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ABSTRACT

The Demographic Impact of Leadership: Mayoral Education and COVID-19 Excess Mortality in Italy*

This paper investigates whether the characteristics of locally elected officials influenced excess mortality during the COVID-19 pandemic. Using data on Italy, one of the first countries to be severely affected, we examine whether mayoral education influenced municipal-level mortality outcomes. We estimate weekly excess mortality using official death statistics and a Bayesian hierarchical spatio-temporal model. To address endogeneity in political selection, we implement a close-election Regression Discontinuity Design. We find that college-educated mayors significantly reduced mortality during the first wave of the pandemic, by lowering both the likelihood of excess deaths and the excess mortality rate. These effects are not observed in the second wave, likely due to policy convergence and a stronger role played by national and regional institutions. Our design interprets education as a proxy for broader leadership traits, such as decision-making capacity under uncertainty. The findings underscore that political selection can have real demographic consequences, shaping population outcomes during crises.

JEL Classification: D72, J10, H75

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 (Aassve et al. 2020; Dowd 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 (Chang et al. 2022; Díaz Ramírez et al. 2022; Foster et al. 2024; Sorci et al. 2020). 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).

However, the observed correlation between politicians' attributes and COVID-related mortality is unlikely to reflect a causal relationship, as politicians are elected by voters with distinct characteristics that 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 local officials' education 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 a bundle of characteristics proxied by education has a significant demographic impact in a period of acute crisis (Marshall 2024). Indeed, more skilled politicians may perform better at managing crises—for example, by designing and implementing more effective policies under high uncertainty.

The intuition behind the close-election RDD is simple. If random factors—for example, unexpected breaking news or rainfall on election day—played even a small role in determining electoral outcomes, the narrow victory or defeat of a college-educated candidate versus a non-college-educated rival could be considered as-if randomly assigned. By focusing on close elections where the top two candidates differ in education, municipalities assigned to a college-educated mayor should be statistically similar—in both observable and unobservable characteristics (e.g., elderly population share or average income)—to municipalities assigned to a non-college-educated mayor. This identifying assumption can be partially validated, as municipal characteristics prior to the pandemic should display no discontinuity around the electoral threshold, that is, at the zero margin of victory between the top two candidates.

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 undergoing 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.

Our results show that local political selection indeed mattered. We find that more 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. No statistically significant effect of mayoral education is detected in the second wave, likely due to policy convergence and institutional learning, as well as a stronger role played by national and regional institutions. These effects are not driven by other politician characteristics that may be compounded with education, such as gender, age, political orientation, and experience. Overall, our findings provide strong evidence that political selection, as captured by mayors' education, shaped the mortality toll of the pandemic.

2 Determinants of COVID-Related Mortality

A first issue in the study of COVID-related mortality concerns how to measure it. 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).

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). To address this limitation, and in a trade-off between internal and external validity, we apply an RDD that accounts for unobservable factors at the municipality level, consistently estimating local average treatment effects in close races (Imbens & Rubin 2015).

3 Politicians' Characteristics and Health Outcomes

Empirical studies in political economy—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 favoring the election of legislators who prioritized 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.

The educational background of politicians emerges as a key element investigated by previous studies on political selection.[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).

[1] 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; Baltrunaite et al. 2014; Besley et al. 2011; Besley & Reynal-Querol 2011; Bordignon et al. 2020; Carreri 2021; Dal Bó et al. 2017; Gagliarducci & Nannicini 2013; Galasso & Nannicini 2011; Galasso & Nannicini 2024). See Dal Bó & Finan (2018) for a review of the literature.

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, their performance indicators refer to routine times, whereas our analysis centers on performance during crises, when decision-making follows different dynamics. 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 more educated local leaders may differ from others both in *preferences* and *skills*.

First, just as more educated individuals held different preferences regarding the nature and timing of pandemic policy responses, more 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 (Callaghan et al. 2021; Lazarus et al. 2021; Schwarzinger et al. 2021). 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, more educated politicians may possess a set of skills that enhance the quality of pandemic policy responses—for example, by improving the timing and implementation of public health measures such as lockdowns, mask mandates, and hospitalization protocols. They may also exhibit stronger communication and leadership abili-

[2]Outside the political realm, demographic research consistently finds that higher educational attainment is strongly associated with 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).

ties, 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).

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

4 Italian Institutional Setting

4.1 The COVID-19 Emergency

Italy's response to the pandemic unfolded rapidly starting in late January 2020, following closely the evolution of the epidemic curve. The first COVID-19 cases were confirmed in Rome on January 29, followed by the declaration of a national state of emergency. Local outbreaks in northern regions prompted the government to impose a lockdown in eleven municipalities on February 23, with containment measures such as movement restrictions, school closures, bans on public gatherings, and the suspension of non-essential services. Nationwide school closures, the suspension of sporting events, and the imposition of social distancing were announced on March 4 to slow the spread of the virus. The first phase of the COVID-19 emergency started on March 9 with the nationwide extension of the restrictions—that is, a lockdown at the national level. On March 22, nearly all retail and service activities in non-essential economic sectors were suspended. Measures were prolonged until May 3, which marked the end of the first phase. By the end of March, all-cause deaths had increased by 49.4%, and half of them were related to COVID-19 (Istat 2020), showing stark territorial differences—with a peak at +568% in the province of Bergamo—and a concentration among the elderly population, particularly those aged 70 and over.

As new COVID-19 infections slowed down significantly over the summer, the following phases of the emergency were characterized by a gradual relaxation of containment measures and the resumption of non-essential activities. Movement was allowed within regions and, from June 3, between regions. Schooling activities resumed in person in September under safety protocols, as did public transport services, albeit with capacity constraints. However, as COVID-19 cases began to rise rapidly again in early October, the government introduced back new containment measures. On November 6, a night-time national curfew was enforced, in-person high school classes were suspended, and non-essential activities were further restricted. A tiered system categorizing regions into yellow, orange, and red zones based on the severity of restrictions was introduced, with zone assignments updated periodically according to the local evolution of epidemiological parameters.

While the formal state of emergency remained in effect until March 2022, the peak of the COVID-19 crisis in Italy—in terms of mortality, health system strain, and nationwide restrictions—had largely subsided by the summer of 2021, following the acceleration of the vaccination campaign. Indeed, the vaccination campaign began in early 2021 and a vaccine certification was later introduced to regulate access to most indoor public venues. Following the summer of 2020, the successive waves of the COVID-19 pandemic in Italy exhibited a more geographically uniform spread, although northern regions continued to experience higher levels of excess mortality. During the initial phase of the vaccination campaign in 2021, initially prioritizing high-risk groups before expanding to the general population, the overall mortality impact remained substantial, particularly among older age cohorts (Istat 2022).

4.2 Municipal Governance

Italian municipalities represent the lowest administrative unit, nested in 107 provinces, and, in turn, in 20 regional governments. As of 2020, Italy counted 7,904 municipalities. Each municipal government consists of three institutions: the mayor, the executive committee, and the city council. 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 both the mayor and the members of the city council. The 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 supported by a single list of council candidates, 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 can be supported by 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

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. 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, are distributed as a Poisson with relative risk of death ρ and offset E:

$$y_{ijtk} \sim \; Poisson\left(E_{ijtk} \cdot \rho_{ijtk}\right).$$

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:

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

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). 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:

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

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:

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

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:

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

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:

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

where y_{itk} are observed death counts in municipality i and week t of COVID-19 years, and $\widehat{y}_{itk}$ 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 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 ($y_{itk} > \widehat{y}_{itk}$). 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

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

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 (2025b), 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 (2025a), 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;[5] administrative experience (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

[4] For 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.

[5] Eleven levels are available: no formal education; exemption after 3rd grade; elementary school; vocational training after elementary school; middle school; vocational training after middle school; high school diploma; vocational training after high school; bachelor's degree; master's degree; postgraduate specialization or doctoral degree.

(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.[6]

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 (2025a) or Istat (2025b) 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 cohabitation. Geographical information is retrieved from Istat (2025c) 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 (2025a), 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 municipalities 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.

[6] Municipal 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.

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 more 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 less 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 less educated if they have not attained tertiary education, and as more 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 more educated candidate and the less educated one. Thus, $MV_i \geq 0$ if the more educated candidate wins, and $MV_i < 0$ if they lose. A zero margin represents the cutoff value in our RDD 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 less 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 mayoral education corresponds to the difference in the conditional expectations of excess mortality at the cutoff value:

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

The RDD estimand τ is defined only within an infinitesimal neighborhood of the RDD threshold, trading off external validity to gain internal validity. 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:

$$Y_i = \alpha + \tau E_i + \sigma f(MV_i) + \rho\left[E_i \times f(MV_i)\right] + \varepsilon_i, \tag{1}$$

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 more 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.

[Figure 1 about here.]

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 graph) and at the intensive margin (right graph), during the first pandemic wave (panel a). Policy decisions and communication strategies made by more 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 more 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.

[Figure 2 about here.]

Table 1 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 more educated mayors—a reduction of approximately 22% relative to the mean in control municipalities—statistically significant at the 5% level (Column 3). Adding covariates increases 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 1 about here.]

Table 2 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 more educated mayors recorded 60.3 fewer excess deaths *per* 100,000 people compared to those led by less educated mayors—a reduction 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.

[Table 2 about here.]

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 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 3. Reassuringly, these characteristics vary smoothly at the cutoff—no coefficient is statistically significant at the 5% or 1% level. Municipalities led by more 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 3 about here.]

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 less 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.[7] 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.

[7] More educated candidates are overall more likely to win (53.7%) against less educated candidates (46.3%), a gap that narrows as the margin of victory becomes smaller, as predicted by the RDD setup.

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 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. 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 (Caughey & Sekhon 2017; Sekhon & Titiunik 2012), particularly those examining political selection through candidates' characteristics (Marshall 2024).

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 more educated candidates do not systematically differ from their less 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.

[Figure 3 about here.]

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 mayoral education substantially reduced excess mortality during the first wave of the pandemic in Italy—a phase marked by institutional uncertainty and the absence of clear protocols. This effect fades in later waves, suggesting that mayoral traits mattered most when uncertainty was highest and centralized guidance more limited.

Our results are robust across a wide range of empirical checks, including placebo tests, sensitivity to bandwidth and trimming, balance and density continuity, and compound treatment adjustments. 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. A simple back-of-the-envelope calculation illustrates the policy relevance of our findings: at the onset of the pandemic, about 53% of Italian municipalities were led by mayors without a college degree. Had these mayors possessed different characteristics, there would have been 20,000 fewer excess deaths—up to 32,000 in the upper-bound scenario. In other words, nearly half of the first wave's excess death toll might have been avoided.

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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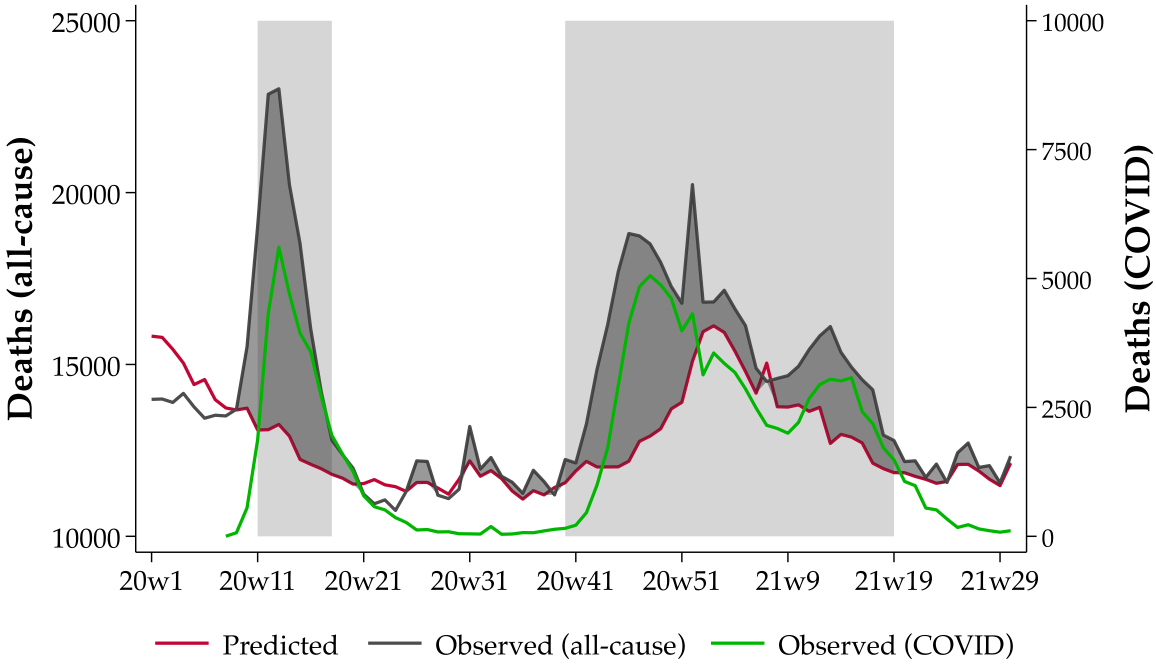

Fig. 1 Evolution of weekly deaths in Italy during the COVID emergency. The shaded area between the curves of observed and predicted all-cause deaths quantify excess mortality. Vertical grey bars identify the two pandemic periods.

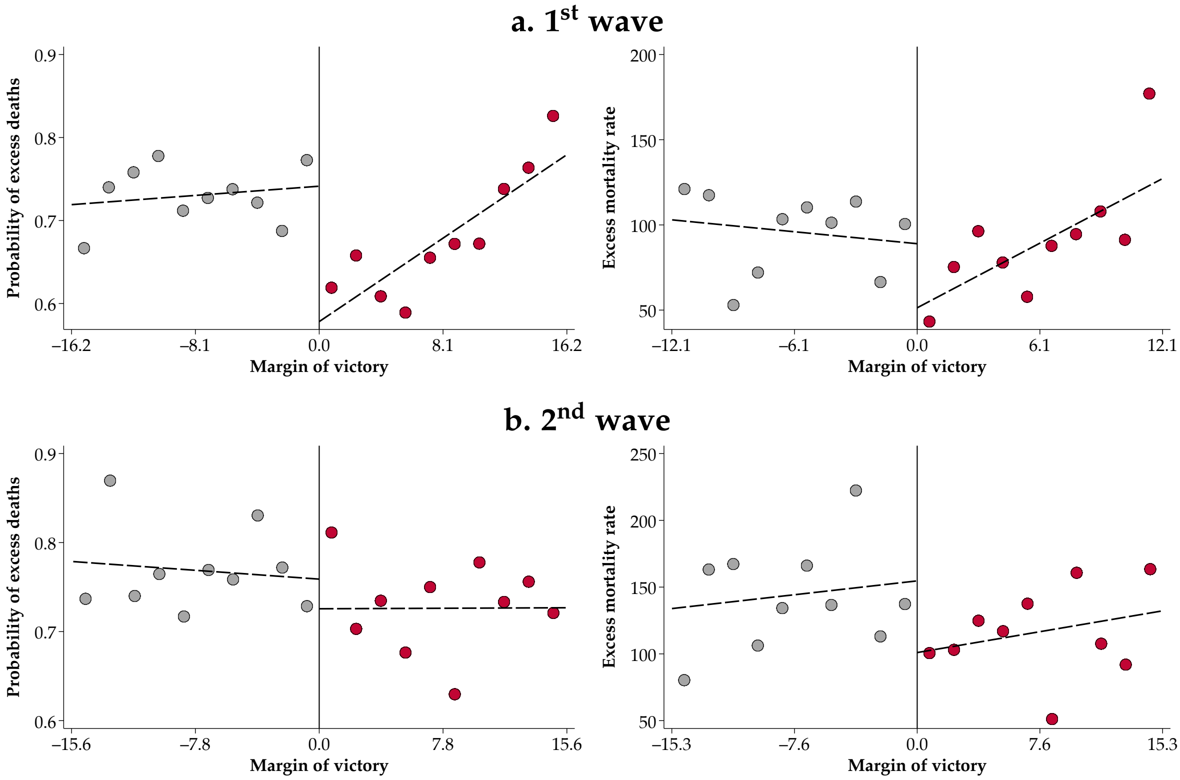

Fig. 2 Regression discontinuity plots of mayoral education and excess mortality at the extensive margin (left) and intensive margin (right) in the first (panel a.) and second (panel b.) pandemic wave. Bias-corrected RD estimates with uniform kernel from local linear regressions—represented by dashed 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 more educated candidate won (lost) against a less educated candidate, which are labeled with red (grey) binned sample means.

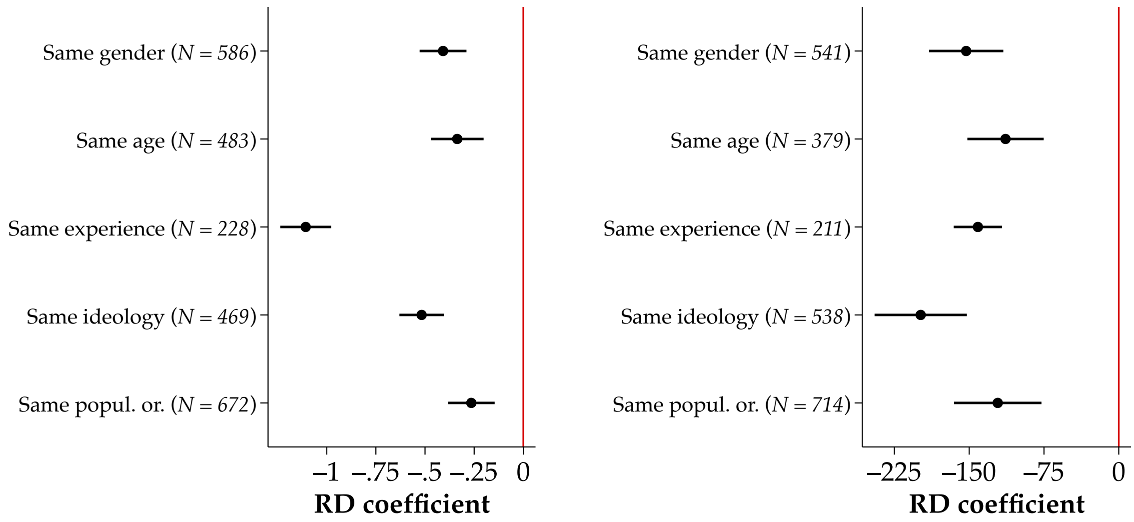

Fig. 3 Sensitivity of treatment effect estimates—i.e. more educated mayor—to the exclusion from the analytic sample of other mixed-characteristics elections in regression discontinuity designs using as outcome excess mortality at the extensive margin (left) and intensive margin (right) in the first pandemic 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.

Table 1 Regression discontinuity estimates of probability of excess deaths on mayors' education

	Pre-COVID		1st wave		2nd wave	
	(1)	(2)	(3)	(4)	(5)	(6)
More educ.	0.064	–0.089	–0.155**	–0.308***	0.058	–0.075
	(0.074)	(0.061)	(0.071)	(0.059)	(0.074)	(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). Robust standard errors, clustered by local labor systems, in parentheses. * p<.1; ** p<.05; *** p<.01

Table 2 Regression discontinuity estimates of excess mortality rate on mayors' education

	Pre-COVID		1st wave		2nd wave	
	(1)	(2)	(3)	(4)	(5)	(6)
More educ.	7.082	–15.916	–60.269**	–73.345***	–25.753	–42.315
	(17.769)	(15.538)	(28.625)	(21.441)	(56.977)	(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). Robust standard errors, clustered by local labor systems, in parentheses.
* p<.1; ** p<.05; *** p<.01

Table 3 Balance tests of covariates

	$\hat{\tau}$	Std. error
Elections		
Turnout (%)	–0.346	(1.405)
Runoff election	0.009	(0.040)
Electoral competition	0.009	(0.013)
Demography		
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 cohabitation (%)	–0.064	(0.127)
Household size	0.033	(0.038)
Geography		
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)
Education		
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)
Employment		
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)
Accomodation 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.
* $p<.1$; ** $p<.05$; *** $p<.01$

A Appendix

Table A1 Descriptive statistics of mayors

	Obs.	Mean	Median	SD	Min.	Max.
Education						
≤ 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
Ideology						
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.
Mortality						
Excess deaths prob. (pre-COVID)	2,545	.38	.00	.48	0	1
Excess deaths prob. (1st wave)	2,545	.69	1	.46	0	1
Excess deaths prob. (2nd 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 (1st wave)	2,545	95.2	50.2	227.6	–583.7	2557.7
Excess mortality rate (2nd wave)	2,545	147.2	117.1	361.3	–2477.5	5021.0
Elections						
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
Demography						
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
Geography						
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
Education						
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
Employment						
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
Accomodation 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
Education				
≤ 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
Ideology				
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 (N = 2,545)		Full (N = 6,906)	
	Mean	SD	Mean	SD
Mortality				
Excess deaths prob. (pre-COVID)	.38	.48	.37	.48
Excess deaths prob. (1st wave)	.69	.46	.68	.47
Excess deaths prob. (2nd wave)	.73	.44	.72	.45
Excess mortality rate (pre-COVID)	–15.56	129.40	–17.97	134.56
Excess mortality rate (1st wave)	95.21	227.64	109.27	265.61
Excess mortality rate (2nd wave)	147.20	361.27	153.16	395.74
Elections				
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
Demography				
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
Geography				
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
Education				
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
Employment				
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
Accomodation 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

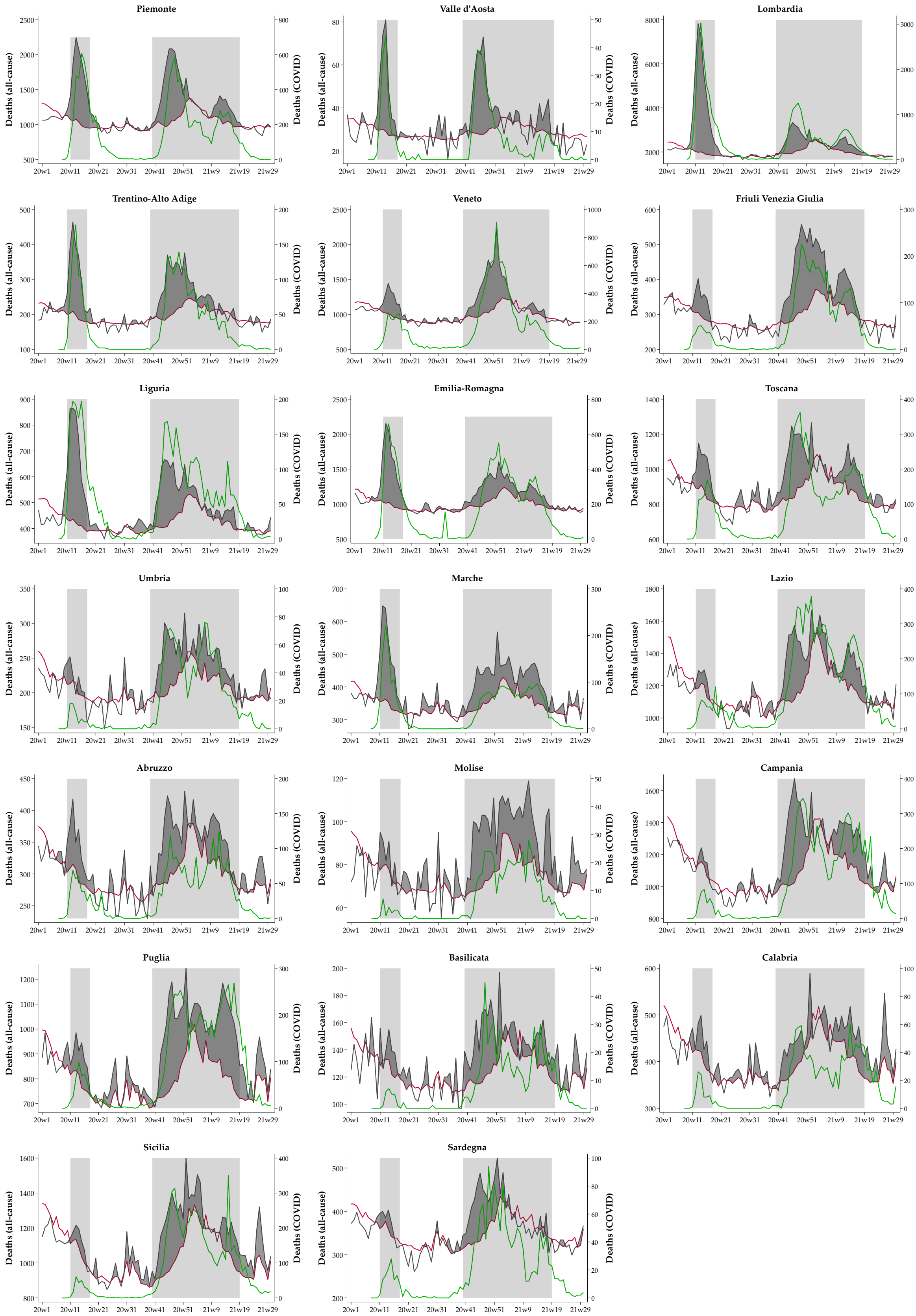

Fig. A1 Evolution of weekly deaths during the COVID emergency by region.

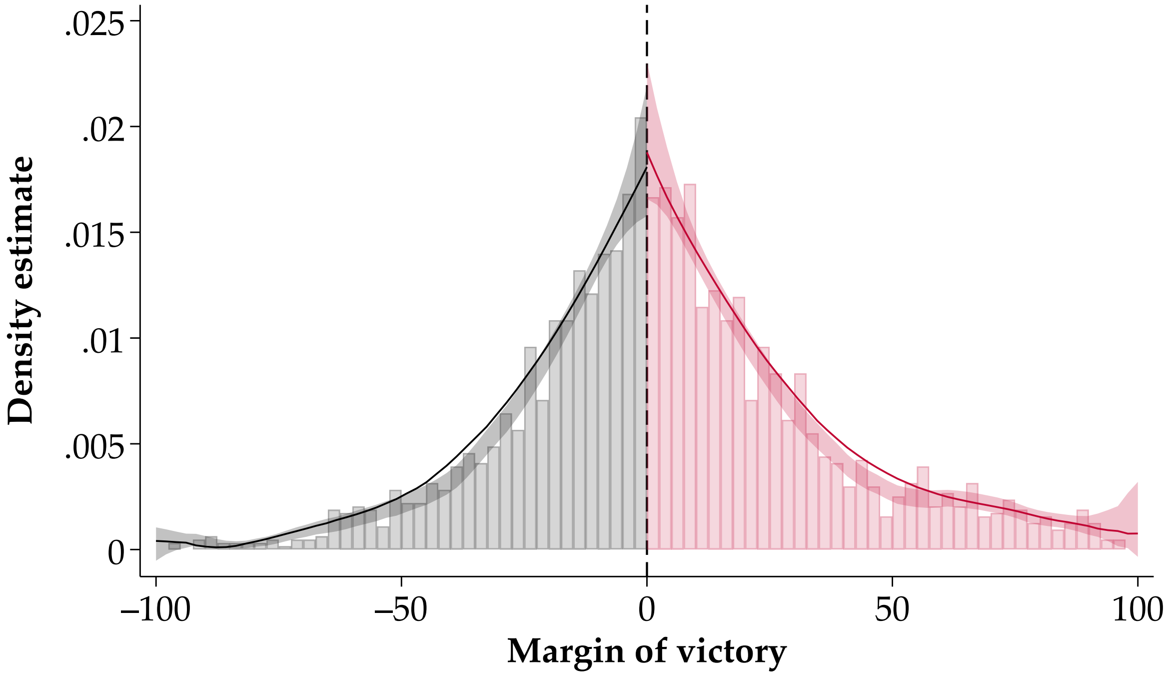

Fig. A2 Manipulation test of the running variable. The robust bias-corrected t-statistic at the cutoff is 0.399 (p = 0.690).

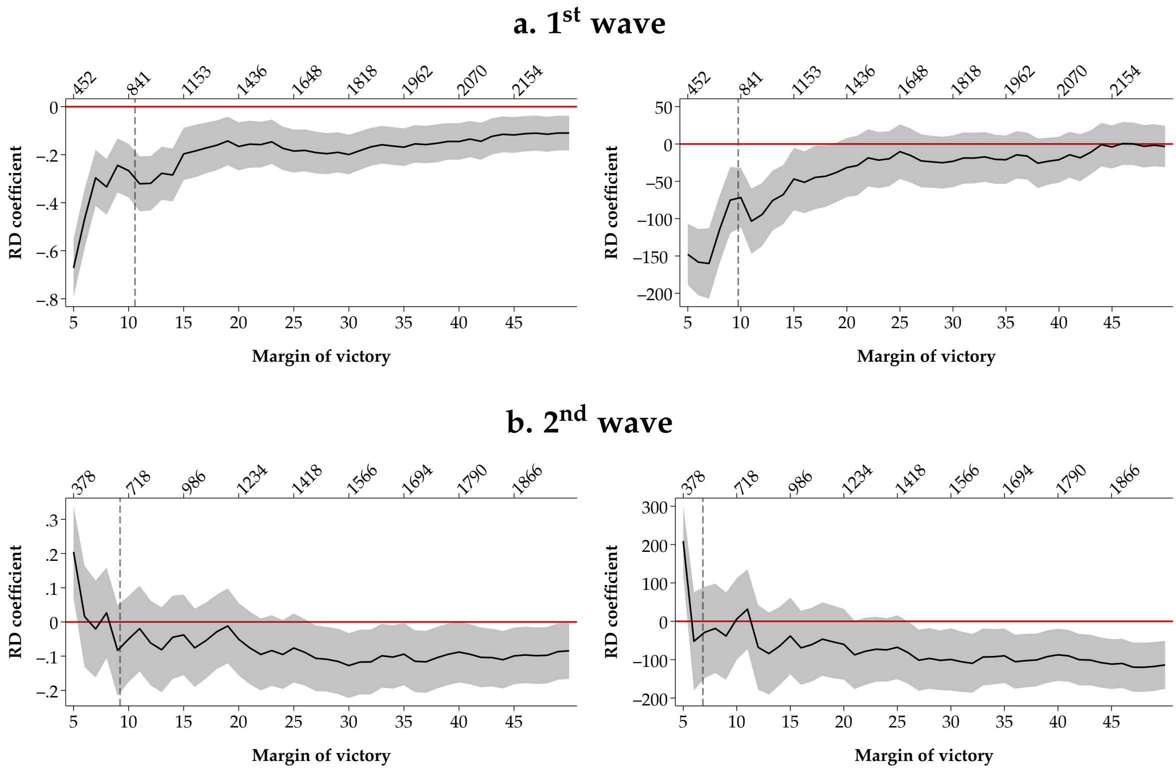

Fig. A3 Sensitivity of treatment effect estimates—i.e. more educated mayor—to bandwidth selection in regression discontinuity designs using as outcome excess mortality at the extensive margin (left) and intensive margin (right) in the first (panel a.) and second (panel b.) pandemic wave. Bias-corrected 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 1 and 2, Columns 4 and 6. Shaded areas are 95% confidence intervals.

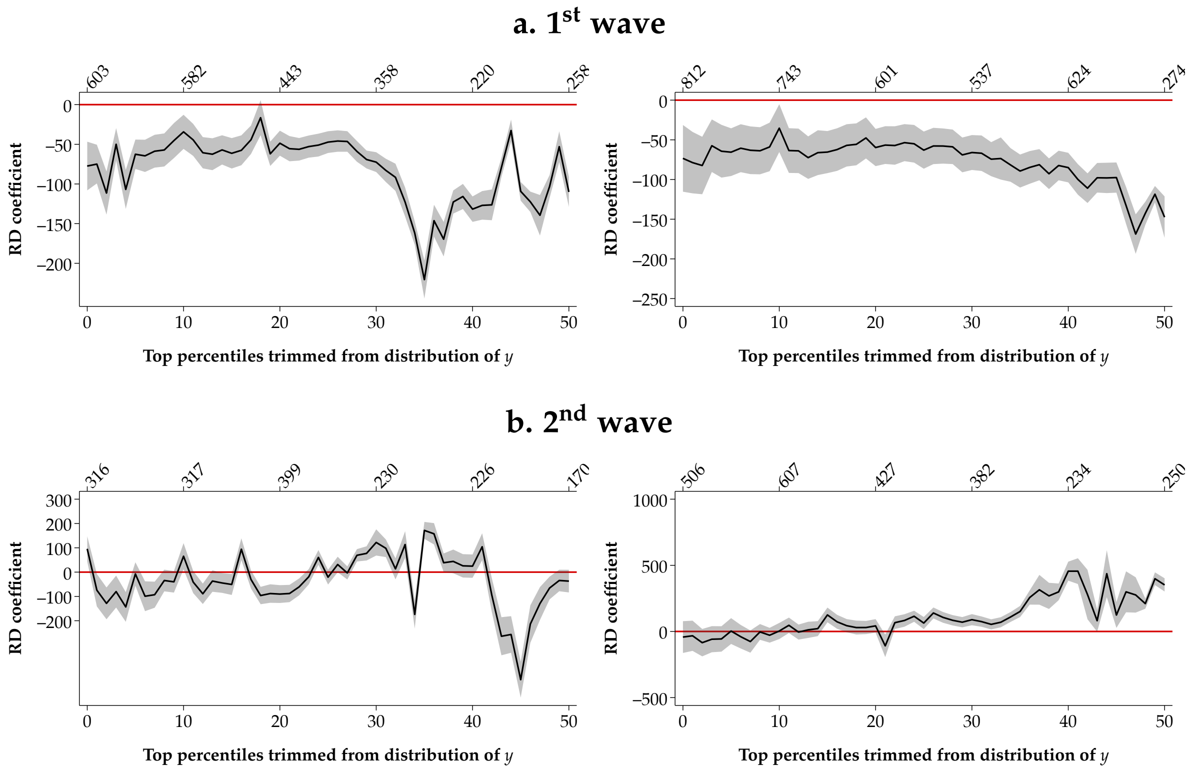

Fig. A4 Sensitivity of treatment effect estimates—i.e. more educated mayor—to extreme values in the distribution of excess mortality rates used as outcomes in regression discontinuity designs in the first (panel a.) and second (panel b.) pandemic wave. Bias-corrected RD estimates with uniform kernel 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.