razed. Or consumers might have a preference for sustainably harvested wood and be willing to pay a higher price for it.

[9]This assumption captures the Brazilian Amazon's sheer vastness; many areas remain untouched by human activities.

[10]Further, given that trees take on average 25 years to mature for harvesting, the opportunity cost of converting agricultural land back to forest land is often times insurmountably large.

abundance of forestland, the direction of trade flow is intuitive.

### 1.3.2 The Open Access Regime

I model the absence of property rights as an exogenous threat of appropriation, similar to Besley (1995). At each moment in time, there exists a probability $\gamma$ that the agent loses his land, and the future stream of all revenue, due to some exogenous threat (violent land grabbing, government expropriation etc.) Thus, at any given moment, his expected total revenue is $(1-\gamma)[P_F f(L) + pR(t) - C(R) + g(1-L)]$.

Though there is a vast area of unused land $N - n$ that could be claimed, the "productive," squatted land is more accessible to roads/markets, making appropriation more attractive than merely claiming unused land. Indeed, violent land grabbing in Brazil typically occurs in areas that are more accessible to roads and rivers. It is also because of this feature that the agent does not claim the unused land in my model. While squatters in reality would prefer start over in the less desirable land following land appropriation, this simplifying assumption allows me to solve the model neatly.

#### Wealth Maximization

The agent chooses the time path for the rate of clearing $\{R(t)\}$ to maximize his expected wealth, given $L(0) = L_0$, and the law of motion $\dot{L} = -R(t)$:

$$EW(t) = \int_0^\infty [P_F f(L(t)) + pR(t) - C(R(t)) + g(1 - L(t))]e^{-(r+\gamma)t}dt. \tag{1.1}$$

I further assume that there are no fixed costs in land clearing: $C(0) = 0 = C'(0)$

The current value Hamiltonian is:

$$H = P_F f(L) + g(1 - L) + pR - C(R) - \lambda R, \tag{1.2}$$

where $\lambda$ is the co-state variable that captures the shadow price of $L$. From this, I can write down the necessary conditions:

$$p - C'(R) = \lambda \tag{1.3}$$

$$\dot{\lambda} = (r + \gamma)\lambda - [P_F f'(L) - g'(1 - L)] \tag{1.4}$$

and the transversality conditions:

$$\lim_{t \to \infty} e^{-(r+\gamma)t} \lambda(t) \geq 0, \qquad \lim_{t \to \infty} e^{(r+\gamma)t} \lambda(t) L(t) = 0. \tag{1.5}$$

Substituting the $\lambda$ from (1.3) into (1.4) allows us to equate the two necessary conditions:

$$\dot{\lambda} = (r + \gamma)[p - C'(R)] - [P_F f'(L) - g'(1 - L)]. \tag{1.6}$$

Replacing $R$ with $-\dot{L}$ from the law of motion and differentiating equation 1.3 with respect to time allows us to substitute $\dot{lambda}$ in 1.6 with:

$$C''(-\dot{L})\ddot{L} = (r + \gamma)p - (r + \gamma)C'(-\dot{L}) - P_F f'(L) + g'(1 - L). \tag{1.7}$$

The solution to this second order differential equation characterizes the optimal path of $L(t)$, the land allocated to sustainable forestry.

**Steady State Characterization**

In steady state, conversion of land will stop, i.e. $R = 0$. This implies that $\dot{L} = 0 = \dot{\lambda}$ and $C(0) = C'(0) = 0$. Then, modifying (1.6) gives us:

$$(r+\gamma)\Big[p - C'(0)\Big] = P_F f'(L_\infty) - g'(1 - L_\infty), \tag{1.8}$$

where $L_\infty$ is the steady state allocation of land. The equation states that land conversion will stop when the marginal cost in terms of interest lost on conversion profits equals the marginal net benefit of not converting. To ensure an interior steady state solution, i.e. $0 < L_\infty < 1$, the following must hold in addition to the properties of the cost function $C(R)$ mentioned above: (i) $P_F f'(0) > g'(1) + (r+\gamma)p$, and (ii) $P_F f'(1) < g'(0) + (r+\gamma)p$.

Figure 1.1 illustrates one optimal conversion path to steady state and the tradeoffs in the land allocation decision. Here, I denote $r^\circ \equiv (r+\gamma)$. From equation 1.6, the squatter faces the following dilemma: converting the marginal unit of land for agricultural production nets him $g'(1-L)+r^\circ\lambda$, while keeping that unit in sustainable forestry results in $P_F f'(L) + \dot{\lambda}$. The figure also shows that the marginal conversion profit, $\lambda$, increases over time, but the rate at which it increases, $\dot{\lambda}$ decreases with time.

As discussed in Hartwick et al. (2001), this feature results in three phases of land clearing. In the first phase, for all land allocation values between $L_0$ and $L_1$, the marginal timber profit $\lambda$ is negative. In this phase, the return to agriculture is so high that the squatter, in a hurry to obtain agricultural land, will clear land rapidly and incur a loss in land conversion. The point at which the timber byproduct of land clearing starts becoming profitable (i.e. $\lambda = 0$) is denoted by $L_1$.

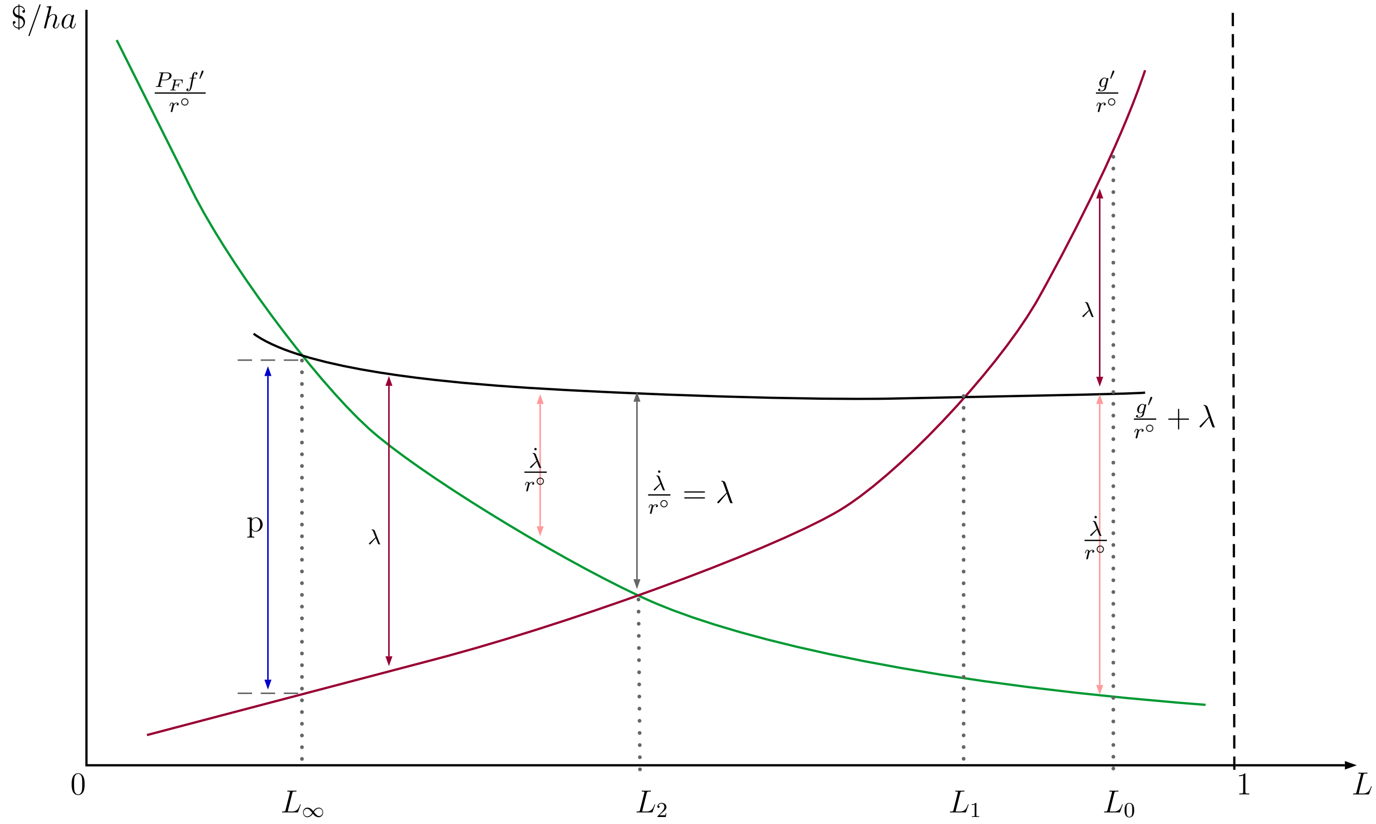



Figure 1.1: Second-Best Steady State Convergence Path

Marginal profit continues to increase as $L$ approaches $L_\infty$ because the rate of clearing slows down over time. The land allocations between $L_1$ and $L_2$ mark the second phase, when both the positive timber profits, as well as the higher return to agriculture, drive the land conversion. However, the marginal return to agriculture falls steadily with each additional unit of land that is converted; at $L_2$ the marginal return to sustainable forestry equals the marginal return to agriculture and $r^\circ\lambda = \dot{\lambda}$. Beyond $L_2$, in the final phase, timbering continues to be profitable, but the cleared land that can be utilized for agriculture gets the squatter less revenue than leaving the land in sustainable forestry; $\lambda$ increases, but by less than $r$, as the economy approached steady state. At $L_\infty$, $\dot{\lambda} = 0$ and the shadow price of land $\lambda = p$. Initially, the primary "product" is the cleared land, whereas in the final phase, timber from land clearing is the primary product.

In Figure 1.2, I sketch the phase diagram of the dynamic land allocation decision. Equation 1.3 gives us the $\dot{L} = 0$ locus. Since $\dot{L} = -R = 0$, $\lambda = p - C'(0) = p$. The $\dot{\lambda} = 0$ locus comes from equation 1.4. For convenience, I define the marginal benefit of forests $MB_F(L) = P_F f'(L) - g'(1-L)$. So, $\dot{\lambda} = r^\circ\lambda - MB_F(L)$ and at the $\dot{\lambda} = 0$ locus, $\lambda = MB_F(L)/r^\circ$, which is decreasing in $L$. The intersection of the two loci gives us the steady state forest land allocation $L_\infty$.

First, let us look at the $\dot{L} = 0$ locus. If $\lambda < p$, then $C'(R) > 0 \Rightarrow \dot{L} < 0$ because $R$ is positive; thus, $L$ is decreasing and moves left. If $\lambda > p$, then $C'(R) < 0$, which implies $L$ should move to the right. However, because land clearing is irreversible in this model, $C(R)$ is not defined for $R \leq 0$. so, the equilibrium saddle path above $\lambda > p$ is not available for the agent.

Now to determine the movement of $\lambda$, notice that $\dot{\lambda}$ is decreasing in $MB_F(L)$ and thus, increasing in $L$. If the agent moves to the right of the $\dot{\lambda} = 0$ locus, then $L$

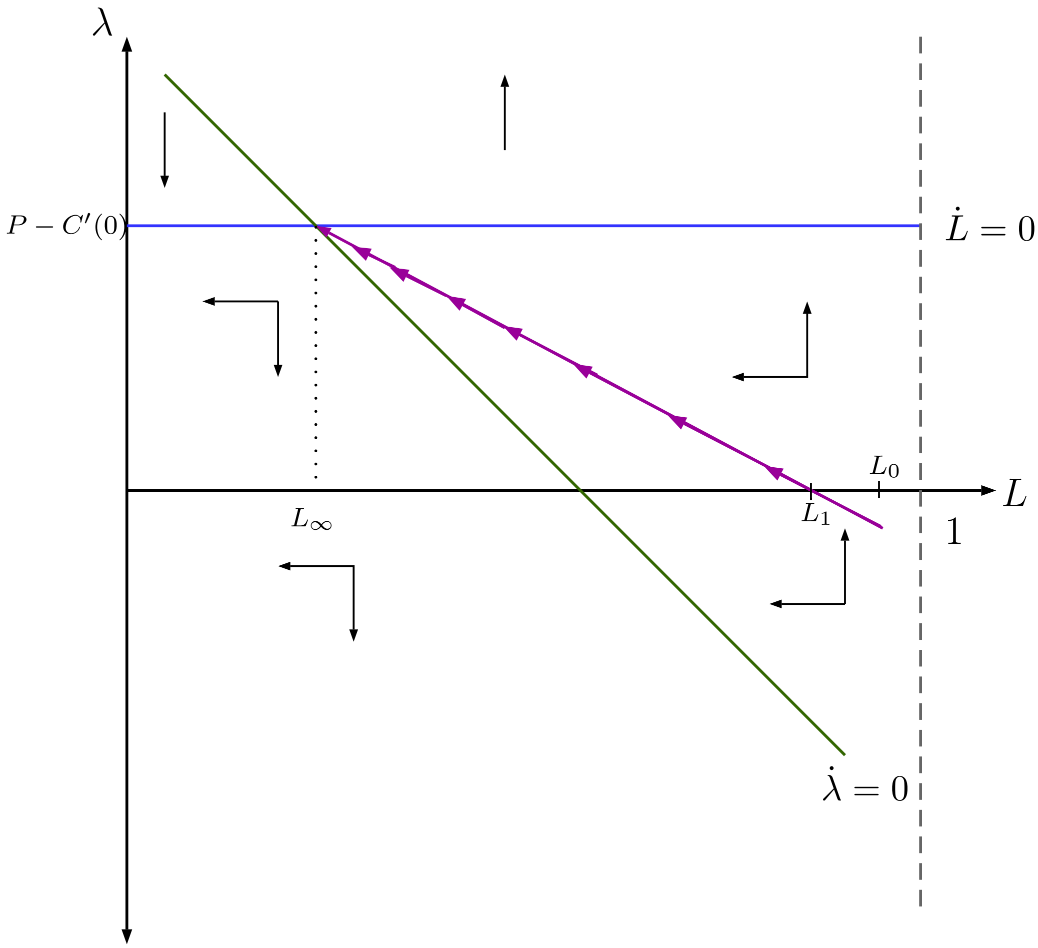



Figure 1.2: Phase Diagram

increases $\longrightarrow \dot{\lambda} > 0 \longrightarrow \lambda \uparrow$ and vice-versa if he is to the left of the locus.

Hence, there is only one equilibrium saddle path, which approaches the steady state from the bottom left. In this example, when the agents starts land clearing at $L_0$, he clears trees rapidly, which generates marginal timbering loss. The point where the saddle path intersects the $\lambda$-axis at 0 is the $L_1$ allocation, beyond which point land clearing becomes profitable.

### 1.3.3 Property Rights Establishment

The government introduces a new policy that allows the agent to acquire rights to his land at no cost. In the property rights regime, the threat of appropriation $\gamma$ falls to zero. This results in the new solution to the wealth maximization problem:

$$\lambda = P - C'(R) \tag{1.9}$$

$$\dot{\lambda} = r\lambda - [P_F f'(L) - g'(1 - L)] \tag{1.10}$$

The only change from the open access problem is the elimination of the additional term in the discount factor. The marginal returns to forestry and agriculture shift up because the actual interest rate is less than the effective interest rate under open access, i.e. $r < r^{\circ}$.

Figure 1.3 incorporates the changes following property rights establishment into the phase diagram presented above. The $\dot{L} = 0$ locus does not move because the price of timber is constant and equal to $p$. The new locus $\dot{\lambda}^{PR} = 0$ becomes steeper because $r < r^{\circ}$ and pivots around the point where $\lambda = 0$ where the two equal the marginal benefit of forestry. Thus, the new steady state, $L_{\infty}^{PR}$, moves to the right of the old steady state land allocation $L_{\infty}^{OA}$.

Under the property rights regime, the agent stops land clearing sooner and allocates more land to the forests because the decrease in the effective interest rate induces more forward-looking behaviour. Because of the uncertainty created by the lack of property rights, the agent overexploited the forests to pad his income with the additional timbering profits: the characteristic "tragedy" of the commons. I state this model prediction more formally:

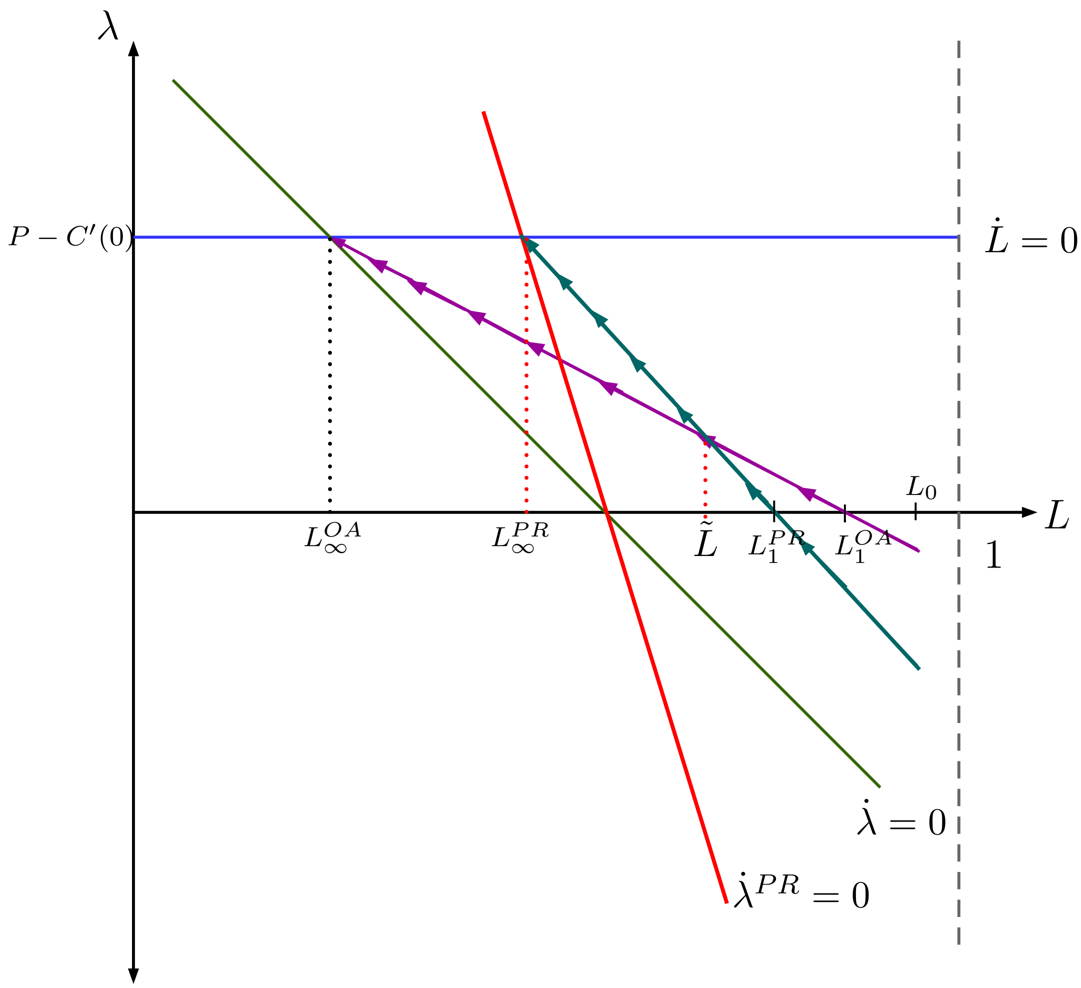



Figure 1.3: Phase Diagram following Property Rights Establishment

**Theorem 1** ('Tragedy of the Commons' Correction)**.** *For* $L_x > L_\infty^{PR}$*, where* $L_x$ *is the agent's allocation of forest land at the onset of property rights, the agent stops land clearing sooner and reaches the new steady state allocation of land* $L_\infty^{PR} > L_\infty^{OA}$*.*

*Proof.* The steady state allocation of land under the open access regime is determined by $p = MB_F(L_\infty^{OA})/r^\circ$, where $r^\circ \equiv r + ln(1/\gamma)$. With the introduction of property rights, $p = MB_F(L_\infty^{PR})/r$ determines the new steady state. Given constant world price of timber $p$ that remains unchanged, it follows that $MB_F(L_\infty^{OA})/r^\circ = MB_F(L_\infty^{PR})/r$. $r < r^\circ$ for $\gamma \in (0,1)$, which necessitates $MB_F(L_\infty^{PR}) > MB_F(L_\infty^{OA})$ for the equality to hold. Since $MB_F$ is decreasing in $L$, $L_\infty^{OA} < L_\infty^{PR}$. □

However, if property rights are established too late, and the agent has already cleared land beyond the new steady state convergence point, i.e. $L_x < L_\infty^{PR}$, at the onset of property rights, then the agent cannot attain first-best and is stuck at $L_x$ because he cannot replant trees on the agricultural land. This corollary to Theorem 1 is stated below:

**Corollary 1.1.** *If $L_x < L_\infty^{PR}$ at the onset of property rights, then $R = 0$ and $L_x \geq L_\infty^{OA}$ is the forestry land allocation in the property rights regime.*

It is also possible for the land clearing rate to increase following property rights establishment for $L_x > L_\infty^{PR}$, when transitioning to the new, steeper saddle path $L(t)^{PR}$. $L(t)^{PR}$ intersects the old equilibrium path $L(t)^{OA}$ at $\tilde{L}$ if the slope of the new saddle path is steeper; one such example is depicted in Figure 1.3. If $L_x > \tilde{L}$ when the agent obtains the land rights, then the agent will *increase* the rate of land clearing $R$ and catches the new saddle path from below. I call this effect the "productivity boost." More formally,

**Theorem 2** ('Productivity Boost')**.** *If $L(t)^{PR} = L(t)^{OA} = \tilde{L}$ is the point of intersection of the two equilibrium land allocation paths, and $L_x > \tilde{L} > L_\infty^{PR}$, where $L_x$ is the agent's forest land allocation at the onset of property rights $\Longrightarrow$ the rate of clearing $R \uparrow$ following property rights establishment. If $L_x < \tilde{L}$, then $R \downarrow$.*

In lieu of a generalized proof, I provide an example in Section 1.3.4, where I assign functional forms to $f(.), g(.)$ and $C(.)$ and show that the slope of the saddle path increases following property rights establishment in the closed form solution. I also show that the rate of land clearing $R$ can increase or decrease.

Thus, if the agent is to the right of $\tilde{L}$ when he obtains property rights, he increases his clearing rate $R$ to access the valuable agricultural land more rapidly until he reaches $\tilde{L}$. This increase in the rate of land clearing results in more deforestation in the short run and consequently, an *increase* in exports. However, if he is already to the left of $\tilde{L}$ at the onset of property rights, then he decreases his rate of clearing $R$ to get on the new equilibrium saddle path.

### 1.3.4 An Example

Drawing on the example provided in Hartwick et al. (2001), suppose $f(L) = L - L^2/2$, $g(1-L) = (1-L) - (1-L)^2/2$, and $C(R) = R^2/2$. Given $r^\circ \equiv (r+\gamma)$ under open access, we can rewrite equation (1.7) as

$$\ddot{L} - r^\circ \dot{L} - (1 + P_F)L = r^\circ p - P_F. \tag{1.11}$$

The solution to this second order differential equation is:

$$L(t) = (L_0 - L_\infty)e^{-\beta t} + L_\infty, \tag{1.12}$$

where $L_\infty = \frac{P_F - r^\circ p}{P_F + 1}$, and $\beta > 0$ is the slope of the saddle path:

$$\beta(r) = \frac{1}{2}\left[\sqrt{(r^\circ)^2 + 4(1+P_F)} - r^\circ\right]. \tag{1.13}$$

Following property rights introduction, $\gamma = 0$ and $r^\circ > r$. Given that $L_\infty(r)$ and

$\beta(r)$ are decreasing in $r$, $L_\infty^{PR} > L_\infty^{OA}$ and $\beta^{PR} > \beta^{OA}$. Thus, the slope of the saddle path increases.

The rate of land clearing $R = -\dot{L} = \beta(L_0 - L_\infty)e^{-\beta t} > 0$. The comparative statics of $R$ with respect to $L_\infty$ and $\beta$ is given by:

$$\frac{\partial R}{\partial L_\infty} = \beta e^{-\beta t} > 0 \tag{1.14}$$

$$\frac{\partial R}{\partial \beta} = (L_0 - L_\infty)e^{-\beta t}\left[1 - \beta^2\right] \gtrless 0 \tag{1.15}$$

Thus, the rate of land clearing $R$ increases in $L_\infty^{PR}$, but can increase or decrease in $\beta$, the magnitude of the slope of the saddle path.

To show $R^{PR} > R^{OA} \iff \beta^{PR}(L_0 - L_\infty^{PR})e^{-t\beta^{PR}} > \beta^{OA}(L_0 - L_\infty^{OA})e^{-t\beta^{OA}}$. Assuming $t = 0$ and $L_0 = 1$, this can be rewritten as:

$$\frac{1 + rp}{1 + r^\circ p} > \frac{\beta^{OA}}{\beta^{PR}} = \frac{\sqrt{(r^\circ)^2 + 4(1 + P_F)} - r^\circ}{\sqrt{r^2 + 4(1 + P_F)} - r} \tag{1.16}$$

In Table 1.1, I provide some numerical values for the parameters and world prices where the rate of deforestation can increase or decrease. The first scenario shows that the rate of deforestation increases after property rights establishment, i.e. $R^{PR} > R^{OA}$, and vice-versa in Column (2). This exercise shows that if the timber price $p$ and threat of appropriation $\gamma$ are high, then the deforestation rate will decrease in the property rights regime.

This model shows that the effect of property rights establishment on land clearing is complex and depends on many factors. If the agent obtains rights to his land early enough, he will allocate more land to forests in the long run, a pareto-improving outcome. However, the path to the new steady state might entail more deforestation

Table 1.1: Parameter Values and World Prices in Two Scenarios

| *Parameters* | (1) $R^{PR}\uparrow$ | | (2) $R^{PR}\downarrow$ | |
|---|---|---|---|---|
| Forestry good price $P_F$ | 1.2 | | 1.2 | |
| Timber price $p$ | 0.1 | | 0.5 | |
| Discount rate $r$ | 0.05 | | 0.05 | |
| Threat of Appropriation $\gamma$ | 0.1 | | 0.2 | |
| Effective Discount rate $r^{\circ}$ | 0.15 | | 0.25 | |
| *Solutions* | Open Access | Property Rights | Open Access | Property Rights |
| Steady State Land Allocation $L_{\infty}$ | 0.539 | 0.543 | 0.489 | 0.534 |
| Slope of Saddle path $\beta$ | 2.820 | 2.917 | 2.727 | 2.917 |
| Rate of Deforestation $R$ | 1.301 | 1.332 | 1.394 | 1.359 |

Note: $R = \beta(1 - L_{\infty})$ at $t = 0$, assuming $L_0 = 1$

in the short run transition, depending on how far along the agent is in the land-clearing process when property rights are granted.

## 1.4 Policy Briefing: *Terra Legal*

To combat the severe threat to the Amazon and quell land disputes and violent land-grabbing, the government introduced its latest titling scheme in 2009 called *Terra Legal*. The provision grants rights to squatters who had been utilizing the land for at least five years; so, if a person had been living on a piece of land in the Amazon from 2004, he would be eligible to start the titling process in 2009. However, it is unclear whether the programme benefits the targeted recipients and whether it will succeed in its goals of reducing deforestation, particularly due to widespread corruption and weak enforcement.[11]

[11] "Brazil grants land rights to squatters living in Amazon rainforest." The Guardian. June 26, 2009. http://www.theguardian.com/environment/2009/jun/26/amazon-land-rights-brazil.
"Illegal Land Occupation in Terra Legal." O Eco Amazonia. August 6, 2010. http://www.//oeco amazonia.com/en/news/brazil/54-grilagem-no-terra-legal-

Though this is the latest in Brazil's long line of land reform measures, this policy differs in its registration efforts and pricing scheme. To make registration easier, officials go to the Amazon to register the squatters rather than requiring them to travel to the bureaucratic headquarters. In the past, many squatters did not take advantage of their constitutional right to obtain land rights due to the high costs involved with titling and the bureaucratic backlog Araujo et al. (2009). By taking the registration to the squatters, the government hopes to increase uptake.

The rollout of the programme is not strictly phased (in that a squatter could travel to get his land registered whenever he chooses), but the extensive registration process effectively turns it into a phased rollout. Officials travel to each municipality to promote the titling scheme and then examine the property being registered to ensure it had been squatted upon for at least five years, as well as to determine the exact border and geocode it. Registering typically took about two weeks, depending on how large the municipality is and the number of people applying for a title, after which the officials moved on to the next municipality. Since this is a federal programme with federal employees and officials, there is not much influence the state can exert on the rollout.

Of course, the concern that the rollout could be endogenous to deforestation probably comes to mind; if the programme hopes to reduce deforestation and targets the areas with the highest deforestation, problems with identification can arise. To get more clarity on the programme, I met with *Terra Legal* employees. This programme is part of a larger scheme to gecode and catalogue the lands in the Amazon. At a monthly meeting, heads of the different agencies, such as FUNAI that is responsible for the indigenous people, the Environmental Ministry and Terra Legal, gather and determine which portions of a particlular (large) area of land, called a "gleba," fall under each of their purview.[12] If all the agencies gathered do not claim any of these parcels in the *gleba*, then *Terra Legal* then steps in and tries to get those areas titled, in no particular order. The government hopes that this comprehensive effort will help its monitoring in the future because it can easily identify on whose land the trees are being cut, thanks to the national catalogue or "Cadastro" of land owners.

There are two points to observe here: that *Terra Legal* truly is a transfer of public to private land ownership, since no one has claimed the land that are titled. Second, this extensive selection process, though not random, does not create problems for my research question because it is not endogenous to deforestation, agriculture or exports, other than the fact that the land had to be squatted on before 2004.

The other interesting aspect of the programme, apart from the rollout, is its controversial pricing scheme that makes titling effectively free for many squatters, incentivizing them further to take up the titling. The pricing, like the rollout of the programme, also varies by municipality. For land that is under a certain size (less than one *modulo fiscale*), the cost of the titling is free; if it is between one and four *modulo fiscales*, the land owner must pay some price that is less than market value, with payments phased out over 20 years; above this level, the land owners can get rights to the land, but must pay market price, again over a 20 year period.[13] Each municipality defines the exact size of its *modulo fiscale*, ranging from 5 to 100 hectares, with the average *modulo fiscale* equaling about 70 hectares, which is less than one square kilometre.

Between July 2009 and November 2012, more than 100,000 applicants had already

[12] *Glebas* appear to have been determined around the time when colonists settled Brazil; perhaps it is a remnant of that time, but my contact was not able to explain it further than that. It is certainly nothing that is determined because of deforestation in the near present.

[13] The law specifies the sale at market value, but it is unclear how exactly the government determines this price or what it is.

registered their land. 95% of the registered farms are in the Amazon region, in the states of Amapa (AP), Amazonas (AM), Acre (AC), Mato Grosso (MT), Maranhao (MA), Para (PA), Roraima (RR), Rondonia (RO) and Tocantins (TO).

In Table 1.2, I provide some fast facts about the registered and titled farms. The total area registered is around 125,000 square kilometres (or over 31 million acres); this is approximately 2.58% of the Brazilian Amazon. The average size of a registered farm is 124 hectares (1.24 square kilometers) and 1.76 *modulo fiscales (mfs).* Each municipality has an average of 245 registrations, and the average area registered in a municipality is 305 square kilometers.

About 3% of those registered have already obtained land titles; the titled area spans roughly 2,250 square kilometers (about 558,000 acres) and is solely in the Amazon. More than half the registrations are the "small" farms measuring less than 1 *mf*; "medium" farms make up roughly 30% of the total registrations and only 14% of the registrations are "large" farms. The "small" farms go through the process fairly painlessly, which is reflected in the fact that almost 3 in 4 titled properties are small; large properties receive a thorough and careful review before being approved for the title.

Though the small farms make up the largest share of registrations at 31% on average in 2012, they only comprise about 12% of the total registered area. Properties in Category II (between 1 and 4 *mf*) make up 24% of of the total area registered. Finally, the share of registered area covered by the largest properties is 37% of the total registered area; this is unsurprising given that large farms, by their very definition, should cover a large area.

Because of the discounted pricing, cattle ranchers, large farms and logging firms have an incentive to increase their land holdings by buying land cheaply. The pro-

Table 1.2: *Terra Legal* - Fast Facts (as of 2012)

| | |
|---|---|
| Number of Registrations | 107,280 |
| ....by "Small" Farms ($a < 1mf$) | 60,505 |
| ....by "Medium" Farms ($1mf \leq a < 4mf$ ) | 31,396 |
| ....by "Large" Farms ($a \geq 4mf$) | 15,379 |
| Number of Titles | 3,015 |
| ....by "Small" Farms ($a < 1mf$) | 2,184 |
| ....by "Medium" Farms ($1mf \leq a < 4mf$ ) | 710 |
| ....by "Large" Farms ($a \geq 4mf$) | 121 |
| Total Area Registered | 126,374 sqkm |
| Total Area Titled | 2,257 sqkm |
| Average Number of Registrations (in a Municipality) | 245 |
| Average Area Registered | 305 sqkm |
| Average Size of Registered Property | 1.24 sqkm (1.76 *mf*) |
| Average size of 1 *modulo fiscale (mf)* | 0.8 sqkm |
| Share of Registered Properties with Titles | 3% |
| Share of Registered Area that is Titled | 1.8% |

gramme prohibits immediate sale of the newly titled lands, but after three years, the land can be sold on the market, most likely to large farms and logging firms. However, given that over 7000 registered farms are equal to or larger than 1000 hectares (10 square kilometres or 2400 acres) in the registration data, it appears to be the case that large farms and logging firms might have already carved out a piece of the Amazon for themselves.

Figure 1.4 shows the total area occupied by registered farms in the entire sample and their location, overlaid onto a satellite image of the Brazillian Amazon in 2010. This shows the impact land titling could potentially have on the forest stock.

To combat deforestation and promote sustainable forestry, *Terra Legal* has instituted a few safeguards. The land title states that the property owners must maintain an 80% legal reserve on the land, i.e. 80% of their property should consist of forests/trees. Failure to do could result in the appropriation of the property. To that

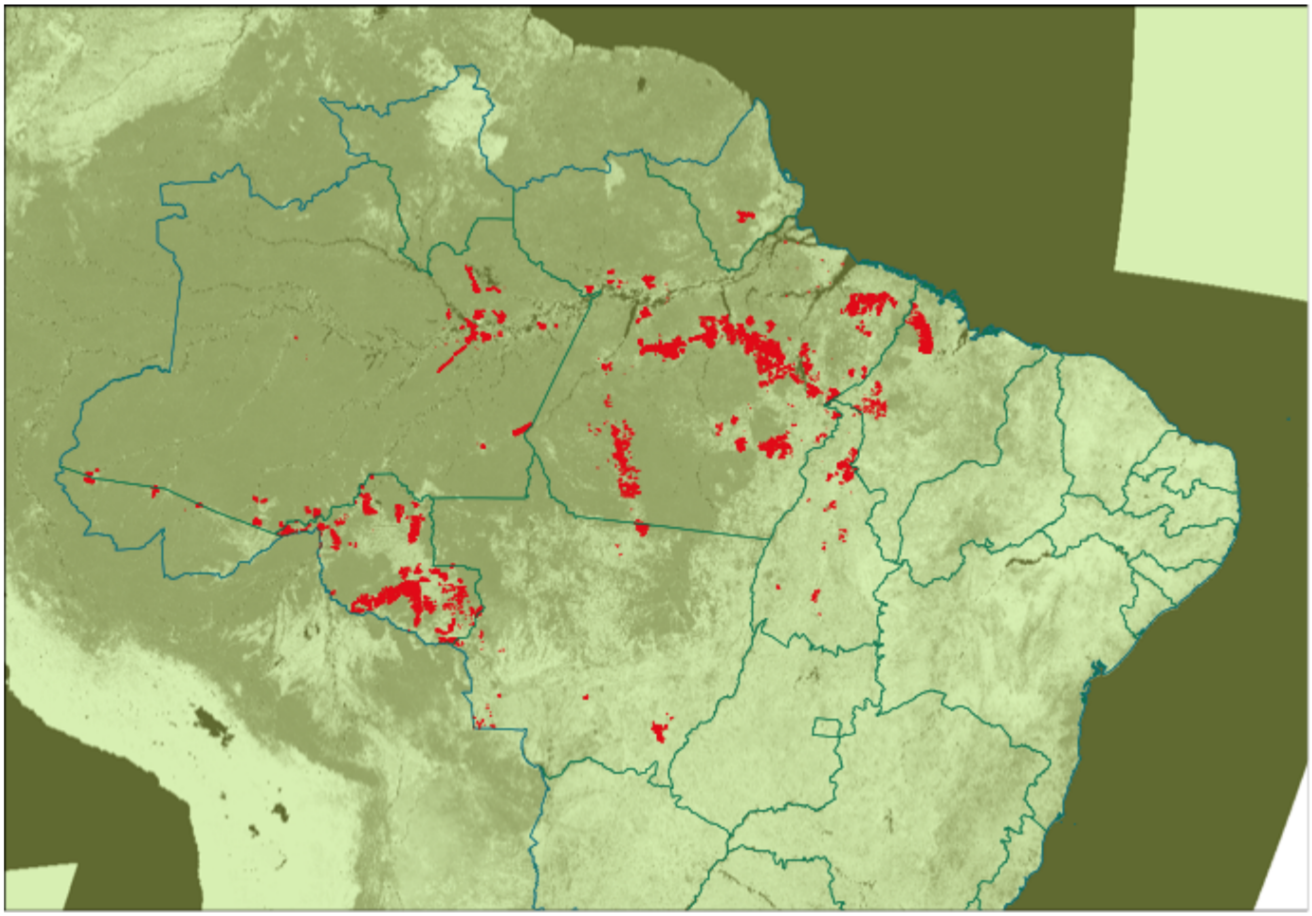


Figure 1.4: Snapshot of the Brazilian Amazon in 2010 overlaid with the entire sample of registered farms from 2009-2012

end, the small farms that can be obtained at no cost are only given a provisional title for 10 years; the final deed is only approved if this legal reserve has been met.[14] The provisional title is enough to use as collateral, however, so investments need not put on hold. Officials can also drop by unannounced to examine the property.

From what I could observe, this rule is not adhered to strictly. During my visit with the *Terra Legal* officials to a farm with the provisional title at the outskirts of

[14]Those who pay something for the property receive their actual deed, rather than the provisional one, though they are constrained from reselling it for a period of 3 years.

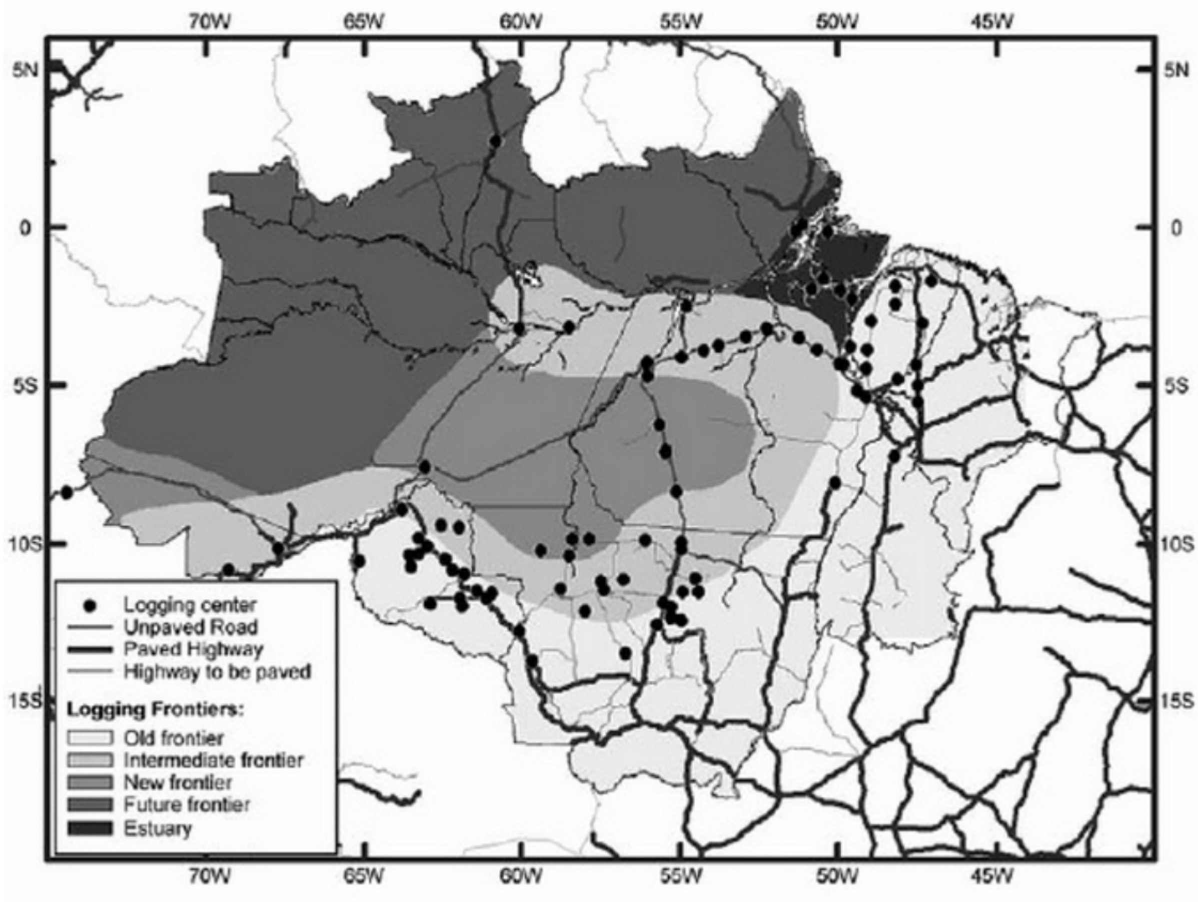
Manaus, the farmer proudly displayed the water pump he had newly installed that he was able to put in due to the loan he received using the title as collateral. When I enquired about the lack of trees, certainly nowhere near the 80% mark, he shrugged it off, saying that a fire consumed them. He had not taken any steps to replant them, at least during the time of my visit, and the officials simply shrugged it off.

### 1.4.1 Brazilian Logging Industry

Here, I provide a brief overview of the logging industry in Brazil; a more detailed account can be found in Merry et al. (2006). The value of wood-related production in Brazil equaled approximately $3.2 billion in 2003. 35% of this production came from natural forests (i.e. not plantations). In 2003, 84% of the wood production came from the northern Amazon states, where *Terra Legal* has been most prevalent. The main product from the Amazon forests is sawnwood, which is typically used for housing frames; the second most important is firewood, of which 14.16 million were harvested from natural forests in 2003 (30% of all production). The demand for timber from natural forests comes mainly from charcoal production; the percentage of natural forest wood in charcoal production increased from 28% in 1997 to 50% in 2003. This change could be explained by reduction in investments in tree plantation by the iron smelting operations, where charcoal is a main input. Natural forests yielded some 20.6 million $m^3$ of logs in 2003.

The states of Acre (AC), Mato Grosso (MT), Para (PA), and Rondonia (RO) are responsible for over 90% of the timber production. 75% of the *Terra Legal* registrations happened in these states; 72% of the titled farms (around 2,200) are also in these four states. Figure 1.5 shows the location of the logging firms in Brazil in

2003. This, coupled with Figure 1.4 should help convince readers regarding the impact of the titling scheme on the logging industry. The ten municipalities with the greatest number of firms and volume produced are: Paragominas (PA), Sinop (MT), Ariquemes (RO), Tome acu (PA), Marcelandia (MT), Jacunda (MT), Tailandia (PA), Breu Branco (PA), Vilhena (RO) and Claudia (MT). About 5000 farms are registered in these municipalities.




Source: Merry e. al. (2006), p. 284

Figure 1.5: The Logging Industry

The logging industry appears to be characterized by numerous entries and exits; the sawnwood market is particularly in flux with rapid entry and exit of firms in response to price and cost changes. Logging generally becomes more costly over time, especially if new technology is not adopted, as the easily accessible areas are harvested first. This causes the less productive firms to shut down, as they are unable to compete. On the other hand, due to lack of regulation in the forests, many smaller firms enter the logging industry.

Merry et al. (2006) also find that many mills subcontract their logging; only 36% of the mills do their own logging. The firms that tend to do their own logging are located in areas that are characterized by many small farms that own a large percentage of the forest. However, purchase of logs is quite common due to the high bureaucratic cost imposed on firms. All of these facts suggest that the titling scheme will have an impact on wood exports. One must bear in mind that Merry et al. (2006) compiled these statistics from IBGE, the Brazilian Institute of Geography and Statistics, a government source. As such, illegal logging and corruption are not taken into account, but lack of enforcement makes such practices highly likely.

## 1.5 Data

I obtain the Brazilian export data from *Aliceweb*, a website that is maintained by the Bureau of Foreign Trade and the Ministry of Development. The website publishes value in USD, net weight in Kilograms and quantity[15] information for exports and allows researchers to download the data in a time dimension (as low as monthly) and

[15]Quantity is not consistently reported, however and is zero for many observations that otherwise have Value and Net weight, so I do not consider quantity in my regressions.

up to two other dimensions; for example, one can download port and product level data (at the HS 6 level) every month or product and country destination data every year to name a few.

I downloaded monthly HS6 product-municipal level data from January 2007 to June 2013, but I aggregate to the quarterly level for my analysis to reduce noise.[16] As of June 2014, however, the Ministry has revoked public access to the export data at the municipal-HS 6 level, so my data is accidentally confidential. The Ministry attributes an export product to a particular municipality if the firm headquarters is located in that municipality. Thus, products produced at a plant located at municipality B, but headquartered at municipality A, would be recorded as an export from municipality A. However, I argue that this is not a major problem for this paper because of the nature of logging firms in the region, which I previously described in greater detail. Similarly, firms that export agricultural commodities from the Brazilian Amazon also do not have multiple plants in other municipalities.

Chapters 44-49 in the Harmonized System comprise of wood-based commodities. These products use wood as input to varying degrees, and are broadly grouped by how far-removed the finished product is from wood. Chapter 44, 'Articles of Wood' products are mostly raw timber, such as semi-finished sawn wood etc. Other chapters include cork, straw, wood-pulp, paper and printed material. Most of the exports from the Brazilian Amazon in my sample fall into the 'Articles of Wood' category.

Agricultural commodities span 24 chapters in the Harmonised System (HS 1 - 24). Of these, the first five are animal-related products, such as live animals, cattle and so forth. Chapters 6 to 15 are plant-based "Vegetable" products; these include coffee, tobacco, grains, nuts, fruits etc. Finally, the last nine categories are the so-

[16]The results are consistent at the monthly level, as well, and can be provided on request.

called “Prepared Foodstuffs” (HS 16 - 24), which are refined products that are more production-intensive, such as sugar, baked goods, prepared meat, tobacco and fruit juices to name a few. Typically, there is a “raw” component in the first 15 chapters that is associated with some of the refined foodstuffs. For instance, Chapters 2 - 5 include raw fish, meat of animals etc., while Chapter 16 under “Prepared Foodstuff” consists of sausages, prepared seafood and caviar.

The second dataset that I have is detailed records from *Terra Legal* of registrations from July 2009 to October 2012. This includes information on the person who applies for titling, the amount of land registered, the month and year of registration, the location of the farm, and the status of the application. My attempts to follow up on this data set and get it updated went unanswered, so I proceed with the data that I have, with registrations serving as a proxy for titling. Since very few applications for title get rejected (and the rejections are more due to incorrect completion of the application or duplication rather than any confounding reason), the registrations are an almost perfect correspondence to titling. Using this registration information, I construct my explanatory variables at the municipal-quarter level.

I determine when on-site registration reaches a municipality based on the number of registrations in the municipality each quarter. Because eager squatters can travel to the state *Terra Legal* office to register their property, it is possible for a municipality to have a few registrations before officials specifically go there to register properties. However, the month or quarter when rollout does occur, there is a distinct spike in the number of registrations in that municipality. I denote this as the time period $t$ when rollout reached municipality $i$. Following on-site registration, it is still possible for the squatters who snoozed during rollout to go register at the office. However, only a few properties register outside of the on-site time frame. For a majority of the municipalities, registrations are zero before and after rollout and only positive during.

Finally, I augment my dataset with a third dataset from IBGE, the Brazilian Institute of Geography and Statistics; this dataset provides information on the area cultivated in a municipality annually, which I use as a control in my regression.

Table 1.3 provides the summary statistics of the variables that I use in my regressions, broken down by category. Columns (1) and (2) provide the statistics for all wood-based exports and 'Articles of Wood' exports respectively. Column (3) provides the statistics on all agricultural exports; columns (4-6) provide the statistics for the subsamples: Animal-based, Plant-based and Prepared food. The outcome variable, 'export value,' is in thousands of dollars, and export net-weight is reported in thousands of kilograms.

The number of municipal-product-quarter observations for agricultural commodities is roughly 1.5% higher than for wood-based exports. This is not only because more municipalities export agricultural products, but also because there are more products that are exported. Thus, the average export value and net-weight for the agricultural sample (Column 3) is an order of magnitude larger than the values reported for wood-based exports (Column 1). "Consecutive Exporters" are municipalities that export for any two consecutive quarters in the sample period; interestingly, a majority of the municipalities that export wood products do so for consecutive quarters (70%-75%), while those that export agri-based products are roughly half the number of observations.

"Rollout" is 1 only during the quarter that registrations occur on-site and 0 otherwise, while"Post Rollout" is 1 for all quarters after registrations occur on-site. A third of the wood-based observations are post-rollout, while it is double that (67%)

Table 1.3: Summary Statistics by Product Category

| VARIABLES | (Wood-based) | (Wood Only) | (Agri-based) | (Animal) | (Plant) | (Foodstuffs) |
|---|---|---|---|---|---|---|
| Export Value (in thousands of USD) | 255 | 243 | 1,316 | 773 | 2,090 | 730 |
| | (1,920) | (1,303) | (8,845) | (4,131) | (11,500) | (7,572) |
| Export Net Weight (in thousands of Kg) | 383 | 356 | 2,827 | 273 | 5,352 | 1,666 |
| | (3,938) | (3,337) | (23,700) | (1,829) | (32,000) | (20,400) |
| Consecutive-Quarter Exporters | 0.70 | 0.76 | 0.54 | 0.49 | 0.63 | 0.68 |
| | (0.46) | (0.43) | (0.50) | (0.50) | (0.48) | (0.47) |
| Rollout | 0.032 | 0.031 | 0.466 | 0.302 | 0.381 | 0.609 |
| | (0.176) | (0.173) | (0.499) | (0.459) | (0.486) | (0.488) |
| Post Rollout | 0.331 | 0.331 | 0.672 | 0.584 | 0.619 | 0.755 |
| | (0.470) | (0.470) | (0.469) | (0.493) | (0.486) | (0.430) |
| Eligible for Rollout | 0.74 | 0.72 | 0.75 | 0.72 | 0.40 | 0.86 |
| | (0.44) | (0.45) | (0.43) | (0.45) | (0.49) | (0.35) |
| Area of Municipality (in sqkm) | 10,880 | 10,743 | 9,972 | 8,163 | 9,918 | 11,790 |
| | (16,420) | (17,215) | (12,550) | (11,251) | (12,932) | (12,917) |
| Total Area Registered (in sqkm) | 227 | 248 | 121 | 116 | 114 | 136 |
| | (872) | (862) | (556) | (427) | (577) | (628) |
| .....by “Small” Farms (in sqkm) | 25.5 | 28.2 | 13.6 | 14.5 | 13.6 | 12.8 |
| | (83.51) | (87.5) | (59.1) | (53.6) | (64.9) | (55.3) |
| .....by “Medium” Farms (in sqkm) | 70.0 | 74.9 | 43.4 | 39.3 | 40.0 | 52.4 |
| | (258.1) | (269.0) | (175.8) | (141.9) | (182.6) | (194.0) |
| .....by “Large” Farms (in sqkm) | 132 | 144 | 64.0 | 62.5 | 60.5 | 70.5 |
| | (534.9) | (559.1) | (343.2) | (260.7) | (353.6) | (394.2) |
| Area Cultivated (in sqkm) | 284.6 | 325.2 | 557 | 506 | 802 | 253 |
| | (935) | (1,007) | (1,375) | (1,507) | (1,519) | (864) |
| Observations | 20,581 | 17,480 | 36,048 | 10,184 | 15,028 | 10,632 |

Note: Observations are at the municipal/product/quarter-year level. The sample consists of 807 municipalities in Brazilian Amazon and spans from 2007 Q1 - 2012 Q4. All products are the HS6 level. Column 1 considers all wood-related products(HS Chapters 44 - 49); Column 2 limits the sample to “Articles of Wood” products only (Chapter 44). Column 3 includes all agricultural exports (Chapters 1-24). Columns 4-6 break this down into Animal-based (Chapters 1-5), Plant-based (Chapters 6-15) and Prepared food (Chapters 16-24) exports. “Rollout”, “Post”, “Consecutive Exporters” and “Eligible for Registrations” are dummy variables. “Rollout” is 1 only during the quarter that registrations occur on site and 0 otherwise, while“Post Rollout” is 1 for all quarters post rollout. “Eligible for Registration” municipalities are those that have any registrations during the sample period. “Consecutive Exporters” are municipalities that export for any two consecutive quarters in the sample period. All “Share” variables are true percentages. Registrations are broken down by size, defined as follows: “Small” Farms - less than 1 *mf*; “Medium” - between 1-4 *mf*; “Large” Farms - more than 4 *mf*.

for agri-based product subsample. "Eligible for Registration" municipalities are those that have any registrations during the sample period. Municipalities that export vegetable products have the lowest number of observations that have any registrations (40%), while 75% of the wood-based observations are also rollout-eligible. The subsample of 'Prepared Foodstuffs' has the largest number of observations that are treated; the mean for this subsample is high for all three variables.

Total area registered area, measured in square kilometers, is a stock variable; it is divided into three subcategories according to the size of the properties: "Small" Farms each have area less than 1 *modulo fiscale (mf)*, approximately 0.8 hectares. "Medium" properties are of size 1-4 *mf*, while the "Large" Farms are bigger than 4 *mf*. In comparing the wood sample in Column (1) with the agricultural sample of Column (3), one can see that the average area registered is almost double for the former. This holds across all size variables. Not surprisingly, the average area registered by large farms is larger than those registered by small and medium farms. Finally, the subsample 'prepared foodstuffs' (Column (6) has the highest average in registered area variables within the agricultural sample.

Regarding the controls, the average area of a municipality that exports wood-based products (Column 1) is slightly larger than those that export agricultural commodities (Column 3), though those that export 'Prepared Foodstuffs' (Column 6) have the highest average area. Understandably, the area cultivated is higher for municipalities that export agri-based products and is highest for plant-based exporters.

## 1.6 Empirical Strategy: Fixed Effects

To estimate the effect of *Terra Legal* and registrations on exports, I use a fixed effects model, exploiting temporal and spatial variation in the timing of the rollout for identification. The main equation that I estimate follows:

$$\begin{aligned} Log(Value)_{itk} = \alpha + \beta_1 PostRollout_{it} + \beta_2 Log(RegArea\_Total)_{it} + \\ \beta_3 Log(RegArea\_Medium)_{it} + \beta_4 Log(RegArea\_Large)_{it} + \\ \Gamma Controls_{iy} + \eta_{ik} + \nu_t + \epsilon_{itk} \end{aligned} \tag{1.17}$$

where $Log(Value)_{itk}$ is the log-transformation of export value in USD from municipality $i$ in quarter-year $t$ of product $k$. $PostRollout_{it}$, an indicator variable that takes the value of 1 for municipality $i$ following on-site registration at time $t$, captures the effect of property rights establishment. $Log(RegArea\_Total)_{it}$ is the log-transformation of the total area registered (a stock variable measured in square kilometers) in municipality $i$ at time $t$. $Log(RegArea\_Medium)_{it}$ is the log of area registered by medium-sized properties in municipality $i$ in quarter-year $t$; these are farms sized between 1 *mf* and 4 *mf*. $Log(RegArea\_Large)$ is the log of area registered by properties sized above 4 *mf*. I do not the small-farm analogue due to collinearity; the sum of the three size variables equal the total area registered.

The main control that I use is the log-transformation of total area cultivated in municipality $i$ in year $y$, which I obtain from IBGE. If the forest is being cleared for agricultural expansion, reflected in the the amount of land cultivated each year, I would expect the effect of registrations on exports of wood products to decrease after including this control. I also include population estimates as a control. Because of the lack of controls at the municipal level, I include municipal-product fixed effect $\eta_{ik}$ to address unobserved factors affecting products in a particular municipality; for instance, if municipality $j$ subsidized production of product $a$, this will not be captured by the product or municipal effect. Quarter-year fixed effect $\nu_t$ addresses time shocks such as crises and droughts. Finally, given the likelihood that the regressors and errors are correlated within municipalities, plus the fact that the control variable is at a more aggregate level than the unit of observation, I cluster the errors at the municipal level.

$\beta_1$ captures the average "treatment" or post-rollout effect of *Terra Legal* on quarterly export value within a municipality; specifically, the magnitude of the predicted effect is $(e^{\beta_1} - 1)$. $\beta_2, \beta_3$ and $\beta_4$ are elasticities and measure the response of export value to the amount of area registered. A 1% increase in total area registered in municipality $i$ at time $t$ increases or decreases average quarterly export value within a municipality by $\beta_2$% per product. Similarly, a 1% increase in the total area registered by medium and large farms, leads to a $\beta_3$% and $\beta_4$% change respectively.

I log-transform the outcome variable, export value, for two reasons. Running the regression on the level variable is problematic because larger municipalities most likely export more, which means the average effect on export value would be overstated for small municipalities and understated for larger ones. By log-transforming the variable, I can estimate the percentage effect. The second reason for log-transforming the variable is due to the high level of positive skewness in export value, which has a long righthand tail. Municipalities either do not export at all or export products worth thousands, if not millions, of dollars. Positive skewness results in a larger mean, which is problematic because the fixed effect model demeans the variable. Log-transforming export value results in a more normal distribution.

Rather than running a log-level regression, I choose the log-log regression, i.e. I also log my explanatory 'registered area' variables. One of the main reasons I do so is to improve the linearity of the model, a key assumption for OLS. In addition, as in the case of export value, registered area variables are also positively skewed. A large number of municipalities have little registered area, but a small number of them have a large share of registered area.[17] Thus, the log-log regression better fits the data, leads to more well-behaved standard errors and is also easier to interpret.

### 1.6.1 Econometric Issues

My identification depends on the exogenous rollout of the programme. However, if the government officials treat the areas with more deforestation first, for example, then my estimates will be biased due to endogeneity. After extensive conversation with *Terra Legal* officials that I clarified above, I find that while the rollout is not completely random, it is not based upon the level of deforestation or exports in a municipality. A region that spans many municipalities is considered for rollout after monthly or bimonthly conversations with other federal agencies; then, officials implement titling within the approved regions based on proximity to their regional offices.

A second issue that I encounter is the measurement error in the exports at the municipal-level. The data attributes the timber exports to a particular municipality if the firm that exports the product has its headquarters located in that municipality. I argue that this is not major concern because the nature of logging firms is such that they do not produce across municipalities. In fact, loggers clear one area before moving onto the next to keep costs low. For those who remain skeptical, consider the

[17] The amount of registrations in a municipality is determined initially by the policy, after which there is the take-up.

nature of the measurement error. Since the measurement error is in the dependent variable, it creates an additional error term in the estimating equation. As long as the conditional error is still zero, i.e. the error term induced by imperfect measurement is orthogonal to the explanatory variable (percentage of land registered), the estimates would still be unbiased and consistent. I believe that this is indeed the case; exports attributed to an incorrect municipality is not because of registrations and vice versa. Thus, my estimate would still be unbiased. It can still lead to a larger variance than what I calculate, which can subject my coefficient to Type I error; however, my large sample size addresses this issue, leading to better estimators of variance. Thus, my estimation should be unbiased and consistent.

In terms of issues that I can verify, the first is the underlying "parallel trends" assumption of Difference-in-Differences methodology that I inherently make when calculating the treatment effect $\beta_1$. 'Post-rollout' effect can be attributed to the rollout only if the outcome variable in the treated and control groups had a parallel trend prior to the treatment. I can verify this assumption by comparing the trend in export value before rollout for the "treated" municipalities with high registrations, which I define as those with registered area above the median, and the "control" municipalities with low registrations (municipalities with registrations below the median). I do not use municipalities that do not receive registrations as the control because they might not have the same characteristics as those that are eligible for rollout.

Finally, my dataset contains a large number of zeroes because the municipalities that have positive registered area might not export at all, while those that export might have no registrations, and those that export might only do so briefly before exiting the export market. Clearly the logarithmic function is not well-definted at zero. To deal with this, I add one to the variable before taking the log; $Log(Export\_Value)$

is in actuality $Log(Export\_Value + 1)$. This should not bias my results because the values of export value and registered area are so large. The alternative, throwing out the zero observations, would be tantamount to throwing the baby with the bath water. I do run the main specification for this subset of municipalities that have positive exports as well positive registrations, but this only provides us information about the effect of property rights at the *intensive* margin. Nevertheless, to assuage any fears of bias stemming from the arbitrary addition of a constant to the variables, I run my regression using an inverse hyperbolic sine (IHS) transformation instead; the IHS has the advantage of being well-defined at zero, but behaves like its log counterpart for large values and can be interpreted as such. Additionally, I also run a regression explicitly controlling for the presence of zeroes.

## 1.7 Results

Granting property rights to squatters in the Brazilian Amazon increases the average export value of wood-based products. Specifically, bringing *Terra Legal* to a municipality increases its quarterly export value between 48%-63% on average for each product. I also find that the effect of on-site rollout intensifies for the first few months and peaks at the third quarter post-rollout, before petering out. In addition, beyond the post-rollout effect, I find that an increase in total area registered by 10% decreases average quarterly wood export value by approximately 2% within a municipality-product pair. However, if that additional 10% increase is by "Medium" farms, then wood export value increases by almost 2%. These findings suggest that the "medium" farms which obtain their land cheaply are the real offenders who cut down trees.

In performing a similar analysis with agricultural commodities, I find that the post-rollout effect is mostly insignificant, but the effect of registered area varies by category, depending on whether the products are animal-based, plant-based, or prepared foodstuffs. The "Medium" Farm effect persists for agricultural commodities as well; a 10% increase in the area registered by "Medium" farms increases overall agricultural export value by roughly 2.5% per quarter for a municipal-product pair on average, and animal-based export value by a little bit over 4%. Additionally, area registered by "Large" Farms has a significant, positive effect on prepared foodstuffs, around 2%-3% for the same 10% increase. Given that prepared foodstuffs are more production-intensive, economies of scale definitely plays a role. There is no effect of rollout or registrations on plant-based exports, a reflection of the subsistence farming that is prevalent in the Amazon, particularly by small farms.

### 1.7.1 Wood-related Exports

I present the results of my main specification in Table 1.4 for wood-based products. 'Post Rollout' captures the effect of property rights intervention and is '1' following on-site registration. To rule out municipal-product specific shocks, such as woodchips factory workers striking in municipality $x$ only, I use a municipal-product fixed effect to control for unobserved heterogeneity. The quarter-year fixed effect controls for seasonal/time shocks. In addition, I use annual area cultivated within a municipality as a control. Robust standard errors clustered at the municipal level are in parentheses.

The sample consists of 412 Amazonian municipalities that export wood and/or

Table 1.4: Effect of *Terra Legal* on Wood Export Value

| | Log(Value) of Wood Exports | | | |
|---|---|---|---|---|
| VARIABLES | (1) | (2) | (3) | (4) |
| Post Rollout | 0.394* | 0.372* | 0.492* | 0.491** |
| | (0.210) | (0.212) | (0.266) | (0.250) |
| Log(Area Registered) | -0.188** | -0.186* | -0.201* | -0.170 |
| | (0.084) | (0.106) | (0.112) | (0.137) |
| Log(Area Registered by Medium Farms) | 0.148** | 0.145** | 0.175** | 0.163* |
| | (0.072) | (0.074) | (0.087) | (0.093) |
| Log(Area Registered by Large Farms) | 0.044 | 0.045 | 0.028 | 0.022 |
| | (0.057) | (0.058) | (0.079) | (0.080) |
| Observations | 20,581 | 15,221 | 14,504 | 10,704 |
| R-squared | 0.060 | 0.060 | 0.084 | 0.085 |
| Municipality Product FE | YES | YES | YES | YES |
| Quarter-Year FE | YES | YES | YES | YES |
| Eligible for Registrations | | YES | | YES |
| Consecutive Exports | | | YES | YES |

*** p <0.01, ** p <0.05, * p <0.1

Note: Robust standard errors in parentheses clustered at the municipal level. Sample of 412 municipalities in the Brazilian Amazon spans quarters Q1 2007 - Q4 2012; export products limited to "Wood and Wood-related" products (HS 44-49). I control for the total area cultivated, annual population estimates, as well as Municipality-Product trend and Quarter-Year trend in all specifications. 'Post-Rollout' is 1 following registration on-site during a particular quarter and remains 1 thereafter. Registered properties sized between 1-4 *mf* are "Medium" farms; "Large" farms have area greater than 4 *mf*. Column 1 includes the entire sample; Column (2) considers municipalities that are eligible for registrations only, while (3) limits the sample to municipalities that export wood products for two consecutive quarters. Column 4 restricts the sample to eligible municipalities that export wood products for any two consecutive quarters.

have registrations spanning the quarters Q1 2007 - Q4 2012.[18] This is reported in Column 1. Column 2 limits the sample to the municipalities that are eligible for registrations; the coefficients are slightly smaller for this subsample because it includes municipalities that eventually get rollout, but might never export. Column 3 considers municipalities that export a product for any two consecutive quarters, the so-called "consecutive exporters." The effect of registration and rollout on wood

[18] I exclude municipalities that neither export nor have registrations during the sample period from all regressions.

export value is larger for this subset of municipalities because consecutive exporters also are more likely to export higher volumes. Finally, the last column reports the effect at the intensive margin because it is the subset of consecutive exporters who eventually get the property rights intervention. The coefficients are slightly smaller in magnitude compared to Column (3), which includes all consecutive exporters during this time period, but larger than the first two columns.

Delving a bit more in detail, the post-rollout effect is strongly positive and significant across the board. Following on-site registration, average quarterly export value of wood-based products increases by 48% for each municipal-product pair in the overall sample (Column 1). Municipalities that export for two consecutive quarters, meanwhile, experience a 64% increase in quarterly export value on average (Columns 3,4). In addition to the rollout effect, a 10% increase in total area registered within a municipality decreases its average export value by 1.7% - 2.0%, though the effect is not significant at the intensive margin. If that increase in registered area is specifically captured by the "Medium" farms, wood export value *increases* by 1.45% - 1.75% per quarter on average, depending on the subset of municipalities. Surprisingly, "Large" farms have no effect on export value beyond the effect of rollout, which suggests that more than economies-of-scale is at play.

Recall that the titling programme, *Terra Legal*, determines the cost of obtaining the land title by the size of the registered property. Small farms, whose registered area is less than 1 *modulo fiscale*, obtain the property title at no cost; "medium" properties, whose area is between 1 and 4 *modulo fiscale*, obtain the title cheaply, whereas the large properties, with area greater than 4 *modulo fiscale*, pay the market price for the land. These results imply that the properties that pay less for the land than what is worth[19] are the ones who intensify deforestation and export the wood product. This is counterintuitive because we might expect large property holders to recoup the high cost of acquiring the title by stepping up the production, but that is clearly not the case. The land owners with "medium" properties are sized "just right;" they are large enough to take advantage of economies of scale and increase production, but small enough to still fly under the radar of law enforcement.

Property rights establishment has a large effect on imports, but it is unlikely that the effect remains that high for all quarters following rollout. The panel data allows me to explore the heterogenous effect of rollout, the result of which is in Table 1.5. I breakdown the 'Post rollout' variable from Table 1.4 by quarter. 'Rollout' is 1 for municipality $i$ only for quarter $t$, when officials register on-site; it is 0 otherwise. Similarly, each of the 'Post' variables capture how much time has lapsed following rollout. '1st Quarter Out' is 1 for municipality $i$ at quarter $t+1$ and 0 otherwise, '2nd Quarter Out' at $t+2$ and so forth. I include 12 such post-rollout variables for completeness,[20] though only the first 8 quarters are reported here.

Table 1.5 confirms that the effect of rollout indeed varies over time. As before, the magnitude of the effect is smallest for the subset of municipalities that are eligible for registration (Column 2) and highest for consecutive exporters (Column 3). Immediately following rollout, the average export value of wood-based products increases by 59%-79% within a municipality, with a predicted 69% increase for the overall sample. The wood harvesting that begins with the advent of property rights, intensifies over the next three quarters. One quarter following rollout, average wood export value

[19] The policy specifically states that the land owners pay "less than the market value" for properties in this category, though it does not mention how that is determined.

[20] Rollout begins in the third quarter of 2009, and my sample ends in the the fourth quarter of 2012.

Table 1.5: Effect of *Terra Legal* Rollout on Wood Export Value Over Time

| | Log(Value) of Wood Exports | | | |
|---|---|---|---|---|
| VARIABLES | (1) | (2) | (3) | (4) |
| Rollout/Treatment | 0.532*** | 0.465*** | 0.582*** | 0.490** |
| | (0.160) | (0.158) | (0.193) | (0.193) |
| Post Rollout By Quarter | | | | |
| .....1st Quarter Out | 0.567** | 0.441* | 0.832*** | 0.697*** |
| | (0.236) | (0.248) | (0.284) | (0.288) |
| .....2nd Quarter Out | 0.639*** | 0.511* | 0.867*** | 0.732** |
| | (0.248) | (0.279) | (0.297) | (0.325) |
| .....3rd Quarter Out | 0.655*** | 0.515* | 0.866*** | 0.717** |
| | (0.230) | (0.295) | (0.291) | (0.365) |
| .....4th Quarter Out (1 Year) | 0.392 | 0.200 | 0.489 | 0.279 |
| | (0.247) | (0.346) | (0.309) | (0.436) |
| .....5th Quarter Out | 0.280 | 0.041 | 0.332 | 0.050 |
| | (0.267) | (0.352) | (0.334) | (0.467) |
| .....6th Quarter Out | 0.264 | -0.0034 | 0.358 | 0.031 |
| | (0.323) | (0.432) | (0.428) | (0.583) |
| .....7th Quarter Out | 0.161 | -0.209 | 0.201 | -0.258 |
| | (0.361) | (0.476) | (0.480) | (0.648) |
| .....8th Quarter Out (2 Years) | 0.077 | -0.418 | 0.008 | -0.607 |
| | (0.357) | (0.508) | (0.470) | (0.689) |
| Registrations | | | | |
| .....Log(Area Registered) | -0.170** | -0.187** | -0.181* | -0.178 |
| | (0.078) | (0.0956) | (0.101) | (0.124) |
| .....Log(Area Registered by Medium Farms) | 0.131** | 0.129* | 0.151* | 0.141* |
| | (0.067) | (0.068) | (0.081) | (0.0813) |
| ......Log(Area Registered by Large Farms) | 0.047 | 0.054 | 0.035 | 0.041 |
| | (0.055) | (0.056) | (0.075) | (0.077) |
| Observations | 20,581 | 15,221 | 14,504 | 10,704 |
| R-squared | 0.062 | 0.064 | 0.087 | 0.091 |
| Municipality Product FE | YES | YES | YES | YES |
| Quarter-Year FE | YES | YES | YES | YES |
| Eligible for Registrations | | YES | | YES |
| Consecutive Exports | | | YES | YES |

*** p <0.01, ** p <0.05, * p <0.1

Note: Robust standard errors in parentheses clustered at the municipal level. Sample of 807 municipalities in the Brazilian Amazon spans quaters Q1 2007 - Q4 2012; export products limited to "Wood and Wood-related" products (HS 44-49). I control for the total area cultivated, annual municipal population estimates, as well as Municipality-Product trend and Quarter-Year trend in all specifications. Rollout is 1 for the particular quarter when *Terra Legal* registers on site and zero otherwise; each of the "Post" variables is 1 for only one quarter post-rollout and 0 otherwise; for example, "5th Quarter Out" is 1 for the fifth quarter after rollout and 0 otherwise. Regressions are run with all 12 quarters post rollout, but only the first 8 are presented in this table. Registered properties sized between 1-4 *mf* are "Medium" farms; "Large" farms have area greater than 4 *mf*. Column 1 includes the entire sample; Column (2) considers municipalities that are eligible for registrations only, while (3) limits the sample to municipalities that export wood products for two consecutive quarters. Column 4 restricts the sample to eligible municipalities that export wood products for two consecutive quarters.

increases by 76% within a municipality for the overall sample; municipalities that export for any two consecutive quarters increase export value by a whopping 100-129% depending on whether they are eligible for exports. Six months after on-site registrations, average municipal export value is 89% higher (Column 1) and is as high as 137%.

The effect of rollout on export value peaks in the third quarter, with export value increasing by 93% for the whole sample; however, the effect is slightly smaller in the third quarter for consecutive exporters (Columns 3,4). One year post rollout, the treatment effect falls sharply and is no longer significant, suggesting that all adjustments take place within three quarters post-rollout. In fact, the export value continues to fall over time, and eventually turns negative 7 quarters after rollout (Column 2,4), though this could also be attributed to the lack of municipalities that have been registered for that long.

The "Area registered" coefficients are minutely smaller in magnitude when compared to Tabletab:xv1. Total area registered continues to be negative, but this now makes sense. Once the immediate adjustment following property rights intervention subsides, increasing total area registered by 10% decreases export value by 1.7%. However, the "Medium-farm" effect continues to be positive, which suggests that either these farms have started to engage in forest management, cutting down a small portion of their tree each quarter and replanting, or they are the main offenders who are intent on fully converting the forest land for alternate uses.

Nevertheless, these findings follow the predictions of my theoretical model. If the marginal agricultural revenue is high enough, the new land owner will engage in rapid clearing, which will increase wood exports during the transition. However, they will stop conversion and reach steady state sooner. While I cannot make any empirical claims regarding the latter, the results show that in the immediate aftermath of rollout, deforestation increases. Whether or not that is due to an intention in engaging in agricultural endeavors cannot be determined by looking at land conversion alone.

### 1.7.2 Agricultural Exports

I now turn my attention to exports of agricultural commodities to determine whether land conversion occurs due to agricultural incentives. If agents are indeed clearing the forests to grow more crops, and suppose, for whatever reason, exporting out of the Amazon is relatively painless, then we might also expect an increase in the export of food-related products.

Table 1.6 presents the results of the impact of property rights establishment on the quarterly export value of all Agricultural commodities (Columns 1,2) and then pooled based on the type of product: Animal-based (Columns 3,4), Plant-based (Columns 5,6) and prepared foodstuffs (Columns 7,8). As in the case of wood exports, the sample consists of Amazonian municipalities where registrations occurred or which export agricultural products. I also continue to include quarter-year fixed effects and municipal-product fixed effects in all specifications to control for unobserved heterogeneity and the time trend. In addition, I control for total area cultivated, the results of which are significant across the board. Finally, I cluster the errors at the municipal-level because I expect the errors to be correlated within a municipality.

Overall, I find that there is no post-rollout effect on the export value when considering all agricultural commodities (Columns 1, 2).[21] This finding is reasonable, given that adjusting agricultural commodity production is more "sticky" than for

[21] I do not find any significant effect if I parse the post-rollout effect over time.

Table 1.6: Effect of Registrations and Rollout on Export Value - Agricultural Commodities

| | Log(Value) of Exports) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Full Sample | | Animal-based | | Plant-based | | Foodstuffs | |
| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post Rollout | 0.848 | 0.838 | 0.629 | 0.260 | 0.347 | 0.822 | 1.50** | 1.13 |
| | (0.600) | (0.798) | (0.609) | (0.722) | (0.478) | (0.874) | (0.687) | (0.995) |
| Log (Area Registered) | -0.151 | -0.362 | -0.137 | -0.388** | -0.148 | -0.159 | -0.171 | -0.420 |
| | (0.155) | (0.231) | (0.217) | (0.198) | (0.112) | (0.205) | (0.179) | (0.344) |
| Log(Area Registered by Medium Farms) | 0.072 | 0.247** | 0.184 | 0.427*** | 0.0089 | 0.019 | 0.009 | 0.166 |
| | (0.091) | (0.111) | (0.168) | (0.105) | (0.079) | (0.128) | (0.129) | (0.193) |
| Log(Area Registered by Large Farms) | 0.096 | 0.157 | 0.001 | 0.166 | 0.139 | 0.0276 | 0.217* | 0.335* |
| | (0.082) | (0.1136 | (0.132) | (0.135) | (0.052) | (0.140) | (0.113) | (0.179) |
| Log (Area Cultivated) | -1.26** | -3.36*** | -1.25** | -2.11** | 0.384 | 2.55* | -2.19*** | -5.55*** |
| | (0.55) | (1.19) | (0.54) | (1.03) | (0.52) | (1.48) | (0.79) | (1.17) |
| Observations | 35,784 | 9,589 | 10,184 | 3,429 | 15,028 | 3,376 | 10,572 | 2,784 |
| R-squared | 0.061 | 0.081 | 0.088 | 0.084 | 0.034 | 0.087 | 0.134 | 0.170 |
| Municipality Product FE | YES | YES | YES | YES | YES | YES | YES | YES |
| Quarter Year FE | YES | YES | YES | YES | YES | YES | YES | YES |
| Eligible for Registration | | YES | | YES | | YES | | YES |
| Consecutive Exports | | YES | | YES | | YES | | YES |

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors in parentheses clustered at the municipal level. Sample of 574 municipalities in the Brazilian Amazon spans Q1 2007 - Q4 2012; export products limited to agricultural commodities only (HS Chapter 1-24). Given the presence of zeroes, variables are log-transformed with the addition of 1, i.e. log(1+x). "Post Rollout" becomes 1 following registration on-site and remains so thereafter. "Medium" farms are sized 1-4 *mf* each; "Large" farms are greater than *4mf*. I control for annual population, Municipal-Product trend and Quarter-Year trend in all regressions. Columns (1-2) include the entire sample of agricultural commodities, while the rest of the table breaks them down into subsamples. Columns (3,4) look at Animal-based products only (HS Chapters 1-5), while columns (5,6) concentrate on plant-based exports(6-15); finally, Columns (7,8) are food products that are prepared (HS Chapters 16-24). Finally, the second regression in each category (Columns 2,4,6,8) only considers eligible municipalities that export for any two consecutive quarters.

wood. Immediately after getting my land rights, I can go purchase a saw or bulldozer and start cutting down the trees, but it is difficult to immediately conjure crops; the land needs to be prepared, seeds acquired and planted and then, I can harvest the product a few weeks, months or years later. However, the "Medium-farm" effect continues to be positive for consecutive exporters that are programme-eligible: a 10% increase in area registered by medium farms increases average quarterly export value of agricultural commodities by 2.5% within a municipality.

Column 4 shows that animal-based exports drive this effect. For the same subset of municipalities (consecutive exporters, rollout-eligible), the average export value of Animal-based products increases by 4.3% if medium-farm area increases by 10% within a municipality, but decreases by 3.9% if total area registered goes up by the same. Animal-based exports, particularly cattle ranching, are lucrative, but require a lot of land for grazing. These results suggest that the medium farms most likely convert their forest land for this purpose.

Plant-based crop commodities do not respond to rollout or registrations, after controlling for total area cultivated; this is not surprising, given that many of the small farms that grow crops engage in subsistence farming, which will not translate into exporting. Indeed, in my other paper, co-authored with Molly Lipscomb, we find that temporary crop cultivation increases in response to registrations, rather than permanent crop cultivation.

The only exception where post-rollout effect is statistically significant is for the so-called 'Prepared foodstuffs': a staggering 348% on average. However, the effect vanishes when restricting the sample to eligible-for-rollout municipalities that export for consecutive quarters, suggesting that the result might be a one-off.[22]In addition,

[22]The effect is most likely inflated because I find that the parallel-trend assumption is violated;

increasing the area registered by "Large" Farms has a significant, positive effect on prepared foodstuffs, around 2%-3% for a 10% increase (Columns 7,8). This suggests that the large properties, which do not engage in land clearing to the same extent as medium farms, concentrate on more value-added production following rollout.

This foray into agricultural exports shows that the predictions of my theoretical model hold, at least in the short run. While rollout itself does not affect agricultural exports, I find that the same subset of registered properties which engage in rapid land clearing, do so to increase their agricultural revenue (in this case, livestock). The medium-sized farms, which obtain their land rights cheaply, are sized "just right," small enough to engage in the illegal land clearing[23] and go unnoticed, but are large enough to engage in the world market and reap the benefits.

## 1.8 Robustness Checks

I check the validity and accuracy of my regression coefficients in a few ways. First, I check whether the results are consistent for export net weight, rather than export value as the outcome variable; if the underlying price of the exports also change, in addition to quantity, the effect of registration could be overstated.[24] I find that export net weight also increases following property rights, though by slightly less than the increase in export value, confirming that the results are not merely driven by a

so, the treatment effect cannot be attributed to the treatment alone. See the next section for more details.

[23]The provisions of *Terra Legal* require property owners to maintain a 80% legal reserve on their land

[24]I originally use 'Export Value' rather than 'Net Weight' because many of these values were missing; i.e some municipalities only reported the value. This could bias my results, particularly if the such municipalities have some observed characteristic in common that is correlated with the outcome variable.

price effect. I show this with the main log-log regression, but I also run a level-level regression with wood export yield.

Second, I check whether the parallel-trend assumption in export value pre-treatment holds for municipalities with high and low registrations. I find that it does for a subsample of Wood exports ('Articles of Wood' Only) and a subsample of Agricultural Exports (Animal-based for fewer quarters pre-rollout). In running the regressions for these subsamples, I find that the effect of the rollout is greater than previously stated.

Third, I consider whether I introduce some bias with the use of a log-log regression. Specifically, to deal with the presence of zeroes, I transform the variables by adding the constant '1' before taking the logs. Instead of the log transformation, I use the inverse hyperbolic sine (IHS) transformation, which is well-defined at '0' and can be interpreted like the log-transformation. I find that the results do not change substantially, and if anything, the log-log original specification slightly underreports the impact of the property rights intervention. I also control for the zero more explicitly with the use of an indicator variable; with this approach, I find that the effect of registrations, where registrations are positive, is higher in absolute value. Total area registered has a more negative effect on average export value, and the "medium" farm effect is larger and positive.

Finally, given that *Terra Legal* begins rollout on the heels of the Great Recession, I find that exports decline steeply in the middle of my pre-treatment sample. To check whether this biases my estimates downward, I do a post-crisis analysis. Indeed, the coefficients are larger, but the main limitation here is that the pre-rollout observations are limited to two quarters only: the first and second quarter of 2009.

### 1.8.1 Export Net Weight

Since both price and quantity are buried in export value, I try to isolate the effect of registration by looking at quantity alone. Though the Brazilian Ministry provides two measures of export quantity—'Net weight' and 'quantity'—the latter is undefined and missing for many observations. Thus, I limit my analysis to 'Net Weight' only, which is report in kilograms.

Table 1.7 shows the effect of rollout overtime with the net weight outcome variable. Across the board, the magnitude of the coefficients decrease slightly from those reported in Table 1.5. For example, the average treatment effect is approximately a 63% increase in export net weight for the whole sample (Column 1); this is 7% less than the impact on export value (Table 1.5, column 1). Similarly, three quarters post rollout, average export net weight increases by 97% in a municipality that is eligible for rollout and exports a product for two consecutive quarters (Column 4), which is approximately 8% less than impact on export value.

The effect of the rollout also peters out more rapidly overtime, turning negative in the 5th quarter post rollout for municipalities that export for two consecutive quarters (Column 2) and are additionally also eligible for rollout (Column 4). Previously, this happened only in the 6th quarter for the former and the 7th quarter for the latter (Table 1.5, Columns (2,4)). Indeed, two years post rollout, the effect is negative (and insignificant) across all subsamples: those that are eligible for the programme, those that export for two consecutive quarters, and those that are both (Columns 2-4).

The registration coefficients are smaller as well, but to a very minute degree. A 10% increase in registered area decreases average export net weight by 1.57% and export value by 1.7% within a municipal-product pair. For *Terra Legal*-eligible

Table 1.7: Effect of *Terra Legal* Rollout on Wood Export Net Weight Over Time

| | Log(Net Weight) of Wood Exports | | | |
|---|---|---|---|---|
| VARIABLES | (1) | (2) | (3) | (4) |
| Rollout/Treatment | 0.486*** | 0.419*** | 0.549*** | 0.460** |
| | (0.154) | (0.155) | (0.190) | (0.191) |
| Post Rollout By Quarter | | | | |
| .....1st Quarter Out | 0.543*** | 0.414* | 0.808*** | 0.672** |
| | (0.209) | (0.229) | (0.252) | (0.263) |
| .....2nd Quarter Out | 0.601*** | 0.467* | 0.855*** | 0.719** |
| | (0.215) | (0.258) | (0.259) | (0.293) |
| .....3rd Quarter Out | 0.615*** | 0.474 | 0.821*** | 0.678* |
| | (0.214) | (0.295) | (0.276) | (0.360) |
| .....4th Quarter Out (1 Year) | 0.363 | 0.168 | 0.467 | 0.260 |
| | (0.242) | (0.357) | (0.299) | (0.435) |
| .....5th Quarter Out | 0.183 | -0.063 | 0.241 | -0.045 |
| | (0.263) | (0.378) | (0.333) | (0.483) |
| .....6th Quarter Out | 0.199 | -0.079 | 0.311 | -0.025 |
| | (0.326) | (0.461) | (0.417) | (0.595) |
| .....7th Quarter Out | 0.106 | -0.276 | 0.163 | -0.305 |
| | (0.361) | (0.504) | (0.466) | (0.658) |
| .....8th Quarter Out (2 Years) | 0.030 | -0.475 | -0.010 | -0.630 |
| | (0.363) | (0.541) | (0.462) | (0.703) |
| Registrations | | | | |
| .....Log(Area Registered) | -0.157** | -0.175** | -0.167* | -0.162 |
| | (0.074) | (0.089) | (0.095) | (0.115) |
| .....Log(Area Registered by Medium Farms) | 0.119* | 0.117* | 0.137* | 0.126* |
| | (0.066) | (0.066) | (0.079) | (0.077) |
| .....Log(Area Registered by Large Farms) | 0.046 | 0.053 | 0.031 | 0.037 |
| | (0.052) | (0.053) | (0.072) | (0.073) |
| Observations | 18,543 | 13,183 | 12,474 | 9,674 |
| R-squared | 0.069 | 0.071 | 0.096 | 0.1 |
| Municipality Product FE | YES | YES | YES | YES |
| Quarter-Year FE | YES | YES | YES | YES |
| Eligible for Registrations | | YES | | YES |
| Consecutive Exports | | | YES | YES |

*** p <0.01, ** p <0.05, * p <0.1

Note: Robust standard errors in parentheses clustered at the municipal level. Sample of 807 municipalities in the Brazilian Amazon spans quarters Q1 2007 - Q4 2012; export products limited to "Wood and Wood-related" products (HS 44-49). I control for the total area cultivated, annual population as well as Municipality-Product trend and Quarter-Year trend in all specifications. Rollout is 1 for the particular quarter when registers on site and zero otherwise; each of the "Post" variables is 1 for only one quarter and 0 otherwise; for example, "5th Quarter Out" is 1 for the fifth quarter after rollout and 0 otherwise. Regressions are run with all 12 quarters post rollout, but only the first 8 are presented in this table. Registered properties sized between 1-4 *mf* are denoted "Medium" farms; "Large" farms have area greater than 4 *mf*. Column 1 includes the entire sample; Column (2) considers municipalities that are eligible for registrations only, while (3) limits the sample to municipalities that export wood products for two consecutive quarters. Column 4 restricts the sample to eligible municipalities that export wood products for two consecutive quarters.

Table 1.8: Effect of Registrations and Rollout on Export Net Weight - Agricultural Commodities

| | Log(Net Weight3) of Exports) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Full Sample | | Animal-based | | Plant-based | | Foodstuffs | |
| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Post Rollout | 0.751 | 0.890 | 0.598 | 0.353 | 0.307 | 0.957 | 1.29** | 0.94 |
| | (0.544) | (0.739) | (0.546) | (0.640) | (0.459) | (0.864) | (0.617) | (0.909) |
| Log (Area Registered) | -0.139 | -0.335 | -0.118 | -0.355** | -0.154 | -0.159 | -0.141 | -0.367 |
| | (0.141) | (0.219) | (0.200) | (0.180) | (0.111) | (0.214) | (0.162) | (0.321) |
| Log(Area Registered by Medium Farms) | 0.071 | 0.223** | 0.159 | 0.369*** | 0.0076 | 0.027 | 0.011 | 0.144 |
| | (0.085) | (0.107) | (0.155) | (0.093) | (0.079) | (0.131) | (0.113) | (0.179) |
| Log(Area Registered by Large Farms) | 0.085 | 0.146 | 0.0003 | 0.164 | 0.081 | 0.020 | 0.180* | 0.321* |
| | (0.075) | (0.112) | (0.123) | (0.121) | (0.074) | (0.152) | (0.107) | (0.173) |
| Log (Area Cultivated) | -1.05** | -3.19*** | -1.14** | -1.87** | 0.394 | 2.68* | -1.87*** | -5.08*** |
| | (0.481) | (1.130) | (0.493) | (0.913) | (0.509) | (1.54) | (0.690) | (1.09) |
| Observations | 34,775 | 8,582 | 9,184 | 3,329 | 15,021 | 3,070 | 10,170 | 2,780 |
| R-squared | 0.051 | 0.083 | 0.084 | 0.089 | 0.030 | 0.093 | 0.111 | 0.165 |
| Municipality Product FE | YES | YES | YES | YES | YES | YES | YES | YES |
| Quarter Year FE | YES | YES | YES | YES | YES | YES | YES | YES |
| Eligible for Registration | | YES | | YES | | YES | | YES |
| Consecutive Exports | | YES | | YES | | YES | | YES |

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors in parentheses clustered at the municipal level. Sample of 807 municipalities in the Brazilian Amazon spans Q1 2007 - Q4 2012; export products limited to agricultural commodities only (HS Chapter 1-24). Given the presence of zeroes, variables are log-transformed with the addition of 1, i.e. log(1+x). “Post Rollout” becomes 1 following registration on-site and remains so thereafter. “Medium” farms are sized 1-4 *mf* each; “Large” farms are greater than *4mf*. I control for annual population, as well as Municipal-Product trend and Quarter-Year trend in all regressions. Columns (1-2) include the entire sample of agricultural commodities, while the rest of the table breaks them down into subsamples. Columns (3,4) look at Animal-based products only (HS Chapters 1-5), while columns (5,6) concentrate on plant-based exports(6-15); finally, Columns (7,8) are food products that are prepared (HS Chapters 16-24). Finally, the second regression in each category (Columns 2,4,6,8) only considers eligible municipalities that export for any two consecutive quarters.

municipalities that export for two consecutive quarters, increasing the area registered by "Medium" farms by 10% increases wood export net weight by 1.26% and wood export value by 1.4%.

Thus, the underlying factor driving the increase in export value following property rights intervention is the increase in physical units of wood being exported. This necessarily implies that agents are cutting down trees following *Terra Legal*.

Table 1.8 shows the results of a similar exercise for agricultural commodities. While this distinction is not as important as in the case of wood-based products, it is nevertheless worth mentioning for completeness. Compared to the effect of rollout and registrations on export value, the effect on export net weight is once again slightly smaller in magnitude across the board. There are no other changes. As in its Export Value counterpart (Table 1.6), the rollout of *Terra Legal* does not have an strong, consistent effect overall on agricultural exports. The "Medium farm" effect continues to be positive, though significant only for Animal-based exports. Increasing the area registered by large farms by 10% also increases export net weight of prepared foodstuffs, by 1.8 % - 3.2 %.

Therefore, the use of export value, rather than export net weight, appears to slightly amplify the effect of rollout and registrations. However, given that some observations are missing for export net weight, these estimates could be biased if they are not missing at random.

### 1.8.2 Pre-Trends

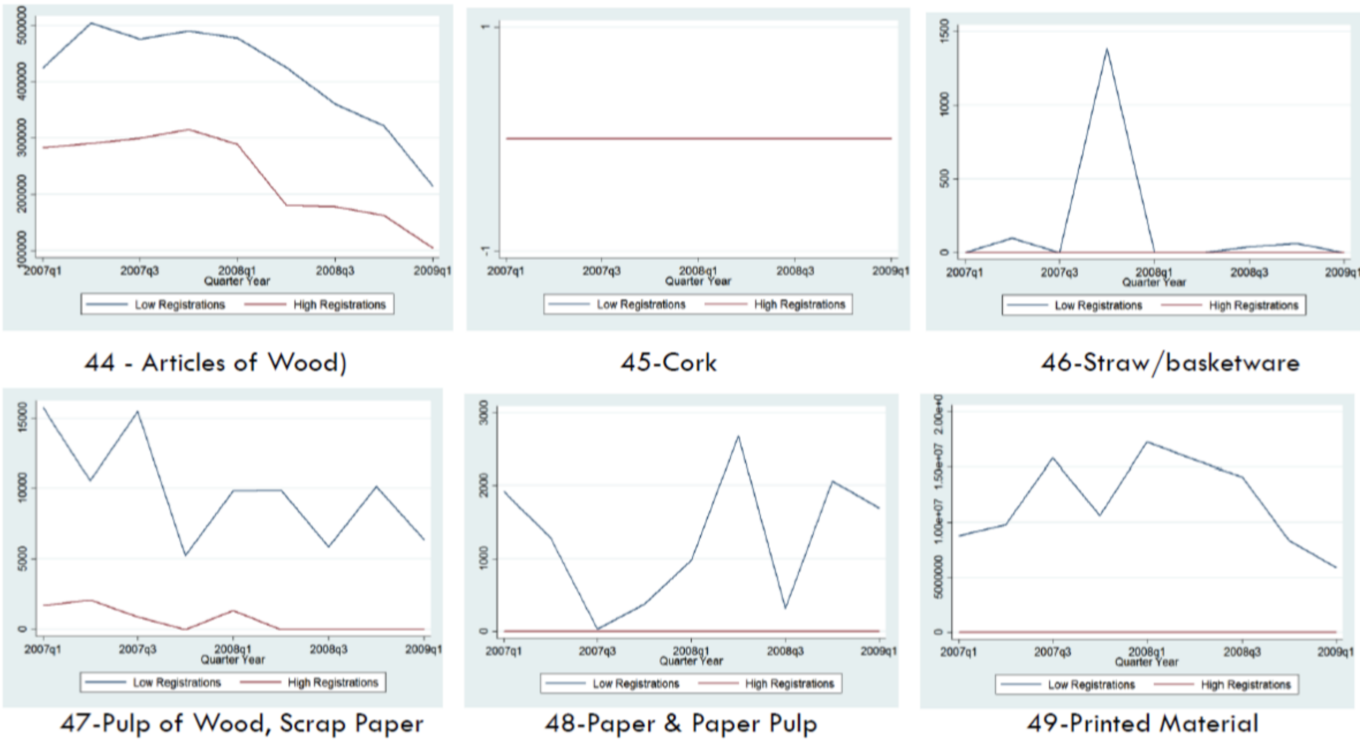
Since I use a difference-in-differences estimation strategy, my coefficients are valid only if both the treated (municipalities with high registrations) and control (municipalities with low registrations) groups follow the same trend in the outcome variable. To test this assumption, I plot the municipal export data from January 2007 until March 2009, nine quarters before the implementation of the programme. I divide the municipalities into high versus low registrations based on the median share of registered area; those with above-median share of registered area are the treated group and those with below-median share of registered area are the control group. I first perform the exercise of wood exports, followed by agricultural exports.

I separate the various wood products according to their classification chapter headings: articles of wood, cork, straw/basketware, pulp of wood/scrap paper, paper/paper pulp, and printed materials. Notice that though HS 44-49 are classified as wood products, there are considerable differences between the amount of wood contained in the products across the categories.

As Figure 1.6 shows, the parallel trend assumption holds only for Chapters 44 and 45, Articles of Wood and Cork respectively. In fact, for Chapter 45 the trend is exactly the same for both municipalities with high and low registrations, with the average export value being zero in both categories. For the rest, there is barely any correlation in the trends. The violation of this assumption could inflate the treatment effect if the municipalities with high registrations are also the big exporters. To address this concern, I limit the sample to HS 44 products only.[25]

Table 1.9 shows the effect of rollout and registrations on quarterly export value of Chapter 44 'Articles of Wood' products. In comparing these numbers to those presented in Table 1.5, one first notices that the number of observations do not drop

[25] I exclude Chapter 45 because there are very few exports of such products from the Amazonian municipalities; only 3 municipalities have any positive value of cork exports, and that too, only during one quarter each. Thus of 78 cork-related observations, only 3 are different from zero, and it is clear that these are outliers.




Note: Municipalities with high registrations are classified as those with above-median share of registered area. Export data plotted at the quarterly level from the first quarter of 2007 to the first quarter of 2009.

Figure 1.6: Pre-Trends in Wood Exports (HS 44-49) in Municipalities with High v. Low Registrations

significantly when limiting the sample to 'Articles of Wood' products only: 17,048 vs. 20,543. This suggests that most of the wood exports from the Brazilian Amazon are'Articles of wood' anyway.

In addition, the magnitude of the effect of the "Rollout" and "Post" variables increase across the board. "Rollout" becomes significant at the 1% level in Column (4), which limits the sample to municipalities that export a product for any two consecutive quarters in addition to being eligible for registrations; in Table 1.9, it is significant at the 5% level. However, 'Area registered' coefficients fall slightly in absolute value,

Table 1.9: Effect of Registration and Rollout on Export Value - Articles of Wood Only

| | Log(Value) of Wood Exports | | | |
|---|---|---|---|---|
| VARIABLES | (1) | (2) | (3) | (4) |
| Rollout/Treatment | 0.563*** | 0.518*** | 0.585*** | 0.529*** |
| Post Rollout By Quarter | | | | |
| | (0.176) | (0.173) | (0.211) | (0.206) |
| .....1st Quarter Out | 0.579*** | 0.515** | 0.804*** | 0.753*** |
| | (0.218) | (0.218) | (0.266) | (0.264) |
| .....2nd Quarter Out | 0.614*** | 0.587** | 0.820*** | 0.821*** |
| | (0.225) | (0.243) | (0.271) | (0.294) |
| .....3rd Quarter Out | 0.596*** | 0.583** | 0.797*** | 0.822** |
| | (0.219) | (0.270) | (0.286) | (0.348) |
| .....4th Quarter Out (1 Year) | 0.470** | 0.457 | 0.565** | 0.585 |
| | (0.234) | (0.309) | (0.291) | (0.388) |
| Registrations | | | | |
| .....Log(Area Registered) | -0.140** | -0.126 | -0.141 | -0.092 |
| | (0.072) | (0.092) | (0.092) | (0.114) |
| .....Log(Area Registered by Medium Farms) | 0.113** | 0.106* | 0.115* | 0.097 |
| | (0.0581) | (0.059) | (0.066) | (0.067) |
| .....Log(Area Registered by Large Farms) | 0.013 | 0.012 | -0.003 | -0.011 |
| | (0.054) | (0.055) | (0.074) | (0.077) |
| Observations | 17,480 | 12,624 | 13,232 | 9,504 |
| R-squared | 0.078 | 0.082 | 0.100 | 0.107 |
| Municipality Product FE | YES | YES | YES | YES |
| Quarter-Year FE | YES | YES | YES | YES |
| Eligible for Registrations | | YES | | YES |
| Consecutive Exports | | | YES | YES |

*** p <0.01, ** p <0.05, * p <0.1

Note: Robust standard errors in parentheses clustered at the municipal level. Sample of 807 municipalities in the Brazilian Amazon spans quarters Q1 2007 - Q4 2012; export products now limited to "Articles of Wood" products only (HS 44). I control for the total area cultivated, annual population as well as Municipality-Product trend and Quarter-Year trend in all specifications. Rollout is 1 for the particular quarter when *Terra Legal* registers on site and zero otherwise; each of the "Post" variables is 1 for only one quarter post-rollout and 0 otherwise; for example, "5th Quarter Out" is 1 for the fifth quarter after rollout and 0 otherwise. Regressions are run with all 12 quarters post rollout, but only the first 8 are presented in this table. Registered properties sized between 1-4 *mf* are"Medium" farms; "Large" farms have area greater than 4 *mf*. Column 1 includes the entire sample Column (2) considers municipalities that are eligible for registrations only, while (3) limits the sample to municipalities that export wood products for two consecutive quarters. Column 4 restricts the sample to eligible municipalities that export wood products for two consecutive quarters.

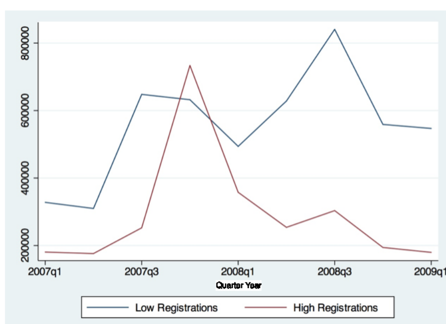
though the difference is small; more importantly, they also lose significance. The coefficients on total area registered are no longer significant for columns (2-4), which report the effect for municipalities that are eligible for registrations, that export a product for any two consecutive quarters, and that do both. The "medium" farm effect also disappears for the subset of municipalities that are consecutive exporters and eligible for registrations. This could also be due to the drop in the number of observations; combined with the strong fixed effects that I use, there might be very little variation.

The results that include all wood-related products understate the true effect of the programme rollout to a small degree, while slightly overstating the importance of the registered area variable. The main takeaway from this exercise is that the mere granting of property rights through programme rollout has a significant, persistent impact of wood exports, and deforestation by extension. The amount of area registered matters to a lesser degree, though "medium" farms appear to be the worst offenders.

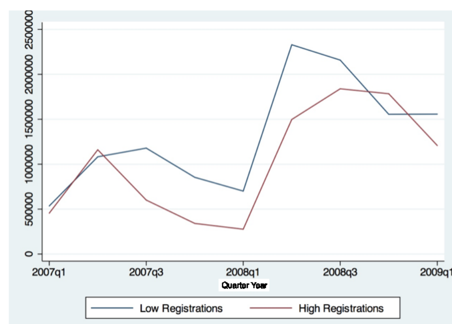
I perform the same exercise with agricultural commodities. I plot the trend in export value in municipalities with high vs low registrations between 2007 and 2009, pre-rollout; once again, we are looking for a parallel tread pre-rollout, so that the changes that occur after the property rights intervention can be attributed *to* the intervention.

Figure 1.7 shows the trend in export value for the three broad categories of agricultural commodities: Animal-based, Plant-based and Prepared foodstuffs. Upon first look, it appears as if there no parallel trend before the policy rights in any of the three categories. However, it is important to keep in mind that these are grouping many types of exports together, even more than HS2 Chapters, as was the case for wood.

The pre-trend in the export-value of Prepared Foodstuffs in municipalities with high versus low registrations looked promising with a similar trend between the "treated" and "control" municipalities during the first six or seven quarters, but the trend diverged the three quarters that immediately preceded the start of the programme. So there could be some bias in the estimates for these group of exports, i.e. something else could be driving the effect that gets lumped into the effect of rollout, which was significant and positive (Column 7, Table 1.6). Vegetable Products do not




Animal (HS 1 - 5)




Vegetable (HS 6 - 15)


Foodstuffs (HS 16 - 24)

Note: Municipalities with high registrations are classified as those with above-median share of registered area. Export data plotted at the quarterly level from the first quarter of 2007 to the first quarter of 2009.

Figure 1.7: Pre-Trends in Agricultural Exports (HS 1-24) in Municipalities with High v. Low Registrations by Category

follow a similar trend and did not have significant estimates, so those results can be disregarded with impunity.

Finally, only the Animal-based exports display a parallel trend in municipalities with high versus low registrations, but only for the last four quarters before *Terra Legal* started its property rights rollout (from the second quarter in 2008 to the second quarter in 2009). This suggests the regressions that are limited to the Animal-based exports are most valid. Given that the pre-trend holds only for the last four quarters before rollout, I present the results on the subsample in Table 1.10. I also include the results presented in Columns 3-4 of Table 1.6 for ease of comparison.

In comparing the results with the limited pre-treatment sample (Columns 3-6) with those that include all the pre-treatment quarters Table 1.6 (Columns 1-2), the post-rollout effect continues to be insignificant. The only exception is for the subsample of municipalities eligible for registration; post-rollout, quarterly Animal-based export value increases by approximately 200% within these municipalities. The registration variables are strongly significant for rollout-eligible municipalities that also export for two consecutive quarters for the sample with all pre-rollout quarters (Column 2) and for those with just the last 4 quarters (Column 6). A 10% increase in registered area decreases the average export value of Animal-based products by approximately 3.7% for such municipalities, but if that increase was specifically granted to the "medium" farms, average export value increases by 2.8%. The regression that includes the quarters where the export-trend are not parallel overestimate the effects marginally.

Table 1.10: Effect of Registrations on Animal-based Export Value

| | Log(Value) of Exports) | | | | | |
|---|---|---|---|---|---|---|
| | From Table 1.6 | | From Q2 2008 - Q4 2012 | | | |
| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) |
| Post Rollout | 0.656 | 0.260 | 0.772 | 1.11* | 0.178 | 0.249 |
| | (0.5) | (0.722) | (0.692) | (0.666) | (0.707) | (0.695) |
| Log (Area Registered) | -0.135 | -0.388** | -0.161 | -0.130 | -0.311* | -0.371* |
| | (0.216) | (0.198) | (0.174) | (0.178) | (0.182) | (0.199) |
| Log(Area Registered by Medium Farms) | 0.181 | 0.427*** | 0.109 | 0.092 | 0.299** | 0.284* |
| | (0.168) | (0.105) | (0.138) | (0.137) | (0.151) | (0.149) |
| Log(Area Registered by Large Farms) | -0.0025 | 0.166 | 0.112 | 0.109 | 0.204 | 0.226 |
| | (0.132) | (0.135) | (0.105) | (0.107) | (0.151) | (0.154) |
| Log (Area Cultivated) | -1.28** | -2.11** | -1.58** | -1.87*** | -2.53*** | -3.513*** |
| | (0.541) | (1.03) | (0.612) | (0.737) | (0.916) | (1.20) |
| Observations | 10,224 | 3,430 | 7,628 | 5,450 | 4,131 | 2,909 |
| R-squared | 0.088 | 0.084 | 0.081 | 0.092 | 0.044 | 0.065 |
| Municipality Product FE | YES | YES | YES | YES | YES | YES |
| Quarter Year FE | YES | YES | YES | YES | YES | YES |
| Eligible for Registration | | YES | | YES | | YES |
| Consecutive Exports | | YES | | | YES | YES |

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors in parentheses clustered at the municipal level. Export products limited to Animal-based commodities only (HS Chapter 1-5). Sample of 807 municipalities in the Brazilian Amazon spans Q1 2007 - Q4 2012 for Columns 1-2 and Q2 2008 - Q4 2012 for Columns 3-6. Given the presence of zeroes, variables are log-transformed with the addition of 1, i.e. log(1+x). "Post Rollout" becomes 1 following registration on-site and remains so thereafter. "Medium" farms are sized 1-4 *mf* each; "Large" farms are greater than *4mf*. I control for annual population, Municipal-Product trend and Quarter-Year trend in all regressions. Finally, Columns (2) and (6) are the subsample of eligible municipalities that export for any two consecutive quarters; column (4) considers eligible municipalities and column (5): municipalities that export for 2 consecutive quarters.

### 1.8.3 Log Transformation

In my main specification, I use log-transformed variables because the data displays a high level of positive skewness and to improve the linearity of the model. Given the prevalence of zeroes in the data, the log-transformed variables I use are, in fact, $log(x+1)$ rather than $log(x)$; it maps zeroes to zeroes and behaves no differently than the log transformation for large values of $x$. Since the positive values of the variables of interest are large (thousands of dollars worth of exports or hundreds of square kilometers registered), the addition of the '1' for the nonzero observations should have no discernible effect. However, for small values of $x$, the log(1+x) transformation might be problematic. And arbitrarily adding '1' to zeroes might not be not palatable.

I provide two alternate approaches to this problem. The first is replacing the log transformed variables with the inverse hyperbolic sine (IHS) transformation: $log(x + \sqrt{(}x^2 + 1))$. The IHS transformation behaves like the log transformation for large, positive values of $x$, but it is well-defined at zero, handles negative values and small changes better than its logged counterpart. The other approach is to capture the effect of the 'zeroes' with a binary variable; for example, if a municipality has 0 registered area in a particular quarter, the 'zero' variable will equal 1 for that quarter. The results of these two exercises are presented in Tables 1.11 and 1.12.

The coefficients on the IHS transformation are interpreted like they are in log transformations.[26] In comparing Table 1.11 to its counterpart, Table 1.5, one can see that the direction of the coefficients remain unchanged, and the magnitudes are slightly higher across the board for the binary variables and lower for the 'Area'

[26]Except for small values of $x$, the inverse sine is approximately equal to $log(2x) = log(2) + log(x)$. The true effect of $\beta$ can be found by carrying out the reverse transformation: $x = 0.5\frac{\exp(2y)-1}{\exp(y)}$ or the hyperbolic sine. For example, suppose the coefficient is 0.53; applying the reverse transformation results in 0.55. This minute difference is usually not worth the effort.

variables.

For example, the arrival of *Terra Legal* to a municipality increases quarterly export value by 74% on average within a municipality-product pair under this transformation (compared to 70% in Table 1.5). The effect of the rollout 1 year later ('4 Quarters Out') has also become significant at the 10% level in this specification when considering the whole sample (Column 1) and for the municipalities that export for any two consecutive quarters (Column 3). Finally, the effect of the registered area on quarterly export value becomes less negative across all 4 columns by a marginal amount; for example, a 10% increase in the total area registered decreases quarterly export value by 1.6% (Column 1, Table 1.11), instead of 1.7% (Column 1, Table 1.5) on average within a municipal-product pair. The coefficient is smaller for "Medium" farms as well: 0.124 versus 0.131. These findings allow me to conclude that the $log(1+value)$ transformation does not bias the results.

Table 1.12 explicitly controls for zero registered area in the data. The variable 'zero registered area' is 1 if the area registered in a municipality is 0 at quarter $t$. A municipality that switches from having some registered area to no registered area, will see quarterly export value fall by 66% in each product on average, though the effect is insignificant. The inclusion of this variable does not alter the magnitude of the 'Rollout' and 'Post' effects. However, the effect of area registered (both total and by "medium" farms) on export value is more pronounced; a 10% increase in total registered area decreases quarterly export value by 4%, but if that increase is by "medium" farms, then the average quarterly export value increases by 2% for each municipal-product pair. Note, however, that the effect is conditional on having any registrations; the 10% increase is not a change from 0 to 0.1, but rather from 1 to 1.1. This conditionality also explains why the "Rollout" and "Post" variables are not

Table 1.11: Effect of Rollout on Export Value - Inverse Hyberbolic Sine Transformation

| | Value+ of Wood and Wood-related Exports | | | |
|---|---|---|---|---|
| VARIABLES | (1) | (2) | (3) | (4) |
| Rollout/Treatment | 0.567*** | 0.489*** | 0.624*** | 0.518*** |
| | (0.170) | (0.167) | (0.205) | (0.203) |
| Post Rollout By Quarter | | | | |
| .....1st Quarter Out | 0.603** | 0.465* | 0.890*** | 0.741** |
| | (0.255) | (0.267) | (0.308) | (0.314) |
| .....2nd Quarter Out | 0.691*** | 0.549* | 0.935*** | 0.782** |
| | (0.267) | (0.298) | (0.321) | (0.349) |
| .....3rd Quarter Out | 0.711*** | 0.554* | 0.938*** | 0.769** |
| | (0.245) | (0.311) | (0.309) | (0.389) |
| .....4th Quarter Out (1 Year) | 0.428* | 0.215 | 0.534* | 0.300 |
| | (0.262) | (0.366) | (0.326) | (0.465) |
| .....5th Quarter Out | 0.315 | 0.052 | 0.367 | 0.057 |
| | (0.286) | (0.371) | (0.356) | (0.497) |
| .....6th Quarter Out | 0.300 | -0.0013 | 0.402 | 0.034 |
| | (0.343) | (0.455) | (0.456) | (0.620) |
| ....7th Quarter Out | 0.195 | -0.217 | 0.241 | -0.271 |
| | (0.383) | (0.501) | (0.511) | (0.688) |
| .....8th Quarter Out (2 Years) | 0.110 | -0.436 | 0.039 | -0.643 |
| | (0.378) | (0.536) | (0.497) | (0.732) |
| Registrations | | | | |
| .....Area Registered+ | -0.161** | -0.177** | -0.170* | -0.166 |
| | (0.074) | (0.0901) | (0.096) | (0.116) |
| .....Area Registered by Medium Farms+ | 0.124** | 0.121* | 0.142* | 0.131* |
| | (0.064) | (0.065) | (0.078) | (0.077) |
| .....Area Registered by Large Farms+ | 0.045 | 0.051 | 0.032 | 0.038 |
| | (0.052) | (0.053) | (0.072) | (0.073) |
| Observations | 20,703 | 15,255 | 14,592 | 10,704 |
| R-squared | 0.062 | 0.063 | 0.086 | 0.090 |
| Municipality Product FE | YES | YES | YES | YES |
| Quarter-Year FE | YES | YES | YES | YES |
| Eligible for Registrations | | YES | | YES |
| Consecutive Exports | | | YES | YES |

*** p <0.01, ** p <0.05, * p <0.1

+ Variables are transformed by the inverse-hyberbolic sine function

Note: Robust standard errors in parentheses clustered at the municipal level. Sample of 807 municipalities in the Brazilian Amazon spans quarters Q1 2007 - Q4 2012; export products now limited to “Articles of Wood” products only (HS 44). I control for the total area cultivated, annual population as well as Municipality-Product trend and Quarter-Year trend in all specifications. Rollout is 1 for the particular quarter when *Terra Legal* registers on site and zero otherwise; each of the “Post” variables is 1 for only one quarter post-rollout and 0 otherwise; for example, “5th Quarter Out” is 1 for the fifth quarter after rollout and 0 otherwise. Regressions are run with all 12 quarters post rollout, but only the first 8 are presented in this table. Registered properties sized between 1-4 *mf* are“Medium” farms; “Large” farms have area greater than 4 *mf*. Column 1 includes the entire sample Column (2) considers municipalities that are eligible for registrations only, while (3) limits the sample to municipalities that export wood products for two consecutive quarters. Column 4 restricts the sample to eligible municipalities that export wood products for two consecutive quarters.

Table 1.12: Effect of Rollout on Wood Export Value - Controlling for Zeroes

| | Log(Value) | |
|---|---|---|
| VARIABLES | (1) | (2) |
| Rollout/Treatment | 0.549*** | 0.497*** |
| | (0.157) | (0.190) |
| Post Rollout By Quarter | | |
| .....1st Quarter Out | 0.572*** | 0.703*** |
| | (0.224) | (0.280) |
| .....2nd Quarter Out | 0.630*** | 0.726** |
| | (0.240) | (0.314) |
| .....3rd Quarter Out | 0.641*** | 0.709** |
| | (0.219) | (0.354) |
| .....4th Quarter Out (1 Year) | 0.396* | 0.284 |
| | (0.227) | (0.417) |
| Registrations | | |
| .....Zero Registered Area | -1.080 | -0.926 |
| | (0.794) | (0.109) |
| .....Log(Area Registered) | -0.404** | -0.357 |
| | (0.199) | (0.285) |
| .....Log(Area Registered by Medium Farms) | 0.211** | 0.203* |
| | (0.093) | (0.120) |
| .....Log(Area Registered by Large Farms) | 0.115 | 0.096 |
| | (0.076) | (0.109) |
| Observations | 20,781 | 10,704 |
| R-squared | 0.063 | 0.091 |
| Municipality Product FE | YES | YES |
| Quarter-Year FE | YES | YES |
| Eligible for Registrations | | YES |
| Consecutive Exports | | YES |

*** p <0.01, ** p <0.05, * p <0.1

Note: Robust standard errors in parentheses clustered at the municipal level. Sample of 807 municipalities in the Brazilian Amazon spans quarters Q1 2007 - Q4 2012; export products limited to Wood-based products only (HS 44-49). The variable "Zero" is 1 if registered area is zero. I control for the total area cultivated, annual population as well as Municipality-Product trend and Quarter-Year trend in all specifications. Rollout is 1 for the particular quarter when *Terra Legal* registers on site and zero otherwise; each of the "Post" variables is 1 for only one quarter post-rollout and 0 otherwise; for example, "5th Quarter Out" is 1 for the fifth quarter after rollout and 0 otherwise. Regressions are run with all 12 quarters post rollout, but only the first 8 are presented in this table. Registered properties sized between 1-4 *mf* are"Medium" farms; "Large" farms have area greater than 4 *mf*. Column 1 includes the entire sample; Column 2 restricts the sample to municipalities that export wood products for any two consecutive quarters, in addition to being eligible for rollout.

markedly different from the results in Table 1.5; if rollout occurs, then registered area is by definition positive. Thus, the presence of municipalities with zero registrations in the sample understates the effect of registered area in both directions. The naive $log(value+1)$ transformation of Table 1.5 finds that the effect of total registered area ranges between -0.17 and -0.187 depending on the subsample, while the conditional effect is between -0.35 and -0.4. Similarly, the effect of area registered by "Medium" farms is understated by as much as 53%.

Both the IHS transformation and the 'zero' indicator approach, show that the results of the main specification are conservative, but not tremendously different; more importantly, they do not reveal any inconsistencies.

### 1.8.4 The Great Recession

*Terra Legal* begins rollout on the heels of the financial crisis of 2008 and the onset of the Great Recession. During this time period, global trade, which was on a upward trajectory, declined sharply. The pre-treatment period in my sample begins in the first quarter 2007 and lasts for 10 quarters, exactly when the decline can be observed. While the use of quarter-year fixed effect, coupled with the dampening effect of the log transformation, deals with the issue, it does so by placing a downward bias on the estimates. Here, I test whether the estimates vary when limiting the sample to the post-crisis period: 1st quarter of 2009 - the last quarter of 2012. One caveat is that the pre-treatment period is only 2 quarters in this sample.

Table 1.13 presents the results. I include both the log transformation and the inverse hyperbolic sine transformation. I still include quarter-year and municipal-product fixed effects in all regressions to deal with unobserved heterogeneity post

Table 1.13: Effect of Rollout on Wood Export Value - Post Crisis

| VARIABLES | Value of Wood and Wood-related Exports (1) (Log) | (2) (IHS) | (3) (Log) | (4) (IHS) |
|---|---|---|---|---|
| Rollout/Treatment | 0.729*** | 0.784*** | 0.819*** | 0.878*** |
| | (0.213) | (0.233) | (0.263) | (0.287) |
| Post Rollout By Quarter | | | | |
| .....1st Quarter Out | 0.787*** | 0.845*** | 1.092*** | 1.168*** |
| | (0.302) | (0.332) | (0.374) | (0.410) |
| .....2nd Quarter Out | 0.883*** | 0.955*** | 1.190*** | 1.277*** |
| | (0.324) | (0.356) | (0.417) | (0.453) |
| .....3rd Quarter Out | 0.921*** | 0.998*** | 1.241*** | 1.331*** |
| | (0.303) | (0.330) | (0.408) | (0.439) |
| .....4th Quarter Out (1 Year) | 0.670** | 0.729** | 0.856** | 0.919** |
| | (0.293) | (0.319) | (0.421) | (0.453) |
| .....5th Quarter Out | 0.584* | 0.642* | 0.735* | 0.789* |
| | (0.334) | (0.367) | (0.426) | (0.457) |
| .....6th Quarter Out | 0.649* | 0.713* | 0.844 | 0.902 |
| | (0.357) | (0.387) | (0.532) | (0.570) |
| .....7th Quarter Out | 0.580 | 0.644 | 0.654 | 0.702 |
| | (0.402) | (0.437) | (0.597) | (0.638) |
| .....8th Quarter Out (2 Years) | 0.535 | -0.599 | 0.408 | 0.439 |
| | (0.395) | (0.428) | (0.616) | (0.658) |
| Registrations | | | | |
| .....Area Registered | -0.148 | -0.146* | -0.142 | -0.142 |
| | (0.094) | (0.088) | (0.136) | (0.128) |
| .....Area Registered by Medium Farms | 0.093 | 0.091 | 0.074 | 0.072 |
| | (0.074) | (0.069) | (0.093) | (0.087) |
| .....Area Registered by Large Farms | 0.047 | 0.045 | 0.049 | 0.047 |
| | (0.052) | (0.049) | (0.072) | (0.067) |
| Observations | 13,701 | 13,823 | 7,136 | 7,136 |
| R-squared | 0.011 | 0.011 | 0.018 | 0.018 |
| Municipality Product FE | YES | YES | YES | YES |
| Quarter-Year FE | YES | YES | YES | YES |
| Eligible for Registrations | | | YES | YES |
| Consecutive Exports | | | YES | YES |

*** p <0.01, ** p <0.05, * p <0.1

Note: Robust standard errors in parentheses clustered at the municipal level. Sample of 807 municipalities in the Brazilian Amazon spans quarters Q1 20009 - Q4 2012; eight quarters in 2007-2008 are not included. Export products limited to "Wood and Wood-related" products (HS 44-49). I control for the total area cultivated, annual population as well as Municipality-Product trend and Quarter-Year trend in all specifications. Export value and registration variables are log-transformed in columns (1) and (3), and inverse hyperbolic sine-transformed in columns (2) and (4).Rollout is 1 for the particular quarter when registers on site and zero otherwise; each of the "Post" variables is 1 for only one quarter and 0 otherwise; for example, "5th Quarter Out" is 1 for the fifth quarter after rollout and 0 otherwise. Regressions are run with all 12 quarters post rollout, but only the first 8 are presented in this table. Registered properties sized between 1-4 *mf* are denoted "Medium" farms; "Large" farms have area greater than 4 *mf*. Finally, Columns (1) and (2) include the entire sample; Columns (3) and (4) restricts the sample to eligible municipalities that export wood products for two consecutive quarters.

recession. The main takeaway from the table is that the effect of the rollout over time is much larger and more persistent for the post-crisis sample. First, the coefficients are much larger in magnitude for "Rollout" and "Post." The impact of rollout is tremendous in the post-crisis sample: an increase of 107% on average, versus a 74% increase for the entire sample. Also, Table 1.5 shows that the effect of rollout peters out by the 4th Quarter, both in terms of magnitude and significance. However, for the post-crisis sample, the effect of rollout is still significant 6 quarters post rollout, and the magnitude falls much more gradually. Finally, the coefficient on total area registered becomes less negative, though not by much, and the scale variables are no longer significant.

Given that pre-rollout observations exist for two quarters only, it might be insufficient to draw conclusions about the parallel trends in municipalities with high versus low registrations. Thus, the results should be taken with a grain of salt. What this exercise shows, however, is that the main regression results presented in Table 1.5 are a lower bound for the effect of the rollout over time and not markedly different for the registered area variables.

In summary, these robustness checks show that the main results conservatively estimate the effect of *Terra Legal* rollout and registered area. Limiting the original sample to fewer product-categories or to fewer years pre-rollout increase the magnitude of the predicted effect in general.

## 1.9 Conclusion

This paper provides empirical evidence that between 2009-2012, Brazil's latest titling scheme *Terra Legal*, dismayingly *increases* wood-based exports from the Amazon, and consequently, deforestation, at least in the short run. Moreover, the medium-sized farms that obtain the title cheaply appear to be the real culprits behind forest exploitation, clearing land to make way for livestock production.

However, all might not be lost if land clearing stops sooner in the long run, as predicted by my theoretical model. It would be worthwhile to perform the empirical exercise again in 10-20 years to see whether wood exports and the rate of deforestation have decreased. Extending the model to the large open economy case where the agent's actions can affect future prices would be interesting, as well. The findings in my paper show that changes in the property rights regime is an important area of research.

Meanwhile, future policies should target ways in which to reenumerate agents for the standing forests because they provide a global service. Establishing property rights is the first step in this direction because we now know to whom such payments should be made. Thus, the findings in this paper should not discourage property rights establishment, but encourage such action sooner rather than later.

# Chapter 2

# Property Rights, Agricultural Productivity and Deforestation in the Brazilian Amazon

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## 2.1 Introduction

The pareto-improving aspects of property rights establishment has long been touted by economists. It reduces overexploitation (the typical tragedy of the commons story), allows agents to bargain and reach the efficient outcome (Coase, 1960), and increases investment in farmers (Besley, 1995). However, when two (positive) outcomes are inherently at odds with each other, property rights might not be the panacea we would like it to be. Because land is a competing factor in forest management and agricultural production, increasing investment in land and cultivating more crops comes at the cost of deforestation. Moreover, the dynamic model presented in Chapter 1 showed that depending on the timing of the property rights establishment, forested land could be converted for agricultural uses in the short run, but more land will be allocated to forests in the long run. Thus, it is unclear how Brazil's latest titling scheme, *Terra Legal*, affects agricultural production and the Amazon rainforest at large.

This paper provides evidence that the latest titling scheme increases deforestation. Moreover, this effect comes through the investment channel because registrations increase the access to loans that are explicitly for agricultural investment purposes. This finding suggests that it is more profitable to engage the land for agricultural purposes, at least in the short run. Furthermore, the substitution away from temporary crop cultivation toward permanent crop cultivation in response to titling indicates that agents engaged in less profitable ventures in order to maintain their claim on the land prior to property rights establishment. Our empirical contribution informs three different strands of literature: property rights and related issues, land allocation/competing land uses, and papers on Brazil's land reform efforts, in particular.

The property rights intervention, which started in July 2009, is different from previous titling efforts in Brazil because it took titling to the squatters. This resulted in a phased rollout over time because officials travelled to the sites to inspect the properties, as well as to advertise and increase uptake.[1] This feature, in addition to the pricing scheme, made titling free for many squatters; small farms below a certain size threshold[2] obtained the property for free, while medium properties paid some discounted price; only the large properties paid the full market price for the title.[3] The "catch" is the requirement that land owners maintain forests on 80% of their property, the so-called "Legal Reserve," or risk losing the title, though evidence shows that agents probably do not adhere to this.

[1] However, it was not strictly phased in that agents could still register their property at any time if they were willing to travel to the field office.

[2] The size threshold varied by municipality, based on population and proximity to cities.

[3] However, it is not clear how the government determined which the market price or the discounted price that the land owners must pay, nor do I have information on how much they paid.

Given that trees need to mature for at least 25-30 years before they are ready for harvestation, sustainable forestry and diversification might be more feasible for large properties sprawling many square kilometres, which can take advantage of economies of scale. Meanwhile, intensifying agriculture in a small plot of land is the best option for a small farmer to increase his revenue. Indeed, we find heterogenous responses based on the size of the properties registered. Access to credit increases the most for small farms, both at the intensive and extensive margin; we find that a 1 percentage point (p.p.) increase in the area registered by small farms leads to a 30% in the financing and a 5 % increase in the number of credit contracts for investment purposes. However, we find no evidence indicating that the small farms engage in deforestation; they decrease their temporary crop cultivation, but do not increase their permanent crop cultivation. These findings suggest that the smallest farms, unwilling to risk losing their provisional title, decrease their land use ever so slightly.

The largest farms that pay the most for their land engage in land conversion activities; a 1 p.p increase in the area registered by such properties increases the area deforested by 0.15% on average within a municipality, but the annual rate of deforestation *decreases*, by 4%. These properties increase the cultivation of cash crops—tobacco, soy and cocoa—by approximately 4 % on average within a municipality, with cocoa cultivation in particular driving the effect.

Increasing the area registered by medium-sized farms that obtain their land cheaply leads to an increase in the area deforested, as well as an increase in the rate of deforestation, by 0.3% and 4.4%, respectively, in response to a 1 p.p increase in their area registered. This finding is consistent with the results in Chapter 1 of my thesis, which shows that wood exports increase in response to area registered by medium-sized properties. The medium-sized farms appear to enjoy the “Goldilocks” phenomenon; they are small enough to escape scrutiny from officials, but large enough to take advantage of economies of scale. In addition, the crop cultivation outcomes are not responsive to an increase in the area registered by these properties, suggesting that these farmers probably use their land for other purposes, such as cattle ranching.

## 2.2 Literature Review

Our paper contributes to three different strands of literature: property rights and related issues, land allocation/competing land uses, and papers on Brazil's land reform efforts, in particular.

Previous work on property rights and squatting include the seminal paper by Besley (1995), whose theoretical model shows that having a land title allows individuals to invest in their land because it gives them security and freedom from expropriation; he provides empirical evidence from Ghana that confirms the theoretical prediction. Mendelsohn (1994) presents two theoretical model of squatters; one shows that rent dissipates if property rights are excessively defended, while the other shows that even relatively low threats of appropriation can discourage investment in long-term assets such as forestry. We add evidence from Brazil to this literature, providing a direct link between property rights and access to credit

A few papers endogenize property rights establishment and model it as a consequence of trade. Free trade increases the value of renewable resources over time; this coupled with faster resource implementation under open access might galvanize governments to grant property rights to protect their assets (Copeland and Taylor, 2009). Their theoretical model shows that an economy with the right combination of properties, such as a high time preference and fast growing resource stock, can make the transition into optimal resource management following trade liberalization. Hotte et al. (2000) model another aspect of endogeneity: a firm's decision to enforce its property rights over renewable resources. If the cost of enforcement exceeds the resource value, enforcement is not profitable, resulting in de facto open access despite well-defined property rights. They analyze trade liberalization in this context and find that switching from open access in autarky to enforcement under free trade might lead to resource conservation, but welfare loss because the more valuable resource demands more costly enforcement.

Recognizing that certain economic activity can result in resource destruction, some theoretical models incorporate the land conversion issue and resource loss more generally. Skonhoft (1998) finds that the social planner will allocate less land for the resource habitat in the long run if the alternative uses for the land is more profitable. Barbier and Burgess (1997) develop a model that determines the optimal allocation of land use between forestation and agriculture that depends on the degree to which agents internalize the positive externalities of forests. Bulte and Horan (2003) study habitat conversion in a open access scenario and find that multiple equilibria exist that depend on the extent of the spillover. Hartwick et al. (2001) models the land allocation decision of a social planner dynamically within a small open economy. They first present their findings for the baseline model when land conversion is irreversible and then extend it to a case where the land is cleared and then allowed to recover. In the first chapter of my thesis, I extend this model to account for lack of property rights over land. Our main contribution to this literature is the empirical evidence we provide on the land allocation decision.

Brazil's property rights problems are also well documented. Alston et al. (1999) use game theory to model rural conflict involving squatters in Brazil; they also provide empirical evidence using state-level data from 1988-1995 and find that the Brazil's land reform policy might incentivize agents to engage in more violence. Araujo et al. (2009) model deforestation as a way to secure property rights and also incorporate violent grabbing. Empirically, they find that insecure land rights could have contributed to higher deforestation rates in the Brazilian Amazon between 1988-2000. Our paper consider the impact of clear public-to-private transfer of land, when squatters become bonafide land owners, and its impact on credit, deforestation and crop choice.

## 2.3 Policy Briefing: *Terra Legal*

To combat the severe threat to the Amazon and quell land disputes and violent land-grabbing, the government introduced its latest titling scheme in 2009 called *Terra Legal*. The provision grants rights to squatters who had been utilizing the land for at least five years; so, if a person had been living on a piece of land in the Amazon from 2004, he would be eligible to start the titling process in 2009. However, it is unclear whether the programme benefits the targeted recipients and whether it will succeed in its goals of reducing deforestation, particularly due to widespread corruption and weak enforcement.[4]

Though this is the latest in Brazil's long line of land reform measures, this policy differs in its registration efforts and pricing scheme. To make registration easier, officials go to the Amazon to register the squatters rather than requiring them to

[4] "Brazil grants land rights to squatters living in Amazon rainforest." The Guardian. June 26, 2009. http://www.theguardian.com/environment/2009/jun/26/amazon-land-rights-brazil. "Illegal Land Occupation in Terra Legal." O Eco Amazonia. August 6, 2010. http://www.oecoamazonia.comen/news/brazil/54-grilagem-no-terra-legal-

travel to the bureaucratic headquarters. In the past, many squatters did not take advantage of their constitutional right to obtain land rights due to the high costs involved with titling and the bureaucratic backlog Araujo et al. (2009). By taking the registration to the squatters, the government hopes to increase uptake.

The rollout of the programme is not strictly phased (in that a squatter could travel to get his land registered whenever he chooses), but the extensive registration process effectively turns it into a phased rollout. Officials travel to each municipality to promote the titling scheme and then examine the property being registered to ensure it had been squatted upon for at least five years, as well as to determine the exact border and to geocode it. Registering typically took about two weeks to a month, depending on how large the municipality is and the number of people applying for a title, after which the officials moved on to the next municipality. Since this is a federal programme with federal employees and officials, the state cannot exert as much influence on the rollout.

Of course, the concern that the rollout could be endogenous to deforestation probably comes to mind; if the programme hopes to reduce deforestation and targets the areas with the highest deforestation, problems with identification can arise. To get more clarity on the programme, one of us met with *Terra Legal* employees. This programme is part of a larger scheme to geocode and catalogue the lands in the Amazon. At a monthly meeting, heads of the different agencies, such as FUNAI that is responsible for the indigenous people, the Environmental Ministry and Terra Legal, gather and determine which portions of a particlularly (large) area of land, called a "gleba," fall under each of their purview.[5] If the other agencies do not claim any of

[5] *Glebas* appear to have been determined around the time when colonists settled Brazil; perhaps it is a remnant of that time, but our contact was not able to explain it further than that. It is certainly nothing that is determined because of deforestation in the near present.

these parcels in the *gleba*, *Terra Legal* then steps in and tries to get those areas titled, in no particular order. The government hopes that this comprehensive effort will help its monitoring efforts in the future because it can easily identify on whose land the trees are being cut, thanks to the national catalogue or "Cadastro" of land owners.

Another interesting aspect of the programme, apart from the rollout, is its controversial pricing scheme that makes titling effectively free for many squatters, incentivizing them further to take up the titling. The pricing, like the rollout of the programme, also varies by municipality. For land that is under a certain size (less than one *modulo fiscale*), the cost of the titling is free; if it is between one and four *modulo fiscales*, the land owner must pay some price that is less than market value, with payments phased out over 20 years; above this level, the land owners can get rights to the land, but must pay market price, again over a 20 year period.[6] Each municipality defines the exact size of its *modulo fiscale*, ranging from 5 to 100 hectares, with the average *modulo fiscale* equaling about 70 hectares, which is less than one square kilometre.

Between July 2009 and November 2012, more than 100,000 applicants had already registered their land. 95% of the registered farms are in the Amazon region, in the states of Amapa (AP), Amazonas (AM), Acre (AC), Mato Grosso (MT), Maranhao (MA), Para (PA), Roraima (RR), Rondonia (RO) and Tocantins (T0). In Table 2.1, I provide some fast facts about the registered and titled farms. The total area registered is around 125,000 square kilometres (or over 31 million acres); this is approximately 2.58% of the Brazilian Amazon.

About 3% of those registered have already obtained land titles; the titled area

[6] The law specifies the sale at market value, but it is unclear how exactly the government determines this price or what it is.

spans roughly 2,250 square kilometers (about 558,000 acres) and is solely in the Amazon. More than half the registrations are the "small" farms measuring less than 1 *mf*; "medium" farms make up roughly 30% of the total registrations and only 14% of the registrations are "large" farms. The "small" farms go through the process fairly painlessly, which is reflected in the fact that almost 3 in 4 titled properties are small; large properties receive a thorough and careful review before being approved for the title.

The average size of a registered farm is 124 hectares (1.24 square kilometers) and 1.76 *modulo fiscales (mfs)*. Each municipality has an average of 245 registrations, and the average area registered in a municipality is 305 square kilometers.

Table 2.1: *Terra Legal* - Fast Facts (as of 2012)

| | |
|---|---|
| Number of Municipalities with Any Registrations | 410 (out of 807) |
| Number of Registrations | 107,280 |
| ....by "Small" Farms ($a < 1mf$) | 60,505 |
| ....by "Medium" Farms ($1mf \leq a < 4mf$) | 31,396 |
| ....by "Large" Farms ($a \geq 4mf$) | 15,379 |
| Number of Titles | 3,015 |
| ....by "Small" Farms ($a < 1mf$) | 2,184 |
| ....by "Medium" Farms ($1mf \leq a < 4mf$) | 710 |
| ....by "Large" Farms ($a \geq 4mf$) | 121 |
| Total Area Registered | 126,374 sqkm |
| Total Area Titled | 2,257 sqkm |
| Share of Registered Properties with Titles | 3% |
| Share of Registered Area that is titled | 1.8% |
| Average Number of Registrations (in a Municipality) | 245 |
| Average Area Registered | 305 sqkm |
| Average Size of Registered Property | 1.24 sqkm (1.76 *mf*) |
| Average size of 1 *modulo fiscale (mf)* | 0.8 sqkm |

Though the small farms make up the largest share of registrations at 31% on average in 2012, they only comprise about 12% of the total registered area. Properties in Category II (between 1 and 4 $mf$) make up 24% of of the total area registered. Finally, the share of registered area covered by the largest properties is 37% of the total registered area; this is unsurprising given that large farms, by their very definition, should cover a large area.

Because of the discounted pricing, cattle ranchers, large farms and logging firms have an incentive to increase their land holdings by buying land cheaply. The programme prohibits immediate sale of the newly titled lands, but after three years, the land can be sold on the market, most likely to large farms and logging firms. However, given that over 7000 registered farms are equal to or larger than 1000 hectares (10 square kilometres or 2400 acres) in the registration data, it appears to be the case that large farms and logging firms might have already carved out a piece of the Amazon for themselves.

Figure 2.1 shows the total area occupied by registered farms in the entire sample and their location, overlaid onto a satellite image of the Brazillian Amazon in 2010. This shows the impact land titling could potentially have on the forest stock.

To combat deforestation and promote sustainable forestry, *Terra Legal* has instituted a few safeguards. The land title states that the property owners must maintain an 80% legal reserve on the land, i.e. 80% of their property should consist of forests/trees. Failure to do could result in the appropriation of the property. To that end, the small farms that can be obtained at no cost are only given a provisional title for 10 years; the final deed is only approved if this legal reserve has been met.[7] The provisional title is enough to use as collateral, however, so investments need not put on hold. Officials can also drop by unannounced to examine the property.

[7]Those who pay something for the property receive their actual deed, rather than the provisional one, though they are constrained from reselling it for a period of 3 years.

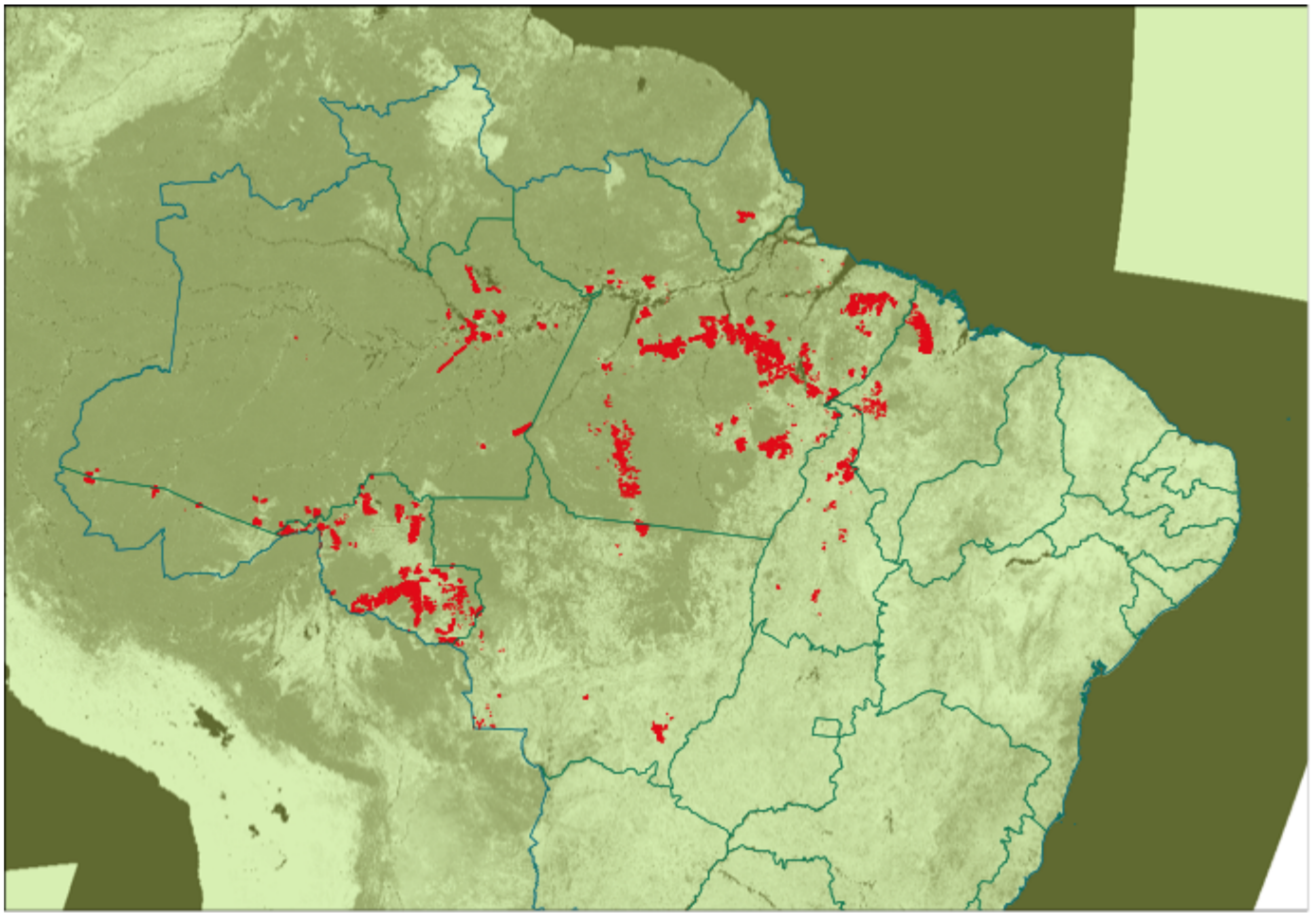

Figure 2.1: Snapshot of the Brazilian Amazon in 2010 overlaid with the entire sample of registered farms from 2009-2012

From what I could observe, this rule is not adhered to strictly. During my visit with the *Terra Legal* officials to a farm with the provisional title at the outskirts of Manaus, the farmer proudly displayed the water pump he had newly installed that he was able to put in due to the loan he received using the title as collateral. When I enquired about the lack of trees, certainly nowhere near the 80% mark, he shrugged it off, saying that a fire consumed them. He had not taken any steps to replant them, at least during the time of my visit, and the officials simply shrugged it off.

## 2.4 Data

The dataset we have obtained from *Terra Legal* provides detailed records of registrations from July 2009 to October 2012. This includes information on the person who applies for titling, the amount of land registered, the month and year of registration, the location of the farm, and the status of the application. Our attempts to follow up on this data set and get it updated went unanswered, so we proceed with the data that we have, with registrations serving as a proxy for titling. Since very few applications for title get rejected (less than 2%, and the rejections are more due to incorrect completion of the application or duplication rather than any confounding reason), the registrations are an almost perfect correspondence to titling. Using this registration information, we construct our explanatory variables at the municipal-year level.

Total area registered area, measured in square kilometers, is a stock variable; it is divided into three subcategories according to the size of the properties: "Small" Farms each have area less than 1 *modulo fiscale (mf)*, approximately 0.8 hectares. "Medium" properties are of size 1-4 *mf*, while the "Large" Farms are bigger than 4 *mf*.

Our crop cultivation data comes from IPEA, the Institute of Applied Economic Research, augmented with data from IBGE, the Brazilian Institute of Geography and Statistics, since the IPEA dataset is only available until 2010. IPEA publishes the data collected by IBGE, but it is cleaner and more user-friendly. This data provides information on the area cultivated in a municipality annually, as well as the amount of various crops, the quantity produced and the value of the production at the municipal-year level. In addition, the IPEA also provides information on municipal population estimates annually, as well as municipal GDP, which is subdivided into whether or not its agricultural. We have this data from 2000-2012 and construct our cultivation outcome variables.

The total area cultivated, measured in hectares, is a flow variable. Area cultivated by temporary crops are crops that require annual planting, such as wheat, rice, soy, tobacco and corn, while area cultivated by permanent crops can be harvested over multiple years, such as coffee, cocoa and oranges. These are all variables provided by IBGE. In addition, we also consider area cultivated by cash crops, a variable we create by summing up the area cultivated by tobacco, cocoa and soy; though soy and tobacco are temporary crops, they are more valuable.

Our credit data comes from PRONAF, the National Program to strengthen Family Farming, a rural credit programme that was established in the 1990s to improve agrarian outcomes of small farms. They have the lowest credits rates, as well as the lowest default rates among Brazil's credit systems. Farmers can apply for credit by filling out a form, which asks them for what purposes they seek credit. This could be for the purchase of seedlings, for example, but mainly for investment in equipment, machinery or infrastructure, as well as commercial purposes. Our data, available from 2007 -2012, is aggregated at the municipal level, and provides information on the number of contract, the value of the loans, and what type of loan it is. From this information, we construct our credit outcome variables: 'Number of Investment Contracts,' which is the number of loans issued by PRONAF in a municipality for investment purposes, and 'Value of Investment Credit,' which is the amount of financing extended extended for credit purposes.

Our deforestation data comes from the PRODES project, a government programme which has been monitoring deforestation in the Amazon via satellites since 1998. It is part of the collaboration between the Environmental Ministry (MMA), the Brazilian Institute of Environment and Renewable Natural Resources (IBAMA), as well as the Ministry of Science, Technology and Innovation (MCTI). The images are reviewed every two weeks at a resolution of 20-30 meters; the consolidated data presents the annual deforestation estimates, after accounting for cloud cover and so forth. Apart from their numbers, they also make the maps available for researchers. We use the numbers published by PRODES, however, because it is highly reliable. This dataset spans the years 2000-2012 and includes information on the area deforested in square kilometers, the area forested, cloud cover and so forth. Our two deforestation outcome variables are 'Area Deforested' and 'Rate of Deforestation,' which we obtain by looking at the change in the area deforested.

Finally, we obtain the election information from the Superior Electoral Tribunal (TSE), which collects and publishes information on the candidates who run for any election in Brazil, their occupation, party, whether or not there is a runoff, as well as the final outcome of the election. Using this information, we construct additional controls for credit access: 'Re-election' which is 1 if either the municipality's mayor or the state's governor are up for reelection. Both governors and mayors are up for reelection every four years, but elections occur every two years. Gubernatorial elections occurred in 2006 and 2010, while mayoral election occurred in 2008 and 2012. Roughly 40% of the mayors and 80% of governors run for re-election. In addition, we also consider 'Incumbent Victory,' which is 1 if the incumbent wins the re-election. Of the 40% of the mayors running for re-election, roughly 50% win the second term; meanwhile, 80% of the governors running for the second term win. Additionally, we also include the municipal-governor party match, which occurs roughly 25% of the time.

Table 2.2: Summary Statistics

| Variables | Full Sample | Post Rollout |
|---|---|---|
| Total Area Registered | 1.22 | 1.83 |
| | (2.29) | (2.60) |
| .....by Small Farms | | |
| | | |
| .....by Medium Farms | 0.75 | 1.10 |
| | (1.77) | (2.05) |
| .....by Large Farms | 0.72 | 1.06 |
| | (1.83) | (2.14) |
| | | |
| Total Area Cultivated | 8.87 | 8.85 |
| | (1.75) | (1.78) |
| .....with Permanent Crops | 5.21 | 5.17 |
| | (2.34) | (2.34) |
| .....with Temporary Crops | 8.70 | 8.68 |
| | (1.83) | (1.85) |
| .....with Cash Crops | 3.45 | 3.49 |
| | (4.27) | (4.30) |
| .....with Cocoa | 0.938 | 0.989 |
| | (2.22) | (2.27) |
| | | |
| Area Deforested | 5.72 | 5.73 |
| | (2.94) | (2.94) |
| Deforestation Rate | 1.37 | 1.25 |
| | (1.57) | (1.47) |
| Investment Contract | 3.31 | 3.37 |
| | (1.96) | (1.91) |
| Investment Value | 11.49 | 11.71 |
| | (4.84) | (4.72) |
| | | |
| Population Estimates | 10.27 | 10.29 |
| | (1.07) | (1.07) |
| Non-agricultural GDP | 11.03 | 11.09 |
| | (1.24) | (1.23) |
| | | |
| Re-election$^+$ | 0.254 | 0.281 |
| | (0.436) | (0.450) |
| Incumbent Victory$^+$ | 0.168 | 0.193 |
| | (0.374) | (0.394) |
| Party Match$^+$ | 0.131 | 0.106 |
| | (0.337) | (0.308) |
| | | |
| Observations | 4,660 | 3,113 |

$^+$ Indicator variables

Note: Observations are at the municipal-year level. Standard deviation in parentheses.The sample consists of 790 municipalities in Brazilian Amazon and spans from 2006 - 2012. All variables are transformed by the inverse hyperbolic sine function, except the indicator variables. Column (2) limits the sample to the years 2009-2012, when rollout takes place. Registrations are broken down by size, defined as follows: "Small" Farms - less than 1 *mf*; "Medium" - between 1-4 *mf*; "Large" Farms - more than 4 *mf*.

Table 2.2 presents the summary statistics of the inverse hyperbolic since transformation of the variables. Column (1) spans the sample from the years 2006-2012 for 790 municipalities in the Brazilian Amazon, while Column (2) restrict the data to the post-rollout only (2009-2012).

## 2.5 Empirical Strategy: Fixed Effects

We estimate the effect of three different outcomes—deforestation, crop cultivation, and credit—using a fixed effects model. We assume that the regressors are exogenous, i.e. the errors are not correlated with area registered. Given the complicated criteria used by *Terra Legal* to determine the eligible areas for rollout, we argue that this is a reasonable assumption.

Second, we apply the inverse hyperbolic sine (IHS) transformation to the variables of interest. The IHS transformation behaves like the log transformation for large, positive values of $x$, but it is well-defined at zero, handles negative values and small changes better than its logged counterpart. The coefficients on the IHS transformation are interpreted like they are in log transformations.[8] Given the presence of a large number of zeroes in the explanatory variable, we choose the IHS transformation over the log transformation.

Running the regression on levels is problematic because there is more area to cultivate in larger municipalities, which means the average effect on area cultivated would be overstated for small municipalities and understated for larger ones. By

[8]Except for small values of $x$, the inverse sine is approximately equal to $log(2x) = log(2) + log(x)$. The true effect of $\beta$ can be found by carrying out the reverse transformation: $x = 0.5\frac{\exp(2y)-1}{\exp(y)}$ or the hyperbolic sine. For example, suppose the coefficient is 0.53; applying the reverse transformation results in 0.55. This minute difference is usually not worth the effort.

transforming the variable, we can estimate the percentage effect. The second reason for transforming the variable is due to the high level of positive skewness in our outcome variables, which have a long righthand tail. Positive skewness results in a larger mean, which is problematic when combined with fixed effects, given that it de-means the variable. Transforming the outcome variables results in a more normal distribution. Finally, we also transform the explanatory 'registered area' variables to improve the linearity of the model, a key assumption for OLS. In addition, as in the case of the outcome variables, registered area variables are also positively skewed. A large number of municipalities have little registered area, but a small number of them have a large share of registered area.[9] Thus, the IHS-IHS regression better fits the data, leads to more well-behaved standard errors and is also easier to interpret.

We fit the following model for all outcome variables, but present the equation for crop cultivation only. Note that all variables are transformed using the inverse hyperbolic sine function (IHS):

$$Area\_Cultivated_{it} = \alpha + \beta_1 Area\_Registered_{it} + \Gamma Controls_{it} + \mu_i + \nu_t + \epsilon_{it}, \quad (2.1)$$

where $Area\_Cultivated_{it}$ is either the total area cultivated in municipality $i$ in year $t$, and the main variable of interest, $Area\_Registered_{it}$, a stock variable, is the area registered in municipality $i$ in year $t$. $Controls_{it}$ include the annual nonagricultural municipal GDP and annual population estimates. Additionally, we use political economy variables such as reelection prospects for incumbent mayors and governors, as well as party match between the two, as controls for testing the effect of registrations

[9] The amount of registrations in a municipality is determined initially by the policy, after which there is the take-up.

on access to rural credit. In addition, we also account for unobserved heterogeneity in municipalities such as proximity to markets with $\mu_i$, the municipal fixed effects term, while $\nu_t$ controls for shocks over time. Finally, we cluster our errors at the municipal level.

To estimate the scale effects, we split the registered area into three size categories:

$$\begin{aligned} Area\_Cultivated_{it} = \quad & \alpha + \beta_2 Area\_Reg\_Small_{it} + \beta_3 Area\_Reg\_Med_{it} + \\ & \beta_4 Area\_Reg\_Large_{it} + \Gamma Controls_{it} + \mu_i + \nu_t + \epsilon_{it}, \end{aligned} \tag{2.2}$$

where $Area\_Reg\_Small_{it}$ is the area registered in municipality $i$ in year $t$ by the smallest farms (less than 1 *mf*), which obtain the title for free. $Area\_Reg\_Medium_{it}$ is the area registered by "medium" farms only, sized between 1 and 4 *mf*, and $Area\_Reg\_Large_{it}$, the area registered by "large" farms sized above 4 *mf* in municipality $i$ in year $t$.

$\beta_1$ measures the response of area cultivated to the total area registered and is an elasticity. So, a 1% increase in total area registered in municipality $i$ at time $t$ increases/decrease average area cultivated within a municipality by $\beta_1$%. Similarly, a 1% increase in the area registered by small farms leads to a $\beta_2$ % change in the average area cultivated within a municipality. $\beta_3$ and $\beta_4$ capture the percentage change response of area cultivated to area registered by medium and large farms.

In addition to the effect on total area cultivated, we also consider the effect on temporary crop cultivation, permanent crop cultivation, and cash crop cultivation, by replacing the outcome variable with these secondary outcomes. Even if there is no effect overall, we suspect that agents will substitute temporary crop cultivation with more permanent crop cultivation following property rights establishment given increased land security. Apart from crop cultivation outcomes, we estimate the impact on deforestation and access to credit.

## 2.6 Results

We estimate the effect of registrations on access to credit, land choice (deforestation), and crop choice. We find that the effect varies based on the size of the properties registered. We find that access to credit indeed increases in response to the total area registered, particularly for small farms, both at the intensive and extensive margin.

### 2.6.1 Credit

First, we estimate the effect of registrations on access to credit. Specifically, we concentrate on the number of contracts issued for agricultural investment purposes by PRONAF, as well as the overall financing amount, a.k.a the value of loans.

Evidence shows that a 1 percentage point increase in registered area increases the amount of financing by roughly 23% (Columns 1 and 2) and the number of contracts by 4% (Columns 5 and 6) on average within a municipality; the results are statistically significant at the 1% level. We further find that the smallest farms are the main beneficiaries; the provisional title allows them to borrow for investment purposes, a welfare-improving outcome. Specifically, a 1 percentage point increase in area registered by properties sized less than 1 $mf$ increases the amount of financing by 30% and the number of contracts by 5%. Since PRONAF is a credit programme that specifically targets small family farms, it is not surprising that the changes to credit access predominantly come through the small farms. It is possible that the larger properties also get increased access to the credit market, but these results indicate

Table 2.3: The Impact of *Terra Legal* Registrations on Rural Credit Access

| | Rural Credit for Agricultural Investment | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Contract | | | | Value | | | |
| VARIABLES | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Area Registered | 0.0424*** | 0.0360*** | | | 0.226*** | 0.225*** | | |
| | (0.0111) | (0.0113) | | | (0.0553) | (0.0590) | | |
| .....by Small Farms | | | 0.0526** | 0.0517* | | | 0.311** | 0.301* |
| | | | (0.0266) | (0.0273) | | | (0.153) | (0.167) |
| .....by Medium Farms | | | -0.0103 | -0.0136 | | | -0.162 | -0.188 |
| | | | (0.0277) | (0.0290) | | | (0.169) | (0.178) |
| .....by Large Farms | | | 0.0106 | 0.00381 | | | 0.180 | 0.213 |
| | | | (0.0207) | (0.0217) | | | (0.128) | (0.131) |
| Re-election+ | | 0.824*** | | 0.845*** | | 1.731*** | | 1.750*** |
| | | (0.176) | | (0.177) | | (0.474) | | (0.476) |
| Incumbent Victory+ | | -0.677*** | | -0.681*** | | -0.978** | | -0.957** |
| | | (0.168) | | (0.169) | | (0.397) | | (0.400) |
| Party Match | | -0.201 | | -0.209 | | -0.253 | | -0.257 |
| | | (0.141) | | (0.141) | | (0.369) | | (0.369) |
| Population | 0.907* | 0.686 | 0.828 | 0.624 | 4.131** | 3.926** | 4.015** | 3.808** |
| | (0.541) | (0.553) | (0.531) | (0.546) | (1.618) | (1.781) | (1.613) | (1.774) |
| Observations | 3,084 | 2,304 | 3,084 | 2,304 | 3,084 | 2,304 | 3,084 | 2,304 |
| R-squared | 0.021 | 0.040 | 0.021 | 0.039 | 0.017 | 0.027 | 0.017 | 0.028 |
| Municipal FE | YES | YES | YES | YES | YES | YES | YES | YES |
| Year FE | YES | YES | YES | YES | YES | YES | YES | YES |

+These are indicator variables leading by 1 year

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors clustered at the municipal level in parenthesis. Sample consists of 790 municipalities in the Brazilian Amazon from 2006-2012. All variables are inverse hyberbolic sine transformed, except the indicator variables. Columns (1-4) present the effect on the number of contracts, issued by PRONAF for agricultural investment pruposes, while (5-8) on the value of the financing. The odd-columns present the results of total registered area, while the even-numbered ones break down the effect by size. Registered properties sized less than 1 *mf* are "Small" farms, between 1-4 *mf*—"Medium", and greater than 4 *mf*— "Large". Columns (3-4) and (7-8) include additional control variables—re-election outcomes—to limit omitted variable bias. 'Re-election' is 1 if the mayor or governor is up for re-election the previous year; 'Incumbent Victory' is 1 if the mayor or governor wins the re-election. Finally, 'party match' is 1 if the mayor and governor are both of the same political party. We control for annual non-agricultural GDP and population in a municipality in addition to municipal fixed effects and year fixed effects in all regressions.

that it is not through PRONAF. Interestingly, the coefficient on medium farms only is negative, though insignificant.

We include the re-election variables as controls in Columns (3-4, 7-8). It is likely that political leaders try to curry favor with their voting base by increasing access to credit in the year leading up to the election. Indeed, we find that both the 'Re-Election' variable and the subsequent 'Incumbent Victory' are strongly significant. The inclusion of these variables limits our omitted variable bias. In addition, the coefficients on our registration regressors do not change much in magnitude, but the standard errors are slightly larger.

Thus, we conclude that the titling programme increases the access to rural credit for investment purposes, both intensively (amount of financing) as well as extensive (number of contracts).

### 2.6.2 Deforestation

Next, we predict the effect of registrations on deforestation. The theoretical predictions of the dynamic model presented in Chapter 1 suggest that deforestation could increased or decrease; the empirical finding of increased wood exports following titling, however, implies greater deforestation, particularly by the medium-sized properties. Indeed, we find that the titling scheme leads to an increase in the total area deforested overall; a 1 p.p increase in the area registered increases te the effect of overall registered area decreases the area deforested, a 1 percentage point (p.p.) increase in the area registered by medium properties increases the total area deforested by approximately 0.3% on average within a municipality that is eligible for rollout, while the annual rate of deforestation increases by 4% within the same.

Tables 2.4 and 2.5 present the results for the total area deforested and the annual deforestation rate, respectively. A 1 p.p. increase in the total area registered increases the area deforested by approximately 0.1% on average within a municipality; the magnitude of the effect is larger (0.3%) if we restrict the sample to just the municipalities that are eligible for rollout, as reported in Column (3). Upon decomposing the total area registered by size, we see that the 'medium' properties, and to a lesser extent the largest ones, drive the overall effect. If that 1 p.p increase is attributed to "medium" or "large" farms, the area deforested increases by 0.19% and 0.13%, respectively. Limiting the sample to just the municipalities eligible for rollout leads a slight increase in the magnitudes of the coefficient, though the increase is larger for medium farms (0.27%). The small farm effect is negative, though insignificant. These findings are consistent with those in Chapter 1; the medium-sized properties are small enough to escape scrutiny, but large enough to take advantage of economies of scale by clearing land.

In Table 2.5, we consider the annual deforestation rate as the outcome variable. Surprisingly, the overall registration effect on the deforestation is negative for the entire sample, but turns positive and insignificant when limiting the sample to the municipalities that are eligible for rollout. This is due to the differing responses of medium and large farms. The medium-farm effect continues to be positive, though only significant for the subsample of municipalities eligible for rollout. A 1 p.p. increase in the area registered by 'medium' farms increases the rate of deforestation by approximately 4.4% on average within a municipality. Not only is the overall registered area increasing, but these properties also cut the trees down more rapidly. Surprisingly, the largest properties that pay the most for their title deforest, but at a lesser pace, following registrations. A 1 p.p. increase in the area registered by such

Table 2.4: Effect of Registrations on Cumulative Deforestation in the Amazon

| | Cumulative Area Deforested | | | |
|---|---|---|---|---|
| Variables | (1) | (2) | (3) | (4) |
| Total Area Registered | 0.00122** | | 0.00294*** | |
| | (0.000476) | | (0.000518) | |
| ....by Small Farms | | -0.00162 | | -0.00153 |
| | | (0.00119) | | (0.00119) |
| ....by Medium Farms | | 0.00186 | | 0.00267** |
| | | (0.00132) | | (0.00131) |
| ....by Large Farms | | 0.00134* | | 0.00156* |
| | | (0.000786) | | (0.000794) |
| Population Estimates | 0.0283*** | 0.0283*** | 0.0174 | 0.0190 |
| | (0.0105) | (0.0105) | (0.0123) | (0.0125) |
| Investment Credit Value | 0.000298** | 0.000299** | 0.000348** | 0.000369** |
| | (0.000124) | (0.000124) | (0.000142) | (0.000143) |
| Observations | 4,629 | 4,629 | 2,251 | 2,251 |
| R-squared | 0.260 | 0.263 | 0.342 | 0.347 |
| Municipal FE | YES | YES | YES | YES |
| Year FE | YES | YES | YES | YES |
| Agriculture Controls | YES | YES | YES | YES |
| Eligible for Rollout | | | YES | YES |

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors clustered at the municipal level in parenthesis. Sample consists of 790 municipalities in the Brazilian Amazon from 2006-2012. All variables are inverse hyperbolic sine transformed. In addition, 'Area' variables are stock variables. Columns (1, 3) estimate the effect of total registered area, while (2, 4) decompose the effect by size. Registered properties sized less than 1 *mf* are "Small" farms, between 1-4 *mf*—"Medium", and greater than 4 *mf*— "Large". Columns (1,2) consider the entire sample, while columns (3,4) restrict the sample only to the municipalities that are eligible for rollout. Agricultural controls include the total area cultivated, as well as investment credit financing. We additionally control for annual nonagricultural GDP and population in a municipality, in addition to municipal fixed effects and year fixed effects, in all regressions. Registrations data provided by *Terra Legal*; deforestation data from the PRODES project.

farms *decreases* the rate of deforestation by approximately 4% on average within a municipality. Though the overall area deforested increases, the decrease in the rate suggests that these agents will stop deforestation sooner than their medium-farm counterparts.

These results further support our theory that the medium-sized properties are small enough to skirt the law, but large enough to expand their production. Though large and medium farms engage in deforestation, only the medium-sized properties increase their intensity. We now turn our attention to the crop choice question to see which activities take place on the newly deforested land.

### 2.6.3 Crop Cultivation

Given small farms' increased access to credit and the rothers' land clearing activities, we expect agents to change their crop choice decisions. More security following property rights establishment, as well as increased credit, allows agents to invest in their land and cultivate more crops overall, or cultivate more lucrative permanent crops. Evidence shows that the overall registration effect is positive on permanent crop cultivation and negative on temporary crop cultivation, with the largest farms increasing cash crop cultivation, particularly of cocoa.

Tables 2.6 and 2.7 present the overall registration effect and the scale effect, respectively. Surprisingly, the overall registration effect on total cultivation is negative; a 1 p.p. increase the total area registered decrease the overall crop cultivation within a municipality by 0.5% on average; given that the overall registration effect on area deforested was positive, this suggests an alternative activity, such as cattle grazing, could be in play. In line with our intuition, temporary crop cultivation decreases

Table 2.5: Effect of Registrations on the Deforestation Rate in the Amazon

| | Rate of Deforestation | | | |
|---|---|---|---|---|
| Variables | (1) | (2) | (3) | (4) |
| Area Registered | -0.0160** | | 0.00548 | |
| | (0.00760) | | (0.0114) | |
| ....by Small Farms | | -0.00941 | | -0.00327 |
| | | (0.0239) | | (0.0246) |
| ....by Medium Farms | | 0.0342 | | 0.0440* |
| | | (0.0245) | | (0.0247) |
| .....by Large Farms | | -0.0410*** | | -0.0375** |
| | | (0.0152) | | (0.0155) |
| Observations | 4,603 | 4,603 | 2,243 | 2,243 |
| R-squared | 0.101 | 0.102 | 0.128 | 0.133 |
| Municipal FE | YES | YES | YES | YES |
| Year FE | YES | YES | YES | YES |
| Agriculture Controls | YES | YES | YES | YES |
| Eligible for Rollout | | | YES | YES |

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors clustered at the municipal level in parenthesis. Sample consists of 790 municipalities in the Brazilian Amazon from 2006-2012. All variables are inverse hyberbolic sine transformed. ‘Deforestation rate’ is the annual change in total area deforested. Columns (1, 3) estimate the effect of total registered area, while (2, 4) decompose the effect by size. Registered properties sized less than 1 *mf* are “Small” farms, between 1-4 *mf*—“Medium”, and greater than 4 *mf*— “Large”. Columns (1,2) consider the entire sample, while columns (3,4) restrict the sample only to the municipalities that are eligible for rollout. Agricultural controls include the total area cultivated, as well as investment credit financing. We additionally control for annual nonagricultural GDP and population in a municipality, in addition to municipal fixed effects and year fixed effects, in all regressions. Registrations data provided by *Terra Legal*; deforestation data from the PRODES project.

by 0.6%, while permanent crop cultivation increases by 1%. Thus, agents do indeed respond to the increased land security by engaging in more long-term production.

In addition, we consider the outcome variables 'Cash Crops'—cocoa, tobacco and soy— and 'Cocoa' cultivation. Squatters, who might have previously shied away from planting these more lucrative crops due to fears of appropriation, might now choose to engage in such activities as new land owners. Indeed, a 1 p.p increase in the overall area registered increases cash crop cultivation by 1.8%, and particularly of cocoa by 1.6%.

Table 2.6: The Effect of Overall Area Registered on Crop Cultivation

| | Area Cultivated | | | | |
|---|---|---|---|---|---|
| | | Temporary | Permanent | Cash | |
| | Total | Crops | Crops | Crops | Cocoa |
| VARIABLES | (1) | (2) | (3) | (4) | (5) |
| Total Area Registered | -0.00501* | -0.00582** | 0.0108** | 0.0177** | 0.0158*** |
| | (0.00289) | (0.00294) | (0.00447) | (0.00827) | (0.00597) |
| Non-agricultural GDP | 0.145** | 0.136** | -0.0640 | 0.118 | -0.203* |
| | (0.0690) | (0.0690) | (0.157) | (0.215) | (0.114) |
| Observations | 3,193 | 3,193 | 3,193 | 3,193 | 3,193 |
| R-squared | 0.028 | 0.027 | 0.013 | 0.007 | 0.028 |
| Municipal FE | YES | YES | YES | YES | YES |
| Year FE | YES | YES | YES | YES | YES |

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors clustered at the municipal level in parenthesis. Sample consists of 790 municipalities in the Brazilian Amazon from 2006-2012. All variables are inverse hyberbolic sine transformed. Column (1) is the effect of registrations on total area cultivated, which is divided into its two subsamples in Columns (2,3): area cultivated with temporary and permanent crops. Column (4) limits the sample further to just the area cultivated by "cash" crops, which include cocoa, tobacco and soy; Column (5) further limits the sample to just cocoa cultivation. We control for annual nonagricultural GDP and population in a municipality, in addition to municipal fixed effects and year fixed effects, in all regressions.

The breakdown of the overall effect by size shows the cash crop effect is driven by the largest properties that pay the most for the land. A 1 p.p increase in the area registered by such farms leads to a 4 % increase in the area cultivated with cash crops on average within a municipality; cocoa cultivation alone increases by 2.3%.

In addition, we find that both small and large farms decrease temporary crop cultivation, thought the coefficient is only significant for small farms: a 1 p.p increase in the area registered by small farms decreases the area cultivated with temporary crops by .9%. Combined with the deforestation results presented above, evidence suggests that on average, the smallest farms, unwilling to lose their provisional title for failing to maintain forests on 80% of their land, reduce deforestation slightly along with overall crop cultivation by cultivating less temporary crops. Indeed, if temporary crops were less profitable, but only served the purpose of allowing the squatter to maintain his claim on the land, then removing the threat of appropriation would result in the farmer pursuing more profitable crop choices. The statistical insignificance of permanent crops does not allow us to conclude that they maintain or increase their permanent crop cultivation; it is also possible that agents reduce the total area cultivated to make room for cattle ranching or other livestock production.

Surprisingly, medium-sized farms appear to decrease cocoa cultivation, but we are at a loss to explain why. Given that this is the only category for which the medium-farm coefficient is significant, we suspect that such properties engage in other land activities, such as cattle grazing. Evidence from my first chapter indicates that this is cattle ranching, given that exports of animal-based products increase in response to an increase in medium-farm registrations.

Table 2.7: Registered Area and Crop Cultivation: Scale Effects

| | Area Cultivated | | | | |
|---|---|---|---|---|---|
| | | Temporary | Permanent | Cash | |
| | Total | Crops | Crops | Crops | Cocoa |
| VARIABLES | (1) | (2) | (3) | (4) | (5) |
| Area Registered | | | | | |
| .....by Small Farms | -0.00685 | -0.00896* | 0.000304 | -0.0106 | 0.0190 |
| | (0.00504) | (0.00515) | (0.00940) | (0.0262) | (0.0115) |
| .....by Medium Farms | -0.00473 | -0.00380 | 0.0160 | -0.00455 | -0.0190* |
| | (0.00603) | (0.00617) | (0.0114) | (0.0237) | (0.0103) |
| .....by Large Farms | 0.00792 | 0.00740 | -0.00360 | 0.0410*** | 0.0227** |
| | (0.00546) | (0.00554) | (0.00983) | (0.0120) | (0.00956) |
| Non-agricultural GDP | 0.151** | 0.141** | -0.0678 | 0.130 | -0.197* |
| | (0.0691) | (0.0691) | (0.157) | (0.214) | (0.113) |
| | | | | | |
| Observations | 3,193 | 3,193 | 3,193 | 3,193 | 3,193 |
| R-squared | 0.028 | 0.028 | 0.014 | 0.011 | 0.031 |
| Municipal FE | YES | YES | YES | YES | YES |
| Year FE | YES | YES | YES | YES | YES |

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors clustered at the municipal level in parenthesis. Sample consists of 790 municipalities in the Brazilian Amazon from 2006-2012. All variables are inverse hyberbolic sine transformed. Registered properties sized less than 1 *mf* are "Small" farms, between 1-4 *mf*—"Medium", and greater than 4 *mf*— "Large". Column (1) is the effect of registrations on total area cultivated, which is divided into its two subsamples in Columns (2,3): area cultivated with temporary and permanent crops. Column (4) limits the sample further to just the area cultivated by "cash" crops, which include cocoa, tobacco and soy; Column (5) further limits the sample to just cocoa cultivation. We control for annual nonagricultural GDP and population in a municipality, in addition to municipal fixed effects and year fixed effects, in all regressions.

## 2.7 Robustness Checks

Since we use a difference-in-differences estimation strategy, our coefficients are valid only if both the treated (municipalities with high registrations) and control (municipalities with low registrations) groups follow the same trend in the outcome variable. A parallel tread pre-rollout allows us to attribute the changes that occur after the property rights intervention can be attributed *to* the intervention. To test this assumption, we plot the municipal cultivation and deforestation from 2000 until 2008, eight years before the implementation of the programme. We divide the municipalities into high versus low registrations based on the median share of registered area; those with above-median share of registered area are the treated group and those with below-median share of registered area are the "control" group.

As the Figure 2.2 shows, the trends before rollout are more or less parallel. In-

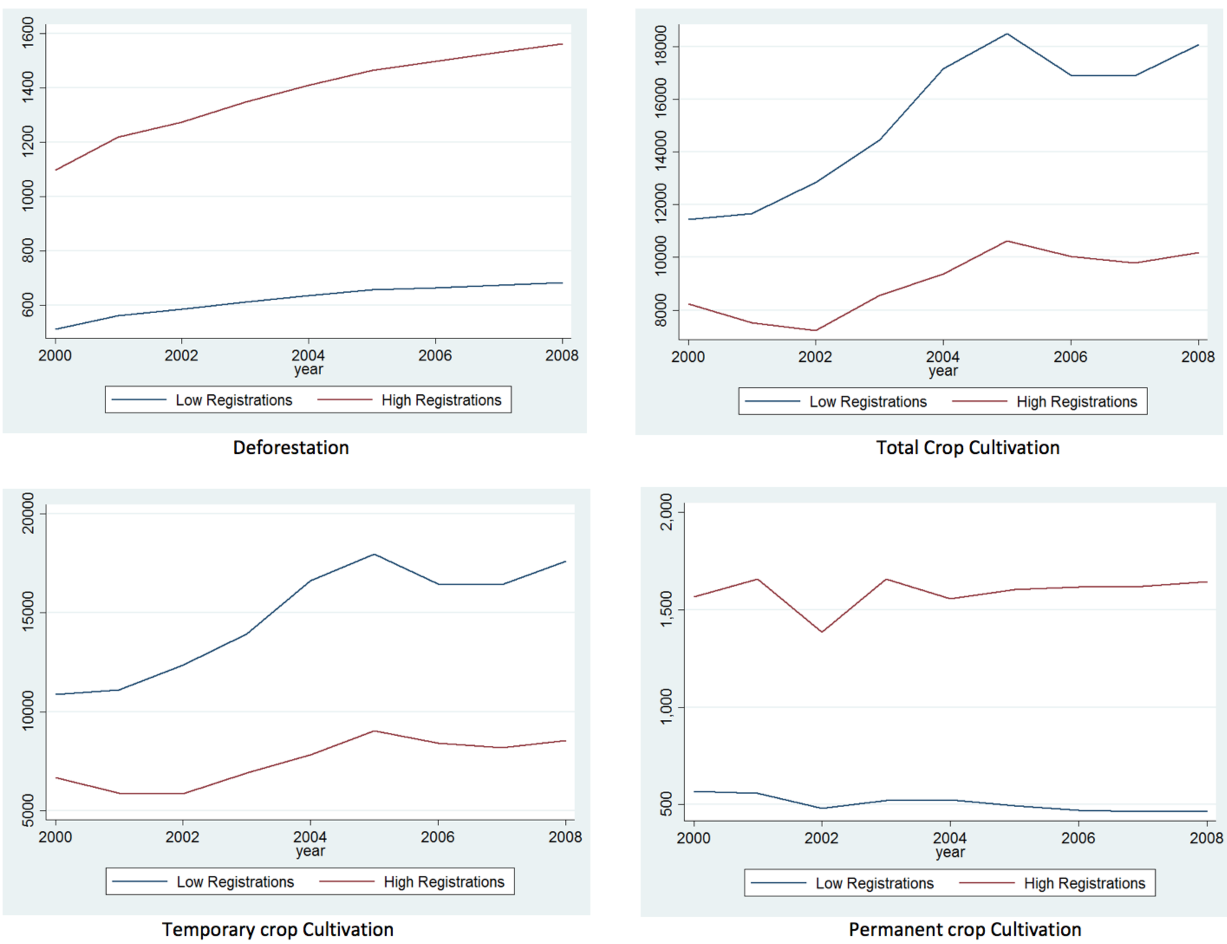


Figure 2.2: Pre-trends in Crop Cultivation and Deforestation before Rollout (2000-2008)

terestingly, the municipalities with registrations on the lower half of the spectrum have both higher total cultivation and temporary crop cultivation compared to the municipalities on the other end of the spectrum. The trend for permanent crops are parallel after 2006, still 3 years before the titling started. Our sample only considers the year 2006-2012, and should therefore not pose any issues.

Given the validity of the parallel-trend assumption, we conclude with confidence that the policy effect is not confounded by other factors. As a result of registrations, total crop cultivation decreases, while access to credit and deforestation increases.

## 2.8 Conclusion

We analyze the impact of Brazil's latest titling scheme *Terra Legal* through the credit channel. We first show that the accessibility to rural credit, particularly for agricultural investment purposes, increases in response to the overall area registered; moreover, the smallest properties that obtain the land for free are the main beneficiaries of this credit. In addition, we show that medium-farm effect of registration on the area deforested, as well as the rate of deforestation, is strongly positive, at least in the short-run; though the large properties also engage in deforestation, they do so to a lesser extent. Finally, we explore what agents do with their land following property rights establishment. Removing the threat of expropriation, along with increased access to credit, results in an increase in the area cultivated with permanent crops, as well as cash crops, specifically cocoa. The largest properties that pay the most for their land drive the overall effect.

The improvements in accessibility to credit and more profitable cultivation choices are assuredly welfare-improving for Brazilian farmers, but come at the cost of forest loss. Morever, our findings show that the size of the property, how much agents pay for the title, as well as the ease of obtaining the rights, play crucial roles in influencing the agents' behaviour. Policy-makers must give careful considerations to each of these if their goal is also to reduce deforestation.

However, these findings should not serve to discourage property rights implementation, given that we analyze only the short-term adjustments. The theoretical model presented in Chapter 1 shows that even if deforestation increases in the short term, more land will be allocated to forests in the long run steady state. It would be worthwhile revisiting these properties at a later date and study the long term behavior. But in the meanwhile, if we wish to reduce the accelerating forest loss of the Amazon, property rights alone is not the panacea. Targeting ways in which to reenumerate agents for the global service that they provide with standing forests is also required to effect change.

# Chapter 3

# Asymmetric Effects of Crises on Urban v. Rural Areas in Latin America: A Study Using Nightlights

## 3.1 Introduction

With the key finding that the intensity of lights observed from space proxies real income quite handily (Henderson et al., 2012), nightlights are illuminating economic research in exciting ways. I demonstrate one application of nightlights in this paper: using this proxy, I test how shocks propagate through the population of an economy. Though international macroeconomists have extensively studied the effect of shocks on various indicators using many sophisticated methods, they were commonly limited by data. Empirical analysis is typically performed at the aggregate level because macroeconomic variables are collected only at the national level in most countries. Moreover, it is well known that not all countries, particularly those in emerging markets, report accurate or reliable GDP data, which could confound results. Because of these considerations, most of the work in this field has focused on developed markets, still at the aggregate level (national or sub-national). However, with the advent of nightlights, studies on the effects of crises need not be constrained by data any longer, at least for real income. I go beyond the aggregate and consider the effect of crises in urban versus rural areas in one particular emerging market: Latin America.

The idea behind using nightlights as a proxy for economic activity stems from the fact that almost all activity at night requires the use of light. The level of income influences the decision to consume light. In many developing parts of the world, consumption of lights eats up a significant portion of household income and is even considered a luxury. Thus, during difficult times a family might quite reasonably choose to consume less light (and thereby curtail its nighttime production/consumption activities) because they are unable to afford it. However, most households in advanced economies rarely choose to curb their consumption of lights or nighttime activities in response to a crisis. This limited response makes developed markets more difficult to analyze using nightlights. For this reason, Latin America is ideally suited for my analysis because this region has experienced different types of crises between 1992-2010, and it consists of a good mix of rural and urban areas; moreover, it retains the sensitivity of lights to shocks like crises.

The asymmetric effect of shocks within a country is not a new concept. Economic activity does not uniformly decline in a country following a crisis, just as unemployment levels do not uniformly increase across a country during a recession. Similarly, fiscal and monetary policy tools also do not stimulate an economy evenly; some regions or sectors bounce back much more rapidly than others in response to policy stimuli. Moreover, not all shocks affect macroeconomic indicators in the same manner; global systemic crises are particularly debilitating to an economy, as evidenced by the Great Recession of the recent past. Therefore, it is logical to question how and why certain types of crises affect different regions more than others.

Evidence from my paper can add to the small literature on intranational macroeconomics, most of which looks at states within an advanced economy. Research shows that there is substantial home bias even within a country, and stock/bond portfolio allocation is biased geographically (Hess and van Wincoop, 2000a) Moreover, risk-sharing, though greater within a country, is still imperfect, and the level of imperfection depends upon the effectiveness of the financial system in place (Hess and van Wincoop, 2000b). In this paper, I look at a country at an even more granular extent, and I also concentrate on emerging markets.

Geographic bias might possibly penetrate even deeper than the state-level; companies might be more likely to invest in other companies in their city or perhaps in other cities nearby and avoid rural areas altogether due to lack of familiarity. This would lead to inefficient shock propagation between urban and rural areas throughout the country even if states in that country co-move in a similar manner. Moreover, risk-sharing could be even more imperfect between urban and rural areas, since financial markets are not well developed outside of cities and towns. If I find that there is indeed significant deviation in the effect of crises on the growth of lights, this would support the hypothesis that risk sharing is imperfect.

Different types of shocks might not propagate evenly across variously populated areas due to additional reasons. Primarily, urban and rural areas have some fundamental differences; they differ in the type of economic activities they engage in, income levels, consumption patterns as well as the development of the financial sector. Rural areas, particularly in developing countries, typically engage more in agricultural activities, consume less “luxury” goods, might not have a formal or extensive banking system and incomplete insurance markets, though informal risk-sharing networks might be in place. Incomplete insurance markets and difficulties in accessing credit might lead to a drop in agricultural productivity and income following a shock if farmers are forced to sell productive assets in order to consumption smooth (Rosenzweig and Wolpi, 1993).

If rural villagers are distrustful of banking systems and hold wealth in real terms rather than monetarily, a sovereign default that causes the devaluation of monetized wealth might not affect them as much as their urban counterparts. Similarly, currency crises might also not have a huge impact on villagers because they are more reliant on real economy to handle transactions, i.e. they might engage in actual exchange of goods for other goods. At the same time, if a country exports manufacturers, the boost in exports might help urbanites offset the price hike of imports. However, a fall in demand following a debt default episode might have a bigger impact on villages, leading to a bigger drop in rural income. If a debt restructuring leads to austerity measures that affect agricultural subsidies, rural areas would feel the ill effects more keenly. Evidence shows that they are also less likely to insure against other idiosyncratic risks if their income is lower (Townsend, 1994), which might lead to a further decline in their income.

Comparatively, urban areas are more involved in manufacturing or services sector, have a more developed financial system and likely easier access to credit. This suggests that a systemic banking crisis could affect urbanites more than their rural counterparts. However, if urbanites are better at risk management and take advantage of formal insurance markets, they should be able to smooth income and consumption more effectively. From these examples, one can see how different types of crises could potentially affect urban and rural areas asymmetrically and why a formal analysis is warranted to shed light on this matter.

To carry out my analysis, I use satellite data for nightlight luminosity and population density; each observation is a five square kilometre pixel that has a light intensity value and a population count. I estimate the effect using grid-level fixed effects and cluster the standard errors at the country-level. Given the low number of clusters in my sample, this requires asymptotic refinement: the pairs cluster bootstrap-t procedure, as laid out by Cameron et al. (2008). Evidence shows that shocks do indeed propagate asymmetrically in urban versus rural areas; the growth of lights in rural Latin America decline with the advent of currency crises and banking crises; in real terms, the decline translates into a 0.5% - 1% decline in real GDP.

## 3.2 Literature Review

This paper mainly contributes to two distinct strands of economic research: (i) the asymmetric effect of shocks and regional real business cycles (RBCs) and (ii) the relatively novel use of nightlights as a proxy for economic growth. The asymmetric diffusion of shocks within an economy dates back to Burns and Mitchell (1946), but the topic experienced a surge of interest in the 1990s, when the European Community was poised to form a currency union. Fears abounded that replacing individual exchange rate stabilization with a single, European monetary policy would eliminate a key tool that helped smooth business cycles across Europe. Thus, most of the research during this time on regional business cycles and intranational macroeconomics focused on states within advanced economies. However, to my knowledge, the asymmetric effect of shocks in urban versus rural areas has not been explored heretofore, especially in developing countries, mainly due to the lack of reliable real income data at such a granular level. With nightlights, I am able to overcome this problem and look beyond administrative borders to study how shocks impact urban and rural areas.

Another topic studied intranationally is the existence of perfect risk-sharing, i.e. whether or not people smooth income by insuring fully against risk. Mace (1991) finds that consumption levels and growth rates of households in the U.S. are determined by their associated average consumption, i.e. they are independent of income levels; she attributes this to complete financial markets and full insurance—perfect risk-sharing. Asdrubali et al. (1996) similarly finds that about 75% of output shocks are smoothed across U.S. states via capital/credit markets or transfers from the federal government. However, Townsend (1994) rejects this hypothesis for India and finds that people do not insure fully against risk, though the overall effect of income on consumption is not great. Using regional consumption and income data, in Hess and van Wincoop (2000b), Crucini and Hess find that there is greater risk-sharing within a country than across, though it is still imperfect. Using sales of non-durables within the U.S., two studies—Negro (2002) and Hess. and Shin (1998)— show that consumption is less correlated across states than output, which is further evidence of imperfect risk-sharing.

One reason for imperfect risk-sharing is the existence of home bias in portfolio allocation; i.e. people tend to hold significantly more stocks and bonds from their region; evidence suggests that there is indeed substantial home bias within a country, i.e. people hold significantly more stocks and bonds from their region. In Hess and van Wincoop (2000a), Huberman finds that money market funds invest more in local firms and that consumers hold more shares in their local Regional Bell Operating Companies in the U.S.. This geographic bias in portfolio allocations implies that capital markets are not integrated fully even within an advanced economy; given that a significant portion of this risk-sharing is achieved through financial markets (45% to 60%), risk-sharing might be even more imperfect in a developing country, where capital markets are further disintegrated and financial markets are not extensively developed, particularly in rural areas.

Evidence from my paper can add to this literature because it considers emerging markets at an even more granular extent. The so-called home bias in portfolio allocation might possibly penetrate even deeper than the state level, leading to asymmetric business cycles between urban and rural areas, particularly if transfers between urban and rural areas are practically nonexistent. There is also evidence to suggest that risk-sharing might differ between urban and rural areas, particularly in developing countries. Using data from rural India, Rosenzweig and Wolpi (1993) show that incomplete insurance markets and difficulties in accessing credit might lead to a drop in agricultural productivity and income following a shock if farmers are forced to sell productive assets in order to consumption smooth. Townsend (1994) finds that people in rural India are less likely to insure against idiosyncratic risks if their income is lower, which might lead to a further decline in their income. Therefore, extending the study of asymmetric shocks in urban and rural areas in emerging markets is the logical next step, and I am able to perform this analysis using nightlights as a proxy for income.

The second strand of literature I contribute to is the work on nightlights. The usefulness of nightlights in capturing human economic activity has been documented by researchers in different fields. Imhoff et al. (1997) show how satellite images of nighttime lights can be used to pinpoint human settlements globally. Doll et al. (2006) find a strong linear relationship between lights and nominal GDP, using the 1996 nightlights data of eleven European countries. Sutton et al. (2007) extends the study to US, India, China and Turkey and finds a similar relationship between lights and nominal GDP using data the year 2000. Others have established this result for Mexico and India, once gain using a cross-section of one year (Ghosh et al., 2009), (Bhandari and Roychowdhury, 2011).

While the previous papers have strongly attested that lights might capture economic growth, Henderson et al. (2012) explicitly show that lights can be used as a proxy for real income growth, using an unbalanced panel of 188 countries over 17 years. They demonstrate that estimates of growth are more accurate when augmenting the usual GDP measures with lights, particularly for countries with poor GDP data. They further show that lights can be used to predict income growth where such data is unavailable or is unreliable. They calculate the best fit elasticity of measured GDP growth with respect to lights growth to be roughly 0.3; this means that if, for example, lights grew by 0.131 log points in some region over a year, this translates into about a 4.2% growth in real GDP. These results suggest that nightlights can be used to answer questions that require detailed income data, not only nationally, but also within countries, because the lights data is available at a granular level (per every km2) for almost every region in the world.

Chen and Nordhaus (2011) answer whether the relationship between lights and income growth is indeed still preserved at a more disaggregate level, using the G-Econ database from Yale University. This dataset is a compilation of annual global output information, available at 1 x 1 degree cells, while the lights are available annually at 30-arc second pixels; therefore, each cell in the G-Econ data contains 120 x 120 light pixels. The authors argue that using lights as a proxy for income produces an error of at least 25% at this level; nevertheless, they still conclude that luminosity is a good proxy, particularly for countries of low data quality. However, one should take their results with a grain of salt because the G-Econ data has its own set of measurement errors, and furthermore, it only measures output. The lights data captures not only production, but also consumption; thus, the two are not fully comparable.

Other studies have also attempted to characterize the relationship between lights and income at a more granular level, but each of these studies has its own limitations. Bhandari and Roychowdhury (2011) test whether lights are a valid proxy for income using district-level GDP in India. They find lights underestimate income in cities and overestimates in regions predominantly devoted to agriculture and forestry; thus, lights are unable to accurately capture economic activity at very high or very low population densities. However, as pointed out earlier, they only study levels and not growth, using data from the year 2008 only. Kulkarni et al. (2011) explore the suitability of the lights proxy at the subnational level in the U.S., India and China during the period 2001 to 2007; they use data at the county-, district- and prefecture-level for the three countries for the years 2001, 2004 and 2007. They find that lights can only be used as a proxy for 25% of the counties in the U.S., 20% of the districts in India, and about 10% of the prefectures in China. However, they use a distinct, bi-directional, bi-variate spatial analysis to identify particular areas where lights can be used a proxy for measuring sub-regional economic growth; this particular study is exploratory in nature and statistically robust.

Finally, studies have also showed that lights do respond to crises and conflicts. Henderson et al. (2011) illustrate merely using images the impact of the Rwandan Genocide on economic activity in Rwanda and impact of the Asian financial crisis in Indonesia. Agenw (2008) use lights to evaluate whether the U.S. was indeed successfully in stabilizing Iraq and rebuilding the infrastructure following the invasion in 2007. Shortland et al. (2013) similarly use lights to analyze the impact of war in

Somalia for different social groups, using a panel of 14 cities over 6 to 14 years. These results suggest that lights might also similarly respond to economic crises, such as a systemic banking crisis or a sovereign debt default.

Though some of the results are conflicting, a consensus has nevertheless emerged that lights can indeed be used as a proxy for income, particularly if one focuses on growth rather than levels. This is indeed what I study for urban versus rural areas. Additionally, the changes in lights under-predict economic growth/decline in very high-density areas and very low-density areas due to top-coding/oversaturation and the sensor's inability to pick up faint light emissions. Therefore, one should be cautious in drawing conclusions for these regions.

## 3.3 Data

In order to perform my analysis, I need data on nightlights, population density and crises dates. The nightlights provide a measure of economic activity and allow me to construct my key variable of interest in this paper. The population density information allows me to separate grids into urban and rural categories. The crises serve as the exogenous variables in my estimation.

### 3.3.1 Nightlights

The National Geophysical Data Centre (NGDC) provides the data for the visible lights that emanate from Earth at night, the so-called nightlights. U.S. Air Force weather satellites capture daily images of every location on Earth between 20:30 and 22:00 hours local time. These images then go through an extensive data-cleaning process; the NGDC removes images that capture auroral activity, forest fires, the bright half of the lunar cycle, cloud coverage and long summer days, leaving predominantly "man-made" lights. The resulting average yearly composite of these images comprises the average stable lights data set for the years 1992-2010. Figure 3.1 shows a snapshot of a Brazil and surrounding countries from the 1992 annual composite.

The satellites pick up outdoor and some indoor use of electric light; so, light emanating from kerosene lamps, for instance, would probably not be sensed by the satellites. Then, to some extent, I undercount lights, especially in low-income, low-density areas. Also, the satellites are periodically replaced due to aging; between 1992 and 2010, six different satellites were used to capture images. This implies that the sensory of the satellites vary across satellites and over time. However, I use year fixed-effects to address this issue.

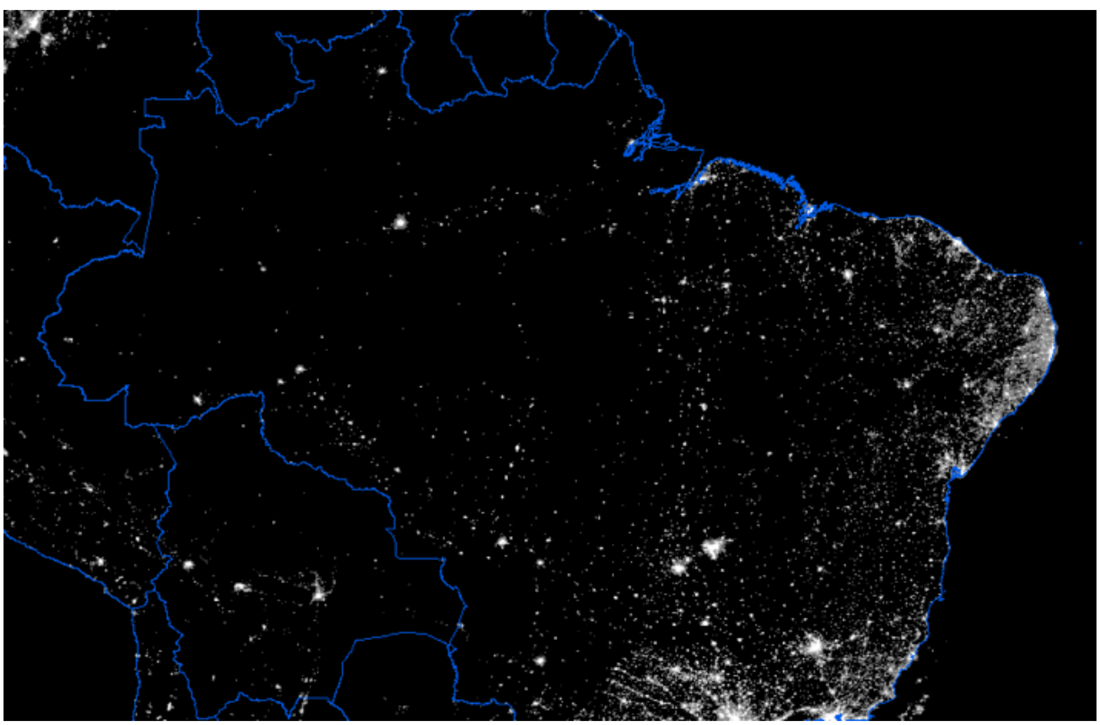

Figure 3.1: A Snapshot of Nightlights in and around Brazil in 1992

A data point measures the intensity of the nightlights in a grid of length 30 arc seconds (approximately 0.86 square kilometres at the equator); this intensity is reported by a digital number between 0 and 63, with the former detecting no light and the latter capturing the greatest intensity, top-coded. The luminosity digital number reported is not exactly proportion to "true radiance" or physical amount of light received by the satellites due to sensor saturation and a scaling factor performed in the data-cleaning process. However, Henderson et al. (2012) perform an experiment for one year that shows that the difference between the two is minor and does not affect results. Roughly 0.1% of the data points are censored at 63, but the problem is predominantly in densely populated rich countries like the Netherlands, where 1.58% of the points are top-coded. Nevertheless, the censoring could be a problem in measuring growth in densely populated urban areas.

This paper uses the annual average stable nightlights from Latin America and the Caribbean. I re-project the data to equal area using ArcGIS to remove distortion from the curvature of the earth; this ensures that a pixel in northern Brazil, which has the equator running through it, has the same area as a pixel in the southern tip of Chile. Then, I extract the mean luminosity for the region per every five square kilometres. The five square kilometre pixels that show no visible light throughout the entire period are dropped because there is no variation in economic activity.

The sample consists of 2,171,374 pixel-year observations with luminosity between 0 and 63 inclusive, of which 917,265 (roughly 42% of the sample) have no visible light. On the other end, 15,793are top-coded at 63, roughly 0.73% of the sample. Approximately 38% of the sample—830,467 1,951,252 pixel-year observations to be exact—have a strictly positive luminosity number of less than 10.

### 3.3.2 Population

The population data comes from the Global Rural-Urban Mapping Project, version 1 (GRUMPv1). The data is available globally per 30 arc second grids (the same unit of area as the nightlights data) and assigns a count of persons per grid; values range from 0 to 9870. Before extracting the data per five square kilometres, I similarly re-project the data to equal area as with the nightlights to remove distortion due to the curvature of the earth. The data are available for years 1990, 1995 and 2000, but I use only the population count from 1990 to separate the grids into bins based on population cutoffs.

In order to analyze the asymmetric effects of the crises on urban versus rural population, I separate the pixels into four separate bins depending upon the population count observed in the year 1990. The "rural" bin consists of pixels with population count greater than 0 and less than 10 people inclusive per five square kilometres; this category contains 126,984 pixel-year observations, 5.84% of the sample. The next "semi-rural" bin consists of pixels with greater than 10 people but less than 100 people per five square kilometres; there are 441,222 pixel-year observations in this category, which is 20.3% of the sample. The middle "town" bin, which has 625,298 pixel-year observations, consists of pixels with greater than 100 people but less than 500; this bin comprises 28.8% of the sample. The fourth and final "urban" bin comprises of pixels greater than 500 people per square kilometres; there are 977,870 pixel-year observations in this bin (about 45% of the sample). Table 3.1 below summarizes the breakdown.

Table 3.1: Sample Breakdown by Population Category

| Number of people (per 5 sqkm) | Rural $0 < n \leq 10$ | Semi-Rural $10 < n \leq 100$ | Town $100 < n \leq 500$ | Urban $n > 500$ |
|---|---|---|---|---|
| Observations | 126,984 | 441,222 | 625,298 | 977,870 |
| Percentage of Sample | (5.84%) | (20.3%) | (28.8%) | (45%) |

### 3.3.3 Crises Identifiers

This paper considers five different types of crises: a systemic banking crisis, a currency crisis, a sovereign debt default and/or restructuring, each of which come from the widely used Laeven and Valencia database (2008, 2012), and twin and triple crises. Laeven and Valencia (2008) define a systemic banking crisis as one where:

> ...[A] country's corporate and financial sectors experience a large number of defaults and financial institutions and corporations face great difficulties repaying contracts on time. As a result, non-performing loans increase sharply and all or most of the aggregate banking system capital is exhausted. This situation may be accompanied by depressed asset prices (such as equity and real estate prices) on the heels of run-ups before the crisis, sharp increases in real interest rates, and a slowdown or reversal in capital flows.

A currency crisis is defined according to Laeven and Valencia (2008) as "a nominal depreciation of the currency of at least 30 percent that is also at least a 10 percent increase in the rate of depreciation compared to the year before." In Laeven and Valencia (2012) database, a country experiences a sovereign debt default episode the moment it defaults to private lending; a sovereign debt restructuring occurs the year the debt is rescheduled. Both of these are pooled due to identification issues that does not allow for separate estimation of their effects.

Table 3.2 provides a history of crises in Latin American and the Caribbean between 1992 and 2010. In the sample, there are 16 systemic banking crises, 15 currency crises, and 24 defaults/debt-restructuring in Latin America and the Caribbean. Chile is only country not to experience any crises during the sample period.

Finally, I define a twin crisis as one where two of the three crises described above occur in the same year or in consecutive years. For example, a currency crisis in 1992 and sovereign default in 1993 in a country is considered to be a twin crisis episode only in 1993, as per convention. There are 6 such outcomes in Latin America during this time period.

Table 3.2: Summary of Crises by Country in Latin America and the Caribbean (1992 - 2010)

| Country | Banking Crises | Currency Crises | S. Debt Default | Twin | Total |
|---|---|---|---|---|---|
| Argentina | 2 | 1 | 2 | 0 | 5 |
| Bolivia | 1 | 0 | 1 | 0 | 2 |
| Brazil | 1 | 2 | 1 | 1 | 5 |
| Chile | 0 | 0 | 0 | 0 | 0 |
| Colombia | 1 | 0 | 0 | 0 | 1 |
| Costa Rica | 1 | 0 | 0 | 1 | 2 |
| Dominica | 0 | 0 | 1 | 0 | 1 |
| Dom. Republic | 1 | 1 | 2 | 0 | 4 |
| Ecuador | 1 | 1 | 5 | 0 | 7 |
| Grenada | 0 | 0 | 2 | 0 | 2 |
| Guyana | 1 | 0 | 1 | 0 | 2 |
| Haiti | 1 | 2 | 0 | 0 | 3 |
| Honduras | 0 | 0 | 1 | 0 | 1 |
| Jamaica | 1 | 0 | 2 | 1 | 3 |
| Mexico | 1 | 1 | 0 | 1 | 2 |
| Nicaragua | 1 | 0 | 1 | 1 | 2 |
| Panama | 0 | 0 | 1 | 0 | 1 |
| Paraguay | 1 | 1 | 1 | 0 | 3 |
| Peru | 0 | 0 | 1 | 0 | 1 |
| Suriname | 0 | 2 | 0 | 0 | 2 |
| Uruguay | 1 | 1 | 2 | 0 | 4 |
| Venezuela | 1 | 3 | 0 | 1 | 4 |
| Total | 16 | 15 | 24 | 6 | 61 |

Note: Data from Laeven and Valencia Database, IMF.

## 3.4 Empirical Strategy: Grid-Level Fixed Effects

Given my lack of controls at such a minute level, I use grid-level fixed effects to eliminate the unobserved heterogeneity across grids. For example, a grid that is located at the border or near ports might experience the shocks differently than one that is in the interior of the a country. In addition, I include year dummies to remove the time trend. I estimate the following equation:

$$Log\_Lum_{ict} = \alpha + \beta_1 Banking_{ct} + \beta_2 Currency_{ct} + \beta_3 Debt_{ct} + \beta_4 Twin_{ct} + \mu_i + \nu_t + \epsilon_{ict} \tag{3.1}$$

where $Log\_Lum_{ict}$ is the log transformation of the average light luminosity in grid $i$ in country $c$ in year $t$. Each of the crises variables is a dummy variable that takes the value 'one' in the year $t$ that the occurs in country $c$. $\mu_i$ controls for unobserved heterogeneity in grid $i$, while $\nu_t$ eliminates the time trend.To estimate the asymmetric effects of the crises, I separate grid observations based on their population density and run the regression above on each subsample.

I log-transform the light luminosity measure because Henderson et al. (2012) show that this measure serves as the best proxy. Naturally, this limits the sample to only the observations that record some light captured by the satellites, Thus, a banking crisis in country $c$ at time $t$ leads to a $100 * [e_1^{\beta} - 1]$ % change in the average light luminosity within a grid.

Given the likelihood that the errors are not independent and identically distributed within countries, I cluster the errors at the country-level. Failing to do so will result in underestimating the true standard error and over-rejection of the null hypothesis. However, because the number of clusters in my sample is small (22), coupled with the fact that the clusters are greatly unbalanced, I cannot employ standard heteroskedastic "sandwich" estimators to account for within-cluster dependence because these estimators rely on asymptotics, i.e. the number of clusters approaching infinity. As Cameron et al. (2008) show, using such estimators when cluster size is small also result in underestimating the standard error and over-rejection of the null.

To address this issue, I use pairs cluster bootstrap $t$-procedure, as described in Cameron et al. (2008) and Cameron and Miller (2015). Typical pairs cluster bootstrapping without asymptotic refinement involves forming $G$ clusters of pairs $\{(y_1^*, X_1^*), ..., (y_G^*, X_G^*)\}$ by resampling with replacement $G$ times from the original sample of *clusters* and computing the estimate of $\beta$; repeating this procedure $B$ times results in parameter estimates $\{\hat{\beta}_1, ..., \hat{\beta}_B\}$. The variance can then be computed from these stored estimates.

However, for a sample with too few clusters, instead of obtaining the variance co-variance matrix, one obtains the Wald test statistic for each bootstrap iteration, tracing out the density of the Wald t-statistic, which is then used for inference purposes in place a standard normal or T distribution.

The procedure starts in the same way as for the typical bootstrap estimator. I start by obtaining $G$ clusters of pairs $\{(y_1^*, X_1^*), ..., (y_G^*, X_G^*)\}$ by replacing with replacement $G$ times from the original sample of clusters and computing the OLS estimate $\hat{\beta}_b$, for the $b$-th bootstrap iteration. Then, I obtain the Wald test statistic $w_b^* = (\hat{\beta}_b^* - \hat{\beta})/se_{\hat{\beta}_b^*}$, where $se_{\hat{\beta}_b^*}$ is the cluster-robust standard error of $\hat{\beta}_b^*$, and $\hat{\beta}$ is the OLS estimate of $\beta$ from the original sample. This is repeated $B$ times; for my estimation, I choose $B = 1200$.

Then, the Wald test-statistics $\{w_1^*, ..., w_B^*\}$ are ordered to form the density function of the Wald t-statistic. So, instead of standard normal values of -1.96 and 1.96, the 95% confidence interval is constructed from the lower 2.5 percentile and upper 97.5 percentile of this ordered set of Wald test-statistics. The $p - value$ is given by the fraction of t-statistics from the bootstrap iterations below the initial t-statistic for the original sample $w$, i.e. $|w| > |w_b^*|, b = 1, ..., B$. Note that the standard errors are not reported with this procedure, but only the confidence interval and the p-values for making inferences.

The STATA user-written program $ClusterBS$ by Andrew Menger carries out this procedure. I use this to obtain my estimates, p-values and confidence intervals.

## 3.5 Results

### 3.5.1 Pairs Cluster Bootstrap Estimates with Asymptotic Refinement

First, I split the sample over two main statistics—the population median and mean—to check whether there are any asymmetric effects of the different crises. The average median population density in Latin America is 51.54 people per 5 sqkm though it varies anywhere from 6.1 people per 5 sqkm in Colombia to 218 people per the same in Ecuador. Meanwhile, the average population density is an entire order of magnitude higher at 540.8 people per 5 sqkm, and it ranges from 197 people per 5 sqkm to 1445 people over the same area.

Table 3.3 presents the results, with a 95% confidence interval in square brackets appearing in place of standard errors. This is due to the pairs cluster bootstrap-$t$

Table 3.3: Effect of Crises on Average Annual Luminosity Growth by Mean and Median Population Density

| | Log(Average Annual Luminosity) | | | | |
|---|---|---|---|---|---|
| Population in 1990 | All | Below Median | Above Median | Below Mean | Above Mean |
| Banking Crisis | -0.0124 | -0.0164* | -0.0090 | -0.0122 | -0.0147 |
| | [-0.390, 0.014] | [-0.038, 0.005] | [-0.043, 0.025] | [-0.038, 0.013] | [-0.059, 0.029] |
| Currency Crisis | -0.0175*** | -0.0244** | -0.0109 | -0.0183** | -0.0142 |
| | [-0.030,-0.005] | [-0.039, -0.010] | [ -0.025, 0.003] | [-0.032, -0.004] | [-0.040, 0.012] |
| Sovereign Debt Default | 0.0194 | 0.0073 | 0.0327 | 0.0197 | 0.0186 |
| | [-0.026, 0.065] | [-0.026, 0.041] | [-0.044, 0.109] | [-0.031, 0.070] | [-0.031, 0.068] |
| Twin Crises | 0.0069 | 0.0023 | 0.0114 | 0.0039 | 0.0227 |
| | [-0.007, 0.021] | [-0.017, 0.021] | [-0.006, 0.028] | [-0.009, 0.017] | [-0.023, 0.068] |
| Observations | 1,214,051 | 612,113 | 601,938 | 1,023,071 | 190,980 |
| R-squared | 0.426 | 0.449 | 0.408 | 0.452 | 0.307 |
| Grid FE | YES | YES | YES | YES | YES |
| Year FE | YES | YES | YES | YES | YES |

*** p <0.01, ** p <0.05, * p <0.1

Note: 95% CI in brackets. Robust standard errors clustered at the country level with asymptotic refinement using pairs cluster bootstrap t-procedure with 1200 repetitions due to too few clusters. Sample consists of 22 countries in Latin America and the Caribbean from 1992-2010, broken up into 5 sqkm grids. Only grids with positive light luminosity measures are included. Median and mean population density is calculated by country in the year 1990. Crises Dummies from Laeven-Valencia Database, IMF.

procedure I use to cluster the errors, which results in a non-normal distribution.

Column 1 reports the effect of the crises for the entire sample. Only the 'Currency Crisis' coefficient is significant, at the 1% level. The advent of a currency crisis decreases the growth of lights on average by roughly 1.73% within a 5 sqkm pixel in a given year; the impact could be as small as a mere 0.5% or as large as a 3% decrease in lights growth, as suggested by the 95% confidence intervals. Using the Henderson et al. (2012) best-fit elasticity between lights growth and real GDP growth of 0.31, the 1.73% average decline in lights growth translates to a 0.56% decline in real income growth within a pixel.

After separating the sample by population, I find that the impact of the crises is mainly borne by the less-populated regions. While the coefficient on banking crisis was insignificant for the entire sample, it becomes significant at the 10% for the subsample of pixels with population density below the median. A banking crisis decreases the growth of lights by roughly 1.63% on average within a grid with population below the median; this roughly implies a 0.51% decline in real GDP for those remote areas with less than 3 people per square kilometer. However, the effect becomes insignificant if the sample is divided by the mean, rather than the median (Column 4). This exercise, however, does show that the effect of the crisis does indeed vary over population.

Meanwhile, the coefficient on 'currency crises' is highly significant, but only for the less-populated areas. This is true whether the grids have a population density that is less than the median or the mean, though the effect is more severe for the former. Following a currency crisis episode in the country, the growth of light luminosity decreases by 2.4% on average within pixels with population less than the median; the growth declines by 1.8% within pixels with population less than the mean. The coefficient for both of these subsamples is significant at the 5%. These numbers translate into roughly 0.74% and 0.56% decline in real GDP growth

These findings suggest that there is indeed asymmetric propagation of crises within a country in Latin America, and more so that they systematically impact rural areas with greater severity. It is likely that the increase in the price of imports following currency devaluations disproportionately affects the rural economy in Latin America. The more populous regions, with greater manufacturing and export capabilities, might be better able to offset the price increase by boosting production through exports, something their rural counterpart is unable to do.

These results might not provide the full story because they only show the short-term impact. However, crises can start a decline in economic activity that might plunge a country into a recession a year or two later. It is also not clear how elastic light consumption is with respect to income in Latin America; if they are slow to adjust, the effects will not be captured immediately. Nevertheless, the main take-away from this exercise is the evidence of deviation in shock propagation based upon population.

Table 3.4 presents the results of the same regression on smaller subsamples that are divided more minutely based on the population density. Once again, I cluster at the country level using the pairs cluster bootstrap-t procedure due to too few clusters in my sample. This results in an asymmetric distribution, and so, the table displays the 95% confidence intervals, rather than standard errors, in square brackets below the coefficient. All regressions control for grid and time fixed effects.

I include the regression results on the entire sample in Column 1 for ease of comparison. The rest of the columns show the coefficients for each population bin. The 'Rural' sub-sample considers areas with less than 2 people per square kilometre, and the 'Semi-Rural'—between 2 and 20 people per the same. The urban categories

Table 3.4: Effect of Various Crises on Annual Luminosity Growth in Rural v. Urban Areas

| | Average Annual Luminosity of Night Lights | | | | |
|---|---|---|---|---|---|
| | | Rural | Semi-Rural | Town | Urban |
| Population Count in 1990 | All | $n \leq 10$ | $10 < n \leq 100$ | $100 < n \leq 500$ | $n > 500$ |
| Banking Crisis | -0.0124 | -0.0338** | -0.0215 | -0.0071 | -0.0045 |
| | [-0.039, 0.014] | [-0.059, - 0.008] | [-0.052, 0.009] | [-0.033, 0.018] | [-0.056, 0.048] |
| Currency Crisis | -0.0175** | -0.0305 | -0.0272* | -0.0157 | -0.0045 |
| | [-0.030, -0.005] | [-0.086, 0.025] | [-0.057, 0.003] | [-0.052, 0.020] | [-0.022, 0.013] |
| Sovereign Debt Default | 0.0194 | 0.0177 | 0.0169 | 0.0401 | 0.0320 |
| | [-0.026, 0.064] | [-0.075, 0.110] | [-0.050, 0.084] | [-0.060, 0.140] | [-0.075, 0.139] |
| Twin Crises | 0.007 | -0.0311** | 0.0013 | -0.006 | 0.0088 |
| | [-0.007, 0.021] | [-0.051, -0.011] | [-0.035, 0.038] | [-0.038, .025] | [-0.023, 0.040] |
| Observations | 1,214,051 | 47,954 | 173,141 | 300,869 | 692,087 |
| R-squared | 0.426 | 0.282 | 0.433 | 0.471 | 0.430 |
| Grid FE | YES | YES | YES | YES | YES |
| Year FE | YES | YES | YES | YES | YES |

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

Note: 95% CI in brackets. Robust standard errors clustered at the country level with asymptotic refinement using pairs cluster bootstrap t-procedure with 1200 repetitions due to too few clusters. Sample consists of 22 countries in Latin America and the Caribbean from 1992-2010, broken up into 5 sqkm grids. Population count reported for every 5 square kilometres. Only grids with positive light luminosity measures are included. Crises Dummies from Laeven-Valencia Database, IMF.

include the subsamples 'Town,' with more than 20 people but less than 100 per square kilometer, and 'Urban'—over 100 people per square kilometre.

The effects of a systemic banking crisis seem to be borne mostly by the rural population. The coefficient for the entire sample in Column 1 is negative, yet insignificant. Upon separating the sample according to population, the coefficient becomes strongly significant at the 5% level for the 'Rural' only subsample, as well as almost tripling in magnitude. The advent of a banking crisis in Latin America decreases the growth of lights by 3.3%; in real GDP terms, growth declines by 1% on average in areas with a population density of less than 2 people per square kilometer. This result can be explained by the fact that credit squeezes are especially hard on people in rural areas who have less income and rely on borrowing in order to grow crops for the next season. Though the coefficient is insignificant for the rest of the categories, it is of interest to note that the magnitude of the coefficient decreases with population count, i.e. the severity of a banking crisis appears to diminish as population density increases.

Similarly, the severity of a currency crisis also diminishes across the columns as population density increases, though the coefficient is insignificant for all subsamples except the 'Semi-rural' areas with a population density between 2 and 20 people per square kilometre. The currency crisis leads on average to a 2.7% decline in lights growth within a grid (equivalent to roughly a 0.83% decline in real GDP growth). It appears as if the urbanites are able to weather currency devaluations better than their rural counterparts; perhaps the boost in export manufacturing allows them to offset the increase in the price of imports.

Finally, the point estimates for sovereign debt default and twin crises continue to be insignificant across the board, with one exception. The coefficient on twin crises is negative and significant at the 5% level, once again for rural Latin America. The growth of lights decline by a tremendous 3% when any two crises occur simultaneously. Given the lack of outside options, diversification in rural areas is highly unlikely.

These tables inform us of the asymmetric propagation of shocks within countries in Latin America. Specifically, crises have a devastating effect on rural economic growth; the magnitude of the crises effect also appears to decrease as population density increases. This result implies that there is indeed imperfect risk-sharing intranationally over population.

### 3.5.2 Cluster-Robust "Sandwich" Estimates

Here, I show that using typical heteroskedastic, cluster-robust, "sandwich" estimators to cluster the standard errors at the country level does indeed result in over-rejection of the null when the number of clusters is too small. However, the point estimates obtained by this procedure are comparable to the ones presented above that were obtained via the pairs cluster bootstrap-t procedure. This similarity should allay concerns that bootstrapping might have inflated the point estimates because of multi-collinear draws (See (Cameron and Miller, 2015)).

In comparing the estimates from Table 3.3 to the cluster-robust "sandwich" estimates presented in Table 3.5, two things become apparent. First is the overwhelming number of "stars" in Table 3.5, which indicates instances where the null hypothesis (of zero effect) is rejected. Where the null hypothesis was rejected in only four cases before, naively clustering without asymptotic refinements leads to rejection in 10 cases and with much greater confidence. The coefficient on banking and currency crises are significant across the board except for the subsample 'Above Median,' which includes

Table 3.5: Crises and Lights Growth Revisited: Cluster-Robust “Sandwich” Estimates

| | Log(Average Annual Luminosity) | | | | |
|---|---|---|---|---|---|
| Population in 1990 | All | Below Median | Above Median | Below Mean | Above Mean |
| Banking Crisis | -0.0134* | -0.0194** | -0.00895 | -0.0136* | -0.0145* |
| | (0.007) | (0.007) | (0.010) | (0.007) | (0.009) |
| Currency Crisis | -0.0182*** | -0.0259*** | -0.0111 | -0.0192*** | -0.0139* |
| | (0.007) | (0.008) | (0.008) | (0.007) | (0.008) |
| Sovereign Debt Default | 0.0207 | 0.0088 | 0.0315** | 0.0216 | 0.0175 |
| | (0.013) | (0.013) | (0.015) | (0.013) | (0.012) |
| Twin Crises | 0.0083 | 0.0026 | 0.0137 | 0.0050 | 0.0238*** |
| | (0.009) | (0.010) | (0.012) | 0.010) | (0.009) |
| | | | | | |
| Observations | 1,214,051 | 612,113 | 601,938 | 1,023,071 | 190,980 |
| R-squared | 0.426 | 0.449 | 0.408 | 0.452 | 0.307 |
| | | | | | |
| Grid FE | YES | YES | YES | YES | YES |
| Year FE | YES | YES | YES | YES | YES |

*** p <0.01, ** p <0.05, * p <0.1

Note: Robust standard errors clustered at the country level in parantheses. Heteroskedastic, Cluster-robust, “sandwich” estimates used without any correction for small number of clusters. Sample consists of 22 countries in Latin America and the Caribbean from 1992-2010, broken up into 5 sqkm grids. Only grids with positive light luminosity measures are included. Median and mean population density is calculated by country in the year 1990. Crises Dummies from Laeven-Valencia Database, IMF.

Table 3.6: Crises and Lights Growth in Urban v. Rural Areas Revisited without Asymptotic Refinement

| | Average Annual Luminosity of Night Lights | | | | |
|---|---|---|---|---|---|
| | | Rural | Semi-Rural | Town | Urban |
| Population Count in 1990 | All | $p \leq 10$ | $10 < p \leq 100$ | $100 < p \leq 500$ | $p > 500$ |
| Banking Crisis | -0.0134* | -0.0396** | -0.0249** | -0.00756 | -0.00480 |
| | (0.00701) | (0.0177) | (0.0106) | (0.00933) | (0.0115) |
| Currency Crisis | -0.0182*** | -0.0258 | -0.0240* | -0.0161* | -0.00567 |
| | (0.00678) | (0.0191) | (0.0124) | (0.00874) | (0.00849) |
| Sovereign Debt Default | 0.0207 | 0.00698 | 0.0146 | 0.0399*** | 0.0288 |
| | (0.0130) | (0.0242) | (0.0173) | (0.0150) | (0.0192) |
| Twin Crises | 0.00830 | -0.0265 | 0.00330 | -0.00431 | 0.0120 |
| | (0.00919) | (0.0278) | (0.0148) | (0.0120) | (0.0145) |
| Observations | 1,214,051 | 47,954 | 173,141 | 300,869 | 692,087 |
| R-squared | 0.426 | 0.282 | 0.433 | 0.471 | 0.430 |
| Grid FE | YES | YES | YES | YES | YES |
| Year FE | YES | YES | YES | YES | YES |

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

Note: Robust standard errors clustered at the country level in parantheses. Heteroskedastic, Cluster-robust, "sandwich" estimates used without any correction for small number of clusters. Sample consists of 22 countries in Latin America and the Caribbean from 1992-2010, broken up into 5 sqkm grids. Population count reported for every 5 square kilometres. Only grids with positive light luminosity measures are included. Crises Dummies from Laeven-Valencia Database, IMF.

grids with population above a country's median population.

Second, the magnitudes of the "sandwich" estimates are slightly larger than the bootstrap-t coefficients across the board. However, the fact that they are in the same ballpark is reassuring because bootstrapping could potentially result in large and potentially unreliable point estimates when draws are multi-collinear. Thus, one can consider the pairs cluster bootstrap-t procedure to conservatively estimate the effect of crises on lights growth, both in terms of the magnitude and the inference.

Table 3.6 revisits Table 3.4, once again clustering naively without accounting for the too few clusters in my sample. As in the previous exercise, the p-values are much smaller, leading to more instances of rejecting the null hypothesis. The one exception is the coefficient on 'Twin Crises' for the 'Rural' subsample. In Table 3.4, the bootstrap-t coefficient is negative and significant at the 5% level, but the "sandwich" estimate is not. The former is also slightly larger in magnitude. Thus, it is probably best to take those results with a grain of salt. With this one exception, the bootstrap-t estimates continue to be much more conservative in rejecting the null. The point estimates continue to be of similar magnitude for these subsamples as well.

## 3.6 Conclusion

This paper sought to answer whether various types of crises impact areas within a country differently based on population by using nightlights as a proxy for economic activity. To my knowledge, this is the first paper to look at effect of shocks at such a disaggregate level, and the results show that there is indeed significant divergence in how shocks impact urban versus rural dwellers. Evidence shows that the growth of lights (and by proxy real income) decline noticeable in rural areas, suggesting that capital markets are not fully integrated between urban and rural regions and that risk is imperfectly shared.

These findings raise more questions about why such a bias exists and how the shocks propagate over time. Expanding the analysis to include emerging markets in the rest of the world would also be a worthwhile pursuit. In addition, it is difficult to determine what drives the asymmetries given the lack of controls at the five kilometre grid-level. A theoretical model might more ably provide an explanation for where the breakdown in capital market integration occurs and through what channels the shocks propagate inefficiently.

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