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

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ABSTRACT

Natural disasters raise challenging trade-offs between public health safety and inalienable rights like the active involvement in political choices through voting. We exploit a quasi-experimental setting provided by multiple ballots across regions and municipalities during the Italian 2020 elections to estimate the effect of voters' turnout on the spread of COVID-19. By employing an event-study design with a two-stage Control Function strategy, we find that post-poll new COVID infections increased by an average of 1.1% for each additional percentage point of turnout. Based on these estimates and real political events, we also show through a simulation that in-person voting during a high-infection regime may have a large impact on public health outcomes, more than doubling new infections, deaths and hospitalizations. These findings suggest that policy-makers' responses to natural disasters should be flexible and contingent to the emergency severity, in order to minimize social costs for citizens.

1. Introduction

Politicians and healthcare policy-makers are faced with hard times to communicate and impose restrictions to civil rights and freedoms, especially in times of widespread decrease in the satisfaction with governmental policies and, more in general, the way democracy works (Becher et al., 2021). However, they may be faced with an even tougher policy dilemma in the decision whether to hold or postpone official voting polls. Citizens seem to be willing to give up some of their civil

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liberties to reduce health insecurity (Alsan et al., 2020), but they are also very resisting to the adoption of illiberal policies, even if endorsed by health experts, like postponing elections indefinitely (Arceneaux et al., 2020). Elections are gatherings of vital importance for the functioning of democratic countries, and their postponement or cancellation can undermine the citizens' trust in the political institutions of a country (Levitsky and Ziblatt, 2018). Despite this fact, public health concerns related to COVID-19 have resulted in at least 78 countries postponing national or regional elections between February 2020 and July 2021, while more than 128 countries still held polls as previously scheduled (Institute for Democracy and Electoral Assistance (IDEA), 2021). Such heterogeneous response is due to the fact that holding polls during an epidemic requires politicians to face an important trade-off: preserving the spirit of democratic institutions in the long run, but exposing the lives of citizens to the likely contagion, and their political careers to a premature oblivion, should the voting gathering sensibly amplify the spread of the virus; or acting conservatively in the short run, but at the cost of risking future political instability and a fade in the values of democracy. Moreover, the historical evidence has shown that there are few alternatives to physical voting for a general election in settings when voting can be rigged.[1] For these reasons, collecting quantitative evidence on the likely short-term contagion risk borne by holding in-person elections is paramount for politicians and healthcare policy-makers in order to evaluate the best course of action to adopt when official polls are scheduled.

With this work, we aim to shed light on this important public health issue linking voting and the spread of infections diseases, and to quantify the value of the trade-off between public health and political rights. In particular, our analysis adds to the literature that evaluates empirically whether and by how much voting can increase COVID contagion (Cotti et al., 2021; Bertoli et al., 2020; Bach et al., 2021), but it is also one of the few studies doing so by exploiting a natural experiment (Palguta et al., 2022).

We also contribute to the empirical literature investigating the economics of social trade-offs. The economics of privacy (Posner, 1981; Acquisti et al., 2016), the regulation of competition (Cutler and Reber, 1998; Leroy and Lucotte, 2017; Cunningham et al., 2021), and the equity-efficiency trade-off in healthcare (Wagstaff, 1991; Bleichrodt et al., 2005) and tax systems (Browning and Johnson, 1984; Albouy, 2012) are some of the traditional topics belonging to this literature. The advent of the COVID-19 pandemic has impressed a steady growth to this literature, as the implementation of public health measures to fight COVID has raised new trade-offs between the right of citizens' physical health and other inalienable rights: the right to education (Engzell et al., 2021; González and Bonal, 2021), the right to work (Adams-Prassl et al., 2020), gender equality (Zamarro and Prados, 2021), freedom of movement (Ramji-Nogales and Goldner Lang, 2020) and the right to preserve mental health (Pierce et al., 2020; Rossi et al., 2020; Proto and Quintana-Domeque, 2021). Our work, instead, focuses on the trade-off of public health with the active partaking to choices in a representative democracy, i.e. voting, which is a fundamental right for the functioning of democratic institutions.

Before the COVID-19 outbreak, the risks of holding elections during a pandemic were still unclear and not quantitatively measured. The lack of empirical evidence is likely due to the fact that country-level epidemics, or pandemics like the COVID-19 one, are usually rare and unpredictable events, but also that evaluating the impact of going to the polls on the spread of a virus through observational data is prone to bias in the effect of interest: the choice of voters whether to go or not to the polls is most likely endogenous to the local stage of the epidemic. Such issues put a serious threat to make any causal claim about the effect of interest, but they are overcome by the framework provided by our institutional setting.

During Fall 2020, an election day with multiple polls took place in Italy: in all Italian regions, citizens casted ballots for a constitutional referendum aimed at reducing the number of Parliament members; in 7 out of the 20 Italian administrative regions, citizens also casted ballots for electing the new regional governments and the regional assembly representatives; finally, in 955 of the 7903 Italian municipalities, citizens voted even for appointing the new municipality mayor. Such institutional setting resulted in a 22% average increase in the turnout rate for the constitutional referendum in the municipalities where an administrative poll (i.e. either regional, mayoral elections, or both) occurred on top of the referendum.

We build a unique dataset of weekly new COVID-19 infections and voters' turnout at Italian municipality level, including also municipality, province and region characteristics. We then employ an original event-study Control Function design, i.e. an event study where the continuous treatment variable (i.e. the referendum turnout) is instrumented through a Control Function strategy, to examine the weekly evolution of coronavirus infections before and after the September 2020 polls as a function of the referendum turnout rate. This quasi-experimental design has the obvious advantage to greatly reduce the extent of the aforementioned endogeneity bias, as the variation to identify the effect of interest is due to administrative reasons, and so it is independent of the local epidemic status. Furthermore, the focus on turnout allows policy-makers to elaborate cost-benefit simulations based on realistic scenarios of the expected voters' participation at the polls, which may guide them in the decision whether to keep or postpone elections during an epidemic.[2]

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

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

The event-study regressions include municipality and week fixed effects and municipality-clustered standard errors, and we also estimate event-study models after pre-processing our sample through different types of matching based on municipality pre-COVID and pre-poll characteristics (e.g. population density, number of schools per capita, residents' average age), in order to reduce the bias from voters' selection-on-observables. Moreover, we tease out the contribution of civic capital to the spread of COVID-19 infections at the municipality level, because this unobservable is cross-sectionally correlated with turnout, as shown by our analysis, as well as social distancing rules preventing the proliferation of the virus (Barrios et al., 2021; Durante et al., 2021) before any vaccine was available. Last but not least, our empirical strategy copes with the self-selection of voters based on unobservables correlated with a risk-compensating behaviour in municipalities with different population and territorial characteristics, but it cannot directly overcome that the timing of public ballots can be manipulated by governments and public authorities depending on the stage of an epidemic. As such, in order to gauge the impact of voting during a high-infections regime and based on our event study estimates, we also perform a cost-benefit simulation of the healthcare costs and lives saved in Italy by averting an early general election at the start of 2021, when the more transmissible COVID-19 "English" or Alpha variant became prevalent.

Our analysis shows that post-poll new COVID-19 cases increased by 1.1% for each additional percentage point of turnout rate for the constitutional referendum. The magnitude and significance levels of our estimates are largely confirmed even when using matching as a pre-processing technique and when accounting for the bias due to civic capital. These findings suggest that in-person polls have indeed the possibility to increase the spread of airborne diseases like COVID-19, thus potentially triggering or amplifying national-level waves of contagion when they are held during peak periods of an epidemic. These results are informative for politicians and healthcare policy-makers regarding the public health threats posed by voting during a pandemic, and other gathering events that are similar in nature. To further illustrate the relevance of our results, our cost-benefit calculations show that avoiding an early election at the beginning of 2021, following the collapse of the Government in charge till January 2021, has spared Italy up to about € 361.755 millions in hospital care costs and almost 23 thousand more deaths, which are value of lives saved is worth about € 7.539 billions.

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

2. Background and data

2.1. Related literature

Overall, few studies have investigated the impact of voting on public health outcomes during the COVID-19 pandemic. The closest study to ours is Palguta et al. (2022), which examines the impact of the Czech Republic 2020 Senate elections on the spread of COVID-19 and documents a higher increase in the growth rates of COVID-19 infections and hospitalizations in sub-regions where this electoral round took place. The findings from both works are qualitatively and quantitatively similar, since we find that post-poll new COVID-19 infections were about 1.1% higher for each additional percentage point of turnout. However, our analysis presents several distinctive contribution with respect to Palguta et al. (2022): 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; and we employ an event-study design with a Control Function strategy, as we are concerned with the endogeneity of turnout due to self-selectivity of voters stemming from the unobservable trade-off between the individual expected utility from casting a ballot and the health risk of contracting the virus. Moreover, our study differs from Palguta et al. (2022) as we analyze the effect of voters' turnout as a measure of treatment intensity rather than just treatment assignment (i.e. holding versus not holding polls). Examining the impact of turnout is likely more relevant for policy-makers, since the spread of new infections is a function of the "gathering intensity" provided by voters' turnout, not just by whether in-person elections are held or not.

Other existing contributions have investigated our research topic, but without exploiting a source of exogenous variation to identify the causal effect of holding elections on COVID-19 spread. For instance, Cotti et al. (2021) exploits county-level variation in overall turnout at the Wisconsin presidential primary election in an event-study specification similar to ours, finding a 1.8% increase in the rate of COVID-19 positive tests for each percentage point of voter density per polling station.[3] While the functional form that we use is quite similar to the one chosen by Cotti et al. (2021), the variation in turnout in our study stems from the natural experiment given by the occurrence of the constitutional referendum in all regions alongside regional and municipality elections happening only in a subset of the sample. An attempt to deal with the endogeneity of turnout is made by Bertoli et al. (2020), which documents a positive effect of turnout on excess mortality by instrumenting the former with the amount of local electoral competition in the context of the March 2020 French municipal elections. The accuracy of Bertoli et al. (2020)'s results is questioned by Bach et al. (2021), who focus on the narrower research question whether there was a causal relationship between active participation and mortality among elderly male candidates to the same French local elections, using individual death records and a regression discontinuity design. Bach et al. (2021) do not

[3] Instead, based on Bayesian statistical techniques, Leung et al. (2020) concludes that the Wisconsin presidential primary election was a relatively safe event.

find any significant effect on candidate politicians' mortality, but the use of a selected sample in their analysis may cast doubts on the external validity of the effect; this is also because the turnout was particularly low, due to the likely strategic risk-compensating behaviour of voters.

With respect to the voters' self-selection problem, Picchio and Santolini (2021) investigate how mortality during the first COVID-19 wave affected turnout for the Italian mayoral elections held in Fall 2020. The authors find that a 1 percentage point increase in elderly mortality rate decreased voter turnout by 0.5 percentage points, with a stronger effect in more densely populated municipalities. Hence, their results reinforce the concerns of endogeneity due to reverse causality and self-selection into voting linked to the local stage of the COVID-19 epidemic.

In another recent paper, Cipullo and Le Moglie (2021) exploit the exogenous schedule of the September 2020 regional elections in Italy to investigate the effect of pre-electoral rallies on COVID-19 spread, whereas we focus on the effect of voter's turnout at the polls. They implement a DiD and an event-study design at the regional level, and find that the number of infections, hospitalizations and deaths due to COVID-19 increased faster since the start of the electoral campaign in Italian regions ahead of the September 2020 regional elections. Differently from our municipality-level setting, the regional-level analysis by Cipullo and Le Moglie (2021) does not allow to explicitly account for the fact that 12% of Italian municipalities held also mayoral elections, which is a factor that may have further contributed to the spread of the contagion.[4]

Overall, these conflicting findings and the quite different approaches implemented in the literature raise a case for a causal analysis like ours, in which identification relies on a natural experiment and the causal effect of interest is identified over the whole population and not a restricted sub-sample. In addition to this, the Control Function approach helps overcoming the unobservables bias due to self-selection of voters into the ballots.

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

Last but not least, James and Alihodzic (2020) investigate the legal foundation of what can be considered the companion research question of our work, i.e. "*when is it democratic to postpone an election*" due to natural disasters like COVID-19. They postulate five main criteria upon which the popular vote must be cast: *full opportunities of deliberation* for the voters; *equality of voters' participation* across social and economic groups; *equality of contestation* giving a level playing field to all candidates; *robust electoral management quality*; and, finally, *institutional certainty*, i.e. clarity about the rules of the game. These criteria have relevant implications that we discuss in Section 6.

2.2. Institutional framework

Italy is organized in 20 regions (NUTS-2 level), whose Presidents are elected every 5 years. Regional governments legislate on all matters related to the provision of health, education and transports, as well as on other fundamental services that are not expressively under the competence of the central Government. At the time of the election events used in this study, Italy comprised 7903 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 President and governments in 7 Italian regions (Campania, Liguria, Marche, Toscana, Puglia, Valle d'Aosta and Veneto). Moreover, citizens with the right to vote were also called to cast a ballot to appoint new mayors and municipality councils in 955 Italian municipalities (across all regions except for Sicily and Sardinia).[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. Importantly, all the above polls had the same pool of voters, i.e. the citizens over 18 years of age.

Figure 1 displays in the left map the regions (in darker blue) and the municipalities (red crosses) undergoing respectively a regional or a mayoral election, and in the right map the different turnout rates for the constitutional referendum across regions. The turnout was always higher where voters were asked both to approve the referendum question and to appoint either the new regional President and/or the new municipality mayor. The political nature of administrative elections cer-

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

[5] Around 70% of Italian municipalities have less than 5000 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.

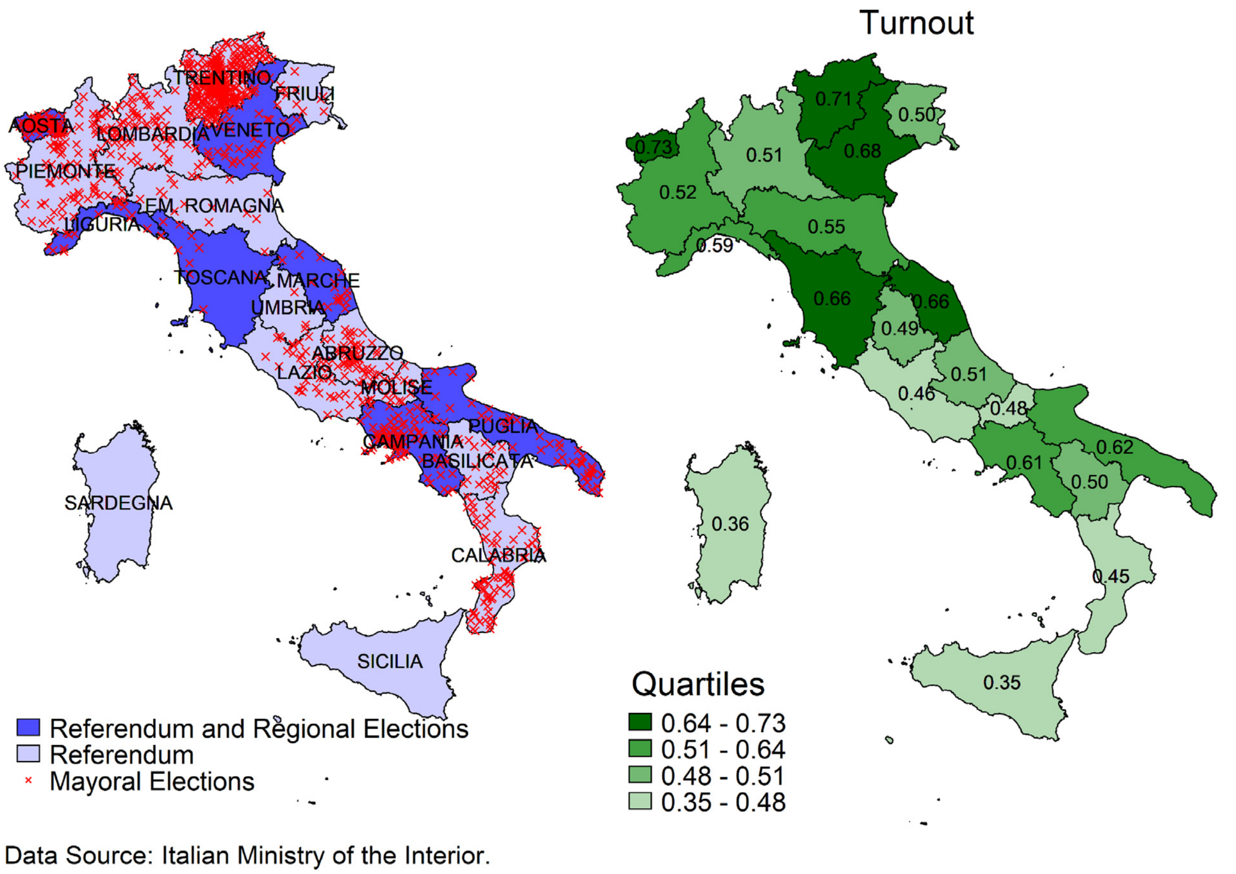


Fig. 1. Regional turnout rates for the constitutional referendum.

tainly led to additional ballots for the referendum that would not be cast otherwise, also because the referendum object enjoyed a wide consensus among most political parties and the general public.[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.

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 7903 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 time frame covers the two months around the election date, namely from the week starting from August 24th to that of October 12th. This period corresponds to four weeks before and four weeks after the date of the September 2020 constitutional referendum.[9] Importantly, and differently from other sources of COVID infections like those reported by the Italian Department of Civil Protection, the date of COVID cases in our weekly sample refer to when the COVID tests were taken. Indeed, the ISS data we use had been collected with the aim to monitor the evolution of the COVID infections, including the formation of infection clusters, and to forecast its future developments over time at the municipality level. Moreover, due to data confidentiality reasons, records have been censored by ISS officials whenever the number of new weekly coronavirus cases is in the range [1,4].[10]

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

[9] Because the constitutional referendum was held on Sunday 20 (polls open from 7AM to 23PM) and Monday 21 September 2020 (polls open from 7AM to 15PM), therefore at the turn of two different calendar weeks, throughout the paper we assume the election week to be the one starting on Monday 14 September 2020 and ending on the first and main election day.

[10] Most of the results provided in this study 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.

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.[11] From the same source, we also collected the municipality-level turnout rates for the previous four elections held nationally[12], which we use in Section 4.3 to construct a proxy for civic capital. To control for the number of schools that are present in every Italian municipality, we instead rely on data collected by the Ministry of Education.[13]

Furthermore, we gathered information on the following municipality characteristics (as of 1st January 2020) from the Italian National Institute of Statistics (ISTAT): number of residents (in total, by gender and by age), orography, altitude from sea level, urbanicity and proximity to the coast.[14] Using ISTAT data, we also construct a measure of excess mortality at the municipality level during the first COVID-19 wave (from March to June 2020), which we use as a matching covariate in Section 3.2. Finally, we gathered data on the weekly number of PCR tests performed by Italian regions during our period of interest. These data are accessible from the official repository of the Italian Department for Civil Protection.[15]

3. Methods

3.1. Baseline model: Fixed-effects Poisson event study

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

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

where i denotes the municipality and r the region it belongs to. t denotes the week, going from 3 weeks before to 4 weeks after the week of the polls[16], this latter denoted by t_0 and used as reference category.

NC_{irt} is the number of new COVID-19 infections in municipality i and week t. APT_i is instead the average municipality-level turnout at the previous four elections held nationally (two referenda, one general election for the Italian Parliament and one general election for the members of the European Parliament). Its inclusion allows us to control for the habitual participation of voters and to identify our effect of interest by exploiting the exogenous variation in the referendum turnout outlined in Section 2.2. Moreover, it controls for the compliance of voters to social distancing rules and NPIs (Durante et al. 2021; Barrios et al. 2021) in the ballot box, if we assume it as a proxy for the municipality-level civic capital as in Putnam et al. (1994).[17]

The vector $\mathbf{X}_{irt}$ also includes the event-study variables of interest, i.e. the interaction of the referendum turnout in municipality i, $TURN_i$, with weekly pre and post poll indicators, alongside other observable confounders that we describe below. The main object of interest is the event-study vector of coefficients γ_t. For $t > t_0$, the coefficients quantify the effect of one point of referendum turnout in excess of APT_i on new coronavirus infections, for each of the four post-poll weeks in our sample.[18] In our setting, most of the within-variation in the excess turnout at the referendum comes from the number of polls held in September 2020, which was scheduled months ahead of the election date(s) and therefore unrelated to the municipality-level epidemic stage. This fact is confirmed by Fig. 2, which compares the growth rate of new weekly COVID-19 infections between treated and control municipalities. Both groups display parallel trends (Card and Krueger, 1993; Dimick and Ryan, 2014; Wing et al., 2018) only until the election week. After then, new COVID-19 infections have accelerated faster in treated municipalities, which on average were characterized by higher turnout rates as a result of the additional voting incentive induced by local administrative elections.

[11] https://dati.interno.gov.it/elezioni/open-data, https://elezioni.interno.gov.it/opendata

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

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

[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.

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

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

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

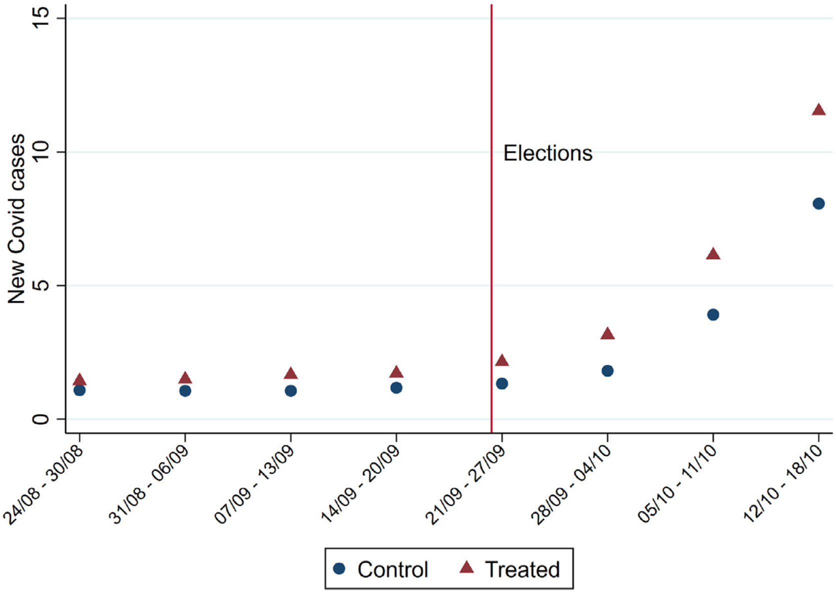


Fig. 2. Trends in new COVID-19 cases.

The variable PCR_{rt} corresponds to the total number of PCR tests performed per 10,000 inhabitants in region r and week t. μ_i and $\sum_{t' \neq t_0} \mathbb{1}(t = t')$ are municipality and week fixed effects, respectively. They control for characteristics that are invariant within municipality (e.g. population) and time (e.g. seasonality) in our sample period. PD_i is instead population density in municipality i, which is interacted with the week indicators to capture its (possibly) time-varying link with COVID-19 spread (see also Carozzi 2020). OCT_{ir} is instead an indicator variable for those few municipalities that had either the first or the second ballot for the mayoral elections on 4th and 5th October 2020. By interacting it with indicators for the last two week in our sample, we control for the effects that this additional electoral round might have had on the spread of COVID-19.

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

3.2. Matching and bias from observables

The previous model assumes that, by controlling for municipality and week fixed effects, the evolution of the COVID-19 outbreak as a function of the referendum turnout rate can be comparable over time across municipalities. However, Table 1 shows that the groups of treated and control municipalities differ substantially not only in the turnout rate for the constitutional referendum, but also in some predetermined characteristics. A legitimate concern is whether these features may contribute to explain the post-polls heterogeneous increase in coronavirus infections displayed in Fig. 2.[20] Although this potential issue should be alleviated by the inclusion of municipality fixed effects, we also estimate Eq. (1) but after pre-processing the data with a nearest neighbor propensity score matching approach without replacement (Rosenbaum and Rubin, 1983; Dehejia and Wahba, 2002; Abadie and Imbens, 2006).

We match municipalities with respect to demographic characteristics, which are known to play an important role in explaining both the turnout rate (Blais, 2006; Geys, 2006; Gallego, 2009; Bhatti et al., 2012) and the severity of COVID-19 symptoms (Bhopal and Bhopal, 2020; Jin et al., 2020), as well as to geographical and urban characteristics, which are factors that can significantly affect COVID-19 transmission (see for instance Gupta et al. 2020; Ahmadi et al. 2020). This approach

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

[20] 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.

Table 1
Summary statistics.

	Treated		Control			
	Mean	Std. Dev.	Mean	Std. Dev.	Δ	*t*-test
Municipality						
Residents	8783.2	(27608.42)	6848.56	(48832.81)	1934.64	1.95*
Share of Female Residents	0.51	(0.02)	0.5	(0.02)	0.00	4.63***
Average Age	46.22	(3.42)	47.04	(3.32)	-0.82	-10.43***
Population Density	0.35	(0.80)	0.28	(0.53)	0.07	4.69***
Average Income (€ 1000)	18.68	(3.89)	18.89	(4.32)	-0.21	-2.13**
Wave I Excess Mortality	0.67	(2.64)	1.4	(3.56)	-0.74	-9.65***
Schools pca	1.47	(1.03)	1.45	(1.14)	0.02	0.75
Turnout	69.03	(8.57)	47.48	(8.56)	21.56	107.52***
APT	57.87	(7.01)	57.7	(7.87)	0.17	0.98
Covid Cases						
Zero cases	0.2	(0.40)	0.28	(0.45)	-0.08	-7.73***
Weekly Covid Rate						
week -3: 24/08 - 30/08	12.93	(54.20)	11.59	(44.06)	1.34	1.19
week -2: 31/08 - 06/09	14.14	(75.12)	12.36	(50.10)	1.78	1.26
week -1: 07/09 - 13/09	15.17	(44.53)	14.53	(66.01)	0.64	0.46
week 0: 14/09 - 20/09	18.08	(58.93)	14.9	(65.97)	3.18	2.14**
week 1: 21/09 - 27/09	18.98	(61.29)	20.74	(117.31)	-1.76	-0.74
week 2: 28/09 - 04/10	29.81	(98.12)	27.24	(183.37)	2.58	0.70
week 3: 05/10 - 11/10	57.88	(150.97)	48.28	(198.16)	9.60	2.25**
week 4: 12/10 - 18/10	104.1	(163.00)	95.48	(184.78)	8.62	2.08**
Municipality-Week observations	22,808		40,416			
Municipalities	2851		5052			

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

allows us to construct a more balanced sample of units in terms of pre-poll characteristics, and to estimate an effect of turnout on COVID-19 spread which is less likely to be confounded by other differences between municipalities.

We obtain estimates of the propensity score for each municipality from a logit regression with an indicator for treated municipalities as dependent variable and the share of female residents, average population age, average municipality income, population density, number of schools per capita of 1000 inhabitants, excess mortality during the first COVID-19 wave, indicators for coastal towns, municipality altitude (i.e. Flat Land, Inner Mountain, Coastal Mountain, Inner Hill, Coastal Hill) and degree of urbanization (i.e. Rural, Small Town, City) as independent variables. Then, we match each treated municipality with a single control unit (where only the constitutional referendum occurred) having the closest propensity score (i.e. nearest neighbor).[21] The nearest neighbor matching is achieved by imposing a caliper of 0.01 in the propensity score, so that only very good matches are retained.

This pre-processing approach implies a considerable reduction in the units of our sample, with 2195 treated municipalities and as many controls. Its summary statistics are reported in Table A.1. The matching approach is successful in making the set of municipalities much more similar between treatment arms, and without any significant difference in the pre-determined demographic or geographical characteristics.[22] A similar conclusion can be drawn from Fig. 3, which instead displays the bias reduction for each covariate following the matching implementation. The most striking improvements are recorded in terms of excess mortality in the first COVID-19 wave, population age and coastal indicator. Overall, the propensity score matching procedure allows us to reduce the overall mean bias in the predetermined time-invariant municipality characteristics between the treatment and the control group from 12.3% to 1.7%. The analysis described in Section 3.1 is then replicated on this matched sub-sample, in order to test the robustness of the findings to the pre-treatment differences between treated and control municipalities.

We also provide results with entropy balance matching (Hainmueller, 2012; Hainmueller and Xu, 2013), which is an alternative matching approach that avoids any sample size reduction. This method generates weights for all the municipalities that had at least one 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 A.2. The baseline model, Eq. (1), is then estimated using the full sample, but with weights produced by the entropy balance approach.

[21] 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 Eq. (1). This is because the municipalities with zero cases in all weeks do not contribute to the likelihood of the model, due to the inclusion of municipality and week fixed effects.

[22] This is also confirmed by Fig. A.1, comparing the propensity score distributions before and after the matching is applied.

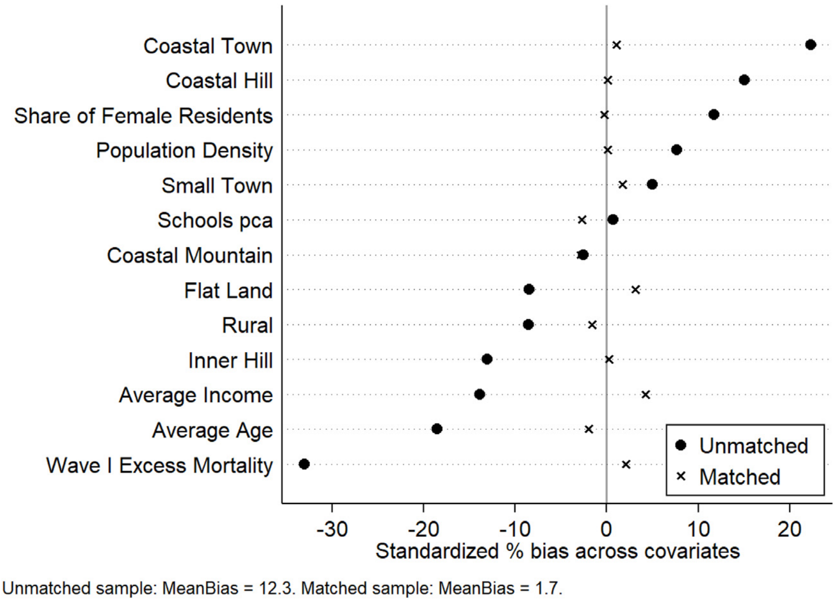


Fig. 3. Covariate bias reduction after matching.

3.3. Control Function and bias from unobservables

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

There is a wide array of factors related to municipal population that we cannot explicitly control for, e.g. the mobility of residents, the share of commuters and the propensity to indulge in risky behaviors. Such latent factors could contribute to explain both the turnout rate and the trajectory of COVID-19 spread at the municipality level. In particular, a modified attitude to risk is one of our main concerns, given the results by Picchio and Santolini (2021) showing that Italian municipalities with a higher excess mortality among the elderly experienced a decrease in turnout at the mayoral elections, especially in densely populated areas.[23]

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

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

We then estimate the second-stage Poisson regression as:

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

where $\hat{r}_i = TURN_i - \widehat{TURN}_i$ are the estimated residuals from the first-stage model for the referendum turnout rate (2).[24]

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

[24] Other, more complex Control Function approaches have been suggested to identify the average treatment effect (ATE) or the average treatment effect among the treated (ATT) when the endogenous regressor of interest is continuous. For example, Florens et al. (2008) use a non-parametric strategy and

TR_i is the instrumental variable that we use to identify the model in Eq. (3), as it provides a legitimate and significant source of exogenous variation in the municipality-level turnout rate at the constitutional referendum (see also Table A.3).[25] In the second stage, we interact the predicted residuals $\hat{r}_i$ with the week indicators to control for the time-varying effects of unobservables that might still pollute our estimates.[26]

The standard errors of the second-stage outcome Eq. (3) are bootstrapped with 1000 replications and clustered at municipality-level to account for the two-step procedure (Murphy and Topel, 1985).

3.4. Spatial spillover effects in COVID-19 infections

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

First, we compute a spatial weighting matrix (Anselin, 2001; LeSage, 2015) whose entries record the geographic distance of each municipality from its neighbors.[27] 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). The second-stage of our Control Function model is then augmented with this additional covariate, which is meant to control for the spatial spillover effects of coronavirus clusters.

4. Results

4.1. Summary statistics

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. Fig. 4 plots the incidence rates of COVID-19 in the four weeks preceding and in the four weeks following the polls. The most prominent rise in contagion occurred in Valle d'Aosta, which suffered an increase from 48 to 525 new coronavirus cases every 100,000 inhabitants. Remarkable rises in infections were also recorded in Campania and Toscana, where new COVID-19 infections went from approximately 70 to more than 300 every 100,000 inhabitants. Among regions where no regional elections took place, Umbria is where the outbreak worsened the most, since new cases went from 63 to 314 every 100,000 inhabitants within a few weeks.

Our sample is made of a total of 2851 treated municipalities and 5052 control ones. Summary statistics for these two groups of units are provided in Table 1. 20% of the former municipalities does not record any new COVID-19 infection in the period under study. This share is higher and equal to 28% in the control group. On average, treated municipalities have more residents than control municipalities. Usually, they also have a higher share of female residents and a younger population. The average turnout in the four past national-level elections (i.e. APT) was 58% for both groups of municipalities. Treated municipalities present, on average, a higher population density and a slightly greater number of schools per capita. They were also hit less by the first wave of COVID-19 in Spring 2020, as this wave hit fiercely some Northern Italian regions like Lombardia, Piemonte and Emilia-Romagna, whose municipalities mostly belong to the control group, as these were regions where only the constitutional referendum took place in September 2020.

4.2. Baseline fixed-effects Poisson regression model

The estimates of Eq. (1) are provided in Table 2. Panel A reports the event-study coefficients. We do not find any significant pre-trend as a function of the referendum turnout. On the contrary, we do find semi-elasticities for the turnout-week interactions in the post-poll period that are significant at least at the 5% level, showing how higher voters' participation in September 2020 contributed to the spread of COVID-19 infections. These results are quite consistent regardless of whether

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.

[25] In analogy with the LATE framework (Imbens and Angrist, 1994; Angrist and Imbens, 1995; Angrist et al., 1996), the variation in the referendum turnout rate induced by TR_i, conditional on the other controls included in Eq. (2), represents the share of voters acting like *compliers*, i.e. voters who cast their vote for both the referendum and the administrative elections only because they had an incentive to vote for the regional or mayoral government, but who

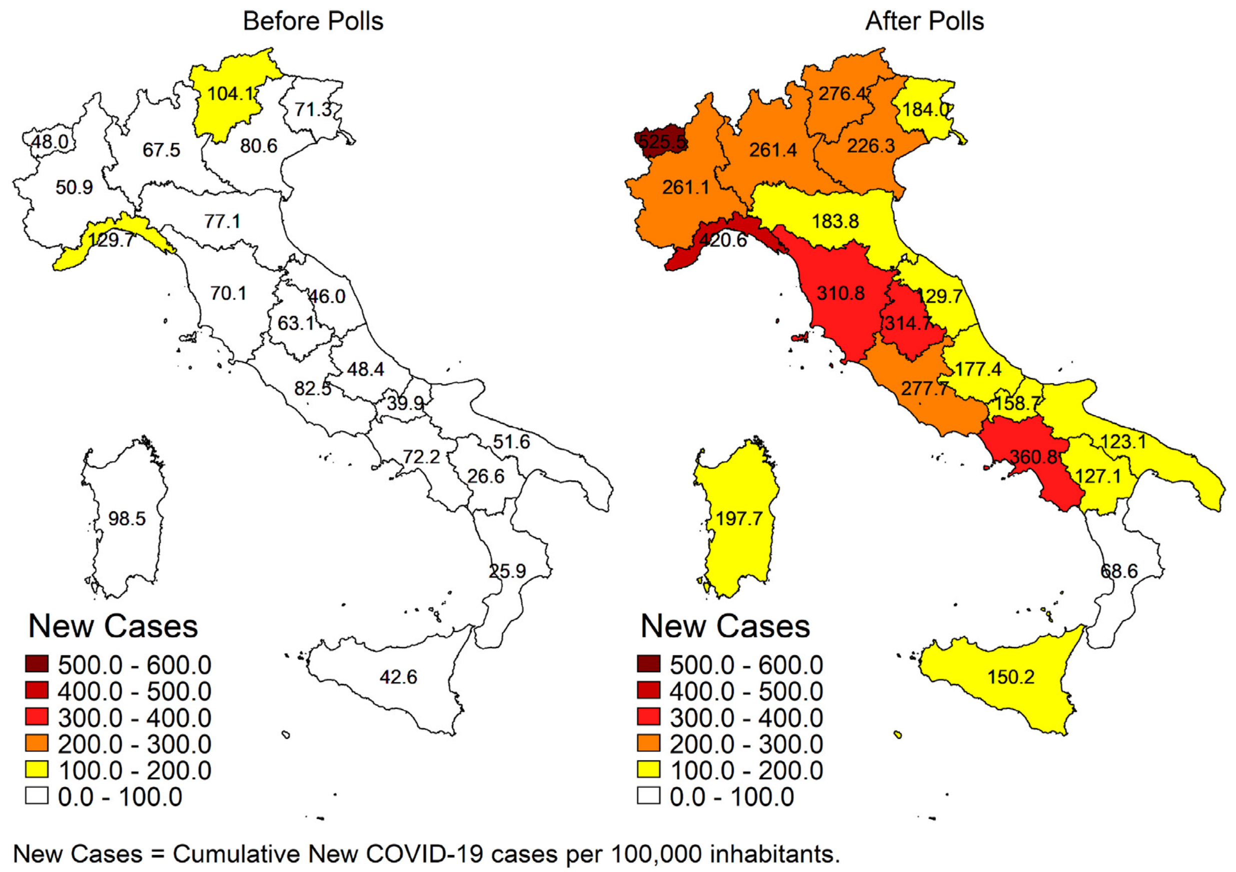


Fig. 4. Regional COVID-19 rates around the election date.

the sample is matched or not, although the magnitude of the semi-elasticities gets smaller especially after the implementation of the entropy balance weighting scheme. For instance, Column 1 indicates that one additional point in the referendum turnout was associated with a 1.3% increase in new COVID-19 infections after two weeks from the polls. Given that COVID infections reported by the ISS refer to the date when the COVID test was taken, the effects estimated by our event-study, i.e. a mild increase in infections due to the turnout in Week 1 and a more pronounced effect of the turnout from Week 2 onwards, are consistent with symptom onsets occurring about five days after exposure to old COVID-19 strains (Lauer et al., 2020; McAloon et al., 2020) plus one or two additional days needed to arrange for a test, in a period of high capacity and low infection rates levels as September 2020.

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

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

would have not voted for the constitutional referendum otherwise. Hence, the estimates of the effects of interest from Eq. (3) can be considered as local treatment effects, while the estimates from Eq. (1) represents a general treatment effect over the Italian municipalities.

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

[27] Specifically, the rows of this 7903x7903 matrix contain the inverse distances of a given municipality from all the remaining ones in the sample.

Table 2
Effects of Turnout on COVID-19 infections.

	New COVID-19 cases					
	(1)		(2)		(3)	
Panel A: Event-Study						
3 weeks pre-poll * Turnout	0.001	(0.004)	0.001	(0.004)	-0.001	(0.004)
2 weeks pre-poll * Turnout	0.001	(0.004)	0.002	(0.004)	-0.002	(0.004)
1 week pre-poll * Turnout	0.004	(0.003)	0.003	(0.003)	0.001	(0.003)
1 week post-poll * Turnout	0.008***	(0.002)	0.005**	(0.002)	0.007***	(0.002)
2 weeks post-poll * Turnout	0.013***	(0.003)	0.010***	(0.003)	0.010***	(0.003)
3 weeks post-poll * Turnout	0.011***	(0.004)	0.010***	(0.004)	0.007**	(0.004)
4 weeks post-poll * Turnout	0.015***	(0.003)	0.014***	(0.004)	0.011***	(0.003)
3 weeks pre-poll * APT	0.014*	(0.009)	0.019***	(0.007)	0.014*	(0.008)
2 weeks pre-poll * APT	0.008	(0.006)	0.006	(0.006)	0.012**	(0.006)
1 week pre-poll * APT	0.004	(0.004)	0.006	(0.004)	0.004	(0.004)
1 week post-poll * APT	-0.021***	(0.004)	-0.012***	(0.004)	-0.021***	(0.005)
2 weeks post-poll * APT	-0.018***	(0.005)	-0.009	(0.006)	-0.022***	(0.006)
3 weeks post-poll * APT	-0.005	(0.005)	-0.002	(0.006)	-0.005	(0.005)
4 weeks post-poll * APT	0.003	(0.005)	0.006	(0.005)	0.001	(0.005)
Panel B: DiD						
Post-poll	0.854***	(0.212)	0.560**	(0.232)	1.055***	(0.203)
Post-poll * Turnout	0.015***	(0.003)	0.012***	(0.003)	0.014***	(0.003)
Post-poll * APT	-0.016***	(0.004)	-0.008*	(0.004)	-0.017***	(0.004)
Sample	Unmatched		Matched (NN)		Matched (EB)	
Treated Municipalities	2267		2195		2267	
Control Municipalities	3620		2195		3620	
Municipality-Week observations	47,096		35,120		47,096	

Notes: Fixed-effects Poisson semi-elasticities in the full sample (Column 1), nearest neighbor matched subsample (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 Fig. 3. 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 Section 3.3, whereas the corresponding semi-elasticities and the first stage key coefficients are reported in Table A.4.

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

From the first stage regression in Panel A of Table A.4, we note how the 2020 referendum turnout is positively associated with both the ‘treatment’ indicator for regional or mayoral elections and the civic capital proxy. Instead, it is negatively associated with both high excess mortality during the first COVID-19 wave (March to June 2020) and population density. These estimates suggest that voters were sensitive to the incentive to cast their referendum ballot in municipalities subject to an additional administrative election. Moreover, voters acted strategically choosing to show themselves at the ballots according to their expected gains from the trade-off between exercising their right to vote, which is likely a positive function of civic capital, and risking to catch COVID-19, which is positively associated with a high first wave excess mortality and high population density, especially for the elderly. This strategic choice at municipal population level is consistent with the associations between the 2020 voters’ turnout and the first wave excess mortality shown by Picchio and Santolini (2021).

Despite the interactions between the first-stage residuals and the post-poll week indicators are statistically significant, the semi-elasticities for turnout with Control Function are almost identical to the point estimates of Eq. (1) reported in Table 2. This is also informative of the fact that the estimates of local treatment effects obtained through the Control Function strategy are not very different from the ATT effect measured by Eq. (1).

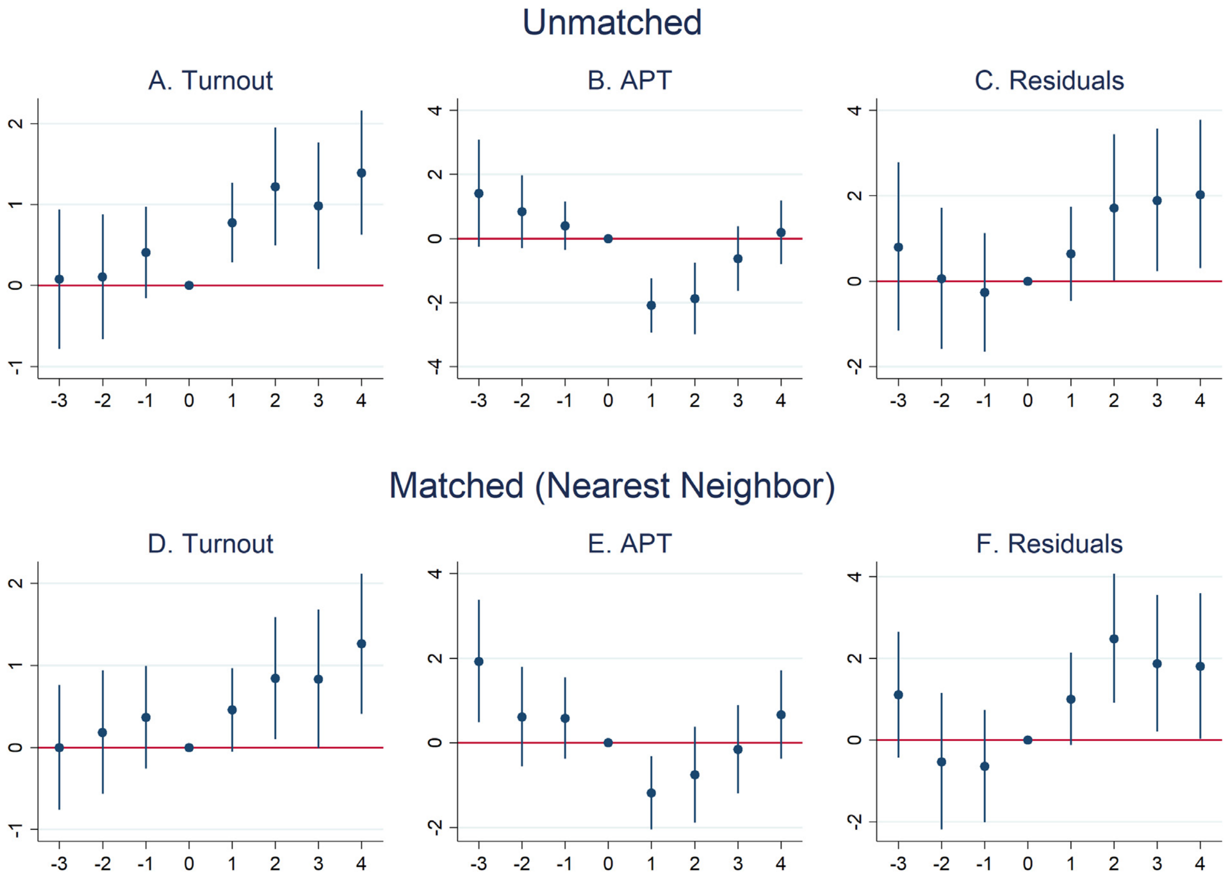


Fig. 5. Effects of Turnout with Control Function: Elasticities and 95% Confidence Intervals.

4.4. Controlling for spatial autocorrelation in COVID infections

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

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

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

4.5. Cost-benefit simulation: Healthcare expenditures and lives saved from preventing a national-level general election

An additional, policy-relevant research question is the quantification of the impact of electoral turnout on infections if ballots occur during a high-infection rate regime. The answer to such question cannot be quantified directly through our quasi-experiment, and it would be problematic also when using cross-national data on electoral turnout and infection spread, because governments can choose to hold or postpone elections based on the epidemic stage, i.e. due to selectivity in the timing of the voting, which cannot be easily overcome. However, based on the results of the event-study analyses above, real political events in the recent Italian history and a series of assumptions, we are able to undertake a cost-benefit scenario simulation shedding some light on this issue. By doing so, we quantify the likely monetary and non-monetary costs (i.e. lives lost) implied by holding national-level elections during a period of high infection rates and higher transmissibility of a virus.

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

	W^{10km}	W^{30km}	W^{60km}
	(1)	(2)	(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	2267	2267	2267
Control Municipalities	3620	3620	3620
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 (1000 iterations) in parenthesis. Significance levels: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

In January 2021 the Italian coalition Government in charge collapsed over disputes among its supporting political parties about the plans for spending the EU recovery funds to face the COVID crisis.[28] The two scenarios that opened up were either the appointment of a new coalition Government, or having early nation-wide general elections to renew the members of the Italian Parliament. We exploit these political events and simulate a real-case scenario of the 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. The set of assumptions (***A***) on which our calculations are based are reported in Section A.1. 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 results of the cost-benefit simulation are reported in Table 4. The upper panel (*Panel A*) reports the main inputs for the computations. The lower panel (*Panel B*) reports the estimates of interest in terms of prevented new COVID-19 cases, ICU and non-ICU hospitalizations, and lives saved. For brevity’s sake, the way we calculated each table entry is explained in the notes of Table 4; the results also draw upon the computations from Table A.9, 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.

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

Table 4

Cost-benefit simulation of the impact of avoiding national level political elections during a high-infection regime.

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	€ 9796	83.57	€ 74,159.00	1.5
Panel B: Estimates	**Coefficient estimates (K2)**	**Coefficient standard error (L2)**	**COVID-19 strain (M2)**	**Predicted Additional Cases (N2)**	**Predicted averted additional non-ICU hospitalizations (O2)**	**Predicted averted additional costs (€) of non-ICU hospitalizations (P2)**	**Predicted averted additional ICU hospitalizations (Q2)**	**Predicted averted additional costs (€) of ICU hospitalizations (R2)**	**Predicted lives saved (S2)**	**Predicted value (€) of lives saved (T2)**
Post-poll (DiD)	0.011 [0.005; 0.018]	0.003	Pre-B.1.1.7	481,443 [218,182; 790,585]	22,869 [10,364; 37,553]	193,837,644 [87,845,264; 318,299,228]	5585 [2531; 9171]	47,384,460 [21,452,756; 77,733,396]	15,262 [6916; 25,062]	5,026,053,605 [2,277,564,325; 8,253,371,475]
			B.1.1.7	722,165 [327,272; 1,185,878]	34,303 [15,545; 56,329]	290,752,228 [131,759,420; 477,444,604]	8377 [3796; 13,756]	71,003,452 [32,174,896; 116,595,856]	22,893 [10,375; 37,592]	7,539,080,408 [3,416,675,806; 12,379,727,895]

Notes: 95% confidence intervals bounds in squared brackets. (A2): The number of new coronavirus infections in the whole Italy between March 1, and March 28, (4 weeks); data source: Italian Civic Protection Department. (B2): Ordinary hospitalizations / currently infected, i.e. the average share of (total) infected people by COVID-19 requiring non-ICU hospitalization between March 1, and March 28, (4 weeks); data source: Italian Civic Protection Department. (C2): New ICU admissions / New infections, i.e. the average share of new infected people by COVID-19 requiring ICU between March 1, and March 28, (4 weeks); data source: Italian Civic Protection Department. (D2): Raw one week Case Fatality Rate (CFR), i.e. the number of dead among the number of diagnosed COVID-19 cases only, as estimated by Our World in Data (https://ourworldindata.org/mortality-risk-covid?country=~ITA) based on COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University. (F2-G2) Source: estimates by the ALTEMS research team (https://altems.unicatt.it/altems-Report%2046-compresso.pdf). (H2) Source: https://www.macrotrends.net/countries/ITA/italy/life-expectancy. (I2) Source: Ryen and Svensson (2015). (J2) Source: Volz et al. (2021). (K2-L2) Source: authors computations, Table 3. Cells in (N2) = $100 * [exp(K2) - 1] * (A2)^* (E2)$. (O2) = (N2)*(B2). (P2) = (O2)*(F2). (Q2) = (N2)*(C2). (R2) = (Q2)*(F2). (S2) = (N2)*(D2). (T2) = (N2)* € 329,318.15 as computed in Table A.9, 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 A.9.

According to our preferred summary estimate of the effect of interest (i.e. the DiD specification based on the Control Function model after nearest neighbor matching and the virus trasmission of the COVID variant B.1.1.7), an early general election in the Spring 2021 would have generated up to additional 722,165 COVID-19 infections in Italy within four weeks from the election date. This increase would have translated into approximately 8377 ICU (**Q2**) and 34,303 non-ICU (**O2**) hospitalizations, which imply monetary costs worth respectively about € 71.003 millions (**R2**) and € 290.752 millions (**P2**) for the Italian NHS, i.e. a total of € 361.755 millions. This sum is not negligible and equal to 1.79% of the total hospital admissions costs sustained by the Italian State from the start of the epidemic till end of March 2021.[29] Moreover, the additional death toll would have been equal to 22,893 (**S2**), corresponding to a value of about € 7.539 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.

5. Robustness checks

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

5.1. Left-censoring of the outcome variable

First, we check how results change if we treat censored values in the number of new weekly COVID-19 cases differently. This analysis is crucial, because 30,59% of non-zero weekly municipality infections in our sample are censored in the interval [1,4] for privacy reason by the data provider (ISS). To do so, we examine how our baseline estimates for Eq. (1) vary: (i) in the worst and in the best case scenarios, namely when we replace the censored values respectively with new weekly infection values of 4 and 1; (ii) and when we randomize censored coronavirus infections using 2000 draws from a uniform distribution with 1 and 4 as extreme values, clustered by each province-week pair in our sample.[30]

Results for these alternative specifications are provided in Table A.5, while the elasticities of interest are displayed in Fig. 6. 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.

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

Second, we provide alternative specifications with respect to the PCR tests control variable. Indeed, the latter may depend on the stage of the epidemic, thus it might also be affected by the occurrence of the polls. For this reason, in Table A.6 we report estimates of variants of Eq. (3), where the variable PCR_{rt} has been either omitted (Column 1) or replaced with either: (i) the “frozen” average number of regional tests performed in the first three pre-poll weeks, interacted with a post-poll indicator (Columns 2); or (ii) the total number of regional PCR tests performed, but weighted by municipal population density (Column 3).

These specifications provide different ways to deal with the possibility that PCR_{rt} might eventually be considered a bad control in our models, despite such variable is measured at a higher aggregation level (regional) than the treatment of interest (municipality). All estimates from these three alternative specifications provide very similar coefficients of interest on the turnout-week interactions, which are in line with the coefficients discussed in Section 4.3. The only exception is the point estimate for the fourth week post polls, which is smaller in Table A.6. As such, it seems that the effect of turnout on COVID-19 spread does not depend on controlling for the number of PCR tests run.

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

The treatment examined in this paper falls exactly around the Italian school opening date, which happened in most regions on the Monday after the polls.[31] Thus, it is important to check for the possible confounding of school openings in our effect of interest.[32] To do so, we augment Eq. (3) by interacting the week indicators with the time-invariant number of schools in a given municipality. The results of these specifications are provided in Table A.7, where we use the number of schools in Column 1, and the number of schools per capita in Column 2.

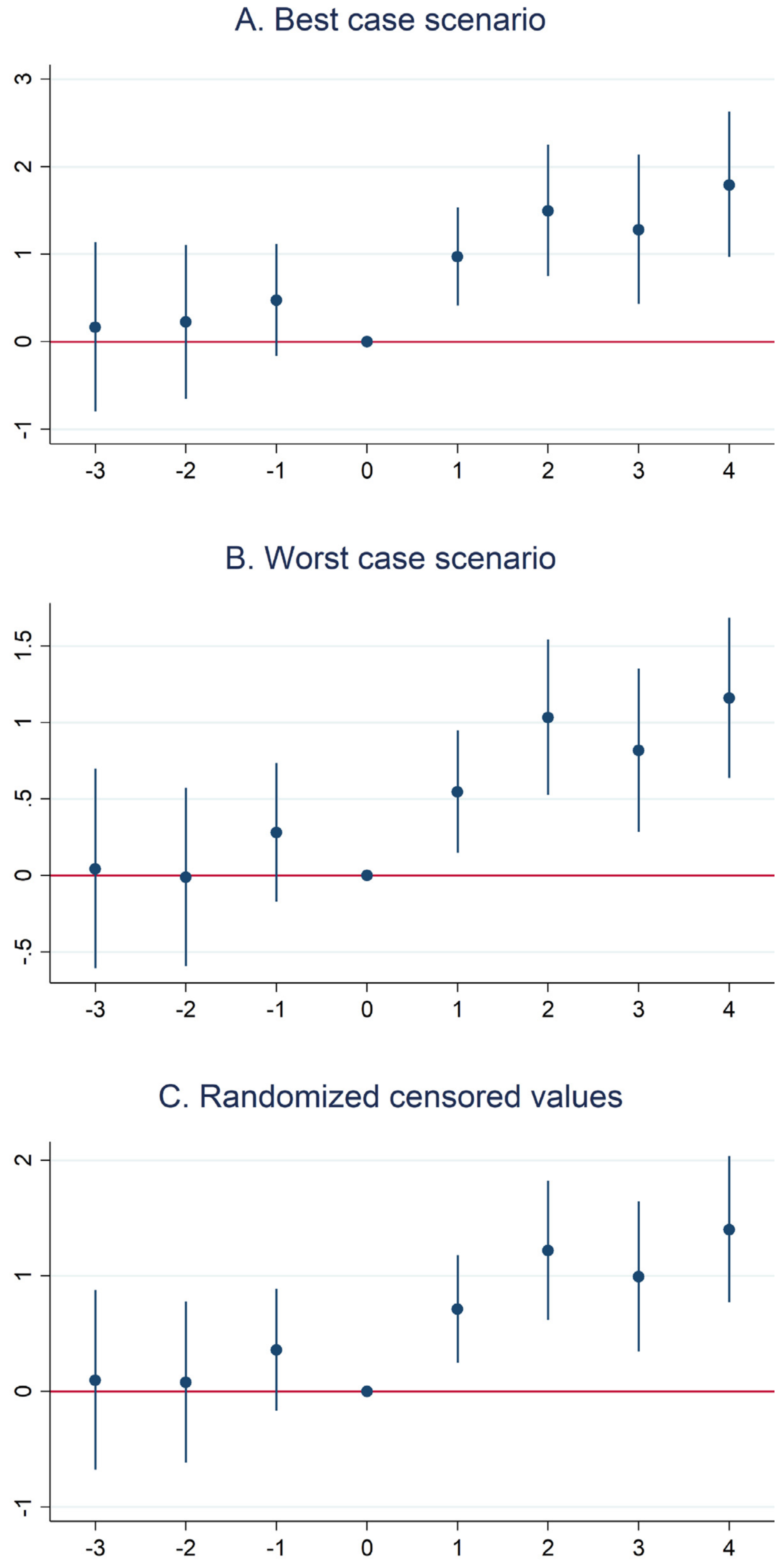
We find a positive and significant relationship between school density and new weekly COVID-19 infections only if we weigh the number of schools by municipality population, at least in the first two weeks following the polls. Nevertheless,

[29] € 20,153,168,964 as estimated by the ALTEMS research team, based on Diagnoses Related Group (DRG) (https://altems.unicatt.it/altems-Report%2046-compresso.pdf).

[30] 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.

[31] 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.

[32] On the contrary, there are no concerns related to holiday periods or mid-term school breaks, which are known to affect students' mobility and consequently COVID-19 spread (Mangrum and Niekamp 2020) but did not occur during the short time window under analysis.



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

Fig. 6. Robustness checks to left censoring.

our main coefficients of interest are significant and mostly unchanged in magnitude. Hence, the re-opening of schools cannot explain the findings of this study.

5.4. Including time-varying effects of all predetermined variables

Table A.8 tests the robustness of our findings by including in Eq. (3) the interactions between the week indicators and all the predetermined municipality characteristics included in the first stage explaining the municipality turnout. The post-poll effects of interest are still significant, although slightly smaller in magnitude in the third and fourth weeks post-polls than those reported in Table A.4. This finding indicates how our results hold even after controlling for the time-varying effect of a rich set of demographic and geographic characteristics at the municipality-level on the spread of COVID-19.

6. Conclusions

Up until recently, there was no available clear-cut evidence about the effects of holding polls on the spread of highly infectious airborne diseases, as during the current pandemic. This lack of evidence has left the choice of whether to hold or postpone forthcoming elections to the discretion of politicians and their public health advisers. Our study tries to fill this gap, providing one of the first causal estimates of the effect of voters' turnout on the spread of COVID-19. By exploiting an exogenous variation in the turnout rate stemming from the heterogeneous number of polls held in September 2020 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 rate for the constitutional referendum was associated with at least an average 1.1% increase in post-poll COVID-19 infections.

Our findings are robust to a series of sensitivity checks like the pre-processing of the sample by multiple matching approaches or the inclusion of spatial lags in the number of coronavirus infections to control for the spatial spillovers of coronavirus clusters. Results including a Control Function reveal the presence of self-selection of voters based on unobservables that are correlated with COVID-19 spread, but the Control Function estimates of the effect of turnout are similar to those obtained via the baseline Poisson event-study model. This finding is likely explained by two main reasons. First, the endogeneity of turnout is primarily dealt by exploiting a natural experiment as the source of exogenous variation in turnout; this alone yields a great deal of robustness against the endogeneity bias that we want to prevent. The incremental use of methods as nearest neighbour matching and control function in our strategy is then done with the purpose of controlling for additional sources of bias, respectively from voters' selection-on-observables and selection-on-unobservables. These two issues depend on population characteristics at the municipality level likely correlated with the spread of COVID-19 and cannot be entirely eliminated by the exogenous variation in turnout due to the natural experiment. Second, we examine a period of low infection rates, when the compensating behavior by electors might have either been not particularly strong or might have not mattered much for contagion. Indeed, we find evidence of voters' compensating behaviour based on population density and the excess mortality during the first COVID-19 wave: it simply does not impact substantially our estimates on turnout, possibly because the low infection regime decreased not only the baseline infection risk, but also the perception of such risk among prospective voters.

In terms of mechanisms, we deem unlikely that the contagion effect found by our analysis and linked to the polls' turnout is due to pre-electoral rallies, as the exogenous variation in turnout identified by the type of elections held in Italian municipalities in September 2020 (a constitutional referendum and administrative elections for the new municipality mayor and regional government) does not seem to explain the pre-poll evolution of the epidemic. In the absence of individual-level, experimental data with records of voters' behavior, actions and choices, we speculate that there are two likely mechanisms at play for the poll-related infection spread: the lack of abidance to NPIs while at the ballots, and the lack of abidance to NPIs after the ballots. Both cases would arise from instances like the incorrect use of masks or the lack of social distancing between people while queuing to vote or post-vote gatherings.

Overall, our study suggests that national-level polls might contribute to the spread of airborne diseases like COVID-19, and thus they can spark national waves of contagion if held during peak periods of an epidemics. These findings are in line with a recent analysis by Palguta et al. (2022), who exploit a similar institutional setting in the Czech Republic to examine the epidemic effects of the second round of the 2020 Senate elections, which were held only in a random subset of all the national constituencies. We provide an estimate of the causal effect of turnout on new COVID-19 infections, which is informative for policy-makers about the public health consequences of holding in-person polls during a pandemic, given an expected turnout rate. This is a subtle but important point, as knowing the impact of holding elections at a given turnout rate rather than not holding them at all provides politicians and public health policy-makers a way to quantify the likely disruption for holding the elections, hence a way to assess whether such elections are better to be postponed. In this regard, and based on our estimates, we provide a cost-benefit calculation of the monetary and lives-saving gains from having averted national-level general elections in Italy in the first months of 2021, following the collapse of the coalition Government in charge till January 2021. Our simulation suggests that the appointment of a government of national unity and the prevention of an early general election might have spared Italy up to € 361.755 millions on hospital care costs and € 7.539 billions in terms of value of lives lost to COVID. This is possibly the opposite of what happened between March and April 2021 in India, when the country experienced a record surge in COVID-19 infections, hospitalizations and deaths concomitantly with campaign rallies and voting for a series of state and local council elections. Hence, the cost-benefit figures that we show also represent what James and Alihodzic (2020) define as a “humanitarian case” for postponing elections, given the inevitable trade-off involved by holding in-person elections during a pandemic between the exercise of the democratic right to vote versus the value of individual and public health.

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. This is because the first-stage equation of our Control Function strategy shows that the turnout rate was likely affected by inequalities due to a number of municipality characteristics, like population density and the latent health frailty proxied by the excess mortality experienced during the first COVID-19 wave. Whether any of the other four criteria (i.e. full deliberation, equality of contestation, robust electoral management quality and institutional certainty) postulated by James and Alihodzic (2020) was also compromised, during the Italian polls we studied or other in-person ballots held over the global COVID-19 pandemic, is instead an interesting question that is open for future research.

Finally, our investigation suggests that there are benefits from the adoption of voting methods such as electronic or postal voting, especially under extreme circumstances. Resorting to these voting methods is considered risky in countries like Italy, due to the potential influence on the vote by the organized crime. However, at least during emergency situations such as a pandemic, these alternative voting methods should be considered as a valid solution both to protect the most vulnerable groups of the population and to guarantee an equal access to the ballots to all voters.

Declaration of Competing Interest

None.

Appendix A. Appendix

A1. Cost-benefit simulation assumptions.

- ***A1***. The early election should have occurred by early to mid-March 2021. 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.
- ***A2***. Given the necessary constraints to a general election spelled in ***A1***, the baseline value of new COVID-19 cases is the total number of new infections registered in Italy during the first four weeks of March 2021.[33]
- ***A3***. The case fatality rate (CFR) is equal to the one observed in March 2021, according to computations based on the COVID-19 Data Repository at Johns Hopkins University[34]
- ***A4***. The transmisibility of the strain B.1.1.7 to be only 50% higher than pre-existing lineages, which corresponds to the lower bound of this strain’s transmissibility found by Volz et al. (2021) and Davies et al. (2021), whereas the estimated upper bound was either a 90% or 100% higher virus transmissibility.[35]
- ***A5***. The expected life lost by COVID-19 patients older than 80 years is set to zero, 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.
- ***A6***. As shown in Table A.9, patients over 75 years old 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.
- ***A7***. The post-election spread of the virus is assumed to follow the DiD point estimate valued 0.011 (from the CF model with a pre-processed sample through nearest neighbor matching, as reported in Table A.4, third Column, Panel B) based on the monthly effect of the 2020 referendum turnout variable.
- ***A8***. 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.
- ***A9***. The cost-benefit simulation provides only 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.[36]
- ***A10***. Voters’ attitude towards COVID-19 infection risk would have been similar in September 2020 and in the averted general elections in March 2021.[37]

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

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

[35] 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).

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

[37] Despite this assumption may seem rather strong, it is counterbalanced by the conservativeness of assumptions A4-A9.

A2. Additional tables and figures.

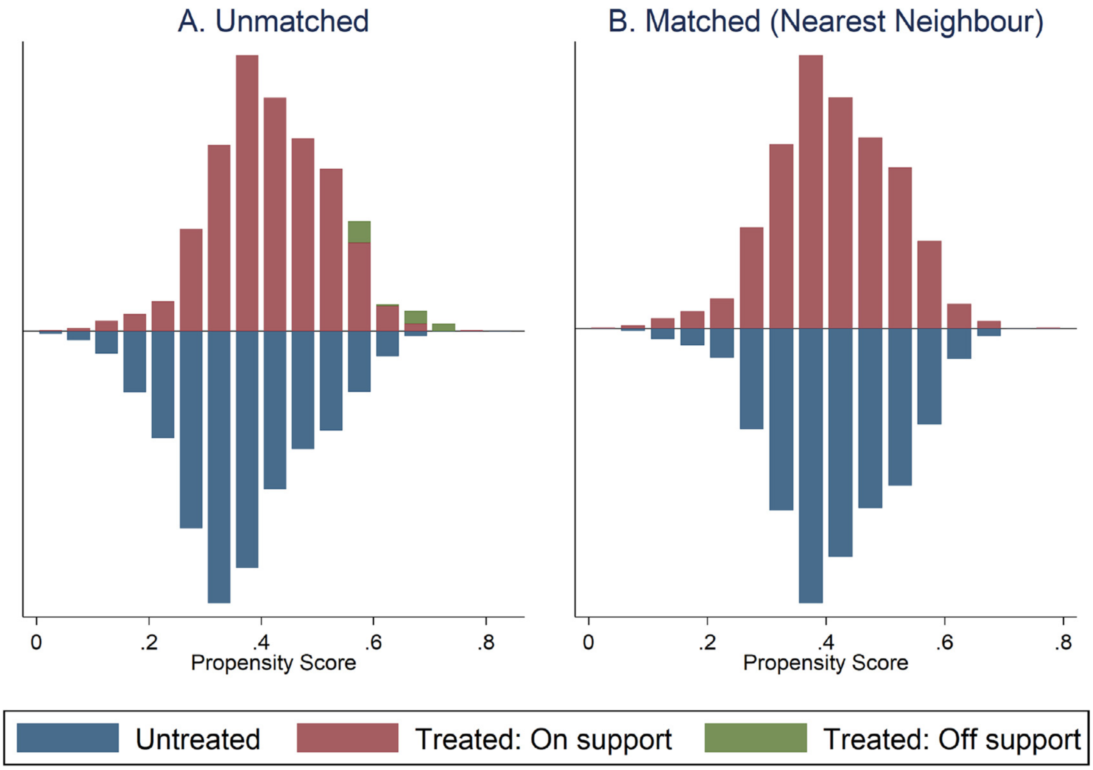


Fig. A.1. Propensity score distributions.

Table A.1
Summary statistics in the matched sub-sample (nearest neighbor).

	Treated		Control			
	Mean	Std. Dev.	Mean	Std. Dev.	Δ	*t*-test
Municipality						
Residents	10078.04	(23557.95)	10779.26	(66798.29)	-701.22	-0.46
Share of Female Residents	0.51	(0.01)	0.51	(0.01)	-0.00	-0.10
Average Age	45.8	(3.02)	45.86	(2.61)	-0.06	-0.68
Population Density	0.37	(0.70)	0.37	(0.64)	0.00	0.04
Average Income (€ 1000)	19.34	(3.77)	19.17	(4.18)	0.17	1.41
Wave I Excess Mortality	0.69	(1.97)	0.64	(2.26)	0.05	0.82
Schools pca	1.37	(0.82)	1.39	(0.82)	-0.02	-0.91
Turnout	68.47	(7.94)	46.04	(8.57)	22.43	89.94***
APT	58.75	(6.59)	57.37	(7.75)	1.37	6.33***
Weekly Covid Rate						
24/08 - 30/08	16.47	(61.22)	14.53	(47.81)	1.94	1.17
31/08 - 06/09	17.72	(84.60)	16.44	(50.07)	1.29	0.61
07/09 - 13/09	19.2	(49.71)	18.16	(58.85)	1.04	0.63
14/09 - 20/09	23.12	(66.23)	18.88	(68.17)	4.24	2.09**
21/09 - 27/09	23.84	(68.65)	30.16	(157.12)	-6.32	-1.73*
28/09 - 04/10	37.35	(109.62)	39.9	(251.64)	-2.55	-0.44
05/10 - 11/10	72.86	(168.12)	68.32	(275.82)	4.55	0.66
12/10 - 18/10	129.42	(173.00)	128.94	(202.64)	0.48	0.08
Municipality-Week observations	17,560		17,560			
Municipalities	2195		2195			

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

Table A.2
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 A.3
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	7903	7903	7903	7903	7903

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 A.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: 2st 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: 2st 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	2851		2267		2195	
Control Municipalities	5052		3620		2195	
Municipality-Week observations	7903		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 Fig. 3. Municipality-level clustered bootstrapped standard errors (1000 iterations) in parenthesis. Significance levels: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.5
Robustness checks for censored values.

	Best case scenario	Worst case scenario	Randomization
	(1)	(2)	(3)
3 weeks pre-poll * Turnout	0.002	0.000	0.001
	(0.005)	(0.003)	(0.004)
2 weeks pre-poll * Turnout	0.002	-0.000	0.001
	(0.004)	(0.003)	(0.004)
1 week pre-poll * Turnout	0.005	0.003	0.004
	(0.003)	(0.002)	(0.003)
1 week post-poll * Turnout	0.010***	0.005***	0.007***
	(0.003)	(0.002)	(0.002)
2 weeks post-poll * Turnout	0.015***	0.010***	0.012***
	(0.004)	(0.003)	(0.003)
3 weeks post-poll * Turnout	0.013***	0.008***	0.010***
	(0.004)	(0.003)	(0.003)
4 weeks post-poll * Turnout	0.018***	0.012***	0.014***
	(0.004)	(0.003)	(0.003)
Sample	Unmatched	Unmatched	Unmatched
Treated Municipalities	2267	2267	2267
Control Municipalities	3620	3620	3620
Municipality-Week observations	47,096	47,096	47,096
CF	No	No	No

Notes: Fixed-effects Poisson semi-elasticities in the full sample. Censored number of COVID-19 infections replaced with 1 in Columns 1 and 2. Censored number of COVID-19 infections replaced with 4 in Columns 3 and 4. Randomized (2000 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 A.6
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	2267	2267	2267
Control Municipalities	3620	3620	3620
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: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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Table A.7
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	2267	2267
Control Municipalities	3620	3620
Municipality-Week observations	47,096	47,096
Schools	Number of Schools	Number of Schools per 1000 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 A.8
Fully-interacted Control Function.

	New COVID-19 cases	
	(1)	
Panel B: 2st Stage Event-Study Design		
3 weeks pre-poll * Turnout	0.003	(0.004)
2 weeks pre-poll * Turnout	0.002	(0.004)
1 week pre-poll * Turnout	0.004	(0.003)
1 week post-poll * Turnout	0.005**	(0.003)
2 weeks post-poll * Turnout	0.009**	(0.004)
3 weeks post-poll * Turnout	0.007*	(0.003)
4 weeks post-poll * Turnout	0.010***	(0.003)
3 weeks pre-poll * APT	-0.001	(0.009)
2 weeks pre-poll * APT	0.001	(0.008)
1 week pre-poll * APT	0.009	(0.006)
1 week post-poll * APT	-0.010*	(0.006)
2 weeks post-poll * APT	-0.010	(0.008)
3 weeks post-poll * APT	-0.004	(0.007)
4 weeks post-poll * APT	-0.000	(0.007)
3 weeks pre-poll * Residuals	0.008	(0.008)
2 weeks pre-poll * Residuals	0.001	(0.008)
1 week pre-poll * Residuals	-0.001	(0.006)
1 week post-poll * Residuals	0.009*	(0.005)
2 weeks post-poll * Residuals	0.022***	(0.007)
3 weeks post-poll * Residuals	0.023***	(0.007)
4 weeks post-poll * Residuals	0.026***	(0.007)
Panel C: 2st Stage DiD		
Post-poll	-2.815**	(1.334)
Post-poll * Turnout	0.010***	(0.002)
Post-poll * APT	-0.002	(0.005)
Post-poll * Residuals	0.018***	(0.005)
Sample	Unmatched	
Treated Municipalities	2267	
Control Municipalities	3620	
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 (1000 iterations) clustered at the municipality level in parenthesis. Significance levels: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.9
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%.

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