UNEQUAL DEMOCRACIES

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Electoral Formulas, Party Magnitude and Class Representation in List Systems

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ABSTRACT:

Proportionality in electoral systems is often seen as the best means to produce statistically representative government. In this paper, I argue that majority bonuses—whereby the party that obtains a plurality of votes automatically obtains an absolute majority in the legislature—can bolster the number of statistically atypical working-class candidates that manage to obtain office in PR systems. The difference is mechanically driven, as workers are systematically granted lower placements on party lists. Majority bonuses reduce the number of parties in the legislature, by increasing the number of seats allocated to winning parties. Formula disproportionality thus allows a larger number of workers with low list placements to enter politics. As a test of theory, I present evidence from a natural experiment on a municipality-level change in electoral formulas, which took place in Italy in 1993. Leveraging a difference-in-discontinuities design, I find that the bonus improves working class representation in municipal councils. Effects are driven by a positive change in the proportion of workers that enter councils via winning party lists.

ACKNOWLEDGEMENTS:

1 Introduction

A large body of work on electoral systems has found that legislatures grow more statistically balanced under proportional representation (PR).[2] The main theory underscoring this finding is fairly straight-forward. When there is a larger number of seats to compete for, parties have strong incentives to include a more diverse set of candidates on their lists, in order to capture votes from a wider part of the electorate (Engstrom, 1987; Rule, 1987; Norris, 2006). Critical in this work is the theorized mechanism. The effects of PR on statistical representation are most often predicted to stem from differences in district magnitude, i.e. the number of seats assigned to a given polity. For where district magnitude is higher, singular parties can compete in elections with longer candidate lists.

Going further, more recent work on political selection has probed the structure of party lists in PR systems. The findings of these accounts uncover the prevalence of hierarchical class-sorting within lists themselves. In other words: while parties under PR may well be able to advance a more socio-economically diverse set of candidates for electoral competition, the expansion of party lists typically translates into a scenario in which statistically atypical candidates—such individuals from lower social classes—are systematically granted lower list placements (Lucardi, 2019; Buisseret et al., 2022). The class sorting tendency is likely exacerbated by the fact that the lower social classes have traditionally been absent in the political arena, and political experience tends to be the strongest determinant of high list placement (Cirone et al., 2021; Meserve et al., 2020; Put et al., 2021).

Hierarchical class sorting implies that changes to district density may improve representational outcomes not solely by means of altering party incentive structures, but also from purely mechanical effects that operate via a change in party magnitude (Lucardi, 2019; Lucardi and Micozzi, 2022). When parties obtain additional seats in a given legislature, they can place more candidates with low list rankings in office. If list placements are sorted along class lines, this automatically results in legislatures that are more socio-economically diverse.

Evaluating this proposition in an empirically robust manner, however, is notoriously difficult. Changes in party magnitude are typically the result of an expansion and/or compression to district magnitude, or legislature size. This makes it hard to isolate the effects of party magnitude from those brought about by other electoral rules. To overcome this inference problem, I draw on a

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[1]Magnier (2004, pg. 166)

[2]Here, one subset of work focuses on the representation of women (e.g. Kittilson and Schwindt-Bayer, 2012; Krook, 2018; Matland and Studlar, 1996; Matland, 1998; Norris, 1985; Profeta and Woodhouse, 2021; Rule, 1987); another focuses on ethnic minority representation (Barker and Coffé, 2018; Hughes, 2016; Le Lohé, 2004; Moser, 2008), and a third subset looks at the representation of young voters (Joshi, 2013; Stockemer and Sundström, 2018).

natural experiment to assess how differences in party magnitude can operate independently of a change in legislature size. For this I make use of a 1993 reform of the electoral formula utilized for Italian local elections. The reform expanded the use of a so-called 'majority bonus' to all Italian municipal elections. At its core, a majority bonus is a quasi-majoritarian formula. It grants the party list that obtains a plurality of votes in an election, an automatic super-majority of seats in the legislature (66%). Given this, it automatically inflates the party magnitude of winning parties, at the cost of opposition party seat shares.[3]

Importantly, the Italian 1993 reform affected only municipalities with a population size of +5000 inhabitants, as localities with smaller resident numbers already made use of the bonus at time of the rule change. This variation in electoral rules enables me to leverage a difference-in-discontinuities design, in which I compare working class representation above and below the population size cut-off, before and after the 1993 formula change.

In line with theoretical predictions, I find that the installment of majority bonuses led to an increase in working class representation in municipal councils. This effect is driven by a higher number of working class candidates entering from winning party lists, which also retain a larger proportion of seats in legislatures as a result of the reform. Additional analyses provide further evidence in support of the theory. I fail to uncover that the policy induces any significant change in the social class background of mayors: individuals that stand for elections as list toppers (or 'capolistas') of their respective parties. Taken in sum, findings thus imply that majority bonuses— a highly disproportional electoral formula adopted in political systems characterized by legislative stagnation—may constitute a form of 'electoral sweet spot': an efficiency-enhancing rule that also improves class equity in political representation.

2 Class sorting and list structures under PR

Elections can only produce statistically representative government when political candidate pools are characterized by a high degree of socio-economic diversity. Focusing on this question, work in political selection consistently shows that candidacy is much more frequent among individuals with higher levels of education (e.g. Bovens and Wille, 2017; Lamprinakou et al., 2017; Lindgren et al., 2019; Carnes and Lupu, 2016b), who pursue non-manual labor professions (e.g. Carnes, 2013; Kirkland, 2021; Poertner, 2022). Setting aside differences in class-specific propensities to run for election, the transmission of candidacy to actual office-holding is noticeably more complex in PR list systems. For in these settings, the likelihood of obtaining office is heavily co-determined by an individual candidate's list placement. List topping candidates, also referred to as *capolistas*, are often guaranteed a seat in legislatures, while it is virtually impossible for low-ranked individuals to

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[3]Majority-assuring PR systems such as this are particularly widespread in sub-national elections—e.g. in France, Italy and Andorra—but they even determine the composition of some national parliaments, such as in Armenia, Greece and San Marino (Bedock and Sauger, 2014). In the past, they have also been made use of for parliamentary elections in Chad, Mexico, South Korea and Italy (D'Alimonte, 2015; Shugart and Wattenberg, 2001).

enter office (Cirone et al., 2021; Cox et al., 2021). As a result, party vote shares most intensely affect the electoral fortunes of candidates that are placed in mid-list positions (Buisseret et al., 2022). Bearing this in mind, it becomes clear that descriptive representation under PR requires two contemporaneous conditions to hold. First, party lists must include candidates from a wider set of the overall population. But aside from this, candidates with diverging socio-economic characteristics must also be evenly distributed across the full length of individual party lists (Lucardi and Micozzi, 2022). Extant evidence, however, suggests that the latter condition is rarely met.

There are not many studies that conduct extensive empirical tests on candidate list placement, as systematic list-level data is scarce in most corners of the world. Arguably, the most comprehensive study undertaken is that of Buisseret et al. (2022), who evaluate the extent to which socio-economic characteristics correlate with list placement in Swedish municipal elections. The focus of this study is different in nature from this paper, as it seeks to evaluate the extent to which intra-party competition can foster legislator competence. But their findings from a country characterized by prolonged social democratic hegemony and high levels of worker mobilization, are highly informative. For even in this least likely setting, the authors uncover that candidates with lower levels of education are systematically granted lower list placements. Moreover, they highlight that the tendency is particularly strong among parties that have a strong likelihood of being able to control the executive. In Sweden, during the period that they examine (1991–2014), this is most frequently the Social Democratic Workers' Party.[4] The implication of this finding is thus that left-wing parties are as likely as their right-wing counterparts to place individuals with less formalized schooling on low list positions—if not more so.

The results of Buisseret et al. (2022), as well as studies from other Scandinavian countries (e.g. Cox et al., 2021), thus suggest that there aspects of the intra-party competition under PR that systematically bring about hierarchically class-ordered party lists. Extant work suggests that there are a number of mechanisms at the stage of list formation, which may cause this phenomenon.

List placement is a function of within-party competition that occurs when party selectorates, i.e. intra-party bodies tasked with list formulation, rank political candidates according to their perceived likelihood of winning elections. In this process, a number of studies have shown that statistically atypical candidates fare poorly (Bloom and Thames, 2021; Chiru and Popescu, 2017). This comes about for a number of reasons. On the one hand, the ingrained biases of party selectorates may well bring about lists where atypical candidates are given unattractive list positions (Dancygier et al., 2015; Geese and Schacht, 2019; Lindgren et al., 2022). In these cases, party selectorates may leverage e.g. educational background or professional status as a heuristic device to gauge legislator competence.[5] In a similar fashion, homofily may well be a core driver of hierarchi-

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[4]More specifically, 57% of municipal councils throughout this period were controlled by the Social Democrats (Sveriges Kommuner och Länsting, 2018). Given Swedish data restrictions, replication to evaluate this proposition more carefully is not possible for researchers based outside of the Nordic countries.

[5]This heuristic may or may not be accurate. The study of legislator competence admittedly falls beyond the scope of this paper.

cal class-ordering. Party selectorates are usually current or former politicians themselves, and are thus disproportionately upper or upper-middle class. Extant work tells us selectorate bodies tend to favor political candidates that display traits that they themselves are in possession of (Cheng and Tavits, 2011; Rehmert, 2020). This bias can therefore also exacerbate class skews in the structure of party lists.

To the extent that hierarchical class ordering is a universal phenomenon, changes to electoral law can alter the statistical representation of political minority groups in democratically elected legislatures. This comes about as electoral rules affect the ability of individual parties to place candidates with low list rankings in office. In this paper, I focus on the electoral rule that has the most direct impact on party seat allocation within legislatures: the electoral formula.

3   Electoral formulas and statistical representation

An electoral formula can be defined as a mathematical function that dictates how the votes of an electorate get converted into party seats within its legislature (Benoit, 2000). Formula choice has strong implications for representational outcomes, as it—together with e.g. legislature size and the prevalence of electoral thresholds—effectively alters the proportionality of the electoral system.

While majoritarian multi-member systems do exist (See e.g. Eggers, 2015), electoral formula choice is most relevant for PR settings, where it has very direct consequences for both the number of parties that manage to gain entry to the legislature, as well as the number of seats that any given party can hope to attain within it. Most directly, formulas have a 'mechanic' effect on representational outcomes (Duverger, 1959) This occurs as disproportional formulas raise the share of seats within a legislature that is allocated to winning parties, and decrease the number of parties that can effectively enter the legislature itself (Becher et al., 2022; Benoit, 2001; Eggers, 2015). If parties systematically grant working class candidates low list placements, disproportional formulas should thus serve to improve statistical working class representation. To illustrate, I present a stylized example:

Consider two parties—Party Liberal and Party SocDem—competing for control of a legislature that consists of ten seats. The class make-up of their respective party lists are displayed below in figure 1. Party SocDem puts forth a higher number of working class candidates for election, but both parties systematically rank workers low on their respective lists. Now assume that an election is held and Party Liberal receives 53 percent of the vote share, while Party SocDem obtains 47 percent. Under a full proportionality rule, Party Liberal obtains six seats, and the rest goes to Soc-Dem. As a result, no worker gets elected into office. Under a majority-bonus system, things look different. Party Liberal now automatically gets seven seats, while Party SocDem receives three: as as a result one worker now gets elected into office. Importantly, this change will occur without any changes to the class composition of party lists—and in spite of the more class-inclusive party having effectively lost one council post.

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              Party Liberal                              Party SocDem

    ┌─────────────────────────────────┐      ┌─────────────────────────────────┐
    │  1.  Middle Class Candidate     │      │  1.  Middle Class Candidate     │
    │  2.  Middle Class Candidate     │      │  2.  Middle Class Candidate     │
    │  3.  Middle Class Candidate     │      │  3.  Middle Class Candidate     │
    │  4.  Middle Class Candidate     │      │  4.  Middle Class Candidate     │
    │  5.  Middle Class Candidate     │      │  5.  Working Class Candidate    │
    │  6.  Middle Class Candidate     │      │  6.  Middle Class Candidate     │
    │  7.  Working Class Candidate    │      │  7.  Working Class Candidate    │
    │  8.  Middle Class Candidate     │      │  8.  Working Class Candidate    │
    │  9.  Middle Class Candidate     │      │  9.  Working Class Candidate    │
    │ 10.  Working Class Candidate    │      │ 10.  Working Class Candidate    │
    └─────────────────────────────────┘      └─────────────────────────────────┘
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Figure 1: A Stylized Example of Party Lists

There are a number of conditions that need be upheld in order for these mechanic effects to occur. As highlighted, class bias in list composition must be systematic: i.e. be detectable in party lists across a wide ideological spectrum. In light of extant evidence from least-likely cases, this assumption is relatively weak in nature. Aside from this, however, bias need take the form of lower list placement—but not in the full exclusion—of working-class candidates. In other words, parties that have lower incentives to capture e.g. working class votes can not fully abstain from including workers on their lists. If this is the case, a rule change will, on average, yield null or negative effects on working class representation. From a methodological standpoint, however, a wide-spread prevalence of working-class exclusion from party lists would serve mainly to reduce the likelihood of uncovering the formula effects that I predict.

4 Research design

To test my theory, I run a series of models based on a difference-in-discontinuities design, also known as a DDD. In this, I exploit a 1993 reform introduced in Italy, in which a subset of municipalities shifted away from a classic PR formula, by adopting a majority bonus.

Italian municipal elections serve to determine office-holding in two main political bodies: the mayorship (the executive), and the municipal council (the legislature). Prior to 1993, mayoral and council elections were nearly always independent. The allocation of municipal council posts was then determined by means of the D'Hondt formula: a full proportionality rule. There was, however, an important exception to this. Council elections in municipalities with fewer than 5000 inhabitants were directly linked to mayoral elections and quasi-majoritarian in nature. Under this system— proportionality with a majority bonus—the party list associated with the winning mayoral candidate automatically received two thirds of all council seats. The rest were then allocated proportionally across remaining parties, resulting in a PR formula that was heavily disproportional in nature.[6]

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[6]Party lists were closed, but voters were permitted to express a preference vote in council elections. The preference vote could only be expressed on the party list of their preferred mayoral candidate.

In 1993, significant changes were made to Italian electoral law. As a results of these, the majority bonus system was rolled out to all of Italy's roughly 8000 municipalities, regardless of their population size (Donovan, 1995). The change thus affected most Italian municipalities, but it did not alter rules for municipalities with fewer than 5000 inhabitants, where the plurality rule was in force already prior to reform enactment. As such, the statistical identification strategy I employ is based on comparing differences in outcomes across municipalities with above or below a population size of 5000 inhabitants, as well as how these differences change before and after reform enactment. The design thus exploits both temporal variation in the observation of treatment—as is typical of difference-in-difference designs—as well as cross-sectional variation close to an arbitrary cutoff point, which serves as the premise of an RDD set-up.

Diff-and-disc designs such as this have been used in a number of other papers on Italian municipal politics.[7] The strength of the design is that it enables researchers to relax a core assumption that must hold when deriving LATE estimates under a conventional RDD. Namely, that only treatment itself differs across municipalities at the cut-off point (Eggers et al., 2018). In real life, this condition is almost never met as multiple policies change simultaneously at specific threshold values of municipal population size. In the case I analyze, the other main policy that shifts at the 5000 inhabitant cut-off is the wage level of the executive (i.e. the mayor), which becomes noticeably higher (Gamalerio and Trombetta, 2022; Grembi et al., 2016).[8] As elaborated upon by Grembi et al. (2016), the diff-in-disc enables me to overcome this fundamental inference problem, under a number of testable assumptions.

First, to ensure that municipalities did not self-select into treatment assignment, there should be no detectable sorting of the running variable around the treatment cut-off point. To validate this assumption, I run density tests on municipal population size around the treatment assignment threshold of 5000 inhabitants, as proposed by Cattaneo et al. (2020). The results of density tests are presented in figure 2, and suggest that the non-sorting assumption is upheld.

A second assumption is that alternative potential outcomes and municipality-variant traits are balanced around the cut-off during the entirety of the period examined. Probing this, I run a series of RDD tests on municipality-level covariates, where my dependent variable is the running variable, i.e. municipality population size. Results are presented in the appendix (see section A.4). These show that covariate balance is retained across municipalities assigned to treatment and control both before and after the majority bonus was adopted. Finally, just as is the case in conventional difference-in-differences tests, municipalities just below/above the cutoff point must display parallel trends in outcomes post-reform: i.e. after electoral formulas are identical across all municipalities. As presented in the results section, this proposition also holds.

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[7]See e.g. Andreoli et al. (2021); Bordignon et al. (2016); Gamalerio and Trombetta (2022); Grembi et al. (2016); Profeta and Woodhouse (2021).

[8]This discontinuity in mayoral wages dates back to the late 1960s (Gamalerio and Trombetta, 2022).

Figure 2: Pre-reform discontinuity tests on the density of population size around the cut-off

MSE-optimal local polynomial fit   IMSE-optimal local polynomial fit
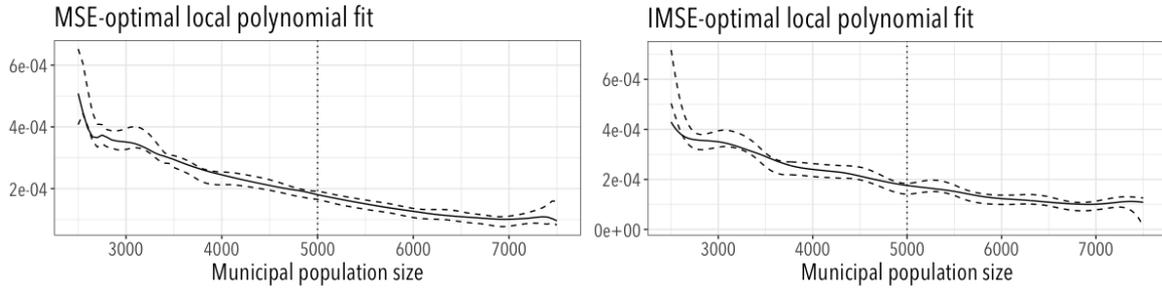
Notes: Data from the Italian national census of 1991. Solid lines represent split fourth-order polynomials of municipal population size under MSE- and IMSE-optimal local fits. Dashed lines indicate 95% confidence intervals. Vertical dotted line depicts treatment assignment cut-off of 5000 inhabitants. Results are not subject to variation over time (see appendix figure A.4).

To ensure that my analysis is conducted on localities that are comparable in nature, I restrict my sample based on municipality population size. More specifically, I examine only municipalities that have above 3000 and below 7000 inhabitants, as the mayoral wage rate further decreases at the 3000 inhabitant cut-off point. This selection band also ensures that legislature size remains constant across municipalities assigned to treatment vs. control, during the entirety of the time span examined.[9] Aside from this, I exclude municipalities that engaged in mergers post-1993, as well as those that have been run by state-mandated commissioners. In doing this, I obtain a sample of 2388 unique municipalities located across all of Italy's 107 provinces. To ensure robustness, however, I also perform additional checks on a wider selection of bandwidth specifications with smaller sample sizes.

To code my main variables of interest, I make use of annually compiled archival data from the Italian Ministry of Internal Affairs. This data contains information on the educational attainment and occupations of all municipal legislators that have been elected into office, and it has been collated since 1988. In order to minimize problems of data missingness, I focus on the period of 1988-2011: i.e. round of elections that occur prior to the 1993 reform, as well as three post-reform election rounds.

My outcomes of interest concern the class composition of municipal councils. To capture this, I first classify individual legislators into four social classes, based on text-based occupational descriptions provided by the ministry. Using occupation as an indicator of social class is commonplace in large-N social scientific research—and it is so for good reason. An individual's occupation serves as a fairly accurate proxy of their workplace characteristics, as well as the income and unemployment risks that they face. These factors have been consistently shown to underscore class-based divides in public opinion and political behavior[10]

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[9]The 1993 reform also compressed legislature sizes for all municipal councils in Italy, but treated and control municipalities were identically affected by this reform.

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

The coding scheme I use builds on the theoretical work of labor market sociologist Oesch (2006) and is identical to that used by Ray (2022). Table 1 provides examples of how raw occupations were coded along the four-category class scheme—as well as the main deviations between this coding and that of Oesch (2006). Like Ray (2022), I refer to class groupings as upper-middle, lower-middle and working class. Aside from these groups, I also code a residual category of persons that are inactive in the labor market. This latter group consists mainly of pensioners (61%) and students (23%).[11]

Table 1: Social class coding based on occupational belonging

| Occupations *(examples from raw data)*	Oesch (2006) coding	Four cat. coding
Company directors, Senior civil servants, Lawyers, Engineers, Financial analysts, University professors, Journalists	Higher grade service class	Upper middle class
Hospitality managers, Police inspectors, Technicians, Sales agents, Designers, Librarians and curators	Lower grade service class	Lower middle class
Bank-tellers, Travel consultants, Legal secretaries, Customer service reps, Fire fighters, Craftsmen	Skilled workers	Working class
Drivers, Machine operators, Assemblers, Farm workers, Waiters, Hairdressers, Cleaners	Unskilled workers	Working class
Homemakers, students, retirees, unemployed	—	Inactive in labor market

Source: Ray (2022), pg. 12.

Overall, 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 variables that capture the shares of a municipality's council posts, that were obtained by individuals from each of these social classes. Figure 3 plots the distributions of these variables, per election round. Data on persons that were inactive in labor markets is accounted for when generating my outcome variables, but I only examine their council shares as a separate outcomes in robustness checks of this paper, which are included in this paper's appendix (see A.1).

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 mid-skill jobs have disappeared from Italy in the past thirty years—and, as a result, larger shares of

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[11]Due to problems of missing data on pre-retirement occupations, it is not possible to classify the social class of pensioners.

the working-age population now have either high- or low-skill jobs (Basso, 2020; Goos et al., 2009). Also detectable is a decline in working class representation. In the pre-reform election round—when elections took place mainly in 1990—working class legislators made up roughly 17% of the average municipal council included in the sample. By the first election round, this number was down to 12%. 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). 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 the working class to have constituted between 40-60% of the Italian labor market force.

Figure 3: Average class-shares of Italian municipal councils in sample
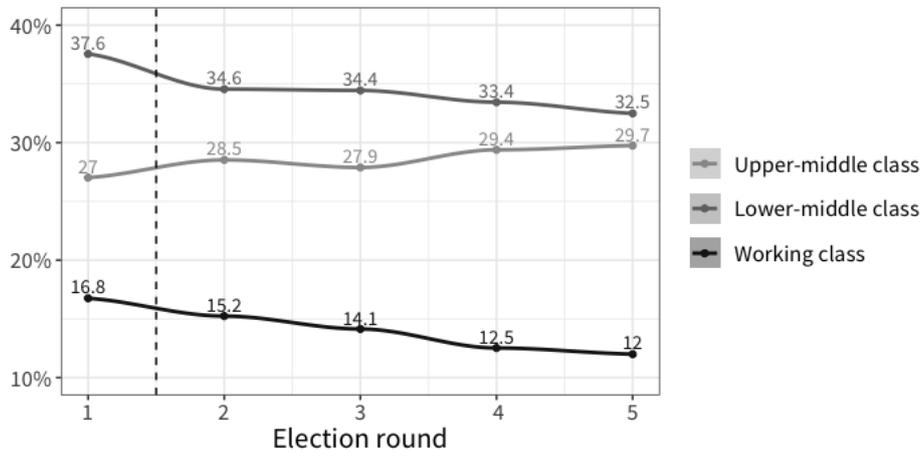
Note: As elections take place in different calendar years and the annual number of elections varies noticeably, averages are plotted by election rounds included in the sample. Dashed vertical line indicates point in time in electoral formula was changed.

4.1 Model choices

To test my theory, I run a series of OLS regressions. The baseline model specification can be formalized as:

$$R_{ijt} = \beta_1(ReformMunicipality_k) + \beta_2(ElectionRound_j) + \\ \beta_3(ReformMunicipality_k * ElectionRound_j) + \beta_4 X_{it} + \epsilon_{it}$$

(1)

where R_{ijt} is a continuous variable indicating the fraction of a given social class i prevalent in a municipal council j, during an election year t. $ReformMunicipality_k$ is a dichotomous treatment assignment variable, which denotes if a municipality k had a population size above or below 5000 inhabitants, where the latter is coded into a control group. In these municipalities, the

allocation of council positions was subject to a majority bonus rule throughout the entirety of the period examined.

Because of historical break-downs of municipal government, Italian municipalities hold their elections on different calendar years. Given this, I code an alternative variable $ElectionRound_j$ to account for temporal variation. This is a categorical variable indicating the election round that a given council was elected into office. It is centered around the set of elections that took place directly after the 1993 reform.

$ReformMunicipality_k * ElectionRound_j$ is an interaction term, where the coefficient β_3 captures my main estimate of interest: namely, the effect of adopting the quasi-majoritarian electoral rule on differences in class representation between municipalities that who adopted the rule vs. those who always had the rule in place.

X_{it} is a vector of municipal-level control variables, indexed to time t. These capture the socio-economic composition of the individual municipality, which may affect the available supply of potential legislators with working class occupations. Controls included are a municipality's population size, its population size squared, its unemployment rate and an indicator of municipality-level educational attainment. The latter variable is operationalized as the proportion of a municipality's population aged 15 or above, that is in possession a tertiary schooling degree. All controls are generated on the basis of historical Italian census data, which is compiled every ten years. As such, time is indexed based on the census closest in time to a given council election.[12] Finally, ϵ_{it} is an error term. Standard errors are clustered at the level of treatment assignment: i.e. the municipality.

5 Results

5.1 Main findings

How did electoral system change affect working class representation in municipal politics? Elucidating on this question, figure 4 displays estimates from the main analysis. Sub-figure 4a depicts over-time variation in outcomes by treatment assignment. Sub-figure 4b elaborates on the statistical significance of these estimates, by plotting the marginal treatment effects across the two subgroups of municipalities.

Consistent with my proposed theory, subplot 4a shows that treated municipalities—which were affected by the 1993 reform change—experienced a noticeable increase in working class representation after electoral formulas were changed. In contrast, the number of working class legislators in control group municipalities were higher than that of treated municipalities prior to reform—and these numbers remained unaffected by the reform itself. These predicted probabilities also highlight that parallel trends assumptions are upheld for my analysis. Post-reform, the working class share of councils assigned to both treatment and control develop fully in in tandem.

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[12]For this I follow the matching procedure of the Italian Ministry of Interior.

The overall shift in patterns of working class representation yields a highly statistically marginal treatment effect at the 99% confidence interval, more clearly visualized by sub-plot 4b. Effects on treated municipalities are remarkably stable in post-treatment election rounds—and at a magnitude of 3.5 percentage points, they are notable in size. When compared against the pre-treatment baseline, where an average of less than 17 percent of these councils consisted of workers, it represents roughly a 21 percent increase in working class representation.

Figure 4: Treatment effects on working class representation in municipal councils

(a) Over-time trends

(b) Marginal effects
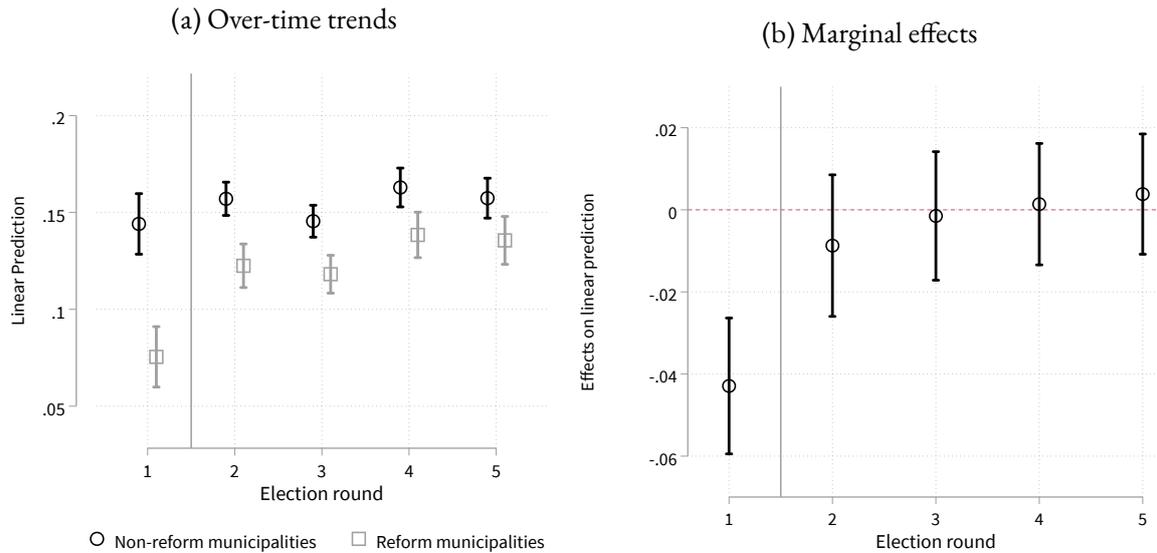
Note: Bandwidths indicate 99% confidence intervals; solid vertical line indicates when electoral formula was changed. Model specification includes municipal-level controls. Standard errors are clustered on a municipality level.

To further probe robustness, figure 5 plots the distribution of this outcome variable by the running variable, for the first four election rounds in the sample. From it, we detect that outcomes in the first election round—i.e. prior to the homogenization of electoral formula rules—grow discontinuous at the cut-off point. This does not hold in successive election rounds, and by election round four the trend looks largely linear at the cutoff.

Given the noticeable discontinuity at the RDD cutoff point in the pre-reform election round, obtained estimates are robust to a range of bandwidth specifications around the treatment assignment threshold of 5000 inhabitants. Figure 6 presents marginal treatment effects when comparing pre-reform outcomes to those detectable in the first post-reform election round. Results highlight that obtained treatment effects are statistically identical and significant, in estimations run on eight separate bandwidth-specifications.

Figure 5: RDD plots by election round
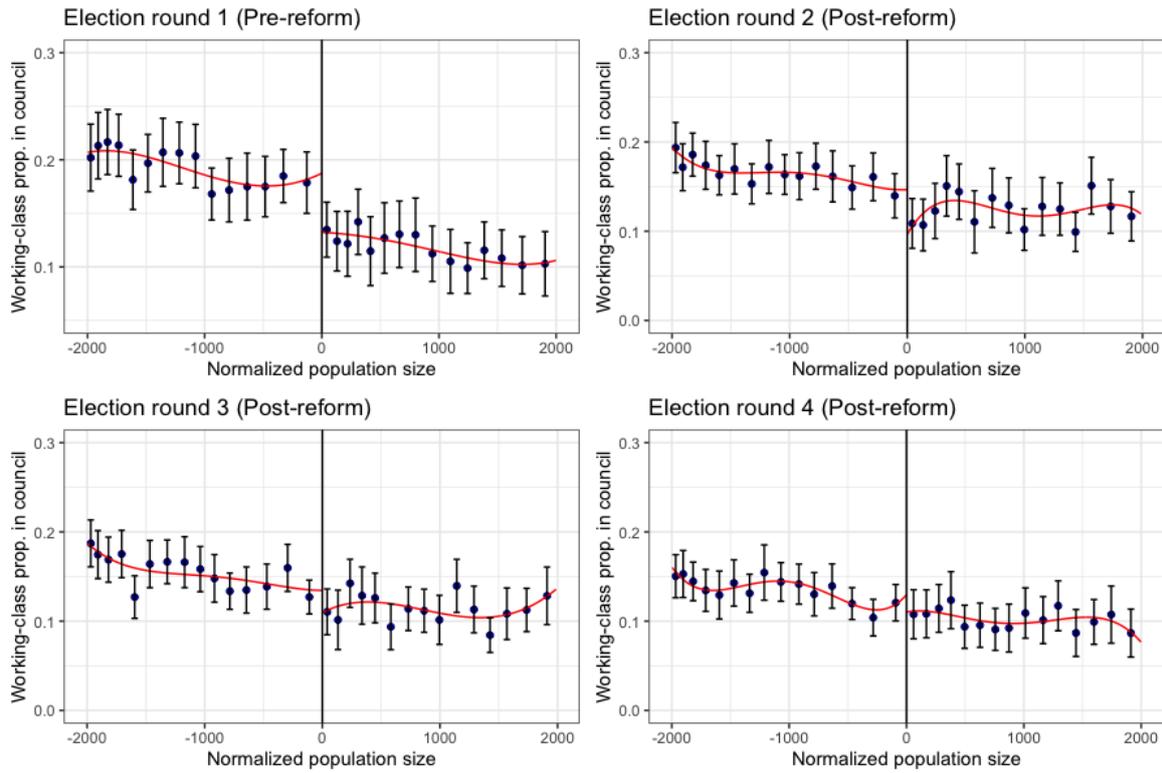
Note: Data binned into quantiles. Horizontal axis depicts actual population size minus 5000 (cut-off point); Local linear regression overlaid is a spline 4th-order polynomial fit. Bandwidths indicate 95% confidence intervals. For alternative estimates with evenly spaced bins, see appendix figure A.7.

Figure 6: Diff-in-disc estimates across bandwidth specifications
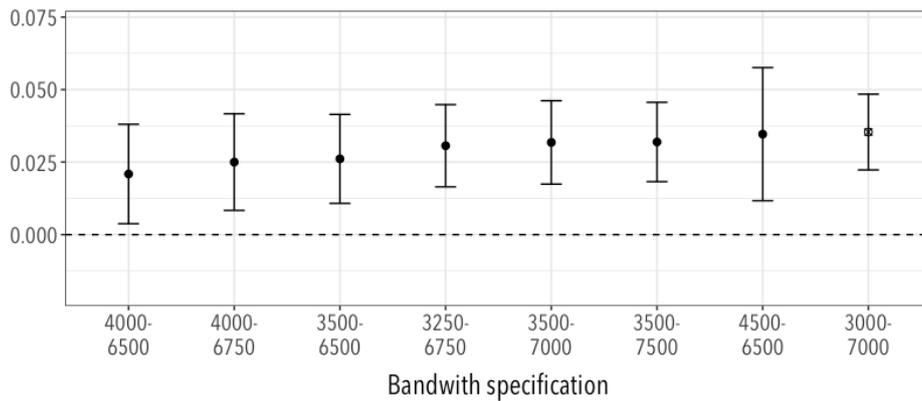
Note: Dots display marginal treatment effects obtained from estimations run on two-period specifications, where data is delimited to examine only in immediate pre- and post-reform election rounds. Bands indicate 95% confidence intervals.

If workers gain as a result of majority bonuses, who lose out? Figure 7 displays how the representation of upper- and lower-middle classes fared in light of the electoral rule change. Estimates show that the proportion of upper-middle class legislators was largely unaffected by the reform. Instead, the number of lower-middle class council-members decreased the most upon the adoption of a quasi-majoritarian formula. This decline, however, only becomes borderline statistically significant in the third post-reform election round ($p < 0.1$).[13] As such, it appears that direct gains to working class representation did not disproportionately affect the political opportunity of either of the middle-classes in a negative fashion. This suggests that list rankings for both upper and lower middle class candidates were more symmetrically distributed on party lists, prior to the 1993 reform. Over-time, working class gains seem to have negatively affected mainly the political opportunity of the lower-middle class.

Figure 7: Marginal treatment effects on middle class representation in municipal councils

(a) Upper-middle class   (b) Lower-middle class
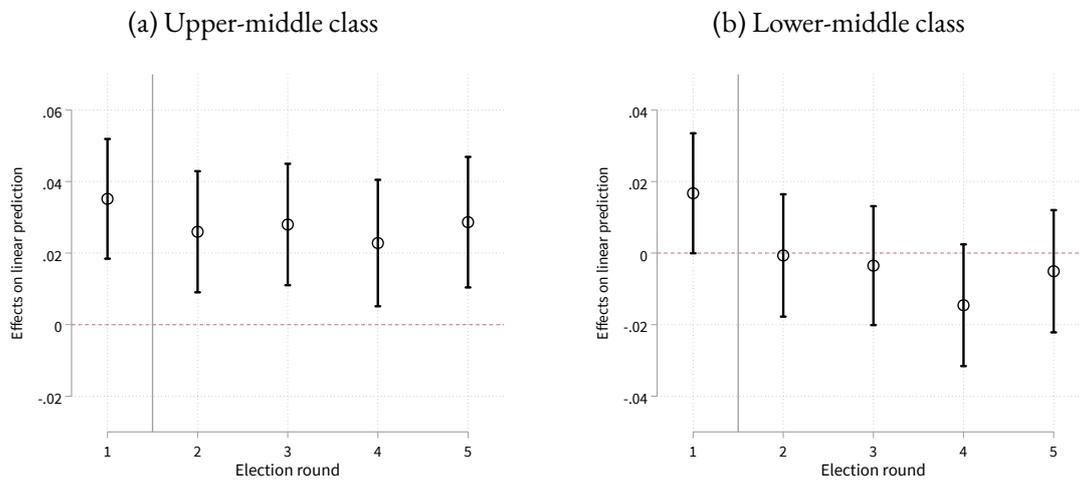
Note: Y-axis scales vary across plots, to better display effect sizes and confidence intervals. Bandwidths indicate 95% confidence intervals. Model specifications include municipal-level controls; standard errors are clustered at the municipality level.

6   Mechanism tests

The best means to probe my proposed mechanism would naturally be to examine party list data. Unfortunately, Italian authorities have not systematically collected information on party lists for municipal council elections. I therefore run a series of alternative tests designed to closer probe my theoretical mechanism. First, I perform robustness tests to ensure that the formula change positively affected the party magnitude of election-winning parties, and that it reduced the number of political parties in office. Aside from this, I also test how the reform changed within-party shares

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[13]Additional analyses also confirms that the reform did not affect council shares of persons of the residual group of legislators with low labor market attachment (see appendix A.1).

of working class legislators. In these tests, I distinguish between 'winning' mayoral parties and minor opposition parties. Finally, I conduct tests designed to rule out an alternative potential mechanism: a shift in party incentives to cater to broad-based (working-class) voter constituencies.

6.1 Effects on party magnitude

I theorize that a disproportional electoral formula can improve working class representation mainly via mechanical factors, as it automatically inflates the party magnitude of winning parties. To probe this proposition more carefully, I replicate my analysis on a series of four alternative outcomes. First, I make sure that the installation of majority bonuses did indeed a) instill an increase winning party council seats, and that it b) reduced the number of parties in legislatures. Second, I examine whether the formula alters within-party shares of working class legislators—both in winning, as well as in opposition party lists. Importantly, parallel trends assumptions are met for each of the four outcomes and presented in a more extensive fashion in appendix A.2.

Figure 8 displays the results of this exercise. It shows that winning parties indeed obtain higher seat shares after reform enactment, and that party numbers decrease noticeably in tandem ($p < 0.01$ for both outcomes). In the elections that follow the rule change, winning parties obtain, on average, seven percentage points more of the legislature—and councils are composed of 0.7 fewer parties.

Figure 8: Marginal treatment effects on winning party magnitudes and council party numbers
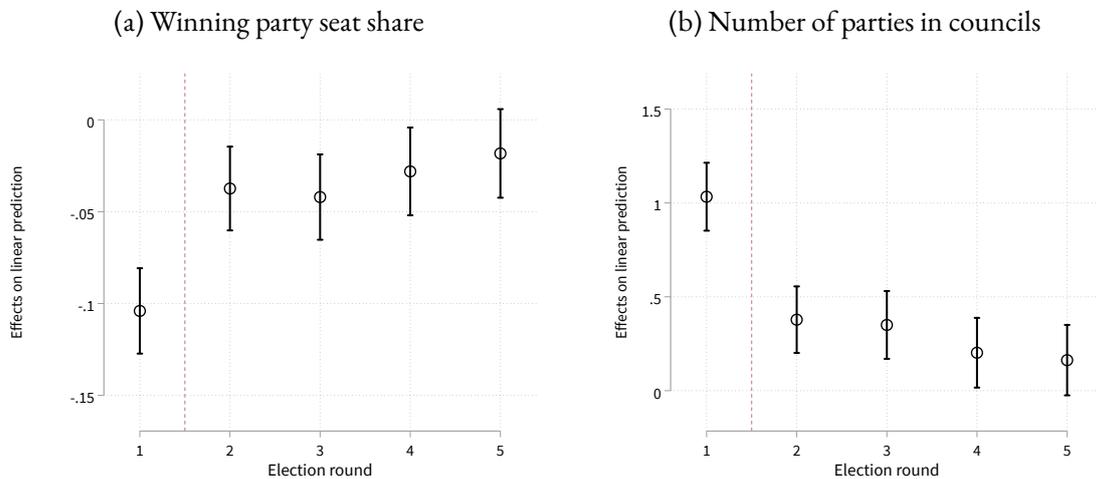
(a) Winning party seat share   (b) Number of parties in councils

Note: Bandwidths indicate 99% confidence intervals; dashed line indicates time at which electoral formula was changed. Standard errors are clustered on a municipality level.

Second, to further explore my proposed mechanism, I examine the extent to which working-class policymakers enter office as a result of being included on winning vs. opposition party lists. For this, I operationalize two alternative outcome variables. This first indicates the proportion of winning party legislators in a given council that belong to the working class. The second variable

captures the same outcome, but for opposition parties.

Figure 9 displays the estimates obtained from these models. Supporting my hypotheses, we see that, as winning parties acquire larger seat shares in councils, the proportion of the seats that they allocate to working class individuals increases by five percentage points. A difference that is highly significant at the 99% confidence interval. In contrast, the share of workers within opposition parties remains constant throughout the period I evaluate. Overall, these findings imply a low likelihood of obtained treatment effects stemming from a universal shift in the incentives of political parties to advocate for more working class legislators.

In theory, observed trends could stem from the reform having reduced anti-worker sentiment among electorates. Several studies have, however, failed to detect that voters systematically discriminate between political candidates based on social class (Carnes and Lupu, 2016a; Schwarz and Coppock, 2022). Moreover, it is difficult to construe of a theoretical mechanism through which a majoritarian formula change would serve to reduce any such latent bias.

Figure 9: Treatment effects on working class share of winning vs. opposition party council seats

(a) Winning parties

(b) Opposition parties
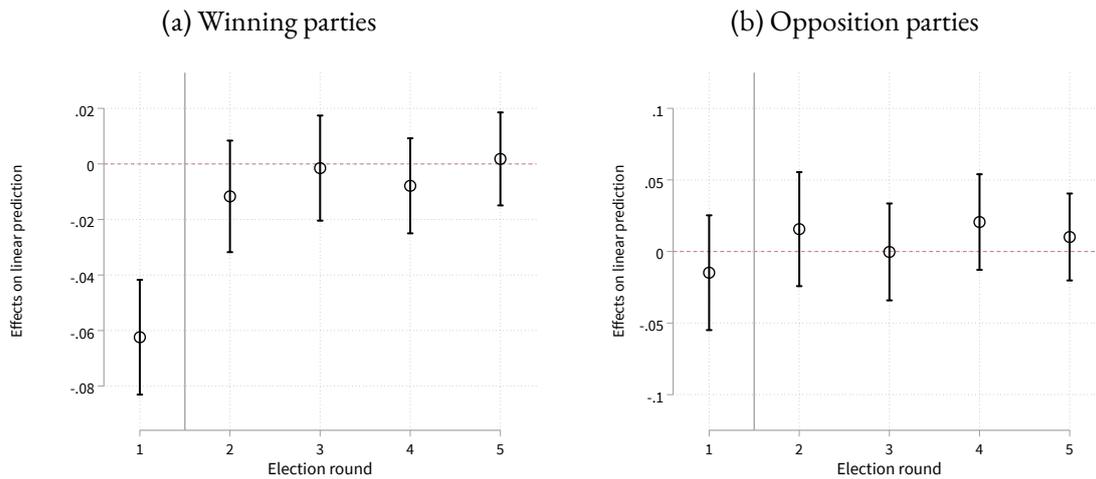
Note: Bandwidths indicate 99% confidence intervals; dashed vertical line indicates time at which electoral formula was changed. Standard errors are clustered on a municipality level.

6.2 Changes in party nomination incentives

Aside from altering party magnitudes, electoral formulas have been theorized to affect the strategic considerations parties. This, in turn, may have consequences for the socio-demographic composition of party lists. Under a full proportionality formula, it is commonly accepted that 'parties have strong electoral incentives to maximize their collective appeal in lists by including candidates representing all the major social cleavages in the electorate' (Norris, 2006, p.41). A series of correlational studies have shown this to be associated with higher rates of office-holding by statistically atypical policymakers, such as women (e.g. Kittilson and Schwindt-Bayer, 2012; Krook, 2018; Matland, 1998) or persons belonging to ethnic minorities (Hughes, 2016; Le Lohé, 2004; Moser, 2008).

When electoral formulas are less proportional in nature, aforementioned incentives should be noticeably lower as parties need obtain only a plurality of votes to gain full control of the legislature.

As highlighted, electoral system disproportionality is mainly theorized by scholars to cause an overall *reduction* in party incentives to advocate for statistically atypical candidates. If this line of theory holds, it thus implies that my analysis yields lower-bound estimates of reform effects. More problematic for my theory would be a scenario in which the plurality rule encourages parties to cater directly to e.g. low income voters by facilitating the campaigns of working class candidates.

Assuming that a majority bonus system makes parties more likely to endorse worker candidates, this change in incentives should arguably also manifest in the altered class composition of other electoral institutions, aside from the council. Under the bonus system, the locus of political competition is actually the mayorship: a voter's choice for mayor will always automatically translate to their vote for a council that is fully controlled by the mayoral candidate's party list.[14] Indeed, mayoral candidates serve as automatic capolistas of their respective party lists. In failing to obtain the executive post, they most often become opposition party councillors if their party manages to enter the local legislature. The most effective means by which parties can attract votes via descriptive representation is thus via its selection of a mayoral candidate (Freschi and Mete, 2020; Magnier, 2004).

To test this notion, I re-run my models with an alternative outcome: a dummy variable indicating whether a mayor in a given municipality is working class (1) or not (0). In these models, obtained estimates thus indicate a difference in the proportion of working class mayors, across municipalities that did vs. did not enact the majority bonus reform in 1993.

Figure 10 presents the findings of this analysis, and shows that the reform instilled no difference in the capacity of workers to obtain the mayorship. Differences are insignificant even at 90% confidence intervals. It is difficult to attribute the absence of a change to a potential ceiling effect, as only 3 percent of mayors were working class in treated municipalities prior to the 1993 reform.

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[14] Recall here that—among the municipalities in my sample—voters are unable to vote for a party list different from that of their preferred mayoral candidate.

Figure 10: Treatment effects on likelihood of electing a working-class mayor
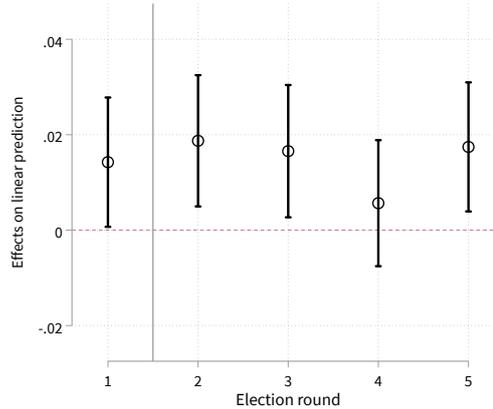
Note: Bandwidths indicate 90% confidence intervals. Models include for municipal-level controls.
Standard errors are clustered on a municipality level.

7 Discussion

In sum, the analysis presented in this paper finds that disproportionality in electoral formulas can serve to bolster working-class representation in politics under PR list systems. This occurs as disproportionality inflates the party magnitude of election-winning parties, allowing them to fill more seats in the legislature. In support of my theory, I show that the within-party proportion of working-class legislators increases among winning parties because of the reform, while this remains constant for opposition parties. I also find that formula change had insignificant effects on the propensity of municipalities to elect working-class mayors into office. As a number of studies have shown that voters do not systematically discriminate against working-class political candidates, I interpret this as tentative evidence of the reform not having systematically affected party nomination incentives.

Some aspects of my findings deserve additional elaboration. First, one may ask: if party selectorates are systematically biased against the working-class—why do we see any workers in government? To this extent, I highlight that the premise of work on political selection is that selectorates are implicitly biased in their appraisal of potential candidates as they rely on heuristics such as education to gauge legislator competence. This is a process very different from party elites proactively structuring lists so that workers are kept out of office. This also makes the study of e.g. gender representation in Italian politics distinct from that of class, as elite bias rooted in socially conservative gender norms has traditionally been much stronger.

Second, the treatment effects I uncover are remarkably stable over time. This raises a further question. If selectorates retain an anti-worker bias, why do they not change their nomination strategies over time? Here I highlight the local nature of the elections that I examine. In these, selectorates are local in nature and typically consist of incumbents or/and other persons that have

formerly served in municipal councils. If workers thus manage to enter office, the class composition of the selectorate will thus also change, effectively altering its median preference. Moreover, incumbency and a proven ability to win elections is seen by most scholars as the key valence trait assessed by party elites tasked with composing election lists. It is thus also conceivable that middle-class members of the selectorate change their priors in light of more experience with working class legislators.

The big theoretical question surrounding my findings is whether formula change induced attitudinal changes to local electorates. For one, this could occur as disproportional election formulas reduce the likelihood of an individual vote being decisive for affecting legislature party composition (Cox, 1999; Eggers, 2015). This, in turn, compresses voter turnout as voters' stop believing that their vote will change election outcomes. Examining this question in French municipal elections, Eggers (2015) finds that turnout increases by one percentage point under a PR majority bonus system, relative to a pure majoritarian system where seats are allocated entirely by plurality rule.

Aforementioned theory certainly implies that increased formula disproportionality in the Italian case may have served to reduce voter turnout. It is noteworthy to highlight, however, that proportionality differences between fully majoritarian and PR majority bonus systems in France are likely larger than those detectable across PR and PR majority bonus systems in the Italian municipalities that I analyse.[15] As such, the small magnitude of obtained treatment effects in the French case implies a low likelihood of uncovering similar findings in the Italian setting. It also renders it unlikely that a change in turnout would effectively result in a change to legislature party composition—and, in extension, the numerical prevalence of working-class councilpersons.

Alternative to affecting turnout, one may also conjecture that election formulas directly improved the election fortunes of left-wing parties, which may include a higher number of workers on their party lists. To the best of my knowledge, there are no theoretical accounts (formal or otherwise) that have postulated electoral system disproportionality as a driver of e.g. left-party vote shares. What also speaks against this mechanism in isolation, is that I fail to detect any changes to the working-class proportion of opposition party legislators. This should, in theory, decrease if right-wing parties with lower numbers of workers on lists, start replacing left-of-center parties the legislature's opposition.

In sum, I therefore deem alternative, psychological effects as being an unlikely driver of my empirical results. That said, it is difficult to rule out this line of theory entirely without—at minimum— examining additional data on voting patterns. To address this, I am currently in the process of digitising electoral returns data from the Italian municipal elections of 1988-1990. Doing this will enable to assess how increased formula disproportionality affected turnout, blank voting, left-party vote shares and party win-margins. These tests should yield additional insights on the mechanisms underscoring obtained treatment effects.

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[15] The size of the French bonus is smaller at a 50 percent share of the local legislature.

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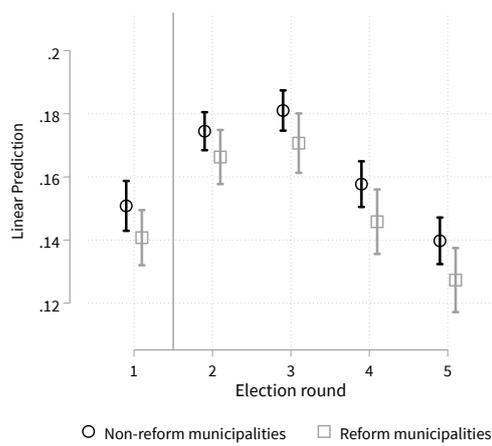Sveriges Kommuner och Länsting (2018). Styren i kommuner och landsting, 1994-2014.

Appendix

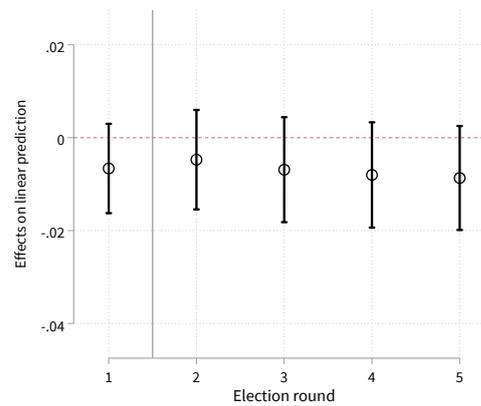## A   Additional estimations

A.1   Tests on individuals inactive in labor market

Figure A.1: Treatment effects on persons inactive in labor markets

(a) Over-time trends

(b) Marginal effects

Note: To highlight statistical insignificance of effects, bars display 90% confidence intervals. Dashed vertical line indicates points in time in which electoral formulas were changed.

A.2   Tests on parallel trends assumptions

Figure A.2: Parallel-trends tests on middle class representation

(a) Upper-middle class

(b) Lower-middle class
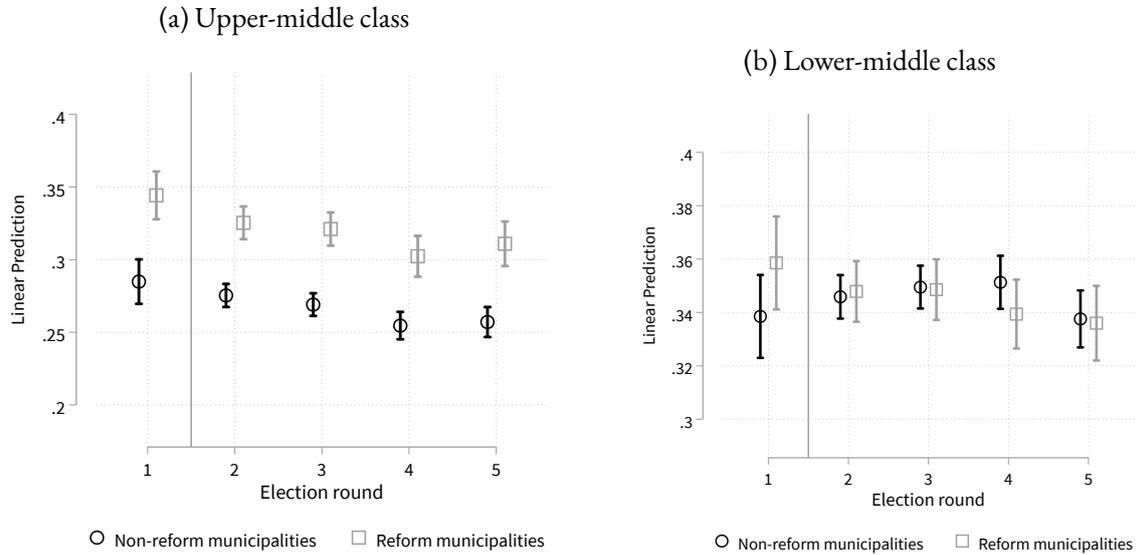
Note: Bars display 95% confidence intervals. Dashed vertical line indicates points in time in which electoral formulas were changed.

Figure A.3: Winning party seat shares and council party numbers by treatment assignment
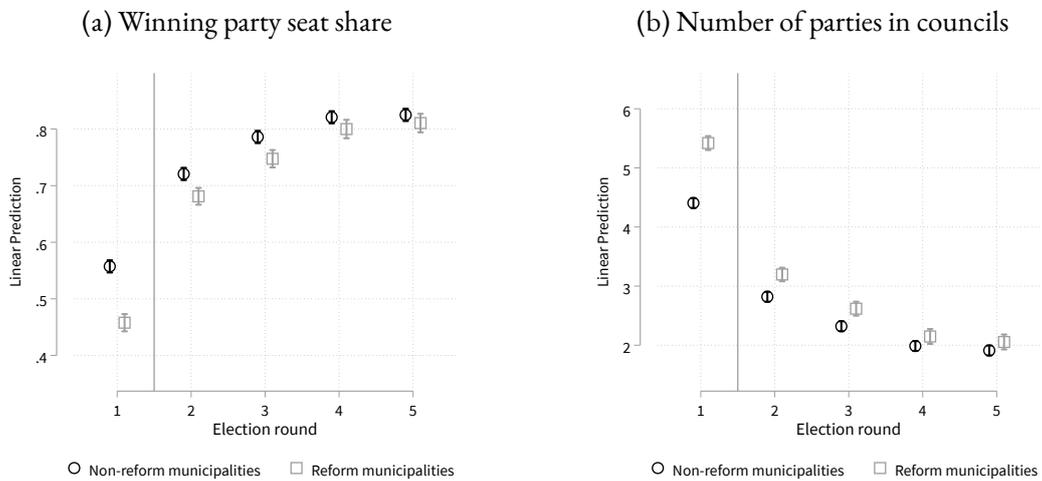
(a) Winning party seat share

(b) Number of parties in councils

Note: Bandwidths indicate 99% confidence intervals; dashed line indicates time at which electoral formula was changed. Standard errors are clustered on a municipality level.

A.3   Density tests

Figure A.4: Discontinuity tests on the density of population size around the cut-off, by election round
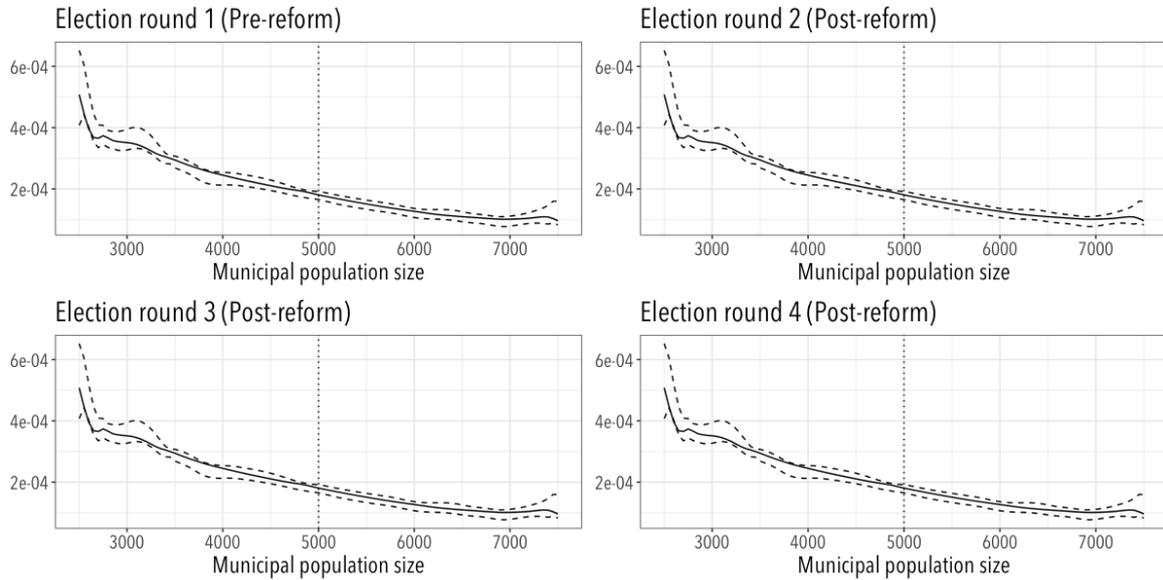
Notes: Data from the Italian national censuses of 1991, 2001 and 2011. Solid lines represent split fourth-order polynomials of municipal population size under MSE-optimal local fits. Dashed lines indicate 95% confidence intervals. Vertical dotted line depicts treatment assignment cut-off of 5000 inhabitants.
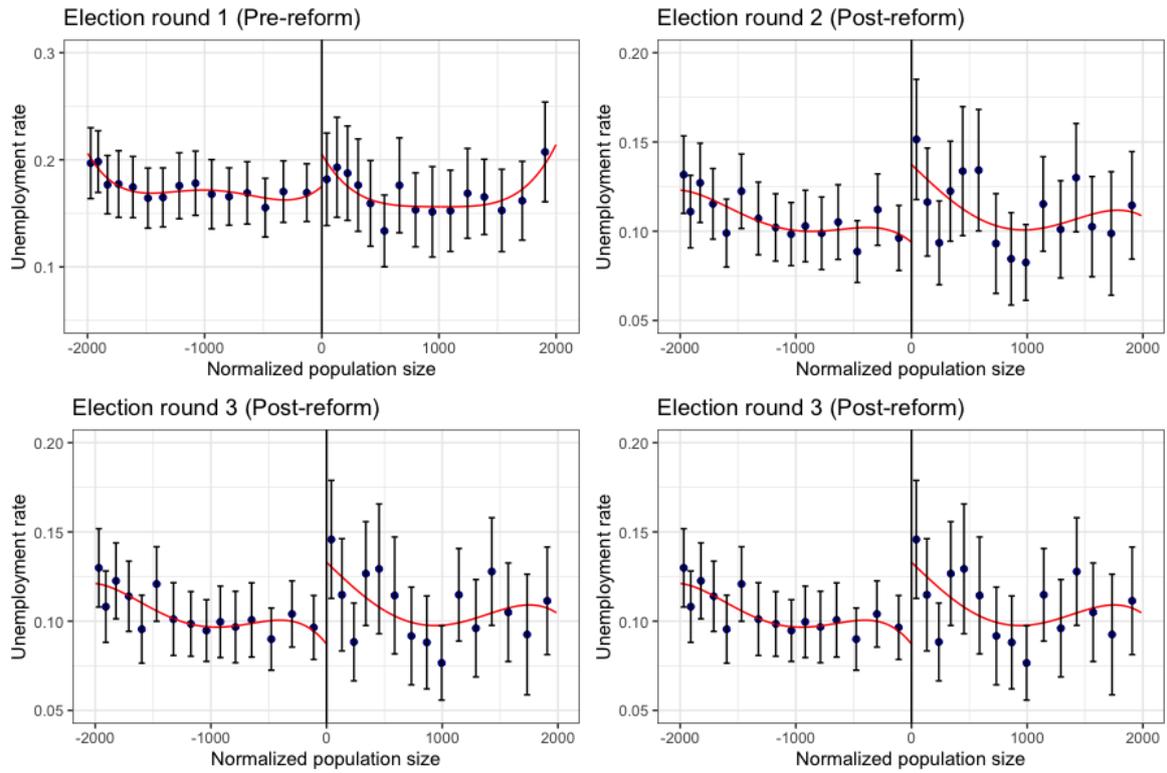
A.4   Covariate balance checks

Figure A.5: Educational attainment by running variable, per election round
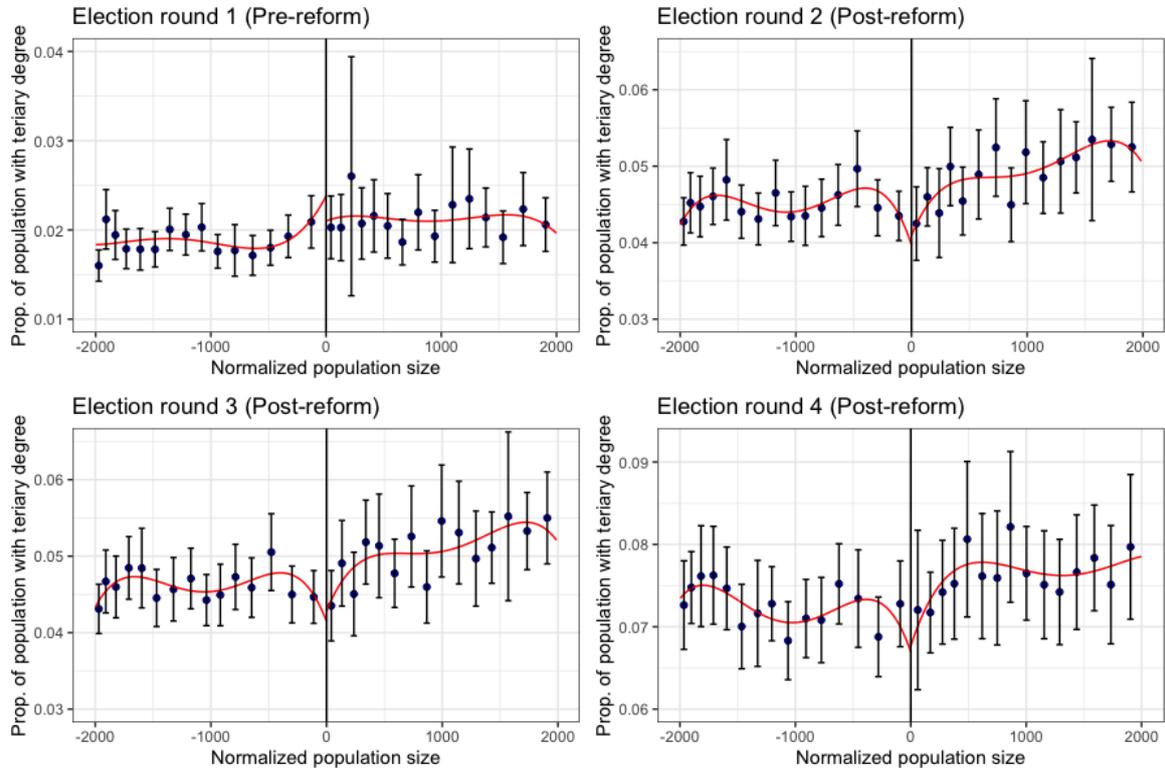
Note: Data binned into quantiles. Horizontal axis depicts actual population size minus 5000 (cut-off point); Local linear regression overlaid is a spline 4th-order polynomial fit. Bandwidths indicate 95% confidence intervals.
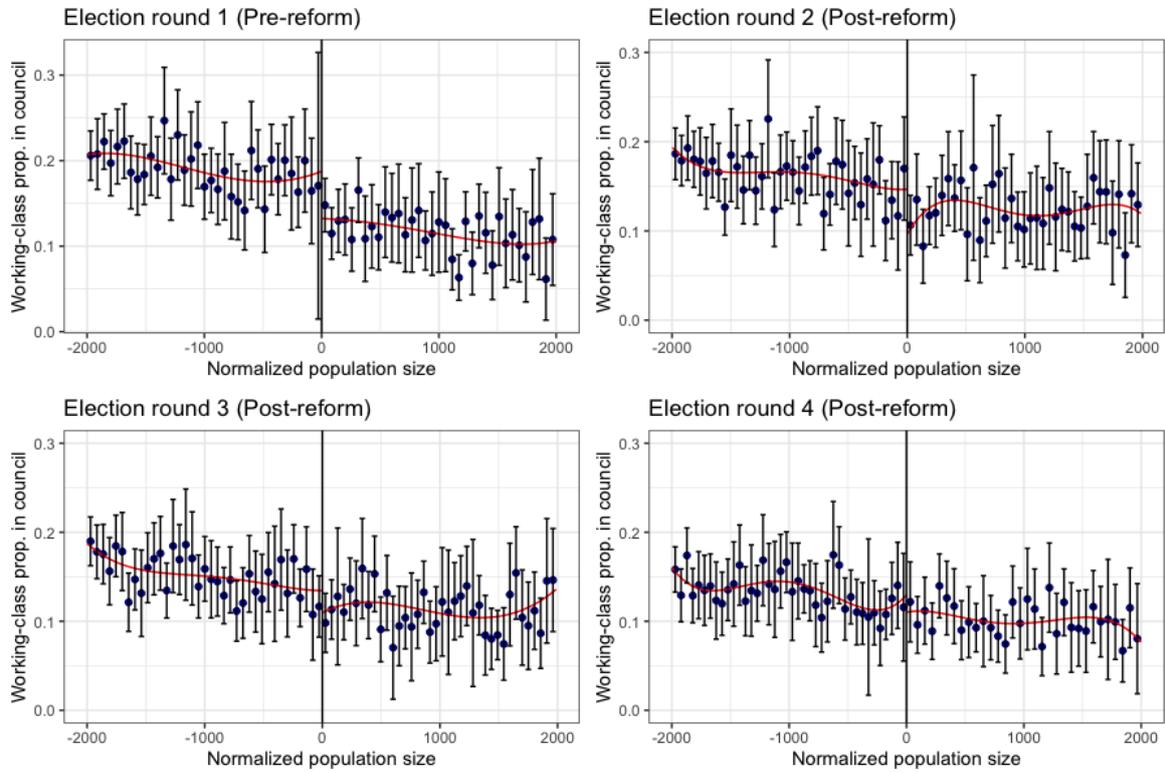
Figure A.6: Unemployment rate by running variable, per election round

Note: Data binned into quantiles. Horizontal axis depicts actual population size minus 5000 (cut-off point); Local linear regression overlaid is a spline 4th-order polynomial fit. Bandwidths indicate 95% confidence intervals.

A.5   RD plots

Figure A.7: RDD plots by election round

Note: Binned data where bins have been evenly spaced and variance-mimicked using spacings estimators. Vertival line indicates cut-off point for treatment assignment; Local linear regression overlaid is a spline 4th-order polynomial fit. Bandwidths indicate 95% confidence intervals.