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# ABSTRACT

## Voting, Contagion and the Trade-Off between Public Health and Political Rights: Quasi-Experimental Evidence from the Italian 2020 Polls*

We exploit a quasi-experimental setting provided by an election day with multiple polls to estimate the effect of voters' turnout on the spread of new COVID-19 infections and to quantify the policy trade-off implied by postponing elections during high infection periods. We show that post-poll new COVID cases increased by 1.1% for each additional percentage point of turnout. The cost-benefits analysis based on our estimates and real political events shows that averting an early general election has saved Italy up to about €362 million in additional hospital care costs and €7.5 billion in values of life saved from COVID.

**JEL Classification:** C23, D72, H51, I18

**Keywords:** COVID-19, voting, civic capital, public health, event-study, endogeneity, control function

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# 1 Introduction

The COVID-19 outbreak has caused important changes and disruptions in the day-by-day routine of billions of people around the world, with deep impacts both on individuals (Fetzer et al., 2020; Proto and Quintana-Domeque, 2021; Giuntella et al., 2021) and businesses (Chetty et al., 2020; Balduzzi et al., 2020; Adams-Prassl et al., 2020). In most of the affected countries, governments had to impose profound societal and organizational changes, including a widespread use of non-pharmaceutical interventions, to minimize the impact of the contagion on the physical and economic lives of citizens (Singh et al., 2021). Among some of the most common, yet remarkable changes to their pre-epidemic living standards, the citizens of the affected countries have had to adapt to new routines like working-from-home, remote teaching, home-schooling and online shopping for groceries and other goods; large decreases of work and leisure travels; repeated cancellations and postponements of mass gathering events like indoor music concerts and festivals, crowd support to sport teams, religious ceremonies. The common thread in the limitation or suspension of the above activities and gatherings is motivated by their pro-social, interactive nature, and the risks posed by such interactions due to the infectious nature of the SARS-COV-2 virus and its variants: in other words, human interactions, and especially mass gatherings events, have the potential to multiply exponentially the spread of infections when social distances cannot be maintained (Memish et al., 2019).

Politicians and healthcare policy-makers are already faced with hard times to communicate and impose restrictions to civil rights and freedoms in most of the aforementioned circumstances, but they might be faced with an even tougher policy dilemma in the case of mass gatherings (McCloskey et al., 2020) like official voting polls, e.g. elections and referenda. Official voting events are mass 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. However, 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 elections as previously scheduled (Institute for Democracy and Electoral Assistance (IDEA), 2021). In fact, holding official polls during an epidemic requires politicians to deal with a typical economic trade-off: preserving the spirit of democratic institutions in the long run, but exposing the lives of the citizens to the likely contagion, and their political careers to a premature oblivion, should the voting mass 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. The extent of this trade-off is likely to be very heterogeneous depending on a given country's culture and widespread common beliefs, including but not limited to the average value of a statistical life in said country.

Furthermore and most importantly, historical evidence has shown that there are many obstacles, and so few alternatives, to replacing physical voting with electronic and/or postal voting for a general election. Electronic voting has been trialled in several developed and developing countries, but unlike in the US such attempts have often had scarce success, leading to abandoning Internet voting, for the most disparate reasons such as unconstitutionality concerns (e.g. Germany) and negative evaluations due to either risks outweighing the benefits or cybersecurity concerns (e.g. Canada, France, Finland).[1] In other countries, like Italy, the introduction of electronic and postal voting might be problematic due to past histories of authoritarian regimes and/or the presence of criminal organizations like "mafia" that could interfere with the polls, raising concerns about the secrecy and independence of voters' choices. Unless all these issues and concerns were to be solved, it is unlikely that physical voting could be easily and completely replaced in most countries by either postal or internet voting, which are safer during epidemics as they prevent the occurrence of mass gatherings (although this is not even true in case of local pre-electoral rallies).

As such, gathering quantitative evidence on the likely short-term contagion risk borne by holding official polls is paramount for politicians and healthcare policy-makers in order to evaluate the best course of action to adopt when official polls are scheduled. Up to now, the risks of holding polls/elections during a pandemic are still unclear and have never been measured. The lack of much empirical evidence on this research question so far is likely due to the fact that country-level epidemics, or pandemics like the COVID 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 local voters whether to go or not to the polls is most likely endogenous to the stage of the epidemic in the geographic location where they reside and they are going to vote. Such issues put a serious threat either to the possibility to conduct an empirical investigation at all, or to be able 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 regions, citizens were called to cast ballots for a constitutional referendum aimed at reducing the number of Parliament members; in 7 out of the 20 Italian administrative regions, they were called to cast ballots for electing the new regional governments and the regional assembly

[1] https://en.wikipedia.org/wiki/Electronic_voting_by_country.

representatives; finally, in 955 of the 7,903 Italian municipalities, citizens voted even for appointing the new municipality mayor. Such institutional setting resulted in an exogenous increase in the turnout rate for the constitutional referendum, by almost 22% on average, in municipalities where an administrative poll (i.e. regional or municipality elections) 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 given by the end of the regional and mayoral governments in a large number of municipalities across the whole Italy, and so it is independent on the local epidemic status. Our event-study regression models include municipality 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 municipalities' pre-COVID and/or pre-polls characteristics (e.g. population density, number of schools per capita, residents' average age), to reduce the bias from observables. Furthermore, we tease out the contribution of civic capital to the spread of the new COVID-19 infections at municipality level, because this unobservable is cross-sectionally correlated with the turnout, as shown by our analysis, as well as the 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 analysis 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 results show that post-poll new COVID cases increased by 1.1% for each additional percentage point of turnout rate for the constitutional referendum. The magnitude and significance level 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 the national-level polls have indeed the possibility to increase the spread of airborne diseases like COVID, thus potentially triggering national-level waves of contagion when polls are held during peak periods of an epidemic, and are informative for politicians, healthcare policy-makers and the general public regarding the public health threats posed by voting during a pandemic, and other mass gathering events that are similar in nature.

To further illustrate the relevance of our results, our cost-benefits analysis, based on rather conservative assumptions, shows 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 is equivalent to a value of €7.538 billion of lives saved from COVID.

With this work, we contribute to the literature of political economics by shedding light to this important public health issue that relates to mass gatherings, voting, spread of infections diseases and the possible governments interventions to balance the trade-offs 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 a partial renewal of the Czech Parliament led to an increase in the number of COVID-19 infections and hospitalizations where the ballots took place.[2] Our study is complementary, yet it differs from Palguta et al. (2021) in a number of ways: we analyze the effect of voters' turnout in Italy, a country with a large population and with one of the highest COVID-19 death tolls and infection rates during the first epidemic wave; we analyze the effect of voters' turnout as measure of treatment intensity, not just treatment assignment; we control for the confounding effects at demographic and geographic level, in particular population density and civic capital; we implement a Control Function strategy to cope with selectivity into voting based on unobservable health gains at municipality level; we account for the possibility of spatial correlations across municipalities in the spread of COVID-19; finally, we provide a cost-benefit analysis, based on real events, that allows to understand the health versus political rights trade-off faced by policy-makers to decide whether to hold large scale voting events when the epidemic situation is deteriorating and not under control. Aside from methodological issues, we argue that focusing on the impact of turnout is more relevant for policy-makers than focusing just on the choice whether to hold polls or not. This because the spread of the new infections is a function of the "mass gathering intensity" provided by the voters' turnout, not just by holding in-person elections. Focusing on the turnout intensity

[2] We also notice that a more descriptive analysis in a similar spirit has also been recently conducted for the effect of State-level elections in India (https://thewire.in/politics/election-rally-covid-19-case-spike).

also allows policy-makers to elaborate cost-benefits analyses based on realistic scenarios of an expected turnout at the polls, that may guide them in the heavy decision whether to keep or postpone the polls during an epidemic.

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

# 2 Background and Data

## 2.1 Related literature

Our work is related to a range of contributions in the fields of economics, public health medicine, politics, and interdisciplinary COVID-related research in general. In particular, our study is mostly linked with previous research that analyzes the determinants and impacts of the spread of infectious diseases like COVID-19 on population health outcomes and social, political and economic activities.

The closest study to ours is a very 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 peak 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%. Palguta et al. (2021) show that the infection spread acceleration slowed down since the fourth week after the elections, hence the Czech elections produced a one-time increase in the growth rate of COVID-19 cases, which afterwards returned to grow at the national rate although starting from a higher base level.

Our findings are complementary to those of Palguta et al. (2021), for several reasons. They compare COVID-19 growth rates between voting and non-voting geographical authorities, while we provide a measure of the effect of the turnout rate on the increase of new COVID-19 infections. Both study identify an ATT, although our study provides also an estimate of a Local ATT when we instrument the turnout rate through the occurrence of local administrative elections. The findings from both works are quite comparable in terms of magnitude, since we find that new COVID-19 infections were about 1.1% higher for each additional percentage point of turnout (the 95% confidence interval for the Difference-in-Differences point estimate is [0.8%,1.4%]), which implied a differential increase in new COVID cases by about 23.85% between municipalities holding ballots only for the referendum and municipalities holding also mayoral or regional administrative elections. Moreover, our work does not find any significant effect of holding elections on mortality, which is consistent with the risk-avoidance by older voters documented in Palguta et al. (2021).

However, our work presents an original and distinctive contribution with respect to the analysis by Palguta et al. (2021) in a number of ways: we control for the effects of population and schooling density as possible confounders; 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; finally, we use an event-study design employing a control function strategy, as we are concerned with the endogeneity of the turnout rate due to self-selectivity of voters based on the individual unobservable trade-off between the expected gain from voting and the risk to contract the virus.

Other existing studies report mostly associations, as they lack a source of exogenous variation to identify the causal effect of holdings elections on COVID-19 spread. For instance, Feltham et al. (2020) examines the pandemic evolution following the 2020 Democratic primary elections in the USA by pre-processing the set of US counties through a matching procedure. This approach, however, ignores the influence that unobservable socio-economic characteristics may have on both the turnout rate (Blais, 2006; Geys, 2006) and COVID-19 prevalence (Stojkoski et al., 2020; Hawkins et al., 2020). Leung et al. (2020) and Berry et al. (2020) focus only on the April 2020 Wisconsin primary elections, which were held as scheduled despite the healthcare concerns leading other US states to postpone the elections or to switch to email voting. In both cases, the authors do not find a significant contribution of the Democratic primary elections on the spread of COVID-19.[3]

Bertoli et al. (2020) attempt to overcome the lack of an exogenous variation 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. This result is somehow confirmed by Cassan and Sangnier (2020), who find a positive effect of the French municipal elections on hospitalizations, but is contradicted by the analyses by Duchemin

[3]Cotti et al. (2021), instead, finds a positive and significant impact of the 2020 Wisconsin primary elections on COVID-19 positive test rate in the second and third week following the election date.

et al. (2020) and Zeitoun et al. (2020).[4]

Another fundamental reference literature for our work is the one studying the relationship between mass gatherings and infectious diseases with a focus of mass gatherings medicine, an area of growing research interest in the last fifteen years. A mass gathering is defined by the World Health Organization (WHO) as "a planned or spontaneous event where the number of people attending could strain the planning and response resources of the community or country hosting the event" (World Health Organization (WHO), 2016). Summarizing the evidence gathered by other studies, Memish et al. (2019) report that mass gatherings events at the Kumbh Mela pilgrimage festival in India and the Hajj pilgrimage in Saudi Arabia respectively increases by 5% the incidence of diarrhoeal diseases and the developing of respiratory tract infections due to different viral strands in the pilgrims. With regards to the COVID pandemic, Ahammer et al. (2020) show that one additional sport mass gathering event increased COVID-related deaths by 11% during the COVID first wave in the US, and Parshakov (2021) documents a 0.15%-to-0.48% increase in the number of COVID-19 cases associated to a 1% growth in soccer matches attendances in Belarus.

Population density is also another important factor for the spread of the COVID-19 contagion and intensity of its effects. Bhadra et al. (2021) show that population density in India is significantly correlated with both new infections ($\rho = 0.49$) and mortality ($\rho = 0.59$). For the US, Gerritse (2020) shows that a one log point increase in population density yields about a 0.06 points higher transmission rate at the onset of the epidemic, while Sy et al. (2021) find that an increase in one unit of log population density increased the $R_0$ transmission rate by 0.16. Since Italian municipalities exhibit large variations in population density, we control for this factor in our analysis.

The relevance of the role of school attendance in the spread of COVID-19 is at the heart of a ongoing heated debate, showing mixed evidence: Isphording et al. (2021), for example, investigate this research question finding that school openings in Germany after the Summer 2020 have not increased the infection rates; however, Auger et al. (2020) show that school closures in US were associated with a decline in both COVID incidence (-62%) and mortality (-58%) during the first wave, while for Italy Amodio et al. (2021) find that school openings are associated with the increase of COVID spread during the second wave. In many Italian

[4] There is also mixed evidence about the effects of the pre-electoral rallies, which have been studied in the US context and particularly in relation to the Trump electoral campaign (Bernheim et al., 2020; Dave et al., 2021).

regions, schools officially re-opened exactly the day after the last day of the September 2020 polls. To account for the possible impact of school openings on the COVID-19 contagion spread in Italy, and its possible compositional effects, in our analysis we present robustness checks either controlling for the number of schools per capita in each municipalities, or including the number of schools per capita in the computation of the propensity score or entropy balance weights.

There are also studies in the social sciences that have investigated the relationship between calamities and voting, although such studies have mainly focused on the effects of natural disasters on voting, and not on our research question of interest, i.e. the effect of voting as a mass gathering on the spread of an infection disease (e.g. COVID-19). Sinclair et al. (2011) report that flooding due to Hurrican Katrina affected the turnout for the 2006 mayoral election in New Orleans by decreasing overall participation, although such decrease was U-shaped as voters who experienced more than 6 ft of flooding were more likely to participate in the election than those experiencing lower flooding levels; this is suggestive of a complex relationship between participation and the costs and benefits of turnout. Picchio and Santolini (2021) investigate how mortality during the COVID first wave affected turnout for municipalities elections that were held alongside the national referendum and the regional government election in Fall 2020 in Italy; they find that a 1 percentage point increase in the elderly mortality rate decreased the voter turnout by 0.5 percentage points, with a stronger effect in more densely populated municipalities. The results of both the aforementioned works reinforce our concerns of endogeneity due to reverse causality and self-selection into voting linked to the local stage of the COVID-19 epidemic during the Fall 2020 polls in Italy, but doing so they also implicitly validate our empirical strategy exploiting the exogenous variation in turnout to prevent (or limit) these sources of endogeniety bias. Indeed, a similar institutional framework has been exploited by Basu (2021) to show with a descriptive analysis how political rallies in India might have triggered the recent surge in COVID-19 cases linked to the diffusion of the COVID-19 Delta variant (strain B.1.617.2).

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 earthquakes or pandemics 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 the Conclusions of this study (see Section 7).

## 2.2 Institutional framework

Italy is organized in 20 regions (NUTS-2 level), whose governors 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.[5]

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 governor 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).[6] 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.[7] 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. Also, all the above polls have a very similar 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 governor 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 governor and/or the new municipality mayor. The political

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

[6] 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.

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

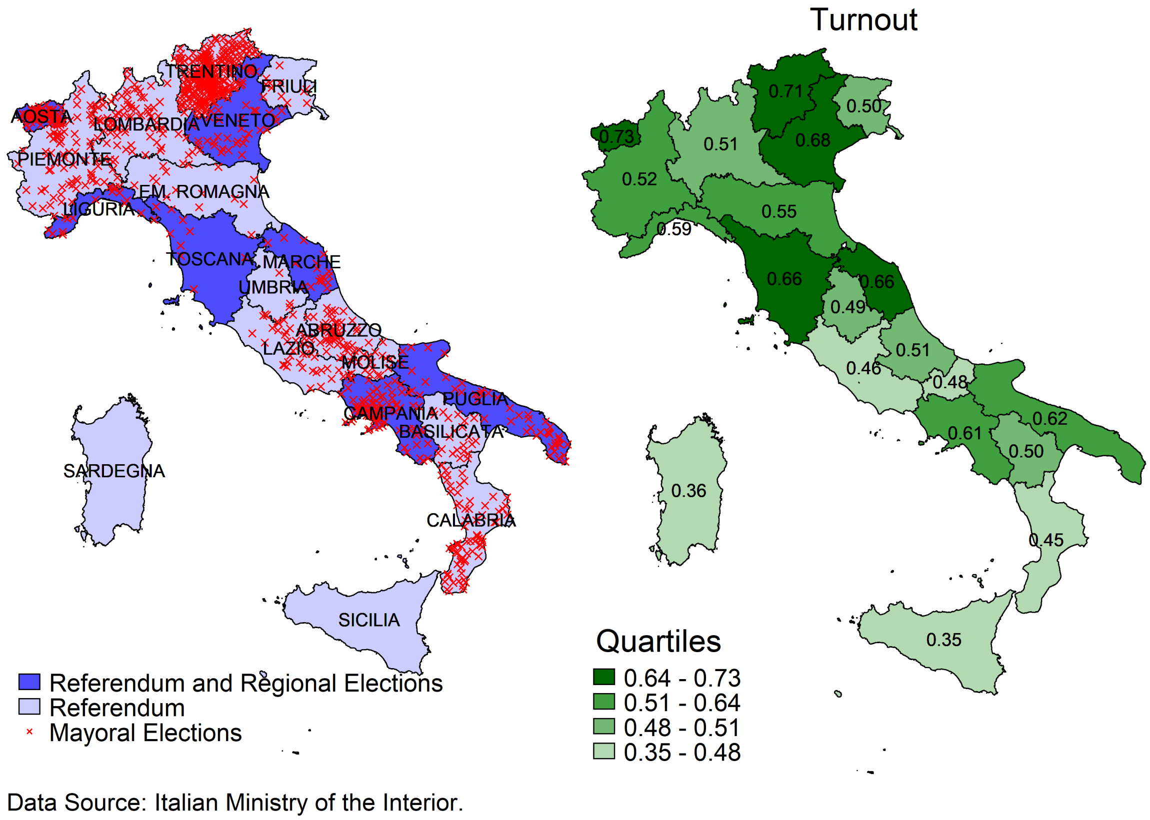


Figure 1: Regional turnout rates for the constitutional referendum

nature of administrative elections certainly led to additional ballots for the referendum that might have not been cast otherwise, also because its object enjoyed a wide consensus among most political parties and the general public.[8] 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 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.

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

## 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 commencing in 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 $[1, 4]$.[9]

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[10]. From the same source, we also collect the municipality-level turnout rates for the previous four elections held nationally[11], which we used in Section 4.2 to estimate a model based on the differences in the historical turnout rates. The 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).[12]

To control for the number of schools that are present in every Italian municipality, we use data collected by the Ministry of Education [13]

To perform the pre-processing of the municiplaities sample with either nearest neighbor or entropy matching balancing we gather 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,

[9]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.

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

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

[12]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 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'

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

urbanicity and proximity to the coast.[14] Using ISTAT data, we also construct a measure of excess mortality at the municipality level (see Section 4.6 for more details) during the first COVID-19 wave (from March to June 2020), that we use as a covariate in the matching and a stratification variable in the heterogeneity analysis.

Finally, we gather 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[15].

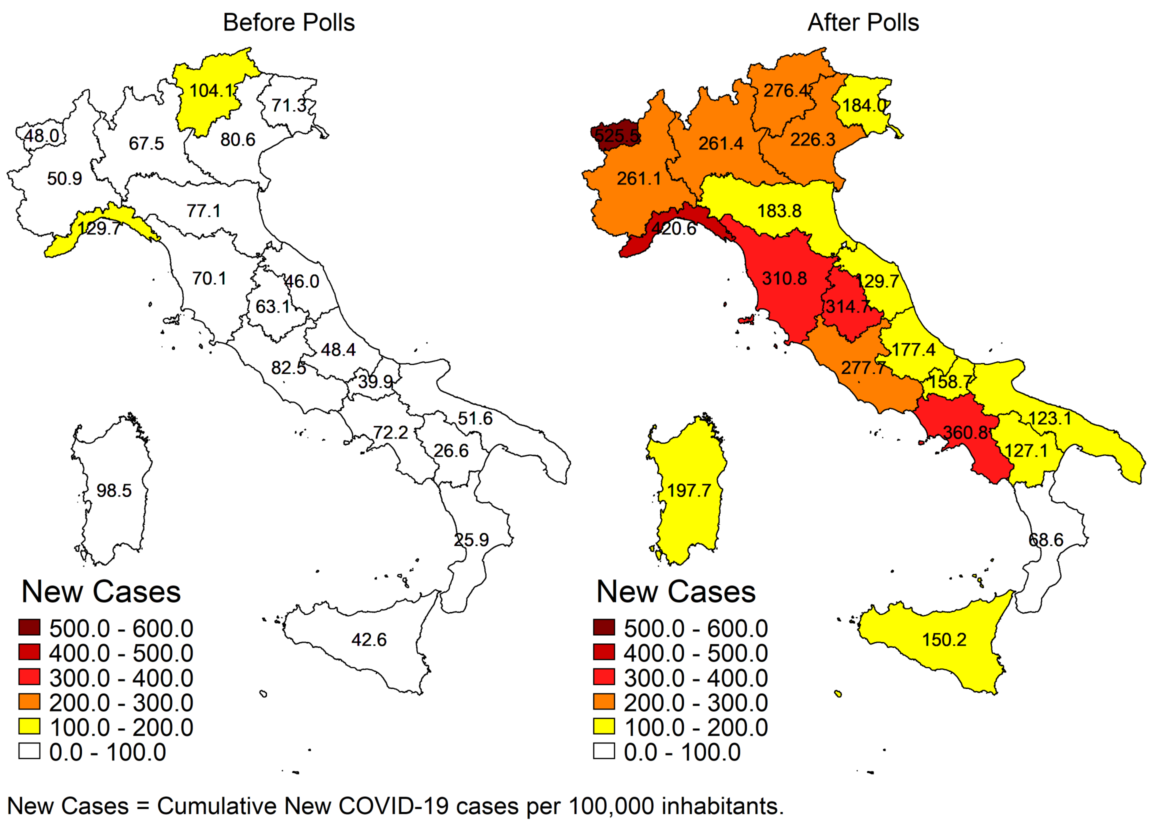


Figure 2: Regional COVID-19 rates around the election date

## 2.4 Descriptive statistics

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 2 plots the incidence rates of COVID-19 in the four weeks preceding

[14]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.

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

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.

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*** |
| *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.

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 the control municipalities. Usually, they also have an higher share of female residents and a younger population. The first group of municipalities presents, on average, a higher population density and a slightly greater number of schools per capita. Finally, treated municipalities were hit less by the first wave of COVID-19 during 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.

# 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, $Turnout_i$:

$$\mathbb{E}(NC_{irt}|\mathbf{X_{irt}}) = exp\left\{\alpha_0 + \alpha_1 PCR_{rt} + \mu_i + \sum_{t'\neq t_0} \beta_t \mathbb{1}(t=t') + \sum_{t'\neq t_0} \gamma_t Turnout_i \mathbb{1}(t=t') + \sum_{t'\neq t_0} \delta_t PD_i \mathbb{1}(t=t') + \sum_{t'>t_2} \zeta_t OCT_i \mathbb{1}(t=t')\right\} \tag{1}$$

where: $i$ denotes the municipality; $t$ denotes the week, going from 3 weeks before to 4 weeks after the week of the polls[16], denoted by $t_0$ and used as reference category; $NC_{it}$ is the number of new COVID-19 infections in municipality $i$ and week $t$.

The vector $\mathbf{X_{irt}} \equiv [Turnout_i; \mu_i; PCR_{rt}; PD_i; \mathbb{1}_t; OCT_i]$ includes the event-study variables of interest, i.e. the interaction of the referendum $Turnout_i$ of municipality $i$ with weekly pre and post poll indicators, alongside other confounders that we describe below. $PCR_{rt}$ corresponds to the total number of PCR tests performed per 10,000 inhabitants in region $r$ and week $t$. $\mu_i$ 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 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); since $PD_i$ is measured in January 2020, i.e. prior to the COVID-19 outbreak and the September 2020 election day, it is by definition an exogenous variable in our model, and thus it may be considered as a secondary effect of

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

interest in our analysis. $OCT_{ir}$ 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 the last two week indicators, it controls 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: the spread of viruses like COVID-19 is characterized by an exponential growth; the count nature of the dependent variable, with the presence of many zero-valued observations; the fact that the Poisson QMLE is a consistent estimator for our parameters of interest (Gourieroux et al., 1984).

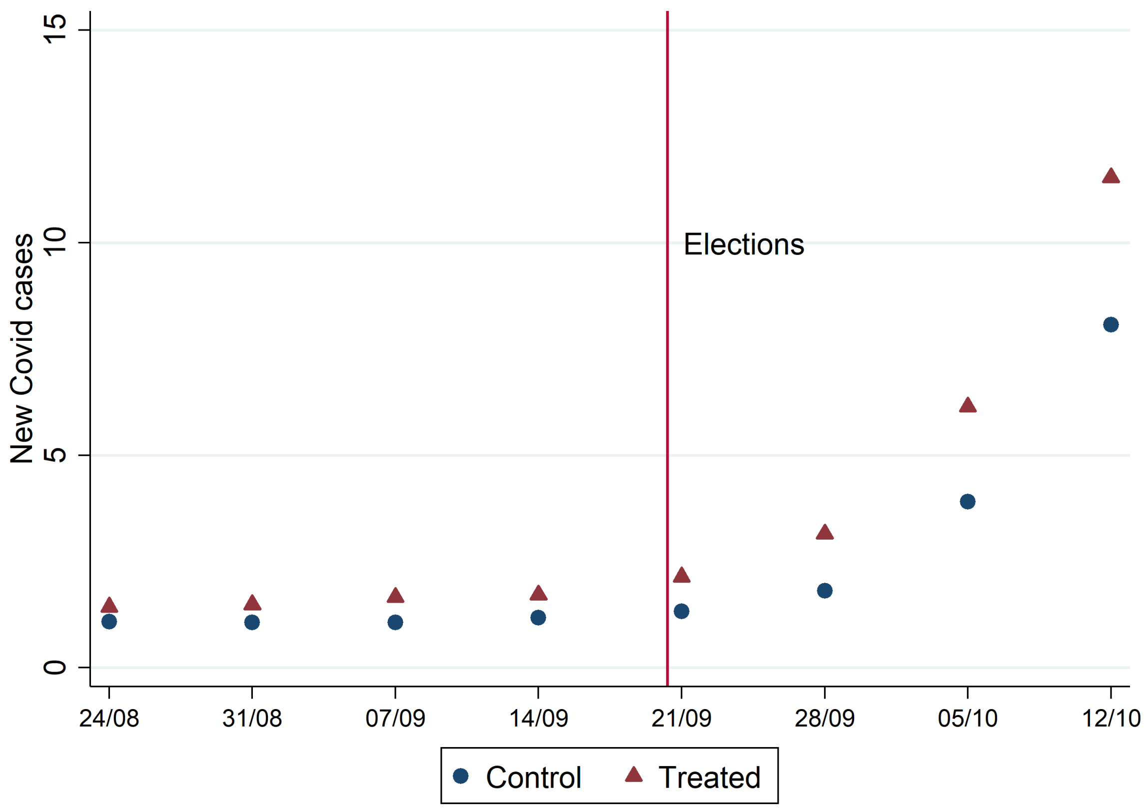


Figure 3: Before polls trends in new COVID-19 cases.

The main object of interest is the event-study vector of coefficients $\gamma_t$. For $t > t_0$, the coefficients quantify the effect of one additional point of turnout rate on the post-poll rise in coronavirus infections. The $\gamma_t$ coefficients have a causal interpretation as Average Treatment Effect on the Treated (ATT) provided that the variations in the treatment intensity variable, $Turnout_i$, are exogenous with respect to the outcome variable. In our case, most of the variation in the turnout variable $Turnout_i$ is driven by the difference in the number of polls occurring between the "treated" and the "control" municipalities. Moreover, as shown by Figure 3, which compares the growth rate of new weekly COVID-19 infections, the two groups of municipalities 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 the "treated" municipalities, which were characterized on average by higher turnout rates as a result of the institutional setting outlined in Section 2.2.

All the fixed-effects Poisson models are estimated by pseudo-maximum likelihood (Gourieroux et al., 1984) and with standard errors that are clustered at the municipality level (Wooldridge, 1999, 2015b).[17]

## 3.2 Matching and bias from observables

The previous models assume that, by controlling for municipality and week fixed effects, the evolution of the COVID-19 outbreak as a function of the turnout rate can be comparable over time across municipalities. However, we also show in Table 1 that the groups of treated and control municipalities differ substantially not only in the turnout rate for the constitutional referendum, as we would have expected given the additional incentive to vote for the new municipality and regional governments, but also in some predetermined characteristics. A legitimate concern is whether the concentration of such predetermined characteristics may contribute to explain the post-polls heterogeneous increase in coronavirus infections. 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.

Although this potential issue should be alleviated by the inclusion of municipality fixed effects, we also estimate models as in 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). This 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 aim to analyze a set of municipalities with comparable demographics, which are known to play an important role in explaining both the turnout rate (Gallego, 2009; Bhatti

[17] 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.

et al., 2012) and the severity of COVID-19 symptoms (Bhopal and Bhopal, 2020; Jin et al., 2020). Similarly, we wish to select municipalities that share the same geographical and urban characteristics, which are factors that can significantly affect COVID-19 transmission (see for instance Gupta et al. 2020; Ahmadi et al. 2020).

For these reasons, we perform a logit regression with an indicator for treated municipalities as the 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 the independent variables. All the above are either exogenous demographic or geographical characteristics that were measured prior to the start of the COVID pandemic, or, in case of the excess mortality during the first COVID-19 wave, a predetermined variable that is plausibly exogenous to the spread of the virus after the end of the first COVID wave, when the September 2020 elections took place.

Based on such logit regressions, we obtain estimates of the propensity score for each municipality and 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).[18] 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 also 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.[19] A similar conclusion can be drawn from Figure 4, 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

[18]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 due to the inclusion of the municipality and week fixed effects.

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

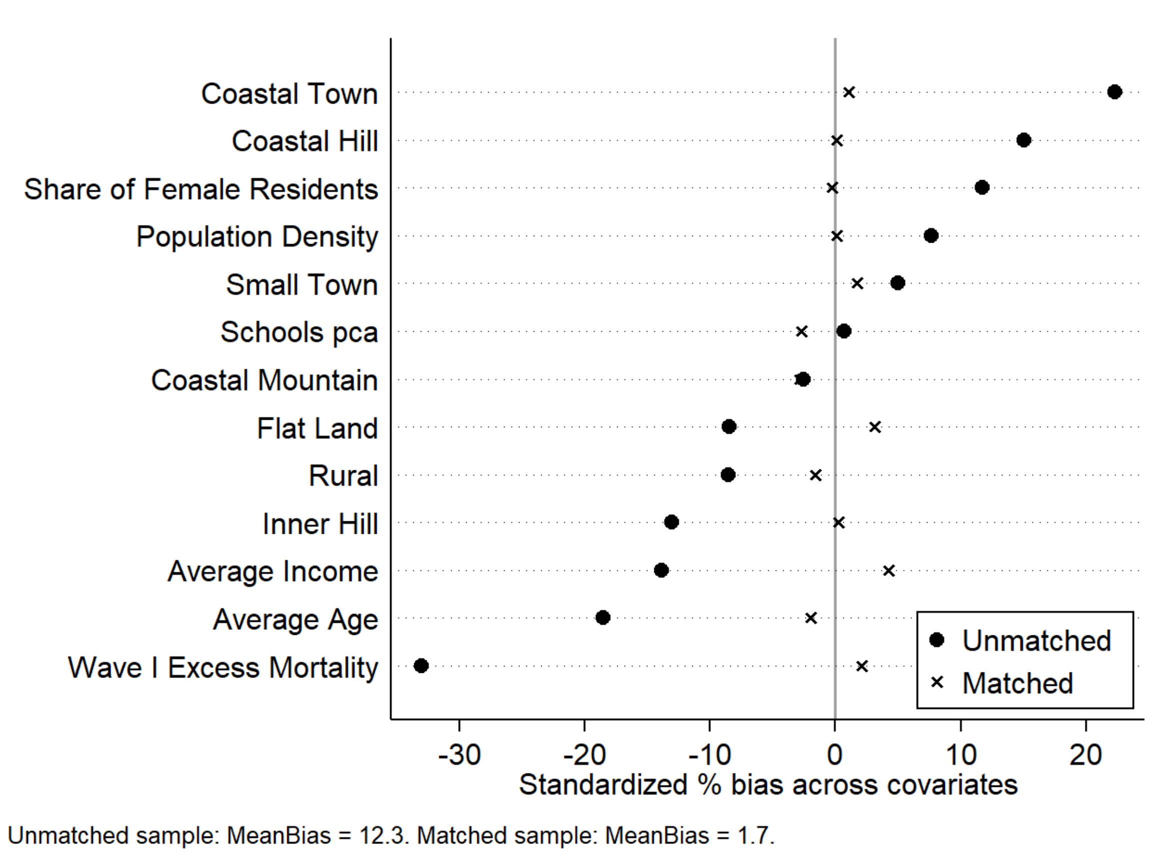


Figure 4: Covariate bias reduction after matching.

control group from 12.3% to 1.7%.

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 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 analysis in Equation 1 is then replicated using the full sample, but with weights produced by the entropy balance approach.

### 3.3 Accounting for the bias from time-invariant cross-sectional confounders.

Equation 1 and its modifications based on matching estimate the causal effect of interest provided that the variation underlying the $Turnout_i$ treatment variable is exogenous and that the bias from other observables is removed, either through the matching, or through the inclusion of either standalone covariates like $PCR_{rt}$ or of interactions with week dummies for the population density variable, $PD_i$.

Similarly to the case of population density, there might be other time-invariant factors at municipality-level that cannot be fully captured by the municipality fixed effects if they have a time-varying impact on the spread of COVID-19 and such effect is potentially correlated with our effect of interest. In our analysis, the unobservable factor with the highest potential to bias the estimates of interest is *civic capital* at municipality level, as other studies (Durante et al., 2021; Barrios et al., 2021) have highlighted the key role that civic capital has played in observing social distancing measures and the spread of COVID during the first wave in the US and Europe.

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), as long as they are stable and unaffected by other institutional factors forcing or constraining the local degree of cooperation among citizens. We follow Putnam et al. (1994) and use voters' turnouts in the previous four national-level polls (two referenda, one general election for the Italian Parliament and one general election for the members of the European Parliament) 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 at Italian provincial level and that we do not have access to.

A first way to control for the bias from unobservable civic capital can be achieved with the following specification:

$$\mathbb{E}(NC_{irt}|\mathbf{X_{irt}}) = exp\left\{\alpha_0 + \alpha_1 PCR_{rt} + \mu_i + \sum_{t'\neq t_0}\beta_t \mathbb{1}(t=t') + \sum_{t'\neq t_0}\gamma_t \Delta Turnout_i \mathbb{1}(t=t') + \sum_{t'\neq t_0}\delta_t PD_i \mathbb{1}(t=t') + \sum_{t'>t_2}\zeta_t OCT_i \mathbb{1}(t=t')\right\} \tag{2}$$

where $\Delta$ denotes the change in the turnout rates, and we interact the week indicators with the municipality-level difference in the turnout rates between the September 2020 constitutional referendum and the average of the past four national-level polls: the 2019 European elections, the 2018 Political elections, the December 2016 constitutional referendum and the April 2016 abrogating referendum.

In this case, $\gamma_t$ measures the effects on average of one additional point of excess turnout on new weekly coronavirus infections, where the excess is defined with respect to the historical average turnout at municipality level. Also this specification exploits the heterogeneity in the turnout rates across municipalities that comes from the exogenous variation generated by the multiple polls held in September 2020 in the "treated" Italian municipalities, and it has the advantage to factor-out any time-invariant characteristics explaining the habitual turnout of voters from a given municipality.

However, Equation 2 is unsatisfactory in that it does not tease out the effect of civic capital in our model, it just factors it out. To recover also the estimates for the time-varying effect of civic capital we exploit the following, equivalent specification:

$$\mathbb{E}(NC_{irt}|\mathbf{X_{irt}}) = exp\left\{\alpha_0 + \alpha_1 PCR_{rt} + \mu_i + \sum_{t'\neq t_0}\beta_t \mathbb{1}(t=t') + \sum_{t'\neq t_0}\gamma_t Turnout_i \mathbb{1}(t=t') + \sum_{t'\neq t_0}\omega_t APT_i \mathbb{1}(t=t') + \sum_{t'\neq t_0}\delta_t PD_i \mathbb{1}(t=t') + \sum_{t'>t_2}\zeta_t OCT_i \mathbb{1}(t=t')\right\} \tag{3}$$

where $APT_i$ is the average of the past four national-level turnouts at municipality level, held prior to the September 2020 election day. Just as in the case of $PD_i$, the estimated coefficients of the week interactions with the civic capital proxy may be considered as a secondary effect of interest in our analysis, since $APT_i$ is measured prior to the COVID-19 outbreak and the September 2020 election day, thus representing an exogenous variable in our model.

## 3.4 Control Function and bias from unobservables

The models based on Equation 1 and Equation 3 provide estimates of $\gamma_t$ that have a causal interpretation as Average Treatment Effect on the Treated (ATT) if we assume the exogeneity of the turnout rate and the possibility to control for other sources of confounding bias from observables or unobservables that we can proxy for.

However, there may still be municipality-level unobservable factors that we cannot explicitly proxy for and that pose an identification threat to our estimates if they are correlated with both the outcome and the main variable of interest, $Turnout_i$. 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 unobservable factors might be difficult to hold in a dynamic context like the one characterizing a COVID epidemic at municipal, regional and national levels.

There is a wide array of factors related to the local population at municipality-level that we cannot explicitly control for, e.g. the mobility of residents, the share of commuters, the propensity to indulge in risky behaviors and the compliance to laws; such latent factors could contribute to explain both the election day 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, especially in densely populated areas.[20]

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 we estimate a control function (Wooldridge, 2015a) modification of our Equation 3, which is meant to tackle the endogeneity of the turnout rate due to time-varying unobservables.

[20] After the first COVID-19 wave in 2020, and before the availability of vaccines or valid therapies to cure COVID, voters might have acted strategically and chosen whether to participate to 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).

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 for dependent variable, as a function of the "treated" municipalities indicator, $TR_i$, the same covariates used for the calculation of the propensity score, $Z_i$, and Italian provinces (NUTS-3) dummies, $\pi_i$, to capture common time-invariant factors at medium area level that can affect the turnout:

$$Turnout_i = \theta_0 + \theta_1 TR_i + \theta_2 APT_i + \theta_3 Z_i + \pi_i + r_i. \tag{4}$$

We then estimate the second stage Poisson regression as:

$$\begin{aligned}\mathbb{E}(NC_{irt}|\mathbf{X_{irt}}) = exp\Bigg\{&\alpha_0 + \alpha_1 PCR_{rt} + \mu_i + \sum_{t'\neq t_0}\beta_t \mathbb{1}(t=t') + \sum_{t'\neq t_0}\gamma_t Turnout_i \mathbb{1}(t=t') \\ &+ \sum_{t'\neq t_0}\omega_t APT_i \mathbb{1}(t=t') + \sum_{t'\neq t_0}\delta_t PD_i \mathbb{1}(t=t') \\ &+ \sum_{t'>t_2}\zeta_t OCT_i \mathbb{1}(t=t') + \sum_{t'\neq t_0}\rho_t \hat{r}_i \mathbb{1}(t=t')\Bigg\},\end{aligned} \tag{5}$$

where $\hat{r}_i = Turnout_i - \widehat{Turnout}_i$ are the estimated residuals from the first-stage model for the municipality turnout rate (4).

Other, more complex control function approaches have been suggested before in the literature to identify the ATE or the effect of the treatment among the treated (TT) when the endogenous regressor of 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 of interest.

In the first stage Equation 4, $TR_i$ is the instrumental variable that we use to identify the model in Equation 5 and thus the effect of interest of $Turnout_i$. $TR_i$ provides a legitimate source of exogenous variation in the municipality-level turnout rate of the referendum, as we know that the administrative term of both the regional elections and the mayoral elections was unrelated to the municipality-level epidemic stage in September 2020, and that it was scheduled months ahead of the election date. $TR_i$ is also a strong predictor of the difference in the turnout rate of the referendum between treated and control municipalities, as shown descriptively in Table 1 and Figure 1, and as we show also with the results in Table A5.

In the second stage, we interact the predicted residuals $\hat{r}_i$ with the week indicators to control for the time-varying unobservables that might still pollute our estimates after controlling for the municipality fixed effects.[21] In analogy with the related Instrumental Variable (IV) setting (Imbens and Angrist, 1994; Angrist and Imbens, 1995; Angrist et al., 1996), the estimates of Equation 5 can be thought as Local ATT effects, where the variation in the average turnout rate between the treated and the control groups of municipalities, conditional on the set of controls in the first stage, represents the share of voters acting like *compliers*, i.e. voters who cast their vote for both the referendum and the regional or mayoral 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. Indeed, according to our institutional framework, we should expect that the monotonicity condition holds: voters in treated municipalities had a positive incentive to cast their votes compared to voters in the control group, as in Italy regional governments are the local authorities in charge of policies related to public health and healthcare, and municipality councils are the local authorities in charge of other relevant policies like setting municipality-level taxes, fines and guaranteeing local law enforcement and security; this would rule out the presence of voters acting like *defiers* with respect to our instrument, both at the individual level and at the municipality level.

The standard errors of the second stage outcome Equation 5 are bootstrapped with 1,000 replications and clustered at municipality-level to account for the two-step procedure (Murphy and Topel, 1985).

### 3.5 Spatial spillover effects in COVID-19 infections

Another legitimate concern is that Equation 5 does not account for the existence of spatial relationships among 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 and August and the first twenty days of September; thus, the mobility of commuting workers, citizens and holidaymakers could introduce some confounding in our

[21] This interaction is also needed for a control function to be defined in this case, as Equation 4 is time-invariant. To the best of our knowledge, we are among the first to implement a control function approach in this particular fashion.

estimates. For this reason, we also implemented a variation to our baseline strategy in order to control 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. [22] 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 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). Our Control Function model in Equation 5 is augmented with this additional covariate, which is meant to control for the spatial spillover effects of coronavirus clusters. Therefore the modified specification is:

$$\begin{aligned}\mathbb{E}(NC_{irt}|\mathbf{X_{irt}}) = exp\Bigg\{ & \alpha_0 + \alpha_1 PCR_{rt} + \mu_i + \sum_{t'\neq t_0} \beta_t \mathbb{1}(t=t') + \sum_{t'\neq t_0} \gamma_t Turnout_i \mathbb{1}(t=t') \\ & + \sum_{t'\neq t_0} \omega_t APT_i \mathbb{1}(t=t') + \sum_{t'\neq t_0} \delta_t PD_i \mathbb{1}(t=t') \\ & + \sum_{t'>t_2} \zeta_t OCT_i \mathbb{1}(t=t') + \sum_{t'\neq t_0} \rho_t \hat{r}_i \mathbb{1}(t=t') + \sum_{t'} \iota_t \sum_{j} w_{ij} NCpca_{jt} \mathbb{1}(t=t') \Bigg\}\end{aligned} \tag{6}$$

where $w_{ij}$ denotes the spatial weight between municipality $i$ and $j$. $NCpca_{jt}$ is the number of new weekly COVID cases per 100,000 inhabitants in municipality $j$ and week $t$.

The vector of coefficients $\iota_t$ now accounts also for the spatial structure of the data, specifically controlling for the effects that an increase in coronavirus infections has on neighboring municipalities, in each of the weeks in our sample.

[22] 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.

# 4 Results

## 4.1 Baseline fixed-effects Poisson regression model

Panel A of Table 2 reports the estimates of Equation 1, both in the umatched sample (Column 1) and in the matched sample obtained with the nearest neighbor approach (Column 2) and the entropy balance weights (Column 3). In Panel B, instead, we report the estimates for the Difference-in-Difference specification of Equation 1, in which the week indicators have been replaced by a post-poll dummy variable.

The semi-elasticities displayed suggest that the turnout for the September 2020 constitutional referendum contributed to the post-poll rise in COVID-19 infections, and that this effect was increasing over time.[23] In the unmatched sample, the interactions between the post-poll week indicators and the turnout rate are positive and significant starting from the second week after the election days, when a one-point increase in the turnout rate for the constitutional referendum determines an increase in weekly coronavirus infections that ranges from 0.9% in weeks 2 and 3 to 1.6% in week 4. At the same time, we do not find any significant pre-poll trend in new coronavirus infections as a function of the turnout rate, which is consistent with the descriptive statistics shown in Figure 3 and confirms the soundness of our estimation strategy.

Our findings are not substantially affected after the pre-processing of the sample with the matching approaches outlined in Section 3.2. The semi-elasticities with nearest neighbor matching (Columns 2) are very similar to those in Columns 1. The coefficients of the weighted Poisson regression with the entropy balance matching (Column 3) have slightly smaller magnitudes of 0.6% (significant at 10%) and 1.1% (significant at 1%) increases for each additional point of turnout rate, respectively in the third and fourth week after the polls. Overall, the matching regression results imply that differences in demographic, geographical and pre-polls characteristics between the treated and control municipality groups do not drive our main findings.[24]

We also notice that the coefficient for the number of regional PCR tests performed is positive and highly significant in all specifications, highlighting the importance to control for testing capacity in our models. Moreover, the coefficients of the interactions between the last two weeks and the indicator for those municipalities that had either the first or the second ballot of the mayoral elections in the first week of October 2020 indicate that even this electoral round might have favored the spread of COVID-19. Finally, the remaining

[23] The estimates of Equation 1 without controls are reported in Appendix Table A4.

[24] A plot of the event study in Table 2 is provided in the Appendix Figure A2.

Table 2: Effects of Turnout on COVID-19 infections: baseline Fixed-Effects Poisson semi-elasticities.

| | New COVID-19 cases | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| *Panel A: Event Study* | | | |
| 3 weeks pre-poll * Turnout | 0.005 | 0.006 | 0.002 |
| | (0.004) | (0.004) | (0.004) |
| 2 weeks pre-poll * Turnout | 0.003 | 0.003 | 0.001 |
| | (0.004) | (0.004) | (0.004) |
| 1 week pre-poll * Turnout | 0.005* | 0.005* | 0.002 |
| | (0.003) | (0.003) | (0.003) |
| 1 week post-poll * Turnout | 0.003 | 0.002 | 0.002 |
| | (0.002) | (0.002) | (0.002) |
| 2 weeks post-poll * Turnout | 0.009*** | 0.008** | 0.005 |
| | (0.003) | (0.003) | (0.003) |
| 3 weeks post-poll * Turnout | 0.009*** | 0.009*** | 0.006* |
| | (0.003) | (0.004) | (0.004) |
| 4 weeks post-poll * Turnout | 0.016*** | 0.015*** | 0.011*** |
| | (0.004) | (0.004) | (0.004) |
| 3 weeks pre-poll | -0.484** | -0.513** | -0.417* |
| | (0.235) | (0.223) | (0.252) |
| 2 weeks pre-poll | -0.352* | -0.330 | -0.247 |
| | (0.207) | (0.206) | (0.226) |
| 1 week pre-poll | -0.330** | -0.331** | -0.186 |
| | (0.158) | (0.160) | (0.170) |
| 1 week post-poll | -0.092 | -0.011 | -0.023 |
| | (0.138) | (0.135) | (0.136) |
| 2 weeks post-poll | -0.139 | -0.051 | 0.112 |
| | (0.187) | (0.182) | (0.195) |
| 3 weeks post-poll | 0.472** | 0.517** | 0.679*** |
| | (0.205) | (0.205) | (0.219) |
| 4 weeks post-poll | 0.615*** | 0.628*** | 0.859*** |
| | (0.208) | (0.214) | (0.221) |
| PCR | 0.089*** | 0.092*** | 0.093*** |
| | (0.013) | (0.016) | (0.018) |
| 1 week post October poll * October poll | 0.187** | 0.155 | 0.183** |
| | (0.086) | (0.097) | (0.083) |
| 2 weeks post October poll * October poll | 0.157 | 0.113 | 0.174* |
| | (0.106) | (0.124) | (0.102) |
| *Panel B: DiD* | | | |
| Post-poll | 0.140 | 0.204 | 0.317** |
| | (0.127) | (0.143) | (0.135) |
| Post-poll * Turnout | 0.011*** | 0.010*** | 0.009*** |
| | (0.002) | (0.003) | (0.002) |
| 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 (Column 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. List of variables used for matching as in Figure 4. Municipality-level clustered standard errors in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

coefficients of the week interactions with the municipality time-invariant population density (omitted for brevity's sake here, but reported in Appendix Table A3) suggest that the new COVID-19 infections rose especially in municipalities where population density was higher and highlight the importance to control for this important, pre-determined confounder in studies like ours.

Finally, the DiD coefficient in Panel B of Table 2 indicates that, depending on the model, one additional point of turnout rate was associated with a 0.9-1.1% increase in weekly new COVID-19 infections within one month from the election date.

## 4.2 Accounting for the effect of civic capital

The coefficients for the estimates of Equation 3 are reported in Table 3. We still do not find any significant difference from zero in the pre-poll interactions of the week dummies with turnout, but we do find significant semi-elasticities of the same interactions in the post-poll period, suggesting that higher voting turnout contributed to the spread of COVID infections.

The estimates of interest from this model are similar to those in Table 2, but in this case the increase in new COVID infections is positive and significant (at least at 5%), and also larger in magnitude, even for the first week after the election days. The main likely reason why this happens is the inclusion of the weekly interactions with $APT_i$ to proxy for the time-varying effects of civic capital; such interactions are negative and significant in unmatched and matched models, in the first week post-polls in all models, and also in the second week post-polls in the unmatched and entropy-balance matched model. Intuitively this is plausible, as in municipalities with higher civic capital we could expect a more prevalent abidance to distancing rules and use of NPIs, hence fewer new COVID infections even with a large voting turnout. These results highlight the importance to account for the confounding effects of factors that are potentially highly correlated with the main effect of interest, like civic capital, in studies like ours.

Table 3: Effects of Turnout on COVID-19 infections accounting for civic capital proxied by average past turnout.

| | 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) |
| 3 weeks pre-poll | -1.148** | (0.494) | -1.334*** | (0.440) | -1.065** | (0.489) |
| 2 weeks pre-poll | -0.723** | (0.358) | -0.585 | (0.361) | -0.763** | (0.369) |
| 1 week pre-poll | -0.512** | (0.233) | -0.584** | (0.251) | -0.362 | (0.243) |
| 1 week post-poll | 0.828*** | (0.241) | 0.492** | (0.243) | 0.882*** | (0.258) |
| 2 weeks post-poll | 0.677** | (0.325) | 0.331 | (0.340) | 1.066*** | (0.342) |
| 3 weeks post-poll | 0.703** | (0.315) | 0.615* | (0.333) | 0.902*** | (0.328) |
| 4 weeks post-poll | 0.478 | (0.327) | 0.366 | (0.343) | 0.818** | (0.359) |
| *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 4. Municipality-level clustered standard errors in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

### 4.3 Control Function event study.

Figure 5 reports the estimated elasticities after we implement the Control Function (CF) approach described in subsection 3.3, whereas the corresponding semi-elasticities and the first stage key coefficient are reported in Table 4.

The results' pattern is consistent with those presented in Table 3. Interestingly, the residuals obtained from the estimation of the first stage model captures some positive correlations between the first stage and the outcome equation. 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 the excess turnout to the referendum with respect to the historical average past turnout at municipality level. The second component is given by the time-varying effects of the civic capital proxied by the average past turnout. The third component is instead given by the time-varying effects of the aggregate 'selection into voting' at municipality level.

From the first stage regression in Table 4 we can see that the observed turnout of the 2020 referendum is positively associated with both the 'treatment' indicator for regional or majoral elections and the civic capital proxy, and negatively associated with both high excess mortality during the first COVID 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, and that on average they acted strategically choosing to show themselves at the ballots according to their expected gains from the trade-off between exercising their right to vote, that is likely a positive function of civic capital, and risking to be exposed to and 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 municipality population level is consistent both with the concept of expected gains from the participation to a programme (Heckman, 1997) and with the results on the 2020 polls turnout as a function of the first wave excess mortality shown by Picchio and Santolini (2021) on a subset of the municipalities that we use in our sample.

Despite the significance of the weekly interactions with the first stage residuals, the point estimates of interest for the $Turnout_i$ variable from the CF approach are almost identical to the the point estimates from Equation 3 reported in Table 3. Since Equation 3 estimates an ATT effect, whereas Equation 5 is supposed to estimate a LATT effect, we conclude that the LATT approximated by Equation 5 is very close to the ATT, which provides more generality to our findings.

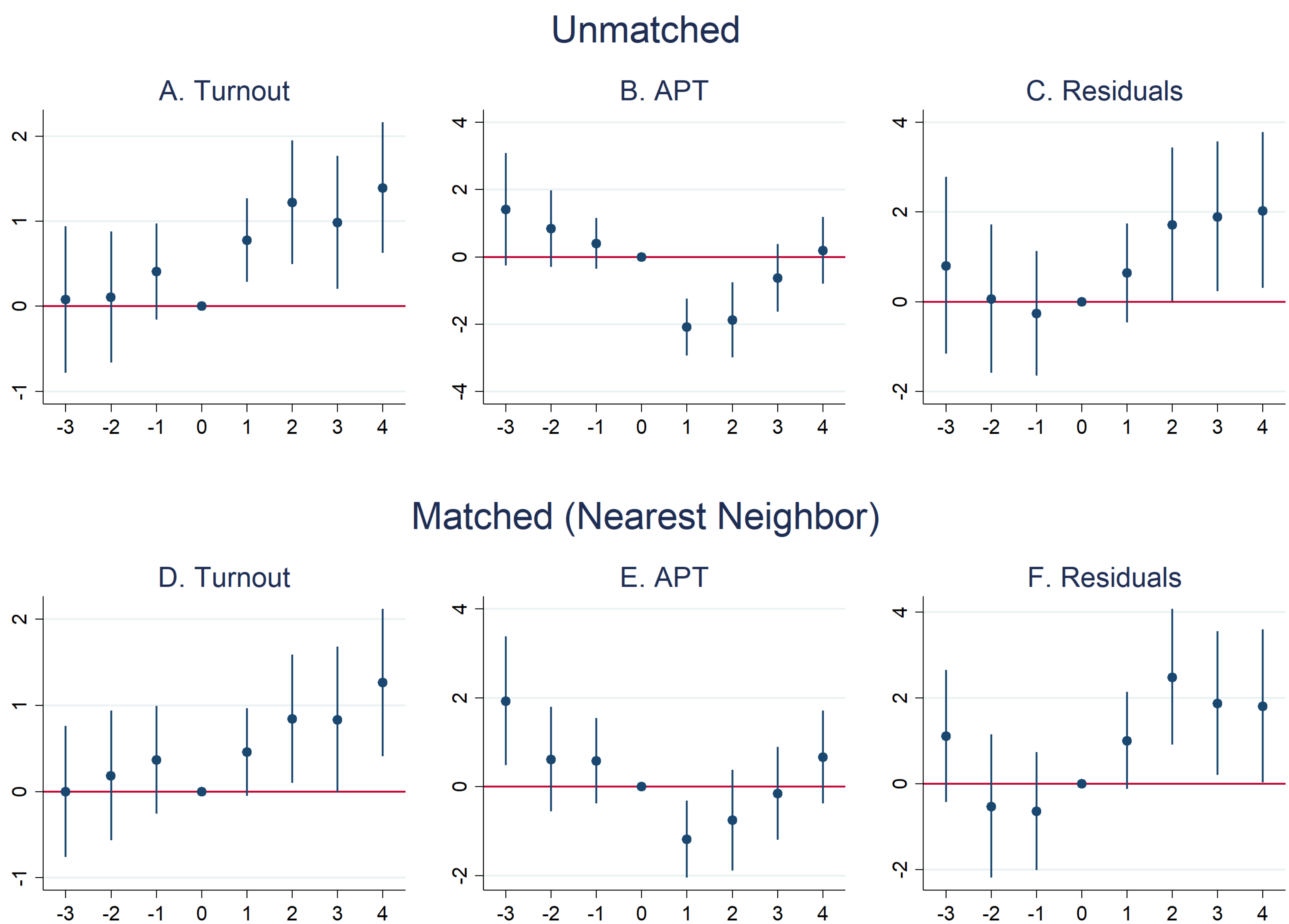


Figure 5: Effects of Turnout with Control Function: Elasticities and 95% Confidence Intervals

Table 4: 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 4. Municipality-level clustered bootstrapped standard errors (1,000 iterations) in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

## 4.4 Controlling for spatial autocorrelation in COVID cases

Table 5 reports estimates of Equation 6, 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 5. The magnitude of the spatial effects is higher for larger distance thresholds of the spatial autocorrelation matrix: this 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 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 interactions with the turnout variable) are in line with those reported in the previous sections. We interpret this as evidence that the spillover effects are not a serious confounder for our analysis.

## 4.5 Heterogeneous effects by municipality characteristics

In this subsection, we investigate how some of the fixed municipality-level characteristics drive the main findings of this study. Table 6 explores the heterogeneity of our results with respect to the following three variables, that play key roles in explaining or describing the epidemic curve: population age (measured in January 2020), population density (measured in January 2020) and excess mortality during the first COVID-19 wave. In the first two cases, we split the sample between municipalities whose population age or density falls below or above the median of the sample. In the last case, we distinguish between: (i) municipalities with a negative first wave excess mortality; (ii) municipalities with a positive but below the median (computed only among municipalities with a positive excess mortality) first wave excess mortality; (iii) municipalities with a positive and above the median first wave excess mortality. For this analysis we use the CF strategy estimated on the matched sample of municipalities, as this ensures that we have a balanced groups of units with respect to the three aforementioned characteristics.[25]

The first set of estimates shows that there was a higher number of COVID infections in the first week post polls in municipalities with higher turnouts and population age below the

[25] Qualitatively similar results, available from the authors upon request, are obtained with this heterogeneity analysis performed on the unmatched sample.

Table 5: 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: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table 6: Heterogeneous effects of Turnout on COVID-19 infections by municipality-level population age, density and excess mortality.

| | Age | | Population Density | | Wave I Excess Mortality | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| 3 weeks pre-poll * Turnout | -0.004 | 0.008 | -0.009 | 0.002 | 0.002 | 0.004 | -0.015* |
| | (0.005) | (0.007) | (0.008) | (0.005) | (0.007) | (0.006) | (0.009) |
| 2 weeks pre-poll * Turnout | -0.001 | 0.008 | -0.011 | 0.004 | -0.001 | 0.005 | -0.004 |
| | (0.005) | (0.007) | (0.008) | (0.004) | (0.008) | (0.006) | (0.008) |
| 1 week pre-poll * Turnout | 0.005 | 0.001 | -0.007 | 0.005 | 0.002 | 0.007* | -0.006 |
| | (0.004) | (0.005) | (0.006) | (0.003) | (0.006) | (0.004) | (0.007) |
| 1 week post-poll * Turnout | 0.006** | 0.001 | -0.017*** | 0.010*** | 0.007 | 0.005 | 0.001 |
| | (0.003) | (0.005) | (0.006) | (0.003) | (0.005) | (0.004) | (0.006) |
| 2 weeks post-poll * Turnout | 0.008* | 0.011* | -0.007 | 0.011*** | 0.013* | 0.006 | 0.007 |
| | (0.004) | (0.006) | (0.007) | (0.004) | (0.007) | (0.005) | (0.007) |
| 3 weeks post-poll * Turnout | 0.009 | 0.008 | -0.004 | 0.010** | 0.015 | 0.004 | 0.011 |
| | (0.006) | (0.006) | (0.007) | (0.005) | (0.009) | (0.005) | (0.007) |
| 4 weeks post-poll * Turnout | 0.011* | 0.015*** | 0.000 | 0.014*** | 0.019** | 0.007 | 0.018*** |
| | (0.006) | (0.006) | (0.007) | (0.005) | (0.010) | (0.005) | (0.007) |
| 3 weeks pre-poll * APT | 0.015** | 0.029* | 0.021 | 0.018** | 0.010 | 0.014 | 0.075*** |
| | (0.008) | (0.016) | (0.017) | (0.008) | (0.012) | (0.010) | (0.018) |
| 2 weeks pre-poll * APT | 0.006 | -0.001 | 0.007 | 0.007 | 0.003 | -0.001 | 0.035** |
| | (0.007) | (0.014) | (0.015) | (0.007) | (0.013) | (0.008) | (0.016) |
| 1 week pre-poll * APT | -0.002 | 0.015 | 0.027*** | 0.001 | 0.002 | -0.002 | 0.043** |
| | (0.006) | (0.012) | (0.010) | (0.006) | (0.010) | (0.006) | (0.018) |
| 1 week post-poll * APT | -0.013*** | -0.008 | 0.013 | -0.016*** | -0.016* | -0.006 | -0.024** |
| | (0.005) | (0.010) | (0.011) | (0.005) | (0.008) | (0.007) | (0.011) |
| 2 weeks post-poll * APT | -0.015** | -0.000 | 0.017 | -0.010 | -0.003 | -0.010 | -0.027* |
| | (0.007) | (0.013) | (0.015) | (0.006) | (0.012) | (0.009) | (0.015) |
| 3 weeks post-poll * APT | -0.004 | -0.002 | 0.013 | -0.003 | 0.008 | -0.005 | -0.024 |
| | (0.007) | (0.013) | (0.014) | (0.006) | (0.013) | (0.008) | (0.015) |
| 4 weeks post-poll * APT | 0.003 | 0.010 | 0.016 | 0.008 | 0.003 | 0.007 | -0.005 |
| | (0.007) | (0.013) | (0.013) | (0.006) | (0.013) | (0.008) | (0.014) |
| 3 weeks pre-poll * Residuals | 0.012 | 0.014 | 0.033* | 0.008 | -0.003 | 0.018 | 0.006 |
| | (0.010) | (0.013) | (0.020) | (0.009) | (0.016) | (0.012) | (0.015) |
| 2 weeks pre-poll * Residuals | -0.007 | 0.002 | 0.039** | -0.012 | -0.012 | -0.001 | -0.003 |
| | (0.012) | (0.011) | (0.019) | (0.009) | (0.017) | (0.014) | (0.012) |
| 1 week pre-poll * Residuals | -0.011 | 0.003 | 0.025* | -0.011 | -0.011 | -0.006 | 0.002 |
| | (0.009) | (0.010) | (0.014) | (0.008) | (0.013) | (0.010) | (0.012) |
| 1 week post-poll * Residuals | 0.004 | 0.020** | 0.027* | 0.010* | 0.002 | 0.008 | 0.023** |
| | (0.006) | (0.010) | (0.014) | (0.006) | (0.012) | (0.009) | (0.010) |
| 2 weeks post-poll * Residuals | 0.011 | 0.057*** | 0.056*** | 0.021** | 0.005 | 0.037*** | 0.026* |
| | (0.010) | (0.012) | (0.020) | (0.008) | (0.015) | (0.011) | (0.014) |
| 3 weeks post-poll * Residuals | 0.006 | 0.049*** | 0.055*** | 0.014 | -0.001 | 0.027** | 0.028*** |
| | (0.011) | (0.012) | (0.014) | (0.009) | (0.019) | (0.012) | (0.010) |
| 4 weeks post-poll * Residuals | 0.010 | 0.040*** | 0.070*** | 0.010 | 0.010 | 0.028** | 0.018 |
| | (0.012) | (0.013) | (0.014) | (0.010) | (0.019) | (0.012) | (0.011) |
| Sample | Matched (NN) | Matched (NN) | Matched (NN) | Matched (NN) | Matched (NN) | Matched (NN) | Matched (NN) |
| Treated Municipalities | 1,096 | 1,099 | 1,052 | 1,052 | 757 | 779 | 659 |
| Control Municipalities | 1,099 | 1,096 | 1,143 | 1,143 | 828 | 623 | 744 |
| Municipality-Week observations | 17,560 | 17,560 | 17,560 | 17,560 | 12,680 | 11,216 | 11,224 |
| Median Age | Below | Above | All | All | All | All | All |
| Median Population Density | All | All | Below | Above | All | All | All |
| Wave I Excess Mortality | All | All | All | All | Negative | Low | High |
| CF | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Fixed-effects Poisson semi-elasticities in the nearest neighbor matched sub-sample. 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 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. APT = Average turnout in the four past elections held nationally. List of variables used for matching as in Figure 4. Municipality-level clustered bootstrapped standard errors (1,000 iterations) in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

median, while there was a higher number of COVID infections in the forth week post polls in municipalities with higher turnouts and population age above the median. This would be consistent with a pattern where COVID infections manifested early on in municipalities with a younger population, consistently with the findings from Palguta et al. (2021), whereas the turnout effect took some time to build up but eventually erupted in its strength in the fourth week after the polls in municipalities with an older population, where elderly citizens might have been more careful but might have been infected by possibly asymptomatic neighbors or family members.

The set of coefficients displayed in Columns 3 and 4 confirm that our baseline effects are mostly driven by municipalities with a higher population density, which is compatible with new COVID-19 infections due to the poll gatherings circulating faster given the larger number of residents per square kilometer.

Finally, the last heterogeneity analysis (Columns 5-7) suggests that the voters' turnout effects mostly come either from the set of municipalities recording a negative excess mortality between March and June 2020 (Column 5) or the set of municipalities recording high excess mortality between March and June 2020 (Column 7). This is also plausible because voters in municipalities less affected by the first COVID wave, i.e. those with a negative excess mortality, might have been less risk averse and so less careful in respecting distancing rules and complying with the use of NPIs, while the population of municipalities with high excess mortality in the first wave may be observably and unobservably sicker or frailer, hence more at risk of contagion because of the polls-related mass gatherings.

### 4.6 Excess Mortality

In this sub-section we investigate 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.[26]

[26]This is similar, yet slightly different, to the definition of municipality-level excess mortality used in Section 4.1 to Section 4.5, 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 in each of the two periods, i.e,

We then estimate the following linear model for excess mortality:

$$
\begin{aligned}
ln(EM_{it} + \sqrt{EM_{it}^2 + 1}) = \alpha_0 + \mu_i + \sum_{t' \neq t_0} \beta_t \mathbb{1}(t = t') + \sum_{t' \neq t_0} \gamma_t Turnout_{ir} \mathbb{1}(t = t') + \\
\sum_{t' \neq t_0} \omega_t APT_i \mathbb{1}(t = t') + \sum_{t' \neq t_0} \delta_t PD_{ir} \mathbb{1}(t = t') + \\
\sum_{t' > t_2} \zeta_t OCT_i \mathbb{1}(t = t') + \sum_{t' \neq t_0} \rho_t \hat{r}_i \mathbb{1}(t = t') + \varepsilon_{irt},
\end{aligned} \tag{7}
$$

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 A3), as well as for the fact that this transformation allows us the interpretation of 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).[27]

The model is estimated as a linear model, given that the support of the dependent variable corresponds to the entire real line, and it includes municipality fixed effects. We estimate this model first without the inclusion of $APT_i * weeks$ interactions, then by including these terms and using a CF strategy, with the results of the two specifications respectively in Columns 1 and 2 of Table A8.

The estimated 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 in September 2020, infections translate into extremely low COVID-19 deaths, hence we have both that new COVID-19 infections lead to fewer deaths and that excess overall mortality becomes a very lousy proxy of COVID-19 related mortality, unlike during periods of high contagion.

---

$FWEM_i = \frac{\#Deaths_i^{2020}}{\#Residents_i^{2020}} - \frac{\overline{\#Deaths_i}^{2015/2019}}{\overline{\#Residents_i}^{2015/2019}}.$

[27] 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 7 provides qualitatively similar findings even when we rescale our measure of excess mortality.

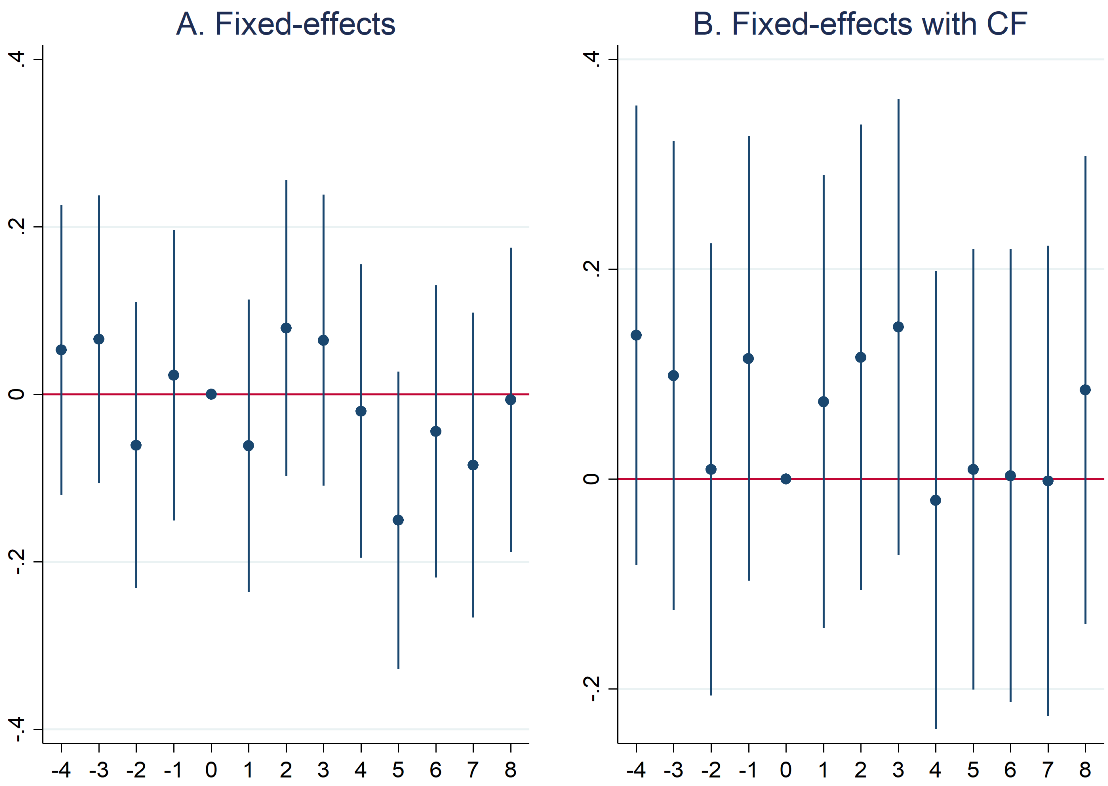


Figure 6: Effect of Turnout on excess mortality

# 5 Robustness checks

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

*Partial left-censoring of the outcome variable.*
First, we verify how results change if we treat censored values in the number of new weekly COVID-19 cases differently. This robustness check 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 vary: (i) in the worst and in the best case scenarios, namely when we replace the censored values respectively with new daily 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.[28] Results for these alternative specifications are provided in Table A9, 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.

*Inclusion and exclusion of the number of PCR test as control.*
Second, we provide alternative specifications to the baseline with respect to the PCR tests control variable. Indeed, the latter may depend on the stage of epidemic spread, thus it might also be affected by the occurrence of the polls. For this reason, in Table A10 we report estimates of variants of Equation 5, 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 municipality 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 turnout treatment of interest (municipality). All estimates from these three alternative specifications provide very similar coefficients of interest on the $Turnout_i * week$ interactions, which are very similar to the coefficients reported in Table 4, except for the point estimate of the coefficient in the fourth week post polls, which

[28] 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.

is smaller in Table A10. As such, it seems that the effect of turnout on COVID-19 spread does not depend on controlling for the number of COVID tests run.

*Confounding due to the start of the compulsory schooling term.*

The treatment examined in this paper falls exactly around the Italian schools' opening date, which happened in most regions on the Monday after the polls.[29] Thus, it is important to check for the possible confounding of school openings on our effect of interest. To do so, we augment our baseline model 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 A11, 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 schools and new weekly infections only if we weigh the number of schools by municipality population, in the first two weeks following the polls. Nevertheless, our main coefficients of interest are significant and mostly unchanged in magnitude by the inclusion of the controls for school openings, except for slightly smaller coefficients (Column 1) for the effect of turnout in the third and fourth weeks post-polls, with respect to those reported in Table 4. Hence, the re-opening of schools cannot explain the findings of this study.

*Including time-varying effects of all predetermined variables.*

In Table A12 we test the robustness of our findings by including in the outcome equation of the CF strategy the interaction of the week indicators with all the predetermined municipality characteristics that we included in the matching procedures and in the first stage explaining the municipality turnout. The post-polls effects of interest are still significant, although slightly smaller in magnitude in the third and fourth weeks post-polls than those reported in Table 4.

*Confounding due to pre-polls electoral rallies for mayoral elections.*

Last but not least, the final set of estimates are meant to check the robustness of our findings to the assignment mechanism of our treatment of interest, hence to implicitly test the validity of the exclusion restriction used in the CF strategy.[30] The only case that might partially compromise the exclusion restriction is given by mayoral elections, as some pre-electoral

[29]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.

[30]Our main results about the effect of turnout on COVID-19 infection spread does not rely on the CF approach, as they are already teased out by a model like Equation 3. In this respect, the CF strategy based on Equation 4 and Equation 5 is important to show that, if there is self-selectivity from gains into voting at municipality level, it is not substantial to bias our estimates of interest, which seems to be the case.

rallies by the candidates to the municipality council may have happened in the interested municipalities during the weeks before the September 2020 polls. In this instance, our main estimates of interest of the effect of turnout might be downward biased, as the contagion by mass gathering would have started to build up before the polls in this part of the treated municipalities (N = 955). Instead, the municipalities undergoing only a regional government election together with a referendum vote (N = 1,896) are not expected to be impacted sensibly by this issue, as they are very numerous in each region and so the candidates to regional election could have not scheduled pre-electoral rallies in all the municipalities within that region during the weeks immediately prior to the September 2020 polls. To test this concern, we re-estimate the models from Equation 3 and Equation 5 by excluding from the sample the municipalities holding a mayoral election.[31] The results, reported in Table 7, show larger coefficients for the weekly interactions with turnout in the event-study compared to those in Table 4, and a DiD coefficient equal to 0.020 which is almost double the one reported in Table 4. There are two main implications for our study. First, it is possible that the effect of turnout on COVID-19 spread due to the September 2020 polls was even larger than what suggested by the estimates in Table 4. Second, a significant effect in Table 7 suggests that there are definitely other mechanisms at play in the spread of COVID-19 due to the voters' turnout that are different from pre-electoral rallies, as the latter were much less likely in municipalities holding only a regional government election alongside the referendum, which constitute the bulk of the treated group. We speculate on the likely alternative mechanisms at play in the concluding section.

[31] In the first stage of the CF for this model province fixed effects are not included as they would be collinear with the treatment indicator, which is defined in this case just at regional level, with provinces nested into regions.

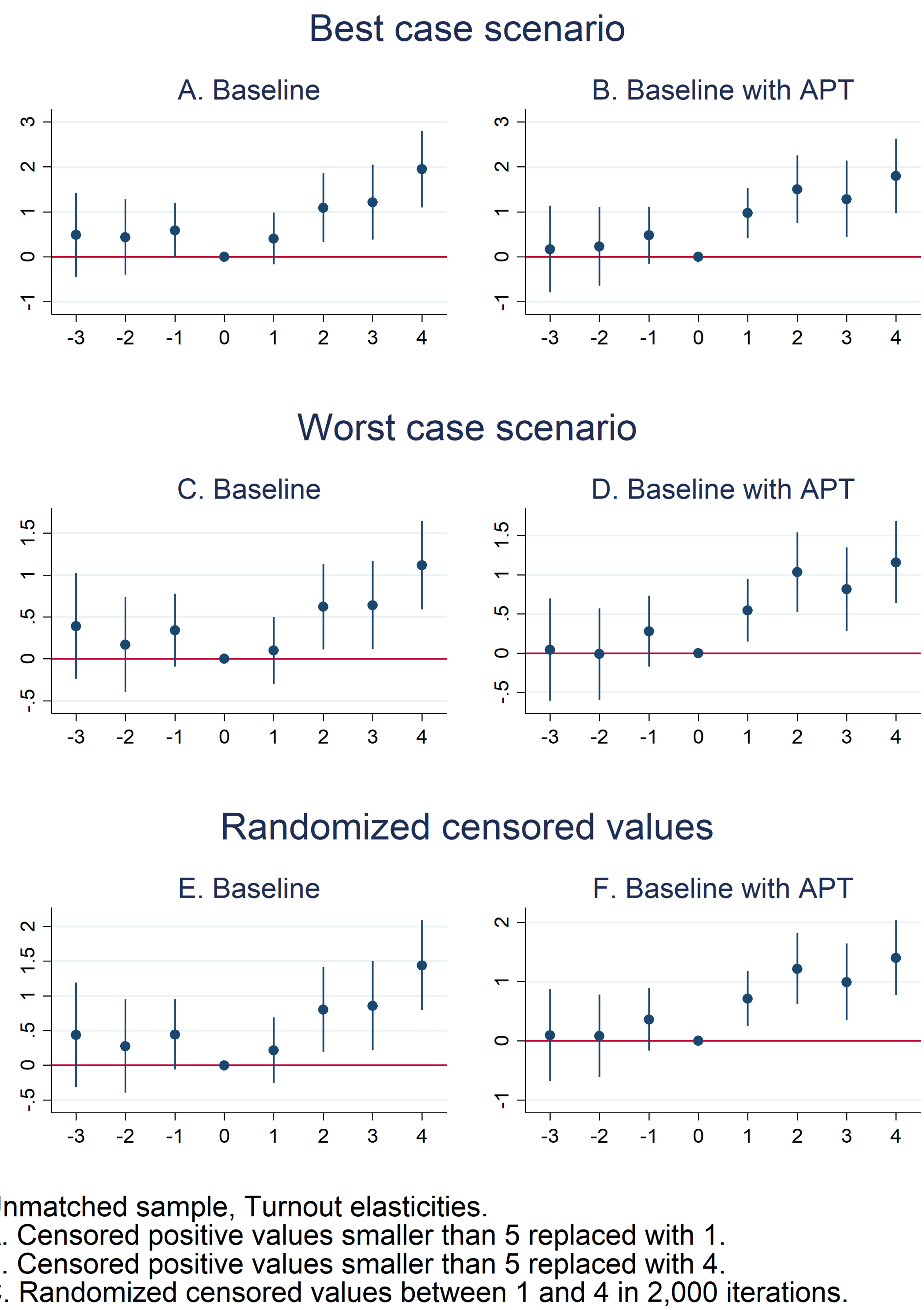


Figure 7: Robustness checks to left censoring

Table 7: Effects of turnout on COVID-19 without Mayoral Elections.

| | New COVID-19 cases | | | | Turnout |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: $1^{st}$ stage* | | | | | |
| Treated | | | | | 17.276*** |
| | | | | | (0.142) |
| APT | | | | | 0.736*** |
| | | | | | (0.013) |
| *Panel B: $2^{nd}$ Stage Event-Study Design* | | | | | |
| 3 weeks pre-poll * Turnout | 0.006 | 0.012* | -0.000 | 0.007 | |
| | (0.006) | (0.006) | (0.007) | (0.008) | |
| 2 weeks pre-poll * Turnout | 0.005 | 0.009* | 0.001 | 0.007 | |
| | (0.005) | (0.005) | (0.005) | (0.006) | |
| 1 week pre-poll * Turnout | 0.007* | 0.009* | 0.006 | 0.010* | |
| | (0.004) | (0.005) | (0.005) | (0.006) | |
| 1 week post-poll * Turnout | 0.011*** | 0.011*** | 0.009*** | 0.010*** | |
| | (0.003) | (0.003) | (0.003) | (0.004) | |
| 2 weeks post-poll * Turnout | 0.019*** | 0.021*** | 0.013*** | 0.016** | |
| | (0.004) | (0.005) | (0.005) | (0.007) | |
| 3 weeks post-poll * Turnout | 0.016*** | 0.021*** | 0.011* | 0.016** | |
| | (0.005) | (0.006) | (0.006) | (0.008) | |
| 4 weeks post-poll * Turnout | 0.020*** | 0.025*** | 0.012** | 0.018** | |
| | (0.004) | (0.006) | (0.006) | (0.008) | |
| 3 weeks pre-poll * APT | 0.009 | -0.002 | 0.014 | 0.004 | |
| | (0.011) | (0.008) | (0.011) | (0.010) | |
| 2 weeks pre-poll * APT | 0.003 | -0.010 | 0.006 | -0.007 | |
| | (0.007) | (0.007) | (0.007) | (0.007) | |
| 1 week pre-poll * APT | 0.002 | -0.006 | 0.002 | -0.008 | |
| | (0.005) | (0.006) | (0.005) | (0.007) | |
| 1 week post-poll * APT | -0.025*** | -0.020*** | -0.024*** | -0.020*** | |
| | (0.004) | (0.005) | (0.005) | (0.005) | |
| 2 weeks post-poll * APT | -0.025*** | -0.025*** | -0.021*** | -0.021** | |
| | (0.006) | (0.007) | (0.007) | (0.009) | |
| 3 weeks post-poll * APT | -0.010** | -0.018*** | -0.006 | -0.014* | |
| | (0.005) | (0.007) | (0.006) | (0.009) | |
| 4 weeks post-poll * APT | -0.001 | -0.011 | 0.006 | -0.004 | |
| | (0.005) | (0.007) | (0.006) | (0.009) | |
| 3 weeks pre-poll * Residuals | | | 0.060*** | 0.057*** | |
| | | | (0.014) | (0.014) | |
| 2 weeks pre-poll * Residuals | | | 0.033** | 0.022 | |
| | | | (0.013) | (0.014) | |
| 1 week pre-poll * Residuals | | | 0.005 | -0.007 | |
| | | | (0.014) | (0.016) | |
| 1 week post-poll * Residuals | | | 0.014 | 0.012 | |
| | | | (0.009) | (0.010) | |
| 2 weeks post-poll * Residuals | | | 0.047*** | 0.047*** | |
| | | | (0.013) | (0.016) | |
| 3 weeks post-poll * Residuals | | | 0.050*** | 0.048*** | |
| | | | (0.015) | (0.017) | |
| 4 weeks post-poll * Residuals | | | 0.061*** | 0.062*** | |
| | | | (0.015) | (0.017) | |
| *Panel C: $2^{nd}$ Stage DiD* | | | | | |
| Post-poll | 0.967*** | 0.615** | 0.990*** | 0.637** | |
| | (0.226) | (0.255) | (0.238) | (0.252) | |
| Post-poll * Turnout | 0.021*** | 0.021*** | 0.020*** | 0.020*** | |
| | (0.004) | (0.005) | (0.005) | (0.006) | |
| Post-poll * Past Turnout | -0.023*** | -0.017*** | -0.022*** | -0.016** | |
| | (0.005) | (0.007) | (0.006) | (0.007) | |
| Post-poll * Residuals | | | 0.012 | 0.018 | |
| | | | (0.011) | (0.014) | |
| Sample | Unmatched | Matched (NN) | Unmatched | Matched (NN) | Unmatched |
| Treated Municipalities | 1551 | 1533 | 1551 | 1533 | 1896 |
| Control Municipality | 3620 | 1533 | 3620 | 1533 | 5052 |
| Municipality-Week observations | 41368 | 24528 | 41368 | 24528 | 6948 |
| CF | No | No | Yes | Yes | |

Notes: Fixed-effects Poisson semi-elasticities in the full sample (Columns 1 and 3) and nearest neighbor matched sub-sample (Columns 2 and 4). Event study design in Panel A, Difference-in-difference model in Panel B. Control Function correction in Columns 3 and 4. First-stage residuals computed from the turnout model displayed in Column 5. APT = Average turnout in the four past elections held nationally. Municipality-level clustered standard errors in parenthesis of Columns 1 and 2. Bootstrapped standard errors (1,000 iterations) clustered at the municipality level in parenthesis of Columns 3 and 4. Robust standard errors in parenthesis of Column 5. Significance levels: $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01.

# 6 Cost-benefit analysis: healthcare and lives saved gains 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 cases as an effect of the higher polls turnout. Given the low level of infections rates in the weeks before the 2020 Italian polls (see Figure 2), it is uncertain how dramatic the impact of these polls and the related rallies 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 exercise, 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.[32]

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 that 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.[33] 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 impellent 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.[34]

We exploit these political events and simulate a real-case scenario of the "What If?"

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

[33] https://www.repubblica.it/politica/2021/01/27/news/crisi_governo_sondaggio_elezioni_conte-284457528/; https://www.ilgiorno.it/politica/sondaggio-no-voto-1.5952867; https://www.tpi.it/app/uploads/2021/01/sondaggio-sole.pdf.

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

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 30th 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 cases is the total number of new cases registered in Italy during the first four weeks of March 2021 (***A2***).[35] We also assume that the case fatality rate (CFR) is equal to the one observed in March 2021 (***A3***) according to computations based on the COVID-19 Data Repository at Johns Hopkins University [36]

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 15th April 2021 (Di Giallonardo et al., 2021; ISS, 2021).

In particular, we assume that transmisibility of the strain B.1.1.7 is only 50% higher than pre-existing lineages (***A4***), which corresponds to the lower bound of this strain's transmissibility found by two important studies recently published (Volz et al., 2021; Davies et al., 2021), whereas the estimated upper bound was of either a 90% or 100% higher virus transmissibility.

We assume a zero-valued expectation for the life lost by COVID-19 patients older than 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 A13, 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

[35] https://altems.unicatt.it/altems-Report%2046-compresso.pdf.
[36] https://ourworldindata.org/mortality-risk-covid?country=~ITA.

point estimate valued 0.011 (from the CF model with a sample pre-processed through nearest neighbor matching, as reported in Table 4, 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***). This approach clearly ignores the possible longer-term impacts of holding the 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.

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 more than counterbalanced by assumptions A4 through A9, whose contribution is to make the impacts of our cost-benefits analysis rather conservative.

The results of the cost-benefit analysis are reported in Table 8. In the upper panel (*Panel A*) we report the main inputs for the computations. In the lower panel (*Panel B*) we report 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 formulas for the estimate in each column are shown in the note of Table 8; the results also draw upon the computations from Table A13, 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 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 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 Diagnoses Related Group (DRG) hospital admissions costs sustained by the Italian State from the start of the epidemic till end of March 2021 [37] and 23.3% of the same costs above for a single month of the epidemic [38]. 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.

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

[38] €20,153,168,964 divided by 13 months, from end of February 2020 to end of March 2021, is equal to €1,550,243,766.46

Table 8: Cost-benefit analysis 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 B.1.1.7 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 | Any pre-B.1.1.7 strain | 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 (English variant) | 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 2. 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 A13, 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 A13.

# 7 Conclusions

Up until recently, there was no available clear-cut evidence about the effects of organizing official voting polls on the increase in the spread of highly infectious airborne diseases, as during the current pandemic. This lack of evidence has left the choice whether to hold or postpone forthcoming elections to 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 new COVID-19 infections. By exploiting an exogenous variation in the turnout rate across 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 is associated with a 1.1% increase in post-poll weekly COVID-19 cases.

These findings are robust to a series of sensitivity analyses like the inclusion of spatial lags in the number of coronavirus infections to control for the spatial spillovers of coronavirus clusters. They are also consistent to a set of excess turnout models which use the municipality-level difference between the turnout rate for the 2020 constitutional referendum and four past national-level elections as treatment intensity variable. Furthermore, the analysis documents how the results are mostly driven by municipalities with an high population density and that were hit less by the first wave of COVID-19 started in March 2020. At the same time, we do not find any significant increase in excess mortality up to two months from the elections, which is likely due to the fact that we analyze a period characterized by low levels of infections.

The mechanism behind the contagion caused by the polls may be explained only partly by pre-electoral rallies, as the estimates of interest on a sample where such rallies were much less likely to occur are even larger in magnitude. The other mechanisms for the polls-related infection spread are most likely two: the lack of abidance to NPIs while at the ballots, or 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. In the absence of individual-level, experimental data with records of voters' behavior, actions and choices, we can only speculate about the most likely mechanism at play.

Overall, our study indicates that national-level polls might contribute to the spread of airborne diseases like COVID-19, and that 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. However, our work does not focus on the comparison between voting and non voting local authorities (as in Palguta et al. 2021), which provides only an estimate of the effect of choosing to hold elections on the spread of COVID-19. Instead, we provide an estimate of the causal effect of the turnout rate 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 versus 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 been postponed.

In this regard, and based on our estimates, we provide a cost-benefit evaluation 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 back of the envelope calculations suggest that the appointment of a government of national unity and the prevention of an early general election might have spared Italy around €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. Our cost-benefit figures also represent what James and Alihodzic (2020) defines as a "humanitarian case" for postponing elections, given the inevitable trade-off for holding in-person elections during a pandemic between the exercise of the democratic right to vote versus the value of individual and public health. 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, our first stage regression show that such equality might have been affected with respect to a number of characteristics, like the population density and the latent health frailty proxied by the excess mortality in the municipality of residence during the first COVID 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 affected, during the Italian polls we studied or other in-person ballots held over the global COVID-19 pandemic, is instead an interesting question that we leave for future research.

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# A Appendix

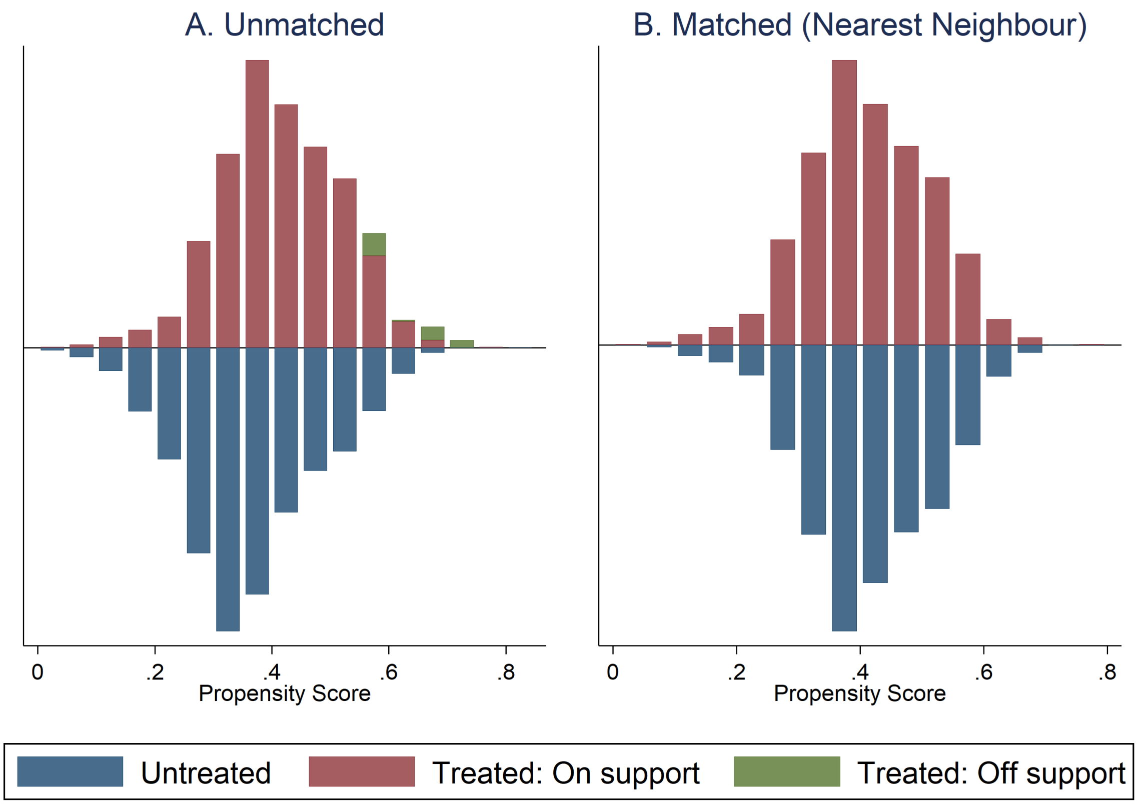


Figure A1: Propensity score distributions

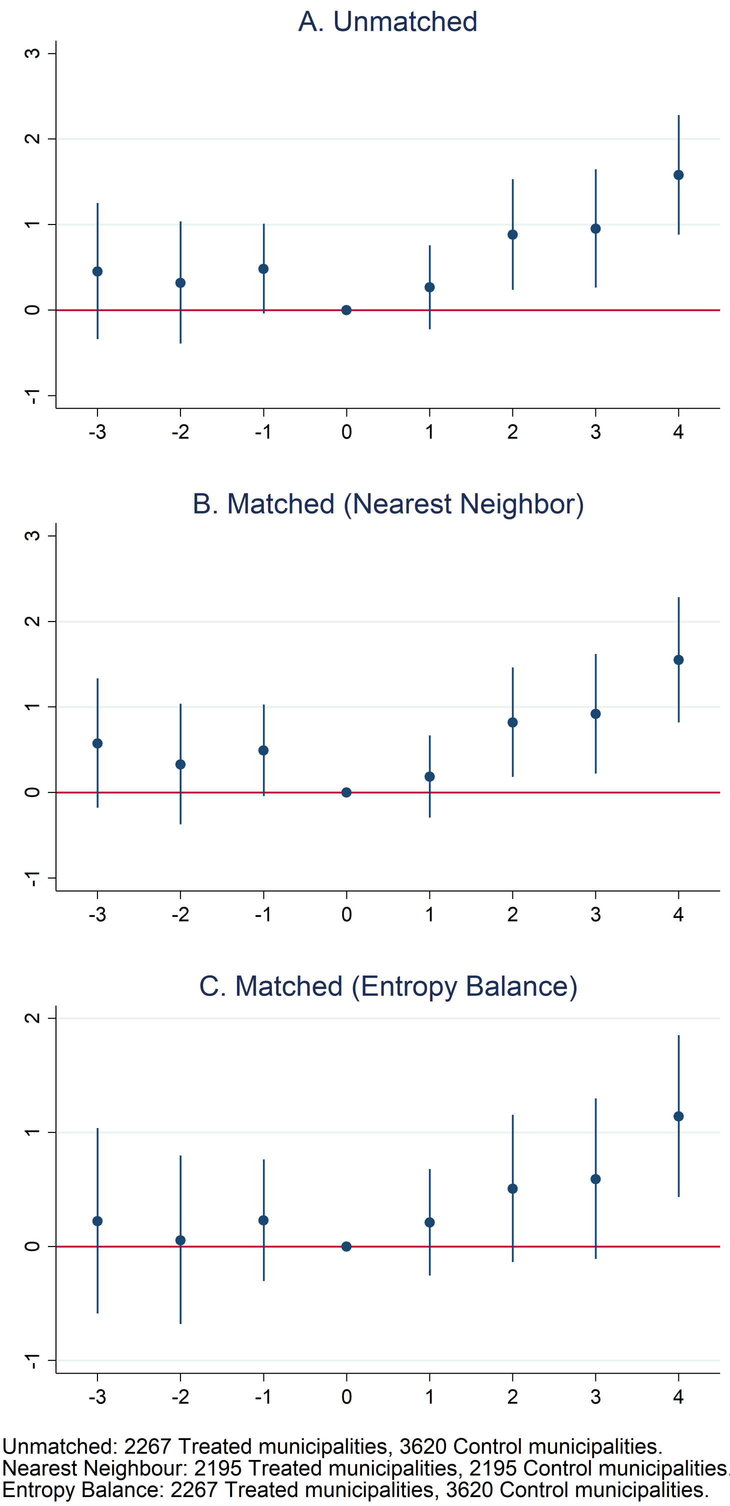


Figure A2: Effect of Turnout on COVID-19 infections (Equation 1)

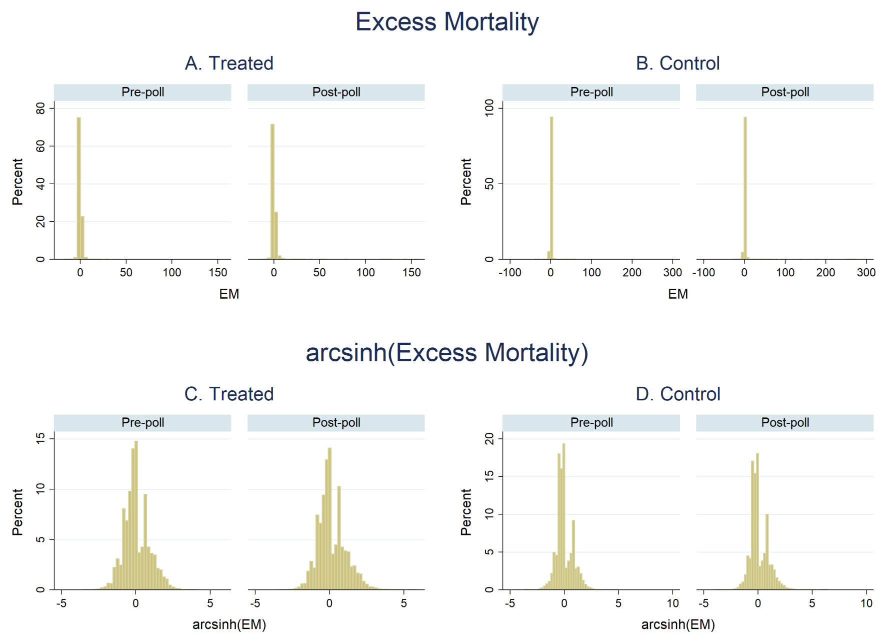


Figure A3: Excess mortality distributions by treatment and post-poll indicators

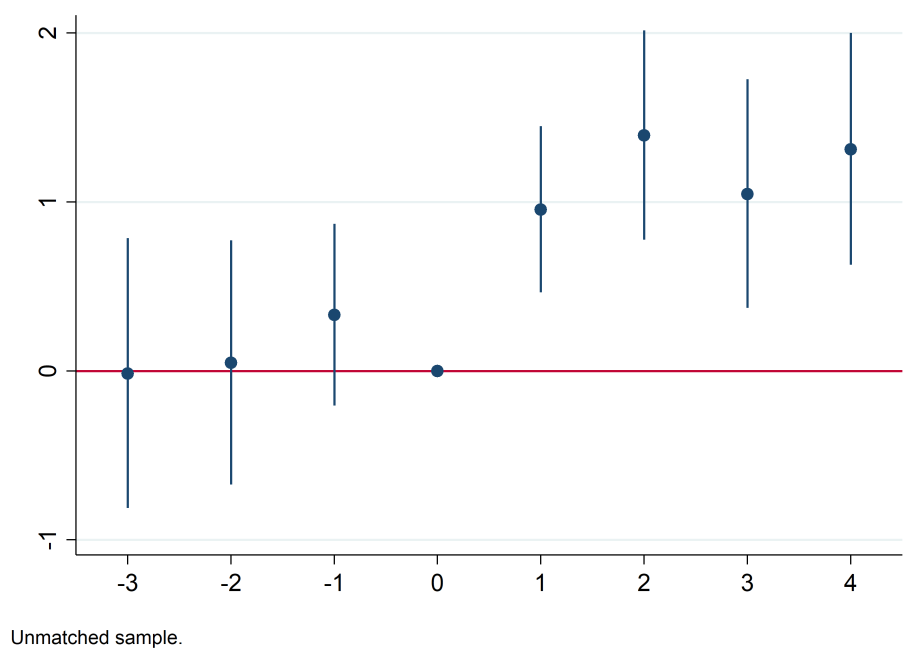


Figure A4: Effect of excess Turnout on COVID-19 infections (Equation 2).

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*** |
| *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.

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: Effects of Population density on COVID-19 infections: baseline Fixed-Effects Poisson semi-elasticities.

| | New COVID-19 cases | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| *Panel A: Event Study* | | | |
| 3 weeks pre-poll * Population Density | 0.084*** | -0.019 | 0.121*** |
| | (0.027) | (0.031) | (0.012) |
| 2 weeks pre-poll * Population Density | 0.024 | -0.038 | 0.064*** |
| | (0.023) | (0.027) | (0.011) |
| 1 week pre-poll * Population Density | 0.006 | -0.014 | 0.010* |
| | (0.009) | (0.021) | (0.006) |
| 1 week post-poll * Population Density | 0.057*** | 0.013 | 0.038*** |
| | (0.021) | (0.018) | (0.014) |
| 2 weeks post-poll * Population Density | 0.073*** | 0.012 | 0.054*** |
| | (0.021) | (0.023) | (0.016) |
| 3 weeks post-poll * Population Density | 0.074*** | 0.026 | 0.082*** |
| | (0.013) | (0.027) | (0.008) |
| 4 weeks post-poll * Population Density | 0.078*** | 0.049** | 0.084*** |
| | (0.014) | (0.024) | (0.014) |
| *Panel B: DiD* | | | |
| Post-poll | 0.140 | 0.204 | 0.317** |
| | (0.127) | (0.143) | (0.135) |
| Post-poll * Population Density | 0.015 | 0.012 | -0.015 |
| | (0.013) | (0.021) | (0.010) |
| 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 (Column 2) and entropy balance weighted sample (Column 3). Event study design in Panel A, Difference-in-difference model in Panel B. List of variables used for matching as in Figure 4. Municipality-level clustered standard errors in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table A4: Effects of Turnout on COVID-19 infections: fixed-effects Poisson models without controls.

| | New COVID-19 cases | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| 3 weeks pre-poll * Turnout | 0.003 | 0.007 | -0.003 |
| | (0.004) | (0.004) | (0.004) |
| 2 weeks pre-poll * Turnout | 0.003 | 0.004 | -0.002 |
| | (0.004) | (0.004) | (0.004) |
| 1 week pre-poll * Turnout | 0.005* | 0.005* | 0.002 |
| | (0.003) | (0.003) | (0.003) |
| 1 week post-poll * Turnout | 0.001 | 0.001 | -0.001 |
| | (0.003) | (0.003) | (0.003) |
| 2 weeks post-poll * Turnout | 0.008** | 0.009*** | 0.003 |
| | (0.003) | (0.003) | (0.003) |
| 3 weeks post-poll * Turnout | 0.007* | 0.008** | 0.001 |
| | (0.004) | (0.003) | (0.004) |
| 4 weeks post-poll * Turnout | 0.008** | 0.010*** | 0.000 |
| | (0.004) | (0.003) | (0.004) |
| 3 weeks pre-poll | -0.301 | -0.606** | 0.136 |
| | (0.273) | (0.244) | (0.260) |
| 2 weeks pre-poll | -0.283 | -0.389* | 0.082 |
| | (0.222) | (0.217) | (0.224) |
| 1 week pre-poll | -0.325** | -0.351** | -0.155 |
| | (0.162) | (0.174) | (0.165) |
| 1 week post-poll | 0.137 | 0.069 | 0.231 |
| | (0.151) | (0.136) | (0.147) |
| 2 weeks post-poll | 0.096 | -0.013 | 0.374* |
| | (0.210) | (0.187) | (0.209) |
| 3 weeks post-poll | 0.848*** | 0.716*** | 1.270*** |
| | (0.226) | (0.205) | (0.231) |
| 4 weeks post-poll | 1.454*** | 1.266*** | 2.002*** |
| | (0.253) | (0.200) | (0.252) |
| 1 week post October poll * October poll | 0.185** | 0.160* | 0.167** |
| | (0.085) | (0.096) | (0.083) |
| 2 weeks post October poll * October poll | 0.182* | 0.165 | 0.163* |
| | (0.097) | (0.112) | (0.095) |
| 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 (Column 2) and entropy balance weighted sample (Column 3). List of variables used for matching as in Figure 4. Municipality-level clustered standard errors in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table A5: 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 A6: Effects of excess turnout on COVID-19 infections: Fixed-Effects Poisson semi-elasticities.

| | New COVID-19 cases | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| 3 weeks pre-poll * ΔTurnout | -0.000 | -0.003 | -0.001 | -0.001 | 0.006 |
| | (0.004) | (0.003) | (0.004) | (0.004) | (0.004) |
| 2 weeks pre-poll * ΔTurnout | 0.000 | 0.001 | -0.001 | -0.000 | 0.001 |
| | (0.004) | (0.002) | (0.004) | (0.004) | (0.004) |
| 1 week pre-poll * ΔTurnout | 0.003 | 0.001 | 0.002 | 0.004 | 0.003 |
| | (0.003) | (0.002) | (0.003) | (0.003) | (0.003) |
| 1 week post-poll * ΔTurnout | 0.010*** | 0.008*** | 0.008*** | 0.010*** | 0.004* |
| | (0.002) | (0.002) | (0.003) | (0.002) | (0.002) |
| 2 weeks post-poll * ΔTurnout | 0.014*** | 0.009*** | 0.012*** | 0.014*** | 0.011*** |
| | (0.003) | (0.002) | (0.003) | (0.003) | (0.003) |
| 3 weeks post-poll * ΔTurnout | 0.010*** | 0.004** | 0.009** | 0.010*** | 0.012*** |
| | (0.003) | (0.002) | (0.003) | (0.004) | (0.004) |
| 4 weeks post-poll * ΔTurnout | 0.013*** | 0.005** | 0.011*** | 0.012*** | 0.016*** |
| | (0.003) | (0.002) | (0.003) | (0.004) | (0.004) |
| 3 weeks pre-poll | -0.239*** | -0.250*** | -0.265*** | -0.259*** | -0.363*** |
| | (0.048) | (0.048) | (0.089) | (0.069) | (0.106) |
| 2 weeks pre-poll | -0.176*** | -0.172*** | -0.197** | -0.182** | -0.195** |
| | (0.046) | (0.046) | (0.089) | (0.072) | (0.099) |
| 1 week pre-poll | -0.053 | -0.062 | -0.020 | -0.003 | -0.140 |
| | (0.038) | (0.040) | (0.062) | (0.051) | (0.090) |
| 1 week post-poll | 0.088** | 0.079** | 0.214*** | 0.211*** | -0.041 |
| | (0.035) | (0.034) | (0.063) | (0.049) | (0.062) |
| 2 weeks post-poll | 0.393*** | 0.375*** | 0.580*** | 0.551*** | 0.087 |
| | (0.042) | (0.043) | (0.072) | (0.059) | (0.084) |
| 3 weeks post-poll | 1.036*** | 1.016*** | 1.162*** | 1.149*** | 0.722*** |
| | (0.045) | (0.046) | (0.078) | (0.066) | (0.097) |
| 4 weeks post-poll | 1.537*** | 1.519*** | 1.704*** | 1.671*** | 1.134*** |
| | (0.053) | (0.052) | (0.080) | (0.073) | (0.104) |
| Sample | Unmatched | Unmatched | Unmatched | Unmatched | Unmatched |
| Treated Municipalities | 2,267 | 2,267 | 2,267 | 2,267 | 2,267 |
| Control Municipalities | 3,620 | 3,620 | 3,620 | 3,620 | 3,620 |
| ΔTurnout | APT | European 2019 | Political 2018 | Constitutional 2016 | Abrogating 2016 |
| Municipality-Week observations | 47,096 | 47,096 | 47,096 | 47,096 | 47,096 |

Notes: Fixed-effects Poisson semi-elasticities in the full sample. Controls included (but not reported): 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; Regional PCR tests performed per 10,000 inhabitants. APT = Average turnout in the four past elections held nationally. Municipality-level clustered standard errors in parenthesis. Significance levels: *p<0.1; **p<0.05; ***p<0.01.

Table A7: Effects of excess turnout on COVID-19 with Control Function.

| | New COVID-19 cases | | | |
|---|---|---|---|---|
| | (1) | | (2) | |
| *Panel B: 2nd Stage Event-Study Design* | | | | |
| 3 weeks pre-poll * ΔTurnout | 0.001 | (0.004) | -0.000 | (0.004) |
| 2 weeks pre-poll * ΔTurnout | 0.001 | (0.004) | 0.002 | (0.004) |
| 1 week pre-poll * ΔTurnout | 0.004 | (0.003) | 0.004 | (0.003) |
| 1 week post-poll * ΔTurnout | 0.008*** | (0.002) | 0.005* | (0.003) |
| 2 weeks post-poll * ΔTurnout | 0.012*** | (0.004) | 0.008** | (0.004) |
| 3 weeks post-poll * ΔTurnout | 0.010** | (0.004) | 0.008* | (0.004) |
| 4 weeks post-poll * ΔTurnout | 0.014*** | (0.004) | 0.013*** | (0.004) |
| 3 weeks pre-poll * APT | 0.015* | (0.008) | 0.019*** | (0.007) |
| 2 weeks pre-poll * APT | 0.009 | (0.006) | 0.008 | (0.006) |
| 1 week pre-poll * APT | 0.008* | (0.004) | 0.009** | (0.005) |
| 1 week post-poll * APT | -0.013*** | (0.004) | -0.007* | (0.004) |
| 2 weeks post-poll * APT | -0.007 | (0.006) | 0.001 | (0.006) |
| 3 weeks post-poll * APT | 0.003 | (0.006) | 0.007 | (0.006) |
| 4 weeks post-poll * APT | 0.016** | (0.006) | 0.019*** | (0.006) |
| 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) |
| 3 weeks pre-poll | -1.100** | (0.497) | -1.294*** | (0.438) |
| 2 weeks pre-poll | -0.724* | (0.374) | -0.610* | (0.364) |
| 1 week pre-poll | -0.528** | (0.249) | -0.605** | (0.265) |
| 1 week post-poll | 0.874*** | (0.257) | 0.535** | (0.247) |
| 2 weeks post-poll | 0.804** | (0.350) | 0.401 | (0.345) |
| 3 weeks post-poll | 0.845** | (0.350) | 0.674* | (0.345) |
| 4 weeks post-poll | 0.633* | (0.368) | 0.425 | (0.359) |
| *Panel C: 2nd Stage DiD* | | | | |
| Post-poll | 0.951*** | (0.234) | 0.620** | (0.255) |
| Post-poll * ΔTurnout | 0.015*** | (0.003) | 0.011*** | (0.003) |
| Post-poll * APT | -0.002 | (0.004) | 0.003 | (0.004) |
| Post-poll * Residuals | 0.012* | (0.006) | 0.016** | (0.007) |
| Sample | Unmatched | | Matched (NN) | |
| Treated Municipalities | 2,267 | | 2,195 | |
| Control Municipalities | 3,620 | | 2,195 | |
| Municipality-Week observations | 47,096 | | 35,120 | |

Notes: Fixed-effects Poisson semi-elasticities with Control Function in the full sample (Column 1) and nearest neighbor matched sub-sample (Column 2). 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 A8: 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 A9: Robustness checks for censored values.

| | Best case scenario | | Worst case scenario | | Randomization | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| 3 weeks pre-poll * Turnout | 0.005 | 0.002 | 0.004 | 0.000 | 0.004 | 0.001 |
| | (0.005) | (0.005) | (0.003) | (0.003) | (0.004) | (0.004) |
| 2 weeks pre-poll * Turnout | 0.004 | 0.002 | 0.002 | -0.000 | 0.003 | 0.001 |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.004) |
| 1 week pre-poll * Turnout | 0.006* | 0.005 | 0.003 | 0.003 | 0.004 | 0.004 |
| | (0.003) | (0.003) | (0.002) | (0.002) | (0.003) | (0.003) |
| 1 week post-poll * Turnout | 0.004 | 0.010*** | 0.001 | 0.005*** | 0.002 | 0.007*** |
| | (0.003) | (0.003) | (0.002) | (0.002) | (0.002) | (0.002) |
| 2 weeks post-poll * Turnout | 0.011*** | 0.015*** | 0.006** | 0.010*** | 0.008*** | 0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) |
| 3 weeks post-poll * Turnout | 0.012*** | 0.013*** | 0.006** | 0.008*** | 0.009*** | 0.010*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) |
| 4 weeks post-poll * Turnout | 0.019*** | 0.018*** | 0.011*** | 0.012*** | 0.014*** | 0.014*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) |
| Sample | Unmatched | Unmatched | Unmatched | Unmatched | Unmatched | Unmatched |
| Treated Municipalities | 2,267 | 2,267 | 2,267 | 2,267 | 2,267 | 2,267 |
| Control Municipalities | 3,620 | 3,620 | 3,620 | 3,620 | 3,620 | 3,620 |
| Municipality-Week observations | 47,096 | 47,096 | 47,096 | 47,096 | 47,096 | 47,096 |
| APT | No | Yes | No | Yes | No | Yes |
| CF | No | No | No | 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 A10: 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 A11: 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 A12: Effects of Turnout on COVID-19 infections with fully-interacted Control function.

| | New COVID-19 cases | |
|---|---|---|
| | (1) | |
| *Panel B: $2^{nd}$ 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) |
| 3 weeks pre-poll | $3.542^{*}$ | (1.909) |
| 2 weeks pre-poll | 2.341 | (1.813) |
| 1 week pre-poll | 0.276 | (1.404) |
| 1 week post-poll | 1.109 | (1.265) |
| 2 weeks post-poll | 0.642 | (1.685) |
| 3 weeks post-poll | -0.042 | (1.864) |
| 4 weeks post-poll | 0.778 | (1.741) |
| *Panel C: $2^{nd}$ Stage DiD* | | |
| Post-poll | $-2.815^{**}$ | (1.334) |
| Post-poll * Turnout | $0.010^{***}$ | (0.002) |
| Post-poll * Past Turnout | -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 A13: 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) = €$74,159 * (B1) * (E1)/3\%$.