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Voting, contagion and the trade-off between public health and political rights: quasi-experimental evidence from the Italian 2020 polls

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Abstract

In September 2020, a national-level constitutional referendum held alongside local administrative elections took place in Italy, resulting in a 22% average increase in the referendum turnout rate where more than one poll occurred. We exploit this quasi-experimental setting to estimate the effect of voters' turnout on the spread of COVID-19, by employing an event-study design with a two-stage Control Function strategy. The estimated elasticities show that post-poll new COVID infections increased by an average of 1.1% for each additional percentage point of turnout. The findings suggest that national-level polls have the possibility to amplify nation-wide waves of contagion if held during peak periods of an epidemic. A cost-benefit simulation based on our estimates and real political events shows that averting an early general election in Spring 2021 has spared Italy up to about 362 million euros in additional hospital care costs and 22,900 deaths from COVID.

Keywords: COVID-19, voting, civic capital, Control Function.
JEL Codes: C23, D72, H51, I18.

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1 Introduction

Politicians and healthcare policy-makers are faced with hard times to communicate and impose restrictions to civil rights and freedoms in order to minimize the spread of COVID-19, but they may be faced with an even tougher policy dilemma in the case of official voting polls. Elections are gatherings of vital importance for the functioning of democratic countries, and their postponement or cancellation can undermine the citizens' trust in the political institutions of a country. Despite this, public health concerns related to COVID-19 have made at least 78 countries to postpone national or regional elections between February 2020 and July 2021, while more than 128 countries still decided to hold polls as previously scheduled (Institute for Democracy and Electoral Assistance (IDEA), 2021). In fact, holding polls during an epidemic requires politicians to face an important trade-off: preserving the spirit of democratic institutions in the long run, but exposing the lives of citizens to the likely contagion, and their political careers to a premature oblivion, should the voting gathering sensibly amplify the spread of the virus; or acting conservatively in the short run, but at the cost of risking future political instability and a fade in the values of democracy. Moreover, historical evidence has shown that there are few alternatives to physical voting for a general election. [1]

For these reasons, collecting quantitative evidence on the likely short-term contagion risk borne by holding in-person elections is paramount for politicians and healthcare policy-makers in order to evaluate the best course of action to adopt when official polls are scheduled. Before the COVID-19 outbreak, the risks of holding elections during a pandemic were still unclear and not quantitatively measured. The lack of empirical evidence is likely due to the fact that country-level epidemics, or pandemics like the COVID-19 one, are usually rare and unpredictable events, but also that evaluating the impact of going to the polls on the spread of a virus through observational data is prone to bias in the effect of interest: the choice of voters whether to go or not to the polls is most likely endogenous to the local stage of the epidemic. Such issues put a serious threat to make any causal claim about the effect of interest, but they are overcome by the framework provided by our institutional setting.

During Fall 2020, an election day with multiple polls took place in Italy: in all Italian

[1] Electronic voting has been trialled in several developed and developing countries, but such attempts have often had scarce success and lead to its abandonment as a voting option, apart from the US and few other countries, for the most disparate reasons such as unconstitutionality concerns and cybersecurity risks. In some countries, like Italy, the introduction of electronic and postal voting might be problematic due to past histories of authoritarian regimes and the presence of criminal organizations like "Mafia" that could interfere with the polls, raising concerns about the secrecy and independence of voters' choices. Electronic or postal voting are safer during epidemics as they prevent the occurrence of gatherings among voters (although this is not even true in the case of local pre-electoral rallies), but it is unlikely that they could replace completely physical voting without a solution to the aforementioned concerns.

regions, citizens casted ballots for a constitutional referendum aimed at reducing the number of Parliament members; in 7 out of the 20 Italian administrative regions, citizens also casted ballots for electing the new regional governments and the regional assembly representatives; finally, in 955 of the 7,903 Italian municipalities, citizens voted even for appointing the new municipality mayor. Such institutional setting resulted in a 22% average increase in the turnout rate for the constitutional referendum in the municipalities where an administrative poll (i.e. either regional, mayoral elections, or both) occurred on top of the referendum.

We build a unique dataset of weekly new COVID-19 infections and voters' turnout at Italian municipality level, including also municipality, province and region characteristics. We then employ an original event-study Control Function design, i.e. an event study where the continuous treatment variable (i.e. the referendum turnout) is instrumented through a Control Function strategy, to examine the weekly evolution of coronavirus infections before and after the September 2020 polls as a function of the referendum turnout rate. This quasi-experimental design has the obvious advantage to greatly reduce the extent of the aforementioned endogeneity bias, as the variation to identify the effect of interest is due to administrative reasons, and so it is independent of the local epidemic status.

The event-study regressions include municipality and week fixed effects and municipality-clustered standard errors, and we also estimate event-study models after pre-processing our sample through different types of matching based on municipality pre-COVID and pre-poll characteristics (e.g. population density, number of schools per capita, residents' average age), in order to reduce the bias from observables. Moreover, we tease out the contribution of civic capital to the spread of COVID-19 infections at the municipality level, because this unobservable is cross-sectionally correlated with turnout, as shown by our analysis, as well as social distancing rules preventing the proliferation of the virus (Barrios et al., 2021; Durante et al., 2021) before any vaccine was available. Last but not least, based on our model's estimates, we also perform a cost-benefit simulation for the potential healthcare costs and lives saved in Italy by averting an early general election at the start of 2021, when the more transmissible COVID-19 "English" or Alpha variant became prevalent.

Our analysis shows that post-poll new COVID-19 cases increased by 1.1% for each additional percentage point of turnout rate for the constitutional referendum. The magnitude and significance levels of our estimates are largely confirmed even when using matching as a pre-processing technique and when accounting for the bias due to civic capital. These findings suggest that in-person polls have indeed the possibility to increase the spread of airborne diseases like COVID-19, thus potentially triggering or amplifying national-level waves of contagion when they are held during peak periods of an epidemic. These results are informative for politicians and healthcare policy-makers regarding the public health threats posed by voting during a pandemic, and other gathering events that are similar in nature. To further illustrate the relevance of our results, our cost-benefit calculations show that avoiding an early election at the beginning of 2021, following the collapse of the Government in charge till January 2021, has spared Italy up to about €361.751 million on hospital care costs and almost 23 thousand more deaths, which are worth about €7.538 billion.

With this work, we aim to shed light on this important public health issue linking voting and the spread of infections diseases, and to quantify the value of the trade-off between public health and political rights. Up to the best of our knowledge, we are among the first to provide an empirical evaluation of whether and by how much voting can increase contagion, using a quasi-experimental framework. Previous works on this topic document only associations, with the exception of a recent study by Palguta et al. (2021), showing that elections for one-third of Senate members in Czech Republic led to an increase in the number of COVID-19 infections and hospitalizations where the ballots took place. Our study differs from Palguta et al. (2021) as we analyze the effect of voters' turnout as a measure of treatment intensity rather than just treatment assignment (i.e. holding versus not holding polls). Examining the impact of turnout is likely more relevant for policy-makers, since the spread of new infections is a function of the "gathering intensity" provided by voters' turnout, not just by whether in-person elections are held or not. Furthermore, the focus on turnout allows policy-makers to elaborate cost-benefit simulations based on realistic scenarios of the expected voters' participation at the polls, which may guide them in the decision whether to keep or postpone elections during an epidemic. [2]

The remainder of the paper is organized as follows. The next section provides links to the related literature on COVID-19 and voting. It also describes the institutional framework and the data used for this study. Section 3 illustrates the empirical strategy. Section 4 and Section 5 report respectively the main results and the robustness checks, while Section 6 describes the assumptions and the findings of the cost-benefit simulation. Section 7 concludes.

2 Background and Data

[2]Focusing on the impact of turnout also distinguishes our work from the analysis by Cipullo and Moglie (2021), which instead estimates the impact of the pre-electoral rallies preceding the September 2020 Italian regional elections on COVID-related outcomes.

2.1 Related literature

This study is related to a range of contributions in the fields of economics, public health medicine, politics, and interdisciplinary COVID-related research in general.

The closest study to ours is a recent working paper by Palguta et al. (2021), which examines the impact of the second round of the 2020 Senate elections held only in one third of the constituencies in Czech Republic on the spread of COVID-19. The authors document a more pronounced increase in the growth rates of COVID-19 infections and hospitalizations where this additional electoral round took place. These effects peaked around the third week following the election date (October 9-10), when for instance the 14-day growth rate of COVID-19 cases was 24.6% higher in voting municipalities, despite the average turnout for the second round of the Senate elections was only 16,7%. Our findings are complementary to those of Palguta et al. (2021): they compare COVID-19 growth rates between voting and non-voting geographical authorities, while we provide a measure of the effect of turnout on new COVID-19 infections. The findings from both works are qualitatively and quantitatively similar, since we find that post-poll new COVID-19 infections were about 1.1% higher for each additional percentage point of turnout. However, our work presents a distinctive contribution with respect to Palguta et al. (2021) in a number of additional ways: we account for the possible spillover effects of new COVID-19 infections by means of a spatial model including weighted averages of new weekly COVID-19 cases in neighboring municipalities as additional controls; we estimate the effect of turnout on mortality; and we employ an event-study design with a Control Function strategy, as we are concerned with the endogeneity of turnout due to self-selectivity of voters stemming from the unobservable trade-off between the individual expected utility from casting a ballot and the health risk of contracting the virus.

In another recent working paper, Cipullo and Moglie (2021) exploit the exogenous schedule of the September 2020 regional elections in Italy to investigate the effect of pre-electoral rallies on COVID-19 spread, whereas we focus on the effect of voter's turnout at the polls. They implement a DiD and an event-study design at the regional level, which shows how the number of infections, hospitalizations and deaths due to COVID-19 increased faster since the start of the electoral campaign in Italian regions where both the constitutional referendum and the regional elections took place. Differently from our municipality-level setting, the regional-level framework used by Cipullo and Moglie (2021) does not allow the authors to explicitly account for the fact that 12% of Italian municipalities held also mayoral elections. However, this is another contingent election implying campaign events that are specific to the municipality and that may contribute to the spread of the contagion.[3]

[3] This issue is particularly serious for the Trentino-Alto Adige region, where no regional elections took place and almost all municipalities were called to vote for the new mayor (see Section 2.2).

Picchio and Santolini (2021) investigate how mortality during the first COVID-19 wave affected turnout for the Italian mayoral elections that were held alongside the national referendum and the regional government elections in Fall 2020. The authors find that a 1 percentage point increase in elderly mortality rate decreased voter turnout by 0.5 percentage points, with a stronger effect in more densely populated municipalities. Their results reinforce our concerns of endogeneity due to reverse causality and self-selection into voting linked to the local stage of the COVID-19 epidemic. The other existing studies mostly report associations, as they lack a source of exogenous variation to identify the causal effect of holdings elections on COVID-19 spread (see for instance Feltham et al. 2020; Leung et al. 2020; Berry et al. 2020; Cotti et al. 2021). Bertoli et al. (2020) attempt to overcome this issue by instrumenting turnout with the amount of local electoral competition in the context of the March 2020 French municipal elections, finding a significant and positive association between turnout rate and elderly mortality in the five weeks following the elections.

Other studies that are relevant for our work are those examining the contribution of population density (e.g. Gerritse 2020; Bhadra et al. 2021; Sy et al. 2021) and school openings (e.g. Auger et al. 2020; Amodio et al. 2021; Isphording et al. 2021) to the spread of COVID-19. Since Italian municipalities exhibit large variations in population density, we control for this factor in our analysis. Similarly, to account for the possible impact of school openings on the COVID-19 contagion spread in Italy, our analysis presents robustness checks controlling for school density at the municipality level.

Last but not least, James and Alihodzic (2020) investigate the legal foundation of what can be considered the companion research question of our work, i.e. "*when is it democratic to postpone an election*" due to natural disasters like COVID-19. They postulate five main criteria upon which the popular vote must be cast: *full opportunities of deliberation* for the voters; *equality of voters' participation* across social and economic groups; *equality of contestation* giving a level playing field to all candidates; *robust electoral management quality*; and, finally, *institutional certainty*, i.e. clarity about the rules of the game. These criteria have relevant implications that we discuss in Section 7.

2.2 Institutional framework

Italy is organized in 20 regions (NUTS-2 level), whose Presidents are elected every 5 years. Regional governments legislate on all matters related to the provision of health, education and transports, as well as on other fundamental services that are not expressively under the competence of the central Government. At the time of the election events used in this study, Italy comprised 7,903 municipalities, which are the smallest administrative local authorities and are headed by a mayor whose term also lasts 5 years.[4]

On 20th and 21st September 2020 a multiple electoral appointment took place in Italy. The citizens with the right to vote were called to the polls to appoint their new regional President and governments in 7 Italian regions (Campania, Liguria, Marche, Toscana, Puglia, Valle d'Aosta and Veneto). Moreover, citizens with the right to vote were also called to cast a ballot to appoint new mayors and municipality councils in 955 Italian municipalities (across all regions except for Sicily and Sardinia).[5] Finally, on the same dates, all Italian adults with the right to vote and from any region were called to vote for a constitutional referendum to approve the reduction of the size of the Italian Parliament.[6] Specifically, the referendum question asked whether voters approved to reduce the members of the Chamber of Deputies from 630 to 400, and the Senate members from 315 to 200. All these polls were initially scheduled for the first half of the year, but they were postponed following the beginning of the COVID-19 outbreak. In general, Italian voters must cast their vote in the municipality where they legally reside. Importantly, all the above polls had the same pool of voters, i.e. the citizens over 18 years of age.

Figure 1 displays in the left map the regions (in darker blue) and the municipalities (red crosses) undergoing respectively a regional or a mayoral election, and in the right map the different turnout rates for the constitutional referendum across regions. The turnout was always higher where voters were asked both to approve the referendum question and to appoint either the new regional President and/or the new municipality mayor. The political nature of administrative elections certainly led to additional ballots for the referendum that would not be cast otherwise, also because the referendum object enjoyed a wide consensus among most political parties and the general public.[7] The referendum average turnout rate was 69% in municipalities where at least one between the regional elections and the mayoral elections took place (hereinafter referred as "treated municipalities"), while it was just 47% in municipalities where only the constitutional referendum was held (hereinafter referred as "control municipalities"). The highest participation of voters was recorded in Valle d'Aosta (73%), the lowest in Sicilia (35%). A high turnout rate (71%) was also recorded

[4] Around 70% of Italian municipalities have less than 5,000 residents.

[5] A few other municipality elections occurred during October 2020: the mayoral elections for 60 Sicilian municipalities took place on 4th and 5th October 2020, alongside the second ballot for the mayoral elections of 67 of the aforementioned 955 municipalities; and the mayoral councils of 156 Sardinian municipalities were renewed with an electoral round taking place on 25th and 26st October 2020.

[6] This was the fourth constitutional referendum in the Italian history. The other three were held in 2001, 2006 and 2016.

[7] Indeed, the referendum question to reduce the number of Parliament members was approved with around 70% of voters in favour.

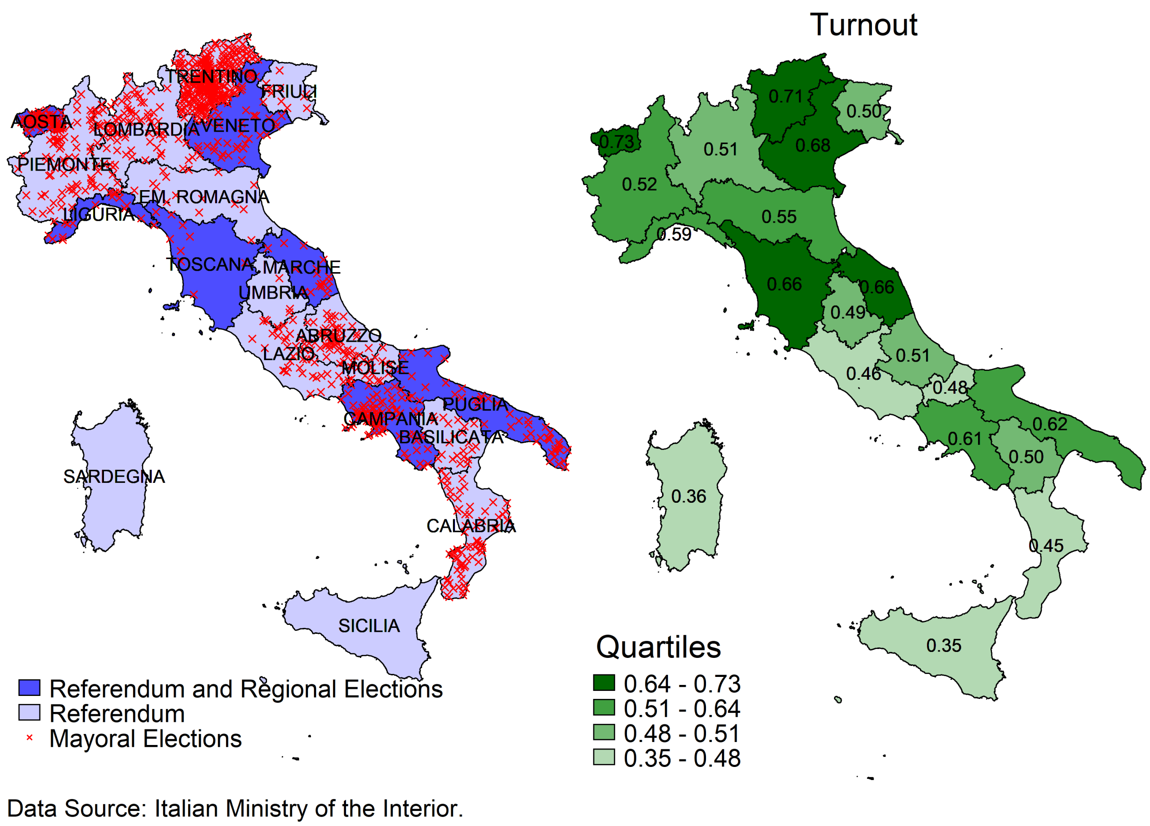


Figure 1: Regional turnout rates for the constitutional referendum

in the Trentino-Alto Adige region, where 269 out of the 282 municipalities had to renew the municipal government. We exploit this exogenously-driven heterogeneity in the referendum turnout rate to evaluate the impact of voting turnout on COVID-19 infections.

2.3 Data sources

We rely on a unique dataset that is made by combining several data sources. The data on weekly coronavirus infections for each of the 7,903 Italian municipalities have been provided by the Italian National Institute of Health (ISS), which is the Italian public body that has been tasked with the surveillance of the COVID-19 pandemic. The timeframe covers the two months around the election date, namely from the week starting from August 24th to that of October 12th. This period corresponds to four weeks before and four weeks after the date of the September 2020 constitutional referendum. For privacy reasons, records have been censored by ISS officials whenever the number of new weekly coronavirus cases is in the range $[1, 4]$.[8]

We then merge the above ISS data on health outcomes with data at municipality-level on the turnout rate for the September 2020 constitutional referendum, which is publicly available from the Ministry of the Interior's website[9]. From the same source, we also collected the municipality-level turnout rates for the previous four elections held nationally[10], which we use in Section 4.3 to construct a proxy for civic capital. Data on mayoral elections were collected from the 'Archivio Storico delle Elezioni' of the Italian Ministry of the Interior and from the official websites of the five Italian special administrative status regions (i.e. Friuli-Venezia Giulia, Sicily, Valle d'Aosta, the autonomous Provinces of Bolzano and Trento in the Trentino-Alto Adige region).[11] To control for the number of schools that are present in every Italian municipality, we instead rely on data collected by the Ministry of Education.[12]

Furthermore, we gathered information on the following municipality characteristics (as of 1st January 2020) from the Italian National Institute of Statistics (ISTAT): number of residents (in total, by gender and by age), orography, altitude from sea level, urbanicity and proximity to the coast.[13] Using ISTAT data, we also construct a measure of excess mortality at the municipality level during the first COVID-19 wave (from March to June 2020) and during our period of study, which we use as a matching covariate in Section 3.2 and as an outcome variable of interest in Section 4.5, respectively. Finally, we gathered data on the weekly number of PCR tests performed by Italian regions during our period of interest. These data are accessible from the official repository of the Italian Department for Civil Protection[14].

[8] Throughout the paper, most of the results provided are obtained by replacing such censored values with 2, but we also run extensive robustness checks to test the sensitivity of our findings to different values imputed to the censored observations. See Section 5.

[9] https://dati.interno.gov.it/elezioni/open-data

[10] These are: the 2019 European elections, the 2018 Political elections, the December 2016 constitutional referendum and the April 2016 abrogating referendum.

[11] See: `https://elezioni.regione.fvg.it`; `http://www.elezioni.regione.sicilia.it`; `https://www.regione.vda.it/amministrazione/Elezioni`; `http://www.2020.elezionicomunali.tn.it`; `https://www.elezionicomunali.bz.it`; mayoral election data for municipalities in Sardinia (the last remaining Italian region with a special administrative status) were not collected, since mayoral elections took place on 25 and 26 October 2020, which is after the termination of our period of study. Finally, we have also added the municipalities of Filetto (CH) and Follonica (GR) to the set of municipalities where also mayoral elections took place on September 2020, because these were not originally included in the 'Archivio Storico delle Elezioni'

[12] `https://dati.istruzione.it/opendata/opendata/catalogo/elements1/?area=Scuole`.

[13] The altitude classification is made by ISTAT itself based on the municipality height from sea level, while the urbanicity and proximity to the coast categories follow the Eurostat definition.

[14] `https://github.com/pcm-dpc/COVID-19`.

3 Methods

3.1 Baseline model: fixed-effects Poisson event study

Our baseline specification models the weekly cases of new COVID-19 infections around the election date as a function of the municipality turnout rate for the September 2020 constitutional referendum, $TURN_i$:

$$\begin{aligned}\mathbb{E}(NC_{irt}|\mathbf{X_{irt}}) = exp\Bigg\{ & \alpha_0 + \alpha_1 PCR_{rt} + \mu_i + \sum_{t' \neq t_0} \beta_t \mathbb{1}(t = t') + \sum_{t' \neq t_0} \gamma_t TURN_i \mathbb{1}(t = t') \\ & + \sum_{t' \neq t_0} \omega_t APT_i \mathbb{1}(t = t') + \sum_{t' \neq t_0} \delta_t PD_i \mathbb{1}(t = t') \\ & + \sum_{t' > t_2} \zeta_t OCT_i \mathbb{1}(t = t') \Bigg\},\end{aligned} \tag{1}$$

where i denotes the municipality and r the region it belongs to. t denotes the week, going from 3 weeks before to 4 weeks after the week of the polls[15], this latter denoted by t_0 and used as reference category.

NC_{irt} is the number of new COVID-19 infections in municipality i and week t. APT_i is instead the average municipality-level turnout at the previous four elections held nationally (two referenda, one general election for the Italian Parliament and one general election for the members of the European Parliament). Its inclusion allows us to control for the habitual participation of voters and to identify our effect of interest by exploiting the exogenous variation in the referendum turnout outlined in Section 2.2. Moreover, it controls for the compliance of voters to social distancing rules and NPIs (Durante et al. 2021; Barrios et al. 2021) in the ballot box, if we assume it as a proxy for the municipality-level civic capital as in Putnam et al. (1994).[16]

The vector $\mathbf{X_{irt}}$ also includes the event-study variables of interest, i.e. the interaction of the referendum turnout in municipality i, $TURN_i$, with weekly pre and post poll indicators, alongside other observable confounders that we describe below. The main object of interest is the event-study vector of coefficients γ_t. For $t > t_0$, the coefficients quantify the effect of one point of referendum turnout in excess of APT_i on new coronavirus infections, for each of the

[15] I.e. $t \in \{t_{-3}, t_{-2}, t_{-1}, t_0, t_1, t_2, t_3, t_4\}$.

[16] Given its unobservable nature, civic capital is often proxied through indirect outcome measures like blood donations (Guiso et al., 2004, 2009) or voters' turnout (Putnam et al., 1994). We follow Putnam et al. (1994) and proxy civic capital using voters' participation at the previous four national-level polls (i.e. APT_i), as these turnouts are publicly available at the municipality level, i.e. our level of analysis, differently from data on blood donations that are collected only at Italian provincial level.

four post-poll weeks in our sample.[17] In our setting, most of the within-variation in the excess turnout at the referendum comes from the number of polls held in September 2020, which was scheduled months ahead of the election date(s) and therefore unrelated to the municipality-level epidemic stage. This fact is confirmed by Figure 2, which compares the growth rate of new weekly COVID-19 infections between treated and control municipalities. Both groups display parallel trends (Card and Krueger, 1993; Dimick and Ryan, 2014; Wing et al., 2018) only until the election week. After then, new COVID-19 infections have accelerated faster in treated municipalities, which on average were characterized by higher turnout rates as a result of the additional voting incentive induced by local administrative elections.

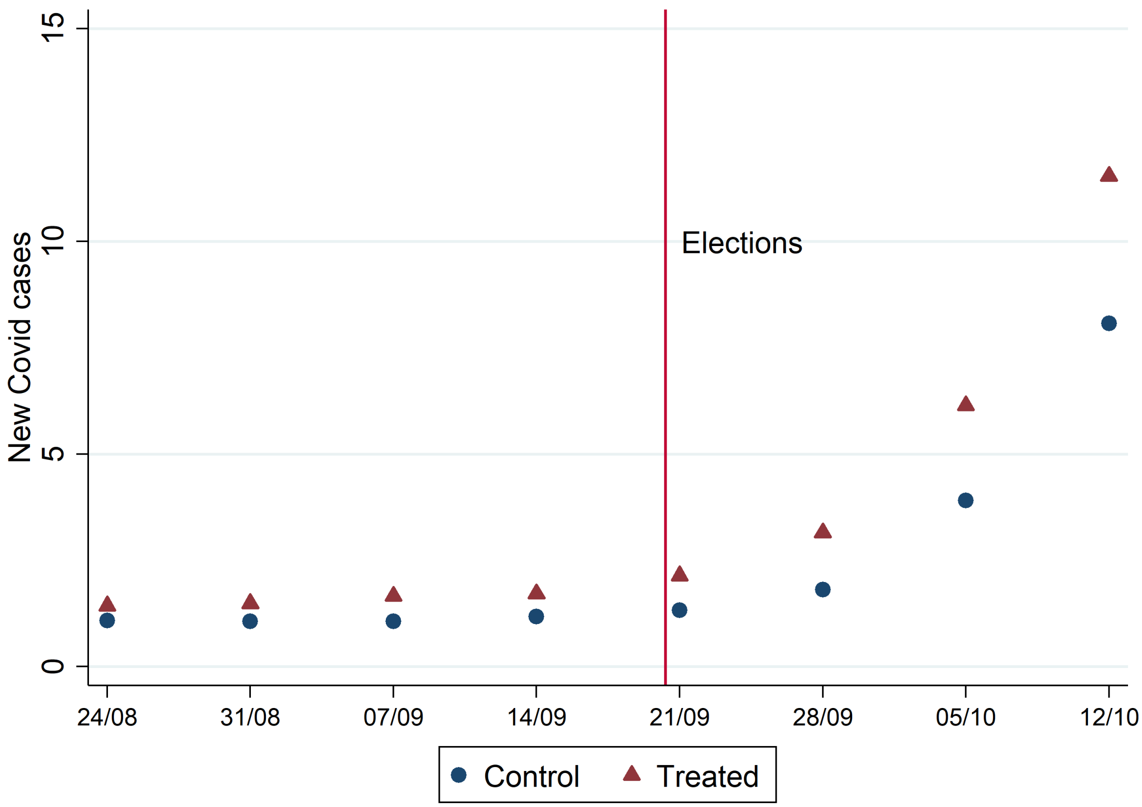


Figure 2: Trends in new COVID-19 cases

The variable PCR_{rt} corresponds to the total number of PCR tests performed per 10,000 inhabitants in region r and week t. μ_i and $\sum_{t' \neq t_0} \mathbb{1}(t = t')$ are municipality and week fixed effects, respectively. They control for characteristics that are invariant within municipality (e.g. population) and time (e.g. seasonality) in our sample period. PD_i is instead population

[17] Equivalently, the vector of coefficients γ_t can be identified from a model in which $TURN_i$ gets replaced with the change in turnout between the September 2020 referendum and the previous four national-level elections, i.e. $\Delta TURN_i = TURN_i - APT_i$.

density in municipality i, which is interacted with the week indicators to capture its (possibly) time-varying link with COVID-19 spread (see also Carozzi 2020). OCT_{ir} is instead an indicator variable for those few municipalities that had either the first or the second ballot for the mayoral elections on 4$^{\text{th}}$ and 5$^{\text{th}}$ October 2020. By interacting it with indicators for the last two week in our sample, we control for the effects that this additional electoral round might have had on the spread of COVID-19.

We model our relation of interest through a Poisson Fixed Effects regression (Hausman et al., 1984; Gourieroux et al., 1984; Cameron and Trivedi, 1986; Winkelmann, 2008) mainly for three reasons: (i) the spread of viruses like COVID-19 is characterized by an exponential growth; (ii) the count nature of the dependent variable, with the presence of many zero-valued observations; (iii) and the fact that the Poisson QMLE is a consistent estimator for our parameters of interest (Gourieroux et al., 1984). As such, all the fixed-effects Poisson models provided in this study are estimated by pseudo-maximum likelihood (Gourieroux et al., 1984) and with standard errors that are clustered at the municipality level (Wooldridge, 1999, 2015b).[18]

3.2 Matching and bias from observables

The previous model assumes that, by controlling for municipality and week fixed effects, the evolution of the COVID-19 outbreak as a function of the referendum turnout rate can be comparable over time across municipalities. However, Table 1 shows that the groups of treated and control municipalities differ substantially not only in the turnout rate for the constitutional referendum, but also in some predetermined characteristics. A legitimate concern is whether these features may contribute to explain the post-polls heterogeneous increase in coronavirus infections displayed in Figure 2.[19] Although this potential issue should be alleviated by the inclusion of municipality fixed effects, we also estimate Equation 1 but after pre-processing the data with a nearest neighbor propensity score matching approach without replacement (Rosenbaum and Rubin, 1983; Dehejia and Wahba, 2002; Abadie and Imbens, 2006).

We match municipalities with respect to demographic characteristics, which are known to play an important role in explaining both the turnout rate (Blais, 2006; Geys, 2006; Gallego, 2009; Bhatti et al., 2012) and the severity of COVID-19 symptoms (Bhopal and Bhopal, 2020; Jin et al., 2020), as well as to geographical and urban characteristics, which

[18]Silva and Tenreyro (2010, 2011) show how Poisson pseudo-maximum likelihood estimators perform well even in the presence of an outcome variable with frequent zeros like NC_{it}.

[19]For instance, the lower excess mortality experienced during the first COVID-19 wave might have induced voters from treated municipalities to take less precautions in going to the ballots than voters from high excess mortality municipalities in the control group.

are factors that can significantly affect COVID-19 transmission (see for instance Gupta et al. 2020; Ahmadi et al. 2020). This approach allows us to construct a more balanced sample of units in terms of pre-poll characteristics, and to estimate an effect of turnout on COVID-19 spread which is less likely to be confounded by other differences between municipalities.

We obtain estimates of the propensity score for each municipality from a logit regression with an indicator for treated municipalities as dependent variable and the share of female residents, average population age, average municipality income, population density, number of schools per capita of 1,000 inhabitants, excess mortality during the first COVID-19 wave, indicators for coastal towns, municipality altitude (i.e. Flat Land, Inner Mountain, Coastal Mountain, Inner Hill, Coastal Hill) and degree of urbanization (i.e. Rural, Small Town, City) as independent variables. Then, we match each treated municipality with a single control unit (where only the constitutional referendum occurred) having the closest propensity score (i.e. nearest neighbor).[20] The nearest neighbor matching is achieved by imposing a caliper of 0.01 in the propensity score, so that only very good matches are retained.

This pre-processing approach implies a considerable reduction in the units of our sample, with 2,195 treated municipalities and as many controls. Its summary statistics are reported in Table A1. The matching approach is successful in making the set of municipalities much more similar between treatment arms, and without any significant difference in the predetermined demographic or geographical characteristics.[21] A similar conclusion can be drawn from Figure 3, which instead displays the bias reduction for each covariate following the matching implementation. The most striking improvements are recorded in terms of excess mortality in the first COVID-19 wave, population age and coastal indicator. Overall, the propensity score matching procedure allows us to reduce the overall mean bias in the predetermined time-invariant municipality characteristics between the treatment and the control group from 12.3% to 1.7%. The analysis described in Section 3.1 is then replicated on this matched sub-sample, in order to test the robustness of the findings to the pre-treatment differences between treated and control municipalities.

We also provide results with entropy balance matching (Hainmueller, 2012; Hainmueller and Xu, 2013), which is an alternative matching approach that avoids any sample size reduction. This method generates weights for all the municipalities that had at least one

[20]Importantly, before performing this exercise we discard municipalities with no COVID-19 infections in the sample, because we need to create a balanced subset only of those units contributing to the estimation of Equation 1. This is because the municipalities with zero cases in all weeks do not contribute to the likelihood of the model, due to the inclusion of municipality and week fixed effects.

[21]This is also confirmed by Figure A1, comparing the propensity score distributions before and after the matching is applied.

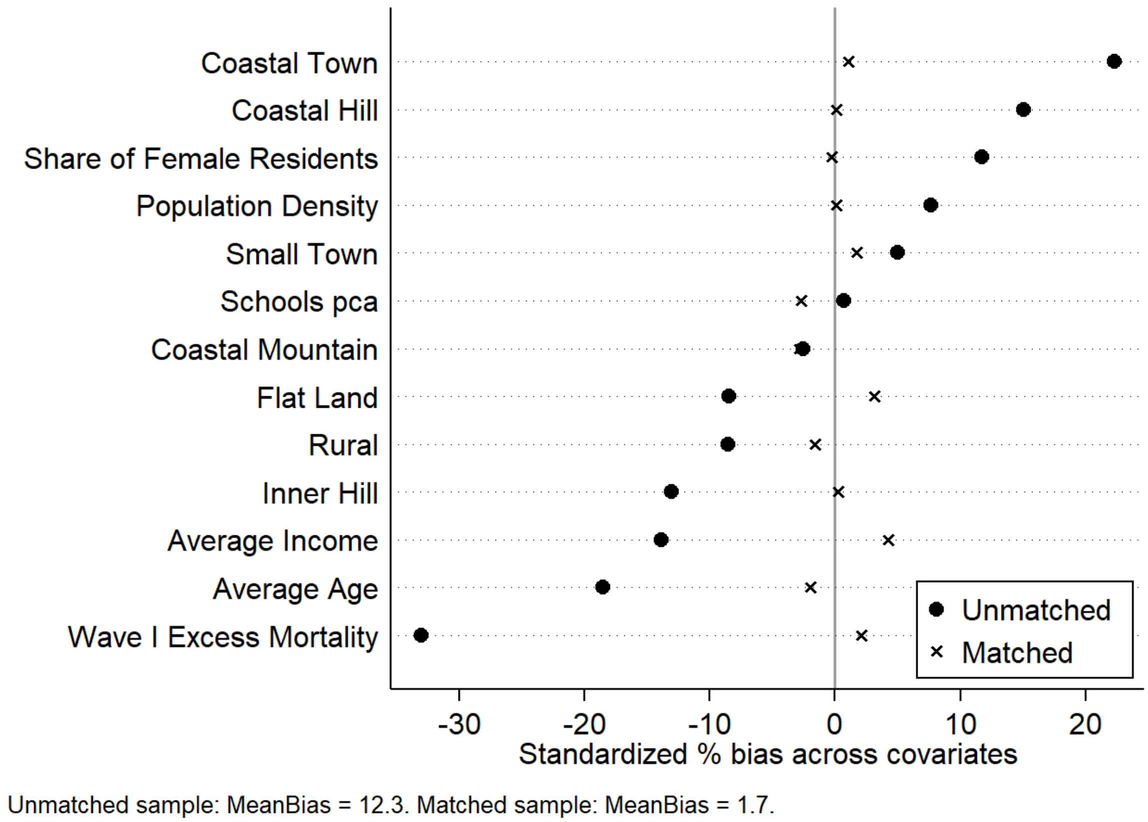


Figure 3: Covariate bias reduction after matching

COVID-19 infection in the period under study, allowing for the balancing of the first three moments of the distribution of the aforementioned municipality characteristics between the treated and control group. The summary statistics for this weighted sample are provided in Table A2. The baseline model, Equation 1, is then estimated using the full sample, but with weights produced by the entropy balance approach.

3.3 Control Function and bias from unobservables

There may still be municipality-level unobservable factors that pose an identification threat to our estimates, if they are correlated with both the outcome and the main regressor of interest, $TURN_i$. If such unobservable confounders were time-invariant at the municipality level, the bias to our estimated semi-elasticities would be removed thanks to the inclusion of municipality fixed effects. However, the time-invariance assumption of these correlated unobservables might be difficult to hold in a dynamic context like the one characterizing the COVID-19 epidemic.

There is a wide array of factors related to municipal population that we cannot explicitly control for, e.g. the mobility of residents, the share of commuters and the propensity to indulge in risky behaviors. Such latent factors could contribute to explain both the turnout rate and the trajectory of COVID-19 spread at the municipality level. In particular, a modified attitude to risk is one of our main concerns, given the results by Picchio and Santolini (2021) showing that Italian municipalities with a higher excess mortality among the elderly experienced a decrease in turnout at the mayoral elections, especially in densely populated areas.[22]

In order to overcome the hurdle posed by bias due to time-varying unobservable factors, we fully exploit the nature of our quasi-natural experiment and estimate a Control Function (Wooldridge, 2015a) modification of Equation 1, which is meant to tackle the left-over endogeneity in the referendum excess turnout. This strategy consists essentially in a two-stage residual inclusion (2SRI) approach (Terza et al., 2008). In the first stage, we estimate a linear model with the municipality turnout rate at the constitutional referendum as dependent variable, which we relate to the "treated" municipalities indicator, TR_i, the average municipality-level past turnout APT_i, the same covariates used for the calculation of the propensity score, Z_i, and Italian provinces (NUTS-3) dummies, π_p, to capture common time-invariant factors at medium area level that affect turnout:

$$TURN_i = \theta_0 + \theta_1 TR_i + \theta_2 APT_i + \theta_3 Z_i + \pi_p + r_i. \tag{2}$$

We then estimate the second-stage Poisson regression as:

$$\begin{aligned}\mathbb{E}(NC_{irt}|\mathbf{X_{irt}}) = exp\Bigg\{&\alpha_0 + \alpha_1 PCR_{rt} + \mu_i + \sum_{t'\neq t_0}\beta_t \mathbb{1}(t=t') + \sum_{t'\neq t_0}\gamma_t TURN_i \mathbb{1}(t=t') \\ &+ \sum_{t'\neq t_0}\omega_t APT_i \mathbb{1}(t=t') + \sum_{t'\neq t_0}\delta_t PD_i \mathbb{1}(t=t') \\ &+ \sum_{t'>t_2}\zeta_t OCT_i \mathbb{1}(t=t') + \sum_{t'\neq t_0}\rho_t \hat{r}_i \mathbb{1}(t=t')\Bigg\},\end{aligned} \tag{3}$$

where $\hat{r}_i = TURN_i - \widehat{TURN}_i$ are the estimated residuals from the first-stage model for the referendum turnout rate (2).[23]

[22] After the first COVID-19 wave in 2020, and before the availability of vaccines, voters might have acted strategically and chosen whether to participate in the ballots depending on the trade-off between the utility from exercising their political rights through voting and their personal risk to catch COVID and spread it to frail relatives. In other words, they might have sorted themselves into voting based on their expected unobservable gains (or losses) from voting (Heckman, 1997).

[23] Other, more complex Control Function approaches have been suggested to identify the average treatment effect (ATE) or the average treatment effect among the treated (ATT) when the endogenous regressor of

TR_i is the instrumental variable that we use to identify the model in Equation 3, as it provides a legitimate and significant source of exogenous variation in the municipality-level turnout rate at the constitutional referendum (see also Table A3).[24] In the second stage, we interact the predicted residuals $\hat{r}_i$ with the week indicators to control for the time-varying effects of unobservables that might still pollute our estimates.[25]

The standard errors of the second-stage outcome Equation 3 are bootstrapped with 1,000 replications and clustered at municipality-level to account for the two-step procedure (Murphy and Topel, 1985).

3.4 Spatial spillover effects in COVID-19 infections

Another legitimate concern is that Equation 3 does not account for the existence of spatial relationships across Italian municipalities. In fact, a local surge in coronavirus infections might spread to neighboring municipalities, if they are highly interconnected with each other and geographically close. This may be a concern since in the period of our study there were no mobility restrictions in place for Italian citizens, given the low level of new COVID-19 cases in Italy during July, August and the first twenty days of September 2020. Thus, the mobility of commuting workers, citizens and holidaymakers could introduce some confounding in our estimates. For this reason, we also implement a variation to our Control Function strategy that accounts for this potential source of bias.

First, we estimate a spatial weighting matrix (Anselin, 2001; LeSage, 2015) whose entries record the geographic distance of each municipality from its neighbors. [26] We provide three alternative matrix specifications, which differ in terms of the distance threshold used to classify two municipalities as neighbors: (i) 10 km; (ii) 30 km; and (iii) 60 km. Whenever two municipalities are not within the chosen distance threshold, their corresponding matrix

interest is continuous. For example, Florens et al. (2008) use a non-parametric strategy and show that both a continuous instrument and a polynomial restriction on the form of the treatment effect heterogeneity are required for identification. For simplicity's sake, we rely on a simpler parametric Control Function strategy, given our different setup with a binary instrument, data available only at aggregate, not individual level, and the complexity implied by need to reconcile a time-invariant first stage with a time-varying outcome equation.

[24]In analogy with the LATE framework (Imbens and Angrist, 1994; Angrist and Imbens, 1995; Angrist et al., 1996), the variation in the referendum turnout rate induced by TR_i, conditional on the other controls included in Equation 2, represents the share of voters acting like *compliers*, i.e. voters who cast their vote for both the referendum and the administrative elections only because they had an incentive to vote for the regional or mayoral government, but who would have not voted for the constitutional referendum otherwise.

[25]This interaction is also needed for a Control Function to be defined in this case, as Equation 2 is time-invariant. To the best of our knowledge, we are among the first to implement a Control Function approach in this particular fashion.

[26]Specifically, the rows of this 7,903x7,903 matrix contain the inverse distances of a given municipality from all the remaining ones in the sample.

cells are set to 0. Non-zero entries are instead row-normalized so that the sum of the weights attached to each municipality will be equal to 1.

Second, we use such spatial weighting matrix to construct a spatially lagged measure of new weekly coronavirus infections. Specifically, we create a weighted average of the number of new COVID-19 cases per 100,000 inhabitants among neighboring municipalities, using the matrix cells as weights (i.e. the normalized inverse distance of each municipality from its neighbors). The second-stage of our Control Function model is then augmented with this additional covariate, which is meant to control for the spatial spillover effects of coronavirus clusters.

4 Results

4.1 Summary statistics

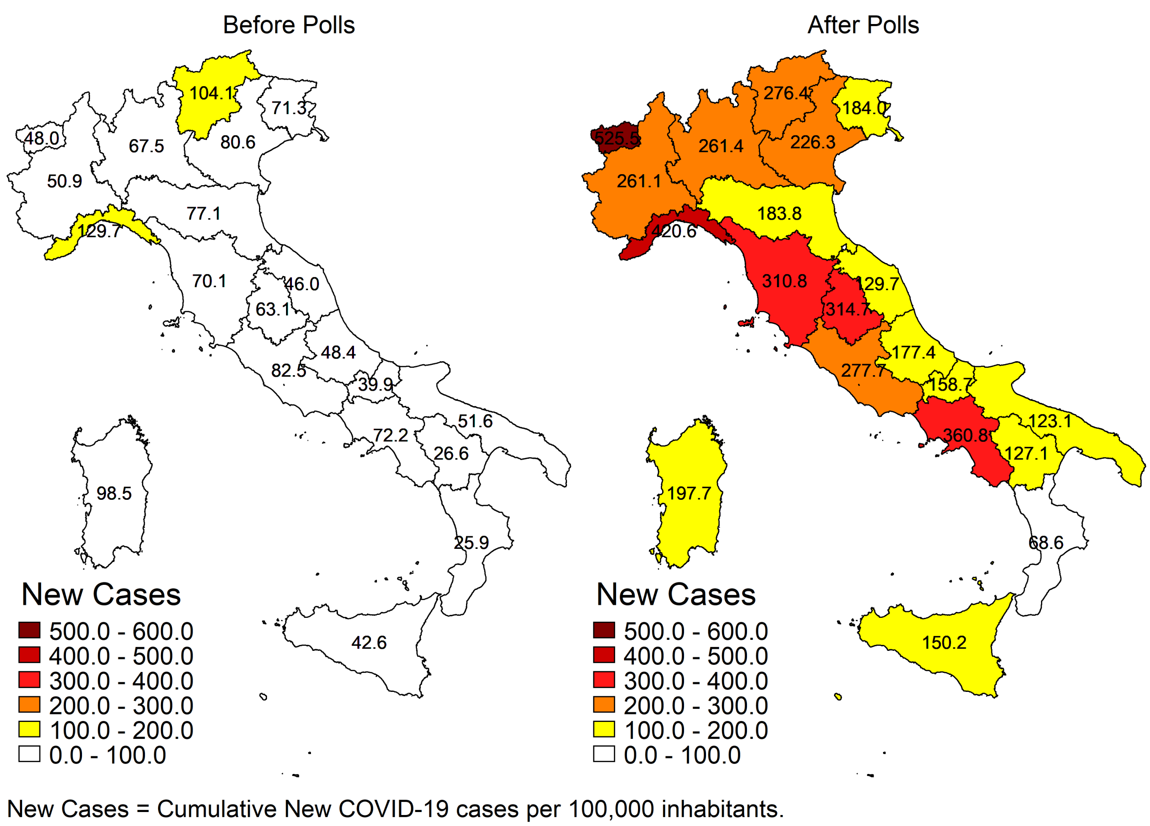


Figure 4: Regional COVID-19 rates around the election date

As for most European countries, over the Summer 2020 Italian rates of COVID-19 infections remained low. The second wave of the outbreak began in late September, right after the polls date. Figure 4 plots the incidence rates of COVID-19 in the four weeks preceding and in the four weeks following the polls. The most prominent rise in contagion occurred in Valle d'Aosta, which suffered an increase from 48 to 525 new coronavirus cases every 100,000 inhabitants. Remarkable rises in infections were also recorded in Campania and Toscana, where new COVID-19 infections went from approximately 70 to more than 300 every 100,000 inhabitants. Among regions where no regional elections took place, Umbria is where the outbreak worsened the most, since new cases went from 63 to 314 every 100,000 inhabitants within a few weeks.

Our sample is made of a total of 2,851 treated municipalities and 5,052 control ones. Summary statistics for these two groups of units are provided in Table 1. 20% of the former municipalities does not record any new COVID-19 infection in the period under study. This share is higher and equal to 28% in the control group. On average, treated municipalities have more residents than control municipalities. Usually, they also have a higher share of female residents and a younger population. The average turnout in the four past national-level elections (i.e. APT) was 58% for both groups of municipalities. Treated municipalities present, on average, a higher population density and a slightly greater number of schools per capita. They were also hit less by the first wave of COVID-19 in Spring 2020, as this wave hit fiercely some Northern Italian regions like Lombardia, Piemonte and Emilia-Romagna, whose municipalities mostly belong to the control group, as these were regions where only the constitutional referendum took place in September 2020.

Table 1: Summary statistics

	Treated		Control			
	Mean	Std. Dev.	Mean	Std. Dev.	Δ	t-test
Municipality						
Residents	8783.2	(27608.42)	6848.56	(48832.81)	1934.64	1.95*
Share of Female Residents	0.51	(0.02)	0.5	(0.02)	0.00	4.63***
Average Age	46.22	(3.42)	47.04	(3.32)	-0.82	-10.43***
Population Density	0.35	(0.80)	0.28	(0.53)	0.07	4.69***
Average Income (€1000)	18.68	(3.89)	18.89	(4.32)	-0.21	-2.13**
Wave I Excess Mortality	0.67	(2.64)	1.4	(3.56)	-0.74	-9.65***
Schools pca	1.47	(1.03)	1.45	(1.14)	0.02	0.75
Turnout	69.03	(8.57)	47.48	(8.56)	21.56	107.52***
APT	57.87	(7.01)	57.7	(7.87)	0.17	0.98
Covid Cases						
Zero cases	0.2	(0.40)	0.28	(0.45)	-0.08	-7.73***
Weekly Covid Rate						
24/08 - 30/08	12.93	(54.20)	11.59	(44.06)	1.34	1.19
31/08 - 06/09	14.14	(75.12)	12.36	(50.10)	1.78	1.26
07/09 - 13/09	15.17	(44.53)	14.53	(66.01)	0.64	0.46
14/09 - 20/09	18.08	(58.93)	14.9	(65.97)	3.18	2.14**
21/09 - 27/09	18.98	(61.29)	20.74	(117.31)	-1.76	-0.74
28/09 - 04/10	29.81	(98.12)	27.24	(183.37)	2.58	0.70
05/10 - 11/10	57.88	(150.97)	48.28	(198.16)	9.60	2.25**
12/10 - 18/10	104.1	(163.00)	95.48	(184.78)	8.62	2.08**
Municipality-Week observations	22,808		40,416			
Municipalities	2,851		5,052			

Notes: Covid Rate is defined as the number of new coronavirus cases every 100,000 residents. Treated municipalities held both the constitutional referendum and either regional or mayoral elections (or both) on September 2020. Control municipalities held only the constitutional referendum on September 2020. APT = Average Past Turnout in the four elections held nationally.

4.2 Baseline fixed-effects Poisson regression model

The estimates of Equation 1 are provided in Table 2. Panel A reports the event-study coefficients. We do not find any significant pre-trend as a function of the referendum turnout. On the contrary, we do find semi-elasticities for the turnout-week interactions in the post-poll period that are significant at least at the 5% level, showing how higher voters' participation in September 2020 contributed to the spread of COVID-19 infections. These results are quite consistent regardless of whether the sample is matched or not, although the magnitude of the semi-elasticities gets smaller especially after the implementation of the entropy balance weighting scheme. For instance, Column 1 indicates that one additional point in the referendum turnout was associated with a 1.3% increase in new COVID-19 infections after two weeks from the polls.

Table 2: Effects of Turnout on COVID-19 infections

	New COVID-19 cases					
	(1)		(2)		(3)	
Panel A: Event-Study						
3 weeks pre-poll * Turnout	0.001	(0.004)	0.001	(0.004)	-0.001	(0.004)
2 weeks pre-poll * Turnout	0.001	(0.004)	0.002	(0.004)	-0.002	(0.004)
1 week pre-poll * Turnout	0.004	(0.003)	0.003	(0.003)	0.001	(0.003)
1 week post-poll * Turnout	0.008***	(0.002)	0.005**	(0.002)	0.007***	(0.002)
2 weeks post-poll * Turnout	0.013***	(0.003)	0.010***	(0.003)	0.010***	(0.003)
3 weeks post-poll * Turnout	0.011***	(0.004)	0.010***	(0.004)	0.007**	(0.004)
4 weeks post-poll * Turnout	0.015***	(0.003)	0.014***	(0.004)	0.011***	(0.003)
3 weeks pre-poll * APT	0.014*	(0.009)	0.019***	(0.007)	0.014*	(0.008)
2 weeks pre-poll * APT	0.008	(0.006)	0.006	(0.006)	0.012**	(0.006)
1 week pre-poll * APT	0.004	(0.004)	0.006	(0.004)	0.004	(0.004)
1 week post-poll * APT	-0.021***	(0.004)	-0.012***	(0.004)	-0.021***	(0.005)
2 weeks post-poll * APT	-0.018***	(0.005)	-0.009	(0.006)	-0.022***	(0.006)
3 weeks post-poll * APT	-0.005	(0.005)	-0.002	(0.006)	-0.005	(0.005)
4 weeks post-poll * APT	0.003	(0.005)	0.006	(0.005)	0.001	(0.005)
Panel B: DiD						
Post-poll	0.854***	(0.212)	0.560**	(0.232)	1.055***	(0.203)
Post-poll * Turnout	0.015***	(0.003)	0.012***	(0.003)	0.014***	(0.003)
Post-poll * APT	-0.016***	(0.004)	-0.008*	(0.004)	-0.017***	(0.004)
Sample	Unmatched		Matched (NN)		Matched (EB)	
Treated Municipalities	2,267		2,195		2,267	
Control Municipalities	3,620		2,195		3,620	
Municipality-Week observations	47,096		35,120		47,096	

Notes: Fixed-effects Poisson semi-elasticities in the full sample (Column 1), nearest neighbor matched sub-sample (Columns 2) and entropy balance weighted sample (Column 3). Event study design in Panel A, Difference-in-difference model in Panel B. Controls included (but not reported): population density interacted with the week (Panel A) or post-poll (Panel B) indicators; post October polls week indicators (Panel A) or dummy (Panel B) interacted with an indicator for municipalities that had a second ballot or the first ballot of mayoral elections on 4th and 5th October 2020. Regional PCR tests performed per 10,000 inhabitants. APT = Average turnout in the four past elections held nationally. List of variables used for matching as in Figure 3. Municipality-level clustered standard errors in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

The interactions between the week indicators and APT_i, included to proxy for the time-varying effects of civic capital, are negative in the first two weeks post polls. Intuitively, this result indicates that voters of municipalities with higher civic capital are more likely to abide to social distancing rules and use of NPIs, which reduces the number of COVID-19 infections at the ballot box even in the case of a large voting turnout.

Panel B reports DiD estimates of our continuous treatment effect. The post-poll effect of turnout is positive and significant at 1% level across all models. On average, new COVID-19 infections increased by 1.5% within four weeks from the polls for each additional point of referendum turnout (Column 1). Again, the effect of our civic capital proxy is negative and significant at 1% level in the unmatched and entropy balance matched samples, but smaller is absolute value and significant at only 10% level in the nearest neighbor matched sub-sample.

4.3 Control Function event study

Figure 5 reports the estimated elasticities after we implement the Control Function (CF) approach described in Section 3.3, whereas the corresponding semi-elasticities and the first stage key coefficients are reported in Table A4.

The results are consistent with those presented in Table 2. Interestingly, the first-stage residuals capture some positive correlations between the model for turnout and the outcome equation for new COVID-19 infections. Through this two-step CF strategy, we are able to decompose the effects of the observed turnout in the three components shown in Figure 5. The first component is given by the time-varying effects of excess turnout at the referendum, still with respect to the average past turnout at municipality level. The second component is given by the time-varying effects of civic capital proxied by the average past turnout. The third component is instead given by the time-varying effects of aggregate 'selection into voting' at municipality level.

From the first stage regression in Panel A of Table A4, we note how the 2020 referendum turnout is positively associated with both the 'treatment' indicator for regional or mayoral elections and the civic capital proxy. Instead, it is negatively associated with both high excess mortality during the first COVID-19 wave (March to June 2020) and population density. These estimates suggest that voters were sensitive to the incentive to cast their referendum ballot in municipalities subject to an additional administrative election. Moreover, voters acted strategically choosing to show themselves at the ballots according to their expected gains from the trade-off between exercising their right to vote, which is likely a positive function of civic capital, and risking to catch COVID-19, which is positively associated with a high first wave excess mortality and high population density, especially for the elderly. This strategic choice at municipal population level is consistent with the associations between the 2020 voters' turnout and the first wave excess mortality shown by Picchio and Santolini (2021).

Despite the interactions between the first-stage residuals and the post-poll week indicators are statistically significant, the semi-elasticities for turnout with Control Function are almost identical to the point estimates of Equation 1 reported in Table 2.

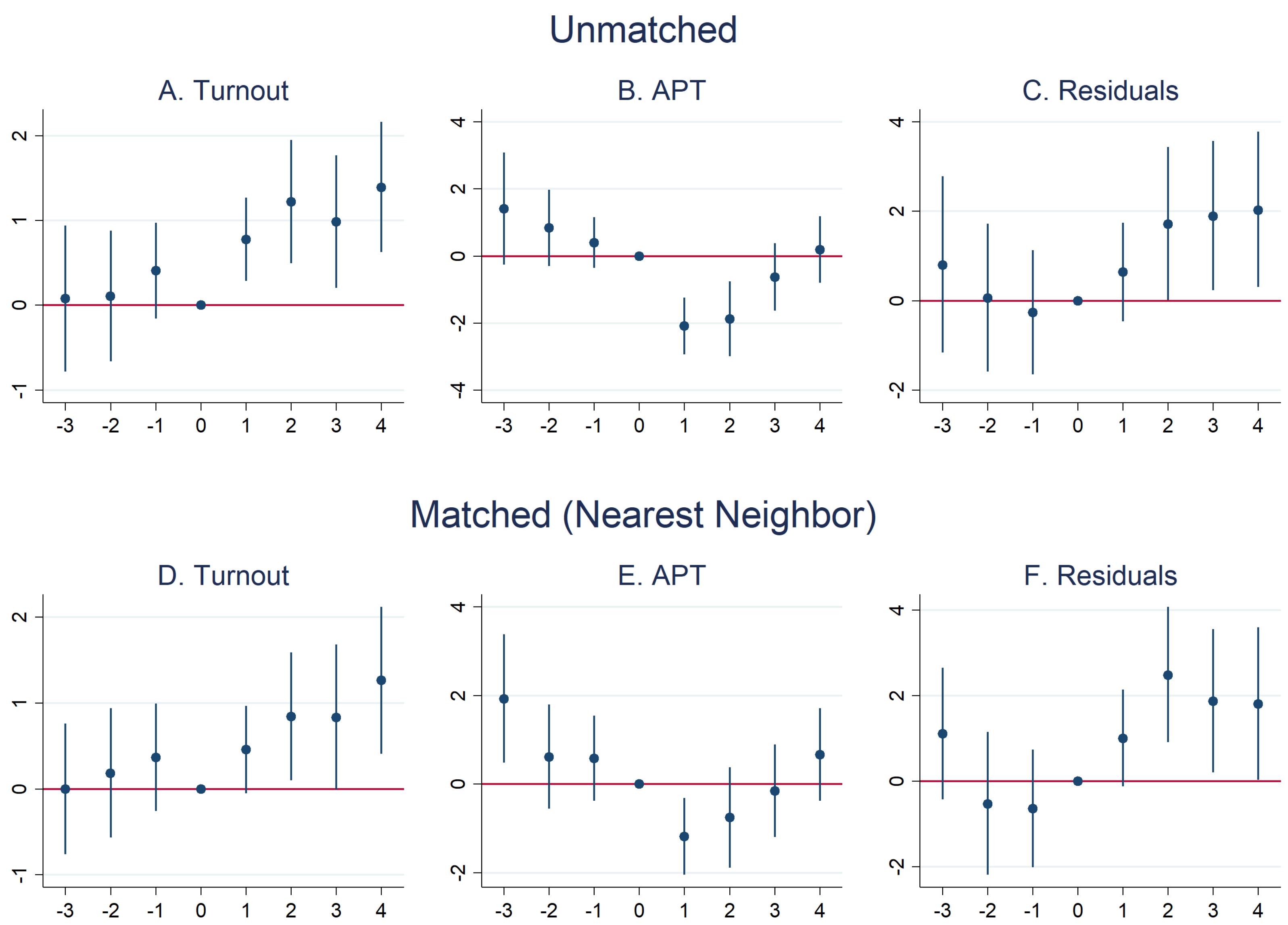


Figure 5: Effects of Turnout with Control Function: Elasticities and 95% Confidence Intervals

4.4 Controlling for spatial autocorrelation in COVID infections

Table 3 reports estimates of Equation 3, but including the interactions between the week indicators and the spatial lag of new COVID infections per 100,000 inhabitants as additional controls. These coefficients indicate a positive and highly significant spatial correlation in most of the weeks of our sample, and especially in the last three weeks, when the spatial lag interactions are significant at the 1% level in all the specifications reported in Table 3.

The magnitude of the spatial effects is higher for larger distance thresholds of the spatial autocorrelation matrix. This finding may be an indication that a wider radius to define neighboring municipalities allows us to better capture the spatial structure of the spread of COVID-19. However, our preferred specification of this model is the one provided in Column 2, based on a 30 km radius, as a very large radius (60 km) is also more likely to capture spurious correlations from urbanized areas, given most municipalities in Italy are placed within a 60 km radius from large towns and province capitals.

Nevertheless, our estimates of interest (i.e. the weekly interactions with the referendum turnout variable) are in line with those presented in the previous sections. We interpret this result as evidence that spillover effects in COVID-19 infections are not a serious confounder for our analysis.

4.5 Excess Mortality

This sub-section investigates whether the post-poll rise in COVID-19 cases had any effect on mortality. This analysis spans over a slightly longer period, going from 4 weeks before to 8 weeks after the 2020 polls, since mortality outcomes due to COVID-19 take time to manifest, with most of the people dying from (or with) COVID being first admitted to hospitals (often in ICUs) before their demise. The measure of excess mortality that we use is:

$$EM_{it} = \#Deaths_{it}^{2020} - \overline{\#Deaths_{it}}^{2015/2019},$$

i.e. the difference between the number of total deaths in municipality i and week t in 2020 and its corresponding average value in the preceding five-year period.[27]

[27] This is similar, yet slightly different, to the definition of municipality-level excess mortality used in Section 4.2 to Section 4.4, where we have standardized the number of deaths during the COVID-19 first wave and in the previous five years by the number of municipality residents, i.e, $FWEM_i = \frac{\#Deaths_i^{2020}}{\#Residents_i^{2020}} - \frac{\overline{\#Deaths_i}^{2015/2019}}{\overline{\#Residents_i}^{2015/2019}}$.

Table 3: Effects of Turnout on COVID-19 infections controlling for spatial autocorrelation

	W^{10km} (1)	W^{30km} (2)	W^{60km} (3)
3 weeks pre-poll * Turnout	-0.000	-0.000	-0.001
	(0.004)	(0.004)	(0.004)
2 weeks pre-poll * Turnout	0.001	0.000	0.000
	(0.004)	(0.004)	(0.004)
1 week pre-poll * Turnout	0.004	0.003	0.003
	(0.003)	(0.002)	(0.003)
1 week post-poll * Turnout	0.007***	0.007***	0.006**
	(0.002)	(0.003)	(0.003)
2 weeks post-poll * Turnout	0.012***	0.012***	0.012***
	(0.003)	(0.003)	(0.004)
3 weeks post-poll * Turnout	0.009**	0.008**	0.008**
	(0.004)	(0.003)	(0.003)
4 weeks post-poll * Turnout	0.012***	0.011***	0.011***
	(0.003)	(0.003)	(0.003)
3 weeks pre-poll * New Cases Spatial Lag	0.003**	0.007**	0.018***
	(0.002)	(0.003)	(0.005)
2 weeks pre-poll * New Cases Spatial Lag	0.000	0.002	0.005
	(0.001)	(0.003)	(0.005)
1 week pre-poll * New Cases Spatial Lag	0.001	0.004**	0.008**
	(0.001)	(0.002)	(0.003)
poll week * New Cases Spatial Lag	0.003*	0.009***	0.014***
	(0.001)	(0.003)	(0.004)
1 week post-poll * New Cases Spatial Lag	0.001	0.003**	0.007***
	(0.001)	(0.001)	(0.002)
2 weeks post-poll * New Cases Spatial Lag	0.001***	0.002**	0.006***
	(0.000)	(0.001)	(0.001)
3 weeks post-poll * New Cases Spatial Lag	0.001***	0.003***	0.005***
	(0.000)	(0.001)	(0.001)
4 weeks post-poll * New Cases Spatial Lag	0.001***	0.003***	0.005***
	(0.000)	(0.001)	(0.001)
Sample	Unmatched	Unmatched	Unmatched
Treated Municipalities	2,267	2,267	2,267
Control Municipalities	3,620	3,620	3,620
Municipality-Week observations	47,096	47,096	47,096
Distance	10km	30km	60km
CF	Yes	Yes	Yes

Notes: Fixed-effects Poisson semi-elasticities in the augmented model with spatially lagged coronavirus infections. Controls included (but not reported): week indicators; population density interacted with the week (Panel A) or post-poll (Panel B) indicators; post October polls week indicators (Panel A) or dummy (Panel B) interacted with an indicator for municipalities that had a second ballot or the first ballot of mayoral elections on 4th and 5th October 2020; Regional PCR tests performed per 10,000 inhabitants. Municipality-level clustered bootstrapped standard errors (1,000 iterations) in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

We then estimate the following linear model for excess mortality:

$$
\begin{aligned}
ln(EM_{it} + \sqrt{EM_{it}^2 + 1}) = \alpha_0 + \mu_i + \sum_{t' \neq t_0} \beta_t \mathbb{1}(t = t') + \sum_{t' \neq t_0} \gamma_t TURN_i \mathbb{1}(t = t') + \\
\sum_{t' \neq t_0} \omega_t APT_i \mathbb{1}(t = t') + \sum_{t' \neq t_0} \delta_t PD_i \mathbb{1}(t = t') + \\
\sum_{t' > t_2} \zeta_t OCT_i \mathbb{1}(t = t') + \sum_{t' \neq t_0} \rho_t \hat{r}_i \mathbb{1}(t = t') + \varepsilon_{it},
\end{aligned}
\tag{4}
$$

where $ln(EM_{it} + \sqrt{EM_{it}^2 + 1})$ is the inverse hyperbolic sine transformation of our excess mortality measure, which we apply to account for the very low excess mortality (i.e. right-skeweness) that characterizes most of our sample (see Figure A2). Moreover, it allows us to interpret the model coefficients as semi-elasticities, similarly to the FE-Poisson models estimated in the other sections of this paper, while still retaining zeros and negative values in the excess mortality dependent variable (Bellemare and Wichman, 2020).[28]

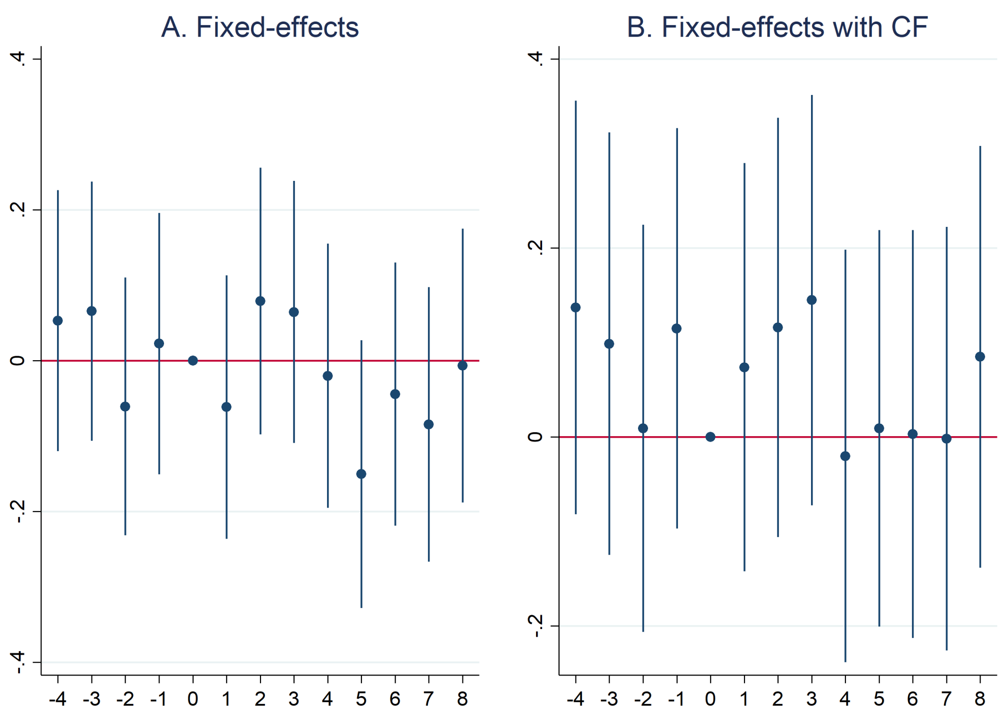


Figure 6: Effect of Turnout on excess mortality

[28]According to Bellemare and Wichman (2020), the elasticity estimates may suffer from a substantial approximation error if the values of the dependent variable to be transformed are not large enough. This issue does not seem to characterize our case, as Equation 4 provides qualitatively similar findings even when we rescale our measure of excess mortality.

The model includes municipality fixed effects and is estimated by OLS, given that the support of the dependent variable corresponds to the entire real line. We estimate this model first without the inclusion of the interaction between the week indicators and APT_i, then by including these terms and using a CF strategy. The results of the two specifications are provided in Columns 1 and 2 of Table A5, respectively.

The vector of coefficients of interest, reported in Figure 6, does not indicate any effect of turnout on excess mortality up to two months from the election date. A likely reason for this is that, under a regime of low infection rates as the one experienced by Italy in September 2020, infections translate into extremely low COVID-19 deaths. As such, overall excess mortality might be a lousy proxy for COVID-19 related mortality in our sample, differently from periods of high contagion.

5 Robustness checks

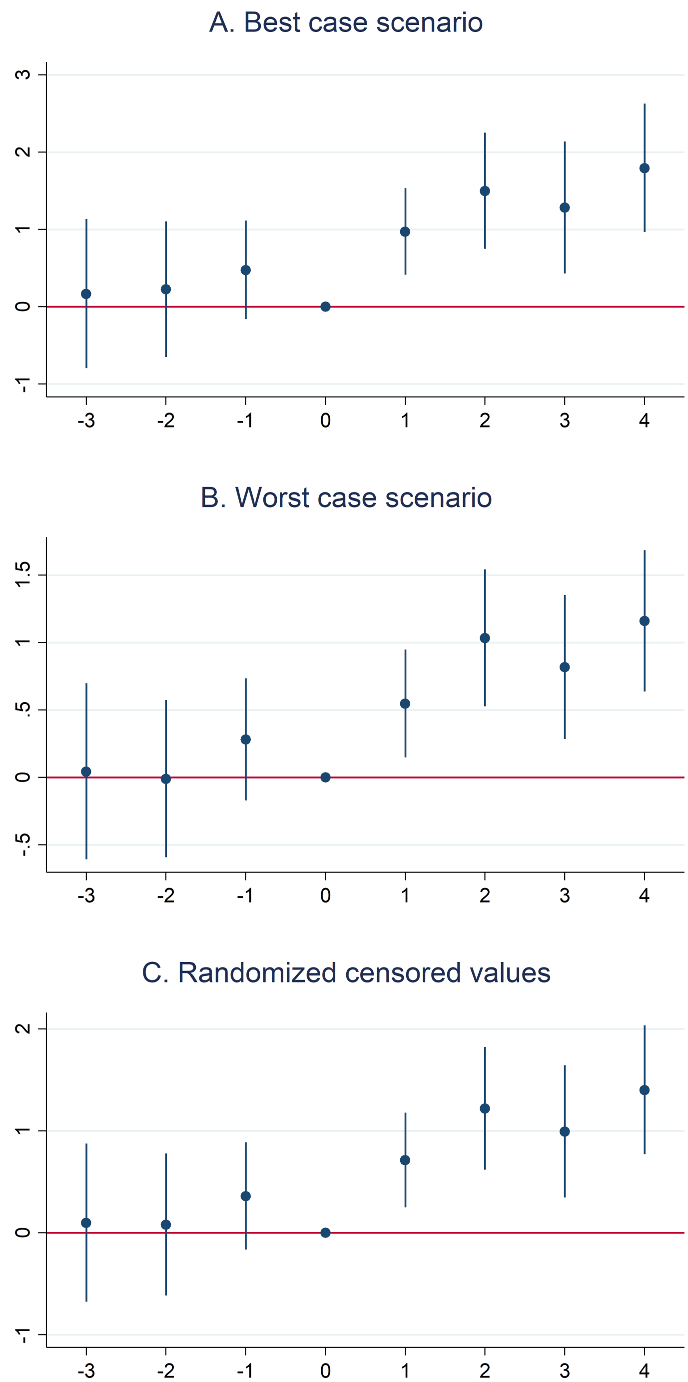
We run several checks to assess the robustness of our findings.

5.1 Left-censoring of the outcome variable

First, we check how results change if we treat censored values in the number of new weekly COVID-19 cases differently. This analysis is crucial, because 30,59% of non-zero weekly municipality infections in our sample are censored in the interval $[1, 4]$ for privacy reason by the data provider (ISS). To do so, we examine how our baseline estimates for Equation 1 vary: (i) in the worst and in the best case scenarios, namely when we replace the censored values respectively with new weekly infection values of 4 and 1; (ii) and when we randomize censored coronavirus infections using 2,000 draws from a uniform distribution with 1 and 4 as extreme values, clustered by each province-week pair in our sample.[29]

Results for these alternative specifications are provided in Table A6, while the elasticities of interest are displayed in Figure 7. The pattern and significance of these estimates are in line with those obtained by replacing censored values with 2, with the only difference that the effects of interest are smaller in magnitude in the worst case scenario. We conclude that the way we handle the censoring does not drive the qualitative findings of this study.

[29] For a likely randomization over time and across municipalities to hold, we necessarily need to cluster at the geography level immediately higher than municipality, i.e. provinces.



Unmatched sample, Turnout elasticities.
A. Censored positive values smaller than 5 replaced with 1.
B. Censored positive values smaller than 5 replaced with 4.
C. Randomized censored values between 1 and 4 in 2,000 iterations.

Figure 7: Robustness checks to left censoring

5.2 Inclusion and exclusion of the number of PCR tests as control

Second, we provide alternative specifications with respect to the PCR tests control variable. Indeed, the latter may depend on the stage of the epidemic, thus it might also be affected by the occurrence of the polls. For this reason, in Table A7 we report estimates of variants of Equation 3, where the variable PCR_{rt} has been either omitted (Column 1) or replaced with either: (i) the "frozen" average number of regional tests performed in the first three pre-poll weeks, interacted with a post-poll indicator (Columns 2); or (ii) the total number of regional PCR tests performed, but weighted by municipal population density (Column 3).

These specifications provide different ways to deal with the possibility that PCR_{rt} might eventually be considered a bad control in our models, despite such variable is measured at a higher aggregation level (regional) than the treatment of interest (municipality). All estimates from these three alternative specifications provide very similar coefficients of interest on the turnout-week interactions, which are in line with the coefficients discussed in Section 4.3. The only exception is the point estimate for the fourth week post polls, which is smaller in Table A7. As such, it seems that the effect of turnout on COVID-19 spread does not depend on controlling for the number of PCR tests run.

5.3 Confounding due to the start of the compulsory schooling term

The treatment examined in this paper falls exactly around the Italian school opening date, which happened in most regions on the Monday after the polls.[30] Thus, it is important to check for the possible confounding of school openings in our effect of interest. To do so, we augment Equation 3 by interacting the week indicators with the time-invariant number of schools in a given municipality. The results of these specifications are provided in Table A8, where we use the number of schools in Column 1, and the number of schools per capita in Column 2.

We find a positive and significant relationship between school density and new weekly COVID-19 infections only if we weigh the number of schools by municipality population, at least in the first two weeks following the polls. Nevertheless, our main coefficients of interest are significant and mostly unchanged in magnitude. Hence, the re-opening of schools cannot explain the findings of this study.

[30]We notice that some schools opened just for a very short period of time in many Italian municipalities because of the beginning of the second national COVID-19 wave.

5.4 Including time-varying effects of all predetermined variables

Table A9 tests the robustness of our findings by including in Equation 3 the interactions between the week indicators and all the predetermined municipality characteristics included in the first stage explaining the municipality turnout. The post-poll effects of interest are still significant, although slightly smaller in magnitude in the third and fourth weeks post-polls than those reported in Table A4. This finding indicates how our results hold even after controlling for the time-varying effect of a rich set of demographic and geographic characteristics at the municipality-level on the spread of COVID-19.

6 Cost-benefit simulation: healthcare expenditures and lives saved from preventing a national-level general election

The results shown so far testify a significant and sizeable increase in the number of new COVID-19 infections as a result of higher poll turnout. Given the low level of infection rates in the weeks before the 2020 Italian polls (see Figure 4), it is uncertain how dramatic the impact of these polls was, although it is plausible that they have played a significant role in reigniting the infection spread, thus contributing to the explosion of the second wave of the epidemic in Italy during Fall 2020.

Most importantly, though, the results of the previous analyses allow us to undertake a cost-benefit simulation, based on real political events in the recent Italian history, which is important to quantify the likely implied monetary and non-monetary costs associated to holding national-level elections during a period of high infection rates and higher transmissibility of a virus.

In January 2021 the Italian coalition Government in charge, led by Giuseppe Conte, collapsed over disputes among its supporting political parties about the plans for spending the EU recovery funds to face the COVID crisis.[31] The two scenarios that opened up back then were either the appointment of a new coalition Government, with Conte or another person as Prime Minister (PM), or having early nation-wide general elections to renew the members of the Italian Parliament. The opinion polls commissioned by the main newspapers showed how the general public was split over this issue, with a range from 20% to 39% of potential voters in favour of calling for an early election.[32]

[31] https://www.bbc.co.uk/news/world-europe-55661781; https://www.businesstimes.com.sg/opinion/italys-political-instability-brings-new-unease-into-the-eu.

[32] https://www.repubblica.it/politica/2021/01/27/news/crisi_governo_sondaggio_elezioni_

In the first instance, though, and following a consolidated institutional approach to solve political crises in Italy, the President of the Republic decided to explore the possibility of a new coalition Government without calling yet for a national early election, motivating his choice with the need for the continuity of the action of a Government with full powers to keep a steady management of three urgent political issues: the COVID healthcare crisis; the planning for the EU Recovery Funds; and the emergency regulations to aid citizens, workers and businesses under distress because of the economic and social impact of COVID.[33] We exploit these political events and simulate a real-case scenario of the "What If?" impact of an early election on the increase of COVID-19 negative outcomes as new COVID infections, Intensive Care Units (ICU) hospitalizations, non-ICU hospitalizations, COVID-related deaths, and the monetary costs in Euro associated to these outcomes.

Our calculations are based on the following assumptions (***A***). The early election should have occurred by early to mid-March 2021 (***A1***). This is because the deadline for the submission of the plans to access the EU Recovery Funds was 30$^{\text{th}}$ April 2021, and it usually takes at least 1.5 months after an election day to elect the new Presidents of the Chambers of the Italian Parliament and to form the Parliamentary Commissions that, together with the Government, lead the legislative process in Italy. For such reason, our baseline value of new COVID-19 cases is the total number of new infections registered in Italy during the first four weeks of March 2021 (***A2***).[34] We also assume that the case fatality rate (CFR) is equal to the one observed in March 2021 (***A3***), as according to computations based on the COVID-19 Data Repository at Johns Hopkins University [35]

We report estimates of the simulated health outcomes impacts depending on whether the coronavirus lineage was either B.1.1.7, the so called "English variant", or a mix of any of the pre-existing COVID-19 strains. The coronavirus strain B.1.1.7 begun circulating in Italy by the end of January 2021, despite travel and border restrictions, accounting for 34% of new cases, i.e. already the relative majority, by end of February 2021, 86% of new cases by mid-March and 91% of new cases by 15$^{\text{th}}$ April 2021 (Di Giallonardo et al., 2021; ISS, 2021). In particular, we assume that transmisibility of the strain B.1.1.7 to be only 50% higher than pre-existing lineages (***A4***), which corresponds to the lower bound of this strain's transmissibility found by Volz et al. (2021) and Davies et al. (2021), whereas the estimated upper bound was either a 90% or 100% higher virus transmissibility.

We assume a zero-valued expectation for the life lost by COVID-19 patients older than

conte-284457528/; https://www.ilgiorno.it/politica/sondaggio-no-voto-1.5952867; https://www.tpi.it/app/uploads/2021/01/sondaggio-sole.pdf.

[33] https://www.youtube.com/watch?v=uSeLmozgWSc.

[34] https://altems.unicatt.it/altems-Report%2046-compresso.pdf.

[35] https://ourworldindata.org/mortality-risk-covid?country=~ITA.

80 years (***A5***), given that the average life expectancy in Italy is of 84 years, despite it is likely that these patients might survive longer, although not in a "perfect health" status, in the absence of COVID-19. Moreover, as shown in Table A10, we assume patients over 75 years old to live on average for five years (i.e. until 80 years) and the following four years (i.e. until 84 years) in health statuses valued respectively at 80% and 50% of their full health (***A6***).

For simplicity's sake, the post-election spread of the virus is assumed to follow the DiD point estimate valued 0.011 (from the CF model with a pre-processed sample through nearest neighbor matching, as reported in Table A4, third Column, Panel B) based on the monthly effect of the 2020 referendum turnout variable (***A7***); whereas the turnout of the early general elections would be equal to 72.94% (***A8***), i.e. the same turnout of the 2018 Italian general elections.

We also focus on a short-to-medium term impact of the elections on the spread of the virus by limiting the time-horizon to the four weeks after the election (***A9***).[36]

Finally, we also implicitly assume that voters' attitude towards COVID-19 infection risk would have been the same in September 2020 and in the averted general elections in March 2021, which is not necessarily the case if voters were to take more precautions to avoid contagion in response to the higher COVID-19 transmission rates during Spring 2021 (***A10***). Despite the latter assumption may seem rather strong, it is counterbalanced by assumptions A4-A9, whose contribution is to make the our calculations rather conservative.

The results of the cost-benefit simulation are reported in Table 4. The upper panel (*Panel A*) reports the main inputs for the computations. The lower panel (*Panel B*) reports the estimates of interest in terms of prevented new COVID-19 cases, ICU and non-ICU hospitalizations, and lives saved. For brevity's sake, the way we calculated each table entry is explained in the notes of Table 4; the results also draw upon the computations from Table A10, in which we estimate the value of lives at risk due to COVID by age categories, using data on life expectancy and COVID mortality for the Italian population.

According to our preferred summary estimate of the effect of interest (i.e. the DiD specification based on the Control Function model after nearest neighbor matching and the virus trasmission of the COVID variant B.1.1.7), an early general election in the Spring 2021 would have generated up to additional 722,165 COVID-19 infections in Italy within four weeks from the election date. This increase would have translated into approximately 8,377

[36] This approach clearly ignores the possible longer-term effects of holding elections, as the transmission of the virus is exponential and so an incremental contagion due to the elections should be expected even beyond the fourth week after the polls. However, the estimation of such extended effects would likely require a more complicated SIR model that is not necessarily consistent with our empirical strategy, and it is beyond the scope of this study.

Table 4: Cost-benefit simulation for avoiding national level political elections in March 2021

Panel A: Inputs	**New Cases (A2)**	**% Non-ICU admissions to hospital (B2)**	**% ICU admissions to hospital (C2)**	**Case Fatality Rate (D2)**	**Turnout 2018 general elections (E2)**	**Average DGR in-hospital stay cost (€) - patient dicharged as alive (F2)**	**Average DGR in-hospital stay cost (€) - patient dicharged as dead (G2)**	**Average years of life expectancy in Italy (H2)**	**Willingness-to-Pay for 1 year of QALY in € (I2)**	**Transmissibility multiplier of SARS-CoV-2 variant B.1.1.7 with respect to previous variants (J2)**
	596,755	4.75%	1.16%	3.17%	72.94%	€ 8,476.00	€ 9,796	83.57	€ 74,159.00	1.5
Panel B: Estimates	**Coefficient estimates (K2)**	**Coefficient standard errors (L2)**	**COVID-19 strain (M2)**	**Predicted Additional Cases (N2)**	**Predicted averted additional non-ICU hospitalizations (O2)**	**Predicted averted additional costs (€) of non-ICU hospitalizations (P2)**	**Predicted averted additional ICU hospitalizations (Q2)**	**Predicted averted additional costs (€) of ICU hospitalizations (R2)**	**Predicted lives saved (S2)**	**Predicted value (€) of lives saved (T2)**
Post-poll (DiD)	0.011	0.003	Pre-B.1.1.7	481,443.5	22,868.6	193,833,964	5,584.7	47,336,294	15,261.8	5,025,974,091
			B.1.1.7	722,165.2	34,302.8	290,750,946	8,377.1	71,004,442	22,892.6	7,538,961,137

Notes. (A2): The number of new coronavirus infections in the whole Italy between March 1 and March 28 (4 weeks); data source: Italian Civic Protection Department. (B2): Ordinary hospitalizations / currently infected, i.e. the average share of (total) infected people by COVID-19 requiring non-ICU hospitalization between March 1 and March 28 (4 weeks); data source: Italian Civic Protection Department. (C2): New ICU admissions / New infections, i.e. the average share of new infected people by COVID-19 requiring ICU between March 1 and March 28 (4 weeks); data source: Italian Civic Protection Department. (D2): Raw one week Case Fatality Rate (CFR), i.e. the number of dead among the number of diagnosed COVID-19 cases only, as estimated by Our World in Data (`https://ourworldindata.org/mortality-risk-covid?country=~ITA`) based on COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University. (F2-G2) Source: estimates by the ALTEMS research team (`https://altems.unicatt.it/altems-Report%2046-compresso.pdf`). (H2) Source: `https://www.macrotrends.net/countries/ITA/italy/life-expectancy`. (I2) Source: Ryen and Svensson (2015). (J2) Source: Volz et al. (2021). (K2-L2) Source: authors computations, Table 3. Cells in (N2) = $100 * [exp(K2) - 1]*$(A2)*(E2). (O2) = (N2)*(B2). (P2) = (O2)*(F2). (Q2) = (N2)*(C2). (R2) = (Q2)*(F2). (S2) = (N2)*(D2). (T2) = (N2)* €329,318.15 as computed in Table A10, based on the specific risks of COVID-19 infection, mortality and computations of the expected years of life lost by age categories as reported in Table A10.

ICU (**Q2**) and 34,302 non-ICU (**O2**) hospitalizations, which imply monetary costs worth respectively about €71 millions (**R2**) and €290.751 millions (**P2**) for the Italian NHS, i.e. a total of €361.751 (USD $428.87) millions. This sum is not negligible and equal to 1.79% of the total hospital admissions costs sustained by the Italian State from the start of the epidemic till end of March 2021.[37] Moreover, the additional death toll would have been equal to 22,893 (**S2**), corresponding to a value of about €7.538 (USD $8.936) billions in terms of lives saved (**T2**).

Finally, these costs estimates do not take into account the additional labor market losses that would have accrued for the extra-patients infected because of the 2021 elections, a part of whom would have been limited to work due to the disease, as well as the extra costs for COVID-19 testing for these patients.

7 Conclusions

Up until recently, there was no available clear-cut evidence about the effects of holding polls on the spread of highly infectious airborne diseases, as during the current pandemic. This lack of evidence has left the choice of whether to hold or postpone forthcoming elections to the discretion of politicians and their public health advisors. Our study tries to fill this gap, providing one of the first causal estimates of the effect of voters' turnout on the spread of COVID-19. By exploiting an exogenous variation in the turnout rate stemming from the heterogeneous number of polls held in September 2020 in Italian municipalities, we overcome the main identification threat to the estimation of the causal nexus between turnout and contagion, and we find that a 1% increase in the turnout for the constitutional referendum was associated with at least an average 1.1% increase in post-poll COVID-19 infections. This finding is robust to a series of sensitivity checks like the pre-processing of the sample by multiple matching approaches or the inclusion of spatial lags in the number of coronavirus infections to control for the spatial spillovers of coronavirus clusters. However, we do not find any significant effect of turnout on excess mortality up to two months from the elections: this may be due to either the strategic risk-avoidance by fragile voters or the fact that we analyze a period characterized by low levels of infections. In terms of mechanisms we deem unlikely that the contagion effect found by our analysis and linked to the polls' turnout is due to pre-electoral rallies, as the exogenous variation in turnout identified by the type of elections held in Italian municipalities in September 2020 (a constitutional referendum and administrative elections for the new municipality mayor and regional government) does not

[37]€20,153,168,964 as estimated by the ALTEMS research team, based on Diagnoses Related Group (DRG) (https://altems.unicatt.it/altems-Report%2046-compresso.pdf).

seem to explain the pre-poll evolution of the epidemic. In the absence of individual-level, experimental data with records of voters' behavior, actions and choices, we speculate that there are two likely mechanisms at play for the poll-related infection spread: the lack of abidance to NPIs while at the ballots, and the lack of abidance to NPIs after the ballots. Both cases would arise from instances like the incorrect use of masks or the lack of social distancing between people while queuing to vote or post-vote gatherings.

Overall, our study suggests that national-level polls might contribute to the spread of airborne diseases like COVID-19, and thus they can spark national waves of contagion if held during peak periods of an epidemics. These findings are in line with a recent analysis by Palguta et al. (2021), who exploit a similar institutional setting in the Czech Republic to examine the epidemic effects of the second round of the 2020 Senate elections, which were held only in a random subset of all the national constituencies. We provide an estimate of the causal effect of turnout on new COVID-19 infections, which is informative for policy-makers about the public health consequences of holding in-person polls during a pandemic, given an expected turnout rate. This is a subtle but important point, as knowing the impact of holding elections at a given turnout rate rather than not holding them at all provides politicians and public health policy-makers a way to quantify the likely disruption for holding the elections, hence a way to assess whether such elections are better to be postponed. In this regard, and based on our estimates, we provide a cost-benefit calculation of the monetary and lives-saving gains from having averted national-level general elections in Italy in the first months of 2021, following the collapse of the coalition Government in charge till January 2021. Our simulation suggests that the appointment of a government of national unity and the prevention of an early general election might have spared Italy up to €361.751 millions on hospital care costs and €7.538 billions in terms of value of lives lost to COVID. This is possibly the opposite of what happened between March and April 2021 in India, when the country experienced a record surge in COVID-19 infections, hospitalizations and deaths concomitantly with campaign rallies and voting for a series of state and local council elections.

Hence, the cost-benefit figures that we show also represent what James and Alihodzic (2020) define as a "humanitarian case" for postponing elections, given the inevitable trade-off involved by holding in-person elections during a pandemic between the exercise of the democratic right to vote versus the value of individual and public health. Finally, our results, along with those of Picchio and Santolini (2021), provide also evidence that polls held during an epidemic may break one of the five criteria postulated by James and Alihodzic (2020) for deciding whether to hold an election, i.e. the need to guarantee the equality of voters' participation to the polls. Indeed, the first-stage model for turnout of our Control Function strategy shows that such equality was likely affected by a number of municipality characteristics, like population density and the latent health frailty proxied by the excess mortality experienced during the first COVID-19 wave. Whether any of the other four criteria (i.e. full deliberation, equality of contestation, robust electoral management quality and institutional certainty) postulated by James and Alihodzic (2020) was also compromised, during the Italian polls we studied or other in-person ballots held over the global COVID-19 pandemic, is instead an interesting question that is open for future research.

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A Appendix

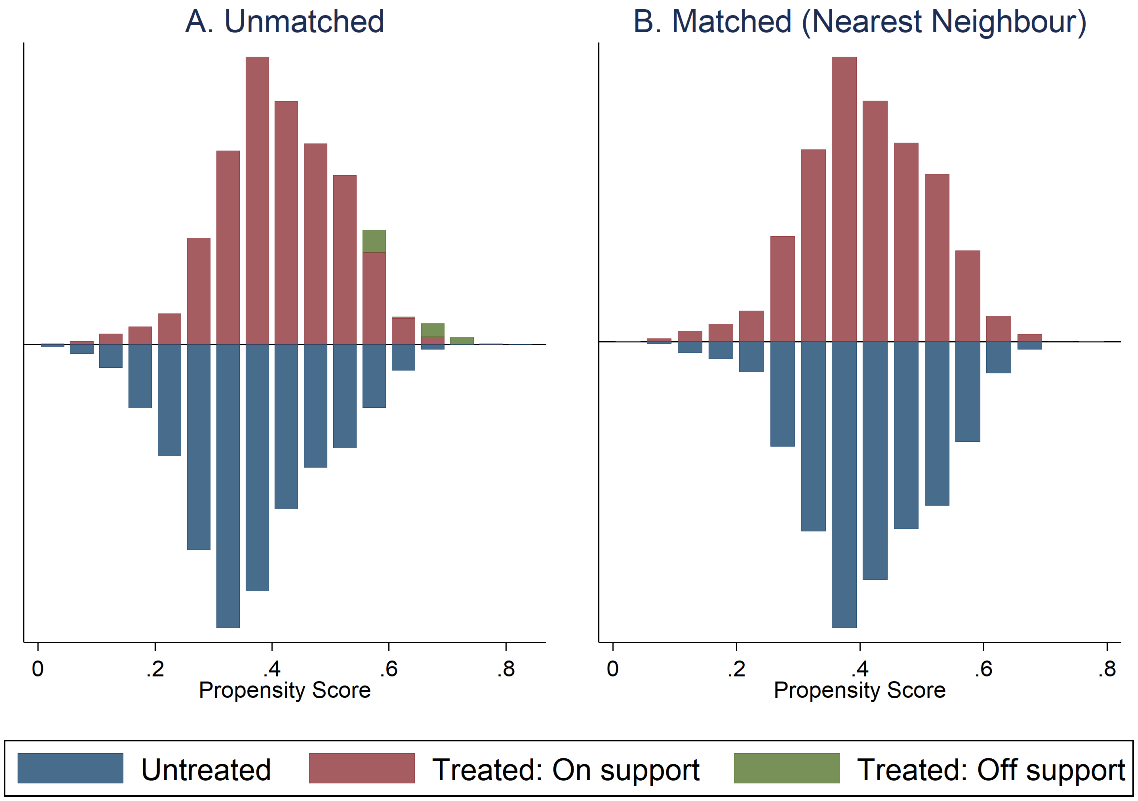

Figure A1: Propensity score distributions

Table A1: Summary statistics in the matched sub-sample (nearest neighbor)

	Treated		Control			
	Mean	Std. Dev.	Mean	Std. Dev.	Δ	t-test
Municipality						
Residents	10078.04	(23557.95)	10779.26	(66798.29)	-701.22	-0.46
Share of Female Residents	0.51	(0.01)	0.51	(0.01)	-0.00	-0.10
Average Age	45.8	(3.02)	45.86	(2.61)	-0.06	-0.68
Population Density	0.37	(0.70)	0.37	(0.64)	0.00	0.04
Average Income (€1000)	19.34	(3.77)	19.17	(4.18)	0.17	1.41
Wave I Excess Mortality	0.69	(1.97)	0.64	(2.26)	0.05	0.82
Schools pca	1.37	(0.82)	1.39	(0.82)	-0.02	-0.91
Turnout	68.47	(7.94)	46.04	(8.57)	22.43	89.94***
APT	58.75	(6.59)	57.37	(7.75)	1.37	6.33***
Weekly Covid Rate						
24/08 - 30/08	16.47	(61.22)	14.53	(47.81)	1.94	1.17
31/08 - 06/09	17.72	(84.60)	16.44	(50.07)	1.29	0.61
07/09 - 13/09	19.2	(49.71)	18.16	(58.85)	1.04	0.63
14/09 - 20/09	23.12	(66.23)	18.88	(68.17)	4.24	2.09**
21/09 - 27/09	23.84	(68.65)	30.16	(157.12)	-6.32	-1.73*
28/09 - 04/10	37.35	(109.62)	39.9	(251.64)	-2.55	-0.44
05/10 - 11/10	72.86	(168.12)	68.32	(275.82)	4.55	0.66
12/10 - 18/10	129.42	(173.00)	128.94	(202.64)	0.48	0.08
Municipality-Week observations	17,560		17,560			
Municipalities	2,195		2,195			

Notes: Covid Rate is defined as the number of new coronavirus cases by 100,000 of residents. Treated municipalities held both the constitutional referendum and either regional or mayoral elections (or both) on September 2020. Control municipalities held only the constitutional referendum on September 2020. APT = Average Past Turnout in the four elections held nationally.

Table A2: Summary statistics in the weighted matched sample (entropy balance)

	Treated			Control		
	Mean	Std. Dev.	Skeweness	Mean	Std. Dev.	Skeweness
Wave I Excess Mortality	0.6446	3.936	1.95	0.6449	3.94	1.952
Coastal Mountain	0.01147	0.01134	9.176	0.01147	0.01134	9.176
Inner Hill	0.2854	0.204	0.9504	0.2854	0.204	0.9501
Coastal Hill	0.1345	0.1165	2.142	0.1346	0.1165	2.142
Flat Land	0.2898	0.2059	0.9266	0.2899	0.2059	0.9263
Small Town	0.4283	0.245	0.2897	0.4283	0.2449	0.2896
Rural	0.5174	0.2498	-0.06974	0.5174	0.2498	-0.06974
Coastal Town	0.206	0.1636	1.454	0.206	0.1636	1.454
Share of Female Residents	0.5064	0.0001562	-1.02	0.5064	0.0001562	-1.02
Average Age	45.69	9.449	0.3439	45.69	9.45	0.3444
Population Density	0.4144	0.7693	6.32	0.4144	0.7693	6.32
Average Income	19.27	14.24	0.4555	19.27	14.24	0.4556
Schools pca	1.365	0.6735	2.506	1.365	0.6735	2.506

Notes: Treated municipalities held both the constitutional referendum and either regional or mayoral elections (or both) on September 2020. Control municipalities held only the constitutional referendum on September 2020.

Table A3: Within municipality difference in turnout rates with respect to past polls

	ΔTurnout: Turnout 2020 Polls - Average Past Turnout (APT)	ΔTurnout: Turnout 2020 Polls - European Elections 2019	ΔTurnout: Turnout 2020 Polls - Political Elections 2018	ΔTurnout: Turnout 2020 Polls - Constitutional Referendum Dec 2016	ΔTurnout: Turnout 2020 Polls - Abrogating Referendum Apr 2016
	(1)	(2)	(3)	(4)	(5)
Treated	31.002***	36.781***	29.213***	28.995***	29.019***
	(0.512)	(0.684)	(0.506)	(0.506)	(0.535)
Wave I Excess Mortality	-0.041*	-0.062	-0.035	-0.030	-0.035
	(0.024)	(0.046)	(0.026)	(0.025)	(0.029)
Coastal Mountain	-0.601	0.216	-0.330	-1.430**	-0.859
	(0.737)	(1.421)	(0.716)	(0.717)	(0.722)
Inner Hill	-0.184	0.046	0.006	-0.509**	-0.280
	(0.213)	(0.433)	(0.214)	(0.217)	(0.233)
Coastal Hill	-0.631	0.328	-0.271	-1.243***	-1.338***
	(0.400)	(0.758)	(0.377)	(0.408)	(0.424)
Flat Land	0.779***	2.059***	1.014***	0.564**	-0.521*
	(0.255)	(0.494)	(0.258)	(0.249)	(0.290)
Small Town	0.410	0.103	0.855**	0.529	0.155
	(0.488)	(0.947)	(0.431)	(0.437)	(0.496)
Rural	1.039**	-0.793	1.198***	1.531***	2.222***
	(0.521)	(1.020)	(0.465)	(0.473)	(0.541)
Coast	-1.589***	-0.455	-1.353***	-1.499***	-3.047***
	(0.323)	(0.622)	(0.299)	(0.303)	(0.335)
Share of Female Residents	-22.359***	-18.424*	-29.421***	-34.795***	-6.796
	(4.662)	(9.493)	(5.609)	(5.043)	(6.568)
Average Age	0.515***	0.595***	0.710***	0.529***	0.226***
	(0.030)	(0.061)	(0.031)	(0.033)	(0.035)
Population Density	-0.319*	-0.073	0.007	-0.247	-0.964***
	(0.172)	(0.320)	(0.161)	(0.176)	(0.194)
Average Income	-0.167***	-0.279***	-0.076**	-0.235***	-0.077**
	(0.032)	(0.059)	(0.033)	(0.030)	(0.034)
Schools pca	-0.129*	-0.373**	-0.201**	-0.145*	0.204**
	(0.074)	(0.147)	(0.078)	(0.079)	(0.087)
Province fixed-effects	Yes	Yes	Yes	Yes	Yes
R^2	0.806	0.631	0.800	0.813	0.805
Municipalities	7,903	7,903	7,903	7,903	7,903

Notes: OLS estimates for the models on excess turnout. Robust standard errors in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table A4: Effects of Turnout on COVID-19 infections with Control Function

	Turnout		New COVID-19 cases			
	(1)		(2)		(3)	
Panel A: 1st stage						
Treated	30.176***	(0.500)				
APT	0.610***	(0.018)				
Wave I Excess Mortality	-0.052**	(0.023)				
Coastal Mountain	-0.719	(0.702)				
Inner Hill	0.243	(0.204)				
Coastal Hill	-0.329	(0.375)				
Flat Land	1.158***	(0.245)				
Coast	-1.928***	(0.308)				
Small Town	0.886*	(0.456)				
Rural	1.747***	(0.488)				
Share of Female Residents	-21.063***	(4.564)				
Average Age	0.393***	(0.030)				
Population Density	-0.400**	(0.169)				
Average Income	0.026	(0.031)				
Schools pca	-0.177**	(0.072)				
Panel B: 2nd Stage Event-Study Design						
3 weeks pre-poll * Turnout			0.001	(0.004)	-0.000	(0.004)
2 weeks pre-poll * Turnout			0.001	(0.004)	0.002	(0.004)
1 week pre-poll * Turnout			0.004	(0.003)	0.004	(0.003)
1 week post-poll * Turnout			0.008***	(0.002)	0.005*	(0.003)
2 weeks post-poll * Turnout			0.012***	(0.004)	0.008**	(0.004)
3 weeks post-poll * Turnout			0.010**	(0.004)	0.008*	(0.004)
4 weeks post-poll * Turnout			0.014***	(0.004)	0.013***	(0.004)
3 weeks pre-poll * APT			0.014*	(0.008)	0.019***	(0.007)
2 weeks pre-poll * APT			0.008	(0.006)	0.006	(0.006)
1 week pre-poll * APT			0.004	(0.004)	0.006	(0.005)
1 week post-poll * APT			-0.021***	(0.004)	-0.012***	(0.004)
2 weeks post-poll * APT			-0.019***	(0.006)	-0.008	(0.006)
3 weeks post-poll * APT			-0.006	(0.005)	-0.002	(0.005)
4 weeks post-poll * APT			0.002	(0.005)	0.007	(0.005)
3 weeks pre-poll * Residuals			0.008	(0.010)	0.011	(0.008)
2 weeks pre-poll * Residuals			0.001	(0.008)	-0.005	(0.009)
1 week pre-poll * Residuals			-0.003	(0.007)	-0.006	(0.007)
1 week post-poll * Residuals			0.006	(0.006)	0.010*	(0.006)
2 weeks post-poll * Residuals			0.017**	(0.009)	0.025***	(0.008)
3 weeks post-poll * Residuals			0.019**	(0.008)	0.019**	(0.008)
4 weeks post-poll * Residuals			0.020**	(0.009)	0.018**	(0.009)
Panel C: 2nd Stage DiD						
Post-poll			0.951***	(0.234)	0.620**	(0.255)
Post-poll * Turnout			0.015***	(0.003)	0.011***	(0.003)
Post-poll * APT			-0.017***	(0.004)	-0.008*	(0.005)
Post-poll * Residuals			0.012*	(0.006)	0.016**	(0.007)
Sample	Unmatched		Unmatched		Matched (NN)	
Treated Municipalities	2,851		2,267		2,195	
Control Municipalities	5,052		3,620		2,195	
Municipality-Week observations	7,903		47,096		35,120	

Notes: First-stage OLS model for Turnout in Column 1. Second-stage Fixed-effects Poisson model for new COVID-19 cases augmented with the first-stage residuals (interacted with the week indicators) in Columns 2 and 3. APT = Average turnout in the four past elections held nationally. List of variables used for matching as in Figure 3. Municipality-level clustered bootstrapped standard errors (1,000 iterations) in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

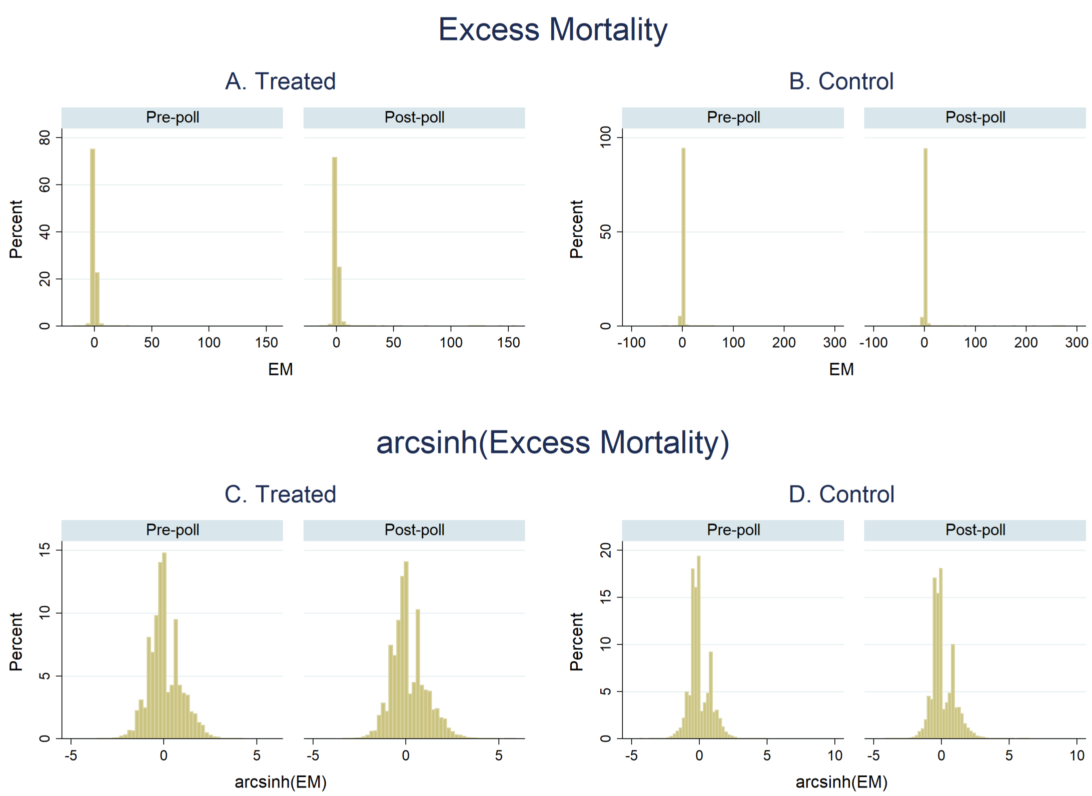

Figure A2: Excess mortality distributions by treatment and post-poll indicators

Table A5: Effects of Turnout on excess mortality: Linear FE model

	arcsinh(EM_{it})	
	(1)	(2)
4 weeks pre-poll * Turnout	0.053	0.137
	(0.088)	(0.112)
3 weeks pre-poll * Turnout	0.066	0.099
	(0.088)	(0.114)
2 weeks pre-poll * Turnout	-0.061	0.009
	(0.087)	(0.110)
1 week pre-poll * Turnout	0.023	0.115
	(0.088)	(0.108)
1 week post-poll * Turnout	-0.062	0.074
	(0.089)	(0.110)
2 weeks post-poll * Turnout	0.079	0.116
	(0.090)	(0.113)
3 weeks post-poll * Turnout	0.064	0.145
	(0.089)	(0.111)
4 weeks post-poll * Turnout	-0.020	-0.020
	(0.089)	(0.111)
5 weeks post-poll * Turnout	-0.151*	0.009
	(0.091)	(0.107)
6 weeks post-poll * Turnout	-0.044	0.003
	(0.089)	(0.110)
7 weeks post-poll * Turnout	-0.085	-0.002
	(0.093)	(0.114)
8 weeks post-poll * Turnout	-0.007	0.085
	(0.093)	(0.114)
Constant	0.133***	0.132***
	(0.011)	(0.012)
Sample	Unmatched	Unmatched
CF	No	Yes

Notes: OLS estimates for the model on excess mortality. Controls included (but not reported): week indicators; population density interacted with week indicators; post October polls week indicators interacted with an indicator for municipalities that had a second ballot or the first ballot of mayoral elections on 4th and 5th October 2020; average past turnout and first-stage residuals interacted with week indicators (only in Column 2). Municipality-level clustered standard errors in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table A6: Robustness checks for censored values

	Best case scenario (1)	Worst case scenario (2)	Randomization (3)
3 weeks pre-poll * Turnout	0.002	0.000	0.001
	(0.005)	(0.003)	(0.004)
2 weeks pre-poll * Turnout	0.002	-0.000	0.001
	(0.004)	(0.003)	(0.004)
1 week pre-poll * Turnout	0.005	0.003	0.004
	(0.003)	(0.002)	(0.003)
1 week post-poll * Turnout	0.010***	0.005***	0.007***
	(0.003)	(0.002)	(0.002)
2 weeks post-poll * Turnout	0.015***	0.010***	0.012***
	(0.004)	(0.003)	(0.003)
3 weeks post-poll * Turnout	0.013***	0.008***	0.010***
	(0.004)	(0.003)	(0.003)
4 weeks post-poll * Turnout	0.018***	0.012***	0.014***
	(0.004)	(0.003)	(0.003)
Sample	Unmatched	Unmatched	Unmatched
Treated Municipalities	2,267	2,267	2,267
Control Municipalities	3,620	3,620	3,620
Municipality-Week observations	47,096	47,096	47,096
CF	No	No	No

Notes: Fixed-effects Poisson semi-elasticities in the full sample. Censored number of COVID-19 infections replaced with 1 in Columns 1 and 2. Censored number of COVID-19 infections replaced with 4 in Columns 3 and 4. Randomized (2,000 replications) censored coronavirus infections in Column 5 and 6. Controls included (but not reported): week indicators; Regional PCR tests performed per 10,000 inhabitants; population density interacted with the week indicators; post October polls indicators interacted with an indicator for municipalities that had a second ballot or the first ballot of mayoral elections on 4th and 5th October 2020. Municipality-level clustered standard errors in parenthesis; average past turnout interacted with week indicators (only in Columns 2, 4 and 6). Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table A7: Robustness checks for number of PCR tests

	New COVID-19 cases		
	(1)	(2)	(3)
3 weeks pre-poll * Turnout	0.001	0.001	0.002
	(0.004)	(0.004)	(0.004)
2 weeks pre-poll * Turnout	0.001	0.001	0.001
	(0.004)	(0.004)	(0.004)
1 week pre-poll * Turnout	0.004	0.004	0.004
	(0.003)	(0.003)	(0.003)
1 week post-poll * Turnout	0.006**	0.006**	0.007***
	(0.002)	(0.003)	(0.003)
2 weeks post-poll * Turnout	0.013***	0.012***	0.012***
	(0.004)	(0.004)	(0.004)
3 weeks post-poll * Turnout	0.009**	0.009**	0.009**
	(0.004)	(0.004)	(0.004)
4 weeks post-poll * Turnout	0.008**	0.008**	0.009**
	(0.004)	(0.004)	(0.004)
Pre-poll PCR		-0.004	
		(0.011)	
Weighted PCR pca			15.641
			(9.667)
Sample	Unmatched	Unmatched	Unmatched
Treated Municipalities	2,267	2,267	2,267
Control Municipalities	3,620	3,620	3,620
Municipality-Week observations	47,096	47,096	47,096
PCR	No	Pre-vote	Weighted
CF	Yes	Yes	Yes

Notes: Fixed-effects Poisson semi-elasticities in the full sample with Control Function. Pre-poll PCR is the average number of Regional PCR tests performed per 10,000 inhabitants in the four weeks preceding the election date. Weighted PCR pca is the weekly number of Regional PCR tests performed per capita, weighted by municipality population density. Controls included (but not reported): week indicators; population density interacted with the week indicators; post October polls indicators interacted with an indicator for municipalities that had a second ballot or the first ballot of mayoral elections on 4th and 5th October 2020; average past turnout and first-stage residuals interacted with week indicators. Municipality-level clustered standard errors in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table A8: Robustness checks for the number of schools

	New COVID-19 cases	
	(1)	(2)
3 weeks pre-poll * Turnout	-0.002	0.001
	(0.004)	(0.004)
2 weeks pre-poll * Turnout	-0.002	0.001
	(0.004)	(0.004)
1 week pre-poll * Turnout	0.001	0.004
	(0.002)	(0.003)
1 week post-poll * Turnout	0.007***	0.008***
	(0.003)	(0.002)
2 weeks post-poll * Turnout	0.010***	0.012***
	(0.003)	(0.004)
3 weeks post-poll * Turnout	0.006*	0.010**
	(0.003)	(0.004)
4 weeks post-poll * Turnout	0.009***	0.014***
	(0.003)	(0.004)
3 weeks pre-poll * Schools	-0.000	0.014
	(0.000)	(0.074)
2 weeks pre-poll * Schools	-0.000	-0.036
	(0.000)	(0.075)
1 week pre-poll * Schools	-0.000	0.035
	(0.000)	(0.061)
1 week post-poll * Schools	-0.000	0.094*
	(0.000)	(0.049)
2 weeks post-poll * Schools	-0.000	0.137**
	(0.000)	(0.063)
3 weeks post-poll * Schools	-0.000	0.093
	(0.000)	(0.069)
4 weeks post-poll * Schools	-0.000	0.047
	(0.000)	(0.069)
Sample	Unmatched	Unmatched
Treated Municipalities	2,267	2,267
Control Municipalities	3,620	3,620
Municipality-Week observations	47,096	47,096
Schools	Number of Schools	Number of Schools per 1,000 inhabitants
CF	Yes	Yes

Notes: Fixed-effects Poisson semi-elasticities in the full sample with Control Function. Controls included (but not reported): week indicators; Regional number of PCR tests performed per 10,000 inhabitants; population density interacted with week indicators; post October polls week indicators interacted with an indicator for municipalities that had a second ballot or the first ballot of mayoral elections on 4th and 5th October 2020; average past turnout and first-stage residuals interacted with week indicators. Municipality-level clustered standard errors in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table A9: Fully-interacted Control Function

	New COVID-19 cases	
	(1)	
Panel B: 2nd Stage Event-Study Design		
3 weeks pre-poll * Turnout	0.003	(0.004)
2 weeks pre-poll * Turnout	0.002	(0.004)
1 week pre-poll * Turnout	0.004	(0.003)
1 week post-poll * Turnout	0.005**	(0.003)
2 weeks post-poll * Turnout	0.009**	(0.004)
3 weeks post-poll * Turnout	0.007*	(0.003)
4 weeks post-poll * Turnout	0.010***	(0.003)
3 weeks pre-poll * APT	-0.001	(0.009)
2 weeks pre-poll * APT	0.001	(0.008)
1 week pre-poll * APT	0.009	(0.006)
1 week post-poll * APT	-0.010*	(0.006)
2 weeks post-poll * APT	-0.010	(0.008)
3 weeks post-poll * APT	-0.004	(0.007)
4 weeks post-poll * APT	-0.000	(0.007)
3 weeks pre-poll * Residuals	0.008	(0.008)
2 weeks pre-poll * Residuals	0.001	(0.008)
1 week pre-poll * Residuals	-0.001	(0.006)
1 week post-poll * Residuals	0.009*	(0.005)
2 weeks post-poll * Residuals	0.022***	(0.007)
3 weeks post-poll * Residuals	0.023***	(0.007)
4 weeks post-poll * Residuals	0.026***	(0.007)
Panel C: 2nd Stage DiD		
Post-poll	-2.815**	(1.334)
Post-poll * Turnout	0.010***	(0.002)
Post-poll * APT	-0.002	(0.005)
Post-poll * Residuals	0.018***	(0.005)
Sample	Unmatched	
Treated Municipalities	2,267	
Control Municipalities	3,620	
Municipality-Week observations	47,096	

Notes: Fixed-effects Poisson semi-elasticities in the full sample with a fully-interacted Control Function specification. Event study design in Panel A, Difference-in-difference model in Panel B. APT = Average turnout in the four past elections held nationally. Bootstrapped standard errors (1,000 iterations) clustered at the municipality level in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table A10: Value of lives at risk due to COVID, by age categories.

Age Group (year)	**Mid-Point of Class Interval (A1)**	**Potential Years of Life Lost (PYYL) (B1)**	**COVID-19 Case rate (C1)**	**COVID-19 Death rate (D1)**	**COVID-19 Age specific Mortality Risk (E1)**	**Expected Monetary Value of Years of Life at Risk (F1)**
0–9	4.5	76.5	5.50%	0.00%	0.00%	€ -
10–19	14.5	66.5	9.60%	0.00%	0.00%	€ -
20–29	24.5	56.5	11.80%	0.00%	0.00%	€ -
30–39	34.5	46.5	12.50%	0.00%	0.00%	€ -
40–49	44.5	36.5	16.10%	0.20%	0.00%	€ 29,431.71
50–59	54.5	27	17.40%	0.60%	0.10%	€ 70,588.03
60–69	64.5	15.8	11.00%	2.70%	0.30%	€ 117,511.49
70–79	74.5	6	8.00%	9.30%	0.70%	€ 111,786.92
80–89	84.5	-	6.00%	20.00%	1.20%	-
90+	94.5	-	2.10%	27.80%	0.60%	-
Total			100%		3%	€ 329,318.15

Notes. (B1) PYYL computation for ages up to 60-69 category: 75 years - mid-point of class interval + 5 years * 0.8 + 4 years * 0.5; PYYL computation for age 70-79 category: 5 years * 0.8 + 4 years * 0.5; PYYL computation for ages above 80-89 category are set to zero. (C1) Source: `https://www.statista.com/statistics/1103023/coronavirus-cases-distribution-by-age-group-italy/`. (D1) Source: `https://www.statista.com/statistics/1106372/coronavirus-death-rate-by-age-group-italy/`. Cells in (E1) = (C1)*(D1). Cells in (F1) = €$74,159 * (B1) * (E1)/3\%$.