// NO.20-002 | 01/2020

DISCUSSION PAPER

// MATTEO ALPINO

Mitigating the Tradeoff between Proportionality and Accountability in Electoral Systems: Evidence from the Italian Senate 1994 – 2006

Leibniz Association

ZEW

Mitigating the tradeoff between proportionality and accountability in electoral systems: Evidence from the Italian Senate 1994-2006

Matteo Alpino*

January 14, 2020

Abstract

First-past-the-post elections in single-member districts make legislators more accountable to their district of election compared to proportional electoral systems. Accountability makes politicians more sensitive to voters' preferences when deciding where and how to allocate public expenditure, and also reduces rent extraction. On the other hand, first-past-the-post elections generate over-representation of majority parties in parliament, potentially hurting minorities and democratic legitimacy. The mixed system used for Italian Senate elections in 1994, 1996 and 2001 mitigates this tradeoff: 3/4 of the seats are assigned to winners in single-member district elections (majoritarian tier), while the rest to the best runners-up based on party-level vote counts (proportional tier). The system mechanically compensates opposition parties, while keeping all legislators equally accountable to their district. In fact, our empirical analysis based on close elections does not find significative differences in targeting of legislative activity to the district, and in absenteeism between senators of different tiers, contrary to what other studies find for mixed systems with two separate ballot lists.

JEL CODE: D72.

Keywords: electoral rules; mixed electoral systems; comparative political economy.

*Bank of Italy, Corso Cavour, 4, 70121 Bari, Italy. E-mail: alpino.mtt@gmail.com. I thank Giuseppe Albanese, Zareh Asatryan, Guglielmo Barone, Raffaello Bronzini, Friedrich Heinemann, Maurizio Lozzi, Tommaso Nannicini, and Paolo Sestito for comments. The views expressed in this paper are my own and do not necessarely reflect those of the Bank of Italy. Declarations of interest: none.

1 Introduction

The two most common electoral systems worldwide are: plurality elections in single-member districts (common in English-speaking countries), and proportional representation in multi-seat constituencies (common in European countries[1]). Existing research has found that adopting one or the other has effects on political representation, economic policy outcomes, and legislators' incentives.[2] Politicians elected in single-member districts are more visible and thus accountable to their constituency. As such, they have more incentives to focus their legislative activity on issues important for their district. This in turn affects the composition of public spending in favour of local public goods and geographically targeted transfers, at the expense of national public goods and broad redistribution.[3] Furthermore, higher accountability makes harder for politicians to extract rents (e.g. in the form of shirking, corruption, or clientelism). On the other hand, systems with single-member districts mechanically result in overrepresentation of large parties relative to their vote shares (winner-take-all rule), and thus penalize smaller parties and political representation. Scholars in electoral engineering thus recognize that the choice of electoral systems necessarely involves some tradeoffs, some of which are listed in Table 1.

Table 1: Some posited tradeoffs between outcomes: plurality versus proportional rules.

Plurality elections in single-seat constituencies	Proportional representation in multiseat constituencies
Single-party governments:	Coalition governments with several political parties:
decisive outcomes and greater government accountability	representation of the entire spectrum of political views
direct accountability of individual politicians to their constituents	stronger and more cohesive political parties
greater political stability	higher ideological congruence between government and citizens
fewer ideological extremists in parliament	**proportionality of seats and votes**
lower budget deficits	enhanced representation of women and racial and ethnic minorities
lower taxes	higher public spending and more public goods and services
	lower economic inequality

Source: Grofman (2016). Note: Bold emphasis added.

In this paper, I analyze empirically how the unique hybrid electoral system in place in the Italian Senate (*Senato della Repubblica*) between 1994 and 2001 affects legislators' behaviour in two dimensions: geographical targeting of their legislative

[1] E.g. Belgium, Netherlands, Norway, Sweden, Denmark, Greece, Spain.

[2] See Section 1.1 for a literature review.

[3] The welfare implications of targeting legislative activity to specific geographical constituencies is ambiguos, and depends on how local public goods are valuable relative to national ones.

activity, and abstenteeism. My findings suggest that this system mitigates one of the most important tradeoffs between the two systems above (in bold in Table 1): it corrects downward the overrepresentation of the majority, while keeping legislators accountable, and focused on their electoral district in terms of legislative activity.

The system assigns 3/4 of the seats to winners in single-member district races. The remaining seats are assigned according to proportional representation. Unlike other mixed systems, most notably the one used in the Italian House (*Camera dei Deputati*) in the same period, in the German parliament (*Deutscher Bundestag*), and in both Italian House and Senate since 2018, the remaining seats are not assigned among candidates in a separate list, but among runners-up from the single-district races. Thus, on one hand the assignment mechanism mechanically reduces the overrepresentation of the party with more district winners (the majority party) by assigning the extra seats to minority parties, that is those with less district winners. On the other hand, both types of elected legislators (winners and runners-up) have gone through the exact same electoral competition, and have endured the same visibility in the eyes of a well defined geographical constituency. As such, we can expect the two types to be equally accountable, and thus to not behave differently in terms of targeting legislative activity, and shirking.

To test this hypothesis, I rely on a Regression Discontinuity Design (RDD) which exploits close elections between winners and runners-up, who both result elected in the Senate. Using micro data at the legislator level on absenteeism rate and on the share of sponsored bills targeted to their state of election, I do not find significant differences between winners and runners-up. My findings are particularly meaningful when compared to the evidence in a related work by Gagliarducci, Nannicini, and Naticchioni (2011), who analyse behaviour of members of the Italian House in the same period. At the time, the House also had a mixed electoral system that, contrary to the Senate, featured two completely separate tiers with separate ballots and candidates: one with single-member districts, the other with proportional representation in closed lists. Exploiting close elections and the fact that candidates could run for both tiers,

Gagliarducci et al. (2011) find that legislators elected in single-member districts target more their legislative activity to the district of election, and show lower abstenteeism, as compared to their colleagues elected under closed-list proportional rule in the other tier. Using data from the same source and a very similar empirical strategy, I do not find the same differences between winners and runners-up in the Senate.

My findings are informative for policy-makers interested in designing an electoral system which guarantees (somewhat) proportional representation of parties in parliament, while keeping its members accountable to a specific geographic constituency. Compared to a standard single-member majoritarian system, the mixed system in place in the Italian Senate between 1994 and 2001 generates a seat distribution more proportional to the underlying vote distribution. At the same time, it keeps all legislators equally accountable and visible to their constituents, contrary to what happens in mixed systems made of two majoritarian and proportional tiers with separate ballots.

1.1 Literature

The present paper contributes to the literature on comparative electoral systems, especially to the stream analysing differences between proportional and majoritarian systems. The theoretical literature has used different political economic models to analyze the impact of these systems on the provision of broad versus targeted public goods (Lizzeri and Persico, 2001; Persson and Tabellini, 2000; Milesi-Ferretti, Perotti, and Rostagno, 2002). Despite differences in modelling choices, these papers share the prediction that majoritarian systems result in more targeted public spending, and in lower expenditure in broad public goods. Milesi-Ferretti, Perotti, and Rostagno (2002) and Funk and Gathmann (2013) find evidence consistent with this prediction using, respectively, a panel of countries and of Swiss cantons. The model by Persson and Tabellini (2000) also predicts that politicians elected in majoritarian system extract less rents than those elected in proportional systems. Using a different model, Myerson (1999) shows that this prediction holds true only if the greater accountability of the majoritarian system more than compensates the fact that proportional systems have

lower entry-barrier for honest politicians.

More recently economists have exploited the existence of mixed systems with a majoritarian and a proportional tier to test the predictions above. Gagliarducci et al. (2011) are the first to provide causal evidence on the effect of majoritarian electoral system relative to proportional system using politician-level data. As discussed in the introduction, they exploit the mixed system of the Italian House in the period 1994-2001, which featured two separate tiers with separate ballots. Using an RDD based on close elections, they find that politicians elected in single-member districts (majoritarian tier) target more their legislative activity to the district of election, and show lower abstenteeism, as compared to their colleagues elected under closed-list proportional rule (proportional tier). The estimated effect are quite large (30% for abstenteeism, 100% for targeting). Maaser and Stratmann (2018) analyse politicians' behaviour in German state parliaments, whose electoral systems featured two separate tiers with different lists. They find that politicians elected in first-past-the-post elections are more likely to be members of parliamentary committees which are crucial for targeted redistribution. Other papers exploit laws mandating different systems for municipalities above or below certain population threshold. Pellicer and Wegner (2013) compare single-member districts to proportional representation in closed list elections in Morocco, and find that the former favor clientelistic parties over programmatic parties. Eggers (2015) compares at-large elections with plurality voting to proportional representation with majority bonus in France, and finds turnout higher in the latter. Very recently Albanese, Cioffi, and Tommasino (2019) have analyzed legislators' behaviour in the Italian Senate following the reform from the system analyzed in the present paper to a closed-list proportional system. They find that legislative targeting to districts, and legislative effort both drop after the reform.

The rest of the paper is structured as follows. Section 2 describes the details of the electoral rules in the Italian Senate and House in the period under consideration; Section 3 lays out the research question and the identification strategy; Section 4 presents the empirical findings; finally, Section 5 concludes.

2 The Italian two-tier electoral system

Italy is a parliamentary democracy. The parliament is composed of two chambers: the House and the Senate. The two chambers differ in size, and in the eligibility criteria to elect and to be elected. The 630 House members must be at least 25 years old, and are elected by voters older than 18 years old; the 315 Senators must be at least 40 years old and are elected by voters older than 25 years old. The two chambers of parliament have the same political powers. In particular, legislative bills must be approved separately by both chambers, and the government must win a vote of confidence separately in both chambers. Elections to choose members of the two chambers have always been held on the same day, although in principle scattered elections are possible. All the features described so far are prescribed by the Constitution, and have not been revised since its adoption in 1946. On the contrary, electoral rules for the parliament do not have constitutional rank, and have changed frequently over time. The focus of the present paper is on the electoral system in place during the XII (1994-1996), XIII (1996-2001) and XIV (2001-2006) legislatures. In this period the electoral rule differed across the House and the Senate, but members of both chambers were elected under a two-tier system. Candidates running for a seat in the House were forbidden to run for the Senate, and vice versa.

Electoral rule in the Senate According to the electoral rule, the country was divided in 232 single-member districts. Voters received only one ballot for the Senate to cast one vote for one candidate in their single-member district. Each party might field at most one candidate in each of the 232 districts. Out of the total 315 seats, 232 were assigned to the candidates who received the most votes in their district (first place finishers). Furthermore, 83 additional seats were assigned to some runners-up[4] according to a peculiar proportional rule, designed to partly compensate opposition parties. According to this rule, popular votes at the the state[5] level for each party

[4] I adopt the definition of "runner-up" by the Merriam-Webster dictionary: *"the competitor that does not win first place in a contest"*. Thus the definition is not restricted to second place finishers.

[5] The 20 states (*Regioni*) are the highest-order level of sub-national government in the Italian system.

were calculated without considering the votes earned by the first-ranked candidates. Available seats in each state were then allocated to parties proportionally to these popular vote counts using the D'Hondt method. Within each party, the seats were assigned to the runners-up who received the highest vote shares in their districts.

A simple example helps to clarify this complicated electoral rule. An exemplar state is composed of three districts, and assigns three seats in the majoritarian tier, and two in the proportional tier. In each district, four parties field candidates. The absolute number of votes received by each candidate is reported in Table 2. The three candidates who collect the highest number of votes in each district are elected in the majoritarian tier (in bold in Table 2). The sums in the fifth column, Σ_{losers}, are calculated disregarding the votes obtained by the three winners. Based on the votes collected by the losers, the D'Hondt method assigns one seats in the proportional tier to the Liberals ($\Sigma_{losers} = 450$), and one to the Republicans ($\Sigma_{losers} = 450$).[6] The seat of the Liberals is assigned to the candidate ranked second in district 3, as her vote share (25%) is higher than those gained by her party colleagues in the other districts (0% and 20%). The seat of the Republicans is assigned to the candidate ranked third in district 1, as her vote share (25%) is higher than those gained by her party colleagues in the other districts (0% and 20%).

Table 2: Electoral rule in the Senate: example

Party	District 1	District 2	District 3	Σ_{losers}	$0.5\times\Sigma_{losers}$
Democrats	**450**	200	**500**	200	100
Republicans	250	0	200	450	225
Liberals	0	200	250	450	225
Populists	300	**600**	50	350	175
Total	1000	1000	1000		

Note: votes by party and district. In bold candidates elected as first place finishers (majoritarian tier); in boxes candidates elected as runners-up (proportional tier).

Under this system, the first ranked candidates own their election only to their performance in the district. On the contrary, elected in the proportional tier own their

[6]The D'Hondt method to allocate x seats works as follows. The sum of the losers in each party, Σ_{losers}, are divided by the integers between 1 and x. Then, the x highest of these quotients receive a seat. In the example $x = 2$, and the quotients are reported in the last two columns of Table 2.

election to the overall party performance at the state level (which also depends on their own performance, the more so the fewer the districts in the state), and also on their personal performance relative to their party colleagues in the other districts of the same state. For both types, election also depends on the assignment to safer versus more competitive districts decided by the party leadership. In the Senate also the members elected in the proportional tier have a strong attachment to their geographical district and to their state, and are likely to have less obligations vis-a-vis their party leadership.

Electoral rule in the House In the House, 75% of the seats were allocated according to plurality voting in 475 first-past-the-post single-member districts, while the remaining 25% was allocated under proportional representation. Voters received two ballots for the House: one to vote their preferred candidate in their single-member district, and another to cast a vote for a party in the proportional tier. The 475 single-member districts were geographically contiguous ad-hoc districts comparable to districts in the US House or in the UK House of Commons. In the proportional tier, seats were allocated to parties according to the vote shares at the national level.[7] The seats won by each party in the proportional tier at the national level were distributed to candidates in closed party lists across 26 multi-member districts (2 to 12 seats per district). Most multi-member districts corresponded to a state, although larger states were split in two or three. Politicians were forbidden to run in more than one single-member district. However, they were allowed to contemporaneously run in one single-member district in the majoritarian tier, and in one multi-member district in the proportional tier. Candidates elected in both tiers were forced to accept the seat in the majoritarian tier.[8] Legislators elected in different tiers had arguably different incentives and responsibilities in the eyes of the public opinion. Those elected in the majoritarian tier were arguably perceived as the representative of their district, i.e. of a specific geographic constituency. They owned their election to their personal

[7] Using the Hare-Niemeyer method.

[8] Gagliarducci et al. (2011) exploit this last feature for identification using a Regression Discontinuity Design (RDD). Candidates running in both tiers were assigned to the majoritarian rather than the proportional one based on the margin of victory in their single-member districts.

performance in the district-level election. They also partly owned their election to the party leaders, who decide which candidates to assign to safer or more competitive districts. On the contrary, those elected in the proportional tier owned their seats only to their position in the closed-list, that is decided by the party leadership.

3 Research question and empirical strategy

The aim of this paper is to test whether senators elected as district winners (majoritarian tier) behave differently from those elected among the runners-up (proportional tier) under the electoral system in place for Italian Senate elections in 1994, 1996 and 2001. Existing evidence from mixed systems with completely separate tiers and ballots (e.g. Italian House in the same period, German parliament, Scottish parliament) has found that legislators elected in single-member districts focus their legislative activity more to their constituents, shirk less (Gagliarducci et al., 2011), and are more likely to enter committees crucial for targeted redistribution (Maaser and Stratmann, 2018) than those elected in the proportional tier.

I hypothesize that these differences do not arise in the system analyzed here, because both types of senators own their election to their own performance in their district, and because both go through the same election process, which involves the same degree of visibility and attachment to a specific geographic constituency. I formulate two hypotheses:

Hypothesis 1: Senators elected as runners-up (proportional tier) do not carry out less geographically targeted policies than those elected among first place finishers (majoritarian tier).

Hypothesis 2: Senators elected as runners-up (proportional tier) do not extract more rents than politicians elected among the first place finishers (majoritarian tier).

3.1 Identification

The identification strategy is adapted from Gagliarducci et al. (2011). The sample is composed of the universe of the elected senators. I define the treatment variable D_i equal to 1 if Senator i was elected as first-ranked in his district, and equal to 0 if he was elected as one of the runners-up. Treatment assignment depends on the margin of victory V_i between candidate i and the winning candidate in his single-member district. First of all, identification of causal effect of D_i requires the standard continuity assumption.

Assumption RDD: The regression functions

$$\lim_{v \downarrow 0} \mathbb{E}[Y_i|V_i] \quad \text{and} \quad \lim_{v \uparrow 0} \mathbb{E}[Y_i|V_i]$$

are continuous at the threshold $V_i = 0$.

Consider now the treatment assignment mechanism: $D_i = 1(V_i \geq 0)$, where $1(.)$ is the indicator function. The treatment assignment is more involved on the left side of the threshold: if $V_i < 0$, we have either $D_i = 0$ (if i was elected as runner-up), or $D_i = .$ (if i was not elected). This treatment assignment mechanism asks for an additional assumption. In particular, I must assume that in a left-neighborhood of the threshold, senators elected as runners-up are a representative sample of the population of all candidates who did not rank first in their district. Let us define U_i the individual-level unobservable characteristics. Formally the assumption reads:

Assumption LHS

$$\lim_{v \uparrow 0} \mathbb{E}[U_i|V_i = v, D_i = 0] = \lim_{v \uparrow 0} \mathbb{E}[U_i|V_i = v, D_i = .\,].$$

To assess the credibility of this assumption, we must consider the mechanism that assigns seats to runners-up. Conditional on $V_i < 0$, seat assignment to candidate i hinges on two factors. First, on total votes earned by the candidates (excluding the first-ranked) affiliated with the same party as i in the same state (including i himself)

relative to those in other parties. This determines the number of seats, if any, assigned to the party affiliated with i in his state. Second, seat assignment to i hinges on his own performance relative to other runners-up in his own party in the same state. This determines who gets the additional seats within each party, if any seat is available. In other words, seat assignment to losers depends on the absolute vote share for candidate i, which I define W_i, and on the electoral results in the other districts of the same state. The variables W_i and V_i are correlated, but they are different as the margin victory also depends on the vote share earned by the candidate with most votes in each district. To see why, consider the simple example illustrated in Table 3, which reports vote shares for an exemplar state that assigns two majoritarian seats to the Democrats, and one proportional seat to the Republicans. Both Republican candidates have the same margin of victory V_i (-10%), but different vote shares W_i (41% and 31%).

Table 3: Seat assignment to losers: example

Party	District 1	District 2	Σ_{losers}
Democrats	**51**	**41**	0
Republicans	41	31	72
Liberals	8	28	36
Total	100	100	
$V_{Republican}$	-10%	-10%	

Note: votes by party and district. $V_{Republican}$ is the margin of victory from the perspective of the Republican candidate. In bold candidates elected in the majoritarian tier; in boxes candidates elected in the proportional tier.

In general, conditional on the same negative margin of victory, candidates who earn higher vote shares have more chances to be elected. Furthermore, we must also assume that there are no spillovers across districts in the same state. In order words, candidate i can not affect the results in other districts of the same state. The discussion in this section highlights that Assumption LHS is not innocuous. However, I provide ample evidence in support of its validity in section 4.1.

3.2 Data

The dataset used by Gagliarducci et al. (2011) appears to be part of a larger research project. The website of one of the authors makes available a dataset with information on the Italian members of parliament from 1987 to 2008.[9] I download the dataset and restrict my attention to Senators elected between the XII and XIV legislatures (elections in 1994, 1996 and 2001). Among others, I have access to the following informations which I employ in the analysis: electoral-tier, district of election, vote share, age, gender, number of kids, marital status, previous job, years of education, state of residence, experience in local governments, party affiliation, details on bills sponsorship and electronic parliament votes missed without any legitimate reason.

I construct the outcome variables as in Gagliarducci et al. (2011): the measure of rent is the absenteeism rate; the measure of redistribution is the share of bills targeted to the state of election over total bills proposed. The official classification of the parliament is used to determine whether bills are geographically targeted or not. Examples of geographically targeted bills are laws to protect ethnic minorities living in a specific state, the creation of a national park, or of a new province. Examples of general interest bills are reforms to the national welfare state or the national education system. Both outcome variables are only available at the term level, and not year by year. The summary statistics of the outcome variables are reported in Table 4; summary statistics of the other variables used in the analysis including the running variable are reported in Table 5.

Table 4: Summary statistics of outcome variables

	Prop.	Maj.	Δ	Sample	Obs. prop.	Obs. maj.
Targeted bills	0.13	0.11	0.02	Full	247	694
Targeted bills	0.15	0.11	0.04**	CCT	183	398
Absenteeism	0.49	0.35	0.14**	Full	247	689
Absenteeism	0.48	0.38	0.10***	CCT	132	236

Note: Targeted bills is the share of bills targeted to the state of election as a fraction of total bills proposed over the term. Absenteeism is the fraction of electronic parliament votes missed without any legitimate reason. CCT refers to the sample in the optimal bandwidth for local linear regression selected using the algorithm by Calonico, Cattaneo, and Titiunik (2014b).

[9] http://www.tommasonannicini.eu/en/works/?category=datasets-experiments

Table 5: Summary statistics of other variables

	Proportional	Majoritarian	Δ
Margin of victory	-8.78	12.35	-21.13***
Vote share	34.41	45.37	-10.96***
Age in years	53.49	54.39	-0.90
Male	0.89	0.92	-0.03
Married	0.83	0.82	0.01
Number of kids	1.81	1.75	0.07
Lawyer	0.15	0.13	0.02
Not resident in region	0.09	0.11	-0.02
Years of schooling	15.97	16.35	-0.37**
Director	0.11	0.13	-0.02
Politician	0.04	0.03	0.01
Entrepreneur	0.09	0.09	0.00
Teacher	0.11	0.08	0.03
Self employed	0.10	0.09	0.01
Freshman	0.53	0.46	0.07*
Physician	0.07	0.08	-0.01
Local government experience	0.64	0.56	0.08**
Centre-right	0.52	0.53	-0.01
Majority status	0.30	0.58	-0.27***
District in North	0.49	0.50	-0.00
Observations	247	691	

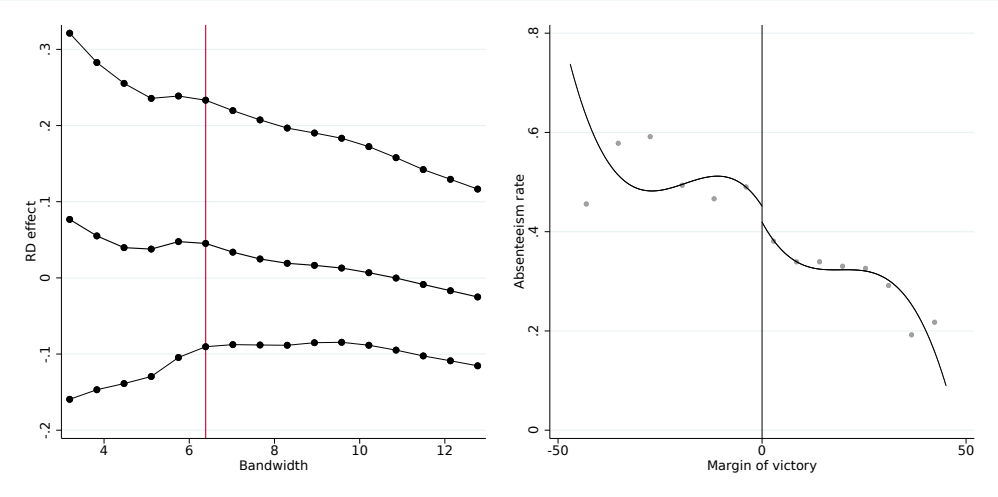
Note: Full sample. Stars denote significance at the 0.10 / 0.05 / 0.01 level.

4 Estimation and findings

In this section, I assume that Assumptions RDD and LHS hold. Validity tests aimed at supporting the credibility of the empirical strategy are presented in the next section. For each of the two outcomes, I estimate the RD effect with linear local polynomial estimator and triangular kernel using sixteen different bandwidths, including the optimal bandwidth by Calonico, Cattaneo, and Titiunik (2014b). The smallest bandwidth is half this optimal bandwidth, and the largest is double the optimal bandwidth. I report graphically the point estimates alongside the 95% robust confidence intervals by Calonico, Cattaneo, and Titiunik (2014b) as a function of the bandwidth (exact figures at the optimal bandwith are reported in Table 6). I also report standard RD graphs that plot local averages of the outcome variables in bins of the margin of victory, together with a third order global polynomial fitted separately on the two sides

of the threshold. The bins are automatically selected using the MSE-optimal evenly spaced method with spacings estimators by Calonico, Cattaneo, and Titiunik (2014a). In Figure 1 the outcome variable is the absenteeism rate; in Figure 2 the outcome variable is the share of targeted bills.

Figure 1: Effect on absenteeism rate

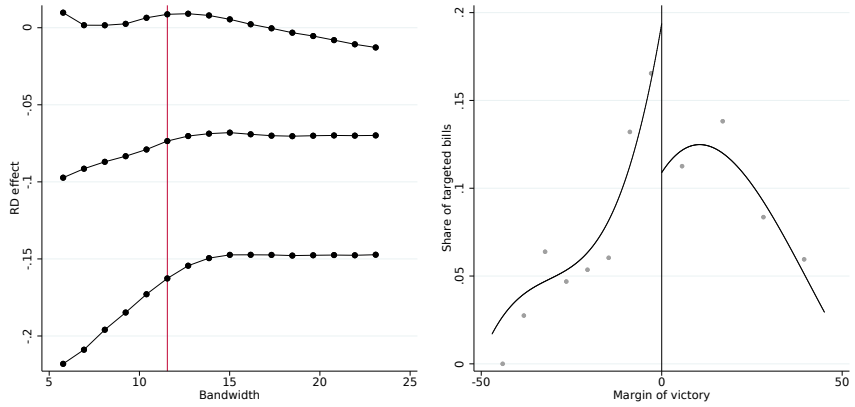


Note: the outcome variable is the absenteeism rate. The left panel reports RD estimates and 95% robust confidence interval plotted against the bandwidth used. Vertical red lines indicate the optimal bandwidth by Calonico et al. (2014b). Estimation by linear local polynomial estimator with triangular kernel. The right panel plots local averages of the outcome variables in bins of the margin of victory, along side a third order global polynomial fitted separately on the two sides of the threshold. The bins are automatically selected using the IMSE-optimal evenly spaced method using spacings estimators by Calonico et al. (2014a).

The estimated effect on absenteeism rate is not significantly different from zero at the 95% level for any of the bandwidths considered (between 3 and 13). The point estimate is 4.5 percentage points at the optimal bandwidth. The point estimates are positive for small bandwidths and turn negative for large bandwidths. From the graphical analysis, there is arguably no visible discontinuity at the threshold. In short, there is no evidence that proportional members shirk at an higher rate relative to their colleagues in the majoritarian tier, contrary to what found by Gagliarducci et al. (2011) in the House.

The estimated effect on the share of targeted bills is negative and significantly different from zero at the 90% level for all the bandwidths considered (between 6 and 23), and also the 95% level for large bandwidths (higher than 15). The size of the effect is between -10 and -7 percentage points. At the optimal bandwidth, the

Figure 2: Effect on the share of targeted bills



Note: the outcome variable is the share of targeted bills. The left panel reports RD estimates and 95% robust confidence interval plotted against the bandwidth used. Vertical red lines indicate the optimal bandwidth by Calonico et al. (2014b). Estimation by linear local polynomial estimator with triangular kernel. The right panel plots local averages of the outcome variables in bins of the margin of victory, along side a third order global polynomial fitted separately on the two sides of the threshold. The bins are automatically selected using the IMSE-optimal evenly spaced method using spacings estimators by Calonico et al. (2014a).

effect is -7 percentage points, which corresponds to a 45% decrease over the sample mean calculated in the bandwidth to the left of the threshold. The RD plot shows a downward jump in the estimated conditional expectation function at the threshold, although the extent of the jump might be exaggerated by the polynomial specification. A conservative way of interpreting these estimates is that there is no evidence of a positive effect of being elected in the majoritarian tier, contrary to what estimated by Gagliarducci et al. (2011) for the House. If anything, the effect is negative.

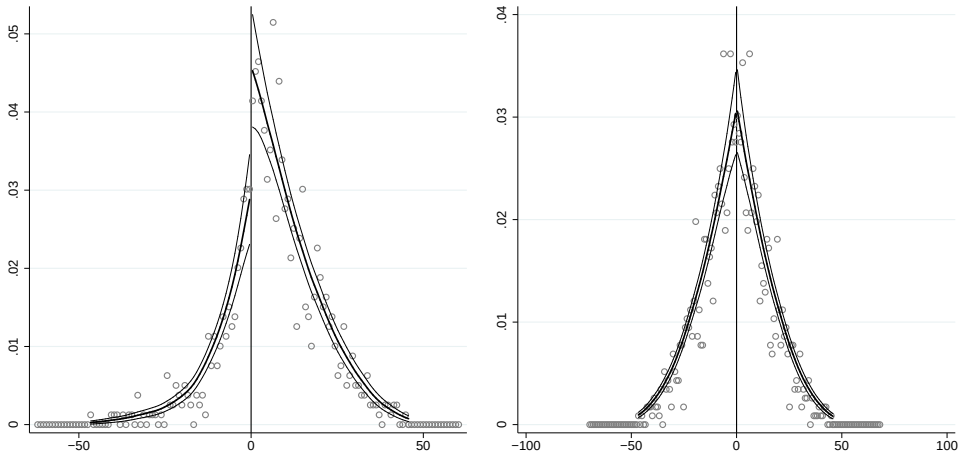
4.1 Validity tests

This section presents an array of exercises aimed at supporting the credibility of the empirical strategy, as well as a battery of robustness tests.

Density test A standard validity test in RD studies is the McCrary (2008) density test to detect manipulation in the running variable. Applications to close election are not particularly prone to problems of manipulation of the running variable, as all candidates have strong incentives to win as many votes as possible. However, in our

case the test is likely to detect a discontinuity in the density of the running variable. The reason is that all the losers who did not get assigned a seat as best losers do not appear in the sample. As such, many observations are missing from the distribution of V_i on the left side of the threshold. Indeed, the test calculated on our sample detects the expected break in the density function (left panel of Figure 3): the mass on the right side of the threshold is larger than the mass on the left hand side. How to check that this is indeed due to losers dropping out of the sample on the left-hand side, rather than to special candidates sorting on the right hand side (for example due to electoral fraud)? I have access to data on the vote shares of all the candidates who ranked second in their district, but who did not get assigned a seat. Thus I can re-introduce these vote shares in the distribution of V_i. The new distribution includes the vote shares of all the first-ranked candidates, all the second-ranked candidates, and all the candidates ranked below second who did get assigned a seat. As such, it is likely that in a left-neighborhood of the threshold, very few candidates are missing. When I estimate the McCrary (2008) density using the new distribution of V_i, the test fails to detect any discontinuity at the threshold (right panel of Figure 3). As such, there is no evidence of manipulation of the running variable.

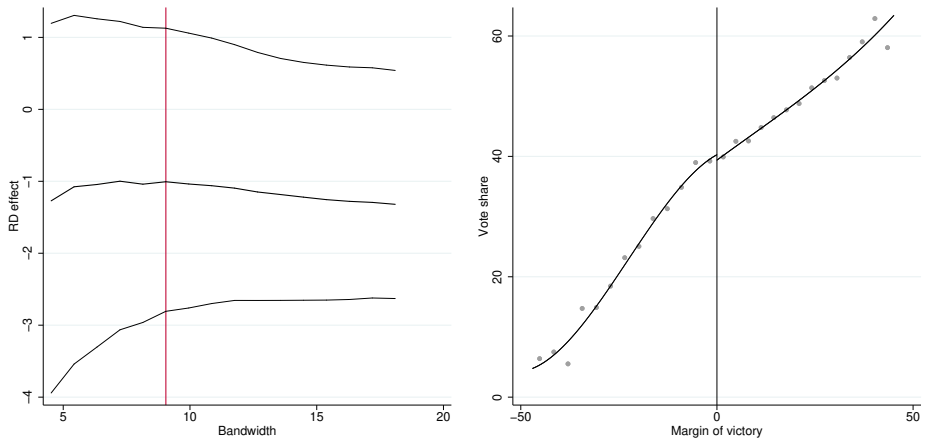
Figure 3: McCrary (2008) density.



Note: estimates of density function of the margin of victory. The left panel is the density of the margin of victory in the population of the elected senators. The right panel is the density of the margin of victory in the population of: all first-ranked candidates; all second-ranked candidates; and all candidates ranked below the second place who received a seat.

Tests of Assumption LHS The discussion in Section 3.1 highlights why Assumption LHS may be violated. The key concern is that conditional on the same negative value of the margin of victory, candidates with higher absolute vote share are more likely to get assigned a seat. If this is empirically relevant, senators on the left-hand side of the threshold, $D_i = 0$, would have on average higher vote shares than the candidates who did not get assigned a seat, $D_i = .$, and assumption LHS would be violated. This imbalance would imply that the vote share should exhibit a discontinuity at the threshold $V_i = 0$. We can test if this is the case using the same RD specification as in the baseline and the vote share as outcome variable. Reassuringly, I find no evidence of such a discontinuity, as apparent from the estimates in Figure 4.

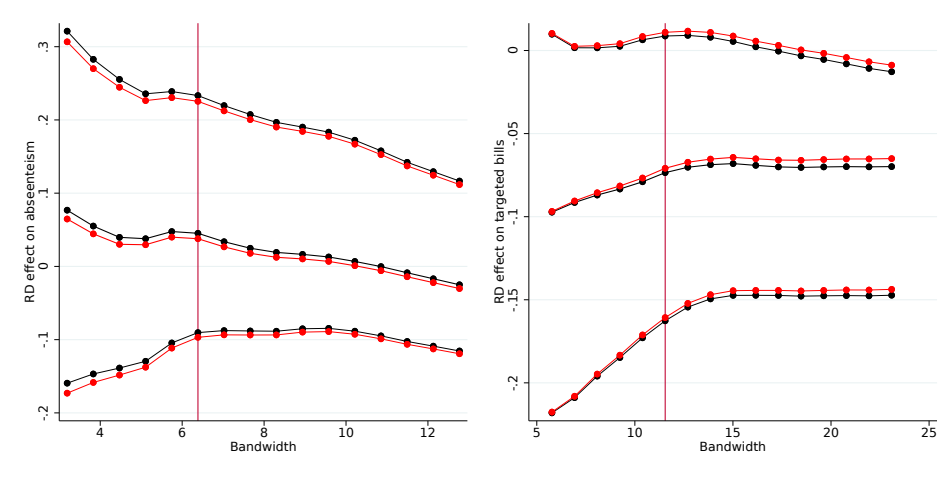
Figure 4: Discontinuity in vote share.



Note: the outcome variable is the absolute vote share; the running variable is the margin of victory. The left panel reports RD estimates and 95% robust confidence interval plotted against the bandwidth used. Vertical red lines indicate the optimal bandwidth by Calonico et al. (2014b). Estimation by linear local polynomial estimator with triangular kernel. The right panel plots local averages of the outcome variables in bins of the margin of victory, along side a third order global polynomial fitted separately on the two sides of the threshold. The bins are automatically selected using the IMSE-optimal evenly spaced method using spacings estimators by Calonico et al. (2014a).

As an additional robustness check, I re-estimate the RD effect controlling also for the vote share (not only for the margin of victory). Figure 5 reports point estimates and 95% robust confidence interval: in black from the model without controlling for the vote share W_i, in red from the model with it. The results for absenteeism rate (left) and share of targeted bills (right) are virtually identical with or without controlling for the vote share.

Figure 5: Controlling for vote share



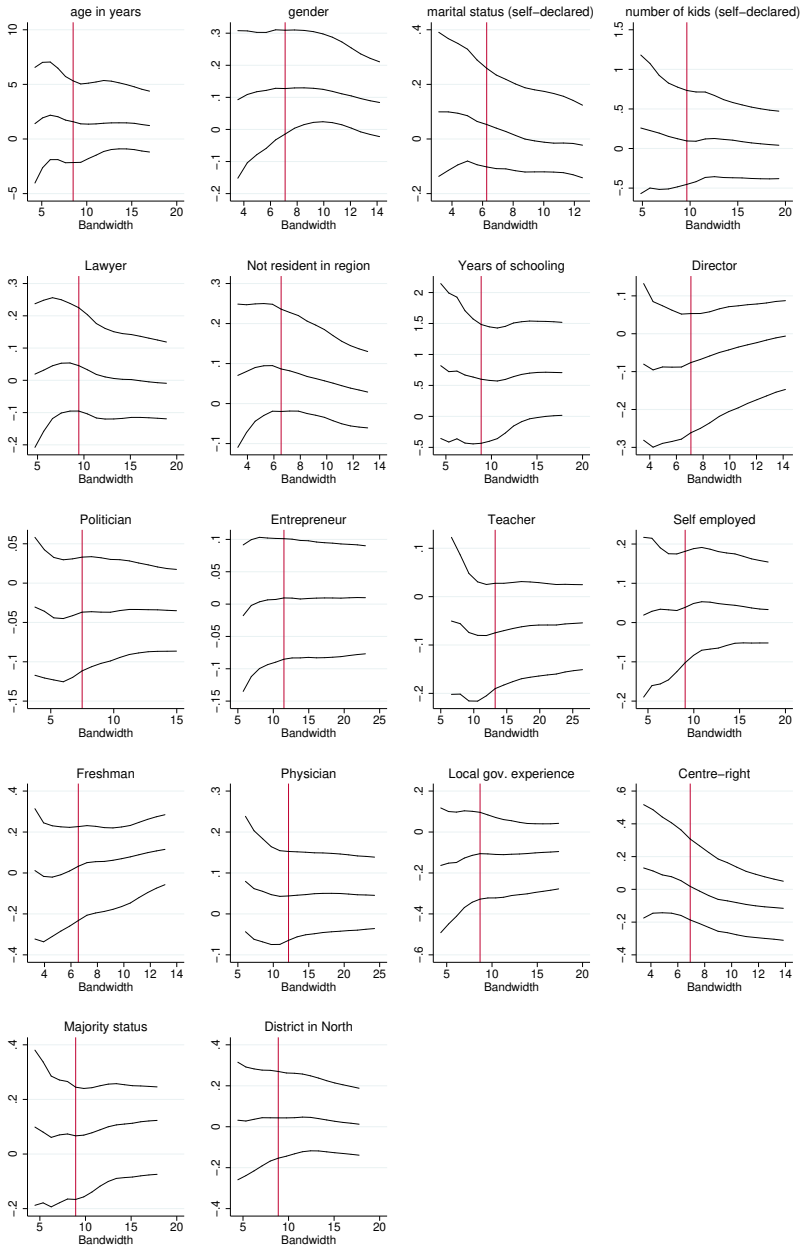
Note: on the left the outcome is the absenteeism rate, on the right the share of targeted bills. Both panels reports RD estimates and 95% robust confidence interval plotted against the bandwidth used: in black from the model without controlling for vote share; in red from the model controlling for vote share. Vertical red lines indicate the optimal bandwidth by Calonico et al. (2014b). Estimation by linear local polynomial estimator with triangular kernel.

Finally, consider the following. Assumption RD implies that in a neighborhood of $V_i = 0$ unobservables are balanced across three groups of observations: $D_i = 1$, $D_i = 0$, and $D_i = . .$ If Assumption LHS is violated, in a neighborhood of $V_i = 0$ unobservables are not balanced across the two groups $D_i = 0$, and $D_i = . .$ Thus, as already noticed by Gagliarducci et al. (2011), Assumption RDD and Assumption LHS *"are jointly verified if and only if politicians' observables and unobservables characteristics are balanced around the threshold"*. As such, standard tests used in the RD literature to assess the validity of Assumption RDD can be also used to assess the overall validity of the present empirical strategy.

Covariates imbalances In light of the discussion above, I test whether predetermined covariates are imbalanced across the threshold. In particular, I estimate the RD effect with the baseline estimation strategy using a number of predetermined covariates as outcome variable. Failure to find systematic differences in covariates enhances the credibility of the empirical exercise. I consider the following covariates: age, number of kids, years of schooling; dummies for: married, lawyer, state of election different from state of residence, director, professional politician, entrepreneur, teacher,

self-employed, freshman, physician, local government experience, centre-right, majority status, district in the North. Most of these estimates are not significantly different from zero at the 95% level (Figure 6).

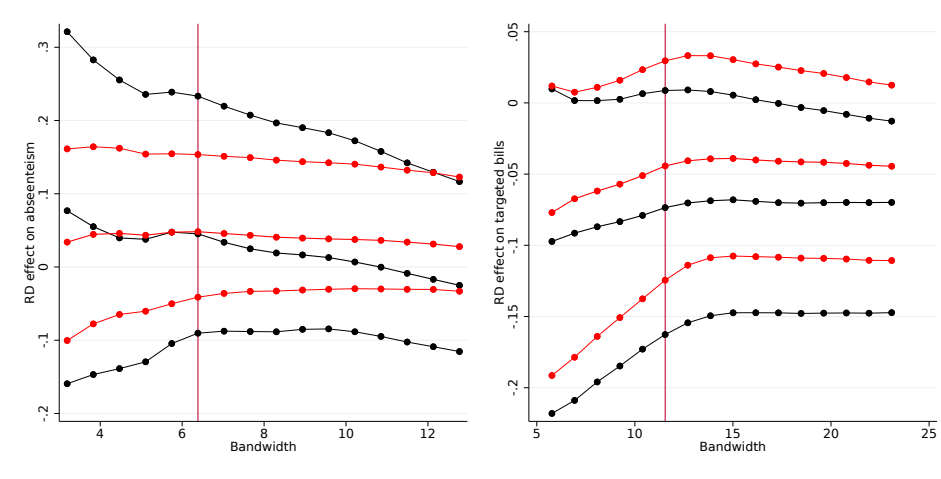
Figure 6: Balance test of predetermined covariates



Note: Several predetermined covariates as outcome variables. RD estimates and 95% robust confidence interval plotted against the bandwidth used. Vertical red lines indicate the optimal bandwidth by Calonico et al. (2014b). Estimation by linear local polynomial estimator with triangular kernel.

Finally, I also estimate the RD effect on the two outcome variables of interest controlling for all the covariates listed above. Figure 7 reports point estimates and 95% robust confidence interval: in black from the model without controlling for covariates, in red from the model with covariates. The estimates of the effect on absenteeism rate (left panel) are similar with or without covariates. The confidence intervals are tighter, but always include the zero. When controlling for covariates, the RD effect on targeted bills (right panel) is closer to zero by approximately 2.5 percentage points, compared to the estimates without covariates. The estimates are never significant at the 95% level. Thus there is no robust evidence of a negative effect of the treatment on the share of targeted bills.

Figure 7: Controlling for predetermined covariates



Note: on the left the outcome is the absenteeism rate, on the right the share of targeted bills. Both panels reports RD estimates and 95% robust confidence interval plotted against the bandwidth used: in black from the model without controlling for covariates; in red from the model controlling for covariates. Vertical red lines indicate the optimal bandwidth by Calonico et al. (2014b). Estimation by linear local polynomial estimator with triangular kernel.

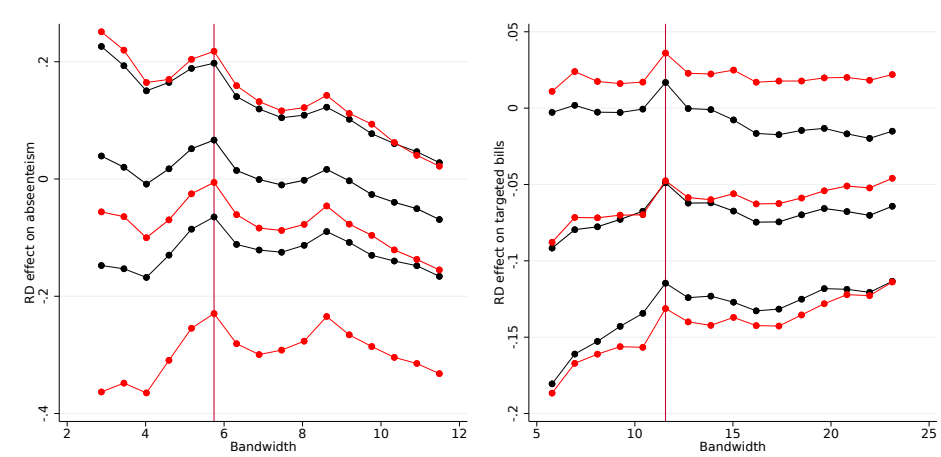
Controlling for district-election fixed effects The electoral rule in the Senate is such that some districts elect only one senator (the first-ranked), others more than one (the first-ranked and one or more best losers). Senators elected as first may behave differently if they are the unique representative of their district, or if instead some of their competitors are elected as well. In this section, I account for this by re-estimating the RD effect by OLS in bandwidths of different size, controlling for district-election

fixed effects.[10] This specification,

$$Y_{ijt} = \beta D_{ijt} + \gamma V_{ijt} + \delta\ V_{ijt} \times 1(V_{ijt} > 0) + \text{FE}_{jt} + \varepsilon_{ijt}, \tag{1}$$

will compare majoritarian senators to the proportional senators elected in the same district j and in the same election t. Thus those senators who are the unique representative in their district do not contribute to the identifying variation. The estimates from this specification (red lines in Figure 8) are quite similar to the model without controls (black lines in the same figure). None of the new estimates are significant at the 95% level for any of the two outcomes.

Figure 8: Controlling for district-election fixed effects



Note: on the left the outcome is the absenteeism rate, on the right the share of targeted bills. Both panels reports RD estimates and 95% confidence interval plotted against the bandwidth used: in black from the model without controlling for district-election fixed effects; in red from the model including these fixed effects. Vertical red lines indicate the optimal bandwidth by Calonico et al. (2014b). Estimation by OLS, and robust standard errors.

Finally, I combine all the previous validity tests together. In particular, I estimate by OLS in different bandwidths the following equation

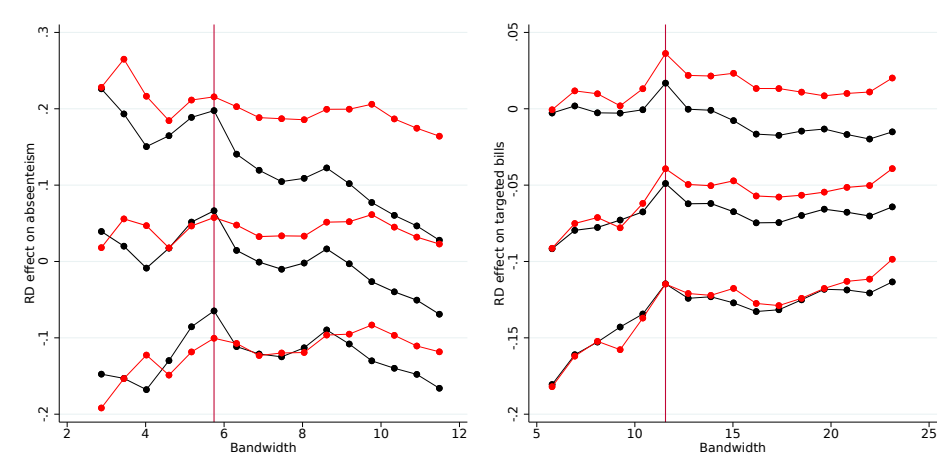
$$Y_{ijt} = \beta D_{ijt} + \gamma V_{ijt} + \delta V_{ijt} \times 1(V_{ijt} > 0) + \lambda X_{ijt} + \text{FE}_{jt} + \varepsilon_{ijt} \tag{2}$$

where X_{ijt} include all the available covariates including the vote share. These conservatives estimates (red lines in Figure 9) are not too dissimilar from the model without

[10] I turn to OLS to be able to include the fixed effects.

controls, and are never significant at the 95% level.

Figure 9: Controlling for district-election fixed effects, covariates and vote share.



Note: on the left the outcome is the absenteeism rate, on the right the share of targeted bills. Both panels reports RD estimates and 95% confidence interval plotted against the bandwidth used: in black from the model without additional controls; in red from the model with additional controls. Vertical red lines indicate the optimal bandwidth by Calonico et al. (2014b). Estimation by OLS, and robust standard errors.

Table 6 summarizes point estimates from all models and specifications presented thus far, restricting the attention to the optimal bandwidth. Overall, there is no evidence that senators elected in the proportional tier target less their district of election, nor that they shirk more, compared to their colleagues in the other tier.

Table 6: RD effects at the optimal CCT bandwidth, different models.

Outcome: absenteeism rate

Estimation	Bandwidth	Controls	Effect	95% C.I.	S.E.	p-value	Obs.
LLP triangular	6.4	-	0.04	-0.09; +0.23	-	0.38	368
LLP triangular	6.4	vote share	0.04	-0.1; +0.22	-	0.43	368
LLP triangular	6.4	individual controls	0.05	-0.04; +0.15	-	0.25	348
OLS	6.4	district-year FE	-0.06	-0.28; +0.16	0.08	0.58	368
OLS	6.4	all the above	0.05	-0.1; +0.2	0.11	0.54	353

Outcome: share of targeted bills

Estimation	Bandwidth	Controls	Effect	95% C.I.	S.E.	p-value	Obs.
LLP triangular	11.5	-	-0.07	-0.16; +0.01	-	0.08	581
LLP triangular	11.5	vote share	-0.07	-0.16; +0.01	-	0.09	581
LLP triangular	11.5	individual controls	-0.04	-0.12; +0.03	-	0.22	557
OLS	11.5	district-year FE	-0.04	-0.13; +0.03	0.04	0.26	581
OLS	11.5	all the above	-0.04	-0.11; +0.03	0.04	0.31	557

Note: the table reports figures referring to the estimates at the optimal CCT bandwitdh; these results are also reported graphically elsewhere in the paper. Individual controls include: age, number of kids, years of schooling; dummies for: married, lawyer, state of election different from state of residence, director, professional politician, entrepreneur, teacher, self-employed, freshman, physician, local government experience, centre-right, majority status, district in the North.

5 Conclusion

This paper has discussed the salient features of the mixed electoral system in place for Senatorial elections in Italy in 1994, 1996 and 2001. Under this system, most seats are assigned in first-past-the-post districts. The remaining seats are assigned to the most voted runners-up proportionally to the vote shares of their parties. As such this system mitigates a classic tradeoff between majoritarian and proportional systems: it improves representation, while keeping all legislators equally accountable to a well-defined geographical constituency.

A regression discontinuity design based on close elections has confirmed the latter insight. There is no evidence that senators elected in the proportional tier (runners-up) exhert less legislative effort targeted to their district, nor that they shirk more, compared to their colleagues elected in the majoritarian tier (first place finishers).

Taken together, the evidence is informative for policy-makers interested in designing an electoral system that results in quasi proportional representation of parties in parliament, while keeping all its members equally accountable to their local constituency. The latter feature has important implications for the distribution of public expenditure across space and items, and for incentivizing politicians to behave in the interest of their voters.

The analysis presented here is purely positive. A welfare comparison of adopting the system under investigation instead of other systems is beyond the scope of this paper, and could be subject of future research.

References

Albanese, Giuseppe, Marika Cioffi, and Pietro Tommasino (2019), "Legislators' behaviour and electoral rules: Evidence from an italian reform." *European Journal of Political Economy*, 59, 423 – 444.

Calonico, Sebastian, Matias D Cattaneo, and Rocio Titiunik (2014a), "Optimal Data-Driven Regression Discontinuity Plots ." *Journal of the American Statistical Association*, 110, 0–0.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik (2014b), "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica*, 82, 2295–2326.

Eggers, Andrew C. (2015), "Proportionality and turnout: Evidence from french municipalities." *Comparative Political Studies*, 48, 135–167.

Funk, Patricia and Christina Gathmann (2013), "How do electoral systems affect fiscal policy? evidence from cantonal parliaments, 18902000." *Journal of the European Economic Association*, 11, 1178–1203.

Gagliarducci, Stefano, Tommaso Nannicini, and Paolo Naticchioni (2011), "Electoral rules and politicians' behavior: A micro test." *American Economic Journal: Economic Policy*, 3, 144–174.

Grofman, Bernard (2016), "Perspectives on the comparative study of electoral systems." *Annual Review of Political Science*, 19, 523–540.

Lizzeri, Alessandro and Nicola Persico (2001), "The provision of public goods under alternative electoral incentives." *American Economic Review*, 91, 225–239.

Maaser, Nicola and Thomas Stratmann (2018), "Election rules, legislators' incentives, and policy outcomes: Evidence from the mixed member system in germany." *European Journal of Political Economy*, 54, 227 – 239. Political Economy of Public Policy.

McCrary, Justin (2008), "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of Econometrics*, 142, 698–714.

Milesi-Ferretti, Gian Maria, Roberto Perotti, and Massimo Rostagno (2002), "Electoral systems and public spending*." *The Quarterly Journal of Economics*, 117, 609–657.

Myerson, Roger B (1999), "Theoretical comparisons of electoral systems." *European Economic Review*, 43, 671 – 697.

Pellicer, Miquel and Eva Wegner (2013), "Electoral rules and clientelistic parties: A regression discontinuity approach." *Quarterly Journal of Political Science*, 8, 339–371.

Persson, Torsten and Guido Tabellini (2000), *Political Economics - Explaining Economic Policy*. MIT University Press, Cambridge, MA.

Download ZEW Discussion Papers from our ftp server:

http://ftp.zew.de/pub/zew-docs/dp/

or see:

https://www.ssrn.com/link/ZEW-Ctr-Euro-Econ-Research.html
https://ideas.repec.org/s/zbw/zewdip.html

IMPRINT

ZEW – Leibniz-Zentrum für Europäische Wirtschaftsforschung GmbH Mannheim
ZEW – Leibniz Centre for European Economic Research

L 7,1 · 68161 Mannheim · Germany
Phone +49 621 1235-01
info@zew.de · zew.de