World Development 181 (2024) 106646

Contents lists available at ScienceDirect

World Development

journal homepage: www.elsevier.com/locate/worlddev

Regular Research Article

The economic consequences of environmental enforcement: Evidence from an anti-deforestation policy in Brazil

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ARTICLE INFO

Keywords:
Deforestation
Environmental enforcement and monitoring
Economic development
Amazon region
Brazil

ABSTRACT

Environmental degradation and economic development are two of the most pressing issues facing the world today, and public policy that aims to address one of these may unintentionally affect the other. I study the effect of an increase in environmental law enforcement on local economic conditions in rural Brazil. In the first part of this paper, I use data on more targeted anti-deforestation law enforcement activities, guided by satellite alerts, and link that to local forest conversion. I then exploit the staggered introduction of a policy that increases monitoring and enforcement in municipalities with a history of high deforestation, and link this to a range of economic development outcomes. I find that more targeted law enforcement reduces conversion rates of forest to farm land. Furthermore, economic conditions in municipalities with stricter monitoring improve, indicating that environmental enforcement and economic development need not be at odds.

1. Introduction

Environmental degradation and enabling inclusive economic development are two of the most pressing issues facing the world today, and the relationship between these two issues is complex, non-linear, and likely bidirectional (Jayachandran, 2021). A descriptive Environmental Kuznets Curve, where increases in incomes lead to more environmental degradation in the early stages of development only to reverse at higher income levels, may not adequately capture the relationship between economic development and environmental quality (Grossman & Krueger, 1995; Stern, 2017). At the same time, public policy that aims to address one of these issues may unintentionally affect the other. These subtleties in the relationship between the economy and the environment are yet to be fully understood.

This paper looks at the economic consequences of legislation that aims to protect a specific environmental good: primary forest in the Amazon region of Brazil. More specifically, this paper aims to address the question how an anti-deforestation policy affects local economic conditions in the Amazon region of Brazil between 2006 and 2017. Previous studies found that this policy successfully reduced deforestation rates (Assunção & Rocha, 2019; Cisneros, Zhou, & Börner, 2015; Koch, zu Ermgassen, Wehkamp, Oliveira Filho, & Schwerhoff, 2019). Deforestation and economic conditions are linked through decisions made by actors on the ground. For instance, rural households may use the forests as safety nets against agricultural risks (Delacote, 2007). Deforestation decisions are also influenced by demand for forest products (Foster & Rosenzweig, 2003), credit constraints and labour markets (Zwane, 2007), and the relative costs of investments in land quality and further forest clearing. The anti-deforestation policy implicitly increased the costs of forest clearing relative to investments in existing land, which could pave the way for agricultural intensification and economic development.

In 2004, the Brazilian federal government launched a comprehensive plan to combat deforestation in the Amazon Region. This plan institutionalized cooperation between different government institutions and modernized deforestation monitoring. Novel satellite capabilities enabled a more timely identification of ongoing deforestation activity. As a result, targeted law enforcement could be sent to those exact locations in an attempt to catch the perpetrators red-handed. In 2008, the Brazilian Ministry of the Environment launched the flagship policy when it published a list that included a first set of municipalities with exceptionally high deforestation rates (henceforth: *Priority List*). It allowed federal agencies to divert a large share of their funding and attention to this subset of municipalities, resulting in a more intensified monitoring and enforcement on the ground. More than half of the

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[1] Thanks to Martina Björkman Nyqvist, Abhijeet Singh, Pamela Campa, Oliver Engist, Felix Schafmeister, and Mara Balasa for useful feedback, as well as seminar participants at the Stockholm School of Economics and Misum for additional comments. Financial support by the Jan Wallander and Tom Hedelius foundation is gratefully acknowledged.

https://doi.org/10.1016/j.worlddev.2024.106646
Accepted 20 April 2024
Available online 14 May 2024

deforestation satellite alerts and law enforcement efforts take place in municipalities on the *Priority List*, even though these municipalities only account for 10 percent of the Amazonian municipalities.

In the first part of this paper, I use geolocated data on deforestation satellite alerts and law enforcement activity, and link this to forest conversion to farm land to identify deterring effects of targeted law enforcement. However, the occurrence of targeted law enforcement may be a result of forest conversion activities themselves, as law enforcement acts on signals of ongoing forest clearing. To deal with this potential issue of endogeneity, I use average cloud cover throughout the year as an instrument for deforestation satellite alerts. Satellites cannot pick up deforestation hotspots if the area is clouded, which in turn reduces the likelihood of law enforcement on the ground.[2]

My findings suggest that targeted law enforcement activity was effective in preventing standing forest to be converted to farm land. Both satellite deforestation alerts and actual on-the-ground law enforcement action reduced the probability that plots of land became farm land in the year after. These negative effects on conversion rates fade away as the distance between the law enforcement action and the plot of land increases. Using cloud cover as an instrument for law enforcement confirms this conclusion. Taken together, these results signal that this policy may have pushed local agricultural actors away from additional land clearing into more productive activities.

In the second part of the paper, I exploit the staggered spatial expansion of the *Priority List* policy. The number of municipalities with higher monitoring and enforcement increased from 36 (out of 552) in 2008 to 52 between 2009 and 2017. At the municipal level, I find that economic outcomes such as GDP per capita and poverty rates develop positively after these municipalities were placed on the *Priority List*. My findings suggest that in this specific setting of anti-deforestation policies in Brazil, economic development and environmental protection are not at odds. I estimate that municipalities that are placed on the *Priority List* experience an increase of about six percent in GDP per capita. The share of families that receive income from a nation-wide conditional cash transfer reduces by about two percentage point in treated municipalities. In a dynamic setting, it seems that these positive effects are not instantaneous, but instead materialize after about five years, albeit it noisily. This all indicates that favourable economic changes take time to come about.[3]

I test a number of competing hypotheses that could partially explain the findings and can rule out most of these hypotheses. Migration may be a mechanism, as farmers in treated municipalities may suddenly face higher deforestation costs and therefore look for plots of land elsewhere. I find that municipalities on the *Priority List* experience slightly lower rates of population growth, but that this effect is noisy. Infrastructure projects, as proxied by federal roads, are not targeted to these municipalities on the *Priority List* either and cannot explain economic development. As for sectoral changes in the municipality, I find some evidence that agriculture becomes more important as a share of municipal GDP. Exports of agricultural products also increase more than exports of all other goods. In addition, the use of fertilizer and being connected to the electricity grid increased in targeted municipalities as well, while average production revenues at farms was also higher. All of this points towards agricultural intensification as the most plausible mechanism.

This paper contributes to two branches of literature. Most prominently, it builds on existing knowledge about the effectiveness of the anti-deforestation policy in question, but provides more detailed, and additional insights. Previous papers have predominantly used municipality-level data. Linking *Priority List* municipalities to reduced level of deforestation, Assunção et al. (2023) use a similar approach as this paper, but keep the municipality as the unit of analysis. My more detailed approach, using more precise geolocated law enforcement, confirms their results. Looking at agricultural outcomes directly, Koch et al. (2019) find that cattle productivity (defined as heads of cattle per hectare) increases by 13–36 percent in *Priority List* municipalities. Moffette, Skidmore and Gibbs (2021) estimate that being placed on the *Priority List* increased cattle productivity increased by about 12 percent. Harding, Herzberg, and Kuralbayeva (2021) stress the importance of commodity prices as a driver of the success of the *Priority List* policy. This paper takes a step back and aims to uncover the main drivers of the agricultural adjustments behind the success of this anti-deforestation policy. It does so by looking at the effect of enforcement of environmental regulation on local forest clearance behaviour and whether these productivity improvements found in the literature translate in gains for the local economy.

Second, the paper also contributes to our understanding of the costs and benefits of environmental enforcement in a rural and remote region. The effectiveness of environmental monitoring and enforcement practices have been studied in a variety of settings. However, most of the literature looks at high-income countries where regulatory environments are supposedly strong (Gray & Shimshack, 2011; Grooms, 2015; Telle, 2013), and find that monitoring and enforcement is generally successful in reducing violations of environmental legislation (see Shimshack (2014) for a literature review). Three recent studies show that more targeted monitoring and enforcement may lead to higher compliance with existing regulation (Blundell, Gowrisankaran, & Langer, 2020; Duflo, Greenstone, Pande, & Ryan, 2018; Kang & Silveira, 2021). Our knowledge on environmental enforcement linked to deforestation is rapidly expanding (Assunção et al., 2023). This paper fits next to that of Ferreira (2023), who finds that use of satellite alerts is successful in increasing the probability to detect ongoing deforestation activity.

The following section sketches the background of the policy change. The third section introduces the data and provides some summary statistics. The section thereafter elaborates on the empirical strategy, after which I present the results. The last section concludes.

2. Background

The purpose of this section is threefold. First, I provide an insight into the setting of anti-deforestation laws in Brazil in the last two decades and then focus on the main policy. In the second part, the use of satellite imagery in the setting of anti-deforestation law enforcement is discussed. The third part elaborates on the link between deforestation and local economic conditions.

2.1. Anti-deforestation policies since 2004

In the early 2000s, the federal government of Brazil implemented a set of policies aimed at reducing deforestation levels across the Amazon region. The first phase of this Action Plan (*Plano de Prevenção e Controle do Desmatamento na Amazônia Legal*, or PPCDAm) was implemented in 2004 and contained several conservation policies, institutionalized cooperation between different government institutions, and modernized deforestation monitoring. This policy was introduced against changing deforestation patterns in the early 2000s, when clearings by large landowners became quantitatively less important, while the number of smallholder clearings remained stable (Godar, Gardner, Tizado, & Pacheco, 2014; Rosa, Souza, & Ewers, 2012).

[2] A more aggregated version of this instrument, at the municipality level, is also used in Assunção, Gandour, and Rocha (2023), who find that deforestation is greatly reduced by law enforcement action on the ground.

[3] Existing evidence on the relationship between income and deforestation is mixed (Alix-Garcia, McIntosh, Sims, & Welch, 2013; Khan & Khan, 2009; Oldekop, Sims, Karna, Whittingham, & Agrawal, 2019).

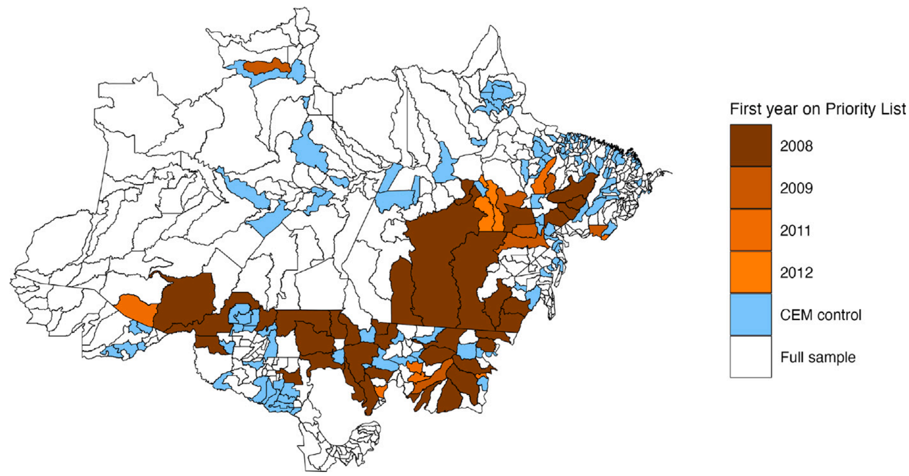

Fig. 1. Expansion of the *Priority List* and control groups. *Notes:* This figure displays the location of *Priority List* municipalities, colour-coded by year of addition to the list. All municipalities shown are included in the analysis, unless otherwise stated. The CEM control group contains municipalities that are relatively close matches to the *Priority List* municipalities, see Section 3.2 for more details. Data comes from the Brazilian Ministry of the Environment. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Priority list

The second phase of the PPCDAm started a few years later and included the main policy of interest in this paper. The *Priority List*, which was signed in 2007 and entered into force in 2008, was a published list of municipalities that had the highest incidence of deforestation. More concretely, the policy allowed for specific targeting of federal resources towards these identified municipalities to strengthen the enforcement of anti-deforestation policies in municipalities on that list.[4] There were 36 municipalities on this original *Priority List*. The *Priority List* is updated every year, and since 2009 also includes criteria for municipalities to be removed from the list. Fig. 1 provides an overview of the number of municipalities on the *Priority List* over time. In total, 52 of the 553 municipalities were added to the list between 2008 and 2016.

Being placed on the *Priority List* meant that federal agencies such as the Brazilian Institute for the Environment and Renewable Natural Resources (Portuguese abbreviation: IBAMA) were allowed to target these municipalities with a larger share of their resources. This increase in resources meant that monitoring and enforcement of existing anti-deforestation legislation in these *Priority List* municipalities was more likely. IBAMA created field offices in the targeted municipalities to reduce the cost of sending law enforcement teams to ongoing deforestation hotspots, which they identified by the analysis of real-time satellite data that monitors land cover changes (DETER). In case a deforestation hotspot was identified, law enforcement would travel to the location and inspect adherence to the Forest Code.[5] A direct result of the *Priority List* was that farms/landholders in municipalities on the *Priority List* were more likely to be fined/embargoed when their deforestation activities triggered an alarm in DETER, compared to non-treated municipalities. In economic terms, this policy increased the expected costs of forest conversion, reducing the incentives to do so for all actors.

The *Priority List* was successful in reducing deforestation, with estimates suggesting around 30 percent lower deforestation (Assunção & Rocha, 2019; Cisneros et al., 2015). Neighbouring municipalities also experienced a reduction in deforestation. Spatial spillovers from 'listed' municipalities also increased the risk of being detected for illegal deforestation activities in neighbouring municipalities (Andrade & Chagas, 2016; Assunção, McMillan, Murphy, & Souza-Rodrigues, 2023). As prior literature found that PPCDAm was cost-effective in reducing deforestation, a number of mechanisms was put forward that could explain why municipalities responded to this federal policy of being placed on the *Priority List*. Among these were additional administrative requirements (land titles of farmers had to be registered in a centralized registry - CAR), reputational risk, and external pressure from NGOs and other levels of government (Cisneros et al., 2015).

2.2. Satellite alerts and law enforcement

Satellite imagery has been used for remote sensing of land on earth since the launch of Landsat 1 in 1972. However, these first generation earth observation satellites were not always equally successful in generating complete images of the Amazon rainforest due to cloud cover. As satellites, their payload, and analytical tools improved, satellite-derived maps of deforestation patterns became more useful for various actors, including law enforcement agencies (although Moffette, Alix-Garcia, Shea and Pickens (2021) find that it this is only useful under certain conditions). Since 2004, a satellite-based monitoring system developed by Brazil's Space Agency (INPE) is an integral part of the enforcement of Brazil's anti-deforestation policies. Every two weeks, this system provides deforestation alerts of either clean-cut areas, or ongoing forest degradation through logging, mining, or forest fires (Sales, Strobl, & Elliot, 2022). This information is then submitted to the federal environmental police department of IBAMA, or to state environmental agencies.

After a DETER satellite alert of ongoing deforestation activities, IBAMA can decide to send a law enforcement team to that location with the aim to catch the perpetrators red-handed. Over time, the quality of alerts to correctly detect deforestation has greatly improved. For instance, Ferreira (2023) finds that the share of false positive satellite alerts decreased from about 50 percent in 2011–2012 to less than 30 percent since 2017. Regardless, once a violation of the Forest Code has been established, IBAMA could write out fines, embargo the plot of land, or confiscate machinery used in these illegal activities. The increased focus on *Priority List* municipalities in enforcing the Forest Code is also reflected in the number of law enforcement actions. Arima, Barreto, Araújo, and Soares-Filho (2014) calculate that in municipalities on the *Priority List* between 2007 and 2008 the

[4] The criteria to be placed on the original list of 2008 were: the total deforested area, total deforestation in the last three years, and whether there was an increase in the annual deforestation rate for three or more times in the last five years. The actual and precise criteria were not known in 2008, but were published two years later.

[5] The Forest Code meant that private landowners had to maintain 80% forest cover in its native state, though state laws may reduce this to 50% for agricultural production zones (Brandão et al., 2020).

number of embargoes increased by 53 percent (versus a 11 percent increase in other municipalities), while the number of fines increased by 13 percent (versus a decline of 10 percent in other municipalities). The combination of higher probability of law enforcement and high monetary costs in case of fines and embargoes means that deforestation became more costly in *Priority List* municipalities.

2.3. From deforestation to local economic consequences

As described above, the *Priority List* policy was successful in achieving its primary goal of reducing deforestation. There are a number of potential mechanisms that drove the reduction in deforestation rates, with some indication that commodity prices played a role (Harding et al., 2021) as well as a reduction in access to credit (Assunção, Gandour, Rocha, & Rocha, 2020). The satellite alert system and law enforcement activity discussed above may be another factor, if it affects the behaviour of the agricultural actors on the ground. If law enforcement responds to an alert and finds actors pursuing activities in violation of the Forest Code, this could affect future decisions of the people directly affected as well as that of their immediate social network.

In effect, this policy is aimed towards raising the costs of deforestation, which is nearly always illegal in the Amazon region of Brazil. It does so implicitly by increasing the probability of law enforcement identifying ongoing deforestation activity through the satellite alert detection system. It also raises the costs explicitly, by confiscating equipment, writing fines, and embargoing plots of land which are found to be in violation of the Forest Code. These higher costs may deter deforestation activities, if it becomes too costly.[6]

These higher costs of deforestation could push farmers to invest in more intensified uses of existing land as opposed to creating new land for cattle or crops. According to Koch et al. (2019), this is exactly what happened in municipalities on the *Priority List*. They find an increase in cattle production and higher cattle productivity. In similar fashion, Moffette, Skidmore et al. (2021) find that being on the *Priority List* does indeed lead to higher cattle productivity, and that this does not crowd-out the effect of other anti-deforestation policies (such as the G4 agreement that prohibits cattle ranched on deforested land to be sold to the largest slaughterhouses). However, these studies use municipal-level data for their analysis of agricultural outcomes, and are therefore less likely to be able to explain the changes at a more local level. Through the use of more local data, this paper aims to provide insights into the precise agricultural responses. For instance, agricultural intensification has been linked to structural transformation of the economy, and indeed increasing income levels (McArthur & McCord, 2017). Bustos, Caprettini, and Ponticelli (2016) find that technical advancement in crop agricultural practices led to sectoral changes of the local economy of Brazilian municipalities. Whether these changes also took place as a result of an increase in environmental law enforcement and monitoring, and how this translated in local economic development are the main questions of this paper.

3. Data

3.1. Local forest conversion

The analysis on local forest conversion is done at a raster cell level of 3000 × 3000 m. That is to say, I rasterize the entire Amazon Biome region in equally sized squares, and assign the relevant variable values to each raster cell. There are 472,567 raster cells in the Amazon.[7] The observational unit is therefore a square of 9 km^2, with years as relevant time dimension. Descriptive statistics for this section can be found in Table B1.

Forest conversion. The agricultural outcome variables of interest is local forest conversion and comes from the Brazilian Annual Land Use and Land Cover Mapping Project (MapBiomas). This project provides information on a number of agricultural indicators at different spatial dimensions for the years 1985 to 2020 (Souza et al., 2020). I use data at a spatial dimension of 100 × 100 m for the years 2006–2017, and aggregate this to a spatial dimension of 3000 x 3000 m to match my other variables. The main outcome is the conversion of a plot of land from standing forest to farm land. More specifically, I define conversion to farm land as a raster cell being classified as farm land (pasture or crops) in year t, while it was classified as forested land in year $t-1$. About 1.37 percent of the raster cells are converted to farm land during the years 2006 to 2017.

Satellite alerts. The satellite alert program (DETER) introduced in Section 2.2 produces monthly deforestation alerts, which are publicly accessible as shapefiles. I assign these geolocated polygons to the raster covering the Amazon Biome, creating a dummy variable that takes the value of 1 if the satellite alert detected ongoing deforestation in that raster cell. These alerts often cover multiple raster cells, and can indeed assign large pieces of forest as undergoing deforestation activity. About 0.5 percent of the raster cells in the data were part of such a satellite alert between 2005 and 2016.

Law enforcement. Records of law enforcement movement or deployment is not publicly available. As a second best, I therefore use geolocated information on locations where law enforcement successfully identified violations of the Forest Code, and found people or equipment to link to these violations. This information is made public by IBAMA, but not all entries have correct coordinates in the years 2005 through 2016. About 22 percent of the law enforcement records are therefore dropped from the analysis, as they cannot be geolocated within the geographical area of the Amazon Biome of Brazil. For the purpose of this analysis, it does not matter what type of punishment was decided upon, as long as it is an environmental violation. The existence of a record in this database indicates that law enforcement identified a violation, and were able to assign blame to one or more persons. Some entries have relatively coarse coordinates. As I overlay these points with the raster, it is possible that one law enforcement point spans multiple raster cells. This could lead to imprecise assignment of law enforcement activity, but should only lead to classical measurement error in the empirical analysis. In the main dataset, about 1.4 percent of the raster cells are ever part of a law enforcement activity between 2005 and 2016.

Cloud coverage. Similarly to the satellite alerts, DETER also produces monthly polygon shapefiles on cloud coverage. In similar fashion as with satellite alerts, I overlay these polygons on my raster file and assign for every month which raster cells are covered by clouds. This is then aggregated to the yearly level by calculating the share of the year in which cloud coverage prohibited satellites from identifying deforestation hotspots. The average pixel is covered by clouds about half of the year, but there is ample variation between and within pixels in the data. Fig. 2 is a geographical representation of the raw data for the month of October in 2008. Cloud coverage is displayed by the grey polygons, and the satellite alerts of ongoing deforestation activity are the black diamonds. There are only few satellite alerts in areas covered by clouds for that month (though note that the analysis below is conducted at the annual level).

[6] This fits the standard crime literature framework, where the agents undertake illegal activities if the expected proceeds of such action are larger than the perceived costs. The costs are defined as the probability of detection times the punishment severity. As this policy raises the former, one may expect criminal behaviour to be reduced.

[7] The total number of cells is not an even number, because I only include cells for which the centroid is in a municipality that falls within the Amazon Biome.

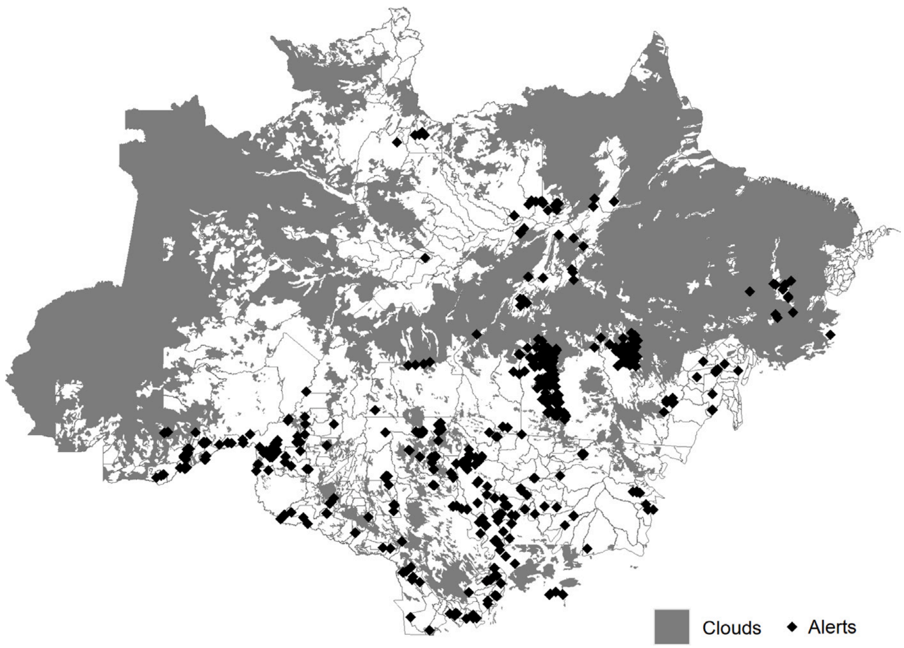

Fig. 2. Cloud coverage and satellite alerts in October 2008. *Notes:* This figure shows the raw cloud coverage and satellite alert data for October 2008, where the polygons are clouds and the diamonds satellite alerts.

Other variables. As this part of the analysis is done at the raster cell level, I assign to each raster cell the corresponding municipality code as well as a dummy indicating whether the centroid of each raster cell is within a municipality on the *Priority List* that year. This latter data comes from the Brazilian Ministry of the Environment (*MMA*, by its Portuguese abbreviation). 15 percent of the raster cell observations are located in *Priority List* municipalities.

Moreover, I control for lagged weather at each location, which comes from the Climatic Research Unit at the University of East Anglia (Harris, Jones, & Osborn, 2021). This data is available by month at the 0.5 × 0.5 degrees grid level, which I assign to each raster cell that is covered by the weather grid pixel. I then calculate the average temperature for each raster cell, as well as the total precipitation by year. Average temperatures range between 24 and 29 degrees, with an average of 27. Total precipitation ranges from no rain in a year to 3800 mm.

3.2. Municipal economic outcomes

For the analysis at the municipality level, I create a panel with all 552 municipalities with a non-zero share of their territory in the Amazon Biome for the years 2006–2017. I exclude the 7 capital cities of the Amazonian states in this analysis. I therefore end up with 545 municipalities for 12 years as final dataset.

Priority list. As discussed in the background Section 2.1, there are 52 municipalities that are ever placed on the *Priority List* between the first list in 2008 and 2016. This list is published every year by the Brazilian Ministry of the Environment (MMA). The dynamics of the number of *Priority List* municipalities by year are shown in Fig. 1. In total, municipalities on the *Priority List* make up 7 percent of the municipal-year observations.

Economic development. The two main dependent variables are all proxies for economic development, but all measure it from a different angle. The first is GDP per capita, and captures the formal sector. Data comes from the Brazilian statistical office (IBGE) and is measured at the municipal-year level. GDP per capita is reported in real terms. I take the logarithm of this variable in my analysis, and its descriptives can be seen in Table B2.

Secondly, I proxy economic development by looking at the share of families that receive income support through the largest conditional cash transfer program in the world: Bolsa Familia.[8] Data on recipients at the municipal level is available from the Brazilian Ministry of Social Development (MDS, since 2019 Ministry of Citizenship). It provides monthly information on the number of recipient families in each municipality and the value of the expenditures on Bolsa Familia for each month between 2006 and 2017. I use this data to create my poverty measure: the share of families in each municipality that receive at least some support from Bolsa Familia. Table B2 displays the summary statistics for this outcome. On average, 43 percent of the families in this rural region of Brazil receive at least some form of payment from Bolsa Familia.

Secondary outcomes. There are a number of potential channels through which the patterns in the main economic development outcomes can come about. These mechanisms include migration, sectoral change of the local economy, federal infrastructure projects, and agricultural intensification.

I proxy migration by looking at population growth, with annual data coming from IBGE. Sectoral change of the local economy, expressed as share of different sectors in municipal GDP, is also available annually from IBGE. International trade data comes from the COMEX database collected by the Ministry of Industry and International Trade. Data on federal infrastructure projects is less frequently published. More precisely, for federal road projects there are two data points available; 2005 (pre-treatment) and 2014 (post-treatment), while data on water supply is available for 2008 (pre-treatment) and 2017 (post-treatment). For agricultural intensification, I rely on IBGE agricultural censuses of

[8] Bolsa Familia is one of the largest and oldest Conditional Cash Transfer (CCT) programs in the world. It started in 2003, and since covers the entire country of Brazil. The program has a number of specific payment schemes, based on household characteristics. Households characterized as extremely poor receive a fixed amount per month. Households characterized as poor receive an amount based on fulfilment of a number of conditions such as prenatal and postnatal health care visits for pregnant women, and attendance requirements for families with school-aged children as well as required vaccinations (as per Brazilian *Caixa bank*, responsible for the program).

2006 (pre-treatment) and 2017 (post-treatment). I look at the distribution of farm sizes (small, medium-sized, large), fertilizer use, per farm production value, and electricity connectivity. See Table B2 for summary statistics.

Control variables. A set of control variables is needed to isolate the effect of being on the *Priority List* on municipal economic outcomes. The first control variables are lagged weather, which means taking the weather data from Section 3.1 and aggregate that to the municipality level, to create municipality-year values for average temperature and total precipitation. Additionally, I create a municipality-year specific commodity price series. More concretely, I use the 2006 (pre-treatment) share of agricultural land for rice, sugar cane, cassava, corn, soy beans, and cattle using the 2006 Agricultural census of IBGE, and interact that with annualized average prices for these products in the Brazilian state of Paraná. This creates a weighted-price index at the municipality-year level. I then apply principal component analysis, and use the first component as a control variable in the relevant specifications, following Assunção, Gandour, and Rocha (2015) and Assunção et al. (2023).

In order to find a more comparable set of control municipalities, I apply a matching algorithm developed by Blackwell, Iacus, King, and Porro (2009). I use 5 pre-treatment covariates to find a more comparable control group. More specifically, the 2004 levels of standing forest, GDP per capita, the share of agriculture in GDP, and population density are used, as is the area size of the municipality. As a result, all 52 treated municipalities are matched with a more comparable control group of 115 municipalities.[9]

4. Empirical strategy

I use several distinct approaches to quantify the consequences of the anti-deforestation policy. First, I look at the agricultural effects of the policy. In the Amazon region, deforestation is mostly an agricultural decision, so this is a natural starting point. This analysis is done at the local level, namely raster cells of 3000 m × 3000 m (see Section 3.1). Second, I look at a number of economic outcomes at the municipality level to study the potential tension between agricultural preservation and economic development. As these empirical strategies require distinct approaches, they are introduced separately in Sections 4.1 and 4.2, respectively.

4.1. Agricultural response to anti-deforestation policies

4.1.1. Law enforcement and satellite alerts

The first part of the empirical strategy exploits the main operational tool of the anti-deforestation policy; satellite alerts of on-going deforestation efforts that feed into law enforcement agency actions. As discussed in the data section (Section 3.1), these satellite alerts and law enforcement actions are geolocated. I use this feature of the data to analyse the effect of these satellite alerts and law enforcement action on the probability that a plot of forest becomes farm land.

In the OLS specification, I estimate the following baseline regression:

$$\begin{aligned} Y_{i,t} \; = \; & \alpha_1 Enforcement_{i,t-1} + \alpha_2 Alert_{i,t-1} + \alpha_3 PL_{i,t-1} + \\ & \delta X_{i,t-1} + \gamma_t + \gamma_i + \varepsilon_{i,t} \end{aligned} \tag{1}$$

where $Y_{i,t}$ is the variable of interest: the probability of forest conversion to farm land in year t in pixel i. $Enforcement_{i,t-1}$ is a dummy that takes the value 1 if pixel i in year $t-1$ is visited by law enforcement officials, and a violation of the Forest Code has been registered. Similarly, $Alert_{i,t-1}$ is a dummy variable that takes the value 1 if pixel i is included in one of the satellite alerts denoting on-going deforestation in year $t-1$. $PL_{i,t-1}$ is a dummy that takes the value 1 if pixel i is in a municipality on the *Priority List* in year $t-1$. $X_{i,t-1}$ are the control variables, average annual temperature and total annual precipitation. The γ's are year and pixel fixed effects, respectively. I also estimate a specification where I include an interaction term of *Enforcement* and *PL* to estimate *Priority List*-specific effects.

An open question that remains is the spatial nature of the effect of these satellite alerts and on-the-ground law enforcement actions. In a situation where deforestation is detected by the satellite, and law enforcement takes action, it is impossible to ex-ante define the spatial spillovers on neighbouring plots of land. The precise limit of expected effects is unknown, which I address by providing point estimates for different radii around the geolocated satellite alerts to study these spatial dynamics. I expect forest conversion in the vicinity of the satellite alerts to be more affected than those plots of land further away.

4.1.2. Law enforcement, satellite alerts, and cloud coverage

A concern with estimating the effect of law enforcements on agricultural outcomes using the strategy outlined above is that of simultaneity. The relationship between agricultural outcomes and law enforcement goes two ways; from law enforcement to agricultural outcomes (what I am interested in identifying) and agricultural outcomes leading to law enforcement action. More specifically, I aim to estimate the effect of law enforcement action on the behaviour of local economic actors by looking at their decision to convert land to farm land. However, the reverse causal relationship may also exist, where law enforcement acts based on already ongoing agricultural behaviour on the ground. Below, I will first elaborate on the estimation strategy to address this potential endogeneity, and then discuss the identification and some validity tests.

4.1.3. Cloud cover as instrument for law enforcement

I use an instrumental variable approach following Assunção et al. (2023) and Sales et al. (2022). I instrument law enforcement by a measure of cloud coverage throughout the year. Clouds are a natural phenomenon throughout the rainforest, though less so in the dry season than during wetter months. Cloud cover is a valid instrument for satellite alerts if, conditional on average annual temperature, total annual precipitation, and location fixed effects, cloud cover is uncorrelated with the outcome variable other than through the deforestation alerts. For a more detailed discussion of using cloud cover as an instrument for law enforcement in this setting, see Section A1.1 in the Appendix.

For the current generation of satellite payload, clouds inhibit detection of on-going deforestation. This, in turn, reduces the likelihood of law enforcement presence on the ground. I loosely follow the approach used by Assunção et al. (2023), but depart from their paper as they conduct their analysis at the municipality level, where average municipal cloud coverage instruments law enforcement in the entire municipality. My analysis in this part of the paper is done at the raster cell level, which allows for a more precise estimation of the agricultural response to anti-deforestation policies.

4.2. Economic consequences of anti-deforestation policies

In the analysis at the municipality level, I employ a generalized difference-in-differences specification to estimate the effect of the deforestation policy on local economic outcomes. The unit of observation is a municipality, and the time unit is a calender year. Every specification also includes municipality and year fixed effects. As a baseline, I estimate the following:

$$Y_{i,t} = \beta PL_{i,t} + \delta X_{i,t} + \gamma_i + \gamma_t + \varepsilon_{i,t} \tag{2}$$

where $Y_{i,t}$ is the dependent variable of interest, $PL_{i,t}$ is a dummy that takes the value of 1 if the municipality i is on the *Priority List* in year t. $X_{i,t}$ are a set of control variables; the average annual temperature, the total annual rainfall, a municipality-specific price index of agricultural

[9] See Table B5 in the Appendix for descriptive statistics of this matching exercise.

good based on agricultural production in 2000, the lagged number of IBAMA embargos and fines, and the lagged number of DETER satellite alerts. γ_i and γ_t are municipality and year fixed effects, respectively.

In order to overcome the potential problem of comparing municipalities at the deforestation frontier of the Amazon with municipalities deep inside the interior of the Amazon, I also apply a coarsened exact matching algorithm (Blackwell et al., 2009) to match treated and untreated municipalities that share observable and comparable baseline characteristics. The baseline characteristics are the share of standing forest, population density, GDP per capita, the share of agriculture in municipal GDP, and the area size of the municipality. All characteristics except for the last one are pre-treatment values for the year 2004. Through this algorithm, I manage to find a more comparable control group to the municipalities on the *Priority List*, see Table B5.

The assumption that lends the β a causal interpretation is that of parallel local economic dynamics in the absence of the deforestation policy. An empirical approach to test this parallel-trends assumption is to include forward-looking dummies that should capture anticipation effects. The absence of statistical significance of these dummies is an indication that the local economic dynamics in the run-up to the introduction of the policy are similar. In addition to these forward-looking dummies, I also include backward-looking dummies to identify the dynamics of the effect beyond the introduction year of the policy. Potentially, local economic changes take time to materialize. These backward-looking dummies then capture these dynamics. More precisely, I estimate the following dynamic specification:

$$Y_{i,t} = \sum_{\tau=-q}^{-1} \alpha_\tau PL_{i,t+\tau} + \sum_{\tau=0}^{p} \beta_\tau PL_{i,t+\tau} + \delta X_{i,t} + \gamma_i + \gamma_t + \epsilon_{i,t} \quad (3)$$

The backward-looking dynamics are captured by the α terms, and I include three such dummies in my main specification. The dynamic effects after the policy has started are captured by the β terms, where β_0 is the immediate treatment effect. I include seven lags in this specification, to capture the effects over a sufficiently large time period.

TWFE considerations. A recent set of papers has identified a number of potential caveats in two-way fixed effects (TWFE) regressions, often in settings with heterogeneous treatment effects across groups and/or over time (De Chaisemartin & D'Haultfoeuille, 2022). The potential problems mostly relate to situations where a staggered introduction of the policy forces standard (dynamic) TWFE models to estimate the treatment effect of units that are treated "late" by using "early" treated units as comparison group. However, if we cannot rule out heterogeneous treatment effects, Goodman-Bacon (2021) show that this comparison yields a negative weight on one of average treatment effects. On top of that, Borusyak, Jaravel, and Spiess (2021) show that in settings with a staggered introduction of a policy and a binary treatment indicator, such as the policy introduction in this paper, negative weights may lead to misleading estimates even in the absence of heterogeneous treatment effects. As a result, I present the dynamic TWFE results using three different estimators (Borusyak et al., 2021; Callaway & Sant' Anna, 2021; De Chaisemartin & D'Haultfoeuille, 2022).

The main results will use the approach outlined in Borusyak et al. (2021), while the other two estimators are presented in the Appendix. The reason for this is the intuitively simple "imputation" method that they introduce. Their estimators are computed by running a regression of any outcome on group and time fixed effects, but only for the untreated sample. This is then used to predict a counterfactual outcome for the treated sample, where the treatment effect is the difference between the actual outcome and the calculated counterfactual. The alternative proposed by De Chaisemartin and D'Haultfoeuille (2022) has the benefit of allowing the treatment to be turned off (which happens once municipalities are removed from the *Priority List*), and the Callaway and Sant' Anna (2021) estimator requires a weaker parallel trends assumption and provides a range of estimators based on different levels of aggregation.

Table 1
Results — cloud coverage and law enforcement (IV and OLS).

Dependent variable:	First stage	Second stage	OLS
	Law enforcement	Probability of conversion	
	(1)	(2)	(3)
Cloud coverage	−0.586*** (0.042)		
Law enforcement		−0.004*** (0.001)	−0.002*** (0.000)
Controls	Average temperature & total rainfall		
Fixed effects	Raster cell & Year		
Observations	5,667,036	5,667,036	5,667,036
F-statistic	195.74		
Dep. var mean	0.015	0.013	0.013

Notes: This table shows the first-stage results following Eq. (4) in columns 1 and the second-stage result following Eq. (5). In the first column, the dependent variable is a dummy indicating law enforcement actions taken place in raster cell i in year t. The F-statistic for weak instruments the Kleibergen–Paap Wald F-statistic. In the second column, the dependent variable denotes the probability of conversion from forest to farm land in raster cell i in year t. The third shows the law enforcement coefficient, estimated using OLS. This result comes from column 1 in Table B3. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.10$.

5. Results

5.1. Agricultural response to enforcement

This section starts off by estimating the effect of the increase in enforcement and monitoring on probability of forest conversion to farm land, following the strategy outlined in Section 4.1. It aims to answer the question to what degree satellite alerts and corresponding law enforcement actions have influenced local actors' decisions whether or not to engage in illegal deforestation behaviour.[10]

5.1.1. Does law enforcement affect land use outcomes?

A graphical representation of the estimated relationship between law enforcement action and the probability of forest conversion to farm land is shown in Fig. 3.[11]

Fig. 3 combines estimated effects using OLS (the solid lines for *Priority List* municipalities and other municipalities) and IV (the dashed line for all municipalities). Results from the OLS approach shows that a raster cell located in a *Priority List* municipality is 0.4 percent less likely to be converted to farm land if law enforcement action occurred in that specific cell. This effect is much smaller for non-*Priority List* municipalities, and indeed not significantly different from zero. For both groups, the estimated effects become weaker the plot of land is from the geolocated law enforcement action. If a plot of land in *Priority List* municipalities is more than 50 km from where the closest law enforcement has occurred, it no longer is affected by law enforcement.

As discussed in Section 4.1.2, OLS may yield biased results, as reverse causality is a concern (law enforcement as a result of ongoing forest conversion). Table 1 shows the results for the instrumental variable approach using cloud cover as an instrument for law enforcement.[12] Column 1 shows the First-Stage results of estimating Eq. (4), and indicates that cloud cover is a strong instrument for law enforcement, with an F-statistic of 196. More clouds indicate a lower probability of law enforcement. Using cloud cover as an instrument for law enforcement in column 2, I find that the presence of law

[10] A similar analysis on a secondary agricultural outcome, pasture quality, can be found in the Appendix, Section A1.2.

[11] Numerical analogues of the estimated effect in the "within cell" category can be found in Table B3 for OLS and Table 1 for IV.

[12] The estimation specification and validity of the instrument are discussed in Section A1.1.

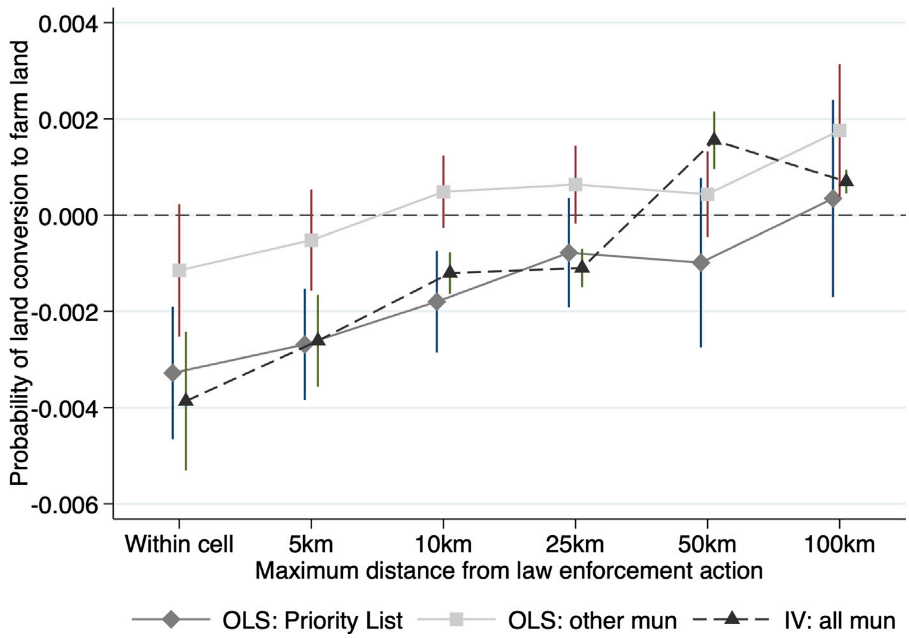

Fig. 3. Law enforcement & probability of forest conversion. *Notes:* This figure shows the estimated effect of law enforcement on the probability of conversion to farm land for different radii around the geolocated law enforcement. The solid lines depict the results of an OLS estimation following Eq. (1), for *Priority List* municipalities (dark grey) and other municipalities (lightgray). The dashed line comes from the instrumental variable approach using Eq. (5). The vertical bars indicate the 95 percent confidence interval, with clustered standard errors at the municipality level.

enforcement in a certain plot leads to a 0.4 percentage point lower probability of conversion, or about 30 percent of the sample mean. This is about twice as large as a similarly estimated OLS coefficient, as presented in column 3. These point estimates are graphically depicted in Fig. 3 in the "within cell" category.[13] Also here do we see a gradual weakening of the estimated effect the further away the plots are. There is no significant effect on the probability of conversion to farm land for plots further than 25 km away from the law enforcement occurrence.

5.2. Municipal economic outcomes

This section presents the results of being placed on the *Priority List* on local economic outcomes, following the empirical strategy of Section 4.2. First, I look at the three main economic outcomes of interest that are proxies for local economic development. In the subsequent part, I use alternative sources of data to test a number of mechanisms that could drive these results.

5.2.1. Local economic development

For the two main economic outcomes of interest, I present results in similar fashion by estimating Eqs. (2) and (3). Eq. (2) allows me to estimate the impact of being on the *Priority List*, in effect a constant state of treatment. Using Eq. (3), and the accompanying event study figures, I estimate the effect of being added to the *Priority List*. For each table, the first two columns provide results for the full sample, without and with control variables, respectively. The third column removes non-treated neighbouring municipalities from the control group, to account for potential spatial spillovers. The last column presents the results for a matched sample using baseline municipality characteristics (see Section 4.2). Every specification includes municipality and year fixed effects. For the figures, the treatment of being added to the *Priority List* happens between time periods −1 and 0. This allows for an analysis of dynamic impacts in the years after treatment started.

[13] Note that an interaction term such as in the OLS estimation method does not work given the endogeneity of *Priority List* in the second stage.

An analysis of the impact on the first measure of economic activity, GDP per capita, indicates that municipalities on the *Priority List* have about 6 percent higher GDP per capita. Table 2 shows that this impact is stable for different specifications. Column 1 and 2 display estimated effect for the whole sample, with and without control variables, respectively. Column 3 drops the untreated neighbours, which has no effect on the estimated coefficient. The fourth column, with a matched sample of control municipalities, is not statistically different from the other columns. A careful conclusion that being on the *Priority List* has positive effects on local economic development.

The dynamic specification of Eq. (3), where I estimate the impact of being placed on the *Priority List* can be found in Fig. 4. The first conclusion is that treated municipalities were not systematically different than control municipalities prior to the policy change (years −3 through −1). In the immediate aftermath of being placed on the *Priority List*, no differences can be found between treated and non-treated municipalities. There is some weak evidence that it takes a few years before GDP per capita is higher in the *Priority List* municipalities.

The poverty rates of the Brazilian Amazon region are high, with about 40 percent of the households receiving at least some income support through a large nation-wide conditional cash transfer program. I find that this crude measure of poverty is lower in *Priority List* municipalities by about 2 percentage points, see Table 3. This translates into a reduction in household poverty of a little less than 5 percent, indicating that the increased economic activity at least partially reaches the poor households too. This finding is robust for different specifications. In the dynamic specification of Eq. (3), we see that it also takes several years before this positive impact sets in. Fig. 5 shows that the strongest effects can be found 3 or more years after being first placed on the *Priority List*.

5.2.2. Mechanisms

In this subsection, I discuss potential drivers of the local economic development results presented above. The findings all point towards a preliminary conclusion, where an increase in enforcement and monitoring of anti-deforestation policies in certain high-risk areas leads to positive spillovers in terms of economic development. While these results may not seem obvious at first sight, the mechanisms presented below may provide suggestive evidence of underlying reasons driving

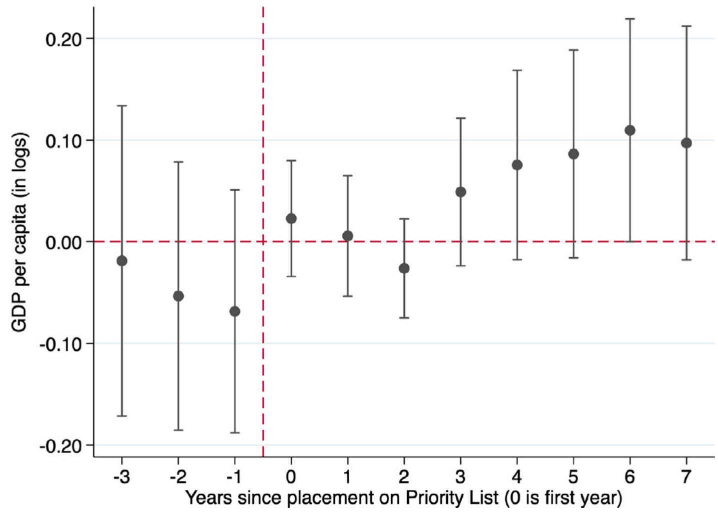

Fig. 4. Event study: *Priority List* & GDP per capita. *Notes:* This figure shows the results from Eq. (3), estimating the effect of being a municipality on the *Priority List* on the GDP per capita (in logs) using the estimation approach by Borusyak (2022). The municipality is treated between years −1 and 0. The vertical bars indicate the 95 percent confidence interval.

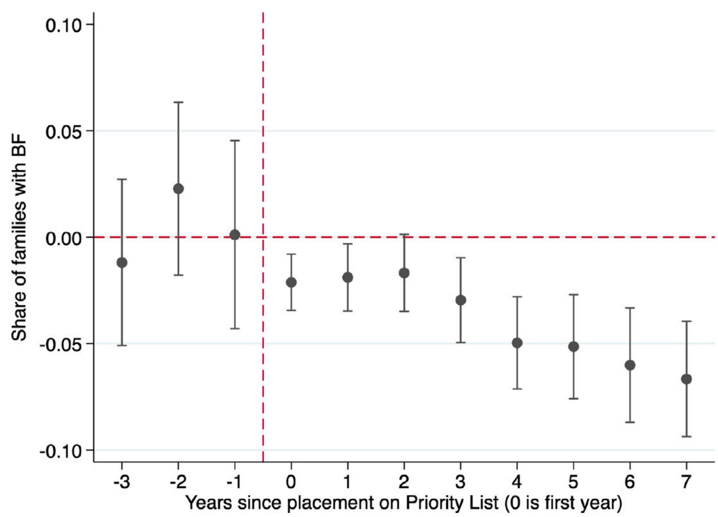

Fig. 5. Event study: *Priority List* & share w/ income support. *Notes:* This figure shows the results from Eq. (3), estimating the effect of being a municipality on the *Priority List* on the % of with income support using the estimation approach by Borusyak (2022). The municipality is treated between years −1 and 0. The vertical bars indicate the 95 percent confidence interval.

these results. However, one should be careful not to over-interpret any one of these results, and these channels warrant more analysis and discussion.

I first look at shifts in the local economy expressed as sectoral shares in municipal GDP. Further, I test whether migration patterns, international trade or infrastructure investments explain part of these results. Lastly, I use agricultural census data to test whether there are changes 'at the farm' that could provide insights into the mechanisms.

Sectoral changes. Changes in agricultural practices may lead to structural transformation of the local economy (Bustos et al., 2016). As I have shown above, there is some evidence that the increase in monitoring and enforcement of existing anti-deforestation legislation made farmers invest in pasture quality. This may spur agricultural productivity, free some labourers from agricultural work to other sectors and in turn boost the local economy. Table 4 provides evidence that this structural transformation has not yet happened. There is some weak evidence that agriculture became more important in municipalities on the *Priority List*, leaving the other sectors unaffected. One potential explanation for this is that these structural changes to the economy take more time to materialize, and can therefore not be captured in the time period I have data for.

Migration and trade. Two alternative mechanisms are migration flows and federal infrastructure projects. As annual migration data for each municipality is not available, I proxy this by data on population growth. Differences in population growth could indicate migration, but this is not the only feasible channel that explains differences in population growth. Column 1 in Table 5 shows that there is a small negative effect of being a *Priority List* municipality on population growth. The population still grew in these treated municipalities, but less so than in others. This could point towards more outward migration of a subset of the population in these places, but the effect is not very precisely estimated. Looking at international trade, I find no evidence that municipal exports are different in *Priority List* municipalities (Column 2). However, the share of agricultural exports in total exports tends to be higher in *Priority List* municipalities, by around 10 percentage points. This is a substantial increase from the control mean of 15 percent, so agricultural products play a larger role in municipalities on the *Priority List*.

Table 2
Panel results: *Priority List* & GDP per capita.

Dependent variable:	GDP per capita (in logs)			
	No controls (1)	Full model (2)	No neighbours (3)	Matched sample (4)
Priority list	0.059* (0.031)	0.059* (0.031)	0.057* (0.031)	0.091** (0.041)
Fixed effects	Municipality & Year			
Controls	None	Temp, rain, price vector		None
Observations	6540	6432	4992	2004
Dep. var. mean	8.888	8.902	8.839	9.184

Notes: This table shows the estimated effect of being a municipality on the *Priority List* on GDP per capita (in logs). Each column presents the results from separate OLS regressions, as per Eq. (2). The first column only includes municipality and year fixed effects. Column 2 adds controls, which are the (lagged) average temperature, total precipitation, and the agricultural price vector. Column 3 drops non-treated neighbouring municipalities from the control group, and Column 4 uses the coarsened exact matching algorithm of Blackwell et al. (2009) to match similar treated and untreated municipalities using baseline (2004) characteristics. Standard errors in parenthesis are clustered at the municipality level. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.10$.

Table 3
Panel results: *Priority List* & share w/ income support.

Dependent variable:	Share of population with income support			
	No controls (1)	Full model (2)	No neighbours (3)	Matched sample (4)
Priority list	−0.022* (0.012)	−0.021* (0.011)	−0.025** (0.012)	−0.026** (0.013)
Fixed effects	Municipality & Year			
Controls	None	Temp, rain, price vector		None
Observations	6436	6328	4898	1993
Dep. var. mean	0.426	0.422	0.442	0.318

Notes: This table shows the estimated effect of being a municipality on the *Priority List* on the share of Bolsa Familia receiving families in the municipality. Each column presents the results from separate OLS regressions, as per Eq. (2). The first column only includes municipality and year fixed effects. Column 2 adds controls, which are the (lagged) average temperature, total precipitation, and the agricultural price vector. Column 3 drops non-treated neighbouring municipalities from the control group, and Column 4 uses the coarsened exact matching algorithm of Blackwell et al. (2009) to match similar treated and untreated municipalities using baseline (2004) characteristics. Standard errors in parenthesis are clustered at the municipality level. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.10$.

Table 4
Mechanism results: *Priority List* & sectoral GDP shares.

Dependent variable:	Agriculture (1)	Industry (2)	Services (3)	Government (4)
Priority list	0.018* (0.010)	−0.008 (0.008)	−0.004 (0.005)	−0.006 (0.006)
Fixed effects	Municipality & Year			
Observations	6432	6432	6432	6432
Dep. var. mean	0.246	0.090	0.222	0.389

Notes: This table shows the estimated effect of being a municipality on the *Priority List* on sectoral shares in municipal GDP. Each column presents the results from separate OLS regressions, as per Eq. (2). Column 1 uses the share of agriculture in municipal GDP, Column 2 the share of industry, Column 3 the share of services, and Column 4 the government expenditures in GDP. Standard errors in parenthesis are clustered at the municipality level. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.10$.

Infrastructure. As for infrastructure projects, it is a known finding that these projects in rural areas spur economic development (Banerjee, Duflo, & Qian, 2020; Donaldson & Hornbeck, 2016) and agricultural land values (Donaldson, 2018). Using data on federal roads in the Amazon Region for the years 2005 (pre-treatment) and 2014 (post-treatment), I test whether these projects were allocated differently in *Priority List* municipalities than in other municipalities. Columns 1 and 2 of Table 6 show that federal roads are not more likely to exist, nor are they longer in *Priority List* municipalities. Similarly, I find no evidence that *Priority List* municipalities are more likely to have distribution system of water supply (column 3). This indicates that I can plausibly rule out infrastructure projects decided at a higher political level to be the driving force behind the results in Section 5.2.

Table 5
Mechanism results: *Priority List* & migration and trade.

Dependent variable:	Pop growth (1)	Exports (in logs) (2)	% Agri in total exports (3)
Priority list	−0.009* (0.005)	0.027 (0.680)	0.104** (0.047)
Fixed effects	Municipality & Year		
Observations	6302	6540	6540
Dep. var. mean	0.015	4.660	0.147

Notes: This table shows the estimated effect of being a *Priority List* municipality on migration and international trade. Each column presents the results from separate OLS regressions, as per Eq. (2). The dependent variable in Column 1 is population growth, as a proxy for migration. Column 2 uses total municipal exports as dependent variable (in logs), and the dependent variable in Column 3 is the share of agricultural exports in total exports. Standard errors in parenthesis are clustered at the municipality level. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.10$.

Table 6
Mechanism results: *Priority List* & infrastructure.

Dependent variable:	Federal road (1)	Fed. road length (2)	Water supply (3)
Priority list	−0.001 (0.069)	−3.305 (2.195)	−0.044 (0.079)
Fixed effects	Municipality & Year		
Observations	1092	1092	1090
Dep. var. mean	0.758	25.446	0.750

Notes: This table shows the estimated effect of being a *Priority List* municipality on infrastructure. Each column presents the results from separate OLS regressions, as per Eq. (2). The dependent variable in Column (1) is a dummy whether a federal road passes through the municipality, and Column (2) is a measure of federal road length (in metres) per square kilometre. Column 3 is a dummy variable whether there is a water supply system through a general distribution network in the municipality. Standard errors in parenthesis are clustered at the municipality level. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.10$.

Agricultural census. The results in Section 5.1, as well as existing literature (Koch et al., 2019; Moffette, Skidmore et al., 2021), point towards agricultural intensification in municipalities on the *Priority List*. In addition to the findings of these two papers that only find effects in the cattle industry, I test a number of 'on-the-farm' outcomes using Agricultural census data from 2006 (pre-treatment) and 2017 (post-treatment). More specifically, I test whether farm characteristics in municipalities on the *Priority List* developed significantly different than in other municipalities. As I only have two data points for each municipality, this is a simplified version of Eq. (2). Table 7 provides insights into this question. In the first column, I find that a significantly larger share of the farmers use fertilizer in *Priority List* municipalities, where fertilizer is used by around a third more. I find that around 18 percentage points more farms are connected to the electricity grid in municipalities on the *Priority List*, which is around a third of the dependent variable mean. I also find that the per farm average revenue is about 55 to 60 percent higher in *Priority List* municipalities (Column 3). Lastly, I find that there are fewer small farms (with a total area of less than 5 hectares) in *Priority List* municipalities. If we assume economies of scale, fewer small farms can be an explanation of agricultural intensification in the aggregate and indeed be a partial explanation of the finding in Column 3.

6. Conclusion

This paper studies the economic consequences of an anti-deforestation policy in Brazil. This policy raised the implicit costs

Table 7
Mechanism results: *Priority List* & farm level outcomes.

Dep. Var.:	Fert. (1)	Elec. (2)	Farm $ (3)	Small (%) (4)	Medium (%) (5)	Large (%) (6)
Priority list	0.049***	0.182***	0.572***	−0.042***	−0.010	0.005
	(0.017)	(0.028)	(0.109)	(0.010)	(0.018)	(0.019)
Fixed effects	Municipality & Year					
Observations	1090	1090	1090	1090	1090	1090
Dep. var. mean	0.138	0.605	3.461	0.243	0.505	0.201

Notes: This table shows the estimated effect of being a municipality on the *Priority List* on farm level outcomes. Each column presents the results from separate OLS regressions, as per Eq. (2). The dependent variable in Column 1 is a dummy for fertilizer use. Column 2 looks at whether a farm is connected to the electricity grid. The dependent variable in Column 3 is the average total revenue *per farm*. Columns 4 to 6 are dummies for the share of farms within each range (small farms are less than 5 ha., large ones over 100 ha.). Standard errors in parenthesis are clustered at the municipality level. *** denotes $p < 0.01$, ** denotes $p < 0.05$, and * denotes $p < 0.10$.

of deforestation, and moved actors away from deforestation towards intensification of existing pasture land. Novel satellite capabilities allowed for a more real-time identification of ongoing deforestation activity, leading to quicker and more targeted law enforcement actions. My results indicate that this policy successfully reduced deforestation around the Amazon region.

Taking this one step further, I then study how this affected the wider economy. High-risk deforestation municipalities were singled out by the Ministry of the Environment and placed on a *Priority List*, to which law enforcement agencies were allowed to allocated a disproportional share of their resources. My findings that farmers moved away from investing in clearing additional land to improving existing land fits well with the municipality-level results of Koch et al. (2019) and Moffette, Skidmore et al. (2021) who find that cattle productivity increased in these *Priority List* municipalities. I then show that this policy indeed had positive spillovers onto the municipal economy by increasing economic activity and lowering poverty rates. One plausible mechanism I find is that agricultural intensification on the farm (e.g. fertilizer use and electricity connectivity) played a role.

My analysis has a few limitations. First, regarding the conversion to farm land metric, I cannot distinguish between conversion of primary or secondary forest to farm land. While forest clearing in and by itself has negative effects on the capacity to store carbon, these are likely larger in the case of the former. Moreover, the lack of detailed annual data on agricultural practices, the mechanisms behind the seemingly positive relationship between environmental protection and local economic development cannot be specified further.

This paper shows that a policy change that intensified monitoring and enforcement of environmental legislation in a remote region of Brazil pushed farmers away from forest clearing. This has positive effects on the wider economy, and may serve as blueprint for other countries struggling with deforestation. Future research could employ qualitative methods to uncover the precise decision-making process for farmers given the increase in relative costs of deforestation. A more detailed analysis of agricultural and economic data that is not available in the public domain could also shed light on mechanisms of the results found above.

CRediT authorship contribution statement

Erik Merkus: Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Software, Validation, Visualization, Writing – original draft, Writing – review & editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Acknowledgements

Thanks to Martina Björkman Nyqvist, Abhijeet Singh, Pamela Campa, Oliver Engist, Felix Schafmeister, and Mara Balasa for useful feedback, as well as seminar participants at the Stockholm School of Economics and Misum for additional comments. Financial support by the Jan Wallander and Tom Hedelius foundation is gratefully acknowledged.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.worlddev.2024.106646.

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