Accountability despite misinformation *

Philipp Kerler† Victor Araújo‡

May 16, 2024

Abstract

Misinformation jeopardizes democratic accountability. Can voters punish incompetent rulers despite widespread misinformation? We address this question through the lens of healthcare access during the Covid-19 pandemic in Brazil. Despite a massive misinformation campaign in an already polarized society, Bolsonaro lost the elections in 2022. We argue that real-world information that contradicts misleading narratives counteracts electoral biases rooted in misinformation. We show, that voters in electoral districts closer to larger intensive care units (ICUs), which function as salient markers of the true state of the world, punished Bolsonaro more in the 2022 Presidential Election. The effect is sizeable, an inter-quartile range decrease in ICU presence could have flipped the election result.

Keywords: Retrospective voting, accountability, information processing, misinformation, Covid-19, Brazil

*For helpful comments we thank Kiran Auerbach, Paola Galano, Katharina Michaelowa, Reto Mitteregger, Christian Ochsner, participants at the meeting of the Political Economy and Development Group and pre-publication seminar at the Department of Political Science at the University of Zürich, and participants in the Zürich Political Economy Seminar Series

†Corresponding author. University of Zürich (UZH), Department of Political Science and Center for Comparative and International Studies (CIS) at ETH Zürich and UZH, philipp.kerler@pw.uzh.ch

‡School of Politics, Economics, and International Relations at the University of Reading, v.araujosilva@reading.ac.uk

Introduction

Misinformation as a political strategy is widely used, and often, associated with populist politics. Misinformation threatens the democratic principle of accountability. When collective preferences are based on wrong facts, voters may not recognize a politician's (objectively) poor performance. Consequently, the electorate may choose a leader they would not have chosen if they were correctly informed. Overcoming misinformation is difficult (Jerit and Zhao, 2020). Kuklinski et al. (2000, p. 810) remark "Unless they [people] are "hit between the eyes" with the right facts, they continue to judge policy based on their mistaken beliefs. In fact, likely, even those "hit between the eyes" with facts will eventually return to their original beliefs and preferences." How does it stand about accountability, when elites deliberately spread misinformation to abscond from retrospective punishment at the ballot? In this paper, we study whether salient cues about the true state of the world "hit hard enough" to induce electoral accountability. We investigate this question through the case of the Brazilian presidential election in the face of the Covid-19 pandemic.

Theories of retrospective voting suggest citizens hold leaders accountable for bad performance during a pandemic (Ferejohn, 1986). In Britain, Boris Johnson and the Conservative Party lost major support due to voters' dissatisfaction with the pandemic management (Green et al., 2020). In the US, Republican voters were critical of Trump's Covid-politics. However, stark polarization seems to have inhibited a majority of Republican voters from voting against Trump in 2020, despite disagreement over Covid-19 policies (Mehlhaff et al., 2023). The Brazilian electorate is highly polarized, too, which raises doubts about whether voters would punish Bolsonaro even if they blamed Bolsonaro for mismanaging the pandemic.[1] Moreover, selective information-seeking and

[1] The Brazilian president at the time, Jair Bolsonaro, had assumed an anti-scientific position, promoting ill-advised medical treatments and neglecting the severity of Covid-19 (Taylor et al., 2021; Fonseca et al., 2021).

directional motives, supported by a powerful misinformation campaign (Batista Pereira and Nunes, 2022; de Oliveira and Veronese, 2023), may obstruct the processing of political information (Healy and Malhotra, 2013; Alt et al., 2016; Weitz-Shapiro and Winters, 2017; Allcott et al., 2020; Graham and Singh, 2023) and jeopardize accountability.

We argue that real-world information, i.e., direct observations or information transmitted through interpersonal social networks that counteract political misinformation, can remove some of the voters' biases and sway some of them to punish poorly performing incumbents. Crucially, we believe that direct observation or information about life experiences from trusted individuals is persuasive compared to information received through media because there is either no intermediate source or the source is credible. Certainly, not all support for Bolsonaro is rooted in misinformation. However, debunking false information spread by Bolsonaro should at least sway some voters, either those on the margin or those whose support was indeed rooted in false information. In our empirical setup, intensive care units (ICUs) act as salient markers of the true state of the world. Stories about overworked medical personnel, over-crowded waiting rooms (even for non-Covid patients), and the sheer number of people who could not be saved originate from ICUs. The difference between what we refer to as real-world information and information distributed otherwise is that there is a priori no "sender" involved

The presence of intensive care units (ICUs) can affect retrospective voting through two mechanisms. First, ICUs can have a positive impact on local pandemic outcomes. Second, ICUs disseminate information about the true state of the world. People closer to ICUs are more likely to receive information, countering the false narrative of the Bolsonaro administration.

If ICUs impact voting through better local pandemic outcomes (first mechanism), either a positive effect or no effect of ICU presence on Bolsonaro's vote share is to be expected. The second mechanism yields the opposite prediction. Biases aligning with government propaganda are more likely to be refuted by real-world information, directly observed or received through interpersonal social networks, in the proximity to ICUs. Whether refuting biases is sufficient to sway a polarized electorate remains questionable as Mehlhaff et al. (2023) shows.

Our empirical results show that voters in locations with more ICUs punished Jair Bolsonaro more at the ballot. To investigate how the Brazilian electorate reacted to local variation in ICU presence, we employ the universe of electoral districts (secao, Engl. section) level results from Brazilian presidential elections 2002-2022, combined with comprehensive data on hospital locations and equipment. The election data accounts for more than 450,000 electoral districts nested in over 80,000 geo-coded voting locations. ICUs play a crucial role in treating Covid-19 patients in need of respiratory support. Hence, through the outbreak of the Covid-19 pandemic ICUs gained unforeseen additional importance. We leverage this unforeseen importance to partial-out the effect of pre-Covid-19 ICU presence on voters' decisions in the 2022 Brazilian presidential election. Using different fixed effects (TWFE) specifications (electoral district, voting location, and municipality-specific time trends) we can absorb much of the location-specific confounding variation between infection rates and mortality, and voting. Since parallel trends cannot be easily assumed in our setup, we resort to synthetic difference-in-difference (SDID) methods to obtain a more credible causal estimate (Arkhangelsky et al., 2021; Pailañir and Clarke, 2023). Since the results of conventional TWFE and SDID are similar and even stronger for SDID, we report both, as this leads us to believe that bias from non-parallel trends attenuates the results of TWFE, if anything.

Our research contributes to the larger literature that considers the role of citizens' information for retrospective voting and accountability, e.g., Gomez and Wilson (2001, 2006); Duch and Stevenson (2008); Healy and Malhotra (2009, 2013); Ashworth and

De Mesquita (2014); Ansolabehere et al. (2014); Alt et al. (2016); Weitz-Shapiro and Winters (2017). In that literature, we specifically contribute to recent work that engages with procedural intricacies of retrospective voting vis-à-vis polarization (Mehlhaff et al., 2023; Graham and Singh, 2023; Singh and Thachil, 2023). Like our work, this literature is heavily influenced by literature studying biases in political information processing (Kuklinski et al., 2000; Kahan, 2015; Berinsky, 2018; Bisgaard, 2019; Peterson and Iyengar, 2021; Allcott et al., 2020; Jerit and Zhao, 2020).

Theoretical framework

Existing empirical research paints a grim picture of citizens' behavioral response to Bolsonaro's misinformation campaign about Covid-19. Bolsonaro's widely disseminated claim that Covid-19 is not dangerous and can be treated with generic malaria drugs, for example, caused a sharp decline in social distancing (Ajzenman et al., 2020; Mariani et al., 2020). Such behavior seems to be particularly prevalent among Bolsonaro's supporters. Leone (2021) find that social distancing was lower in municipalities with higher vote shares for Bolsonaro in 2018. This is also reflected in a steeper death rate acceleration during outbreaks in municipalities with higher vote shares for Bolsonaro in 2018 (Rache et al., 2021). While those results represent ad hoc reactions, they raise doubt about whether or not voters were willing and able to recognize Bolsonaro's failures in managing the pandemic and consequently punished him at the polls. To understand democratic accountability in the face of the pandemic better, we employ the lens of healthcare access during the pandemic. The presence of ICUs established before the pandemic creates variation in pandemic exposure that became politically relevant during and after the pandemic. Determining how the presence of ICUs impacted voting in 2022 requires a thorough examination of the electoral composition vis-à-vis the process of retrospective voting. Therefore, we first theoretically analyze the process of retrospective voting on the individual level.

The process of retrospective voting

Healy and Malhotra (2013) describe retrospective voting as a four-step process. Step 1 is observation. The voter observes some real-world event, e.g., economic growth, stagnation, corruption, (un)employment, a natural disaster, or – as in this case – death tolls of a pandemic. In Step 2, the voter attributes responsibility for the observation to an actor. Step 3 is performance evaluation. Is the observed outcome better, worse, or on par with expectations? The voter then adjusts his or her voting intention accordingly. Step 4 constitutes the politician's strategic response to the incentives set by the behavior of voters. Thereby, a feedback loop is created, as the policy outcomes are then again observed by the voters. For our purpose, we are mainly concerned with steps 1 through 3. Steps 1 to 3 often largely overlap with what is often understood as *information processing* in the literature (e.g. Alt et al., 2016; Weitz-Shapiro and Winters, 2017).

We use a perfectly rational and perfectly *informed* voter as the benchmark. The informed voter only selects credible sources of information, and is, therefore, able to assess the state of the world accurately. Even when signals about the state of the world are noisy by nature, the informed voter can discern the signal from noise to a high degree. The informed voter attributes responsibilities correctly, and subsequently adjusts behavior accordingly. While this benchmark may be hardly ever matched in reality, different deviations from the benchmark at different steps in information processing warrant discussion. Not all deviations imply the same behavioral adjustments. Therefore, to assess accountability in the aggregate, a typology of deviations has to be established. The relative frequencies of types then determine the aggregate electoral response to events.

In the literature, we find different antidotes to our perfectly informed voter. We want to emphasize the importance of distinct roles of noisy information with random errors (e.g. Duch and Stevenson, 2008; Gomez and Wilson, 2001, 2006), versus biased information (e.g. Graham and Singh, 2023; Allcott et al., 2020).[2] Both observations and attributions can be either accurate, noisy, or biased. In the aggregate, however, noise cancels out, but biases remain present. Therefore, we focus on biases even though both are relevant on the individual level.

Myopic voting is another phenomenon that is often studied in the context of retrospective voting (Healy and Malhotra, 2009; Bechtel and Hainmueller, 2011; Aytaç, 2021). While myopia is typically understood as voters strongly discounting information in distant (past) times, the concept boils down to availability heuristics and can therefore easily be extended to spatial myopia. Voters placing more weight on spatially distant outcomes can have multiple reasons. Voters may simply care more about local outcomes than aggregate outcomes when they are more affected. The literature on the electoral effects of local trade shocks, for example, follows this line of thought (e.g., Colantone and Stanig, 2018; Autor et al., 2020; Milner, 2021; Walter, 2021). Parochial altruism (Bernhard et al., 2006) can play a role in favoring positive outcomes for people close by over positive outcomes for those far away. Information updating can also lead to spatial myopia. When voters learn the state of the world, they usually do so from multiple sources. One source of information is the own (local) experience. Therefore, local outcomes receive some positive weight when inferring the state of nature. Reeves and Gimpel (2012) and Ansolabehere et al. (2014) show that local information impacts perceptions of the macro-economy. Both studies highlight that even when voters would

[2] Alt et al. (2016) argue that some voters struggle, due to lower cognitive capacity, to identify credible sources of information. Duch and Stevenson (2008); Gomez and Wilson (2001, 2006) highlight the difficulties less politically affluent voters have to correctly identify responsibilities for economic outcomes, making attribution more noisy for those voters. This implies that both observations and attributions can be noisy, due to individual limitations, but it does not imply directional biases.

be sociotropic (i.e., only care about aggregate welfare) spatial myopia would bias their economic voting decisions based on local economic outcomes.

Bisgaard (2019) shows that even when facts are accurately observed, stronger partisans selectively attribute credit and blame such that it fits their own perspective, fitting a partisan motivated reasoning (PMR) explanation. Partisans try to protect their identity by framing information such that the resulting beliefs preserve the individual's status in an identity-defining group (Kahan, 2015). In a similar vein, Graham and Singh (2023) show responsibilities are attributed selectively for similarly perceived events during the first weeks of the pandemic in the US. Credit for positive developments was given to co-partisans and blame for negative developments was attributed to the opposite party. Graham and Singh (2023), however, argue that the perceived competence of the leadership determines selective attribution more than PMR. The important takeaway is that both mechanisms lead to partisan biases in attribution, not observation. The PMR literature gives many examples of how voters end up making biased observations via selective information seeking. Even in incentivized settings, monetarily (Peterson and Iyengar, 2021; Allcott et al., 2020) and non-monetarily (Berinsky, 2018), substantive partisan gaps in factual beliefs are found. Altogether, this suggests that both observations and attributions can be partisan biased.

Evaluation can be either *consistent*, causing intentions and behavior to be adjusted or *apathetic* leading to no response in intentions or behavior. Alt et al. (2016) show that less politically affluent voters do update their priors, as suggested in other research (Zaller, 1992), but then often fail to adjust behavioral intentions accordingly. The apathetic evaluation, even when observation and attribution are correctly processed, can also be justified, if the voter simply does not care enough about the issue. Because apathetic evaluation is an absorbing state and mutes behavioral responses we do not further consider this case.

A general comprehensive typology of retrospective voting developed by synthesizing the patterns document above is reported in Appendix A. In Appendix 6 we apply the general typology to the case of retrospective voting in Brazil after the pandemic.

ICUs and voting

In principle, there are two main mechanisms for how ICUs can affect retrospective voting. The first mechanism is a public service provision argument. ICUs are the public service most relevant to treating severe cases of Covid-19. Therefore, a higher presence of ICUs improves the local pandemic outcome.

For some voters, e.g., those who care about local outcomes, and are accurately observing the state of the world, the presence of ICUs as a means to improve the pandemic outcome induces rewarding behavior. For some other voters, the local pandemic outcome does not matter. There is, however, no rationale to punish Bolsonaro for the higher presence of ICUs, if only considering that ICUs improve the pandemic outcome.[3] There exists no type of voter, who would increase their propensity to vote against Bolsonaro when local outcomes become better, ceteris paribus. Even a Partido dos Trabalhadores (PT, engl. Workers' Party) partisan would not punish Bolsonaro more when local death rates were lower. Under this mechanism, we would expect a positive or flat relationship between ICU presence and voting for Bolsonaro.

The second mechanism runs through information dissemination. Considering that information about the true severity of Covid-19 contradicts the false narrative of the Bolsonaro administration, ICUs can be seen as markers of the true state of the world. The key notion here is that the presence of ICUs alters the information available to citizens. Closer to ICUs, voters are more likely to experience firsthand the fatal consequences of Covid-19. While only a minority of the population received treatment in an

[3] See Apendix 6 for a detailed discussion on the behavioral reactions of each type of voter

ICU, information about family members, friends, or neighbors being treated (successful or unsuccessful) is likely to spread rapidly through interpersonal social networks. Even non-Covid-patients are likely to observe what is going on and spread their experiences. The same is true for those who have been there not for treatment. Overworked medical personnel and over-crowded waiting rooms are observed and experienced by medical and non-medical staff, who more likely reside closer to ICUs than further away, and will share those stories with their friends and neighbors. Increased transportation of sick people to the hospital and people who could not be saved out of the hospital are more observable in the vicinity of larger hospitals with ICUs.

The information disseminated from ICUs is often highly credible in contrast to information obtained from media sources. Either direct observation or reports from social contacts are unlikely to be dismissed. As Alt et al. (2016) and Weitz-Shapiro and Winters (2017) show, credible information is key to behavioral change. On the psychological level increased anxiety could boost the learning effect of the information disseminated from ICUs. Mehlhaff et al. (2023) show that in the U.S., for Republicans, co-partisan cues did not decrease the support of safety measures against Covid-19, when voters were anxious.

Since pro-Bolsonaro biases in observation and attribution are directly supported by Bolsonaro's propaganda, local variation in credible counter-information would induce some voters closer to ICUs to drop biases that would prevent them from punishing Bolsonaro. E.g., they would be less likely to believe that Covid-19 is ”only a mild flu”. It could even be possible that voters overreact. E.g., someone who would have held no bias, absent the information disseminated from an ICU, could have a higher propensity to punish Bolsonaro, when being exposed to information about the fatality of Covid-19. Either way, the information mechanism suggests that voters closer to larger ICUs are more likely to punish Bolsonaro.

Data

Brazilian electoral data

The Superior Electoral Court (Brazilian Portuguese: Tribunal Superior Eleitoral, TSE) supplies publicly available data for turnout and election results for the universe of electoral districts (secao, $n > 450,000$) in Brazil. Data is available for presidential and local elections. It contains candidates' total votes, turnout totals, and totals for blank and invalid votes. Polling station-level demographic averages of age, gender, marital status, and education are available in a separate file and can be linked. Since the electoral districts are such fine-grained units, several electoral districts vote in the same location at different ballots. Geo-coding of voting locations ($n > 80,000$) is provided by Hidalgo (2021).

Covid-19 data

We employ two sources for data on Covid-19. The first is from Cota (2020). This source supplies the officially registered number of cases and deaths on the municipal level. The second source is Oliveira (2023), which collects civil registry information about deaths in municipalities from a government website and publishes them as a unified data set. The latter data allows us to compute excess mortality compared to pre-Covid-19 mortality rates.

Hospital beds and ICUs

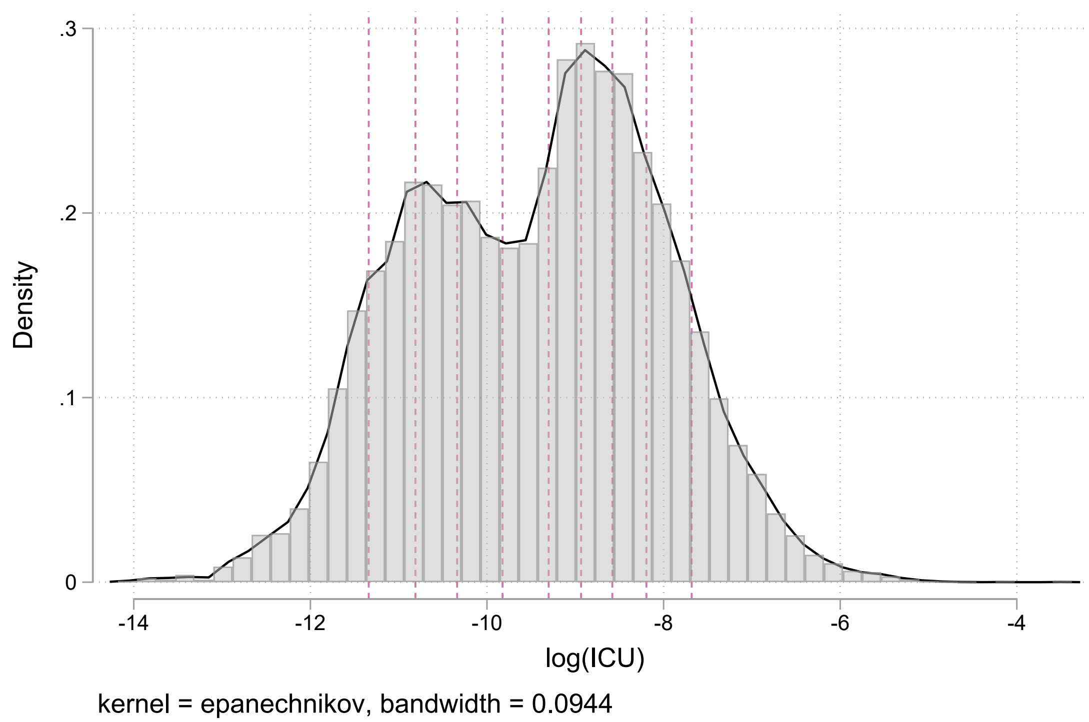
Data on hospital beds in general, and ICUs specifically, is openly provided by DATA-SUS (2018). The database is maintained by the Brazilian Ministry of Health and lists the universe of hospitals in Brazil. Among other things, the data contains the number and type of available care units. Hospital locations are geo-coded by Pereira and

Goncalves (2023). The latest available geo-coding is from 2015. Using official register identifier codes we can match the hospital locations in 2015 to the up-to-date information about care units from September 2018, the month before Jair Bolsonaro was elected president. We choose this point in time to ensure that our measures of ICU presence are not associated with subsequent changes in service provision that may be attributed to Bolsonaro's government. From over 7,000 hospitals registered in 2018, we can successfully match over 99% to the location from 2015, only 48 entities are not listed in geo-coded data from 2015.

The presence of hospital beds

We construct two indices. The first captures the presence of ICUs (which we simply denote ICU). The second captures all hospital beds ABA (all beds available). Both indices are computed in the same way, based on geo-coded hospitals. ICU uses the number of available ICU beds, while ABA uses the number of all available beds in a location. We first find the 10 nearest hospitals for every voting location. While 10 is arbitrary, this is a conservative choice, since the 10th hospital contributes only marginally when weighted by inverse distance. We then construct a measure summing the total number of available beds in those 10 locations weighted by inverse distance. This is our main variable of interest. Using both the distance and the number of beds best captures the information dissemination capacity of ICUs. We then group the raw measure into decile groups, as the raw data does not have a natural interpretation. In Figure 1 we show the distribution of the logarithmic raw values. We opt for a logarithmic display due to the skewness of the distribution. We also compute an alternative measure, adjusting for the number of active voters in the respective voting location, as more voters would indicate more competition for a bed in case of need, and consequently lower access. We then adjust the measure for competition across voting locations (again using inverse distance weighting), since people at different voting locations may frequent the same hospitals. This measure best captures the capacity of ICUs to improve local pandemic outcomes. We will report results using the latter measures for health care access in Appendix E. Note that the results are quantitatively and qualitatively similar, which is not surprising since both measures are strongly correlated.

Figure 1: Event study plot



Note: The histogram shows the relative frequency of logarithm ICU presence. Red lines demarcate the deciles later used to group the index values. The solid black line shows a kernel density estimate. We opt to display logarithmic data due to the skewness of the distribution.

Empirical strategy

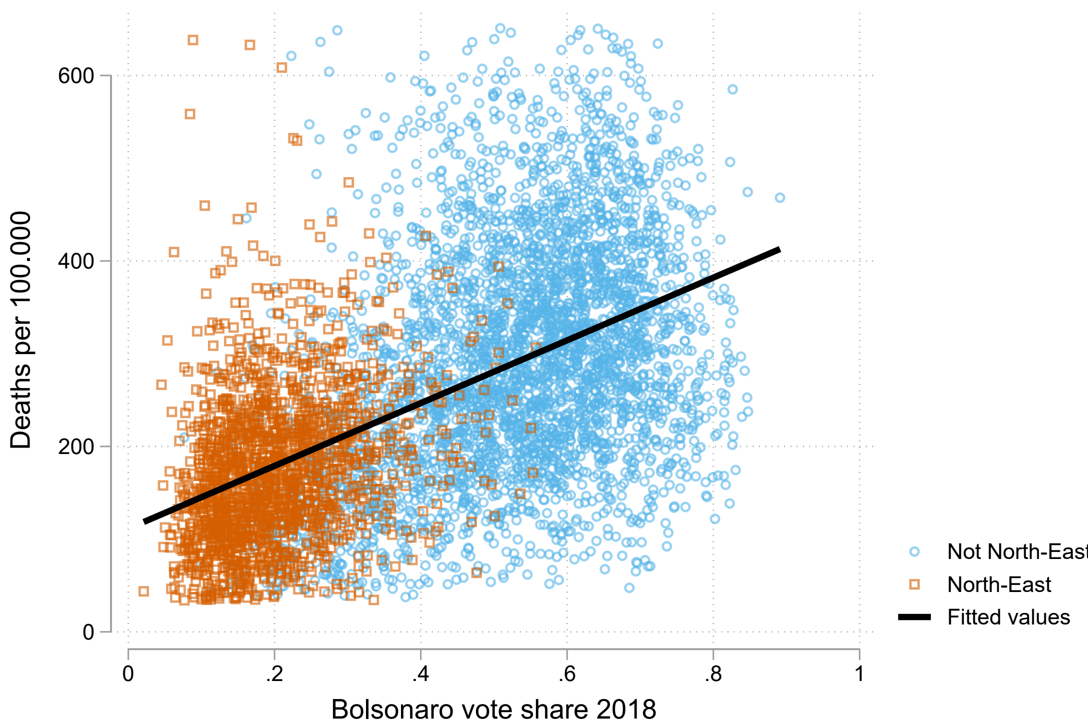
First, we briefly turn to the relationship between voting for Bolsonaro in 2018 and death rates during the pandemic on the municipality level. We then turn to the electoral district level. Here we examine the impact of a compound treatment, i.e., the presence of ICUs during the pandemic, on the change in vote shares for Jair Bolsonaro between the 2018 and 2022 Presidential Elections in Brazil. The treatment captures the presence of ICUs before the election in 2018 to be free of influences of later politically salient investments or pandemic-related policies. The treatment is compound in the sense that it can affect the population through multiple mechanisms. ICU presence reduces individual probability of dying from Covid-19, conditional on being infected. ICU presence also reduces mortality on the local aggregate, conditional on infection rates, and therefore, improves the local pandemic outcome. Moreover, ICU presence affects the local supply of information about Covid-19.

The key feature why pre-September 2018 ICUs as a treatment are attractive is that before the pandemic, ICUs were not an important topic in policy. Arguably, healthcare has always been an important issue in Brazilian politics at various levels. However, through their life-saving function, and consequently the extreme demand for ICUs during the pandemic peak times, ICUs experienced stark politicization and gained social relevance, and salience, unlike any other healthcare services. Hence, the treatment can be understood, not only as the spatial distribution of ICUs but their unexpected emergence as a pertinent issue between the 2018 and 2022 elections.

Voting for Bolsonaro and Covid-19 deaths

We can graphically investigate the relationship between Bosonaro's vote shares and Covid-19 death rate on the municipal level in Figure 2. In Figure 2 we also highlight the geographic difference between the PT-dominated and poorer Northeast meso-region and the rest of the country. Results confirm what the prior research has shown (Rache et al., 2021). A simple OLS regression adding state fixed effects (a somewhat more nuanced approach but also picking up meso-region differences) confirms the picture (see Table 1). Depending on the specification we find that with a 10 pp increase in vote share for Bolsonaro in the first round the death rate per 100,000 increases by about 15.0 - 26.5. For the second round, a 10 pp increase is associated with an increase of 12.2 - 22.8 deaths per 100,000 inhabitants. Looking at excess mortality as the outcome confirms the result. All reported associations above are statistically significant at $p < 0.01$.

Figure 2: Bolsonaro vote shares 2018 and Covid-19 death rate



Note: The unit of observation in this figure is the municipality. The x-axis shows Bolsonaro's vote share in the first round in 2018. The y-axis shows death rates per 100,000 inhabitants. The top and bottom percentile of death rates have been removed, because some outliers were suspicious to be false entries, suggesting an absolute death rate larger than 1.

The myth of electoral Darwinism

A common argument discussed in the public debate states that due to higher death rates in municipalities with larger shares of Bolsonaro supporters, a Darwinistic selection takes place, as more Bolsonaro supporters are more likely to die from Covid-19. We test this argument in a simple back-of-the-envelope calculation. Assume a hypothetical

Table 1: Mortality and first round vote share for Bolsonaro 2018

	(1) Deaths	(2) Deaths	(3) Excess	(4) Excess
First round 2018				
Share Bolsonaro	264.8742	150.1019	0.0997	0.1725
	(14.1812)	(15.4116)	(0.0324)	(0.0370)
Second round 2018				
Share Bolsonaro	227.6816	122.3446	0.0829	0.1485
	(12.9799)	(14.0325)	(0.0299)	(0.0340)
Controls added		✓		✓
Mean outcome	255.0340	255.0340	0.3226	0.3226
N	5,418	5,418	4,846	4,846

Robust standard errors in parentheses

Note: This table shows results from an OLS regression, with deaths per 100,000 inhabitants and excess mortality as the outcomes and Jair Bolsonaro's vote share in the first and second round of the 2018 Brazilian presidential election as the regressor of interest. In columns (2) and (4) we control for municipal averages in gender composition, age, education, and marital status.

scenario, where voters vote the same way as in 2018. However, the voting population is adjusted by the municipality-specific death rate. For now, also assume that the death rate within a municipality is the same for all voters, i.e., independent of political preferences. The aggregate comparison of the hypothetical to the real 2018 results reveals that the effect of such a selection is negligibly small. Bolsonaro would have lost 0.00012 pp in the first round and 0.00014 pp in the second round.[4] Those numbers are far from having electoral significance.

The positive association between death rates and voting for Bolsonaro on the municipal level suggests that even within municipalities supporters of Bolsonaro may face a higher risk of death from Covid. Therefore, we now assume a scenario, where voters vote the same way as in 2018 (like before) but now we adjust the death rate for Bolsonaro voters upward. To test an extreme scenario, we take the highest estimate from the municipal level association between vote shares and death rate, and then add the death rate corresponding to a 100 pp increase in Bolsonaro vote share to the Bolsonaro

[4] The formulas used for the calculations in this paragraph are detailed in the Appendix Section B.

voters' specific death rate. In this extreme scenario, Bolsonaro would have lost 0.00134 pp in the first round and 0.00161 pp in the second round. While larger by an order of magnitude, even under the extreme scenario Darwinistic selection would not significantly affect the electoral outcome. We conclude from this exercise that behavioral adjustments to the pandemic and its management by the Bolsonaro administration are much more likely to have explanatory power for the electoral impact of Covid-19 overall than Darwinistic arguments.

ICU presence and voting

Moving to the electoral district level, we investigate the effect of ICU presence on the change in vote shares for Bolosonaro. We estimate variations of the following regression equation:

$$v^{r}_{s,t} = \beta\,[ICU_{l,t}, ABA_{l,t}] \times d_{2022} + \gamma\,[ICU_{l,t}, ABA_{l,t}] + FE_{s,l,m,t} + \eta\,X_s + \varepsilon_{l,t} \tag{1}$$

The left-hand side $v^{c,r}_{s,t}$ denotes the vote share for candidate c (either Bolsonaro or any PT candidate, depending on the panel) in round r in electoral district s and election year t. The coefficient vector β captures the impact of ICUs and all hospital beds (depending on which index is used) on the change in vote share. The coefficient γ captures the change in voting absent the treatment. Note that depending on the fixed effects specification gamma defaults to 0, as the time-invariant factors on the voting location level will be absorbed. Fixed effects are specified according to our strategy outlined above, including election year, municipality, voting location, electoral district, and municipality-specific time trends.

Vote shares for Bolsonaro are available on the electoral district level, while the treatment occurs at the voting location level, where multiple districts are pooled to cast their votes. Following Abadie et al. (2023), to account for the clustered nature of the data we use clustering robust standard errors in all our models unless otherwise specified.

The data structure lends itself to fixed effects models. We can use fixed effects to absorb time-invariant variation on the electoral district level and variation over time that is constant between districts. This allows us to isolate the association between ICU presence and the change in voting behavior. The district-level fixed effects are especially appealing, since the pandemic environment, e.g., infection rates or mobility restrictions, which potentially inform individual decision-making, are held constant.

We further run models using electoral district level and election year fixed effects. This fixed effects specification boils down to the canonical difference-in-difference specification with continuous treatment. Here we compare the change in vote shares within voting locations, while ICU presence varies across voting locations. Note that, since the treatment occurs at the cluster above the electoral district, the estimation is equivalent to treatment-level fixed effects, unless electoral district-specific controls are used. Additionally, we also include municipality times election year fixed effects to account for municipality-specific time trends in voting, eliminating potential confounding variables on the municipal level.

Hospital location is not random. Therefore selection bias is the main threat to identification. Urban clustering comes to mind as a potential mechanism for selection bias. The decision to reside close to an ICU is likely confounded with other spatial characteristics and individual preferences. Unit or treatment-level fixed effects absorb time-invariant sorting characteristics. Municipality-specific time trends allow us to capture sorting dynamics on the municipal level. Not being able to control for selection effects within municipalities is the main limitation of our identification strategy.

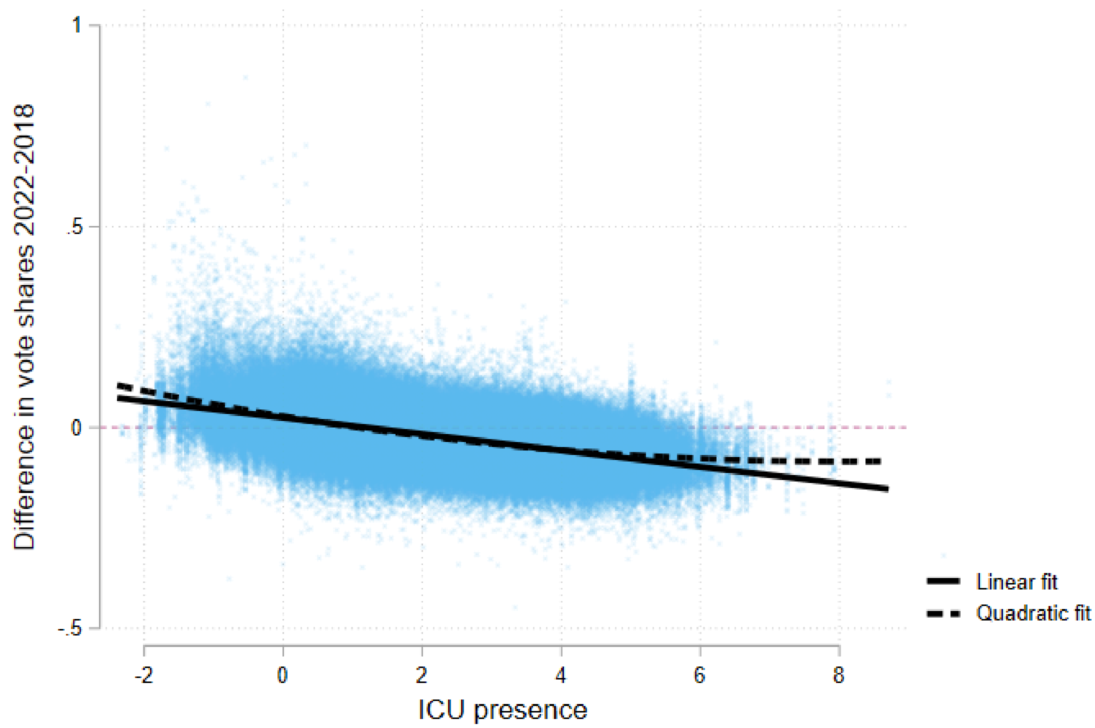
However, we propose several sample splits in the robustness section to help ease this concern. If rural-urban sorting confounded the main analysis we would expect at least some visible heterogeneity along the districts' voter population. While the absence of such heterogeneities does not completely rule out selection, it corroborates the identifying assumption. If our results were driven by unobserved characteristics that correlate with health infrastructure, including ICUs, we should find no effect of ICUs on voting where non-ICU healthcare is well developed, which is not the case.

Another limitation is that we cannot directly test, whether absent the treatment, electoral districts would have evolved along a parallel path. However, we can use a longer panel on PT vote shares as an auxiliary test. This approach is especially valid for runoff elections where voting against Bolsonaro meant voting for PT in 2018 and 2022. Since the PT has competed in all runoff elections in Brazil since the transition to democracy, vote shares are accessible over longer periods (until 2002 without electoral district demographics, until 2010 including demographics), hence allowing checking for common trends.

Descriptive statistics and common trends

In Table 3 we report the change in vote shares for Bolsonaro between the runoff elections 2018 and 2022 by ICU presence. Here we observe the general pattern that the higher the ICU presence, the more voters voted against Bolsonaro. The electoral districts with the lowest ICU presence display increased voting for Bolsonaro. Overall, electoral support for Bolsonaro decreases as ICU presence increases. The correlation we observe here cannot be causally interpreted but serves as a descriptive indication of what we later confirm in the causal analysis.

Figure 3: Change in vote share and ICU presence



Note: The y-axis depicts the average change in votes for Bolsonaro in the runoff elections from 2018 to 2022 at each voting location. The x-axis depicts the natural log of ICU presence.

0.1 Parallel trends assumption

The two-way fixed effects model identifies an average treatment effect on the treated (ATT) only under the assumption of common pre-trends (Wooldridge, 2021). I.e., the treatment should not affect the outcome in pre-treatment periods. Since our outcome of interest exists only in two time periods we cannot directly test this assumption. However, in both elections, 2018 and 2022, the PT was the main competitor of Bolsonaro competing in the runoff elections. The PT has also competed in all other runoff elections since 1989 when Brazil transitioned to democracy. Therefore, we can indirectly test parallel trends focusing on the PT candidate's vote shares, which are for the runoff elections in 2018 and 2022 the complementary share of Bolsonaro's vote shares. Electoral district-level data on vote shares is available from the TSE back until the presidential election of 2002. Before 2010, there are no demographic variables available.

We test parallel trends with the common event study approach since the continuous treatment does not allow us to visually inspect whether pre-treatment vote shares evolve in parallel in the treatment and control group. Figure 4 clearly shows a violation of parallel trends. In the event study framework, the pre-treatment period is fixed as the reference group. We first note that the increase in PT vote share in electoral districts with high ICU presence in 2022 relative to 2018 compares well in magnitude to the estimated loss of Bolsonaro in those districts. Relative to to 2018 all other coefficients on the interactions turn out sizable and significantly negative, except for 2002 which shows a positive coefficient.

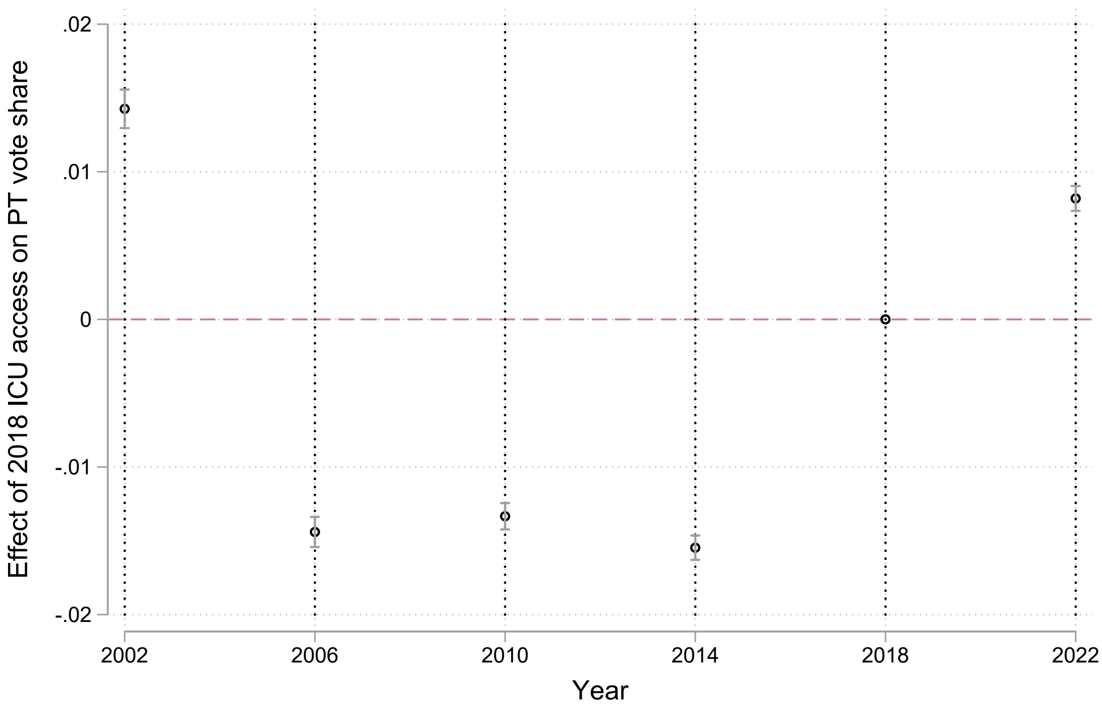
Following the recommendation of Wooldridge (2021), we next include control variables (electoral district averages of age, education, gender, and marital status), to test whether the data satisfies the conditional common trends assumption. Data for demographics on the electoral district level are available from 2010 onward. Hence, we obtain a shorter panel for the event study in this case. The coefficients, reported in Appendix C, are similar to the coefficients without control variables. Again we have to reject parallel trends. Moving forward we need to adjust for the problem of non-parallel trends using SDID.

Results

Two-way fixed effects

In this section, we report results from panel fixed effects regressions, which suggest that overall voters in electoral districts with higher presence of ICUs punished Bolsonaro more strongly. We first report results from a difference-in-difference (DiD) model with

Figure 4: Event study plot



Note: The event study plot shows the coefficients of election year dummies interacted with ICU presence. The specification absorbs electoral district-level fixed effects and municipality-specific time trends. Confidence intervals are obtained for the 99% level from clustered standard errors at the voting location times year level to allow for time and location-specific heteroscedasticity. See Appendix C for conditional parallel trends and alternative measures of treatment.

continuous treatment for the simple two-period case. Since treatment is defined by a combination of a pre-determined characteristic (ICU presence) on the voting location level and the unexpected rise to relevance of this characteristic due to Covid-19 between the observation periods, we are worried about confounding variation in ICU presence. We employ demographic control variables (education, age, gender, and marital status) on the electoral district level (below the voting location level), corroborating identification by picking up some potentially confounding variation. Moreover, municipality-specific time trends allow us to control for municipality-specific heterogeneities.

In Table 2 we find a stable, negative, and statistically significant ($p < 0.01$) effect of hospital presence on voting for Bolsonaro. We interpret this as evidence that hospital presence during the Covid-19 pandemic led voters to decrease their support of Jair Bolsonaro. The estimates are quantitatively similar when using ICU presence or any care unit. Slightly larger coefficients for ICUs fit the notion that especially ICUs convey relevant information about the true state of the world. Estimates are not sensitive to the inclusion of control variables and are comparable across election rounds (see Appendix Table 3 for first-round vote shares). The effect appears to be somewhat stronger for the runoff election than the first round. This is unsurprising, as voters, who want to vote against Bolsonaro but are not necessarily supporters of PT or any other party, have the strongest incentives to turn out in the second round. One index unit (equivalent to a decile) more presence of ICUs decreases Bolsonaro's vote share by 0.7 to 1.27 pp. Consequently, an inter-quartile-range jump in ICU presence decreases Bolsonaro's vote share by 3.50 to 6.35 pp. One index unit higher presence of any hospital bed decreases Bolsonaro's vote share by 0.53 to 1.13 pp. And an inter-quartile-range jump would decrease Bolsonaro's vote share by 2.65 to 5.65 pp. Those values are substantial in the context of a highly contested election like in Brazil 2022, which was decided by less than 2 pp.

Synthetic difference-in-difference

Extensive new literature suggests synthetic control approaches to deal with violated parallel trends (Lindner and McConnell, 2019; Ham and Miratrix, 2022; Wooldridge, 2021; Arkhangelsky et al., 2021; Pailañir and Clarke, 2023). We implement synthetic difference-in-difference (SDID, Arkhangelsky et al., 2021; Pailañir and Clarke, 2023). Intuitively, by weighting untreated units that are more similar to treated units before treatment more, the two-way fixed effects regression becomes more robust to non-common trends. To implement SDID, we have to make some concessions, though. SDID allows only for binary treatments. We therefore use different cut points to define

Table 2: DiD health care presence on 2nd round vote shares

	(1)	(2)	(3)	(4)	(5)	(6)
ICU x 2022	-0.0078	-0.0127	-0.0070			
	(0.0001)	(0.0001)	(0.0001)			
ABA x 2022				-0.0057	-0.0113	-0.0053
				(0.0001)	(0.0001)	(0.0001)
age		0.0025	0.0021		0.0026	0.0021
		(0.0000)	(0.0000)		(0.0000)	(0.0000)
education		0.1349	0.1219		0.1504	0.1204
		(0.0019)	(0.0019)		(0.0019)	(0.0019)
education^2		-0.0111	-0.0084		-0.0126	-0.0082
		(0.0002)	(0.0002)		(0.0002)	(0.0002)
sex		-0.0745	-0.0726		-0.0764	-0.0725
		(0.0012)	(0.0012)		(0.0012)	(0.0012)
married		-0.0608	0.0141		-0.0602	0.0139
		(0.0018)	(0.0018)		(0.0019)	(0.0018)
Mun.×year FE	✓		✓	✓		✓
N	872,604	872,617	872,599	872,604	872,617	872,599

Standard errors in parentheses

Note: The outcome is Bolsonaro's vote share in the runoff election. The treatment variable is health care access measured by ICU presence and any care unit respectively. All regressions in this table control for year and unit fixed effects. Standard errors are clustered at the treatment level (voting location).

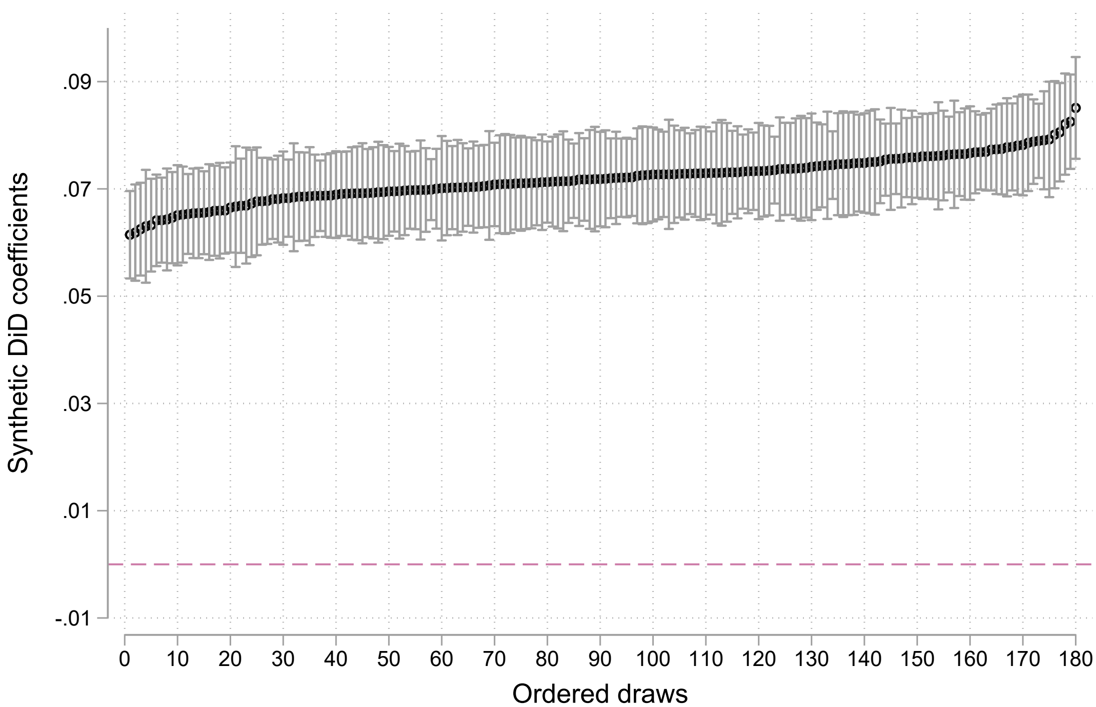
binary indicators based on the original 10-point ICU index. Moreover, SDID does not allow higher-dimensional fixed effects. Thus, we cannot include municipality-specific time trends. Further, SDID is computationally demanding, which prohibits us from running SDID on the full sample. We circumvent the latter problem by using a Monte Carlo cross-validation technique. We draw random sub-samples from the full sample without replacement and estimate the SDID model on the sub-samples. This way we convert the problem of exponentially increasing computing time to linear additive computing time over sub-samples. We first generated a random number $rn \sim U(1, 10)$ for each panel unit. We then split the sample into equally sized intervals (180) along rn and perform estimation on each sub-sample. We plot the distribution of coefficients in Figure 5. The specification reported in Figure 5 is based on the panel of PT vote shares in the runoff elections from 2010 to 2022, to allow including electoral district-level controls. The cutoff index value for dichotomizing the treatment is set to ICU > 4.[5]

The results from the SDID are in line with the previous fixed effect models. The magnitudes of the estimates from the SDID are difficult to directly compare to fixed effects models due to the dichotomous treatment. The dichotomous treatment pools index values below the cutoff and index values above the cutoff. It is not surprising that the coefficients are somewhat larger in SDID, between 0.059 and 0.075 (or 5.9 to 7.5 pp) loss for Bolsonaro. Those values compare best to interquartile-range jumps in the original index as the average distance between treated and untreated after dichotomization corresponds to the interquartile-range. Compared to the estimated effect of an interquartile-rage jump in ICU from the two-way fixed effects regression with unit fixed effects and municipality-specific time trends, which amounts to 1.10 and 3.55 pp, the SDID estimates are still larger but within a reasonable range. We view the larger

[5] In Figure 9a we report results for treatment defined as $\mathbf{1}(ICU > 2)$. In Figure 9b we report results for treatment defined as $\mathbf{1}(ICU > 6)$.

SDID coefficients as evidence that bias from non-parallel trends attenuates estimates in conventional DiD.

Figure 5: SDID coefficient distribution from Monte Carlo cross validation



Note: The SDID model uses PT vote share as the outcome and $\mathbf{1}(ICU > 4)$ in 2022 as the treatment of interest. The model includes time-varying control variables on the electoral district level, age, education, education^2, gender, and marital status. Standard errors are clustered on the electoral district level. We show 95% confidence intervals.

Robustness

To assess the validity of our results and to probe for heterogeneities in effects we investigate an array of sample splits and alternative treatment definitions, see Appendix F. We begin with the sample of the Northeast only, a meso-region traditionally dominated by the PT, displaying the highest poverty rates in the country. Table 4 reveals that the Northeast is not particular when it comes to the effect of ICU presence on voting.

The main concern for identification is that ICUs are not placed randomly. TWFE ensures that comparisons are within electoral districts over time. Yet, people closer to larger ICUs may react differently to the pandemic because they are different (in unobserved characteristics), which made them settle somewhere with higher ICU presence in the first place. If rural-urban sorting confounded the main analysis we would expect at least some visible heterogeneity along the districts' voter population. While the absence of such heterogeneities does not completely rule out selection, it corroborates the identifying assumption. In Table 7 it becomes clear that there is little difference between less and more populated electoral districts. If anything, the effect appears to be stronger in less populated districts.

We further test the implication of heterogeneity in general hospital access, which may also be indicative of sorting. In Tables 10 and 11 we focus on the impact of ICU presence only within electoral districts that have any kind of hospital within 20km distance. We then further restrict the sample by increasing the number of available beds within that distance. If our results were driven by sorting, we would expect heterogeneous effects between districts with an abundance of hospital beds in their vicinity. In other words, if our effects were driven by unobserved characteristics that correlate with health infrastructure, including ICUs, we should find no effect of ICUs on voting where non-ICU healthcare is well developed. Again the evidence is reassuring of the main results. There are no substantial heterogeneities even when severely restricting the sample to districts with more than 200 hospital beds within 20km.

Other heterogeneities may still exist. Hence, we test the main specification across states independently. Reinforcing the main results, we find that all coefficients are negative and significant across all states, with few outliers in magnitude, see 10. Thus, the general results prove to be valid across states.

There might be a concern that the informative impact of ICU presence on voting behavior varies depending on how strongly an area was affected by the pandemic.

In Table 5 we show that estimates do not vary substantially across the quartiles of municipality-level death rates. The absence of any directional trend along the quartiles further limits concerns about systematic heterogeneities.

Similarly, there may be a concern that strongholds of parties react heterogeneously to real-world information. Therefore, we split the sample along the quartiles of 2018 Bolsonaro vote shares and re-estimate the main model. The results reported in Table 6 suggest that there are no heterogeneous effects that depend on the political leaning across districts.

If our argument holds, we would expect to find similar results when we employ different treatment definitions. In Table 8, columns (1)-(3), we report coefficients from the main models on the raw distance to the closest ICU. The results suggest being 100km closer to the closest ICU decreases Bolsonaro's vote by 4-6 pp. Using a binary indicator for a hospital with an ICU in 20km, 30km, or 50km distance leads to similar conclusions. Having an ICU within 20km reduces Bolsonaro's vote share by 2.1-6.9 pp, within 20km it is 1.6-7.2 pp and within 50km it is 0.12-7.33 pp (see Table 8, columns (4)-(6), and Table 9).

Conclusion and discussion

We study how accountability operates in an environment of large-scale governmental misinformation. We employ the case of voting in the 2022 Brazilian Presidential Election after Brazil was severely hit by the Covid-19 pandemic. We argue that ICUs functioned as salient cues about the true state of the world, which counteracted the government's narrative, and eventually enforced (some) accountability despite misinformation. Brazil is an exemplary case because the Bolsonaro administration assumed a course of action and rhetoric that rejected scientific consensus and the guidelines of the international medical community (Taylor et al., 2021; de Oliveira and Veronese, 2023). Instead, the administration adopted a misinformation campaign (Béland et al., 2021). This included, for example, the promotion of ineffective Malaria generic as a treatment for Covid-19 (Nowak, 2023; Fonseca et al., 2021).

Analyzing rich electoral data from several Brazilian Presidential elections, combined with geo-located data on hospital equipment, we find that an inter-quartile range increase in our measure for ICU presence (taking into account the size of the ICU and spatial distance), decreases the vote share for Bolsonaro by 2 to 8 pp. This suggests that information based on proximity to ICUs significantly contributed to Bolsonaro's downfall in the 2022 election. Bolsonaro lost the runoff election by 1.8 pp. Hence, an inter-quartile range lower exposure to ICUs would have sufficed to overturn the elections. Alternatively, in the public debate, Darwinistic arguments for Bolsonaro's defeat were discussed. In a back-of-the-envelope calculation, we show that selection based on higher death rates among Bolsonaro supporters is unlikely to have any substantial impact on the election. The death rate differential required to create any substantial effect is far from realistic.

A large literature on political misinformation and biases in political information processing raises serious doubts about whether electoral accountability can be upheld in environments of stark misinformation (Kuklinski et al., 2000; Jerit and Zhao, 2020; Allcott et al., 2020; Bisgaard, 2019; Berinsky, 2018; Peterson and Iyengar, 2021; Mehlhaff et al., 2023). Our analysis contributes to the understanding of mechanisms that help to ensure electoral accountability through retrospective voting, despite misinformation.

It is beyond the scope of this paper to study information (or information correction) mechanisms comparatively. We want to note, however, that information observed in proximity to ICUs has the interesting property, that there is no political actor disseminating information. Typically politically relevant information stems from politically involved actors, including media outlets. Hence, there is always reason to speculate about the credibility of signals. Real-world observations propagated through inter-personal social networks may be more effective in combating misinformation than traditional information dissemination, since credibility within networks may be high. Relative to (Mehlhaff et al., 2023) we show that there are circumstances in polarized societies, where we can expect that information sways voters. Defining those circumstances requires further investigation.

References

Abadie, Alberto, Athey, Susan, Imbens, Guido W, and Wooldridge, Jeffrey M. When should you adjust standard errors for clustering? *The Quarterly Journal of Economics*, 138(1):1–35, 2023. Back to page: 18

Ajzenman, Nicolás, Cavalcanti, Tiago, and Da Mata, Daniel. More than words: Leaders' speech and risky behavior during a pandemic. *Available at SSRN 3582908*, 2020. Back to page: 5

Allcott, Hunt, Boxell, Levi, Conway, Jacob, Gentzkow, Matthew, Thaler, Michael, and Yang, David. Polarization and public health: Partisan differences in social distancing during the coronavirus pandemic. *Journal of public economics*, 191: 104254, 2020. Back to page: 3, 5, 7, 8, 29, 37

Alt, James E, Marshall, John, and Lassen, David D. Credible sources and sophisticated voters: When does new information induce economic voting? *The Journal of Politics*, 78(2):327–342, 2016. Back to page: 3, 5, 6, 7, 8, 10

Ansolabehere, Stephen, Meredith, Marc, and Snowberg, Erik. Mecro-economic voting: Local information and micro-perceptions of the macro-economy. *Economics & Politics*, 26(3):380–410, 2014. Back to page: 5, 7

Arkhangelsky, Dmitry, Athey, Susan, Hirshberg, David A, Imbens, Guido W, and Wager, Stefan. Synthetic difference-in-differences. *American Economic Review*, 111 (12):4088–4118, 2021. Back to page: 4, 23

Ashworth, Scott and De Mesquita, Ethan Bueno. Is voter competence good for voters?: Information, rationality, and democratic performance. *American Political Science Review*, 108(3):565–587, 2014. Back to page: 4

Autor, David, Dorn, David, Hanson, Gordon, and Majlesi, Kaveh. Importing political polarization? the electoral consequences of rising trade exposure. *American Economic Review*, 110(10):3139–3183, 2020. Back to page: 7

Aytaç, Selim Erdem. An experimental investigation of voter myopia in economic evaluations. *Electoral Studies*, 74:102393, 2021. Back to page: 7

Baccini, Leonardo and Weymouth, Stephen. Gone for good: Deindustrialization, white voter backlash, and us presidential voting. *American Political Science Review*, 115 (2):550–567, 2021. Back to page: 38

Batista Pereira, Frederico and Nunes, Felipe. Presidential influence and public opinion during crises: The case of covid-19 in brazil. *International Journal of Public Opinion Research*, 34(2):edac014, 2022. Back to page: 3

Bechtel, Michael M and Hainmueller, Jens. How lasting is voter gratitude? an analysis of the short-and long-term electoral returns to beneficial policy. *American journal of political science*, 55(4):852–868, 2011. Back to page: 7

Berinsky, Adam J. Telling the truth about believing the lies? evidence for the limited prevalence of expressive survey responding. *The Journal of Politics*, 80(1):211–224, 2018. Back to page: 5, 8, 29

Bernhard, Helen, Fischbacher, Urs, and Fehr, Ernst. Parochial altruism in humans. *Nature*, 442(7105):912–915, 2006. Back to page: 7

Bisgaard, Martin. How getting the facts right can fuel partisan-motivated reasoning. *American Journal of Political Science*, 63(4):824–839, 2019. Back to page: 5, 8, 29, 37

Béland, Daniel, Rocco, Philip, Segatto, Catarina Ianni, and Waddan, Alex. Trump, bolsonaro, and the framing of the covid-19 crisis: How political institutions shaped presidential strategies. *World Affairs*, 184(4):413–440, 2021. doi: 10.1177/00438200211053314. URL https://doi.org/10.1177/00438200211053314. Back to page: 29

Colantone, Italo and Stanig, Piero. The trade origins of economic nationalism: Import competition and voting behavior in western europe. *American Journal of Political Science*, 62(4):936–953, 2018. Back to page: 7

Cota, Wesley. Monitoring the number of COVID-19 cases and deaths in brazil at municipal and federative units level. *SciELOPreprints:362*, May 2020. doi: 10.1590/scielopreprints.362. URL https://doi.org/10.1590/scielopreprints.362. Back to page: 11

DATASUS, . Leitos 2018, 2018. https://s3.sa-east-1.amazonaws.com/ckan.saude.gov.br/Leitos_SUS/Leitos_2018.csv, accessed: 10.08.2023. Back to page: 11

Oliveira, Gustavo Mourade and Veronese, Marília Verissimo. Brazil and the "bolsonaro phenomenon": Politics, the economy, and the covid-19 pandemic, 2019–2020. *Latin American Perspectives*, 2023. Back to page: 3, 29

Duch, Raymond M and Stevenson, Randolph T. *The economic vote: How political and economic institutions condition election results*. Cambridge University Press, 2008. Back to page: 4, 7

Ferejohn, John. Incumbent performance and electoral control. *Public choice*, pages 5–25, 1986. Back to page: 2

Fonseca, Elize Massard da, Nattrass, Nicoli, Lazaro, Lira Luz Benites, and Bastos, Francisco Inácio. Political discourse, denialism and leadership failure in brazil's response to covid-19. *Global public health*, 16(8-9):1251–1266, 2021. Back to page: 2, 29

Gomez, Brad T and Wilson, J Matthew. Political sophistication and economic voting in the american electorate: A theory of heterogeneous attribution. *American Journal of Political Science*, pages 899–914, 2001. Back to page: 4, 7

Gomez, Brad T and Wilson, J Matthew. Cognitive heterogeneity and economic voting: A comparative analysis of four democratic electorates. *American Journal of Political Science*, 50(1):127–145, 2006. Back to page: 4, 7

Graham, Matthew H. and Singh, Shikhar. An outbreak of selective attribution: Partisanship and blame in the covid-19 pandemic. *American Political Science Review*, page 1–19, 2023. doi: 10.1017/S0003055423000047. Back to page: 3, 5, 7, 8, 37

Green, J, Evans, G, and Snow, D. The government is losing support over its handling of coronavirus, especially among its new 2019 voters. *Nuffield College Elections Unit*, 9, 2020. Back to page: 2

Ham, Dae Woong and Miratrix, Luke. Benefits and costs of matching prior to a difference in differences analysis when parallel trends does not hold. *arXiv preprint arXiv:2205.08644*, 2022. Back to page: 23

Healy, Andrew and Malhotra, Neil. Myopic voters and natural disaster policy. *American Political Science Review*, 103(3):387–406, 2009. Back to page: 4, 7

Healy, Andrew and Malhotra, Neil. Retrospective voting reconsidered. *Annual review of political science*, 16:285–306, 2013. Back to page: 3, 4, 6

Hidalgo, F Daniel. Geocoding brazilian polling stations with administrative data sets. *https://github.com/fdhidalgo/geocode_br_polling_stations*, 2021. Back to page: 11

Jensen, J Bradford, Quinn, Dennis P, and Weymouth, Stephen. Winners and losers in international trade: The effects on us presidential voting. *International Organization*, 71(3):423–457, 2017. Back to page: 38

Jerit, Jennifer and Zhao, Yangzi. Political misinformation. *Annual Review of Political Science*, 23:77–94, 2020. Back to page: 2, 5, 29

Kahan, Dan M. The politically motivated reasoning paradigm. *Emerging Trends in Social & Behavioral Sciences, Forthcoming*, 2015. Back to page: 5, 8

Kuklinski, James H, Quirk, Paul J, Jerit, Jennifer, Schwieder, David, and Rich, Robert F. Misinformation and the currency of democratic citizenship. *The Journal of Politics*, 62(3):790–816, 2000. Back to page: 2, 5, 29

Leone, Tharcisio. The harmful effects of denial: when political polarization meets covid-19 social distancing. 2021. Back to page: 5

Lindner, Stephan and McConnell, K John. Difference-in-differences and matching on outcomes: a tale of two unobservables. *Health Services and Outcomes Research Methodology*, 19:127–144, 2019. Back to page: 23

Mariani, Lucas Argentieri, Gagete-Miranda, Jessica, and Rettl, Paula. Words can hurt: How political communication can change the pace of an epidemic. *Covid Economics*, 12:104–137, 2020. Back to page: 5

Mehlhaff, Isaac D., Ryan, Timothy J., Hetherington, Marc J., and MacKuen, Michael B. Where motivated reasoning withers and looms large: Fear and partisan reactions to the covid-19 pandemic. *American Journal of Political Science*, n/a(n/a), 2023. doi: https://doi.org/10.1111/ajps.12808. URL https://onlinelibrary.wiley.com/doi/abs/10.1111/ajps.12808. Back to page: 2, 4, 5, 10, 29, 30

Milner, Helen V. Voting for populism in europe: Globalization, technological change, and the extreme right. *Comparative Political Studies*, 54(13):2286–2320, 2021. Back to page: 7

Nowak, Jörg. The pandemic politics of the bolsonaro government in brazil: Covid-19 denial, the chloroquine economy and high death rates. In *The Political Economy of Global Responses to COVID-19*, pages 105–123. Springer, 2023. Back to page: 29

Oliveira, Marcelo. Brazil civil registry data. *https://github.com/capyvara/brazil-civil-registry-data*, 2023. Back to page: 11

Pailañir, Daniel and Clarke, Damian. Sdid: Stata module to perform synthetic difference-in-differences estimation, inference, and visualization. 2023. Back to page: 4, 23

Pereira, Rafael H. M. and Goncalves, Caio Nogueira. *geobr: Download Official Spatial Data Sets of Brazil*, 2023. URL https://github.com/ipeaGIT/geobr. R package version 1.7.0999. Back to page: 11

Peterson, Erik and Iyengar, Shanto. Partisan gaps in political information and information-seeking behavior: motivated reasoning or cheerleading? *American Journal of Political Science*, 65(1):133–147, 2021. Back to page: 5, 8, 29

Rache, Beatriz, Lago, Miguel, Falbel, Fernando, and Rocha, Rudi. Political behavior and covid-19 mortality: How many lives in one vote? 2021. Back to page: 5, 14

Reeves, Andrew and Gimpel, James G. Ecologies of unease: Geographic context and national economic evaluations. *POLITICAL BEHAVIOR*, 34(3):507–534, SEP 2012. ISSN 0190-9320. doi: 10.1007/s11109-011-9167-8. Back to page: 7

Singh, Shikhar and Thachil, Tariq. Why citizens don't hold politicians accountable for air pollution, Aug 2023. URL osf.io/yf7qb. Back to page: 5

Taylor, Luke and others, . We are being ignored': Brazil's researchers blame anti-science government for devastating covid surge. *Nature*, 593(7857):15–16, 2021. Back to page: 2, 29

Walter, Stefanie. The backlash against globalization. *Annual Review of Political Science*, 24:421–442, 2021. Back to page: 7

Weitz-Shapiro, Rebecca and Winters, Matthew S. Can citizens discern? information credibility, political sophistication, and the punishment of corruption in brazil. *The Journal of Politics*, 79(1):60–74, 2017. Back to page: 3, 5, 6, 10

Wooldridge, Jeffrey M. Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators. *Available at SSRN 3906345*, 2021. Back to page: 20, 21, 23

Wright, John R. Unemployment and the democratic electoral advantage. *American Political Science Review*, 106(4):685–702, 2012. Back to page: 38

Zaller, John. *The nature and origins of mass opinion*. Cambridge university press, 1992. Back to page: 8

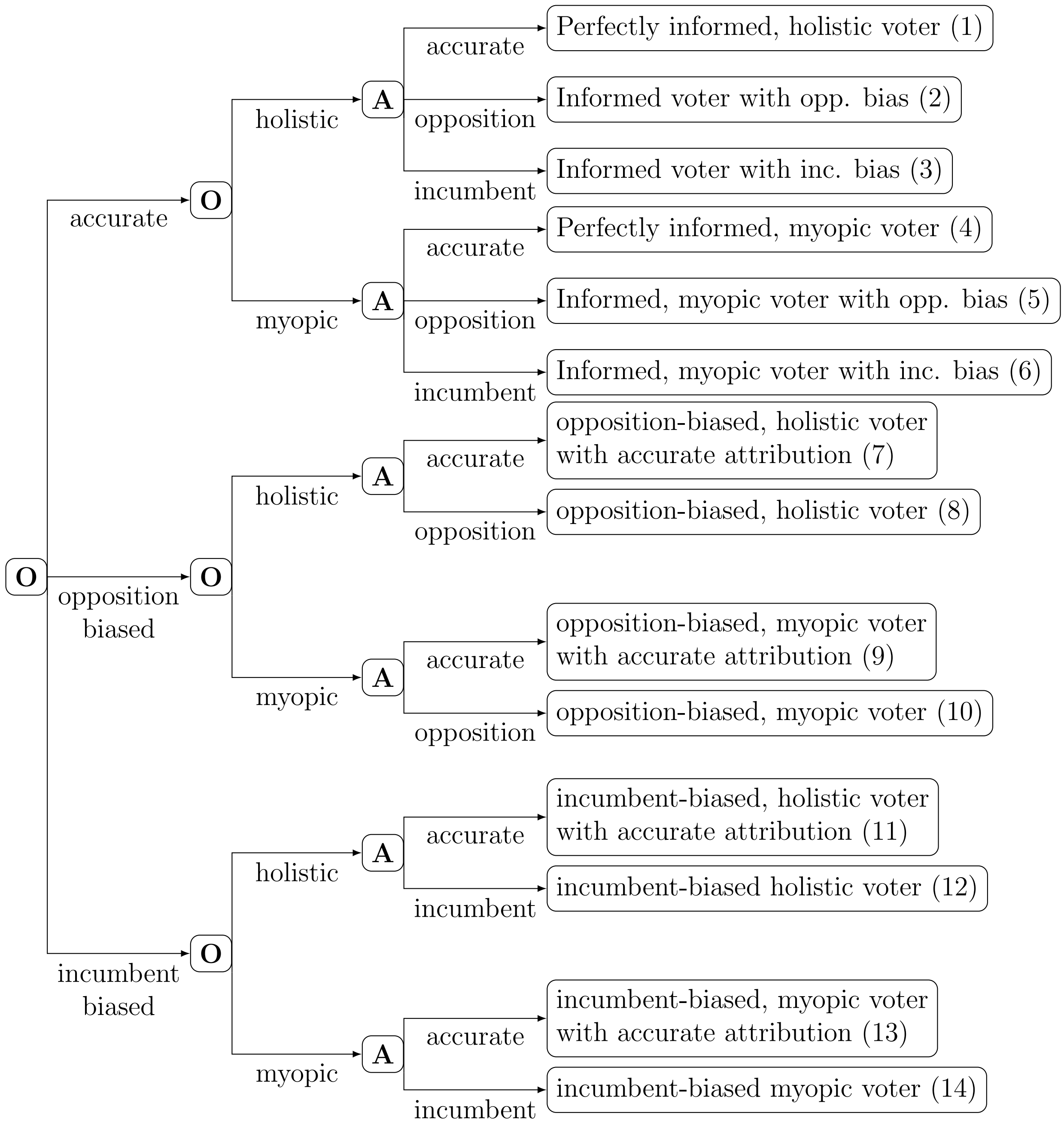
A Typology of retrospective voting

Assuming for simplicity a unidimensional political spectrum, this leaves us with 3 categories for steps 1 and 2 of retrospective voting: Accurate, biased towards the incumbent, and biased towards the opposition.[6] Observation is, moreover, divided into myopic and holistic. Leaving us with 6 categories for observation. Step 3, evaluation, can be consistent or apathetic. This leads in total to 32 ($3 \times 2 \times 3 \times 2$) possible paths a voter can take through information processing. 18 of those paths warrant no detailed examination of observation and attribution because apathetic evaluation is an absorbing state. This means whatever path a voter was on that ends with apathetic evaluation ends without adjustment of intentions or behavior. Consistent evaluation, however, leads to behavioral adjustments according to the path the voter took in the information processing steps 1 and 2. Moreover, 4 cases seem unrealistic and are consequently omitted. These are the paths that include pro-incumbent-biased observation paired with pro-opposition-biased attribution and vice-versa. It seems unrealistic that a voter would seek biased facts about the world in one direction but hold the opposite bias when it comes to attribution. In the application of this model to concrete cases, even more combinations might be omitted due to the nature of the context. In total, this generates 14 possible distinct types. These types are summarized in Figure 6. One might add a 15th *apathetic* type, who would not react to information. Since this type of voter is not of particular interest here, we omit this category.

Next to the 2 informed voters (1, 4), there are 4 types of "misinformed" voters in terms of attribution (2, 3, 5, 6). Those voters correctly observe the state of the world but then attribute responsibilities according to their personal biases. Those

[6] Depending on the setting of retrospective voting to which this kind of model is applied, it makes sense to adapt the poles of the political spectrum accordingly, e.g., left-right, for economic voting settings. In our setting, we will later insert Bolsonaro as the de facto incumbent and the PT as the opposition. Only for now, we keep a more general description.

Figure 6: Typology of information processing: Observation and attribution



Note: This figure details the typology of voter types that arise from different paths that can be taken during the processing of politically relevant information. **O** marks nodes that belong to observation and **A** marks elements of attribution. Apathetic voters are omitted for clarity.

are the voters described by Bisgaard (2019), who hold similar beliefs about facts, but attribute responsibilities for the same facts to different political entities. Then there are voters, who are biased in their observation of the world (7 - 14), e.g., through selective information seeking (Graham and Singh, 2023; Allcott et al., 2020). Those can then again either apply correct attributions to their directionally selected observations or even assign responsibilities in the same biased way. Whether this makes a difference is subject to case-specific evaluation. However, it is easily conceivable that, if the state of the world were perceived as more negative than the true state, due to opposition-biased observation, the propensity to punish the incumbent would be stronger, when attribution is also biased. A similar logic applies to incumbent-biased observation, where attribution can then be either accurate or incumbent-biased. Here, one can think of an example, where the state of the world is perceived more positively than it is. The incumbent-biased voter would then over-proportionally credit the incumbent for the perceived positive state than the voter who perceives the same positive state but attributes responsibility accurately.

Applying the model to the context of the pandemic in Brazil

The Brazilian presidential election, after the country was severely hit by the Covid-19 pandemic offers an especially interesting case to study how voters react to a government that performed poorly by unbiased accounts but presented itself as successful, despite wildly disregarding scientifically based recommendations. To accurately capture the situation, we need to adapt the general typology of retrospective voting presented above to the specific context. The most important feature here is that the Bolsonaro administration took the opposite of a factually accurate position in its actions and communications during the pandemic. This gives partisan biased information a concrete meaning vis-à-vis accurate information. The main opposition party Partido dos Trabalhadores (PT, engl. Workers' Party), sided with the factually accurate position of the international and medical community.

We define accurate observations as correct and complete information about local and aggregated pandemic situation. Voters whose observations are opposition-biased perceive worse outcomes locally and on the aggregate. Voters maintaining opposition-biased observations perceive better outcomes locally and on the aggregate. Whether or not a voter makes decisions based on local or aggregate outcomes is captured by holistic and myopic types. The adapted model is presented in Figure 7.

Accurate attribution requires voters to correctly identify who is responsible for what. This means blaming the Bolsonaro administration for their failure on the aggregate but independent of local outcomes. Voters who are opposition-biased in their attribution, however, will attribute bad local outcomes to Bolsonaro. Evidence for the relevance of local conditions for blame attribution at the aggregate comes from the economic voting literature (e.g., Wright, 2012; Jensen et al., 2017; Baccini and Weymouth, 2021). Analogously, incumbent-biased attribution requires voters to avoid blaming the Bolsonaro administration for high local death rates and crediting the administration for lower death rates.

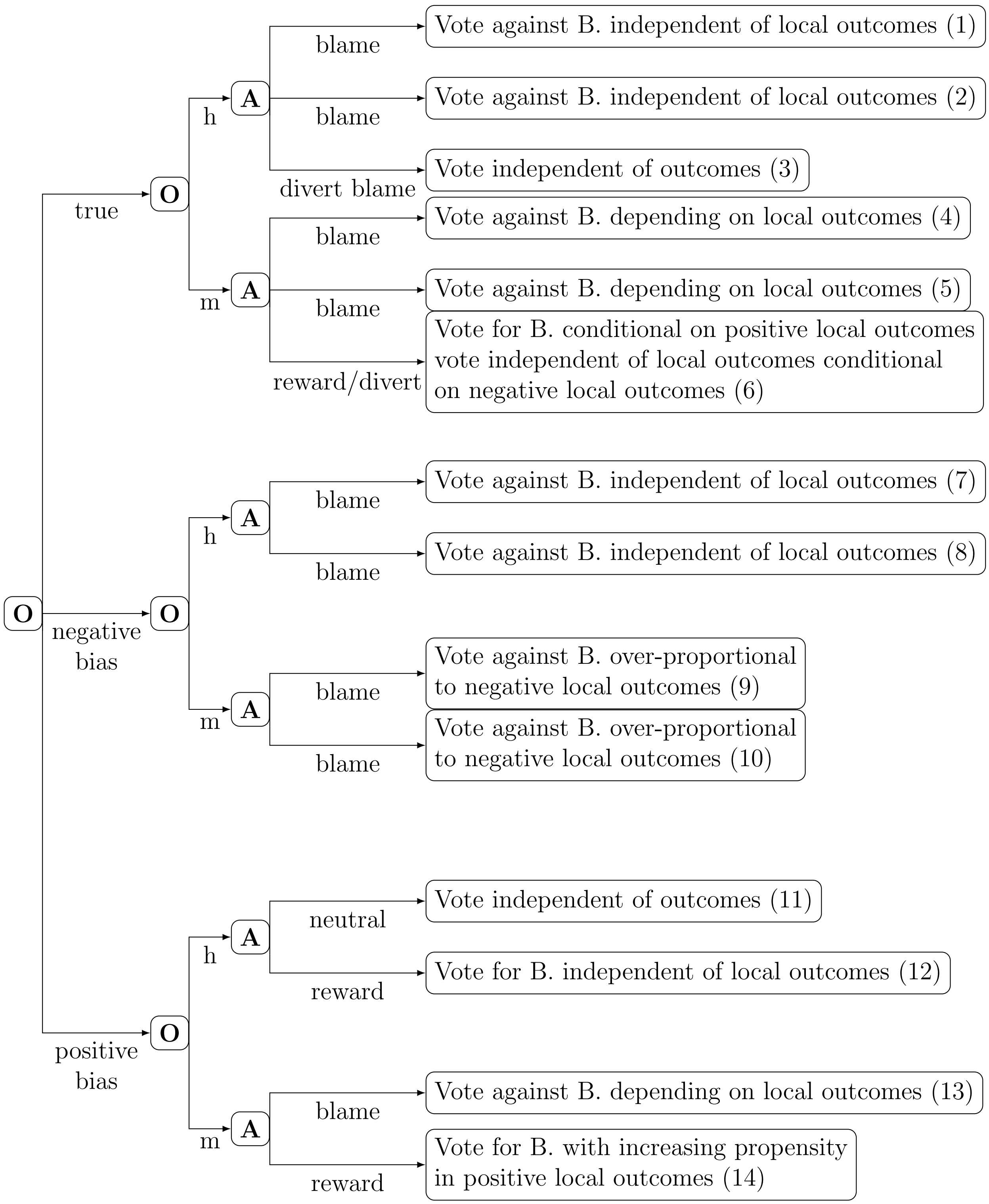
Starting with voters who accurately observe and accurately attribute responsibilities, we expect those voters to lower their propensity to vote for Bolsonaro compared to 2018 but independent of local death rates. This is because accurate attribution means that the voter understands that, if Covid-19 mortality were low in the respective location the Bolsonaro administration must not be credited for this. Maintaining the same accurate observation voters who are opposition-biased in their attribution, however, would blame the Bolsonaro administration if local death rates were high. If local death rates were low those voters would not credit Bolsonaro, and therefore, not change in their propensity to vote for Bolsonaro. Finally, voters with the same accurate observations may be biased toward the Bolsonaro administration in their attributions. Those voters would credit Bolsonaro if the local death rates were low, while they would not blame Bolsonaro in case the rates were high. Hence, the accurately informed, but incumbent-biased voter would be more likely to vote for Bolsonaro conditional on low local rates, while he or she would not change their vote in the face of high rates.

Voters whose observations are opposition-biased would perceive Covid-19 death rates as higher than they actually are. Given that they then attribute the responsibility for those higher rates accurately, they would blame the Bolsonaro administration. However, they do so independently of local rates, as they correctly identify that the Bolsonaro administration is not to blame for the local but for the overall higher rates. If voters who perceive inflated death rates are opposition-biased they would blame Bolsonaro for high local rates. Compared to voters who maintain accurate observations the propensity to vote against Bolsonaro conditional on the true death rate is then stronger because the perceived rates are higher for voters maintaining opposition-biased observations.

Voters maintaining incumbent-biased observations perceive a lower death rate. If they attribute responsibility for their observations accurately they remain neutral and do not change their voting intentions. This is because they accurately attribute responsibility for aggregate outcomes but not local outcomes to the administration. If voters maintaining incumbent-biased observations are also incumbent-biased in their attribution of responsibility, lower local rates will be credited to the Bolsonaro administration, while worse outcomes (despite some discount in perception from the bias) will not be blamed on Bolsonaro. Therefore, those voters would adjust their vote more pro Bolsonaro the lower the local death rates.

The typology helps us categorize electoral behavior with respect to variation in local pandemic experience. Holistic voters will in general vote independent of local

Figure 7: Behavioral implications of information processing



Note: This figure details the typology of voter types that arise from different paths that can be taken during the processing of politically relevant information. **O** marks nodes that belong to observation and **A** marks elements of attribution. Bolsonaro is abbreviated with B. Apathetic voters are omitted for clarity.

outcomes. Depending on their observational biases they will vote either for or against Bolsonaro or vote independent of the pandemic. A holistic voter, who observes the true state of the world but attributes the true negative outcome not to Bolsonaro (3), for example, is not affected in his or her decision whether or not to vote for Bolsonaro. The same is true for a holistic voter, with a biased observation towards Bolsonaro and accurate attribution (11). He or she perceives the overall state as better than true. Yet, attribution is accurate so he or she will not reward Bolsonaro, since he is not responsible for a positive outcome. A holistic voter with a positive bias towards the pandemic outcome and biased attribution (12), however, will reward Bolsonaro, but independently of the local experience. Lastly, holistic voters perceiving the pandemic as even worse than it was, will blame Bolsonaro independently of local outcomes (7, 8). The level of blame is higher for (8) than for (7) because (8) attributes responsibility for negative outcomes to Bolsonaro that were beyond his control.

Myopic voters will generally vote depending on the local outcomes they observe. Among those some blame Bolsonaro accurately (4), over-proportionally (5,9,10), or under-proportionally (13). The key difference among those voters is the level of blame. While (13) perceives a world where things are better than reality, and hence only has little blame to attribute towards Bolsonaro, (9) perceives the world as worse than it is, and hence attributes more blame to Bolosonaro. (10) blames Bolsonaro even more, as (10), compared to (9), even attributes unjustified blame. Myopic voters, who are biased towards Bolsonaro in their attribution, either divert blame if local outcomes are bad or even reward Bolsonaro for good outcomes (6, 14). The propensity to divert blame for (6) is higher than for (14), because (14) perceives outcomes to be more positive. Consequently, the propensity to reward is higher for (14).

B Back-of-the-envelope calculation for electoral Darwinism

The aggregate vote share of Bolsonaro in 2018 is defined as

$$S_{2018} := \frac{\sum_m q^m_{2018}}{\sum_m s^m_{2018}}. \tag{2}$$

The parameter q denotes the quantity of votes for Bolsonaro in municipality $m \in \{1, ..., M\}$ in 2018. The parameter s denotes the number of valid votes in a municipality m in 2018. So the aggregate vote share for Bolsonaro is simply calculated as the total votes for Bolsonaro over all municipalities, divided by the total number of valid votes over all municipalities.

For the first hypothetical scenario, the elements of the sums in the denominator and enumerator are weighted by the municipality-specific survival rate,

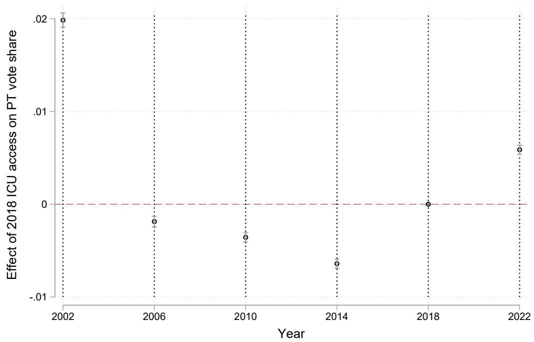
$$S^{hypothetical\ 1}_{2018} := \frac{r_m * \sum_m q^m_{2018}}{r_m * \sum_m s^m_{2018}}. \tag{3}$$

The survival rate is defined as $r_m = 1 - d_m$, where d_m is the municipality-specific death rate (in absolute terms, not per 100,000 inhabitants). The difference between the actual result and the hypothetical scenario tells us how much Bolsonaro would have lost in terms of aggregate vote share, due to municipalities with a larger share of Bolsonaro voters having a higher death rate. Compared to the true results, in the hypothetical, municipalities with higher mortality rates, contribute less to the aggregate as they lost more voters between 2018 and 2022. This assumes voters keep their choice constant and that within the municipality, the death rate is equal for Bolsonaro voters and others.

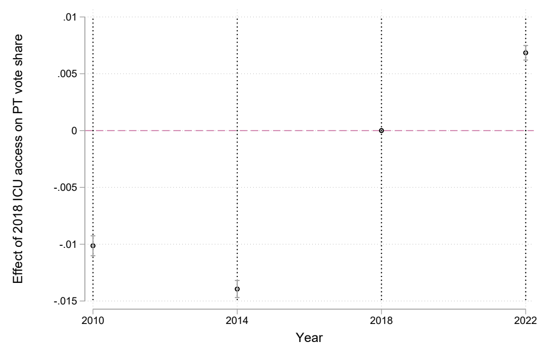
The second hypothetical scenario relaxes the latter assumption. The survival rate in the enumerator is replaced by a lower rate according to $r^b_m = 1 - d_m - d_b$, where d_b is the predicted change in death rate (not per 100,000) for a 100 pp change in municipal level vote share for Bolsonaro. According to our estimates (see Table 1) $d_b = \frac{264.8742}{100{,}000}$. The aggregate share is computed as

$$S_{2018}^{hypothetical\ 2} := \frac{r_m^b * \sum_m q_{2018}^m}{r_m * \sum_m s_{2018}^m}. \tag{4}$$

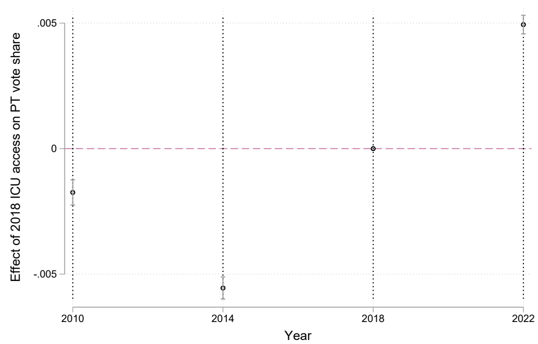
C Alternative specifications for parallel trends test



(a) Any care unit unconditional



(b) r1



(c) Any care unit unconditional

Note: The event study plots shows the coefficients interactions of election year dummies interacted with any hospital beds or ICUs, repsectively. The specification absorbs electoral district-level fixed effects and municipality-specific time trends. Panels (b) and (c) also condition on demographic control variables. Confidence intervals are obtained for the 99% level from clustered standard errors at the voting location times year level to allow for time and location-specific heteroscedasticity.

D Alternative specifications for the main analysis

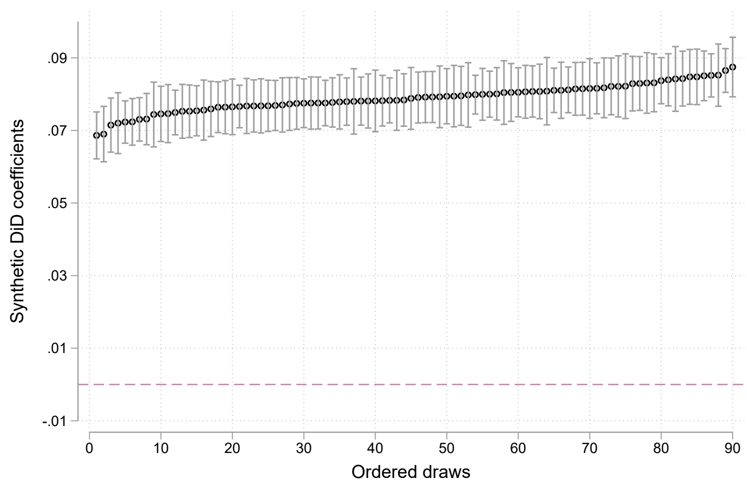
Table 3: DiD health care access on 1st round vote shares

	(1)	(2)	(3)	(4)	(5)	(6)
ICU x 2022	-0.0053	-0.0105	-0.0044			
	(0.0001)	(0.0000)	(0.0001)			
ABA x 2022				-0.0046	-0.0094	-0.0041
				(0.0001)	(0.0001)	(0.0001)
age		0.0020	0.0018		0.0021	0.0018
		(0.0000)	(0.0000)		(0.0000)	(0.0000)
education		0.1208	0.1253		0.1335	0.1228
		(0.0016)	(0.0017)		(0.0017)	(0.0017)
education^2		-0.0094	-0.0091		-0.0105	-0.0088
		(0.0002)	(0.0002)		(0.0002)	(0.0002)
sex		-0.0860	-0.0855		-0.0875	-0.0854
		(0.0011)	(0.0012)		(0.0012)	(0.0011)
married		-0.0327	0.0077		-0.0321	0.0077
		(0.0017)	(0.0018)		(0.0017)	(0.0018)
Mun.×year FE	✓		✓	✓		✓
N	872,604	872,617	872,599	872,604	872,617	872,599

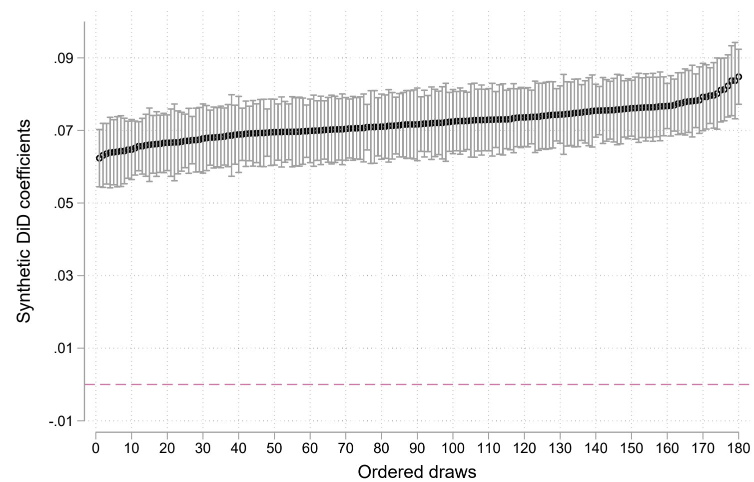
Standard errors in parentheses

Note: The outcome is Bolsonaro's vote share in the first election round. The treatment variable is health care access measured by ICU presence and any care unit respectively. All regressions in this table control for municipality-specific time trends. Standard errors are clustered at the treatment level (voting location).

E Different treatment cutoff for SDID



(a) Treatment = $\mathbf{1}(ICU > 2)$



(b) Treatment = $\mathbf{1}(ICU > 6)$

Note: The SDID model takes PT vote shares as outcomes and treatments are defined by the cut-off values as indicated. The models include demographic control variables on the electoral district level, age, gender, education, education^2, and marital status. Standard errors are clustered on the electoral district level. We report 95% confidence intervals.

F Robustness

F.1 Sample split by meso-region

Table 4: DiD sample split by Northeast region

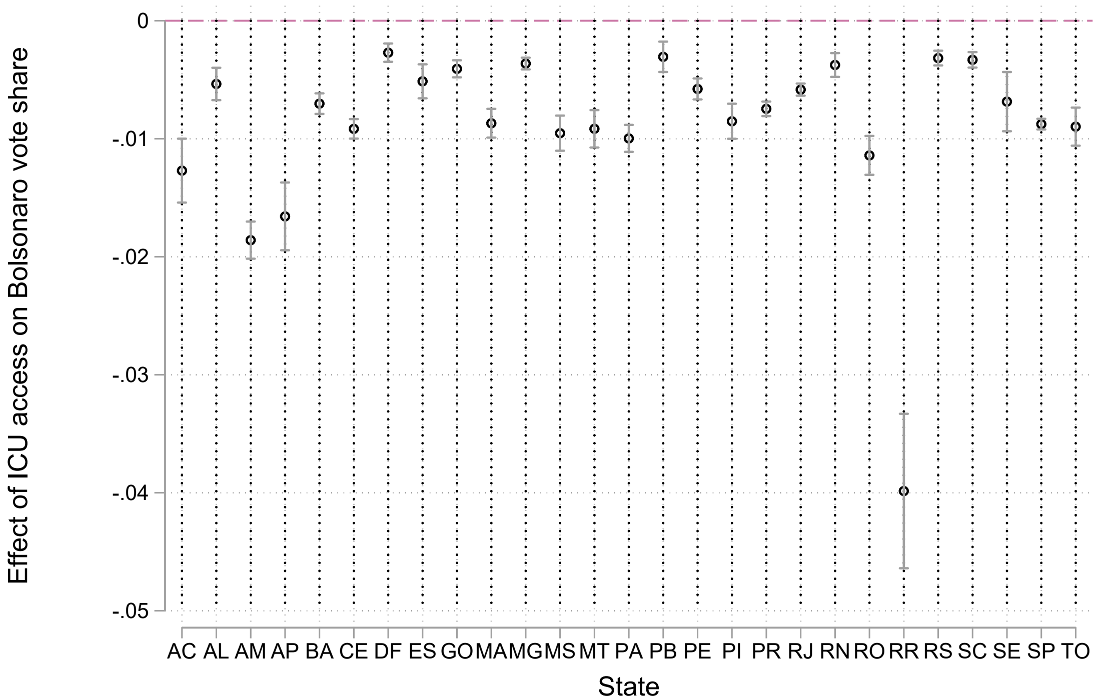
	(1)	(2)	(3)	(4)	(5)	(6)
			Non-Northeast region			
ICU x 2022	-0.0080	-0.0122	-0.0070			
	(0.0001)	(0.0001)	(0.0001)			
ABA x 2022				-0.0062	-0.0105	-0.0056
				(0.0001)	(0.0001)	(0.0001)
age		0.0025	0.0022		0.0026	0.0022
		(0.0000)	(0.0000)		(0.0000)	(0.0000)
education		0.1384	0.1354		0.1546	0.1337
		(0.0024)	(0.0025)		(0.0025)	(0.0025)
education^2		-0.0115	-0.0101		-0.0129	-0.0099
		(0.0003)	(0.0003)		(0.0003)	(0.0003)
sex		-0.0657	-0.0688		-0.0670	-0.0687
		(0.0014)	(0.0014)		(0.0014)	(0.0014)
married		-0.0509	0.0076		-0.0505	0.0073
		(0.0021)	(0.0021)		(0.0021)	(0.0021)
N	632,886	632,899	632,881	632,886	632,899	632,881
			Northeast region			
ICU x 2022	-0.0075	-0.0067	-0.0072			
	(0.0002)	(0.0001)	(0.0002)			
ABA x 2022				-0.0046	-0.0068	-0.0047
				(0.0001)	(0.0001)	(0.0001)
age		0.0015	0.0014		0.0015	0.0014
		(0.0001)	(0.0001)		(0.0001)	(0.0001)
education		0.0240	0.0488		0.0228	0.0456
		(0.0028)	(0.0028)		(0.0028)	(0.0028)
education^2		0.0030	0.0012		0.0033	0.0016
		(0.0004)	(0.0003)		(0.0004)	(0.0003)
sex		-0.0784	-0.0785		-0.0789	-0.0782
		(0.0021)	(0.0021)		(0.0021)	(0.0021)
married		-0.0195	0.0340		-0.0165	0.0342
		(0.0027)	(0.0029)		(0.0027)	(0.0029)
Mun.×year FE	✓		✓	✓		✓
N	239,718	239,718	239,718	239,718	239,718	239,718

Standard errors in parentheses

Note: The sample is restricted to observations from the Northeast meso-region or not. The outcome is Bolsonaro's vote share in the runoff election. The treatment variable is health care access measured by ICU presence and any care unit respectively. All regressions in this table control for year and unit fixed effects. Standard errors are clustered at the treatment level (voting location).

F.2 Sample split by state

Figure 10: Heterogeneous effects by state



Note: This figure reports coefficients from the DiD model with control variables and municipality-specific time trends. The outcome is Bolsonaro's vote shares in the runoff election. The treatment is ICU presence.

F.3 Sample split by death rate

Table 5: DiD by levels of death rate

Death-rate	1^{st} quartile	2^{nd} quartile	3^{rd} quartile	4^{th} quartile
ICU x 2022	-0.0063	-0.0055	-0.0083	-0.0067
	(0.0003)	(0.0002)	(0.0002)	(0.0002)
age	0.0017	0.0018	0.0025	0.0023
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
education	0.0709	0.1079	0.1524	0.1445
	(0.0036)	(0.0043)	(0.0047)	(0.0042)
education^2	-0.0022	-0.0066	-0.0115	-0.0108
	(0.0004)	(0.0005)	(0.0005)	(0.0004)
sex	-0.0697	-0.0746	-0.0751	-0.0714
	(0.0029)	(0.0024)	(0.0021)	(0.0021)
married	0.0306	0.0277	0.0067	0.0046
	(0.0041)	(0.0039)	(0.0031)	(0.0035)
N	218,070	217,065	219,149	218,315

Standard errors in parentheses

Note: For each column, the sample is restricted to observations within the respective quartile of municipality-level reported death rates. The outcome is Bolsonaro's vote share in the runoff election. The treatment variable is health care access measured by ICU presence and any care unit respectively. All regressions in this table control for year and unit fixed effects, and municipality-specific time trends. Standard errors are clustered at the treatment level (voting location).

F.4 Sample split by 2018 vote share

Table 6: DiD by 2018 vote share

2018 vote share	1^{st} quartile	2^{nd} quartile	3^{rd} quartile	4^{th} quartile
ICU x 2022	-0.0041	-0.0050	-0.0033	-0.0033
	(0.0002)	(0.0002)	(0.0001)	(0.0001)
age	0.0011	0.0015	0.0010	0.0012
	(0.0001)	(0.0000)	(0.0000)	(0.0000)
education	0.0424	0.0836	0.0549	0.0442
	(0.0032)	(0.0040)	(0.0028)	(0.0026)
education^2	0.0007	-0.0049	-0.0035	-0.0026
	(0.0004)	(0.0004)	(0.0003)	(0.0003)
sex	-0.0605	-0.0660	-0.0376	-0.0333
	(0.0025)	(0.0023)	(0.0018)	(0.0016)
married	0.0354	-0.0020	-0.0130	0.0225
	(0.0037)	(0.0029)	(0.0022)	(0.0022)
N	217,446	217,300	217,491	217,668

Standard errors in parentheses

Note: For each column, the sample is restricted to observations within the respective quartile of 2018 Bolsonaro vote shares. The outcome is Bolsonaro's vote share in the runoff election. The treatment variable is health care access measured by ICU presence and any care unit respectively. All regressions in this table control for year and unit fixed effects, and municipality-specific time trends. Standard errors are clustered at the treatment level (voting location).

F.5 Sample split by number of registered voters

Table 7: DiD by number of registered voters in a district

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
ICU x 2022	-0.0098	-0.0074	-0.0074	-0.0065	-0.0059	-0.0063	-0.0061	-0.0059	-0.0067	-0.0077
	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
age	0.0021	0.0017	0.0015	0.0015	0.0015	0.0018	0.0020	0.0020	0.0024	0.0024
	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
education	0.1124	0.0909	0.0986	0.1061	0.1238	0.1242	0.1163	0.1358	0.1563	0.1156
	(0.0086)	(0.0053)	(0.0058)	(0.0057)	(0.0050)	(0.0058)	(0.0063)	(0.0056)	(0.0064)	(0.0074)
education^2	-0.0065	-0.0042	-0.0058	-0.0065	-0.0087	-0.0091	-0.0088	-0.0110	-0.0126	-0.0082
	(0.0011)	(0.0006)	(0.0006)	(0.0006)	(0.0005)	(0.0006)	(0.0007)	(0.0006)	(0.0007)	(0.0008)
sex	-0.0754	-0.0696	-0.0613	-0.0538	-0.0606	-0.0640	-0.0581	-0.0699	-0.0707	-0.0852
	(0.0073)	(0.0044)	(0.0042)	(0.0038)	(0.0034)	(0.0033)	(0.0035)	(0.0035)	(0.0034)	(0.0035)
married	0.0847	0.0551	0.0333	0.0290	0.0355	0.0127	-0.0063	-0.0148	-0.0091	-0.0410
	(0.0117)	(0.0061)	(0.0062)	(0.0054)	(0.0048)	(0.0047)	(0.0052)	(0.0049)	(0.0043)	(0.0052)
N	84,936	84,644	84,916	87,022	84,908	87,318	86,870	80,704	89,764	88,727

Standard errors in parentheses

Note: For each column, the sample is restricted to observations within the respective decile of the number of registered voters in 2018. The outcome is Bolsonaro's vote share in the runoff election. The treatment variable is health care access measured by ICU presence and any care unit respectively. All regressions in this table control for year and unit fixed effects, and municipality-specific time trends. Standard errors are clustered at the treatment level (voting location).

F.6 Distance to closest ICU

Table 8: DiD with alternative treatment definitions: distance

	(1)	(2)	(3)	(4)	(5)	(6)
proximity × 2022	-0.0005	-0.0006	-0.0004			
	(0.0001)	(0.0000)	(0.0001)			
ICU in 50km × 2022				-0.0123	-0.0733	-0.0118
				(0.0008)	(0.0004)	(0.0008)
age		0.0028	0.0022		0.0028	0.0022
		(0.0000)	(0.0000)		(0.0000)	(0.0000)
education		0.1825	0.1281		0.1815	0.1293
		(0.0022)	(0.0019)		(0.0020)	(0.0019)
education^2		-0.0159	-0.0090		-0.0157	-0.0091
		(0.0002)	(0.0002)		(0.0002)	(0.0002)
sex		-0.0784	-0.0728		-0.0776	-0.0728
		(0.0012)	(0.0012)		(0.0012)	(0.0012)
married		-0.0575	0.0133		-0.0562	0.0133
		(0.0019)	(0.0018)		(0.0018)	(0.0018)
Mun.×year FE	✓		✓	✓		✓
N	872,604	872,617	872,599	872,604	872,617	872,599

Standard errors in parentheses

Note: The outcome is Bolsonaro's vote share in the runoff election. The treatment variable is the distance to the closest ICU in columns (1) - (3). In columns (4) - (6) the treatment is defined as a binary indicator for whether an ICU exists within 50km of the electoral district. All regressions in this table control for year and unit fixed effects. Standard errors are clustered at the treatment level (voting location).

Table 9: DiD with alternative treatment definitions: distance

	(1)	(2)	(3)	(4)	(5)	(6)
ICU in 20km × 2022	-0.0223	-0.0687	-0.0210			
	(0.0007)	(0.0004)	(0.0007)			
ICU in 30km × 2022				-0.0167	-0.0717	-0.0162
				(0.0008)	(0.0004)	(0.0008)
age		0.0027	0.0022		0.0027	0.0022
		(0.0000)	(0.0000)		(0.0000)	(0.0000)
education		0.1591	0.1287		0.1651	0.1292
		(0.0020)	(0.0019)		(0.0020)	(0.0019)
education^2		-0.0134	-0.0091		-0.0140	-0.0091
		(0.0002)	(0.0002)		(0.0002)	(0.0002)
sex		-0.0765	-0.0728		-0.0766	-0.0728
		(0.0012)	(0.0012)		(0.0012)	(0.0012)
married		-0.0572	0.0132		-0.0545	0.0132
		(0.0018)	(0.0018)		(0.0018)	(0.0018)
Mun.×year FE	✓		✓	✓		✓
N	872,604	872,617	872,599	872,604	872,617	872,599

Standard errors in parentheses

Note: The outcome is Bolsonaro's vote share in the runoff election. In columns (1) - (3) the treatment is defined as a binary indicator for whether an ICU exists within 20km of the electoral district. In columns (4) - (6) the treatment is defined as a binary indicator for whether an ICU exists within 30km of the electoral district. All regressions in this table control for year and unit fixed effects. Standard errors are clustered at the treatment level (voting location).

F.7 Sample restriction on hospital size and proximity

Table 10: Sample restriction on hospital size and proximity

	(1)	(2)	(3)	(4)	(5)	(6)
ICU x 2022	-0.0075	-0.0122	-0.0066	-0.0064	-0.0066	-0.0079
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0003)
age		0.0026	0.0022	0.0022	0.0022	0.0027
		(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0001)
education		0.1369	0.1261	0.1419	0.1459	0.1277
		(0.0020)	(0.0019)	(0.0030)	(0.0046)	(0.0086)
education^2		-0.0113	-0.0087	-0.0106	-0.0111	-0.0091
		(0.0002)	(0.0002)	(0.0003)	(0.0005)	(0.0009)
sex		-0.0755	-0.0734	-0.0764	-0.0781	-0.0839
		(0.0012)	(0.0012)	(0.0015)	(0.0021)	(0.0034)
married		-0.0649	0.0097	0.0043	0.0081	-0.0112
		(0.0018)	(0.0018)	(0.0024)	(0.0036)	(0.0050)
N	824,546	824,647	824,541	441,262	232,565	78,003

Standard errors in parentheses

Note: The outcome is Bolsonaro's vote share in the runoff election. The treatment is defined as the ICU index. The sample is restricted to observations with any care unit within a 20km distance for all columns. In columns (4) - (6) the sample is further restricted by the number of non-ICU care units within 20km. Column (4) considers only observations with more than 50 care units. Column (5) considers only observations with more than 100 care units. Column (6) considers only observations with more than 200 care units. than All regressions in this table control for year and unit fixed effects. Standard errors are clustered at the treatment level (voting location).

Table 11: Sample restriction on hospital size and proximity

	(1)	(2)	(3)	(4)	(5)	(6)
ICU in 20km × 2022	-0.0094	-0.0656	-0.0087	-0.0069	-0.0117	-0.0227
	(0.0008)	(0.0004)	(0.0007)	(0.0016)	(0.0035)	(0.0081)
age		0.0027	0.0022	0.0023	0.0023	0.0028
		(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0001)
education		0.1621	0.1332	0.1512	0.1575	0.1430
		(0.0021)	(0.0020)	(0.0030)	(0.0047)	(0.0089)
education^2		-0.0136	-0.0095	-0.0115	-0.0122	-0.0106
		(0.0002)	(0.0002)	(0.0003)	(0.0005)	(0.0009)
sex		-0.0773	-0.0737	-0.0767	-0.0784	-0.0842
		(0.0012)	(0.0012)	(0.0015)	(0.0021)	(0.0034)
married		-0.0609	0.0089	0.0035	0.0073	-0.0121
		(0.0018)	(0.0018)	(0.0024)	(0.0036)	(0.0050)
Mun.×year FE	✓		✓	✓	✓	✓
N	824,546	824,647	824,541	441,262	232,565	78,003
	Mean		Standard deviation		Min	Max
Non-ICU care units	83.8257		89.8908		0	1,014

Standard errors in parentheses

Note: The outcome is Bolsonaro's vote share in the runoff election. The treatment is defined as a binary indicator for whether an ICU exists within 20km of the electoral district. The sample is restricted to observations with any care unit within a 20km distance for all columns. In columns (4) - (6) the sample is further restricted by the number of non-ICU care units within 20km. Column (4) considers only observations with more than 50 care units. Column (5) considers only observations with more than 100 care units. Column (6) considers only observations with more than 200 care units. than All regressions in this table control for year and unit fixed effects. Standard errors are clustered at the treatment level (voting location).