Random Votes to Parties and Policies in Coalition Governments*

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June 2024

Abstract

We exploit a natural experiment involving a randomization of votes across parties within coalitions in all local elections in Italy for over a decade. A lottery on the position of party symbols in the ballot papers allows estimating the causal effect of increasing votes to parties for coalition policies. A non-marginal random boost of votes shifts budgetary spending towards the treated party's platform, but only for issues that are salient in that party's political manifesto. We study the chains of mechanisms mapping votes into policies and link it to an increase in bargaining power within legislative majorities. Parties leverage their higher electoral support to gain the appointment of politically affiliated cabinet members. Empowering different parties also leads to the selection of cabinets with different socio-demographic characteristics. The unintentional experiment helps shed new light on mechanisms mapping votes to parties into coalition policies.

JEL Classification: D70, H70, P16.

Keywords: Coalition Governments; Party Electoral Platforms, Public Policies; Selection of Policy Makers; Random Lotteries.

*We thank four anonymous referees for their extensive comments and useful suggestions. We are grateful to David Card, Ernesto Dal Bó, Enrico Cantoni, Ruben Durante, Frederico Finan, Olle Folke, Margherita Fort, Gianmarco León-Ciliotta, Matthew Gentzkow, Robert Gold, Simon Luechinger, Massimo Morelli, Maria Petrova, Per Pettersson-Lidbom, Paolo Pinotti, Vincent Pons, Giacomo Ponzetto, Johanna Rickne, Lukas Schmid, Francesco Trebbi, and Guo Xu for their valuable comments. Thanks also go to participants of the Workshop on Institutions, Individual Behaviour and Economic Outcomes in Alghero, SIEP in Catania, the Calgary Workshop in Political Economy, the Spring Meeting of Young Economists in Brussels, the Workshop on Political Economy in Dresden, the Annual Meeting of the French Economic Association (AFSE) in Paris, the European Meeting of the Econometric Society (EEA - ESEM) in Cologne, the European Public Choice Society (EPCS) in Jerusalem, seminar participants in Lucerne University and to Fiorita De Santis and Rosalba Salvato (Italian Ministry of Interior), Giacomo Opocher and Gianmarco Torchetti for support in data collection.

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1 Introduction

The working of representative democracies critically hinges on the electoral procedures through which millions of citizens express their preferences over parties and politicians. While this selection process is vital, it is fraught with challenges (Besley, 2005). Parties select candidates and control the appointment of administrators with heterogeneous characteristics and (latent) preferences (e.g., Dal Bó and Finan, 2018; Dal Bó et al., 2017; Dal Bó et al., 2023). In theory, voters reward parties that keep their electoral promises, providing incentives to avoid shirking away from announced policies (e.g., Barro, 1973; Ferejohn, 1986; Ashworth, 2012), but evidence of significant frictions raises questions on the quality of voters' representation. Voters have limited information on politicians and platforms (Ferraz and Finan, 2008; Kendall, Nannicini and Trebbi, 2015; Pons, 2018) and the process of voter preference transmission is noisy (e.g., Shue and Luttmer, 2009; Augenblik and Nicholson, 2016). Ideological parties should provide intrinsic policy commitment making representative democracies work (Alesina, 1988; Wittman, 1989). Nonetheless, in multi-party governments which are now the rule in Western democracies, no party alone is ultimately responsible for coalition choices and the link between voters and policies can be jeopardized even with ideological parties (e.g., Sartori, 1976; Martin and Vanberg, 2020; Kam et al, 2020).[1]

The policy-making power of a party within coalitions does not directly stem from votes but from the formation of legislative majorities and the subsequent appointment and control of cabinet members. Votes are mechanically mapped into seats by electoral rules but the role of bargaining power of parties in legislative bodies is debated (e.g., Austen-Smith and Banks, 1988; Baron, 1993; Morelli, 1999; Baron and Diermeier, 2001; Lijphart, 2012). Existing evidence focuses on marginal gains of seats in legislatures by block majorities or single parties (e.g., Pettersson-Lidbom, 2008; Folke, 2014; Fiva et al., 2018) with mixed findings. The distinctive feature of parliamentary systems is the formation of the executive (e.g., Martin and Vanberg, 2014). There is no formal rule on the partisan composition of governments, and there is no reason why seats should be expected to map one-to-one into executive power (e.g., Powell, 2006). Limited control associated with inter-party delegation, and the need

[1]The share of coalition governments steadily increased from about 45% in 1960 to over 65% in 2016 (see also Armingeon et al. 2017).

to coordinate on a common policy, make the appointment of cabinets the critical juncture for the policy-making power of parties in coalitions (e.g., Thies, 2001). In the end, the capability of a party to affect coalition policies, and its credibility towards voters, rests on the preferences and the behavior of appointed cabinet members (Kiewiet and McCubbins, 1991). Whether, and how, votes to parties impact the bargaining power within ruling majorities, the selection of policymakers, and ultimately coalition policies are open empirical questions (see Literature Section 2).

This paper investigates these questions in the context of a large-scale natural experiment that exploits the random reallocation of votes to parties within coalitions (see Section 3). We look at the universe of the approximately 1,200 local elections held over the decade 2002-2012 in around 600 municipalities, home to over 35 million inhabitants in Italy. We exploit a law that introduced lotteries that independently randomize the position of party symbols within each coalition running in a given municipality and election year. We document that the articulated electoral rules and a particular graphical design of the ballot papers created a behavioral focal point leading to a systematic random reshuffling of votes across parties. We assess the existence and quantify the magnitude of systematic noise induced by these electoral procedures and document that, for over a decade, a non-marginal share of votes was systematically and randomly reallocated between parties within running coalitions supporting a given candidate for mayor.[2]

The natural experiment allows us to study how a party's higher electoral support impacts coalition governments' main policies in terms of welfare, education, property taxes, and security (see Section 4). Four main blocks of national parties – left, center-left, center-right, and populist right – also run at the local level. These parties feature very different electoral platforms as documented by the favorable mentions of each policy issue in their electoral manifesto. For the left, welfare and education are the most prominent issues, while the center-right emphasizes low taxes and the populist right focuses on security. The platform of the center-left party is instead more moderate and is closer to the mean mentions of all issues

[2]The voting rule allows casting multiple votes with multiple patterns. Together with the particular graphical structure of the voting ballots, this induces a horizontal adjacency focal point that favors the party whose symbol is randomly located directly to the right of the name of the candidate for mayor heading the coalition. Reshuffling of votes occurs within ruling and non-ruling coalitions, impacts parties of all sizes, and is unrelated to local characteristics and past political outcomes and policies. See Section 3.

across parties, with no policy area standing out as salient. Identification of the causal impact of treating a party with a given ideology is achieved, as in randomized control trials, by comparing a given party when it is randomly treated with a boost of votes and when it is not. We estimate both intention-to-treat specifications and the effect of increasing vote shares within coalitions in instrumental variable specifications. The latter allows us to estimate the impact of a change in vote shares on policy issues.[3] The results document that the comparatively small, but non-marginal, random reshuffling of votes significantly changes coalition policies. Specifically, the impact on policies can be interpreted as a shift of expenditure away from the underlying average spending on policies that would be preferred by the rest of the coalition and towards the budgetary issue that is more prominent for the treated party (if such a salient policy exists).[4] The effects are sizable and can be detected for small and big parties.

A great effort is devoted to exploring the chain of mechanisms (see Section 5). We first study how a random boost in votes affects the bargaining power of parties within the legislative body. The features of the natural experiment allow ruling out that the impact of parties on policies runs through the process of formation of legislative majorities. The results document that the change in policies is due to a change in the relative bargaining power of the treated party within ruling coalitions, rather than an increase in legislative representation *per se*, supporting theories of efficient bargaining.[5] Next, we study the implications for the formation of the government for which no evidence is available in the literature. To this end, we assemble a database with information on the partisan affiliation and socio-demographic fea-

[3]We perform extensive tests to check the randomization procedure. The results show that the treatment is monotonic, it can be detected for both ruling (majority) and non-ruling (minority) coalitions, and for both small and big parties. None of the parties has a different probability of being treated and treated and non-treated parties are observationally identical. The results consistently emerge with alternative specifications and samples.

[4]In line with the lack of a specific policy standing out as prominent, no systematic and significant impact on coalition policies is detected when increasing the vote share of the center-left party.

[5]In our setup, running coalitions are pre-determined before randomization and voting take place, and we document that the treatment only reshuffles votes within, but not across, running coalitions. These features make it possible to account for latent confounders related to the formation of ruling coalitions. We also document that randomly empowering parties in legislative minorities does not affect coalition policies. The evidence shows that the effects are associated to changes in bargaining power within legislative majorities and is against the alternative (more extreme and opposite) views that parties can matter for policies only if they are pivotal in forming legislative majorities or that even parties outside legislative majorities can impact coalition policies by trading their legislative support to the government. See discussion of the literature in Section 2.

tures of cabinet members and the policy departments under their control.[6] Differently from the tight mapping between vote shares and seat shares in legislative majorities, the association between vote shares and the share of cabinet members affiliated with a party is positive but very loose and noisy. The raw data, therefore, supports the view that cabinet members are likely selected based on unobservable individual features like, e.g., their expertise, skills, or personal ties to the mayor or councilors, and that the process of formation of cabinets is driven by factors other than parties' legislative representation. Yet, the random treatment allows going beyond these loose correlations and permits isolating the causal impact of votes to a party on the selection of cabinet members. The evidence documents that parties leverage the exogenous increases in vote shares to get affiliated politicians appointed as cabinet members. Also, on top of changing the partisan composition of the government, treating different parties materializes in cabinets with significantly different socio-demographic characteristics. This is because parties gain the appointment of affiliated cabinet members with distinct features who tend to control specific policy departments. The findings hold for all parties featuring salient policy issues in their electoral manifesto.[7] Finally, the magnitude of the effects tends to be larger when treated parties are confronted with politically weaker mayors, further suggesting that the effect on policies comes from parties successfully leveraging their higher electoral support.

2 Literature

This paper contributes to the empirical political economics literature by providing identification through a lottery that randomizes the order of parties in the ballot papers. This randomization process is independent across running coalitions, municipalities, and election years.

[6] We retrieve information on political affiliation and socio-demographic characteristics for about 8,000 cabinet members and, for a small subset, also on the policy departments assigned to their control.

[7] The causal estimates also show that the features of appointed cabinet members align well with the prototypical features of voters reporting interests over specific policies, as documented in the existing literature (see Section 2). Treating the left party leads to more elderly (and women) in the cabinet, treating the center-right party brings more professionals, and the populist-right more men in the cabinet. The results align well with the existing evidence documenting that these socio-demographic features are typically observed in voters reporting a specific interest in welfare, low taxes, and security, respectively. Furthermore, consistent with the lack of significant effects on policies, we find no effect on the socio-demographic composition of cabinets when treating the center-left party.

A unique feature of the experiment is that it involves randomization of the actual distribution of votes to parties rather than information disclosure to either voters or politicians. Ferraz and Finan (2008) exploit random audits of politicians to identify the effect of disclosing information to voters, while Ferraz and Finan (2011) and Avis, Ferraz and Finan (2018) look at the (corrupt) behavior of politicians. Kendall, Nannicini and Trebbi (2015) and Pons (2018) implement large-scale randomized information campaigns on election outcomes. This paper also relates to the recent body of evidence that investigates the role of electoral procedures on relevant outcomes like, e.g., turnout (Fujiwara, 2015; Cantoni and Pons, 2021); strategic voting choices (Pons and Tricaud, 2018), or, party advertising (Gulzar, Robinson and Ruiz, 2022). The analysis contributes to the behavioral literature on the role of voting procedures for vote casting, which has documented several cases of mistakes or randomness induced by electoral systems. The literature on ballot-order effects has shown that when candidates are listed on the ballot, the first (or the last) sometimes tend to receive a higher share of votes (see Taebel, 1975; Ho and Imai, 2006). Wand et al. (2001) show that the voting mistakes induced by the butterfly ballot used in Florida in the 2000 US presidential elections likely affected election results. Shue and Luttmer (2009) show that poorly educated, poor, and third-party voters are more likely to misvote. Augenblik and Nicholson (2016) investigate the fatigue induced by a long list of voting decisions. The focal point documented in this paper is associated with a pattern of horizontal adjacency, which makes it a more involved setup as compared to instances of ballot order that focus on specific positions, like, e.g., being the first party on the list.[8] There is no evidence that the focal point was known by parties or voters.[9] A unique feature of the quasi-natural experiment is that it involves a reshuffling of votes to parties within running coalitions. Evidence on the policy impact of mistakes or randomness in vote casting is scant in general and we lack any evidence for coalition governments. Shi and Singleton (2023) exploit a ballot order effect and show that an additional

[8]Instances of horizontal adjacency effects that impact vote casting have been detected even in comparatively simple and well-designed voting settings. For instance, votes can be cast in support of little-known candidates merely because their name appears horizontally adjacent to that of a popular politician (see Dee, 2007; Shue and Luttmer, 2009).

[9]We cannot detect any effect for the first party on the list. In this respect, the focal point differs from contexts in which being first on the list delivers an advantage that is known to parties and can be exploited, for instance, by trying to be first on the list in the electoral settings in which the list of parties is not random but depends on the order of registration in the electoral offices, or by strategically changing effort in campaigning (see Gulzar, Robinson, and Ruiz, 2022).

educator elected in school boards in California impacts choices on charter schooling and teacher salaries. Besides quantifying the impact on vote casting, the evidence presented in this paper goes beyond the results in the literature by showing that the (ill) design of election papers systematically impacts the actual budgetary spending of municipalities.

Theories of legislative representation deliver contrasting predictions. In the most restrictive view, a party only should matter if it is pivotal in forming the majority (Baron, 1993; Morelli, 1999) while in the most extensive, all parties can trade their support to the government and might matter irrespective of whether they belong to the majority (see Lijphart, 2012). Instead, for theories of efficient bargaining, coalition policies should be a (weighted) function of the ideal positions of parties within majorities (e.g., Austen-Smith Banks 1988; Baron and Diermeier, 2001) and the power of a party should increases in its vote share (e.g., Muller and Strom, 2003).[10] The existing empirical research on the causal role of parties for policies exploits close electoral outcomes. Regression discontinuity designs have been extensively applied to majoritarian settings (e.g., Lee, Moretti, and Butler, 2004; Ferreira and Gyourko 2009; Gerber and Hopkins, 2011; Kirkland and Phillips (2018); Thompson, 2020, among others).[11] In a coalition government, the definition of electoral closeness is conceptually non-straightforward.[12] Pettersson-Lidbom (2008) restricts attention to coalitional majority thresholds while Fiva, Folke and Sørensen, (2018) build on Folke (2014) to measure the closeness between left and right blocks and the councils' overall left-right ideologies.[13] Closer to our question, Folke (2014) looks at seat wins by a close vote margin and

[10]Furthermore, rational voters should strategically anticipate the process of coalition formation, e.g., by voting for extreme parties to avoid watering down coalition policies, see Kedar (2005).

[11]Lee, Moretti, and Butler (2004) document the role of partisan votes by politicians. Ferreira and Gyourko (2009) fail to find effects on the size of city governments, local public spending, and crime rates at the local level in the US (a result possibly explained by Tiebout competition). Gerber and Hopkins (2011) show that US mayors affect policies in areas where there is local policy discretion, while Kirkland and Phillips (2018) show that divided governments impose delays in policy making and budgeting, that are larger when the personal and political costs of stalemates are low. Thompson (2020) finds no evidence of partisanship in elected sheriffs when applying law enforcement.

[12]In this context, Folke (2014) is the main reference for applications that exploit closeness as an identification strategy or as a measure of political competition. See also Freier and Odendahl (2015) and Luechinger et al. (2020), for alternative methods to identify close margins and a methodological discussion. See also Fiva and Halse (2016) for an application to political home bias.

[13]Pettersson-Lidbom (2008) finds that left-wing majorities spend and tax more and have lower unemployment. Fiva, Folke and Sørensen, (2018) find that the right block reduces taxes while a shift towards right-wing ideologies reduces spending in child care while increasing spending on the elderly but does not affect local public goods.

finds effects for small parties on secondary policies but not on bigger parties and primary policies.[14] Strategies based on close electoral outcomes do not allow telling apart bargaining power within ruling coalitions from the process of coalition formation.[15] The natural experiment that we exploit allows telling apart the different predictions on the role of parties within legislative bodies. The analysis isolates the role of the relative power of parties within ruling coalitions, providing, so far missing, support for theories of efficient bargaining (e.g., Austen-Smith Banks, 1988). We find that a boost of votes to parties impacts policies by increasing the bargaining power within legislative majorities. The treatment does not affect the formation of governing majorities, ruling out that a party matters only when it is pivotal in the coalition formation process (e.g., Morelli, 1999).[16] Another novelty is that we look at primary fiscal policies closely related to each party's political manifesto. The results document that treating a party shifts primary coalition policies, like education, welfare, security spending, and taxes, toward that party's political platform. Rather than party size, what seems to matter is how salient each policy area is for the treated party and how divisive is a given issue as compared to coalition partners.

Compared to existing literature, the natural experiment allows us to push the investigation of the mechanisms beyond legislative representation and study the impact of party votes for government formation and cabinet members' selection in multi-party systems. A key difference to the process of legislative representation is that cabinet members are not elected but appointed, with no formal link to votes or seats. Hence, the capability of a party to shape government formation ultimately rests on its factual bargaining power. The formation of coalition cabinets can be complex as it involves a process of bargaining and delegation of policy-makers that crosses party lines (e.g., Lijphart, 1984; Powell, 2006). This limits, by construction, the ability of a party to control policies and poses a threat to electoral account-

[14]Folke (2014) looks at local elections in Sweden and finds that a seat marginally gained by the Green Party, or by New Democracy, as compared to the social democrats (or other parties in dyadic comparisons), impacts environmental scores and number of refugees accepted in a municipality, respectively. No effects are detected for bigger parties and primary policies like taxes.

[15]In the words of Folke (2014, p. 1392): "It is not possible to directly link the as good as random seat shifts to differential probabilities of entering into a governing coalition".

[16]The analysis on treated parties not belonging to the ruling coalition fails to detect any impact on policies. These findings support theories of efficient bargaining (e.g. Baron, 1993) and are against arguments suggesting a role for parties represented in legislative bodies even if belonging to minorities, (see e.g., Lijphart, 2012).

ability, particularly in highly fragmented coalitions (Kam et al., 2020). Ideological parties can select and discipline like-minded politicians with genuine policy preferences but cannot control policymakers affiliated with other parties.[17] The literature puts forward two main, *non mutually exclusive*, strategies available to parties to increase control of policymakers and keep tabs on each other in coalition governments. First, bargain to get politically affiliated cabinet members to control salient policy departments for a given party and then exert party discipline and control (e.g., Laver and Shepsle, 1990, 1994). Second, select cabinet members with genuine policy preferences that are aligned with their electoral platforms and possibly irrespective of their political affiliation (e.g., Kiewiet and McCubbins, 1991; Thies, 2001). Understanding the empirical role of parties for the features of policymakers is a main challenge in political economics (e.g., Dal Bó and Finan, 2018).[18] Using data for Sweden, Dal Bó et al. (2017) and Dal Bó et al. (2023) find, respectively, that politicians are broadly representative of the population and that the populist party attracts politicians with specific socio-economic backgrounds. Our analysis contributes evidence that bridges the literature on the selection of politicians to the studies on parties and the mechanism through which they influence policies. In our data, different policy departments are assigned to cabinet members with features that are broadly in line with the existing evidence that links voters' policy preferences to individual characteristics in terms of gender, age, and occupations (see Page and Shapiro, 2010 and Alesina and Giuliano, 2011, for surveys). The results provide the first causal evidence supporting theories highlighting the key role of the selection of policy makers with specific ideologies and intrinsic preferences. The findings show that treating parties with electoral manifesto that emphasize a subset of salient issues leads to the appointment of politically affiliated cabinet members with socio-demographic features that are broadly consistent with the prototypical features of voters that support these policies. The findings suggest that empowering parties impact coalition policies by changing both the partisan composition of cabinets and by selecting cabinet members with different socio-demographic features (and

[17] See Wittman (1989) and theories by Snyder and Ting (2002), Levy (2004), Morelli (2004) and Geys and Vermier (2014), among others. See also Aldrich (1995) and Caillaud and Tirole (2002).

[18] Comparatively large theoretical attention has been paid to the drivers of the quality, or valence, of politicians (e.g. Caselli and Morelli, 2004 and Besley, 2005). Most empirical evidence looks at education and documents the role of economic incentives (Ferraz and Finan, 2009; Gagliarducci and Nannicini, 2013; Dal Bó et al. 2013); and, Galasso and Nannicini (2011) for political careers within parties.

latent policy preferences).

3 A "Vote Randomization" Natural Experiment

In this section, we present that the details of the voting rules, the randomization of the parties' position on the voting ballots, and their particular graphical design imply that a non-negligible share of the votes is randomly (re)allocated across parties within coalitions running in a given municipality and election year.

3.1 Institutional background

We focus on municipalities where the electoral rule involves multi-party governments. These are medium to large municipalities with more than 15,000 inhabitants, home to about 35 million people and 60 percent of the Italian population.[19] Municipalities have administrative and financial autonomy and control spending on local services like public order and local police, kindergarten and primary schools, and social welfare, among others. Municipal expenditure is covered, in addition to transfers from the central government, by fees on local public goods and services, house property taxes, and a local income tax (within margins set by national laws). The political system resembles a national parliamentary system with a city council akin to a parliament formed on average by 25 elected councilors. The mayor, akin to a prime minister, heads the cabinet that, in our sample, is composed, on average, of 8 members. Candidates run for mayor supported by a coalition of parties formed *before* the election. The candidate for mayor that receives the majority of votes wins the election.[20]

The electoral system is designed to empower the executive while making it accountable

[19]Electoral rules in the smaller municipalities, with a size below 15,000 inhabitants, differ, with only one party ruling in the local government. Data for 2011, the last available census in our window of observation (Italian National Institute of Statistics—*Source*: http://demo.istat.it). The city size distribution is skewed with a mean of around 50,000 inhabitants and a standard deviation of about 140,000. See also Table A23 for summary statistics. In the autonomous region of Sicily, the multi-party electoral system applies to municipalities with a lower population threshold (10,000 rather 15,000 inhabitants). In Sicily, the sample includes some slightly smaller municipalities (specifically 19 municipalities with more than 10,000 and less than 15,000 inhabitants).

[20]Elections can take place in two rounds. A candidate that obtains more than 50% votes in the first round is elected mayor. Otherwise, the two candidates with the largest vote shares in the first round compete in a runoff election to select the mayor and the associated ruling coalition. The allocation of seats in the council is based on the votes that each party has obtained in the first round of voting.

to the legislative majority. A super-majority rule ensures that the coalition that supported the winning mayor is allocated at least 60% of seats in the city council (or more if it receives larger shares of votes), thereby becoming the ruling coalition in the city council. Cabinet members, as ministers in national governments, are in charge of policy design and implementation: they draft the yearly budget, set local taxes on properties, fees for public services, and allocate funds to different chapters of public expenditure. The elected mayor forms the local government by proposing a list of cabinet members to the council and a broad program for a vote of confidence.[21] In short, the cabinet's formation is a critical juncture for the mayor and the parties.

3.2 Randomization of order of parties in the ballot papers

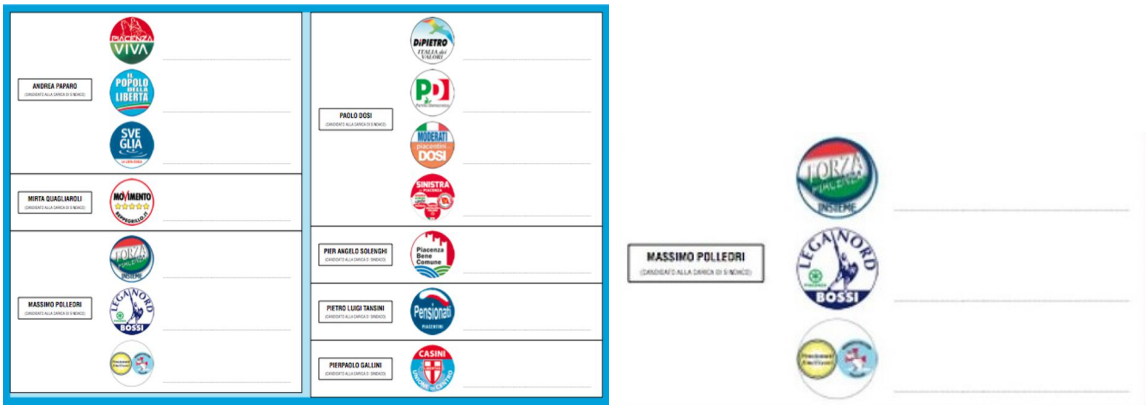
The ballot papers display multiple graphical "blocks", one for each running coalition supporting a given candidate for mayor. The symbols of parties are presented within each graphical block as a vertical list located on the right-hand side of the name of each candidate for mayor. The name of the candidate for mayor is vertically centered in the middle of the list of parties of the coalition. Both the order of blocks within the ballot and the order of the parties within each graphical block are randomized.[22] For any given set of running coalitions and parties, in a municipality and election year, the lottery pins down a unique graphical structure of the ballot paper distributed to all voters on the election day.

The voting procedure can be illustrated with Figure 1(a), which depicts a facsimile of a voting ballot, and Figure 1(b) which focuses on the lower-left block of the facsimile. Voters can either mark a cross to the left of the "Lega Nord" party symbol, corresponding to the name of the candidate for mayor, or somewhere to its right in correspondence to a party symbol. In both cases, the vote is valid but with very different implications for its allocation to parties within the running coalition. All votes marked to the left-hand side of the party

[21] A majority among all councilors is needed. Failing to pass the vote of confidence leads to new elections, which is extremely costly for the mayor and the parties. Accordingly, this is a rare event, and the bargaining process can be lengthy and cumbersome. As for national governments, in practice, when the mayor decides to present the cabinet list, the ruling coalition has already agreed on the cabinet's partisan composition and the cabinet members' names.

[22] The running coalitions register at the local electoral office within a deadline. After the deadline has expired and a month before the election, the head of the electoral office performs the random draw in the presence of police officials and one representative of each of the running parties.

Figure 1: A BALLOT PAPER AND COALITION BLOCKS



(a) Facsimile Ballot Paper (b) A Coalition Block

Notes: (a) Example of ballot paper from a first round election (facsimile). The order of the blocks (one for each candidate for mayor and their respective supporting coalitions) and the order of the party symbols within each block are randomized. (b) Votes can be cast both to the left of the party symbol list (corresponding to the name of the candidate for mayor) or to its right (corresponding to the symbol of a party, or both). See text for details.

symbols are intended as votes *for the mayor* and, accordingly, are proportionally distributed to all parties supporting her candidacy. In contrast, if a vote is cast on the right-hand side, it still counts for the coalition (and the candidate for mayor) but it is allocated to the party whose symbol is closest to the cross.[23] From an experimental design perspective, the possibility of casting multiple votes with multiple patterns and various consequences and the graphical design of the ballot paper implies a setup that is fragile in terms of implementation errors. Voters can easily get confused over what they are allowed to do and the implications of different voting patterns. Even educated and comparatively well-informed voters can make mistakes and end up casting a vote for the party on the side of the name of the mayor thereby unintentionally allocating their preference to the party whose symbol is randomly aligned with the name of the mayor.[24] The left-right structure within the blocks on the ballot and the

[23] Voters can also do both (i.e., mark a cross on both the left- and the right-hand side). The vote is valid. The left counts for the mayor, while the vote on the right-hand side is allocated to a specific party. A "panachage" rule allows voters to vote for a given mayor but choose a party belonging to another running coalition. In this case, the vote for the candidate counts in determining the winning mayor, while the vote for the party counts for the distribution of seats inside the coalition. Split votes are rare as documented by the very small differences between the total votes obtained by mayors and by the coalitions supporting them.

[24] For instance, voters can fail to understand that they can cast only one vote to support a mayor and end up marking crosses on both sides. Similarly, voters intending to cast their preference for a given mayor but not interested in expressing a preference for a specific party may mistakenly mark a cross on the right-hand side rather than on the left-hand side.

vertically centered alignment also implies that the name of the mayor can work as a focal point even for voters who are well informed on the electoral procedures. For instance, votes that are (close to) indifferent between different parties within the same coalition but willing to express their vote may end up voting for the party on the focal point.

Ballot-order effects have been linked to the limited time available for deciding on the voting booth that, in a context of bounded rationality, may favor heuristic behavior (Kahneman and Frederick, 2002). The complexity of the voting system and the electoral law also make it costly to learn the rules accurately.[25] In our context, the cognitive reference, or focal point (Rosch, 1975) is the position of the name of the candidate for mayor and the associated horizontally aligned party symbol. As discussed above, the possibility to vote on both the left and the right of the name of the mayor can also induce mistakes.

3.3 Randomization of Votes within Running Coalitions

We collect information on all ballot papers issued in all municipal elections held between 2002 and 2012. Local elections in Italy are staggered and are held, in some municipalities, every calendar year.[26] Information on the votes (received by each candidate for mayor and those allocated to each party) and the number of seats allocated to each party in municipal elections is extracted from the official repository made available by the Ministry.[27]

Figure 2 presents the average vote share allocated to parties by their order in the list of party symbols reported on the ballot paper.[28] Running coalitions are set before the elections.

[25]The literature also suggests that in complex settings, choosing to devote limited attention and following behavioral patterns can minimize the likelihood of making worse mistakes (e.g., invalidating the vote). As analyzed by Van Damme and Weibull (2002) and Gul et al. (2014) and, in the context of trembling hand equilibria, by Selten (1988) and Pearce (1984), when making decisions in complex settings, individuals may endogenously select their attention level since they are aware of the difficulty of avoiding errors and want to minimize the negative impact of mistakes. In this respect, voting for the party in the focal point can be perceived as a safe strategy since the vote for the mayoral candidate will be valid but it will be allocated to a specific party within the coalition.

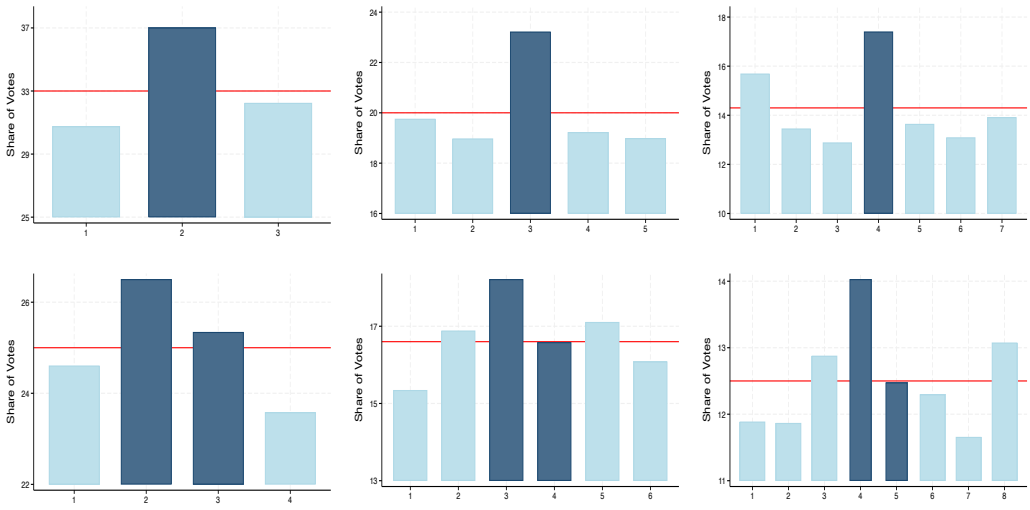
[26]The data are kindly made available by the Italian Ministry of Internal Affairs for the full period (except 2003, for which raw information on the ballot papers is not systematically available in the archives). See Cervellati (2024a).

[27]Data can be directly accessed online at: http://elezionistorico.interno.it/, Eligendo (2024). Variable description and data sources are reported in Tables V1 and V2, while summary statistics are presented in Tables A23 and A24.

[28]We focus on coalitions with at least three running parties. For coalitions formed by two parties, there is no obvious focal point identifying a "treated" party, and the control group is arbitrary. For coalitions with at least

Absent any ballot order effect, the average vote share within a coalition obtained by a party should not depend on the order of the parties in the ballot. For a coalition of N parties, the average vote share of a party listed in any of the positions (e.g., the first) should approach $1/N$ given the randomization of the order of parties on the ballot and the law of large numbers. Figure 2 nonetheless shows that parties that are randomly located in the focal position, that is, to the right of the name of the respective mayoral candidate, systematically receive a higher share of votes. For instance, for coalitions with three parties, the party ordered second is located precisely to the right of the mayoral candidate's name and receives 4 percentage points higher vote share than the average. Boosts in votes for the parties with a favorable position on the ballot can be detected for coalitions of any size.[29]

Figure 2: SHARE OF VOTES BY ORDER OF PARTY ON THE BALLOT PAPER



Notes: The graphs report the average share of votes within running coalitions as a function of the order of the party symbol in the list of parties in the same running coalition; that is, the order in the assigned graphical block of the ballot paper associated to each respective mayoral candidate. Graphs refer to coalitions with a number of running parties between 3 to 8. The dark bars indicate the party in the focal position (i.e., aligned with the name of the mayoral candidate). For coalitions with an even number of parties (reported in the second row), the dark bars indicate the two parties closest to the name of the mayoral candidate. The horizontal red line represents the theoretical share of votes each party would receive absent any ballot-order effect. The graphs report average vote shares for about 13,500 observations for parties running in a given municipality and election year.

three parties, there is always a well-defined control group and a nonempty set of parties that are not treated. The number of coalitions with less than three parties is small, and the results are not sensitive to their inclusion.

[29] A unique focal position is detectable for coalitions with an odd number of running parties. In coalitions with an even number, both parties located to the right of the mayoral candidate's name tend to receive a boost in votes, with the party located just above gaining slightly more. This reinforces the view that the behavioral focal point is due to the alignment of parties with the names of candidates for mayor.

The impact of the treatment on the vote share within coalitions is estimated by comparing treated and non-treated parties in specifications as

$$Y_{i,c,e,m} = \beta_0 + \beta_1 T_{i,c,e,m} + \delta_4 F_{c,e,m} + \alpha_e + \delta_1 X_m + \delta_2 V_{e,m} + \delta_3 M_{c,e,m} + \varepsilon_{i,c,e,m} \qquad \text{(E1)}$$

where $Y_{i,c,e,m}$ denotes the share of votes of party i, belonging to coalition c, running during election e, in municipality m, while $T_{i,c,e,m}$ is a dummy equal to one if that party is treated, i.e., it is randomly placed in the focal position, and zero otherwise.[30] We include fixed effects for the number of parties in the running coalition, denoted by $F_{c,e,m}$, since the probability of being treated and the magnitude of the re-allocation of votes, depend on the number of parties running in each coalition. Accordingly, in the regression analysis, we must control for the number of parties in each running coalition. X_m denotes time-invariant geographical characteristics of the municipality, $V_{e,m}$ includes electoral outcomes in election year e in municipality m while the characteristics of the candidate running for mayor supported by a running coalition c in election year e in municipality m are denoted by $M_{c,e,m}$.

The results in Table 1 are obtained by estimating the specification (E1).[31] The largest sample includes all parties that participated in all the elections held over the period 2002-2012 (except 2003 for lack of data) in running coalitions with at least three parties and amount to around 13,500 observations covering 1,200 elections in around 600 municipalities.

The findings document the randomization of the order of parties within running coalitions together with the existence of a behavioral focal point implying that over the decade 2002-2012, a non-negligible share of votes was randomly re-allocated across parties supporting the same candidate for mayor. Running parties randomly allocated to the focal point enjoy an average increase in vote shares of around 2.5 percentage points (corresponding to an increase of 14 percent of the average vote share of the control group).[32]

[30] In coalitions with an odd number of parties the treated party is that to the immediate right of the mayoral candidate's name. To be conservative, in the regression analysis reported below, we code as focal both parties near the mayoral candidate's name for coalitions with an even number of parties. Results are very similar when coding as focal only the party listed just above to the right of the candidate's name.

[31] Table A1 in the online appendix replicates Table 1 by including the municipality fixed effect in the regression, and the results are almost identical.

[32] In Table 1 the number of municipalities in "ruling coalitions" is lower than the number of municipalities in "any coalition", because there are municipalities where winning coalitions include only one or two parties and non-winning coalitions include more than two parties. These municipalities are counted in the sample "any"

Table 1: A BALLOT ORDER EFFECT (ALL PARTIES)

Dep. Variable:	SHARE OF VOTES W/I COALITION								
Coalition:	ANY			RULING			NON-RULING		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
TREATED PARTY	2.514***	2.513***	2.513***	2.392***	2.392***	2.391***	2.624***	2.624***	2.624***
	(0.402)	(0.402)	(0.402)	(0.563)	(0.564)	(0.565)	(0.542)	(0.542)	(0.543)
# Running Parties FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Years FE	✗	✓	✓	✗	✓	✓	✗	✓	✓
All Covariates	✗	✗	✓	✗	✗	✓	✗	✗	✓
Mean Dependent	18.03	18.03	18.03	16.21	16.21	16.21	19.98	19.98	19.98
Observations	13,564	13,564	13,564	6,790	6,790	6,790	6,774	6,774	6,774
N. Elections	1,209	1,209	1,209	1,160	1,160	1,160	1,001	1,001	1,001
N. Municipalities	605	605	605	589	589	589	550	550	550
R-Square	0.16	0.16	0.16	0.13	0.13	0.13	0.16	0.16	0.16

Notes: The dependent variable is the party share of votes within coalitions. See Table V1 for details and Table A24 for summary statistics. Treated Party is a dummy variable equal to one if the party is in the focal point on the ballot paper and zero otherwise. The sample includes coalitions with more than two running parties in municipalities with more than 15,000 inhabitants in the period 2002-2012 (see text for details). Mean Dependent is the average of the dependent variable for the control group. Description of covariates, data sources, and summary statistics are reported in Tables V2 and A23. OLS regressions with robust standard errors clustered at the legislature level. *, **, and *** denote significance at 10%, 5%, and 1%, respectively.

Discussion. The treatment results from an independent random draw of the position of party symbols in all coalitions within any given municipality and in a given election. This implies that, by design, the lotteries are independent across coalitions, municipalities, and election years. Accordingly, for large enough samples as the one reported in Table 1, adding fixed effects and covariates should not alter the estimated impact of the treatment and should not add explanatory power since they are orthogonal to the random draw. The findings consistently show that the impact of the treatment on the votes of a party within coalitions and the R-square of the regression does not change when adding fixed effects and covariates that include the characteristics of the municipality, socio-economic characteristics of the candidate for mayor, and election-specific information.[33]

but not counted in the sample "winning". The same reasoning explains why the number of municipalities in "non-ruling coalitions" is lower than the number of municipalities in "any coalitions".

[33] *Geographical* characteristics include municipal area (in square km), population size, and level of urbanization and location in terms of distance from water bodies (from the sea, rivers, and the presence of any watercourse, or whether the city is on the coast), altitude and the share of mountains, which can be informative on remoteness, as well as account for the level of seismicity of the municipality, see Istat (2024). Running *mayors* characteristics include age, gender, level of schooling, and the profession (employee or professional), see Italian Public Administrators Archive (2024). Characteristics of each *election* include electoral turnout, the total share of votes of the mayor, whether the mayor was elected in a run-off election, and the existence of alliances. To account for the size of political constituencies, we control for the number of citizens entitled to vote and the size of the city council (in terms of the total number of seats). Finally, we include a dummy for

The results do not display any trend across time, suggesting the absence of learning effects of voters or parties (see Figure A1). The random reshuffling of votes does not systematically differ depending on the characteristics of the municipality or the size and features of the population (education and literacy rates, proxies of social capital, etc.).[34] The impact of the treatment on the reshuffling of votes within running coalitions is not significantly different for coalitions that support either elected or non-elected candidates labeled "Ruling" and "Non-Ruling" coalitions, respectively. The random reshuffling of votes does not systematically differ depending on the features of the candidate for mayor, including whether she ran for a second term, and the votes obtained by the mayor or the voting turnout, past and present (see Figure A2).[35] The results show that the random treatment has a very stable impact within running coalitions. The stability of the treatment estimates suggests that both systematic mistakes induced by the complexity of the voting rules and the existence of a behavioral focal point for quasi-indifferent voters can be at play.

4 Random Votes to Parties: Impact on Policies

In the decade under consideration, the Italian political landscape was characterized by four main blocs of national parties that also ran at the local level, labeled here as *Left*, *Center-Left*, *Center-Right,* and *"Populist Right,"* the latter referring to the "Lega Nord" party.[36] The electoral importance of these parties implies that they have a comparatively large presence in local ruling coalitions. The main national parties alone accounted for about 70 percent of the votes in local elections.[37]

the very small number of cases where, due to split votes, the coalition receiving the majority of votes does not coincide with the winning mayor.

[34]Learning is usually associated with an initial pattern of mistakes in voting, while education and literacy rates affect information on voting procedures. Data on education and literacy rates are made available in Istat (2012), sources for the data on social capital are Cartocci (2007) and Nannicini et al. (2013).

[35]Ruling coalitions got more votes, so their supporting parties are likely to be more popular. The same reasoning applies to parties supporting second-term mayors and mayors with a large vote base, or elections with a large turnout.

[36]Besides the main blocks of national parties, municipal elections involve the participation of some minor parties that run in few scattered municipalities and of so-called "civic lists" and that often only run in a given municipality and for a specific election round. These lists are typically the result of local political initiatives and are often created to support a mayoral candidate.

[37]Figure A3 reports the geographic distribution of the observations in the estimation samples. Ruling coalitions including the main parties are evenly distributed throughout the Italian territory except for the populist

4.1 Treating Main Parties: Verifying Randomization.

As a preliminary step, we present the checks on the randomization of votes that will enable us to interpret the findings on the treatment of each party reported below as causal.

Randomness of the treatment of each party and balance tests. Table 2 shows that none of the main parties has a significantly different probability of being treated compared to any other party running in the same coalition.[38] Figure 3 shows that treated and non-treated parties are not systematically different in any observable dimension. The figure reports the coefficient estimates of regressions that condition on the number of running parties fixed effect, as in equation (E1), pooling observations including any of the main parties (see also Tables A2-A5 reporting over 160 balancing tests for the main parties).[39] Further results document that also past outcomes are balanced.[40]

Random Votes: Effect of the Treatment. To estimate the causal impact of randomly boosting the votes of a given party, we need to restrict attention to the sub-sample of ruling coalitions that include each of the main parties. Main parties have a significant presence in local ruling coalitions, which allows running regressions with a total of around 1,600 observations in the pooled sample of ruling coalitions that include any of the main parties. We

right which was part of ruling coalitions mostly in the center-north of the country.

[38] We estimate specifications such as $T_{i,c,e,m} = \alpha_1 P_{i,c,e,m} + \alpha_{i,e} + \delta_1 X_m + \delta_2 V_{e,m} + \delta_3 M_{e,m} + \delta_4 F_{c,e,m} + \varepsilon_{i,c,e,m}$ where $T_{i,c,e,m}$ is a dummy equal to one if the party's symbol is in the focal point, and 0 otherwise, while the variable $P_{i,c,e,m}$ is a dummy equal to 1 if the focal party is a specific party (e.g., the left), and 0 otherwise. We run unconditional and fully conditioned specifications. For comparability to the regressions run in the sub-samples of coalitions, when pooling observations for coalitions including any of the main parties in columns (1) and (2), we also condition on a set of coalition and electoral year FE, $\alpha_{i,e}$. The number of parties in the running coalition, denoted by $F_{c,e,m}$ is included since the probability of being treated depends on the number of parties in each coalition. Coefficients are never significantly different from zero, and their magnitude is very small compared to the unconditional probability of being treated, ranging from 0.25 to 0.3.

[39] In Figure 3, statistical significance at 5 and 10 percent levels are depicted in green and red, respectively. For easy interpretation of the magnitudes of the differences of standardized means, the horizontal lines depict a conservative threshold of 0.2 standard deviations, as suggested by Imbens and Rubin (2015). The result documents that around 5 and 10 percent of tests are significantly different from zero at the respective levels of significance, with no systematic patterns on any specific variable, and the point estimates largely lie within 0.2 standard deviation bounds.

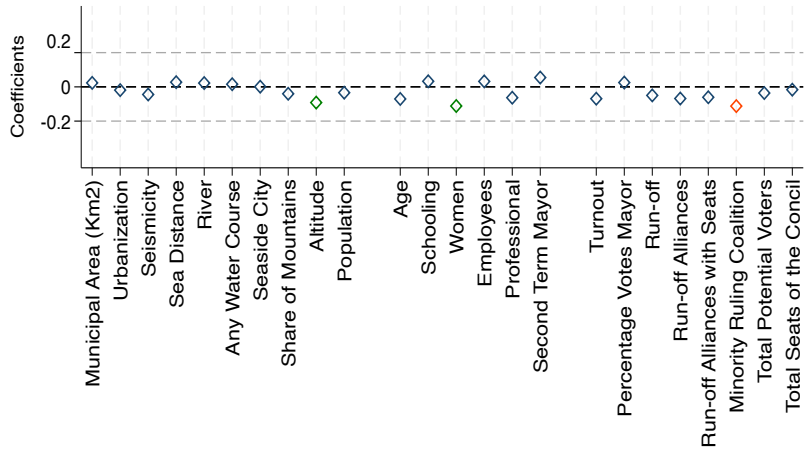
[40] In Table A6 we also report balance tests on lagged fiscal policies. Table A7 further shows that the party share of votes in the previous election, the number of seats in the previous legislature, and the probability of being in the ruling coalition during the last term are well balanced between treated and control group.

Table 2: PROBABILITY OF BEING TREATED

Dependent Variable:	BEING TREATED WITHIN RUNNING COALITION									
Party Treated	MAIN PARTY		LEFT		CENTER-LEFT		CENTER-RIGHT		POPULIST R.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PARTY OF INTEREST	-0.001	-0.001	-0.005	-0.005	-0.011	-0.011	0.009	0.009	0.010	0.010
	(0.009)	(0.009)	(0.016)	(0.016)	(0.017)	(0.017)	(0.018)	(0.018)	(0.035)	(0.035)
# Running Parties FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Years FE	✗	✗	✓	✓	✓	✓	✓	✓	✓	✓
Coal. FE × Years FE	✓	✓	✗	✗	✗	✗	✗	✗	✗	✗
All Covariates	✗	✓	✗	✓	✗	✓	✗	✓	✗	✓
Mean Dependent	0.27	0.27	0.25	0.25	0.26	0.26	0.28	0.28	0.30	0.30
Observations	16,958	16,958	4,813	4,813	5,543	5,543	5,057	5,057	1,545	1,545
N. Elections	1,192	1,192	762	762	958	958	957	957	318	318
N. Municipalities	600	600	463	463	541	541	543	543	220	220
R-Square	0.07	0.07	0.07	0.07	0.07	0.07	0.08	0.08	0.07	0.07

Notes: The dependent variable is a dummy variable equal to one if each party is in the focal point on the ballot paper and zero otherwise. The estimation sample in each column includes all parties in all running coalitions that include the party of interest. Mean Dependent is the average of the dependent variable for the control group. Description of covariates, data sources, and summary statistics are reported in Tables V2 and A23. OLS regressions with robust standard errors in parenthesis. *, **, and *** denote significance levels at 10%, 5%, and 1%, respectively.

Figure 3: BALANCE TESTS



Notes: Diamonds portray the coefficient estimates of regressions that condition on the number of running parties fixed effect, as in equation (E1), restricting attention to the subset of ruling coalitions, including the main parties (Column 1 & 2 of Table 3). Red and green coefficients are significantly different from zero at the 5% and 10% levels, respectively. The horizontal dashed lines indicate the threshold of 0.2 standard deviations. Variable descriptions and data sources are reported in Tables V2. All unconditional and conditional estimates, separately for each main party, are reported in Tables A2-A5, in the online appendix.

also run regressions for split samples for each of the main parties. This involves a more sizable reduction in sample sizes with a maximum of 602 observations for the biggest parties and a minimum of 151 for ruling majorities including the populist right party. Table 3 reports estimates of the treatment on votes shares restricting attention to the subset of ruling coalitions including the main parties. Results for the sample of all coalitions including any of the main parties confirm that the treatment induces an average reshuffling of votes within coalitions of 3.2 percentage points. The Table reports the estimates for the smaller subsets of coalitions including each of the main parties. Samples are substantially smaller and estimates are less precise, ranging from 2 to 5 percentage points. Relative to the mean in the untreated group, the impact on vote shares is relatively larger for the smaller parties.[41]

Table 3: VOTES RANDOMIZATION: MAIN PARTIES

Dep. Variable:	SHARE OF VOTES W/I RULING COALITION									
Ruling Coalition:	MAIN PARTIES		LEFT		CENTER-LEFT		CENTER-RIGHT		POPULIST R.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
TREATED PARTY	3.583***	3.238***	1.878***	1.998***	4.703***	3.949***	4.073***	4.855***	5.825**	4.908**
	(0.646)	(0.625)	(0.572)	(0.582)	(1.280)	(1.105)	(1.435)	(1.386)	(2.435)	(2.470)
# Running Parties FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Years FE	✗	✗	✓	✓	✓	✓	✓	✓	✓	✓
Coal. FE × Years FE	✓	✓	✗	✗	✗	✗	✗	✗	✗	✗
All Covariates	✗	✓	✗	✓	✗	✓	✗	✓	✗	✓
Mean Dependent	32.75	32.75	8.85	8.85	45.60	45.60	42.71	42.71	19.45	19.45
Observations	1,638	1,638	462	462	602	602	423	423	151	151
N. Elections	1,085	1,085	462	462	602	602	423	423	151	151
N. Municipalities	574	574	330	330	404	404	316	316	122	122
R-Square	0.74	0.77	0.23	0.31	0.50	0.67	0.35	0.47	0.41	0.62

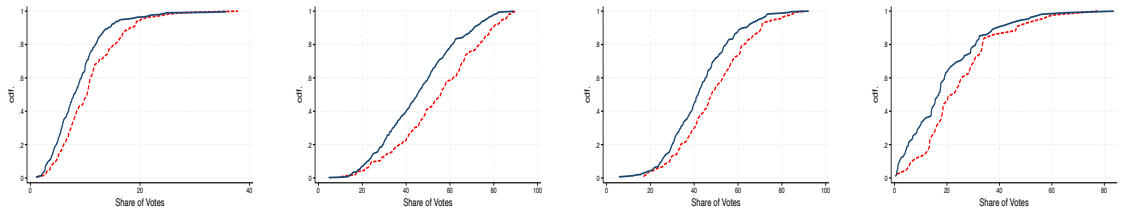
Notes: The dependent variable is the party share of votes within the ruling coalition. See Table V1 for details and Table A24 for summary statistics. Treated Party is a dummy variable equal to one if a given party is in the focal point on the ballot paper and zero otherwise. Samples of ruling coalitions including each of the main parties and with more than two running parties in municipalities with greater than 15,000 inhabitants in the period 2002-2012 (see text for details). Mean Dependent is the average of the dependent variable for the control group. Description of covariates, data sources, and summary statistics are reported in Tables V2 and A23. OLS regressions with robust standard errors in parenthesis. *, **, and *** denote significance levels at 10%, 5%, and 1%, respectively.

As a final test for the validity of the randomization, we check the monotonicity of the treatment. Figure 4 depicts the unconditional effect of the treatment on vote shares. Each esti-

[41] For instance, for the left party, for which the average share of votes within coalitions in the untreated group is 8.9 percent, the estimate of 2 percentage points implies that the random treatment increases the vote share for this party within the coalition on the order of 22 percent. For the populist right, with an average share of votes in the untreated group of 19.5 percent, the relative effect is similar, on the order of 25 percent. For the biggest parties – the center-left and the center-right (with 46 and 43 percent share of votes within ruling coalitions in the untreated group) – the relative effect is lower in magnitude but still sizable and around 10 percent.

mation sample, including each of the main parties, plots the cumulative distribution function, cdf, of vote shares for the same party when treated and non-treated. The figures document two interesting facts. First, the treatment is monotonic, and the vote share of treated parties first-order stochastically dominates that of non-treated parties. Second, the probability of being treated does not depend on the party size, even within the subsamples of coalitions that contain each main party.

Figure 4: IMPACT OF TREATMENT ON VOTES W/I RULING COALITIONS



(a) LEFT (b) CENTER-LEFT (c) CENTER-RIGHT (d) POPULIST RIGHT

Notes: Each graph reports the cumulative distribution function (cdf) of the shares of votes obtained by each party when treated (red line) and when not treated (black line). Each subsample includes all ruling coalitions containing a given party.

A further finding presented in Section 5 is worth mentioning already at this stage. The evidence shows that the treatment does not affect the probability that a running coalition wins the election, implying that the randomization does not induce sample selection in this setting. In other words, the treatment does not affect the probability that a treated party is part of a majority coalition. The results are consistent with the existence of "horizontal adjacency" effects within, but not across, the graphical blocks of running coalitions.

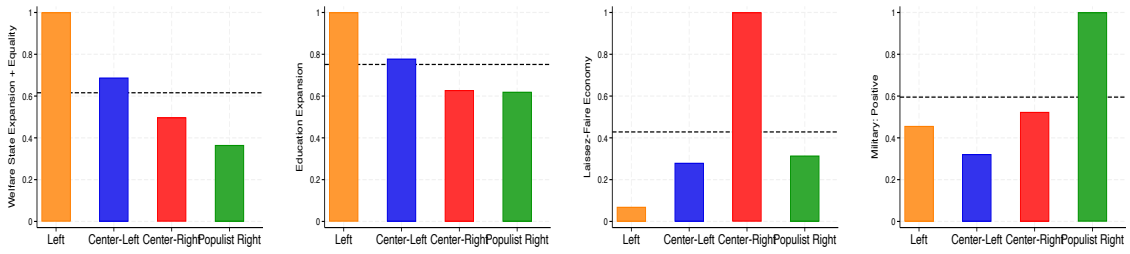
4.2 Electoral Platforms and Policy Issues

Political Manifesto. We code the electoral platform over multiple policy issues by exploiting information from political manifestos at the national level. We look at mentions of various issues extracted from the database of the party manifesto by Volkens et al. (2018).[42] The first, labeled *Welfare*, refers to whether a party's electoral manifesto favorably mentions

[42]The electoral manifesto at the country level offers the best representation of each party's electoral identity in terms of broad ideological location across different issues in the policy space. These are conceptually unrelated to idiosyncrasies driven by unobserved local conditions. Details are reported in the online appendix.

the need to protect underprivileged social groups and implement a fair distribution of resources, and support the expansion of the welfare state and public social services.[43] The second dimension refers to mentions of the need for *Education* expansion at all levels (i.e., not specifically for underprivileged social groups). The third, defined as *Free Market*, refers to supportive mentions of the free market, protection of private property rights, and freedom of personal initiative.[44] The final dimension, called *Security*, refers to the need to increase expenditure on safety and defense.[45] The relative positions of each party concerning these broad issues are depicted in Figure 5. To visualize the relative ideological positioning of the different parties, the indices are normalized using the score of the party most favorable towards each respective policy dimension. To visualize the relative mention of each issue for each party compared to the average mention of the same issue by other parties, we depict the average score across all parties in terms of the horizontal line.

Figure 5: MENTION OF ISSUES ON ELECTORAL MANIFESTO



(a) WELFARE (b) EDUCATION (c) FREE MARKET (d) SECURITY

Notes: Information on mentions of each issue from the Comparative Manifestos Project, CMP, by Volkens et. al. (2018). See text and online appendix for details. *Source*: https://manifesto-project.wzb.eu/datasets.

For the left party, welfare and education stand out as the most prominent issues. The mentions of welfare by this party also stand out to other parties, as apparent by the comparison with average mentions of each issue (depicted by the horizontal line). For the center-left party, education and welfare are relatively emphasized even if, compared to other parties,

[43] These services comprise, in particular, health care, child care, elderly care and pensions, and social housing. Note that this dimension does not include education, coded as a separate issue.

[44] This dimension specifically includes favorable references to the laissez-faire economy, the superiority of individual enterprise over state and control systems, private property rights; personal enterprise and initiative; the need for unhampered individual enterprises.

[45] See Table A8 for the detailed description of the four dimensions.

these mentions are essentially in line with the average across parties. Attitudes towards welfare are more divisive than education, with the populist right having significantly fewer favorable mentions of this policy issue also compared to the center-right party. Positions about free markets vary across parties, being by far the most prominent issue for the center-right party. For the populist right security is the most salient issue both relative to other issues and in comparison to the position of other parties on the same policy area. The center-left is lower than the average for mentions of free market and security, even though it mentions free market more than the left.[46] When compared to the average mentions of each issue within a party electoral platform and across parties, the left stands out positively for welfare and education, the center-right for free markets, and the populist rights for security (and negatively for welfare).

Fiscal Policies. Measures of policies corresponding to the issues mentioned in the political manifesto are built by extracting information on different types of expenditure and taxation from the budgets of Italian municipalities. Information is available from the Italian Ministry of Internal Affairs.[47] We look at expenditure chapters related to the policy issues discussed above. In the observation period, current local expenditure in the around 600 municipalities is roughly 750 euros per capita and covers more than 35 million people for around 30 billion euros per year—a substantial share of total local public expenditure in Italy. Around 530 euros per capita are available to cover the different chapters of public expenditure year-by-year (net of the payment of public employees).[48] For *welfare policies*, we look at current

[46]The data on the party's manifesto also contains an ideological index, which is an average of several indexes on a wider set of dimensions. While of no direct interest to our analysis, it is useful for locating each party on a traditional left-right scale. The ideological positions of the left, center-left, and center-right parties follow an intuitive ordering from left to right, with the center-left being, in fact, closer to ideological neutrality than the center-right. In these years, the populist right party pushed an agenda based on federalism and local (fiscal) autonomy. While this is an important aspect of the party's identity over the observation period, it is not studied here. The emphasis on local autonomy also implies that its ideology is less markedly characterized by the nationalistic values typically associated with rightist ideologies. See Figure A4.

[47]Data were scraped by the authors online at https://finanzalocale.interno.gov.it/apps/floc.php/in/cod/4 Cervellati et al. (2024b). Tables V1 report variable descriptions and data sources. For budgetary spending and taxes, the dependent variable is at yearly frequencies within each legislature ruled by a given majority, which lasts around four years on average. As discussed below, the dependent variable is the logarithm of the fiscal item per capita.

[48]Around a third of ordinary current expenditure covers public employee salaries. This expenditure depends on public employment stock and is little affected by short-term changes in political choices.

expenditure on social services, which amounts to 133 euros per capita and 25 percent of the resources available for current expenditure (net of the payment of public salaries). For *education*, we consider total current expenditure on public education (at all levels), which amounts to around 77 euros per capita and absorbs around 15 percent of the resources. For policies relating to *security*, we look at current expenditure on local police and justice services, which amounts to around 50 euros per capita and absorbs around 10 percent of the available resources. These three expenditure chapters jointly absorb more than 50 percent of current resources. Finally, as a proxy for policies that reflect attitudes towards free markets, protection of private property, and limited taxation, we look at the revenues per capita on the municipal real estate *tax* on home properties. In the observation period, this source of local revenues amounts to around 190 euros per capita or 35 percent of the total net current expenditure per capita.[49]

Empirical Specifications. The natural experiment allows identifying the causal effect of randomly increasing the political power of each main party within ruling coalitions. The effect of empowering a specific party within majorities, for instance, the left party, is identified by comparing the cases in which this party is randomly treated to those in which the same party is not treated. The implementation of the identification strategy, therefore, requires restricting attention to the sub-sample of ruling coalitions that include each of the main parties i. In the following, we estimate the causal impact of treating a given party both in intention-to-treat and instrumental variables specifications.

We start by estimating the average treatment effect, ATE, for each of the samples including a given party i when treated and non-treated in intention-to-treat specifications like,

$$Y_{t,e,m} = \beta_0 + \beta_1 T_{e,m} + \alpha_t + \rho_y + \delta_1 X_m + \delta_2 V_{e,m} + \delta_3 M_{e,m} + \delta_4 F_{e,m} + \varepsilon_{t,e,m}, \qquad \text{(E2)}$$

where the dependent variable $Y_{t,e,m}$ is the log of per capita budgetary item in calendar year t, in municipality m where a given party, e.g., the left, is part of the coalition that won election

[49]This tax, labeled ICI (*Imposta Comunale sugli Immobili*), was eliminated by the government headed by Prime Minister Berlusconi (at the time, leader of the center-right party) in 2011 and subsequently replaced by another tax called IMU (*Imposta Municipale Unica*). The results for property tax refer to the period up until 2011 (in the last year of our sample, 2012, the new tax was not yet in place).

in year e. As in the analysis above, the treatment variable, $T_{e,m}$, is a dummy equal to one if a party running in a coalition that won the election e, in municipality m was randomly treated by a favorable ballot position and zero otherwise.[50] The estimation conditions on fixed effects for the number of parties in the running coalition, denoted by $F_{e,m}$ since the probability of being treated depends on the number of parties in each coalition. Conditioning on covariates is customary in natural experiments that estimate treatment effects on a limited number of observations since it allows to increase the precision of estimates, even if they are not required to estimate effects consistently (e.g., Angrist and Pischke, 2009, p. 2018). To this end, we include geographical characteristics of the municipality, X_m, time-varying electoral outcomes, $V_{e,m}$, and the characteristics of the elected mayors, $M_{e,m}$. Finally, year fixed effects α_t are included to control for time-varying shocks common to all the municipalities and year of the legislature fixed effect ρ_y to control for, within-terms, budget cycles.
We also estimate equivalent pooled specifications like,

$$Y_{i,t,e,m} = \beta_0 + \beta_1 T_{i,e,m} + \alpha_{i,t} + \rho_y + \delta_1 X_m + \delta_2 V_{e,m} + \delta_3 M_{e,m} + \delta_4 F_{e,m} + \varepsilon_{i,t,e,m} \quad \text{(E3)}$$

where the dependent variable $Y_{i,t,e,m}$ denotes the log per capita budgetary item on the salient policy area of each party i (i.e., the spending on the issue with the highest positive mentions in the electoral platform as discussed above).[51] For comparability with specification (E2), and to account for possible time-varying effects that are specific to ruling coalitions for each of the main parties, we include fixed effects accounting for specificities for each coalition and year, $\alpha_{i,t}$. Besides allowing improved statistical power, the pooled specification (E3) allows, as done in Tables 1 and 3, to estimate a unique average treatment effect for all parties.

Ultimately, we are interested in isolating and quantifying the causal effect of increasing the vote share of a party within coalitions. To this end, we can run IV regressions of the votes

[50]The effect of treating a given party should be interpreted as resulting from empowering this specific party relative to the other parties in the same running coalition that end up being represented in the city council. Recall that running coalitions are typically formed by several parties, often including civic lists and in some cases more than one main national party (e.g., coalitions including both the left and the center-left or the center-right and populist right.

[51]This implies restricting attention to spending on welfare, education, and security when estimating the effect of treating the left party, center-right, and populist right parties, respectively, and to the *reduction* of property taxes when estimating the treatment effect for the center-right.

shares instrumented with the random treatment (a favorable ballot position). The results reported above already document that the treatment is a valid instrument. The institutional features of the natural experiment and the evidence presented above also suggest that the only plausible way the ballot position can affect outcomes is through their impact on vote shares within winning coalitions and that the exclusion restriction should be valid.[52] Conceptually, the estimates of the first stage regression are obtained in specifications like (E3) where the dependent variable is the shares of votes within coalitions, e.g. $V_{i,e,m}$, and the treatment is $T_{i,e,m}$. Local average treatment effects, LATE, in terms of the second stage of 2SLS regressions are estimated in specifications like,

$$Y_{i,t,e,m} = \beta_0 + \beta_1 \tilde{V}_{i,e,m} + \alpha_{i,t} + \rho_y + \delta_1 X_m + \delta_2 V_{e,m} + \delta_3 M_{e,m} + \delta_4 F_{e,m} + \varepsilon_{i,t,e,m}, \quad \text{(E4)}$$

where the explanatory variable $\tilde{V}_{i,e,m}$ denotes the vote shares within ruling coalitions instrumented by the random treatment.

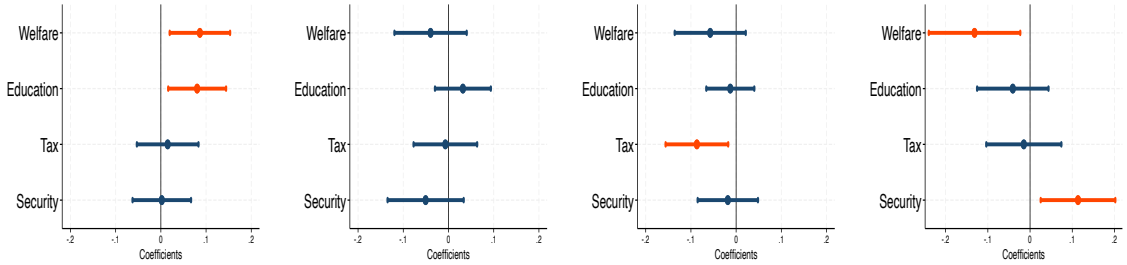
Results. Figure 6 depicts the estimates of the effect of treating each of the main parties on each of the policies implemented by the ruling coalition. The results refer to fully conditioned intention-to-treat specifications like (E2). The dependent variables are the spending in each budgetary item measured in terms of *log of* per capita, so that coefficients can be interpreted as percentage changes.

The results show that the coalition policies shift towards the political agenda of the treated party but only for the policy areas that are clearly salient in each party's electoral platform. Treating the left party increases spending on welfare and education by around 8 percent, respectively. In contrast, treating the center-left party moves policies toward education, but the effect is not statistically significant.[53] Treating the center-right party, comparable to the center-left party in size and magnitude of treatment, reduces taxation of real estate properties by 9 percent. The evidence ligns with the party's emphasis on laissez-faire, protection

[52]This conjecture is supported by further evidence and counterfactual analysis in Section 5. In particular, the analysis shows that treatment only implies a reshuffling of votes within, but not across, running coalitions and that no significant effect on outcomes can be detected by treating parties not belonging to ruling majorities.

[53]The lack of policy effects for the center-left is coherent with the policy mentions in its political manifesto: there is no issue standing out, suggesting that, even when the center-left party is empowered, it does not sway away policies from the ones otherwise chosen by the ruling coalition.

Figure 6: RANDOM VOTES TO PARTIES: IMPACT ON POLICIES



(a) LEFT (b) CENTER-LEFT (c) CENTER-RIGHT (d) POPULIST RIGHT

Notes: The graph reports coefficient estimates of random treatment of each party estimated as in equation (E2) in fully conditioned empirical specifications that account for all fixed effects and covariates. *Dependent variables*: log current expenditure per capita devoted to public social services (welfare); log current expenditure per capita devoted to public education (education); log revenues per capita from the real estate tax on home properties (Tax); log current expenditure per capita devoted to local police and justice services (security). *Treated Party* is a dummy variable equal to one if the party is randomly located in the focal point on the ballot paper and zero otherwise. *Sample*: the different sub-figures report the estimates using the sample of all ruling coalitions including the respective party when treated and non treated. Coefficients in red (green) are significant at the 5% (10%) levels, respectively. The bars illustrate confidence intervals at a limit (10%) significance level.

of private property, and reduction in taxation. Finally, treating the populist right party increases security expenditure by around 11 percent and reduces welfare spending by around 13 percent.[54]

Results for pooled regressions obtained estimating specification (E3) reported in Table 4 columns (1) and (3) in unconditional and fully conditioned regressions, show that treating a party changes the fiscal policy that is salient for the treated party by 7.9 and 5.8 percent, respectively.[55] First-stage estimates are reported in columns (4-6). Finally, the IV results estimating specification (E4), reported in columns (7-9), show that an increase of 1 percentage point in the share of votes within coalitions changes the fiscal policy by 2 percent.[56] The results can also be interpreted in terms of elasticity by multiplying the coefficient of the second

[54]Looking at realized differences in vote shares across parties would not allow isolating the causal effect of the role of a party for coalition policies. Results obtained by regressing the policies implemented by the coalition on the share of votes allocated to a given party are at odds with each party's political manifesto, and no effects can be detected for small parties. See Figure A6.

[55]We also replicate analysis by excluding the center-left party since the estimated impact on policies is lower in magnitude and not statistically significant. In line with the estimated effects reported in Figure 6, the results show that the average treatment effect in full conditioned specifications is larger and in the order of about 10 percent. See Table A9. Table A10 confirms the findings with Weak-IV 95 percent Anderson-Rubin (AR) confidence sets which are calculated using the two-step approach of Andrews (2018) using the Stata package from Sun (2018).

[56]We have checked the robustness of the results by estimating confidence intervals using permutations based on Monte Carlo simulations and non-parametric bootstrap estimations. See Figures A7 and A8.

stage by the mean of the party share of votes. The elasticity of fiscal policy to a party share of votes within coalition is 0.7, suggesting that the percentage change in party vote share, induced by our natural experiment, translates into a roughly one-to-one percentage change in policies.[57]

Table 4: IMPACT OF VOTES TO PARTIES ON POLICY

Dep. Variable:	FISCAL POLICY			SHARE OF VOTES			FISCAL POLICY		
	ITT	ITT	ITT				IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
TREATED PARTY	0.079***	0.075***	0.058**	3.508***	3.508***	2.879***			
	(0.027)	(0.027)	(0.025)	(0.839)	(0.839)	(0.810)			
SH. OF VOTES W/I COALITION							0.021***	0.021**	0.020**
							(0.008)	(0.009)	(0.010)
# Running Parties FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Coal. FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Coal. FE × Years FE	✗	✓	✓	✗	✓	✓	✗	✓	✓
Legislative Years FE	✗	✓	✓	✗	✓	✓	✗	✓	✓
All Covariates	✗	✗	✓	✗	✗	✓	✗	✗	✓
F-statistic Instrument				17.47	17.47	12.62			
Observations	6,598	6,598	6,598	6,598	6,598	6,598	6,598	6,598	6,598
N. Elections	1,056	1,056	1,056	1,056	1,056	1,056	1,056	1,056	1,056
N. Municipalities	560	560	560	560	560	560	560	560	560

Notes: The dependent variable is the log of per capita budgetary item on the salient policy area of each party (see text for details). See Table V1 for details and Table A26 for summary statistics. Treated Party is a dummy variable equal to one if a given party is in the focal point on the ballot paper and zero otherwise. Share of votes w/i coalition is the party share of votes within the ruling coalition. Samples of ruling coalitions including each of the main parties and with more than two running parties in municipalities with greater than 15,000 inhabitants in the period 2002-2012 (see text for details). Mean Dependent is the average of the dependent variable for the control group. Description of covariates, data sources, and summary statistics are reported in Tables V2 and A23. OLS regressions with robust standard errors clustered at the municipality level in parenthesis. *, **, and *** denote significance levels at 10%, 5%, and 1%, respectively.

5 Mechanisms

The analysis in Section 4 isolates and quantifies the impact of random perturbations of votes within ruling coalitions. Treating parties with votes shifts coalition policies towards the salient policy areas for the treated party. The policy-making power of a party, and its political

[57]The mean of the party share of votes in pooled specifications is around 33 percent. While more noisy and featuring weaker first stages, these magnitudes are confirmed by running IV in each separate sample. The results of the first and second stages are presented in Tables A11 and A12, respectively. An increase by 1 pp of the vote share of each party implies the following change in fiscal policy: 4.3 percent for welfare and 4 percent for education (left party); 2.2 percent lower tax revenues (center-right party); 2 percent security and minus 2.3 percent welfare (populist-right); 0.7 percent education (center-left party). All elasticities are around or below 1 (in absolute value).

credibility towards voters, ultimately rests on its factual capability to impact the partisan composition of the government, and the selection and control of appointed cabinet members. This section explores these mechanisms.

As discussed in Sections 1 and 2, the theoretical literature on multi-party governments delivers contrasting predictions on whether and how votes to a party could be expected to impact coalition policies, and existing causal evidence has limited attention to legislative representation, by exploiting marginal perturbations of seat allocation to parties within councils. While votes are mechanically mapped into seats applying electoral procedures, the partisan representation within the cabinet is not driven by any formal rule since cabinet members are appointed. The problem of government formation is conceptually nontrivial for parties within coalitions. Parties with higher bargaining power should gain higher presence in the governments (e.g., Laver and Shepsle, 1990, 1994) but partisan appointments do not eliminate the problem of cross delegation and control across parties (e.g., Thies, 2001) and the fact that cabinet members need to coordinate to adopt one common policy on each issue (Martin and Vanberg, 2014). Taken together, the ability of a party to impact policies depends on whether the policy preferences and the behavior of appointed cabinet members align with its electoral constituency and platform. Empirically, the process of government formation can be cumbersome and fairly noisy and can be affected, among others, by the strength and ideology of the mayor (vis-à-vis the majority parties). Furthermore, cabinet members are likely selected also because of unobservable features like, e.g., their personal or professional ties to the mayor or councilors, or their latent skills or expertise. We still lack any causal investigation of the chain of mechanisms through which votes impact the bargaining power of parties within ruling majorities and of whether, and how, the latter affects the formation of the executive.

To unfold the chain of mechanisms, we collect data on local politicians from multiple sources. Information on seats in councils is available from the official statistics from the Ministry of Internal Affairs' *Register of Local Politicians*. The same source is used to recover information on the socio-demographic characteristics of the universe of cabinet members over the observation period (between 2002 and 2012). For the elections in our sample, this amounts to about 10,000 individuals. The political affiliation of councilors results directly

from the application of electoral rules on the list of candidates and is therefore available without noise for all parties. Cabinet members' political affiliation is, instead, self-reported and transmitted to the Ministry of Interiors by the electoral offices of each municipality after the appointment by the mayor. Hence the *register* contains only partial information on cabinet members' political affiliation. In about 13% of elections, the electoral office of the municipality did not submit to the ministry any information on cabinet members' affiliation. We cross-check official statistics by scraping information available online from the non-official repository of information on politicians "Openpolis".[58] These checks suggest that it is not uncommon that cabinet members are not affiliated with any party and have instead been appointed as technocrats with specific expertise or ties to the mayor. To limit the impact of missing data, we restrict attention to the sample of elections in which information on any political affiliation is available for at least 20% of cabinet members. We code a cabinet member as non-politically affiliated if no information on her affiliation is available from official or non-official records.[59] This delivers a baseline sample of about 8,000 cabinet members of which about 70% are politically affiliated with a party.

Vote shares are transformed into elected councilors by applying the proportional "d'Hondt method". Figure 7 (a) graphically depicts the unconditional correlation between the votes and the seats of a party within ruling coalitions. The figure shows that, while not perfect, the mapping between votes and seats is roughly one-to-one.[60] In contrast, there is no formal automatism and no reason suggesting that the mapping of votes, or seat shares, within majorities should be expected to map one-to-one into representation in the government. Figure

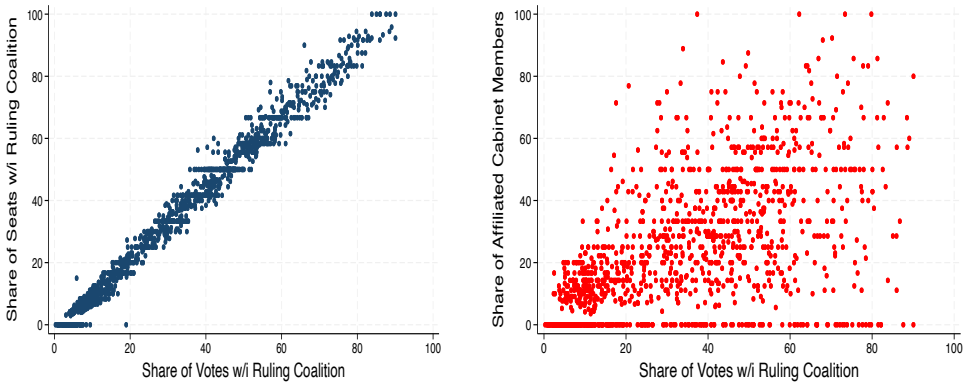
[58]Openpolis is a public foundation that aims at making citizens aware of its representatives and that, among others, collects data on politicians appointed at all levels of government including municipal cabinet members, Politicians Openpolis (2022).

[59]The main reason for truly missing information is the lack of transmission of the data by part of the municipal electoral office. In some cases, the register reports the political affiliation of a few members only because they were already included in the database (e.g., former councilors). For this reason, we set the threshold at 20%, which amounts to not considering, in the baseline sample, cabinets for which the political affiliation is not available for at least 2 out of 8 cabinet members. Results are very similar, however, when retaining cabinets with at least a politically affiliated member.

[60]The allocation of votes and seat shares does not perfectly coincide for marginal vote changes in view of the integer nature of the latter and the difference between the two at the margin has been exploited as a strategy for identification. Fujiwara and Sanz, (2020) identify the role of rank effects in government formation by exploiting the impact of votes shares at the margin for the same number of seat shares. Nonetheless, for non-marginal vote changes, however, the two are essentially proportional as the d'Hondt is the method that maximizes the fraction of exactly proportionally represented votes (see Sainte-Laguë, 1910 and Medzihorsky, 2019).

7 (b) depicts the unconditional correlation between a party vote shares within ruling coalitions and the shares of its affiliated cabinet members over the whole cabinet. While positive, the correlation between vote shares and affiliated cabinet members' shares is loose, and the data display substantial variability. In line with the arguments discussed above, the pattern implies the existence of multiple latent factors that impact the partisan composition of governments above and beyond vote shares and legislative representation. Given these patterns, it is a-priori not obvious that small perturbations of votes should be expected to impact the composition of cabinets significantly. The figure only plots correlations, however. An appealing feature of the natural experiment is that the randomization of votes to parties induced by the ballot treatment is conceptually orthogonal to the endogenous drivers of both vote shares and cabinet appointments and can, therefore, be exploited to investigate the chain of mechanisms behind the reduced form results presented in Section 4.

Figure 7: VOTES TO PARTIES: MAPPING INTO SEATS, AND CABINET MEMBERS



(a) SEATS IN COUNCILS (b) CABINET MEMBERS

Notes: Scatterplots of the correlation between the share of votes received by a party within ruling coalitions and: in panel (a) with the share of seats by the same party within legislative majorities; in panel (b) with the share of cabinet members affiliated with the same party. Samples include ruling coalitions with at least one of the main parties and ruling coalitions for which information on any political affiliation is available for at least 20% percent of cabinet members (see text for details).

5.1 Bargaining Power of Parties within the Legislative

The empirical setup allows us to explicitly test and tell apart the contrasting theoretical predictions on legislative bodies. In line with theories of legislative bargaining, below we doc-

ument that the impact of the treatment is due to an increase in legislative bargaining *within* ruling coalitions. As discussed in Section 2 this channel is considered the most likely in the literature but existing empirical strategies could not isolate the role of increasing votes of a party within ruling coalitions from its potential impact on the formation of ruling majorities.

The institutional setting ensures that the treatment cannot influence coalition formation because the latter occurs before the treatment. As a confirmation of this institutional constraint and, more importantly, a test of the lack of spillovers of the treatment on the votes of the candidate for mayor (discussed in Section 4.1), we study whether the treatment impacts the formation of ruling majorities. To this end, we exploit the extended sample with all running coalitions (winning and non-winning) that include each of the main parties and test if the votes randomization impacts votes only within, but not across, running coalitions.[61] Table 5 columns (1-2) show that randomly treating any of the main parties has no impact on the likelihood that their candidate for mayor wins the election. This implies that shifts in block majorities or the role of treated parties being pivotal in the process of coalition formation do not drive the results. Counterfactual analysis also shows that boosts of votes to parties outside legislative majorities have no impact on policies implying that the mechanism is specifically related to a change of bargaining power within ruling coalitions, rather than an increase in legislative representation *per se* (see Table A13 and Figure A9).[62]

As the treatment does not impact the likelihood that a party is included in the estimation sample of ruling coalitions, we can estimate the causal impact of seats *within* coalitions. Table 5 columns (3-4) report the effect of the treatment on the share of seats within the ruling coalition. The magnitude, around 3.4 pp increase in the share of seats, is comparable to the 3.2 pp increase in the share of votes reported in Table 3.[63]

Figure 8 unfolds the average treatment effect by graphically depicting the impact of the treatment on seat shares plotting again the cumulative distribution function (cdf) of seat

[61] Recall that running coalitions are formed before randomization of ballot order and before the vote. The existence of horizontal adjacency effects associated with the list of parties supporting a mayor, and the particular graphical structure of each block, imply that we should expect the treatment to reshuffle votes within but not across the different coalitions on the ballot paper.

[62] Table A15 in the online appendix replicates columns (1-2) of Table 5 by the main party in the ruling coalition, and the results are very consistent even in different sub-samples.

[63] Table A16 in the online appendix replicates columns (3-4) of Table 5 for each of the main parties. The results are confirmed even in different sub-samples.

Table 5: TREATMENT AND PROBABILITY THAT A COALITION WINS

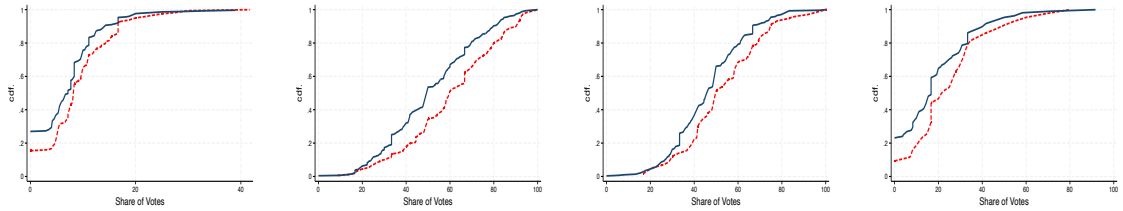
Dep. Variable:	BEING IN A WINNING COALITION		SHARE OF SEATS W/I RULING COALITION	
Ruling Coalition:	MAIN PARTIES		MAIN PARTIES	
	(1)	(2)	(3)	(4)
TREATED PARTY	0.012	0.003	3.820***	3.415***
	(0.020)	(0.014)	(0.759)	(0.735)
# Running Parties FE	✓	✓	✓	✓
Coal. FE × Years FE	✓	✓	✓	✓
All Covariates	✗	✓	✗	✓
Mean Dep.	0.59	0.59	35.65	35.65
Obs.	2,844	2,844	1,638	1,638
N. Elections	1,085	1,085	1,085	1,085
N. Municipalities	574	574	574	574
R-Square	0.14	0.53	0.74	0.76

Notes: The dependent variable is a dummy variable equal to one if the mayoral candidate, running with a coalition of parties that includes the party of interest, wins the election and zero otherwise in columns (1) and (2); the party share of seats within coalitions in columns (3) and (4). Treated Party is a dummy variable equal to one if the party is in the focal point on the ballot paper and zero otherwise. Samples of coalitions including each of the main parties and with more then two running parties in municipalities with greater than 15,000 inhabitants in the period 2002-2012 (see text for details). Mean Dependent is the average of the dependent variable for the control group. Description of covariates, data sources, and summary statistics are reported in Tables V2 and A23. OLS regressions with robust standard errors in parenthesis. *, **, and *** denote significance levels at 10%, 5%, and 1%, respectively.

shares for the same party when treated and non-treated. As in Figure 4, the treatment is monotonic and can be detected for all parties. Interestingly, compared to the cdf of vote shares, the figure highlights that for the smaller parties (left and populist right), the treatment disproportionately affects the probability of having at least some seats in majorities.[64] On average, ruling coalitions are formed by five parties. The results of SUR models that document how the votes gained by the treated party do not exclusively come at the expense of the votes of other main parties (when they rule jointly in the same coalition) but also from other parties of the same coalition (see Table A14). This suggests, roughly speaking, that the impact on policies could be interpreted as moving expenditure away from the (latent) average spending on policies that would be preferred by the rest of the coalition and towards the budgetary issue that is more salient for the treated party.

[64]Treating the parties in the four samples implies relative increases in seat share, with respect to their mean seat share ranging from 8 (center-left) to 32 percent (left). The relative increase in the number of councilors for the bigger parties is around 8 percent, while for the smaller parties is larger and around 31 and 28 percent for the left-wing and the populist right party, respectively.

Figure 8: IMPACT OF TREATMENT ON SEATS WITHIN LEGISLATIVE MAJORITIES



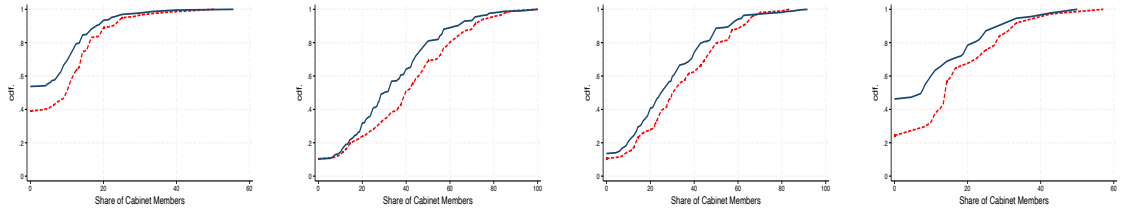
(a) LEFT (b) CENTER-LEFT (c) CENTER-RIGHT (d) POPULIST RIGHT

Notes: Each graph reports the cumulative distribution function (cdf) of the shares of seats in the council obtained by each party when treated (red line) and when not treated (black line).

5.2 Partisan Composition of the Cabinet

Having isolated the impact of the treatment on the size of parties within legislative majorities, we turn our attention to checking whether, and how, the perturbation of bargaining power in the legislative spills over to the the appointment of cabinet members. Figure 9 depicts the impact of the treatment on the share of cabinet members affiliated with any given main party by looking again at the cdf of treated and non-treated. The impact can be detected for all parties and is monotonic. The effect is more than proportional for the smaller parties. To interpret the findings it is important to recall that, on average, cabinets are composed of 10 members (as compared to 15 councilors in ruling majorities) (see Table A25).

Figure 9: CABINET MEMBERS AFFILIATED TO TREATED PARTY



(a) LEFT (b) CENTER-LEFT (c) CENTER-RIGHT (d) POPULIST RIGHT

Notes: Each graph reports the cumulative distribution function (cdf) of the shares of cabinet members obtained by each party when treated (red line) and when not treated (black line).

Table 6 reports results for fully conditioned ITT and IV pooled specifications.[65] The treatment increases the share of affiliated members by 4.7 pp, while an increase of 1 pp

[65]The results hold for the full sample (including municipalities with no affiliated member, see Table A18 and when restricting the sample to municipalities with at least one affiliated member, see Table A19.

in the share of votes of a party within the coalition raises its share of affiliated members by 1.4 pp. The remaining columns show that the treated party gains cabinet members by reducing (roughly in the same proportion) the share of cabinet members affiliated to other parties (columns 3-4) and the share of non politically affiliated cabinet members (columns 5-6). When looking at cabinets affiliated with parties, the negative effect is stronger for the non main national parties (e.g., civic lists) (columns 7-8).[66] Further results show that the treatment does not affect the overall size (i.e. the number of cabinets) or the cabinet stability as measured by the likelihood that cabinet members are replaced during the legislature (see Table A17). The counterfactual analysis also confirms the lack of effect for treated parties within legislative minorities, as no cabinet member is affiliated with these parties in the data.

5.3 Socio-Demographic Features of Cabinet Members

The results so far document that the treatment induces perturbations of ruling majorities and that the resulting change in bargaining power impacts access to policy-making through the appointment of cabinet members affiliated to the treated party. In theory, a main function of parties relates to the (self)selection of like-minded individuals (Caillaud and Tirole, 2002; Snyder and Ting, 2002). Existing evidence documents that individuals with different features have different preferences over public policies (e.g., Page and Shapiro, 2010; Alesina and Giuliano, 2011 among others) thereby linking the observable socio-demographic characteristics are informative on latent policy preferences. Evidence also documents that parties select candidates with different characteristics (Dal Bó et al., 2017 and Dal Bó et al., 2022). We lack any evidence of a causal link between the electoral ideologies of parties and the selection of cabinet members with different features and (latent) preferences. To investigate this channel, we exploit information on gender, age, occupation, and education of cabinet members.[67] We look at the observational features of cabinet members appointed to differ-

[66]Table A20 replicates the results of Table 6 excluding the center-left from the sample, and the results are consistent. Table A21 replicates the results of Table 6 for each of the main paties, and also, in this case, results are confirmed in the different sub-samples.

[67]Summary statistics are reported in Table A27. Information on gender, age, and education degrees requires no explanation. For occupation, we classify the category "employees" as public administrative, private administrative, social services, teachers and professors, and police agents. "Professionals" include lawyers, engineers, architects, doctors, managers, entrepreneurs, and bankers.

Table 6: IMPACT ON CABINET MEMBERS AFFILIATED TO THE TREATED PARTY

Dep. Variable	SHARE OF CABINET MEMBERS (AFFILIATED TO)							
	MAIN PARTY		OTHER PARTIES		NON AFFILIATED		NON NATIONAL PARTIES	
	ITT	IV	ITT	IV	ITT	IV	ITT	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TREATED PARTY	0.047***		-0.025		-0.022		-0.032**	
	(0.012)		(0.016)		(0.016)		(0.016)	
SH. OF VOTES W/I COALITION		0.014***		-0.007		-0.007		-0.010**
		(0.004)		(0.005)		(0.005)		(0.005)
# Running Parties FE	✓	✓	✓	✓	✓	✓	✓	✓
Coal. FE × Years FE	✓	✓	✓	✓	✓	✓	✓	✓
All Covariates	✓	✓	✓	✓	✓	✓	✓	✓
F-statistic Instrument		23.24		23.24		23.24		23.24
Mean Dependent	0.23	0.23	0.46	0.46	0.31	0.31	0.31	0.31
Observations	1,244	1,244	1,244	1,244	1,244	1,244	1,244	1,244
N. Elections	816	816	816	816	816	816	816	816
N. Municipalities	491	491	491	491	491	491	491	491
N. Cabinet Members	8,187	8,187	8,187	8,187	8,187	8,187	8,187	8,187

Notes: The dependent variable is the share of cabinet members affiliated with the party of interest reported in the heading of the columns. See Table V1 for details and Table A28 for summary statistics. Treated Party is a dummy variable equal to one if the party is in the focal point on the ballot paper and zero otherwise. Share of votes w/i coalition is the party share of votes within the ruling coalition instrumented with the treatment. Mean Dependent is the average of the dependent variable for the control group. The sample includes coalitions with more than two running parties in municipalities with more than 15,000 inhabitants in the period 2002-2012 (see text for details). The sample includes coalitions for which we find information for at least 20% of affiliated members (see text for details). Description of covariates, data sources, and summary statistics are reported in Tables V2 and A23. OLS regressions with robust standard errors in parenthesis. *, **, and *** denote significance levels at 10%, 5%, and 1%, respectively.

ent departments. Information on the policy areas is not systematically available in official statistics. We retrieve data on the budgetary responsibility for around 1,800 cabinet members by scraping information from the web. Table 7 links cabinet members' features with the departments assigned to their control.[68]

Welfare policies are disproportionately controlled by older individuals (a difference of 11 pp) and females (a difference of 12 pp); education by females (a difference of 12 pp) and non-professionals (a difference of 11 pp) and younger (a difference of 11 pp); business development by professionals (a difference of 11 pp) and more likely males; security by males (a difference of 10 pp). The level of education of cabinet members has smaller differences

[68]The baseline source of information is "Openpolis" (http://www.openpolis.it). To illustrate the data, we adopt a binary classification and dichotomize age at the retirement age and education by whether the cabinet members hold a university degree (that roughly coincides with a dichotomization at the median year of education).

Table 7: DEPARTMENT OF CABINET MEMBERS: SOCIO-DEMOGRAPHIC FEATURES

	Male		Female		(1)	(2)	Others		Professionals		(1)	(2)
	mean	SD	mean	SD	diff.	p-value	mean	SD	mean	SD	diff.	p-value
Social Welfare	0.30	0.46	0.43	0.50	0.12	0.00	0.35	0.48	0.30	0.46	-0.04	0.06
Education/Cult	0.31	0.46	0.43	0.50	0.12	0.00	0.38	0.48	0.27	0.44	-0.11	0.00
Business Dev	0.67	0.47	0.47	0.50	-0.19	0.00	0.58	0.49	0.69	0.46	0.11	0.00
Security	0.14	0.35	0.05	0.21	-0.10	0.00	0.13	0.33	0.12	0.32	-0.01	0.62
	Younger		Elderly		(1)	(2)	W/O Degree		With Degree		(1)	(2)
	mean	SD	mean	SD	diff.	p-value	mean	SD	mean	SD	diff.	p-value
Social Welfare	0.33	0.47	0.44	0.50	0.11	0.07	0.34	0.47	0.33	0.47	-0.01	0.68
Education/Cult	0.34	0.47	0.23	0.42	-0.11	0.03	0.30	0.46	0.36	0.48	0.06	0.00
Business Dev	0.62	0.48	0.58	0.50	-0.05	0.43	0.65	0.48	0.60	0.49	-0.05	0.03
Security	0.13	0.33	0.08	0.27	-0.05	0.14	0.17	0.37	0.09	0.29	-0.08	0.00

Notes: Variables description and data sources are reported in Tables V1. The sample includes the 1,842 cabinet members for whom we find information on the departments and policy areas assigned (see text for details). For each variable, means and standard deviations in both groups are reported. Column (1) reports the mean difference between the two groups; Column (2) reports the p-value of the test on the equality of means.

across departments. A multi-dimensional classification of profiles essentially confirms these insights.[69] While only suggestive, these patterns align well with existing evidence on the role of each socio-demographic feature for attitudes toward different policies, thereby providing a rough validation of existing evidence that refers to individuals and voters, also for policymakers.

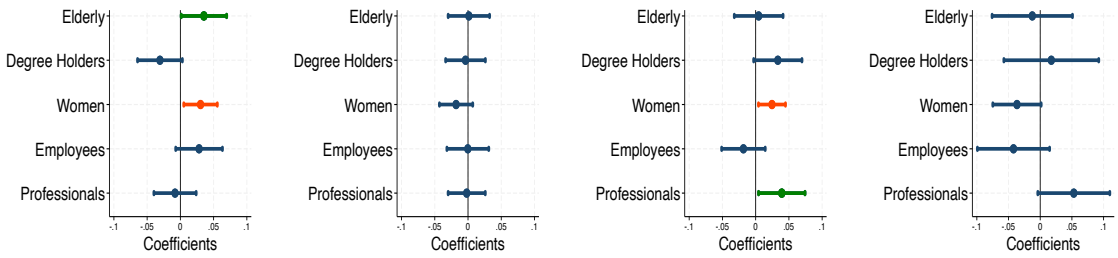
With this background in mind, we estimate the causal impact of treating each party on the socio-demographic characteristics of appointed cabinet members. Figure 10 provides a summary of the results by depicting the point estimates obtained by estimating the impact of treating a party on the average features of the cabinet with the empirical model (E2) in fully conditioned specifications with all covariates.

Complementing the evidence on policies in Section 4, the results show that treating different parties materializes in cabinets with significantly different socio-demographic characteristics.[70] Treating the left party increases the share of older cabinet members by 3.5 pp (compared to a mean in the untreated group of 51 percent) and women by 3 pp (compared to a mean in the untreated group of 24 percent). The results align well with the emphasis of

[69] See Figure A11. The prototypical types in charge of social welfare and environment are older females, the one for education and culture are non professional females, for business development are professionals (either male or female), and for security are males.

[70] Again, causal estimates provide a very different picture from the one that could be obtained with plain correlation regressions. See Figure A12.

Figure 10: TREATMENT OF PARTIES AND FEATURES OF CABINET MEMBERS



(a) LEFT (b) CENTER-LEFT (c) CENTER-RIGHT (d) POPULIST RIGHT

Note: The graph reports coefficient estimates of equation (E1) in fully conditioned specifications with all covariates. The *dependent variables* are the share of cabinet members that are: elderly, educated; women; employees; and professionals. *Treated Party* is a dummy variable equal to one if the party is in the focal point on the ballot paper and zero otherwise. *Sample*: all ruling coalitions containing the respective party. Point estimates in red (respectively green) are significant at least at the 5% (respectively 10%) level with confidence intervals at the limit (10%) significance level.

this party on welfare and education and the fact that these departments are disproportionally assigned to older members and women. Treating the center-right leads to a 3.9 pp increase in the share of professionals (compared to a mean in the untreated group of 46 percent), which is consistent with the focus on laissez-faire and the prominent role of professionals in business development.[71] The main impacts of treating the populist right are on males, an increase of 3.7 pp (mean 0.16) and a reduction in employees by 4.2 pp (mean 0.3) (see also Figure A13). Similarly to the results on policies, no significant effect is detected for the center-left.[72]

We replicate the analysis on the pooled samples (as in Table 4). The dependent variable is the share of cabinet members with a prototypical feature of each main party i given each party's electoral manifesto. Specifically, we look at the share of elderly for the left, the share of professionals for the center-right, and the share of males for the populist right. For the center-left, since no significant impact can be detected on any feature, we choose the share of employees. Table 8 reports the results of pooled specifications for both ITT and IV specifications. For comparability with Figure 10, columns (1-2) first look at the impact of treating a given party on the share of all cabinet members featuring the feature of interest. This implies,

[71]The increase in the share of women, which is seemingly unrelated to policies, is consistent with the presence of young female politicians in the center-right party during the observation period, which has also been interpreted as a communication/media strategy implemented by Berlusconi's party (see e.g., Statham, 1996).

[72]Finally, counterfactual treatment of parties in losing coalitions implies no systematic change in the features of cabinets (see Figure A10).

e.g., for the left, looking at the share of older members in the whole cabinet. Results show that treating a main party changes the share of cabinet members with the (party) prototypical feature by 2.8 pp. In the IV specification, an increase of 1 pp in the party share of votes within ruling coalitions changes the share of cabinet members with the party prototypical feature by 0.8 percentage points. Columns (3-4) look at how much of this effect is driven by changes in the prototypical features of cabinet members affiliated with the treated party. This implies, e.g., looking at the share, in the whole cabinet, of older members affiliated to the left. The results show that the impact on the cabinet is actually driven by a change in the features of the cabinet members affiliated with the treated party. The remaining columns confirm this insight by showing that treating a given party does not affect the same prototypical features when looking at cabinet members that are politically affiliated to other parties (see columns 5-6), that are not politically affiliated (see columns 7-8), or that are affiliated to parties different from any of the main parties (e.g., civic lists) (see columns 9-10).[73]

Table 8: FEATURES OF CABINET MEMBERS

Dep. Variable	FEATURES OF CABINET MEMBERS (AFFILIATED TO)									
	WHOLE CABINET		MAIN PARTY		OTHER PARTIES		NON AFFILIATED		NON NATIONAL PARTIES	
	ITT (1)	IV (2)	ITT (3)	IV (4)	ITT (5)	IV (6)	ITT (7)	IV (8)	ITT (9)	IV (10)
TREATED PARTY	0.028**		0.030***		0.004		-0.006		-0.008	
	(0.012)		(0.008)		(0.011)		(0.010)		(0.009)	
SH. OF VOTES W/I COALITION		0.008**		0.009***		0.001		-0.002		-0.002
		(0.004)		(0.003)		(0.003)		(0.003)		(0.003)
# Running Parties FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Coal. FE × Years FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
All Covariates	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
F-statistic Instrument		23.24		23.24		23.24		23.24		23.24
Mean Dependent	0.49	0.49	0.11	0.11	0.23	0.23	0.15	0.15	0.09	0.09
Observations	1,244	1,244	1,244	1,244	1,244	1,244	1,244	1,244	1,244	1,244
N. Elections	816	816	816	816	816	816	816	816	816	816
N. Municipalities	491	491	491	491	491	491	491	491	955	491
N. Cabinet Members	8,187	8,187	8,187	8,187	8,187	8,187	8,187	8,187	8,187	8,187

Notes: The dependent variable is the share of cabinet members with the salient features of each main party. See Table V1 for details and Table A29 for summary statistics. Treated Party is a dummy variable equal to one if the party is in the focal point on the ballot paper and zero otherwise. The IV estimates votes w/i coalition is the party share of votes within the ruling coalition. Mean Dependent is the average of the dependent variable for the control group. The sample includes coalitions with more than two running parties in municipalities with more than 15,000 inhabitants in the period 2002-2012 (see text for details). The sample includes coalitions for which we find information for at least 20% affiliated members (see text for details). Description of covariates, data sources, and summary statistics are reported in Tables V2 and A23. OLS regressions with robust standard errors in parenthesis. *, **, and *** denote significance levels at 10%, 5%, and 1%, respectively.

[73]Table A22 in the online appendix replicates the results of Table 8 excluding the center-left from the sample, and the results are consistent.

Finally, we look at the prototypical policy departments assigned to politically affiliated cabinet members. This test is very demanding and pushes the analysis to its limits by focusing on the small sub-sample of cabinet members for which we have retrieved information on the policy departments. This amounts to information on 1,800 cabinet members as compared to 8,000 (and 361 elections, as compared to 816). Notwithstanding this drastic reduction and the associated larger noise (standard errors are two to three times larger), the ITT results in Table 9 replicate and confirm the findings in Tables 6 and 8 (see columns 1-2). Next, we define the party prototypical department as the one in charge of implementing the party prototypical policy: social welfare for the left, education for the center-left, business development for the center-right, and security for the populist right. Column (3) documents a significant increase in the probability that that treated party gets a politically affiliated cabinet member who is also put in control of the politically salient policy department. The magnitude of the effect is an increase of 3.6 pp, which is comparable to the estimates of the ITT effect on the features in Table 8. Finally, column (4) only suggestively pushes the analysis further and detects a positive, although insignificant, probability that a treated party gets an affiliated cabinet member with the prototypical feature who also controls the salient policy department.[74]

5.4 Bargaining Power: Discussion

The previous Sections document that random boosts of votes to a party move coalition policies and expenditure towards the most salient policy for the treated party as stated in its electoral manifesto. The analysis of the mechanisms shows that the effect on policies is due to changes in bargaining power within legislative majorities and that the treated parties successfully manage to exploit their larger representation during the process of government formation. When treating parties with political ideologies focusing on specific issues, changes in the partisan composition of the government also come with changes in the appointed cabinet members' socio-demographic features (and latent preferences). Instead, when a party's

[74]Treating a party increases its share of affiliated cabinet members by 4.1 pp on a mean of 22 percent (on the full sample the effect was 4.7 pp for a mean of 23 percent). The share of cabinet members who are both affiliated with the party and display the prototypical feature increases by 4.7 pp on a mean of 11 percent (compared to 2.8 pp with a mean of 49 percent in the full sample). The treatment increases the share of cabinet members that are (i) affiliated to the party, (ii) have the (party) prototypical feature, and (iii) are assigned the (party) prototypical department by 1.9 pp over a mean of the dependent of 6 percent.

Table 9: POLICY DEPARTMENTS OF AFFILIATED CABINET MEMBERS

Dep. Variable	SHARE OF CABINET MEMBERS WITH			
	AFFILIATION	AFFILIATION + FEATURE	AFFILIATION + DEPARTMENT	AFFILIATION + DEPARTMENT + FEATURE
	(1)	(2)	(3)	(4)
TREATED PARTY	0.041**	0.047**	0.036*	0.019
	(0.019)	(0.020)	(0.019)	(0.015)
# Running Parties FE	✓	✓	✓	✓
Coal. FE × Years FE	✓	✓	✓	✓
All Covariates	✓	✓	✓	✓
Mean Dependent	0.22	0.11	0.12	0.06
Observations	571	571	571	571
N. Elections	361	361	361	361
N. Municipalities	515	515	515	515
N. Cabinet Members	1,841	1,841	1,841	1,841

Notes: The sample includes coalitions with greater than two running parties in municipalities with more than 15,000 inhabitants in the period 2002-2012 for which we find information for at least 20% affiliated members and for which information on the policy department is available (see text for details). Mean Dependent is the average of the dependent variable for the control group. Description of covariates, data sources, and summary statistics are reported in Tables V2 and A23. OLS regressions with robust standard errors in parenthesis. *, **, and *** denote significance levels at 10%, 5%, and 1%, respectively.

political manifesto mentions issues more evenly, no effect can be detected on the features of cabinets and policies. When exploring heterogeneous effects, we find, related to the question of the bargaining power of parties *vis-à-vis* the mayor in legislative majorities, that the effect of the treatment on policies tends to be larger when parties are confronted with mayors that are politically weaker because they had to face a run-off election, and that are not affiliated with the treated party. Similar patterns can be detected in terms of the partisan composition of the cabinet. The findings broadly align with the view that parties affect policies because of an increased relative bargaining power (see Figure A5).

6 Concluding Remarks

We have studied the impact of a random reshuffling of votes to parties in the context of a large-scale natural experiment covering the universe of local elections for over a decade in Italy. We show that the electoral rules, a lottery on the order of party symbols, and the peculiar graphical design of ballot papers implied a systematic reallocation of votes within running coalitions. The reallocation of votes is random, pervasive, and non-marginal but, due to the use of the lottery, is independent across coalitions, municipalities, and years.

Exploiting the natural experiment, the analysis provides evidence that is informative on the long-lasting debate on the role of votes to parties for policy setting in multi-party representative democracies. We focus attention on the main parties and isolate the effect of empowering each of the parties within ruling coalitions with a boost of votes on the policies implemented by the coalition. The results show that votes to parties imply perturbations of bargaining power within ruling majorities that materialize in sizable changes in primary policies. The effects can also be detected for small parties and are larger for the more politically divisive issues and when electoral platforms feature salient policy issues.

We explore the chain of mechanisms mapping votes to parties into legislative representation and to the appointment of cabinets. The results show that the capability of parties to shift coalition policies is strictly related to the bargaining process in the context of the formation of governments. Parties leverage their higher electoral support to gain politically affiliated cabinet members appointed to policy departments that are salient for the party. The findings also document that the cabinet members become closer regarding socio-demographic characteristics to the typical party-affiliated politician.

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