The Economic and Distributional Impacts of Environmental Policies: Winners and Losers in Brazil's Priority Municipalities

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Abstract

This paper explores the economic consequences of deforestation policies on households, with a focus on distributional outcomes. In the context of the Priority Municipalities policy in Brazil, it asks whether increased regulation and enforcement, and the resulting reduction in deforestation, affected employment and income in households situated in targeted areas. The identification strategy exploits the assignment mechanism for priority status to first estimate the average economic effects of the policy using an differences-in-differences specification. It then uses a "changes-in-changes" analysis from Athey and Imbens (2006) to examine effects at different parts of the distribution. In line with previous work on this topic, which considers effects at the municipality level, the study finds no evidence of economic impacts on average for municipalities in the sample. However, the changes-in-changes analysis suggests heterogeneous impacts at different parts of the income distribution, especially for those employed in agriculture. In particular, it appears that the lack of impact on average obscures regressive effects for this sector, perhaps due to a substitution from labor-intensive to more capital-intensive agricultural production.

1 Introduction

Deforestation is a key cause of global greenhouse gas emissions, with tropical forests accounting for the majority of this loss. Moreover, tropical forests are predominantly located in the developing world, where poverty remains a pressing concern. As well as having geographic overlap, these two challenges interact directly, though their relationship is 'complex, non-linear and likely bidirectional' (Merkus, 2024). For instance, economic motives drive deforestation, and deforestation itself can have mixed economic impacts, especially on poor, marginalized communities (Barbier & Burgess, 2001; Pfaff et al., 2008; Walker et al., 2020). Consequently, policies targeting poverty reduction or deforestation may have unintended impacts on one another, though the exact nature of these is context dependent and not fully understood (Alix-Garcia et al., 2013; Ferraro & Simorangkir, 2020). Given the magnitude and interconnectedness of these challenges, research exploring trade-offs and synergies between poverty alleviation and deforestation mitigation policies is important.

This paper considers the economic and distributional consequences of policies to reduce deforestation, focusing on the Brazilian Amazon, where trade-offs between deforestation and economic development are particularly pronounced. Indeed, Brazil's vast forest cover and rich biodiversity make it a critical area for global conservation efforts, especially amid ongoing economic pressure for agricultural expansion. Over the past two decades, significant efforts have been made to curb deforestation, resulting in a substantial increase in policies and measures aimed at forest conservation (Merkus, 2024). At the same time, poverty and inequality remain pressing challenges and represent an important policy focus, as evidenced by Brazil's large social protection portfolio. Poverty is disproportionately high in the Amazon region, with households facing challenges relating to smallholder agriculture, tenure insecurity, and limited access to markets or social services (Guedes et al., 2012).

The Priority Municipalities policy (PM), first implemented in 2008, is a key component of Brazil's flagship anti-deforestation Action Plan, the PPCDAm.[1] The policy targets municipalities identified as deforestation hotspots, emphasizing increased monitoring of illegal deforestation and stricter enforcement of related penalties, such as fines and embargoes, by the Brazilian Institute of Environment and Renewable Natural Resources (IBAMA). It is estimated to have led to substantial decreases in deforestation—up to 43% according to the most recent analysis—and to have a high benefit cost ratio (Assunção et al., 2023). These substantial effects, alongside the broad, municipal-level application of the policy, imply significant adjustments made within targeted municipalities, potentially by large numbers of Brazilians. As such, the policy provides an interesting setting in which to explore the effect of a deforestation reduction measure on economic outcomes, and in particular, the ways such effects are distributed across the population.

Understanding the distributional impacts of environmental policies is important for two

[1] The Action Plan for the Prevention and Control of Deforestation in the Legal Amazon

main reasons. First, from an equity perspective, if policymakers are concerned with social outcomes such as poverty and inequality, then understanding how policies affect different groups is important for responsible policy design (Baumol & Oates, 1988). Second, from an effectiveness perspective, distributional consequences can create political economy constraints: if certain groups experience significant harm, they may resist, undermine, or fail to comply with the policy, ultimately jeopardizing its success (Fullerton, 2008). The political economy challenges relating to the distributional consequences of environmental policies are reflected in recent events, such as in the 'Gilet Jaune' protests of 2018 in France (Jetten et al., 2020; Mehleb et al., 2021) and 'Yellow Vest Canada' protests in Alberta (Bergler, 2019). Such resistance can undermine both the implementation and long-term sustainability of environmental policy, with potentially far-reaching consequences for climate and environmental goals.

This paper examines the economic and distributional impacts of the Priority Municipalities policy, exploring how it affected household employment and income a) on average, and b) across the municipal income distribution. The empirical strategy uses recent insights about the policy's selection process to compare treated and untreated municipalities with similar values of the selection criteria. In particular, it exploits the relevance of two eligibility criteria, as well as state-level prioritization in the policy's application, to select a subsample of municipalities near the threshold for inclusion for their state. The analysis proceeds in two parts, using data from the Brazilian census to generate estimates at the household-level.

First, it asks how residing in a Priority Municipality impacts household employment and income on average at the threshold for inclusion, using a differences-in-differences analysis, later supplemented by a fuzzy regression discontinuity design. Then, given the potential importance of distributional effects, it applies the changes-in-changes strategy, introduced by Athey and Imbens (2006), to assess the spread of income effects within municipalities in the sample.

Changes-in-changes is a generalization of differences-in-differences to the entire distribution of potential outcomes. Under certain assumptions, Athey and Imbens (2006) show how three observable distribution functions: those at baseline for the treatment and control groups, and for the control group at endline; can be used to estimate the counterfactual distribution of the outcome of interest for the treatment group at endline, had the group not received treatment. The method allows for more intuitive counterfactuals than quantile regression methods, comparing a quantile of interest in treatment to whichever part of the control distribution had similar values at baseline.

Changes-in-changes is useful in this setting for multiple reasons. First, as implied, it allows for the estimation of impacts at different parts of the income distribution, which is a key purpose of this paper. Second, changes-in-changes' focus on distributions rather than specific observations makes it appropriate for analysis using repeated cross-sectional data, rather than requiring a panel. This makes it a useful instrument for studies using census data, which is often sampled in this way. Third, the method does not require average potential outcomes or their evolution to be the same in treatment and control municipalities, unlike the differences-in-differences (DID) approach, which assumes parallel trends. Though I use knowledge of the priority selection process to maximize similarity between these groups, and thus consider parallel trends to be a plausible assumption for my sample, it lends extra credibility to estimates not to have to rely on this.

Results suggest it is not possible to reject the null hypothesis of no average effect of the Priority Municipalities policy on household employment and income at the inclusion thresholds 1-2 years after implementation. This result aligns with existing research considering the short-term economic effects of the policy at the municipality level (Assunção & Rocha, 2019; Merkus, 2024). However, the changes-in-changes analysis suggests that average effects obscure considerable effect heterogeneity across the income distribution. These results point to heterogeneous impacts, especially on agricultural income, with negative effects felt most strongly towards the bottom of these distributions. Results are robust to accounting for spatial spillovers, though there is suggestive evidence that treatment effects could even be slightly underestimated when spillovers are not accounted for.

This paper contributes to literature on the distributional impacts of environmental policies. Research in this area has most commonly asked whether carbon tax policies affect poor households disproportionately, be this on the 'use side' (due to poor households spending a higher proportion of their income on e.g. fossil-fuel intensive products) or the 'source side' (if taxes lead to disproportionate losses to income for poor households) (Fullerton, 2008; Harberger, 1962). Work in this area paints a fairly mixed picture, in which tax incidence tends to vary with policy details and by local context (Fullerton & Heutel, 2013; Ohlendorf et al., 2021; Pizer & Sexton, 2019; Shang, 2023).

In addition, existing work considers the distributional effects of other types of environmental policies, such as permit systems or command-and-control regulations (Fullerton, 2011; Vona, 2023). These effects are often even more complex to characterize, with additional distributional implications to consider. For instance, moves away from particular technologies or practices, such as in the wake of new command and control regulation, can lead to difficult periods of transition, in which it is not immediately possible redeploy inputs from a polluting activity elsewhere. When the poorer and more vulnerable are most exposed to the negative effects of such transitions, policies will have regressive effects (Fullerton, 2008).

The types of distributional consequences described above can be difficult to characterize empirically, due both to data availability and to the complex ways in which outcomes of interest are interconnected. As a result, empirical studies on the economic impacts of land-use policies—such as those aimed at reducing deforestation— often report average effects or focus on specific subpopulations, such as individuals living in poverty. While the literature generally finds that policies like protected areas (PAs) and payments for ecosystem services (PES) do not increase poverty on average (Alix-Garcia et al., 2015; Andam et al., 2010; Arriagada et al., 2015; Canavire-Bacarreza & Hanauer, 2013; Clements et al., 2014; Gurney et al., 2014; Naughton-Treves et al., 2011; Robalino et al., 2014;

Sims, 2010), it also emphasizes that the relationship between deforestation reduction and poverty at the local level is not straightforward. In particular, areas with high deforestation reduction potential do not necessarily overlap with those that offer the highest poverty alleviation potential (Alix-Garcia et al., 2015; Andam et al., 2010; Bulte et al., 2008; Ferraro et al., 2011; Jack et al., 2008). Moreover, environmental policies may have worse poverty impacts in areas where deforestation-reduction potential is highest (Villalobos et al., 2023), and impacts can vary significantly within affected areas depending on household characteristics (Baird & Leslie, 2013; Bandyopadhyay & Tembo, 2010; Hu et al., 2023; Richardson et al., 2012; Tumusiime & Sjaastad, 2014).

When it comes to the policy of study in this paper—the Brazilian Priority Municipalities policy—existing work has focused on average economic impacts at the level of the municipality. Studies find limited effects on municipal economic outcomes in the short term, but some positive impacts in the longer term (Assunção & Rocha, 2019; Merkus, 2024). In particular, it seems Priority Municipality status may have induced agricultural intensification within municipalities, both in the crop and livestock sectors, due to the increased cost of extensification (Koch et al., 2019; Merkus, 2024; Moffette et al., 2021). Such impacts would likely have distributional consequences, though these are not explored in existing work.

Against this backdrop, this work offers the following contributions. Firstly, it adds to work on the economic effects of policies to reduce deforestation by offering an explicit consideration of distributional effects across the income distribution, rather than inferring this from average effects or focusing on specific subgroups. Such an analysis provides a more comprehensive picture of how environmental policies impact different segments of the population. As such, it is important for identifying potential trade-offs and synergies between environmental goals and economic outcomes, with implications for policy effectiveness and for social equity, as described above.

Secondly, the changes-in-changes methodology employed for the distributional analysis has not much been used in this literature previously (with the exception of Assuncao et al (2023), who used it at the municipality level). The method allows for the use of census data, despite its repeated cross-sectional nature, and as such, enables analysis at a larger scale than has been possible in studies using smaller household surveys or municipality-level outcomes. Furthermore, a household-level analysis can offer greater accuracy than those reliant on municipal data, since outcomes like municipal GDP may miss income from some sectors, such as those from the informal economy. Census data also allows for a breakdown of total household income by source, which is useful for pinpointing any changes more specifically, and for considering effect mechanisms.

The study proceeds with background on the Priority Municipalities policy in Section 2. Section 3 describes the empirical strategy employed in the paper, Section 4 the data used and Section 5 the results. Section 6 discusses the robustness of the results to accounting for spatial spillovers, and Section 7 provides a discussion and some limitations of the study. Section 8 concludes.

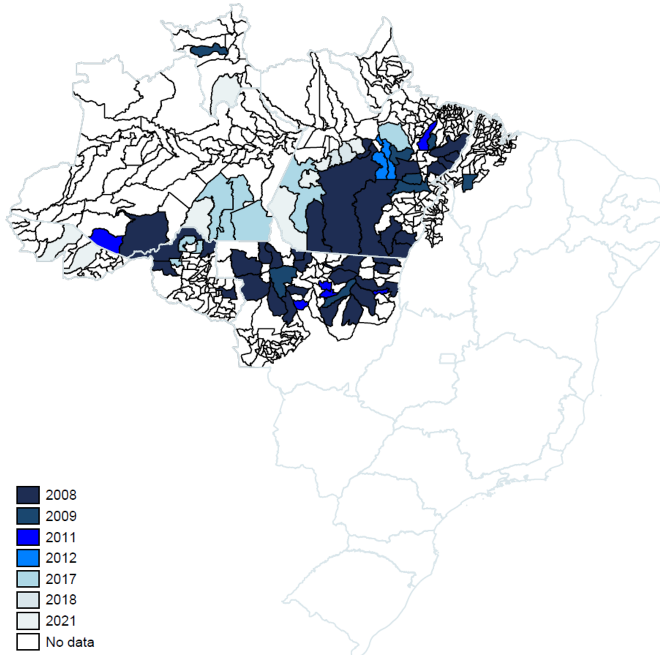
2 Background: The Priority Municipalities

Concerted efforts to tackle high levels of deforestation in the Brazilian Amazon were made from the early 2000s, with the Plan for the Prevention and Control of Illegal Deforestation (PPCDAm) first introduced in 2004. This included various measures, such as improved satellite monitoring of deforestation activity, the expansion of protected areas, and increased cooperation between different government entities involved in tackling deforestation (Merkus, 2024).

The Priority Municipalities policy marked the second phase of the PPCDAm from 2008 (signed in December 2007), involving targeting municipalities with high deforestation records for a number of extra measures. These included increased resources for monitoring and enforcement, assigned by IBAMA, the Brazilian Institute for the Environment and Renewable Resources, as well as more stringent licensing and geo-referencing rules for rural properties (Decree 6321, 2007). As a result, IBAMA was able to greatly increase its field presence within Priority Municipalities to facilitate access to these areas. Deforestation was monitored using a satellite alert system named DETER, and local IBAMA officers monitoring hotspots this way could quickly establish presence in areas with unusual activity. Consequently, penalties for deforestation such as fines and embargoes greatly increased in Priority Municipalities during this time (Assunção & Rocha, 2019).

The map in Figure 1 displays the geographic evolution of the Priority List over time. 34 municipalities were assigned priority status in 2008 (7% of the municipalities in the Amazon biome) and 8 in 2009, with more being added to the list between the years of 2011 and 2021 for a total of 70 over this period.

Figure 1: Year Assigned Priority Status



Notes: Figure 1 displays the Priority Municipalities that have been selected since the policy's introduction in 2008. Those selected in 2008 and 2009 will be the focus of this study.

3 Empirical Strategy

3.1 Priority Municipality Selection

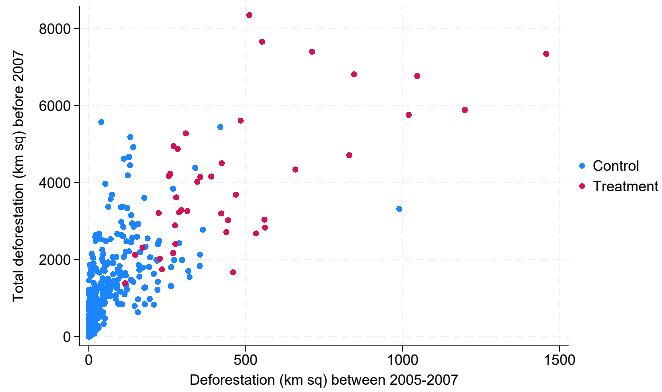
The original decree announcing the Priority Municipalities policy stated that allocation rules were to be based on a) total historic municipal deforested area, b) total deforested area in the three previous years, and c) whether or not a municipality had experienced an increase in deforestation in three of the five previous years (Decree 6321, 2007). However, subsequent empirical work has determined that municipality selection depended largely on the first two of these three rules, with recent increases in deforestation playing almost no part (Assunção et al., 2023).

Figure 2 demonstrates the link between treatment status and the values of the two eligibility criteria for the municipalities added to the priority list in 2008 and 2009. Assunção et al (2023) find that the two criteria assign priority status in 2008 with 98% accuracy, using a threshold of 2137km^2 total municipal deforestation and 222km^2 deforestation between 2005 and 2007.[2] Their thresholds predict priority status in 2008 and 2009 with 97% accuracy.

The analysis in this paper leverages the importance of the two eligibility criteria described

[2]Assunção et al's paper considers only the allocation of the 2008 municipalities. Priority Municipalities are identified with 94% accuracy, and those not assigned priority status with 98% accuracy.

Figure 2: Eligibility Criteria by Priority Status



Notes: Figure 2 displays values of the two key eligibility criteria for municipalities in the Amazon biome assigned priority status before 2010 (treatment) and those not assigned this status (control).

above to characterize municipality selection and choose a sub-sample of comparable municipalities for estimation. It further accounts for possible state-level differences in the application of the eligibility criteria, given the varying deforestation pressures and histories across different Amazonian states (dos Santos Massoca & Brondízio, 2022). State-level allocation is supported by the data, since there are several examples of municipalities that would not have qualified for priority status in a simple application of the two criteria to the entire Amazon biome, but whose selection can be explained by differing priority levels by state. A graphical depiction of the relevance of state of origin for selection can be found in Figures A1-A3 in Appendix A, with the graphs demonstrating reduced overlap in values of the eligibility criteria when the treatment and control groups are compared at the state level.

Building on the single threshold established in Assunção et al (2023), state-level thresholds for inclusion in the Priority Municipalities policy are defined for this analysis. Each (de facto) threshold is identified using the values of the two eligibility criteria for the treated municipality with the lowest deforestation record in a particular state. Allowing thresholds for inclusion to vary by state reduces incorrect classification into the treatment and control groups by 27% compared to the method employed by Assunção et al (2023), from around 3% to 2%.[3] However, the analysis that follows is robust to both threshold definitions.

This paper leverages the variation in treatment status explained by the eligibility criteria both for sample selection and in its analysis. Firstly, to select a sub-sample of municipalities that are comparable except for in their exposure to the Priority Municipalities policy, the sample is restricted at various bandwidths around the state-level thresholds. This approach involves a trade-off: narrower bandwidths enhance internal validity—by comparing municipalities that are more similar on observable characteristics—but reduce statistical

[3]The Assunção et al (2023) method correctly classifies 86% of Priority Municipalities selected in 2008 and 2009 and 98% of those not selected for priority status. The state-level characterization correctly classifies all of the Priority Municipalities (by design) along with 98% of non-priority municipalities.

power due to smaller sample sizes. Robustness checks using both narrower and wider bandwidths, presented in the appendix, confirm the consistency of the main findings, which are estimated using an intermediate bandwidth selected to balance power and internal validity as much as possible.

Secondly, the threshold variable is used as a proxy for treatment status, estimating the impact of falling above the threshold for inclusion within the selected sub-sample. In my first specification, average intent-to-treat effects are estimated using a differences-in-differences design. In the second, distributional impacts of the Priority Municipalities policy are explored using a changes-in-changes framework (Athey & Imbens, 2006).[4]

I restrict the analysis to the states of Pará, Rondônia and Mato Grosso, since these states contained 93% of the municipalities selected for priority status in 2008 and 2009, and faced comparable deforestation pressures as part of the 'arc of deforestation'. Pressures in these areas were primarily driven by the expansion of agricultural and livestock production, spurred by rising global commodity demand and regional infrastructure projects (dos Santos Massoca & Brondízio, 2022; Vera-Diaz et al., 2009). In contrast, the more remote states of Amazonas, Roraima, and Maranhão each had only one municipality selected for priority status,[5] and experienced slightly different pressures, such as from land speculation and smaller-scale agriculture.[6]

3.2 Differences-in-Differences Analysis

Equation 1 summarizes the main strategy for estimating the (local) average economic effects of the Priority Municipalities policy—a differences-in-differences regression in which the threshold dummy is used as a proxy for treatment. Y_{hmst} is a given outcome of interest for household h in municipality m and state s at time t. $Threshold_{ms}$ is a dummy for whether or not a municipality exceeds the de facto threshold for inclusion in the PM policy for its state (equal to the deforestation record of the treatment municipality with the lowest record in that state), and $g(distance from threshold)_{ms}$ is the standardized distance to the threshold of the eligibility criteria with the lower standardized value (Cattaneo et al., 2024). X_{mst} are state-time interactions and ε_{hmst} is a zero-mean disturbance.

The estimation makes use of two periods' worth of data from the Brazilian census (2000 and 2010), and the coefficient (β_3) on the interaction between the $Threshold_{ms}$ dummy and the dummy for the second period (2010_t) is the intent-to-treat coefficient of interest. Though the census data is not a panel, the estimation strategy removes any time-invariant municipality characteristics that could risk biasing the regression. Standard errors are clustered at the municipality level, and sample weights are applied at the household level.

[4] In the main body of the paper, I use the the threshold for inclusion as a proxy for treatment in an intent-to-treat analysis, since this approach demands less power and aligns with the changes-in-changes methodology used in the distributional analysis that follows. A two-stage least squares setup, using the threshold variable to predict treatment in a fuzzy RD design, is presented in the appendix. This second strategy offers easier interpretation, but is more demanding in terms of power. Results from the two are consistent with one another.

[5] These states had no other municipalities close to the threshold that could be used as controls.

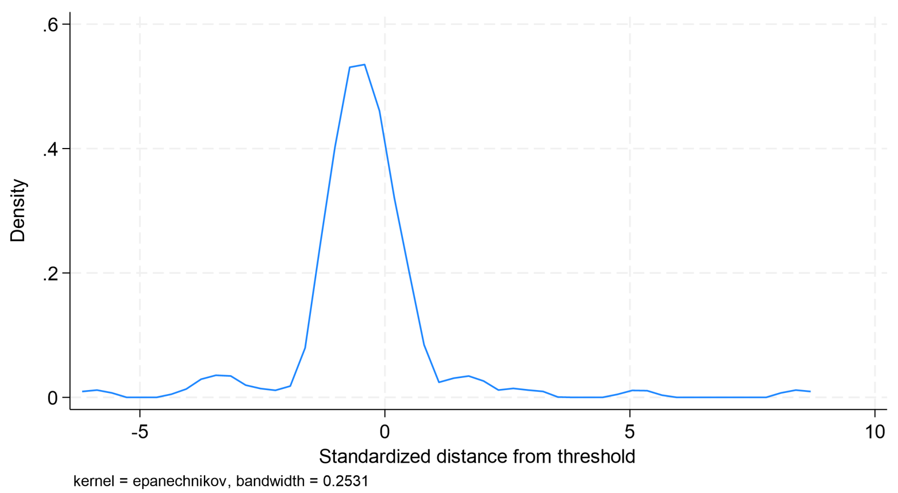
[6] Though Maranhão was closer to the frontier than Amazonas and Roraima at this time, it is excluded from the central analysis due to the lack of a suitable control within the state.

$$Y_{hmst} = \beta_0 + \beta_1 Threshold_{ms} + \beta_2 2010_t + \beta_3 (2010_t * Threshold_{ms}) + \\ g(distance\ from\ threshold)_{mst} + X_{mst} + \varepsilon_{hmst} \tag{1}$$

Bandwidths between 1 and 2 standard deviations (SDs) either side of the threshold are used, to ensure the robustness of results given trade-offs between power and internal validity. (See Figure 3 for the distribution of the standardized distance variable, measuring how far municipalities fall from the de facto threshold for inclusion in their state.) The analysis using the middle bandwidth, 1.5 SD, is reported in the main body of the paper, with the remaining bandwidths shown in the appendix. Note that municipalities that meet neither threshold for priority status are excluded from the analysis, as they are considered likely too different from treatment municipalities to be comparable. To account for potential smooth differences in outcomes around the eligibility cutoff, the analysis includes the distance to the threshold variable graphed in Figure 3 as a control.

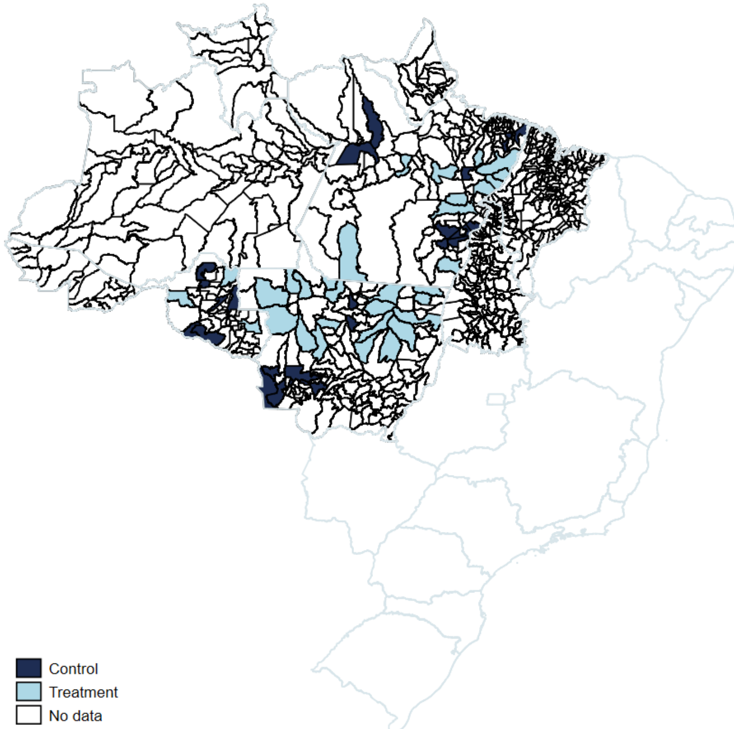
The final sample is displayed in Figure 4. Of the 97 municipalities included, 32 were assigned priority status in 2008 or 2009 (27 in 2008 and 5 in 2009), and 65 were not. Eight of the control municipalities were later assigned priority status (between 2011 and 2018), but the rest have never had this status.

Figure 3: Distribution of Standardized Distance from the Threshold



Notes: Figure 3 displays the density plot for the standardized distance variable. The distance to the threshold of the lower value of the two eligibility criteria is standardized at the state level to create this variable.

Figure 4: Sample Municipalities



Notes: Figure 4 displays the municipalities used for treatment estimation in the main analysis undertaken in this paper. In total, 32 treatment and 65 control municipalities are chosen using a 1.5 standard deviation cutoff around the threshold.

3.2.1 Identifying Assumptions

To interpret the estimated 'local average intent-to-treat' effects of Priority Municipality status as causal estimates, several assumptions must hold. These reflect both the regression discontinuity logic underpinning sample selection, and the difference-in-differences framework used in the estimation. I list the key identifying assumptions below:

- The relevance assumption dictates that the threshold dummy variable must be a strong predictor of treatment status, since this variable is used both as a proxy (main analysis) and an instrument for Priority Municipality treatment status (appendix). The relevance of the threshold variable is demonstrated in Table 1 in the results section.

- The threshold dummy must not have been manipulated in an attempt to alter priority status assignments. In this case, since the criteria relate to historic deforestation records, manipulation is unlikely. Thus, by restricting attention to municipalities near the eligibility threshold and focusing only on variation in treatment status that is explained by these pre-determined criteria, the analysis seeks to mitigate concerns about selection on unobservables.

- Continuity requires that, absent treatment, potential outcomes would have evolved smoothly at the threshold. This is similar to a local parallel trends assumption, specific to a sample of municipalities near the cutoff. Smoothness in municipality and household characteristics at the threshold allows for the interpretation of post-treatment divergence as attributable to treatment, rather than to pre-existing differences or differential trends. To support this assumption, I test for discontinuities in pre-treatment outcomes at the threshold. The results of this analysis at the municipality- and the household-level can be found in the results section, in Table 2 and Table 3, respectively.

- The stable unit treatment value assumption states that there should not be spillovers between treated and control units. In other words, households' outcomes in a particular municipality must not be affected by the treatment status of another municipality. Since there is evidence that the Priority Municipalities policy may have reduced deforestation in neighboring municipalities (Assunção et al., 2023), I test for spatial spillovers in the discussion section of this paper, limiting my control group only to municipalities that do not border a treatment municipality. My results are robust to this specification.

- Finally, I assume a linear relationship between distance to the state-level threshold for inclusion and potential outcomes in the main analysis, though results robust to other functional forms and to the exclusion of this control variable are presented in Appendix D.

3.3 Changes-in-Changes Analysis

Changes-in-changes (CIC) is a non-linear generalization of the more commonly used differences-in-differences framework to the whole distribution of potential outcomes (Athey & Imbens, 2006). It uses the distributions of the treatment and control groups at baseline, and of the control group at endline, to estimate the counterfactual distribution of the outcome of interest for the treatment group at endline (i.e. the distribution if the group not received treatment).[7] The advantage of a changes-in-changes framework in this context is threefold:

- Firstly, CIC allows for the estimation of the impact of the Priority Municipalities policy at different parts of the income distribution—a key purpose of this paper.

- Secondly, the fact that CIC considers shifts in distributions rather than tracking specific households means that it does not rely on assumptions about specific household

[7] While the Changes-in-Changes (CIC) method is well-suited for estimating distributional treatment effects and quantile treatment effects (QTEs), it does not directly identify the average treatment effect (ATE). This is because CIC does not assume rank invariance, which means that the average of QTEs does not generally equal the ATE. Given this, I rely on the DID analysis to estimate average treatment effects under the assumption of parallel trends in means, and CIC to explore heterogeneous impacts of the policy across the income distribution.

error terms for estimating impacts over time. For this reason, estimates do not depend on having panel data, and changes-in-changes is appropriate in the case of a repeated cross section.

- Finally, the method is useful for a case where households within the Priority Municipalities have different potential outcomes or potential outcomes that evolve differently to households in control. I consider this to be unlikely, given my selection of a control group with similar values of the eligibility criteria for policy inclusion, and since my outcome of interest is not the same as the policy's key outcome of interest. However, it lends credibility to estimates not to have to rely on potential outcomes being independent of treatment status over time.

Since CIC is less commonly used than differences-in-differences frameworks, I introduce it briefly below before detailing the application to my research question.

3.3.1 Introduction to Changes-in-Changes

As with quantile regression methods, CIC allows a researcher to estimate the treatment effect for a particular quantile θ of the treatment group. In a two-by-two setup, with a treatment and control group observed in a pre- and a post-treatment period, the true value of this effect is the difference between the observed outcome for this quantile at endline, and the counterfactual outcome for this quantile under a no treatment scenario (not observed). For instance, if $Y(0)_{11}$ and $Y(1)_{11}$ represent the outcome for the treatment group at endline in the no-treatment and treatment scenarios, the true effect at quantile θ (this particular quantile treatment effect on the treated) can be represented by:

$$\Delta_{\theta}^{QTT} = F^{-1}_{Y(1)_{11}}(\theta) - F^{-1}_{Y(0)_{11}}(\theta) \tag{2}$$

Here, $F_{Y(j)_{gt}}$ represents the conditional distribution function (CDF) of the potential outcome $Y(j)_{gt}$ where j is the policy scenario (has a household's municipality been assigned priority status (1) or not (0)?), g is the group (is a household residing in what will be a Priority Municipality (1), be this before or after policy implementation, or not (0)?) and t is time (0 in the pre- and 1 in the post-treatment period). Inverse conditional distribution functions ($F^{-1}_{Y(j)_{gt}}$) are used to locate the relevant values of potential outcomes for the quantile of interest θ (though note this cannot be done in Equation 1 for the unobserved counterfactual).

Athey and Imbens (2006) show that, under certain assumptions, the distribution of the unobserved treatment counterfactual $F_{Y(0)_{11}}$ can be estimated using a combination of observed data: specifically, the CDFs of the outcome for the treatment and control groups in the pre-treatment period and for the control group post-treatment. Their approach constructs this counterfactual using changes over time for a given quantile in the control group to infer the evolution of the corresponding quantile in the treated group in the absence of treatment.

Changes-in-changes is more flexible than quantile-based methods, such as quantile DID, when determining which quantile to use as a counterfactual in the control group. Indeed, approaches like quantile DID generally assume a stable relationship between corresponding quantiles of the treatment and control groups over time, using quantile θ in the control group as the counterfactual for quantile θ in the treatment group. In contrast, CIC does not require this fixed relationship. Instead, it compares quantile θ in treatment with whichever part of the control distribution had similar values at baseline, observing how that part of the distribution evolves over time. This approach is more flexible and intuitive, as it allows for the distributions of potential outcomes to differ in the treatment and control groups while maintaining a focus on baseline comparability.

Equation 3 displays the logic for estimating the counterfactual outcome for quantile θ at endline using the three distributions mentioned.

$$\hat{F}^{-1}_{Y(0)_{11}}(\theta) = F^{-1}_{Y(0)_{10}}(F_{Y(0)_{00}}(F^{-1}_{Y(0)_{01}}(\theta'))) \tag{3}$$

$\hat{F}^{-1}_{Y(0)_{11}}\theta$ is the estimated counterfactual outcome for quantile θ of the treatment group at endline, $F_{Y(0)_{10}}$ is the observed baseline distribution of the outcome variable for the treatment group, and $F_{Y(0)_{00}}$ and $F_{Y(0)_{01}}$ are the CDFs of the control group at baseline and endline, respectively.[8]

The CIC estimator for this quantile is the difference between two quantile treatment effects: the observed change for the quantile of interest in the treatment group and the estimated counterfactual change for the corresponding quantile in the control group (here θ'), calculated as shown above.

$$\hat{\Delta}^{CIC}_{\theta} = \Delta^{QTE}_{\theta,1} - \Delta^{QTE}_{\theta',0} \tag{4}$$

3.3.2 Identifying Assumptions

The key assumptions for changes-in-changes to provide unbiased estimates of the effects of interest are: monotonicity between unobservables and potential outcomes; time invariance of the distribution of unobservables; and common support between the outcome distributions in treatment and control (Athey & Imbens, 2006). Their meaning and appropriateness for this context are discussed in detail in Appendix C of this paper. I summarize the main ways they could matter for identification below:

- Monotonicity requires unobservables to affect potential outcomes consistently, with higher values of these corresponding to higher potential outcomes (potential earnings, here). The assumption is reasonable where unobservables can be characterized as

[8] To calculate the treatment counterfactual at quantile θ, changes-in-changes estimation identifies a counterfactual quantile in the baseline control distribution ($F_{Y(0)_{00}}$), finding the outcome for this quantile at endline ($F^{-1}_{Y(0)_{01}}\theta'$). It uses this evolution in the control distribution between baseline and endline to estimate corresponding changes for the treatment group in the absence of treatment, yielding the counterfactual outcome for quantile θ of the treatment group at endline $\hat{F}^{-1}_{Y(0)_{11}}\theta$

a household characteristic, such as ‘productive capacity’, but precludes multiple unobserved traits with separate or varying effects. I consider monotonicity to be sufficiently flexible for this setting, especially in combination with the ways that changes-in-changes allows policy effects to interact with group status, unobservables, and time. I conceptualize unobservables as representing ‘productive capacity’ in this context.

- Time invariance requires the distribution of unobservables to remain constant over time, conditional on group status and on observable characteristics. It would be threatened in this context by changes to the distribution of household ‘productive capacity’, such as a case where those with low capacity receive training or support finding work in the study period. The time frame of this study coincides with a few such changes in the policy environment, and I discuss the possible implications of some relevant shifts in Appendix C. I argue that contemporaneous changes in policy and the economic context likely did not affect the plausibility of time invariance assumption in this case.
- Finally, the common support assumption requires there to be sufficient overlap in the distributions of treatment and control groups for counterfactuals to be generated in estimation. Given my inclusion of only municipalities with a similar deforestation landscape in my sample, I consider this to be a plausible assumption. I ensure the distributions are visually comparable in Appendix C and also refrain from making estimates right at the extremes of the outcome distributions, to be cautious.

3.3.3 Changes-in-Changes Estimation

Changes-in-changes is estimated non-parametrically to minimize assumptions made about the nature of the relationship between treatment and outcomes. However, a semi-parametric specification is also possible, in which the impact of covariates on an outcome of interest is estimated in a first stage, and then partialled out before changes-in-changes is applied to the residual (Assunção et al., 2023; Athey & Imbens, 2006). I employ this semi-parametric specification in my analysis, though results are robust to non-parametric estimation.

Changes-in-changes analysis is applied to the two available waves of census data (2000 and 2010) as follows. Since the analysis requires a binary treatment variablem, the dummy $Threshold_{ms}$ variable is employed to estimate an intent-to-treat specification. In a first stage, versions of Equation 1 are used to estimate the impacts of the binary treatment variables on the outcomes of interest, controlling for covariates. Following Athey and Imbens (2006), the results of Equations 1 are used to partial out the impact of the covariates on the outcome (see Equation 5), before applying the CIC specification to the model's residuals.

$$\hat{Y}_{hmst} - \hat{\beta}_0 - \hat{X}_{mst} - h(distance\ fr\hat{o}m\ threshold)_{mst} =$$
$$\hat{\beta}_1 Threshold_{ms} + \hat{\beta}_2 2010_t + \hat{\beta}_3(2010_t * Threshold_{ms}) + \hat{\varepsilon}_{hmst} \quad (5)$$

The results of this semi-parametric specification are presented below. Under the assumptions of monotonicity, time invariance and common support (discussed in detail in the Appendix C), the changes-in-changes estimates represent the relative change in the outcome of interest between 2000 and 2010 for municipalities above the threshold for inclusion as compared to those below. It can be estimated for any part of the distribution.

4 Data

The outcome data for this study is taken from the 2000 and 2010 waves of the Brazilian census. The Brazilian census is collected every ten years, asking a few key questions to every household in the country, and supplementing this with a longer survey for a subset of households. The exact fraction of households chosen for the longer survey depends on the population of the municipality in which they reside—it was between 10% and 20% in the year 2000 and between 5% and 50% in 2010. Households chosen for the subsample differ from wave to wave. The questionnaire is representative at the level of the census tract (beyond the level of the municipality).

The key outcome variables used in this analysis include questions asked about income and employment in the subsample survey. Employment outcomes are dummy variables, equal to 1 if a household member is employed, or employed in the relevant activity, and 0 otherwise. Income variables are continuous. For the purpose of the analysis, income variables are coded as missing when they are equal to zero, since their extensive margin is explored through the employment outcomes. This aids interpretation of the changes-in-changes analysis, since distributional outcomes are more difficult to interpret if many zeros ae included in the lower parts of the distribution.[9]

Treatment status data and municipality characteristics at the municipality-year level, used as control variables or for robustness tests, are taken from various publicly-available sources. For instance, data on the treatment status of municipalities can be accessed from the Brazilian Ministério do Meio Ambiente, and data on municipal GDP, agricultural production and population were taken from the Brazilian Institute of Geography and Statistics (IBGE). Deforestation records used as eligibility criteria are taken from the replication package in Assunção et al. (2023). Finally, data on exposure to the Bolsa Família program was provided by Rocha and Meyer (2023), and is not yet publicly available in the format used.

[9] Further, this characterization of income variables seems reasonable given that estimates of changes on the extensive margin cannot reject the null of no effect.

5 Results

5.1 The Threshold Variable: Relevance and Continuity

5.1.1 Relevance of the Threshold Variable

As mentioned above, the main specification presented in this section uses the threshold dummy as a proxy for Priority Municipality treatment status, whilst robustness tests in Appendix B use this variable as an instrument for priority status. Both analyses rely on the threshold variable being a strong predictor of treatment. Table 1 displays the results of regressions of priority status on the threshold dummy used in this paper, as well as on the threshold used in Assunção et al (2023). Falling above these thresholds yields a 84 and 77 percentage point higher probability of treatment status, respectively.

Table 1: Relevance: Predicting Treatment Status using the Eligibility Criteria

	Assunção et al threshold	State-level threshold
Assunção et al threshold	0.77***	
	(0.07)	
State-level threshold		0.84***
		(0.06)
Standardized distance from threshold	0.01	0.02
	(0.02)	(0.01)
Mato Grosso	-0.04	-0.04
	(0.08)	(0.04)
Pará	-0.05	-0.05
	(0.08)	(0.04)
Constant	0.11*	0.04**
	(0.06)	(0.02)
Observations	104	104
R-squared	0.61	0.76
SEs Clustered Muni	Yes	Yes
Cragg-Donald Wald F stat.	137.30	287.29
Kleibergen-Paap Wald rk F stat.	122.40	222.45

Notes: Table 1 presents results of the first stage regression at the municipality level to estimate the link between treatment status and the eligibility criteria identified. Cragg-Donald Wald and Kleibergen-Paap Wald F statistics are displayed at the bottom of the table. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

5.1.2 Continuity at the Threshold

Tables 2 and 3 provide evidence for the lack of discontinuities in other characteristics at the cutoff before treatment, showing these at the municipality and household level, respectively. Given the availability of municipality-level data over multiple years, Table

2 presents an event study analysis of pre-treatment trends, focusing on the interaction between year dummies and the threshold variable in the years preceding the policy. In contrast, since household-level data are only available in the 2000 census, Table 3 presents a cross-sectional regression using this baseline wave. Here, the coefficient on the threshold dummy allows me to assess the comparability of household-level outcomes at baseline.

The outcomes chosen for the continuity analysis mimic the outcome variables of interest. In Table 2, variables are proxies for the study's household-level outcomes of interest at the level of the municipality: municipality GDP, agricultural output as a percentage of this, GDP per capita, and agricultural value added per capita. Access to pre-trends for these variables is helpful, since household-level outcomes in the pre-treatment period are only available in the year 2000. The null hypothesis of parallel trends close to the threshold prior to 2008 cannot be rejected for any of these municipality-level outcomes.

Table 3 displays results for the study's key outcomes of interest taken from census data in the year 2000. The outcomes include: employment status, the number of household members in employment, agricultural employment, the number of household members employed in agriculture, total household income, household labor income, total income for households with some labor income, household agricultural income, and total income for households with some agricultural income. Again, the null of no discontinuity at the threshold cannot be rejected for any of these outcomes.

Table 2: Baseline Continuity – Municipality (Pre-Trend from 2002-2007)

	(1) GDP	(2) GDP per Capita	(3) Ag. GDP %	(4) Ag. Value Added per Capita
2003	68,126.42***	1.37***	2.94***	0.63***
	(16,925.37)	(0.27)	(0.90)	(0.19)
2004	146,114.5***	2.95***	3.39**	1.34***
	(35,620.48)	(0.59)	(1.31)	(0.39)
2005	173,296.5***	3.26***	2.08	1.05***
	(46,511.38)	(0.59)	(1.62)	(0.35)
2006	206,939.2***	3.65***	0.48	0.84*
	(59,007.15)	(0.83)	(1.67)	(0.46)
2007	287,871.7***	4.94***	1.16	1.31**
	(73,849.27)	(0.93)	(1.91)	(0.53)
2008	381,446.6***	7.31***	2.36	2.45***
	(94,151.91)	(1.20)	(2.27)	(0.71)
2009	424,033.2***	8.04***	2.88	2.70***
	(104,745.4)	(1.33)	(2.76)	(0.80)
2010	494,385.9***	9.09***	0.75	2.34**
	(122,759.4)	(1.56)	(2.95)	(0.89)
2002*Above threshold	17,818.91	-1.23	2.16	-0.74
	(94,809.36)	(0.86)	(2.70)	(0.45)
2003*Above threshold	4,641.45	-0.40	-0.30	-0.06
	(79,402.14)	(0.65)	(2.46)	(0.51)
2004*Above threshold	15,051.49	3.76	-0.45	3.32
	(51,889.82)	(2.80)	(2.56)	(2.52)
2005*Above threshold	-5,636.22	0.08	0.69	0.21
	(44,328.36)	(0.55)	(1.68)	(0.34)
2006*Above threshold	5,632.95	-0.85	1.21	-0.60
	(28,519.78)	(0.85)	(0.95)	(0.58)
2008*Above threshold	-8,061.05	1.04	0.25	0.88
	(35,926.5)	(1.43)	(1.13)	(0.96)
2009*Above threshold	-43,403.77	1.20	2.95	1.20
	(41,828)	(1.30)	(2.08)	(0.90)
2010*Above threshold	-58,275.57	-0.75	2.30	-0.08
	(41,621.28)	(0.70)	(2.29)	(0.67)
ATE	-41,182.31	0.29	1.30	0.33
	(64,431.65)	(1.13)	(1.95)	(0.68)
Constant	3.44e+07	407.41	1708.90	150.83
	(2.89e+07)	(355.42)	(769.94)	(221.78)
Observations	873	873	873	873
Controls	Yes	Yes	Yes	Yes
SEs Clustered Muni	Yes	Yes	Yes	Yes

Notes: Table 2 presents results from event study regressions used to test for pre-treatment trends in municipality characteristics around the eligibility threshold. Coefficients correspond to year-specific effects relative to the base year (2007, the year before the policy's introduction). All regressions include state-specific linear time trends and control for standardized distance to the eligibility threshold. Robust standard errors clustered at the municipality level are shown in parentheses. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

Table 3: Baseline Continuity – Household (2000)

	(1) HH member employed	(2) No. HH members employed	(3) HH member works in ag. (all)	(4) No. HH members in ag.	(5) HH income (manual)	(6) HH labor income	(7) HH income (labor HH)	(8) HH ag. income	(9) HH income (ag HH)
Above threshold	0.01	0.01	-0.00	-0.01	29.75	68.90	40.54	76.58	73.23
	(0.01)	(0.03)	(0.04)	(0.05)	(73.85)	(68.97)	(76.54)	(100.69)	(115.60)
Constant	0.86***	1.40***	0.22***	0.28***	1,651.9***	1,719.3***	1,880.8***	1,179.6***	1,567.0***
	(0.01)	(0.03)	(0.03)	(0.05)	(64.69)	(58.05)	(63.96)	(74.07)	(89.90)
Observations	78,356	78,356	78,356	78,356	78,356	66,598	66,598	20,554	20,554
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SE Clustered Muni	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 3 presents results of OLS regressions to test for continuity of household characteristics in the sample before treatment. Data from the 2000 census wave is used for this placebo analysis, and a sample of municipalities includes those 1.5 SDs or closer to their state-level threshold for inclusion. All regressions control for state, and the standardized distance to the eligibility threshold. Robust standard errors clustered at the municipality level are displayed below coefficients in parentheses. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

5.2 Average Effects

The estimates of the local average effects, as detailed in Equation 1, are presented in Table 4. These can be interpreted as local intent-to-treat (ITT) effects—that is, the average impact of residing in a municipality that fell above the threshold for policy inclusion, as compared to other municipalities near the cutoff. Thus, estimates capture the causal effect of treatment eligibility, rather than actual exposure to treatment, and are valid for municipalities close to the eligibility threshold.

In line with Table 3, impacts are estimated for employment and income. Again, employment outcomes include: whether any household members were employed in the reference week, how many household members were employed during this week, whether household members were employed in agriculture in the reference week, and the number of household members employed in agriculture at this time. The outcomes relating to income include total household income, household labor income, total income for households with some labor income, household agricultural income, and total income for households with some agricultural income. Income variables reflect monthly earnings.

It is not possible to reject the null of no effects on household employment or income at the 5% level. Likewise, income components expected to be most responsive, labor and agricultural income, show no evidence of being affected by the Priority Municipalities policy on average.

These limited results are in line with existing work (Merkus, 2024), which finds no effect on average when it comes to GDP per capita at the municipality level in the first years of the Priority Municipality policy.

Table 4: Economic Impacts: Local Average Treatment Effects

	(1) HH member employed	(2) No. HH members employed	(3) HH member works in ag.	(4) No. HH members in ag.	(5) Total HH income	(6) HH labor income	(7) HH income (labor HH)	(8) HH ag. income	(9) HH income (ag. HH)
Interaction (2010 × Above threshold)	0.00	0.00	-0.01	-0.01	32.76	69.63	61.37	31.79	34.19
	(0.02)	(0.04)	(0.02)	(0.03)	(96.70)	(100.90)	(95.22)	(89.10)	(80.71)
Above threshold	0.00	-0.00	0.00	-0.00	-42.63	-48.80	-19.22	20.37	22.04
	(0.01)	(0.03)	(0.04)	(0.05)	(90.43)	(96.19)	(93.03)	(106.91)	(97.12)
Year 2010	-0.01	0.06**	-0.10***	-0.11***	506.1***	582.2***	473.2***	139.5**	122.8**
	(0.01)	(0.03)	(0.03)	(0.04)	(74.99)	(80.93)	(80.20)	(65.56)	(61.53)
Constant	0.84***	1.37***	0.25***	0.31***	1,411.0***	1,630.0***	1,472.2***	916.5***	935.1***
	(0.01)	(0.02)	(0.02)	(0.03)	(73.49)	(77.19)	(75.94)	(59.81)	(57.71)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SE Clustered Muni	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	187,003	187,003	187,003	187,003	187,003	152,616	152,616	40,144	46,512

Notes: Each column presents results of DID regressions for binary (1-4) and continuous (5-8) dependent variables on the interaction of the binary threshold variable with the dummy for the second period (2010). Results are ITT estimates due to imperfect predictive capacity of the eligibility criteria. All regressions control for state, and the standardized distance to the eligibility threshold (all interacted with the time trend). Robust standard errors clustered at the municipality level are displayed below coefficients in parentheses. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

5.3 Distributional Effects

Results of changes-in-changes analysis are presented in Table 5. The table displays the impact at the 10th, 50th and 90th percentiles of residing in a municipality with a deforestation record that places it above the threshold for inclusion in the PM policy. As such, results should again be interpreted as local intent-to-treat effects. The outcomes of interest reflect the main household-level income variables evaluated above: total household income, household labor income, total income for households with some labor income, household agricultural income, and total income for households with some agricultural income.

The changes-in-changes analysis suggests that the local average effects obscure heterogeneity across the distribution. Indeed, it is possible to reject the null hypothesis of constant effects across the distribution for the agricultural income variable, as well as the total household income variable for households with members earning agricultural income. Furthermore, it seems that impacts towards the bottom of the distribution tend to be negative, whilst those at the top are more likely to be positive, especially for those involved in agriculture.

For example, household agricultural income (Column 4) appears to have decreased in municipalities eligible for priority status by around 67 reais per month at the 10th percentile, and 71 reais at the 50th percentile relative to those not eligible. However, it appears to have increased by around 200 reais at the 90th percentile. Furthermore, the estimates suggest that total income for households involved in agriculture decreased for eligible households by 85 reais at the 10th percentile, and increased by 46 and 185 reais 50th percentile and the 90th percentiles, respectively (though this latter coefficient is not statistically significant).

Thus, it seems that the Priority Municipalities policy (or at least being eligible for this policy) had regressive effects on agricultural income, as well as on total household income for agricultural households. Furthermore, differences in level changes reflect very different percentage changes for households with smaller and larger incomes at baseline. For instance, the estimated treatment effect on agricultural income at the 10th percentile corresponds to approximately 35% of baseline agricultural income at that percentile, while the effect at the 90th percentile represents about 10% of the corresponding baseline level.

Table 5: Distributional Effects of Residing in a Municipality above the Threshold for Inclusion (ITT)

	(1) Total HH income	(2) HH labor income	(3) Total HH income if labor	(4) HH agricultural income	(5) Total HH income if agriculture
10th percentile	-29.25*	25.51	61.26***	-67.05***	-84.63***
	(15.76)	(16.49)	(7.55)	(10.99)	(30.11)
50th percentile	36.88***	40.95**	42.36*	-71.06***	45.95**
	(10.31)	(17.05)	(22.46)	(18.95)	(22.91)
90th percentile	73.65	139.1*	239.4***	199.7**	184.5
	(78.07)	(78.55)	(79.04)	(85.52)	(124.1)
P Values: *Kolmogorov-Smirnov (KS) and Cramer-von-Mises-Smirnov (CMS) Stats*					
No effect	***	**	***	***	**
Constant effect				***	**
Effect>0 for all				***	**
Effect<0 for all	***	**	***		*

Notes: Each column presents results of CIC regressions for income variables on the binary threshold variable at the 10th, 50th and 90th percentiles. Results are ITT estimates since this variable explains treatment eligibility but not treatment status perfectly. All regressions control for state dummies, and the standardized distance to the eligibility threshold (all interacted with the time trend). Robust standard errors are clustered at the municipality level are displayed below coefficients in parentheses. P-values in the bottom panel test for any effect, constant effects across the distribution, and effects that are all negative or all positive. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

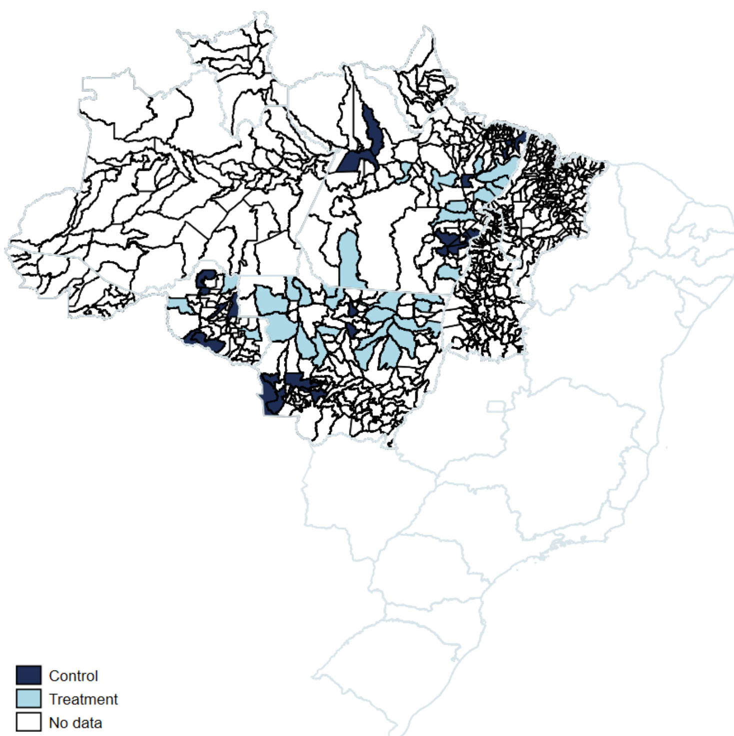
6 Robustness

6.1 Accounting for Possible Spatial Spillovers

In case of spillovers from treatment municipalities to neighboring controls that could bias the results, I rerun the analysis excluding the control municipalities in the sample that neighbor a municipality assigned priority status in 2008 or 2009. Spillovers would matter for estimates of economic impacts if Priority Municipality status diverted economic activity to neighboring municipalities, or if it suppressed activity in surrounding areas due to concern about stricter deforestation monitoring extending to these municipalities (Assunção et al., 2023). In these instances, the stable unit treatment value assumption (SUTVA) would be violated, and treatment effect estimates would be biased.

Tables 6 and 7 present the results of the DID and changes-in-changes estimation when control municipalities that border a Priority Municipality are excluded. After removing neighboring controls, 56 municipalities remain—32 treatment and 24 control (see Figure 5). Though this restricts the power of the analysis somewhat, it is helpful for starting to consider the question of spillovers, and to encourage future work on economic effects of environmental policies to take them into account.

Figure 5: Sample Municipalities



Notes: Figure 5 displays the municipalities used for the spillover analysis. In total, 32 treatment and 24 control municipalities are used for this exercise.

Estimates of treatment effects on this reduced sample correspond with those for the sample used in the main analysis. However, the coefficients on the changes-in-changes estimates in Table 7 suggest effects that are more regressive than initially found above. In particular, these estimates suggest larger gains (losses) at the top (bottom) of the agricultural income distribution, respectively.

Thus, it appears that an analysis that doesn't account for spillovers could underestimate the economic effects of the Priority Municipalities policy if these spread beyond the municipality itself. These estimates corroborate results in Assunção et al (2023) suggesting municipalities bordering those assigned priority status also saw significant reductions in deforestation. However, given the small sample on which these results are estimated, more work should be done to consider the relevance of economic spillovers as a result of deforestation policies.

Table 6: Economic Impacts: Excluding Neighboring Control Municipalities

	(1) HH member employed	(2) No. HH members employed	(3) HH member works in ag.	(4) No. HH members in ag.	(5) Total HH income	(6) HH labor income	(7) HH income (labor HH)	(8) HH ag. income	(9) HH income (ag. HH)
Interaction (2010 × Above threshold)	0.00	-0.00	-0.00	0.00	6.77	27.66	35.33	20.27	15.12
	(0.00)	(0.06)	(0.03)	(0.03)	(137.07)	(135.71)	(142.57)	(83.49)	(106.18)
Above threshold	0.01*	-0.01	0.02	0.02	-53.99	-10.20	-56.50	59.12	60.58
	(0.00)	(0.05)	(0.05)	(0.06)	(125.94)	(129.72)	(131.46)	(98.25)	(124.06)
Year 2010	-0.02***	0.04	-0.08*	-0.08	462.98***	433.60***	539.07***	169.52**	379.60***
	(0.00)	(0.04)	(0.04)	(0.06)	(116.56)	(121.90)	(123.26)	(83.93)	(104.76)
Constant	0.85***	1.38***	0.23***	0.29***	1,409.6***	1,450.2***	1,619.8***	816.8***	1,153.2***
	(0.00)	(0.04)	(0.04)	(0.05)	(111.10)	(115.15)	(115.14)	(83.97)	(102.36)
Observations	120,806	120,806	120,806	120,806	120,806	98,352	98,352	24,310	24,310
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SE Clustered Muni	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column presents results of DID regressions for binary dependent variables on the interaction of the binary threshold variable with the dummy for the second period (2010). Results are ITT estimates due to imperfect predictive capacity of the eligibility criteria. All regressions control for state, and the standardized distance to the eligibility threshold (all interacted with the time trend). Robust standard errors clustered at the municipality level are displayed below coefficients in parentheses. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

Table 7: Distributional Effects Excluding Neighboring Control Municipalities

	(1) Total HH income	(2) HH labor income	(3) Total HH income if labor	(4) HH agricultural income	(5) Total HH income if agriculture
10th percentile	-97.40***	-13.61	20.69**	-143.9***	-217.5***
	(20.65)	(10.79)	(10.27)	(12.86)	(23.39)
50th percentile	-13.43	8.717	-11.52	-81.18***	1.484
	(14.61)	(16.04)	(20.33)	(29.63)	(24.90)
90th percentile	41.83	-27.09	106.9	170.5	247.7*
	(77.30)	(81.42)	(106.7)	(116.7)	(130.9)
P Values: *Kolmogorov-Smirnov (KS) and Cramer-von-Mises-Smirnov (CMS) Stats*					
No effect	***			***	***
Constant effect				***	***
Effect>0 for all	***			***	***
Effect<0 for all			**		*

Notes: Each column presents results of CIC regressions for income variables on the binary threshold variable at the 10th, 50th and 90th percentiles. All regressions control for state, and the standardized distance to the eligibility threshold (all interacted with the time trend). Robust standard errors are clustered at the municipality level are displayed below coefficients in parentheses. P-values in the bottom panel test for any effect, constant effects across the distribution, and effects that are all negative or all positive. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

7 Discussion

As mentioned in the introduction, existing work on the Priority Municipalities policy finds evidence to suggest it induced agricultural intensification within municipalities, both in the crop and livestock sectors, due to the increased cost of extensification (Koch et al., 2019; Merkus, 2024; Moffette et al., 2021). This section explores whether there is evidence that the heterogeneous effects estimated above are driven by the intensification mechanism mentioned in other work. Specifically, it investigates which households and individuals working in the agricultural sector are most likely to be situated at the lower end of the income distribution, and considers possible mechanisms for and implications of the estimated effects given these characteristics.

7.1 Descriptives across the Agricultural Income Distribution

Table 8 displays descriptive statistics for the agricultural sector at the individual and the household level. It compares the characteristics of those in the bottom, middle and top

10% of this sub-sample at baseline.[10]

Individuals in the bottom 10% of the agricultural income distribution are more likely to be younger, less educated, and to be of Black, mixed-race or Indigenous descent. Exclusive work in crop farming is also more common among this group, whilst work in livestock is more common towards the top of the distribution. Though there is not a clear pattern in terms of employment vs self-employment in general, of those employed, individuals at the bottom of the income distribution are less likely to be employed formally. They also work fewer hours per week, as compared to those with higher income. These individual-level results are reflected in the household-level data (also displayed).

Given the repeated cross-sectional structure of the Brazilian census data, it is not feasible to estimate treatment effects on income for small subgroups—such as for individuals with informal employment contracts or those who work in crop farming—because the composition of these groups is likely to change over time. This makes it difficult to distinguish between treatment effects and changes to sub-group membership for other reasons.

Despite this, the information in Table 8 paints a picture of relative vulnerability for those towards the bottom of the agricultural income distribution. Indeed, the age, racial, and employment characteristics of these individuals coincide with well-documented patterns within labor markets in Brazil: with younger workers, those identifying as Black, mixed-race, or Indigenous, and individuals with lower levels of education overrepresented in precarious employment and in unemployment (Firpo & Portella, 2024; Lima & Durán, 2021).

Moreover, the fact that those who experience income losses are more likely to work in crop farming—a relatively labor-intensive activity, to be employed as informal laborers, and to work fewer hours at baseline is consistent with (though not evidence of) a scenario in which the Priority Municipalities policy reduced demand for agricultural labor by prompting a shift away from labor-intensive and toward more capital-intensive forms of agricultural production. If this is the case, complementary policies——such as targeted income support, retraining programs, or investment in rural labor markets——could perhaps mitigate adverse distributional effects and reduce possible political resistance.

Where suitable panel data are available, future research should more directly investigate agricultural intensification as a mechanism for regressive impacts of deforestation restrictions. However, even in the absence of causal evidence on this point, these data reveal ways in which those at the bottom of the agricultural income distribution are disadvantaged across multiple dimensions. This, combined with the observed negative income effects of the Priority Municipalities policy for this group, suggests the policy may have deepened existing socioeconomic vulnerabilities. These findings underscore the importance of identifying populations at risk of adverse outcomes from environmental regulations and considering complementary measures in an attempt to mitigate unintended harms.

[10]These deciles were chosen for comparability with the changes in changes results, but the results are robust to splitting the population differently.

Table 8: Descriptives at Different Deciles of the Agricultural Income Distribution: 2000

	(1) Bottom Decile	(2) Middle Decile	(3) Top Decile	(1)-(2) Bottom -Middle	(1)-(3) Bottom - Top	(2)-(3) Middle - Top	**N**
Individual Level							
Age	36.76	37.67	42.74	-0.92**	-5.98***	-5.07***	9683
	(0.24)	(0.29)	(0.28)				
< Secondary Education	1.00	0.99	0.91	0.00	0.09***	0.09***	9683
	(0.00)	(0.00)	(0.01)				
Black, Mixed-Race or Indigenous	0.74	0.68	0.49	0.06***	0.25***	0.20***	9556
	(0.01)	(0.01)	(0.01)				
Crop Farming Only	0.83	0.68	0.57	0.15***	0.25***	0.11***	9683
	(0.01)	(0.01)	(0.01)				
Livestock Farming Only	0.16	0.30	0.39	-0.14***	-0.23***	-0.09***	9683
	(0.01)	(0.01)	(0.01)				
Employed in Ag.	0.26	0.32	0.10	-0.06***	0.16***	0.22***	9683
	(0.01)	(0.01)	(0.01)				
Formally Employed in Ag.	0.07	0.14	0.36	-0.07***	-0.29***	-0.22***	2288
	(0.01)	(0.01)	(0.03)				
Hours Worked Last Week	43.30	46.75	49.02	-3.45***	-4.73	-1.27***	9683
	(0.21)	(0.26)	(0.31)				
Household Level							
Age (HH Head)	43.33	42.05	46.06	1.28***	-2.73***	-4.01***	6236
	(0.31)	(0.35)	(0.33)				
< Secondary Education (HH Head)	0.99	0.99	0.91	0.01**	0.09***	0.08***	6236
	(0.00)	(0.00)	(0.01)				
Black, Mixed-Race or Indigenous (HH Head)	0.73	0.70	0.49	0.03**	0.25***	0.22***	6159
	(0.01)	(0.01)	(0.01)				
HH Crop Farming Only	0.81	0.58	0.60	0.23***	0.21***	-0.02	6236
	(0.01)	(0.01)	(0.01)				
HH Livestock Farming Only	0.17	0.42	0.41	-0.25***	-0.24***	0.01	6236
	(0.01)	(0.01)	(0.01)				
HH Member Employed in Ag.	0.26	0.49	0.17	-0.24***	0.08***	0.32***	6236
	(0.01)	(0.01)	(0.01)				
HH Member Formally Employed in Ag.	0.08	0.25	0.25	-0.17***	-0.17***	-0.00	1877
	(0.01)	(0.02)	(0.03)				

Notes: Columns 2-4 present sample averages for the bottom 10%, middle 10% and top 10% of the sample working in agriculture. Columns 5-7 present t-tests for the null hypothesis that averages at these deciles are the same. Individual-level results capture all individuals working in agriculture between the ages of 15 and 75 years old, whilst household-level results capture households for whom at least one member works in the agricultural sector. Robust standard deviations are displayed below sample averages in parentheses. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

8 Conclusion

This paper considers the economic and distributional consequences of the Priority Municipalities policy to decrease deforestation in Brazil, first implemented in 2008. Using details of the initial selection mechanism for Priority Municipalities, it estimates local average treatment effects on employment and income, before exploring income effects across the distribution using a changes-in-changes analysis. As such, it offers the first household-level analysis of the policy's effects, as well as being the first analysis to look beyond average impacts to investigate 'winners' and 'losers'.

Results support previous work on the economic impacts of the Priority Municipalities policy at the municipality level, finding limited evidence of effects on employment and income on average. However, the changes-in-changes analysis finds null average effects to be obscuring considerable heterogeneity. Indeed, effects of the Priority Municipalities policy vary across the income distribution, especially in the agricultural sector. It appears that households towards the bottom of the agricultural income distribution suffered negative economic effects, whilst those towards the top benefited.

An initial exploration of potential mechanisms for these heterogeneous effects finds that those with earnings towards the bottom of the income distribution were more likely to belong to more vulnerable groups, and in particular, to groups associated with more precarious forms of employment. Though work tracking the employment outcomes of individuals over time would be needed to causally isolate this mechanism, these characteristics are consistent with a scenario in which the Priority Municipalities policy prompted a shift away from labor-intensive and toward more capital-intensive forms of agricultural production (Koch et al., 2019; Moffette et al., 2021).

The paper has some limitations worth discussing. Firstly, the paper mostly assumes effects to be contained within the Priority Municipalities themselves. Though a preliminary analysis to test for the presence of spillovers yields broadly similar results, some suggestive differences emerge when these are accounted for. A more in-depth characterization of effects in neighboring control municipalities would yield a better understanding of the economic implications of this policy (Assunção et al., 2023).

Furthermore, given the need to identify a suitable control group for Priority Municipalities, this study relies upon a subset of municipalities that lie close to the thresholds for inclusion. While this focus on internal validity is a priority for generating reliable estimates, it likely has some implication for the external validity of the estimated effects. In future, a consideration of how economic effects might vary across, as well as within, municipalities could be an important supplement to this work (Ferraro et al., 2011). A better understanding of the ways in which economic consequences of deforestation policies vary across space could aid the understanding of effect mechanisms, and help policymakers design policy targeting or complementary support.

This work sheds light on the economic effects of a significant environmental policy. It asks an important question, since policies with negative or uneven economic impacts can exacerbate existing inequalities, have reduced environmental benefits, and may even generate broader economic or political impacts (Grabs et al., 2021). In this case, it seems that the policy in question may have increased poverty and instability in a vulnerable part of the population, at least in the short term. Further work to consider any long-term economic consequences once the most recent census is available would help to increase understanding of the unintended impacts of this policy. More broadly, research to measure the distributional consequences of other environmental policies would help researchers and policymakers to consider how vulnerable households can be supported during transitions such as this one.

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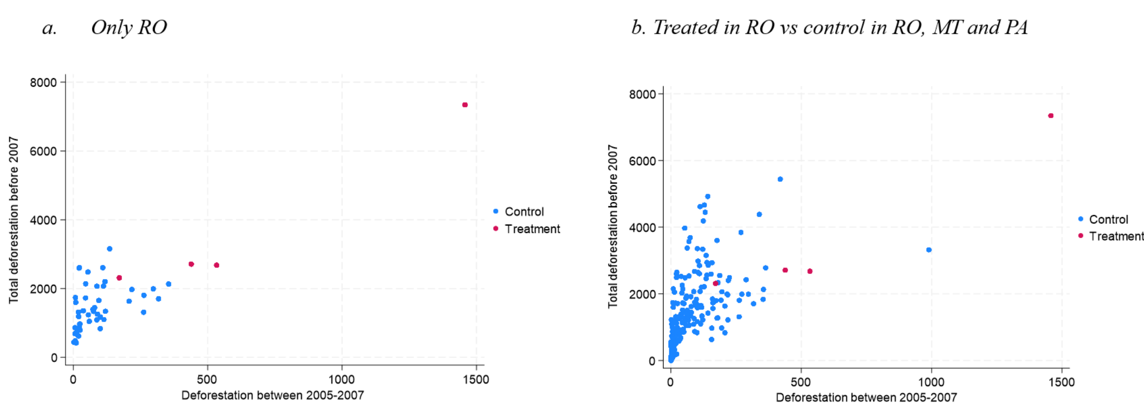
A State-Level Prioritization

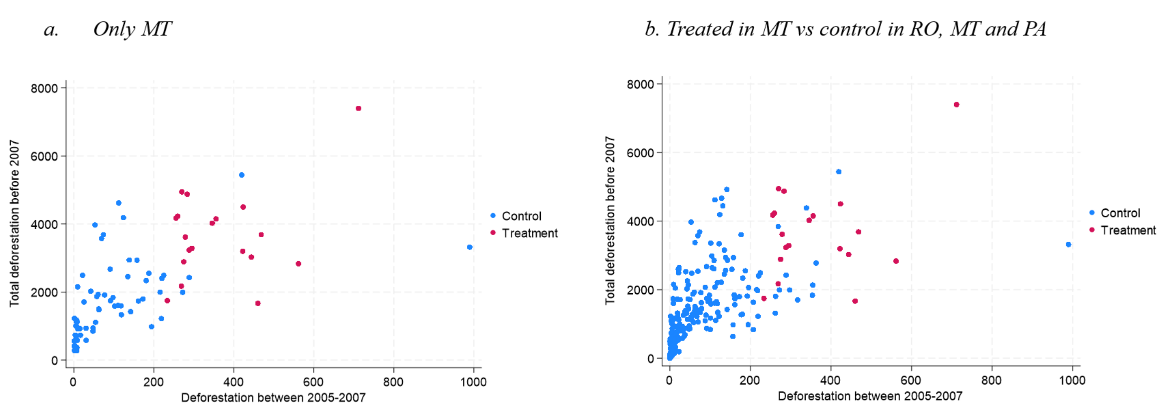
Figures A1-A3 illustrate the reduced overlap in the two key eligibility criteria between treatment and control municipalities when characterizing selection at the state level. Graphs on the left show treatment and control municipalities at the state level for Rondônia, Mato Grosso and then Pará. The right panel shows treatment municipalities in each of these states compared to control municipalities in all three of the states. Overlap of the eligibility criteria between treatment and control is significantly reduced when they are applied at the state level.

Figure A1: Rondônia (RO)



Notes: Figure A1 shows the reduced overlap of values of the eligibility criteria between treatment and control, when municipalities are compared at the state level.

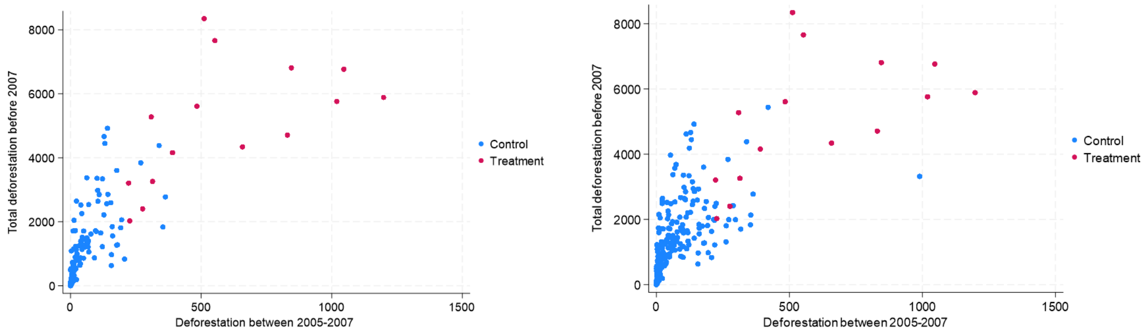
Figure A2: Mato Grosso (MT)



Notes: Figure A2 shows the reduced overlap of values of the eligibility criteria between treatment and control, when municipalities are compared at the state level.

Figure A3: Parà (PA)

a. Only PA *b. Treated in PA vs control in RO, MT and PA*



Notes: Figure A3 shows the reduced overlap of values of the eligibility criteria between treatment and control, when municipalities are compared at the state level.

B Robustness: Two-Stage Least Squares (2SLS) Analysis

To assess the robustness of the results estimated using the eligibility threshold as a proxy for Priority Municipality status, this section reruns the local average 'intent-to-treat' analysis introduced in Section 3, using a 2SLS design. In this specification, the threshold dummy is used as an instrument for treatment status. This specification allows for easier interpretation of estimates as local average treatment effects, but is more power intensive and less comparable with the changes-in-changes analysis, hence its placement in the appendix.

Equations 6 and 7 summarize this estimation strategy, where Y_{hmst} is a given outcome of interest for household h in municipality m and state s at time t. $Threshold_{ms}$ is a dummy for whether or not a municipality exceeds the de facto threshold for inclusion in the PM policy for its state (equal to the deforestation record of the treatment municipality with the lowest record in that state), $Prioity_{ms}$ equals one if a municipality was assigned priority status before 2010 and zero otherwise, and $f(distancefromthreshold)_{ms}$ and $g(distancefromthreshold)_{ms}$ are standardized values of the distance variable for each municipality (Cattaneo et al., 2024). X_{ms} and X_{mst} are vectors of controls for municipality m, including state-level dummies, and ϵ_{ms} and ε_{hmst} are zero-mean disturbances.

Equations 6 and 7 capture the two stages of a two-stage least squares estimator in which the variation in treatment status that can be explained by the eligibility criteria is used to estimate local average treatment effects (LATE). Under the standard assumptions of a fuzzy RD estimator, β_3 captures the average causal impact of priority status at the threshold (Cattaneo et al., 2019).

$$Priority_{ms} = \alpha_0 + \alpha_1 Threshold_{ms} + f(distance\ from\ threshold)_{ms} + X_{ms} + \epsilon_{ms} \tag{6}$$

$$Y_{hmst} = \beta_0 + \beta_1 \hat{Priority}_{ms} + \beta_2 2010_t + \beta_3(2010_t * \hat{Priority}_{ms}) + g(distance\ from\ threshold)_{mst} + X_{mst} + \varepsilon_{hmst} \quad (7)$$

As with the main analysis, estimation makes use of two periods' worth of data from the Brazilian census (2000 and 2010), this time running a difference-in-discontinuities type analysis in the second stage (Wuepper & Finger, 2023).

B.1 Results: Local Average Treatment Effects

The estimates of local average treatment effects, as detailed in Equation 6, are presented in Table A1. These can be interpreted as the effect of residing in a Priority Municipality at the threshold for PM eligibility. In line with the results presented in the main body of the paper, it is not possible to reject the null of no effects on household employment or income at the 5% level.

Table A1: 2SLS: Local Average Treatment Effects

	(1) HH member employed	(2) No. HH members employed	(3) HH member works in ag.	(4) No. HH members in ag.	(5) Total HH income	(6) HH labor income	(7) HH income (labor HH)	(8) HH ag. income	(9) HH income (ag. HH)
Interaction (2010 × Priority Muni. Status (Predicted))	-0.00	-0.04	-0.01	-0.01	-0.04	40.34	27.78	104.3	104.3
	(0.04)	(0.05)	(0.01)	(0.01)	(71.87)	(88.61)	(82.60)	(125.36)	(125.36)
Priority Muni. Status (Predicted)	0.01	0.01	-0.00	-0.01	-25.00	-33.77	0.53	-57.21	-57.21
	(0.03)	(0.05)	(0.02)	(0.02)	(71.53)	(89.58)	(83.68)	(138.00)	(138.00)
Year 2010	-0.01	0.07*	-0.10***	-0.12***	517.0***	591.7***	481.4***	113.9	113.9
	(0.02)	(0.04)	(0.01)	(0.02)	(57.80)	(74.11)	(70.94)	(120.98)	(120.98)
Constant	0.84***	1.37***	0.25***	0.31***	1,402.7***	1,622.1***	1,464.9***	941.1***	941.1***
	(0.02)	(0.03)	(0.01)	(0.01)	(54.41)	(67.81)	(64.50)	(107.91)	(107.91)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SE Clustered Muni	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	187,003	187,003	187,003	187,003	187,003	152,616	152,616	40,144	40,144

Notes: Each column presents results of 2SLS regressions for binary (1-4) and continuous (5-8) dependent variables on the fitted treatment variable. Results are LATE estimates due to imperfect predictive capacity of the eligibility criteria, and represent the second stage of the two-stage fuzzy RD estimation process. All regressions control for state dummies and the standardized distance to the eligibility threshold (all interacted with the time trend). Robust standard errors clustered at the municipality level are displayed below coefficients in parentheses. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

C Changes-in-Changes Assumptions

The assumptions needed for changes-in-changes regression to generate unbiased estimates of treatment effects are discussed below. I draw on the discussion of these assumptions in the context of the Priority Municipalities policy provided in Assunção et al's 2023 paper. However, since both my outcome of interest (income) and my level of analysis (household) differ from theirs, I discuss the assumptions' suitability for my case.

In the following treatment, Y_{hgt} will refer to the outcome (in this case, income) for household h in group g (treatment or control) at time t. In addition, the letter j will reflect the potential outcome under a particular policy scenario (Priority Municipality (j=1) or not (j=0)). The potential income of a particular household is thus given by (Y^j_{hgt}), which is equal to $f_j(X_{hgt}, U_{hgt}, t)$ for $j \in \{0, 1\}$, where X_{hgt} is a vector of observed factors, such as education level or sector of employment,[11] U_{hgt} is a term relating to unobservable components of household earning potential, and the function f_j reflects the effect of different policy scenarios.

Assumption 1: Monotonicity: *the functions $f_j(x, u, t)$, for $j \in \{0, 1\}$, are strictly increasing in u.*

The first assumption for changes-in-changes to generate unbiased treatment effect estimates is monotonicity. This assumption requires all unobservables impacting potential household income to be captured in one scalar, affecting potential outcomes in a consistent direction, such as if characterized as overall 'productive capacity', for example. For instance, a household with higher earning potential should have a higher value of u (or a higher underlying rank for its unobserved characteristics). Though this assumption creates a limit to the possible effect heterogeneity within this setup (e.g. it precludes multiple unobserved traits with separate or varying effects), I consider it to be flexible enough for my purpose when combined with f_j for the following reasons.[12]

- Firstly, though monotonicity implies that u affects potential outcomes in a consistent direction, f_j (the policy effect) can vary flexibly across different values of u. This combination of u and f_j allows for a flexible relationship between unobservables and potential outcomes, and thus for the heterogeneous effects that I am interested in estimating. For instance, conditional on observables, this setup allows for a situation in which households with higher productive capacity (and thus greater earning potential) are more (or less) able to preserve their income streams in the face of the Priority Municipalities policy than those with lower productive capacity, who might be more (or less) sensitive in the face of regulation.
- Secondly, this setup allows for interactions between u and t, reflecting the fact that

[11] These could be observables at the municipality or the household level relating to the earning potential of households.

[12] Note, monotonicity is an important assumption for changes-in-changes and for differences-in-differences analysis.

unobservables relevant for earning potential might change over time.

- Thirdly, the fact that f_j itself can vary over time allows for dynamic impacts of the Priority Municipalities policy, such as time needed for adjustment, or delayed effects (as seen in Merkus, 2024).

Rank invariance—where the ranking of municipalities by unobserved characteristics remains constant across different policy scenarios—is not required for the monotonicity assumption to hold here. This is because the focus of the model is the treatment effect for particular parts of the distribution of unobserved characteristics, rather than for specific households. Furthermore, the flexibility of the f_j term in the model allows for variations in treatment effects across different values of u, and even for u to change as a result of the policy. Therefore, while u must influence outcomes in a consistent way within a given policy context, this influence does not need to remain constant across different policy environments or over time.

Assumption 2: Time Invariance: *Time Invariance:* $U \perp T \mid G, X$

Time invariance requires that the distribution of the unobservable u should be stable over time, conditional on group (G—treatment or control) and on observables (X). This assumption is important because it allows for the construction of a counterfactual using the observed changes over time in the control distribution.

Time invariance plays a similar role to the parallel trends assumption crucial for differences-in-differences analysis, though it is less restrictive:

- For instance, it allows for the values of a particular household's unobservable characteristics to vary or to persist over time, and for that household's rank to change within the distribution.

- It also permits the distributions of unobservables to differ between treatment and control groups to start with, allowing for treatment effect heterogeneity between these groups.

- Thirdly, whilst differences-in-differences imposes the same time effects on potential outcomes across groups, these effects can be group-specific in changes-in-changes, based on observed and unobserved characteristics.

Assessing the appropriateness of the time invariance assumption for this research question requires considering possible changes in the distribution of household 'productive capacity' between 2000 and 2010. For example, changes in skills, attitudes to work or access to work, perhaps as the result of other policy or price changes over the period of interest, could threaten this assumption if occurring differently in treatment and control municipalities.

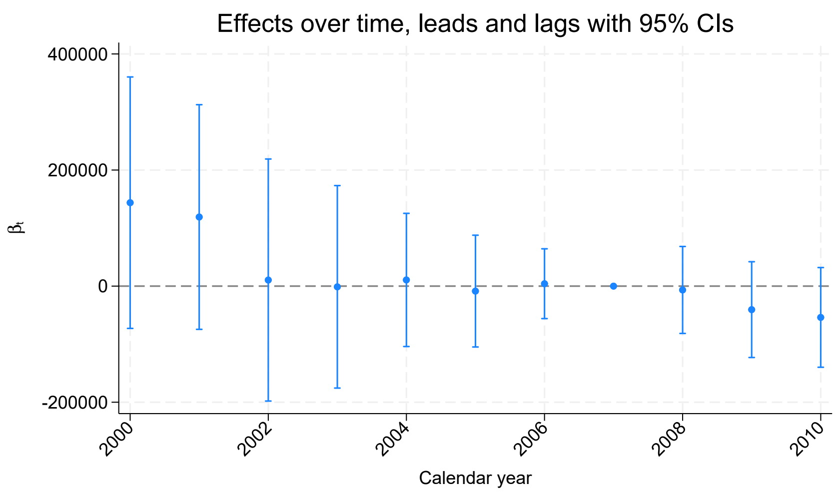
There are a few changes over the 2000-2010 period that could present threats to the time invariance assumption by impacting the distribution of 'productive capacity'.

The Effects of the Commodity Boom

The commodity boom, around 2004, affected the demand for and prices of agricultural goods, such as beef and soy, and likely affected economic activity in the area of study. The boom could have affected the distribution of 'productive capacity' differently in municipalities above and below the threshold for inclusion if it led to uneven job creation across regions that was correlated with historic deforestation.

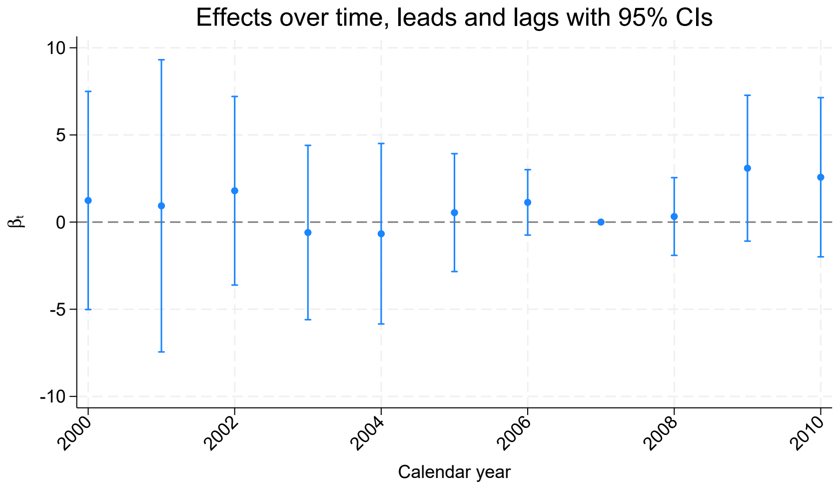
A test of economic trends at the municipality level can serve as an initial sense check for the time invariance assumption in the face of the commodity boom (and more generally). Though this analysis shows parallel trends on average rather than comparing distributions over time, it provides a general picture of stable economic trends between sampled municipalities above and below the threshold for inclusion in the Priority Municipalities policy. Figures A4 and A5 graphically present the results of the analysis from Section 5 for GDP and agricultural value added, respectively.

Figure A4: GDP



Notes: Figure A4 displays the graphical results of a differences-in-differences regression comparing municipalities in the sample that fell above and below the de facto threshold for inclusion in their state over the years 2000-2010 (pre-trends 2000-2007).

Figure A5: Agricultural GDP %



Notes: Figure A5 displays the graphical results of a differences-in-differences regression comparing municipalities in the sample that fell above and below the de facto threshold for inclusion in their state over the years 2000-2010 (pre-trends 2000-2007).

Since a test of parallel economic trends could miss distributional shifts that preserve average trends, the analysis below offers further tests of the time invariance assumption. In particular, it controls for exposure to policies that could affect the distribution of productive capacity in affected municipalities, exploring whether these might explain the observed treatment effects.

The Plan for the Prevention and Control of Illegal Deforestation

As mentioned in the paper, efforts to reduce deforestation in Brazil took off in the early 2000s, especially with the introduction of the Plan for the Prevention and Control of Illegal Deforestation (PPCDAm) in 2004. This range of measures, implemented before the Priority Municipalities were selected, would represent a threat to time invariance between 2000 and 2010 if they shifted the distribution of unobservables in the sample differently for municipalities above and below the threshold for inclusion. This could occur if municipalities more exposed to anti-deforestation measures before 2008 felt economic impacts in the years preceding the Priority Municipalities.

Given the general application of the PPCDAm across the Amazon, and my selection of municipalities with comparable deforestation histories, my empirical strategy has sought to reduce the likelihood that municipalities either side of the threshold experienced different exposure to these policies before 2008. However, to ensure the results are not being driven by these earlier attempts to reduce deforestation, I control for the effect of exposure to PPCDAm over time (fines awarded before 2008 for illegal deforestation at the municipality level interacted with a dummy for the second period) in Tables A2 and A3.

Changes to Social Protection Support

Another potentially relevant change for time invariance is the expansion of the social protection program Bolsa Família in 2009, of around 15% nationwide (Rocha & Meyer, 2023). This expansion directly affects the outcome of interest (income) rather than unobservables per se. However, time invariance would be threatened if the change impacted the distribution of unobservables that might explain earning potential in the sample (conditional on group and on observables). For example, if a household receiving more support were less likely to worry about finding employment, increased Bolsa Família exposure could result in a reduced u for that household. In a scenario where such changes affect the distribution of effort to find employment differently for municipalities above and below the threshold for inclusion (such as if those with the lowest potential were to reduce their potential further) the time invariance assumption could be violated.

In their 2021 paper about the labor market outcomes of the 2009 Bolsa Família expansion, Gerard et al. find evidence to suggest multiplier effects in the local labor market as a result of the policy, as well as suggestive results implying lower employment effort by some recipients. Though it is unclear whether these effects would shift the distribution of unobservables affecting earning potential on aggregate, this is a possibility.

Luckily, since data exists on the Bolsa Familia expansion, it is possible to test whether the results are robust to these changes by controlling for BF exposure in the analysis.

Results of the analysis controlling for exposure to the PPCDAm and the Bolsa Famìlia changes is presented in Tables A2 and A3 below, and is consistent with those presented in the main body of the paper.[13]

[13]Results are also robust to controlling for just one of each of these variables.

Table A2: Controlling for Policy Shocks: Local Average Intent-to-Treat (ITT) Effects

	(1) HH member employed	(2) No. HH members employed	(3) HH member works in ag.	(4) No. HH members in ag.	(5) Total HH income	(6) HH income (labor HH)	(7) HH labor income	(8) HH ag. income	(9) HH income (ag. HH)
Interaction (2010 × Above threshold)	-0.00	-0.00	-0.01	-0.01	22.31	60.26	51.30	8.23	16.17
	(0.02)	(0.04)	(0.03)	(0.04)	(80.27)	(86.10)	(81.91)	(85.41)	(76.31)
Above threshold	0.00	-0.00	0.00	-0.00	-42.63	-48.80	-19.22	20.37	22.04
	(0.01)	(0.03)	(0.04)	(0.05)	(90.43)	(96.19)	(93.03)	(106.91)	(97.12)
Year 2010	-0.05***	-0.05*	-0.03	-0.03	249.41***	355.11***	259.22***	32.78	35.63
	(0.02)	(0.03)	(0.03)	(0.04)	(80.81)	(84.64)	(79.70)	(94.65)	(80.10)
Fines pre-2008*2010	0.00***	0.00***	-0.00***	-0.00***	0.62***	0.55***	0.51***	0.34*	0.27**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.08)	(0.08)	(0.07)	(0.19)	(0.12)
Bolsa Família % Change*2010	-0.46	-1.13	0.75*	1.12*	-2,845.9*	-2,758.1*	-2,151.6	-692.3	-570.3
	(0.39)	(0.81)	(0.39)	(0.59)	(1,597.49)	(1,575.36)	(1,481.6)	(1,292.5)	(1,145.6)
Constant	0.84***	1.37***	0.25***	0.31***	1,411.0***	1,630.0***	1,472.2***	916.5***	935.1***
	(0.01)	(0.02)	(0.02)	(0.03)	(73.49)	(77.19)	(75.94)	(59.82)	(57.71)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SE Clustered Muni	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	187,003	187,003	187,003	187,003	187,003	152,616	152,616	40,144	46,512

Notes: Each column presents results of DID regressions for binary dependent variables on the interaction of the binary threshold variable with the dummy for the second period (2010). Results are ITT estimates due to imperfect predictive capacity of the eligibility criteria. All regressions control for state, the standardized distance to the eligibility threshold, fines for illegal deforestation delivered since baseline, and exposure to changes in the Bolsa Família program (all interacted with the time trend). Robust standard errors clustered at the municipality level are displayed below coefficients in parentheses. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

Table A3: Controlling for Policy Shocks: Distributional Effects

	(1) Total HH income	(2) HH labor income	(3) Total HH income if labor	(4) HH agricultural income	(5) Total HH income if agriculture
10th percentile	50.50***	26.51	22.52***	-78.90***	-88.69***
	(18.59)	(19.72)	(6.95)	(10.21)	(32.45)
50th percentile	3.78	20.85	22.52	-111.0***	-5.56
	(9.06)	(13.64)	(24.10)	(20.24)	(24.13)
90th percentile	63.73	142.4*	216.6***	160.3*	140.9
	(80.89)	(80.24)	(73.46)	(83.09)	(120.4)
P Values: *Kolmogorov-Smirnov (KS) and Cramer-von-Mises-Smirnov (CMS) Stats*					
No effect	**		***	***	*
Constant effect			**	**	
Effect>0 for all				***	**
Effect<0 for all	***		***		

Notes: Each column presents results of CIC regressions for income variables on the binary threshold variable at the 10th, 50th and 90th percentiles. Results are ITT estimates since this variable explains treatment eligibility but not treatment status perfectly. All regressions control for state dummies, the standardized distance to the eligibility threshold, fines for illegal deforestation delivered since baseline, and exposure to changes in the Bolsa Família program (all interacted with the time trend). Robust standard errors are clustered at the municipality level are displayed below coefficients in parentheses. P-values in the bottom panel test for any effect, constant effects across the distribution, and effects that are all negative or all positive. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

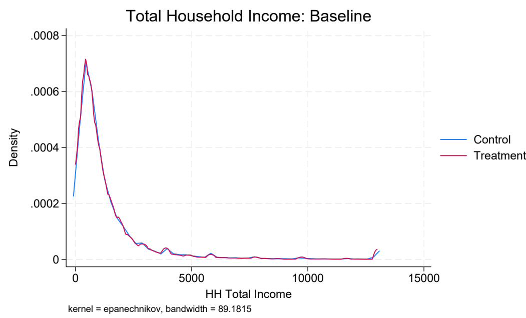
Assumption 3: Common Support: $Y(j)_{gt} = Y_{gt}$ *for j,g,t=0,1.*

The common support assumption specifies that the policy scenario (j) cannot affect the support of the distribution of outcomes. This is important for ensuring that a counterfactual for the treatment group can be located within the control distribution, and vice versa if the research question requires this. The assumption implies that $\mathbb{Y}_{10} \subseteq \mathbb{Y}_{00}$ and that $\mathbb{Y}^0{}_{11} \subseteq \mathbb{Y}_{01}$, both necessary for the point-identification of the treatment counterfactual. However, where the common support assumption doesn't hold, changes-in-changes analysis can estimate treatment bounds rather than point estimates.

Since I compare households in Priority Municipalities to others in control municipalities with very similar deforestation histories, I consider it likely that these areas have similar economic characteristics, and that income distributions are comparable ex ante. Furthermore, I do not expect the Priority Municipalities policy to dramatically affect these distributions in a way that would leave the treatment group unsupported by control, especially given my focus on short-term impacts, and the fact that economic outcomes were not the goal of the policy (but rather a bi-product).

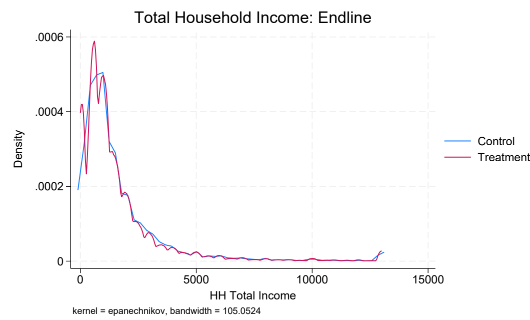
Below, I compare the distributions of the treatment and control groups visually at baseline and endline, to get a picture of the likely appropriateness of the assumption. These overlap very closely, implying that common support is plausible in this case. However, to ensure I do not rely on results at the extremes of the distribution, I focus my analysis of quantile effects at the 'bottom' of the distribution on the 10th percentile, and those for the 'top' of the distribution on the 90th percentile.

Figure A6: Common Support Figures: Total Income



(a)

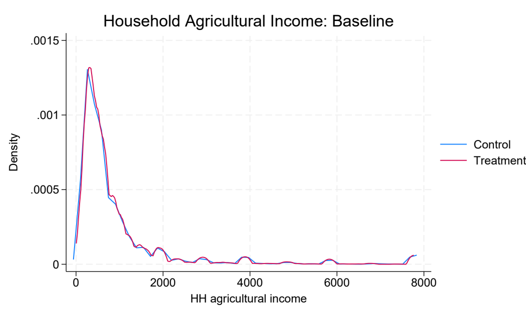
Notes: Figure A7a displays the distribution of household monthly income for municipalities above and below the threshold at baseline (in the year 2000).



(b)

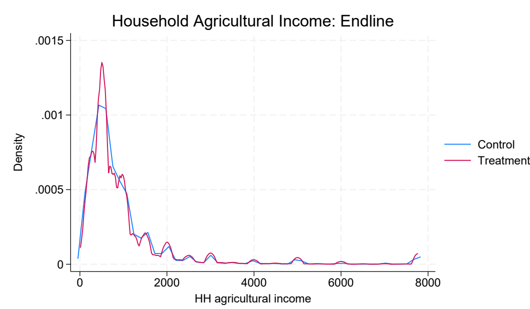
Notes: Figure A7b displays the distribution of household monthly income for municipalities above and below the threshold at endline (in the year 2010).

Figure A7: Common Support Figures: Agricultural Income



(a)

Notes: Figure A7a displays the distribution of household monthly agricultural income for municipalities above and below the threshold at baseline (in the year 2000).



(b)

Notes: Figure A7b displays the distribution of household monthly agricultural income for municipalities above and below the threshold at endline (in the year 2010).

D Alternative Bandwidths and Polynomials

This section displays robustness tests for treatment effects estimation over various bandwidths of the distance variable and for altering the order of the polynomial characterizing this variable.

Results are stable across different bandwidths (Table A4 displays the average effect estimates and Table A5, the estimates of distributional effects). Indeed, both the average and the distributional effect coefficients remain very similar across bandwidths. However, in the case of the distributional analysis, smaller bandwidths result in a loss of effect significance, perhaps as a result of reduced power.[14]

Results are also robust to different polynomials of the distance variable, as well as to its exclusion from regressions (Tables A6 and A7).

[14]To assess whether treatment effects differ across the distribution, I consider p-values from both Kolmogorov–Smirnov (KS) and Cramér–von Mises–Smirnov (CMS) tests. Tables report statistical significance only when both tests yield consistent evidence. However, given the focus on localized distributional changes, the KS test is likely the most appropriate statistic. This test rejects the constant effect test more frequently than the CMS test.

Table A4: Varying the Estimation Bandwidth: Local Average 'ITT' Effects

	(1) HH member employed	(2) No. hh members employed	(3) HH member works in ag.	(4) No. HH members in ag.	(5) Total HH income	(6) HH labor income	(7) HH income (labor HH)	(8) HH ag. income	(9) HH income (ag. HH)
Local Average ITT Effect (1 SD Bandwidth)	0.00	-0.00	0.01	0.01	2.83	26.53	30.63	0.02	17.30
	(0.02)	(0.04)	(0.03)	(0.04)	(105.53)	(112.14)	(107.47)	(105.55)	(118.21)
Local Average ITT Effect (1.25 SD Bandwidth)	-0.00	-0.01	-0.01	-0.01	23.79	61.91	57.26	18.51	25.42
	(0.02)	(0.04)	(0.03)	(0.03)	(97.84)	(101.86)	(96.41)	(91.32)	(82.99)
Local Average ITT Effect (1.5 SD Bandwidth)	0.00	0.00	-0.01	-0.01	32.76	69.63	61.37	31.79	34.19
	(0.02)	(0.04)	(0.02)	(0.03)	(96.70)	(100.90)	(95.22)	(89.10)	(80.71)
Local Average ITT Effect (1.75 SD Bandwidth)	0.00	0.01	0.00	0.01	1.69	25.66	21.73	22.83	26.21
	(0.02)	(0.04)	(0.02)	(0.03)	(92.81)	(97.23)	(91.94)	(83.73)	(75.28)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SEs Clustered Muni	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column presents results of DID regressions for binary (1-4) and continuous (5-8) dependent variables on the interaction of the binary threshold variable with the dummy for the second period (2010), for multiple bandwidths. Results are ITT estimates due to imperfect predictive capacity of the eligibility criteria. All regressions control for state dummies, and the standardized distance to the eligibility threshold (all interacted with the time trend). Robust standard errors clustered at the municipality level are displayed below coefficients in parentheses. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

Table A5: Varying the Estimation Bandwidth: Distributional Effects

	Total HH income	HH labor income	HH income (labor HH)	HH ag. income	HH income (ag. HH)	N (munis)
1 SD Bandwidth						
10th percentile	-44.28	-1.51	-29.10***	-45.01***	-27.96	
	(19.03)	(8.35)	(10.63)	(12.25)	(36.39)	
50th percentile	22.46	20.93	40.00**	-10.84	49.49***	
	(21.08)	(14.94)	(15.74)	(24.12)	(16.91)	
90th percentile	14.35	20.16	98.82	107.2	98.77	
	(50.72)	(81.12)	(90.15)	(106.0)	(134.5)	
Constant effect						80
1.25 SD Bandwidth						
10th percentile	-40.71**	46.03***	43.33***	-74.34***	-106.2***	
	(17.78)	(12.18)	(6.849)	(11.85)	(34.47)	
50th percentile	10.54*	41.18***	39.73***	-80.81***	40.60*	
	(5.567)	(13.81)	(14.08)	(20.46)	(24.33)	
90th percentile	96.96	83.54	183.5***	177.6*	156.1	
	(64.75)	(57.38)	(70.22)	(95.54)	(103.8)	
Constant effect			*	**	**	92
1.5 SD Bandwidth						
10th percentile	-29.25*	25.51	61.26***	-67.05***	-84.63***	
	(15.76)	(16.49)	(7.545)	(10.99)	(30.11)	
50th percentile	36.88***	40.95**	42.36*	-71.06***	45.95**	
	(10.31)	(17.05)	(22.46)	(18.95)	(22.91)	
90th percentile	73.65	139.1*	239.4***	199.7**	184.5	
	(78.07)	(78.55)	(79.04)	(85.52)	(124.1)	
Constant effect				***	**	97
1.75 SD Bandwidth						
10th percentile	-38.14**	13.79	20.51***	-33.95***	-69.15**	
	(17.03)	(13.06)	(7.606)	(10.69)	(29.08)	
50th percentile	-0.552	43.93***	21.68	-36.40*	43.45**	
	(7.917)	(13.84)	(15.77)	(21.12)	(17.67)	
90th percentile	86.94	61.37	165.8***	129.1	168.1*	
	(66.35)	(83.23)	(55.04)	(83.20)	(101.4)	
Constant effect			*		**	100

Notes: Each column presents results of CIC regressions for income variables on the binary threshold variable for multiple bandwidths. Results are ITT estimates due to imperfect predictive capacity of the eligibility criteria. All regressions control for state dummies, and the standardized distance to the eligibility threshold (all interacted with the time trend). Robust standard errors clustered at the municipality level are displayed below coefficients in parentheses. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

Table A6: Varying the Distance Variable Polynomial: Local Average 'ITT' Effects

	(1) HH member employed	(2) No. HH members employed	(3) HH member works in ag.	(4) No. HH members in ag.	(5) Total HH income (monthly)	(6) HH labor income (monthly)	(7) HH income (labor HH)	(8) HH ag. income (monthly)	(9) HH income (ag. HH)
Local Average ITT Effect (Variable Excluded)	-0.00	-0.00	-0.01	-0.00	10.00	36.60	30.22	22.46	23.02
	(0.02)	(0.04)	(0.02)	(0.03)	(101.30)	(104.81)	(99.89)	(92.07)	(83.54)
Local Average ITT Effect (1st Order)	0.00	0.00	-0.01	-0.01	32.76	69.63	61.37	31.79	34.19
	(0.02)	(0.04)	(0.02)	(0.03)	(96.70)	(100.90)	(95.22)	(89.10)	(80.71)
Local Average ITT Effect (2nd Order)	-0.00	-0.01	0.00	0.01	-21.95	3.68	-2.00	9.75	10.55
	(0.02)	(0.04)	(0.03)	(0.04)	(106.04)	(111.55)	(106.21)	(88.27)	(81.14)
Local Average ITT Effect (3rd Order)	-0.00	-0.01	0.00	0.01	-20.31	4.79	-0.94	9.54	10.07
	(0.02)	(0.05)	(0.03)	(0.04)	(106.56)	(111.56)	(106.05)	(88.79)	(81.78)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SE Clustered Muni	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column presents results of DID regressions for binary (1-4) and continuous (5-8) dependent variables on the interaction of the binary threshold variable with the dummy for the second period (2010), for multiple characterizations of the distance variable polynomial. Results are ITT estimates due to imperfect predictive capacity of the eligibility criteria. All regressions control for state dummies, and the standardized distance to the eligibility threshold (all interacted with the time trend). Robust standard errors clustered at the municipality level are displayed below coefficients in parentheses. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.

Table A7: Varying the Distance Variable Polynomial: Distributional Effects

	Total HH income	HH labor income	HH income (labor HH)	HH ag. income	HH income (ag. HH)
Variable Excluded					
10th percentile	44.83**	-23.92*	4.807	-62.85***	-107.5***
	(17.41)	(13.53)	(4.840)	(18.14)	(34.06)
50th percentile	9.657	0.717	2	-10	37.68
	(12.89)	(19.82)	(21.03)	(10.97)	(24.05)
90th percentile	23.84	104.7	200***	130	155.6
	(73.81)	(69.92)	(72.34)	(105.0)	(129.5)
Constant effect		*	*		*
1st Order					
10th percentile	-29.25*	25.51	61.26***	-67.05***	-84.63***
	(15.76)	(16.49)	(7.545)	(10.99)	(30.11)
50th percentile	36.88***	40.95**	42.36*	-71.06***	45.95**
	(10.31)	(17.05)	(22.46)	(18.95)	(22.91)
90th percentile	73.65	139.1*	239.4***	199.7**	184.5
	(78.07)	(78.55)	(79.04)	(85.52)	(124.1)
Constant effect				***	**
2nd Order					
10th percentile	-26.24	-34.21*	0.212	-64.89***	-146.0***
	(19.67)	(17.84)	(5.226)	(13.03)	(33.91)
50th percentile	43.11***	33.83**	48.58**	-99.43***	-7.639
	(9.888)	(16.58)	(22.00)	(15.92)	(23.53)
90th percentile	95.32	154.8**	235.4***	161.9*	144.4
	(77.43)	(76.06)	(78.66)	(87.64)	(126.9)
Constant effect		***	***	***	**
3rd Order					
10th percentile	-76.97***	-27.78	-17.22**	-69.40***	-135.0***
	(17.39)	(18.32)	(7.731)	(12.38)	(33.59)
50th percentile	-19.56*	-19.32	-16.88	-95.84***	-1.300
	(10.78)	(17.46)	(24.11)	(16.83)	(22.33)
90th percentile	21.71	73.50	160.5**	163.0*	151.6
	(77.37)	(77.47)	(77.32)	(87.40)	(126.7)
Constant effect		*	**	**	*

Notes: Each column presents results of CIC regressions for income variables on the binary threshold variable for multiple characterizations of the distance variable. Results are ITT estimates due to imperfect predictive capacity of the eligibility criteria. All regressions control for state dummies, and the standardized distance to the eligibility threshold (all interacted with the time trend). Robust standard errors clustered at the municipality level are displayed below coefficients in parentheses. Asterisks denote a statistically significant difference at the 1% ***, 5% **, or 10% * levels.