

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 local 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 higher spending, decreasing marginal returns of campaign money, and 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 signal their quality and inform voters about their platform (Prat, 2002a). 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 (Baron, 1994; Grossman and Helpman, 1994; Prat, 2002b; Chamon and Kaplan, 2013). Absent campaign finance rules leveling the playing field, outsider candidates may not have access to the same resources as incumbents even if they are of high quality (Stratmann, 2005; Iaryczower and Mattozzi, 2012).

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 (Fouirnaies, 2021; Gulzar et al., 2022; Avis et al., 2022). However, we lack evidence on rules which go one step further and provide for the reimbursement of campaign expenditures by the state. While such rules generate an obvious burden for the public budget, they might further equalize 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 departmental and municipal election candidates competing in 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.

We make three main contributions. Using the population threshold, we causally estimate the joint impact of spending limits and the reimbursement of campaign expenditures in departmental elections held after 1995. Next, we disentangle the contribution of spending limits and the

reimbursement of candidate expenditures to these effects. Finally, we shed light on the contexts in which campaign finance rules affect electoral outcomes the most by comparing the effects in departmental and municipal elections.

We find that campaign finance rules do level the playing field between incumbents and other candidates in departmental elections. Incumbents experience a sharp decline in their reelection rate, increasing the likelihood of electoral turnovers. The campaign finance rules reduce the incumbent's reelection probability by 14.5 percentage points and increase the chances of winning of the previous election's runner-up by 5.2 percentage points and the likelihood of a victory by a candidate who was not present in that election by 9.3 percentage points.

These results are driven by two effects. First, the rules increase the competitiveness of the race and decrease the incumbent's vote share and chances of winning conditional on participating in the race by 3.0 to 7.6 percentage points and 10.5 to 18.9 percentage points, respectively, with opposite effects for the previous election's runner-up.

Second, candidates may change their decision to enter the race if they anticipate these effects. We use a simple conceptual framework to discuss how candidates are likely to balance their expected benefits from competing with the reduced cost, resulting from the reimbursement of their expenditures. On net, incumbents' lower odds of winning dominate, decreasing their probability of running for reelection by 7.4 percentage points. Previous runner-ups, for whom campaign finance rules both increase expected benefits and reduce the cost, become more likely to run again, by 8.4 percentage points. The net expected effect on the entry of smaller candidates is more ambiguous, since the tightening of the race between the main contenders may reduce the attention others get, their vote share, and their consumption value of participating in the election. Overall, we find null effects on the number of new entrants and on the total number of candidates.

Campaign finance rules do 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, they increase 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 rules since they receive fewer private donations than right-wing candidates and contribute less of their own money to their campaign beforehand.

The second part of our analysis disentangles the contribution of spending limits and the reimbursement of candidate expenditures to these effects. We first 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. We then exploit the 1992 and 1994 departmental elections, which were held after expenditure ceilings were introduced but before campaign expenditures started to be reimbursed, unlike the elections after 1995 which constitute our main sample.

We do not find any effect in this secondary sample of elections, suggesting that our main effects are primarily driven by the reimbursement of candidates.

Beyond introducing public reimbursement, the 1995 reform also tightened spending limits and banned corporate donations. However, we show that our results hold in districts where these other regulatory changes were least likely to be binding. Furthermore, data on candidates' contributions and expenditures above the threshold only show modest bunching at 100 percent of the spending ceiling both before and after 1995. This suggests that the spending limit was not binding before 1995, explaining the null effects found in the corresponding elections, and that it did not become more binding after 1995. By contrast, we observe large increases in total expenditures and personal contributions after the 1995 reform, with bunching of both distributions at the reimbursement threshold (50 percent of the ceiling). We also observe a disproportional increase in the personal contributions and the spending to ceiling ratio for the competitors of the incumbent as well as for left-wing candidates, who are the ones benefiting electorally from campaign finance rules. These different pieces of evidence all support the same conclusion: our main effects are primarily driven by the reimbursement of campaign expenditures.

The third part of our analysis asks when campaign finance rules affect electoral outcomes the most. We first consider how the effects vary depending on the baseline level of competition in the district. In departmental elections, we find larger effects in races of intermediate closeness, where there is scope to level the playing field, than in stronghold districts and in districts that are already very competitive *ex ante*. We then turn to municipal elections where, in contrast to departmental elections, we do not find any significant effect despite the campaign finance rules being the same. We attribute these null effects to the list format used in municipal elections: while departmental election candidates run in single-member constituencies, municipal candidates can split campaign costs with the other members of their list, so receiving public funding may make less of a difference for them. 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 – we provide suggestive evidence that this is indeed the case.¹

Our results contribute to a burgeoning literature using quasi-experimental evidence to estimate the effects of campaign finance rules. Fourinaies (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), Masket and Miller (2015),

¹Section 7.2 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)

²Griffith and Noonan (2022) study a different form of public funding: the distribution of vouchers which voters

and Kilborn and Vishwanath (2022) exploit the fact that some U.S. states offer public funding to candidates respecting pre-set spending limits to measure effects on electoral competitiveness, representativeness, 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 literature measuring the impact of campaign money on vote shares (e.g., Jacobson, 1978; Abramowitz, 1988; Levitt, 1994; Gerber, 1998, 2004; Ben-Bassat et al., 2015; François et al., 2022; Bekkouche et al., 2022). While we do not provide direct evidence on that relationship, the effects that we observe on electoral outcomes would be difficult to understand if they were not mediated by the changes in the amount of money spent by different types of candidates. In fact, the candidates whose likelihood of winning increases the most following the campaign finance rules – runner-ups vs. incumbents, and candidates on the left vs. on the right – are also those whose relative spending increases the most after the introduction of reimbursements, in departmental elections. In municipal elections, our null effects may be explained by a lower return of money on votes.

Finally, 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 presents our conceptual framework. Section 3 introduces our research setting, and Section 4 describes our empirical strategy. Section 5 provides the main results, focusing on departmental elections. Section 6 disentangles the role of spending limits and reimbursements, while Section 7 investigates the contexts in which campaign finance rules matter the most. Section 8 concludes.

can donate to their candidate of choice.

2 Conceptual framework

We provide a simple conceptual framework to guide the empirical analysis.

Setup

Each candidate i competing in a race faces a monetary cost M_i , corresponding to their campaign expenditures, and derives two types of benefits. R_i is the (consumption) value of being present in the race, whether or not the candidate wins. This benefit captures the value of defending one's ideas and gaining notoriety, net from the opportunity cost of the time spent campaigning. R_i increases with the candidate's vote share, as their visibility grows with electoral returns. B_i is the (instrumental) benefit of winning and being elected to office, which the candidate obtains with probability p_i . p_i , the candidate's probability of winning, depends on the set of candidates present in the race, \mathbb{C} , on the amount of money spent by the candidate, M_i , and on the amount spent by their competitors, M_{-i} . It can thus be written as $p_i(\mathbb{C}, M_i, M_{-i})$. For a given set of candidates, p_i increases with M_i and decreases with M_{-i} . The set of candidates \mathbb{C} who choose to enter the race as well as their level of campaign expenditures M are endogenous: they depend on other candidates' choices and on expected probabilities of winning.

In the paper, we study the joint impact of two types of campaign finance regulations. Campaign spending limits cap M_i , and the reimbursement of campaign expenditures generates a subsidy S_i conditional on the candidate's vote share being above a certain threshold (five percent, in the French case). We write the probability of passing that threshold as $q_i(\mathbb{C}, M_i, M_{-i})$.

Campaign finance rules affect both candidates' entry decision and the outcome of the race.

Effects on vote shares and on the outcome of the race

For a given set of candidates, campaign finance rules first affect candidates' campaign expenditures, their vote shares, and their probabilities of winning: using superscripts 1 and 2 to designate the states of the world without and with campaign finance rules, $M_i^1 \neq M_i^2$, and $p_i^1 \neq p_i^2$. We expect the rules to level the playing field between candidates with better access to external funding (e.g., from their party or from private donations) and their competitors. In particular, the rules will likely diminish the advantage of incumbents if holding office makes it otherwise easier for them to raise money in the next election (Ashworth, 2006; Meirowitz, 2008; Pastine and Pastine, 2012; Fourinaies and Hall, 2014; Holbrook and Weinschenk, 2014). Denoting the incumbent with I and a competitor with C , we expect the campaign finance rules to decrease the gap between the amount of money they spend ($M_I^2 - M_C^2 < M_I^1 - M_C^1$); to decrease the difference between their respective vote shares and probabilities of winning ($p_I^2 - p_C^2 < p_I^1 - p_C^1$); and to increase the closeness of the race.

Effects on entry decisions

Second, candidates anticipate these effects, which affects their decision to enter the race: $\mathbb{C}^1 \neq \mathbb{C}^2$. Candidates decide whether or not to enter the race based on the following calculation. Absent campaign finance rules, candidate i enters if $R_i^1 + p_i^1 B_i > M_i^1$. With campaign finance rules, they enter the race if $R_i^2 + p_i^2 B_i > q_i^2 (M_i^2 - S_i) + (1 - q_i^2) M_i^2$. In order to make predictions about the effects of campaign finance rules on candidates' entry decisions, it is useful to distinguish two types of potential candidates.

Big candidates (type A) are certain to clear the reimbursement threshold ($q_i^2 = 1$) and they have a chance of winning ($p_i^1, p_i^2 > 0$). The campaign finance rules can be expected to decrease these candidates' cost of running: $M_i^2 - S_i < M_i^1$, which may increase their likelihood of entering the race. Indeed, all are certain to receive the subsidy S_i . Moreover, due to the spending limit, candidates who would have spent more than the limit are forced to spend less, and their competitors who anticipate this may choose to spend less themselves accordingly ($M_i^2 < M_i^1$). By contrast, the effects on the benefits of entering the race differ across candidates. As discussed above, candidates who have better access to external funding, such as incumbents, see their advantage diminish and may thus expect their vote share to decrease, lowering both their chance of winning ($p_i^2 < p_i^1$) and their consumption value of participating in the race ($R_i^2 < R_i^1$). The net effect of the campaign rules on their likelihood to enter the race is thus ambiguous. Instead, candidates with worse access to funding see their benefits of running increase. Together with the reduced cost of running, this should increase their likelihood of entering the race.

Small candidates (type B) do not have any chance of winning ($p_i^1 = p_i^2 = 0$), so their decision to enter the race only depends on the consumption value of participating in the race and the associated cost. Similarly as for big candidates, the campaign finance rules decrease these candidates' cost of running: $q_i^2 (M_i^2 - S_i) + (1 - q_i^2) M_i^2 < M_i^1$. Indeed, these candidates receive the subsidy S_i with probability q_i^2 . Furthermore, they may decide to spend less money ($M_i^2 < M_i^1$) since the campaign expenditures of bigger candidates are now capped. On the other hand, the effect of the campaign finance rules on these candidates' consumption value of competing R is ambiguous and may be negative: While leveling campaign expenditures across candidates may increase the vote share of smaller ones, the campaign finance rules may also increase race closeness between the main contenders and induce voters to strategically rally them (e.g., Kawai and Watanabe, 2013). This would reduce the vote share of smaller candidates, the media attention they get, and, thus, R_i ($R_i^2 < R_i^1$). On net, the overall effect of the campaign finance rules on the entry of small candidates is thus ambiguous. It is more likely to be negative for candidates who anticipate that they are unlikely to reach the five percent subsidy threshold, since their costs of running will remain largely unchanged while their visibility may decrease.

3 Research setting

3.1 Campaign finance rules in France

Many Western democracies started regulating campaign finance in the 1960s (Alexander and Felderman, 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.³ 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.

We consider two reforms of French campaign rules which took place in 1990 and 1995, respectively. The 1990 law 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).⁴ Accordingly, we have comprehensive data on candidate spending above the threshold.

The 1995 law 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,⁵ provided they obtain more than five

³See <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>.

⁴This rule was modified in 2011 such that only candidates obtaining more than one percent of the votes have to submit this information.

⁵The maximum reimbursement was reduced to 47.5 percent in 2011.

percent of the candidate votes (valid votes cast for a candidate, as opposed to blank and null votes) in the first round.⁶ 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.⁷

Our main analysis focuses on departmental elections taking place after 1995. We thus estimate the combined impact of reimbursement and spending limits, since both vary at the 9,000 inhabitants threshold. In districts below the population cutoff, candidates face no spending limit, they do not have to disclose their accounts to the CNCCFP, and they are not eligible for reimbursement. Unless otherwise specified, when we allude to the impact of “campaign finance rules” in the rest of the paper, we refer to the joint impact of spending limits and reimbursement. In Section 6, 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 regulations.⁸ Appendix Figure A1 provides a timeline showing the timing of the two laws and the election years used in the analysis.

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., 2024). 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.

3.2 French departmental elections

Our analysis focuses on departmental elections. These 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

⁶Before 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.

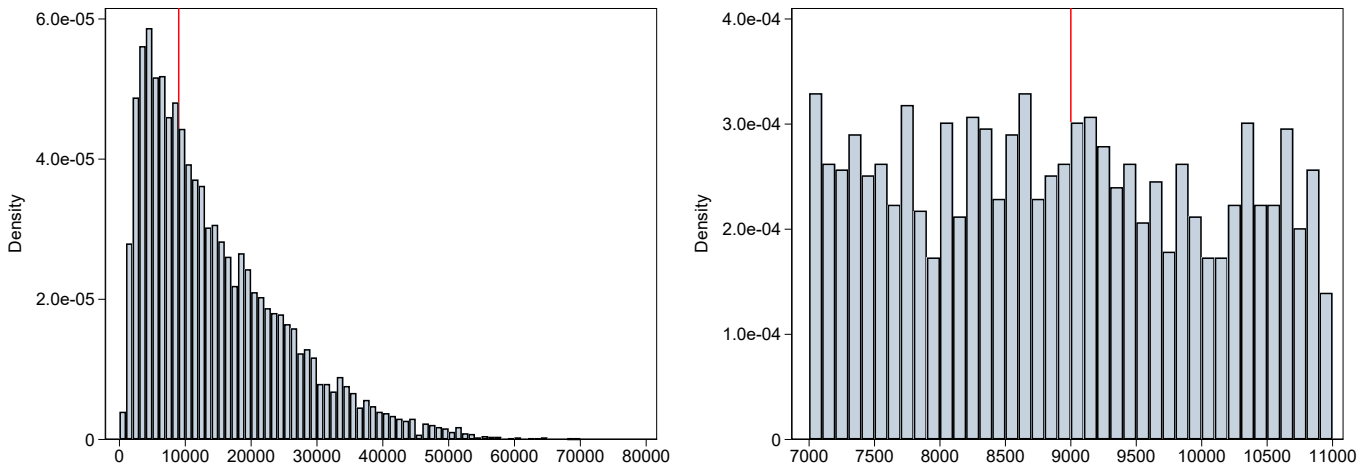
⁷The spending limit is a step-wise function of the district population. Districts above 9,000 inhabitants (in which the spending limit applies) are divided into seven population brackets. In each bracket, a coefficient multiplies the number of inhabitants to determine the spending limit. The 1995 law tightened the spending limits by reducing those coefficients, while keeping the same population brackets.

⁸Section 6 also investigates the role of the ban on corporate donations introduced at the same time as reimbursement by the 1995 law, and provides evidence that this ban is unlikely to explain the results.

for at least 25 percent of the registered citizens. If no majority is obtained in the first round, the top-two candidates and all other candidates above a certain vote share threshold qualify for the second round. The qualification threshold was 10 percent of registered citizens until 2011, and 12.5 percent afterwards. The second round takes place a week later and uses plurality voting: the candidate receiving the most votes is elected. There is no term limit. Until a 2013 reform, each canton elected one representative for a length of six years, and half of the seats were up for election every three years. There were a total of 4,035 cantons, with populations ranging from 270 to 69,335 inhabitants. The reform of 2013 aligned the calendar of all elections, it homogenized cantons' size within departments, cut the number of cantons in half, and led to the redistricting of all cantons' boundaries. Post reform, the population of 98 percent of the cantons was above the 9,000 inhabitants threshold. Therefore, we do not use departmental elections which took place after the reform.⁹

Figure 1 shows the population distributions of cantons, pooling the five election years we consider in the main analysis (1998, 2001, 2004, 2008, and 2011). The left-hand side graph considers all cantons, while the right-hand side graph focuses on districts close to the threshold. Reassuringly, we do not see any specific patterns around the 9,000 inhabitants cutoff, and Section 4.4 further provides formal evidence of the absence of manipulation.

Figure 1: Population distributions of cantons



Notes: The vertical red line corresponds to the 9,000 inhabitants threshold. The left-hand side graph considers all districts, while the right-hand side graph focuses on districts close to the threshold, between 7,000 and 11,000 inhabitants.

⁹The 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.

4 Empirical strategy

4.1 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 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:

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

where $Y_{i,t}$ is the outcome in district i and election year t , $X_{i,t}$ is the running variable, defined as the district population centered around 9,000 inhabitants, and $D_{i,t}$ is the assignment variable, a dummy taking value one for districts with 9,000 inhabitants or more (i.e., if $X_{i,t}$ is positive). The parameter of interest, τ , captures the causal impact of campaign finance rules.

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.¹⁰ We follow the optimal MSERD algorithm proposed by Calonico et al. (2014) to construct the bandwidths. The bandwidths differ across outcomes since they are selected based on the data. 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.

¹⁰In Appendix Tables C11 and C12, 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.

4.2 Data and definitions

Electoral results come from the Ministry of the Interior. 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). 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 label. 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 combining 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. These data do not exist for districts below the threshold, where candidates do not need to report their revenues and expenditures to the CNCCFP. While we cannot use our RDD to measure effects on these outcomes, we do provide evidence on the spending patterns of different types of candidates above the threshold and on the changes which followed the introduction of campaign expenditures’ reimbursement in Section 6.3. See Appendix I for a detailed discussion of the contribution and expenditure data and of the quality

checks we conducted on them.

4.3 Sampling frame

Our main sample includes the 1998, 2001, 2004, 2008, and 2011 departmental elections.¹¹

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 census for the 2011 departmental elections. Importantly, except for the 2008 departmental elections, each election was preceded by a different national census, leading to changes in all districts' official population.¹² Therefore, our estimates generally capture the impact of being treated once. The 2008 departmental elections are an exception: in most districts, the population and, therefore, the running and assignment variables, were the same as in the 2001 departmental elections. Therefore, we do not use the 2008 elections for the internal validity tests (described below in Section 4.4), 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 Appendix C.

We check the consistency of all election results, and drop one race in the 2001 departmental elections, for which we detect inconsistencies.¹³ 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 $t-1$.¹⁴

Reassuringly, districts above the discontinuity are not more likely to be linkable with the last election than those below, as shown in Appendix Table B1 (column 1). In Appendix C, 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

¹¹We also use data from the 1992 and 1994 departmental elections to define incumbents, challengers, and outsider candidates in the first elections in the sample (namely, the 1998 and 2001 departmental elections).

¹²The 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.

¹³We 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; 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.

¹⁴Overall, we detect inconsistencies in the $t-1$ election for one departmental race (corresponding to that 2001 race with inconsistencies).

without linking elections over time.

Overall, our main sample includes 9,938 linkable departmental races (52,651 candidates).¹⁵ Table 1 gives summary statistics for our main sample of analysis. 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 of the candidates.

Beyond our main sample, we use the 1992 and 1994 departmental election results when exploring the mechanisms driving our results, in Section 6. These elections help us disentangle the contribution of spending limits and candidate expenditures' reimbursement since the former was implemented before these elections but the latter afterward.¹⁶

Table 1: Summary statistics

	Mean	S.D.	Min.	Max.	Observations
Number of inhabitants	14,421	10,818	270	69,335	9,938
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

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.

4.4 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, no other voting rule or institutional feature changes at this threshold. In fact, in de-

¹⁵When we add non-linkable elections, our sample includes 10,083 departmental races (53,600 candidates).

¹⁶We also use data from the 1985 and 1988 departmental elections to define incumbents, challengers, and outsider candidates in the 1992 and 1994 elections.

partmental elections, no other policy than the campaign finance rules is determined based on a population threshold.¹⁷

Second, districts cannot sort at the threshold. Indeed, the centralized nature of French censuses leaves no room for the manipulation of population figures by local politicians. Furthermore, new official population counts occurring between censuses, due to redistricting, are established by independent administrators, preventing manipulation by elected officials.

Third, we conduct a large number of validity tests, as well as falsification and robustness tests to provide empirical support for our identification strategy. We list these tests below and present the corresponding tables and figures in Appendix B (for the validity tests) and Appendix C (for the falsification and robustness tests).

Validity tests. First, we make sure that the likelihood of experiencing a redistricting between elections $t-1$ and t or of having been treated at $t-1$ does not jump at the threshold (Appendix Table B1). Such discontinuities could otherwise suggest that incumbents are able to manipulate their population to benefit from the campaign finance regime that they like the most. Second, Appendix Figure B1 provides a broader test of manipulation by checking that there is no jump in the density of the running variable at the threshold (McCrary, 2008; Cattaneo et al., 2018).¹⁸ 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 show that this predicted value does not jump at the threshold (Appendix Table B2 and Figure B2). Fourth, we also show balance tests on each of these sociodemographic variables taken individually (Appendix Table B4 and Figure B3).¹⁹ Fifth, we check that outcomes defined at election $t-1$ do not jump at the threshold either (Appendix Table B6 and Figure B4).

Falsification and robustness tests. We first evaluate (and reject) the possibility that our main results 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 (Appendix Tables C9 to C11).²⁰ Second, we check the robustness of

¹⁷Eggers et al. (2018) provide a list of other policies (affecting for instance the salary of the mayor or the number of municipal councilors) that change at some population threshold in French *municipalities*. These policies are only relevant for our analysis of municipal elections performed in Section 7.2, and none of them changes at the 9,000 inhabitants threshold.

¹⁸The 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 a p -value of 1.00.

¹⁹Only 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. The individual and general balance tests yield similar results when we add non-linkable districts (Appendix Tables B3 and B5).

²⁰The number of significant results is not higher than would be expected: eight out of 70 point estimates are

our results to employing a quadratic specification and to controlling for all the sociodemographic variables used in the general balance test (Appendix Tables C12 and C13). Third, we assess the sensitivity of the results to bandwidth selection. For each outcome of interest, we plot the point estimates and associated 5 percent robust confidence intervals for bandwidths ranging from plus to minus 1,000 inhabitants around the data-driven bandwidth selected based on Calonico et al. (2014), using either a linear or a quadratic specification (Appendix Figures C1 to C3). We also replicate our analyses using a small bandwidth of 1,000 inhabitants for all our main outcomes (Appendix Table C14). Fourth, Appendix Figures C4 to C6 and Tables C15 to C17 show the results of donut estimations to make sure that our results are not driven by observations right at the threshold (Barreca et al., 2011; Noack and Rothe, 2023). We also check the robustness of the results to excluding observations with a running variable ranging between ± 200 and ± 500 to make sure that our effects are not driven by a particular subset of observations close to but not exactly at the threshold either (Appendix Table C18). Finally, given the large support of our running variable, we check the robustness of our results to excluding districts far away from the threshold before selecting the bandwidth, to make sure that outliers are not driving the bandwidth selection and, thus, the estimated effects (Appendix Table C19). Overall, the point estimates and their significance remain very similar.

5 Main results

5.1 Effects on winner identity

We first consider our main outcome, winner identity, and test the hypothesis that campaign finance rules decrease incumbents' chances of victory. We compare incumbents to challengers, to see whether the rules level the playing field between the top candidates from the previous election, and to outsiders, to see whether they bring new candidates to power.

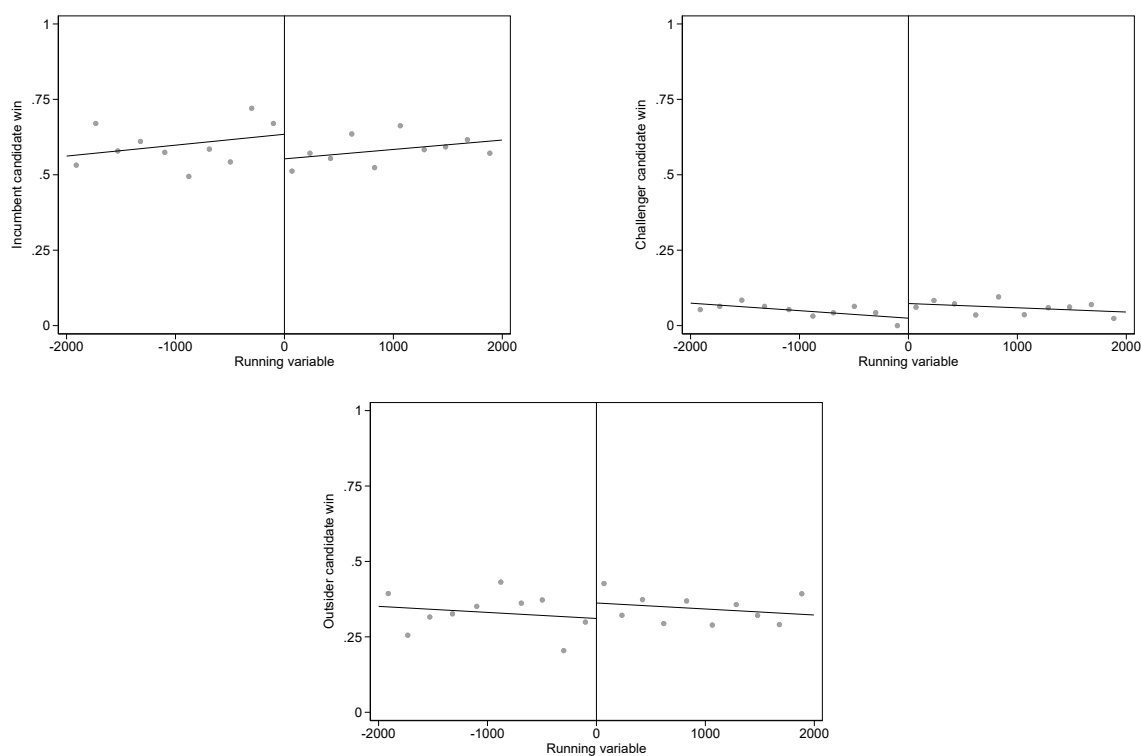
We begin with a graphical analysis, in Figure 2. Each dot represents the average value of the outcome variable within a given bin of the running variable. We observe a clear negative jump at the threshold for the probability of incumbents winning the election, and clear positive jumps for challengers and outsider candidates. The corresponding point estimates, shown in Table 2, are sizable and all significant at the 1 or 5 percent level. The probability of the incumbent winning declines by 14.5 percentage points (21.2 percent of the mean in districts just below the cutoff), while the probabilities of the challenger and outsider candidates winning increase by 5.2 percentage points (a nearly three-fold increase) and 9.3 percentage points (32.4 percent), respectively. In

significant at the 10 percent level, among which four are significant at the 5 percent level, and one at the 1 percent level.

absolute terms, the effects on challengers and outsiders add up to the effect on incumbents, indicating that the campaign finance rules increase the winning chances of the former at the expense of the latter.²¹

The outcome indicating whether the incumbent wins is equal to 0 both when the incumbent runs and does not win and when they do not run. Therefore, the negative effect on incumbents' reelection probability could be driven by negative effects both on winning, conditional on running, and on running. The conceptual framework makes predictions for both channels. First, conditional on running, the campaign finance rules should decrease incumbents' advantage and thus decrease their vote share and probability of winning, conditional on running. Second, candidates who anticipate these effects may change their decision whether to enter the race or stay out. We explore both effects in the next two sections.

Figure 2: Impact on winner identity



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 linear fit. To facilitate visualization, the graph is truncated at 2,000 inhabitants around the cutoff.

²¹Appendix Table C1 shows the robustness of these results to excluding the 2008 elections (so that we measure the effect of being treated only once). While the effects on outsider candidates become nonsignificant, our results on challengers and incumbents remain significant at the 1 and 10 percent level, respectively.

Table 2: Impact on winner identity

Outcome	(1)	(2)	(3)
	Incumbent win	Challenger win	Outsider win
Treatment	-0.145*** (0.046)	0.052** (0.020)	0.093** (0.043)
Robust p -value	0.002	0.011	0.024
Observations	1,390	1,816	1,680
Polyn. order	1	1	1
Bandwidth	1,574	2,036	1,880
Mean, left of threshold	0.683	0.018	0.287

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.

5.2 Effects on competitiveness and on winning and vote shares, conditional on running

Since campaign finance rules level the playing field between candidates, we should expect them to make the election more competitive overall and to decrease the vote share of incumbents and their likelihood of winning, conditional on running, with opposite effects for challengers.

We first investigate the impact on the competitiveness of the election, measured with three indicators: the fragmentation of vote shares in the first round, the probability of any candidate winning in the first round, and the ultimate winner's vote share margin in the first round (defined as the difference between that candidate's vote share and the vote share of the other strongest candidate 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 show the results in Table 3 and Figure 3. All the effects point to an increase in competitiveness. While the effect on fragmentation is not significant, the probability that the election is won in the first round and the winner's vote margin in the first round decrease by 10.9 and 2.8 percentage points (30.9 percent and 14.7 percent), which is significant at the 5 and 10 percent level,

respectively.²²

These results indicate that the campaign finance rules tend to penalize front-runners. We now measure effects on individual candidates. Since we do not know the full set of potential candidates, we focus on the incumbent and the challenger, who can be identified based on the results of the previous election. We estimate the impact of the rules on these candidates' vote share and probability of winning, conditional on them participating in the race.

To do so, 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 will see in the next section that the campaign finance rules also 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 present the method intuitively here, and Appendix J provides the algebra.

The impact of campaign finance rules on incumbents' probability of winning conditional on running can be decomposed into several components. It first depends on the impact of the rules on the unconditional probability of winning, which was reported in Table 2 and is shown again in Table 4, Panel A. Second, it depends on the impact of campaign finance rules on the probability of running, which we also observe in the data (see the next Section 5.3). Third, it depends on the probability that "compliers" – defined as incumbents who do not run in districts above the threshold due to the presence of campaign finance rules – would have won in districts just above the threshold, had they decided to run. This term is unobservable by definition, so we need to make assumptions about it.

To obtain the largest possible effect (the upper bound), we assume that incumbent compliers would never win in districts subject to the campaign finance rules, had they run. This amounts to assuming that the unconditional effect on winning found in Section 5.1 is entirely driven by the effect on winning conditional on running. To compute the lower bound, we assume that compliers would have the same probability of winning as incumbents running in districts below the discontinuity, where there is no campaign finance rule. This yields a conservative estimate, as this probability is higher than the probability of winning of incumbents who run in districts above the discontinuity: 87.1 against 76.7 percent.

We use the same method to derive bounds on vote shares conditional on running and on chal-

²²The point estimates are very similar when we exclude the 2008 elections (Appendix Table C2) and when we include non-linkable districts (Appendix Table C3). While the effect on the winner's vote margin becomes nonsignificant without the 2008 election, it is significant at the 5 percent level when adding non-linkable districts, and the effect on the probability that the election is won in the first round is significant at the 1 or 5 percent level in all tables.

lengers' probability of winning and vote shares conditional on running.²³ 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 above, and repeat these steps 10,000 times.

The results are shown in Table 4, Panel B. Conditional on running, the campaign finance rules reduce incumbents' first round vote share by 3.0 to 7.6 percentage points (6.3 to 16.1 percent) 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.²⁴

Table 3: Impact on competition

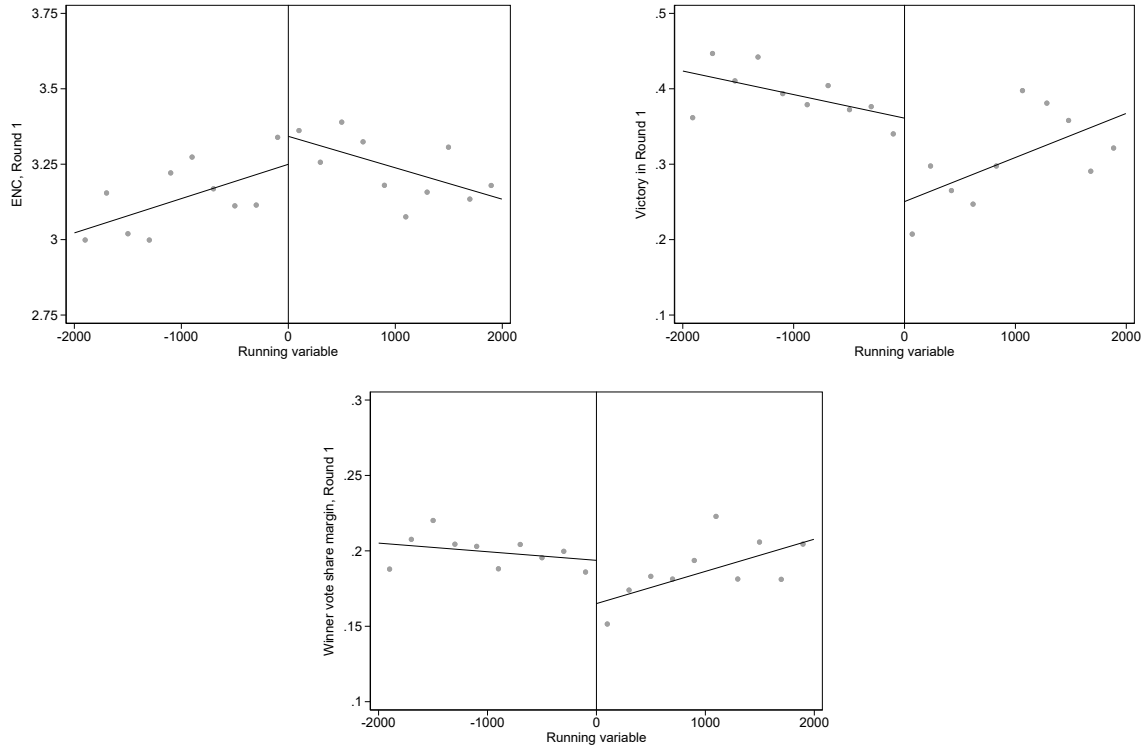
Outcome	(1)	(2)	(3)
	ENC r1	Victory in first round	Winner vote margin in first round
Treatment	0.086 (0.089)	-0.109** (0.044)	-0.028* (0.016)
Robust <i>p</i> -value	0.245	0.012	0.061
Observations	2,454	2,151	2,065
Polyn. order	1	1	1
Bandwidth	2,746	2,410	2,308
Mean, left of threshold	3.246	0.353	0.190

Notes as in Table 2.

²³We can decompose the impact of campaign finance rules on the incumbent's vote share conditional on running into the following components: the impact on the unconditional vote share (where the vote share is set to 0 if the candidate does not run), shown in Table 4, Panel A; the impact on the probability to run, shown in Section 5.3; and the vote share that compliers would have obtained in districts just above the threshold, had they decided to run (the unobservable term).

²⁴These 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.

Figure 3: Impact on competition



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 linear fit. To facilitate visualization, the graph is truncated at 2,000 inhabitants around the cutoff.

Table 4: Impact on winning and vote shares, conditional on running

Outcome	(1)	(2)	(3)	(4)
	Incumbent		Challenger	
	win	vote share, R1	win	vote share, R1
<i>Panel A. Unconditional effects</i>				
Treatment	-0.145*** (0.046)	-0.058*** (0.021)	0.052*** (0.020)	0.034*** (0.012)
Robust <i>p</i> -value	0.002	0.005	0.011	0.003
Observations	1,390	1,871	1,816	1,908
Polyn. order	1	1	1	1
Bandwidth	1,574	2,106	2,036	2,154
Mean	0.683	0.367	0.018	0.044
<i>Panel B. Conditional effects</i>				
Upper bound	-0.189** (0.093)	-0.076** (0.033)	0.198** (0.080)	0.130*** (0.042)
Lower bound	-0.105 (0.075)	-0.030 (0.020)	0.110 (0.069)	0.034 (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.

5.3 Effects on entry

We now investigate whether the campaign finance rules also affect candidates' decision to enter the race, thus contributing to the overall effects on winner identity.

Remember from the conceptual framework introduced in Section 2 that the effect on incumbents' entry is ambiguous: while the reduced cost of campaigning may encourage them to run for reelection, we just showed a negative effect on incumbents' vote share and chances of winning, conditional on running, which may deter them from entering if they anticipate it. In practice, the latter force seems to dominate: as shown in Table 5, column 1, and in the first graph of Figure 4, the rules decrease the likelihood that the incumbent runs by 7.4 percentage points (9.6 percent).²⁵

²⁵Incumbents who do not run for reelection do not necessarily exit politics: they may run again in the future or compete for higher offices.

By contrast, the increase in challengers' chances of winning, conditional on running, combined with the lower cost of running should increase their likelihood to enter. Indeed, challengers' likelihood to be present increases by 8.4 percentage points (47.7 percent, column 2).

The same logic applies to outsider candidates who have chances to win, but the expected effect on the entry of small outsider candidates is unclear. Indeed, these candidates may not be certain to reach the five percent reimbursement threshold, and their vote share (and, thus, the consumption value of competing) may be higher or lower than absent campaign finance rules. In column 3, we report the overall impact on the total number of outsider candidates, and find an effect that is close to zero and nonsignificant (column 3). Similarly, the overall effects on the total number of candidates (whether they were present in the previous election or not) and on first-round turnout are small and nonsignificant (columns 4 and 5).²⁶

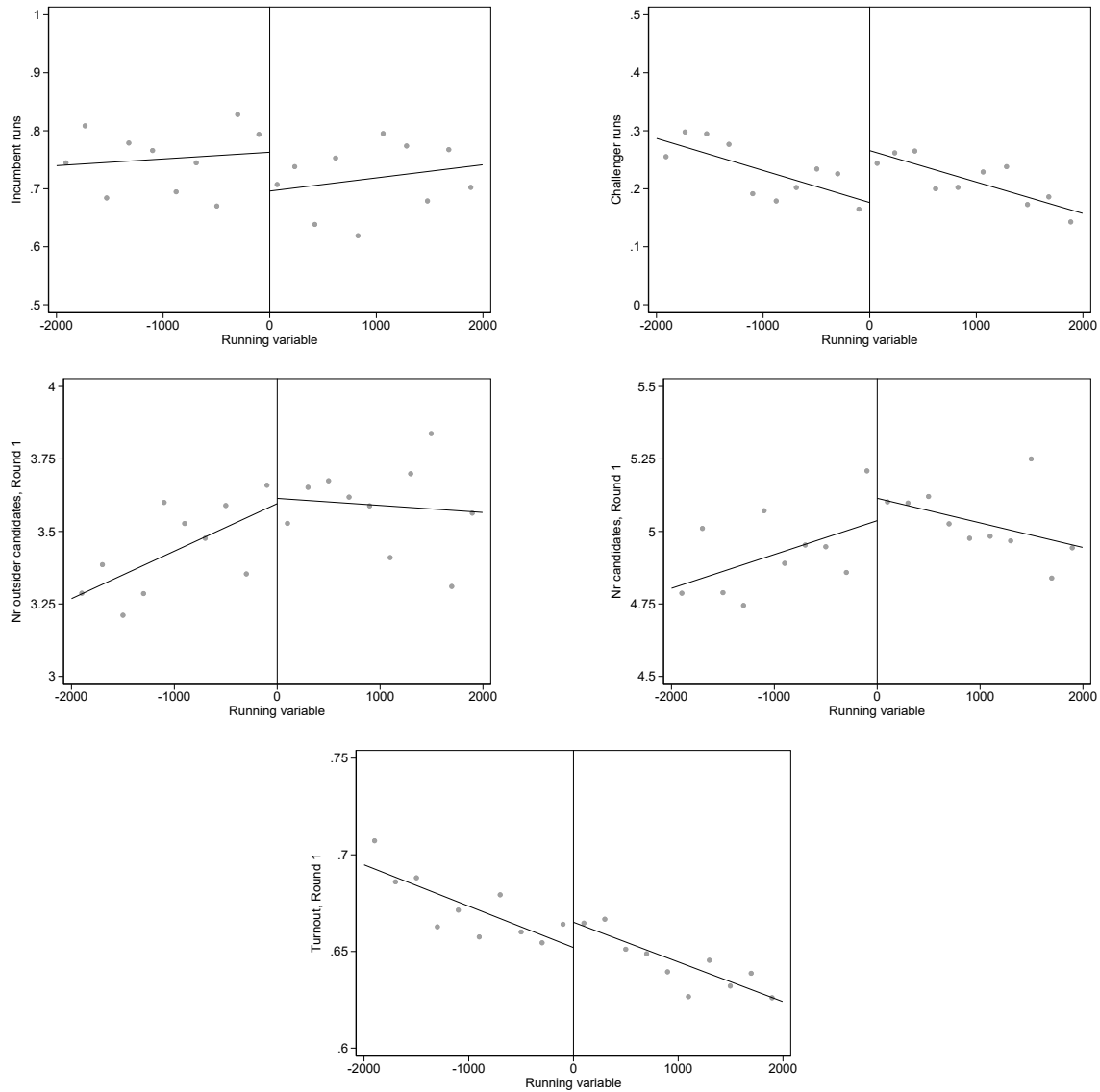
Table 5: Impact on entry

Outcome	(1)	(2)	(3)	(4)	(5)
	Incumbent run	Challenger run	Number of Outsiders	Number of Candidates	Turnout r1
Treatment	-0.074** (0.032)	0.084** (0.038)	0.014 (0.120)	0.046 (0.119)	0.010 (0.009)
Robust <i>p</i> -value	0.023	0.021	0.825	0.514	0.235
Observations	2,576	1,829	2,629	2,331	2,304
Polyn. order	1	1	1	1	1
Bandwidth	2,866	2,058	2,953	2,617	2,574
Mean, left of threshold	0.767	0.176	3.593	5.055	0.656

Notes as in Table 2.

²⁶The estimates are similar when we exclude the 2008 elections (Appendix Table C5). While the impact on the probability that the incumbent runs is no longer significant (*p*-value of 0.103), it is of similar magnitude (-7.3 vs. -7.4 percentage points) and the impact on the probability that the challenger runs remains significant at the 5 percent level. The effects on the number of candidates and on turnout remain nonsignificant when we exclude 2008 or include districts that cannot be linked over time (Appendix Table C6).

Figure 4: Impact on entry



Notes as in Figure 3.

5.4 Effects on representativeness and winner quality

To further characterize the effects of campaign finance rules on electoral outcomes, we finally ask whether they affect the representativeness of the results or the quality of the winner.

Outreach efforts funded by campaign money are an important way in which voters get educated 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 ma-

jority (Austen-Smith, 1987; Coate, 2004). Therefore, a possible concern is if campaign finance rules decrease the overall amount of money spent in equilibrium and, thus, the quantity of information available to voters. This concern may be somewhat alleviated when spending limits are combined with the reimbursement of campaign expenditures, like in the present case (Prat, 2004).

A second concern is that campaign finance rules may restrict high-quality candidates' ability to signal their quality by spending more (Ashworth, 2006; Prat et al., 2010), resulting in the victory of worse candidates. The compression of differences in money spent across candidates may further strengthen outsiders and lead voters to split their votes across multiple candidates of the same orientation, leading to suboptimal outcomes such as the defeat of the Condorcet winner (Gordon et al., 2007; Pons and Tricaud, 2018). It may also improve performance by candidates from non-mainstream platforms and increase polarization. On the other hand, if some types of candidates had a privileged access to donors, eliminating this unfair advantage may result in a more representative outcome and, possibly, the election of candidates of higher quality.

Effects on the political orientation of the winner

We first 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.

Table 6 shows that candidates on the left benefit from the campaign finance rules electorally. Campaign finance rules 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 (column 1). Victories by right-wing candidates become less likely, by 5.3 percentage points, but this estimate is not statistically significant (column 2).²⁷

One possible interpretation of these results, for which we provide evidence in Section 6.3, is that left-wing candidates have a lower access to private money at baseline than those on the right, and that the reform alleviated this imbalance. We now go beyond political orientation and directly estimate effects on polarization and representativeness measured at the district level.

Effects on polarization and winner's representativeness

We start by measuring 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

²⁷These results are robust to including non-linkable districts (Appendix Table C8, columns 1 and 2). 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 C7, column 1). We focus on elections won by the left and the right, as they represent 95 percent of the victories at the threshold. Appendix Table A1 shows the results for all six political orientations (far-left, left, center, right, far-right, and non-classified).

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 the 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.²⁸

We next assess the winner's representativeness by using first-round results as a proxy for voter preferences. Indeed, voters are likely to express their true preferences in the first round of two-round elections (Piketty, 2000).²⁹ We compute the vote share of each of the five political orientations (far-left, left, center, right, and far-right) by aggregating the first-round vote shares of the candidates belonging to the same orientation. We then consider two outcomes. First, we look at the impact of campaign finance rules on the first round vote share of the winner's orientation. Second, we consider a dummy equal to 1 if the winner's orientation obtained the most votes in the first round. We find a negligible effect on the first outcome (column 4) and a negative but small and nonsignificant effect on the second (column 5), indicating that the campaign finance rules do not decrease the representativeness of the winner with respect to the distribution of first round vote choices.

Effects on the quality of the winner

Finally, despite the lack of any direct measure of winners' quality, we build a proxy by considering their vote share in the next election. Indeed, an increase in the winner's vote share would signal that voters are satisfied with their performance. As shown in Appendix Table A2, 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).³⁰

²⁸The effects on polarization and representativeness remain small and nonsignificant when we exclude 2008 and include non-likable districts (Appendix Tables C7 and C8).

²⁹For a discussion of this prediction and for papers stressing the possibility of other voting equilibria, see Bouton and Gratton (2015) and Bouton et al. (2022).

³⁰All the specifications in Appendix Table A2 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, the probability that the winner runs in the next election does not jump at the threshold (Appendix Table A3). 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 A4).

In sum, we do not find any evidence of adverse effects of the campaign finance rules on the representativeness and the quality of the winner.

Table 6: Impact on winning orientation, polarization, and winner’s representativeness

	(1)	(2)	(3)	(4)	(5)
Outcome	Left win	Right win	Polarization	Vote share winner’s orientation	Top orientation winning
Treatment	0.085* (0.047)	-0.053 (0.041)	-0.082 (0.083)	-0.002 (0.014)	-0.037 (0.029)
Robust <i>p</i> -value	0.059	0.202	0.341	0.887	0.170
Observations	2,528	3,359	2,153	2,289	1,870
Polynomial order	1	1	1	1	1
Bandwidth	2,808	3,780	2,761	2,559	2,097
Mean, left of threshold	0.475	0.477	4.868	0.583	0.922

Notes: The sample in column 3 is restricted to races for which each candidate can be matched to a ParlGov position on the [0-10] left-right scale, which excludes 14.0 percent of the sample. The outcomes in columns 4 and 5 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.

6 Spending limits versus reimbursement

We now investigate whether the effects uncovered in the previous section are primarily driven by spending limits or by the reimbursement of candidate expenditures. While estimating the joint impact of both regulations 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.

6.1 Impact of reimbursement at the candidate level

We first test whether the reimbursement of campaign spending matters for candidates’ decision to run. To do so, we exploit 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 helps candidates with less resources to be more competitive, we would expect candidates who obtained more than five percent of the votes in the last election to be more likely to compete in the next election, given that

they received additional resources. We run a separate RDD at the candidate level around the five percent threshold, using the following specification:

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

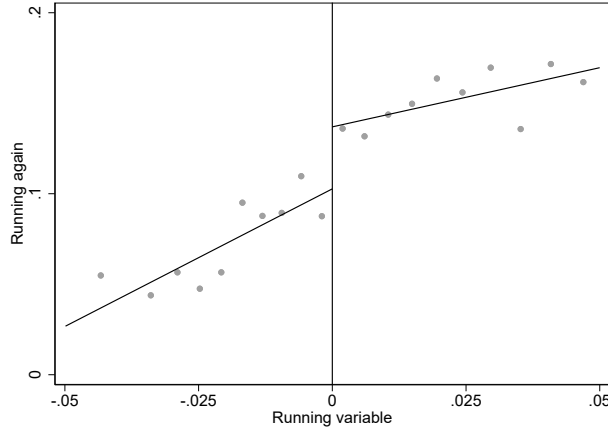
where $Y_{j,t+1}$ is a dummy equal to 1 if candidate j , present at election t , runs again in election $t + 1$, $X_{j,t}$ is the running variable, defined as the candidate's vote share at t centered around five percent, and $D_{j,t}$ is the assignment variable, a dummy taking value one if $X_{j,t}$ is positive.³¹ The sample is restricted to districts above 9,000 inhabitants which are linkable between t and $t + 1$, in departmental elections post 1995. 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 their algorithm, and cluster our standard errors at the district level.

As shown in Figure 5, 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 7 provides the point estimate: an increase by 4.2 percentage points (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 A5 and A6, we do not find any effect in the 1992 and 1994 departmental elections (before public reimbursement was introduced) and in districts below 9,000 inhabitants (in which candidates' expenditures are never reimbursed).

While these results provide evidence that reimbursement matters for small candidates, they do not necessarily hold for the main candidates. For a broader assessment of the importance of reimbursement, we next compare elections taking place before versus after the 1995 reform that introduced it.

³¹Using a similar empirical 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.

Figure 5: Effect of being reimbursed in election t on running in election $t + 1$



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 linear 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 only includes districts above 9,000 inhabitants and which can be linked with election $t + 1$.

Table 7: Impact of being reimbursed in election t on running in election $t + 1$

Outcome	(1)
	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 only includes districts above 9,000 inhabitants and which can be linked with election $t + 1$.

6.2 Effects in the 1992 and 1994 elections

In this subsection, we run the same analysis as in Section 5, but focusing on the 1992 and 1994 elections. These elections took place after the 1990 reform enforcing spending limits for districts above the discontinuity, but before the 1995 reform enacting the reimbursement of candidates, and they were thus not included in our main sample. 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 A7, the effects in the 1992 and 1994 elections are lower than in the post-1995 elections (our main sample of analysis), and they 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. We reject the null hypothesis that the coefficients before and after 1995 are equal for the probability of incumbent, challenger, and outsider candidates winning, as well as the probability of challengers running. Appendix Table A7 also reports Sidak-Holm p -values, which correct for multiple testing. All the effects in elections after 1995 remain significant, but none of the effects in the 1992-1994 elections is so.

As an additional way of comparing the effects between the 1992-1994 elections and the post-1995 elections, we finally run a difference-in-discontinuity estimation (Grembi et al., 2016; Eggers et al., 2018): We focus on districts close to the threshold and regress each outcome on the treatment variable (a dummy equal to 1 for districts above 9,000 inhabitants) interacted with a dummy equal to 1 for elections taking place after 1995.³² The estimates on the interaction capture the differential impact of being above the 9,000 inhabitants threshold after 1995 (and thus subject to both the spending limit and reimbursement) relative to being above the threshold before 1995 (and thus only subject to the spending limit). As shown in Appendix Tables A8 and A9, the post-1995 effect is significant both for the probability that the incumbent wins and for the probability that the challenger wins. Moreover, the estimates are close in magnitude to our main results, consistent with the null effects found in the 1992 and 1994 elections.

6.3 Changes in candidate spending and contribution patterns over time

While these results suggest that effects post 1995 are driven by the public reimbursement of campaign money, alternative interpretations remain possible. The tightening of spending limits and ban on corporate donations concomitant to the introduction of reimbursement, in 1995, could play a role. 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

³²To define the analysis sample, we either consider the optimal bandwidth used in the main analysis, or we take the average of the optimal bandwidth used in the main analysis and the one used for the 1992-1994 analysis.

above the threshold.

Figure 6 and Appendix Figures A2 and A3 plot the distribution of spending to ceiling ratios as well as personal contributions and donations to ceiling ratios for all candidates (upper left graph), separately for incumbents, challengers, and outsiders (upper right graph and middle graphs), and separately for left-wing and right-wing candidates (lower graphs). Each graph contains two histograms, corresponding to the 1992-1994 and 1998-2001 elections.

We first observe large outward shifts of the spending distribution to the right, after the 1995 reform (Figure 6). This is a first piece of evidence that the key element that mattered in the 1995 reform was the introduction of reimbursement. Indeed, if anything, we would expect the strengthening of spending limits and the ban on corporate donations to have the opposite effect. In addition, the increased spending is largely driven by an increase in personal contributions (Appendix Figure A2), pointing again to the role of reimbursement. Indeed, recall that only personal contributions get reimbursed.

Second, both for total expenditures and personal contributions, we only see bunching at 50 percent of the ceiling post 1995. 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 five percent of the votes). Moreover, the bunching is slightly stronger for personal contributions, which 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.

Contrasting with the bunching at 50 percent of the ceiling, we observe limited bunching of overall spending at 100 percent, corresponding to candidates who spend nearly exactly the maximum amount of money authorized, whether we consider elections taking place before or after the 1995 reform. This suggests that the tightening of the limit did not affect candidates' spending, and that, more generally, the spending limit is not binding.

Third, the increase in spending, personal contributions, and the bunching at 50 percent are all larger for challengers and outsiders than for incumbents. For instance, total spending as a share of the ceiling more than doubled for outsiders and increased by 59 percent for challengers, whereas it increased by 30 percent for incumbents.³³ Similarly, the shifts are larger for left-wing candidates than right-wing candidates. The former experienced an average increase in spending

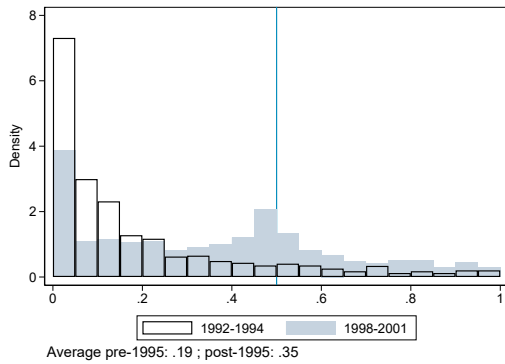
³³The shifts in spending and personal contributions as well as the bunching at 50 percent are larger for outsider candidates compared to challenger candidates. This can be explained by the fact that outsiders are more likely to rely exclusively on personal expenditures. Indeed, in the 1998-2001 elections, 47 percent of outsiders relied exclusively on personal expenditures, against 33 percent of challengers.

of 135 percent against 33 percent for the latter. Overall, the reimbursement introduced by the 1995 reform disproportionately benefited candidates with lower resources and decreased differences in spending across candidates. Furthermore, remember that challengers, outsiders, and left-wing candidates are also those who benefited electorally from campaign finance rules. This suggests that the changes in spending across candidates due to the reimbursement is a key channel explaining the ultimate effect on electoral outcomes.

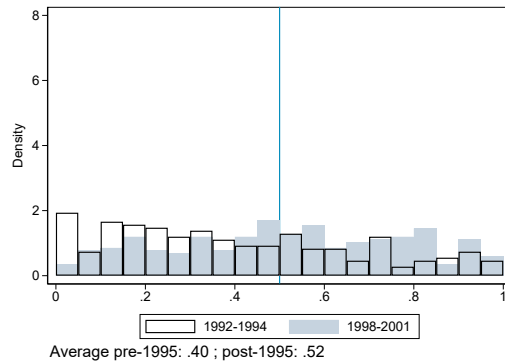
Finally, we explore changes in the amount of donations received, given that the 1995 reform also banned corporate donations. As shown in Appendix Figure A3, we see a decrease in donations as a share of the ceiling after 1995. However, this decrease is of comparable magnitude across the different types of candidates (relative to the pre-1995 level), suggesting that this part of the reform is unlikely to explain the electoral effects we find.

This graphical evidence underscores the dramatic changes in campaign spending which resulted from the 1995 reform, and from the introduction of personal expenditures' reimbursement more specifically.

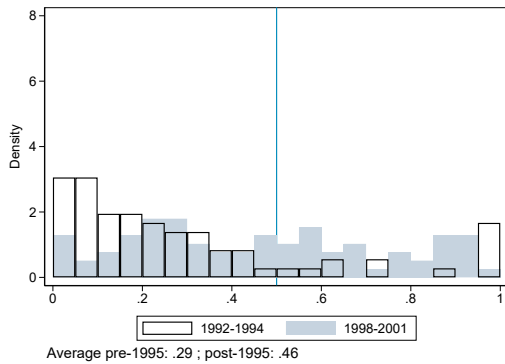
Figure 6: Expenditures to ceiling ratios



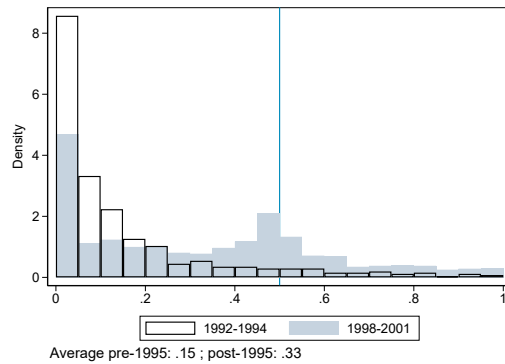
All candidates



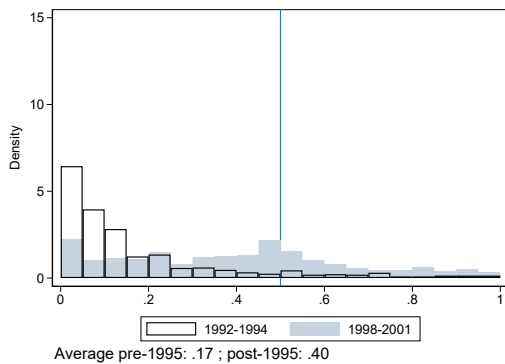
Incumbent candidates



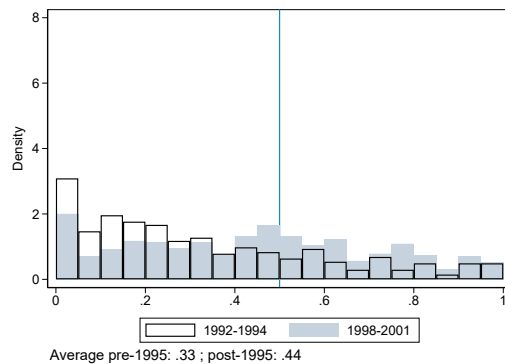
Challenger candidates



Outsider candidates



Left-wing candidates



Right-wing candidates

Notes: The level of analysis is the candidate and the sample only includes 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).

6.4 Heterogeneity analysis

As a last piece of evidence that our results are primarily driven by the reimbursement of personal expenditures, we show that our results hold when focusing on districts where the other regulatory changes that took place in 1995 (the tightening of spending limits and the ban on corporate donations) are least likely to be binding.

We first consider districts where spending limits are unlikely to be binding. To identify them, we focus on our main sample of analysis (the post-1995 elections), and restrict the sample to districts just above the threshold (between 9,000 and 10,000 inhabitants). We consider the spending to ceiling ratio of incumbents, who generally spend more money than other candidates. We 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 département fixed effects.³⁴ We then use the coefficients from this regression to predict incumbents' spending to ceiling ratio in all districts. 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. We verify that, in districts of this subsample, the distribution of the incumbent spending to ceiling ratio is to the left of the distribution for all districts just above the discontinuity, and that it does not show any bunching at the limit (Appendix Figure A4). And yet, effects in this subsample, shown in Appendix Table A10, are similar as in the full sample. In particular, the effects on the probability of a victory by the incumbent and the challenger are -15.7 and 7.1 percentage points, as compared to -14.5 and 5.2 percentage points in the main sample, and they are significant at the five percent level.

We next investigate whether our results hold when focusing on districts where the ban on corporate donations is the least likely to be binding. We use a different approach than above, since we only have information on corporate donations for the 1994 elections.³⁵ We focus on areas in which there were only few corporate donations before 1995, making the ban less likely to matter. Specifically, we identify the 41 percent of districts where the incumbent did not receive any corporate donations in 1994. We then rank départements based on their share of such districts, and focus on the top 25 percent départements. As expected, and as shown in Appendix Figures A5 and A6, in this subsample, the distribution of the share of corporate donations as a percentage of the ceiling in 1994 is to the left of the distribution for all districts, whether we only consider the incumbent or all candidates. Appendix Table A11 runs our main estimation on the post-1995 elections, focusing on those départements. The effects in this subsample, where the ban on corporate donations should be the least binding, are similar as in the full sample and, if anything, slightly larger.

³⁴See Appendix Table A10 for a more detailed description of this regression.

³⁵The 1992 contribution data only report the total donation amount received by candidates, without distinguishing between corporate and non-corporate donations.

All the evidence in this section points to the conclusion that our results are driven by the reimbursement of campaign expenditures rather than spending limits or the ban on corporate donations.

7 When are campaign finance rules most impactful?

7.1 Effects depending on the closeness of the race

In this last section, we ask when campaign finance rules affect electoral outcomes the most and first study the moderating influence of race competitiveness.

In districts that are very competitive even absent any campaign finance regulation, e.g., because the leading candidates spend similar amounts of money at baseline, there is little room for campaign finance rules to affect relative vote shares and we may expect only modest effects on electoral outcomes. We should also expect small effects in districts that are strongholds of one party and where that party's candidate will win the race by a landslide whether or not campaign finance rules are in place. We thus expect to observe the largest effects in districts with intermediate competitiveness.

Indeed, we find that the effects are not linear in race closeness but that they follow an inverse U shape. Our analysis considers the winner of the last election and proxies a district's competitiveness by their vote share margin in the first round of that election, defined as in Section 5.2. We then split our main sample into terciles based on that variable. The average winner's margin in the last election is 1.1, 15.5, and 37.7 percent in the first, second, and third terciles. Furthermore, the difference in amount spent across candidates is much lower in competitive districts before the 1995 reform (Appendix Table A12).³⁶

Appendix Tables A13 to A15 show the effects of campaign finance rules in each tercile separately. Our effects on winner identity, competitiveness, and running are all mainly driven by the second tercile.

7.2 Effects in municipal elections

We finally investigate whether campaign finance rules have similar effects in municipal elections as in departmental elections.

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. Departmental and municipal elections have different electoral

³⁶Appendix Table A12 considers districts "at baseline" by looking at the 1992 and 1994 elections, before reimbursement was introduced. We focus on districts just above the discontinuity and compare the average spending of the winner and of the runner-up, as a ratio of the ceiling.

calendars (except for 2001 and 2008, when the two 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.

Around the 9,000 inhabitants threshold, municipal councils count 27 members (including the mayor), so competing lists must include 27 candidates.³⁷ 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. In these municipalities, elections follow a two-round list system with proportional representation.³⁸

Our sample includes the 2001, 2008, and 2014 municipal elections.³⁹ As for departmental elections, 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 $t-1$.⁴⁰ In municipal elections before 2014, we further require that the district population was above 3,500 inhabitants both at $t-1$ and t , so that the electoral rule was identical in both years. Overall, our main sample includes 7,653 linkable municipal races (23,709 lists). Appendix Figure D1 shows the population distribution of municipalities in our sample and Appendix Table D1 provides summary statistics.

The same campaign finance rules as those described in Section 3.1 apply in municipal elections, in municipalities above 9,000 inhabitants. We thus use the same empirical strategy as the one described in Section 4.1 to measure the joint effect of spending limits and reimbursements on municipal electoral outcomes.

All validity and robustness tests are shown in Appendices E and F, for brevity. While the balance tests on baseline characteristics pass, we observe a positive jump in the density of the running variable, which is driven by the 2014 elections. Similar to Corbi et al. (2019), we check

³⁷Municipal 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.

³⁸If 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. Lists with more than five percent of the votes in the first round can merge with lists qualified for the second round. 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.

³⁹Electoral results for all municipalities above 3,500 inhabitants come from the Ministry of the Interior. For the 2001 municipal elections, these data aggregate results across candidates of the same political orientation, so we obtained candidate-level data from Bach et al. (2012) and Cagé (2020) and completed them by consulting and manually inputting results published in local newspapers present in French archives. We also use data from the 1995 municipal elections to define incumbents, challengers, and outsider candidates in the 2001 municipal elections. The pairing between the 1995 and 2001 municipal elections required inputting results from local newspapers for the 1995 municipal elections.

⁴⁰We detect inconsistencies in the $t-1$ election for 185 races in the 2001 municipal elections, due to inconsistencies in the 1995 election results obtained from newspaper sources.

the robustness of our results to considering each municipal election separately, to make sure that they are driven neither by the potentially problematic 2014 elections nor by the fact that most treated districts in the 2008 municipal elections had already been treated a first time in 2001, since no major census took place in between.⁴¹

Table 8 shows the effects on our main outcomes. They are lower than in departmental elections, and none of them is statistically significant. We obtain similar null results when we exclude 2014 and when we consider the 2001, 2008, and 2014 municipal elections separately (Appendix Tables F1 through F4), with only one significant estimate out of 28 (at the 10 percent level).

Table 8: Impact in municipal elections

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger Win	Outsider	Victory In first round	Winner margin	Incumbent Run	Challenger
Treatment	-0.030 (0.054)	0.038 (0.033)	-0.022 (0.055)	-0.009 (0.059)	0.018 (0.026)	-0.022 (0.049)	0.001 (0.054)
Robust <i>p</i> -value	0.686	0.209	0.653	0.820	0.486	0.788	0.913
Observations	1,484	1,318	1,218	1,320	1,450	1,779	1,467
Polyn. order	1	1	1	1	1	1	1
Bandwidth	1,973	1,846	1,667	1,775	1,938	2,297	2,022
Mean, left of threshold	0.563	0.061	0.374	0.606	0.215	0.719	0.269

Notes: We consider our main sample of municipal elections, as defined in Section 7.2, which include the 2001, 2008 and 2014 municipal elections. Other notes as in Table 2.

These null effects can be explained by two main reasons. The first is that candidates' ability to reach their desired amount of spending is likely to depend less on reimbursement by the state, in municipal elections. Indeed, campaign costs can be split between the mayoral candidate and the 26 other members of the list, unlike in departmental elections where the candidate does not have any running mate. Municipal candidates also rely less exclusively on their own contributions and receive more private donations: as shown in Appendix Table D2, in districts just above the threshold, donations account for 13.1 percent of the spending ceiling in municipal elections, against 4.3 percent in departmental elections (columns 1 and 2). In addition, all candidates on the list can devote time to reach out to voters, and time may be a substitute for money. Hence, there is less room for the reimbursement of personal expenditures to make a difference in municipal elections.

⁴¹We do not consider the positive jump in the 2014 elections as definitive evidence of manipulation, given the difficulty to bend the rules used to determine municipalities' official population, and because one would expect manipulation to go in the opposite direction. If anything, incumbent mayors may try to maintain the population of their municipality below the cutoff in order to avoid campaign finance restrictions, which would generate a negative jump in the density of the running variable at the threshold.

The second possible reason for the null effects of campaign finance rules in municipal elections is that the impact of spending on electoral outcomes is likely to be lower in these elections.

First, the number of competitors is lower in municipal elections: on average, 3.1 lists compete in the first round, against 5.3 candidates in departmental elections (Table 1 and Appendix Table D1). This is consistent with the list system allowing ideologically close competitors to join the same list, which is not possible in departmental elections. As a result, municipal races are less competitive, with 63.6 percent of first-round victories against 31.4 for departmental elections. Given that campaign finance rules have a lower impact on electoral outcomes in lopsided races, as shown in Section 7.1, we can thus expect them to have a lower effect in municipal elections.

Second, voters are likely to have more information in municipal elections, making it more difficult and costly to win them over. Indeed, municipal elections are more local: they elect the mayor, whereas departmental elections elect département-level representatives. Moreover, the presence of multiple candidates in each list increases the odds that voters know at least one of them. While we lack direct evidence on voters' level of information about municipal and departmental election candidates, a CEVIPOF survey shows sizable differences in trust levels: between 2009 and 2024, the share of citizens stating that they trust their mayor was consistently 10 percentage points or more higher than the share trusting their departmental representative.⁴²

Third, average expenditures are higher in municipal elections: 0.87 euros per capita, versus 0.31 euros per capita in departmental elections (columns 3 and 4 of Appendix Table D2). Together with voters' higher level of information, this may decrease the marginal returns of campaign money (including any additional money spent in anticipation of reimbursement) in municipal elections. We provide suggestive evidence that this is indeed the case, by regressing candidates' first round vote shares on their expenditures per capita. We focus on the 2008 municipal and departmental elections, which took place on the same day, eliminating any possible confound due to election timing. We consider all candidates in districts above the 9,000 inhabitants threshold that we can link over time. We control for district fixed effects as well as candidate-level variables listed in the table's notes. As shown in Appendix Table D3, candidates' spending amounts are strongly and positively correlated with their vote shares. Given that many unobserved factors can confound the analysis, we cannot interpret the coefficients as a causal impact. However, endogeneity concerns may be somewhat alleviated when comparing the effect between municipal and departmental elections, since similar biases may be present in both cases. The point estimate is almost twice as large in departmental elections, whether we consider all districts (columns 1 and 2) or only districts close to the threshold (columns 4 and 5). Moreover, when we run the regression on both election types and include an interaction term for municipal elections, the coefficient on the interaction is negative, significant at the 1 percent level, and represents a 43 or 42 percent decrease compared to

⁴²See https://www.sciencespo.fr/cevipof/sites/sciencespo.fr/cevipof/files/BConf_V15_Extraction1_modif.pdf.

the coefficient for departmental elections (columns 3 and 6).

8 Conclusion

This paper investigates how campaign finance rules affect electoral outcomes by exploiting reforms that took place in France in the early 1990s. After the reforms, the rules differed for districts 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 likelihood of winning, conditional on running. The weakening of incumbents benefited their runner-ups in the previous race as well as new candidates and it helped the left, whose candidates are often outspent by right-wing competitors absent public funding. Importantly, this policy did not increase the polarization of the results, nor did it decrease our measure of winner quality or 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 their design and to the interplay with other electoral rules and institutions.

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 Fourinaies (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, we find that campaign finance rules' effects vary substantially across relatively similar settings: 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. In these elections, the list system decreases the scope for public funding to make a difference since fellow candidates can contribute time and money to the campaign. In addition, higher baseline spending levels decrease the marginal returns and the equalizing power of public money.

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Appendix (for online publication only)

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Appendix I: Additional analyses on the main sample (Departmental elections)

A. Additional tables and figures

Table A1: Impact on winning orientation

	(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 (0.003)	0.085* (0.047)	-0.021 (0.014)	-0.053 (0.041)	-0.000 (0.000)	0.010 (0.008)
R. <i>p</i> -value	0.181	0.059	0.146	0.202	0.334	0.261
Obs.	2,113	2,528	2,564	3,359	1,604	2,128
Polyn.	1	1	1	1	1	1
Bdw	2,367	2,808	2,854	3,780	1,799	2,386
Mean	0.003	0.475	0.044	0.477	0.000	0.001

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: Impact on the quality of the winner

Outcome	(1)	(2)	(3)	(4)	(5)
	Vote share difference	Residual			
		Vote share at $t+1$ restricted	unrestricted	Vote share difference restricted	unrestricted
Treatment	0.027 (0.029)	-0.004 (0.021)	-0.003 (0.021)	-0.003 (0.021)	-0.004 (0.021)
Robust p -value	0.259	0.995	0.924	0.998	0.862
Observations	1,028	1,268	1,691	1,262	1,768
Polyn. order	1	1	1	1	1
Bandwidth	1,931	2,355	3,181	2,345	3,326
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 A1.

Table A3: Impact on winner in election t running again in $t+1$ - elections linkable between t and $t+1$

Outcome	(1)
	Winner run again
Treatment	0.017 (0.054)
Robust p -value	0.558
Observations	1,021
Polyn. order	1
Bandwidth	1,909
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 A1.

Table A4: Impact on the quality of the winner - 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 (0.015)	0.026** (0.010)	0.011 (0.013)	0.026** (0.010)	0.010 (0.013)
Robust p -value	0.220	0.029	0.549	0.023	0.594
Observations	1,224	1,246	1,003	1,260	982
Polyn. order	1	1	1	1	1
Bandwidth	2,976	3,024	2,427	3,065	2,372
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 again 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 A1.

Table A5: Impact of being reimbursed in election t on running in election $t + 1$ - 1992-1994 elections in districts above 9,000 inhabitants and linkable between t and $t + 1$

Outcome	(1)
	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 A6: Impact of being reimbursed in election t on running in election $t + 1$ - districts below 9,000 inhabitants and linkable between t and $t + 1$

Outcome	(1)
	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 A5.

Table A7: Impact on the main outcomes - 1992-1994 vs. main sample

Outcome	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		(9)		(10)		(11)		(12)		(13)		(14)		
	92-94	Main sample	Incumbent win	Main sample	92-94	Main sample	Challenger win	Main sample	92-94	Main sample	Outsider win	Main sample	92-94	Main sample	Victory in first round	Main sample	92-94	Main sample	Winner vote margin	Main sample	92-94	Main sample	Incumbent run	Main sample	92-94	Main sample	Challenger run	Main sample	
Treatment	0.038 (0.066)	-0.145*** (0.046)	-0.051* (0.030)	0.052** (0.020)	-0.050 (0.070)	0.093** (0.043)	-0.015 (0.064)	-0.109** (0.044)	-0.005 (0.022)	-0.028* (0.016)	-0.013 (0.051)	-0.074** (0.032)	-0.045 (0.061)	0.084** (0.038)	0.038 (0.066)	-0.145*** (0.046)	-0.051* (0.030)	0.052** (0.020)	-0.050 (0.070)	0.093** (0.043)	-0.015 (0.064)	-0.109** (0.044)	-0.005 (0.022)	-0.028* (0.016)	-0.013 (0.051)	-0.074** (0.032)	-0.045 (0.061)	0.084** (0.038)	
Robust p -value	0.534	0.002	0.061	0.011	0.533	0.024	0.861	0.012	0.798	0.061	0.934	0.023	0.427	0.021	0.534	0.002	0.061	0.533	0.024	0.861	0.012	0.798	0.061	0.934	0.023	0.427	0.021		
Observations	1,041	1,390	588	1,816	8,71	1,680	1,114	2,151	1,042	2,065	1,175	2,576	1,021	1,829	1,041	1,390	588	1,816	8,71	1,680	1,114	2,151	1,042	2,065	1,175	2,576	1,021	1,829	
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	3,365	1,574	1,914	2,036	2,805	1,880	3,599	2,410	3,378	2,308	3,816	2,866	3,320	2,058	3,365	1,574	1,914	2,036	2,805	1,880	3,599	2,410	3,378	2,308	3,816	2,866	3,320	2,058	
Mean, left of threshold	0.619	0.683	0.0498	0.0184	0.345	0.287	0.356	0.353	0.192	0.190	0.800	0.767	0.309	0.176	0.619	0.683	0.0498	0.0184	0.345	0.287	0.356	0.353	0.192	0.190	0.800	0.767	0.309	0.176	
Difference p -value	0.978	0.012	0.358	0.066	0.978	0.080	0.992	0.066	0.992	0.080	0.992	0.080	0.965	0.080	0.978	0.066	0.992	0.066	0.992	0.080	0.992	0.080	0.080	0.080	0.073	0.080	0.080	0.080	0.080
Sidak-Holm p -value	0.978	0.012	0.358	0.066	0.978	0.080	0.992	0.066	0.992	0.080	0.992	0.080	0.965	0.080	0.978	0.066	0.992	0.066	0.992	0.080	0.992	0.080	0.080	0.080	0.073	0.080	0.080	0.080	0.080

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 two rows report the p -value of the test of the difference between coefficients on the same outcome before vs. after 1995 and the Sidak-Holm p -values, respectively. Other notes as in Table A1.

Table A8: Impact on the main outcomes - Difference-in-discontinuities estimates - Main sample bandwidths

Outcome	(1) Incumbent	(2) Challenger win	(3) Outsider	(4) Victory	(5) Winner margin in first round	(6) Incumbent	(7) Challenger run
Treated * Post 1995	-0.220** (0.109)	0.097*** (0.037)	0.141 (0.100)	-0.109 (0.085)	-0.042 (0.029)	-0.070 (0.070)	0.144 (0.092)
Treated	0.075 (0.094)	-0.046 (0.029)	-0.048 (0.084)	-0.001 (0.076)	0.014 (0.024)	-0.004 (0.058)	-0.060 (0.078)
Post 1995	0.064 (0.078)	-0.029 (0.029)	-0.051 (0.070)	-0.006 (0.060)	0.010 (0.020)	-0.032 (0.049)	-0.143** (0.064)
Bandwidth	1,574	2,036	1,880	2,410	2,308	2,866	2,058
Observations	1,872	2,446	2,262	2,897	2,781	3,470	2,463

Notes: We measure the differential impact of campaign finance rules on our main outcomes in districts above 9,000 inhabitants after 1995, using the same estimation procedure as in Grembi et al. (2016). We perform local linear regressions and use kernel weighting. The bandwidths are the optimal bandwidths used for the post-1995 sample (our main sample).

Table A9: Impact on the main outcomes - Difference-in-discontinuities estimates - Average bandwidths

Outcome	(1) Incumbent	(2) Challenger win	(3) Outsider	(4) Victory	(5) Winner margin in first round	(6) Incumbent	(7) Challenger run
Treated * Post 1995	-0.150* (0.088)	0.100*** (0.038)	0.120 (0.090)	-0.087 (0.076)	-0.026 (0.026)	-0.064 (0.066)	0.124 (0.080)
Treated	0.057 (0.075)	-0.048* (0.029)	-0.051 (0.075)	-0.013 (0.069)	0.004 (0.023)	-0.007 (0.054)	-0.055 (0.068)
Post 1995	0.022 (0.061)	-0.031 (0.029)	-0.036 (0.062)	-0.002 (0.053)	0.003 (0.019)	-0.035 (0.046)	-0.133** (0.055)
Bandwidth	2,469	1,975	2,343	3,004	2,843	3,341	2,689
Observations	2,971	2,358	2,822	3,598	3,433	3,997	3,214

Notes: We measure the differential impact of campaign finance rules on our main outcomes in districts above 9,000 inhabitants after 1995, using the same estimation procedure as in Grembi et al. (2016). We perform local linear regressions and use kernel weighting. The bandwidths are the average of the optimal bandwidths used for the 1992-1994 and post-1995 samples.

Table A10: Impact on the main outcomes - Elections with below median predicted incumbent spending to ceiling ratio

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger	Outsider	Victory	Winner margin	Incumbent	Challenger
		win		in first round		run	
Treatment	-0.157** (0.063)	0.071** (0.029)	0.077 (0.055)	-0.066 (0.061)	-0.035 (0.023)	-0.147*** (0.048)	0.051 (0.048)
Robust <i>p</i> -value	0.017	0.014	0.162	0.386	0.118	0.001	0.224
Observations	936	1,001	1,187	1,265	1,154	1,416	1,218
Polyn. order	1	1	1	1	1	1	1
Bandwidth	1,987	2,122	2,493	2,674	2,423	2,984	2,554
Mean, left of threshold	0.671	0.005	0.313	0.410	0.217	0.790	0.184

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.

Table A11: Impact on the main outcomes - Elections in départements where the ban on corporate donations should be the least binding

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger win	Outsider	Victory in first round	Winner margin in first round	Incumbent run	Challenger run
Treatment	-0.230*** (0.071)	0.088** (0.041)	0.133* (0.079)	-0.118* (0.070)	0.006 (0.023)	-0.152** (0.067)	0.060 (0.071)
Robust <i>p</i> -value	0.001	0.030	0.055	0.076	0.938	0.030	0.482
Observations	355	559	421	798	779	652	636
Polyn. order	1	1	1	1	1	1	1
Bandwidth	1,443	2,277	1,722	3,264	3,198	2,632	2,541
Mean, left of threshold	0.734	0.008	0.270	0.346	0.167	0.797	0.175

Notes: We restrict the sample to districts within our main sample of departmental elections that belong to the top 25 percent of départements with the highest share of districts in which the incumbent did not receive any corporate donations in 1994. When ranking the départements, we removed 12 départements in which the incumbent did not run again in any of the districts up for election in 1994. Other notes as in Table A1.

Table A12: Average candidate expenditures depending on the competitiveness of the race - 1992-1994 elections close to the threshold

Winner margin terciles	Total spending divided by the ceiling	
	Winner	Runner up
T1	0.451	0.437
T2	0.447	0.312
T3	0.362	0.204

Notes: We focus on the 1992 and 1994 elections and on districts close to the cutoff (between 9,000 and 11,000 inhabitants). We then split the sample into terciles based on the ultimate winner's vote share margin in the first round (defined as the difference between that candidate's vote share and the vote share of the second strongest candidate in the first round). The first tercile (T1) contains the most competitive districts and the third tercile (T3) contains the least competitive districts.

Table A13: Impact on the main outcomes - Competitiveness tercile 1

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger win	Outsider	Victory	Winner margin in first round	Incumbent	Challenger run
Treatment	0.099 (0.072)	0.064** (0.031)	-0.088 (0.067)	0.003 (0.064)	0.011 (0.026)	0.018 (0.054)	0.011 (0.057)
Robust <i>p</i> -value	0.258	0.036	0.254	0.794	0.987	0.859	0.828
Observations	647	633	692	614	487	967	791
Polyn. order	1	1	1	1	1	1	1
Bandwidth	2,312	2,270	2,469	2,197	1,667	3,456	2,794
Mean, left of threshold	0.542	0.016	0.378	0.169	0.128	0.743	0.211

Notes: We restrict the sample to districts in the first competitiveness tercile (see Section 7.1). Other notes as in Table A1.

Table A14: Impact on the main outcomes - Competitiveness tercile 2

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger win	Outsider	Victory	Winner margin in first round	Incumbent	Challenger run
Treatment	-0.228*** (0.080)	0.071** (0.035)	0.163** (0.073)	-0.158** (0.062)	-0.036* (0.019)	-0.161** (0.061)	0.114* (0.061)
Robust <i>p</i> -value	0.004	0.042	0.020	0.016	0.095	0.010	0.051
Observations	589	860	629	927	1026	895	859
Polyn. order	1	1	1	1	1	1	1
Bandwidth	2,039	3,006	2,191	3,245	3,596	3,109	3,004
Mean, left of threshold	0.672	0.037	0.262	0.350	0.173	0.792	0.187

Notes: We restrict the sample to districts in the second competitiveness tercile (see Section 7.1). Other notes as in Table A1.

Table A15: Impact on the main outcomes - Competitiveness tercile 3

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger win	Outsider	Victory in first round	Winner margin	Incumbent	Challenger run
Treatment	-0.188** (0.089)	-0.011 (0.010)	0.151* (0.082)	-0.107 (0.074)	-0.031 (0.028)	-0.080 (0.062)	0.071 (0.077)
Robust <i>p</i> -value	0.034	0.493	0.067	0.125	0.201	0.205	0.269
Observations	613	645	711	976	1008	1008	655
Polyn. order	1	1	1	1	1	1	1
Bandwidth	1,913	2,019	2,209	3,027	3,134	3,133	2,048
Mean, left of threshold	0.733	0.006	0.268	0.542	0.270	0.766	0.165

Notes: We restrict the sample to districts in the third competitiveness tercile (see Section 7.1). Other notes as in Table A1.

Figure A1: Timeline

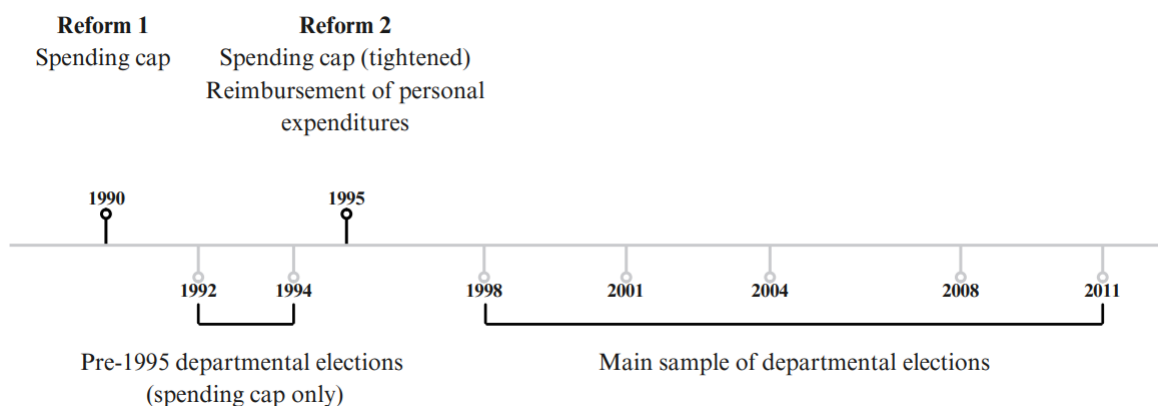
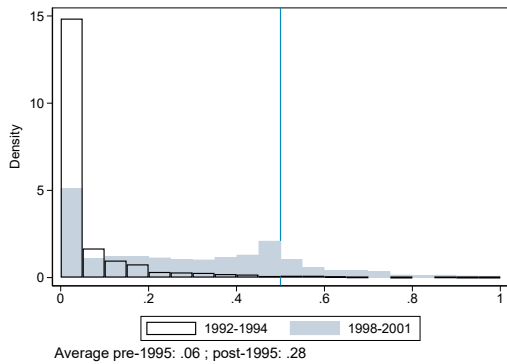
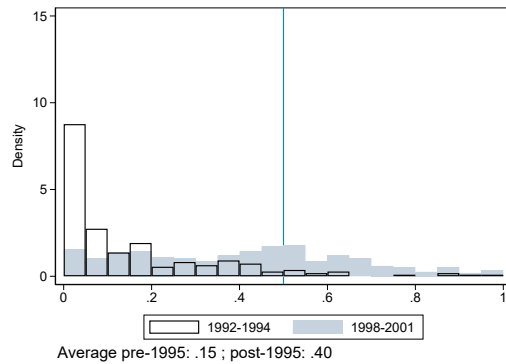


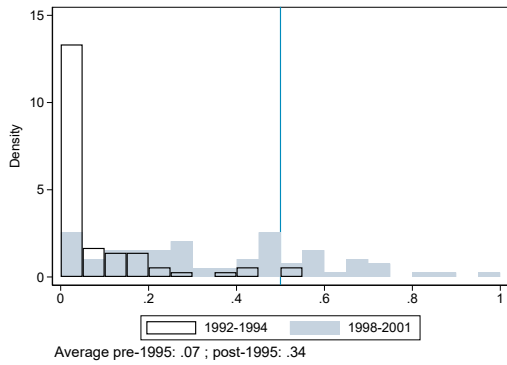
Figure A2: Personal contributions to ceiling ratios



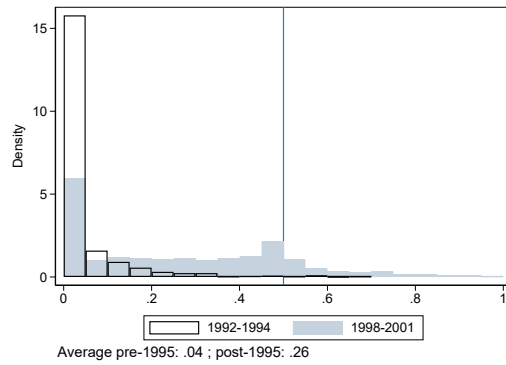
All candidates



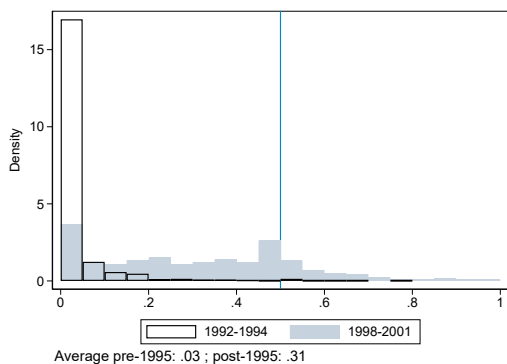
Incumbent candidates



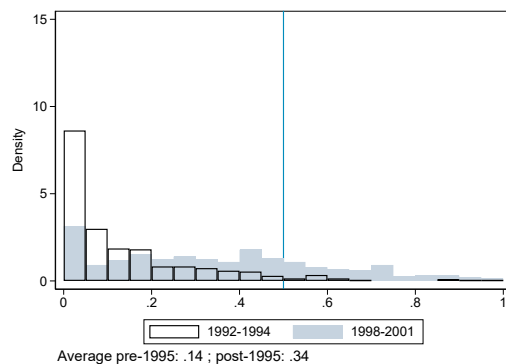
Challenger candidates



Outsider candidates



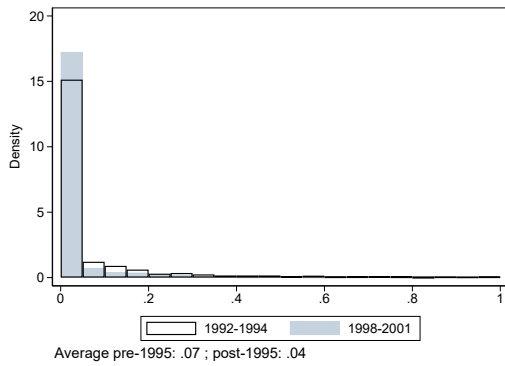
Left-wing candidates



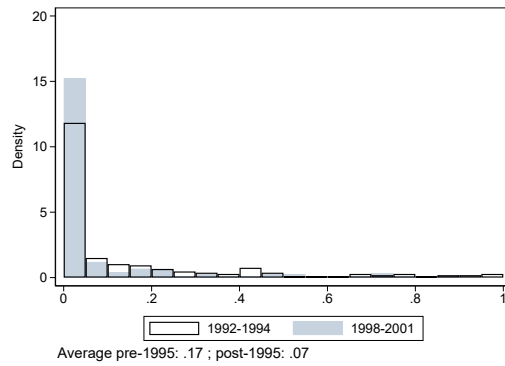
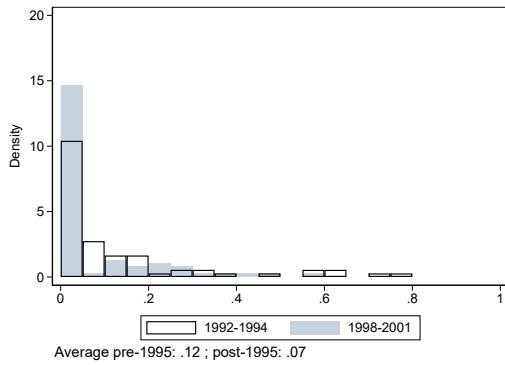
Right-wing candidates

Notes: The level of analysis is the candidate and the sample only includes 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).

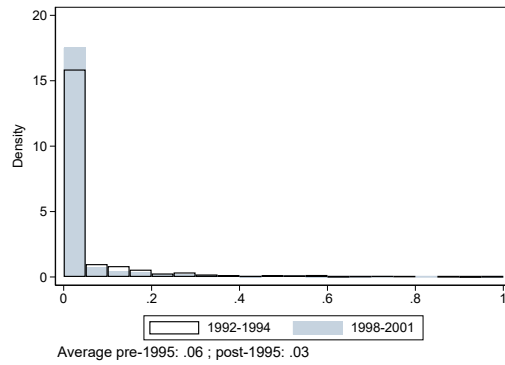
Figure A3: Donations to ceiling ratios



All candidates

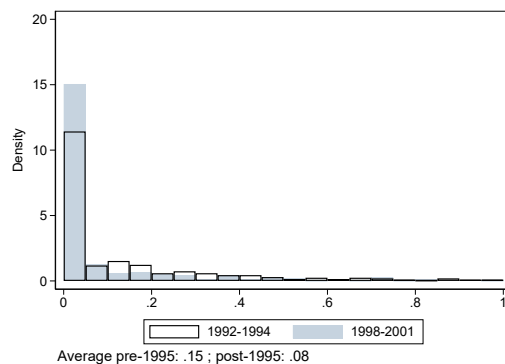
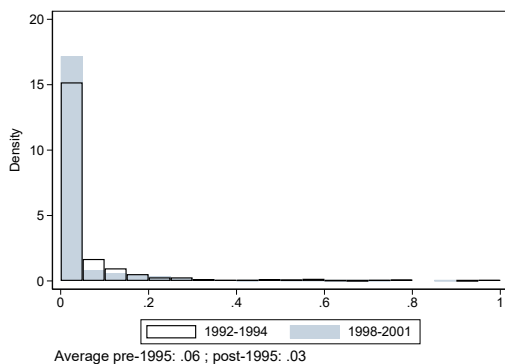


Incumbent candidates



Challenger candidates

Outsider candidates

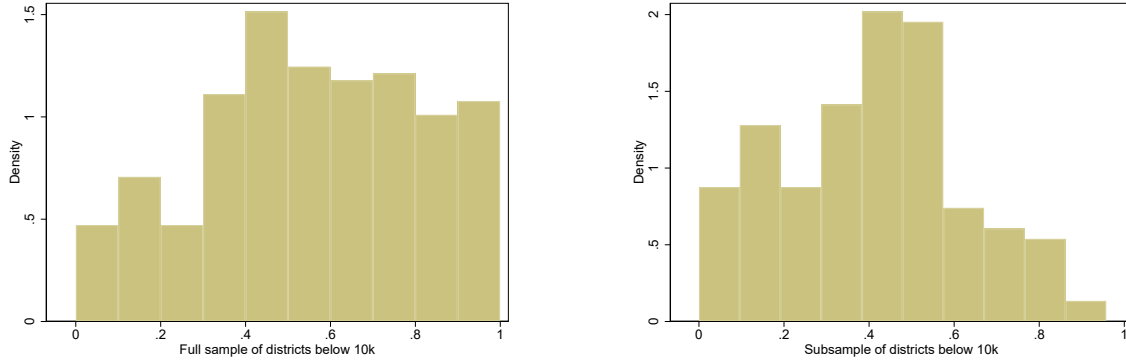


Left-wing candidates

Right-wing candidates

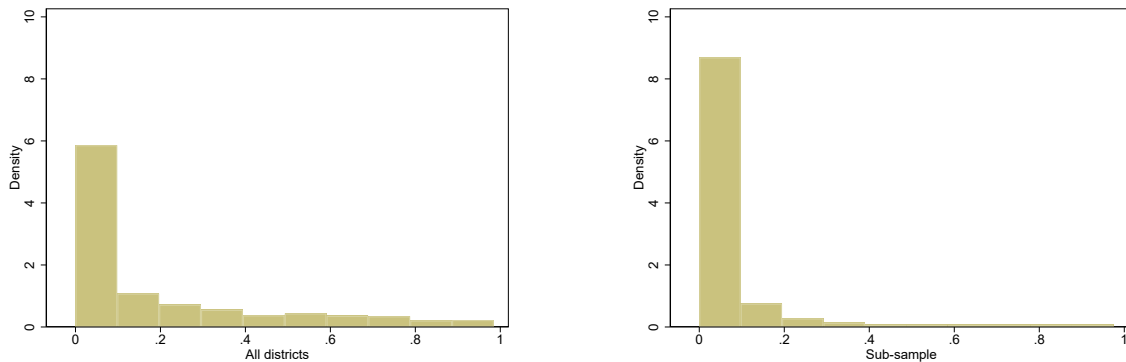
Notes: The level of analysis is the candidate and the sample only includes districts between 9,000 and 11,000 inhabitants, to focus on candidates running in districts close to the cutoff. The graphs are trimmed at 1, thus excluding a handful of candidates whose expenditures exceeded the ceiling. We exclude the 0.3 percent of candidates with at least one inconsistency in their contribution and expenditure data (see Appendix I).

Figure A4: Distribution of incumbent spending to ceiling ratios - Districts between 9,000 and 10,000 inhabitants



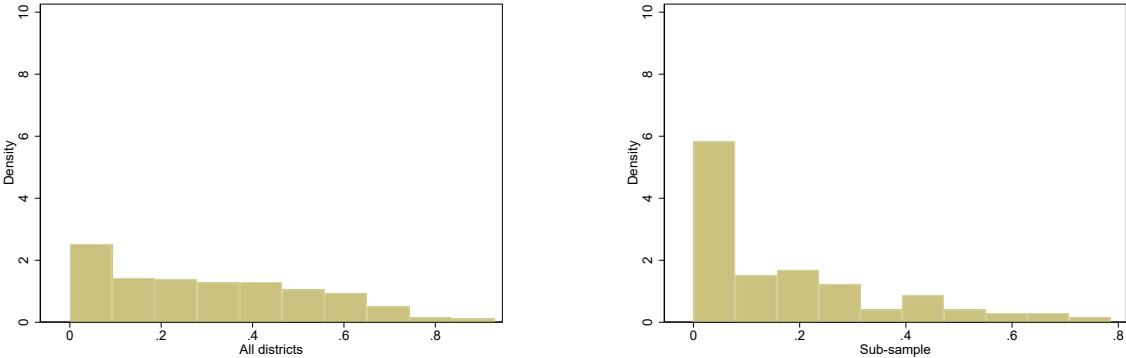
Notes: The left-hand side graph includes all districts between 9,000 and 10,000 inhabitants where the incumbent runs while the right-hand side focuses on districts within this sample where the predicted incumbent spending to ceiling ratio lies below the median predicted ratio on all districts (0.57). 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).

Figure A5: Distribution of incumbent corporate donations to ceiling ratios - 1994 election



Notes: The left-hand side graph includes all districts above the discontinuity in 1994 in which the incumbent runs. The right-hand side graph focuses on districts within this sample that belong to the top 25 percent of départements with the highest share of districts where the incumbent did not receive any corporate donations. We exclude districts with at least one candidate with at least one inconsistency in their contribution and expenditure data (see Appendix I).

Figure A6: Distribution of the share of total corporate donations in total contributions - 1994 election



Notes: The variable of interest is the sum of corporate donations to all candidates in the district divided by the sum of total contributions to all candidates. The left-hand side graph includes all districts above the discontinuity in 1994. The right-hand side focuses on districts within this sample that belong to the top 25 percent of départements with the highest share of districts where the incumbent did not receive any corporate donations. We exclude districts with at least one candidate with at least one inconsistency in their contribution and expenditure data (see Appendix I).

B. Validity tests

Table B1: Changes since election $t-1$

Outcome	(1)	(2)	(3)
	Linkable	Redistricted	Treated in $t-1$
Treatment	-0.007 (0.006)	0.007 (0.006)	0.052 (0.084)
Robust p -value	0.378	0.378	0.848
Observations	2,859	2,859	547
Polyn. order	1	1	1
Bandwidth	3,190	3,191	1,028
Mean, left of the threshold	1.000	0.000	0.365

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 3 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 3, for the same reason.

Table B2: General balance test

Outcome	(1)
	Predicted treatment
Treatment	0.020 (0.020)
Robust p -value	0.370
Observations	2,144
Polyn. order	1
Bandwidth	3,044
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 B3: General balance test - Including non-linkable districts

Outcome	(1)
	Predicted treatment
Treatment	0.020 (0.020)
Robust p -value	0.362
Observations	2,151
Polyn. order	1
Bandwidth	3,030
Mean, left of threshold	0.565

Notes: The sample also includes non-linkable districts. Other notes as in Table B2.

Table B4: Balance tests, sociodemographic characteristics

	(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 (0.002)	0.005 (0.005)	0.004** (0.002)	-0.004 (0.003)	-0.007 (0.005)	-0.000 (0.004)	-0.002 (0.004)	0.002 (0.003)	-0.002 (0.009)	-0.003 (0.005)	0.002 (0.005)	0.004 (0.002)	-0.003 (0.005)
Robust p -value	0.317	0.280	0.043	0.137	0.326	0.782	0.620	0.466	0.933	0.523	0.856	0.102	0.524
Observations	874	2,226	2,003	1,548	2,018	1,454	1,864	2,169	1,851	1,669	2,058	1,459	2,057
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	1.813	3.414	3.065	2.360	3.086	2.229	2.820	3.336	2.811	2.545	3.165	2.243	3.163
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: 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 B5: Balance tests, sociodemographic characteristics - 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 (0.002)	0.005 (0.005)	0.004** (0.002)	-0.004 (0.003)	-0.007 (0.005)	-0.000 (0.004)	-0.002 (0.004)	0.002 (0.003)	-0.001 (0.009)	-0.003 (0.005)	0.002 (0.005)	0.004 (0.002)	-0.003 (0.005)
Robust <i>p</i> -value	0.313	0.273	0.043	0.144	0.319	0.779	0.598	0.483	0.949	0.510	0.888	0.101	0.533
Observations	882	2,248	1,990	1,564	2,020	1,463	1,870	2,188	1,865	1,673	2,043	1,474	2,067
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,824	3,427	3,036	2,380	3,081	2,242	2,827	3,357	2,830	2,548	3,136	2,256	3,174
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

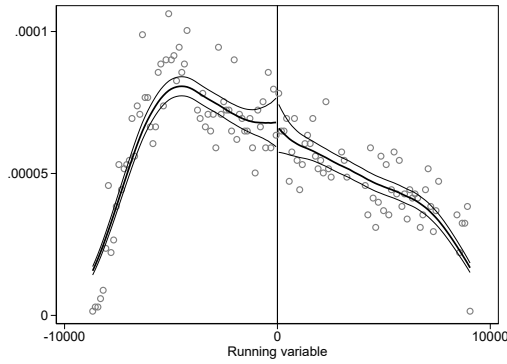
Notes: The sample also includes non-linkable districts. Other notes as in Table B4.

Table B6: Placebo tests, main outcomes defined in $t-1$

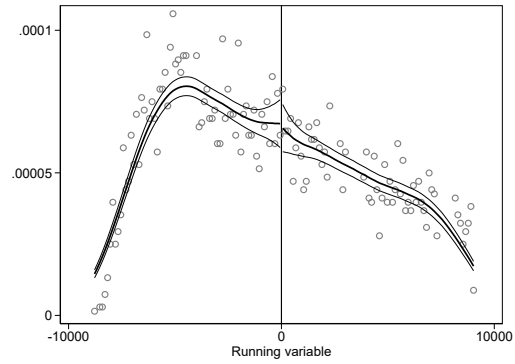
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger win	Outsider	Victory	Winner margin in first round	Incumbent	Challenger run
Treatment	0.063 (0.054)	0.001 (0.024)	-0.042 (0.051)	-0.061 (0.050)	-0.021 (0.018)	0.057 (0.043)	-0.010 (0.047)
Robust p -value	0.402	0.963	0.530	0.197	0.183	0.286	0.889
Observations	1,471	1,317	1,638	1,705	1,325	1,728	1,426
Polyn. order	1	1	1	1	1	1	1
Bandwidth	2,942	2,649	3,282	3,413	2,666	3,437	2,846
Mean, left of threshold	0.552	0.046	0.357	0.322	0.165	0.728	0.229

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.

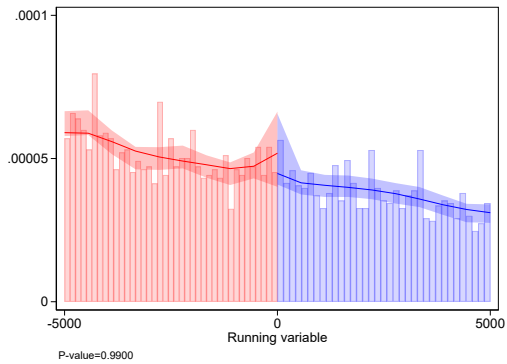
Figure B1: McCrary (2008) and Cattaneo et al. (2018) density tests



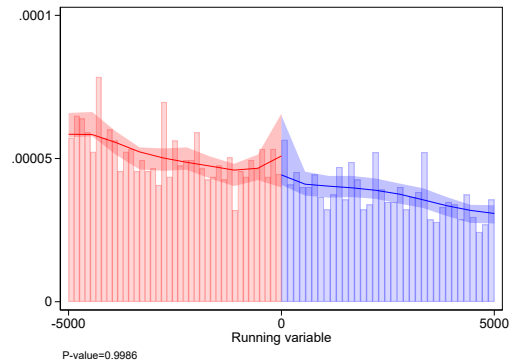
McCrary test - Main sample



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



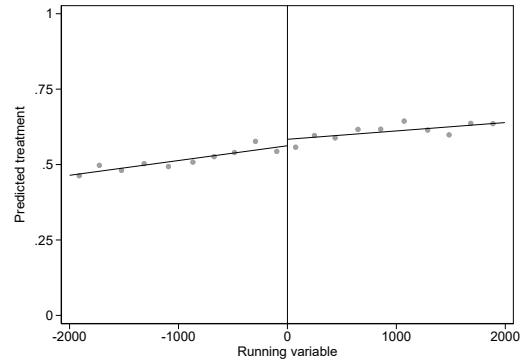
RD Density test- Main sample



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

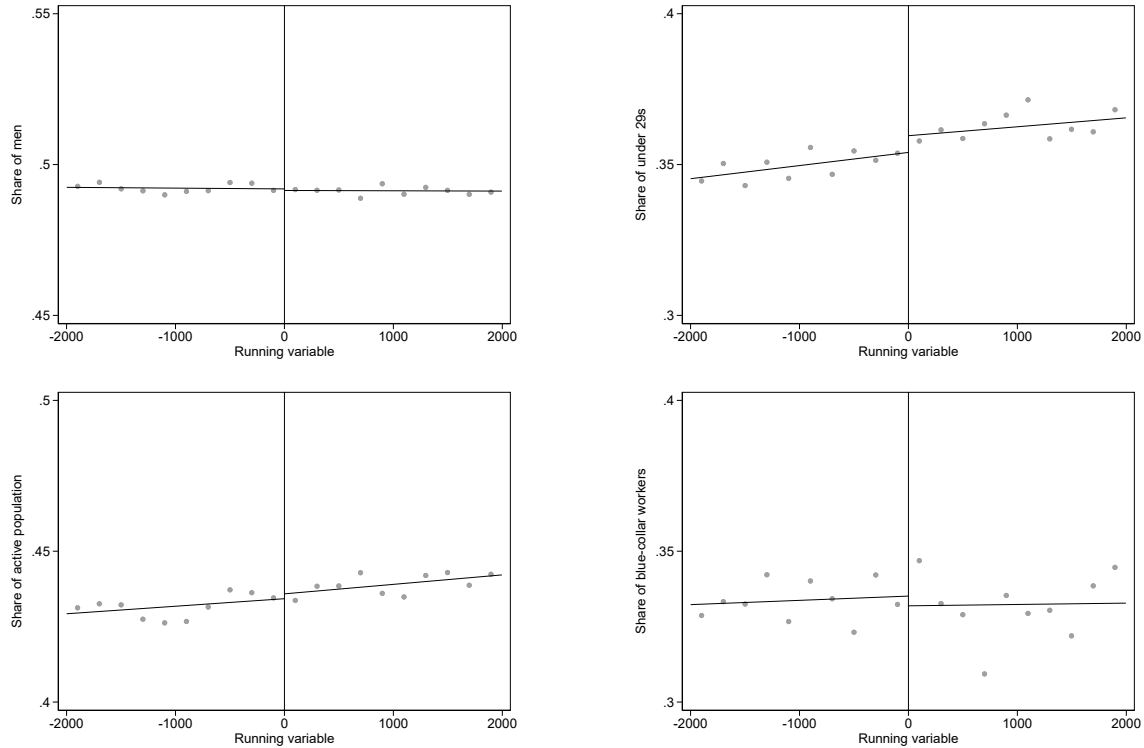
Notes: We test for a jump at the threshold in the density of the running variable (the district population centered around 9,000 inhabitants), using McCrary (2008)'s method in the top panel. The solid line represents the density of the running variable, while the thin lines represent the confidence intervals. 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: General balance test



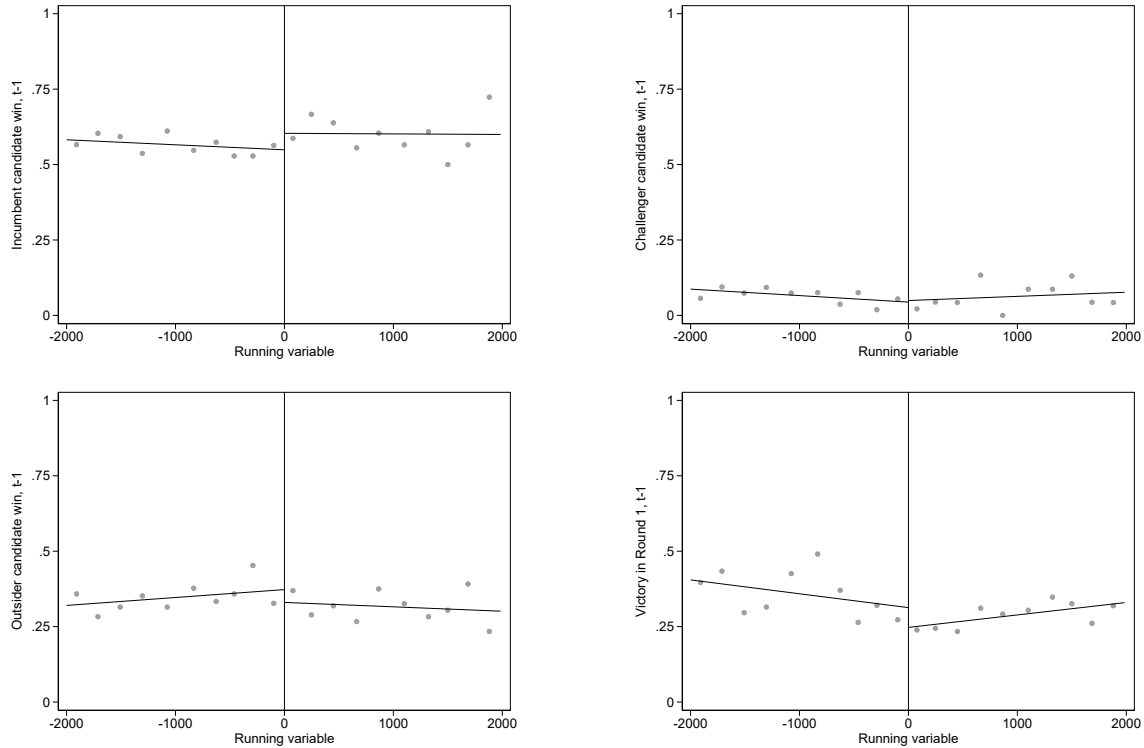
Notes: Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the district population centered around 9,000 inhabitants) is split into quantile-spaced bins. The continuous lines represent a linear fit. To facilitate visualization, the graph is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

Figure B3: Balance tests, sociodemographic characteristics



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 linear 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.

Figure B4: Placebo tests, main outcomes defined in $t-1$



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 linear 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.

C. Falsification and robustness tests

Robustness tests - Sample definition

Table C1: Impact on winner identity - Excluding 2008

Outcome	(1)	(2)	(3)
	Incumbent win	Challenger win	Outsider win
Treatment	-0.100* (0.055)	0.078*** (0.025)	0.022 (0.048)
Robust p -value	0.065	0.002	0.599
Observations	1,332	1,297	1,772
Polyn. order	1	1	1
Bandwidth	1,887	1,837	2,506
Mean, left of threshold	0.635	0.007	0.337

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 - Excluding 2008

Outcome	(1)	(2)	(3)
	ENC r1	Victory in first round	Winner vote margin in first round
Treatment	0.136 (0.117)	-0.103** (0.049)	-0.021 (0.018)
Robust p -value	0.177	0.033	0.201
Observations	1,399	1,736	1,644
Polyn. order	1	1	1
Bandwidth	1,989	2,455	2,321
Mean, left of threshold	3.351	0.312	0.180

Notes: as in Table C1.

Table C3: Impact on competition - Including non-linkable districts

	(1)	(2)	(3)
Outcome	ENC r1	Victory in first round	Winner vote margin in first round
Treatment	0.073 (0.085)	-0.111*** (0.044)	-0.030** (0.016)
Robust <i>p</i> -value	0.280	0.010	0.047
Observations	2,768	2,219	2,080
Polyn. order	1	1	1
Bandwidth	3,097	2,469	2,312
Mean, left of threshold	3.251	0.355	0.190

Notes: We include all departmental elections, including districts that we cannot link over time. Other notes as in Table C1.

Table C4: Impact on winning and vote shares, conditional on running - Excluding 2008

Outcome	(1)	(2)	(3)	(4)
	Incumbent		Challenger	
	win	vote share, R1	win	vote share, R1
<i>Panel A. Unconditional effects</i>				
Treatment	-0.100*	-0.055**	0.078***	0.044***
	(0.055)	(0.025)	(0.025)	(0.015)
Robust <i>p</i> -value	0.065	0.024	0.002	0.003
Observations	1,332	1,381	1,297	1,413
Polyn. order	1	1	1	1
Bandwidth	1,887	1,969	1,837	2,008
Mean	0.635	0.346	0.007	0.043
<i>Panel B. Conditional effects</i>				
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 entry - Excluding 2008

Outcome	(1)	(2)	(3)	(4)	(5)
	Incumbent run	Challenger run	Number of Outsiders	Number of Candidates	Turnout r1
Treatment	-0.073 (0.043)	0.112** (0.050)	0.030 (0.156)	0.103 (0.155)	0.014 (0.010)
Robust <i>p</i> -value	0.103	0.016	0.722	0.353	0.115
Observations	1,799	1,317	1,624	1,345	1,799
Polyn. order	1	1	1	1	1
Bandwidth	2,542	1,869	2,296	1,912	2,542
Mean, left of threshold	0.745	0.180	3.782	5.278	0.639

Notes as in Table C1.

Table C6: Impact on entry - Including non-linkable districts

Outcome	(4)	(5)
	Number of Candidates	Turnout r1
Treatment	0.044 (0.118)	0.009 (0.009)
Robust <i>p</i> -value	0.523	0.262
Observations	2,460	2,330
Polyn. order	1	1
Bandwidth	2,737	2,600
Mean, left of threshold	5.055	0.656

Notes as in Table C3.

Table C7: Impact on winning orientation, polarization, and winner’s representativeness - Excluding 2008

	(1)	(2)	(3)	(4)	(5)
Outcome	Left win	Right win	Polarization	Vote share winner’s orientation	Top orientation winning
Treatment	0.081 (0.054)	-0.056 (0.049)	0.025 (0.087)	-0.016 (0.016)	-0.039 (0.034)
Robust <i>p</i> -value	0.111	0.242	0.777	0.313	0.246
Observations	1,939	2,347	1,800	1,625	1,557
Polynomial order	1	1	1	1	1
Bandwidth	2,741	3,342	2,942	2,300	2,216
Mean, left of threshold	0.480	0.467	4.906	0.581	0.918

Notes as in Table C1.

Table C8: Impact on winning orientation, polarization, and winner’s representativeness - Including non-linkable districts

	(1)	(2)	(3)	(4)	(5)
Outcome	Left win	Right win	Polarization	Vote share winner’s orientation	Top orientation winning
Treatment	0.084* (0.047)	-0.055 (0.041)	-0.076 (0.083)	-0.003 (0.014)	-0.044 (0.029)
Robust <i>p</i> -value	0.063	0.189	0.369	0.818	0.116
Observations	2,554	3,370	2,179	2,320	1,833
Polynomial order	1	1	1	1	1
Bandwidth	2,818	3,763	2,776	2,577	2,042
Mean, left of threshold	0.475	0.477	4.868	0.583	0.924

Notes as in Table C3.

Falsification tests

Table C9: Placebo discontinuities - Impact on winner identity

Panel A. Incumbent wins

	(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 (0.042)	0.015 (0.043)	-0.011 (0.040)	-0.008 (0.045)	0.005 (0.044)	0.049 (0.036)	0.013 (0.032)	0.014 (0.038)	-0.043 (0.033)	-0.045 (0.038)
Robust p -value	0.599	0.538	0.792	0.744	0.921	0.196	0.679	0.618	0.277	0.322
Observations	2,203	2,003	2,286	1,656	1,900	2,645	3,415	2,676	3,488	2,766
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	2,015	1,909	2,234	1,671	1,948	3,303	4,381	3,509	4,769	3,917
Mean, left of threshold	0.629	0.590	0.591	0.599	0.593	0.578	0.596	0.591	0.611	0.598

Panel B. Challenger wins

	(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 (0.022)	-0.024 (0.026)	-0.003 (0.021)	0.016 (0.022)	-0.015 (0.021)	-0.036* (0.022)	0.006 (0.016)	0.012 (0.018)	0.014 (0.019)	-0.015 (0.021)
Robust p -value	0.137	0.293	0.921	0.360	0.639	0.093	0.755	0.495	0.458	0.484
Observations	1,594	1,576	2,233	1,565	2,229	1,744	2,956	2,767	2,803	2,569
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,478	1,503	2,174	1,592	2,301	2,118	3,795	3,679	3,813	3,649
Mean, left of threshold	0.028	0.083	0.070	0.051	0.067	0.076	0.054	0.055	0.056	0.068

Panel C. Outsider wins

	(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 (0.041)	0.005 (0.042)	0.007 (0.040)	0.007 (0.042)	0.010 (0.040)	-0.029 (0.030)	-0.027 (0.032)	-0.032 (0.036)	0.030 (0.031)	0.073** (0.033)
Robust p -value	0.886	0.850	0.822	0.787	0.865	0.371	0.378	0.306	0.449	0.046
Observations	2,077	1,903	2,270	1,758	1,992	3,455	3,020	2,580	3,799	3,491
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,895	1,812	2,211	1,783	2,049	4,258	3,863	3,391	5,194	4,936
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 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 C10: Placebo discontinuities - Impact on competitiveness*Panel A. Victory in first round*

	(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	-0.016	-0.021	0.005	-0.051
	(0.053)	(0.052)	(0.051)	(0.047)	(0.051)	(0.052)	(0.048)	(0.044)	(0.037)	(0.039)
Robust <i>p</i> -value	0.082	0.212	0.923	0.777	0.328	0.554	0.565	0.550	0.863	0.227
Observations	1,842	1,994	1,943	2,173	1,964	1,660	1,895	2,207	3,165	2,945
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,674	1,897	1,916	2,174	2,013	2,003	2,403	2,934	4,327	4,150
Mean, left of threshold	0.566	0.507	0.431	0.397	0.404	0.358	0.348	0.334	0.294	0.305

Panel B. Winner's vote margin in first round

	(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	-0.065***	-0.002	-0.009	-0.013	-0.003	0.009	-0.018	-0.010	0.004	-0.001
	(0.025)	(0.020)	(0.020)	(0.016)	(0.017)	(0.016)	(0.017)	(0.015)	(0.013)	(0.015)
Robust <i>p</i> -value	0.008	0.993	0.619	0.483	0.975	0.687	0.234	0.478	0.694	0.905
Observations	1,293	1,919	2,238	2,512	2,086	2,096	1,789	2,219	2,827	2,861
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,197	1,827	2,176	2,494	2,147	2,556	2,292	2,950	3,858	4,039
Mean, left of threshold	0.275	0.220	0.224	0.219	0.213	0.190	0.201	0.189	0.180	0.177

Notes as in Table C9.

Table C11: Placebo discontinuities - Impact on entry*Panel A. Incumbent runs*

	(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 (0.042)	0.033 (0.039)	0.026 (0.037)	-0.003 (0.037)	0.005 (0.032)	0.052 (0.032)	0.057* (0.034)	0.009 (0.034)	-0.061 (0.034)	-0.074** (0.030)
Robust <i>p</i> -value	0.615	0.496	0.683	0.740	0.848	0.109	0.091	0.717	0.103	0.024
Observations	1,960	2,172	2,276	1,850	2,819	2,854	2,527	2,457	2,430	3,192
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1786	2068	2217	1877	2911	3552	3259	3230	3342	4483
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. Challenger runs

	(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 (0.044)	-0.029 (0.041)	-0.025 (0.040)	0.030 (0.038)	-0.049 (0.035)	-0.050 (0.040)	0.073** (0.033)	0.079* (0.037)	-0.064 (0.045)	-0.040 (0.039)
Robust <i>p</i> -value	0.210	0.381	0.497	0.381	0.280	0.159	0.050	0.059	0.117	0.247
Observations	1,375	1,983	1,993	1,901	2,631	1,734	2,759	2,197	1,562	1,910
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,274	1,890	1,975	1,939	2,746	2,105	3,558	2,920	2,180	2,742
Mean, left of threshold	0.194	0.275	0.274	0.255	0.283	0.219	0.180	0.202	0.291	0.258

Notes as in Table C9.

Quadratic specification and inclusion of controls**Table C12: Impact on the main outcomes - Quadratic fit**

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger win	Outsider	Victory	Winner margin in first round	Incumbent	Challenger run
Treatment	-0.139*** (0.048)	0.057*** (0.021)	0.102** (0.047)	-0.125** (0.051)	-0.032* (0.018)	-0.089* (0.044)	0.106** (0.045)
Robust <i>p</i> -value	0.004	0.008	0.027	0.015	0.057	0.078	0.021
Observations	2,787	3,481	2,858	3,375	3,506	2,842	2,810
Polyn. order	2	2	2	2	2	2	2
Bandwidth	3,140	3,946	3,221	3,806	3,962	3,193	3,168
Mean, left of threshold	0.674	0.0160	0.281	0.353	0.190	0.785	0.169

Notes as in Table C9, except for the fact that the polynomial order is two in all columns.

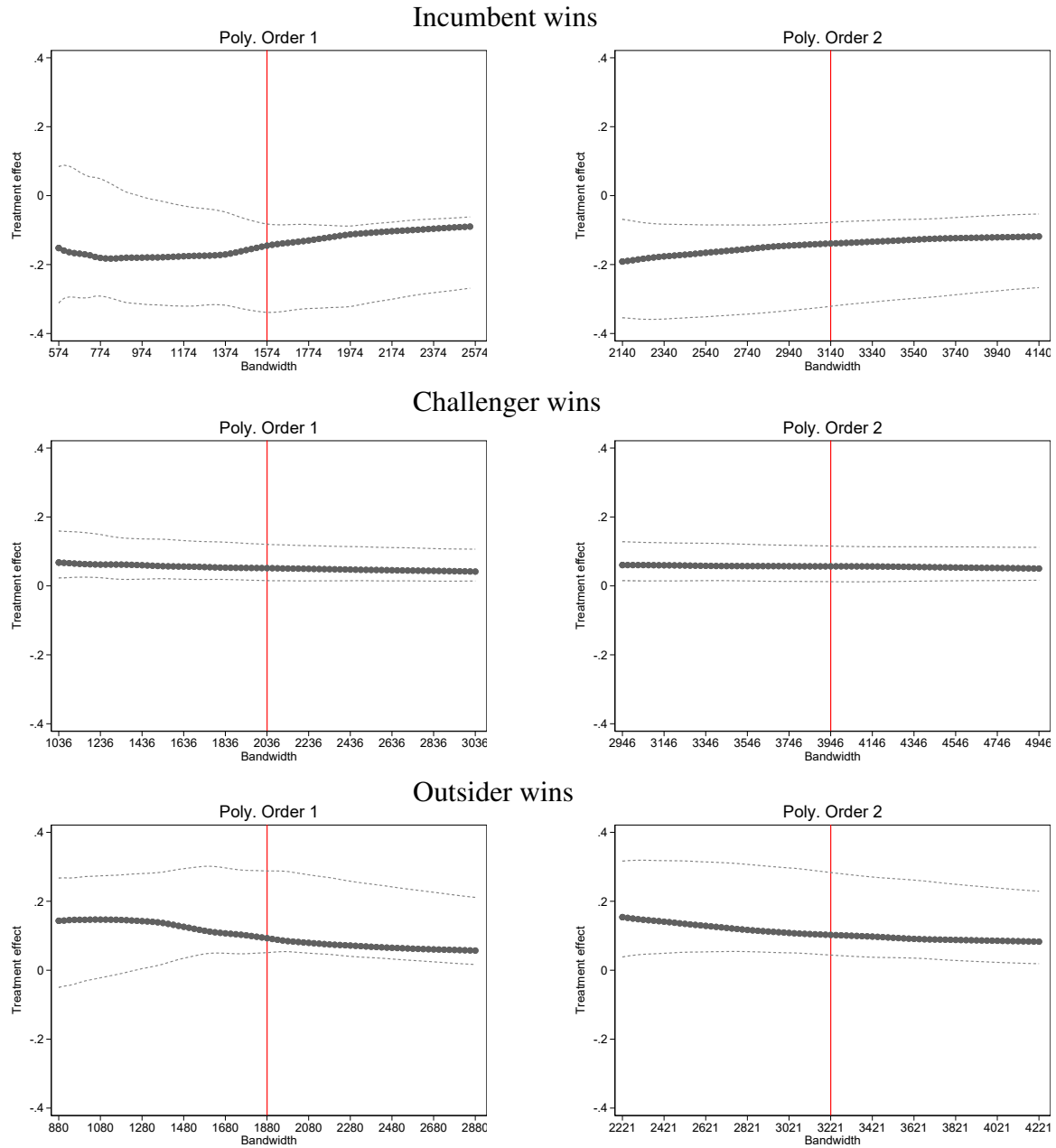
Table C13: Impact on the main outcomes - Including controls

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger win	Outsider	Victory in first round	Winner margin	Incumbent	Challenger run
Treatment	-0.112** (0.044)	0.048** (0.020)	0.067* (0.040)	-0.113*** (0.042)	-0.023* (0.015)	-0.069** (0.031)	0.078** (0.037)
Robust <i>p</i> -value	0.010	0.018	0.079	0.005	0.092	0.023	0.026
Observations	1,562	1,851	1,868	2,209	2,254	2,795	1,943
Polyn. order	1	1	1	1	1	1	1
Bandwidth	1,752	2,074	2,092	2,474	2,523	3,151	2,192
Mean, left of threshold	0.672	0.019	0.297	0.354	0.190	0.765	0.176

Notes: We add as controls the sociodemographic variables shown in Appendix Table B4: 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 C9.

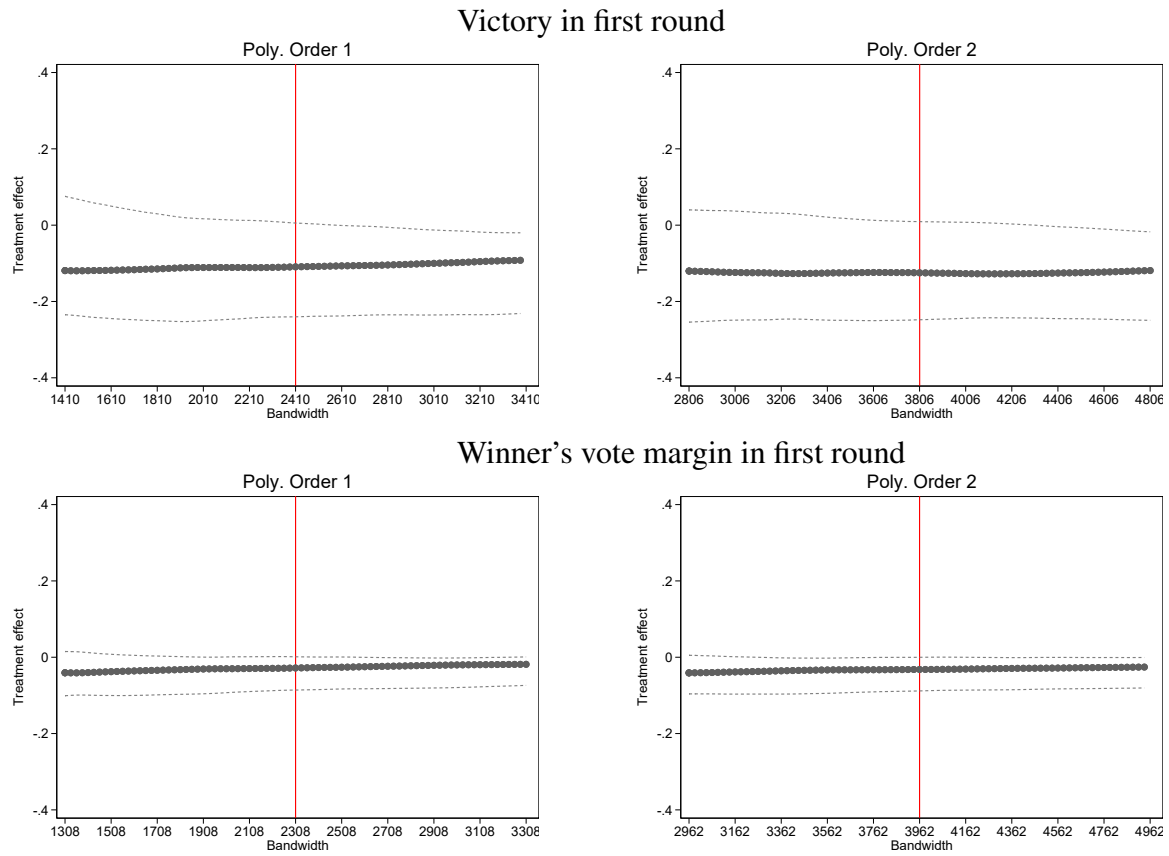
Sensitivity to bandwidth choice

Figure C1: Sensitivity to bandwidth - Impact on winner identity



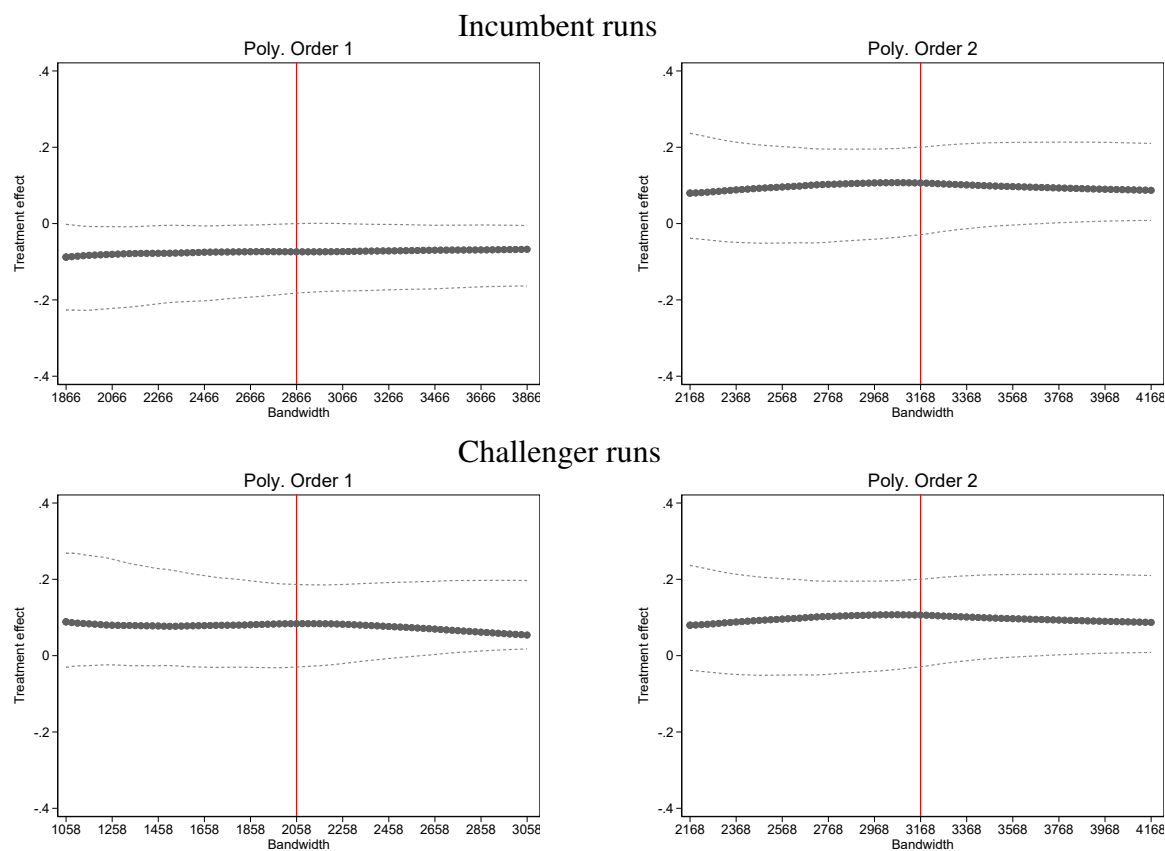
Notes: We show the sensitivity of the point estimates to bandwidth choice, either using a linear (left-hand side) or quadratic specification (right-hand side). The vertical red line represents the value of the MSERD optimal bandwidth. The dots represent the estimated treatment effect using different bandwidths, while the dotted lines represent the 95 percent robust confidence intervals computed according to Calonico et al. (2014). We report all estimates for values of the bandwidth from -1,000 to +1,000 inhabitants, in steps of 25 inhabitants.

Figure C2: Sensitivity to bandwidth - Impact on competitiveness



Notes as in Figure C1.

Figure C3: Sensitivity to bandwidth - Impact on entry



Notes as in Figure C1.

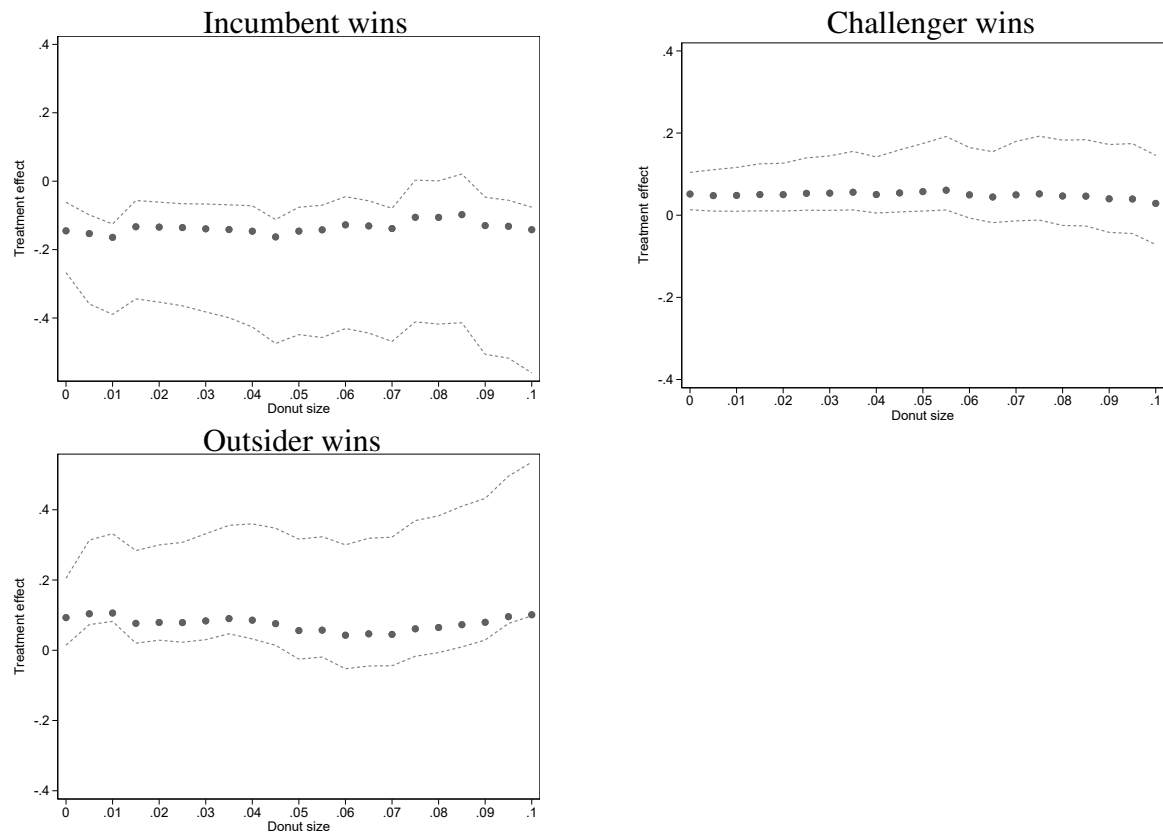
Table C14: Impact on the main outcomes - Bandwidth of 1,000 inhabitants

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Incumbent	Challenger	Outsider	Victory	Winner margin	Incumbent	Challenger
		win		in first round		run	run
Treatment	-0.179**	0.070**	0.146	-0.099	-0.039*	-0.105	0.094
	(0.056)	(0.025)	(0.056)	(0.065)	(0.023)	(0.053)	(0.054)
Robust <i>p</i> -value	0.039	0.010	0.121	0.324	0.097	0.205	0.143
Observations	927	927	927	927	927	927	927
Polyn. order	1	1	1	1	1	1	1
Bandwidth	1,000	1,000	1,000	1,000	1,000	1,000	1,000
Mean, left of threshold	0.711	0.002	0.248	0.341	0.190	0.820	0.175

Notes: We use a bandwidth of 1,000 inhabitants for all outcomes. Other notes as in Table C9.

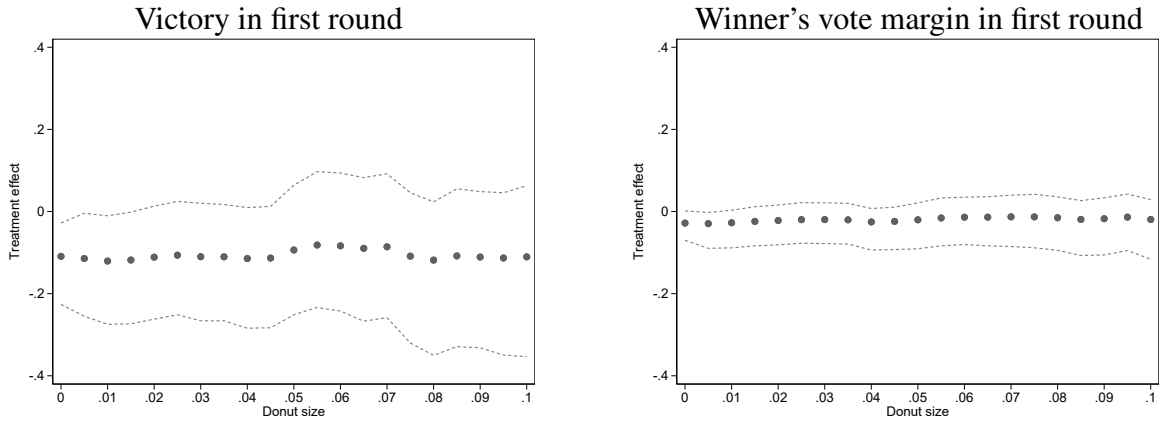
Donut estimations

Figure C4: Donut estimations - Impact on winner identity



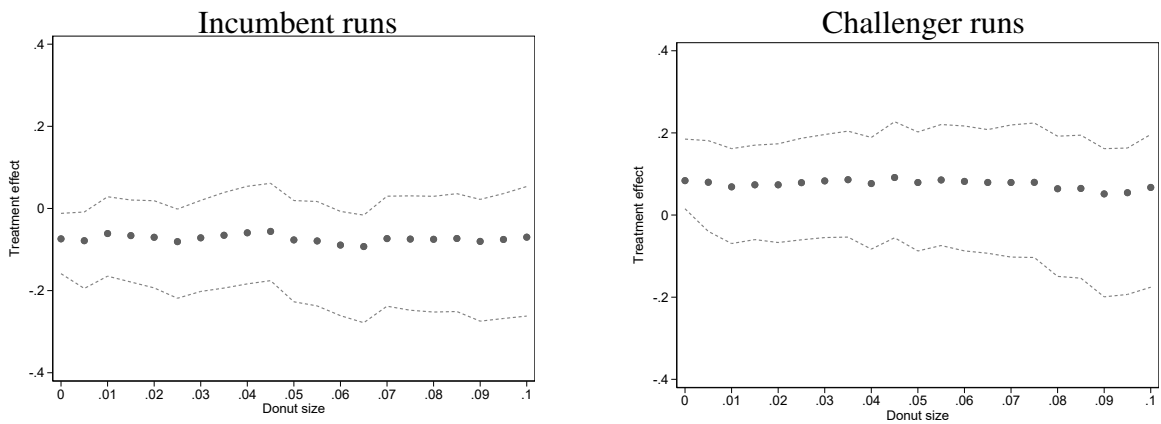
Notes: We show the sensitivity of the point estimates to removing observations close to the threshold. We define the donut size as a percentage of the optimal bandwidth used in the main regression, and we report all estimates obtained for values of the donut hole from 1 to 10 percent, in steps of 0.5 percent, for each outcome separately. The dots represent the estimated treatment effect using different donut holes, while the dotted lines represent the 95 percent robust confidence intervals computed according to Calonico et al. (2014).

Figure C5: Donut estimations - Impact on competitiveness



Notes as in Figure C4.

Figure C6: Donut estimations - Impact on entry



Notes as in Figure C4.

Table C15: Impact on the main outcomes - Donut size: 1%

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger win	Outsider	Victory	Winner margin in first round	Incumbent	Challenger run
Treatment	-0.164*** (0.047)	0.048** (0.020)	0.106*** (0.044)	-0.121** (0.046)	-0.027* (0.016)	-0.061 (0.033)	0.069 (0.039)
Robust <i>p</i> -value	0.000	0.021	0.001	0.035	0.068	0.167	0.434
Observations	1,375	1,795	1,662	2,126	2,041	2,549	1,808
Polyn. order	1	1	1	1	1	1	1
Bandwidth	1,574	2,036	1,880	2,410	2,308	2,866	2,058
Mean, left of threshold	0.701	0.020	0.277	0.360	0.188	0.767	0.182

Notes: We drop observations that deviate from the threshold by at most 1 percent of the optimal bandwidth used in the main regression. Other notes as in Table C9.

Table C16: Impact on the main outcomes - Donut size: 2%

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger win	Outsider	Victory	Winner margin in first round	Incumbent	Challenger run
Treatment	-0.134*** (0.049)	0.050** (0.021)	0.079** (0.045)	-0.111* (0.047)	-0.022 (0.017)	-0.070 (0.035)	0.074 (0.040)
Robust <i>p</i> -value	0.005	0.021	0.018	0.076	0.184	0.107	0.385
Observations	1,359	1,780	1,644	2,104	2,021	2,521	1,791
Polyn. order	1	1	1	1	1	1	1
Bandwidth	1,574	2,036	1,880	2,410	2,308	2,866	2,058
Mean, left of threshold	0.694	0.021	0.283	0.355	0.186	0.766	0.186

Notes: We drop observations that deviate from the threshold by at most 2 percent of the optimal bandwidth used in the main regression. Other notes as in Table C9.

Table C17: Impact on the main outcomes - Donut size: 5%

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger win	Outsider	Victory	Winner margin in first round	Incumbent	Challenger run
Treatment	-0.146*** (0.055)	0.057** (0.026)	0.056* (0.050)	-0.094 (0.049)	-0.020 (0.018)	-0.077* (0.038)	0.079 (0.044)
Robust <i>p</i> -value	0.006	0.028	0.095	0.244	0.222	0.098	0.438
Observations	1,314	1,715	1,585	2,031	1,948	2,429	1,728
Polyn. order	1	1	1	1	1	1	1
Bandwidth	1,574	2,036	1,880	2,410	2,308	2,866	2,058
Mean, left of threshold	0.671	0.026	0.293	0.351	0.187	0.759	0.195

Notes: We drop observations that deviate from the threshold by at most 5 percent of the optimal bandwidth used in the main regression. Other notes as in Table C9.

Additional robustness tests

Table C18: Impact on the main outcomes - Excluding observations with a running variable ranging between +/-200 and +/-500

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger win	Outsider	Victory	Winner margin in first round	Incumbent	Challenger run
Treatment	-0.110** (0.056)	0.058** (0.024)	0.047* (0.052)	-0.127* (0.053)	-0.035* (0.020)	-0.048 (0.039)	0.098 (0.047)
Robust <i>p</i> -value	0.011	0.016	0.066	0.067	0.076	0.296	0.122
Observations	1,098	1,524	1,388	1,859	1,773	2,284	1,537
Polyn. order	1	1	1	1	1	1	1
Bandwidth	1,574	2,036	1,880	2,410	2,308	2,866	2,058
Mean, left of threshold	0.654	0.009	0.328	0.366	0.192	0.751	0.154

Notes: We exclude observations with a running variable ranging between +/-200 and +/-500. Other notes as in Table C9.

Table C19: Impact on the main outcomes - Excluding very large districts

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger win	Outsider	Victory	Winner margin in first round	Incumbent	Challenger run
Treatment	-0.139*** (0.045)	0.052** (0.020)	0.080** (0.041)	-0.111** (0.046)	-0.027* (0.016)	-0.074** (0.033)	0.080** (0.041)
Robust <i>p</i> -value	0.002	0.012	0.042	0.019	0.065	0.028	0.036
Observations	1470	1744	1851	1988	2140	2494	1610
Polyn. order	1	1	1	1	1	1	1
Bandwidth	1648	1967	2073	2235	2401	2777	1811
Mean, left of threshold	0.678	0.018	0.296	0.350	0.190	0.767	0.179

Notes: We exclude districts with more than 18,000 inhabitants Other notes as in Table C9.

Appendix II: Additional analyses on municipal elections

D. Additional tables and figures

Table D1: Summary statistics - Municipal elections

	Mean	S.D.	Min.	Max.	Observations
Number of inhabitants	15,479	26,132	3,500	447,340	7,653
Registered voters	9,937	15,029	1,024	254,538	7,653
Proportion of turnout	0.640	0.078	0.329	1.000	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.

Table D2: 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

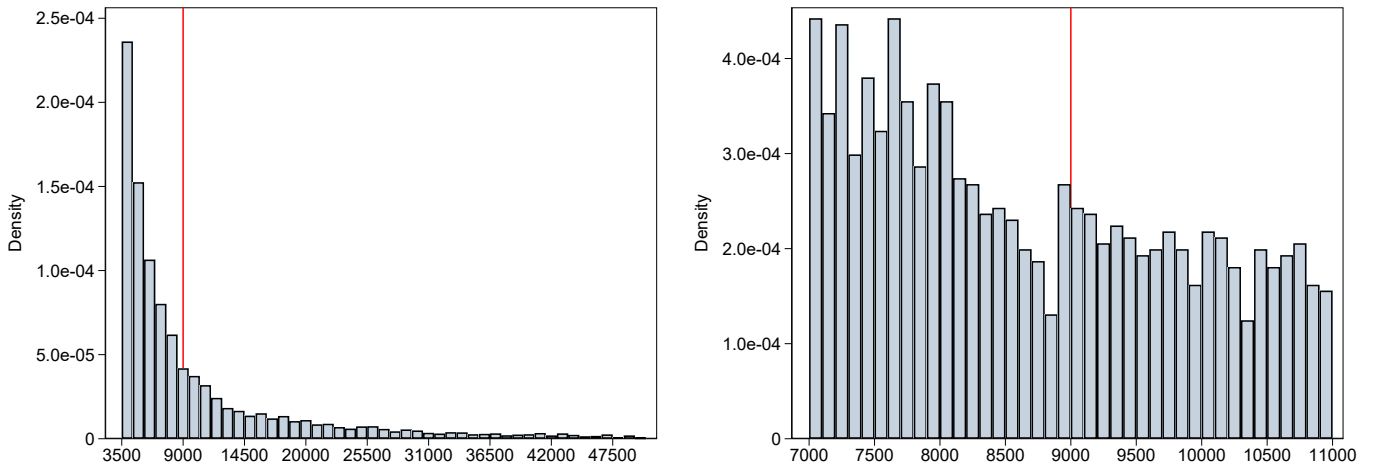
Notes: This table provides average measures by candidate and by election for each of the outcomes defined as a percentage of the spending ceiling in the first two columns and in euro per capita in the last two columns. We focus on districts close to the cutoff (between 9,000 and 11,000 inhabitants). For municipal elections, we provide statistics for the 2008 and 2014 elections only, as we did not digitize the CNCCFP's booklets reporting the expenditures and breakdown of contributions received by candidates for the 2001 municipal elections, for which the data were only available for half of the candidates. To make districts across municipal and departmental elections comparable, 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 D3: Correlation between vote shares and spending by type of election

Outcome	(1)	(2)	(3)	(4)	(5)	(6)
	Vote share in the first round					
	All districts			Close to the discontinuity		
	Departmental	Municipal	Both	Departmental	Municipal	Both
Spending per capita	0.332*** (0.008)	0.196*** (0.005)	0.341*** (0.008)	0.301*** (0.012)	0.174*** (0.008)	0.308*** (0.012)
Spending*Municipal			-0.146*** (0.008)			-0.129*** (0.013)

Notes: We consider the 2008 departmental and the 2008 municipal elections, which took place on the same day. We focus on districts above the threshold (for which we have spending data) and that we can link in time (our main sample of analysis). We conduct the analysis at the candidate level and we exclude the 0.3 percent of candidates with at least one inconsistency in their contribution and expenditure data (see Appendix I). The dependent variable is the candidate's vote share in the first round. The independent variable is the candidate's expenditures per capita. The regression includes the following controls: district fixed effects, dummies for the six political orientations (far-left, left, center, right, far-right, and non-classified), a dummy equal to one if the candidate is affiliated to a party, the vote share of the candidate's orientation in the last presidential election (2007), a dummy equal to one if the candidate has the same orientation as the district's incumbent, the candidate's gender, a dummy equal to one if the candidate is the incumbent, the challenger, or an outsider, and the candidate's ParlGov ideological score. To avoid dropping observations, for each control variable, we include a dummy equal to one when the variable is missing and replace missing values by 0s. In columns 4 to 6, the sample is restricted to municipalities between 9,000 and 10,000 inhabitants.

Figure D1: Population distribution of municipalities



Notes: The vertical red line corresponds to the 9,000 inhabitants threshold. The left-hand side graph considers all districts, while the right-hand side graph focuses on districts close to the threshold, between 7,000 and 11,000 inhabitants.

E. Validity tests

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

Outcome	(1)	(2)	(3)
	Linkable	Redistricted	Treated in $t-1$
Treatment	-0.054 (0.031)	0.004 (0.008)	-0.044 (0.115)
Robust p -value	0.117	0.698	0.516
Observations	1,006	1,615	418
Polyn. order	1	1	1
Bandwidth	1,329	2,006	919
Mean, left of the threshold	0.978	0.004	0.414

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 3 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 (0.038)
Robust p -value	0.757
Observations	790
Polyn. order	1
Bandwidth	1,642
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	0.989
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

Outcomes	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	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 (0.010)	-0.001 (0.004)	0.002 (0.004)	-0.003 (0.010)	-0.004 (0.007)	0.004 (0.010)	0.001 (0.012)	-0.030 (0.020)	0.013* (0.008)	0.001 (0.008)	0.000 (0.004)	0.002 (0.002)
Robust p -value	0.371	0.856	0.705	0.563	0.940	0.524	0.646	0.816	0.101	0.085	0.818	0.702	0.334
Observations	878	791	975	946	1,056	1,072	970	762	540	761	805	662	1,165
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	1.816	1.680	2.002	1.951	2.138	2.166	1.995	1.608	1.159	1.600	1.695	1.398	2.347
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

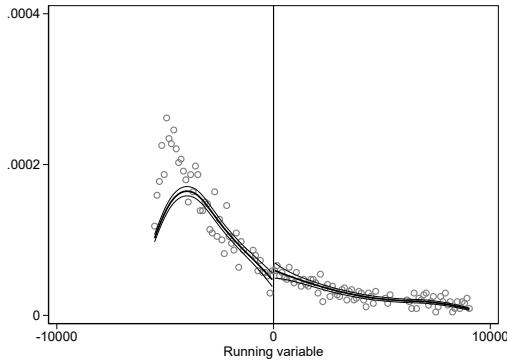
Notes: Clustered standard errors are in parentheses. Robust p -values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10 percent, respectively. Each column reports the results from a separate local polynomial regression. The independent variable is a dummy 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

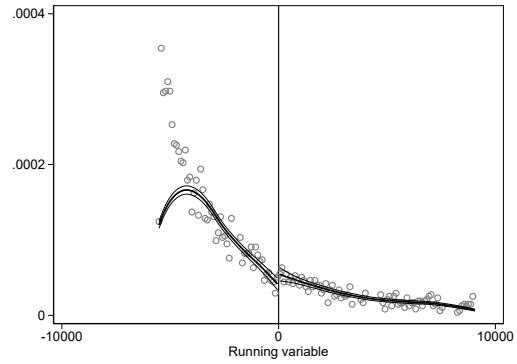
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Outcomes	Men	Under 29	30-44	45-59	Over 60	Working population	Unemployed	Skilled workers	Blue-collar	Employees	Intermediate	Artisans	Farmers
Treatment	0.004 (0.003)	0.003 (0.009)	-0.000 (0.004)	0.001 (0.004)	-0.006 (0.010)	-0.002 (0.006)	0.001 (0.009)	0.005 (0.012)	-0.031* (0.019)	0.010 (0.007)	0.003 (0.008)	-0.001 (0.004)	0.002 (0.002)
Robust <i>p</i> -value	0.303	0.893	0.945	0.745	0.709	0.763	0.829	0.539	0.082	0.140	0.622	0.996	0.439
Observations	939	869	1,358	959	1,047	1,492	1,129	782	583	924	885	946	1,280
Polyn. order	1	1	1	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,802	1,699	2,514	1,839	1,999	2,713	2,130	1,534	1,182	1,779	1,723	1,815	2,385
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 tests



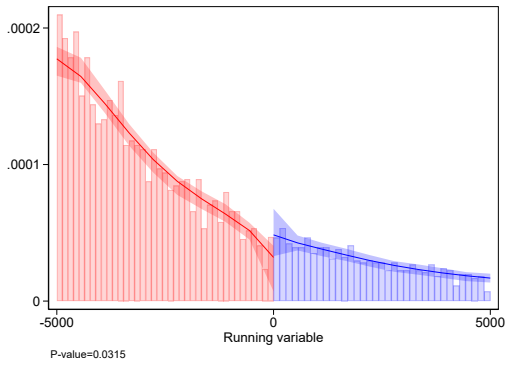
McCrary test - Main sample



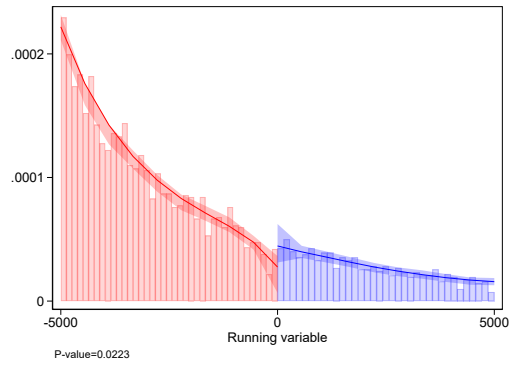
McCrary test
Including non-linkable districts

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



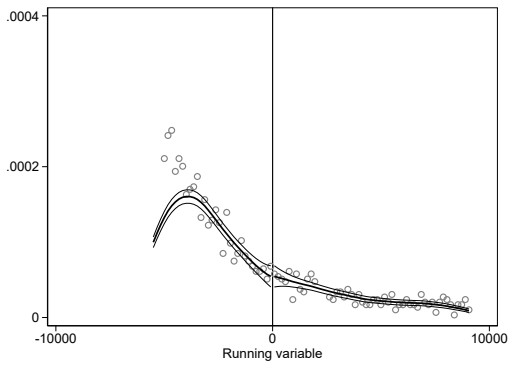
RD Density test - Main sample



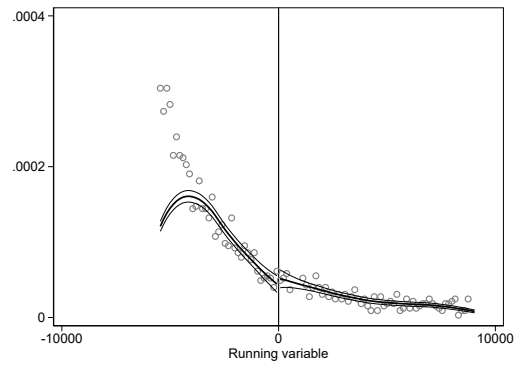
RD Density test
Including non-linkable districts

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).

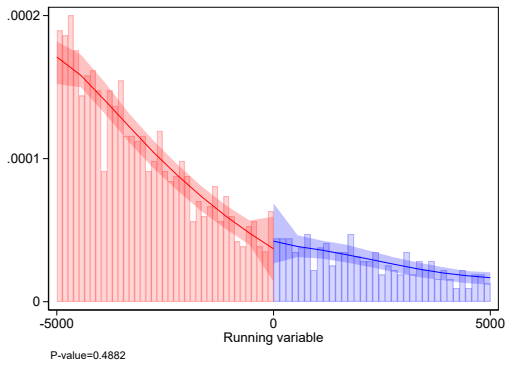
Figure E3: McCrary (2008) and Cattaneo et al. (2018) density tests - 2001 elections



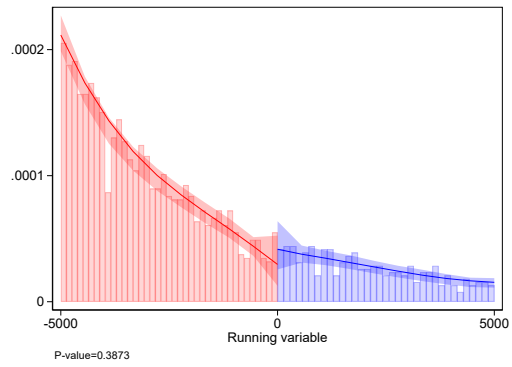
McCrary test - Main sample



McCrary test
Including non-linkable districts



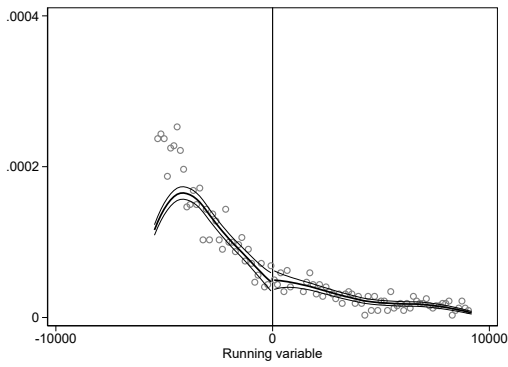
RD Density test - Main sample



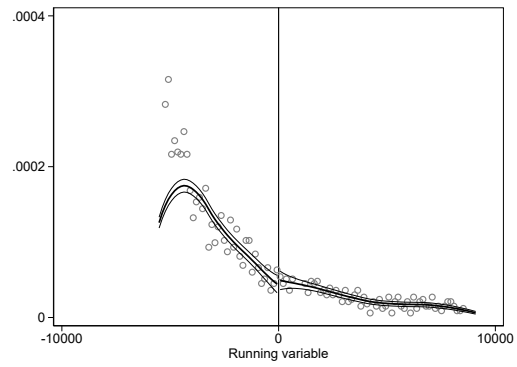
RD Density test
Including non-linkable districts

Notes as in Figure E2.

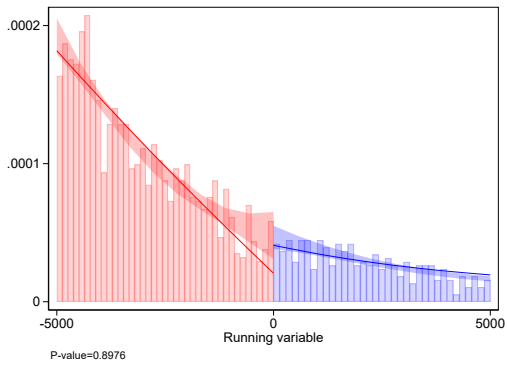
Figure E4: McCrary (2008) and Cattaneo et al. (2018) density tests - 2008 elections



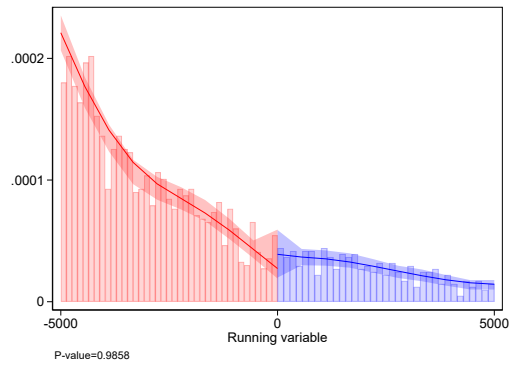
McCrary test - Main sample



McCrary test
Including non-linkable districts



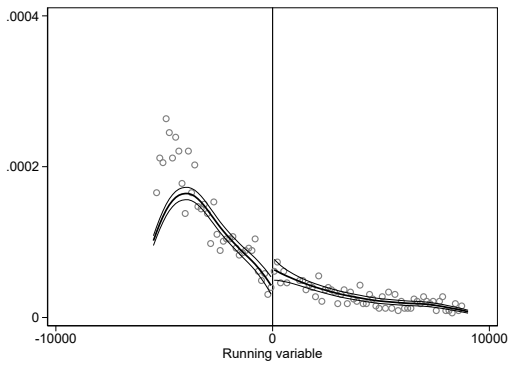
RD Density test - Main sample



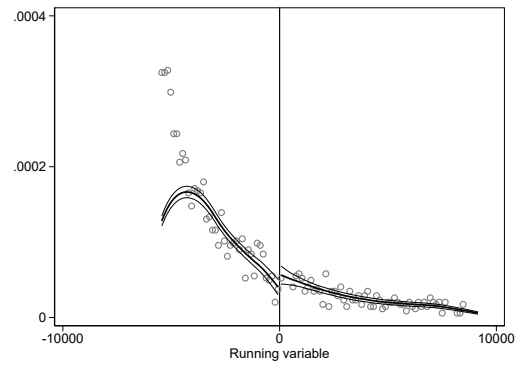
RD Density test
Including non-linkable districts

Notes as in Figure E2.

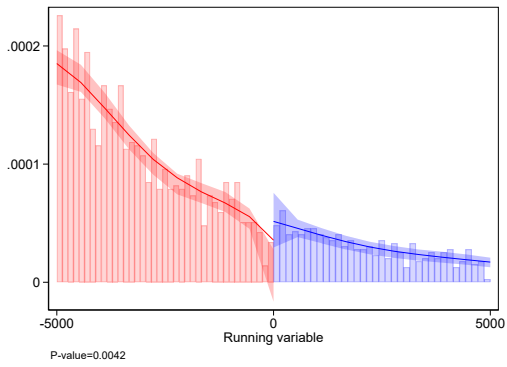
Figure E5: McCrary (2008) and Cattaneo et al. (2018) density tests - 2014 elections



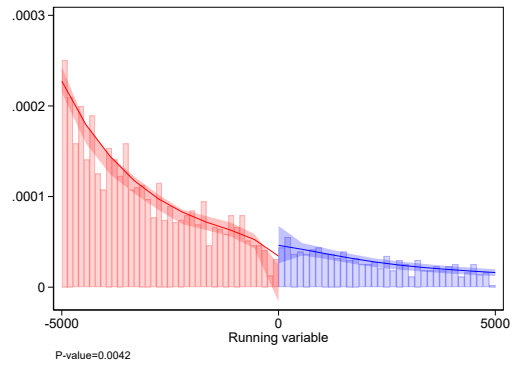
McCrary test - Main sample



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 tests

Table F1: Main results - Main sample of municipal elections - Excluding 2014

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger Win	Outsider	Victory	Winner margin In first round	Incumbent	Challenger Run
Treatment	-0.061 (0.059)	0.067* (0.036)	-0.006 (0.054)	-0.006 (0.077)	0.035 (0.037)	-0.077 (0.050)	-0.054 (0.062)
Robust p -value	0.381	0.069	0.760	0.805	0.447	0.161	0.412
Observations	1,090	939	883	751	834	1,499	1,086
Polyn. order	1	1	1	1	1	1	1
Bandwidth	2,221	2,046	1,890	1,658	1,809	2,949	2,315
Mean, left of threshold	0.594	0.039	0.358	0.622	0.215	0.741	0.282

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: Main results - Main sample of municipal elections - 2001

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger Win	Outsider	Victory	Winner margin In first round	Incumbent	Challenger Run
Treatment	-0.157 (0.114)	0.025 (0.048)	0.148 (0.096)	-0.078 (0.113)	-0.017 (0.046)	-0.104 (0.108)	-0.179 (0.117)
Robust p -value	0.278	0.586	0.206	0.469	0.624	0.465	0.109
Observations	401	379	573	362	489	360	296
Polyn. order	1	1	1	1	1	1	1
Bandwidth	1,908	1,849	2,571	1,741	2,219	1,732	1,484
Mean, left of threshold	0.697	0.020	0.290	0.716	0.234	0.761	0.317

Notes as in Table F1.

Table F3: Main results - Main sample of municipal elections - 2008

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger Win	Outsider	Victory	Winner margin In first round	Incumbent	Challenger Run
Treatment	0.005 (0.107)	0.099 (0.062)	-0.129 (0.091)	0.040 (0.103)	0.080 (0.048)	-0.050 (0.094)	0.001 (0.104)
Robust <i>p</i> -value	0.894	0.114	0.183	0.855	0.160	0.559	0.887
Observations	411	590	483	512	456	439	461
Polyn. order	1	1	1	1	1	1	1
Bandwidth	1,657	2,305	1,884	1,980	1,791	1,742	1,896
Mean, left of threshold	0.522	0.057	0.421	0.553	0.202	0.717	0.280

Notes as in Table F1.

Table F4: Main results - Main sample of municipal elections - 2014

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Incumbent	Challenger Win	Outsider	Victory	Winner margin In first round	Incumbent	Challenger Run
Treatment	0.049 (0.097)	-0.028 (0.063)	-0.046 (0.110)	0.011 (0.095)	0.007 (0.043)	0.073 (0.093)	0.091 (0.091)
Robust <i>p</i> -value	0.462	0.758	0.640	0.770	0.732	0.430	0.332
Observations	591	499	485	681	479	571	621
Polyn. order	1	1	1	1	1	1	1
Bandwidth	2,090	1,864	1,756	2,375	1,740	2,024	2,289
Mean, left of threshold	0.482	0.110	0.415	0.557	0.208	0.692	0.234

Notes as in Table F1.

Table F5: Impact on competition - All municipal elections including non-linkable districts

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Victory in the first round					Winner's margin in first round				
	All	Excl 2014	2001	2008	2014	All	Excl 2014	2001	2008	2014
Treatment	-0.038 (0.056)	-0.055 (0.074)	-0.122 (0.102)	-0.007 (0.105)	-0.000 (0.092)	0.014 (0.025)	0.028 (0.034)	-0.017 (0.042)	0.066 (0.048)	0.011 (0.043)
Robust <i>p</i> -value	0.453	0.380	0.212	0.815	0.846	0.600	0.542	0.598	0.258	0.672
Observations	1,390	808	442	497	728	1,535	850	528	455	463
Polyn. order	1	1	1	1	1	1	1	1	1	1
Bandwidth	1,767	1,641	1,815	1,891	2,469	1,931	1,714	2,133	1,752	1,641
Mean	0.604	0.613	0.695	0.553	0.566	0.214	0.211	0.227	0.203	0.207

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 campaigning 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 5.4). 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 districts 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 elections, 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 unnoticed, 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 Quêtelet) 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

<i>denominator of term (%)</i>	Check 1 <i>Districts above 9k</i>	Check 2 <i>Districts below 9k</i>	Check 3 <i>Districts above 9k</i>	Missing data <i>All districts</i>	# elections	# elections with pop \geq 9k <i>All districts</i>
Municipal elections						
1995	NA	NA	NA	46 2%	2,481	988 40%
2001	NA	NA	NA	4 0%	2,697	1,025 38%
2008	0 0%	0 0%	33 3%	5 0%	2,859	1,070 37%
2014	0 0%	0 0%	1 0%	0 0%	3,048	1,127 37%
Total municipal elections (%)	0 0%	0 0%	34 1%	55 0%	11,085	4,210 38%
Departmental elections						
1992	2 0%	0 0%	20 2%	44 2%	1,980	1,177 59%
1994	0 0%	0 0%	36 3%	42 2%	1,957	1,149 59%
1998	0 0%	0 0%	7 1%	45 2%	1,993	1,190 60%
2001	0 0%	0 0%	1 0%	0 0%	2,011	1,215 60%
2004	4 0%	0 0%	5 0%	0 0%	2,034	1,228 60%
2008	1 0%	0 0%	112 9%	0 0%	2,020	1,222 60%
2011	2 0%	0 0%	1 0%	0 0%	2,026	1,264 62%
Total departmental elections (%)	9 0%	0 0%	182 2%	131 1%	14,021	8,445 60%
All elections pooled (%)	9 0%	0 0%	216 2%	186 1%	25,106	12,655 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 elections, 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 which 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,⁴³ 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.⁴⁴ 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.
- 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 account balance is not negative.
- We checked that the total expenditures declared by the candidate *plus* the corrections made by the CNCCFP add up to the withheld total expenditures amount. We could only run this

⁴³In the 1998, 2001, and 2004 departmental elections, personal contributions are further broken down into own contributions, loans, and unpaid expenses.

⁴⁴The 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.

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 6.3.

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 means 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:

$$\begin{aligned}
\underbrace{E(W_1R_1 - W_0R_0|x = 0)}_{RD \text{ effect on } W} &= \underbrace{Prob(R_1 > R_0|x = 0)}_{RD \text{ effect on } R} \cdot \underbrace{E(W_1|x = 0, R_1 < R_0)}_{Unobservable} \\
&+ \underbrace{E[W_1 - W_0|x = 0, R_0 = 1]}_{\text{Effect on win cond on being always-taker or complier}} \cdot \underbrace{E(R_0|x = 0)}_{\lim_{x \uparrow 0} E[R|x]}
\end{aligned}$$

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:

$$\begin{aligned}
\underbrace{E[W_1 - W_0|x = 0, R_0 = 1]}_{\text{Effect on win cond on being always-taker or complier}} &= \underbrace{\frac{1}{E(R_0|x = 0)}}_{\lim_{x \uparrow 0} E[R|x]} \underbrace{[E(W_1R_1 - W_0R_0|x = 0)]}_{RD \text{ effect on } W} \\
&\quad - \underbrace{Prob(R_1 > R_0|x = 0)}_{RD \text{ effect on } R} \cdot \underbrace{E(W_1|x = 0, R_1 < R_0)}_{Unobservable}
\end{aligned}$$

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.

When we derive bounds on *challengers'* probability of winning conditional on running, using the same method, we rely on a different no defiers assumption. 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.