Electoral Rules and Politicians' Behavior: A Micro Test*

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Abstract

Theory predicts that the majoritarian electoral system should produce more targeted redistribution and lower politicians' rents than proportional representation. We test these predictions using micro data on the mixed-member Italian House of Representatives, and address the nonrandom selection into different electoral systems by exploiting a distinctive feature of the two-tier elections held in 1994, 1996, and 2001: candidates could run for both the majoritarian and proportional tier, but if they won in both tiers they had to accept the majoritarian seat. Focusing on elections decided by a narrow margin allows us to generate quasi-experimental estimates of the impact of the electoral rule. The results confirm theoretical predictions, as majoritarian representatives put forward more bills targeted at their constituency and show lower absenteeism rates than their proportional colleagues. Results are stronger for behaviors at the beginning of the term, when most politicians expect to run for reelection in the same tier.

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1 Introduction

Electoral rules are usually clustered around two opposing types: majoritarian versus proportional systems. In majoritarian elections—like in the US or the UK—members of parliament are elected in single-member districts with plurality voting, also known as the winner-take-all rule. In proportional elections—like in the Netherlands, Spain, South Africa, and many other countries—party lists compete for votes in multiple-member districts and parliament seats are allocated to each list according to its vote share.

Political scientists have long studied the impact of electoral systems on political outcomes, such as the number of parties or government structure. Economists have recently contributed to the subject by investigating how the electoral system influences politicians' equilibrium behavior and, ultimately, public policies. On the one hand, the electoral rule determines which groups in society are pampered by political candidates, that is, whether politicians address society at large (by, for example, proposing a platform that would please the median voter) or follow a particularistic strategy (by using targeted benefits to build a coalition of diversified interests). In this respect, the majoritarian system—as opposed to proportional representation—is shown to be associated with more targeted redistribution and less public goods (Persson and Tabellini, 1999; Lizzeri and Persico, 2001; Milesi-Ferretti, Perotti, and Rostagno, 2002). On the other hand, the electoral rule also decides how effectively voters can keep elected officials accountable for their actions. Assuming that elected officials can extract rents, such as shirking or corruption, the interests of voters and politicians diverge. Theoretical predictions on this point are ambiguous. If majoritarian elections increased the accountability of elected officials, this would result in lower rents (Persson and Tabellini, 1999; 2000). If proportional representation lowered entry barriers for honest competitors, however, this could also reduce rent extraction (Myerson, 1993).

To the best of our knowledge, in this paper we provide the first micro test of the causal effect of the electoral system on the behavior of elected officials.[1] We use individual data for the mixed-member Italian House of Representatives from 1994 to 2006, in order to compare the in-office activities of politicians elected in single-member majoritarian districts with those of politicians elected under proportional representation.

Many authors have tested the predictions of the theoretical literature with cross-country aggregate data, finding that proportional systems are associated with broader redistribution and higher perceived corruption.[2] The effect of electoral rules on country-level outcomes, however, may operate not just through politicians' incentives, but also through other con-

[1]Frechette, Kagel, and Morelli (2009) use experimental data to investigate the trade-off faced by potential legislators between the provision of public goods and targeted redistribution.

[2]See Persson and Tabellini (1999, 2003), Milesi-Ferretti, Perotti, and Rostagno (2002), Persson, Tabellini, and Trebbi (2003), and Kunicova and Rose-Ackerman (2005). See Section 2.2 for a discussion.

founding channels, such as the government structure (single-party versus multiple-party), that cannot be easily disentangled with macro data. Furthermore, political institutions are equilibrium outcomes, whose effect is difficult to estimate with macro data because of the lack of convincing sources of exogenous variation.

This endogeneity problem, of course, might arise with individual-level data too. For example, candidates with strong local ties, such as those who served in local governments or have their private business established in a specific area, may be more likely to run in majoritarian districts, and once elected they will carry out more locally targeted policies simply because of their preferences and expertise. The electoral system of the Italian House of Representatives from 1994 to 2006, however, had distinctive features that can be used to control for endogeneity by applying a Regression Discontinuity Design (RDD). Specifically, the system had two tiers: 75% of members were elected in single-member districts with plurality voting, and 25% were elected under proportional representation. Candidates could run for both the majoritarian and proportional tier; but if they were elected in both tiers, they had to accept the majoritarian seat. As a result, if random factors—for example, unexpected breaking news or rain on election day—play even a small role in determining electoral outcomes, the selection into the majoritarian tier mimics random assignment for the elected officials who won or lost by a narrow margin in single-member districts.[3]

We use this quasi-experimental framework to estimate the effect of being elected in the majoritarian system—as opposed to being elected in the proportional system—on two individual outcomes: the amount of geographically targeted activities carried out after election and rents, both averaged over the term. As a measure of local activities, we use the share of bills targeted at the region to which the district of election belongs over the total number of bills presented. As a proxy for politicians' rents, we use the absenteeism rate, that is, the percentage of parliamentary votes missed without any legitimate reason. We find that being elected in the majoritarian tier more than doubles the share of bills targeted at the region of election, and it decreases the absenteeism rate by about one-third.

Note that the above results refer to the treatment "being elected" rather than "running for reelection" under the majoritarian system. This is because the Italian institutional framework only provides a credible source of exogenous variation in the assignment to the system of election and not to the system of reelection (indeed, if the assignment to the system of reelection were as good as random, the current behavior of incumbent politicians should not be affected by their future assignment). The differential incentives set by the system of election could be divided into two categories: (i) "reelection" incentives, if politicians want to be re-appointed for an additional term; (ii) "commitment" incentives, which are the

[3]See Lee, Moretti, and Butler (2004), Hainmueller and Kern (2008), and Lee (2008) for different RDD exercises using a narrow margin of victory in (single-member) plurality elections.

intrinsic motivations for delivering their electoral promises. Reelection incentives are also associated with the system of election because politicians, once they have been assigned to a given electoral tier, tend to persist there (this is indeed what we observe in our data). But congressmen who do not expect to run for reelection do not stop sponsoring bills or attending vote sessions, because of their commitment incentives and voters' monitoring.

Our RDD micro test captures the joint impact of reelection and commitment incentives in majoritarian versus proportional elections. While we cannot separate the two, we actually find that the effect of the system of election is much stronger on first-year behaviors, when the subjective probability of running in the same tier is higher (see Section 6.2 for a more detailed discussion). Being elected in the majoritarian tier almost triplicates the share of targeted bills and has a negative impact of about one-half on the absenteeism rate. On the contrary, the effect of the system of election on last-year variables is lower (bill sponsorship) or insignificant (absenteeism rate).

The rest of the paper is organized as follows. In Section 2, we discuss the theoretical and empirical studies that our contribution builds on. Section 3 describes the Italian institutional and electoral system. Section 4 formally introduces our identification strategy. We discuss the data in Section 5 and the empirical results in Section 6. We conclude with Section 7.

2 Related Literature

2.1 Theory

Various models in political economics have studied the impact of electoral rules on the provision of broad versus targeted policies. Persson and Tabellini (1999, 2000) compare electoral systems within a probabilistic-voting model, where two office-seeking candidates (or parties) make binding electoral promises. They show that in the proportional system political competition focuses on swing voters in the population at large, while in the majoritarian system competition focuses on swing districts only. In the latter case, the interests of safe districts are not internalized by the equilibrium platforms, so that targeted policies are overprovided at the expense of public goods.

In Lizzeri and Persico (2001), politicians are still fully committed to their platform, but voters are homogeneous. In the proportional system, elections are won by the candidate who gets more than 50% of the votes in a nationwide district. In the majoritarian system, elections are won by the candidate who gets more than 50% of the votes in more than 50% of the local districts, 25% of the votes being just enough to gain general elections. The majoritarian system therefore lowers the size of the minimum winning coalition that can be built with targeted redistribution, and it is less likely to provide public goods.

Milesi-Ferretti, Perotti, and Rostagno (2002) use a different rationale to link the electoral rule to targeted policies. They build a citizen-candidate model with no commitment to preelection platforms. Citizens are heterogeneous both in terms of social group and geographical district. Under the assumption that the distribution of social groups is the same across districts, government officials belong to the same group in the majoritarian system. As a result, the median voter in each district chooses a representative biased toward locally targeted policies, anticipating that policies targeted at social groups are not contentious.

Summing up, all of these models share a common prediction about the effect of the electoral system on politicians' equilibrium behavior.

Hypothesis 1 (H1): *Politicians elected in the majoritarian system carry out more geographically targeted policies than politicians elected in the proportional system.*

Politicians' rents are another outcome usually thought to be influenced by the electoral system. If monitoring is less than perfect, elected officials can shirk, that is, put low effort into their public duties to cultivate private interests, or they can exploit their discretionary authority to obtain bribes. Either in the form of shirking or plain corruption, politicians' rents depend on the degree of voters' monitoring over elected officials and on the intensity of the punishment for misbehaviors, and the electoral system determines both elements.

In Persson and Tabellini's (1999) model discussed above, rents are a component of the electoral promise made by candidates. In the majoritarian system, only swing districts are relevant and, because voters in these districts are more reactive to policy changes, political competition is stiffer; politicians become more disciplined and extract lower equilibrium rents. Persson and Tabellini (2000) use a different setup to derive the same result. They build a career-concern model in which elected officials care about reelection. Under majoritarian elections, characterized by individual-candidate ballot, reelection opportunities are based on individual reputation. On the contrary, under proportional representation with closed party lists, there is a free-rider problem among candidates in the same list. As a result, rents are lower in the majoritarian system.

Unlike the prediction about targeted policies, the relationship between the electoral rule and politicians' rents is not unambiguous. Myerson (1993) sets up a game-theoretic model showing that the proportional system may reduce entry barriers for honest politicians and, consequently, equilibrium rents (see also Myerson, 1999). Political parties differ along two dimensions: ideology and honesty. Some voters prefer the leftist party, while others prefer the rightist party; but all voters prefer honest parties. With plurality voting, a dishonest party can still clinch power, because one of the possible equilibria is the self-fulfilling prophecy that a close race between two dishonest candidates will take place. On the contrary, under proportional representation, voters are free to pick their first-best choice; equilibrium rents are therefore lower than in the majoritarian system.

We can now derive a second prediction about the effect of the electoral system on politicians' equilibrium behavior.

Hypothesis 2 (H2): *If the accountability effect dominates the entry-barrier effect, politicians elected in the majoritarian system extract less rents than politicians elected in the proportional system.*

2.2 Macro tests

The models discussed in the previous section have motivated a large number of empirical studies that use cross-country data to test the effects of the electoral rule on aggregate outcomes. Persson and Tabellini (2003) find a negative and significant effect of the majoritarian system on both welfare state spending (as a proxy for broad, nontargeted redistribution) and the perceived level of corruption (as a proxy for politicians' rents).[4] These results are robust to the use of different estimation strategies (OLS, matching estimators, parametric selection correction models, fixed-effect panel models, and IV).

Milesi-Ferretti, Perotti, and Rostagno (2002) use OLS and panel estimators with country-specific shocks to evaluate the effect of the electoral system on both public goods (intended here as a measure of policies targeted to geographic constituencies) and transfers (as a measure of policies targeted to social constituencies). They find a positive and significant relationship between the degree of proportionality and transfer spending in OECD countries, but no conclusive evidence on the provision of public goods. Funk and Gathmann (2009) instead use data on Swiss cantons since 1890 and a diff-in-diff approach to estimate the policy impact of the adoption of proportional representation. They find that the proportional system is associated with greater spending in public goods such as education and welfare benefits, while it decreases targeted outlays such as roads and agricultural subsidies.

The above studies find support for the hypotheses that the majoritarian system increases targeted policies and reduces politicians' rents. However, although macro tests detect important correlations that are consistent with the theory, it is doubtful whether they are able to disclose causal effects. OLS and matching rely on the conditional independence assumption. But the electoral rule, like any other political institution, is an equilibrium outcome also determined by unobservable factors. Panel estimators can accommodate for (time-invariant) country heterogeneity, but usually within-country variation in the electoral rule is either insufficient to obtain accurate estimates, or so concentrated in certain period (e.g., the 1990s) to be exposed to time-specific confounding factors. Among the estimators employed in the

[4]See also Persson and Tabellini (1999) and Persson, Tabellini, and Trebbi (2003).

macro tests, only IV can claim to disclose causal effects. This claim, however, relies on the plausibility of untestable exclusion restrictions, which are not always compelling (see Acemoglu, 2005). Furthermore, even assuming that macro tests disclose causal effects, it is still doubtful whether they are actually testing the hypotheses H1 and H2. Most macro studies implicitly assume that the impact on politicians' equilibrium behavior is the only link in the chain of causation from the electoral system to country-level outcomes. Suppose, on the contrary, that the electoral system influenced aggregate outcomes not only through the effect on politicians' behavior, but also through an effect on the number of parties and the government structure (e.g., see Persson, Roland, and Tabellini, 2007). In this case, macro studies, far from testing H1 and H2, would estimate the joint impact of the *direct* and *indirect* effects of the electoral rule on aggregate outcomes.

3 The Italian Two-Tier Electoral System

The electoral rules for the Italian Parliament have changed frequently over time. Up to the legislative term XI (1992–1994), members of parliament were elected under an open-list proportional system with large districts (32 for the House of Representatives; 21 for the Senate). Starting with the legislative term XII (1994–1996) and up to the XIV (2001–2006), members of parliament were instead elected with a two-tier system (25% proportional and 75% majoritarian).[5] Electoral rules changed again with the legislative term XV (2006–2008), switching to a closed-list proportional system with 27 districts in the House and 20 in the Senate. In every legislative term, the total number of seats has remained unchanged at 945, of which 630 are in the House of Representatives and 315 in the Senate. The switch in 1994 from the proportional to the mixed-member rule was accompanied by major political changes, including the breakdown of the existing party system that followed judicial scandals for corruption charges involving the leaderships of all government parties. As a result, the 1994 elections featured new parties competing under the mixed electoral system, which favored the emergence of competition between two multi-party coalitions: center-right (which won the general election in 1994 and 2001) versus center-left (which won in 1996). The switch back to proportional representation in 2006 was instead decided by the (center-right) government coalition in the last year of the term.

[5]Triggered by the increasing diffusion of two-tier electoral systems worldwide, political scientists have recently turned their attention to this hybrid system. Lancaster and Patterson (1990) find that German majoritarian representatives quote targeted projects as important for their reelection more often than proportional representatives. Stratmann and Baur (2002) find that German majoritarian representatives are more likely to be assigned to "district-type" than to "party-type" committees. Kunicova and Remington (2008) find that majoritarian members of the Russian State Duma, when voting over the federal budget, show less party loyalty than their proportional colleagues.

We use data for the three legislative terms with two-tier elections (1994–96, 1996–2001, 2001–06). In particular, we focus on the House of Representatives, because only in this branch of parliament were legislators actually elected under two separate systems, with voters receiving two ballots on election day: one to cast a vote for a candidate in their single-member district, and another to cast a vote for a party list in their larger proportional district. 75% of House members were elected with plurality voting in 475 single-member districts, while 25% were elected from closed party lists in 26 multiple-member districts (2 to 12 seats per district) under proportional representation. On the contrary, in the Senate, voters received only one ballot to cast their vote for a candidate in a single-member district, and the best losers in the 232 majoritarian districts were assigned to the remaining 83 seats according to the proportional rule. Therefore, only for the House were the two electoral systems perceived as distinct by voters. Indeed, Ferrara (2004a) shows that—in terms of electoral outcomes—the majoritarian tier was not contaminated by the proportional tier in the House elections. The two tiers represented separate playing fields, where political actors made different electoral promises and were then called to answer for them. Unlike proportional politicians elected with open lists and preference votes, Italian representatives in the proportional tier relied more on party loyalty than personal reputation to be appointed, because party leaders had complete control over their inclusion and their ranking in the party list. Majoritarian politicians had instead to rely on a mix of party loyalty and constituency services to strengthen their nomination chances (see Ferrara, 2004b).

In this paper, we exploit a distinctive institutional feature of the two-tier electoral system for the Italian House of Representatives. Candidates could run for both the majoritarian and proportional tier. If they were elected in both tiers, however, they had to accept the majoritarian seat. If they lost the majoritarian competition, they could still obtain a parliament seat, as long as they were ranked high on their party list. The visibility of each dual candidate was then based on the electoral tier he eventually wound up being elected in: if he had been elected in the majoritarian tier, he was recognized as the representative of the district and asked to provide constituency services; if he had been elected in the proportional tier, he was perceived as one of the members of the national party elite and had a higher probability of receiving government or parliament appointments. Of course, not all candidates were running for both tiers. National leaders were more likely to be dual candidates to increase their probability of election, but usually not in marginal (nonsafe) districts. In the next section, we formally describe how our econometric strategy exploits this Italian institutional feature, that is, dual candidates in close (nonsafe) elections.

4 Econometric Framework

We are interested in estimating the causal effect of the treatment "being elected in a majoritarian system"—as opposed to "being elected in a (closed-list) proportional system"—on two outcomes: geographically targeted bills and politicians' rents. Using a potential-outcome framework, define $Y_i(1)$ as the potential outcome of politician i if elected in the majoritarian tier, and $Y_i(0)$ as the potential outcome of the same politician if elected in the proportional system. The variable T_i defines treatment status: $T_i = 1$ ($T_i = 0$) if i was elected in the majoritarian (proportional) tier. The observed outcome is thus $Y_i = T_i \cdot Y_i(1) + (1 - T_i) \cdot Y_i(0)$. The conditional comparison of the observed outcomes of treated and untreated politicians does not provide an unbiased estimate of the average treatment effect, because politicians with different unobservable characteristics affecting the outcome may self-select into different systems. For instance, individuals with strong local ties may be more likely to run in the majoritarian tier to take advantage of their local popularity. Once elected, they will carry out more geographically targeted policies simply because of their preferences and expertise.

4.1 Identification

The fact that some politicians are candidates in both tiers can be exploited to evaluate the causal effect of the electoral system with RDD. Assume that candidates in the House election run for both a majoritarian and a proportional seat; that is, they are all dual candidates. Voters decide who is assigned to the majoritarian tier, as a politician who wins in a single-member district must accept that seat. Treatment assignment can thus be specified as: $T_i = 1[MV_i \geq 0]$, where MV_i is the margin of victory in the single-member district and $1[.]$ the indicator function. The margin of victory is defined as the difference between the vote share of i and the vote share of the next-best candidate: if i won, MV_i measures his distance from the candidate who scored second; if i lost, MV_i measures the distance from the candidate who scored first. This assignment rule is an example of *sharp* RDD, as treatment assignment has a sharp discontinuity at the threshold $MV_i = 0$.

Define U_i as all unobservable individual characteristics (e.g., political skills) affecting $Y_i(1)$, $Y_i(0)$, MV_i, and the observed individual characteristics X_i at the same time. Following Lee (2008), we constraint the relationship between U_i and MV_i to meet two conditions.

Assumption 1 *Define $F(MV|U_i = u)$ as the cumulative distribution function of MV_i conditional on U_i and, for each u in the support of U_i, assume that:*

a. $0 < F(0|U_i = u) < 1$*;*

b. $F(MV|U_i = u)$ *is continuously differentiable in MV at $MV = 0$.*

Assumption 1 states that politicians can affect their electoral outcome, but their (positive or negative) margin of victory includes some random element, so that their probability of winning in the majoritarian district is never equal to 0 or 1 (condition a). Furthermore, for each politician the probabilities of winning or losing the majoritarian race by a narrow margin are the same (condition b).[6] In other words, electoral outcomes depend on both predictable elements and random chance (such as heavy rain on election day), which is then crucial only for close races. Furthermore, even if it is plausible that political parties identify close races in advance and exert extra effort to win them, this is true for all parties; as a result, political competition prevents each party from sorting above the threshold.

Lee (2008) shows that, under Assumption 1, the average treatment effect at the threshold can be identified as:

$$ATE_{rdd} \equiv E(Y_i(1) - Y_i(0)|MV_i = 0) = \lim_{\epsilon \downarrow 0} E(Y_i|MV_i = \epsilon) - \lim_{\epsilon \uparrow 0} E(Y_i|MV_i = \epsilon). \quad (1)$$

Note that ATE_{rdd} is a local effect, which cannot be extrapolated to the whole population without additional homogeneity assumptions. As usual in RDD, the gain in internal validity is associated with a loss in external validity. Yet this local effect, defined for close electoral races only, has first-order theoretical relevance in our case. As a matter of fact, Persson and Tabellini (1999) identify political competition in swing districts exactly as the driving force behind the effect of the electoral rule on targeted policies and politicians' rents.

We are aware, however, that in close races the treatment of the electoral system may interact with the safeness of the parliament seat. Galasso and Nannicini (2009) show that the degree of contestability of single-member districts is positively associated with both ex-ante measures of politicians' quality and ex-post effort in parliamentary activity.[7] Our econometric strategy can effectively control for ex-ante selection, as long as winners and losers in close races share the same observable and unobservable characteristics under the RDD assumptions, but the resulting estimates end up comparing two peculiar variants of the majoritarian and proportional electoral rule. Specifically, because of the RDD setup, we focus on a majoritarian system with a high degree of political competition; because of the Italian institutions, we focus on a proportional system with both closed lists and centralized party control over the allocation of political candidates into districts.

Furthermore, like in most evaluation studies and in the spirit of Rubin's (1974) potential-outcome framework, we are assuming that the Stable Unit Treatment Value Assumption (SUTVA) holds. In other words, the interpretation of ATE_{rdd} as a causal effect rests on the

[6] These conditions are equivalent to the standard RDD assumption that potential outcomes must not show any discontinuity at the threshold (see Hahn, Todd, and Van der Klaauw, 2001).

[7] See also Persico, Rodriguez-Pueblita, and Silverman (2009). In Section 6.5, we further discuss this issue and implement robustness checks assessing the sensitivity of our results to the degree of seat safeness.

assumption that the potential outcomes of every politician are unaffected by the treatment assigned to other politicians. This should hold in our setting, as the two tiers of the electoral system were indeed perceived as entirely separate playing fields by voters and politicians. In Section 6.5, however, we present empirical evidence supporting the SUTVA plausibility.

Not all politicians in our sample are dual candidates, though. Because of a data restriction, we cannot implement our evaluation strategy on dual candidates only. As a matter of fact, we can identify *proportional* dual candidates—that is, those proportional representatives who also ran, and lost, in a single-member district—but we are not able to identify *majoritarian* dual candidates. This gives rise to a treatment assignment slightly different from the mechanism specified above: if $MV_i < 0$, we have either $T_i = 0$ (if i was a dual candidate) or $T_i = .$ (if i was only a majoritarian candidate). This problem can be addressed thanks to an additional aspect of candidates selection. National leaders tend to be dual candidates, but they also get safe districts where the race is lopsided in favor of their party (see also Ferrara, 2004b). We indeed observe that national leaders are overrepresented in safe districts: their presence nearly doubles in districts where their political party won by more than 10 percentage points in the last election (39% versus 19%); and their presence doubles in districts where their party previously won (26% versus 13%). The remaining dual candidacies are allocated to runners in marginal districts as a compensation device or "parachute". Because there are not enough dual candidacies to secure all marginal runners, however, some of them do not receive any parachute, even if they are very similar to those who obtain it. We can thus state the following assumption for close (nonsafe) districts.

Assumption 2 *In a small left-neighborhood of the threshold, dual candidates are a representative sample of all candidates in single-member districts, that is:*

$$\lim_{\epsilon \uparrow 0} E(U_i | MV_i = \epsilon, T_i = .) = \lim_{\epsilon \uparrow 0} E(U_i | MV_i = \epsilon, T_i = 0).$$

Under Assumption 1 and Assumption 2, in a sample made up of all representatives elected in the majoritarian tier ($MV_i \geq 0$) and of those representatives elected in the proportional tier who were also dual candidates ($MV_i < 0$), equation (1) can be used to estimate the causal effect of the electoral rule. We are aware that Assumption 2 is not innocuous, but its plausibility can be assessed with a large set of testing procedures. Indeed, it is straightforward to show that Assumptions 1 and 2 are jointly verified if (and only if) politicians' observable and unobservable characteristics are balanced around the threshold. This means that we can apply the same array of tests commonly used in the RDD literature to assess the validity of our evaluation strategy. First, the pretreatment characteristics X_i should not display any discontinuity at the threshold (balance tests). Second, the estimated ATE_{rdd} should be insensitive to the introduction of covariates (balance tests of relevant covariates). Third—

as pretreatment outcomes are partly available—the implementation of an RDD on these additional data should produce a zero ATE_{rdd} (falsification tests). Fourth, the outcome should display no discontinuities at fake thresholds different from $MV_i = 0$ (placebo tests).

4.2 Estimation

Various estimation methods have been proposed to implement equation (1), which is basically a problem of estimating the boundary points of two regression functions. We first apply a split polynomial approximation, which uses the whole sample and chooses a flexible specification to fit the relationship between Y_i and MV_i on either side of the threshold. The estimated discontinuity at the threshold is the treatment effect. Specifically, we estimate

$$Y_i = \alpha + \tau T_i + (\delta_1 MV_i + ... + \delta_p MV_i^p) + (\beta_1 T_i \cdot MV_i + ... + \beta_p T_i \cdot MV_i^p) + \eta_i \quad (2)$$

using OLS. We cluster standard errors at the individual level, because the same politician may be observed in different terms. The coefficient τ identifies ATE_{rdd}.

To assess the robustness of the baseline estimates, we also apply a local linear regression by restricting the estimation to a compact support and fitting linear functions to the observations within a distance h on either side of the threshold. In other words, we restrict the sample to the interval $MV_i \in [-h, +h]$ and estimate

$$Y_i = \alpha + \tau T_i + \delta MV_i + \beta T_i \cdot MV_i + \eta_i \quad (3)$$

using OLS. We select the bandwidth h using cross-validation methods.[8]

5 The Data

5.1 Data sources

We use data on all members of the Italian House of Representatives from 1994 to 2006 (terms XII, XIII, and XIV), which is the period when a two-tier electoral system was in place (see Section 3). The dataset contains the following information at the individual level: demographic characteristics (age, gender, place of residence, education); self-declared

[8] In particular, as proposed by Ludwig and Miller (2007), the cross-validation method we implement consists in choosing h so as to minimize the loss function: $CV_Y(h) = \frac{1}{N}\sum_{i=1}^{N}(Y_i - \hat{\mu}_h(MV_i))^2$, where the predictions $\hat{\mu}_h(MV_i)$ are retrieved as follows. For every MV_i to the left (right) of the threshold, we predict its value as if it were at the boundary of the estimation, using only observations in the interval $[MV_i - h, MV_i]$ ($[MV_i, MV_i + h]$). Following Imbens and Lemieux (2008), we calculate the loss function for a subsample of politicians, discarding 50% of the observations on either side of the threshold $MV_i = 0$.

previous job; parliament appointments (president, vice president, and secretary either of the parliament or of a legislative committee); government appointments (minister, vice minister); party affiliation and experience (member of the party directive board at the local, regional, and national level); local government experience (mayor, city councillor, president of a region, etc.); system of election, district, and vote share; detailed information on bill sponsorship; number of (electronic) parliament votes missed without any legitimate reason.[9]

To test the hypotheses H1 and H2 derived in Section 2.1, we use two outcomes: (1) the fraction of bills targeted to the region of election over the total number of bills presented as main sponsor; (2) the fraction of (electronic) parliament votes missed without any legitimate reason. To control for the possible change of reelection incentives during the term, we construct measures averaged over the term, in the first year, and in the last year.

Whether or not a bill was targeted at the region of election is computed using the official classification (*TESEO*), which consists of 9,602 geographical places (single entities, like a museum, included). For each bill, the Congressional Library (*Biblioteca della Camera dei Deputati*) reports each administrative level that was interested by the bill. Depending on their geographical size (all single-member districts have the same population, but can have different topographical dimensions), electoral districts can overlap with one city, one province, or more. We therefore classified the bill as "targeted" if at least one of the listed geographical places was in the same region (*Regione*) of the district of election. We decided to use the region as geographical reference both to minimize measurement error and to maintain comparability between the legislative activity of majoritarian and proportional politicians, given that the size of the electoral districts varies between the two systems, with proportional districts being larger than majoritarian districts, but never larger than a region. Furthermore, we decided to use the fraction, instead of the number of targeted bills, to control for the different levels of intensity in bill sponsorship between majoritarian and proportional representatives. The share of bills tailored to the district of election can be seen as a proxy of targeted redistribution, because of the resources moved by the bills themselves or by assuming that the hierarchy of interests shown by politicians in their bill sponsorship is unchanged in other activities (for example, bargaining for funds with the Treasury).

To assess the sensitivity of our results to the above choices on the definition of targeted activities and on the region as geographical reference, we also constructed two alternative measures based on bill sponsorship. First, we calculated the fraction of "general interest" bills, that is, bills targeted neither at any geographical administrative unit nor any single entity (wherever located). Second, we introduced a narrower definition of "targeted" bills,

[9]The sources we used to collect the data include: the Annals of the Italian Parliament (*La Navicella*) for demographic and professional information; the online archive of bills for the legislative activity; and the Press Office of the Italian Parliament (*Ufficio Stampa*) for data on individual attendance.

that is, bills tailored to sub-regional administrative units or entities within the region of election. Results for these alternative measures of bill sponsorship are reported in Appendix I and discussed below together with our baseline empirical results.[10]

The use of the absenteeism rate as an additional outcome rests on the idea that shirking is a type of rent. As shown by Gagliarducci, Nannicini, and Naticchioni (2010), the absenteeism rate is positively correlated with the amount of politicians' outside income, supporting the view that shirking allows the cultivation of private interests. Yet, absences are a broader measure of rents than outside income. This is because they embrace not only the time used to attend outside economic activities, but also any other personal interest. Absences do not refer to any committee's activity but only to electronic parliament votes, and cases of non-attendance because of parliament missions or cabinet meetings are not counted.[11]

5.2 Preliminary evidence

After dropping observations containing at least one missing value for some of the relevant variables (outcomes, running variable, and observable covariates), we end up with a sample of 1,699 observations, of whom 1,305 were elected in the majoritarian tier and 394 in the proportional tier.[12] Table 1 provides descriptive statistics for this sample, comparing majoritarian (i.e., treated) and proportional (i.e., untreated) politicians. As expected, these two groups display different characteristics, suggesting that self-selection in the choice of the electoral system is at work: females and national politicians are more likely to be elected in the proportional tier. Available proxies for local attachment, such as "local government" (previous institutional experience at the region, province, or town level) and "different residence" (province of residence different from the district of election), are also not balanced, majoritarian politicians being on average more attached to their local constituency.

Descriptive statistics on bills sponsorship and absences are reported in Table 2. Majoritarian representatives, on average, present more bills than their proportional colleagues, although the difference is not significantly different from zero. The fraction of targeted bills is significantly higher for majoritarian (11.3%) than for proportional politicians (7.3%). Conversely, the absenteeism rate is significantly higher for proportional (36.6%) than for majoritarian politicians (30.9%). With respect to the timing, a large fraction of bills (almost one half) is presented in the first year of the term. The difference between majoritarian and

[10] In Appendix II, we report some relevant examples of individual bills classified as targeted, general interest, and narrowly targeted.

[11] Note that electronic votes account for about 90% of total parliament votes (and for almost the totality of votes on final bill's approval), the rest being held with hand counting.

[12] The 1,699 observations of the final sample correspond to 1,218 politicians, of whom 871 were always elected in the majoritarian tier, 237 were always elected in the proportional tier, and 110 switched from one tier to the other across the three legislative terms.

proportional politicians is the same in the first year of the term, but it is no longer significant in the last year. For absences, the difference between majoritarian and proportional politicians is even greater if we consider the first-year measure.

Although this descriptive evidence is far from detecting causal effects of the electoral rule, the gross effects captured by the mean differences (e.g., +0.040 for the share of targeted bills and –0.057 for the absenteeism rate over the term) also have a meaningful interpretation: they describe the joint impact of the causal relationship, selection on observables, and unobservable self-selection. In Table 3—which reports some preliminary evidence on the association between the treatment and the outcomes of interest—we also control for selection on observables by using OLS with a full set of covariates (panel A). The impact of being elected in the majoritarian system is positive on targeted bills (+0.027, about +37% with respect to the average of proportional politicians) and negative on the absenteeism rate (-0.044, about -12%), and both effects are statistically significant at the 1% level.

In Table 3, we also use the time variation provided by the two electoral reforms in 1994 and 2006 (see Section 3) to implement a diff-in-diff specification. The first diff-in-diff exercise in panel B compares the (fully proportional) terms X-XI with the (mixed-treatment) terms XII-XIII-XIV; the second diff-in-diff exercise in panel C compares the (mixed-treatment) terms XII-XIII-XIV with the (fully proportional) term XV. We report both estimations without (column I) and with individual fixed effects (column II). Overall, the diff-in-diff estimates are in the same ballpark of the OLS estimates, detecting a positive (negative) association between the majoritarian system and targeted bills (absences).

The diff-in-diff results should be interpreted with caution, however. The estimation without fixed effects, exactly like OLS, does not account for unobservable self-selection into different systems, that is, it is affected by *composition bias*. The estimation with fixed effects could in principle accommodate for composition bias, but it draws inference from a very self-selected sample of politicians who survived the electoral reform.[13] For instance, if only a few national politicians—always secure about their reelection—survived the political turmoil associated with the electoral reform, the effect of the system of election captured by the fixed-effect specification would be biased. In the next section, we thus present our RDD estimates, which control for composition bias and isolate the causal effect of the majoritarian system in a more appropriate quasi-experimental framework.

Finally, Table 4 describes the distribution of the margin of victory MV_i, which is the assignment variable in our RDD exercise. Note that this table provides evidence supporting Assumption 2 of the identification strategy. In fact, if proportional dual candidates were

[13]Note that the two reforms in 1994 and 2006 were accompanied by other major political changes. In particular, the shift from the XI (1992–94) to the XII (1994–96) term was marked by the breakdown of the old party system and the emergence of new political actors. Members of parliament who survived these political transitions are therefore highly self-selected.

representative of all candidates who lost in single-member districts, we would observe very similar numbers in the two sides of the distribution of MV_i, positive for majoritarian politicians and negative for proportional politicians. Table 4 shows that the two sides of MV_i are very close to one another, especially in small neighborhoods of the threshold level $MV_i = 0$, where they are almost identical. The difference between the absolute value of MV_i for majoritarian and proportional politicians is never significantly different from zero, excluding the case of the large interval [–20,20]. Robust statistical evidence supporting Assumption 2, however, can only come from the RDD validity tests discussed in Section 6.3.

6 RDD Empirical Results

6.1 Estimated effects of the electoral rule

The RDD estimates on the fraction of geographically targeted bills reported in Table 5 provide a way of testing H1, that is, whether politicians in the majoritarian system carry out more pork-barrel activities than politicians in the proportional system because of the differential incentives set by the system of election. The final RDD sample consists of all majoritarian representatives (1,305) and proportional dual candidates (141), for a total of 1,446 observations.[14] We present results on bill sponsorship over the entire term, as well as in the first or last year only. Estimation (I) uses a (third-order) split polynomial approximation; below we present robustness checks on the (third-order) functional form assumption. Estimation (II) uses a local linear regression with optimal bandwidth.[15] Both estimations are implemented without and with covariates.

Being elected in the majoritarian system entails an increase in the share of geographically targeted bills of 8.4 percentage points, that is, it more than doubles the share of targeted bills with respect to the predicted value of 6.3 for proportional representatives at the threshold (6.7 for proportional representatives in the 5%-neighborhood). The two estimates without and with control variables are almost identical, supporting the assumption that relevant covariates (i.e., covariates affecting the outcome) do not display any discontinuity at the threshold. This provides first evidence on the validity of our evaluation framework. The effect is even greater for bills presented in the first year, when most politicians expect to run for reelection in the same tier: at that time, being elected in the majoritarian system increases the share of targeted bills by 10.1 percentage points, that is, almost twice as much than the average amount of 5.4 for proportional politicians. As expected, in the last year

[14] Observations in the estimation sample are lower than in the initial sample because—as discussed in Section 4—the RDD setup discards proportional representatives who did not run in the majoritarian tier.

[15] The bandwidth h is selected using the cross-validation method discussed in Section 4.2, and it is equal to 15 for the term average, 12 for the first-year measure, and 15 for the last-year measure.

of the term, when changed reelection incentives for a subsample of politicians may interfere with the incentives set by the system of election, the treatment effect on targeted bills is lower (split polynomial) or even insignificant (local linear regression).[16] For the term average and the first-year measure, all the estimated effects of the majoritarian system reported in Table 5 are statistically significant at either the 1% or 5% level.[17]

The RDD estimates on the absenteeism rate, reported in Table 6, provide a way of testing H2, that is, whether politicians in the majoritarian system extract lower rents than politicians in the proportional system. Here, we carry out the same estimations of Table 5, but we make use of a slightly different sample because of missing values in the outcome variable. In particular, yearly information on absences are not available for the XII term, so that we must restrict the estimations with the first-year and last-year measures to the terms XIII and XIV. According to the baseline estimate with (third-order) polynomial approximation, being elected in the majoritarian system entails a fall in the absenteeism rate equal to 14.9 percentage points, that is, a fall of more than 30% with respect to the predicted value of 47.7 for proportional representatives at the threshold (42.4 for proportional representatives in the 5%-neighborhood). Taking into account available covariates, the effect is slightly lower, equal to a fall of 10.9 percentage points. The two estimates, however, are not statistically different from one another. The point estimates obtained with local linear regression and optimal bandwidth are also very similar to the previous ones.[18]

Also for the absenteeism rate, the impact of being elected in the majoritarian system is much stronger on first-year political behaviors: in this case, it decreases absences by 26.8 (split polynomial) or 21.8 (local linear regression) percentage points, that is, by about 47% or 38% with respect to the predicted value of 57 for proportional representatives at the threshold (even more with respect to 45.3 for proportional representatives in the 5%-neighborhood). Interestingly, the impact of the tier of election on the absenteeism rate in the last year is never statistically different from zero, meaning that at the end of the term

[16]Information on the system of (eventual) future reelection provide descriptive evidence on the timing and impact of altered incentives. First, we find that the attenuation in the effect of the electoral rule is no longer there for those politicians who run for reelection in the same tier. In this (self-selected) subsample, the effect of the majoritarian system on first-year targeted bills is 0.153 (s.e., 0.071), while the effect on last-year targeted bills is 0.134 (s.e., 0.053). Second, we find that incentives are only slightly altered by the system of reelection, but in the direction the theory would predict. Conditional on the tier of election, the difference in targeted bills between politicians reelected in the majoritarian and in the proportional tier is never statistically significant over the term or in the first year. In the last year, instead, majoritarian politicians reelected in the majoritarian tier have a higher share of targeted bills (6.4) than majoritarian politicians reelected in the proportional tier (1.6); difference significant at the 10% level.

[17]In Appendix I, as a robustness check, we report RDD estimates for the two alternative measures of bill sponsorship described in Section 5: the share of general interest (Table 12) and narrowly targeted bills (Table 13). The empirical results are qualitatively identical to those for targeted bills, the only difference (if any) being that the estimated effects for the last-year measure are less robust or not statistically significant.

[18]The bandwidth h is selected using the cross-validation method discussed in Section 4.2, and it is equal to 14 for the term average, 11 for the first-year measure, and 15 for the last-year measure.

reelection incentives may interact with rent extraction.[19] On the contrary, for the term average and the first-year measure, all of the estimated effects of the majoritarian system reported in Table 6 are significant at either the 1% or 5% level.[20]

We provide a graphical representation of the effect of the electoral system on the outcome variables at the threshold in Figure 1, which reports the estimated (third-order) split polynomial and its 95% confidence interval, together with the scatter of the observations averaged over 5-point intervals of the margin of victory. Politicians below zero were elected in the proportional tier, while politicians above zero were elected in the majoritarian tier. Both for bill sponsorship and the absenteeism rate, the jump at zero is clearly visible from the scatter and from the estimated polynomial in the case of term averages and first-year measures, while the jump is small or insignificant for last-year measures. The width of the confidence interval shows that the small sample size of (dual) proportional politicians makes the estimation of the polynomial less accurate on the left of the zero threshold.

The shape of the estimated polynomial away from zero also conveys interesting information. Indeed, the higher the distance from the threshold, the lower the share of targeted bills (especially on the right of zero). This is consistent with our proposed interpretation: politicians in close electoral races strongly commit themselves to their constituency, while politicians who safely won do not target their activity at the local district. Political competition is thus an additional factor influencing politicians' behavior in the majoritarian system. With respect to the absenteeism rate, instead, the behavior of majoritarian representatives does not change much when we get farther from the threshold (i.e., the curve is flat on the right of zero). On the contrary, proportional representatives make even more absences if they lost by a large margin in the majoritarian tier.

6.2 Interpretation

The above RDD estimates strongly support the theoretical hypotheses H1 and H2, showing—with respect to the latter—that the accountability effect of the majoritarian system dominates the entry-barrier effect. In other words, the quasi-experimental evidence reported

[19] Like in the case of bill sponsorship, information on the system of (eventual) future reelection provide descriptive evidence on the timing and impact of altered incentives. In this case, we find that the attenuation in the effect of the electoral rule is significantly reduced for those politicians who run for reelection in the same tier. In this (self-selected) subsample, the effect of the majoritarian system on first-year absences is -0.124 (s.e., 0.116), while the effect on last-year absences is -0.052 (s.e., 0.098), with none of the two being statistically significant. Furthermore, we find that majoritarian politicians reelected in the majoritarian tier have a lower absenteeism rate over the term (33.6) than majoritarian politicians reelected in the proportional tier (42.3); difference significant at the 5% level. The differences for the first-year and last-year absenteeism rate, instead, are never statistically significant.

[20] As a robustness check, we estimated the same RDD specifications in Table 6 using the GLM estimator proposed by Papke and Wooldridge (1996), because the absenteeism rate is bounded between 0 and 1. The estimation results are almost identical (available upon request).

in the previous subsection shows that the differential incentives set by majoritarian versus proportional elections increase geographically targeted policies and reduce rent-seeking. Our interpretation of these results rests on the idea that the incentives set by the system of election can be plausibly divided into two categories: (a) "reelection" incentives; (b) "commitment" incentives. We now discuss them in turn.

First of all, note that also reelection incentives are associated with the system of election because, once politicians have been assigned to a given electoral tier, they tend to persist there. Amongst the members of parliament reelected in the next term, the probability of being reelected in the majoritarian tier is about 87% for majoritarian politicians versus 57% for proportional politicians (difference significant at the 1% level). In races decided by a margin lower than 15 percentage points, the same difference is 84% versus 64% (significant at the 5% level) and, despite the smaller sample size, in races decided by a margin lower than 5 points, the difference (84% versus 67%) is significant at the 10% level. To sum up, both in all and close races, politicians tend to run for reelection in the tier where they have been previously appointed.

Furthermore, the disclosed *timing* of political behaviors over the legislative term confirms the presence of differential reelection concerns associated with the system of election. Here, we make the extra assumption that, even for those (few) politicians who do not persist in the same electoral tier, the subjective probability of running for reelection in the same tier is greater at the beginning of the term, because this is the standard upshot, and switches between tiers are only decided a few months before the next election. Accordingly, we find that the impact of the electoral rule on first-year outcomes is much stronger than the impact on last-year outcomes. Of course, the above assumption is untestable *per se*, but we find it very credible in the Italian context, and it is supported by the robustness check showing that the attenuation in the impact of the electoral rule is no longer there (or greatly reduced) for those politicians who run for reelection in the same tier.

Reelection concerns, however, are not the only differential incentives set by the system of election. Also congressmen who do not expect to run for reelection do not stop sponsoring bills or attending vote sessions (this is indeed what we observe in our data). And they may do so for different reasons, such as the willingness to leave a positive legacy, the intrinsic motivations for delivering their electoral promises, or the need to build a good reputation for future careers in the political or private sector. These commitment devices are strengthened by the monitoring of voters, who expect politicians elected under different electoral tiers to deliver different policies according to their promises. This is indeed what the theoretical literature discussed in Section 2 implicitly assumes.

6.3 Validity tests

The validity of our evaluation strategy can be assessed with different testing procedures. Remember that in the previous section we have already verified that the inclusion of pre-treatment covariates does not influence point estimates, which are never significantly different from those without covariates. This is like a balance test of relevant covariates: only if pre-treatment variables with a strong effect on the outcome variable were not balanced in the neighborhood of the threshold would the estimate with covariates diverge from the baseline estimate. Here, we perform three additional types of validity tests. First, we check whether all of the covariates X_i are balanced in the neighborhood of the threshold. Second, we run falsification tests by using pretreatment information. Some politicians, in fact, were in office before the electoral reform of 1994, when all members of parliament were elected under proportional representation. As we observe their bills and absences in this pretreatment period (specifically, in term X, from 1987 to 1992, and in term XI, from 1992 to 1994), we repeat the RDD estimation on these data, where we should find no significant effects. Third, we implement placebo tests by estimating the treatment effect at fake thresholds.

Table 7 reports the balance tests, that is, estimations with a (third-order) split polynomial approximation using each covariate as dependent variable. We perform these tests both in the sample used for bill sponsorship (*RDD-I*) and in the sample used for absences (*RDD-II*). Only the self-employment dummy and the freshman dummy (in the sample for targeted bills, but not in the sample for absenteeism) show a significant discontinuity. The other covariates are perfectly balanced around the threshold.[21] Note that, among these covariates, two variables can be plausibly considered as correlated with the main unobservable element we cannot control for, that is, the attachment of different politicians to their local constituency. The fact that these two variables—i.e., different residence and local government experience—are balanced indirectly supports the plausibility of the RDD hypothesis on unobservables.

In Table 8, we implement estimations with a (third-order) split polynomial approximation using as dependent variable both the share of geographically targeted bills and the absenteeism rate in the pretreatment period. In particular, we regress the two outcome measures in term X, term XI, and both terms X and XI on the dummy of the electoral system and a third-order polynomial on either side of zero.[22] To apply these falsification tests, we restrict our sample to those members observed at least once in the pretreatment terms (X and XI) and once in the treatment terms (XII, XIII, and XIV). If some politicians elected in the majoritarian tier during the legislative terms XII, XIII, or XIV had some unobserv-

[21]In Appendix I, we report another type of balance tests, that is, estimations with local linear regression using each covariate as dependent variable (see Table 14). The results of these tests are qualitatively identical to those with a split polynomial approximation.

[22]Yearly information on bill sponsorship and absences are not available for the X and XI terms, so that we cannot implement the falsification tests on first-year and last-year political behaviors.

able feature affecting the outcome variables, they would have presented more geographically targeted bills and made fewer absences also in the pretreatment period. The results of the falsification tests show that in the pretreatment period the impact of the (future) electoral system is never statistically different from zero, as one should expect. This directly supports the RDD hypothesis that also unobservables are balanced around the threshold.

Finally, in Table 9, we run placebo tests at fake discontinuity points. For both the share of targeted bills and the absenteeism rate (over the term, in the first year, and in the last year, respectively), we estimate the jump at the median on either side of $MV_i = 0$ with a (third-order) split polynomial approximation. The jumps at these fake thresholds are never significantly different from zero, although we are aware that the rejection of the null hypothesis may be due to the scarce number of observations, at least on the left of the true threshold (that is, for dual proportional politicians).

6.4 Further robustness checks

In Table 10 and Table 11, we report an array of robustness checks aimed at assessing the sensitivity of the baseline results in various directions. In both tables, panel A shows the results with a split polynomial approximation (estimation I) and local linear regression (estimation II) after dropping from the sample those proportional politicians who were ranked first in their party list and therefore secure to obtain a parliament seat. This is meant to assess how large is the interaction of seat safeness with our treatment of interest, at least below the threshold. Above the threshold, as discussed in Section 4, the local nature of the RDD estimand ends up delivering the effect of the majoritarian system in close races. In other words, all majoritarian politicians just above the threshold faced a tough electoral competition. On the contrary, for proportional politicians, the degree of seat safeness can vary, according to their rank in the party list. By dropping those who were secure to be appointed, we therefore assess the sensitivity of our results in this dimension. Both Table 10 for bill sponsorship and Table 11 for absences show that the baseline estimates are robust to the sample restriction to marginal (unsecure) proportional politicians.

Panel B in both Table 10 and Table 11 is aimed at making treated and control observations more comparable. According to the RDD assumptions, politicians just below and above the threshold should be perfectly comparable both in terms of observable and unobservable characteristics. However, if we further restrict the sample to those majoritarian politicians who defeated the (dual) proportional politicians, comparability improves along the whole MV_i spectrum, because on the right of $MV_i = 0$ we only observe winning candidates matched with the losing candidates on the left of $MV_i = 0$. The power of this robustness check, however, is considerably reduced by the small sample size. We could find a matched majoritarian politician only for 126 out of the 141 dual proportional politicians, because of missing values for the remaining 15. Therefore, we perform the estimation with a split polynomial approximation both for all observations (estimation I) and for the 126 dual proportional politicians with an observable match (estimation II). Table 10 shows that the baseline results on bill sponsorship disappear in this exercise, while Table 11 confirms the robustness of the baseline estimates on the absenteeism rate.

Finally, in panel C of both tables, we assess the sensitivity of the split (third-order) polynomial estimates to the choice of the degree of the polynomial. Estimation (I) uses a second-order polynomial, while estimation (II) uses a fourth-order polynomial. Table 10 shows that all estimates on bill sponsorship are robust. In Table 11, we observe that the estimates on first-year and last-year absences are also robust, while the term average is robust with the second-order but not with the fourth-order polynomial.

6.5 Discussion

The above validity tests and robustness checks tend to confirm the assumptions of our identification strategy, but—as discussed in Section 4—the interpretation of our estimates as causal effects also rests on untestable assumptions that deserve further discussion. First, we have assumed that there are no spillovers, that is, politicians are unaffected by the existence of an alternative treatment status. Second, we have assumed that the SUTVA holds, that is, politicians are unaffected by the treatment received by their colleagues. We now discuss the plausibility of these assumptions.

The no-spillover assumption could be violated if, for instance, a representative elected in the proportional tier sought reelection in the majoritarian tier, responding to the incentives of the second system instead of the first. Or vice versa. Our data show that possible spillovers are limited, because over the three general elections in our sample (1994, 1996, and 2001), only 9% of House members (14% of our observations) switched from one tier to the other. Furthermore, in Section 6.1, we presented evidence that—conditional on being reelected—persistence in the same tier is high both in all and close races, although a large share of the (small) fraction of reelected proportional politicians end up in the majoritarian tier. As our evaluation strategy partly relies on dual candidates, it is also important to note that the persistence in the status of dual candidate is low: only 27% of all dual candidates received this parachute more than once, and this number decreases to 17% if we disregard national leaders. If a politician had the chance to be a dual candidate, he could not safely expect to have this opportunity again, unless he was a national leader. In Section 6.1, to control for reelection motives, we also presented results on first-year political behaviors, under the assumption that spillovers (if any) should start biting at the end of the legislative term, when reelection decisions are taken. We have shown that the results are indeed stronger for first-year outcomes. All of this evidence supports the idea that spillovers are limited.

Even if some spillovers were actually at work, however, our estimates would result in a lower bound of the true causal effect, unless the size of spillovers were implausibly high. Assume that $0 \leq \alpha \leq 1$ is the belief of a majoritarian candidate to run for reelection in the proportional tier, while $0 \leq \beta \leq 1$ is the belief of a proportional candidate to rerun in the majoritarian tier. In other words, α (β) is the spillover of the proportional (majoritarian) tier on the majoritarian (proportional) tier. In this case, the true potential outcomes linked to the incentives of the majoritarian versus proportional system—$Y^*(1)$, $Y^*(0)$—differ from the potential outcomes—$Y(1)$, $Y(0)$—of our framework:

$$Y(1) = (1-\alpha)Y^*(1) + \alpha Y^*(0) \tag{4}$$

$$Y(0) = (1-\beta)Y^*(0) + \beta Y^*(1). \tag{5}$$

It is simple to show that, as long as $\alpha + \beta < 1$, the ATE_{rdd} that we estimate is a lower bound of the true ATE^*_{rdd}, that is: $ATE_{rdd} = (1-\alpha-\beta)ATE^*_{rdd}$. If, for example, majoritarian and proportional representatives are similar with respect to their probability of changing tier, this means that the fraction of those who want to switch must never be greater than 50% in each tier. In the case in which there is a greater fraction of proportional representatives running for reelection in the majoritarian tier, instead, we need to assume that this is compensated by a lower fraction of majoritarian representatives rerunning in the proportional tier. Based on the evidence provided above, we can safely rule out that $\alpha + \beta > 1$. This increases the power of our tests of H1 and H2, as long as we detect a significant effect of the electoral system on the outcome variables. Indeed, the lower-bound argument is indirectly confirmed by the fact that our estimates for the first-year outcome measures are always higher than those for the term averages.

Finally, our evaluation framework rests on the SUTVA. This assumption could be violated if representatives of the same political party helped each other: for example, proportional politicians could take care of the national duties of their colleagues in order to let them focus on targeted policies. In other words, the mixed-member setup could alter the technology available to politicians elected under a universal system (fully majoritarian or proportional) in order to build consensus. We believe that this is not plausible in the Italian institutional framework: because of the reasons discussed in Section 3, the two tiers of the electoral system for the House of Representatives were separate playing fields, and political agents aimed at winning in both tiers. Once in office, representatives owed their visibility to the tier where they had been elected, being in charge of the promises they had made there. Furthermore, evidence from the pretreatment period—when all members of parliament were elected under proportional representation—supports our view on the SUTVA plausibility. If this assumption were violated, politicians elected in the proportional tier of a mixed system should carry out less targeted bills and make more absences than politicians elected in a fully proportional system. But this is not the case. In the XI legislative term, immediately before the electoral reform that introduced the two-tier system, representatives had an average share of targeted bills equal to 6% (as opposed to 7% of proportional representatives in the two-tier system) and an average absenteeism rate equal to 40% (as opposed to 37%). These average values for the two groups are never significantly different from each other.

7 Conclusion

In this paper, we have provided micro evidence about the effect of the majoritarian electoral system, as opposed to proportional representation, on the equilibrium behaviors of elected officials. We believe that the use of individual-level data is particularly important here, as it allows us to identify the exact chain of causation that links the electoral rule to policy outcomes in representative democracies. Furthermore, the particular features of Italian two-tier elections have allowed us to implement an RDD and disclose the causal effects of the electoral rule. We have shown that the majoritarian system increases the amount of geographically targeted bills and reduces representatives' shirking in a way that is both statistically significant and large in magnitude.

The normative implications of our empirical findings are mixed. The majoritarian system increases the possibility of monitoring politicians and their accountability (Persson and Tabellini, 2000), improving their commitment to parliamentary work. At the same time, the majoritarian system stimulates the adoption of locally targeted (pork-barrel) projects, which may end up being overprovided at the expense of general interest policies (Lizzeri and Persico, 2001). In this light, our findings call for a new effort by scholars in electoral engineering to devise a system that could both reduce the incentive for pork-barreling and keep politicians accountable for their actions.

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Tables and Figures

Table 1: Pre-treatment characteristics by electoral rule

	Proportional	Majoritarian	Difference
Male	0.756	0.914	-0.158***
Age	48.566	48.248	0.318
Years of schooling	16.102	15.976	0.125
Different residence	0.094	0.033	0.061***
Local government experience	0.431	0.564	-0.133***
National politician	0.274	0.207	0.067***
Freshman	0.728	0.776	-0.048**
Incumbent	0.365	0.400	-0.034
Switching	0.299	0.101	0.198***
Center-right	0.383	0.405	-0.021
Parliament appointment	0.089	0.074	0.015
Clerk	0.051	0.051	0.000
Lawyer	0.119	0.135	-0.016
Executive	0.145	0.137	0.008
Politician	0.201	0.162	0.039*
Entrepreneur	0.086	0.100	-0.013
Teacher	0.109	0.090	0.019
Self employed	0.071	0.111	-0.040**
Physician	0.053	0.090	-0.036**
No. of observations	*394*	*1,305*	

Notes. Terms XII, XIII, and XIV; ministers excluded. All variables are dummies, except *Age* and *Schooling* (expressed in years). *Different Residence* stands for living in a province different from the province of election. *Local Government Experience* stands for previous experience at the local level (e.g., mayor of a city or president of a regional government). *Freshman* means that the previous parliamentary experience is lower than a full term. *Incumbent* refers to politicians elected in the same district in the previous term. *Switching* stands for politicians elected in different tiers of the electoral system across the three legislative terms. *Parliament appointment* indicates that the politician has previously held some special Parliament appointment (e.g., president or vice-president of the Parliament or of a legislative committee). Job dummies refer to the (self-declared) preelection occupation. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 2: **Bill sponsorship and absenteeism rate by electoral rule**

	Proportional	Majoritarian	Difference
		Bill sponsorship	
No. of bills	8.025	8.489	-0.464
No. of bills (first year)	3.754	4.038	-0.284
No. of bills (last year)	1.320	1.404	-0.084
Share of targeted bills	0.073	0.113	-0.040***
Share of targeted bills (first year)	0.054	0.090	-0.035***
Share of targeted bills (last year)	0.042	0.058	-0.016
No. of observations	*394*	*1,305*	
		Absenteeism rate	
Absenteeism rate	0.366	0.309	0.057***
No. of observations	*368*	*1,260*	
Absenteeism rate (first year)	0.351	0.273	0.078***
Absenteeism rate (last year)	0.295	0.238	0.056***
No. of observations	*234*	*828*	

Notes. Terms XII, XIII, and XIV; ministers excluded. *No. of bills* is the total number of bills presented as main sponsor (over the term, in the first year, and in the last year, respectively). *Share of targeted bills* is the fraction of bills targeted at the region of election (over the term, in the first year, and in the last year, respectively). *Absenteeism Rate* is the percentage of electronic votes missed without any legitimate reason (over the term, in the first year, and in the last year, respectively). Yearly observations on the absenteeism rate are only available for the XIII and XIV terms. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 3: OLS and diff-in-diff estimates

	(I) Without FE		(II) With FE	
	Effect of majoritarian system	No. of obs.	Effect of majoritarian system	No. of obs.
	A. OLS (terms XII-XIII-XIV)			
Share of targeted bills	0.027*** [0.010]	1,699		
Absenteeism rate	-0.044*** [0.012]	1,628		
	B. Diff-in-diff (terms X-XI vs. XII-XIII-XIV)			
Share of targeted bills	0.028** [0.012]	2,239	0.031* [0.017]	2,239
Absenteeism rate	-0.053*** [0.016]	2,151	-0.046*** [0.017]	2,151
	C. Diff-in-diff (terms XII-XIII-XIV vs. XV)			
Share of targeted bills	0.027** [0.012]	2,272	0.017 [0.015]	2,272
Absenteeism rate	-0.090*** [0.015]	2,188	-0.035** [0.016]	2,188

Notes. Ministers excluded. Dependent variable: *Share of targeted bills*, i.e., the fraction of bills targeted at the region of election (over the term); *Absenteeism rate*, i.e., the percentage of electronic votes missed without any legitimate reason (over the term). All estimations include term dummies, an indicator for whether the politician had never been elected in the majoritarian tier, as well as all the variables listed in Table 1 as controls (excluding in Panel C the variables *Different Residence* and *Incumbent*, not available for the term XV). Estimation (I) does not include individual fixed effects, while estimation (II) includes individual fixed effects. Standard errors clustered at the individual level are in brackets. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 4: Distribution of the margin of victory

	Proportional		Majoritarian		All sample	
	Obs.	Mean	Obs.	Mean	Obs.	Mean
MV_i	141	-12.75	1,305	13.56	1,446	10.99
$MV_i \in [-30, 30]$	125	-9.40	1,175	10.71	1,300	8.77
$MV_i \in [-20, 20]$	107	-6.54	987	8.02	1,094	6.59
$MV_i \in [-10, 10]$	83	-4.39	646	4.59	729	3.57
$MV_i \in [-5, 5]$	53	-2.64	362	2.33	415	1.70
$MV_i \in [-1, 1]$	10	-0.47	92	0.49	102	0.40

Notes. Terms XII, XIII, and XIV; ministers excluded. *Margin of Victory* (MV_i) is defined as the difference between the vote share of the (winning or losing) politician in the majoritarian district and the vote share of the next-best candidate (in percentage points).

Table 5: Bill sponsorship, RDD estimates

	(I) All sample			(II) $MV_i \in [-h, h]$		
	Effect of majoritarian system	No. of treated obs.	No. of control obs.	Effect of majoritarian system	No. of treated obs.	No. of control obs.
Without covariates						
Share of targeted bills	0.084** [0.034]	1,305	141	0.069** [0.029]	845	99
Share of targeted bills (first year)	0.101*** [0.035]	1,305	141	0.112*** [0.037]	733	92
Share of targeted bills (last year)	0.065** [0.028]	1,305	141	0.040 [0.025]	845	99
With covariates						
Share of targeted bills	0.083*** [0.031]	1,305	141	0.064** [0.027]	845	99
Share of targeted bills (first year)	0.094*** [0.033]	1,305	141	0.107*** [0.034]	733	92
Share of targeted bills (last year)	0.056* [0.029]	1,305	141	0.027 [0.028]	845	99

Notes. Terms XII, XIII, and XIV; ministers excluded. Dependent variable: *Share of targeted bills*, i.e., the fraction of bills targeted at the region of election (over the term, in the first year, and in the last year, respectively). Estimation (I): split polynomial approximation (*p=3*). Estimation (II): local linear regression, where h is the optimal bandwidth selected using cross-validation methods; $h = 15$ for the term average, $h = 12$ for the first year, $h = 15$ for the last year. The estimations with covariates include the variables listed in Table 1 as additional controls. Standard errors clustered at the individual level are in brackets. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 6: Absenteeism rate, RDD estimates

	(I) All sample			(II) $MV_i \in [-h, h]$		
	Effect of majoritarian system	No. of treated obs.	No. of control obs.	Effect of majoritarian system	No. of treated obs.	No. of control obs.
			Without covariates			
Absenteeism rate	-0.149***	1,260	134	-0.128***	773	89
	[0.051]			[0.043]		
Absenteeism rate (first year)	-0.268***	828	81	-0.218***	493	60
	[0.073]			[0.063]		
Absenteeism rate (last year)	-0.063	828	81	-0.065	595	68
	[0.063]			[0.051]		
			With covariates			
Absenteeism rate	-0.109**	1,260	134	-0.102**	773	89
	[0.047]			[0.040]		
Absenteeism rate (first year)	-0.206***	828	81	-0.189***	493	60
	[0.058]			[0.051]		
Absenteeism rate (last year)	-0.045	828	81	-0.060	595	68
	[0.058]			[0.048]		

Notes. Terms XII, XIII, and XIV; ministers excluded. Dependent variable: *Absenteeism rate*, i.e., the percentage of electronic votes missed without any legitimate reason (over the term, in the first year, and in the last year, respectively). Yearly observations on the absenteeism rate are only available for the XIII and XIV terms. Estimation (I): split polynomial approximation (p=*3*). Estimation (II): local linear regression, where h is the optimal bandwidth selected using cross-validation methods; $h = 14$ for the term average, $h = 11$ for the first year, $h = 15$ for the last year. The estimations with covariates include the variables listed in Table 1 as additional controls. Standard errors clustered at the individual level are in brackets. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 7: Balance tests of pre-treatment characteristics

	RDD-I sample			RDD-II sample		
	Jump at the threshold	No. of treated obs.	No. of control obs.	Jump at the threshold	No. of treated obs.	No. of control obs.
Male	-0.087	1,305	141	-0.087	1,260	134
	[0.061]			[0.068]		
Age	0.877	1,305	141	0.544	1,260	134
	[2.035]			[2.120]		
Years of schooling	-0.161	1,305	141	-0.200	1,260	134
	[0.467]			[0.511]		
Different residence	-0.054	1,305	141	0.018	1,260	134
	[0.062]			[0.032]		
Local govt. experience	0.068	1,305	141	0.091	1,260	134
	[0.117]			[0.122]		
National politician	0.072	1,305	141	0.050	1,260	134
	[0.088]			[0.097]		
Freshman	0.209**	1,305	141	0.172	1,260	134
	[0.098]			[0.105]		
Incumbent	-0.150	1,305	141	-0.195	1,260	134
	[0.115]			[0.121]		
Center-right	-0.080	1,305	141	-0.130	1,260	134
	[0.114]			[0.118]		
Parl. appointment	-0.036	1,305	141	-0.013	1,260	134
	[0.064]			[0.062]		
Clerk	-0.003	1,305	141	0.010	1,260	134
	[0.031]			[0.030]		
Lawyer	-0.039	1,305	141	-0.018	1,260	134
	[0.079]			[0.075]		
Executive	-0.146	1,305	141	-0.139	1,260	134
	[0.097]			[0.104]		
Politician	-0.005	1,305	141	0.003	1,260	134
	[0.076]			[0.078]		
Entrepreneur	-0.103	1,305	141	-0.136	1,260	134
	[0.076]			[0.085]		
Teacher	0.070	1,305	141	0.060	1,260	134
	[0.066]			[0.074]		
Self employed	0.179***	1,305	141	0.172***	1,260	134
	[0.039]			[0.044]		
Physician	-0.017	1,305	141	-0.038	1,260	134
	[0.081]			[0.081]		

Notes. Terms XII, XIII, and XIV; ministers excluded. Dependent variables: pre-treatment characteristics (see Table 1 for a description of each variable). Estimation method: split polynomial approximation ($p=3$). *RDD-I sample* is the sample used in the RDD estimation for the share of targeted bills (see Table 5). *RDD-II sample* is the sample used in the RDD estimation for the absenteeism rate (see Table 6). Standard errors clustered at the individual level are in brackets. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 8: Falsification tests, bill sponsorship and absenteeism rate

	Effect of majoritarian system	No. of treated obs.	No. of control obs.
Share of targeted bills X	-0.001	57	18
	[0.003]		
Share of targeted bills XI	0.048	192	47
	[0.122]		
Share of targeted bills X-XI	0.038	207	49
	[0.092]		
Absenteeism rate X	-0.140	54	18
	[0.104]		
Absenteeism rate XI	0.029	187	46
	[0.095]		
Absenteeism rate X-XI	0.032	203	48
	[0.079]		

Notes. Terms XII, XIII, and XIV; ministers excluded. Subsamples of politicians elected also in the X and/or XI legislative terms. Dependent variables: (pre-treatment) share of bills targeted at the region of election in the X term, XI term, or both, respectively; (pre-treatment) absenteeism rate in the X term, XI term, or both, respectively. Estimation method: split polynomial approximation (p=*3*). Standard errors clustered at the individual level are in brackets. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 9: Placebo tests, bill sponsorship and absenteeism rate

	Jump at the fake threshold	No. of treated obs.	No. of control obs.
Share of targeted bills:			
- 50^{th} left	0.068	72	69
	[0.090]		
- 50^{th} right	0.023	654	651
	[0.038]		
Share of targeted (first year):			
- 50^{th} left	0.074	72	69
	[0.078]		
- 50^{th} right	0.057	654	651
	[0.041]		
Share of targeted (last year):			
- 50^{th} left	0.128	72	69
	[0.172]		
- 50^{th} right	-0.039	654	651
	[0.045]		
Absenteeism rate:			
- 50^{th} left	0.072	67	67
	[0.111]		
- 50^{th} right	0.058	630	630
	[0.038]		
Absenteeism rate (first year):			
- 50^{th} left	-0.012	41	40
	[0.168]		
- 50^{th} right	0.002	418	410
	[0.061]		
Absenteeism rate (last year):			
- 50^{th} left	-0.133	41	40
	[0.160]		
- 50^{th} right	-0.020	418	410
	[0.054]		

Notes. Terms XII, XIII, and XIV; ministers excluded. Dependent variables: share of bills targeted at the region of election (over the term, in the first year, and in the last year, respectively); absenteeism rate (over the term, in the first year, and in the last year, respectively). Estimation method: split polynomial approximation (*p=3*) at fake thresholds, i.e., the median of the margin of victory on either side of the true threshold ($MV_i = 0$). Standard errors clustered at the individual level are in brackets. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 10: Bill sponsorship, robustness checks

	Estimation (I)			Estimation (II)		
	Effect of majoritarian system	No. of treated obs.	No. of control obs.	Effect of majoritarian system	No. of treated obs.	No. of control obs.
A. Marginal proportional politicians only						
Share of targeted bills	0.081**	1,305	80	0.068**	845	63
	[0.034]			[0.030]		
Share of targeted bills (first year)	0.108***	1,305	80	0.111***	733	59
	[0.026]			[0.026]		
Share of targeted bills (last year)	0.066*	1,305	80	0.055*	845	63
	[0.038]			[0.032]		
B. Matched majoritarian politicians only						
Share of targeted bills	0.012	126	141	-0.005	126	126
	[0.050]			[0.053]		
Share of targeted bills (first year)	0.041	126	141	0.031	126	126
	[0.054]			[0.057]		
Share of targeted bills (last year)	0.094	126	141	0.087	126	126
	[0.067]			[0.068]		
C. Second-order and fourth-order polynomial						
Share of targeted bills	0.076***	1,305	141	0.070*	1,305	141
	[0.025]			[0.040]		
Share of targeted bills (first year)	0.070***	1,305	141	0.089**	1,305	141
	[0.025]			[0.041]		
Share of targeted bills (last year)	0.043*	1,305	141	0.081**	1,305	141
	[0.024]			[0.035]		

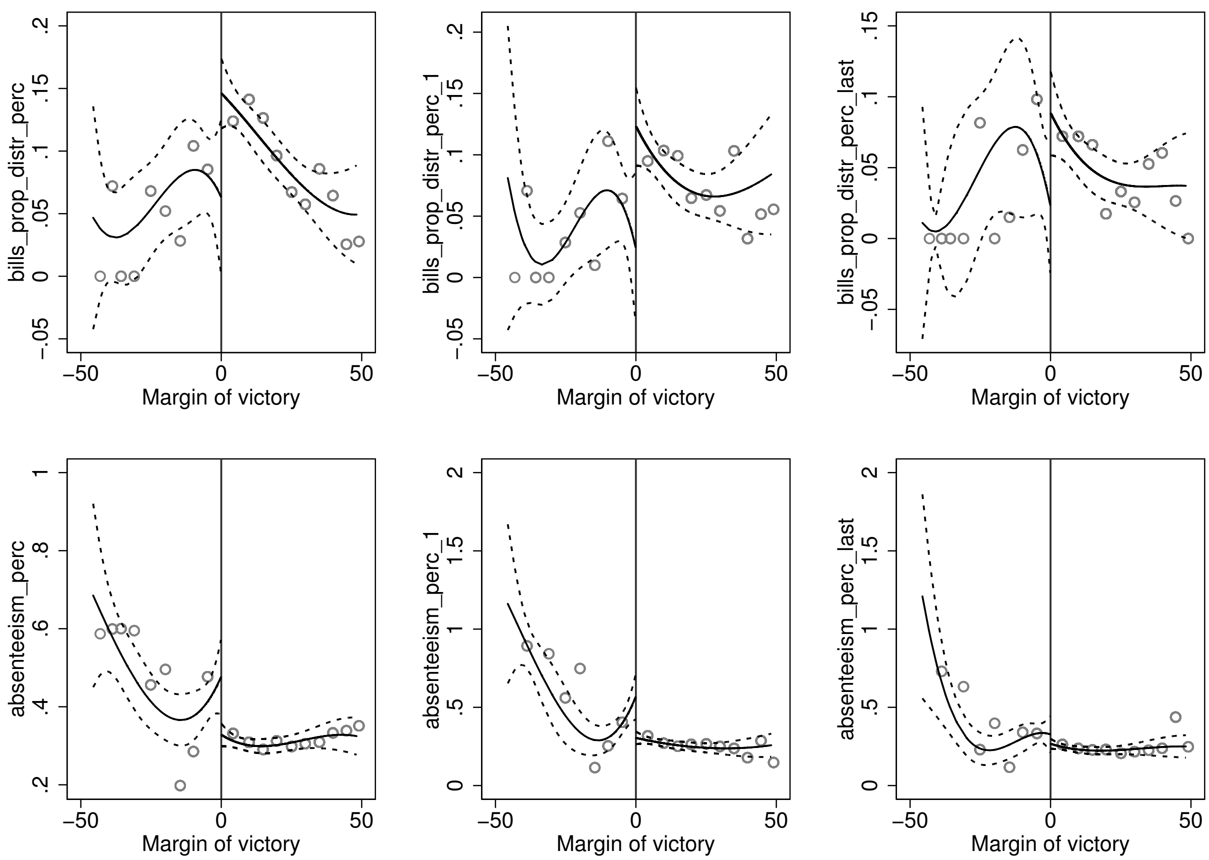
Notes. Terms XII, XIII, and XIV; ministers excluded. Dependent variable: *Share of targeted bills*, i.e., the fraction of bills targeted at the region of election (over the term, in the first year, and in the last year, respectively). Panel A: sample restricted to proportional politicians who did not rank first in their (closed) party list. Panel B: sample restricted to majoritarian politicians who defeated a dual candidate. Panel C: all sample. Estimation (I) in A and B: split polynomial approximation (p=*3*). Estimation (II) in A: local linear regression, where h is the optimal bandwidth selected using cross-validation methods; $h = 15$ for the term average, $h = 12$ for the first year, $h = 15$ for the last year. Estimation (II) in B: split polynomial approximation (p=*3*); further sample restriction to matched proportional politicians. Estimation (I) in C: split polynomial approximation (p=*2*). Estimation (II) in C: split polynomial approximation (p=*4*). Standard errors clustered at the individual level are in brackets. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Table 11: Absenteeism rate, robustness checks

	Estimation (I)			Estimation (II)		
	Effect of majoritarian system	No. of treated obs.	No. of control obs.	Effect of majoritarian system	No. of treated obs.	No. of control obs.
A. Marginal proportional politicians only						
Absenteeism rate	-0.107**	1,260	76	-0.084*	773	58
	[0.054]			[0.047]		
Absenteeism rate	-0.198**	828	51	-0.186**	493	40
	[0.097]			[0.072]		
Absenteeism rate (last year)	0.033	828	51	0.019	595	44
	[0.083]			[0.058]		
B. Matched majoritarian politicians only						
Absenteeism rate	-0.189***	121	134	-0.176**	121	121
	[0.067]			[0.069]		
Absenteeism rate (first year)	-0.310***	74	81	-0.288**	74	74
	[0.114]			[0.116]		
Absenteeism rate (last year)	-0.050	74	81	-0.039	74	74
	[0.088]			[0.091]		
C. Second-order and fourth-order polynomial						
Absenteeism rate	-0.153***	1,260	134	-0.073	1,260	134
	[0.040]			[0.065]		
Absenteeism rate (first year)	-0.239***	828	81	-0.156*	828	81
	[0.057]			[0.093]		
Absenteeism rate (last year)	-0.132***	828	81	-0.066	828	81
	[0.049]			[0.080]		

Notes. Terms XII, XIII, and XIV; ministers excluded. Dependent variable: *Absenteeism rate*, i.e., the percentage of electronic votes missed without any legitimate reason (over the term, in the first year, and in the last year, respectively). Yearly observations on the absenteeism rate are only available for the XIII and XIV terms. Panel A: sample restricted to proportional politicians who did not rank first in their (closed) party list. Panel B: sample restricted to majoritarian politicians who defeated a dual candidate. Panel C: all sample. Estimation (I) in A and B: split polynomial approximation (p=3). Estimation (II) in A: local linear regression, where h is the optimal bandwidth selected using cross-validation methods; $h = 14$ for the term average, $h = 11$ for the first year, $h = 15$ for the last year. Estimation (II) in B: split polynomial approximation (p=3); further sample restriction to matched proportional politicians. Estimation (I) in C: split polynomial approximation (p=2). Estimation (II) in C: split polynomial approximation (p=4). Standard errors clustered at the individual level are in brackets. Significance at the 10% level is represented by *, at the 5% level by **, and at the 1% level by ***.

Figure 1: RDD graphical analysis, bill sponsorship and absenteeism rate



Notes. Terms XII, XIII, and XIV; ministers excluded. The solid line is a split third-order polynomial in the margin of victory, fitted separately on each side of the zero threshold. The dashed lines are the 95% confidence interval of the polynomial. Scatter points are averaged over 5-unit intervals.