

# Electoral Rules and Politicians' Behavior: A Micro Test\*

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## Abstract

Theory provides many explanations about the influence of the electoral rule on politicians' equilibrium behavior. With a few exceptions, all models converge to one point. Majoritarian systems, characterized by plurality voting in single-member districts, are associated with more geographically targeted policies and less politicians' rents than proportional representation. We provide a micro test of these predictions by using a unique dataset about members of the Italian House of Representatives from 1994 to 2006. To account for non-random selection into different electoral systems, we exploit a particular feature of two-tier elections in Italy: 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. Main results confirm the predictions of the theoretical literature, as majoritarian politicians put forward a higher proportion of bills targeted to local areas and show lower absenteeism rates than their proportional colleagues.

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# 1 Introduction

Electoral rules differ from each other along many dimensions, but they are usually clustered around two opposite poles: majoritarian versus proportional systems. In majoritarian elections, the members of parliament are elected with plurality voting (also known as the *winner-take-all* rule) in single-member districts. In proportional elections, party lists compete for votes in multiple-member districts and parliament seats are attributed to each list according to its vote share.

Political scientists have long studied the effect of these two electoral systems on political outcomes, such as the number of political parties or government structure. Economists have recently contributed to the subject by developing micro-founded models with rational voters and self-seeking politicians, showing how different electoral systems influence politicians' equilibrium behaviors and, ultimately, public policies. From a first point of view, 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 (that is, build a coalition of diversified interests by means of targeted benefits). The majoritarian system, as opposed to the proportional system, is usually thought to be associated with more targeted redistribution and less nationwide public goods.<sup>1</sup> From a second point of view, the electoral rule decides how effectively voters can keep elected officials accountable for their actions. Assuming that politicians can extract endogenous rents—such as shirking to cultivate private affairs or corruption—from holding an office, the interests of voters and politicians diverge. As far as majoritarian elections increase the accountability of elected officials, this results in lower equilibrium rents.<sup>2</sup>

Many authors have confronted the predictions of the theoretical literature with cross-country data, finding that proportional systems are associated with broader redistribution

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<sup>1</sup>See Persson and Tabellini (1999), Lizzeri and Persico (2001), and Milesi-Ferretti, Perotti, and Rostagno (2002). See Section 2.1 for a discussion.

<sup>2</sup>See Persson and Tabellini (1999, 2000). Myerson (1993) proposes a model with a different prediction. See Section 2.2 for a discussion.

and higher perceived corruption.<sup>3</sup> The effect of electoral rules on aggregate outcomes, however, may depend not just on politicians' behavior, but also on other channels that cannot be easily disentangled with macro data. Moreover, political institutions are equilibrium outcomes whose effect is difficult to estimate with country-level data, because of the lack of a convincing source of exogenous variation (Acemoglu, 2005).

To the best of our knowledge, this paper provides the first micro test that directly estimates the causal effect of the electoral rule on observed behaviors by elected officials.<sup>4</sup> We use a dataset about the members of the Italian House of Representatives from 1994 to 2006, in order to compare in-office activities of politicians elected under different electoral systems in Italian two-tier elections, which in that period combined single-member districts in the first tier with proportional representation in the second tier.

We claim that the use of individual-level data allows us to avoid the usual drawbacks of the macro studies (that is, confounding indirect channels and endogeneity). Consider the following chain of causation: first, the electoral rule shapes both the motivations and incentives of candidates running for office; second, the behaviors of politicians selected under different electoral rules influence the size and target of public policies. Macro tests can only estimate the overall impact of this chain, capturing both the *direct* and *indirect* effects of the electoral rule on aggregate policies. Conversely, our micro test focuses on the first step of the chain, and it is thus able to tell us whether the electoral system directly influences politicians' behavior in the way predicted by the theoretical models.

At the same time, we are aware that endogeneity may be an issue also with individual-level data. Candidates with strong local ties (for instance, those who served in local governments or had their business established in a specific area) may be more likely to run in majoritarian districts to take advantage of their local popularity, and once elected they will carry out policies more geographically targeted simply because of their

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<sup>3</sup>See Persson and Tabellini (1999, 2003), Milesi-Ferretti, Perotti, and Rostagno (2002), and Persson, Tabellini, and Trebbi (2003).

<sup>4</sup>Frechette, Kagel, and Morelli (2007) use experimental data (i.e., not real observational data) to investigate the trade-off of potential legislators between the provision of public goods and targeted redistribution. They find that the share of public goods provided in mixed-region equilibria decreases with the weight legislators place on particularistic goods.

preferences and expertise. The Italian electoral system during the period 1994-2006, however, presented two particular features that can be used to apply a quasi-experimental Regression Discontinuity Design (RDD) and control for self-selection issues. First, the electoral system had two tiers: 75% of House members were elected in single-member districts, and 25% were elected with proportional representation at the regional level. Second, 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 long as random factors play an even small role in determining electoral outcomes, the selection into the majoritarian system mimics random assignment for those elected officials who won or lost by a narrow margin in a single-member district.<sup>5</sup>

We estimate 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 individual outcomes: the amount of geographically targeted activities carried out after election; rents or shirking. As a measure of geographic activities, we use the percentage of bills targeted to the region of election over the total of bills presented in a legislative term. As proxies for politicians’ rents, we use the absenteeism rate (that is, the percentage of parliament votes missed without any legitimate reason). Being elected in the majoritarian system more than doubles the fraction of targeted bills. Moreover, being elected in the majoritarian system increases the absenteeism rate by about one third. The main predictions of the theoretical literature are thus confirmed by the quasi-experimental evidence we provide.

The remainder of the paper is organized as follows. Section 2 discusses the relevant literature. Section 3 describes the Italian electoral law. Section 4 presents the formal evaluation framework that we use to estimate the causal effects of interest. Section 5 describes the data sources and the used sample. Section 6 presents the estimation results and a large set of validity tests. Section 7 concludes.

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<sup>5</sup>Different examples of RDD using narrow-margin elections have been proposed by Lee, Moretti, and Butler (2004), Hainmueller and Kern (2006), and Lee (2007).

## 2 Related Literature

In this section, we review the theoretical and empirical studies our contribution builds on. In particular, Section 2.1 surveys the models that investigate the political trade-off between broad and targeted redistribution under different electoral rules. Section 2.2 surveys the models that link politicians' rent extraction to the electoral system. In these two sections, we derive the theoretical hypotheses that our evaluation exercise tests. Section 2.3 discusses the empirical studies that estimate the aggregate effects of the electoral rule using cross-country data. Section 2.4 surveys political science studies providing descriptive evidence on the behaviors of elected officials in two-tier systems.

### 2.1 Theory (1): Who Is the Target?

Persson and Tabellini (1999) compare different electoral systems within a probabilistic-voting model with full commitment, where two office-seeking candidates offer a binding electoral platform that includes the tax rate, targeted transfers, public good, and politicians' rents. Voters are rational and divided into three groups (or districts). Two electoral systems are analyzed. Under proportional elections, a candidate wins if he gets more than 50% of the total vote in a nationwide district. Under majoritarian elections, each district is attributed according to the winner-take-all rule, and a candidate gains the general election if he wins in at least two out of the three districts. In the proportional system, political competition focuses on swing voters in the population at large (i.e., across districts), while in the majoritarian system competition focuses on swing districts only. In the latter case, the interests of safe (non-swing) districts are not represented in the equilibrium platform presented by the two candidates. This mechanism leads to more geographically targeted policies at the expense of public good provision in the majoritarian system.

Lizzeri and Persico (2001) study the political trade-off between public good provision and targeted redistribution under different electoral systems. Two office-seeking candidates, again, make binding electoral promises; but voters are now homogeneous. Their contribution is composed of two different analyses. In the first analysis, two electoral rules

are compared: winner-take-all versus proportional. Under the former, all the benefits from election (influence over policy making, exogenous rents, etc.) go to the winner. Under the latter, the benefits are divided among the candidates in proportion to their vote share. Candidates promise to provide the public good only if this proposal cannot be defeated by the alternative to redistribute tax-collected money to some voters. With the proportional rule, the incentive to redistribute is a function of the margin of victory and declines as the public good becomes more valuable. On the contrary, with the winner-take-all rule, the incentive to redistribute is not related to the value of the public good: as soon as a distributive strategy guarantees more than 50% of the votes, it yields 100% of the benefits from victory. If the public good is very desirable, winner-take-all is associated with more targeted redistribution than the proportional rule and vice versa.

In the second Lizzeri and Persico's analysis, electoral systems are classified along a different dimension—the magnitude of the electoral district. On one side—as in proportional elections—there is a unique nationwide district, and elections are won by the candidate who gets more than 50% of the votes. On the other side—as in majoritarian elections—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 the public good.

Both Persson and Tabellini's and Lizzeri and Persico's models study pre-election politics under the assumption of full commitment. Moreover, both models focus on the competition between national candidates who do not represent any specific area; the dynamics of nationwide political competition under different electoral rules lead candidates to target policies either to local areas or to society at large. The political trade-off between policies with *diffuse* versus *concentrated* benefits—the latter also being known as pork-barrel projects—has been studied from slightly different perspectives 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 over provide policies whose benefits are targeted to specific groups, as legislators seeking reelection favor projects for which they can easily claim credit with the voters (Mayhew, 1974). In the literature that has formalized this intuition, pork-barrel projects arise from a common-pool problem among legislators, as they have an incentive to provide projects that concentrate the benefits in their geographic constituencies and spread the costs across all constituencies through general taxation.<sup>6</sup>

Lancaster (1986) builds a bridge between the pork-barrel literature and the electoral system. His study predicts an inverse relationship between district magnitude and pork activity: the larger the number of representatives elected in the same district, the fewer pork-barrel projects. This is because a free-rider problem among representatives arises in multiple-member districts: if you are the only legislator representing a local constituency, it is easier to get political credit for pork activity. Note, however, that all the pork-barrel literature focuses on post-election rather than pre-election politics. As a different result from the models reviewed above, the relevant treatment coincides with *seeking* reelection under a certain electoral rule, rather than *being* elected under a certain rule.

Returning to voting models in political economics, 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 pre-election platforms; once appointed, politicians simply implement their bliss point. Citizens are heterogeneous along two dimensions: they belong to three different social groups and to three different regions. Decisions take place in two stages. The first is the electoral stage: three representatives are elected either under the majoritarian system (that is, each region elect a representative with plurality voting) or under proportional representation (that is, all representatives are elected in a nationwide district). The second stage coincides with

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<sup>6</sup>See, among others, Weingast, Shepsle, and Johnsen (1981). Note that this literature—unlike the models reviewed above—uses the implicit assumption that representatives who belong to the same political party are imperfect substitutes between each other from the voter’s point of view.

government formation: a representative, randomly selected to be the prime minister, asks another representative to join the government, subject to the constraint that policies must maximize the joint utility of government members. Under the crucial assumption that the distribution of social groups is the same across districts, both 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 policies targeted to the local district, anticipating that policies targeted to social groups are not contentious in this system. The opposite holds under proportional representation, where the median voter prefers a representative biased toward policies targeted to the social group.

Summing up, all the voting models reviewed in this section 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.*

## 2.2 Theory (2): Politicians' Rent Extraction

Politicians' rents are another outcome usually thought to be decided by the electoral system. Various models assume that politicians can extract endogenous rents from public appointments. If the public's monitoring is less than perfect, elected officials can shirk, that is, put low effort in their public duties, to cultivate their own private interests, or they can exploit their discretionary authority to get bribes. Either in the form of shirking or plain corruption, politicians' rents depend on the degree of voters' monitoring over elected officials and the intensity of the punishment for misbehaviors. And the electoral system crucially influences both elements.

In Persson and Tabellini's (1999) model discussed in the previous section, rents are a component of the policy vector proposed by candidates before the election. In the majoritarian system, competition focuses on swing districts only. Since voters in marginal districts are more reactive to policy changes, competition is thus stiffer under majoritarian elections; politicians become more disciplined and extract lower equilibrium rents. Pers-



son and Tabellini (2000) use a different setup to derive the same result—a negative effect of the majoritarian system on rents. 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, as in majoritarian elections, the lower politicians' rents.<sup>7</sup>

Unlike the prediction about targeted activities (H1), however, the negative relationship between the majoritarian system and politicians' rents is not unambiguous in the theoretical literature. Myerson (1993) builds a game-theoretic model showing that the proportional system may reduce entry barriers for honest politicians and, as a result, equilibrium rents.<sup>8</sup> In his model, political parties differ along two dimensions: ideology (left versus right) and honesty (honest versus dishonest). Some voters prefer the leftist party, while others prefer the rightist party; but all voters prefer honest parties. Assume, for example, that there are four rival parties: L1 and L2 have a leftist platform; R1 and R2 have a rightist platform. Moreover, L1 and R1 are known to be dishonest; L2 and R2 are known to be honest. Myerson's analysis shows that, with plurality voting in a single-member district, 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 L1 and R1 take place. If voters believe that their first-best choice has no chance of winning, they rationally vote for the dishonest party whose ideology they share.

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<sup>7</sup>The intuitions behind Persson and Tabellini's (1999, 2000) results have been echoed by Kunicova and Rose-Ackerman (2005). In their framework, the same features of the majoritarian system that create incentives for pork-barrel activity also reduce corruption, as in this system voters and political opponents have both greater incentive and ability to monitor in-office politicians.

<sup>8</sup>See also Myerson (1999).

The outcome is different under proportional representation. Assume that each party's vote share decides its seats in parliament, and that the realized government policy depends on whether a majority of the seats are allocated to leftist or rightist parties. In this setup, voters are free to vote for their first-best choice, because by doing so they will reduce corruption without affecting the balance between left and right in the parliament. All leftist voters pick L2, and all rightist voters pick R2. Equilibrium rents (or corruption) are thus lower than in the majoritarian scenario. The magnitude of the electoral district—which in turn affects the degree of entry barriers for well-behaving candidates—is the crucial feature lying behind this result.

Summing up, from the endogenous-rent models discussed above, we can 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 more rents than politicians elected in the proportional system.*

## 2.3 Macro Tests

The models discussed in the previous sections have motivated an array of empirical studies that have used cross-country data to test the effects of the electoral rule on aggregate outcomes. Persson and Tabellini (2003) use OLS, matching estimators, parametric selection corrections, fixed-effect panel models, and IV strategies to evaluate the effects of the majoritarian system, as opposed to proportional representation, at the country level.<sup>9</sup> They find a negative and significant effect of the majoritarian system on both welfare state spending (as a proxy for broad, non-targeted, redistribution) and the perceived level of corruption (as a proxy for politicians' rents). These results are robust to the utilization of the different estimation strategies mentioned above. The finding that the proportional system is associated with higher levels of corruption is confirmed by the cross-country evidence provided by Kunicova and Rose-Ackerman (2005).

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<sup>9</sup>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).

Milesi-Ferretti, Perotti, and Rostagno (2002) use OLS and panel techniques with country-specific shocks to estimate the effect of the electoral system on both public goods (interpreted as a measure of policies targeted to geographic constituencies) and transfers (interpreted 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 party a parliament seat in a district of average magnitude, and they use it in association with other measures commonly used in political science, such as the sum of the deviations of each party's share of seats from its share of votes. 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 and on Latin America.

Summing up, the above studies find a weak support to the hypothesis that the majoritarian system increases targeted policies, and a stronger support to the hypothesis that it reduces politicians' rents. These macro tests, however, come with two drawbacks: a data problem and, more relevant, an identification problem. On the data side, results may be sensitive both to the classification of electoral systems and to the choice of the aggregate variables used to approximate the variables specified in the theoretical models.<sup>10</sup> On the identification side, although macro tests detect important correlations that are consistent with the theoretical literature, it is doubtful that they are able to disclose causal effects of the electoral system. OLS and matching estimates, in fact, rely on the conditional independence assumption, i.e., on the exogeneity of the electoral rule. But the electoral rule, like any other political institution, is an equilibrium outcome determined by numerous factors that cannot be fully controlled in the empirical models.<sup>11</sup> Panel techniques provide a way to control for time-invariant confounding factors, but within-country variation in the electoral rule is usually not sufficient to obtain estimates with both accuracy and external validity. Among the estimators employed in the macro tests, only IV strategies

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<sup>10</sup>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.

<sup>11</sup>See the discussion in Acemoglu (2005).

can claim to disclose causal effects. But this claim crucially relies on the plausibility of their (untestable) exclusion restrictions, which are not always compelling.<sup>12</sup>

Moreover, assuming that macro tests are able to disclose true causal effects of the electoral rule, it is not beyond question that they are actually testing the main predictions of the models discussed in Section 2.1 and Section 2.2. 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 aggregate outcomes. If this were not the case, macro tests, rather than confirming the theoretical hypotheses H1 and H2, would estimate the overall impact of the electoral system; that is, the joint impact of both *direct* and *indirect* effects of the electoral system on country-level variables. Assume, for example, that the electoral system affects aggregate outcomes both through its direct effect on politicians' behaviors predicted by the theory and through an indirect effect on the equilibrium number of parties, as recently suggested by Persson, Roland, and Tabellini (2007). In this setting, macro tests, far from testing H1 and H2, only estimate the joint impact of the direct and indirect effects of the electoral rule on aggregate variables.<sup>13</sup> An alternative evaluation setup is needed to test the direct effects of the electoral system on politicians' incentives and behaviors.

## 2.4 Politicians in Two-Tier Systems

Triggered by the increasing diffusion of two-tier electoral systems worldwide, political scientists have recently turned their attention to this hybrid system in which single-member districts coexist with proportional representation.<sup>14</sup> Some studies have investigated the actions and perceptions of members of parliament elected under different tiers. Lancaster and Patterson (1990) use survey responses by German legislators and find that majori-

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<sup>12</sup>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 an electoral system instead of the other 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.

<sup>13</sup>The only exception is represented by Persson, Roland, and Tabellini (2007), who use cross-country data and find that the impact of the electoral rule on aggregate outcomes is indeed coming from its indirect effect on the number of political parties.

<sup>14</sup>See Massicotte and Blais (1999) for an overview.

tarian representatives quote targeted projects as important for their reelection more often than proportional representatives. Stratmann and Baur (2002) look at committee assignments in Germany under the assumption that politicians elected in different tiers of the electoral system have different incentives to seat in particular committees. They classify parliament committees in three categories: district, party, neutral. This classification is based on the authors' perception of the type of work that is performed in each committee, with projects targeted to geographic areas being more likely to be discussed in "district" committees. They find that majoritarian representatives are more likely to be assigned to this type of committee than their proportional colleagues.

Both outcome measures used in the above studies, however, are necessarily subjective, either with respect to politicians' responses in the first study or with respect to the authors' classification of parliament committees in the second. Kunicova and Remington (2005) use a less subjective, but equally indirect measure of targeted activities. They study roll-call votes in the mixed-member Russian Duma. Their study finds that majoritarian representatives, when voting over the federal budget, show less party loyalty than proportional representatives do. This result suggests that majoritarian politicians may trump their partisan ties to cultivate their regional constituencies.

Unlike the macro tests discussed in the previous section, these studies focus on the direct effect of the electoral system on politicians' behaviors, but they do not aim at controlling for self-selection into different tiers, being far from estimating causal effects of the electoral system. Moreover, this literature only considers the prediction on activities targeted to local districts (H1), disregarding the prediction on politicians' rents (H2).

### **3 The Italian Electoral System**

Before discussing our evaluation framework, it is important to describe the institutional features of the Italian electoral system. The rules for the election of the Italian parliament have frequently changed 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, the number of seats per district varying from 3 to 54 depending on the population; 21 for the Senate, the number of seats per district varying from 1 to 47). Starting from 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). Electoral rules changed again with the legislative term XV (2006-current), switching to a closed-list proportional system with 27 districts in the House (from 3 to 44 seats per district) and 20 in the Senate (from 1 to 47 seats per district).<sup>15</sup> In every legislative term, the number of seats has remained unchanged to 945, of which 630 are in the House of Representatives and 315 in the Senate.

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, since 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 (majoritarian tier), while 25% of members were elected with closed-list proportional representation in 26 multiple-member districts (the number of seats per district varying from 1 to 12 depending on the population). 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 majoritarian districts (232) were assigned to the remaining 25% seats (83) according to proportional representation. Hence, only for the House of Representatives the two tiers of the system were perceived as distinct by voters.

In our evaluation exercise, we exploit another particular institutional feature of the two-tier electoral system for the House of Representatives from 1994 to 2006. Candidates could run for both the majoritarian and proportional tier. If they were elected in both tiers, 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

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<sup>15</sup>With the 2006 election and for the first time in Italy, 12 and 6 members of the House and the Senate, respectively, were appointed in a district representing the Italians leaving abroad.

list. Note that not all candidates were running for both tiers; national leaders were more likely to be dual candidates, but usually not in marginal (non-safe) districts. These features allow us to provide quasi-experimental evidence on the effects of the electoral rule, on the basis of the evaluation strategy formally described in the next section.

## 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 rents.<sup>16</sup> 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 were elected in the majoritarian tier, and  $T_i = 0$  if he were elected in the proportional tier. 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 (like 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 true causal effect of the electoral system on the outcome variables.<sup>17</sup>

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<sup>16</sup>See Section 5 for a precise description of the outcome variables.

<sup>17</sup>See Van der Klaauw (2007) and Imbens and Lemieux (2007) for a survey of identification and estimation issues in RDD.

## 4.1 Identification

Assume, to begin with, that candidates in the House election run for both a majoritarian seat and a secure 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 wins in both tiers. Treatment assignment can be specified as:

$$T_i = 1[MV_i \geq 0], \quad (1)$$

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 percentage of votes obtained by  $i$  and the percentage of votes obtained by either the best candidate behind him (in case  $i$  won) or the winner of the election (in case  $i$  lost). 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  $MV_i = 0$ .  $MV_i$  is a random variable depending on observable and unobservable individual characteristics, as well as on general factors at the moment of the election.

Various empirical studies have exploited the assignment mechanism generated by the margin of victory in single-member plurality elections to implement an RDD and estimate a causal effect of interest.<sup>18</sup> In what follows, we borrow Lee’s (2007) setup in stating the identification conditions required by a sharp RDD of this type. We then discuss an additional assumption required by our data context and present an array of validity tests.

Define  $U_i$  as an unobservable individual characteristic, affecting potential outcomes  $Y_i(1)$ ,  $Y_i(0)$ , observed individual characteristics  $X_i$ , and the margin of victory  $MV_i$ . The relationship between  $U_i$  and  $MV_i$  is assumed to meet the following conditions.

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<sup>18</sup>Lee (2007) estimates the effect of incumbency on the probability of winning the next election in the US. Hainmueller and Kern (2006) estimate the contamination effect of the electoral outcome in the majoritarian tier on the electoral outcome in the proportional tier in German mixed-member elections. 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.



**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  $MV = 0$ .

In other words, individuals 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). Moreover, for each politician the probabilities of winning or losing the majoritarian race by a narrow margin are the same (condition b).<sup>19</sup> Assumption 1 states that electoral outcomes depend on both predictable elements and random chance, which is then crucial only for close races. This is a plausible assumption and it is no surprise that it has been widely applied in the literature. For instance, heavy rain on election day may influence the turnout rate and, as a result, the victory of a candidate instead of the other in marginal districts. Moreover, 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 both major parties; as a result, the forces of political competition prevent each party from sorting above the threshold.

Lee (2007) shows that under Assumption 1:

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

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

In other words, selection into treatment mimics random assignment in a small neighborhood of the threshold. It follows that:

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

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

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<sup>19</sup>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 majoritarian elections.

Hence, 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 near the threshold:

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

Assumption 1 allows us to estimate a true causal effect, but it should be noted that the  $ATE_{rdd}$  is a local effect, defined in a neighborhood of  $MV_i = 0$ , that cannot be extrapolated to the whole population without additional homogeneity assumptions. The gain in internal validity is associated with a loss in external validity. We prefer this approach, as we believe that when facing a similar trade-off in any evaluation problem, internal validity is more important than extrapolation.

A valuable feature of the RDD is that it comes with a set of validity tests that can indirectly support (or reject) the plausibility of this identification strategy. The key idea is that, although implication (2) is untestable by definition, implication (3) can be tested. And rejecting (3) would cast serious doubts on (2) and the validity of the RDD identification strategy (particularly for those  $X_i$  that may be affected by the same unobservables that influence potential outcomes). Three validity tests proposed in the literature rest on a similar reasoning. First, pre-treatment characteristics should not display any discontinuity at the threshold. Second, the estimated  $ATE_{rdd}$  should be insensitive to the introduction of covariates, as the utilization of additional covariates should only translate into an efficiency gain. Third, assuming that pre-treatment outcomes are available, the implementation of an RDD on these additional data should produce a zero  $ATE_{rdd}$  (falsification test).

In Italy, however, not all politicians are dual candidates. In particular, some of them only run for election in the majoritarian tier. This gives rise to a treatment assignment slightly different from the mechanism in equation (1). If  $MV_i < 0$ , we observe either  $T_i = 0$  (if  $i$  was a dual candidate) or  $T_i = .$  (if  $i$  was only a majoritarian candidate). We have to deal with the problem that some candidates with  $MV_i < 0$  are not observed. In this setting, to get a consistent estimate of  $ATE_{rdd}$ , we need an additional assumption.

**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:*

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

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 also were 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. We believe, however, that it is worth making for at least three reasons. First, besides this additional assumption, our evaluation strategy is a sharp RDD, which relies on mild and plausible identification conditions; in a fuzzy RDD, for example, a different but equally strong assumption, i.e., the local conditional independence assumption, would be required.<sup>20</sup> Second, some other features of the Italian political system make our assumption defensible. National leaders—who are not representative of the population of politicians—tend to be dual candidates, but they also get secure districts where the race is lopsided in favor of their party. The other dual candidacies are allocated to runners in marginal districts. Since there are not enough dual candidacies to satisfy all marginal runners, however, some of them are excluded even if they are “similar” to those who get a dual candidacy, making the representativeness assumption particularly plausible in non-safe districts (i.e., districts near the threshold).<sup>21</sup> Third, and most important, it is straightforward to show that, if Assumption 2 did not hold, also implications (2) and (3) would break down. As a result, the same array of validity tests commonly used in the sharp RDD (balance tests of covariates near the threshold; irrelevance of covariates in the estimation procedure; falsification test) can be implemented to assess the plausibility of our entire identification strategy.

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<sup>20</sup>In the fuzzy RDD, the local conditional independence assumption is required to identify the  $ATE_{rdd}$  defined in equation (6). Alternatively, if a weaker monotonicity condition is assumed, only the  $ATE$  for compliers near the threshold can be identified, reducing even further external validity.

<sup>21</sup>In Section 5, we provide descriptive evidence supporting this claim.

## 4.2 Estimation and Validity Tests

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 (2007), and the local linear regression suggested by Imbens and Lemieux (2007).

The first method uses the whole sample and chooses a flexible specification to fit the relationship between  $Y$  and  $MV$  both to the right and to the left of the threshold. The estimated discontinuity in  $Y$  at the threshold represents the treatment effect. Specifically, we estimate the model

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

using ordinary least squares. 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 errors  $\eta_i$ . Since  $MV_i$  is equal to zero at the threshold, the coefficient  $\tau$  identifies the  $ATE_{rd}$ . Usually, a third-grade polynomial ( $p = 3$ ) is used in the empirical literature. The sensitivity of the results to the choice of  $p$  should be assessed.

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, 2007). 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

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

using ordinary least squares. Again, standard inference procedures—with cluster correction at the individual level—can be applied. The bandwidth parameter  $h$  can be selected

applying the cross-validation method developed by Ludwig and Miller (2007) and formalized by Imbens and Lemieux (2007).<sup>22</sup> The sensitivity of the results to the choice of  $h$  should be assessed.

As anticipated in the previous section, the validity of our estimation strategy can be assessed with various tools. We perform three types of validity tests. First, we check whether the covariates  $X_i$  are balanced to the right and to the left of the threshold: if they are, no discontinuity in  $X_i$  will be observed at  $MV_i = 0$ . Hence, repeating the above estimation methods with each covariate in the place of the outcome variable should produce zero estimates of the discontinuity points. Second, we add  $X_i$  to the right-hand side of the above regressions and check whether the estimated  $\tau$  is sensitive to this step; the inclusion of pre-treatment covariates should simply improve efficiency without influencing point estimates. Third, we run a falsification test by using pre-treatment information on the outcome variables for a particular subsample. Some politicians in our sample, in fact, were in office before the electoral reform of 1994, when all representatives were elected under proportional representation. As we observe their outcome variables in this pre-treatment period (specifically, in the legislative term X, from 1987 to 1992, and in the legislative term XI, from 1992 to 1994), we repeat the above estimation methods with the past (pre-treatment) outcome in the place of  $Y_i$ . To support the plausibility of the identification strategy, we should find a zero treatment effect. As past outcomes are likely to be influenced by the same unobservables that determine post-treatment outcomes, this falsification test—if not rejected—supports the claim that (2) is satisfied.

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<sup>22</sup>This method consists in choosing  $h$  so as to minimize the loss function

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

where the predictions  $\hat{\mu}_h(MV_i)$  are retrieved as follows. For every observation  $MV_i$  to the left of the zero threshold, we predict its value as if it were at the boundary of the estimation, using only observations in the interval  $[MV_i - h, MV_i]$ . For every observation  $MV_i$  to the right of the zero threshold, we predict its value as if it were at the boundary of the estimation, using only observations in the interval  $[MV_i, MV_i + h]$ . Following Imbens and Lemieux (2007), we calculate the loss function only for a subsample of  $N$  politicians, discarding 50% of the observations on either side of the threshold  $MV_i = 0$ .

## 5 Data

We use a unique dataset about the members of the Italian House of Representatives for the period 1994-2006 (legislative terms XII to XIV). Although the original dataset also included the legislative terms X (1987-1992) and XI (1992-1994), as well as complete information for the Senate, we do not use these data here for the institutional reasons discussed in Section 3. The sources we used to collect the dataset include: the Annals of the Italian Parliament (*La Navicella*) for demographic and professional information;<sup>23</sup> the Tax Declarations Archive of the members of the Italian Parliament for individual income information; the online archive of bills for all the legislative activity;<sup>24</sup> and the Italian Parliament Statistical Office for data on individual attendance and salaries.<sup>25</sup>

The dataset we use in this paper contains the following individual information:

- Demographic characteristics (age, gender, marital status, number of children, place of birth, place of residence, level of education, field of education);
- Self-declared previous job (before entering parliament for the first time);
- Number and type of bills as main sponsor (geographical area interested by the bill, from town to regional level, and a classification by subject);<sup>26</sup>
- Absenteeism (the number of electronic votes missed without any legitimate reason);
- Appointments in the parliament (president, vice president, and secretary either of the parliament or of one 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);

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<sup>23</sup>“I Deputati e i Senatori del Parlamento Repubblicano”, edited by Editoriale Italiana. More information available at: <http://www.editoriale.it/deputati/index.htm>.

<sup>24</sup>Available at: <http://www.senato.it/App/Search/sddl.asp?style=Avanzata>.

<sup>25</sup>The whole dataset will be made available at: <http://www.empirical-economics.com>.

<sup>26</sup>Bills are classified using 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. We then matched this information with the district of election of the representative who presented the bill.

- Local government experience (mayor, city counselor, president of a region, etc.);
- Electoral system under which the representative was elected (majoritarian or proportional), district of election, and number of votes.

Since we drop the observations containing at least one missing for some of the relevant variables, we end up with a sample of 1,722 observations, among whom 1,317 were elected in the majoritarian tier and 405 in the proportional tier.<sup>27</sup> Table 1 provides descriptive statistics for this sample, comparing treated (i.e., majoritarian) and untreated (i.e., proportional) individuals. As expected, these two groups display different characteristics, suggesting that self-selection in the choice of the electoral system is at work: majoritarian representatives are more likely to be male, national politicians, and to have longer parliamentary tenure. Available proxies for local attachment, such as “local government” (previous institutional experience at the regional, province, and city level) or the inverse of “different residency” (living in a different province with respect to the one of election), are not balanced. These patterns of selection on observables suggest that also unobservables may be different for politicians running in a tier instead of the other; if this were the case, simple OLS estimates of the effect of the electoral rule would be biased.

To test the hypotheses H1 and H2 derived in Section 2.1 and Section 2.2, we use two outcome variables:

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.

The interpretation of the first variable as the level of geographically targeted activities is straightforward. We use the fraction, instead of the number of targeted bills, to control for

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<sup>27</sup>The 1,722 observations of the final sample correspond to 1,233 politicians, among whom 878 were always elected in the majoritarian tier, 242 were always elected in the proportional tier, and 113 switched from one tier to the other across legislative terms.

the different levels of intensity in bills sponsorship between majoritarian and proportional representatives. Assuming that the hierarchy of interests shown by members of parliament in their bills is unchanged in the other activities (for example, bargaining for funds with the Treasury Minister), we can interpret our measure as a proxy of targeted redistribution. The use of the second outcome variable rests on the idea that shirking is a type of rent. As shown by Gagliarducci, Nannicini, and Naticchioni (2007), the absenteeism rate is positively correlated with the amount of politicians' outside income (that is, money made when in office and streaming from different sources with respect to the parliament salary), supporting the idea that shirking allows cultivating private interests and earning rents.

Descriptive statistics about bills sponsorship and absenteeism 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%) than for proportional politicians (7%). Conversely, the absenteeism rate is significantly higher for proportional (37%) than for majoritarian politicians (30%). This descriptive evidence is consistent with the hypotheses H1 and H2, but it is far from detecting a true effect of the electoral rule on the outcome variables because of the already discussed selection issues.

To deal with self-selection and implement the RDD described in Section 4, we exploit the presence of dual candidates and close races in single-member districts. If a majoritarian candidate had a positive margin of victory ( $MV_i \geq 0$ ), he had to accept the seat. If he had a negative margin ( $MV_i < 0$ ), he could either be elected in the proportional tier (if he were a dual candidate high-ranked in the party list) or stay home (if he were not a dual candidate). Note that, 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. Under Assumption 1 and Assumption 2 in Section 4.1, we use all majoritarian representatives and those proportional representatives who were dual candidates to identify the causal effects of interest. In other words, we only use observations



with a non-missing margin of victory in a single-member district, which is our treatment assignment variable (with threshold at  $MV_i = 0$ ). Table 3 shows some characteristics of the distribution of the margin of victory. This table provides evidence supporting Assumption 2 of our identification strategy. In fact, if we had no missing information for the losing candidates in single-member districts (that is, if all of them won a proportional seat), by definition, we would observe symmetric figures in the two sides of the margin of victory, positive for majoritarian politicians and negative for proportional politicians. This is not the case because our proportional dual candidates are a subsample of all candidates who lost in single-member districts. Table 3, however, shows that the two sides of the margin of victory are very close to one another, especially in small neighborhoods of the threshold level  $MV_i = 0$ , where they are basically identical, reinforcing the plausibility of Assumption 2 with respect to a crucial variable as the margin of victory.

## 6 Estimation Results

### 6.1 Effect of the Electoral System on Targeted Bills

The RDD estimates of the effect of the majoritarian system on the outcome variable “geographically targeted bills,” reported in Table 4, provide a way of testing H1. For the reasons discussed in the previous section, the final RDD sample consists of all majoritarian representatives (1,317) and those proportional representatives who were dual candidates (142), for a total of 1,459 observations. In columns (I) and (II), the estimated model is the split polynomial approximation of equation (7), which makes use of all observations. The treatment effect (i.e., the coefficient of the variable “majoritarian”) is identified as the difference between the boundary points of the polynomials fitted on either side of the threshold. A third-grade polynomial approximation is used.<sup>28</sup> Column (I) reports the estimate without covariates, while in column (II) we add the set of covariates.

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<sup>28</sup>Our results do not change much choosing other polynomial functions in the margin of victory, for instance choosing a lower or higher order of the polynomial approximation.

Being elected in the majoritarian system entails an increase in the share of geographically targeted bills of 8.3 percentage points, that is, it more than doubles the share of targeted bills with respect to the average of 7.4 for proportional representatives. As expected, the two estimates of columns (I) and (II) are very close to one another, supporting the assumption that relevant covariates (i.e., variables affecting the outcome) do not display discontinuities at the threshold. This provides first evidence on the validity of our evaluation framework. The covariates that significantly affect the fraction of targeted bills have the expected sign, as politicians with local government experience do more for their local constituency, while national politicians and representatives with parliament appointments do less.

The robustness of the RDD estimates with respect to the utilization of observations far away from the threshold is checked in columns (III) and (IV), where the estimated model is the local linear regression of equation (8). This method uses only the observations in the  $[-h, h]$  neighborhood of the threshold. The bandwidth  $h$  is selected using the cross-validation method, and it is equal to 13.<sup>29</sup> Point estimates are nearly identical to the previous ones. The inclusion of covariates, again, does not affect the result.

We provide a graphical representation of the results in Figure 1, which refers to the estimate of column (I). The vertical axis displays the share of geographically targeted bills, while in the horizontal axis the margin of victory is reported with the vertical line at zero marking the threshold. All observations to the left (right) of the vertical line concern the proportional (majoritarian) representatives. The two curves at the two sides of the vertical line represent the fitted values derived using the polynomial specification in the margin of victory. Each data point represents a local average of the outcome variable, the share of geographically targeted bills, for intervals of margin of victory with a width of 1. The noticeable discontinuity in zero provides the measure of the causal effect of the electoral system on the outcome (0.083). It is also worth noting that the higher is the distance from the vertical line, the lower is the share of geographically targeted bills. This

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<sup>29</sup>The results are not sensitive to changes in the value of the bandwidth.

is consistent with our interpretation: candidates in close electoral races strongly commit themselves to their constituency, while candidates that are sure to win (or to lose) in the majoritarian district do not target their activity to the local constituency.

## 6.2 Effect of the Electoral System on Shirking

The RDD estimates of the effect of the majoritarian system on the outcome variable “absenteeism rate,” reported in Table 5, provide a way of testing H2. We carry out the same estimation methods presented in Section 4.2 and used in the previous section, but we make use of a slightly different sample because of missing values for the new outcome variable. Columns (I) and (II) show the results of the split polynomial approximation with or without covariates, respectively. Columns (III) and (IV) show the results of the local linear regression with or without covariates, respectively. The optimal bandwidth  $h$ , chosen with the cross-validation method, is now equal to 15.<sup>30</sup>

According to the baseline estimate 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 about 40% with respect to an average absenteeism rate of 36.7 for proportional representatives. Taking into account the set of covariates, the estimated treatment effect is slightly lower, equal to a fall of 11.2 percentage points (30% with respect to the average). The two estimates are not statistically different from one another, and they are both significant at the 1% level. The point estimates obtained with local linear regression—in columns (III) and (IV)—are nearly identical to the previous ones.

The treatment effect at the zero threshold is illustrated in Figure 2, which is the counterpart of Figure 1 and refers to the estimate of column (I) in Table 5. Differently from the case of targeted bills, the behaviors of politicians do not change much when we get farther from the threshold level: both majoritarian and proportional politicians display quite flat absenteeism rates along the horizontal axis.

Summing up, the RDD estimates in Table 4 and Table 5 strongly support the theoretical hypotheses H1 and H2, showing—with respect to the latter—that the accountability

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<sup>30</sup>The results, again, are not sensitive to changes in the value of the bandwidth.

effect of the majoritarian system dominates other possible effects and reduces the amounts of politicians' rents under this type of electoral rule. As a final remark, it should be noted that our treatment coincides with the fact of *being* elected under a certain rule, as opposed to the fact of *seeking* reelection under a certain rule. As the majoritarian and the proportional tier coexist in our data, one might argue that there are 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). We claim that the possible spillovers are limited in our context, as politicians switching from one tier to the other are just a few: 113 politicians over 1,233 (9%), or 256 observations over 1,722 (15%), in the whole sample. Moreover, if spillovers were actually at work, our estimates would result in a lower bound of the true causal effect of the electoral rule. For instance, if some politicians elected in the proportional (majoritarian) tier carried out more (less) targeted activities in order to be reelected in a different tier at the next election, the true effect of the majoritarian system on geographically targeted activities would be even greater than 8.3 percentage points.

### 6.3 Validity Tests

The RDD estimates presented in the previous two sections crucially rely on Assumption 1 and Assumption 2 of the identification strategy discussed in Section 4.1. There, we have presented an array of validity tests that can support (or reject) the plausibility of the assumptions. In this section, we present the results of these tests.

The RDD assumes that both observables and unobservables have continuous density functions in the neighborhood of the threshold level. As for observable characteristics, it is possible to test whether pre-treatment variables are balanced in close neighborhoods of the threshold. Table 6 reports balance tests for all the covariates in the  $\pm 5\%$  neighborhood of the zero threshold. Except for parliament tenure, all the differences are not statistically different from zero. This general finding is not driven by the small sample size, as also the group means for proportional and majoritarian politicians are usually similar.

Another way to test any discontinuity of covariates at the threshold level is to carry out models with split polynomial approximation, substituting the outcome variable with each observable covariate. We regress each of the covariates on a third-degree polynomial function of the margin of victory on either side of the threshold. Table 7 shows that for most of the covariates there is no discontinuity at the threshold level. The only exception concerns the self-employed dummy, which reports a significant discontinuity.

As for the two variables that are not completely balanced (parliament tenure and the self-employed dummy), Table 4 and Table 5 show that the former is correlated with absenteeism but not with geographically targeted bills, while the opposite holds for the latter. It should be noted, however, that the comparison of column (I) against column (II), or column (III) against column (IV), in both Table 4 and Table 5 is itself a balance test of relevant covariates. Only if covariates with a strong effect on the outcome variable were not balanced in the neighborhood of zero, in fact, the estimate with covariates would diverge from the baseline estimate without covariates. The estimates are nearly identical in the case of targeted bills, and not too dissimilar in the case of absenteeism.

Moreover, in the set of covariates, two variables can be plausibly considered as correlated with the main unobservable issue we cannot control for, i.e., that some representatives might be more attached to their local constituency. Table 6 and Table 7 point out that these two variables—that is, “different residency” (living in a different province with respect to the one of election) and local government experience—are balanced in the neighborhood of the threshold, both in the  $\pm 5\%$  neighborhood and using the polynomial approximation. This finding indirectly supports the plausibility of the RDD assumption on unobservables.

Table 8 provides additional evidence in favor of this assumption. We apply the same RDD exercise with split polynomial approximation, using as dependent variable the share of geographically targeted bills in the pre-treatment period, the legislative terms X and XI, when the electoral system was entirely proportional. In particular, we regress the share of geographically 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 the threshold. 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 a pre-treatment period.<sup>31</sup> The results of this falsification test show that in the pre-treatment period, the impact of the (future) electoral system is never significantly different from zero.

Summing up, all the evidence coming from the above validity tests—no discontinuity of observables at the threshold, irrelevance of additional covariates, zero treatment effect in the pre-treatment period—strongly support our evaluation strategy.

## 7 Conclusion

In this paper, we have provided first micro evidence about the effect of majoritarian electoral systems, as opposed to proportional systems, on the behaviors pursued by elected officials. We believe that the use of individual-level data is particularly interesting here, as it allows to identify the exact chain of causation that links the electoral rule to the macro policies implemented in representative democracies.

The particular features of Italian two-tier elections have allowed us to implement an RDD and estimate true causal effects of the electoral rule. Confirming the main results of the theoretical literature, we have shown that the majoritarian system increases targeted redistribution and reduces politicians' rents in a way that is both statistically significant and economically relevant.

The normative implications of our empirical findings are mixed. We find that the majoritarian system increases the possibility of monitoring politicians and their accountability, improving their commitment to parliamentary work. At the same time, however, the majoritarian system stimulates the adoption of pork-barrel projects, which may end up being over provided at the expense of broader, and more efficient, public goods.

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<sup>31</sup>To apply this falsification test we restrict our sample to those members observed at least once in the first two legislatures (X and XI) and once in the next three legislature (XII, XIII, XIV).

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## Tables and Figures

Table 1: Descriptive Statistics by Treatment Status (All Sample)

	Proportional	Majoritarian	Difference	-diff95%	+diff95%
Male	0.756	0.912	-0.156	-0.192	-0.120
Married	0.657	0.754	-0.097	-0.146	-0.048
Age	48.758	48.235	0.523	-0.555	1.602
Schooling	16.064	15.970	0.094	-0.171	0.359
Different Residency	0.091	0.034	0.057	0.034	0.081
Local Govt. Experience	0.427	0.563	-0.136	-0.192	-0.081
National Politician	0.272	0.207	0.065	0.019	0.111
Parliament Tenure	1.059	0.784	0.275	0.126	0.424
Incumbent	0.728	0.878	-0.150	-0.208	-0.093
Center-Right Coalition	0.486	0.419	0.067	0.012	0.122
Parl. Appointments	0.086	0.074	0.012	-0.018	0.042
White Collar	0.049	0.050	-0.001	-0.025	0.024
Lawyer	0.136	0.160	-0.024	-0.065	0.016
Manager	0.141	0.136	0.005	-0.034	0.043
Politician	0.215	0.178	0.037	-0.006	0.081
Entrepreneur	0.084	0.099	-0.015	-0.047	0.018
Teacher	0.106	0.090	0.017	-0.016	0.049
Self Employed	0.069	0.110	-0.041	-0.074	-0.007
Physician	0.052	0.089	-0.037	-0.067	-0.007
No. of Observations	405	1,317			

Note. 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*, *Schooling* (both expressed in years), and *Parliament Tenure* (expressed in number of terms). *Different Residency* stands for living in a different province with respect to the one of election; *Local Government Experience* stands for having had previous institutional experience at the local level (e.g., mayor of a city or president of a regional government); *Incumbent* refers to politicians who had been elected in the same region in the previous term. Job dummies refer to the pre-election occupation and are self-reported.

Table 2: Outcome Variables (All Sample)

	Proportional	Majoritarian	Difference	-diff95%	+diff95%
No. of Bills	8,077	8,497	-0.421	-1.829	0.987
No. of Targeted Bills	0.662	0.976	-0.315	-0.508	-0.121
Fraction of Targeted Bills	0.074	0.112	-0.038	-0.059	-0.016
No. of Observations	405	1,317			
Absenteeism Rate	0.367	0.305	0.059	0.034	0.083
No. of Observations	379	1,271			

Note. 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 *Fraction of Targeted Bills* is calculated over the total number of bills. Only bills presented as main sponsor are considered. *Absenteeism Rate* is the percentage of votes missed without any legitimate reason during the legislative term.

Table 3: Margin of Victory (*MV*)

	Proportional		Majoritarian		All	
	Obs.	Mean	Obs.	Mean	Obs.	Mean
<i>MV</i>	142	-12.71	1,317	13.59	1,459	11.03
$MV \in [-30, 30]$	126	-9.38	1,185	10.73	1,311	8.80
$MV \in [-20, 20]$	108	-6.54	995	8.04	1,103	6.61
$MV \in [-10, 10]$	84	-4.42	649	4.60	733	3.56
$MV \in [-5, 5]$	53	-2.64	363	2.34	416	1.70
$MV \in [-1, 1]$	10	-0.47	92	0.49	102	0.40

Note. Ministers excluded. *Margin of Victory* is expressed in percentage points.

Table 4: Share of Geographically Targeted Bills, RDD Estimation

	(I)		(II)		(III)		(IV)	
	All		All		$MV \in [-h, +h]$		$MV \in [-h, +h]$	
	Coeff.	P-value	Coeff.	P-value	Coeff.	P-value	Coeff.	P-value
Majoritarian	0.083	0.015	0.083	0.008	0.086	0.009	0.079	0.010
Male			0.021	0.144			0.022	0.295
Age			-0.001	0.223			0.000	0.796
Schooling			0.003	0.261			0.002	0.544
Diff. Residency			-0.029	0.340			0.017	0.767
Lawyer			0.011	0.538			-0.004	0.891
Manager			0.062	0.004			0.064	0.046
Politician			0.025	0.166			0.016	0.512
Entrepreneur			0.041	0.070			0.005	0.861
Teacher			0.038	0.062			0.023	0.416
Self Employed			0.058	0.005			0.046	0.13
Physician			-0.017	0.425			-0.027	0.364
Parliament Tenure			0.003	0.698			0.000	0.993
Incumbent			0.016	0.173			0.017	0.328
Local Govt. Exp.			0.033	0.001			0.031	0.028
National Politician			-0.024	0.078			-0.025	0.172
Parl.Appointments			-0.046	0.022			-0.069	0.003
Center-Right			-0.015	0.178			-0.008	0.615
Region of Election	no		yes		no		yes	
Legislative Term	no		yes		no		yes	
No. of Proportional	142		142		95		95	
No. of Majoritarian	1,317		1,317		767		767	
No. of Observations	1,459		1,459		862		862	

Note. 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 (third-grade) polynomial approximation. Models (III) and (IV): local linear regression (where  $h=13$  is the optimal bandwidth selected using the cross-validation method). Standard errors are clustered at the individual level. See Table 1 for a description of covariates.

Table 5: Absenteeism Rate, RDD Estimation

	(I)		(II)		(III)		(IV)	
	All		All		$MV \in [-h, +h]$		$MV \in [-h, +h]$	
	Coeff.	P-value	Coeff.	P-value	Coeff.	P-value	Coeff.	P-value
Majoritarian	-0.149	0.004	-0.112	0.017	-0.135	0.001	-0.114	0.004
Male			0.006	0.759			-0.007	0.775
Age			-0.002	0.002			-0.004	0.000
Schooling			0.001	0.733			0.002	0.534
Diff. Residency			0.122	0.006			0.103	0.085
Lawyer			0.055	0.011			0.037	0.151
Manager			0.009	0.694			-0.018	0.521
Politician			0.005	0.825			-0.004	0.898
Entrepreneur			0.021	0.393			-0.025	0.414
Teacher			-0.030	0.174			-0.064	0.018
Self Employed			0.010	0.663			0.022	0.457
Physician			0.038	0.114			0.023	0.442
Parliament Tenure			0.041	0.000			0.049	0.000
Incumbent			-0.026	0.090			-0.043	0.020
Local Govt. Exp.			-0.030	0.010			-0.029	0.046
National Politician			0.084	0.000			0.072	0.001
Parl. Appointments			0.028	0.299			0.035	0.332
Center-Right			0.055	0.000			0.073	0.000
Region of Election	no		yes		no		yes	
Legislative Term	no		yes		no		yes	
No. of Proportional	135		135		94		94	
No. of Majoritarian	1,271		1,271		816		816	
No. of Observations	1,406		1,406		910		910	

Note. 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 (third-grade) polynomial approximation. Models (III) and (IV): local linear regression (where  $h=15$  is the optimal bandwidth selected using cross-validation method). Standard errors are clustered at the individual level. See Table 1 for a description of covariates.

Table 6: Balance Tests near the Threshold,  $MV \in [-5, 5]$

	Proportional	Majoritarian	Difference	-diff95%	+diff95%
Male	0.941	0.922	0.019	-0.059	0.096
Married	0.804	0.784	0.020	-0.101	0.141
Age	48.353	48.640	-0.287	-3.040	2.467
Schooling	16.431	16.158	0.273	-0.381	0.928
Different Residency	0.078	0.030	0.048	-0.007	0.103
Local Govt. Exp.	0.529	0.568	-0.038	-0.185	0.108
National Politicians	0.137	0.166	-0.029	-0.138	0.080
Parliament Tenure	1.098	0.579	0.519	0.201	0.837
Incumbent	0.431	0.335	0.096	-0.044	0.236
Center-Right Coalition	0.529	0.479	0.050	-0.097	0.197
Parl. Appointments	0.059	0.075	-0.016	-0.093	0.061
White Collar	0.039	0.047	-0.008	-0.070	0.054
Lawyer	0.196	0.191	0.005	-0.111	0.121
Manager	0.196	0.119	0.077	-0.021	0.175
Politician	0.157	0.144	0.013	-0.091	0.117
Entrepreneur	0.118	0.094	0.023	-0.064	0.111
Teacher	0.078	0.114	-0.035	-0.127	0.057
Self Employed	0.039	0.125	-0.085	-0.179	0.008
Physician	0.098	0.091	0.007	-0.079	0.092
No. of Observations	51	361			

Note. Ministers excluded. *-diff95%* and *+diff95%* represent the lower and upper bound of the 95% confidence interval of *Difference*, respectively.

Table 7: Balance Tests with Polynomial Approximation

	Discontinuity	P-value	Obs.
Male	-0.082	0.265	1,459
Married	-0.075	0.510	1,459
Age	1.193	0.597	1,459
Schooling	-0.334	0.542	1,459
Different Residency	-0.049	0.540	1,459
Local Gov. Exp.	0.008	0.951	1,459
National Politicians	0.068	0.497	1,459
Parliament Tenure	-0.627	0.119	1,459
Incumbent	-0.055	0.675	1,459
Center-Right Coalition	-0.221	0.101	1,459
Parl. Appointments	-0.040	0.621	1,459
White Collar	0.015	0.592	1,459
Lawyer	0.073	0.441	1,459
Manager	-0.138	0.253	1,459
Politician	0.011	0.902	1,459
Entrepreneur	-0.135	0.156	1,459
Teacher	0.085	0.276	1,459
Self Employed	0.178	0.000	1,459
Physician	-0.043	0.594	1,459

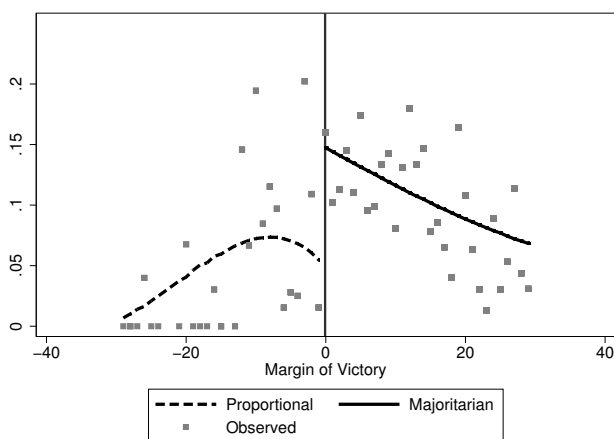
Note. Ministers excluded. Each of the listed variables is used in a regression with a dummy for the discontinuity point, third-grade polynomial terms, and a full set of interactions.

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

	Discontinuity	P-value	Obs.
Targeted Bills X	-0.002	0.643	81
Targeted Bills XI	0.052	0.659	255
Targeted Bills X-XI	0.041	0.649	271

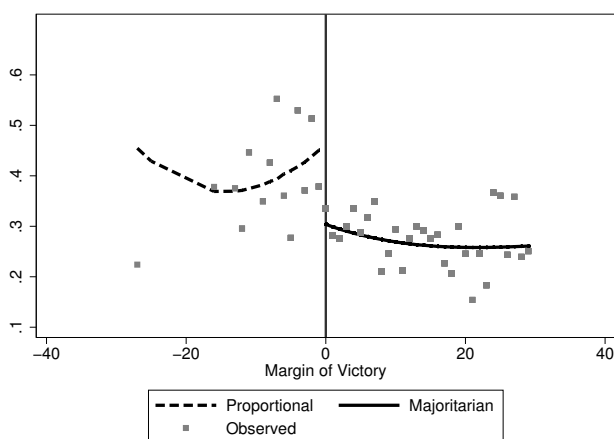
Note. Ministers excluded. Dependent variables: percentage of bills targeted to the region of election over the total number of bills presented in the X legislative term, XI legislative term, or both. Each of these three dependent variables is used in a regression with a dummy for the (future) discontinuity point, third-grade polynomial terms, and a full set of interactions.

Figure 1: Share of Targeted Bills, RDD Fitted Values



Note. RDD estimation with third-grade polynomial terms and interactions included. Dependent variable: percentage of bills targeted to the region of election over the total number of bills presented. Observed points are averaged over each 1-unit interval of margin of victory.

Figure 2: Absenteeism Rate, RDD Fitted Values



Note. RDD estimation with third-grade polynomial terms and interactions included. Dependent variable: absenteeism rate (i.e., the percentage of votes missed without any legitimate reason). Observed points are averaged over each 1-unit interval of margin of victory.