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SPENDING LIMITS, PUBLIC FUNDING, AND ELECTION OUTCOMES

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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. 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 of results and winners' representativeness and quality 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 (Prat, 2002). 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 (Grossman and Helpman, 1994). Absent campaign finance rules levelling the playing field, outsider candidates may not have access to the same resources as incumbents even if they are of high quality (Stratmann, 2005).

Despite its importance, much of this debate is framed around principles and anecdotes rather than sound empirical evidence (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 (Gulzar et al., 2022; Fouirnaies, 2021; Avis et al., 2022). 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 1990s to estimate the effects of far-reaching 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 threshold 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 campaign finance rules cause 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 an outsider candidate by 9.2 percentage points.

While the effect on victory by outsider candidates 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 are the ones benefitting 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. As an additional piece of evidence indicating that our effects are driven by the reimbursement of campaign expenditures, we predict incumbents' expenditures to ceiling ratio based on districts' sociodemographic characteristics and on the outcomes of the previous election, and find similar effects on our main outcomes in the subsample of districts where spending limits are least likely to be binding. We finally provide direct evidence that public reimbursement affects candidates' behavior: using a separate RDD at the candidate level, we show that candidates who pass the five percent vote share threshold required to be reimbursed are significantly more likely to compete in the next election.

The public reimbursement of candidates does not affect the polarization of elections, the representativeness of the winner's orientation with respect to first round vote choices, or the quality of the winner as proxied by their vote share in the next election. However, it increases 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 fewer private donations than 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, we do not find any significant effect of campaign finance rules in municipal elections. To understand this result, we note that mayoral candidates can split the campaign costs with the other members of their list and that they are more likely to receive private donations, so receiving public funding may make less of a difference for them and have less equalizing power than for candidates in departmental elections. Moreover, mayoral candidates are more likely to be known by voters and they tend to spend more on average, making the marginal returns of campaign expenditures possibly lower than in departmental elections. Finally, 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 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 sum, 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. 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 (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), or increase incumbency advantage, if incumbents have non-pecuniary resources which challengers can only hope to overcome by outspending them (Pastine and Pastine, 2012). The reimbursement of campaign expenditures exacerbates this tension. It decreases imbalances in candidates' access to money but lowers high quality candidates' ability to signal their quality by spending more (Ashworth, 2006; Prat et al., 2010).[1] Our results indicate that, on net, campaign finance regulations do level the playing field and decrease the incumbency advantage, and that they do not seem to decrease the quality of the winner.

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 (Austen-Smith, 1987; Coate, 2004b). On the other hand, private donors may seek to extract favors in exchange for their contributions, which could create a wedge between enacted policies and public interest (Baron, 1994; 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. Fouirnaies (2021) and Avis et al. (2022) find that limits on overall spending tend to increase competitiveness and reduce incumbency advantage, and Gulzar et al. (2022) 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' reimbursement is much less solid.[2] Malhotra (2008) and Masket and Miller (2015) exploit the fact that some U.S. states offer public funding to candidates respecting pre-set spending limits to measure effects on electoral competitiveness and on the legislative behavior of winners. 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 electoral rules and policies (e.g., Bordignon et al., 2016; Eggers et al., 2018; Corbi et al., 2019).

Beyond studies on campaign finance regulation, our paper also contributes to the broader litera-

[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 instead work against the cartelization of politics.

[2]Griffith and Noonen (2022) study a different form of public funding: the distribution of vouchers which voters can donate to their candidate of choice.

ture 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., effects of campaign expenditures on vote shares have been found to be modest overall, but larger for challengers than incumbents (Jacobson, 1978; Abramowitz, 1988; Levitt, 1994; Gerber, 1998, 2004).[3] 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.[4]

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, due to data limitations, but expect them to be important, given evidence that elected officials on the left and on the right implement different policies (Pettersson-Lidbom, 2008; Folke, 2014; Beland, 2015; Fiva et al., 2018, 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 and municipal elections, respectively. Section 6 discusses the mechanisms at play, and Section 7 concludes.

[3] 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. (2022), and Bekkouche et al. (2022).

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

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 (The Law Library of Congress, 2009; Gunlicks, 2019). 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. 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 regulations 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 14 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.[5] The rules prevalent in France and in the other aforementioned countries are 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 9,000 population threshold must provide a detailed account of their expenditures and revenues to a dedicated government agency, the CNCCFP (French National Commission on Campaign Accounts and Political Financing).[6] 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. 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 op-

[5] 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.

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

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

posed to blank and null votes) in the first round.[8] 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 banned corporate donations and 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, they do not have to disclose their accounts to the CNCCFP, and they are not eligible for reimbursement. Our main estimates measure the combined impact of reimbursement and spending limits, 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 disentangle the effects of the two treatments.

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., 2023). However, these changes affected districts 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 they receive 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

[8]Before 1995, 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 1995, 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.

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.[9]

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, 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.[10] Lists with more than five percent of the votes in the first round can merge with lists qualified for the second round.[11] The list winning a majority of votes in the second round receives half of the seats and the other seats are divided proportionally between all the lists which received more than five percent of the votes in the second round.

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 6.1.

[9]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.

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

[11]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.

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 population threshold. In districts below 9,000 inhabitants, candidates are not reimbursed and they face no spending limits, while candidates running in districts with 9,000 inhabitants or more 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:

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

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.[12] 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 official population was based on the same census for both elections.

3.2 Data and definitions

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

[12] In Appendix Tables C12 and C13, 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, and to controlling for districts' sociodemographic characteristics.

of the same political orientation, so we obtained candidate-level data from Cagé (2020) and Bach et al. (2012) and completed them by consulting and manually inputting results published in local newspapers present in French archives.

In each district, we link 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).[13] Among insiders, we check whether the winner and the runner-up from the previous election (the "incumbent" and the "challenger") run again.

We exploit political labels attributed by the Ministry of the Interior 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 left-right axis "non-classified." We classify 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; Döring et al., 2022). Appendix G provides further details on the mapping between political labels, political orientations and the ParlGov party positions.

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. Changes in the official population can occur following national censuses or out-of-census complementary decrees affecting small subsets of districts. 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 French 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. Appendix H explains the procedure we followed to determine the population of each district over time, which involved meticulously combinating and cross-checking these different data sources.

Finally, we digitized booklets from the commission monitoring party and candidate expenditures (CNCCFP). These booklets report the expenditures and breakdown of contributions received by candidates running in all districts above 9,000 inhabitants.[14] 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 pro-

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

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

vide 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. See Appendix I for a detailed discussion of the contribution and expenditure data and of the quality checks we conducted on them.

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 (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,[15] 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 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 manipulation by elected politicians.

We further provide empirical support for our identification assumption by conducting several manipulation and balance 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, Appendix Tables B1 and E1 show that this is the case neither for departmental nor municipal elections (columns 1 and 2). 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). Third, we conduct a general balance test to verify that the districts are similar on either side of the threshold: we regress the treatment variable T on a set of sociodemographic variables coming from the census, such as the distribution of age and occupation in the population and the unemployment rate; use the coefficients from this regression to predict the treatment status of each district; and test whether this predicted value jumps at the threshold. Fourth, we conduct balance tests on each of these sociodemographic variables taken individually. Fifth, we check that outcomes defined at election *t-1* do not jump at the threshold either. The results of these tests are presented in Sections 4.1 and 5 for departmental elections and municipal elections, respectively.

[15] 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.

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).

In Appendix H, we provide a comprehensive description of the national censuses and sources used to determine districts' official population, for each election in the sample. Broadly speaking, 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.[16] Therefore, our estimates generally capture the impact of being treated once. 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 F.

We check the consistency of all election results, and drop one race in the 2001 departmental elections, for which we detect inconsistencies.[17] Furthermore, our main outcomes require linking districts over time: for instance, we cannot define the incumbent, and, thus, we cannot measure 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*.[18] In municipal elections before 2014, we further require that the district population was above 3,500 inhabitants both at *t-1* and *t*,

[16] 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.

[17] 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.

[18] 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).

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 Appendix Tables B1 and E1 (column 3). In Appendices C and F, 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).[19] Table 1 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 are less competitive: the number of lists 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 6.2. 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.[20]

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

[20]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 1: 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	0.329	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

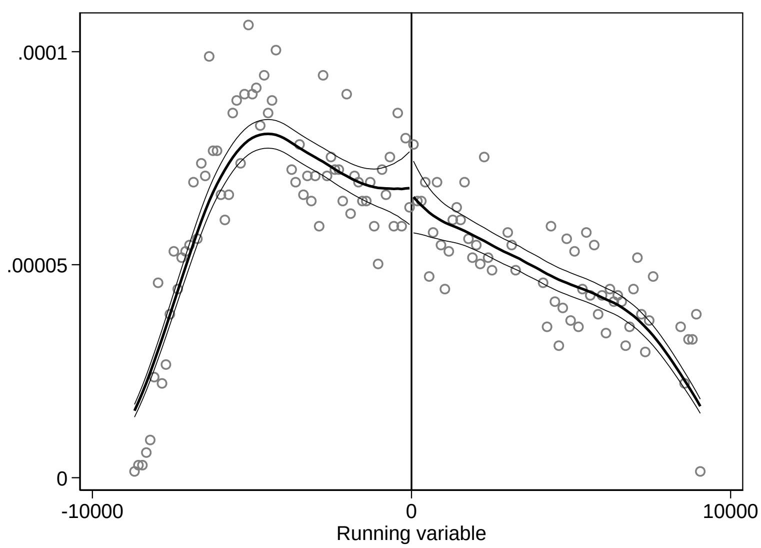
Notes: S.D. refers to standard deviation, min. to minimum, and max. to maximum. The outcome "Challenger victory" is missing for districts where only one candidate ran in the previous election.

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 do not indicate any discontinuity at the threshold either, the p-value of the manipulation test described in Cattaneo et al. (2018) is equal to 0.99, and adding non-linkable districts in the sample yields similar results (Appendix Figure B1).

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

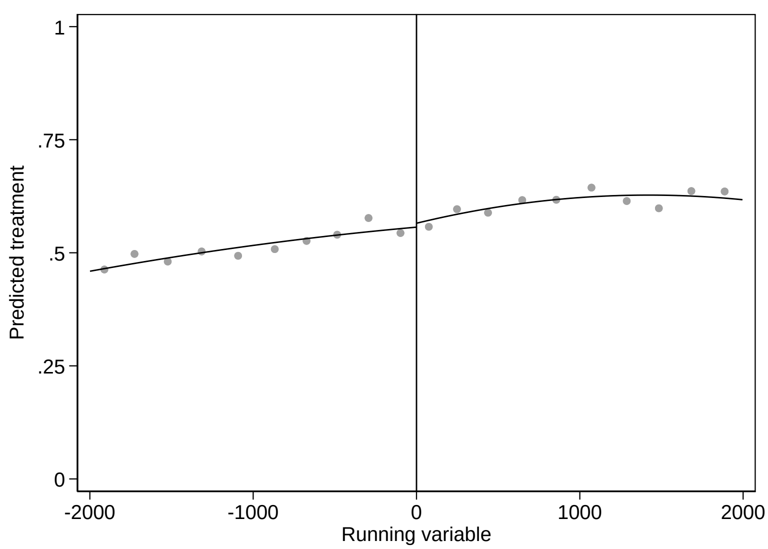


Notes: We test for a jump at the threshold in the density of the running variable (the district population centered around 9,000 inhabitants), using McCrary (2008)'s method. The solid line represents the density of the running variable, while the thin lines represent the confidence intervals. We exclude the 2008 elections since the running variable was the same as in 2001 in most districts (the same major census was in place for both elections).We also exclude out-of-cycle 2004 departmental races held to replace council members elected in 2001, for the same reason.

Appendix Table B2 and Appendix Figure B2 show placebo effects on the main outcomes defined in the previous election. None of them is statistically significant. Furthermore, Figure 2 shows the lack of jump at the cutoff when conducting the general balance test described in Section 3.3. Each dot represents the average value of the predicted treatment within a given bin of the running variable. We fit a quadratic polynomial on each side of the population threshold to facilitate visualization. Appendix Table B3 reports formal estimates obtained using our preferred specification and confirms the absence of a jump: the point estimate on the predicted treatment variable is small and nonsignificant. We do not find any jump either when we use the sample including non-linkable races (Appendix Table B4). Appendix Tables B5 and B6 also show balance tests on individual 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, 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.

Figure 2: General balance test - 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. The outcome is the value of the treatment predicted by sociodemographic variables: the share of men in the population; the share of the population under 29 years old, between 30 and 44 years old, between 45 and 59 years old, and above 60 years old; the share of working population; the share of unemployed (among working population); and the shares of skilled workers, blue-collar workers, employees, intermediate professions, artisans, and farmers (among the working population). To avoid dropping observations, for each regressor, we include a dummy equal to one when the variable is missing and replace missing values by 0s. The independent variable is a dummy equal to one if the district has a population greater or equal to 9,000 in year t.

4.2 Effects on competition

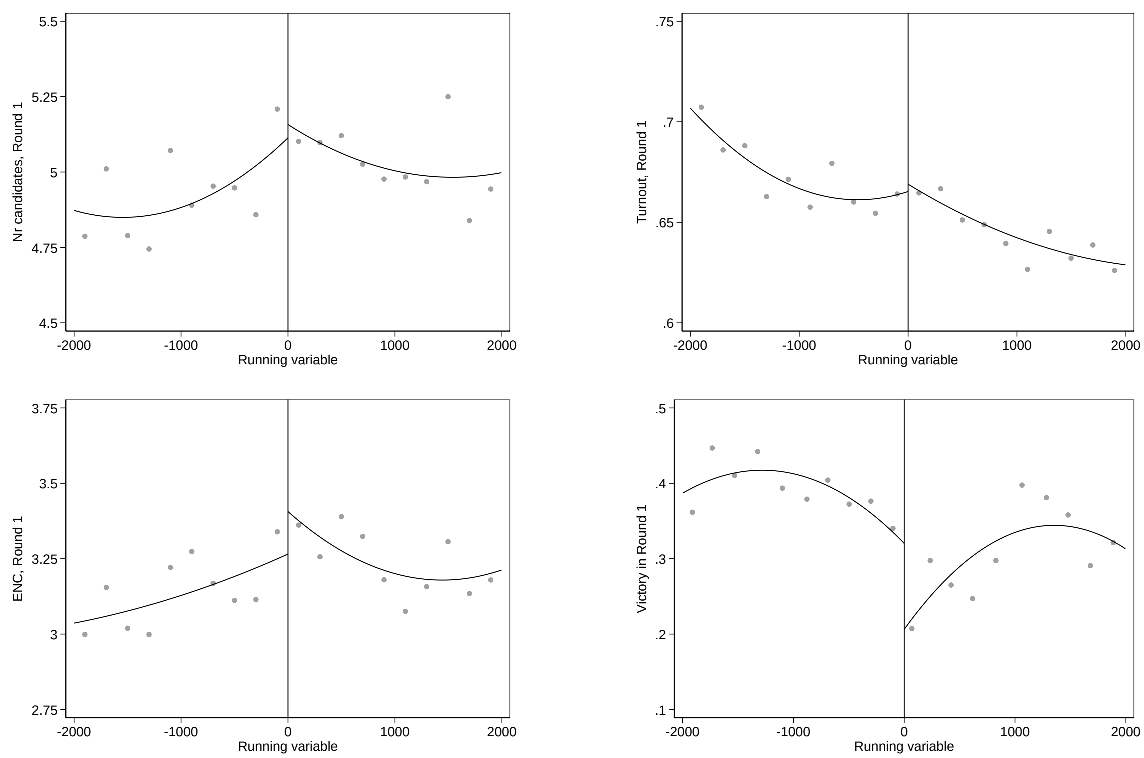
Our first set of outcomes relate to the competitiveness of elections. We first estimate effects on electoral supply: the total number of candidates, the number of outsider candidates (who were not present in the previous race in the district), and the number of insider candidates (who were present). 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 more than five percent of the votes. 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 candidates' vote shares by increasing the amount of money spent by smaller candidates relative to established candidates. We consider 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 3, before providing formal estimates. 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 and make it more difficult for them to win in the first round.

Table 2 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 nonsignificant. 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.

Figure 3: 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.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of			Turnout	ENC	Victory
	Candidates	Outsiders	Insiders	r1	r1	in first round
Treatment	0.046	0.014	0.028	0.010	0.086	-0.109**
	(0.119)	(0.120)	(0.066)	(0.009)	(0.089)	(0.044)
Robust p-value	0.513	0.825	0.472	0.235	0.246	0.012
Observations	2,326	2,629	2,359	2,306	2,451	2,151
Polyn. order	1	1	1	1	1	1
Bandwidth	2,610	2,953	2,648	2,577	2,741	2,410
Mean, left of threshold	5.055	3.593	1.464	0.656	3.246	0.353

Notes: Clustered 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

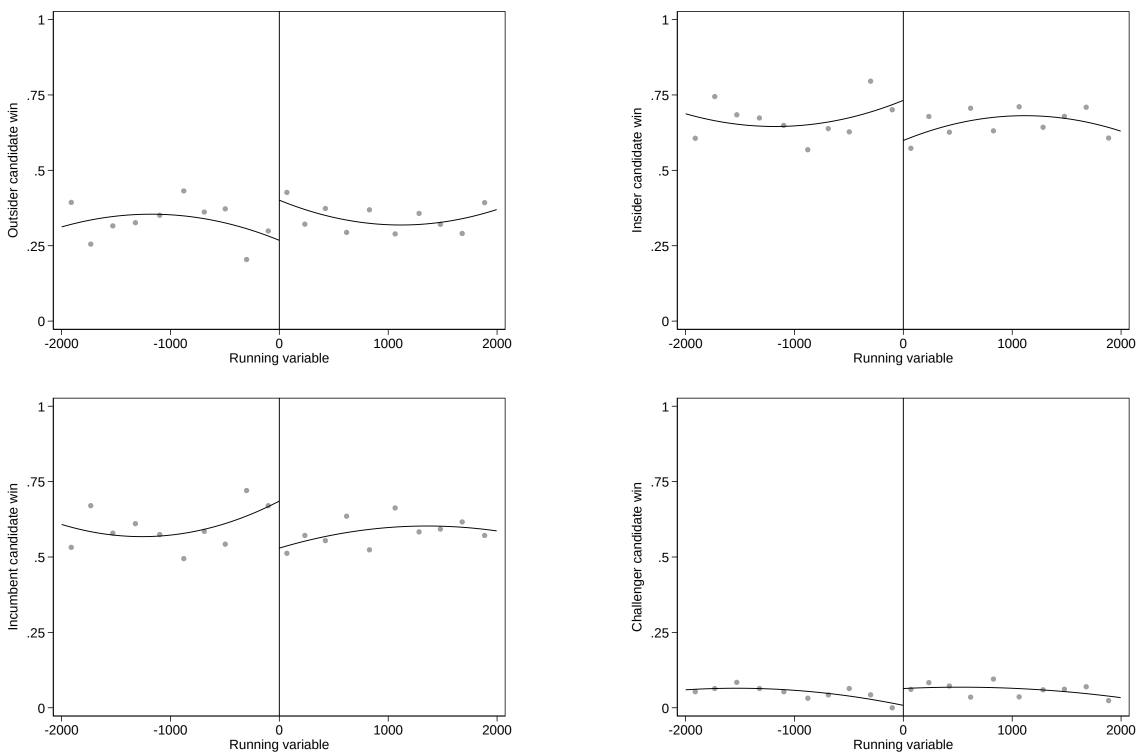
Despite their lack of effect on the total number of candidates, spending limits and the reimbursement of campaign expenditures may affect the selection of candidates who choose to enter the race and, in particular, the likelihood that the incumbent and the challenger of the previous race run again. Furthermore, 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.

Figure 4 plots four outcomes: the probability of a victory by an outsider, an insider, the incumbent, and their challenger.

We observe 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 3, 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 (a nearly three-fold increase), 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.

Figure 4: 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.

Table 3: 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 2.

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. Some incumbent candidates might choose not to run because they anticipate that they will not be able to outspend their competitors. Indeed, they know that their own expenditures will be limited and they may expect their competitors to take advantage of public reimbursement and spend more money than they would otherwise. The same reasoning may increase challengers' likelihood to run, contributing to their increased 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 2, column 2.[21]

We test and verify the hypotheses regarding the incumbent and challenger candidates' likelihood of running in Panel A of Table 4. 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 3, for reference. Columns 3 and 6 show effects on unconditional vote shares (equal to 0 if the candidate does not compete in the election). 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.

[21] Moreover, we do not find any significant impact on the likelihood that at least one outsider candidate participates in the election, as shown in Appendix Table A1, column 1.

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 incumbent 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 who choose to run 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. (2023) who adapt Lee (2009)'s method to derive bounds in a regression discontinuity design context. We show how we compute bounds on the effect on the incumbent's probability of winning conditional on running in Appendix J, for brevity. We then use the same method to derive bounds on challengers' probability of winning conditional on running. Furthermore, 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 described in Appendix J, and repeat these steps 10,000 times.

Panel B of Table 4 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 4: Impact on running, winning, and vote shares - Main sample of departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)
	Incumbent			Challenger		
Outcome	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		(0.093)	(0.033)		(0.080)	(0.042)
Lower bound		-0.105	-0.030		0.110	0.033
Boot. std error		(0.075)	(0.020)		(0.069)	(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 2. 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, representativeness, and winner quality

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.

We 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 by candidate orientation, 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 5 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. These results are robust to including non-linkable districts (Appendix Table C6). When excluding the 2008 elections, the effect on the likelihood of a victory by a left-wing candidate remains positive, but it becomes nonsignificant (p-value=0.11, Appendix Table C5).

Table 5: Impact on winning orientation - Main sample of departmental elections

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

Notes as in Table 2.

4.4.2 Effects on polarization

To further characterize the effects of campaign finance rules on electoral outcomes, we consider three possible drawbacks. First, while campaign finance rules level the playing field, improved performance by candidates from non-mainstream platforms could increase polarization. Second, 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) (see Section 4.4.3). Third, by compressing differences in money spent across candidates, the reforms may eliminate a valuable signal of differences in quality and lead to the victory of worse candidates (see Section 4.4.4).

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 position 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 first round vote share, and p_i, the ideological positioning of their party or affiliation (see Appendix G for further information on ParlGov data). 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 6 (column 1), the impact on this outcome is small and nonsignificant, indicating that campaign finance rules do not increase polarization.

4.4.3 Effects on representativeness

We now assess whether the legislation affected the representativeness of the winner.

We proxy voter preferences using first round results, as voters are likely to express their true preferences in the first round of two-round elections (Piketty, 2000). We aggregate first round vote shares by orientation and measure effects on the first round vote share of the winner's orientation and on a dummy equal to 1 if that orientation obtained the most votes in the first round. We find a negligible effect on the first outcome (column 2) and a negative but small and nonsignificant effect on the second (column 3), indicating that the campaign finance rules above the threshold do not decrease the representativeness of the winner with respect to the distribution of first round vote choices.

Table 6: 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
Polynomial order	1	1	1
Bandwidth	2,770	2,565	2,098
Mean, left of threshold	4.868	0.583	0.922

Notes: The sample in column 1 is restricted to departmental races for which each candidate can be matched to a ParlGov position on the [0-10] left-right scale, excluding 14.0 percent of the sample. The outcomes in columns 2 and 3 are the first round vote share of the orientation of the departmental election's winner and a dummy equal to 1 if that orientation had obtained the most votes. Other notes as in Table 2.

4.4.4 Effects on winner quality

Despite the lack of any direct measure of winners' quality, we can build a proxy by considering their vote share in the next election. An increase in the winner's vote share would signal that voters are satisfied with their performance. As shown in Table 7, column 1, we do not find any significant effect on the difference between the vote share of election t's winner at $t+1$ and t. Of course, $t+1$ vote shares are affected by many factors beyond candidate quality. To control for other determinants, we next regress the winner's vote share in election $t+1$ or the difference in their vote share between $t+1$ and t on a large number of candidate, electoral, and sociodemographic factors (listed in Appendix K) and use the residuals as proxy for the winner's quality. We do not find any effect on these outcomes either (columns 2 to 5).

While all the specifications in Table 10 set the vote share at $t+1$ to 0 if the winner does not run again, to avoid dropping observations, we note that this choice is unlikely to drive the results. Indeed, Appendix Table B7 shows that the probability that the winner runs in the next election does not jump at the threshold. Furthermore, we obtain qualitatively similar results when we restrict the sample to districts in which the election t winner runs again at $t+1$ (Appendix Table C7).

Table 7: Impact on the quality of the winner - Main sample of departmental elections linkable between t and $t+1$

	(1)	(2)	(3)	(4)	(5)
		Residual			
Outcome	Vote share difference	Vote share at *t+1*		Vote share difference	
		restricted	unrestricted	restricted	unrestricted
Treatment	0.027	-0.004	-0.003	-0.003	-0.004
	(0.030)	(0.021)	(0.021)	(0.021)	(0.021)
Robust *p*-value	0.265	0.994	0.923	0.999	0.863
Observations	1,028	1,268	1,694	1,262	1,770
Polyn. order	1	1	1	1	1
Bandwidth	1,934	2,354	3,184	2,344	3,329
Mean, left of threshold	-0.091	0.003	0.005	0.003	0.007

Notes: The sample includes all districts from the main sample that can be linked between election t and $t+1$. Column 1 takes as outcome the difference in vote share of the winner in t between election $t+1$ and t. Columns 2 to 5 take as outcomes the residuals of regressions predicting the vote share of election t's winner in $t+1$, in columns 2 and 3, and the difference between their vote share in $t+1$ and t, in columns 4 and 5. These predictive regressions use a sample restricted to observations between 8,000 and 10,000 inhabitants, in columns 2 and 4, and the entire sample, in columns 3 and 5. In districts where the incumbent does not run at $t+1$, we set their vote share at $t+1$ to 0. Other notes as in Table 2.

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 C8 through C11. 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 and to controlling for all the sociodemographic variables used in the general balance test in Appendix Tables C12 and C13, respectively. 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 E2 and E3 show the general balance tests for the main sample and the sample also including non-linkable districts, while Appendix Tables E4 and E5 show the balance tests on individual sociodemographic variables. The general balance tests show no significant jump, and only two out of 26 point estimates corresponding to the individual tests are significant at the 10 percent level.

Appendix Figures E1 and E2 test the assumption of no sorting across the threshold using the McCrary (2008) 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 E4 through E6 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 8 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 F1 through F3), and when we include non-linkable districts in the sample used to measure effects on competition (Appendix Tables F4 through F7). We investigate the mechanisms driving the difference between results in departmental and municipal elections in the next section.

Table 8: 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	-0.032	-0.018	0.003	0.036	-0.008
	(0.135)	(0.130)	(0.069)	(0.009)	(0.099)	(0.059)
Robust *p*-value	0.778	0.775	0.901	0.567	0.762	0.822
Observations	1,426	1,433	2,260	1,189	1,455	1,315
Polyn. order	1	1	1	1	1	1
Bandwidth	1,908	1,916	2,807	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	0.061

Notes as in Table 2.

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 these differences 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 likely 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 26 other 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 Appendix Table D1, 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 (columns 1 and 2).

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 Appendix Table D1).

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).[22] We find that party-affiliated incumbents are driving the results: campaign finance rules reduce their 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 nonsignificant, for non-party-affiliated incumbents. These results suggest that, in departmental races above the threshold, where electoral competition is greater, political parties prevent incumbents that they expect to be defeated from running again.

[22] 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.

By contrast, as shown in Appendix Table D2, 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 F8 through F10 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 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.

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 more direct evidence through four separate exercises.

6.2.1 Effects in 1992 and 1994 departmental elections

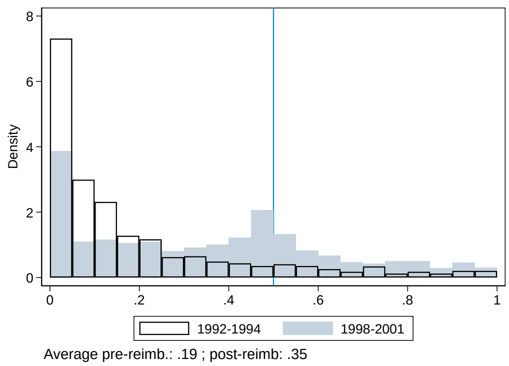
First, we exploit the departmental elections held in 1992 and 1994. These elections enable us to isolate the effect of spending limits because they took place after the 1990 reform enforcing limits for districts above the discontinuity, but before the 1995 reform enacting the reimbursement of candidates. 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 are generally nonsignificant. 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. Moreover, we report the *p*-values of the test of the difference of coefficients before and after 1995 and reject the null hypothesis that the two coefficients are equal for the probability of incumbent, challenger, and outsider candidates winning, as well as the probability of challengers running. We only fail to reject it for the probability of the incumbent running and the probability of a candidate's victory in one round.

6.2.2 Changes in candidate spending and contribution patterns over time

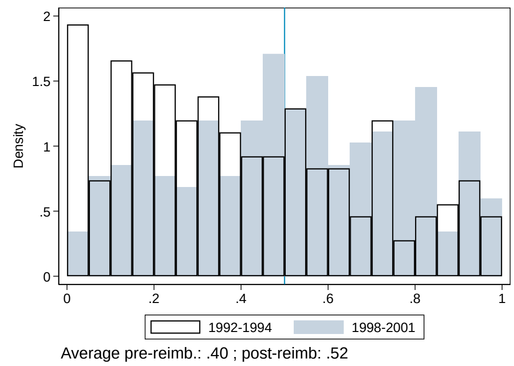
While these results suggest that effects post 1995 are driven by reimbursement rather than spending limits, alternative interpretations remain possible. The tightening of spending limits and ban on corporate donations concomitant to the introduction of public reimbursement, in 1995, could play a role, and limits and reimbursement may be complementary and jointly explain the effects. Therefore, we provide additional 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 5 and 6 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 the ones benefitting 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). Moreover, 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 appear at the 50 percent threshold 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.

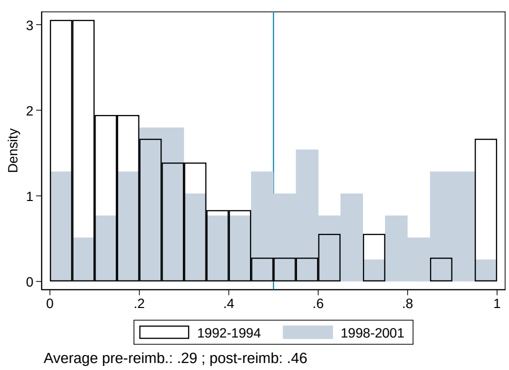
Figure 5: Expenditures to ceiling ratios - Main sample of departmental elections



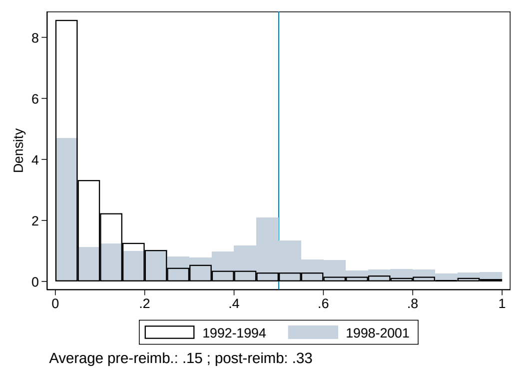
All candidates



Incumbent candidates



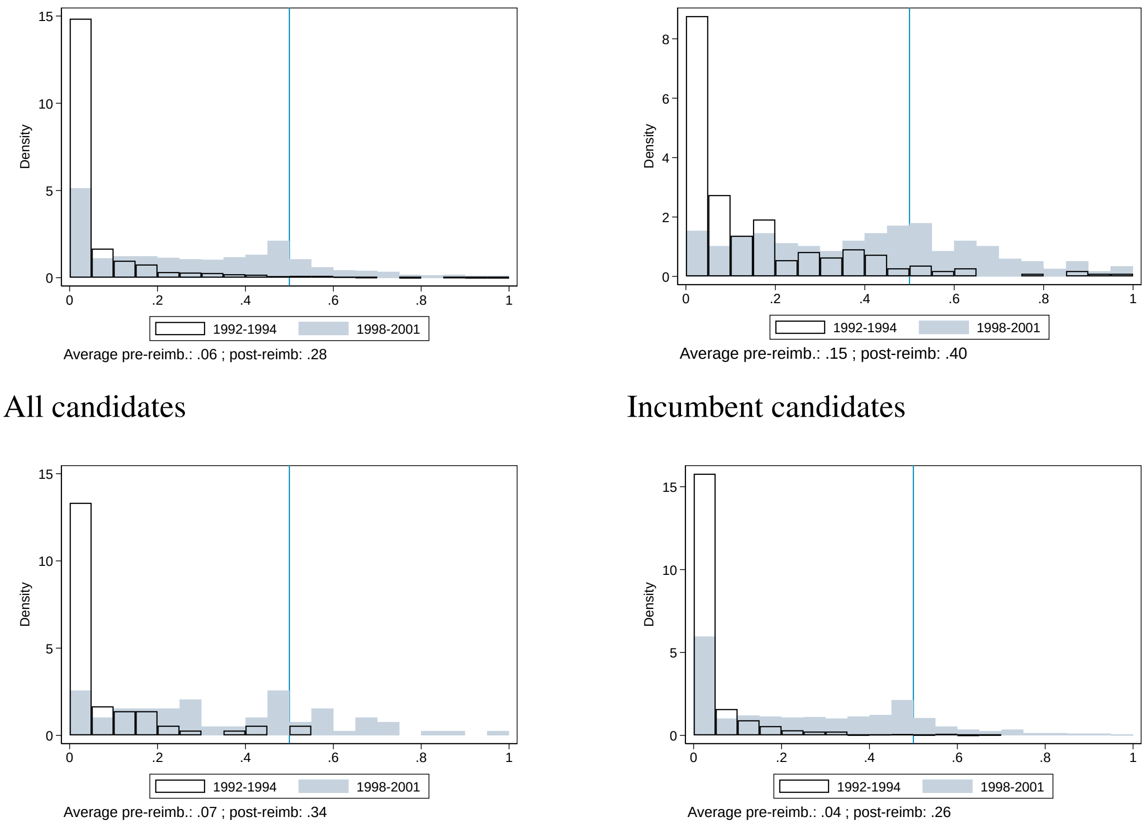
Challenger candidates



Outsider candidates

Notes: The level of analysis is the candidate and the sample includes only districts between 9,000 and 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. We exclude the 0.3 percent of candidates with at least one inconsistency in their contribution and expenditure data (see Appendix I).

Figure 6: 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 between 9,000 and 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. We exclude the 0.3 percent of candidates with at least one inconsistency in their contribution and expenditure data (see Appendix I.I).

6.2.3 Impact on districs with non-binding spending ceilings

As an additional test of the relative importance of reimbursements versus spending limits, we investigate whether our results hold when focusing on districts where spending ceilings are least likely to be binding. Since incumbents generally spend more money than other candidates, we consider the incumbent spending to ceiling ratio. We first restrict the sample to districts just above the threshold (between 9,000 and 10,000 inhabitants) and regress this variable on previous electoral outcomes (including measures of electoral competitiveness), the set of sociodemographic variables used in the general balance test, as well as year and department fixed effects.[23] We then use the coefficients from this regression to predict the incumbent spending to ceiling ratio in all districts.

[23]See Appendix Table A6 for a more detailed description of this regression.

Finally, we focus on districts in which the predicted ratio is below its median (0.57), and in which spending limits are thus likely to be the least binding. Indeed, in districts of this subsample just above the discontinuity, the distribution of the incumbent spending to ceiling ratio is to the left of the distribution for all districts just above the discontinuity, and it does not show any bunching at the limit (Appendix Figure A1). And yet, effects in this subsample, shown in Appendix Table A6, are similar as in the full sample. In particular, the effects on the probability of a victory by the incumbent and the challenger are -15.5 and 6.8 percentage points, which is close to the point estimates in the main sample (-14.5 and 5.2 percentage points), and they are significant at the five percent level.

6.2.4 RDD at the candidate level

Finally, we provide direct evidence at the candidate level on the impact of public reimbursement by exploiting the fact that candidates are only eligible for it if they obtain more than five percent of the votes in the first round. If public reimbursement makes a difference, we would expect candidates who obtained more than five percent of the votes in the last election to be more likely to compete again. We run a separate RDD around this threshold, using the following specification:

$$Y_{j,t+1} = \alpha + \tau D_{j,t} + \beta X_{j,t} + \gamma X_{j,t} D_{j,t} + \varepsilon_{j,t}, \tag{2}$$

where $Y_{j,t+1}$ is a dummy equal to 1 if candidate j, present at election t, runs again in election $t+1$, $X_{j,t}$ is the running variable, defined as the candidate's vote share at t centered around five percent, and $D_{j,t}$ is the assignment variable, a dummy taking value one if $X_{j,t}$ is positive.[24] The sample is restricted to districts above 9,000 inhabitants which are linkable between t and $t+1$, in departmental elections post 1995.[25] As for our main RDD, we use a non-parametric estimation, apply Calonico et al. (2014)'s estimation procedure, construct the optimal data-driven bandwidth following Calonico et al. (2019)'s algorithm, and cluster our standard errors at the district level.

As shown in Figure 7, candidates who obtain more than five percent of the votes are significantly more likely to compete in the next election than those below the threshold. Table 9 provides the point estimate: an increase by 4.2 percentage points at the threshold (48.8 percent of the mean). This effect is unlikely to be driven by other factors than public reimbursement, such as a psychological effect of passing a symbolic threshold: as shown in Appendix Tables C14 and C15, we do not find any effect in the 1992 and 1994 departmental elections (before public reimbursement was

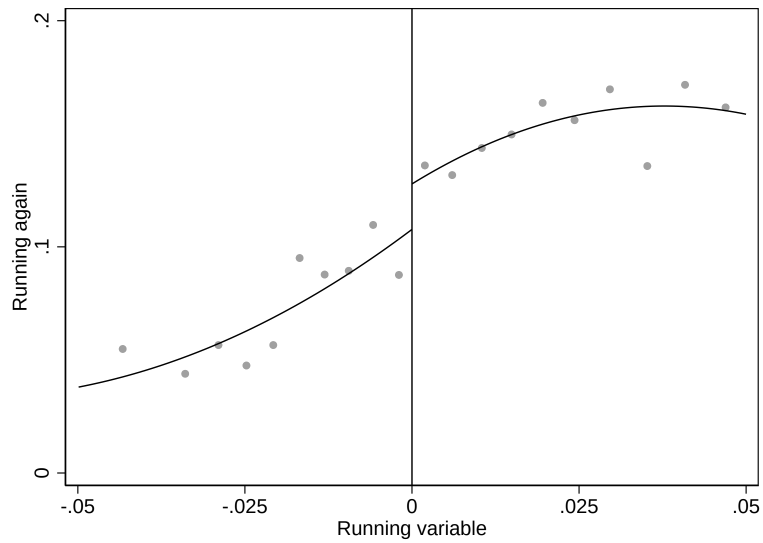
[24] Using a similar strategy in South Korean municipal elections, Song (2020) does not find any overall effect on candidates' likelihood of running again on average, but substantial effects for female candidates.

[25] We do not include municipal elections because the 5 percent threshold determines not just candidates' eligibility for public reimbursement but also the possibility for them to merge their list with another list between the first and second rounds.

introduced) and in districts below 9,000 inhabitants (in which candidates' expenditures are never reimbursed).

Taken together, these results all point to the conclusion that reimbursement, not spending limits, drives our results.

Figure 7: Effect of being reimbursed in election t on running in election $t+1$ - Departmental elections linkable between election t and $t+1$



Notes: Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the vote share centered around five percent) is split into quantile-spaced bins. The continuous lines represent a quadratic fit. To facilitate visualization, the graph is truncated at five percent around the cutoff. The outcome is a dummy equal to one if the candidate running in election t runs again in election $t+1$. The independent variable is a dummy equal to one if the candidate running in election t obtains more than five percent of the votes. The level of analysis is the candidate and the sample includes only districts above 9,000 inhabitants and which can be linked with election $t+1$.

Table 9: Impact of being reimbursed in election t on running in election $t+1$ - Departmental elections linkable between election t and $t+1$

	(1)
Outcome	Run next election
Treatment	0.042*
	(0.022)
Robust p-value	0.066
Observations	3,663
Polyn. order	1
Bandwidth	0.014
Mean, left of threshold	0.086

Notes: Clustered standard errors are in parentheses. Robust p-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the candidate running in election t obtains more than five percent of the votes. Separate polynomials are fitted on each side of the threshold. The polynomial order is one and the bandwidth is derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity. The level of analysis is the candidate and the sample includes only districts above 9,000 inhabitants and which can be linked with election *t+1*.

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 large 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 and it helps the left, whose candidates are often outspent by right-wing competitors absent public funding. Importantly, we note that this policy does not increase the polarization of the results and decreases neither our measure of winner quality nor 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.

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.

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Appendix (for online publication only)

Appendix I: Departmental elections

A. Additional tables and figures

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

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Outsider candidates			Insider candidates		
	run	win	vote share, R1	run	win	vote share, R1
Treatment	-0.004	0.092**	0.019	-0.057***	-0.092**	-0.019
	(0.007)	(0.042)	(0.020)	(0.020)	(0.042)	(0.020)
Robust p-value	0.645	0.024	0.387	0.009	0.024	0.387
Observations	2,153	1,686	2,576	3,155	1,686	2,576
Polyn. order	1	1	1	1	1	1
Bandwidth	2,414	1,886	2,868	3,538	1,886	2,868
Mean, left of threshold	0.995	0.288	0.529	0.934	0.712	0.471

Notes: Clustered 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 by candidate orientation

	Far-left	Left	Center	Right	Far-right	Non-classified
Panel A. 1992-1994 elections						
Expenditures	0.104	0.172	*NA*	0.328	0.036	0.083
Personal contributions	0.035	0.032	*NA*	0.139	0.013	0.041
Party contributions	0.008	0.087	*NA*	0.056	0.017	0.019
Donations	0.064	0.062	*NA*	0.145	0.002	0.031
Panel B. 1998-2001 elections						
Expenditures	0.092	0.396	0.558	0.438	0.196	0.134
Personal contributions	0.087	0.310	0.470	0.344	0.178	0.075
Party contributions	0.005	0.035	0.013	0.014	0.004	0.024
Donations	0.005	0.032	0.059	0.077	0.001	0.036

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 in-kind 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. Before 2001, there were no centrist candidates running in departmental elections. We exclude the 0.3 percent of candidates with at least one inconsistency in their contribution and expenditure data.

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		(0.111)	(0.045)
Lower bound		-0.111	-0.050*
Boot. std error		(0.086)	(0.027)
Mean		0.838	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)
	Incumbent		
Outcome	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		(0.109)	(0.051)
Lower bound		0.002	0.005
Boot. std error		(0.084)	(0.032)
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 vs. main sample of departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Outcome	Incumbent run		Incumbent win		Challenger run		Challenger win		Outsider run		Outsider win		Victory in first round	
	92-94	Main sample	92-94	Main sample	92-94	Main sample	92-94	Main sample	92-94	Main sample	92-94	Main sample	92-94	Main sample
Treatment	-0.013	-0.074**	0.038	-0.145***	-0.045	0.084**	-0.051*	0.052**	-0.009	-0.004	-0.050	0.092**	-0.015	-0.109**
	(0.051)	(0.032)	(0.066)	(0.046)	(0.061)	(0.038)	(0.030)	(0.020)	(0.009)	(0.007)	(0.070)	(0.042)	(0.064)	(0.044)
Robust p-value	0.934	0.023	0.535	0.002	0.429	0.020	0.061	0.012	0.186	0.645	0.533	0.024	0.861	0.012
Observations	1,175	2,579	1,041	1,392	1,021	1,827	588	1,819	729	2,153	871	1,686	1,114	2,151
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	3,818	2,876	3,368	1,578	3,322	2,056	1,910	2,037	2,346	2,414	2,807	1,886	3,601	2,410
Mean, left of threshold	0.800	0.767	0.619	0.683	0.309	0.176	0.050	0.0184	1.001	0.995	0.345	0.288	0.356	0.353
Difference p-value	0.312		0.023		0.073		0.004		0.638		0.082		0.227	

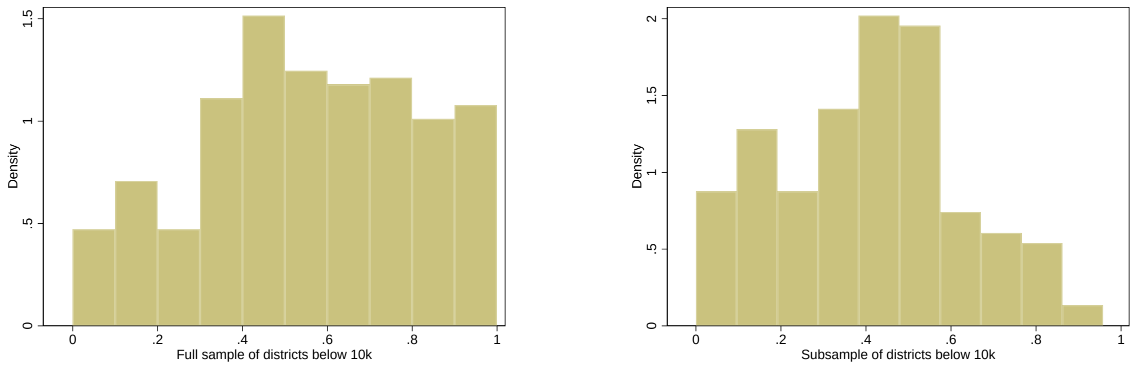
Notes: In odd columns (resp. in even columns), we consider the 1992 and 1994 departmental elections (resp. the post-1995 departmental elections, which constitute our main sample of analysis). The last row reports the p-value of the test of the difference between coefficients on the same outcome before vs. after 1995, when public reimbursement was first introduced. Other notes as in Table A1.

Table A6: Impact on the main outcomes - Subsample of departmental elections with below median predicted incumbent spending to ceiling ratio

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Incumbent		Challenger		Outsider		Victory in
	run	win	run	win	run	win	first round
Treatment	-0.147***	-0.157**	0.051	0.071**	-0.003	0.077	-0.067
	(0.048)	(0.064)	(0.048)	(0.029)	(0.008)	(0.055)	(0.061)
Robust p-value	0.002	0.017	0.224	0.014	0.798	0.160	0.383
Observations	1,419	939	1,220	1,001	1,549	1,192	1,267
Polyn. order	1	1	1	1	1	1	1
Bandwidth	2,990	1,988	2,560	2,121	3,301	2,499	2,682
Mean, left of threshold	0.790	0.671	0.184	0.00511	0.995	0.313	0.410

Notes: We restrict the sample to districts within our main sample of departmental elections for which the predicted incumbent spending to ceiling ratio lies below the median of the predicted ratio in all districts (*approx.* 0.57). We construct this measure by first regressing the incumbent spending to ceiling ratio on sociodemographic variables and previous election outcomes as well as year and department fixed effects in districts between 9,000 and 10,000 inhabitants in which the incumbent runs. Sociodemographic variables include: the share of men in the population; the share of the population under 29 years old, between 30 and 44 years old, between 45 and 59 years old, and above 60 years old; the share of working population; the share of unemployed (among working population); and the shares of skilled workers, blue-collar workers, employees, intermediate professions, artisans, and farmers (among working population). Previous election outcomes include: the number of candidates, the effective number of candidates, and a dummy indicating whether only one candidate ran; dummies indicating whether the incumbent ran, the challenger ran, and an outsider ran; voter turnout; the closeness of the election; a dummy indicating whether the election was won in the first round; a dummy indicating whether the top two candidates had the same orientation; dummies indicating whether the incumbent won, the challenger won, and an outsider won; dummies indicating whether a left, right, center, far-right, and far-left candidate won; and dummies indicating whether the winner was not affiliated to a party and whether a female candidate won. To avoid dropping observations, for each regressor, we include a dummy equal to one when the variable is missing and replace missing values by 0s. Then, we use the coefficients from this regression to predict the incumbent spending to ceiling ratio in all districts. We exclude the 3.2 percent of districts with at least one candidate with at least one inconsistency in their contribution and expenditure data from the prediction stage (see Appendix I.I). Other notes as in Table A1.

Figure A1: Distribution of incumbent spending to ceiling ratios - Departmental elections, districts between 9,000 and 10,000 inhabitants



Notes: The left-hand side graph includes all districts between 9,000 and 10,000 inhabitants where the incumbent runs while the right-hand side focuses on districts within this sample where the predicted incumbent spending to ceiling ratio lies below the median predicted ratio on all districts (0.57). We exclude the 3.2 percent of districts with at least one candidate with at least one inconsistency in their contribution and expenditure data (see Appendix I).

B. Validity

Table B1: Changes since election *t-1* - Departmental elections

	(1)	(2)	(3)
Outcome	Redistricted	Treated in *t-1*	Linkable
Treatment	0.007	0.052	-0.007
	(0.006)	(0.086)	(0.006)
Robust *p*-value	0.378	0.852	0.378
Observations	2,846	547	2,846
Polyn. order	1	1	1
Bandwidth	3,186	1,031	3,186
Mean, left of the threshold	0.000	0.364	1.000

Notes: Clustered 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 column 2 since the same major census was in place for both the 2001 and 2008 elections. We also exclude out-of-cycle 2004 departmental races held to replace council members elected in the 2001 elections in column 2, for the same reason.

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

Outcome	(1) Incumbent run	(2) Incumbent win	(3) Challenger run	(4) Challenger win	(5) Outsider run	(6) Outsider win	(7) Victory in 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: Clustered 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 the population and, therefore, the running and assignment variables, were the same as in the 1992 (resp. 2001) elections in most districts. We also exclude out-of-cycle 2004 departmental races held to replace council members elected in the 2001 elections, for the same reason.

Table B3: General balance test - Main sample of departmental elections

	(1)
Outcome	Predicted treatment
Treatment	0.020
	(0.020)
Robust p-value	0.370
Observations	2,143
Polyn. order	1
Bandwidth	3,041
Mean, left of threshold	0.563

Notes: Clustered 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. The outcome is the value of the treatment predicted by sociodemographic variables: the share of men in the population; the share of the population under 29 years old, between 30 and 44 years old, between 45 and 59 years old, and above 60 years old; the share of working population; the share of unemployed (among working population); and the shares of skilled workers, blue-collar workers, employees, intermediate professions, artisans, and farmers (among working population). To avoid dropping observations, for each regressor, we include a dummy equal to one when the variable is missing and replace missing values by 0s. The independent variable is a dummy equal to one if the district has a population greater or equal to 9,000 in year t. Separate polynomials are fitted on each side of the threshold. The polynomial order is one 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 since in most districts, the population and, therefore, the running and assignment variables, were the same as in the 2001 elections. We also exclude out-of-cycle 2004 departmental races held to replace council members elected in the 2001 elections, for the same reason.

Table B4: General balance test - All departmental elections, including non-linkable districts

	(1)
Outcome	Predicted treatment
Treatment	0.020
	(0.020)
Robust p-value	0.361
Observations	2,151
Polyn. order	1
Bandwidth	3,031
Mean, left of threshold	0.565

Notes: The sample also includes non-likable districts. Other notes as in Table B3.

Table B5: 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	Working population	Unemployed	Skilled workers	Blue-collar	Employees	Intermediate	Artisans	Farmers
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	28,19	3,341	2,812	2,550	3,171	2,244	31,68
Mean, left of threshold	0.493	0.356	0.208	0.186	0.252	0.436	0.116	0.0658	0.335	0.278	0.183	0.0735	0.0642

Notes: Clustered 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). We also exclude out-of-cycle 2004 departmental races organized to replace council members elected in the 2001 elections, for the same reason.

Table B6: 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	Working population	Unemployed	Skilled workers	Blue-collar	Employees	Intermediate	Artisans	Farmers
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

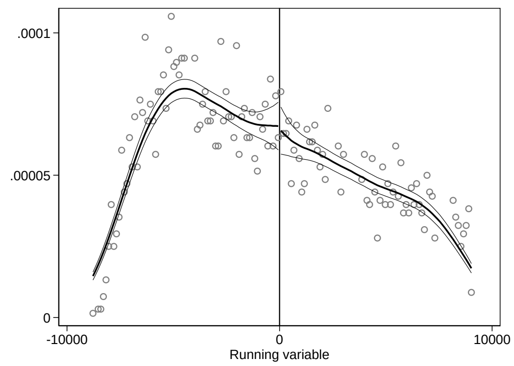
Notes as in Table B5.

Table B7: Impact on winner in election *t* running again in *t+1* - Main sample of departmental elections linkable between *t* and *t+1*

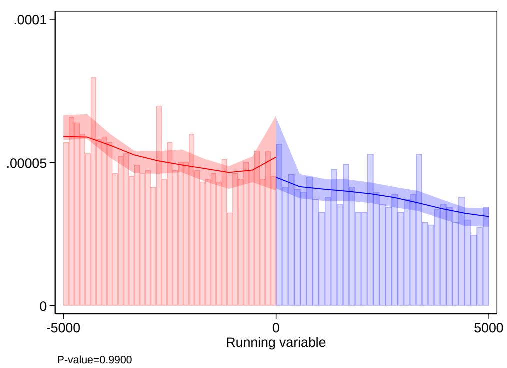
	(1)
Outcome	Winner run again
Treatment	0.016
	(0.054)
Robust *p*-value	0.569
Observations	1,022
Polyn. order	1
Bandwidth	1,913
Mean, left of threshold	0.751

Notes: The sample consists of districts from the main sample which can be linked from election *t* to *t+1*. Other notes as in Table B1.

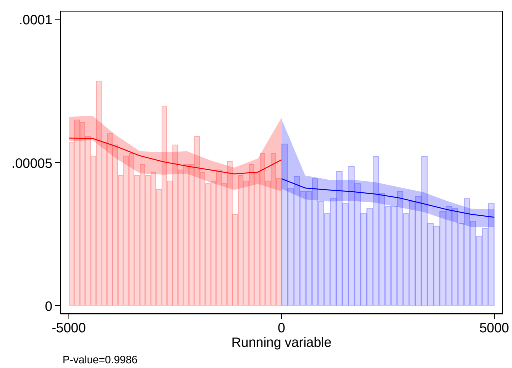
Figure B1: McCrary (2008) and Cattaneo et al. (2018) density tests



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



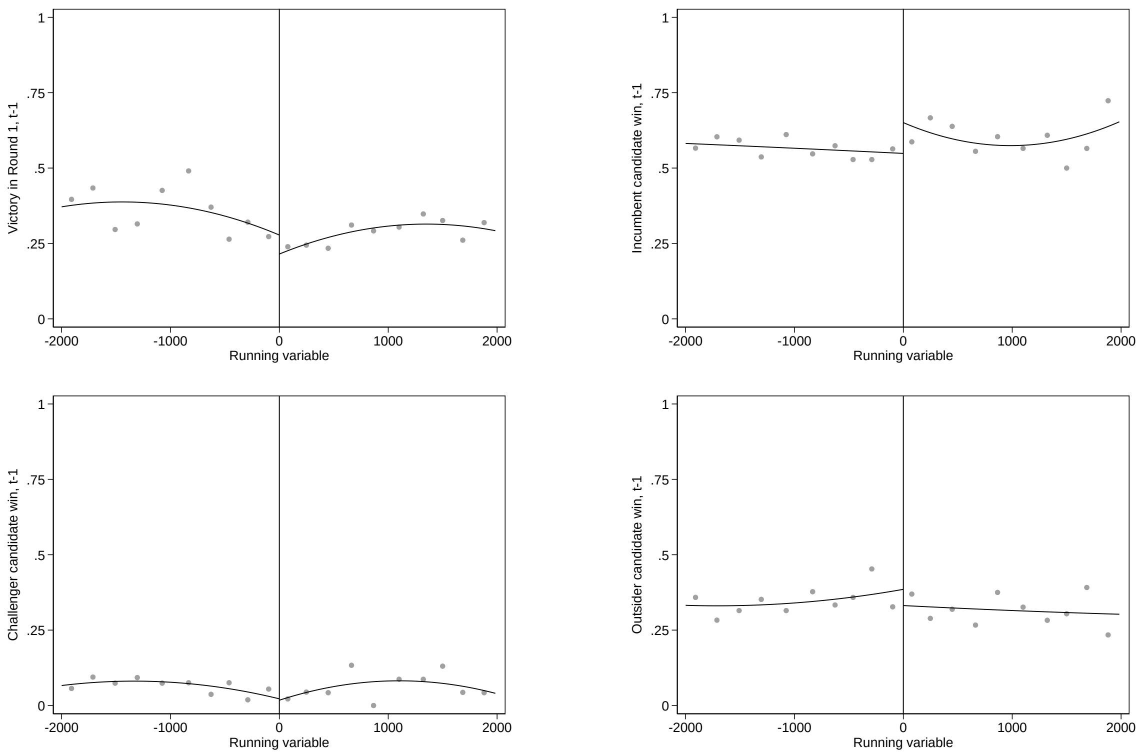
RD Density test- Main sample of departmental elections



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

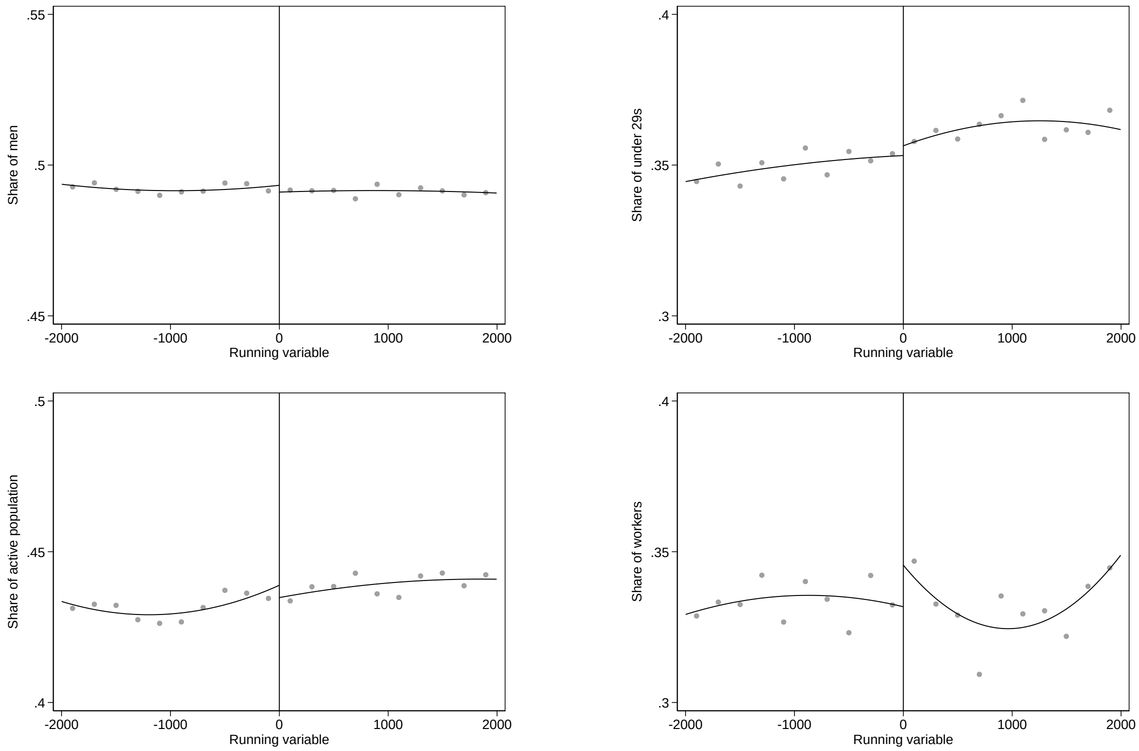
Notes: We test for a jump at the threshold in the density of the running variable (the district population centered around 9,000 inhabitants), using McCrary (2008)'s method in the top panel. The solid line represents the density of the running variable, while the thin lines represent the confidence intervals. 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). We also exclude out-of-cycle 2004 departmental races organized to replace council members elected in the 2001 elections, for the same reason.

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). We also exclude out-of-cycle 2004 departmental races organized to replace council members elected in the 2001 elections, for the same reason.

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). We also exclude out-of-cycle 2004 departmental races organized to replace council members elected in the 2001 elections, for the same reason.

C. Robustness

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

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of			Turnout	ENC	Victory
	Candidates	Outsiders	Insiders	r1	r1	in first round
Treatment	0.103	0.030	0.013	0.014	0.136	-0.103**
	(0.155)	(0.157)	(0.079)	(0.010)	(0.118)	(0.049)
Robust p-value	0.356	0.723	0.653	0.117	0.177	0.033
Observations	1,345	1,624	2,055	1,802	1,397	1,737
Polyn. order	1	1	1	1	1	1
Bandwidth	1,910	2,297	2,894	2,545	1,987	2,457
Mean, left of threshold	5.278	3.782	1.501	0.639	3.351	0.312

Notes: Clustered 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. We exclude the 2008 departmental elections where, in most districts, the population and, therefore, the running and assignment variables, were the same as in the 2001 departmental elections.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Number of			Turnout	ENC	Victory
	Candidates	Outsiders	Insiders	r1	r1	in first round
Treatment	0.044	0.014	0.028	0.009	0.073	-0.111***
	(0.118)	(0.120)	(0.066)	(0.009)	(0.085)	(0.044)
Robust p-value	0.524	0.825	0.472	0.263	0.279	0.010
Observations	2,460	2,629	2,359	2,336	2,768	2,222
Polyn. order	1	1	1	1	1	1
Bandwidth	2,736	2,953	2,648	2,604	3,093	2,473
Mean, left of threshold	5.055	3.593	1.464	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	0.0650	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)
	Incumbent			Challenger		
Outcome	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		(0.101)	(0.040)		(0.082)	(0.042)
Lower bound		-0.053	-0.029		0.152**	0.036*
Boot. std error		(0.075)	(0.023)		(0.073)	(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 - Sample of departmental elections excluding 2008

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Far-left win	Left win	Center win	Right win	Far-right win	Non-classified win
Treatment	-0.004	0.081	-0.025	-0.056	-0.000	0.010
	(0.004)	(0.054)	(0.018)	(0.049)	(0.000)	(0.010)
Robust *p*-value	0.159	0.110	0.157	0.241	0.294	0.322
Observations	1,574	1,933	2,056	2,341	1,272	1,734
Polyn. order	1	1	1	1	1	1
Bandwidth	2,237	2,732	2,898	3,333	1,804	2,454
Mean, left of threshold	0.004	0.480	0.048	0.467	0.000	0.004

Notes as in Table C1.

Table C6: Impact on winning orientation - All departmental elections, including non-linkable districts

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Far-left win	Left win	Center win	Right win	Far-right win	Non-classified win
Treatment	-0.003	0.084*	-0.021	-0.055	-0.001	0.014
	(0.003)	(0.047)	(0.014)	(0.041)	(0.001)	(0.009)
Robust *p*-value	0.266	0.063	0.144	0.189	0.360	0.152
Observations	2,236	2,559	2,600	3,373	1,730	2,116
Polyn. order	1	1	1	1	1	1
Bandwidth	2,488	2,822	2,881	3,768	1,939	2,350
Mean, left of threshold	0.003	0.475	0.043	0.477	0.000	0.002

Notes as in Table C1.

Table C7: Impact on the quality of the winner - Subsample of departmental elections where incumbents run again in *t+1*

	(1)	(2)	(3)	(4)	(5)
		Residual			
Outcome	Vote share difference	Vote share at *t+1*		Vote share difference	
		restricted	unrestricted	restricted	unrestricted
Treatment	0.019	0.026**	0.011	0.026**	0.010
	(0.015)	(0.010)	(0.013)	(0.010)	(0.013)
Robust *p*-value	0.220	0.029	0.549	0.023	0.594
Observations	1,224	1,248	1,005	1,262	983
Polyn. order	1	1	1	1	1
Bandwidth	2,976	3,026	2,429	3,068	2,374
Mean, left of threshold	0.024	-0.015	-0.016	-0.015	-0.013

Notes: The sample includes all districts from the main sample that can be linked between election t and *t+1* and where the winner at t runs agains at $t+1$. Column 1 takes as outcome the difference in vote share of the winner in t between election $t+1$ and t. Columns 2 to 5 take as outcomes the residuals of regressions predicting the vote share of election t's winner in $t+1$, in columns 2 and 3, and the difference between their vote share in $t+1$ and t, in columns 4 and 5. These predictive regressions use a sample restricted to observations between 8,000 and 10,000 inhabitants, in columns 2 and 4, and the entire sample, in columns 3 and 5. In districts where the incumbent does not run at $t+1$, we set their vote share at $t+1$ to 0. Other notes as in Table C1.

Table C8: 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 C9: 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 C10: 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	34,91
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 C11: 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 C12: Impact on the main outcomes - Quadratic fit - Main sample of departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	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.

Table C13: Impact on the main outcomes - Including controls - Main sample of departmental elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Incumbent		Challenger		Outsider		Victory in
	run	win	run	win	run	win	first round
Treatment	-0.069**	-0.112**	0.078**	0.048**	-0.005	0.067**	-0.113***
	(0.031)	(0.044)	(0.037)	(0.020)	(0.007)	(0.040)	(0.042)
Robust p-value	0.023	0.010	0.026	0.019	0.573	0.079	0.005
Observations	2,809	1,564	1,942	1,856	2,121	1,871	2,207
Polyn. order	1	1	1	1	1	1	1
Bandwidth	3,163	1,756	2,192	2,076	2,375	2,100	2,471
Mean, left of threshold	0.937	-0.076	-0.115	-0.051	1.046	0.907	0.231

Notes: We add as controls the sociodemographic variables shown in Appendix Table B3: the share of men in the population; the share of the population under 29 years old, between 30 and 44 years old, between 45 and 59 years old, and above 60 years old; the share of working population; the share of unemployed (among working population); and the shares of skilled workers, blue-collar workers, employees, intermediate professions, artisans, and farmers (among working population). To avoid dropping observations, for each variable, we include a dummy equal to one when the variable is missing and replace missing values by 0s. Other notes as in Table C1.

Table C14: Impact of being reimbursed in election t on running in election $t+1$ - 1992-1994 departmental elections in districts above 9,000 inhabitants and linkable with election $t+1$

	(1)
Outcome	Run next election
Treatment	0.015
	(0.032)
Robust p-value	0.601
Observations	2,408
Polyn. order	1
Bandwidth	0.018
Mean, left of threshold	0.152

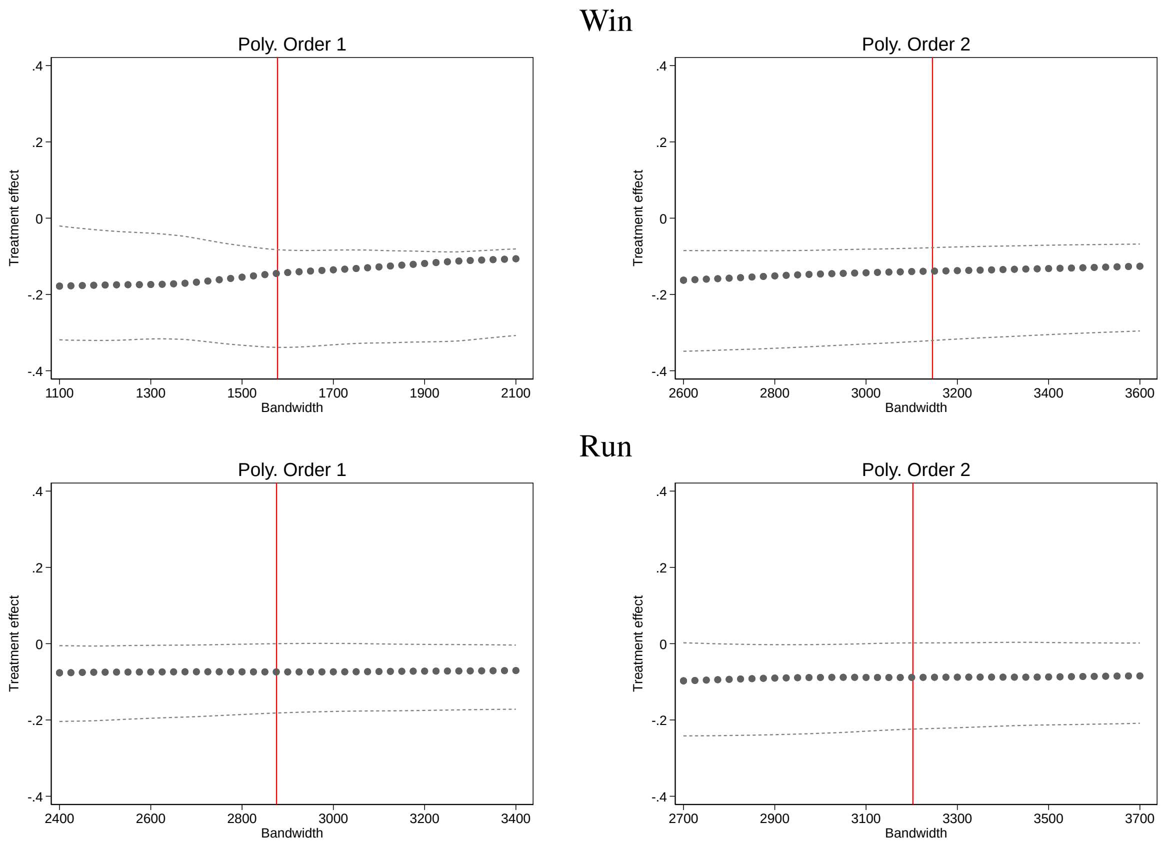
Notes: Clustered standard errors are in parentheses. Robust p-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. The column reports the results from a separate local polynomial regression. The independent variable is a dummy equal to one if the candidate running in election t obtains more than five percent of the votes. Separate polynomials are fitted on each side of the threshold. The polynomial order is one and the bandwidth is derived under the MSERD procedure. The mean indicates the mean value of the outcome of interest at the cutoff below the discontinuity. The level of analysis is the candidate and the sample only includes districts above 9,000 inhabitants in 1992 and 1994, when candidates were not yet eligible for reimbursement if they obtained more than five percent of the votes, which can be linked with election $t+1$.

Table C15: Impact of being reimbursed in election t on running in election $t+1$ - departmental elections in districts below 9,000 inhabitants and linkable between election t and $t+1$

	(1)
Outcome	Run next election
Treatment	0.017
	(0.026)
Robust p-value	0.450
Observations	2878
Polyn. order	1
Bandwidth	0.023
Mean, left of threshold	0.105

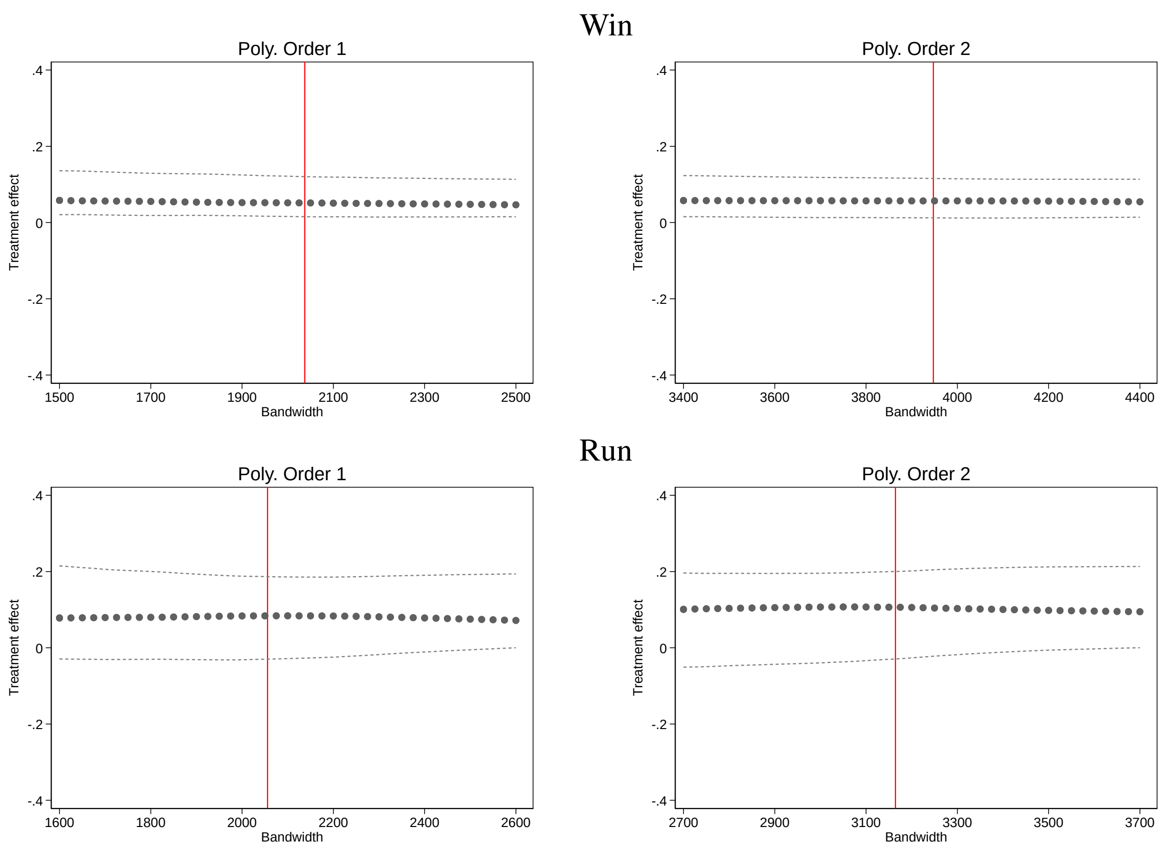
Notes: The level of analysis is the candidate and the sample only includes districts below 9,000 inhabitants in our main sample of departmental elections, which can be linked with election $t+1$. Other notes as in Table C14.

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



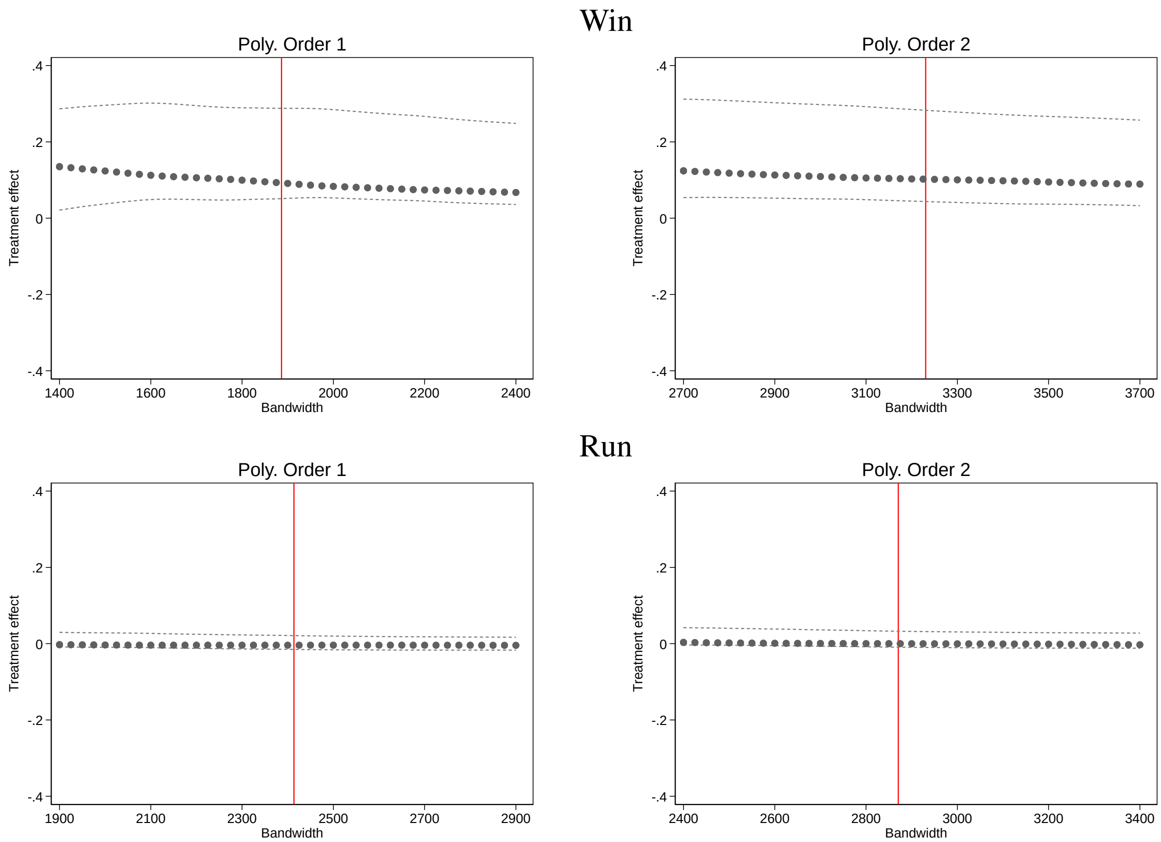
Notes: We show the sensitivity of the effect on the incumbent candidate to bandwidth choice, either using a linear (left-hand side) or quadratic specification (right-hand side). The vertical red line represents the value of the MSERD optimal bandwidth. The dots represent the estimated treatment effect 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



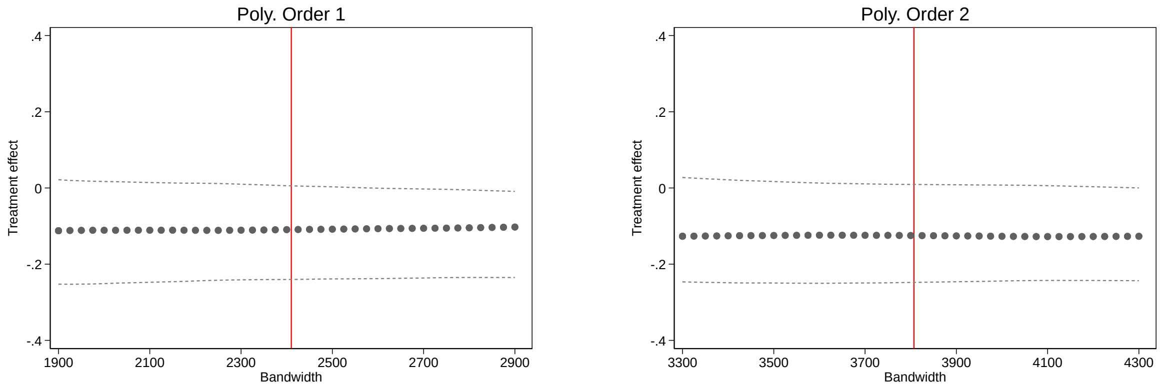
Notes: We show the sensitivity of the effect on the challenger candidate to bandwidth choice, either using a linear (left-hand side) or quadratic specification (right-hand side). The vertical red line represents the value of the MSERD optimal bandwidth. The dots represent the estimated treatment effect 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. Additional tables and figures

Table D1: Composition of candidates' campaign contributions by type of election

	% of spending ceiling		EUR per capita	
	Municipal	Departmental	Municipal	Departmental
Total expenditures	0.589	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
In-kind contributions	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 outcomes defined as a percentage of the spending ceiling in the first two columns and in euro 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. We exclude the 0.3 percent of candidates with at least one inconsistency in their contribution and expenditure data (see Appendix I).

Table D2: Impact on running - Main sample of municipal elections

	(1)	(2)	(3)
Outcome	Incumbent	Challenger	Outsider
Treatment	-0.022	0.001	-0.001
	(0.049)	(0.054)	(0.028)
Robust p-value	0.788	0.914	0.959
Observations	1,779	1,475	1,774
Polyn. order	1	1	1
Bandwidth	2,298	2,025	2,280
Mean, left of threshold	0.719	0.269	0.908

Notes: Clustered 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.

E. Validity

Table E1: Changes since election *t-1* - Municipal elections

	(1)	(2)	(3)
Outcome	Redistricted	Treated in *t-1*	Linkable
Treatment	0.004	-0.044	-0.054
	(0.008)	(0.114)	(0.031)
Robust *p*-value	0.698	0.515	0.117
Observations	1,605	418	1,006
Polyn. order	1	1	1
Bandwidth	2,001	920	1,331
Mean, left of the threshold	0.004	0.413	0.978

Notes: Clustered 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 since the same major census was in place for both the 2001 and 2008 elections.

Table E2: General balance test - Main sample of municipal elections

	(1)
Outcome	Predicted treatment
Treatment	-0.016
	(0.039)
Robust p-value	0.758
Observations	788
Polyn. order	1
Bandwidth	1,640
Mean, left of threshold	0.407

Notes: Clustered 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. The outcome is the value of the treatment predicted by sociodemographic variables: the share of men in the population; the share of the population under 29 years old, between 30 and 44 years old, between 45 and 59 years old, and above 60 years old; the share of working population; the share of unemployed (among working population); and the shares of skilled workers, blue-collar workers, employees, intermediate professions, artisans, and farmers (among working population). To avoid dropping observations, for each regressor, we include a dummy equal to one when the variable is missing and replace missing values by 0s. The independent variable is a dummy equal to one if the district has a population greater or equal to 9,000 in year t. Separate polynomials are fitted on each side of the threshold. The polynomial order is one 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 since in most districts, the population and, therefore, the running and assignment variables, were the same as in the 2001 elections.

Table E3: General balance test - All municipal elections, including non-linkable districts

	(1)
Outcome	Predicted treatment
Treatment	-0.004
	(0.036)
Robust p-value	0.988
Observations	855
Polyn. order	1
Bandwidth	1,642
Mean, left of threshold	0.386

Notes as in Table E2.

Table E4: 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	Working population	Unemployed	Skilled workers	Blue-collar	Employees	Intermediate	Artisans	Farmers
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

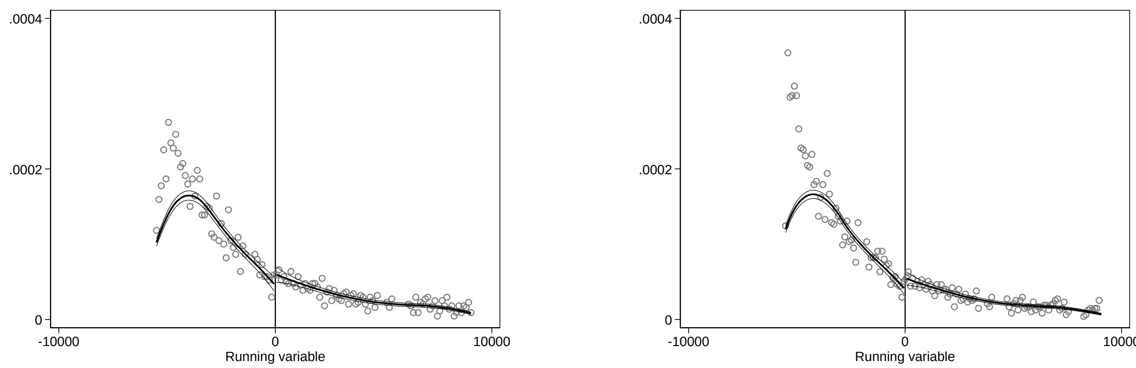
Notes: Clustered standard errors are in parentheses. Robust p-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy 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 E5: 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	Working population	Unemployed	Skilled workers	Blue-collar	Employees	Intermediate	Artisans	Farmers
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	2,14	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 E4.

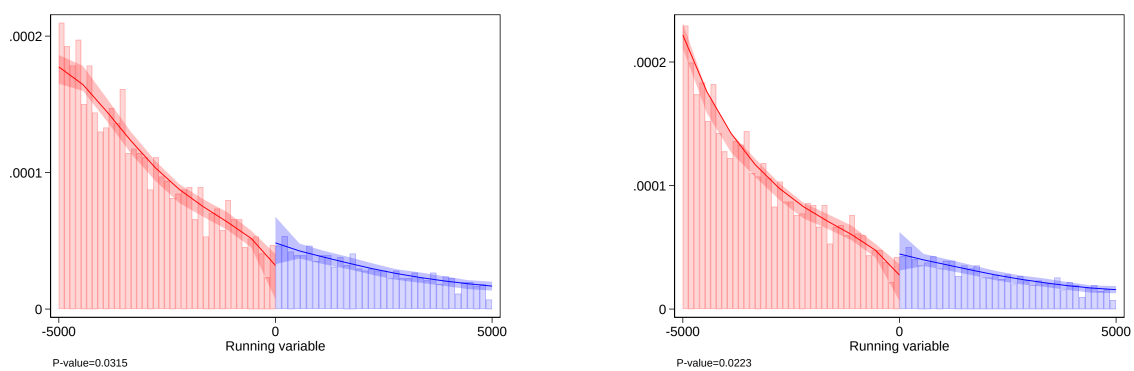
Figure E1: McCrary (2008) density test



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).

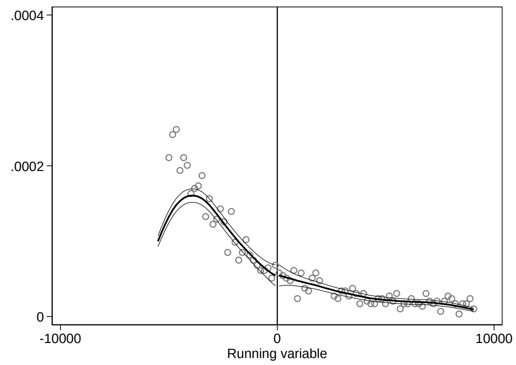
Figure E2: Cattaneo et al. (2018) density tests



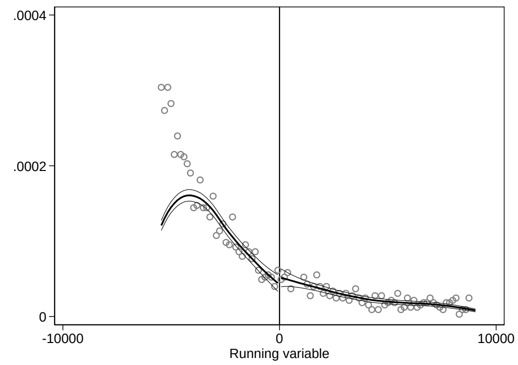
RD Density test - Main sample RD Density test - Including non-linkable districts

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).

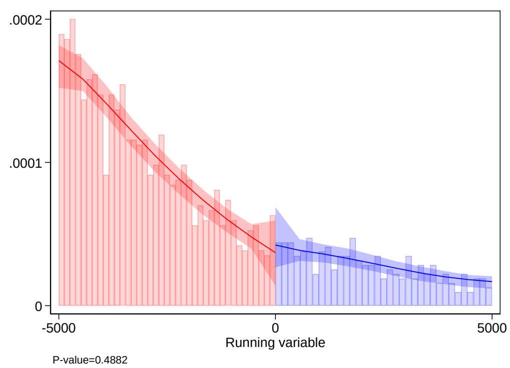
Figure E3: McCrary (2008) and Cattaneo et al. (2018) density tests - 2001 elections



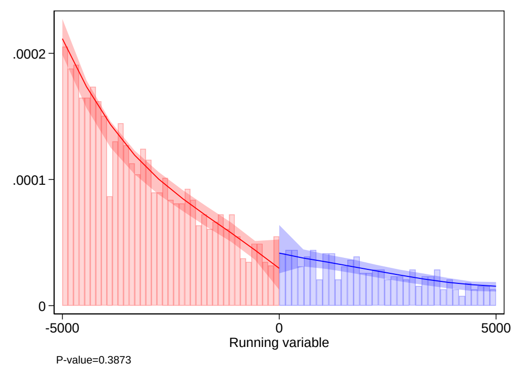
McCrary test - Main sample



McCrary test - Including non-linkable districts



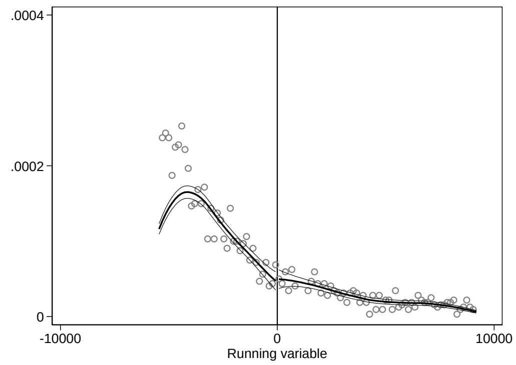
RD Density test - Main sample



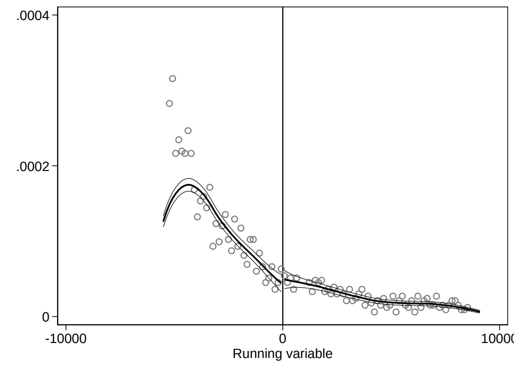
RD Density test - Including non-linkable districts

Notes as in Figure E2.

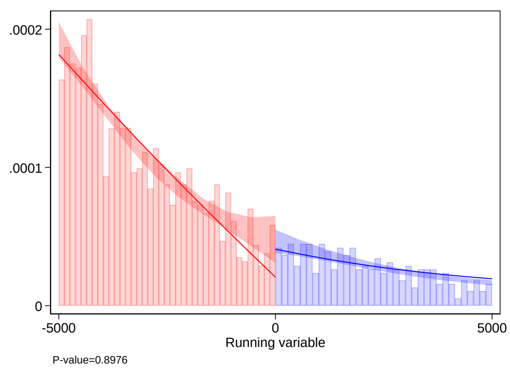
Figure E4: McCrary (2008) and Cattaneo et al. (2018) density tests - 2008 elections



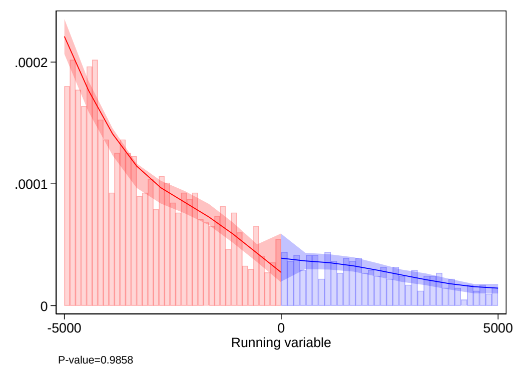
McCrary test - Main sample



McCrary test - Including non-linkable districts



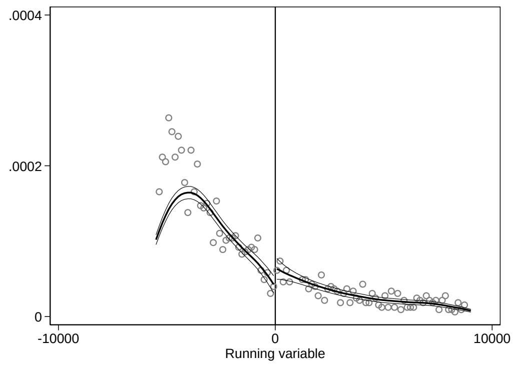
RD Density test - Main sample



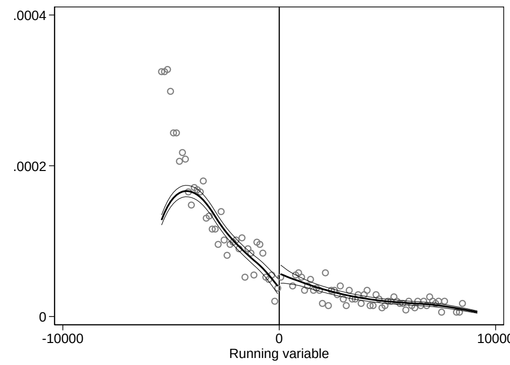
RD Density test - Including non-linkable districts

Notes as in Figure E2.

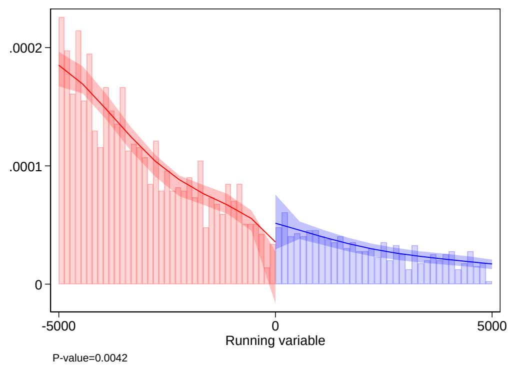
Figure E5: McCrary (2008) and Cattaneo et al. (2018) density tests - 2014 elections



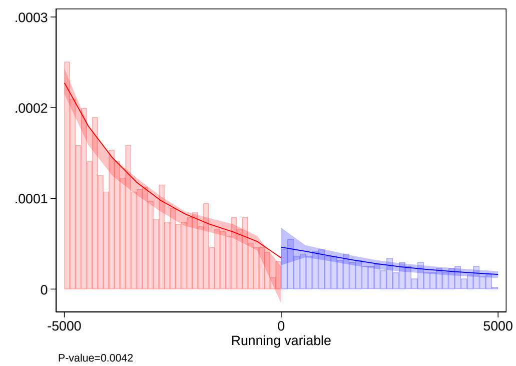
McCrary test - Main sample



McCrary test - Including non-linkable districts



RD Density test - Main sample



RD Density test - Including non-linkable districts

Notes as in Figure E2.

F. Robustness

Table F1: 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	-0.128	0.016	0.049	-0.078
	(0.208)	(0.217)	(0.143)	(0.014)	(0.165)	(0.113)
Robust *p*-value	0.901	0.555	0.327	0.266	0.759	0.469
Observations	590	669	506	342	533	362
Polyn. order	1	1	1	1	1	1
Bandwidth	2,622	2,914	2,308	1,664	2,405	1,741
Mean, left of threshold	2.772	1.631	1.139	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: Clustered 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 F2: 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	1,883	1,883	1,657	2,304
Mean, left of threshold	0.421	0.579	0.522	0.057

Notes as in Table F1.

Table F3: 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 F1.

Table F4: 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	-0.032	-0.018	-0.001	0.090	-0.038
	(0.137)	(0.130)	(0.069)	(0.008)	(0.101)	(0.056)
Robust *p*-value	0.807	0.775	0.901	0.969	0.394	0.454
Observations	1,429	1,433	2,260	1,562	1,432	1,394
Polyn. order	1	1	1	1	1	1
Bandwidth	1,801	1,916	2,807	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 F1.

Table F5: 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	-0.128	0.007	0.134	-0.122
	(0.183)	(0.217)	(0.143)	(0.012)	(0.151)	(0.101)
Robust *p*-value	0.647	0.555	0.327	0.526	0.303	0.211
Observations	760	669	506	460	625	442
Polyn. order	1	1	1	1	1	1
Bandwidth	2,874	2,914	2,308	1,881	2,446	1,817
Mean, left of threshold	2.786	1.631	1.139	0.640	2.381	0.695

Notes as in Table F1.

Table F6: 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 F1.

Table F7: 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 F1.

Table F8: Impact on running - Main sample of municipal elections - 2001

	(1)	(2)	(3)
Outcome	Incumbent	Challenger	Outsider
Treatment	-0.105	-0.179	0.074
	(0.107)	(0.117)	(0.078)
Robust p-value	0.464	0.109	0.297
Observations	360	296	405
Polyn. order	1	1	1
Bandwidth	1,734	1,484	1,918
Mean, left of threshold	0.761	0.317	0.824

Notes as in Table F1.

Table F9: 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 F1.

Table F10: 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 F1.

Appendix III: Additional information on the data and analysis

G. Political orientation, party affiliation, and polarization

The French Ministry of the Interior attributes a political label to each candidate (resp. list) running in each departmental (resp. municipal) election. To do so, it takes several indicators into account, including candidates' or lists' self-reported political affiliation, party endorsement, past candidacies, and public declarations (Granzier et al., 2023). Using these labels, we allocated each candidate and list to one of five political orientations (far-left, left, center, right, or far-right) or categorized them as "non-classified" if they could not be placed on the left-right axis. Additionally, we classified candidates and lists as "party" or "non-party," depending on whether the political label assigned by the Ministry corresponds to a party organization. To do so, we relied on the work of Pons and Tricaud (2018), Dano et al. (2022), and Granzier et al. (2023). We directly used their classifications for departmental elections, and we followed their methodology to map political labels into political orientations and the party vs. non-party dummy for municipal elections. In both municipal and departmental elections, candidates and lists belonging to a party could all be assigned an orientation such that the non-classified category is a subset of the non-party category.

To build our measure of polarization, we used the ParlGov dataset that provides information on approximately 1,700 parties across most OECD democracies (Döring and Manow, 2012; Döring et al., 2022). ParlGov reports the political label of each party and a [0-10] left-right position reflecting time-invariant unweighted mean values of expert responses on the party's positions. We merged these data with our candidates and lists using their political labels. In some cases, the labels assigned by the Ministry of the Interior corresponded to several parties in the Parlgov dataset (due to mergers of parties under a common label). Then, we gave the candidate or list the average of the Parlgov positions of the different parties corresponding to their label. Overall, we were able to assign a Parlgov left-right position to 97 percent of candidates in our main sample of departmental elections and 94 percent of lists in our main sample of municipal elections. The position is missing for independent candidates and lists running under no label or under the label "regional," corresponding to candidates and lists campaining to obtain more autonomy for their region.

Using the candidates' and lists' individual positions, we then computed our measure of polarization at the race level (see Section 4.4.2). The polarization measure is missing for the races in which at least one candidate or list has a missing left-right position, corresponding to 14 percent of the sample. Reassuringly, there is no jump at the discontinuity when we take as outcome a dummy equal to 1 if the polarization measure is missing, for both departmental and municipal elections (p-value of 0.63 and 0.88, respectively).

The tables below provide, for each election and political label, the label's political orientation, a variable indicating whether this label corresponds to a party organization, and the assigned ParlGov position.

1998 Departmental elections

Political label (code)	Political orientation	Party affiliation	ParlGov position
Communiste (COM)	Left	Yes	1.4
Divers (DIV)	Non-classified	No	
Divers Droite (DVD)	Right	No	7.7
Divers Gauche (DVG)	Left	No	3.3
Autres Écologistes (ECO)	Non-classified	No	2.5
Extrême Droite (EXD)	Far-right	No	8.8
Extrême Gauche (EXG)	Far-left	No	1.3
Front National (FRN)	Far-right	Yes	9.7
Mouvement des Citoyens (MDC)	Left	Yes	1.3
Mouvement des Radicaux de Gauche (RDG)	Left	Yes	4.1
Rassemblement pour la République (RPR)	Right	Yes	7.5
Parti Socialiste (SOC)	Left	Yes	3.2
Union pour la Démocratie Française (UDF)	Right	Yes	6.1
Les Verts (VEC)	Left	Yes	3.2

2001 Departmental elections

Political label (code)	Political orientation	Party affiliation	ParlGov position
Communiste (COM)	Left	Yes	1.4
Chasse, Pêche, Nature et Traditions (CNPT)	Right	Yes	7.8
Divers (DIV)	Non-classified	No	
Démocratie Libérale (DL)	Right	Yes	7.1
Divers Droite (DVD)	Right	No	7.7
Divers Gauche (DVG)	Left	No	3.3
Autres Écologistes (ECO)	Non-classified	No	2.5
Extrême Gauche (EXG)	Far-left	No	1.3
Front National (FN)	Far-right	Yes	9.7
Mouvement des Citoyens (MDC)	Left	Yes	1.3
Mouvement National Républicain (MNR)	Far-right	Yes	8.3
Parti Radical de Gauche (PRG)	Left	Yes	4.1
Régionalistes (REG)	Non-classified	No	
Rassemblement pour la France (RPF)	Right	Yes	7.4
Rassemblement pour la République (RPR)	Right	Yes	7.5
Parti Socialiste (SOC)	Left	Yes	3.2
Union pour la Démocratie Française (UDF)	Center	Yes	6.1
Les Verts (VEC)	Left	Yes	3.2

2004 Departmental elections

Political label (code)	Political orientation	Party affiliation	ParlGov position
Communiste (COM)	Left	Yes	1.4
Chasse, Pêche, Nature et Traditions (CNPT)	Right	Yes	7.8
Divers (DIV)	Non-classified	No	
Divers Droite (DVD)	Right	No	7.7
Divers Gauche (DVG)	Left	No	3.3
Autres Écologistes (ECO)	Non-classified	No	2.5
Extrême Droite (EXD)	Far-right	No	8.8
Extrême Gauche (EXG)	Far-left	No	1.3
Front National (FN)	Far-right	Yes	9.7
Radicaux de Gauche (RDG)	Left	Yes	4.1
Régionalistes (REG)	Non-classified	No	
Parti Socialiste (SOC)	Left	Yes	3.2
Union pour la Démocratie Française (UDF)	Center	Yes	6.1
Union pour un Mouvement Populaire (UMP)	Right	Yes	7.5
Les Verts (VEC)	Left	Yes	3.2

2008 Departmental elections

Political label (code)	Political orientation	Party affiliation	ParlGov position
Autres (AUT)	Non-classified	No	
Communiste (COM)	Left	Yes	1.4
Divers Droite (DVD)	Right	No	7.7
Divers Gauche (DVG)	Left	No	3.3
Autres Écologistes (ECO)	Non-classified	No	2.5
Extrême Droite (EXD)	Far-right	No	8.8
Extrême Gauche (EXG)	Far-left	No	1.3
Front National (FN)	Far-right	Yes	9.7
Nouveau Centre & Majorité (M-NC)	Right	Yes	6.7
Radicaux de Gauche (RDG)	Left	Yes	4.1
Régionalistes (REG)	Non-classified	No	
Parti Socialiste (SOC)	Left	Yes	3.2
Union pour la Démocratie Française (UDFD)	Center	Yes	6.1
Union pour un Mouvement Populaire (UMP)	Right	Yes	7.5
Les Verts (VEC)	Left	Yes	3.2

2011 Departmental elections

Political label (code)	Political orientation	Party affiliation	ParlGov position
Autres (AUT)	Non-classified	No	
Communiste (COM)	Left	Yes	1.4
Divers Droite (DVD)	Right	No	7.7
Divers Gauche (DVG)	Left	No	3.3
Autres Écologistes (ECO)	Non-classified	No	2.5
Extrême Droite (EXD)	Far-right	No	8.8
Extrême Gauche (EXG)	Far-left	No	1.3
Front National (FN)	Far-right	Yes	9.7
Majorité présidentielle (M)	Right	Yes	7.4
Nouveau Centre & Majorité (M-NC)	Right	Yes	6.7
Mouvement Démocrate (MODM)	Center	Yes	6.1
Parti de Gauche (PG)	Left	Yes	1.4
Radicaux de Gauche (RDG)	Left	Yes	4.1
Régionalistes (REG)	Non-classified	No	
Parti Socialiste (SOC)	Left	Yes	3.2
Union pour un Mouvement Populaire (UMP)	Right	Yes	7.5
Les Verts (VEC)	Left	Yes	3.2

2001 Municipal elections

Political label (code)	Political orientation	Party affiliation	ParlGov position
Liste Divers Droite LDD	Right	No	7.55
Liste Divers Gauche LDG	Left	No	3.3
Liste des partis politiques de Droite LDR	Right	Yes	7.4
Liste Divers LDV	Non-classified	No	
Autre Liste Écologiste LEC	Non-classified	No	2.5
Liste du Front National LFN	Far-right	Yes	9.7
Liste des partis politiques de Gauche LGA	Left	Yes	3.3
Liste du Mouvement National Républicain LMN	Far-right	Yes	8.3
Liste Non Classée LNC	Non-classified	No	
Liste Régionaliste LRG	Non-classified	No	
Liste des Verts LVE	Left	Yes	3.2
Liste d'Extrême Gauche LXG	Far-left	No	1.3

2008 Municipal elections

Political label (code)	Political orientation	Party affiliation	ParlGov position
Autre Liste (LAUT)	Non-classified	No	
Liste Centre-MoDem (LCMD)	Center	Yes	6.1
Liste du Parti Communiste (LCOM)	Left	Yes	1.4
Liste Divers Droite (LDVD)	Right	No	7.7
Liste Divers Gauche (LDVG)	Left	No	3.3
Liste d'Extrême Droite (LEXD)	Far-right	No	8.8
Liste d'Extrême Gauche (LEXG)	Far-left	No	1.3
Liste du Front National (LFN)	Far-right	Yes	9.7
Liste Gauche-Centristes (LGC)	Left	No	4.65
Liste de la Majorité (LMAJ)	Right	No	7.4
Liste Majorité-Centristes (LMC)	Right	No	6.8
Liste Régionaliste (LREG)	Non-classified	No	
Liste du Parti Socialiste (LSOC)	Left	Yes	3.2
Liste d'Union de la Gauche (LUG)	Left	Yes	3.3
Liste des Verts (LVEC)	Left	Yes	3.2

2014 Municipal elections

Political label (code)	Political orientation	Party affiliation	ParlGov position
Liste du Parti Communiste (LCOM)	Left	Yes	1.4
Liste Divers (LDIV)	Non-classified	No	
Liste Divers Droite (LDVD)	Right	No	7.7
Liste Divers Gauche (LDVG)	Left	No	3.3
Liste d'Extrême Droite (LEXD)	Far-right	No	8.8
Liste d'Extrême Gauche (LEXG)	Far-left	No	1.3
Liste Front de Gauche (LFG)	Left	Yes	1.4
Liste du Front National (LFN)	Far-right	Yes	9.7
Liste Modem (LMDM)	Center	Yes	6.1
Liste du Parti de Gauche (LPG)	Left	Yes	1.4
Liste du Parti Socialiste (LSOC)	Left	Yes	3.2
Liste Union du Centre (LUC)	Center	Yes	6.75
Liste Union de la Droite (LUD)	Right	Yes	7.4
Liste Union des Démocrates et des Indépendants (LUDI)	Right	Yes	7.4
Liste d'Union de la Gauche (LUG)	Left	Yes	3.3
Liste Union pour un Mouvement Populaire (LUMP)	Right	Yes	7.5
Liste des Verts (LVEC)	Left	Yes	3.2

H. Population data

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. The district population is used by the French National Commission on Campaign Accounts and Political Financing (CNCCFP) to determine which district is subject to the campaign regulations and to compute the spending ceiling for each district and election.

According to the guidelines of the French Ministry of the Interior, we consider the population data from the national censuses as well as information from complementary decrees that can take place between censuses when the population of a municipality has increased by at least 15 percent or following major redistrictings of cantons or municipalities (border changes, mergers, and demergers). Until 1999, national censuses took place every six to nine years, whereas since 2008, yearly national censuses have been published based on the enumeration of one fifth of the

French territory each year. Specifically, a census is published on January of every year based on data collected from year-2 to year+2, so that the most recent comprehensive census in year t is the census published in year t-3. Census data come from INSEE (the National Institute of Statistics and Economic Studies), and we obtained most information on the decrees 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).

The Ministry of the Interior also specifies which population definition to use: the "municipal population" for municipal elections, which excludes individuals having a home in the municipality but actually residing in a different one, as opposed to the "total" population that includes them; the "population without double counting" for departmental elections, which is used to compute the population of districts encompassing several municipalities to avoid counting the same person twice.

We now describe the methodology we used to recover the population of cantons and municipalities for each election year, and Table H1 summarizes the sources used by election type and year.

- Elections taking place after the 2008 census reform (2011 departmental elections and 2014 municipal elections): the guidelines indicate that we should consider the most recent official count of the municipal population that took place before the election. This corresponds to the 2008 census for the 2011 departmental election, and to the 2011 census for the 2014 municipal election. Note that we do not need to retrieve any decree since we can rely on yearly censuses. The census timing is different for some overseas territories. For the 2011 department elections, we considered the 2007 census for Mayotte; for the 2014 municipal elections, we considered the 2012 census for Mayotte and French Polynesia, the 2011 census for Saint-Pierre et Miquelon, and the 2009 census for New Caledonia.

- Elections taking place between 1999 and 2008 (2008, 2004, and 2001 departmental elections, and 2008 and 2001 municipal elections): the guidelines indicate that we should consider the population from the 1999 census, the last published census before the election, or the population established by a complementary decree taking place between 1999 and the election, if any. The Ministry identified two major redistrictings of cantons and we recovered the corresponding decrees on the website Légifrance: the redistricting of the *Rhône* département in June 2000, which affects the population measure in all three elections, and the redistricting of the *Bouches-du-Rhône* département in January 2004, which affects the population measure of the 2004 and 2008 elections. Changes in the population of municipalities are more frequent, and finding an exhaustive list of the complementary decrees proved more challenging than anticipated. To get the up-to-date municipal population, we relied on INSEE's files that indicate for each year which municipality is part of an intermunicipal community (EPCI) and that also report the up-to-date municipal yearly population. While this file proved very useful for most municipalities, it does not include municipalities in overseas territories, for which we recovered specific censuses: the 2007 census in Mayotte and French Polynesia and the 2004 census in New Caledonia. Finally, the EPCI files only provide the overall population for several large municipalities where municipal elections take place at the sub-district level (such as Paris, Lyon, and Marseille). For sub districts, as well as for Saint-Pierre-et-Miquelon overseas territory for which no additional census took place, we used the 1999 population and thoroughly searched by hand for complementary decrees taking place between 1999 and the election on the Légifrance website.

- Elections taking place before 1999 (1998, 1994, and 1992 departmental elections, and 1995 municipal elections): we could not find guidelines from the Ministry for those elections, but we assumed the same rules applied and used the same methodology as described above, taking into account the most recent census (in this case the 1990 census), as well as any supplementary decrees taking place between the census and the election. Contrary to elections taking place after 1999, the Ministry does not provide information on cantons redistrictings, and the EPCI INSEE files providing the yearly municipal populations are not available before 1999. We thus had to find a new data source. We relied on the SIRIUS website that identifies the decrees modifying the population of cantons and municipalities between 1990 and 1999 and that provides the population figures both before and after the decree. In cases where SIRIUS identifies that a decree was published but does not provide the new population, we searched for it on the Légifrance website. Another challenge came from the fact that the 1990 census provides the districts' 1990 population using their 1999 geographies. This creates an issue if the district boundaries changed between 1990 and 1999. We relied again on the SIRIUS website and proceeded as follows. If the redistricting took place between 1990 and the election, we used the population post-redistricting provided by SIRIUS (or Légifrance), as it corresponds to the most up-to-date population before the election. If the redistricting took place after the election but before 1999, we used the population pre-redistricting provided by SIRIUS that corresponds to the 1990 population at the correct geography If no redistricting took place between 1990 and 1999, we used the 1990 census population figure.

Overall, we retrieved the up-to-date population for 99 percent of cantons and municipalities. The population is missing for only 186 districts that are thus dropped from the analysis. Note that most of those missing values (132 out of 186) are concentrated in the election years before 1998 that are not part of our main sample of analysis. This is mainly due to districts disappearing between 1990 and 1999, and thus not covered in the 1990 census data expressed in 1999 geographies.

Finally, we used the data on campaign accounts from the CNCCFP to run consistency checks. Table H2 indicates the number and share of observations for which the population measure is missing or displays some inconsistencies. The following tests could be conducted for all elections for which we could retrieve the CNCCFP files, that is all elections except for the 1995 and 2001 municipal elections.

- We checked that all districts above 9,000 inhabitants according to our population variable are present in the CNCCFP files and are subject to the financing regulations (referred to as Check 1 in Table H2).
- Conversely, we checked that district below 9,000 inhabitants are not subject to the CNCCFP regulations (referred to as Check 2 in Table H2).
- We checked that the district spending ceiling computed based on our population figure is equal to the district spending ceiling reported by the CNCCFP (which is a non-linear function of the number of inhabitants). We could only conduct this test for districts above 9,000 inhabitants that are subject to campaign regulations. While we do not have comprehensive account data for the 2001 municipal election, we also ran this test on a random sample of 100 districts among the subset of districts for which booklets were available (referred to as Check 3 in Table H2).

When we discovered a discrepancy, we accessed alternative sources to double check our data. In particular, we noticed mistakes in the 2001 EPCI files used to determine the 2001 municipal population, due to missing decrees published between the 1999 census and the 2001 election. We used an alternative file from data.gouv that provides the 2001 municipal election results (aggregated by political label) and that contains the 2001 municipal population. More generally, these tests helped us identify additional decrees that our main sources missed.

After checking alternative sources and making sure that no other decree went unoticed, some inconsistencies remained with respect to the spending ceiling (check 3) for about 2 percent of our sample. Further investigations led us to the conclusion that most of these inconsistencies reflect errors in the CNCCFP computation of the spending ceiling (e.g., use of the "total" population instead of the "municipal" population, incorrect inflation correction coefficient, or use of a census that is not the most recent one). In particular, a thorough investigation of the 2008 departmental and municipal elections that display a relatively high share of discrepancies revealed that more than 90 percent of them could be explained by such mistakes.

Table H1: Data sources used to determine population by election type and year

	Main sources used
Municipal elections	
1995	INSEE 1990 census in 1999 geographies SIRIUS Légifrance
2001; 2008	INSEE 1999 census INSEE complementary censuses for overseas territories Légifrance INSEE EPCI 2001 & 2008 data.gouv 2001 municipal election results
2014	INSEE 2011 census INSEE complementary censuses for overseas territories
Departmental elections	
1992; 1994; 1998	INSEE 1990 census in 1999 geographies (provided by Réseau Quételet) SIRIUS Légifrance
2001, 2004, 2008	INSEE 1999 census INSEE complementary census for Mayotte Légifrance
2011	INSEE 2008 census INSEE complementary census for Mayotte

Notes: This table indicates the main sources used to determine the up-to-date districts' population, by election type and year.

Table H2: Number and share of observations with missing or inconsistent population

	Check 1	Check 2	Check 3	Missing data	# elections	# elections with pop≥9k
denominator of term (%)	*Districts above 9k*	*Districts below 9k*	*Districts above 9k*	*All districts*		*All districts*
Municipal elections						
1995	NA	NA	NA	46	2,481	988
				2%		*40%*
2001	NA	NA	NA	4	2,697	1,025
				0%		*38%*
2008	0	0	33	5	2,859	1,070
	0%	*0%*	*3%*	*0%*		*37%*
2014	0	0	1	0	3,048	1,127
	0%	*0%*	*0%*	*0%*		*37%*
Total municipal elections	0	0	34	55	11,085	4,210
(%)	*0%*	*0%*	*1%*	*0%*		*38%*
Departmental elections						
1992	2	0	20	44	1,980	1,177
	0%	*0%*	*2%*	*2%*		*59%*
1994	0	0	36	42	1,957	1,149
	0%	*0%*	*3%*	*2%*		*59%*
1998	0	0	7	45	1,993	1,190
	0%	*0%*	*1%*	*2%*		*60%*
2001	0	0	1	0	2,011	1,215
	0%	*0%*	*0%*	*0%*		*60%*
2004	4	0	5	0	2,034	1,228
	0%	*0%*	*0%*	*0%*		*60%*
2008	1	0	112	0	2,020	1,222
	0%	*0%*	*9%*	*0%*		*60%*
2011	2	0	1	0	2,026	1,264
	0%	*0%*	*0%*	*0%*		*62%*
Total departmental elections	9	0	182	131	14,021	8,445
(%)	*0%*	*0%*	*2%*	*1%*		*60%*
All elections pooled	9	0	216	186	25,106	12,655
(%)	*0%*	*0%*	*2%*	*1%*		*50%*

Notes: For a given election, the first row provides the number of districts with an inconsistency or a missing value, while the second row shows the percentage this represents out of the sample of districts on which the test is conducted. See the main text for a description of the three consistency checks.

I. Expenditure and contribution data

Data on candidates' and lists' expenditures and contributions come from the French National Commission on Campaign Accounts and Political Financing (CNCCFP). We collected data directly on the Commission's website for the 2008 and 2011 departmental elections and the 2008 and 2014 municipal elections (http://www.cnccfp.fr/index.php?art=584). For the 1992, 1994, 1998, 2001, and 2004 departmental elections, we digitized the data from printed booklets made available by the CNCCFP. The data are missing for the 2001 municipal election, as the CNCCFP could not provide us with all the necessary booklets.

Data are only available for districts above 9,000 inhabitants, where campaign regulations apply and candidates thus have to submit their campaign accounts. Above the threshold, we know the campaign expenditures and contributions of 97.2 percent of all candidates and lists. The remaining 2.8 percent are candidates and lists who were not required to submit their accounts because they received less than 1 percent of the candidate votes in the first round and did not get any private donations, or candidates and lists who violated the rule and did not submit their account on time.

For all elections, we observe candidates' and lists' total expenditures, total contributions, account balance, and district level expenditure ceilings. Additionally, we observe the breakdown of contributions between party contributions, private donations, the candidates' and lists' personal contributions,[26] in-kind contributions, and "other contributions." The CNCCFP reports a single value for each variable, corresponding to the total amount spent or received over the entire campaign. When a second round takes place, the amounts in the first and second rounds are added up, preventing us from tracking changes in contribution and expenditure patterns between rounds.[27] To ensure comparability across districts and years, we converted data expressed in francs for years prior to 2002 and data expressed in francs CFP for districts in French Polynesia and New Caledonia into euros.

Finally, we ran the following quality checks at the candidate or list level:

- We checked that the sum of the contribution items adds up to the total contributions.
- We checked that the sum of the personal contribution items adds up to the total personal contributions. We could only run this test for the 1998, 2001, and 2004 departmental elections, for which we observe the breakdown of personal contributions.

[26]In the 1998, 2001, and 2004 departmental elections, personal contributions are further broken down into own contributions, loans, and unpaid expenses.

[27]The only variable changing across rounds is the expenditure ceiling in municipal elections that is loosened between the two rounds. We thus collected the expenditure ceilings both in the first and second rounds for the 2008 and 2014 municipal elections. The ceiling does not change between rounds for departmental elections.

- We checked that the reported account balance is equal to the total contributions *minus* the total expenditures.
- We checked that the account balance is not abnormally large (above 1,000,000 euros).
- We checked that the total contributions *minus* the available individual contribution items (thus corresponding to "other contributions") is not negative.
- We checked that the acount balance is not negative.
- We checked that the total expenditure declared by the candidate *plus* the corrections made by the CNCCFP add up to the witheld total expenditures amount. We could only run this test for the 1992 departmental elections as this is the only election for which the CNCCFP provides such a breakdown in candidates' expenditures.

In total, 127 of the 42,447 candidates in our main sample for which we have expenditure data have at least one inconsistency (0.3 percent), and 1.5 percent of the districts have at least one candidate with some inconsistencies. We exclude the 0.3 percent of candidates with at least one inconsistency (resp. the 1.5 percent of districts with at least one candidate with some inconsistencies) from our candidate level (resp. district level) analyses and descriptive statistics that rely on expenditure and contribution data in Section 6.

J. Effects on winning conditional on running: derivation of the bounds

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 they would have run again in districts below, and conversely.

We then 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 $(R = 0)$, they do not win $(W = 0)$, and we do not observe W had they run. If the incumbent runs in a district above 9,000 inhabitants, we observe whether they win the election but do not know if they 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 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 they 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 and obtain the 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 and obtain the 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 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.

K. Predictors of $t+1$ vote shares

The variables used to predict the vote share of election t's winner at election $t+1$ are as follows:

- Year and département fixed effects
- Variables linked to election t's winner:
  - their vote share in t and $t-1$ (set to 0 if they did not run in $t-1$)
  - a dummy indicating if they ran in $t-1$
  - dummies indicating if they were the incumbent, the challenger, an outsider, a woman, a non-party candidate, if they ran for the left, the far-left, the center, the far-right, the right in election t
  - the aggregate vote share of their orientation in the first round of elections t and $t-1$ (set equal to the individual winner's vote share if they are non-classified)
  - the number of candidates of their orientation in election t (equal to 1 if they are non-classified)
  - the difference in the average vote share of their orientation between t and $t+1$ (equal to 0 if they are non-classified).
- Electoral outcomes at t and $t-1$:
  - dummies indicating if the top two candidates were of the same orientation, if only one candidate ran, if the election was won in the first round
  - the number and the effective number of candidates, turnout, the share of blank and null votes, polarization in the first round, the margin of victory between the winner and the runner-up, the aggregate vote share in the first round of each orientation except for non-classified candidates

  - the difference in the vote share of election $t-1$'s winner between t and $t-1$ (set to 0 if they do not run again)
  - a dummy indicating if election $t-1$'s winner runs in t
  - dummies indicating if election $t-1$'s winner was far-left, left, center, right, and far-right.

- Sociodemographic variables at t and $t-1$:
  - the share of men in the population
  - the share of the population under 29 years old, between 30 and 44 years old, between 45 and 59 years old, and above 60 years old
  - the share of working population
  - the share of unemployed (among working population)
  - the shares of skilled workers, blue-collar workers, employees, intermediate professions, artisans, and farmers (among working population).

The variables used to predict the difference between the vote share of election t's winner at $t+1$ and t are the same excluding the vote share of the winner in t

To avoid dropping observations, for each regressor, we include a dummy equal to one when the variable is missing and replace missing values by 0s.