Agricultural Productivity and Forest Conservation: Evidence from the Brazilian Amazon

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A mix of public policy and market interventions in the mid-2000s led to historic reductions in deforestation in the Brazilian Amazon. The collateral impact of these forest conservation policies on agricultural production is still poorly understood, though evidence is sorely needed given the economic importance of agriculture in Brazil and many other forest-rich countries. We construct a ten-year panel dataset for agriculture and deforestation in the Brazilian Amazon (2004–2014), and use two complementary difference-in-difference strategies to estimate the causal effect of one of Brazil’s flagship anti-deforestation strategies, the Priority List (Municípios Prioritários), on agricultural production and productivity in three sectors: beef, dairy, and crop production. We find no evidence for trade-offs between agriculture and forest conservation. Rather, reductions in deforestation in priority municipalities were paired with increases in cattle production and productivity (cattle/hectare), consistent with a model where policy-induced decreases in the value of clearing new land cause credit-constrained farmers to shift investments from deforestation to capital investments in farming. The policy had no consistent effect on dairy or crop production. Our results suggest that in regions with large yield gaps and where technologies for increasing yields are readily available, efforts to constrain agricultural expansion through improved forest conservation policies may induce intensification.

Key words: Agriculture, Amazon, Brazil, cattle, deforestation, induced intensification, land sparing, productivity.

JEL codes: Q12, Q15, Q23, Q24, Q28.

Tropical deforestation is a major cause of greenhouse gas emissions, biodiversity loss, and the erosion of ecosystem services (Gibson et al. 2011; Baccini et al. 2012; Carrasco et al. 2017). While forest conservation initiatives have had some success at reducing deforestation—notably in the Brazilian Amazon, where deforestation fell 71% from 2004–2016 (PRODES 2017)—a critical question is not only how effective forest conservation measures are at controlling deforestation—notably in the Brazilian Amazon, where deforestation fell 71% from 2004–2016 (PRODES 2017)—a critical question is not only how effective forest conservation measures are at controlling deforestation (Börner et al. 2016), but what the collateral effects of forest conservation initiatives are on local economies, and whether there are conflicts between forest conservation and economic development. In particular, as most deforestation in the tropics is done for agriculture (Gibbs et al. 2010) and forest policies can displace...
agricultural production, questions remain about potential trade-offs between forest conservation and agriculture.

In this article we examine how agricultural production and productivity responded to a key forest conservation policy in the Brazilian Amazon—the government’s priority municipality (Municípios Prioritários) list, which was introduced in 2008 (Decree no. 6,321/07), as part of the action plan to curb deforestation (Portuguese acronym, PPCDAm). The Priority List (PL) targets municipalities with high deforestation rates for a package of interventions, including increased field inspections and fines for deforestation (Assunção and Rocha 2014). We exploit the policy-induced partitioning of municipalities into PL and non-PL municipalities to estimate whether deforestation and agricultural productivity trends of priority municipalities shifted due to increased forest conservation. Our empirical analysis is guided by a simple model assuming a higher risk of punishments under the policy that makes illegal land expansion both more expensive and less valuable, and therefore induces farmers in a growing agricultural market to reinvest into capital rather than land, leading to productivity gains (higher production per hectare).

Nowhere are questions about the linkages between forest and agricultural land uses more relevant than in the Brazilian Amazon, the world’s largest tropical forest. The Brazilian Legal Amazon, made up of the nine states of the Amazon basin, is home to 40% of the Brazilian cattle herd and 36.5% of soy production (IBGE 2015a, b). Even as the cattle herd and soy production grew 14.7% and 94%, respectively, from 2004–2014, Brazil successfully reduced deforestation in the Amazon by 82% (PRODES 2017). This reduction was achieved through a combination of command-and-control efforts on private land (Hargrave and Kis-Katos 2013; Börner et al. 2015), expansion of protected areas (Soares-Filho et al. 2010), and the global economic slowdown (Assunção, Gandour, and Rocha 2015). The productivity of cattle ranching—the dominant land use in the region—remains low, however; stocking rates (the number of cattle per hectare—a key metric of cattle productivity) have changed little since the mid-2000s (Dias et al. 2016), in part because the expansion of cattle pasture is used as a means of land “speculation,” that is, in light of land right uncertainty, farmers cut down forest for future use or sale even though they do not need additional land for production (Bowman et al. 2012). Yet, prior studies indicate that the country has a large productivity potential. Strassburg et al. (2014) show that Brazil’s future demand for beef, soy, biofuels, and reforestation can be accommodated on extant agricultural land if beef productivity increases 50% from the current low baseline.

Our study contributes to the burgeoning policy evaluation literature on forest conservation initiatives in the Brazilian Amazon. The PL, for example, is estimated to have reduced deforestation by 600–11,396 km$^2$ in its first 3–4 years of implementation (Arima et al. 2014; Assunção and Rocha 2014; Cisneros et al. 2015), a substantial reduction, given mean annual deforestation of 6,300 km$^2$ across the Amazon post-2009 (PRODES 2017). We instead focus on the impact of the PL on agriculture, which is largely underexplored in the literature despite the stated policy objective of promoting sustainable land management (MMA 2013).

One notable exception is Assunção and Rocha (2014), where the authors find statistically insignificant policy effects on agricultural GDP and crop production—though their findings are highly uncertain (e.g., the 95% confidence interval for the crop production effects is $[-0.07; 0.07]$) and make little commentary on mechanisms through which the PL could impact agriculture. We build on this work by developing a theoretical model that highlights the economic mechanisms induced by the policy and disentangles production and productivity effects, and we specifically account for changes in the cattle sector—which was neglected in prior studies despite the fact that cattle pasture makes up 65% of deforested land (de Almeida et al. 2016) and is key to understanding deforestation–agriculture trade-offs. Our empirical strategy also addresses important identification issues that have been overlooked in the prior PL literature.

In another study with a similar focus to ours, le Polain de Waroux et al. (2017) examine the impact of forest conservation policies in five South American countries on soy and cattle expansion. Using panel regression for municipal data from 2001–2012, these authors find that only within the Amazon biome have forest conservation policies decreased pasture expansion, coincident with pasture intensification. We seek to take a step forward by (a) looking at a specific policy (rather than using a scaled policy stringency metric that could be subject to measurement error), and
suggest that in regions with large yield gaps and where technologies for increasing yields are readily available—such as in the Amazon—improved forest conservation policies may induce intensification.

The Priority List

The PL, launched in 2008, involved multiple interventions targeted at municipalities with high deforestation rates. Of the 771 municipalities making up the Legal Amazon region, 36 municipalities were included in the initial list. Formally, the criteria for being included are: (a) high total deforestation; (b) high deforestation in the preceding three years; and (c) deforestation increasing in at least three out of the previous five years, though these criteria were applied inconsistently. Of the 36 municipalities with the highest total deforestation, only 20 were included in the initial list; similarly, only 25 of the 36 municipalities with the highest deforestation rates from 2005–2007 (the three-years preceding the policy) were listed (figure 1, panel a). It is this inconsistency in assignment to the treatment group that we take advantage of when building our control group.

Municipalities that were included on the list were subject to (a) increased enforcement by the Brazilian environmental law enforcement, Ibama (figure 1, panel b); (b) improved monitoring—landholdings were required to obtain a georeferenced certification (through the rural cadaster, CAR) as a precondition for authorized forest clearings; (c) access to agricultural credit was, in theory, restricted, subject to proof of compliance with the Forest Code. These municipalities may have also experienced (d) reputational damage—the list was colloquially known as a “blacklist” (lista negra in Portuguese); and (e) positive incentives, notably financial and logistic support from international NGOs and public administrations (Cisneros et al. 2015). The external support included efforts to increase capacity in monitoring and enforcement of deforestation, support in registering properties within the CAR, and efforts to promote sustainable agricultural practices (MMA 2015; Viana et al. 2016). To be removed from the list, municipalities must register 80% of their area in the CAR and reduce deforestation below 40 km²/year. The PL is updated annually and since 2008 eleven
municipalities have been removed and sixteen added (Cisneros et al. 2015).

Conceptual Framework

In this section, we discuss a simple conceptual framework inspired by some salient features of the agriculture sector in the Brazilian Amazon in order to analyze the effect of the priority municipality list on key economic variables. We consider the maximization problem of a farmer producing with capital $K$, already cleared land $L$ and newly cleared land $L^n$. Clearing land has specific costs, so that we model it as a distinct decision variable. Capital includes machines, building material, all kinds of other physical assets, and fertilizer. Total output is given by $Y = f(K, L + L^n)$. We assume that $f$ is twice differentiable, concave, and that cross-derivatives are positive (this allows for a broad range of production functions, including Cobb-Douglas functions).

Capital rental cost are given by the interest rate $r$. We assume that farmers utilize their already acquired land $L$ themselves by default because (a) land can be assumed to be a short-term fixed input factor in agricultural production (Gameiro et al. 2016) and (b) land rental markets in Brazil are underdeveloped (Assunção and Chiavari 2014). For this reason farmers do not optimize regarding how much of their already-owned land to utilize and do not take the opportunity cost into account. We normalize the price of the output to 1.

Since land tenure is not clearly defined in many parts of the Amazon (Araújo et al. 2009), clearing land can be used both to gain land as a production input, and as a means of “reserving” land for future use or sale—a concept that is sometimes referred to as “grilagem” in policy discussions (e.g., Margulis 2003). Cattle ranching in particular is the lowest-cost method of making land claims on deforested land (Bustamante et al. 2012; Strassburg et al. 2014). Bowman et al. (2012) show that from a simple profit maximizing logic, land acquisition for beef production is not profitable across large parts of the Amazon, though farmers do deforest to make new pasture—even though most land clearance is illegal—precisely because they take the future benefits of land speculation into account.¹ In the Amazon state of Rondônia, for example, farmers expect a

Figure 1. Workings of Priority List

Note: Panel a) Venn diagram comparing the priority list’s formal inclusion criteria against the attributes of the 36 municipalities initially included in the list, adapted from Cisneros et al. (2015), i.e. the circle for criterion 1 shows that among the top-36 municipalities in terms of deforestation, 20 were included in the PL. Panel b) under the policy (introduced in 2008, as indicated by the dashed line), priority municipalities experienced an increase in enforcement of environmental legislation. The fine intensity (the mean number of fines per km² of deforestation) is shown relative to the 2004 figure for both groups (i.e., 2004 = 1).

Source: Authors’ own calculations, based on data from (PRODES 2017) and (Ibama 2018).

¹ The requirements of the Brazilian Forest Code, which regulates land clearing, differ between biomes. In the Amazon biome properties are required to maintain 80% forest cover as a Legal Reserve. In the Cerrado, properties must maintain 35% of native vegetation. Thus, deforestation can be illegal in three ways: farmers (a) do not have the rights on the land; (b) go beyond the legal limits they can use in their lands; (c) have the rights on the land and are within the legal limits, but they do not have the authorization.
10% average yearly increase in the value of deforested land (Vale 2015).

We therefore explicitly model the possibility of reserving land. More specifically, clearing new land has a net benefit, \(v\), which is determined by the value of attaining de facto property rights to the land, minus clearance costs. Clearance costs come in two parts: first, the costs of physically clearing the land by removing natural vegetation; this cost is paid \textit{ex ante} and is thus included in the budget constraint. Second, there are expected punishments for illegal deforestation. The cost of the expected punishment is given by the probability of being caught, multiplied by the value of fines (R$5,000/ha) and/or property seized or destroyed by IBAMA, and lost revenues from having the property “blacklisted”, preventing (legal) sales of livestock or crops from the property (Börner et al. 2014).

Concerning the farmer’s budget, we also account for evidence (Assunção et al. 2013; Assunção et al. 2017b) that farmers in the Amazon—particularly in cattle-oriented municipalities—face a binding credit constraint. A large body of evidence highlights the importance of such capital constraints for agricultural development in developing economies in general (e.g., Feder et al. 1990; Boucher and Guirkinger 2007; Conning and Udry 2007; Banerjee and Dufo 2014). Thus, we assume that the farmer must allocate his available resources \(M\) to either physically clearing new land, with a cost of \(cL^n\), or investments into physical capital \(K\).

Taking all these features into account, the optimization problem of the farmer can be written as

\[
\max_{K, L^n} f(K, L + L^n) + vL^n - rK + \lambda(cL^n + K - M),
\]

where \(\lambda\) is the Lagrange multiplier associated with the budget constraint. The farmer chooses investments into capital and deforestation. First-order conditions are \(f_K(K, L + L^n) - r + \lambda = 0\) and \(f_L(K, L + L^n) + v + \lambda c = 0\).

Prior to the introduction of the PPCDAm, the policy environment in the Amazon was characterized by a low expectation of punishments for deforestation and high benefits of obtaining property rights (i.e., \(v\) was high). This encouraged clearing land beyond the optimal level for agricultural production. The introduction of the PL then has two potential effects. First, higher enforcement means that the expected cost of getting punished for illegal deforestation increases. Second, regularizing the use of deforested land in the listed municipalities shrinks the benefits of reserving land through illegal land clearance. Improved land regularization is indirectly achieved through increased enforcement and the CAR. While registering a property in the CAR does not confer the land a formal land title, it does reveal the level of compliance with the Brazilian Forest Code, identifying what must be regularized (i.e., reforested)—at the land owner’s (sometimes considerable) expense (Azevedo et al. 2017; Garcia et al. 2017). The PL thus had the effect of decreasing the value \(v\) of clearing land in affected municipalities.

**Proposition.** A decrease in the benefits of clearing land \(v\) causes: (a) a reduction in the amount of newly cleared land \(L^n\); (b) an increase in capital investments \(K\); (c) an increase in total output, \(Y\); and (d) an increase in output per unit of land, \(\frac{Y}{L^n}\). An increase in \(c\), the cost of clearing new land, has the same four effects.

**Proof.** See online supplementary appendix B.

Note that we obtain a proposition with small but important differences for the case of a farmer without a budget constraint (see online supplementary appendix B). Both with and without a budget constraint, a decrease in the benefits of clearing land causes an increase in productivity (output per unit of land). However, without a budget constraint, the use of both land and capital is reduced, which would lead to a decrease in total production.

The data requirements to fully test this structural model prediction are demanding, particularly due to a lack of capital data on a locally disaggregated level. We therefore resort to a reduced-form test focusing on the policy effect on directly-observed production and productivity measures in different agricultural activities at the municipality level.

We expect that the policy will affect our three agricultural sectors differently. Beef production is the dominant land use in the region—cattle pasture makes up 65% of deforested land (de Almeida et al. 2016)—and is the main sector associated with land speculation; beef production is therefore the most likely to be affected by a policy-induced
change in the costs and benefits of deforestation. Dairy production, in contrast, is a much smaller operation: dairy cattle make up only 5% of cattle in the Legal Amazon (IBGE 2015a), and dairy farming is primarily practiced by family farms (see online supplementary appendix, figure A1). Smallholder deforestation is less controlled than deforestation on larger properties because the Brazilian satellite monitoring system cannot detect deforested patches less than 25 hectares in size, and because it is less cost-effective to patrol small clearings than large ones (Godar et al. 2014; Assunção et al. 2017a). Both these features of enforcement mean that dairy producers may not have perceived a change in the attractiveness of forest clearing. Finally, crop agriculture in the Brazilian Amazon is dominated by soy, which made up 73% of cropland in 2014 (IBGE 2015b). Deforestation for soy farming was, however, already constrained in the Amazon biome by the Soy Moratorium. Introduced in 2006, the Soy Moratorium prohibited the purchase of soy produced on recently deforested land; as deforestation for soy declined by 97% (Gibbs et al. 2015), the priority list is unlikely to have created any additional land scarcity beyond the effect of the Moratorium.

Data

We combined multiple data sources for the Brazilian Legal Amazon to make our panel dataset, from 2004–2014. The Legal Amazon consists of 771 municipalities (we used administrative boundaries from 2007 to ensure consistency over the time period of analysis), but we restricted our analysis to 492 municipalities with >10% forest cover in 2002 for which all data were available, as in (Cisneros et al. 2015). Data sources for covariates are listed in table 1, and data on our agricultural outcome variables are described in more detail below.

Beef productivity was measured as the stocking rate, the number of cattle head per hectare of pasture. No more direct measure of beef productivity (e.g., meat production per hectare) is available at the municipal level, and stocking rates have been used in previous studies of agricultural productivity in Brazil (Strassburg et al. 2014; Dias et al. 2016). The number of cattle per municipality was taken from annual municipal livestock surveys (the Portuguese acronym is PPM).

The primary source of information for the PPM comes from figures from the sales of foot-and-mouth disease vaccines, which are obligatory for all cattle under the Brazilian foot-and-mouth eradication program. Vaccine figures are complemented with farm surveys and are ultimately summed to form the basis of the Brazilian national contribution to Food and Agriculture Organization of the United Nations (FAO) livestock statistics. Pasture area was calculated as the sum of four pasture classifications (pasture with exposed soil, herbaceous pasture, shrubby pasture, and regeneration with pasture) in the high-resolution (ca. 30m²)

Table 1. Data Sources for Covariate Data

| Covariate                                | Data Source                                |
|------------------------------------------|--------------------------------------------|
| Total deforested area in 2007             | PRODES                                    |
| Deforestation 2005                       | PRODES                                    |
| Deforestation 2006                       | PRODES                                    |
| Deforestation 2007                       | PRODES                                    |
| Forest area                              | PRODES                                    |
| Mean rainfall                            | INPE                                      |
| Mean temperature                         | INPE                                      |
| Mean slope                               | INPE                                      |
| Municipality size                        | Cisneros et al. (2015) PLoS ONE           |
| Accessibility (average travel time to municipality center) | Cisneros et al. (2015) PLoS ONE           |
| Population density                       | IBGE Demographic Census                   |
| Municipal GDP                            | IBGE                                      |
| Soy price                                | IBGE-PAM                                  |
| Timber price                             | IBGE-PEVS                                 |
| Farms density                            | IBGE 2006 Agricultural census            |
| Share of small farms                     | IBGE 2006 Agricultural census            |
| Percentage of land owners                | IBGE 2006 Agricultural census            |
| Tractors per farm                        | IBGE 2006 Agricultural census            |
| Share of strictly protected reserves     | IBAMA                                     |
| Share of indigenous territory            | IBAMA                                     |
| Share of settlement projects             | INCRA                                     |
| Field-based enforcement inspections      | Cisneros et al. (2015) PLoS ONE           |
| Value of subsidized agricultural credits  | Brazilian Central Bank                   |
| Federal party affiliation                | TSE                                       |
Terraclass dataset for 2004, 2008, and 2014. We exclude 32 (non-listed) municipalities from the analysis of beef productivity because they have outlier stocking rates (>15 head/ha) in at least one year. In most of these cases they are recorded as having very little pasture in at least one of the measured years—a measurement error caused by cloud cover.

Milk productivity was measured as the milk production per milking cow per year. This was calculated from PPM data: the total milk production per municipality was divided by the number of milking cows. Milk production figures are based on quantities of milk marketed at dairy processing plants and cooperatives, plus milk used for self-consumption or sold directly to consumers.

Our analysis of crop production focused on six major crops, which made up 87.3% of the total agricultural value of the legal Amazon from 2002–2014: soy (48.3%), maize (11.1%), cotton (10.1%), cassava (8.8%), rice (5.0%), and sugar cane (4.0%). Data on crop production, crop area planted, and crop yields came from the municipal agricultural survey (the Portuguese acronym is PAM), an annual dataset based on monthly crop production statistics collected through a network of agricultural technicians and producers in each municipality. To allow aggregation of data from different crops, our main measure for crop productivity is production per hectare (i.e., aggregate gross production value divided by aggregate cropland) and not yield, though we also report results for changes in yield for each individual crop. When calculating the changes in cropland area, we avoid double counting by subtracting the double-cropped (soy-maize) area from the total cropland area per municipality.

Our data inevitably has a number of limitations (including uncertainty in our land classification data, our use of indirect measures of livestock productivity, and a lack of data on local beef prices), which we discuss in online supplementary appendix E.

**Empirical Strategy**

Our “fundamental problem of causal inferences” (Holland 1986) is to construct a tenable counterfactual for deforestation and productivity trends of priority municipalities in the absence of their inclusion on the PL. A simple but naïve counterfactual is provided by the selection criteria of the PL, which leaves us with two natural comparison groups of municipalities. Thus, a first estimate of the policy effect can be obtained from a basic unconditional DID model:

(1) \[ Y_{it} = \alpha + \beta_1 list_{it} + \beta_2 post_{it} + \tau_{DIDtreat_{it}} + \epsilon_{it}, \]

where \( Y_{it} \) refers to the log outcome in municipality \( i \) in year \( t \). Further, \( list_i \) and \( post_i \) are binary variables for the post-treatment period and the inclusion in the PL; \( treat_{it} \) is the interaction term of \( list_i \) and \( post_i \), that is, our variable of interest. The error term, \( \epsilon_{it} \), might be correlated within municipalities; standard errors are thus clustered at the municipality level.

This simplest and most naïve estimate can be further refined to control for factors other than regulatory regime that affect municipality outcomes:

(2) \[ Y_{it} = \alpha + \tau_{DIDtreat_{it}} + \gamma_i + \delta_t + X'_{it} \kappa + \epsilon_{it}. \]

In this regression-based conditioning DID model, we include a set of municipality fixed effects, \( \gamma_i \), accounting for time-invariant differences in productivity outcomes between municipalities, and year dummies, \( \delta_t \), controlling for time shocks that commonly influence productivity outcomes in the municipalities. Moreover, \( X_{it} \) denotes a vector of observable covariates at the municipality level (introduced in table 2 below).

The DID models would identify the causal impact of the PL if we were to assume that no unobserved variables exist that simultaneously influence changes in productivity and the probability of being inserted on the PL. While untestable, this identification assumption is more plausible if trends in outcomes across treatment groups are parallel in the years prior to the policy intervention. A visual inspection of these trends in figure 2 generally support this.\(^3\) Yet, we are still concerned that the identifying assumption is problematic. In fact, panel B of table 2 reveals major differences between the distributions of many pre-policy district characteristics that are potentially related to both the

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\(^2\) Our results are virtually the same if we also include state fixed effects.

\(^3\) Given that we only have three yearly measures of the stocking rate, we also plot the trends in the cattle heads for which we have continuous yearly data.
### Table 2. Balance in Full and Preprocessed Sample

| Variable                          | Full Sample | Preprocessed Sample | Preprocessed Sample with Matching |
|-----------------------------------|-------------|---------------------|----------------------------------|
|                                  | Mean non-priority list | Mean priority list | $\delta_{nom}$ | t-statistic | 95 percentile overlap | $\delta_{nom}$ | t-statistic |
| Cattle productivity              | 1.62        | 1.27                | -0.29           | 1.28        | 0.97                | -0.26           | 1.09        | -0.19 | 0.60 |
| Cattle heads                     | 82,007.37   | 333,094.07          | 1.24            | -11.40***   | 0.92                | 0.34            | -1.46       | 0.43  | -1.33 |
| Pasture area                     | 579.76      | 2,610.91            | 1.78            | -14.99***   | 0.89                | 0.63            | -2.68**     | 0.14  | -0.42 |
| Dairy productivity               | 0.62        | 0.88                | 0.98            | -5.53***    | 1                   | 0.29            | -1.24       | 0.21  | -0.73 |
| Crop productivity                | 1.24        | 1.24                | 0.00            | -0.00       | 1                   | 0.40            | -1.71*      | 0.28  | -0.93 |
| Crop production value            | 92,303.50   | 197,454.03          | 0.27            | -1.40       | 1                   | 0.05            | -0.23       | 0.27  | -0.86 |
| Cropland                          | 72,095.07   | 157,831.38          | 0.29            | -1.59       | 1                   | 0.05            | -0.22       | 0.26  | -0.84 |

Panel A: pre-policy outcome variables

| Variable | Mean non-priority list | Mean priority list | $\delta_{nom}$ | t-statistic | 95 percentile overlap | $\delta_{nom}$ | t-statistic |
|----------|------------------------|--------------------|----------------|-------------|-----------------------|----------------|-------------|
| Total deforested area in 2007   | 994.77                | 4,539.22           | 1.87           | -17.82***   | 0.72                  | 0.96           | -4.07***    | 0.98  | -3.02*** |
| Deforestation 2005              | 24.78                 | 301.75             | 1.64           | -22.01***   | 0.69                  | 1.16           | -4.91***    | 1.13  | -3.39*** |
| Deforestation 2006              | 10.37                 | 134.08             | 1.19           | -16.74***   | 0.69                  | 0.96           | -4.08***    | 0.95  | -2.85**  |
| Deforestation 2007              | 11.58                 | 146.76             | 1.14           | -15.84***   | 0.75                  | 0.87           | -3.7***     | 0.83  | -2.50**  |
| Forest area                      | 0.45                  | 0.63               | 0.71           | -3.45***    | 1                     | 0.89           | -3.77***    | 0.73  | -2.56**  |
| Mean rainfall                    | 2,043.81              | 1,975.58           | -0.21          | 0.97        | 1                     | 0.12           | -0.51       | -0.06 | 0.21   |
| Mean temperature                 | 259.99                | 254.08             | -0.64          | 3.37***     | 0.97                  | -0.01          | 0.03        | 0.26  | -0.96   |
| Mean slope                       | 2.52                  | 2.7                | 0.14           | -0.75       | 1                     | -0.09          | 0.36        | -0.16 | 0.55   |
| Municipality size                | 7,565.84              | 21,696.50          | 0.62           | -5.28***    | 0.92                  | 0.6            | -2.55**     | 0.54  | -1.69   |
| Accessibility (Ø travel time to municipality center) | 702.98 | 906.09 | 0.29 | -1.42 | 1 | 0.68 | -2.88** | 0.49 | -1.68 |
| Population density               | 23.59                 | 2.49               | -0.29          | 1.23        | 0.97                  | -0.56          | 2.39**      | -0.23 | 0.77   |
| Municipal GDP                    | 5.31                  | 7.33               | 0.38           | -2.12***    | 0.97                  | 0.04           | -0.17       | 0.11  | -0.35   |
| Soy price                        | 0.11                  | 0.31               | 0.81           | -1.29***    | 0.99                  | 0.3            | -1.28       | 0.27  | -0.95   |
| Timber price                     | 71.25                 | 120.56             | 0.88           | -4.77***    | 0.94                  | 0.11           | -0.47       | 0.14  | -0.48   |
| Farms density                    | 0.65                  | 0.14               | -0.73          | 3.13***     | 1                     | -0.75          | 3.18***     | -0.67 | 2.54**  |
| Share of small farms             | 0.72                  | 0.64               | -0.47          | 2.53**      | 0.97                  | -0.58          | 2.47**      | -0.73 | 2.46**  |
| Percentage of land owners        | 0.73                  | 0.8                | 0.36           | -1.78*      | 0.97                  | -0.23          | 0.98        | -0.18 | 0.66   |
| Tractors per farm                | 0.14                  | 0.25               | 0.28           | -1.38       | 0.94                  | 0.24           | -1.02       | 0.40  | -1.27   |

Panel B: pre-policy municipality characteristics

Continued
outcome of interest and the likelihood of being included in the PL. Given the observable systematic covariate imbalances, we cannot preclude that differences in productivity trends across treatment groups after the policy introduction are driven by some unobservable shock other than inclusion in the PL.

We therefore turn to two more elaborate methods that resort to a restricted comparison of ex ante observationally equivalent districts to make causal inferences. These methods exploit the fact that for most covariates there is substantial overlap in the central ranges of the variable distributions, as indicated by the proportion of treated municipalities with a covariate value within the 2.5th and 97.5th percentiles of the covariate distribution for control municipalities reported in table 2 (column 5). Our data is, however, not sufficiently rich to facilitate perfect matching; most notably, the number of control districts with very similar deforestation trends is limited by incomplete overlap in distributions. This will have implications for match quality, an issue we carefully address and revisit below.

**Difference-in-Differences with Bias-Adjusted Covariate Matching**

Our first empirical strategy builds on the two-stage approach proposed by Imbens and Rubin (2015).

We begin with “preprocessing” our data to filter out inappropriate control districts from our pool of 440 non-PL municipalities. More specifically, we use propensity score matching (without replacement) to match each treated district to the closest control district in terms of their probability of being listed given observable pre-treatment characteristics. Details on the data-driven algorithm for the specification of the propensity score function (i.e., the choice among the possible correlated covariates) and the parameter estimates are provided in online supplementary appendix C. The preprocessing leaves us with sample of 36 treated and 36 control districts. Table 2 compares the normalized differences and

| Outcome | Preprocessed Sample with Matching | Preprocessed Sample | Full Sample | Mean priority list | Mean non-priority list | Value of subsidized agricultural credits | % strictly protected reserves | % indigeneous territory | % settlement projects | Field enforcement inspections | Federal party affiliation |
|---------|----------------------------------|---------------------|-------------|--------------------|-----------------------|------------------------------------------|----------------------------|-------------------------|----------------------|---------------------------|---------------------------|
| d norm | 0.57                            | 0.57                | 0.59        | 0.18               | 0.18                  | 0.18                                     | 0.18                       | 0.18                    | 0.18                 | 0.18                      | 0.18                      |
| t-statistic | -0.87                          | 1.12               | 0.46        | 0.46               | 0.46                  | 0.46                                     | 0.46                       | 0.46                    | 0.46                 | 0.46                      | 0.46                      |
| 95 percentile overlap | 0.01               | 0.12               | 0.97        | 0.97               | 0.97                  | 0.97                                     | 0.97                       | 0.97                    | 0.97                 | 0.97                      | 0.97                      |
| 2.5th percentile overlap | 0.29               | 1.44               | 0.61        | 0.61               | 0.61                  | 0.61                                     | 0.61                       | 0.61                    | 0.61                 | 0.61                      | 0.61                      |
| 97.5th percentile overlap | 0.87               | 2.59**             | 0.87        | 2.59**             | 0.87                  | 2.59**                                   | 0.87                       | 2.59**                  | 0.87                 | 2.59**                    | 0.87                      |

Asterisks indicate the following: * p < 0.1, ** p < 0.05, and *** p < 0.01.
t-statistics of our various districts characteristics both for the original sample and the preprocessed subsample. The results show that the preprocessing substantially improves the covariate balance, yet many variables still exhibit a considerable degree of imbalance.

With a more appropriate subsample of PL and non-PL municipalities in hand, in the second stage we estimate the average treatment effect on treated (ATT) of the PL policy. The very magnitude of remaining imbalances, however, questions the applicability of linear regression methods. We prefer a non-parametric estimation technique over previous conventional regression approaches such as equation (2) because it puts no functional form assumptions on the distributions of variables that may bias the treatment effect estimate (see also Hargrave and Kis-Katos 2013; Cisneros et al. 2015). Following Heckman et al. (1997, 1998), we implement a non-parametric matched DID approach that compares the before–after productivity of PL municipalities with a weighted average of before–after changes in the non-listed sample. The average policy impact is given by

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6 It is defined as \( \frac{X_t - X_c}{\sqrt{s_c^2 + s_t^2/N_t}} \), where \( X_t \) and \( X_c \) denote the sample mean of covariates by treatment group, \( N_c \) and \( N_t \) are the number of control and treated units, and \( s_c^2 \) and \( s_t^2 \) are the within-group sample variances.

7 In addition, with limited overlap in the distributions of covariates across treatment groups, inferences can be both imprecise and sensitive to ostensibly minor changes in the model specifications (Imbens and Rubin 2015).
$\hat{\tau}_{\text{DID}} = \frac{1}{N_1} \sum_{j \in J_1} \left\{ \left( Y_{jt}(1) - Y_{jt}(0) \right) - \sum_{k \in J_0} w_{jk} \left( Y_{kt}(0) - Y_{kt}(0) \right) \right\}$

Figure 3. Maps of the Brazilian Legal Amazon

Note: Inset is Brazil. Panel a) groups of municipalities used in the difference-in-differences with bias-adjusted covariate matching analysis. Panel b) weightings assigned to municipalities included in the difference-in-differences with entropy balancing analysis, shown for the cattle productivity outcome. Weightings sum to 1; PM = priority list municipalities. Municipalities in gray were excluded from the analysis because they have low forest cover or incomplete data sets.

Using only one control district is more likely to yield an unbiased estimate of the treatment effect, albeit at the cost of sacrificing some precision; Imbens and Wooldridge (2009).
nearest neighbor, within-pair differences are adjusted using a linear regression of the control outcome on the covariate space as suggested in Abadie and Imbens (2006, 2011). This can eliminate a large part of the bias that remains after matching.

**Difference-in-Differences with Entropy Balancing**

Our second empirical strategy differs with respect to the construction of the control group; it constructs the control group by means of a synthetic control method. More specifically, we apply a recent reweighting technique—entropy balancing (Hainmueller 2012)—that focuses directly on the balancing of covariates. It assigns a weight to each of the 440 non-listed control districts such that the moments of the covariates of the reweighted control group are (almost) equal to the moments of the treated group.10

The main advantage of using entropy balancing rather than matching techniques is an increase in balance quality. The reweighting method also retains valuable information by allowing the unit weights to vary smoothly, while units are either discarded or matched with nearest neighbor matching.11 Recall that conventional DID estimates, in contrast, restrict equal weights on the control units.

In this article, the first moment of the covariates, namely the mean, of the treatment and the control group is balanced.12 Given our limited sample size, we also have to restrict the balancing to a limited number of covariates. We select pre-treatment trends of the outcome variables, which technically ensures that the common trend assumption is valid. Additionally, we ensure balance in deforestation trends and those variables that were shown to predict treatment: the accessibility, the share of settlement projects, and the share of strictly protected reserves. Panel b of figure 3 illustrates the weighting of the non-PL control districts. To obtain an estimate of the PL treatment effect, we pass the weights that result from entropy balancing to the conventional DID regressions, that is, we estimate equation (2) using weighted least squares with cluster-robust standard errors. The reweighting makes the parametric causal inferences considerably less model-dependent.13

While the environmental and agricultural economics literature has largely overlooked synthetic counterfactual approaches (with the notable exception of Sills et al. 2015a, who apply the synthetic control approach of Abadie and Gardeazabal 2003 to estimate the impact of the PL on deforestation in the municipality of Paragominas), DID with entropy balancing is emerging as an important tool in health (Markus 2013; Markus and Siedler 2015) and labor economics (Freier et al. 2015).

**Results and Discussion**

In this section we present our treatment effect estimates obtained from the DID matching and DID entropy balancing estimators along with our two “naı¨ve” estimates, namely the basic DID and the more saturated regression DID estimate. Stability of estimates across the two preferred estimators can be interpreted as an indication that the methodologies adequately control for unobserved differences (i.e., potential biases), while instability indicates that the effect estimate is subject to caution.

We begin by re-examining the effects of the PL on municipality deforestation trends. We then present our primary results, the agricultural productivity response in PL municipalities, and assess the plausibility of the underlying identifying assumptions before reflecting on the mechanisms driving the measured productivity increases.14 Figure 4 summarizes our main findings.

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11 The weights are calculated such that a loss function using the directed Kullback (1959) entropy divergence as a distance metric is minimized under a set of pre-specified balance constraints.

12 Matching and entropy balancing require that weights sum up to one. This adding-up restriction (Doudchenko and Imbens 2016) is only plausible if treated units are not outliers relative to control units. Our treated units are not outliers as several high-deforestation districts are not listed and thus fall into the control group.

13 **Table A1** in the online supplementary appendix shows that an alternative weighting scheme balancing the mean of deforestation and both the mean and standard deviation of the other covariates and the pre-treatment outcome deliver very similar results. Balancing the first two moments of all variables is not feasible.

14 We choose entropy balancing over other synthetic control strategies in the literature (Abadie and Gardeazabal 2003; Abadie, Diamond, and Hainmueller 2010, 2015) because the latter methods are more restrictive in that they do not allow for a permanent additive difference in the outcome between treatment and control units (no-intercept assumption; Doudchenko and Imbens 2016). Moreover, these methods are proposed for settings of comparative case studies with a single treated unit.

15 Since we evaluate the significance of multiple related outcomes, which increases the risk of wrongly rejecting a true null
We find that the PL significantly reduced deforestation, as suggested by previous studies (Arima et al. 2014; Assunção and Rocha 2014; Cisneros et al. 2015; Sills et al. 2015a). Our bias-adjusted matching specification shows that the treatment effect amounts to a $-55.8\%$ reduction in deforestation. When we use entropy balancing to construct the control group, the treatment estimate falls to $-49.9\%$. Taken together, both estimates indicate a substantively important percentage decrease in deforestation in priority municipalities over the 2008–2014 period as a result of the policy. When we translate the estimated relative treatment effect into absolute reductions in deforestation, our estimates suggest average post-policy reductions between 128.24 km$^2$ and 143.41 km$^2$ per municipality. At the basin scale this corresponds to avoided deforestation from 2008 to 2014 in the range of 4,617–5,163 km$^2$. Comparing our preferred estimators to the traditional DID estimates (reported in column 3 and 4) suggests that the latter substantially overestimate the impact of the PL. The direction of bias appears sensible because the control group in the simple DID model contains many districts with virtually no deforestation. In contrast, figure 2 shows that our matched control group (based on the two-step nearest neighbor matching procedure) exhibits much more similar pre-2008 deforestation patterns. While matching quality is not perfect for this outcome, our bias adjustment seeks to mitigate any remaining bias.

Our range of the percentage treatment effect is well in line with estimates between $-55.8\%$ and $-44.7\%$ in Assunção and Rocha (2014) for the period 2008–2011 (50 priority municipalities). Our estimates are, however, more pronounced than those found in Cisneros et al. (2015), who estimated a reduction between $-29.7\%$ and $-27.6\%$ for 2008–2012 (50 priority municipalities). Even so, these authors’ corresponding central estimate at the basin scale of 4,022 km$^2$ is close to our evaluation. Compared to Arima et al. (2014), who report absolute reductions per municipality in the interval $[-247.75, -53.59]$, our absolute estimates are at the lower end. The differences in the magnitudes of the PL’s impact estimated in different studies could arise...
because of a number of methodological choices. First, studies differ in the deforestation data used—while we used PRODES data, which includes estimates of how much deforestation occurs under cloud cover (but is not directly observed), Cisneros et al. (2015), for example, use raw deforestation estimates and correct for cloud cover in their regression analysis. Second, given our non-panel study design, we limit our analysis to the first municipalities placed on the list in 2008. Third, we have a longer post-policy time window than previous studies, spanning the period 2008 to 2014. Finally, we use a non-parametric estimator that either controls for bias due to insufficiently comparable treatment groups or builds on a synthetic control group.

Effects on Agricultural Productivity

Cattle grazing productivity. We find no evidence that that PL has displaced agricultural production, instead finding a significant positive effect of the PL on the productivity of cattle ranching. The magnitude of our estimates, however, varies considerably (see figure 4 and table 3). While the matching estimator suggests that priority municipalities have increased their cattle stocking rates by 36%, the estimated policy effect is 13.5% using entropy balancing. This range in the relative treatment effects corresponds to an economically meaningful absolute increase of 0.17 to 0.48 head/hectare.

A closer inspection reveals that the policy-induced productivity gain is driven by an increase in the cattle herd on the given pasture-land, as witnessed by significant positive treatment effects on the number of cattle heads but statistically insignificant effects on the pasture area.

The conventional DID estimates for the stocking rate and number of cattle heads generally indicate similar positive treatment effects of the PL as our preferred estimators. However, the two standard DID estimates for the stocking rate (cattle heads) seem to underestimate (overestimate) the policy impact mainly because the naïve control group includes observationally distinct municipalities with high (small) pre-policy stocking rates (cattle herds), as illustrated in figure 2.

Daily productivity. Next, we study the policy effect on dairy production. Row (3) of table 3 presents results when the outcome variable is dairy productivity as measured by thousands of liters of milk per milked cow. The treatment effect for this outcome levels out at -8% to -10%, but it is statistically insignificant.

Table 3. Estimation Results

| Main DID Estimates | Nearest Neighbor Matching Estimates | Entropy Balancing Estimates | Naïve DID Estimates | Basic +Municipality FE + Time FE + Controls |
|--------------------|-----------------------------------|-----------------------------|---------------------|------------------------------------------|
| (1) Deforestation  | -0.558** (0.223)                 | -0.499*** (0.143)           | -0.708*** (0.132)   | -0.660*** (0.135)                       |
| (2) Cattle productivity | 0.360*** (0.115)               | 0.135*** (0.044)           | 0.111 (0.106)       | 0.098* (0.052)                          |
| Cattle heads      | 0.249** (0.102)                 | 0.198*** (0.053)           | 0.389*** (0.111)    | 0.321** (0.102)                         |
| Pasture area      | -0.0673 (0.069)                 | 0.0689 (0.058)             | 0.034 (0.066)       | 0.053 (0.078)                           |
| (3) Dairy productivity | -0.102 (0.306)                | -0.0802 (0.087)            | -0.015 (0.035)      | -0.013 (0.033)                          |
| (4) Crop productivity | 0.462 (0.533)                 | 0.148** (0.061)            | 0.005 (0.031)       | 0.024 (0.031)                           |
| Production value  | 0.122 (0.709)                  | -0.0659 (0.118)            | 0.086 (0.069)       | 0.039 (0.064)                           |
| Cropland          | -0.439 (0.477)                 | -0.213* (0.12)             | 0.081 (0.064)       | 0.015 (0.055)                           |

Note: Reported results are based on log transformed data and can be interpreted as the average treatment effect in percentage terms. Standard errors appear in parentheses. Asterisks indicate the following: * = p < 0.1, ** = p < 0.05, and *** = p < 0.01.
Put differently, we find no empirical evidence for policy-induced productivity changes.

Crop farming productivity. Finally, we investigate the productivity response in crop farming of PL municipalities. Our main crop productivity measure is the gross crop production value per hectare of cropland of the six main crops (soy, maize, sugar, cotton, rice, and cassava) in the legal Amazon. While we find no significant effect using the matching estimator, the entropy balancing estimator yields a statistically significant point estimate (at the 5% level; see table 3). The measured effect suggests a 14.8% increase in the production value per hectare in municipalities on the PL. The marginally significant negative policy impact on the denominator of our productivity measure (i.e., cropland) suggests that the increase in productivity could be associated with a reduction in the area of cultivated land.

We caution, however, against placing too much significance on these cropping results. First, we find inconsistent results between our two estimation methods. Second, when we break down the aggregate productivity measure and estimate matched treatment effects for each individual crop, none of the estimated effects is statistically significant (table A2 in the online supplementary appendix).

Assessment of Identification Assumptions

Our main identification assumption that gives the above estimates a causal interpretation is that no unobserved variables exist that simultaneously influence changes in productivity and the probability of being inserted on the PL. We subsequently investigate the plausibility of the unconfoundedness assumption.

As a “sniff-test,” we revisit figure 2 plotting the average outcome variables of treated and matched control municipalities. Figure 2 shows that our two-stage research design has produced a subset of treated and control municipalities that exhibits sensibly homogeneous outcome trajectories in the pre-treatment period compared to the naïve control group of all 440 non-listed municipalities. With the only exception of dairy productivity in 2002–2004, outcome trends are parallel prior to 2008. Similarly, panel A of table 2 shows that all normalized differences in pretreatment agricultural outcomes are remarkably low and a simple t-test of equal pretreatment means in the two groups suggests that all differences in means are statistically insignificant. Note that these variables were not used in the two-stage matching procedure.

We can also look at a variable that was not used in the construction of matches to test whether matching achieves balance on unobserved variables. We use agricultural GDP per capita—measured after the introduction of the PL—for this purpose. A quantile-quantile plot for this variable (see figure A2 of the online supplementary appendix) and t-test (t-statistic: -0.32) show that the distributions and means of the agricultural GDP of listed and non-listed municipalities are very similar in the years after the policy enforcement. In contrast, without matching the test of equality of means is rejected at the 1% significance level (t-statistic: -3.93).

Our approach to assessing the tenability of the unconfoundedness assumption more formally relies on the use of outcome data for (a) the year 2007, (b) the years 2007 and 2006, or (c) the last three pre-treatment years. These pseudo-outcomes are known a priori not to be affected by the PL policy precisely because their values are determined prior to the policy introduction. Finding a significant treatment effect in this setting would cast doubt on the credibility of inferences. Table 4 shows that the treatment effect on all pseudo-outcome variables is statistically insignificant. This evidence suggests that the identified treatment effects are not the results of preexisting differences between priority and non-priority municipalities.

In addition to unconfoundedness, we must rule out the possibility of spillover effects from regulated to unregulated municipalities, for example, through leakage or deterrence. This assumption is referred to as the stable unit treatment value assumption (SUTVA). Following Cisneros et al. (2015), this can be tested by using the non-PL neighbors of priority municipalities as if treated. Indeed, 98 of the 440 non-priority municipalities in our sample share at least one border with a PL municipality. Finding an insignificant treatment effect for these direct neighbors would make it more plausible that the no-interference assumption holds. The final row in table 4 shows that the pseudo-treatment effects for all outcome variables are clearly statistically insignificant. To get an indication of the potential bias of our treatment effect estimates in the presence of spatial spillovers, in table A1 of the online supplementary appendix, we present entropy-balancing DID estimates that.
exclude direct neighbors from the control group. While the estimates for the agricultural outcomes are very similar, the lower treatment effect estimate for deforestation suggests that spillover effects may lead us to overestimate the policy impact.

What Drove the Observed Changes in Cattle Productivity?

Before discussing the potential mechanisms that drive the observed productivity gains in the beef sector, we use a simple back-of-the-envelope calculation based on our two estimated cattle productivity responses to translate the treatment effect estimates into tangible land-sparing effects. The measured beef productivity gains are equivalent to 0.17–0.48 greater head/hectare in priority municipalities, which would have required an additional 14,542–41,060 km$^2$ of pasture at 2008 stocking rates. These figures are substantially larger than our estimate of the avoided deforestation, 4,617–5,163 km$^2$ (see online supplementary appendix D for a further discussion of changes in land use under the PL). Though we cannot with our dataset definitively unpick the role of different mechanisms in driving the observed increase of cattle productivity, the magnitude of both the estimated stocking rate and land sparing effect suggest that a mix of factors probably contributed.

As discussed before, the PL makes illegal deforestation less attractive through two channels: (a) the policy increases the costs of clearing land, and (b) it reduces the benefits from clearing land. In doing so, the PL is expected to reduce deforestation and cause a substitution from land to capital. These effects are supported by previous literature on agricultural intensification. Villoria et al. (2014) propose that intensification occurs only where land is a scarce production factor and land opportunity costs are sufficiently high. Specifically in Brazil, Barretto et al. (2013) show that intensification in both cattle and crop production (from 1960 to 2006) occurred more in consolidated regions, where land was more scarce, than in frontier areas. Similarly, using a micro-level analysis of farm production in Rondônia state, Fontes and Palmer (2017) find that cattle stocking rates were higher and deforestation rates lower in farms that were closer to market, where opportunity costs were higher and less forest was available for expansion. These findings lend support to recent calls for prioritizing forest conservation in efforts to promote sustainable land use in the Amazon (Merry and Soares-Filho 2017).

Of course, scarcity-induced substitution requires yield-raising technologies to be available and affordable. Cattle ranching in the Amazon remains, for the most part, a low-input production system and there are many opportunities for increasing productivity through improved farm and pasture management, or the introduction of cattle feedlots (Barbosa et al. 2015). Soy production in the Amazon, is however, a technologically mature industry (more than 70% of Brazilian soybean production is genetically-modified; Garrett, Rueda and Lambin 2013), where yields are already comparatively high—which may make changes in yields in the soy sector less responsive to new efforts to constrain expansion.

Table 4. Assessment the Plausibility of Identification Assumptions

|                      | Deforestation | Cattle Heads | Dairy Productivity | Crop Productivity | Crop Value | Crop-Land |
|----------------------|---------------|--------------|--------------------|-------------------|------------|-----------|
| Unconfoundedness     |               |              |                    |                   |            |           |
| $Y_{2007}$           | 0.085         | 0.226        | −0.014             | 0.124             | 0.092      | −0.038    |
|                      | (0.30)        | (1.63)       | (−0.12)            | (0.68)            | (0.45)     | (−0.17)   |
| $Y_{2007} + Y_{2006}$| 0.350         | 0.103        | 0.102              | −0.197            | 0.102      | −0.030    |
|                      | (1.14)        | (0.91)       | (0.82)             | (−1.09)           | (0.43)     | (−0.12)   |
| $Y_{2007} + Y_{2006} + Y_{2005}$ | 0.021 | −0.023 | 0.067 | −0.103 | −0.100 | 0.003 |
|                      | (0.13) | (−0.17) | (0.96) | (−0.73) | (−0.77) | (0.02) |
| Spatial spillover    |               |              |                    |                   |            |           |
| Non-listed neighbors | −0.170        | 0.005        | 0.005              | −0.011             | 0.029      | −0.028    |
|                      | (−0.99)       | (0.07)       | (0.07)             | (−0.12)           | (0.23)     | (−0.24)   |

Note: The t-statistics appear in parentheses. We only have one pre-treatment observation (2004) for pasture area and thus our cattle productivity measure, which prevents us from estimating pseudo treatment effects for these two outcomes.
There is also evidence in the literature for the importance of incentives for speculative land clearing. A range of studies support the argument that cattle ranching in the Brazilian Amazon is used as a land speculation strategy (Hecht 1993; Walker et al. 2009; Richards, Walker, and Arima 2014), and that uncertain land tenure encourages clearing more land than is strictly required for agricultural production (Brown, Brown, and Brown 2016). In this respect, the increased regularization of land in priority municipalities, through increased enforcement and the adoption of the CAR—one of the requirements to be removed from the PL—may have further reduced incentives to clear new land. Farmers interviewed in Pará, for example, reported that they reduced their use of land as a result of registering their properties in the CAR (Jung et al. 2017), and Alix-Garcia et al. (2018) find that properties registered in the CAR in Mato Grosso and Pará saw a 62.5% reduction in deforestation. Similarly, Azevedo et al. (2017) find that the CAR has reduced deforestation, though the effect varied between states, property-sizes, and years.

While the mechanisms highlighted by our model have strong support in the related literature, we cannot with our dataset definitively rule out a number of alternative channels that could also play some role in explaining the observed productivity effect. First, the support provided by municipal governments and civil society organizations may have helped farmers to increase their productivity. Being included in the PL led to the “crowding in” of international and NGO support for the transition to sustainable land management (Cisneros et al. 2015). PL municipalities signed agreements with national and international donors (including the European Commission, the Fundo Vale, and the Amazon fund), as well as non-governmental organizations (NGOs; including The Nature Conservancy, Instituto Centro da Vida, and Imazon; Sassaki 2014; MMA 2015; Sills et al. 2015b). While in some cases these agreements included commitments to foster sustainable agricultural practices, in practice these efforts were small-scale (Piketty et al. 2015; zu Ermgassen et al. 2018), and municipalities had a far stronger emphasis on measures directly related to the conditions for being removed from the PL—reducing deforestation and supporting the municipalities in regularizing land use (Thaler 2017).

A second factor that may have played a role is reputational damage to “blacklisted” municipalities and farmers (Cisneros et al. 2015). Reputational risk may have added additional costs and incentives to those directly imposed by the policy. Finally, changes in the availability of agricultural credit have also received attention in the literature. While credit restrictions in frontier regions are thought to reduce deforestation rates (Assunção et al. 2013), and the PL was meant to involve tighter restrictions on credit for properties with illegal deforestation, when measured at the municipal-level at least, there appears to have been little effect on credit availability in PL municipalities (Assunção and Rocha 2014; Cisneros et al. 2015).

**Conclusion**

We find no evidence of a trade-off between agricultural production and one of Brazil’s flagship forest conservation policies, the municipal PL. These results corroborate the importance of strong environmental governance in guiding intensification without deforestation (Ceddia et al. 2014). While much work on agriculture-environment trade-offs has focused on how yield increases might spare forests from conversion (e.g., Burney et al. 2010; Tilman et al. 2011; Stevenson et al. 2013; Cohn et al. 2014), our results suggest that the causality can run both ways. At least in areas with large yield gaps and where high-yielding technologies are readily available (as in Brazilian cattle ranching), policies that induce land scarcity may induce intensification (Kaimowitz and Angelsen 2008; Merry and Soares-Filho 2017; Schwerhoff and Wehkamp 2018). We caution, however, that efforts to pair forest conservation and the implementation of high-yielding farming practices are likely to be more successful than any one intervention alone (Phalan et al. 2016).

The PL was a governance initiative working at the sub-national scale to control...
deforestation while maintaining agricultural production. As such, our results are relevant to the debate around multilevel efforts to control deforestation (Pedroni et al. 2009) and new sub-national models of sustainable development, that is, “jurisdictional approaches” for sustainable agricultural production and commodity sourcing (Nepstad 2017). We add to a young body of literature (e.g., Nolte et al. 2017) suggesting that sub-national efforts can be effective at reconciling forest conservation and agriculture.

Supplementary Material

Supplementary material are available at American Journal of Agricultural Economics online.

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