Norwegian School of Economics
Spring 2019, Bergen

INTERGOVERNMENTAL TRANSFERS IN BRAZIL: CORRUPTION AND DEFORESTATION

Master's thesis at the Norwegian School of Economics[1]

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[1] This thesis was written as a part of the Master of Science in Economics and Business Administration at NHH. Please note that neither the institution nor the examiners are responsible through the approval of this thesis for the theories and methods used, or results and conclusions drawn, in this work.

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Abstract

This paper investigates the effect of additional governmental transfers on several different measures of deforestation for the years between 1997 and 2016. We use comprehensive datasets from the MapBiomas database for our deforestation calculations. This relatively new database affords us the opportunity to analyze changes in Brazilian municipalities from a single data source over an extended period rather than having to draw from separate sources with different methodologies. We use a regression discontinuity design to estimate the causal effect of increases in transfers on deforestation in Brazil, emphasizing on municipalities in the Legal Amazon. Results obtained from the entire sample show that larger transfers result in significant increases in gross deforestation. Extending our analysis to pooled thresholds and pooled years, we find some evidence of separate patterns between the period from 1997–2006 and the period from 2008–2016, but mixed effects in both magnitude and statistical significance when we restrict our samples to individual thresholds. While there is evidence in the first period that is in line with our expectation of larger transfers leading to higher levels of deforestation, the surprising positive increases in forest coverage in the second period 2008–2016 suggest the promising possibility of governmental transfers having beneficial impacts on efforts to reduce deforestation in Brazil as well.

Acknowledgements

We would like to express our gratitude to our supervisor, Torfinn Harding, for his invaluable insights into the empirical methods used and for providing us with generous feedback on the structure and direction of our thesis.

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1. INTRODUCTION

In the paper "The Political Resource Curse" examining Brazilian municipalities, Brollo et al. (2013) found that higher federal transfers led to increased corruption. We build upon this here by investigating whether the municipalities that received more money from the central government also had a higher level of deforestation. If we assume that there is a positive relationship between corruption and deforestation, as several previous studies have shown (see section 2.4), then higher transfers should increase the latter. Following Brollo et al. (2013), we utilize the fact that the amount of transfers a municipality is given is based on population size in a stepwise manner. This distribution mechanism creates several thresholds at which the municipalities differ in terms how much money they receive, enabling us to conduct a regression discontinuity (RD) design on its effect on deforestation. Since the thresholds were exogenously set, the municipalities lying close to one should differ only in how much transfers they receive, depending on whether they are above it (more) or below it (less). Our research question is as follows: *Do extra intergovernmental transfers affect deforestation in Brazil?*

As far as we know, this is the first study utilizing a regression discontinuity approach to study the relationship between deforestation and intergovernmental transfers using the population thresholds for FPM transfers in Brazil. In addition, we attempt to include as many municipalities as possible for a twenty-year period.

Tropical rainforests have global significance. Firstly, because they function as a storage place for carbon, and by that act as a counterforce to greenhouse gas emissions. Since deforestation decreases this effect, it contributes to global warming. According to Bierbaum et al. (2009), around 32 percent of global CO_2 emissions were caused by tropical deforestation in the period 1990–2005. Secondly, tropical forests house a large amount of the world's biodiversity (Burgess et al., 2012). Avoidance of a mass extinction of species entails preserving the large tropical forests of the world. There are also other more local concerns, such as deforestation causing increased malaria incidents (Foley et al., 2007), decreased rainfall (Nepstad et al., 2009), and destruction of the homes of indian tribes living in the forests (van Solinge, 2010). It is, therefore, of great importance to work to preserve the tropical rainforests that are still standing. More knowledge about the workings and causes of deforestation will contribute to this.

The main findings of this thesis are, firstly, that increased intergovernmental transfers likely increased deforestation in Brazil in the period 1997–2006. The effect is especially high for the Legal Amazon sample. Secondly, for the period 2008–2016, there are mixed effects.

Depending on the dataset utilized, we find evidence that transfers both increased forest cover and increased deforestation in the period. This result may have been caused by weaknesses in the methodology or the dataset used, but it can also suggest that there are different channels by which transfers affect forest cover.

The following section provides a background and literature review on deforestation and corruption with a focus on Brazil. In addition, we introduce the framework and results found in the paper by Brollo et al. (2013) in more detail. Section 3 gives a description of the data we use in our study. Section 4 explains our empirical strategy, followed by an assessment of the internal validity of our approach in Section 5. Section 6 contains the analysis and discussions on our results and the robustness of our findings. Section 7 concludes our paper.

2. BACKGROUND AND LITERATURE REVIEW

2.1 The Brazilian Amazon and deforestation

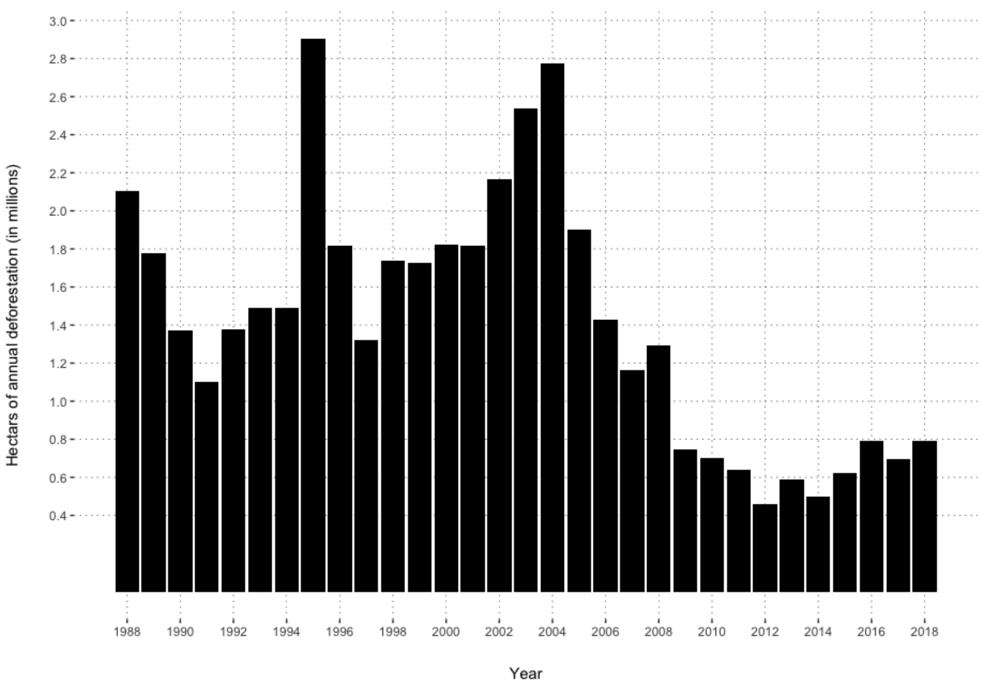

Figure 1: *Annual deforestation in the Legal Amazon. Source: own, created with data from PRODES.*

The Amazon rainforest is the planet's largest and most biodiverse tropical forest. Around 58 percent of it is located inside the borders of Brazil, in the area known as the Legal Amazon. This is the largest of the three socio-geographic divisions in Brazil, and it includes all of the states in the North Region of Brazil, in addition to Mato Grosso and Maranhão, states located in the Central West and North East regions, respectively. Even though it comprises nearly 60 percent of the total area of Brazil, the Legal Amazon contains only around 10 percent of the population.

The rainforest has been in existence at least since the Eocene period, 55–33.9 million years ago (Burnham & Johnson, 2004). Since then, the size of it has fluctuated along with changes in natural conditions. At the beginning of the 20th century, approximately 4 million km^2 out of the 5 million km^2 large Legal Amazon were forested (Kirby et al., 2006). What is considered the "modern era" of deforestation started with the construction of roads in the second half of the 20th century, providing access to previously impenetrable areas (Fearnside, 2017). The construction of the first of them, the Belém-Brasilía highway, was initiated in 1958, with the goal of integrating western and northern states with the rest of the country; while the building of Cuiabá-Porto Velho (BR-364) highway was begun in 1968 to open up the southern part of the Amazon (Moran, 1993; Kirby et al., 2006). Until the late 1990s, these two roads were the only federal highways in the Legal Amazon passable year-round, and, consequently, became the focal region of deforestation in the Brazilian Amazon, an area often called the "arc of deforestation." Roads opened up veins in the rainforest for settlements and economic development, and the military dictatorship (1964–1985) arguably accelerated this process through various incentives given to colonizers and entrepreneurs, especially focusing on cattle ranching (Kirby et al., 2006). In the late 1980s, however, due to increasing international pressure, the government began to issue various policies mitigating the alarming rate of deforestation (Arima et al., 2014).

In the 1970s, satellite-based deforestation data emerged. From the years 1978–1988, only the average rate of deforestation over the entire period is available. The annual mean deforestation was 20,400 km^2. From the 1990s onward, there are deforestation data available for each year, with the exception of 1993. As can be seen in Figure 1, annual deforestation exhibits considerable variability, suggesting that it is sensitive to shifts in the macroeconomic environment and new policies. There is an increasing trend throughout the 1990s and the early 2000s, culminating in 2004 with a deforestation rate of 27.8 thousand km^2. In the period 1995–2007, lagged prices of soy and beef explain more than 75 percent of the deforestation that occurred (Arima et al., 2014). In the years after 2004, the deforestation rate gradually declined, and in 2012 it was a record-low 4,800 km^2. Since then it has slowly increased. Also worth noticing is the record-high peak in 1995, caused by a surge of investments after the economic reforms of 1994 ending the hyperinflation (Fearnside, 2017).

2.1.1 Conservation efforts

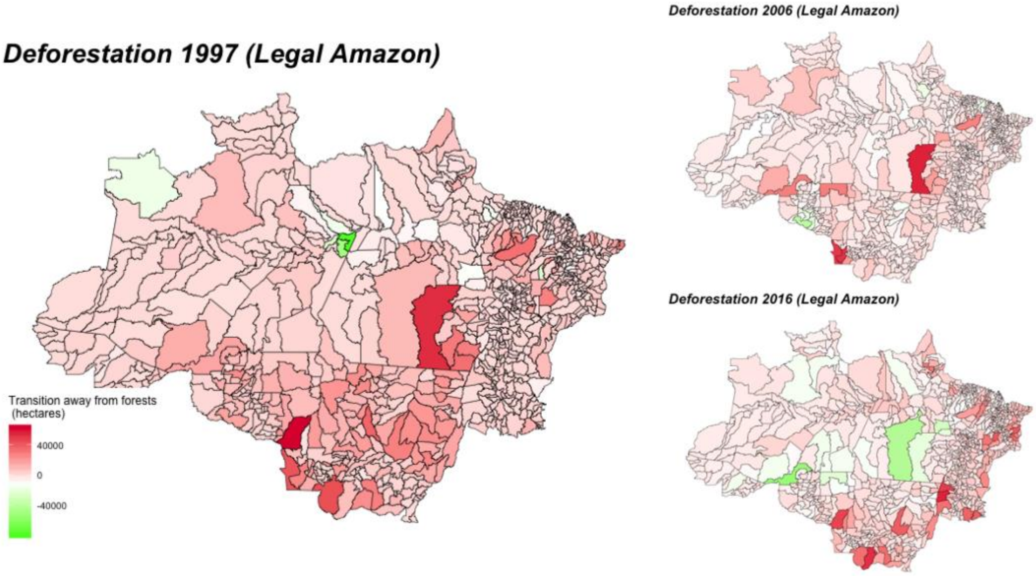

Figure 2: *Map of deforestation in the Legal Amazon. Red indicates deforestation and green indicates increase in forest cover. Source: own, created with data from MapBiomas.*

There have been many efforts to reduce deforestation in Brazil and especially in the Legal Amazon. One such effort is the Soy Moratorium, which is a multiparty initiative to reduce the clearing of land in the Amazon for the production of soy. Soy traders avoid purchasing soy from producers who cleared land in the Amazon and producers who do not comply with the moratorium lose access to the seed and fertilizer market (Gibbs et al., 2015). The Brazilian government joined the effort in 2008 when the Brazilian National Institute for Space Research began satellite monitoring of areas that complied with the Soy Moratorium. Gibbs et al. (2015) find that the Soy Moratorium contributed to the drastic reduction in deforestation for soy production from 30 percent to 1 percent in the period 2004 to 2014. Other Brazilian policies include the expansion of indigenous and protected areas in the Amazon to more than 50 percent today (Soraes-Filho et al., 2010). The National Plan for Recovery of Native Vegetation launched in 2017 that aims to reforest 12 million hectares by 2030 is another program that is definitely an ambitious step towards continuing the trend of reducing, and even reversing deforestation in Brazil (Assunção and Gandour, 2019).

Figure 2 shows this pattern in the Legal Amazon as it depicts a decrease in deforestation in the Legal Amazon from 1997 to 2006 and in 2016. Having said that, deforestation is still occurring in most areas throughout the Legal Amazon. Moreover, there is no discernible trend of municipalities maintaining their lower levels of deforestation as the levels of deforestation among the municipalities fluctuate. The deforestation maps for Brazil as shown in Figure 8 in the appendix also exhibits both the decreasing trend and variability among municipalities over the years.

The involvement of the international community with Brazil in Reducing Emissions from Deforestation and Forest Degradation (REDD+) programs to incentivize the reduction of emissions from forest loss through the sale of carbon credits has also played an important role in reducing deforestation by 79 percent since 2004 (May et al., 2016). This is in spite of the decline in the price of carbon credits and the fact that Brazil was receiving a fixed rate rather than being able to sell their credits in the carbon market (Boucher et al., 2013). Norway's collaboration with and strong support for Brazil's REDD+ efforts, for example, has led to the establishment of the Amazon Fund that funds programs to prevent, monitor and combat deforestation (Amazon Fund, 2016). While the effects of reducing deforestation, reforestation and restoration efforts in Brazil might explain some of the growth of forest cover in Brazil, there are still many opportunities to improve. Moreover, the spike in a set of 2017 levels of deforestation might be the beginning of a worrying trend (University of Maryland, 2018).

What caused the remarkable decline in deforestation from 2004 to 2012? In the period 2005–2007, export prices were still the main drivers of deforestation (Assunção et al., 2015). However, the link disappeared after 2007. Even though commodity prices recovered, deforestation rates continued to decline. This has been attributed to new conservation policies and better surveillance technologies, increasing the risk faced by farmers and companies conducting illegal forest activities, while also reducing the need for new deforestation (Nepstad & Shimada, 2018). One of the most important policies was the publication of blacklisted districts starting in 2008, where districts with high annual forest losses face more administrative hurdles to obtain authorization to clear additional forests (Cisneros et al., 2015).

2.2 "The Political Resource Curse"

In their paper "The Political Resource Curse," Brollo et al. (2013) investigate the effect of windfall resources, in the form of federal (FPM) transfers, on municipality level corruption. Based on a political agency framework, Brollo et al. posit a divergence of interests between politicians and voters—namely, that politicians seek to maximize personal utility (through political rents[4], i.e., corruption), not the voters' utility. They present a model to explain the

[4] They also propose that politicians derive utility through what they call "ego rents," which are personal benefits of being in office other than mere wealth; i.e., they are exogenous to the model.

dynamics between government revenues and elections to political offices. Since the voters have incomplete information of how revenues are used, they will vote for the candidates that are expected to provide the greatest quantity of public goods, represented by the official budget constraint (for the incumbents) and the perceived quality of the candidates (for the opponents).

Brollo et al. (2013) predict, among other things, that higher governmental revenues expand the possibilities of increasing political rents without suffering electoral punishment, since the impact of each dollar stolen decreases relative to the total budget constraint. A second channel in which higher revenues can increase corruption is through the quality of the opponents, indicated by their educational attainment. Political rents are marginally more valuable to lower educated opponents, as their outside options are worse compared to those with higher education. Therefore, an increase in government revenue will lower the quality of the pool of opponents, and by that increase the amount of rents the incumbent is able to extract without suffering electoral punishment. In their empirical analysis Brollo et al. (2013) find that a 10 percent increase in federal transfers to municipal governments raises local broad corruption (which includes mismanagement) by 6 percent and narrow corruption (only severe violations) by 16 percent.

A straightforward analysis of the effects of governmental transfers on corruption would most likely suffer from endogeneity issues. For this reason, they utilize the fact that FPM transfers are distributed in a step-wise manner based on municipality population size. Each state receives a different share of the total transfers, and these shares are in turn distributed to the municipalities according to population brackets. Two municipalities occupying the same bracket and located in the same state should therefore receive the same amount. The thresholds are pre-set and equal for all the states in Brazil. This set-up makes it possible to use a regression discontinuity (RD) design to analyze the effect of FPM transfers on corruption (or deforestation, in our case), with the population thresholds serving as instruments for transfers.

2.3 Misuse and corruption in Brazil

2.3.1 Audits

Ferraz and Finan (2011) detail the types of corruption most common in Brazil. They give examples of how the combination of typical frauds, like over-invoicing and diversion of funds, with public works programs—that are, perhaps intentionally, never finished—presents financial rewards for bad behavior. More elaborate schemes like phantom firms and non-competitive single bidding with the illusion of multiple bidders are also not uncommon. To combat the prevalence of corruption, the Luiz Inácio Lula da Silva government started the Controladoria Geral da União (CGU) in 2003 to conduct random audits of municipal governments. The CGU, together with the Tribunal de Contas da União (TCU), the Ministério Público Federal (MPF) and the Polícia Federal were in charge of identifying and punishing corrupt administrators (Avis et al., 2016)[5]. In their paper, Ferraz and Finan find that mayors with re-election incentives misappropriated 27 percent less resources compared to mayors without these incentives. In another study, Avis et al. (2016) find that the audits were effective in reducing corruption and the amount negative spillover effects to neighboring municipalities. The consequences of legal action against corrupt administrators have helped to reduce corruption as politicians were ineffective in hiding illegal behavior. Furthermore, mayors who were concerned with reelection soon became aware that corrupt mayors were unlikely to be reelected and this was an additional deterrent (Ferraz & Finnan, 2011). Having said that, audits were not effective in reducing mismanagement (Avis et al. 2016).

2.3.2 Municipality funding and corruption

Funding determination aside, the assumption of more funding leading to more positive development is shaky. Brazil's fortune of having valuable oil reserves that led to large municipal revenues, which in turn resulted in increases in public spending, should have had proportional infrastructure quality improvements if the assumption held true. However, Caselli and Michaels (2013) find that improvements in social indicators were much smaller relative to spending, and the problem of "missing money"—a result of political rent-seeking behavior—might be a reason for this discrepancy. Similarly, Brollo et al. (2013) find that a decrease in the quality of the political process follows an increase in public education and healthcare spending. For example, politicians who realized that merely expanding the public sector in the two election cycles between 1997 and 2004 increased their probability of reelection, did enlarge the public sector in their municipality. However, no significant impacts were found in the quality of education or health supply (Monteiro & Ferraz, 2010). Monteiro and Ferraz also note that expansions were likely reversed as public opinions changed and many did not see them as constructive to job creation in the 2008 election cycle. Mayors who enlarged the public sector were thus less likely to be reelected. In other words, politicians simply reacted to voters' preferences, but voters unfortunately often take years to realize that spending increases were the result of resource windfalls. Gadenne (2016) shows that government spending habits also

[5] Incidentally, the results from these audits are what Brollo et al. (2013) use as their corruption measure.

change with different types of revenue inflow. She concludes that governments direct increases in local tax revenue towards public infrastructure spending to a larger degree than if they were increases in FPM revenues. It is also important to note that despite the disproportionate effects in increased spending and quality of improvements, Gadenne (2016), Monteiro and Ferraz (2010) find at worst insignificant improvements in standards of living and not societal regression due to rent-seeking behavior. Gadenne (2006) proposes that local tax collection is a necessity for a well-functioning decentralized structure, further implying the importance and the benefits of maintaining such a system. In addition, Monteiro and Ferraz (2010) assert that the democratic system is critical to avoid even greater corruption.

With regards to rule-based transfers, Litschig (2008) contends that even "technocratic inputs can be circumvented and manipulated for political gain." Methods of manipulating inputs might include gerrymandering for predetermined municipality borders and misreporting of census data to exceed population thresholds in order to receive more transfers.

In addition to political ill-will and circumvention of laws, complexities concerning coordination and supervision, particularly in the case of Brazil, amplify the difficulty to correct the misallocations. Brazil's move from a centralized military rule to a decentralized federal democracy of 26 states and more than 5500 municipalities with substantial political and fiscal autonomy brought about a broad increase in standards of living. However, it also led to increased bureaucracy (Cepaluni, 2015). Moreover, Brazil's proportional representation system has resulted in a multiparty system, which consisted of 15 parties in 1998, more than 30 today, and many others applying to be legally recognized. These intergovernmental relations "unavoidably contain many tensions and contradictions" (Dillinger & Webb, 1998). The highly valuable resources within the Amazon suggests to us that there are greater rent-seeking opportunities for politicians and it would not be surprising if there is a higher likelihood of misallocation and misappropriation of federal transfers.

2.4 Corruption and deforestation in Brazil

Very little research has been done on corruption in forestry (Kolstad & Søreide, 2009). However, a relatively large number of studies have explored this topic compared to research on other types of resources and environmental issues (Sundström, 2016). According to FAO (2001), deforestation caused by corruption is prevalent around the world, especially in forest-rich developing countries. In fact, the forest sector may be more susceptible to corruption than other sectors. Forest activities often take place in big and remote areas, and the local forest offices are often granted large discretionary powers in measuring, classifying and sometimes valuing forest products (FAO, 2001). In addition, local officials often administrate permits and enforce regulations, which generates frequent opportunities for corruption. It has also been suggested that local politicians may use deforestation as a way to launder money derived from corruption (Fearnside, 2017). There are several ways in which corruption are thought to be related to deforestation; for a more comprehensive review, see Sundström or FAO.

There is little reason to doubt that corruption actually influences deforestation in the Brazilian Amazon. For example, Contreras-Hermosilla (2002) writes that a Brazilian high-level commission showed that 80 percent of all logging that happened during the 1990s was illegal. Furthermore, a Greenpeace report states that as much as 90 percent of all logging in the Amazon is illegal (Greenpeace, 1999)[6]. Although illegal logging is not the same as corruption, it is hard to imagine this level of deforestation happening without the aid of the latter.

2.4.1 Empirical research on corruption and deforestation in Brazil

There are other empirical studies that have been conducted on the relationship between corruption and deforestation in the Brazilian Amazon. Mendes and Porto Jr. (2012) use municipality corruption data from CGU and deforestation data from INPE for a cross-sectional analysis for the year 2004. Performing a non-parametric analysis with a Generalized Additive Model (GAM), they find no significant effect for the area as a whole. They also investigate the effect of economic growth on deforestation, which turns out to be significant. However, when restricting the sample to only include the states Pará and Mato Grosso (the states with the highest rates of deforestation at the time), the result indicates a nonzero relationship between corruption and deforestation.

Cisneros et al. (2013), on the other hand, studies the effect of the randomized CGU audits for the period 2002–2009 using deforestation data from the PRODES project. Analyzing 209 municipalities, they find that the municipalities with a one standard deviation higher level of corruption experienced up to 20 percent more deforestation in the period. Furthermore, exploiting the randomness of audits allocation, they find that deforestation actually increased 11 percent on average in the aftermath of the audits. Most of this effect was realized in the first years after the audit happened, and it took on average six years after the audit for a significant decrease of deforestation to happen. The effect is more pronounced for municipalities in which

[6] Keep in mind, however, that these figures are from the 1990s. The ratio of legal/illegal deforestation may have changed in the following two decades. Nevertheless, they suggest that illegal deforestation have been, and most likely still is, a serious problem.

the incumbent mayor faced re-election. In addition, there is evidence for spatial spillovers to neighboring municipalities after an audit. Cisneros et al. suggest that a reason for the increase in deforestation after audits is diversion of corrupt activities towards sectors that are more less directly observable for the public audit system, with the forestry sector being one of these.

Pailler (2018) studies the relationship between re-election and deforestation for the years 2002–2012. She finds deforestation rates increase 8–10 percent in election years when the incumbent mayor runs for re-election. The deforestation effect is especially high if the incumbents have self-funded campaign contributions. Furthermore, these electoral deforestation cycles seem not to be driven by changes related to agricultural activity and policy, but are instead connected to corruption and campaign finance. Pailler writes that "it is clear that Brazil's politicians have the power to misallocate forest resources," and suggests that this finding can be attributed to the level of decentralization of the Brazilian governmental system. Natural resources can become more vulnerable to political manipulation when governed locally, an effect that gets aggravated by corruption and weak institutions. (Pailler, 2018).

2.5 Brief Economic History of Brazil

Dillinger (1997) identifies three major debt crises prior to 1998, with the final one happening as a result of the 1994 Plano Real, which was Brazil's stabilization plan. While the plan, which consisted of monetary policy tightening and fiscal adjustments among other drastic policy changes, was largely successful in reducing inflation from 929 percent in 1994 to 9 percent in 2006, Brazilian states were unable to control their payrolls and this ended in defaults during the 1990s (Dillinger & Webb, 1998). After the Asian Financial Crisis in 1997 and 1998 Russian Financial Crisis, the Central Bank of Brazil's international reserves plunged. In 1999, the crawling-peg was dropped and a floating exchange rate followed by an inflation-targeting policy were successfully implemented. However, Brazil was again affected both externally by recession in the United States and Argentina in 2001, and internally by systemic political negligence in Brazil's energy sector. These factors eventually led to President Lula's election (Ayres et al., 2018). New policies like the previously mentioned establishment of the CGU to fight corruption and fortunate events like worldwide commodity price increases also led to Brazil's prosperous 2004-2008 period. Even without including the hyperinflation periods prior to 1997, these fluctuations in Brazil's economic history might be useful in explaining some of the variations in our data.

Agricultural output as a percentage of Brazil's GDP and total land area use has remained around 4.5 percent and 30 percent respectively since the 1990s (The World Bank, 2018). The stabilization after the periods of hyperinflation and the improvements in global trade liberalization provided the opportunity for Brazil to become the world's largest soybean exporter (Schnepf et al., 2001). However, this growth in soybean production required a conversion from land that were either farmland for other crops or natural forests to soybean farms. Moreover, agricultural output prices have been found to have significant effects on deforestation (Robalino and Herrera, 2010). While the high proportion of land use is consistent with farming in Brazil, this seemingly inefficient usage of land – 30 percent of land used to contribute 4.5 percent to Brazil's GDP – makes us question why agricultural output prices have such grave negative impacts on deforestation in Brazil (Verburg et al., 2014; Robalino & Herrera, 2010).

3. DATA

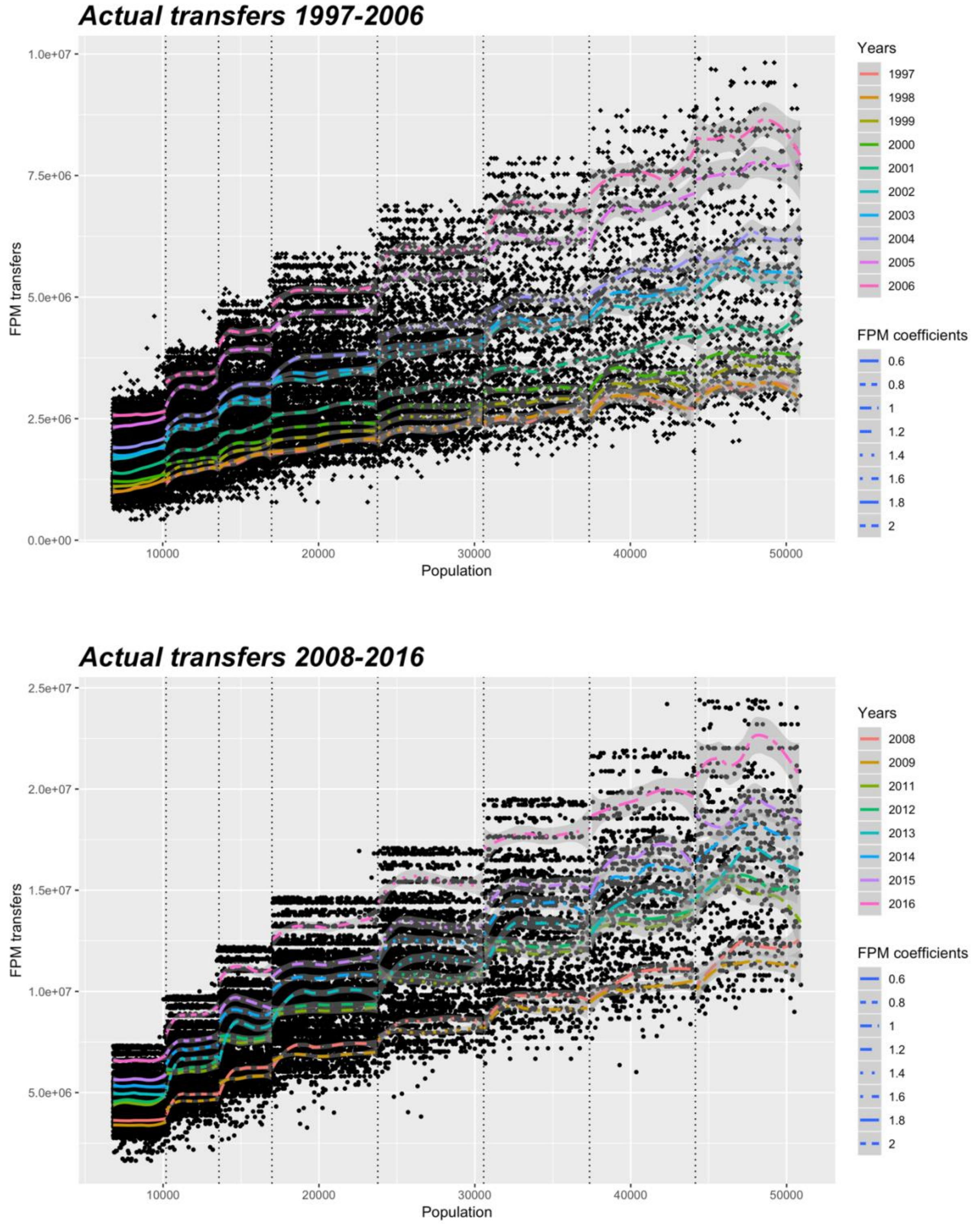

Figure 3: *Plots of actual transfers against population. Years are differentiated by color and points within each threshold are smoothed. Source: own, created with data from the Brazilian treasury.*

We obtained our data from three main sources. Data on FPM transfers were taken from the Brazilian treasury, Tesouro Nacional (Tesouro Nacional, 2019). They provide the sum of FPM transfers to a municipality in a given year. From the same source we also retrieved the coefficients used to calculate the amount of transfers a municipality is eligible to receive. Using the data on FPM transfers, we calculated the sum of the transfers to each state, and, combined with the FPM coefficients, we derived the "theoretical" FPM transfers for each municipality—how much a municipality should have received according to the allocation rule. From Figure 3, we are able to observe some evidence of municipalities receiving more than they should. Although the level of non-compliance is more noticeable in the first period (1997–2006) than in the second (2008–2016), the proportion of municipalities receiving transfers that seem to exceed their theoretical FPM transfer bracket appears to be relatively small. Nevertheless, we believe that this imperfect compliance, especially in the first period, is a strong enough reason to include a fuzzy regression discontinuity design. For summary statistics of FPM transfers, see Table 7 in appendix A.1 below.

Population estimates were obtained from the Instituto Brasileiro de Geografia e Estatística (IBGE, 2018). These are the estimates that the Federal Court of Audit (TCU) uses to calculate the FPM transfers for the following year (Brollo et al., 2013). For our main sample, we have decided to use the same restrictions as Brollo et al. and only include municipalities with populations below 50,940 as they comprised approximately 90 percent of Brazilian municipalities. For the more highly populated municipalities, there would be very few observations near each threshold, especially for the subsample located in the Legal Amazon.

IBGE population data is used to create the running variable for our RD. This variable is generated by first finding the closest threshold a municipality's population is to, and later retaining a negative number if the municipality is short of the threshold or a positive one if it exceeds the threshold.

We derived deforestation data from MapBiomas' extensive database (Project MapBiomas, 2018). The coverage dataset was used to calculate change in forest cover (net deforestation) and total forest cover from the yearly estimated natural forest cover. A positive change indicates an increase in forest cover for the period, and a decrease in cover for a negative change. In addition, we also calculate percentage change of forest cover as a proportion of forest cover change over total forest cover. Since we are aware of the fact that a higher degree of deforestation has been detected in municipalities with larger forests (Hargrave & Kis-katos, 2013), we add this variable as an additional outcome of interest as we expect at least some differences in results between municipalities with similar populations but different sized forests. For completeness, we have also used MapBiomas' transition dataset (Project MapBiomas, 2018), which contains land cover transition values between time periods. We derived yearly gross deforestation on the municipal level by adding up all transitions away from natural forest.

Table 1: *Summary statistics for the Legal Amazon*

	1997–2006			2008–2016		
Population interval	Change in forest cover	Total forest cover	Obs.	Change in forest cover	Total forest cover	Obs.
6793–10,189	-3222 (7994)	383,690 (912,594)	1,064	-174 (8689)	305,790 (534,569)	687
10,189–13,584	-3901 (16214)	386,546 (906,619)	888	214 (8062)	248,272 (411,647)	747
13,585–16,980	-3766 (11,461)	475,566 (922,081)	711	224 (11,476)	475,679 (1,058,283)	539
16,981–23,772	-2724 (9755)	648,436 (1,395,353)	916	151 (13,715)	686,943 (1,417,323)	889
23,773–30,564	-3971 (12,095)	890,411.5 (1,746,310)	578	399 (15,558)	635,226 (1,559,494)	477
30,565–37,356	-5442 (19,515)	1,066,961 (2,128,501)	341	64 (13,111)	707,578 (1,236,967)	354
37,357–44,148	-6994 (21,514)	1,105,708 (1,966,213)	241	146 (17,840)	1,278,931 (2,388,626)	215
44,149–50,940	-8157 (17,558)	909,530 (1,809,783)	140	1414 (13,348)	707,384 (1,104,943)	154

Notes: Cells display average change in forest cover and average total forest cover. Standard deviation in parentheses.

Since this is a study on deforestation, we have restricted our main sample to states in the Legal Amazon. These states are mostly located in the northern region of Brazil. However, we also do a parallel analysis with an extended sample, where we include all municipalities. We have data on 3348 of the 5570 Brazilian municipalities. Out of the 3348 municipalities included, 567 are located in the Brazilian Amazon. Compared to Brollo et al. (2013), whose dataset encompasses the two mayoral terms 2001–2004 and 2005–2008, our study covers the period 1997–2016. We chose to include more years to increase the number of observations. It also makes it possible to compare two periods with different levels of deforestation. In our analysis we have divided these years into the periods: 1997–2006 and 2008–2016. Because IBGE have not published population estimates for 2007 and 2010, these two years are excluded from our dataset.

Table 2: *Summary statistics for Brazil*

	1997–2006			2008–2016		
Population interval	Change in forest cover	Total forest cover	Obs.	Change in forest cover	Total forest cover	Obs.
6793–10,189	-578 (3515)	77,064 (372126)	7360	-52 (3373)	57,736 (215,779)	5404
10,189–13,584	-706 (6830)	83,506 (386,967)	5642	-54 (3631)	62,277 (188,134)	4714
13,585–16,980	-798 (5390)	108,754 (419,862)	4178	-68 (5111)	103,301 (460,244)	3337
16,981–23,772	-597 (4870)	142,622 (616,105)	5512	-112 (6303)	160,502 (668,847)	4706
23,773–30,564	-825 (5797)	195,498 (810,963)	3214	-120 (7168)	148,493 (702,922)	2646
30,565–37,356	-1147 (8999)	237,568 (986,605)	1894	-189 (6343)	702,922 (615,738)	1780
37,357–44,148	-1538 (10,219)	260,000 (964,044)	1242	-60 (8058)	277,836 (1,143,660)	1143
44,149–50,940	-1790 (8874)	212,227 (839,992)	776	102 (6680)	198,428 (198,428)	737

Notes: Cells display average change in forest cover and average total forest cover. Standard deviation in parentheses.

4. EMPIRICAL STRATEGY

4.1 Regression discontinuity design

The econometric approach used in this paper is a regression discontinuity (RD) design. This method was first introduced in a paper by Donald L. Thistlethwaite and Donald T. Campbell in 1960, where they analyzed the impact of merit awards on future academic outcomes (Cook, 2008; Thistlethwaite & Campbell, 1960). From the beginning, its usage has been mostly confined to specific fields of research, of which education has been the most prominent. In the recent decades, however, the popularity of regression discontinuity designs has increased, and it has been applied within a growing range of research areas in economics (Imbens & Lemieux, 2008). The central difference between RD and other econometric methods is that it is able to exploit thresholds in the data to make causal inferences. The data points are designated a treatment status based on whether they are above or below a certain score, also called "threshold" or "cut-off," on a continuous variable, often called the "running variable." This can be formalized as

$$T_i = \mathbf{1}(R_i \geq c) \tag{1}$$

where T_i is the binary treatment status of unit i, R_i is its score on the running variable, c is the cut-off value and $\mathbf{1}(\cdot)$ is an indicator function. Observations close to the threshold should be equal except for whether or not they received treatment. The average distance of the outcome variable between treated and not-treated units at the cut-off is called the treatment effect, as defined below:

$$\tau_{SRD} \equiv \mathbb{E}[Y_i(1) - Y_i(0)|R_i = c] \tag{2}$$

Here, τ_{SRD} is the treatment effect in "sharp" RD designs, $Y_i(1)$ is the outcome for units receiving treatment, and $Y_i(0)$ is the outcome for units not receiving treatment. In practice, τ_{SRD} is estimated by doing separate regressions on each side of c and measuring the distance between the regression lines on c. Translated into an econometric model, we can write it as:

$$Def_i = \alpha + \tau * T_i + \beta_1 * f(R_i) + \beta_2 * f(R_i) * T_i + \epsilon_i \tag{3}$$

Def_i denotes deforestation for unit i, T_i is a treatment dummy, R_i is the running variable, $f(\cdot)$ is the functional form of the running variable and ϵ_i is the error term; the parameter of interest is τ, which is the equivalent to τ_{SRD} mentioned above. The fourth term in the model is an interaction with the running variable and the treatment dummy, allowing for a different slope of the regression line after the cut-off. In the analysis, we use a non-parametric approach to calculate the treatment effect. This means that only data within a certain bandwidth of the running variable are used (Skovron & Titiunik, 2015). Furthermore, the observations within the bandwidths are weighted using a triangular kernel.

The main dependent variable in the analysis is change in total forest cover, but we also provide results for percentage change in total forest cover. These are both indicators of the amount of deforestation, but the weighting is different; two municipalities could exhibit the same amount of deforestation in a year, but if one of them has a larger total forest cover, this municipality will have a smaller percentage change. If the results are robust, then the analysis should show significance for both dependent variables. In addition, we provide results where transition from natural forest is the dependent variable.

4.2 Rdrobust

For the analysis we use the R/Stata package `rdrobust` (Calonico et al., 2015; Calonico et al., 2017). This package is based on data-driven local-polynomial regression with robust biased-corrected confidence intervals for average treatment effects around the cut-offs (Calonico et al., 2015). This is a non-parametric approach, i.e. it only uses the data points near the cut-off for regression lines. The alternative to this approach is a parametric approach, where every observation is used to specify the functional form for the rating variable (Jacob et al., 2012). In general, both approaches are recommended and should ideally complement each other, strengthening the validity of the analysis (Lee & Lemieux, 2010). But since the nonparametric strategy is the only approach available in `rdrobust`, that is what we will use in this paper. Conveniently, the package also provides a function for MSE-optimal bandwidth selection (`rdbwselect`), and a function for making plots (`rdplot`).

4.3 Fuzzy Regression Discontinuity Design

An important distinction made in the econometrics literature is between cases where there is absolute correspondence between treatment assignment and actual treatment and cases where there is not (Lee & Lemieux, 2010). In the first situation, every individual above the cut-off receive the treatment, while every individual below the cut-off do not receive the treatment; i.e., directly at the cut-off the probability of receiving treatment jumps from 0 to 1. A regular SRD design is appropriate in such cases. If, on the other hand, the probability of receiving treatment is not binary, some individuals above the cut-off do not receive treatment, while some individuals below the cut-off do. In other words, the treatment assignment does not perfectly correspond to the actual treatment—the relationship is "fuzzy." As mentioned in Section 3, our data does indeed exhibit fuzziness, especially before 2008. To control for this, a fuzzy regression discontinuity (FRD) design incorporates the two-stage least squares (2SLS) technique, where an actual treatment dummy serves as an instrument for T_i (Lee & Lemieux, 2010). See appendix A.2 for information on how we constructed the treatment dummy.

In the analysis section, we report results for both type of approaches. The SRD results should be considered as the average effect on the amount of deforestation of being assigned to treatment, also called the *intent to treat at the cut-point* (ITTC) effect, while the FRD results are the treatment effects. It should be noted that the precision of an FRD design is often less than that of an SRD design (Jacob et al., 2012). Furthermore, there are too few observations for some of the regressions we conduct in the analysis to do FRD. Consequently, we only provide FRD results for the regressions with pooled thresholds.

4.4 Deforestation: coverage change, percentage change, transition

The differences in the datasets produced by organizations like the Program for the Estimation of Deforestation in the Brazilian Amazon (PRODES) and University of Maryland (UMD) also present challenges for us on what we should consider when calculating net deforestation. These methodological differences lead to large divergences as evidenced by two different 2017 forest cover loss figures: Where PRODES reported 0.69 million hectares of cover loss, UMD's analysis showed 3.47 million hectares of cover loss (Goldman and Weisse, 2018). They estimate that there will be a 46 percent reduction in UMD's deforestation figures if losses in non-primary forests are not accounted for and an additional 13 percent reduction if deforested areas that are smaller than 6.25 hectares are not included. We use an approach similar to UMD's, which includes a larger definition of forest cover and does not distinguish between primary forest loss and secondary forest loss. Despite these disparities, Goldman and Weisse (2018) make a reasonable case for the inclusion of different methodologies as the loss of non-primary forests also negatively impacts the environment.

4.5 Division and pooling

There are multiple thresholds in the population variable in our study. The running variable is a normalized transformation of this, such that its score is the distance in population to the nearest threshold. The score is negative if it lies on the left-hand side of the nearest cut-off and positive if it is on the right-hand side. Since we have multiple cut-offs in the running variable, and data spanning many years, there are several ways to approach the analysis. Municipalities at the first cut-off have a population size around 10,189, while municipalities at the last cut-off have over four times that amount. In addition, several changes took place from 1997, the first year in our dataset, to 2016, the last, both economically and politically. Therefore, the most interesting approach would be to analyze each threshold year-by-year. However, we have chosen to prioritize the pooling of data because there are not enough observations to do analyses by individual years and individual thresholds, unfortunately. For example, in the Legal Amazon sample, only the first two thresholds have a satisfying number of municipalities to do yearly analysis. Hence, since we want to include all the thresholds in the analysis, we have to pool the data.

One option is to pool the observations by thresholds. Another option is to pool them by year. We have done both, but we focus on the latter approach, as that gives us the opportunity to explore potential differences in how municipalities respond to transfers based on their population size. Even though we pool by year, there are still opportunities to split the analysis into different time periods.

We have decided to split the analysis into two time periods, separated by 2007 as IBGE population estimates for 2007 are not available. Furthermore, the Brazilian federal government's decision for FPM coefficients to be attached to their actual population estimates by 2008 also led to our decision to divide our analysis into these two periods. Prior to 2008, there were laws in place to freeze FPM coefficients despite municipalities splitting and their populations reducing during the 1990s (Brollo et al., 2013). This freezing of FPM coefficients led to large inconsistencies. As a result, the government decided on a transition period from 2001 to 2008 to correct these inconsistencies between transfers received by municipalities and their actual population estimates.

Although we have data for the whole of Brazil, the region we are most interested in is the Legal Amazon. The reason for this is that the Amazon rainforest as a whole possess qualities that makes it especially valuable to preserve, as indicated in Section 2. Nevertheless, we present results for both Brazil in general and the Legal Amazon in particular. This division can also be argued for on the grounds that there are special institutions relating to conservation working exclusively in the Amazon region, possibly affecting the relationship between FPM transfers and corruption.

5. INTERNAL VALIDITY

The strength of RD designs derives from the fact that the specific values of the cut-offs are unrelated to the different baseline characteristics of the municipalities near the cut-offs (Smith et al., 2017). As Imbens and Lemieux (2008) writes: "There are generally two main conceptual concerns in the application of RD designs, sharp or fuzzy." These are (1) the possibility of changes other than that of the treatment occurring at the cut-off points and (2) manipulation of the running variable. In this section we discuss each of these concerns in turn.

5.1 Other changes at the cut-offs

Confounding changes in the area around the cut-offs make causal inference difficult, as changes in the deforestation variable can be attributed to factors other than the FPM transfers. An example could be changes in the frequency of government auditing for municipalities above one or more of the population thresholds. Brollo et al. (2013) finds no legislative or institutional discontinuities at the thresholds in their analysis (the same thresholds as we use), except for one close to the first threshold, at 10,189: The wage cap for city councilors increases by 50 percent for municipalities with 10,000 or more inhabitants. This is close enough to be included in the bandwidth used in our analysis, and hence should be kept in mind while considering the results.

5.2 Manipulation of the running variable

An important assumption in RD designs is that there is no precise sorting around the cut-off (Imbens & Lemieux, 2008). Litschig & Morrison (2012) found evidence for manipulation of the population estimates in the 1990s, but other studies focusing on the 2000s have not. For example, Brollo et al. (2013) writes that opportunistic manipulation of the TCU population data cannot be ruled out, and they find that the population estimates from IBGE and TCU do not perfectly coincide; but, they find little evidence of manipulation. If there occurred manipulation of the population estimates at the cut-off, then we should expect to see systematically more observations above the threshold than below, i.e., a discontinuity in the running variable at the cut-off. Litschig's (2008) finding of population estimates manipulation from 1989 to 1991 is an example that might severely affect the relevance of studies on FPM transfers in the 1990s. He contends that manipulation in this period led to significant differences in FPM transfers as municipalities that had populations above the population cut-offs received approximately R$30 million more than those below the cut-offs for the entire length of the 1990s. These FPM coefficients were rarely updated and therefore had long-lasting effects.

Litschig (2008) identifies several ways of manipulation. Municipal administrators could have attained information on how IBGE calculates population estimates and provided IBGE with data that would inflate their population statistics. Municipal administrators could have also paid off IBGE officials in charge of the estimates. Although CGU randomized audits were found to be successful in drastically reducing many forms of corruption (Avis et al., 2018), the impact of benefits gained by municipalities that were successful in overreporting of their population statistics prior to the 2003 anti-corruption program should not be dismissed. Municipalities that experienced a declining population but retained their FPM coefficients also enjoyed these benefits. Aside from illegal manipulation, Freitas and Menezes (2018) found that the increase of FPM transfers at cut-offs leads to an increase in immigration. This could be due to the availability of public programs in larger, more well-funded municipalities. While the McCrary density tests that we conducted did not show significant evidence of manipulation at individual thresholds, there are significant discontinuities for municipalities in thresholds 1–3 in the years 1997, 1998 and 2011.

We can test for violation of the assumption of no manipulation using a McCrary density test, which tests the null hypothesis of continuity of the density of the running variable, against the alternative hypothesis of a jump in the density at the cut-off (McCrary, 2008). Figure 4 displays the McCrary plots of the entire samples for both Brazil and the Legal Amazon. We are unable to reject the null hypothesis of no manipulation since both p-values are above the 10 percent level. Figure 5 displays the result of this test for the pooled thresholds for both time periods and Table 8 in the appendix contains results for each threshold. Histograms of the numbers of observations displayed in Figure 9 is another method of visualizing the possibility of manipulation around the cut-offs. There is no indication of manipulation except for pooled thresholds 1-3 in the first time period. The p-value of the latter is 0.063, so we cannot reject the null hypothesis at the 5 percent level. The possibility of manipulation here is worth keeping in mind. However, as Lee & Lemieux (2010) writes: “If individuals—even while having some influence—are unable to precisely manipulate the assignment variable, a consequence of this is that the variation in treatment near the threshold is randomized as though from a randomized experiment.” The results of these McCrary tests lead us to believe that the likelihood of Brazilian municipalities intentionally manipulating their scores is low.

5.2.1 McCrary density test for entire sample

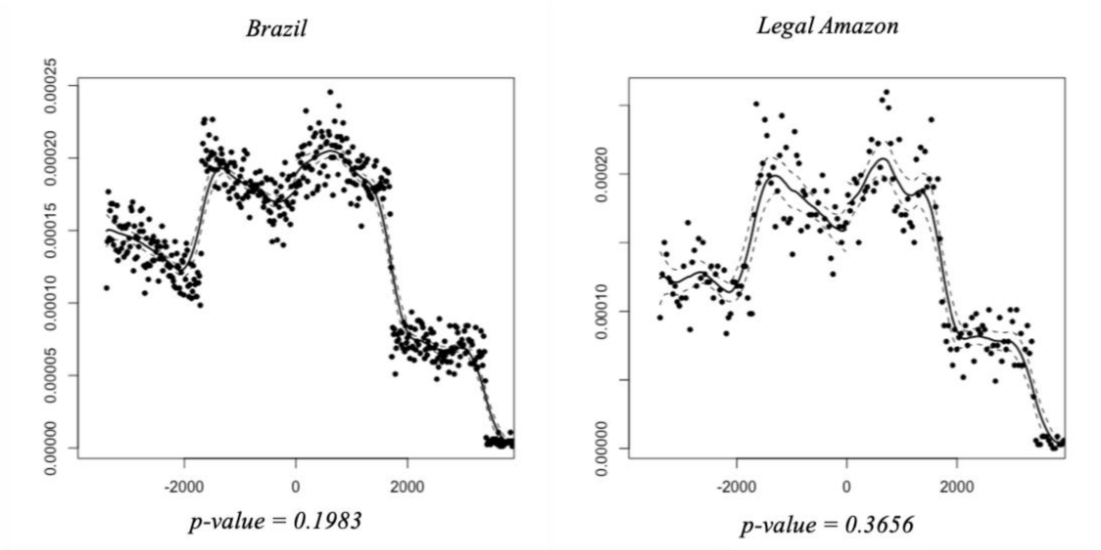

Figure 4: *McCrary density plots for entire dataset for Brazil (left) and the Legal Amazon (right)*

5.2.2 *McCrary density test for pooled thresholds*

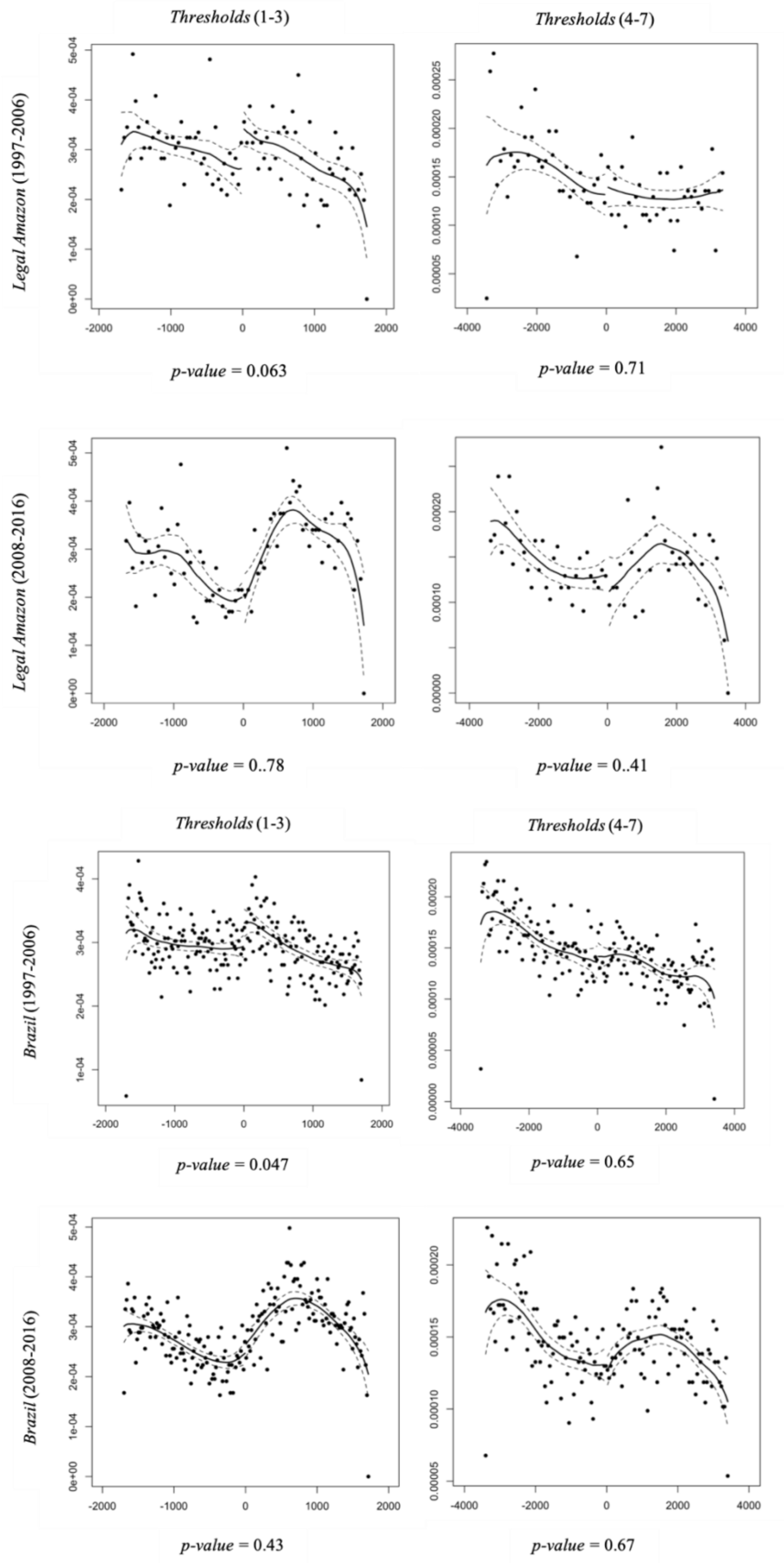

Figure 5: *McCrary density plots for pooled thresholds 1-3 (left), 4-7 (right) and pooled years*

6. ANALYSIS

In this section we present the empirical results of our investigation. We will present four tables of regression outputs that are most relevant to our study. These four tables that we here display our findings for pooled years. Results from our analysis of the non-Legal Amazon and individual years are in the appendix. First, we present an analysis of the Legal Amazon, then we extend the analysis to include all the municipalities in Brazil, both with change in forest cover as the dependent variable. Next, we conduct the same regressions as above, but now with transition from natural forest as the dependent variable.

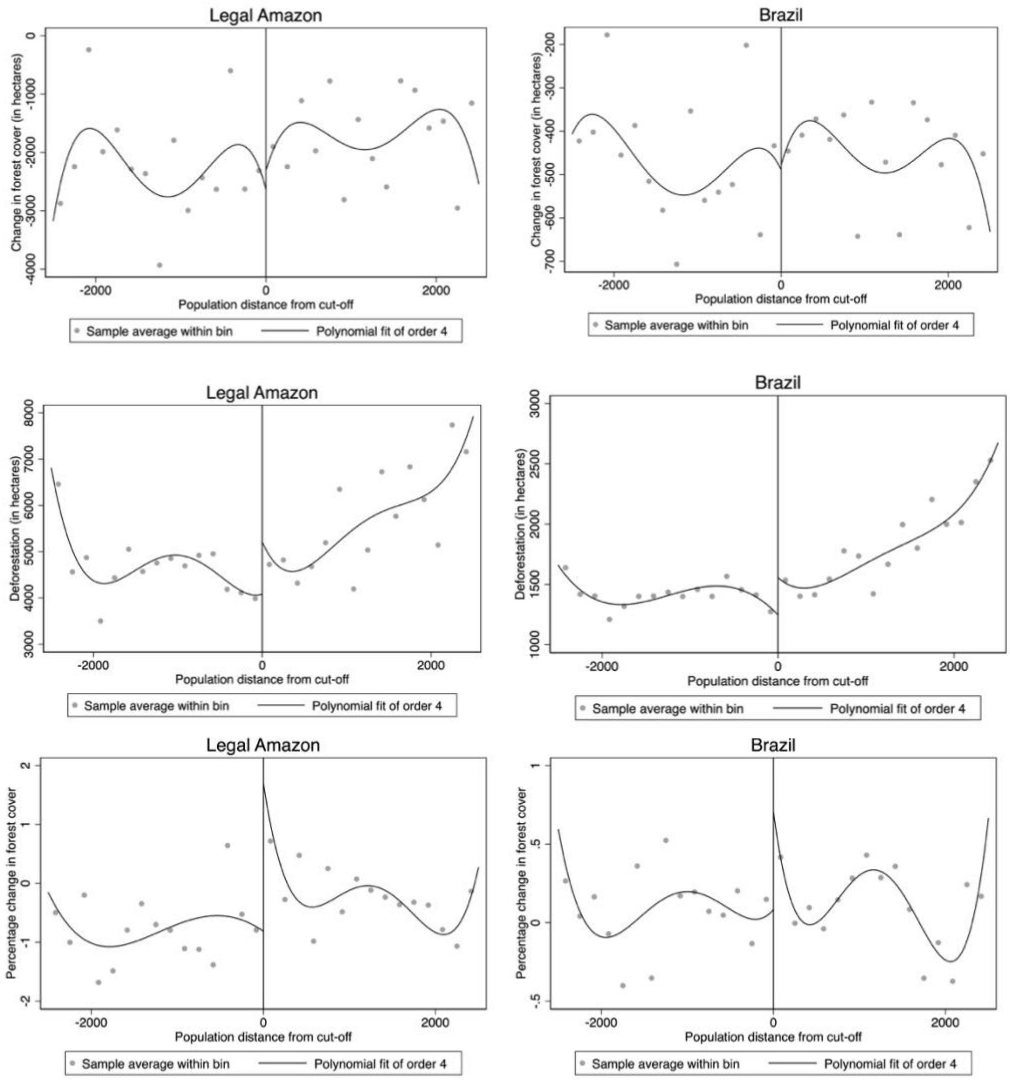

Figure 6: *RD plots for aggregated dataset. Legal Amazon (left), Brazil (right). Coverage change (top), transition away from forest (middle), percentage change (bottom). The plots were created by using the rdplot function in the rdrobust package for STATA.*

Figure 6 presents graphical analyses of the aggregated dataset for the Legal Amazon and Brazil. All years and thresholds are pooled together. The uppermost plots are for change in forest cover as the dependent variable. There seems to be no discontinuity at the cut-offs. In the middle, we show the plots for the transition dataset. Here, there seems to be a discontinuity for both samples, with increased deforestation at the right-hand side of the cut-offs. The bottom-most plots have percentage change in forest cover as the dependent variable, and also here there seem to be discontinuities at the cut-offs, but the bin observations are more spread. A higher value of the dependent variable indicates less deforestation, so here we see an opposite effect compared to the middle graph, with less deforestation above the cut-offs.

6.1 Empirical estimations

6.1.1 Pooled thresholds and years

In table 3, we display our results for the entire sample, including all the years and all the thresholds. We have included the same dependent variables as in the plots shown above: (1) change in forest cover, (2) percentage change in forest cover and (3) transition away from forest. For the first dependent variable, changes in forest cover, changes are positive for the Legal Amazon (less deforestation), and positive with SRD and negative with FRD in Brazil. However, none of these are significant. The second dependent variable, percentage change in forest cover, display positive results (less deforestation) and lower p-values for every regression. The FRD regression for the Legal Amazon stands out with a high decrease in deforestation and a robust significance within the 10 percent level. Increased FPM transfers is associated on average with a 9.4 percent increase in forest cover.

For the regressions on transition away from forest, the results are different. They indicate a positive relationship between deforestation and FPM transfers, and the p-values are generally lower. For the Legal Amazon, the ITTC (SRD) effect is 1066 hectares, while the treatment (FRD) effect is 5983 hectares. The robust bias-corrected p-values for are 0.105 and 0.058, respectively. The effects are smaller for the Brazil sample, but the significance is larger. The average effect of being above a population threshold (SRD) is 357 hectares increased deforestation, while the average effect of receiving more FPM transfers (FRD) is 2205 hectares increased deforestation—both significant within the 5 percent level.

Table 3: *Entire sample, sharp and fuzzy RD design*

	Change in forest cover		*Percentage change in forest cover*		*Transition away from forest*	
	Legal	Brazil	Legal	Brazil	Legal	Brazil
SRD	163.05	4.5684	1.8778	.50843	1066.3	357.3**
	(0.840)	(0.975)	(0.095)	(0.150)	(0.062)	(0.024)
	[0.867]	[0.979]	[0.156]	[0.226]	[0.105]	[0.043]
FRD	871.96	-171.15	9.4491*	1.1626	5983.1*	2204.5**
	(0.817)	(0.787)	(0.035)	(0.156)	(0.046)	(0.012)
	[0.837]	[0.812]	[0.086]	[0.223]	[0.058]	[0.019]
Observations	8941	54,285	8941	54,285	8941	54,285

Notes: Each cell reports the effect on deforestation of being just above a cut-off at the different thresholds. SRD is sharp regression discontinuity design; FRD is fuzzy regression continuity design. `rdrobust` has been used to estimate the regressions and calculate the MSE-optimal bandwidth level, controlling for total forest cover, year dummies and threshold dummies. Standard errors clustered at the municipality level. Bias corrected p-values in parentheses, robust bias-corrected p-values in brackets. For change and percentage change in forest cover, a positive value indicates less deforestation for municipalities just above the threshold. For transition to forest, a positive value indicates more deforestation for municipalities just above the threshold.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

The figures presented in this table corresponds to the plotted results above. So far, the results show:

1. No evidence that FPM transfers increase net deforestation
2. Some evidence that FPM transfers decrease net deforestation (in percentage)
3. Evidence that FPM transfers increase gross deforestation

In the following, we will explore our data further with threshold-by-threshold analyses.

6.1.2 RD plots of pooled results

We begin our exploration of the smaller pooled samples by graphically analyzing the discontinuities. The plots of the pooled samples are shown in A.9, A.10, and A.11. For the plots of all three different measures, there seem to be several pooled thresholds that exhibit discontinuities at the cutoffs. There is also a noticeable trend of more negative discontinuities for the first period and positive jumps for the second period. To investigate further, we will discuss the numerical results from our analyses.

6.1.3 Legal Amazon: change in forest cover in hectares and percentage

Of the following tables, the SRD table for the Legal Amazon with threshold-by-threshold analysis (Table 4) exhibits the most significant results. For the years 1997–2006, thresholds 2, 4, 5 and 6 displays negative change in forest cover in a magnitude of 9298, 4917, 9476 and

21,731 hectares, respectively, for municipalities above the cut-off, all within the five percent significance level, except for threshold 2. The sixth threshold is also significant within the one percent level. In percentage of average total cover, these estimates correspond, respectively, to a 2.2, 0.6, 1.0 and 2.0 percent higher forest loss[7]. Thresholds 1, 3 and 7 are not significant. For the years 2008–2016, the results are insignificant across the board, but threshold 5 indicates a positive effect of being above the cut-off (i.e., less deforestation) within the 10 percent significance level.

The columns to the right in the table are the same regressions as before, but with percentage change in forest cover instead of change measured in acres. Threshold 3 in the first period is significant and positive. Threshold 3 in the second period, and threshold 6 in both periods are significant within the 10 percent level, albeit with mixed effects. For the period 1997–2006, being in the population interval corresponding to the third threshold, for the municipalities around the cut-off, the difference in change in forest cover is 9.62 percent, i.e., the municipalities just above the cut-off experienced less deforestation. For threshold 6, the opposite is true; the difference around the cut-off is –5.39 percent. In the next period, threshold 3 is now negative (–2.81 percent), while threshold 6 is positive (4.04 percent). For the pooled thresholds, the estimates in hectares for the first period are negative, but insignificant, while in percentage thresholds 1–3 is positive (3.15 percent) and significant and 4–7 negative but insignificant. For the second period, the estimates are positive overall, and thresholds 4–7 are significant within the 10 percent level.

Compared to the previous section, there is evidence that indicates a positive relationship between net deforestation (in hectares) and FPM transfers for municipalities in the Legal Amazon. However, they are concentrated in the first period, and only some thresholds are significant. For percentage change, the results are mixed.

6.1.4 Brazil: change in forest cover in hectares and percentage

The results for the Brazil sample are displayed in Table 5 below. For the first period, thresholds 2 and 4 are negative and significant for changes in forest cover, with magnitudes of -2206 and -1554 hectares, respectively. Also significant in the same period but with percentage change in forest cover is threshold 7, where there is a 3.39 percent increase in forest cover for municipalities above the cut-off. For the second period, none of the regressions with change in forest cover are significant; the pooled thresholds are positive, while there are mixed results for

[7] Calculated by dividing the results on the average total forest cover for the respective population intervals.

the individual thresholds. The pooled thresholds for percentage change in forest cover are positive and significant within the 10 and 5 percent level. For the individual thresholds, only threshold 4 is significant, showing an increase in forest coverage by 2.9 percent.

Like in Table 4, the results in Table 5 indicate a positive relationship between net deforestation and FPM transfers. However, for percentage change we only find a negative relationship between net deforestation and FPM transfers. Furthermore, positive relationships are found in the first period, while negative relationships are shown in the second.

Table 4: *Legal Amazon, sharp RD design*

	Deforestation (change in forest cover)			
	97–06 (Hectares)	08–16 (Hectares)	97–06 (%)	08–16 (%)
Thresholds 1–3	-184.98	96.321	3.153**	11.482
	(0.913)	(0.932)	(0.010)	(0.173)
	[0.929]	[0.945]	[0.025]	[0.234]
Thresholds 4–7	-3345.2	5068.1	-1.9275	4.701*
	(0.161)	(0.052)	(0.071)	(0.075)
	[0.227]	[0.101]	[0.134]	[0.091]
Threshold 1	2110.7	-500.64	3.302	28.439
	(0.408)	(0.742)	(0.057)	(0.118)
	[0.498]	[0.783]	[0.109]	[0.162]
Threshold 2	-9298.4*	568.18	-1.945	1.111
	(0.042)	(0.729)	(0.239)	(0.782)
	[0.079]	[0.773]	[0.315]	[0.800]
Threshold 3	-2568.5	-469.76	9.622***	-2.814*
	(0.490)	(0.836)	(0.002)	(0.041)
	[0.552]	[0.861]	[0.004]	[0.081]
Threshold 4	-4916.8**	-367.48	-1.292	7.227
	(0.010)	(0.938)	(0.510)	(0.128)
	[0.023]	[0.948]	[0.592]	[0.151]
Threshold 5	-9474.5**	8456.3*	.4430	7.601
	(0.019)	(0.029)	(0.765)	(0.144)
	[0.047]	[0.067]	[0.803]	[0.079]
Threshold 6	-21731***	1472.8	-5.391*	4.044*
	(0.000)	(0.663)	(0.038)	(0.024)
	[0.002]	[0.719]	[0.084]	[0.079]
Threshold 7	7607.9	7384.6	.5022	-2.405
	(0.411)	(0.481)	(0.844)	(0.283)
	[0.475]	[0.588]	[0.870]	[0.415]
Observations	4,879	4,062	4,879	4,062

Notes: SRD regressions categorized in the columns by period and whether the change in forest cover for a municipality is measured in acres or percent of total cover. Each cell reports the bias-corrected effect on deforestation of being just above a cut-off at the different thresholds. `rdrobust` has been used to estimate the regressions and calculate the MSE-optimal bandwidth level, controlling for total forest cover and year dummies. Standard errors clustered at the municipality level. Bias-corrected p-values in parentheses; robust bias-corrected p-values in brackets.

* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 5: *Brazil, sharp RD design*

	Deforestation (change in forest cover)			
	97–06 (Hectares)	08–16 (Hectares)	97–06 (%)	08–16 (%)
Thresholds 1–3	-843.72*	57.602	-.7527	3.1454*
	(0.059)	(0.795)	(0.103)	(0.024)
	[0.091]	[0.826]	[0.174]	[0.057]
Thresholds 4–7	-601.12	872.95	.12545	1.9972**
	(0.235)	(0.062)	(0.828)	(0.009)
	[0.307]	[0.114]	[0.855]	[0.022]
Threshold 1	379.06	205.39	-.64882	6.8898
	(0.477)	(0.560)	(0.361)	(0.065)
	[0.570]	[0.609]	[0.444]	[0.120]
Threshold 2	-2206**	-47.627	-1.4657	1.3716
	(0.008)	(0.896)	(0.126)	(0.283)
	[0.023]	[0.913]	[0.204]	[0.319]
Threshold 3	-909.68	-249.6	.34874	-.04908
	(0.290)	(0.614)	(0.777)	(0.974)
	[0.347]	[0.675]	[0.807]	[0.978]
Threshold 4	-1554.2**	-581.84	-.41161	2.9001**
	(0.004)	(0.452)	(0.681)	(0.020)
	[0.011]	[0.511]	[0.732]	[0.041]
Threshold 5	-799.73	1233.1	.53659	1.256
	(0.438)	(0.159)	(0.665)	(0.421)
	[0.509]	[0.235]	[0.714]	[0.464]
Threshold 6	-2204.6	1466.5	-.37255	1.8726
	(0.070)	(0.128)	(0.791)	(0.283)
	[0.114]	[0.181]	[0.827]	[0.361]
Threshold 7	3989.4	632.16	3.3875**	-1.0038
	(0.138)	(0.712)	(0.007)	(0.477)
	[0.201]	[0.749]	[0.018]	[0.545]
Observations	29,818	24,467	29,818	24,467

Notes: SRD regressions categorized in the columns by period and whether the change in forest cover for a municipality is measured in acres or percent of total cover. Each cell reports the bias-corrected effect on deforestation of being just above a cut-off at the different thresholds. `rdrobust` has been used to estimate the regressions and calculate the MSE-optimal bandwidth level, controlling for total forest cover and year dummies. Standard errors clustered at the municipality level. Bias-corrected p-values in parentheses; robust bias-corrected p-values in brackets.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 6: *Transition dataset for the Legal Amazon and Brazil, sharp RD design*

	Deforestation (transition from forest)			
	Legal (97–06)	Legal (08–16)	Brazil (97–06)	Brazil (08–16)
Thresholds 1–3	1294.795	4666.46**	468.108	1151.43***
	(0.414)	(0.006)	(0.180)	(0.002)
	[0.487]	[0.015]	[0.250]	[0.006]
Thresholds 4–7	2213.083	2033.942	439.186	376.262
	(0.234)	(0.289)	(0.266)	(0.332)
	[0.300]	[0.365]	[0.328]	[0.387]
Threshold 1	1857.210	3936.998	493.687	688.877
	(0.443)	(0.099)	(0.316)	(0.108)
	[0.519]	[0.145]	[0.405]	[0.176]
Threshold 2	-151.621	6170***	54.422	1749.3***
	(0.956)	(0.003)	(0.915)	(0.001)
	[0.963]	[0.008]	[0.928]	[0.004]
Threshold 3	2515.886	5272.14	1294.5*	1741.6**
	(0.125)	(0.066)	(0.056)	(0.002)
	[0.376]	[0.107]	[0.087]	[0.004]
Threshold 4	1593.02**	2207.966	447.585	217.531
	(0.010)	(0.476)	(0.151)	(0.716)
	[0.023]	[0.543]	[0.196]	[0.746]
Threshold 5	4639.702	2797.756	198.602	600.279
	(0.317)	(0.429)	(0.827)	(0.366)
	[0.387]	[0.456]	[0.847]	[0.432]
Threshold 6	15826**	3257.88	2198.33*	5.667
	(0.001)	(0.385)	(0.055)	(0.993)
	[0.013]	[0.453]	[0.096]	[0.994]
Threshold 7	-2384.162	2598.986	-1529.916	1923.4**
	(0.764)	(0.595)	(0.355)	(0.028)
	[0.804]	[0.651]	[0.435]	[0.038]
Observations	4,292	3,604	25,405	20,861

Notes: SRD regressions categorized in the columns by period and whether the sample is taken from Brazil as a whole or restricted to the Legal Amazon. Each cell reports the effect on deforestation of being just above a cut-off at the different thresholds. `rdrobust` has been used to estimate the regressions and calculate the optimal bandwidth level, controlling for total forest cover and year dummies. Standard errors clustered at the municipality level. P-values in parentheses. The greater the positive value, the greater the transition away from natural forest.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

6.1.5 Brazil and Legal Amazon: deforestation

In Table 6, we see that, unlike the results we obtained from the coverage dataset, we find a greater jump in deforestation during the second period (2008–2016) for municipalities in the pooled and individual thresholds. For the Legal Amazon there are significant positive effects in the fourth (1593 hectares) and sixth (15,826 hectares) thresholds in the first period, and in the pooled thresholds 1–3 (4667 hectares) and the second threshold (6170 hectares). For Brazil as a whole, the significant thresholds are the pooled thresholds 1–3 (1151 hectares), the second (1749 hectares), the third (1742 hectares) and the seventh (1923 hectares), all in the second period. The other regressions are insignificant, and all are positive except for threshold 2 in the first period in the Legal Amazon and threshold 7 in the first period in both samples.

These results are consistent with what we found for the transition dataset in Table 3. However, the deforestation effect seems to be larger in the second period and concentrated to the lower populated thresholds.

6.1.6 Non-Legal Amazon

We also include RD results for the non-Legal Amazon for completeness. We have done the same analysis as in Table 4 and Table 5 for a non-Legal Amazon sample. The results are displayed in Table 9 in appendix A.8.

The estimates for coverage change measured in hectares display no trend and little statistical significance. However, estimate for percentage coverage change show similarities with our other analyses. In other words, the estimates for the 1997–2006 period are mostly negative while the estimate for the 2008–2016 period are all positive. Nevertheless, the results are not statistically significant except for thresholds 1 at the 10 percent level and threshold 7 at the 5 percent level. Moreover, results for threshold 7 have to be carefully considered when taken alone as there are generally much fewer observations and the transfers received at the larger thresholds exhibit more variability. In short, our results for municipalities in the non-Legal Amazon are consistent with our findings for both the Legal Amazon and Brazil.

6.2 Discussion

In this section we will interpret the results described above and suggest some reasons for why we found what we did. Beginning with our findings for the entire sample in table 3, there appears to be conflicting results among our measures of forest cover change, percentage cover change and forest transition. While the jumps at the cut-offs for change measured either in hectares or percentage are mostly positive, the coefficients of the size of forest transition, which we treat as a measure of gross deforestation, are not just positive, but statistically significant. Even if we were to disregard the insignificant results for changes in forest cover, the 9.44 percent increase in forest cover, which is significant at the five percent level, does not reflect the 5983 hectares conversion of natural forest to other means. In other words, we find either a growth in forest cover or a reduction in deforestation, but also an increase in forest conversion for municipalities at the cut-offs. Referring to the results in the second period in Table 4, Table 5 and Table 6, these differences between change and transition persist whether change is recorded in hectares or percentage. While results for coverage change in hectares in the second period in Table 4 and Table 5 are largely insignificant, the positive changes in percentage are statistically significant. There are several possibilities that could explain this result.

First, compliance with regulations like having landowners in the Amazon apportioning 50-80 percent of their property as protected rainforest or for forest recovery that are stipulated within the 1965 forest code only picked up from 2009 (Smith, 2016; Nepstad et al., 2009). Furthermore, even though Brazil already possessed good satellite imaging technology, the effectiveness of using satellite images to identify illegal activity was strengthened only after a federal law requiring land registration was enacted in 2012 (Smith, 2016). Moreover, the process from identification of illegality to punishment is not just painstaking due to Brazil's decentralization, but the landowner is still able to set aside part of their land for legal deforestation. While landowners in the Amazon were able to clear 20 percent of their land legally, landowners outside the Amazon could clear 80 percent of their land legally even after the limits were raised in 1996 (Nepstad et al., 2014). It is therefore possible that the levels of deforestation had not decreased as quickly as forest recovery activity increased. This could result in a positive net change effect but similarly sized transitions.

Second, our outcome variables were derived from different datasets with possibly different classification methods. If we were to only consider results in Table 4 and Table 5 for percentage change and coverage change that are from the same dataset, there are fewer differences in magnitudes and signs of the estimates. Moreover, where differences exist, the findings were not statistically significant. As an example, we find that there is a -185ha dip but a 3.15 percent jump for municipalities in thresholds 1–3 in the first period. However, the -185 hectares drop is not statistically significant. Taking into account some of the factors like MapBiomas' 0.5ha minimum threshold for transition that could change in the future and others that we identify in A.5, we recommend that future researchers revisit the transitions dataset.

More importantly, we find significant results for both analyses of coverage change and transition in the first period. While we might have expected this since we are aware of the high levels of deforestation in the 1990s and early 2000s, the negative coverage changes measured in percentage and hectares, together with the positive jumps in conversion of forests suggests an answer to our research question, albeit for the first period. In other words, additional intergovernmental transfers increase levels of deforestation in the period 1997–2006.

With regards to the second period 2008–2016, if we consider net deforestation (coverage change) and gross deforestation (forest conversion) separately, we reach unexpected conclusions. In fact, there are statistically significant positive jumps in pooled thresholds and several individual thresholds as shown in Table 4 and Table 5. For example, there is a 3.15 percent jump for municipalities in thresholds 1–3 and a 2 percent increase for municipalities in thresholds 4–7 in Brazil. In addition, there are many other positive, but insignificant jumps in coverage change in hectares or percentage in the second period. This seems to suggest some evidence of an alternative response to our research question: additional intergovernmental transfers reduce levels of deforestation in the period 2008–2016. These results might also indicate an increase in forest regrowth rather than just a dip in deforestation. While this might be an unexpected finding, we do acknowledge the differences between change and transition results and discuss several possible explanations above. There is an explanation for the differences between periods if we return to Brazil's economic history in Section 2.5. Bhattarai and Hammig (2001) contend that there is an environmental Kuznets Curve relationship between income and deforestation. In other words, while deforestation with little regrowth is more prevalent in impoverished areas, this is likely to reduce as incomes increase. Moreover, the propensity to replant or to create businesses that support the preservation of forests might even result in reforestation (Bhattarai & Hammig, 2001). As previously mentioned, economic conditions in Brazil has improved over the years as Brazil left its hyperinflation years behind, discovered natural resources and became a dominant force in several commodities markets. In addition, successful policies like the CGU that led to higher fines and lower corruption are reasons for the decrease in deforestation (Barbier, 2005; Hargrave & Kis-katos, 2013). Assunção et al.'s (2015) finding that conservation policies reduce deforestation also raises the importance of Brazil's involvement in comprehensive environmental efforts like REDD+.

Furthermore, the findings of additional intergovernmental transfers leading to higher levels of observed corruption are for the mayoral periods 2001-2005 and 2005-2009 (Brollo et al., 2013). Similar studies on mayoral elections after 2009 in the future might be able to provide some explanations for our findings. Moreover, we believe that the continuation of audits, coupled with conservation policies might be reasons for this difference between periods. Perhaps, it might be possible that studies that show misuse and corruption (Gadenne, 2006;

Caselli & Michaels, 2013; Monteiro & Ferraz, 2010) mentioned in Section 2.3 could find evidence of better use of governmental transfers if they were conducted on more recent periods.

Having said that, there are many instances of insignificant regressions in both pooled and individual thresholds. In addition, even though negative results tend to be located in the first period and positive results located in the second period, we also find thresholds that do not follow the general trends. While most of these conflicting results are insignificant, we recognize that there is a degree of ambiguity in our findings. Nevertheless, we would like to suggest several other possibilities that have contributed to these differences. For instance, a likely explanation for less significant results in the extended Brazilian analysis is that the Legal Amazon is the area containing most of the rainforest in the country. In addition, this is the forest area with the highest conservation priority, increasing the likelihood that occurring deforestation is illegal. In other words, we would expect a stronger relationship between deforestation and corruption in the Legal Amazon than in other parts of the country. Adding the other Brazilian states in the analysis would, therefore, make it more difficult to find a statistically significant relationship because of heterogeneity. While we appreciate the benefits of working with the MapBiomas database that allows for analyses of more than 3000 municipalities in our extended sample and almost 600 municipalities in the Legal Amazon, there are many other ways to dividing and pooling than what we have considered in Section 4.5.

6.3 Robustness

6.3.1 Sensitivity to bandwidth choice

In Table 19 and Table 20, we test the sensitivity of our results to bandwidth choice for our analysis of the Legal Amazon. This is an important falsification test because adding or removing observations either closer to the cut-offs or nearer to the endpoints may affect the results of our analyses. Therefore, even though we have been provided with optimal bandwidths for each of our RD tests, it is important to investigate bandwidths that are larger and smaller than the recommended bandwidths. In other words, our results will be less robust if they are significantly altered when other specific bandwidths are used (Cattaneo et al., 2018).

We have chosen to test three alternate bandwidths for both pooled thresholds and individual thresholds 3, 5 and 6. These individual thresholds were chosen as each of them exhibited at least a 5 percent level of statistical significance. With regards to alternative bandwidths for deforestation measured in hectares, the estimates from the first period show a general increase as bandwidths increase. For the second period, the estimates do not change in a similar manner and they vary in a much smaller margin.

With regards to alternative bandwidths for percentage coverage change in the bottom half of the table, there is mostly a decrease in the magnitude of percentage change as the bandwidths increase. For example, there is an increase in percentage coverage change at the cut-offs in the pooled thresholds 4-7 for the first period 1997-2006 from a loss of 1.63 percent in coverage to a loss of 1.03 percent in coverage. It is important to note that the results for percentage coverage change in threshold 3 are significant at the 1 percent and 5 percent level at 600 and 900 persons away from the cut-off respectively. In the second period for threshold 3, the estimates for percentage coverage change switch from a loss of 1.2 percent to a gain of 0.18 percent to a loss of 1 percent as the bandwidth increased from 200 to 900 persons away from the cut-off. Both the significance and the switch from negative to positive between bandwidths indicate a sensitivity in the choice of bandwidths and prompts us to disregard the significant values that we have found for this particular threshold. That said, the estimates for the rest of the alternative bandwidth tests keep their positive or negative signs within their thresholds. Furthermore, the estimates for percentage coverage change mostly vary within a range of ±0.5 percent and have high p-values.

For alternative bandwidth tests conducted on forest transition shown in Table 20, pooled thresholds 1-7 and 1-3 that show significance at the 5% level in the second period also produced significant positive jumps with some difference in magnitude. That said, there are some thresholds like threshold 2 in the second period and threshold 3 in the first period that produce varying estimates. Even though these thresholds did not produce significant results in these periods, the sensitivity has to be accounted for. Taken together, we propose that our results have some sensitivity to the choice of bandwidths. Finally, we would like to reiterate the importance of bandwidth sensitivity tests and the difficulty in choosing an ideal bandwidth. While we rely on the optimal bandwidths produced by the statistical package, we recognize that the estimated regression discontinuity effects will either be biased for much larger bandwidths or have high variance if they are much smaller than the optimal bandwidths (Cattaneo et al., 2018).

6.3.2 Sensitivity to alternative cut-offs

The purpose of running tests on placebo cut-offs is to check for discontinuities at points away from the true cut-off value. If discontinuities are detected, the underlying assumption of continuity for untreated observations around the cut-off is called into question (Cattaneo et al., 2018). We implement this test by first visualizing the treatment effects in Figure 10. Each plot in Figure 10 shows the effects at cut-offs lower than the true cut-off in red, effects at higher cut-offs in green, the effect at the true cut-off in blue and the confidence intervals as the shaded regions. For both Brazil and the Legal Amazon, we find that, while there are similarly sized effects at points on either side of the true cut-off, the confidence intervals of most of these alternative cut-offs include 0. In other words, these plots show to some extent that potential problems arising from significant effects at artificial cut-offs are not severe. Subsequently, we run analyses on artificial cut-offs at approximately 5, 10 and 15 percent away for pooled thresholds. Moreover, we also present the results of tests for thresholds 3, 5 and 7 that have shown significant results in our main tests in Table 12 and Table 13.

Table 10 and Table 11 show the results of alternative cut-off analyses for pooled thresholds from 1997–2006 and from 2008–2016. Except for the pooled thresholds 1–3 in the years from 1997–2006 at cut-off -50 that is significant at the 10 percent level, the p-values for the other pooled thresholds are high and the absolute estimates are often higher than the estimates at the true cut-off. The p-values for thresholds 1, 2 and 3 are also high, which indicates that the tests are not statistically significant. Moreover, like in the placebo plots in Figure 10, all the confidence intervals in these tests include zero.

Tables 12–18 show the results of alternative cut-off analyses for our transition dataset. We include results for pooled thresholds and individual thresholds that have exhibited statistical significance in Table 6. We find no statistically significant results in the analyses done on pooled thresholds. However, among the tables for individual thresholds, we find some significance in thresholds 6 and 7 in the first period and threshold 4, 6 and 7 in the second period. This suggests a pattern of weakening robustness for our samples in higher thresholds and it could be due to the lack of observations. Nevertheless, we only observe these sensitivities in single thresholds with few observations. Therefore, we believe that the outcomes of interest are not discontinuous at these placebo cut-offs at least for our samples in the smaller thresholds. At the same time, we acknowledge the weakness of our results for samples in large individual thresholds. Taken together, these placebo cutoff tests suggest that we should be cautious when making conclusions on analyses of single thresholds.

7. CONCLUSION

7.1 Summary

The purpose of this study was to investigate the relationship between intergovernmental (FPM) transfers and deforestation in Brazil. We find some evidence for an increase in deforestation for higher FPM transfers in the years 1997–2006. Corruption may be a possible explanation for this link (cf. Brollo et al., 2013; Cisneros et al., 2013; Pailler, 2018; Mendes and Porto Jr., 2012). There is also some evidence for this relationship between increased transfers and deforestation for the years 2008–2016. However, the results in this period are more ambiguous. While our dataset on gross deforestation shows an increase in deforestation for higher FPM transfers, the coverage dataset measured in percentage suggests the opposite effect. A possible explanation for this finding could be that reforestation efforts are strengthened by increased governmental revenues. This indicates that intergovernmental transfers can affect deforestation differently, through different channels, and, consequently, have both a negative and positive effect on forest cover. Nevertheless, our results are mixed, and the data do not support a simple relationship between the two variables. Moreover, we hesitate to make any definitive conclusions after accounting for some weakness detected in Section 6.3.

7.2 Limitations of the study

In addition to our concerns about the accuracy and quality of data in A.5, there is still no consensus on what the best method of measuring deforestation is. As such, the issue of accurately identifying primary or secondary forest loss, for example, could be important or insignificant. Moreover, developments in other databases like the Trase database might allow us to validate parts of the transitions dataset in the future. Presently, the Trase database contains supply chain data for products like corn and beef, but the availability of data differs among products. If the Trase database expands its datasets on Brazilian soy trade to years before 2003 and include municipal level data, this can be used to approximate and validate transition from forest to agricultural in the MapBiomas transitions dataset. Similarly, the inclusion of Brazilian beef trade data for years before 2010 with state and municipal identifiers can be helpful in validating conversion to land use for pastures (Trase, 2019).

Another limitation to our research is the importance of other measures that might significantly impact deforestation as well. For example, education data from the National Institute on Educational Studies and Research (INEP) can be used to test for significant effects especially on the second period of our analysis. Similar development indicators could also be used to substantiate factors like the environmental Kuznets Curve relationship.

7.3 Recommendations for future research

The exceptional detail of the MapBiomas database poses a further challenge for us to select the best of our myriad ideas for further research into this topic. Nonetheless, we present some of them that we deem most interesting and valuable. First, the trends that we have found after pooling periods and years prompts a return to a finer division of the dataset. While in previous studies the separation was perhaps due to an absence of data, this limitation might have been alleviated. For example, one could continue to study the effects of mayoral elections by pooling data by mayoral terms in more recent periods. Second, while focusing on areas within the Amazon, like in previous studies, might seem like the obvious progression, we are interested to see a combined study of regions in the Amazon that are not necessarily located in Brazil. Moreover, other forms of econometric methods like a Geographic Regression Discontinuity design can be used to investigate the effects of national borders on otherwise similar geographies. This would, however, require data from other countries that might not be available.

7.4 External validity

In principle, intergovernmental transfers shares properties with other types of windfall revenues, like natural resources and foreign aid. The fact that increased governmental transfers might both have a benign and adverse effect on deforestation, as indicated in this study, should be of interest to policy makers and organizations. However, on account of the degree of uncertainty associated with the results and the specificity of the setting from which they arise, we hesitate to generalize the findings to other countries and situations. Further research on the relationship between governmental revenues and deforestation is needed.

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APPENDICES

A.1 FPM transfers

Table 7: *Summary statistics for FPM transfers according to the pre-determined coefficients*

		1997–2006			2008–2016		
Population interval	FPM coeffi-cients	FPM transfers	Theoretical FPM transfers	Obs.	FPM transfers	Theoretical FPM transfers	Obs.
6793–10,189	0.6	1,654,175 (574,651.3)	1,562,366 (537,025.9)	7360	4,801,816 (1,124,292)	4,761,965 (1111431)	5404
10,189–13,584	0.8	2,115,980 (738,231.2)	2,072,662 (708,430.2)	5642	6,451,061 (1,485,397)	6,444,349 (1,455,005)	4714
13,585–16,980	1	2,636,956 (937,231.3)	2,652,484 (916,523)	4178	8,033,118 (1,887,962)	8,058,189 (1,868,954)	3337
16,981–23,772	1.2	3,149,616 (1,130,736)	3,197,648 (1,090,273)	5512	9,702,252 (2,216,973)	9,693,626 (2,188,637)	4706
23,773–30,564	1.4	3,689,121 (1,305,860)	3,735,861 (1,262,317)	3214	11,321,976 (2,555,348)	11,344,717 (2,533,491)	2646
30,565–37,356	1.6	4,194,893 (1,503,126)	4,283,024 (1,460,659)	1894	12,920,818 (2,927,833)	12,949,171 (2,889,432)	1780
37,357–44,148	1.8	4,720,542 (1,635,489)	4,827,721 (1,610,076)	1242	14,356,541 (3,261,220)	14,397,027 (3,239,076)	1143
44,149–50,940	2	5,117,950 (1,850,021)	5,298,644 (1,821,822)	776	16,005,039 (3,613,782)	16,068,817 (3,574,517)	737

Notes: Cells display average FPM transfers and average theoretical FPM transfers over the two periods. FPM coefficients are from Brollo et al. (2013). Standard deviation in parentheses.

A.2 The treatment variable in the FRD approach

To create the actual treatment variable, we follow Brollo et al. (2013) and make use of the data on actual FPM transfers and the allocation rule in equation A1 below to obtain the theoretical FPM transfers for each municipality. Municipalities in the same state and population bracket are allocated the same amount of theoretical transfers. Based on this, we identify the municipalities with a certain amount of deviation between theoretical and actual FPM transfers. The municipalities below a cut-off which received an amount of FPM transfers closer to the municipalities (belonging to the same state) above the cut-off are considered treated. The same logic applies to the municipalities above the cut-offs that received less than they should have.

$$FPM_i^k = \frac{FPM_k \lambda_i}{\sum_{i \in k} \lambda_i} \tag{A1}$$

Equation A1 is used to calculate how much FPM transfers each municipality is entitled to receive in a given year. FPM_i^k is the theoretical FPM transfers for municipality i in state k. FPM_k is the total amount of transfers to state k. λ_i is the coefficient for municipality i, and it is dependent on the population size of the municipality. Actual values for λ_i and calculated theoretical FPM transfers are given in Table 7 in appendix A1.

A.3 Theoretical and scaled FPM transfers

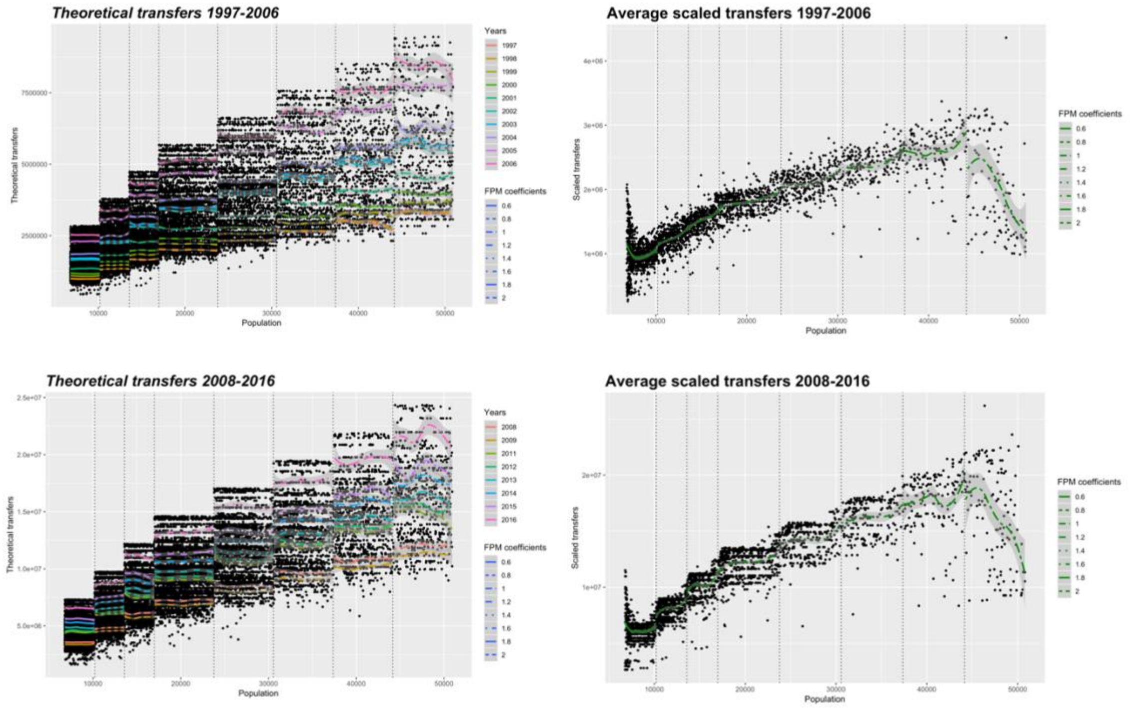


Figure 7: *Plots of theoretical transfers against population (left). Years are differentiated by color and points within each threshold are smoothed. Plots of scaled transfers averaged over the time periods against population. Source: own, created with data from the Brazilian treasury and with the FPM revenue-sharing mechanism. The World Bank deflator was used to scale the transfers.*

A.4 Deforestation maps of Brazil

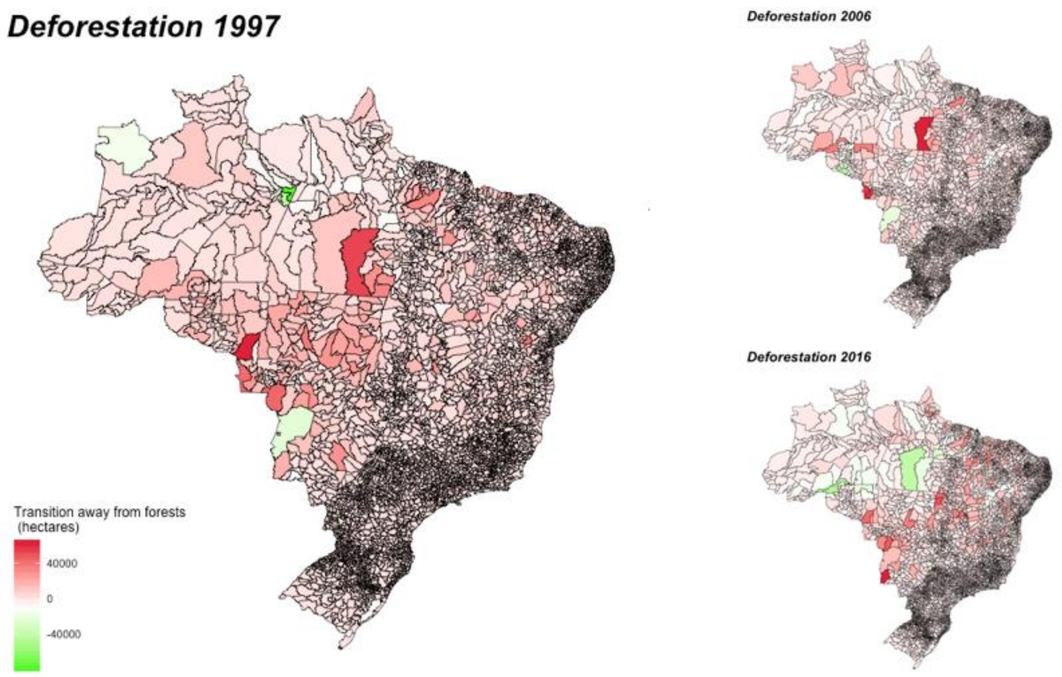


Figure 8: *Map of deforestation in Brazil. Red indicates the total area of natural forest that turned into land for other uses. Source: own, created with data from MapBiomas.*

A.5 Accuracy of land cover and transition data

MapBiomas assesses the accuracy of its datasets by having three technicians evaluate a sample of pixels annually and they decide on the pixel classification only if at least two of the three technicians agree. Thereafter, they compare their classification against reference maps from databases like Globeland30 and TerraClass. The overall accuracy of the MapBiomas database hovers around 80 percent for each biome and land use category, and yearly estimates are reported to have around 5 percent error (Project MapBiomas. 2018). MapBiomas also reports on the quality of their mosaics that are compiled using different Landsat observations. Due to issues like heavy cloud coverage, the “available number of cloudless observations per pixel for selected year”, which is their standard of measurement, varies from 0–23. The proportion of mosaics that MapBiomas considers good quality – composed of more than 6 observations per pixel, has steadily increased from 30 percent in 1997 to 70 percent in 2016.

In addition, MapBiomas sets the minimum area to be considered a transition in their database as 0.5 hectares. They inform the database user that a reduction in the minimum transition area, in other words an increase in specificity, might result in a significantly different dataset. With the increase in quality of Landsat mosaics and the release of future corrections to their databases, other analyses can be conducted if this minimum changes so as to more accurately calculate transitions.

A.6 Density tests

Table 8: *McCrary density tests for pooled/individual thresholds and years.*

	Discontinuity p-values	
	Thresholds 1-3	Thresholds 4-7
1997-2006	0.064*	0.706
2008-2016	0.782	0.412
1997	0.045**	0.620
1998	0.024**	0.845
1999	0.226	0.242
2000	0.631	0.729
2001	0.235	0.557
2002	0.435	0.719
2003	0.717	0.631
2004	0.397	0.163
2005	0.952	0.154
2006	0.594	0.229
2008	0.815	0.038
2009	0.310	0.347
2011	0.046**	0.279
2012	0.822	0.061
2013	0.373	0.235
2014	0.085*	0.237
2015	0.086*	0.351
2016	0.552	0.477
	1997-2006	2008-2016
Threshold 1	0.156	0.767
Threshold 2	0.248	0.281
Threshold 3	0.492	0.468
Threshold 4	0.255	0.369
Threshold 5	0.488	0.430
Threshold 6	0.123	0.890
Threshold 7	0.259	0.486

Notes: To ensure continuity around the cut-off of the regression discontinuity design, McCrary's density tests are run. The table above provides the p-values of the continuities around the cut-off. If the tests are significant, then the null-hypothesis of there being no discontinuity is rejected. This implies that there is manipulation around the cut-offs and RD should be avoided.
** significant at the 5% level
*** significant at the 1% level

A.7 Density histograms

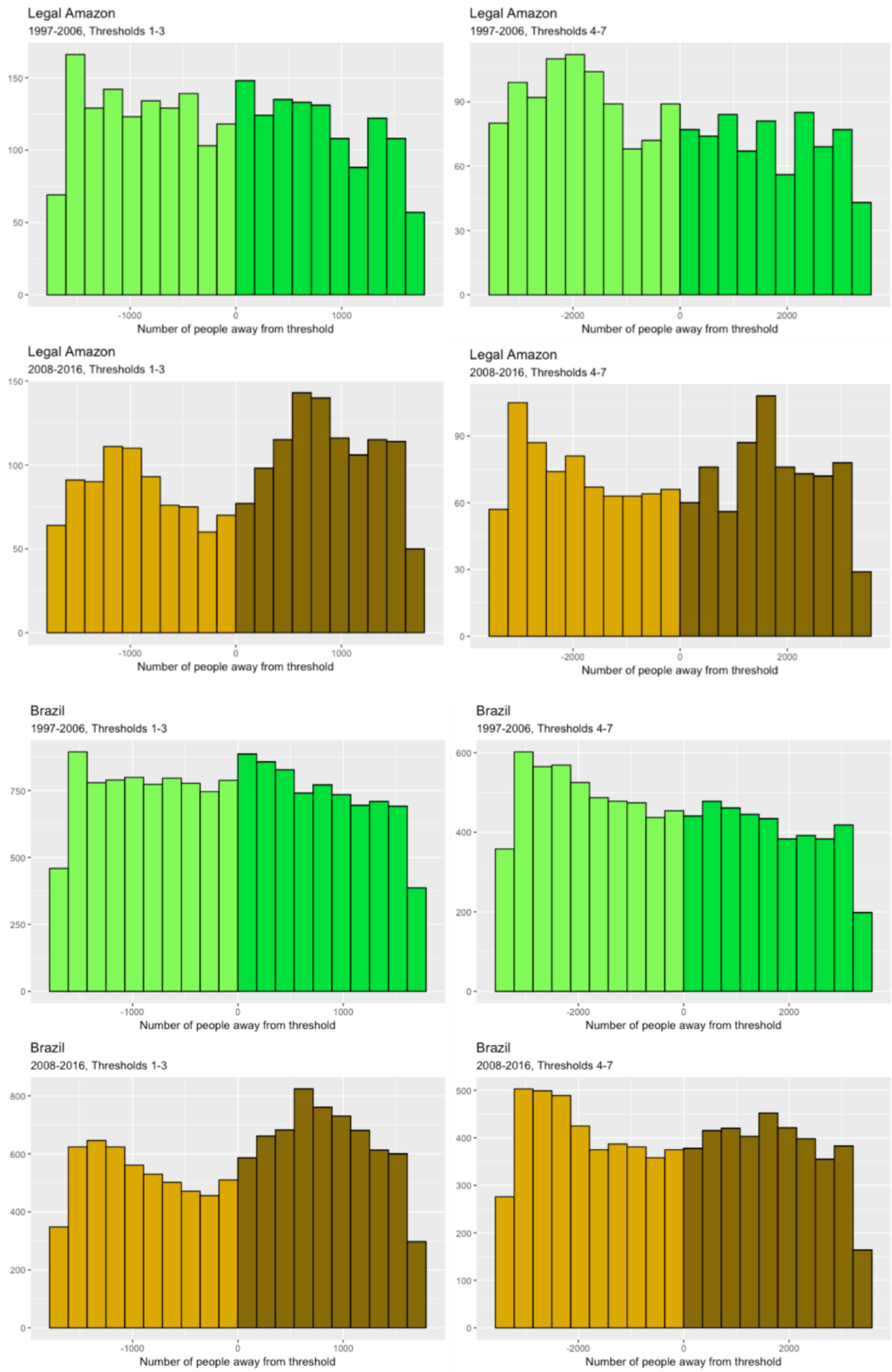


Figure 9: *Density histograms for Legal Amazon (top 4 plots) and Brazil (bottom 4 plots). Alternative method to McCrary density plots to observe if there is any evidence of manipulation at the cut-offs.*

A.8 Results for non-Legal Amazon

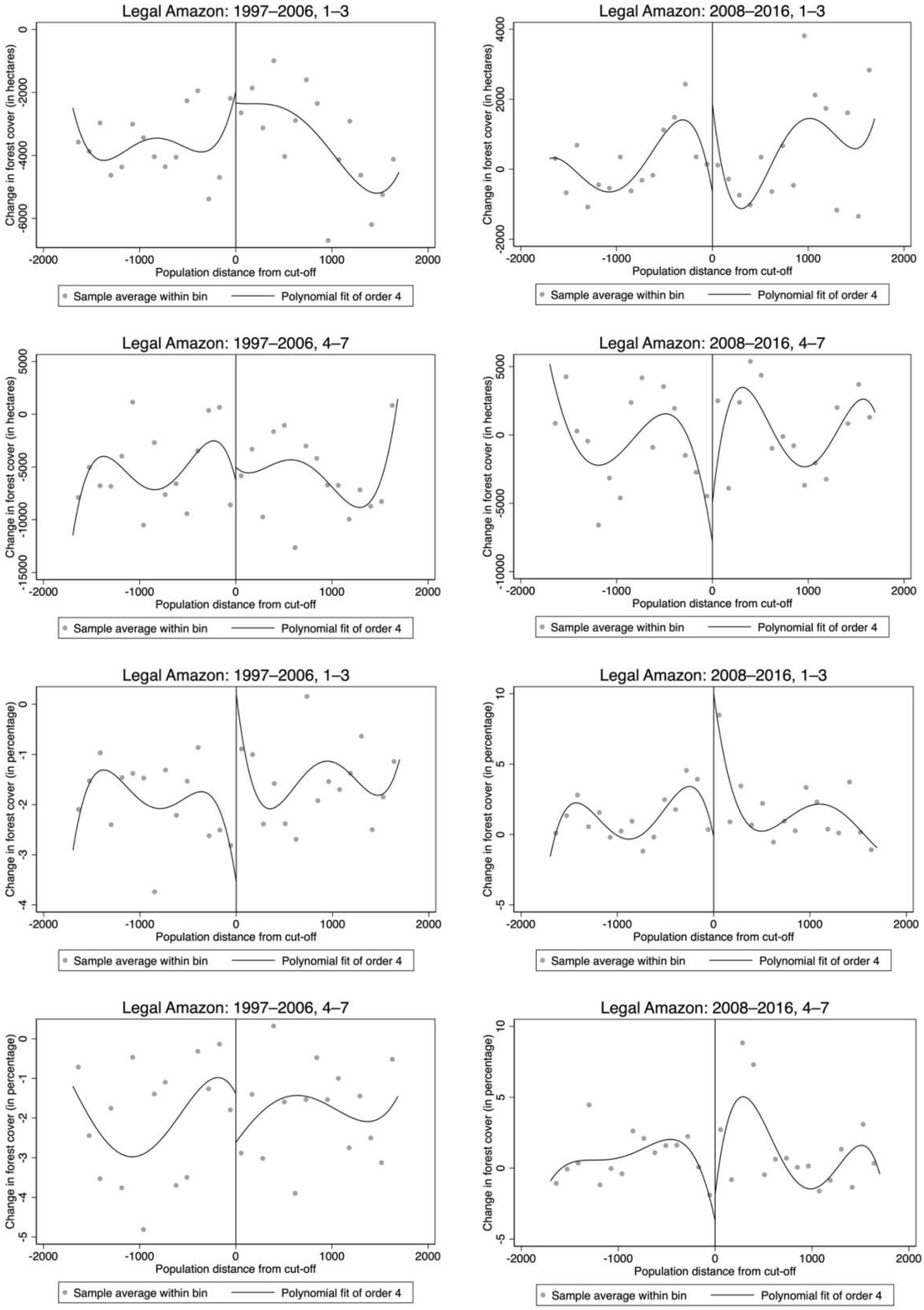
Table 9: *Non-Legal Amazon, sharp RD design*

	Change in forest cover			
	97-06 (Hectares)	08-16 (Hectares)	97-06 (%)	08-16 (%)
Thresholds 1-3	-538.81**	8.0771	-1.4677**	2.2739
	(0.023)	0.958)	(0.012)	(0.051)
	[0.041]	[0.966]	[0.032]	[0.109]
Thresholds 4-7	-47.291	5.6093	.47373	1.4257
	(0.827)	(0.982)	(0.485)	(0.059)
	[0.854]	[0.985]	[0.558]	[0.112]
Threshold 1	188.2	-10.775	-1.387	3.7267
	(0.252)	(0.949)	(0.110)	(0.182)
	[0.342]	[0.958]	[0.179]	[0.277]
Threshold 2	-719.26**	37.27	-1.6348	1.6704
	(0.017)	(0.907)	(0.108)	(0.185)
	[0.040]	[0.924]	[0.179]	[0.236]
Threshold 3	104.78	-372.87	-1.199	.6532
	(0.674)	(0.237)	(0.302)	(0.688)
	[0.722]	[0.304]	[0.385]	[0.744]
Threshold 4	-509.72	-29.112	-.38286	2.1167
	(0.099)	(0.929)	(0.735)	(0.114)
	[0.151]	[0.942]	[0.779]	[0.185]
Threshold 5	174.35	-331.52	.21257	.36486
	(0.636)	(0.600)	(0.868)	(0.792)
	[0.668]	[0.661]	[0.890]	[0.823]
Threshold 6	209.65	850.62	.9597	.66769
	(0.702)	(0.077)	(0.520)	(0.774)
	[0.752]	[0.144]	[0.591]	(0.804)
Threshold 7	-40.358	-233.19	3.9447**	.64151
	(0.965)	(0.622)	(0.003)	(0.633)
	[0.972]	[0.685]	[0.011]	[0.689]
Observations	24,939	20,405	24,939	20,405

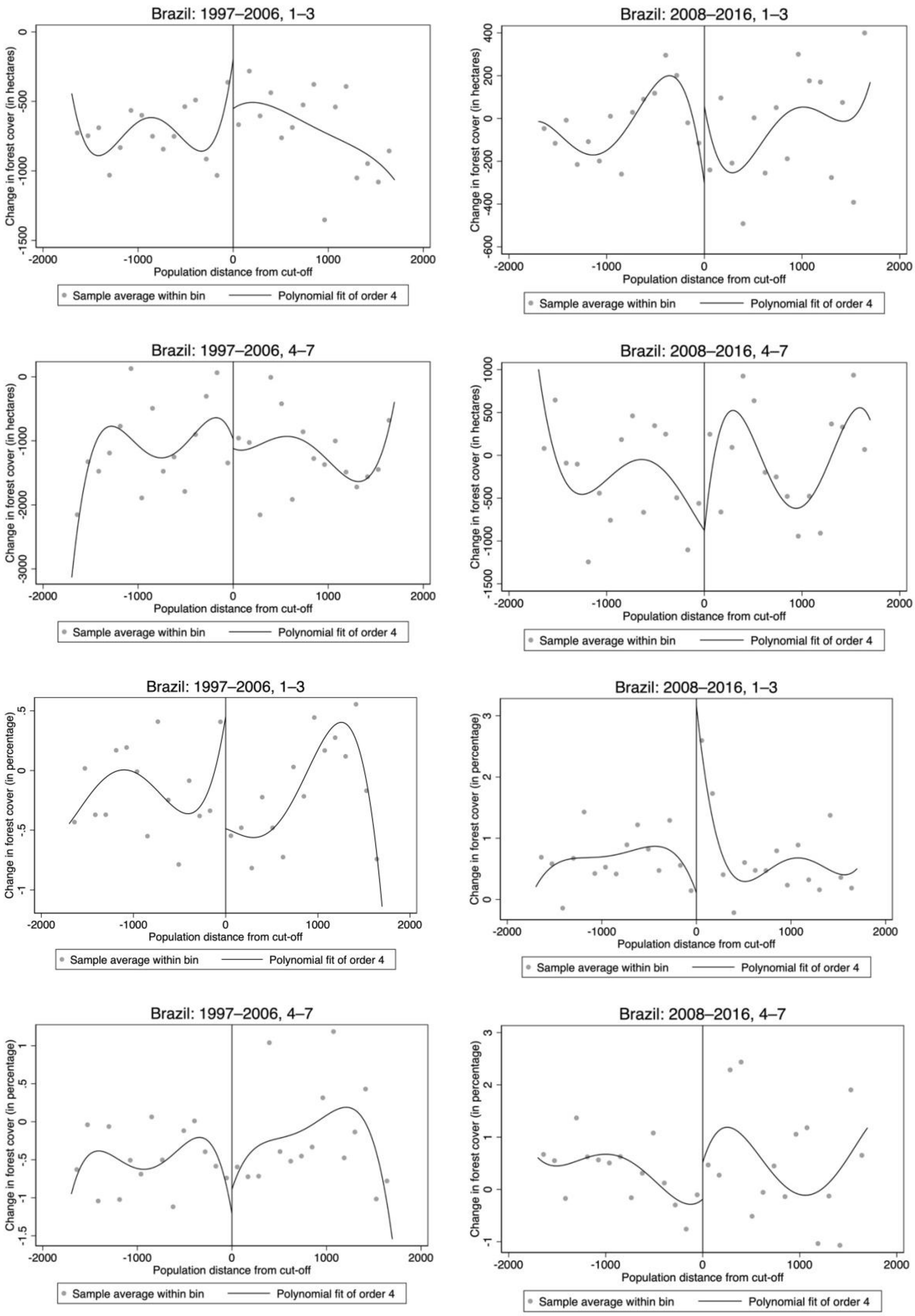
Notes: SRD regressions categorized in the columns by period and whether the change in forest cover for a municipality is measured in hectares or percent of total cover. Each cell reports the effect on deforestation of being just above a cut-off at the different thresholds. `rdrobust` has been used to estimate the regressions and calculate the optimal bandwidth level, controlling for total forest cover and year dummies. Standard errors clustered at the municipality level. Bias-corrected p-values in parentheses; robust bias-corrected p-values in brackets.

* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

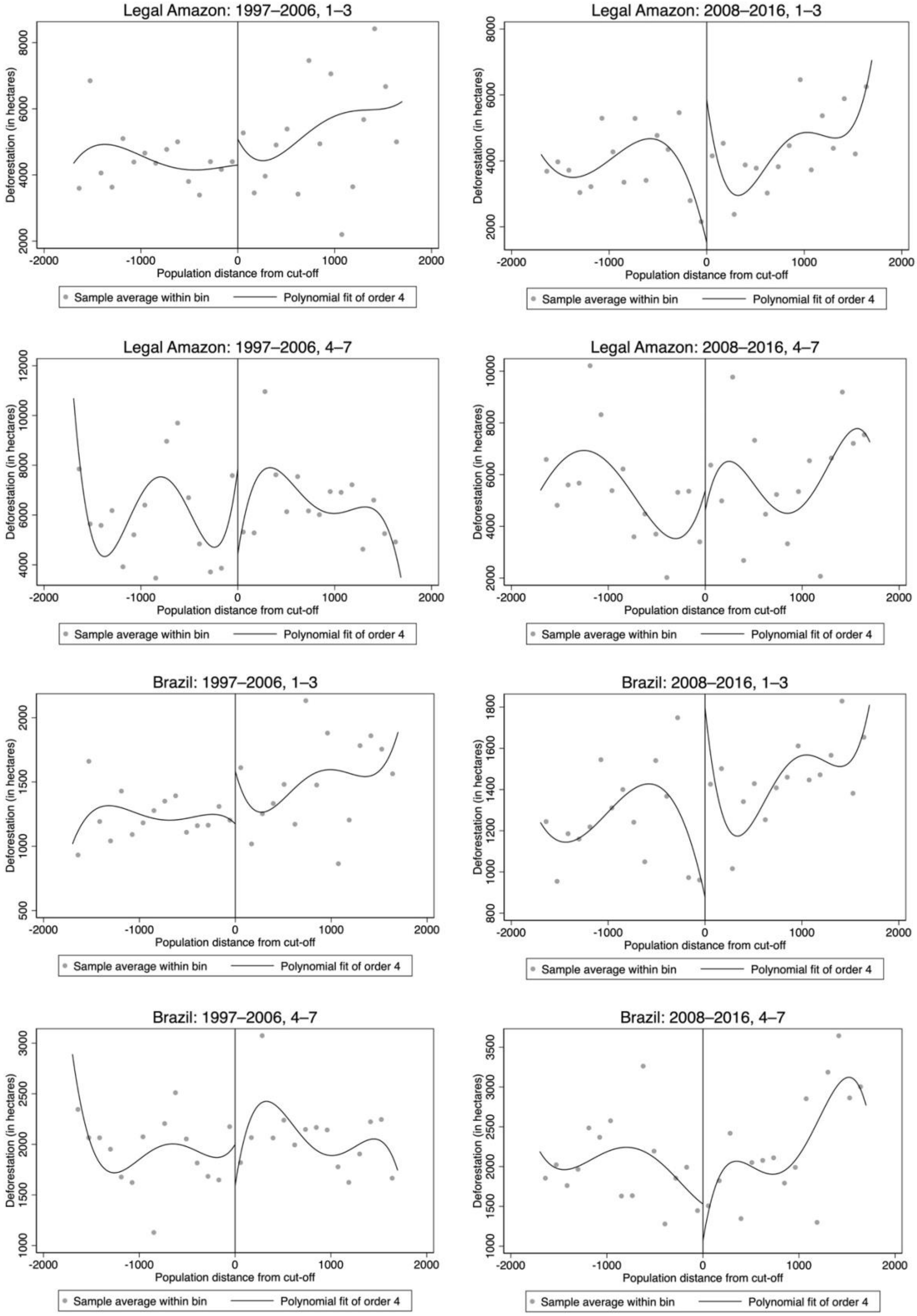
A.9 **Legal Amazon:** ***Coverage change and Percentage coverage change***



A.10 **Brazil:** ***Coverage change and Percentage coverage change***



A.11 Transition from natural forest (ha)



A.12 Alternative (placebo) cut-off plots and tables

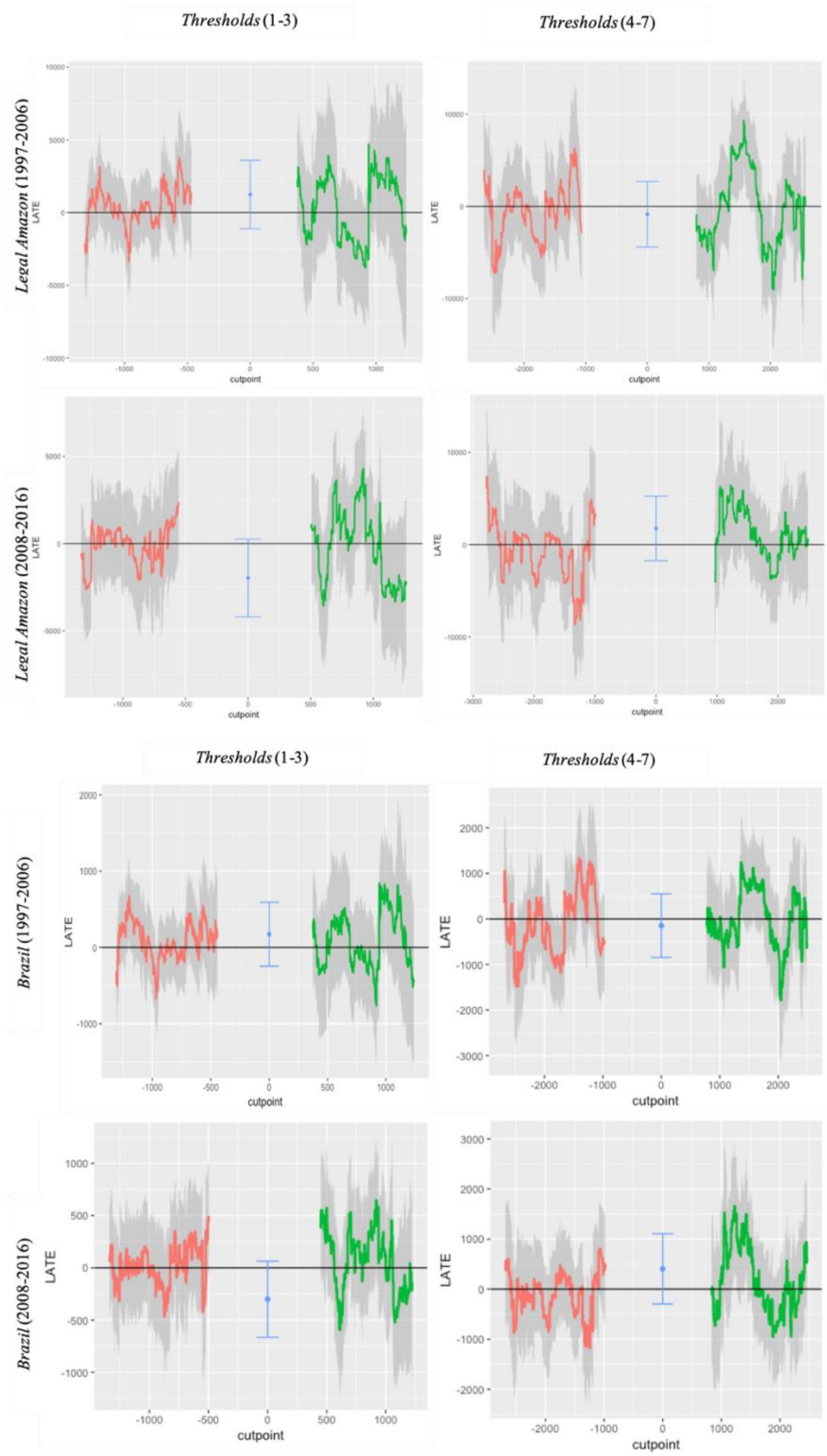


Figure 10: *Plots of placebo tests for the Legal Amazon (top 4 plots) and Brazil (bottom 4 plots). Local Average Treatment Effects (LATE) measured at different cut-offs*

Table 10: *Alternative cut-offs for pooled thresholds in the Legal Amazon from 1997-2006*

	Deforestation (1997-2006)			
	Alternative Cut-off	Hectares %	*p-values*	Confidence Intervals
Thresholds 1-3	-250	-631.296	0.728	[-4189.529, 2926.936]
		2.744	0.157	[-1.054, 6.541]
Thresholds 4-7	-500	1077.274	0.171	[-464.374, 2618.922]
		0.351	0.616	[-1.021, 1.722]
Thresholds 1-3	-150	2027.349	0.197	[-1049.451, 5104.149]
		0.095	0.944	[-2.565, 2.754]
Thresholds 4-7	-300	734.724	0.141	[-243.907, 1713.354]
		-0.390	0.521	[-1.581, 0.801]
Thresholds 1-3	-50	2935.538*	0.054	[-52.677, 5923.754]
		1.905	0.228	[-0.910, 3.824]
Thresholds 4-7	-150	-680.954	0.259	[-1864.159, 502.251]
		-0.826	0.186	[-2.052, 0.399]
Thresholds 1-3	50	652.470	0.683	[-2482.080, 3787.020]
		0.325	0.766	[-1.818, 2.467]
Thresholds 4-7	150	-102.021	0.875	[-1370.126, 1166.083]
		0.484	0.469	[-0.826, 1.794]
Thresholds 1-3	150	525.904	0.691	[-2677.649, 2592.690]
		-0.188	0.829	[-1.896, 1.519]
Thresholds 4-7	300	356.880	0.574	[-887.094, 1600.855]
		0.548	0.388	[-0.696, 1.792]
Thresholds 1-3	250	-418.108	0.770	[-3218.696, 2382.481]
		-1.666	0.148	[-3.922, 0.589]
Thresholds 4-7	500	-496.212	0.401	[-1654.775, 662.351]
		-1.009	0.173	[-2.461, 0.443]

Notes: The artificial cut-offs were chosen at approximately 5%, 10% and 15% away from the true cut-off (0). The outcomes of interest do not jump discontinuously at the artificial cut-offs except at -50 for Thresholds 1-3.

* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 11: *Alternative cut-offs for pooled thresholds in the Legal Amazon from 2008-2016*

	Deforestation (2008-2016)			
	Alternative Cut-off	Hectares %	*p-values*	Confidence Intervals
Thresholds 1-3	-250	-2388.555	0.266	[-6593.130, 1816.019]
		-3.983	0.288	[-11.337, 3.371]
Thresholds 4-7	-500	-2522.495	0.313	[-7420.740, 2375.749]
		-1.067	0.554	[-4.596, 2.463]
Thresholds 1-3	-150	-576.363	0.699	[-1049.451, 5104.149]
		0.018	0.996	[-7.858, 7.895]
Thresholds 4-7	-300	-3424.442	0.249	[-9247.002, 2398.119]
		-3.193	0.155	[-7.591, 1.206]
Thresholds 1-3	-50	526.483	0.681	[-52.677, 5923.754]
		4.045	0.419	[-5.767, 13.858]
Thresholds 4-7	-150	406.785	0.871	[-4497.523, 5311.093]
		-0.821	0.632	[-4.187, 2.544]
Thresholds 1-3	50	468.104	0.735	[-2237.141, 3173.350]
		0.325	0.766	[-1.818, 2.467]
Thresholds 4-7	150	3562.647	0.153	[-1322.525, 8447.819]
		3.611	0.304	[-3.280, 10.502]
Thresholds 1-3	150	-4.570	0.453	[-16.519, 7.379]
		-4.064	0.187	[-10.099, 1.970]
Thresholds 4-7	300	5932.760	0.237	[-3908.735, 15774.255]
		2.026	0.742	[-10.016, 14.067]
Thresholds 1-3	250	130.165	0.926	[-3218.696, 2382.481]
		2.512	0.316	[-3.922, 0.589]
Thresholds 4-7	500	2420.548	0.609	[-6860.485, 11701.582]
		-3.914	0.294	[-11.223, 3.395]

Notes: The artificial cut-offs were chosen at approximately 5%, 10% and 15% away from the true cut-off (0). The outcomes of interest do not jump discontinuously at the artificial cut-offs.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 12: *Alternative cut-offs for individual thresholds in the Legal Amazon that have shown statistically significant results from 1997-2006*

	Deforestation (1997-2006)			
	Alternative Cut-off	Hectares %	*p-values*	Confidence Intervals
Threshold 3	-250	-6493.191	0.124	[-14771.403, 1785.022]
		2.745	0.277	[-2.202, 7.692]
Threshold 3	-150	-2830.023	0.463	[-10384.727, 4724.680]
		-2.758	0.197	[-6.951, 1.436]
Threshold 3	150	3212.761	0.158	[-1250.971, 7676.492]
		0.883	0.466	[-1.493, 3.259]
Threshold 3	250	-1855.772	0.496	[-7193.912, 3482.368]
		-1.428	0.551	[-6.121, 3.264]
Threshold 5	-500	-5116.622	0.167	[-12369.594, 2136.350]
		-2.416	0.143	[-5.651, 0.819]
Threshold 5	-300	2147.201	0.488	[-3914.357, 8208.759]
		-2.111	0.309	[-6.179, 1.958]
Threshold 5	300	6518.158	0.116	[-1611.318, 14647.635]
		1.533	0.113	[-0.362, 3.428]
Threshold 5	500	-70.700	0.986	[-8164.194, 8022.794]
		-1.846	0.358	[-5.786, 2.093]
Threshold 6	-500	14081.272	0.101	[-2728.922, 30891.466]
		1.664	0.605	[-4.644, 7.973]
Threshold 6	-300	19028.77*	0.056	[-464.383, 38521.931]
		3.728	0.383	[-4.640, 12.095]
Threshold 6	300	18175.693	0.233	[-11700.902, 48052.288]
		6.074	0.219	[-3.605, 15.754]
Threshold 6	500	9197.748	0.424	[-6860.485, 11701.582]
		-6.077	0.146	[-14.268, 2.113]

Notes: The artificial cut-offs were chosen at approximately 10% and 15% away from the true cut-off (0). These thresholds were chosen as they were significant in our RD test. Apart from Threshold 6, the outcomes of interest do not jump discontinuously at the artificial cut-offs.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 13: *Alternative cut-offs for individual thresholds in the Legal Amazon that have shown statistically significant results from 2008-2016*

	Deforestation (2008-2016)			
	Alternative Cut-off	Hectares %	*p-values*	Confidence Intervals
Threshold 3	-250	4584.090	0.406	[-6228.476, 15396.656]
		1.028	0.614	[-2.964, 5.019]
Threshold 3	-150	1710.905	0.539	[-3744.542, 7166.352]
		-2.064	0.408	[-6.956, 2.828]
Threshold 3	150	-184.279	0.936	[-1250.971, 7676.492]
		0.001	0.999	[-1.493, 3.259]
Threshold 3	250	4865.836	0.143	[-1647.695, 11379.368]
		-0.648	0.512	[-2.587, 1.290]
Threshold 5	-500	-1709.586	0.613	[-8339.193, 4920.021]
		-5.297	0.126	[-12.081, 1.487]
Threshold 5	-300	335.329	0.880	[-4020.887, 4691.546]
		-2.142	0.266	[-5.916, 1.633]
Threshold 5	300	11935.107	0.182	[-5591.420, 29461.634]
		13.067	0.101	[-2.561, 28.694]
Threshold 5	500	-8992.465	0.166	[-21706.380, 3721.449
		-9.048	0.155	[-21.524, 3.429]
Threshold 6	-500	-7770.963	0.150	[-18352.669, 2810.743]
		0.892	0.637	[-2.817, 4.601]
Threshold 6	-300	-7020.81*	0.085	[-15008.389, 966.777]
		-0.477	0.849	[-5.382, 4.427]
Threshold 6	300	736.769	0.868	[-7980.983, 9454.520]
		-2.510	0.394	[-8.278, 3.258]
Threshold 6	500	-1710.119	0.683	[-9914.192, 6493.953]
		4.729	0.220	[-2.820, 12.277]

Notes: The artificial cut-offs were chosen at approximately 10% and 15% away from the true cut-off (0). These thresholds were chosen as they were significant in our RD test. Apart from Threshold 6, the outcomes of interest do not jump discontinuously at the artificial cut-offs.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 14: *Alternative cut-offs for pooled thresholds in the Legal Amazon that have shown statistically significant results from 1997–2006 (transition)*

	Deforestation (1997-2006)			
	Alternative Cut-off	Hectares	*p-values*	Confidence Intervals
Threshold 1–3	-250	533.99	0.596	-1442.75, 2510.73]
Threshold 1–3	-150	1059.1	0.375	[-1278.63, 3396.78]
Threshold 1–3	150	-1886.7	0.181	[-4648.24, 874.922]
Threshold 1–3	250	-280.82	0.862	[-3445.04, 2883.4]
Threshold 4–7	-500	-4872.8	0.150	[-11505.6, 1760]
Threshold 4–7	-300	-154.28	0.921	[-3202.87, 2894.3]
Threshold 4–7	300	2584.5	0.391	[-3322.63, 8491.61]
Threshold 4–7	500	140.41	0.945	[-3840.04, 4120.86]

Notes: The artificial cut-offs were chosen at approximately 10% and 15% away from the true cut-off (0). These thresholds were chosen as they were significant in our RD test.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 15: *Alternative cut-offs for individual thresholds in the Legal Amazon that have shown statistically significant results from 1997–2006 (transition)*

	Deforestation (1997-2006)			
	Alternative Cut-off	Hectares	*p-values*	Confidence Intervals
Threshold 2	-250	759.58	0.715	[-9276.63, 957.653]
Threshold 2	-150	-4159.5	0.180	[-10233.6, 1914.59]
Threshold 2	150	-3022.1	0.358	[-9462.84, 3418.65]
Threshold 2	250	1204.2	0.632	[-3717.31, 6125.76]
Threshold 3	-250	-806.84	0.630	[-4094.32, 2480.65]
Threshold 3	-150	1575.1	0.406	[-2139.62, 5289.88]
Threshold 3	150	-3259.9	0.113	[-7292.52, 772.676]
Threshold 3	250	4024.6	0.134	[-1240.75, 9289.86]
Threshold 4	-500	274.29	0.799	[-1832.67, 2381.25]
Threshold 4	-300	841.72	0.437	[-1282.4, 2965.84]
Threshold 4	300	-3564.6	0.102	[-7840.42, 711.164]
Threshold 4	500	-1858.3	0.431	[-6486.79, 2770.25]
Threshold 6	-500	-4001.2	0.519	[-16166.2, 8163.78]
Threshold 6	-300	-6519.5	0.159	[-15588, 2549.06]
Threshold 6	300	-18105**	0.035	[-34912, -1297.11]
Threshold 6	500	-8072.5	0.327	[-24226.2, 8081.11]
Threshold 7	-500	-37496**	0.002	[-61555.6, -13435.6]
Threshold 7	-300	5540.9	0.584	[-14310.1, 25392]
Threshold 7	300	-6482	0.425	[-22422.7, 9458.67]
Threshold 7	500	-1137.7	0.854	[-13250.9, 10975.6]

Notes: The artificial cut-offs were chosen at approximately 10% and 15% away from the true cut-off (0).
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 16: *Alternative cut-offs for pooled thresholds in the Legal Amazon that have shown statistically significant results from 2008–2016 (transition)*

	Deforestation (2008–2016)			
	Alternative Cut-off	Hectares	*p-values*	Confidence Intervals
Threshold 1–3	-250	-2757.3	0.148	[-6494.68, 980.146]
Threshold 1–3	-150	-480.05	0.705	[-2962.29, 2002.2]
Threshold 1–3	150	523.53	0.725	[-2390.14, 3437.19]
Threshold 1–3	250	-1309.6	0.375	[-4200.99, 1581.88]
Threshold 4–7	-500	-224.43	0.862	[-2760.27, 2311.4]
Threshold 4–7	-300	1409.1	0.443	[-2194.8, 5012.98]
Threshold 4–7	300	2905	0.310	[-2702.02, 8512.03]
Threshold 4–7	500	-56.733	0.980	[-4459.85, 4346.38]

Notes: The artificial cut-offs were chosen at approximately 10% and 15% away from the true cut-off (0). These thresholds were chosen as they were significant in our RD test.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 17: *Alternative cut-offs for individual thresholds in the Legal Amazon that have shown statistically significant results from 2008–2016 (transition)*

	Deforestation (2008–2016)			
	Alternative Cut-off	Hectares	*p-values*	Confidence Intervals
Threshold 2	-250	-2577.4	0.409	[-8699.09, 3544.31]
Threshold 2	-150	-1362.2	0.207	[-3475.67, 751.293]
Threshold 2	150	-3281.6	0.181	[-8091.4, 1528.21]
Threshold 2	250	-2758.4	0.116	[-6195.7, 678.978]
Threshold 3	-250	-2721.2*	0.096	[-5920.55, 478.153]
Threshold 3	-150	1496.4	0.329	[-1510.01, 4502.86]
Threshold 3	150	5445.4	0.101	[-1070.4, 11961.2]
Threshold 3	250	-6767.3	0.135	[-15639.3, 2104.57]
Threshold 4	-500	-1189.9	0.633	[-6071.1, 3691.38]
Threshold 4	-300	4984.3**	0.043	[147.673, 9820.93]
Threshold 4	300	5616.9	0.334	[-5767.12, 17001]
Threshold 4	500	4205.3	0.301	[-3768.4, 12179]
Threshold 6	-500	12179**	0.045	[-4684.78, -49.2116]
Threshold 6	-300	-142.14	0.930	[-3297.94, 3013.66]
Threshold 6	300	48.686	0.980	[-3666.76, 3764.13]
Threshold 6	500	-3455.5	0.163	[-8310.71, 1399.63]
Threshold 7	-500	-14833**	0.039	[-28936.7, -728.516]
Threshold 7	-300	19977***	0.000	[13219.8, 26733.7]
Threshold 7	300	-6528*	0.057	[-13248.7, 192.636]
Threshold 7	500	-7657.3**	0.016	[-13875.6, -1439.13]

Notes: The artificial cut-offs were chosen at approximately 10% and 15% away from the true cut-off (0).
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 18: *Alternative cut-offs for entire sample thresholds in the Legal Amazon (transition)*

	Deforestation pooled (1997–2016)			
	Alternative Cut-off	Hectares	*p-values*	Confidence Intervals
Threshold 1–7	-500	-1137.5	0.173	[-2771.84, 496.82]
Threshold 1–7	-300	-215.51	0.773	[-1676.65, 1245.62]
Threshold 1–7	-150	734.83	0.270	[-571.898, 2041.57]
Threshold 1–7	150	218.55	0.794	[-1421.41, 1858.5]
Threshold 1–7	300	-1048.3	0.301	[-3035.2, 938.548]
Threshold 1–7	500	577.19	0.564	[-1382.4, 2536.78]

Notes: The artificial cut-offs were chosen at approximately 10% and 15% away from the true cut-off (0). These thresholds were chosen as they were significant in our RD test.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 19: *Alternative bandwidth tests conducted on pooled thresholds and individually significant thresholds in the Legal Amazon.*

	Deforestation (Hectares)					
	97–06	97-06	97-06	08-16	08-16	08-16
Thresholds 1–3: (517, 425)	-2407.337	583.888	822.061	-1408.779	-1024.069	-1523.654
(250/750/1000, 250/750/1000)	(0.395)	(0.664)	(0.458)	(0.423)	(0.288)	(0.102)
Thresholds 4–7: (939, 845)	-2063.808	-1884.543	-1369.853	3845.195	4446.204	3065.339
(750/1250/1500, 750/1250/1500)	(0.521)	(0.452)	(0.568)	(0.276)	(0.127)	(0.104)
Threshold 3: (509, 457)	-1170.287	2895.347	2342.244	1544.727	705.663	-715.265
(250/750/1000, 250/750/1000)	(0.810)	(0.306)	(0.346)	(0.552)	(0.723)	(0.706)
Threshold 5: (707, 777)	-6844.429	-3333.012	-1848.642	2641.139	6967.046	5447.806
(500/1000/1250, 500/1000/1250)	(0.673)	(0.449)	(0.629)	(0.555)	(0.143)	(0.175)
Threshold 6: (1358, 1089)	-18186.579	-7961.103	-6706.643	2574.207	1411.696	-785.397
(1000/1500/1750, 750/1250/1500)	(0.177)	(0.353)	(0.415)	(0.485)	(0.622)	(0.789)
	Deforestation (Percentage)					
	97–06	97-06	97-06	08-16	08-16	08-16
Thresholds 1–3: (484, 515)	3.260	1.158	1.394	9.163	3.679	2.119
(250/750/1000, 250/750/1000)	(0.120)	(0.121)	(0.126)	(0.351)	(0.446)	(0.572)
Thresholds 4–7: (867, 1237)	-1.629	-1.221	-1.037	4.105	3.860	3.202
(750/1250/1500, 1000/1500/1750)	(0.263)	(0.267)	(0.303)	(0.232)	(0.127)	(0.171)
Threshold 3: (307, 350)	9.042	5.442***	3.127**	-1.226	0.175	-1.002
(200/600/900, 200/600/900)	(0.104)	(0.004)	(0.038)	(0.597)	(0.913)	(0.456)
Threshold 5: (755, 891)	0.575	1.323*	0.799	1.901	7.388	6.564
(500/1000/1250, 600/1200/1500)	(0.818)	(0.051)	(0.622)	(0.572)	(0.106)	(0.162)
Threshold 6: (1507, 739)	-3.507	-1.853	-1.680	2.941	2.695	1.282
(1250/1750/2000, 500/1000/1250)	(0.320)	(0.500)	(0.503)	(0.227)	(0.182)	(0.520)

Structure of table: Thresholds *x*: (Optimum bandwidth for 97-06, optimum bandwidth for 08-16) (3 alternative bandwidths for 97-06, 3 alternative bandwidths for 08-16) Each alternative bandwidth corresponds to columns. (e.g. For thresholds 4-7, the optimum bandwidth for 97-06 is 939 and 845 for 08-16. For the result on alternative bandwidth 1250 for 97-06, refer to column 3 and rows 3 and 4. There is a -1884.543 hectare jump at the cut-off with a p-value of 0.452.)

Table 20: *Alternative bandwidth tests conducted on pooled individually significant thresholds in the Legal Amazon (transition)*

	Deforestation (Transition)		
	97-16	97-16	97-16
Thresholds 1-7: (997)	1076.631	1277.274*	1132.370*
(750/1000/1250)	(0.158)	(0.065)	(0.069)

	Deforestation (Transition)					
	97–06	97-06	97-06	08-16	08-16	08-16
Thresholds 1-7: (914, 800)	470.211	449.592	456.507	2350.225**	1340.054*	1018.868*
(750/1250/1500, 600/1000/1200)	(0.633)	(0.578)	(0.543)	(0.048)	(0.043)	(0.058)
Thresholds 1–3: (392, 493)	1149.169	395.477	359.902	3243.450*	2057.541**	1103.804**
(200/600/800, 250/750/1000)	(0.608)	(0.752)	(0.749)	(0.071)	(0.027)	(0.022)
Thresholds 4–7: (829, 845)	795.732	1486.719	1354.598	1781.237	2664.635	2619.974
(750/1250/1500, 750/1250/1500)	(0.707)	(0.420)	(0.457)	(0.392)	(0.118)	(0.304)
Threshold 2: (655, 427)	-1170.287	-658.068	-1370.298	3947.196	904.035	-440.113
(400/800/1000, 250/750/1000)	(0.763)	(0.801)	(0.551)	(0.289)	(0.569)	(0.771)
Threshold 3: (382, 886)	2999.306	553.431	881.805	3318.003	2307.970	1454.301
(200/600/800, 600/1200/1500)	(0.286)	(0.786)	(0.616)	(0.213)	(0.276)	(0.433)
Threshold 4: (1179, 1027)	1178.584	1337.139	1149.219	2040.346	2573.368	2572.270
(1000/1500/1750, 750/1250/1500)	(0.259)	(0.173)	(0.218)	(0.577)	(0.386)	0.361)
Threshold 6: (1021, 699)	9791.070	8109.154	6349.807	3869.836	3392.833	2225.281
(800/1200/1400, 400/900/1200)	(0.157)	(0.240)	(0.038)	(0.616)	(0.303)	(0.350)
Threshold 7: (868, 1178)	-10904.755	-5473.505	-6195.314	-2495.696	1352.844	989.694
(600/1000/130, 1000/1400/1600)	(0.291)	(0.496)	(0.345)	(0.572)	(0.807)	(0.854)

Structure of table: Thresholds *x*: (Optimum bandwidth for 97-06, optimum bandwidth for 08-16)
(3 alternative bandwidths for 97-06, 3 alternative bandwidths for 08-16) Each alternative bandwidth corresponds to columns.

A.13 Yearly analyses

We chose to analyze individual years for several reasons. For example, events like the aforementioned FPM coefficient readjustment period of 2001-2008 affect different years disproportionately. In addition, although we try to provide our rationale for how we divide our analyses, we understand that there are other compelling justifications like splitting by mayoral terms that result in year-to-year differences. Furthermore, it might be useful to investigate the effects of years before and after 2007 and 2010 since the IBGE population estimates for those years are unavailable.

Our yearly findings for Brazil are consistent with our main analyses by thresholds. Although most of the estimates are statistically insignificant, they are mostly negative for the period 1997-2006 and positive for 2008-2016. Notable years that have statistically significant estimates are 1998, 2012, 2014 and 2016. For municipalities in thresholds 4-7 in 1998, there is a 3647 hectare and a 6 percent reduction in forest coverage for municipalities larger than the cut-off that are significant at the 5 percent level. Municipalities in this pool of larger thresholds are also statistically significant at the 5 percent level for 2014. Results for 2012 and 2016 are also interesting as they are statistically significant for both pooled thresholds. However, 2012 had negative estimates for forest coverage change both measured in hectares and percentage while 2016 produced positive estimates. 2012 stands out in the second period as having a change from positive to negative estimates with reductions in approximately 1500 hectares and 5 percent of coverage.

Results from our analysis of the Legal Amazon produced a similar trend of mostly negative results for the first period and positive results for the second period. There are several years with significant jumps, but they are less common. The only year that has at least two statistically significant results at the 5 percent level is 2015, with a 4000 hectare and 5.15 percent growth in coverage.

Table 21: *Yearly analyses for the first period 1997–2006 (Brazil)*

	Deforestation. RD Output:			
	Hectares	Hectares	%	%
Thresholds	(1–3)	(4–7)	(1–3)	(4–7)
1997	-380.84	914.67	1.9498	1.709
	(0.579)	(0.659)	(0.266)	(0.401)
	[0.645]	[0.713]	[0.355]	[0.483]
1998	-590.72	-3646.7 **	-2.8509	-6.0332**
	(0.364)	(0.011)	(0.197)	(0.010)
	[0.439]	[0.029]	[0.277]	[0.030]
1999	955.6	-235.89	-.46525	-1.5036
	(0.160)	(0.826)	(0.809)	(0.425)
	[0.246]	[0.854]	[0.843]	[0.489]
2000	1207.2	-795.51	1.9106	2.263
	(0.169)	(0.580)	(0.272)	(0.225)
	[0.234]	[0.638]	[0.369]	[0.307]
2001	-2212.9	282.92	.65163	-.61925
	(0.122)	(0.842)	(0.528)	(0.745)
	[0.199]	[0.866]	[0.594]	[0.782]
2002	-488.06	-933.23	-1.9715	-.35671
	(0.545)	(0.622)	(0.129)	(0.848)
	[0.594]	[0.689]	[0.195]	[0.874]
2003	-1003.2	407.33	-6.586***	1.4877
	(0.438)	(0.782)	(0.003)	(0.300)
	[0.506]	[0.807]	[0.008]	[0.389]
2004	-368.4	-801.53	-.25525	1.0766
	(0.617)	(0.355)	(0.891)	(0.423)
	[0.660]	[0.432]	[0.910]	[0.487]
2005	-424.72	-1011	-.01611	-.64074
	(0.553)	(0.315)	(0.989)	(0.702)
	[0.625]	[0.370]	[0.991]	[0.754]
2006	298.96	-374.3	1.1135	-.13125
	(0.588)	(0.728)	(0.425)	(0.934)
	[0.650]	[0.775]	[0.502]	[0.945]
Observations	18,973	9,318	18,973	9,318

Note: Bias-corrected standard errors in parentheses; robust bias-corrected standard errors in brackets.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 22: *Yearly analyses for the first period 2008–2016 (Brazil)*

	Deforestation. RD Output:			
Thresholds	Hectares (1-3)	Hectares (4-7)	% (1-3)	% (4-7)
2008	287.15	-365.66	12.651	-.35093
	(0.589)	(0.535)	(0.222)	(0.841)
	[1903]	[988]	[1903]	[988]
2009	426.89	1765.5	.05482	2.2891
	(0.421)	(0.253)	(0.975)	(0.227)
	[1901]	[1004]	[1901]	[1004]
2011	822.86	1163.3	.80329	.2947
	(0.241)	(0.304)	(0.632)	(0.888)
	[1894]	[998]	[1894]	[998]
2012	-1470.9**	-1571**	-5.1166***	-3.1654
	(0.037)	(0.045)	(0.005)	(0.114)
	[1884]	[1012]	[1884]	[1012]
2013	-1888.7	1477.5	6.261	5.723**
	(0.139)	(0.324)	(0.485)	(0.023)
	[1858]	[1043]	[1858]	[1043]
2014	-224.1	2261.5**	1.9817	-4.2438*
	(0.640)	(0.037)	(0.193)	(0.056)
	[1851]	[1045]	[1851]	[1045]
2015	268.12	-1350.5	.56478	-.02117
	(0.511)	(0.140)	(0.637)	(0.988)
	[1849]	[1042]	[1849]	[1042]
2016	1203.3*	1485	6.603**	8.4267***
	(0.054)	(0.289)	(0.021)	(0.009)
	[1847]	[1050]	[1847]	[1050]
Observations	14,987	8,182	14,987	8,182

Note: Bias-corrected standard errors in parentheses; number of observations in brackets.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 23: *Yearly analyses for the first period 1997–2006 (Legal Amazon)*

		Deforestation. RD Output:			
		Hectares	Hectares	%	%
	Thresholds	(1-3)	(4-7)	(1-3)	(4-7)
1997		804.487	24829.632	1.970	-8.881*
		(0.927)	(0.439)	(0.310)	(0.036)
		[243]	[144]	[243]	[144]
1998		5326.362*	-10138.725	-1.578	-6.520
		(0.085)	(0.121)	(0.676)	(0.221)
		[244]	[151]	[244]	[151]
1999		3930.671	-4578.983	7.899***	-4.094
		(0.232)	(0.398)	(0.002)	(0.269)
		[239]	[152]	[239]	[152]
2000		3244.97	-4287.360	6.352	4.582
		(0.526)	(0.460)	(0.174)	(0.505)
		[240]	[156]	[240]	[156]
2001		-9830.761	-3128.166	-1.965	0.614
		(0.253)	(0.501)	(0.440)	(0.812)
		[240]	[166]	[240]	[166]
2002		-3224.293	-3770.082	-4.391	0.912
		(0.310)	(0.630)	(0.200)	(0.719)
		[240]	[165]	[240]	[165]
2003		-6446.036	-5850.575	-3.562	-1.329
		(0.424)	(0.576)	(0.253)	(0.540)
		[236]	[173]	[236]	[173]
2004		5245.33**	-6378.427	0.856	0.923
		(0.038)	(0.217)	(0.850)	(0.712)
		[242]	[171]	[242]	[171]
2005		-454.783	-634.584	1.745	2.323
		(0.880)	(0.893)	(0.534)	(0.568)
		[240]	[173]	[240]	[173]
2006		3232.387	-469.111	9.354	-2.972
		(0.341)	(0.905)	(0.224)	(0.160)
		[242]	[177]	[242]	[177]
Observations		4,879	4,062	4,879	4,062

Note: Bias-corrected standard errors in parentheses; number of observations in brackets.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level

Table 24: *Yearly analyses for the first period 2008–2016 (Legal Amazon)*

	Deforestation. RD Output:			
Thresholds	Hectares (1-3)	Hectares (4-7)	% (1-3)	% (4-7)
2008	-534.228	1876.767	-8.683**	1.492
	(0.800)	(0.658)	(0.042)	(0.313)
	[240]	[168]	[240]	[168]
2009	-2599.528	11077.61**	2.402	4.977
	(0.380)	(0.024)	(0.404)	(0.228)
	[242]	[174]	[242]	[174]
2011	2172.078	11426.621	9.285**	-9.399
	(0.391)	(0.732)	(0.031)	(0.206)
	[247]	[178]	[247]	[178]
2012	4081.948	-3758.622	-6.211	2.645
	(0.112)	(0.492)	(0.291)	(0.516)
	[246]	[179]	[246]	[179]
2013	-12033.581	6844.240	55.263	16.511
	(0.138)	(0.471)	(0.432)	(0.150)
	[236]	[180]	[236]	[180]
2014	-273.004	4917.893	-6.653	0.688
	(0.895)	(0.406)	(0.157)	(0.799)
	[234]	[183]	[234]	[183]
2015	3976.28**	-3880.381	5.152**	1.028
	(0.014)	(0.160)	(0.010)	(0.535)
	[234]	[188]	[234]	[188]
2016	1206.904	14146.555	7.169	6.317
	(0.785)	(0. 142)	(0.349)	(0.107)
	[235]	[191]	[235]	[191]
Observations	4,879	4,062	4,879	4,062

Note: Bias-corrected standard errors in parentheses; numbers of observations in brackets.
* significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level