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ABSTRACT

Electoral Rules and Politicians' Behavior: A Micro Test*

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 for the mixed-member Italian House of Representatives, which allow us to sidestep the identification problems of previous studies based on country-level data. In particular, we address the nonrandom selection into different electoral systems by exploiting a distinctive feature of the Italian two-tier elections from 1994 to 2006: candidates could run for both the majoritarian and the 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 main results confirm theoretical predictions, as majoritarian representatives put forward a higher proportion of bills targeted at local areas and show lower absenteeism rates than their proportional colleagues.

JEL Classification: C20, D72, D78, P16

Keywords: electoral rule, politicians, targeted redistribution, rent-seeking, regression discontinuity design, treatment effect

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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 these different electoral systems on political outcomes, such as the number of political parties or government structure. Economists have recently contributed to the subject by developing theoretical models that show how the electoral system influences politicians' equilibrium behaviors and, ultimately, public policies in democratic countries. First of all, 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 the proportional system, is shown to be associated with more targeted redistribution and less nationwide public goods (Persson and Tabellini, 1999; Lizzeri and Persico, 2001; Milesi-Ferretti, Perotti, and Rostagno, 2002).

From a second perspective, the electoral rule also decides how effectively voters can keep elected officials accountable for their actions. Assuming that politicians can extract rents—such as shirking to cultivate private affairs or corruption—from holding an office, the interests of voters and politicians diverge. On this point, theoretical predictions about the impact of the electoral system 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 would also reduce rent extraction (Myerson, 1993).

In this paper, to the best of our knowledge, we provide the first micro test of the causal effect of the electoral system on the behavior of elected officials.[1] We use unique 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 confounding 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.

The 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 exploited to control for endogeneity by applying a Regression Discontinuity Design (RDD). Specifically, it had two tiers: 75% of members were elected in single-member districts, and 25% were elected with proportional representation. Candidates could run for both the majoritarian and proportional tier; 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

[1] Frechette, Kagel, and Morelli (2007) use experimental data (i.e., not field observational data) to investigate the trade-off of potential legislators between the provision of public goods and targeted redistribution, and their focus is on legislative bargaining rather than electoral competition.

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

outcomes, the selection into the majoritarian tier mimics random assignment for those elected officials who won or lost by a narrow margin in single-member districts.[3]

We use this quasi-experimental framework to estimate the causal effect of the treatment "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. We can focus on *being* elected, rather than *seeking* reelection, under a certain electoral rule, because persistence is very high: in almost 90% of the cases, if a representative was first elected under a certain rule, he then ran for reelection in the same system. As a measure of local activities, we use the share of bills targeted to the region of election over the total number of bills presented in a legislative term. As a proxy for politicians' rents, we use instead the absenteeism rate, that is, the percentage of parliamentary votes missed without any legitimate reason. After controlling for nonrandom selection into the two tiers of the electoral system, we find that being elected in the majoritarian system more than doubles the fraction of targeted bills. At the same time, it decreases the absenteeism rate by about one third.

Our empirical results show that majoritarian elections are *strongly* associated with a greater amount of targeted policies, such as pork-barrel projects favoring local constituencies: projects that—because of a common pool problem—may wind up being overprovided at the expense of more universal policies. An electoral reform leading to proportional representation, however, would produce other side effects, such as rent extraction by elected officials who are less accountable to their voters. 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.

2 Theoretical Framework and Related Literature

In this section, we review the theoretical and empirical studies that our contribution builds on. In particular, Section 2.1 discusses the models that link either targeted redistribution

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

or politicians' rent extraction to the electoral system. There, we derive the theoretical hypotheses that our evaluation exercise tests. Section 2.2 critically reviews the empirical studies that estimate the effects of the electoral rule using country-level data.

2.1 Theory

Various voting models in political economics have studied the impact of electoral rules on the provision of broad versus targeted policies. Persson and Tabellini (1999) compare electoral systems within a probabilistic-voting model, where two office-seeking candidates (or parties) make binding electoral promises. Voters are divided into three groups (or districts). In proportional elections, a candidate wins if he gets more than 50% of the total votes in a nationwide district. In majoritarian elections, each district is decided according to the winner-take-all rule, and a candidate wins the general election if he wins in at least two out of the three districts. It follows that in the proportional system political competition focuses on swing voters in the population at large (that is, across districts), while in the majoritarian system competition focuses on swing districts only. In the latter case, the interests of safe districts are not internalized in the equilibrium platform presented by the candidates. This mechanism leads to more geographically targeted policies at the expense of public good provision in the majoritarian system.

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

Milesi-Ferretti, Perotti, and Rostagno (2002) use a different rationale to link the electoral system to targeted activities. They build a citizen-candidate model with no commitment to preelection platforms; once appointed, politicians implement their bliss point.

Citizens are heterogeneous along two dimensions: they belong to three social groups and three districts. In the first stage, three representatives are elected either under the majoritarian system or proportional representation. In the second stage, the government is randomly formed. 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. The opposite holds under proportional representation, where the median voter prefers a representative biased toward socially targeted policies.

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.*

The political trade-off between policies with *diffuse* versus *concentrated* benefits—the latter also being known as pork-barrel projects—has been studied from a slightly different perspective in political science and Public Choice. On the political demand side, concentrated benefits overshadow diffuse costs, because of the lower transaction costs that smaller groups face when they want to get organized and support their interests (Olson, 1973). On the supply side, politicians have an incentive to provide policies whose benefits are targeted to specific groups, as legislators seeking reelection favor projects for which they can easily claim credit (Mayhew, 1974). Lancaster (1986) builds a bridge between the pork-barrel literature and the electoral rule, predicting an inverse relationship between district size and pork-barrel activity. This is because of a free-rider problem among politicians: if you are the only one representing a district, it is easier to get political credit for locally targeted policies. Note that this literature—unlike the previous models—uses the implicit assumption that politicians who belong to the same party are imperfect substitutes for each other from the voters' point of view. Furthermore, the

focus is on postelection rather than preelection politics, that is, the relevant treatment coincides with seeking reelection, rather than being elected, under a certain rule.

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, Ch. 9) 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. Elected officials have an incentive both to exert effort and to avoid corruption. On the contrary, under proportional representation with closed party lists, reelection depends on the individual rank in the list decided by the party leadership and on the overall performance of the list, which is only loosely linked to individual behaviors. This creates a free-rider problem among candidates in the same list. As a result, the higher the proportion of representatives elected with individual-candidate ballot, like in majoritarian elections, the lower politicians' rents.

Unlike the prediction about targeted activities (H1), however, the relationship between the electoral system 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.[4] Political parties differ along two dimensions: ideology (left versus right) and honesty (honest versus dishonest). Some

[4]See also Myerson (1999).

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. As a matter of fact, one of the possible equilibria is the self-fulfilling prophecy that a close race between two dishonest candidates takes place. If voters believe that their first-best choice (that is, the honest party whose ideology they share) has no chance of winning, they rationally vote for the dishonest party with the same ideology. This cannot happen under proportional representation, where voters are free to pick their first-best choice, because by doing so they will increase honesty without affecting the balance between left and right in the parliament. Equilibrium rents are therefore lower than in the majoritarian case. The size of the electoral district—which in turn affects the degree of entry barriers for well-behaving politicians—is the crucial feature lying behind this result.

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).[5] These results are robust to the use of different estimation strategies (OLS, matching estimators, parametric selection corrections, 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

[5]This extensive empirical analysis on the electoral rule builds on previous work by the authors, such as Persson and Tabellini (1999) and Persson, Tabellini, and Trebbi (2003).

(intended here as a measure of policies targeted to geographic constituencies) and transfers (as a measure of policies targeted to social constituencies). As for the electoral system, they build a new measure of proportionality, inversely related to the share of votes that guarantees a parliament seat in a district of average size, and they use it in association with other measures commonly used in political science. 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.

The above studies find weak support for the hypothesis that the majoritarian system increases targeted policies, and strong support for the hypothesis that it reduces politicians' rents. Macro tests, however, come with two main drawbacks: a data problem and, more relevantly, an identification problem. On the data side, results may be sensitive both to the classification of electoral systems across countries and to the way the variables specified in the theoretical models are approximated in the data.[6] On the identification side, although macro tests detect important correlations that are consistent with the theory, it is doubtful that they are able to disclose causal effects. OLS and matching rely on the conditional independence assumption, that is, on the exogeneity of the electoral rule. However the electoral rule, like any other political institution, is an equilibrium outcome determined by numerous factors which cannot be fully controlled. Panel estimators can accommodate for (time-invariant) country-specific confounding factors, 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 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.[7]

[6]For instance, Milesi-Ferretti, Perotti, and Rostagno (2002) use government expenditure for public goods as a measure of geographically targeted redistribution, while Persson and Tabellini (1999) use the same type of expenditure as a measure of broad redistribution.

[7]Among the instruments used by Persson and Tabellini (2003) in the IV setup, there are: three electoral-reform dating variables, under the assumption that the adoption of a new electoral system follows waves at the international level; language variables, to control for colonial and cultural influences; and latitude. See Acemoglu (2005) for a detailed criticism of this set of instruments.

Furthermore, even if we assume that macro tests are able to disclose the causal effects of the electoral rule, it is not beyond question that they are actually testing the theoretical hypotheses H1 and H2. Most macro studies implicitly assume that the effects of the electoral system on politicians' equilibrium behaviors are the only link in the chain of causation from the electoral system to country-level outcomes. Suppose, on the contrary, that the electoral system affects aggregate outcomes not only through the effect on politicians' behaviors but also through an effect on the number of parties and the government structure (single-party versus multiple-party), as suggested by Persson, Roland, and Tabellini (2007). In this case, macro tests, far from testing H1 and H2, would estimate the joint impact of the *direct* and *indirect* effects of the electoral rule on aggregate variables.

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, with 3 to 54 seats per district depending on the population; 21 for the Senate, with 1 to 47 seats per district). 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).[8] Electoral rules changed again with the legislative term XV (2006–present), switching to a closed-list proportional system with 27 districts in the House (3 to 44 seats per district) and 20 in the Senate (1 to 47 seats per district). 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.

[8]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 proportional members, suggesting that they may trump their partisan ties to cultivate their regional constituencies. All of these studies use subjective measures of targeted activities, disregard self-selection issues, and do not look at rents.

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% of members were elected from closed party lists in 26 multiple-member districts (2 to 12 seats per district). 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 proportional representation. Therefore, only for the House of Representatives were the two electoral systems perceived as distinct by voters. These two tiers represented distinct playing fields, where political actors made different electoral promises and were then called to answer for them in a separate way.

In particular, for the purpose of this paper, we exploit a distinctive institutional feature of the two-tier electoral system for the House. 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 official leader of his political coalition in the district; if he had been elected in the proportional tier, he was perceived as one of the members of the national party elite. And media coverage reinforced citizens' perception in these directions. Of course, not all candidates were running for both tiers. National leaders were more likely to be dual candidates, but usually not in marginal (nonsafe) districts.

In the next section, we formally describe our evaluation strategy and how it exploits the above institutional framework.

4 Evaluation Framework

We are interested in estimating the causal effect of the treatment "being elected in the majoritarian system"—as opposed to "being elected in the proportional system"—on two sets of outcomes: geographically targeted in-office activities, and politicians' rents.[9] Using Rubin's (1974) potential-outcome framework for causal inference, define $Y_i(1)$ as the potential outcome of politician i in the case he is elected in the majoritarian system, and $Y_i(0)$ as the potential outcome of the same politician in the case he is elected in the proportional system. The variable T_i defines the treatment status of i: $T_i = 1$ if he was elected in the majoritarian tier, and $T_i = 0$ if he was elected in the proportional tier. The observed outcome is then written as: $Y_i = T_i \cdot Y_i(1) + (1 - T_i) \cdot Y_i(0)$.

The simple conditional comparison of the observed outcomes of treated and untreated politicians does not generally provide an unbiased estimate of the average treatment effect of interest, as politicians with different unobservable characteristics affecting the outcome may self-select into different systems. For instance, individuals with strong local ties (such as politicians who served in local governments or businessmen rooted in a specific region) may be more likely to run in the majoritarian tier to take advantage of their local popularity. Once elected, these members of parliament will carry out more geographically targeted policies simply because of their preferences and expertise, and not because of the effect of the electoral rule. The fact that some politicians are candidates in both tiers of Italian House elections, however, can be exploited to implement an RDD and evaluate the causal effect of the electoral system on the outcome variables.[10]

[9] See Section 5 for a precise description of the outcome variables.

[10] See Imbens and Lemieux (2008) and Van der Klaauw (2008) for a survey on RDD. Various empirical studies have exploited the assignment mechanism generated by the margin of victory in single-member plurality elections to estimate a causal effect of interest: Lee, Moretti, and Butler (2004) evaluate whether an exogenous shift in the strength of the US Democratic Party makes both the Democratic and Republican nominees in the next election move to the left of the political spectrum, meaning that voters affect policy formation; Lee (2008) estimates the effect of incumbency on the probability of winning the next election in the US; Hainmueller and Kern (2008) estimate the contamination effect of the electoral outcome in the majoritarian tier on the outcome in the proportional tier in German mixed-member elections.

4.1 Identification

Assume, to begin with, 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; in other words, he cannot opt for the proportional tier in the case he also wins in the majoritarian tier. Treatment assignment can be specified as:

$$T_i = 1[MV_i \geq 0], \tag{1}$$

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. As a result, if $MV_i \geq 0$, i must accept the majoritarian seat ($T_i = 1$), while, if $MV_i < 0$, i is elected in the proportional tier ($T_i = 0$). This assignment rule is an example of *sharp* RDD, as the probability of receiving the treatment has a sharp discontinuity (equal to 1) at the threshold $MV_i = 0$. In what follows, we borrow from Lee (2008) in stating the identification conditions required by an RDD of this type. We then discuss an additional assumption required by our data.

The margin MV_i can be seen as a random variable depending on observable and unobservable individual characteristics, as well as on general occurrences on election day. Define U_i as an unobservable individual characteristic (e.g., political skills) affecting $Y_i(1)$, $Y_i(0)$, MV_i, and the observed individual characteristics X_i at the same time. The relationship between U_i and MV_i is assumed to meet the following 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).[11] In other words, electoral outcomes depend on both predictable elements and random chance, which is then crucial only for close races. For instance, heavy rain on election day may influence turnout and, as a result, the victory of one candidate instead of the other in marginal districts. Furthermore, even if it is plausible that political parties identify close electoral 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:

$$\lim_{\epsilon \uparrow 0} E(U_i | MV_i = \epsilon) = \lim_{\epsilon \downarrow 0} E(U_i | MV_i = \epsilon) \tag{2}$$

$$\lim_{\epsilon \uparrow 0} E(X_i | MV_i = \epsilon) = \lim_{\epsilon \downarrow 0} E(X_i | MV_i = \epsilon). \tag{3}$$

It follows that:

$$E(Y_i(0) | MV_i = 0) = \lim_{\epsilon \uparrow 0} E(Y_i(0) | T_i = 0, MV_i = \epsilon) = \lim_{\epsilon \uparrow 0} E(Y_i | MV_i = \epsilon) \tag{4}$$

$$E(Y_i(1) | MV_i = 0) = \lim_{\epsilon \downarrow 0} E(Y_i(1) | T_i = 1, MV_i = \epsilon) = \lim_{\epsilon \downarrow 0} E(Y_i | MV_i = \epsilon). \tag{5}$$

Therefore, the estimable quantity $[\lim_{\epsilon \downarrow 0} E(Y_i | MV_i = \epsilon) - \lim_{\epsilon \uparrow 0} E(Y_i | MV_i = \epsilon)]$ has the causal interpretation of the average treatment effect at the threshold:

$$ATE_{rdd} \equiv E(Y_i(1) - Y_i(0) | MV_i = 0) = \lim_{\epsilon \downarrow 0} E(Y_i | MV_i = \epsilon) - \lim_{\epsilon \uparrow 0} E(Y_i | MV_i = \epsilon). \tag{6}$$

It should be noted 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. Furthermore,

[11] These conditions are equivalent to the standard assumption in RDD that potential outcomes, as a function of the assignment variable, must not show any discontinuity at the threshold (Hahn, Todd, and Van der Klaauw, 2001), but they are more easily interpretable in the setting of plurality elections.

this local effect, defined for close electoral races only, has first-order theoretical relevance in the present application. 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 rents.

Not all politicians in our sample, however, are dual candidates; some of them ran exclusively for election in the majoritarian tier and others in the proportional tier. Because of a data restriction, we cannot implement our evaluation strategy on dual candidates only. As we do not observe the ranking in party lists, we are not able to identify *majoritarian* dual candidates. We can only identify *proportional* dual candidates, that is, those proportional representatives who also ran, and lost, in a single-member district. This gives rise to a treatment assignment slightly different from the mechanism in equation (1). 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, however, can be addressed thanks to an additional aspect of candidates selection. National leaders—who are not representative of the entire population of politicians—tend to be dual candidates, but they also get safe districts where the race is lopsided in favor of their party. 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%) against districts where it won by a lower margin (19%); and their presence doubles in districts where their party won in the last election (26%) against districts where it lost (13%). The remaining dual candidacies are allocated to runners in marginal districts as a compensation device or "parachute". However, because there are not enough dual candidacies to secure all runners in nonsafe districts (75% of the seats being allocated with the majoritarian system against only 25% with proportional representation), some marginal runners do not receive any parachute, even if they are very similar to those who obtain it. In other words, we can state the following assumption for nonsafe candidates.

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 (6) can be used to estimate the causal effects of interest.

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, if equations (2) and (3) were satisfied, this would imply that Assumption 1 and Assumption 2 are both verified. Of course, equation (2) is untestable by definition, but it can be indirectly assessed in various ways, while equation (3) can be directly tested. And rejecting (3) would cast serious doubts on (2), particularly for those X_i that are likely to be affected by the same unobservables that influence potential outcomes. In other words, we can apply the same array of tests commonly used in the RDD literature to assess the overall 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 also available, the implementation of an RDD on these additional data should produce a zero ATE_{rdd} (falsification tests). Fourth, the assignment variable MV_i should display no discontinuities at fake threshold levels different from $MV_i = 0$ (placebo tests).

If all of these validity tests produced the expected results, the joint plausibility of Assumption 1 and Assumption 2 would be robustly supported by the data.

4.2 Estimation

Various semiparametric and nonparametric estimation methods have been proposed to implement equation (6), which is basically a problem of estimating the boundary points

of two regression functions. We apply two methods: the split polynomial approximation used by Lee, Moretti, and Butler (2004) and Lee (2008), and the local linear regression advocated by Imbens and Lemieux (2008).

The first method 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 the model:

$$Y_i = \alpha + \tau T_i + (\delta_1 MV_i + ... + \delta_p MV_i^p) + (\beta_1 T_i \cdot MV_i + ... + \beta_p T_i \cdot MV_i^p) + \eta_i, \qquad (7)$$

using OLS. Standard inference procedures can be applied. As the same politician may be observed in different legislative terms, we use robust standard errors with cluster correction at the individual level to control for intra-politician correlation in the error term η_i. As MV_i is equal to zero at the threshold, the coefficient τ identifies ATE_{rdd}. Usually, a third-grade polynomial ($p = 3$) is used in the empirical literature.

The above method is attractive for many reasons, although a possible concern is that it may be sensitive to outcome values for observations far away from the threshold (Imbens and Lemieux, 2008). To avoid this, the second method restricts the estimation to a compact support, and fits linear regression functions to the observations within a distance h on either side of the threshold. In other words, we restrict the sample to politicians in the interval $MV_i \in [-h, +h]$ and estimate the model:

$$Y_i = \alpha + \tau T_i + \delta MV_i + \beta T_i \cdot MV_i + \eta_i, \qquad (8)$$

using OLS. The bandwidth h can be selected applying the cross-validation method proposed by Ludwig and Miller (2007) and formalized by Imbens and Lemieux (2008), but the sensitivity of the estimates to the choice of h should be assessed.[12]

[12]This method consists in choosing h so as to minimize the loss function:

$$CV_Y(h) = \frac{1}{N}\sum_{i=1}^{N}(Y_i - \hat{\mu}_h(MV_i))^2, \qquad (9)$$

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 $MV_i = 0$.

4.3 Interpretation

A final remark concerns the use of a two-tier system to estimate the effect of the electoral rule under which politicians are elected. Our evaluation framework rests on the implicit assumption that the two tiers of the Italian electoral system were separate playing fields, and that political agents—parties and candidates—aimed at winning in both tiers, where they responded to the different incentives of political competition under majoritarian versus proportional elections. Once elected, representatives owed their visibility to the tier they were elected in, being in charge of the promises their party had made there.

Whenever the majoritarian and proportional tiers coexist, however, one may argue that there are some spillovers between the two systems; for instance, a representative elected in the proportional tier might seek reelection in the majoritarian tier, responding to the incentives of the second system instead of the first. Our data show that the possible spillovers are limited, as only 9% of House members from 1994 to 2006 switched from one tier to the other. And, even more notably, the fraction of politicians simply *trying* to be reelected in a different tier—a decision that was formalized only a few months before the upcoming election—was equally small: only 10% (17%) of majoritarian (proportional) representatives ran for reelection in the proportional (majoritarian) tier. This means that, in almost 90% of the cases, if a representative was first elected under a certain rule, he then ran for reelection in the same system. 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 get this opportunity again, unless he was a national leader.

Furthermore, even if some spillovers were actually at work, 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 run

for reelection in the majoritarian tier. In other words, α is the possible spillover of the proportional on the majoritarian tier, while β is the spillover of the majoritarian on the proportional tier. In this case, the true potential outcomes linked to the incentives of the majoritarian versus the proportional system—$Y^*(1)$, $Y^*(0)$—differ from the potential outcomes—$Y(1)$, $Y(0)$—of our evaluation framework:

$$Y(1) = (1-\alpha)Y^*(1) + \alpha Y^*(0) \tag{10}$$

$$Y(0) = (1-\beta)Y^*(0) + \beta Y^*(1). \tag{11}$$

As a result, as long as $\alpha + \beta < 1$, the ATE_{rdd} that we estimate is a lower bound of the true ATE^*_{rdd}, that is: $ATE_{rdd} = (1-\alpha-\beta)ATE^*_{rdd}$.

Because of the institutional setting and the descriptive evidence just provided, we can safely rule out the possibility that the attempt of seeking reelection in a different tier is so relevant that $\alpha + \beta > 1$. This would increase the power of our tests of H1 and H2, if we detected a significant effect of the electoral system on the outcome variables.

5 Data

We use data about the members of the Italian House of Representatives from 1994 to 2006, which is the period when a two-tier electoral system was in place (see Section 3).[13] The dataset contains the following individual information: demographic characteristics (age, gender, marital status, number of children, place of birth, place of residence, level and field of education); self-declared previous job (before entering parliament for the first time); number and type of bills as main sponsor (geographic area covered by the bill);[14] absenteeism (the number of electronic votes missed without any legitimate reason); appointments in the parliament (president, vice president, and secretary either of the

[13]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 Italian Parliament Statistical Office for data on individual attendance.

[14]Bills are classified using the TE.SE.O. system (*TEsauro SEnato per l'Organizzazione dei documenti parlamentari*), consisting of 3,668 hierarchical terms (e.g., from "art" to "urban architecture") and 9,602 geographical places (single entities, like a museum, included). For each bill, the Documentation Center of the Italian Parliament reports each region, province or town presenting any affinity with the bill.

parliament or of a legislative committee) and in the government (minister, vice minister); party affiliation and political experience (member of the directive board of the party at the local, regional, and national level); local government experience (mayor, city councillor, president of a region, etc.); system of election, electoral district, and vote share.

After dropping observations containing at least one missing value for some of the relevant variables, 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.[15] Table 1 provides descriptive statistics for this sample, comparing treated (i.e., majoritarian) and untreated (i.e., proportional) 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 the dummies for "local government" (previous institutional experience at the region, province, or town level) and "different residence" (the province of residence different from the district of election), are also not balanced, majoritarian politicians being more attached to their local constituency.

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 during the legislative term; (2) the fraction of parliament votes missed without any legitimate reason over the total number of electronic votes during the legislative term. We use the fraction, instead of the number of targeted bills, to control for the different levels of intensity in bills sponsorship between majoritarian and proportional representatives. The share of bills tailored to specific areas 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 bills is unchanged in other activities (for example, bargaining for funds with the Treasury Minister). The use of the absenteeism rate rests on the idea that shirking is a type of rent. As shown by

[15]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.

Gagliarducci, Nannicini, and Naticchioni (2008), the absenteeism rate is positively correlated with the amount of politicians' outside income, supporting the view that shirking allows the cultivation of private interests. Absences, however, are a more precise measure of rents with respect to outside income. This is because they embrace not only the time used to attend outside economic activities, but also leisure, which is another side of rents.

Descriptive statistics about 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 (11.2%) than for proportional politicians (7.3%). Conversely, the absenteeism rate is significantly higher for proportional (36.6%) than for majoritarian politicians (30.9%). Although this descriptive evidence is far from detecting causal effects of the electoral rule, the gross effects captured by the mean differences (0.039 for the share of targeted bills and –0.057 for the absenteeism rate) also have a meaningful interpretation: they describe the joint impact of the causal relationship, selection on observables, and unobservable self-selection. OLS estimates with a full set of covariates—which control for selection on observables but not for unobservable self-selection—give an effect of 0.037 (standard error, 0.011) on the share of targeted bills and –0.078 (standard error, 0.019) on the absenteeism rate. In the next section, we present the RDD estimates, which isolate the causal effect of the majoritarian electoral system and directly test the theoretical hypotheses H1 and H2.

Table 3 describes the distribution of the margin of victory MV_i, which is the assignment variable in the RDD exercise. This table provides evidence supporting Assumption 2 of the identification strategy. In fact, if proportional dual candidates were 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 3 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.

6 Econometric Results

6.1 Estimated Effects of the Electoral System

We provide a graphical representation of the effect of the electoral system on the outcome variables at the threshold in Figure 1 and Figure 2. Figure 1 reports the running-mean smoothing of the share of locally targeted bills with respect to the RDD assignment variable MV_i.[16] The used bandwidth is equal to one percentage point. The smoothing is performed separately on either side of the threshold to let the possible jump at $MV_i = 0$ show up if it exists. Politicians below zero were elected in the proportional tier, while politicians above zero were elected in the majoritarian tier. Indeed, the jump is clearly visible and positive, meaning that in the neighborhood of the threshold majoritarian representatives present more targeted bills than their proportional colleagues. It is also worth noting that the higher the distance from the threshold, the lower the share of targeted bills (especially on the right of $MV_i = 0$). This is consistent with our interpretation: politicians in close electoral races strongly commit themselves to their constituency, while politicians who are sure to win do not target their activity at the local district.

Figure 2 performs the same running-mean smoothing for the absenteeism rate. The jump at zero is visible but negative in this case, meaning that in the neighborhood of the threshold shirking is greater for proportional politicians. Here, the behaviors of majoritarian representatives do not change much when we get farther from the threshold (i.e., the curve is flat on the right of $MV_i = 0$). On the contrary, proportional representatives make even more absences if they lost by a high margin in the majoritarian tier.

The RDD estimates on the fraction of geographically targeted bills reported in Table 4 provide a way of testing H1, that is, whether politicians in the majoritarian system carry

[16] On smoothing scatterplots, see Cleveland (1979).

out more pork-barrel activities than politicians in the proportional system. The final RDD sample consists of all majoritarian representatives (1,305) and proportional dual candidates (141), for a total of 1,446 observations. In columns (I) and (II), the estimated model is the split polynomial approximation, which makes use of all observations; a third-grade polynomial is used. Column (I) reports the estimate without control variables, while in column (II) we add the full set of covariates.

Being elected in the majoritarian system entails an increase in the share of geographically targeted bills of 8.2 percentage points, that is, it more than doubles the share of targeted bills with respect to the predicted value of 6.4 for proportional representatives at the threshold (7.0 for proportional representatives in the 5%-neighborhood). The two estimates of columns (I) and (II) 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. As expected, politicians with local government experience work more for their geographic constituency, while national politicians and representatives with parliament appointments work less. Columns (III) and (IV), where we use the local linear regression model, check the robustness of the RDD estimates with respect to the use of observations far away from the threshold. The bandwidth h is selected using the cross-validation method, and it is equal to 15.[17] Point estimates are very close to the previous ones, and again the inclusion of covariates does not affect the result. All the estimated effects of the majoritarian system reported in Table 4 are statistically significant at either a 1% or 5% level.[18]

The RDD estimates on the absenteeism rate, reported in Table 5, 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 4, but we make use of a slightly different sample because of missing values. According

[17]Results are qualitatively similar for all levels of h varying from 1 to 15.

[18]As a robustness check, we estimated the same RDD specifications in Table 4 using a ML estimator instead of OLS, because the decision of presenting targeted bills may involve two stages: (1) decide whether to present bills or not; (2) in case you present them, choose how many for the district of election. The baseline ML estimates with split polynomial approximation and local linear regression are equal to 0.089 (standard error, 0.039) and 0.073 (standard error, 0.033), respectively.

to the baseline estimate with polynomial approximation in column (I), 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 covariates, in column (II), 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, in columns (III) and (IV), are very similar to the previous ones.[19] All estimated effects of the majoritarian system in Table 5 are significant at either a 1% or 5% level.[20]

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 and reduces the amounts of politicians' rents.

6.2 Validity Tests

The validity of our evaluation strategy—that is, the joint validity of Assumption 1 and Assumption 2—can be assessed with different testing procedures. Remember that in the previous section we have already verified that the inclusion of pretreatment 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 pretreatment 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 a falsification test by using pretreatment information. Some politicians, in fact, were in office before the electoral reform of 1994, when all members of parliament were elected

[19] The optimal bandwidth h, chosen with the cross-validation method, is now equal to 14. Results are qualitatively similar for most levels of h varying from 1 to 15.

[20] As a robustness check, we estimated the same RDD specifications in Table 5 using the GLM estimator proposed by Papke and Wooldridge (1996), because the absenteeism rate is bounded between 0 and 1. The baseline GLM estimates with split polynomial approximation and local linear regression are equal to –0.146 (standard error, 0.052) and –0.125 (standard error, 0.043), respectively.

under proportional representation. As we observe their bills 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 using the past share of targeted bills as dependent variable. Third, we implement placebo tests by estimating the treatment effect at fake threshold levels.

Table 6 reports a first type of balance tests, that is, local linear regressions ($h = 10$) with each covariate as dependent variable. Except for the self-employment dummy, no pretreatment characteristic shows a significant discontinuity at the threshold. Table 7 reports a second type of balance tests, that is, split polynomial approximations with each covariate as dependent variable. Only the self-employment dummy and the freshman dummy (in the RDD sample for targeted bills, but not in the sample for absenteeism) show a significant discontinuity at the threshold. On the whole, these tests on a large set of covariates support the hypothesis that pretreatment observable characteristics are balanced around the threshold. Furthermore, 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. Both Table 6 and Table 7 point out that these two variables—that is, different residence and local government experience—are balanced around the threshold. This indirectly supports the plausibility of the RDD hypothesis on unobservables.

In Table 8, we apply the RDD exercise with split polynomial approximation using as the dependent variable the share of geographically targeted bills in the pretreatment period. In particular, we regress the share of targeted bills in term X (first row), term XI (second row), and both terms X and XI (third row) on the dummy of the electoral system and a third-grade polynomial on either side of zero.[21] If some politicians elected in the majoritarian system during the legislative terms XII, XIII, or XIV had some unobservable attachment to their local constituency, they would have presented more geographically targeted bills even in the pretreatment period. The results of this falsification test show that in the pretreatment period the impact of the (future) electoral system is never statis-

[21]To apply this falsification test we restrict our sample to those members observed at least once in the pretreatment legislative terms (X and XI) and once in the treatment terms (XII, XIII, and XIV).

tically different from zero. This result directly supports the claim that also unobservable characteristics are balanced around the threshold.

In Table 9, we run placebo tests at fake discontinuity points. For both outcome variables, we estimate the jump at the median on either side of $MV_i = 0$ with the split polynomial approximation model. The jumps at these fake thresholds are never statistically significant, 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).

7 Conclusion

In this paper, we have provided the first micro evidence about the effect of majoritarian electoral systems, as opposed to proportional systems, on the 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 the policies implemented in democratic countries. 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, 1999; 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 broader policies (Lizzeri and Persico, 2001). The normative analysis of this trade-off is beyond the scope of this paper, and it is left to future research.

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Tables and Figures

Table 1: Descriptive Statistics by Treatment Status, All Sample

	Proportional	Majoritarian	Difference	-diff95%	+diff95%
Male	0.756	0.914	-0.158	-0.194	-0.122
Married	0.652	0.756	-0.104	-0.154	-0.054
Age	48.566	48.248	0.318	-0.769	1.405
Schooling	16.102	15.976	0.125	-0.143	0.393
Different Residency	0.094	0.033	0.061	0.037	0.085
Local Govt. Exp.	0.431	0.564	-0.133	-0.188	-0.077
National Politician	0.274	0.207	0.067	0.020	0.114
Freshman	0.776	0.728	-0.048	-0.096	0.000
Incumbent	0.400	0.365	-0.034	-0.090	0.020
Switching	0.299	0.101	0.198	0.160	0.237
Center-Right	0.383	0.405	-0.021	-0.077	0.034
Parl. Appointments	0.089	0.074	0.015	-0.015	0.045
Clerk	0.051	0.051	0.000	-0.025	0.025
Lawyer	0.119	0.135	-0.016	-0.054	0.023
Executive	0.145	0.137	0.008	-0.032	0.047
Politician	0.201	0.162	0.039	-0.004	0.081
Entrepreneur	0.086	0.100	-0.013	-0.047	0.020
Teacher	0.109	0.090	0.019	-0.014	0.052
Self Employed	0.071	0.111	-0.040	-0.074	-0.006
Physician	0.053	0.090	-0.036	-0.067	-0.006
No. of Observations	394	1,305			

Ministers excluded. *-diff95%* and *+diff95%* represent the lower and upper bound of the 95% confidence interval of *Difference*, respectively. All variables are dummies, except *Age* and *Schooling* (both expressed in years). *Different Residency* stands for living in a different province with respect to the one of election. *Local Government Experience* stands for previous institutional experience at the local level (e.g., mayor of a city or president of a regional government). *Incumbent* refers to politicians elected in the same region in the previous legislative term. *Freshman* means that the previous parliamentary experience is lower than a full legislative term (5 years). Job dummies refer to the pre-election occupation.

Table 2: Outcome Variables by Treatment Status, All Sample

	Proportional	Majoritarian	Difference	-diff95%	+diff95%
No. of Bills	8.046	8.493	-0.448	-1.876	0.980
No. of Targeted Bills	0.652	0.981	-0.329	-0.525	-0.132
Share of Targeted Bills	0.073	0.112	-0.040	-0.061	-0.018
No. of Observations	394	1,305			
Absenteeism Rate	0.366	0.309	0.057	0.032	0.082
No. of Observations	368	1,260			

Ministers excluded. *-diff95%* and *+diff95%* represent the lower and upper bound of the 95% confidence interval of *Difference*, respectively. *Targeted Bills* are those targeted to the region of election. The *Share of Targeted Bills* is calculated over the total number of bills presented during the legislative term. Only bills presented as main sponsor are considered. The *Absenteeism Rate* is the percentage of votes missed without any legitimate reason during the legislative term.

Table 3: Margin of Victory (MV_i)

	Proportional		Majoritarian		All	
	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

Ministers excluded. The *Margin of Victory* is expressed in percentage points and is defined as the difference between the representative's vote share and the vote share of the next-best candidate.

Table 4: Share of Geographically Targeted Bills, RDD Estimation

	(I) All		(II) All		(III) $MV_i \in [-h, h]$		(IV) $MV_i \in [-h, h]$	
	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.
Majoritarian	0.082	0.034	0.082	0.031	0.068	0.029	0.064	0.027
Male			0.021	0.015			0.015	0.020
Age			-0.001	0.001			0.000	0.001
Schooling			0.002	0.003			0.001	0.004
Different Residency			-0.027	0.031			0.008	0.050
Lawyer			0.022	0.018			0.004	0.025
Executive			0.064	0.021			0.055	0.030
Politician			0.026	0.018			0.011	0.024
Entrepreneur			0.042	0.022			0.010	0.026
Teacher			0.040	0.020			0.020	0.026
Self Employed			0.061	0.020			0.052	0.028
Physician			-0.012	0.020			-0.032	0.026
Freshman			-0.003	0.018			0.036	0.023
Incumbent			0.019	0.012			0.028	0.016
Local Govt. Exp.			0.032	0.010			0.031	0.013
National Politician			-0.024	0.013			-0.024	0.017
Parl. Appointments			-0.045	0.019			-0.058	0.018
Center-Right			-0.017	0.012			-0.017	0.015
Region of Election	no		yes		no		yes	
Term Dummies	no		yes		no		yes	
No. of Proportional	141		141		99		99	
No. of Majoritarian	1,305		1,305		845		845	
No. of Observations	1,446		1,446		944		944	

Ministers excluded. Dependent variable: percentage of bills targeted to the region of election over the total number of bills presented. Only bills presented as main sponsor are considered. Models (I) and (II): split polynomial approximation ($p=3$). Models (III) and (IV): local linear regression (where $h=15$ is the optimal bandwidth selected using the cross-validation method). Standard errors are clustered at the individual level. *Clerk* is the reference category for job dummies. See Table 1 for a description of covariates.

Table 5: Absenteeism Rate, RDD Estimation

	(I) All		(II) All		(III) $MV_i \in [-h, h]$		(IV) $MV_i \in [-h, h]$	
	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.
Majoritarian	-0.149	0.051	-0.109	0.047	-0.128	0.043	-0.102	0.040
Male			0.000	0.019			0.004	0.024
Age			-0.001	0.001			-0.002	0.001
Schooling			0.002	0.003			0.002	0.004
Different Residency			0.125	0.043			0.108	0.060
Lawyer			0.059	0.022			0.031	0.027
Executive			0.012	0.023			-0.026	0.028
Politician			0.003	0.021			-0.022	0.027
Entrepreneur			0.025	0.023			-0.031	0.028
Teacher			-0.040	0.022			-0.083	0.028
Self Employed			0.010	0.022			0.014	0.028
Physician			0.034	0.025			0.011	0.030
Freshman			-0.061	0.018			-0.055	0.024
Incumbent			-0.007	0.014			-0.009	0.018
Local Govt. Exp.			-0.018	0.012			-0.013	0.015
National Politician			0.105	0.016			0.102	0.021
Parl. Appointments			0.063	0.024			0.066	0.034
Center-Right			0.054	0.013			0.074	0.015
Region of Election	no		yes		no		yes	
Term Dummies	no		yes		no		yes	
No. of Proportional	134		134		89		89	
No. of Majoritarian	1,260		1,260		773		773	
No. of Observations	1,394		1,394		862		862	

Ministers excluded. Dependent variable: absenteeism rate (i.e., the percentage of votes missed without any legitimate reason during the legislative term). Models (I) and (II): split polynomial approximation ($p=3$). Models (III) and (IV): local linear regression (where $h=14$ is the optimal bandwidth selected using the cross-validation method). Standard errors are clustered at the individual level. *Clerk* is the reference category for job dummies. See Table 1 for a description of covariates.

Table 6: Balance Tests with Local Linear Regression

	RDD-I Sample			RDD-II Sample		
	discontinuity	s.e.	obs.	discontinuity	s.e.	obs.
Male	-0.043	0.053	729	-0.035	0.060	698
Married	-0.072	0.093	729	-0.130	0.092	698
Age	-0.230	1.959	729	-0.561	2.109	698
Schooling	-0.280	0.477	729	-0.322	0.530	698
Different Residency	-0.020	0.072	729	0.055	0.057	698
Local Govt. Exp.	0.065	0.115	729	0.097	0.125	698
National Politician	0.060	0.086	729	0.043	0.098	698
Freshman	0.175	0.094	729	0.134	0.097	698
Incumbent	-0.114	0.112	729	-0.162	0.121	698
Center-Right	-0.075	0.114	729	-0.121	0.122	698
Parl. Appointments	0.008	0.067	729	0.039	0.065	698
Clerk	0.008	0.031	729	0.021	0.030	698
Lawyer	-0.008	0.074	729	0.013	0.073	698
Executive	-0.126	0.102	729	-0.116	0.112	698
Politician	-0.025	0.075	729	-0.013	0.081	698
Entrepreneur	-0.084	0.076	729	-0.113	0.087	698
Teacher	0.081	0.068	729	0.073	0.078	698
Self Employed	0.166	0.041	729	0.148	0.043	698
Physician	-0.029	0.083	729	-0.046	0.097	698

Ministers excluded. Local linear regressions (*h=10*) with each of the listed covariates as dependent variable. Standard errors are clustered at the individual level. See Table 1 for a description of covariates. *RDD-I Sample* is the sample used in the RDD estimation of the share of targeted bills (see Table 4). *RDD-II Sample* is the sample used in the RDD estimation of the absenteeism rate (see Table 5).

Table 7: Balance Tests with Split Polynomial Approximation

	RDD-I Sample			RDD-II Sample		
	discontinuity	s.e.	obs.	discontinuity	s.e.	obs.
Male	-0.087	0.061	1,446	-0.087	0.068	1,394
Married	-0.018	0.091	1,446	-0.075	0.089	1,394
Age	0.877	2.035	1,446	0.544	2.120	1,394
Schooling	-0.161	0.466	1,446	-0.200	0.511	1,394
Different Residency	-0.054	0.062	1,446	0.018	0.032	1,394
Local Govt. Exp.	0.067	0.117	1,446	0.091	0.122	1,394
National Politician	0.072	0.088	1,446	0.050	0.097	1,394
Freshman	0.209	0.098	1,446	0.172	0.105	1,394
Incumbent	-0.150	0.115	1,446	-0.195	0.121	1,394
Center-Right	-0.080	0.114	1,446	-0.130	0.118	1,394
Parl. Appointments	-0.036	0.064	1,446	-0.013	0.062	1,394
Clerk	-0.003	0.031	1,446	0.010	0.030	1,394
Lawyer	-0.039	0.079	1,446	-0.018	0.075	1,394
Executive	-0.146	0.097	1,446	-0.139	0.104	1,394
Politician	-0.005	0.076	1,446	0.003	0.078	1,394
Entrepreneur	-0.103	0.076	1,446	-0.136	0.085	1,394
Teacher	0.070	0.066	1,446	0.060	0.074	1,394
Self Employed	0.179	0.039	1,446	0.172	0.044	1,394
Physician	-0.017	0.081	1,446	-0.038	0.081	1,394

Ministers excluded. Split polynomial approximations ($p=3$) with each of the listed covariates as dependent variable. Standard errors are clustered at the individual level. See Table 1 for a description of covariates. *RDD-I Sample* is the sample used in the RDD estimation of the share of targeted bills (see Table 4). *RDD-II Sample* is the sample used in the RDD estimation of the absenteeism rate (see Table 5).

Table 8: Falsification Tests, Geographically Targeted Bills in Pre-Treatment Terms

	discontinuity	s.e.	obs.
Targeted Bills X	-0.002	0.003	81
Targeted Bills XI	0.052	0.118	255
Targeted Bills X-XI	0.041	0.089	271

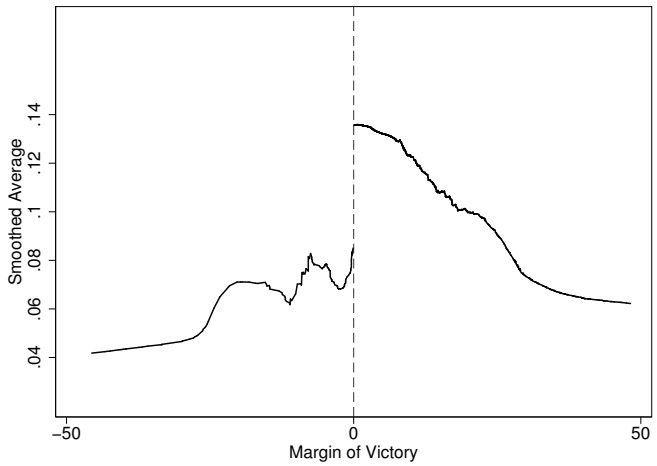
Ministers excluded. Split polynomial approximations (p=3) with the following dependent variables: percentage of bills targeted to the region of election over the total number of bills presented in the X legislative term, XI legislative term, or both.

Table 9: Placebo Tests, Geographically Targeted Bills and Absenteeism Rate

	percentile	discontinuity	s.e.	obs.
Targeted Bills:				
	50th left	0.055	0.079	141
	50th right	0.019	0.038	1,305
Absenteeism Rate:				
	50th left	0.062	0.108	134
	50th right	0.039	0.039	1,260

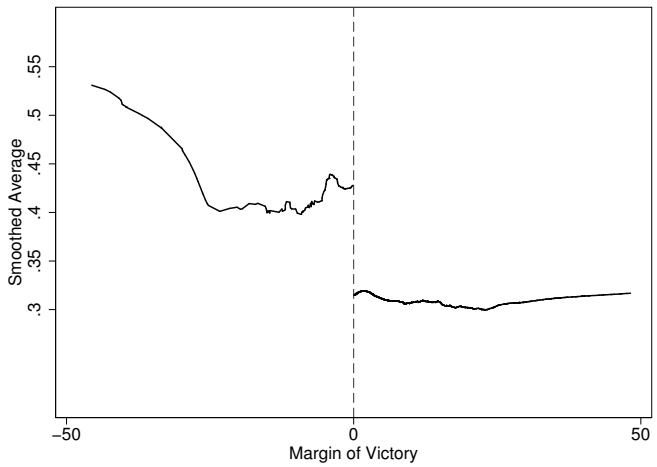
Ministers excluded. Dependent variables: share of geographically targeted bills and absenteeism rate. Split polynomial approximations (p=3) at fake discontinuity points, i.e., the median of the margin of victory on either side of the true threshold ($MV_i = 0$). Standard errors are clustered at the individual level.

Figure 1: Share of Geographically Targeted Bills, Smoothed Average



Running-mean smoothing of the share of targeted bills with respect to the margin of victory (bandwidth equal to 1 percentage point). The smoothing is performed separately above and below the threshold ($MV_i = 0$).

Figure 2: Absenteeism Rate, Smoothed Average



Running-mean smoothing of the absenteeism rate with respect to the margin of victory (bandwidth equal to 1 percentage point). The smoothing is performed separately above and below the threshold ($MV_i = 0$).