Journal of Public Economics 262 (2026) 105740

Contents lists available at ScienceDirect

Journal of Public Economics

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

Information manipulation and repression: Theory and evidence from the COVID-19 response in Russia

Natalia Lamberova[a], Konstantin Sonin[b,*]

[a] University of Texas at Dallas, Richardson, United States
[b] University of Chicago, Chicago, United States

HIGHLIGHTS

- Authoritarian governments used the COVID-19 restrictions to increase control.
- In Russia, the government manipulated data and used restrictions to stifle dissent.
- In autocrats' hands, propaganda and repression complement each other.

ARTICLE INFO

JEL classification:
P16
D7
P4

Keywords:
Authoritarian politics
Information manipulation
Repression
Civil society

ABSTRACT

Were COVID-19 and the associated restrictions used by authoritarian governments to tighten their grip on power? Using data from 83 Russian regions, we show that yes, they were – during the pandemic, it was the quality of institutions and the strength of civil society that limited the government's information manipulation and repression, two main tools of authoritarian regimes. Arrest waves that followed the poisoning of Alexey Navalny, the opposition leader, in August 2020 and his jailing in January 2021 were more pronounced in those regions that manipulated the COVID-19 statistics more. The increase in the usage of authoritarian tools came at a price: misinformation reduced compliance with pandemic restrictions. In addition to two-way fixed effects, we use sensitivity analysis to account for possible omitted variables. Our findings are consistent with the theoretical complementarity between propaganda and repression. Regions that had more political repression also exhibited greater information manipulation, conditional on pandemic intensity and institutional characteristics.

1. Introduction

Managing a major crisis such as the COVID-19 pandemic requires the provision of public goods, which relies on high state capacity. The classic rationale for public intervention is that private actions such as getting vaccinated, wearing masks, and avoiding large gatherings have positive externalities, so they are likely to be under-supplied by individuals on their own (Olson Jr, 1971; Ostrom, 1990). Naturally, one might expect that the power of the state to coerce others into compliance has beneficial effects. It might be, the argument goes, that authoritarian leaders, less constrained by institutions and less accountable to voters' whims, are better equipped to deal with a major emergency.

At the same time, autocrats might have used a crisis to tighten their grip on power without providing any policy benefits (Stasavage, 2020). The threat of a pandemic makes the public more likely to comply with restrictions on freedom, giving the government an opportunity to increase its control over society. In fact, democracy has worsened in more than 80 countries in the year since the onset of the COVID-19 pandemic, with a particularly marked deterioration in highly repressive states (Kavaklı, 2020; Luhrmann et al., 2020; Repucci and Slipowitz, 2020; Guasti, 2020; Fong, 2021). Among others, the Russian government has widely used the pandemic for unrelated political repression. As late as March 2024, two years after the pandemic's end, it used COVID-19 restrictions to prohibit a memorial event for the slain leaders of the opposition Alexei Navalny and Boris Nemtsov.[1]

In this paper, we demonstrate that the evidence of governments using anti-pandemic measures for propaganda and repression goes beyond

* Corresponding author.
Email addresses: natalia.lamberova@utdallas.edu (N. Lamberova), ksonin@uchicago.edu (K. Sonin).

[1] "Russian Opposition Leader Alexei Navalny Is Laid to Rest", *Time*, 03/01/2024.

https://doi.org/10.1016/j.jpubeco.2026.105740
Received 21 August 2025; Received in revised form 17 July 2026; Accepted 21 July 2026
Available online 30 July 2026

cross-country studies or a collection of anecdotes. We focus on the regional variation in the government response to COVID-19 in Russia. Although the Russian regions, with a few prominent exceptions, share a similar culture, language, and history, they vary significantly in the capacity of elites to provide public goods and maintain order, in the strength of civil society, and in the quality of political institutions. We show that high-quality institutions were associated with less information manipulation, i.e., more accurate COVID-19 death reports. Similarly, a strong civil society was associated with fewer arrests for, formally, violations of the COVID safety rules. (See Section 2 for evidence that the COVID-19 safety rules were widely used by the Russian authorities for political repression unrelated to pandemic restrictions.) Regions with greater information manipulation tended to have a higher number of political prosecutions.

Our empirical results provide new evidence on the relationship between information manipulation and repression, two main instruments of authoritarian control (Svolik, 2012). Our notion of complementarity concerns the government's joint use of these instruments. In our model, repression changes the composition of the audience that remains exposed to government information, allowing stronger information manipulation among the remaining citizens. Complementarity therefore predicts that repression and propaganda will tend to be used together. It does not require every institutional constraint that affects one instrument to generate a separately identifiable marginal effect on the other. Generally, 21st-century autocrats have been shown to use information manipulation to present themselves as competent stewards of their nations' prosperity (Treisman, 2011; Rozenas and Stukal, 2019; Guriev and Treisman, 2019, 2022). Across this literature, the emphasis is on propaganda being primarily a substitute for repression: modern dictators use information manipulation instead of relying on brute force as the previous generation of autocrats did. We demonstrate that arrests and information control are natural complements to each other as well.

Theoretically, the complementarity between information manipulation and repression as autocrats' instruments comes from the observation that repressing those who are most skeptical of the regime allows for an increase in the extent of propaganda fed to non-skeptics.[2] When the skeptics are repressed, their incentive compatibility constraint is relaxed, and the rest of the population receives more pro-regime information. We provide empirical evidence consistent with the model's complementarity prediction: conditional on pandemic intensity, regions with more political prosecutions also under-reported COVID-related deaths more (see Table 3).

Furthermore, the government's use of information manipulation and repression during the pandemic was not innocuous. In other countries, the provision of credible information has been shown to make citizens more inclined to follow pandemic-related policies (Besley and Dray, 2023). In contrast, information manipulation negatively affected citizens' self-reported compliance with public health guidelines and their willingness to get vaccinated or recommend the vaccine to vulnerable friends and family (Roozenbeek et al., 2020). We show that the underreporting of COVID-19-related deaths, a propaganda tool, did reduce the willingness of citizens to comply with antipandemic measures and therefore contributed to the impact of the pandemic. Thus, the authoritarian government's supposed advantage in providing public goods – i.e., implementing coercive public health measures – was compromised by the government's own actions to enhance its power.[3]

The 2020–2021 pandemic provided a unique opportunity to study information manipulation by focusing on a well-defined variable: the difference between officially reported COVID-related deaths and excess mortality during the same period. The truthfulness of the reporting strategy for COVID deaths is, essentially, a choice variable for state authorities. There is massive anecdotal evidence that this data was heavily manipulated around the world.[4] In contrast, the mortality data are much more reliable: even in a weakly institutionalized environment, a death enters, as a record, into many independent registers.[5] Manipulating death statistics on a country-wide scale would require the coordination of many unrelated state bodies, both national and local, such as tax authorities, pension and social institutions, and courts that manage inheritance, etc. Given that mortality data are typically well understood and can be used to calculate reliable excess death estimates (Islam et al., 2021), the difference between the reported deaths from COVID and excess mortality is a ready proxy for the government's information manipulation.

Even without targeted manipulation, estimates of COVID-related deaths depended on many factors, including country-specific medical protocols and definitions. Developed and well-institutionalized countries such as the United States and the United Kingdom exhibit discrepancies between excess deaths and official statistics. Still, the differences there are an order of magnitude smaller than the discrepancy observed in countries like Russia. Karlinsky and Kobak (2021) described significant variation when comparing excess deaths and officially reported deaths in different countries, and demonstrated that non-democracies tend to heavily underreport COVID-related deaths (see also Adiguzel et al., 2020 and Knutsen and Kolvani, 2022).

In the main specification of our study of information manipulation in Russian regions, we use OLS for panel data analysis with region- and month-fixed effects to control for unobserved, time-invariant characteristics of each region and time-specific confounders varying by region. For repression, we use pseudo-Poisson maximum likelihood, since arrest data are saturated with zeros. As the total time span of our monthly data is one year, many factors such as rural-urban composition of the region, region size, population density, the share of elderly individuals, etc. would naturally remain relatively stable in our data and would be subsumed into region-fixed effects. However, such variables may still impact the response of regional elites to pandemic pressure in the region and therefore bias the results. Although we cannot account for all unobservable variables, we can quantify their potential impact using sensitivity analysis in Cinelli and Hazlett (2020) and VanderWeele and Ding (2017) for the OLS and PPML specifications, respectively.

The rest of the paper is organized as follows. Section 2 briefly provides the necessary background. Section 3 presents the theoretical framework, the data and our empirical strategy. Section 4 describes our main results, sensitivity analyses, and robustness checks. We relegate the theoretical model to the Online Appendix.

2. The context: Russian politics, propaganda, and COVID-19

During his 20-plus-year rule, Russian leader Vladimir Putin has accumulated unprecedented personal power (Taylor, 2011; Frye, 2022). However, when the COVID-19 pandemic occurred, he chose to delegate

[2] Recent theoretical models of information manipulation as an instrument of authoritarian control include Lorentzen (2013, 2014); Huang (2018); Shadmehr and Bernhardt (2015); Boleslavsky et al. (2021); Rozenas and Stukal (2019); repression has been modeled in Tyson (2018); Dragu and Przeworski (2019); Montagnes and Wolton (2019), and Rozenas (2020).

[3] Recent experimental data from Russia show that Putin's popularity is indeed vulnerable to negative information (Buckley et al., 2022); see also Treisman (2011); Frye et al. (2017); Hale (2022) on determinants of Putin's popularity.

[4] Kilani (2021) shows that countries with low freedom scores are more likely to manipulate their COVID-19 reporting. Governments' efforts to manipulate COVID-19 statistics are documented in Ahinkorah et al. (2020) for African countries, in Pomeranz and Schwid (2021) for Belarus, and in Ricard and Medeiros (2020) for Brazil.

[5] See an extensive discussion in Kofanov et al. (2023); this paper uses data on excess mortality and COVID-19 official reporting in Russian regions to study the impact of local electoral elections and its interaction with the need to hold federal referendum on incentives for local bureaucrats. Including the proximity to elections, as well as its interaction with the pre-referendum dummy – their main variables of interest – does not change our conclusions (see Tables A-7, OA-11, A-8, and OA-3).

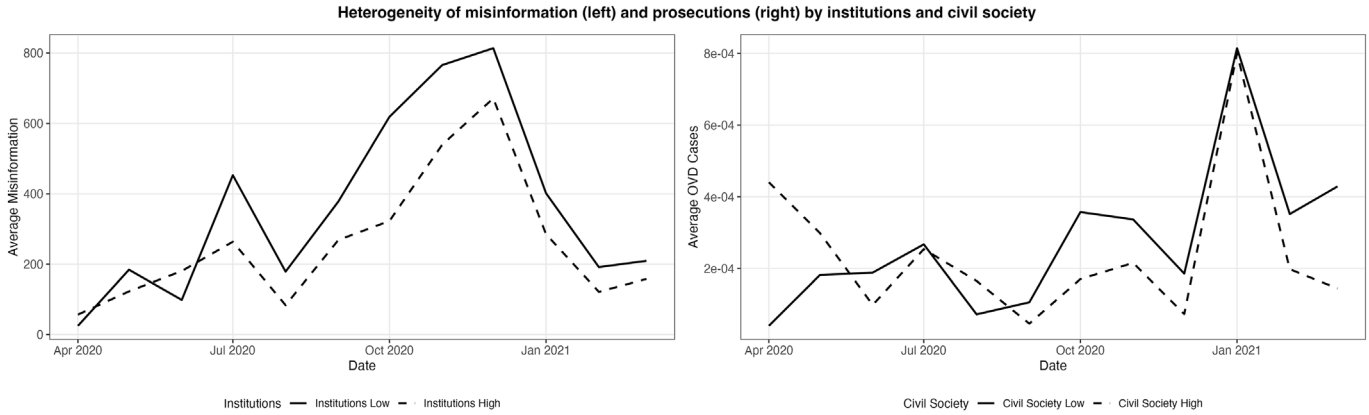

Fig. 1. Misinformation and prosecutions in regions with high and low levels of civil society and institutional quality. *Notes:* Left panel: **Misinformation** = excess deaths (Section 3) - officially reported COVID-19 deaths. Regions are split at the sample median of institutional quality, and values are weighted by region size; Right panel: **Political prosecutions** are normalized by 2019 population; regions are grouped by civil society strength (historical share of people in NGOs); colors as above.

responsibility for fighting the pandemic to regional governments, in contrast to many countries such as Hungary, Philippines, Thailand, Egypt, Turkey, and Jordan, where the pandemic response was highly centralized. On 2 April 2020, Russian regional governors were granted special authority to choose measures for preventing the spread of COVID-19 in their regions (Busygina and Klimovich, 2024).

The regions approached the pandemic in profoundly different ways. About 30 regions chose to require electronic passes for citizens to leave their homes. Only a few regions declared a *force majeure,* while in most cases the regions labeled lockdowns as "non-working days", making it harder for businesses to handle lapses in contractual obligations that arose during the pandemic. Figure OA-3 shows the restriction map for public gatherings in September 2020 (Smirnova et al., 2020).

Just as the policy responses in the regions varied, so did the regions themselves – in the strength of civil society, measured by the share of workers employed by NGOs (Salamon et al., 2020) and in the quality of formal political institutions (Kynev, 2017). They also differ in the extent to which the Putin-led ruling party, *United Russia*, has control of each regional parliament.[6]

The authorities started to use coronavirus restrictions to target opposition as early as March 2020, the first month of the pandemic.[7] In July 2020, dozens of Muscovites were arrested at a protest in Moscow against constitutional reforms under the pretext of COVID-19 restrictions.[8] In January 2021, after Alexey Navalny, the leader of the Russian opposition, returned to Moscow from a medical treatment in Germany, the police warned that any protests against his arrest would violate COVID-19 restrictions.[9] On January 30, a group of opposition leaders was put under a two-month house arrest on charges of allegedly violating coronavirus restrictions during the protests.[10] In March 2021, ten more opposition figures were confined to house arrest "ostensibly for violating coronavirus safety rules".[11] As late as March 2023, the government arrested protesters – now, against the Russia's war in Ukraine – charging them with violating COVID restrictions.[12] In March 2024, three years after the pandemic restrictions ceased to be enforced for non-political matters, the Moscow government used them to block a Navalny memorial event.[13]

The spread of COVID-19 in Russia and the authorities' efforts to suppress information are documented in a number of academic papers (Zemtsov and Baburin, 2020; Lifshits, 2020; Kobak, 2021; Nusratullin et al., 2021; Kofanov et al., 2023) and major news media.[14] Fig. 1 (left panel) shows raw data on the difference between excess mortality and officially reported COVID-related deaths over time, based on the quality of institutions and number of political prosecutions (as a share of 2019 population) by the strength of civil society. It shows that regions with higher-quality institutions produce less information manipulation.

After the onset of the pandemic, the Russian government introduced or amended several normative acts that effectively allowed prosecution of political activists, protesters, and members of the media. Although the new legislation was passed at the federal level, its enforcement has varied widely across regions. On 1 April 2020, Federal Law No. 100 was adopted imposing criminal liability for violations of sanitary and epidemiological rules. Now, Article 236 of the Criminal Code imposes the criminal liability not only for causing a massive disease or poisoning of people through negligence but also for "creating a threat" of such consequences. In particular, the law criminalized the "public dissemination of deliberately false information" about threats to the life and safety of citizens, and/or about measures taken to ensure the safety of the population (Articles 207.1 and 207.2 of the Criminal Code).

Article 236, the presumably anti-COVID-19 rule, was applied almost exclusively to opposition activists, such as members of Alexei Navalny's

[6] *United Russia*, the largest party in the country, has been the ruling party since the early years of Vladimir Putin's presidency. After the 2021 elections, it holds 336 (74.66%) of the 450 seats in the State Duma. The party was formed in December 2001 through a merger of the *Unity* and *Fatherland–All Russia* parties (Hale, 2004; Reuter and Szakonyi, 2019; Frye, 2022).

[7] "Coronavirus Forces Putin Critics to Scale Back Protests Before Big Vote", *Reuters*, 03/20/2020.

[8] "Hundreds Protest in Moscow Against Reforms that Could Keep Putin in Power", *Reuters*, 07/15/2020.

[9] "Police Crack Down on Russian Protests Against Jailing of Kremlin Foe Navalny", *Reuters,* 01/22/2021.

[10] "Over 5,100 arrested at pro-Navalny protests across Russia", *ABC News*, 01/31/2021.

[11] "In Russia, a Virus Lockdown Targets the Opposition", *The New York Times,* 03/19/2021.

[12] "In St. Petersburg, A Solo Protester with Sign "Let Us All Be Friends" Is Detained for Violating COVID Restrictions" [in Russian], *Mediazona*, 03/19/2023.

[13] "Putin Foe Alexei Navalny is Buried in Moscow as Thousands Attend under a Heavy Police Presence", *APNews*, 03/01/2024.

[14] Among other reports: "Russia's COVID Death Toll Could Be 70 Percent Higher Than Official Figure", *The Financial Times,* 05/11/2020, "You Can't Trust Anyone': Russia's Hidden COVID Toll Is an Open Secret", *The New York Times,* 04/10/2021, "Data Suggests Russia's COVID-19 Death Toll Is Far Higher Than Reported", *ABC News,* 07/30/2021, "In Russia, Experts Are Challenging Official Pandemic Figures as Too Low. They Refuse to Be Silenced", *The Washington Post*, 10/16/2021.

team or his supporters (Borodikhin, 2021). Even more widespread was the use of Article 6.3 of the Administrative Code. On 1 April 2020, Article 6.3 of the Code of Administrative Offenses was amended to introduce administrative liability for violating sanitary and epidemiological norms during the spread of a dangerous disease. Some cases against the political opposition combined the use of COVID-19 restrictions with other articles of the criminal code. For example, organizers of the Forum of Municipal Deputies were fined according to Article 6.3, an anti-COVID measure) and Article 20.33 (cooperation with an "undesirable organization", a purely political article).[15] In general, most of the cases opened under this statute were political, including nonviolent solo protesters who maintained social distancing and were fully masked.[16] In our empirical exercise, we use prosecutions under Article 6.3 as a measure of state repression.

As mentioned above, the enforcement of federal statutes, including Article 6.3, has varied significantly between regions. The right panel of Fig. 1 shows the raw data on the political prosecutions collected by investigative journalists in *OVD-Info* and aggregated by the strength of civil society in the region (above or below the median) and the divergence between excess deaths and deaths officially attributed to COVID-19 by the quality of institutions (above or below the median). It suggests that regions with stronger civil society have fewer arrests under Article 6.3.

3. Theoretical framework, data, and empirical strategy

In this section, we introduce our theoretical framework, main data sources, and the empirical strategy.

3.1. Theoretical framework

We consider a strategic interaction between the government which is interested in citizen support and citizens who care about whether the government competently delivers state services. (Full formal details are available in the Online Appendix.) Citizens are willing to support the government only when it is competent; to protest when it is not. The government wants to stay in power; it has two tools at its disposal: repression and information manipulation.

Citizens are heterogeneous in their ex ante alignment with the government. Some are "skeptics" who do not think highly of the government to begin with. Some are "non-skeptics" – their prior about the government's competence is relatively high. Skeptics therefore have a higher baseline propensity to protest.

The government can shape the beliefs of citizens through information manipulation: it establishes an institutionalized mechanism that sends a public signal about the government's competence. In addition, with some probability, it can manipulate reported outcomes after learning the true state. This captures realistic situations where institutions and reporting systems are partly determined in advance, but the government retains some ability to distort the final information that citizens observe. If the probability that information is manipulated ex post is sufficiently small, the citizens' beliefs can be altered.

As is standard in the information manipulation literature, the optimal government policy takes the form of selectively censoring unfavorable information (Kamenica and Gentzkow, 2011; Gehlbach and Sonin, 2014). For citizens, bad news is decisive (it reveals government's incompetence and triggers protest), while good news is ambiguous because it may reflect true competence or manipulation. When citizens observe good news, they support the government only if their personal inclination to support exceeds their posterior belief that the government is competent. Since skeptics are less aligned, it is more difficult to persuade them through positive messaging: they are persuaded only if they know that the censorship of bad news is sufficiently infrequent.

The extent of government manipulation of information is limited by the citizens' access to alternative information channels. If local institutions are strong, real information is available to citizens at a low cost. Where institutions are weak, the cost of avoiding government propaganda channels is higher and citizens are more dependent on manipulated information. So, if the government pushes information manipulation too far, citizens infer that the news is unreliable and opt to look for information elsewhere. As a result, the government chooses an equilibrium level of "slant" that is high enough to boost perceived competence, yet low enough that the targeted citizens still consume and use, in their decision-making, government-provided information. This mechanism yields the core implication: stronger local institutions reduce the extent of manipulation.

The connection between the level of information manipulation that the government chooses and the level of repression is as follows. Choosing the optimal slant, the government faces the following tradeoff. It can target the whole population with relatively mild manipulation (so that even skeptics keep relying on government-provided information), or, alternatively, it can target only the non-skeptics with heavier manipulation. As non-skeptics have a higher prior about the government's competence, their beliefs are more easily manipulated; they can be fed more propaganda.

The optimal strategy depends on population composition and polarization, as well as the availability of alternative sources of information. If skeptics are numerous and not too ideologically distant, the government finds it worthwhile to keep them engaged and uses milder propaganda aimed at everyone. If skeptics are few or very hostile (have low priors), the government instead arrests some of them and concentrates on intensifying persuasion among non-skeptics. Repression reduces the effective weight of skeptics in the audience. If the cost of arresting skeptics, which is determined in part by the strength of civil society, is relatively low, the optimal government strategy shifts away from "light propaganda for all" to "heavy propaganda for non-skeptics." Once many skeptics are arrested or silenced, the government no longer needs to keep manipulation low enough to retain them as consumers of state news.

This logic produces the complementarity prediction: arrests and information manipulation reinforce each other. Repression does not substitute for the need for propaganda; instead, it enables stronger manipulation by narrowing the audience to those most easily persuaded. Therefore, regimes capable of efficient repression can also sustain higher levels of information manipulation and generate greater political support through inflated perceptions of competence. Importantly, this prediction concerns the government's equilibrium choice of instruments rather than comparative statics with respect to every institutional constraint. The model predicts that regimes capable of efficient repression can sustain stronger information manipulation. It does not imply that every factor constraining repression necessarily produces a separately detectable marginal effect on information manipulation, or vice versa. Those relationships depend on where the equilibrium lies and need not be identified separately in finite samples.

3.2. Explanatory variables

Excess deaths. For our empirical strategy, we need a ground-truth measure of true COVID-19 casualties. We make use of the fact that the total mortality data is less susceptible to manipulation by reporting officials. We measure the actual intensity of the COVID-19 pandemic using *excess deaths*, defined as the difference between the expected and reported number of deaths for a given region and month. The expected deaths are estimated by linearly extrapolating the trend for 2015–2019, following the approach of Kobak (2021). This estimation uses five years of pre-pandemic data to predict the number of deaths that would have occurred in the absence of the pandemic in a given region in a given

[15] Tatyana Usmanova, a Forum of Local Assemblymen Coordinator, Is Slapped with a Third Administrative Case [in Russian], *OVD-Info*, 05/14/2021.

[16] In Petrozavodsk, Environmental Activists Defending the Kamenny Bor Park Are Detained for Violation of COVID Restrictions [in Russian], *OVD-Info*, 04/12/2020; "In Vologda, Police Detains Members of the Doctors' Alliance Trade Union" [in Russian], *OVD-Info*, 11/11/2020.

month. Hence, the resulting excess mortality measure accounts for the size of the regional population.

Political institutions in Russian regions. The Russian regions exhibit substantial variation in the quality of their political institutions. Kynev (2017) introduced a composite index that measures institutional quality at the regional level for years 2011, 2015, 2016, and 2017. This index incorporates four key dimensions: political competition, institutional independence of regional parliament deputies, protections for opposition rights in parliament, and local self-governance (see Kynev, 2017, for a detailed description). *Political competition* is defined by the effective number of parties, $N = \frac{1}{\sum p_i^2}$, where p_i is the share of votes for each party in all types of elections at the regional level. *Local self-governance* is assessed through a score-based system. The highest score (5) is assigned when the head of a municipal government is directly elected by citizens. A score of 4 applies when an elected municipal leader governs alongside an appointed city manager. A score of 3 is given when municipal elections are absent and governance is handled by an appointee selected by local deputies. A score of 2 reflects a system where deputies choose the municipal head from a pre-approved list. The lowest score (1) applies to St. Petersburg and Moscow.[17] *Institutional independence* is measured by the share of professional deputies in the regional parliament. These deputies receive a salary and are prohibited from participating in entrepreneurial activities, reducing potential conflicts of interest. *Protection of opposition rights* is captured by the share of parliamentary leadership positions—speaker, vice-speaker, and committee chairs—held by parties other than United Russia.

The Kynev index provides a comprehensive measure of institutional quality by capturing these four dimensions across Russian regions. In the dataset, it ranges from a minimum of 0.6 to a maximum of 23.0, with a mean of 7.3 and a standard deviation of 6 (see Table A.2). For ease of interpretation, we normalize it to take values between 0 and 1.

An alternative measure of regional institutional quality is the widely used Petrov-Titkov index of regional democracy, which relies on expert evaluations. This index takes a holistic approach, assessing political pluralism, the strength of civil society, and the extent to which formal democratic institutions are undermined by corruption and fraud. However, it is only available for 2001 and for the years 2003–2010, missing critical institutional shifts after the 2011 protests and the subsequent reintroduction of regional elections. By contrast, the Kynev index accounts for the multidimensional nature of regional institutions while also capturing the major institutional shocks that have shaped Russia's political landscape.[18] Table OA-8 presents the main specification with these measures, instead of Kynev institutional quality. Unless otherwise stated, the civil-society and institutional-quality measures used in the regression analyses are min–max normalized to range from 0 to 1, so a one-unit change corresponds to moving from the lowest to the highest observed value in the sample.

Civil society. The development of a civil society can be operationalized through different variables. In our data set, the variable *Civil* measures the share of the population employed by NGOs from the economically active population in 2013 (Salamon et al., 2020). The choice of the cut-off date is justified by the fact that, from 2014, the Ministry of Justice was authorized to register independent groups as "foreign agents" with minimal justification and without a court order. As a result, many NGOs stopped registering with the government (Sobolev et al., 2018).

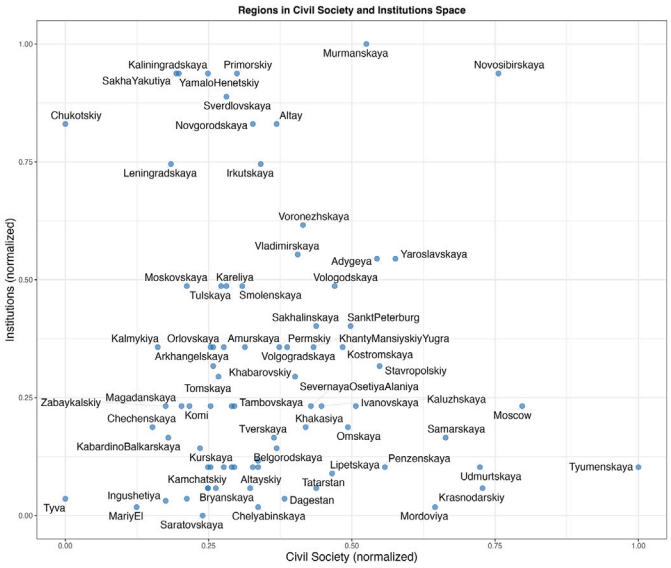

Fig. 2. Russian Regions, institutional Quality, civil Society. *Notes:* Figure places Russian Regions in two dimensions: strength of civil society and institutional quality, both normalized to take values between 0 and 1.

Civil Society and Institutional Quality parameters capture two distinct dimensions: the ability of the citizens of the region to organize, and institutional restrictions on the regional elites. Fig. 2 plots the Russian regions in a space of these variables, normalized to take values between 0 and 1.

Yandex searches. During the period of the study, there were significant opposition protests related to the investigation of Alexei Navalny's poisoning in August 2020. We utilized Yandex search data (which is similar to Google Trends data). The following keywords were used to collect data on the share of the key term in the search volume by region and month: "Navalny", "Dvorets Putina" ("Putin's Palace"), "Protest".

3.3. Outcomes of interest

Official COVID-related deaths. Official data on pandemic-related deaths comes from the Rosstat database (ROSSTAT Russian Bureau of Statistics, 2021), where a death is classified as COVID-related if COVID-19 is listed as the primary cause. In the absence of information manipulation, excess mortality and officially reported COVID-related deaths should be nearly identical. In reality, excess mortality is four to five times higher than official count, indicating significant under-reporting.

Repression. We quantify repression using the number of cases initiated under Article 6.3 of the Administrative Code of Russia, which was introduced in April 2020 in response to the pandemic. Independent journalists (Mediazona) and human rights organizations (OVD-Info) report that most cases under this article have been used for political prosecutions. In Section 2, we provide examples that illustrate how Article 6.3 has been used for political repression that is not related to the pandemic.

Because we cannot rule out that some of these cases are due to genuine violations of pandemic restrictions, we provide a secondary measure of repression using the database of political criminal cases maintained by OVD-INFO, a Russian human-rights and media project that systematically monitors politically motivated criminal proceedings (OVD-Info, 2023). OVD-Info compiles its dataset from daily monitoring of Russian media, social networks, and official websites of investigative bodies and courts, as well as direct reports from defendants, their relatives, cooperating lawyers, and other NGOs. The database includes criminal cases that either have already been recognized as political by

[17] In St. Petersburg and Moscow, the mayor also serves as the regional head, merging city and regional administrations. This structure places them under greater federal control compared to regions where mayors and governors are selected through separate processes.

[18] Figure OA-2 presents correlations between information manipulation, executive and parliamentary transparency measures in 2002 (as used in Yakovlev and Zhuravskaya, 2013), and the Kynev scores.

organizations such as Memorial and Sova or that OVD-Info itself classifies as politically motivated, drawing on international standards for "political prisoners", the use of "anti-extremism" provisions, and a set of "presumptively political" articles (e.g., on undesirable organizations, "foreign agents", and anti-war speech). They treat as political prosecutions those cases in which the defendant is subjected to deprivation of liberty (pre-trial detention, penal colony, house arrest, compulsory psychiatric treatment) or is prosecuted under extremism/"undesirable organization" legislation. Figure OA-7 shows that in all regions the COVID-related article was used much more frequently than explicitly political or extremism-related provisions, suggesting that it became the predominant legal vehicle for low-level political repression.

This measure is subject to a strong sample-selection problem: cases enter the OVD-Info database only if they become visible to the organization through media coverage, official publications, or self-reporting. Awareness of OVD-Info is uneven and likely concentrated among urban, better-connected activists, implying substantial underreporting in the general population. Consistent with this, the regional distribution of OVD-Info-classified political prosecutions is much more biased than that of Article 6.3 cases (Figure OA-7). In the Online Appendix (see Table OA-4) we reproduce our main specifications using the OVD-Info measure as a result. The estimated marginal effects of excess mortality at the levels of civil society (Fig. 3) are similar in magnitude, but have larger standard errors and lose conventional statistical significance. Taken together, this evidence supports the use of Article 6.3 as a conservative but informative proxy for regional variation in political prosecutions.

Isolation index. The Self-isolation index (presented in Figure OA-4) is developed by *Yandex*, Russia's leading GPS service and search engine.[19] This index serves as an inverse measure of highway congestion in major cities, comparing current *levels of Yandex* user activity with those observed on a typical workday before the pandemic. A higher index indicates greater self-isolation. Originally used in Egorov et al. (2021), this measure revealed that mobility reductions following the first local COVID-19 case were more pronounced in Russian cities with higher levels of ethnic fractionalization and xenophobia.

3.4. Other regional covariates

In our robustness checks and sensitivity analyses, we incorporate population density, the pre-pandemic share of *United Russia* in regional parliaments, and the number of months until regional elections. Additionally, for cross-region illustration, we include a set of time-invariant covariates: distance to Moscow, availability of public transportation (railroads and buses), region area and population in 2019. Table A.1 provides descriptions of these variables along with their data sources. Summary statistics are presented in Table A.2, where Panel A reports statistics on regional responses and non-time-varying characteristics, while Panel B focuses on time-varying regional characteristics.

3.5. Identification strategy

We adopt a two-way fixed effects strategy for identification. We exploit both the intensity of COVID-19 and the cross-sectional differences in the prepandemic measures of institutional quality and strength of civil society. Specifically, we estimate the following linear two-way fixed effects model to capture information manipulation:

$$\text{Official deaths}_{it} = \alpha + \beta_1 \text{explanatory}_i \times \text{excess deaths}_{it} + \beta_2 \text{excess deaths}_{it} + r_i + m_t + e_{it},$$

where *Official deaths* is the number of deaths in the region i in months t officially attributed to the COVID-19 pandemic. Explanatory variables include a measure of civil society and a measure of the quality of regional political institutions. Region fixed effects r_i account for the region-specific (but month-invariant) characteristics of the region. The inclusion of month-fixed effects m_t accounts for month-specific (but region-invariant) unobserved confounders. Throughout the regression analysis, civil society and institutional quality refer to the normalized 0–1 versions of these variables.

The identification strategy for repression is similar, but since Article 6.3 prosecutions are rare – many regions have 0 such cases in many months – we estimate the model using Pseudo-Poisson Maximum Likelihood (PPML)[20]:

$$E[y_{rt} \mid X_{rt}] = \exp\Big(\beta_1 (100 \times \text{Excess deaths})_{rt} + \beta_2 (100 \times \text{Excess deaths})_{rt} \times \text{civil society}_r + \beta_3 (100 \times \text{Excess deaths})_{rt} \times \text{institutions}_r + \gamma_r + \delta_t\Big), \quad (1)$$

where $(100 \times \text{Excess deaths})_{rt}$ is 100 times excess deaths in region r at date t, civil society_r and institutions_r are the normalized civil-society and institutional-norm indices, and γ_r and δ_t are region and date fixed effects, respectively. Results are presented in Table 2.

Fig. 3 presents marginal effect of excess deaths by civil society for both measures of prosecution. While the OVD-info classified prosecutions are insignificant, the marginal effects are very similar in magnitude, validating the measurement of prosecutions via Article 6.3. In Table OA-4, we present the model similar to Model 1 with OVD-info classified prosecutions as outcome, restricting the sample to exclude Moscow and St. Petersburg due to large differences in reporting probability in those locations compared to the rest of the sample. The results are similar in magnitude to the results of the main specification, but are statistically insignificant.

Focusing on pretreatment measures alleviates the concern that the pandemic had an impact on them, rather than the other way around. In addition, the strength of civil society and the quality of institutions develop gradually and change slowly over time, making it impossible to exploit their temporal variation within a year. As a result, differences in institutional quality and civil society strength are primarily cross-sectional, making identification dependent on a selection-on-observables assumption. To address this limitation, we use a two-way fixed effects estimator that captures changes over time by comparing differences rather than levels. This approach follows the factorial difference-in-differences as applied in Cao et al. (2022), who examined how social capital influenced famine-related mortality across Chinese regions, and as further developed in Xu et al. (2024). As in that setting, there is no fully untreated control group, and all regions were exposed to the pandemic at the same time but with varying intensity. Therefore, we do not face the issue of staggered adoption.

Although our approach follows the logic of factorial difference-in-differences, it differs from previous applications in key ways. In Cao et al. (2022) and Xu et al. (2024), treatment is binary, distinguishing between high and low levels of social capital and the pre- and post-famine periods. Under standard difference-in-differences assumptions – parallel trends and no anticipation — this framework recovers effect modification, describing how the impact of exposure varies across groups. With an additional assumption of conditional factorial parallel trends, where

[19] Data available at https://datalens.yandex/7o7is1q6ikh23.

[20] PPML specifies the conditional mean as $E(y_{rt} \mid X_{rt}) = \exp(X'_{rt}\beta)$. PPML maximizes the Poisson log-likelihood, but consistency of $\hat{\beta}$ only requires correct specification of the conditional mean rather than the full Poisson distribution (Gourieroux et al., 1984). In contrast to log-linear OLS, PPML remains consistent under heteroskedasticity and avoids the bias induced by taking logs of the dependent variable (Santos Silva and Tenreyro, 2006). An additional advantage is that PPML naturally accommodates zero outcomes—which are frequent in our data—because it is estimated in levels rather than in logs. Simulation evidence further shows that PPML performs well with highly skewed dependent variables and a large fraction of zeros (e.g. Santos Silva and Tenreyro, 2011; Correia et al., 2020).

changes in outcomes are independent of group assignment conditional on observables, the approach allows for a stronger causal interpretation: estimating how the effect of exposure would change if one intervened in the baseline factor defining group membership.

Our setting is different: first, the number of official deaths from COVID is not defined before COVID (although excess mortality is). Similarly, the number of arrests for violations of COVID regulations is not defined before COVID. Hence, our pre- and post- variables are not binary. Instead, we used variation in the intensity of COVID (measured by excess deaths). Second, our strength of civil society and institutional quality variables are also continuous variables. Our results are interpreted in the following way without additional assumptions to the usual two-way fixed effects interpretation with selection on observables: at a given level of COVID intensity (excess deaths), what would be the difference in officially reported deaths (COVID-related prosecutions) in regions where the quality of institutions (strength of civil society) is better by 1 unit. Under additional assumption (factorial parallel trends for binary setting, assuming that the changes in the outcome would be conditionally orthogonal to the institutions/civil society), our interpretation would be stronger: at a given level of COVID intensity (excess deaths), what would the officially reported deaths (COVID-related prosecutions) change if we improved the quality of institutions (strength of civil society) by 1 unit. Although this additional assumption is not directly testable, we find that including control variables that could affect the severity of the pandemic – such as population density, the share of United Russia in regional parliaments, and the time until regional elections – does not alter our results.

Since institutional quality and civil society can also influence the severity of the pandemic itself, we provide further evidence using average excess mortality in neighboring regions as an instrumental variable for the local intensity of COVID-19. Furthermore, to account for the role of COVID-19 growth dynamics rather than absolute levels alone, we incorporate a quadratic term for excess deaths in our analysis. Further details on robustness checks are provided in Section 4.1.

4. Results

In this section, we present our main empirical findings. We first show results for information manipulation, after which we discuss findings on repression. We then summarize our results from a series of robustness checks, the details of which appear in the Appendix.

Columns 3 and 4 of Table 1 suggest that moving from the lowest to the highest institutional quality in the sample increases officially reported COVID-related deaths by approximately 35–44 per 100 excess deaths. At the sample mean of institutional quality (≈ 0.318), the implied marginal effect of each 100 excess deaths is approximately 16.4 officially reported deaths.

In the absence of information manipulation, there should be a strong positive correlation between the official count of deaths from COVID-19 and excess mortality, as official reports would accurately reflect the progression of the disease. Furthermore, if the reports were impartial, it should not be expected that factors such as the strength of the civil society or the quality of the institutions exert any influence on the reporting of COVID at a given level of excess deaths. In contrast, Table 1 suggests that in regions with better institutional quality, an increased number of pandemic deaths leads to a higher number of officially reported COVID deaths than in regions with lower-quality institutions, consistent with our theoretical predictions. In this and the following tables, errors are clustered at the region and month levels.

Next, we document the impact of institutions and civil society on prosecutions under Article 6.3 by estimating the regression presented in Eq. (1).

Consistent with the model, we find that at a given strength of the pandemic, a stronger civil society is associated with a larger proportional decline in prosecutions (Table 2). Since the measure of strength of civil society is normalized between 0 and 1, moving from the weakest to the strongest region on this dimension makes the semi-elasticity of Article 6.3 prosecutions with respect to excess deaths 0.031 log points more negative, i.e. about 3% weaker in multiplicative terms.

Interestingly, while the strength of civil society is negatively correlated with the instances of prosecution for the violation of COVID rules at the given level of pandemic, political institutions are not. Civil society is strongly associated with lower repression, whereas institutional quality is strongly associated with more truthful reporting. The corresponding cross-effects are imprecisely estimated, with confidence intervals that include substantively meaningful effects. Thus, our evidence does not allow us to draw strong conclusions about these secondary comparative statics. Table OA-4 replicates the same analysis, using OVD-classified political prosecutions. The results are directionally the same, but become statistically insignificant. Fig. 3 presents nearly identical marginal effects of excess deaths on political prosecutions for the two measures by different levels of civil society.

Table 1
The number of deaths officially attributed to COVID-19.

	Dependent variable:			
	Official deaths from COVID-19			
	(1)	(2)	(3)	(4)
Excess deaths	33.774***	21.546	24.646**	2.478
	(11.114)	(18.350)	(11.621)	(16.564)
Excess deaths × civil		23.675		38.433
		(43.484)		(39.342)
Excess deaths × institutions			34.949**	43.827***
			(13.963)	(16.446)
Month FE	+	+	+	+
Region FE	+	+	+	+
Observations	984	984	984	984
R^2	0.817	0.821	0.830	0.840
Adjusted R^2	0.798	0.802	0.812	0.823
RMSE	202.45	198.32	190.68	184.33

Notes: Table presents OLS regression estimates. *Official deaths* is the number of deaths in region i in month t officially attributed to the COVID-19 pandemic. *Excess deaths* is the difference between the expected number of deaths over a five-year period for a given region in a given month and the actual number of deaths reported in official statistics. *Civil* measures the share of the population employed by NGOs from the economically active population in 2013. *Institutions* is a composite score of political institutions from Kynev (2017). Standard errors are two-way clustered at the region-month level and reported in parentheses. Stars denote p-values at conventional levels: *p<0.1, **p<0.05, ***p<0.01.

Table 2
Article 6.3 cases.

	Dependent variable:			
	Article 6.3 prosecutions			
	(1)	(2)	(3)	(4)
Excess deaths	−0.032**	−0.008	−0.045**	−0.031*
	(0.012)	(0.009)	(0.017)	(0.015)
Excess deaths × civil		−0.046**		−0.023***
		(0.018)		(0.006)
Excess deaths × institutions			0.054	0.048
			(0.033)	(0.032)
Month FE	+	+	+	+
Region FE	+	+	+	+
Observations	960	960	960	960

Notes: Table presents PPML regression estimates. *Article 6.3 prosecutions* is the number of cases initiated under Article 6.3 of the Administrative Code of Russia per population. *Excess deaths* is the difference between the expected number of deaths over a five-year period for a given region in a given month and the actual number of deaths reported in official statistics. *civil* measures the share of the population employed by NGOs from the economically active population in 2013. *institutions* is a composite score of political institutions from Kynev (2017). Standard errors are two-way clustered at the region-month level and reported in parenthesis. Stars denote p-values at conventional levels: *p<0.1, **p<0.05, ***p<0.01.

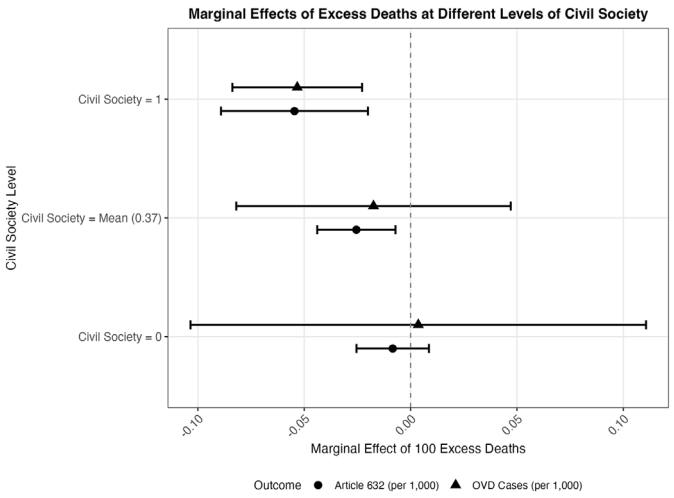

Fig. 3. During COVID, regions with stronger civil society experienced fewer political prosecutions. *Notes:* Figure plots the marginal effects of COVID intensity on political prosecutions by the strength of civil society. The outcomes are political prosecutions per thousand people as classified by OVD-info and number of Article 6.3 cases per thousand people, respectively.

The theory predicts that repression and information manipulation are jointly employed as complementary instruments of authoritarian control. Rather than testing whether constraints on one instrument of authoritarian control necessarily alter the use of the other, Table 3 examines whether the two instruments are positively associated once pandemic intensity and institutional determinants are held constant.

Table 3
Complementarity of information manipulation and repression.

	Dependent variable:	
	Official deaths from COVID-19	
	(1)	(2)
Article 6.3 cases	−114.739*	−96.675*
	(56.780)	(43.958)
Excess deaths	0.797*	0.583*
	(0.359)	(0.278)
Excess deaths × United Russia share	−0.773+	−0.741*
	(0.402)	(0.309)
100 × Excess deaths × institutions	0.003*	0.004**
	(0.001)	(0.001)
100 × Excess deaths × civil society		0.003
		(0.002)
Month FE	+	+
Region FE	+	+
Observations	984	984
Adjusted R^2	0.845	0.853
F Statistic	57.905***	60.881***

Notes: Table presents OLS regression estimates. *Official deaths from COVID-19* is the number of deaths in region i in month t officially attributed to the COVID-19 pandemic. *Article 6.3 cases* is the number of cases initiated under Article 6.3 of the Administrative Code of Russia. *Excess deaths* is the difference between the expected number of deaths over a five-year period for a given region in a given month and the actual number of deaths reported in official statistics. *United Russia share* is the share of seats held by *United Russia* in regional parliaments. *institutions* is a composite score of political institutions from Kynev (2017). *civil society* measures the share of the population employed by NGOs from the economically active population in 2013. Standard errors are two-way clustered at the region-month level and reported in parentheses. Stars denote p-values at conventional levels: +p<0.1, *p<0.05, **p<0.01, ***p<0.001.

4.1. Robustness checks and sensitivity analysis

In observational settings, where a true controlled experiment is unavailable, there is always a risk of the omitted variable bias, functional form misspecification, or the absence of valid alternative explanations. In this section, we quantify the threat posed by each of these factors and provide evidence that our conclusions survive under a reassuring range of parameters.

4.1.1. Omitted variable bias from time-invariant region characteristics

Regions that are more likely to have better institutions might also be more likely to have a different urban/rural share, a different educational makeup, a different set of dominant industries, different levels of labor force engagement, different levels of cultural traditionalism, etc. These factors can interact with pandemic pressure in a way that would not be captured by the region-fixed effect.

The share of *United Russia* in regional parliaments reflects the ability of regional elites to manipulate information (on COVID-19 or elections) and to prosecute dissent. It could be correlated with the capacity of the regional state and thus have an effect on the strength of the pandemic. Its interaction term with excess deaths remains robust, substantively important, and statistically significant across all specifications. Tables A.4 and A.5 show the heterogeneity in official reports of COVID-related deaths based on the share of *United Russia* in regional parliaments: at the same level of the pandemic, regions with a lower share of *United Russia* report a greater number of COVID deaths and experience more prosecutions.

We choose another important factor, population density, which in the Russian context is highly correlated with urban/rural shares, education level, and age composition, and use its interaction term with pandemic pressure as another potential omitted variable. The inclusion of the share of United Russia in the regional parliament and population density does not alter our main results (See Tables A.4, A.5 and 3). These interaction terms would serve as benchmarking variables for sensitivity analysis for information manipulation and prosecutions.

Since inclusion of COVID-19 intensity interactions with all potential region-specific factors is impossible, we report the relative magnitude of the omitted variable strength that would be necessary to invalidate our results in Section OA1 of the Appendix.

Next, we address the possibility that there is a link between regional institutions and civil society and the magnitude of the pandemic shock. For example, places that are more likely to have better regional institutions might be more likely to have different urban/rural shares, educational makeup, dominant industries, levels of labor force engagement, cultural traditions, etc. All of these factors strongly influence accessibility and the way people interact with doctors, hospitals, and the formal healthcare sector, and thus can affect excess mortality, biasing the results.

To break possible confounding links between institutions and civil society and pandemic pressure measured by excess deaths, we instrument excess deaths in a given region by the average excess deaths in neighboring regions. Instrumenting an endogenous part of the endogenous-exogenous variable interaction was shown to be a valid strategy by Balli and Sørensen (2013). Our instrument has high predictive power, with the F-statistic being 824.7 on 953 degrees of freedom in the first stage. As COVID-19 spreads through human contact, it is reasonable to expect significant transmission across regional borders. At the same time, even neighboring regions in Russia tend to have very different institutional quality and strength of civil society. Table OA-12 presents the results. It suggests a smaller, but still significant, heterogeneity of misinformation based on institutional quality.

A lower number of prosecutions in regions with stronger civil society could reflect greater compliance with anti-pandemic measures, as well-organized civil societies can facilitate the adherence to public health guidelines. To test this explanation, we estimate the effect of the severity of the pandemic on the willingness of citizens to stay home, using the *Yandex* self-isolation index for all regional capitals during the pandemic

period. Table A.6 presents the results, evaluating whether self-isolation behavior depends on perceived threat level, institutional quality, or civil society strength. The findings suggest that none of these factors had a significant impact.

Unlike official COVID-19 statistics, excess mortality data are neither widely disseminated by the state media nor readily available due to reporting delays. In contrast, the official COVID-19 case numbers are easily accessible to the public. Table A.6 shows that self-isolation increases when the official COVID-19 statistics increase, highlighting the costs of information manipulation at the regional level. However, neither institutional quality nor the strength of civil society appears to influence citizens' decisions to stay home. This suggests that the findings in Tables 1 and 2 are not driven by differences in citizen compliance with anti-COVID measures.

4.1.2. Functional form misspecification and measurement error

The decision of regional elites to report COVID-related deaths might depend not only on the level of impact of COVID but also on the speed of accumulation of its casualties. To capture this effect, we estimate the following model:

$$\text{Official deaths}_{it} = \alpha + \beta_1 \text{explanatory}_i \times \text{excess deaths}_{it} + \beta_2 \text{excess deaths}_{it} + \beta_3 (\text{excess deaths})^2_{it} + r_i + m_t + e_{it} \quad (2)$$

Table OA-9 presents the results and shows that, while regions report more COVID-related deaths if the pandemic toll grows faster, the main result is that regions with better institutions report more COVID-related deaths at the same level of pandemic deaths. Figure OA-5 presents the marginal effects of excess deaths on official deaths at low (0), average (mean) and high (1) levels of institutional quality.

Similarly, we modify the PPML model of Article 6.3 prosecutions to account for the quadratic excess deaths. The results do not change, compared to the main specification and are reported in Table OA-10. The marginal effects of excess deaths on Article 6.3 prosecutions at low (0), average (mean), and high (1) levels of civil society strength are presented in Figure OA-6.

Finally, given the complexity of our measure of institutional quality, it is possible that our results are due to measurement error. Table OA-8 uses measures of regional political institutions from Yakovlev and Zhuravskaya (2013) and shows that the results of the regressions with alternative specifications are consistent with our main findings.

4.1.3. Alternative explanation: career concerns

Kofanov et al. (2023) found that the need to hold a referendum in July 2020 which allowed Vladimir Putin to run for office again put pressure on those governors who had to run for election themselves soon. As the gubernatorial elections in Russia are, by design, critically dependent on the federal center's formal and informal support for the candidate, manipulation of COVID-19 reports is explained by career concerns of local elites. In Tables A-7, OA-3, A-8, and OA-11, we demonstrate that the inclusion of variables that capture career concerns from Kofanov et al. (2023) does not alter our findings. The complementarity between information manipulation and political repression remains.

But is misinformation helpful for regional elites in alleviating the risk of protests? Our data window covers two significant events for the Russian opposition orthogonal to pandemic: protests related to Navalny's poisoning in August 2020 and his return to Russia in January 2021. The months of mobilization campaigns are captured by the dummy variable I which is equal to 1 if there is a mobilization campaign related to Navalny's poisoning or arrest, 0 otherwise. We also allow for the differential effect of mobilization based on institutions, civil society, and pandemic pressure. Table A.9 explores the differential effect of pandemic strength on official reporting on the different quality of institutions and civil society in the presence of mobilization. The inclusion of a dummy for the protest campaign suggests that during those periods regions reported COVID-related deaths more truthfully.

Table A.10 suggests that, on average, misinformation reduces the number of protests in regions that experienced protests. At the same time, during protest mobilization periods, misinformation can backfire and trigger more protests in regions with better institutions and civil society. In the absence of a mobilization campaign, institutions and civil society do not modify the effect of misinformation on protests.

Finally, we checked whether interest in Navalny, measured by the intensity of Google searches, influenced the level of information manipulation and found the effect to be positive, but statistically insignificant (Table OA-5). At the same time, the number of Navalny-related Yandex searches is positively correlated with Article 6.3 prosecutions, accounting for region and month fixed effects (Table OA-7).

4.1.4. Sensitivity analysis

Although in Section 4.1.1 we included some important characteristics of the region that interact with the intensity of the pandemic, we might still have an omitted variable left out. Hence, we use sensitivity analysis to quantify how strong omitted variables would have to be in explaining both the treatment and the outcome to explain away our results.

There are multiple approaches to sensitivity analysis.[21] We choose the approach of Cinelli and Hazlett (2020) for the following reasons. First, it relaxes the strong assumptions required for most of these methods. Second, it provides readily available summary statistics that illustrate the sensitivity of the analysis to omitted variable bias – "the robustness value" – and parameterizes the overall robustness of a coefficient to unobserved confounding. If the association of confounders with treatment and outcome (measured in terms of partial R^2) is assumed to be less than the robustness value, then such confounders cannot "explain away" the observed effect. Furthermore, it allows us to explore the extreme scenario in which all unexplained variation in outcome (number of officially reported deaths or number of cases of article 6.3) is attributed to the confounding variable. In such cases, we quantify the level of variation in (interaction of excess COVID-19-related deaths with institutions or with the strength of civil society, respectively) that must be observed to completely bias the results to zero.

Our central empirical claims are about *heterogeneous responses* to pandemic intensity: we ask whether excess mortality translates into (i) higher officially recorded COVID-19 deaths and (ii) more political/legal repression, and whether these responses are systematically stronger or weaker in regions with different institutional constraints or civil society strength. In the regressions, these heterogeneous effects are captured by interaction terms such as $D_{it} = \text{ExcessDeaths}_{it} \times Z_i$ (where Z_i is institutions, civil society, or United Russia seat share).

Thus, the remaining concern is time-varying unobserved confounding. Importantly, because our "treatment" is an *interaction term*, an omitted factor would need to do more than correlate with excess deaths or with institutions separately. To explain away the interaction estimate, an unobserved variable must generate a *differential co-movement* of excess deaths with the outcome across regions with different Z_i—that is, it must effectively mimic a different *slope* of the excess-deaths–outcome relationship in high-Z versus low-Z regions.

Sensitivity analysis that separates two ingredients of confounding: *imbalance* and *impact* (Cinelli and Hazlett, 2020). In our setting, **imbalance** means that an unobserved factor U_{it} remains strongly correlated

[21] Some notable examples include Rosenbaum and Rubin (1983); Imbens (2003); Hosman et al. (2010); Imai et al. (2010); VanderWeele and Arah (2011); Dorie et al. (2016); Oster (2019). See Hazlett and Parente (2023) Appendix B for a review of approaches to sensitivity analysis.

with the residual variation in the heterogeneous-effect regressor D_{it} after controlling for fixed effects and covariates. Specifically, U_{it} would have to rise (or fall) disproportionately in high-institution regions *exactly when* excess deaths rise, so that it predicts the interaction ExcessDeaths × Z over and above the controls. **Impact** means that the same unobserved factor also strongly predicts the outcome (official COVID-19 deaths or prosecutions) conditional on the controls *and* the heterogeneous-effect regressor. In substantive terms, U_{it} would need to both (i) track pandemic pressure in a way that is systematically different across institutional environments and (ii) directly shift reporting practices or enforcement intensity.

For the OLS reporting specifications, we apply the Cinelli–Hazlett framework and report *robustness values* (Table A-6). The robustness value for the key heterogeneity coefficient (e.g., ExcessDeaths × Institutions) is 30.9% (26.3% for losing statistical significance at the 5% level), implying that an omitted confounder would need to explain on the order of one-third of the residual variation in *both* the interaction term and official COVID-19 deaths to fully eliminate the estimated heterogeneity. This threshold is large relative to the confounding strength implied by benchmark observed interactions (e.g., COVID intensity interacted with population density or political dominance), suggesting that moderate unobserved confounding is unlikely to account for the estimated moderation by institutions.

For PPML prosecution models, we report *E-values* (VanderWeele and Ding, 2017), which translate the estimated multiplicative effect into the minimum strength of association (on the risk–ratio scale) that an unmeasured confounder would need to have with both the heterogeneous-effect regressor and prosecutions, conditional on controls, to explain away the estimate. In our application, the E-value for the focal heterogeneity term is 1.002, meaning that confounding would need to induce risk–ratio associations of at least 1.002 with *both* the heterogeneous-effect regressor and the repression outcome to eliminate the point estimate.[22]

5. Conclusion

In this paper, we focus on an environment in which an authoritarian government uses a public health crisis to tighten its grip over the society. Using the fact that Russian regions had full authority over COVID-related policies, we found that the quality of political institutions and the strength of civil society influenced the extent to which the government engaged in information manipulation and repression. In addition, we found evidence consistent with the complementarity prediction of the model: conditional on pandemic intensity and regional characteristics, regions that relied more heavily on repression also engaged in greater information manipulation. Our findings are consistent with cross-country findings that democratic backsliding related to the COVID-19 pandemic has been more prominent in countries with weaker institutions.

[22] The confidence interval includes the null, so the E-value for the confidence limit closest to the null is 1.00. Full technical details and isoquant plots are in the Appendix OA1.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A

See Tables A.1–A.10.

Table A.1
Data and sources.

Variable	Description	Source
UR share	The share of seats held by *United Russia* in regional parliaments (region-month)	Websites of regional parliaments
Population in 2019	Population on Jan 1, 2019 (1000 people)	Russian Bureau of Statistics (Rosstat): https://rosstat.gov.ru
Distance to Moscow	Distance from regional capital to Moscow, sq km	ICSID database on economic and political indicators for the Russian regions: https://iims.hse.ru/data/2016/01/11/1134755487/ICSID%20economic%20database%20Codebook%20v1.2.1en.pdf
Auto road density	Paved automobile roads density, per 1000 m	
Railroad density	Railroads density per 10,000 sq km	
Busses	Number of busses per 100,000 people	
Area, 100 sq km	Geographic area of the region	
Civil society	Share of population employed by NGOs from economically active population in 2013	*Nonprofit and Voluntary Sector Quarterly*. 2020, Vol. 49(5) 1058–1081. *Subnational Variations in Civil Society Development: The Surprising Case of Russia*. L.M. Salamon, Y. Skokova, and I. Krasnopolskaya
Institutions	Quality of institutions in 2017, as a measure	A.V. Kynev. 2017."Kachestvo regionalnyh politicheskih institutov: popytka izmerenija" (Attempt at measuring the quality of the regional political institutions)."*Politicheskaja nauka*"(4):259–283
QR code duration	Number of days citizens were required to obtain an electronic pass to leave home	http://www.consultant.ru/law/podborki/theme-koronavirus/
State of emergency	1 if SoE was ever declared during the pandemic, 0 otherwise	
Meetings banned	1 if public gatherings were ever banned during the pandemic, 0 otherwise	OVD-info: https://ovdinfo.org/
Isolation index	1–10 index of self-isolation (higher means more self-isolation)	*Yandex* index of self-isolation: https://datalens.yandex/7o7is1q6ikh23
COVID-attributed deaths	Number of deaths caused by COVID-19	Russian Bureau of Statistics (Rosstat). Available through Wayback Machine: https://rosstat.gov.ru/storage/mediabank/edn06-2021.htm
Excess deaths	Excess mortality is computed relative to the baseline obtained using linear extrapolation of the 2015–2019 trend.	Excess Deaths: COVID-attributed deaths; authors' calculations.
Article 6.3 cases & OVD-info cases	Number of cases opened under Article 6.3 & Political Cases	OVD-info: https://ovdinfo.org/

Notes: Table provides data descriptions and sources for analysis variables.

Table A.2
Summary Statistics: Panel A - Cross-Sectional Variables.

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
UR share	80	0.72	0.15	0.06	0.67	0.74	0.81	0.94
Population in 2019	80	1770.50	1841.18	49.70	745.95	1183.95	2349.02	12,615.30
Distance to Moscow	80	2359.31	2723.96	0	633	1432	2606	11,876
Auto road density	80	169.82	156.61	0.90	39.25	142.50	234.25	672.00
Railroad density	75	166.84	119.87	2	85.5	158	218.5	577
Buses per capita (1000)	80	41.06	24.67	2.08	22.41	36.26	53.34	130.88
Area, 100 sq km	80	191.68	386.80	1.40	33.38	73.10	165.52	3083.50
Civil society	80	0.77	0.38	0.00	0.54	0.69	0.95	2.17
Civil society (normalized)	80	0.36	0.17	0.00	0.25	0.32	0.44	1.00
Institutions	80	7.86	6.27	0.60	2.90	5.80	11.50	23.00
Institutions (normalized)	80	0.32	0.28	0.00	0.10	0.23	0.49	1.00
QR code duration	80	20.64	33.86	0	0	0	42.2	153
State of emergency	79	0.46	0.50	0	0	0	1	1
Meetings banned	80	0.85	0.36	0	1	1	1	1

Table A.3
Summary Statistics: Panel B - Time-Varying Variables.

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
OVD Cases (ratio)	984	0.0003	0.001	0.00	0.00	0.00	0.00	0.01
Art 632 (ratio)	984	0.04	0.06	0.00	0.01	0.02	0.05	0.68
misinformation	984	314.67	471.17	−893	42	158.5	411.2	4239
with_covid	984	259.09	530.55	0	25	102	270	5891
Excess Deaths	984	484.49	720.36	−864	77.8	251	594	6150
from_covid	984	169.81	407.70	0	13	52	164.2	5251
Isolation index	924	1.69	0.49	0.69	1.34	1.60	1.89	3.43

Table A.4
The number of deaths officially attributed to COVID-19.

	Dependent variable:				
	Official deaths from COVID-19				
	(1)	(2)	(3)	(4)	(5)
Excess deaths	33.774***	19.249***	24.646**	90.334***	18.725***
	(11.114)	(6.752)	(11.621)	(34.526)	(5.346)
Excess deaths × Population density		0.013***			0.013***
		(0.002)			(0.001)
Excess deaths × institutions			34.949**		35.516***
			(13.963)		(9.370)
Excess deaths ×UR share				−81.066**	−11.568*
				(41.129)	(6.104)
Month FE	+	+	+	+	+
Region FE	+	+	+	+	+
Observations	984	984	984	984	984
R^2	0.817	0.890	0.830	0.850	0.905
Adjusted R^2	0.798	0.879	0.812	0.835	0.895
RMSE	202.45	145.32	190.68	175.12	135.67

Notes: Table presents OLS regression estimates. *Official deaths* is the number of deaths in region *i* in month *t* officially attributed to the COVID-19 pandemic. *Excess deaths* is the difference between the expected number of deaths over a five-year period for a given region in a given month and the actual number of deaths reported in official statistics. *institutions* is a composite score of political institutions from Kynev (2017). *UR share* is the share of seats held by *United Russia* in regional parliaments at the region-month level. Standard errors are two-way clustered at the region-month level and reported in parenthesis. Stars denote p-values at conventional levels: *p<0.1, **p<0.05, ***p<0.01.

Table A.5
Article 6.3 cases.

	Dependent variable:			
	Article 6.3 prosecutions			
	(1)	(2)	(3)	(4)
Excess deaths	−0.033*	−0.008	0.001	0.005
	(0.013)	(0.009)	(0.030)	(0.025)
Excess deaths × Population density	0.000			0.000
	(0.000)			(0.000)
Excess deaths × civil		−0.046**		−0.044***
		(0.018)		(0.007)
Excess deaths ×UR share			−0.042	−0.020
			(0.046)	(0.034)
Month FE	+	+	+	+
Region FE	+	+	+	+
Observations	960	960	960	960

Notes: Table presents PPML regression estimates. *Article 6.3 prosecutions* is the number of cases initiated under Article 6.3 of the Administrative Code of Russia per population. *Excess deaths* is the difference between the expected number of deaths over a five-year period for a given region in a given month and the actual number of deaths reported in official statistics. *civil* measures the share of the population employed by NGOs from the economically active population in 2013. *UR share* is the share of seats held by *United Russia* in regional parliaments at the region-month level. Standard errors are two-way clustered at the region-month level and reported in parenthesis. Stars denote p-values at conventional levels: *p<0.1, **p<0.05, ***p<0.01.

Table A.6
Citizen behavior and institutions.

	Dependent Variable:				
	Isolation Index				
	(1)	(2)	(3)	(4)	(5)
Panel A: Official deaths					
Official deaths	0.0002*	0.0002*	0.0002	0.001***	0.001*
	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0003)
(Official deaths)^2	−0.00000*	−0.00000	−0.00000	−0.00000**	−0.00000**
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Official deaths × civil		0.00004			−0.00000
		(0.0001)			(0.0001)
Official deaths × institutions			0.000		0.00000
			(0.00001)		(0.00001)
Official deaths ×UR share				−0.0004**	−0.0004*
				(0.0002)	(0.0002)
Constant	2.799***	2.805***	2.799***	2.823***	2.824***
	(0.043)	(0.034)	(0.046)	(0.048)	(0.049)
Observations	920	920	920	920	920
R^2	0.917	0.917	0.917	0.918	0.918
Adjusted R^2	0.909	0.909	0.909	0.909	0.909
Panel B: Excess deaths					
Excess deaths	0.0001	0.0001**	0.0001	0.0001	
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	
(Excess deaths)^2	−0.000	−0.000	−0.000	−0.000	
	(0.000)	(0.000)	(0.000)	(0.000)	
Excess deaths × civil		−0.00004		−0.00003	
		(0.0001)		(0.0001)	
Excess deaths × institutions			0.00000	0.00000	
			(0.00000)	(0.00000)	
Constant	2.813***	2.799***	2.805***	2.795***	
	(0.038)	(0.027)	(0.032)	(0.027)	
Observations	920	920	920	920	
R^2	0.917	0.917	0.918	0.918	
Adjusted R^2	0.909	0.909	0.909	0.909	

Notes: Table presents OLS regression estimates where the dependent variable is the *Yandex* self-isolation index, which ranges from 1 to 10 (higher means more self-isolation). *Official deaths* is the number of deaths in region *i* in month *t* officially attributed to the COVID-19 pandemic. *Excess deaths* is the difference between the expected number of deaths over a five-year period for a given region in a given month and the actual number of deaths reported in official statistics. *civil* measures the share of the population employed by NGOs from the economically active population in 2013. *institutions* is a composite score of political institutions from Kynev (2017). *UR share* is the share of seats held by *United Russia* in regional parliaments at the region-month level. Standard errors are two-way clustered at the region-month level and reported in parenthesis. Stars denote p-values at conventional levels: *p<0.1, **p<0.05, ***p<0.01.

Table A.7
Information manipulation accounting for the number of months until regional elections.

	Dependent variable:				
	Official deaths from COVID-19				
	(1)	(2)	(3)	(4)	(5)
Excess deaths	0.452***	0.351***	0.204	0.251**	0.011
	(0.096)	(0.111)	(0.183)	(0.122)	(0.162)
Election proximity	−0.545	−0.771	−0.793	−0.897	−0.961
	(0.920)	(0.565)	(0.608)	(0.596)	(0.760)
Excess deaths × civil			0.135		0.195
			(0.199)		(0.181)
Excess deaths × institutions				0.015**	0.019***
				(0.006)	(0.007)
Constant	−36.591	53.732	119.198	27.743	115.704
	(45.515)	(74.252)	(82.212)	(70.030)	(71.858)
Month FE	–	+	+	+	+
Region FE	–	+	+	+	+
Observations	980	980	980	980	980
R^2	0.627	0.820	0.826	0.832	0.844
Adjusted R^2	0.626	0.802	0.808	0.814	0.827
F Statistic	819.886	44.049	45.350	47.177	50.822

Notes: Table presents OLS regression estimates of the model in Eq. 3.5, with an additional explanatory variable *Election proximity*, the number of months until the next election. *Official deaths* is the number of deaths in region i in month t officially attributed to the COVID-19 pandemic. *Excess deaths* is the difference between the expected number of deaths over a five-year period for a given region in a given month and the actual number of deaths reported in official statistics. *civil* measures the share of the population employed by NGOs from the economically active population in 2013. *institutions* is a composite score of political institutions from Kynev (2017). Standard errors are two-way clustered at the region-month level and reported in parenthesis. Stars denote p-values at conventional levels: *p<0.1, **p<0.05, ***p<0.01.

Table A.8
Information manipulation, accounting for the referendum and upcoming elections.

	Dependent variable:			
	Official deaths from COVID-19			
	(1)	(2)	(3)	(4)
Excess deaths	0.351***	0.204	0.251**	0.010
	(0.112)	(0.185)	(0.123)	(0.166)
Excess deaths × civil		0.135		0.197
		(0.201)		(0.184)
Excess deaths × institutions			0.015**	0.019***
		(0.006)	(0.007)	
Election proximity × pre-referendum	−0.546	−0.775	−0.884	−1.307
	(1.538)	(1.860)	(1.460)	(1.851)
Constant	56.146	126.929	36.230	133.698
	(56.096)	(110.568)	(49.878)	(99.504)
Observations	980	980	980	980
R^2	0.820	0.826	0.832	0.844
Adjusted R^2	0.801	0.808	0.814	0.827
F Statistic	43.960	45.272	47.073	50.763

Notes: Table presents OLS regression estimates similar to the ones in Table 1, with additional explanatory variables *Election proximity*, the number of months until the next election, and *pre-referendum*, an indicator for the Referendum on Vladimir Putin's eligibility to run for re-election, despite constitutional constraints. *Official deaths* is the number of deaths in region i in month t officially attributed to the COVID-19 pandemic. *Excess deaths* is the difference between the expected number of deaths over a five-year period for a given region in a given month and the actual number of deaths reported in official statistics. *civil* measures the share of the population employed by NGOs from the economically active population in 2013. *institutions* is a composite score of political institutions from Kynev (2017). Standard errors are two-way clustered at the region-month level and reported in parentheses. Stars denote p-values at conventional levels: *p<0.1, **p<0.05, ***p<0.01.

Table A.9
Protest mobilization and official reporting.

	Dependent Variable:			
	Official deaths from COVID-19			
	(1)	(2)	(3)	(4)
Excess deaths	0.351***	0.201	0.012	0.551**
	(0.112)	(0.185)	(0.163)	(0.277)
Navalny	79.488***	31.516	84.620***	33.529**
	(12.631)	(23.407)	(5.389)	(14.765)
Navalny × civil		70.220**		71.044*
		(35.415)		(42.456)
Navalny × institutions			0.727	0.966
			(1.287)	(1.329)
Excess deaths × institutions			0.019***	0.017***
			(0.007)	(0.006)
Excess deaths × civil		0.138	0.195	0.164
		(0.201)	(0.182)	(0.116)
Excess deaths × UR share				−0.716**
				(0.301)
Constant	43.664	114.601	102.933	184.231**
	(80.432)	(86.936)	(73.646)	(90.430)
Observations	980	980	980	980
R^2	0.820	0.827	0.843	0.868
Adjusted R^2	0.802	0.809	0.826	0.854

Notes: Table presents OLS regression estimates. *Official deaths* is the number of deaths in region i in month t officially attributed to the COVID-19 pandemic. *Excess deaths* is the difference between the expected number of deaths over a five-year period for a given region in a given month and the actual number of deaths reported in official statistics. *Navalny* is a dummy variable that equals 1 if there is a mobilization campaign related to Navalny poisoning or arrest, 0 otherwise. *civil* measures the share of the population employed by NGOs from the economically active population in 2013. *institutions* is a composite score of political institutions from Kynev (2017). *UR share* is the share of seats held by *United Russia* in regional parliaments at the region-month level. Standard errors are two-way clustered at the region-month level and reported in parenthesis. Stars denote p-values at conventional levels: *p<0.1, **p<0.05, ***p<0.01.

Table A.10
Information manipulation and protests.

	Dependent Variable:		
	Number of protests		
	(1)	(2)	(3)
Misinformation	−0.002	−0.001**	−0.001**
	(0.001)	(0.0002)	(0.0003)
Navalny	1.866***	1.617***	1.766***
	(0.290)	(0.286)	(0.204)
Misinformation × civil	0.001		
	(0.001)		
Misinformation × institutions		−0.00004	
		(0.0001)	
Misinformation × Navalny	−0.001	0.003**	−0.002
	(0.002)	(0.001)	(0.002)
Misinformation × Navalny × civil	0.003		0.003**
	(0.002)		(0.002)
Misinformation × Navalny × institutions		0.0001	0.0002*
		(0.0001)	(0.0001)
Constant	−0.461	−0.867	−0.733
	(0.592)	(0.636)	(0.757)
Observations	473	473	473
R^2	0.673	0.670	0.673
Adjusted R^2	0.592	0.588	0.592

Notes: Table presents OLS regression estimates. *Number of protests* is the number of COVID-related protests in regions that experienced protests. Regions without protests are excluded from the sample. *Misinformation* is defined as the difference between excess deaths (detailed in Section 3) and officially reported COVID-related deaths. *Navalny* is a dummy variable that equals 1 if there is a mobilization campaign related to Navalny's poisoning or arrest, 0 otherwise. *civil* measures the share of the population employed by NGOs from the economically active population in 2013. *institutions* is a composite score of political institutions from Kynev (2017). Stars denote p-values at conventional levels: *p<0.1, **p<0.05, ***p<0.01.

Appendix B. Supplementary data

Supplementary data for this article can be found online at doi:10.1016/j.jpubeco.2026.105740.

Data availability

Data will be made available upon request.

References

Adiguzel, F.S., Cansunar, A., Corekcioglu, G., et al., 2020. Truth or dare? Detecting systematic manipulation of COVID-19 statistics. J. polit. inst. polit. econ. 1 (4), 543–557.

Ahinkorah, B.O., Ameyaw, E.K., Hagan Jr, J., Seidu, A.-A., Schack, T., 2020. Rising above misinformation or fake news in Africa: another strategy to control COVID-19 spread. Front. Commun. 5, 1–4.

Balli, H.O., Sørensen, B.E., 2013. Interaction effects in econometrics. Empir. Econ. 45, 583–603.

Besley, T., Dray, S., 2023. The political economy of lockdown: does free media matter? Eur. J. Polit. Econ. 78, 102361.

Boleslavsky, R., Shadmehr, M., Sonin, K., 2021. Media freedom in the shadow of a coup. J. Eur. Econ. Assoc. 19 (3), 1782–1815.

Borodikhin, A., 2021. Korona dokazatel'stv. Kak ustroeno "sanitarnoe delo" (Corona of evidence. How "sanitary case" works). mimeo.

Buckley, N., Marquardt, K.L., Reuter, O.J., Tertytchnaya, K., 2022. Endogenous popularity: how perceptions of support affect the popularity of authoritarian regimes. Am. Polit. Sci. Rev. 1–7.

Busygina, I., Klimovich, S., 2024. Pandemic decentralization: COVID-19 and principal–agent relations in Russia. Probl. Post-Communism 71 (1), 1–12.

Cao, J., Xu, Y., Zhang, C., 2022. Clans and calamity: how social capital saved lives during China's great famine. J. Dev. Econ. 157, 102865.

Cinelli, C., Hazlett, C., 2020. Making sense of sensitivity: extending omitted variable bias. J. R. Stat. Soc. B (stat. Methodol.) 82 (1), 39–67.

Correia, S., Guimarães, P., Zylkin, T., 2020. Fast poisson estimation with high-dimensional fixed effects. Stata J. 20 (1), 95–115.

Dorie, V., Harada, M., Carnegie, N.B., Hill, J., 2016. A flexible, interpretable framework for assessing sensitivity to unmeasured confounding. Stat. Med. 35 (20), 3453–3470.

Dragu, T., Przeworski, A., 2019. Preventive repression: two types of moral hazard. Am. Polit. Sci. Rev. 113 (1), 77–87.

Egorov, G., Enikolopov, R., Makarin, A., Petrova, M., 2021. Divided we stay home: social distancing ethnic diversity. J. Public Econ. 194, 104328.

Fong, B.CH., 2021. Exporting autocracy: how China's extra-jurisdictional autocratic influence caused democratic backsliding in Hong Kong. Democratization 28 (1), 198–218.

Frye, T., 2022. Weak Strongman: The Limits of Power in Putin's Russia. Princeton University Press.

Frye, T., Gehlbach, S., Marquardt, K.L., Reuter, O.J., 2017. Is putin's popularity real? Post-sov. aff. 33 (1), 1–15.

Gehlbach, S., Sonin, K., 2014. Government control of the media. J. Public Econ. 118, 163–171.

Gourieroux, C., Monfort, A., Trognon, A., 1984. Pseudo maximum likelihood methods: applications to poisson models. Econometrica 52 (3), 701–720.

Guasti, P., 2020. The impact of the COVID-19 pandemic in central and Eastern Europe: the rise of autocracy and democratic resilience. Democr. Theory 7 (2), 47–60.

Guriev, S., Treisman, D., 2019. Informational autocrats. J. Econ. Perspect. 33 (4), 100–127.

Guriev, S., Treisman, D., 2022. Spin Dictators: The Changing Face of Tyranny in the 21st Century. Princeton University Press.

Hale, H.E., 2004. The Origins of United Russia and the Putin Presidency: The Role of Contingency in Party-System Development. Demokratizatsiya-Washington- 2, pp. 169–194 12.

Hale, H.E., 2022. Authoritarian rallying as reputational cascade? Evidence from putin's popularity surge after Crimea. Am. Polit. Sci. Rev. 116 (2), 580–594.

Hazlett, C., Parente, F., 2023. From "is it unconfounded?" To "how much confounding would it take?": Applying the sensitivity-based approach to assess causes of support for peace in Colombia. J. Polit. 85 (3), 1145–1150.

Hosman, C.A., Hansen, B.B., Holland, P.W., et al., 2010. The sensitivity of linear regression coefficients' confidence limits to the omission of a confounder. Ann. Appl. Stat. 4 (2), 849–870.

Huang, H., 2018. The pathology of hard propaganda. J. Polit. 80 (3), 1034–1038.

Imai, K., Keele, L., Yamamoto, T., 2010. Identification, inference and sensitivity analysis for causal mediation effects. Stat. Sci. 25 (1), 51–71.

Imbens, G.W., 2003. Sensitivity to exogeneity assumptions in program evaluation. Am. Econ. Rev. 93 (2), 126–132.

Islam, N., Shkolnikov, V.M., Acosta, R.J., Klimkin, I., Kawachi, I., Irizarry, R.A., Alicandro, G., Khunti, K., Yates, T., Jdanov, D.A., et al., 2021. Excess deaths associated with COVID-19 pandemic in 2020: age and sex disaggregated time series analysis in 29 high income countries. Br. Med. J. 373.

Kamenica, E., Gentzkow, M., 2011. Bayesian persuasion. Am. Econ. Rev. 101 (6), 2590–2615.

Karlinsky, A., Kobak, D., 2021. Tracking excess mortality across countries during the COVID-19 pandemic with the world mortality dataset. eLife 10, e69336.

Kavaklı, K.C., 2020. Populist governments and democratic backsliding during the COVID-19 pandemic. mimeo.

Kilani, A., 2021. Authoritarian regimes' propensity to manipulate COVID-19 data: a statistical analysis using Benford's law. Commonw. Comp. Polit. 59 (3), 319–333.

Knutsen, C.H., Kolvani, P., 2022. Fighting the disease or manipulating the data? Democracy, state capacity, and the COVID-19 pandemic. Democracy, state capacity, and the COVID-19 pandemic (January 2022). V-Dem Working Paper 127.

Kobak, D., 2021. Excess mortality reveals covid's true toll in Russia. Significance 18 (1), 16–19.

Kofanov, D., Kozlov, V., Libman, A., Zakharov, N., 2023. Encouraged to cheat? Federal incentives and career concerns at the sub-national level as determinants of under-reporting of COVID-19 mortality in Russia. Br. J. Polit. Sci. 53 (3), 835–860.

Kynev, A.V., 2017. Kachestvo regional'nyh politicheskih institutov: popytka izmerenija (attempt at measuring the quality of the regional political institutions). Polit. nauka (4), 259–283.

Lifshits, M.L.N.N.P., 2020. COVID-19 mortality rate in Russian regions: forecasts and reality. R-Econ. 6 (3), 171–182.

Lorentzen, P.L., 2013. Regularizing rioting: permitting public protest in an authoritarian regime. Q. J. Polit. Sci. 8 (2), 127–158.

Lorentzen, P., 2014. China's strategic censorship. Am. J. Polit. Sci. 58 (2), 402–414.

Luhrmann, A., Lachapelle, J., Grahn, S., Edgell, A.B., 2020. Pandemic backsliding: democracy and disinformation seven months into the COVID-19 pandemic. V-Inst. (25).

Montagnes, P., Wolton, S., 2019. Mass purges: top-down accountability in autocracy. Am. Polit. Sci. Rev. 113 (4), 1045–1059.

Nusratullin, I., Drozdov, I., Ermakov, A., Koksharova, E., Mashchenko, E., 2021. Natural population movement and COVID-19: data from Russia. Cuest. Polit. 39 (71), 986–1007.

Olson Jr, M., 1971. The Logic of Collective Action: Public Goods and the Theory of Groups, with a New Preface and Appendix, vol. 124. Harvard University Press.

Oster, E., 2019. Unobservable selection and coefficient stability: theory and evidence. J. Bus. Econ. Stat. 37 (2), 187–204.

Ostrom, E., 1990. Governing the Commons: The Evolution of Institutions for Collective Action. Cambridge University Press.

OVD-Info, 2023. Metodologiya bazy politicheski motivirovannykh ugolovnykh presledovaniy. https://data.ovd.info/metodologiya-bazy-politicheski-motivirovannykh-ugolovnykh-presledovaniy-0 (Accessed: 5 December 2025).

Pomeranz, J.L., Schwid, A.R., 2021. Governmental actions to address COVID-19 misinformation. J. Public Health Policy 42 (2), 201–210.

Repucci, S., Slipowitz, A., 2020. Democracy Under Lockdown: The Impact of COVID-19 on the Global Struggle for Freedom. Freedom House.

Reuter, O.J., Szakonyi, D., 2019. Elite defection under autocracy: evidence from Russia. Am. Polit. Sci. Rev. 113 (2), 552–568.

Ricard, J., Medeiros, J., 2020. Using misinformation as a political weapon: COVID-19 and bolsonaro in Brazil. Harv. Kennedy Sch. Misinformation Rev. 1 (2).

Roozenbeek, J., Schneider, C.R., Dryhurst, S., Kerr, J., Freeman, A.LJ., Recchia, G., Van Der Bles, A.M., Van Der Linden, S., 2020. Susceptibility to misinformation about COVID-19 around the world. R. Soc. Open Sci. 7 (10), 201199.

Rosenbaum, P.R., Rubin, D.B., 1983. Assessing sensitivity to an unobserved binary covariate in an observational study with binary outcome. J. R. Stat. Soc.: B (Methodol.) 45 (2), 212–218.

ROSSTAT Russian Bureau of Statistics, 2021. Mortality by region. data retrieved from World Development Indicators, rosstat.gov.ru.

Rozenas, A., 2020. A theory of demographically targeted repression. J. Confl. Resolut. 64 (7–8), 1254–1278.

Rozenas, A., Stukal, D., 2019. How autocrats manipulate economic news: evidence from Russia's state-controlled television. J. Polit. 81 (3), 982–996.

Salamon, L.M., Skokova, Y., Krasnopolskaya, I., 2020. Subnational variations in civil society development: the surprising case of Russia. Nonprofit Volunt. Sect. Q. 49 (5), 1058–1081.

Santos Silva, J.M.C., Tenreyro, S., 2006. The log of gravity. Rev. Econ. Stat. 88 (4), 641–658.

Santos Silva, J.M.C., Tenreyro, S., 2011. Further simulation evidence on the performance of the poisson pseudo-maximum likelihood estimator. Econ. Lett. 112 (2), 220–222.

Shadmehr, M., Bernhardt, D., 2015. State censorship. Am. Econ. J. Microecon. 7 (2), 280–307.

Smirnova, N., Chertova, A., Shedov, D., Polikhovich, A., 2020. Freedom of assembly in Russia during the pandemic: summary. OVD-info.

Sobolev, A., Zakharov, A., 2018. Civic and political activism in Russia. In: Daniel, T. (Ed.), The New Autocracy: Information, Politics, and Policy in Putin's Russia. Brookings Institution Press, pp. 249–276.

Stasavage, D., 2020. Democracy, autocracy, and emergency threats: lessons for COVID-19 from the last thousand years. Int. Organ. 74 (S1), E1–E17.

Svolik, M.W., 2012. The Politics of Authoritarian Rule. Cambridge University Press.

Taylor, B.D., 2011. State Building in Putin's Russia: Policing and Coercion After Communism. Cambridge University Press.

Treisman, D., 2011. Presidential popularity in a hybrid regime: Russia under yeltsin and putin. Am. J. Polit. Sci. 55 (3), 590–609.

Tyson, S.A., 2018. The agency problem underlying repression. J. Polit. 80 (4), 1297–1310.

VanderWeele, T.J., Arah, O.A., 2011. Bias formulas for sensitivity analysis of unmeasured confounding for general outcomes, treatments, and confounders. Epidemiology 22 (1), 42–52.

VanderWeele, T.J., Ding, P., 2017. Sensitivity analysis in observational research: introducing the E-value. Ann. Intern. Med. 167 (4), 268–274.

Xu, Y., Zhao, A., Ding, P., 2024. Factorial difference-in-differences. arXiv preprint arXiv:2407.11937.

Yakovlev, E., Zhuravskaya, E., 2013. The unequal enforcement of liberalization: evidence from Russia's reform of business regulation. J. Eur. Econ. Assoc. 11 (4), 808–838.

Zemtsov, S., Baburin, V., 2020. Coronavirus in the regions of Russia: features and consequences of the spread. Public Serv. 22 (2), 428–434.