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The Environmental and Agricultural Effects of Electing Candidates Connected to Farming Interests

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Dissertação apresentada à Escola de Economia de São Paulo da Fundação Getulio Vargas como requisito para obtenção do título de Mestre em Economia de Empresas.

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Economia Política

Orientador: Prof. Bruno Ferman
Co-Orientador: Prof. Lucas Novaes

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ABSTRACT

Politically connected individuals obtain favors in the form of public jobs and public credit, but do they also receive preferential treatment when it comes to environmental regulation? Also, what are the effects of electing rural producers? This paper employs a close-election RD design using data from Brazil, a country where environmental protection has global relevance, to estimate the environmental and agricultural effects of electing municipal candidates connected to farming interests. Via previously unused data on official property registries, we locate and georeference farms whose owners finance local electoral campaigns, and evaluate the variation of deforestation and agriculture within spatial buffers centered on those farms, after municipal elections. We also evaluate effects on municipal-level outcomes. Overall, we don't find significant effects in electing farmers as either mayors or councillors on municipal-level planted and deforested areas. However, within small samples consisting only of large farmers, we do find significant and sizable effects, on both outcomes. These municipal-level results seem consistent with large farmers prioritizing - or being more able to prioritize - agriculture over environment, whilst in office. Our farm-level results suggest that electing councillors financed by farmers leads to significant increases (decreases) in farmed (deforested) areas within campaign donors' farms, with effects on farmed areas being far more sizable. Electing mayors financed by farmers leads to significant increases in deforested areas within campaign donors' farms, with insignificant effects on farmed areas. Although all farm-level variations plausibly come at the expense of within-farm vegetation, they seem particularly consistent with systematic manipulation of local environmental regulation only in the case of mayors financed by rural producers. This is coherent with the fact that the margin of environmental regulation that mayors control, enforcement, is far more flexible than the margin that councillors control, environmental legislation. Our farm-level results are considerably robust to buffer radius choice, and both our farm-level and municipal-level results come from samples for which our identification assumptions seem valid. Additionally, the vast majority of our results are robust to variations in the specification of our RD's, and pass innovative tests that assess inference quality in finite samples. The totality of our results benchmark innovative recommendations on how to reduce deforestation rates in Brazil, and raise interest on whether environmental regulation should occur at local levels.

Keywords: Political Connections, Candidate Types, Political Economy of Environmental and Agricultural Policy, Farmers, Agriculture, Deforestation.

JEL Codes: D72, Q58

RESUMO

Indivíduos politicamente conectados obtêm favores na forma de trabalhos públicos e crédito público, mas eles recebem também tratamento preferencial em termos de regulação ambiental? Adicionalmente, quais são os efeitos de eleger produtores rurais? Este trabalho emprega um RD design baseado em eleições apertadas usando dados do Brasil, um país onde proteção ambiental tem relevância global, para estimar os efeitos ambientais e agrícolas de eleger candidatos municipais conectados a interesses agropecuários. Por meio de dados anteriormente inutilizados sobre registros oficiais de propriedade, nós localizamos e georreferenciamos fazendas cujos donos financiam campanhas eleitorais locais, e avaliamos a variações em desmatamento e agricultura dentro de bolas espaciais centradas nessas fazendas, depois de eleições municipais. Nós também testamos efeitos a nível municipal. De forma geral, nós não encontramos efeitos significativos em eleger fazendeiros como prefeitos ou vereadores sobre áreas plantadas e desmatadas, a nível municipal. No entanto, em pequenas amostras que incluem somente grandes fazendeiros, nós encontramos efeitos significativos e consideráveis, sobre essas duas variáveis. Nossos resultados a nível municipal parecem consistentes com apenas os maiores fazendeiros priorizando (ou podendo priorizar) agricultura sobre meio ambiente, durante seu mandato. Nossos resultados a nível fazenda sugerem que eleger vereadores financiados por fazendeiros leva a significativos aumentos (reduções) nas áreas agropecuárias (desmatadas) das fazendas dos doadores de campanha, com efeitos muito maiores sobre as áreas agropecuárias. Eleger prefeitos financiados por fazendeiros leva a aumentos significations nas áreas desmatadas dentro das fazendas dos doadores de campanha, com efeitos insignificativos sobre áreas agropecuárias. Embora todas as variações a nível fazenda plausivelmente ocorrem às custas de vegetação dentro das fazendas, elas parecem particularmente consistentes com manipulação sistemática da regulação ambiental local somente no caso de prefeitos. Isto é coerente com a margem que os prefeitos controlam, enforcement, é mais flexível que a que os vereadores controlam, legislação. Nossos resultados a nível fazenda são consideravelmente robustos à escolha de raio de bola espacial. Tanto nossos resultados a nível fazenda como a nível municipal vêm de amostras onde nossas suposições de identificação parecem válidas. Adicionalmente, a grande maioria de nossos resultados é robusta a variações na especificação de nossos RD's, e passam testes inovadores que avaliam a qualidade da inferência em amostras finitas. A totalidade de nossos resultados serve como base de inovadoras recomendações sobre como reduzir taxas de desmatamento no Brasil, e trazem interesse à questão sobre se regulação ambiental deveria ocorrer a nível local.

Palavras-Chave: Conexões Políticas, Tipos de Candidatos, Economia Política das Políticas Ambiental e Agrícola, Fazendeiros, Agricultura, Desmatamento.

Códigos JEL: D72, Q58

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1 Introduction

When enacting environmental and agricultural policy, officials often deal with a fundamental trade-off: protect the environment, or promote agriculture. For instance, environmental regulation may block agricultural expansion that would require deforestation; rural credit may be used to expand farmed land, to the detriment of vegetation. More importantly, the officials that regulate this delicate balance may be significantly influenced by private interests. For instance, rural producers may request lower environmental regulation, in exchange for their campaign contributions. Also, by a matter of policy preference, expertise or private benefit, candidates who are rural producers themselves may promote agriculture at the expense of the environment, when enacting policy.

In this paper we investigate the environmental and agricultural effects of electing candidates connected to farming interests. By matching electoral data on campaign donors with previously unused data on official property registries, we identify 1,926 candidates officially financed by registered owners of formally established farms. Then, using data on electorally declared goods, we identify 73,411 candidates who possess rural properties. Following a close-election regression discontinuity design, we estimate the effects of electing a farmer on municipal-level agricultural production and deforestation; and of electing a candidate financed by farmers on deforestation and agricultural production, within spatial buffers centered on campaign donors' farms.

Overall, our municipal-level results suggest that the election of farmers in municipal elections does not affect farmed and deforested areas, at municipal level. However, in small samples consisting only of large farmers, we do find significant, positive and sizable effects, on both outcomes. Our farm-level results suggest that electing councillors financed by farmers leads to significant increases (decreases) in farmed (deforested) areas within campaign donors' farms. Electing mayors financed by farmers leads to significant increases in deforested areas within campaign donors' farms, with insignificant effects on farmed areas. These variations seem to come at the expense of within-farm vegetation. Our farm-level results are considerably robust to buffer radius choice. Both our municipal and farm-level results are robust to alternative RD specifications, and assessments of the quality of inference within our smallest samples support our conclusions. Finally, sorting and balancing tests suggest that our estimates are indeed capturing causal effects.

Our municipal-level results seem consistent with only the largest farmers prioritizing - or being able to prioritize - agriculture over environment, whilst in office. This first set of results represent the first test of the environmental and agricultural effects of electing a particular type of candidate: rural producers. Additionally, our farm-level results seem consistent with systematic manipulation of environmental regulation, among other possible mechanisms. This second set of results represent the first test of the possibility of politically connected individuals obtaining favors in the form of environmental regulatory leniency. Taken together, our results contribute to the general literature on the political economy of environmental and agricultural policy, which has not yet considered the policy implications of electing candidates connected to farming interests.[1]

In fact, although the effects of electing particular types of candidates have been studied, the effects of electing rural producers have not. Examples of types of candidates whose election has been studied include Muslim candidates (Meyersson, 2014); black candidates (Vogl, 2014); female candidates (Ferreira and Gyourko, 2014; Brollo and Troiano, 2016); and public employees (Hyytinen et al., 2018). Additionally, although other forms of state capture have been studied, strategic environmental regulatory leniency has not. Examples of forms of state capture which have been studied include protectionist trade policy destined to favor special interest groups (Grossman and Helpman, 1995; Maggi and Rodriguez-Clare, 1998; Goldberg and Maggi, 1999; Mitra, 1999; Gawande and Bandyopadhyay, 2000); the reallocation of public credit towards connected individuals (Khwaja and Mian, 2005; Claessens et al., 2008; Li et al., 2008; Schoenherr, 2019); and the reallocation of public jobs towards connected individuals (Hsieh et al., 2011; Enikolopov, 2014; Xu, 2018; Gagliarducci and Manacorda, 2020).

Ex-ante, there are several reasons why rural producers may prioritize agriculture over environment more, when compared to candidates who are not rural producers. First, rural producers have a personal stake over the state of local agriculture. Second, their profession may lead them to believe, rationally or not, that prioritizing agriculture over the environment is best for their electorate. Third, they may have more expertise in terms of agricultural policy, by virtue of their own professional experience. Additionally, there are several reasons why, a priori, donor-politician relationships may be based on strategic environmental regulatory leniency. For instance, donors who are rural producers may find little advantage in obtaining public sector jobs. Also, some politicians may find it easier to reduce environmental regulation within specific farms, than allocating public credit towards their owners.

Although the aforementioned ideas refer to the entire universe of candidates connected to farming interests, we can expect heterogeneities to exist, within this general set. Mayors financed by farmers might use their power over environmental regulation to decrease environmental restrictions in specific locations, while councillors financed by farmers may use their control over the municipal budget to bargain an allocation of rural credit towards specific individuals. Additionally, a given municipal office may be used differently, by different types of farmer candidate. Candidates who possess more farms may have larger stakes on the state of local agriculture, and may possess more expertise at handling agricultural matters. This might lead

[1] Other works pertaining to this general literature have focused on the effects of other attributes of the political system on environmental policy, such as political instability and corruption (Fredriksson and Svensson, 2003); the distribution of the externalities stemming from negative environmental outcomes (Fredriksson and Svensson, 2003); regulatory loopholes (Polk and Schmutzler, 2005); democratic liberties (Farzin and Bond, 2006); electoral platforms (List and Sturm, 2006); political decentralization (Burgess et al., 2012); and lobbies focused on particular pieces of environmental legislation (Meng and Rode, 2019).

them to prioritize agriculture over environment relatively more, when enacting policy, than farmer candidates who have fewer farms. Another possibility is that preferences over public policy don't vary in the number of owned farms, but the capability of enacting a preferred policy does. Richer, more educated farmer politicians may be more effective in their dealings with local councillors, rendering them more able to enact their preferred policies.

It is important to consider that, when evaluating settings in which to study the political economy of environmental and agricultural policy, the Brazilian municipal scene emerges as a particularly interesting alternative. There are three main reasons underlying this fact. First, to the extent that there exists an unavoidable agricultural-environmental trade-off, Brazilian environmental policy involves relatively large opportunity costs. In fact, in Brazil, agribusiness accounts for half the balance of trade, a third of GDP, and a fifth of total employment (Vieira Filho et al., 2016; CEPEA, 2019). This inherently raises the cost of environmental regulations that block agricultural expansion, and of promoting agricultural production. Simultaneously, Brazil hosts more than 15% of the world's biological diversity, 60% of the world's largest rainforest, and almost the entirety of its most biodiverse savannah (UN, 2019; WWF, 2019; Greenpeace, 2019). This raises the cost of having loose environmental restrictions.

Second, rural producers are big players, in Brazilian municipal elections. Our benchmark definition of farmer candidates implies that over 10% of total candidates and 20% of winning candidates participating in the 2008 and 2012 Brazilian municipal elections were rural producers. Third, Brazilian mayors and councillors have at their disposal several institutional mechanisms through which they may affect the agricultural-environmental balance, at local level. Besides control over municipal expenditures, taxation, public credit and appointed public sector jobs, municipal office-holders have power over the environmental regulation of local hotspots, both in terms of legislation, and of enforcement. The totality of these facts raises the social relevance of our estimates, and enables us to study not only the types of policies enacted by rural producers, but also the possibility of localized manipulation of environmental regulation.

Finally, this paper offers two main policy-relevant contributions. First, we provide a politically-informed reduced-form recommendation on how to reduce deforestation rates in Brazil. This complements a literature that has so far focused on the reduced-form effects of alternative policies on deforestation rates, without evaluating which types of candidates are more or less prone to enact them.[2] Second, our results raise the question of whether municipalities should have power over environmental regulation. Although municipal agencies may be effective at monitoring local hotspots, they could be susceptible to capture by local rural elites.

[2] For instance, Assunção et al. (2015) focus on the environmental effects of alternative agricultural policies; Assunção and Rocha (2019), of prioritized environmental monitoring; and Assunção et al. (2020), of rural credit. Moreover, Souza-Rodrigues (2019) provides an evaluation of the relative effectiveness of alternative policies, in reducing deforestation rates.

The remainder of this paper is organized as follows. Section 2 presents an overview of the most relevant institutional aspects of our empirical setting. Section 3 describes our data sources, provides descriptive statistics, and discusses the transformations that lead to the dependent variables present in our regressions. Section 4 discusses our empirical strategy, and presents evidence supporting the design of our study. Section 5 presents our results, exploring interesting heterogeneities. Section 6 concludes the paper. The Appendix to this text provides robustness checks for all empirical results.

2 Institutional Setting

In this section we briefly discuss the key institutional features of our empirical setting. We first present the specific mechanisms through which Brazilian mayors and councillors may affect environmental and agricultural outcomes, at a local level. We then discuss the structure of Brazilian municipal elections. The institutional peculiarities of these elections not only determine how rural producers may finance local candidates or become local politicians themselves, but also directly affects how we construct the forcing variable of our close-election RD's.

2.1 Municipal Policy

In Brazil, municipal governments are able to enact environmental and agricultural policy, with subsequent effects on local agricultural and environmental outcomes. As an initial set of motivating examples, consider the following municipal government acts. In 2011, the São Bernardo do Campo municipality specified the institutional mechanisms required for the implementation of its formal municipal environmental policy (São Bernardo do Campo, 2011). In 2015, the Rondinha municipality set forth a set of monetary penalties associated to activities prejudicial to the environment (Rondinha, 2015). In 2014, the Encantado municipality created a program to promote agricultural output, based on tax benefits (Encantado, 2014). In 2017, the Pinhalzinho municipality enacted a very similar policy (Pinhalzinho, 2017).

As the examples above illustrate, there are different mechanisms through which Brazilian councillors (*vereadores*) and mayors (*prefeitos*) may enact municipal environmental and agricultural policy, at a local level. In fact, councillors, as members of the legislative branch of municipal governments, have influence over the existence and stringiness of municipal environmental legislation, and can propose or vote for laws that promote the agricultural sector. Mayors, on the other hand, can reduce funding of municipal environmental agencies, and can appoint lenient regulators to them. They may also require localized regulatory lenience from potential regulators, in exchange for patronage. Mayors are thus fundamentally able to affect local enforcement of environmental regulations.[3]

In fact, by 2009, the first year for which we will evaluate agricultural and environmental outcomes in this paper, 46.8% of Brazilian municipalities had enacted municipal environmental legislation, 56.3% had established a formal municipal environmental council; and 31% had engaged in local environmental licensing (IBGE, 2009). Similarly, by 2017, the earliest year for which we were able to find this information, and one year after the end of our empirical analysis, 47.45% of Brazilian municipalities had a program to promote agribusiness at place; and 67,27% had established a formal municipal agricultural development council (IBGE, 2017b).

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[3] Of the 3,135 Brazilian municipalities which had a municipal environmental council, 87,59% had part of its members directly appointed by the city mayor, and 47,88% of such agencies had monitoring duties (IBGE, 2017b).

Brazilian municipal-level environmental policy, in particular, has a long-standing *de jure* basis.[4] In 1981, the Brazilian National Environmental Policy Law (Brazil, 1981) created the Brazilian National Environmental System (*SISNAMA*), in which municipalities were given the power to impose environmental fines and the responsibility to monitor activities that may harm the environment, at local level. This arrangement was then reinforced by the 1988 Brazilian Federal Constitution (Brazil, 1988) and a federal executive decree enacted in 1990 (Brazil, 1990). More recently, a 2011 federal law (Brazil, 2011) further specified that the responsibility to monitor and impose environmental fines over enterprises environmentally licensed by municipalities was to be borne by the municipalities themselves.

There are additional channels through which candidates with connections to the agricultural sector may affect agriculture and deforestation at a local level. These channels should be considered, when interpreting our empirical results. For instance, councillors have influence over municipal taxes, such as the service tax (*ISS*). The rate of this tax can vary across services, enabling a possible reduction in services that are inputs or outputs of the agricultural sector. Moreover, councillors are able to bargain their support for the incumbent mayor, indirectly affecting municipal expenditures, environmental enforcement, and appointments to municipal environmental and agricultural councils. Mayors, on the other hand, are able to indirectly affect environmental and tax legislation approved by the municipal council, using the size and distribution of municipal expenditures as a bargain. More directly, mayors can increase expenditures on public irrigation pivots, or in technical education destined to increase the productivity of professionals of the agricultural sector. Under the approval of the municipal council, mayors are able to direct municipal public credit to specific rural producers, and can pardon their standing debts towards the municipal treasury.

Finally, consider the relevant environmental constraint that an arbitrary Brazilian rural producer actually faces. It is given by the degree to which the farmer cannot clear forest, within her or his own farm. In Brazil, this restriction takes the form of "Legal Reservation Areas" (*Áreas de Reserva Legal*), fractions of farmland that are destined to environmental conservation. This legally obliged fraction varies in minimum size, according to the biome in which the farm is located. If the farm is located in the Amazon biome, this corresponds to at least 80% of total land area; if in the Cerrado biome, 35%; if in other biome, 20%.[5] Farmers interested in increasing the agricultural output produced by their farms have a clear incentive to keep this proportion closer to the minimum required level. Moreover, they could ignore this legal constraint to some extent, if they were guaranteed an absence of environmental monitoring.

Taken together, these facts imply that the political economy of environmental and agricultural policy has a relevant municipal dimension, in Brazil. There are clear channels

[4] This is in contrast with municipal-level agricultural policy, which seems to lack this federal-level historical juridical basis. Brazilian municipal-level agricultural policy occurs, mostly, *de facto*.

[5] These percentages have been the same since the 1965 Brazilian Forest Code.

through which local politicians connected to farming interests may affect agriculture and deforestation, both at municipal and more localized levels.

2.2 Municipal Elections

Municipal elections in Brazil occur in four-year cycles. In these elections both mayors and councillors are chosen, for four-year terms. All Brazilian citizens affiliated to an official party above the age of 21 (18) are allowed to run for mayorship (councillorship). Throughout the two municipal elections in our sample - 2008 and 2012 -, candidates could receive campaign contributions from corporate and non-corporate donors, besides using party funds.[6] Mayors can only be re-elected once, but councillors face no term limits. In municipalities with more than 200,000 registered voters, candidates running for mayorship face a second-round of voting, if an absolute majority is not achieved in the first-round. Candidates for mayorship run within a common first-past-the-post system, but councillor candidates run within the Brazilian proportional representation system.[7]

In this system, parties form coalitions (*coligações*), in terms of which the electoral dispute occurs. Each coalition presents a set of candidates to the election and, at the end of the election, the sum of all within-coalition votes is used to determine the allocation of legislative seats, across coalitions. After this allocation of seats across coalitions occurs, the allocation of candidates to those seats is settled, determined by the performance of each candidate relative to the members of his or her own coalition.[8] These institutional peculiarities have direct implications in the way the forcing variable of our close-election RD's is defined. For candidates running for mayorship, subjected to a common first-past-the-post system, we will simply follow standard practice and set the difference in vote shares as the forcing variable of our RD's. This follows, for example, Brollo and Troiano (2016); Meyersson (2014); Ferreira and Gyourko (2014); Eggers and Hainmueller (2009); Lee et al. (2004); DiNardo and Lee (2004).

For candidates running for councillorship, however, this is not the case, as their vote shares at municipal level are not the sole criterion that determines their electoral outcome. It is our view that, for these candidates, one may construct two different running variables, each capturing a different dimension of electoral competition. First, one could compare votes across coalitions, capturing the effect of an additional seat being allocated to a particular coalition. Alternatively, one may compare votes within coalitions, capturing instead the effect of electing a particular candidate. In this paper we use this last form of running variable, since we are interested in the effects of electing candidates, not coalitions, connected to farming interests.

[6] Since the 2016 Brazilian municipal elections, candidates can no longer receive campaign contributions from corporate donors.

[7] For more informations on the Brazilian municipal electoral system, refer to TSE (2020).

[8] For more information on the Brazilian proportional system, refer to TSE (2018).

3 Data

Naturally, not every farmer candidate is financed by farmers, nor is every candidate financed by farmers a farmer herself. This differentiates the sample in which we estimate municipal-level effects, which consists only of farmer candidates, from the sample in which we estimate farm-level effects, which consists only of candidates financed by farmers. Additionally, we have data on agricultural outcomes, at either level of analysis, for the entire Brazilian territory. But we only have municipal-level data on deforestation for the Amazon biome, and farm-level data on deforestation for the the Amazon and Cerrado biomes. This differentiates the samples in which we estimate effects on agriculture from the samples in which we estimate effects on deforestation, at both level of analysis.

Taken together, these facts imply that our estimates of the effects of electing candidates connected to farming interests on agriculture and deforestation come from four main regression subsamples, one for each outcome-level pair. For the purposes of clarity, we present data sources in separate subsections, one for each type of candidate: farmer candidates; candidates financed by farmers. We then present descriptive statistics for the main regression subsamples associated to each outcome, at the end of each subsection. A final subsection presents dependent variable transformations that proved necessary, given the structure of our data.

3.1 Farmer Candidates

Data on our first municipal-level outcome, planted area at municipal level, was obtained from the Brazilian Institute of Geography and Statistics (*IBGE*). This source provides yearly municipal-level data on area of planted temporary and permanent crops, from 2009 to 2016, covering 5,518 of the 5,570 Brazilian municipalities. We sum these areas to obtain total planted area, for each municipality-year. Data on our second municipal-level outcome, total deforested area at municipal level, was obtained from the National Institute for Space Research (*INPE*). This source provides yearly municipal-level data on deforested area from 2009 to 2016, covering the 760 municipalities located in the Amazon rainforest.

To construct the forcing variable of our RD's, which we properly define in Section 4, we obtained data on electoral performance from the Brazilian Superior Electoral Court (*TSE*). This source provides candidate-level data on the municipality in which the candidate ran, the votes that she received, and the coalition to which she belonged. In addition, TSE provides data on candidate characteristics, information that we use when testing for balance in our RD's. Finally, TSE provides data on the belongings and profession that each candidate declared, in each election. We use this information to identify different types of farmer candidates.

We first defined “rural properties” and “rural professions”. A good was considered a “rural property” if in its description at least one of the following words appeared: “Rural” (Rural); “Sítio” (Farm); “Fazenda” (Farm); “Gado” (Cattle); “Gleba” (Land); or if it was categorized as “Terra Nua” (Unused Land). The professions that we considered “rural” were: “Agricultor” (Farmer); “Pecuarista” (Rancher); and “Produtor Agropecuário” (Agricultural Producer).

There is not an unique form of identifying farmer candidates, given these definitions. Moreover, different definitions imply significantly different samples of farmer candidates, in terms both of size and observables. Consider, for this brief discussion, four statistics: the share of farmer candidates within the universe of 712,749 candidates who participated in the 2008 and 2012 Brazilian municipal elections; the share of college-educated farmer candidates; the share of farmer candidates running for mayorship; and the share of farmer candidates who were elected.

If we define as farmers candidates who declared at least one rural property, we identify 73,411 farmer candidates (10.3% of total candidates). Of these candidates, 20.82% graduated from college; 12.79% ran as mayors; and 30.54% were elected. If, on the other hand, we define as farmers candidates who declared a farmer-like profession, we identify 73,496 farmer candidates (10.3% of total candidates). Of these candidates, only 2.26% graduated from college; only 3.99% ran as mayors; and only 21.26% were elected. The intersection between these two sets of farmer candidates is not large: there are only 23,926 candidates who declared at least one-farm like good and declared a farmer-like profession (3.4% of total candidates). Finally, as an example of candidates that declare many rural properties, consider the 7,297 candidates who declared at least three rural properties (1% of total candidates). Of these candidates, 31.40% were college-educated; 34.49% ran for mayorship; and 42.55% were elected.

These considerable differences suggest that, for different definitions of farmer candidates, we are estimating effects for different sub-populations of the more general population of rural producers. Moreover, we don’t expect, ex-ante, homogeneous treatment effects, across these sub-populations. In fact, it seems plausible to believe that, the larger the farmer, the more she is prone to prioritize agriculture over the environment, whilst in office. In terms of our theoretical framework, these larger farmers have a larger stake on the state of local agriculture, and probably have more expertise, when handling agricultural matters. The results presented in Section 5 are consistent with this interpretation.

Whatever the case, we need a benchmark criterion, to define the sample from which we obtain our more general results, and in which we evaluate the validity of our empirical strategy. We believe that the more sensible choice is to consider as farmers the candidates who declared at least one rural property. First, given the preceding facts and discussion, this seems more interesting than using a profession-based criterion, since candidates with farmer-like professions appear to be small rural producers. Second, this seems less ad-hoc than choosing a certain quantity of rural properties above which candidates are considered farmers.

Table 1 presents summary statistics for three relevant subsamples. First, for the 63,576 farmer candidates (according to our benchmark criterion) for whom we have post-electoral data on municipal-level planted area, for the municipality in which they ran. This first sample serves as the main regression subsample for the estimation of municipal-level effects on agriculture. Second, for the 13,593 farmer candidates (according to our benchmark criterion) for whom we have post-electoral data on municipal-level deforested area, for the municipality in which they ran.[9] This second sample serves as the main regression subsample for the estimation of municipal-level effects on deforestation. Third, for the 639,090 non-farmer candidates (according to our benchmark criterion) who ran in these elections. This third sample serves to illustrate the differences between farmer and non-farmer municipal candidates, within our setting.

Table 1 – Descriptive Statistics - Farmer and Non-Farmer Candidates

	Mean	S.D.	Min.	Q1	Median	Q3	Max.	Obs
Farmer candidates - Municipalities with Data on Agriculture								
Candidate was elected	.33	.47	0	0	0	1	1	63,576
Candidate ran for mayor	.13	.34	0	0	0	0	1	63,576
Total votes received by the candidate	1,153	12,506	0	105	252	662	2,708,768	63,576
Candidate ran in the 2008 election	.49	.5	0	0	0	1	1	63,576
Number of declared rural properties	1.5	1.6	1	1	1	2	64	63,576
Candidate ran in a Legal Amazon municipality	.2	.4	0	0	0	0	1	63,576
Agriculture share in municipal GDP	.22	.15	0	.10	.19	.32	.81	63,550
Candidate completed college	.21	.4	0	0	0	0	1	63,576
Male candidate	.87	.33	0	1	1	1	1	63,576
Married candidate	.75	.43	0	0	1	1	1	63,576
Farmer candidates - Municipalities with Data on Deforestation								
Candidate was elected	.27	.45	0	0	0	1	1	13,593
Candidate ran for mayor	.12	.32	0	0	0	0	1	13,593
Total votes received by the candidate	1,008	8,426	1	88	210	521	495,460	13,593
Candidate ran in the 2008 election	.50	.5	0	0	0	1	1	13,593
Number of declared rural properties	1.5	1.3	1	1	1	2	54	13,593
Candidate ran in a Legal Amazon municipality	1	0	1	1	1	1	1	13,593
Agriculture share in municipal GDP	.28	.15	.0016	.16	.27	.38	.80	13,593
Candidate completed college	.15	.36	0	0	0	0	1	13,593
Male candidate	.86	.35	0	1	1	1	1	13,593
Married candidate	.66	.47	0	0	1	1	1	13,593
Non-farmer candidates								
Candidate was elected	.14	.35	0	0	0	0	1	639,090
Candidate ran for mayor	.03	.16	0	0	0	0	1	639,090
Total votes received by the candidate	452	8,715	0	35	102	264	3,790,558	639,090
Candidate ran in the 2008 election	.43	.5	0	0	0	1	1	639,090
Number of declared rural properties	0	0	0	0	0	0	0	639,090
Candidate ran in a Legal Amazon municipality	.16	.37	0	0	0	0	1	639,090
Agriculture share in municipal GDP	.15	.14	0	.023	.11	.23	.81	638,629
Candidate completed college	.19	.39	0	0	0	0	1	639,085
Male candidate	.72	.45	0	0	1	1	1	639,085
Married candidate	.58	.49	0	0	1	1	1	639,085

Notes: This table reports summary statistics for the candidates that participated in the 2008 and 2012 Brazilian municipal elections. "Farmer candidates" are candidates that declared at least one rural property.

9 Recall that, while we have data on planted area at municipal level covering the entirety of the Brazilian territory, we only have data on deforested area at municipal level for the 760 municipalities located in the Amazon biome.

Farmer and non-farmer candidates are different. Farmer candidates get elected more often; run more frequently for the more powerful municipal office, mayorship; run more frequently in municipalities located in the Brazilian Legal Amazon[10]; and, on average, run in municipalities in which agriculture is more relevant to the local economy.

3.2 Candidates Financed by Farmers

Data on our first farm-level outcome, within-farm deforested area, was obtained from *PRODES* (Project for Monitoring Deforestation in the Legal Amazon), an INPE project. This source provides yearly georeferenced data on deforested area, from 2009 to 2016, covering the Cerrado and Amazon biomes. This data is very finely-grained, capturing variations as small as 20 meters, long and wide. It is based on LANDSAT 8 satellite imagery, capturing year-by-year variations in deforested area. Data on our second farm-level outcome, within-farm agricultural and pastoral area, was obtained from *Monitoramento da Cobertura e Uso da Terra do Brasil* (Monitoring of Land Cover and Land Use), an IBGE project. This source provides biennial georeferenced data on type of land use, from 2010 to 2016, covering the entirety of the Brazilian territory. This data is not as finely-grained as our deforestation data, only capturing variations larger than 250 meters, long and wide. It is based on MODIS 1-2 satellite imagery, captured once every two years. For the purposes of our analysis, we considered two types of land use as agricultural or pastoral: “Área Agrícola” (Agricultural Area) and “Pastagem com Manejo” (Manipulated Pasture).

TSE also serves as the source for data on electoral performance and candidate characteristics, in this sample of candidates financed by farmers. For this sample, we also use the tax identification numbers of electorally declared non-corporate and corporate campaign donors, to exactly identify them as owners of formally established farms. To do this, we use public, identified, and georeferenceable data from *Receita Federal*, the Internal Revenue Service of Brazil (Federal, 2019). This source provides, for all Brazilian firms formally established since 1998, their *CNPJ* (the tax identification number associated to formal Brazilian companies), their economic activity code(s) (*CNAE*), their operating address, the date in which they started doing business (formally and informally), and the date in which they entered formal bankruptcy, if it ever happened. For the few firms which are corporate partners of other formal firms, this public data source provides the *CPF* (the tax identification number associated to Brazilian citizens) of their legal representative(s).[11]

We use CNAE to identify firms which are formally established farms. More specifically, we considered as farms the firms which had at least one associated CNAE belonging to the class “Agricultura, Pecuária e Serviços Relacionados” (Agriculture, Ranching and Related Services). We thus identified 426,586 farms which were formally registered in the Receita Federal at some

[10] The Brazilian Legal Amazon contains the nine states located in the Amazon basin: Acre, Amapá, Amazonas, Pará, Rondônia, Roraima, Tocantins, Mato Grosso and Maranhão.

[11] A given firm may have more than one associated economic activity code, and more than one legal representative.

point since 1998, of which 359,902 are still operating. These 359,902 formally operating farms correspond to 7.1% of the 5,073,324 farms which existed in Brazil, in 2017 (IBGE, 2017a). In addition, of these 359,902 formally operating farms, the CPF of their legal representative(s) is available only for 6,524 of them (1.8%).[12]

We now explain how we identified donors, corporate and non-corporate, as owners of formally established farms. We begin with non-corporate donors, who are identified by their CPF. We took the CPF of each non-corporate donor and matched it with the CPF of the legal representatives of active formal farms, for each election. This gave us the set of non-corporate donors who owned active formal farms, at the time of each election. The post-electoral outcomes of matched farms were then associated to the candidates to whom their legal representatives contributed. We now cover corporate donors, which are identified by their CNPJ. We consider two different cases, separately: the case in which we know the CPF of the legal representative(s) of the corresponding CNPJ; the case in which we don't.

For corporate donors for which we don't have the CPF of their legal representative(s), we have two sub-cases to consider. First, if the CNPJ of the corporate donor has at least one CNAE that matches that of an active formal farm, then it is itself a farm, and so its post-electoral outcomes become associated to the candidates to which the corporate donor contributed.[13] Second, if the CNPJ of the corporate donor does not have at least one CNAE that matches that of an active formal farm, the campaign donor at hand becomes irrelevant to our study. For corporate donors for which we have the CPF of their legal representatives, we check whether such representatives are themselves representatives of formal farms.[14] If they are, then the post-electoral outcomes of such farms become associated to the candidate(s) to whom the corporate donor contributed. If they are not, then this is not a corporate donor relevant to our study. This completes the construction of the set of formally established farms which are associated to the municipal candidates being studied.

Following this procedure, we identified 968 formally established farms whose owners officially contributed to the campaigns of 1,926 candidates running in the 2008 and 2012 Brazilian municipal elections. Of these 968 farms, 490 were identified via corporate donors, and 478 via non-corporate donors. These numbers imply that, given our data restrictions - not knowing the CPF of the legal representative(s) of 98.2% of the legally established farms, most notably - we are able to classify only 0.3% (968) of the 359,902 active formal farms as associated to candidates running in the 2008 and 2012 municipal elections.

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[12] As stated in the previous paragraph, these correspond to the relatively few farms which are corporate partners of other formal firms.

[13] Do realize that we don't need to know the legal representative(s) of a farm to decide whether or not its post-electoral outcomes ought to be associated to a particular candidate.

[14] This covers the case in which we know the legal representative(s) of the corporate donor and the corporate donor is, itself, a formal farm.

Additionally, we are only able to identify 0.3% (1,926) of the 712,749 candidates running in such elections as financed by owners of formally established farms. Moreover, of the 1,262,955 non-corporate donors which officially contributed in the 2008 and 2012 Brazilian municipal elections, only 478 (0.04%) are owners of formal farms for which we know the CPF of their legal representative(s). And, of the 360,809 corporate donors which officially contributed in the 2008 and 2012 Brazilian municipal elections, only 490 (0.10%) are associated to the owners of formal farms.

We now explain how we matched farms to their spatio-temporal agricultural and environmental outcomes. We first used our data on the operating address of each farm to obtain the spatial coordinates of its spatial centroid, via Google Maps. This gave us a grid of spatial points, with each point being the centroid of a formally established farm. Since we have no information on total farm area, we created spatial buffers centered in the farms' centroids, of arbitrary - though sensible - radii. These spatial buffers then served as approximations of the actual farms, with deforestation and agriculture within these buffers proxying deforestation and agriculture within campaign donors' farms. To obtain these outcomes, we intersected the spatial buffers with our shapefiles on deforested and farming areas, through time. This gave us our measure of total deforested and farming area, for each farm, at each moment of time.

An important assumption we make, when constructing our sample of candidates financed by farmers, is that, within any farm, there ought to be both farmed and deforested areas. Hence, if, for a given farm, there are no intersections between its spatial buffer and either the farmed area shapefile or the deforested area shapefiles, it is not because there is no agricultural or deforested land within such farm, but rather because our data does not provide us with information on the outcomes of such farm. In other words, outcomes for this farm are missing, so this farm cannot enter our regressions. Under this reasoning, for a given buffer radius, we first intersected the buffers with the deforested lands shapefile. This gave us the subset of campaign donor farms for which we have information on deforested area. We then intersected the buffers with the farmed lands shapefile. This gave us the subset of campaign donor farms for which we have information on farming area. It is within these subsets that we run our regressions.

Before presenting descriptive information, we discuss our choice of buffer radii. We constructed spatial buffers of three different radius sizes, when measured in decimal degrees: 0.01, 0.05 and 0.10. These roughly correspond to 1.1 km, 5.5 km and 11 km in the northernmost parts of Brazil (which are closer to the equator), and to 1 km, 5 km and 10 km in the southernmost regions (which are closer to the earth's poles).[15] These radii were chosen for two main reasons. First, we believe that formally established farms of electorally declared campaign donors ought to be relatively large in size, when compared to other Brazilian farms. This assumption led us to

[15] This is a technical detail that comes from the fact that the earth is not flat. This implies that, when partitioning the entire globe in degrees, a given degree length corresponds to larger uni-dimensional lengths in the equator, than in the poles.

construct spatial buffers able to cover the entire extent of the largest Brazilian farms. Second, the largest Brazilian farms reach 1,000 km^2 of total land area, and IBGE uses 10 km^2 as a cutoff value above which farms are considered large estates (*latifúndios*). We believe that under smaller buffer sizes we would be incapable of capturing variation in deforested and farming areas, within these (supposedly) large farms.

Since these choices were nevertheless arbitrary, we now discuss how our sample of candidates financed by farmers behaves, under these alternative choices of buffer radii. Differently from the decision on which candidates to consider as farmers, in this case the set of candidates is fixed, given by the set of candidates who were financed by owners of formal farms, as described above. What changes across buffers sizes are the intersections between the buffers and the outcome shapefiles; in essence, the share of missing information on our outcome variables. Therefore, we now describe how these shares vary, across buffer sizes.

Consider, first, farming area. Recall that the scope of this data is the entire Brazilian territory, and we use it to estimate farm-level effects on agriculture. For an approximate 1 km buffer radius, we have outcome data on 374 farms, with 854 candidates financed by their owners. For an approximate 5 km buffer radius, we have outcome data on 669 farms, with 1,231 candidates financed by their owners. For an approximate 10 km buffer radius, we have outcome data on 733 farms, with 1,403 candidates financed by their owners. Given that we identified 968 formal farms whose owners donated to municipal candidates, the share of missing outcome data, at farm level, ranges from 61.4% to 24.3%, depending on buffer size. These imply regression subsamples at candidate-election level that range from 854 to 1,403, in size.

Now consider deforested area. Recall that the scope of this data are the Cerrado and Amazon biomes, and we use it to estimate farm-level effects on deforestation. In this case, for an approximate 1 km buffer radius, we have outcome data on 46 farms, with 309 candidates financed by their owners. For an approximate 5 km buffer radius, we have outcome data on 292 farms, with 743 candidates financed by their owners. For an approximate 10 km buffer radius, we have outcome data on 380 farms, with 895 candidates financed by their owners. Given that we identified 968 formal farms whose owners donated to municipal candidates, the share of missing outcome data, at farm level, ranges from 95.2% to 60.7%, depending on buffer size. These imply regression subsamples at candidate-election level that range from 309 to 895, in size.

Overall, our results are not sensible to buffer size, as we show in Section 5. For the majority of our regressions, results are qualitatively similar under approximate 5 km and 10 km buffer radii, and almost always insignificant under an approximate 1 km buffer radius. We interpret this fact according to our belief that formally established farms of electorally declared campaign donors are probably large, in terms of total area. This would imply that the smallest buffer size is unable to capture the totality of land covered by these plausibly large farms.[16]

[16] An alternative interpretation would be that our spatial outcome data is not fine enough to capture variations within these smaller buffers. This isn't consistent, however, with the fact that our spatial outcome data captures

Finally, when deciding on which buffer radius to use as a benchmark for obtaining descriptive statistics, our main results, and evaluating the validity of our empirical strategy, we opted for the less extreme buffer radius: approximately 5 km. Our belief is that this buffer radius should be large enough to capture within-farm variation for large farms, without excessively capturing variation outside the farms.

We now depict the 968 formal farms whose owners donated to municipal candidates in the 2008 or 2012 Brazilian municipal elections. Figure 1 plots these farms within the borders of the six main Brazilian biomes: Amazon, a tropical rainforest; Cerrado, a savannah; Caatinga, a semi-arid; the Pantanal wetlands; Atlantic Forest, a coastal biome; and the Pampas lowlands. We can see that there are few farms located in the Amazon rainforest, with most farms being located in the Cerrado and Atlantic Forest biomes.

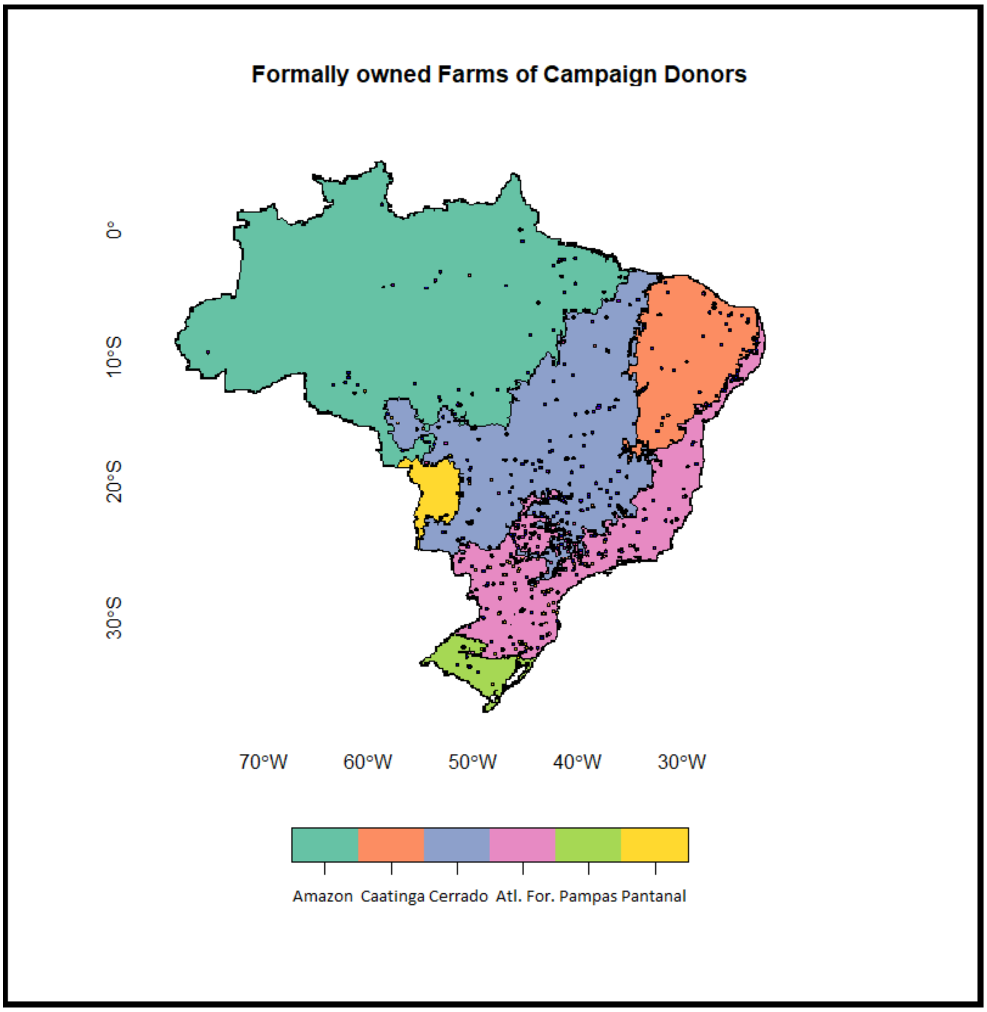

Figure 1 – Geographical distribution of campaign donors' farms, within the borders of Brazilian biomes.

uni-dimensional variations much smaller than 1 km, as described earlier.

Table 2 – Descriptive Statistics - Candidates Financed and Not Financed by Farmers

	Mean	S.D.	Min.	Q1	Median	Q3	Max.	Obs
Candidates financed by farmers - Farms with data on agriculture								
Candidate was elected	.46	.5	0	0	0	1	1	1,231
Candidate ran for mayor	.45	.5	0	0	0	1	1	1,231
Total votes received by the candidate	13,640	43,853	1	442	2,257	7,591	778,514	1,231
Candidate ran in the 2008 election	.71	.45	0	0	1	1	1	1,231
Candidate ran in the state where all donor farms are located	.75	.43	0	1	1	1	1	1,192
Number of farmers financing the candidate;	1.2	1.1	1	1	1	1	25	1,231
Total donations received from farmers (R$)	18,158	93,158	.01	500	3,000	10,000	2,010,000	1,231
Candidate ran in a Legal Amazon municipality	.09	.29	0	0	0	0	1	1,231
Agriculture share in municipal GDP	.16	.16	0	.021	.093	.25	.72	1,231
Candidate completed college	.44	.5	0	0	0	1	1	1,231
Male candidate	.85	.35	0	1	1	1	1	1,231
Married candidate	.72	.45	0	0	1	1	1	1,231
Candidates financed by farmers - Farms with data on deforestation								
Candidate was elected	.41	.49	0	0	0	1	1	743
Candidate ran for mayor	.36	.48	0	0	0	1	1	743
Total votes received by the candidate	11,490	37,993	1	195	1,406	5,830	371,083	743
Candidate ran in the 2008 election	.75	.43	0	1	1	1	1	743
Candidate ran in the state where all donor farms are located	.68	.47	0	0	1	1	1	716
Number of farmers financing the candidate;	1.1	.57	1	1	1	1	9	743
Total donations received from farmers (R$)	17,054	70,489	1	140	1,300	6,430	861,250	743
Candidate ran in a Legal Amazon municipality	.12	.32	0	0	0	0	1	743
Agriculture share in municipal GDP	.18	.17	0	.024	.11	.29	.74	743
Candidate completed college	.38	.48	0	0	0	1	1	743
Male candidate	.84	.36	0	1	1	1	1	743
Married candidate	.72	.45	0	0	1	1	1	743
Candidates not financed by farmers								
Candidate was elected	.16	.36	0	0	0	0	1	710,725
Candidate ran for mayor	.04	.19	0	0	0	0	1	710,725
Total votes received by the candidate	491	9,375	0	37	108	278	3,790,558	710,725
Candidate ran in the 2008 election	.44	.5	0	0	0	1	1	710,725
Candidate ran in the state where all donor farms are located	.	.	.	.	.	.	.	710,725
Number of farmers financing the candidate	0	0	0	0	0	0	0	710,725
Total donations received from farmers (R$)	0	0	0	0	0	0	0	710,725
Candidate ran in a Legal Amazon municipality	.16	.37	0	0	0	0	1	710,725
Agriculture share in municipal GDP	.15	.15	0	.027	.12	.24	.81	710,233
Candidate completed college	.19	.39	0	0	0	0	1	710,720
Male candidate	.73	.44	0	0	1	1	1	710,720
Married candidate	.60	.49	0	0	1	1	1	710,720

Notes: This table reports summary statistics for the candidates that participated in the 2008 and 2012 Brazilian municipal elections. "Candidates financed by farmers" are candidates with at least one electorally declared campaign donor who is the owner of a formally established farm. "Candidates not financed by farmers" are candidates that don't have any such donors. Under an approximate 5 km buffer radius, we have data on farm-level agriculture for 669 farms, and data on farm-level deforestation for 292 farms located in the Amazon or Cerrado biomes. These samples serve as regression samples for estimating effects on farm-level agriculture and deforestation, respectively.

Table 2 presents summary statistics for each of the following samples. First, for the 1,231 candidates financed by farmers for whom we have post-electoral data on farming area within at least one of the farms associated to them, under an approximate 5 km buffer radius. This first sample serves as our main regression subsample, when estimating effects on farm-level agriculture. Second, for the 743 candidates financed by farmers for whom we have post-electoral data on deforested area within at least one of the farms associated to them, under an approximate 5 km buffer radius. This second sample serves as our main regression subsample, when estimating effects on farm-level deforestation. Third, for the 710,725 candidates without formal farmer donors who ran in these elections. This third sample serves to illustrate the differences between candidates financed and not financed by farmers.

Candidates financed by farmers are different from candidates not financed by farmers. Candidates financed by farmers have higher election rates, run for mayorship more often, and are more educated than candidates not financed by farmers. Interestingly, these differences mirror the differences between farmer and non-farmer candidates.

3.3 Normalizations

There is a timing mismatch between our outcomes, which have either an annual or biennial frequency; and our treatment, which occurs only once every four years: being elected. This led us to transform our municipal and farm level outcomes, in order to capture effects that encompass the entirety of an electoral cycle.[17] Moreover, for the particular case of farm-level outcomes, there are additional transformations to be made, in order to capture effects over the entire set of farms associated to a particular candidate.[18] This subsection formally presents the totality of these transformations, which lead to the dependent variables that effectively appear in our RD's.

Recall that, in the sample of farmer candidates, we are interested in estimating effects on municipal-level agriculture and deforestation. Since we are interested in effects over the entirety of the four-year election cycle, we transform our *yearly* outcome data as follows. Consider an arbitrary farmer candidate i, who ran for office in municipality j, in electoral cycle τ. Let $Y_{j,\tau,t}$ be either planted or deforested area at municipality j, in year t, during electoral cycle τ. We then define $Y_{i,\tau}$, the dependent variable of our municipal-level regressions, as the across-year within-electoral cycle average of $Y_{j,\tau,t}$. That is:

$$Y_{i,\tau} = \frac{\sum_{t} Y_{j,\tau,t}}{4} \tag{1}$$

This is the only transformation that is required in our sample of farmer candidates. When it comes to candidates financed by farmers, however, we need to handle not only this matter, but also the fact that the relevant effect is over the entire set of farms whose owners donated to a particular candidate, in a given election. Recall that, in this sample, we are interested in effects on farm-level agriculture and deforestation. These outcomes are measured by the area of the intersections between spatial buffers centered on campaign donors' farms, and agriculture and deforestation shapefiles.

Being so, let h be an arbitrary spatial intersection between the buffer of radius r surrounding farm j in time t, and either a farming or deforested land shapefile. Farm j belongs to a campaign donor of candidate i, who is running for an office during electoral cycle τ. Intersection

[17] We define an electoral cycle as the four-year period between municipal elections.

[18] This is clearly not an issue for farmer candidates, since they can only be associated to the outcomes of one municipality: the municipality in which they ran.

h has an area equivalent to $Y_{j,\tau,r,t,h}$. The first step we take is to sum the areas of all intersections, within each farm-time. This gives us $Y_{j,\tau,r,t}$, the total farming or deforested area associated to farm j, in time t, during electoral term τ, under buffer radius r:

$$Y_{j,\tau,r,t} = \sum_h Y_{j,\tau,r,t,h} \tag{2}$$

We then take the across-time within-electoral cycle average of $Y_{j,\tau,r,t}$, obtaining $Y_{j,\tau,r}$, the average farmed or deforested area associated to farm j, during the four-year electoral cycle τ, under buffer radius r:

$$Y_{j,\tau,r} = \frac{\sum_t Y_{j,\tau,r,t}}{T} \tag{3}$$

Where $T = 4$, for our *yearly* georeferenced deforestation data, and $T = 2$, for our *biennial* georeferenced farming data. Finally, let $N_{i,\tau}$ be the number of farms associated to candidate i in the election preceding electoral cycle τ. We then take the average of outcome $Y_{j,\tau,r}$ across all farms whose owners donated to candidate i, in the election associated to τ. This defines $Y_{i,\tau,r}$, the dependent variable of our farm-level regressions, as average deforested or farmed area associated to candidate i, during electoral cycle τ, under buffer radius r, across the farms of his/her campaign donors:

$$Y^*_{i,\tau,r} = \frac{\sum_j Y_{j,\tau,r}}{N_{i,\tau}} \tag{4}$$

Table 3 presents summary statistics for these variables, which are the dependent variables of our regressions, within each regression subsample.

Table 3 – Descriptive Statistics - Normalized Outcomes

	Mean	S.D.	Min.	Q1	Median	Q3	Max.	Obs
Farmer candidates								
Municipal planted area (km^2)	137	403	0	16	45	115	10,885	63,576
Municipal deforested area (km^2)	1,493	2,071	0	233	929	1,950	17,605	13,593
Candidates financed by farmers								
Across-donor average farming area (km^2)	40	25	.0011	18	42	54	95	1,231
Across-donor average deforested area (km^2)	.77	.88	.012	.16	.48	.69	7.1	743

Notes: This table reports the summary statistics of the dependent variables of our regressions. "Farmer candidates" are candidates that declared at least one rural property. "Candidates financed by farmers" are candidates who were financed by at least one owner of a formally established farm. Municipal-level outcomes, related to farmer candidates, are across-time averages of the outcome under consideration, evaluated in the municipality in which the candidate ran, over the four years after the election. Farm-level outcomes, related to candidates financed by farmers, are across-donor and time averages of the outcome under consideration, evaluated in farms whose owners donated to the candidate, over the four years after the election, under a buffer radius size of approximately 5 km.

4 Empirical Strategy

In this section we formally delineate our empirical strategy. We first specify our regressions, and then provide evidence supporting the validity of our study design, for each regression subsample.

4.1 Specification

We essentially have two parameters of interest. First, the causal effect of electing a farmer candidate on post-electoral agricultural production and deforestation, within the municipality in which he ran. Second, the causal effect of electing a candidate financed by farmers on post-electoral deforestation and agricultural production, within the farms of the campaign donors of such candidate. We estimate the first parameter using our sample of farmer candidates; the second, using our sample of candidates financed by farmers. Suppose the following population model:

$$Y_{i,\tau} = \alpha + \beta T_{i,\tau} + W'_{i,\tau}\rho + \nu_{i,\tau} \tag{5}$$

Where $Y_{i,\tau}$ is any of the dependent variables defined in the previous section; $T_{i,\tau}$ is a dummy indicating whether candidate i was elected in the election associated to electoral cycle τ; $W'_{i,\tau}$ is a vector of controls, including election and candidate fixed effects; and $\nu_{i,\tau}$ is an idiosyncratic error term. We are interested in estimating β, interpreted as the causal effect of electing a particular type of candidate - a farmer, or a candidate financed by farmers - on outcome $Y_{i,\tau}$.

A naive empirical strategy would then consist on, in each of our main subsamples, regressing $Y_{i,\tau}$ against $T_{i,\tau}$ and $W'_{i,\tau}$. The main issue with this strategy is, obviously, that we cannot guarantee that $\mathbb{C}(T_{i\tau}, \nu_{i\tau}|W_{i,\tau}) = 0$. That is, even if we control for the covariates that are observable to us - gender, education, civil status, among others - and for election and candidate fixed effects, we might still have unobservables that evolve through time and correlate with $T_{i\tau}$ and $Y_{i\tau}$, biasing the estimate of β, our parameter of interest.

As an example, consider political skill. It is certainly an unobservable characteristic, and it plausibly evolves through time. It also probably affects both the probability of being elected, and the capacity to affect agricultural and environmental outcomes, either at municipal or farm level. In terms of our theoretical framework, a politically skillful candidate might be better able to steer policy towards her own priorities, and may be able to repay her campaign debts more effectively, via environmental regulation.

This threat motivates our quasi-experimental design. We exploit close municipal elections to identify a regression discontinuity design, based on the theoretical results found in Lee (2008). A vast and growing applied literature employs this same strategy, based on Lee's seminal contribution (Meyersson, 2014; Boas and Hidalgo, 2011; Ferreira and Gyourko, 2014; Brollo and Troiano, 2016; Eggers and Hainmueller, 2009). The standard approach in this literature is defining a running variable such that the cutoff of the RD represents a perfect electoral tie. The most intuitive implication is that, for candidates with running variable in a sufficiently small neighborhood located around the cutoff, the electoral outcome is as-if-random.

More formally, the validity of this strategy depends on the conditional distribution of potential outcomes being continuous, with respect to the running variable, around this cutoff. Under this condition, our strategy provides consistent estimates of the average treatment effect, for the population with margin of votes located in the neighborhood of the cutoff (Hahn et al., 2001; Imbens and Lemieux, 2008). An established investigation of the plausibility of this hypothesis consists in testing for the presence of mass points in the density of the running variable, at either side of the cutoff. Additionally, one may obtain RD estimates of the effect on baseline covariates, to evaluate the local randomization interpretation of this empirical strategy (Cattaneo et al., 2019a; Lee, 2008). We run both of these formal tests in the next subsection, where we obtain evidence supporting the validity of our design.

We now properly define our running variable, and the cutoff of our RD's. As was discussed in Section 2, the institutional peculiarities of the Brazilian electoral system led us to construct a running variable that captures electoral performance relative to the members of a candidate's coalition (*coligação*), as this is the criterion that determines the candidate's electoral outcome. For generality, consider that, for mayorship elections, this system works as if all candidates belong to the same coalition.

More formally, let $V_{i,c,\tau}$ be the share of within-coalition votes of candidate i, belonging to coalition c, at election τ. Also let $Y_{i,c,\tau}$, $T_{i,c,\tau}$ and $\nu_{i,\tau}$ be as before. If the candidate is running for mayorship, fix $c = C$, so that the election behaves as if all candidates belonged to the same coalition. Now let $\Psi_{i,c,\tau}$ be defined as follows:

$$\Psi_{i,c,\tau} = \begin{cases} \max_k \{V_{k,c,\tau}, \text{if } T_{k,c,\tau} = 0\}, \text{if } T_{i,c,\tau} = 1 \\ \min_k \{V_{k,c,\tau}, \text{if } T_{k,c,\tau} = 1\}, \text{ if } T_{i,c,\tau} = 0 \end{cases} \tag{6}$$

Our running variable is then defined as $M_{i,c,\tau} = V_{i,c,\tau} - \Psi_{i,c,\tau}$[19] This variable is clearly defined only for elected candidates belonging to a coalition with at least one unelected member, and for unelected candidates belonging to a coalition with at least one elected member. We will follow Ferreira and Gyourko (2014) and refer to this forcing variable as *margin of votes*. Under this definition, the cutoff of our RD's simply corresponds to a margin of votes equal to zero. This is the point corresponding to an exact electoral tie between first-placed candidates belonging to the same electoral coalition.

[19] This definition of forcing variable, adjusted to the peculiarities of the Brazilian proportional system, is similar to the definition in Boas and Hidalgo (2011).

We now justify our preferred specification, before formalizing it. In our preferred specification, we estimate our parameters of interest via local linear regression. This decision was made in light of Gelman and Imbens (2019), who argue that RD's based on higher-order polynomials lead to lower coverage rates, and wider confidence intervals. We also use CER-optimal bandwidths. Calonico et al. (2018) explain that, since both MSE-based and CER-based bandwidths are asymptotically valid, "the choice must reflect each researcher's preference for length vs. coverage error". We also use triangular kernel weights. In terms of inference, standard errors are clustered at the candidate-election level, as this is the level at which treatment occurs.[20]

The inclusion of covariates in regression discontinuity designs seems to be an unsettled topic in the econometric literature on RD's. For instance, while Imbens and Lemieux (2008) and Calonico et al. (2019) recognize that covariates may mitigate bias stemming from excessively large bandwidths, Lee and Lemieux (2010) consider their usefulness only insofar as they may improve the precision of the RD estimate, by reducing the variance of the error term. We thus opted to control only for a vector of time-specific dummies, and evaluate heterogeneities not via factor variables, but rather by testing for effects in different subsamples.[21]

We now formalize our preferred specification. Fix F as a vector of time-specific dummies, and consider subscripts r and l as indicating whether the associated variable corresponds to the right or the left of the RD cutoff, respectively. Finally, let $H_{CER(Y,r)}$ be the CER-optimal bandwidth for outcome Y to the right of the cutoff, and $H_{CER(Y,l)}$ the CER-optimal bandwidth for outcome Y to the left of the cutoff. Our RD's then have the following main specification:

$$Y_{i,c,\tau} = \alpha_L + (\alpha_R - \alpha_L)T_{i,c,\tau} + \beta_L M_{i,c,\tau} + (\beta_R - \beta_L)T_{i,c,\tau}M_{i,c,\tau} + F'\rho + \nu_{i,c,\tau} \tag{7}$$

Such that

$$\begin{cases} |M_{i,c,\tau}| \leq |H_{CER(Y,L)}|, \text{if } M_{i,c,\tau} < 0 \\ |M_{i,c,\tau}| \leq |H_{CER(Y,R)}|, \text{if } M_{i,c,\tau} \geq 0 \end{cases} \tag{8}$$

And

$$\begin{cases} \exists(k,c,\tau) : T_{k,c,\tau} = 1, \text{if } T_{i,c,\tau} = 0 \\ \exists(k,c,\tau) : T_{k,c,\tau} = 0, \text{if } T_{i,c,\tau} = 1 \end{cases} \tag{9}$$

[20] Appendices A and B provide robustness checks for all these specification details.
[21] Appendices A and B provide results using alternative control sets.

In Equation 7, our parameter of interest is $(\alpha_R - \alpha_L)$, the difference in the intercepts of the local linear regressions to the right and to the left of the cutoff (Imbens and Lemieux, 2008). Conditional on our identification assumption, this is the average treatment effect either of electing a farmer candidate (first subsample) or of electing a candidate financed by farmers (second subsample) on outcome Y, for the population of candidates with margin of votes located within a sufficiently small neighborhood around the RD cutoff.

Additionally, Equation 8 simply restricts the sample to observations with a sufficiently small margin of votes, given the CER-optimal bandwidth for the specified outcome. Equation 9 is an immediate consequence of our running variable being defined only for elected (unelected) candidates belonging to a coalition with at least one unelected (elected) member.

4.2 Validity

We now properly run sorting and balancing tests, to investigate the validity of our identification assumption. We first present density tests for each regression subsample. This first set of tests will help us gauge the plausibility of our main identification assumption: local continuity of the conditional distribution of potential outcomes, with respect to the running variable. We then present balancing tests for each regression subsample. This second set of tests will help us evaluate whether a local randomization interpretation of our strategy makes sense.

We ran manipulation tests based on Cattaneo et al. (2019b)[22]. Under the null of this test, there is no discontinuity in the density of the running variable, around the cutoff. Hence, a failure to reject this null would constitute evidence in favor of our empirical strategy. Figures 2 and 3 present the results of running this manipulation test in each regression subsample of the sample of farmer candidates. In both cases we fail to reject the null of no manipulation at conventional levels, but only marginally. Figures 4 and 5 present the results of running this manipulation test in each regression subsample of the sample of candidates financed by farmers. Interestingly, for candidates associated to farms for which we have data on deforested area, we fail to reject the null; but for candidates associated to farms for which we have data on farming area, we again only marginally don't reject the null, at conventional levels.

We believe that these results don't favor a rejection of the null, of absence of manipulation in the running variable. On a more general note, we believe that sorting in close elections is implausible. Candidates are probably unable to sort themselves towards marginal wins, instead of marginal losses, even if they are able to anticipate close elections. In fact, there is a recent literature supporting the general validity of regression discontinuity designs based on close-elections. For instance, Eggers et al. (2015), using data on over 40,000 close-elections, conclude that "the assumptions behind the RD design are likely to be met in a wide variety of electoral settings".

[22] We opted to run this test instead of the one found in McCrary (2008) since Cattaneo et al. (2019b) report simulations in which they demonstrate the larger statistical power of their test, when compared to McCrary's.

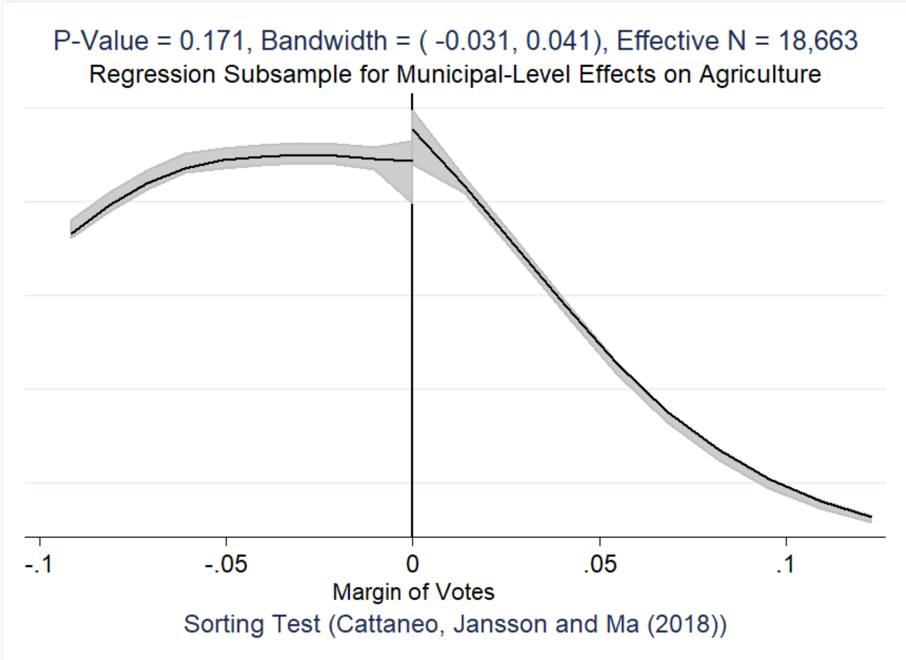

Figure 2 – Density test for the subsample of candidates who declared at least 1 rural property, and ran in a municipality for which we have data on planted area at municipal level.

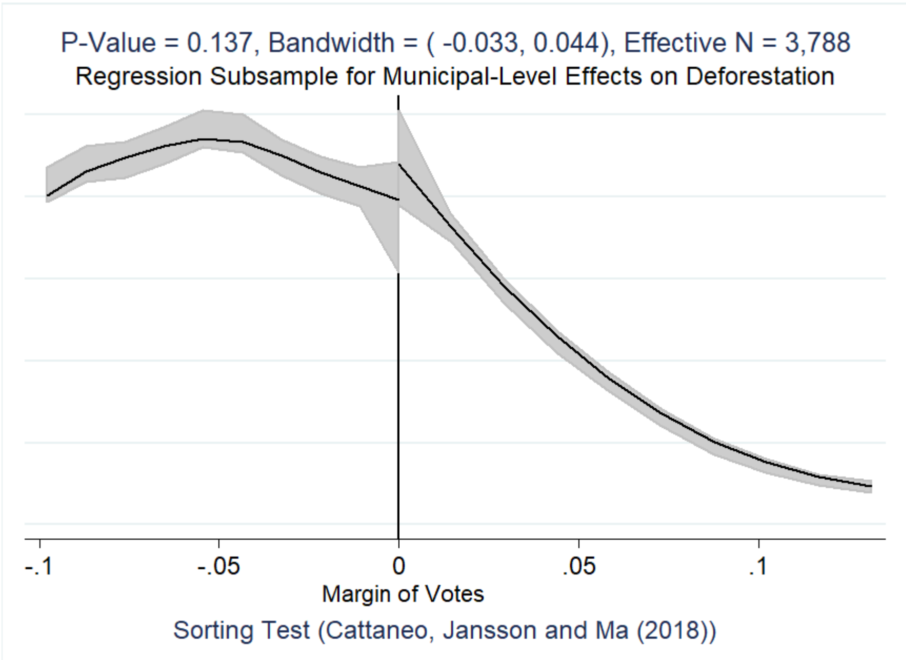

Figure 3 – Density test for the subsample of farmer candidates who declared at least 1 rural property, and ran in a municipality for which we have data on deforested area at municipal level.

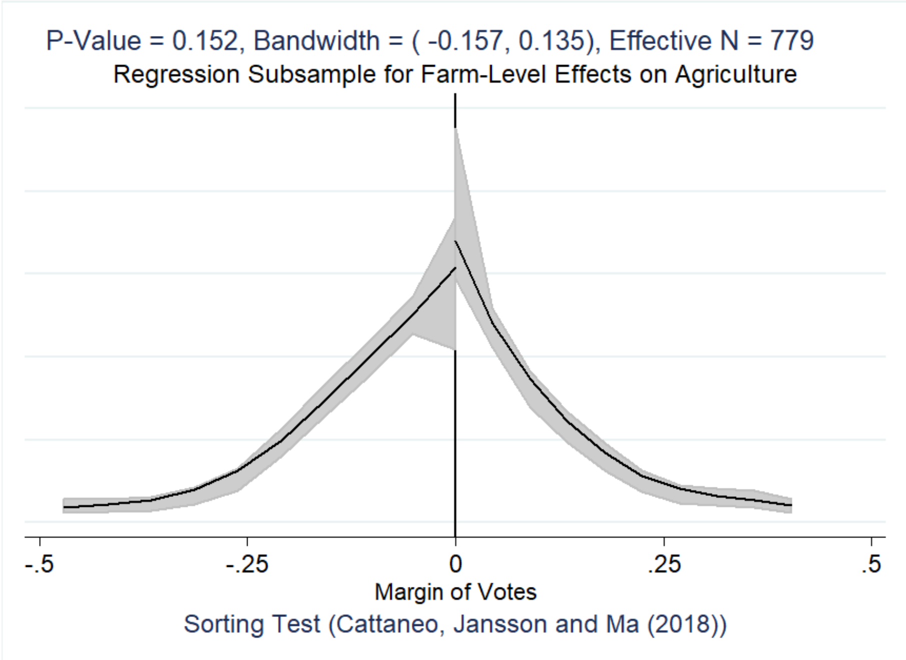

Figure 4 – Density test for the subsample of candidates associated to at least one farm for which we have data on farming area within a buffer centered on it, under a buffer radius of approximately 5 km.

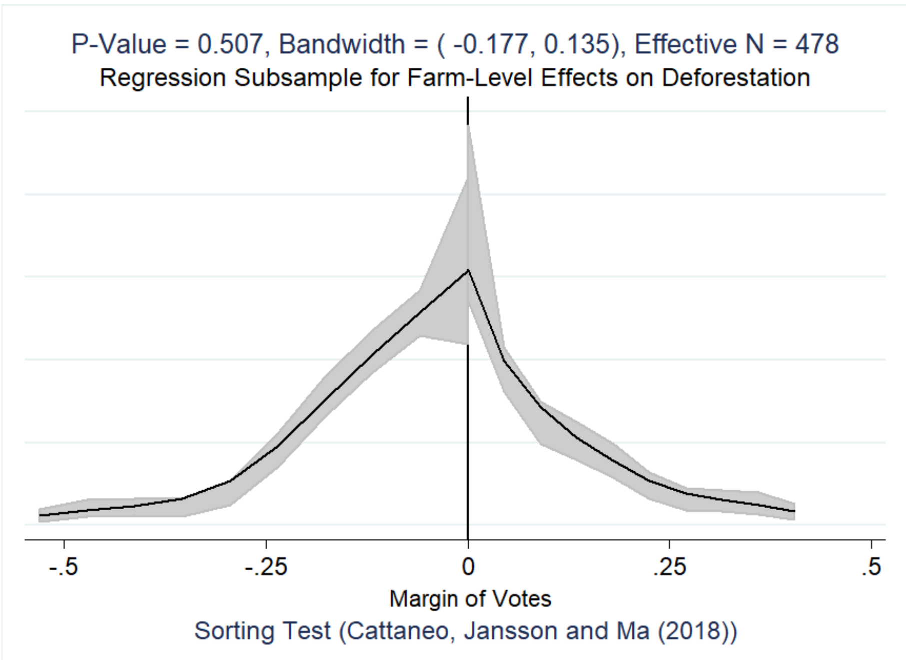

Figure 5 – Density test for the subsample of candidates associated to at least one farm for which we have data on deforested area within a buffer centered on it, under a buffer radius of approximately 5 km.

Additionally, De la Cuesta and Imai (2016) argue that "When multiple-testing problems are also addressed, we find that evidence for sorting in US House elections is substantially weaker and highly dependent on estimation methods".

We now run balancing tests, to evaluate our design in its more demanding interpretation: as a locally randomized experiment. If, in the neighborhood of our cutoff, being elected is as-if-random, then baseline variables should be balanced across treatment assignment, within this range of the running variable. Following Cattaneo et al. (2019a) and Lee (2008), we obtain RD estimates of the treatment effect on available baseline covariates, using our preferred specification. Table 4 reports the results of these regressions, for each regression subsample. We can see that, out of more than 30 regressions, there is only one in which the coefficient is significant, at standard levels. Standard multiple hypotheses testing arguments imply that these tests very strongly support our research design, serving as further support of the claim that our estimates are indeed capturing causal effects.

Table 4 – Balance - Candidates Connected to Farming Interests

	Dependent Variable Mean	RD Estimate	RD Standard Error	P-Value	CER-Optimal Bandwidth	Effective Observations
Farmer candidates - Municipalities with Data on Agriculture						
Candidate ran for mayor	0.068	-0.009	0.009	0.313	0.025	13,537
Number of declared rural properties	1.554	0.049	0.039	0.209	0.038	20,216
Candidate ran in a Legal Amazon municipality	0.182	0.006	0.011	0.603	0.045	23,742
Agriculture share in municipal GDP	0.229	0.009	0.004	0.041	0.045	23,343
Candidate completed college	0.189	0.000	0.011	0.995	0.043	22,396
Male candidate	0.899	-0.012	0.009	0.172	0.041	21,635
Married candidate	0.779	0.007	0.011	0.502	0.048	24,967
Farmer candidates - Municipalities with Data on Deforestation						
Candidate ran for mayor	0.062	-0.016	0.018	0.380	0.033	3,513
Number of declared rural properties	1.443	0.026	0.072	0.715	0.047	4,830
Agriculture share in municipal GDP	.283	.009	.008	.297	.059	5,950
Candidate completed college	0.158	-0.013	0.020	0.510	0.064	6,352
Male candidate	0.873	-0.028	0.020	0.149	0.051	5,181
Married candidate	0.697	-0.001	0.025	0.968	0.057	5,796
Candidates financed by farmers - Farms with data on agriculture						
Candidate ran for mayor	0.378	0.015	0.079	0.853	0.101	603
Candidate ran in the state where all donor farms are located	0.754	-0.045	0.078	0.568	0.096	572
Number of farmers financing the candidate	1.264	0.163	0.385	0.672	0.098	591
Total donations received from farmers (R$)	12,347.580	4,241.305	5,429.601	0.435	0.098	590
Candidate ran in a Legal Amazon municipality	0.081	-0.071	0.055	0.195	0.095	567
Agriculture share in municipal GDP	0.139	0.017	0.025	0.516	0.087	536
Candidate completed college	0.448	0.029	0.093	0.754	0.080	498
Male candidate	0.880	-0.014	0.054	0.797	0.090	550
Married candidate	0.729	0.005	0.070	0.941	0.116	665
Candidates financed by farmers - Farms with data on deforestation						
Candidate ran for mayor	0.308	-0.021	0.092	0.817	0.111	377
Candidate ran in the state where all donor farms are located	0.659	-0.035	0.109	0.746	0.100	347
Number of farmers financing the candidate	1.114	0.179	0.183	0.327	0.133	420
Total donations received from farmers (R$)	11,267.830	-1,379.477	6,426.979	0.830	0.094	322
Candidate ran in a Legal Amazon municipality	0.089	-0.075	0.059	0.200	0.109	370
Agriculture share in municipal GDP	0.149	0.012	0.031	0.693	0.094	322
Candidate completed college	0.397	0.072	0.101	0.474	0.112	378
Male candidate	0.870	-0.047	0.062	0.446	0.084	299
Married candidate	0.732	0.029	0.090	0.746	0.113	381

Notes: This table reports the RD estimates of the effect of being elected on several baseline covariates, for each regression subsample. "Farmer candidates" are candidates that declared at least one rural property. Farm-level outcomes are evaluated under an approximate 5 km buffer radius. The forcing variable of each RD is the margin of votes of each candidate. In all regressions RD bandwidths are CER-Optimal; the degree of the RD polynomial is unitary (local linear regression); and the RD kernel is triangular. We only control for a 2008 election fixed effect. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

5 Results

We now present and discuss our estimates of the environmental and agricultural effects of electing candidates connected to farming interests. For the purposes of clarity, we present our results in two separate subsections, one for each type of candidate: farmer candidates, and candidates financed by farmers. For each subsection, we discuss our priors; present results under our benchmark definitions of farmers and buffer radius; and explore heterogeneities in terms of office disputed.

Since municipal-level effects vary considerably across definitions of farmer candidates, we present this additional heterogeneity, in the subsection centered on farmer candidates. Since farm-level effects are considerably robust to buffer radius choice, we present results under alternative radii only in Appendix B. Appendix A provides robustness checks for the sample of farmer candidates, and Appendix B does so for the sample of candidates financed by farmers. Both appendices provide sorting, balancing and inference-assessing tests for our smallest regression samples. These follow Cattaneo et al. (2019b), Lee (2008) and Ferman (2019), respectively.

5.1 Farmer Candidates

Recall that the first main contribution of this study is testing for the environmental and agricultural effects of electing a particular type of candidate: rural producers. Ex-ante, we expect larger post-electoral deforested and planted areas in the municipalities associated to farmer candidates that marginally won, when compared to the municipalities associated to farmer candidates that marginally lost. This is because we hypothesize that candidates who are rural producers prioritize - or are more able to prioritize - agriculture over environment more, when compared to candidates who are not rural producers, when enacting policy. This can occur via public credit, expenditures, taxation and, crucially, environmental regulation. These differences in policy decisions may, in turn, be based on differences in ideology, policy expertise, or pecuniary interests.

Table 5 presents results for the sample of candidates who declared at least one rural property, our benchmark criterion for identifying farmer candidates.[23] For each outcome, we provide estimates for three different subsamples: farmer candidates running for either councillorship or mayorship (columns (1) and (4)); farmer candidates running for mayorship, only (columns (2) and (5)); and farmer candidates running for councillorship, only (columns (3) and (6)). We can see that, under this definition of farmer candidates, all coefficients are insignificant and of very small magnitude, relative to dependent variable averages. We interpret this first result as evidence that the "average" farmer candidate does not prioritize agriculture over environment, whilst in office.

[23] Recall that it was under this definition of farmer candidates that we presented descriptive statistics, and investigated the plausibility of our identification assumptions.

Table 5 – The Agricultural and Environmental Effects of Electing Farmers

Dependent Variable	Planted Area (km^2) (1)	Planted Area (km^2) (2)	Planted Area (km^2) (3)	Deforested Area (km^2) (4)	Deforested Area (km^2) (5)	Deforested Area (km^2) (6)
RD Estimate	-2.91	9.17	-0.84	-47.85	30.84	-90.54
	(8.95)	(33.68)	(11.62)	(109.83)	(276.66)	(133.54)
Councillors/Mayors	Both	Only Mayors	Only Councillors	Both	Only Mayors	Only Councillors
Dependent Variable Mean	131.751	141.468	127.416	1,378.674	1,137.901	1,342.912
RD Bandwidth	0.054	0.104	0.032	0.056	0.133	0.042
Effective Observations	27,361	3,560	16,268	5,631	757	4,028
Observations	63,576	8,374	55,202	13,593	1,567	12,026

Notes: This table reports the municipal-level effects of electing farmer candidates, with farmer candidates being defined as candidates who declared at least 1 rural property. The forcing variable of each RD is the margin of votes of each candidate. The dependent variable in each column corresponds to the across-time average of the outcome under consideration, evaluated in the municipality in which the candidate ran, over the four years after the election. The estimates in columns (1) to (3) come from the sample of farmer candidates running in the 2008 and 2012 Brazilian municipal elections for whom we have post-electoral data on municipal-level planted area, for the municipality in which they ran. The estimates in columns (4) to (6) come from the sample of farmer candidates running in the 2008 and 2012 Brazilian municipal elections for whom we have post-electoral data on municipal-level deforested area, for the municipality in which they ran. In all regressions RD bandwidths are CER-Optimal; the degree of the RD polynomial is unitary (local linear regression); and the RD kernel is triangular. We only control for a 2008 election fixed effect. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

We now evaluate whether the agricultural and environmental effects of electing farmers vary in the type of farmer being analyzed. We do this by estimating these effects within subsamples that impose different minimum amounts of declared rural properties. We first evaluate this possible heterogeneity for municipal-level agriculture. Figure 6 presents point estimates and 95% confidence intervals of the effect of electing a farmer candidate on municipal-level planted area (measured in km^2), under alternative definitions of farmer candidates.[24] We can see that point estimates increase steadily, for samples in which the minimum number of declared rural properties is greater than or equal to 5. Although confidence intervals widen as we increase this minimum requirement, effects become significant at 7 or more and 8 or more rural properties, with p-values of 8.2% and 7.1%, respectively. For this outcome, sample sizes range from 63,576, when we require only one rural property to consider a candidate as a farmer, to 575, when we require at least 8.[25]

[24] All regressions follow our preferred RD specification: local linear regression, CER-based bandwidths, triangular kernel and a control set consisting only of an election fixed effect.

[25] Appendix A provides sorting, balancing and inference-assessing tests which support our decision of obtaining RD estimates from these small samples.

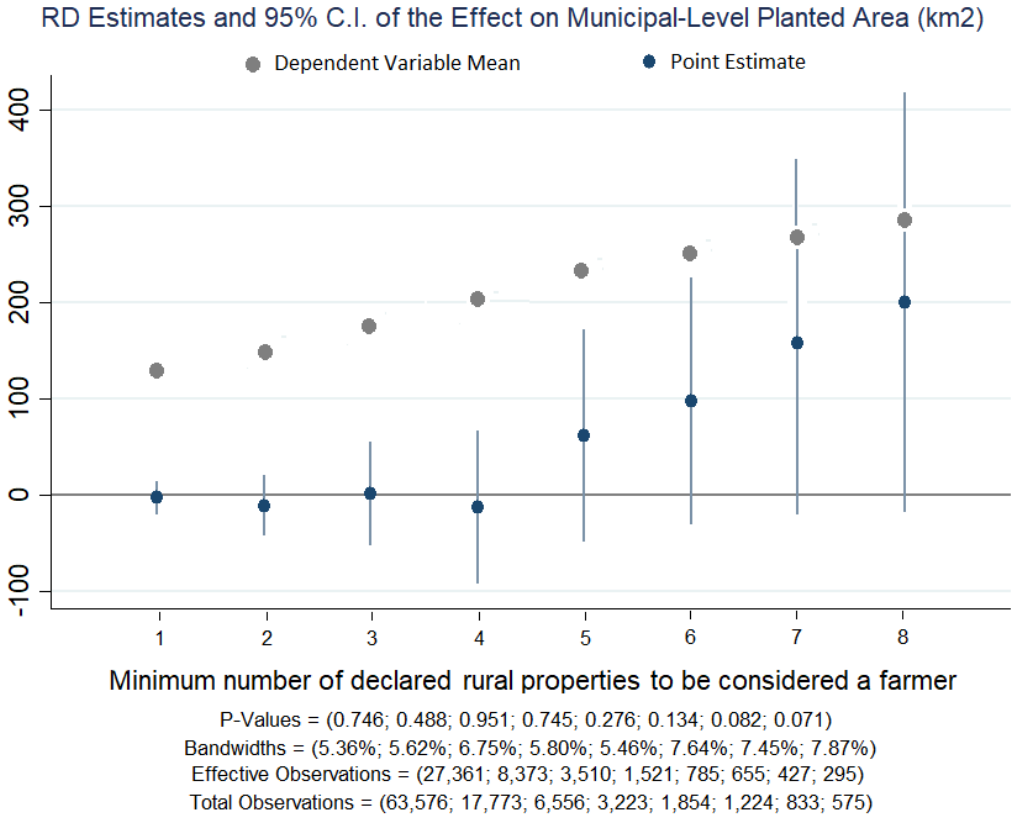

Figure 6 – Estimates and 95% confidence intervals of the effect of electing a farmer candidate on municipal-level planted area, under different definitions of farmer candidates.

Before interpreting these results, we empirically evaluate the existence of this heterogeneity in declared rural properties, in terms of municipal-level deforestation. Figure 7 presents point estimates and 95% confidence intervals of the effect of electing a farmer candidate on municipal-level deforested area (measured in km^2), under alternative definitions of farmer candidates. We can see that point estimates increase steadily, for samples in which the minimum number of declared rural properties to be considered a farmer is greater than or equal to 3. Although confidence intervals widen as we increase this minimum requirement, effects become significant at 3 or more and 4 or more rural properties, with p-values of 4.5% and 6.8%, respectively. For this outcome, sample sizes range from 13,593, when we require only one rural property to consider a candidate as a farmer, to 444, when we require at least 4.[26]

The results depicted in Figures 6 and 7 are consistent with the idea that, even though "average" farmers do not prioritize agriculture over environment whilst in office, larger farmers do so. Be it via public credit, expenditures, taxation, or environmental regulation, these largest farmers seem to be promoting an expansion of planted areas within the municipalities in which they are elected, increasing deforested areas in the process.

[26] Appendix A provides sorting, balancing and inference-assessing tests which support our decision of obtaining RD estimates from these small samples.

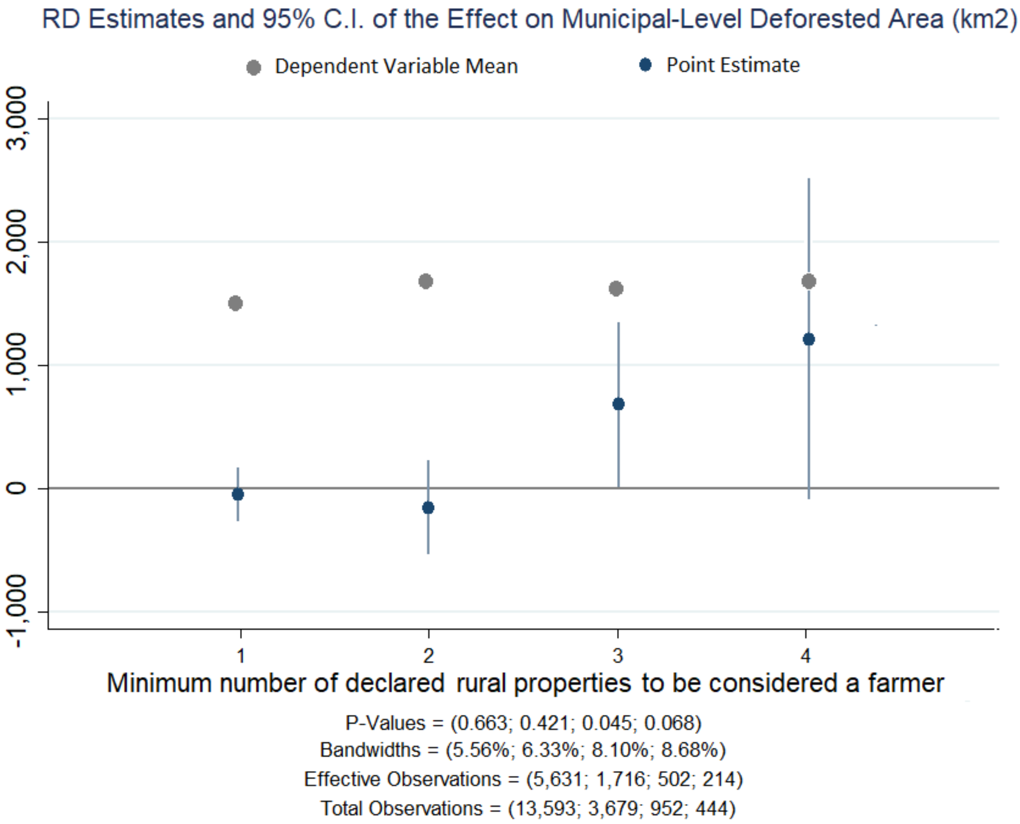

Figure 7 – Estimates and 95% confidence intervals of the effect of electing a farmer candidate on municipal-level deforested area, under different definitions of farmer candidates.

Taken together, our municipal-level results suggest that the environmental and agricultural effects of electing a particular type of candidate, rural producers, depend on the extent to which these candidates fit into this particular type of politician. In a more general sense, this "intensive margin" of candidate type may be relevant to understanding the effects of electing particular types of politician. This possibility has not received much attention by the literature, which has generally followed a "extensive margin" interpretation of candidate types.

5.2 Candidates Financed by Farmers

We now focus on the second main contribution of this study: testing for the possible existence of a donor-politician relationship based on strategic environmental regulatory leniency. Ex-ante, this relationship could exist, since donors and politicians may prefer this arrangement over other, more established forms of quid-pro-quo. For instance, donors who are farmers have little to gain by obtaining public sector jobs and, in some settings, a localized reduction in environmental regulation might be more feasible, for politicians, than allocating public credit toward specific individuals.

Nevertheless, the possibility of campaign financing strategies based on alternative forms of state capture does exist. In fact, candidates may prefer to repay the rural producers who finance their campaigns in different ways, depending on the municipal office for which they were elected. For instance, mayors may use their power over environmental regulation to decrease environmental restrictions, in specific locations. On the other hand, councillors may use their power over the municipal budget to bargain an allocation of rural credit towards specific farmers, affecting areas with vegetation only indirectly, via financed expansions of farmed land. Under these hypotheses, one could expect larger farmed areas within the farms of campaign donors of candidates running for either municipal office, but larger deforested areas only for the campaign donors who financed candidates running mayorship. In both cases, however, these effects would be coming at the expense of areas with vegetation, located within these farms.[27]

We now present our farm-level results, under our benchmark buffer radius of approximately 5 km.[28,29] Table 6 presents these results, where we again provide estimates, for each outcome, for three different subsamples: candidates who are running for either councillorship or mayorship (columns (1) and (4)); candidates who are running for mayorship, only (columns (2) and (5)); and candidates who are running for councillorship, only (columns (3) and (6)).[30]

Table 6 – The Agricultural and Environmental Effects of Electing Candidates Financed by Farmers

Dependent Variable	Farming Area (km^2) (1)	Farming Area (km^2) (2)	Farming Area (km^2) (3)	Deforested Area (km^2) (4)	Deforested Area (km^2) (5)	Deforested Area (km^2) (6)
RD Estimate	5.51	0.02	7.83*	-0.35	0.43*	-0.67**
	(4.07)	(7.82)	(4.76)	(0.25)	(0.24)	(0.33)
Councillors/Mayors	Both	Only Mayors	Only Councillors	Both	Only Mayors	Only Councillors
Dependent Variable Mean	39.276	40.116	38.165	0.751	0.468	0.882
RD Bandwidth	0.103	0.096	0.095	0.098	0.103	0.098
Effective Observations	611	222	350	338	111	229
Observations	1,231	553	678	743	265	478

Notes: This table reports the farm-level effects of electing candidates financed by farmers, with outcomes evaluated within spatial buffers of an approximate 5 km radius, centered on campaign donors' farms. The forcing variable of each RD is the margin of votes of each candidate. The dependent variable in each column corresponds to the across-time and donor average of the outcome under consideration, evaluated in the farms associated to each candidate, over the four years after the election. The estimates in columns (1) to (3) come from a sample of candidates running in the 2008 and 2012 Brazilian municipal elections for whom we have post-electoral data on farm-level farming area, for at least one of the farms associated to them. The estimates in columns (4) to (6) come from a sample of candidates running in the 2008 and 2012 Brazilian municipal elections for whom we have post-electoral data on farm-level deforested area, for at least one of the farms associated to them. In all regressions RD bandwidths are CER-Optimal; the degree of the RD polynomial is unitary (local linear regression); and the RD kernel is triangular. We only control for a 2008 election fixed effect. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

We can see that, under this buffer radius choice, pooling municipal offices leads to statistically insignificant effects, at standard levels. In fact, the coefficients in columns (1) and (4) have associated p-values of 17.7% and 16%, respectively. Nonetheless, when we dissociate the effects of electing candidates financed by farmers as mayors from the effects of electing

27 In fact, a given farm should consist in: farmed areas; areas with vegetation; deforested areas. If farm size is fixed, increases in farmed and deforested areas can only occur at the expense of areas with vegetation.

28 Recall that it was under this buffer radius size that we presented descriptive statistics, and investigated the plausibility of our identification assumptions.

29 We present farm-level results under alternative buffer radius choices in Appendix B.

30 Appendix B provides sorting, balancing and inference-assessing tests which support our decision of obtaining RD estimates from these small subsamples.

them as councillors, we find interesting results, under this buffer radius choice. While electing candidates financed by farmers as councillors leads to significant increases (decreases) in farming (deforested) area within campaign donors' farms, with positive effects on farmed areas that are far more sizable, electing them as mayors leads to significant increases in deforested area within campaign donors' farms, with insignificant effects on farming area. Under fixed farm areas, these results imply that electing mayors or councillors financed by rural producers leads to a reduction in the areas with vegetation that are located within the farms of their campaign donors.

These results, although possibly explained by systematic manipulation of local environmental regulation by candidates running for both types of municipal offices, could also be explained by other mechanisms. Rural credit, for instance, may explain the relatively large positive effects on farmed areas within campaign donors' farms, associated to electing councillors financed by farmers. Campaign donors may use this directed credit to finance expansions of farmed areas within their farms, thus leading to reductions in deforested areas and areas with vegetation located within such farms (under fixed farm sizes). On the other hand, the fact that electing a mayor financed by rural producers leads only to increases in deforested areas, with no significant effect on farmed areas, suggests that mayors may in fact be engaging in systematic manipulation of local environmental regulation.

This heterogeneity in terms of municipal office might be explained by the different institutional mechanisms that each type of municipal official is more able to control. Mayors can demand regulatory lenience toward specific farmers, from appointed members of local environmental agencies, in exchange for patronage. They can also appoint lenient individuals, to these agencies. This would enable rural producers to clear more forest within their farms, with no obvious effects on farmed land. On the other hand, councillors may use their power over the municipal budget to demand, from the mayor, an allocation of rural credit towards the rural producers that financed their electoral campaigns. These rural producers may then use this credit to turn previously deforested areas and areas with vegetation within their farms into productive farmed land.

Taken together, our farm-level results suggest that a donor-politician relationship based on strategic environmental regulatory leniency might, in fact, exist. Furthermore, this seems particularly likely, when it comes to mayors financed by rural producers. This makes sense, since the margin of environmental regulation that mayors control, enforcement, is far more flexible than the margin that councillors control, legislation. Furthermore, our results suggest that the long-standing focus of the state capture literature on patronage, public credit and trade policy may have left uncovered other occurring forms of quid-pro-quo between public office holders and private agents, such as manipulation of environmental policy.

6 Conclusion

There is vast evidence supporting the idea that politicians favor specific individuals through the powers vested in them. Additionally, there is a solid body of evidence supporting the idea that the election of different types of candidates leads to different types of policy. What we still don't know is whether and how can environmental policy be used to promote the interests of connected individuals, and what types of policies rural producers enact, when given a shot at public office. This paper tries to answer those questions by comparing farms and cities associated to candidates that participate in close elections. More specifically, we test whether the marginal election of a rural producer leads to larger planted and deforested areas, in the municipality in which she or he were elected. Additionally, we test whether the marginal election of a candidate financed by farmers leads to larger farmed and deforested areas, within the farms of his or her campaign donors.

The first main fact suggested by our results is that the policies enacted by rural producers heavily depend on the extent to which the defining characteristic of these individuals is, in fact, being a farmer. Indeed, while the election of candidates who declare smaller amounts of farms has no significant effect on the state of municipal agriculture and environment, the election of candidates who declare larger number of farms does. The municipalities that elect these types of candidates present larger post-electoral increases in planted and deforested areas. These variations might in turn be driven by differential policy decisions in terms of public credit, expenditures, taxation and, most interestingly, environmental regulation.

The second main fact suggested by our results is that campaign financing strategies based on state capture differ, across municipal offices. While it seems that councillors are settling campaign debts towards rural producers via mechanisms other than the manipulation of environmental regulation, it does seem that mayors are favoring their campaign donors via environmental policy. In fact, farms whose owners financed winning mayors present larger post-electoral deforested areas, when compared to the farms whose owners financed losing mayors. Since these effects do not exist for councillors financed by farmers, they might be driven by mayors using their larger power over local enforcement of environmental regulation to provide their campaign donors with reductions in the environmental restrictions they are constrained by.

To conclude, our results have two main policy implications. First, we estimate sizable reduced-form environmental effects of electing candidates with clear connections to the rural sector. This suggests that reducing the vote shares of these candidates could directly affect deforestation rates, in Brazil. Second, our results raise the question of whether municipalities should have power over environmental regulation. Although municipal agencies might be effective at monitoring local hotspots, they also seem susceptible to capture by private interests.

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Appendix

In this Appendix we provide robustness checks for our main results. These are: the insignificant (positive) effects on municipal-level agriculture and deforestation of electing candidates who declared few (many) rural properties; the positive (insignificant) effects on farmed area within campaign donors' farms of electing councillors (mayors) financed by farmers; and the positive (negative) effects on deforested area within campaign donors' farms of electing mayors (councillors) financed by farmers.

For each main result, we present estimates under alternative choices of optimal bandwidth, RD polynomial, kernel, and control set. We then run placebo tests under our preferred specification, setting false cutoffs, following the logic found in Cattaneo et al. (2019a). Additionally, we present sorting tests, based on Cattaneo et al. (2019b), and balancing tests, ran in accordance with Lee (2008). Finally, in order to assess the quality of inference within the small samples from which we derive our main results, we run simulations that follow Ferman (2019).

For the purposes of clarity, we present these robustness checks in separate appendices, one for each type of candidate: farmer candidates (Appendix A); candidates financed by farmers (Appendix B). For the particular case of farm-level effects, we also present results under alternative choices of buffer radius.

Appendix A

In this Appendix we provide robustness checks for our estimates of the effects of electing farmer candidates on municipal-level agriculture and deforestation. Tables 7 and 8 present robustness checks for the effects of electing candidates who declared at least one rural property on municipal-level agriculture and deforestation, respectively. In each table, columns (1) to (4) present results under alternative choices of optimal bandwidth, RD polynomial, kernel and control set, respectively. Columns (5) and (6) present results under placebo cutoffs of -0.10 and 0.10, respectively. We can see that an absence of effects on municipal-level agriculture and deforestation for candidates who declare few rural properties is a robust result.

Table 7 – The Agricultural Effects of Electing Farmers - Robustness to RD Specification, Few Declared Rural Properties

Dependent Variable	Planted Area (km^2) (1)	Planted Area (km^2) (2)	Planted Area (km^2) (3)	Planted Area (km^2) (4)	Planted Area (km^2) (5)	Planted Area (km^2) (6)
RD Estimate	-4.61	-1.65	-2.74	-2.99	-21.36	-3.05
	(7.04)	(11.72)	(9.11)	(8.95)	(20.06)	(10.45)
Bandwidth Criterion	MSE-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal
Degree of RD Polynomial	Local Linear Regression	Local Quadratic Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression
RD Kernel	Triangular	Triangular	Epanechnikoff	Triangular	Triangular	Triangular
Election Fixed Effect	Yes	Yes	Yes	No	Yes	Yes
Placebo Cutoff	No	No	No	No	+0.10	-0.10
Dependent Variable Mean	131.164	131.084	131.377	131.706	131.398	131.445
RD Bandwidth	0.093	0.069	0.049	0.054	0.063	0.059
Effective Observations	41,792	33,676	25,279	27,377	31,076	29,635
Observations	63,576	63,576	63,576	63,576	63,576	63,576

Notes: This table reports robustness checks for the municipal-level agricultural effects of electing municipal candidates who declared at least 1 rural property. The forcing variable of each RD is the margin of votes of each candidate. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 8 – The Environmental Effects of Electing Farmers - Robustness to RD Specification, Few Declared Rural Properties

Dependent Variable	Deforested Area (km^2) (1)	Deforested Area (km^2) (2)	Deforested Area (km^2) (3)	Deforested Area (km^2) (4)	Deforested Area (km^2) (5)	Deforested Area (km^2) (6)
RD Estimate	-98.96	-7.79	-36.96	-41.98	-208.72	-88.82
	(87.02)	(129.25)	(109.59)	(110.39)	(171.05)	(111.08)
Bandwidth Criterion	MSE-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal
Degree of RD Polynomial	Local Linear Regression	Local Quadratic Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression
RD Kernel	Triangular	Triangular	Epanechnikoff	Triangular	Triangular	Triangular
Election Fixed Effect	Yes	Yes	Yes	No	Yes	Yes
Placebo Cutoff	No	No	No	No	+0.10	-0.10
Dependent Variable Mean	1,410.754	1,410.900	1,371.646	1,378.141	1,385.965	1,400.164
RD Bandwidth	0.089	0.090	0.052	0.055	0.056	0.073
Effective Observations	8,289	8,290	5,214	5,607	5,705	7,108
Observations	13,593	13,593	13,593	13,593	13,593	13,593

Notes: This table reports robustness checks for the municipal-level environmental effects of electing municipal candidates who declared at least 1 rural property. The forcing variable of each RD is the margin of votes of each candidate. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 9 presents robustness checks for the effects of electing candidates who declared at least seven rural properties on municipal-level agriculture. Table 10 presents robustness checks for the effects of electing candidates who declared at least three rural properties on municipal-level deforestation. In each table, columns (1) to (4) present results under alternative choices of optimal bandwidth, RD polynomial, kernel and control set, respectively. Columns (5) and (6) present results under placebo cutoffs of -0.10 and 0.10, respectively. We can see that positive effects on municipal-level agriculture and deforestation for candidates who declare many rural properties is a robust result.

Table 9 – The Agricultural Effects of Electing Farmers - Robustness to RD Specification, Many Declared Rural Properties

Dependent Variable	Planted Area (km^2) (1)	Planted Area (km^2) (2)	Planted Area (km^2) (3)	Planted Area (km^2) (4)	Planted Area (km^2) (5)	Planted Area (km^2) (6)
RD Estimate	118.33	201.73*	150.94*	156.45*	-263.77*	114.84
	(81.04)	(105.54)	(91.30)	(90.60)	(144.47)	(172.35)
Bandwidth Criterion	MSE-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal
Degree of RD Polynomial	Local Linear Regression	Local Quadratic Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression
RD Kernel	Triangular	Triangular	Epanechnikoff	Triangular	Triangular	Triangular
Election Fixed Effect	Yes	Yes	Yes	No	Yes	Yes
Placebo Cutoff	No	No	No	No	+0.10	-0.10
Dependent Variable Mean	229.565	228.969	212.012	209.599	211.725	229.864
RD Bandwidth	0.104	0.108	0.071	0.075	0.076	0.117
Effective Observations	518	530	414	428	431	552
Observations	833	833	833	833	833	833

Notes: This table reports robustness checks for the municipal-level agricultural effects of electing municipal candidates who declared at least 7 rural properties. The forcing variable of each RD is the margin of votes of each candidate. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 10 – The Environmental Effects of Electing Farmers - Robustness to RD Specification, Many Declared Rural Properties

Dependent Variable	Deforested Area (km^2) (1)	Deforested Area (km^2) (2)	Deforested Area (km^2) (3)	Deforested Area (km^2) (4)	Deforested Area (km^2) (5)	Deforested Area (km^2) (6)
RD Estimate	629.96**	641.42*	733.84**	695.31**	191.78	-916.11**
	(320.04)	(375.90)	(353.33)	(339.50)	(402.62)	(387.41)
Bandwidth Criterion	MSE-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal
Degree of RD Polynomial	Local Linear Regression	Local Quadratic Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression
RD Kernel	Triangular	Triangular	Epanechnikoff	Triangular	Triangular	Triangular
Election Fixed Effect	Yes	Yes	Yes	No	Yes	Yes
Placebo Cutoff	No	No	No	No	+0.10	-0.10
Dependent Variable Mean	1,545.623	1,567.110	1,610.138	1,593.254	1,551.773	1,545.575
RD Bandwidth	0.114	0.124	0.076	0.081	0.093	0.094
Effective Observations	640	662	488	501	553	556
Observations	952	952	952	952	952	952

Notes: This table reports robustness checks for the municipal-level environmental effects of electing municipal candidates who declared at least 3 rural properties. The forcing variable of each RD is the margin of votes of each candidate. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

We now present sorting and balancing tests, and assess the quality of inference, for the small regression samples in which we estimate the following effects: electing candidates who declared at least 7 rural properties, on municipal-level agriculture; electing candidates who declared at least 8 rural properties, on municipal-level agriculture; electing candidates who declared at least 3 rural properties, on municipal-level deforestation; electing candidates who declared at least 4 rural properties, on municipal-level deforestation.

Figures 8, 9, 10 and 11 present sorting tests, for each of these small regression samples. We can see that, overall, we fail to reject the null, of absence of manipulation of the forcing variable, around the RD cutoff.

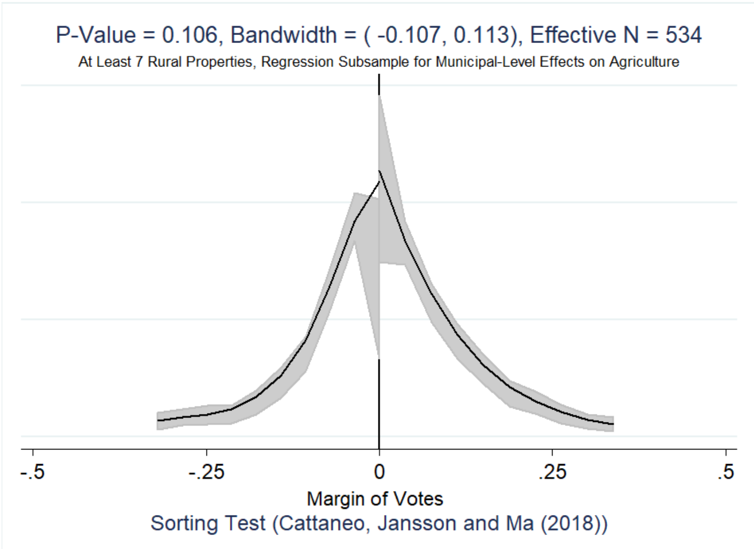

Figure 8 – Density test for the subsample of candidates who declared at least 7 rural properties, and ran in a municipality for which we have data on planted area at municipal level.

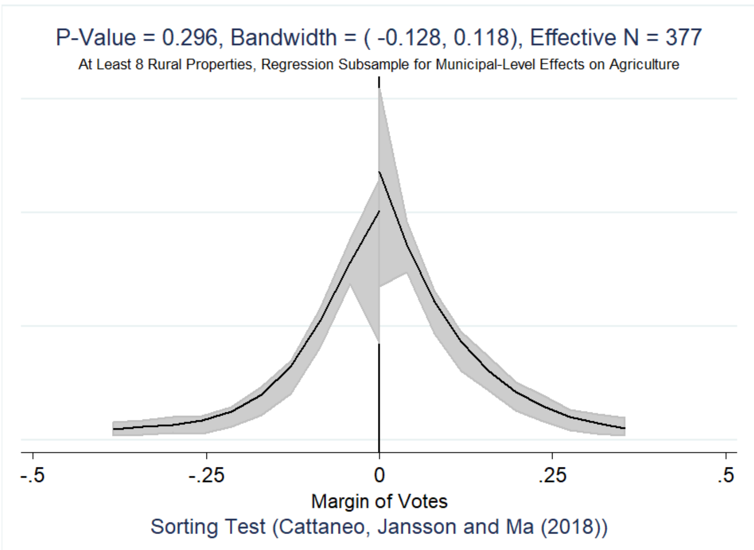

Figure 9 – Density test for the subsample of candidates who declared at least 8 rural properties, and ran in a municipality for which we have data on planted area at municipal level.

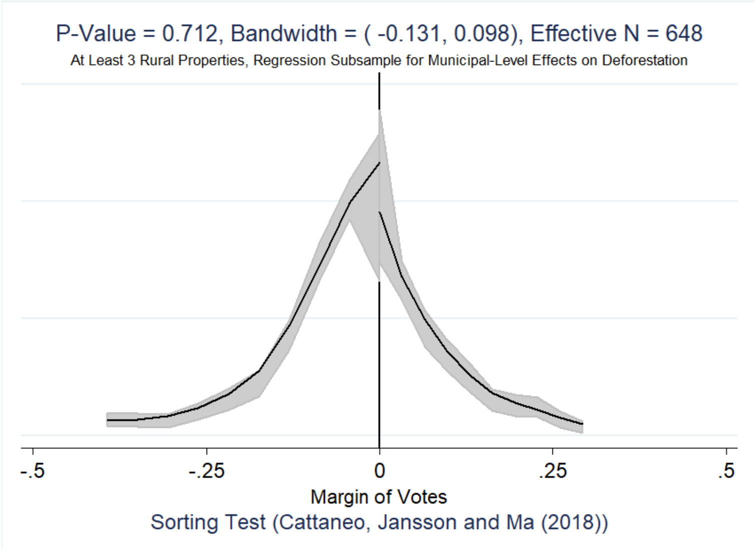

Figure 10 – Density test for the subsample of candidates who declared at least 3 rural properties, and ran in a municipality for which we have data on deforested area at municipal level.

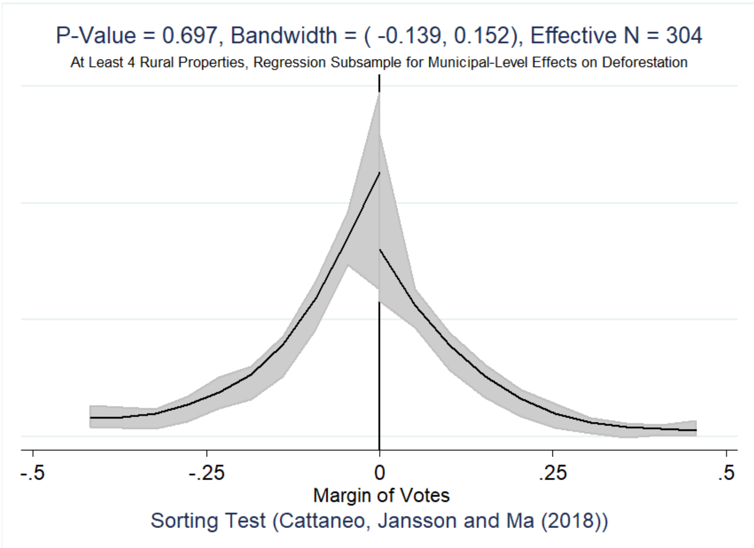

Figure 11 – Density test for the subsample of candidates who declared at least 4 rural properties, and ran in a municipality for which we have data on deforested area at municipal level.

Table 11 provides balancing tests, for each of these regression samples. We can see that, overall, we fail to reject the null, of statistically similar candidates, around the cutoff. This is particularly likely if one considers standard multiple hypotheses testing arguments. These results suggest that the estimates we are obtaining from these smaller samples are indeed capturing causal effects.

Table 11 – Balance - Farmer Candidates, Smaller Samples

	Dependent Variable Mean	RD Estimate	RD Standard Error	P-Value	CER-Optimal Bandwidth	Effective Observations
At Least 7 Rural Properties - Municipalities with Data on Agriculture						
Candidate ran for mayor	0.424	0.043	0.094	0.651	0.078	434
Number of declared rural properties	10.505	0.541	1.087	0.619	0.063	382
Candidate ran in a Legal Amazon municipality	0.109	0.104	0.054	0.057	0.076	433
Agriculture share in municipal GDP	0.237	0.022	0.036	0.527	0.081	439
Candidate completed college	0.364	-0.151	0.095	0.110	0.095	486
Male candidate	0.927	0.040	0.049	0.420	0.102	509
Married candidate	0.834	-0.026	0.063	0.681	0.104	517
At Least 8 Rural Properties - Municipalities with Data on Agriculture						
Candidate ran for mayor	0.443	0.046	0.112	0.682	0.087	309
Number of declared rural properties	12.159	0.771	1.410	0.585	0.068	271
Candidate ran in a Legal Amazon municipality	0.093	0.159	0.058	0.006	0.061	248
Agriculture share in municipal GDP	0.239	0.026	0.041	0.530	0.095	328
Candidate completed college	0.341	-0.217	0.120	0.070	0.090	311
Male candidate	0.943	0.021	0.061	0.726	0.105	349
Married candidate	0.842	-0.050	0.074	0.503	0.101	342
At Least 3 Rural Properties - Municipalities with Data on Deforestation						
Candidate ran for mayor	0.230	0	0.085	0.697	0.051	343
Number of declared rural properties	4.320	1.019	0.739	0.168	0.097	574
Agriculture share in municipal GDP	0.293	-0.004	0.031	0.900	0.088	528
Candidate completed college	0.240	-0.118	0.084	0.161	0.064	420
Male candidate	0.900	0.093	0.050	0.061	0.100	588
Married candidate	0.755	0.021	0.068	0.756	0.098	575
At Least 4 Rural Properties - Municipalities with Data on Deforestation						
Candidate ran for mayor	0.319	0.101	0.125	0.422	0.069	191
Number of declared rural properties	6.111	2.687	1.608	0.095	0.123	278
Agriculture share in municipal GDP	0.295	0.018	0.053	0.737	0.097	237
Candidate completed college	0.316	-0.060	0.149	0.686	0.083	209
Male candidate	0.889	0.177	0.092	0.055	0.112	261
Married candidate	0.784	0.060	0.108	0.577	0.090	218

Notes: This table reports the RD estimates of the effect of being elected on several baseline covariates, for each regression subsample. The forcing variable of each RD is the margin of votes of each candidate. In all regressions RD bandwidths are CER-Optimal; the degree of the RD polynomial is unitary (local linear regression); and the RD kernel is triangular. We only control for a 2008 election fixed effect. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

We end this subsection by presenting the results of simulations destined to evaluate the quality of inference, within the four small regression samples currently under evaluation. More specifically, for each regression sample, we obtain RD estimates and 95% confidence intervals of the effect on a normally distributed placebo outcome of null correlation with the running variable. We then compare rejection rates on 1,000 simulations to the actual size of the test, 5%. Table 12 presents the results of these simulations.

Table 12 – Inference Assessment - Farmer Candidates, Small Samples

	Total Observations	Empirical Rejection Rate
Effects on Municipal-Level Agriculture		
Candidates who declared at least 7 rural properties	833	5.0%
Candidates who declared at least 8 rural properties	575	6.6%
Effects on Municipal-Level Deforestation		
Candidates who declared at least 3 rural properties	952	5.2%
Candidates who declared at least 4 rural properties	444	7.0%

Notes: This table reports the results of simulations destined to evaluate the quality of inference, within four small regression samples from which we derive main municipal-level results. More specifically, we obtain RD estimates and 95% confidence intervals of the effect on a normally distributed placebo outcome of null correlation with the running variable, in each regression sample. We report sample sizes and rejection rates on 1,000 simulations.

We can see that the samples from which we obtain the estimates in Tables 9 and 10 have rejection rates that are essentially equal to 5%: 5% and 5.2%, respectively. Moreover, the samples from which we obtain the estimate associated to the largest minimum amount of declared rural properties in Figures 6 and 7 have rejection rates of 6.6% and 7.0% respectively, not far from the actual size of the test.

Appendix B

In this Appendix we provide robustness checks for our estimates of the effects of electing candidates financed by farmers on agriculture and deforestation within campaign donors' farms. We start this subsection by exploring how our farm-level results vary, under alternative choices of buffer radius. Table 13 presents these results. The estimates in columns (1) to (3) evaluate outcomes under an approximate 10 km buffer radius, with columns (2) and (3) restricted to candidates running for mayorship and councillorship, respectively. The estimates in columns (4) to (6) evaluate outcomes under an approximate 1 km buffer radius, with columns (5) and (6) restricted to candidates running for mayorship and councillorship, respectively.

Table 13 – The Agricultural Effects of Electing Candidates Financed by Farmers - Robustness to Buffer Radius Choice

Dependent Variable	Farming Area (km^2) (1)	Farming Area (km^2) (2)	Farming Area (km^2) (3)	Farming Area (km^2) (4)	Farming Area (km^2) (5)	Farming Area (km^2) (6)
RD Estimate	24.68	15.49	30.09	-0.01	-0.36	0.15
	(15.43)	(31.29)	(19.52)	(0.22)	(0.44)	(0.26)
Buffer Radius	Approximately 10km	Approximately 10km	Approximately 10km	Approximately 1km	Approximately 1km	Approximately 1km
Councillors/Mayors	Both	Only Mayors	Only Councillors	Both	Only Mayors	Only Councillors
Dependent Variable Mean	147.131	152.853	140.733	1.389	1.601	1.247
RD Bandwidth	0.096	0.096	0.078	0.123	0.111	0.103
Effective Observations	675	256	360	458	167	252
Observations	1,403	624	779	854	376	478

Notes: This table reports the effects of electing candidates financed by farmers on farmed areas within spatial buffers centered in campaign donors' farms, under alternative choices of buffer radius. The forcing variable of each RD is the margin of votes of each candidate. In all regressions RD bandwidths are CER-Optimal; the degree of the RD polynomial is unitary (local linear regression); and the RD kernel is triangular. We only control for a 2008 election fixed effect. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

We can see that our first main farm-level result, the positive (insignificant) effects on farmed area within campaign donors' farms, of electing councillors (mayors) financed by farmers, is reasonably robust to an approximate 10 km buffer radius. In fact, the p-value associated to the positive coefficient in column (3) is 12.3%. However, this result is not robust to an approximate 1 km buffer radius. As we explained in Section 3, we believe that an approximate 1 km buffer radius is not able to capture variation within the probably large formally established farms of official campaign donors. Additionally, we believe that effects are less significant under an approximate 10 km radius than under our benchmark, an approximate 5 km buffer radius, since an approximate 10 km buffer radius is probably capturing variation outside these farms, and this variation has no relation to the possible favors that rural producers might be receiving.

When evaluating deforestation within the farms of campaign donors that are located in the Amazon and Cerrado biomes, we are unable to run local linear regressions, in samples consisting only of candidates running for mayorship, under an approximate 1 km buffer radius. This leads us to evaluate the robustness of our second main farm-level result to buffer radius choice using only one alternative buffer radius size: an approximate 10 km buffer radius. Table 14 presents these results. The estimates in columns (2) and (3) are restricted to candidates running only for mayorship and councillorship, respectively.

Table 14 – The Environmental Effects of Electing Candidates Financed by Farmers - Robustness to Buffer Radius Choice

Dependent Variable	Deforested Area (km^2)	Deforested Area (km^2)	Deforested Area (km^2)
	(1)	(2)	(3)
RD Estimate	-1.25	1.54	-2.63**
	(0.78)	(1.33)	(1.08)
Buffer Radius	Approximately 10km	Approximately 10km	Approximately 10km
Councillors/Mayors	Both	Only Mayors	Only Councillors
Dependent Variable Mean	3.288	2.911	3.448
RD Bandwidth	0.121	0.113	0.100
Effective Observations	484	154	288
Observations	895	344	551

Notes: This table reports the effects of electing candidates financed by farmers on deforested areas within spatial buffers centered in campaign donors' farms, under alternative choices of buffer radius. The forcing variable of each RD is the margin of votes of each candidate. In all regressions RD bandwidths are CER-Optimal; the degree of the RD polynomial is unitary (local linear regression); and the RD kernel is triangular. We only control for a 2008 election fixed effect. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

We can see that our second main farm-level result, the positive (negative) effects on deforested area, within campaign donors' farms, of electing mayors (councillors) financed by farmers, is moderately robust to an approximate 10 km buffer radius. Although the p-value associated to the coefficient in column (3) is 1.5%, the p-value associated to column (2) is large, 24.7%. The fact that the negative effects associated to councillors actually become more significant for a larger buffer radius may indicate that councillors financed by farmers are promoting reforestation outside the farms of their campaign donors, rather than inside them. Additionally, the fact that the positive effects associated to mayors disappear once we consider a larger buffer radius may be explained by the aforementioned fact that variation outside the farms of campaign donors is completely unrelated to possible favors that these rural producers may be receiving from the politicians whose electoral campaigns they finance.

We now follow the steps we took in Appendix A, under our benchmark radius of approximately 5 km. Tables 15 and 16 present robustness checks for the effects of electing councillors financed by farmers on farmed and deforested areas within campaign donors' farms, respectively. In each table, columns (1) to (4) present results under alternative choices of optimal bandwidth, RD polynomial, kernel and control set, respectively. Columns (5) and (6) present results under placebo cutoffs of -0.10 and 0.10, respectively. We can see that positive (negative) effects on farmed (deforested) areas within campaign donors' farms, of electing councillors financed by farmers, is a robust result.

Table 15 – The Agricultural Effects of Electing Candidates Financed by Farmers - Robustness to RD Specification, Candidates Running as Councillors

Dependent Variable	Farming Area (km^2) (1)	Farming Area (km^2) (2)	Farming Area (km^2) (3)	Farming Area (km^2) (4)	Farming Area (km^2) (5)	Farming Area (km^2) (6)
RD Estimate	6.99*	8.47	7.84	9.19*	-2.74	5.10
	(4.17)	(5.88)	(4.82)	(5.03)	(8.29)	(4.90)
Bandwidth Criterion	MSE-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal
Degree of RD Polynomial	Local Linear Regression	Local Quadratic Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression
RD Kernel	Triangular	Triangular	Epanechnikoff	Triangular	Triangular	Triangular
Election Fixed Effect	Yes	Yes	Yes	No	Yes	Yes
Placebo Cutoff	No	No	No	No	+0.10	-0.10
Dependent Variable Mean	38.249	38.280	38.134	38.034	38.272	38.028
RD Bandwidth	0.132	0.124	0.089	0.088	0.123	0.076
Effective Observations	444	427	336	333	426	301
Observations	678	678	678	678	678	678

Notes: This table reports robustness checks for the effects of electing councillors financed by farmers on farmed areas within campaign donors' farms. The forcing variable of each RD is the margin of votes of each candidate. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 16 – The Environmental Effects of Electing Candidates Financed by Farmers - Robustness to RD Specification, Candidates Running as Councillors

Dependent Variable	Deforested Area (km^2) (1)	Deforested Area (km^2) (2)	Deforested Area (km^2) (3)	Deforested Area (km^2) (4)	Deforested Area (km^2) (5)	Deforested Area (km^2) (6)
RD Estimate	-0.59**	-0.75*	-0.66**	-0.66*	-0.26	-0.13
	(0.26)	(0.39)	(0.32)	(0.37)	(0.21)	(0.18)
Bandwidth Criterion	MSE-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal
Degree of RD Polynomial	Local Linear Regression	Local Quadratic Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression
RD Kernel	Triangular	Triangular	Epanechnikoff	Triangular	Triangular	Triangular
Election Fixed Effect	Yes	Yes	Yes	No	Yes	Yes
Placebo Cutoff	No	No	No	No	+0.10	-0.10
Dependent Variable Mean	0.825	0.793	0.894	0.901	0.837	0.914
RD Bandwidth	0.133	0.152	0.093	0.086	0.116	0.072
Effective Observations	293	322	215	202	265	180
Observations	478	478	478	478	478	478

Notes: This table reports robustness checks for the effects of electing councillors financed by farmers on deforested areas within campaign donors' farms. The forcing variable of each RD is the margin of votes of each candidate. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Tables 17 and 18 present robustness checks for the effects of electing mayors financed by farmers on farmed and deforested areas within campaign donors' farms, respectively. In each table, columns (1) to (4) present results under alternative choices of optimal bandwidth, RD polynomial, kernel and control set, respectively. Columns (5) and (6) present results under placebo cutoffs of -0.10 and 0.10, respectively. We can see that positive (insignificant) effects on deforested (farmed) areas within campaign donors' farms, of electing mayors financed by farmers, is a robust result.

Table 17 – The Agricultural Effects of Electing Candidates Financed by Farmers - Robustness to RD Specification, Candidates Running as Mayors

Dependent Variable	Farming Area (km^2) (1)	Farming Area (km^2) (2)	Farming Area (km^2) (3)	Farming Area (km^2) (4)	Farming Area (km^2) (5)	Farming Area (km^2) (6)
RD Estimate	1.28	-0.81	-0.18	0.11	9.78	-12.86
	(6.91)	(9.01)	(7.90)	(7.66)	(7.40)	(11.60)
Bandwidth Criterion	MSE-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal
Degree of RD Polynomial	Local Linear Regression	Local Quadratic Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression
RD Kernel	Triangular	Triangular	Epanechnikoff	Triangular	Triangular	Triangular
Election Fixed Effect	Yes	Yes	Yes	No	Yes	Yes
Placebo Cutoff	No	No	No	No	+0.10	-0.10
Dependent Variable Mean	41.819	42.021	40.204	40.306	40.085	41.097
RD Bandwidth	0.132	0.160	0.088	0.099	0.096	0.104
Effective Observations	277	333	211	225	220	233
Observations	553	553	553	553	553	553

Notes: This table reports robustness checks for the effects of electing mayors financed by farmers on farmed areas within campaign donors' farms. The forcing variable of each RD is the margin of votes of each candidate. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 18 – The Environmental Effects of Electing Candidates Financed by Farmers - Robustness to RD Specification, Candidates Running as Mayors

Dependent Variable	Deforested Area (km^2) (1)	Deforested Area (km^2) (2)	Deforested Area (km^2) (3)	Deforested Area (km^2) (4)	Deforested Area (km^2) (5)	Deforested Area (km^2) (6)
RD Estimate	0.32	0.62*	0.43*	0.41*	0.26	0.33
	(0.21)	(0.32)	(0.25)	(0.24)	(0.41)	(0.29)
Bandwidth Criterion	MSE-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal	CER-Optimal
Degree of RD Polynomial	Local Linear Regression	Local Quadratic Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression	Local Linear Regression
RD Kernel	Triangular	Triangular	Epanechnikoff	Triangular	Triangular	Triangular
Election Fixed Effect	Yes	Yes	Yes	No	Yes	Yes
Placebo Cutoff	No	No	No	No	+0.10	-0.10
Dependent Variable Mean	0.485	0.514	0.479	0.472	0.480	0.489
RD Bandwidth	0.137	0.145	0.092	0.108	0.118	0.090
Effective Observations	131	142	105	114	119	102
Observations	265	265	265	265	265	265

Notes: This table reports robustness checks for the effects of electing mayors financed by farmers on deforested areas within campaign donors' farms. The forcing variable of each RD is the margin of votes of each candidate. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

We now present sorting and balancing tests, and assess the quality of inference, for the small regression samples in which we estimate the following effects: electing mayors financed by farmers, on farmed area within campaign donors' farms, under an approximate 5 km buffer radius; electing councillors financed by farmers, on farmed area within campaign donors' farms, under an approximate 5 km buffer radius; electing mayors financed by farmers, on deforested area within campaign donors' farms, under an approximate 5 km buffer radius; electing councillors financed by farmers, on deforested area within campaign donors' farms, under an approximate 5 km buffer radius.

Figures 12, 13, 14 and 15 present sorting tests, for each of these regression samples. We can see that, overall, we fail to reject the null, of absence of manipulation of the forcing variable, around the RD cutoff.

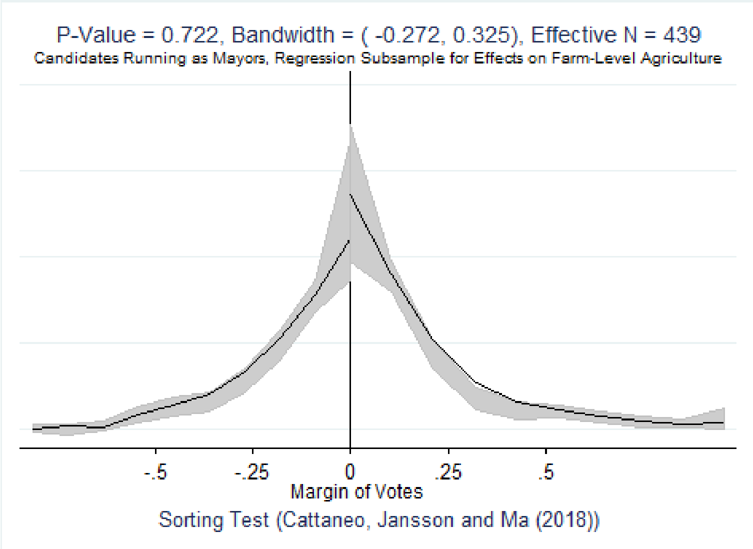

Figure 12 – Density test for the subsample of candidates running as mayors who are associated to at least one farm for which we have data on farmed area within a buffer centered on it, under a buffer radius of approximately 5 km.

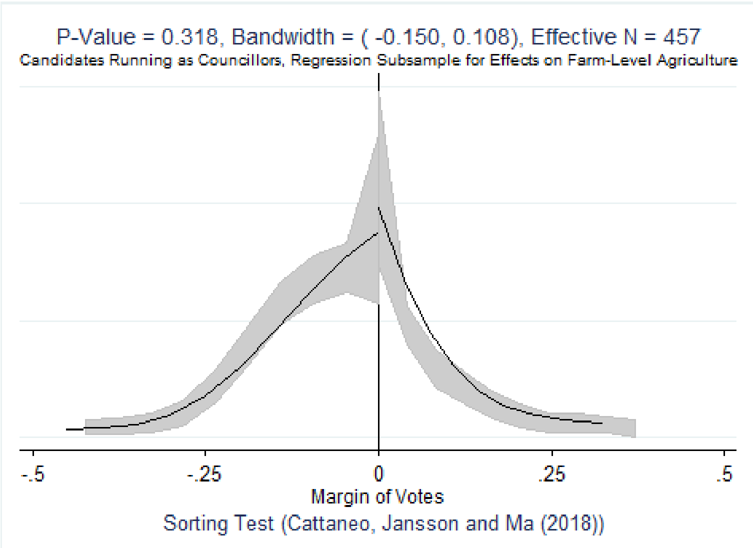

Figure 13 – Density test for the subsample of candidates running as councillors who are associated to at least one farm for which we have data on farmed area within a buffer centered on it, under a buffer radius of approximately 5 km.

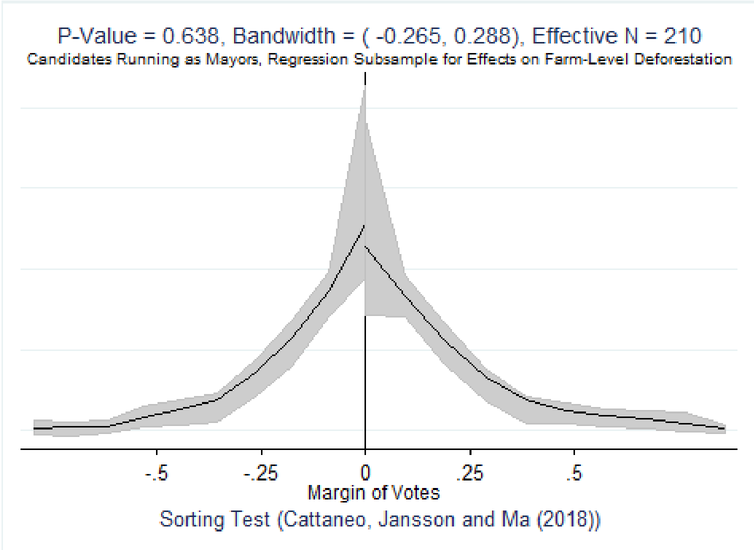

Figure 14 – Density test for the subsample of candidates running as mayors who are associated to at least one farm for which we have data on deforested area within a buffer centered on it, under a buffer radius of approximately 5 km.

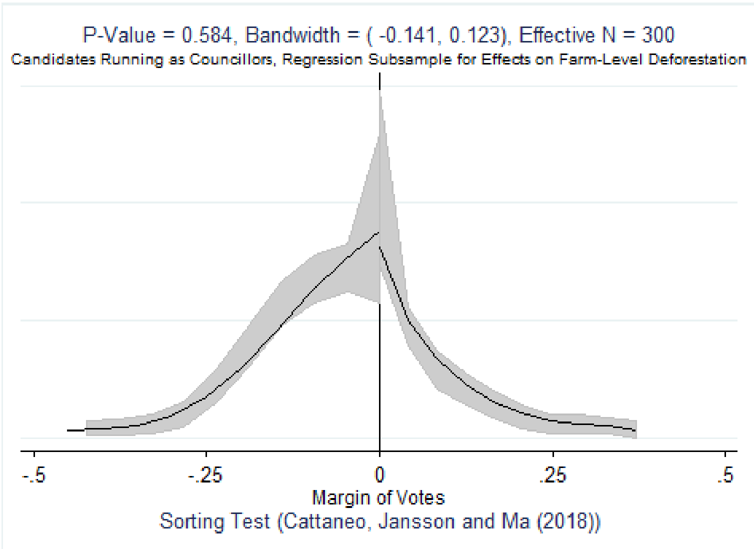

Figure 15 – Density test for the subsample of candidates running as councillors who are associated to at least one farm for which we have data on deforested area within a buffer centered on it, under a buffer radius of approximately 5 km.

Table 19 provides balancing tests, for each of these regression samples. We can see that, overall, we fail to reject the null, of statistically similar candidates, around the cutoff. This is particularly likely if one considers standard multiple hypotheses testing arguments. These results suggest that the estimates we are obtaining from these smaller samples are indeed capturing causal effects.

Table 19 – Balance - Candidates Connected to Farming Interests, Smaller Samples

	Dependent Variable Mean	RD Estimate	RD Standard Error	P-Value	CER-Optimal Bandwidth	Effective Observations
Candidates Running as Mayors - Farms with Data on Agriculture						
Candidate ran in the state where all donor farms are located	0.742	0.029	0.125	0.816	0.112	246
Number of farmers financing the candidate	1.215	0.121	0.232	0.601	0.101	228
Total donations received from farmers (R$)	23,175.570	4,441.010	15,689.710	0.777	0.106	235
Candidate ran in a Legal Amazon municipality	0.088	-0.114	0.102	0.260	0.101	228
Agriculture share in municipal GDP	0.201	0.022	0.039	0.573	0.094	219
Candidate completed college	0.586	0.091	0.142	0.521	0.082	198
Male candidate	0.915	-0.046	0.091	0.610	0.104	234
Married candidate	0.793	0.097	0.092	0.296	0.107	237
Candidates Running as Councillors - Farms with Data on Agriculture						
Candidate ran in the state where all donor farms are located	0.756	-0.088	0.102	0.388	0.082	313
Number of farmers financing the candidate	1.292	0.217	0.591	0.714	0.098	366
Total donations received from farmers (R$)	5,237.538	2,189.609	2,159.472	0.311	0.096	355
Candidate ran in a Legal Amazon municipality	0.080	-0.054	0.050	0.280	0.117	414
Agriculture share in municipal GDP	0.099	0.009	0.026	0.723	0.069	284
Candidate completed college	0.358	-0.010	0.096	0.917	0.096	355
Male candidate	0.862	-0.001	0.064	0.988	0.086	327
Married candidate	0.679	-0.064	0.108	0.554	0.096	352
Candidates Running as Mayors - Farms with Data on Deforestation						
Candidate ran in the state where all donor farms are located	0.587	-0.030	0.197	0.880	0.108	114
Number of farmers financing the candidate	1.229	0.048	0.332	0.885	0.126	122
Total donations received from farmers (R$)	25,313.920	-15,341.610	24,550.360	0.532	0.115	118
Candidate ran in a Legal Amazon municipality	0.143	-0.271	0.106	0.010	0.092	105
Agriculture share in municipal GDP	0.217	0.073	0.050	0.148	0.100	109
Candidate completed college	0.613	0.113	0.194	0.560	0.103	111
Male candidate	0.886	-0.181	0.142	0.202	0.108	114
Married candidate	0.848	0.017	0.127	0.890	0.107	112
Candidates Running as Councillors - Farms with Data on Deforestation						
Candidate ran in the state where all donor farms are located	0.700	-0.053	0.131	0.687	0.092	214
Number of farmers financing the candidate	1.064	0.261	0.228	0.251	0.117	267
Total donations received from farmers (R$)	4,365.325	3,085.552	2,368.062	0.193	0.101	240
Candidate ran in a Legal Amazon municipality	0.070	-0.009	0.049	0.858	0.120	272
Agriculture share in municipal GDP	0.113	-0.003	0.035	0.941	0.087	204
Candidate completed college	0.301	0.088	0.102	0.389	0.144	306
Male candidate	0.877	-0.015	0.048	0.758	0.079	187
Married candidate	0.675	0.036	0.120	0.765	0.107	249

Notes: This table reports the RD estimates of the effect of being elected on several baseline covariates, for each regression subsample. Farm-level outcomes are evaluated under an approximate 5 km buffer radius. The forcing variable of each RD is the margin of votes of each candidate. In all regressions RD bandwidths are CER-Optimal; the degree of the RD polynomial is unitary (local linear regression); and the RD kernel is triangular. We only control for a 2008 election fixed effect. The reported statistics describe the subsample relevant to each regression. Robust standard errors are reported in parentheses.

***Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

We end this subsection by presenting the results of simulations destined to evaluate the quality of inference, within the four small regression samples currently under evaluation. More specifically, for each regression sample, we obtain RD estimates and 95% confidence intervals of the effect on a normally distributed placebo outcome of null correlation with the running variable. We then compare rejection rates on 1,000 simulations to the actual size of the test, 5%. Table 20 presents the results of these simulations.

Table 20 – Inference Assessment - Candidates Financed by Farmers, Small Samples

	Total Observations	Empirical Rejection Rate
Effects on Farm-Level Agriculture (5 km Buffer Radius)		
Candidates who ran as mayors	553	6.1%
Candidates who ran as councillors	678	4.5%
Effects on Farm-Level Deforestation (5 km Buffer Radius)		
Candidates who ran as mayors	265	7.2%
Candidates who ran as councillors	478	6.7%

Notes: This table reports the results of simulations destined to evaluate the quality of inference, within four small regression samples from which we derive main farm-level results. More specifically, we obtain RD estimates and 95% confidence intervals of the effect on a normally distributed placebo outcome of null correlation with the running variable, in each regression sample. We report sample sizes and rejection rates on 1,000 simulations.

We can see that the samples from which we obtain the estimates in columns (2), (3), (5) and (6) of Table 6 have rejection rates of 6.1%, 4.5%, 7.2% and 6.7% respectively, not far from the actual size of the test.