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. (JEL: D72, K16, P00, P16)

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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; Avis et al. 2022; Gulzar et al. 2022). However, we lack evidence on rules that 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% of the ceiling if they obtain more than 5% 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 3.0–7.6 percentage points and 10.5–18.9 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 5% 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 campaign expenses.

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% 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% 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. Fouirnaies (2021) and Avis et al. (2022) find that limits on overall spending tend to increase competitiveness and reduce incumbency advantage, and Gulzar et al. (2022) show that looser individual contribution limits increase the number of public contracts assigned to donors of the elected candidate. Existing evidence about the effects of campaign expenditures' reimbursement is much less solid.[2] Malhotra (2008), Masket and Miller (2015), and Kilborn and Vishwanath (2022) exploit the fact that some U.S. states offer public funding to candidates respecting preset spending limits to measure effects on electoral competitiveness, representativeness, and the legislative behavior of winners. However, candidates who choose public funding may differ from those funded privately on other

1. Section 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, Nannicini, and Tabellini 2016)

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

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, Nannicini, and Tabellini 2016; Eggers et al. 2018; Corbi, Papaioannou, and Surico 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, Dahan, and Klor 2015; Bekkouche, Cagé, and Dewitte 2022; François, Visser, and Wilner 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—challengers versus incumbents and candidates on the left versus 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, Folke, and Sørensen 2018, but see Ferreira and Gyourko 2009) and that electoral turnovers impact performance (Akhtari, Moreira, and Trucco 2022; Marx, Pons, and Rollet 2025).

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.

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 (5%, 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, we have $M_i^1 \neq M_i^2$, 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; Fouirnaies 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 $R_i^2 + p_i^2 B_i > q_i^2 \left(M_i^2 - S_i\right) + \left(1 - q_i^2\right) M_i^2$. 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: $q_i^2 \left(M_i^2 - S_i\right) + \left(1 - q_i^2\right) M_i^2 < M_i^1$. 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 5% 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 Federman 1989), hoping to limit the influence of money in politics and to increase the transparency and fairness of the election process (The Law Library of Congress 2009; Gunlicks 2019). France did not regulate campaign finance until the late 1980s, prompted by rising amounts of campaign money and numerous scandals uncovering the widespread illegal funding of parties. A series of reforms regulating campaign spending, campaign contributions, and other aspects of political campaigns were adopted from 1988 to 1995. France now has a stable and relatively strict system of campaign finance legislation.

For the sake of brevity and clarity, we focus on the aspects of the French regulations that are relevant to our analysis. Democracies can level the playing field by limiting campaign expenditures or by providing for their reimbursement by the state. France, similarly to 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% of the spending limit,[5] provided they obtain more than 5% 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% 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

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

4. This rule was modified in 2011 such that only candidates obtaining more than 1% of the votes have to submit this information.

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

6. Before 1995, candidates had been reimbursed for official propaganda-related costs, for example, 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 5% of the votes, both above and below the population threshold.

7. The spending limit is a stepwise 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.

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] Online Appendix Figure A1 provides a timeline showing the timing of the two laws and the election years used in the analysis.

The French reforms that started in the late 1980s also changed rules affecting other aspects of elections, including TV and radio advertising (which was prohibited) and contribution limits (Cagé, Le Pennec, and Mougin 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% of total public spending. France counts 101 départements divided into 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% of the candidate votes, accounting for at least 25% 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% of registered citizens until 2011, and 12.5% afterward. 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, 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% 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

8. Section 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.

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

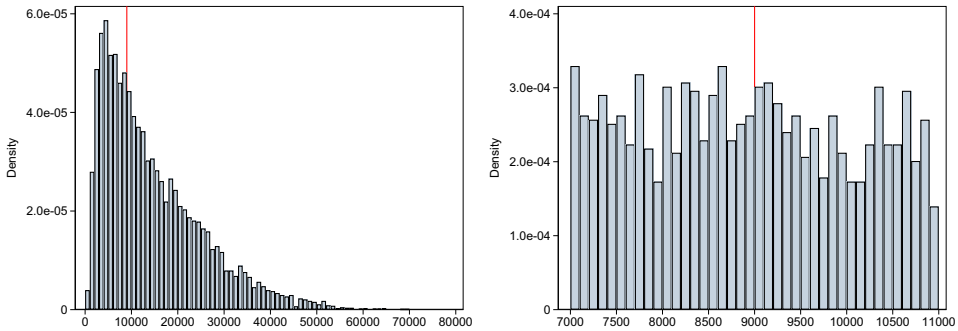


FIGURE 1. Population distributions of cantons. The vertical red line corresponds to the 9,000 inhabitants threshold. The left-hand side graph considers all districts, while the right-hand side graph focuses on districts close to the threshold, between 7,000 and 11,000 inhabitants.

the 9,000 inhabitants cutoff, and Section 4.4 further provides formal evidence of the absence of manipulation.

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 5% of the candidate votes in the first round.

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

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

where $Y_{i,t}$ is the outcome in district i and election year$t$, $X_{i,t}$ is the running variable, defined as the district population centered around 9,000 inhabitants, and $D_{i,t}$ is the assignment variable, a dummy taking value one 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, Cattaneo, and Titiunik (2014), we use a nonparametric 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, Cattaneo, and Titiunik (2014) to construct the bandwidths. The bandwidths differ across outcomes since they are selected based on the data. Applying Calonico, Cattaneo, and Titiunik (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.

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 "nonparty 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 "nonclassified." We classify candidates into five orientations—far-left, left, center, right, and far-right—and place them on ParlGov's 0–10 left-right scale (Döring and Manow 2012; Döring, Huber, and Manow 2022). Online 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%, 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

10. In Online 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.

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. Online 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 that followed the introduction of campaign expenditures' reimbursement in Section 6.3. See Online 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 Online 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 that 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 Online Appendix C.

11. We 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).

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

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 nonparty candidates	1.50	1.32	0	10	9,938
Number of nonclassified 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.

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 Online Appendix Table B1 (column 1). In Online 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 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, 10,000 voters are registered to vote, 63.6% of them vote, and 60.8% cast a valid vote for one of the candidates.

13. We consider elections as problematic if a second round took place even though a candidate obtained a majority of votes and 25% 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.

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

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

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, Todd, and Van der Klaauw 2001). We are confident that this assumption is satisfied.

First, no other voting rule or institutional feature changes at this threshold. In fact, in departmental 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 Online Appendix B (for the validity tests) and Online 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 (Online 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, Online 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, Jansson, and Ma 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

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

17. Eggers et al. (2018) provide a list of other policies (affecting, e.g., 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.

18. The p-value of the manipulation test described in Cattaneo, Jansson, and Ma (2018) is equal to 0.99, and adding non-linkable districts in the sample yields a p-value of 1.00.

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 (Online Appendix Table B2 and Figure B2). Fourth, we also show balance tests on each of these sociodemographic variables taken individually (Online Appendix Table B4 and Figure B3).[19] Fifth, we check that outcomes defined at election $t - 1$ do not jump at the threshold either (Online 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 RDD at ten false population thresholds below and above the true 9,000 inhabitants cutoff (Online Appendix Tables C9–C11).[20] Second, we check the robustness of our results by employing a quadratic specification and by controlling for all the sociodemographic variables used in the general balance test (Online 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% robust confidence intervals for bandwidths ranging from plus to minus 1,000 inhabitants around the data-driven bandwidth selected based on Calonico, Cattaneo, and Titiunik (2014), using either a linear or a quadratic specification (Online Appendix Figures C1–C3). We also replicate our analyses using a small bandwidth of 1,000 inhabitants for all our main outcomes (Online Appendix Table C14). Fourth, Appendix Figures C4–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 by 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 online (Online 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 (Online Appendix Table C19). Overall, the point estimates and their significance remain very similar.

19. Only one out of 13 variables is statistically significant (at the 5% 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 (Online Appendix Tables B3 and B5).

20. The number of significant results is not higher than would be expected: Eight out of 70 point estimates are significant at the 10% level, among which four are significant at the 5% level, and one at the 1% level.

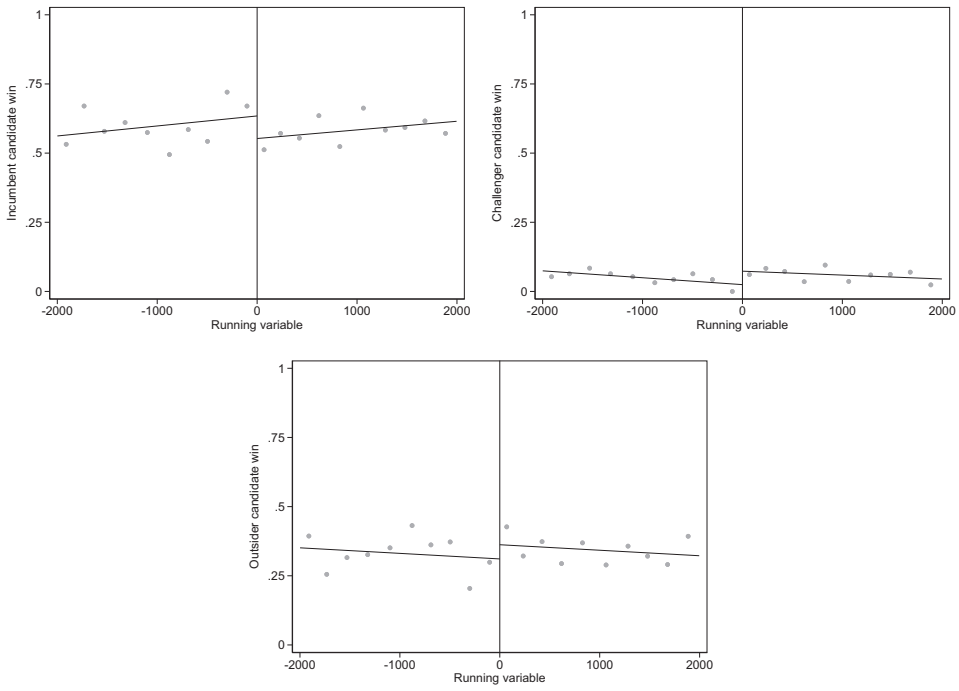


FIGURE 2. Impact on winner identity. Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the district population centered around 9,000 inhabitants) is split into quantile-spaced bins. The continuous lines represent a linear fit. To facilitate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

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% level. The probability of the incumbent winning declines by 14.5 percentage points (21.2% 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

TABLE 2. Impact on winner identity.

	(1)	(2)	(3)
Outcome	Incumbent win	Challenger win	Outsider win
Treatment	−0.145***	0.052**	0.093**
	(0.046)	(0.020)	(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%, 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.

(a nearly three-fold increase) and 9.3 percentage points (32.4%), respectively. In 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.

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.

21. Online Appendix 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% level, respectively.

TABLE 3. Impact on competition.

	(1)	(2)	(3)
Outcome	ENC, R1	Victory, R1	Winner vote margin, R1
Treatment	0.086	−0.109**	−0.028*
	(0.089)	(0.044)	(0.016)
Robust *p*-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.

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): $ENC = 1/\sum_1^n v_i^2$, 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% and 14.7%), which is significant at the 5% and 10% 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.

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

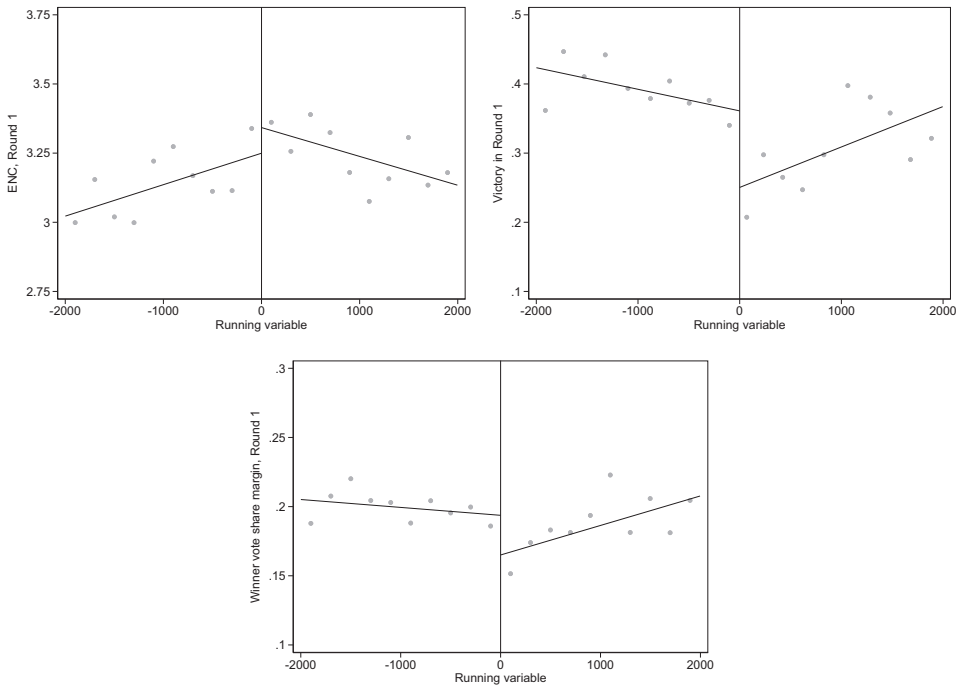


FIGURE 3. Impact on competition. Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the district population centered around 9,000 inhabitants) is split into evenly spaced bins for continuous outcomes and into quantile-spaced bins for binary outcomes. The continuous lines represent a linear fit. To facilitate visualization, the graph is truncated at 2,000 inhabitants around the cutoff.

To circumvent this difficulty, we follow Anagol and Fujiwara (2016) and Granzier, Pons, and Tricaud (2023), who adapt Lee (2009)'s method to derive bounds in an RDD context. We present the method intuitively here, and Online 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 have won in districts subject to the campaign finance rules had they run. This amounts to assuming that the unconditional effect on winning found

TABLE 4. Impact on winning and vote shares, conditional on running.

	(1)	(2)	(3)	(4)
	Incumbent		Challenger	
Outcome	Win	Vote share, R1	Win	Vote share, R1
Panel A. Unconditional effects				
Treatment	−0.145***	−0.058***	0.052***	0.034***
	(0.046)	(0.021)	(0.020)	(0.012)
Robust *p*-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
Panel B. Conditional effects				
Upper bound	−0.189**	−0.076**	0.198**	0.130***
Boot. std. error	(0.093)	(0.033)	(0.080)	(0.042)
Lower bound	−0.105	−0.030	0.110	0.034
Boot. std. error	(0.075)	(0.020)	(0.069)	(0.021)
Mean	0.871	0.473	0.139	0.254

Notes: Panel A and Panel B show effects on unconditional outcomes and bounds of effects conditional on running, respectively. The notes for Panel A are as in Table 2. In Panel B, the mean, left of the threshold, indicates the value of the outcome for the candidates on the left of the threshold, conditional on running. ***, **, and * indicate significance at 1%, 5%, and 10%, respectively, of the bootstrapped standard errors.

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%.

We use the same method to derive bounds on vote shares conditional on running and on challengers' 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 3.0–7.6 percentage

23. We 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).

TABLE 5. Impact on entry.

Outcome	(1) Incumbent run	(2) Challenger run	(3) Number of Outsiders	(4) Number of Candidates	(5) Turnout R1
Treatment	−0.074**	0.084**	0.014	0.046	0.010
	(0.032)	(0.038)	(0.120)	(0.119)	(0.009)
Robust *p*-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.

points (6.3%–16.1%) and their likelihood of reelection by 10.5–18.9 percentage points (12.1%–21.7%). By contrast, challengers' vote share and likelihood of winning increase by 3.3–13.0 percentage points (13.0%–51.2%) and 11.0–19.8 percentage points (79.1%–142.4%), respectively, conditional on running. The upper bounds of these effects are statistically significant, but the lower bounds are not.[24]

5.3. Effects on Entry

We now investigate whether the campaign finance rules also affect candidates' decisions 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%).[25]

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%, 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

24. These results are robust to excluding the 2008 elections: as shown in Online 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% level for winning, and at the 10% level for vote shares.

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

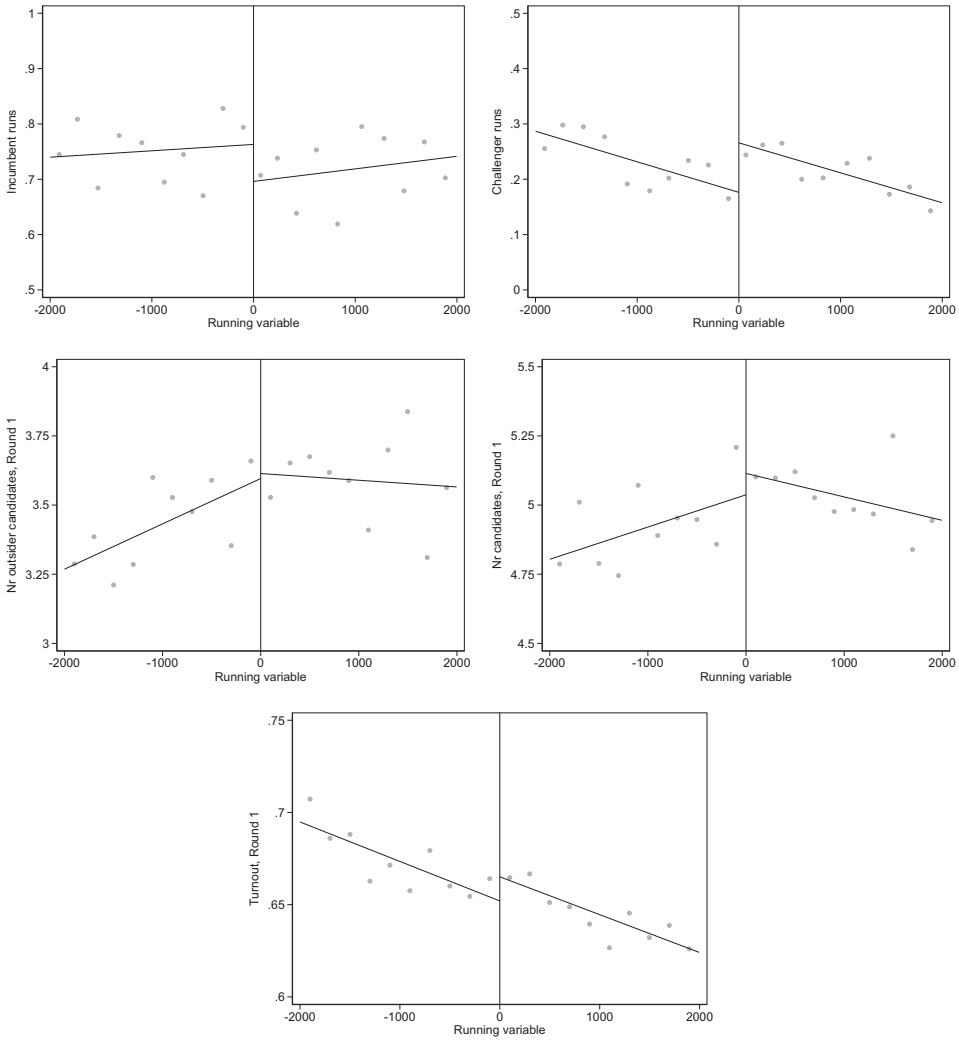


FIGURE 4. Impact on entry. Notes as in Figure 3.

candidates may not be certain to reach the 5% 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]

26. The estimates are similar when we exclude the 2008 elections (Online Appendix Table C5). While the impact on the probability that the incumbent runs is no longer significant (p-value of 0.103), it

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 majority (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, Huber, and Landa 2007; Pons and Tricaud 2018). It may also improve performance by candidates from nonmainstream platforms and increase polarization. On the other hand, if some types of candidates had 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%), which is significant at the 10% 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]

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% 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 (Online Appendix Table C6).

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

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*	−0.053	−0.082	−0.002	−0.037
	(0.047)	(0.041)	(0.083)	(0.014)	(0.029)
Robust *p*-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% 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.

One possible interpretation of these results, for which we provide evidence in Section 6.3, is that left-wing candidates have 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% of departmental races for which each candidate can be matched to a ParlGov position on the [0–10] left-right scale, we follow Dalton (2008) and build the following measure of polarization: $\sqrt{\sum v_i \left((p_i - \bar{p})/0.5\right)^2}$, 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 Online 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

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

29. For 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).

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 Online 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 Online Appendix K) and use the residuals as proxy for the winner's quality. We do not find any effect on these outcomes either (columns 2–5).[30]

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.

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 5% of the votes in the first round. If public reimbursement helps candidates with less resources to be more competitive, we would expect candidates

30. All the specifications in Online 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 (Online 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 $t + 1$ (Online Appendix Table A4).

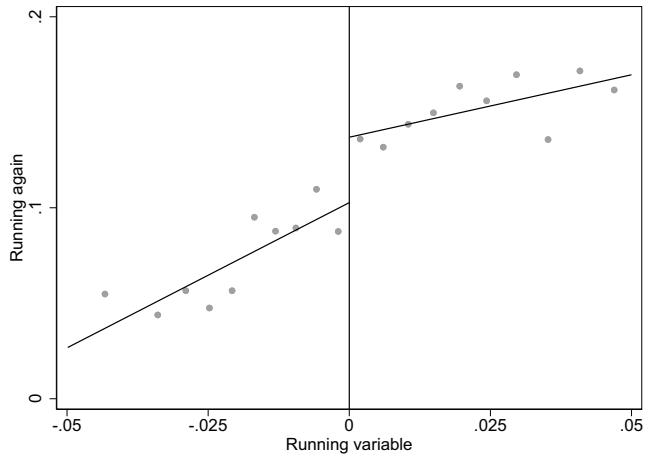


FIGURE 5. Effect of being reimbursed in election t on running in election $t+1$. Each dot is the average of the outcome variable within a given bin of the running variable. The running variable (the vote share centered around 5%) is split into quantile-spaced bins. The continuous lines represent a linear fit. To facilitate visualization, the graph is truncated at 5% 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 5% of the votes. The level of analysis is the candidate and the sample only includes districts above 9,000 inhabitants and that can be linked with election $t+1$.

who obtained more than 5% 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 5% threshold, using the following specification:

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

where $Y_{j,t+1}$ is a dummy equal to 1 if candidate j, present at election t, runs again in election $t+1$, $X_{j,t}$ is the running variable, defined as the candidate's vote share at t centered around 5%, 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 nonparametric estimation, apply Calonico, Cattaneo, and Titiunik (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 5% 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

31. Using 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.

TABLE 7. Impact of being reimbursed in election t on running in election $t + 1$.

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

Notes: Clustered standard errors are in parentheses. Robust p-values are used to compute statistical significance. ***, **, and * indicate significance at 1, 5, and 10%, 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 5% 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 that can be linked with election $t + 1$.

(48.8% 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 Online 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.

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 campaign expenses, 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 Online 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% 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. Online 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 are 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, Nannicini, and Troiano 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 Online 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.

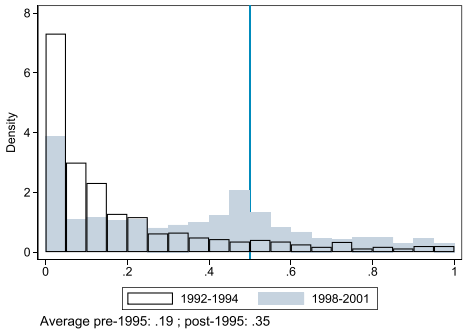
6.3. Changes in Candidate Spending and Contribution Patterns over Time

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 above the threshold.

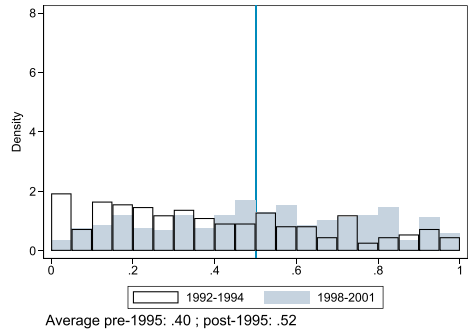
Figure 6 and Online Appendix Figures A2 and A3 plot the distribution of spending-to-ceiling ratios as well as personal contributions and donations-to-ceiling ratios for all candidates (upper left graph), separately for incumbents, challengers, and outsiders (upper right graph and middle graphs), and 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 the 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 (Online Appendix Figure A2),

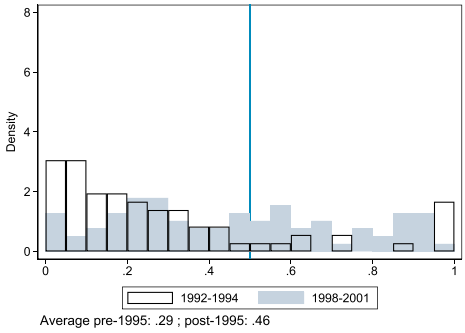
32. To define the analysis sample, we either consider the optimal bandwidth used in the main analysis, or we take the average of the optimal bandwidth used in the main analysis and the one used for the 1992–1994 analysis.



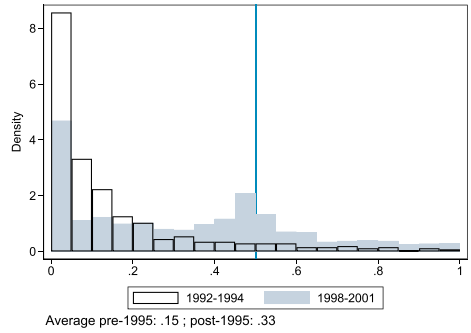
All candidates



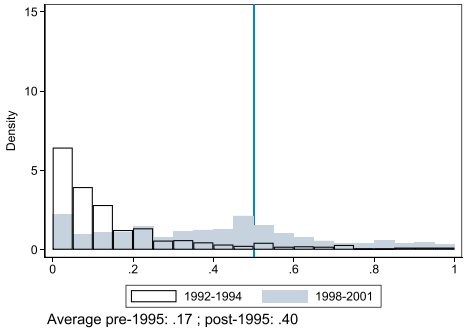
Incumbent candidates



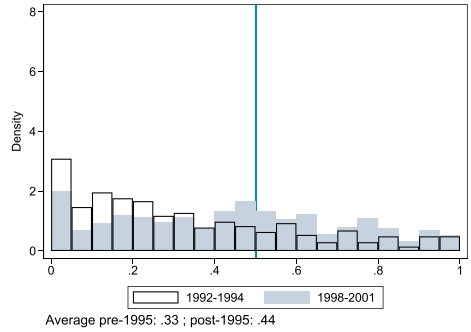
Challenger candidates



Outsider candidates



Left-wing candidates



Right-wing candidates

FIGURE 6. Expenditures-to-ceiling ratios. 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% of candidates with at least one inconsistency in their contribution and expenditure data (see Online Appendix I).

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% of the ceiling post 1995. This pattern underlines the role played by reimbursement even more directly, since 50% of the ceiling is the maximum amount of expenditures that candidates can submit for reimbursement (conditional on obtaining more than 5% 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% mark is not relevant for other sources of campaign money. Candidates who contribute 50% of the ceiling with their own money but also receive private donations or party contributions will appear at the 50% threshold in the graph plotting personal contributions but above that mark in the graph plotting total spending.

Contrasting with the bunching at 50% of the ceiling, we observe limited bunching of overall spending at 100%, 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% 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% for challengers, whereas it increased by 30% for incumbents.[33] Similarly, the shifts are larger for left-wing candidates than right-wing candidates. The former experienced an average increase in spending of 135% against 33% for the latter. Overall, the reimbursement introduced by the 1995 reform disproportionally 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 Online 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 that resulted from the 1995 reform, and from the introduction of personal expenditures' reimbursement more specifically.

33. The shifts in spending and personal contributions as well as the bunching at 50% 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% of outsiders relied exclusively on personal expenditures, against 33% of challengers.

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 (Online Appendix Figure A4). And yet, effects in this subsample, shown in Online Appendix Table A10, are similar to those 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 5% 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% 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% of départements. As expected, and as shown in Online 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. Online 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

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

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

the least binding, are similar to those in the full sample and, if anything, slightly larger.

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, for example, 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% 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 (Online Appendix Table A12).[36]

Online Appendix Tables A13–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.

36. Online Appendix 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.

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 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). Online Appendix Figure D1 shows the population distribution of municipalities in our sample and Online 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.

37. Municipal councils have discretion over local urban services, municipal police, nurseries, primary schools, sports facilities, road maintenance, and urban public transportation. Their expenditures account for 11% of total public spending.

38. If a list obtains the absolute majority in the first round, half of the seats are attributed to this list, and the other seats are divided proportionally between all the lists that received more than 5% of the votes. If no majority is reached in the first round, the top-two lists and all lists above 10% qualify for the second round taking place a week later. Lists with more than 5% 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 that received more than 5% of the votes in the second round.

39. Electoral 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.

40. We 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.

All validity and robustness tests are shown in Online 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, Papaioannou, and Surico (2019), we check the robustness of our results by 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 (Online Appendix Tables F1 through F4), with only one significant estimate out of 28 (at the 10% level).

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 Online Appendix Table D2, in districts just above the threshold, donations account for 13.1% of the spending ceiling in municipal elections, against 4.3% 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.

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 Online 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% 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.

41. We 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.

TABLE 8. Impact in municipal elections.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome	Incumbent	Challenger Win	Outsider	Victory	Winner margin R1	Incumbent Run	Challenger
Treatment	−0.030	0.038	−0.022	−0.009	0.018	−0.022	0.001
	(0.054)	(0.033)	(0.055)	(0.059)	(0.026)	(0.049)	(0.054)
Robust p-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 includes the 2001, 2008, and 2014 municipal elections. Other notes as in Table 2.

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 Online 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 Online 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% level, and represents a 43% or 42% decrease compared to 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 an RDD.

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

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 Fouirnaies (2021), where limits have been tightened over time, or the local Brazilian elections studied by Avis et al. (2022), where ceilings are set based on the maximum spending in the previous race.

Second, 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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Supplementary Data

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