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The impact of 2020 French municipal elections on the spread of COVID-19

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Abstract

Soon after the onset of the COVID-19 pandemic, the French government decided to still hold the first round of the 2020 municipal elections as scheduled on March 15. What was the impact of these elections on the spread of COVID-19 in France? Answering this question leads to intricate econometric issues as omitted variables may drive both epidemiological dynamics and electoral turnout, and as a national lockdown was imposed at almost the same time as the elections. In order to disentangle the effect of the elections from that of confounding factors, we first predict each department's epidemiological dynamics using information up to the election. We then take advantage of differences in electoral turnout across departments to identify the impact of the election on prediction errors in hospitalizations. We report a detrimental effect of the first round of the election on hospitalizations in locations that were already at relatively advanced stages of the epidemic. Estimates suggest that the elections accounted for at least 3,000 hospitalizations, or 11% of all hospitalizations by the end of March. Given the sizable health cost of holding elections during an epidemic, promoting ways of voting that reduce exposure to COVID-19 is key until the pandemic shows signs of abating.

Keywords COVID-19 · Hospitalizations · Electoral turnout · Municipal elections · Prediction errors

JEL Classification I18 · I10 · D72 · P16

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1 Introduction

The two rounds of 2020 French municipal elections were planned to take place on March 15 and 22, 2020. By the beginning of March, the early spread of the COVID-19 epidemic led to a debate in France about whether the first round should be postponed. This option was finally rejected and Emmanuel Macron—the French President—announced on the evening of March 12 that the election would take place as planned. This decision was accompanied by the announcement of the closing of all schools and universities by March 16 and was followed by an announcement by Edouard Philippe—the then-Prime Minister—on March 14 about the closing of all non-essential public spaces by the next day to prevent the spread of COVID-19. This marks the start of anti-contagion policies in France.

According to an Odoxa opinion poll (Odoxa 2020) published on March 12, 64% of French people approved of the decision to maintain the election and 61% of voters reported that the epidemic would not change their decision to vote. On March 15, 19,863,660 out of 44,650,472 voters in metropolitan France cast their vote, with no alternative but to go to the voting booth in order to do so. Although people were advised to maintain distance while voting and to use hand sanitizer, there was little availability of masks at the time and the recommendation for the general public was to not wear them since it was then believed that COVID-19 mainly spread via droplets rather than via aerosols, so that maintaining distance and hand hygiene would be enough to prevent contamination. On March 16, Emmanuel Macron announced that strict lockdown measures would be put in place from March 17 onwards and that the second round of the municipal elections was postponed *sine die*. The second round eventually took place on June 28, 2020, after anti-contagion policies had drastically reduced the circulation of COVID-19.

In this paper, we show that the first round of 2020 municipal elections caused an acceleration of the COVID-19 epidemic in metropolitan France. Our estimates suggest that elections accounted for at least 3,000 excess hospitalizations by the end of March, which represents 11% of all hospitalizations by this time.

Our methodology takes advantage of electoral turnout differences between *départements*—the third-highest administrative level—to distinguish the impact of the election on hospitalizations from that of simultaneously implemented anti-contagion policies. Our approach builds on methods from the abnormal financial returns and public policies evaluation literatures.¹ We proceed in two steps. We first fit for each department a simple epidemic model of hospitalizations for COVID-19 suspicion over the period that excludes hospitalizations that might relate to events

¹ See MacKinlay (1997), Duflo (2001), Fisman (2001), Guidolin and La Ferrara (2007), DellaVigna and Ferrara (2010), Coulomb and Sangnier (2014), and Cassan (2019) among others.

that took place by March 15 or in the following days. We then use these models to predict the evolution of the epidemic in each department if propagation conditions were held constant and compute daily predictions errors as the difference between the realized and predicted cumulative number of hospitalizations in each department.

Second, we relate prediction errors to turnout and to differences in the epidemic stage across departments by that date. This approach allows us to assess the causal effect of elections on hospitalizations while accounting for other contemporaneous events, such as anti-contagion policies, that were *a priori* uniform throughout the country. This approach explicitly accounts for different dynamics at the local level and builds on the assumption that prediction errors should not be related to turnout and epidemic stage on March 15 in the absence of an effect of the election on hospitalizations. We show that post-calibration errors are increasing with turnout in departments where COVID-19 was actively circulating on the day of the election. In contrast, turnout is not related to post-calibration errors in locations with low COVID-19 activity by March 15. These results reveal the impact of the first round of municipal elections on the COVID-19 epidemic. Applying the same methodology to the second round of the election, held on June 28 after a severe lockdown was implemented, we do not find a detrimental effect of the second round.

Our identification strategy is akin to a quadruple-differences method, effectively taking advantage of the following differences: the within-department difference between realized and predicted hospitalizations; the within-department difference between periods before and after the election; the between-departments difference in electoral turnout; and the between-departments difference in epidemic stage on election day. This combination of differences allows us to assess the causal impact of the elections on hospitalizations related to COVID-19. Importantly, our estimation strategy also allows us to effectively remove the first-order effect of factors that might explain differences in the dynamics of the epidemic, such as population density or the population structure.

As highlighted by Hsiang et al. (2020), most studies that analyze the impact of policies on COVID-19 rely on complex epidemiological models that require a detailed knowledge of the fundamental epidemiological parameters of the epidemic. Our approach, taken from the standard methods of reduced-form econometrics commonly used to assess the impact of public policies (Angrist and Pischke 2009), does not require such detailed information. It allows us to disentangle the impact of the election from others confounding shocks that may have hidden it without requiring much information about mechanisms of the epidemic.

The structure of the paper is as follows. Section 2 reviews the literature on the link between elections and the COVID-19 spread. Section 3 presents and discusses our methodology. Section 4 presents the results of our analysis of March 15 elections. Section 5 retrospectively discusses the expected impact of the second round of the municipal elections. Finally, Section 6 concludes.

2 Literature review

A large and growing literature aims at evaluating the impact of various events on the COVID-19 spread.² In this literature, numerous studies investigate the role of elections on the spread of COVID-19.³ They can broadly be categorized based on the two types of approaches they adopt.

A first strand of papers relies on epidemiological methods. Berry et al. (2020) report no impact of the Wisconsin primary elections, comparing the epidemic trajectory of Wisconsin to that of the rest of the USA, i.e., assuming that, absent the primary elections, Wisconsin would have followed the same epidemic trajectory as other US states. Leung et al. (2020) also study the Wisconsin primary elections, with a simple before-after epidemiological approach, showing that the number of cases and the effective reproduction number did not increase after the primary. Due to its before-after methodology, this paper does not use a counterfactual and relies on the strong assumption that the impact of the elections on the epidemic would have been so strong so as to be detectable directly by looking at changes of the fundamental parameters of the epidemic. Zeitoun et al. (2021) take a similar approach to study the French municipal elections. They compare the post-election epidemic trajectories of departments with high turnout and low turnout. While this method improves on the simple before-after difference used by Leung et al. (2020), it does not allow departments to follow idiosyncratic pre- and post-elections trajectories. It amounts to assume that departments would have followed similar epidemic dynamics if elections had not taken place, no matter how different their underlying characteristics (e.g., population density or share of elderly population) are. It leads (Zeitoun et al. 2021) to report no effect of the French local elections on the spread of the epidemic in France. Finally, Duchemin et al. (2020) present a Bayesian investigation of an epidemiological model that uses the number of deaths at the regional level to assess the effect of a variety of events on the COVID-19 epidemic in France. They report no effect of the first round of municipal elections. However, Duchemin et al. (2020) make clear that their approach would be able to uncover such an effect only if massive.

A second strand of papers uses econometrics methods. Feltham et al. (2020) study the presidential primary elections in the USA. They find no evidence of effect of these elections on the COVID-19 dynamics by using two methods: a matching difference-in-differences between counties with elections and counties without, and an epidemiological model. This paper relies on the assumption that the effect of holding an election would be the same no matter the turnout and no matter the local

²For example, Fang et al. (2020), Qiu et al. (2020), Brauner et al. (2021), Amuedo-Dorantes et al. (2021), Bonacini et al. (2021), Deopa and Fortunato (2021), Kahanec et al. (2021), and Juranek and Zoutman (2021) look at the effect of policies against the COVID-19 spread. Bernheim et al. (2020), Dave et al. (2020a), Dave et al. (2020b), Dave et al. (2021), and Harris (2020) study the role of various events in the COVID-19 spread. See also Adda (2016) for evidence about the role of economic activities on viral spread.

³There is also a literature that studies the impact of the COVID-19 epidemic on electoral outcomes. See, for example, (Baccini et al. 2021; Leromain and Vannoonrenbergh 2021; Pulejo and Querubín 2021).

spread of the epidemic, making the findings potentially biased downwards by elections taking place in area with low COVID-19 circulation or with low turnout. Palguta et al. (2022) is an ambitious paper that studies the October 2020 Czech elections. These elections provide a interesting natural experiment as, for a random subset of constituencies, a one round local election was combined with a two rounds national election. For this random subset of constituencies, turnout was higher in the local elections (which combined both the one round local election and the first round of the national election) and a second round took place. They compare the evolution of the epidemic in constituencies with two elections and two rounds of elections to that in constituencies with only one election and one round. Palguta et al. (2022) find that the former group experienced an increase in the number of cases and hospitalizations, but no increase in tests positivity rate. This interesting finding is hard to interpret as it is impossible to disentangle how much of the change in epidemiological trajectories is due to the higher turnout in the first round of the election relatively to the holding of a second round. Bertoli et al. (2020) study the effect of the French municipal elections on excess mortality at home in the subset of French municipalities that have no hospital. Using an instrumental variable approach to predict turnout at the very local level, they report a qualitatively strong impact of the election on excess mortality. However, Bach et al. (2021) provide evidence that results reported by Bertoli et al. (2020) are driven by measurement error. Using individual level data and various econometrics methods, Bach et al. (2021) show that local politicians who participated in the 2020 French municipal elections did not face a higher mortality risk after the elections. This finding can be interpreted in two rather different fashions. Either that the excess hospitalizations we uncover did not predominantly concern this very specific population or, that candidates, being more healthy than the general population of the same age, may have been infected and hospitalized because of the election, but did not die.⁴

Our approach differs from these studies in several dimensions. As we combine an epidemiological approach with econometric methods, we take advantage of both approaches while avoiding to rely on unrealistic assumptions that each approach taken alone requires: we explicitly model each département's counterfactual epidemiological evolution, absent both the election and the containment measures, which prevents us from comparing very different areas with one another as most of the epidemiological literature does and which may prove problematic given the highly non linear nature of epidemiological spread. From the econometrics approach, we take advantage of quadruple differences methods that avoid the unrealistic assumption behind the before-after approaches taken in the epidemiological literature, which can not account for other shocks, such as changes in containment policies for example. In addition, we explicitly look into how the holding of the election may have different consequences as a function of the initial level of circulation of the virus.

This allows us to present findings that are both methodologically sound and easily interpretable. In addition, the context of the French municipal elections allows for a

⁴Using only mortality data, Bach et al. (2021) can not distinguish between these two opposite interpretations.

precise interpretation of our findings. Indeed, since a severe lockdown started almost at the same time as the elections, and that only one round of the election took place in March, we precisely know what our estimates are capturing: the causal effect on hospitalizations of having gone voting on March 15. Because of the lockdown, chains of contagions were almost entirely cut, except within household. That is, the hospitalizations that we capture can only be caused by direct contamination while voting or indirect contamination within the household by a person contaminated while voting. This is as close as one can realistically be to measuring the direct effect of the election on contaminations.

3 Data and methodology

This section presents the data used in this paper as well as the methodology we rely on to assess the impact of the March 15 elections on the spread of the COVID-19 epidemic.

3.1 Data

Our analysis relies on two main datasets: hospitalizations for COVID-19 suspicion and electoral turnout at the 2020 French municipal elections.

In the absence of systematic testing policy by spring 2020, incidence rates cannot be used to measure the epidemic spread. Hospitalization data are the best data that allow to accurately observe the epidemic by this time. French hospitalization data (Santé publique France 2022) are open access data published by a governmental agency. Data are based on hospitals' reports and present the daily counts of hospitalization decisions for COVID-19 suspicion at department level from February 24 onwards. These widely trusted data are the official source for COVID-19 related hospitalizations in France.

2020 electoral turnout data (Ministère de l'intérieur 2020) for the first round of municipal elections are official electoral records available at the city-level. We aggregated turnout data at the department level.

3.2 Methodology

We use the daily cumulated number of hospitalizations for COVID-19 suspicion to fit a series of department-level epidemic trajectories up to the date at which individuals contaminated on March 15 start being hospitalized. We separately estimate the following standard logistic model of epidemiological trajectory for each department d :

$$\text{Cumulated hospitalizations}_{d,t} = \frac{a_d}{1 + \exp(-b_d(t - c_d))}, \quad (1)$$

where a_d , b_d and c_d capture the asymptotic level, the inflection date and the scale of the epidemic trajectory in department d , respectively. We estimate Eq. 1 using all dates t until March 26, i.e., 11 days after the elections took place. This 11-day lag is one day shorter than the median estimate of the number of days from infection to

hospitalization suggested by the clinical studies literature.^{5,6} As a result, the model's forecasts can be interpreted as departments trajectories in the absence of any event that took place since March 15.

We estimated model Eq. 1 for each of the 96 departments of metropolitan France. The model was successfully estimated for 91 departments. The 5 departments for which we are not able to calibrate the model are departments that do not exhibit sufficient variation in hospitalizations until March 26 to allow for parameters' estimation. These departments account for 1.6% of the total French population.

Following insights from the literature on short term epidemiological forecast (see Chowell et al. 2019, Roosa et al. 2020a, b among others), we use the series of estimated parameters $\hat{a}_d$, $\hat{b}_d$ and $\hat{c}_d$ to predict for each department the daily cumulated number of hospitalizations up to 7 days after the end of the calibration period, i.e., up to April 2.^{7,8}

Predicted trajectories proxy the evolution of the epidemic in each department in the absence of the election and of any other shock contemporary or posterior to the election, such as lockdown policies. We next use the actual number of hospitalizations for COVID-19 suspicion in each department to construct prediction errors in hospitalizations per 100,000 inhabitants. As shown by Appendix Fig. 9(a) and (b), predictions errors are generally positive over the post-calibration period, which suggests that most departments surpass their predicted epidemic trajectory after March 15. Our interest is however not to assess whether it is possible to correctly predict the evolution of the epidemic, nor to estimate whether policies implemented after this date were able to twist trajectories. In contrast, our interest lies in whether deviations of epidemic trajectories depend on the March 15 elections.

We take advantage of two sources of variations to assess whether the March 15 elections impacted the spread of the COVID-19 epidemic. First, we distinguish between departments depending on the local stage of the epidemic by the day of the election. Second, we use differences in electoral turnout to proxy for difference in

⁵Using Chinese data, Li et al. (2020), Chan et al. (2020), and Guan et al. (2020) estimate that the time from infection to onset of symptoms is between 4 and 5 days. Li et al. (2020), Huang et al. (2020), Wang et al. (2020), Cai et al. (2020), Chan et al. (2020), and Chen et al. (2020) and (Guan et al. 2020) estimate that the time from symptoms to hospitalization is between 5 and 12 days. The French Institut Pasteur (2020) relies on these estimates to announce a 5-day period from infection to onset of symptoms, followed by a 7-day period from symptoms to hospitalization.

⁶We show in Section 4.3 that results are robust to the use of 10 or 12 days instead of 11.

⁷The literature on forecasting the COVID-19 spread in the very early days of the epidemic tends to focus on relatively short term predictions, typically between 5 and 15 days. See, for example, Roosa et al. (2020a) and Roosa et al. (2020b) and Read et al. (2021). Forecasts made at later stages of epidemic can use a longer horizon as they benefit from a long time span on which to fit their model, making the prediction more precise over a longer period of time. See, for example, Tariq et al. (2021). Also see Chowell (2017) about the link between the length of the period of fit and the quality of the forecast and Chowell and Viboud (2016) about the unprecision of forecasts in the early days of an epidemic.

⁸We show in Section 4.3 that doubling the forecasting period to 2 weeks does not alter the results.

exposure across departments at comparable stages of the epidemic. Accordingly, we estimate the following expression:

$$\begin{aligned} \text{Prediction error}_{d,t} = & \sum_{t=1}^T \beta_t \times \text{Turnout}_d \times (1 - \text{High COVID-19 circulation}_d) \times \tau_t \\ & + \sum_{t=1}^T \gamma_t \times \text{Turnout}_d \times \text{High COVID-19 circulation}_d \times \tau_t \\ & + \sum_{t=1}^T \delta_t \times \tau_t \\ & + \sum_{t=1}^T \zeta_t \times \text{High COVID-19 circulation}_d \times \tau_t \\ & + \mathbf{Y}_d + \mathbf{Z}_{d,t} + \alpha + \epsilon_{d,t}, \end{aligned} \quad (2)$$

where $\text{Prediction error}_{d,t}$ is the difference between actual and predicted cumulated hospitalizations per 100,000 inhabitants in department d on day t , Turnout_d is electoral turnout on March 15 in department d , τ_t is a variable equal to 1 on day t , $\text{High COVID-19 circulation}_d$ is a variable equal to 1 for departments at advanced stages of the epidemic on March 15, series of δ and ζ coefficients account for daily patterns in prediction errors across departments in both groups, $\mathbf{Y}_d$ is a vector of department fixed effects which account for department-specific patterns, α is a constant term, and $\epsilon_{d,t}$ is the error term.

$\mathbf{Z}_{d,t}$ is a vector of interactions between day fixed effects and two sets of departments' characteristics. We first include the population density and the share of population aged above 60 as time invariant characteristics that might affect the dynamics of the epidemic at the department-level. We collect official total population and population aged above 60 in each department from (Institut national de la statistique et des études économiques 2021) official population records and construct population density at the department level using departments area information (OpenStreetMap 2018).

Second, we control for two department-specific shocks that might be associated with both turnout and the local dynamics of the epidemic after the election: local weather conditions on March 15 and population compliance with lockdown restrictions. We obtain meteorological information from Météo-France (2021), average station-level data into department-level series, construct an index of weather conditions from the first principal component of daily precipitations and mean temperature in each department, and save the value of this index on March 15.⁹ Changes in behavior may also problematic for our estimation is they turned out to be correlated with our prediction error, turnout and epidemic spread on election day. As the French government implemented a national lockdown, we expect this changes in behavior to vary homogeneously across departments. However, one may worry that the compliance with the lockdown varies across departments. No official information allow to measure population compliance with anti-contagion policies. To overcome this lack of information, we proxy for compliance with lockdown restrictions at the department-level using the ratio from mobility during the lockdown period to mobility in normal times, as originally constructed from smartphones geo-located data by GEO4CAST's *Covimoov* application and made available for March 26 and April 2

⁹There are no meteorological station in 5 departments of metropolitan France. For each of these departments, we reconstructed daily meteorological information from the average of values observed in immediately neighbouring departments.

in a *Le Journal du Dimanche*'s (2021) newspaper article. We construct an index of compliance with lockdown restrictions from the average across these two dates in each department.¹⁰ As the effect of these shocks will likely differ depending on the extent of epidemic spread on the election day—which also corresponds to the eve of the start of strict lockdown—, we further interact their daily coefficients with the High COVID-19 circulation variable.

Consistent with the aforementioned 11-day lag between infection and hospitalization, we use cumulated hospitalizations per 100,000 inhabitants on March 26 to construct the High COVID-19 circulation variable that distinguishes between departments depending on the stage of the epidemic by the day of the election. We arbitrarily distinguish between departments in the bottom third of the COVID-19 epidemic according to this measure and others.¹¹ The latter are considered as locations at relatively more advanced stages of the epidemic.¹²

As shown by Appendix Fig. 9, some departments exhibit very large prediction errors compared to others as we move away from the end of the calibration period. This feature is likely to let outliers drive the estimation of coefficients of interest for these days. We mitigate this threat by weighting post-calibration observations by the inverse of the standard errors of daily predictions returned by model Eq. 1.

We estimate expression Eq. 2 using the abovementioned weights in a linear regression and two-way standard errors clustered at the department and day levels. The sample is made of the 91 departments for which model Eq. 1 was successfully estimated and of all days from March 1 to April 2, 2020.

In expression Eq. 2, the main parameters of interest are the estimated series of β_t and γ_t . These coefficients indicate the impact of electoral turnout on hospitalizations for departments with low and high COVID-19 circulation by the day of the elections, respectively. Under the assumption that the March 15 elections impacted epidemic trajectories only in locations that were at advanced stages of the epidemic by that day, we expect β s to be close to zero and γ s to be positive in the post-calibration period.

3.3 Threat to identification

A key assumption for the above presented approach to allow us to safely assess the impact of municipal elections on the dynamics of the COVID-19 epidemic is that electoral turnout on March 15 is unrelated to the stage of the epidemic by that date. Namely, turnout was low as only 45% of voters cast their vote, compared to 64% at the 2014 municipal elections. There is a wide consensus in the French society that this low turnout was mainly caused by the fear of contagion. This might actually be the case but would be a threat to identification only if differences in turnout across departments ended up being related to differences in the epidemic across departments. We find no evidence of such a correlation between the level of turnout in a

¹⁰Maps available from *Le Journal du Dimanche* (2021) display mobility ratio in 5% intervals. We randomly draw a value within each interval to construct averages within departments across dates.

¹¹As shown by Appendix Fig. 10, this threshold correspond to 14 hospitalizations per 100,000 inhabitants.

¹²We show in Section 4.3 that results are robust to alternative definitions of the threshold used to distinguish between departments with low or high COVID-19 circulation on the elections day.

department and the information on the spread of the epidemic in that department on the day of the election. This is best illustrated by Fig. 1(a) which plots turnout against publicly known cumulated hospitalizations on March 15. Turnout appears evenly distributed at each stage of the epidemic.

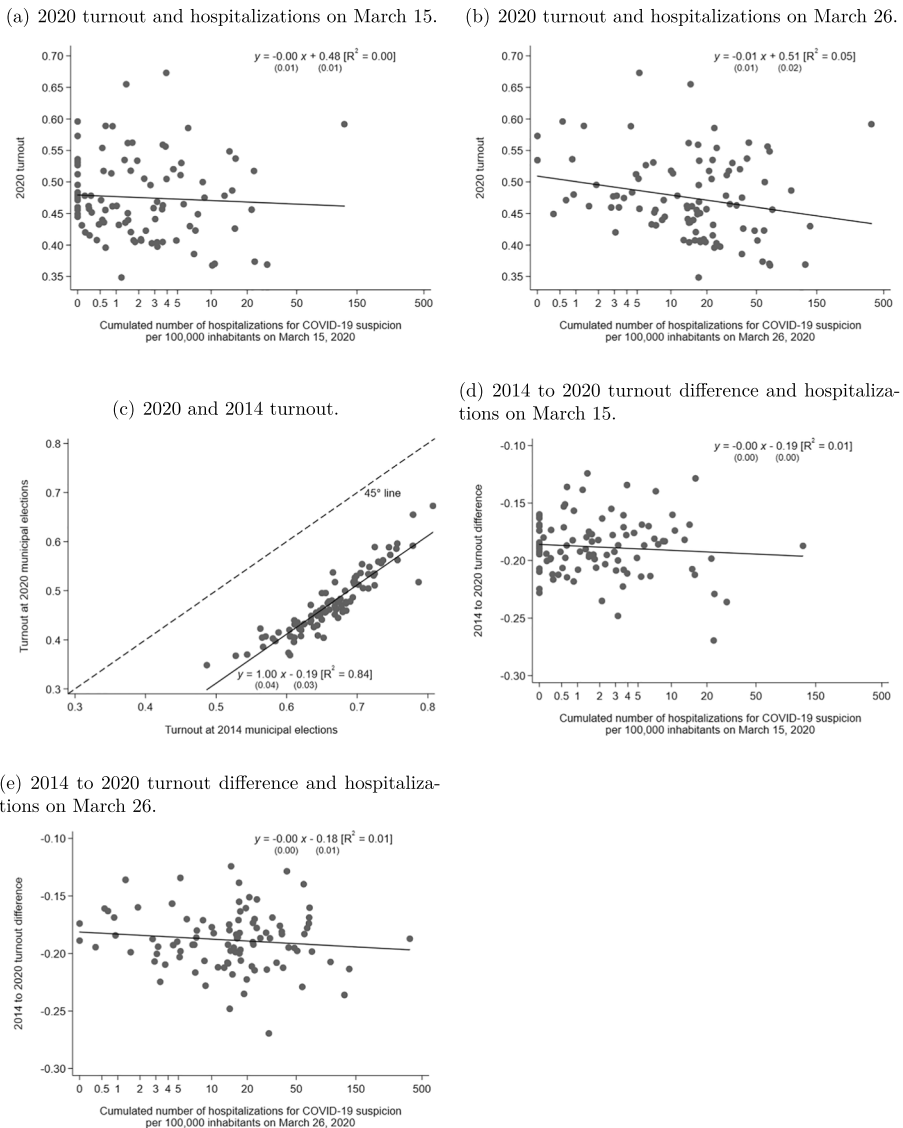


Fig. 1 Electoral turnout and the COVID-19 epidemic. Sources: Authors' calculation using Santé publique France, Ministère de l'intérieur and Institut national de la statistique et des études économiques data. Regression lines of each sub-figure represent the linear relationship between the variable represented on the y-axis and the variable represented in the x-axis. Associated equations displays estimated coefficients and their standards errors (in parentheses)

Figure 1(b) further accounts for the 11-day lag from infection to hospitalization to better capture the underlying stage of the epidemic in each department and only reveals a weakly decreasing link between turnout and hospitalizations. In contrast, turnout at the 2020 municipal elections is strongly correlated with turnout at the preceding municipal elections that took place in 2014 as shown by Fig. 1(c) which we constructed by supplementing 2020 data with 2014 electoral turnout data (Ministère de l'Intérieur 2014). It shows that the shift in turnout was uniform across departments. Figure 1(d) and (e) further illustrate this claim by plotting the 2014 to 2020 turnout difference against cumulated hospitalizations on March 15 and 26, respectively.

All in all, while the COVID-19 epidemic might have impacted turnout at the 2020 municipal elections—a question that is beyond the scope of this paper—, differences in the spread of the epidemic by March 15 did not translate into differences in turnout across departments, thereby allowing us to confidently interpret estimates that will be delivered by our identification strategy.

3.4 Discussion of the methodology

The outcome of interest of our approach is the extent to which the first-step predictive model fails to predict the evolution of hospitalizations. In the absence of an effect of the election on hospitalization, our model should make similar errors of predictions across departments, no matter their turnout.

However, if the election had an effect on the epidemic, the prediction errors should be relatively larger in departments with relatively higher turnout. Indeed, if elections did contribute to spread the epidemic, the predictive model should underestimate by a larger amount the number of cases in departments with high turnout compared to departments with low electoral turnout. And this stronger underestimation should start only when individuals infected on the election day are hospitalized, not before. Similarly, the effect of turnout on the epidemic should only exist in departments in which contagious individuals are indeed present: a high turnout in a department with no or few contagious individuals should result in 0 additional contagions.

We therefore analyse prediction errors via a triple-difference approach: not only do we compare departments with high and low turnout before and after the elections, but we study how this double-difference varies between departments with very low infection rates around the election date and other departments. We would expect turnout to only have an effect on the epidemic in departments already affected by the epidemic at the time of the election. Since our outcome is a difference, and we analyse it using a triple difference, our methodology is akin to a quadruple differences approach. This alleviates concerns about omitted variables that one may have in the context of a double difference approach. Indeed, with this approach, an omitted variable is a threat to identification if and only if it is correlated : (i) with the difference between actual and predicted epidemic spread (not just the actual epidemic spread); (ii) with turnout; (iii) with time; (iv) with the level of epidemic spread on election day. Note in particular that many variables may be correlated with actual epidemic spread (think for example of the share of elderly). However, these variables would also directly contribute to our model's prediction. Therefore, while they may be correlated with actual epidemic spread, they will also be correlated with predicted

epidemic spread, but it is unlikely that they would be correlated with the difference between actual and predicted epidemic spread, together with the other 3 dimensions of correlation required for an omitted variable to be a threat to identification. Note however that it is still possible that *changes* in behavior post election may be both correlated with turnout, prediction error and epidemic spread on election day. Given the implementation of a full lockdown 24h after the election, we believe this is unlikely to have happened in practice. However, we explicitly controls for differences in the enforcement of lockdown across departments.

This approach has several advantages. First, it does not require blind faith in the ability of the predictive model to deliver accurate predictions. In fact, it does rely on the model's predictions being wrong while a priori uncorrelated with turnout under the null assumption that elections had no impact on the spread of the COVID-19 epidemic. Second, the event study aspect of the approach allows us to exactly observe when the prediction errors become correlated with turnout: predictions error should start being correlated with turnout *only* when people infected on the election day start showing up at hospital, that is, only when enough (but not too much) time has passed since the election for the symptoms to be severe enough to lead to hospitalization. This approach therefore automatically implements a sanity check as the correlation between the model's prediction errors and turnout should emerge with a lag compared to the election date, but not too long a lag.

A drawback of our approach is however that these type of simple predictive models are typically precise in the short run only, so that predictions are likely to become more and more noisy the further away we move from the end of the calibration period, which should result in imprecise estimates. This is the reason why we stop the analysis 7 days after the end of the model's fit. This time span is however likely to cover most of the additional hospitalizations that could be related to the March 15 elections as severe lockdown policies were implemented in the days that immediately followed the elections, thereby limiting further transmission by people who would have been contaminated on that day. In a robustness check, we extend the analysis up to 15 days after the election.

4 Results

In this section, we first present and interpret the results of the study. We next investigate their robustness.

4.1 Relationship between electoral turnout and hospitalizations

Figure 2 presents the series of β_t and γ_t coefficients estimated from Eq. 2. The series of β_t coefficients stays small and insignificant over both the calibration and prediction periods. This shows that turnout did not have any impact on hospitalizations in departments with very low infection rates on the day of the election. Similarly, the series of γ_t coefficients is close to zero and statistically insignificant over the calibration period. In contrast, this series starts to increase by March 27 and becomes

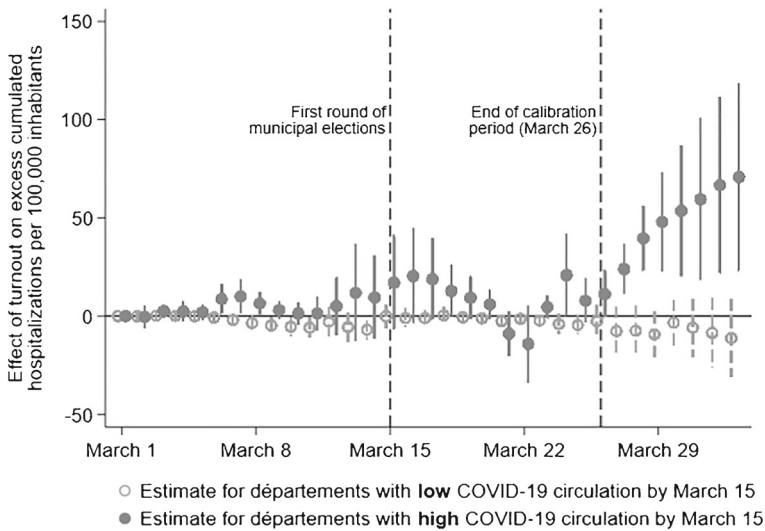


Fig. 2 Relationship between electoral turnout and excess hospitalizations. Estimates of β_t and γ_t from Eq. 2. See Section 3 for more details. Vertical lines are 95% confidence intervals. *Départements with high (low) COVID-19 circulation by March 15* are departments in the top two thirds (bottom third) of the distribution of cumulated hospitalizations for COVID-19 suspicion by March 26

unambiguously statistically significant. This suggests that turnout is positively associated with hospitalizations in departments in which there were a relatively high number of contagious individuals by the election day exactly 12 days after the day of the election, in line with the 12-day lag between infection and hospitalization estimated by the literature.

As discussed in Section 3, the uncovered positive relationship can be interpreted as evidence of a causal relationship from the election to hospitalizations. However, beyond the increasing pattern of the series of γ_t coefficients after March 27, Fig. 2 also displays increasing standard errors of the estimates as close as 3 days from the end of the calibration period. This feature calls for caution in the interpretation of the point estimates.

4.2 Quantification of the total effect

As shown by Appendix Table 1, γ coefficients estimated for March 27, 28 and 29 correspond to 23.9 (p -value=0.000), 39.4 (0.000) and 48.0 (0.000) excess cumulated hospitalizations per 100,000 inhabitants, respectively, for an hypothetical change in turnout from 0% to 100% in departments at relatively advanced stages of the epidemic by the election day. This contrasts with coefficients estimated on the following days that are larger and less precisely estimated. For instance, coefficients estimated for April 1 and 2 correspond to 66.7 (p -value = 0.005) and 70.7 (0.005) excess cumulated hospitalizations per 100,000 inhabitants, respectively, for the same hypothetical change.

Actual electoral turnout data can help us quantify the contribution of the March 15 elections to the COVID-19 epidemic. To this end, we use estimated coefficients of Eq. 2 and compute turnout-related excess cumulated hospitalizations per

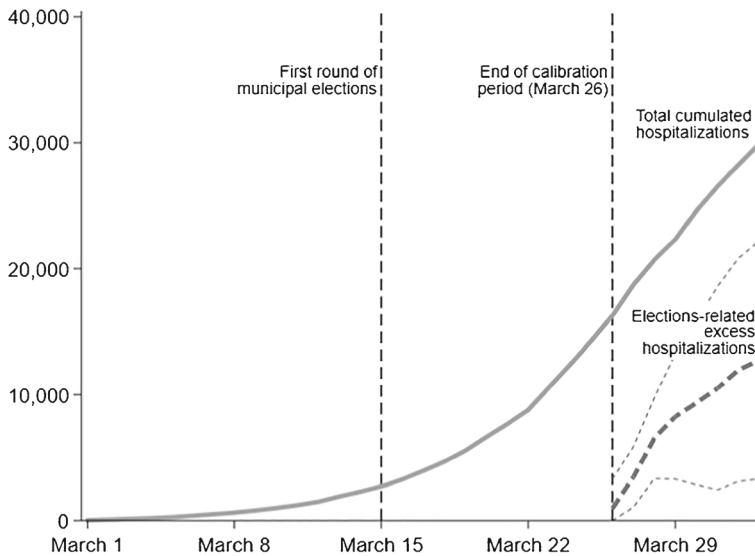


Fig. 3 Election-related excess hospitalizations. Daily election-related excess hospitalizations are computed using Eq. 3. Short dashed lines are bounds of 95% confidence intervals. See the text for more details

100,000 inhabitants on each day from the end of the calibration period to April 2 in departments that were at advanced stages of the epidemic on March 15 as:

$$\text{Excess}_{d,t} = (\hat{\gamma}_t - \bar{\gamma}_0) \times \text{Turnout}_d, \quad (3)$$

where $\bar{\gamma}_0$ is the average of γ estimates over the calibration period. We then multiply these figures by each department population to obtain absolute figures, set excess hospitalizations to zero in departments with low COVID-19 activity on the election day, and sum daily excess hospitalizations across departments. We proceed identically with the bounds of 95% confidence intervals of γ estimates. Figure 3 plots elections-related excess and actual cumulated hospitalizations at the national level. Our point estimates suggest that the March 15 municipal elections accounted for about 10,000 cumulated hospitalizations by the end of March. This figure represents about 40% of cumulated hospitalizations by that time. Estimates are however imprecisely estimated as discussed above. Namely, upper bounds of confidence intervals suggest that March 15 elections resulted in about 20,000 hospitalizations. More conservative figures conveyed by estimates' lower bounds suggest that elections resulted in at least 3,000 hospitalizations, which represents 11% of all cumulated hospitalizations for COVID-19 suspicion in metropolitan France by March 31. All in all results indicate that elections accounted for thousands hospitalizations.

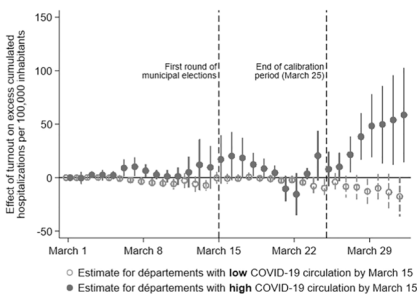
4.3 Robustness checks

We conducted a series of tests to demonstrate the robustness of reported results.

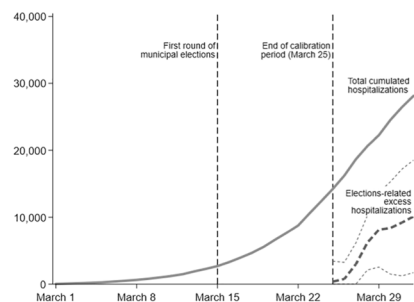
Ten or 12 days as time from infection to hospitalization

One of the key feature of the estimation framework we use is the fact that hospitalizations reflect the epidemic situation with some delay. In order to test the sensitivity of reported results to this feature, we replicated the data construction and estimation steps using 10 or 12 days, rather than 11 days, as lag from infection to hospitalization. In the first case, March 25 is thus used in lieu of March 26 as the date at which the calibration period ends and as the day at which we distinguish between departments with low or high COVID-19 circulation by the time of the municipal elections. As the prediction model is calibrated on a shorter period, model (1) is successfully estimated for only 88 out of the 96 departments. The 8 left-aside departments account for 4.0% of the French population. Figure 4(a) displays coefficients of interest when estimating Eq. 2 using March 25 in lieu of March 26. Although less precisely estimated, the patterns of coefficients over the days after the end of the calibration period is similar to that found using March 26. Importantly, the coefficient estimated for March 26 remains insignificant while this date is now included in the post-calibration period. This is consistent with effects of the elections showing up only after 11 days.

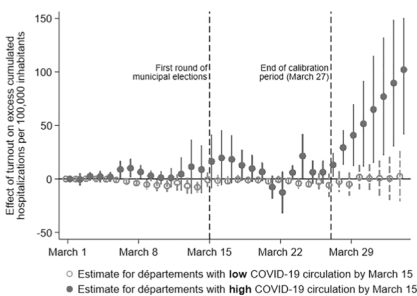
(a) Relationship between electoral turnout and excess hospitalizations, 10-day lag from infection to hospitalization.



(b) Election-related excess hospitalizations, 10-day lag from infection to hospitalization.



(c) Relationship between electoral turnout and excess hospitalizations, 12-day lag from infection to hospitalization.



(d) Election-related excess hospitalizations, 12-day lag from infection to hospitalization.

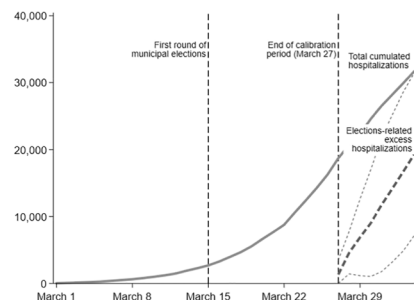
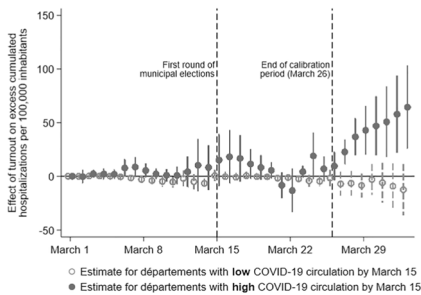


Fig. 4 Estimates using 10 or 12 days as time from infection to hospitalization. Sub-figures (a) and (c) mimic Fig. 2. Vertical lines are 95% confidence intervals. Sub-figures (b) and (d) mimic Fig. 3. Short dashed lines are bounds of 95% confidence intervals. Sub-figures (a) and (b) use March 25 in lieu of March 26. Sub-figures (c) and (d) use March 27 in lieu of March 26

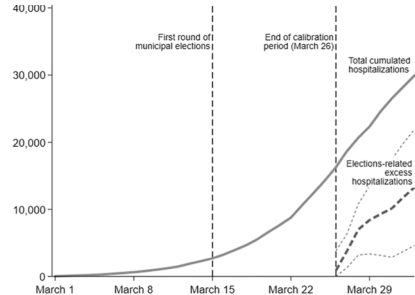
Figure 4(b) displays the corresponding total excess hospitalizations associated with the elections.

Figures 4(c) and (d) plot results we obtain when using 12 days as time from infection to hospitalization. March 27 is thus used in lieu of March 26. In this case, model Eq. 1 was successfully estimated for the same 91 departments as when using March 26. The patterns of the series of estimates of interest is qualitatively similar to baseline results. However, estimates of the effect of the elections become very large as we move away from the end of the calibration period. So do their standard errors. These observations suggest that these estimates are less reliable than baseline ones, presumably because March 27 is now included in the calibration period while this date is likely to already include the first excess hospitalizations linked to the March 15 municipal elections.

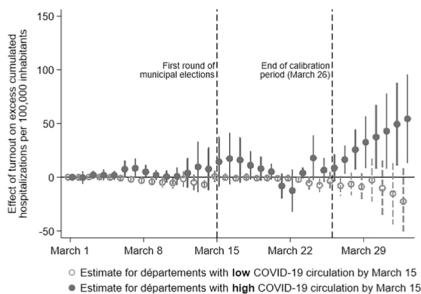
(a) Relationship between electoral turnout and excess hospitalizations, 25th percentile cut-off.



(b) Election-related excess hospitalizations, 25th percentile cut-off.



(c) Relationship between electoral turnout and excess hospitalizations, 7-days of hospitalizations increase.



(d) Election-related excess hospitalizations, 7-days of hospitalizations increase.

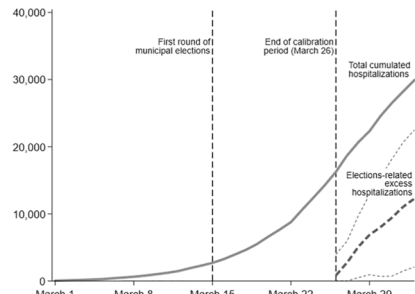


Fig. 5 Estimates using alternative definitions of advanced epidemic stage by March 15. Sub-figures (a) and (c) mimic Fig. 2. Vertical lines are 95% confidence intervals. Sub-figures (b) and (d) mimic Fig. 3. Short dashed lines are bounds of 95% confidence intervals. Sub-figures (a) and (b) use the 25th percentile of the distribution of cumulated hospitalizations per 100,000 inhabitants across departments on March 26 to construct the group of departments considered as at advanced stage of the epidemic by March 15. Sub-figures (c) and (d) identify the group of departments that experienced more than 7 days of increase in hospitalization until March 26 as departments with high COVID-19 circulation

Alternative definitions of advanced epidemic stage

One of the source of variation we exploit to identify the effect of March 15 elections on the COVID-19 epidemic is the circulation of the virus in departments on the day of the elections. We arbitrarily distinguished between departments using the first tercile of the distribution of cumulated hospitalizations per inhabitant by this date. We use two alternative definitions of high virus circulation to study whether reported results hold when modifying the abovementioned arbitrary choice.

We first use the 25th percentile of the distribution of cumulated hospitalizations per 100,000 inhabitants across departments on March 26 to construct the group of departments considered as at advanced stage of the epidemic by March 15. Figure 5(a) presents the estimated coefficients of Eq. 2 when using this alternative cut-off. Figure 5(b) plots the corresponding total excess hospitalizations associated with the elections.

Second, we identify departments that experienced more than 7 days of increase in hospitalization until March 26 as departments with high COVID-19 circulation. Figure 5(c) and (d) display results obtained when using this categorization.

As show by Fig. 5(a)–(d), both alternative definitions lead to results that are qualitatively and quantitatively similar to baseline ones.

Removing departments one by one

To test the sensitivity of results to a particular department, we re-estimate Eq. 2, but omitting each department one by one. Figure 6 displays the series of estimated coefficients. It allows us to visualize how the inclusion of each department affects

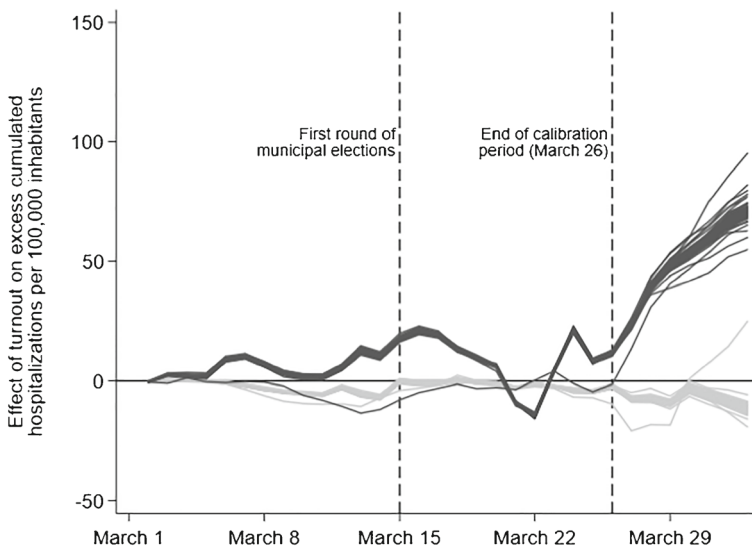


Fig. 6 Estimates removing departments one by one. Series of estimates of β_t and γ_t from Eq. 2 (see Section 3). Each line corresponds to a series of coefficients obtained when excluding a given department from the sample

estimates. While some series are actually distinct from others, thereby showing the large influence of some departments, the overall patterns are consistent with previously reported results.

Fourteen-day forecasts

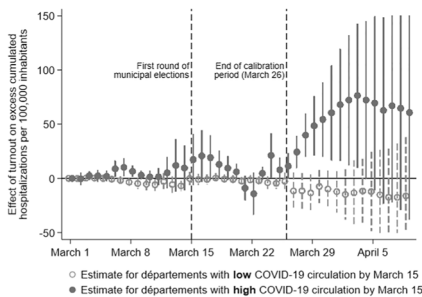
Our estimation framework builds on a simple logistic model that we calibrate for each French department and whose predictions are then used in a second step. Figure 7(a) and (b) display results obtained when extending the prediction and estimation period up to 14 days after the end of the calibration period. As shown by Fig. 7(a), standard errors of estimated coefficients that build on more than 1 week ahead forecasts becomes very large. This best illustrates the weak capacity of the estimation framework to apply to longer time horizons and highlights again that only short-term estimates can reasonably be trusted. However, results obtained for this longer period calls for two comments. First, the structures of estimated series is qualitatively similar to that obtained previously. Second, estimates of the relationship between turnout and excess hospitalizations reaches a plateau about 1 week after the end of the calibration period. This latter observation is consistent with elections-related excess hospitalizations we capture being circumvented to exposed individuals as strict lockdown was enforced after the elections.

5 Analysis of the second round

According to our analysis, measures implemented on March 15 to prevent contamination in voting stations by the first round of the 2020 French municipal elections were not fully effective and resulted in thousands additional hospitalizations.

On May 22, the French government announced that the second round of the municipal elections would take place on June 28 in municipalities in which no list gained majority in the first round. About 15,400,000 voters—mostly in the largest municipalities—were called to vote again. In Cassan and Marc (2020)—an earlier

(a) Relationship between electoral turnout and excess hospitalizations, 14-day forecast.



(b) Election-related excess hospitalizations, 10-day lag from infection to hospitalization.

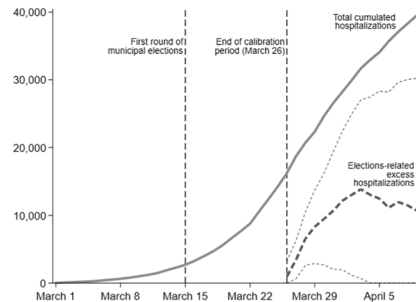


Fig. 7 Estimates using up to 14 days after the end of the calibration period. Sub-figures (a) and (b) mimic Figs. 2 and 3, but use observations up to 14 days after March 26. Vertical lines are 95% confidence intervals. Short dashed lines are bounds of 95% confidence intervals

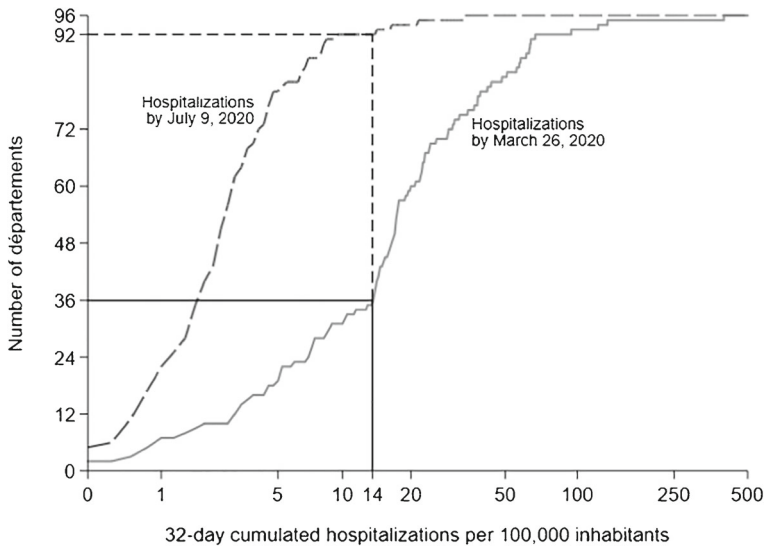


Fig. 8 Thirty-two-day cumulated hospitalizations per 100,000 inhabitants on March 26 and July 9. Ninety-six departments of metropolitan France. Distributions of hospitalizations per 100,000 inhabitants cumulated over 32 days until March 26 and July 9, 2020. The vertical line at 14 hospitalizations per 100,000 inhabitants corresponds to the bottom third of the distribution for March 26 when excluding the 5 departments for which model Eq. 1 cannot be calibrated because of insufficient variation in hospitalizations until March 26

version of this paper made public on June 22, 2020—we argued that it was very unlikely that holding elections on June 28 would cause a statistically detectable number of new contaminations. This prediction built on the fact that on June 19, when we last accessed available data, most departments qualified as safe places using the threshold above which we detected a worsening of epidemic trajectories because of the March 15 election. In addition, mask wearing and social distancing were much more widespread in the second round than they were during the first round.

Updated data allow us to actually assess the situation of French departments after the second round of municipal elections. To this end, we reconstruct the 32-day cumulated number of hospitalizations per 100,000 inhabitants by July 9—i.e., 11 days after the second round has taken place—in each department and compare it to the March 26 distribution and the abovementioned threshold.¹³ As shown by Fig. 8, only 4 departments were still, by the date of the second round, above the threshold of hospitalizations that we use to classify departments as unsafe. After more than 2 months of lockdown and severe anti-contagion policies, the epidemic situation in June was not comparable to that in March. While the first round took place at the beginning of the exponential part of epidemic curve, the lockdown essentially amounted to a reset of infections. Infection levels were thus much lower on June 28 than they were on March 15. As a consequence, it is likely that holding elections on June 28 did not cause a statistically detectable number of

¹³Thirty-two days is the exact length of the period—February 24 to March 26—used to construct the advanced COVID-19 epidemics indicator for the analysis of the first round.

hospitalizations. In Appendix B, we further illustrate this claim by replicating the analysis around the second round of municipal elections. Results show that the approach used for the first round does not allow to uncover a significant effect of June 28 turnout on subsequent hospitalizations.

6 Conclusion

Combining simple epidemiological modelling with a quadruple differences flavoured econometric methods, we estimated the impact of the first round of the 2020 French municipal elections, held on March 15, on the spread of COVID-19. While evaluating the impact of policies on an epidemic spread poses various challenges, the methodology we propose makes possible to causally link events to the subsequent evolution of the COVID-19 epidemic. As discussed above, only very peculiar omitted factors, could threaten the estimation we propose and the interpretation of estimates. For example, behavioural changes in voting or shielding that would differ across locations depending on early exposure to the epidemic could bias estimates in an a priori unknown direction. However, limited information on the epidemic by the time of the elections and the documented absence of correlation between turnout and the spread of the epidemic suggest that such specific effects are unlikely to be at play.

We show that March 15 elections resulted in at least 3,000 hospitalizations, which represents 11% of all cumulated hospitalizations for COVID-19 suspicion in metropolitan France by the end of March. This contrast with the absence of impact of the second round, held on June 28 in a context where the circulation of the virus was considerably lower after a severe lockdown was implemented and with much more knowledge of anti-contagion gestures. From a policy perspective, our results match findings by Palguta et al. (2022) and inform us about the health cost of holding elections without proper anti-contagion measures in times of active virus circulation

Appendix A: Supplementary figures and table

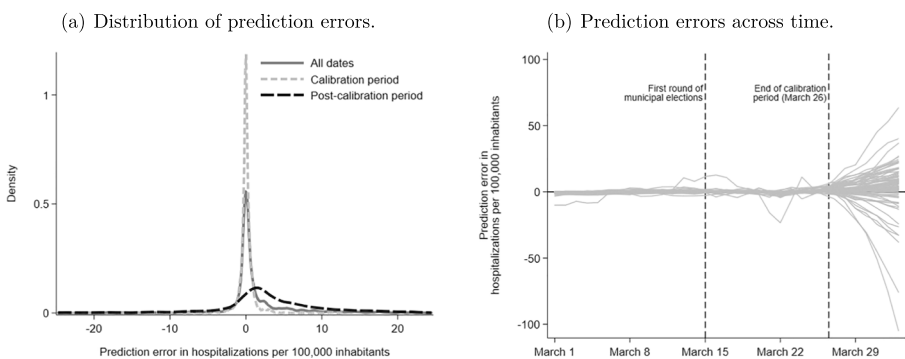


Fig. 9 Prediction errors. Sub-figures (a) and (b) plot the prediction errors of model Eq. 1 calibrated until March 26. Predictions are computed up to 7 after the end of the calibration period. See Section 3 for more details. Figure (a) excludes prediction errors out of the $[-25, 25]$ range

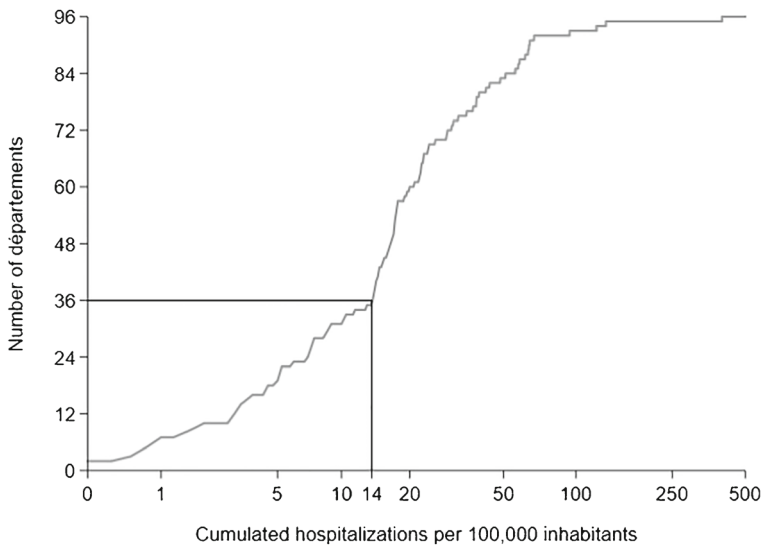


Fig. 10 Distribution of cumulated hospitalizations per 100,000 inhabitants on March 26. 96 departments of metropolitan France. The vertical line at 14 hospitalizations per 100,000 inhabitants correspond to the bottom third of the distribution when excluding the 5 departments for which model Eq. 1 cannot be calibrated because of insufficient variation in hospitalizations until March 26

Table 1 Estimates of the effect of turnout on excess hospitalizations

	Coefficient (standard error) [<i>p</i> -value]	
	β_t , effect of turnout for departments with low COVID-19 circulation by March 15	γ_t , effect of turnout for departments with high COVID-19 circulation by March 15
March 25	−4.579 (2.287) [0.054]	7.950 (5.343) [0.147]
March 26	−2.646 (4.085) [0.522]	11.344 (5.725) [0.056]
March 27	−7.620 (6.129) [0.223]	23.902 (5.929) [0.000]
March 28	−7.464 (6.146) [0.233]	39.455 (7.934) [0.000]

Table 1 (continued)

	Coefficient (standard error) [<i>p</i> -value]	
	β_t , effect of turnout for departments with low COVID-19 circulation by March 15	γ_t , effect of turnout for departments with high COVID-19 circulation by March 15
March 29	−9.277 (5.746) [0.116]	48.023 (12.222) [0.000]
March 30	−3.345 (5.775) [0.566]	53.616 (16.120) [0.002]
March 31	−5.869 (7.276) [0.426]	59.461 (19.971) [0.006]
April 1	−8.491 (8.653) [0.334]	66.684 (21.873) [0.005]
April 2	−11.164 (9.767) [0.261]	70.724 (23.325) [0.005]

Estimates of β_t and γ_t from Eq. 2 from March 25 onwards. See Section 3 for more details. See Fig. 2 for a graphical representation. *P*-values of two-sided tests in brackets. Standard errors clustered at the day and department levels between parentheses. The sample is made of 3,003 observations (91 departments $\times$ 33 days). *Départements with high (low) COVID-19 circulation by March 15* are departments in the top two thirds (bottom third) of the distribution of cumulated hospitalizations for COVID-19 suspicion by March 26

Appendix B: Results around the second round of municipal elections

We replicated the main analysis around the second round of municipal elections that took place on June 28, 2020. More precisely, we estimated model Eq. 1 for each department using 32 days—the exact length of the period used to calibrate the model for the main analysis—to July 9, i.e., 11 days after the date of the second round, and compute prediction errors up to 7 days after this date. Because of lower epidemic activity by that time, the model was successfully calibrated for 82 departments only (the 14 left-aside departments account for 6.0% of the French population).

We then use expression Eq. 2 to relate these prediction errors to differences in June 28 turnout and to the stage of the epidemics by that date. The estimated expression only differs from Eq. 2 by the exclusion from $\mathbf{Z}_{d,t}$ of a measure of population compliance with lockdown restrictions as no comparable measure is available for late June. Figure 11 displays the series estimated coefficients. Both series are close

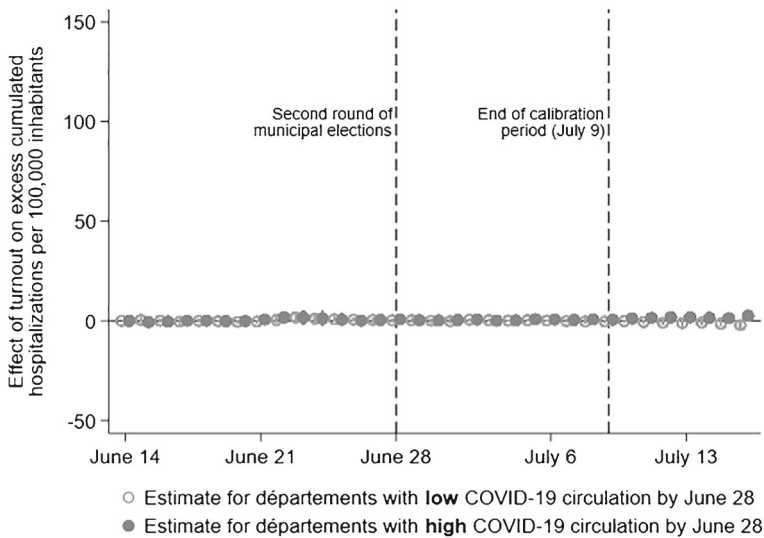


Fig. 11 Relationship between electoral turnout and excess hospitalizations around the second round of municipal elections. Estimates of β_t and γ_t from Eq. 2. See Section 3 for more details. Vertical lines are 95% confidence intervals. *Départements with high (low) COVID-19 circulation by June 28* are départements in the top two thirds (bottom third) of the distribution of 32-day cumulated hospitalizations for COVID-19 suspicion by July 9

to zero and precisely estimated. This suggests that the method used for the first round does not allow to uncover a significant effect of June 28 turnout on subsequent hospitalizations, or that the effect is precisely zero.

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Data and code availability All the data used in this article is publicly available. Replication codes are available from the authors’ websites.

Declarations

Conflict of interest The authors declare no competing interests.

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