To Vote, or not to Vote? Electoral Campaigns and the Spread of COVID-19*

Davide Cipullo[a], Marco Le Moglie[b]

[a] *Uppsala University*
[b] *Catholic University of the Sacred Heart*

Abstract

The possibility to run fair electoral campaigns is necessary for the legitimization of modern democracies through elections. Yet, during a pandemic, the risk that electoral campaigns would enhance the spread of the disease is substantive. In this paper, we estimate the causal impact of electoral campaigns on the spread of COVID-19. Exploiting plausibly exogenous variation in the schedule of local elections across Italy, we show that the electoral campaign preceding this latter led to a significant worsening of the epidemiological situation related to the disease. Our results strongly highlight the importance of undertaking stringent measures along the entire electoral process to minimize its epidemiological consequences.

Keywords: COVID-19, Voting, Elections, Difference-in-Differences

JEL Classification: D72, H51, I18

This version: August, 2021

*We are grateful to thank the editor Thomas Stratmann, two anonymous referees, Daniele Capocefalo, Gianmarco Daniele, Osea Giuntella, Giuseppe Sorrenti, Veronica Toffolutti, and Filippo Trentini for valuable comments and suggestions. Davide Cipullo acknowledges financial support from the Jan Wallander och Tom Hedelius foundation.

Email addresses: `davide.cipullo@outlook.it` (Davide Cipullo), `marco.lemoglie@unicatt.it` (Marco Le Moglie)

1. Introduction

Elections represent a primary tool for legitimating governments' mandate (Nadeau and André, 1993; Norris, 2014). This legitimization is crucial for the correct functioning of any democracy and becomes even more needed during a pandemic when decisions that limit individual rights substantially are taken. Yet, holding elections during a pandemic might be problematic for a number of reasons.[1]

First, the fear to get infected may increase the cost of voting, especially for individuals more at risk of developing a severe disease (Fernandez-Navia et al., 2021; Palguta et al., 2021). In turn, voter turnout might be significantly lower during a pandemic[2], thus reducing the actual government legitimization through elections (Grillo, 2019).

Second, the possibility for all candidates to run a proper electoral campaign is essential for providing the government a legitimate mandate (Wolak, 2014; Jacobson, 2015). However, restrictions imposed by health authorities to curb the epidemic or fear to get infected may limit candidates' opportunities to campaign and to inform voters correctly about their platform (Bernheim et al., 2020; Dave et al., 2020).

Last but not least, holding elections during a pandemic might worsen the general epidemiological scenario by further enhancing the spread of the disease. On the election date, the epidemic may easily spread if many voters find themselves simultaneously at the polling station (see, e.g., Bertoli et al., 2020; Cotti et al., 2021; Lim et al., 2021; Palguta et al., 2021). Moreover, during the electoral campaign, candidates and their staffs meet voters, shake their hands, and take pictures with them in several in-person gatherings in inside or outside venues, assemblies in local communities, and receptions (Madestam et al., 2013; Jacobson, 2015) where social distancing measures are challenging to enforce.[3]

In this work, we identify the causal impact of electoral campaigns on the diffusion of

[1]See, e.g., Giommoni and Loumeau (2020); Thompson et al. (2020); Baccini et al. (2021); Lake and Nie (2021).

[2]Such a shock to voter turnout has further political economy consequences if certain groups of voters become more likely to abstain than others and the identity of the median voter moves further away from the median entitled citizen (Aggeborn, 2016).

[3]In many countries, election schedules concur with the spread of the infection from SARS-CoV-2 (WHO, 2020), and the related illness known as COVID-19, thus imposing a trade-off between the accountability of elected officials and diffusion of the disease (International IDEA, 2021). Countries with elections planned in the year 2020 debated on whether to organize or postpone elections (Radjenovic et al., 2020; International IDEA, 2020a). Where elections occurred, a wide range of country-specific precautionary policies was adopted to limit the contagion and ensure their smooth running (International IDEA, 2020b).

COVID-19 by exploiting a natural experiment taking place in Italy at the beginning of the *second wave* of the pandemic. Regional elections in Italy usually take place every fifth year, with different regions voting at different points in time because of past shocks. According to this schedule, thirteen out of twenty regions had their last regional election before the beginning of the COVID-19 pandemic, while regional elections took place in the other seven regions during September 2020, on the same day as a national referendum to amend the constitution. This plausibly exogenous variation to the intensity of the electoral campaign allows us to compare, in a Difference-in-Differences setting, the epidemiological outcomes in areas with and without regional elections before and after the start of the campaign.

We find that the electoral campaign preceding the regional elections did lead to a significant increase in new infections (7%), percentage of positive tests (15%), ordinary hospitalizations (24%), entries in intensive care units (5.3%), and deaths (0.6%) related to the disease. Trends in all epidemiological outcomes begin to diverge before the election date, suggesting that our results are mainly driven by contagions during the campaign. To strengthen our interpretation of the findings, we document that individuals were more likely to attend gathering venues such as public parks and squares in the treated regions during the weeks ahead of elections.

We also find that the campaign induced a significant reduction in the number of test for detecting the disease (8%). We investigate whether reduction in the number of performed tests is driven by a reduction in demand for testing (i.e., individuals' willingness to get tested) or by a reduction in supply of tests (i.e., regions' propensity to provide COVID-19 tests to citizens). We provide suggestive evidence that the negative impact of the electoral campaign on the number of performed tests is mainly driven by a reduction in regional testing capacity. Indeed, treated regions do no show differential trends in searches on Google for COVID-19 tests during the electoral campaign, thus narrowing the relevance of the demand-related explanation. Instead, they are less effective in tracing and testing the close contacts of positive individuals and are less likely to give private healthcare providers allowance to perform COVID-19 tests.

Taken together, our results inform the debate on how managing elections at the time of COVID-19 in three critical manners. First, they point towards the existence of a substantive impact of elections on the spread of COVID-19, thus proving the necessity to enforce a strict safety protocol to minimize such impact. Second, they show that a large portion of the epidemiological risk connected to elections is concentrated during the electoral campaign preceding the vote when sanitary precautions are more difficult to enforce than at the polling station. Lastly, by showing the significant negative impact of electoral campaigns on testing, our work discloses a further channel through which campaigns can ease the spread of the disease. Failing to promptly test and isolate positive individuals may as well worsen the epidemiological situation if these individuals infect others. This latter channel is consistent with recent developments in the literature on *political budget cycles* according to which cycles have, in turn, a direct impact on individual outcomes.[4]

2. Background

Since the first documented case of local transmission on February 20th, 2020, the ascending number of infected individuals and that of hospitalizations and deaths related to COVID-19 induced the Italian government to enforce a strict lockdown over the entire national territory. The lockdown lasted more than two months (i.e., from March 11th to May 17th). By the end of May, the number of new daily cases stabilized around a few hundred. This level remained almost unaltered until the beginning of August when the number of new infections started to rise again. By the beginning of October, Italy was again experiencing a substantive increase in the daily number of new infections that led to a new set of public interventions to slow down the spread. Figure A.1 in the Appendix shows a graphical overview of the evolution of daily infections, deaths, and hospitalized individuals.[5]

On September 20st and 21st, at the dawn of the *second wave* of the pandemic, Italy went to the polls. All Italians were asked in a referendum whether they intended to confirm a constitutional reform to reduce the number of members of the national parliament. The referendum was initially scheduled on March 29th and then postponed due to the epidemic.

In the same days, seven out of twenty regions also went to the polls for renewing their regional president and council because of the regular expiration of the legislative term, according to an electoral calendar strictly disciplined by the law and set by the national government.[6] Panel (a) of Figure 1 shows that such regions' geographical distribution is homogeneous across the country. Similar to the referendum, regional elections were postponed from their original

[4] Bertoli and Grembi (2021) documents that proximity to elections decreases the number of traffic fines and increases the likelihood of car accidents while Berdejó and Yuchtman (2013) documents that judges punish crimes more severely just before an election.

[5] See the Appendix A.1 for details on testing for COVID-19 in Italy.

[6] These regions are Valle d'Aosta, Veneto, Liguria, Toscana, Marche, Campania, and Puglia.

schedule dates in the Spring.[7]

Regardless of the type of voting held in each region, the national government implemented a strict protocol to guarantee safety at the polling stations. This protocol included physical distancing between different voting booths, compulsory masks for all voters and workers, and the use of hand sanitizers before touching the ballot or the voting pencil. Public events during the campaign were allowed both outdoors and indoors, in private and public venues with no restriction on the number of participants allowed in the venues. Participants were supposed to be standing or seating at a distance of one meter from each other and face masks were compulsory in indoor events. The possibility of organizing large gatherings during the electoral campaign comes at odds with the restriction implemented in other contexts taking place in similar venues such as sports and concerts: a maximum of 1,000 (200) individuals were allowed to attend outdoors (indoors) sports events and concerts.

The referendum did not have to achieve any participation quorum to be valid, thus ultimately narrowing citizens' incentives to go to the polling station. Moreover, since the constitutional reform underneath the referendum had the support of most parties represented in the parliament, the political campaign carried on in favor and against the confirmation was almost absent, or at least very mild.[8]

Regional elections, instead, were an important battleground. First, many candidates competed for a regional council seat, which is a powerful and well-rewarded position. Second, regions are assigned by the constitution the authority on healthcare policies – which is a vital issue at stake during a pandemic. Third, the opposition motivated their voters to signal their dissent to the national government, and, by contrast, members of the government coalition cam-

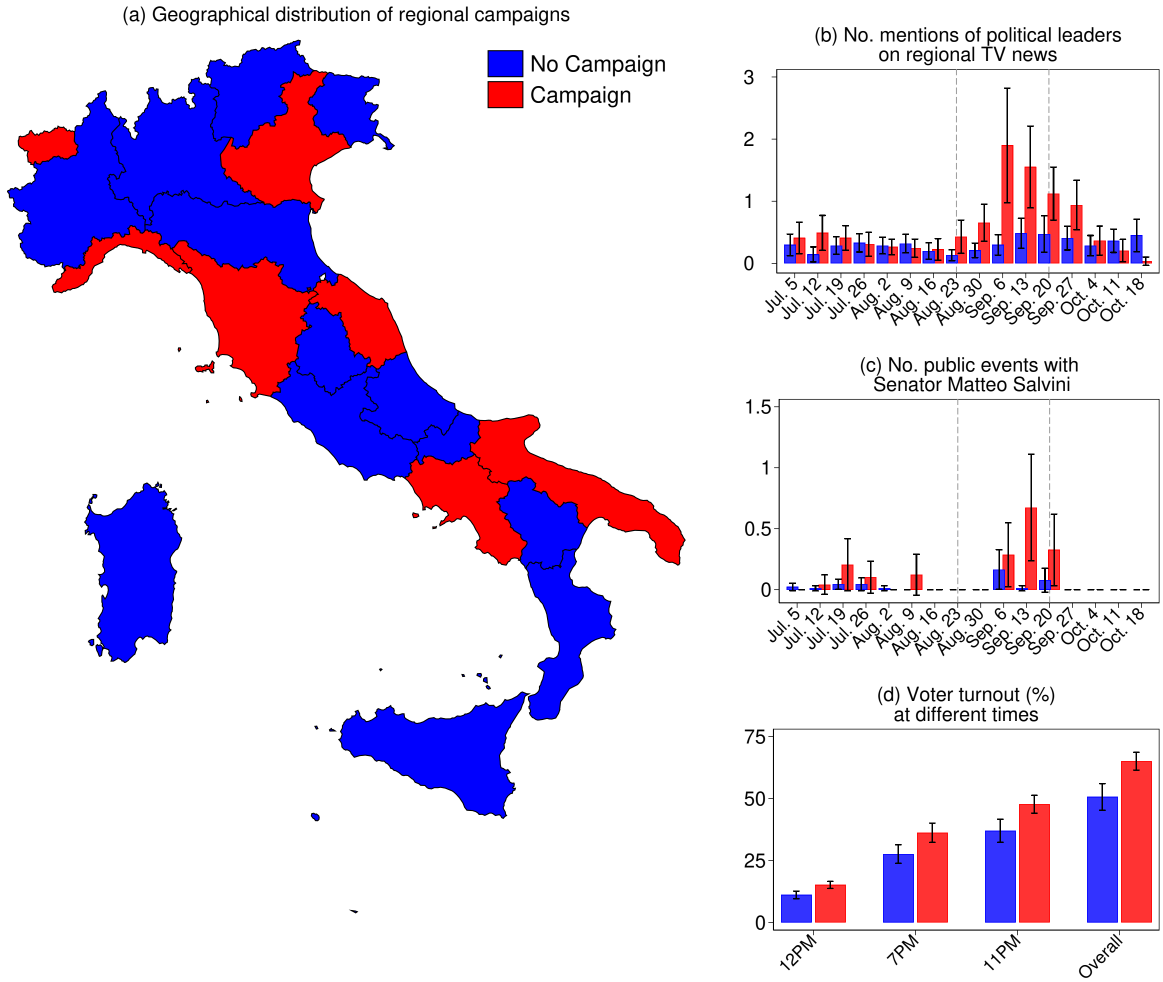
[7] Regional elections in Italy usually take place every 5 years, except when the government is unseated via a vote of no confidence, the President dies or resigns. 6 out of 7 treated regions had their previous elections during 2015 so that their natural schedule was to vote during Spring 2020. Instead, one region (Valle d'Aosta) had an early election during 2020 because its President resigned in December 2019. Moreover, 12 out of 13 control regions had their elections between 2016 and January 2020; in turn, they did not have an election planned after the beginning of the pandemic. Instead, one control region (Umbria), in which the elections were originally due in Spring 2020, had voted during 2019 because of the resignation of its President during May 2019. The two cases of early termination (Valle d'Aosta and Umbria) do not affect our identification because both resignations happened before the first documented case of COVID-19 in Italy and were due to documented corruption scandals at the highest levels of the regional bureaucracy. Lastly, still worth to notice for our identification is that the decision to run the two types of voting together to minimize the risk of contagious was only taken on the beginning of July 2020.

[8] The reform was approved by the Italian parliament in October 2019, with 97% of deputies and 80% of senators in its favor, and confirmed by 70% of the voters at the referendum.

paigned in support of their candidates. As a result, candidates and party leaders run an intense campaign both on the media and in-person. For instance, panel (b) of Figure 1 reports the average number of national political leaders' mentions on regional TV news. The figure shows that, in the period between the official start of the electoral campaign and the election days, such number was significantly higher in regions voting for renewing their government than in the others. Moreover, panel (c) of Figure 1 shows that the opposition leader, Senator *Matteo Salvini* (*Lega*) participated in the same period to a much greater number of public events in the former regions. In turn, as shown in panel (d) of Figure 1, voter turnout was approximately 20 percentage points higher in regions where both the referendum and the electoral round took place than in those voting only for the former.[9]

[9]We calculate the average turnout at the regional level by using the information provided by Italian Ministry of the Interior, which surveyed it at 12pm, 7pm and 11pm of September 20th, and at the polling stations' closing at 3pm of September 21st.

Figure 1: The Elections of Mid-September



Notes: Red-colored areas and bars refer to regions voting for both the referendum and the renewal of their president and council, while blue-colored areas and bars refer to regions voting only for the referendum. Political leaders included in panel (b) are Di Maio, Meloni, Renzi, Salvini, and Zingaretti. Turnout measures in panel (d) are computed on September 20th at 12pm, 7pm, 11 pm and September 21st at 3pm and refer to participation in the referendum. In panels (b)-(d), bars represent sample averages by treatment status while vertical short line over each bar represents 95% confidence intervals. In panels (b) and (c), the vertical dashed lines represent the date in which the electoral campaign officially started (August 21st, lines on the left) and the election date (September 21st, lines on the right).

3. Data

We rely on the daily epidemiological bulletin on the spread of COVID-19 in Italy, compiled by the Department of Civil Protection (DPC), the governmental agency responsible for monitoring the disease's evolution across the country. From the information contained in the bulletin, we calculate six epidemiological outcomes measuring the spread of COVID-19, all normalized per 100,000 inhabitants. Specifically, we focus on i) the daily number of new infections, ii) the daily number of tests performed, iii) the daily percentage of positive tests, iv) the daily number of individuals currently undertaking ordinary care in hospitals, v) the daily number of patients currently in intensive care units (ICUs), and vi) the daily number of deaths.

To interpret coefficients as percentage changes, we perform a logarithmic transformation of all outcomes. To limit the noise due to delay in reporting, we use the seven-day moving average within each region. We focus on bulletins published between June 29th and October 15th to keep the comparison across regions as reliable as possible. Indeed, the first day corresponds to the day recording the minimum number of cases between the first and the second Italian wave of the pandemic at the national level. In contrast, regions started to diverge in their adoption of specific restrictions to limit the second wave on the last day included in the sample.[10]

To ensure that different regions were experiencing, apart from the electoral campaign, similar risks of spread in the period under consideration, we employ regional data from the *Google COVID-19 Community Mobility Reports*. In particular, we focus on daily changes in individual mobility among workplaces, retailing stores, stations, and residential areas compared to the beginning of the epidemic. We also assess the robustness of our findings by employing information on the number of municipal elections held in each region on September 20-21st as well as several geographic, socio-economic, and demographic characteristics from the Italian National Institute of Statistics (ISTAT).

To better understand the possible mechanisms underlying the main results, we still exploit *Google COVID-19 Community Mobility Reports* as well as the regional data on searches made by individuals on *Google* during the reference period. From the former we retrieve the information on daily changes in individual mobility in the category labelled as "parks", that also include venues connected to the campaign such as public gardens and squares. Using the latter, we investigate whether the campaign affected individual searches for keywords related to the COVID-19 disease and for information on how to get tested.

[10] As for example the different restrictions imposed on schools re-opening.

Appendix A.2 provides a detailed list and description of all variables employed in our analysis, while Table A.1, still in the Appendix, shows the summary statistics for all of them.

4. Identification strategy

We compare, in a Difference-in-Differences setting (DiD), regions that held both the referendum and the regional election on September 20-21st – the treatment group – with regions only voting for the referendum – the control group – before and after the official start of the electoral campaign – 30 days before the vote.

Treatment assignment is plausibly exogenous because different regions vote in different years depending on their electoral cycle timing that usually repeats every five years and law disciplines calendars of voting and campaign dates. Moreover, the national government announced the new election calendar in the mid of July when the diffusion of COVID-19 was at its minimum level.

In particular we estimate the following linear regression model

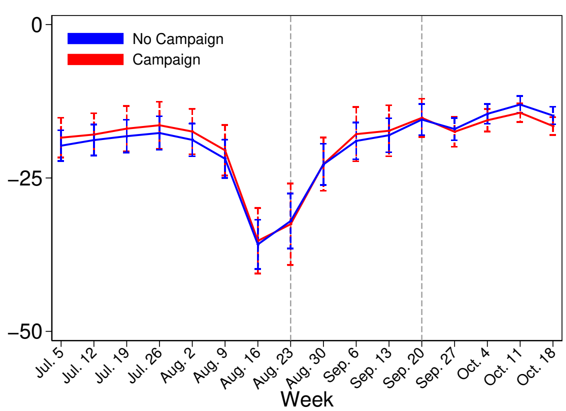
$$y_{r,t} = \gamma_0 + \gamma_1 Campaign_r \times Post_t + \zeta^{'}\mathbf{X}_{r,t} + \epsilon_{r,t}, \qquad (1)$$

where $y_{r,t}$ is one of the epidemiological outcomes measured in the region r at date t, $Campaign_r$ is a dummy equal to one for treatment regions, $Post_t$ is a dummy equal to one for all dates after the official starting of the electoral campaign. $\mathbf{X}_{r,t}$ is a set of control variables which include: i) region fixed effects aimed at capturing all the time-invariant heterogeneity across regions; ii) regional-by-week linear pre-treatment trends allowing to control for the linear effect of weekly shocks preceding the treatment and that are specific to each region; iii) adjacent regions-date fixed effects that control for daily shocks common to adjacent treatment and control regions. We account for serial and spatial correlations in the error terms $\epsilon_{r,t}$ using Conley-HAC standard errors (Conley, 1999) with maximum cutoffs for both spatial and time lags.

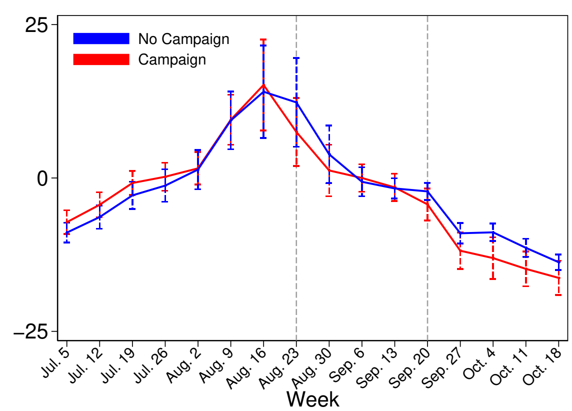
The coefficient of interest is γ_1, representing the differential effect of the electoral campaign between treatment and control regions. Yet, its causal interpretation relies on the actual comparability between the two groups of regions, particularly on the assumption that, in the absence of the electoral campaign, the dynamics of the outcomes would have been the same between the two groups. Our analysis deals with the validity of this assumption in different manners. First, in Figure 2 we exploit *Google*'s data on individual mobility to show the absence of significant differences in mobility patterns between treated and control regions over the entire observational window, apart for the difference induced directly by the impact of the campaign and documented in Figure 4. This implies that the two groups of regions were indeed experiencing similar risks of spread in the period under consideration. Second, by including in our regression adjacent regions-date fixed effects, we are constantly comparing changes in the outcome of interest between regions that share part of their border and are thus more comparable. Third, the inclusion of adjacent region-date fixed effects also helps to rule out the impact of weekly trends among adjacent regions, such as those related to summer holidays. Fourth, the inclusion of regional-by-week linear pre-treatment trends should capture several potential region-specific differences in the dynamics of the outcomes before the electoral campaign, making treatment and control regions strongly comparable once the campaign starts.

Figure 2: The Dynamics of Individual Mobility Through Google's Data

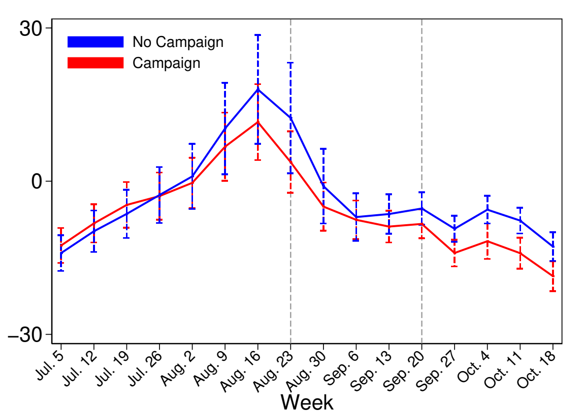
(a) Workplaces



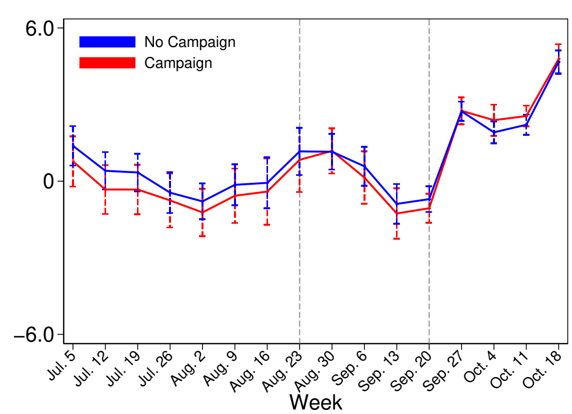
(b) Retailing



(c) Stations



(d) Residential



Notes: The figures report the average and 95% confidence intervals of the daily changes in individual mobility compared to February 2020, in treatment (red line) and control (blue line) regions. In all panels, the vertical dashed lines represent the date in which the electoral campaign officially started (i.e. August 21st, lines on the left) and the week in which the election took place (i.e., September 20-21st, lines on the right).

Lastly, we propose a dynamic specification in which the dummy for the post-treatment period ($Post_t$) is replaced with a set of week dummies to empirically exclude that the dynamics of the six epidemiological outcomes were significantly different between treatment and control regions also before the campaign. This specification also allows us to get further insights on the dynamics of the effect after the beginning of the campaign. More specifically, we estimate the following regression model:

$$y_{r,t} = \gamma_0 + \sum_{w \in [-6,8], w \neq -1} \gamma_w Campaign_r \times \mathbf{1}(Week_t = w) + \zeta^{'} \mathbf{X}_{ij} + \epsilon_{r,t} \; , \qquad (2)$$

5. The electoral campaign and the spread of COVID-19

Table 1 presents the estimates for Equation 1. The daily number of new infections rose by 7% (p-value=0.037) due to the electoral campaign. Even if significantly different from zero, this coefficient probably reflects a lower bound of the electoral campaign's impact on the number of new infections, given that the campaign also led to a reduction in the daily number of tests performed equal to 8.3% (p-value$<$0.001). As better discussed later in Section 5.2, the reduction in testing in treated region with respect to the control ones can be either demand-driven (i.e., individuals test less) or supply-driven (i.e., regional authorities supply fewer tests), or a mix of them. Regardless of its actual origins, once we account for the reduction in testing, the electoral campaign's epidemiological impact appears to be sizeable. In particular, the percentage of positive COVID-19 tests rose by 15% (p-value$<$0.001). The substantive impact further reflects into a significant increase in the daily number of ordinary hospitalizations, the number of people hospitalized in ICUs, and deaths related to the epidemic, which since the start of the former rose by 24% (p-value$<$0.001), 5.3% (p-value=0.007) and 0.6% (p-value=0.011), respectively.

Figure 3 plots the estimated coefficients for the interaction terms of Equation 2. First, we notice the absence of differential pre-treatment dynamics in all epidemiological outcomes between treatment and control regions before the campaign's official start. The absence of pre-treatment dynamics is crucial to supporting the actual comparability between the two groups of regions, and ultimately the possible causal interpretation of our results. Second, the treatment effect dynamics reveal both similarities and differences across outcomes. For all outcomes, treatment effects become visible with a certain lag from the beginning of the campaign and become statistically significant at the 5% critical level only after some weeks. These patterns are consistent with a cumulative epidemiological effect of the electoral campaign and the 7-14 days' average

Table 1: The Impact of the Electoral Campaign on the Spread of COVID-19

	(1) Infections	(2) Tests	(3) % Posit.	(4) Hospit.	(5) ICUs	(6) Deaths
Campaign × Post	0.070**	-0.083***	0.146***	0.240***	0.053***	0.006**
	(0.033)	(0.019)	(0.031)	(0.052)	(0.020)	(0.002)
Observations	2,060	2,060	2,060	2,060	2,060	2,060
Baseline (value)	0.315	83.578	0.404	0.904	0.0032	0.012
Region FE	✓	✓	✓	✓	✓	✓
Regional pre-trends	✓	✓	✓	✓	✓	✓
Date × Adjacent reg. FE	✓	✓	✓	✓	✓	✓

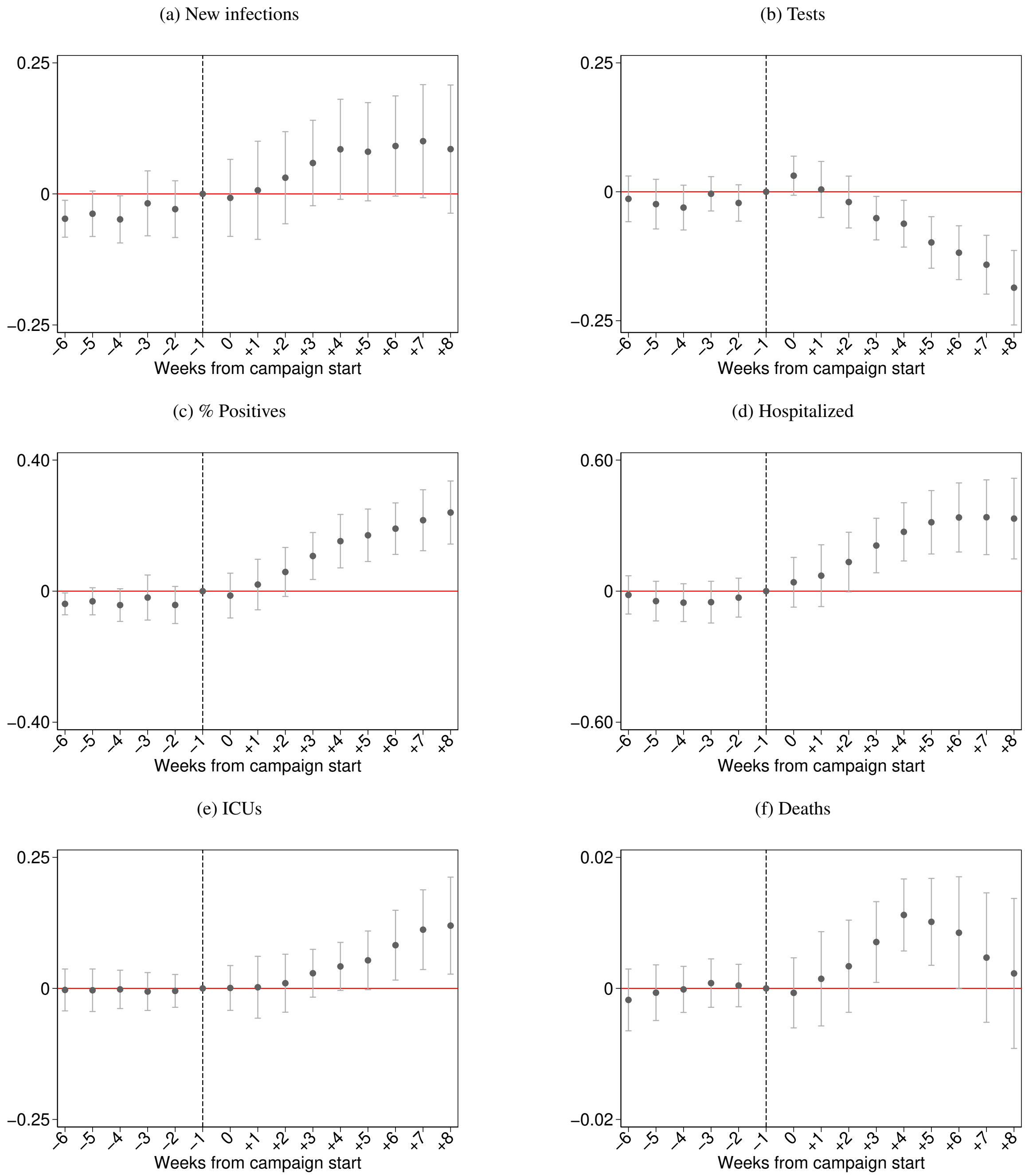
Notes: The table shows the differential impact of electoral campaign between regions voting both for the referendum and the regional election (treatment) and those only voting for the referendum (control), as obtained through the estimation of Equation 1. All dependent variables are the natural logarithm of the seven-day moving average of the daily number at the regional level. See Section 3 for details. *Campaign* is a dummy equal to 1 for treatment regions, and 0 otherwise. *Post* is a dummy equal to 1 for all the dates since August 21st, and 0 otherwise. Standard errors are robust to serial and spatial correlation and computed imposing maximum cutoff for both spatial and time lags. *,**,*** represent the 10%, 5%, 1% significance levels, respectively.

incubation period of the disease (Qin et al., 2020).[11]

While most of the outcomes stabilize after the fourth post-treatment week, few others still increase, like the percentage of positive tests and the number of hospitalizations in ICUs. Concerning the former, its increase after the fourth week is entirely due to the further reduction in the number of test. Regarding the latter, hospitalizations in ICUs still increase to the sixth week after the campaign's start and then finally stabilized. The fourth post-treatment week corresponds to the week in which the campaign ends, and the vote occurs. Therefore, the results of this study document that the electoral campaign enhanced the spread of COVID-19 already before the actual election date. To further corroborate such conclusion, we still turn to the *Google COVID-19 Community Mobility Reports*, and we document in Figure 4 how the campaign, starting from four weeks ahead of the election, induced greater mobility patterns in urban aggregation areas such as parks and public squares, where large and small political gatherings are likely to take place. Individual mobility in these areas was, on the contrary, equal in the two groups of regions both before the electoral campaign and after the elections.

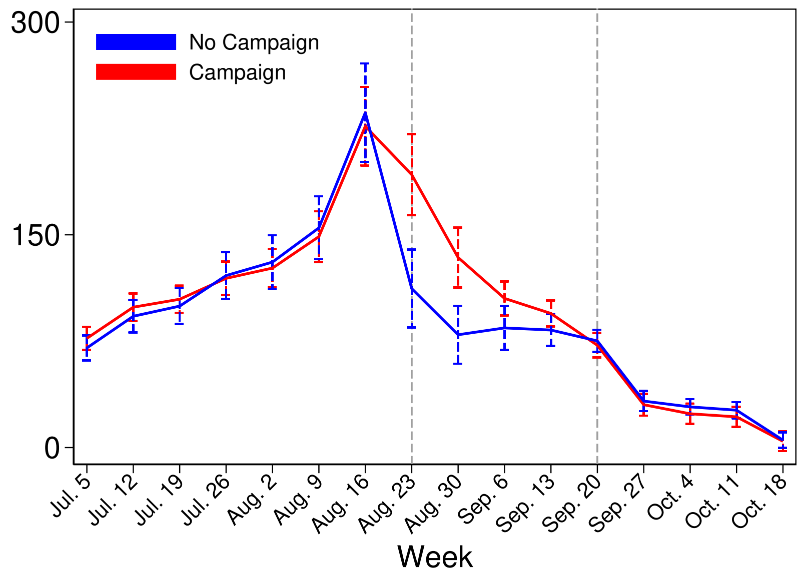
[11] See Figure A.2 for a version of Figure 3 in which we plot daily – instead of weekly – coefficients.

Figure 3: The Impact of Electoral Campaign on the Spread of COVID-19: Dynamic Specification



Notes: The figures report estimates and 95% confidence intervals for the coefficients of the interactions included in Equation 2, between the dummy $Campaign$, which is equal to one for treatment regions, and the week dummies. $week = -1$ is the reference week (i.e., week 33 of 2020). All dependent variables are the natural logarithm of the seven-day moving average of the daily number at the regional level. See Section 3 for details. $Campaign$ is a dummy equal to 1 for treatment regions, and 0 otherwise. Confidence intervals are based on standard errors that are robust to serial and spatial correlation and computed imposing maximum cutoff for both spatial and time lags. In all panels the vertical dashed line represents the date in which the electoral campaign officially started (i.e. August 21st).

Figure 4: Individual Mobility Patterns in Public Parks and Squares



Notes: The figures report the average and 95% confidence intervals of the daily changes in individual mobility compared to February 2020, in treatment (red line) and control (blue line) regions. The vertical dashed lines represent the date in which the electoral campaign officially started (i.e. August 21st, lines on the left) and the week in which the election took place (i.e., September 20-21st, lines on the right).

5.1. Robustness tests

Besides showing that the six epidemiological outcomes of interest were following the same dynamics in treated and control regions before the start of the campaign (see Figure 3), we performed various robustness tests to validate our estimates and research design further.

First, in Table A.2, we replace the linear pre-treatment weekly regional trends with weekly trends in several observable characteristics that can affect the spread of COVID-19. The variables included in the analysis are defined and reported in Table A.1. On the one hand, this exercise allows establishing that this paper's main results do not rely on the control for pre-trends in the outcomes. On the other hand, the exercise is beneficial to exclude that observed patterns are due to differences in other dimensions, which can confound our results. In particular, this robustness check excludes the possibility that our results depend – among others – on differences in touristic activities, availability of smart-working solutions, the share of elderly individuals who are in care, diffusion of pre-school services, and commuting times.

Second, in Figure A.3, we show that our main results do not depend on the specific choice of considering all weeks after week 34 as post-treatment and the weeks before as pre-treatment. In each iteration, we estimate a version of Equation 1 in which the $Post_t$ indicator is equal to 1 starting from the week specified on the horizontal axis and 0 otherwise. Week 30 was the week when the national government announced the election dates, while week 38 is the week during which the election took place. This exercise allows us to establish that our results do not depend on our definition of the campaign period. More specifically, it might be the case that party leaders and candidates start a campaign before the official start reported in the law, which is 30 days before the election. Our results show that the choice to consider week 34 – as in the law – in the main specification is not driving our results. Indeed, the estimates are qualitatively equal if we allow the campaign period to begin during week 33 or 32 (i.e., up to 45 days before the election).

Third, in Figure A.4, we show that our results do not rely on a specific region's presence in the sample. In each iteration, we estimate Equation 1 by removing from the sample the region specified on the horizontal axis. We find that the estimated coefficients are very stable, excluding the possibility that our estimates depend on one outlier region, which either has low diffusion of COVID-19 and no regional election, or high spread of the infection and the presence of regional elections.

Fourth, in Table A.3, we document that our main results do not rely on defining our outcome variables as a 7-day moving average. More specifically, the results are quantitatively similar if we aggregate the data to the weekly level and use weekly averages to construct the dependent variables. Losing a substantial fraction of the observations leads to a limited reduction in statistical precision.

Lastly, we assess the robustness of our statistical inference. The Conley HAC standard errors (Conley, 1999) account both for spatial correlation of units that are geographically close and for auto-correlation over time. However, they come with the limitation that the choice of cutoffs is arbitrary. In Figure , we show that our analysis's statistical inference bases on very conservative cutoffs. In details, at each iteration, we estimate Equation 1 by imposing a non-zero correlation among observations within the distance cutoff specified on the horizontal axis and the lag cutoff specified on the vertical axis, and we report the p-values for the $Campaign_r \times Post_t$ indicator. For all epidemiological outcomes, we find that our main results draw statistical inference using the largest feasible p-values.

5.2. The impact of the electoral campaign on COVID-19 tests

The results presented in column (2) of Table 1 and in panel (b) of Figure 3 document that, besides causing an increase in the spread of COVID-19, the electoral campaign approaching regional elections lead to a sizeable reduction in the number of tests performed daily. In principle, this pattern could be explained both by a reduction in individual demand for getting tested and by a reduction in the region's supply of COVID-19 tests.

On the one hand, the focus on the electoral campaign may reduce individual attention towards COVID-19 precautionary measures, including the willingness of getting tested in case of mild symptoms. To test whether this possibility is consistent with our empirical results, we rely on information from *Google Trends*. More specifically, we compare the dynamics of individual searches on Google for the keywords "*COVID*" and "*Tampone*" (i.e., the Italian word for the COVID-19 test) in treated and control regions. The first keyword is a proxy for the population's attention towards the disease as it also includes individuals searching for news in the media about the spread and the severity of COVID-19. The second keyword is a more direct measure of the willingness to getting tested, as individuals may search for "*Tampone*" to get information on where to get tested and how to book and appointment.

Figure 5 excludes the possibility that individuals living in control regions were searching more frequently for COVID-19 tests-related keywords during the campaign period, relative to the pre-campaign and post-election periods. Trends of *Google* searches are very similar across groups of regions for "*COVID*", while trends of *Google* searches for "*Tampone*" are, if anything, slightly more accentuated during the campaign and after the election in treated than in control regions. This evidence suggests that individuals search more actively for COVID-19 tests when the spread of the disease happens at a quicker rate and more daily tests are performed.

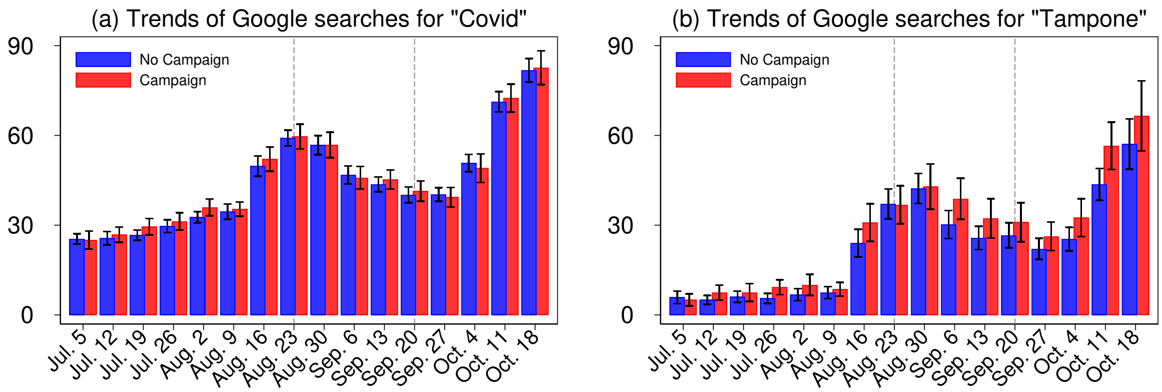
On the other hand, the electoral campaign might reduce the supply of COVID-19 tests (i.e., a region's testing capacity) either because of the incumbents' strategic incentives approaching the elections (Pulejo and Querubín, 2021) or because of an increase in uncertainty in the bureaucratic structure approaching the leadership's turnover (Alesina et al., 1996). The possibility that reduction in the number of performed tests reflects a reduction in the region's supply of tests is plausible in the Italian setting since the Constitution assigns regions the authority over public healthcare provision. In turn, regions have the competence over tracing and testing the close contacts of positive individuals, to organize large-scale testing events in localities where the disease is spreading at a particularly fast rate, and to decide whether private healthcare providers should be allowed to supply COVID-19 tests to private citizens (see Appendix A.1 for details on how COVID-19 tests are performed in Italy).

Even if we cannot empirically test this channel directly, anecdotal and post-election evidence suggests that a reduction in testing capacity relative to the control regions appears as a key mechanism behind the effects of electoral campaigns on the number of COVID-19 tests performed. First, one of the key responsibilities of regional governments in the fight against COVID-19 is to effectively trace the contacts of all individuals who tested positive, and in turn isolate them for a period of ten days. At the end of the isolation period, the individual is subject to a compulsory test. Although the expectation is to trace the contacts of all individuals who tested positive, evidence demonstrates that this not necessarily the case (see, e.g., Fetzer and Graeber, 2020). Unfortunately, information on the share of positive individuals for whom an effective contact tracing has been performed in each region is not available for the time span included in our analysis. However, this data is available starting from October, 2020 since the Government later decided to include this measure among the ones considered to select a region's risk and restriction level.[12] During October 2020, i.e. the month right after the regional elections, control regions were, on average, capable to trace the close contacts of 93% of positive individuals, while treated regions only traced the contacts of 77% of positive individuals during the same month (testing for equal average across groups yields N=20, p=0.059).

Second, regions have the authority to decide whether private healthcare providers are allowed to supply COVID-19 tests to citizens. According to *Corriere della Sera*, which is the

[12] The national government introduced on November 3 a four-level restrictions system. Each region was automatically assigned to a restriction level for one week based on 21 measures of spread and severity of the disease, and response of the local healthcare system.

Figure 5: Electoral campaign and trends in online searches for COVID-19 tests



Notes: Red-colored areas and bars refer to regions voting for both the referendum and the renewal of their president and council, while blue-colored areas and bars refer to regions voting only for the referendum. Panel (a) documents the trends in Google searches for the word "COVID" in treated and control regions, while panel (b) documents the trends in Google searches for the word "Tampone", which is the Italian word for the PCR COVID-19 molecular test. Bars represent sample averages by treatment status while the vertical short line over each bar represents 95% confidence intervals. The vertical dashed lines represent the date in which the electoral campaign officially started (August 21st, lines on the left) and the election date (September 21st, lines on the right).

most acknowledged and popular daily newspaper in Italy, as of October 8, 2020, only 7 out of the 20 regions in Italy had given to healthcare providers in the private sector the faculty to conduct molecular PCR COVID-19 tests, which were the only ones counted in the official reports until January 2021. Out of these 7 regions, 6 were in the control group (46%), while only one was in the treated group (14%). All regions opened up the possibility for private providers to supply COVID-19 tests later in the year.

6. Conclusions

In this study, we have estimated the causal impact of electoral campaigns on the spread of COVID-19 in Italy. By exploiting variation in the elections schedule across Italian regions in a Difference-in-Differences setting, we have found that the electoral campaign increased the spread of COVID-19 significantly. More specifically, our results show that the incidence of the disease, the number of patients seeking care in hospitals, and the number of deceased individuals increased more steeply in the treated than in the control regions at the dawn of the pandemic's *second wave* in Italy.

Our findings lead to a couple of concluding remarks and policy recommendations. First, our findings shed light on the negative epidemiological impact of electoral gatherings during which the enforcement of prevention policies is limited. Authorities should bear in mind such a epidemiological cost when deciding whether to organize elections and deciding which measures to take. While several prevention measures can be successfully implemented to limit the spread of the disease at the polling station – distancing, face masks, hand sanitizers, and postal voting – it remains an open question of how to conduct fair electoral campaigns safely.

Second, our findings show that the electoral campaigns also negatively impacted on testing capacity, which in Italy is under the authority of regional governments. While we provide evidence that such impact is mostly supply-driven by a reduction in regional testing capacity, our study remains agnostic on whether such reduction is due to the temporary uncertainty or intentional. Nonetheless, this result is strongly in line with the reduction, as elections approach, in the stringency of public-health measures adopted to contain the epidemic, which has been highlighted by Pulejo and Querubín (2021), and more in general with the recent evidence on the effects of electoral cycles on individual outcomes (Berdejó and Yuchtman, 2013; Bertoli and Grembi, 2021). However, reduced testing capacity can contribute to the spread of the disease because infected asymptomatic individuals are not isolated promptly, and their close contacts are not traced (Ferretti et al., 2020; Fetzer and Graeber, 2020). Reduced testing capacity eventually represents an additional indirect channel through which electoral campaigns can affect the spread of the disease, and thus to consider for minimizing their epidemiological impact.

References

Aggeborn, L. (2016). Voter Turnout and the Size of Government. *European Journal of Political Economy 43*, 29–40.

Alesina, A., S. Özler, N. Roubini, and P. Swagel (1996). Political Instability and Economic Growth. *Journal of Economic Growth 1*(2), 189–211.

Baccini, L., A. Brodeur, and S. Weymouth (2021). The COVID-19 Pandemic and the 2020 US Presidential Election. *Journal of Population Economics 34*, 739–767.

Berdejó, C. and N. Yuchtman (2013). Crime, Punishment, and Politics: an Analysis of Political Cycles in Criminal Sentencing. *Review of Economics and Statistics 95*(3), 741–756.

Bernheim, B. D., N. buchmann, Z. Freitas-Groff, and S. Otero (2020). The Effects of Large Group Meetings on the Spread of COVID-19: The Case of Trump Rallies. *Stanford Institute for Economic Policy Research (SIEPR) Working Papers No. 20-043*.

Bertoli, P. and V. Grembi (2021). The Political Cycle of Road Traffic Accidents. *Journal of Health Economics 76*, 102435.

Bertoli, S., L. Guichard, and F. Marchetta (2020). Turnout in the Municipal Elections of March 2020 and Excess Mortality during the COVID-19 Epidemic in France. *IZA Discussion Paper No. 13335*.

Conley, T. G. (1999). GMM Estimation with Cross Sectional Dependence. *Journal of Econometrics 92*(1), 1–45.

Cotti, C., B. Engelhardt, J. Foster, E. Nesson, and P. Niekamp (2021). The Relationship Between in-person Voting and COVID-19: Evidence from the Wisconsin Primary. *Contemporary Economic Policy (Forthcoming)*.

Dave, D. M., A. I. Friedson, K. Matsuzawa, D. McNichols, C. Redpath, and J. J. Sabia (2020). Risk Aversion, Offsetting Community Effects, and COVID-19: Evidence from an Indoor Political Rally. *NBER Working Paper No. 27522*.

Fernandez-Navia, T., E. Polo-Muro, and D. Tercero-Lucas (2021). Too Afraid to Vote? The Effects of COVID-19 on Voting Behaviour. *European Journal of Political Economy*, 102012.

Ferretti, L., C. Wymant, M. Kendall, L. Zhao, A. Nurtay, L. Abeler-Dörner, M. Parker, D. Bonsall, and C. Fraser (2020). Quantifying SARS-CoV-2 Transmission Suggests Epidemic Control with Digital Contact Tracing. *Science 368*(6491).

Fetzer, T. and T. Graeber (2020). Does Contact Tracing Work? Quasi-Experimental Evidence from an Excel Error in England. *CEPR Discussion Paper No. 15494*.

Giommoni, T. and G. Loumeau (2020). Lockdown and Voting Behaviour: A Natural Experiment on Postponed Elections during the COVID-19 Pandemic. *Available at SSRN*.

Grillo, A. (2019). Voter Turnout and Government's Legitimate Mandate. *European Journal of Political Economy 59*, 252–265.

International IDEA (2020a). Elections and COVID-19. *International IDEA Technical Paper 1/2020*.

International IDEA (2020b). Risk Mitigation Measures for National Elections during the COVID-19 Crisis. *Available at International IDEA's web page*.

International IDEA (2021). Global Overview of COVID-19: Impact on Elections. *Available at International IDEA's web page*.

Jacobson, G. C. (2015). How Do Campaigns Matter? *Annual Review of Political Science 18*(1), 31–47.

Lake, J. and J. Nie (2021). Did COVID-19 Cost Trump the Election? *CESifo Working Paper No. 8856*.

Lim, J. T., K. Maung, S. T. Tan, S. E. Ong, J. M. Lim, J. R. Koo, H. Sun, M. Park, K. W. Tan, J. Yoong, A. R. Cook, and B. S. L. Dickens (2021). Estimating Direct and Spill-over Impacts of Political Elections on COVID-19 Transmission using Synthetic Control Methods. *PLOS Computational Biology 17*(5), 1–15.

Madestam, A., D. Shoag, S. Veuger, and D. Yanagizawa-Drott (2013). Do Political Protests Matter? Evidence from the Tea Party Movement. *The Quarterly Journal of Economics 128*(4), 1633–1685.

Nadeau, R. and B. André (1993). Accepting the Election Outcome: The Effect of Participation on Losers' Consent. *British Journal of Political Science 23*(4), 553–563.

Norris, P. (2014). *Why Electoral Integrity Matters*. Cambridge University Press.

Palguta, J., R. Levínskỳ, and S. Škoda (2021). Do Elections Accelerate the COVID-19 Pandemic? Evidence from a Natural Experiment. *GLO Discussion Paper No. 891*.

Pulejo, M. and P. Querubín (2021). Electoral Concerns Reduce Restrictive Measures during the COVID-19 Pandemic. *Journal of Public Economics 198*, 104387.

Qin, J., C. You, Q. Lin, T. Hu, S. Yu, and X.-H. Zhou (2020). Estimation of Incubation Period Distribution of COVID-19 using Disease Onset forward time: A Novel Cross-sectional and Forward Follow-up Study. *Science Advances 6*(33).

Radjenovic, A., R. Mańko, and G. Eckert (2020). Elections and COVID-19. *European Parliamentary Research Service Briefings PE651.969*.

Thompson, D. M., J. A. Wu, J. Yoder, and A. B. Hall (2020). Universal Vote-by-mail Has No Impact on Partisan Turnout or Vote Share. *Proceedings of the National Academy of Sciences 117*(25), 14052–14056.

WHO (2020). World Health Organization, Modes of Transmission of Virus Causing COVID-19: Implications for IPC Precaution Recommendations. *Scientific brief 27 March 2020*.

Wolak, J. (2014). How campaigns promote the legitimacy of elections. *Electoral Studies 34*, 205–215.

Appendix

A.1. Testing for COVID-19 in Italy

Testing in Italy is mainly conducted within the public healthcare services, which is area regional responsibility under the responsibility of the regional government. There are five main reasons why a person might get tested.

First, if the person has been recognized as a "close contact" of an individual who got tested positive. The national ministry of health specifies the criteria for being considered a "close contact," but it is the healthcare provider's responsibility to trace and evaluate the contacts of all positive individuals. Regions have discretion over the decision on whether a positive's contact is to be considered a "close contact", and therefore is subject to compulsory test and precautionary isolation or not. Moreover, reports that regions present to the national government document a non-negligible variation in the proportion of positive individuals for whom the region had effectively conducted a contact tracing.

Second, an individual who has symptoms that are potentially related to COVID-19 should get tested. In this case, the individual receives from her doctor the authorization to book an appointment for free at the public healthcare provider.

Third, an individual may need a test for professional or travel purposes. For instance, professional sports require athletes to be subject to tests regularly. These tests are usually conducted by private labs and are added to the aggregate figures if the health ministry recognizes the performed test. Until January 2021, only PCR Covid-19 tests (the traditional molecular tests) were computed in the official figures. However, private testing provision was a limited market during summer 2020. According to "Corriere della Sera" (the most popular and acknowledgeable newspaper in Italy), as of October 2020, only seven regions had allowed private sector healthcare providers to supply PCR Covid-19 tests.

Fourth, regions have the faculty to propose special testing days in which a significant fraction of citizens who live in a limited area are tested after a surge of contagions in that area.

Fifth, and individual who had previously tested positive needs to take other tests (the first of which not earlier than ten days from the positive test) until a negative result allows the person to leave from her isolation. After 21 days, and individual is considered healthy even in the absence of a negative COVID-19 test.

A.2. Data Appendix

List of data sources (Article)

Daily epidemiological bulletin on the spread of COVID-19 in Italy.

(Publicly available on GitHub.com).

We access this publicly available source to retrieve information on the daily number of infections for each region and the daily stock of individuals seeking care in ordinary hospitals and ICUs. We also retrieve the daily number of deceased individuals and the daily number of tests conducted in each region by subtracting the stock at date $t-1$ from the stock at date t. Lastly, we retrieve the percentage of positive tests dividing the daily number of positive tests by the daily number of tests conducted and multiplying it by 100. We restrict our attention to bulletins published between June 29th, 2020 and October 15th, 2020. For the *Trentino-Alto Adige Region*, we manually compute the region-level aggregate figures because raw data contain separate information for the province of Trento and the province of Bolzano/Bozen.

We then apply the following transformations to all variables to achieve a better comparability across regions. First,

$$\widehat{y}_{r,t} = \log\left(\frac{\tilde{y}_{r,t} \times 100,000}{Population_r}\right), \tag{A.1}$$

where $\tilde{y}_{r,t}$ is each of the six epidemiological outcomes of interest as originally included in the data source or as described in the previous paragraph, $Population_r$ is the region's population according to ISTAT data (see below) as of January 1st, 2020, and $\widehat{y}_{r,t}$ is the transformed indicator. Second, we compute a seven-day moving average as follow to get to the six outcomes $y_{r,t}$ estimated using equation 1 and equation 2. Formally,

$$y_{r,t} = \frac{\sum_{s\in[-6,0]} \widehat{y}_{r,t-s}}{7}. \tag{A.2}$$

In turn, our final sample observes 103 daily seven-day moving averages for each of the twenty Italian regions, for a total of 2,060 observations.

Voter turnout on September 20th and 21st, 2020 referendum.

(available at).

This data contains information on the number of individuals eligible to vote for the constitutional referendum in each municipality, as well as the number of individuals who decided to vote by 12 pm, 7 pm, 11 pm on September 20*th*, and by the closing time at 3 pm on September 21*st*.

We aggregate data from all municipalities belonging to the same region to get such measures at the regional level. We then define turnout as follow,

$$Turnout_{r,h} = \frac{Voters_{r,h}}{Eligible_r}. \tag{A.3}$$

Mentions of political leaders on regional TV news.

(data are available upon request).

We accessed this data by visiting the Italian public TV provider (RAI) and collect information on all the times each of the prominent political leaders in Italy have been mentioned on public TV. We search for Luigi di Maio, leader *de facto* of the 5-Star Movement; Giorgia Meloni, president of *Fratelli d'Italia*; Matteo Renzi, leader *de facto* of *Italia Viva*; Matteo Salvini, secretary of *Lega*; Nicola Zingaretti, secretary of *Partito Democratico*.

We then restricted our attention to mentions during the editions of local TV news (*Tg Regionale*), which go on air on the third-main channel of the broadcaster daily at 2 pm; 7.30 pm; 0.10 am (on weekends at 2 pm; 7.30 pm; 11 pm).

Lastly, we summed the number of mentions of all leaders for each date and region.

Public events with Senator Matteo Salvini

(Publicly available on Lega's web page).

We manually counted the number of events reported in the list for each day and region.

Google COVID-19 Community Mobility Reports

(Publicly available on Google).

We use daily information from each of the twenty regions relative to the variables labeled as *Retail and recreation percent change from baseline*; *Grocery and Pharmacy percent change from baseline*; *Workplaces percent change from baseline*; *Residential change from baseline*; *Parks percent change from baseline*. We exclude the remaining measure (i. e., *Transit Stations percent changes from baseline*) because of several missing observations in the period under consideration.

Retail and recreation percent change from baseline, *Grocery and Pharmacy percent change from baseline*, *Workplaces percent change from baseline*, and *Parks percent change from baseline* measure the percentage change in the number of Google customers who are located by their smartphone in each of those areas. *Residential change from baseline*, instead, measures the percentage change in the average number of hours that Google customers are located by their smartphone in their home.

We restrict our focus to data covering mobility between June 29th and October 15th, 2020.

Google Trends Data

(Publicly available on Google Trends) Google Trends allows us to retrieve, for each region, a measure of the number of Google searches for "COVID" and for "Tampone" (i.e., the Italian word for the COVID-19 test) for each region in each week, relative to the week in which each word was most searched in the region. In turn, the week with the highest number of searches is assigned the score of 100.

List of data sources (Appendix)

Municipality elections held on September 20th and 21st, 2020

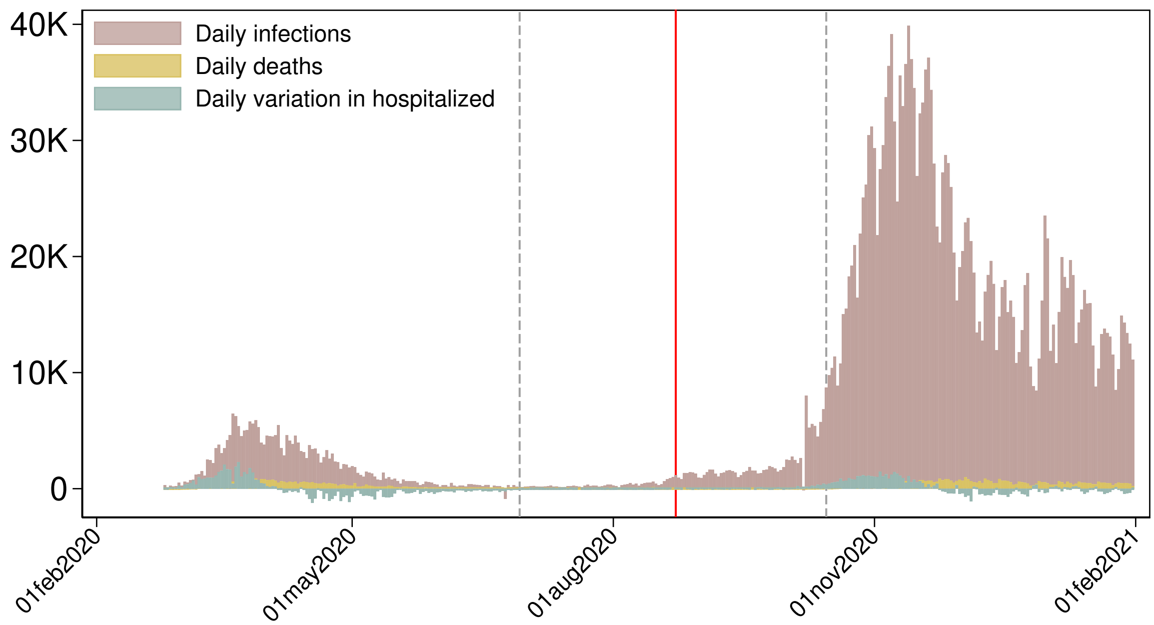
We retrieve the list of all municipality elections that took place on the same day as the constitutional referendum and the seven regions' regional elections to calculate the share of each region's voters subject to these elections. We use data on the population eligible to vote at the municipality level described in the previous sub-section to calculate the shares.

Geographic, socio-economic, and demographic characteristics from ISTAT

In Table A.2, we propose a specification in which regional-by-week linear pre-treatment trends are replaced by weekly pre-treatment trends in observable characteristics that might explain the spread of COVID-19. These characteristics include (a) the share of voters subject to municipality elections just described; (b) the region's surface; (c) population density; (d) percentage of hospitalized individuals coming from another region; (e) unemployment rate; (f) employment rate; (g) employment rate among senior (>54 years old) individuals; (h) percentage of elderly in care; (i) percentage of population covered by ultra-broadband internet connection; (j) average travel times from central urban areas; (k) the average number of daily tourists per 100 residents; (l) percent of municipalities providing childcare services; (m) mountainous surface; coastal surface; (n) number of museums visitors per km^2; (o) per capita export volume; (p) an indicator equal to 1 if the region is located in Southern Italy. We collected all the latter variables from the Italian National Institute of Statistics (ISTAT). Table A.1 reports the descriptive statistics of these and all other variables employed in this analysis.

A.3. Additional Tables and Figures

Figure A.1: An Overview of COVID-19 Dynamics in Italy



Notes: The figure reports information on the aggregate number of individuals who tested positive to COVID-19 in Italy between February 2020 and January 2021. The dashed vertical lines represent the first and the last day included in our estimation sample. The solid vertical line represents the beginning of the campaign period used in the main specifications.

Table A.1: Descriptive Statistics

	(1) Mean	(2) St. Dev.	(3) Obs.
(a) Measures of diffusion			
Cases	0.60	0.45	2,060
Tests	4.44	0.52	2,060
% Positive	0.594	0.350	2,060
Hospitalized	0.85	0.49	2,060
ICU	0.12	0.12	2,060
Deaths	0.01	0.02	2,060
(b) Regional characteristics			
Population ($\times$ 100K)	30.12	25.40	20
Surface (km^2)	17,398	9,712	20
Density (inh./km^2)	317	368	20
% Healthcare regional migration	8.92	6.20	20
Unemployment rate (%)	10.47	5.19	20
Employment rate (%)	59.38	10.20	20
Senior Employment rate (%)	55.15	6.64	20
Elderly care patients (%)	1.24	0.92	20
Fiber Internet connection (%)	14.50	2.97	20
Travel times from urban areas (mins.)	51.45	10.60	20
Daily tourists (per 100 inh.)	9.59	10.83	20
Childcare provision (%)	58.20	23.91	20
Mountainous surface (km^2)	2.59	1.23	20
Coastal surface (km^2)	0.18	0.22	20
No. Museums (per inh.)	0.01	0.01	20
Export volume (per inh.)	44,066	45,580	20
South	0.45	0.51	20
(c) Election characteristics			
Campaign	0.35	0.49	20
Share municipality election	0.08	0.11	20
Voter turnout (Sunday at 12PM)	0.12	0.03	20
Voter turnout (Sunday at 19PM)	0.30	0.08	20
Voter turnout (Sunday at 11PM)	0.40	0.09	20
Voter turnout (Overall)	0.55	0.11	20

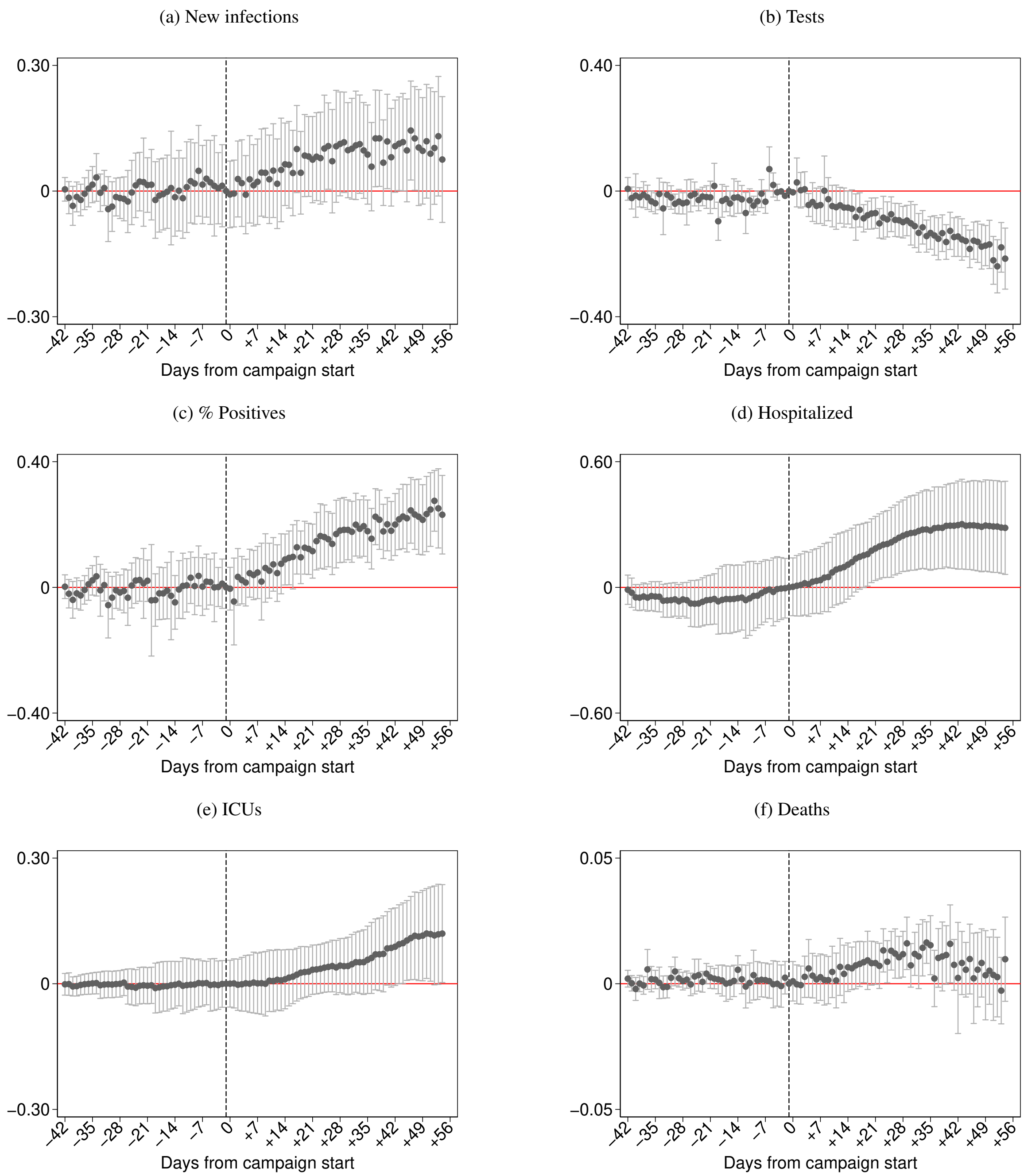
Notes: The table show the mean, the standard deviation and the number of observations for each variable employed in the analysis. The summary statistics in panel (a) are calculated considering the entire estimation sample, while those showed in panels (b) and (c) refer to the regional value of the characteristics as in 2019 and during the election days, respectively.

Table A.2: Robustness Test I: Controlling for Trends in Observables

	(1) Infections	(2) Tests	(3) % Posit	(4) Hospit.	(5) ICUs	(6) Deaths
Campaign × Post	0.070**	-0.083***	0.146***	0.240***	0.053***	0.006**
	(0.033)	(0.019)	(0.31)	(0.052)	(0.020)	(0.002)
Observations	2,060	2,060	2,060	2,060	2,060	2,060
Mean Dep. Var.	0.602	4.437	1.208	0.851	0.119	0.014
Region FE	✓	✓	✓	✓	✓	✓
Pre-trends in observables	✓	✓	✓	✓	✓	✓
Date × Adjacent reg. FE	✓	✓	✓	✓	✓	✓

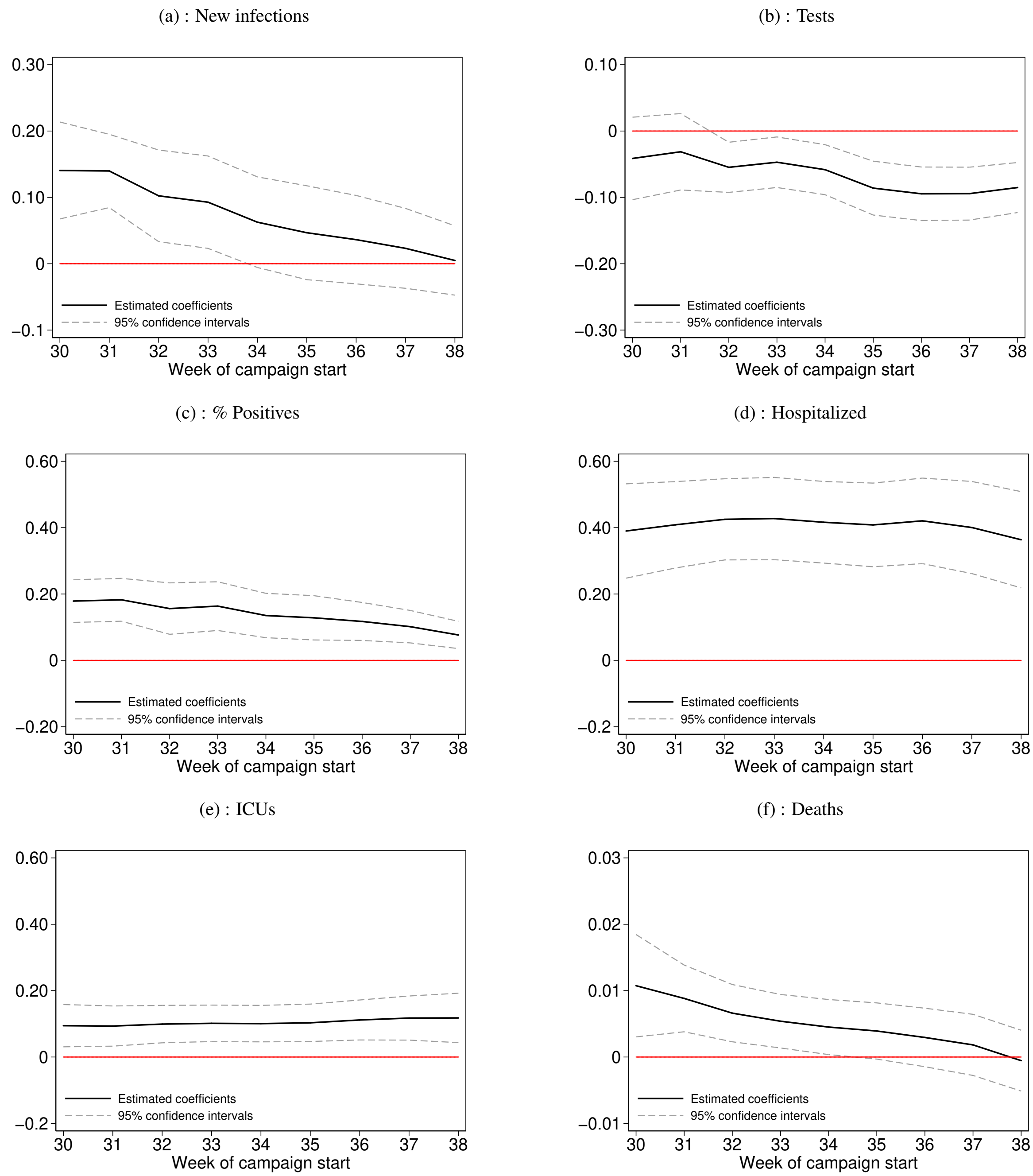
Notes: The table shows the differential impact of electoral campaign between regions voting both for the referendum and the regional election (treatment) and those only voting for the referendum (control), as obtained through the estimation of Equation 1 showed in the article. All dependent variables are the natural logarithm of the seven-day moving average of the daily number at the regional level. See Section 3 for details. $Campaign$ is a dummy equal to 1 for treatment regions, and 0 otherwise. $Post$ is a dummy equal to 1 for all the dates since August 21st, and 0 otherwise. All columns include region fixed effects, pre-treatment trends in region-specific characteristics and date-adjacent regions fixed effects. Standard errors are robust to serial and spatial correlation and computed imposing maximum cutoff for both spatial and time lags. *,**,*** represent the 10%, 5%, 1% significance levels, respectively.

Figure A.2: Dynamic Specification - Daily coefficients



Notes: The figures report estimates and 95% confidence intervals for the coefficients of the interactions included in Equation 2, between the dummy $Campaign$, which is equal to one for treatment regions, and the week dummies. $day = -1$ is the reference day (i.e., August 20, 2020). All dependent variables are the natural logarithm of the seven-day moving average of the daily number at the regional level. See Section 3 for details. $Campaign$ is a dummy equal to 1 for treatment regions, and 0 otherwise. Confidence intervals are based on standard errors that are robust to serial and spatial correlation and computed imposing maximum cutoff for both spatial and time lags. In all panels the vertical dashed line represents the date in which the electoral campaign officially started (i.e. August 21st).

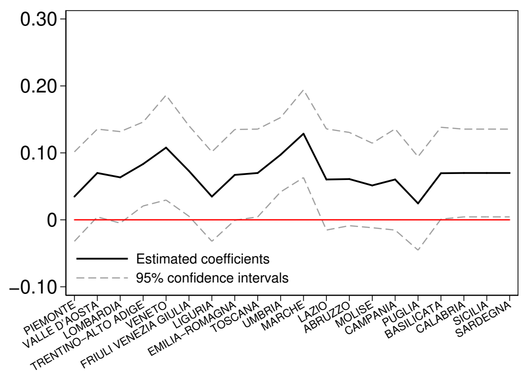
Figure A.3: Robustness Test II: Start of the Electoral Campaign



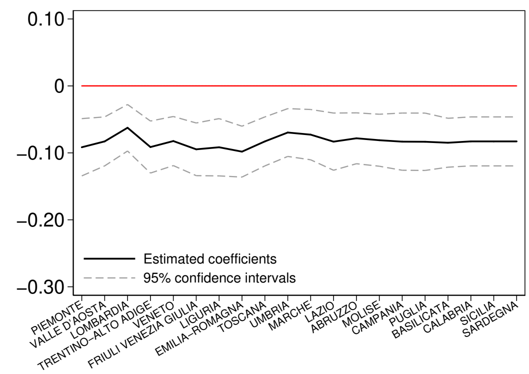
Notes: The figures shows the differential impact of electoral campaign between regions voting both for the referendum and the regional election (treatment) and those only voting for the referendum (control), as obtained through the estimation of Equation 1 showed in the article, for different definitions of the starting week of the electoral campaign (horizontal axis), and together with the 95% confidence intervals (dashed lines). All dependent variables are the natural logarithm of the seven-day moving average of the daily number at the regional level. See Section 3 of the article for details. $Campaign$ is a dummy equal to 1 for treatment regions, and 0 otherwise. Confidence intervals are based on standard errors that are robust to serial and spatial correlation and computed imposing maximum cutoff for both spatial and time lags.

Figure A.4: Robustness Test III: Dropping one Region at the Time

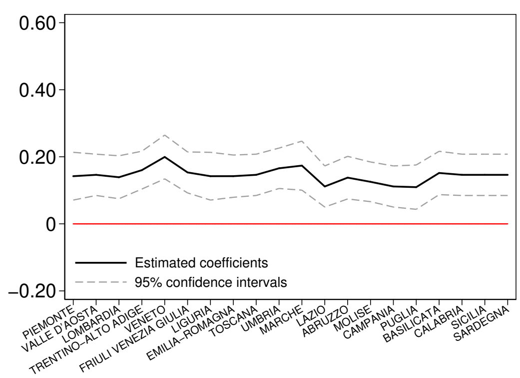
(a) : New infections



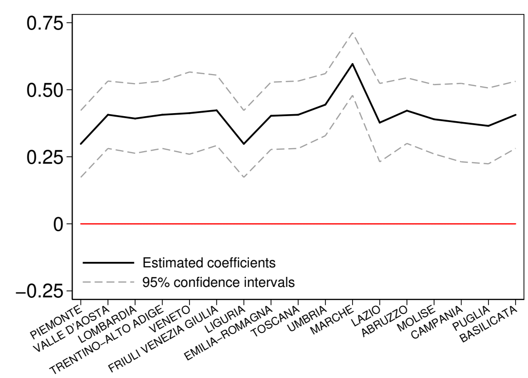
(b) : Tests



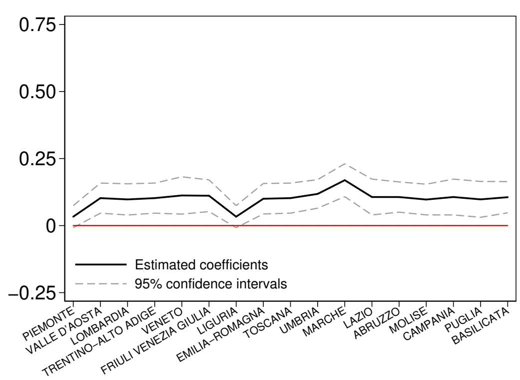
(c) : % Positives



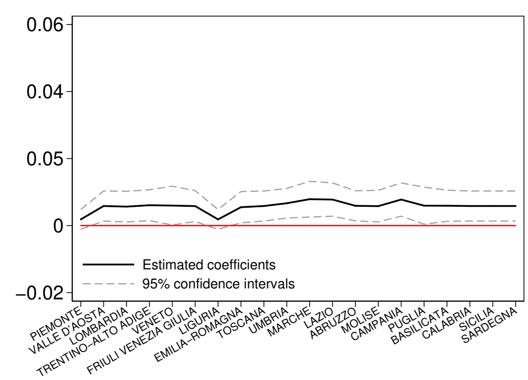
(d) : Hospitalized



(e) : ICUs

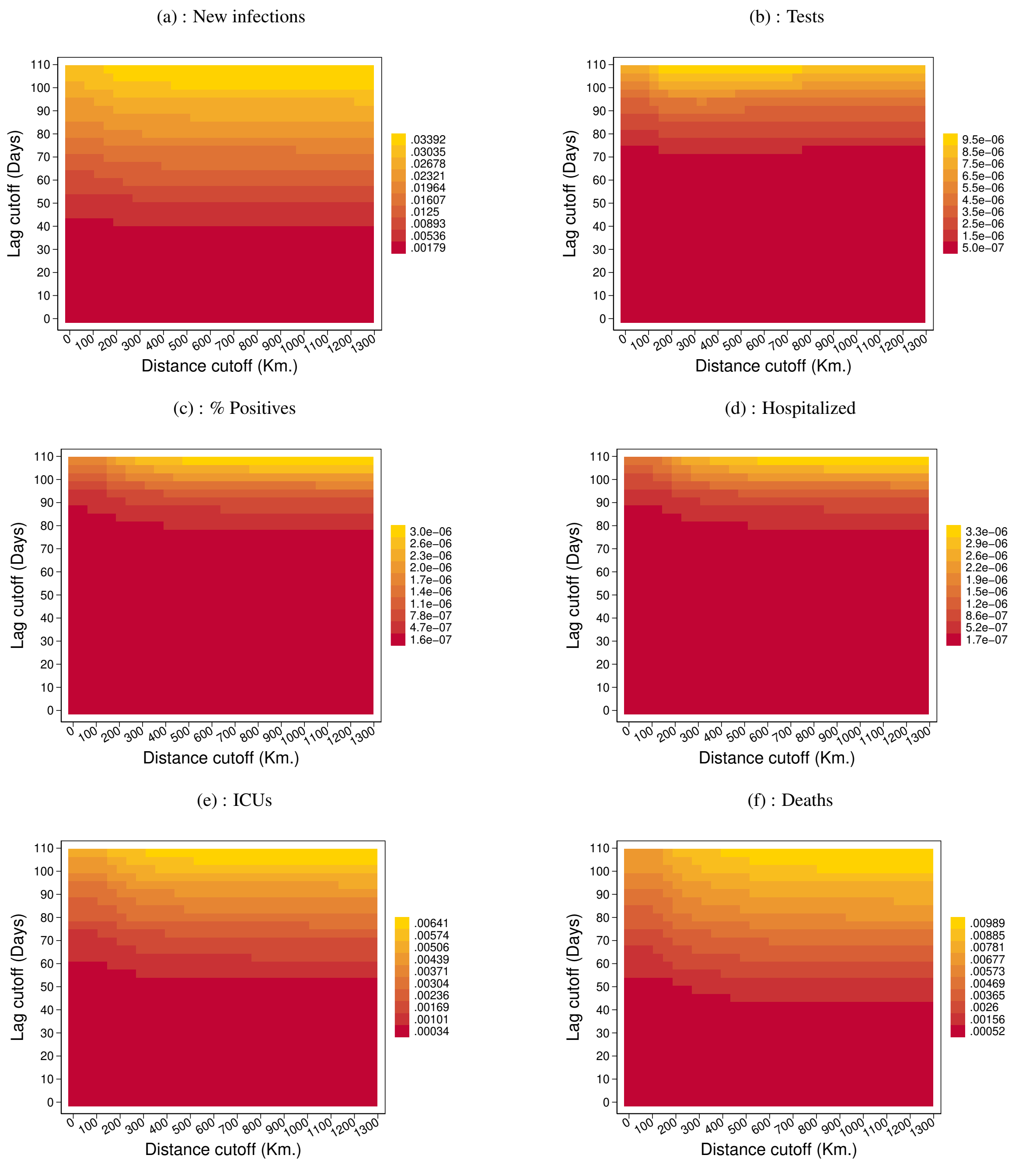


(f) : Deaths



Notes: The figures shows the differential impact of electoral campaign between regions voting both for the referendum and the regional election (treatment) and those only voting for the referendum (control), as obtained through the estimation of Equation 1 showed in the article, for different sub-samples of observations from which the region specified on the horizontal axis has been removed, and together with the 95% confidence intervals (dashed lines). All dependent variables are the natural logarithm of the seven-day moving average of the daily number at the regional level. See Section 3 of the article for details. $Campaign$ is a dummy equal to 1 for treatment regions, and 0 otherwise. Confidence intervals are based on standard errors that are robust to serial and spatial correlation and computed imposing maximum cutoff for both spatial and time lags.

Figure A.5: Robustness Test IV: Conley HAC Standard Errors



Notes: The figure reports p-values from estimating Equation 1 showed in the article in multiple regressions using Conley HAC standard errors (Conley, 1999) with time and spatial lags vary from their minimum to their maximum feasible value. Each iteration represents and increase to the distance cutoff equal to 25 km. or an increase in the time cutoff equal to 3 days. All dependent variables are the natural logarithm of the seven-day moving average of the daily number at the regional level. See Section 3 of the article for details. $Campaign$ is a dummy equal to 1 for treatment regions, and 0 otherwise. Confidence intervals are based on standard errors that are robust to serial and spatial correlation and computed imposing maximum cutoff for both spatial and time lags.

Table A.3: Robustness Test V: Estimates using weekly averages

	(1) Infections	(2) Tests	(3) % Posit.	(4) Hospit.	(5) ICUs	(6) Deaths
Campaign × Post	0.083*	-0.078**	0.169***	0.242***	0.058**	0.006*
	(0.044)	(0.031)	(0.042)	(0.065)	(0.026)	(0.003)
Observations	300	300	300	300	300	300
Baseline (value)	0.290	75.776	0.422	0.913	0.032	0.011
Region FE	✓	✓	✓	✓	✓	✓
Regional pre-trends	✓	✓	✓	✓	✓	✓
Date × Adjacent reg. FE	✓	✓	✓	✓	✓	✓

Notes: The table shows the differential impact of electoral campaign between regions voting both for the referendum and the regional election (treatment) and those only voting for the referendum (control), as obtained through the estimation of Equation 1 showed in the article. All dependent variables are the natural logarithm of the weekly average at the regional level. $Campaign$ is a dummy equal to 1 for treatment regions, and 0 otherwise. $Post$ is a dummy equal to 1 for all the dates since August 21st, and 0 otherwise. Standard errors are robust to serial and spatial correlation and computed imposing maximum cutoff for both spatial and time lags. *,**,*** represent the 10%, 5%, 1% significance levels, respectively.