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Abstract

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Nikolaj Broberg,† Vincent Pons,‡ and Clemence Tricaud§

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Abstract

This paper investigates the effects of campaign finance rules on electoral outcomes. In French departmental and municipal elections, candidates competing in districts above 9,000 inhabitants face spending limits and are eligible for public reimbursement if they obtain more than five percent of the votes. Using an RDD around the population threshold, we find that these rules increase competitiveness and benefit the runner-up of the previous race as well as new candidates, in *departmental* elections, while leaving the polarization and representativeness of the results unaffected. Incumbents are less likely to get reelected because they are less likely to run and obtain a lower vote share, conditional on running. These results appear to be driven by the reimbursement of campaign expenditures, not spending limits. We do not find such effects in *municipal* elections, which we attribute to the use of a proportional list system instead of plurality voting.

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1 Introduction

Policies regulating the influence of money in politics often generate heated debates. Advocates of limited regulation see campaign contributions as a form of political expression and campaign expenditures as an opportunity for candidates to inform voters about their platform. Differences in money raised and spent across competitors may not only be acceptable but even desirable if they help signal their relative quality to the public (e.g., Bailey, 2004). In contrast, supporters of stronger regulation argue that the unregulated use of campaign money can lead to a wasteful arms' race and facilitate the capture of the democratic process by wealthy individuals and interest groups (e.g., Bailey, 2004; Grossman and Helpman, 1992, 2001). They highlight the importance of levelling the playing field for outsider candidates who may not be able to access the same resources as incumbents even if they are of high quality (e.g., Stratmann, 2005).

Despite its importance, much of this debate is framed around principles and anecdotes rather than sound empirical evidence (e.g., Scarrow, 2007). Indeed, while most countries with political pluralism have adopted some form of campaign finance regulation (OECD, 2016), these rules are generally rolled out at the same time throughout the entire territory, rendering their evaluation difficult. A handful of recent papers exploit local variation to estimate the impact of limits to individual campaign contributions and to total campaign expenditures (Avis et al., 2022; Fouirnaies, 2021; Gulzar et al., 2021). However, we lack empirical evidence on rules which go one step further and provide for the reimbursement of campaign expenditures by the state. While such rules have a clear cost, they might further increase the equality of resources across candidates and could therefore be even more impactful than spending limits.

In this paper, we take advantage of reforms implemented in France in the early 1990s to fill this gap and estimate the effects of campaign finance rules on candidate selection and electoral outcomes. Since 1995, all candidates competing in departmental and municipal elections of districts with a population *above 9,000 inhabitants* are subject to a spending ceiling and they are eligible for the reimbursement of their expenditures up to 50 percent of the ceiling if they obtain more than five percent of the votes. Beyond France, rules combining spending limits and reimbursement exist in other countries including Ireland, South Korea, Portugal, Canada, Italy, and the U.S. Importantly for our empirical strategy, in France, campaign expenditures of candidates running in districts *below the 9,000 inhabitants threshold* are neither capped nor reimbursed. We use a Regression Discontinuity Design (RDD) to compare districts located just above the population discontinuity and just below. Differences in electoral results can be attributed to the difference in campaign finance rules since no other regulation changes at this threshold.

The impact of the legislation varies greatly across elections. While we observe strong effects in departmental elections, which use two-round plurality voting in single-member constituencies,

we do not find any significant effect in municipal elections, which use a two-round list system with proportional representation.

In *departmental elections*, spending limits and the reimbursement of campaign expenditures do not affect the total number of candidates but they make elections more competitive: the odds that any candidate obtains a majority of votes and wins the election in the first round decrease by 10.9 percentage points. Most importantly, incumbents experience a sharp decline in their reelection rate at the benefit of the runner-up in the previous election and of candidates who were not present in that election. The reimbursement and capping of campaign expenditures causes a reduction in the incumbent's reelection probability by 14.5 percentage points, an increase in the previous runner-up's chances of winning by 5.2 percentage points, and an increase in the likelihood of a victory by a candidate absent from the previous election by 9.2 percentage points.

While the effect on victory by candidates absent from the previous election does not result from an increased number of new entrants, the effects on the likelihood of a victory by the incumbent or the previous runner-up can be decomposed into two parts. First, the treatment reduces the probability that the incumbent runs for reelection by 7.4 percentage points and it increases the likelihood that their challenger in the previous election runs again by 8.4 percentage points. Second, we derive bounds to estimate effects on candidates' chances of winning *conditional on running*. Similarly as the effects on running, conditional effects on winning are negative for the incumbent (between -10.5 and -18.9 percentage points) but positive for the runner-up (between 11.0 and 19.8 percentage points).

In theory, both the reimbursement of candidates' expenditures and spending limits could contribute to levelling the playing field and increase the likelihood of electoral turnovers. We exploit the 1992 and 1994 departmental elections to disentangle the influence of these two dimensions. Unlike the elections in our main sample, these elections were held after the spending ceiling was introduced (above the population threshold), in 1990, but before campaign expenditures started to be reimbursed, in 1995. We do not find any effect in this secondary sample of elections, suggesting that our main effects are driven by the reimbursement of candidates more than expenditure ceilings. Data on candidates' contributions and expenditures above the threshold bring further support for this interpretation. After the 1995 reform, we observe a disproportional increase in the personal contributions and the spending to ceiling ratio for the competitors of the incumbent, who also benefit electorally from the reform. By contrast, spending limits are binding for only a few candidates and they do not become more binding over time: bunching at the ceiling is modest, both before and after 1995.

Finally, the public reimbursement of candidates does not affect the polarization of elections or the representativeness of the winner's orientation, but it increases the probability of a change in the winning orientation and the probability that a candidate from the left is elected. This effect is

consistent with the fact that left-wing candidates stand to gain the most from the reimbursement of campaign expenditures since they receive less than half the amount of private donations received by right-wing candidates and contribute less of their own money to their campaign beforehand. After the reform, their expenditure to ceiling ratio increases dramatically relative to candidates on the right.

In contrast to departmental elections, in *municipal elections*, we do not find any significant difference between municipalities immediately to the left and to the right of the campaign finance threshold. To understand this result, we note that mayoral candidates can ask other members of their list to contribute time and money to the campaign, so receiving public funding may make less of a difference for them and have less equalizing power than for candidates in departmental elections. In addition, we provide suggestive evidence that the negative impact of campaign finance rules on incumbents' likelihood to run for reelection, in departmental elections, results in part from political parties asking some incumbents to drop out. Incumbent mayoral candidates may be better able to withstand such pressure because they can invite possible rivals to join their list and they know that they will most likely obtain a seat on the municipal council themselves even if they fail to be reelected as mayor. In other words, the different results we obtain in departmental and municipal elections likely reflect important differences between the single-member constituencies characterizing the former and the list format used in the latter.

1.1 Contribution to the literature

We first build on a large theoretical literature studying the relationship between money and politics (see Stratmann, 2005 for a review). Two distinct tradeoffs investigated by theoretical models are directly relevant for campaign finance regulation. First, differences in the amount of money spent by candidates can signal differences in quality, if higher-quality candidates are able to raise more money (Coate, 2004a; Ortin et al., 2000; Prat, 2002), but they may also reflect differences in access to donors that are orthogonal to quality. Spending limits may benefit high quality challengers, if incumbents have easier access to campaign money irrespective of their quality (Iaryczower and Mattozzi, 2012; Pastine and Pastine, 2012), or increase incumbency advantage, if incumbents have non-pecuniary resources which challengers can only hope to overcome by outspending them (Sahuguet and Persico, 2006). The reimbursement of campaign expenditures exacerbates this tension. It decreases imbalances in candidates' access to money but decreases high quality candidates' ability to signal their quality by spending more (Ashworth, 2006; Prat et al., 2010).[1] Our results

[1]While we focus on the public funding of individual candidates, a separate literature studies the public funding of national parties, based for instance on their past vote shares. Katz and Mair (1994)'s theory on the cartelization of politics argues that systems of party financing are designed by elected party legislators to prevent the entry of new parties. Interestingly, by facilitating the entry of new candidates, the public funding of individual candidates may work

indicate that, on net, campaign finance regulations do level the playing field and decrease the incumbency advantage.

A second tradeoff relates to the representativeness of elected officials and their policies. On one hand, campaign money funds outreach efforts which educate voters about candidates' policy positions, contributing to the democratic ideal of an informed electorate and increasing the likelihood that the winner's policies are aligned with the preferences of the majority (e.g., Austen-Smith, 1987; Hinich et al., 1989; Prat, 2002). On the other hand, private donors may seek to extract favors in exchange for their contributions, which could instead create a wedge between enacted policies and public interest (e.g., Baron, 1994; Coate, 2004b; Grossman and Helpman, 1996). Limits on individual contributions and on total candidate spending can alleviate the risk of such capture but they also reduce the intensity of campaign communication. While the reimbursement of campaign expenditures by the state generates an obvious burden for the public budget, it can in principle help mitigate this tradeoff (Coate, 2004a). Indeed, we do not find any negative effect on winners' representativeness.

Empirically, we contribute to a burgeoning literature using quasi-experimental evidence to estimate the effects of campaign finance rules. Avis et al. (2022) and Fouirnaies (2021) find that limits on overall spending tend to increase competitiveness and reduce incumbency advantage, and Gulzar et al. (2021) show that looser individual contribution limits increase the number of public contracts assigned to donors of the elected candidate. Existing evidence about the effects of campaign expenditures's reimbursement is much less solid. Malhotra (2008) and Masket and Miller (2015) exploit the fact that some states in the U.S. offer the possibility for candidates to receive public funding in exchange for respecting pre-set spending limits. They find that districts where candidates accept the state's money experience higher competitiveness and lower incumbency advantage as well as more moderate policy outcomes, as a result of not relying on ideological donors. However, candidates who choose public funding may differ from those funded privately on other dimensions, which may bias the comparison between them. Our RDD is insulated from such endogeneity issues. It draws on other studies using RDDs around population thresholds to estimate the impact of other electoral rules and policies (e.g., Corbi et al., 2019; Gadenne, 2017; Eggers et al., 2018; Bordignon et al., 2016).

Beyond studies on campaign finance regulation, our paper also contributes to the broader literature measuring the impact of campaign money on vote shares. Indeed, the differences in campaign finance rules above and below the 9,000 inhabitants threshold generate exogenous variation in the amount of money spent by different types of candidates. Campaign spending limits and reimbursement may advantage challengers if they increase their spending relative to incumbents and if any additional money they spend translates into a larger increase in vote shares. In the U.S., ef-

against the cartelization of politics (see e.g., Dinas and Foos, 2017; Katz and Mair, 2009).

fects of campaign expenditures on vote shares have been found to be modest overall, but larger for challengers than incumbents (Abramowitz, 1988; Jacobson, 1978; Palda and Palda, 1998).[2] However, these results may not apply to our setting since the amount of money spent in French local elections is lower than in the U.S., and campaign money may have decreasing marginal returns. Furthermore, public money (specifically, expenditures that will be reimbursed by the state) may have different effects than private money, which can signal quality but also foreshadow policy bias towards donors' requests. We do find that challengers benefit from the rules prevalent above the threshold, but the effects on the identity of the winner are only present in departmental elections, where campaign expenditures are lower on average. This result is consistent with the possibility that effects of relative spending decrease with the total amount of money spent.[3]

While most of the literature focuses on the distinction between challengers and incumbents, differences across orientations may be even more important. Because left-wing candidates tend to rely less on private donations (Bekkouche et al., 2022), they stand to benefit more from public funding than candidates on the right. Our results confirm this prediction. We cannot measure downstream effects on policymaking but expect them to be important, given evidence that elected officials on the left and on the right implement different policies (Pettersson-Lidbom, 2008, but see Ferreira and Gyourko, 2009) and that electoral turnovers impact performance (Akhtari et al., 2022; Marx et al., 2022).

The remainder of the paper is structured as follows. Section 2 introduces our research setting, and Section 3 describes our empirical strategy. Sections 4 and 5 provide the main results for departmental elections and municipal elections, respectively. Section 6 discusses the mechanisms at play, and Section 7 concludes.

2 Research setting

2.1 Campaign finance rules in France

Many Western democracies started regulating campaign finance in the 1960s (Alexander and Federman, 1989), hoping to limit the influence of money in politics and to increase the transparency and fairness of the election process (see e.g., Gunlicks, 2019 and The Law Library of Congress, 2009). France did not regulate campaign finance until the late 1980s, prompted by rising amounts of campaign money and numerous scandals uncovering the widespread illegal funding of parties.

[2]For recent papers measuring the effect of campaign spending on vote shares outside the U.S., see for instance Ben-Bassat et al. (2015), François et al. (2016), and Bekkouche et al. (2022).

[3]Section 6.1 discusses the difference between the effects found in municipal and departmental elections at greater length. Regardless of the exact interpretation, these results complement the vast literature studying the impact of differences across voting systems (Bordignon et al., 2016; Eggers, 2015; Myerson and Weber, 1993).

A series of reforms regulating campaign spending, campaign contributions, and other aspects of political campaigns were adopted from 1988 to 1995. France now has a stable and relatively strict system of campaign finance legislation.

For the sake of brevity and clarity, we focus on the aspects of the French reforms that are relevant to our analysis. Democracies can level the playing field by limiting campaign expenditures or by providing for their reimbursement by the state. France, similarly as other countries including Ireland, South Korea, Portugal, Canada, Italy, and, to some extent, the U.S., does both. In the U.S., presidential election candidates and candidates for state offices in fourteen states face an opt-in system. To receive public funding, they need to respect a spending cap; those who go over this cap become ineligible for public funding.[4] The policy in France and in the other aforementioned countries is more binding. In elections where public reimbursement of expenditures and spending limits apply, complying with them is not at candidates' discretion.

The first reform we exploit is a 1990 law, which introduced spending limits in departmental and municipal districts above 9,000 inhabitants. These limits depend on district size. Candidates must respect these limits, lest they become liable to serious sanctions, up to ten years of prison. Furthermore, all candidates running in districts above the population threshold must provide a detailed account of their expenditures and revenues to a dedicated commission, the CNCCFP.[5] Accordingly, we have comprehensive data on candidate spending above the threshold.

The second reform is a 1995 law which introduced the reimbursement of candidates' expenditures in the same set of districts, with population above 9,000 inhabitants.[6] Candidates running in these districts are eligible for the reimbursement of 50 percent of the spending limit,[7] provided they obtain more than five percent of the candidate votes (valid votes cast for a candidate as opposed to blank and null votes) in the first round. Candidates can only ask for the reimbursement of expenditures covered with their own money: expenditures covered by contributions from donors, political parties, etc. are not reimbursed. The 1995 reform also tightened the spending limits first introduced in 1990 to 70 percent of the previous level.

Districts below the population cutoff were not affected by the 1990 and 1995 reforms, such that candidates running in these districts face no spending limit and they are not eligible for reimbursement. We generally measure the combined impact of reimbursement and spending limits,

[4] See https://www.fec.gov/introduction-campaign-finance/understanding-ways-support-federal-candidates/presidential-elections/public-funding-presidential-elections/ and https://www.ncsl.org/research/elections-and-campaigns/public-financing-of-campaigns-overview.aspx.

[5] This rule was modified in 2011 such that only candidates obtaining more than one percent of the votes have to submit this information.

[6] Before 1990, candidates had been reimbursed for official propaganda related costs, e.g., the printing of ballots, posters put up in front of polling stations, and manifestos sent to voters, all accounting for a very small share of campaign expenditures. After 1990, candidates remained eligible for the reimbursement of these specific expenditures provided they obtained more than five percent of the votes, both above and below the population threshold.

[7] The maximum reimbursement was reduced to 47.5 percent in 2011.

since both vary at the 9,000 inhabitants threshold. We also separately study the 1992 and 1994 departmental elections, where candidates running above the threshold were only subject to the 1990 law, to isolate the effect of spending limits.

The French reforms which started in the late 1980s also changed rules affecting other aspects of elections, including TV and radio advertising (which were prohibited) and contribution limits (Cagé et al., 2021). However, these changes affected cantons and municipalities both above and below the 9,000 inhabitants threshold. Therefore, they do not contribute to the effects we measure at the discontinuity.

2.2 French departmental and municipal elections

Our sample includes two types of elections, characterized by different voting rules.

Departmental elections elect members of departmental councils, which exert responsibility over culture, local development, social assistance, education, housing, transportation, and tourism, and account for 7 percent of total public spending. France counts 101 départements divided in single-member constituencies, called cantons. Departmental elections follow a two-round plurality voting rule. In each canton, the top candidate wins the race in the first round if she receives more than 50 percent of the candidate votes, accounting for at least 25 percent of the registered citizens. If no majority is obtained in the first round, the top-two candidates and all other candidates above a certain vote share threshold qualify for the second round. The qualification threshold was 10 percent of registered citizens until 2011, and 12.5 percent afterwards. The second round takes place a week later and uses plurality voting: the candidate receiving the most votes is elected. There is no term limit. Until a 2013 reform, each canton elected one representative for a length of six years, and half of the seats were up for election every three years. There were a total of 4,035 cantons, with populations ranging from 270 to 69,335 inhabitants. The reform of 2013 aligned the calendar of all elections, it homogenized cantons' size within departments, cut the number of cantons in half, and led to the redistricting of all cantons' boundaries. Post reform, the population of 98 percent of the cantons was above the 9,000 inhabitants threshold. Therefore, we do not use departmental elections which took place after the reform.[8]

Municipal elections are held every six years and elect the mayor and other members of the municipal council in each of the 35,000 French municipalities, with populations ranging from a handful of inhabitants to 450,000. Around the 9,000 inhabitants threshold, municipal councils count 27 members (including the mayor), so competing lists include 27 candidates. Like in departmental elections, there is no term limit. Municipal councils have discretion over local urban services,

[8]The 2013 reform also changed the election format: instead of electing a single representative, each canton elects a ticket composed of a woman and man. Dealing with this additional change would further complicate the analysis, which is conducted at the individual candidate level for all other departmental elections.

municipal police, nurseries, primary schools, sports facilities, road maintenance, and urban public transportation. Their expenditures account for 11 percent of total public spending. We restrict our analysis to the sample of municipalities with more than 3,500 inhabitants because electoral rules differed significantly below this threshold until the 2014 elections. Despite a few municipality mergers, this represents a fairly stable sample of 2,500 to 3,000 municipalities per election year. In these municipalities, elections follow a two-round list system with proportional representation. If a list obtains the absolute majority in the first round, half of the seats are attributed to this list and the other seats are divided proportionally between all the lists which received more than five percent of the votes. If no majority is reached in the first round, the top-two lists and all lists above 10 percent qualify for the second round taking place a week later.[9] Lists with more than five percent of the votes in the first round can merge with lists qualified for the second round.[10]

Since municipal and departmental elections have different voting rules, we study them separately throughout the analysis. These two types of elections also have different electoral calendars (except for 2001 and 2008, when both types of elections coincided) and their districts do not overlap: multiple small municipalities are often included in the same canton and, conversely, large municipalities are generally split into multiple cantons. We find different effects of campaign finance rules in departmental and municipal elections, as shown in Sections 4 and 5, and interpret these differences in Section 6.

3 Empirical strategy

3.1 Evaluation framework

Measuring the impact of campaign finance rules is typically difficult as such rules are usually applied uniformly within countries and differences across countries or election types overlap with many other differences. We circumvent this difficulty by exploiting local variation in campaign finance rules in French departmental and municipal elections generated by the 1990 and 1995 reforms. In districts below 9,000 inhabitants, candidates are not reimbursed and they face no spending limits, while candidates running in districts above 9,000 inhabitants must respect spending limits and they are reimbursed provided they obtain more than five percent of the candidate votes in the first round.

Formally, we estimate the impact of these rules with a sharp regression discontinuity design. We use the following specification:

[9]The spending limit is looser for lists qualified for the second round than those eliminated after the first round.

[10]This can lead to potential changes in the lists' composition, including the first candidate on each list, as well as changes in the lists' political orientation.

$$Y_{i,t} = \alpha + \tau D_{i,t} + \beta X_{i,t} + \gamma X_{i,t} D_{i,t} + \varepsilon_{i,t}, \quad (1)$$

where $Y_{i,t}$ is the outcome in district i and election year t, $X_{i,t}$ is the running variable, defined as the district population centered around 9,000 inhabitants, and $D_{i,t}$ is the assignment variable, a dummy taking value one if $X_{i,t}$ is positive.

Following Imbens and Lemieux (2008) and Calonico et al. (2014), we use a non-parametric estimation, which equates to fitting two linear regressions within a certain bandwidth on either side of the threshold.[11] We follow the optimal MSERD algorithm proposed by Calonico et al. (2019) to construct optimal data-driven bandwidths for each outcome. Applying Calonico et al. (2014)'s estimation procedure, we obtain robust confidence interval estimators.

We cluster our standard errors $\varepsilon_{i,t}$ at the district level. This allows for the assignment to treatment to be correlated at the district level over time, which is particularly important for the 2008 elections. Indeed, in the majority of districts, population and therefore assignment to treatment remained identical between the 2001 and 2008 elections, since the population was based on the same census for both elections. We discuss the identification assumption required to interpret our estimates causally in Section 3.3.

3.2 Data

Electoral results for all municipalities above 3,500 inhabitants and all cantons come from the Ministry of the Interior. For the 2001 municipal elections, these data aggregate results across candidates of the same political orientation. We obtained candidate-level data from Cagé (2020) and Bach (2012), and completed them by consulting and manually inputting results published in local newspapers present in French archives.

In each district, we pair election results across years to identify which candidates were present in the previous election (which we call "insider" candidates) and which ones were absent ("outsider" candidates).[12] Among insiders, we check whether the incumbent and the runner-up from the previous election (the "challenger") run again.

We exploit political labels attributed by the Ministry of the Interior and information obtained from the research center CEVIPOF to identify "non-party candidates," namely candidates who do not have any party labels. Within this group, we call candidates who cannot be placed on the

[11] In Appendix Table C11, we also show the robustness of our main results to employing a quadratic specification by adding $X_{i,t}^2$ and its interaction with $D_{i,t}$ in equation 1.

[12] The pairing between the 1995 and 2001 municipal elections also required inputting results from local newspapers for all the 1995 municipal elections.

left-right axis "non-classified." We classified candidates into five orientations, far-left, left, centre, right, and far-right, and place them on ParlGov's 0 to 10 left-right scale (Döring and Manow, 2012).

Importantly, our identification strategy requires to know the exact official population of each district at each election, in order to compute the running and assignment variables $X_{i,t}$ and $D_{i,t}$ accurately. Obtaining reliable population data proved more difficult than anticipated. It required combining and carefully cross-checking many data sources. Changes in the official population can occur following national censuses or out-of-census complementary decrees affecting small subsets of municipalities. Until 1999, national censuses took place every six to nine years. Complementary decrees could occur between censuses, when the population of a municipality had increased by at least 15 percent or following major redistrictings of cantons or municipalities (border changes, mergers, and demergers). Since 2008, yearly national censuses have been published based on the enumeration of one fifth of the territory each year. Our population data come from INSEE (the National Institute of Statistics and Economic Studies) for the national censuses; and from Légifrance (the official website used by the French government to publish new legislation, regulations, and legal information) as well as SIRIUS (IT Service of Interdisciplinary Urban and Spatial Research) for the complementary decrees.

Finally, we digitized booklets from the commission monitoring party and candidate expenditures (CNCCFP), reporting the expenditures and breakdown of contributions received by candidates running in all districts above 9,000 inhabitants.[13] These data do not exist for districts below the threshold, where candidates do not need to report their revenues and expenditures to the CNCCFP. While we cannot use our RDD to measure effects on these outcomes, we do provide evidence on the spending patterns of different types of candidates above the threshold and on the changes which followed the introduction of campaign expenditures' reimbursement.

3.3 Identification assumptions

The estimates obtained from equation 1 identify the local average treatment effect around the threshold conditional on assuming that potential outcomes are continuous at the 9,000 inhabitants threshold (e.g., Imbens and Angrist, 1994; Hahn et al., 2001). We are confident that this assumption is satisfied, first, because no other voting rule or institutional feature changes at this threshold,[14] and second, because districts cannot sort at the threshold. Indeed, the centralized nature of French censuses leaves no room for the manipulation of population figures by mayors or departmental

[13] We did not digitize the booklets for the 2001 municipal elections, for which the data were only available for half of the candidates.

[14] See Eggers et al. (2018) for a list of policy changes affecting for instance the salary of the mayor or the number of municipal councilors at *other* population thresholds in French municipalities.

councilors. Furthermore, mayors can only ask for their municipality's population to be updated, leading to a complementary decree, if there is evidence that the population increased by 15 percent at least. In that case, the new official population is established by an independent administrator, preventing the manipulation of the threshold.

We further provide empirical support for our identification assumption by conducting several manipulation tests. First, we check whether the likelihood of experiencing a redistricting between elections *t-1* and *t* or of having been treated at *t-1* jumps at the threshold. Such discontinuities could suggest that incumbents are able to manipulate their population to benefit from the campaign finance regime that they like the most. Fortunately, the results shown in Table 1 for both municipal and departmental elections show that this is not the case. Second, we provide a broader test of manipulation by checking if there is a jump in the density of the running variable at the threshold (McCrary, 2008; Cattaneo et al., 2018, 2020). Third, we conduct balance tests on census variables such as the age distribution in the population, the share of women, and the distribution of occupations. Fourth, we check that outcomes defined at election *t-1* do not jump at the threshold either. The results of these tests are shown in Sections 4.1 and 5 for departmental elections and municipal elections, respectively.

Table 1: Changes since election *t-1* - Departmental and municipal elections

	(1)	(2)	(3)	(4)	(5)	(6)
	Departmental elections			Municipal elections		
Outcome	Redistricted	Treated in *t-1*	Linkable	Redistricted	Treated in *t-1*	Linkable
Treatment	0.007	0.052	-0.007	0.004	-0.044	-0.054
	(0.006)	(0.086)	(0.006)	(0.008)	(0.114)	(0.031)
Robust *p*-value	0.378	0.852	0.378	0.698	0.515	0.117
Observations	2,846	547	2,846	1,605	418	1,006
Polyn. order	1	1	1	1	1	1
Bandwidth	3,186	1,031	3,186	2,001	919	1,331
Mean, left of the threshold	0.000	0.364	1.000	0.004	0.413	0.978

Notes: Standard errors are in parentheses. Robust *p*-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity. We exclude the 2008 elections from the analysis for the outcome "Treated in *t-1*" in columns 2 and 5 since the same major census was in place for both the 2001 and 2008 elections.

3.4 Sampling frame

Our main sample includes the 2001, 2008, and 2014 municipal elections and the 1998, 2001, 2004, 2008, and 2011 departmental elections. We also use data from the 1995 municipal elections and the 1992 and 1994 departmental elections to define incumbents, challengers, and outsider candidates in the first elections in the sample (namely, the 2001 municipal elections and the 1998 and 2001 departmental elections).

Table 2 indicates the national census used to determine districts' official population, for each election in the sample. We use data from the 1990 and 1999 censuses (as well as complementary decrees which took place in between) to determine the official population for all elections until 2008. We use data from the 2008 and 2011 censuses for the 2011 departmental and 2014 municipal elections, respectively. Importantly, except for the 2008 municipal and departmental elections, each election was preceded by a different national census, leading to changes in all districts' official population.[15] Therefore, our estimates generally capture the impact of being treated once.

Table 2: Relevant censuses in place for each election in the main sample

Departmental elections	Relevant census	Municipal elections	Relevant census
1998	1990	2001; 2008	1999
2001; 2004; 2008	1999	2014	2011
2011	2008		

The 2008 municipal and departmental elections are exceptions: in most districts, the population and, therefore, the running and assignment variables, were the same as in the 2001 municipal and departmental elections, respectively. Therefore, we do not use the 2008 elections for the internal validity tests, as keeping them would double count districts where census variables and population figures do not evolve. We include the 2008 elections in all our other analyses but show the robustness of our results to excluding them in Appendices C and E.

We check the consistency of all election results, and drop one race in the 2001 departmental elections, for which we detect inconsistencies.[16] Furthermore, our main outcomes require linking districts over time: for instance, we cannot define the incumbent, and, thus, we cannot measure

[15] The 2001 and 2004 departmental elections both used population figures from the 1999 census, but they took place in different sets of districts, since only half of the seats were up for election until the 2013 reform.

[16] We consider elections as problematic if a second round took place even though a candidate obtained a majority of votes and 25 percent of the registered citizens in the first round, or vice versa; if the number of registered voters, turnout, or the number of total candidate votes is missing (we exclude this test for the 1995 municipal elections, as many newspaper sources did not report this outcome); if a candidate appears in the second round even though their first round vote share was below the qualification threshold; or if the sum of individual candidate votes does not add up to the total number of candidate votes.

effects on the likelihood that they are reelected, if the district is new. We define a district as linkable if it does not experience any major redistricting between elections in *t-1* and *t* and if there were no inconsistencies in the district's electoral results in election *t-1*.[17] In municipal elections before 2014, we further require that the district population was above 3,500 inhabitants both at *t-1* and *t*, so that the electoral rule was identical in both years.

Reassuringly, districts above the discontinuity are not more likely to be linkable with the last election than those below, as shown in Table 1. In Appendices C and E, we show the robustness of our results to including non-linkable districts in the sample for outcomes such as turnout or the probability of a candidate's victory in the first round, which can be constructed without linking elections over time.

Overall, our main sample includes 7,653 linkable municipal races (23,709 lists) and 9,938 linkable departmental races (52,651 candidates).[18] Table 3 gives summary statistics for both types of elections. In an average departmental race, 5.3 candidates compete in the first round, ten thousand voters are registered to vote, 63.6 percent of them vote and 60.8 percent cast a valid vote for one the candidates. Municipal elections appear less competitive: the number of candidates averages 3.1 and only 36.4 percent of races are decided in the second round, as compared with 68.6 percent for departmental elections. On the other hand, the average number of registered voters, turnout rate, and the share of elections won by the incumbent, challenger, or outsider candidates are very similar across both types of elections.

Beyond our main sample, we use the 1992 and 1994 departmental election results when exploring the mechanisms driving our results, in Section 6. These elections help us disentangle the contribution of spending limits and candidate expenditures' reimbursement since the former was implemented before these elections but the latter after.[19]

[17] Overall, we detect inconsistencies in the *t-1* election for one departmental race (corresponding to that 2001 race with inconsistencies) and for 185 races in the 2001 municipal elections (due to inconsistencies in the 1995 election results obtained from newspaper sources).

[18] When we add non-linkable elections, our sample includes 8,604 municipal races (26,164 lists) and 10,083 departmental races (53,600 candidates).

[19] We also use data from the 1985 and 1988 departmental elections to define incumbents, challengers, and outsider candidates in the 1992 and 1994 elections.

Table 3: Summary statistics

	Mean	S.D.	Min.	Max.	Observations
Panel A. Departmental elections					
Registered voters	10,010	6,920	289	48,783	9,938
Proportion of turnout	0.636	0.122	0.205	0.919	9,938
Proportion of candidate votes	0.608	0.115	0.197	0.894	9,938
Number of candidates	5.30	1.74	1	15	9,938
Number of female candidates	1.06	1.05	0	7	9,938
Number of non-party candidates	1.50	1.32	0	10	9,938
Number of non-classified candidates	0.23	0.53	0	5	9,938
Proportion of second rounds	0.686	0.464	0	1	9,938
Incumbent victory	0.578	0.494	0	1	9,938
Challenger victory	0.056	0.229	0	1	9,928
Outsider victory	0.348	0.477	0	1	9,938
Panel B. Municipal elections					
Registered voters	9,937	15,029	1,024	254,538	7,653
Proportion of turnout	0.640	0.078	0.330	1	7,653
Proportion of candidate votes	0.605	0.083	0.246	0.908	7,653
Number of candidates	3.10	1.52	1	12	7,653
Number of female candidates	0.53	0.78	0	7	7,653
Number of non-party candidates	1.74	1.22	0	9	7,653
Number of non-classified candidates	0.18	0.48	0	7	7,653
Proportion of second rounds	0.364	0.481	0	1	7,653
Incumbent victory	0.569	0.495	0	1	7,653
Challenger victory	0.065	0.246	0	1	7,219
Outsider victory	0.359	0.480	0	1	7,653

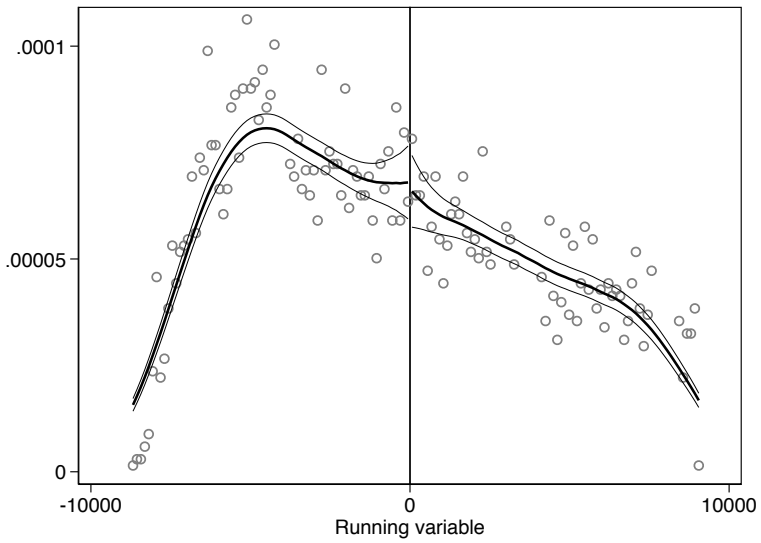
Notes: S.D refers to standard deviation, min. to minimum, and max. to maximum. The outcome "Challenger victory" is missing for districts where only one candidate ran in the previous election.

4 Effects in departmental elections

4.1 Validity checks

As discussed in Section 3.3, our RDD results can only be interpreted causally if districts do not sort across the 9,000 inhabitants cutoff. Figure 1 tests this assumption by checking that the density of the running variable does not jump at the threshold, in our main sample of departmental elections, using McCrary (2008)'s test. The Cattaneo et al. (2018) density plots shown in Appendix Figure B1 do not indicate any discontinuity at the threshold either, and the p-value of the manipulation test described in Cattaneo et al. (2018) is equal to 0.99. Adding non-linkable districts in the sample yields similar results.

Figure 1: McCrary (2008) density test - Main sample of departmental elections



Notes: We test for a jump at the threshold in the density of the running variable (the district population centered around 9,000 inhabitants), using McCrary (2008)'s method. The solid line represents the density of the running variable, while the thin lines represent the confidence intervals. We exclude the 2008 elections since in most districts, the running variable is the same as in 2001 (the same major census was in place for both elections).

Table 4 and Appendix Figure B2 show placebo effects on the main outcomes defined in the *previous* elections. None of them is statistically significant. Furthermore, Appendix Tables B1 and B2 show balance tests on sociodemographic variables, for the main sample as well as the sample including non-linkable races (see Appendix Figure B3 for the corresponding graphs, for a subset of outcomes). Only one out of 13 variables, the share of 30 to 44 years old, is statistically significant

(at the 5 percent level), which is in line with what would be expected and consistent with districts close to the left and to the right of the threshold having similar average characteristics.

Overall, we do not find any evidence that departmental election districts sort at the threshold, increasing our confidence in the reliability of our empirical strategy.

Table 4: Placebo tests, main outcomes defined in *t-1* - Main sample of departmental elections

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent		Challenger		Outsider		Victory in
	run	win	run	win	run	win	first round
Treatment	0.058	0.063	-0.010	0.001	0.006	-0.042	-0.061
	(0.043)	(0.054)	(0.047)	(0.024)	(0.010)	(0.051)	(0.050)
Robust p-value	0.284	0.402	0.890	0.963	0.570	0.530	0.195
Observations	1,728	1,471	1,428	1,317	1,030	1,638	1,705
Polyn. order	1	1	1	1	1	1	1
Bandwidth	3,438	2,941	2,848	2,648	2,059	3,284	3,411
Mean, left of threshold	0.728	0.552	0.229	0.046	0.995	0.357	0.322

Notes: Standard errors are in parentheses. Robust p-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election *t*. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The dependent variables refer to our main outcomes defined in election *t-1*. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity. We exclude the 1998 (resp. 2008) elections since in most districts, the population and, therefore, the running and assignment variables, were the same as in the 1992 (resp. 2001) elections.

4.2 Effects on competition

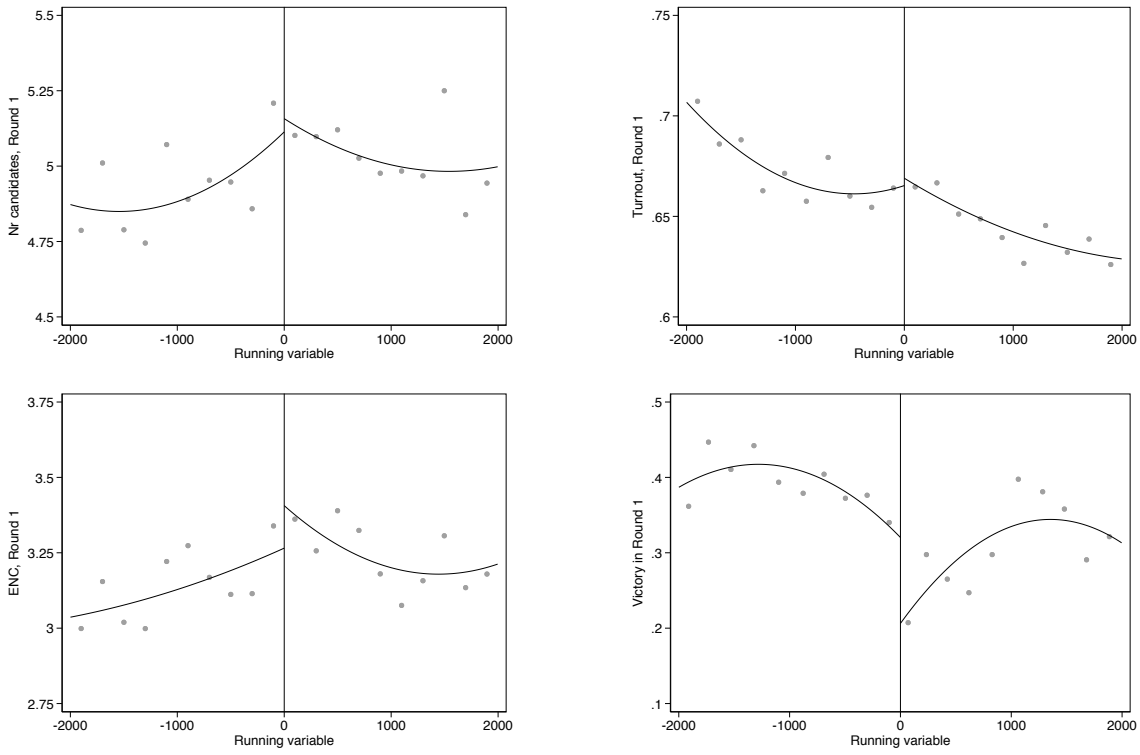
Our first set of outcomes relate to the competitiveness of elections. We first estimate effects on electoral supply: the total number of candidates, the number of outsider candidates (who were not present in the previous race in the district), and the number of insider candidates (who were present). Outsider candidates might be more likely to run above the threshold, as they know that mainstream candidates face a spending limit and they can expect their own campaign expenditures to be reimbursed, conditional on getting five percent of the votes or more. However, in equilibrium, two forces may limit the number of candidates. First, insider candidates might respond to the increased competition by staying out of the race or striking alliances. Second, if the number of potential candidates is too high, smaller candidates may reason that they are unlikely to obtain the

five percent vote share required to get reimbursed and decide to stay out.

Beyond effects on the number of candidates, the campaign finance rules that we evaluate may affect electoral competitiveness through a second channel: by increasing the amount of money spent by smaller candidates relative to established candidates. We measure election competitiveness using two indicators: the fragmentation of vote shares in the first round and, relatedly, the probability of any candidate winning in the first round. Our metric of fragmentation is the effective number of candidates as defined by Laakso and Taagepera (1979): $ENC = \frac{1}{\sum_1^n v_i^2}$, where n is the number of candidates and v_i the first round vote share of candidate i. We also estimate effects on voter turnout, which could increase due to higher competitiveness or to a larger and more diverse set of candidates.

We begin with a graphical analysis, in Figure 2, before providing formal estimates.

Figure 2: Impact on competition - Main sample of departmental elections



Notes: Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the district population centered around 9,000 inhabitants) is split into evenly-spaced bins for continuous outcomes and into quantile-spaced bins for binary outcomes. The continuous lines represent a quadratic fit. To facilitate visualization, the graph is truncated at 2,000 inhabitants around the cutoff.

In these graphs, each dot represents the average value of the outcome within a given bin of the running variable. We fit a quadratic polynomial on each side of the population threshold to

facilitate visualization. While there is not any clear effect on the number of candidates, turnout, and the effective number of candidates, we observe a large negative jump of the probability of a victory in the first round at the cutoff. These results suggest that, although there is no overall increase in fragmentation, the campaign finance rules penalize front-runners, preventing any of them from winning in the first round.

Table 5 reports formal estimates obtained using our preferred specification. Consistent with the graphs, we find that campaign finance rules which apply above the threshold reduce the probability that the election is won in the first round by 10.9 percentage points (30.9 percent), which is significant at the 5 percent level. The point estimates for other outcomes are small and non-significant. These results are robust to excluding the 2008 elections (so that we measure the effect of being treated only once), and to including districts that cannot be linked over time, as shown in Appendix Tables C1 and C2.

Table 5: Impact on competition - Main sample of departmental elections

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
	Number of			Turnout	ENC	Victory
	Candidates	Outsiders	Insiders	r1	r1	in first round
Treatment	0.046	0.010	0.028	0.010	0.086	-0.109**
	(0.119)	(0.119)	(0.065)	(0.009)	(0.089)	(0.044)
Robust *p*-value	0.513	0.855	0.471	0.235	0.246	0.012
Observations	2,326	2,663	2,407	2,306	2,451	2,151
Polyn. order	1	1	1	1	1	1
Bandwidth	2,610	2,993	2,702	2,577	2,741	2,410
Mean, left of threshold	5.055	3.597	1.461	0.656	3.246	0.353

Notes: Standard errors are in parentheses. Robust *p*-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election *t*. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity.

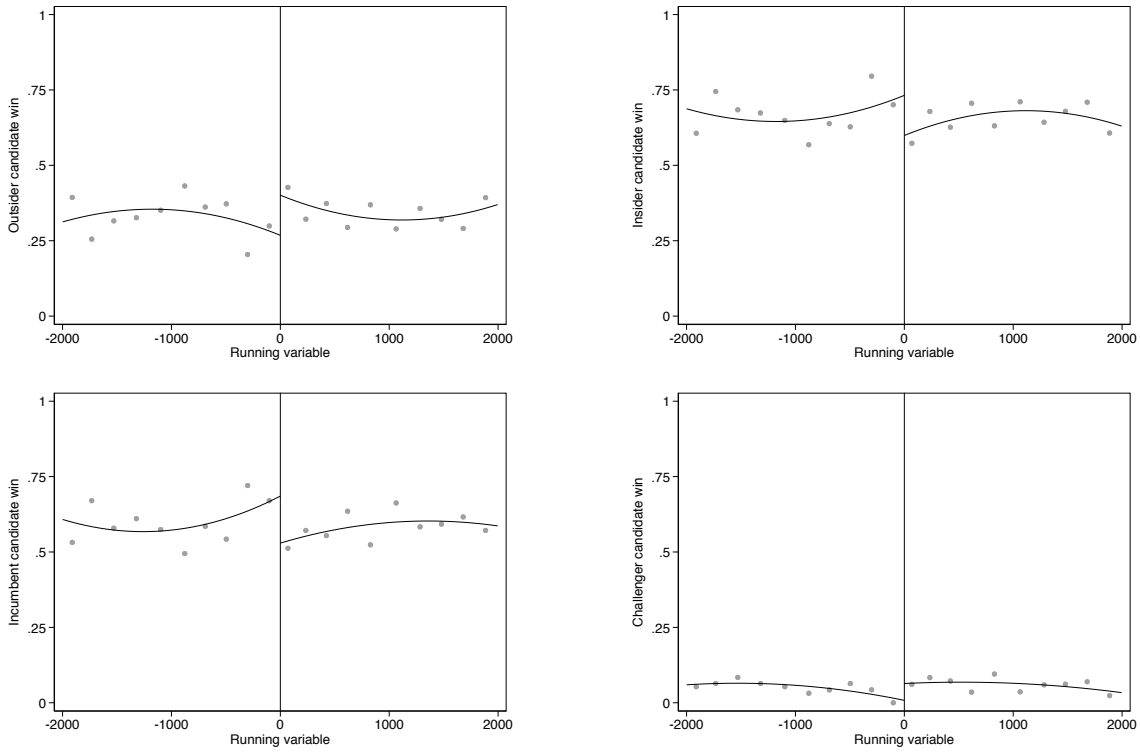
4.3 Effects on candidate selection and winner identity

4.3.1 Effects on winner identity

Despite the lack of effect on the total number of candidates, spending limits and the reimbursement of campaign expenditures may affect the selection of candidates who choose to enter the race and, in particular, the likelihood that the incumbent and the challenger of the previous race run again. Furthermore, the increase in election competitiveness indicated by the lower likelihood of a victory in the first round could affect the relative chances of different types of candidates and the identity of the winner. Therefore, we now explore effects on the outcomes of *specific candidates*.

We start with a graphical investigation of the impact of the campaign finance rules on the probability of a victory by an outsider, an insider, the incumbent, and their challenger.

Figure 3: Impact on winner identity - Main sample of departmental elections



Notes: Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the district population centered around 9,000 inhabitants) is split into quantile-spaced bins. The continuous lines represent a quadratic fit. To facilitate visualization, the graph is truncated at 2,000 inhabitants around the cutoff.

Figure 3 shows clear positive jumps at the threshold for the probabilities of outsider and challenger candidates winning the election, and negative jumps for incumbents and insider candidates. The corresponding point estimates, shown in Table 6, are sizeable and all significant at the 1 or 5 percent level. The probability of outsider and challenger candidates winning increases by 9.2 percentage points (31.9 percent) and 5.2 percentage points (288.9 percent), respectively, while the probability of the incumbent winning declines by 14.5 percentage points (21.2 percent). In absolute terms, the effects on challengers and outsiders almost perfectly add up to the effect on incumbents. In other words, the campaign finance rules level the playing field and increase the winning chances of new candidates and challengers from the previous race at the expense of the incumbent.

Once again, we check the robustness of these results to excluding the 2008 elections, in Appendix Table C3. While the effects on insider and outsider candidates become nonsignificant, our results on challengers and incumbents remain significant at the 5 and 10 percent level, respectively.

Table 6: Impact on winner identity - Main sample of departmental elections

	(1)	(2)	(3)	(4)
Outcome	Outsider win	Insider win	Incumbent win	Challenger win
Treatment	0.092**	-0.092**	-0.145***	0.052**
	(0.042)	(0.042)	(0.046)	(0.020)
Robust *p*-value	0.024	0.024	0.002	0.012
Observations	1,686	1,686	1,392	1,819
Polyn. order	1	1	1	1
Bandwidth	1,886	1,886	1,578	2,037
Mean, left of threshold	0.288	0.712	0.683	0.018

Notes as in Table 5.

4.3.2 Effects on candidate selection

The effects on candidates' probability of winning could come both from voters becoming less likely to vote for incumbents when they are in the race, and from candidates adjusting their entry decision. The outcomes used in Table 6 are unconditional winning probabilities, such that candidates who do not compete in the election are assigned a value of 0. Therefore, the negative impact on the reelection of the incumbent could result in part from the fact that some incumbent candidates choose not to run because they know that they will not be able to outspend their competitors. Indeed, they know that their own expenditures will be limited and they can reasonably expect their competitors who are likely to be reimbursed to spend more money than they would otherwise. The

same reasoning may increase challengers' likelihood to run, contributing to the positive impact on their likelihood of winning. By contrast, the positive effect on the likelihood of a victory by an outsider candidate should not be driven by increased entry, given the null effect on the number of outsider candidates shown in Table 5, column 2.[20]

We test and verify the hypotheses regarding the incumbent and challenger candidates' likelihood of running in Panel A of Table 7. Columns 1 and 4 show a reduction in incumbents' probability to run by 7.4 percentage points (9.6 percent) and an increase in challengers' likelihood to run by 8.4 percentage points (47.7 percent). Columns 2 and 5 report effects on the unconditional likelihood of winning which we already showed in Table 6, for reference. Columns 3 and 6 show effects on unconditional vote shares. These effects are more difficult to interpret but they are an ingredient of the conditional estimates reported in Panel B, which we turn to now.

4.3.3 Effects on winning conditional on running

We now investigate whether campaign finance rules affect the chances of winning and the vote share of the winner and of their previous challenger, *conditional on participating in the race*. We cannot simply compare the elections below and above the discontinuity in which incumbents or challengers are present. Indeed, the regression discontinuity framework does not imply that incumbents and challengers who choose to run in districts just above the discontinuity are similar to those running in districts just below. In fact, we just showed that the rules affect these candidates' likelihood of entering the race.

To circumvent this difficulty, we follow Anagol and Fujiwara (2016) and Granzier et al. (2019) who adapt Lee (2009)'s method to derive bounds in a regression discontinuity design context. Focusing on incumbent candidates, we define $T = 0$ when districts are below 9,000 inhabitants and $T = 1$ otherwise. We further define R_0 and R_1 as potential outcome indicators for running when $T = 0$ or $T = 1$, respectively. In the data, we only observe $R = TR_1 + (1-T)R_0$. We know whether the incumbent runs for reelection in districts above 9,000 inhabitants but do not know if she would have run again in districts below, and conversely.

In a second step, we define W_0 and W_1 as potential outcomes for winning the election conditional on running, such that we only observe $W = R[TW_1 + (1-T)W_0]$. If the incumbent does not run again $(R = 0)$, she does not win $(W = 0)$, and we do not observe W had she run. If the incumbent runs in a district above 9,000 inhabitants, we observe whether she wins the election but do not know if she would have won in a district below, and conversely.

We then classify incumbent candidates as belonging to four categories. "Always takers" are incumbents who always run again, regardless of T; "never takers" are incumbents who never run

[20]Moreover, we do not find any significant impact on the likelihood that outsider candidates participate in the election, as shown in Appendix Table A1.

again; "compliers" are incumbents who run again only if they are in a district below the threshold, where the lack of spending limits and of public reimbursement of campaign expenditures mean they can expect to face less competition; "defiers" are incumbents who would run in a district above the threshold, but not below.

We need to assume that there are no defiers to be able to derive bounds on our estimates: incumbents who run in districts above 9,000 inhabitants would also run in districts below. Assuming away such "defiers" yields $R_1 \leq R_0$, such that we can decompose the impact on the unconditional probability of the incumbent winning as:

$$\underbrace{E(W_1R_1 - W_0R_0|x=0)}_{RD\ effect\ on\ W} = \underbrace{Prob(R_1 > R_0|x=0)}_{RD\ effect\ on\ R} \cdot \underbrace{E(W_1|x=0, R_1 < R_0)}_{Unobservable}$$

$$+ \overbrace{E[W_1 - W_0|x=0, R_0=1]}^{Effect\ on\ win\ cond\ on\ being\ always-taker\ or\ complier} \cdot \underbrace{E(R_0|x=0)}_{lim_{x\uparrow 0}E[R|x]}$$

In words, the impact on the incumbent's victory sums the impact on the incumbent running, multiplied by the probability that an incumbent complier would win if they entered the race, in districts closely above the discontinuity; and the effect of winning conditional on being an always taker or complier, multiplied by the probability that incumbents in districts just below the threshold run for reelection. Rewriting the equation above, we can decompose the impact on the incumbent winning conditional on running as:

$$\overbrace{E[W_1 - W_0|x=0, R_0=1]}^{Effect\ on\ win\ cond\ on\ being\ always-taker\ or\ complier} = \underbrace{\frac{1}{E(R_0|x=0)}}_{lim_{x\uparrow 0}E[R|x]} [\underbrace{E(W_1R_1 - W_0R_0|x=0)}_{RD\ effect\ on\ W}$$

$$- \underbrace{Prob(R_1 > R_0|x=0)}_{RD\ effect\ on\ R} \cdot \underbrace{E(W_1|x=0, R_1 < R_0)}_{Unobservable}]$$

The only unobservable term in this equation, $E(W_1|x=0, R_1 < R_0)$, refers to the probability that a complier would win if she ran in districts closely above the threshold, an outcome which we cannot observe, by definition. Since all the other terms of the equation are observable, we simply need to make assumptions about this term to derive lower and upper bounds on the effects on winning conditional on running.

To derive a lower bound (largest possible impact of spending rules on the incumbent probability of winning), we assume that compliers would never win in districts closely above the threshold: $E(W_1|x=0, R_1 < R_0) = 0$. To derive an upper bound (lowest possible impact on the incumbent

probability of winning), we assume that compliers would, at most, have the same probability of winning as incumbents running in districts below the discontinuity: $E(W_1|x=0, R_1 < R_0) = 0.871$. This yields a conservative estimate, as this probability is higher than the probability of winning of incumbents who run in districts above the discontinuity: 76.7 percent.

We extend this analysis in two ways. First, we use the same method to derive bounds on *challengers*' probability of winning conditional on running. Since challengers are more likely to run above the discontinuity, our no defiers assumption states that challengers who run in districts below 9,000 (where they might be at a disadvantage due to the lack of limit on incumbents' spending) would also run in districts above. Second, we use our effects on unconditional vote shares to derive bounds on the effects on incumbents and challengers' *vote shares* conditional on running.

We use a bootstrapping procedure to estimate the standard errors of the bounds. For each outcome of interest, we draw a sample of districts with replacement, compute the lower and upper bounds following the method stated above, and repeat these steps 1,000 times.

Panel B of Table 7 shows the results. Conditional on running, the campaign spending rules present above the threshold cause a reduction in incumbents' first round vote share and in their probability of getting reelected. Their vote share decreases by 3.0 to 7.6 percentage points (6.3 to 16.1 percent of the mean incumbent vote share in districts just below the cutoff) and their likelihood of reelection by 10.5 to 18.9 percentage points (12.1 to 21.7 percent). By contrast, challengers' vote share and likelihood of winning increase by 3.3 to 13.0 percentage points (13.0 to 51.2 percent) and 11.0 to 19.8 percentage points (79.1 to 142.4 percent), respectively, conditional on running. The upper bounds of these effects are statistically significant, but the lower bounds are not.

These results are robust to excluding the 2008 elections: as shown in Appendix Table C4, the effects on incumbents' winning probability are a bit lower in this sample, but effects on challengers are larger, with lower bounds significant at the 5 percent level for winning, and at the 10 percent level for vote shares.

Overall, our results suggest that the negative impact of campaign spending rules on the incumbent's probability of winning is driven both by their lower probability to enter the race in the first place, and by voters' lower propensity to vote for them conditional on running. Similarly, the positive impact on challengers' probability of winning is driven both by increased entry and an increased vote share, conditional on running.

Table 7: Impact on running, winning, and vote shares - Main sample of departmental elections

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
	Incumbent			Challenger		
	run	win	vote share, R1	run	win	vote share, R1
Panel A. Unconditional effects						
Treatment	-0.074**	-0.145***	-0.058***	0.084**	0.052**	0.034***
	(0.032)	(0.046)	(0.020)	(0.038)	(0.020)	(0.012)
Robust p-value	0.023	0.002	0.005	0.020	0.012	0.003
Observations	2,579	1,392	1,874	1,827	1,819	1,911
Polyn. order	1	1	1	1	1	1
Bandwidth	2,876	1,578	2,113	2,056	2,037	2,159
Mean	0.767	0.683	0.367	0.176	0.018	0.044
Panel B. Conditional effects						
Upper bound		-0.189**	-0.076**		0.198**	0.130***
Boot. std error		(0.096)	(0.034)		(0.081)	(0.042)
Lower bound		-0.105	-0.030		0.110	0.033
Boot. std error		(0.075)	(0.019)		(0.068)	(0.021)
Mean		0.871	0.473		0.139	0.254

Notes: Panel A and Panel B show effects on unconditional outcomes and bounds of effects conditional on running, respectively. The notes for Panel A are as in Table 5. In Panel B, the mean, left of the threshold, indicates the value of the outcome for the candidates on the left of the threshold, conditional on running. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively, of the bootstrapped standard errors.

4.4 Effects on the winning orientation, polarization, and representativeness

4.4.1 Effects on the winning orientation

To understand how the campaign finance rules affect the political landscape, we now explore their effects on the winner's political orientation

The first outcome that we consider, in Table 8, column 1, is a dummy equal to 1 if the orientation of the winner is identical to the orientation of the incumbent. Indeed, the negative impact on the reelection of the incumbent would perhaps be of little consequence if the candidate replacing them (whether this candidate is the previous race's challenger or an outsider) was of the same orientation. Instead, we find that the campaign finance rules increase the likelihood that the seat falls to a candidate of a new political orientation by 8.2 percentage points, which is significant at the 5

percent level, and more than half the size of the effect on incumbents' reelection.

We then go one step further and ask whether changes in the orientation of the winner compensate each other across districts or whether they tend to go in the same direction and to systematically benefit one specific orientation. Spending patterns by candidates on the left and on the right suggest that the former stood to benefit from the reform at the expense of the latter. Appendix Table A2 compares average expenditures to ceiling ratios as well as contributions to ceiling ratios for left and right-wing candidates, in districts just above the threshold, in departmental elections that preceded (1992 and 1994) and followed (1998 and 2001) the introduction of campaign expenditures' reimbursement. Prior to the 1995 reform, expenditures from candidates on the left only accounted for 17.2 percent of the spending limit, compared to 32.8 percent for their counterparts on the right. These differences in spending reflect differences in personal contributions by the candidates (3.2 percent of the ceiling for candidates on the left against 13.9 percent for candidates on the right) and in donations they received (6.2 percent against 14.5 percent). Given these baseline spending patterns, the 1995 reform, that introduced the reimbursement of campaign expenditures, dramatically increased relative spending by candidates on the left. After the reform, personal contributions by right-wing candidates more than doubled, as a ratio of spending limits, but they increased nearly tenfold for candidates on the left. On average, left-wing and right-wing candidates contributed 31.0 percent and 34.4 percent of the ceiling with their own money, and they spent 39.6 and 43.9 percent of the limit. In other words, differences in average campaign expenditures between these two groups were much lower after than before the reform.

Table 8 confirms that candidates on the left also benefited from the reform electorally. Campaign finance rules above the threshold increase the likelihood of a victory by a left-wing candidate by 8.5 percentage points (17.9 percent), which is significant at the 10 percent level. Victories by center and right-wing candidates become less likely, by 2.1 and 5.3 percentage points respectively, but these estimates are not statistically significant.

Table 8: Impact on winning orientation - Main sample of departmental elections

Outcome	(1) Incumbent or. win	(2) Far-left win	(3) Left win	(4) Center win	(5) Right win	(6) Far-right win	(7) Non-classified win
Treatment	-0.082**	-0.003	0.085*	-0.021	-0.053	-0.000	0.010
	(0.037)	(0.003)	(0.047)	(0.014)	(0.041)	(0.000)	(0.008)
R. p-value	0.024	0.255	0.059	0.149	0.203	0.334	0.263
Obs.	1,534	2,196	2,531	2,576	3,362	1,604	2,126
Polyn.	1	1	1	1	1	1	1
Bdw	1,709	2,459	2,813	2,865	3,784	1,799	2,383
Mean	0.862	0.003	0.475	0.043	0.477	0.000	0.001

Notes as in Table 5. "Or." stands for "orientation."

4.4.2 Effects on polarization and representativeness

While campaign finance rules level the playing field, improved performance by candidates from non-mainstream platforms could increase polarization. Moreover, by strengthening outsiders, these reforms could lead voters to split their votes across multiple candidates of the same orientation, which could result in suboptimal outcomes such as the defeat of the Condorcet winner (Pons and Tricaud, 2018).

To further characterize the effects of the reforms on electoral outcomes, we first measure the polarization of the results. Using the sample of 86 percent of departmental races for which each candidate can be matched to a ParlGov ranking on the [0-10] left-right scale, we follow Dalton (2008) and build the following measure of polarization: $\sqrt{\sum v_i \left(\frac{p_i - \bar{p}}{0.5}\right)^2}$, where $\bar{p} = \sum v_i p_i$, v_i is candidate i's vote share, and p_i, the ideological positioning of their party or affiliation. This index takes the value 0 when all candidates converge to the same position and 10 when they are equally split between the two most extreme positions. As shown in Table 9, the impact on this outcome is small and non-significant, indicating that campaign finance rules do not increase polarization.

Second, we assess whether the legislation affected the representativeness of the winner. We proxy voter preferences using first round results and aggregate first round vote shares by orientation. We measure effects on the first round vote share of the winner's orientation and on a dummy equal to 1 if that orientation had obtained the most votes. We find a negligible effect on the first outcome (column 2) and a negative but small and non-significant effect on the second (column 3), indicating that the rules above the threshold do not decrease the representativeness of the winner with respect to the distribution of first round vote choices.

The results presented in Sections 4.4.1 and 4.4.2 are robust to adding non-linkable districts (Appendix Table C6). The effects on the likelihood of a victory by the incumbent orientation and by a left-wing candidate remain negative and positive, respectively, but they become insignificant when excluding the 2008 elections (p-value=0.31 and 0.11, respectively, Appendix Table C5).

Table 9: Impact on polarization and winner's representativeness - Main sample of departmental elections

	(1)	(2)	(3)
Outcome	Polarization	Vote share winner's orientation	Top orientation winning
Treatment	-0.082	-0.002	-0.037
	(0.083)	(0.014)	(0.029)
Robust p-value	0.340	0.888	0.171
Observations	2,161	2,297	1,871
Polyn. order	1	1	1
Bandwidth	2,770	2,565	2,098
Mean, left of threshold	4.868	0.583	0.922

Notes as in Table 5.

4.5 Additional robustness checks

To assess the robustness of our findings, we first evaluate the possibility that the main results on the probability of victory in the first round and on the likelihood that incumbents, challengers, and outsider candidates run and win may arise from chance rather than reflecting a causal relationship. To do so, we implement our regression discontinuity design at ten false population thresholds below and above the true 9,000 inhabitants cutoff, in Appendix Tables C7 through C10. The number of significant results is not higher than would be expected: six out of 70 point estimates are significant at the 10 percent level, and only one is also significant at the 5 percent level.

Second, we check the robustness of our results to employing a quadratic specification, in Appendix Table C11. The point estimates and their significance remain very similar.

Finally, we check the sensitivity of the results to bandwidth selection, in Appendix Figures C1 through C4. For each outcome of interest, these graphs plot the point estimates and associated 5 percent confidence intervals for bandwidths ranging from plus to minus 500 inhabitants around the data-driven bandwidth selected based on Calonico et al. (2019). Overall, our results are very robust to changes in bandwidth size, whether we use a linear or quadratic specification.

5 Effects in municipal elections

This section investigates the impact of the campaign finance rules in municipal elections.

We first conduct the validity tests discussed in Section 3.3. Appendix Tables D1 and D2 show balance tests on sociodemographic variables for the main sample and the sample also including non-linkable districts. Two out of 26 point estimates are significant at the 10 percent level.

Appendix Figures D1 and D2 test the assumption of no sorting across the threshold using the McCrary (2008) graph and the Cattaneo et al. (2018) density plots. Both graphs show positive jumps at the threshold and we reject the null of no manipulation using Cattaneo et al. (2018)'s test, whether non-linkable municipal districts are excluded (p-value=0.032) or not (p-value=0.022). We conduct an election-by-election investigation of this result in Appendix Figures D3, D4, and D5 and notice that the jump in the density of the running variable is driven by the 2014 election (p-value = 0.004), while the 2001 and 2008 elections do not show any evidence of a jump (p-value=0.488 and 0.898). We do not consider the positive jump in the 2014 election as definite evidence of manipulation, given the difficulty to bend the rules used to determine municipalities' official population which we described in Section 3.3, and because one would expect manipulation to go in the opposite direction. Indeed, if anything, incumbent mayors may try to maintain the population of their municipality *below* the cutoff in order to limit competition, which would generate a negative jump in the density of the running variable at the threshold. Similar to Corbi et al. (2019), we check the robustness of our results to considering each municipal election separately, to make sure that they are driven neither by the potentially problematic 2014 election year nor by the fact that most treated districts in the 2008 municipal election had already been treated a first time in 2001. Indeed, recall that the populations in place in the 2001 and 2008 elections were mostly identical since no major census took place in between.

Table 10 shows the effects on competition in Panel A, and on winner identity in Panel B. These effects are lower in magnitude than in departmental elections, and, unlike in departmental elections, none of them is statistically significant. We obtain similar null results when we consider the 2001, 2008, and 2014 municipal elections separately (Appendix Tables E1 through E3), and when we include non-linkable districts in the sample used to measure effects on competition (Appendix Tables E4 through E7). We investigate the mechanisms driving the difference between results in departmental and municipal elections in the next section.

Table 10: Impact on competition and winner identity - Main sample of municipal elections

Panel A. Competition

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	-0.040	-0.034	-0.017	0.003	0.036	-0.008
	(0.135)	(0.131)	(0.069)	(0.009)	(0.099)	(0.059)
Robust *p*-value	0.778	0.763	0.911	0.567	0.762	0.822
Observations	1,426	1,433	2,258	1,189	1,455	1,315
Polyn. order	1	1	1	1	1	1
Bandwidth	1,908	1,913	2,803	1,618	1,939	1,773
Mean, left of threshold	2.920	1.816	1.106	0.637	2.425	0.606

Panel B. Winner identity

	(1)	(2)	(3)	(4)
Outcome	Outsider win	Insider win	Incumbent win	Challenger win
Treatment	-0.022	0.022	-0.030	0.038
	(0.054)	(0.054)	(0.054)	(0.033)
Robust *p*-value	0.653	0.653	0.686	0.209
Observations	1,219	1,219	1,487	1,318
Polyn. order	1	1	1	1
Bandwidth	1,670	1,670	1,975	1,848
Mean, left of threshold	0.374	0.626	0.562	0.0610

Notes as in Table 5.

6 Mechanisms

The results shown in Sections 4 and 5 indicate that the effects of campaign finance rules vary across election types. In particular, the rules decrease incumbents' likelihood to run again and get reelected in departmental elections but not in municipal elections. In this section, we discuss the reasons that could account for this difference and we ask whether the effects in departmental elections are driven primarily by campaign spending limits or by the reimbursement of campaign expenditures.

6.1 Municipal versus departmental elections

One possible explanation for the differences in results between departmental and municipal elections is that they reflect differences between the voting rules used in these two types of elections. Municipal elections use a two-round list system with proportional representation, while departmental elections are held under a single candidate two-round plurality voting rule. These institutional differences may explain our results through three complementary mechanisms.

First, in municipal elections, candidates' ability to reach their desired amount of spending is likely to depend less on reimbursement by the state. Indeed, campaign costs can be split between the mayoral candidate and the other 26 members of the list, unlike in departmental elections where the campaign is carried out by the candidate alone. In addition, municipal election candidates rely less exclusively on their own contributions because they are more likely to receive private donations: as shown in Table 11, in municipalities just above the threshold (with 9,000 to 11,000 inhabitants), donations account for 13.1 percent of the spending ceiling in municipal elections, against 4.1 percent in departmental elections.

Second, in departmental elections, spending limits and reimbursement benefit challengers and outsider candidates because they level the playing field. In municipal elections, the marginal returns of campaign expenditures may be lower, decreasing the equalizing effect of these rules. Indeed, the presence of multiple candidates in each list increases the odds that voters know at least one of them, and voters' higher baseline level of information may make it more difficult and costly to win them over. In addition, all candidates on the list can devote time to reach out to voters, and time may be a substitute for money. Finally, marginal returns may simply be lower due to higher average expenditures in municipal elections: 0.87 euros per capita, versus 0.31 euros per capita in departmental elections (columns 3 and 4 of Table 11).

Table 11: Composition of candidates' campaign contributions by type of election:

	% of spending ceiling		EUR per capita	
	Municipal elec.	Departmental elec.	Municipal elec.	Departmental elec.
Total expenditures	0.588	0.401	0.87	0.31
Donations	0.131	0.043	0.19	0.03
Party contributions	0.019	0.017	0.03	0.01
Personal contributions	0.439	0.339	0.65	0.26
Natural advantages	0.016	0.016	0.02	0.01
Other contributions	0.001	0.001	0.00	0.00

Notes: This table provides average measures by candidate and by election for each of the defined outcomes as a percentage of the spending ceiling in the first two columns and in EUR per capita in the last two columns. To make districts across municipal and departmental elections comparable, we focus on districts close to the cutoff (between 9,000 and 11,000 inhabitants) and on nearby elections years for which we have expenditure data for both municipal and departmental elections. Namely, we compare the 2008 and 2014 municipal elections with the 2008 and 2011 departmental elections. Note that the sum of contributions does not necessarily add up to total expenditures of candidates, as contributions need not be exhausted.

Third, the factors affecting candidates' decision to compete or stay out of the race may also differ across election types. In departmental elections, we find suggestive evidence that the negative impact on incumbents' likelihood to run for reelection is partly driven by pressure exerted on them by their party. We compare effects for incumbents affiliated with a party (Appendix Table A3) and for those who are not (Appendix Table A4).[21] We find that party-affiliated incumbents are driving the results: In the corresponding districts, campaign finance rules reduce incumbents' probability of running by 9.4 percentage points and their unconditional probability of winning by 16.4 percentage points. Effects on running and winning are much lower, and non-significant, for non-party-affiliated incumbents. These results suggest that, in departmental races above the threshold, where electoral competition is greater, political parties successfully prevent incumbents that they expect to be defeated from running again.

By contrast, as shown in Table 12, we do not find any negative effect on incumbents' presence (or on the presence of challengers and outsider candidates) in municipal elections (see Appendix Tables E8 through E10 for separate 2001, 2008, and 2014 results). Incumbents' ability to withstand pressure to drop out of their reelection bid, in these races, may come again from the list format. Incumbents can invite loyal party members as well as possible opponents to join their list, before the first round or between rounds, which increases their bargaining power. In addition, they know

[21] We identify party-affiliated incumbents as those who had a party label in the previous election, irrespective of the present election, to avoid endogeneity concerns.

that they will most likely obtain a seat on the municipal council if they run, even if they fail to be reelected as mayor, which decreases the risk of entering the race. In fact, 99 percent of incumbents who do run again get a seat.

Table 12: Impact on running - Main sample of municipal elections

	(1)	(2)	(3)
Outcome	Incumbent	Challenger	Outsider
Treatment	-0.022	0.002	-0.001
	(0.049)	(0.054)	(0.028)
Robust p-value	0.788	0.882	0.959
Observations	1,779	1,457	1,774
Polyn. order	1	1	1
Bandwidth	2,298	2,008	2,280
Mean, left of threshold	0.719	0.269	0.908

Notes as in Table 5.

6.2 Spending limits versus reimbursement

We now investigate whether the effects in departmental elections are driven primarily by spending limits or by the reimbursement of candidate expenditures. While estimating the joint impact of both rules is interesting, as many countries condition public funding of electoral campaigns on complying with spending limits, disentangling their respective importance is helpful to better understand the mechanisms underlying our results and to inform future campaign finance reforms.

The result in Section 4.4 showing that left-wing candidates, who benefit from the reimbursement more than their right-wing counterparts, are also those whose electoral outcomes improve the most, is a first piece of evidence suggesting that the reimbursement of campaign expenditures plays an important role.

We bring additional evidence by exploiting the departmental elections held in 1992 and 1994. These elections enable us to isolate the effect of spending limits because they took place after the 1990 reform enforcing limits for districts above the discontinuity, but before the 1995 reform enacting the reimbursement of candidates. We should expect null effects in these earlier elections if reimbursement is the main driver of the effects we observe in subsequent elections. This is indeed what we find. As shown in Appendix Table A5, point estimates are of a lower magnitude in the 1992 and 1994 elections than afterwards, and they are generally not significant. The only exception is the effect on challengers' victories, which is significant at the 10 percent level, but has

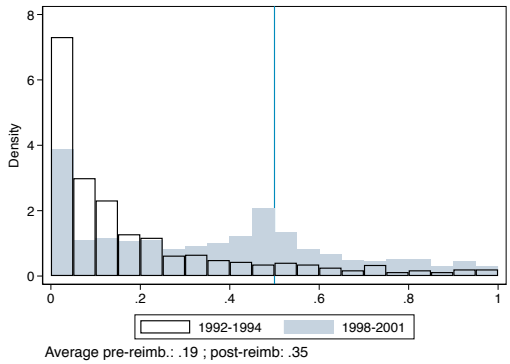
a negative sign, contrary to the positive effect observed after the introduction of reimbursement.

While these results suggest that effects post 1995 are driven by reimbursement rather than spending limits, alternative interpretations remain possible. The tightening of spending limits concomitant to the introduction of public reimbursement, in 1995, could play a role, and limits and reimbursement may be complementary and jointly explain the effects. Therefore, we go one step further and provide direct evidence on changes in candidate spending and contribution patterns between the 1992-1994 and the 1998-2001 departmental elections, in districts just above the threshold. Figures 4 and 5 plot the distribution of spending to ceiling ratios as well as personal contributions to ceiling ratios for all candidates (upper left graph) and separately for incumbents, challengers, and outsiders (upper right graph and lower graphs).

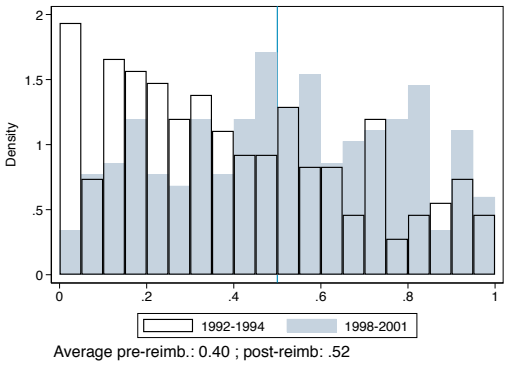
We first observe large outward shifts of both distributions to the right, after the 1995 reform. Expenditures and personal contributions rise as a share of the ceiling for all types of candidates, but the increase is much larger for challengers and outsiders than for incumbents. The fact that these candidates are also the ones who benefit from the reform electorally points to the important role of the reimbursement. Second, both sets of histograms show bunching at 50 percent of the ceiling post 1995 only, particularly for challengers and outsiders. This pattern underlines the role played by reimbursement even more directly, since 50 percent of the ceiling is the maximum amount of expenditures which candidates can submit for reimbursement (conditional on obtaining more than 5 percent of the votes). Interestingly, the bunching is slightly stronger for personal contributions and driven by challengers and outsiders. This is consistent with the fact that the reimbursement only applies to personal expenditures, so that the 50 percent mark is not relevant for other sources of campaign money. Candidates who contribute 50 percent of the ceiling with their own money but also receive private donations or party contributions will fall just below 50 percent in the graph plotting personal contributions but above that mark in the graph plotting total spending. Third, we observe a bit of bunching of overall spending at 100 percent, corresponding to candidates who spend nearly exactly the maximum amount of money authorized. However, this bunching is similar before and after 1995, and it is much lower than the bunching at 50 percent, which again only appears after 1995.

In sum, this graphical evidence underscores the dramatic changes in campaign spending which resulted from the introduction of personal expenditures' reimbursement in 1995. By contrast, while the spending limit does constrain a small subset of candidates, it does not become more binding after 1995. These patterns, combined with the stark difference between effects on our main outcomes in departmental elections before and after 1995 all point to the conclusion that reimbursement, not spending limits, drives our results.

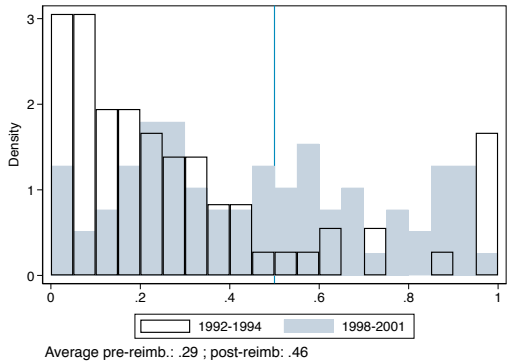
Figure 4: Expenditures to ceiling ratios - Main sample of departmental elections



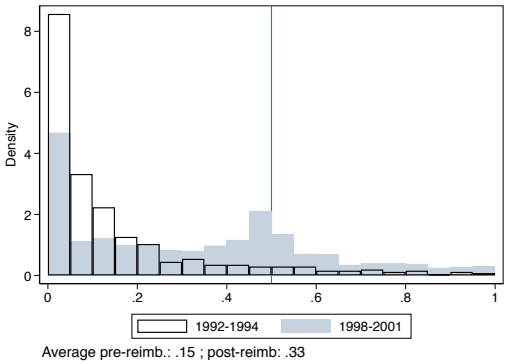
All candidates



Incumbent candidates



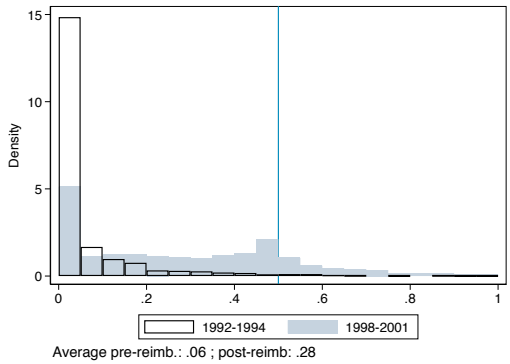
Challenger candidates



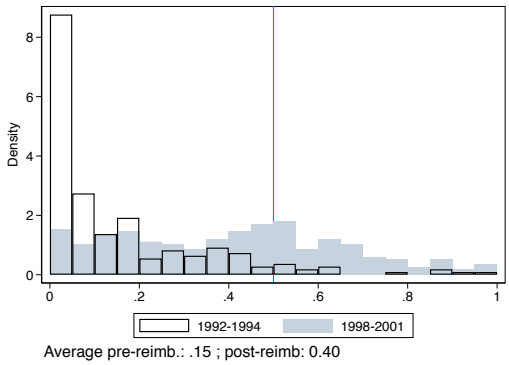
Outsider candidates

Notes: The level of analysis is the candidate and the sample includes only districts above 9,000 inhabitants, for which data on campaign spending are available. We further exclude districts above 11,000 inhabitants to focus on candidates running in districts close to the cutoff. The graphs are trimmed at 1, thus excluding a handful of candidates whose expenditures exceeded the ceiling.

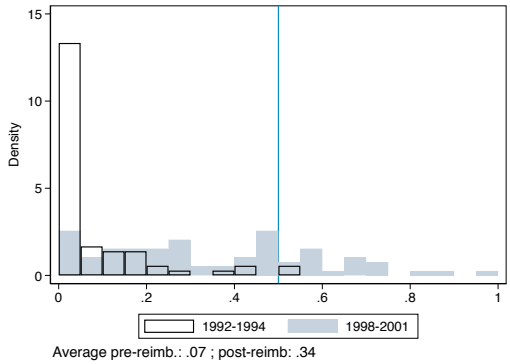
Figure 5: Personal contributions to ceiling ratios - Main sample of departmental elections



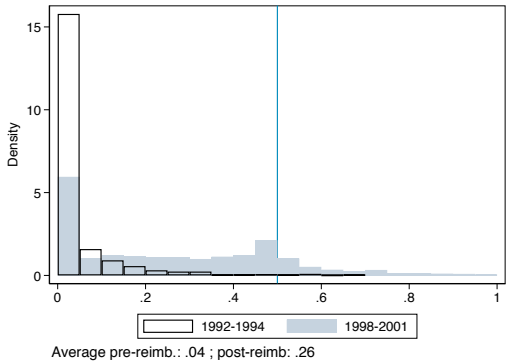
All candidates



Incumbent candidates



Challenger candidates



Outsider candidates

Notes: The level of analysis is the candidate and the sample includes only districts above 9,000 inhabitants, for which data on campaign spending are available. We further exclude districts above 11,000 inhabitants to focus on candidates running in districts close to the cutoff. The graphs are trimmed at 1, thus excluding a handful of candidates whose expenditures exceeded the ceiling.

7 Conclusion

This paper investigates how campaign finance rules affect candidate selection and electoral outcomes by exploiting two reforms that took place in France in the early 1990s. After the reforms, the rules differed for cantons and municipalities above and below 9,000 inhabitants, allowing us to estimate their effects with a regression discontinuity design.

Our results first show that the reimbursement of campaign expenditures by the state has the potential to level the playing field and to substantially reduce incumbents' advantage.

In departmental elections, the amount of money spent by competitors increased relatively to incumbents, after the introduction of public reimbursement in districts above the cutoff in

1995. Overall, public funding decreased incumbents' likelihood to be reelected by 14.5 percentage points, due to negative effects on their likelihood to run and on their vote share and winning, conditional on running. The weakening of incumbents benefits the candidate who was their runner-up in the previous race as well as new candidates, who all see their chances of winning improve, and it increases the likelihood of a change in the orientation of the elected official. Overall, these turnovers tend to benefit the left, whose candidates are often outspent by right-wing competitors absent public funding. Importantly, we note that this policy neither increases the polarization of results nor decreases the representativeness of the winner's orientation with respect to the distribution of first round vote choices.

Our results also show that the effects of campaign finance rules can be mitigated due to weaknesses in the exact design of these rules and due to the format of some elections.

First, we do not find any effect of spending limits when we examine the 1992 and 1994 departmental elections in which limits already existed but reimbursement had not been implemented yet. The lack of effects of spending limits contrasts with recent papers finding substantial effects on electoral competition. This difference may come from the fact that the spending ceiling is less stringent and binding in the elections that we study than in other contexts, including the British elections to the House of Commons studied by Fouirnaies (2021), where limits have been tightened over time, or the local Brazilian elections studied by Avis et al. (2022), where ceilings are set based on the maximum spending in the previous race.

Second, unlike the large effects observed in departmental elections post 1995, we do not find any effect of the reimbursement of campaign expenditures in municipal elections. We attribute this difference to important differences in the voting rule used in these two types of elections: plurality voting in single-member constituencies versus a proportional list system. In municipal elections, campaign expenditures can be split across the mayoral candidate and the other members of their list, and the latter can also devote time campaigning on behalf of the list beyond just contributing money. Resources brought by fellow candidates may decrease the scope for public funding to make a difference. In addition, incumbents' ability to invite allies and rivals alike to join their list puts them in a more powerful position to withstand political parties pressuring them to stay out of the race.

In sum, our results suggest that the list format which characterizes proportional elections makes the reimbursement of campaign expenditures less impactful than in elections using single-candidate plurality voting. This insight could inform the design of future campaign finance reforms, in France and beyond. Naturally, further work is needed to verify its external validity.

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Appendix I: Departmental elections

A. Additional tables and figures

Table A1: Impact on outsider and insider candidates - Unconditional outcomes - Main sample of departmental elections

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
	Outsider candidates			Insider candidates		
	run	win	vote share, R1	run	win	vote share, R1
Treatment	-0.004	0.092**	0.018	-0.055**	-0.092**	-0.018
	(0.007)	(0.042)	(0.020)	(0.020)	(0.042)	(0.020)
Robust *p*-value	0.645	0.024	0.393	0.011	0.024	0.393
Observations	2,153	1,686	2,582	3,289	1,686	2,582
Polyn. order	1	1	1	1	1	1
Bandwidth	2,414	1,886	2,880	3,706	1,886	2,880
Mean, left of threshold	0.995	0.288	0.530	0.932	0.712	0.470

Notes: Standard errors are in parentheses. Robust *p*-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election *t*. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity.

Table A2: Average expenditures and contributions to ceiling ratios in districts between 9,000 and 11,000 inhabitants - Left versus right-wing candidates

	1992-1994 elections		1998-2001 elections	
	Left candidates	Right candidates	Left candidates	Right candidates
Expenditures	0.172	0.328	0.396	0.439
Personal contributions	0.032	0.139	0.310	0.344
Party contributions	0.087	0.056	0.035	0.014
Donations	0.062	0.145	0.032	0.077

Notes: We focus on districts close to the cutoff (between 9,000 and 11,000 inhabitants). Personal contributions, party contributions, and donations are the three largest sources of candidates' contributions. Other sources of contributions include natural contributions and other contributions such as revenue from investments or of a commercial nature. The sum of contributions does not always add up to total expenditures of candidates, as contributions need not be exhausted.

Table A3: Impact on the incumbent's probability of running, winning, and vote share - Main sample of departmental elections - Party candidate

	(1)	(2)	(3)
Outcome	Incumbent		
	run	win	vote share, R1
Panel A. Unconditional effects			
Treatment	-0.094**	-0.164***	-0.082***
	(0.046)	(0.056)	(0.027)
Robust *p*-value	0.036	0.003	0.002
Observations	1,509	1,053	1,209
Polyn. order	1	1	1
Bandwidth	2,530	1,763	2,032
Mean	0.762	0.666	0.366
Panel B. Conditional effects			
Upper bound		-0.215*	-0.108**
Boot. std error		(0.115)	(0.046)
Lower bound		-0.111	-0.050*
Boot. std error		(0.089)	(0.029)
Mean		0.839	0.471

Notes: The sample is restricted to elections where the incumbent is affiliated to a party. Panel A and Panel B show effects on unconditional outcomes and bounds of effects conditional on running, respectively. The notes for Panel A are as in Table A1. In Panel B, the mean, left of the threshold, indicates the value of the outcome for the candidates on the left of the threshold, conditional on running. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively, of the bootstrapped standard errors.

Table A4: Impact on the incumbent's probability of running, winning, and vote share - Main sample of departmental elections - Non-party candidate

	(1)	(2)	(3)
Outcome	Incumbent		
	run	win	vote share, R1
Panel A. Unconditional effects			
Treatment	-0.058	-0.049	-0.024
	(0.054)	(0.076)	(0.033)
Robust p-value	0.365	0.455	0.509
Observations	1,098	644	978
Polyn. order	1	1	1
Bandwidth	3,726	2,185	3,295
Mean	0.794	0.660	0.377
Panel B. Conditional effects			
Upper bound		-0.062	-0.030
Boot. std error		(0.108)	(0.050)
Lower bound		0.002	0.005
Boot. std error		(0.083)	(0.031)
Mean		0.878	0.482

Notes: The sample is restricted to elections where the incumbent is not affiliated to a party. Other notes as in Table A3.

Table A5: Impact on the main outcomes - 1992-1994 departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Incumbent		Challenger		Outsider		Victory
	run	win	run	win	run	win	R1
Treatment	-0.013	0.038	-0.045	-0.051*	-0.009	-0.050	-0.015
	(0.051)	(0.066)	(0.061)	(0.030)	(0.009)	(0.070)	(0.064)
Robust p-value	0.934	0.535	0.429	0.061	0.186	0.533	0.861
Observations	1,175	1,041	1,021	588	729	871	1,114
Polyn. order	1	1	1	1	1	1	1
Bandwidth	3,818	3,368	3,322	1,910	2,346	2,807	3,601
Mean left of threshold	0.800	0.619	0.309	0.050	1.000	0.345	0.356

Notes as in Table A1.

Table B1: Balance tests, sociodemographic characteristics - Main sample of departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Outcome	Men	Under 29	30-44	45-59	Over 60	Active population	Unemployed	Skilled jobs	Workers	Employees	Intermediary workers	Artisans	Agriculture
Treatment	-0.001	0.005	0.004**	-0.004	-0.007	-0.000	-0.002	0.002	-0.002	-0.003	0.002	0.004	-0.003
	(0.002)	(0.005)	(0.002)	(0.003)	(0.005)	(0.004)	(0.004)	(0.003)	(0.009)	(0.005)	(0.005)	(0.002)	(0.005)
Robust *p*-value	0.314	0.279	0.043	0.139	0.326	0.780	0.619	0.467	0.931	0.524	0.854	0.102	0.524
Observations	874	2,224	1,998	1,552	2,017	1,453	1,864	2,173	1,852	1,673	2,062	1,459	2,061
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,814	3,410	3,059	2,365	3,081	2,229	2,819	s3,341	2,812	2,550	3,171	2,244	3,168
Mean, left of threshold	0.493	0.356	0.208	0.186	0.252	0.436	0.116	0.066	0.335	0.278	0.183	0.074	0.064

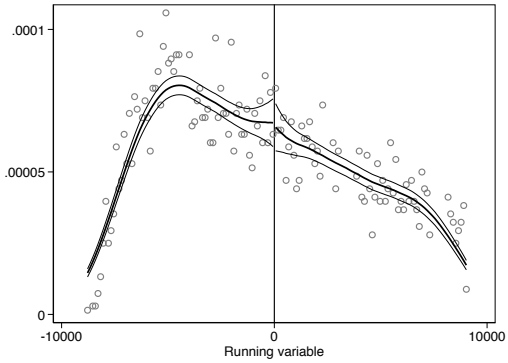
Notes: Standard errors are in parentheses. Robust *p*-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election *t*. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. All outcomes refer to shares of the whole population. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity. We exclude the 2008 elections since in most districts, the running variable is the same as in 2001 (the same major census was in place for both elections).

Table B2: Balance tests, sociodemographic characteristics - All departmental elections, including non-linkable districts

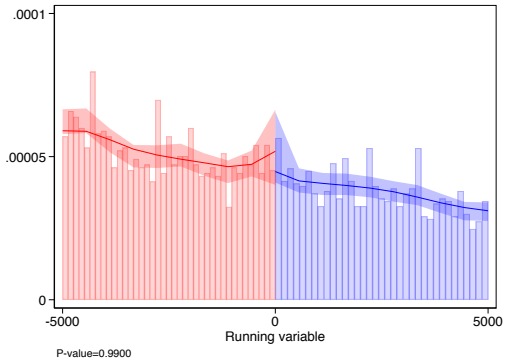
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Outcome	Men	Under 29	30-44	45-59	Over 60	Active population	Unemployed	Skilled jobs	Workers	Employees	Intermediary workers	Artisans	Agriculture
Treatment	-0.001	0.005	0.004**	-0.004	-0.007	-0.000	-0.002	0.002	-0.001	-0.003	0.002	0.004	-0.003
	(0.002)	(0.005)	(0.002)	(0.003)	(0.005)	(0.004)	(0.004)	(0.003)	(0.009)	(0.005)	(0.005)	(0.002)	(0.005)
Robust *p*-value	0.314	0.274	0.042	0.145	0.319	0.779	0.598	0.485	0.949	0.510	0.887	0.103	0.534
Observations	882	2,245	1,990	1,564	2,020	1,466	1,870	2,188	1,865	1,672	2,044	1,472	2,067
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,824	3,423	3,035	2,378	3,081	2,244	2,826	3,357	2,830	2,547	3,137	2,256	3,173
Mean, left of threshold	0.493	0.356	0.208	0.186	0.252	0.436	0.116	0.066	0.335	0.278	0.183	0.074	0.064

Notes as in Table B1.

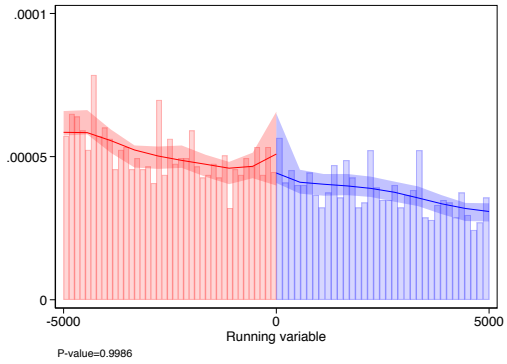
Figure B1: McCrary (2008) and Cattaneo et al. (2018) density tests



McCrary test - All departmental elections, including non-linkable districts



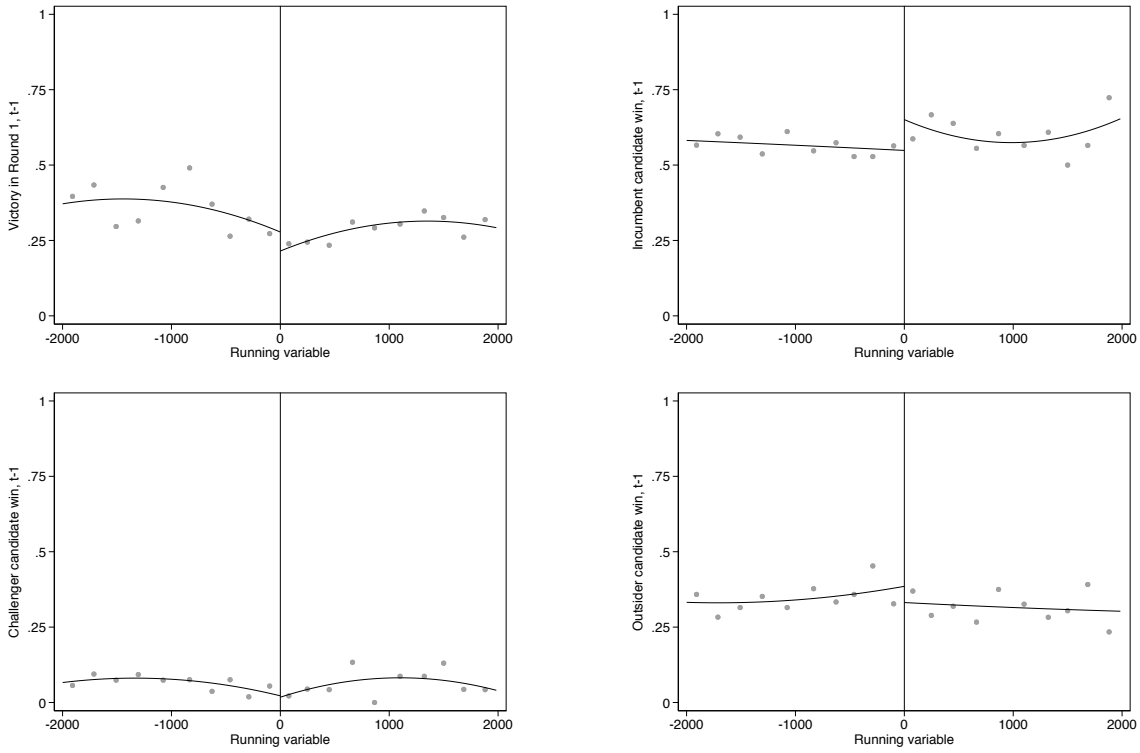
RD Density test- Main sample of departmental elections



RD Density test - All departmental elections, including non-linkable districts

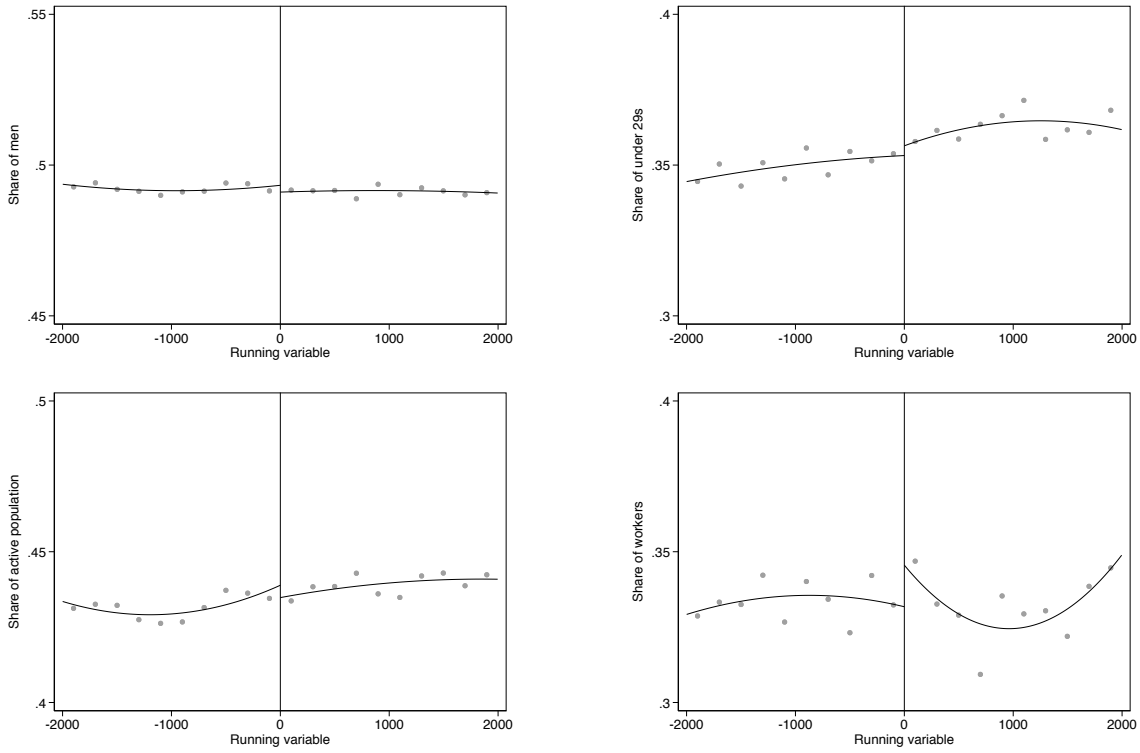
Notes: We test for a jump at the threshold in the density of the running variable (the district population centered around 9,000 inhabitants), using McCrary (2008)'s method in the top panel. The solid line represents the density of the running variable, while the thin lines represent the confidence intervals. The bottom two figures similarly test for a jump at the threshold in the density of the running variable using the method developed by Cattaneo et al. (2018). The solid line represents the density of the running variable, while the shaded bands represent the 95 percent confidence intervals. The graphs also report the p-value of the bias-corrected density test. To facilitate visualization, the graph is truncated at 5,000 inhabitants around the cutoff. We exclude the 2008 elections since in most districts, the running variable is the same as in 2001 (the same major census was in place for both elections).

Figure B2: Placebo tests, main outcomes defined in *t-1* - Main sample of departmental elections



Notes: Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the district population centered around 9,000 inhabitants) is split into quantile-spaced bins. The continuous lines represent a quadratic fit. To facilitate visualization, the graph is truncated at 2,000 inhabitants around the cutoff. We exclude the 1998 (resp. 2008) elections since in most districts, the running variable is the same as in 1992 (resp. 2001).

Figure B3: Balance tests, sociodemographic characteristics - Main sample of departmental elections



Notes: Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the district population centered around 9,000 inhabitants) is split into evenly-spaced bins. The continuous lines represent a quadratic fit. To facilitate visualization, the graph is truncated at 2,000 inhabitants around the cutoff. We exclude the 2008 elections since in most districts, the running variable is the same as in 2001 (the same major census was in place for both elections).

C. Robustness

Table C1: Impact on competition - Main sample of departmental elections excluding 2008

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
	Number of			Turnout	ENC	Victory
	Candidates	Outsiders	Insiders	r1	r1	in first round
Treatment	0.103	0.025	0.012	0.014	0.136	-0.103**
	(0.155)	(0.156)	(0.079)	(0.010)	(0.118)	(0.049)
Robust *p*-value	0.356	0.747	0.666	0.117	0.177	0.033
Observations	1,345	1,637	2,099	1,802	1,397	1,737
Polyn. order	1	1	1	1	1	1
Bandwidth	1,910	2,314	2,973	2,545	1,987	2,457
Mean, left of threshold	5.278	3.787	1.499	0.639	3.351	0.312

Notes: Standard errors are in parentheses. Robust *p*-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election *t*. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity.

Table C2: Impact on competition - All departmental elections, including non-linkable districts

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
	Number of			Turnout	ENC	Victory
	Candidates	Outsiders	Insiders	r1	r1	in first round
Treatment	0.044	0.010	0.028	0.009	0.073	-0.111***
	(0.118)	(0.119)	(0.065)	(0.009)	(0.085)	(0.044)
Robust *p*-value	0.524	0.855	0.471	0.263	0.279	0.010
Observations	2,460	2,663	2,407	2,336	2,768	2,222
Polyn. order	1	1	1	1	1	1
Bandwidth	2,736	2,993	2,702	2,604	3,093	2,473
Mean, left of threshold	5.055	3.597	1.461	0.656	3.251	0.355

Notes as in Table C1.

Table C3: Impact on winner identity - Sample of departmental elections excluding 2008

	(1)	(2)	(3)	(4)
Outcomes	Outsider win	Insider win	Incumbent win	Challenger win
Treatment	0.022	-0.022	-0.100*	0.078**
	(0.048)	(0.048)	(0.055)	(0.025)
Robust *p*-value	0.600	0.600	0.065	0.002
Observations	1,769	1,769	1,331	1,299
Polyn. order	1	1	1	1
Bandwidth	2,504	2,504	1,886	1,838
Mean, left of threshold	0.337	0.663	0.635	0.007

Notes as in Table C1.

Table C4: Impact on running, winning, and vote shares - Sample of departmental elections excluding 2008

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Incumbent			Challenger		
	run	win	vote share, R1	run	win	vote share, R1
Panel A. Unconditional effects						
Treatment	-0.073	-0.100*	-0.055**	0.112**	0.078***	0.044***
	(0.043)	(0.055)	(0.025)	(0.050)	(0.025)	(0.015)
Robust *p*-value	0.102	0.065	0.024	0.016	0.002	0.003
Observations	1,799	1,331	1,381	1,322	1,299	1,415
Polyn. order	1	1	1	1	1	1
Bandwidth	2,542	1,886	1,969	1,877	1,838	2,012
Mean	0.745	0.635	0.346	0.180	0.007	0.043
Panel B. Conditional effects						
Upper bound		-0.134	-0.074*		0.265***	0.151***
Boot. std error		(0.102)	(0.041)		(0.084)	(0.043)
Lower bound		-0.053	-0.029		0.152**	0.036*
Boot. std error		(0.077)	(0.024)		(0.074)	(0.021)
Mean		0.835	0.459		0.105	0.253

Notes: Panel A and Panel B show effects on unconditional outcomes and bounds of effects conditional on running, respectively. The notes for Panel A are as in Table C1. In Panel B, the mean, left of the threshold, indicates the value of the outcome for the candidates on the left of the threshold, conditional on running. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively, of the bootstrapped standard errors.

Table C5: Impact on winning orientation, polarization, and winner's representativeness - Sample of departmental elections excluding 2008

Outcome	(1) Incumbent or. win	(2) Far-left win	(3) Left win	(4) Center win	(5) Right win	(6) Far-right win	(7) Non-classified win	(8) Polarization	(9) Vote share . winner or.	(10) Top or. win
Treatment	-0.039	-0.004	0.081	-0.025	-0.056	-0.000	0.010	0.025	-0.016	-0.039
	(0.042)	(0.004)	(0.054)	(0.018)	(0.049)	(0.000)	(0.010)	(0.087)	(0.016)	(0.034)
Robust *p*-value	0.308	0.159	0.110	0.157	0.241	0.294	0.322	0.778	0.312	0.246
Observations	1,449	1,574	1,933	2,056	2,341	1,272	1,734	1,800	1,624	1,556
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	2,061	2,237	2,732	2,898	3,333	1,804	2,454	2,943	2,298	2,214
Mean, left of threshold	0.840	0.004	0.480	0.048	0.467	0,000	0.004	4.906	0.581	0.918

Notes as in Table C1. "Or." stands for "orientation."

Table C6: Impact on winning orientation, polarization, and winner's representativeness - All departmental elections, including non-linkable districts

Outcome	(1) Incumbent or. win	(2) Far-left win	(3) Left win	(4) Center win	(5) Right win	(6) Far-right win	(7) Non-classified win	(8) Polarization	(9) Vote share . winner or.	(10) Top or. win
Treatment	-0.082**	-0.003	0.084*	-0.021	-0.055	-0.001	0.014	-0.076	-0.003	-0.044
	(0.037)	(0.003)	(0.047)	(0.014)	(0.041)	(0.001)	(0.009)	(0.083)	(0.014)	(0.029)
Robust *p*-value	0.024	0.266	0.063	0.144	0.189	0.360	0.152	0.368	0.819	0.118
Observations	1,534	2,236	2,559	2,600	3,373	1,730	2,116	2,182	2,320	1,835
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,709	2,488	2,822	2,881	3,768	1,939	2,350	2,778	2,578	2,046
Mean, left of threshold	0.862	0.003	0.475	0.043	0.477	0,000	0.002	4.868	0.583	0.924

Notes as in Table C1. "Or." stands for "orientation."

Table C7: Placebo discontinuities - Incumbent candidates - Main sample of departmental elections

Panel A. Run

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	-0.022	0.033	0.026	-0.003	0.005	0.052	0.057*	0.009	-0.061	-0.074**
	(0.043)	(0.039)	(0.037)	(0.037)	(0.032)	(0.032)	(0.034)	(0.034)	(0.034)	(0.030)
Robust *p*-value	0.619	0.496	0.683	0.740	0.847	0.109	0.092	0.717	0.106	0.024
Observations	1,957	2,176	2,274	1,850	2,819	2,851	2,530	2,457	2,433	3,202
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,783	2,071	2,216	1,879	2,909	3,549	3,262	3,227	3,343	4,496
Mean, left of threshold	0.714	0.705	0.725	0.760	0.754	0.713	0.726	0.755	0.780	0.756

Panel B. Win

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	-0.032	0.015	-0.011	-0.008	0.005	0.049	0.013	0.014	-0.043	-0.045
	(0.042)	(0.043)	(0.039)	(0.045)	(0.044)	(0.036)	(0.032)	(0.038)	(0.034)	(0.038)
Robust *p*-value	0.601	0.540	0.792	0.744	0.919	0.193	0.680	0.619	0.279	0.321
Observations	2,202	2,004	2,286	1,662	1,907	2,635	3,415	2,679	3,484	2,769
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	2,015	1,911	2,232	1,674	1,952	3,298	4,380	3,513	4,767	3,922
Mean, left of threshold	0.629	0.590	0.591	0.599	0.593	0.578	0.596	0.591	0.611	0.598

Notes as in Table C1.

Table C8: Placebo discontinuities - Challenger candidates - Main sample of departmental elections

Panel A. Run

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	0.054	-0.029	-0.024	0.030	-0.049	-0.050	0.073*	0.079*	-0.064	-0.040
	(0.045)	(0.041)	(0.039)	(0.038)	(0.036)	(0.040)	(0.033)	(0.037)	(0.045)	(0.039)
Robust *p*-value	0.211	0.382	0.499	0.380	0.281	0.160	0.050	0.057	0.119	0.250
Observations	1,373	1,990	1,995	1,901	2,630	1,735	2,760	2,194	1,562	1,911
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,273	1,896	1,979	1,938	2,743	2,106	3,559	2,916	2,180	2,745
Mean, left of threshold	0.194	0.275	0.274	0.255	0.283	0.219	0.180	0.202	0.291	0.258

Panel B. Win

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	0.034	-0.024	-0.003	0.016	-0.015	-0.035*	0.006	0.012	0.014	-0.015
	(0.022)	(0.026)	(0.021)	(0.022)	(0.021)	(0.022)	(0.016)	(0.018)	(0.020)	(0.021)
Robust *p*-value	0.141	0.293	0.921	0.359	0.637	0.094	0.754	0.495	0.461	0.483
Observations	1,598	1,576	2,234	1,565	2,229	1,746	2,963	2,765	2,804	2,568
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,479	1,503	2,176	1,592	2,301	2,122	3,801	3,673	3,819	3,643
Mean, left of threshold	0.028	0.083	0.070	0.051	0.067	0.076	0.054	0.055	0.056	0.068

Notes as in Table C1.

Table C9: Placebo discontinuities - Outsider candidates - Main sample of departmental elections

Panel A. Run

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	0.006	-0.005	-0.003	-0.009	-0.012	0.002	-0.011	0.007	-0.007	-0.003
	(0.020)	(0.015)	(0.014)	(0.013)	(0.013)	(0.006)	(0.007)	(0.008)	(0.006)	(0.006)
Robust *p*-value	0.682	0.815	0.845	0.379	0.307	0.476	0.190	0.264	0.382	0.811
Observations	1,432	1,666	1,592	1,709	2,130	2,335	3,244	2,012	3,377	3,330
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,327	1,593	1,588	1,744	2,194	2,882	4,162	2,691	4,621	4,718
Mean, left of threshold	0.964	0.977	0.978	0.984	0.990	0.995	0.999	0.991	0.996	0.993

Panel B. Win

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	0.010	0.005	0.007	0.007	0.010	-0.029	-0.027	-0.032	0.030	0.073
	(0.041)	(0.043)	(0.040)	(0.042)	(0.040)	(0.030)	(0.032)	(0.036)	(0.031)	(0.033)
Robust *p*-value	0.887	0.851	0.823	0.787	0.865	0.368	0.378	0.308	0.448	0.046
Observations	2,073	1,903	2,270	1,766	1,995	3,447	3,020	2,585	3,795	3,491
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,893	1,811	2,208	1,788	2,051	4,251	3,861	3,397	5,184	4,937
Mean, left of threshold	0.323	0.324	0.331	0.321	0.321	0.339	0.335	0.339	0.317	0.310

Notes as in Table C1.

Table C10: Placebo discontinuities - Victory in the first round - Main sample of departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Discontinuity	5,500	6,000	6,500	7,000	7,500	10,500	11,000	11,500	12,000	12,500
Treatment	-0.094*	-0.074	-0.001	0.020	0.044	-0.016	-0.017	-0.021	0.005	-0.051
	(0.054)	(0.052)	(0.051)	(0.047)	(0.051)	(0.052)	(0.048)	(0.044)	(0.037)	(0.039)
Robust *p*-value	0.084	0.211	0.924	0.776	0.330	0.556	0.565	0.549	0.865	0.225
Observations	1,834	1,994	1,943	2,170	1,964	1,664	1,894	2,212	3,170	2,951
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,670	1,897	1,917	2,172	2,009	2,011	2,400	2,941	4,333	4,154
Mean, left of threshold	0.566	0.507	0.431	0.397	0.404	0.358	0.348	0.334	0.294	0.305

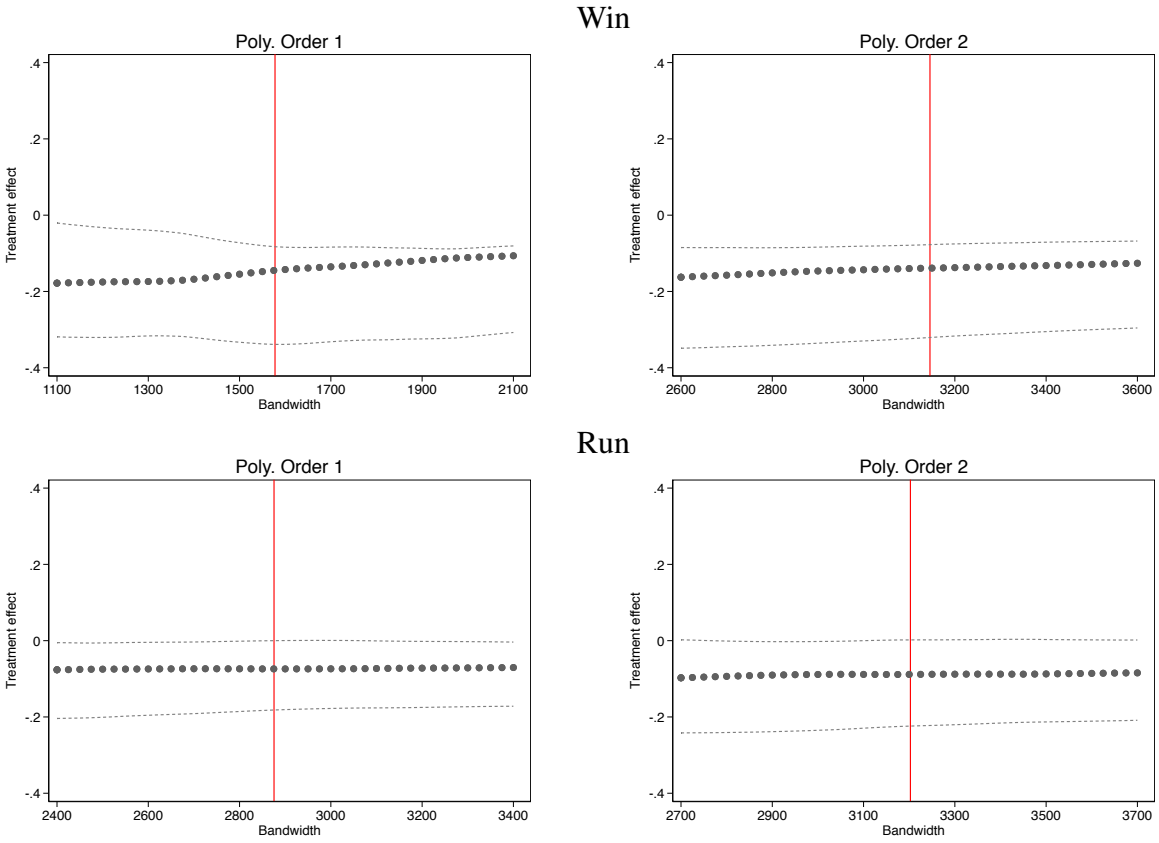
Notes as in Table C1.

Table C11: Impact on the main outcomes - Quadratic fit - Main sample of departmental elections

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent		Challenger		Outsider		Victory
	run	win	run	win	run	win	1st round
Treatment	-0.088*	-0.139***	0.106**	0.057***	0.000	0.102**	-0.125**
	(0.044)	(0.048)	(0.045)	(0.021)	(0.009)	(0.047)	(0.051)
Robust *p*-value	0.079	0.003	0.021	0.008	0.835	0.027	0.015
Observations	2,848	2,789	2,808	3,483	2,576	2,860	3,375
Polyn. order	2	2	2	2	2	2	2
Bandwidth	3,203	3,146	3,164	3,947	2,871	3,231	3,807
Mean, left of threshold	0.785	0.674	0.169	0.016	0.993	0.281	0.353

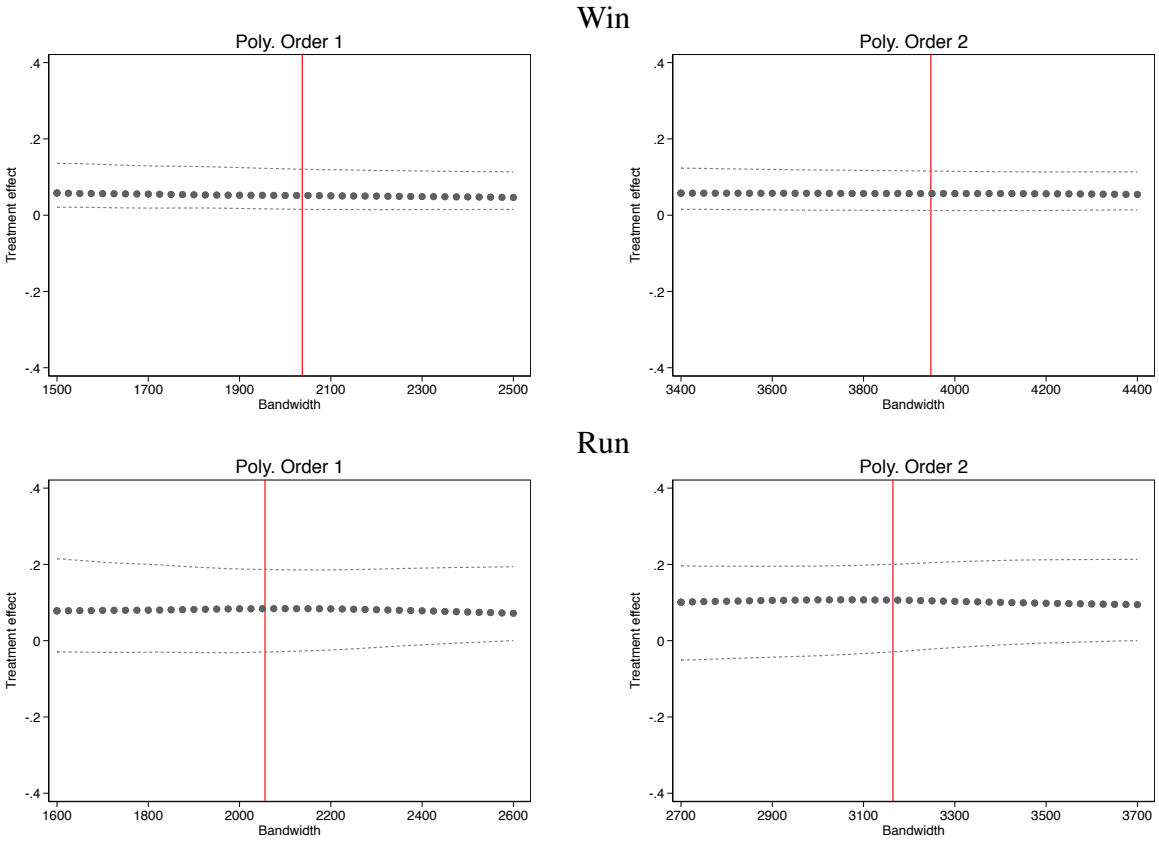
Notes as in Table C1, except for the fact that the polynomial order is two in all columns.

Figure C1: Sensitivity to bandwidth - Incumbent candidate - Main sample of departmental elections



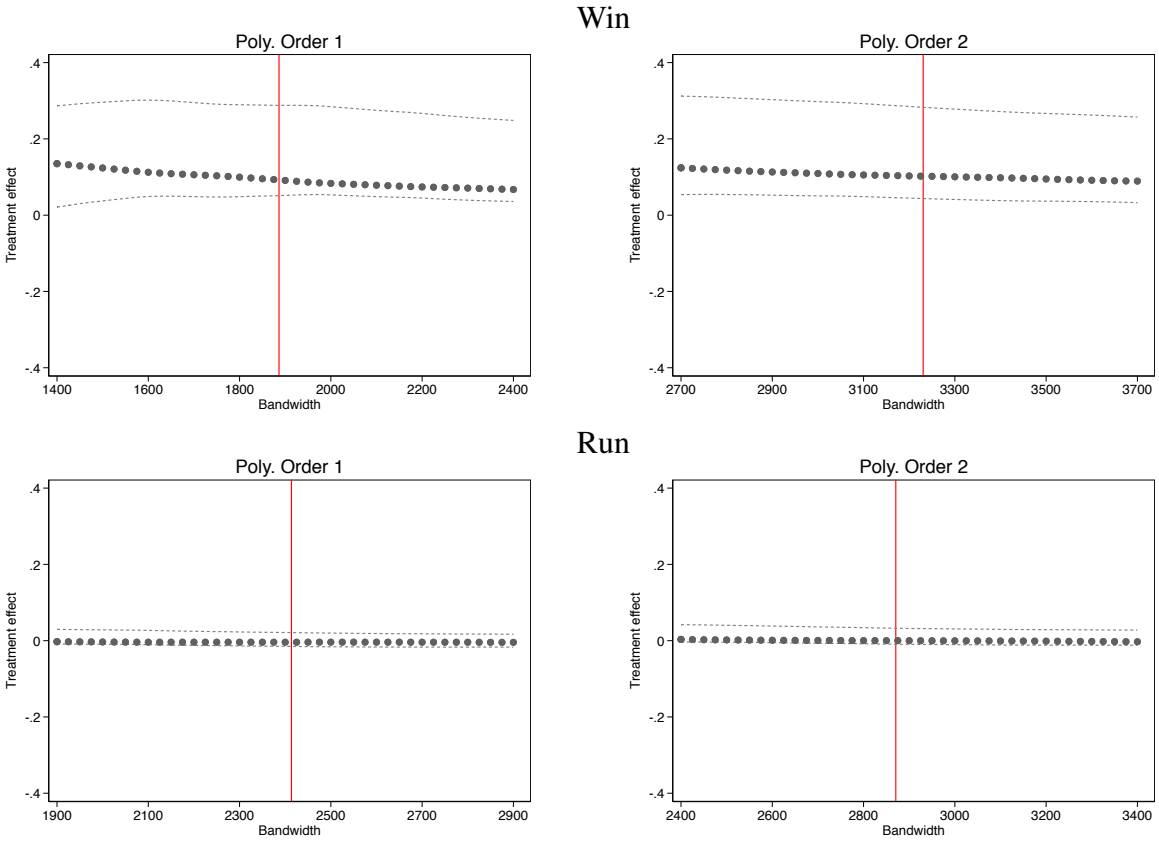
Notes: We show the sensitivity of the effect on the incumbent candidate to bandwidth choice, either using a linear (left-hand side) or quadratic specification (right-hand side). The vertical red line represents the value of the MSERD optimal bandwidth. The dots represent the estimated treatment effect using different bandwidths, while the dotted lines represent the 95 percent confidence intervals. We report all estimates for values of the bandwidth from -500 to +500 inhabitants, in steps of 25 inhabitants.

Figure C2: Sensitivity to bandwidth - Challenger candidate - Main sample of departmental elections



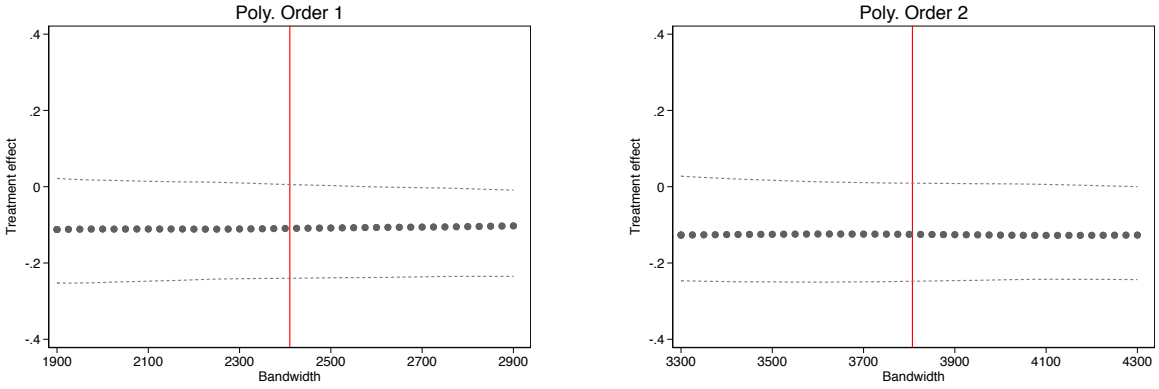
Notes: We show the sensitivity of the effect on the challenger candidate to bandwidth choice, either using a linear (left-hand side) or quadratic specification (right-hand side). The vertical red line represents the value of the MSERD optimal bandwidth. The dots represent the estimated treatment effect using different bandwidths, while the dotted lines represent the 95 percent confidence intervals. We report all estimates for values of the bandwidth from -500 to +500 inhabitants, in steps of 25 inhabitants.

Figure C3: Sensitivity to bandwidth - Outsider candidate - Main sample of departmental elections



Notes: We show the sensitivity of the effect on outsider candidates to bandwidth choice, either using a linear (left-hand side) or quadratic specification (right-hand side). The vertical red line represents the value of the MSERD optimal bandwidth. The dots represent the estimated treatment effect using different bandwidths, while the dotted lines represent the 95 percent confidence intervals. We report all estimates for values of the bandwidth from -500 to +500 inhabitants, in steps of 25 inhabitants.

Figure C4: Sensitivity to bandwidth - Victory in the first round - Main sample of departmental elections



Notes: We show the sensitivity of the effect on the probability of a victory in the first round to bandwidth choice, either using a linear (left-hand side) or quadratic specification (right-hand side). The vertical red line represents the value of the MSERD optimal bandwidth. The dots represent the estimated treatment effect using different bandwidths while the dotted lines represent the 95 percent confidence intervals. We report all estimates for values of the bandwidth from -500 to +500 inhabitants, in steps of 25 inhabitants.

Appendix II: Municipal elections

D. Validity

Table D1: Balance tests, sociodemographic characteristics - Main sample of municipal elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Outcomes	Men	Under 29	30-44	45-59	Over 60	Active population	Unemployed	Skilled jobs	Workers	Employees	Intermediary workers	Artisans	Agriculture
Treatment	0.004	0.000	-0.001	0.002	-0.003	-0.004	0.004	0.001	-0.030	0.013*	0.001	0.000	0.002
	(0.003)	(0.010)	(0.004)	(0.004)	(0.010)	(0.007)	(0.010)	(0.012)	(0.020)	(0.008)	(0.008)	(0.004)	(0.002)
Robust p-value	0.369	0.857	0.705	0.563	0.940	0.524	0.646	0.815	0.102	0.085	0.818	0.703	0.334
Observations	879	792	972	947	1,056	1,072	970	762	540	761	806	662	1,164
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,818	1,680	2,001	1,953	2,139	2,166	1,994	1,608	1,160	1,605	1,697	1,399	2,346
Mean, left of threshold	0.481	0.378	0.204	0.196	0.223	0.457	0.135	0.121	0.281	0.303	0.241	0.061	0.006

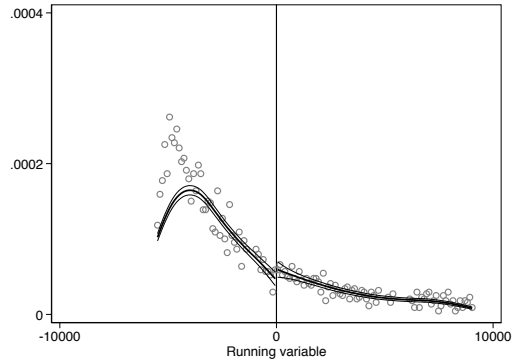
Notes: Standard errors are in parentheses. Robust p-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election t. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The sample includes the 2001 and 2014 elections. All outcomes refer to shares of the whole population. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity.

Table D2: Balance tests, sociodemographic characteristics - All municipal elections, including non-linkable districts

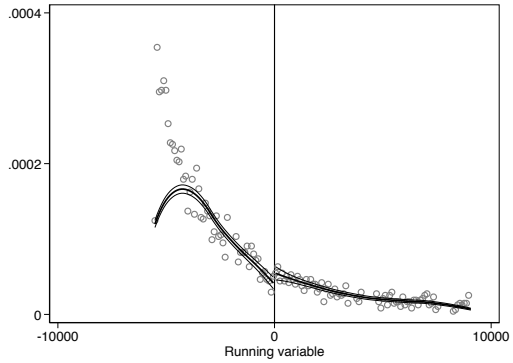
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Outcomes	Men	Under 29	30-44	45-59	Over 60	Active population	Unemployed	Skilled jobs	Workers	Employees	Intermediary workers	Artisans	Agriculture
Treatment	0.004	0.003	-0.000	0.001	-0.006	-0.002	0.001	0.005	-0.031*	0.010	0.003	-0.001	0.002
	(0.003)	(0.009)	(0.004)	(0.004)	(0.010)	(0.006)	(0.009)	(0.012)	(0.019)	(0.007)	(0.008)	(0.004)	(0.002)
Robust *p*-value	0.304	0.893	0.945	0.747	0.708	0.762	0.829	0.540	0.081	0.140	0.621	0.995	0.439
Observations	942	869	1,360	959	1,048	1,492	1,131	783	584	926	887	946	1,281
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,805	1,698	2,516	1,838	2,000	2,714	2,133	1,537	1,185	1,780	1,728	1,814	2,387
Mean, left of threshold	0.482	0.379	0.204	0.196	0.222	0.456	0.137	0.116	0.282	0.304	0.240	0.060	0.006

Notes as in Table D1.

Figure D1: McCrary (2008) density test



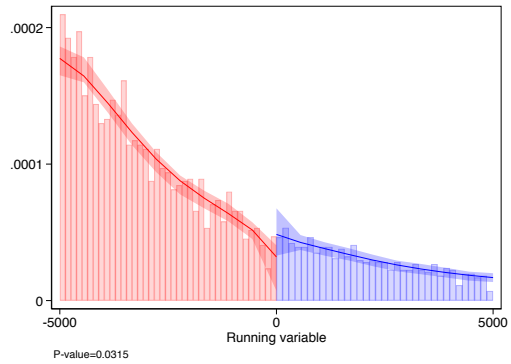
McCrary test - Main sample



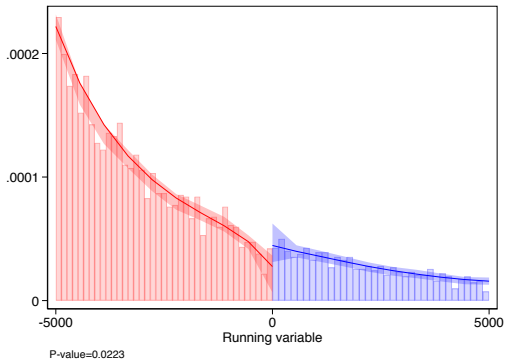
McCrary test - Including non-linkable districts

Notes: We test for a jump at the threshold in the density of the running variable (the district population centered around 9,000 inhabitants), using McCrary (2008)'s method. The solid line represents the density of the running variable, while the thin lines represent the confidence intervals. We exclude the 2008 elections since in most districts, the running variable is the same as in 2001 (the same major census was in place for both elections).

Figure D2: Cattaneo et al. (2018) density tests



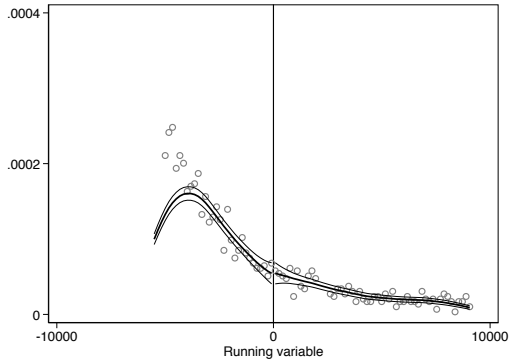
RD Density test - Main sample



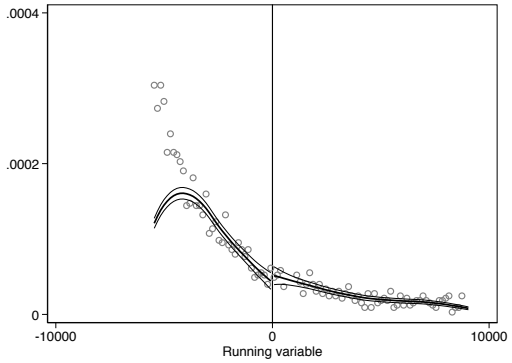
RD Density test - Including non-linkable districts

Notes: We test for a jump at the threshold in the density of the running variable (the district population centered around 9,000 inhabitants), using McCrary (2008)'s method in the top panel. The solid line represents the density of the running variable, while the thin lines represent the confidence intervals. The bottom two figures similarly test for a jump at the threshold in the density of the running variable using the method developed by Cattaneo et al. (2018). The solid line represents the density of the running variable, while the shaded bands represent the 95 percent confidence intervals. The graphs also report the p-value of the bias-corrected density test. To facilitate visualization, the graph is truncated at 5,000 inhabitants around the cutoff. We exclude the 2008 elections since in most districts, the running variable is the same as in 2001 (the same major census was in place for both elections).

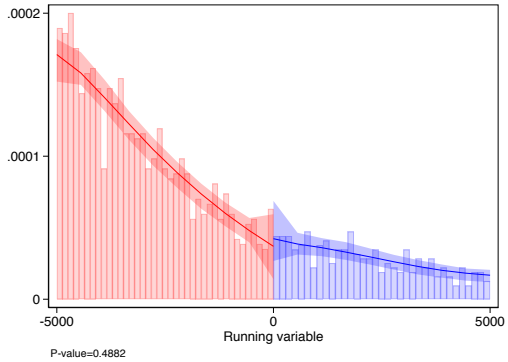
Figure D3: McCrary (2008) and Cattaneo et al. (2018) density tests - 2001 elections



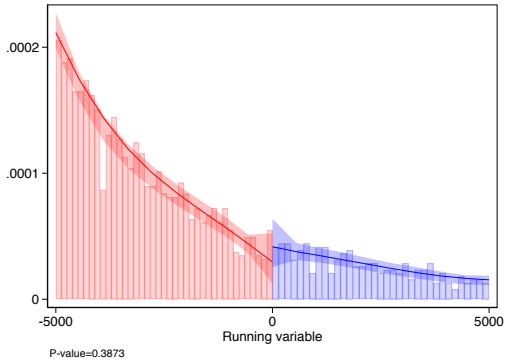
McCrary test - Main sample



McCrary test - Including non-linkable districts



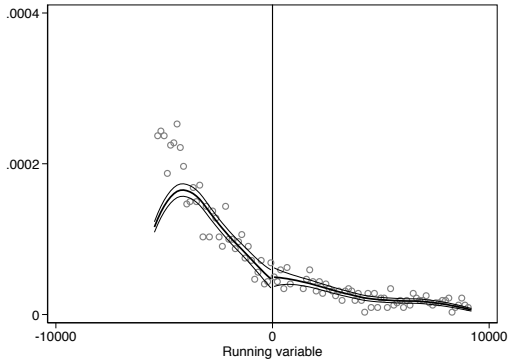
RD Density test - Main sample



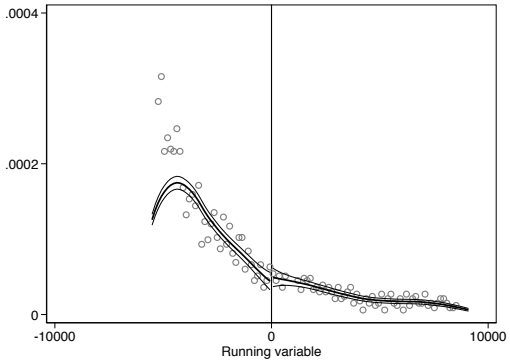
RD Density test - Including non-linkable districts

Notes as in Figure D2.

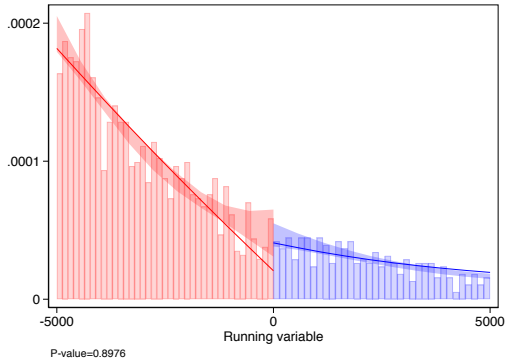
Figure D4: McCrary (2008) and Cattaneo et al. (2018) density tests - 2008 elections



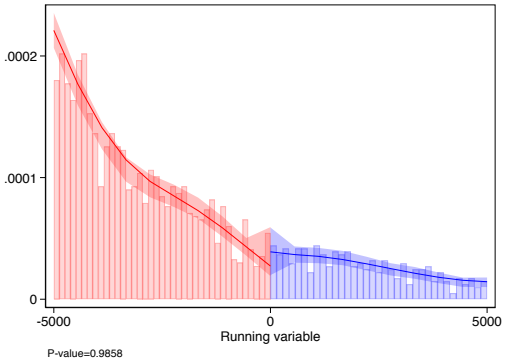
McCrary test - Main sample



McCrary test - Including non-linkable districts



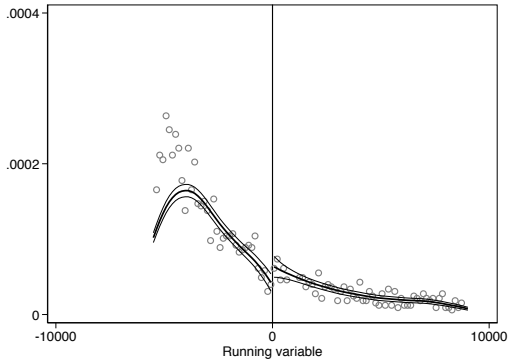
RD Density test - Main sample



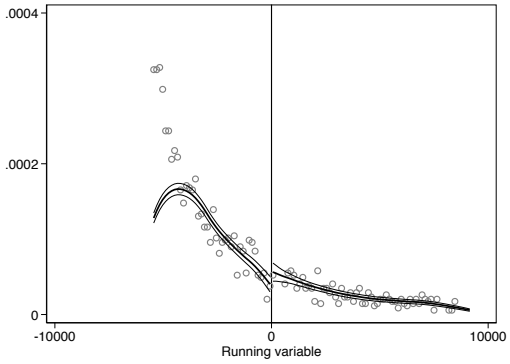
RD Density test - Including non-linkable districts

Notes as in Figure D2.

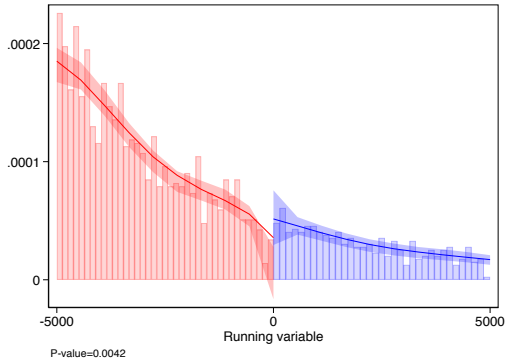
Figure D5: McCrary (2008) and Cattaneo et al. (2018) density tests - 2014 elections



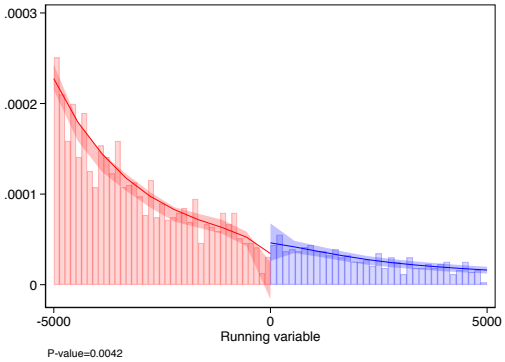
McCrary test - Main sample



McCrary test - Including non-linkable districts



RD Density test - Main sample



RD Density test - Including non-linkable districts

Notes as in Figure D2.

E. Robustness

Table E1: Impact on competition and winner identity - Main sample of municipal elections - 2001

Panel A. Competition

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	-0.026	0.090	-0.125	0.016	0.049	-0.078
	(0.208)	(0.217)	(0.142)	(0.014)	(0.165)	(0.113)
Robust *p*-value	0.901	0.559	0.346	0.266	0.759	0.469
Observations	590	673	509	342	533	362
Polyn. order	1	1	1	1	1	1
Bandwidth	2,622	2,928	2,332	1,664	2,405	1,741
Mean, left of threshold	2.772	1.631	1.138	0.641	2.355	0.716

Panel B. Winner identity

	(1)	(2)	(3)	(4)
Outcome	Outsider win	Insider win	Incumbent win	Challenger win
Treatment	0.148	-0.148	-0.158	0.025
	(0.095)	(0.095)	(0.114)	(0.048)
Robust *p*-value	0.206	0.206	0.277	0.586
Observations	574	574	401	379
Polyn. order	1	1	1	1
Bandwidth	2,574	2,574	1,910	1,849
Mean, left of threshold	0.290	0.710	0.697	0.020

Notes: Standard errors are in parentheses. Robust *p*-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the district has a population above 9,000 inhabitants in election *t*. Separate polynomials are fitted on each side of the threshold. The polynomial order is one in all columns and the bandwidths are derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity.

Table E2: Impact on competition and winner identity - Main sample of municipal elections - 2008

Panel A. Competition

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	-0.093	-0.043	-0.052	0.001	-0.042	0.040
	(0.216)	(0.201)	(0.134)	(0.012)	(0.169)	(0.103)
Robust *p*-value	0.782	0.882	0.901	0.862	0.941	0.855
Observations	427	498	534	497	407	512
Polyn. order	1	1	1	1	1	1
Bandwidth	1,708	1,936	2,035	1,928	1,639	1,985
Mean, left of threshold	2.827	1.728	1.110	0.642	2.402	0.554

Panel B. Winner identity

	(1)	(2)	(3)	(4)
Outcome	Outsider win	Insider win	Incumbent win	Challenger win
Treatment	-0.129	0.129	0.005	0.099
	(0.091)	(0.091)	(0.108)	(0.062)
Robust *p*-value	0.185	0.185	0.895	0.114
Observations	482	482	411	590
Polyn. order	1	1	1	1
Bandwidth	1883	1883	1657	2304
Mean, left of threshold	0.421	0.579	0.522	0.0569

Notes as in Table E1.

Table E3: Impact on competition and winner identity - Main sample of municipal elections - 2014

Panel A. Competition

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	-0.066	-0.171	0.119	-0.001	0.016	0.011
	(0.212)	(0.214)	(0.159)	(0.013)	(0.174)	(0.095)
Robust *p*-value	0.727	0.319	0.458	0.979	0.949	0.769
Observations	654	712	577	509	563	683
Polyn. order	1	1	1	1	1	1
Bandwidth	2,269	2,484	2,036	1,811	1,993	2,378
Mean, left of threshold	3.173	2.112	1.062	0.626	2.534	0.557

Panel B. Winner identity

	(1)	(2)	(3)	(4)
Outcome	Outsider win	Insider win	Incumbent win	Challenger win
Treatment	-0.046	0.046	0.049	-0.027
	(0.110)	(0.110)	(0.097)	(0.063)
Robust *p*-value	0.640	0.640	0.462	0.760
Observations	485	485	592	501
Polyn. order	1	1	1	1
Bandwidth	1,757	1,757	2,092	1,869
Mean, left of threshold	0.415	0.585	0.482	0.110

Notes as in Table E1.

Table E4: Impact on competition - All municipal elections including non-linkable districts

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	0.030	-0.034	-0.017	-0.001	0.090	-0.038
	(0.137)	(0.131)	(0.069)	(0.008)	(0.101)	(0.056)
Robust *p*-value	0.807	0.763	0.911	0.969	0.394	0.454
Observations	1,429	1,433	2,258	1,562	1,432	1,394
Polyn. order	1	1	1	1	1	1
Bandwidth	1,801	1,913	2,803	1,956	1,807	1,767
Mean, left of threshold	2.918	1.816	1.106	0.637	2.431	0.604

Notes as in Table E1.

Table E5: Impact on competition - All municipal elections, including non-linkable districts - 2001

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	0.049	0.090	-0.125	0.007	0.134	-0.122
	(0.183)	(0.217)	(0.142)	(0.012)	(0.151)	(0.101)
Robust *p*-value	0.647	0.559	0.346	0.526	0.303	0.211
Observations	760	673	509	460	625	442
Polyn. order	1	1	1	1	1	1
Bandwidth	2,874	2,928	2,332	1,881	2,446	1,817
Mean, left of threshold	2.786	1.631	1.138	0.640	2.381	0.695

Notes as in Table E1.

Table E6: Impact on competition - All municipal elections, including non-linkable districts - 2008

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	0.046	-0.043	-0.052	-0.003	0.046	-0.007
	(0.242)	(0.201)	(0.134)	(0.012)	(0.181)	(0.106)
Robust *p*-value	0.764	0.882	0.901	0.939	0.699	0.816
Observations	425	498	534	535	411	497
Polyn. order	1	1	1	1	1	1
Bandwidth	1,663	1,936	2,035	2,002	1,601	1,894
Mean, left of threshold	2.823	1.728	1.110	0.643	2.400	0.553

Notes as in Table E1.

Table E7: Impact on competition - All municipal elections, including non-linkable districts - 2014

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Nr candidates	Nr outsiders	Nr insiders	Turnout	ENC	Victory in r1
Treatment	-0.049	-0.171	0.119	-0.003	0.041	-0.000
	(0.203)	(0.214)	(0.159)	(0.012)	(0.164)	(0.093)
Robust *p*-value	0.722	0.319	0.458	0.718	0.909	0.846
Observations	710	712	577	584	629	728
Polyn. order	1	1	1	1	1	1
Bandwidth	2,413	2,484	2,036	2,014	2,142	2,467
Mean, left of threshold	3.166	2.112	1.062	0.628	2.522	0.566

Notes as in Table E1.

Table E8: Impact on running - Main sample of municipal elections - 2001

	(1)	(2)	(3)
Outcome	Incumbent	Challenger	Outsider
Treatment	-0.105	-0.173	0.074
	(0.107)	(0.116)	(0.078)
Robust *p*-value	0.464	0.121	0.297
Observations	360	298	405
Polyn. order	1	1	1
Bandwidth	1,734	1,501	1,918
Mean, left of threshold	0.761	0.315	0.824

Notes as in Table E1.

Table E9: Impact on running - Main sample of municipal elections - 2008

	(1)	(2)	(3)
Outcome	Incumbent	Challenger	Outsider
Treatment	-0.050	0.001	-0.010
	(0.094)	(0.104)	(0.047)
Robust *p*-value	0.563	0.888	0.989
Observations	441	461	685
Polyn. order	1	1	1
Bandwidth	1,744	1,897	2,530
Mean, left of threshold	0.717	0.280	0.944

Notes as in Table E1.

Table E10: Impact on running - Main sample of municipal elections - 2014

	(1)	(2)	(3)
Outcome	Incumbent	Challenger	Outsider
Treatment	0.073	0.091	-0.080
	(0.092)	(0.090)	(0.055)
Robust *p*-value	0.428	0.331	0.134
Observations	570	622	527
Polyn. order	1	1	1
Bandwidth	2,023	2,293	1,876
Mean, left of threshold	0.692	0.234	0.962

Notes as in Table E1.