# Can Disproportionality Help the Worker? Party Magnitude and Class Inclusivity under PR

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**Abstract**

Proportionality in electoral systems is typically viewed as essential for representative government. This paper shows that disproportionality in seat allocation rules under PR can increase working-class representation. The effect is mechanical: because working-class candidates are typically ranked lower on party lists, increases in party magnitude expand the range of list positions that enter office. I study a natural experiment generated by a municipality-level electoral reform in Italy in 1993 that introduced a majority bonus in legislative elections, guaranteeing the plurality-winning party a super-majority of seats. Using a reverse difference-in-discontinuities design, I find that the reform increased the number of working-class councilors, even as it reduced both the number of parties represented in office and the number of parties competing for seats, without affecting voter turnout, the vote shares of election-winning parties, or the ideological composition of municipal governments. Pre-reform differences in working-class representation emerged only in competitive PR contexts where no party secured a clear majority, suggesting that disproportional seat-allocation rules enhance descriptive representation when party competition otherwise limits list penetration.

**[Still in draft]**

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# 1 Introduction

The social composition of legislatures matters. A growing body of research in political economy and comparative politics shows that the presence of working-class legislators in elected office has substantive consequences for both democratic legitimacy and policy outcomes. At the individual level, descriptive representation by class enhances political trust, self-efficacy and participation among working-class citizens (Barnes and Saxton, 2019; Heath, 2018; Poertner, 2023). At the macro level, legislatures that include more working-class members are more likely to prioritize issues pertaining to labor rights and social insurance (Carnes and Lupu, 2015; O'Grady, 2019; Rosset, 2016), and ultimately enact more redistributive social policy (Hemingway, 2020). In short, class-based inclusion in representative bodies is not merely a symbolic ideal—it has measurable effects on electoral behavior and material outcomes.

Against this backdrop, electoral systems play a crucial role in shaping who gets elected. A foundational claim in political economy is that proportional representation (PR) fosters more socially representative legislatures (Norris, 1997, 2006). The mechanism is twofold. First, PR tends to produce multiparty competition, which can facilitate the electoral success of parties that explicitly mobilize underrepresented constituencies, including those defined by social class (Clark and Golder, 2006; Golder, 2006). Second, PR systems feature multi-member districts and larger legislative assemblies, enabling parties to field longer candidate lists. These structural features expand the opportunity space for politically marginalized groups to gain entry: when more seats are at stake, parties have stronger incentives to nominate a diverse set of candidates in order to capture votes from a broader electorate (Campos Fernandez Hott and Menezes-Filho, 2023; Lucardi, 2019).

However, recent empirical research complicates this conventional wisdom by highlighting persistent class-based exclusion within PR systems (Carnes and Lupu, 2021; Folke and Rickne, 2024). On both sides of the left-right spectrum, parties tend to include working-class candidates, but these individuals are systematically placed low on party lists, where their chances of election are lower.[1] Because list rank strongly predicts electoral success under PR, this practice of *hierarchical class sorting* effectively curtails the legislative presence of working-class individuals—even in systems ostensibly designed to promote social inclusivity.

[1] See e.g. Akter et al. (2026), Buisseret et al. (2022), Cirone et al. (2023), Elsässer (2024), Elsässer and Wenker (2025), Folke and Rickne (2024) and Galasso and Nannicini (2015).

I argue that this pattern reflects a trade-off embedded in the stage of party list formation. Party lists under PR are typically generated by party selectorates—local elites, incumbents, and/or internal committees. Electorally, selectorates have incentives to include working-class candidates on party lists in order to signal descriptive responsiveness to a majority working-class electorate. At the same time, selectorates are concerned with governing once in office. Legislative effectiveness, party discipline, and government survival depend on internal cohesion, which is easier to sustain when legislators share similar socio-economic backgrounds, career trajectories, and political socialization. As a result, selectorates tend to reserve higher list positions for candidates drawn from middle-class backgrounds, while placing working-class candidates lower on the list. Together, these incentives generate hierarchical class sorting. Importantly, this phenomenon is best understood as reflecting selectorate preferences over legislative cohesion rather than hostility toward working-class representation per se, and persists across electoral contexts.

The empirical pattern presents a paradox: if party elites systematically limit working-class advancement within PR, then reforms that reduce party fragmentation and strengthen the majoritarian character of elections—by concentrating legislative power in fewer, larger parties—may, counterintuitively, enhance class representation as they expand the share of seats controlled by election-winning parties. The logic is institutional. By inflating the share of seats awarded to election-winning parties, quasi-majoritarian reforms weaken the legislative constraints that help sustain hierarchical list ordering under PR. Expanding party seat shares thus mechanically increases the number of lower-ranked candidates who enter office, even if selection patterns remain stable.

This paper explores this paradox by examining how the disproportionality of electoral formulas—that is, the degree to which seat allocation rules inflate the legislative power of the election-winning party (Benoit, 2000)—shapes the presence of working-class legislators in elected office. I argue that the seat advantages conferred to dominant parties through disproportional rules can outweigh the positive effects of party system fragmentation in shaping social class inclusion. Under list-based electoral competition, disproportionality can therefore enhance descriptive representation precisely when socially underrepresented candidates are concentrated below the top ranks of party lists, because larger seat shares extend how far party lists penetrate into office. In doing so, quasi-majoritarian systems can produce more class-inclusive outcomes than fragmented but strictly proportional systems. The paper thus contributes to a growing literature on the unintended consequences of electoral design and the political economy of legislative recruitment (see e.g. Becher and González, 2019; Lucardi, 2019; Lucardi and Micozzi, 2022).

Evaluating this proposition in a causally credible way is challenging. Electoral laws tend to come in bundles, making it difficult to isolate the effects of seat-allocation rules from those of related institutional features, such as district magnitude, legislature size, or electoral thresholds. To overcome this inference problem, I leverage a natural experiment that alters the disproportionality of seat allocation across all parties—systematically inflating the seat shares of election-winning parties while reducing those of their competitors—while keeping other key institutional parameters constant.

The empirical setting is a 1993 reform to municipal electoral rules in Italy. The reform extended the use of a *majority bonus* to all municipalities. A majority bonus is a quasi-majoritarian mechanism that grants the party list receiving a plurality of votes a clear majority of seats in the legislature. In practice, this sharply inflates the seat share of the winning party while reducing representation for its competitors.[2]

Crucially, the 1993 reform applied only to municipalities with populations over 5,000. Smaller localities already operated under electoral rules that guaranteed clear legislative majorities and were therefore unaffected by the legal change. This institutional discontinuity enables a reverse difference-in-discontinuities design, comparing working-class representation above and below the 5,000-resident threshold, before and after the reform. By leveraging institutionally induced variation in electoral rules, the analysis moves beyond associational evidence to identify causal effects on legislative composition. Moreover, unlike most existing studies—which rely on cross-sectional comparisons—this design allows for the evaluation of how electoral rules shape representational outcomes over time.

In line with theoretical predictions, I find that the introduction of majority bonuses increased working-class representation in municipal councils, and that effects were persist over time. Direct manipulation tests further show that the reform reduced party system fragmentation, and inflated the party magnitudes of winning parties, without increasing their corresponding vote shares. Additional robustness checks reveal no changes in voter turnout or shifts in support for parties of any ideological leaning. The rise in descriptive representation, in other words, does not appear to reflect changes in voter preferences, but rather institutional restructuring. Moreover, heterogeneity tests indicate that positive effects are driven by municipalities that retained competitive PR systems pre-reform and therefore experienced larger shifts in winning party magnitudes. This pattern is consistent with the core theoretical mechanism: that increased working-class representation operates through inflated party magnitude rather than electoral realignment

[2]Majority-assuring PR systems like this are common in subnational elections—e.g., in France, Italy, and Andorra—and also shape national legislatures in countries such as Armenia, Greece, and San Marino (Bedock and Sauger, 2014). Historically, similar systems have been used in Chad, Mexico, South Korea, and Italy (D'Alimonte, 2015; Shugart and Wattenberg, 2001).

or changes in partisan control.

Overall, this study contributes to a more differentiated understanding of how electoral systems shape political inclusion. Rather than treating PR as a uniform category, it shows that variation in how votes are translated into seats—specifically through the inflation of seat shares for winning parties—can condition who gains access to elected office. In doing so, the paper bridges debates on class-based descriptive representation, electoral design, and party gatekeeping, and provides new causal evidence on how seat-allocation rules shape the social composition of legislatures.

## 2 Proportional Representation and Social Inclusivity

Proportional representation (PR) is widely regarded as more conducive to social inclusion than majoritarian electoral systems (Norris, 1997, 2006). Empirical research documents these inclusive effects across multiple dimensions of representation, including gender (e.g. Kittilson and Schwindt-Bayer, 2012; Krook, 2018; Matland and Studlar, 1996; Matland, 1998; Norris, 1985; Profeta and Woodhouse, 2021; Rule, 1987), ethnic minorities (Barker and Coffé, 2018; Hughes, 2016; Le Lohé, 2004; Moser, 2008), and youth (Joshi, 2013; Stockemer and Sundström, 2018).

Standard accounts attribute these inclusive effects to two institutional features. First, by lowering electoral thresholds and encouraging multiparty competition, PR increases party system fragmentation (Clark and Golder, 2006; Golder, 2006), creating opportunities for smaller or group-based parties to gain representation (Lublin, 2017). Second, PR systems employ multi-member districts, allowing parties to win multiple seats within a constituency and to field longer candidate lists. In doing so, PR increases party magnitude—that is, the number of seats won by individual parties. Larger party magnitudes are commonly viewed as facilitating social diversity, as they allow parties to nominate a broader range of candidates while remaining electorally viable (Campos Fernandez Hott and Menezes-Filho, 2023; Lucardi, 2019). These two features—party fragmentation and party magnitude—are often treated as complementary mechanisms through which PR expands access to office.

Yet recent work shows that social class diversity remains limited even in PR systems (Carnes and Lupu, 2021). While PR enables party lists to incorporate a wide array of candidates, entry into legislatures depends not only on nomination, but on the interaction between list rank and the number of seats a party ultimately secures. Candidates placed at the top of party lists are highly likely to enter office, whereas

those ranked lower face sharply diminishing probabilities of election. This rank–election relationship is most direct under closed-list systems, but it persists in partially open-list settings (Buisseret et al., 2022; Marcinkiewicz and Stegmaier, 2015; Lutz, 2010), as voters frequently rely on party-provided rankings as heuristics (Miller and Krosnick, 1998) and rarely overturn list order through preference votes (Koppell and Steen, 2004).

Crucially, recent studies show that working-class candidates are systematically ranked in lower list positions relative to their middle- and upper-class counterparts, even when included on party lists—a pattern observed across both left- and right-leaning parties.[3] I refer to this systematic placement of working-class candidates in electorally disadvantaged list positions as *hierarchical class sorting*. Overall, there is little evidence that form of social stratification is rooted in voter bias. Studies examining demand-side drivers of worker exclusion largely report null findings (Carnes and Lupu, 2016; Schwarz and Coppock, 2022) or substantively negligible effects (Motolinia and Klasnja, 2024), while some suggest that working-class backgrounds may even confer modest electoral advantages (Campbell and Cowley, 2013; Vivyan et al., 2020). These findings shift attention toward the supply side of candidate selection. In what follows, I develop a theory of intra-party recruitment and list formation and clarify how distinct dimensions of seat allocation shape the representational consequences of these selection dynamics.

## 2.1 Seat Allocation and Working Class Inclusivity

To understand how seat allocation shapes working-class representation, it is first necessary to explain why hierarchical class sorting emerges within parties. Existing theory conceptualizes social stratification on party lists as the product of strategic trade-offs faced by party selectorates. Galasso and Nannicini (2015) for example, show that parties allocate different candidate types to distinct list positions depending on electoral risk. In their framework, loyalists—who enhance discipline and cohesion—are disproportionately placed in electorally safe positions, while electorally valuable candidates are used to attract votes in more competitive ranks. List ordering thus serves as a strategic instrument through which party elites reconcile vote maximization with internal control.

[3] See Akter et al. (2026), Buisseret et al. (2022), Cirone et al. (2023), Elsässer (2024), Elsässer and Wenker (2025), Folke and Rickne (2024) and Galasso and Nannicini (2015).

Similarly, Buisseret et al. (2022) argue that parties strategically allocate candidate quality across list positions in anticipation of governing. Under their 'Top-Down' hypothesis, electorally advantaged parties place higher-quality candidates in upper ranks, reflecting expectations about executive responsibility. Building on this logic, Matakos et al. (2024) argue that ideologically distant candidates are systematically relegated to electorally unsafe positions. Importantly, they show that greater disproportionality under PR increases the ideological variance of elected legislators by expanding the depth of list penetration within winning parties.

Building on this literature, I posit that trade-offs between electoral appeal and legislative stability also induce secondary effects on the social class composition of legislatures. On the one hand, parties have strong electoral incentives to include working-class candidates on their lists in order to signal descriptive responsiveness to a large share of the electorate drawn from the working class. On the other hand, selectorates must also consider legislative performance once in office. Governing effectively requires internal cohesion, policy coordination, and leadership stability—objectives that are easier to sustain when legislators share similar socio-economic backgrounds, professional trajectories, and political networks. This reflects the fact that shared socialization experiences and material interests reduce preference variance among co-partisan legislators, thereby facilitating cooperation. Consistent with this view, research shows that legislators who share similar social backgrounds or informal ties are more likely to collaborate, vote similarly, and coordinate their legislative behavior (see e.g. Cohen and Malloy, 2014), which in turn increases their political effectiveness (Battaglini et al., 2020). Related work further finds that political elites tend to prefer co-partisans who share their socio-economic attributes, reinforcing patterns of social homophily within legislatures (Rehmert, 2020).

Balancing the objectives of electoral appeal and legislative efficiency helps explain why hierarchical class sorting emerges within party lists. Party selectorates prioritize socially proximate candidates in electorally secure list positions in order to safeguard legislative coordination and discipline, while relegating working-class candidates to lower ranks of the list. In doing so, parties can signal electoral inclusiveness while preserving control over the candidates most likely to enter office.

With these dynamics in mind, the social composition of legislatures under PR reflects the interaction between systematic class-based ranking patterns and the number of seats won by each party. Consequently, institutional rules that determine how votes translate into seats shape which segments of party lists ultimately convert into office. When parties win only a small number of seats, representation is drawn primarily from the upper ranks of their lists. When they secure larger seat shares, lower-ranked candidates—among whom working-class individuals are disproportionately concentrated—are more likely to enter office.

Importantly, the two institutional mechanisms typically credited with enhancing inclusion under PR— party system fragmentation and the allocation of seats to individual parties—should operate on distinct margins once hierarchical class sorting is present. Fragmentation determines how many parties enter the legislature. The number of seats won by each party, by contrast, determines how deeply its candidate list penetrates into office. When working-class candidates are systematically concentrated in lower list positions, increases in fragmentation do not necessarily alter which ranks convert into seats within parties. Expansions in the seat share of winning parties, however, directly extend representation beyond upper-ranked candidates and into lower-ranked positions, where working-class candidates are more prevalent. Larger seat shares thus reduce the extent to which list ranking can restrict access to office. Because this institutional constraint on selectorate behavior remains relaxed beyond the first post-reform election, the representational effects of seat inflation should persist across subsequent electoral cycles. From this, I derive a first set of testable hypotheses:

H1. *Seat Inflation:* Institutional reforms that increase the seat shares of election-winning parties will increase the proportion of working-class legislators in legislative office.

H2. *Institutional Persistence:* The positive effect of seat inflation on working-class representation will persist beyond immediate post-reform elections.

H3. *Stable Selectorate Preferences:* Increases in the number of working-class legislators will not be accompanied by changes in the socio-economic composition of upper-ranked candidates.

H4. *Party System Concentration:* The positive effect of seat inflation on working-class representation will persist even as reforms reduce the number of parties entering the legislature.

Importantly, the theoretical framework assumes that differences in representational outcomes are driven by supply-side dynamics rather than shifts in voter demand. In other words, voters are not expected to change their preferences in response to altered seat-allocation rules. To assess this assumption, I derive a set of hypotheses designed to rule out alternative explanations based on electoral behavior:

H5. *No Vote-Share Effect:* Institutional reforms that increase the seat shares of election-winning parties will not systematically increase the vote shares of those parties.

H6. *No Change in Turnout:* Seat-inflation reforms will not systematically affect voter turnout.

H7. *No Ideological Realignment:* Seat-inflation reforms will not systematically alter the ideological composition of legislatures.

Finally, the mechanism outlined above also implies cross-sectional differences in representational outcomes under varying levels of party competition. Even in PR systems *without* seat-inflation rules, electoral competition can generate variation in the seat shares obtained by election-winning parties, simply as a result of voter preferences. Where dominant parties win larger seat shares, candidate lists are penetrated more deeply, increasing the likelihood that lower-ranked working class candidates enter office. This yields the following prediction:

H8. *Competitive Baseline:* The positive effect of seat-inflation rules on working-class representation will be concentrated in settings where election-winning parties secure smaller seat shares under PR.

# 3 Research design

To test my theory, I estimate a series of models based on a difference-in-discontinuities design (hereafter, 'diff-in-disc'). For this, I exploit a 1993 reform introduced in Italy, in which a subset of municipalities shifted away from a classic PR formula to a disproportional one, characterized by its use of a majoritarian bonus. I commence by describing the institutional context of Italian municipal government and detail the specifics of the reform, to better elaborate on the logic and advantages of this empirical approach.

## 3.1 Municipal government in Italy and Reform n. 81 of 1993

Italy has around 8,000 municipalities that collectively manage about 15% of national public expenditures. These local governments oversee a broad range of public services, such as land zoning, water and waste

management, local transportation, cultural initiatives, social housing, and pre-primary and primary education. Most municipalities are small: 95% of them have fewer than 15'000 inhabitants.

Municipal elections operate on polity-wide proportionality, i.e. they do not make use of separate electoral districts.[4] They serve to determine office-holding in two main political bodies: the mayorship (*Sindaco*) and the municipal council (the legislature, dubbed the *Consiglio*). The mayor, in turn, appoints an executive committee (a *Giunta*), which is charged with co-drafting policy propositions. In municipalities with fewer than 15'000 inhabitants, office-holders may cumulate mandates across the council and the executive committee—a practice that is very commonplace.

Before April 1993, mayoral and council elections were, in most Italian municipalities, fully proportional in nature. Under this system, *List PR*, voters chose a closed party list, and within it they were able to express a maximum of five preference votes for individual candidates. The allocation of council posts was determined by means of the D'Hondt formula: a common full proportionality rule.[5]

List PR constituted the predominant local electoral system pre-1993, but there was an important exception to this institutional setting. Municipalities with fewer than 5000 inhabitants maintained a system referred to as *Plurality with Panachage*, in which elections were quasi-majoritarian in nature (see Gulino, 2021). Under these electoral rules, parties could compete with lists consisting to a maximum of 2/3 of the total number of seats available within a city council. Each voter was then granted a large number of preference votes—4/5 of council seats—which they could freely distribute across any party list. For example: In a locality with 4500 inhabitants, councils consisted of 20 members, and voters were given 16 preference votes to distribute across lists consisting of a maximum of 13 candidates. The abundance of preference votes in this system naturally placed a heavy cognitive burden on the individual voter. Hence, voters would often either a) vote by default: i.e. assign all preferences at their disposal to a single list and forsake the remaining, or b) grant everyone on their preferred list a vote, plus add singular candidates from other lists.[6] As a consequence, elections nearly always resulted in two- or three-party councils, where the largest party held a super-majority (2/3) of seats. Indeed, in the elections that took place just before the 1993 reform, 96% of Italian elections in municipalities with 3000-5000 inhabitants produced such legislative super-majorities—and less than 1% failed to produce a majority party in the local legislature.[7]

[4] Conceptually, district magnitude is equal to legislature size in these systems.
[5] For a discussion concerning variation in the proportionality of electoral formulas under PR, see Benoit (2000).
[6] In the 1985 elections, De Mucci (1990) finds that approx. 75 percent of voters in these municipalities voted by default.
[7] Authors' own calculations.

Under both pre-1993 systems, voters placed their votes on closed party lists and municipal councilpersons thereafter selected both a mayor and an executive committee from within their own ranks. The core difference between the systems was therefore electoral system disproportionality: while Plurality with Panachage inflated the party magnitudes of mayoral parties, List PR continuously engendered proportional local parliaments. Thus, a systematic drawback of List PR was that it, by 1990, started engendering local parliaments consisting of many small political parties, where the mayoral party retained a slim majority of seats. This, coupled with the legislature's small size, increased the occurrence of preterm government collapses, as singular lawmakers could withhold and/or withdraw support for the incumbent executive. In contrast, the municipal executive branch was nearly always supported by an absolute majority of council members under Plurality with Panachage, thereby inducing a much higher level of stability in local policy-making.

In April 1993, onset by a systemic corruption scandal commonly known as *Mani pulite*, municipal electoral rules were changed throughout Italy. Large parts of this reform were undertaken to improve legislative efficiency under the old List PR system. And as a result of the rule changes, all Italian municipalities, regardless of population size, introduced a so-called 'Majority Bonus PR' system, which remains in force today. Under this quasi-majoritarian system, voters place their votes for a mayoral candidate that is affiliated with one or several party lists. The mayoral candidate that wins a plurality of votes obtains the mayorship, and their associated list (or list coalition) automatically obtains a super-majority (2/3) of all council seats. The remaining seats are thereafter proportionally allocated to the other lists using the D'Hondt formula.[8]

The 1993 reform thus affected all Italian municipalities—but it did not fundamentally alter party magnitudes in municipalities with fewer than 5000 inhabitants, where a de facto plurality rule was in force already before reform enactment. As such, the statistical identification strategy I employ is based on comparing differences in outcomes across municipalities with more or fewer than 5000 inhabitants, as well as how these differences change before and after the formula change. The design thus exploits both cross-sectional variation close to an arbitrary cutoff point—as is typical of a regression discontinuity design (RDD)—as well as temporal variation in the observation of treatment, which serves as the premise of a difference-in-differences design (DID).

[8]Until 2012, voters were permitted one preference vote, which had to be expressed on their preferred party list. In 2012, this number was increased to two gender-balanced votes.

## 3.2 Empirical design and identification assumptions

The longitudinal diff-and-disc design was first formalized by Grembi et al. (2016), and has since been used in a number of other papers on local politics.[9] The strength of the design is two-fold. On one hand, arbitrary cross-sectional variation in the observation of outcomes permits us to rule out other time-varying variables that correlate with the timing of electoral system change. This is critical for the case analyzed for several reasons. First, the Tangentopoli crisis that triggered the reform also completely re-wrote the Italian party system. As both of Italy's largest parties—Democrazia Cristiana (DC) and Partito Socialista Italiano (PSI)—were embroiled in the scandal and thus collapsed after 1993.[10] In the immediate aftermath, municipal legislators from these parties often abandoned politics, or shifted alliances to compete on locally oriented Civic List parties (Daniele et al., 2020). As the scandal was felt nation-wide, however, all municipalities—irrespective of population size—were affected by these party political trends. Second, the 1993 reform did not only impact the electoral formulas of municipalities, but additionally compressed the size of both municipal councils and executive committees, as well as introduced a (temporary) gender quota on party lists. These rule changes too, however, affected all sampled municipalities in an identical manner.

Aside from reducing over-time confounding, the temporal variation of a diff-in-disc set up enables researchers to relax a core assumption that must hold when deriving local average treatment effects (LATEs) under a conventional RDD. Namely, that only the treatment itself differs across municipalities at a given cut-off point (Eggers et al., 2018). This condition is almost never met, as multiple policies often change simultaneously at specific threshold values of municipal population size, which induces problems of compound treatments. In the case analyzed, the other main policy that shifts at the 5000-inhabitant cut-off is executive compensation (Gamalerio and Trombetta, 2022; Grembi et al., 2016). As shown in table 1, both mayoral and executive committee wages increase by nearly 30 percent at the 5000-inhabitant cut-off—a discontinuity originally introduced in 1960.

As a result, monetary incentives to office-holding are notably stronger in municipalities above the cut-off, which has an independent effect on political selection dynamics (Caria et al., 2023; Gagliarducci and Nannicini, 2013). The diff-in-disc design enables me to overcome this fundamental inference problem under a number of indirectly testable assumptions.

[9] Papers on Italy, more specifically, include Andreoli et al. (2021); Baltrunaite et al. (2019); De Benedetto and De Paola (2019); Dominici (2023); Gamalerio and Trombetta (2022); **?** and Profeta and Woodhouse (2021).

[10] See e.g. Bull and Rhodes (2014), Katz (1996), Morlino (1996) and Salvati (1995).

Table 1: Policy changes at legislative thresholds for Italian municipalities under 15'000 inhabitants

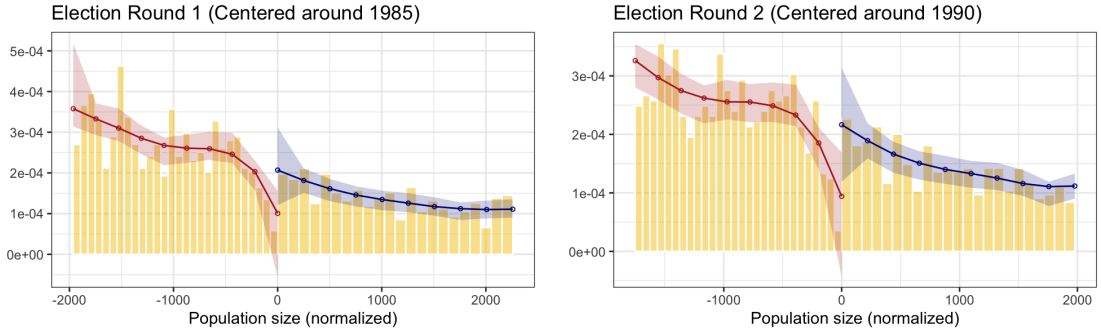
| | Pre-Reform (85-92) | | | Post-Reform (93-09) | | | | |
|---|---|---|---|---|---|---|---|---|
| Population | Ex. Com. size | Council members | Electoral rule | Ex. Com. size | Council members | Electoral rule | Mayoral wage | Ex. Com wage |
| Below 1000 | 4 | 15 | Plurality w. Panachage | 4 | 12 | Plurality Single-Round | 1'291 | 15% |
| 1000-2999 | 4 | 15 | Plurality w. Panachage | 4 | 12 | Plurality Single-Round | 1'446 | 20% |
| 3000-4999 | 6 | 20 | Plurality w. Panachage | 4 | 16 | Plurality Single-Round | 2'169 | 20% |
| 5000-9999 | 6 | 20 | List PR | 4 | 16 | Plurality Single-Round | 2'789 | 50% |
| 10'000-15'000 | 6 | 30 | List PR | 4 | 20 | Plurality Single-Round | 3'099 | 55% |

Note: Adapted from Gulino (2021) and Gagliarducci and Nannicini (2013). Grey-marked cases highlight municipalities within sample frame. Ex. Com size denotes the maximum number of members that a mayor can appoint to the executive committee. Mayoral wages refer to monthly gross wages in 2000; executive committee wages are expressed as a percentage of these. Wages are indexed to inflation and were adjusted on a near-annual basis, implying largely negligible overtime variation in real income.

A first assumption is that municipalities should not be able to self-select into treatment; that is, determine the value of the running variable (population size). This proposition can be indirectly assessed by examining the density of observations around the cut-off point, where continuity implies a low likelihood of sorting. As depicted in Figure 1, however, there is a detectable discontinuity at the treatment-assignment threshold in pre-reform election rounds, implying some degree of manipulative sorting. In local politics, such sorting occurs when public officials are partially able to influence the outcome of the population census. The phenomenon is well documented and is primarily explained by remuneration incentives (Eggers et al., 2018). If municipal executives anticipate that census results will fall close to a given population threshold, they face strong material incentives—in this case, a 30 percent increase in remuneration—to ensure that additional residents are registered in their municipality.

Notably, strategies to attract inhabitants are mainly feasible where municipalities lie very close to the cut-off point (Eggers et al., 2018, p.220). This is visible in Figure 1, where the discontinuity is concentrated within approximately $\pm 150$ residents around the threshold. Moreover, during the pre-reform period, similar discontinuities in the running variable appear at other thresholds where only mayoral wages change while electoral rules remain constant (see Figure B.1, Appendix B.1). Taken together, this pattern suggests that municipalities are not sorting in response to electoral institutions, but rather to financial incentives tied to office-holding.

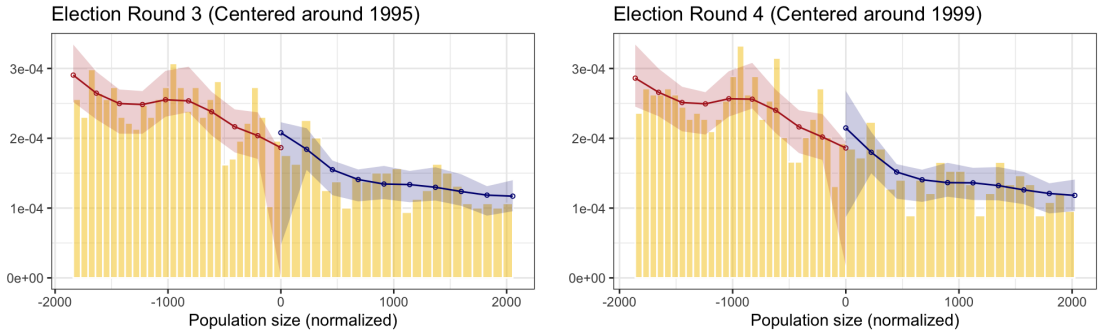
Figure 1: Discontinuity tests on the density of population size around the cut-off, by election round




(a) $p = 0.023$

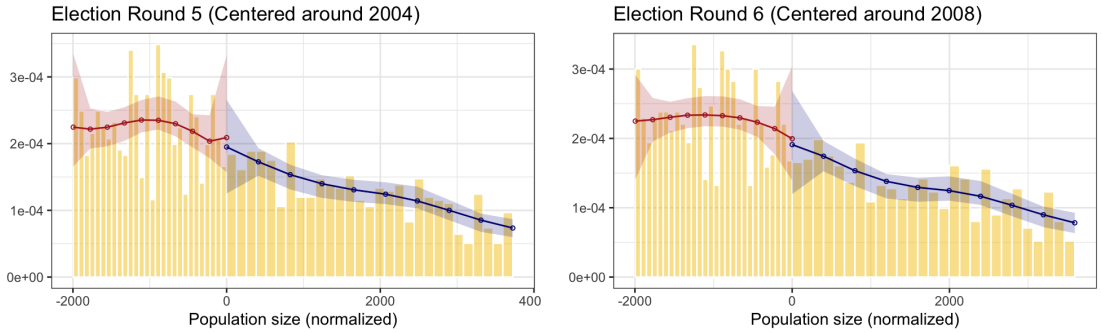



(b) $p = 0.038$




(c) $p = 0.492$

(d) $p = 0.274$




(e) $p = 0.398$

(f) $p = 0.630$

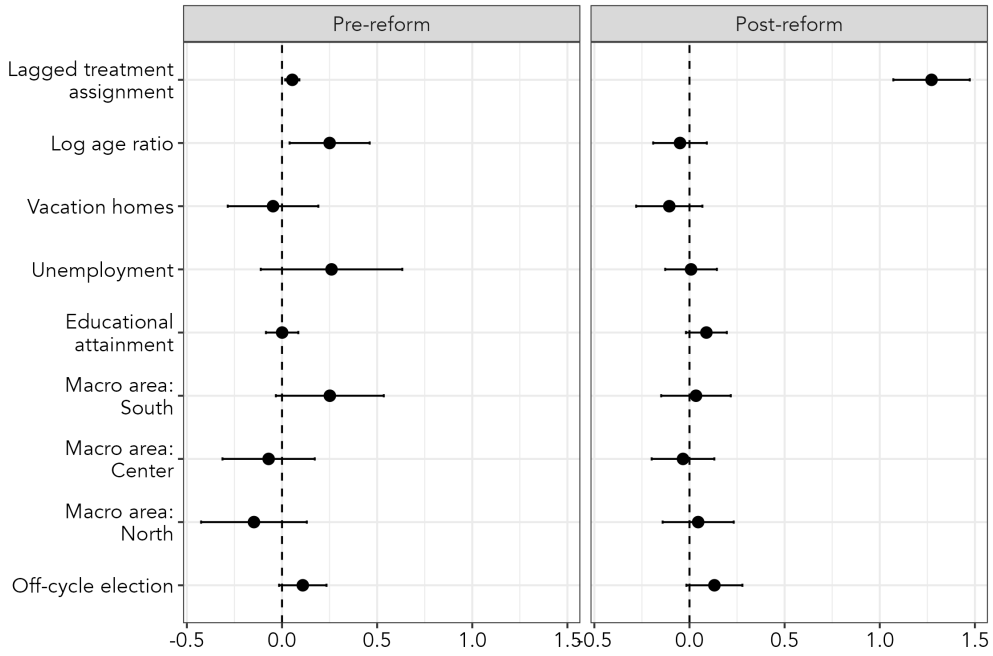
*Notes:* Solid lines represent split local polynomial density estimates of municipal population size under MSE-optimal local fits, with a triangular weighting kernel (Cattaneo et al., 2020). Bandwidths indicate robust bias-corrected 95% confidence intervals. Population sizes were calculated every ten years during the period examined, while legislative terms were set to five or four years (temporarily shortened between 1993 and 1999). Results of the 1981 general census thus determines treatment assignment for election rounds 1 and 2; the 1991 census sets assignment for election rounds 3 and 4, and the census of 2001 does so for election rounds 5 and 6.

To adjust for imbalances stemming from sorting, I undertake several measures. First, I refrain from employing any form of regression weights in my analyses that grant disproportional importance to units lying closest to the cut-off. Second, as advised by Eggers et al. (2018), I include a series of control variables in my estimations that are correlated with sorting behavior, as they moderate the ability of local officials to impact census counting procedures. Third, to ensure robustness, I run a series of donut-hole estimations as proposed by Barreca et al. (2011), in which I exclude municipalities that lie in the immediate vicinity of

the cut-off.[11]

A second identification assumption is that potential outcomes and municipality-variant traits are balanced around the cut-off during the full period examined. Probing the feasibility of this proposition, I run a series of RD tests on municipality-level covariates, the results of which are presented below in figure 2. They suggest that two sets of covariates are imbalanced across treatment groups: a lagged treatment assignment dummy and an indicator of the log age ratio: both of these variables have been shown to correlate with sorting behavior in Italian local elections, and are adjusted for tests.

Figure 2: RD estimates on treatment effects on covariates, by reform period



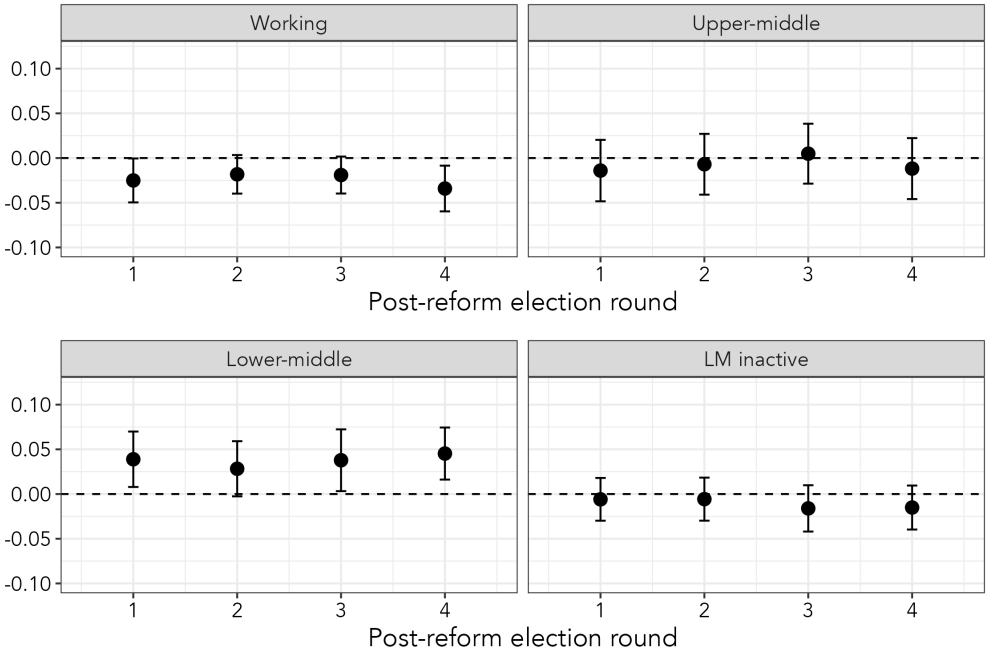

*Notes:* Outcomes are statistically standardized to improve legibility. Bars indicate 95% robust bias corrected confidence intervals, derived from municipality-clustered standard errors. Estimates obtained using local polynomial estimators with uniform kernel and two-sided MSE-optimal bandwidths, and adjust for election round fixed effects. Outcome variables are a) a lagged treatment assignment indicator (dummy), b) log of old/young inhabitants (continuous), c) proportion of properties that are vacation homes (continuous), d) fraction of adult residents in unemployment, e) fraction of adult residents with tertiary schooling degree, f) location in Southern Italy (dummy), g) location in Central Italy (dummy), h) location in Northern Italy (dummy), i) election not held during a main election cycle year (dummy).

Finally, a third assumption is that the independent effects of electoral formulas and executive wages are constant over time. Specific to this study, this implies that municipalities just above and below the 5000-inhabitant threshold would have displayed parallel trends if they had the same electoral system pre-1993. To probe the feasibility of this assumption, we can examine how outcome trends related to the descriptive representation of social classes looked like after the reform—i.e. when municipalities diverged only in terms of local executive compensation. Figure 3 plots RD estimates of all main outcomes in post-reform

[11] See also Dowd (2021), for a formal discussion of these designs.

election rounds. Trends develop fully in parallel for all four social groups examined, which bolsters the credibility of the identifying assumption. The main difference across treatment groups is that treated municipalities have a higher rate of lower-middle-class representatives in their councils and lower rates of working-class councilors. This, however, is to be expected, as the independent effect of higher remuneration—shown to skew legislatures toward individuals with higher levels of education—remains present in the post-reform period.

Figure 3: Post-reform trends in statistical class representation, by election round




*Note:* Bars indicate 95% robust confidence intervals, derived from municipality-clustered standard errors. Estimates obtained using local polynomial estimators with uniform kernel and two-sided MSE-optimal bandwidths. All models adjust for controls included in main specification (NUTS-2 region, unemployment, educational attainment level and a lagged treatment assignment dummy).

## 3.3 Data and measurement

**Social class composition of legislatures**: To code my main variables of interest, I make use of annually compiled data from the Italian Ministry of Internal Affairs (*Anagrafe degli Amministratori Locali*). This data contains information on the occupations of municipal legislators that have been elected to office and has been collated since 1987. As most local politicians obtained office prior to this year, this allows me to examine the class composition in two legislatures voted into office before reform enactment for 82% of municipalities in my sample—for remaining 18%, I only have information on only one. As further changes are made to legislature sizes at the 5000 inhabitant cut-off in the 2010s (see Bellodi and Morelli,

2026), I delimit myself to examine four election cycles after reform adoption. I also make several sample delimitations to ensure that I obtain appropriate counterfactuals. I exclude municipalities from the five autonomous regions, as threshold mechanisms are only valid there, if rules are compatible with their own special statutes.[12] I also delimit localities if they engage in municipal mergers or splits, and omit municipalities that were appointed a central state caretaker government during the period analyzed.[13]

My outcomes of interest concern the class composition of municipal councils. To capture this, I use a four-category indicator on the social classes of Italian local legislators, coded by Ray (2022). The coding scheme builds on the work of labor market sociologist Oesch (2006), who uses ISCO occupational indicators in micro-level labor market surveys to classify individuals into social classes, based on their exposure to an assortment of labor market risks (e.g. unemployment and income shortfalls). Table 2 provides examples of how raw occupations are coded along the four-category class scheme. Using occupation as an indicator of social class is commonplace in large-N social scientific research—and it is so for good reason. Aside from capturing group-specific variation to labor market risk, occupations inform us about individual-level educational attainment and workplace characteristics—factors that have been consistently shown to underscore class-based divides in public opinion and political behavior.[14] Importantly, elected council members are not salaried, and thus retain their primary occupations while in office.[15] This ensures that I capture legislators' *current* social class rather than, for example, their class origin.

Table 2: Social class coding based on occupational belonging

| **Occupations** ***(examples from raw data)*** | **Social class coding** |
|---|---|
| Company directors, Senior civil servants, Lawyers, Engineers, Financial analysts, University professors, Journalists | Upper middle class |
| Hospitality managers, Police inspectors, Technicians, Sales agents, Designers, Librarians and curators | Lower middle class |
| Bank-tellers, Travel consultants, Legal secretaries, Customer service reps, Fire fighters, Craftsmen, Machine operators, Hairdressers | Working class |
| Homemakers, students, retirees, unemployed | Labor market inactive |

Adapted from Ray (2022), pg. 12.

Ultimately, I'm interested in obtaining an estimate of the probability of individuals from a given social class to obtain a seat on a municipal council. Given this, I use class indicators to generate a series of

[12] These are Aosta Valley, Friuli-Venezia Giulia, Sardinia, Sicily and Trentino-South Tyrol.

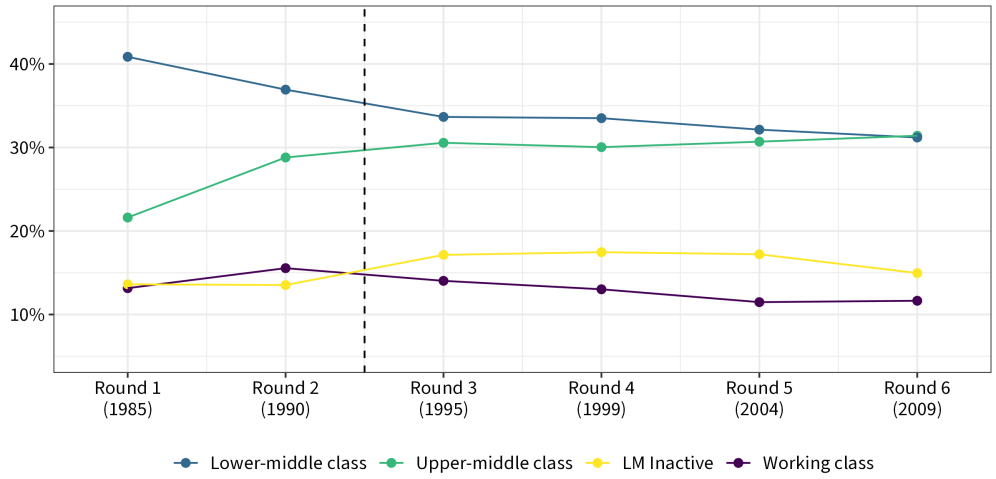
[13] The central government can exercise this power in cases of gross fiscal mismanagement and/or proven cases of systematic corruption, such as mafia infiltration (Galletta, 2017).

[14] See e.g. Evans (2000); Manza and Brooks (2008); Oesch (2006); Rennwald (2020); Carnes and Lupu (2015).

[15] They receive small lump-sum payments to compensate for time spent in council sessions.

variables that capture the shares of a municipality's council posts, that were obtained by individuals from each of these social classes. Figure 4 plots the distributions of these variables, per election round.

Figure 4: Over-time change in the class composition sampled municipal councils




*Note:* Based on data from municipalities with 4000-6000 inhabitants. Dashed line indicates time at which electoral formula was changed. Data pooled into election rounds, where bracketed year indicates the modal calendar year within a given election cycle.

Some points deserve specific elaboration. First, a noticeable decline in statistical representation is experienced by persons from the lower-middle class. At least in part, this tendency reflects general macro-trends in the Italian labor market, detectable since the early 1990s. A large amount of clerical mid-skill jobs have disappeared from Italy in the past thirty years—and, as a result, larger shares of the working-age population now have either high- or low-skill jobs (Basso, 2020; Goos et al., 2009). Instead, increasing numbers of upper-middle class persons entered local politics throughout the 1990s (+ 10 percentage points), as did labor market inactives (+ 4 points). The latter group consists principally of pensioners: the educational profiles these suggest they are, on average, a mix of individuals from the lower- and upper-middle classes.

Also detectable is a decline in working class representation. In the immediate pre-reform election round—when elections took place mainly in 1990—working class legislators made up roughly 14% of the average municipal council included in the sample. By the early 2000s, this number was down to 10%. This downward trend is noteworthy, as Italian employment growth since the early 2000s has occurred principally among working class occupational groups. The country has seen an overall decline in the high-skill share of the labor market since the mid-2000s (Basso, 2020) and job creation has primarily occurred in low-skill hospitality sectors. To summarize, the broad trends displayed by the data thus suggest that

there is an increasing deficit in working class representation over time. Overall, the deficit is also notable in size throughout the whole period examined. During this time, labor market sociologists estimate that the working class to have constituted between 40-60% of the Italian active labor market force.

### 3.3.1 Electoral returns

For mechanism tests, I leverage data on turnout and party-level electoral returns. Post-reform data is collated from the digital elections repository of the Italian Ministry of Interior Affairs. To complement this, I manually clean and collate data from pre-reform elections rounds, centered around the calendar years of 1990 and 1985.[16] For this, I draw on hard copy electoral volumes obtained from the Ministry's elections archive in Rome. Focusing on elections that took place in municipalities with between 3000 and 7000 inhabitants, this yields information on the vote and seat shares of 10888 unique parties, nested in 2722 elections (97% of elections in primary sample).

A limitation of the legislator-level data is that individual legislators are linked to parties through standardized administrative labels rather than ballot-specific list identifiers. This is particularly consequential in smaller municipalities, where multiple local parties—dubbed *Civic Lists*—often compete simultaneously yet are recorded under the same generic administrative category. As a result, individual legislators cannot be matched with certainty to the exact electoral list through which they entered office. This limits fine-grained party-level analysis, particularly when tracing the origin of specific candidates. It does not, however, affect the identification of institutional treatment effects, which rely on municipality-level variation in electoral rules rather than on candidate assignment to individual lists.

## 3.4 Model choices

To test my theory, I run a series of linear WLS regressions to obtain RD estimates of local average treatment effects (LATEs). I calculate distinct RD estimates for the periods before the formula change, as well as after. The difference between these RD estimates can, in turn, be interpreted as the diff-in-disc estimate that I am principally concerned with. The baseline RD model can be formally expressed as:

$$Y_{ijt} = \alpha + \tau(D_{it}) + \beta(X_{it} - c) + \gamma(X_{it} - c) * D_{it} + \theta(W_i) + \lambda(Z_{it}) + \epsilon_{it} \quad (1)$$

where $Y_{it}$ is a continuous variable indicating the fraction of council seats in a given municipality $i$,

[16] The data partially allows for automated extraction procedures; however, this induces exceptionally high error rates due to the characteristics of its tabular presentation, suboptimal font choice and issues of bleed-through caused by low paper quality.

that is held by a given social class $j$, after an election round $t$. As Italian local elections take place during disparate calendar months and years, I use the election round as a time indicator to better capture election cycle effects. The election round indicator is centered around the set of elections that take place directly after 28 March 1993: i.e. when electoral systems were homogenized across all municipalities in my sample.

$X_{it}$ is the running variable, denoting the population size of municipality $i$ at time $t$; $c$ represents the legal threshold value of 5000 inhabitants, and $D$ is a dummy that takes the value of 1 if a municipality's population size exceeds this threshold, or 0 if elsewise. $W_i$ denotes the inclusion of election round fixed effects, as data is pooled across multiple election cycles.

$Z_{it}$ is a vector of control variables, all of which are indexed to time $t$. First, these include region fixed effects, where the region is coded at the NUTS 2 level. Second, I introduce controls capturing the socio-economic composition of the individual municipality, which may affect the available supply of potential legislators with working class occupations. These are a municipality's unemployment rate and an indicator of municipality-level educational attainment, operationalized as the proportion of the local population that is in possession a tertiary schooling degree. Both these controls are generated from Italian decennial census data, where time is indexed based on the census closest in time to a given council election (see appendix table A.1). Third, I include a lagged treatment indicator dummy, which adjusts for potential sorting imbalances (Eggers et al., 2018) and ensures that obtained effects are not driven by treatment reassignment after a given census.[17] In robustness checks, I introduce two additional control variables that can influence the ability of local officials to inflate census figures: a log ratio of young/old inhabitants, as well as the proportion of vacation homes within the municipality.

RD estimates are derived using local polynomial methods, developed by Cattaneo et al. (2020). In this, two linear regressions are fitted separately on outcomes: one on each side of the cut-off point. LATEs are thereafter obtained via examining the local differences between intercepts of the regressions. I opt for a uniform kernel linear model, where the latter choice reduces the risk of overfitting and erratic behavior near the cut-off (Gelman and Imbens, 2019). As the density of observations differs around either side of the discontinuity threshold, I compute MSE-optimal bandwidths as per Calonico et al. (2020), which allow for separate bandwidths sizes on each side of the cut-off point. Given that bandwidths are determined via a data-driven procedure, bandwidth sizes—and, in extension, the number of observations—varies across models run on different outcome variables. In practice, the estimator produces consistent

[17] The latter can in theory occur if municipality inhabitant numbers systematically change in all sampled municipalities over time, driven by processes of e.g. urbanization.

bandwidths, and always restricts the analysis to municipalities with between 3600 to 7500 inhabitants. Other important variables, such as legislature size, thus remain constant across municipalities assigned to treatment vs. control, during the entirety of the time span examined (see table 1).

Finally, to obtain diff-in-disc estimates, I calculate the difference across pre- and post-reform RD estimates. Like Klašnja and Titiunik (2017) and Bellodi and Morelli (2026), I derive standard errors of these estimates as:

$$SE_{DD} = \sqrt{SE^2_{RD|t=1} + SE^2_{RD|t=0}} \tag{2}$$

where $t = 1$ refers to the post-reform period, and $t = 0$ to the period before reform adoption. RD estimate standard errors are clustered at the municipality level, i.e. the level of treatment assignment.

# 4 Results

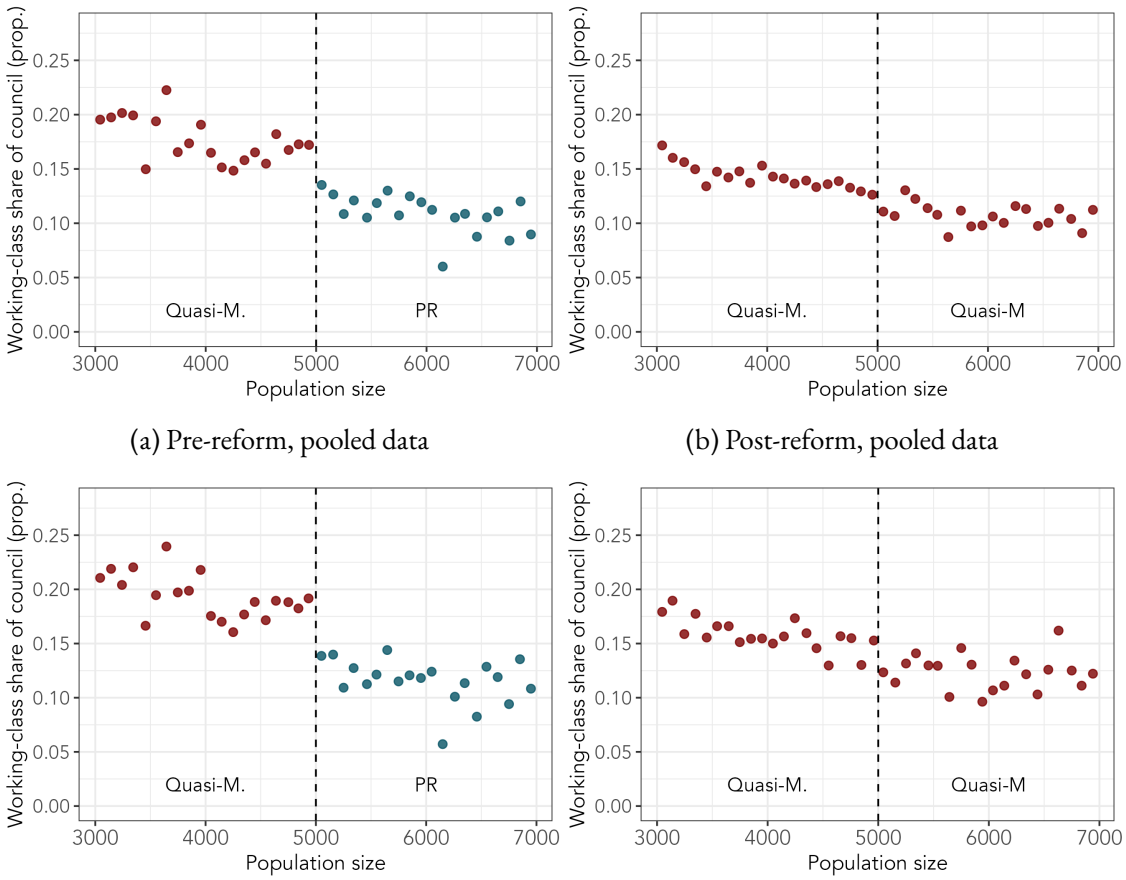
The empirical analysis proceeds in four steps. First, I estimate the effect of seat inflation on working-class representation (H1) and assess whether these effects persist across electoral cycles (H2). I then examine whether the mechanism is consistent with stable candidate selection patterns (H3). Next, I test whether the effect persists even as reforms reduce the number of parties entering the legislature (H4), while ruling out alternative explanations based on electoral behavior by assessing changes in party vote shares, voter turnout, and the ideological orientation of election-winning parties (H5–H7). Finally, I examine how the effects of seat-inflation rules vary across municipalities with differing levels of party competition under proportional representation (H8).

## 4.1 Seat Inflation and Working-Class Representation

I begin by evaluating the core implication of H1: whether the quasi-majoritarian reform increased the numerical presence of working-class representatives in municipal councils. First, I provide graphical evidence of quasi-majoritarian reform effects on the numerical presence of working-class persons in municipality councils. Figure 5 displays this in RD plots, split by reform period. Consistent with theory, the outcome grows visibly discontinuous at the cut-off point before the reform, as councils under the majority-assuring system consist to a larger share of workers. Importantly, this difference is not simply an artifact of cases clustered just around the 5000-inhabitant threshold, where sorting might bias results. The discontinuity dissipates post-reform when all elections become majority-assuring, and outcomes grow fully

linear at the cutoff. Sub-figures 5c and 5d further highlight that this change in difference is detectable even when comparing only immediate pre- and post-reform elections.

Figure 5: RDD plots on working class representation in councils — Pre. vs. Post-reform




(a) Pre-reform, pooled data

(b) Post-reform, pooled data

(c) Immediate pre-reform election round

(d) Immediate post-reform election round

*Note:* Plot displays binned average values of the dependent variable (working class share of council) by municipality population size.

Table 3 presents the magnitude and statistical significance of the reform's effects. The Diff-in-Disc coefficients are positive and statistically significant across all specifications, indicating that quasi-majoritarian elections lead to more descriptively class-inclusive councils. Estimated effects range from 2.2 to 4.0 percentage points and reflect a substantial narrowing of pre-existing disparities: in the most conservative specifications, the gap in working-class representation between treatment and control municipalities is reduced by nearly 50 percent relative to the pre-reform difference.

To assess robustness, column (2) adjusts for variation in the potential supply of working-class legislators through the inclusion of relevant controls. Columns (3) and (4) further include covariates identified by Eggers et al. (2018) as correlates of manipulative sorting in Italian municipalities. While treatment effects diminish somewhat in these latter specifications, they remain comparable in size, directionally con-

sistent with the hypothesis, and statistically significant at the 90% confidence level.

Table 3: Electoral reform effects on working-class representation in councils

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Diff-in-disc: $RD_1$ - $RD_0$ | 0.040*** | 0.032** | 0.022* | 0.023* |
| | (0.015) | (0.014) | (0.013) | (0.013) |
| $RD_0$: Pre-reform | -0.065*** | -0.055*** | -0.046*** | -0.047*** |
| | (0.013) | (0.011) | (0.011) | (0.011) |
| $RD_1$: Post-reform | -0.025*** | -0.023*** | -0.024*** | -0.024*** |
| | (0.008) | (0.008) | (0.007) | (0.007) |
| *Covariate-adjustments* | | | | |
| Region FEs | ✓ | ✓ | ✓ | ✓ |
| Election-round FEs | ✓ | ✓ | ✓ | ✓ |
| Lagged treatment assignment | ✓ | ✓ | ✓ | ✓ |
| Unemployment | | ✓ | | |
| Edu. attainment | | ✓ | | |
| Log age ratio | | | ✓ | ✓ |
| Vacation homes | | | ✓ | ✓ |
| Off-cycle election | | | | ✓ |
| *Bandwidths & Ns* | | | | |
| $RD_0$: $h_l \mid h_r$ | 1037 \| 2073 | 1096 \| 2219 | 1331 \| 2482 | 1288 \| 2405 |
| $RD_0$: $N_l \mid N_r$ | 778 \| 2104 | 823 \| 2089 | 956 \| 2032 | 931 \| 1887 |
| $RD_1$: $h_l \mid h_r$ | 978 \| 1721 | 827 \|1493 | 1042 \| 1798 | 1020 \| 1760 |
| $RD_1$: $N_l \mid N_r$ | 1549 \| 5817 | 1254 \| 6087 | 1661 \| 5723 | 1624 \| 5710 |

*Notes:* Diff-in-disc estimates are calculated as differences between pre- and post-reform RD estimates. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust bias-corrected standard errors clustered at the municipality level are reported in parentheses. Estimates use local linear regressions with a uniform kernel and two-sided MSE-optimal bandwidths. See Figure 2 for details on control variable construction.

The size of the estimated treatment effect merits substantive contextualization. A three percentage point increase in working-class representation corresponds to roughly half a council seat on average. While modest in absolute terms, this effect is comparable to those found in studies of more overt institutional interventions. For example, studies typically find that gender parity laws in European local elections have led to increases of 4 to 7 percentage points in female office-holding in municipal legislatures.[18]

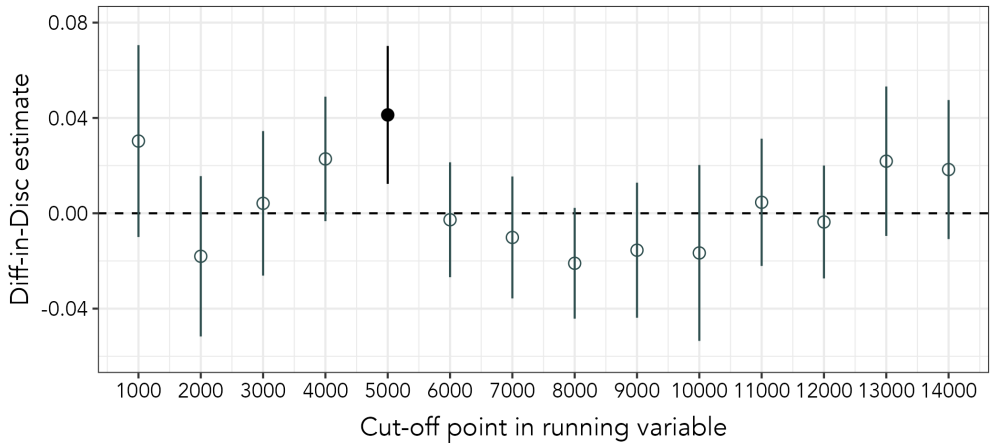
Notably, the reform occurs during a period in which working-class representation in Italian municipal councils was otherwise declining. This pattern is consistent with the broader theoretical framework developed in the paper. In addition to introducing majority-assuring seat allocation rules, the 1993 reform also reduced the size of municipal councils on both sides of the 5,000-inhabitant threshold by four seats

[18] See e.g. Bagues and Campa (2021) for quota effects in Spain, De Paola et al. (2010) for Italy, and Lassébie (2020) for France.

(see Table 1). Smaller legislatures restrict access to office by limiting the number of candidates who can enter councils. In isolation, such reductions serve to reduce descriptive representation (see e.g. Becher and González, 2019; Matakos et al., 2024). The fact that working-class representation nevertheless increases at the threshold suggests that the seat-inflation mechanism associated with majority bonuses offset these countervailing institutional pressures.

To probe the credibility of my main finding, I conduct a series of falsification and robustness tests. First, I conduct placebo tests using 13 alternative cut-off points among municipalities with single-round elections (see Figure 6). None of these yield effects comparable in magnitude or statistical significance. Second, I estimate 200 covariate-adjusted models using symmetric bandwidths around the 5,000-inhabitant threshold (see Appendix D.1). Imposing identical bandwidth limits on either side of the cut-off reduces and stabilizes the estimated effect at 2.8 percentage points. Nevertheless, the effect remains statistically distinguishable from zero at or near the 95% confidence threshold across all specifications ($0.059 \leq p \leq 0.054$). Lastly, I perform doughnut estimations, in which I exclude observations closest to the 5000-inhabitant threshold, to further account for potential sorting bias. Results highlight that treatment effects are stable and consistently statistically significant at the 95% confidence level, across several exclusion bands (see Appendix D.2).

Figure 6: Diff-in-disc estimates of working class representation at placebo cut-offs




*Note:* Bars indicate 95% robust bias-adjusted confidence intervals, derived from municipality-clustered standard errors. All estimates control for lagged treatment variable, and adjust for region and election round fixed effects. Dark coefficient denotes cut-off for main analysis. Estimate from test at 4000 inhabitant cut-offs is significant at the the 90% confidence interval (no policies change at this threshold); all others are statistically insignificant. Mayoral wage rates are subject to kinks at 1000, 5000, and 10000 inhabitant cut-offs.

The fact that workers entered office at higher rates following the majority-assuring reform naturally implies that individuals from other social backgrounds became less likely to do so. This pattern is evident

in Table 4, which reports estimates for the numerical representation of other social classes. The main difference under the quasi-majoritarian system is a decline in the presence of upper-middle-class individuals, whose representation in municipal councils falls by 4.9 percentage points. These results are highly consistent across model specifications (see Appendix D.3). Nearly the entire decline appears to be absorbed by the increase in working-class representation, implying a substitution effect under the new electoral rules.

Table 4: Electoral reform effects on middle-class, and labor market inactive representation in councils

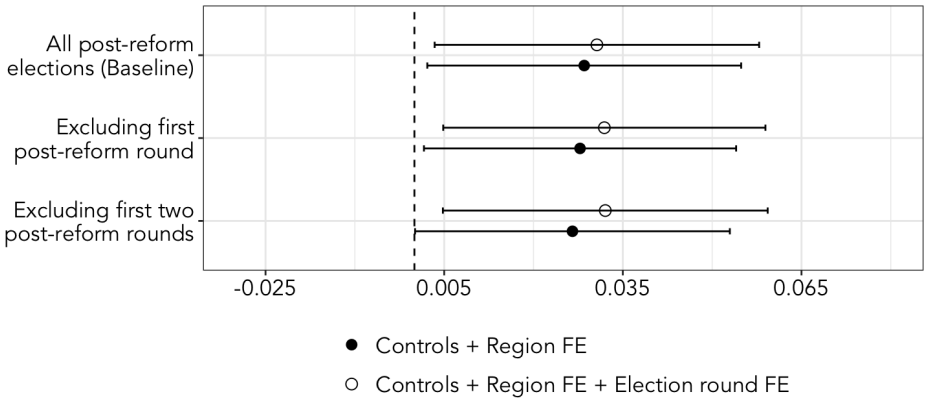
| *Outcome* | (1) Upper-middle | (2) Lower-middle | (3) LM Inactive |
|---|---|---|---|
| Diff-in-disc: $RD_1$ - $RD_0$ | -0.049*** | 0.026 | 0.003 |
| | (0.016) | (0.016) | (0.015) |
| $RD_0$: Pre-reform | 0.044*** | 0.008 | -0.001 |
| | (0.012) | (0.013) | (0.013) |
| $RD_1$: Post-reform | -0.005 | 0.035*** | 0.002 |
| | (0.011) | (0.010) | (0.008) |
| *Covariate-adjustments* | | | |
| Region FEs | ✓ | ✓ | ✓ |
| Election-Round FEs | ✓ | ✓ | ✓ |
| Lagged treatment ass. | ✓ | ✓ | ✓ |
| *Bandwidths & Ns* | | | |
| $RD_0$: $h_l \mid h_r$ | 1290 \| 2382 | 1032 \| 2153 | 628 \| 1338 |
| $RD_0$: $N_l \mid N_r$ | 975 \| 2312 | 776 \| 2782 | 431 \| 2748 |
| $RD_1$: $h_l \mid h_r$ | 1085 \| 1818 | 1019 \| 1765 | 932 \| 1659 |
| $RD_1$: $N_l \mid N_r$ | 1748 \| 5571 | 1626 \| 5916 | 1483 \| 5946 |

*Notes:* Diff-in-disc estimates are calculated as differences between pre- and post-reform RD estimates. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust bias-corrected standard errors clustered at the municipality level are reported in parentheses. Estimates use local linear regressions with a uniform kernel and two-sided MSE-optimal bandwidths.

Finally, I probe institutional persistence (H2): i.e. whether positive reform effects on working-class representation endure beyond the immediate post-reform period. Figure 3 in the research design section already suggests considerable stability, as election round-specific RD estimates remain consistently positive across successive rounds. As a stronger test, I additionally conduct a set of sensitivity analyses in which I successively exclude the earliest post-reform elections when re-estimating the baseline specification: first, all elections from the immediate post-reform round, and second, all elections from both the first and second post-reform rounds. Figure 7 presents the resulting diff-in-disc estimates under specifications both with and without election-round fixed effects. Comparing the two allows me to assess whether the observed persistence reflects a stable treatment effect or instead broader election-specific shocks com-

mon to all municipalities. Across all estimations, coefficients remain positive, statistically significant at conventional levels, and highly stable in magnitude, providing strong evidence in support of H2.

Figure 7: Persistence in treatment effects - Diff-in-Disc Estimates




*Note:* Bars indicate 95% robust bias-adjusted confidence intervals, derived from municipality-clustered standard errors. Aside from mentioned FE, all specifications control for lagged treatment variable, educational attainment and unemployment.

## 4.2 Upper-Tier Candidacies and Stable Selectorate Preferences

Having shown that seat inflation increases the proportion of working-class legislators in municipal councils, I next examine whether this change reflects a shift in selectorate preferences (H3). The most direct test would be to analyze party list rankings, but Italian authorities do not systematically collect candidate-list data for municipal elections, and pre-2010 information is largely unavailable. I therefore rely on an indirect proxy based on executive-linked officials.

In the municipalities examined here, elected officials are permitted to cumulate mandates across the legislature and the executive, by serving on the municipality's executive committee. Executive committee members may be drawn either from within or outside the council, but in practice they are overwhelmingly selected from among sitting councillors: among municipalities with 3,000 to 10,000 inhabitants in my sample, 95% of executive committee members also served in the legislature. Because executive members play a central role in drafting and advancing legislation, parties have strong incentives to ensure the election of politically central candidates who are likely to assume executive responsibilities. This makes executive-linked legislators a plausible proxy for candidates occupying electorally secure positions, even if the measure does not directly observe list rankings.

As shown in Table 5, I find no evidence that the social composition of executive committee members changes following the reform. Additional tests likewise show no reform effects on middle-class represen-

tation or the representation of labor-market inactives in executive committees (Appendix Table C.1). This pattern is consistent with stable selectorate priorities: parties do not appear to alter the socio-economic composition of candidates occupying the most politically influential positions in municipal government.

Table 5: Electoral reform effects on working-class representation in executive committees

| Model | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Diff-in-disc: $RD_1$ - $RD_0$ | -0.006 | 0.005 | -0.005 | -0.010 |
| | (0.022) | (0.021) | (0.021) | (0.021) |
| $RD_0$: Pre-reform | -0.025 | -0.031* | -0.024 | -0.019 |
| | (0.019) | (0.019) | (0.018) | (0.018) |
| $RD_1$: Post-reform | -0.031*** | -0.026** | -0.029*** | -0.029*** |
| | (0.011) | (0.010) | (0.011) | (0.011) |
| *Covariate-adjustments* | | | | |
| Region FEs | ✓ | ✓ | ✓ | ✓ |
| Election-Round FEs | ✓ | ✓ | ✓ | ✓ |
| Lagged treatment ass. | ✓ | ✓ | ✓ | ✓ |
| Unemployment rate | | ✓ | | |
| Edu. attainment | | ✓ | | |
| Log age ratio | | | ✓ | ✓ |
| Frac. vacation homes | | | ✓ | ✓ |
| Off-cycle election | | | | ✓ |
| *Bandwidths & Ns* | | | | |
| $RD_0$: $h_l \mid h_r$ | 865 \| 1585 | 869 \| 1633 | 937 \| 1880 | 1002 \| 1901 |
| $RD_0$: $N_l \mid N_r$ | 596 \| 1878 | 599 \| 1824 | 629 \| 1852 | 684 \| 1789 |
| $RD_1$: $h_l \mid h_r$ | 932 \| 1673 | 957 \| 1707 | 954 \| 1666 | 951 \| 1669 |
| $RD_1$: $N_l \mid N_r$ | 1449 \| 5532 | 1483 \| 5735 | 1480 \| 5494 | 1478 \| 5492 |

*Notes:* Diff-in-disc estimates are calculated as differences between pre- and post-reform RD estimates. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust bias-corrected standard errors clustered at the municipality level are reported in parentheses. Estimates use local linear regressions with a uniform kernel and two-sided MSE-optimal bandwidths.

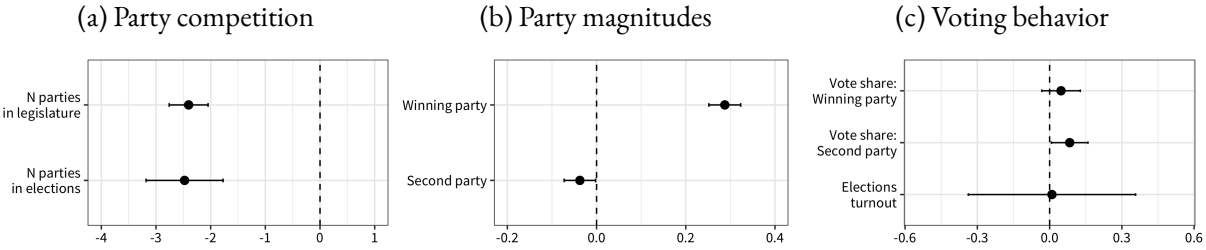
Because executive appointments occur after elections and committee members can also be appointed from outside the legislature, this proxy cannot definitively identify list-ranking decisions. Nonetheless, the absence of any detectable change at the executive level suggests that parties did not fundamentally revise the socio-economic composition of their governing core following reform. Instead, the increase in working-class representation appears concentrated among rank-and-file council members, consistent with the argument that seat inflation expands access to office primarily by extending representation deeper into party lists.

## 4.3 Party System Concentration and Electoral Behavior

Next, I examine whether the reform altered party system concentration and voter behavior. More specifically, the theoretical framework implies that the observed increase in working-class representation should occur despite a reduction in the number of effective parties in government, and without significant changes in other dimensions of electoral behavior. To evaluate these propositions, I conduct a series of tests using alternative electoral outcomes, drawing on archival data on vote shares and legislative returns.

Figure 8 presents a first set of results. As anticipated, the reform led to a clear consolidation of the local party system (H4). Figure 8a shows that, on average, two fewer parties gained legislative representation following the reform ($p < 0.001$). Similarly, the number of parties actively contesting elections declined by 2.3 on average ($p < 0.001$). These patterns are consistent with electoral coordination under more majoritarian rules: as legislative rewards become concentrated, competition narrows around fewer viable contenders.

Figure 8: Diff-in-disc estimates of reform effects on electoral system traits




*Notes:* RD bandwidths fixed to MSE optimal for working class representation in municipal councils. Bars indicate robust bias-corrected 95% confidence intervals. All estimations adjust for region fixed effects, election round fixed effects, educational attainment, unemployment and a lagged treatment assignment indicator. See table C.2 in appendix for full regression results.

To contextualize these shifts, it is useful to note that prior to reform an average of 5.4 parties contested elections in municipalities operating under proportional representation. At the same time, the reform sharply increased the legislative dominance of winning parties. As shown in Figure 8b, the average seat share of the election-winning party rose by 26 percentage points ($p < 0.001$), corresponding to approximately 4.1 additional seats in a 16-member council. By contrast, the second-largest party lost 3.3 percentage points in seat share, equivalent to roughly half a seat ($p < 0.1$).

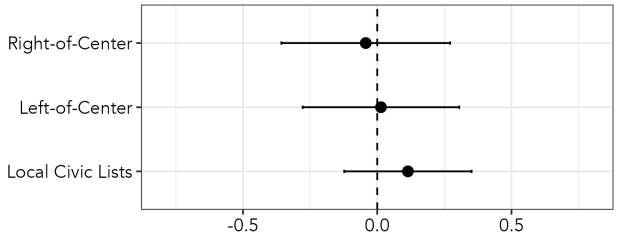
Importantly, these shifts in party magnitude were not driven by changes in electoral demand. Figure 8c shows no statistically significant effect on either the vote share of the leading party (H5), or voter turnout (H6; $p > 0.1$ for both). Notably, the vote share of the second-largest party increased by 9.4 percentage points following reform ($p < 0.01$), yet this gain failed to translate into additional seats. This

illustrates the increased disproportionality induced by the majority bonus: even where runner-up parties gained electoral support, revised seat-allocation rules prevented those gains from converting into legislative representation.

The underlying reason lies in the distribution of pre-reform vote shares. Under the old proportional system, winning parties already secured relative majorities on average (47% of votes), while second-ranked parties remained substantially behind (26% on average). Under majority-assuring rules, gains by runner-up parties therefore often remained insufficient to displace the leading list, even when electoral support increased. The post-reform expansion of winning-party seat shares thus reflects a change in how votes were translated into seats rather than a shift in underlying voter preferences.

As a final test of electoral behavior, I examine whether the reform systematically improved the electoral fortunes of parties that directly target working-class constituencies (H7). To do so, I classify the party family of the mayoral party or governing coalition using coded electoral returns.[19] Outcomes are coded as binary indicators for whether the winning party or coalition is classified as: (a) left-of-center, (b) right-of-center, or (c) civic list, defined as local parties without representation in regional or national politics. Coding follows the ideological classification scheme of Döring et al. (2022), which provides the most comprehensive categorization of historical Italian parties. Left-of-center parties include Communist and Social Democratic parties; right-of-center parties include Christian Democratic, Conservative, and Liberal parties.[20]

Figure 9: Diff-in-disc estimates of reform effects on mayoral party ideology




*Notes:* RD bandwidths set to two-sided MSE optimal as per (Calonico et al., 2015). Bars indicate robust bias-corrected 95% confidence intervals. All estimations adjust for region, election round fixed effects, educational attainment, unemployment and a lagged treatment assignment indicator. See table C.3 in appendix for full regression results.

As shown in Figure 9, the reform did not systematically alter the ideological orientation or local character of ruling parties. Increased disproportionality therefore did not induce voters to favor parties that

[19] Mayors are often affiliated with more than one party list. In such cases, coding is applied at the coalition-list level.

[20] Green parties, radical right parties, and special-issue parties are not substantively present in the municipalities studied here. 'Coalizione Area Governativa' coalitions prevalent in pre-1992 records are coded as left-of-center, as Christian Democrats typically contested with separate lists in local elections before 1993, making these coalitions predominantly PSI-led. Reclassifying them as right-of-center does not substantively alter the findings.

were more socially inclusive in class terms, further supporting the interpretation that the observed gains in working-class representation reflect institutional restructuring rather than electoral realignment.

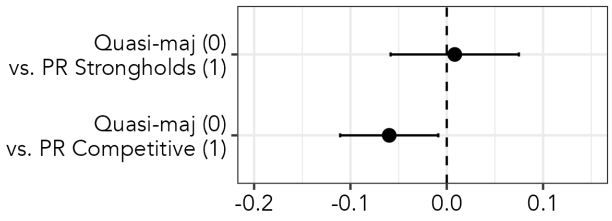
## 4.4 Strongholds vs. Competitive Polities under PR

As a final probe of the mechanism, I examine whether pre-reform differences in working-class representation varied across municipalities with different levels of electoral competition. The purpose of this exercise is not to establish an additional causal effect, but to assess whether pre-reform patterns align with the argument that larger party seat shares deepen list penetration and thereby facilitate working-class inclusion.

As discussed by (Caria et al., 2023), a subset of municipalities operating under PR prior to the 1993 reform already exhibited de facto majoritarian outcomes, with single parties securing absolute majorities in local councils (48% of the pre-reform sample). In these strongholds, mayoral parties routinely captured more than 50 percent of the vote, producing clear legislative dominance even before electoral rules were altered. Where party seat shares were already large under PR, majority-assuring rules should generate comparatively limited additional list penetration.

To examine this implication, I first estimate separate pre-reform RD models for stronghold municipalities and electorally competitive ones. If the mechanism outlined in this paper is relevant, pre-reform discontinuities between quasi-majoritarian and PR municipalities should appear more clearly where election-winning parties remained more electorally seat-constrained under PR. The subgroup RD estimates shown in Figure 10 are broadly consistent with this expectation. Pre-reform differences between quasi-majoritarian and PR municipalities are concentrated among electorally competitive municipalities, where no single party secured a stable governing majority under proportional representation. By contrast, no comparable discontinuity is visible when comparing quasi-majoritarian municipalities to local strongholds.

Figure 10: RD estimates of electoral system effects on working class rep. in councils (pre-reform)




*Notes:* RD bandwidths set to two-sided MSE optimal as per (Calonico et al., 2015). Bars indicate robust bias-corrected 95% confidence intervals. All estimations adjust for region, election round fixed effects, educational attainment, unemployment and a lagged treatment assignment indicator. See table C.4 in appendix for full regression results.

A complementary descriptive comparison reinforces this interpretation. Among municipalities operating under proportional representation before the reform, working-class representation was significantly higher in local strongholds than in electorally competitive municipalities (12.4 versus 10.3 percent; $p < 0.01$). This pattern is consistent with the argument that larger winning-party seat shares deepen list penetration even in the absence of formal seat-inflation rules. Taken together, these findings suggest that party seat magnitude conditions how electoral rules translate into class-based representation.

# 5 Discussion

Taken together, the findings of this paper show that disproportionality in electoral formulas can strengthen working-class representation in closed-list PR. The central mechanism is institutional: by inflating the seat share of election-winning parties, disproportional rules increase the depth with which party lists convert into office, thereby extending representation beyond upper-ranked candidates. Consistent with this argument, the 1993 reform increased the number of working-class legislators elected to municipal councils, largely at the expense of upper-middle-class representatives. These gains persisted across electoral cycles and emerged despite a clear reduction in party-system fragmentation, without corresponding changes in vote shares, voter turnout, or the ideological orientation of governing parties. Moreover, they were concentrated in municipalities where pre-reform party competition kept winning-party seat shares comparatively low under PR. At the same time, the social composition of politically central executive candidates remained unchanged, suggesting that parties did not fundamentally revise upper-tier candidate priorities following reform. Overall, the evidence suggests that electoral formulas shape class-based descriptive representation chiefly by altering how far party lists penetrate into office, while leaving underlying patterns of hierarchical candidate selection largely intact.

One important scope condition concerns the local character of the empirical setting. Party incentives to include working-class candidates may be especially strong in municipal elections, where working-class voters constitute a numerically large and geographically diffuse share of the electorate. Unlike highly educated professionals, who concentrate in larger urban labor markets, working-class populations remain broadly distributed across municipalities. This implies that the local median voter is often socioeconomically closer to the working class than her national counterpart, making working-class candidacies more electorally valuable in local elections. At the same time, this does not necessarily limit the broader

relevance of the findings. Local office often constitutes a critical entry point in political careers, particularly for candidates from working-class backgrounds, who face steeper barriers to promotion into higher office because their professional trajectories are less readily interpreted as politically meritorious by party selectorates. Expanding access to local office may therefore matter beyond the municipal level by shaping who becomes available for advancement within broader party hierarchies.

A second question concerns external validity beyond Italy. First, while Italy provides a rich empirical setting because of its dense municipal structure, this is not unusual: large numbers of municipalities continue to shape local political representation across much of Western Europe, including Germany, France, Spain, and Switzerland. Second, Italy is also marked by pronounced internal territorial heterogeneity. Differences between northern and southern regions in socio-economic development, labor-market structure, and local political organization are substantial, allowing the mechanism examined here to be observed across markedly different social contexts within a common institutional setting.

The argument developed here focuses on disproportionality generated through seat-allocation formulas, but similar dynamics may arise through other dimensions of electoral-system design. In PR, effective disproportionality is also shaped by legislature size and formal vote thresholds, both of which influence how many seats parties secure and therefore how deeply candidate lists convert into office. Smaller legislatures and higher entry thresholds reduce party magnitudes even when allocation formulas remain formally proportional, potentially reinforcing the same representational constraints identified here. While legislature size has received some attention in recent work, the implications of formal vote thresholds for class-based descriptive representation remain comparatively underexplored.

More broadly, the findings suggest that research on descriptive representation should move beyond treating proportional and majoritarian systems as fixed opposites in their inclusive consequences. Electoral-systems scholarship has long emphasized that disproportionality varies within both families of electoral rules, yet work on social inclusion still often assumes that greater proportionality uniformly benefits underrepresented groups. The results presented here qualify that expectation and suggest that limited increases in disproportionality can, under certain conditions, strengthen the descriptive representation of groups that remain disadvantaged under fully proportional rules.

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# Appendix

## A Data

### A.1 Covariate variables

Table A.1: Census data used for municipality-level control variables

| Variable operationalization | Census wave | Used for period: |
|---|---|---|
| *Lagged treatment indicator* | | |
| Dummy coded as 1 if population > 5000, 0 elsewise | Census 1991 | 2001-2009 |
| Dummy coded as 1 if population > 5000, 0 elsewise | Census 1981 | 1992-2001 |
| Dummy coded as 1 if population > 5000, 0 elsewise | Census 1971 | 1983-1991 |
| *Educational attainment* | | |
| Fraction of population aged 9 and above with tertiary degree | Census 2011 | 2007-2009 |
| Fraction of population aged 6 and above with tertiary degree | Census 2001 | 1997-2006 |
| Fraction of population aged 6 and above with tertiary degree | Census 1991 | 1983-1996 |
| *Unemployment* | | |
| Fraction of population in unemployment, aged 15 and above | Census 2011 | 2007-2009 |
| Fraction of population in unemployment, aged 15 and above | Census 2001 | 1997-2006 |
| Fraction of population in unemployment, aged 15 and above | Census 1991 | 1983-1996 |
| *Log age ratio* | | |
| Log of Under-20 / Over-65 ratio | Census 1991 | 2001-2009 |
| Log of Under-20 / Over-65 ratio | Census 1981 | 1992-2001 |
| Log of Under-20 / Over-65 ratio | Census 1971 | 1983-1991 |
| *Vacation homes* | | |
| Fraction of properties that are vacation homes | Census 1991 | 1983-2009 |

## A.2 Elections data

Pre-1991 archival data on municipal election turnout, blank voting and party-level electoral returns is extracted from historical election dossiers of the Italian Ministry of Interior Affairs. Table A.2 provides an overview of primary source material used to generate these variables; raw data from elections that took place in 1985-1989 was digitized and shared by Caria et al. (2023).

Table A.2: Municipal elections data

**Municipal elections of 1988**

- Ministero dell'Interno dell'Italia (1991), *Elezioni amministrative del 1988: Risultati*, Roma: Istituto Poligrafico e Zecca dello Stato, pp. 1-479.

**Municipal elections of 1990**

- Ministero dell'Interno dell'Italia (1996), *Elezioni communali del 6 Maggio 1990, Volume VI, Risultati nei comuni a sistema maggioritario, Parte I: Italia Settentrionale (Tomo I)*, Roma: Istituto Poligrafico e Zecca dello Stato, pp. 1-1072.
- Ministero dell'Interno dell'Italia (1995), *Elezioni communali del 6 Maggio 1990, Volume VI, Risultati nei comuni a sistema maggioritario, Parte I: Italia Settentrionale (Tomo II)*, Roma: Istituto Poligrafico e Zecca dello Stato, pp. 1079-1570.
- Ministero dell'Interno dell'Italia (1995), *Elezioni communali del 6 Maggio 1990, Volume VI, Risultati nei comuni a sistema maggioritario, Parte II: Italia Centrale*, Roma: Istituto Poligrafico e Zecca dello Stato.
- Ministero dell'Interno dell'Italia (1995), *Elezioni communali del 6 Maggio 1990, Volume VI, Risultati nei comuni a sistema maggioritario, Parte III: Italia Medidionale e Insulare*, Roma: Istituto Poligrafico e Zecca dello Stato.
- Ministero dell'Interno dell'Italia (1992), *Elezioni communali del 6 Maggio 1990, Volume V, Risultati nei comuni a sistema proporzionale, Parte I: Italia Settentrionale*, Roma: Istituto Poligrafico e Zecca dello Stato.
- Ministero dell'Interno dell'Italia (1992), *Elezioni communali del 6 Maggio 1990, Volume V, Risultati nei comuni a sistema proporzionale, Parte II: Italia Centrale, Medidionale e Insulare*, Roma: Istituto Poligrafico e Zecca dello Stato.

**Municipal elections of 1989, 1991-2010**

- Digital elections depository of the Italian Ministry of Interior Affairs (Eligendo)

Table A.3: Municipal councils, by election year and election round

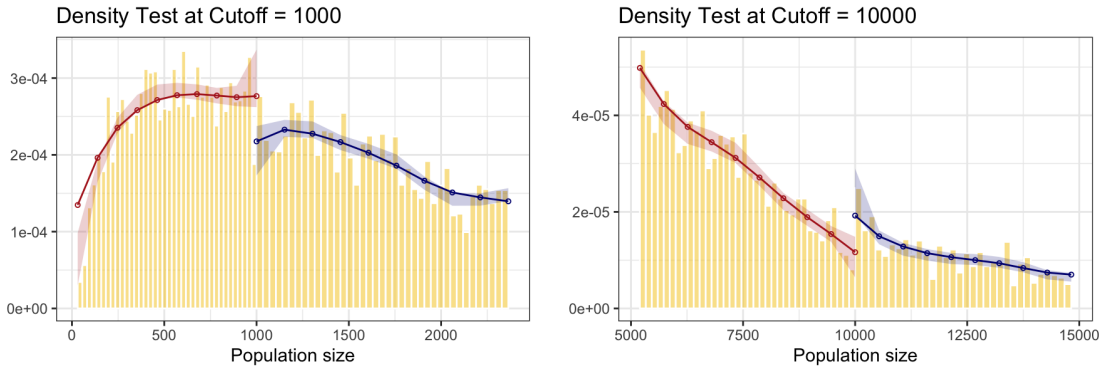
| | *Election Round* | | | | | | |
|---|---|---|---|---|---|---|---|
| *Election year* | 1st pre-reform round | 2nd pre-reform round | 1st post-reform round | 2nd post-reform round | 3rd post-reform round | 4th post-reform *Total* | |
| 1983 | 22 | 0 | 0 | 0 | 0 | 0 | 22 |
| 1984 | 80 | 1 | 0 | 0 | 0 | 0 | 81 |
| 1985 | 4983 | 15 | 0 | 0 | 0 | 0 | 4998 |
| 1986 | 32 | 0 | 0 | 0 | 0 | 0 | 32 |
| 1987 | 45 | 30 | 0 | 0 | 0 | 0 | 75 |
| 1988 | 74 | 864 | 0 | 0 | 0 | 0 | 938 |
| 1989 | 9 | 156 | 0 | 0 | 0 | 0 | 165 |
| 1990 | 73 | 5,075 | 0 | 0 | 0 | 0 | 5,148 |
| 1991 | 0 | 77 | 0 | 0 | 0 | 0 | 77 |
| 1992 | 0 | 149 | 0 | 0 | 0 | 0 | 149 |
| 1993 | 0 | 0 | 1,139 | 0 | 0 | 0 | 1,139 |
| 1994 | 0 | 0 | 414 | 35 | 0 | 0 | 449 |
| 1995 | 0 | 0 | 4,578 | 68 | 0 | 0 | 4,646 |
| 1996 | 0 | 0 | 68 | 139 | 4 | 0 | 211 |
| 1997 | 0 | 0 | 138 | 1,071 | 9 | 0 | 1,218 |
| 1998 | 0 | 0 | 5 | 453 | 53 | 0 | 511 |
| 1999 | 0 | 0 | 17 | 4237 | 48 | 0 | 4,302 |
| 2000 | 0 | 0 | 2 | 102 | 151 | 3 | 258 |
| 2001 | 0 | 0 | 3 | 98 | 939 | 11 | 1,051 |
| 2002 | 0 | 0 | 1 | 59 | 566 | 29 | 655 |
| 2003 | 0 | 0 | 0 | 4 | 217 | 64 | 285 |
| 2004 | 0 | 0 | 2 | 16 | 4068 | 102 | 4188 |
| 2005 | 0 | 0 | 0 | 2 | 93 | 175 | 270 |
| 2006 | 0 | 0 | 0 | 3 | 92 | 950 | 1045 |
| 2007 | 0 | 0 | 0 | 1 | 56 | 557 | 614 |
| 2008 | 0 | 0 | 0 | 0 | 4 | 275 | 279 |
| 2009 | 0 | 0 | 0 | 2 | 16 | 3757 | 3775 |
| 2010 | 0 | 0 | 0 | 0 | 4 | 133 | 137 |
| Total | 5318 | 6367 | 6367 | 6290 | 6320 | 6056 | 36718 |

*Notes:* The number of elections is lower in the first election round as the Ministry of Interior only started collating data in 1987. It is also somewhat declines somewhat over time, as municipalities that engage in mergers/splits in the post-treatment period are dropped only once territorial reforms have been enacted.

# B Additional descriptives

## B.1 Additional density tests

Figure B.1: Discontinuities in running variable at alternative compensation cutoffs (pre-reform)




(a) $p = 0.04$ (b) $p = 0.00001$

*Notes:* Tests are based on pooled data from pre-reform election rounds (Rounds 1 and 2), during which municipalities on either side of the 1,000-inhabitant cutoff retained a quasi-majoritarian electoral system, while those both left and right of the 10,000-inhabitant cutoff used PR. Solid lines represent split local polynomial density estimates of municipal population size under MSE-optimal local fits, with a triangular weighting kernel (Cattaneo et al., 2020). Bandwidths indicate robust bias-corrected 95% confidence intervals.

# C Regression tables

Table C.1: Electoral reform effects on middle-class, and labor market inactive representation in executive committees

| *Outcome* | (1) Upper-middle | (2) Lower-middle | (3) LM Inactive |
|---|---|---|---|
| Diff-in-disc: $RD_1$ - $RD_0$ | -0.024 | 0.038 | -0.011 |
| | (0.027) | (0.027) | (0.026) |
| $RD_0$: Pre-reform | 0.049** | -0.014 | -0.010 |
| | (0.019) | (0.019) | (0.022) |
| $RD_1$: Post-reform | 0.024 | 0.024 | -0.021 |
| | (0.019) | (0.019) | (0.014) |
| *Covariate-adjustments* | | | |
| Region FEs | ✓ | ✓ | ✓ |
| Election-Round FEs | ✓ | ✓ | ✓ |
| Lagged treatment ass. | ✓ | ✓ | ✓ |
| *Bandwidths & Ns* | | | |
| $RD_0$: $h_l \mid h_r$ | 1273 \| 2374 | 1639 \| 2890 | 968 \| 1711 |
| $RD_0$: $N_l \mid N_r$ | 933 \| 1878 | 1292 \| 2350 | 678 \| 2326 |
| $RD_1$: $h_l \mid h_r$ | 1017 \| 1741 | 928 \| 1613 | 1032 \| 1832 |
| $RD_1$: $N_l \mid N_r$ | 1585 \| 5463 | 1439 \| 5502 | 1613 \| 5858 |

*Notes:* Diff-in-disc estimates are calculated as differences between pre- and post-reform RD estimates. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust bias-corrected standard errors clustered at the municipality level are reported in parentheses. Estimates use local linear regressions with a uniform kernel and two-sided MSE-optimal bandwidths.

Table C.2: RD and Diff-Disc estimates from manipulation tests

| | Coefficient | SE | $p$ value | $h_l$ | $h_r$ | $N_h$ | Controls |
|---|---|---|---|---|---|---|---|
| **No. of parties in legislature** | | | | | | | |
| $RD_0$: Pre-reform | 1.96 | 0.14 | 0.00 | 4511 | 6044 | 560 | Yes |
| $RD_1$: Post-reform | -0.31 | 0.12 | 0.01 | 4612 | 5683 | 904 | Yes |
| Difference | -2.26 | 0.19 | 0.00 | | | | |
| **No. of parties in elections** | | | | | | | |
| $RD_0$: Pre-reform | 2.05 | 0.27 | 0.00 | 4522 | 5884 | 483 | Yes |
| $RD_1$: Post-reform | -0.25 | 0.11 | 0.03 | 4527 | 5809 | 1185 | Yes |
| Difference | -2.30 | 0.29 | 0.00 | | | | |
| **Seat share: Winning Party** | | | | | | | |
| $RD_0$: Pre-reform | -0.26 | 0.02 | 0.00 | 4605 | 5671 | 421 | Yes |
| $RD_1$: Post-reform | 0.00 | 0.00 | 0.59 | 4242 | 6212 | 1746 | Yes |
| Difference | 0.26 | 0.02 | 0.00 | | | | |
| **Seat share: Second Largest Party** | | | | | | | |
| $RD_0$: Pre-reform | 0.05 | 0.02 | 0.00 | 4568 | 5781 | 452 | Yes |
| $RD_1$: Post-reform | 0.02 | 0.01 | 0.06 | 4447 | 5945 | 1172 | Yes |
| Difference | -0.03 | 0.02 | 0.07 | | | | |
| **Vote share: Winning Party** | | | | | | | |
| $RD_0$: Pre-reform | 0.01 | 0.04 | 0.79 | 4700 | 5620 | 318 | Yes |
| $RD_1$: Post-reform | 0.01 | 0.02 | 0.44 | 4437 | 5942 | 1300 | Yes |
| Difference | 0.00 | 0.05 | 0.99 | | | | |
| **Vote share: Second Largest Party** | | | | | | | |
| $RD_0$: Pre-reform | -0.07 | 0.03 | 0.01 | 4743 | 5512 | 311 | Yes |
| $RD_1$: Post-reform | 0.02 | 0.01 | 0.05 | 4378 | 6105 | 1262 | Yes |
| Difference | 0.09 | 0.03 | 0.00 | | | | |
| **Electoral turnout** | | | | | | | |
| $RD_0$: Pre-reform | 0.03 | 0.04 | 0.48 | 4065 | 6307 | 1095 | Yes |
| $RD_1$: Post-reform | 0.00 | 0.01 | 0.88 | 4424 | 6054 | 1312 | Yes |
| Difference | -0.03 | 0.04 | 0.51 | | | | |

*Note:* All bandwidths are two-sided MSE optimal, as computed via rdrobust (Calonico et al., 2014) in R. Estimates adjusted for NUTS-2 region, educational attainment, unemployment, and a lagged treatment indicator. Standard errors are robust bias-corrected, and clustered at the municipality level.

Table C.3: RD and Diff-Disc estimates on mayoral party ideology

| | Coefficient | SE | $p$ value | $h_l$ | $h_r$ | $N_h$ | Controls |
|---|---|---|---|---|---|---|---|
| **Left-of-center parties** | | | | | | | |
| $RD_0$: Pre-reform | 0.04 | 0.12 | 0.72 | 4541.00 | 5896.00 | 588 | Yes |
| $RD_1$: Post-reform | 0.06 | 0.09 | 0.55 | 4427.00 | 6087.00 | 658 | Yes |
| Difference | 0.01 | 0.15 | 0.93 | | | | |
| **Right-of-center parties** | | | | | | | |
| $RD_0$: Pre-reform | -0.02 | 0.14 | 0.87 | 4644.00 | 5717.00 | 492 | Yes |
| $RD_1$: Post-reform | -0.07 | 0.07 | 0.36 | 4559.00 | 5717.00 | 589 | Yes |
| Difference | -0.04 | 0.16 | 0.79 | | | | |
| **Civic Lists** | | | | | | | |
| $RD_0$: Pre-reform | -0.04 | 0.08 | 0.62 | 4618.00 | 5798.00 | 308 | Yes |
| $RD_1$: Post-reform | 0.07 | 0.09 | 0.40 | 4535.00 | 5762.00 | 658 | Yes |
| Difference | 0.11 | 0.12 | 0.34 | | | | |

*Note:* All bandwidths are two-sided MSE optimal, as computed via rdrobust (Calonico et al., 2014) in R. Estimates adjusted for NUTS-2 region, educational attainment, unemployment, and a lagged treatment indicator. Standard errors are robust bias-corrected, and clustered at the municipality level.

Table C.4: Effects of Electoral System on Working Class Representation in Councils, by Party System Competitiveness

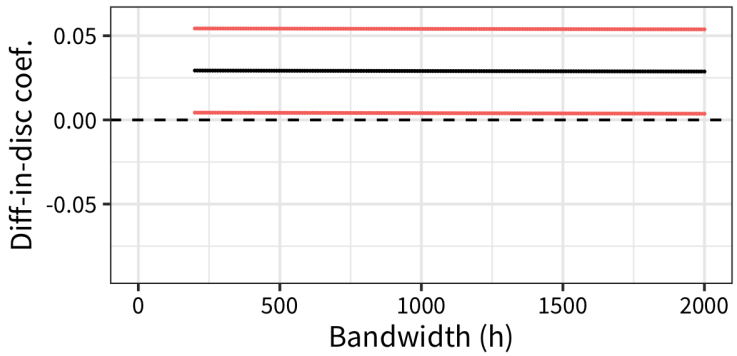
| | (1)<br>Quasi-Maj vs. Competitive PR | (2)<br>Quasi-Maj vs. Stronghold PR |
|---|---|---|
| **RD estimate** | -0.060** | 0.008 |
| | (0.026) | (0.034) |
| *Covariate adjustments* | | |
| Region FEs | ✓ | ✓ |
| Election-Round FEs | ✓ | ✓ |
| Lagged treatment ass. | ✓ | ✓ |
| Unemployment rate | ✓ | ✓ |
| Edu. attainment | ✓ | ✓ |
| $N$ | 443 | 452 |
| $N_l \mid N_r$ | 333 \| 110 | 375 \| 77 |
| $h_l \mid h_r$ | 4445 \| 6004 | 4414 \| 6030 |

*Notes:* * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust bias-corrected standard errors clustered at the municipality level are reported in parentheses. Estimates use local linear regressions with a uniform kernel and two-sided MSE-optimal bandwidths, as per (Calonico et al., 2015).

# D Robustness

## D.1 Alternative bandwidths

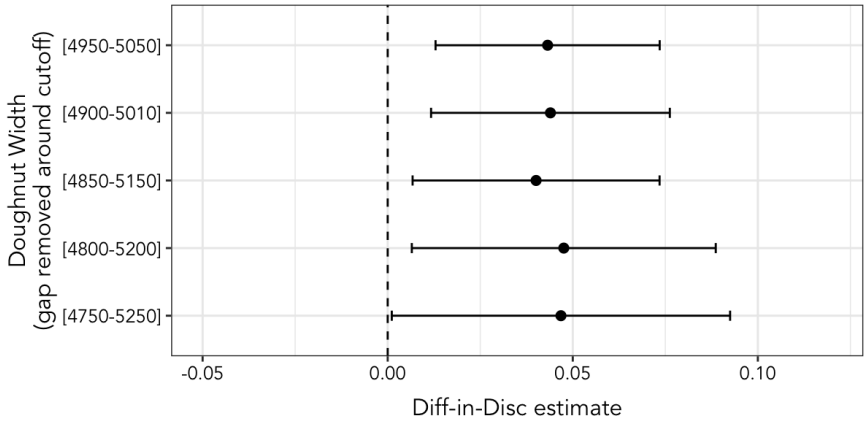
Figure D.1: Two hundred bandwidths on working class representation in municipal councils




*Note:* Black dots represent diff-in-disc estimates obtained from local linear models with uniform kernel weights. Red dots indicate 90% robust bias-corrected confidence intervals, derived from municipality clustered standard errors ($0.059 \leq p \leq 0.054$). All estimations adjust for region fixed effects, election round fixed effects, educational attainment, unemployment and a lagged treatment assignment indicator.

## D.2 Doughnut specifications

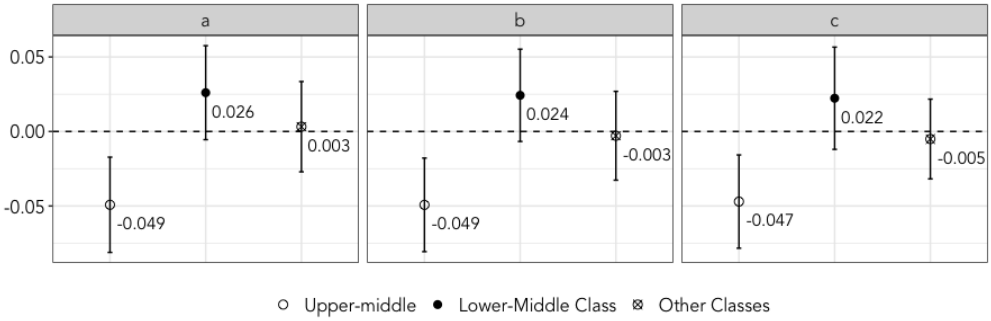
Figure D.2: Results of doughnut specifications




*Note:* Bars indicate 95% robust bias-adjusted confidence intervals, derived from municipality-clustered standard errors. All estimates control for lagged treatment variable, and adjust for region and election round fixed effects.

## D.3 Additional tests on middle-class representation and the LM inactives

Figure D.3: Diff-in-disc estimates for other social classes, per model specification




*Note:* Bars indicate 95% robust bias-adjusted confidence intervals, based on standard errors clustered at the municipality level. The baseline model (Model A) controls for the lagged treatment assignment indicator and includes region and election-round fixed effects. Model B adds controls for unemployment and educational attainment. Model C additionally includes controls for the log age ratio, number of vacation homes, and off-cycle elections.