Cutting Special Interests by the Roots: Evidence from the Brazilian Amazon*

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Abstract

Government policies may impact economic outcomes directly but also indirectly through effects on political incentives. This paper examines the effects of the PPCDAm – a centralized set of environmental policies that effectively raised the expected cost of illegal deforestation – on the behavior of a powerful special-interest group operating in the Brazilian Amazon: farmers. Using different identification strategies, we document that municipalities governed by farmer politicians experienced larger declines in deforestation, greenhouse gases (GHG) emissions, and violence than municipalities governed by other politicians after this set of policies were implemented. Our findings are consistent with the PPCDAm changing political incentives in a persistent way that amplifies its impact on environmental and social outcomes.

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1 Introduction

Government policies may impact economic outcomes directly but also indirectly through effects on political equilibria (Acemoglu and Robinson, 2013; de Janvry et al., 2012). In the case of environmental destruction, when a particular equilibrium exists with politicians catering to voters' interests for resource extraction, conservation policy can have an additional effect by lowering the local electoral returns of depredation. Understanding whether these political effects exist is important to guide the design of environmental policies.[1]

This paper examines this question in the context of conservation policies in the Brazilian Amazon. Specifically, we examine the extent to which the *Action Plan for the Prevention and Control of Deforestation in the Legal Amazon* (hereafter, PPCDAm) – a centralized set of environmental policies implemented by the federal government in November 2004 that raised the expected cost of illegal deforestation – affected the behavior of organized pro-deforestation groups.[2]

One challenge for studying the political effects of government policy is to identify special interest groups whose behavior is affected by specific policies. We overcome this challenge by focusing on politicians connected to agriculture (henceforth, *farmers*). These politicians constitute a powerful interest group in Brazil notoriously opposed to conversation policies (Helfand, 1999; Richardson, 2012). Therefore, we expect their incentives to promote environmental degradation to be strongly influenced by the introduction of more rigorous conservation policies.

We test the effects of the PPCDAm on the behavior of farmer politicians exploiting detailed data on politicians' characteristics, elections, deforestation, and emissions for the

[1]A burgeoning literature raises the point of political effects in the context of scaling up interventions (Manacorda et al., 2011; Banerjee et al., 2017; Muralidharan and Niehaus, 2017). In the case of deforestation, Abman (2014) provides evidence that mayors with reelection incentives responded more to a blacklisting initiative that imposed sanctions to high-deforestation municipalities in the Amazon.

[2]See Nepstad et al. (2009), Assunção et al. (2015) and Burgess et al. (2018) for detailed information about the PPCDAm.

period 2001-16. We begin by classifying politicians as farmers if they report having an agricultural occupation. We document that these politicians correspond to 17.9% of the candidates for mayor and 18% of the mayors elected in the Amazon in the period 2000-12. Our classification is stable across electoral terms and predicts joining the rural caucus among politicians elected to Congress.

We then test whether environmental degradation declines differentially in municipalities governed by farmer politicians after the PPCDAm using two different identification strategies. First, we compare deforestation and emissions over time in municipalities governed by farmer politicians or not. Second, we restrict the comparison to municipalities with close elections between farmers and non-farmers and implement a regression discontinuity design pooling data from multiple periods.

Using Hansen et al. (2013)'s deforestation data, we document that municipalities governed by farmers had higher deforestation rates before but not after the implementation of the PPCDAm. Before, municipalities governed by farmer politicians deforested 90-150 square kilometers more than municipalities governed by other politicians. This represents 0.8-1.4 of the mean deforestation rate observed during this period. These differences decline persistently after the introduction of the PPCDAm – differences in the coefficients pre and post this set of policies are large and statistically significant. We document qualitatively and quantitatively comparable effects for CO2e emissions.

The differences in deforestation rates before the PPCDAm are solely explained by differences in the conversion of forests located outside protected areas into pasture. This is consistent with the existence of penalties and effective monitoring of deforestation inside protected areas even before the implementation of the PPCDAm. Furthermore, this suggests that environmental regulations are not inhibiting the growth of high productivity crop agriculture but rather of low productivity cattle ranching.

Because deforestation and related activities are often connected to land conflict, we further investigate whether the PPCDAm differentially influenced land conflict and violence in municipalities governed by farmer politicians. While some estimates are imprecise, we find suggestive evidence that homicide rates and land-related murders were higher before (but not after) the PPCDAm municipalities governed by farmers politicians. Our findings are consistent with previous evidence linking activities related to deforestation such as land grabbing, illegal logging, and illegal mining (Fetzer and Marden, 2017; Chimeli and Soares, 2017; Pereira and Pucci, 2021); and further document a cause of land conflict previously not discussed in the literature (Miguel et al., 2004; Burke et al., 2015).

Our findings are robust to numerous robustness exercises. First, we provide evidence in favor of the identification hypothesis of both designs used in the paper. Second, we provide evidence that the pre-post results are not driven by changes in the set of municipalities governed by farmer politicians. Third, we provide evidence that the regression discontinuity results are robust to the choice of bandwidth and the exclusion of outliers. Fourth, the results are robust to different normalizations of the main outcomes.

Taken together, our work documents the PPCDAm changed political incentives at the local level in a way that increased its impact on environmental and social outcomes. By doing so, we contribute to different strands of the literature.

First, we contribute to the literature on the politics of deforestation (Burgess et al., 2012; Abman, 2014; Pailler, 2018; Morjaria, 2018; Sanford, 2021). By documenting the importance of local politicians for environmental degradation in Brazil's Amazon, we complement the work of (Abman, 2014) and Pailler (2018) who document the role of reelection incentives for deforestation in the region. Furthermore, by documenting the multiplier effects of environmental policies operating through the incentives of special interest groups operating in the region, we complement the work of (Abman, 2014) who documents multiplier effects of the environmental policies operating through reelection incentives.

Second, we contribute to the literature on the PPCDAm. Existing work documents that the PPCDAm reduced deforestation by increasing the enforcement of environmental legislation in the Amazon (Assunção et al., 2015; Burgess et al., 2018; Assunção et al., 2020, Forthcoming). Our findings indicate this increase in enforcement decreased deforestation both directly by increasing penalties associated with illegal deforestation and indirectly by eliminating the incentives for local politicians to cater to pro-deforestation interests.

Third, our work contributes to the literature on government capture. A growing body of empirical research examining how capture and corruption at the local level influence the effectiveness of public policy (Reinikka and Svensson, 2004; Baicker and Staiger, 2005; Olken, 2007; Ferraz et al., 2012; Banerjee et al., 2018). Our findings indicate that policies can weaken local special-interest groups by affecting the resources on which an extraction equilibrium existed before and, therefore, have a larger impact than originally anticipated. This mechanism is often discussed theoretically (Stigler, 1971; Becker and Stigler, 1974; Grossman and Helpman, 1994; Bardhan and Mookherjee, 2000). However, to the best of our knowledge, the empirical evidence on it is limited to cross-country studies (Ades and Di Tella, 1999).

The remainder of the paper proceeds as follows. Section 2 discusses the context, focusing on environmental policy and the opposition of farmers and ranchers to it. Section 4 describes the data. Section 3 details the conceptual and empirical frameworks. Section 5 discusses the impacts of politicians representing agricultural interests on deforestation and other outcomes. Section 6 concludes.

2 Institutional Background

2.1 Deforestation and Environmental Policies in the Amazon

Covering 60% of the country's territory, the Amazon was sparsely populated until the 1960s. Most of the region was isolated with its mostly indigenous population living from either subsistence or the extraction of rubber. Non-indigenous population was concentrated around few cities which prospered during the rubber boom from 1860-1920.[3]

The dynamics of occupation of the Amazon changed during the military dictatorship that governed the country from 1964 to 1985. The military government believed that increasing migration to the region would serve both to increase exports of minerals and agricultural products and ease pressures for land reform in other regions of the country (Houtzager and Kurtz, 2000). Incentives for the occupation of the Amazon included the construction of roads, hydroelectric dams, and mining projects (Hecht and Cockburn, 2010). It also included the titling of occupied and, therefore, deforested plots (Pfaff, 1999). Environmental policies were non-existent in the region during this period. Indeed, population growth, road building, and the possibility of securing property rights by deforesting land have contributed to the escalation of deforestation in the region in this period (Pfaff, 1999; Pfaff, Robalino, Walker, Aldrich, Caldas, Reis, Perz, Bohrer, Arima, Laurance et al., 2007; Alston, Libecap and Mueller, 2000).

Policies to promote forest conservation began to gain prominence in the late 1980s with the creation of institutions like the Ministry of the Environment (MMA) and the Environmental Protection Agency (IBAMA). The *de jure* protection of forests increased in the following decade with the enactment of different pieces of legislation that increased the share within properties that farmers and ranchers were not allowed to deforest and established criminal and administrative penalties farmers and ranchers would receive in case they violate environmental law. Importantly, the legal framework established that any unauthorized deforestation in the Amazon was a crime and that the environmental police was allowed to seize the equipment (tractors, trucks, chainsaws etc.) found on site in any illegally cleared land. Nevertheless, the lack of coordination between agencies and tools to effectively monitor and punish individuals engaged in illegal deforestation severely limited the effectiveness of this legislation (Assunção, Gandour and Rocha, 2015; Burgess,

[3]See Hecht and Cockburn (2010) for a historical account of the occupation of the Brazilian Amazon and Barham and Coomes (1994b) and Barham and Coomes (1994a) for detailed accounts of the rubber boom in the region.

Costa and Olken, 2018). Thus, forest clearing continued to grow with deforestation in the Brazilian Amazon peaking in the early 2000s.

This spurred the creation of the *Action Plan for the Prevention and Control of Deforestation in the Legal Amazon* (PPCDAM) in November 2004. From its beginning, the PPCDAM promoted a major change in monitoring policies with the adoption of the Real-Time Detection of Deforestation (DETER), a satellite-based monitoring of deforestation in the Amazon. DETER uses geo-referenced images on Amazon forest cover in 15-day intervals to identify deforestation hot spots and target law enforcement efforts. This significantly increased IBAMA's ability to punish illegal deforestation and is considered responsible for the decrease in deforestation which occurred after 2004 (Nepstad, Soares-Filho, Merry, Lima, Moutinho, Carter, Bowman, Cattaneo, Rodrigues, Schwartzman et al., 2009; Assunção, Gandour and Rocha, 2015; Burgess, Costa and Olken, 2018). Evidence indicates that better monitoring decreased deforestation in the region by 60% (Assunção, Gandour and Rocha, Forthcoming). Later, the PPCDAm led to institutional changes focused in sanctioning and increasing monitoring of municipalities with high deforestation, expediting the prosecution of environmental crimes, and restricting credit to producers non-compliant with the environmental legislation. These policies were implemented in 2007 and 2008, being responsible for further decreases in deforestation.[4]

Our work explores whether the PPCDAm influenced the way local officials promote or discourage deforestation. We are particularly interested in understanding the effects of better monitoring of deforestation on the behavior of these officials. To explain how this effect might operate, we next describe the incentives mayors in municipalities in the

[4]The 'priority list' of municipalities facing economic sanctions and increased environmental monitoring was established by Decree No. 6,321, enacted in 2007. See Assunção and Rocha (2019) and Sills, Herrera, Kirkpatrick, Brandão Jr, Dickson, Hall, Pattanayak, Shoch, Vedoveto, Young et al. (2015) for evidence of the effectiveness of the 'priority list' in reducing deforestation. Regulatory modifications facilitating IBAMA's law enforcement actions were established by Decree No. 6,514 enacted in 2008. See Assunção, Gandour and Rocha (Forthcoming) for a discussion of the impact of stricter environmental enforcement on deforestation. The restriction of credit for producers not compliant with the environmental legislation was instituted by Resolution 3,545 enacted in 2008 by the National Monetary Council. See Assunção, Gandour, Rocha and Rocha (2020) for evidence of the effects of this policy on deforestation.

Amazon might have to enact policies to influence deforestation.

2.2 Local Politics, Farmers, and Environmental Policies

Municipalities are the smallest administrative division in Brazil. Municipal governments are managed by a mayor elected using plurality rule in municipalities with less than 200,000 voters and majority rule in municipalities with more than 200,000 voters. Mayors serve a four-year term which can be renewed once.

The decentralization following the 1988 Federal Constitution transformed the municipalities in the main providers of public services in the country (Arretche, 1999). Municipal governments are responsible for managing childcare centers, primary schools and health centers, for improving and maintaining infrastructure, for commissioning the construction of housing projects, for selecting eligible households for a number of federal policies, among others.

Enforcing environmental policies to directly combat deforestation does not fall under the *de jure* jurisdiction of municipal governments, it being the responsibility of state and federal governments. This, however, does not preclude municipalities from *de facto* indirectly influencing deforestation through local policy (Harding et al., 2022). For instance, the consent of local governments is essential for the occurrence of activities like land grabbing or illegal logging.[5][6] Moreover, local governments might influence the incidence of federal policies. Bribes or lobbying might be used to reduce the enforcement of environmental regulations, to facilitate the disbursement of credit to local farmers and ranchers or to increase the number of matching grants earmarking resources to promote agricultural activities in the municipality. The decisions to enact these policies will typically depend on the costs and benefits politicians obtain by encouraging deforestation. These costs and

[5] See Fearnside (2001) and Ludewigs, Brondízio, Hetrick et al. (2009) for a discussion of land grabbing, land tenure and their likely impacts on deforestation and Chimeli and Boyd (2010) for a discussion of illegal logging in the Amazon.

[6] See Alston, Libecap and Mueller (2000) for a discussion of the impact of land tenure on deforestation.

benefits, in their turn, might depend on the extent to which local politicians represent the interests of the farmers operating in their municipality.

Farmers and their associations exert a strong influence in politics in Brazil. The lobby of this sector has influenced politics in Brazil since the country became independent in the 1800s. However, the sector became more politically organized during re-democratization in the 1980s due to fears that democratization would weaken property rights, promote land reform, and end the preferential access to credit the sector enjoyed since the dictatorship (Helfand, 1999).

This reorganization of farmers' political interests created one of the most powerful lobbies in the country - one quarter of all members of Congress are members of the so-called rural caucus, which represents farmers and their interests.[7] In the 1990s, this lobby thrived in ensuring land distribution initiatives did not hurt farmers and in expanding farmers' access to credit from state-owned banks. In the 2000s, the tightening of the conservation policies brought this issue to the center of rural politics in Brazil (Richardson, 2012). Because these policies tighten land constraints, they suffer intense opposition from farmers and their representatives.[8]

At the national-level, politicians representing agricultural interests lobbied for undermining environmental regulations and for appointing bureaucrats aligned with their

[7]The coordinator of a presidential campaign told reporters of Revista Piauí in 2014 that "in thirty years doing political campaigns I have never seen someone be elected without the support (from agri-business)" (*Revista Piauí*, July 2014, p.22). Indeed, the rural caucus openly supported the winning bid of President Jair Bolsonaro in the 2018 presidential election.

[8]The position against anti-deforestation policies of the representatives of farmers' interests in Brazil often receives attention in the international media. In 2012, The Economist reported the tension between farmers and environmentalists in the discussion of the reform of the country's Forest Code ("Environmental Law in Brazil: Compromise or Deadlock?", *The Economist*, June 2, 2012). In 2014, the National Public Radio reported how the growing power of the rural caucus could undermine environmental policies. It wrote that "the make up of Brazil's new legislative body will have a big impact on the world because of a surge in the so-called ruralist bloc and their track record on environmental protections in the Amazon" ("In Brazil, Conservationists Worried New Congress Could Harm Amazon", *National Public Radio*, October 17, 2014). Indeed, The Guardian recently described how the increasing power of farmers and their representatives is threatening conservation policies. It wrote that "beef and soy barons have strengthened their grip on power. (...) (President) Michel Temer appointed several ruralistas to his cabinet and moved to dismantle and dilute the institutions and laws that slowed forest clearance." ("Wild Amazon faces destruction as Brazil's farmers and loggers target national park", *The Guardian*, May 27, 2017).

agenda for the ministries of agriculture and environment.[9] At the local-level, politicians representing agricultural interests might enact policies encouraging deforestation.

The incentives for politicians representing agricultural interests to encourage deforestation will depend on rents policies which encourage illegal deforestation. In the absence of effective monitoring, the returns of deforesting will be high, which, in turn, will induce farmers to pressure politicians to encourage deforestation in their municipalities. To the extent that politicians connected to agriculture are more responsive to these pressures, they will be more likely to encourage deforestation in their municipalities for political reasons. Furthermore, if politicians connected to agriculture have economic interests aligned with those of this industry, they will also be more likely to encourage deforestation for personal reasons. However, in the presence of effective monitoring, the returns from deforesting will be lower which will reduce the political and personal incentives for politicians connected to agriculture to get from promoting deforestation.

Therefore, we expect the real time remote-sensing monitoring system implemented in the first phase of the PPCDAm to differentially influence deforestation in municipalities governed and not governed by politicians connected to agriculture. Other policies implemented in the second phase of the PPCDAm might have a similar impact from the monitoring. By increasing the legal penalties from deforesting or restricting the access to credit to producers non-compliant with the environmental legislation, these policies might further lower the returns from deforesting. Furthermore, by punishing municipalities with high deforestation, these policies might induce competition between municipalities with the goal of leaving (or staying out) of the environmental blacklist.

Our empirical analysis combines data on local politicians, local elections, and forest cover to test the hypotheses laid out above. We specifically test two hypotheses. First, we

[9] The current Secretary of Agriculture, Ms. Tereza Cristina, is the former chair of the rural caucus in Congress while the current Secretary of Environment, Mr. Ricardo Salles, is a former Secretary of the Environment of the state of São Paulo with known connections to agriculture whose appointment was supported by the main farmers' associations.

test whether deforestation was different in municipalities governed and not governed by mayors connected to agriculture before the implementation of the PPCDAm. Second, we test whether this difference decreased after the implementation of the PPCDAm.

3 Conceptual and Empirical Frameworks

3.1 Conceptual Framework

Politicians connected to agriculture might enact policies that promote deforestation and environmental degradation through three main mechanisms: *preferences*, *monetary returns*, and *electoral returns*. We discuss below how each mechanism operates, both in isolation and when interacting with the PPCDAm.

Preferences. There is a large literature documenting the effects of politicians' preferences on economic outcomes. This literature documents that traits such as gender (Chattopadhyay and Duflo, 2004; Beaman et al., 2009; Brollo and Troiano, 2016), ethnicity (Franck and Rainer, 2012), ideology (Pettersson-Lidbom, 2008; Ferreira and Gyourko, 2009), religion (Meyersson, 2014), age (Alesina et al., 2018), and education (Besley et al., 2011) influence policy choice and, as a consequence, public investments, social indicators, and economic performance. In our context, farmer politicians might be more likely to hold pro-deforestation preferences than other politicians, implying they are more likely to implement policies that promote agriculture and environmental degradation. Through this mechanism we would expect that electing a farmer mayor would raise deforestation locally. However, because we do not expect the PPCDAm to directly affect politicians' preferences, it is unlikely that its implementation would change how farmer mayors impact deforestation.

Monetary Returns. Politicians may enact policies that directly benefit them financially. In our context, promoting deforestation might be financially attractive to farmer politicians because of their involvement with agriculture. For example, deforestation expands local land supply, which reduces land prices and increases agricultural profits. It is also tied to land grabbing, which benefits some types of landowners, and to illegal logging, which benefits sellers of timber. This implies that electing farmer mayors could raise deforestation locally. Implementing the PPCDAm, on the other hand, directly increases the expected cost of deforestation and lowers direct monetary gains farmer mayors could have from the activity. Thus, through this mechanism, we expect the program to decrease the effect of electing farmer mayors on deforestation.

Electoral Returns. The literature on voting indicates that politicians often support or enact policies to signal voters their type and competence (Besley and Case, 1995; Coate and Morris, 1995; Banks and Sundaram, 1998; Ashworth, 2005; List and Sturm, 2006; Besley, 2006; Ashworth, 2012). In our context, farmer politicians might find it easier to signal their commitment to voters who benefit from deforestation due to their association with agricultural interests. Therefore, through this mechanism we expect farmer politicians to be more likely to promote deforestation. However, by increasing the penalties on individuals engaged in forest clearing, implementing the PPCDAm changes the benefits voters obtain from policies that promote it. In principle, the direction of the effect is not obvious. Their return could increase if local policies help individuals engaged in forest clearing to dodge the restrictions imposed by the PPCDAm or decrease otherwise. Yet, as discussed in Section 2.2, there is not much municipal governments could do to help individuals evade the PPCDAm. Thus, we expect its implementation to decrease electoral returns from catering to such voters and therefore to reduce the effect of electing farmer mayors on deforestation.

In Appendix Section A, we build a model to formalize the discussion above. In the model, politicians are heterogeneous in their preferences for deforestation, the private benefits they obtain from it, and their ability to attract voters by enacting policies that promote deforestation. Because of these differences in preferences, monetary returns, and electoral returns, municipalities governed by farmer politicians have higher deforestation than municipalities governed by other types of politicians. Moreover, because stricter environmental policies decrease monetary and electoral returns from enacting policies that promote deforestation more from farmer politicians than other types of politicians, implementing the PPCDAm reduces the differences in deforestation between municipalities governed or not by farmer politicians.

3.2 Empirical Framework

To evaluate the effects caused by the PPCDAm on the behavior of farmer politicians, we test whether its implementation influences the differences in deforestation between municipalities governed by these politicians and municipalities governed by other politicians.

We begin by estimating term-specific differences in environmental outcomes between municipalities governed by farmer politicians and municipalities governed by other politicians:

$$Y_{it} = \beta_t P_{it} + \gamma_t' \mathbf{X}_{it} + \delta_t + \epsilon_{it}, \tag{1}$$

in which Y_{it} is an environmental outcome (deforestation or emissions) in municipality i during term t, P_{it} is a dummy denoting whether the municipality i is governed by a farmer politician during term t, $\mathbf{X}_{it}$ is a vector of controls, δ_t is a term-specific intercept, and ϵ_{it} is an idiosyncratic error term.

We estimate Equation (1) using data from four electoral terms: 2001-04, 2005-08, 2009-12, 2013-16. The parameters of interest are β_{01-04}, β_{05-08}, β_{09-12}, and β_{13-16}. Following the discussion in the preceding Section, we expect farmer politicians to enact policies that promote deforestation, implying $\beta_t \geq 0, \forall t$. Moreover, because the PPCDAm reduced the incentives for farmer politicians promote deforestation, we expect $\beta_t - \beta_{01-04} < 0$ for the terms t after the implementation of the PPCDAm.

The parameters β_t identify the term-specific effects of farmer politicians on environmental outcomes under the hypothesis that electing a farmer politician is not correlated with other determinants of environmental outcomes. However, the difference between β_t before and after the implementation of the PPCDAm is identified under the weaker hypothesis that, if were not for differences in the type of politician holding office, changes in environmental outcomes after the implementation of the PPCDAm would be identical in municipalities governed by farmer politicians and municipalities governed by other politicians.

This hypothesis cannot be tested directly. However, it is possible to test whether changes in environmental outcomes were similar in municipalities using data before the implementation of stricter environmental policies. To test this, we estimate year-specific changes in deforestation in municipalities governed by farmer politicians or not both before and after the PPCDAm using the following model:

$$Y_{it} - Y_{i2004} = \sum_{\tau=2001}^{\tau=2016} \beta_\tau P_{it} + \sum_{\tau=2001}^{\tau=2016} \gamma_\tau' \mathbf{X}_{it} + \delta_t + \epsilon_{it}, \tag{2}$$

The parameters of interest in Equation (2) are β_τ. These parameters capture the changes in environmental outcomes between year t and the year 2004 in municipalities governed by farmer politicians and municipalities governed by other politicians. Because 2004 is the last year before the implementation of the PPCDAM, we expect $\beta_\tau = 0, \forall \tau < 2004$ and $\beta_\tau < 0, \forall \tau > 2004$.

Equation (1) is similar to a difference-in-differences design. The main contrast is that in this case P_{it} changes over time. The different β_t's are obtained from comparisons of different groups of municipalities and therefore the differences between coefficients over time might reflect not only the effects of the PPCDAm on the behavior of farmer politicians, but also a change in composition of what municipalities were affected. An equivalent problem influences the interpretation of the differences in the coefficients of interest from Equation (2).[10]

Appendix B demonstrates that the changes in coefficients of interest from Equation (1) are a weighted mean of the evolution of deforestation on *switchers*, i.e. municipalities in which P_{it} changes between periods, and *stayers*, i.e. municipalities in which P_{it} does not change between periods. Thus, it is possible to understand whether changes in P_{it} drive the estimates obtained using the all municipalities by separately estimating Equation (1) in sub-samples of municipalities in which P_{it} changes or not. To compute the effects on *switchers*, we build three datasets composed by municipalities in which P_{it} changed from the period before and at least one period after the PPCDAm (2001-04 and 2005-08, 2001-04 and 2009-12, or 2001-04 and 2013-16). To compute the effects on *stayers*, we build three datasets composed by municipalities in which P_{it} did not change from the period before before and at least one period after the PPCDAm (2001-04 and 2005-08, 2001-04 and 2009-12, or 2001-04 and 2013-16). We then re-estimate Equation (1) on each of these six datasets.[11]

One concern with the estimation of Equation (1) is that unobserved determinants of deforestation might also influence the election of farmers to office, biasing the coefficients of interest. The correlation between levels unobserved determinants of deforestation and the identity of the mayors hampers the identification of the levels of the coefficients while the correlation of changes in changes of unobserved determinants of deforestation and the identity of the mayors hampers the identification of the changes of these coefficients before and after the PPCDAm. To mitigate this concern, we re-estimate Equation (1) restricting attention to close elections. Specifically, we include term-specific linear splines

[10]The fact P_{it} is not fixed in time also has implications for statistical inference. It means that treatment is defined at the unit-time level and not at the unit level as in typical difference-in-differences designs. Following the literature on clustering, this implies standard errors should not be clustered (see Abadie et al. (2017) for a discussion about this issue).

[11]In practice, we estimate the three Equations for *switchers* and the three Equations for *stayers* together interacting dataset fixed effects with the regressors. This enables us to pool the estimates of β_{01-04} from each Equation into one single estimate.

of the farmer politicians' margin of victory as additional controls in these and re-estimate them using municipalities in which farmers won or lost the municipal election by less than h percentage points.[12] This is equivalent to estimating separate RD designs using local linear regressions, uniform weights, and bandwidth h. We provide evidence the regression discontinuity hypothesis are valid in our setting: the distribution of the margin of victory is continuous at the discontinuity and the means of predetermined outcomes are similar at both sides of the discontinuity (see Section 4.2).[13] We further provide extensive evidence that neither the choice of bandwidth nor the weighting procedure influence our results (see Section 5).

4 Data

4.1 Data Sources

Our empirical investigation examines whether politicians connected to agriculture (*farmer* politicians) implemented different environmental policies than other politicians both before and after the introduction of the PPCDAm by the federal governmental. For this, we combine official electoral records, geo-referenced information on deforestation and land use, administrative data on public finances, survey information on land-related conflicts, and census data. We describe each of these sources in detail.

Elections. We obtain information on politicians and electoral outcomes using an administrative dataset of politicians running for office at the local level provided by the Brazilian Electoral Court (TSE) and pre-processed by *Data Basis* (Dahis et al., 2022). Our dataset covers a total of four electoral terms (2001-04, 2005-08, 2009-12, 2013-16). For each candi-

[12] We do not include elections in which the winner and the runner up was a farmer.

[13] The original RD literature argued validity of this empirical design required outcomes to be "as-good-as random" at the discontinuity. However, as highlighted in the recent literature on the topic (e.g., Cattaneo et al. (2015) and de la Cuesta and Imai (2016)) a valid RD design does not require local randomization but rather continuity of expected potential outcomes around the discontinuity.

date for office, this data contains information on his/her electoral performance as well as information on his/her political party, gender, age, occupation, and educational level. We use this dataset to build a measure of candidates connected to agriculture. We define as *farmers* the politicians who self-reported an occupation related to agriculture in our data.[14] For example, farmer candidates amount to 18.5% of all candidates for mayor in the Amazon in 2000 and 16.9% in 2004. We perform validity tests of this measure in Section 4.2. We also classify politicians as *left*-leaning based on Power and Zucco (2009).[15]

Deforestation. We measure deforestation using geo-referenced data on "tree cover" and "tree cover loss" at 30m^2 resolution for the period 2001-16 provided by the Global Forest Change v1.5 (Hansen et al., 2013). Tree cover is defined as all vegetation greater than 5 meters in height and tree cover loss (deforestation) is defined as the complete removal or significant disturbances of the forest canopy. We aggregate the pixel-level information to the municipality-level, resulting in a panel counting the number of deforestation events that are observed in each municipality at a given year. We convert the number of events to square kilometers to obtain a measure that is comparable to the other deforestation and land use measures used in the paper.

In robustness exercises, we use the municipality-level deforestation measure provided by the Project for Monitoring Deforestation in the Legal Amazon of the Brazilian Institute of Spatial Research (PRODES/INPE). Data is available at the municipality-level for the period 2001-16. It includes information on forest cover, deforestation, cloud coverage,

[14]In particular, in Portuguese, we encode an indicator for whether the candidate's occupation is "agricultor", "agronomo", "agropecuarista", "fazendeiro", "pecuarista", "produtor agropecuário", "proprietario de estabelecimento agricola da pecuaria e florestal", "operador de implemento de agricultura pecuaria e exploracao florestal", "tecnico agropecuario", "tecnico de agrimensura", "técnico em agronomia e agrimensura", "trabalhador agrícola", "trabalhador da pecuária", "trabalhador rural", and "vaqueiro". In English this translates roughly as "farmer", "agronomist", "agriculturalist", "farmer", "cattle rancher", "agricultural producer", "owner of a livestock and forestry agricultural establishment", "operator of livestock and forestry farming", "agriculture technician", "survey technician", "agronomy and surveying technician", "agricultural worker", "livestock worker", "rural worker", and "herdsman".

[15]We adapt the definition from Power and Zucco (2009) to the list of parties existing during our sample period. We define the following parties as *left*: PDT, PT do B, PC do B, PT, PSB, Rede, PV, PSOL, PMN, PSTU.

and unobserved areas.[16]

CO2e Emissions. Deforestation is the main source of greenhouse CO2e emissions in the Amazon. However, it misses the effects of other processes such as forest degradation (Qin et al., 2021) and pasture degradation (Bragança et al., 2022) that also influence CO2e emissions. To account for these processes as well as to measure directly the consequences of farmer politicians on climate, we use direct information on C02e emissions as additional outcomes. Specifically, we collect municipality-level information on CO2e emissions from the Greenhouse Gas Emissions Estimation System (SEEG)[17] for the period 2001-16. The data is divided by source of emissions, type of gas, and economic activity involved. Our analysis focuses on emissions from agriculture and land-use and land-use change (LULUC).[18]

Land Use. We obtain information on land use using geo-referenced data on land use at 30m^2 resolution provided by the MapBiomas project (MapBiomas, 2018), also part of the SEEG platform. The data set is generated from Landsat 5, 7 and 8, spans 2001-16, and covers all Brazilian biomes (Amazon, Atlantic Forest, Caatinga, Cerrado, Pampa and Pantanal). It classifies each pixel as being covered by forest, pasture, crops, non-forest, among others. We aggregate the data to municipality-level.

Public Finance. We obtain municipality-level indicators of revenues and expenditures using two distinct sources of data. First, we use data on total revenues and expenditures provided by the National Treasury's Series on Local Public Finances (SICONFI) and pre-processed by *Data Basis* (Dahis et al., 2022). This dataset contains revenues and expenditures classified by type and source for the period 2001-16. Second, we use data for

[16] PRODES defines deforestation as the annual deforestation increment - the area of forest cleared over the 12 months leading up to August of a given year. The annual deforestation increment of year t therefore measures the area, in km2, deforested between 1 August of $t-1$ and 31 July of t.

[17] See `http://seeg.eco.br/en/`.

[18] Appendix Table C.1 describes the relationship between deforestation and emissions. The correlation between deforestation and emissions is strong, particularly so in the Amazon (see Column 1). This correlation comes mostly from emissions in agriculture and land-use and land-use change (LULUC) (see Columns 2-5). However, roughly 40% of the variation in emissions is not explained by deforestation.

matching grants between municipalities and the federal government ("convênios") collected from the Transparency Portal.[19] For each individual grant, this dataset contains information on value, date completed, category, and originating institution.

Violence and Land Conflict. We obtain information about the number of homicides from the *Sistema de Informações de Mortalidade* (SIM) as pre-processed by *Data Basis* (Dahis et al., 2022). We obtain indicators of land conflict manually processing data provided by the *Comissão Pastoral da Terra* (CPT). This commission records all occupations, land conflicts, and violence connected to land conflicts occurring in Brazil since the 1980s. We construct a municipality-year panel of conflicts, murders, and settlements for the period 2001-2012 from the commission's annual reports.

Other. We use data from the 2000 Census provided by the Brazilian Institute of Geography and Statistics (IBGE) to construct municipality-level measures on income, education, health, infrastructure, and demography. We use these measures to test the validity of our empirical design.

4.2 Descriptive Statistics and Validity Tests

4.2.1 *Farmer* Politicians

In this section we show that our classification of *farmer* politicians is meaningful, in the sense of classifying politicians with interests aligned with agriculture and deforestation, and stable across elections. We first report descriptive statistics for farmer and non-farmer candidates for mayor before and after the PPCDAm in Appendix Table C.2. Farmers are disproportionately older, male, less educated, and leaning right in the political spectrum. These patterns hold similarly after the PPCDAm, with candidates being slightly less frequently incumbents and more frequently to the left.

[19] Available at `http://www.portaldatransparencia.gov.br/convenios`.

More significantly, our farmer dummy reliably predicts whether a candidate eventually joins the rural caucus (*Frente Parlamentar da Agropecuária*, or *bancada ruralista*) in Congress. Our validation exercise is the following. We start with all politicians elected to Congress from within the Legal Amazon states in the years we have reliable data about rural caucus membership, i.e. 2010, 2014, and 2018. Among the 273 (= 91 x 3) observations, we classify each for whether they ever self-declared as farmers in our data (= 27) or not (= 246). Among each group, we compute the conditional probability of belonging to the rural caucus: 63% (= 17/27) for farmers and 45% (= 112/246) for non-farmers.[20] Despite the small sample size for three elections, this difference is evidence that our *farmer* measure reliably classifies candidates connected to agricultural interests. To the extent that the *farmer* measure is noisy, our results in Sections below would be attenuated to zero.[21]

We also study the occupational transition dynamics in our data to assess how stable our measure is. In particular, we observe that, among candidates who are ever classified as farmers in our data, they appear as farmers in 73.2% of candidacies (i.e., a candidate-election pair). The next largest categories of candidacies are "other", with 6.8%, "elected official", with 5.4%, "business, with 3.8%, and all others, with 10.8%.

Moreover, we find similar occupational stability when studying one-term transitions in Appendix Table C.3. This exercise includes data for both elections, before and after the PPCDAm, and for all candidates (for both mayor and the local council). We find that, among candidates running for office again four years later, 64% of farmers pre-PPCDAm are also farmers post-PPCDAm. This rate stands high when compared to other occupation categories, such as bureaucrats (47%), business (54%), education (56%), health (71%).

[20] Analogously, for states outside the Legal Amazon these numbers are 69.5% (= 57/82) for farmers and 43.5% (= 516/1184) for non-farmers.

[21] Other data sources we in principle could use for validation, such as campaign donations or self-declared wealth, only have reliable time-series data starting in the 2010s and would require new caveats for interpretation. Identified data on individuals' farm ownership from the Cadastro Ambiental Rural (CAR) is not available.

4.2.2 Sample Selection

We set the unit of observation in our data as a municipality-election term, which accounts for four years each (between 2001-04 and 2013-16). Starting from a total of 772 municipalities in the Legal Amazon and four electoral terms, we build our estimation sample in steps as follows. First, we form our *base sample* by restricting attention to municipalities that are not state capitals and where there was some deforestation during our time frame. This reduces the number of municipalities from 772 to 645. Second, we form our *RD sample* by restricting attention to elections where there was exactly one farmer candidate among the top two most-voted candidates. This reduces the sample from 2580 (=645 x 4) elections[22] to 606, split between 169 pre-PPCDAm and 437 post. Finally, the *RD-BW10 sample* is the RD sample restricted to those elections where the margin between the farmer candidate and competitor was less than 10 percentage points. This reduces the sample from 606 elections to 246, split between 56 pre-PPCDAm and 190 post.

Table 1 reports baseline descriptive statistics for the different groups of municipalities described above: base sample (Columns 1-2), RD sample (Columns 3-4), RD-BW10 sample (Columns 5-6), and the remaining not in the base sample (Columns 7-8). We include data on municipality size and income, forestry and agriculture, and politics. For each outcome and group, we compute the mean and standard error in parenthesis.

We notice a few patterns. First, the municipalities in our sample, in both pre and post periods, are smaller in size when compared to the rest of the Amazon. They are also covered by less forest area, and have a lower proportion of forest covered by protected areas. Within our sample, municipalities present in the pre period are also smaller in size and with less forest area when compared to those in the post period. Along political characteristics, mechanically given our sample construction, we find that municipality-terms in our sample have more farmer candidates running for office than those outside

[22] For most of the regressions estimated throughout the paper we make the additional restriction of requiring all outcomes, variables of interest, and controls to be non-missing. This reduces the base sample from 2580 to 2534 elections.

our sample.

We follow standard practice and provide evidence to support the RD design described in Section 3.2 as a valid identification strategy (Cattaneo and Titiunik, 2022). First, we estimate our RD specification on predetermined outcomes and report that they are balanced at the cutoff. Appendix Table C.4 shows that most of the coefficients are close to zero and statistically insignificant. Appendix Figures C.2 to C.5 provide graphical evidence of the results from Appendix Table C.4 for the four outcomes used as controls in our empirical models: ln(area), ln(tree cover in 2000), ln(population at the beginning of the term), and ln(GDP per capita at the beginning of the term). Second, the RD design further requires the distribution of the running variable to be continuous at the cutoff. Appendix Figure C.1 reports the results of the density test proposed by Cattaneo et al. (2020). We find no evidence of discontinuous changes in the distribution of the margin of victory jumps at the cutoff.

A meaningful comparison of RD coefficients estimated using data from different elections requires the hypothesis the PPCDAm did not influence the occurrence of a close election involving farmer politicians. Appendix Table C.5 provides indirect evidence supporting this assertion. We compare groups' averages before and after PPCDAm, and test each difference statistically with the $Post \times In\ Sample$ and $Post \times Close\ Election$ dummies. We find that none of the selected list of covariates is statistically different across groups before and after the PPCDAm. These results are in line with the descriptive statistics reported in Table 1.

5 Results

5.1 Farmer Politicians and Deforestation

Table 2 reports estimates from Equation (1). Panel A reports the results using deforestation as the dependent variable and Panel B the results using emissions as the dependent variable. All estimates include logs of area, population, initial tree cover, and GDP per capita as controls.

Column 1 reports the estimates using all municipalities. Column 1, Panel A reports that – before the PPCDAm (2001-04) – deforestation was about 95 km^2 higher in municipalities governed by farmer politicians compared to municipalities governed by other politicians ($\approx$ 0.4 standard deviations). The difference in deforestation between municipalities governed or not by farmer politicians declines substantially immediately after the implementation of the PPCDAm (Δ (2005-08)-(2001-04) negative and statistically significant). This suggests that the policies implemented at the beginning of the PPCDAm (specifically the DETER) were important in changing the incentives of farmer politicians to promote deforestation.

The coefficient on farmer politicians fluctuates in subsequent periods. This might be in part due to changes in the selection of politicians caused by the implementation of stricter environmental policies. However, the coefficients continue below the level observed before the PPCDAm. Indeed, the difference between coefficients in the term before and the three terms after this plan was enacted (Δ Post - Pre]) is qualitatively similar to the difference in the coefficients in the term before the the term immediately after this plan was enacted (Δ (2005-08) - (2001-04)), suggesting the effects of the PPCDAm on the incentives of farmer politicians are persistent.

Column 1, Panel B reports broadly comparable results for CO2e emissions. Before the PPCDAm (2001-04), emissions were roughly 6.5 million tons ($\approx$ 0.3 standard deviations) higher in municipalities governed by farmer politicians compared to municipalities governed by other politicians. After the PPCDAm (2005-16), the effects of farmer politicians decline substantially.

To better understand the effects' dynamics, we estimate Equation (2) with yearly data. Figure 1 reports the results. Three patterns emerge from this figure. First, the evolution of deforestation and emissions before the implementation of the PPCDAm was similar in municipalities governed or not by farmer politicians. Second, deforestation and emissions decline differentially in municipalities governed by farmer politicians immediately after the implementation of the PPCDAm. Third, these effects persist until the end of the period.

As discussed in Section 3.2, the coefficients on farmer politicians are a weighted mean of the effects on municipalities in which the identify of the mayor changes between periods (*switchers*) and in which the identity of the mayor does not change between periods (*stayers*). Table 2, Columns 2 and 3 separately estimate the effects in these two groups.

Table 2, Column 2 examines the effects on *switchers* by including municipality fixed effects as additional controls. The effects are qualitatively identical to the ones obtained without fixed effects. The term-specific effects of farmer politicians on deforestation and emissions are positive and statistically before the PPCDAm but decline sharply after its implementation. However, the magnitudes are slightly different. The effects of farmer politicians on deforestation and emissions before the PPCDAm estimated in this subsample are lower than the effects estimated using all municipalities. Moreover, the decline of the effects of farmer politicians on deforestation and emissions immediately after the implementation of the PPCDAm is lower than the decline estimated using all observations. Nevertheless, the decline in effects of electing farmer politicians using all periods is quantitatively similar in both samples.

Table 2, Column 3 examines the effects on *stayers* by restricting the sample to municipalities in which the identity of the mayor does not change as discussed in Section

3.2. Qualitatively, the results are identical to the ones obtained in the previous Columns. Farmer politicians increased deforestation and emissions more before than after the implementation of the PPCDAm. Quantitatively, the magnitude of effects of farmer politicians of deforestation and emissions before the PPCDAm computed in this sub-sample are larger than the effects computed using all municipalities. However, the declines of these effects observed after the PPCDAm are comparable.

As discussed in Section 3.2, to ensure municipalities governed by farmers and governed by other politicians are comparable, we re-estimate Equation (1) restricting attention to close elections. We focus on elections in which a farmer was either the winner or the runner up and the margin of victory was less than 10 percentage points. Table 2, Column 4 reports the results. The results are qualitatively identical to the results presented in the previous Columns. Farmer politicians significantly increased deforestation and emissions before, but not after the implementation of the PPCDAm. However, focusing on close elections increases the magnitudes substantially. Comparing the coefficients of the period immediately before (2001-04) and the period immediately after (2005-08) the implementation of the PPCDAm, we document a relative decline of deforestation in municipalities governed by farmer politicians of 171km^2 ($\approx$ 1.15 standard deviations). Comparing all periods before and after the PPCDAm produces similar results (147km^2, $\approx$ 1 standard deviation). Quantitatively similar results are observed for emissions.

Figures 2 and 3 provide graphical evidence of the results presented in Table 2, Column 4. The Figures show the results presented in the previous paragraph are due to a large and discontinuous increase in deforestation and emissions observed before the PPCDAm in municipalities immediately to the right of the cutoff.[23] Figures 4 and 5 report the coefficients before (after) the PPCDAm are (not) statistically significant for bandwidths substan-

[23]The pattern observed in these Figures raise the concern that the results are driven by one bin with extremely high deforestation and emissions to the right of the discontinuity. This municipality is easily observed in Appendix Figures C.6 and C.7. These Figures plot linear splines along with the raw (instead of binned) data. To test the robustness of our design to excluding this municipality, we implement a doughnut RD design excluding it. Results are unchanged.

tially larger than the one used. The Figures further document that the chosen bandwidth is not too different from the optimal bandwidth computed using Calonico et al. (2014)'s procedure.

One concern with the estimates of Table 2 is that the results are mechanically driven by changes over time in the size of the municipalities governed by farmer politicians. This is particularly concerning for the regression discontinuity estimates as the number of observations close to the discontinuity is quite limited (roughly 60 close elections between farmers and non-farmers per term). Table 3 tests this by re-estimating the specifications from Table 2 using deforestation and emissions divided by municipality area as dependent variables.

Table 3, Panel A reports the results for deforestation as % of the municipality area. The coefficients from Column 1 do not exhibit such a clear decline after the PPCDAm as the coefficients obtained in levels. However, despite being more imprecisely estimated than the coefficients obtained in levels, the coefficients from Columns 2-4 clearly decline after the implementation of the PPCDAm. This decline is qualitatively comparable to the declines observed in Panel A from Table 2. Table 3, Panel B reports the results for emissions per square kilometers. The coefficients from all Columns are qualitatively and quantitatively comparable to the coefficients obtained measuring in emissions in levels reported in Panel B from Table 2.[24]

We report other robustness tests in the Appendix. First, we show the results are broadly robust to other normalizations of deforestation used in the literature (Appendix Table C.6, Columns 1-3). Second, we report the results are robust to measuring deforestation using other datasets (Appendix Table C.6, Columns 4-5). Third, we show the results are robust to trimming and winsorizing the dependent variable (Appendix Table

[24]Figures C.8 and C.9 provide graphical evidence of the results presented in Table 3, Column 4. Before the PPCDAm, we observe a discontinuous increase in deforestation as % of the municipality area and emissions per square kilometer in municipalities farmers barely won the election. Figures C.10 and C.11 report the findings reported in Table 3, Column 4 are robust to the choice of bandwidth. The optimal bandwidth in these regressions is, however, substantially larger than the 10 p.p. bandwidth used in our specifications.

C.6, Columns 6-7). Fourth, we re-estimate the models for deforestation and emissions restricting attention to municipalities outside the Legal Amazon but in the Cerrado – a biome with considerable deforestation but not legally impacted by the PPCDAm. We find substantially different patterns. Farmer politicians typically do not influence deforestation or emissions in the RD specifications and their effects do not change much over time (Appendix Table C.7).

5.2 How do local politicians influence deforestation?

Having established that farmer politicians caused more deforestation and emissions prior to the PPCDAm, we now turn to shedding light on what exactly are the patterns of deforestation observed. Table 4 reports the findings. Panel A reports results using a specification identical to the one used in Table 2, Column 1, while Panel B reports results using a specification identical to the one used in Table 2, Column 4.

Table 4, Column 2 report that the differences in deforestation in municipalities governed or not by farmer politicians are exclusively due to differences in deforestation observed outside protected areas.[25] This result is consistent with the literature finding that protected areas in Colombia and Brazil successfully curb deforestation within their boundaries (Nepstad et al., 2006; Nolte et al., 2013; Gandour, 2018; Bonilla-Mejía and Higuera-Mendieta, 2019; Baragwanath and Bayi, 2020). Table 4, Columns 3-5 indicate that cleared land is converted into pastureland and not cropland. This goes in line with the land use dynamics observed in the Amazon in which extensive, low productivity cattle ranching is the predominant land use.

The findings on patterns of deforestation are corroborated by the effects farmer politicians on economic outcomes. Appendix Table C.8, Columns 1-3 report the effects. There is no evidence the PPCDAm differentially influences agricultural outcomes in municipal-

[25] Protected areas are defined as indigenous lands and conservation units.

ities governed by farmer politicians both in the cross-section and the regression discontinuity estimates. Appendix Table C.8, Columns 4-7 further indicate the PPCDAm does not have clear effects on the level and the distribution of the grants obtained by municipalities to finance specific investments.[26] Both cross-section and regression discontinuity estimates provide evidence the level of grants was not influenced by the PPCDAm. Moreover, cross-section estimates provide some support for the idea that politicians tried to compensate for the increase in the penalties for deforestation by directing more grants to agriculture, while regression discontinuity estimates indicate the opposite.

Taken together, these results suggest that the forests cleared in municipalities governed by farmer politicians are largely used for low productivity activities and therefore do not promote local economic growth. This is consistent with the literature evaluating the PPCDAm which finds no negative effects of this set of policies on economic growth (Assunção, Gandour and Rocha, 2015; Assunção, Gandour, Rocha and Rocha, 2020; Assunção, Gandour and Rocha, Forthcoming). The results further indicate that politician effort does not change in observable dimensions. Nevertheless, it is important to note that, because deforestation is illegal, farmer politicians might be influencing economic activity and public policies in unobserved dimensions.

5.3 Violence and Conflict

As discussed in Section 2, deforestation in the Amazon happens in an environment of weakly defined property rights (Alston, Libecap and Schneider, 1995, 1996; Chiavari, Lopes, Chiavari and de Araujo, 2021). Evidence indicates that the presence of contestable

[26] In Brazil, one type of spending involving significant mayoral discretion and requiring high mayoral effort is called matching grants. These are grants between municipalities and the federal government for specific investments. Their resources are typically used to buy equipment or build infrastructure. In education and health, this typically means refurbishing buildings, buying equipment etc. In agriculture, this typically means buying tractors for cooperatives or farmers' associations. Because the tax base of the municipalities is typically small, obtaining more resources from these grants is an alternative for increasing spending in priority areas. Indeed, evidence indicates mayors put considerable effort in lobbying members of the congress and bureaucrats to obtain these resources (Azulai, 2017).

land often generates conflict and violence (Fetzer and Marden, 2017). Moreover, deforestation is often associated with illegal logging or mining also associated with conflict and violence (Chimeli and Soares, 2017; Pereira and Pucci, 2021). Farmer politicians might worsen that situation if they enact policies that influence land use dynamics or weaken local enforcement.

Table 5 tests this hypothesis using data on violent deaths and land conflict. Panel A reports the results using all municipalities. Column 1 reports that violent deaths were significantly larger in municipalities governed by farmer politicians before, but not after, the implementation of the PPCDAm. The implementation of this set of policies generates a relative decline of roughly 20-28 homicides per 100,000 inhabitants in these municipalities ($\approx$ 0.3-0.4 standard deviations).[27] Columns 2-3 obtain comparable results using data on land-related conflicts and murders. Results are less precise but are qualitatively comparable to the results for homicides. Column 4 reports no effects on settlements. Panel B reports results using close elections. Consistent with the larger effects of farmer politicians on deforestation in close elections, the point estimates become substantially larger. The coefficients for homicides become less precise while the coefficients for land-related murders become more precise.

5.4 Electoral Outcomes

As discussed in Section 3.1 (and highlighted in Appendix Section A), farmers might enact policies that promote environmental degradation in their municipalities to increase their electoral support. One implication of this mechanism is that the PPCDAm might differentially influence the electoral support obtained by farmer politicians. Table C.9 tests this claim.

Panel A reports the results using all municipalities. Columns 1-3 provide evidence

[27] It is important to notice that we are measuring the total homicide rate during the electoral term. To obtain effects for yearly homicide rates ons has to divide the coefficients by four.

that before the PPCDAm the electoral performance of farmers in the next election was better in municipalities governed by farmers. Column 4 indicates this difference in electoral performance was not driven by differences in incumbency effects. These electoral advantages decrease after the PPCDAm. However, the decline in the coefficients is not sufficiently strong for the differences in the coefficients pre and post this plan to be statistically significant at the 5% level. Panel B reports the results restricting the sample to close elections. There is no evidence that before the PPCDAm the electoral performance of farmers in the next election was better in municipalities governed by farmers, nor that after the PPCDAm their electoral performance declined.

Taken together, our findings do not clearly support the view that the PPCDAm influenced the behavior of farmer politicians by changing the *electoral returns* they could receive from promoting environmental degradation in their municipalities. One possible interpretation is that our main results are solely driven by changes in *monetary returns* from promoting environmental degradation. We believe this to be a strong claim, however, given how noisy our classification of farmer candidates and how underpowered our political regressions may be.

6 Conclusion

This paper documents that centralized government policies might have indirect effects by changing the incentives faced by local politicians. For this, we exploit the implementation of the *Action Plan for the Prevention and Control of Deforestation in the Legal Amazon* (hereafter, PPCDAm) in November 2004 – a centralized set of environmental policies that effectively raised the expected cost of illegal deforestation – to provide evidence that it reduced the incentives of farmer politicians to promote environmental degradation. We find deforestation and emissions connected to agriculture and land use have differentially declined in municipalities governed by farmer politicians after this plan was enacted. The declines are persistent, implying the PPCDAm had long-lasting effects on local political incentives. We interpret our findings as follows: by improving monitoring and increasing penalties associated with illegal deforestation outside protected areas, the PPCDAm reduced the rents from illegal deforestation, thereby reducing the incentives for farmer politicians to cater to local special-interest groups and enact pro-deforestation policies.

We also provide suggestive evidence that the differential declines in deforestation led to differential declines in land-related violence – highlighting the close connection between illegal activities and violence in settings with weak governance. We find much weaker evidence the PPCDAm reduced the electoral competitiveness of farmer politicians and little evidence it influenced politician effort or economic performance in observable dimensions.

Taken together, our findings have important policy implications. First, they show that restricting the rents local politicians extract from helping interest groups circumvent environmental regulations is important to break the link between local politics and environmental degradation. Implementing better monitoring systems or increasing penalties are two promising avenues to achieve this. Second, they imply the design of environmental policies should take into account the changes in political incentives it generates. Third, they suggest the dismantling of federal environmental enforcement witnessed in Brazil in recent years could cause a double-reversal where local special-interest groups regain strength to degrade the forest (Burgess et al., 2019).

This paper leaves various avenues for future research open. For example, documenting how are the careers of farmer politicians would be important to better understand their monetary and electoral incentives. Moreover, documenting specifically how farmer politicians organize to promote their interests and shape policy in state or national-level politics in Brazil remains a largely unexplored topic.

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Tables and Figures

Table 1: Descriptive Statistics

	Base Sample		RD Sample		RD-BW10 Sample		Not Base Sample	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Size and Income								
Population (000's)	22.30	25.75	15.70	19.07	17.56	20.15	54.51	62.69
	(30.51)	(35.30)	(15.39)	(19.46)	(17.89)	(19.41)	(204.49)	(234.84)
Area (km2 000's)	7.28	7.28	5.16	5.46	4.50	5.83	2.72	2.72
	(14.69)	(14.68)	(5.71)	(9.46)	(4.15)	(9.55)	(3.82)	(3.81)
GDP (R$ p.c. 000's)	4.74	9.05	6.31	11.07	4.67	10.62	5.29	10.38
	(6.75)	(10.45)	(10.13)	(12.44)	(3.91)	(11.05)	(5.89)	(10.19)
Forestry and Agriculture								
Tree Cover (km2 000's)	5.86	5.86	3.31	3.92	2.58	4.30	0.97	0.97
	(14.14)	(14.13)	(4.86)	(8.89)	(2.85)	(8.90)	(2.91)	(2.91)
% Protected Area	11.53	11.53	7.04	6.79	8.27	8.13	5.50	5.50
	(25.37)	(25.35)	(17.69)	(18.65)	(17.69)	(20.63)	(18.05)	(18.01)
Ever a Priority Municipality	0.08	0.08	0.09	0.08	0.05	0.09	0.01	0.01
	(0.27)	(0.27)	(0.28)	(0.27)	(0.23)	(0.29)	(0.09)	(0.09)
Agricultural Frontier	0.75	0.75	0.82	0.79	0.80	0.74	0.96	0.96
	(0.43)	(0.43)	(0.38)	(0.40)	(0.40)	(0.44)	(0.20)	(0.19)
Amazon Biome	0.74	0.74	0.67	0.73	0.75	0.77	0.17	0.17
	(0.44)	(0.44)	(0.47)	(0.45)	(0.44)	(0.42)	(0.37)	(0.37)
Cerrado Biome	0.25	0.25	0.32	0.27	0.25	0.23	0.83	0.83
	(0.43)	(0.43)	(0.47)	(0.44)	(0.44)	(0.42)	(0.37)	(0.37)
Politics								
Candidates Mayor	2.75	2.89	2.54	2.80	2.93	2.96	2.61	2.70
	(1.06)	(1.10)	(0.93)	(1.02)	(0.99)	(1.09)	(1.13)	(1.11)
Farmer Candidates Mayor	0.50	0.40	1.16	1.12	1.23	1.16	0.54	0.41
	(0.70)	(0.63)	(0.45)	(0.35)	(0.60)	(0.39)	(0.70)	(0.63)
Candidates Council	64.10	66.91	55.02	58.42	60.11	62.76	58.56	65.55
	(35.39)	(37.43)	(26.29)	(28.48)	(27.69)	(30.60)	(86.06)	(109.58)
Farmer Candidates Council	15.62	13.30	18.89	14.51	21.52	15.31	9.88	8.11
	(11.78)	(9.91)	(13.00)	(9.81)	(14.88)	(10.31)	(6.64)	(6.23)
Council Seats	10.20	9.60	9.85	9.38	9.86	9.47	10.40	10.14
	(1.89)	(1.49)	(1.45)	(1.07)	(1.51)	(1.18)	(3.81)	(4.22)
Observations	645	1935	169	437	56	190	127	381

Notes: This Table presents descriptive statistics at the election level. Samples are defined as in Section 4.2. An election held within the Legal Amazon enters our base sample if the municipality is not a state capital if the municipality had some positive deforestation throughout our time frame. Elections in our base sample enter our RD sample if there was exactly one farmer candidate among the top two most-voted candidates in that election.

Table 2: Effects of electing farmer mayors on environmental degradation (in levels)

	Cross Section	Switchers	Constant Selection	RD
	(1)	(2)	(3)	(4)
Panel A: Deforestation (km^2)				
2001-04	94.80***	31.20**	136.58**	153.19**
	(33.68)	(12.21)	(56.69)	(78.11)
2005-08	5.05	-2.12	26.99*	-18.31
	(13.75)	(10.75)	(14.84)	(44.29)
2009-12	26.74**	-41.37	80.09**	63.76
	(12.90)	(33.96)	(31.70)	(72.70)
2013-16	-19.12	-85.78**	39.69	-14.58
	(16.87)	(42.84)	(45.78)	(59.55)
Δ (2005-08) - (2001-04)	-89.76**	-33.32***	-109.59**	-171.50*
	(35.82)	(12.90)	(54.55)	(89.02)
Δ Post - Pre	-89.19***	-74.29***	-87.66**	-147.61*
	(34.09)	(25.49)	(41.24)	(84.18)
Total observations	2534	3814	2903	246
Distinct municipalities	644	644	585	195
Municipality FE	-	✓	-	-
Mean Pre	113.73	113.73	106.58	104.97
SD Pre	253.72	253.58	246.01	148.87
Panel B: CO2e Emissions (mm tons)				
2001-04	6.53**	2.07**	9.45**	18.53***
	(2.69)	(0.90)	(4.42)	(6.95)
2005-08	0.52	-0.68	2.26**	-1.03
	(0.97)	(0.80)	(1.03)	(2.95)
2009-12	2.48***	-3.72	4.74*	6.39
	(0.91)	(2.57)	(2.75)	(6.19)
2013-16	0.95	-4.12	4.41**	-3.57
	(0.79)	(3.34)	(2.25)	(2.94)
Δ (2005-08) - (2001-04)	-6.00**	-2.75***	-7.19*	-19.56***
	(2.80)	(0.96)	(4.25)	(7.49)
Δ Post - Pre	-5.26*	-4.91**	-5.65*	-18.34**
	(2.69)	(1.98)	(3.24)	(7.30)
Total observations	2534	3814	2903	246
Distinct municipalities	644	644	585	195
Municipality FE	-	✓	-	-
Mean Pre	7.78	7.78	6.80	9.58
SD Pre	18.58	18.57	17.72	12.69

Notes: This Table presents results from Equation (1) estimated for deforestation and CO2e emissions. Column (4) restricts observations to being within the 10 p.p. vote share bandwidth. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Robust standard errors in parenthesis. p<0.01 ***, p < 0.05 **, p < 0.1 *.

Table 3: Effects of electing farmer mayors on environmental degradation (normalized)

	Cross Section	Switchers	Constant Selection	RD
	(1)	(2)	(3)	(4)
Panel A: Deforestation (%)				
2001-04	0.46*	0.24	0.56*	3.08*
	(0.27)	(0.15)	(0.31)	(1.68)
2005-08	0.45*	-0.16	0.17	-1.97
	(0.23)	(0.19)	(0.35)	(1.42)
2009-12	0.31*	-0.43	0.08	0.03
	(0.16)	(0.30)	(0.29)	(0.60)
2013-16	-0.41*	-0.92**	-0.65**	0.51
	(0.25)	(0.42)	(0.30)	(1.39)
Δ *(2005-08) - (2001-04)*	-0.00	-0.40*	-0.38	-5.05**
	(0.36)	(0.21)	(0.33)	(2.18)
Δ *Post - Pre*	-0.28	-0.75***	-0.69***	-3.69**
	(0.30)	(0.21)	(0.24)	(1.83)
Total observations	2534	3814	2903	246
Distinct municipalities	644	644	585	195
Municipality FE	-	✓	-	-
Mean Pre	2.11	2.11	2.02	2.31
SD Pre	2.56	2.56	2.45	2.88
Panel B: CO2e Emissions (1,000 tons/km^2)				
2001-04	0.58***	0.27***	0.76***	3.04*
	(0.22)	(0.09)	(0.26)	(1.60)
2005-08	0.31**	-0.42***	0.30	-0.82
	(0.14)	(0.14)	(0.19)	(0.72)
2009-12	0.18**	-0.51**	0.16	0.39
	(0.08)	(0.26)	(0.17)	(0.30)
2013-16	0.02	-0.26	0.23	-0.16
	(0.10)	(0.26)	(0.23)	(0.35)
Δ *(2005-08) - (2001-04)*	-0.27	-0.69***	-0.47**	-3.86**
	(0.26)	(0.15)	(0.22)	(1.74)
Δ *Post - Pre*	-0.39*	-0.66***	-0.54***	-3.31**
	(0.23)	(0.15)	(0.18)	(1.63)
Total observations	2534	3814	2903	246
Distinct municipalities	644	644	585	195
Municipality FE	-	✓	-	-
Mean Pre	1.92	1.92	1.77	2.49
SD Pre	2.10	2.10	2.02	2.40

Notes: This Table presents results from Equation (1) estimated for deforestation and CO2e emissions. Column (4) restricts observations to being within the 10 p.p. vote share bandwidth. Controls include logs of population, tree cover area in 2000, and GDP per capita. Robust standard errors in parenthesis. p<0.01 ***, p < 0.05 **, p < 0.1 *.

Table 4: Effects of electing farmer mayors on patterns of deforestation

	Deforestation	Inside PA	To Pasture	To Crops	To Other
	(1)	(2)	(3)	(4)	(5)
Panel A: Cross-Section					
2001-04	94.80***	90.45***	49.67***	0.10	-1.15
	(33.68)	(33.80)	(14.15)	(0.85)	(0.86)
2005-08	5.05	7.04	10.86	-1.05**	-0.74
	(13.75)	(12.68)	(8.38)	(0.50)	(0.88)
2009-12	26.74**	22.46**	22.10*	1.29	0.74
	(12.90)	(10.58)	(11.57)	(2.58)	(1.23)
2013-16	-19.12	-20.13	18.07	-2.20	0.00***
	(16.87)	(12.36)	(25.67)	(1.74)	(0.00)
Δ *(2005-08) - (2001-04)*	-89.76**	-83.40**	-38.81**	-1.15	0.41
	(35.82)	(35.42)	(16.18)	(0.95)	(1.23)
Δ *Post - Pre*	-89.19***	-85.65**	-33.30**	-0.70	1.06
	(34.09)	(33.91)	(16.04)	(1.27)	(1.14)
Total observations	2534	2534	2534	2534	1899
Distinct municipalities	644	644	644	644	644
Mean Pre	113.73	111.44	61.44	1.55	11.42
SD Pre	253.72	252.76	111.49	8.98	10.09
Panel B: Regression Discontinuity					
2001-04	153.19**	151.97**	117.86**	-7.98	-2.19
	(78.11)	(76.82)	(52.89)	(6.92)	(3.78)
2005-08	-18.31	-11.44	28.08	-1.13	-2.44
	(44.29)	(42.58)	(28.60)	(2.03)	(3.36)
2009-12	63.76	20.27	124.45	-11.14	4.22
	(72.70)	(43.23)	(82.66)	(7.13)	(8.48)
2013-16	-14.58	-20.83	-22.31	6.10	0.00***
	(59.55)	(59.53)	(38.77)	(5.36)	(0.00)
Δ *(2005-08) - (2001-04)*	-171.50*	-163.40*	-89.78	6.86	-0.24
	(89.02)	(87.19)	(59.53)	(6.87)	(4.95)
Δ *Post - Pre*	-147.61*	-157.69**	-79.53	6.44	2.49
	(84.18)	(80.35)	(61.83)	(6.79)	(5.46)
Total Observations	246	246	246	246	185
Distinct municipalities	195	195	195	195	160
Mean Pre	82.63	79.03	51.14	2.11	11.54
SD Pre	117.76	116.03	69.14	10.87	8.66

Notes: This Table presents results from Equation (1) estimated for patterns of deforestation in km^2. Panel B restricts observations to being within the 10 p.p. vote share bandwidth. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Robust standard errors in parenthesis. p<0.01 ***, p < 0.05 **, p < 0.1 *.

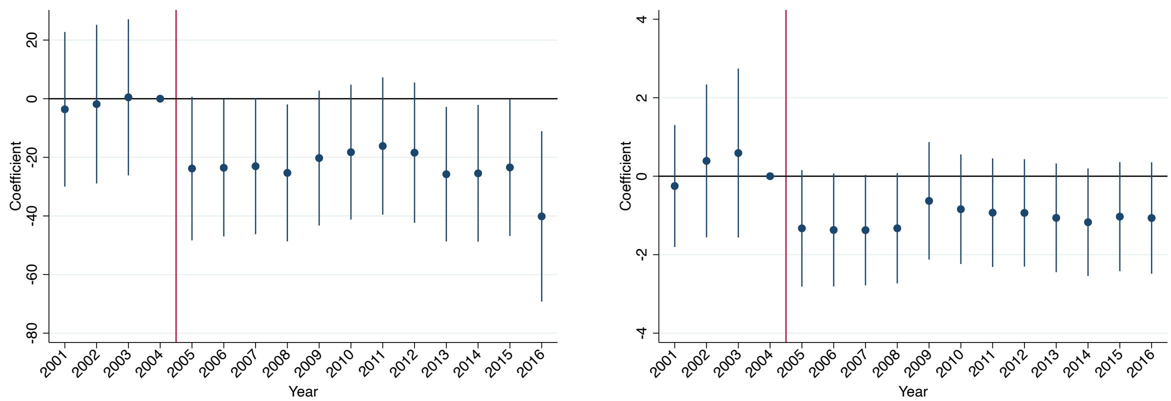
Table 5: Effects of electing farmer mayors on homicides and land conflict

	Homicide Rate	Conflicts	Murders	Settlements
	(1)	(2)	(3)	(4)
Panel A: Cross-section				
2001-04	27.11***	0.08	0.11***	-0.00
	(7.13)	(0.08)	(0.04)	(0.03)
2005-08	6.94	0.04	0.04	-0.03
	(6.53)	(0.10)	(0.04)	(0.02)
2009-12	-6.80	-0.04	0.08	0.02
	(7.51)	(0.11)	(0.05)	(0.03)
2013-16	-9.11			
	(7.74)			
Δ (2005-08) - (2001-04)	-20.17**	-0.05	-0.07	-0.02
	(9.65)	(0.13)	(0.06)	(0.04)
Δ Post - Pre	-28.83***	-0.09	-0.07	-0.00
	(8.24)	(0.10)	(0.05)	(0.03)
Total observations	2534	2534	2534	2534
Distinct municipalities	644	644	644	644
Mean Pre	66.44	0.67	0.08	0.10
SD Pre	71.04	0.93	0.34	0.35
Panel B: Regression Discontinuity				
2001-04	37.82	0.76	0.67**	-0.02
	(50.00)	(0.52)	(0.29)	(0.17)
2005-08	-33.93	0.15	-0.04	-0.18**
	(27.75)	(0.37)	(0.15)	(0.09)
2009-12	10.08	1.01**	0.19	0.38**
	(32.73)	(0.51)	(0.20)	(0.15)
2013-16	-10.21			
	(34.46)			
Δ (2005-08) - (2001-04)	-71.75	-0.61	-0.71**	-0.16
	(55.67)	(0.64)	(0.33)	(0.19)
Δ Post - Pre	-51.87	-0.59	-0.63**	0.04
	(52.46)	(0.56)	(0.31)	(0.18)
Total observations	246	246	246	246
Distinct municipalities	195	195	195	195
Mean Pre	88.71	0.65	0.06	0.12
SD Pre	66.32	0.96	0.27	0.39

Notes: This Table presents results from Equation (1) estimated for the ratio of homicides per 100,000 people and various types of land conflict under an inverse hyperbolic sine transformation. Panel B restricts observations to being within the 10 p.p. vote share bandwidth. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Robust standard errors in parenthesis. p<0.01 ***, p < 0.05 **, p < 0.1 *.

Figure 1: Cross-section effects of electing farmer mayors on deforestation and CO2 emissions

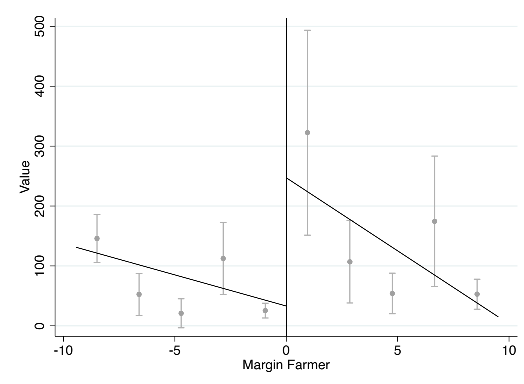
(a) Deforestation (km2) (b) CO2 emissions (mm tons)



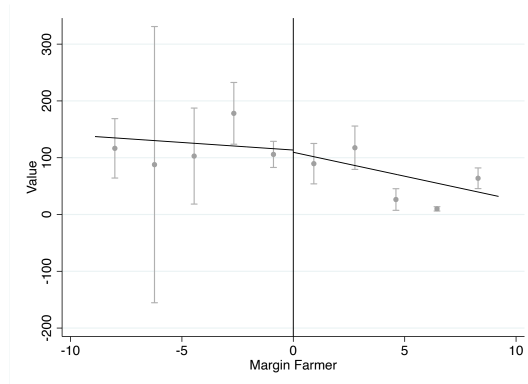
Notes: This Figure presents results from Equation 2 estimated for deforestation and CO2e emissions. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Bands represent the 95% confidence intervals.

Figure 2: Regression-discontinuity effects of electing farmer mayors on deforestation

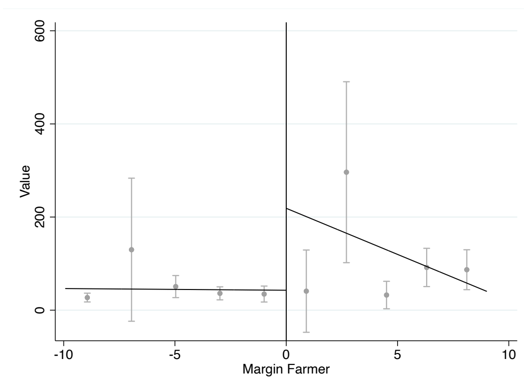
(a) 2001-04



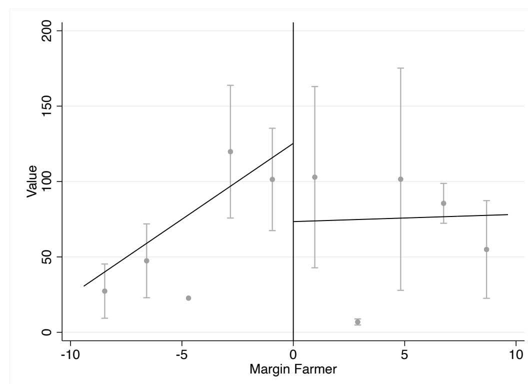
(b) 2005-08



(c) 2009-12

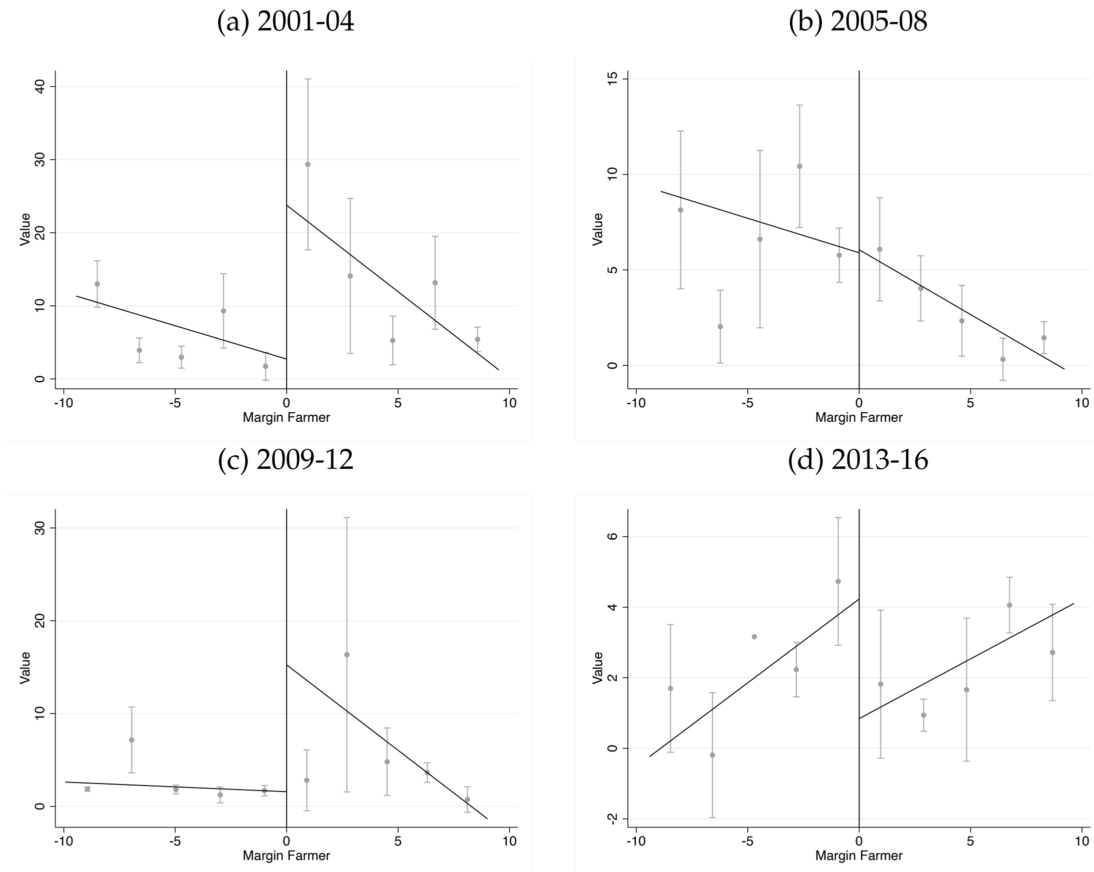


(d) 2013-16



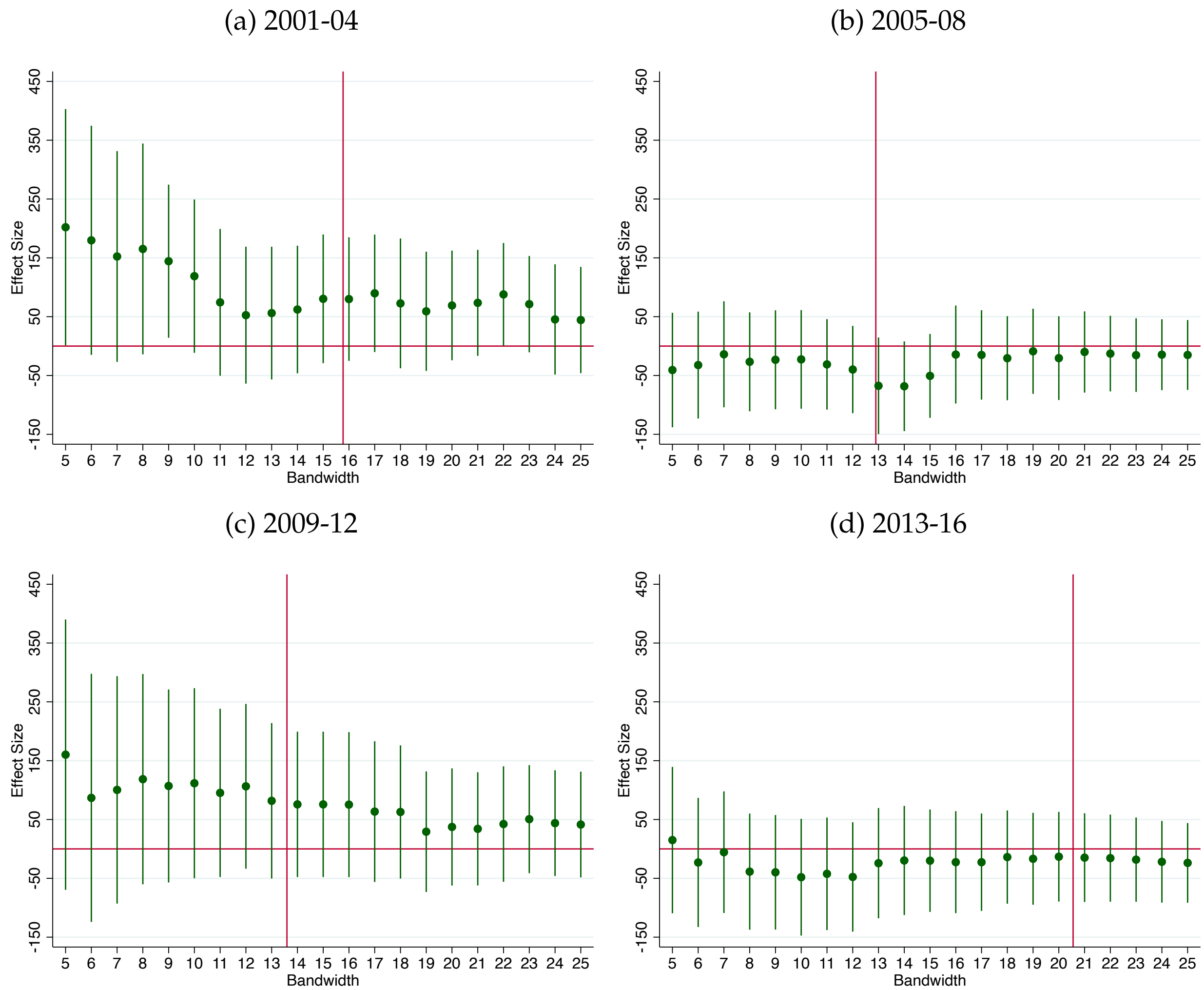
Notes: This Figure plots results from Equation (1) estimated for deforestation. Each plot restricts observations to its specific term and within the 10 p.p. bandwidth. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Bands plot the 95% confidence intervals.

Figure 3: Regression-discontinuity effects of electing farmer mayors on CO2e emissions (mm tons)



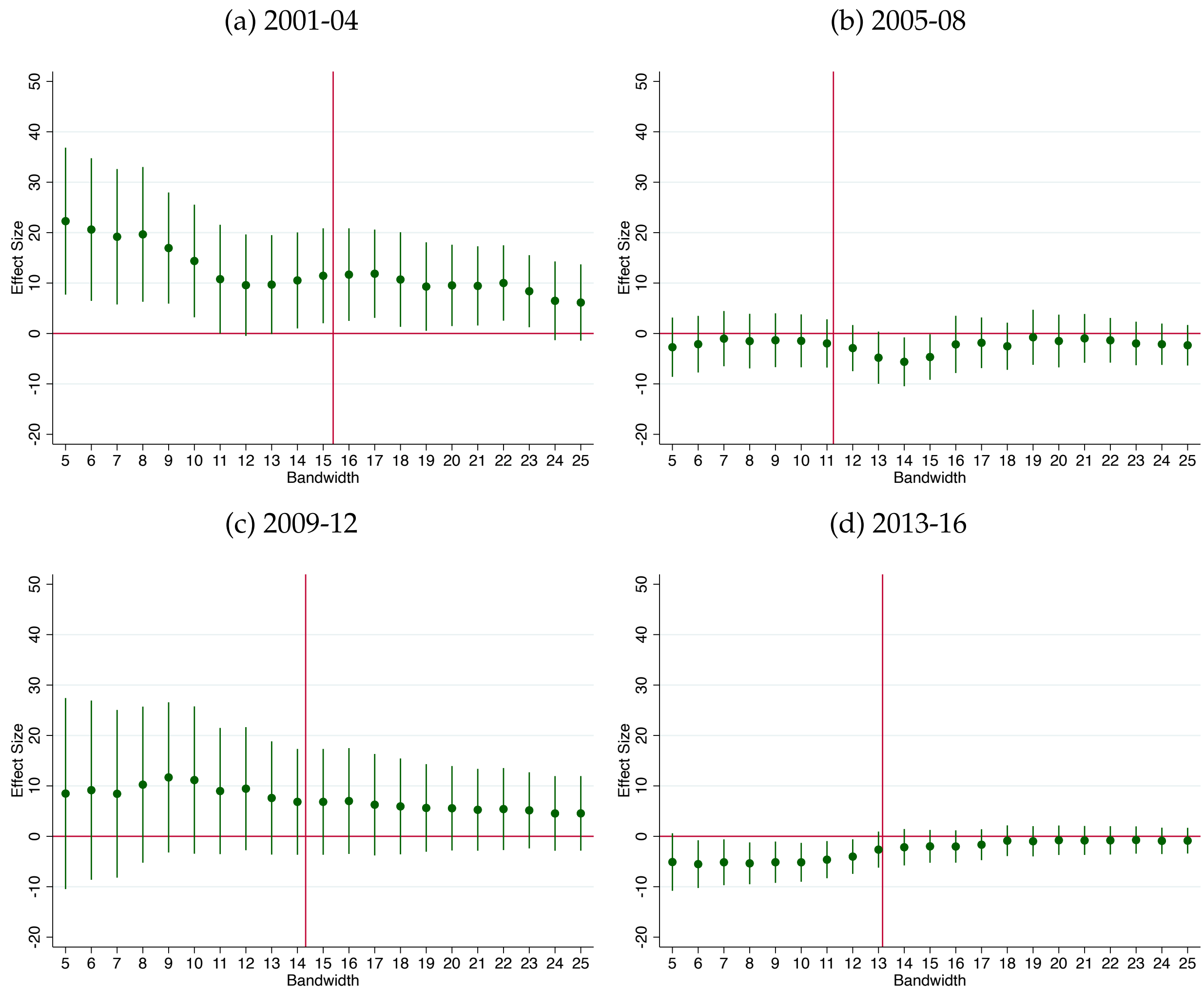
Notes: This Figure plots results from Equation (1) estimated for CO2e emissions. Each plot restricts observations to its specific term and within the 10 p.p. bandwidth. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Bands plot the 95% confidence intervals.

Figure 4: Regression-discontinuity effects of electing farmer mayors on deforestation with varying bandwidths



Notes: This Figure plots results from Equation (1) estimated for deforestation with varying bandwidths, as discussed in Section 5.1. Each plot restricts observations to its specific term. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Bands plot the 95% confidence intervals.

Figure 5: Regression-discontinuity effects of electing farmer mayors on CO2e emissions (in mm tons) with varying bandwidths



Notes: This Figure plots results from Equation (1) estimated for CO2e emissions with varying bandwidths, as discussed in Section 5.1. Each plot restricts observations to its specific term. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Vertical lines mark each panel's optimal bandwidth. Bands plot the 95% confidence intervals.

Online Appendix to "Cutting Special Interests by the Roots: Evidence from the Brazilian Amazon"

Table of Contents

A Theoretical Model

To motivate our empirical framework, we build a model in which incumbents implement policies that increase deforestation to attract support from voters and donors who benefit from forest clearing, thereby increasing their likelihood of being reelected. Our model is in the spirit of the political agency literature (Besley and Case, 1995; Coate and Morris, 1995; Banks and Sundaram, 1998; Ashworth, 2005; List and Sturm, 2006; Besley, 2006; Ashworth, 2012). We follow closely the approach to modeling multiple policy issues proposed by List and Sturm (2006) and Besley (2006).

Environment. We consider a model with two periods denoted by $t = \{1, 2\}$. In each period, an incumbent politician chooses the level of taxes and expenditures ("government size") and whether or not to implement policies to promote deforestation ("deforestation").

There are two types of voters – *ordinary* and *pro-deforestation* – representing shares ω and $1 - \omega$ of the electorate. Ordinary voters derive utility solely from government size. Their utility is $|g - g^*|\Gamma$ in which g is their preferred government size and g^* is the government size implemented by the politician in office. Pro-deforestation voters derive utility solely from "deforestation". Their utility is Δ if the politician in office implements pro-deforestation policies and 0 if not. Besides their preferences on public policies, voters receive a popularity shock δ for the incumbent. We let δ be uniformly distributed in the support $[-1/2\epsilon, +1/2\epsilon]$. Voters discount the future with discount factor $\beta \in (0, 1)$.

There are two types of politicians – *ordinary* and *farmers* – denoted by $p \in \{O, F\}$. Politicians' preferences on government size are public information, while their preferences on deforestation are private information. Preferences on government size are identical regardless of the type of politician. However, preferences on deforestation are heterogeneous depending on the type of politician. There is a probability π^p a politician is pro-deforestation and a probability $1 - \pi^p$ it is not. In the former case, implementing pro-deforestation policies is not costly. However, in the latter, it has a cost c drawn from a uniform distribution defined over the support $[0, C]$. Both types of politicians receive a rent R from holding office and discount the future with a discount factor $\beta \in (0, 1)$.

The differences in the share of pro-deforestation politicians across types might reflect two distinct mechanisms discussed in Section 3.1. First, politicians might have different in *preferences* regarding deforestation. Second, politicians from different groups might obtain different amounts in *monetary returns* they obtain from deforestation. Consistent with the discussion in the main text, we suppose suppose $\pi^F > \pi^O$. This effectively means that being a farmer signals whether the politician is pro-deforestation.[1]

Timing is as follows. In the beginning of period 1, nature draws the incumbent's type, her preferences regarding government size, and her cost shock c. The politician then decides which policies to implement and voters derive utility from them. At the end of period 1, politicians draw a popularity shock δ, and voters decide whether to reelect the incumbent or replace her by a randomly chosen opponent from the pool of politicians from the other type. In period 2, the politician in office decides which policies to implement, voters derive utility from them, and the game ends.

Equilibrium. We solve for the perfect Bayesian equilibrium of the game between voters and politicians. In this equilibrium, politicians and voters behave optimally in both periods. The politicians choose policies to maximize their expected utility given the reelection rule used by the voters. Voters decide whether to reelect the incumbent by comparing the expected utility from reelecting her conditional on the existing information with the expected utility of replacing her by a randomly chosen opponent of the other type. Voters use the policies implemented by the incumbent to infer their type (using Bayes' rule).

The decisions on government size are straightforward. Because the incumbent's preferences on government size are publicly known, ordinary voters decide whether to re-

[1] Our model encapsulates all heterogeneity across politician types in the share of pro-deforestation politicians, π^p. A more general approach would let types to index the full distribution of costs of implementing pro-deforestation policies. Results would be qualitatively identical.

elect her by checking whether the incumbent's preferences are closer to theirs than the preferences of a randomly chosen opponent. We denote the lead of the incumbent among ordinary voters by η.

The interesting behavior by politicians occurs regarding deforestation. In period 2, there is no strategic behavior and politicians implement their preferred policies. However, because the incumbent's preferences on deforestation are not publicly known, there are incentives for politicians to implement pro-deforestation policies in period 1 to attract pro-deforestation voters and increase her probability of reelection.

Let Π^p be the voters' belief that a politician of type p which implemented pro-deforestation policies in period 1 is pro-deforestation. Bayes' rule implies

$$\Pi^p = \frac{\pi^p}{\pi^p + (1 - \pi^p)\lambda^p} \tag{A.1}$$

in which λ^p is the probability that a politician of type p chooses pro-deforestation policies in period 1 when it is costly to her.

Pro-deforestation voters use this posterior Π^p and the prior $\pi^{p'}$ to compare the expected utility of reelecting the incumbent with the expected utility of replacing him by an opponent of type p'. Note that $\Pi^p > \pi^p$ for all λ. This implies that politicians build reputation among pro-deforestation voters by enacting policies that cater to their interests.

Because $\pi^F > \pi^O$, reputation building is more effective for farmers than for other politicians. Farmers always obtain an electoral advantage by enacting pro-deforestation policies because $\Delta\left(\pi^F - \pi^O\right) > 0$ regardless of λ^A. However, ordinary politicians only obtain an electoral advantage by enacting pro-deforestation policies if the following condition holds:

$$\lambda^O < \frac{\pi^0/(1-\pi^0)}{\pi^F/(1-\pi^F)}. \tag{A.2}$$

Equation (A.2) states that the signaling effect of enacting pro-deforestation policies for ordinary politicians must be strong enough to revert their disadvantage with pro-deforestation voters. If this does not occur, the reputational effect is negative, signaling is ineffective, and ordinary politicians do not implement pro-deforestation policies when it is costly for them.

Combining the decisions of the two types of voters, it is straightforward to see that an incumbent that implements pro-deforestation policies gets reelected if and only if $\omega\eta + (1-\omega)\Delta(\Pi^p - \pi^{p'}) + \delta > 0$. Conversely, an incumbent that does not implement these policies gets reelected if and only if $\omega\eta + \delta > 0$. Integrating over the distribution of δ, we find that the politician implements pro-deforestation policies if:

$$c < \beta R \epsilon (1-\omega)\Delta(\Pi^p - \pi^{p'}) \tag{A.3}$$

Integrating over the distribution of c, we obtain the following expression for the probability that a politician of type p chooses pro-deforestation policies when it is costly to her is:

$$\lambda^p = \left(\frac{\beta\epsilon R(1-\omega)\Delta}{C}\right)(\Pi^p - \pi^{p'}) = \Gamma(\Pi^p - \pi^{p'}), \tag{A.4}$$

Equation (A.4) states that the probability that a politician of type p chooses pro-deforestation policies when it is costly to her is the product of the share of pro-deforestation politicians, a measure of return of reelecting incumbent from the perspective of the voters ($\Delta(\Pi^p - \pi^{p'})$), and a measure of return being reelected from the perspective of the politicians ($\epsilon\beta R/C$).

Equations (A.1)-(A.4) enable us to characterize the equilibrium behavior of the politicians. Farmers implement pro-deforestation policies in period 1 if $c < \beta R\epsilon(1-\omega)\Delta(\pi^F - \pi^O)$ and are reelected with probability $1/2 + \omega\eta(1-\omega)\Delta(\pi^F - \pi^0)$. If Condition (A.2) is satisfied, ordinary politicians opposed to deforestation do not implement pro-deforestation policies and are reelected with probability $1/2 + \omega\eta$. Conversely, if Condition (A.2) is not satisfied, ordinary politicians opposed to deforestation implement pro-deforestation policies in period 1 if $c < \beta R\epsilon(1-\omega)\Delta(\Pi^O - \pi^F)$ and are reelected with probability $(1-\omega)\Delta(\Pi^O - \pi^F)$.

Using these equilibrium conditions, we obtain the following result:

Result 1. *The probability of enacting pro-deforestation policies and being reelected is higher for farmers than for other politicians.*

Proof. The probability a politician of type p chooses deforestation is:

$$p\left(D|p\right) = \pi^p + (1-\pi^p)\lambda^p \tag{A.5}$$

We will prove that $p\left(D|F\right) > p\left(D|O\right)$, that is, that farmers are more likely to enact pro-deforestation policies.

We derived the following expression for λ^p:

$$\lambda^p = \Gamma(\Pi^p - \pi^{p'}), \tag{A.6}$$

in which $\Gamma = \left(\beta\epsilon R(1-\omega)\Delta\right)/C$.

Define $x^p = \pi^p - \pi^{p'}$ as the difference between the probabilities of politicians of types p and p' being pro-deforestation. This difference is a sufficient statistic for the difference between politicians of types F and O. Using this definition and the definition of Π^p, it is possible to re-write (A.6) as

$$\lambda^p - \Gamma\left(\frac{\pi^p(1-\pi^p)(1-\lambda^p)}{\pi^p + (1-\pi^p)\lambda^p} + x^p\right) = 0 \tag{A.7}$$

Using the implicit function theorem, we obtain:

$$\frac{\partial \lambda^p}{\partial x^p} = \frac{\Gamma}{1 + \frac{\Gamma \pi^p (1-\pi^p)\lambda^p}{(\pi^p + (1-\pi^p)\lambda^p)^2}} > 0 \tag{A.8}$$

This implies that the share of politicians of type p who chooses pro-deforestation policies when it is costly to them is increasing in the difference in the probability politicians of groups p and p' are pro-deforestation. Because $x^F > 0 > x^O$, this implies $\lambda^F > \lambda^O$. Thus,

$$\begin{aligned} p\,(D|F) - p\,(D|O) &= \pi^F + (1-\pi^F)\lambda^F - \pi^O + (1-\pi^O)\lambda^O \\ &= \underbrace{x^F}_{>\,0}(1-\lambda^F) + (1-\pi^0)\underbrace{(\lambda^F - \lambda^O)}_{>0} > 0, \end{aligned} \tag{A.9}$$

Equation (A.9) completes the proof. ■

Result 1 establishes that farmers will deforest more and be reelected more often than the other politicians. This result reflects three theoretical mechanisms: *preferences*, *monetary returns* and *electoral returns*. The *preferences* and *monetary returns* channels are tied to the fact that farmers are more likely to be pro-deforestation, while the *electoral returns* channel is tied to the fact the increase in votingfrom implementing pro-deforestation policies is higher for farmers.

It is possible to use our model to evaluate the effects of the introduction of centralized conservation policies by the federal government. Conservation policies might reduce the returns from deforesting from the perspective of voters in the extensive margin through a decrease in the number of pro-deforestation voters ($\uparrow \omega$) and in the intensive margin through a decrease in the benefit these voters obtain from pro-deforestation policies ($\downarrow \Delta$). This implies the introduction of conservation polices reduces Γ. The following result establishes that the equilibrium effects of these policies.

Result 2. *The introduction of conservation policies influences the political equilibrium, reducing deforestation and reelection rates. Both effects are stronger in municipalities governed by farmers* *than in municipalities governed by ordinary politicians.*

Proof. We begin proving that the tightening (loosening) of conservation policies decrease (increase) deforestation. In our model, changes in conservation policies corresponds to changes in Γ. Thus, we will prove that

$$\frac{\partial p\left(D|p\right)}{\partial \Gamma} = (1-\pi^{p})\frac{\partial \lambda^{p}}{\partial \Gamma} > 0 \tag{A.10}$$

Thus, it suffices to prove that λ^{p} is increasing in Γ. Differentiating λ^{p} with respect to Γ, we obtain:

$$\frac{\partial \lambda^{p}}{\partial \Gamma} = \frac{\Pi^{p} - \pi^{p'}}{1+\frac{\Gamma\pi^{p}(1-\pi^{p})\lambda^{p}}{\left(\pi^{p}+(1-\pi^{p})\lambda^{p}\right)^{2}}} > 0 \tag{A.11}$$

The intuition of this result is straightforward. Tightening (loosening) conservation policies reduces (increases) the electoral incentives and increases (decreases) the costs of enacting pro-deforestation policies.

Notice that the fact that tightening (loosening) of conservation policies decreases (increases) deforestation in general implies directly that it reduces reelection rates. This comes from the fact that, in our model, reelection rates are a function of the extent of signaling using pro-deforestation policies. Thus, a reduction in the number of politicians signaling using pro-deforestation policies reduces reelection rates.

We then prove that the tightening (loosening) of conservation policies decreases (increases) deforestation more in municipalities governed by farmers. Because x^{p} is a sufficient statistic of the politician type, this is equivalent to proving that the effect of Γ on deforestation is increasing in x^{p}:

$$\frac{\partial^{2} p\left(D|p\right)}{\partial \Gamma \partial x^{p}} > 0 \tag{A.12}$$

Using the Chain rule, it is possible to write the derivative above as:

$$\frac{\partial^2 p\left(D|p\right)}{\partial\Gamma\partial\lambda^p} = (1-\pi^p)\left(\frac{\partial^2\lambda^p}{\partial\Gamma\partial x^p}\right) \tag{A.13}$$

Using the implicit function theorem, it is straightforward to show that

$$\frac{\partial^2\lambda^p}{\partial\Gamma\partial x^p} = \frac{1}{\left(\pi^p + (1-\pi^p)\lambda^p\right)^4} > 0 \tag{A.14}$$

Thus, the effect of Γ on deforestation is increasing in x^p. This establishes that the tightening (loosening) of conservation policies decreases (increases) deforestation more in municipalities governed by farmers. The intuition for this result is straightforward. Farmer politicians are more responsive to changes in the electoral returns of enacting pro-deforestation policies because a larger fraction of these politicians use deforestation to build reputation and increase their reelection odds.

Notice that the fact that the stronger effect on deforestation of tightening (loosening) on municipalities governed by farmers than in municipalities governed by other politicians implies that these municipalities experience a stronger reduction in reelection rates. As mentioned earlier, this comes from the fact that, in our model, reelection rates are a function of the extent of signaling using pro-deforestation policies. ■

Result 2 establishes that the introduction of conservation policies generates political spillovers. In terms of environmental outcomes, these spillovers reinforce the effects of conservation policies, further reducing deforestation, especially in municipalities governed by politicians connected to agricultural interests. In terms of political outcomes, these spillovers increase political turnover, in general, and reduce the competitiveness of politicians connected to agricultural interests, in particular.

Notice that this result was obtained assuming the share of pro-deforestation politicians is not affected by the introduction of stricter conservation policies, π^p. However, to the extent that the differences in this share reflect differences in monetary returns obtained from deforestation (as opposed to differences in intrinsic preferences), it is possible that conservation policies affect it. In particular, these policies might reduce the difference in pro-deforestation politicians between *farmers* and *ordinary* politicians as discussed in Section 3.1. This channel further reduces the difference in deforestation between municipalities governed by farmers and municipalities governed by other politicians. However, it does not generate the changes in the electoral competitiveness of farmer politicians the change in electoral returns does.

B Composition Effects

Section 3.2 discusses how the coefficients from Equation (1) are computed from comparisons between different groups of municipalities. This implies that these coefficients changing before and after the PPCDAm might simply reflect a change in sample composition, instead of real effects on the behavior of farmer politicians.

In this Section, we formalize this intuition and explain how we circumvent the composition problem with the empirical exercises estimated in Section 3.2. For simplicity, consider a version of Equation (1) without controls and estimated with two time periods, one before the PPCDAm and one after ($t = \{\text{pre}, \text{post}\}$). Divide the municipalities into four groups according to their treatment status in the two periods: $00, 01, 10, 11$. We refer to municipalities whose treatment status does not change (00 and 11) as *stayers* and municipalities whose treatment status changes (01 and 10) as *switchers*. Let N^{00}, N^{01}, N^{10}, and N^{11} denote the number of observations in each group.

It is possible to write the coefficients from Equation (1) as:

$$\widehat{\beta}_{\text{pre}} = s^{1}_{\text{pre}}\overline{Y}^{11}_{\text{pre}} + (1 - s^{1}_{\text{pre}})\overline{Y}^{10}_{\text{pre}} - s^{0}_{\text{pre}}\overline{Y}^{00}_{\text{pre}} - (1 - s^{0}_{\text{pre}})\overline{Y}^{01}_{\text{pre}}$$

$$\widehat{\beta}_{\text{post}} = s^1_{\text{post}}\overline{Y}^{11}_{\text{post}} + (1 - s^1_{\text{post}})\overline{Y}^{01}_{\text{post}} - s^0_{\text{post}}\overline{Y}^{00}_{\text{post}} - (1 - s^0_{\text{post}})\overline{Y}^{10}_{\text{post}},$$

where s^1_t is the share of stayers among municipalities governed by farmers and s^0_t is the share of stayers among municipalities not governed by farmers in period t.

The difference between coefficients pre and post the PPCDAm is:

$$\begin{aligned} \widehat{\beta}_{\text{post}} - \widehat{\beta}_{\text{pre}} = & \underbrace{\left[\left(s^1_{\text{post}}\overline{Y}^{11}_{\text{post}} - s^1_{\text{pre}}\overline{Y}^{11}_{\text{pre}}\right) - \left(s^0_{\text{post}}\overline{Y}^{00}_{\text{post}} - s^0_{\text{pre}}\overline{Y}^{11}_{\text{pre}}\right)\right]}_{\text{Effect on stayers}} + \\ & \underbrace{\left[\left((1 - s^1_{\text{post}})\overline{Y}^{01}_{\text{post}} - (1 - s^1_{\text{pre}})\overline{Y}^{10}_{\text{pre}}\right) - \left((1 - s^0_{\text{post}})\overline{Y}^{10}_{\text{post}} + (1 - s^1_{\text{pre}})\overline{Y}^{01}_{\text{post}}\right)\right]}_{\text{Effect on switchers}} \end{aligned} \tag{B.1}$$

Supposing s^1_t and s^0_t do not change over time ($s^1_t = s^1, s^0_t = s^0, \forall t$) simplifies this expression to:

$$\begin{aligned} \widehat{\beta}_{\text{post}} - \widehat{\beta}_{\text{pre}} = & \underbrace{\left[s^1\left(\overline{Y}^{11}_{\text{post}} - \overline{Y}^{11}_{\text{pre}}\right) - s^0\left(\overline{Y}^{00}_{\text{post}} - \overline{Y}^{00}_{\text{pre}}\right)\right]}_{\text{Effect on stayers}} + \\ & \underbrace{\left[(1 - s^1)\left(\overline{Y}^{01}_{\text{post}} - \overline{Y}^{10}_{\text{pre}}\right) - (1 - s^0)\left(\overline{Y}^{10}_{\text{post}} - \overline{Y}^{01}_{\text{pre}}\right)\right]}_{\text{Effect on switchers}} \end{aligned} \tag{B.2}$$

Equations (B.1) and (B.2) show that $\widehat{\beta}_{\text{post}} - \widehat{\beta}_{\text{pre}}$ is a combination of two difference-in-differences estimates computed on stayers and switchers weighted by their respective shares in the treatment and control groups in each period. Notice that changes in the group of municipalities governed by farmers influences the effect on switchers but not the effect on stayers. Thus, to understand whether our coefficients are influenced by changes in composition of what municipalities are governed by farmer politicians, we report estimates of $\widehat{\beta}_{\text{post}} - \widehat{\beta}_{\text{pre}}$ computed for stayers and switchers separately in the main text.

In practice, there are three periods post the PPCDAm, implying it would be possible to construct a total of sixteen groups according to the changes in treatment status of the municipalities. To keep the discussion as simple as possible, we focus on two period comparisons of the pre period (2001-04) and each of the post periods (2005-08, 2009-12, and 2013-16). We then we build six datasets composed by stayers and switchers in these two period comparisons (2001-04 and 2005-08, 2001-04 and 2009-12, or 2001-04 and 2013-16) and re-estimate Equation (1) on each of these six datasets.

C Additional Tables and Figures

Table C.1: Assessing variation in CO2e emissions and deforestation

	Total	Agriculture	Energy	Industry	LULUC	Residuals
	(1)	(2)	(3)	(4)	(5)	(6)
Deforestation x Amazon	23.93***	-0.69***	0.44*	0.07	24.11***	-0.00
	(3.66)	(0.19)	(0.24)	(0.06)	(3.70)	(0.07)
Deforestation	3.16***	0.35*	-0.39*	-0.07	3.27***	0.00
	(0.94)	(0.19)	(0.24)	(0.06)	(0.90)	(0.07)
Observations	83,398	83,398	83,398	83,398	83,398	83,398
R-squared	0.79	0.98	0.90	0.86	0.74	0.97
Mean	260.4	90.84	48.85	2.881	104	13.84
SD	1330	177.3	290.8	101.1	1197	102.7

Notes: This Table presents the association between CO2e emissions (measured in thousands of tons) and deforestation (measured in km^2) in Brazil. All regressions include year and municipality fixed effects and controls. Controls include population, area, tree cover area, and GDP per capita. Robust standard errors in parenthesis. p<0.01 ***, p < 0.05 **, p < 0.1 *.

Table C.2: Descriptive statistics of farmers and non-farmer candidates for mayor in elections

	Pre		Post	
	Farmer	Non-Farmer	Farmer	Non-Farmer
Age	49.49	46.33	49.45	46.72
Male	.97	.89	.95	.85
Primary School	.14	.09	.15	.07
High School	.25	.28	.34	.28
College	.05	.35	.1	.43
Married	.78	.73	.75	.69
Incumbent at Office	.23	.19	.11	.16
Left	.17	.19	.26	.31

Notes: This Table presents descriptive statistics on individual candidates in elections pre-PPCDAm (2000) and post (2004, 2008, 2012). Data sources are discussed in Section 4.1.

Table C.3: Occupation transition matrix

	Art	Bureaucrat	Business	Domestic	Driver	Education	Environment	Farmer	Health	Journalism	Law	Missing	Other	Politician	Retired	Security	Sports	Technician
Art	0.37	0.06	0.12	0.01	0.02	0.03	0.00	0.03	0.01	0.01	0.00	0.01	0.19	0.05	0.02	0.01	0.00	0.07
Bureaucrat	0.00	0.47	0.07	0.01	0.04	0.07	0.00	0.03	0.03	0.00	0.01	0.01	0.10	0.08	0.04	0.03	0.00	0.03
Business	0.00	0.04	0.54	0.01	0.02	0.02	0.00	0.06	0.01	0.00	0.01	0.01	0.11	0.10	0.02	0.00	0.00	0.04
Domestic	0.00	0.07	0.10	0.37	0.00	0.03	0.00	0.08	0.02	0.00	0.00	0.01	0.18	0.06	0.04	0.00	0.00	0.03
Driver	0.00	0.10	0.10	0.00	0.43	0.01	0.00	0.09	0.00	0.00	0.00	0.01	0.11	0.09	0.03	0.00	0.00	0.02
Education	0.00	0.11	0.06	0.01	0.00	0.56	0.00	0.03	0.01	0.00	0.01	0.01	0.07	0.07	0.03	0.00	0.00	0.02
Environment	0.00	0.07	0.11	0.00	0.03	0.01	0.04	0.40	0.00	0.00	0.00	0.00	0.12	0.13	0.01	0.02	0.00	0.03
Farmer	0.00	0.04	0.06	0.00	0.02	0.01	0.00	0.64	0.00	0.00	0.00	0.01	0.08	0.10	0.02	0.00	0.00	0.02
Health	0.00	0.08	0.02	0.01	0.00	0.01	0.00	0.01	0.71	0.00	0.00	0.01	0.05	0.06	0.02	0.01	0.00	0.01
Journalism	0.01	0.03	0.14	0.00	0.00	0.02	0.00	0.01	0.00	0.51	0.02	0.01	0.11	0.09	0.01	0.00	0.00	0.01
Law	0.00	0.03	0.03	0.00	0.00	0.01	0.00	0.01	0.00	0.00	0.79	0.01	0.02	0.07	0.02	0.00	0.00	0.01
Missing	0.01	0.12	0.18	0.02	0.04	0.06	0.00	0.15	0.03	0.00	0.02	0.03	0.13	0.09	0.04	0.02	0.00	0.07
Other	0.01	0.10	0.20	0.02	0.04	0.04	0.00	0.12	0.02	0.00	0.01	0.02	0.20	0.09	0.04	0.02	0.00	0.09
Politician	0.00	0.08	0.16	0.01	0.03	0.04	0.00	0.15	0.02	0.00	0.02	0.01	0.09	0.28	0.04	0.01	0.00	0.04
Retired	0.00	0.04	0.06	0.01	0.01	0.03	0.00	0.04	0.01	0.00	0.01	0.01	0.11	0.07	0.55	0.01	0.00	0.03
Security	0.00	0.10	0.03	0.00	0.01	0.01	0.00	0.01	0.02	0.00	0.01	0.01	0.07	0.06	0.05	0.62	0.00	0.01
Sports	0.00	0.08	0.12	0.01	0.01	0.16	0.01	0.01	0.01	0.00	0.00	0.02	0.26	0.09	0.01	0.02	0.16	0.02
Technician	0.00	0.06	0.12	0.01	0.02	0.01	0.00	0.04	0.01	0.00	0.00	0.01	0.15	0.07	0.03	0.00	0.00	0.47

Notes: This Table presents descriptive statistics on the occupational transition patterns of candidates in Brazil between the 2000 and 2004 elections, as discussed in Section 4.1.

Table C.4: Testing continuity of covariates

	ln(Population)	ln(Area)	ln(Forest Area)	ln(GDP p.c.)	% Protected Area	Ever Priority	Agricutural Frontier	Amazon Biome	Farmer Candidates
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Pre									
Elected Farmer	0.52	0.35	0.78	-0.21	0.72	0.20	0.11	0.27	-0.23
	(0.36)	(0.58)	(0.58)	(0.33)	(9.84)	(0.14)	(0.25)	(0.22)	(0.44)
Observations	55	56	56	55	56	56	56	56	56
Mean	9.290	7.990	7.210	8.530	5.850	0.0400	0.790	0.710	1.320
SD	0.690	1.120	1.260	0.660	13.59	0.190	0.420	0.460	0.720
Panel B: Post									
Elected Farmer	0.20	0.14	0.13	0.35	-0.66	0.01	0.01	-0.13	0.06
	(0.27)	(0.35)	(0.42)	(0.21)	(5.13)	(0.09)	(0.12)	(0.12)	(0.11)
Observations	190	190	190	190	190	190	190	190	190
Mean	9.360	7.920	7.230	10.14	8.840	0.0900	0.760	0.800	1.160
SD	0.990	1.070	1.360	0.710	21.84	0.280	0.430	0.400	0.400

Notes: This Table presents results from Equation (1) estimated for covariates as discussed in Section 4.2. Each regression restricts observations to being within the 10 p.p. vote share bandwidth. Robust standard errors in parenthesis. p<0.01 ***, p < 0.05 **, p < 0.1 *.

Table C.5: Sample selection

	ln(Population)	ln(Area)	ln(Forest Area)	ln(GDP p.c.)	% Protected Area	Ever Priority	Agr. Frontier	Amazon Biome	Farmer Candidates
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A									
Post x In Sample	-0.00	-0.12	-0.08	0.00	-0.78	-0.01	-0.03	0.08*	0.15***
	(0.10)	(0.12)	(0.15)	(0.07)	(1.83)	(0.03)	(0.04)	(0.04)	(0.05)
In Sample	-0.19**	0.19*	0.18	0.17***	-3.95***	0.02	0.04	0.03	0.74***
	(0.08)	(0.10)	(0.13)	(0.06)	(1.50)	(0.02)	(0.03)	(0.04)	(0.04)
Post	0.13**	0.03	0.02	1.57***	0.53	-0.00	0.00	-0.02	-0.19***
	(0.05)	(0.07)	(0.08)	(0.03)	(0.90)	(0.01)	(0.01)	(0.02)	(0.02)
Observations	3,070	3,084	3,084	3,067	3,860	3,860	3,860	3,855	3,719
R-squared	0.01	0.00	0.00	0.45	0.00	0.00	0.00	0.00	0.23
Mean	9.490	7.830	7.160	8.380	11.29	0.0700	0.720	0.690	0.260
SD	1.080	1.410	1.680	0.620	24.88	0.250	0.450	0.460	0.650
Panel B									
Post x Close Election	0.08	0.07	0.11	-0.01	-0.56	0.02	-0.09**	0.06	0.03
	(0.11)	(0.14)	(0.18)	(0.09)	(2.11)	(0.03)	(0.04)	(0.05)	(0.05)
Close Election	-0.27***	-0.04	-0.16	0.06	-3.64***	-0.01	0.09***	-0.04	0.80***
	(0.08)	(0.08)	(0.10)	(0.05)	(1.23)	(0.01)	(0.02)	(0.03)	(0.03)
Post	0.12***	-0.01	-0.01	1.57***	0.77	-0.00	0.02	-0.01	-0.10***
	(0.05)	(0.06)	(0.07)	(0.03)	(0.82)	(0.01)	(0.01)	(0.02)	(0.02)
Observations	3,024	3,036	3,036	3,021	3,719	3,719	3,719	3,718	3,719
R-squared	0.01	0.00	0.00	0.45	0.00	0.00	0.00	0.00	0.14
Mean	9.600	7.840	7.150	9.950	11.88	0.0700	0.770	0.650	0.180
SD	1.090	1.390	1.650	0.780	26	0.250	0.420	0.480	0.520

Notes: This Table presents results on whether baseline characteristics of municipalities in samples before and after the PPCDAm change, as discussed in Section 4.2. Robust standard errors in parenthesis. p<0.01 ***, p < 0.05 **, p < 0.1 *.

Table C.6: Robustness to alternative deforestation outcomes

	Log	Normalized	Log Odds Ratio	PRODES	MapBiomas
	(1)	(2)	(3)	(4)	(5)
Panel A: Cross-section					
2001-04	0.42***	0.18**	0.43***	84.63*	128.78***
	(0.14)	(0.09)	(0.14)	(50.72)	(41.45)
2005-08	0.44***	0.10*	0.45***	11.55	15.07
	(0.12)	(0.06)	(0.12)	(13.04)	(16.48)
2009-12	0.29***	-0.02	0.29***	22.91*	51.57*
	(0.11)	(0.06)	(0.11)	(13.07)	(30.31)
2013-16	-0.07	-0.27**	-0.07	7.51	-12.68
	(0.12)	(0.11)	(0.12)	(12.91)	(32.11)
Δ *(2005-08) - (2001-04)*	0.02	-0.08	0.02	-73.07	-113.71***
	(0.18)	(0.11)	(0.19)	(51.39)	(43.72)
Δ *Post - Pre*	-0.16	-0.22**	-0.17	-70.49	-109.34**
	(0.16)	(0.10)	(0.16)	(50.41)	(43.02)
Total observations	2534	2534	2534	2534	2534
Distinct municipalities	644	644	644	644	644
Mean Pre	3.08	0.32	-4.80	207.08	191.98
SD Pre	2.10	0.89	1.70	387.76	338.05
Panel B: Regression Discontinuity					
2001-04	0.70	0.28	0.72	556.90***	252.41**
	(0.63)	(0.45)	(0.64)	(129.44)	(126.11)
2005-08	-0.49	-0.52*	-0.52	-15.58	30.32
	(0.55)	(0.30)	(0.56)	(39.68)	(73.06)
2009-12	0.03	-0.11	0.03	88.29	311.40
	(0.51)	(0.26)	(0.52)	(92.45)	(203.57)
2013-16	-0.27	0.65	-0.26	23.23	-50.96
	(0.51)	(0.48)	(0.52)	(34.37)	(91.57)
Δ *(2005-08) - (2001-04)*	-1.19	-0.80	-1.24	-572.48***	-222.09
	(0.84)	(0.54)	(0.85)	(133.29)	(144.93)
Δ *Post - Pre*	-0.97	-0.31	-1.01	-531.15***	-170.84
	(0.69)	(0.49)	(0.70)	(131.69)	(148.59)
Total observations	246	246	246	246	246
Distinct municipalities	195	195	195	195	195
Mean Pre	3.11	0.22	-4.80	76.55	141.93
SD Pre	1.99	0.80	1.62	140.02	188.88

Notes: This Table presents results from Equation (1) estimated for different measures of deforestation, as discussed in Section 5.1. Panel B restricts observations to being within the 10 p.p. vote share bandwidth. Controls include logs of population, area, tree cover area in 2000, and GDP per capita in 2000. Robust standard errors in parenthesis. p<0.01 ***, p < 0.05 **, p < 0.1 *.

Table C.7: Placebo with municipalities inside the Cerrado biome but outside the Legal Amazon

	Cross Section	Switchers	Constant Selection	RD
	(1)	(2)	(3)	(4)
Panel A: Deforestation (km^2)				
2001-04	-1.73***	-0.05	-1.55**	-9.25**
	(0.64)	(0.33)	(0.74)	(4.08)
2005-08	-0.88	0.18	-2.14**	-0.78
	(0.64)	(0.26)	(0.83)	(1.29)
2009-12	-0.63	-0.58	-2.56**	-1.08
	(0.70)	(0.67)	(1.04)	(1.64)
2013-16	-1.08	-1.73	-1.39	-1.36
	(0.71)	(1.37)	(1.02)	(1.42)
Δ *(2005-08) - (2001-04)*	0.85	0.23	-0.59	8.47**
	(0.88)	(0.39)	(0.73)	(4.16)
Δ *Post - Pre*	0.87	-0.66	-0.48	8.19*
	(0.74)	(0.63)	(0.61)	(4.19)
Total observations	3277	4925	3593	411
Distinct municipalities	830	830	743	313
Municipality FE	-	✓	-	-
Mean Pre	3.91	3.91	4.52	3.29
SD Pre	12.29	12.29	13.34	10.99
Panel B: CO2e Emissions (mm tons)				
2001-04	-0.15	-0.04	-0.14	-0.48
	(0.10)	(0.03)	(0.11)	(0.60)
2005-08	-0.07	-0.02	-0.17	-0.13
	(0.07)	(0.05)	(0.12)	(0.24)
2009-12	-0.09	0.07	-0.35**	0.09
	(0.09)	(0.07)	(0.15)	(0.37)
2013-16	-0.07	-0.04	-0.09	-0.13
	(0.07)	(0.08)	(0.12)	(0.29)
Δ *(2005-08) - (2001-04)*	0.08	0.02	-0.03	0.35
	(0.12)	(0.06)	(0.11)	(0.64)
Δ *Post - Pre*	0.08	0.05	-0.06	0.41
	(0.11)	(0.05)	(0.07)	(0.63)
Total observations	3277	4925	3593	411
Distinct municipalities	830	830	743	313
Municipality FE	-	✓	-	-
Mean Pre	1.04	1.04	1.07	1.06
SD Pre	1.88	1.88	2.02	1.59

Notes: This Table presents results from Equation (1) estimated for deforestation and CO2e emissions with the sample of municipalities restricted to those inside the Cerrado biome and outside the Legal Amazon. Column 4 restricts observations to being within the 10 p.p. vote share bandwidth. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Robust standard errors in parenthesis. p<0.01 ***, p < 0.05 **, p < 0.1 *.

Table C.8: Effects of electing farmer mayors on agriculture and matching grants

	Agriculture			Matching Grants			
	Area Planted	Value	Cattle	Total p.c.	% Agriculture	% Education	% Health
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Cross-section							
2001-04	-0.01	-0.90	5.26	-58.64*	-2.39	1.14	1.50
	(0.76)	(1.03)	(3.39)	(31.96)	(1.69)	(1.04)	(1.54)
2005-08	0.16	-0.79	8.77**	-14.49	1.62	-0.35	0.67
	(0.88)	(1.18)	(3.83)	(45.52)	(1.99)	(1.40)	(1.65)
2009-12	-0.64	-1.97	7.74**	2.53	2.02	0.53	-0.97
	(0.93)	(1.64)	(3.82)	(55.32)	(2.53)	(1.75)	(1.67)
2013-16	3.36*	11.21**	-1.48	22.28	4.52	1.36	0.06
	(1.94)	(5.12)	(4.83)	(112.93)	(4.37)	(0.84)	(1.76)
Δ *(2005-08) - (2001-04)*	0.17	0.11	3.51	44.14	4.00	-1.49	-0.83
	(1.15)	(1.52)	(5.10)	(54.68)	(2.56)	(1.74)	(2.25)
Δ *Post - Pre*	0.76	2.91	0.47	59.34	4.87**	-0.77	-1.53
	(1.03)	(1.86)	(4.13)	(50.65)	(2.31)	(1.34)	(1.82)
Total observations	2532	2532	2534	2534	2406	2406	2406
Distinct municipalities	644	644	644	644	644	644	644
Mean Pre	4.14	4.65	30.28	222.40	12.74	4.98	22.00
SD Pre	7.60	10.31	34.17	319.09	17.89	8.83	14.73
Panel B: Regression Discontinuity							
2001-04	-7.68	-7.77	16.52	23.33	21.38*	-7.19	-1.82
	(4.73)	(5.41)	(12.48)	(112.03)	(11.25)	(6.00)	(9.65)
2005-08	4.53	1.65	-13.55	331.62	-8.01	4.93	-6.18
	(7.01)	(8.96)	(14.98)	(202.32)	(10.20)	(5.63)	(6.86)
2009-12	-11.75	-19.58	32.57*	-393.98	8.35	-2.20	7.56
	(9.14)	(15.43)	(18.71)	(307.61)	(7.13)	(7.60)	(8.66)
2013-16	5.92	23.84**	-23.69	-201.91	5.09	0.60	2.72
	(4.36)	(11.66)	(16.65)	(130.57)	(13.69)	(1.37)	(8.26)
Δ *(2005-08) - (2001-04)*	12.21	9.42	-30.07	308.29	-29.39*	12.12	-4.36
	(8.56)	(10.53)	(19.48)	(233.85)	(15.62)	(8.01)	(11.88)
Δ *Post - Pre*	8.16	10.77	-20.62	-65.81	-20.75	8.76	2.27
	(5.93)	(8.57)	(15.41)	(176.79)	(12.96)	(6.67)	(10.71)
Total Observations	246	246	246	246	233	233	233
Distinct municipalities	195	195	195	195	189	189	189
Mean Pre	6.10	10.85	44.79	410.49	17.04	7.29	13.19
SD Pre	13.73	23.77	38.38	541.84	24.07	11.00	15.77

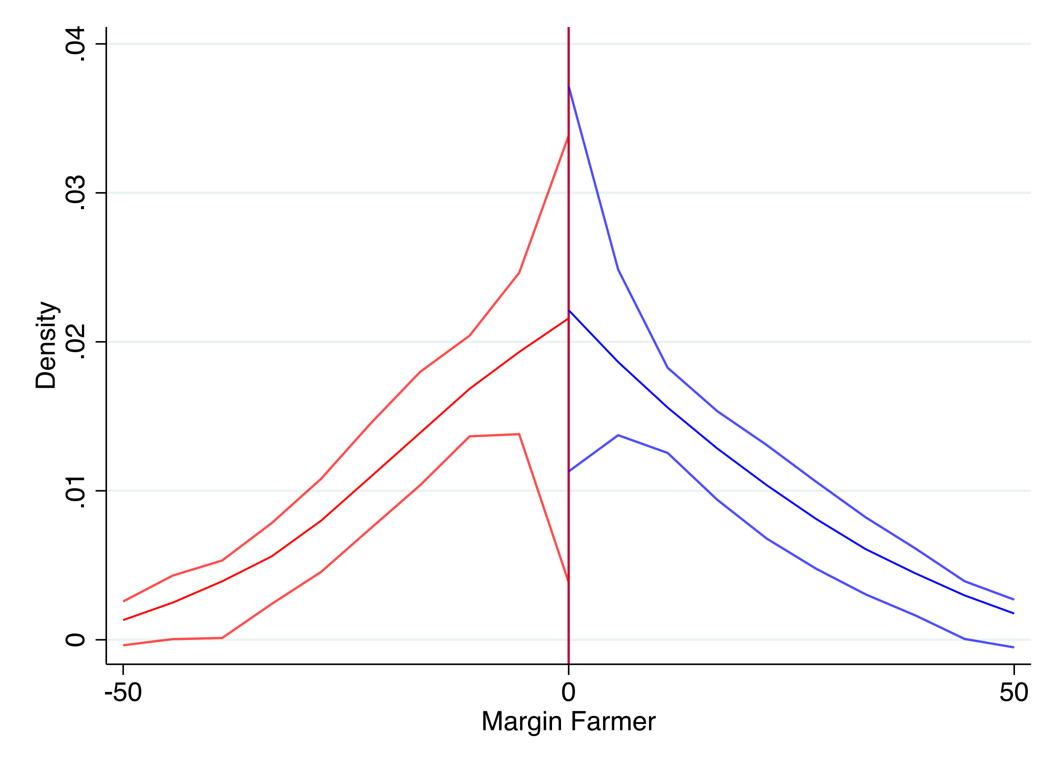
Notes: This Table presents results from Equation (1) estimated for patterns of agricultural outcomes and discretionary matching grants. Agricultural outcomes are normalized by area in km^2. Panel B restricts observations to being within the 10 p.p. vote share bandwidth. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Robust standard errors in parenthesis. p<0.01 ***, p < 0.05 **, p < 0.1 *.

Table C.9: Effects of electing farmer mayors on politics

	% Farmer Cand. Next Term	% Vote Farmer Next Term	Farmer Next Term	Incumbent Reelected
	(1)	(2)	(3)	(4)
Panel A: Cross-Section				
2001-04	12.73***	14.13***	0.20***	0.02
	(3.16)	(3.43)	(0.05)	(0.04)
2005-08	10.54***	10.99***	0.10**	-0.07
	(2.82)	(3.08)	(0.04)	(0.05)
2009-12	6.04**	6.60**	0.07*	-0.10**
	(2.72)	(3.00)	(0.04)	(0.05)
2013-16	4.25	6.56*	0.11**	-0.03
	(3.30)	(3.70)	(0.05)	(0.05)
Δ *(2005-08) - (2001-04)*	-2.19	-3.14	-0.10	-0.08
	(4.20)	(4.56)	(0.07)	(0.06)
Δ *Post - Pre*	-5.36	-6.28*	-0.11*	-0.05
	(3.41)	(3.75)	(0.06)	(0.05)
Total observations	2492	2492	2409	2409
Distinct municipalities	644	644	643	643
Mean Pre	16.87	17.52	0.18	0.23
SD Pre	25.45	27.56	0.38	0.42
Panel B: Regression Discontinuity				
2001-04	7.19	-2.26	0.08	-0.22
	(13.79)	(17.25)	(0.27)	(0.24)
2005-08	-5.60	-6.89	-0.13	0.11
	(11.53)	(13.56)	(0.19)	(0.20)
2009-12	-0.03	0.01	-0.09	0.05
	(13.40)	(15.46)	(0.25)	(0.17)
2013-16	-4.37	-3.71	-0.17	0.24
	(10.64)	(13.11)	(0.26)	(0.21)
Δ *(2005-08) - (2001-04)*	-12.79	-4.62	-0.21	0.33
	(17.84)	(21.74)	(0.33)	(0.31)
Δ *Post - Pre*	-10.83	-1.65	-0.22	0.36
	(15.18)	(18.84)	(0.30)	(0.26)
Total Observations	240	240	237	237
Distinct municipalities	190	190	187	187
Mean Pre	23.45	24.32	0.25	0.22
SD Pre	25.37	28.50	0.44	0.42

Notes: This Table presents results from Equation (1) estimated for different measures of political performance, as discussed in Section 5.1. Panel B restricts observations to being within the 10 p.p. vote share bandwidth. Controls include logs of population, area, tree cover area in 2000, and GDP per capita in 2000; and a dummy for current mayor being an incumbent. Robust standard errors in parenthesis. p<0.01 ***, p < 0.05 **, p < 0.1 *.

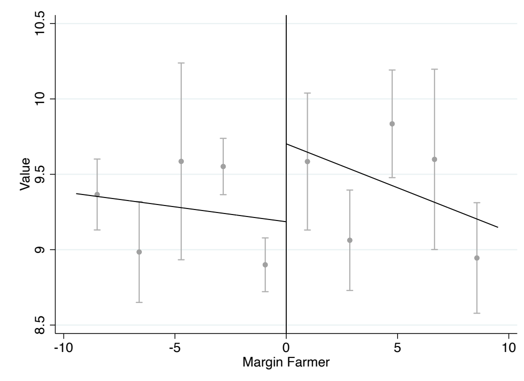
Figure C.1: Farmer vote share density test



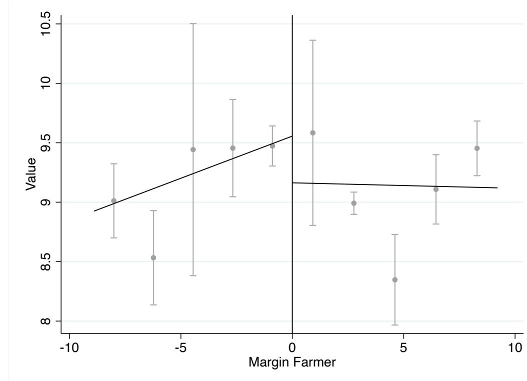
Notes: This Figure plots the density test proposed by Cattaneo et al. (2020) estimated on farmer candidates' vote share, as discussed in Section 4.2.

Figure C.2: Continuity test for covariates: ln(Population)

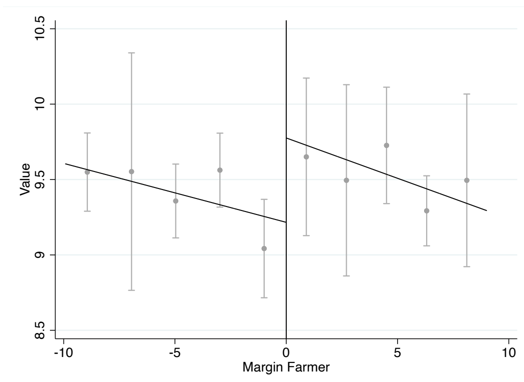
(a) 2001-04



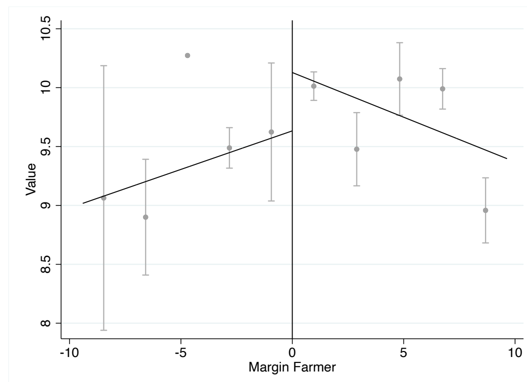
(b) 2005-08



(c) 2009-12



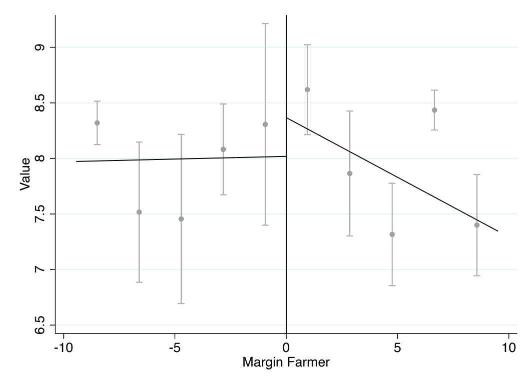
(d) 2013-16



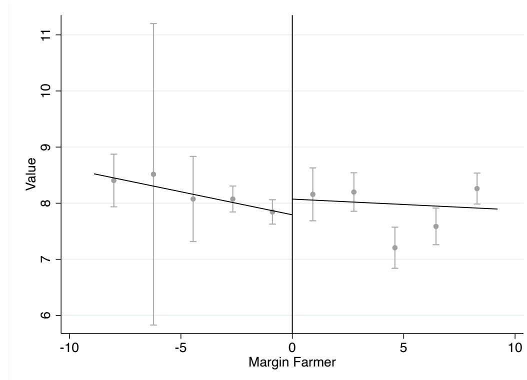
Notes: This Figure plots results from Equation (1) estimated for controls. Each plot restricts observations to its specific term and within the 10 p.p. bandwidth. Bands plot the 95% confidence intervals.

Figure C.3: Continuity test for covariates: ln(Area)

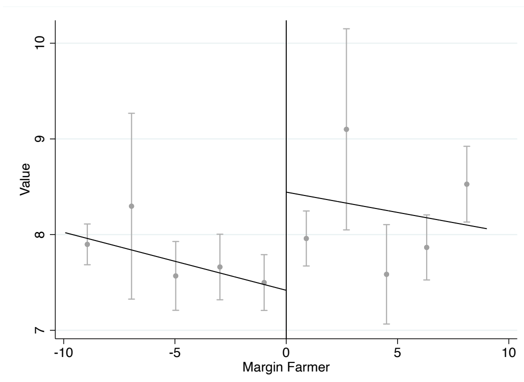
(a) 2001-04



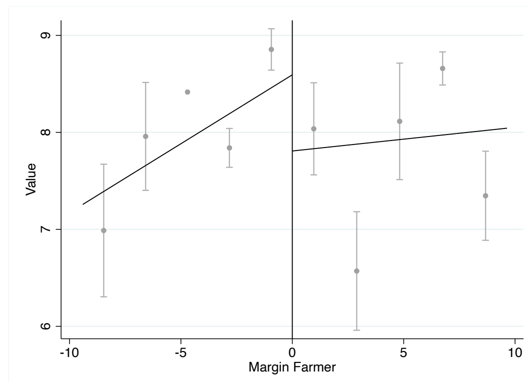
(b) 2005-08



(c) 2009-12



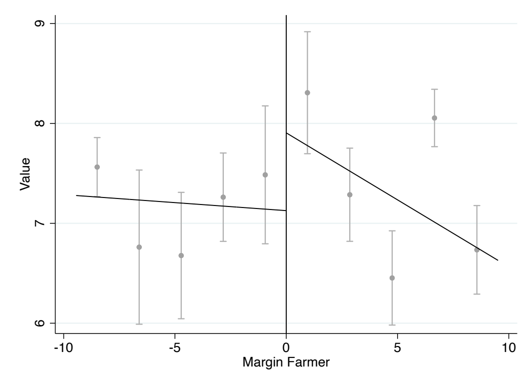
(d) 2013-16



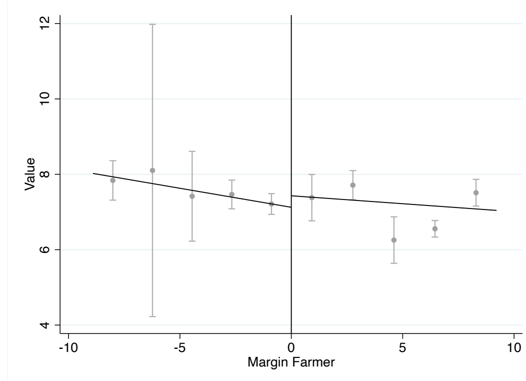
Notes: This Figure plots results from Equation (1) estimated for controls. Each plot restricts observations to its specific term and within the 10 p.p. bandwidth. Bands plot the 95% confidence intervals.

Figure C.4: Continuity test for covariates: ln(Tree Cover in 2000)

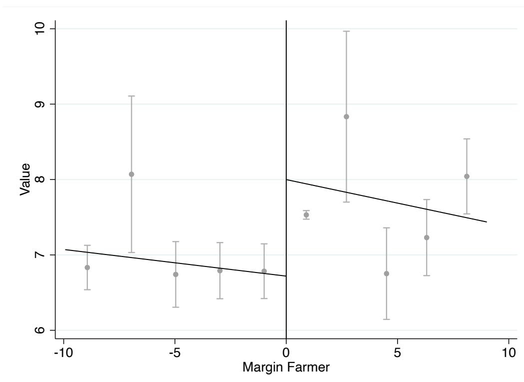
(a) 2001-04



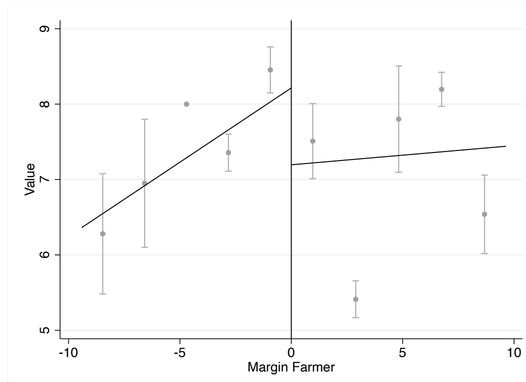
(b) 2005-08



(c) 2009-12



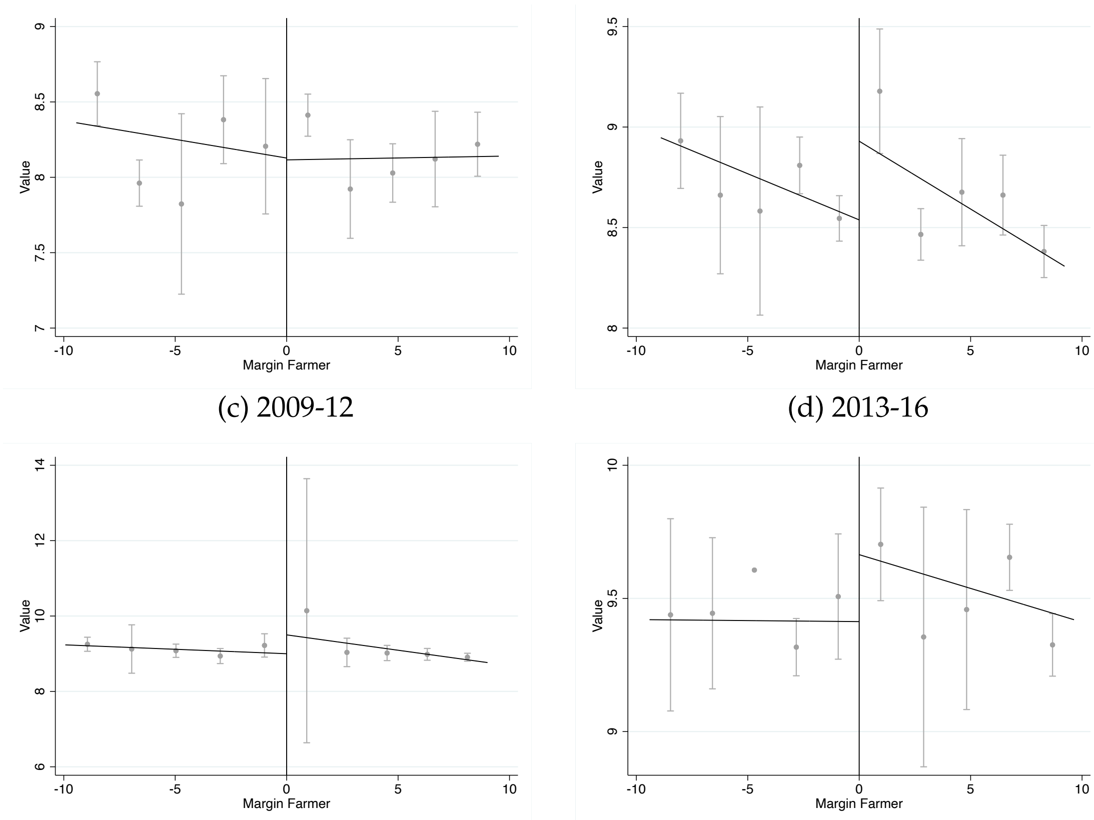
(d) 2013-16



Notes: This Figure plots results from Equation (1) estimated for controls. Each plot restricts observations to its specific term and within the 10 p.p. bandwidth. Bands plot the 95% confidence intervals.

Figure C.5: Continuity test for covariates: ln(GDP per capita)

(a) 2001-04 (b) 2005-08

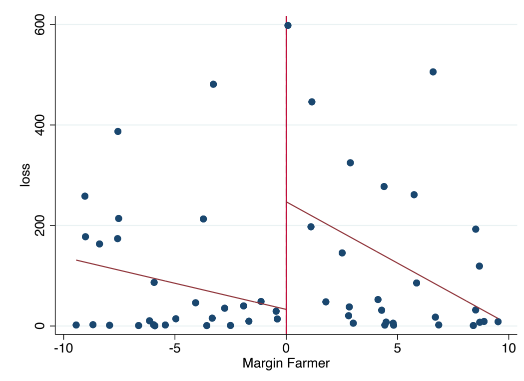


(c) 2009-12 (d) 2013-16

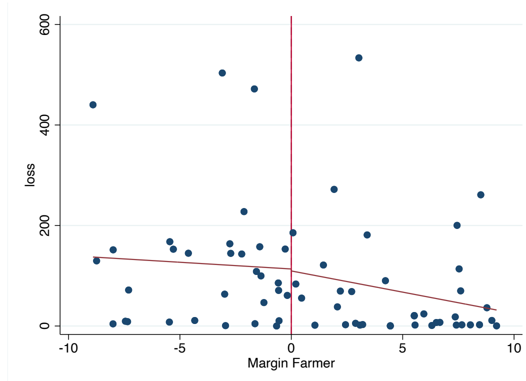
Notes: This Figure plots results from Equation (1) estimated for controls. Each plot restricts observations to its specific term and within the 10 p.p. bandwidth. Bands plot the 95% confidence intervals.

Figure C.6: Regression discontinuity raw data scatter plot for deforestation (km^2)

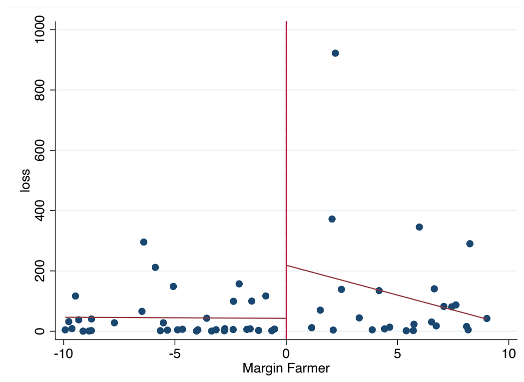
(a) 2001-04



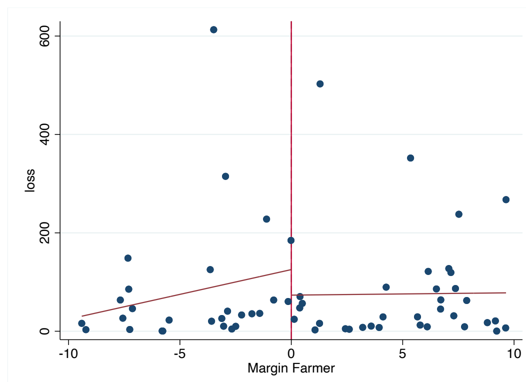
(b) 2005-08



(c) 2009-12

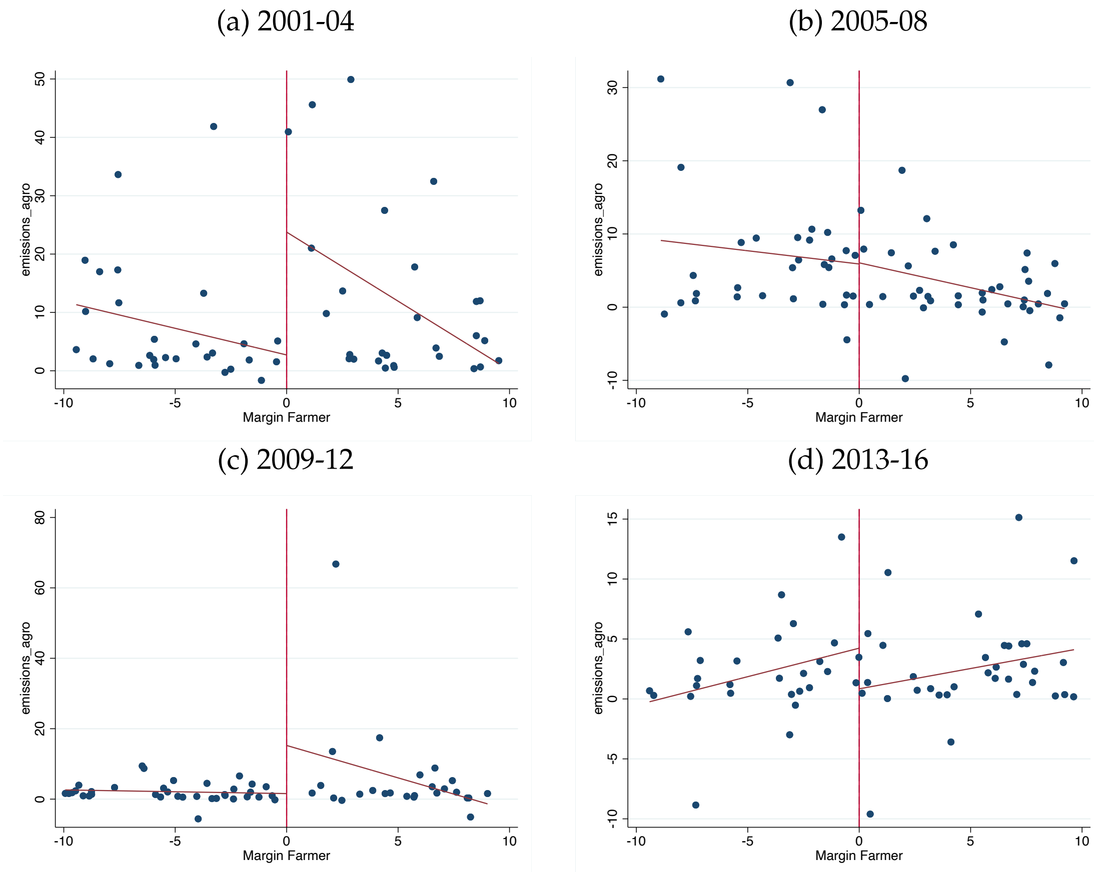


(d) 2013-16



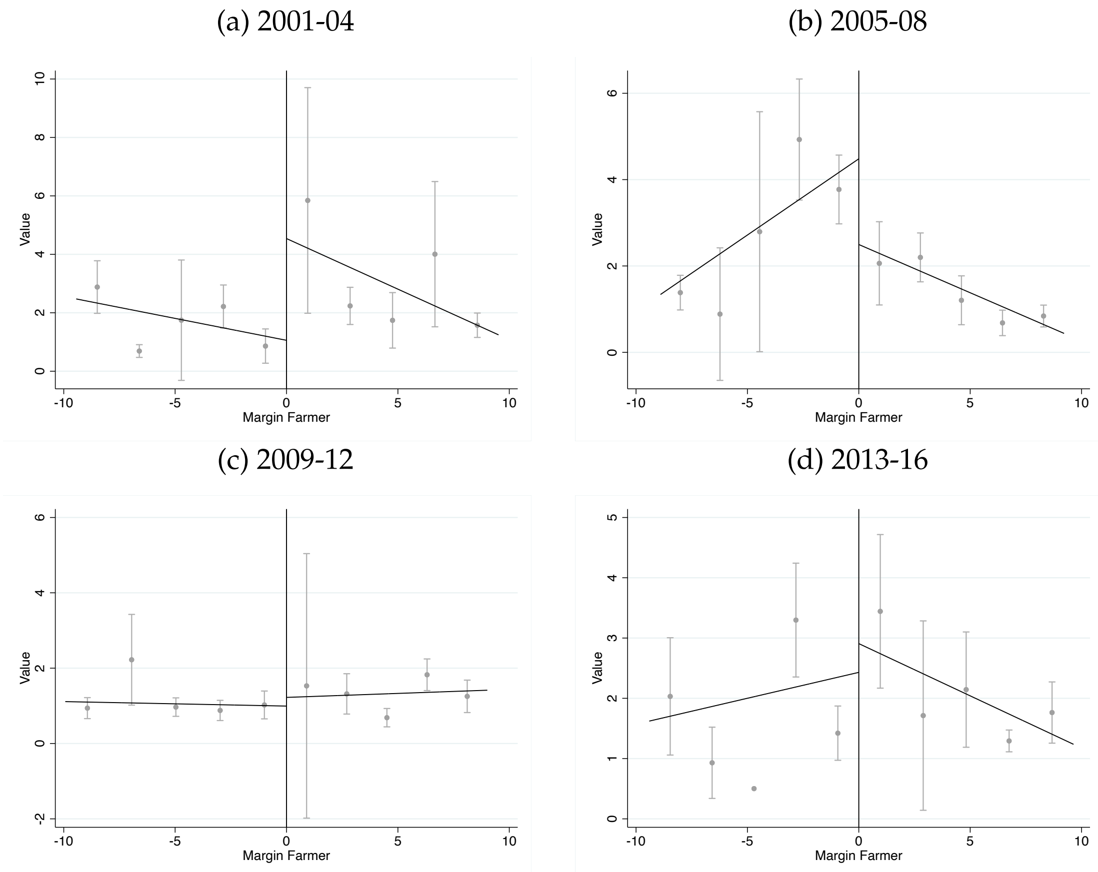
Notes: This Figure plots the raw data on deforestation within the 10 p.p. vote share bandwidth for each electoral term.

Figure C.7: Regression discontinuity raw data scatter plot for CO2e emissions (mm tons)



Notes: This Figure plots the raw data on CO2 emissions within the 10 p.p. vote share bandwidth for each electoral term.

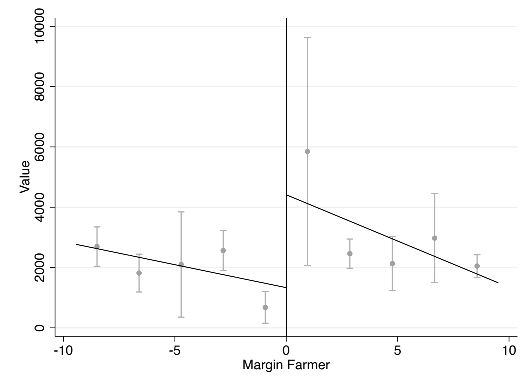
Figure C.8: Regression-discontinuity effects of electing farmer mayors on deforestation (% area)



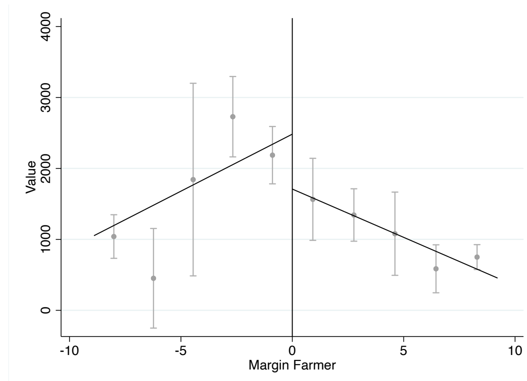
Notes: This Figure plots results from Equation (1) estimated for normalized deforestation. Each plot restricts observations to its specific term and within the 10 p.p. bandwidth. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Bands plot the 95% confidence intervals.

Figure C.9: Regression-discontinuity effects of electing farmer mayors on CO2e emissions (1,000 tons/km^2)

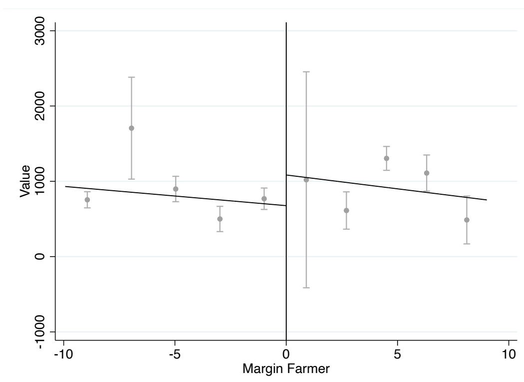
(a) 2001-04



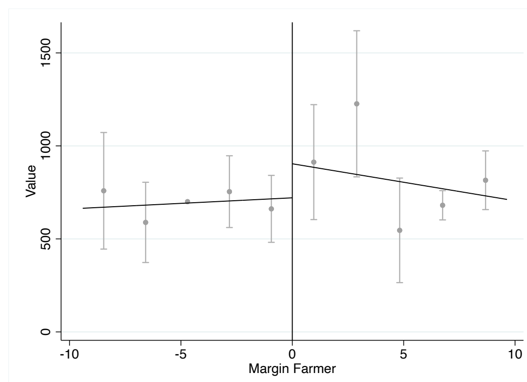
(b) 2005-08



(c) 2009-12

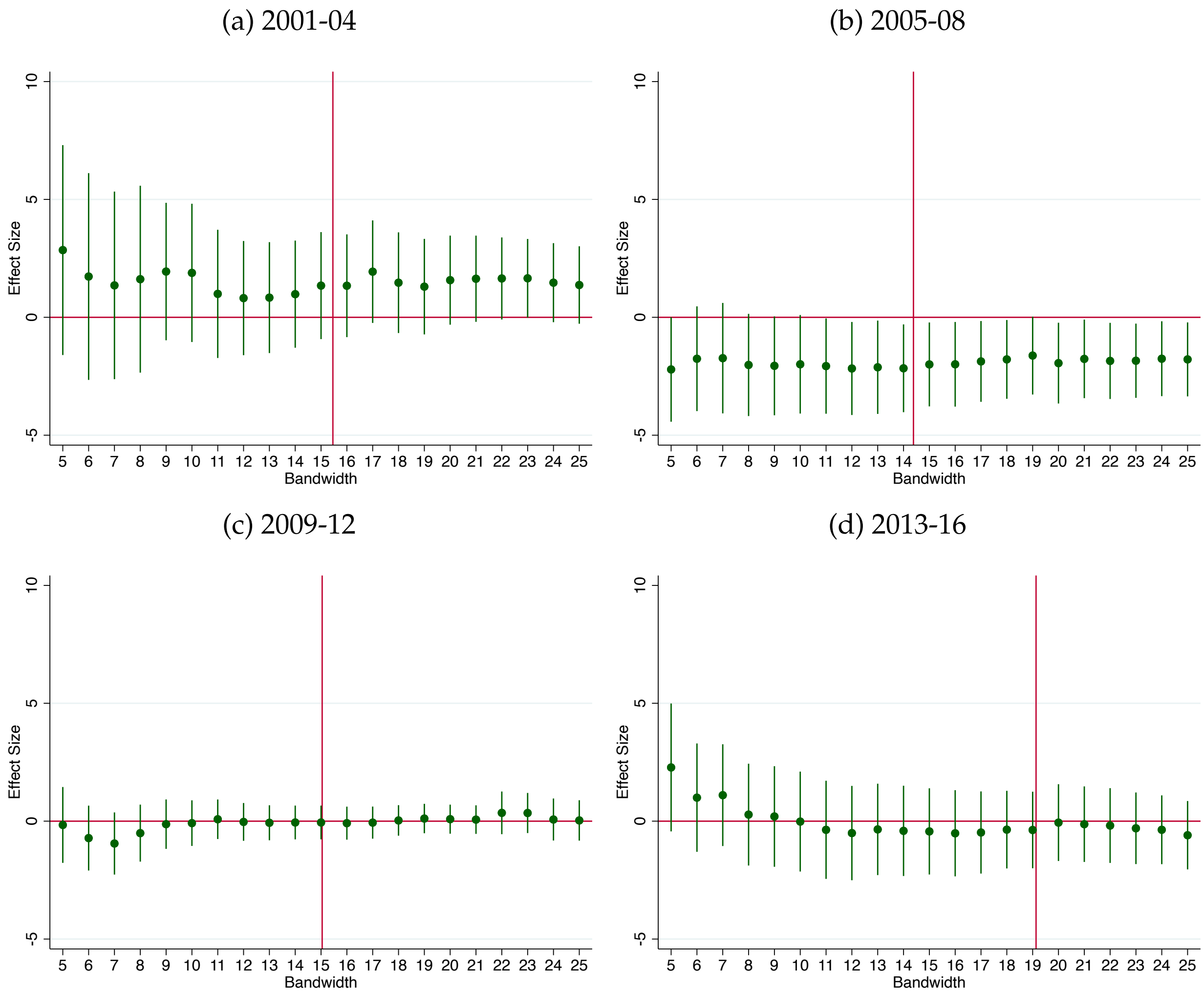


(d) 2013-16



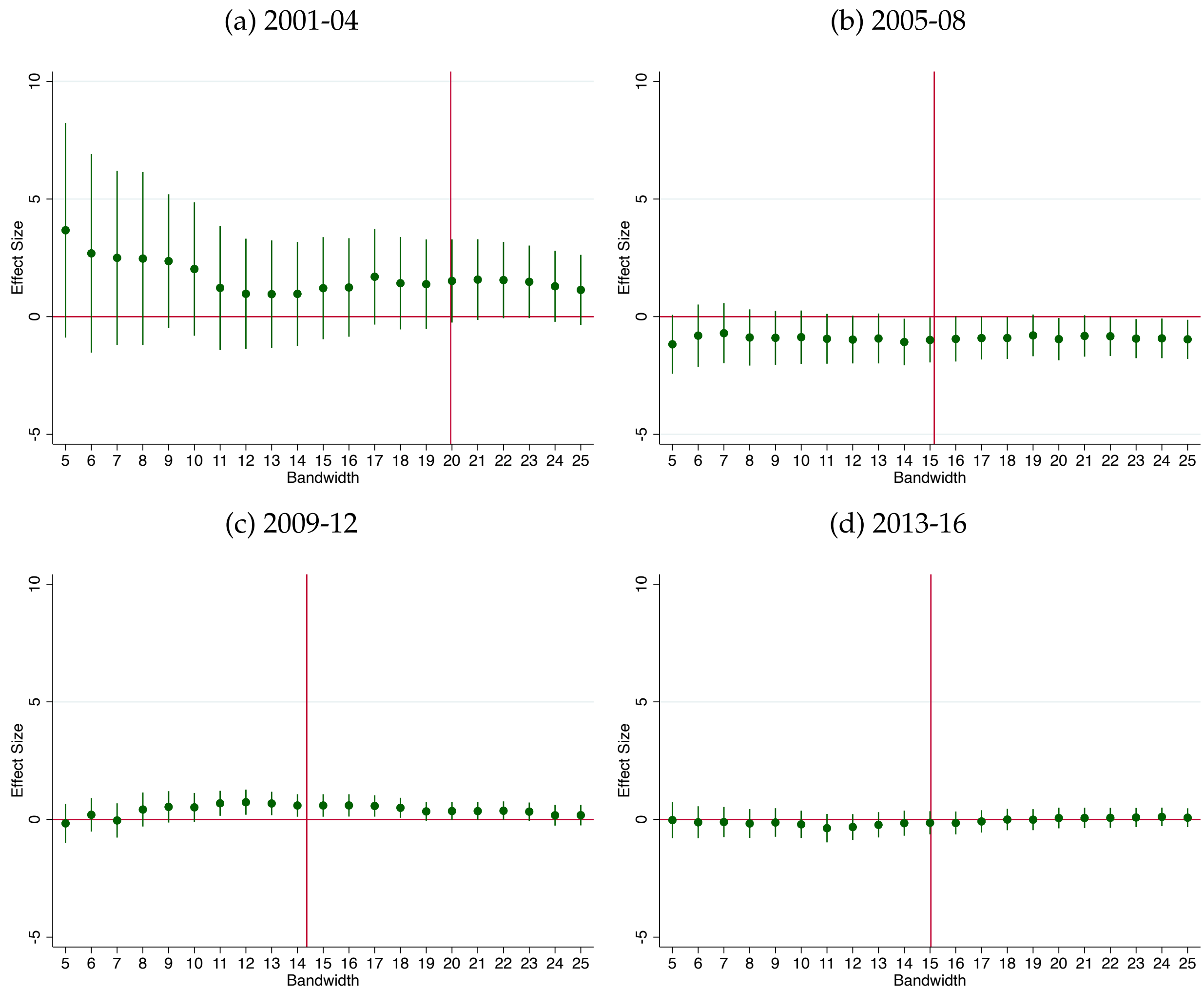
Notes: This Figure plots results from Equation (1) estimated for CO2e emissions. Each plot restricts observations to its specific term and within the 10 p.p. bandwidth. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Bands plot the 95% confidence intervals.

Figure C.10: Regression-discontinuity effects of electing farmer mayors on deforestation (% area) with varying bandwidths



Notes: This Figure plots results from Equation (1) estimated for deforestation with varying bandwidths, as discussed in Section 5.1. Each plot restricts observations to its specific term. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Bands plot the 95% confidence intervals.

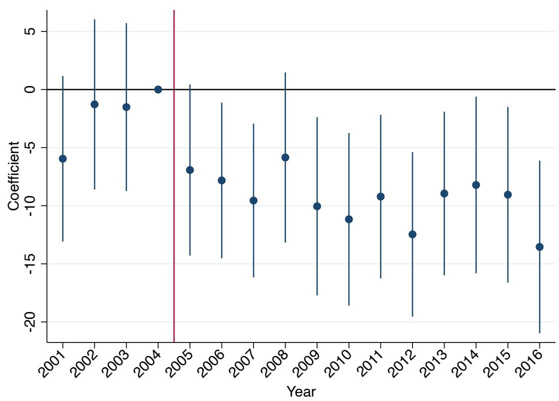
Figure C.11: Regression-discontinuity effects of electing farmer mayors on CO2e emissions (in 1,000 tons/km^2) with varying bandwidths



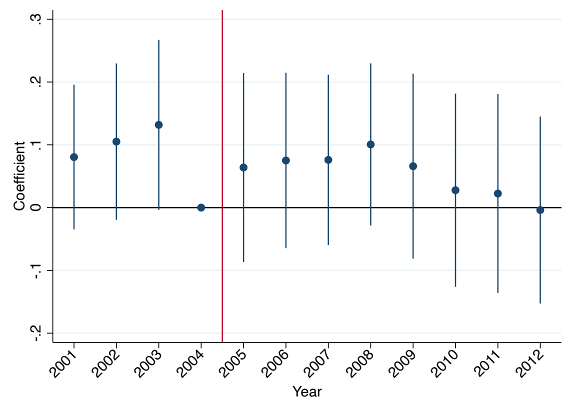
Notes: This Figure plots results from Equation (1) estimated for CO2e emissions with varying bandwidths, as discussed in Section 5.1. Each plot restricts observations to its specific term. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Vertical lines mark each panel's optimal bandwidth. Bands plot the 95% confidence intervals.

Figure C.12: Cross-section effects of electing farmer mayors on homicides and land conflict

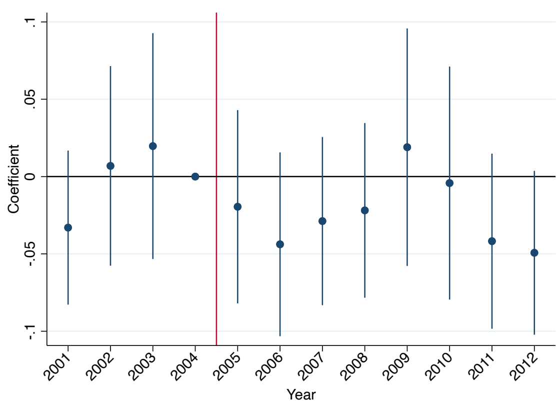
(a) Homicide Rate



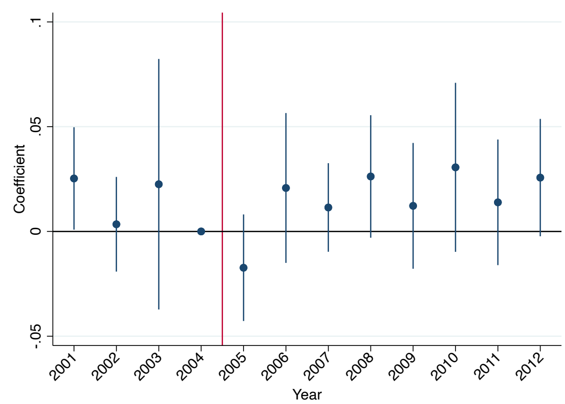
(b) Conflicts



(c) Murders



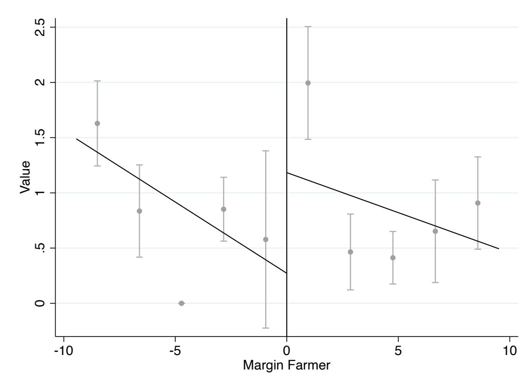
(d) Settlements



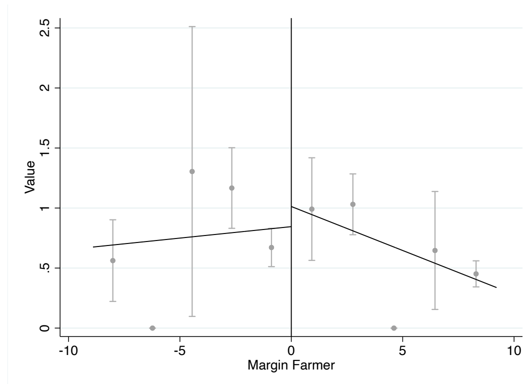
Notes: This Figure presents results from Equation 2 estimated for homicide rate and measures of land conflict. Controls include logs of population, area, and tree cover area in 2000. We do not control for log GDP per capita because municipality GDP data starts only in 2002. Bands represent the 95% confidence intervals.

Figure C.13: Regression-discontinuity effects of electing farmer mayors on land conflict: conflicts

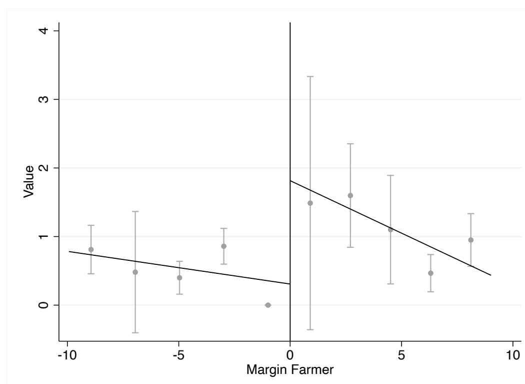
(a) Conflicts - 2001-04



(b) Conflicts - 2005-08



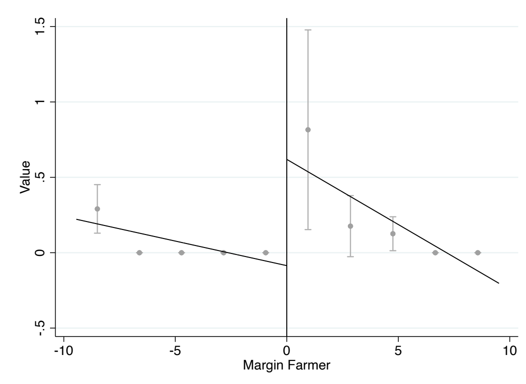
(c) Conflicts - 2009-12



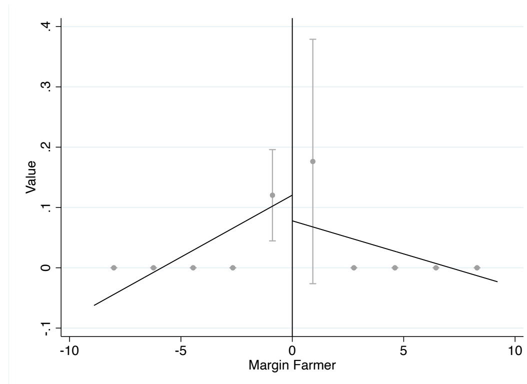
Notes: This Figure plots results from Equation (1) estimated for land-related conflicts. Each plot restricts observations to its specific term and within the 10 p.p. bandwidth. We exclude the term 2013-16 for measures of conflict because data is not available, as described in Section 4. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Bands plot the 95% confidence intervals.

Figure C.14: Regression-discontinuity effects of electing farmer mayors on land conflict: murders

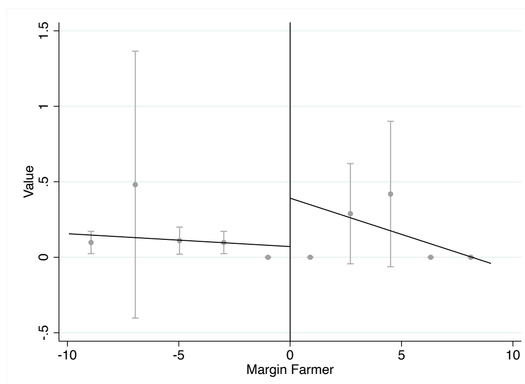
(a) Murders - 2001-04



(b) Murders - 2005-08



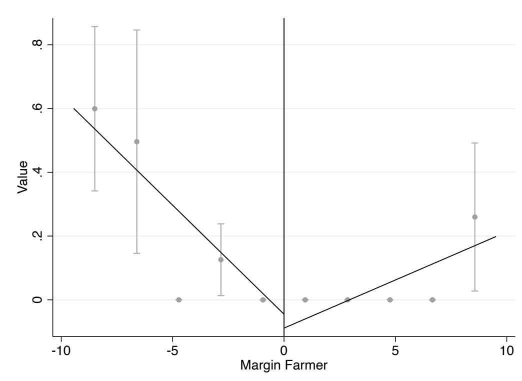
(c) Murders - 2009-12



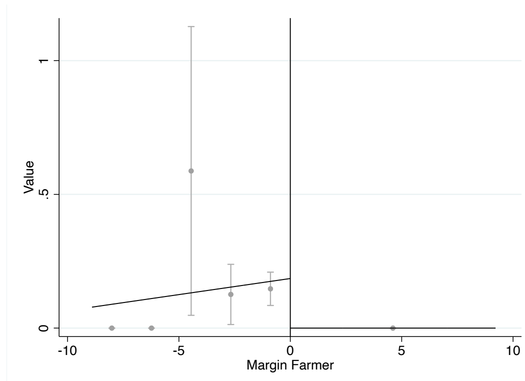
Notes: This Figure plots results from Equation (1) estimated for land-related murders. Each plot restricts observations to its specific term and within the 10 p.p. bandwidth. We exclude the term 2013-16 for measures of conflict because data is not available, as described in Section 4. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Bands plot the 95% confidence intervals.

Figure C.15: Regression-discontinuity effects of electing farmer mayors on land conflict: settlements

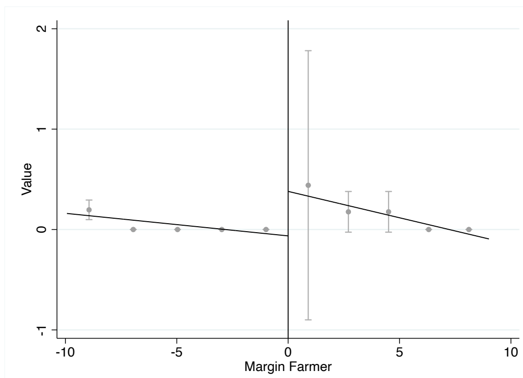
(a) Settlements - 2001-04



(b) Settlements - 2005-08



(c) Settlements - 2009-12



Notes: This Figure plots results from Equation (1) estimated for land-related settlements. Each plot restricts observations to its specific term and within the 10 p.p. bandwidth. We exclude the term 2013-16 for measures of conflict because data is not available, as described in Section 4. Controls include logs of population, area, tree cover area in 2000, and GDP per capita. Bands plot the 95% confidence intervals.