SSM - Population Health 16 (2021) 100912

Contents lists available at ScienceDirect

SSM - Population Health

journal homepage: www.elsevier.com/locate/ssmph

Political regime and COVID 19 death rate: Efficient, biasing or simply different autocracies?An econometric analysis.

Guilhem Cassan [a], Milan Van Steenvoort [b,*]

[a] University of Namur, CEPR, DEFIPP, CRED and CEPREMAP, Rue de Bruxelles 61, 5000, Namur, Belgium
[b] Maastricht University Tongersestraat 53, 6211LM, Maastricht, Netherlands

A R T I C L E I N F O

Keywords:
COVID 19
Political regimes
Democracy
Autocracy
Public health

A B S T R A C T

The difference in COVID 19 death rates across political regimes has caught a lot of attention. The "*efficient autocracy*" view suggests that autocracies may be more efficient at putting in place policies that contain COVID 19 spread. On the other hand, the "*biasing autocracy*" view underlines that autocracies may be under reporting their COVID 19 data. We use fixed effect panel regression methods to discriminate between the two sides of the debate. Our results present a more nuanced picture: once pre-determined characteristics of countries are accounted for, COVID 19 death rates equalize across political regimes during the first months of the pandemic, but remain largely different a year into the pandemic. This emphasizes that early differences across political regimes were mainly due to omitted variable bias, whereas later differences are likely due to data manipulation by autocracies. A year into the pandemic, we estimate that this data manipulation may have hidden approximately 400,000 deaths worldwide.

1. Introduction

While democratic countries have previously been shown to overperfom compared to autocracies with respect to health outcomes (Besley & Kudamatsu, 2006; Bollyky et al., 2019; Franco et al., 2004; Kudamatsu, 2012; Pieters et al., 2016), data shows that, in the specific case of the COVID 19 pandemic, democratic countries may be fairing much worse (Sorci, Faivre, & Morand, 2020). Fig. 1 presents the evolution of the cumulative COVID 19 death rate across political regimes. It can be seen that the divergence between autocracies and democracies took place in two distinctive moments. A "first wave" of divergence happened in the first 25–50 days of the pandemic, during which democracies strongly diverged from autocracies. Then, the gap between the regimes only slowly increased over time until a "second wave" took place roughly 8 months into the pandemic, when democracies' divergence from autocracies accelerated drastically. Therefore, a year after the beginning of the pandemic,[1] democratic countries' COVID 19 death rate is on average larger than that of non democratic countries by approximately 42 per 100,000 inhabitants. That is, a year into the pandemic, the fatality rate in a democracy is on average 3.7 times larger than in an autocracy.

During the first wave of divergence, a debate (Ang, 2020) has emerged trying to unpack the reasons behind such wide differences across political regimes: a priori, *all other things equal*, the political regime should not be related to the spread of a disease. We distinguish three main hypotheses to explain this difference.

A first interpretation relates to the relative efficiency of social distancing measures in democracies and autocracies. Some have argued that democracies may be less well equipped to implement and enforce social distancing policies (Cepaluni et al., 2020; Narita & Sudo, 2021; Sorci et al., 2020), or that they may be implementing them with a suboptimal timing (Cheibub et al., 2020; Karabulut et al., 2021; Sebhatu et al., 2020). That is, in this view, autocracies are more able to implement social distancing measures. We will refer to this interpretation as the *efficient autocracy* hypothesis.

A second interpretation is that there may be *voluntary* misreporting of COVID 19 data, in particular by non democratic countries. For example, Tuite, Ng, et al. (2020) report that Egypt may have underreported its number of cases, Tuite, Sherbo, et al. (2020) report that Iran may also have underreported its number of cases, while Kavanagh (2020) discusses that China's political regime may have hindered its initial response to the pandemic. Adiguzel et al. (2020), Kapoor et al.

* Corresponding author.
E-mail addresses: guilhem.cassan@unamur.be (G. Cassan), m.vansteenvoort@maastrichtuniversity.nl (M. Van Steenvoort).

[1] Defined as when the number of cases reaches 0.4 per 100,000 inhabitants in a country.

https://doi.org/10.1016/j.ssmph.2021.100912
Received 22 June 2021; Received in revised form 31 August 2021; Accepted 31 August 2021
Available online 14 September 2021

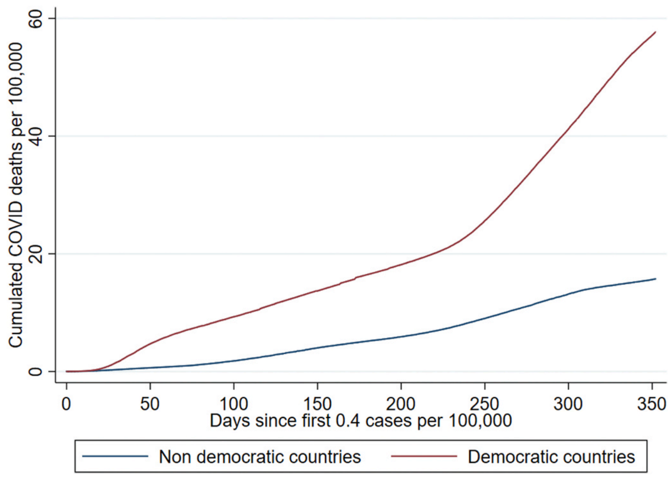


Fig. 1. Evolution of COVID 19 data reporting by political regime, time since first 0.4 cases per 100,000.

(2020) and Badman et al. (2021) document that autocracies' COVID 19 data present signs of manipulation, while Annaka (2021) show that correlates of data manipulation contribute to explaining the difference in death rate between democracies and autocracies. In this view, there are systematic differences between the *real* and the *reported* death rate. When these differences are *voluntary*, they are systematically linked to the type of political regime. We will refer to this interpretation as the *biasing autocracy* hypothesis.

A third interpretation has caught less attention (Ashraf, 2020): democracies and autocracies tend to have systematically different characteristics apart from their political regimes. These differences, once accounted for, may in fact be sufficient to explain the difference in both the *real* and *reported* death rate. This would leave the contributions due to *voluntary* under-reporting or differences in policies to matter only marginally. An example of such differences would be that autocracies tend to have much younger populations (and therefore, a much smaller *real* death rate, all other things equal) but also a lower ability to test (and therefore, a much smaller *reported* death rate, all other things equal). We refer to this interpretation as the *simply different autocracy* hypothesis.

The three aforementionned hypotheses are not mutually exclusive, and simple reduced form econometric methods can help measuring how much each of them contributes to explaining the differences observed across political regimes. Take the case where the econometrician only observes a *reported* death rate rather than the *real* death rate but can observe the variables determining COVID 19 *real* and *reported* death rate. Also assume that there are two such types of variables: fixed characteristics[2] (say, the share of the population aged 65 or older who would determine real death rates or the number of hospital beds per capita who would determine both real and reported death rates) and policy response. Under the *efficient autocracy* hypothesis, regressing the *reported* death rate on a measure of democracy and controlling for all fixed parameters would lead to a positive and significant coefficient on democracy. However, further controlling for policy response in the regression should bring the coefficient on democracy close to zero and render it non significant. That is, all the differences observed between democracies and autocracies in their *reported* death rate, once fixed characteristics are accounted for, would be due to the difference in policy response across these two types of regime. In this case, there may be a difference between the *real* and the *reported* death rate, but this difference is not systematically linked to the political regime. In fact, these results would indicate that the policy response of autocracies is better than that of democracies, from the perspective of COVID 19 death rate.

Under the *biasing autocracy* hypothesis, in a regression of *reported* death rates on a measure of democracy and all relevant controls (including policy response), the coefficient on democracy should be positive and significant. That is, despite controlling for all relevant characteristics and policy response, there still is a systematic difference between democratic and non democratic countries which is not accounted for. In that case, the only reason why a difference may remain would be due to systematic underreporting of casualties by non democratic regimes. This would be due to the fact that the difference between the *real* and the *reported* death rate is always larger for autocracies.[3] While the *real* death rate would be identical once all confounding factors are accounted for, the *reported* death rates remain different *even when controlling for the characteristics influencing non voluntary under reporting.*

We use daily level data of COVID 19 death rates of 137 countries for the first year of the epidemic and resort to simple reduced form econometric methods using the panel structure of the data. First we start by looking into the evolution of daily total death rates across political regimes, using a regression with no controls except country fixed effects (Regression 1). We then include controls for fixed characteristics of countries that are likely to determine the real COVID 19 death rate and allow them to matter differently across time (Regression 2). Finally, we also include controls for the stringency of social distancing measures and allow these to matter differently across time (Regression 3). Comparing Regression 2 to Regression 3 addresses the *efficient autocracy* hypothesis: any difference between the coefficient on democratic regime between Regressions 2 and 3 would be due to the differential in policy response across political regimes. An increase would indicate that autocracies implement more stringent social distancing measures that are successful in decreasing the death rate. Comparing Regression 1 to Regression 3 addresses the *biasing autocracy* and the *simply different autocracy* hypothesis: once all controls for both fixed characteristics and policy response are accounted for, does the difference between autocratic and democratic regimes remain (*biasing autocracy* hypothesis) or vanish (*simply different autocracy* hypothesis)?

Our results indicate that the inclusion of controls for country characteristics and policy response is in fact enough to remove almost all cross regime difference in COVID 19 mortality rates during the first wave of divergence. However, these controls are not sufficient to account for the second wave of divergence. Therefore, while the initial differences across political regimes were due to the fact that democracies and autocracies are *simply different*, we find evidence for the *biasing autocracy* hypothesis during the second wave of divergence. A year into the pandemic, we estimate that around 400,000 deaths - or 13% of total deaths - may have been hidden due to data manipulation. We find little support for the *efficient autocracy* hypothesis.

Section 2 of this paper presents the data. Section 3 elaborates on the methodology used to test our hypotheses. Section 4 and 5 present our main results. Finally, Section 6 provides a discussion of our main findings and Section 7 concludes.

2. Data

In order to investigate our hypotheses, we assemble a dataset that comprises information on daily cases and deaths in the first 352 days of the pandemic for 137 countries. Our dependent variable, the daily country-level total number of reported cases and reported deaths due to

[2] We call fixed characteristics the variable that are pre determined and can not be changed in the time horizon of interest in the paper. In the long run, all characteristics determining the death rate such as, say, the GDP per capita, can of course be considered at least partly as an outcome of the political regime (Acemoglu et al., 2019).

[3] Note that our methodology is neutral with respect to which political regime may be biasing and allows for democratic regimes to be underreporting more than non democratic regimes.

the COVID-19 virus is from Dong et al. (2020) .[4] Our main variable of interest, the classification of political regimes along the autocratic-democratic scale, comes from the Polity 5 project (Center for Systemic Peace, 2018).

Under the *simply different autocracy* hypothesis, accounting for the differences in characteristics of countries would suffice to explain the difference in reported mortality rates across political regimes. We therefore collected an extensive array of country level variables. To proxy for income and health infrastructure differences, we gathered data on gross domestic product per capita in 2018 from the World economic outlook survey (IMF), and completed it with the World Factbook (CIA). Furthermore, information on the number of available hospital beds (per thousand inhabitants) is retrieved from the World Bank to account for differences in health infrastructure that may drive the mortality difference (actual and reported death rates).[5]

To capture differences in demographic characteristics which may explain the speed of the spread of the disease, we use data on countries' total population and density in 2019 from the World Bank, and data on countries' urbanization rate in 2019 from the World in Data website. To control for the effect of geographical characteristics, we collect data on the latitude and longitude of each country's capital from the World Cities Database, and classify each country according to its World Bank region.[6] Finally, to control for population risk of mortality, we include the share of population aged 65 or older (from the World Bank) and, since air pollution has been shown to be associated with COVID 19 death rates (Zhu et al., 2020), we use summary exposure values to ambient ozone pollution and ambient particle matter pollution from the Global Burden Disease dataset (2017).

To test the *efficient autocracy* hypothesis, we use information on countries' different COVID 19 containment policies from the "Variation in Government Responses to COVID-19" dataset (Hale et al., 2020). This dataset includes a daily policy stringency index based on the aggregation of 17 policy indicators.[7]

Given that the data on our dependent variable is at the daily level, this allows us to construct a panel dataset that comprises a total of 137 countries,[8] classified as either democratic or non democratic, for which we have information on all the previously mentioned national characteristics. Therefore, our dataset displays information (by day and by country) on the total number of reported deaths due to the COVID-19 virus, on the stringency of policy measures taken by a given country, and on all other relevant characteristics of that country. We focus on the first year since the beginning of the pandemic in each country, which we define as having more than 0.4 cases per 100,000 inhabitants. [9]

3. Methodology

Given the country-day panel structure of our data, we resort to fixed effect panel reduced form econometric methods to look into the differences in COVID 19 casualty rates across political regimes and time. This method allows us to remove the influence of all time invariant differences across countries by including countries fixed effects. This further allows us to control for an extensive set of countries' pre-determined characteristics and for differences in containment policies across countries.

We specify the following regression equation, which we run using Ordinary Least Squares:

$$\begin{aligned} DeathRate_{ct} = {} & \sum\nolimits_{t=1}^{T} \beta_t * democratic_c * time.from.start_t \\ & + \sum\nolimits_{t=1}^{T} \alpha_t * time.from.start_t \\ & + \sum\nolimits_{t=1}^{T} \delta_t * X_c * time.from.start_t \\ & + \sum\nolimits_{t=1}^{T} \gamma_t * Y_{ct} * time.from.start_t + \delta_c + \omega_{ct} \\ & + \varepsilon_{ct} \end{aligned} \tag{1}$$

$DeathRate_{ct}$ is the inverse hyperbolic sine transformation of daily declared total deaths per 100,000 inhabitants in country c[10], t days after the beginning of the pandemic in country c. $democratic_c$ is the 2018 polity score of country c. $time.from.start_t$ is a set of fixed effect for each day since the beginning of the pandemic. The interaction of $democratic_c$ with $time.from.start_t$ allows us to track day by day the evolution of the difference in death rates across political regimes, a standard approach in economics (see Duflo (2001) or Cassan (2019) among others).

X_c is a large set of controls for countries' pre determined characteristics: GDP per capita, number of hospital beds per 1000, population, density, urbanization rate, share of population aged 65 or older, summary exposure value to particle matters pollution, summary exposure value to ambient ozone pollution, as well as for the square level of these variables, World Bank regions fixed effects, latitude and longitude. We interact all these variables with the $time.from.start_t$ fixed effects to allow their effect to vary over time.

Y_{ct} is a measure of country policy response to the pandemic. It is a stringency index of governmental response (as measured at t-15 to allow for lags in its effect: policy response affects the probability of infection on a given day, which affect mortality only two weeks after). We also include the square level of this variable to allow for non-linear effects. Furthermore, we interact these variables with the set of $time.from.start_t$ fixed effects, to allow their effect to vary over time. δ_c is a set of country fixed effects. Finally, ω_{ct} is a set of day of the week fixed effect interacted with $time.from.start_t$ fixed effects, to control for variations in reporting across days of the week.

We perform this regression iteratively. First, we do not implement any of the X_c and Y_{ct} controls (Regression 1). This allows us to see the evolution of the difference in casualty rates across political regimes when no confounding factors are accounted for. Then, we implement X_c but not Y_{ct} (Regression 2). This will allow us to see how much of the difference across political regimes survives once the different pre-determined characteristics of countries are accounted for. Finally, we add the Y_{ct} policy response controls (Regression 3).

This iterative procedure allows us to address the different sides of the debate on the role of political regime in fighting COVID 19. Comparing Regression 2 to Regression 3 addresses the *efficient autocracy* hypothesis: any difference between the β_t coefficients on democratic regime between Regressions 2 and 3 would be due to the differential in policy response across political regimes. An increase would indicate that autocracies implement more stringent social distancing measures that are successful in decreasing the COVID 19 death rate.

Comparing Regression 1 to Regression 3 addresses the *biasing autocracy* and *simply different autocracy* hypotheses: once all controls for both fixed characteristics and policy response are accounted for, do the β_t coefficients remain positive (*biasing autocracy*) or do they equalize to

[4] Last accessed: 17.07.2021.

[5] The data is from 2010, and when missing, completed with data from 2000.

[6] These are: East Asia and Pacific, Europe and Central Asia, Latin America and Caribbean, Middle East and North Africa, North America, South Asia and Sub-Saharan Africa.

[7] Policy indicators include policies with respect to closures or movement restrictions as well as economic and health system policies. Last accessed: 17.07.2021.

[8] See Appendix A.2 for the list of the countries present in our dataset and their classification as democratic or non democratic.

[9] Because our outcome of interest is death per capita, it makes sense to use cases per capita rather than the absolute number of cases to determine the beginning of the pandemic. In Appendix A.4, we show that results are robust to using alternatives thresholds. In Appendix A.3, we show that the timing of the beginning of the pandemic does not seem to differ significantly across political regimes.

[10] Given the large number of zero in the variable, this transformation is preferable to that of taking the logarithm and has a similar interpretation.

zero (*simply different autocracy*)? Note that these hypotheses are not mutually exclusive: autocracies may well be efficient, biasing and different at the same time. Our methodology allows to capture this possibility: if the β_t coefficients decrease but remain large and significant when passing from Regression 1 to Regression 3 and change but remain large and significant between Regression 2 and Regression 3, then this would support the simultaneous presence of the three hypotheses.

4. Results

Fig. 2 presents the β_t coefficients from Equation (1) for all three versions of the specification.[11] The first panel presents the results of Regression 1, when no controls excepting country and day of the week fixed effects are included. The divergence between political regimes takes place in two waves. During the first two months of the pandemic, a first wave of divergence drives death rate between political regimes apart. From around day 60 to day 230, this wave slowly recedes, whereas a second wave of divergence emerges from day 230 onward.

The second panel includes controls for pre-determined characteristics interacted with day fixed effects. The β_t coefficients become close to zero and statistically insignificant for the first wave, but start to increase slowly from day 30 onwards, to become significant roughly 200 days into the pandemic. That is, once countries' differences in characteristics are taken into account, the difference in death rates across political regimes during the first wave does not survive. However, over the course of time, these characteristics alone are not sufficient to fully account for the difference.

The third panel adds controls for countries' policy response to the pandemic. Our coefficients of interest β_t remain virtually unaffected. That is, our results do not support the *efficient autocracy* hypothesis.[12] Therefore, once systematic differences across countries' characteristics and policy responses are taken into consideration, the differences in death rates apparent in Fig. 1 and in the first panel of Fig. 2 vanish for the first wave of divergence, but are still present for the second wave. The reason why reported COVID 19 death rates differ across political regimes is fully accounted for by factors which systematically differ between democracies and autocracies in the first months of the pandemic, but not over the long run. That is, our results indicate that the *simply different autocracy* hypothesis is prevailing during the first wave of divergence, but are also consistent with the *biasing autocracy* hypothesis in the longer run. Our results do not support the hypothesis that autocracies are more efficient at controlling the pandemic but show that they may be voluntarily under reporting casualty more often, even if the latter was probably not dominant in the first months of the pandemic.[13]

5. How many deaths are hidden?

The β_t coefficients of Regression 3, presented in the third panel of Fig. 2, are strongly suggestive that even once a very large set of characteristics and policies are taken into account, a year into the pandemic, there are differences in COVID 19 mortality rates across political regimes that are not accounted for. Under the assumption that our extensive set of controls captures the determinants of COVID 19 mortality rates, this suggests that autocracies may be manipulating their reported COVID 19 death rate.

In this section, we use the results of our estimations to answer the following question: how many COVID 19 deaths are hidden because of manipulations by autocracies? The β_t coefficients can be interpreted as the percentage change in deaths per capita due to an increase of one unit of the Polity score of a country. Therefore, we can compute the unbiased COVID 19 death rate as:

$$UnbiasedDeathRate_{ct} = DeathRate_{ct}(1 + beta_t * (10 - polity_c)) \tag{2}$$

Where $DeathRate_{ct}$ is the declared COVID 19 death rate of country c, t days after the beginning of the pandemic, $beta_t$ the coefficient on democratic of Regression 3 and $polity_c$ country c's polity score. This unbiased death rate tells us what the reported death rate of country c would have been if that country has had a polity score of 10,[14] all other things equal. Based on this unbiased death rate, we compute each country's unbiased number of deaths. Fig. 3 presents the results of this exercise. It can be seen that a year into the pandemic, around 400,000 deaths have been hidden due to data manipulation by autocracies, which represents roughly 13% of total deaths in the world.

6. Discussion

A few remarks are in order to help interpret our results. First, one should keep in mind that the variables that we consider pre-determined characteristics, such as the GDP per capita, are only pre-determined in the time horizon that we are considering. Over the long run, they are an outcome of the political regime. See for example Acemoglu et al. (2019), who show that democracy causes growth. In that sense, our results do not take into consideration the long term effect of political regimes on the variables that may determine COVID 19 death rates.

For instance, a better health care system will lead to both a lower *real* death rate (infected individuals are better treated) and a higher *reported* death rate (infected individuals' death is better attributed to COVID 19). If, as has been argued in the literature (Besley & Kudamatsu, 2006; Bollyky et al., 2019; Franco et al., 2004; Kudamatsu, 2012; Pieters et al., 2016), democracies tend to have better health care policies; in the long run, the health care system (which we consider as pre-determined) will be better in democracies because of the political regime, which will causally affect both *real* and *reported* death rates across political regimes.

Second, it is important to remember that our methodology has some limitations. It can not account for all forms of misreporting of the data. Since our method is in essence comparative, we can only estimate differences in misreporting. So our results can only be interpreted as a lower bound of overall misreporting.[15]

Another limitation of our method is its residual approach: once confounding factors are taken into account via the extensive set of control variables that we include in our regressions, the remaining variation should not be correlated with the political regime in the absence of data manipulation. This is true if indeed we control for all confounding factors that are correlated both with political regime and COVID 19 death rate. It is however impossible to be certain that all such variables are controlled for. Therefore, our results can be interpreted as suggesting that during the second wave, autocracies manipulated COVID 19 data, but not as a definitive proof of such manipulation.

Third, our focus is only on COVID 19 death rates. Arguably, however, one may have wanted to study death rates from all causes rather than just from COVID 19. Even in times of pandemic, governments should

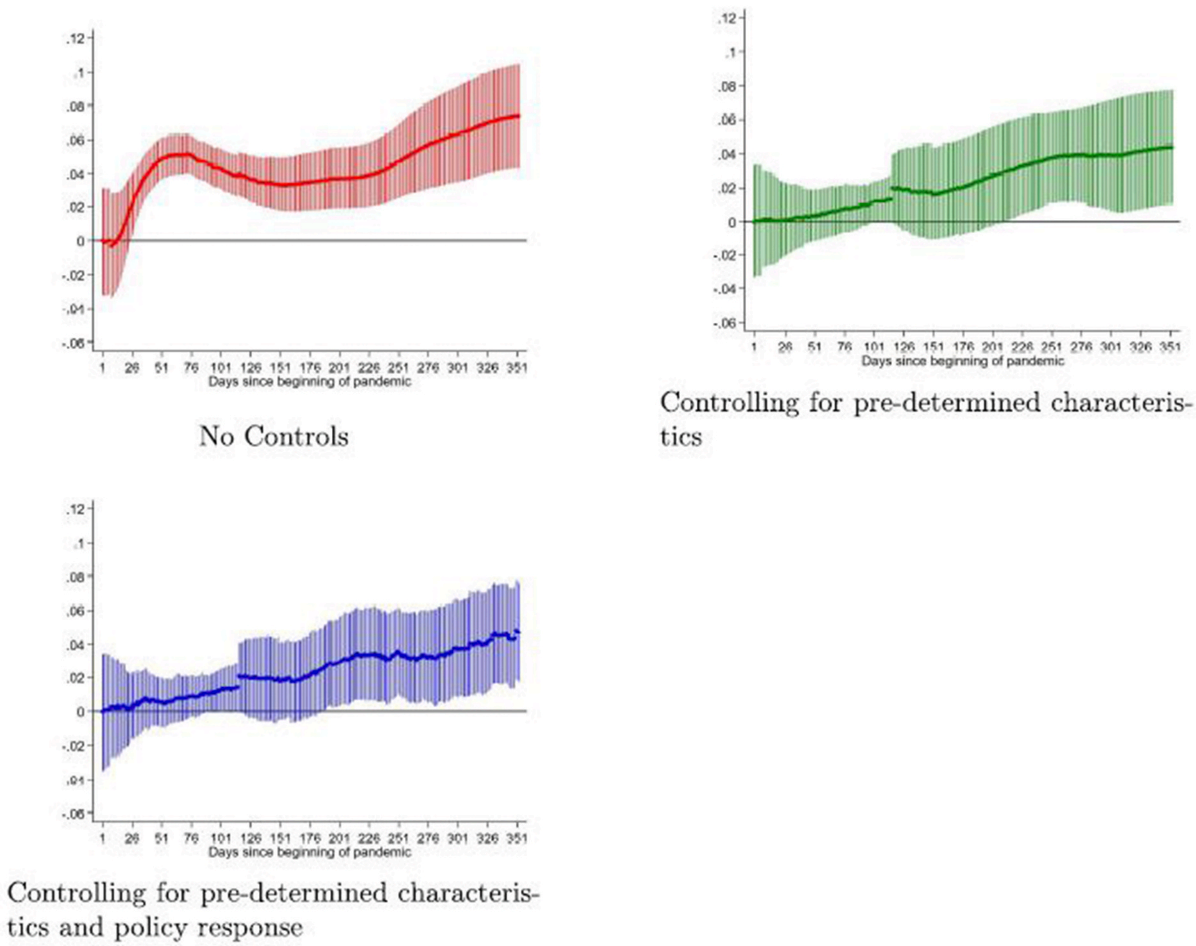
[11] In Appendix A.4, we show that results are robust to using alternative definitions of the start of the pandemic in a country. In Appendix A.6, we show that our results are robust to alternative definitions of democracy.

[12] To test if these results are driven by an outlier country, we run Regression 1, 2 and 3137 times, removing one country at a time. We plot the 353 coefficients of interests of each of these 411 regressions in Fig. 6 of Appendix A.5. It can be seen that the results are robust to the omission of any single country.

[13] In Appendix A.7, we explore which countries' characteristics affect the evolution of coefficients the most.

[14] Countries with a polity score of 10 include, among others: Canada, France, Germany, Sweden, Norway. See Appendix A.2 for the list of each country's polity score.

[15] Think for exemple of the case in which democracies are biasing their data. Our methodology can only show that autocracies are biasing their data differently than democracies, but can not measure the baseline level of bias. Our methodology also does not allow us to detect biases which are uncorrelated with political regimes.



Standard errors are two way clustered at the country and day level.

Fig. 2. Evolution of COVID 19 deaths per 100,000 since 0.4 cases per 100,000.95% CI.

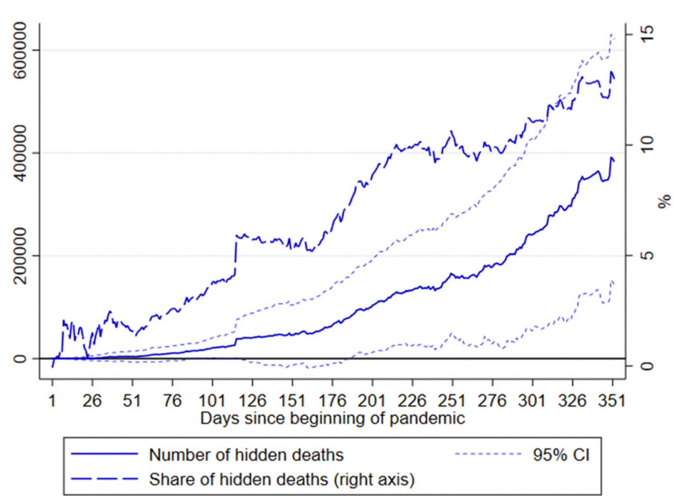


Fig. 3. Number and share of hidden deaths linked to lack of democracy.

aim at preserving the health of their citizens from all sources of harm, not from one specific cause only. In a time during which most of the attention is drawn towards COVID 19 death rates rather than towards death rates in general, a pro-democracy argument would be that while there does not seem to have been differences across political regimes for COVID 19 death rates during the first wave of divergence, this may hide the fact that autocracies have focused on decreasing COVID 19 death rate at the expense of deaths from other sources.

One could develop this idea even further and argue that democracies have higher COVID 19 mortality rates because they are better at preventing non COVID 19 deaths, leading to a population which is on average older and therefore more likely to die if infected by COVID 19. This question can unfortunately not be tackled with the available data, and we leave it to future research (when mortality data from all causes will be available for a sufficient number of countries), but note that the differences in countries' population's susceptibility to die from COVID 19 upon contamination seem to be one of the main drivers of the difference in COVID 19 mortality rates across political regimes during the first wave of divergence, all other things equal.

Fourth, our findings do not contradict previous studies on under reporting of COVID 19 data during the first wave of divergence, in particular country specific studies. Indeed, because of the statistical analysis used, our results do not imply that no single country under-reported or manipulated its COVID 19 mortality data. However, our results do address the widespread idea that autocracies were systematically and willingly under reporting COVID 19 casualties during the first wave of divergence. What our results do indicate is that under reporting during the first wave of divergence (by any political regime) was primarily due to the different characteristics of countries that are correlated with the political regime rather than a direct causal effect of the political regime. This however, is not true for the second wave of divergence, for which we find support for the view that autocracies manipulated COVID 19 death rates.

That is, a plausible interpretation is that autocratic governments may well have been under reporting data during the first wave while not manipulating it. One could argue that even if autocracies were under reporting COVID 19 death rates, this may have been primarily driven by their overall incapacity to link death to its cause rather than to a direct attempt at data manipulation. The low reported COVID 19 death rate in autocracies may in part be due to the lower level of development of both the public health infrastructure and the statistical apparatus of autocracies. However, and this goes back to our first point, over the long run, public health infrastructure and statistical apparatus may well be determined by the political regime.

7. Conclusion

We investigated the COVID 19 death rate gap between democratic and autocratic countries. We uncovered that it widened in two phases that we call waves of divergence. We formulated three main hypotheses based on the previous literature to explain them: they can be due to the fact that autocracies are more efficient at implementing restricting policy measures; that autocracies are underreporting their COVID 19 data and that autocracies simply have different characteristics that can explain the death rate gap. Our analysis, relying on simple econometric tools, allows to make progress in the debate around the sources of the observed differences in COVID 19 death rates across political regimes.

We show that once pre-determined characteristics and policy responses are taken into account, COVID 19 death rates do not exhibit any difference across political regimes during the first wage of divergence: the coefficients on democracy become precisely estimated zeros. For this initial period, our results therefore do not show support neither for the *efficient autocracy* nor for the *biasing autocracy* hypotheses, as we do not find evidence that autocracies are neither systematically better at preventing COVID 19 death nor that they are more often under reporting casualties.

However, several months into the pandemic, a second wave of divergence between democracies and autocracies emerged. Our results indicate that this second wave can not be fully explained by differences of characteristics across political regimes. We therefore find support for both hypotheses of *biasing autocracy* and *simply different autocracy*. According to our estimates, had all the countries in our sample been fully democratic, the number of reported deaths would have increased by approximately 400,000 or 13% of the deaths at the time.

Author statement

Guilhem Cassan; Planning: original idea and implementation, Conduct: data analysis. Reporting: Writing.

Milan Van Steenvoort; Conduct: data collection and analysis, Reporting: Writing.

Funding

We are grateful to Jeremie Decalf, Romain Lutaud, Glenn Magerman, Marc Sangnier and Vincenzo Verardi for helpful discussions and suggestions. We thank seminar participants at UNamur. Guilhem Cassan thanks CEPREMAP and the FNRS for financial support. Research on this project was financially supported by the Excellence of Science (EOS) Research project of FNRS O020918F. All errors remain our own.

Declaration of competing interest

None.

Appendix

A.1 Data sources and descriptive statistics

Table 1 presents the data sources used to compute the variables exploited in our analysis.

Table1
Data sources

Data	Source
COVID19 Death Rate	Dong et al. (2020)
COVID19 Cases	Dong et al. (2020)
Democratic	Polity 5 project (Center for Systemic Peace, 2018)
	Freedom in the world (Freedom House, 2021)
Stringency Index of Policy Response	Variation in Government Responses to COVID-19 (Hale et al., 2020)
Gross Domestic Product per capita	World Economic Outlook, IMF (2018) + World Factbook, CIA (2018)
Share of 65+	World Bank (2019)
Population Density	World Bank (2019)
Population	World Bank (2019)
Urbanization Rate	World in Data
Hospital Beds per 1000	World Bank
Summary Exposure Value to Air Pollution	Global Burden of Disease (2017)
Summary Exposure Value to Ambient Ozone Pollution	Global Burden of Disease (2017)
Latitude and Longitude	World Cities Database
World Regions	World Bank

Table 2 presents the descriptive statistics for our variables. Our final dataset comprises a total of 137 countries for which we have all the variables and for which we observe 352 days of data since the beginning of the pandemic.

Table2
Descriptive statistics

	Mean	sd	Min	Max
Polity score	4.51	6.13	−10.00	10.00
Democratic (polity>0)	0.76	0.43	0.00	1.00
Democratic (polity>6)	0.55	0.50	0.00	1.00
Free or Partly Free according to Freedom House	0.74	0.44	0.00	1.00
Total deaths per 100,00 - Inverse Hyperbolic Sine Transformation	1.72	1.48	0.00	5.96
Share of 65+	9.56	6.64	1.16	28.00
GDP Per Capita	23,632.97	23,539.24	727.17	132,886.39
Population in million	52.75	169.22	0.55	1397.71
Hospital beds per 1000	2.96	2.48	0.10	13.40
Population Density	206.56	777.60	2.06	8829.05
Urbanization Rate	61.96	21.96	13.37	100.00
Summary exposure value to ambient ozone pollution - Age standardized	36.25	12.55	2.22	52.87
Summary exposure value to ambient particulate matter pollution - Age standardized	32.35	17.27	6.80	90.02
Stringency t-15	60.66	21.07	0.00	100.00
Observations	48,351			

A.2 Polity score and freedom classification, by country

Table 3 presents the polity 5 score and freedom classification of all countries that are included in our sample.

Table 3
Polity score and Freedom classification, by country

Country	Polity Score	Freedom Classification
Afghanistan	−1	Not Free
Albania	9	Partially Free
Algeria	2	Not Free
Argentina	9	Free
Australia	10	Free
Austria	10	Free
Azerbaijan	−7	Not Free
Bahrain	−10	Not Free
Bangladesh	−6	Partially Free
Belarus	−7	Not Free
Belgium	8	Free
Benin	7	Partially Free
Bhutan	7	Partially Free
Bolivia	7	Partially Free
Botswana	8	Free
Brazil	8	Free
Bulgaria	9	Free
Burkina Freeaso	6	Partially Free
Burundi	−1	Not Free
Cabo Verde	10	Free
Cambodia	−4	Not Free
Cameroon	−4	Not Free
Canada	10	Free
Central African Republic	6	Not Free
Chile	10	Free
China	−7	Not Free
Colombia	7	Partially Free
Costa Rica	10	Free
Croatia	9	Free
Cuba	−5	Not Free
Cyprus	10	Free
Czech Republic	9	Free
Denmark	10	Free
Djibouti	3	Not Free
Dominican Republic	7	Partially Free
Ecuador	5	Partially Free
Egypt, Arab Rep.	−4	Not Free
Country	Polity Score	Freedom Classification
El Salvador	8	Partially Free
Estonia	9	Free
Eswatini	−9	Not Free
Ethiopia	1	Not Free

(*continued on next page*)

Table 3 (*continued*)

Country	Polity Score	Freedom Classification
Fiji	2	Partially Free
Finland	10	Free
France	10	Free
Gabon	3	Not Free
Gambia, The	4	Partially Free
Georgia	7	Partially Free
Germany	10	Free
Ghana	8	Free
Greece	10	Free
Guatemala	8	Partially Free
Guinea	4	Partially Free
Guyana	7	Free
Haiti	5	Partially Free
Honduras	7	Partially Free
Hungary	10	Partially Free
India	9	Free
Indonesia	9	Partially Free
Iran	−7	Not Free
Iraq	6	Not Free
Ireland	10	Free
Israel	6	Free
Italy	10	Free
Jamaica	9	Free
Japan	10	Free
Jordan	−3	Partially Free
Kazakhstan	−6	Not Free
Kenya	9	Partially Free
Korea, Rep.	8	Free
Kuwait	−7	Partially Free
Kyrgyz Republic	8	Partially Free
Latvia	8	Free
Lebanon	6	Partially Free
Liberia	7	Partially Free
Libya	−7	Not Free
Country	Polity Score	Freedom Classification
Lithuania	10	Free
Luxembourg	10	Free
Madagascar	6	Partially Free
Malawi	6	Partially Free
Malaysia	7	Partially Free
Mali	5	Partially Free
Mauritius	10	Free
Mexico	8	Partially Free
Moldova	9	Partially Free
Mongolia	10	Free
Morocco	−4	Partially Free
Mozambique	5	Partially Free
Myanmar	8	Not Free
Nepal	7	Partially Free
Netherlands	10	Free
New Zealand	10	Free
Nicaragua	6	Not Free
Niger	5	Partially Free
Nigeria	7	Partially Free
Norway	10	Free
Oman	−8	Not Free
Pakistan	7	Partially Free
Panama	9	Free
Paraguay	9	Partially Free
Peru	9	Free
Philippines	8	Partially Free
Poland	10	Free
Portugal	10	Free
Qatar	−10	Not Free
Romania	9	Free
Russian Federation	4	Not Free
Saudi Arabia	−10	Not Free
Singapore	−2	Partially Free

(*continued on next page*)

Table 3 (*continued*)

Country	Polity Score	Freedom Classification
Slovakia	10	Free
Slovenia	10	Free
Spain	10	Free
Sri Lanka	6	Partially Free
Sudan	−4	Not Free
Suriname	5	Free
Sweden	10	Free
Switzerland	10	Free
Syrian Arab Republic	−9	Not Free
Tajikistan	−3	Not Free
Tanzania	3	Partially Free
Thailand	−3	Partially Free
Timor-Leste	8	Free
Togo	−2	Partially Free
Trinidad and Tobago	10	Free
Tunisia	7	Free
Turkey	−4	Not Free
Uganda	−1	Not Free
Ukraine	4	Partially Free
United Arab Emirates	−8	Not Free
United Kingdom	8	Free
United States	5	Free
Uruguay	10	Free
Uzbekistan	−9	Not Free
Venezuela	−3	Not Free
Vietnam	−7	Not Free
Yemen, Rep.	3	Not Free
Zambia	6	Partially Free
Zimbabwe	4	Partially Free

A.3 Political regime and start of the pandemic

Our analysis focuses on the evolution of the death rates across time since the beginning of the pandemic in each country. We verify whether the political regime determines when these first contaminations are reached, which may imply that the timing that we rely on is biased. In order to do so, we run the following OLS regressions:

$$\text{time to start}_c = \alpha + \beta_1 * \text{democratic}_c + X_c + \varepsilon_c \tag{3}$$

time.to.start_c is the number of days between the start of the pandemic in country c and the 20th of January. We specify three variations of Equation (3): if the country has declared 4 or 6 per 100,000 cases or has reached 100 cases. Table 4 presents the results. We find no statistically significant difference across political regimes for the start of the pandemic.

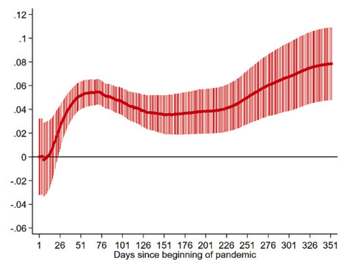
Table 4
Time to first cases and political regime

	0.4 c. per 100,000	0.6 c. per 100,000	100 c.
Democracy	−0.27	−0.20	0.95
	(0.54)	(0.59)	(0.79)
R-sq	0.58	0.58	0.42
Observations	137	137	137

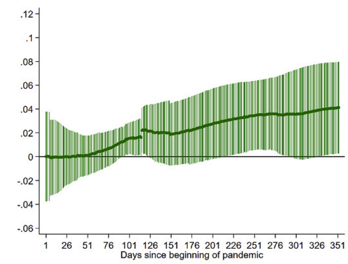
Heteroskedasticity-robust standard errors in parentheses * p < .10 **p < .05 ***p < .01. Controls included are: GDP per capita, population, density, urbanization rate, share of 65 and above, number of hospital beds per capita and the square of all preceding variables, latitude, longitude, World Bank region fixed effect.

A.4 Alternative definitions of the start of the pandemic

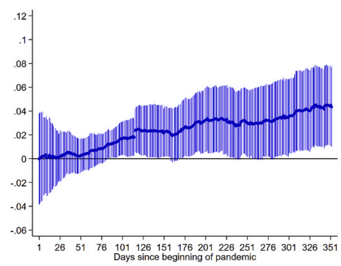
Figs. 4 and 5 reproduce Fig. 2, using 0.6 per 100,000 and 100 reported cases as the definition of the start of the pandemic in a country, respectively. Results remain unaffected by this change in definition of the beginning of the pandemic. Note that the number of countries reaching both 100 total cases and for which we observe 352 days since the first 100 case is 134: three countries do not reach 352 days after the first 100 cases in our data compared to our main sample. Moreover, the number of countries reaching both 0.6 per 100,000 reported cases and for which we observe 352 days since the first 100 case is 136: one country does not reach 352 days after the first 0.6 per 100,000 cases in our data compared to our main sample.



No Controls



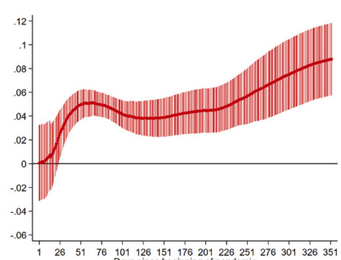
Controlling for pre-determined characteristics



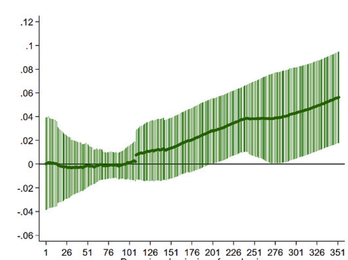
Controlling for pre-determined characteristics and policy response

Standard errors are two way clustered at the country and day level.

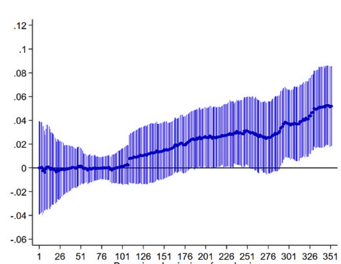
Fig. 4. Evolution of COVID 19 deaths per 100,000 since 0.6 cases per 100,000.95% CI.



No Controls



Controlling for pre-determined characteristics



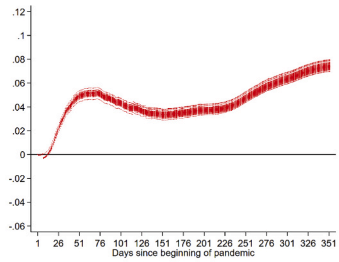
Controlling for pre-determined characteristics and policy response

Standard errors are two way clustered at the country and day level.

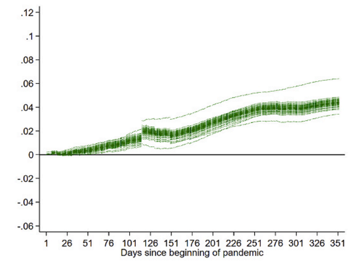
Fig. 5. Evolution of COVID 19 deaths per 100,000 since 100 cases. 95% CI.

A.5 Robustness check: removing one country at a time

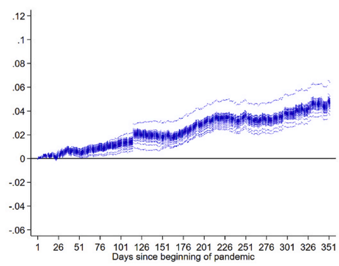
In order to test if an outlier country is driving our findings, we run each regression 137 times, removing one country at a time. We plot the 352 coefficients of interests of each of these 411 regressions in Fig. 6, which replicates Fig. 2. Overall, it can be seen that the results are robust to the omission of any single country. Note that one country (Eswatini) seems to significantly drive the average effect downwards, as can be seen from the higher coefficient on our variable polity when this country is left out. We take a conservative approach by including this country, as we see no apparent reason to leave it out, which reduces the effect of our variable polity during the second wave of divergence. Note that we do not observe any outlier during the first wave of divergence.



No Controls



Controlling for pre-determined characteristics



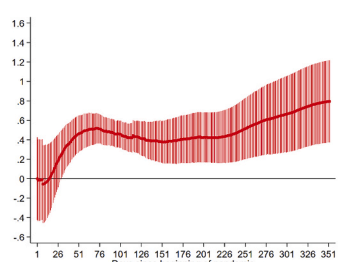
Controlling for pre-determined characteristics and policy response

CI not reported for readability

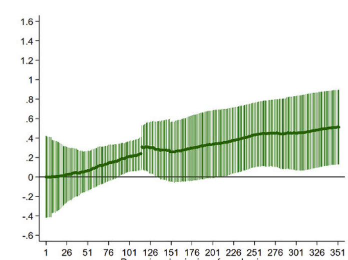
Fig. 6. Removing countries one at a time: Evolution of COVID 19 deaths per 100,000 since 0.4 cases per 100,000

A.6 Alternative definitions of democracy

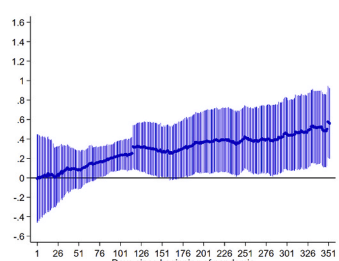
Figs. 7, 8 and 9 reproduce Fig. 2, using three alternative definitions of democracy: having a polity score larger than 0, having a polity score larger than 6, and being categorized by Freedom House as either "Free" or "Partly Free" (Freedom House, 2021). Results remain qualitatively similar.



No Controls



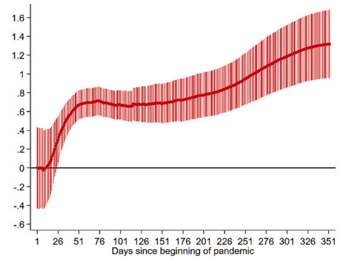
Controlling for pre-determined characteristics



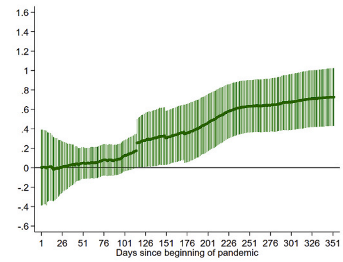
Controlling for pre-determined characteristics and policy response

Standard errors are two way clustered at the country and day level.

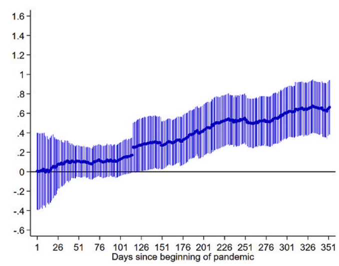
Fig. 7. Evolution of COVID 19 deaths per 100,000 since 0.4 cases per 100,000.95% CI. Polity>0



No Controls



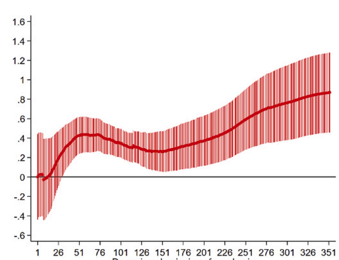
Controlling for pre-determined characteristics



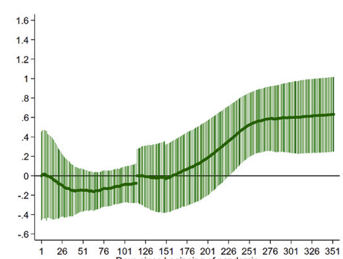
Controlling for pre-determined characteristics and policy response

Standard errors are two way clustered at the country and day level.

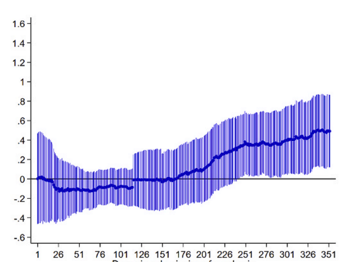
Fig. 8. Evolution of COVID 19 deaths per 100,000 since 0.4 cases per 100,000.95% CI. Polity>6



No Controls



Controlling for pre-determined characteristics



Controlling for pre-determined characteristics and policy response

Standard errors are two way clustered at the country and day level.

Fig. 9. Evolution of COVID 19 deaths per 100,000 since 0.4 cases per 100,000.95% CI. Free or Partly Free

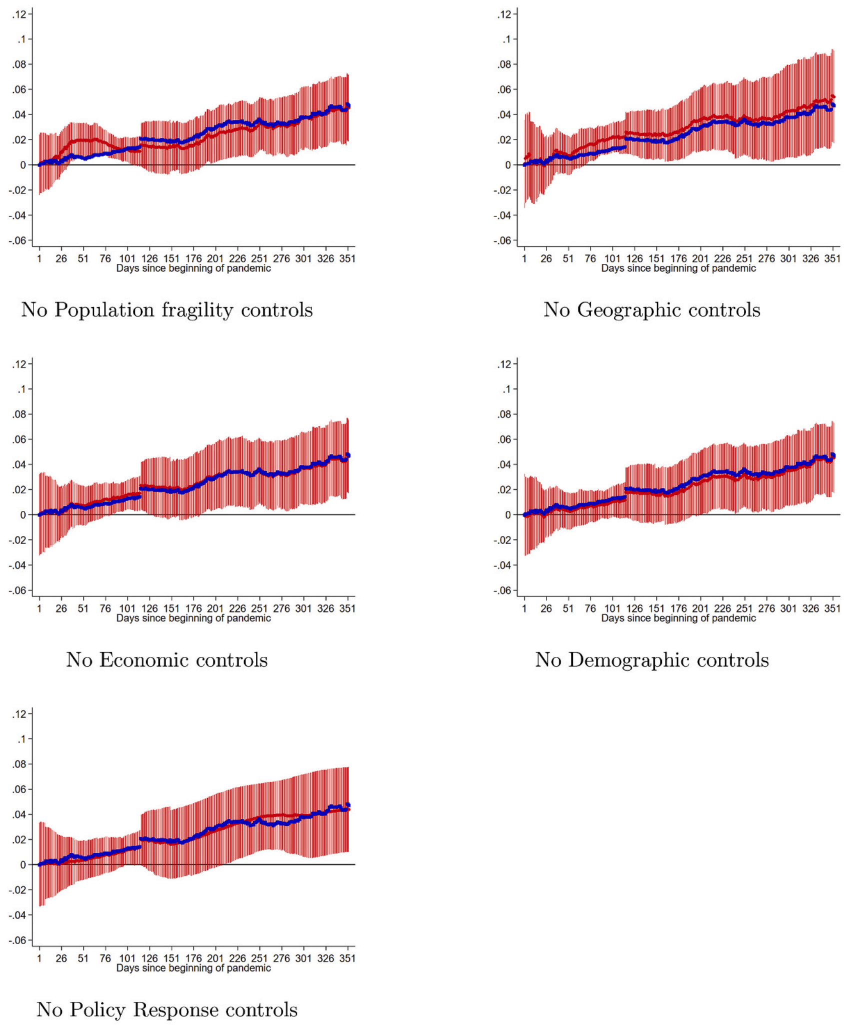
A.7 Which characteristics matter?

Having seen that the inclusion of controls is sufficient to remove the "political regime" effect on COVID 19 death rate during the first wave of divergence, we now move to a related question: which characteristics are contributing to closing the COVID 19 death rate gap between autocratic and democratic countries? In order to do so, we group our control variables in five categories:

- Geographical controls (latitude, longitude, World Bank region fixed effects)

- Wealth controls (GDP per capita, hospital beds per capita). These will proxy for the quality of the health system in the country, and will likely influence both the *real* and the *reported* death rate.
- Demographic controls (population, density, urbanization rate): these are likely to influence the speed of the spread of the pandemic.
- Population fragility controls (share of population aged 65+ and exposure to pollution): these are likely to influence the lethality of COVID 19 for a given spread of the disease.
- Policy response.

We run Regression 1, removing each of these groups of controls one at a time. Fig. 10 presents the β_t coefficients for each of these regressions. It also includes the coefficients of the original results of Regression 1 for comparison (we do not report the confidence intervals on these coefficients for readability).



Standard errors are two way clustered at the country and day level.

Fig. 10. Coefficient on “Democracy” and removal of controls

Analysing the period of the first wave of divergence, we observe that the set of controls related to population fragility (which notably contains the share of population aged 65 or older) stands out as being the set of controls that matters most. That is, the main reason why mortality rates from COVID 19 seem to differ across political regimes during the first wave of divergence may be that autocratic regimes tend to have a population that is less susceptible to die from COVID 19.

References

Acemoglu, D., Naidu, S., Restrepo, P., & Robinson, J. A. (2019). Democracy does cause growth. *Journal of Political Economy, 127*(1), 47–100.

Adiguzel, F. S., Cansunar, A., Corekcioglu, G., et al. (2020). Truth or dare? Detecting systematic manipulation of covid-19 statistics. *Journal of Political Institutions and Political Economy, 1*, 543–557.

Ang, Y. Y. (2020). When covid-19 meets centralized, personalized power. *Nature Human Behavior, 4*, 445–447.

Annaka, S. (2021). Political regime, data transparency, and covid-19 death cases.SSM. *Population Health, 15*, Article 100832.

Ashraf, B. N. (2020). *Devastation caused by covid-19: Is democracy to blame?*. Working Paper).

Badman, R. P., Wu, Y., Inukai, K., & Akaishi, R. (2021). *Blessing or curse of democracy?: Current evidence from the covid-19 pandemic*. arXiv preprint arXiv:2105.10865.

Besley, T., & Kudamatsu, M. (2006). Health and democracy. *The American Economic Review, 96*(2), 313–318.

Bollyky, T. J., Templin, T., Cohen, M., Schoder, D., Dieleman, J. L., & Wigley, S. (2019). The relationships between democratic experience, adult health, and cause-specific mortality in 170 countries between 1980 and 2016: An observational analysis. *The Lancet, 393*(10181), 1628–1640.

Cassan, G. (2019). Affirmative action, education and gender: Evidence from India. *Journal of Development Economics, 136*(C), 51–70.

Center for Systemic Peace. (2018). *Polity 5 project*.

Cepaluni, G., Dorsch, M. T., & Branyiczki, R. (2020). *Political regimes and deaths in the early stages of the covid-19 pandemic* (Working Paper).

Cheibub, J. A., Hong, J. Y. J., & Przeworski, A. (2020). *Rights and deaths: Government reactions to the pandemic* (Working Paper).

Dong, E., Du, H., & Gardner, L. (2020). An interactive web-based dashboard to track covid-19 in real time. *The Lancet Infectious Diseases, 20*(5), 533–534.

Duflo, E. (2001). Schooling and labor market consequences of school construction in Indonesia: Evidence from an unusual policy experiment. *The American Economic Review, 4*(91), 795–813.

Franco, Á., Álvarez-Dardet, C., & Ruiz, M. T. (2004). Effect of democracy on health: Ecological study. *BMJ, 329*(7480), 1421–1423.

Freedom House. (2021). *Freedom in the world*.

Hale, T., Webster, S., Petherick, A., Phillips, T., & Kira, B. (2020). *Oxford covid-19 government response tracker*.

Kapoor, M., Malani, A., Ravi, S., & Agrawal, A. (2020). *Authoritarian governments appear to manipulate covid data*.

Karabulut, G., Zimmermann, K. F., Bilgin, M. H., & Doker, A. C. (2021). Democracy and covid-19 outcomes. *Economics Letters, 203*, Article 109840.

Kavanagh, M. M. (2020). Authoritarianism, outbreaks, and information politics. *The Lancet Public Health, 5*(3), e135–e136.

Kudamatsu, M. (2012). Has Democratization reduced infant mortality in Sub- Saharan Africa? Evidence from micro data. *Journal of the European Economic Association, 10* (6), 1294–1317.

Narita, Y., & Sudo, A. (2021). *Curse of democracy: Evidence from 2020*. Available at: SSRN 3827327.

Pieters, H., Curzi, D., Olper, A., & Swinnen, J. (2016). Effect of democratic reforms on child mortality: A synthetic control analysis. *The Lancet Global Health, 4*(9), e627–e632.

Sebhatu, A., Wennberg, K., Arora-Jonsson, S., & Lindberg, S. I. (2020). Explaining the homogeneous diffusion of covid-19 nonpharmaceutical interventions across heterogeneous countries. *Proceedings of the National Academy of Sciences, 117*(35), 21201–21208.

Sorci, G., Faivre, B., & Morand, S. (2020). Why does covid-19 case fatality rate vary among countries? *Available at: SSRN 3576892*.

Tuite, A. R., Ng, V., Rees, E., Fisman, D., Wilder-Smith, A., Khan, K., & Bogoch, I. I. (2020a). Estimation of the covid-19 burden in Egypt through exported case detection. *The Lancet Infectious Diseases, 20*(8), 894.

Tuite, A. R., Sherbo, R., Watts, A., Fisman, D., & Khan, K. (2020b). Estimation of coronavirus disease 2019 (covid-19) burden and potential for international dissemination of infection from Iran. *Annals of Internal Medicine, 10*(172), 699–701.

Zhu, Y., Xie, J., Huang, F., & Cao, L. (2020). Association between short-term exposure to air pollution and covid-19 infection: Evidence from China. *The Science of the Total Environment, 727*, Article 138704.