CLIMATE POLICY INITIATIVE
NÚCLEO DE AVALIAÇÃO
DE POLÍTICAS CLIMÁTICAS
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The Effect of Rural Credit on Deforestation: Evidence from the Brazilian Amazon

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September 2016

Abstract

In 2008 the Brazilian government made the concession of rural credit in the Amazon conditional upon stricter requirements as an attempt to curb forest clearings. This paper studies the impact of this innovative policy on deforestation. Difference-in-differences estimation based on a panel of municipalities shows that the policy change led to a substantial reduction in deforestation, mostly in municipalities where cattle ranching is the leading economic activity. The results suggest that the mechanism underlying these effects was a restriction in access to rural credit, one of the main support mechanisms for agricultural production in Brazil.

Keywords

rural credit, land use, deforestation, conservation policies.

Jel codes

Q14, Q23, Q28.

Acknowledgments

Arthur Bragança, Luiz Felipe Brandão, Pedro Pessoa, and Ricardo Dahis provided excellent research assistance. We thank the Ministry of the Environment, particularly Francisco Oliveira, Juliana Simões, and Roque Tumolo Neto, for their continuous support. We are also grateful to Dimitri Szerman, Gabriel Madeira, Joana Chiavari, and Pedro Hemsley, as well as to participants at the 2012 ANPEC Annual Meeting, 2013 North American Summer Meeting of the Econometric Society, 2013 AERE Summer Conference, 2013 EAERE Conference, and 2013 Economics of Low-Carbon Markets workshop for helpful comments.

1. Introduction

Concerns regarding the potential impacts of large-scale deforestation — including, but not limited to, biodiversity loss, water quality and availability, and climate change — have increasingly pushed for greater protection of rainforests. Indeed, nearly 20% of recent global greenhouse gas emissions have been attributed to tropical deforestation.[2] As a response, policymakers around the world have devoted substantial efforts to design and implement a range of law enforcement instruments and incentive-based policies in an attempt to curb forest clearings. The understanding of how and which of these policies have been effective, however, is still limited.

In this paper we evaluate the impact on deforestation of a rather innovative credit policy implemented in the Brazilian Amazon. In 2008, the Brazilian Central Bank published Resolution 3,545, which made the concession of subsidized rural credit in the Amazon conditioned upon proof of compliance with legal titling requirements and environmental regulations. Since all credit agents were obligated to abide by the new rules, Resolution 3,545 thus represented a potential restriction of access to rural credit, one of the main support mechanisms for agricultural production in Brazil.

A key aspect of our empirical context helps design the analysis. Resolution 3,545's conditions applied solely to landholdings inside the administrative definition of the Amazon biome, such that properties outside the biome were not subject to the policy.[3] We explore this characteristic and adopt a difference-in-differences approach, using municipalities along the outside border of the Amazon biome as a control group to evaluate the policy's impact inside the biome. As the Amazon region is large and potentially heterogeneous in non-observables, we only consider municipalities located within a short distance to the border. Our benchmark sample is comprised of municipalities that are within 100km of the border, while alternative samples consider 50km and 200km municipality-to-biome-border distances. This helps ensure that we select treatment (inside biome) and control (outside biome) groups that are similar in terms of pre-trends. Indeed, we show that in neither of the samples control and treatment municipalities portray differential trends in observables prior to policy implementation.

Our analysis is based on a 2003 through 2011 municipality-by-year panel data set. Data on deforestation is built from satellite-based images publicly released by the National Institute for Space Research's Project for Monitoring Deforestation in the Legal Amazon

[2] In particular, extensive forest clearings in Indonesia and in the Brazilian Amazon accounts for most of the acceleration in global deforestation rates observed through the mid-2000s (Hansen and DeFries, 2004; Hansen et al., 2008; Stern, 2008; Ministério do Meio Ambiente, 2012).

[3] The definition of the Amazon biome, although based on technical criteria, is somewhat arbitrary at the local level - areas immediately inside and outside the biome present similar trends over time.

(INPE/PRODES). We also use restricted administrative contract-level data compiled by the Brazilian Central Bank to build rural credit variables at the municipal level. These data are merged with other publicly available information at the municipal level to account for the potential confounding effects of agricultural prices and other concurrent environmental policies.

Our reduced-form estimates show that Resolution 3,545 helped reduce deforestation. We estimate that total deforested area during the sample period was about 60% smaller than it would have been in the absence of the policy. The effects are particularly larger for municipalities where cattle ranching is the main economic activity. Several robustness checks validate our empirical strategy and corroborate the results.

Having explored the reduced-form policy effects on deforestation, we thus investigate its two potential mechanisms. Resolution 3,545 determined that eligibility for accessing rural credit should be conditioned on legal titling requirements as well as on documentation attesting the environmental regularity of the establishment. In a context of precarious property rights, such as that of the Brazilian Amazon, the requirements regarding legal titling of land should be immediately binding and restrictive. If this is the case, the effects of Resolution 3,545 on deforestation should directly reflect a reduction in access to rural credit. On the other hand, Resolution 3,545 conditions were such that borrowers who proved that they had the intention to comply with environmental regulation were allowed access to credit. This meant that producers who feared the resolution might affect their future access to credit could signal an intent to change their deforestation behavior in the future and be considered compliant with environmental regulation in the present. It is thus possible that farmers who were not meeting environmental regulation in the present altered their deforestation behavior for reasons other than a direct reduction in credit. In this case, producers would have suffered no credit effect, as their intention to comply made them compliers, but would still have reduced deforestation.

We follow the same difference-in-differences strategy, and show that the policy change caused a sizable reduction in the concession of rural credit. In particular, the reduction in loans specific to cattle ranching activities accounts for 75% of this effect. We also find that only large and medium loans were affected. This is consistent with the fact that policy requirements were less stringent for small-scale producers.

The overall evidence therefore suggests that Resolution 3,545 has affected deforestation through a reduction in credit concessions. In our final exercise, we thus explore Resolution 3,545 as a source of exogenous variation for credit concessions in a 2SLS approach to test the more general question of whether credit affects deforestation. In theory, the relationship between credit and deforestation is ambiguous. On the one hand, credit should have no impact on forest clearings under complete markets. Because farmers can take advantage of arbitrage in this setup, choices do not depend on the availability of income (Karlan et al., 2013). On the other hand, when markets are not complete, exogenous variations in credit are expected to affect agricultural production decisions and, thus, land clearings. The direction of this effect is, however, unclear. Should credit be used to increase agricultural production by expanding new areas and converting them into agriculture, increased credit availability would likely lead to rising deforestation (Zwane, 2007; Angelsen, 1999; Binswanger, 1991). Yet, should it be used to fund capital expenditures required to improve agricultural technology and productivity, increased credit availability could contain deforestation depending on the relative prices of intensification and clearings (Zwane, 2007).

The validity of our 2SLS approach is dependent upon the assumption that the policy affected deforestation only through the credit channel. The available evidence as well as the actual implementation of the new policy indeed lend support to this assumption. The policy was implemented such that the requirements regarding land titling were immediately binding, while the environmental conditions were more flexible. Under this assumption, farmers with irregular titling suddenly lost access to subsidized credit sources and faced an exogenous variation in credit. We thus rely on Banerjee and Duflo (2012) and assume that the rationing in the availability of subsidized credit induced by Resolution 3,545 exogenously tightened credit constraints.

Our second-stage estimates show a positive relationship between credit and deforestation in the Amazon. This serves as evidence for the existence of credit constraints in the region, and indicates that the activities undertaken in the region are land-intensive, since a tighter credit constraint induced a reduction in deforestation.

These results provide novel evidence to the scant and mixed empirical literature on the effects of rural credit on deforestation. Only a few papers explicitly address access to credit. Data limitations, concerns regarding the endogeneity of credit supply and demand, and a limited ability to generalize context-specific findings have made it difficult to obtain a broader understanding of how credit policies affect deforestation. Pfaff (1999) and Hargrave and Kis-Katos (2012) estimate the effect of different potential drivers of deforestation by exploring panel data at the regional level for Brazil, while Barbier and Burgess (1996) perform a similar exercise for Mexico. The results for the relationship between credit variables and deforestation are mixed and face identification concerns. More recently, Jayachandran (2013) explores a randomized experiment in which a sample of forest owners in Uganda was offered a Payment for Environmental Services (PES) contract. The author finds suggestive evidence that facilitated access to credit can induce contract take-up and thus deter forest owners from cutting trees to meet emergency needs.

Unlike the existing studies, we explore a policy-induced source of variation in access to large-scale credit loans. Considering that rural credit is the main channel through which governments of developing countries support agriculture, and that agricultural production is a first-order driver of deforestation worldwide, our findings shed light on a key policy parameter. More generally, our results also contribute with additional evidence to a broader literature on rural credit. Previous studies have found beneficial effects of the availability of credit in rural contexts. Credit supply has been positively associated with poverty reduction (Burgess and Pande, 2005), and agricultural investment and consumption smoothing (Rosenzweig and Wolpin, 1993; Conning and Udry, 2007; Giné and Yang, 2009). In this paper, we unfold a potential negative externality of rural credit concession by documenting that the greater availability of rural credit may lead to increased forest clearings.

Finally, our analysis suggests that the financial environment in the Brazilian Amazon is characterized by significant credit constraints. In light of this, policies that increase the availability of financial resources could potentially lead to more forest clearings. This issue lies at the core of the recent debate about PES efforts.[4] Although the implementation of PES often occurs in a context different to the one assessed in this paper — namely, one in which payments are conditional upon environmental deliveries — our results highlight the importance of sustained monitoring and enforcement of conditionalities for PES.

The remainder of the paper is organized as follows. Section 2 describes the institutional context and Section 3 presents the data. Section 4 discusses the empirical strategy, focusing on the identification hypothesis. Section 5 presents the reduced-form effects of the policy change on deforestation. Section 6 discusses mechanisms, while in Section 7 we examine the more general relationship between rural credit and deforestation. Section 8 closes with final remarks. The appendix provides a conceptual framework to analyze the relationship between credit constraints and deforestation.

2. Institutional Context

In February 2008, the Brazilian Central Bank published Resolution 3,545, which conditioned the concession of rural credit for agricultural activities in the Amazon biome upon proof of borrowers' compliance with legal titling requirements and environmental regulation. More specifically, Resolution 3,545 established that, in order to prove eligibility for accessing rural credit, the borrower had to present: (i) the Certificate

[4]For a more detailed discussion, see Angelsen and Wertz-Kanounnikoff (2008); Angelsen (2010); Alston and Andersson (2011).

of Registry of the Rural Establishment (CCIR), to meet legal titling requirements;[5] and (ii) a state-issued document attesting the environmental regularity of the establishment hosting the project to be financed, as well as a declaration stating the absence of current embargoes due to economic use of illegally deforested areas. All requirements applied not only to landowners, but also to associates, sharecroppers, and tenants.

In a context of historically precarious property rights such as that of the Brazilian Amazon, the requirements regarding legal titling of land were immediately binding and restrictive. Yet, requirements on environmental conditions were flexible in practice. The state-issued document attesting the establishment's environmental regularity could be replaced by a certificate indicating that the regularization process was ongoing. In this sense, borrowers did not have to attest current environmental regularity, but only a commitment to adapt to environmental regulations in the future. Only establishments that were under full or partial embargo were exceptions to this rule, and were to be denied access to official rural credit in all circumstances.

Resolution 3,545 applied to all rural establishments within the Amazon biome. Implementation of the resolution's terms by all public banks, private banks, and credit cooperatives was optional as of May 1st 2008, and obligatory as of July 1st 2008. Since all credit agents were obligated to abide by the new rules, and given that the requirements regarding legal titling were restrictive, Resolution 3,545 thus represented a potential restriction of access to rural credit, one of the main support mechanisms for agricultural production in Brazil. According to the Ministry of Agriculture and Supply, about 30% of the resources needed in a typical harvest year in Brazil are funded by rural credit, while the remaining 70% come from producers' own resources, as well as from other agents of agribusiness (such as trading companies) and other market mechanisms (Brasil, Ministério da Agricultura, Pecuária e Abastecimento, 2003).[6]

Although restrictive at first, Resolution 3,545 was subject to a series of qualifications that eased conditions for the concession of rural credit for specific groups. First, small-scale producers were particularly favored. In its original text, Resolution 3,545 already established exemptions for some groups of small-scale credit takers. Soon after the compulsory adoption of the resolution, new measures further loosened the requirements

[5]The CCIR constitutes proof that the rural establishment is registered under Brazil's National Institute for Colonization and Agrarian Reform (Incra). Registry is required for sale, rental, partition, and mortgage operations involving the establishment.

[6]A harvest year is the period covering July of a current year through June of the following year. Rural credit is used to finance short-term operations, investment, and commercialization of rural production. The largest share of rural credit (typically, over half) is loaned under fixed per-year interest rates (8.75% up to 2006/2007, and 6.75% thereafter). The interest rates thus contain a significant government subsidy, considering the Central Bank's annualized overnight rate of over 18% in the beginning of the 2000s, and over 8% in the early 2010s.

for the concession of rural credit to small-scale producers.

Second, Resolution 3,545's impact on rural credit concession and, consequently, on deforestation may have differed across economic sectors due to structural heterogeneity. A key structural difference regards the composition of resources used to meet financial requirements for crop *vs* cattle production. According to FAO (2007), the participation of rural credit contracts for crop production has decreased in particular, as agricultural financing has increasingly been obtained through contracts with trading companies, input and processing industries, and retailers and market operators.[7] A crop farming sector that is not heavily dependent on rural credit, as appears to be the case in Brazil, could thus compensate a decrease in access to rural credit imposed by Resolution 3,545 with alternative sources of financing. Producers operating in this sector would therefore be able to sustain investment and deforestation at pre-policy levels. Moreover, crop production in Brazil has also experienced relevant technological advances, particularly with the widespread adoption of direct seeding (FAO, 2007). Indeed, crop farmers likely invest a larger share of rural credit loans in the intensification of production, instead of expanding production by operating in the extensive margin as cattle ranchers do. In this case, a decrease in rural credit for crop farmers might not lead to a decrease in forest clearings, since resources were not originally being used to extend farmland into forest areas.

No such patterns are observed for livestock farming in the country, which remains a low-productivity practice and relatively more dependent on official rural credit. In this case, heterogeneities may have influenced the way in which Resolution 3,545 impacted access to credit and, thus, deforestation across different producers, sectors and regions. We explore these heterogeneities in our empirical analysis.

3. Data

Our analysis is based on a municipality-by-year panel dataset covering the 2003 through 2011 period. We use a geocoded map containing municipalities' location and the Amazon biome's limits to create sub-samples of municipalities, both inside and outside the Amazon biome, located within specific distances from the biome's border. Figure 1 illustrates our benchmark sample, composed of the 179 municipalities whose centroid is located within 100 km of the border, and that are situated entirely inside or outside the Amazon biome. Throughout the analysis, we vary the sample of municipalities according

[7]For instance, regarding the financial requirements of the soybean production sector in Brazil, most of the funds have been actually provided by traders and the processing industry (40%), the input industry (15%), and farmers' own resources (10%), with the remaining 5% being attributed to other sources, such as manufacturers of agricultural machinery (FAO, 2007).

to alternative distance-to-biome-border thresholds. All samples exclude municipalities crossed by the biome border, since only landholdings that were entirely or partially located within the Amazon biome in frontier municipalities were subject to the resolution's condition. The exclusion of frontier municipalities is therefore needed to ensure that all landholdings in treatment municipalities were subject to the policy change.

3.1. Data on Deforestation

Data on deforestation is built from satellite-based images that are processed at the municipal level, and publicly released by the National Institute for Space Research's Project for Monitoring Deforestation in the Legal Amazon (PRODES/INPE). We define deforestation as the area of forest in square kilometers cleared over the twelve months leading up to August of a given year.[8] This time window is chosen to match that of PRODES deforestation data. For this same reason, we recode credit loans and all other variables in this paper accordingly, summing up monthly into annual data, where year t refers to the twelve months leading up to August of t.

To smoothen cross-sectional variation in deforestation that arises from municipality size heterogeneity, we use a normalized measure of the annual deforestation increment. The normalization ensures that our analysis considers relative variations in deforestation increments within municipalities. The variable is constructed according to the following expression:

$$Deforest_{it} = \frac{ADI_{it} - \overline{ADI}_{it}}{sd\left(ADI_{it}\right)} \tag{1}$$

where $Deforest_{it}$ is the normalized annual deforestation increment for municipality i and year t; ADI_{it} is the annual deforestation increment measured in municipality i between the 1^{st} of August of $t-1$ and the 31^{st} of July of t; and $\overline{ADI}_{it}$ and $sd\left(ADI_{it}\right)$ are, respectively, the mean and the standard deviation of the annual deforestation increment calculated for each i over the 2003 through 2011 period.[9]

For any given municipality, cloud cover during the period of remote sensing may compromise the accuracy of satellite images, requiring images to be produced at a different time. As a result, image records for different years may span from less to more than twelve months. Clouds' shadows and forest fires also jeopardize visibility in satellite imagery. To control for measurement error, variables indicating unobservable areas are included

[8] More precisely, the annual deforestation increment of year t measures the area in square kilometers deforested between the 1^{st} of August of $t-1$ and the 31^{st} of July of t.

[9] Our sample excludes municipalities that showed no variation in deforestation throughout sample years, as this variation is needed to calculate the normalized variable.

in all regressions. These data are also publicly available at the municipality-by-year level from PRODES/INPE.

3.2. Data on Rural Credit

Data on annual rural credit are constructed from a contract-level microdata set of rural credit loan contracts, originally compiled by the Brazilian Central Bank from data in the Common Registry of Rural Operations (Recor). This is an administrative microdata set encompassing all rural contract records negotiated by official banks (both public and private) and credit cooperatives in the states of Acre, Amazonas, Amapá, Maranhão, Mato Grosso, Pará, Rondônia, Roraima, and Tocantins, all of which are partly or entirely located in the Amazon biome. It contains detailed information about each contract, such as the exact day on which it was signed, its value in BRL, the contracted interest rate and maturation date, its intended use by agricultural activity, and the category under which credit was loaned (short-term operating funds, investment, or commercialization). All contracts are linked to a code identifying the municipality in which the establishment hosting the activity to be financed is located. We add up the value of the contract loans across all days in each year and each municipality to collapse the microdata into a municipality-by-year panel.

To smoothen the large cross-sectional variation in aggregate values of credit contracts generated by different municipality sizes, we use a normalized measure of rural credit. This normalization again ensures that our analysis captures relative variations in credit lending within municipalities. The variable is constructed according to the following expression:

$$Credit_{it} = \frac{C_{it} - \overline{C}_{it}}{sd\left(C_{it}\right)} \tag{2}$$

where $Credit_{it}$ is the normalized amount of rural credit loaned in municipality i and year t; C_{it} is the amount of rural credit loaned in municipality i and year t in BRL; and $\overline{C}_{it}$ and $sd\left(C_{it}\right)$ are, respectively, the mean and standard deviation of the amount of rural credit loaned in municipality i from 2003 through 2011. Table 1 summarizes the data described above in the municipalities whose centroid is within 100km of the Amazon Biome border from 2003 to 2011.

3.3. Controls

We consider two sets of relevant controls. First, we include controls for agricultural commodity prices, which have been shown to be drivers of deforestation (Panayotou and Sungsuwan, 1994; Barbier and Burgess, 1996; Angelsen and Kaimowitz, 1999; Assunção et al., 2015). As agricultural prices are endogenous to local agricultural production and, thus, local deforestation activity, we construct an output price series that captures exogenous variations in the demand for agricultural commodities produced locally. We follow Assunção et al. (2015) to construct annual indices of crop and cattle prices using prices from the Brazilian non-Amazon state of Paraná and agricultural data from the annual Municipal Crop Survey (PAM) and Municipal Livestock Survey (PPM). We introduce cross-sectional variation by weighing Paraná output prices according to the local (municipal) relevance of each product. We also combine crop prices into a single index using principal component analysis. Agricultural price series are expressed in calendar years, not PRODES years.

Second, we control for other relevant conservation policies implemented during the sample period. In particular, we account for: (i) the extent of protected territory in each municipality, including the total area of protected areas and indigenous lands (data from the Ministry of the Environment and the National Native Foundation); (ii) a dummy variable for priority municipalities, which were selected by the Ministry of the Environment based on their recent history of deforestation and were subjected to stricter monitoring and law enforcement; and (iii) the log of the annual number of environmental fines applied at the municipality level in the previous year. A greater number of fines is regarded as indicative of more stringent monitoring and law enforcement. By including controls for relevant policies in our estimations, we seek to ensure that the effect of Resolution 3,545 on credit and, consequently, on deforestation, is isolated from confounding effects of other concurrent conservation efforts.

4. Empirical Strategy

In this paper we evaluate Resolution 3,545's impacts on deforestation. In order to do so, we explore the fact that the resolution's conditions applied solely to properties located inside the Amazon biome. This generated an explicit geographic cleavage between two groups of municipalities — those entirely located inside the Amazon biome (and thus subject to the resolution's conditions) and those entirely located outside it (and thus exempt from any conditions). This cleavage allows us to create a treatment group, composed of municipalities located entirely within the Amazon biome, and a control group, composed of municipalities located entirely outside the Amazon biome. We thus combine the geographic break in Resolution 3,545 with annual data at the municipality-by-year level to design a difference-in-differences evaluation approach. More specifically, we identify the reduced-form effects of Resolution 3,545 on deforestation by estimating the following regression:

$$Deforest_{it} = \alpha_i + \phi_t + \beta_1(Biome_i * Post2009_t) + \beta_2 Prices_{it} + \beta_3 OtherPol_{it} + \epsilon_{it} \quad (3)$$

where $Deforest_{it}$ is the normalized deforested area in municipality i and year t. Our variable of interest is the interaction of a dummy indicating whether the municipality is located within the Amazon biome, $Biome_i$, with a variable that marks the period after the implementation of Resolution 3,545, $Post2009_t$. This latter variable indicates all years from 2009 onwards.[10] The term α_i represents municipality fixed effects, which absorb initial conditions and persistent municipality characteristics, such as geography and transport infrastructure. The term ϕ_t represents year fixed effects to control for common time trends, such as seasonal fluctuations in agricultural activity, macroeconomic conditions, common rural policies, and the political cycle. The term $Prices_{it}$ proxies for municipality-specific demand for agricultural land, as it includes annual cattle and crop price indices (current and lagged) varying over time at the municipality level. Finally, the term $OtherPol_{it}$ indicates other environmental policies, namely: (i) the percentage of municipal territory under protection, including protected areas and indigenous lands; (ii) a dummy variable for priority municipalities; and (iii) the log of the annual number of environmental fines applied at the municipality level in the previous year. In all specifications, standard errors are clustered at the municipality level to allow for correlation at a given time, as well as across time within municipalities.

Because the Brazilian Amazon spans over a large and heterogeneous region, taking municipalities in the treatment and control groups that are far from each other could result in a comparison of municipalities with very different initial economic conditions and non-observable trends. We thus restrict our treatment and control samples to those municipalities whose centroid is within 100 km of the Amazon biome border. Municipalities in our treatment and control groups are therefore relatively close to each other. In Sections 5 and 6 we show that our results remain robust to changes in the distance-to-biome-border threshold and in their respective samples of municipalities.

The parameter of interest β_1 in equation 3 captures the causal effect of Resolution 3,545 on deforestation if the residuals contain no omitted variables simultaneously correlated with the policy change and with any latent determinant of forest clearings. In this case, the validity of our difference-in-differences specification hinges on two key conditions. First, our approach should be robust to the influence of regional time-varying

[10]We consider a post-policy period beginning in 2009, because the policy was implemented in July 2008 and panel variables are measured in PRODES years, meaning that year t is considered to be the period between the 1st of August of $t-1$ and the 31st of July of t.

shocks that unevenly hit treatment and control municipalities. Second, pre-trends for treatment and control groups should be parallel, so as to ensure that estimates are not spuriously driven by region-specific time trends.

Regarding the first condition, there is no evidence that the selection of Amazon biome municipalities into treatment was made as a response to the business cycle or to region-specific economic booms or downturns. As for the second condition, Figure 2 supports the view that treatment and control municipalities portrayed similar pre-policy trends in forest clearings. Yet, deforestation in control and treatment municipalities exhibit divergent behavior immediately after the implementation of Resolution 3,545, with forest clearings in treatment municipalities dropping sharply.

Having explored the reduced-form impact of Resolution 3,545 on deforestation, we thus investigate its two potential mechanisms. Resolution 3,545 determined that eligibility for accessing rural credit should be conditioned on legal titling requirements as well as on documentation attesting the environmental regularity of the establishment. In a context of precarious property rights, such as that of the Brazilian Amazon, the requirements regarding legal titling of land should be immediately binding and restrictive. If this is the case, the effects of Resolution 3,545 on deforestation should directly reflect a reduction in access to rural credit.

On the other hand, Resolution 3,545 conditions were such that borrowers who proved that they had the intention to comply with environmental regulation were allowed access to credit. This meant that producers who feared the resolution might affect their future access to credit could signal an intent to change their deforestation behavior in the future and be considered compliant with environmental regulation in the present. Although unlikely, it is therefore possible that farmers who were not meeting environmental regulation in the present alter their deforestation behavior for reasons other than a direct reduction in credit. If this is the case, producers will suffer no credit effect, as their intention to comply makes them compliers, but still reduce deforestation.

In order to test for these two concurrent hypotheses, we draw on a model analogous to equation 3 to investigate the direct impact of Resolution 3,545 on credit. The estimation is based on the following equation:

$$Credit_{it} = \alpha_i + \phi_t + \beta_1(Biome_i * Post2009_t) + \beta_2 Prices_{it} + \beta_3 OtherPol_{it} + \epsilon_{it} \qquad (4)$$

where $Credit_{it}$ is the normalized total value of credit concessions in municipality i and year t. All other variables are defined as in equation 3. In addition, we further test whether the estimated coefficient β_1 in equations 3 and 4 is sensitive to the inclusion of controls for environmental policies concerning monitoring and law enforcement, namely embargoed areas and the number of environmental fines. The inclusion of these controls should help absorb the reduced-form effects if either current or future compliance with environmental regulation are indeed jointly correlated with the demand for credit and deforestation.

5. Policy Impact on Deforestation

We start by testing whether Resolution 3,545 affected deforestation in the Amazon. After discussing the reduced-form effects, we present robustness checks and explore heterogeneities.

5.1. Main Results

Table 2 presents estimated coefficients for Resolution 3,545's effect on forest clearings based on equation 3. Controls are added gradually. Column 1 includes only municipality and year fixed-effects, as well as control variables for unobservable areas in satellite imagery. Column 2 adds agricultural prices while column 3 includes environmental policies that were not directly related with Resolution 3,545 – such as the expansion of protected territory and the creation of priority municipalities. Column 4 adds environmental policies concerning monitoring and law enforcement, namely embargoed areas and number of environmental fines. This latter column is our most complete specification. In the remaining two columns, we vary the distance-to-biome-border thresholds to test whether the results are robust to sample selection.

We observe negative, sizable, and robust coefficients across all specifications. We find a point estimate of -0.57 standard deviations in the first column. The coefficient remains stable upon inclusion of agricultural prices as controls, and drops only marginally to -0.53 when the confounding influence of concurrent conservation policies are also absorbed. The coefficient also remains stable when environmental monitoring and law enforcement policies are accounted for. Further, the results hold across alternative samples. We find a slightly larger point estimate in the smaller sample (50km), and a smaller but still sizable coefficient in the larger sample (200km). This pattern is to be expected in light of the fact that the Amazon biome border largely coincides with the so-called Arc of Deforestation, an area that loosely marks the agricultural expansion frontier in the area and concentrates the vast majority of forest clearing — as we increase the distance-to-biome-border threshold, we include municipalities that are located further away from the Arc of Deforestation and that therefore experience lower deforestation pressure.

We use counterfactual simulations to quantify the contribution of Resolution 3,545 in terms of avoided forest clearings. Our baseline specification is the one presented in column 4 of Table 2. This specification delivers the predicted trend in deforestation for each sample municipality, by using the estimated coefficients. Given the estimated parameters, we are able to recalculate the predicted deforestation under the alternative condition $(Biome_i * Post2009_t) = 0$. This calculation delivers the predicted municipality trend in annual deforestation in a hypothetical scenario in which Resolution 3,545 was not implemented. We then sum up the predicted deforestation, across all sample municipalities and all sample years, in both scenarios. We find that, in the absence of Resolution 3,545, total deforestation would have been 2,000 square kilometers greater than the actually observed from 2009 through 2011 in the 100-kilometer sample of municipalities, which represents a reduction of 60% if one considers the baseline deforestation in 2008. Resolution 3,545 has therefore played an important role in curbing forest clearings in the Amazon biome in the late 2000s.

5.2. Robustness Checks

We now examine the main concern regarding the validity of our reduced-form strategy – namely, whether there exist relevant deforestation pre-trends between treatment and control groups. Table 3 shows the results for multiple robustness exercises. Column 1 replicates the coefficient of our preferred specification from Table 2, column 4. Column 2 restricts the sample period up through 2008, and regress deforestation on a linear time trend interacted with the Amazon biome dummy. This specification formally checks whether pre-trends in municipalities inside (treatment) and outside (control) the biome were significantly different before the policy change — if pre-trends across treatment and control groups were the same, the coefficient of the interaction variable should not be significant. Estimated coefficients therefore provide no support for the view that treatment and control municipalities exhibited different forest clearing trends before the implementation of Resolution 3,545.

In column 3, we return to our preferred, full-sample specification, but now add three interactions, each consisting of the Amazon biome dummy and one of the three years immediately preceding the implementation of Resolution 3,545. With this, we test whether knowingly fake policy-implementation years yield significant results — if so, it might well be that our main findings are also due to some spurious, non-policy-related impact. The results show that the coefficient capturing the effect of the actual policy (post-2009 interaction) remains negative and significant, while all other interactions are statistically non-significant. This indicates that the post-policy difference in forest clearings is not correlated with unobservable shocks in the years immediately preceding policy implementation. In our two remaining robustness checks, we control for an interaction term between a linear trend and the Amazon biome dummy (column 4) and for municipality-specific linear trends (column 5). The coefficients remain negative and robust throughout. Combined, the evidence lends support to our estimation strategy, as well as to our interpretation of the results.

5.3. Heterogeneity

The evidence presented so far indicates that Resolution 3,545 reduced deforestation. Yet, the policy might have had differential effects across different regions. We explore one such dimension of regional heterogeneity, looking at how the relationship between credit and forest clearings may differ between municipalities with different leading economic activities. To test this, we rerun our most complete specification for cattle and crop-oriented samples of municipalities separately. We define municipalities as cattle-oriented if their main economic activity, as measured by the annual average value of credit prior to implementation of Resolution 3,545, was cattle ranching. Otherwise, we define the municipality as crop-oriented.

Table 4 presents the results, with coefficients for the cattle and crop-oriented samples in columns 1 and 2, respectively. In cattle-oriented municipalities, the point estimate is quite similar to that of our preferred specification (Table 2, column 4). In contrast, the estimated coefficient for the crop-oriented sample suggests that Resolution 3,545 had no impact on deforestation where crop farming is the leading agricultural activity. This is consistent with reports documenting that crop production in Brazil has been less dependent on credit and has undergone several technological improvements, allowing production to increase at the intensive margin.

6. Mechanisms

Having explored the reduced-form impact of Resolution 3,545 on deforestation, we now investigate its potential mechanisms. If requirements regarding legal titling of land were immediately binding, the effects of Resolution 3,545 on deforestation should be a direct response of a reduction in access to rural credit. On the other hand, it is also possible that farmers who were not meeting environmental regulation in the present altered their deforestation behavior for reasons other than a direct reduction in credit. As argued in Section 4, if this is the case, producers would suffer no credit effect, as their intention to comply makes them compliers, but still reduce deforestation. In order to identify the role of these two mechanisms, we thus examine the impact of Resolution 3,545 on rural credit loans. Analogously to the latter section, we also present robustness checks and explore heterogeneities.

6.1. Main Results

Table 5 presents estimated coefficients for regressions based on equation 4. Again, controls are added gradually. The results indicate that Resolution 3,545 was associated with an overall reduction in rural credit concession in the Amazon biome. The coefficient of interest is largely stable throughout gradual inclusion of controls and sample selection.

Further, the inclusion of environmental monitoring and law enforcement controls does not affect Resolution 3,545's impacts in particular. This suggests that Resolution 3,545 did not affect credit concession via reduced demand from borrowers fearing potential future credit restrictions. In general, the results from Table 5 indicate that the effects of Resolution 3,545 on deforestation thus directly reflect a reduction in deforestation as a response to a reduction in access to rural credit.

We perform a counterfactual analysis to quantify the magnitude of the policy impact.[11] We estimate that Resolution 3,545 caused a reduction in total credit loans of approximately BRL 579 million (USD 290 million) over the 2009 through 2011 period in our benchmark sample of treated municipalities. Observed credit concession was therefore 16% smaller as compared to a counterfactual scenario in which the resolution did not exist.

6.2. Robustness Checks

As discussed in Section 5.2, the validity of our estimations hinges on ensuring that our treatment and control groups followed parallel pre-policy trends. We now test whether this was the case for credit concessions inside and outside the Amazon biome. We do it so by rerunning our preferred specification (Table 5, column 4) with additional controls for region and municipality-specific time trends, as well as by conducting falsification tests for policy implementation date.

Table 6 presents the results. Column 1 replicates the coefficient of our preferred specification. In the second column we test pre-policy trends by restricting the sample period up through 2008, and interacting a linear time trend with the Amazon biome dummy. In the following column we then add interaction terms to our full-sample specification, each consisting of the Amazon biome dummy times one of the three years immediately preceding policy implementation. We find no evidence that treatment and control municipalities portrayed different trends in credit concessions before the implementation of Resolution 3,545. Finally, we further test if time trends are driving our results by controlling for an interaction term between a linear time trend and the Amazon biome dummy (column 4) and for municipality-specific linear trends (column 5). The coefficients capturing the effect of Resolution 3,545 remain negative and significant throughout, and are even slightly larger in absolute terms in both tests.

6.3. Heterogeneity

We now test whether Resolution 3,545 had differential effects across different types of credit contracts. We start by separating credit to be used in crop farming *vs*

[11] This exercise is analogous to the one performed in Section 5.1, but now based on coefficients from Table 5, column 4.

cattle ranching activities. The first column of Table 7 shows that Resolution 3,545's impact on credit for use in cattle ranching activities is negative and significant. In contrast, column 2 shows that the effect on credit concessions for use in crop farming is smaller and statistically insignificant. This finding is consistent with crop farming being relatively less dependent on official credit, as discussed in Section 2. Alternative sources of financing through trading companies, input and processing industries, retailers, and market operators may have replaced official sources of rural credit constrained by Resolution 3,545.

We also investigate the policy impact on concessions by loan size. The dependent variable is now the number of credit contracts in each municipality categorized in groups according to agricultural activity (cattle ranching or crop farming) and contract size (small: up to the median; medium: between the median and the 75^{th} percentile; and large: above the 75^{th} percentile). Given that small-scale producers benefited from less stringent conditions for credit concession, we expect Resolution 3,545 to have relatively stronger impacts on medium and large contracts. Columns 3 through 8 of Table 7 confirm this view, particularly for cattle-specific credit contracts. We find large and significant coefficients for large contracts, but no significant impact on the number of small loans. We also observe smaller and a series of non-significant coefficients for crop-specific credit contracts. This result is consistent with the view that both small and large crop producers were able to overcome the credit restrictions, but for different reasons. While small producers faced less stringent conditions, large crop producers could more easily replace official credit by alternative sources of financing. Finally, we again explore regional heterogeneity in credit concessions using our cattle and crop-oriented sub-samples of municipalities. Columns 9 and 10 show that the estimated coefficients remain negative and robust in both sub-samples. Although the magnitude of the estimated coefficient is larger for crop-oriented municipalities, standard errors are large due to small sample size, and the difference between coefficients is not statistically significant.

Overall, the results indicate that while the reduction in credit loans was widespread across different regions, credit concessions for use in cattle ranching were the most affected by Resolution 3,545. Together with the results from Table 4, this suggests that access to credit and deforestation are particularly correlated in cattle-ranching activities.

7. The General Relationship of Credit and Deforestation

The extent to which access to credit affects deforestation is ambiguous in theory. On the one hand, credit should have no impact on forest clearings under complete markets. Because farmers can take advantage of arbitrage in this setup, choices do not depend on the availability of income (Karlan et al., 2013). On the other hand, when markets are not complete, exogenous variations in credit are expected to affect agricultural production decisions and, thus, land clearings. The direction of this effect is, however, ambiguous. Should credit be used to increase agricultural production by clearing forest areas and converting them into agriculture, increased credit availability would likely lead to rising deforestation (Zwane, 2007; Angelsen, 1999; Binswanger, 1991). Yet, should it be used to fund capital expenditures required to improve agricultural technology and productivity, increased credit availability could contain deforestation depending on the relative prices of intensification and clearings (Zwane, 2007). We provide a detailed conceptual discussion on the ambiguity of the relationship between credit and deforestation in Appendix A.

While theory alone provides ambiguous answers, only a few papers empirically address how and to what extent access to credit affects deforestation. A recent stream of papers empirically analyzes the relationship between availability of financial resources and deforestation in developing countries, where landowners are typically credit constrained. These studies often focus on household income as a proxy for the availability of financial resources, and look at scenarios in which subsistence agriculture constitutes the main economic activity (instead of large-scale, export-led agricultural production). Alix-Garcia et al. (2013) show that a conditional cash transfer program increased deforestation in Mexico, while Zwane (2007) finds evidence of a positive relationship between income and forest clearings in Peru.[12]

Only a few papers explicitly address access to credit. Pfaff (1999) and Hargrave and Kis-Katos (2012) estimate the effect of different potential drivers of deforestation by exploring panel data at the regional level for Brazil, while Barbier and Burgess (1996) perform a similar exercise for Mexico. The results for the relationship between credit variables and deforestation are mixed and face identification concerns. Jayachandran (2013) explores a randomized experiment in which a sample of forest owners in Uganda was offered a PES contract. The author finds suggestive evidence that facilitated access to credit can induce contract take-up and thus deter forest owners from cutting trees to meet emergency needs.

Data limitations, concerns regarding the endogeneity of credit supply and demand, and a limited ability to generalize context-specific findings, however, have made it difficult to obtain a broader understanding of how credit policies affect deforestation. In this paper we do not address access to resources directly linked to environmental or

[12] In particular, Alix-Garcia et al. (2013) find that additional household income significantly increases consumption, with recipient households shifting strongly into land-intensive goods such as beef and milk. The literature contains several other efforts to test the relationship between household income and forest resources, though many with unresolved identification issues (for examples, see Barbier and Burgess (1996); Wunder (2001); Baland et al. (2010); Pfaff (1999); Fisher et al. (2005); Foster and Rosenzweig (2003); Deininger and Minten (2002); Shortle and Abler (1999); Cropper and Griffiths (1994)).

poverty alleviation programs, but rather investigate overall access to credit markets for agricultural production. Thus, unlike existing studies, we explore a policy-induced source of variation in access to large-scale credit loans. Sections 5 and 6 presented compelling evidence that Resolution 3,545 had negative effects on both deforestation and credit concession. Further, the overall evidence suggests that Resolution 3,545 has affected deforestation only through a reduction in credit concessions. In this case, we explore our empirical context and use Resolution 3,545 as a source of exogenous variation for credit concessions in a 2SLS approach, in which we test the more general question of whether credit affects deforestation.

More specifically, the validity of our 2SLS approach hinges on two hypotheses. First, that the policy had a strong effect on credit concession — Section 6 makes this case. Second, that the policy affected deforestation strictly through the credit channel. This hypothesis could be violated by the fact that Resolution 3,545 also included environmental conditions. However, Resolution 3,545's combination of immediately binding legal titling requirements and flexible environmental conditions overcomes this concern. In particular, there is no evidence that farmers altered their deforestation behavior for reasons other than a direct reduction in credit. As argued in Section 4, if this was the case, producers would have suffered no credit effect, as their intention to comply makes them compliers, but had still reduced deforestation.

Taking these two hypotheses as plausible, we can use Resolution 3,545 as an instrument for credit in the deforestation regression. While the first stage is given by equation 4, the second stage equation is defined below:

$$Deforest_{it} = \alpha_i + \phi_t + \beta_1 Credit'_{it} + \beta_2 Prices_{it} + \beta_3 OtherPol_{it} + \epsilon_{it} \quad (5)$$

where $Credit'_{it}$ is instrumented by the interaction term $Biome_i * Post2009_t$. As before, the term α_i represents municipality fixed effects, and the term ϕ_t represents year fixed effects. We also control for agricultural prices and other environmental policies, just as in the previous reduced-form estimations.

Panel A of Table 8 presents the results of OLS specifications, where deforestation is the dependent variable and credit concession is our variable of interest. The coefficients are statistically insignificant, and the point estimates vary in sign across different specifications. This is in line with previous results reported in the literature, whenever credit endogeneity is not fully accounted for. Panel B presents the 2SLS second-stage regressions. We find positive and significant 2SLS coefficients, irrespective of the inclusion of controls and sample selection. In particular, the coefficient remains practically unaltered once we control for environmental policies concerning monitoring and law enforcement, namely embargoed areas and the number of environmental fines. The point estimate decreases as we increase distance to biome border, but remain statistically significant across the different samples.

These results provide evidence on the existence of binding credit constraints in the Amazon biome. Farmers appear to have responded to a reduction in the availability of subsidized credit by changing their optimal allocation of resources, and thereby reducing deforestation. As discussed in Appendix A, in the absence of binding credit constraints, farmers' actions would not have resulted in a change in deforestation during the post-policy period.[13] Our results also support the view that credit in the Amazon biome is used to expand production at the extensive margin (through the clearing of forest areas for conversion into agricultural lands), and not at the intensive margin (through increased productivity). The predominance of cattle ranching in the region and the correlation between this activity and extensive land use in the Amazon is consistent with these results.

8. Final Comments

In this paper we evaluated the impact on deforestation of Resolution 3,545, a rather innovative policy that made the concession of subsidized rural credit in the Amazon conditioned upon proof of compliance with legal titling requirements and environmental regulations. We documented that Resolution 3,545 helped contain deforestation in the Amazon biome. The effects are particularly larger for municipalities where cattle ranching is the main economic activity. This finding suggests that the expansion of agriculture, in particular of cattle ranching, at the extensive margin is financially constrained in the biome. Our estimates indicate that total observed deforested area from 2009 through 2011 was about 60% smaller than it would have been in the absence of credit restrictions.

Having explored the reduced-form impact of Resolution 3,545 on deforestation, we thus investigated its potential mechanisms. We presented evidence that Resolution 3,545 had negative effects on both deforestation and credit concession. The available evidence as well as the actual implementation of the new policy indeed lend support to the assumption that the policy affected deforestation only through the credit channel. Given this evidence, in a final exercise, we explored our empirical context and used Resolution 3,545 as a source of exogenous variation for credit concessions in a 2SLS approach. This allowed us to test the more general question of whether credit affects deforestation.

Our results yield two policy implications. First, the evidence indicates that

[13]Consider the case in which producers are not credit-constrained and have project returns that are not high enough to cover the cost of the market interest rates, but are high enough to cover the cost of subsidized credit. In this case, producers would not invest in these projects in the baseline (pre-policy period), as they could earn more by investing in financial markets and earning the basic interest rates.

the conditioning of rural credit is an effective policy instrument to combat illegal deforestation. Yet, differential effects across sectors and regions suggest that it should complement, rather than substitute, other conservation efforts. Our finding that credit reduction came mostly from a reduction in cattle loans rather than crop loans also indicate that the economic environment matters for policy effectiveness. Implementation details also matter — less stringent requirements and exemptions for small producers determined that medium and large producers were more affected than small-scale ones.

Second, our analysis suggests that the financial environment in the Amazon is characterized by significant credit constraints and/or financial imperfections. Specially in municipalities where cattle ranching is the leading economic activity, fewer financial resources correspond to less deforestation. This is a relevant finding with implications for policy design. In particular, policies that increase the availability of financial resources could lead to higher deforestation rates, depending on the economic environment and existing resources in the area. Our results do not suggest that these policies will necessarily increase deforestation, but that policy design should take into account the nature of financial constraints prevailing in the context to avoid potentially adverse rebound effects.

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Appendices

A. Conceptual Framework

Many studies have pointed out that imperfect insurance and credit constraints are associated with less investment, smaller profits, and limited growth, thus standing as barriers to development in rural areas.[14] We draw on Banerjee and Duflo (2012) to present a framework that shows how imperfect markets and financial constraints affect agricultural production choices and, consequently, deforestation. In the absence of credit constraints, changes in the availability of subsidized rural credit would not affect agricultural choices. However, when different production technologies are available to a producer who faces credit constraints, agricultural choices can be affected by changes in the availability of resources.

Suppose a farmer operates in a forest area and chooses one among two agricultural production technologies — traditional or modern. With the traditional technology, the farmer produces agricultural output using labor and land inputs. This traditional technology is described by:

$$f(L, T) \tag{6}$$

where L is labor employed and T is area used for production. With the modern technology, in addition to labor and land, the farmer also uses other inputs, K, such as tractors and fertilizers. This modern technology is described by:

$$F(K, L, T) = A(K)f(L, T) \tag{7}$$

Assume that labor can be paid at the end of the harvest period, but that expenditures with non-labor inputs must be paid in advance. Taking M as total working capital available to the farmer, working capital constraints are given by $p_T T \leq M$ and $p_K K + p_T T \leq M$ for the traditional and modern technologies, respectively. These constraints allow for the possibility of existing binding credit financing as in Feder (1985) and Udry (2010). A farmer using the traditional technology therefore faces the following decision problem:

$$\begin{aligned} \pi_{traditional}(M) \quad = \quad & \max_{L,T} f(L, T) - p_L L - p_T T \\ & \text{subject to} \;\; p_T T \leq M \end{aligned} \tag{8}$$

[14]For excellent literature reviews, see: Dowd (1992); Ghosh et al. (2000); Conning and Udry (2007); Giné and Yang (2009).

Similarly, the decision problem for a farmer using the modern technology can be described as:

$$\begin{aligned} \pi_{modern}(M) &= \max_{K,L,T} A(K)f(L,T) - p_K K - p_L L - p_T T \\ & \quad \text{subject to} \;\; p_K K + p_T T \leq M \end{aligned} \tag{9}$$

Thus, a farmer with available working capital M chooses the modern technology if, and only if, $\pi_{modern}(M) \geq \pi_{traditional}(M)$. Define M_0 such that $\pi_{modern}(M_0) = \pi_{traditional}(M_0)$. We assume that p_k and $A(K)$ are such that all farmers with $M \geq M_0$ choose the modern technology. In summary:

$$\pi(M) = \begin{cases} \pi_{traditional}(M) & \text{if} M < M_0 \\ \pi_{modern}(M) & \text{if} M \geq M_0 \end{cases} \tag{10}$$

In this framework, with the farmer operating in a forest area, the choice of area to be used for production is equivalent to deforestation. We are therefore particularly interested in how optimal farmland size is affected by the availability of capital when the farmer is allowed a choice of production technology.

To simplify the analysis, we consider specific functional forms for the production functions, assuming that $A(K) = K^\alpha$ and $f(L,T) = L^\beta T^\gamma$, where $\alpha > 0$, $\beta > 0$, $\gamma > 0$ and $\alpha + \beta + \gamma < 1$. The assumption of decreasing returns to scale helps determine a finite optimal farmland size. We focus on the characterization of the optimal land input. For the traditional technology, the optimal choice of farmland is given by:

$$T_{traditional}(M) = \begin{cases} \frac{M}{p_T}, & \text{if} M < \overline{M} \\ T^*_{traditional} & \text{if} M \geq \overline{M} \end{cases} \tag{11}$$

where $T^*_{traditional} \equiv \left(\frac{\gamma}{p_T}\right)^{\frac{1-\beta}{1-\beta-\gamma}} \left(\frac{\beta}{p_L}\right)^{\frac{\beta}{1-\beta-\gamma}}$ and $\overline{M} = p_T T^*_{traditional}$. For the modern technology, the optimal choice of farmland is given by:

$$T_{modern}(M) = \begin{cases} \frac{\gamma}{\alpha+\gamma}\frac{M}{p_T} & \text{if} M < \overline{\overline{M}} \\ T^*_{modern} & \text{if} M \geq \overline{\overline{M}} \end{cases} \tag{12}$$

where $T^*_{modern} \equiv \left(\frac{\alpha}{p_K}\right)^{\frac{\alpha}{1-\alpha-\beta-\gamma}} \left(\frac{\beta}{p_L}\right)^{\frac{\beta}{1-\alpha-\beta-\gamma}} \left(\frac{\gamma}{p_T}\right)^{\frac{1-\alpha-\beta}{1-\alpha-\beta-\gamma}}$ and $\overline{\overline{M}} = p_K K^*_{modern} + p_T T^*_{modern}$.

The relative values of M_0, $\overline{M}$, and $\overline{\overline{M}}$ define different possible cases. For example, a configuration such that $M_0 < \overline{M} < \overline{\overline{M}}$ implies the optimal farmland size curve shown in Figure 3.

Define M^* as the farm's total investment if the farmer can borrow as much as he wants at the interest rate r. Thus,

$$M^*(r) = \arg\max_{M} \Pi(M) - (1+r)M \tag{13}$$

represents the first-best investment level.

We assume that a typical farmer can be financed by two different sources and ignore, for the sake of simplicity, the possibility of self-financing. A subsidized rural credit line is available at cost r_b, which is below the market interest rate r_m, $r_b < r_m$. Denoting the amounts of subsidized rural credit and market credit as M_b and M_m, respectively, total investment is given by $M = M_b + M_m$. Following Banerjee and Duflo (2012), we say that a farmer is *credit rationed* at the subsidized interest rate if $M_b < M^*(r_b)$, and that a farmer is *credit constrained* if $M < M^*(r_m)$.

As we will argue in Section 2, the policy change may have reduced the availability of subsidized rural credit for some farmers in the Amazon biome. Yet, the supply of credit offered at the market rate by agents other than official (private and public) banks and credit cooperatives was not directly affected by the policy change. Our conceptual framework provides intuition on how farmers are expected to react to this change in the supply of credit, and thereby potentially affect deforestation, under different assumptions about the availability of financial resources.

To restrict the analysis to a simple, yet interesting, situation, consider the case depicted in Figure 3, where $M_0 < \overline{M} < \overline{\overline{M}}$. Other configurations can be considered analogously. Start with the region where total investment lies below $\overline{\overline{M}}$. Increases in the availability of resources within each technology region — $(0, M_0)$ or $(M_0, \overline{\overline{M}})$ — affect land size positively. If there is no change in the choice of production technology, a reduction in credit leads to a decrease in optimal farmland size and thereby reduces deforestation. However, changes in the availability of resources that cause farmers to switch between technology regions — from $(0, M_0)$ to $(M_0, \overline{\overline{M}})$ or vice-versa — have an ambiguous effect on land size. A reduction in credit may lead the farmer to substitute the modern technology for the traditional one, potentially leading to an increase in optimal farmland size and deforestation. In the region where total investment lies above $\overline{\overline{M}}$, farmers are not credit constrained, so changes within this region do not affect optimal farmland size. Thus, a reduction in M_b that keeps the farmer in the unconstrained region does not affect deforestation, but a reduction in the availability of resources that pushes the farmer into the $(M_0, \overline{\overline{M}})$ interval will reduce optimal farmland size and deforestation. An even stronger reduction in the availability of resources that pushes the farmer further into the $(0, M_0)$ interval has an ambiguous impact on deforestation. Propositions 1-3 summarize these results in the context of the credit reduction implied by the policy change.

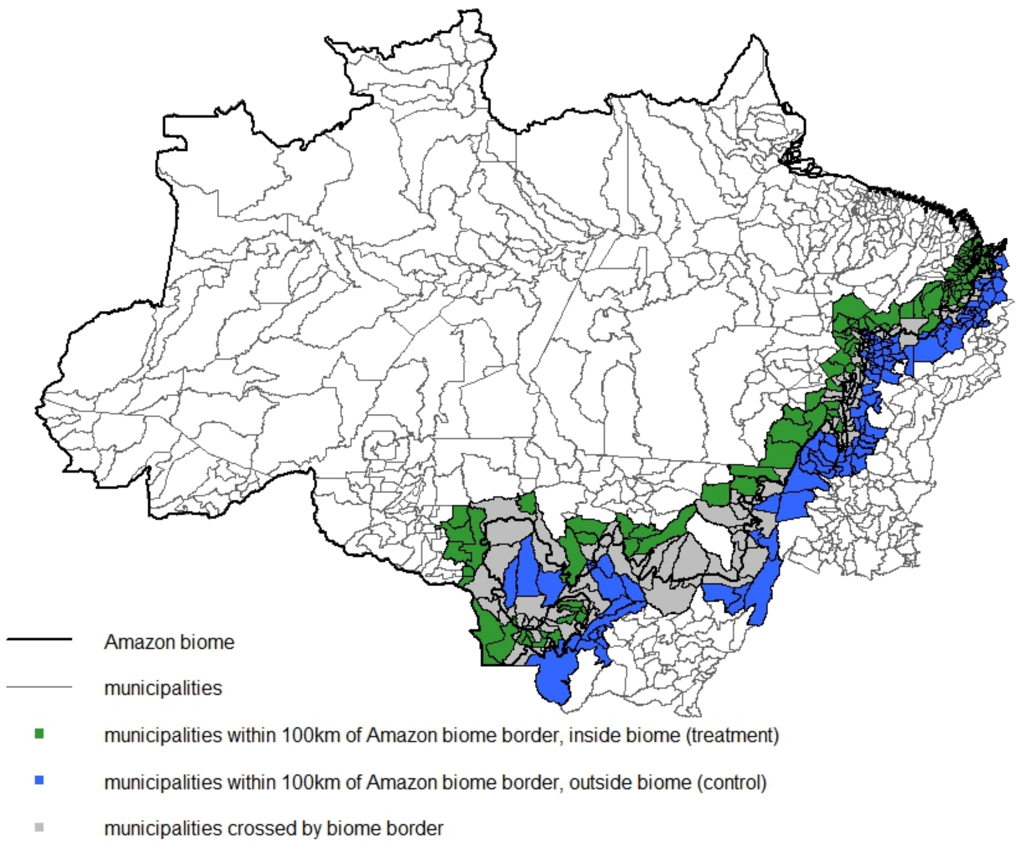
Proposition 1: If the reduction in the availability of subsidized rural credit causes a reduction in deforestation, we can conclude that: (i) farmers are credit constrained; and (ii) credit and deforestation have a positive relationship within technology regions.

Proposition 2: If the reduction in the availability of subsidized rural credit does not affect the amount of cleared land, we can conclude that: (i) either farmers are not credit constrained (they could simply be substituting subsidized rural credit by market credit); or (ii) farmers are credit constrained, but are changing from the modern to the traditional technology.

Proposition 3: If the reduction in the availability of subsidized rural credit implies an increase in deforestation, we can conclude that: (i) farmers are credit constrained; and (ii) they are changing from the modern to the traditional technology.

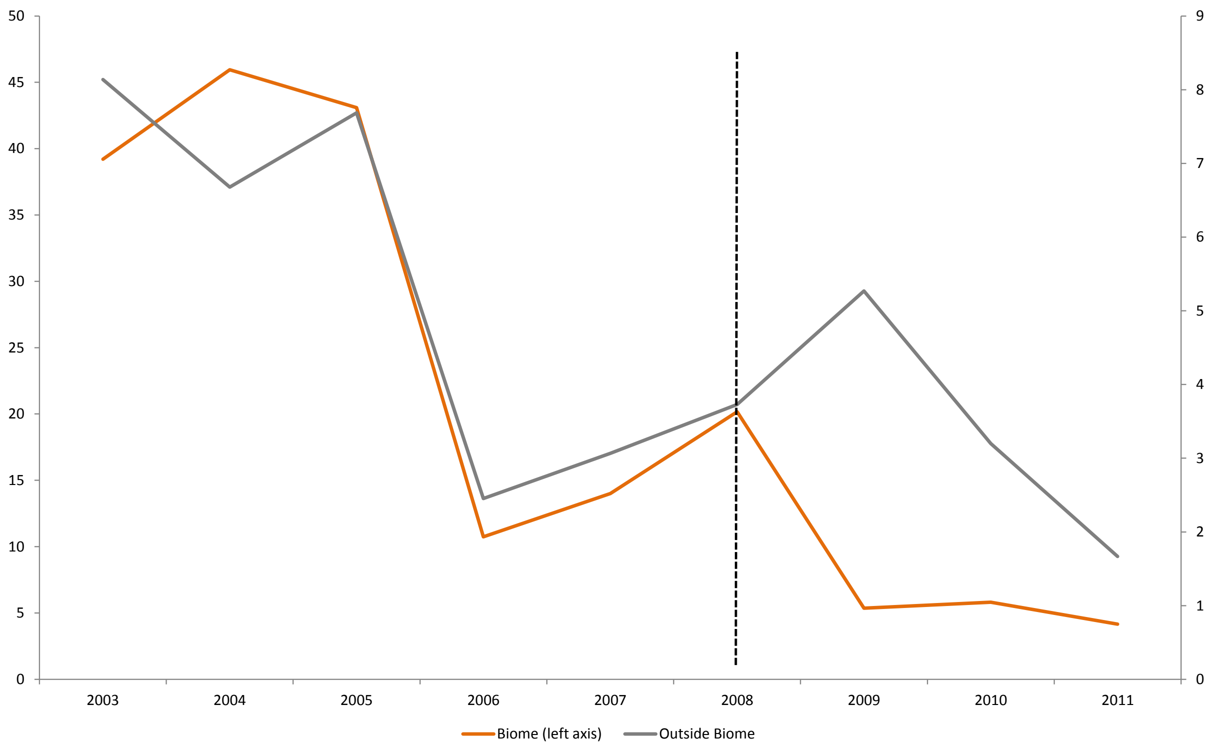
In summary, a subsidized credit policy restriction can: (i) serve as evidence of credit constraints if we observe an impact on deforestation; and (ii) reveal whether the relevant margin is change in optimal farmland size for a given technology (decreasing deforestation) or change across production technologies (increasing deforestation).

Figure 1: Brazilian Amazon Biome and Benchmark Sample



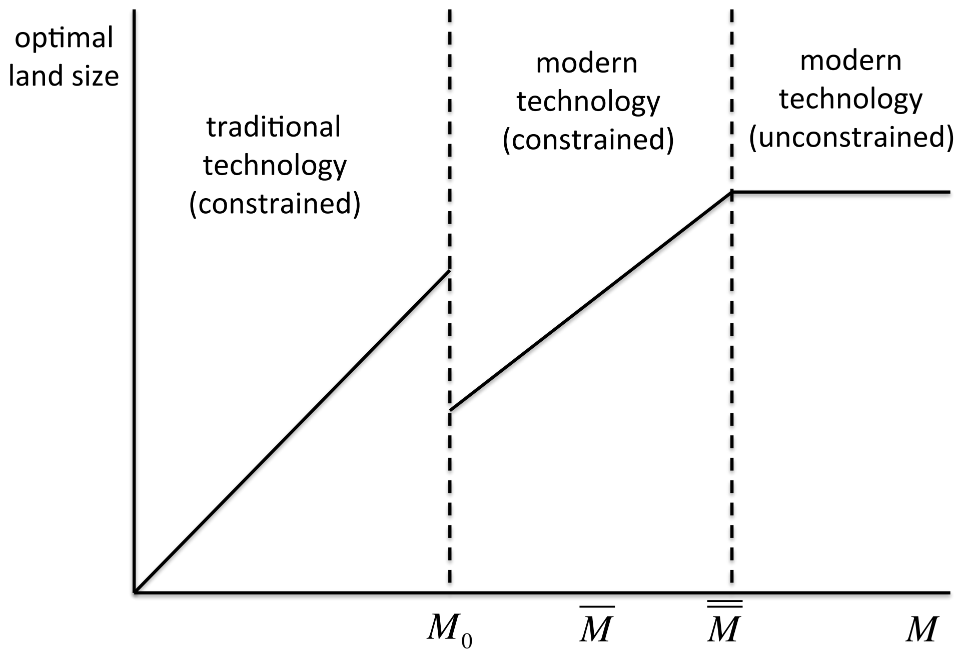
Notes: Figure illustrates the Amazon Biome border, as well as municipality limits for all municipalities in the states of Acre, Amazonas, Amapá, Maranhão, Mato Grosso, Pará, Rondônia, Roraima, and Tocantins (all of which are partly or entirely located in the Amazon biome). Our benchmark sample is composed of treatment (green) and control (blue) municipalities located within 100km of Amazon biome border. Alternative samples consider the distance-to-biome-border thresholds at 50km and 200km (not shown in figure). Frontier municipalities (grey) — those crossed by the biome border — are not included in any sample.

Figure 2: Deforestation in Control and Treatment Municipalities, 2003–2011



Notes: Figure shows municipality-level average deforestation in square kilometers from 2003 through 2011. The policy marker in 2008 helps separate pre- and post-policy trends. Data originally from PRODES/INPE.

Figure 3: Appendix A: Conceptual Framework Model — Optimal Farmland Size



Note: Figure illustrates optimal farmland size choice for setup in which $M_0 < \overline{M} < \overline{\overline{M}}$.

Table 1: Descriptive Statistics, Municipalities within 100 km to Amazon Biome Border, 2003 to 2011

	2003	2004	2005	2006	2007	2008	2009	2010	2011
Deforestation	23.874	26.021	25.662	6.601	8.489	12.002	5.325	4.514	2.879
	(57.979)	(62.618)	(62.646)	(16.490)	(24.258)	(28.001)	(10.856)	(12.320)	(6.892)
Total Credit	9.133	11.079	15.241	13.571	9.952	11.404	11.777	14.667	15.346
	(26.921)	(28.717)	(41.310)	(30.283)	(21.167)	(22.133)	(27.656)	(35.134)	(32.321)
Credit Cattle	3.071	3.608	4.171	5.353	5.011	5.527	4.930	6.689	7.672
	(5.649)	(6.526)	(7.335)	(7.569)	(7.730)	(8.129)	(8.284)	(10.803)	(11.940)
Credit Crop	6.062	7.472	11.070	8.218	4.941	5.877	6.847	7.978	7.675
	(25.492)	(27.154)	(39.779)	(29.096)	(17.273)	(18.789)	(25.278)	(31.728)	(29.357)
Number of small Cattle Contracts	175.514	321.343	363.571	634.114	398.760	240.989	164.794	179.617	166.954
	(346.277)	(491.452)	(486.185)	(1,137.839)	(750.608)	(362.000)	(232.415)	(241.615)	(264.141)
Number of Medium Cattle Contracts	46.109	70.657	82.846	122.863	140.040	81.543	41.137	106.971	105.057
	(126.175)	(268.967)	(210.523)	(226.415)	(298.218)	(142.683)	(71.820)	(159.428)	(151.476)
Number of Large Cattle Contracts	58.897	66.560	67.800	110.200	87.309	85.949	72.674	85.617	89.229
	(105.778)	(122.115)	(121.923)	(174.477)	(138.560)	(130.377)	(121.203)	(122.157)	(125.713)
Number of small Crop Contracts	243.903	377.097	432.497	763.389	552.137	337.486	225.069	304.840	307.086
	(468.074)	(666.772)	(618.486)	(1,263.721)	(942.991)	(462.196)	(256.151)	(311.682)	(358.376)
Number of Medium Crop Contracts	5.663	41.486	28.131	55.960	39.943	34.166	22.251	34.223	23.480
	(16.678)	(88.246)	(75.557)	(121.517)	(106.540)	(87.939)	(68.363)	(106.060)	(65.797)
Number of Large Crop Contracts	30.954	39.977	53.589	47.829	34.029	36.829	31.286	33.143	30.674
	(114.344)	(104.965)	(160.225)	(119.475)	(66.822)	(79.916)	(71.420)	(80.854)	(73.163)

Notes: Total Credit, Credit Cattle and Credit Crops are in Million of Reais. Deforestation is in Square Kilometers. Data from INPE and BACEN.

Table 2: Resolution 3,545's Impacts on Deforestation

	Dependent Variable: Deforestation					
	(1)	(2)	(3)	(4)	(5)	(6)
Resolution 3,545	-0.573	-0.584	-0.534	-0.529	-0.593	-0.382
	(0.098)***	(0.099)***	(0.099)***	(0.099)***	(0.127)***	(0.078)***
Observations	1,575	1,575	1,575	1,575	1,197	2,502
Number of municipalities	175	175	175	175	133	278
Municipality and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Agricultural Prices	No	Yes	Yes	Yes	Yes	Yes
Conservation Policies	No	No	Yes	Yes	Yes	Yes
Embargoed Areas and Fines	No	No	No	Yes	Yes	Yes
Sample	<100km	<100km	<100km	<100km	<50km	<200km

Notes: The dependent variable is normalized the deforestation increment at the municipality by year level, covering the 2003 through 2011 period. All regressions include municipality and year fixed effects. The sample includes all Legal Amazon municipalities that are not crossed by the Amazon Biome border and are within 100km of the biome border (columns 1-4), 50km (column 5) or 200km (column 6). Column 2 adds agricultural prices, column 3 adds concurrent environmental policies (municipal territory under protection and a dummy for priority municipalities), while column 4 also includes the log of the annual number of environmental fines applied at the municipality level in the previous year as well as the share of embargoed areas. Robust standard errors are clustered at the municipality level. Significance: *** p<0.01, ** p<0.05, * p<0.10.

Table 3: Resolution 3,545's Impacts on Deforestation: Robustness Checks

	Dependent Variable: Deforestation				
	(1)	(2)	(3)	(4)	(5)
Biome * Post 2009	-0.529		-0.469	-0.834	-0.858
	(0.099)***		(0.140)***	(0.167)***	(0.191)***
Biome * 2008			0.286		
			(0.178)		
Biome * 2007			0.132		
			(0.154)		
Biome * 2006			-0.101		
			(0.145)		
Biome * Time Trend		0.049			
		(0.039)			
Observations	1,575	1,050	1,575	1,575	1,575
Number of municipalities	175	175	175	175	175
Linear Trends	None	None	None	Biome	Municip
Years	All	<=2008	All	All	All
Sample	<100km	<100km	<100km	<100km	<100km

Notes: The dependent variable is normalized the deforestation increment at the municipality by year level. In all columns, the full list of fixed-effects and controls included is the same as the one used in the specification of column 4, Table 2. The samples include all Legal Amazon municipalities that are not crossed by the Amazon Biome border and are within 100km of the biome border. All samples cover the 2003 through 2011 period, except column 2, which covers only the 2003 through 2008 period. Robust standard errors are clustered at the municipality level. Significance: *** p<0.01, ** p<0.05, * p<0.10.

Table 4: Resolution 3,545's Impacts on Deforestation: Cattle vs Crop-Oriented Municipalities

	Dependent Variable: Deforestation	
	(1)	(2)
Resolution 3,545	-0.598	-0.216
	(0.119)***	(0.159)
Observations	1,269	306
Number of municipalities	141	34
Sample	Cattle-Oriented	Crop-Oriented

Notes: The dependent variable is normalized the deforestation increment at the municipality by year level. In all columns, the full list of fixed-effects and controls included is the same as the one used in the specification of column 4, Table 2. The samples include all Legal Amazon municipalities that are not crossed by the Amazon Biome border and are within 100km of the biome border. All samples cover the 2003 through 2011 period. The cattle-oriented sub-sample (column 1) is composed of municipalities in which the pre-2009 average value of annual credit loans for cattle ranching was higher than that for crop production; the crop-oriented sub-sample (column 2) is defined analogously. Robust standard errors are clustered at the municipality level. Significance: *** p<0.01, ** p<0.05, * p<0.10.

Table 5: Resolution 3,545's Mechanisms: Effects on Rural Credit Concessions

	Dependent Variable: Rural Credit Concessions					
	(1)	(2)	(3)	(4)	(5)	(6)
Resolution 3,545	-0.346	-0.436	-0.404	-0.397	-0.368	-0.363
	(0.127)***	(0.121)***	(0.121)***	(0.121)***	(0.136)***	(0.102)***
Observations	1,575	1,575	1,575	1,575	1,197	2,502
Number of municipalities	175	175	175	175	133	278
Municipality and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Agricultural Prices	No	Yes	Yes	Yes	Yes	Yes
Conservation Policies	No	No	Yes	Yes	Yes	Yes
Embargoed Areas and Fines	No	No	No	Yes	Yes	Yes
Sample	<100km	<100km	<100km	<100km	<50km	<200km

Notes: The dependent variable is normalized number of rural credit concessions at the municipality by year level, covering the 2003 through 2011 period. All regressions include municipality and year fixed effects. The sample includes all Legal Amazon municipalities that are not crossed by the Amazon Biome border and are within 100km of the biome border (columns 1-4), 50km (column 5) or 200km (column 6). Column 2 adds agricultural prices, column 3 adds concurrent environmental policies (municipal territory under protection and a dummy for priority municipalities), while column 4 also includes the log of the annual number of environmental fines applied at the municipality level in the previous year as well as the share of embargoed areas. Robust standard errors are clustered at the municipality level. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 6: Resolution 3,545's Effects on Rural Credit Concessions: Robustness Checks

	Dependent Variable: Rural Credit Concessions				
	(1)	(2)	(3)	(4)	(5)
Biome * Post 2009	-0.397		-0.401	-0.408	-0.462
	(0.121)***		(0.141)***	(0.164)**	(0.176)***
Biome * 2008			-0.138		
			(0.170)		
Biome * 2007			0.135		
			(0.177)		
Biome * 2006			0.010		
			(0.166)		
Biome * Time Trend		-0.019			
		(0.034)			
Observations	1,575	1,050	1,575	1,575	1,575
Number of municipalities	175	175	175	175	175
Linear Trends	None	None	None	Biome	Municip
Years	All	<=2008	All	All	All
Sample	<100km	<100km	<100km	<100km	<100km

Notes: The dependent variable is normalized number of rural credit concessions at the municipality by year level. In all columns, the full list of fixed-effects and controls included is the same as the one used in the specification of column 4, Table 2. The samples include all Legal Amazon municipalities that are not crossed by the Amazon Biome border and are within 100km of the biome border. All samples cover the 2003 through 2011 period, except column 2, which covers only the 2003 through 2008 period. Robust standard errors are clustered at the municipality level. Significance: *** p<0.01, ** p<0.05, * p<0.10.

Table 7: Resolution 3,545's Effects on Rural Credit Concessions by Loan Use, Size and Region

	Dependent Variable: Rural Credit Concessions (by Loan Use, Size and Region)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Resolution 3,545	-0.517	-0.105	-0.054	-0.165	-0.597	-0.132	-0.166	-0.059	-0.413	-0.588
	(0.126)***	(0.128)	(0.092)	(0.130)	(0.122)***	(0.128)	(0.115)	(0.125)	(0.140)***	(0.230)**
Observations	1,575	1,575	1,575	1,557	1,575	1,575	1,548	1,575	1,269	306
Number of municipalities	175	175	175	173	175	175	172	175	141	34
Use	Cattle	Crop	Cattle	Cattle	Cattle	Crop	Crop	Crop	All	All
Size	All	All	Small	Mid	Large	Small	Mid	Large	All	All
Cattle/Crop-Oriented	All	All	All	All	All	All	All	All	Cattle	Crop

Notes: The dependent variable is normalized number of rural credit concessions at the municipality by year level. In all columns, the full list of fixed-effects and controls included is the same as the one used in the specification of column 4, Table 2. The samples include all Legal Amazon municipalities that are not crossed by the Amazon Biome border and are within 100km of the biome border. All samples cover the 2003 through 2011 period. The dependent variables by size are the normalized number of credit contracts in each municipality categorized in groups according to agricultural activity (cattle ranching or crop farming) and contract size (small: up to the median; medium: between the median and the 75th percentile; and large: above the 75th percentile). The cattle-oriented sub-sample (column 9) is composed of municipalities in which the pre-2009 average value of annual credit loans for cattle ranching was higher than that for crop production; the crop-oriented sub-sample (column 10) is defined analogously. Robust standard errors are clustered at the municipality level. Significance: *** p<0.01, ** p<0.05, * p<0.10.

Table 8: The Effects of Rural Credit Concessions on Deforestation: OLS and 2SLS Estimates

	Dependent Variable: Deforestation				
	(1)	(2)	(3)	(4)	(5)
			Panel A: OLS		
Rural Credit	-0.009	-0.015	-0.010	-0.029	0.000
	(0.025)	(0.024)	(0.025)	(0.027)	(0.018)
			Panel B: 2SLS		
Rural Credit	1.656	1.321	1.331	1.612	1.051
	(0.691)**	(0.485)***	(0.495)***	(0.738)**	(0.380)***
Observations	1,575	1,575	1,575	1,197	2,502
Number of municipalities	175	175	175	133	278
Sample	<100km	<100km	<100km	<50km	<200km

Notes: The dependent variable is the normalized deforestation increment at the municipality by year level, covering the 2003 through 2011 period. In all columns, the full list of fixed-effects and controls included is the same as the one used in the specification of column 4, Table 2. The sample includes all Legal Amazon municipalities that are not crossed by the Amazon Biome border and are within 100km of the biome border (columns 1-3), 50km (column 4) or 200km (column 5). Panel B reports 2nd stage coefficients from 2SLS specifications where rural credit is instrumented by Resolution 3,545's ($Biome * Post2009$) Robust standard errors are clustered at the municipality level. Significance: *** p<0.01, ** p<0.05, * p<0.10.