THE EFFECT OF RURAL CREDIT ON DEFORESTATION: EVIDENCE FROM THE BRAZILIAN AMAZON[*]

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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 article studies the impact of this innovative policy on deforestation. Difference-in-differences estimations based on a panel of municipalities show 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.

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.[1] 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 article 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 3545, which made the concession of subsidised 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 3545 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 3545's conditions applied solely to landholdings inside the administrative definition of the Amazon biome, such

[1] 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).

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that properties outside the biome were not subject to the policy.[2] 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 100 km of the border, while alternative samples consider 50 km and 200 km 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 (INPE) under its Project for Monitoring Deforestation in the Legal Amazon (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 3545 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 3545 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 3545 on deforestation should directly reflect a reduction in access to rural credit. On the other hand, Resolution 3545 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 behaviour 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 behaviour 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 sizeable 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.

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[2] 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.

The overall evidence therefore suggests that Resolution 3545 has affected deforestation through a reduction in credit concessions. In our final exercise, we thus explore Resolution 3545 as a source of exogenous variation for credit concessions in a two-stage least squares (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 set-up, choices do not depend on the availability of income. 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 (Binswanger, 1991; Angelsen, 1999; Zwane, 2007). 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 subsidised credit sources and faced an exogenous variation in credit. We thus rely on Banerjee and Duflo (2014) and assume that the rationing in the availability of subsidised credit induced by Resolution 3545 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 generalise 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 (2013) 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 randomised experiment in which a sample of forest owners in Uganda was offered a Payment for Environmental Services (PES) contract. The author found suggestive evidence that facilitated access to credit can induce contract take-up and thus deter forest owners from cutting trees to meet emergency needs. Yet, the context in which deforestation occurs in the Brazilian Amazon differs from that of Uganda, where poverty, the reliance on subsistence agriculture, and presumably low returns to deforestation may explain why increased financial resources would contain stress-induced forest clearings. While forest peasant household activities are present in the Brazilian Amazon, particularly in association with logging and subsistence agriculture, it is commercial agriculture that drives most tropical forest conversion.[3]

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[3] See Coomes and Barham (1997) for an early assessment of the livelihood practices of forest peasant households and indigenous peoples in the Amazon.

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 article, 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 characterised 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 article – namely, one in which payments are conditional upon environmental deliveries—our results highlight the importance of sustained monitoring and enforcement of conditions for PES.

The remainder of the article is organised as follows. Section 1 describes the institutional context and Section 2 presents the data. Section 3 discusses the empirical strategy, focusing on the identification hypothesis. Section 4 presents the reduced-form effects of the policy change on deforestation. Section 5 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 analyse the relationship between credit constraints and deforestation.

1. Institutional Context

In February 2008, the Brazilian Central Bank published Resolution 3545, 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 3545 established that, in order to prove eligibility for accessing rural credit, the borrower had to present: (*i*) the Certificate of Registry of the Rural Establishment (CCIR), to meet legal titling requirements. The CCIR proves the property hosting the project to be financed is duly accounted for in federal registries, and (*ii*) a state-issued document attesting the environmental regularity of the establishment hosting the project to be financed, as well as a declaration attesting the property was not currently under any embargoes originating from illegal deforestation. In the Amazon, embargoes are an administrative sanction that can be applied to landowners as punishment for illegal forest clearings inside private property. Areas under embargo cannot be used for productive use. 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

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

attesting the establishment's environmental regularity could be replaced by a certificate stating a formal commitment to adapt to environmental regulations in the future. 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. The clearing of tropical vegetation by private parties in the Amazon is only legal if conducted inside private property and if the specific area to be cleared has been duly authorised or licensed by environmental authorities. Private landholders must also comply with the Brazilian Forest Code, which sets legal guidelines for land cover conversion and protection of native vegetation inside private properties. Because environmental regulation regarding deforestation inside private property is closely related to the property itself, obtaining legal titling rights are a natural first step in the environmental compliance process. In that sense, the requirement that access to credit should be made available only to those with legal property rights was a device to restrict credit to non-compliers, and thus curb illegal forest clearings.

Resolution 3545 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 3545 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 (MAPA, 2003).[5]

Although restrictive at first, Resolution 3545 was subject to a series of qualifications that eased conditions for the concession of rural credit for specific groups. In its original text, Resolution 3545 established exemptions for some groups of small-scale credit takers. The resolution allowed them to present self-declaratory environmental documentation instead of state-issued ones. A subset of these small producers were even entirely exempt from having to provide supporting documentation. Soon after the compulsory adoption of the resolution, new measures further loosened the requirements for the concession of rural credit to small-scale producers, mostly via the inclusion of new groups of small producers to the list of credit takers who were exempt from meeting the resolution's original requirements.

Resolution 3545'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 versus 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

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[5] 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 commercialisation 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 Brazilian Central Bank's annualised overnight rate of over 18% in the beginning of the 2000s, and over 8% in the early 2010s.

market operators.[6] 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 3545 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 3545 impacted access to credit and, thus, deforestation across different producers, sectors and regions. We explore these heterogeneities in our empirical analysis.

2. Data

Our analysis is based on a municipality-by-year panel data set covering the 2003 through 2011 period. We use a georeferenced 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 175 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 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.

2.1. *Data on Deforestation*

Data on deforestation are built from satellite-based images that are processed at the municipal level, and publicly released by PRODES/INPE.[7] INPE uses images from Landsat class satellites to detect forest clearings throughout the full extent of the Brazilian Legal Amazon (BLA) at an annual basis. Annual data generated via PRODES do not refer to a calendar year. For a given year t, PRODES records the area deforested between the 1st of August of $t-1$ and the 31st of July of

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[6] For instance, regarding the financial requirements of the soya bean 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).

[7] We use official data that are both processed and made publicly available by INPE on municipality-level deforestation increment. INPE (2013) provides a detailed account of PRODES methodology for processing deforestation data. In addition to being validated via internal accuracy checks (Adami *et al.*, 2018), PRODES data have systematically passed external accuracy tests that measure deforestation using different images and/or techniques (Souza *et al.*, 2013; Turubanova *et al.*, 2018).
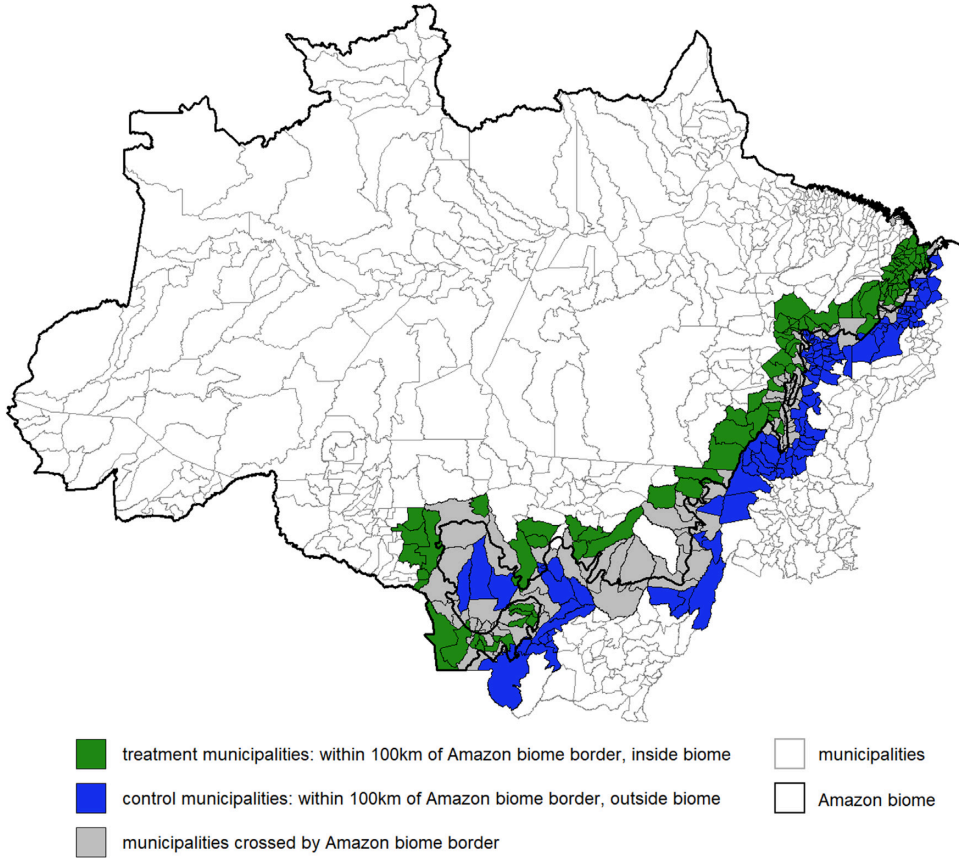
Fig. 1. *Brazilian Amazon Biome and Benchmark Sample.*
Notes: The figure illustrates the Amazon Biome border, as well as municipality limits for the states of
Acre, Amazonas, Amapá, Maranhão, Mato Grosso, Pará, Rondônia, Roraima, and Tocantins (all of which
are partly or entirely located in the Amazon biome). Our benchmark sample is composed of treatment
and control municipalities located within 100 km of Amazon biome border. Alternative samples consider
distance-to-biome-border thresholds of 50 km and 200 km (not shown in figure). Frontier municipalities—
those crossed by the biome border—are not included in any sample. (Figure available in colour online).

*t*rb illegal forest clearings. This measure is the deforestation increment for year *t*. We therefore
define deforestation as the area of forest in square kilometres cleared over the 12 months leading
up to August of a given year. We define deforestation as the area of forest in square kilometres
cleared over the 12 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 article accordingly, summing up monthly into annual data, where year
t refers to the 12 months leading up to August of *t*.

   To smoothen cross-sectional variation in deforestation that arises from municipality size
heterogeneity, we use a normalised measure of the annual deforestation increment. The

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[8] More precisely, the annual deforestation increment of year *t* measures the area in square kilometres deforested
between the 1st of August of *t* − 1 and the 31st of July of *t*.

normalisation ensures that our analysis considers relative variations in deforestation increments within municipalities.[9] The variable is constructed according to the following expression:

$$Deforest_{it} = (ADI_{it} - \overline{ADI}_{it})/(sd\,(ADI_{it})), \qquad (1)$$

where $Deforest_{it}$ is the normalised annual deforestation increment for municipality i and year t; ADI_{it} is the annual deforestation increment measured in municipality i between the 1st of August of $t-1$ and the 31st of July of t; and $\overline{ADI}_{it}$ and $sd(ADI_{it})$ are, respectively, the mean and the standard deviation of the annual deforestation increment calculated for each i over the 2003 through 2011 period.[10]

For any given municipality, clouds, shadows cast by clouds, and smoke may obstruct visibility of land surface in satellite imagery.[11] To control for measurement error, variables indicating unobservable areas are included in all regressions. These data are also publicly available at the municipality-by-year level from PRODES/INPE.

2.2. *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. 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 Brazilian currency (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 commercialisation). 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 match deforestation data, the relevant annual time window for our analysis is defined as the 12 months leading up to August of a given year (see Subsection 2.1 for details).

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

$$Credit_{it} = (C_{it} - \overline{C}_{it})/(sd\,(C_{it})), \qquad (2)$$

where $Credit_{it}$ is the normalised 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 $sd(C_{it})$ are, respectively, the mean and standard deviation of the amount of rural credit loaned in municipality i from 2003 through 2011. Table 1 summarises the data described above in the

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[9] See Subsection 6.3 for robustness checks using alternative normalisation procedures.

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

[11] PRODES uses imagery from the Amazon's dry season, during which visual obstructions are usually not a major issue. Still, if an area is blocked from view in a given image, it will likely be visible in the next batch of images, which, given Landsat satellites' revisit interval of 16 days, is typically only a couple of weeks later.

Table 1. *Descriptive Statistics for Deforestation and Rural Credit Data.*

	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: The table reports means and standard deviations (in parentheses) for benchmark sample. Units are BRL 1,000,000 for total credit, credit cattle, and credit crops; and square kilometres for deforestation. Data from PRODES/INPE and Brazilian Central Bank.

municipalities whose centroid is within 100 km of the Amazon Biome border from 2003 to 2011.[12]

2.3. *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 and Municipal Livestock Survey.[13] 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 3545 on credit and, consequently, on deforestation, is isolated from confounding effects of other concurrent conservation efforts.

3. Empirical Strategy

In this article we evaluate Resolution 3545'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 3545 with annual data at the municipality-by-year level to design a difference-in-differences evaluation approach.

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[12] Appendix B replicates Table 1, but present statistics for the normalised outcome variables both for the entire benchmark sample (Table B1) and separately for treatment (Table B2) and control (Table B3) municipalities.

[13] Paraná prices are integrated with international commodity prices and can be interpreted as the benchmark taken by local producers. More generally, our results remain robust if Paraná prices are replaced by international prices in the construction of control variables for commodities prices (results upon request).

More specifically, we identify the reduced-form effects of Resolution 3545 on deforestation by estimating the following regression:

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

where $Deforest_{it}$ is the normalised 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 3545, $Post2009_t$. This latter variable indicates all years from 2009 onwards.[14] 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. These variables absorb potentially confounding effects of the most relevant conservation efforts conducted alongside the implementation of Resolution 3545.[15] 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 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.[16] Although all sample municipalities are part of the BLA, treatment and control groups are officially located in different biomes. The Brazilian territory is divided into six biomes, which are officially defined in ecological, geographical, and climatic terms. The Amazon biome covers half of the country, and shares nearly the entirety of its southeast border with the *Cerrado* biome (IBGE, 2004). As depicted in Figure 1, this border crosses the BLA, which is a socio-political division that has been used since the early 1950s to territorially define and apply policy efforts.[17] Indeed, the pivotal conservation effort of the 2000s, the Action Plan for the Prevention and Control of Deforestation in the Legal Amazon (PPCDAm), was designed to cover the full extent of the BLA. This reflects the federal government's concern regarding the need to protect native tropical vegetation within the BLA as a whole, and not only inside

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[14] 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.

[15] See Section 6 for a more detailed discussion on the Brazilian Amazon context and robustness checks regarding the validity of our approach in light of potential dynamic and spillover effects of protected territory, blacklisting, and law enforcement.

[16] In Sections 4 and 5 we show that our results remain robust to changes in the distance-to-biome-border threshold and in their respective samples of municipalities.

[17] The BLA encompasses the entire states of Acre, Amapá, Amazonas, Mato Grosso, Pará, Rondônia, Roraima, and Tocantins, as well as the western part of Maranhão state.
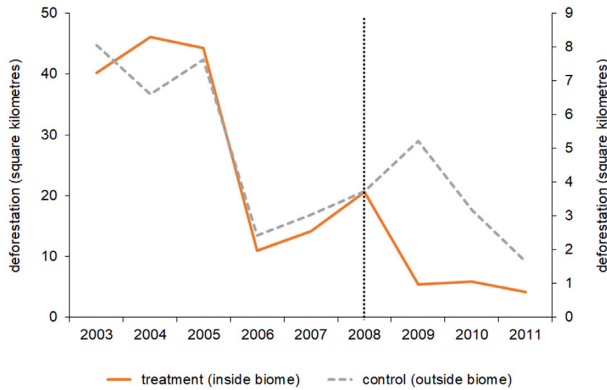
Fig. 2. *Deforestation in Treatment and Control Municipalities.*
Notes: The figure shows deforestation trends for average municipal deforestation in square kilometres over the sample period (2003 through 2011). The policy marker at 2008 helps separate pre- and post-policy trends. Data from PRODES/INPE.

the Amazon biome. In fact, Resolution 3545 stands out amongst the set of PPCDAm policies because of its restriction to the Amazon biome.

The practice of using the entire BLA to target policy efforts is consistent with the fact that, despite the sharp borders that officially define the biomes, on the ground, the Amazon biome gradually transitions into the *Cerrado* biome. This transition is largely characterised by the presence of tropical forest, which gradually changes from dense to open ombrophilous vegetation as one moves towards the BLA's southeast outer limits and enters the Brazilian savannah. This results in a mix of tropical and savannah-like vegetation in areas near the biomes' frontier, with occurrences of non-tropical vegetation inside the Amazon biome, as well as that of tropical vegetation outside it. Drawing on PRODES satellite data, which also covers the full extent of the BLA, we observe that, in 2008, from more than 105 thousand square kilometres of tropical forest remaining in our benchmark sample, roughly a third was located outside the Amazon biome. Control municipalities therefore held an area about the size of Belgium in tropical Amazon forest in the pre-policy period. In restricting our benchmark sample to municipalities that are within 100 kilometres from the biome border, whether inside or outside the biome, we are thus selecting municipalities that are more likely to be similarly suitable to tropical forest growth and deforestation.

The parameter of interest β_1 in equation (3) captures the causal effect of Resolution 3545 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 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. After

a record peak in 2004, we observe downward trends in forest clearings in both control and treatment municipalities through 2006, followed by upward trends through 2008. This pattern is quite similar to the variation in agricultural commodity prices observed over the same period, which has been shown to have been a significant driver of Amazon deforestation in the second half of the 2000s (Assunção *et al.*, 2015). Combined with the fact that agriculture accounts for a relevant share of cleared areas in the BLA, these patterns suggest that forest clearings on either side of the biome border were driven by a similar set of economic factors. Yet, deforestation in control and treatment municipalities exhibit divergent behaviour immediately after the implementation of Resolution 3545, with forest clearings in treatment municipalities dropping sharply.

Having explored the reduced-form impact of Resolution 3545 on deforestation, we thus investigate its two potential mechanisms. Resolution 3545 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 3545 on deforestation should directly reflect a reduction in access to rural credit.

On the other hand, Resolution 3545 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 behaviour 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 behaviour 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 3545 on credit. The estimation is based on the following equation:

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

where $Credit_{it}$ is the normalised 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.

4. Policy Impact on Deforestation

We start by testing whether Resolution 3545 affected deforestation in the Amazon. After discussing the reduced-form effects, we present robustness checks and explore heterogeneities.

4.1. *Main Results*

Table 2 presents estimated coefficients for Resolution 3545's effect on forest clearings based on equation (3). Controls are added gradually. Column 1 includes only municipality and year

Table 2. *Resolution 3545's Effects on Deforestation.*

	Dependent variable: normalised deforestation					
	(1)	(2)	(3)	(4)	(5)	(6)
Resolution 3545	− 0.573	− 0.584	− 0.534	− 0.529	− 0.556	− 0.382
	(0.098)***	(0.099)***	(0.099)***	(0.099)***	(0.131)***	(0.078)***
Observations	1,575	1,575	1,575	1,575	900	2,502
Number of municipalities	175	175	175	175	100	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	<100 km	<100 km	<100 km	<100 km	<50 km	<200 km

Notes: The sample includes all BLA municipalities that are not crossed by the Amazon Biome border and that are within 100 km of the biome border (columns 1 through 4). Columns 5 and 6 report coefficients estimated using alternative samples composed of BLA municipalities that are not crossed by the Amazon Biome border and that are within 50 km or 200 km of the biome border, respectively. All columns cover the 2003 through 2011 period. Robust standard errors are clustered at the municipality level. Significance: $***p < 0.01$, $**p < 0.05$, $*p < 0.10$.

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 3545, 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, sizeable, 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 (50 km), and a smaller but still sizeable coefficient in the larger sample (200 km). This pattern is to be expected in light of the fact that the southeast region of the Amazon biome, where our samples are located, largely overlaps with the so-called Arc of Deforestation, a region encompassing areas that have seen acute deforestation and that includes the Amazon agricultural frontier. 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 3545 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 $(Biome_i \times Post2009_t) = 0$. This calculation delivers the predicted municipality trend in annual deforestation in a hypothetical scenario in which Resolution 3545 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 3545, total deforestation would have been 2,000 square kilometres greater than the actually observed from 2009 through 2011 in the 100 km sample of municipalities, which

Table 3. *Resolution 3545's Effects on Deforestation: Robustness Checks.*

	(1)	(2)	(3)	(4)	(5)
	Dependent variable: normalised deforestation				
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	Municipality
Years	All	≤2008	All	All	All
Sample	<100 km	<100 km	<100 km	<100 km	<100 km

Notes: All columns include the full list of fixed effects and controls used in the benchmark specification (Table 2, column 4). The sample includes all BLA municipalities that are not crossed by the Amazon Biome border and that are within 100 km of the biome border. Column 2 covers the 2003 through 2008 period; all other columns cover the 2003 through 2011 period. Robust standard errors are clustered at the municipality level. Significance: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

represents a reduction of 60% if one considers the baseline deforestation in 2008. Resolution 3545 has therefore played an important role in curbing forest clearings in the Amazon biome in the late 2000s.

4.2. *Pre-Trends and Validity of Empirical Approach*

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 3545.

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 3545. 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

Table 4. *Resolution 3545's Effects on Deforestation: Cattle vs Crop-Oriented Municipalities.*

	Dependent variable: normalised deforestation	
	(1)	(2)
Resolution 3545	− 0.598	− 0216
	(0.119)***	(0.159)
Observations	1,269	306
Number of municipalities	141	34
Sample	Cattle-oriented	Crop-oriented

Notes: All columns include the full list of fixed effects and controls used in the benchmark specification (Table 2, column 4). The sample includes all BLA municipalities that are not crossed by the Amazon Biome border and that are within 100 km of the biome border. 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. All columns cover the 2003 through 2011 period. Robust standard errors are clustered at the municipality level. Significance: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

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.

4.3. *Heterogeneity*

The evidence presented so far indicates that Resolution 3545 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 3545, 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 3545 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.

5. Mechanisms

Having explored the reduced-form impact of Resolution 3545 on deforestation, we now investigate its potential mechanisms. If requirements regarding legal titling of land were immediately binding, the effects of Resolution 3545 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 behaviour for reasons other

Table 5. *Resolution 3545's Effects on Rural Credit Concessions.*

	Dependent variable: normalised rural credit concessions					
	(1)	(2)	(3)	(4)	(5)	(6)
Resolution 3545	− 0.346	− 0.436	− 0.404	− 0.397	− 0.317	− 0.363
	(0.127)***	(0.121)***	(0.121)***	(0.121)***	(0.148)**	(0.102)***
Observations	1,575	1,575	1,575	1,575	900	2,502
Number of municipalities	175	175	175	175	100	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	<100 km	<100 km	<100 km	<100 km	<50 km	<200 km

Notes: The sample includes all BLA municipalities that are not crossed by the Amazon Biome border and that are within 100 km of the biome border (columns 1 through 4). Columns 5 and 6 report coefficients estimated using alternative samples composed of BLA municipalities that are not crossed by the Amazon Biome border and that are within 50 km or 200 km of the biome border, respectively. All columns cover the 2003 through 2011 period. Robust standard errors are clustered at the municipality level. Significance: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

than a direct reduction in credit. As argued in Section 3, 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 3545 on rural credit loans. Analogously to the latter section, we also present robustness checks and explore heterogeneities.

5.1. *Main Results*

Table 5 presents estimated coefficients for regressions based on equation (4). Again, controls are added gradually. The results indicate that Resolution 3545 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 3545's impacts in particular. This suggests that Resolution 3545 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 3545 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.[18] We estimate that Resolution 3545 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.

5.2. *Pre-Trends and Validity of Empirical Approach*

As discussed in Subsection 4.2, 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

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[18] This exercise is analogous to the one performed in Subsection 4.1, but now based on coefficients from Table 5, column 4.

Table 6. *Resolution 3545's Effects on Rural Credit Concessions: Robustness Checks.*

	Dependent variable: normalised rural credit concessions				
	(1)	(2)	(3)	(4)	(5)
Biome × Post 2009	− 0.397 (0.121)***		− 0.401 (0.141)***	− 0.408 (0.164)**	− 0.462 (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	Municipality
Years	All	≤2008	All	All	All
Sample	<100 km	<100 km	<100 km	<100 km	<100 km

Notes: All columns include the full list of fixed effects and controls used in the benchmark specification (Table 2, column 4). The sample includes all BLA municipalities that are not crossed by the Amazon Biome border and that are within 100 km of the biome border. Column 2 covers the 2003 through 2008 period; all other columns cover the 2003 through 2011 period. Robust standard errors are clustered at the municipality level. Significance: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

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 3545. 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 3545 remain negative and significant throughout, and are even slightly larger in absolute terms in both tests.

5.3. *Heterogeneity*

We now test whether Resolution 3545 had differential effects across different types of credit contracts. We start by separating credit to be used in crop farming versus cattle ranching activities. The first column of Table 7 shows that Resolution 3545'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 1. 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 3545.

Table 7. *Resolution 3545's Effects on Rural Credit Concessions: By Loan Use, Size, and Region.*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Dependent variable: normalised rural credit concessions (by loan use, size, and region)									
Resolution 3545	−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)	(0115)	(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
Loan use	Cattle	Crop	Cattle	Cattle	Cattle	Crop	Crop	Crop	All	All
Loan size	All	All	Small	Medium	Large	Small	Medium	Large	All	All
Cattle-crop-oriented	All	All	All	All	All	All	All	All	Cattle	Crop

Notes: All columns include the full list of fixed effects and controls used in the benchmark specification (Table 5, column 4). The sample includes all BLA municipalities that are not crossed by the Amazon Biome border and that are within 100 km of the biome border. The dependent variables by loan use and size are the normalised number of credit contracts in each municipality categorised into 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. All columns cover the 2003 through 2011 period. Robust standard errors are clustered at the municipality level. Significance: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

We also investigate the policy impact on concessions by loan size. The dependent variable is now the number of credit contracts in each municipality categorised 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).[19] Given that small-scale producers benefited from less stringent conditions for credit concession, we expect Resolution 3545 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 3545. Together with the results from Table 4, this suggests that access to credit and deforestation are particularly correlated in cattle-ranching activities.

6. Robustness Checks

6.1. *Concurrent Conservation Policies*

As detailed in Section 3, our benchmark specification includes a series of control variables to absorb the confounding influence of concurrent environmental policies. In this section, we run additional robustness checks to further test the validity of our empirical approach in light of these policies' potential dynamic and spillover effects. We also provide a more detailed discussion regarding localised conservation efforts.

In the second half of the 2000s, the Brazilian federal government sought to actively inhibit tropical forest clearings and promote forest conservation by launching a series of conservation policies. Two such policies stand out for having been executed alongside Resolution 3545—the strengthening of monitoring and law enforcement, and the expansion of protected areas (PAs). The strengthening of monitoring and law enforcement was largely due to the adoption, in the mid-2000s, of remote sensing-based monitoring technology in the Real-Time System for Detection of Deforestation (DETER). The system captures and processes satellite imagery on forest cover to locate recent deforestation hot spots and issue associated georeferenced alerts, which serve as the basis for targeting law enforcement activities in the Amazon. Enforcement was further enhanced with the blacklisting of municipalities at the end of the decade. Municipalities classified as blacklisted were those in need of priority action to prevent, monitor, and combat illegal

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[19] Per Brazilian legislation, a small rural property in our sample is defined as a landholding smaller than 75 hectares. The contract-level data used in our analysis, however, do not contain information on property size. We therefore use loaned amounts as an approximation, since properties are used as collateral in rural credit contracts and, as such, smaller properties are typically associated with smaller contracts.

Table 8. *Robustness Checks: Concurrent Conservation Policies.*

	(1) Benchmark	(2) No policy controls	(3) Exclude if policy	(4) Neighbours	(5) Dynamics & roads
Panel A: Dependent variable: normalised deforestation					
Biome × Post 2009	− 0.529	− 0.584	− 0.489	− 0.504	− 0.591
	(0.099)***	(0.099)***	(0.110)***	(0.099)***	(0.104)***
Panel B: Dependent variable: normalised rural credit concessions					
Biome × Post 2009	− 0.397	− 0.436	− 0.419	− 0.399	− 0.351
	(0.121)***	(0.121)***	(0.129)***	(0.120)***	(0.123)***
Observations	1,575	1,575	1,224	1,539	1,575
Number of municipalities	175	175	136	171	175
Municipality and year FE	Yes	Yes	Yes	Yes	Yes
Agricultural prices	Yes	Yes	Yes	Yes	Yes
Conservation policies	Yes	No	No	Yes	Yes
Sample	<100 km	<100 km	<100 km	<100 km	<100 km

Notes: Column 1 replicates benchmark specifications (Tables 2 and 5, column 4). All remaining columns are variations of this benchmark specification as follows: exclusion of benchmark controls for concurrent conservation efforts (column 2); exclusion of municipalities that experienced variation in concurrent policies during the period of analysis (column 3); inclusion of controls for conservation policies implemented in contiguous neighbouring municipalities (column 4); and inclusion of linear time trends for paved/unpaved roads, as well as interaction terms between these trends and conservation policies (column 5). The sample includes all BLA municipalities that are not crossed by the Amazon Biome border and that are within 100 km of the biome border. All columns cover the 2003 through 2011 period. Robust standard errors are clustered at the municipality level. Significance: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

deforestation. The first blacklist was issued in early 2008, and included 36 priority municipalities. Differential action taken in these localities consisted of more rigorous environmental monitoring and law enforcement. Parallel to enforcement efforts, the creation of PAs gained momentum in the mid-2000s. From 2004 through 2009, total protected area in the BLA increased by over 520,000 square kilometres. By the end of the decade, approximately 43% of BLA territory was under protection.

The existing literature has provided compelling evidence on the effectiveness of these conservation efforts, prompting us to control for the direct effect of PAs, environmental law enforcement, and municipality blacklisting in our benchmark specifications.[20] However, recent studies have further documented that the impact of conservation efforts may extend beyond direct effects and programme borders, as inputs can be spatially reallocated and interventions can interact with both geographical features and other policies (Herrera, 2015; Andrade, 2016; Pfaff and Robalino, 2017; Robalino *et al.*, 2017). Hence, in this section, we test whether our estimates are robust to the potential influence of dynamic and spillover effects triggered by PAs and enforcement operations.

Table 8 presents the results. Panel A displays estimates for deforestation as dependent variable, while Panel B reports results for credit. The first column replicates our benchmark specifications—the first column of Panels A and B are identical to column 4 of Tables 2 and 5, which present coefficients for deforestation and credit, respectively. In column 2 we exclude our benchmark controls for other conservation efforts, namely, the share of municipal area under protection, a dummy that indicates blacklisted municipalities, and the lag of the annual number

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[20] See, amongst others: Börner *et al.* (2015a); Cisneros *et al.* (2015); Assunção *et al.* (2017); Assunção and Rocha (2019) for evaluations of law enforcement impacts, and Andam *et al.* (2008); Pfaff *et al.* (2014; 2015a,b) for assessments of protected territory.

of environmental fines issued at the municipality level in the previous year. We observe that estimates slightly increase in module, but remain statistically stable.

In column 3, we check whether our results are robust to sample selection once we exclude municipalities that experienced variation in concurrent policies during the period of analysis. More specifically, we exclude from our sample all municipalities that experienced any change in PA coverage at any time during our period of analysis.[21] We also exclude the nine sample municipalities that were blacklisted by the federal government (eight were blacklisted in 2008, one was blacklisted in 2009). Finally, we exclude municipalities that concentrated enforcement activity. To do this, we consider the municipality-level distribution of the number of environmental fines. This distribution is skewed, with most of the fines concentrated in a small number of municipalities. We therefore exclude municipalities above the 75th percentile of the fines distribution, as computed in the baseline year (2008). Because we consider a one-year lag for our enforcement variable, municipalities where more than two fines were issued in 2007 were dropped from the sample.[22] Overall, we observe point estimates and standard errors that are similar to our benchmark results. This indicates that our findings are not driven by specific trends or dynamics from relevant concurrent conservation efforts.

We also test for the presence of confounding spillover effects in column 4 of Table 8. Pfaff and Robalino (2017) argue that input reallocation can move forest clearings beyond programme borders. In particular, there is empirical evidence that PAs interact spatially with other policies and economic dynamics, and thereby affect private land use choice in unprotected territory (Herrera, 2015). The same reasoning applies to enforcement operations, which could account for documented spillover effects from blacklisting on neighbouring municipalities (Andrade, 2016). We address these issues by adding control variables to our benchmark specification that absorb the potentially confounding effects of conservation policies implemented in contiguous neighbouring municipalities. More specifically, we include the average share of municipal area under protection and the total number of environmental fines issued the previous year in neighbouring municipalities, as well as the number of contiguous neighbouring blacklisted municipalities in each year. Again, we observe that point estimates and standard errors remain stable in both panels.

Finally, we test for dynamic effects by interacting policies with road density, which has been widely acknowledged as one of the most relevant determinants of deforestation in the Amazon region (Chomitz and Gray, 1996; Angelsen and Kaimowitz, 1999; Pfaff, 1999; Chomitz and Thomas, 2003; Pfaff *et al.*, 2007). Although road density is expected to be constant in the short time window around the adoption of Resolution 3545, and therefore absorbed in fixed-effects, it may interact with conservation efforts to generate potentially confounding dynamic trends. Thus, in column 5, we include interaction terms between a linear time trend in roads and concurrent conservation policies. More specifically, for each municipality, we first build interaction terms between a linear time trend and the density of roads (kilometres per area) in 2010, separating paved and unpaved roads. We then interact these specific trends with the share

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[21] From a total of 175 sample municipalities, we only observe variation in PAs in two municipalities in 2006, three municipalities in 2007, and four municipalities in 2011. We do not observe any variation in PAs in our benchmark sample from 2008 through 2010, which includes the period during which Resolution 3545 was implemented.

[22] The determinants and effectiveness of enforcement inspections depend on local characteristics. In particular, inspections tend to be more frequent, and their costs lower, in areas with relatively lower travel times. As such, low enforcement costs occur in locations with low access costs (Börner *et al.*, 2014; 2015b). Yet, because travel times and access costs are reasonably fixed in a short panel of Amazon municipalities, they should be absorbed in municipality fixed-effects.

of municipal area covered by PAs, the municipality-level number of environmental fines, and the blacklisted municipality dummy. Hence, in column 5, we add both specific time trends in roads and their interactions with policies to our benchmark specification. We observe statistically similar point estimates. Overall, results from Table 8 strengthen the case for our estimates' robustness to the presence of other relevant concurrent conservation policies and their dynamic effects.

The policies addressed thus far were targeted at the entire BLA. Yet, the Brazilian Amazon is a dynamic and complex setting, which has seen multiple conservation initiatives be undertaken by a variety of actors, including NGOs, local governments, and private stakeholders. Such initiatives need not be heterogeneous in nature and spatial extent. While it is difficult to systematically assess the effects of this assortment of local and heterogeneous initiatives, we argue that, technically, there is no reason to expect our estimates would suffer from any confounding influence from such interventions. First, our empirical exercises are robust to outliers, both because we use standardised dependent variables, and because we tested specifications based on alternative dependent variables—all of which delivered robust estimates (see Subsection 6.3). Second, localised initiatives are often a response to external and federal pressure, which have already been controlled for in our empirical models. For instance, the municipality of Paragominas launched a local initiative to restrict deforestation as a response to having been blacklisted (Sills *et al.*, 2015). Indeed, in 2010, it was the first municipality to exit the blacklist. Yet, although Paragominas is a well-known and apparently successful local initiative, it is ultimately endogenous to blacklisting, and, as shown in Table 8, our results remain robust across samples and controls for blacklisted municipalities.

Furthermore, to the best of our knowledge, there was no coordinated scaling-up of site-specific conservation efforts on either side of the Amazon Biome around the time Resolution 3545 was implemented. At first sight, this claim could be challenged by the also well-known Programa Municípios Verdes (PMV), a programme launched by Pará state that sought to engage municipal governments in actions to reduce deforestation and thereby comply with environmental goals set by national policies. However, PMV was officially launched only in 2011, when our period of analysis ends. Moreover, according to an assessment of the programme's impact:

> (...) while all but one of the blacklisted municipalities in the PMV are actively engaged in implementing the programme, 85% of the non-blacklisted municipalities in the PMV have made little progress implementing the programme. We detect no effect of the programme on these non-blacklisted municipalities who are part of the PMV on paper only, while actively implementing the programme appears to have caused very small reductions in deforestation in non-blacklisted municipalities.
>
> Sills *et al.* (2015, p. 6)

Hence, there does not seem to be any particular reason why one should expect a confounding effect of PMV on our estimates. Similar reasoning applies to the new Brazilian Forest Code and the federal Rural Environmental Registry (CAR), the most relevant property rights and land tenure-related efforts concerning our sample region. Both were enacted in 2012 and therefore do not overlap with our sample period. While some states did have CAR-like state systems prior to 2012, such systems provided little to no coverage of rural properties at the time Resolution 3545 was implemented. For Pará, for example, less than 0.1% of the estimated private property area was registered in the state system by late 2007 (TNC, 2015). In light of this, one should not expect our estimates to have been driven by a confounding effect of changes in municipal land tenure structures.

6.2. *Potential Leakage*

There are multiple mechanisms through which conservation programmes may impact areas beyond originally targeted territory (Pfaff and Robalino, 2017). Particularly relevant to our context are concerns that spillovers could arise through input reallocation and/or general equilibrium changes in market prices. Input reallocation or deforestation slippage occurs when:

> (...) profit maximisers facing restrictions on the use of some of their land may alter uses of other parts of their land. Thus, even if a conservation programme that imposes some restrictions generates an impact relative to baseline on forest lands that are treated, it could still have no net impact if reoptimisation leads to an equal amount of slippage, i.e., above-baseline clearing of other lands owned by the affected landowner.
>
> (Pfaff and Robalino, 2017, p. 301)

Regarding market prices, on the other hand:

> (...) conservation program can shift the supplies of agricultural and forest goods, which in turn, can shift the demands for inputs into the production of those goods, such as labour and capital. Shifts in quantities, if sufficiently large, can shift relative scarcities enough to bring about changes in market prices, which can then affect land use outside of the program area. The price shifts can generate relocations of production to untreated lands.
>
> (Pfaff and Robalino, 2017, p. 302)

Similarly, Alix-Garcia *et al.* (2012) note that:

> (...) even if forest conservation programs do induce additional conservation on enrolled lands, these benefits may be undermined by new deforestation in other locations. In the context of forest-conservation payments, a substitution slippage effect occurs when a landowner who removes one parcel of land from production (enrolling it in the program) shifts the planned production to another parcel within his landholdings. An output price slippage effect occurs if the removal of multiple parcels of land from production or the introduction of payments alters market prices and these changes induce additional deforestation.
>
> Alix-Garcia *et al.* (2012, pp. 3–4)

In light of this, it is important to examine whether leakage is an issue in our empirical setting. We begin with qualitative remarks. According to Alix-Garcia *et al.* (2012), whether or not these changes will manifest spatially close to enrolled lands depends on the size of the relevant markets. The authors focus on a PES scheme, and theoretically demonstrate that, for credit constrained households, the introduction of a PES programme could: (*i*) perversely increase deforestation in other locations within the household's property through a substitution effect, and (*ii*) increase deforestation in other locations outside the household's property through output price effects. The standard channel for the latter effect would be an increase in prices resulting from a decrease in the supply of the agricultural good. Ultimately, the authors note that these effects will be detectable only where markets are localised, for instance in the presence of poor road infrastructure. While this reasoning is both theoretically and empirically sound for nearby areas (e.g., within the same property or in its immediate surroundings), it may become less relevant if individuals are unable to rapidly reallocate inputs across large areas and less integrated markets (e.g., across areas that are hundreds of kilometres apart, on the other side of the biome border). Moreover, different to a PA initiative and opposite to a PES scheme, Resolution 3545 imposed a tighter restriction on access to credit, potentially restricting reallocation decisions.

Still, it is theoretically possible that land use restrictions inside the Amazon Biome led private actors who were restricted to respond by shifting production to lands outside the biome. We address this empirically open question by performing two sets of exercises. First, we provide

descriptive figures on access to credit based on identified data. To do this, we placed an additional request for non-anonymous contract-level data at the Brazilian Central Bank, enabling us to use identification codes for farmers across credit contracts. It is important to highlight that rural credit contracts in Brazil must explicitly indicate the specific property that will host the agricultural activity to be financed. We then counted the number of farmers with contracts referring to properties inside the biome over the 2003 through 2008 (pre-policy) period, and, from these individuals, counted the number of farmers with contracts referring to properties outside the biome after Resolution 3545 was implemented through 2011. We observe that, out of 62,421 farmers who had any pre-policy contracts inside the biome, only 108 individuals had a contract referring to a property outside the biome after Resolution 3545 was in place. Interestingly, we also find that, out of 60,091 farmers who had any pre-policy contracts outside the biome, only 106 had a contract referring to a property inside the biome after Resolution 3545 was in place. While this symmetry indicates that mobility in credit loans is not correlated with the resolution, the rather small number of operations that moved across the border is consistent with farmers being unable to rapidly reallocate inputs across distant areas in the Brazilian Amazon.

Second, while direct slippage effects do not seem to be a relevant issue in our context, it is true that changes in the supply of agricultural goods inside the Amazon biome could potentially shift the demand for inputs for the production of those goods, such as labour and capital. As mentioned by Pfaff and Robalino (2017), if sufficiently large, shifts in quantities can change relative scarcities enough to bring about changes in market prices and spillover effects on deforestation. Ultimately, if this mechanism is active in our context and Resolution 3545 freed up workers inside the biome, we should observe an increase in labour mobility across the border. To test this hypothesis, we examine whether population density (the log of total population per municipal area) changed inside versus outside the biome after Resolution 3545. More specifically, we test whether population responds to Resolution 3545 by running our benchmark specification using the log of total population per municipal area as dependent variable. We find an insignificant coefficient of −0.02 (standard error of 0.02). Combined, the evidence suggest that leakage is not a significant issue in our empirical setting.

6.3. *Additional Robustness Checks*

Our choice of standardisation technique for outcome variables aimed at making units of analysis comparable and the interpretation of estimated coefficients as straightforward as possible. Nevertheless, there are alternative ways of doing so. Table 9 presents results for a variety of robustness checks based on different normalisation procedures. Panel A displays estimates for deforestation, while Panel B reports results for credit. Column 1 replicates our benchmark specifications, so coefficients shown in Panels A and B are identical to those of column 4 in Tables 2 and 5, respectively. In column 2, the standardisation is built by subtracting units' averages from municipality-year outcomes and dividing by the unit average; in column 3, we divide by municipal area. We observe that coefficients are robust across alternative specifications. The magnitude of the coefficients are also relevant. If we divide the coefficients in column 2 by the standard deviations of their respective dependent variable, we find −0.513 and −0.397, which are both quite similar to our benchmark coefficients of −0.529 and −0.397 for deforestation and credit, respectively. The same exercise for column 3 returns −0.697 and −0.417, indicating a relatively larger effect for deforestation and a similar one for credit, respectively.

Table 9. *Robustness Checks: Alternative Normalisation Procedures.*

	Benchmark (1)	Divide by average (2)	Divide by area (3)	Divide by forest 2008 (4)	ln(.) (5)
Panel A: Dependent variable: normalised deforestation					
Biome × Post 2009	− 0.529	− 0.763	− 0.005	− 0.039	− 0.704
	(0.099)***	(0.203)***	(0.001)***	(0.013)***	(0.112)***
Pre-policy descriptive statistics					
DepVar mean	0.1060	0.0637	0.0003	0.0504	1.7560
DepVar SD	0.9260	1.3630	0.0065	0.1290	1.7490
DepVar mean, treatment	0.1660	0.1390	0.0007	0.0446	2.4580
DepVar SD, treatment	0.9090	1.1590	0.0079	0.0571	1.8590
DepVar mean, control	0.0480	− 0.0087	− 0.0001	0.0561	1.0770
DepVar SD, control	0.9380	1.5320	0.0048	0.1711	1.3200
Observations	1,575	1,575	1,575	1,575	1,575
Number of municipalities	175	175	175	175	175
Panel B: Dependent variable: normalised rural credit concessions					
Biome × Post 2009	− 0.397	− 0.001	− 0.002	− 0.001	− 0.182
	(0.121)***	(0.000)**	(0.001)***	(0.000)**	(0.063)***
Pre-policy descriptive statistics					
DepVar mean	− 0.1510	0.0005	0.0010	0.0007	1.9660
DepVar SD	0.9440	0.0027	0.0052	0.0044	1.4230
DepVar mean, treatment	− 0.0936	0.0006	0.0011	0.0009	2.1250
DepVar SD, treatment	0.9580	0.0029	0.0056	0.0057	1.3670
DepVar mean, control	− 0.2060	0.0005	0.0008	0.0005	1.8130
DepVar SD, control	0.9270	0.0025	0.0047	0.0027	1.4600
Observations	1,575	1,575	1,575	1,566	1,575
Number of municipalities	175	175	175	174	175
Municipality and year FE	Yes	Yes	Yes	Yes	Yes
Agricultural prices	Yes	Yes	Yes	Yes	Yes
Conservation policies	Yes	Yes	Yes	Yes	Yes
Sample	<100 km	<100 km	<100 km	<100 km	<100 km

Notes: Column 1 replicates benchmark specifications (Tables 2 and 5, column 4). All remaining columns are analogous to these benchmark specifications, but use alternative normalisations for the dependent variables as follows: divide demeaned variable by its average (column 2); divide demeaned variable by municipal area (column 3); divide by the stock of forested areas (Panel A) or total loaned credit (Panel B) in 2008 (column 4); and apply the natural logarithm transformation (column 5). The table also displays dependent variable means and standard deviations for the pre-policy period (through 2008) for the full benchmark sample, as well as individually for treatment (inside Amazon biome) and control (outside Amazon biome) sample municipalities. The sample includes all BLA municipalities that are not crossed by the Amazon Biome border and that are within 100 km of the biome border. All columns cover the 2003 through 2011 period. Robust standard errors are clustered at the municipality level. Significance: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

We also consider an alternative denominator that reflects the relative stock of forested areas when Resolution 3545 is implemented. This arguably captures the pre-policy potential for deforestation at the municipality level. More specifically, in column 4 of Panel A, we measure the dependent variable as the total annual deforestation increment as a share of the stock of forested areas in 2008. We observe a robust and negative coefficient. The conditional difference-in-differences coefficient of −0.039 indicates that Resolution 3545 significantly decreased annual deforestation inside the biome by 3.9 percentage points, which corresponds to 77% of the dependent variable average computed over the pre-policy period. For the sake of completeness, we report the analogous specification for credit in Panel B. Finally, in column 5, the dependent

variables are simply the natural logarithm of the annual deforestation increment and of the total amount of credit loans, in Panels A and B respectively. The coefficients indicate that Resolution 3545 is associated with a decrease in deforestation of about 70%, and a decrease in credit loans of around 18%.

Overall, we observe that our results remain robust to different ways of measuring the outcome variables. Moreover, the magnitude of the point estimates is always quantitatively relevant. A likely explanation for this is that municipality fixed effects absorb not only differences in levels, but also much of the influence of the transformations applied to our dependent variables.

Additional checks further test whether municipalities on either side of the Amazon Biome border were subject to comparable economic processes prior to the policy intervention. These checks repeat the specification from Table 6 (column 3), which includes flexible interactions between the Amazon Biome dummy with pre-policy year dummies, using relevant municipality economic indicators as dependent variables. Appendix Table C1 reports estimated coefficients for the test of differential pre-trends in population density (column 1), municipality total gross domestic product (GDP) (column 2), municipality agricultural GDP (column 3), and bovine heads per hectare (column 4). The results indicate that, in the years preceding Resolution 3545, economic dynamics did not differ significantly across municipalities inside *vs* outside the Amazon Biome. Coefficients are statistically insignificant, while point estimates do not suggest any particular trend. These findings support the proposed identification strategy, which hinges on the comparability across treatment and control groups. Overall, the evidence suggests that both sides of the biome border saw similar economic processes in the pre-policy period. Furthermore, economic growth does not appear to differ across treatment and control municipalities after the policy intervention, as captured by the insignificant estimated effect of the post-policy interaction term on municipality GDP. This finding is consistent with the literature that assesses the impact of the post-2004 conservation policies on economic outcomes in the Brazilian Amazon. The results typically show that municipality-level GDP and agricultural output were not affected by conservation policies (Assunção *et al.*, 2015). This suggests that farmers might have substituted expansion of agricultural production along the extensive margin with that in the intensive margin.

7. The General Relationship Between 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. 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 (Binswanger, 1991; Angelsen, 1999; Zwane, 2007). 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 analyses the relationship between availability of financial resources and deforestation in devel-

oping 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 programme increased deforestation in Mexico, while Zwane (2007) finds evidence of a positive relationship between income and forest clearings in Peru.[23]

Only a few papers explicitly address access to credit. Pfaff (1999) and Hargrave and Kis-Katos (2013) 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 randomised 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 generalise context-specific findings, however, have made it difficult to obtain a broader understanding of how credit policies affect deforestation. In this article we do not address access to resources directly linked to environmental or poverty alleviation programmes, 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 4 and 5 presented compelling evidence that Resolution 3545 had negative effects on both deforestation and credit concession. Further, the overall evidence suggests that Resolution 3545 has affected deforestation only through a reduction in credit concessions. In this case, we explore our empirical context and use Resolution 3545 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 5 makes this case. Second, that the policy affected deforestation strictly through the credit channel. This hypothesis could be violated by the fact that Resolution 3545 also included environmental conditions. However, Resolution 3545'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 3, 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 3545 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:

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

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[23] 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 Cropper and Griffiths, 1994; Barbier and Burgess, 1996; Pfaff, 1999; Shortle and Abler, 1999; Wunder, 2001; Deininger and Minten, 2002; Foster and Rosenzwei, 2003; Fisher *et al.*, 2005; Baland *et al.*, 2010).

Table 10. *Rural Credit Concessions' Effects on Deforestation.*

	(1)	(2)	(3)	(4)	(5)
	Dependent variable: normalised deforestation				
Panel A: OLS					
Rural credit	− 0.009	− 0.015	− 0.016	− 0.057	0.000
	(0.025)	(0.024)	(0.024)	(0.031)*	(0.018)
Panel B: 2SLS					
Rural credit	1.656	1.321	1.331	1.758	1.051
	(0.691)**	(0.485)***	(0.495)***	(0.940)*	(0.380)***
Observations	1,575	1,575	1,575	900	2,502
Number of municipalities	175	175	175	100	278
Municipality and year FE	Yes	Yes	Yes	Yes	Yes
Agricultural prices	No	Yes	Yes	Yes	Yes
Conservation policies	No	Yes	Yes	Yes	Yes
Embargoed areas and fines	No	No	Yes	Yes	Yes
Sample	<100 km	<100 km	<100 km	<50 km	<200 km

Notes: Panel A reports OLS coefficients, and Panel B reports second-stage coefficients from 2SLS specifications where rural credit is instrumented by Resolution 3545 (*Biome* × *Post*2009). The sample includes all BLA municipalities that are not crossed by the Amazon Biome border and that are within 100 km of the biome border (columns 1 through 3). Columns 4 and 5 report coefficients estimated using alternative samples composed of BLA municipalities that are not crossed by the Amazon Biome border and that are within 50 km or 200 km of the biome border, respectively. All columns cover the 2003 through 2011 period. Robust standard errors are clustered at the municipality level. Significance: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

where $Credit'_{it}$ is instrumented by the interaction term $Biome_i \times Post2009_t$. 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 10 presents the results of ordinary least squares (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 subsidised credit by changing their optimal allocation of resources, and thereby reducing deforestation. As discussed in A, in the absence of binding credit constraints, farmers' actions would not have resulted in a change in deforestation during the post-policy period.[24] 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

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[24] 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 subsidised 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.

(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 article we evaluated the impact on deforestation of Resolution 3545, a rather innovative policy that made the concession of subsidised rural credit in the Amazon conditioned upon proof of compliance with legal titling requirements and environmental regulations. We documented that Resolution 3545 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 3545 on deforestation, we thus investigated its potential mechanisms. We presented evidence that Resolution 3545 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 3545 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 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. Moreover, the reach and effectiveness of Resolution 3545 is expected to depend, to some extent, on land tenure conditions. If tenure conditions are poorly defined, environmental liability is also poorly defined, and access to credit is already limited. Other conservation efforts—namely, monitoring and law enforcement, municipality blacklisting, and Payment for Environmental Services schemes—similarly depend on land tenure conditions. These conditions are therefore of paramount importance for liability assignment and effective policymaking.

Second, our analysis suggests that the financial environment in the Amazon is characterised 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.

Finally, it is important to mention that there is a trade-off between internal versus external validity in our setting. Internal validity is arguably strengthened as we restrict the sample of municipalities to those near the border, while external validity can hardly be achieved in such a large and heterogeneous region. In fact, we should not expect Resolution 3545 to have had a relevant effect in places that had not actively been exposed to land use conversion and forest clearings. This reasoning corroborates the relevance of our results to the extent that we explore a sample of municipalities undergoing intense conversion activity, on both sides of the border. More specifically, our results apply to municipalities within the so-called Arc of Deforestation, where approximately half of the world's tropical deforestation occurs and where fires have concentrated (Araújo *et al.*, 2012). Our results therefore shed light on a particularly important setting, where both agricultural activities and forest clearings are most active, and are thus expected to be generally informative to policymaking in contexts where deforestation is a pressing issue.

Appendix 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.[25] We draw on Banerjee and Duflo (2014) 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 subsidised 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 labour and land inputs. This traditional technology is described by:

$$f(L, T), \tag{A1}$$

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

$$F(K, L, T) = A(K)f(L, T). \tag{A2}$$

Assume that labour can be paid at the end of the harvest period, but that expenditures with non-labour 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:

$$\pi_{traditional}(M) = \max_{L,T} f(L, T) - p_L L - p_T T \tag{A3}$$

$$\text{subject to } p_T T \leq M.$$

---

[25] 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:

$$\pi_{modern}(M) = \max_{K,L,T} A(K)f(L,T) - p_K K - p_L L - p_T T \qquad (A4)$$

$$\text{subject to } p_K K + p_T T \leq M.$$

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:

$$\pi(M) = \begin{cases} \pi_{traditional}(M) \text{ if } M < M_0, \\ \pi_{modern}(M) \quad \text{if } M \geq M_0. \end{cases} \qquad (A5)$$

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 characterisation of the optimal land input. For the traditional technology, the optimal choice of farmland is given by:

$$T_{traditional}(M) = \begin{cases} \frac{M}{p_T}, & \text{if } M < \overline{M}, \\ T^*_{traditional} & \text{if } M \geq \overline{M}, \end{cases} \qquad (A6)$$

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:

$$T_{modern}(M) = \begin{cases} \frac{\gamma}{\alpha+\gamma} \frac{M}{p_T} \text{ if } M < \overline{\overline{M}}, \\ T^*_{modern} \text{ if } M \geq \overline{\overline{M}}, \end{cases} \qquad (A7)$$

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 A1.

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

$$M^*(r) = \arg\max_M \Pi(M) - (1+r)M \qquad (A8)$$

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 subsidised 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 subsidised 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 (2014), we say that a farmer is *credit rationed* at the subsidised interest rate if $M_b < M^*(r_b)$, and that a farmer is *credit constrained* if $M < M^*(r_m)$.
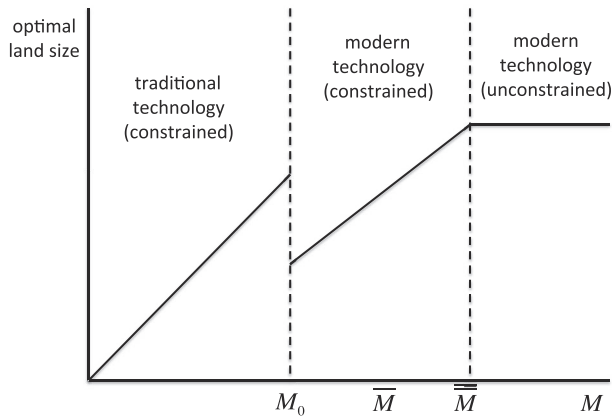
Fig. A1. *Conceptual Framework Model: Optimal Farmland Size.*
Note: The figure illustrates optimal farmland size choice for set-up in which $M_0 < \overline{M} < \overline{\overline{M}}$.

As argued in Section 1, the policy change may have reduced the availability of subsidised 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 A1, 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 $(M_0, \overline{M})$—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 $(M_0, \overline{M})$ 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 summarise these results in the context of the credit reduction implied by the policy change.

PROPOSITION 1. *If the reduction in the availability of subsidised 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 subsidised 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 subsidised 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 subsidised 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 subsidised 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).

Appendix B. Supplementary Descriptive Statistics

Table B1. *Descriptive Statistics for Full Sample: Alternative Normalisations for Dependent Variables.*

	2003	2004	2005	2006	2007	2008	2009	2010	2011
Treatment and control municipalities									
Deforestation: benchmark normalisation	0.2561	0.1992	0.7793	−0.3662	−0.2985	0.0654	−0.1679	−0.4960	−0.6207
	(0.9683)	(0.8291)	(1.1710)	(0.5871)	(0.5927)	(0.7708)	(1.0542)	(0.4774)	(0.3277)
Deforestation: demeaned divided by average	0.2241	0.1345	1.0670	−0.5405	−0.4694	−0.0337	0.2431	−0.6536	−0.7865
	(1.3237)	(1.1377)	(2.0964)	(0.6438)	(0.7470)	(1.0383)	(2.7208)	(0.5505)	(0.2732)
Deforestation: demeaned divided by area	0.0013	0.0019	0.0035	−0.0026	−0.0023	0.0000	−0.0014	−0.0030	−0.0036
	(0.0070)	(0.0069)	(0.0077)	(0.0039)	(0.0042)	(0.0061)	(0.0079)	(0.0045)	(0.0045)
Deforestation: divided by forest 2008	0.0579	0.0558	0.0790	0.0297	0.0289	0.0514	0.0709	0.0268	0.0219
	(0.1400)	(0.1296)	(0.1538)	(0.1016)	(0.0889)	(0.1396)	(0.2256)	(0.1181)	(0.1097)
Deforestation: ln	1.9004	1.9478	2.2569	1.2730	1.3533	1.8037	1.3610	1.1058	0.9319
	(1.8994)	(1.9507)	(1.8436)	(1.4311)	(1.5300)	(1.5917)	(1.3593)	(1.2809)	(1.1236)
Credit: benchmark normalisation	−0.8488	−0.4004	−0.2142	0.2803	0.0870	0.1928	−0.0342	0.3811	0.5564
	(0.7254)	(0.7318)	(0.9613)	(1.0023)	(0.8748)	(0.8328)	(0.7734)	(0.8180)	(0.9027)
Credit: demeaned divided by average	−0.0000	0.0001	−0.0001	0.0016	0.0008	0.0009	0.0003	0.0008	0.0009
	(0.0014)	(0.0016)	(0.0007)	(0.0047)	(0.0023)	(0.0032)	(0.0014)	(0.0025)	(0.0020)
Credit: demeaned divided by area	−0.0004	0.0001	0.0001	0.0020	0.0020	0.0019	0.0007	0.0017	0.0023
	(0.0023)	(0.0036)	(0.0035)	(0.0052)	(0.0071)	(0.0068)	(0.0030)	(0.0043)	(0.0044)
Credit: divided by credit 2008	0.0003	0.0002	−0.0000	0.0022	0.0012	0.0003	0.0003	0.0008	0.0012
	(0.0044)	(0.0022)	(0.0011)	(0.0083)	(0.0041)	(0.0016)	(0.0014)	(0.0021)	(0.0032)
Credit: ln	1.5252	1.8198	1.9366	2.2337	2.1094	2.1741	2.0550	2.2689	2.3354
	(1.4462)	(1.4451)	(1.5556)	(1.3487)	(1.2603)	(1.3576)	(1.3733)	(1.3974)	(1.4368)

Notes: The table presents annual means and standard deviations (in parentheses) for the normalised dependent variables used in regressions. Statistics refer to the full benchmark sample, encompassing municipalities both inside (treatment) and outside (control) the Amazon biome. Covered normalisation procedures are: (*i*) 'Benchmark normalisation'—divided demeaned variable by its standard deviation over the 2003 through 2011 period (see Subsections 2.1 and 2.2 for details), (*ii*) 'Divided by average'—divided demeaned variable by its average, (*iii*) 'Divided by area'—divided demeaned variable by municipal area, (*iv*) 'Divided by forest/credit 2008'—divided by the stock of forested areas or total loaned credit in 2008, and (*v*) 'ln'—applied natural logarithm transformation. Data from PRODES/INPE and Brazilian Central Bank.

Table B2. *Descriptive Statistics for Treatment Sub-sample: Alternative Normalisations for Dependent Variables.*

	2003	2004	2005	2006	2007	2008	2009	2010	2011
Treatment municipalities									
Deforestation: benchmark normalisation	0.2268	0.4706	0.6810	−0.3454	−0.2005	0.1620	−0.6089	−0.6322	−0.7251
	(0.8646)	(0.8184)	(1.0736)	(0.7224)	(0.7149)	(0.8001)	(0.3451)	(0.3787)	(0.3452)
Deforestation: demeaned divided by average	0.1667	0.4468	0.8480	−0.4440	−0.2822	0.0958	−0.6796	−0.6808	−0.7651
	(0.9178)	(0.9230)	(1.8285)	(0.6910)	(0.8112)	(0.9169)	(0.3249)	(0.3499)	(0.2750)
Deforestation: demeaned divided by area	0.0022	0.0044	0.0041	−0.0037	−0.0031	0.0002	−0.0049	−0.0047	−0.0054
	(0.0083)	(0.0087)	(0.0075)	(0.0050)	(0.0054)	(0.0077)	(0.0057)	(0.0056)	(0.0053)
Deforestation divided by forest 2008	0.0483	0.0574	0.0577	0.0263	0.0330	0.0451	0.0137	0.0117	0.0111
	(0.0578)	(0.0527)	(0.0651)	(0.0474)	(0.0604)	(0.0515)	(0.0190)	(0.0130)	(0.0305)
ln deforestation	2.5817	2.8225	2.8650	1.8882	2.0581	2.5342	1.5144	1.5203	1.3228
	(2.0461)	(2.0567)	(2.0049)	(1.5286)	(1.6367)	(1.6334)	(1.3801)	(1.3716)	(1.2052)
Credit: benchmark normalisation	−0.7966	−0.3391	−0.1840	0.2999	0.2213	0.2369	−0.2393	0.2641	0.5367
	(0.6649)	(0.8232)	(1.0457)	(0.9507)	(0.8657)	(0.8691)	(0.7616)	(0.7614)	(0.9501)
Credit: demeaned divided by average	0.0001	0.0003	−0.0000	0.0018	0.0009	0.0006	0.0002	0.0006	0.0007
	(0.0016)	(0.0021)	(0.0008)	(0.0056)	(0.0024)	(0.0022)	(0.0016)	(0.0016)	(0.0017)
Credit: demeaned divided by area	−0.0003	0.0003	0.0002	0.0020	0.0027	0.0018	0.0003	0.0011	0.0019
	(0.0018)	(0.0028)	(0.0029)	(0.0046)	(0.0092)	(0.0074)	(0.0026)	(0.0025)	(0.0037)
Credit: divided by credit 2008	0.0006	0.0004	−0.0000	0.0028	0.0012	0.0001	0.0002	0.0007	0.0012
	(0.0060)	(0.0030)	(0.0013)	(0.0110)	(0.0041)	(0.0014)	(0.0016)	(0.0020)	(0.0035)
Credit: ln	1.7081	1.9539	2.0730	2.3460	2.3247	2.3458	2.0774	2.3636	2.4502
	(1.3525)	(1.4079)	(1.4985)	(1.2811)	(1.2001)	(1.3533)	(1.3135)	(1.3240)	(1.4149)

Notes: The table presents annual means and standard deviations (in parentheses) for the normalised dependent variables used in regressions. Statistics refer to the treatment sub-sample. Covered normalisation procedures are: (*i*) 'Benchmark normalisation' −divided demeaned variable by its standard deviation over the 2003 through 2011 period (see Subsections 2.1 and 2.2 for details), (*ii*) 'Divided by average' −divided demeaned variable by its average, (*iii*) 'Divided by area' −divided demeaned variable by municipal area, (*iv*) 'Divided by forest/credit 2008' —divided by the stock of forested areas or total loaned credit in 2008, and (*v*) 'ln' —applied natural logarithm transformation. Data from PRODES/INPE and Brazilian Central Bank.

Table B3. *Descriptive Statistics for Control Sub-sample: Alternative Normalisations for Dependent Variables.*

	2003	2004	2005	2006	2007	2008	2009	2010	2011
Control municipalities									
Deforestation: benchmark normalisation	0.2844	−0.0631	0.8744	−0.3864	−0.3932	−0.0280	0.2582	−0.3645	−0.5199
	(1.0630)	(0.7555)	(1.2567)	(0.4202)	(0.4272)	(0.7338)	(1.3071)	(0.5259)	(0.2761)
Deforestation: demeaned divided by average	0.2795	−0.1674	1.2786	−0.6338	−0.6503	−0.1588	1.1347	−0.6273	−0.8071
	(1.6263)	(1.2446)	(2.3169)	(0.5836)	(0.6328)	(1.1347)	(3.5916)	(0.6925)	(0.2714)
Deforestation: demeaned divided by area	0.0005	−0.0006	0.0029	−0.0016	−0.0016	−0.0002	0.0020	−0.0014	−0.0018
	(0.0055)	(0.0030)	(0.0078)	(0.0021)	(0.0024)	(0.0039)	(0.0081)	(0.0020)	(0.0024)
Deforestation divided by forest 2008	0.0672	0.0543	0.0996	0.0329	0.0249	0.0574	0.1261	0.0415	0.0324
	(0.1879)	(0.1747)	(0.2044)	(0.1350)	(0.1099)	(0.1895)	(0.3066)	(0.1642)	(0.1506)
ln deforestation	1.2420	1.1026	1.6692	0.6785	0.6723	1.0979	1.2128	0.7054	0.5542
	(1.4825)	(1.4027)	(1.4586)	(1.0321)	(1.0425)	(1.1861)	(1.3298)	(1.0473)	(0.8942)
Credit: benchmark normalisation	−0.8992	−0.4597	−0.2433	0.2613	−0.0427	0.1501	0.1639	0.4941	0.5755
	(0.7799)	(0.6302)	(0.8770)	(1.0547)	(0.8686)	(0.7987)	(0.7361)	(0.8584)	(0.8593)
Credit: demeaned divided by average	−0.0001	−0.0001	−0.0001	0.0013	0.0007	0.0012	0.0004	0.0009	0.0011
	(0.0012)	(0.0007)	(0.0007)	(0.0038)	(0.0021)	(0.0039)	(0.0012)	(0.0031)	(0.0022)
Credit: demeaned divided by area	−0.0005	0.0000	0.0001	0.0020	0.0013	0.0021	0.0011	0.0023	0.0027
	(0.0027)	(0.0041)	(0.0040)	(0.0057)	(0.0039)	(0.0065)	(0.0032)	(0.0054)	(0.0051)
Credit: divided by credit 2008	−0.0000	−0.0001	−0.0000	0.0016	0.0011	0.0005	0.0004	0.0009	0.0012
	(0.0016)	(0.0006)	(0.0008)	(0.0044)	(0.0041)	(0.0018)	(0.0013)	(0.0023)	(0.0029)
Credit: ln	1.3485	1.6902	1.8049	2.1253	1.9013	2.0082	2.0333	2.1774	2.2245
	(1.5179)	(1.4765)	(1.6063)	(1.4096)	(1.2885)	(1.3486)	(1.4359)	(1.4664)	(1.4571)

Notes: The table presents annual means and standard deviations (in parentheses) for the normalised dependent variables used in regressions. Statistics refer to the control sub-sample. Covered normalisation procedures are: (*i*) 'Benchmark normalisation' − divided demeaned variable by its standard deviation over the 2003 through 2011 period (see Subsections 2.1 and 2.2 for details, (*ii*) 'Divided by average' — divided demeaned variable by its average, (*iii*) 'Divided by area' —divided demeaned variable by municipal area, (*iv*) 'Divided by forest/credit 2008'—divided by the stock of forested areas or total loaned credit in 2008, and (*v*) 'ln'—applied natural logarithm transformation. Data from PRODES/INPE and Brazilian Central Bank.

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Appendix C. Supplementary Robustness Checks

Table C1. *Robustness Checks: Pre-existing Economic Processes.*

	Population density (1)	ln(GDP) (2)	ln(Ag GDP) (3)	Cattle density (4)
Biome × Post 2009	− 0.027	− 0.018	0.030	− 0.023
	(0.024)	(0.031)	(0.048)	(0.014)
Biome × 2008	− 0.011	0.011	0.059	− 0.007
	(0.022)	(0.037)	(0.057)	(0.012)
Biome × 2007	0.018	− 0.001	0.022	0.001
	(0.011)	(0.034)	(0.049)	(0.014)
Biome × 2006	0.007	0.015	0.029	0.010
	(0.009)	(0.033)	(0.042)	(0.011)
Observations	1,575	1,575	1,575	1,575
Number of municipalities	175	175	175	175
Municipality and year FE	Yes	Yes	Yes	Yes
Agricultural prices	Yes	Yes	Yes	Yes
Conservation policies	Yes	Yes	Yes	Yes
Embargoed areas and fines	Yes	Yes	Yes	Yes
Sample	<100 km	<100 km	<100 km	<100 km

Notes: Dependent variables displayed above numbered columns are as follows: municipal population by municipal area (column 1); natural logarithm of municipal GDP (column 2); natural logarithm of municipal GDP for agricultural sector (column 3); and bovine headcount per hectare (column 4). All columns include the full list of fixed effects and controls used in the benchmark specification (Table 2, column 4). The sample includes all BLA municipalities that are not crossed by the Amazon Biome border and that are within 100 km of the biome border. All columns cover the 2003 through 2011 period. Robust standard errors are clustered at the municipality level. Significance: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

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Additional Supporting Information may be found in the online version of this article:

Replication Package

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