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**Essays in Political Economy: Turnout Decline,
Political Selection, and Accountability in
Democracies under Strain**

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Abstract

Advanced democratic systems are operating under mounting strain. Pressures arising from macro-level shocks, together with long-term trends in political participation, polarization, and demographic change, put democratic resilience to the test. This dissertation examines how core components of electoral processes in democracies—who turns out, who governs, and who exerts control—operate under such conditions of stress. Across three chapters, it combines causal inference and computational methods in the analysis of large-scale administrative data, policy and social media text corpora, and survey data to provide new evidence on the electoral consequences of turnout decline, the role of political selection in crisis, and the accountability costs of ideological defection.

The first chapter studies the electoral consequences of a sudden contraction in voting access. I exploit a reform that cut the number of polling stations by 30 percent in Italy and use a fuzzy difference-in-differences design at the municipal level together with an original Wikipedia-based measure of party ideology, geo-referenced ITANES survey information, and precinct-level electoral data matched with interpolated sociodemographic characteristics by census tract. Three findings emerge. First, restricting access to the polls reduces turnout, with effects that persist over multiple elections consistent with habit formation. Second, this decline reflects selective demobilization: marginal voters with low political engagement, weak partisan anchoring, and specific economic attitudes display higher elasticity to voting costs and are the first to exit the electorate. Third, this access-induced turnout decline reshapes electoral outcomes, generating a lasting rightward shift in the ideological distribution of the electorate, redistributing vote shares from extreme to moderate party groups, and affecting support differently across party ideologies.

The second chapter asks whether the characteristics of locally elected officials shaped excess mortality during the COVID-19 pandemic. Focusing on Italian municipalities, it studies whether having a college-educated mayor affected weekly excess deaths.

I estimate excess mortality using official death statistics and a Bayesian hierarchical spatio-temporal model, and address endogeneity in political selection with a close-election regression discontinuity design. The results show that college-educated mayors significantly reduced both the likelihood and the rate of excess mortality during the first wave, when local discretion mattered most, while effects disappear in the second wave. To examine mechanisms, the chapter introduces newly collected large-scale datasets on municipal policy activity and social media communication, constructed through web scraping and analyzed with natural language processing methods. The evidence points to two channels: more timely and targeted local policy responses, and more engaging public communication with a distinctive tone. Interpreting mayoral education as a proxy for a broader bundle of leadership traits, the findings show how political selection can shape local demographic vulnerability and population resilience during acute mortality shocks.

The third chapter turns to ideological polarization and electoral accountability. It studies whether voters sanction or reward incumbent mayors who change ideological camp between elections. Using three decades of Italian local elections, an original classification of parties into ideological poles, and detailed fiscal indicators, I compare the re-election prospects of ideological switchers with those of incumbents who remain loyal to their original camp. The analysis reveals an electoral penalty for mayors who cross ideological poles, on the order of 4 to 12 percentage points in re-election probability, even after conditioning on performance in office. Ideological switching emerges as an independent electoral liability, highlighting that voters value ideological congruence as a symbolic dimension of accountability, not merely policy outcomes.

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Chapter 1

Sounds of Silence:

Electoral Consequences of Turnout Decline and Selective Demobilization

Sounds of Silence

Electoral Consequences of Turnout Decline and Selective Demobilization

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Abstract

Declining voter participation has raised concerns about the responsiveness and sustainability of democratic systems. Research into the consequences of turnout variations provides mixed evidence and mostly relies on debated weather-induced shocks for identification. I shed new light on this topic by studying the effects of a reform that suddenly cut polling stations by 30% in Italy as part of broader fiscal consolidation measures. Leveraging variation at the municipality level in a fuzzy difference-in-differences design combined with a new Wikipedia-based measure of political ideology, I provide causal evidence on the intertwined relationship between voting access, participation, and electoral outcomes. Potential mechanisms are explored through the analysis of precinct-level and geo-referenced individual survey data. The paper provides three main contributions. First, restricting voting access determines a decline in voter participation consistent with habit-formation. Second, lower participation is driven by demobilization of disaffected marginal voters with economic grievances, who derive little satisfaction from turning out. Third, the exogenous reduction in turnout induces a persistent rightward shift in the political position of the electorate, redistributes vote shares from extreme to moderate party groups, and affects support differently across party ideologies.

Keywords – Political ideology, Voter turnout, Demobilization, Voting access, Fuzzy difference-in-differences

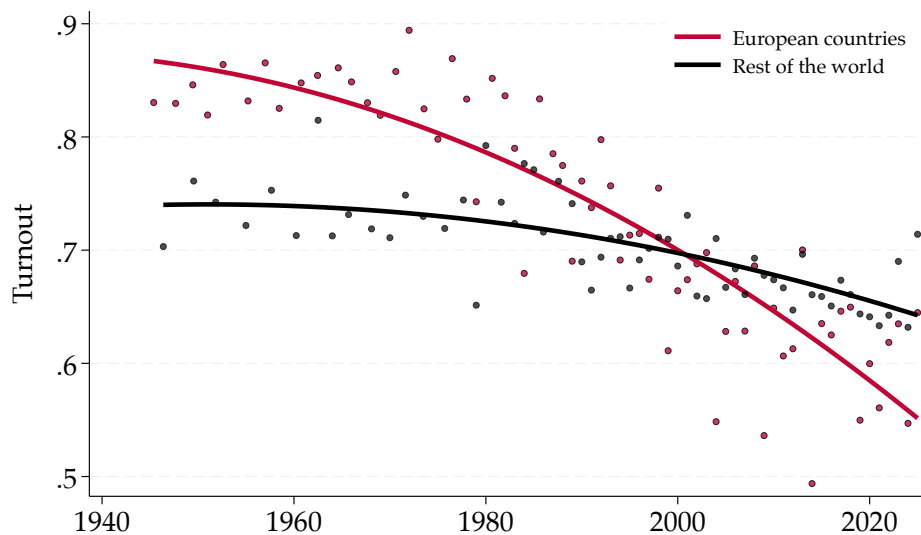
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1 Introduction

Voter participation is a defining feature of democratic life, one of the clearest expressions of the quality of democracy and the legitimacy of democratic governance (Dahl 2008; Norris 2011). When voters abstain, the electorate that selects representatives becomes smaller and potentially less representative of the underlying population. As such, worldwide declines in voter participation, with turnout falling to around 50% in several European countries [Figure 1](#), raise core questions about representation and policy responsiveness.

A large literature has examined why turnout declines, documenting the role of registration rules, electoral timing, competitiveness, and broader contextual factors, as well as persistent inequalities in participation by education, income, and age (Smets & van Ham 2013; Geys 2006; Gallego 2010; Kostelka & Blais 2021; Franklin 2004). Low and socially skewed turnout is a democratic problem because it biases representation against less-advantaged citizens (Lijphart 1997).

Fig. 1 Voter participation over time in European vs. non-European countries.



Notes: Binscatter based on data from [IDEA \(2025\)](#).

By contrast, the electoral consequences of turnout decline remain less settled¹.

¹An exception is represented by [Anzia \(2011\)](#), who compares off- and on-cycle elections showing that smaller turnout in the former increases the influence of teacher unions and, in turn, leads to higher teacher salaries.

Different sources of turnout decline need not demobilize the same voters, and therefore need not produce the same electoral consequences. Existing evidence reflects this ambiguity. Reforms that expand or facilitate participation show that changes in the electorate can alter public spending, policy priorities, and partisan outcomes ([Lizzeri & Persico 2004](#); [Miller 2008](#); [Fujiwara 2015](#); [Fowler 2013](#)). Evidence on turnout-reducing shocks is more limited and often relies on weather-induced variation, which raises concerns about exclusion-restriction violations ([Gomez et al. 2007](#); [Hansford & Gomez 2010](#); [Fowler 2015](#); [Mellon 2025](#)). The broader lesson is that the consequences of turnout decline depend on the source of the decline and on the voters moved across the participation margin ([Grofman et al. 1999](#)).

This paper advances this debate by exploiting a large contraction in voting access in Italy as a natural experiment. In late 1997, as part of a broader fiscal consolidation package aimed at stabilizing public finances, the Italian parliament passed a reform that mandated a 30 percent reduction in the number of polling stations nationwide. The reform was motivated by budgetary concerns, not by partisan or ideological goals. Nevertheless, it directly affected the infrastructure of voting by reducing the availability of polling places and increasing potential congestion at those that remained. I use this reform to study the electoral consequences of an austerity-driven restriction in voting access, focusing on turnout decline as the main mechanism.

The argument is that restrictions in voting access affect elections by changing who votes. Polling-station cuts raise participation costs through travel, information, and waiting costs. Standard turnout models imply that citizens at the participation margin should be most sensitive to such increases ([Downs 1957](#); [Riker & Ordeshook 1968](#); [Palfrey & Rosenthal 1985](#)). If marginal voters differ politically from regular voters, turnout decline should reshape electoral outcomes. In a multiparty system, this may redistribute support not only between left and right blocs, but also between extreme and moderate parties. The theoretical framework below develops these expectations in detail.

I test these arguments using the Italian polling-station reform as a natural exper-

iment. The empirical strategy relies on fuzzy difference-in-differences designs that exploit variation across municipalities in exposure to the reform targets based on population thresholds. I combine administrative data on polling stations, turnout, and election results with original measures of party ideology based on Wikipedia classifications of parties' left-right position and ideology tags, validated against expert-survey measures. To study mechanisms, I complement the municipal analysis with geo-referenced individual survey data from the Italian National Election Study and precinct-level evidence from Milan.

I find three main results. First, I show that restricting voting access causes a sizeable and persistent decline in turnout. Municipalities more exposed to the reform experience lower participation, and this gap grows over subsequent electoral rounds, consistent with habit formation in voting. Second, by exploiting the *de facto* longitudinal component of the Italian National Election Study, I investigate the micro-foundations of this aggregate decline. The turnout decline is driven by demobilization of marginal voters characterized by declining political interest, low perceived instrumental partisan differences, economic grievances, and procedural dissatisfaction with parliamentary decision-making. Complementary precinct-level evidence from Milan shows that turnout losses are larger in local contexts with higher shares of citizens likely to face greater participation costs or weaker attachment to electoral politics, including older, less educated, inactive, and retired residents. Together, these results provide evidence that voting-cost shocks selectively push disaffected and weakly attached marginal voters out of the electorate. Third, the induced turnout decline reshaped electoral outcomes. Lower turnout shifted the ideological center of gravity of the electorate to the right and redistributed vote shares away from parties at the extremes of the left-right scale toward more moderate parties. Decompositions by party position and ideology indicate that the largest losses were concentrated among left-wing, communist, and social-democratic parties, while moderate and some center-right party families gained.

The paper contributes to several debates. First, it contributes to the literature on turnout decline by studying one specific source of decline: restricted access to

voting. The findings show that turnout decline generated by administrative barriers can have lasting electoral consequences, and that these consequences depend on the political profile of demobilized voters. Second, it contributes to research on voting costs and electoral administration by studying a nationwide contraction in polling-station availability, rather than a local polling-place change or a reform that facilitates voting. Third, it extends evidence on the electoral effects of turnout beyond two-party settings by showing that participation shocks can redistribute support between extreme and moderate parties, not only between left and right blocs. Finally, it contributes to the political economy of austerity and public-service retrenchment. Whereas existing work emphasizes attitudinal responses to perceived state withdrawal ([Bansak et al. 2021](#); [Cremaschi et al. 2025](#); [Ciobanu 2024](#); [De Vries & Hobolt 2020](#)), I study a distinct channel: a fiscal-consolidation measure that changed the costs of voting and, through them, the composition of the electorate.

The broader implication is that not all turnout decline is politically equivalent. A decline driven by apathy, electoral timing, weather, or voting-access restrictions may move different voters across the participation margin and therefore produce different electoral effects. In the case studied here, austerity-driven restrictions in voting access selectively demobilized disaffected marginal voters, shifted support away from ideological extremes, and shifted the electorate's ideological center of gravity to the right.

2 Theoretical Framework

This section develops the theoretical logic linking a reduction in polling-station availability to turnout decline, selective demobilization, and electoral outcomes. The argument proceeds in three steps. First, polling-station cuts increase the costs of voting and should reduce participation among citizens at the participation margin. Second, because marginal voters differ from regular voters, turnout decline changes the composition of the electorate. Third, in a multiparty system, compositional change can

redistribute support not only between left and right blocs, but also between extreme and moderate parties.

2.1 Voting costs and selective demobilization

The starting point is the standard calculus-of-voting framework ([Downs 1957](#); [Riker & Ordeshook 1968](#)). Citizens decide whether to vote by comparing the expected benefits of participation with its costs. In its canonical form, the net reward from voting depends on the expected instrumental benefit of influencing the outcome, the expressive or civic benefit of participation, and the costs of turning out. Participation occurs when the expected benefits outweigh the costs.

The standard model raises the well-known paradox of voting: in large elections, the probability that an individual vote is pivotal is extremely small, so purely instrumental expected benefits should rarely justify the costs of participation ([Ferejohn & Fiorina 1974](#); [Feddersen 2004](#)). Subsequent work has addressed this problem by incorporating civic duty, ethical voting, expressive benefits, social pressure, and social-image concerns into turnout models. [Feddersen & Sandroni \(2006\)](#), for example, develop a model in which voting is costly and no individual vote is pivotal, but ethical agents participate when they determine that agents of their type have an obligation to vote. Field experimental evidence similarly shows that social incentives matter: social pressure can substantially increase turnout ([Gerber et al. 2008](#)), and voters may derive utility from being able to tell others that they voted ([Dellavigna et al. 2017](#)). These contributions are useful for the present argument because they imply that the relevant benefits of voting are not only instrumental. Citizens differ in civic-duty benefits, expressive rewards, social-image concerns, and susceptibility to social pressure, all of which affect how close they are to the participation margin.

This framework implies that increases in voting costs should reduce turnout, holding other factors constant. These costs can take several forms: longer travel distances, higher time costs, uncertainty about polling locations, more complex procedures, or longer queues at polling stations. The effect should be strongest among citizens close

to the participation margin. Building on this logic, [Palfrey & Rosenthal \(1985\)](#) model turnout as a participation game under uncertainty, in which citizens have heterogeneous preferences and costs and form beliefs about others' propensity to vote. In equilibrium, some voters are effectively indifferent between voting and abstaining. These marginal voters are the most likely to be moved out of the electorate by an exogenous increase in the costs of voting. Laboratory evidence provides support for key comparative statics of this class of models: turnout falls in larger electorates, rises in closer elections, and is higher among supporters of the less popular alternative ([Levine & Palfrey 2007](#)). By contrast, core voters with strong preferences, low costs, stronger partisan attachments, or high civic-duty benefits should be less sensitive to the same shock.

Polling-station cuts fit this framework directly. Reducing the number of polling stations can increase the costs of voting through several channels. It can increase travel costs if voters are reassigned to more distant polling places. It can increase information costs if voters must learn a new polling location. It can also increase waiting costs if more eligible voters are assigned to each remaining polling station, raising congestion at peak hours. Prior empirical work supports each component of this mechanism. [Brady & McNulty \(2011\)](#) show that precinct consolidation in Los Angeles County reduced turnout, especially where the reform increased distance to the polls and among voters facing higher transportation constraints. Related work finds that longer distances to polling stations and adverse changes in polling locations reduce participation, particularly among voters with higher mobility costs or weaker access to transportation ([Haspel & Knotts 2005](#); [Cantoni 2020](#)). These studies point to the same implication: restrictions in voting access should depress turnout by raising the costs of participation.

The logic also implies that turnout decline should be selective. A voting-cost shock does not randomly remove voters from the electorate. It should disproportionately affect those whose expected benefits from voting are lowest or whose participation costs are highest. These voters may be less politically interested, less strongly attached to parties, less convinced that partisan alternatives differ meaningfully, or more dissatisfied

with representative politics. They may also be citizens for whom voting is less habitual or more contingent on short-term incentives. Accordingly, a reform that increases voting costs should change not only the level of turnout but also the composition of the electorate.

2.2 Unequal participation and electoral consequences

Whether selective demobilization affects electoral outcomes depends on whether marginal voters differ politically from regular voters. If the voters who abstain after a cost shock have the same preferences as those who continue voting, turnout decline should have limited partisan or ideological consequences. If, instead, marginal voters differ systematically from regular voters, turnout decline can shift electoral outcomes.

A large literature on unequal participation suggests that this condition is often likely to hold. Participation is socially stratified: lower-income, younger, less-educated, and more mobile citizens tend to participate at lower rates and often face higher barriers to voting ([Smets & van Ham 2013](#); [Lijphart 1997](#); [Verba et al. 1995](#)). These inequalities matter because social groups differ in political preferences, redistributive demands, and attachments to parties. Low and socially skewed turnout can therefore bias representation toward the preferences of high-propensity voters.

Empirical evidence is consistent with the idea that changes in the electorate can change electoral and policy outcomes, although the direction of the effect depends on context. Studies of participation-expanding reforms show that bringing previously underrepresented voters into the electorate can alter the class of politicians elected and the policies they pursue. The introduction of electronic voting in Brazil simplified voting and reduced invalid ballots, effectively enfranchising poorer and less educated voters; this shifted support toward left parties and increased pro-poor public spending ([Fujiwara 2015](#)). Historical work on suffrage expansion similarly shows that extending participation can increase public expenditure and redirect policy priorities ([Lizzeri & Persico 2004](#); [Miller 2008](#)). Evidence from compulsory voting also indicates that higher turnout can change partisan and policy outcomes ([Fowler 2013](#)).

At the same time, the electoral effects of turnout are not universal. [Grofman et al. \(1999\)](#) emphasize that the partisan consequences of turnout depend on which voters are mobilized or demobilized. Weather-based studies in the United States generally find that lower turnout benefits Republican candidates ([Gomez et al. 2007](#); [Hansford & Gomez 2010](#); [Fowler 2015](#)), but these estimates rely on a specific source of turnout variation and a specific two-party context. Moreover, recent work raises concerns that weather affects elections through channels other than turnout, including campaign activity, economic behavior, and mobilization efforts ([Mellon 2025](#)). The broader lesson is that turnout decline should not be expected to have the same electoral consequences across all settings. Its effects depend on the source of the turnout shock and on the political profile of the voters induced to abstain.

This distinction is central in the present setting. A decline in turnout generated by voting-access restrictions may demobilize voters differently from a decline caused by low electoral competitiveness, political disinterest, weather, or weakened party mobilization. The relevant theoretical object is therefore not turnout decline in general, but turnout decline induced by an increase in the costs of voting.

2.3 Multiparty competition and the moderation of the electorate

In a multiparty system, the electoral consequences of turnout decline need not be limited to shifts between left and right blocs. Lower participation may also redistribute support between mainstream and extreme parties. This is important because voters can support non-mainstream parties for different reasons. Some are core ideological voters with strong commitments to extreme positions. Others are weakly attached, alienated from mainstream politics, or motivated by expressive protest. Still others may vote strategically for smaller or more extreme parties to balance expected policy outcomes in coalition politics ([Kedar 2005](#)). These different motivations imply different turnout elasticities.

The distinction between alienation and indifference is useful here. Voters may abstain because all available parties are too distant from their ideal point, or because

available alternatives are not sufficiently differentiated to make participation worthwhile ([Adams et al. 2006](#)). This logic matters for the electoral consequences of voting-cost shocks. If extreme-party supporters are core ideological voters, they may remain highly participatory even when voting becomes more costly. If, instead, a relevant share of support for extreme or niche parties comes from alienated, weakly attached, or protest-oriented voters, these parties should be more vulnerable to cost-induced demobilization.

Mainstream parties may also be more insulated from moderate increases in voting costs. Their voters are more likely to include habitual voters, citizens with stronger partisan attachments, and individuals embedded in civic or partisan networks. To the extent that mainstream parties also have stronger organizational capacity, they may be better able to inform, contact, and mobilize their supporters when polling places change or when voting becomes more costly.

This moderation logic is related to broader work on turnout decline and party responsiveness. If lower turnout changes the composition of the electorate and increases the relative weight of some groups over others, parties may respond to the preferences of those who remain. Recent work argues that declining turnout can increase party responsiveness to the median voter, because moderate voters play a central role in turnout changes ([Ezrow & Krause 2023](#)). My focus is different: I study immediate electoral consequences rather than party repositioning. But the underlying point is similar. Turnout decline can change the political weight of different segments of the electorate.

The ideological direction of the shift is more conditional. If the demobilized voters are disproportionately located on the left, turnout decline should move the electorate to the right. If they are disproportionately located on the right, the opposite should occur. If they are distributed symmetrically across the ideological spectrum but concentrated at the extremes, turnout decline should produce moderation without a clear left–right shift. The rightward shift is therefore not a general implication of lower turnout. It is an empirical implication that follows only if the voters pushed out of the electorate are

disproportionately attached to left parties.

In the Italian case, there are reasons to expect both moderation and a possible rightward shift. The voters most sensitive to an increase in voting costs are likely to be those with weaker political interest, weaker party attachment, economic grievances, and dissatisfaction with representative procedures. Such voters may be disproportionately drawn to protest or non-mainstream options. To the extent that these voters are also located more on the left, turnout decline should shift the ideological center of gravity of the electorate to the right. To the extent that they are drawn from both sides of the spectrum but concentrated among non-mainstream voters, the main effect should be a redistribution of support from extremes toward moderate parties.

2.4 Empirical expectations

The theoretical framework yields four empirical expectations.

H1 (Voting access and turnout). *Reducing access to polling stations lowers voter turnout.* Municipalities more exposed to the reform should experience larger turnout declines after implementation. Because voting is habit-forming, these effects may persist beyond the first post-reform election.

H2 (Selective demobilization). *Polling-station cuts demobilize voters at the participation margin.* Exposure to the reform should increase the probability that previously active voters abstain after the reform. Demobilization should be concentrated among citizens with weaker political interest, lower perceived partisan stakes, economic dissatisfaction, or weaker attachment to representative procedures.

H3a (Moderation). *Cost-induced turnout decline redistributes support from extreme to moderate parties.* If extreme and niche parties rely more heavily on weakly attached, alienated, or protest-oriented voters, polling-station cuts should reduce their vote shares relative to mainstream and moderate parties.

H3b (Ideological direction). *The left–right direction of the electoral shift depends on the political profile of demobilized voters.* If the voters pushed out of the electorate are dis-

proportionately left-leaning or attached to left parties, turnout decline should shift the electorate's ideological center of gravity to the right.

3 Municipal voting precincts and the 1998 reform

The division of Italian municipalities in voting precincts is regulated at the national level and implemented locally by municipal electoral commissions in accordance with national law. Electoral commissions are responsible for drafting the lists of eligible voters, dividing the municipal area in precincts, and assigning voters to polling stations (mostly on the basis of residential proximity). The revision of lists, precincts and assignments occurs twice a year. Each precinct is served by its own polling station, yet the same venue can host multiple polling stations. As of 2025 there exist 61,540 polling stations in Italy hosted in 22,072 buildings (of which 71.9% are schools, 15.8% public offices, and the rest include health, sport and military facilities). Historically, each municipality was divided in precincts such that between 100 and 800 eligible voters were assigned to each polling station². Each polling station requires the creation of an electoral office to carry out voting activities in any election. This is made up of a president, a secretary, and four or five scrutineers, each of whom receives a monetary compensation for their work before, during, and after every electoral consultation. In 1997 each of the 92,005 polling stations cost 1.15 million Lire (approximately €595), a lower bound estimate that includes exclusively the cost of personnel and disregards other variable and fixed costs.

A reform passed in late 1997 and supported by 75% of MPs mandated a reduction of 30% in the overall number of polling stations, as part of a broader set of measures aimed at stabilizing public finance by containing expenditures. The corresponding savings were estimated in 350 billion Lire for 1998 by the Italian Treasury³. The minimum and maximum number of eligible voters *per* polling station shifted upwards

²Law 1058 of October 7th 1947, and Decree of the President of the Republic number 223 of March 20th 1967.

³Agenzia Nazionale Stampa Associata (1997), Document ansa000020010929dt9s01dxx.

between 500 and 1,200⁴. The 30% reduction had to be achieved in each municipality by the end of 1998, and the partition of the voting population in polling stations was redefined and scaled differently across municipalities depending on population size. In order to minimize the distance between stations and voters' houses, the cut had to be implemented by merging stations already located in the same building (preferably other than schools) and, only if necessary, by merging stations in different buildings. Smaller reductions were allowed only in places where the exercise of voting rights was jeopardized by these provisions (e.g. extreme residential density, large numbers of residents abroad, difficult access to roads, lack of public transportation), and had to be compensated by larger cuts in more populous municipalities so as to achieve the 30% reduction necessarily at the province level.

As Table 1 summarizes, different targets were set depending on municipal population size. A target in terms of polling stations (PS) was set to one in municipalities below 1,200 residents, and to two in municipalities between 1,201 and 2,000 residents. Another target defined as the lower limit of the average number of eligible voters *per* polling station (EV) was set to 750 for municipalities between 2,001 and 40,000 residents, to 825 for those between 40,001 and 500,000, and to 900 for those above 500,000 residents⁵. I consider the municipalities complying with these targets by mid-1998 as *not exposed* to the reform, whereas those above the allowed number of polling stations or with an average number of eligible voters below the limits as *exposed* to the reform. The main analyses are carried out at the municipal level due to data limitations⁶. Exposure figures and rates are summarized in Column 5 of the table. In order to measure baseline treatment effects, I define exposure as binary for municipalities in brackets 3 and 4 using as threshold the 75th percentile of the distribution of eligible voters *per* polling station in the respective bracket. Results are robust to moving the binary exposure

⁴Law 449 of December 27th 1997.

⁵See the Decree of the Ministry of Internal Affairs of April 2nd 1998 for further details.

⁶In fact, the Ministry of Internal Affairs does not collect electoral outcomes systematically at the precinct level. As a result, estimates of effects on access and participation might be biased downwards, as I need to aggregate by municipality sub-municipal areas that experienced substantial cuts with areas that were unaffected or even benefited from the reform. Precinct-level data are sparingly made available by municipal offices. I retrieved data going back in time enough only for the municipality of Milan, which I use to confirm baseline analyses and test mechanisms.

threshold up or down, and to using a continuous measure of exposure as shown in sensitivity tests.

Figure 2 shows the reduction occurred in the aggregate number of polling stations (−34.5%), and the consequential increase in the average number of eligible voters *per* polling station (+52.9%) by the end of 1998. Figure A1 unpacks aggregate trends by municipal population brackets and highlights substantial heterogeneity in the implementation of the reform. The smallest municipalities were the least affected (−20.6% in PS, +26% in EV), followed by municipalities in the second bracket (−24.9% and +33.4%) and in the third one (−34.2% and +52.4%). The largest municipalities are next (−35.9% and +55.5%), while the most affected are those between 40,001 and 500,000 residents (−37.9% and +61.3%). Thus, the intensity of the reform’s effect grows monotonically up to the fourth population bracket, and shows the largest marginal increase between the second and the third bracket (a difference of 9.3 percentage points in the reduction of PS, of 19 points in the increase of EV).

Fig. 2 Aggregate number of polling stations and average eligible voters across the 1998 reform

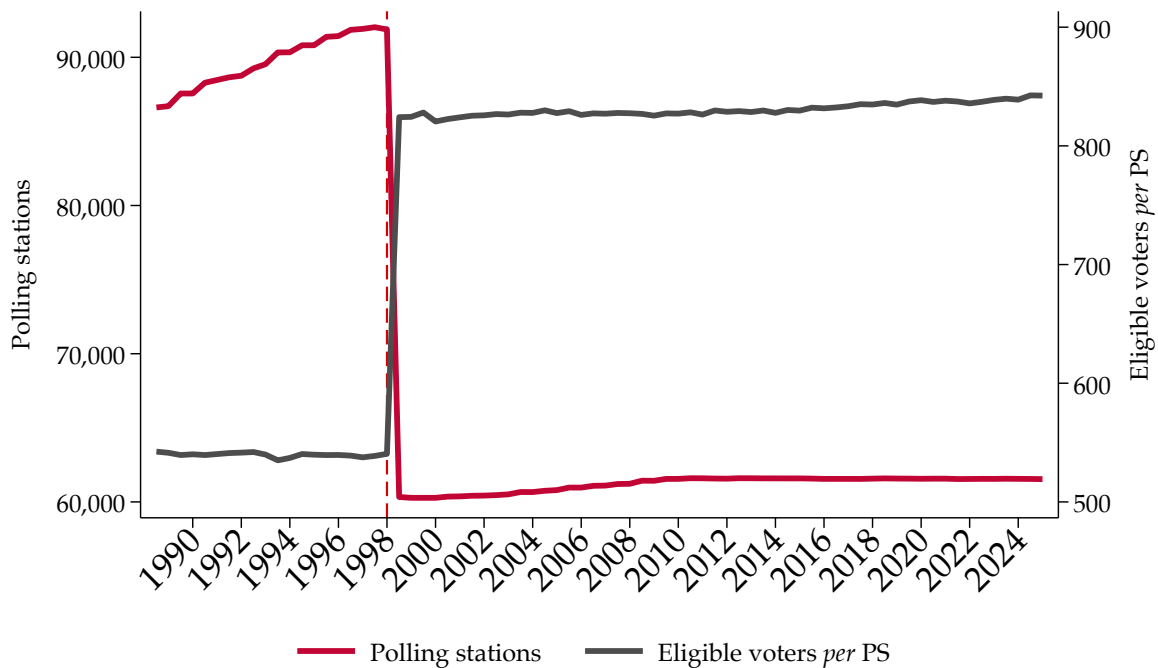


Table 1 Reform targets, municipalities' exposure and implementation outcomes

Bracket	Population	Targets			PS		EV	
		Polling stations (PS)	Elig. voters per PS (EV)	Exposed	Pre	Post	Pre	Post
1	$\leq 1,200$	1	–	420 [41.0]	1.49	1.20	419.9	532.7
2	1,201 – 2,000	2	–	360 [62.9]	2.87	2.14	527.6	713.6
3	2,001 – 40,000	–	≥ 750	1,370 [75.0]	11.4	7.55	553.9	823.5
4	40,001 – 500,000	–	≥ 825	45 [73.8]	109.1	65.88	530.4	867.0
5	$\geq 500,001$	–	≥ 900	–	–	–	–	–

Note: Column 5 reports numbers of municipalities exposed to the reform in 1998s1 according to binary thresholds and the corresponding percentages in brackets. Average values of PS and EV in Columns 6 to 9 are computed in 1998s1 (Pre) and 1998s2 (Post).

4 Data

4.1 Voting access

Information on voting access is retrieved from the *Biannual Survey of the Electorate* managed by the Ministry of Internal Affairs. The series on population, number of polling stations, number and composition of eligible voters (by gender, achieved voting age – i.e. voters turning 18 years of age – and residence abroad) is available from 1988 onward twice a year, on June 30th (s1) and on December 31st (s2), and released at the municipality level. Voting access is operationalized along two dimensions: (i) availability, measured as number of polling stations, and (ii) potential congestion, measured as (hundreds of) eligible voters (net of residents abroad) *per* polling station⁷.

4.2 Electoral data

Electoral data is retrieved from the *Historical Electoral Archive* maintained by the Ministry of Internal Affairs. For each municipality-election the information available includes the number of eligible voters and the number of voters who turned out (which I combine to compute turnout rates expressed as percentages), and the number of votes received by each list, coalition of lists, and candidate. Results are recorded in accordance with the details of the electoral system regulating each election. As my goal is to isolate the effect that a shock to voting access produced on participation and outcomes I must ensure neutrality in political and electoral incentives across observational units. Results from national elections represent the ideal source as they (i) occur simultaneously in

⁷Around 420 municipalities have miscoded series over the reform period (from 1994s2 and 2002s2), i.e. duplicated records from rows above. While the Ministry of Internal affairs has recognized this miscoding issue upon request and is working to fix it, I adopt an imputation strategy to generate number of polling stations and eligible voters in the missing period. In particular, for miscoded municipalities I project onwards (backwards) the number of polling stations in the last (first) pre-(post-)reform period with correct data up to the reform break in 1998s1. As a safety test of the strategy I observe that 83.7% of municipalities with correct data throughout have an equal number of polling stations between 1994s1 (when the series stops for most miscoded units) and 1998s1 (14.1% have more polling stations in 1998s1 than in 1994s1, so the strategy will make me underestimate the magnitude of the cut, if any), and 95.8% between 1998s2 and 2003s1 (when the series resumes for most miscoded units). Eligible voters in missing periods for miscoded municipalities are interpolated using as benchmark official yearly population statistics retrieved from Istat.

all municipalities, (ii) are regulated by round-specific electoral rules common to all municipalities, (iii) provide almost perfect homogeneity in political supply⁸, (iv) feature all national-level parties whose positions are known to the electorate. In particular, I consider results from 9 national electoral rounds from 1989 onward in municipalities holding other elections off-cycle. In fact, concurrent elections affect the demand-side incentives to turn out and the resulting vote choice (Kogan et al. 2018; Cantoni et al. 2021)⁹. My analysis focuses on votes to lists running for the Lower House (*Camera dei Deputati*) as the voting age has remained stable across the sample period¹⁰. In the rounds of 1994, 1996 and 2001 the electoral system for the Lower House was mixed (regulated by the *Mattarella Law*), assigning part of seats on the basis of a proportional system and part on a majoritarian basis. Accordingly, voters cast votes using two separate ballots which presented different sets of lists and coalitions. For these rounds I leverage larger variation generated from votes cast under the proportional system, as parties tended to run in separate lists rather than in coalition¹¹.

4.3 Political and ideological positions

Electoral outcomes of interest summarize voters' political stances as emerging from electoral results. To build the outcomes I combine party vote shares and information on party positions. Among several approaches to score party positions (e.g. standard surveys, expert surveys, scaling of party manifestos, analysis of parliamentary speeches), I rely on a novel method that leverages information from parties' Wikipedia pages (Brown 2011). A party's Wikipedia page shows an infobox containing key information about the party that editors can optionally add following a pre-defined set of tags

⁸Lists and coalitions of lists are the same across the country with only minor exceptions in some districts especially in the past.

⁹Separate correlational analyses confirm sizable concurrency effects in my sample as well. Turnout increases significantly by 1.8 points with two concurrent election, 4.2 with three, 3.2 with four (not significant). Average ideology moves significantly to the right with two concurrent elections, to the left with three and four.

¹⁰In 2022, the voting age for the Upper House (*Senato della Repubblica*) was lowered from 25 to 18, aligning it with the voting age for the Lower House.

¹¹On average, 12.9 lists were available in the proportional ballot vs 4.5 in the majoritarian in 1994, 11.6 vs 3.3 in 1996, 15.4 vs 4.4 in 2001. In my sample proportional and majoritarian turnout rates show a correlation of 1, .99 and .99 in the three rounds, while average ideology scores of .87, .78 and .82, respectively.

(e.g. president, secretary, founder, foundation/dissolution date, headquarters, etc.). Of interest for this work are the tags *political position* and *ideology*. The former records the party's placement on the left-right spectrum using an ordered 7-level scale. The latter summarizes the political philosophy(ies) that inspire parties' platforms. I compile a list of unique political parties, electoral lists, parliamentary groups, and political movements with a Wikipedia page either in English ($N=1,423$) or Italian ($N=1,536$). Next, I identify a subset of 688 of such political subjects that have run in elections at any level throughout Italian history, and I scrape their Wikipedia pages to collect all position and ideology tags from infoboxes: 449 and 549 (299 and 314) of these have, respectively, a position and ideology tag in the English (Italian) Wikipedia. English and Italian pages are then matched when they point to the same subject and their tags harmonized using the same English dictionary. In assigning tags to parties I prioritize information retrieved from the English Wikipedia¹², while complementing it with information from the Italian Wikipedia to maximize coverage¹³. A party's infobox might show tags identified by time periods: in such cases I split the unique party into different parties defined by the same periods. Finally, tags accruing to party factions/minorities are discarded in baseline analyses.

Table A1 summarizes unique parties running in any national election in the sample period by political position from the respective Wikipedia page coded on an ordered scale from 1 to 7¹⁴: out of 274 unique parties, 162 have a Wikipedia page (59.1%) and 147 have a position tag (52.2%)¹⁵. The benefits of this approach over previous ones in terms of coverage are substantial, especially in political systems characterized by high fragmentation: tagged parties account for 99.2% of all valid votes cast over time¹⁶, and parties with missing position have received on average .006% of total votes. Wikipedia

¹²English pages are more comprehensive, better documented, and built using inner Wikipedia references that a broader community of editors and users contribute to.

¹³As a validity check, 249 subjects that have a position tag in both languages show a correlation coefficient of .97.

¹⁴Most parties appear both in the Italian and English Wikipedia (the cross-language correlation of positions is .924, and preferring one over the other does not affect results), while a few appear only in either of them.

¹⁵Of these, 23 have two tags. In these cases I compute intermediate average positions.

¹⁶On a total of over 326 million votes. Three major expert surveys, CHES, ParlGov, and V-Dem, code respectively 43, 44 and 37 unique parties, accounting for 87.6%, 72.6% and 86.6% of valid votes.

pages list on average 2.5 ideology tags *per* party, a number that increases for parties in extreme positions. Unique parties' positions are averaged when parties run in coalition and coalitions are not labeled separately in the electoral dataset.

To assess the validity of Wikipedia-based position codes, [Figure A2](#) plots correlations between the position of unique parties identified on Wikipedia and the respective ideology scores in three major expert surveys (used as ground truth), considering national elections since 1946 and both Parliament Houses to maximize matches. Correlation coefficients range from .86 to .96, highlighting a striking similarity between coding by experts and crowd-based coding on Wikipedia (see [Herrmann & Döring 2023](#) for further validations of a similar approach).

4.4 Outcomes of interest

For each municipality-election i I build an overall political position defined as the average of parties' positions weighted by the respective votes gained in the election, to be interpreted as a left-right score from 1 (far-left) and 7 (far-right):

$$P_i = \frac{\sum_p P_p v_p}{\sum_p v_p}$$

where P_p is the Wikipedia-based left-right position of party p and v_p are total votes cast for party p . To assess robustness I also define a median position as the Wikipedia-based position of the party at the center of the cumulative distribution of votes ordered from left to right.

In addition, party vote shares are aggregated by political position to identify which parts of the spectrum drive overall dynamics induced by lower voting access. First, I add up vote shares by distinguishing parties in (spatially) *moderate* as opposed to *extreme* positions. Second, I split the spectrum in five political levels and aggregate party vote shares within them¹⁷. Similarly, I aggregate party vote shares by ideology tag to understand whether there occur specific dynamics not simply captured on the

¹⁷The aggregated moderate and extreme vote shares are defined, respectively, as $V_i^{Mod} = \sum_{p \in [3,5]} v_p$ and $V_i^{Ext} = \sum_{p \notin [3,5]} v_p$. The five positions are *left*, *centre-left*, *centre*, *centre-right*, *right*, with intervals, respectively $p_L = [1, 3)$, $p_{CL} = [3, 4)$, $p_C = [4, 4.5)$, $p_{CR} = [4.5, 5)$, $p_R = [5, 7]$. I consider five political levels to ensure that at least one party *per* level runs in each municipality-election in the sample.

left-right spectrum. Since a single party is tagged with several ideologies potentially uncorrelated with political positions (which instead are mutually exclusive), analyses that adopt these vote shares as outcomes are not compositional and require careful interpretation.

4.5 Additional municipal-level data

Data on composition of the population by gender, age, education, labor force participation, and citizenship is retrieved from the last pre-reform Census occurred in 1991 and administered by the Italian Statistics Office (Istat). I build municipality-level measures of the prevalence of specific socio-demographic strata in order to test mechanisms about systematic (de-)mobilization patterns determined by the reform. The same data is available also at the level of sub-municipal census tracts, which I use to measure residential segregation along the main socio-demographic dimensions by building municipality-level generalized indices of dissimilarity ([Sakoda 1981](#)). For each socio-demographic characteristic S with mutually exclusive groups s the index is constructed as:

$$D_S = \frac{\sum_s \sum_c |N_{cs} - E_{cs}|}{2 \sum_s NP_s(1 - P_s)}$$

where N_{cs} is the number of people in group s and census tract c , $E_{cs} = \frac{N_c N_s}{N}$ is their expected frequency (given the total number of people in c , N_c , in s , N_s), $P_s = \frac{N_s}{N}$ is the proportion of people in s among total municipal population N . The index ranges between 0, i.e. perfectly even distribution of people by characteristic S across census tracts, and 1, i.e. complete segregation. Dissimilarity indices are added to the analysis to test whether any change in participation and voting is driven, mechanically, by the fact that reduced voting access is correlated with an uneven distribution of specific segments of voters within municipalities (thereby remedying to the unavailability of sub-municipal access data).

As the composition of the electorate is subject to variations from one electoral round to the next, model estimates should be adjusted to reflect deviations from the composition of voting-age population observed in 1996 (the last round before the

reform). Using annual data from Istat, I compute for each municipality-year of election a rate of deviation from 1996 population by specifying a voting-age population balance:

$$Dev_{t'} = \frac{\sum_{t=t'}^{1995} (N_t + M_t) \cdot \mathbb{I}\{t' < 1996\}}{P_{1996}} - \frac{\sum_{t=1996}^{t'} (N_t + M_t) \cdot \mathbb{I}\{t' > 1996\}}{P_{1996}}$$

where $N_{t'} = B_{t'} - D_{t'}$ is the natural increase in voting-age population given by new eligible voters, $B_{t'}$, minus deaths, $D_{t'}$, $M_{t'} = I_{t'} - E_{t'}$ is net migration defined as difference between internal and international immigration, $I_{t'}$, and emigration, $E_{t'}$, and P_{1996} is total population in 1996. The rate summarizes how larger or smaller population in year t should be in percentage terms to resemble 1996 population. As this term introduces post-treatment bias in the specification, I add it as a control only as a robustness test aimed at discounting the effect of inflows and outflows related to entry into and exit from municipalities' electorates, thus keeping constant the composition of the population exposed to the shock and to habit formation.

Weather-related variables are used both as time-varying controls in robustness checks, and as alternative instruments to compare both quantitatively and qualitatively the effects of diverse sources of variation to turnout on electoral outcomes. Weather data is taken from Copernicus ERA5-Land climate reanalysis dataset by the European Centre for Medium-Range Weather Forecasts (ECMWF)¹⁸, which provides consistent daily series of atmospheric and land surface variables at 9km grid spatial resolution. The main variables retained for analysis, which are supposed to influence voter participation, are air temperature at 2m-height in Celsius degrees, accumulated water precipitation in millimeter-depth, and wind speed at 10m-height in meters *per* second. Gridded data is aggregated over official municipal boundaries from a shapefile of Italian municipalities by ISTAT in order to obtain municipality-level estimates of weather conditions. I alternatively consider weather conditions on election days and weather shocks, defined as deviations of election-day weather from the corresponding averages over the previous day, week, and month.

¹⁸<https://cds.climate.copernicus.eu>

4.6 Individual-level data

In order to test mechanisms, I bring the analysis at the individual level to document the direct impact of the reform on people’s decision to turn out. Geo-coded data at the individual level comes from the 2001 edition of the *Italian National Election Studies* (ITANES), a representative survey of the Italian population on topics related to politics and elections. Face-to-face CAPI interviews were administered after the 2001 national elections in 258 municipalities to a stratified sample of eligible voters randomly drawn from municipal electoral registries. I retain the sample of respondents to questions about the 1996 elections (last pre-reform round) and 2001 (first post-reform round), and link them to the measure of exposure to the reform in their municipalities of residence. This allows me to assess how the reform affected voting decisions using *de facto* within-individual longitudinal variation.

4.7 Precinct-level data

While precinct-level data is not systematically collected by Italian central institutions, single municipalities make this information available on their websites with varying degrees of aggregation, coverage, and disclosure. The only municipality with retrievable detailed data that enable quantitative analysis across the reform period is the city of Milan¹⁹. I collect electoral data by polling station²⁰, and match polling stations over time combining their serial numbers with the street address of their hosting venues by election²¹. Zooming in on a single city that experienced a reduction from 2,086 polling stations to 1,251 allows me to validate baseline results by leveraging within-precinct variation. To uncover contextual mechanisms of declining participation, I match precinct-level electoral data with fine-grained socio-demographic characteristics

¹⁹Data is available at <https://siel.comune.milano.it>.

²⁰Complete data for *Chamber* elections are available from 1994. Differently from smaller municipalities, in big municipalities like Milan precincts are nested into different electoral districts, each characterized by unique sets of running candidates by lists. The number of districts dropped from 11 (up to 2013) to 6 (in 2018) and to 3 (in 2022). To ensure comparability of political supply I analyze the period 1994-2013.

²¹Before 2001 the address of polling stations is not available in any format as confirmed by Milan’s archives, statistics offices and top-level managers upon contact. To ensure that the same polling stations are followed over time I implement several checks described later.

by 1991 Census tracts after spatial interpolation as described in [Pinto \(2023\)](#).

5 Empirical strategy

A basic approach to quantify the relationship between voting access and turnout leverages time variation in both variables within municipalities as illustrated in [Equation \(1\)](#):

$$T_{i,t} = \beta A_{i,t} + \mu_i + \nu_t + \varepsilon_{i,t} \quad (1)$$

where $T_{i,t}$ is the turnout recorded in municipality i and year of election t , $A_{i,t}$ is a measure of access to voting in i and t , μ_i and ν_t are municipality and year of elections fixed effects that absorb time invariant municipal characteristics and common shocks occurred at specific points in time, and $\varepsilon_{i,t}$ is an error term. Similarly, time variation in turnout within municipalities can be exploited to explain downstream electoral results. A correlational approach like this would likely suffer from endogeneity along several dimensions: for example, (i) polling stations might be more numerous in localities where voter participation has been historically higher, (ii) their number might have been manipulated strategically to ease or make it hard to monitor local voting trends, or (iii) participation might increase because voters anticipate the closeness of an election.

The 1998 reform of electoral precincts represents a unique opportunity both to measure the effects of an unprecedented cut in voting access and to leverage exogenous variation in access to identify the causal relationship between access and turnout on the one hand, and between turnout and electoral outcomes on the other hand. The details of the reform, the resulting exposure to it, and its actual roll-out allow me to identify these effects through two different strategies that I describe below. [Figure 3](#) summarizes the immediate effects of the reform on voting access at the extensive margin (top panel) and intensive margin (bottom panel) in Italian municipalities, averaged by population percentiles²². Three main insights emerge. First, voting access is less

²²The last bracket cannot be shown as a separate percentile bin, because only 6 municipalities have more than 500,000 residents. See [Figure A3](#) for a zoom on each threshold separately and a double series of bins according to exposure to the reform.

(more) affected at the lower (higher) extreme in each bracket. For example, a closer look at the first bracket reveals that most small municipalities had a single polling station (i.e. the reform produced no effect), whereas many municipalities right below 1,200 residents had to cut dramatically to comply with the reform (e.g. reductions $\geq 50\%$ in case of 2 or more stations in mid-1998). Second, both the probability and intensity of reductions in polling stations increase gradually with population size within brackets. Average changes are substantial from a bracket to the next, but trends do not exhibit sharp jumps around thresholds²³. Third, exposed bins always dominate non-exposed ones in probability and intensity of reductions. This is reassuring in that the (exogenous) exposure of municipalities to the reform strongly predicts the (endogenous) implementation of cuts performed by electoral commissions, even if data are aggregated by municipality.

My goal is to understand whether (i) the restriction in voting access due to the reform affects turnout, and (ii) the change in turnout rates produces electoral consequences. In this framework, turnout stands as an endogenous treatment variable that is affected by reduced voting access. Since it is possible that also municipalities less exposed to the reform might have experienced changes in voter participation, the treatment effect of interest cannot be identified sharply (i.e. the treatment rate rather than the treatment status is different in exposed municipalities compared to not exposed).

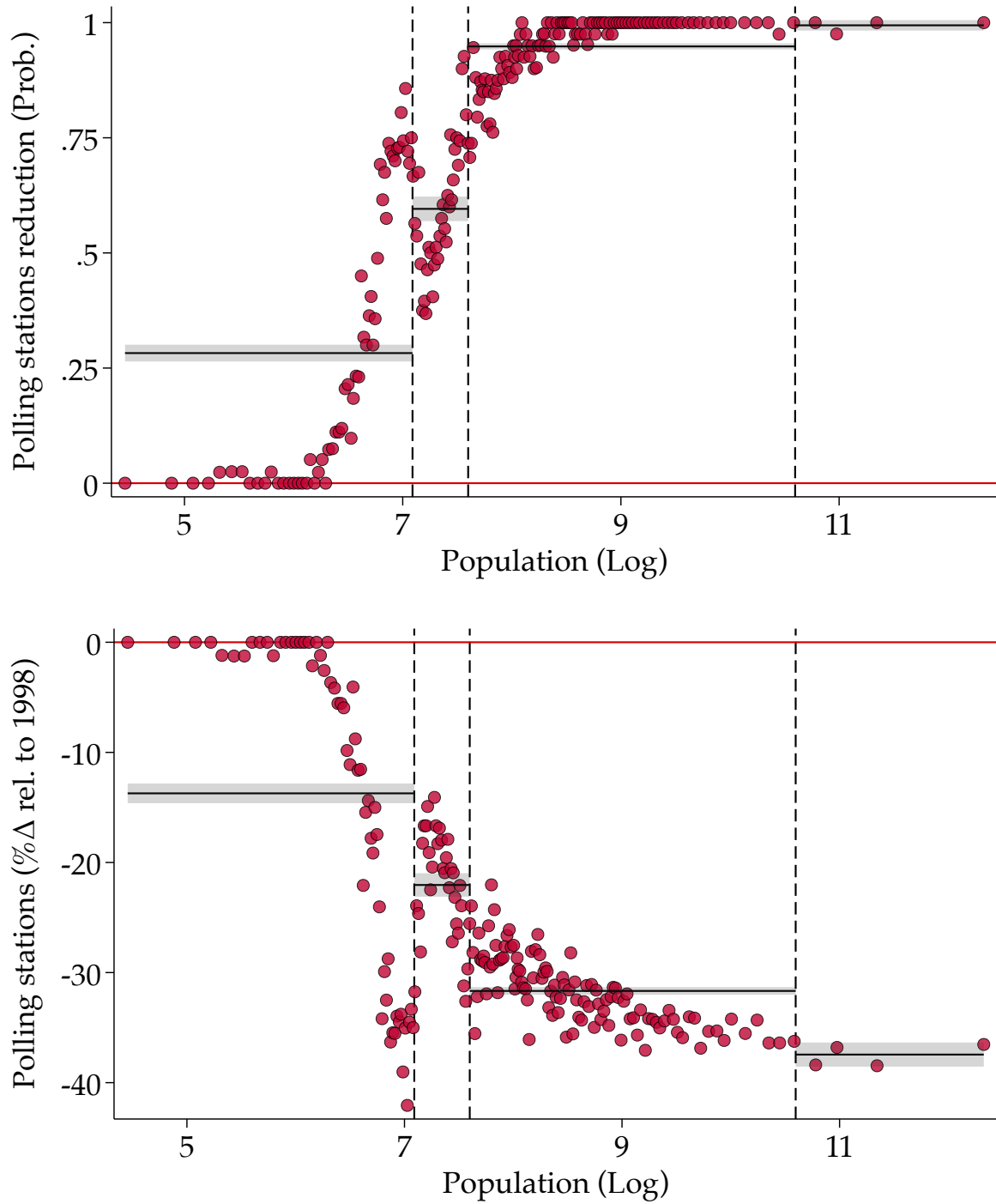
5.1 Exposure to treatment within population brackets

My main identification strategy relies on a fuzzy difference-in-differences (fuzzy DiD) specification that leverages variation in exposure to the reform across municipalities before and after 1998. The first-stage specification considers voter turnout as an outcome as shown in [Equation \(2\)](#):

$$T_{i,t} = \beta(\mathbb{I}\{V_i \neq V_b^*\} \times \mathbb{I}\{t > 1998\}) + \mu_i + \nu_t + \varepsilon_{i,t} \quad (2)$$

²³This prevents me from adopting a regression discontinuity design to measure treatment effects.

Fig. 3 Polling stations reduction following the 1998 reform



Notes: Binscatters of the variation in the probability (top panel) and intensity (bottom panel) of polling stations reduction at the municipality level after 1998s1 by population. Vertical dash lines indicate population thresholds identifying different reform targets. Horizontal solid lines summarize average changes by population bracket, surrounded by the respective 95% confidence intervals in grey.

where $\mathbb{I}\{V_i \neq V^*\}$ is a dummy variable equal to one if PS/EV V_i in 1998s1 makes municipality i *not* compliant with target V^* in the respective population bracket b (i.e.

exposed to the reform) and zero otherwise (*not exposed*)²⁴, $\mathbb{I}\{t > 1998\}$ is another dummy taking value one in election years after 1998 and zero before (i.e. treatment timing is the same for all units). Their interaction is used as an instrument for turnout in municipality i and year of election t . β is a DiD estimator that captures the effect of the reform on voter participation. Estimated levels of turnout $\widehat{T}_{i,t}$ from Equation (2) are then plugged in the second-stage specification of Equation (3):

$$Y_{i,t} = \gamma \widehat{T}_{i,t} + \pi_i + \rho_t + \eta_{i,t} \quad (3)$$

where $Y_{i,t}$ is an electoral outcome of interest, and γ is a local average treatment effect (LATE) estimator of voter turnout on election outcomes among treated *switchers*, i.e. those municipalities where turnout rates incrementally changed only as a result of the reform. The presence of two-way fixed effects (TWFE) both for municipality and election year ensures that estimated coefficients capture effects within municipalities net of time trends common to all municipalities. I estimate a model using the full sample of municipalities—thus collapsing binary exposures based on different targets—and a series of models that focus on municipalities in the same bracket—within which the reform targets are homogeneous and the scope for omitted variable bias is smaller.

Valid identification of the LATE in a fuzzy DiD requires meeting a set of identifying assumptions. The strength of the instrument is tested by computing Kleibergen-Paap F-statistic from estimates of Equation (2). The robustness of inference in presence of a weak instrument is tested through the Anderson-Rubin Wald test. The exclusion restriction at the core of this natural experiment requires that exposure to the 1998 reform, aimed at containing public finance by cutting on polling stations, has affected local electoral outcomes only through its effect on participation (Brady & McNulty 2011; Cantoni 2020; Garmann 2017; Harada et al. 2024). The restriction is likely to hold in this setting because my instrument combines (i) a temporal component with, (ii) population thresholds for reform targets that, to the best of my knowledge, have

²⁴In particular, $\mathbb{I}\{V_i > V^*\}$ if $i \in \{b_1, b_2\}$, whereas $\mathbb{I}\{V_i < V^*\}$ if $i \in \{b_3, b_4, b_5\}$. See Figure A3 for a visual intuition of the strategy based on comparison of exposed and non-exposed units featuring a differential reduction in access after the reform within brackets.

never been adopted to roll out other policies, and (iii) reform exposure thresholds in terms of polling stations or eligible voters *per* polling station that, in the same vein, are orthogonal to other well-known thresholds. Together, these insights exclude the possibility that the reform effect is compounded by other policies. The hypothesized mechanism is an increase in the cost of voting, as less widespread polling stations reduce voting access by altering the availability of voting infrastructure and the potential congestion at the station. At the same time, increased voter density resulting from a larger number of polling stations could strengthen voter secrecy and reduce monitoring opportunities by parties (Caselli & Falco 2022). A recurring finding in the literature on turnout- and vote-buying literature that emphasizes the role of voting access is that higher availability of polling stations (i) increases turnout rates and (ii) has a simultaneous effect on electoral outcomes that varies with the study's context (Larreguy et al. 2016; Jimenez et al. 2017). The causal pathway implied in this literature is similar to that underlying my strategy. Therefore, voter secrecy could feature as a potential alternative or complementary mechanism driving the relationship between voting access and participation, rather than as a violation of the exclusion restriction (i.e. it does not link access to electoral outcomes independently of turnout).

Next, in order for the non-exposed set of municipalities to represent a valid comparison group it is required that electoral outcomes in exposed municipalities would have followed, in the absence of treatment, the same trend over time as to non-exposed ones. This parallel trends assumption is tested through event studies by specifying a dynamic version of the reduced-form equation: the exposure dummy is interacted separately with election year dummies (omitting the interaction in the last pre-reform election of 1996), and a failure to reject that interactions before 1996 equal zero are interpreted as evidence in favor of parallel pre-trends in ideology.

In TWFE DiD specifications like these γ identifies the switchers' LATE under two additional assumptions (de Chaisemartin & D'Haultfœuille 2018; de Chaisemartin & D'Haultfœuille 2020): stable treatment effect over time, which requires that an equal change in turnout corresponds to an equal electoral effect at any point in time, and

homogeneous treatment effect between groups, imposing the same LATE between treated and control switchers²⁵. To assess the plausibility of these additional assumptions I test the equality between alternative estimators discussed in [de Chaisemartin & D'Haultfœuille \(2018\)](#) that identify the LATE without imposing restrictions on treatment effects, i.e. the time-corrected (TC) and the changes-in-changes (CIC) Wald estimators.

5.2 Treatment intensity across population thresholds

The second strategy aims at identifying a treatment effect by exploiting the different intensity of the reform below and above thresholds. I adopt a fuzzy DiD specification whose second stage is similar to what seen in [Equation \(3\)](#). The first stage is modified as in [Equation \(4\)](#)

$$T_{i,t} = \delta(\mathbb{I}\{P_i > P_c^*\} \times \mathbb{I}\{t > 1998\}) + \sigma_i + \tau_t + \zeta_{i,t} \quad (4)$$

where $\mathbb{I}\{P_i > P^*\}$ is a dummy variable equal to one if population size in municipality i , P_i , lies above the limit population P^* at threshold c and to zero otherwise. This variable dichotomizes treatment intensity on opposite sides of thresholds exploiting the monotonic increase of intensity across population thresholds as shown in [Figure 3](#). The DiD estimator δ is the coefficient on the interaction between the latter dummy and the usual time dummy variable: it captures to what extent participation has changed after 1998 in municipalities experiencing heavier reductions in voting access compared to municipalities subject to lighter cuts in polling stations. Separate models are estimated using samples of municipalities next to each of the three thresholds, such that municipalities lying between different thresholds, $P_c^* < P_i < P_{c+1}^*$, feature as treated units when the threshold of interest is P_c^* and as control units in the case of P_{c+1}^* . An overall model is estimated by stacking the separate samples and adding sample-by-time fixed effects to the econometric specification²⁶.

²⁵The pre-reform difference in turnout between exposed and not exposed municipalities is 0.39 percentage points in the full sample. This difference appears too small to indicate heterogeneous electoral effects due to baseline turnout rates between groups.

²⁶Results from specifications following this strategy are presented separately in Appendix B, in particular in [Tables B8 to B14](#).

6 Results

6.1 Turnout effects of exposure to reform

The relationship between voting access and participation is first explored descriptively in Table 2 by estimating Equation (1) and considering, separately by panel, the two dimensions of access. The table reports OLS estimates in different samples defined by population brackets of the reform. Larger availability of polling stations improves voter participation, an effect concentrated among municipalities in bracket 3. According to the estimates, a 30% reduction as mandated by the reform would reduce turnout by .47 percentage points in these municipalities. Panel B shows that higher congestion harms participation throughout, with stronger effects again in brackets 3, where the same 30% reduction in polling stations would decrease turnout by about 1.6 points.

Table 2 Voting access and turnout - OLS estimates

	Full sample	Bracket 1	Bracket 2	Bracket 3	Bracket 4
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Availability</i>					
Polling stations (Log)	1.059*** (0.363)	0.367 (0.620)	-0.979 (0.767)	1.320*** (0.456)	-1.399 (2.398)
Adj. R^2	0.82	0.73	0.81	0.89	0.96
<i>Panel B: Congestion</i>					
Eligible voters (Log)	-4.097*** (0.423)	-3.397*** (0.724)	-1.116* (0.664)	-4.375*** (0.540)	-0.049 (2.552)
Adj. R^2	0.83	0.74	0.81	0.89	0.96
N	31,374	9,225	5,157	16,443	549
Municipality FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Mean $_{\gamma}$	78.67	76.97	77.59	79.91	80.06
Std. Dev. $_{\gamma}$	[10.63]	[11.20]	[10.51]	[10.20]	[9.36]

Note: Robust standard errors, clustered by municipality, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Table 3 reports first-stage estimates of Equation (2)²⁷. Overall, the reform reduced turnout by 0.65 points. The first-stage effect is strong and negative only in bracket 3 (−1.4 pp, corresponding to around 0.14 standard deviations), where the instrument proves strongest by commonly accepted standards, while the AR test suggest robust LATE in presence of weak instruments both overall and in bracket 3. Two insights emerge from the analysis of other brackets. First, weak effects are consistent with OLS results in Table 2, i.e. suggesting that availability of voting facilities does not affect participation in municipalities below 2,000 residents and in the very large ones, but only in the medium-sized. Second, positive turnout effects might indicate that the implementation of the reform in small municipalities did not affect or even reverted the costs of voting (for example, moving to a unique voting facility closer to the city center might make access on average easier than two polling stations in more remote areas). However, poor IV diagnostics do not suggest any effects of the reform outside the bracket 3 sample²⁸.

Table 3 Exposure to reform and turnout – First-stage estimates

	Full sample	Bracket 1	Bracket 2	Bracket 3	Bracket 4
	(1)	(2)	(3)	(4)	(5)
Exposed × Post-1998	−0.653*** (0.216)	0.819* (0.492)	1.844*** (0.488)	−1.444*** (0.269)	0.410 (0.686)
Adj. R^2	0.82	0.73	0.81	0.89	0.96
KP F -stat	9.2	2.8	14.3	28.9	0.4
AR Wald test	146.1	49.8	8.7	23.8	0.1
Mean _Y	78.7	77.0	77.6	79.9	80.1
Std. Dev. _Y	[10.63]	[11.20]	[10.51]	[10.20]	[9.36]
N	31,374	9,225	5,157	16,443	549
Municipality FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Note: Robust standard errors, clustered by municipality, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

²⁷As a preliminary step Figure A4 shows that voting access indeed decreases in all samples using either the binary or continuous exposure variables, reassuring on the validity of the operationalization of exposure to the reform.

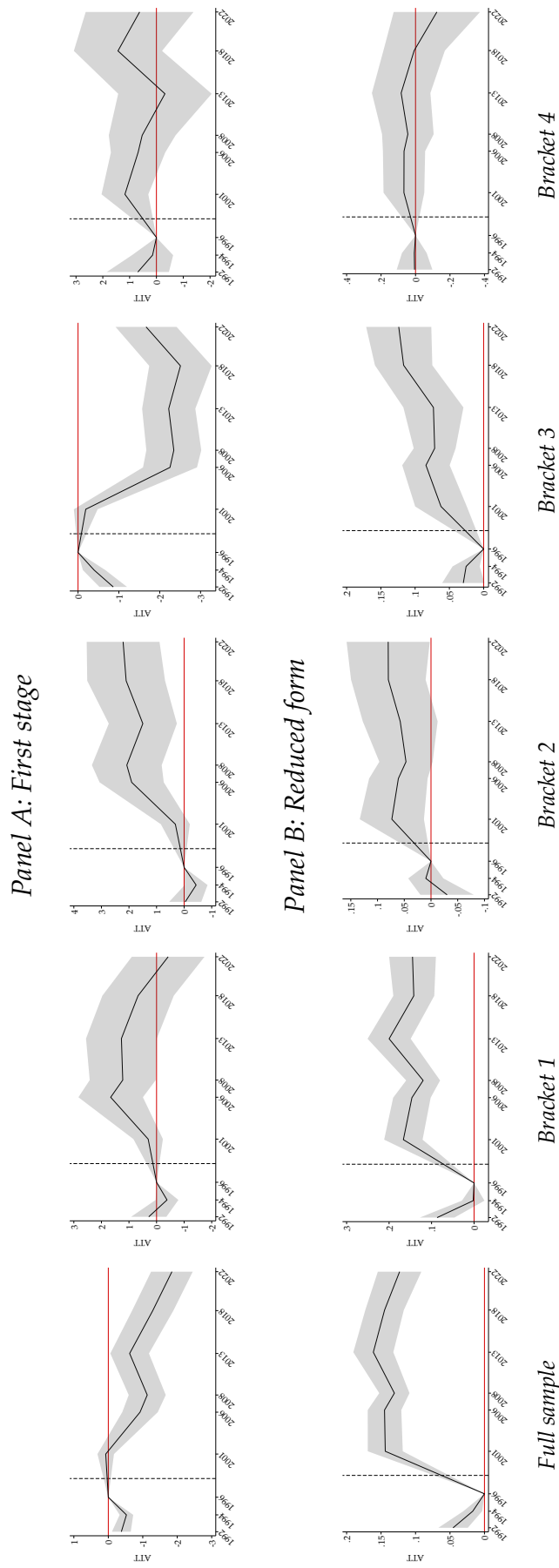
²⁸Using total valid votes rather than actual voters as numerator of turnout—i.e. removing (invalid) blank and null votes from votes to parties—leaves results almost identical, suggesting that the reform reduced participation and not party attachment conditional on voting (invalid votes are commonly interpreted as protest votes).

Panel A of [Figure 4](#), shows event studies with turnout as outcome. Although not required for identification of the LATE in the second stage ([de Chaisemartin & D'Haultfœuille 2018](#)), pre-trends are fairly parallel and substantially smaller in magnitude compared to the post-reform period. The dynamic first-stage effect is consistently negative in the full sample and in bracket 3. The non-significant effect in 2001 might be explained via the calculus of voting framework. Voters who turned out for the first time after the reform (being general elections significantly more attractive than lower or higher level elections occurred between 1998s1 and 2001) incurred the cost of either (i) discovering the new location of their polling station, or (ii) traveling longer distances to get to the new polling station, or (iii) facing unprecedented congestion at the polling station, especially during peak hours, or (iv) a combination of the three. Since voting is habit-forming ([Fujiwara et al. 2016](#); [Meredith 2009](#); [Gerber et al. 2003](#); [Coppock & Green 2016](#)), the experience of sudden and partly transitory higher costs in 2001 by such voters might have prevented them from turning out in future elections, shifting them from an inertial state of voting to one of non-voting ([Plutzer 2002](#)). These considerations help explain the long-lasting collapse in turnout in more exposed municipalities from the second electoral round after the reform to the end of the period.

6.2 Mechanisms of turnout decline

To substantiate the turnout effects of the reform I perform two complementary exercises using both geo-referenced individual-level data from the ITANES survey and precinct-level data from the municipality of Milan. The former exercise allows me to identify which voters are more sensitive in response to an increase in voting costs, while the latter informs how the contextual social and demographic correlates of turnout decline.

Fig. 4 Exposure to reform, turnout, and political position – Event studies



Notes: Dynamic difference-in-differences estimates with 95% confidence intervals from regressions of turnout (Panel A) and political position (Panel B) on exposure to reform interacted with election year dummies, by population bracket. The interaction in 1996, the last pre-reform election year, is omitted.

6.2.1 Micro-foundations of turnout decline

The working hypothesis is that a reduction in access raises the costs of voting and demobilizes voters. Yet, lower turnout is not observed suddenly in 2001 at the aggregate level. Thanks to a *de facto* longitudinal component of the survey, I can measure demobilization within individuals across the reform period by building a dummy equal to one if the respondent turns out to vote in 1996 but not in 2001. Individual demobilization is regressed on municipality-level exposure, characterizing this as a first-difference specification.

Table 4 presents results from analysis of respondents residing in any municipality (Panel A) or in a municipality in bracket 3 (Panel B)²⁹. Columns 1 and 2 show OLS estimates without and with individual socio-demographic controls (i.e. gender, age, education, marital status, labor force status, social class, migration status from residence of birth, and macro-region fixed effects). In Column 3 estimates are weighted by Inverse Probability of Treatment (IPTW), with weights derived by matching respondents in exposed vs not exposed municipalities over the vector of socio-demographic characteristics. Column 4 uses the doubly robust Augmented Inverse-Probability Weighted (AIPW) estimator that models both the probability of outcome and of treatment (Tan 2010). Finally, the table presents second-stage estimates from specifications where municipal exposure is used as instrument for percentage changes across the reform in polling station availability (Column 5) and congestion (Column 6). The positive coefficients in Columns 1 to 4 confirm that exposure to the reform increased the probability of voters demobilization by 2 to 3 percentage points. The last two columns show that the positive causal effect on demobilization is channeled by reduced voting access.

The ITANES survey collects a wide array of respondents' opinions and preferences on political and economic topics. I include these measures in the models to test by which dimension demobilized individuals in exposed vs non exposed municipalities differ. This allows me to assess who is the voter most sensitive to a voting-cost shock. Table 5 shows estimates of specifications as in Column 2 of Table 4, where municipal

²⁹See Table A6 for descriptive statistics of ITANES analytic sample.

Table 4 Exposure to reform, voting access, and demobilization – Individual-level estimates

	OLS		IPTW	AIPW	TSLS	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Full sample</i>						
Exposed	0.022** (0.010)	0.017* (0.010)	0.020** (0.009)	0.023*** (0.009)		
Availability					−0.019* (0.011)	
Congestion						0.008* (0.005)
<i>N</i>	1,867	1,834	1,834	1,834	1,834	1,769
KP <i>F</i> -stat					287.2	378.2
Mean _{<i>y</i>}	0.04	0.04	0.04	0.04	0.04	0.04
Std. Dev. _{<i>y</i>}	[0.19]	[0.19]	[0.19]	[0.19]	[0.19]	[0.19]
<i>Panel B: Bracket 3</i>						
Exposed	0.030*** (0.011)	0.025** (0.011)	0.030*** (0.010)	0.030*** (0.010)		
Availability					−0.026** (0.012)	
Congestion						0.011** (0.005)
<i>N</i>	1,019	1,007	1,007	1,007	1,007	963
KP <i>F</i> -stat					165.4	255.8
Mean _{<i>y</i>}	0.04	0.04	0.04	0.04	0.04	0.04
Std. Dev. _{<i>y</i>}	[0.19]	[0.19]	[0.19]	[0.19]	[0.19]	[0.19]
Controls	×	✓	✓	✓	✓	✓

Note: The dependent variable is a demobilization dummy equal to 1 if the respondent voted in 1996 and did not vote in 2001. Availability and congestion in Columns 5 and 6 are treated as endogenous regressors that quantify, respectively, a 10-percentage-point change in polling stations and eligible voters *per* polling station across 1998, instrumented by exposure to the reform. Robust standard errors in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

exposure is interacted with a series of moderating individual-level variables. The resulting profile of the demobilized voter is that of a disaffected marginal voter with economic grievances (Fowler 2015; Riker & Ordeshook 1968; Adams et al. 2006; Plutzer 2002; Brady et al. 1995). In particular, the most robust results suggest that the reform has greater demobilization effects among voters that (i) disengaged from politics³⁰, (ii) perceive low instrumental stakes in partisan competition³¹, (iii) are dissatisfied with the economic performance of the incumbent³², (iv) perceive economic issues as salient³³, (v) holds preferences for rapid one-man-leadership in place of parliamentary procedures³⁴.

This evidence characterizes the voters who drop out when a shock occurs to the cost of voting of the sort implied by a reduction in access. They are concentrated among those lying at the margin of electoral participation. These voters enter the calculus-of-voting framework (Riker & Ordeshook 1968) as individuals with low levels of political satisfaction from voting. In presence of a shock to voting cost common to all, they are the first to rationally expect negative rewards from voting, thus their decision to abstain.

6.2.2 Contextual heterogeneity in turnout decline

Precinct-level heterogeneity allows to understand whether the same voting-cost shock translates into larger turnout losses based on the prevalence of groups that face

³⁰The question asks: *'Has your interest in politics always been more or less the same as it is now, or were you more or less interested in the past?'*

³¹I compute the first principal component of answers to 9 questions asking *'In your opinion, who is better able to deal with the following issues: a center-left government or a center-right government?'* with respect to *'Having a functioning healthcare system', 'Addressing the immigration issue', 'Having a fairer tax system (i.e., fairer taxes)', 'Improving education', 'Fighting unemployment', 'Ensuring a functioning justice system', 'Making public administration more efficient', 'Fighting crime', 'Managing relations with Europe'.*

³²I compute the first principal component of answers to 4 questions asking *'Can you tell me now whether you think the actions of center-left governments have had very positive, fairly positive, neither positive nor negative, fairly negative, or very negative results'* with respect to *'The economic situation of Italy', 'Unemployment', 'Inflation'.*

³³I compute the first principal component of answers to 4 questions asking *'Now I will list a series of social and economic issues that have been discussed during the most recent election campaign. In your opinion, how important is each of these issues for Italy?'* with respect to *'Unemployment', 'The economic underdevelopment of the South', 'Inflation and rising prices'.*

³⁴The question asks: *'I will now read you some statements about politics and economics that are commonly made. For each one, please tell me whether you agree very much, quite a lot, a little, or not at all: Today in politics it is better for decisions to be made by a single person, and quickly, rather than following all parliamentary procedures'.*

Table 5 Moderators of demobilization – Individual-level estimates

Moderator:	Declining interest in politics	Non-partisan instrumental beliefs	Economic griev. twr. incumbent	Economic issues salience	Dissatis- faction with parliam.ism
	(1)	(2)	(3)	(4)	(5)
Exposed	−0.063** (0.031)	0.027** (0.013)	0.025** (0.012)	0.024** (0.011)	−0.009 (0.014)
Moderator	−0.008 (0.007)	−0.006 (0.008)	−0.013 (0.010)	−0.044* (0.026)	−0.001 (0.005)
Exp. × Mod.	0.043*** (0.015)	0.028** (0.013)	0.027** (0.012)	0.058** (0.027)	0.017** (0.008)
Controls	✓	✓	✓	✓	✓
N	1,006	834	913	980	960

Note: The dependent variable is a demobilization dummy equal to 1 if the respondent voted in 1996 and did not vote in 2001. The sample includes ITANES respondents residing in municipalities in bracket 3. Robust standard errors in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

different marginal costs. Being the second largest Italian city, Milan lies above the 500,000 residents threshold, and accordingly belongs to bracket 5 for reform implementation. The exposure margin for a precinct p is simply the distance in eligible voters, prior to the reform, from the 900 target. The analysis of turnout at the precinct-level that I carry out follows a continuous difference-in-differences model with precinct and year fixed effects, and takes the following form:

$$T_{p,t} = \delta(\{900 - EV_p^{1998}\} \times \mathbb{I}\{t > 1998\}) + \sigma_p + \tau_t + \nu_d \times \tau_t + \zeta_{p,t}$$

Coefficient δ captures larger intensity of exposure in the post-reform period (this margin is divided by 100, and thus expressed as hundredths of EV below the target), and district-by-year fixed effects $\nu_d \times \tau_t$ ensure that more and less exposed precincts are compared to each other only within districts-rounds in which the same local sets of candidates run.

Table 6 presents estimation results in samples that incrementally refine the over-time matching of precincts³⁵. In Column 1 precincts are matched from a round to the next

³⁵I exclude a few hospital polling stations (i.e. *sezioni ospedaliere*) that record only actual rather than

only based on their serial numbers, while in Column 2 I retain only precincts that belong to the same borough (i.e. *zone* or *municipi*)—a sub-municipal administrative unit— throughout the period. Column 3 retains precincts located in the same street from 2001 onward, while Column 4 keeps only those located at the same street number, i.e. for which a stable location over time is almost certain. Being below the target by an additional 100 EV significantly reduces voter participation between 0.38 and 0.56 percentage points.

Table 6 Precinct-level exposure and turnout - Difference-in-Differences estimates

	All precincts	Same borough	Close address	Same address
	(1)	(2)	(3)	(4)
Exposure $\times$ Post-1998	-0.388*** (0.137)	-0.392*** (0.142)	-0.540*** (0.202)	-0.557** (0.222)
Mean _Y	83.47	83.40	82.59	82.64
Std. Dev. _Y	[6.73]	[6.72]	[7.49]	[7.55]
<i>N</i>	8,993	8,599	4,521	4,018
Adj. <i>R</i> ²	0.82	0.83	0.85	0.85
Precinct FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Year $\times$ District FE	✓	✓	✓	✓

Note: Exposure is expressed as hundredths of eligible voters below the bracket 5 target in 1998s1. Robust standard errors, clustered by precinct, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Next, I augment the specification by interacting the difference-in-differences estimator with relevant contextual socio-demographics, i.e. define triple-differences models. Building on [Pinto \(2023\)](#) I compute pre-reform shares of population with a certain characteristic by spatially interpolating Istat census tracts over precinct polygons. [Figure 5](#) reports coefficient on the triple interaction term upon standardization of precinct-level socio-demographics. The results indicate that the turnout decline is attenuated in precincts with higher concentrations of prime-age residents (especially ages 35 to 54), higher educational attainment (upper-secondary and tertiary), students, and individuals active in the labor market, as well as in precincts with larger shares of foreign-born residents. Conversely, the reform's turnout decline is amplified in precincts with large eligible voters.

shares of women, older populations (65 and above), low educational attainment (no education or primary education), and greater inactivity and retirement³⁶.

Taken together, the individual- and precinct-level analyses provide complementary evidence on the channels through which reduced access depresses turnout. Individual-level evidence shows that demobilization concentrates among disengaged marginal voters, who perceive low benefits from participation. Precinct-level evidence shows that, in the same city, the shock produces larger turnout losses in local contexts characterized by higher shares of groups arguably facing higher voting costs or smaller benefits, and smaller losses in precincts with higher concentrations of socially and economically integrated residents. This combination of micro and contextual heterogeneity is consistent with voting-cost shocks disproportionately pushing low-benefit voters out of the electorate, especially where local composition and social structure provide fewer buffers against increased participation costs.

6.3 Electoral effects of turnout decline

6.3.1 Effects on political position

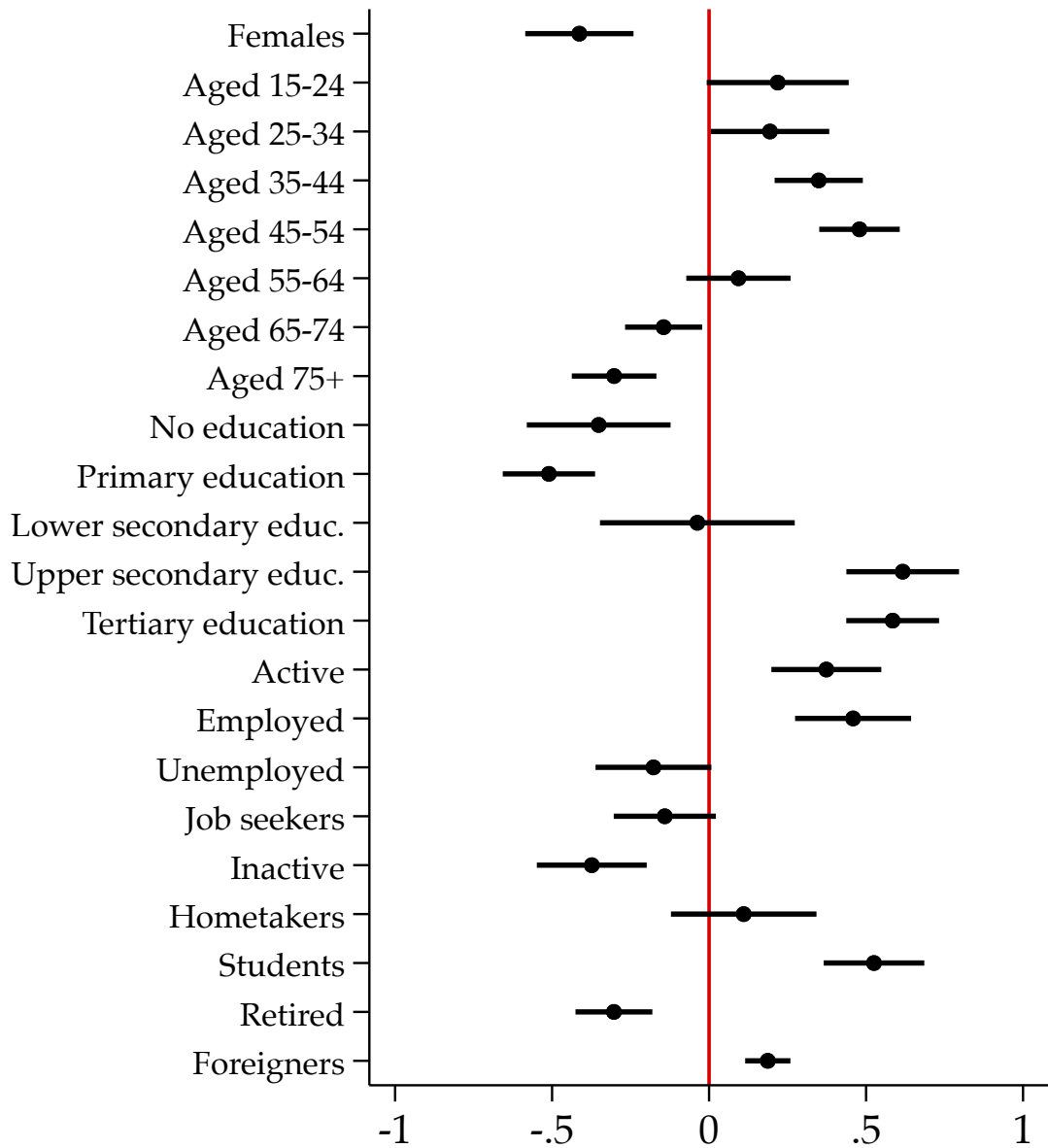
Table 7 estimates second-stage specifications described in Equation (3). In presence of negative first stages, the direction of second-stage coefficients must be flipped to correctly interpret the effects of the reform via turnout³⁷. Accordingly, the reduction in turnout observed in the full sample and in bracket 3 shifts the electorate to the right. To grasp the magnitude of effects I convert the estimates using beta coefficients: a 1-SD reduction in turnout increases left-right political position on a scale from 1 to 7 by 3.8 SDs in the full sample, and by .96 SDs in bracket 3. Results in brackets other than 3 are reported only for completeness, since they rely on weak and invalid first-stage metrics as highlighted in previous section.

Figure A5 shows the sensitivity of results to using alternative thresholds in the

³⁶Very similar patterns can be observed with analyses at the municipality level, exploiting variation in socio-demographics across municipalities in a triple-interaction version of Equation (2).

³⁷Because the instrument decreases turnout, a negative coefficient of turnout on outcome implies the reform-induced turnout decline increases the outcome.

Fig. 5 Precinct-level heterogeneity in turnout decline - Triple-difference estimates



Notes: Coefficients are estimated from separate precinct-level triple-differences models where the standardized share of people with characteristic listed on the left of the figure is interacted with the continuous difference-in-differences estimator. Black bars indicate 95% confidence intervals.

Table 7 Exposure to reform, turnout and LR score – Second-stage estimates

	Full sample	Bracket 1	Bracket 2	Bracket 3	Bracket 4
	(1)	(2)	(3)	(4)	(5)
Turnout	−0.185*** (0.063)	0.151 (0.092)	0.040** (0.017)	−0.049*** (0.014)	0.049 (0.151)
Mean _Y	4.18	4.29	4.17	4.13	3.92
Std. Dev. _Y	[0.51]	[0.50]	[0.50]	[0.52]	[0.37]
<i>N</i>	31,374	9,225	5,157	16,443	549
Municipality FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Note: Robust standard errors, clustered by municipality, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

distribution of EV for the assignment of the binary exposure status in bracket 3. The robustness of first- and second-stage estimates extends throughout and beyond the interquartile range of thresholds.

Table A3 shows results using a continuous margin as an interaction term in the DiD estimator, thus capturing the intensity of exposure to the reform. I compute it as the distance from the reform target of polling stations (for brackets 1 and 2) or (hundreds of) eligible voters *per* polling station (for brackets 3 and 4) in the last pre-reform period³⁸. Also in the continuous case results are strong and robust only in bracket 3, where turnout decreases by .79 points for every 100 voters below the minimum reform target, inducing, in turn, a rightward ideological shift by .059 units. Because the weighted average of ideology used as dependent variable could be influenced, for example, by extreme parties receiving many votes in an electoral round, I use also the ideology of the party at the median of the distribution. Table A4 reports estimates with median ideology as dependent variable. Results in full and bracket 3 samples are larger than baseline estimates and go in the same direction. This suggests that the rightward shift results from an overall movement of the ideological distribution rather than being

³⁸Equation (2) is modified as follows:

$$T_{i,t} = \beta(Exp_i \times \mathbb{I}\{t > 1998\}) + \mu_i + \nu_t + \varepsilon_{i,t}$$

where $Exp_i = V_i - V^*$ if $i \in \{b_1, b_2\}$ or $Exp_i = V^* - V_i$ if $i \in \{b_3, b_4, b_5\}$. Since the underlying quantities are very different across brackets, results in the full sample are not reported although significant and in the same direction of the binary case.

driven by extreme parties in specific municipality-elections.

6.3.2 Effects on party vote shares

Is the overall rightward shift of the electorate driven by dynamics in specific parts of the ideological distribution? To shed light on this, I substitute the overall political position as dependent variable with cumulative party vote shares by distinguishing parties in *moderate* (i.e. *centre-left*, *centre*, *centre-right*) as opposed to *extreme* positions (i.e. *left-wing* and *right-wing*). The sample is restricted to municipality-elections in which at least one party at each of 5 levels can be voted on ballots³⁹. Table 8 reports the corresponding second-stage estimates with municipalities in all brackets (odd-numbered columns) and in bracket 3 only (even-numbered columns). A decline in turnout benefits more moderate parties at the expenses of those farther away from the centre. The electoral penalty of a 1-point reduction in turnout for extreme party groups ranges between 2.5 and 6 percentage points⁴⁰.

Table 8 Turnout and vote shares by closeness to the centre – Second-stage estimates

	Extreme		Moderate	
	Full	Bracket 3	Full	Bracket 3
	(1)	(2)	(3)	(4)
Turnout	6.041*** (2.039)	2.447*** (0.637)	-6.254*** (2.109)	-2.460*** (0.635)
Mean _Y	36.74	36.71	62.67	62.64
Std. Dev. _Y	[19.37]	[19.81]	[19.56]	[20.02]
<i>N</i>	30,837	16,082	30,837	16,082
Municipality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: Dependent variables are cumulative vote shares obtained by parties closer to the centre of the political spectrum (centre-left, centre, centre-right) or to the extremes (left-wing, right-wing). Samples include municipality-elections in which parties from all 5 political position levels were running. Robust standard errors, clustered by municipality, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

³⁹Sample sizes drop significantly if I use all 7 levels of the Wikipedia scale separately because far-right and far-left parties do not run in all nine electoral rounds and in all municipalities. Therefore I collapse *far-left* and *left*, and *far-right* and *right* parties in two macro-levels to construct a 5-level scale.

⁴⁰The corresponding event studies are shown in Figure A6.

To assess the mechanisms underlying the decline in extremist vote *shares*, I decompose the effects of the reform in bracket 3 using vote *levels* relative to eligible voters as outcome variables in reduced-form regressions, and the fact that total valid votes to parties *per* eligible voter (v) equals the sum of extremist (e) and moderate votes (m) *per* eligible ($v = e + m$). Extremist votes *per* eligible decline by 3.9 pp, while moderate votes increase by 2.4 pp, resulting in an implied change in valid turnout of $v = -1.47$ that closely matches the directly estimated turnout effect (-1.44 pp in [Table 3](#)). As a consequence, the reduction in extremist vote shares cannot be explained by mechanical effects alone. Instead, two mechanisms operate simultaneously. First, extremist voters are disproportionately less likely to cast a vote probably due to demobilization. Second, vote reallocation among remaining participants shifts support toward moderate parties, conditional on turning out. Second-stage estimates based on vote shares thus capture the combined effects of turnout reductions and within-electorate vote reallocation, whereas vote levels *per* eligible voter provide more direct evidence on which groups gain or lose electoral support.

A back-of-the-envelope calculation based on pre-1998 means indicates that approximately 21% of the decline in extremist votes *per* eligible voter is attributable to reduced turnout, while roughly 67% is attributable to changes in vote shares among participants. The remaining difference reflects interaction effects and within-municipality heterogeneity.

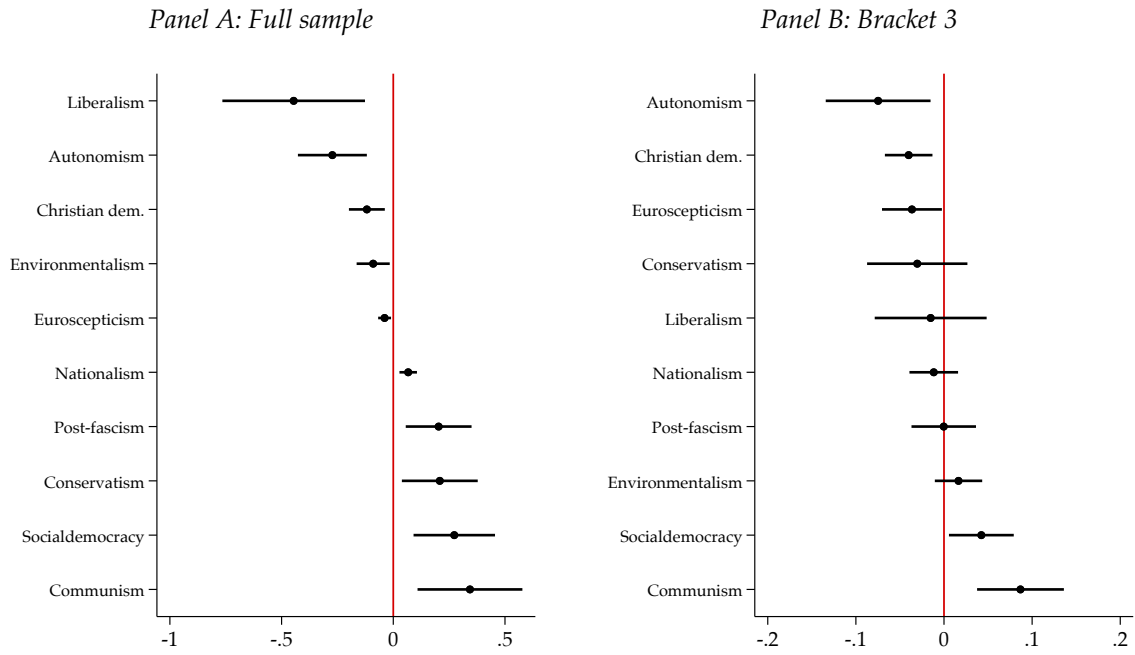
As shown in Appendix [Table A7](#) these spatial dynamics are non-linear in ideology, as they depend on heterogeneous movement on the left and on the right of the political spectrum. A reduction in turnout by 1 point has detrimental effects on vote shares for left-wing parties (which lose between 2.7 and 7.9 pp), while parties in the centre-left, centre, and right-wing thrive in low participation as they gain about 2.2, .5 and 0.3 pp respectively (in bracket 3)⁴¹.

Finally, I aggregate party vote shares by ideology tag from Wikipedia, I combine tags that point to the same core political ideology, and standardize the resulting vote

⁴¹Compositional data analysis that relies on vote share of any level as the benchmark for other vote shares reveals similar dynamics.

shares. [Figure 6](#) reports second-stage estimates for both samples, sorted in increasing order of electoral benefit from a turnout decline. The effects that are consistent across both samples are positive for autonomist, christian democratic, and eurosceptic parties, while negative for parties belonging to communism and social democracy.

Fig. 6 Turnout and vote shares by ideology tags – Second-stage estimates



Notes: The figure summarizes estimates of [Equation \(3\)](#) in municipalities in the full sample (Panel A) and in bracket 3 (Panel B) using as dependent variables aggregate party vote shares by Wikipedia-based ideology tags. Black bars indicate 95% confidence intervals.

6.4 Design validity and further robustness checks

The causal interpretation of these findings is strengthened by insights from Panel B of [Figure 4](#), where estimates from event studies are shown. Pre-trends in political positions are fairly parallel and substantially lower in magnitude compared to the post-reform period. The dynamic reduced-form effect appears sizable and long-lasting in all samples with the exception of bracket 4, although substantively linked to turnout decline only in the full sample and in bracket 3.

[Table A5](#) reports results obtained using alternative estimators with the procedure outlined in [de Chaisemartin & D'Haultfoeuille \(2018\)](#). The Wald-DID estimates in

the first line of the table rely on treatment effect stability and homogeneity, and are significantly negative both in the full sample and bracket 3 as in the baseline. Without imposing assumptions on treatment effects, Wald-TC estimates in line 2 require conditional parallel trends, whereas Wald-CIC estimates in line 3 require monotonicity and time invariance of unobservable characteristics. The alternative estimates are negative and significant both in the full sample and in bracket 3. Tests reported at the bottom of the table reject the null hypothesis of equality between pairs of estimators, indicating that the assumptions on treatment effects underlying the main results might be violated. However, the alternative estimators that do not rely on such assumptions are similar to the baseline in direction and significance, even if smaller in magnitude.

The choice to analyze national elections in a balanced sample of municipalities is supposed to hold constant supply-side electoral incentives and the demand-side exposure to them. Nevertheless, I adjust baseline estimates with covariates aimed at reducing further the scope for such confounding effects.

On the supply side, the list of parties running in a municipality depends on the electoral district the municipality is included in at each round. A higher number of parties increases ideological options for heterogeneous electorates, leading to better representative average ideology measures at the municipality level. At the same time, numerous symbols on the ballot might confound voters and lead them to unintended choices. While the inclusion of double fixed effects absorbs most of this variation, I add as time-varying covariates the number of parties running in each municipality-election and the cumulative vote share of parties whose position could be retrieved on Wikipedia.

On the demand side, relying on aggregate municipal data does not ensure that the composition of the electorate is stable across municipalities or over time. As a result, the estimated effects of the reform on participation and ideology might be biased depending on the sources of instability. First, voters from specific socio-demographic groups might be unevenly distributed within municipalities. A municipality-wide measure of voting access fails to account for different participation incentives at the baseline faced by

residentially segregated groups, and the related supply-side shock might only capture mechanically this uneven distribution. Second, municipal electorates are constantly subject to inflows and outflows. The voting-age population exposed to the reform in electoral round t might be different from that in $t+1$. As a remedy to these sources of instability, I adjust the econometric specifications with covariates described in the Data section: (i) pre-reform levels of residential dissimilarity⁴² interacted with the post-1998 dummy, and (ii) time-varying rates of deviation from the composition of pre-reform voting-age population.

Covariate-adjusted estimates are presented in [Table A2](#), including also deviations of temperature, precipitation and wind levels on election days from the respective averages over the previous month⁴³. Results in the full sample and in bracket 3 prove robust to the adjustment and provide evidence of a generalized demobilization pattern of voters induced by the reform. Second-stage coefficients increase in magnitude, revealing a previous downward bias.

7 Conclusion

This paper has examined the electoral consequences of a specific source of turnout decline: an austerity-driven restriction in access to voting. Exploiting a nationwide reform that mandated a large reduction in polling stations in Italy, I show that a fiscal-consolidation measure aimed at reducing administrative costs changed the infrastructure of electoral participation. By reducing polling-station availability and increasing the number of eligible voters per station, the reform raised the costs of voting and generated persistent political consequences.

The first result is that voting access matters for participation. Municipalities more exposed to the reform experienced lower turnout after implementation, and these gaps persisted over subsequent electoral rounds. This pattern is consistent with a

⁴²I add a dissimilarity index for each of the following characteristics of residents and houses: gender, age, citizenship, marital status, education, labor force status, job sector, type of employment, type of inactive status, housing occupancy, house construction date, and house size.

⁴³Including alternatively deviations from the previous week average or the previous day, or the simple levels on election days provide similar results.

standard calculus-of-voting logic in which higher participation costs reduce turnout among citizens close to the participation margin (Downs 1957; Riker & Ordeshook 1968; Palfrey & Rosenthal 1985). It is also consistent with work showing that voting is habit-forming: once citizens are pushed out of the electorate, abstention may become more likely in later elections. The results therefore suggest that administrative changes to the organization of voting can produce effects that outlast the immediate election in which access is curtailed.

The second result is that the turnout decline was selective. Using the *de facto* longitudinal component of the Italian National Election Study, I show that reform exposure increased the probability that individuals who voted before the reform abstained afterward. The voters most likely to be demobilized were not a random subset of the electorate. They were characterized by declining political interest, low perceived instrumental differences between partisan alternatives, economic grievances, and dissatisfaction with parliamentary decision-making. Complementary precinct-level evidence from Milan points in the same direction: turnout losses were larger in local contexts with higher shares of citizens likely to face greater participation costs or weaker attachment to electoral politics. These findings support the theoretical claim that voting-cost shocks operate at the participation margin. They shrink the electorate by pushing out citizens whose attachment to electoral participation is already fragile.

The third result is that this selective demobilization reshaped electoral outcomes. Lower turnout shifted the ideological center of gravity of the electorate to the right and redistributed vote shares away from parties at the extremes of the left–right scale toward more moderate parties. Disaggregating parties by position and ideology shows that the largest losses were concentrated among left-wing, communist, and social-democratic parties, while moderate and some center-right party families gained. These patterns clarify why the electoral consequences of turnout decline cannot be summarized only as a left–right effect. In a multiparty system, changes in participation can also alter the balance between extreme and moderate parties. Whether turnout decline moves the electorate left, right, or toward the center depends on who is demobilized.

The findings speak to several broader debates. First, they contribute to research on unequal participation and democratic representation. Classic work argues that low and socially skewed turnout biases representation against less-advantaged citizens (Lijphart 1997; Verba et al. 1995). This paper shows one institutional channel through which such skew can emerge: technical administrative decisions over polling-place access. Second, the results contribute to the literature on turnout and electoral outcomes. Existing work shows that participation-expanding reforms can change public spending, policy priorities, and partisan outcomes (Lizzeri & Persico 2004; Miller 2008; Fujiwara 2015; Fowler 2013), while weather-based evidence suggests that turnout-reducing shocks can have partisan consequences but raises concerns about exclusion restrictions (Gomez et al. 2007; Hansford & Gomez 2010; Mellon 2025). The Italian reform provides a different source of variation: a direct administrative contraction in voting access. Third, the paper contributes to the political economy of austerity and public-service retrenchment. Whereas much of this literature emphasizes attitudinal responses to perceived state withdrawal (Cremaschi et al. 2025; De Vries & Hobolt 2020), this paper identifies a distinct mechanism: fiscal consolidation can change politics by changing the cost of participation itself.

The analysis has limitations. The estimates identify the consequences of turnout decline generated by voting-access restrictions, not the effects of turnout decline in general. A decline caused by apathy, electoral timing, weakened party mobilization, or adverse weather may move different voters across the participation margin and produce different electoral consequences. The second-stage interpretation also relies on the assumption that the reform affected electoral outcomes primarily through turnout. The individual and precinct-level evidence supports this mechanism, but polling-station closures may also have signaled public-service retrenchment or state withdrawal to some voters. Finally, the analysis stops at electoral outcomes. Whether the observed changes in turnout and party support translated into changes in policy responsiveness remains an important question for future research.

The broader implication is that turnout decline should be understood as a composi-

tional shock to representation, not merely as a fall in participation. When the costs of voting rise, the electorate does not simply become smaller. It becomes different. In the case studied here, an austerity-driven contraction in voting access selectively demobilized marginal and disaffected voters, shifted support away from ideological extremes, and moved the electorate's ideological center of gravity to the right. A technical cut to electoral infrastructure therefore changed not only how many citizens voted, but which citizens were represented.

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Appendix A Additional Materials

Fig. A1 Polling stations and average eligible voters across the 1998 reform by municipal population brackets

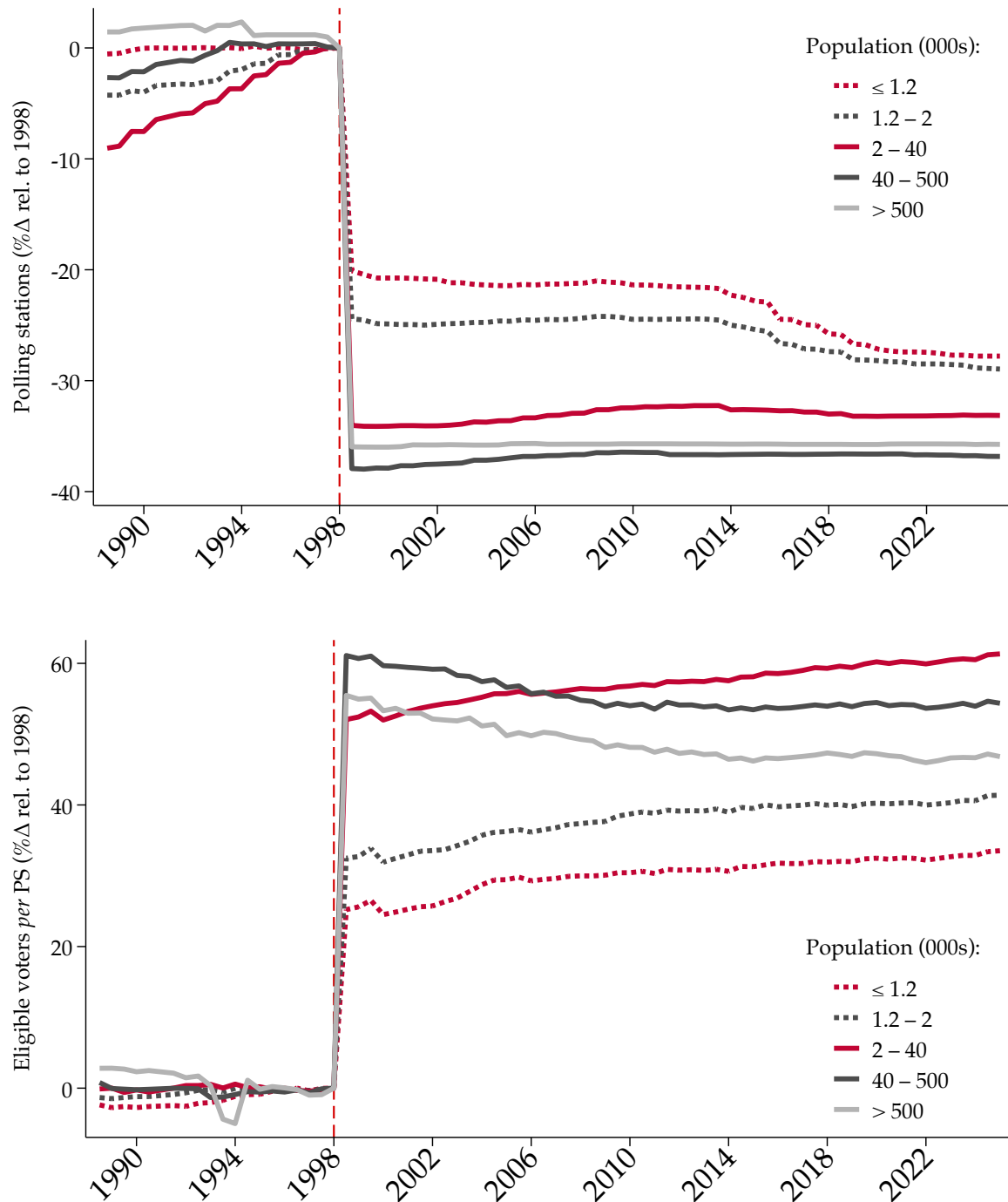
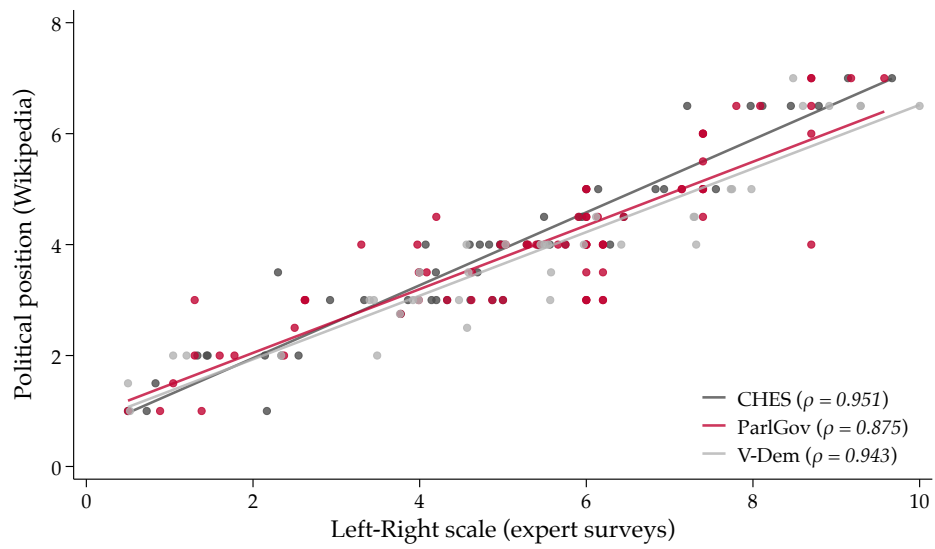
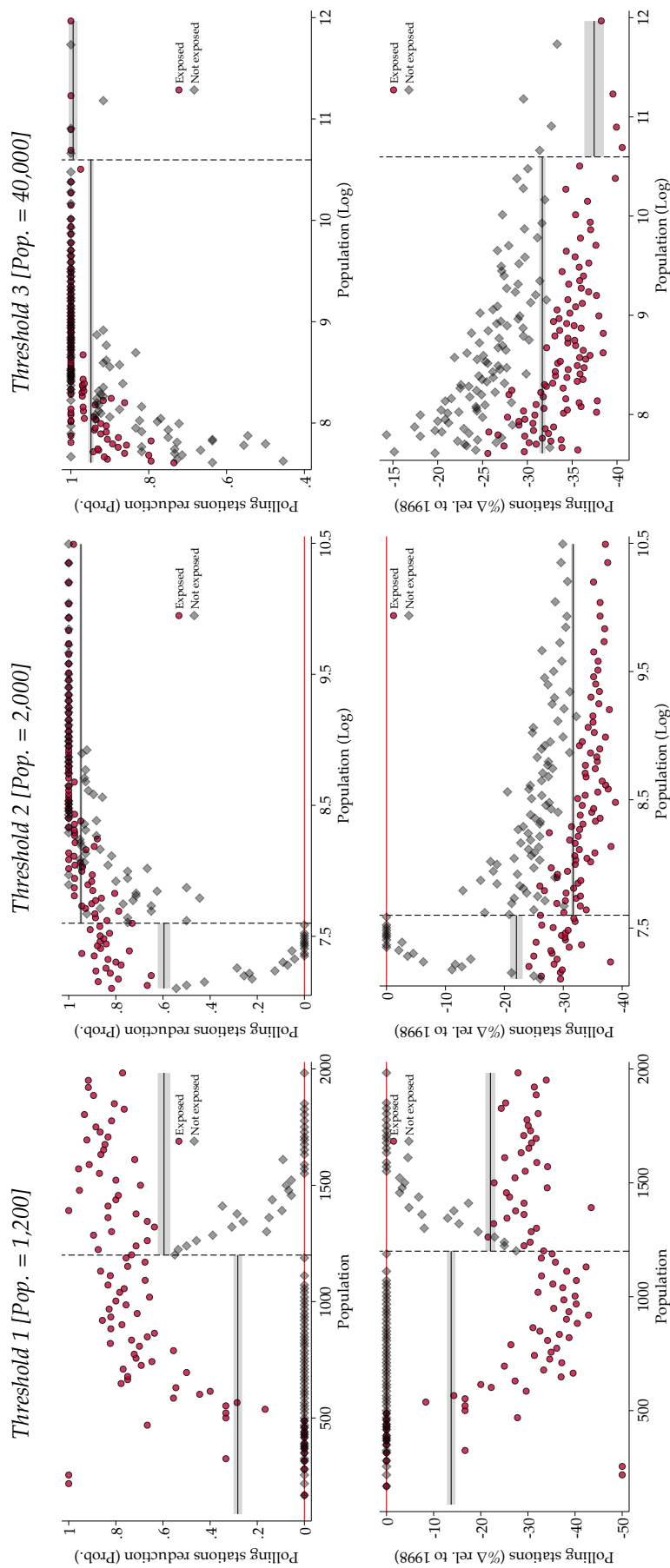


Fig. A2 Parties' ideology scores based on Wikipedia and expert surveys – Validation



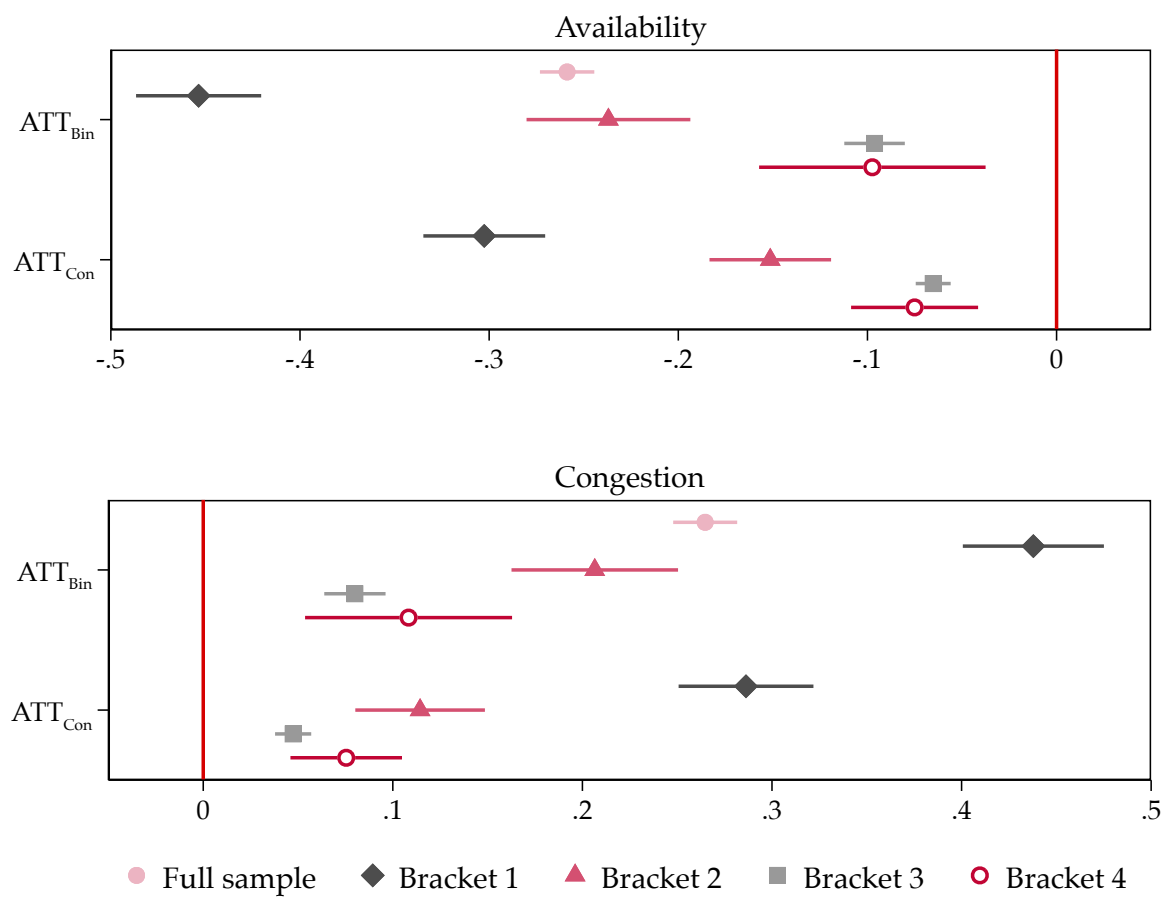
Notes: Chapel Hill Expert Survey ($N = 54$), Parliaments and Governments Database ($N = 73$), Varieties of Democracy project ($N = 37$).

Fig. A3 Polling stations reduction following the 1998 reform at reform population thresholds and by exposure status



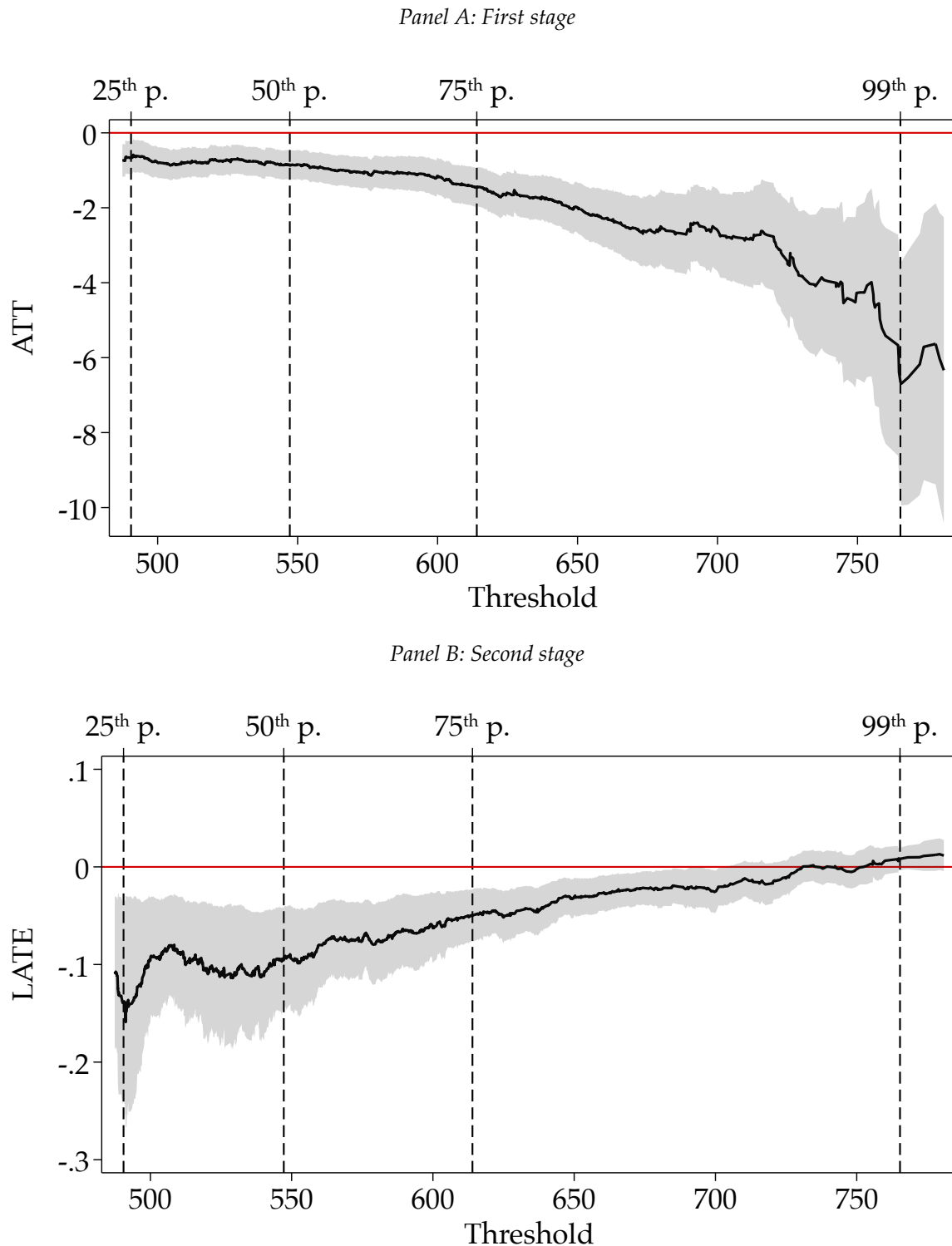
Notes: Binscatters of probability (top panel) and intensity (bottom panel) of polling stations change after the implementation of the reform. Red circles (grey diamonds) correspond to percentile bins of the distribution of population in mid-1998 in exposed (non exposed) municipalities. Vertical dash lines indicate population thresholds set for the implementation of the reform. Horizontal solid lines summarize average changes by population bracket, and are surrounded by the respective 95% confidence intervals in grey.

Fig. A4 Exposure to reform and voting access



Notes: The figure summarizes DiD estimates of both binary and continuous exposure to reform (respectively ATT_{Bin} and ATT_{Con}) on the two dimensions of voting access, availability (top panel) and congestion (bottom panel).

Fig. A5 Exposure to reform, turnout and ideology – Sensitivity to alternative exposure thresholds



Notes: The figure summarizes estimates of [Equation \(2\)](#) (Panel A) and [Equation \(3\)](#) (Panel B) in municipalities in bracket 3 sample using varying values in the distribution of pre-reform eligible voters *per* polling station as thresholds $V_{p_3}^*$ (reported on bottom horizontal axes) at different percentiles (reported on top horizontal axes). The threshold used for baseline estimates in Column 4 of [Tables 3](#) and [7](#) corresponds to the 75th percentile. Grey areas indicate 95% confidence intervals.

Table A1 Political position of parties in national elections (1992-2022)

Level	Position	Parties (N)	Ideol. tags (avg.)	Vote share (cum.)	Vote share (avg.)
1	Far-left	6	2.5	3.82	0.64
1.5	—	4	2.8	1.03	0.26
2	Left-wing	17	2.6	10.64	0.63
2.5	—	2	2.0	2.14	1.07
3	Centre-left	19	2.1	18.15	0.96
3.5	—	3	2.0	3.74	1.25
4	Centre	44	2.0	16.42	0.37
4.5	—	8	1.6	4.43	0.55
5	Centre-right	13	2.3	19.82	1.52
5.5	—	1	3.0	0.16	0.16
6	Right-wing	8	3.1	0.19	0.02
6.5	—	5	4.2	17.02	3.40
7	Far-right	13	3.8	1.58	0.12
Missing (with Wiki)		19	2.1	0.33	0.02
Missing (without Wiki)		112	—	0.51	0.00

Note: number of parties by political position from unique Wikipedia pages, including intermediate levels (i.e. when > 1 position reported); average number of ideology tags reported on Wikipedia by party; cumulative vote share is computed as number of votes received in the whole period by parties in the respective position over total valid votes (> 326 millions); positions such as *Big tent* are classified as *Centre*.

Table A2 Exposure to reform, turnout and ideology – Covariate-adjusted fuzzy DiD estimates

	Full sample	Bracket 1	Bracket 2	Bracket 3	Bracket 4
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: First stage (DV: Turnout)</i>					
Exposed $\times$ Post-1998	-0.498** (0.201)	0.450 (0.450)	1.394*** (0.466)	-1.416*** (0.253)	0.407 (0.682)
Adj. R^2	0.83	0.75	0.82	0.90	0.96
KP F -stat	6.1	1.0	8.9	31.5	0.4
AR Wald test	121.3	48.5	11.8	30.1	2.8
Mean _Y	78.7	77.0	77.6	79.9	80.1
Std. Dev. _Y	[10.63]	[11.21]	[10.51]	[10.20]	[9.36]
<i>Panel B: Second stage (DV: Ideology)</i>					
Turnout	-0.213** (0.089)	0.269 (0.270)	0.056** (0.024)	-0.052*** (0.013)	0.226 (0.332)
Mean _Y	4.18	4.29	4.17	4.13	3.92
Std. Dev. _Y	[0.51]	[0.50]	[0.50]	[0.52]	[0.37]
N	31,330	9,201	5,157	16,423	549
Municipality FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Note: Time-varying covariates included are: number of running parties, cumulative vote share of parties with a political position tag, rates of deviation from the composition of pre-reform voting-age population, deviation on the election day of temperature, precipitation and wind speed on election day from their averages over the previous month. Pre-reform levels of residential dissimilarity along several characteristics of residents and houses are added after interaction with the post-1998 dummy. Robust standard errors, clustered by municipality, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Table A3 Continuous exposure to reform, turnout and LR score – Fuzzy DiD estimates

	Bracket 1	Bracket 2	Bracket 3	Bracket 4
	(1)	(2)	(3)	(4)
<i>Panel A: First stage (DV: Turnout)</i>				
Exposed (cont.) $\times$ Post-1998	0.324 (0.337)	0.686** (0.305)	-0.786*** (0.128)	0.061 (0.390)
Adj. R^2	0.73	0.81	0.89	0.96
KP F -stat	0.9	5.1	37.9	0.0
AR Wald test	32.9	12.1	46.2	0.6
Mean _Y	77.0	77.6	79.9	80.1
Std. Dev. _Y	[11.20]	[10.51]	[10.20]	[9.36]
<i>Panel C: Second stage (DV: Ideology)</i>				
Turnout	0.223 (0.231)	0.069* (0.038)	-0.059*** (0.014)	-0.457 (3.273)
Mean _Y	4.29	4.17	4.13	3.92
Std. Dev. _Y	[0.50]	[0.50]	[0.52]	[0.37]
N	9,225	5,157	16,443	549
Municipality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: Robust standard errors, clustered by municipality, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Table A4 Exposure to reform, turnout and LR score (median) – Second-stage estimates

	Full sample	Bracket 1	Bracket 2	Bracket 3	Bracket 4
	(1)	(2)	(3)	(4)	(5)
Turnout	−0.223*** (0.077)	0.098 (0.069)	0.049* (0.025)	−0.099*** (0.025)	0.029 (0.367)
Mean _Y	4.18	4.34	4.19	4.09	3.80
Std. Dev. _Y	[0.87]	[0.83]	[0.83]	[0.90]	[0.71]
<i>N</i>	31,373	9,225	5,157	16,442	549
Municipality FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Note: Robust standard errors, clustered by municipality, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Table A5 Exposure to reform, turnout and ideology - Alternative fuzzy DiD estimates

	Full sample	Bracket 1	Bracket 2	Bracket 3	Bracket 4
	(1)	(2)	(3)	(4)	(5)
W_{DID}	-.187** (0.091)	.145 (.752)	.039** (.019)	-.048*** (.014)	.056 (1.010)
W_{TC}	-.018*** (.001)	-.021 (.003)	-.009** (.005)	-.010*** (.002)	-.014 (.009)
W_{CIC}	-.009*** (.001)	-.010 (.003)	-.004 (.004)	-.005*** (.002)	-.010 (.008)
N	31,374	9,225	5,157	16,443	549
$ W_{DID}-W_{TC} $	.169* (.001)	.166 (.003)	.048 (.005)	.038*** (.002)	.069 (.009)
$ W_{DID}-W_{CIC} $	.178* (.001)	.155 (.003)	.043 (.004)	.043*** (.002)	.066 (.008)

Note: Alternative Wald estimators outlined in [de Chaisemartin & D'Haultfœuille \(2018\)](#); block bootstrap standard errors by municipality using 200 replications in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Table A6 Descriptive statistics of ITANES respondents

	Obs.	Mean	SD	Min.	Max.
Demobilized	1,867	.039	.194	0	1
<i>Moderators</i>					
Declining political interest	1,864	2.024	0.600	1	3
Non-partisan instr. beliefs	1,578	−0.188	0.873	−0.823	1.633
Econ. grievances incumbent	1,716	−0.052	0.962	−2.532	2.019
Economic issues salience	1,827	−0.002	0.572	−3.051	0.479
Dissat. parliamentarianism	1,804	2.336	1.107	1	4
Female	1,867	1.448	0.497	1	2
<i>Age class</i>					
18 - 24	1,867	.033	.179	0	1
25 - 34	1,867	.223	.416	0	1
35 - 44	1,867	.191	.393	0	1
45 - 54	1,867	.192	.394	0	1
55 - 64	1,867	.172	.378	0	1
65 - 74	1,867	.127	.333	0	1
≥75	1,867	.062	.240	0	1
<i>Education</i>					
Primary	1,867	.237	.425	0	1
Lower secondary	1,867	.334	.472	0	1
Upper secondary	1,867	.321	.467	0	1
Tertiary	1,867	.108	.310	0	1
<i>Marital status</i>					
Single	1,867	.226	.418	0	1
Married	1,867	.645	.479	0	1
Co-habiting	1,867	.019	.138	0	1
Widowed	1,867	.066	.248	0	1
Separated	1,867	.031	.174	0	1
Divorced	1,867	.012	.110	0	1
<i>Occupation status</i>					
Employed	1,867	.511	.500	0	1
Unemployed/Looking	1,867	.050	.219	0	1
Homemaker	1,867	.133	.339	0	1
Student	1,867	.030	.171	0	1
Pensioner	1,867	.276	.447	0	1
<i>Social class</i>					
Bourgeoisie	1,834	.101	.302	0	1
Middle class	1,834	.244	.429	0	1
Urban petty bourg.	1,834	.207	.405	0	1
Agrarian petty bourg.	1,834	.031	.172	0	1
Working class	1,834	.417	.493	0	1
Internal migrant	1,867	.147	.354	0	1
<i>Macro-region of residence</i>					
North-west	1,867	.282	.450	0	1
North-east	1,867	.196	.397	0	1
Center	1,867	.223	.417	0	1
South	1,867	.205	.404	0	1
Islands	1,867	.094	.292	0	1
Municipal exposure	1,867	.780	.414	0	1
<i>Population bracket</i>					
1	1,867	.031	.172	0	1
2	1,867	.024	.153	0	1
3	1,867	.546	.498	0	1
4	1,867	.268	.443	0	1
5	1,867	.132	.338	0	1

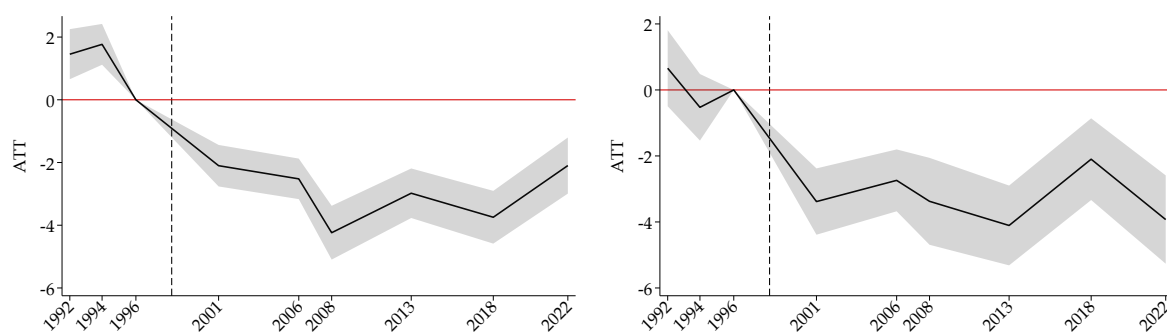
Table A7 Turnout and vote shares by level of political position - Second-stage estimates

	Left-wing	Centre-left	Centre	Centre-right	Right-wing
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Full sample</i>					
Turnout	7.866*** (2.634)	-5.050*** (1.725)	-0.035 (0.316)	-1.170** (0.503)	-1.825*** (0.664)
<i>N</i>	30,837	30,837	30,837	30,837	30,837
Mean _Y	15.87	22.08	18.03	22.56	20.87
Std. Dev. _Y	[15.05]	[14.34]	[18.49]	[13.76]	[13.95]
<i>Panel B: Bracket 3</i>					
Turnout	2.760*** (0.709)	-2.155*** (0.556)	-0.472** (0.202)	0.167 (0.182)	-0.313* (0.175)
<i>N</i>	16,082	16,082	16,082	16,082	16,082
Mean _Y	16.82	22.39	18.07	22.18	19.89
Std. Dev. _Y	[16.13]	[14.68]	[18.29]	[13.70]	[13.52]
Municipality FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Note: Dependent variables are cumulative vote shares obtained by parties belonging to specific levels of ideology. Left-(Right-)wing includes Far-left(-right) party vote shares. Samples include municipality-elections in which parties from all 5 political position levels were running. Robust standard errors, clustered by municipality, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Fig. A6 Exposure to reform and extreme parties' vote share – Event studies



Notes: Dynamic difference-in-differences estimates with 95% confidence intervals from regressions of extreme party vote shares on exposure to reform interacted with election year dummies, by population bracket. The interaction in 1996, the last pre-reform election year, is omitted.

Appendix B Additional Results

Table B8 Voting access and turnout - OLS estimates

	Full sample	Thres. 1	Thres. 2	Thres. 3	Thres. 4
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Availability</i>					
Polling stations (Log)	1.059*** (0.363)	0.038 (0.495)	0.606 (0.406)	1.430*** (0.449)	-1.399 (2.398)
Adj. R^2	0.82	0.76	0.87	0.89	0.96
<i>Panel B: Congestion</i>					
Eligible voters (Log)	-4.097*** (0.423)	-2.849*** (0.568)	-3.509*** (0.405)	-4.367*** (0.536)	-0.049 (2.552)
Adj. R^2	0.83	0.76	0.87	0.89	0.96
N	29,916	13,482	20,763	16,434	540
Municipality FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Mean _Y	78.67	77.19	79.36	79.92	80.06
Std. Dev. _Y	[10.63]	[10.96]	[10.32]	[10.17]	[9.36]

Note: Robust standard errors, clustered by municipality, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Table B9 Exposure to reform, turnout and ideology – Fuzzy DiD estimates

	Stacked	Threshold 1	Threshold 2	Threshold 3
	(1)	(2)	(3)	(4)
<i>Panel A: Reduced form (DV: Ideology)</i>				
Higher exp. × Post-1998	0.065*** (0.005)	0.095*** (0.015)	0.054*** (0.014)	−0.037 (0.030)
Adj. R^2	0.83	0.80	0.82	0.83
<i>Panel B: First stage (DV: Turnout)</i>				
Higher exp. × Post-1998	−1.165*** (0.116)	−0.596* (0.353)	−1.513*** (0.275)	−2.135*** (0.292)
Adj. R^2	0.85	0.76	0.87	0.89
KP F -stat	101.4	2.9	30.3	53.4
AR Wald test	147.2	40.3	15.3	1.5
Mean _Y	78.95	77.19	79.36	79.92
Std. Dev. _Y	[10.51]	[10.96]	[10.32]	[10.17]
<i>Panel C: Second stage (DV: Ideology)</i>				
Turnout	−0.056*** (0.007)	−0.159 (0.099)	−0.036*** (0.011)	0.017 (0.014)
Mean _Y	4.16	4.25	4.14	4.12
Std. Dev. _Y	[0.51]	[0.51]	[0.51]	[0.51]
N	52,974	14,382	21,600	16,992
Municipality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: Column 1 includes sample-by-time fixed effects. Robust standard errors, clustered by municipality, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Table B10 Turnout and vote shares by closeness to the centre – Second-stage estimates

	Extreme		Moderate	
	Stacked	Thresh. 2	Stacked	Thresh. 2
	(1)	(2)	(3)	(4)
Turnout	2.510*** (0.299)	1.837*** (0.479)	-2.642*** (0.311)	-1.991*** (0.497)
Mean _Y	36.69	36.62	62.71	62.76
Std. Dev. _Y	[19.49]	[19.68]	[19.69]	[19.88]
<i>N</i>	51,998	21,161	51,998	21,161
Municipality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: Dependent variables are cumulative vote shares obtained by parties closer to the centre of the political spectrum (centre-left, centre, centre-right) or to the extremes (left-wing, right-wing). Samples include municipality-elections in which parties from all 5 political position levels were running. Robust standard errors, clustered by municipality, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Table B11 Turnout and vote shares by level of political position - Second-stage estimates

	Left-wing	Centre-left	Centre	Centre-right	Right-wing
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Stacked sample</i>					
Turnout	2.799*** (0.336)	-1.726*** (0.231)	-0.647*** (0.105)	-0.269*** (0.087)	-0.290*** (0.091)
<i>N</i>	51,998	51,998	51,998	51,998	51,998
Mean _Y	16.16	22.18	18.07	22.46	20.54
Std. Dev. _Y	[15.35]	[14.43]	[18.49]	[13.74]	[13.83]
<i>Panel B: Threshold 2</i>					
Turnout	1.973*** (0.530)	-1.208*** (0.382)	-0.762*** (0.206)	-0.022 (0.154)	-0.136 (0.161)
<i>N</i>	21,161	21,161	21,161	21,161	21,161
Mean _Y	16.58	22.32	18.14	22.30	20.04
Std. Dev. _Y	[15.78]	[14.55]	[18.49]	[13.72]	[13.63]
Municipality FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓

Note: Dependent variables are cumulative vote shares obtained by parties belonging to specific levels of ideology. Left-(Right-)wing includes Far-left(-right) party vote shares. Samples include municipality-elections in which parties from all 5 political position levels were running. Robust standard errors, clustered by municipality, in parentheses.
 * $p < .1$; ** $p < .05$; *** $p < .01$.

Table B12 Exposure to reform, turnout and ideology – Alternative fuzzy DiD estimates

	Stacked sample	Threshold 1	Threshold 2	Threshold 3
	(1)	(2)	(3)	(4)
W_{DID}	−0.025*** (0.002)	−0.018*** (0.002)	−0.026*** (0.002)	−0.027*** (0.002)
W_{TC}	−0.030*** (0.002)	−0.024*** (0.002)	−0.032*** (0.003)	−0.029*** (0.004)
W_{CIC}	−0.029*** (0.003)	−0.025*** (0.003)	−0.033*** (0.004)	−0.027*** (0.004)
N	50,679	13,482	20,763	16,434
$ W_{DID}-W_{TC} $	0.005*	0.007*	0.006**	0.002
$ W_{DID}-W_{CIC} $	0.005	0.007*	0.007*	0.000

Note: Alternative Wald estimators outlined in [de Chaisemartin & D’Haultfœuille \(2018\)](#); block bootstrap standard errors by municipality using 200 replications in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Table B13 Exposure to reform, turnout and ideology – Covariate-adjusted fuzzy DiD estimates

	Stacked	Threshold 1	Threshold 2	Threshold 3
	(1)	(2)	(3)	(4)
<i>Panel A: Reduced form (DV: Ideology)</i>				
Higher exp. × Post-1998	0.062*** (0.006)	0.078*** (0.015)	0.061*** (0.013)	0.055* (0.029)
<i>N</i>	52,910	14,358	21,580	16,972
Adj. <i>R</i> ²	0.84	0.81	0.84	0.85
<i>Panel B: First stage (DV: Turnout)</i>				
Higher exp. × Post-1998	-1.129*** (0.123)	-0.667* (0.352)	-1.303*** (0.268)	-2.245*** (0.395)
Adj. <i>R</i> ²	0.86	0.78	0.88	0.90
KP <i>F</i> -stat	84.7	3.6	23.7	32.3
AR Wald test	113.4	26.9	21.0	3.5
Mean _Y	78.95	77.19	79.36	79.93
Std. Dev. _Y	[10.51]	[10.96]	[10.32]	[10.17]
<i>Panel C: Second stage (DV: Ideology)</i>				
Turnout	-0.055*** (0.008)	-0.117* (0.067)	-0.047*** (0.014)	-0.024 (0.015)
Mean _Y	4.16	4.25	4.14	4.12
Std. Dev. _Y	[0.51]	[0.51]	[0.51]	[0.51]
<i>N</i>	52,910	14,358	21,580	16,972
Municipality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: Time-varying covariates included are: number of running parties, cumulative vote share of parties with ideology score, rates of deviation from the composition of pre-reform voting-age population, deviation of temperature, precipitation and wind speed on election day from their averages over the previous month. Pre-reform levels of residential dissimilarity along several characteristics of residents and houses are added after interaction with the post-1998 dummy. Column 1 includes sample-by-time fixed effects. Robust standard errors, clustered by municipality, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Table B14 Exposure to reform, turnout and LR score (median) – Fuzzy DiD estimates

	Stacked	Threshold 1	Threshold 2	Threshold 3
	(1)	(2)	(3)	(4)
<i>Panel A: Reduced form (DV: Median Ideology)</i>				
Higher exp. × Post-1998	0.068*** (0.010)	0.052** (0.024)	0.119*** (0.023)	−0.211*** (0.061)
Adj. R^2	0.62	0.57	0.61	0.62
<i>Panel B: Second stage (DV: Median Ideology)</i>				
Turnout	−0.058*** (0.010)	−0.087 (0.065)	−0.079*** (0.021)	0.099*** (0.027)
Mean _Y	4.15	4.29	4.12	4.09
Std. Dev. _Y	[0.88]	[0.83]	[0.88]	[0.89]
N	52,972	14,382	21,599	16,991
Municipality FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Note: Column 1 includes sample-by-time fixed effects. Robust standard errors, clustered by municipality, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$.

Chapter 2

The Demographic Impact of Leadership:

Mayoral Education and COVID-19

Excess Mortality in Italy

The Demographic Impact of Leadership

Mayoral Education and COVID-19

Excess Mortality in Italy

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Abstract

During the COVID-19 pandemic, excess mortality rose sharply across Europe, with striking territorial disparities even within countries. This paper examines whether the characteristics of locally elected officials influenced excess mortality in Italy—one of the first countries severely affected. Using weekly death statistics and a Bayesian hierarchical spatio-temporal model, we estimate excess mortality at the municipal level. To address endogeneity in political selection, we exploit close mayoral elections within a Regression Discontinuity Design. Results show that college-educated mayors significantly reduced excess mortality during the first wave by lowering both the likelihood and the rate of excess deaths. These effects disappear in the second wave. Analysis of newly collected large-scale datasets on municipal policy activity and social media communication points to two channels: timely and more targeted local policy responses, and more engaging public communication with distinctive tone. Interpreting mayoral education as a proxy for leadership capacity, the findings highlight how political selection can shape local demographic vulnerability and population resilience during acute mortality shocks.

Keywords – Political selection, COVID-19, Mortality, Regression discontinuity design

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1 Introduction

The COVID-19 pandemic profoundly affected demographic dynamics, with mortality—though not the only domain—being the most directly impacted ([Dowd et al. 2020](#); [Aassve et al. 2020](#)). Cross-country and within-country comparisons have identified multiple factors shaping regional variation in COVID-related mortality. These include inequality, urbanization, demographic composition, healthcare quality, climate conditions, and policy failures ([Sorci et al. 2020](#); [Chang et al. 2022](#); [Díaz Ramírez et al. 2022](#); [Foster et al. 2024](#)).

In recent decades, demographers have increasingly acknowledged that mortality is not driven solely by biological or socioeconomic determinants but is deeply embedded in institutional and political contexts ([Sasson 2025](#)). A new agenda in mortality research highlights the importance of political determinants of health, that is, how governance, accountability, and policy frameworks shape population health outcomes ([Raalte 2021](#); [Navarro et al. 2006](#)). Political processes and institutions shape how resources and risks are distributed. Policy responses are, in turn, influenced by competing interests and forms of power, with direct implications for population health ([Ottersen et al. 2014](#); [McCartney et al. 2019](#); [Primrose et al. 2024](#)). [Dawes \(2020\)](#) provides a clear formulation, identifying voting, government, and policy as key channels through which political systems influence health inequalities. More specifically, within spatial and subnational analyses, variation in mortality has been linked to differences in state-level welfare generosity, political leadership, and institutional capacity (e.g. [Macmillan et al. 2018](#)).

Comparative evidence has also highlighted the potential role of political variables in mediating the effects of COVID-19, particularly across national contexts ([Martín-Martín et al. 2024](#)). Relevant political dimensions that may have influenced excess mortality include the quality of democratic institutions (e.g., government effectiveness; [Rodríguez-Pose & Burlina 2021](#)), reelection incentives (e.g., timing relative to the next election; [Pulejo & Querubin 2021](#)), the ideological orientation of elected officials (e.g., populist vs. non-populist; [Charron et al. 2023](#)), and the personal characteristics of

political leaders (e.g., gender; [Aldrich & Lotito 2020](#)).

These strands suggest that who governs at local levels may matter for health outcomes; however, the demographic literature has seldom operationalized local political traits—such as the education or decision-making capacity of local leaders—as independent predictors of mortality. By focusing on mayoral education and leveraging quasi-experimental identification, this paper contributes to bridging this gap, adding a fine-grained, local-level political dimension to population health research.

A related and largely unexplored issue concerns the causal interpretation of this relationship. The observed correlation between politicians' attributes and COVID-related mortality may reflect selection rather than causal effects, since politicians are elected by voters whose characteristics may themselves influence the spread of the pandemic. Place-specific unobservable dimensions may thus confound both political selection and spatial variation in COVID deaths. For instance, [Chauvin \(2024\)](#) finds that Brazilian municipalities with both higher population density and greater income experienced higher mortality. Yet, larger and wealthier cities are also more likely to elect very different types of mayors than smaller or poorer municipalities.

We employ a close-election Regression Discontinuity Design (RDD) to address these endogeneity concerns and estimate the causal effect of narrowly electing a college-educated mayor, relative to a non-college-educated opponent, on the containment of mortality during the COVID-19 crisis in Italy. Although political ability is a multidimensional trait, education captures a relevant component, reflecting both human capital accumulation ([Becker 1962](#)) and signaling ([Spence 1973](#)). Detecting a non-zero effect would suggest that in a period of acute crisis there is a significant demographic impact in municipalities headed by a type of politician with a bundle of characteristics proxied by education ([Marshall 2024](#); [Bertoli & Hazlett 2025](#)). Indeed, more skilled politicians may perform better at managing crises—for example, by designing and implementing more effective policies under high uncertainty.

Italy serves as our testing ground, being the second country in the world, after China, to confront the onset of the pandemic, experiencing its full force early on and undergo-

ing distinct waves. We leverage daily official statistics and apply the methodology by [Blangiardo et al. \(2020\)](#) to estimate excess mortality at the municipality level across the weeks of 2020 and 2021. We also use official data on the personal characteristics of mayors to measure political selection at the municipality level. Especially in the early phase of the pandemic, mayors played a key role in coordinating the local response to the emergency—by implementing restrictive measures on mobility, mask usage, and public space closures, and by informing citizens about effective ways to prevent contagion. [Ravenda et al. \(2023\)](#) document the importance of COVID-19 communication by Italian municipalities on their Facebook pages, covering both restrictive and support measures. Moreover, they find that the tone and effectiveness of this communication crucially depended on mayoral characteristics, such as age, ideology, and reelection incentives. Their findings underscore that mayors had tools to make a difference. Building on these insights, we move beyond a surface interpretation of education and examine what college-educated leadership meant in practice. To explore mechanisms, we collect original large-scale data on municipal policy activity and social media communication. Analyzing these new text corpora with natural language processing methods, we test whether educated mayors differed in two key domains where local leaders had concrete margins of action.

Our results show that local political selection indeed mattered. We find that college-educated mayors reduced excess mortality at both the extensive and intensive margins. College-educated mayors who narrowly won close elections—compared to non-college-educated mayors who also won by a narrow margin—reduced the probability that their municipality experienced excess deaths by 22% (extensive margin), and the excess mortality rate by 65% (intensive margin). These effects are observed exclusively during the first wave, underscoring the importance of sound decision-making when facing unprecedented challenges and limited central guidance. These effects are not driven by other politician characteristics that may be compounded with education, such as gender, age, political orientation, and experience. The analysis of mechanisms suggests that college-educated mayors pursued more timely and targeted policy implementation, and

adopted public communication characterized by greater engagement and a distinctive tone. Overall, our findings provide strong evidence that political selection, as captured by mayors' education, shaped the mortality toll of the pandemic, underscoring the importance of integrating political determinants of health into demographic analyses of population crises.

2 Determinants of Excess Mortality

A large body of research has examined why excess mortality varied widely across regions during the COVID-19 pandemic. Major differences have been attributed to a range of structural, socioeconomic, environmental, and political factors. Health system capacity played a critical role in shaping mortality outcomes, particularly in the early phase of the pandemic. Areas with fewer hospital beds or limited medical staff faced greater challenges in treating patients effectively. In parallel, the preparedness and resilience of public health infrastructure influenced the ability to implement response measures and to ensure continuity in essential services ([Kontis et al. 2020](#)).

Several demographic and environmental factors were associated with higher excess mortality in the first year of the pandemic. These include high population density, air pollution, and larger shares of elderly residents ([Díaz Ramírez et al. 2022](#)). Features of the built environment—such as overcrowded housing and reliance on public transportation—further shaped virus transmission ([Davies et al. 2021](#)). Pre-existing inequalities amplified the pandemic's toll. Vulnerable populations—such as racial and ethnic minorities, the elderly, and those facing poverty or chronic health conditions—suffered disproportionately high levels of excess mortality ([Foster et al. 2024](#); [Kontis et al. 2020](#)). Excess mortality was not simply driven by urban density, but by the socioeconomic conditions more prevalent in urban settings: poverty, overcrowded housing, and increased exposure through essential work roles ([Davies et al. 2021](#)).

Political decisions and institutional capacity have also been shown to play a role. Areas that implemented timely and stringent measures—such as lockdowns, widespread

testing, contact tracing, and isolation—were more successful in limiting virus spread and easing pressure on healthcare systems (Kontis et al. 2020). Regional variation in institutional quality was also associated with differential pandemic impacts. Notably, low and declining government effectiveness at the national level was positively correlated with excess mortality in the first half of 2020 (Rodríguez-Pose & Burlina 2021). Additionally, excess mortality was higher where citizen distrust in institutions was greater. Polarization in institutional trust—often fueled by political fragmentation—was associated with worse health outcomes (Charron et al. 2023). Regarding politicians' characteristics, countries governed by populist leaders showed higher excess mortality rates than those led by non-populist governments (Bayerlein et al. 2021).

However, the causal interpretation of most studies on the political determinants of COVID-related mortality relies on a “selection on observables” assumption (i.e., that all relevant differences across cases can be measured and controlled for). While these studies provide valuable evidence on the political determinants of COVID-related mortality, they leave open questions about causality.

3 Why Politicians Matter

Empirical studies—using different sources of exogenous variation, from accidental leader deaths to random gender quotas—have shown that political selection matters both for economic growth at the national level (Jones & Olken 2005) and for public good provision at the local level (Chattopadhyay & Duflo 2004). Fujiwara (2015) shows that the introduction of electronic voting in Brazil changed political selection by reducing invalid votes disproportionately cast by low educated voters, thereby affecting the composition of the electorate favoring the election of legislators more responsive to health policy. This shift, in turn, led to improvements in infant health outcomes, underscoring how the identity of elected officials can influence demographic indicators. Macmillan et al. (2018) show that higher female parliamentary representation is associated with lower mortality, especially in lower-income and less democratic countries, suggesting

that politician characteristics might play a role in shaping population health. Closest to our study, (Chauvin & Tricaud 2024) show that female mayors adopted different local containment policies during COVID-19, with corresponding differences in mortality.

The educational background of politicians emerges as a key element investigated by previous studies on political selection. In political economy, many studies have used education as a proxy for politicians' ability, together with administrative experience, market income, military IQ tests, and in-office performance indicators (Avellaneda 2009; Galasso & Nannicini 2011; Besley & Reynal-Querol 2011; Besley et al. 2011; Gagliarducci & Nannicini 2013; Baltrunaite et al. 2014; Dal Bó et al. 2017; Bordignon et al. 2020; Carreri 2021; Galasso & Nannicini 2024)¹. Leaders with higher levels of education have been associated with better governance and stronger economic performance, likely through more informed policy decisions (Besley et al. 2011; Dal Bó & Finan 2018).

These studies typically interpret politicians' education as a proxy for human capital, civic engagement, and ultimately ability. Yet, Carnes & Lupu (2016) question the empirical validity of this assumption. Analyzing Latin American legislators, they find limited and often ambiguous evidence linking college degrees to legislative performance. Their study, however, focuses on a specific political context and type of legislative activity. Moreover, existing performance indicators typically capture routine governance, leaving open the question of how political performance operates during crises that directly affect population outcomes. More broadly, we interpret education as a proxy for a bundle of politician characteristics (Marshall 2024), and our theoretical prior is that this bundle may causally affect the quality of policy choices during a pandemic emergency, as high educated local leaders may differ from others both in *preferences* and *skills*.

First, just as high educated individuals held different preferences regarding the nature and timing of pandemic policy responses, high educated politicians may also have held distinct beliefs and preferences in this respect. For instance, studies have shown a positive correlation between educational attainment and vaccine acceptance in different contexts (Schwarzinger et al. 2021; Lazarus et al. 2021; Callaghan et al. 2021).

¹See Dal Bó & Finan (2018) for a review of the literature.

In addition, education has emerged as a powerful barrier against the spread of anti-scientific skepticism on social media—a phenomenon that is, in turn, associated with lower compliance with public health measures and higher excess mortality (Beccari et al. 2024). This indirect role of education might extend to the realm of political leadership—particularly at the local level, where leaders must make critical decisions during health emergencies mainly based on their beliefs and preferences.²

Second, high educated politicians may possess a set of skills that enhance the quality of pandemic policy responses. They may be better able to process complex epidemiological information, coordinate with experts and bureaucrats, and identify which measures require rapid local implementation. Research on COVID-19 non-pharmaceutical interventions shows that the timing and design of policy responses were central to limiting transmission and mortality (Flaxman et al. 2020; Haug et al. 2020; Amuedo-Dorantes et al. 2021).

They may also exhibit stronger communication and leadership abilities, which facilitate effective information dissemination and community mobilization during a public health crisis (Van Bavel et al. 2020). Indeed, research shows that clear and credible communication is crucial for managing emergencies and fostering public understanding of complex situations (Lammers et al. 2020). This argument requires us to move beyond treating education as a surface-level attribute. A large estimated effect of mayoral education would be substantively incomplete without zooming on what educated mayors actually did differently. We focus on two domains in which Italian mayors had concrete margins of action during the pandemic: policy activity and public communication.

First, college-educated mayors may have been more likely to recognize the severity of the shock, process epidemiological information, consult technical expertise, and activate municipal administrative tools in a more timely manner. Italian mayors did not

²Outside the political realm, demographic research consistently finds that, compared with less-educated individuals, those with higher educational attainment tend to experience better health outcomes, lower mortality rates, and reduced health risks across a wide range of indicators (Baker et al. 2011; Baker et al. 2017; Montez et al. 2019). Education can also exert indirect effects on mortality: for example, maternal education has been shown to play a protective role in children's health and survival (Desai & Alva 1998; Ware 1984; Wu 2022).

control hospitals or national lockdown rules, but they did control or influence a wide set of local actions, such as emergency ordinances, civil protection coordination, procurement of protective equipment, adaptation of municipal services, school transport and school-space arrangements, food support, local welfare interventions, public-space arrangements. These actions may not individually be equivalent to a national lockdown, but together they can affect exposure, compliance, access to support, and the speed with which vulnerable residents receive assistance.

Second, pandemics require mass behavioral adaptation. Populations must understand risks, update beliefs as information changes, comply with restrictions, avoid unnecessary contacts, use available services appropriately, and trust that authorities are responding competently. Communication is therefore part of the policy response itself. Crisis and emergency risk communication conceptualizes public communication during emergencies as a multidimensional process that shapes knowledge, beliefs, emotions, trust, and protective behavior under conditions of uncertainty ([Reynolds & Seeger 2005](#); [Seeger et al. 2018](#)). Public health guidance similarly treats risk communication as a core component of emergency response because credible and actionable information helps people understand risks and take protective action ([WHO 2017](#); [CDC 2018](#)). Research shows that protective behavior during pandemics is associated with perceived severity, perceived susceptibility, beliefs about response efficacy, anxiety, and trust in authorities ([Bish & Michie 2010](#); [Berg et al. 2021](#)). This framework motivates our attention to the tone of mayoral communication. In a crisis, the same substantive information can be presented through different communicative registers, each with different implications for threat perception, uncertainty reduction, compliance, and collective action.

Next, we discuss why Italy offers an ideal testing ground for these hypotheses.

4 Italian Institutional Setting

4.1 The COVID-19 Emergency and Municipal Governance

Italy was among the first European countries severely hit by COVID-19. After the first cases were detected in January 2020 (see [Table 1](#)), the government declared a national state of emergency and rapidly imposed strict containment measures. Following local outbreaks in the northern regions, nationwide lockdown restrictions were introduced on March 9, including school closures, bans on gatherings, and the suspension of non-essential activities. By the end of March, all-cause mortality had risen by nearly 50 percent, with stark territorial disparities—peaking at +568 percent in the province of Bergamo—and a concentration of deaths among older adults ([Istat 2020](#)).

After a temporary relaxation of restrictions during the summer of 2020, a second wave emerged in the autumn, leading to renewed curfews, school closures, and the introduction of a regional tiered system (yellow, orange, and red zones) based on epidemiological indicators. Subsequent phases were marked by alternating restrictions and reopenings until the vaccination campaign, initiated in early 2021, progressively reduced mortality and pressure on the health system.

Although the formal state of emergency lasted until March 2022, the period between March and May 2020 represented the most acute phase of the crisis, with mortality reaching unprecedented levels and displaying marked subnational heterogeneity.

As of 2020, Italy counted 7,904 municipalities. Italian municipalities represent the lowest administrative unit, nested in 107 provinces, and, in turn, in 20 regional governments. The mayor has executive power and carries out several tasks prescribed by the Italian law, such as supervising the organization of municipal services and the execution of laws; appointing the members of the executive committee (who cooperate with the mayor in the administration of delegated functions); issuing executive decrees; and appointing external professionals.

Municipal elections take place every five years. Voters directly elect the mayor. The

Table 1 Key Phases of the COVID-19 Pandemic in Italy (2020–2022)

Period	Main Measures	Epidemiological Context	Demographic Impact
Jan–Feb 2020	First cases detected; national state of emergency declared; local lockdowns in northern municipalities	Local outbreaks (Lombardy, Veneto)	Early localized excess mortality
Mar–May 2020 (First Wave)	Nationwide lockdown (March 9); suspension of non-essential activities; mobility restrictions	Rapid surge of cases and deaths; health system overload	+49% all-cause deaths by end of March; extreme spatial heterogeneity (+568% in Bergamo)
Jun–Sep 2020	Gradual reopening; intra-regional and inter-regional mobility restored	Low infection rates	Temporary normalization
Oct 2020–Jan 2021 (Second Wave)	Renewed restrictions; curfew and regional tier system (yellow/orange/red)	Nationwide diffusion of infections	Second peak in mortality; more geographically uniform
Feb–Dec 2021	Vaccination campaign; “Green Pass” introduced for access to public venues	Progressive reduction in mortality and hospital strain	Mortality decline, concentrated among older cohorts
Jan–Mar 2022	Gradual phase-out of restrictions; end of national emergency (March 31)	Stable epidemiological indicators	Return to pre-pandemic mortality patterns

Sources: [Istat \(2020\)](#); [Istat \(2022\)](#); Ministry of Health decrees.

electoral system varies depending on the size of the municipality. In municipalities with fewer than 15,000 residents, a single-round system is used: each mayoral candidate is linked to a single list of council candidates endorsing him/her, and the candidate receiving a relative majority of votes becomes mayor. A majority bonus is awarded, granting two-thirds of the city council seats to the winning list. In municipalities with 15,000 residents or more, a two-round runoff system applies: if no candidate receives an absolute majority in the first round, the top two candidates compete in

a second round. Each mayoral candidate is linked to one or more council lists, and the winning coalition receives at least 60% of the council seats. Mayors are subject to a two-consecutive-term limit: after completing two consecutive terms, they are ineligible to run again immediately, unless at least one of the two terms lasted less than two and a half years. Since 2014, a third consecutive term is permitted in municipalities with fewer than 3,000 residents.

5 Data

5.1 Estimation of Excess Deaths

Accurately estimating the death toll of the pandemic is complicated by several factors. Relying on official COVID-19 death counts can lead to biased estimates of pandemic severity, as it overlooks interactions among causes of death ([Castro et al. 2023](#)) and is affected by inconsistencies in testing strategies, case reporting, death certification, and other health system variables—issues documented both across and within countries ([Buonanno et al. 2020](#)). These challenges are particularly salient in our setting, where such reporting issues may be endogenously related to the way local elected officials manage and organize public services. As is standard in epidemiological research ([Leon et al. 2020](#); [Woolf et al. 2021](#)), a common strategy to address these limitations is to measure excess all-cause mortality. We adopt this approach precisely because of the endogeneity concerns mentioned above, and in particular we follow the methodology proposed by [Blangiardo et al. \(2020\)](#).

We estimate COVID-related deaths by measuring excess all-cause deaths throughout the pandemic period: deaths in excess (or deficit) are computed as the difference between (i) all-cause deaths observed during the pandemic and (ii) counterfactual deaths that would have occurred absent the pandemic, estimated using a Bayesian model of municipality-level mortality based on data from 2016–2019. Mortality data is retrieved from the Italian Institute of Statistics (Istat), which makes available daily series of all-cause death counts by municipality, gender, and age class starting in 2011

(Istat 2023).

Given the granular spatio-temporal resolution required for our analysis, we adopt a disease mapping approach (Lawson & Lee 2017) to estimate counterfactual deaths, leveraging the methodology presented in Blangiardo et al. (2020) to measure excess mortality at the sub-national level. Disease mapping combined with Bayesian estimation is particularly well-suited to producing stable estimates in low-population areas while accounting for spatial, temporal, and spatio-temporal dependencies (Waller et al. 1997).

In particular, following Blangiardo et al. (2020), we specify a Bayesian hierarchical model and assume that death counts y in municipality i , nested in province j , observed in week t of year k , separately by gender, follow a Poisson distribution with relative risk of death ρ and offset E :

$$y_{ijtk} \sim \text{Poisson}(E_{ijtk} \cdot \rho_{ijtk}).$$

The offset term E measures the expected number of deaths in each municipality-week during COVID-19 years, adjusted for age structure through indirect standardization using the overall Italian population as reference. Indirectly standardized weekly expected deaths by municipality-year are computed as follows:

$$E_{ijtk} = \sum_a \left(P_{ijka} \cdot \frac{D_a}{P_a} \right) / 52,$$

where P_{ijka} are population counts by age group $a = \{0-14, 15-24, 25-34, 35-44, 45-54, 55-64, 65-74, 75+\}$ in municipality i and COVID-19 year k , while D_a and P_a are, respectively, death and population counts by age group a at the country level over the pre-COVID-19 period (2016–2019) (following Nepomuceno et al. 2022). Dividing yearly expected deaths by 52 produces a weekly series. The relative risk of death ρ_{ijtk} is estimated using the following log-linear random effects model of risk factors:

$$\log(\rho_{ijtk}) = \beta_{0k} + u_i + v_j + \omega_{jt} + f(x_{it}),$$

where the risk factor $\beta_{0k} = \beta_0 + \varepsilon_k$ is an intercept term that captures year k 's deviation ε_k , specified as a yearly random effect such that $\varepsilon_k \sim N(0, \sigma_\varepsilon)$, from the average relative

risk β_0 . Factors u_i and v_i are included to smooth the over-dispersion in the relative risk of mortality across a large number of municipalities with varying size (Besag et al. 1991): u_i is an intrinsic conditional auto-regressive component that captures spatial autocorrelation between municipality i and neighboring municipalities, whereas v_i is an unstructured random effect that captures non-spatial heterogeneity. The conditional distribution of the spatial random effect for municipality i has the following form:

$$u_i | \mathbf{u}_{-i} \sim N \left(\frac{\sum_j w_{ij} u_j}{\sum_j w_{ij}}, \frac{\sigma_u^2}{\sum_j w_{ij}} \right),$$

where $w_{ij} = 1$ if municipalities i and j are contiguous (otherwise $w_{ij} = 0$), so that μ_{u_i} corresponds to the average of random effects across neighbors, and σ_{u_i} decreases in the number of neighbors. The unstructured random effect is distributed as:

$$v_i \sim N(0, \sigma_v^2).$$

The relative risk model includes weekly random effects for each province following a first-order random walk, ω_{jt} , and a non-linear function of weekly temperature in each municipality following a second-order random walk, x_{it} (Blangiardo et al. 2020).

After selecting priors for all parameters and fitting the Bayesian model using data up to 2019, we draw 1,000 random samples through Monte Carlo simulation from the posterior distribution to predict counterfactual weekly death counts by municipality throughout the COVID-19 period.³ Model fit and predictions are performed operationally in R using INLA, Integrated Nested Laplace Approximation (Blangiardo & Cameletti 2015). Finally, excess deaths are measured as follows:

$$y_{itk}^e = y_{itk} - \hat{y}_{itk},$$

where y_{itk} are observed death counts in municipality i and week t of COVID-19 years, and $\hat{y}_{itk}$ are counterfactual death counts predicted using the Bayesian model fitted on pre-COVID-19 data and averaged across 1,000 simulations.

We adopt both a binary and a continuous measure of COVID-related mortality

³Inverse variances of the parameters of risk factors are modeled through a distribution $\log\text{-Gamma}(1, 0.1)$. As for the global intercept, $\beta_0 \sim N(0, 10^6)$.

based on excess deaths. First, we define the extensive margin of excess mortality as a dummy variable taking value one if excess deaths are larger than zero, that is, if observed deaths exceed predicted deaths ($y_{itk} > \hat{y}_{itk}$). Second, we define the intensive margin of excess mortality using the excess mortality rate, computed as the number of excess deaths *per* 100,000 residents in a municipality. We expect the first measure to reduce measurement error, and the second to add some granularity to our analysis. In the empirical analysis, we aggregate municipal excess deaths by distinguishing three periods: (i) a pre-COVID-19 (placebo) period covering weeks 1 to 6 of 2020 (January 1 to February 11); (ii) the first wave, spanning weeks 11 to 18 of 2020 (March 11 to May 5); and (iii) the second wave, extending from week 40 of 2020 to week 19 of 2021 (September 30, 2020 to May 13, 2021).

5.2 Electoral and Mayoral Data

Election outcomes are gathered from the [Ministry of Internal Affairs \(2025\)](#), which provides open data on the universe of municipal elections in Italy since 1989. For each municipal election, we collect candidates' full names, their endorsing lists/parties, and the number of votes cast for each candidate and list/party.⁴ We focus on municipalities whose elected mayor (i) won in a race with at least another running candidate, and (ii) was in office when the pandemic started in March 2020. Thus, for each municipality we identify the most recent valid election before March 2020 and drop municipalities where legislatures were terminated earlier (e.g., due to the mayor's resignation or death, or dissolution upon organized crime infiltration).

Data on elected mayors and mayoral candidates is retrieved from [Ministry of Internal Affairs \(2025\)](#), which lists the characteristics of local administrators by year and municipality. When missing, key characteristics are retrieved from the online search engine on local administrators. We match candidates' election outcomes to their characteristics using full names—available in both datasets—as identifiers after standardizing them. The characteristics include: educational attainment; administrative experience

⁴For municipalities in four regions with special autonomy (i.e., Aosta Valley, Friuli-Venezia Giulia, Sicily, and Trentino-Alto Adige), the same data is gathered from regional electoral offices.

(i.e., time in any municipal office); gender; age on election day; incumbency; place of birth (i.e., local candidate if municipality of birth and election coincide); ideology (i.e., affiliation to a left/center/right party or civic list); and populist orientation (i.e., affiliation to a party classified as populist by PopuList, <https://popu-list.org>). To estimate the causal effect of mayoral education via RDD, we restrict our main analysis to a sample of mixed-education electoral races—that is, elections where the top two candidates differ in education. This analytical sample consists of 2,545 observations. Descriptive statistics on mayoral characteristics are reported in Appendix Table A1.⁵ Figure A6 shows the geographical distribution of municipalities in our analytical sample, differently labeling those headed by high and low educated mayors.

5.3 Municipality-Level Covariates

We compile a rich dataset of municipality characteristics observed in the most recent pre-COVID-19 period, relying on a variety of institutional sources. Demographic covariates are constructed using either Istat (2025) or Istat (2025) and include the number of residents, population density, average household size, and shares of the population that are aged 65 and over, women, foreigners, living in households, or living in collective residences. Geographical information is retrieved from Istat (2025) and includes the macroregion of reference (North-West, North-East, Center, South, Islands), altitude, coastal proximity, level of urbanization (i.e., city, town, or rural municipality). We derive variables on education and employment from Istat (2025), including shares of the population by educational attainment, employment status (employed, out of the labor force), firm size (i.e., average number of workers per firm), and shares of workers by job sector (e.g., manufacturing, construction, trade).

Descriptive statistics of municipality-level variables in our analytic sample—including excess mortality outcomes by COVID-19 period—are reported in Appendix Table A2. Tables A3 and A4 further compare the descriptive statistics of mayors and municipi-

⁵Municipal elections were held in late 2020 in 13% of our municipalities. To ensure consistency in political leadership across pandemic waves, we exclude these municipalities from the analysis of the second wave, as their mayors in office may have changed compared to the first wave. Using the same sample in the second-wave analysis does not affect the results.

policies in the analytic sample to those in the full sample with non-missing values for all variables (6,906 observations). Excluding tertiary education, which is mechanically higher in the analytic sample due to the focus on mixed-education races, all mayoral and municipal characteristics are strikingly similar across the two samples. This similarity reinforces the external validity of our results, at least within the Italian context.

5.4 Policy and Communication

To shed light on the mechanisms through which mayors may have shaped excess mortality, we assemble two original large-scale municipality-level datasets on policy and communication activity during the pandemic collected via web-scraping and analyzed through natural language processing.

Policy. We begin by scraping the municipal webpages used to publish official administrative acts. We retrieve 22,658,805 acts from 2,882 municipal pages with standardized domains, spanning 1980–2025. The acts cover five document types: deliberations (*delibere*), executive orders (*determine*), mayoral ordinances (*ordinanze*), decrees (*decreti*), and payment authorizations (*liquidazioni*). Focusing on the COVID period, we retain a corpus of 5,157,918 acts issued between 2019 and 2021 by 2,325 municipalities⁶ that produced at least one act before and after February 2020—a restriction we impose to ensure that municipalities were consistently issuing acts across the pre- and post-onset period—and for which we observe key metadata including date of publication, document type, issuing office, and object description. COVID-related acts are identified through a dictionary-based classifier that we developed iteratively to minimize false positives and negatives. The classifier: (i) detects core COVID-related tokens in the act’s object field (e.g., *covid*, *coronavirus*, *sars-cov-2*, *quarantena*, *isolamento*, *vaccino*), (ii) flags acts containing formal emergency language co-occurring with epidemic context

⁶Municipal webpages differ substantially in HTML structure, content-management systems, and archival organization, making uniform scraping unfeasible. As a result, our data collection does not cover all Italian municipalities, but only those with sufficiently accessible and machine-readable online archives. They represent approximately 30% of all Italian municipalities, and only 788 match with our RD sample. This prevents us from carrying out the same RD analysis on these policy variables in a directly comparable way.

words (e.g., *ordinanza* or *zona rossa/gialla/bianca* jointly with *epidemia*, *pandemia*, *contagio*, *sanitario*), and (iii) excludes false positives related to non-COVID isolation or vaccination or animal-health emergencies. Applied to the 2019–2021 corpus, this classifier identifies 150,254 COVID-related acts, equal to 2.91% of all acts in this window. Objects of COVID-related acts are slightly longer than the rest of acts (they contain on average 25.7 words and 185.5 characters, against 21.2 and 150.3).

For text analysis, we pre-process the object field of COVID-related acts to isolate substantive policy content. We follow standard NLP practice: normalize text, standardize Unicode, lowercase all strings, remove URLs, email addresses, digits, and residual formatting noise. We then strip Italian stopwords, time expressions, legal-administrative boilerplate, organizational suffixes, and generic COVID labels that are no longer informative once a document has already been classified as pandemic-related. We further detect frequent collocations using a Gensim Phrases model trained on the COVID corpus, requiring a minimum frequency of 50 occurrences and a phrase-formation threshold of 12, so that recurrent multiword expressions are treated as single tokens (e.g. *solidarieta_alimentare*, *protezione_civile*, *trasporto_scolastico*, and *dispositivi_protezione*).

We estimate topics using a BERTopic model with multilingual sentence-transformer embeddings, UMAP for dimensionality reduction, and HDBSCAN for clustering. To reduce the share of mechanically unassigned documents, we reallocate initial outliers only when a document lies sufficiently close, in embedding space, to the centroid of an existing non-outlier topic. We then evaluate candidate reduced-topic solutions on fixed development subsets using coherence-based criteria and retain the final specifications selected by that procedure. The retained solution contains 24 topics which capture distinct domains of municipal COVID-related activity—including food support, closures and containment measures, protective equipment procurement, remote working, school-space adaptation, testing, school transport, and public-health measures.

These data are used to construct municipal measures of COVID policymaking. First, we examine COVID-related policy adoption separately by pandemic wave. Second, we analyze differences in policy content by measuring topic-specific outcomes. Finally, we

study the timing of COVID policymaking.

Communication. To capture institutional communication with the public, we scraped posts from the official Facebook pages of Italian municipalities using Facebook Graph API and covering the period January 2020 to August 2021. The raw corpus includes 1,065,643 posts from 1,679 municipal pages retrieved using Facebook page IDs collected manually by prioritizing municipalities in our analytic sample. After restricting the sample to pages that published at least one post before and after February 2020, the final corpus comprises 975,000 posts from 1,282 municipalities. For each post we distinguish original publications from reposts of third-party content, and we record user engagement counts – shares, reactions, and comments.

To identify COVID-related posts, we apply a dictionary-based classifier similar to the one used for administrative acts, tailored to the less standardized and noisier register of social media. We refine the classifier iteratively until we reach a satisfying solution with minimized false positives and false negatives. The classifier identifies 233,828 COVID-related posts, equal to 23.9% of all posts. Relative to non-COVID posts, COVID-related posts are longer (they contain on average 131.1 words and 894.9 characters, against 76.2 and 520.6), more likely to be original posts rather than reposts (61.5% against 57.7%), and receive more user engagement (13.5 shares, 34.9 reactions, and 3.57 comments, against 10.7, 31.2, and 2.6).

For text analysis, we preprocess posts to extract four complementary sets of communication measures from COVID-related posts. First, we recover substantive communication topics by adapting a BERTopic model similar to the one used for administrative acts to the characteristics of the social media corpus (longer, noisier, less standardized, more conversational texts), obtaining a solution with 17 topics from the COVID corpus. Second, we construct tone measures for COVID-related posts using a weakly supervised classification procedure that combines multilingual sentence-transformer embeddings with semantic prototypes and lexical gates to reduce false positives. We encode preprocessed posts using a multilingual sentence-transformer model that attaches a semantic score based on cosine similarity between each post and positive/negative

prototype statements for six, theory-driven communicative tones: risk-alerting (7.2%), reassuring-efficacy (1.2%), administrative-instructional (34.3%), directive-compliance (12.2%), empathetic (4.9%), and mobilizing-action (3.8%). We classify posts into tones using tone-specific thresholds chosen adaptively from the score distributions. For the analysis, we use the dominant tone of COVID-related posts assigned to the highest-scoring tone among those exceeding its tone-specific threshold. The resulting variables are the shares of COVID-related posts whose dominant tone is risk-alerting, reassuring-efficacy, administrative-instructional, directive-compliance, empathetic, or mobilizing-action. Third, we measure sentiment by adopting a dictionary-based affective register scorer that counts positive and negative evaluative stems and phrases, scaled by post length. This produces post-level positive sentiment, negative sentiment, and net polarity scores averaged by municipality-wave⁷. Finally, we process also user engagement measures: we compute fractions of reposts, average shares, reactions and comments by post per 100 residents (comparing originality and engagement across COVID and non-COVID-related posts). The resulting dataset allows us to study not only *what* municipalities communicated during the pandemic, but also *how* they communicated.

6 Empirical Strategy

To quantify the effect of mayoral education on mortality outcomes, we adopt a sharp RDD based on a politician characteristic. Specifically, we model excess mortality $Y_i(E_i)$ in municipality i as a function of the mayor's education, where $E_i = 1$ if the mayor is high educated and $E_i = 0$ otherwise. Our outcome variable, observed excess mortality Y_i , corresponds to one of the two potential outcomes $Y_i(1)$ or $Y_i(0)$ —that is, the excess mortality that would be observed under a more or low educated mayor, respectively. The focus is on close elections in which the first two ranked candidates differ in educational attainment, i.e., mixed-education elections. Education is dichotomized: candidates are classified as low educated if they have not attained tertiary education

⁷As a robustness check, we also compute sentiment using FEEL-IT, a validated classifier for Italian social-media text trained on Italian Twitter posts and benchmarked for sentiment and emotion classification (Bianchi et al. 2021).

(i.e. college-level education), and as high educated if they have completed tertiary education or higher. The candidate who obtains a positive margin of victory over the other in the decisive ballot is the elected mayor. The margin of victory MV_i —which serves as the RDD running variable—is defined as the difference between the vote share of the high educated candidate and the low educated one. Thus, $MV_i \geq 0$ if the high educated candidate wins, and $MV_i < 0$ if they lose. A zero margin represents the cutoff value in our RD design. Mayoral education in each municipality is then a deterministic function of this margin: $E_i = \mathbb{I}\{MV_i \geq 0\}$. Accordingly, observed excess mortality in municipality i can be expressed as a function of the two potential outcomes (Imbens & Rubin 2015):

$$Y_i = \begin{cases} Y_i(1) & \text{if } MV_i \geq 0, \\ Y_i(0) & \text{if } MV_i < 0. \end{cases}$$

The causal effect of interest results from the comparison of excess mortality in municipalities headed by more and low educated mayors who barely won against a candidate with the opposite education level. In these races, mayoral education is considered as good as randomly assigned, since municipalities are expected to be similar in all respects except for educational attainment around narrow victory margins. Formally, the local average treatment effect of municipalities electing a high educated type of mayor (Bertoli & Hazlett 2025) corresponds to the difference in the conditional expectations of excess mortality at the cutoff value:

$$\tau = \lim_{m \rightarrow 0_+} \mathbb{E}[Y_i | MV_i = m] - \lim_{m \rightarrow 0_-} \mathbb{E}[Y_i | MV_i = m] = \mathbb{E}[Y_i(1) - Y_i(0) | MV_i = 0].$$

The RDD estimand τ is defined only within an infinitesimal neighborhood of the RDD threshold, trading off external validity to gain internal validity. The estimand identifies the causal effect of electing a mayor with $E_i = 1$ at the municipal level; it should not be interpreted as the structural effect of education per se (Bertoli & Hazlett 2025). The relevant counterfactual is the election of an alternative mayor drawn from the set of candidates without a college degree—that is, a different bundle of correlated characteristics—rather than the same individual mayor absent a college degree. To

estimate this local average treatment effect, one must compute the difference between the limits of two regression functions approaching the RDD cutoff from opposite sides. In practice, we estimate local polynomial regressions of the following form:

$$Y_i = \alpha + \tau E_i + \sigma f(MV_i) + \rho [E_i \times f(MV_i)] + \varepsilon_i, \quad (1)$$

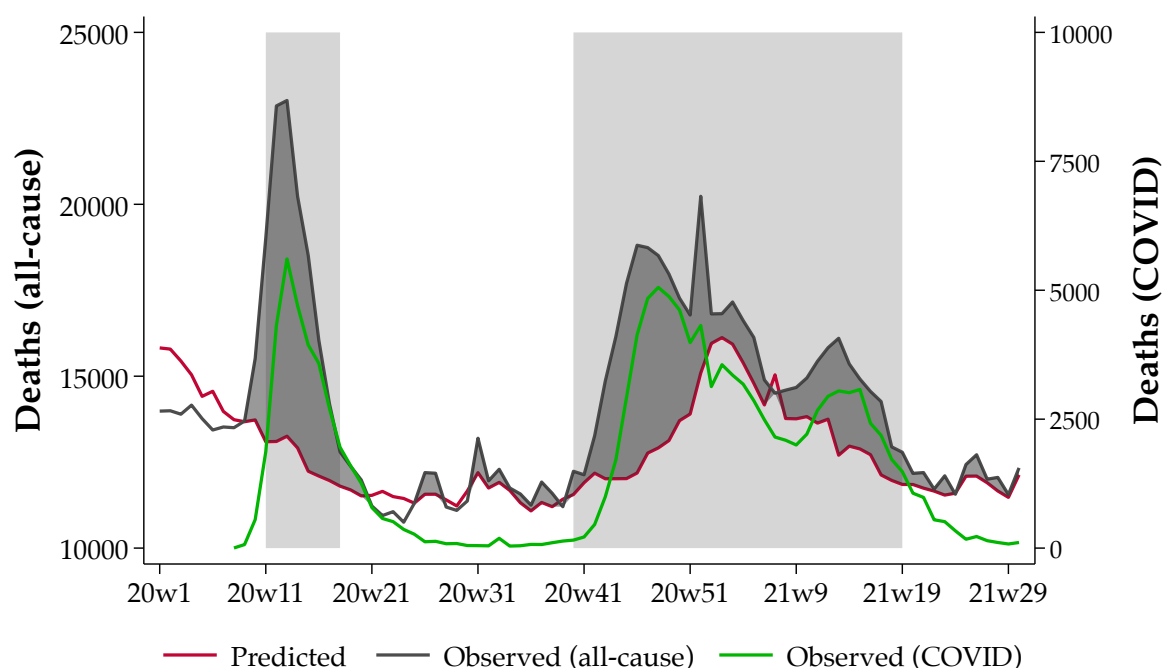
where Y_i is a measure of excess mortality in municipality i , E_i is a dummy variable equal to one if municipality i 's mayor has tertiary education or higher, and zero otherwise, $f(MV_i)$ is a linear or quadratic polynomial of the high educated mayor's margin of victory ([Gelman & Imbens 2019](#)), the interaction term $E_i \times f(MV_i)$ allows for separate regression functions fitted on either side of the cutoff, and ε_i is an error term. The causal effect of interest is captured by τ . We report estimates obtained using bias correction and robust variance estimation, a uniform kernel, and a symmetric optimal bandwidth—selected on both sides of the cutoff—based on the mean squared error (MSE) minimization procedure described in [Calonico et al. \(2014\)](#). Standard errors are adjusted for serial correlation in mortality outcomes and for COVID-19 transmission spillovers within local labor systems, i.e., territorial units defined by Istat based on daily home-to-work commuting flows, independently of administrative boundaries.

7 Main Results

[Figure 1](#) summarizes the trend in deaths predicted by the model at the country level throughout the pandemic, along with trends in observed deaths from all causes and from COVID-19 only. Excess mortality—represented by the gray area marking the difference between observed all-cause and counterfactual deaths—is evident in two distinct periods, which broadly align with the COVID-19 phases previously described. Moreover, observed all-cause deaths closely track the trend of officially reported COVID-19 deaths. [Figure A1](#) in the Appendix breaks down these trends by region.

The effect of mayor's education on excess mortality is visually summarized in [Figure 2](#). Stark discontinuities appear at the cutoff, both at the extensive margin (left

Fig. 1 Evolution of weekly deaths in Italy during the COVID emergency

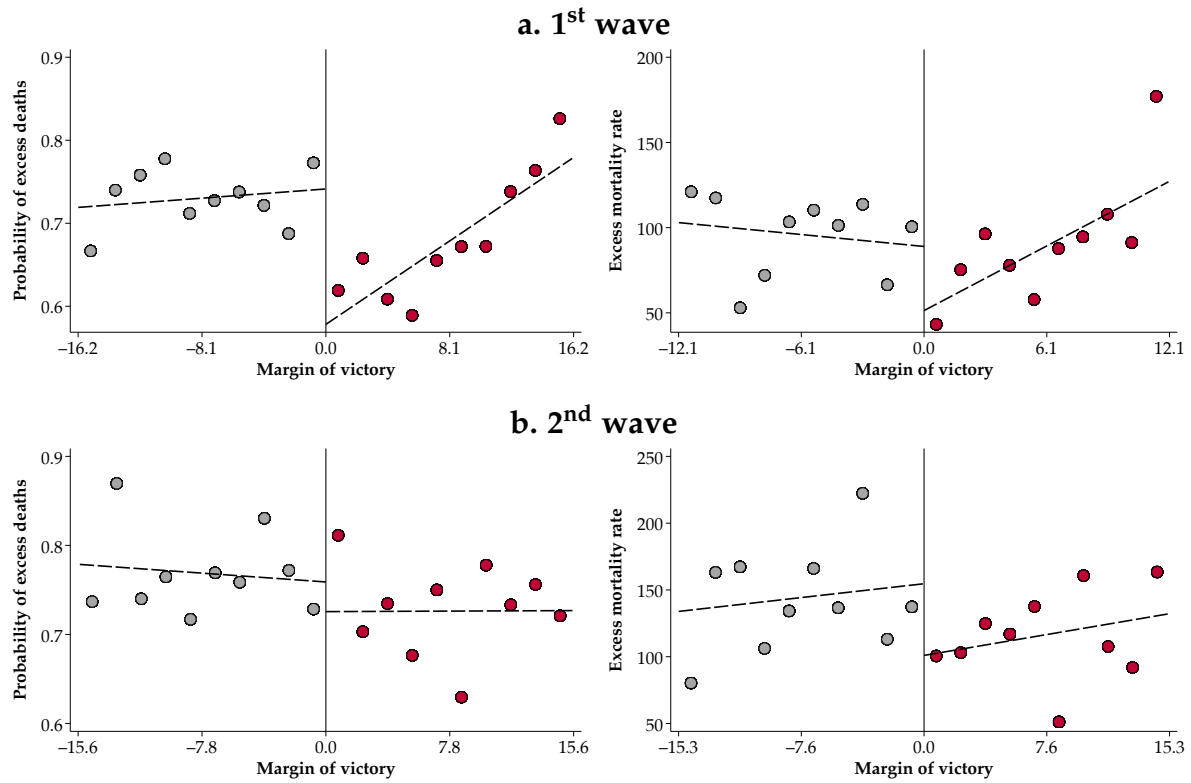


Notes: The shaded area between the curves of observed and predicted all-cause deaths quantify excess mortality. Vertical grey bars identify the two pandemic periods.

graph) and at the intensive margin (right graph), during the first pandemic wave (panel a). Policy decisions and communication strategies made by high educated mayors are associated with systematic reductions in excess mortality. These discontinuities disappear when considering mortality outcomes during the second pandemic wave (panel b). This evidence suggests that high educated mayors made a difference at the onset of the emergency, but not after the summer of 2020, despite higher excess mortality in several regions during the second wave. The same conclusions hold when using a quadratic polynomial rather than local linear regressions as in the figure.

Table 2 reports estimates of Equation 1, where the dependent variable is the probability of excess deaths and a quadratic polynomial is used. Estimates in the central columns confirm the visual intuition of a negative causal effect during the first wave. Specifically, the probability of recording excess deaths is 15.5 percentage points lower in municipalities governed by high educated mayors—a reduction of approximately 22% relative to the mean in control municipalities—and statistically significant at the 5% level (Column 3). Adding candidate-level and municipality-level covariates increases

Fig. 2 Regression discontinuity plots of mayoral education and excess mortality



Notes: Regression discontinuity plots of mayoral education and excess mortality at the extensive margin (left) and intensive margin (right) in the first (panel a.) and second (panel b.) pandemic wave. Bias-corrected RD estimates with uniform kernel from local linear regressions—represented by dashed lines—using MSE-optimal bandwidths equal on both sides of cutoffs – shown on horizontal axes—obtained through the procedure described in [Calonico et al. \(2014\)](#). Positive (negative) margins to the right (left) of cutoffs identify elections where the high educated candidate won (lost) against a low educated candidate, which are labeled with red (grey) binned sample means.

precision, yielding a point estimate statistically significant at the 0.1% level (Column 4). The estimated effect is also larger in magnitude, though not directly comparable to the previous one, as the optimal bandwidth used in the estimation is considerably narrower and the effect tends to grow closer to the threshold.

The effect vanishes both in the pre-COVID-19 period, used as a placebo test (Columns 1 and 2), and during the second wave (Columns 5 and 6). The null result in the pre-COVID-19 period serves as an indirect test of the continuity assumption and confirms that mayoral education was not affecting mortality differently before the start of the emergency. The null result in the second wave, instead, supports the interpretation that local leadership mattered most when the pandemic shock took everyone by surprise,

and not in the more codified environment of later outbreaks.

Table 2 Regression discontinuity estimates of probability of excess deaths on mayors' education

	Pre-COVID		1 st wave		2 nd wave	
	(1)	(2)	(3)	(4)	(5)	(6)
High educ.	0.064 (0.074)	-0.089 (0.061)	-0.155** (0.071)	-0.308*** (0.059)	0.058 (0.074)	-0.075 (0.067)
Observations	1,518	1,011	1,561	880	1,228	666
Bandwidth	22.3	12.8	23.1	10.6	19.7	9.2
Mean Dep. Var.	.374	.386	.718	.733	.726	.728
Covariates	×	✓	×	✓	×	✓

Note: Local quadratic bias-corrected RD estimates. MSE-optimal bandwidth, equal on both sides of the discontinuity, obtained through the procedure described in [Calonico et al. \(2014\)](#). Even-numbered columns include both candidate-level and municipality-level covariates described in the Data section. Robust standard errors, clustered by local labor systems, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$

[Table 3](#) summarizes the effect of mayoral education on the excess mortality rate. In line with the binary case previously analyzed, the only large and statistically significant treatment effect estimates are found during the first pandemic wave. Municipalities led by college-educated mayors recorded 60.3 fewer excess deaths *per* 100,000 people compared to those led by low educated mayors—a difference of approximately 65% (Column 3). The covariate-adjusted estimate, obtained again in a narrower optimal bandwidth, is larger in magnitude and statistically significant at the 0.1% level (Column 4). The placebo tests in Columns 1 and 2 support the validity of the RDD setup. The estimates in Columns 5 and 6 show that, while the direction of the effects remains consistently negative during the second pandemic wave, none of the coefficients are statistically different from zero at conventional levels.

7.1 Robustness Checks

A key identifying assumption in RDD is the continuity of potential outcomes, meaning that, in the absence of treatment, potential outcomes would evolve smoothly

Table 3 Regression discontinuity estimates of excess mortality rate on mayors' education

	Pre-COVID		1 st wave		2 nd wave	
	(1)	(2)	(3)	(4)	(5)	(6)
High educ.	7.082 (17.769)	-15.916 (15.538)	-60.269** (28.625)	-73.345*** (21.441)	-25.753 (56.977)	-42.315 (61.319)
Observations	1,507	931	1,490	812	1,332	506
Bandwidth	21.8	11.6	21.4	9.8	22.9	6.8
Mean Dep. Var.	-14.473	-12.830	93.368	88.853	90.950	94.496
Covariates	×	✓	×	✓	×	✓

Note: Local quadratic bias-corrected RD estimates. MSE-optimal bandwidth, equal on both sides of the discontinuity, obtained through the procedure described in [Calonico et al. \(2014\)](#). Even-numbered columns include both candidate-level and municipality-level covariates described in the Data section. Robust standard errors, clustered by local labor systems, in parentheses.

* $p < .1$; ** $p < .05$; *** $p < .01$

at the cutoff. If this assumption is violated, any observed discontinuity in excess mortality could reflect pre-existing differences in other characteristics between units just above and below the cutoff, rather than the causal effect of the treatment. We indirectly assess the validity of this assumption by estimating [Equation 1](#) using a wide set of predetermined municipal characteristics as outcomes. Results are summarized in [Table 4](#). Reassuringly, these characteristics vary smoothly at the cutoff—no coefficient is statistically significant at the 5% or 1% level. Municipalities led by college-educated mayors are slightly less likely to be located in the North-West and somewhat more likely to be in Southern Italy, but only at a 10% significance level. Given the large number of covariates tested, these slight imbalances might well be attributed to sample noise.

Table 4 Balance tests of covariates

	$\hat{\tau}$	Std. error
<i>Elections</i>		
Turnout (%)	-0.346	(1.405)
Runoff election	0.009	(0.040)
Electoral competition	0.009	(0.013)
<i>Demography</i>		
Population (thousands)	-2.665	(2.680)
Population ≥ 65 (%)	0.179	(0.829)
Female population (%)	0.033	(0.205)
Foreign population (%)	0.582	(0.700)
Population density	38.535	(95.415)
Pop. in household (%)	0.064	(0.127)
Pop. in collective res. (%)	-0.064	(0.127)
Household size	0.033	(0.038)
<i>Geography</i>		
North-West	-0.133*	(0.076)
North-East	-0.051	(0.064)
Center	0.012	(0.046)
South	0.132*	(0.078)
Islands	0.039	(0.053)
Altitude	-26.356	(46.635)
Coastal	0.068	(0.059)
City	-0.013	(0.025)
Town	0.048	(0.076)
Rural	-0.025	(0.076)
<i>Education</i>		
No or Primary education (%)	0.444	(0.671)
Lower secondary education (%)	-0.389	(0.733)
Upper secondary education (%)	-0.008	(0.703)
Tertiary (%)	0.025	(0.524)
<i>Employment</i>		
Employed (%)	-1.760	(1.224)
Out of labor force (%)	1.225	(0.965)
Firm size	0.052	(0.227)
Manufacturing (%)	0.268	(0.917)
Construction (%)	-0.761	(1.161)
Wholesale/retail trade (%)	0.784	(1.082)
Transp. and storage (%)	0.579	(0.499)
Accommodation and food (%)	-0.609	(0.967)
Prof., scient., techn. (%)	-0.579	(0.661)
Health and social (%)	0.580	(0.402)
Other activities (%)	-0.149	(0.903)

Note: Local quadratic bias-corrected RD estimates with uniform kernel and MSE-optimal bandwidths, equal on both sides of the cutoffs, obtained through the procedure described in [Calonico et al. \(2014\)](#). Robust standard errors, clustered by local labor system, included in all models.

* $p < .1$; ** $p < .05$; *** $p < .01$

Another check is the continuity in the density function of the running variable at the cutoff. A violation of this condition may indicate manipulation in treatment assignment, i.e., the ability of units to self-select into treatment or control groups (McCrary 2008). In our setting, a bunching of victories by more or low educated candidates at the cutoff would invalidate the design. While probabilities of victory differ in our sample of mixed-education elections, there is no discontinuity in the density function at the cutoff.⁸ Appendix Figure A2 provides a visual inspection and formally tests the continuity hypothesis using Cattaneo et al. (2020). We fail to reject the null hypothesis of a continuous density—i.e., we find no evidence of sorting around the cutoff, supporting the validity of the research design. Other non-random heaping points are not visually discernible either, further supporting the validity of the design (Barreca et al. 2016).

In Appendix Figure A3, we check the sensitivity of our results to the choice of bandwidth. Equation 1 is estimated within bandwidths defined by margins of victory ranging from 5 to 50 percentage points (in 1-point increments), and the figures plot the estimated effects for both the extensive margin (left) and the intensive margin (right) of excess mortality. While point estimates increase in absolute value as bandwidths shrink, highlighting the well-known trade-off between bias and variance (Cunningham 2021), effects in the first wave (panel a) are uniformly statistically different from zero across all bandwidths (up to 17-point margins for excess mortality rates), not only within the optimal bandwidths. In the second wave (panel b), estimated effects are positive at narrower bandwidths and become negative at wider bandwidths. However, negative effects reach statistical significance only at bandwidths exceeding 25 percentage points, where a causal interpretation of the estimates becomes less credible.

The distribution of excess mortality rates shows pronounced right-skewness—that is, a few municipalities recorded extremely high levels of excess mortality. These extreme values might reflect a compromised healthcare context that limited the scope for

⁸College-educated candidates win a larger share of elections in the full sample of mixed-education elections (53.7% vs. 46.3%). This imbalance diminishes in closer races: as the margin of victory approaches zero, the probability that either candidate wins converges toward 50% within optimal bandwidths.

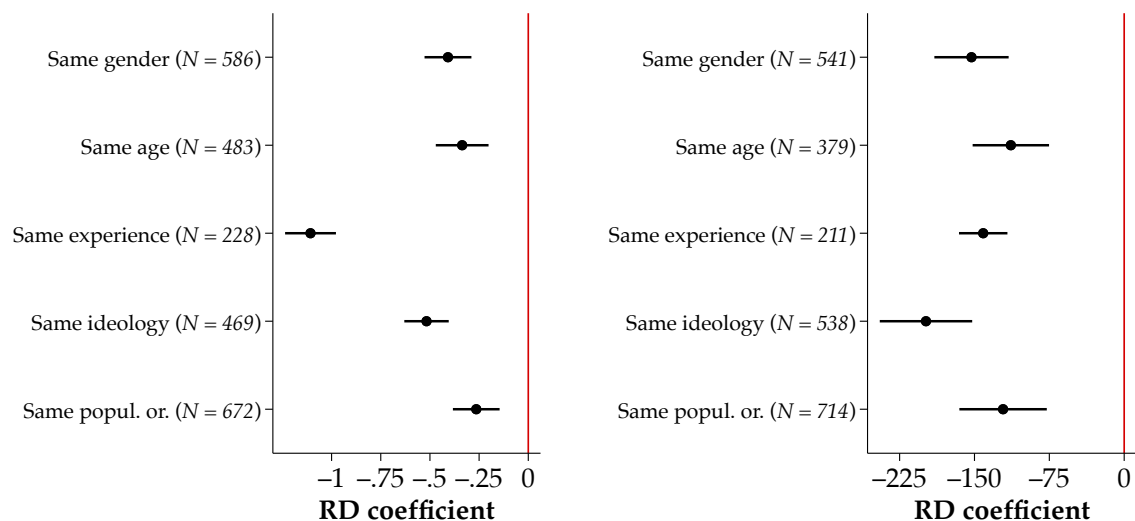
mayoral initiatives in managing the emergency and undermined their effectiveness. Alternatively, they could result from the estimation procedure itself, particularly in small municipalities where deaths are rare events—even when based on 1,000 simulations. In Appendix Figure A4, we test the sensitivity of our estimates to the exclusion of such extreme values. In the first pandemic wave, the negative and significant effect of mayoral education persists even when the distribution is substantially trimmed, down to the median, regardless of the polynomial specification (panel a). By contrast, estimates in the second pandemic wave fluctuate considerably above and below zero, again indicating an inconsistent pattern (panel b).

All of the above tests corroborate the validity of the RDD assumptions and the robustness of our main findings. Yet, the interpretation of the treatment effect may be complicated by the correlation between mayoral education and other relevant traits—that is, the treatment may be compound. In other words, while municipalities just above and below the RDD cutoff appear to be identical in terms of both observable and unobservable characteristics, other mayoral attributes that are highly correlated with education may act as complements or substitutes for tertiary education in explaining the observed reduction in excess mortality. Figure A5 shows that college-educated mayors indeed differ from other mayors along the dimensions of age, gender, experience, and ideology. We thus explore more closely whether candidates' pre-determined characteristics affect the estimated effect of education, in order to minimize the possibility that our estimates capture compound treatments or compensating differentials in other traits. This analysis builds on work that proposes stricter validity conditions for close-election RD designs ([Sekhon & Titiunik 2012](#); [Caughey & Sekhon 2017](#)), particularly those examining political selection through candidates' characteristics ([Marshall 2024](#); [Bertoli & Hazlett 2025](#)).

[Figure 3](#) plots the estimated coefficients τ from re-estimating [Equation 1](#) after progressively excluding from the sample those elections that are mixed along other candidate characteristics. Operationally, we begin with our analytic sample of mixed-education elections and alternatively exclude elections that are also mixed in terms of

gender, age, experience, ideology, or populist orientation. The results are strongly robust: the causal effect of education on excess mortality persists in the residual samples, where high educated candidates do not systematically differ from their low educated counterparts along other traits that could confound the interpretation of the treatment effect at the cutoff. Although we interpret education as a proxy for broader leadership characteristics—potentially including unobservable ability in dealing with high-uncertainty shocks—we can conclude that our findings are not driven by the correlation between education and the other observable traits we account for in [Figure 3](#).

Fig. 3 Sensitivity of treatment effect to the exclusion of other mixed-characteristics elections



Notes: Sensitivity of treatment effect estimates—i.e. high educated mayor—to the exclusion from the analytic sample of other mixed-characteristics elections in regression discontinuity designs using as outcome excess mortality at the extensive margin (left) and intensive margin (right) in the first pandemic wave. Bias-corrected RD estimates with uniform kernel from covariate-adjusted local quadratic (right) regressions using MSE-optimal bandwidths equal on both sides of cutoffs obtained through the procedure described in [Calonico et al. \(2014\)](#). Point estimates—and their 95% confidence intervals indicated by horizontal black lines—come from separate RD regressions that exclude from the sample mixed electoral races in terms of candidate characteristics listed on vertical axes (residual sample size indicated in parentheses). For continuous variables—age and experience—similarity is defined as both candidates being above or below the median level of that variable in the full sample.

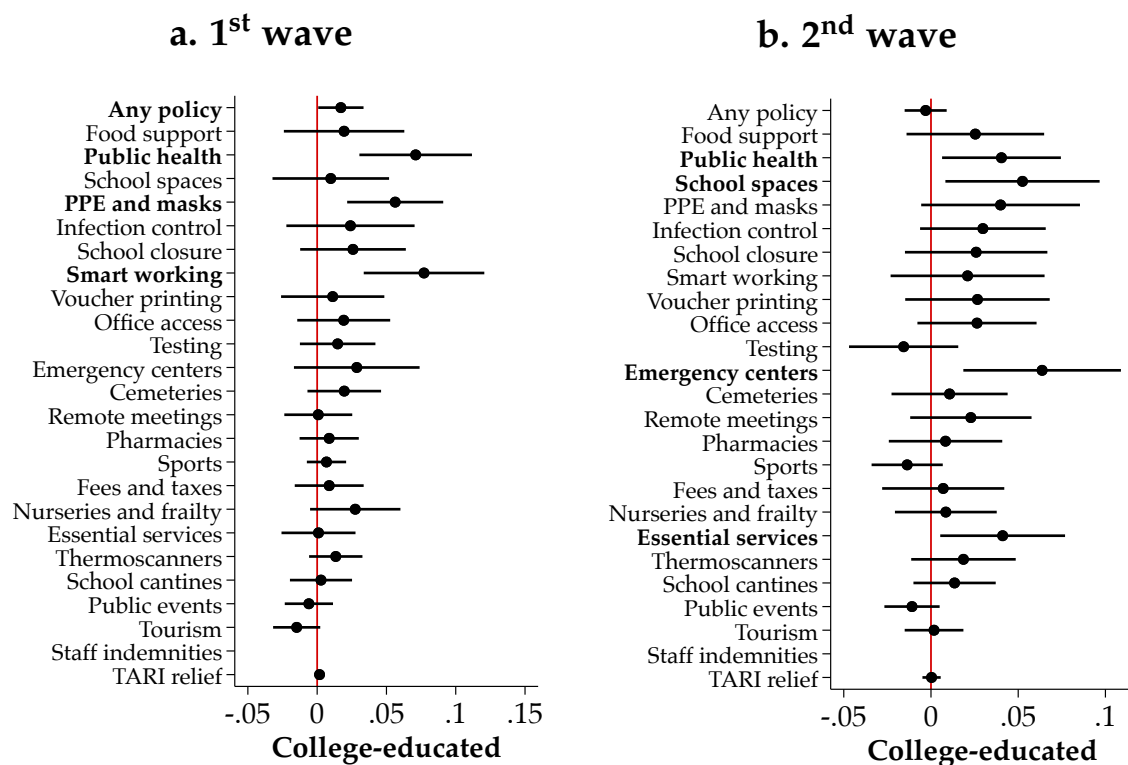
7.2 Mechanisms

Policy. To shed light on the mechanisms linking mayoral education to excess mortality, we examine differences in local policy responses to the pandemic. We focus

on the content COVID-related policy activity, and the timing of COVID policymaking.

Figure 4 reports OLS estimates of mayoral college-education on adoption of COVID-related acts. Panel a. shows that college-educated mayors have a slightly larger probability of adopting a COVID-related act in the first wave. This positive association is particularly pronounced for acts concerning public health, procurement of personal protective equipment and masks, and smart working. Panel b. shows no overall difference in the second wave between high and low educated mayors in adoption of COVID-related policy. Positive differences appear for specific measures related to general public health, rearrangement of school spaces, activation of emergency centers, and acts concerning essential services.

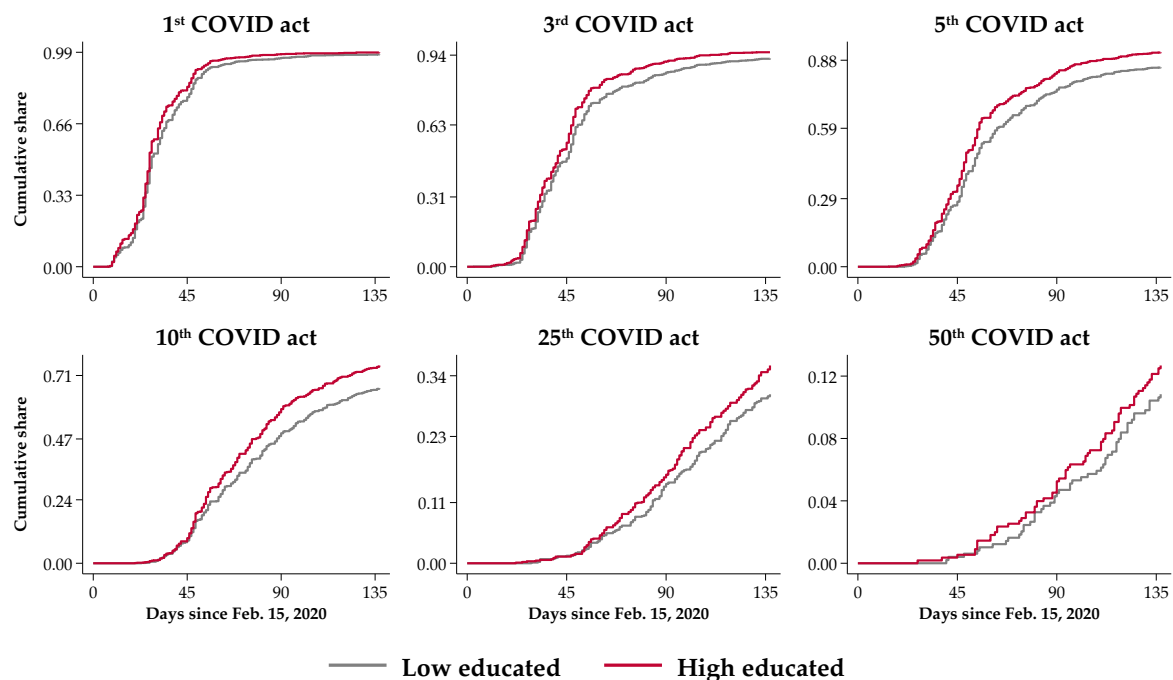
Fig. 4 Association between mayoral education and adoption of COVID-related administrative acts



Notes: Association between mayoral education and adoption of COVID-related administrative acts during the first (left) and second (right) pandemic wave. Each row reports the point estimate (in bold if significant at the 5% level) and 95% confidence interval for the coefficient on a college-educated mayor dummy from a separate regression having as dependent variable a dummy equal to one if at least one act belonging to the topic indicated on the left was adopted during the wave. All regressions include municipality local labor system and election year fixed effects.

Figure 5 reports Kaplan-Meier failure estimates for the time required to adopt the n th COVID-related act—1st, 3rd, 5th, 10th, 25th, and 50th—separately by mayoral education. Across all panels, municipalities governed by more educated mayors reach the corresponding level of COVID policymaking earlier. The gap becomes increasingly visible at higher-order acts. The difference is not limited to an earlier first response. College-educated mayors appear to have built a pandemic response more rapidly over time, accumulating successive interventions faster than their less educated counterparts.

Fig. 5 Kaplan–Meier cumulative failure estimates for the time to adoption of the n^{th} COVID-related administrative act



Notes: Kaplan–Meier cumulative failure estimates for the time to adoption of the n^{th} COVID-related administrative act by mayoral education level. Each panel plots the share of municipalities that have issued at least n COVID-related acts by each day elapsed since February 15, 2020, separately for municipalities governed by college-educated (red) mayors and not (gray). The risk set in each panel is restricted to municipalities that eventually adopted at least n COVID acts before June 30, 2020.

This evidence points to high educated mayors replying more promptly and more decisively to pandemic spread particularly during the first wave. This includes both the earlier adoption of emergency measures and a broader activation across key policy domains. The disappearance of the mortality effect in the second wave is consistent with a mechanism centered on early crisis management: municipal policies mattered

most when the pandemic shock was new, uncertainty was high, and local decision-makers had to act before responses became more standardized and the low educated caught up.

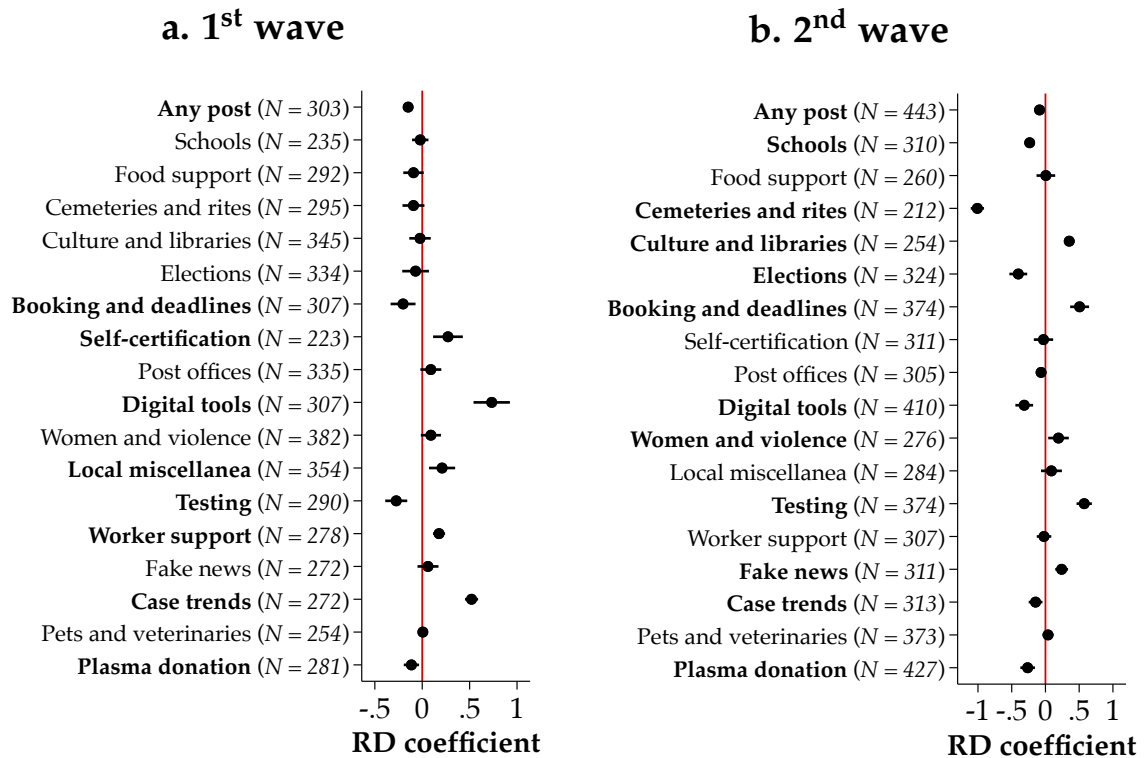
Communication. To investigate whether mayoral education shaped public behavior through institutional communication during the pandemic, we examine three dimensions of municipal social media activity, i.e. *what* municipalities communicated about, *how* they toned their messages, and *how* citizens engaged with this communication.

Figure 6 focuses on topics of COVID-related Facebook communication. When the mayor is college-educated, municipal Facebook pages have a lower probability of posting COVID-related messages both in the first and second pandemic waves. Positive discontinuities appear however when the post is about self-certification, digital tools, locally-targeted messages, worker supports, and information about trends in cases. Negative discontinuities are instead found in the case of posts about booking and deadlines for local services, testing procedures, and donation of immune plasma.

In Figure 7 we analyze the communicative register of COVID-related posts. In the first wave, municipalities led by college-educated mayors appear less likely to use risk-alerting and directive-compliant tones and more likely to rely on reassuring, instructional, and empathetic tones. The same municipalities happen to communicate with larger positive sentiment and sentiment polarity, and lower negative sentiment in the first wave. Some of these trends reverse in the second wave, pointing to the uniqueness of communication by these municipalities at the onset of the crisis.

Taken together, these estimates point to a distinctive first-wave communication strategy among college-educated mayors: less alarmist and less procedurally inward-looking, but more reassuring, more mobilizing, and more explicitly grounded in evidence and public information. In the second wave, these differences become weaker or flipped, and less internally coherent. Some coefficients remain consistent with a less alarmist style and with lower emphasis on evidence-based or transparency-oriented framing, but the overall pattern is more mixed and considerably less pronounced than in the first wave. This mirrors the broader substantive results on mortality and policy:

Fig. 6 Effect of college-educated mayor on municipal COVID-related Facebook communication topics

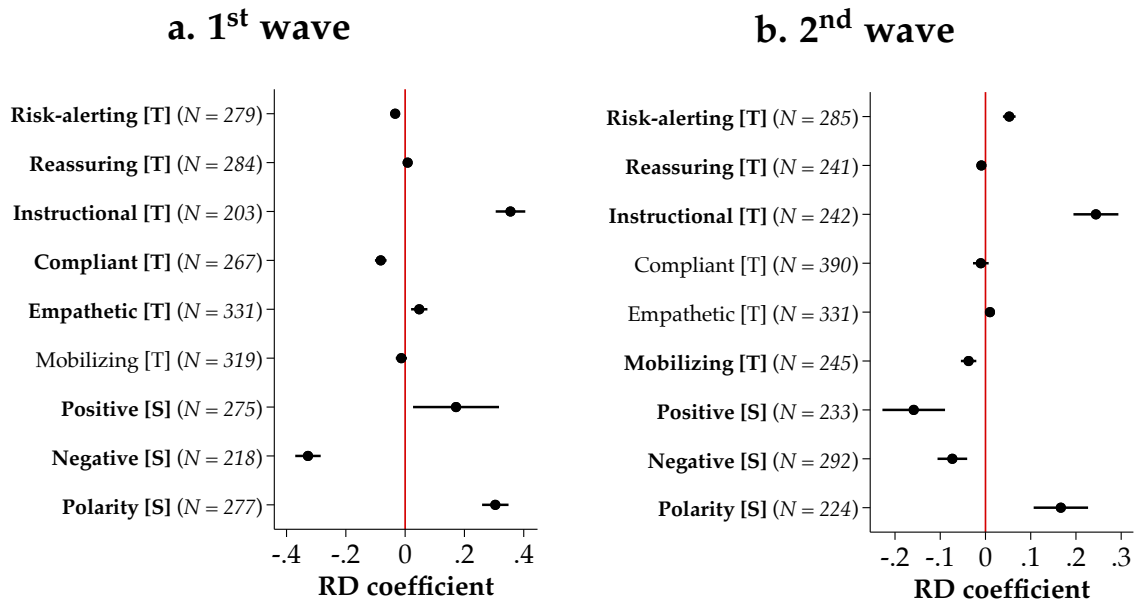


Notes: Bias-corrected RD estimates with uniform kernel from covariate-adjusted local quadratic regressions using MSE-optimal bandwidths, following Calonico et al. (2014). Point estimates (in bold if significant at the 5% level)—and their 95% confidence intervals indicated by horizontal black lines—come from separate RD regressions using, as outcome, a binary indicator for whether the municipality posted at least one COVID-related Facebook post belonging to the communication topic listed on the vertical axis, during the first (left) and second (right) pandemic wave (sample size indicated in parentheses).

communication differences seem to matter most in the early phase of the crisis, when uncertainty was highest and local discretion over public messaging was greatest.

Finally, [Figure 8](#) considers originality and citizen engagement with municipal communication, distinguishing COVID-related from non-COVID-related posts. In the first wave, municipalities governed by college-educated mayors appear systematically less likely to repost rather than publish original COVID-related messages (the opposite happens when considering non-COVID messages). Notable differences emerge also in audience response: posts by municipalities with college-educated mayors receive more reactions and comments especially when COVID-related compared to non-COVID posts, while the evidence for shares is mixed. In the second wave, engagement gaps

Fig. 7 Effect of college-educated mayor on municipal COVID-related Facebook communication tones [T] and sentiments [S]

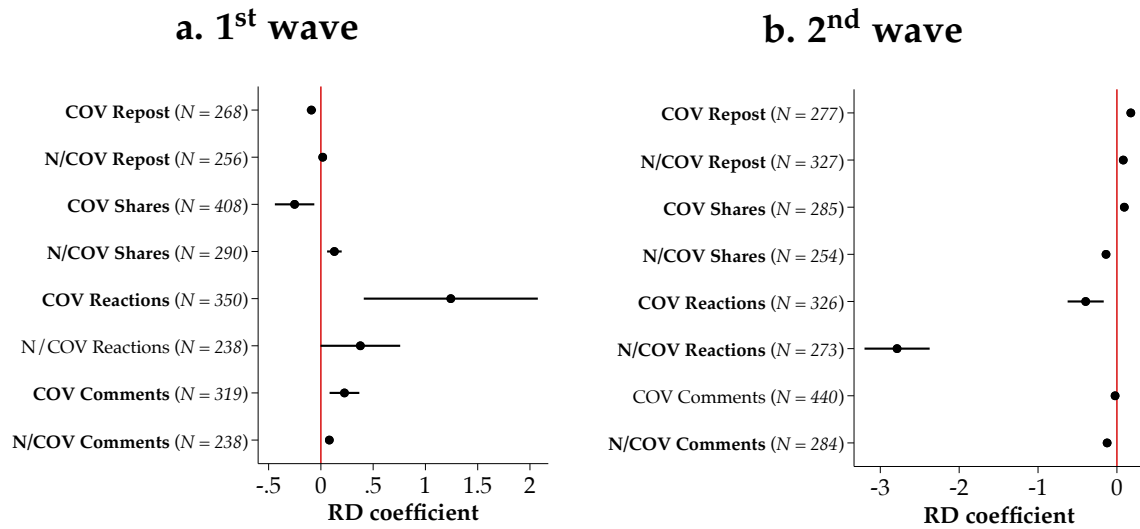


Notes: Bias-corrected RD estimates with uniform kernel from covariate-adjusted local quadratic regressions using MSE-optimal bandwidths, following Calonico et al. (2014). Point estimates (in bold if significant at the 5% level)—and their 95% confidence intervals indicated by horizontal black lines—come from separate RD regressions using, as outcome, shares of COVID-related Facebook posts classified as adopting the tone or sentiment listed on the vertical axis, during the first (left) and second (right) pandemic wave (sample size indicated in parentheses).

largely disappear or reverse. Reaction-based engagement is no longer higher among municipalities with college-educated mayors. This fits with a broader interpretation that the communication advantage of more educated mayors was concentrated in the first phase of the pandemic, when institutional messaging could more plausibly shape citizens' perceptions, compliance, and response to the crisis.

Overall, during the first wave college-educated mayors did not communicate more, rather they communicated differently: they emphasized different topics, adopted a less alarming and more evidence-based and mobilizing register, and generated stronger public engagement with COVID-related content. By the second wave, these differences narrowed and became less coherent. This pattern is consistent with a mechanism centered on early crisis management. When the pandemic first struck, mayoral education appears to have mattered not only for what municipalities did, but also for how they explained the crisis, justified interventions, and interacted with citizens in real time.

Fig. 8 Effect of college-educated mayor on authorship and engagement with municipal COVID and non-COVID-related Facebook communication



Notes: Bias-corrected RD estimates with uniform kernel from covariate-adjusted local quadratic regressions using MSE-optimal bandwidths, following Calonico et al. (2014). Point estimates (in bold if significant at the 5% level)—and their 95% confidence intervals indicated by horizontal black lines—come from separate RD regressions using, as outcome, fraction of reposts, average shares, reactions and comments by post per 100 residents across COVID-related and non-COVID-related Facebook posts, during the first (left) and second (right) pandemic wave (sample size indicated in parentheses).

8 Conclusion

The COVID-19 pandemic was a universal shock, but its effects were far from equal. Our analysis shows that, even among neighboring municipalities operating under the same national policy framework, the characteristics of local leadership can significantly shape health outcomes. In particular, we provide causal evidence that electing a mayor with college education substantially reduced excess mortality during the first wave of the pandemic in Italy—a phase marked by the absence of clear national and local protocols. This effect fades in later waves, suggesting that political selection matters more when uncertainty is higher and centralized guidance more limited.

Our research design builds on a close-election RDD, ensuring that municipalities governed by college-educated and non-college-educated mayors were otherwise comparable. This identification strategy reinforces the causal interpretation of our findings and the robustness of the estimated effects. Interpreting education as a proxy for a

broader bundle of leadership traits, we underscore the demographic significance of political selection processes. In contexts where institutional preparedness is limited or uneven, the attributes and competence of individual officeholders may critically shape population-level responses to unexpected crises.

The mechanism analyses clarify what this bundle of leadership traits meant in practice. Using newly collected large-scale data on municipal COVID-19 policy activity and social media communication, we show that college-educated mayors differed from low educated mayors in two domains where local leaders had concrete margins of action. First, they activated local policy responses in a more timely and targeted way. Second, they adopted a different communication strategy, characterized by greater engagement and distinctive tones. These findings move the interpretation beyond a generic “education effect”: college-educated mayors appear to have reduced mortality not simply because they held a degree, but because the characteristics associated with their education translated into observable differences in local crisis management.

More broadly, these findings invite demographic research to further integrate political determinants of health and mortality into the study of population resilience and crisis dynamics.

A simple back-of-the-envelope calculation illustrates the relevance of our findings. At the onset of the pandemic, approximately 53% of Italian municipalities were led by mayors without a college degree. If those municipalities had instead elected mayors with a different bundle of characteristics, our estimates imply roughly 20,000 fewer excess deaths—up to 32,000 under the upper-bound scenario. Put differently, close to half of the excess mortality observed during the first wave could have been avoided. While large, this magnitude is not out of line with existing evidence linking excess mortality during the COVID-19 pandemic to health system capacity and broader socio-economic conditions, which have been shown to generate sizeable differences across contexts (e.g., [Kneebone-Hopkins et al. 2022](#)).

The evidence from Italy thus speaks to a broader issue: when institutional responses lag or falter, the identity of elected politicians can shape outcomes, even for policy

responses with significant demographic consequences.

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Appendix A Additional Materials

Table A1 Descriptive statistics of mayors

	Obs.	Mean	Median	SD	Min.	Max.
<i>Education</i>						
≤ Lower secondary	2,545	.070	0	.256	0	1
Upper secondary	2,545	.393	0	.488	0	1
≥ Tertiary	2,545	.537	1	.499	0	1
Age	2,545	50.6	51	10.8	20	85
Female	2,545	.149	0	.356	0	1
Experience (years)	2,545	13.9	13	7.7	0	39
Local born	2,545	.301	0	.459	0	1
<i>Ideology</i>						
Right	2,545	.075	0	.264	0	1
Center	2,545	.018	0	.133	0	1
Left	2,545	.046	0	.209	0	1
Independent	2,545	.861	1	.347	0	1
Populist or.	2,545	.080	0	.271	0	1

Table A2 Descriptive statistics of municipalities

	Obs.	Mean	Median	SD	Min.	Max.
<i>Mortality</i>						
Excess deaths prob. (pre-COVID)	2,545	.38	.00	.48	0	1
Excess deaths prob. (1 st wave)	2,545	.69	1	.46	0	1
Excess deaths prob. (2 nd wave)	2,545	.73	1	.44	0	1
Excess mortality rate (pre-COVID)	2,545	-15.6	-18.7	129.4	-583.7	1864.5
Excess mortality rate (1 st wave)	2,545	95.2	50.2	227.6	-583.7	2557.7
Excess mortality rate (2 nd wave)	2,545	147.2	117.1	361.3	-2477.5	5021.0
<i>Elections</i>						
Turnout (%)	2,545	65.21	67.09	10.82	19.50	93.94
Runoff election	2,545	.07	.00	.25	0	1
Electoral competition	2,545	0.50	0.50	0.12	0.19	0.96
<i>Demography</i>						
Population (thousands)	2,545	7.01	2.97	17.84	0.03	565.75
Population ≥ 65 (%)	2,545	25.27	24.86	5.21	8.64	54.84
Female population (%)	2,545	50.51	50.62	1.49	39.10	55.22
Foreign population (%)	2,545	6.63	5.90	4.20	0.00	35.21
Population density	2,545	332.3	121.3	648.7	1.1	9435.7
Pop. in household (%)	2,545	99.41	99.77	1.17	79.07	100.00
Pop. in cohabitation (%)	2,545	0.59	0.23	1.17	0.00	20.93
Household size	2,545	2.28	2.30	0.23	1.16	3.09
<i>Geography</i>						
North-West	2,545	.33	0	.47	0	1
North-East	2,545	.14	0	.35	0	1
Center	2,545	.14	0	.35	0	1
South	2,545	.29	0	.45	0	1
Islands	2,545	.10	0	.30	0	1
Altitude	2,545	339.8	276.0	284.6	0.0	1816.0
Coastal	2,545	.16	0	.36	0	1
City	2,545	.03	0	.18	0	1
Town	2,545	.37	0	.48	0	1
Rural	2,545	.59	1	.49	0	1
<i>Education</i>						
No or Primary education (%)	2,545	23.17	22.79	4.11	3.23	46.94
Lower secondary education (%)	2,545	31.26	30.92	4.24	13.24	48.90
Upper secondary education (%)	2,545	34.96	35.35	4.55	17.22	54.84
Tertiary (%)	2,545	10.63	10.30	3.13	2.04	30.41
<i>Employment</i>						
Employed pop. (%)	2,545	45.11	46.18	7.48	23.39	67.31
Out of labor force pop. (%)	2,545	48.90	48.24	6.03	30.77	72.73
Firm size	2,545	3.20	2.80	1.67	0.63	22.57
Manufacturing (%)	2,545	11.54	10.39	6.43	0.00	66.67
Construction (%)	2,545	15.19	13.97	7.08	0.00	75.00
Wholesale/retail trade (%)	2,545	25.31	24.49	7.89	0.00	100.00
Transp. and storage (%)	2,545	4.12	3.33	3.82	0.00	100.00
Accommodation and food (%)	2,545	10.40	8.05	7.90	0.00	100.00
Prof., scient., techn. (%)	2,545	11.49	11.47	4.78	0.00	40.00
Health and social (%)	2,545	5.33	5.06	3.07	0.00	33.33
Other activities (%)	2,545	16.61	17.14	6.01	0.00	50.00

Table A3 Comparison of descriptive statistics of mayors across samples

	Analytic ($N = 2,545$)		Full ($N = 6,906$)	
	Mean	SD	Mean	SD
<i>Education</i>				
≤ Lower secondary	.07	.26	.10	.29
Upper secondary	.39	.49	.44	.50
≥ Tertiary	.54	.50	.47	.50
Age	50.62	10.77	51.39	10.81
Female	.15	.36	.14	.35
Experience (years)	13.92	7.75	14.6	7.79
Local born	.30	.46	.30	.46
<i>Ideology</i>				
Right	.08	.26	.07	.25
Center	.02	.13	.02	.14
Left	.05	.21	.04	.19
Independent	.86	.35	.87	.33
Populist or.	.08	.27	.07	.26

Table A4 Comparison of descriptive statistics of municipalities across samples

	Analytic (<i>N</i> = 2, 545)		Full (<i>N</i> = 6, 906)	
	Mean	SD	Mean	SD
<i>Mortality</i>				
Excess deaths prob. (pre-COVID)	.38	.48	.37	.48
Excess deaths prob. (1 st wave)	.69	.46	.68	.47
Excess deaths prob. (2 nd wave)	.73	.44	.72	.45
Excess mortality rate (pre-COVID)	-15.56	129.40	-17.97	134.56
Excess mortality rate (1 st wave)	95.21	227.64	109.27	265.61
Excess mortality rate (2 nd wave)	147.20	361.27	153.16	395.74
<i>Elections</i>				
Turnout (%)	65.21	10.82	65.19	10.59
Runoff election	.07	.25	.06	.24
Electoral competition	0.50	0.12	0.58	0.21
<i>Demography</i>				
Population (thousands)	7.01	17.84	7.05	27.59
Population ≥ 65 (%)	25.27	5.21	25.47	5.36
Female population (%)	50.51	1.49	50.41	1.62
Foreign population (%)	6.63	4.20	6.66	4.20
Population density	332.34	648.66	297.42	611.23
Pop. in household (%)	99.41	1.17	99.42	1.19
Pop. in cohabitation (%)	0.59	1.17	0.58	1.19
Household size	2.28	0.23	2.26	0.24
<i>Geography</i>				
North-West	.33	.47	.39	.49
North-East	.14	.35	.14	.35
Center	.14	.35	.12	.32
South	.29	.45	.26	.44
Islands	.10	.30	.09	.29
Altitude	339.81	284.64	347.56	287.49
Coastal	.16	.36	.14	.35
City	.03	.18	.03	.17
Town	.37	.48	.33	.47
Rural	.59	.49	.64	.48
<i>Education</i>				
No or Primary education (%)	23.17	4.11	23.12	4.21
Lower secondary education (%)	31.26	4.24	31.4	4.29
Upper secondary education (%)	34.96	4.55	35.07	4.73
Tertiary (%)	10.63	3.13	10.43	3.23
<i>Employment</i>				
Employed (%)	45.11	7.48	45.38	7.48
Out of labor force (%)	48.90	6.03	48.8	6.06
Firm size	3.20	1.67	3.19	1.80
Manufacturing (%)	11.54	6.43	11.47	6.73
Construction (%)	15.19	7.08	15.86	7.81
Wholesale/retail trade (%)	25.31	7.89	24.87	8.17
Transp. and storage (%)	4.12	3.82	4.29	3.97
Accommodation and food (%)	10.40	7.90	10.68	8.32
Prof., scient., techn. (%)	11.49	4.78	11.26	5.08
Health and social (%)	5.33	3.07	5.19	3.35
Other activities (%)	16.61	6.01	16.38	6.32

Fig. A1 Evolution of weekly deaths during the COVID emergency by region.

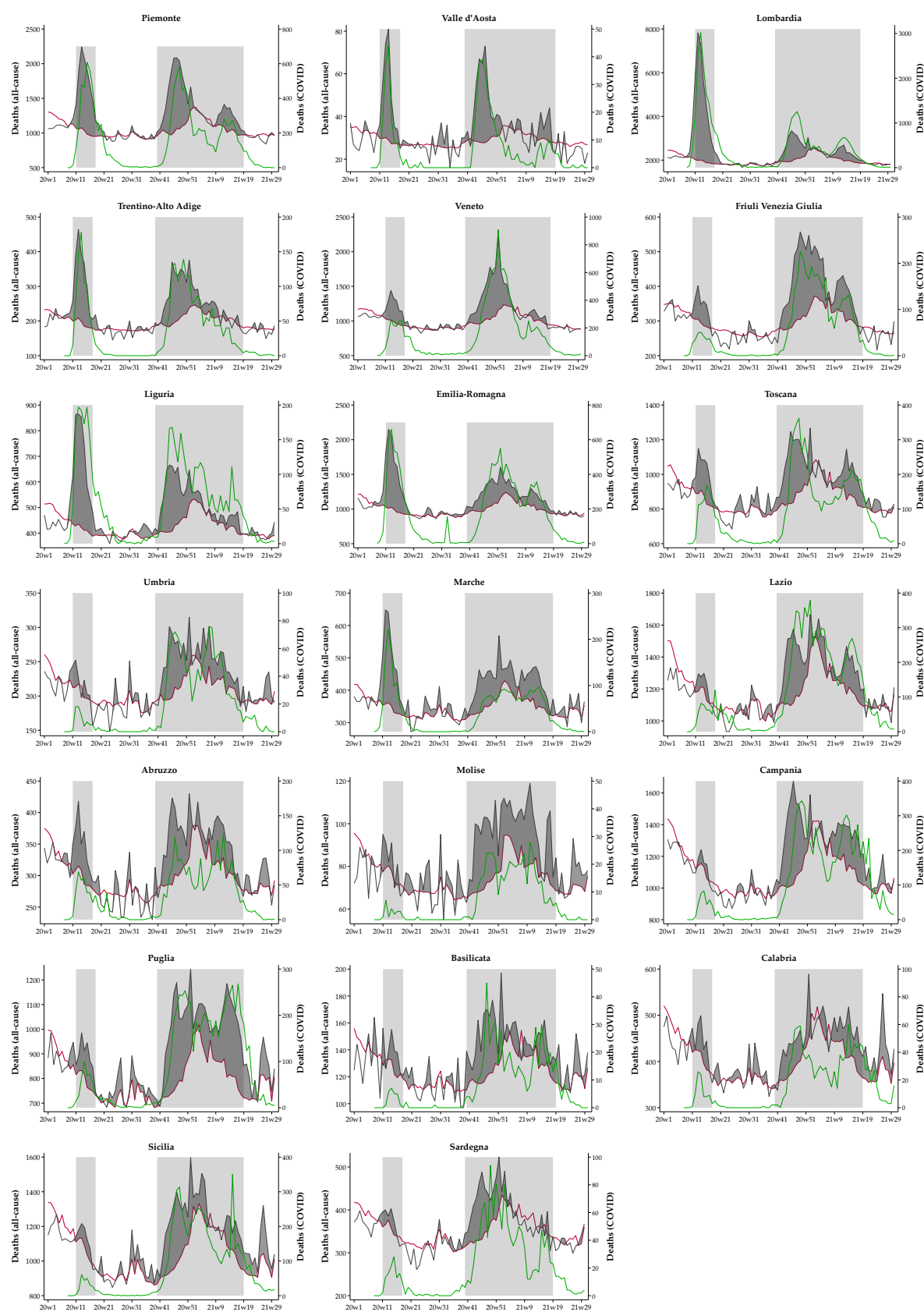
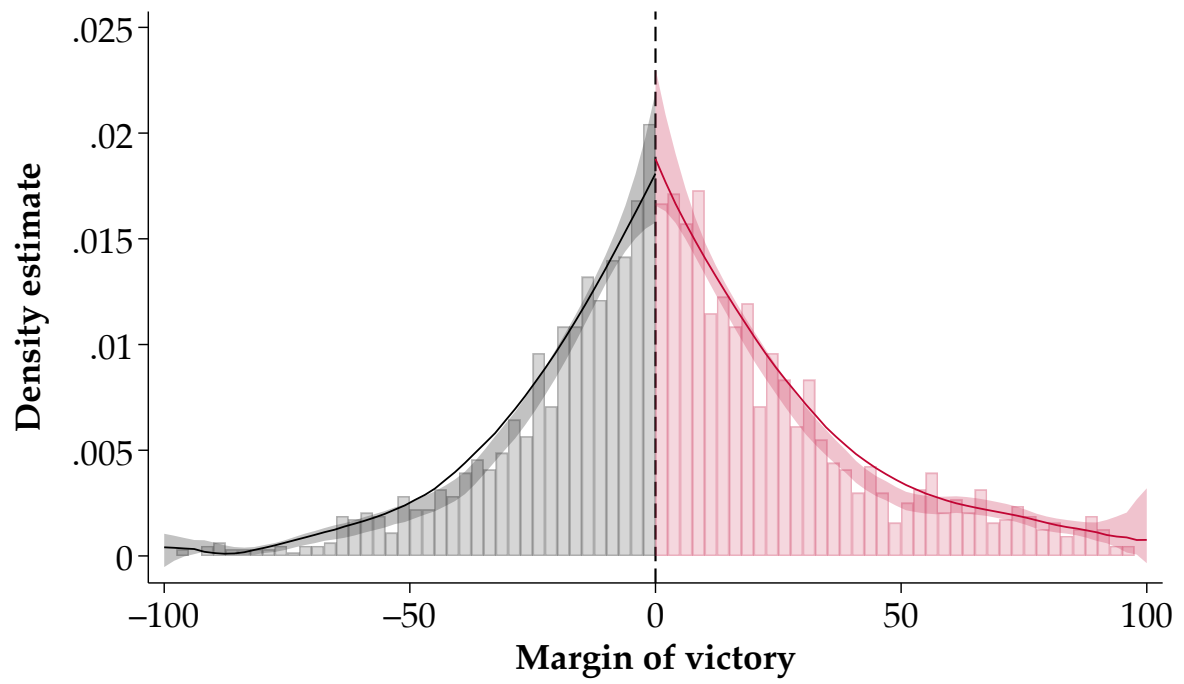
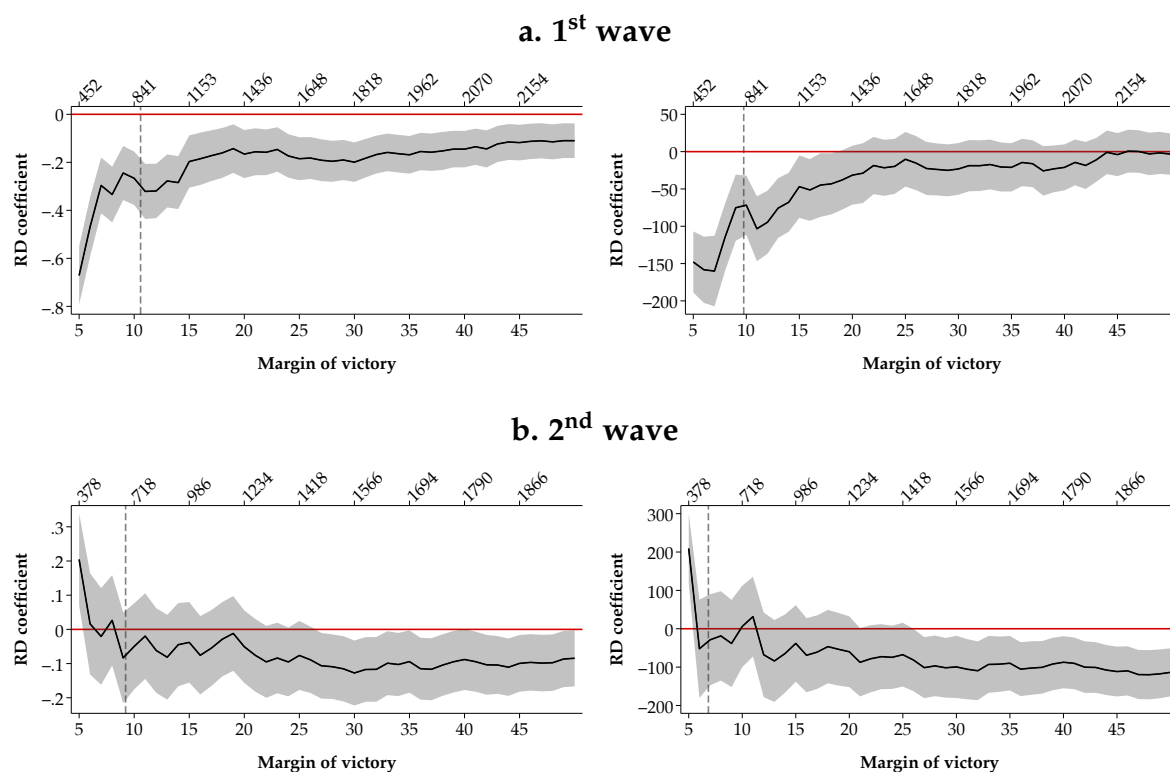


Fig. A2 Manipulation test of the running variable.



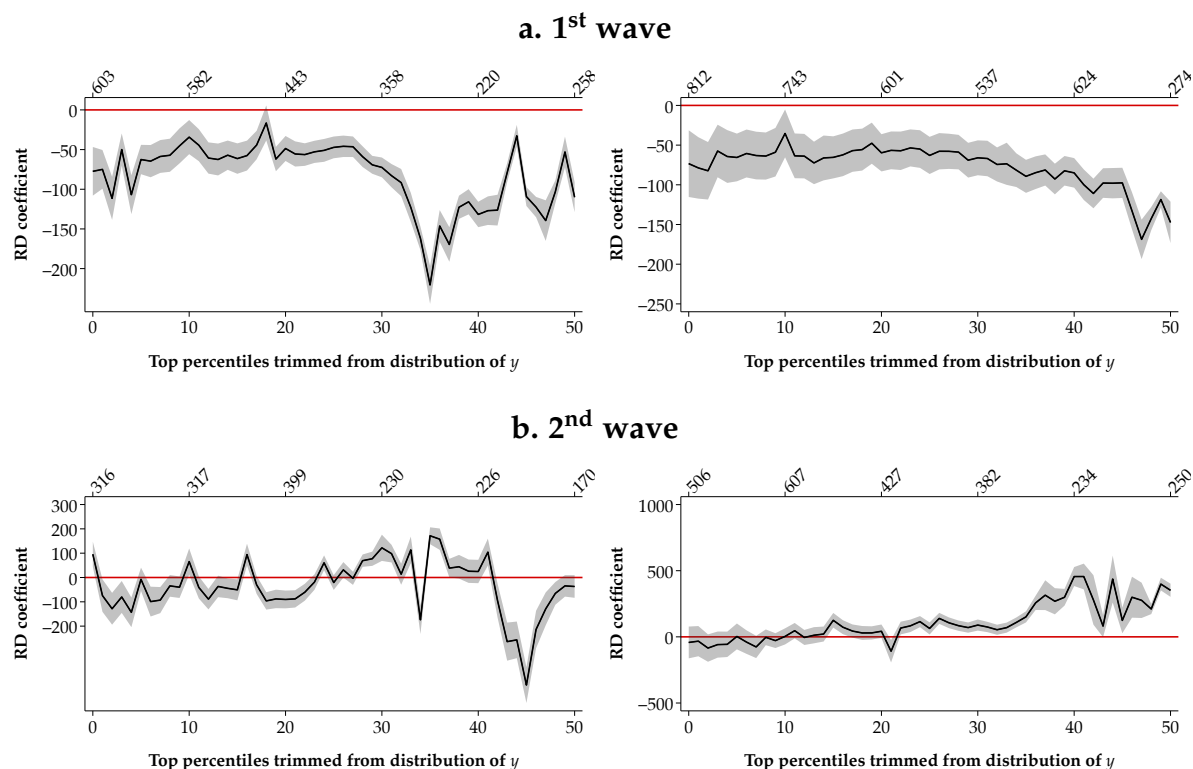
Notes: The robust bias-corrected t-statistic at the cutoff is 0.399 ($p = 0.690$).

Fig. A3 Sensitivity of treatment effect to bandwidth selection



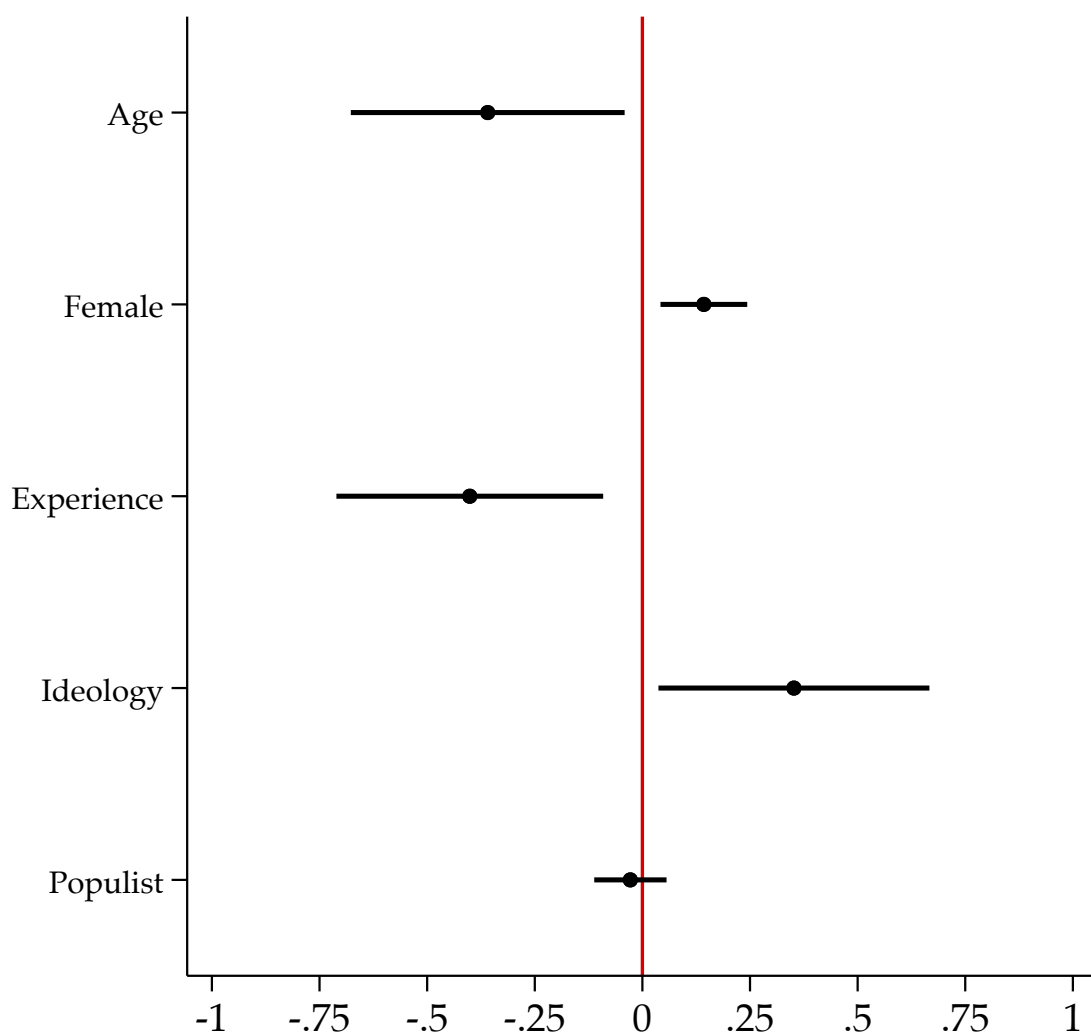
Notes: Sensitivity of treatment effect estimates—i.e. more educated mayor—to bandwidth selection in regression discontinuity designs using as outcome excess mortality at the extensive margin (left) and intensive margin (right) in the first (panel a.) and second (panel b.) pandemic wave. Bias-corrected RD estimates with uniform kernel from covariate-adjusted local quadratic regressions that use as bandwidths margins of victory between 5 and 50 percentage points in the decisive ballot of mixed-education elections, equal on both sides of the cutoff. The horizontal axes at the bottom report the bandwidth used, while those at the top report the respective sample size. The dashed vertical lines indicate MSE-optimal bandwidths obtained through the procedure described in [Calonico et al. \(2014\)](#), and adopted in baseline estimates of Tables 2 and 3, Columns 4 and 6. Shaded areas are 95% confidence intervals.

Fig. A4 Sensitivity of treatment effect to extreme values in the distribution of excess mortality rates



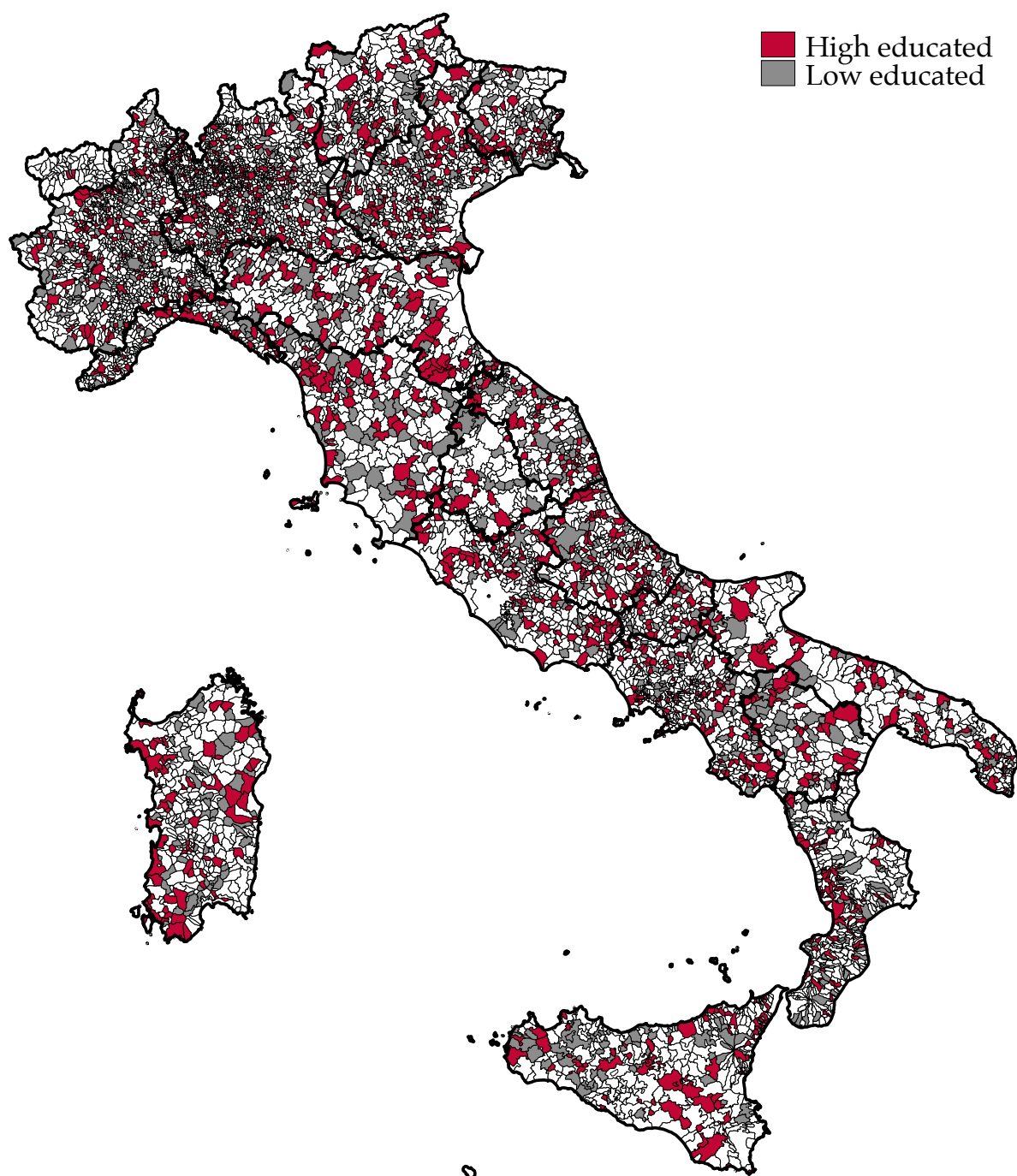
Notes: Sensitivity of treatment effect estimates—i.e. more educated mayor—to extreme values in the distribution of excess mortality rates used as outcomes in regression discontinuity designs in the first (panel a.) and second (panel b.) pandemic wave. Bias-corrected RD estimates with uniform kernel from covariate-adjusted local linear (left) and quadratic (right) regressions using MSE-optimal bandwidths equal on both sides of cutoffs obtained through the procedure described in [Calonico et al. \(2014\)](#). The horizontal axes at the bottom report the number of percentiles trimmed from the upper part of the distribution of dependent variables, while those at the top report the respective sample size. Shaded areas are 95% confidence intervals.

Fig. A5 Correlation between education and other pre-determined mayoral characteristics



Notes: Local quadratic bias-corrected RD estimates with uniform kernel and MSE-optimal bandwidths, equal on both sides of the cutoffs, obtained through the procedure described in [Calonico et al. \(2014\)](#). Robust standard errors, clustered by local labor system, included in all models.

Fig. A6 Geographical distribution of treated and control municipalities in the analytical sample



Chapter 3

This Must Be the Place: Accountability Consequences of Switching Ideology

This Must be the Place

Accountability Consequences of Switching Ideology

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Abstract

This paper examines the electoral consequences of ideology switching through the lens of accountability theory, with a specific focus on whether voters reward or punish ideological shifts at the ballot box. While prior research has produced mixed findings—primarily focusing on high-level elections and often conflating ideological change with performance—this study isolates the ideological component of switching. Using data from local elections in Italy, a context marked by frequent party switching and pronounced political polarization, I assess the re-election prospects of incumbent politicians who change ideological poles compared to those who remain ideologically consistent. Leveraging detailed fiscal performance data and employing propensity score matching to strengthen causal inference, the analysis reveals a significant electoral penalty for ideological switchers, with average vote losses intensifying when the ideological shift spans from one pole to the opposite. These findings offer three key contributions: (1) they underscore the symbolic importance of ideological congruence in retrospective voting; (2) they clarify the accountability dynamics at play in local elections, where voters are more directly connected to political actors; and (3) they highlight ideological switching as a political liability, reinforcing the role of party loyalty in maintaining representative trust and democratic integrity.

Keywords – Ideological switching, Electoral Accountability, Retrospective voting, Propensity score matching

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1 Introduction

Ideology is among the most important drivers of voting decisions. Traditional models of voting choice emphasize the key role of party identification and political cleavages in determining voters' preferences (Campbell et al. 1960; Lipset & Rokkan 1967). A basic prediction of such voting models concerns the required alignment between party positions and voters' preferences to ensure democratic representation. A deviation from this alignment has the potential to endanger the relationship between voters and parties, thus terminating ties of representation. Ample strands of literature have analyzed the dynamics of voter preferences and party systems (Pedersen 1979; Roberts & Wibbels 1999; Dassonneville 2018), documenting their implications for democratic representation. On the contrary, ideological misalignment of single politicians, rather than parties, vis-à-vis electorates has received limited attention in political sciences.

The decision to switch party by politicians is a widespread phenomenon in both established and new democracies, showing the largest incidence in countries that feature weaker party systems (Heller & Mershon 2009). The bulk of scholar inquiry revolves around the conditions under which and the reasons why politicians switch parties. Explanations to the occurrence of party switching include politicians' willingness to escape party discipline (Heller & Mershon 2008), join large dominant parties (Laver & Benoit 2003), avoid competitive candidacy runs (Aldrich & Bianco 1992), maximize electoral, ideological and distributional pay-offs (Desposato 2006), aspire to higher offices (Yoshinaka 2016). In sum, most research has considered switching party a dependent variable to be explained.

In this paper I change perspective and explore the electoral consequences of switching through the lenses of accountability theory (Pitkin 1967). Specifically, I ask whether switching party is a component of political accountability that voters internalize at the ballot box, either rewarding or punishing it. A few contributions report mixed evidence with respect to re-electoral fate of incumbent party switchers, mostly legislators running in high level elections (Grose & Yoshinaka 2003; Kerevel 2017; Yoshinaka &

McKee 2019) with only one exception considering local politicians (Hott & Sakurai 2021). However, previous studies are not able to isolate the ideological dimension of switching from politicians' performance in office, both of which are pivotal determinants of retrospective voting in accountability theory (Fox & Shotts 2009; Ashworth 2012).

The analysis presented here compares the probability of re-election of incumbent politicians switching from an ideological pole to a different one between two electoral rounds with that of politicians remaining loyal to their starting ideology. Data from local elections in Italy are used for this purpose, a country characterized by large incidence of switching among politicians and levels of political polarization that makes ideology salient to voters and candidates. Detailed fiscal data from municipality ensure an adequate measurement of local politicians' performance. Preliminary evidence suggests that politicians switching ideology fare much worse in the next elections compared to their loyal counterparts. A predicted average switching cost of 3 percentage points in the future election drops down to 12 percentage points when the ideologies considered are not adjacent but opposite on the continuum from left to right. Propensity score matching estimation provides a causal interpretation to the estimated relationships.

The contribution of this paper to the field is threefold. First, it reaffirms the relevance of ideology in accountability theory, not just through manifested policy preferences (Fox & Shotts 2009; Rogers 2017) but rather through symbolic ideological congruence. Politicians' switching might be perceived as a principled betrayal to the trustee/delegate relationship between her and voters. Second, it assesses the relationship between switching ideology and accountability in a direct, transparent way. While a vast majority of studies considers high level elections (i.e. they juxtapose many layers between electorates and candidates), my focus on local elections minimizes the distance between voters and politicians, thus allowing the former to hold the trusteeship to steady account for the behavior of the latter (Pitkin 1967). Third, my findings suggest that switching ideology is a liability for politicians. These results have important implications for political parties, voters, and policymakers, as they shed light on the importance of party

loyalty in strengthening democratic representation.

2 Background

Accountability, meaning the holding-to-account of the representative for her actions, stands as a defining component of democratic representation ([Pitkin 1967](#)). In representative democracies, representatives (politicians) and represented (citizens) are bound by a relationship of formal accountability. Elections provide an opportunity for citizens to evaluate the performance of politicians: a politician will be confirmed in office should her performance be evaluated positively by voters, otherwise she will be voted out of office. This retrospective voting framework is central to accountability theory.

Retrospective voting, as opposed to electoral selection (i.e. prospective voting), tends to prevail as heuristic among voters, suggesting a general preference towards accountability ([Woon 2012](#)). [Besley \(2006\)](#) notes, however, that backward-looking enables voters to select the best candidate in the upcoming election, thus reconciling two apparently opposite views. Numerous approaches to retrospective voting have been developed to understand how voters evaluate representatives, e.g. whether they conform to rational choice theory or are subject to cognitive and emotional bias ([Healy & Malhotra 2013](#) for a review).

Within this field of study, a plethora of hypotheses have been put forward concerning what do voters look back to in their decision to reward or remove the incumbent ([Fiorina 1978](#); [Anderson 2007](#); [Berry & Howell 2007](#)). [Ashworth \(2012\)](#) argues that voters observe past performance by the incumbent in order to infer the incumbent's type, such as her competence or ideology. Preference for competence as opposed to ideology has implications on how voters hold incumbents accountable ([Fox & Shotts 2009](#)). Voters who privilege competent types tend to retrospectively evaluate outcomes: in this scenario representatives act as trustees. On the other hand, voters who prefer ideological congruence look back at the incumbent's preferences and policy positions, considering their representative a delegate.

In either case, gathering information about politicians' performance or stance on a variety of issues is a costly process for voters. The expectation is that most voters cast their ballot with limited information about candidates. Under uncertainty, inferring candidates' types becomes harder and more imprecise. In this context, observing highly visible policy outcomes (e.g. tax cuts or infrastructure building) could represent an heuristic for voters to rate the incumbent's competence. Similarly, political cues such as party labels might serve as heuristics for voters to infer the incumbent's ideology (Downs 1957).

This paper focuses on politicians' ideology. Whereas candidates running under a certain party label are expected to take policy positions ideologically congruent with partisan stereotypes (e.g. an environment-friendly policy proposed by a Green candidate), a deviation from the stereotypical issue position would be punished by aware voters (Arceneaux 2008). The paper asks how voters react to the most counter-stereotypical move by partisan candidates: switching to a different party. Leveraging on a retrospective voting framework, I study the electoral consequences of party switching by incumbent politicians holding performance (i.e. inferred competence) constant, in order to investigate whether voters hold incumbents accountable for switching party. The aim is to understand whether party switching is a dimension of political accountability so far overlooked in the related literature.

Contributions to the study of party switching are provided almost entirely by political scientists. In most of the related literature, party switching is considered an outcome variable and scholars aim at rationalizing the drivers leading politicians to switch. The literature follows two main approaches to understand the decision to switch party (Mershon 2014). On the one hand, politicians are considered rational decision-makers who aim at maximizing rewards from being in office (e.g. being elected, rents, prestige, ambition). This is the *strategic approach*: politicians undergo a cost-benefit analysis that eventually lead them or not to decide to switch. On the other hand, the *institutional approach* focuses on the contextual conditions that result in the switching choice. Examples includes understanding the role of the party system

institutionalization, the longevity of democratic regimes, party discipline, electoral system.

Within the tradition of the strategic approach, [Aldrich & Bianco \(1992\)](#) posit a model where ambitious candidates switch party to maximize the probability of winning elections. [Laver & Benoit \(2003\)](#) show that the most dominant party in a legislature attracts the most mobile among the members of parliament (MPs). [Desposato & Scheiner \(2008\)](#) find that the degree of centralization of resources affects party affiliation patterns in Brazil and Japan. Studying Italy, [Heller & Mershon \(2008\)](#) observe that Italian MPs switch partly in order to escape strong discipline.

Examples of research following the institutional include the following. [Desposato \(2006\)](#), while noticing that Brazilian legislators use parties to maximize pork, ideological consistency, and short-term electoral success, find that the motivation for switching depends, but which of these matters most depends on the characteristics of the electoral districts. Similarly, in their paper [Heller & Mershon \(2008\)](#) further highlight that switching patterns depend on the features of the electoral districts.

A few exceptions in the literature consider instead party switching as a key independent variable and look at its effects. For instance, [Grose & Yoshinaka \(2003\)](#) find that the election vote shares for party switchers decrease after switching, although their analysis lacks solid empirical evidence. Differently, [Hott & Sakurai \(2021\)](#) show that the impact of party switching on re-election is positive in Brazilian municipalities. Similarly to the latter, this paper considers party switching a treatment, and evaluates the electoral consequences for politicians who decide to switch.

A desirable political setting to answer the research question is one in which party switching occurs, where it is clear to voters where parties are placed on the ideological spectrum, and where politicians can be held accountable directly for their performance in office. The Italian political context offers the ideal conditions. In fact, party switching has been affecting Italian politics since its unification in 1861. The deep historical roots of party switching led to the creation of a dedicated word to describe it with a negative connotation, namely *trasformismo* ([Valbruzzi 2015](#)). Citizens are aware of this pervasive

phenomenon in Italian politics, and occurrences of party switching often make the headlines of national news. Party switching in upper tiers of (the Italian) government has been already the subject of scholar inquiry. However, MPs do not represent ideal units of observation for my purposes here for two reasons. First, the features of the Italian electoral system vary greatly over time and do not bind politicians to run in specific areas of the country: observing the same politician running twice in the same area does not necessarily occur, making the comparison of the probabilities of election non-rigorous. Second, the large scale of policies at the national level involves a plurality of actors to work on them, which obscures the link between politicians' competence and political outcomes: attribution of responsibility to MPs in case of a positive/negative outcome might be spurious, and other direct measures of politicians' performance can be observed by voters with difficulty. Therefore, the political context under examination in the paper consists of municipalities, and the politicians taken as units of observation are mayors.

3 Institutional setting

As of 2023, Italy counts 7,901 municipalities, a number that has decreased from a maximum of 8,100 due to municipality mergers and unions. Italian municipalities represent the lowest administrative unit, below the *central government*, 20 *regional governments*, and 110 *provincial governments*. The functioning of municipalities is disciplined by *Legislative Decree of 18 August 2000*. Each municipal government is constituted by three institutions: the mayor, the executive committee, and the city council. The mayor holds the executive power and carries out several tasks prescribed by the Italian law, such as supervising over the organization of municipal services and the execution of laws, appointing the members of the executive committee (who cooperate with the mayor in the administration of specific delegated functions), issuing executive decrees, appointing public officers, external professionals and municipality's representatives. In particular, every year the mayor, together with the executive committee, proposes

the *provisional accounts* document, aimed at planning the municipal fiscal policy over the next year: the document sketches the amount and sources of revenues that the municipality plans to collect and the amount and destinations of expenditures it commits to spend. At the end of each financial year, the executive committee drafts the *final accounts* document, which summarizes the fiscal policy pursued over the past year by certifying the amount and the allocation of resources received and paid. Both documents are subject to approval by the city council which votes with majority rule.

In 2000 the length of the municipal legislature was extended from four to five years. Municipal elections take place accordingly. Voters elect the members of the city council and, since 1993, the mayor of the municipality. Municipalities below 15,000 residents adopt a single round electoral system wherein a mayoral candidate is endorsed by one list of candidates to the city council: the mayoral candidate obtaining the relative majority of votes becomes the mayor, and a premium consisting in two thirds of city council's seats is granted to her. Differently, municipalities above 15,000 residents implement a runoff electoral system where, in case no candidate gets the absolute majority of votes in the first round, the winner among the two most voted candidates is determined through plurality rule in the second round: each mayoral candidate is endorsed by one or more lists of candidates to the city council, and at least 60% of city council's seats is granted to the winner. According to an open list system, voters are allowed to cast their preference toward one candidate councillor¹ in the same list endorsing the mayoral candidate they vote for (municipalities below 15,000 residents), or in any available list even if different than the one endorsing their preferred mayoral candidate (municipalities above 15,000 residents). The composition of the city council is determined according to the majority premium granted to the list(s) endorsing the winning mayoral candidate and to a proportional mechanism that assigns the residual seats among the list(s) endorsing losing mayoral candidates. The so-determined number of seats in the city council won by each list is awarded to the council candidate receiving most votes of preference. Mayors face a two-consecutive-

¹Since 2013, voters can indicate up to two preferences for the city council in municipalities above 5,000 residents, provided that the two chosen candidates are of opposite gender.

term limit, meaning that they cannot run for the mayoral position a third consecutive time². Some institutional features present slight differences in five regions *with special autonomy*, however these nuances do not affect the trends shown in analysis³.

4 Data

4.1 Electoral results

Data are sourced from the *Historical Electoral Archive*⁴, an online platform operated by the Italian Ministry of Internal Affairs. It collects electoral results from each election at different levels of government taking place in Italy since 1946. Municipal (i.e. mayoral) election results are available since the 1993 round. Collected information includes: election date, number of eligible and actual voters, number of blank and null votes, full names of running candidates, number of votes they received, winner status, names of their supporting list(s), number of valid votes and numbers of council seats assigned to list(s).

4.2 Ideology

The Italian political context is volatile and fragmented, characterized by frequent changes of party denominations, mergers between different parties, dissolutions of previously united parties. Nested in this context, local Italian politics is likewise highly dynamic. Therefore, it is not straightforward to capture substantive ideological differences in small-scale political environments. While it is correct to claim that a politician first elected in a party is formally a switcher in case she runs under a different party label in the following election, it should not be taken for granted that a name change corresponds to a substantive switch or that electorates rationalize it as such. Categorizing a politician as switcher using a simplistic criterion (i.e. after her move to a

²They can if either of the two consecutive terms lasted less than two years and a half. Since 2014, a third consecutive term is allowed in municipalities below 3,000 residents.

³Nor results are affected by the exclusion of municipalities in such regions.

⁴<https://elezionistorico.interno.gov.it>.

different party label) might lead to (i) define as switchers a huge number of politicians, and to (ii) fail to capture the real content of switching, over- or under-estimating its consequences.

The picture is even more blurred at the municipality level because national political parties constitute only a fraction of lists among which voters can choose: it is in fact common to observe candidates endorsed by *civic lists*, in particular in smaller municipalities. These are tiny local parties whose platforms encompass small-scale policies targeting local issues. Civic lists are disengaged from macro-political trends and act outside of established parties' discipline. Given these peculiarities, a civic lists stand as political entities of their own. They might be hardly, or wrongly, mapped onto clear ideological positions, such as the left–right spectrum.

Key to answering the research question is to distinguish between different lists over an ideological dimension. Whereas civic lists constitute a separate pole and might not be associated to specific political positions, it is possible to measure the relative ideological distance between national political parties. Given the long sample period (i.e. from 1993 to 2023), this is done by combining three expert surveys, namely:

- *Chapel Hill Expert Survey (CHES)*: conducted for six waves since 1999, it asks the opinion of experts about parties' position on several issues and policies (Jolly et al. 2022); in particular, experts assess the position of parties in terms of their overall ideological stance, which ranges between '0 – Extreme Left' to '10 – Extreme right';
- *ParlGov*: it harmonizes and aggregates party positions from other expert surveys⁵; the left/right position included in this dataset mimics that available in CHES and results from aggregation of Mair & Castles (1984), Huber & Inglehart (1995), Benoit & Laver (2006), and the 2010 edition of CHES⁶;
- *V-Dem Varieties of Party Identity and Organisation*: it provides expert assessments of party organization and identity (Lührmann et al. 2020); in particular, experts

⁵<https://www.parlgov.org>.

⁶Original values are first rescaled between 0 and 10, then are averaged to obtain a mean left/right measure; missing party positions are imputed by mean values for the respective party family.

locate parties in terms of their overall ideological stance on economic issues, from '0 – Far-Left' to '6 – Far-right'⁷.

The aggregation of the three expert surveys makes left/right ideology data available for 94 Italian parties, matching with a large share of national party lists supporting candidates in municipal elections. A minority of parties have been assessed separately in all surveys, such that up to three different ideologies *per* party may be available overall (showing pairwise correlation coefficients larger than .92). In subsequent analyses, ideology ratings are assigned to parties based on the hierarchy of sources shown in the list above⁸.

As the goal is to capture the effect of substantive ideological switching, national parties are sorted into three ideological poles according to the distribution of their combined left/right location. Specifically, the overall distribution is divided in tertiles, resulting in:

- A *Left* pole with parties ranging between a score of .5 (*Democrazia proletaria*) and 4.5 (*Partito Socialista Democratico Italiano*);
- A *Center* pole with parties ranging between a score of 4.6 (*Lista Bonino*) and 6 (nine different parties);
- A *Right* pole with parties ranging between a score of 6.1 (*Nuovo Centro Destra*) and 9.7 (*Movimento Sociale Fiamma Tricolore*).

A *Civic* pole featuring independent civic lists is the fourth ideology considered in this paper. It is thus assumed that purely civic candidates, not being endorsed by mainstream party lists, choose not to be placed on the left-right political spectrum: as such, switching to (or from) the *Civic* pole characterizes a substantive switch insofar it involves a clear, explicit choice to abandon (or join) a party label rooted in a polarized political environment.

⁷The variable is rescaled to the range 0–10 to ensure comparability with the other data sources.

⁸Descriptive statistics and results remain virtually identical if either source is preferred as first.

4.3 Measures of performance

The final key information is a measure of mayors' performance in office. Following a retrospective voting framework, voters infer politicians' competence by looking at their performance before deciding to confirm or vote them out. Municipal financial outcomes are ideal proxies of mayoral performance subject to public evaluation for two reasons. First, as indicated in the description of the institutional setting, the executive power of mayors encompasses the definition of the budget, making them directly accountable for municipal fiscal policy. Second, the pervasiveness of municipal operations in several domains of citizens' life grants financial outcomes, such as tax levels, a high visibility to voters' eyes. A rich set of fiscal outcomes derived from final municipal accounts are collected by the Ministry of Internal Affairs over the period 1998–2015. Data are scraped from the Ministry's website⁹ and encompass both aggregate revenues and expenditures and revenue and expenditure components. Fiscal outcomes are considered on a *per capita* basis and expressed in real thousand euros: I compute their growth rates averaged over the three central years of mayors' 5-year-long term¹⁰, so to avoid election years¹¹. Due to unrealistic extreme values observed in the raw data source, the top 1% of the respective distributions is winsorized.

5 Empirical strategy

The analytic sample consists of elected mayors who stand for re-election for another legislature in the same municipality in the period 1993–2023. The goal is to measure the effect of switching ideology on re-election results. Equation (1) summarizes this relationship:

⁹<https://dait.interno.gov.it>.

¹⁰To maximize sample size, for elections taking place in 2013 (2012) I consider the average growth rate over the (two) following year(s).

¹¹Because the date of election occurs, for the vast majority of municipalities, in the middle of the year, fiscal outcomes in election years result from the decisions of two different local administrations, for which the incoming mayor is only partially accountable. Upcoming results remain robust if fiscal outcomes in election years are included in the average. Incidentally, this test suggests that political budget cycles (Nordhaus 1975; Rogoff 1990) have limited influence on the estimates.

$$Re-election_{m,L+1} = \alpha + \beta Switch_m + \gamma Perf_m + \mathbf{X}'\delta + \eta_g + \zeta_y + \varepsilon_{m,L+1} \quad (1)$$

where $Re-election_{m,L+1}$ is a binary variable taking value one if mayor m in office in legislature L is re-elected as mayor for another legislature $L + 1$, $Switch_m$ is a dummy variable equal to one if mayor m switched from an ideology to a different one between legislatures L and $L + 1$ ¹², $Perf_m$ is a measure of mayor performance during her term in office, $\mathbf{X}$ is a vector of covariates capturing characteristics of elections, η_g are geographical fixed effects, ζ_y are years of election fixed effects; α is a constant term, and $\varepsilon_{m,L+1}$ is an error term.

The main coefficient of interest is β , as it captures if and to what extent a mayor switching parties faces a different probability of re-election compared to a non-switching mayor (i.e. a *loyal* mayor). The coefficient γ measures how do voters evaluate past performance by the incumbent mayor. Enriching the equation with a variety of covariates that are expected to be correlated with both the treatment and the outcome reduces bias from omitted variables. The addition of region/province fixed effects ensures that β does not simply capture regional/provincial unbalances in terms of social, economic and political characteristics. Similarly, including year fixed effects isolates underlying time trends common to all municipalities.

In a second specification, switching status (my proxy for ideological coherence) and retrospective performance (my proxy for inferred competence) are interacted so as to understand whether the electoral consequences of switching parties are reinforced or counteracted by competence, a key component of political accountability. This is illustrated in [Equation \(2\)](#):

$$Re-election_{m,L+1} = \alpha + \beta Switch_m + \gamma Perf_m + \lambda Switch_m * Perf_m + \mathbf{X}'\delta + \eta_r + \zeta_y + \varepsilon_{m,L+1} \quad (2)$$

¹²Data do not record when switching actually occurs, impeding a complete understanding of the reasons behind the decision to switch.

A modified specification allows to account for effects stemming from different switching patterns, which depend on the origin and destination ideology. Accordingly, there are $4 \times 3 = 12$ potential patterns of switching, which renders the treatment $Switch_m$ a categorical variable. Similarly, it is possible to distinguish among four different patterns of loyalty depending on what pole current mayors choose to run again for. Equation (3) shows the former specification, while Equation (4) the latter:

$$Re-election_{m,L+1} = \alpha + \beta Switch_{m,P_O,P_D} + \gamma Perf_m + \mathbf{X}'\delta + \eta_r + \zeta_y + \varepsilon_{m,L+1} \quad (3)$$

$$Re-election_{m,L+1} = \alpha + \beta Loyal_{m,P} + \gamma Perf_m + \mathbf{X}'\delta + \eta_r + \zeta_y + \varepsilon_{m,L+1} \quad (4)$$

where $P = \{Left, Center, Right, Civic\}$ indexes the ideological pole mayor m belongs to, being it an origin pole, P_O , different from the destination pole, P_D , in case of switching mayors.

Considerations of selection must be taken into account in order to strengthen the causal interpretation of the relationships. To do so, I implement propensity score matching to measure the effect of switching ideology on the probability of re-election. I estimate the Average Treatment Effect (ATE) and the Average Treatment Effect on the Treated (ATT) through the STATA command `teffects psmatch` by matching each switching mayor to, alternatively, one, five or ten nearest neighbouring non-switching mayors. The covariates considered to perform the propensity score calculations include also mayors' personal characteristics such as sex, age group and education level.

In what follows, the parameters of interest are estimated through a linear probability model to ease the interpretation of results¹³. The first stage of propensity score matching, i.e. the prediction of treatment status based on the values of covariates, is estimated through a logistic regression (not reported). Table 1 presents descriptive statistics of the analytic sample used for estimation.

¹³Results are fully robust to logit and probit estimation.

Table 1 Descriptive statistics

	N	Mean	St. Dev.	Min	Max
Re-election	15,986	0.784	0.411	0	1
Switcher	15,986	0.232	0.422	0	1
<i>Switching trajectory</i>					
Left to Center	15,986	0.004	0.066	0	1
Left to Right	15,986	0.001	0.040	0	1
Left to Civic	15,986	0.078	0.269	0	1
Center to Left	15,986	0.008	0.093	0	1
Center to Right	15,986	0.006	0.078	0	1
Center to Civic	15,986	0.049	0.217	0	1
Right to Left	15,986	0.001	0.042	0	1
Right to Center	15,986	0.002	0.049	0	1
Right to Civic	15,986	0.032	0.176	0	1
Civic to Left	15,986	0.022	0.148	0	1
Civic to Center	15,986	0.010	0.101	0	1
Civic to Right	15,986	0.01	0.114	0	1
Male	15,401	0.916	0.276	0	1
<i>Age group</i>					
18 – 40	15,401	0.140	0.347	0	1
41 – 50	15,401	0.332	0.471	0	1
51 – 60	15,401	0.330	0.470	0	1
61 – 70	15,401	0.169	0.375	0	1
71 – 91	15,401	0.027	0.163	0	1
<i>Education level</i>					
Lower secondary or less	15,119	0.123	0.328	0	1
Upper secondary	15,119	0.450	0.497	0	1
Tertiary	15,119	0.426	0.494	0	1
<i>% Growth in</i>					
Current revenues	8,779	0.531	0.876	-1.309	4.801
Transfers	8,817	1.148	3.109	-2.390	27.762
Total revenues	8,769	0.238	0.439	-1.106	2.548
Current expenditures	8,778	0.065	0.152	-1.058	0.574
Capital expenditures	8,842	8.04	25.017	-2.819	331.515
Total expenditures	8,770	0.175	0.361	-1.105	2.254

6 Results

Table 2 reports results from various estimations of Equation (1), without measures of performance. Column 1 controls for region fixed effects and both year of first and second election fixed effects. On average, a switching mayor faces a probability of being

re-elected of 3 percentage points lower compared to a loyal mayor, which is statistically significant at the 1% level. Column 2 adds controls for both elections, namely the decisive electoral turn, turnout rates, total number of running candidates, number of lists endorsing the incumbent, share of ideological lists within the incumbent's coalition, and the summary pole of the incumbent's coalition. Results remain stable. Column 3 adds province fixed effects, such that the coefficient of *Switching* captures variation within provinces. Still the effect of switching is unaffected. Column 4 is the most demanding estimation featuring two sets of region by year fixed effects aimed at capturing any shock at the regional level occurring in the election year. While the significance and magnitude of the *Switching* effects decreases slightly, the fit of the model increases. Column 5 removes from the sample the civic incumbent mayors, leaving only mayors that are endorsed by ideological parties. The effect size almost doubles, although the relationship loses some degrees of significance¹⁴. Finally, Column 6 removes also mayors affiliated to the center. When switching from opposite political poles, the probability of re-election of incumbent mayors drastically drops by almost 13 percentage points. Incumbent mayors who switch party are confirmed in office significantly less than loyal mayors. In particular, mayors who decide to cross the political spectrum from one election to the other face very negative odds of winning again.

Table 3 adds to the models the incumbents' performance on the revenues side of budget. Odd-numbered columns estimate the full Equation (1), while even-numbered column estimate the interaction model summarised by Equation (2). Controlling for the revenue performance of mayors increases the magnitude of the *Switching* coefficients, which are significant at the 95% level. Surprisingly, a growth in taxes leads voters to re-elect the incumbent, while a decrease in transfers translates into a lower probability of re-election. The overall growth in revenues has a positive effect on re-election outcomes, although none of the performance measure shows a significant relation to the outcome. The interaction models show that the effect of switching does not depend

¹⁴The resulting drop in the number of observation provides less statistical power.

Table 2 Ideology Switching and Probability of Reelection

	Probability of reelection					
	(1)	(2)	(3)	(4)	(5)	(6)
Switching	-0.033*** (0.009)	-0.030*** (0.010)	-0.029*** (0.010)	-0.027** (0.011)	-0.053* (0.027)	-0.126** (0.063)
Region FE	✓	✓	×	✓	✓	✓
Province FE	×	×	✓	×	×	×
Year L FE	✓	✓	✓	✓	✓	✓
Year L+1 FE	✓	✓	✓	✓	✓	✓
Region by year L FE	×	×	×	✓	×	×
Region by year L+1 FE	×	×	×	✓	×	×
Election L controls	×	✓	✓	✓	✓	✓
Election L+1 controls	×	✓	✓	✓	✓	✓
Observations	15,989	15,989	15,989	15,989	2,691	2,044
R ²	0.039	0.079	0.086	0.137	0.134	0.171

Notes: Robust standard errors are reported in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

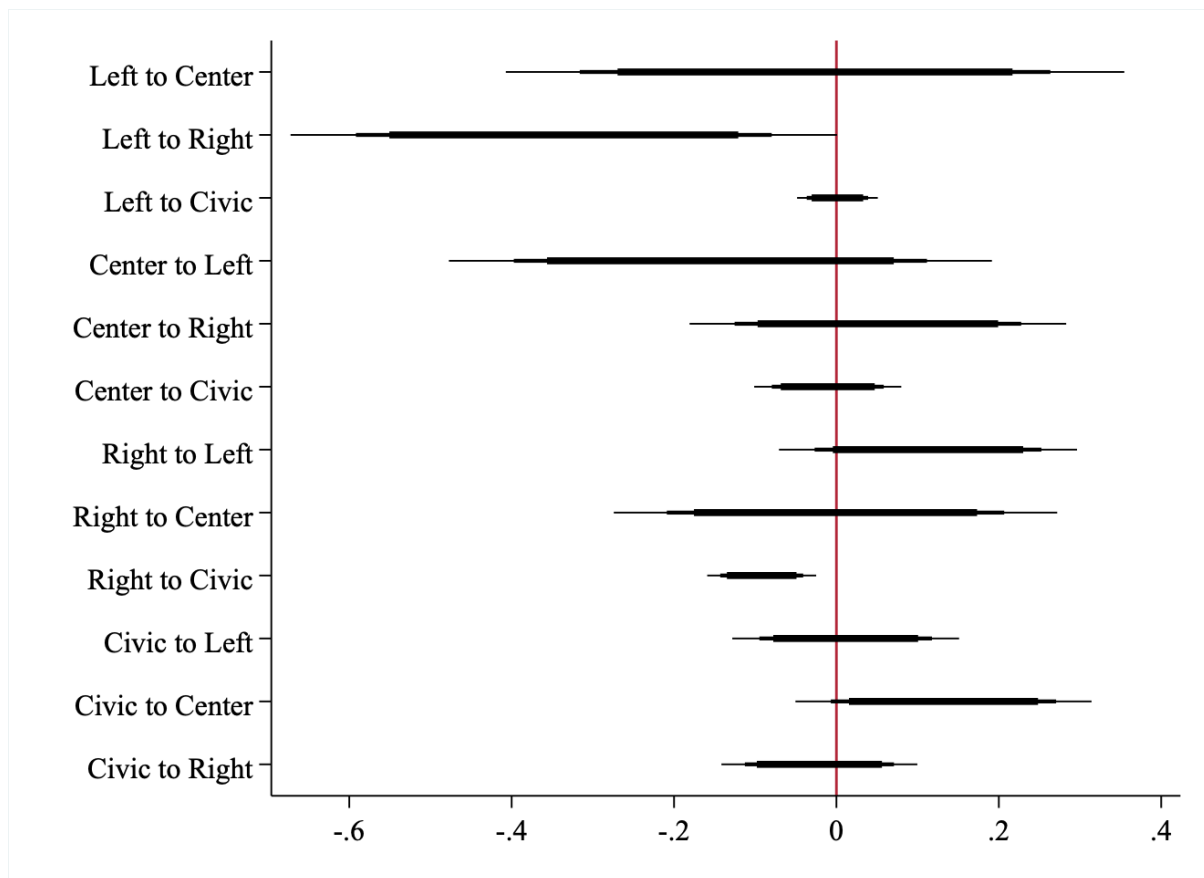
on the revenue performance produced by the incumbent.

[Table 4](#) tests the importance of incumbents' performance in terms of expenditure operations. The coefficients show qualitatively and quantitatively patterns similar to those highlighted in the previous table. A growth in current (capital) or total expenditure is non-significantly associated to larger (smaller) probability of re-election. The interactions are again insignificant, suggesting that modifying the expenditures levels leads switching mayors neither to lose more nor less in the next electoral consultation. Mayors who switch party are not masking a negative budgetary performance. At the same time, voters do not reward switching politicians even if they produced positive performance while in office. Ideology is thus unrelated to performance in the eyes of voters.

[Figure 1](#), which depicts estimation results of [Equation \(3\)](#). When the *Switch* dummy is decomposed, it emerges that the incumbents most damaged by the switching decision are those who move from the left to the right, followed by the center to left pattern and the opposite one from left to center (the latter two not showing significant relationships though). A significant, yet less large negative effect occurs for incumbent mayors affiliated to the right who move towards the civic pole.

On the opposite, [Figure 2](#) shows how loyal mayors are rewarded at the next election,

Fig. 1 Patterns of Switching and Probability of Re-election



Notes: The coefficients plotted relate to the switching trajectories of incumbent mayors from a political ideology of origin to a political ideology of destination. Coefficients are estimated from a regression of the probability of re-election of incumbent mayors which includes region fixed effects, year fixed effects and characteristics of the elections, and performance in terms of revenues and expenditures. Thick, medium, and thin lines correspond to, respectively, 90%, 95% and 99% confidence intervals.

Table 3 Ideology Switching, Revenue Performance and Probability of reelection

	Probability of reelection					
	(1)	(2)	(3)	(4)	(5)	(6)
Switching	-0.039** (0.019)	-0.032 (0.021)	-0.041** (0.019)	-0.045** (0.020)	-0.040** (0.019)	-0.045** (0.021)
Growth in taxes	0.001 (0.007)	0.003 (0.007)				
Switching * Growth in taxes		-0.017 (0.018)				
Growth in transfers			-0.001 (0.001)	-0.001 (0.002)		
Switching * Growth in transfers				0.003 (0.004)		
Growth in total revenues					0.016 (0.011)	0.013 (0.011)
Switching * Growth in total revenues						0.021 (0.030)
Observations	8,779	8,779	8,817	8,817	8,769	8,769
R ²	0.084	0.084	0.084	0.085	0.085	0.086

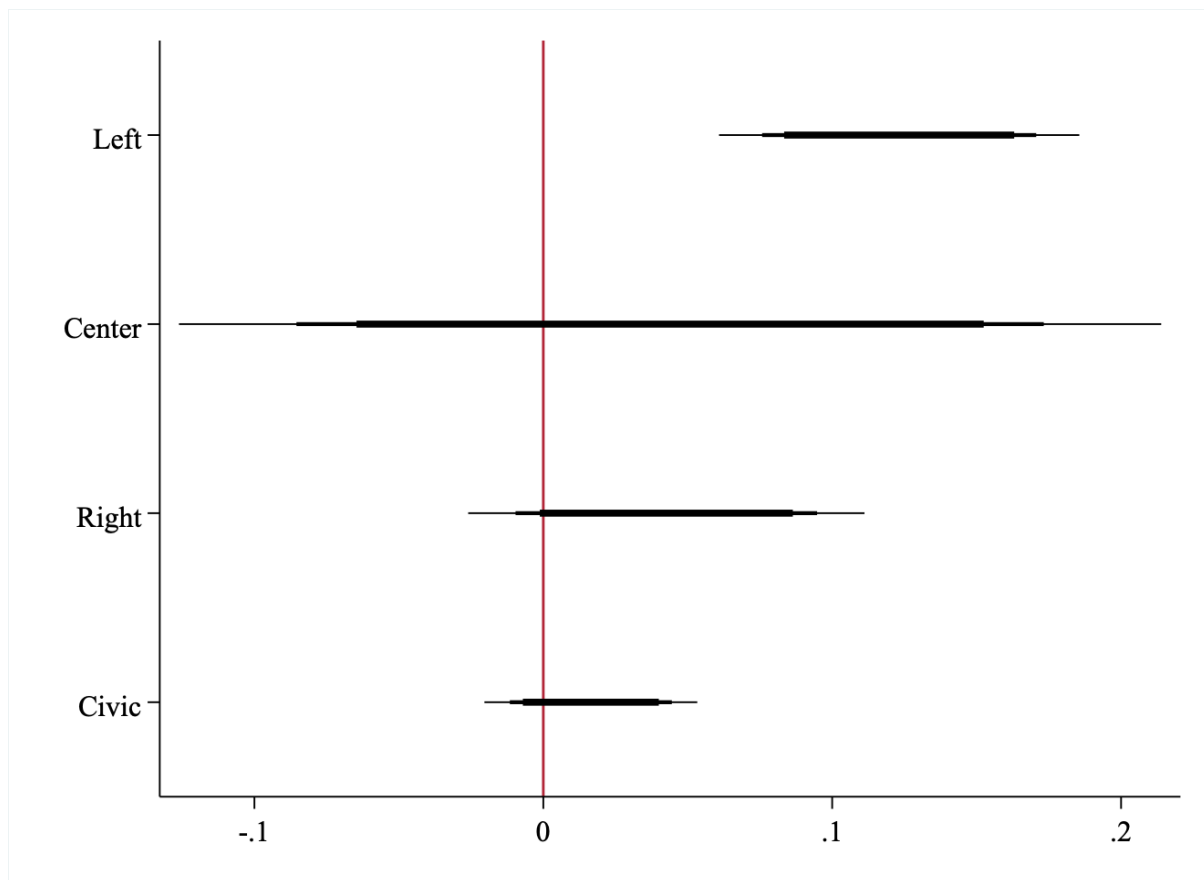
Notes: All specifications include region fixed effects, years of election L and $L + 1$ fixed effects, controls for election L and $L + 1$. Robust standard errors are reported in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

independently of the performance produced during the first term. On average, the mayors who are more likely to be confirmed in office are the ones backed by left political lists.

Table 5 reports propensity score matching estimations. In Panel A the matching is implemented only using mayors' personal characteristics. Estimated effects are of similar magnitude compared to those from the linear probability models and highly statistically significant. Increasing the number of nearest neighbours has none or little effect on coefficients. Average Treatment Effects on the Treated are only slightly larger than Average Treatment Effects, suggesting that treated and control mayors show overall high similarity with respect to the distribution of covariates. Estimations in Panel B also consider the measures of fiscal performance to perform the matching. Effect size increases from 30% to more than 100% depending on the estimation. Overall, this exercise reassures that selection concerns stemming from observable mayoral characteristics and performance do not affect results. Still, I cannot fully exclude the possibility that unobserved confounders are at play or that some of the covariates considered are endogenous.

Fig. 2 Patterns of Loyalty and Probability of Re-election



Notes: The coefficients plotted relate to the political ideologies that incumbent mayors remain loyal to. Coefficients are estimated from a regression of the probability of re-election of incumbent mayors which includes region fixed effects, year fixed effects, characteristics of the elections, and performance in terms of revenues and expenditures. Thick, medium, and thin lines correspond to, respectively, 90%, 95% and 99% confidence intervals.

Table 4 Ideology Switching, Expenditures Performance and Probability of reelection

	Probability of reelection					
	(1)	(2)	(3)	(4)	(5)	(6)
Switching	-0.042** (0.019)	-0.046** (0.020)	-0.039** (0.019)	-0.043** (0.020)	-0.040** (0.019)	-0.045** (0.020)
Growth in Cur. Expenditures	0.025 (0.030)	0.018 (0.032)				
Switching * Growth in Cur. Expenditures		0.051 (0.085)				
Growth in Cap. Expenditures			-0.000 (0.000)	-0.000 (0.000)		
Switching * Growth in Cap. Expenditures				0.000 (0.001)		
Growth in Total Expenditures					0.006 (0.013)	0.001 (0.014)
Switching * Growth in Total Expenditures						0.032 (0.037)
Observations	8,778	8,778	8,842	8,842	8,770	8,770
R ²	0.084	0.084	0.085	0.085	0.084	0.085

Notes: All specifications include region fixed effects, years of election L and $L + 1$ fixed effects, controls for election L and $L + 1$. Robust standard errors are reported in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

7 Conclusion

This paper has examined ideology switching from the perspective of democratic accountability, asking whether voters sanction incumbent mayors who change ideological pole between elections. Focusing on Italian municipal elections over three decades, I have brought together detailed electoral data, a classification of parties into four ideological poles, and rich measures of fiscal performance to separate the symbolic dimension of switching from the competence signals usually emphasized in retrospective voting.

The central empirical result suggests that switching ideology is electorally costly. Across a wide range of specifications, incumbent mayors who move from one ideological pole to another are significantly less likely to be re-elected than ideologically loyal incumbents. The average penalty is on the order of three percentage points in the probability of re-election, a non-trivial loss in typically competitive local races. When attention is restricted to “hard” switches across the left–right divide—excluding civic and centrist trajectories—the estimated penalty becomes much larger, approaching a 13-point drop in re-election probability. These patterns are robust to demanding fixed

Table 5 Ideology Switching and Probability of Reelection – Propensity Score Matching

	<i>nn</i> = 1	<i>nn</i> = 5	<i>nn</i> = 10
<i>Panel A (N = 15, 113)</i>			
ATE	-0.033*** (0.008)	-0.033*** (0.008)	-0.034*** (0.008)
ATT	-0.038*** (0.008)	-0.038*** (0.008)	-0.038*** (0.008)
<i>Panel B (N = 7, 962)</i>			
ATE	-0.069*** (0.018)	-0.053*** (0.015)	-0.055*** (0.015)
ATT	-0.056** (0.018)	-0.049*** (0.015)	-0.050*** (0.014)

Notes: *nn* stands for nearest neighbour. Robust standard errors are reported in parentheses.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

effects, extensive controls for electoral context, and alternative estimators including propensity score matching.

A second key finding is that this switching penalty is not explained away by performance in office. Using granular data on municipal revenues and expenditures, I find little evidence that switching mayors are systematically worse (or better) performers than their loyal counterparts. Voters do respond to some aspects of fiscal policy—such as changes in tax revenues and intergovernmental transfers—but the negative association between switching and re-election persists after conditioning on these outcomes. Interaction models show no meaningful moderation of the switching effect by fiscal performance: competent switchers do not seem to be forgiven, and incompetent loyalists are not punished to the same extent as ideologically inconsistent incumbents. In the eyes of voters, the act of crossing ideological camps appears to carry its own accountability weight, distinct from standard performance considerations.

A third set of results shows that not all switches, and not all loyalties, are treated alike. Decomposing the treatment into specific origin–destination trajectories reveals particularly severe penalties for mayors who cross from the left to the right, and sizable,

albeit less precisely estimated, penalties for moves from ideological poles to purely civic lists. By contrast, mayors who remain loyal to their original camp, especially those backed by left parties, enjoy a systematic re-election advantage. These patterns suggest that voters do not just notice whether incumbents move; they also care about where they move from and to, and they reward consistent positioning in a polarized party environment.

Substantively, the findings speak to the role of ideology in retrospective voting. Existing work has often framed accountability as a trade-off between competence and policy congruence, with voters either rewarding good outcomes or punishing ideological divergence. In this paper, ideology operates in a more symbolic way: switching sends a strong signal about the representative's type—about their reliability as a delegate or trustee—which voters appear to interpret as a breach of the representative link, even when policy performance is held constant. In a context where party labels are salient and party switching has a long and publicly salient history, switching ideology looks less like pragmatic adaptation and more like a violation of expectations about who the representative is and whose side she is on.

The analysis also has implications for how we think about accountability at the local level. Municipal politics offer a setting in which voters arguably have clearer lines of sight to both performance and behavior than in higher-level elections. That voters in this setting penalize ideological switching suggests that they are willing and able to use partisan cues to discipline office-holders, not only for what they do, but also for where they stand. At the same time, the evidence that performance does not fully offset the switching penalty raises uncomfortable questions: voters may be willing to punish ideological inconsistency even when it comes with competent governance, and to reward ideological loyalty even when performance is mediocre. Accountability, in other words, is not purely technocratic; it is also deeply bound up with identity and symbolic representation.

Naturally, the study has limitations. It cannot fully adjudicate between different micro-mechanisms behind voters' reactions: whether they perceive switchers as oppor-

tunists, as traitors to their original camp, or as unreliable future agents. Nor can it rule out all unobserved confounding, despite extensive controls and matching. The Italian local context is also distinctive, with a long history of switching and a fragmented party system; generalizing the exact magnitudes of the switching penalty to other settings should be done with caution. Nonetheless, the core patterns—an independent electoral cost of ideological switching and a premium to loyalty—are likely to travel to other democracies where party labels and cleavages structure political competition.

For parties and politicians, the message is straightforward. Switching ideological camp is not an electorally neutral maneuver; it is a liability that voters notice and often punish. For scholars, the results underline that party switching is not only an outcome to be explained by strategic or institutional incentives, but also an important input into accountability processes. For democratic theory, the findings suggest that the stability of representative linkages depends not only on policy performance and institutional design, but also on the perceived continuity of ideological commitments over time. In this sense, ideology remains the place where voters locate their representatives—and leaving that place comes at a cost.

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