Moderating Political Extremism:

Single Round vs Runoff Elections under Plurality Rule*

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1 Introduction

In some electoral systems, citizens vote twice: in a first round they select a subset of candidates, over which they cast a final vote in a second round. The system for electing the French President, where the two candidates who get more votes in the first round are admitted to the second, is possibly the best known example. But variants of this *runoff* system are increasingly used in many other countries, for example in most presidential elections, in US gubernatorial primary elections, and in many local elections, including Italian municipal elections (Cox 1997; Bormann and Golder 2013). How does the runoff system differ from the more common *single round* plurality rule, where candidates are directly elected at the first round? In spite of its obvious relevance, this question remains largely unaddressed, particularly when it comes to studying the economic policies enacted under these two systems.

This paper contrasts runoff vs single round elections under plurality rule. Our theoretical prior is based on a simple intuition. If the electorate is polarized and moderate parties are large, the runoff system allows moderate candidates to pursue their own platform without being forced to merge and compromise with the neighboring extreme candidates. Intuitively, with single round and under sincere voting, the extremes can threaten to cause the electoral

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defeat of the nearby moderate candidate if he refuses to strike an alliance. Under runoff this threat is empty, provided that enough extremist voters are willing to vote for the closest moderate, rather than abstain, in the second round. Thus, runoff elections are predicted to have more candidates, but also to reduce the influence of (smaller) extremist parties compared to single round elections. This policy moderation effect should reduce policy volatility under runoff elections, conditional on the same degree of political turnover.

In this paper, we test both predictions with data on municipal elections in Italy. Since 1993, Italian mayors are directly elected and have a prominent role in determining policy. Municipalities below 15,000 inhabitants adopt a single round system, while a runoff system is in place above this threshold. This allows us to compare outcomes under the two electoral rules with a quasi-experimental strategy, that is, implementing a Regression Discontinuity Design (RDD) around the 15,000 threshold.

We obtain two main results. First, the number of candidates for mayor is larger under the runoff system, compared to the single round. The positive discontinuity at 15,000 is large and statistically significant: under runoff elections the number of candidates for mayor increases by about 29%. Second, policy volatility is lower under runoff elections, suggesting that this electoral system indeed moderates political extremism, at least in the Italian context.

To measure policy volatility at the municipal level, we focus on one of the main policy tools of municipalities, the business property tax. In 1993, with the introduction of this tax, Italian municipalities were given large discretion in setting the tax rate, whose proceeds could be freely allocated to all municipal functions. We then test whether the volatility of the tax rate is different under the two electoral systems. The intuition for this test is simple. Local taxation is influenced by partisan identity, with left-wing local governments generally imposing higher business property taxes; this is indeed confirmed by our data. Hence, on average, a change in the identity of the mayor should lead to a sharper policy change where the influence of the extremist parties is stronger, namely under single round elections. The RDD evidence supports this prediction. We measure the volatility of the business property tax rate in two ways: by the intertemporal variance (i.e., across legislative terms for the same municipality) and by the cross-sectional variance (i.e., within population bins in the same year). Both indicators display a discontinuity at 15,000, with less volatility above the threshold, which is both large and statistically significant. The estimated coefficients point to an impact of about 61% of runoff elections on the time series volatility of the tax rate, and to an impact of about 71% on the cross-sectional volatility around the population threshold.

Alternative explanations for this (reduced-form) effect of the electoral system on tax volatility are rejected by our data, because the turnover between different mayors is similar in both runoff and single round elections. Moreover, in a small and selected subsample

of municipalities where we can measure the political identity of mayoral candidates, runoff elections have a negative impact on the probability that the leftist political extreme—i.e., the Communist Party—joins the main center-left coalition at the local level, in line with another prediction of our model. Overall, the empirical evidence supports the hypothesis that the runoff system reduces the influence of the political extremes and induces policy moderation.

Our results have important implications for the design of democratic institutions. Political extremism is widespread in many advanced and developing countries (including Italy) and is often counterproductive. It reduces ex-ante welfare if voters are risk averse, and it induces sharp disagreement that often disrupts decision making in governments or legislatures. In this respect, runoff electoral systems might have an advantage over single round systems, as they moderate the influence of extremist groups and reduce the welfare costs associated with (partisan) policy volatility.

The next section discusses the relevant literature and relates it to our theoretical priors. Section 3 presents the evidence.

2 Related literature and theoretical mechanism

The idea that runoff elections produce more than two effective candidates is not new. Since Riker (1982), this is indeed known as Duverger's Hypothesis, in contrast with Duverger's Law that instead predicts that plurality rule yields a two party system (Duverger,1954). Cox (1997) derives this result under strategic voting, while Osborne and Slivinsky (1996) study a citizen-candidate model with sincere voting, and show that the set of parameters supporting a two-candidate equilibrium under runoff is a proper subset of that under simple plurality rule. The empirical evidence has always been weaker, however. For instance, Wright and Riker (1989) and Golder (2006) find support for Duverger's hypothesis, while Engstrom and Engstrom (2008) and Shugart and Taagepera (1994) do not. More recent work on Brazil, that also exploits differences in electoral rules across municipalities for RDD identification, obtains contrasting results: Chamon et al. (2009) find that the runoff rule has more effective candidates, while Fujiwara (2011) finds that the effect of the electoral rule on the number of candidates is not statistically different from zero in most specifications.[1]

This contrasting evidence has motivated a small theoretical literature trying to explain the different results. Callender (2005) makes the point that initial conditions matter. He shows that, even under runoff, the two incumbents may prevent the entrance of a third challenger by strategically positioning themselves on the policy line. Bouton (2013) notices that in a runoff

[1]Fujiwara finds support for Duverger's argument of strategic voting under single round elections, however. In Section 4 we replicate his test with Italian data.

the voters in the two rounds may differ, and he shows that with strategic voting even runoff may generate equilibria with two effective candidates. For instance, shocks to turnout may allow a less preferred candidate to be elected in the second round. To avoid this risk, strategic voters may prefer to concentrate their votes on the two main runners in the first round. In an extension, Bouton and Gratton (2015) refine the equilibria, and show that a two candidate equilibrium always exists under runoff, while the conditions for existence of an equilibrium with three effective candidates are more restrictive. Solow (2015) considers sincere voting and aggregate uncertainty, and finds results more in line with Duverger's hypothesis. In his model, unlike in Osborne and Slivinsky (1996), a runoff equilibrium has candidates running as sure losers, in order to prevent the victory of undesired candidates in the first round.

The second prediction—more policy moderation under runoff—is more unexplored theoretically and to the best of our knowledge it is still untested. Osborne and Slivinsky (1996) predict that the dispersion in the policy position of the two candidates is smaller under runoff compared to single round, or else a centrist candidate could enter, not obtaining plurality in the first round, but winning the final round. The most recent literature, although not directly addressing the issue of policy moderation, is less supportive of this intuition. Bouton (2013) shows that sincere voting equilibria—where all strategic voters vote for their favorite candidate at the first round—may fail to exist under runoff, and as a consequence a Condorcet's winner might not make to the second round. Similarly, in Solow (2015)'s model, a first round Condorcet's loser might reach the second ballot and obtain office.

Our empirical approach is strongly motivated by the theoretical results of Bordignon, Nannicini, and Tabellini (2015), henceforth BNT, who study a model more closely geared to the Italian context. In contrast with the above literature, BNT focus on bargaining between pre-existing candidates, and predict both more candidates and more equilibrium policy moderation under runoff. To illustrate the main argument, consider the following example. Suppose that policy is one-dimensional and is the choice of a scalar in the interval [0,1]. There are four homogeneous groups of voters who vote sincerely: two extremist groups whose policy bliss points are located at 0 and 1, respectively; and two moderate groups with bliss points located on both sides of 1/2, closer to the extremes than to the mid point 1/2. The electorate is symmetric, with a roughly equal number of voters on both sides of 1/2, and the moderates are larger than the extremists. Each group is represented by a political candidate who has the same policy preferences of his voters but also cares about the rents from winning office. Policy is set by whoever wins the election. Before the election, however, candidates are allowed to form political alliances (or merge into parties). Suppose that mergers are allowed only between each moderate candidate and his extremist neighbor (since the moderates are closer to the extremists than to each other, a centrist party resulting from the merger of the

two moderates would not be viable). Two merging candidates have to agree to a common policy platform to be announced ahead of the election, and to an allocation of rents between themselves. No restriction is placed on the bargaining outcome, except that the policy has to lie (weakly) within the two merging candidates bliss points. This ensures that the policy is credible in the eyes of the voters, because it is in the ex-post Pareto set of the merging candidates. Bargaining is conducted under the threat that, if no agreement is reached, then no merger takes place.

Now contrast two electoral systems. Under single round elections, the moderate candidates are always forced to merge with their extremist neighbor on a compromise electoral platform. This is because any candidate running alone (say, the candidate on the left) cannot gain plurality against a moderate-extremist right-wing coalition. The reason is that, under sincere voting, the right wing coalition gets the support of all voters to the right, while the voters to the left split their vote between two candidates. Hence a merger between moderates and extremists is a dominant strategy for both candidates, irrespective of whether the opponents on the other side of 1/2 are expected to merge or not. Note that this happens even though there are more moderate than extremist voters. The extremist candidates have no chances of winning on their own, yet they are an essential player in the coalition because their absence would determine the electoral defeat of the nearby moderate. This gives them some bargaining power, and forces each moderate candidate to accommodate somewhat the extremists' demands, in terms of both policy platform and rent allocation. This result is a version of Duverger's law, but in this context it has the additional implication that the policy platforms presented to the voters also reflect the preferences of extremist candidates and voters.

Under runoff elections, instead, the larger parties (here the moderates) can afford to run alone on a platform that coincides with their bliss point. Since there are more moderate than extremist voters, a moderate candidate running alone always makes it to the second round, irrespective of whether the other moderate candidate has merged with the extremist or not. Furthermore, at the final ballot, a moderate running alone would attract all the closest extremist voters. Anticipating this outcome, both moderates prefer to run alone. Thus, runoff elections favor the moderate parties (and voters) and reduce the influence of the extremes. They also lead to a larger number of candidates.

This result hinges on two central assumptions. First, that the electorate is polarized, in the sense that moderate voters are closer to the extremists than to each other. If this was not the case, the two moderate candidates would merge into a large centrist party, and the two electoral systems would yield similar outcomes. Second, the moderate groups are larger than the extremists. If not, the result could be reversed and runoff elections could

exacerbate political extremism.[2] As discussed below, the Italian political system satisfies these two conditions.

The example discussed above is highly stylized, and relies on many simplifying assumptions. BNT show that none of them is crucial for the main conclusions, however. For instance, the example above assumes that, in the second round of the runoff elections, extremist voters prefer to vote for the closer moderate, rather than abstain. If this was not the case, and all extremist voters were so "attached" to their own candidate that they do not participate in the second round election when the extremist is excluded, then the two electoral systems would be equivalent, since extreme candidates would remain essential players even under runoff. As long as a relevant fraction of extremist voters is not attached in the sense described above, however, the moderating effect of runoff elections remains valid, although it is attenuated by the presence of some attached voters. Again the evidence on Italian elections discussed below is consistent with this assumption, since a relevant share of the voters supporting the excluded candidate in the first round seem to participate again in the second round.[3]

In the Italian elections for mayor, like in many other runoff systems, the excluded candidates are allowed to endorse one of the candidates admitted to the second round. The consequence of such endorsements is to mobilize the attached excluded voters that otherwise might prefer to abstain rather than to vote for a second best candidate. How does this change the results presented above? Suppose that the policy platforms are not negotiable in between the two rounds, but that endorsements can be rewarded by allocating some rents to the endorsing candidates (this assumption is consistent with the details of the Italian electoral system).[4] Then the possibility of endorsements strengthens the moderating effect of the runoff, and increases the bargaining power of the moderates under this electoral rule. The reason is that it is ex-post optimal for the excluded extremists to endorse one of the moderates, both because this increases the chances of having a better policy outcome, and because they gain access to some rents if the endorsed moderate wins. Anticipating this outcome, the moderates can afford not to negotiate with the excluded extremists before the first round elections, even if the extremist voters are attached. In other words, the possibility of endorsements reduces the impact of having many attached voters.

[2]Note that the two smaller moderate candidates might have an incentive to merge into a centrist party, however. BNT show that this incentive is stronger under runoff than single round elections. Hence, even with larger extremist groups, there may be instances in which the runoff favors policy moderation.

[3]Another feature of the Italian runoff system, shared by most other systems, is that a candidate winning more than 50% of the votes in the first round is elected without going through a second round. BNT show that this attenuates but does not nullify the moderating effect of runoff elections.

[4]In Italian municipal elections, endorsement after the first round of voting is highly regulated by the law. The endorsing party has the right to share the winner's majoritarian prize in the city council, but he has to sign the program presented by the endorsed candidate before the first round.

3 Evidence from Italian municipal elections

Under some conditions that seem to be satisfied in the Italian context (see section 3.4 below), the theoretical arguments above deliver three testable predictions: (1) runoff elections increase the number of political candidates compared to single round elections; (2) extremist parties are less likely to merge with moderate parties under runoff; (3) policy moderation is higher—and policy volatility lower—under runoff. In this section, we use an RDD identification strategy to test predictions (1) and (3), and we provide suggestive evidence on prediction (2). We exploit a reform in municipal elections in Italy, which introduced single round vs runoff elections for municipalities of different population size.

3.1 Electoral rules for Italian municipalities

Until 1993, municipal governments in Italy were ruled by a pure parliamentary system. Citizens voted for party lists under proportional representation to elect the legislative body (i.e., the city council); the council then appointed the mayor and the executive office. Since 1993, instead, the mayor has been directly elected under plurality rule, with a single round for municipalities below 15,000 inhabitants, and with a runoff system above (see Law 81/1993).

Specifically, below this population threshold, each party (or coalition) presents one candidate for mayor and a list of candidates for the city council. Voters cast a single vote for the mayor and his supporting list (they can also express preference votes over the candidates for councilor within the same list). The mayoral candidate who gets more votes becomes mayor and his list gains 2/3 of all seats in the council. The remaining 1/3 of the seats are divided among the losing lists in proportion of their vote shares.[5]

Above the 15,000 threshold, parties (or coalitions) present lists of candidates for the council, and declare their support to a specific candidate for mayor. Each mayoral candidate can be supported by more than one list. There are two rounds of voting. At the first round, voters cast two votes, one for a mayoral candidate and one for a party list, and the two votes may be disjoint (i.e., voters are allowed to vote for, say, mayor A and a list supporting mayor B), although both choices are expressed in the same ballot. Most voters, however, leave just one mark on the ballot, expressing a single choice either for a party list (in which case, the preference is also extended to the mayoral candidate supported by that list) or for a mayoral candidate (in which case, the preference is expressed for the candidate only).[6] If a candidate for mayor gets more than 50% of the votes in the first round, he is elected. Otherwise, the

[5]There is a minimum level that a list must obtain in order to gain seats, equal to 4% of the votes.

[6]The share of single votes for a party list cannot be assessed, as in this case the vote is automatically attributed also to the mayoral candidate by the counting agents. The share of single votes for a mayoral candidate, instead, ranges from 2% to 15% with an average value of 7%.

two best candidates run against each other in a second round, taking place two weeks after the first. In this second round, the vote is only over the mayor, not the party lists. In between the two rounds, lists supporting the excluded mayoral candidates are allowed to endorse one of the remaining two candidates, if the endorsed candidate agrees[7]. Like in the single round system, the rules for the allocation of council seats entail the same majority premium for the lists supporting the winning candidate for mayor.

As discussed in Section 6.3, our identification strategy is valid only if there are no other (non-electoral) policies or institutions that vary at or around the threshold of 15,000 inhabitants. The closest policy thresholds based on population size are at 10,000 (where the mayor's wage, the size of the council, and the size of the executive office sharply increase) and at 30,000 inhabitants (where the mayor's wage and the size of the council sharply increase). Both thresholds are outside of our sample (see below).[8]

The 15,000 threshold entails a change in the electoral system for electing both the mayor and the city council. Thus, strictly speaking, our test concerns the consequences of both changes. Nevertheless, there are many reasons to believe that the only relevant difference is the method for electing the mayor. One of the main features and effects of the 1993 reform was the strengthening of the political power of mayors, both formally and effectively. Since 1993, Italian mayors can appoint and dismiss the executive officers at will; they also have the prerogative of appointing the city manager and shaping all municipal policies (see Law 81/1993). It is true that, if the city council approves a vote of no confidence, then the mayor is forced to step down. But this is a very rare event in Italian local politics. As a matter of fact, within the universe of mayoral elections from 1993 to 2007, only in 1.11% the mayor was removed because the council approved a vote of no confidence, and only in 1.69% because the council resigned (therefore ending the term under both circumstances). Naturally this is equilibrium behavior, but it is important to stress that, whenever the mayor steps down, the legislature automatically comes to an abrupt end and new elections for both the mayor and the council are held. The direct election also gives the mayor sufficient leverage to sidestep a tiring bargaining with political parties over every single issue; since 1993 the mayor is indeed the crucial player of municipal politics in Italy (see Di Virgilio, 2005).

Finally, the electoral rules for the council below and above the 15,000 threshold are not very different: in both cases, the system is proportional with open lists and majority premium. The only difference is that below the 15,000 threshold, but not above, the mayor is constrained to receive the support of only one list (what we call "alliance restriction" henceforth), and voters do not have the option of casting a separate vote for a mayoral candidate and a list

[7]BNT show that the possibility of endorsement between rounds increases the moderating effect of runoff.

[8]For a summary of Italian institutions varying with population and of their effects, see Gagliarducci and Nannicini (2013) and Grembi, Nannicini, and Troiano (2016).

supporting another candidate. Note, however, that there are no different constraints on the number of mayoral candidates. In Section 6.4, we present empirical tests supporting the claim that the above institutional variations have second-order effects with respect to the change from single round to runoff elections for mayors.

3.2 Data sources and variables

Because we do not want our estimates to be affected by observations far away from the 15,000 population threshold, and to avoid overlap with other policies, we restrict the sample to Italian municipalities between 10,000 and 20,000 inhabitants (about 10% of all Italian municipalities), for elections that took place after the 1993 reform. The complete sample is made up of 2,027 mayoral terms, referred to 661 towns. Both below and above the 15,000 threshold, mayoral terms lasted for four years from 1993 to 2000, and five years both before and afterward. As explained below, in some regressions we also consider the years preceding the reform (from 1985 onwards) to implement falsification exercises.

The data refer to three kinds of variables. First, we have data on population (both from the 1991 and the 2001 Census) and other general features of the municipality, such as per capita income, geographic location, and demographic features. The source for these data is ANCI (*Associazione Nazionale Comuni Italiani*). Second, we collected political variables at the municipal level, such as the number of candidates for mayor, vote shares, voter turnout, number of council lists, and party alliances. All these variables vary over time. Their source is the Statistical Office of the Italian Ministry of Internal Affairs. Third, we have data on the municipal tax rate on business property, provided by the Italian Ministry of Internal Affairs. This tax instrument was introduced in 1993, at about the same time as the electoral reform, although the technical work leading to the new tax had started a few years earlier (with the approval of law 142/1990). Property taxes are the main source of local tax revenues, covering on average more than 50% of the overall municipal tax revenues. Municipal governments are free to allocate tax proceeds to a variety of alternative uses, such as social assistance, local schools, and public infrastructures. We focus on the business property tax because of its salience in the political debate at the municipal level. The partisan conflict over the appropriate level of business taxation is traditionally sharp, with left-wing candidates pushing for a higher tax rate compared to right-wing ones.[9]

[9]In a subsample of municipalities where we are able to identify the political orientation of the mayor, there is a clear partisan effect on the business real estate tax: on average, left-wing governments set a larger tax rate by 0.209 percentage points (+3.7% over the right-wing average rate of 5.665), and the difference is statistically significant at the 5% level. In a multivariate regression controlling for population, margin of victory, region and time fixed effects, the impact of left-wing governments on the tax rate remains quantitatively similar and statistically different from zero at the 5% level. This is consistent with anecdotal evidence. Consider the

3.3 Econometric strategy

Formally, under the standard assumption of continuity of potential outcomes at the population threshold $P_c = 15,000$, we can identify the local average treatment effect around P_c as: $E[Y_i(1) - Y_i(0)|P_i = P_c] = \lim_{P_i \downarrow P_c} Y_i - \lim_{P_i \uparrow P_c} Y_i$, where $Y_i(1)$ is the potential outcome under runoff elections for municipality i, $Y_i(0)$ the potential outcome under single round elections for the same municipality, P_i population size (as of the last available Census), Y_i the observed outcome, and where we omit time subscripts to simplify notation (see Hahn, Todd, and Van der Klaauw, 2001). This is a local effect because it captures the causal impact of the runoff system only for towns around the threshold P_c. The identifying assumption of continuity of potential outcomes requires that: (i) no other institutions change in a neighborhood of 15,000; (ii) municipalities did not sort around the 15,000 threshold according to their unobservable characteristics after the introduction of the new electoral law. As discussed, the first condition is met in the Italian context. We empirically check for the second condition below.

Various methods can be used to estimate the discontinuity at P_c. We apply both a spline polynomial approximation and local linear regression (see Imbens and Lemieux, 2008). The first method uses the whole sample of municipalities between 10,000 and 20,000 inhabitants and chooses a flexible functional form to fit the relationship between Y_i and P_i on either side of P_c. Specifically, we estimate the model:

$$Y_i = \sum_{k=0}^{p} (\delta_k P_i^{*k}) + D_i \sum_{k=0}^{p} (\gamma_k P_i^{*k}) + \varepsilon_i, \tag{1}$$

where D_i is a treatment dummy equal to one if $P_i \geq P_c$, and the normalized variable $P_i^* = P_i - P_c$ allows us to interpret γ_0 as the jump between the two regression functions at P_c. The local average treatment effect is consistently estimated by $\hat{\gamma}_0$. We show the robustness of the results to multiple orders of the polynomial (namely, $p = 2$, $p = 3$, and $p = 4$).

The second method fits linear regression functions to the observations distributed within a distance h on either side of the threshold. Specifically, we restrict the sample to towns in the interval $P_i \in [P_c - h, P_c + h]$ and estimate the model:

$$Y_i = \delta_0 + \delta_1 P_i^* + D_i(\gamma_0 + \gamma_1 P_i^*) + \varepsilon_i. \tag{2}$$

Again, $\hat{\gamma}_0$ identifies the local average treatment effect. We show the robustness of the results to multiple bandwidths around P_c (namely, $h = 1,000$, $h/2$, and $2h$).

electoral platform of *Rifondazione Comunista*, a small left-wing extremist party (approximately between 4 and 8 percent in national elections). In the 2004 municipal elections the party platform read: "On the real estate tax, an articulated policy is needed, with the aim to reduce the rate on the first residential home for low and medium income households and increase instead the rate on second homes and business real estates."

Finally, to also exploit the (limited) time variation in our data, we estimate the following diff-in-diff model:

$$Y_{it} = \alpha_i + \beta_t + \gamma_0 D_{it} + x'_{it}\rho + \epsilon_{it}, \tag{3}$$

where α_i and β_t are city and year-of-election fixed effects, respectively, while x_{it} is a vector of time-varying covariates. In this case, the identifying variation is coming from municipalities that crossed the threshold P_c between the 1991 and the 2001 Census, and the underlying assumption is that they were on a common trend with respect to the others. This assumption is less compelling than the RDD continuity condition, but we will test its plausibility with a falsification exercise on pre-1993 political outcomes.

3.4 Preliminary evidence

We now provide some empirical evidence showing why Italy is an ideal testing ground. First, because Italian politics perfectly matches the assumptions behind BNT results and informally discussed in Section 2. Second, because the standard RDD validity tests are satisfied and therefore make the identifying assumptions discussed in Section 3.3 credible.

Party system and political polarization. Our theoretical prior on the impact of the runoff system on the number of candidates and policy moderation presumes that there is sufficient polarization in the electorate to prevent the emergence of a large centrist party, and that moderate parties are larger than the extremists. These assumptions fit very well with the Italian environment. Political analysts agree that the party system that emerged from the crisis of the so-called "First Republic" in the early 1990s is strongly polarized.[10] Over our sample period, Italy displayed two main moderate blocks, one on the center-left and one on the center-right (both with an average vote share from 30 to 35 percent in national elections), plus an extremist party on the left (*Rifondazione Comunista*, from 4 to 8 percent) and one on the right (*Lega Nord*, from 4 to 10 percent). Centrist parties tried to break this structure several times, but they never got more than 6 percent as a combined vote share. The two main moderate parties never reached political agreements between them at either the national or local level. Although there existed geographical heterogeneity and some of these parties changed their official name over time, the structure of the national party system remained fairly stable and was reflected in local politics.[11]

[10] See, among others, D'Alimonte and Chiaramonte (2010), Zucchini (2013), De Mucci (2013).

[11] To shed more light on the stability of party preferences just above and just below 15,000, the Online Appendix Table A1 estimates our RDD specifications using as outcomes the vote shares that the main parties obtained in each municipality during the 2001 national election. We use the electoral results in the proportional tier of the mixed-member electoral system for the Parliament, in place in Italy from 1994 to 2001, because voting is more likely to be sincere there (see D'Alimonte, 2001). Specifically, we implement both a spline polynomial approximation as in equation (1) and local linear regression as in equation (2).

Non-attached voters. As argued in Section 2, if all voters were "attached", meaning that they would not turn out in the second round if their candidate lost in the first, there should be no difference between single round and runoff elections. To disclose any effect of runoff, we need some voters to go to the polls and vote for their second-best candidate. To check that this is the case in Italy, we look at the subsample of runoff elections that had two rounds. In the Online Appendix Figure A1, we plot the drop in turnout between the first and second round (on the vertical axis) against the total votes received in the first round by all the excluded candidates (on the horizontal axis); both variables are measured as a fraction of eligible voters. If the drop in participation coincided with the votes for the excluded candidates, all observations should lie along the 45° line. This is obviously not the case: most of them lie well below the line, meaning that in most elections the drop in participation between the two rounds is much smaller than the votes received by the excluded candidates. Thus, the figure suggests that a large fraction of those who voted for losers in the first round vote again in the second.[12]

Strategic voting. The widespread presence of strategic voters would make the comparison between runoff and single round elections ambiguous (see BNT for a discussion). Here, we propose a simple test for strategic voting in the spirit of Fujiwara (2011). The intuition is simple. Fujiwara (2011) finds a strong (negative) effect of runoff on the combined vote share of the first two candidates. This effect may be due to two different reasons: (i) voters strategically favor the first two candidates under single round elections (even if there is an equal number of candidates under single round and runoff), in line with Duverger's Law; (ii) there are more candidates under runoff and therefore the vote share of the first two is mechanically lower. Fujiwara (2011) finds support for the first interpretation in Brazil.[13]

We estimate the effect of runoff on the vote share of the top two candidates (and separately of the first, second, and third candidate), obviously in electoral races where we observe more than two candidates.[14] In particular, in the Online Appendix Table A2, we implement our RDD specifications in two subsamples both above and below 15,000: electoral races with exactly three, and with more than three, candidates. If strategic voting were at work, we

We detect no discontinuity at 15,000 in the vote shares of the moderate, extremist, and centrist parties in national elections. This means that voters' preferences are stable just below and above our threshold.

[12]The figure also reveals that voting for losers in the first round is substantial, ranging from about 5% to more than 50%, with a median value around 30%. But the size of votes for losers is unrelated to the drop in turnout, which remains roughly constant at about 15% of eligible voters. This further suggests that the drop in turnout is not driven by disappointed voters.

[13]More precisely, he concludes that the first interpretation has to be preferred because the effect of runoff on the number of candidates is not precisely estimated to be different from zero (although it is positive in all of his specifications). He interprets his findings as evidence of strategic voting. But he also acknowledges that, because of sample size limitations at the Brazilian threshold of 200,000 registered voters, he cannot completely rule out strategic entry or exit by candidates too.

[14]We do not observe the individual vote shares of candidates ranked below the third position.

should observe that below the threshold voters converge on the first two candidates—at the expense of the third candidate or of the others—when there are more than two candidates. We instead find that this is not the case in Italian municipalities.

This evidence is not a conclusive test of strategic voting, because the strategic behavior of political candidates might be driven by the "anticipation" of strategic voting by voters in some municipalities. Moreover, as we discussed already (see section 2), Bouton (2013) shows that with strategic voting two candidates equilibria may also exist in runoff elections. In other words, municipalities with three or more candidates under both single round and runoff are selected samples, whose comparison should be interpreted with caution. Yet, in contrast with the Brazilian evidence, the hypothesis of widespread strategic behavior by voters finds no support in our data.

Manipulative sorting. Finally, as a preliminary check on the validity of our RDD strategy, we test for manipulative sorting around the 15,000 threshold in response to the electoral reform in 1993. In particular, in the Online Appendix Figure A2, we test if the *difference* between the density in the 1991 Census (before the reform) and the density in the 2001 Census (after the reform) shows a discontinuity at the 15,000 threshold, in the spirit of McCrary (2008). Such a discontinuity would imply that some municipalities reacted to the electoral reform by manipulating their population size, therefore violating the identifying assumption of our RDD. The figure performs this test by using the density difference as outcome and fitting a 3^{rd}-order polynomial in population size on either size of the threshold. There is no evidence of manipulative sorting between the 1991 and the 2001 Census, as the point estimate of the discontinuity is -0.007 (standard error, 0.027).

To further check against the possibility of manipulative sorting, we perform balance tests of both time-invariant and pre-treatment city characteristics. The time-invariant characteristics are geographic location, area size, and altitude from sea level. The pre-treatment characteristics come from the 1991 Census and refer to the age structure, educational attainments, employment, and house facilities. The Online Appendix Table A3 uses the time-invariant variables as outcomes and estimates equation (1) with polynomials of different order and equation (2) with different bandwidths. The Online Appendix Table A4 does the same with the pre-treatment variables from the 1991 Census. None of these variables displays a significant discontinuity at the threshold, and this further supports the validity of our RDD.

3.5 Estimation results on political outcomes

How does the number of candidates vary with the electoral rule? We have data on both the number of candidates for mayor and the number of (party) lists for the city council.[15]

We use data on all 2,027 mayoral terms pooled together, because the outcome of interest (the number of candidates) is time-varying. To accommodate for possible serial correlation, we cluster the standard errors at the city level. Treatment assignment depends on population size as measured by the last available Census, that is, either 1991 or 2001. On average, in municipalities between 15,000 and 20,000 inhabitants, 5.1 candidates run for mayor, as opposed to 3.6 in municipalities between 10,000 and 15,000. The electoral lists supporting the candidates for mayor are 6.9 above 15,000 and 3.7 below.

Clearly, these differences in the number of candidates and lists might be confounded by other factors associated with population size. To identify the causal effect of the electoral rule separately from the effect of city size, we thus implement our RDD strategy as discussed in Section 6.3. In Table 1, we report the main estimates of the impact of runoff elections on political outcomes. We implement both a spline polynomial approximation as in equation (1), with polynomials of three different orders, and local linear regression as in equation (2), with three different bandwidths. In panel A we report the baseline results, while in panel B we also add city characteristics as control variables (namely, macro-region dummies, area size, altitude, per-capita transfers, per-capita income, labor force participation, elderly index, family size, mayor's duration in office, and a dummy identifying second-term mayors). As long as these additional covariates are balanced around the population threshold, their inclusion should not affect the estimates, but should increase accuracy.

The results in Table 1 show a positive and statistically robust effect of runoff on the number of mayoral candidates. Just above the threshold, we observe approximately one more candidate and two more lists. If we look at the baseline estimate of 1.103 in the first column, runoff elections produce a 29% increase in the number of candidates relative to single round elections just below the threshold. The impact on the number of lists is even greater (+51%), but, as said, it may be confounded with the alliance restriction.

To assess the relevance of the alliance restriction, in Table 1 we estimate the effect of the electoral rule on the average number of lists supporting each mayoral candidate (captured by the ratio "lists/candidates"). We also estimate the distinct effects of the electoral rule on the

[15]Note that our main outcome of interest is the number of mayoral candidates, as the number of lists may reflect both different electoral rules and different alliance restrictions above and below 15,000: as already mentioned, below the threshold there has to be a one-to-one correspondence between lists and mayoral candidates, whereas above 15,000 mayoral candidates can be supported by more than one list. Nevertheless, comparing the number of lists is also relevant, particularly because it allows an intertemporal comparison: before 1993 the mayor was not directly elected and we only have data on lists.

number of lists supporting the winning candidate vs the losing candidates. The estimated effect on the ratio between lists and candidates is not statistically different from zero, meaning that the alliance restriction has no impact. This is confirmed by the fact that the effect on the number of lists supporting the losing candidates is statistically significant, but there is no significant discontinuity in the number of lists supporting the elected mayor. As a matter of fact, this last outcome variable can only be affected by the restriction on feasible alliances, as the winning candidate is one by definition, both above and below the threshold, while the result on the number of lists supporting the losing candidates can be driven by the number of candidates itself. The (zero) result on the number of lists supporting the winning candidate (i.e., the future mayor) is also reassuring about the fact that the confounding institutional variations at 15,000 do not affect the future bargaining within the municipal government coalition in ways that are not captured by the theoretical mechanism in BNT.

Figure 1 provides a visual illustration of the results on political outcomes (first four graphs). There, we report both the scatterplot of each outcome (averaged over 250-inhabitant intervals) and the spline third-order polynomial (with the 95% confidence interval). The discontinuities of political outcomes at the threshold are clearly visible both from the scatterplots and from the estimated polynomials, with the exception of the number of lists supporting the winning candidate, for which we have no significant results as expected. Clearly, the RDD setup allows for identification only in a neighborhood of 15,000, but the positive association between runoff and the number of candidates persists far away from the threshold.

In Table 2, we run a falsification test on the only political outcome available for the pre-treatment period. As before 1993 a parliamentary system was in place, we can only run our falsification test on the number of electoral lists. Table 2 reports the RDD estimates for all mayoral terms elected from 1985 to 1992, and for municipalities between 10,000 and 20,000 inhabitants. No significant discontinuity is detected. Before the 1993 electoral reform, the number of lists was exactly equal just below and just above the 15,000 threshold. This provides additional evidence in favor of the robustness of the baseline results.

To further assess the sensitivity of our results, the Online Appendix Figure A3 summarizes a set of 1,000 placebo estimates at false thresholds for the main outcomes. Specifically, to evaluate the possibility that our results arise from random chance rather than a causal relationship, we implement estimations at false population thresholds below and above the 15,000 threshold (namely, any point from 13,501 to 14,000 and from 15,501 to 16,000 in order to stay away from the true threshold). At these false thresholds, we expect to find no systematic evidence of treatment effects similar to our baseline results. For each outcome, the figure reports the cumulative distribution function of the 1,000 placebo point estimates (using a specification with spline 3rd-order polynomial), normalized with respect to the

baseline point estimates from the first column of Table 1. This means, for instance, that a normalized coefficient of 100 stands for a placebo point estimate equal to the true baseline estimate at 15,000. Thus, most normalized coefficients should be close to zero, and we should observe only a few normalized coefficients outside the interval [-100, +100], in fact no more than 5% in both tails. Indeed, only 1.6% of the placebo estimates are larger than the baseline result for the number of candidates in absolute value (but they have the opposite sign), and none of the placebo estimates exceed the baseline result for either the number of lists or the number of opposition lists. All cumulative distribution functions are steeper around zero, where the false estimates tend to concentrate. By contrast, and again as expected, there are no robust results for the number of lists supporting the mayor.

Finally, in Table 3, we implement diff-in-diff estimations on political outcomes as in equation (3). As discussed, the identifying variation comes from municipalities crossing the population threshold from the 1991 to the 2001 Census, under the restriction that movements from above to below and vice versa have symmetric effects. Again, the empirical evidence is in line with our theoretical priors, as point estimates for all political outcomes are quantitatively similar to the RDD results.[16]

Altogether, these results strongly confirm the theoretical idea that runoff increases the number of (serious) candidates, compared to single round. In contrast to Fujiwara (2011), however, this is due to the strategic behavior of candidates, not of voters (recall the previous discussion on the absence of evidence supporting strategic voting in our context).

3.6 Estimation results on policy volatility

Do runoff elections lead to policy moderation? In this section we address this question. Ideally, we would like to test whether extremist parties are more often included in the municipal government coalition, and exert more policy influence, under single round elections. Unfortunately, we cannot do that because of data limitations (although we say something about this

[16]In the Online Appendix Table A5, we remove the symmetry restriction and separately look at the effect of moving from below to above 15,000 (33 municipalities) vs moving from above to below (9 municipalities) in a cross-section of municipalities for which political outcomes are available both in the 1990s and in the 2000s. The two effects are very similar and again in line with the theoretical priors: municipalities that moved to the runoff system in the 2000s experienced an increase in the number of candidates by 27%; those that moved to the single round system experienced a drop by 34%. These (symmetric) results do not support Callander's (2005) claim, discussed in section 2, that the effect of runoff should depend on initial conditions. Furthermore, the Online Appendix Table A5 allows us to evaluate the diff-in-diff assumption of common trend, as in the last row we estimate whether municipalities that crossed the threshold are associated with different pre-treatment levels of political competition (i.e., number of party lists) in the 1980s. This falsification exercise supports the identifying assumption that population variations were fairly exogenous.

point below).[17] Instead, we explore another feature of policy outcomes: whether the average policy volatility is lower in municipalities above 15,000 inhabitants, where the runoff system should moderate the influence of extremist voters. Indeed, a change in the partisan identity of the local government should be associated with a smaller policy change in those municipalities where the extremist parties are excluded from government or less influential. This test, of course, assumes that political turnover is the same above and below the threshold; something that we test and cannot reject in the data (see below).

Policy volatility. We measure policy volatility in two ways. First, we consider the *intertemporal* variation in the business property tax rate. To do this, we measure the unconditional variance of the tax rate across legislative terms in the same municipality. Thus, for each municipality, we average the yearly tax rates over the mayoral term, excluding election years to avoid the overlapping of different mayors over the same calendar year and possible electoral cycle effects.[18] Let τ_t^i denote this average tax rate for municipality i and the mayoral term initiated in year t. We then compute the unconditional variance of these average tax rate across mayoral terms for each municipality, $y^i = Var(\tau_t^i)$, obtaining one observation (i.e., one measure of volatility) per municipality.

Next, we consider the *cross-sectional* variation in the business property tax, within bins of municipalities of similar population size ("similar" meaning within intervals of 100 inhabitants). Specifically, we first compute the same average tax rate τ_t^i defined above, for each municipality i and each mayoral term t. For each term t and each bin b we then compute the unconditional variance of τ_t^i across municipalities of the same bin, $y_t^b = Var(\tau_t^i)$. Finally, for each bean b we compute the simple average of these variances across mayoral terms, and obtain a cross sectional variance for each bin, $y^b = E(y_t^b)$.[19]

The RDD results are reported in Table 4, for both indicators of volatility. The intertemporal variance of the business property tax shows a sharp and negative discontinuity when moving from just below to just above the 15,000 threshold. Point estimates are consistently negative and statistically significant at standard levels, although they are more volatile than those on political outcomes. The baseline estimate of -0.455 in the first column corresponds

[17] In several municipalities and particularly in small ones, candidates for mayor are supported by electoral lists that use local labels, such as the name of the city or electoral slogans. This is how the lists are named in the official data, and this is why we cannot identify the exact political identity of each list in most cases.

[18] All of the results presented in this section are qualitatively similar if we include election years in the calculation of the average tax rates within each term.

[19] The average frequency of municipalities within each bin is around 27, with the minimum value equal to 4 and the maximum to 56. In the two bins just below and just above the 15,000 threshold, the average frequency is around 25 municipalities per bin. All of the following results are qualitatively similar with bin sizes of 10 inhabitants (about 5 municipalities in each bin) and of 50 inhabitants (about 15 municipalities), and they are available upon request. At the price of reducing the outcome variation, we prefer a size of 100 inhabitants because in this case the unconditional variance is more precisely estimated within each bin.

to a decrease of about 61% in the variance of the tax rate just above the threshold. Similar results hold for the cross-sectional variance. Here, all estimates are by weighted least squares (with weights based on the frequency of municipalities in each bin) to account for heteroskedasticity and to accommodate for the different accuracy in the estimation of the variance in bins of different numerosity. The baseline estimate of -0.659 in the first column indicates that, in a neighborhood of the threshold, the runoff system decreases the variance of the property tax by about 71%. Point estimates are stable when comparing specifications without and with covariates (panel A vs panel B).[20]

A graphical representation of the results on policy volatility is provided in Figure 1 (last two graphs), where the negative discontinuities at the threshold are evident both in the scatterplots and in the estimated polynomials. These effects appear to be more local—that is, less persistent far away from the threshold—compared to those on political outcomes, but we cannot assign any causal interpretation to the association between population size and policy volatility once we move away from the electoral rule cutoff at 15,000.

The Online Appendix Figure A3 (again, last two graphs for the policy volatility measures) implements placebo estimations at false thresholds. Results on both the time and cross-sectional volatility of the tax rate are very robust, as only 2.7% (3.5%) of the false estimates are larger than the baseline one for the cross-sectional (time) variance in absolute value.

Potential channels. The above results disclose a (reduced-form) effect of the electoral system on policy volatility. There remains the concern that the lower tax volatility under runoff elections could be driven by other channels, rather than policy moderation. In particular, the electoral system could affect the level of political turnover, by influencing the probability of government crises (through a vote of no confidence by the council) or the probability of political swings between left and right administrations. In the estimates with covariates (panels B in Table 4), we already control for this channel by including two proxies of political turnover (namely, the duration in office of the elected mayor and whether he reaches a second term or not). Nevertheless, we can directly test whether political turnover is affected by the electoral system. In the Online Appendix Table A6, we report the RDD estimates on the two observable outcomes associated with political turnover: the average duration in office (measured in days) and the fraction of mayors in their second term. None of these outcomes shows a significant discontinuity at the 15,000 threshold, and the point estimates display no consistent pattern. This rules out the most plausible alternative explanation of our (reduced-form) results.

[20]There is instead some sensitivity of the point estimates to the functional form of the polynomial and to the estimation method. This might also reflect measurement error in the unconditional variance of the tax rate in relatively small samples. On average, there are only four mayoral terms from which the intertemporal variance is computed, and the cross-sectional variance is computed from bins of heterogeneous size.

Finally, to provide some direct evidence on the political extremism channel, we estimate the effect of runoff elections on the probability that the leftist political extreme (i.e., the Communist Party, *Rifondazione Comunista*) joins the main center-left coalition at the local level.[21] The Italian Ministry of Internal Affairs provides details on the party lists supporting different candidates for mayor in the first round. We manually coded these data to create a dummy variable (*Communist Party alone*) that equals one in elections where the Communist Party ran either alone or allied with other smaller leftist parties (e.g., *La Rete*, *Verdi*, *Pdci*), but not with the more moderate and larger center-left party of the time (e.g., *DS*, *Ulivo*). Here, as discussed above, we face a key problem, because in several municipalities mayoral candidates are supported by electoral lists with local names. These lists may correspond to national political parties or not; we simply do not know. After dropping the municipalities with this missing information, we are left with a (self-selected) sample that is only half the original sample (i.e., 1,045 observations, of which 670 are below the threshold). Another limitation is that in some municipalities where we observe a center-left coalition but we do not observe the Communist Party running alone, it could be either because this extremist party joined the main coalition, or because it was not organized in those municipalities. Both instances are coded as zero in our dummy variable of interest. Measurement error due to these data limitations might be relevant.

In the Online Appendix Table A7, we report RDD estimations where the dependent variable is the dummy variable *Communist Party alone* (which equals one in about 11% of the elections in the small sample). Point estimates are large and positive, as expected, and they are statistically significant at standard levels with most estimation methods. On average, the probability that the Communist Party runs alone in the runoff system more than double compared to single round elections.

On the whole, the quasi-experimental and descriptive evidence discussed in this section supports the conclusion that runoff systems indeed induce policy moderation, because they dampen the influence of extremist parties or exclude them from government coalitions.

4 Concluding remarks

Political extremism is often regarded as harmful, because it enhances policy uncertainty and it hinders the effective functioning of democracies (e.g., Bingham Powell, 1982). Knowing which political institutions can alleviate the adverse consequence of political extremism is

[21] The same exercise cannot be replicated for the center-right coalition, where the extremist parties are either too small at the local level (e.g., *Msi*, *La Destra*), or geographically concentrated in some areas of the country and focused on separatist issues (e.g., *Lega Nord*).

therefore important. This is particularly true for young democracies, where often extremism is rampant and democratic constitutions have to be designed from scratch.

This paper has implemented an RDD to empirically compare single round vs runoff elections from this perspective. In Italy, municipalities just above 15,000 inhabitants (which rely on runoff elections) have a larger number of candidates and less volatile tax rates, compared to municipalities just below 15,000 inhabitants (which have single round elections). As political turnover is constant in the neighborhood of the threshold, the result on policy volatility confirms our prior that runoff elections reduce the policy influence of the political extremes.

While our results clearly speak in favor of the runoff system, one should also be careful not to overstress this implication. Care should be taken, for instance, in extrapolating our results to different institutional contexts, such as primary elections to select parties' candidates. Another important limitation of our work is that we do not study the long run effects on the number of parties and on their ideological positions. One may argue that the runoff system, while reducing the influence of political extremism on policies, allows extremist parties to survive by giving them visibility at the first round. In normal times this is of no concern; but in times of crisis this can make a difference, allowing extremists to gain consensus. Under a single round system, instead, extremists would join some bigger party and in the long run they would disappear inside this party. Conversely, single round elections might create an excessively high barrier to entry for new parties, and this may restrict the menu choices of voters in the long run—something that could be an issue in case of large shocks to voters' preferences. We leave these issues for future research.

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Tables and figures

Table 1 – Impact of runoff on political outcomes, RDD estimates

	Spline 3^{rd}	Spline 2^{nd}	Spline 4^{th}	LLR (h)	LLR ($h/2$)	LLR ($2h$)
A. Estimations without covariates						
No. of candidates	1.103***	1.098**	1.532***	1.300***	1.731**	1.335***
	(0.382)	(0.487)	(0.302)	(0.408)	(0.676)	(0.293)
No. of lists	2.184***	1.497**	2.163***	1.736***	1.739**	2.291***
	(0.526)	(0.624)	(0.413)	(0.540)	(0.794)	(0.436)
Lists/candidates	0.020	-0.145	-0.014	-0.147	-0.118	0.063
	(0.201)	(0.239)	(0.151)	(0.208)	(0.305)	(0.170)
Opposition lists	1.657***	1.050**	1.676***	1.426***	1.147**	1.643***
	(0.374)	(0.448)	(0.297)	(0.384)	(0.569)	(0.299)
Mayor's lists	-0.016	-0.110	-0.031	-0.209	-0.016	0.044
	(0.226)	(0.252)	(0.184)	(0.231)	(0.307)	(0.196)
Obs.	2,027	2,027	2,027	364	175	761
B. Estimations with covariates						
No. of candidates	1.220***	1.147**	1.598***	1.331***	1.779**	1.418***
	(0.375)	(0.472)	(0.297)	(0.396)	(0.677)	(0.287)
No. of lists	2.260***	1.463**	2.254***	1.695***	2.025***	2.406***
	(0.517)	(0.610)	(0.405)	(0.557)	(0.735)	(0.423)
Lists/candidates	0.031	-0.159	0.001	-0.154	-0.116	0.076
	(0.186)	(0.220)	(0.140)	(0.184)	(0.290)	(0.153)
Opposition lists	1.717***	1.055**	1.746***	1.388***	1.358**	1.733***
	(0.384)	(0.462)	(0.301)	(0.392)	(0.560)	(0.302)
Mayor's lists	-0.014	-0.111	-0.024	-0.202	0.058	0.055
	(0.216)	(0.240)	(0.177)	(0.226)	(0.326)	(0.187)
Obs.	2,027	2,027	2,027	364	175	761

Notes. Election years between 1993 and 2007; municipalities between 10,000 and 20,000. Dependent variables: *No. of candidates* running for mayor in the first round; *No. of lists* supporting mayoral candidates in the first round; *Lists/candidates* ratio; *Opposition lists* supporting the losing candidates; *Mayor's lists* supporting the winning candidate. Estimation methods: spline polynomial approximation as in equation (1), with 3^{rd}, 2^{nd}, and 4^{th} polynomial, respectively; local linear regression as in equation (2), with bandwidth $h = 1,000$, $h/2$, and $2h$, respectively. Estimations in Panel B also include the following covariates: macro-region dummies, area size, altitude, transfers, income, participation rate, elderly index, family size, mayor's duration in office (in days), mayor's second-term dummy. Robust standard errors clustered at the city level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 2 – Falsification tests on pre-treatment political outcomes, RDD estimates

	Spline 3^{rd}	Spline 2^{nd}	Spline 4^{th}	LLR (h)	LLR ($h/2$)	LLR ($2h$)
			A. Estimations without covariates			
No. of lists	-0.178	-0.231	-0.121	-0.033	-0.128	-0.336
	(0.449)	(0.544)	(0.349)	(0.506)	(0.668)	(0.365)
Obs.	783	783	783	137	67	284
			B. Estimations with covariates			
No. of lists	-0.034	-0.202	-0.124	0.069	0.070	-0.244
	(0.348)	(0.419)	(0.290)	(0.351)	(0.502)	(0.292)
Obs.	783	783	783	137	67	284

Notes. Election years between 1985 and 1992; municipalities between 10,000 and 20,000. Dependent variable: *No. of lists*, i.e., party lists competing under proportional representation in this pre-treatment period (1985–1992). Estimation methods: spline polynomial approximation as in equation (1), with 3^{rd}, 2^{nd}, and 4^{th} polynomial, respectively; local linear regression as in equation (2), with bandwidth $h = 1,000$, $h/2$, and $2h$, respectively. Estimations in Panel B also include the following covariates: macro-region dummies, area size, altitude, transfers, income, participation rate, elderly index, family size, mayor's duration in office (in days), mayor's second-term dummy. Robust standard errors clustered at the city level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 3 – Impact of runoff on political outcomes, diff-in-diff estimates

	A. Estimations without covariates	B. Estimations with covariates
No. of candidates	1.186***	1.159***
	(0.300)	(0.300)
No. of lists	2.303***	2.259***
	(0.394)	(0.392)
Lists/candidates	0.284*	0.282*
	(0.170)	(0.170)
Opposition lists	1.787***	1.746***
	(0.308)	(0.308)
Mayor's lists	0.143	0.152
	(0.181)	(0.181)
Obs.	2,027	2,027

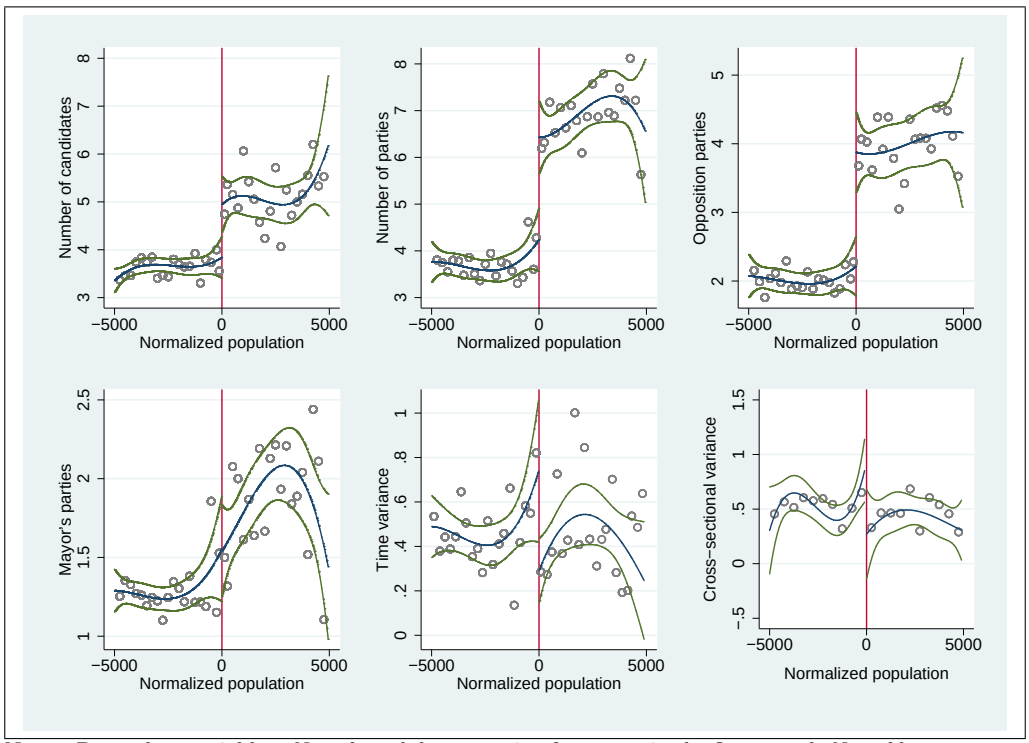
Notes. Election years between 1993 and 2007; municipalities between 10,000 and 20,000. Dependent variables: *No. of candidates* running for mayor in the first round; *No. of lists* supporting mayoral candidates in the first round; *Lists/candidates* ratio; *Opposition lists* supporting the losing candidates; *Mayor's lists* supporting the winning candidate. Estimation methods: diff-in-diff specifications with municipality and year-of-election fixed effects, as in equation (3). Estimations in column B also include the following (time-varying) covariates: transfers, income, participation rate, elderly index, family size. Robust standard errors are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 4 – Impact of runoff on policy volatility, RDD estimates

	Spline 3^{rd}	Spline 2^{nd}	Spline 4^{th}	LLR (h)	LLR ($h/2$)	LLR ($2h$)
			A. Estimations without covariates			
Time variance	-0.455**	-0.647***	-0.238*	-0.651**	-0.697*	-0.378**
of business property tax	(0.182)	(0.240)	(0.140)	(0.255)	(0.389)	(0.160)
Obs.	575	575	575	118	59	236
Cross-sectional variance	-0.659**	-0.937***	-0.313	-0.694**	-0.364	-0.443**
of business property tax	(0.258)	(0.294)	(0.201)	(0.256)	(0.590)	(0.203)
Obs.	92	92	92	19	9	37
			B. Estimations with covariates			
Time variance	-0.450***	-0.614***	-0.237*	-0.563***	-0.167	-0.377***
of business property tax	(0.170)	(0.224)	(0.132)	(0.211)	(0.167)	(0.140)
Obs.	575	575	575	118	59	236
Cross-sectional variance	-0.627**	-0.856***	-0.352*	-0.736**	-0.832	-0.371*
of business property tax	(0.276)	(0.306)	(0.199)	(0.274)	(0.278)	(0.184)
Obs.	92	92	92	19	9	37

Notes. Election years between 1993 and 2007; municipalities between 10,000 and 20,000. Dependent variables: *Time variance* (i.e., variance across terms averaged over the entire sample period) and *Cross-sectional variance* (i.e., variance across municipalities averaged over bins of 100 inhabitants) of the business property tax rate. Estimation methods: spline polynomial approximation as in equation (1), with 3^{rd}, 2^{nd}, and 4^{th} polynomial, respectively; local linear regression as in equation (2), with bandwidth $h = 1,000$, $h/2$, and $2h$, respectively. When the dependent variable is the cross-sectional variance, estimates are by weighted least squares, with weights given by (the inverse of) the numerosity of each bin. Estimations in Panel B also include the following covariates: macro-region dummies, area size, altitude, transfers, income, participation rate, elderly index, family size, mayor's duration in office (in days), mayor's second-term dummy. Robust standard errors clustered at the city level are in parentheses. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Figure 1 – Impact of runoff on political outcomes and policy volatility



Notes. Dependent variables: *No. of candidates* running for mayor in the first round; *No. of lists* supporting mayoral candidates in the first round; *Opposition lists* supporting the losing candidates; *Mayor's lists* supporting the winning candidate; *Time variance* (i.e., variance across terms averaged over the entire sample period) and *Cross-sectional variance* (i.e., variance across municipalities averaged over bins of 100 inhabitants) of the business property tax rate. The central line is a spline 3^{rd}-order polynomial in the normalized population size (i.e., population minus 15,000); the lateral lines represent the 95% confidence interval of the polynomial. Scatter points are averaged over 250-inhabitant intervals. Municipalities between 10,000 and 20,000 only.