# The Effects of COVID-19 Infection on Opposition to COVID-19 Policies: Evidence from the U.S. Congress*


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### Abstract

Elites' skepticism of scientific consensus presents a formidable challenge in addressing critical issues like climate change and global pandemics. While extensive research has explored the capacity of events related to these challenges to act as "exogenous shocks," motivating the general public to reassess their risk perceptions, our understanding of how elites similarly respond to such shocks remains limited. In this article, we investigate whether COVID-19 infections influenced US lawmakers' support for COVID-19 containment measures, focusing on expressed opposition to COVID-19 policies on social media and in press releases throughout the first two years of the pandemic. Employing a staggered difference-in-differences design and matrix completion methods, our analysis reveals that COVID-19 infections caused a reduction of approximately 30% in legislators' expressions of opposition to COVID-19 policies on social media. These findings underscore that elites are indeed responsive to policy shocks – even in highly polarized contexts – when they are personally affected by an issue.



***Keywords***— COVID-19, polarization, partisanship, political communication, political behavior



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# 1 Introduction

From natural disasters to pandemics, leaders worldwide confront unprecedented challenges that demand urgent responses. One critical factor influencing the timely and effective resolution of emergent policy issues in advanced democracies is the alignment between public perceptions and elite stances on these matters (Merkley and Stecula 2021; Bisgaard and Slothuus 2018; Pink et al. 2021). Indeed, extensive scholarly inquiry underscores the significant role of elite cues in shaping public attitudes toward policy problems (Kousser and Tranter 2018; Tappin 2022; Bisbee and Lee 2022), even in cases where scientific consensus overwhelmingly supports a particular stance. Recognizing the mounting influence of elite discourse in shaping public perceptions, it becomes imperative to deepen our understanding of how elites themselves develop attitudes toward pressing policy challenges. In this context, our research investigates the following question: How do personal experiences with a prominent policy issue influence the attitudes of elites? To answer this question, we focus on the attitudes of US Congress members toward the COVID-19 pandemic and explore how firsthand experiences of COVID-19 infections among members correspond to shifts in their expressed policy positions, as evident in social media engagement and official press releases.

Since the onset of the COVID-19 pandemic, governments worldwide have implemented stringent protective measures, ranging from stay-at-home orders to school closures and travel restrictions. Despite their significant impact on civil liberties and the ensuing criticism, these measures garnered increasing public support as essential strategies to combat the pandemic (Amat et al. 2020; Fetzer et al. 2020; Unan et al. 2023). Governments that enforced such measures experienced a surge in popularity, attributed to rally-around-the-flag effects (Bol et al. 2021; De Vries et al. 2021). However, amidst the perceived necessity and benefits of these protective actions, anti-science sentiments and skepticism toward state intervention emerged as prominent themes, particularly championed by populist and anti-establishment figures (Rutjens, Linden, and Lee 2021; Kreps and Kriner 2020). Notably, political discourse surrounding the pandemic became increasingly polarized, fueled in part by the proliferation of conspiracy theories associated with populist rhetoric (Debus and Tosun 2021; Eberl, Huber, and Greussing 2021; Rao et al. 2021; Stecula and Pickup 2021).

One such country where the debate about the pandemic and its consequences has been extremely polarized is the United States (Pennycook et al. 2020; Engel-Rebitzer et al. 2022).

Since the early stages of the pandemic's spread in 2020, then-President Donald Trump propagated numerous false or misleading assertions (Paz 2020; Wolfe and Dale 2020). Concurrently, Trump and congressional Republicans sought to downplay the virus's threat by questioning the accuracy of COVID-19 case counts and dismissing the experiences of healthcare professionals (LeBlanc 2020). However, this stance left little room for acknowledgment of the potential personal susceptibility to infection among the President and many of his supporters.

Previous studies have demonstrated that exposure to the adverse effects of politicized phenomena, such as extreme weather events within the context of climate change, can influence individuals' perceptions of the severity of such phenomena (Baccini and Leemann 2021), and may even prompt changes in political behavior (Visconti 2022). In this article, we contribute to this body of literature by investigating the impact of COVID-19 infection on legislators' conduct. The analysis starts by collecting the universe of US Congressmembers' social media messages on Twitter (now $X$) and official press releases. After identifying all COVID-related content using keyword searches, we then fine-tuned a pre-trained large language model (LLM) to classify the text according to whether the author expressed opposition or support for a given COVID-19 policy or measure. Leveraging data from GovTrack (2022) on the times at which different legislators were infected with COVID-19 allowed us to adopt a staggered difference-in-differences design to identify the effects of infection on opposition to COVID-19 measures. We then employ matrix completion methods and interactive fixed effects specifications to show that legislators who were infected with COVID-19 reduced their expressed opposition to COVID-19 policies such as mask mandates, social distancing, and vaccine mandates on social media and in press releases in the weeks following infection. After showing that these results are robust to several alternative explanations, we conclude that infected legislators reduced the number of opposition messages shared on social media by approximately 30 percent, suggesting that personal experience with the virus can lead to a change in policy attitudes among political elites.

The findings of the article therefore add to our understanding of how elites respond to policy shocks when they are personally affected. Namely, our findings suggest that elites may reconsider their prior policy positions when they are potentially impacted by said positions. Our findings also contribute to the literature on the effects of exogenous events on political behavior (Hersh 2013; Baccini and Leemann 2021; Visconti 2022; Newman and Hartman 2019).

Extending the focus to political elites, our findings provide additional evidence that external shocks can influence political communication and behavior. In the next section, we highlight the literature on policy shocks and COVID-19 attitudes. The third section provides the research design, and the fourth section presents the results. We then outline a number of robustness checks that confirm the substantive findings of the primary analysis, before a discussion on the wider implications of our analysis.

## 2 COVID-19 Opposition and Elite Cues

The sizable literature on socio-psychological correlates of COVID-19 perceptions demonstrates that various factors are associated with attitudes toward the pandemic and protection measures, including psychological traits, trust, risk attitudes and reliance on intuition (Esaiasson et al. 2021; Fetzer et al. 2020; Xu and Cheng 2021; Steffen and Cheng 2021). In particular, extant research shows that individuals tend to underestimate health risks associated with COVID-19, are influenced considerably by framing effects in relation to the pandemic, and display what came to be known as "acceptance bias" when encountering misinformation related to the pandemic (Newman, Lewandowsky, and Mayo 2022; Olmastroni et al. 2021; Pearce and Cooper 2021; Steffen and Cheng 2021; Rachev et al. 2021). Individuals also greatly underestimate their likelihood of contracting and carrying the virus, showing 'unrealistic optimism' for themselves and close others (Salgado and Berntsen 2021). Once infected, however, they may quickly update their perceptions; some – especially those in high risk groups – may even go so far as to overestimate the degree to which they share similar levels of risk with others, as predicted by false consensus bias (Ross, Greene, and House 1977). Taken together, such biases in the human cognitive architecture help explain why lockdowns, face masks and other protection measures became polarizing issues in various political contexts.

Although politicians might be expected to be more welcoming of stringent protection measures, as certain traits such as education and proximity to expert knowledge mitigate the acceptance and persistence of misinformation in relation to COVID-19, a small but growing literature on misperceptions among political elites points to similar cognitive biases that are prevalent also among the public (Broockman and Skovron 2018; Sheffer et al. 2018; Hertel-Fernandez and Stokes 2019). Kertzer (2022), for instance, directly challenges the notion of elite exceptionalism

– the idea that political elites are remarkably different than the public when it comes to decision making – by convincingly showing that elites and masses respond to treatments in experiments in a strikingly similar way.

Not only can political elites have distorted perceptions of constituency opinion (Broockman and Skovron 2018; Pereira 2021), they may also show resistance to becoming more knowledgeable about constituency matters, and they tend to downplay or discount the opinions with which they disagree, even despite electoral pressures (Kalla and Porter 2021; Broockman and Skovron 2018; Pereira and Öhberg 2020). Studies examining elites' dispositions and leadership skills from an evolutionary perspective argue that elites are particularly prone to exhibiting displays of overconfidence (see Van Vugt 2006; Sheffer and Loewen 2019 for an overview). These studies lend strong support for theories of decision-making that assume universalistic cognitive biases that apply to both elites and masses (e.g., Simon 1957; Tversky and Kahneman 1974; Jones 2001).

While there is little to no empirical research as to whether and to what extent politicians differ from individuals in the broader population in perceptions of the COVID-19 pandemic, the literature cited above provides some initial clues. Studies based on different political contexts, including North America, Britain and France, show that individuals greatly underestimate the risk of COVID-19 infection (Asimakopoulou et al. 2020; Attema et al. 2021; Wise et al. 2020). Moreover, it is hard to deny the realities of growing ideological polarization in contemporary American politics and its role in structuring beliefs about science. Recent research shows that political ideology is strongly correlated with science skepticism in general (Rutjens, Linden, and Lee 2021) and COVID-19 attitudes in particular (Debus and Tosun 2021; Evans and Hargittai 2020). World leaders such as Jair Bolsonaro, Rodrigo Duterte, and Donald Trump, previous heads of state in Brazil, the Philippines and the US, respectively, have publicly downplayed the health-related risks associated with the pandemic on numerous occasions (Lasco 2020). Indeed, Kavakli (2020)'s cross-national analysis of government responses to the pandemic confirms that COVID-19 measures adopted by governments are strongly correlated with government ideology, with populist governments being sluggish in adopting drastic measures. In line with this view, Bayerlein et al. (2021) theorize that both citizens and political elites in a populist and highly polarized political environment are more likely to underestimate the consequences of the pandemic.

Perhaps intuitively, skepticism and outright opposition may be mitigated by exogenous events that require an individual to grapple with an issue they are skeptical of or an event that causes personal ramifications for the individual. This proposition has been demonstrated in studies on the effects of extreme weather events on attitudes about climate change. For example, Baccini and Leemann (2021) find that Swiss voters increase support for pro-climate policies by up to 20 percent following a personal experience with a flood. Also examining the influence of floods, Visconti (2022) explores the extent to which Chilean voters shift their voting preferences in the direction of pro-climate politicians. The author exploits variation in exposure to flood severity to find that individuals who experience more severe flooding damage increasingly prefer politicians who similarly view climate change as a greater threat.

A personal experience with an event that causes harm or damage to an individual can have profound effects. In the context of COVID-19, research has shown that unlike social pressure or the advice of medical professionals, personal perceptions of risk shape the behavior of individuals (Sakakibara and Ozono 2020; Wise et al. 2020). Wise et al. (2020) document that the perceived probability of personally being infected is a strong predictor of compliance with protection measures. Latkin et al. (2021) report a similar finding: people who endorse COVID-19 skepticism statements are less likely to believe that they and close others would die from COVID-19. One implication of this is that elected officials – just like individuals in the public with whom they share similar cognitive characteristics (Sheffer et al. 2018; Arceneaux, Dunaway, and Soroka 2018) – likely underestimate the risk of COVID-19 infection and express opposition to stringent protection measures, especially given how polarized the American public is regarding the issue of pandemic policies (Gadarian, Goodman, and Pepinsky 2021).

Consequently, when there are low perceptions of risk from being infected with COVID-19, it is easier to downplay the virus and ignore its potential effects. However, once that risk is realized – either in the case of experiencing an extreme weather event or with being infected by COVID-19 – individuals often reassess the threat level of the risk. We apply this perspective to outline our theoretical expectations for the effects of COVID-19 infection on legislators' revealed preferences regarding opposition to COVID-19 policies. We hypothesize that there are three co-constitutive factors at play that result from COVID-19 infection. First, infection may reduce opposition to COVID-19 by causing legislators to update their perceptions of the level of risk involved. Namely, legislators who downplayed the risks of the virus may do so because their reasoning is shaped by their partisan affiliation (Taber and Lodge 2006), and they may not expect to face tangible personal repercussions from their policy positions on COVID-19. Once infected, legislators may find it more challenging to maintain the partisan illusion that COVID-19 does not pose a substantive threat to their own and others' well-being, and therefore may update their risk perceptions.

Second, legislators may reduce their expression of opposition to COVID-19 measures due to personal strategic considerations. Policy reversals are costly (i.e. "flip-flopping"), so legislators face strong electoral incentives to maintain their stance as part of an attempt to preserve an image of competence (Andreottola 2021). This effect was likely further magnified because journalists were quick to point out the alleged hypocrisy of representatives whose behavior differed from their partisan claims about COVID-19 measures (LeBlanc 2020). Yet, not *all* policy reversals are costly. When representatives change positions to move in the direction of public opinion, the benefits of the reversal can outweigh the costs associated (Doherty, Dowling, and Miller 2016). Consequently, when representatives experience a shock in the form of COVID-19 infection, they may take the opportunity to strategically shift to be more in line with public opinion on the matter, as well as to avoid further scrutiny from both the media and voters. With that said, remaining at odds with cues from the party leadership and in-partisan voters for an extended period of time might harm the electoral prospects of the legislator. This implies that legislators who get infected by COVID-19 may return to their initial stance on pandemic measures after temporarily reducing their opposition.

Additionally, it is certainly possible that some legislators, despite being fully aware of the risks associated with COVID-19, may have strategically chosen to downplay the risks for perceived advantages, namely, to align with the vote base in the regional constituency or with the party leadership. Consequently, the shift in their stance following infection may be less attributable to an altered risk assessment or partisan-motivated reasoning and more indicative of a necessity to mitigate the perceived implications of having contracted the virus after downplaying its severity. Upon being infected with the virus, legislators may tone down their expressions of skepticism about the virus and protection measures with the aim of reducing negative attention.

Another possibility is that legislators who are infected with COVID-19 may be asymptomatic and therefore may not experience the same effects as those who experience more severe symptoms. Nonetheless, we still expect these legislators to update their risk perceptions for two reasons. First, even if they are asymptomatic, legislators are likely to be excluded from representative duties and therefore may still experience the same social pressure regardless of severity. Second, asymptomatic infection still carries with it the possibility of infecting the legislator's colleagues, family members and constituents. Therefore, asymptomatic individuals, or individuals that have only mild cases of COVID-19, may still update their prior beliefs about the chances of future infection for themselves and those around them.

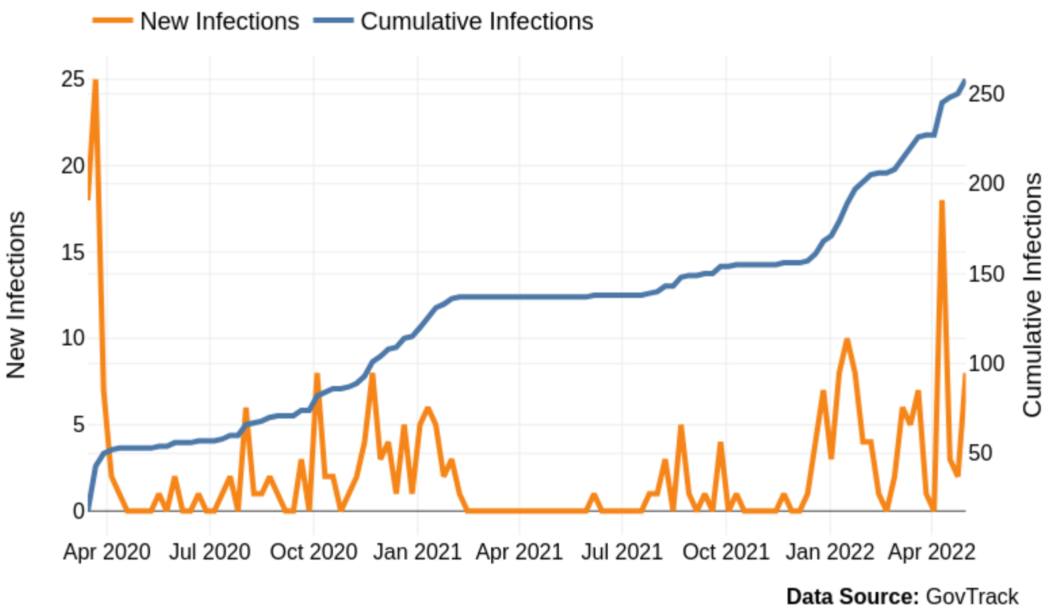
# 3 Research Design

Since the beginning of the pandemic, COVID-19 infection has been common among elected officials from around the world, including high profile politicians such as Boris Johnson, Joe Biden, Donald Trump, Emmanuel Macron, and Jair Bolsonaro, just to name a few. US legislators were no exception. Within three months of the first confirmed case in the US on January 21, 2020, 53 members of the US Congress had been infected with the virus (GovTrack 2022). By the end of 2020, that number would rise to 118 congressmembers. In total, from the time between January 2020 and May 2022, 230 legislators were infected 258 times (several legislators were infected multiple times). We focus on this time period in our analysis.

We rely on data from GovTrack (2022) for the dates at which legislators were infected with COVID-19. The GovTrack team compiled data on legislators' COVID-19 infections using a number of social and traditional media sources, and has produced high-quality legislative data in other domains as well. Figure 1 presents the dates at which legislators were infected with COVID-19. In the figure, new cases are presented in orange (first y-axis) and cumulative cases are presented in blue (second y-axis).

Although GovTrack made a concerted effort to identify all legislators who were infected with COVID-19 and provides sources for each observation, it is possible that some legislators were infected and did not report their infection or did not show symptoms. In the latter scenario, we do not expect an effect because COVID-19 itself would not have led to a change in the individual's environment. However, the former scenario presents an opportunity for bias if there are systematic differences between legislators who hid infection and legislators who did not. There are a few reasons why we do not expect systematic biases in reporting about COVID-

**Figure 1:** COVID-19 Infections Among US Legislators




**Note:** Figure displays weekly infections among US Members of Congress. New infections (orange) correspond with the first y-axis. Cumulative infections (blue) correspond with the second y-axis. Data source: GovTrack (2022).

Figure 1 **Alt Text:** The figure shows the weekly number of new and cumulative infections. The figure indicates that infections occurred 258 times in total.

19 infection among US legislators. First, legislators were required to be tested during several periods (Bresnahan and Caygle 2020) in order to attend events such as the State of the Union Address (NPR 2022). Second, hiding COVID-19 infection is likely to be a challenge for US legislators given a combination of age and public status. Individuals who are over 50 are at a significantly greater risk of hospitalization and mortality from COVID-19 (Biswas et al. 2021), and previous research suggests that older individuals are aware of such risks (Bruine de Bruin 2021). Given that the average age of a US legislator is close to 60 (Cillizza 2021), it would likely be challenging to avoid requiring medical attention – along with the publicity that entails – in the case that legislators were hiding COVID-19 infection. While we proceed in our primary analysis under the assumption that the GovTrack data on legislators' COVID-19 infections are accurate, we return to examine how systematic differences in reporting may affect our results in subsection 5.1.

## 3.1 Measuring Opposition to COVID-19 Policies

We refer to legislators' communication on Twitter (now called $X$) to understand legislators' opposition to policies aimed at reducing the spread of COVID-19. Communication on social media, and Twitter in particular, can be a good measure of dynamic political attitudes and preferences of elites given the frequency with which they share their views online. Twitter also likely provides legislators more leeway to dissent from their party's position without facing the kinds of repercussions associated with breaking with the party in the legislature. Moreover, legislators regularly use Twitter to discuss political issues and communicate with constituents. For instance, Russell (2021) argues that legislators value Twitter because it offers "a birds-eye view" of national public attention, offering US representatives the opportunity to use Twitter to build policy reputations with constituents. Additionally, Twitter messages sent from politicians are a strong source for understanding elites' political interests and issue priorities due to the level of conciseness required in a message that is limited to 280 characters. Elites are also over-represented on Twitter – nearly every US legislator has an active Twitter account and engages on the platform.

To understand elites' individual preferences on COVID-19 containment policies, we identified related messages sent by elected representatives in the US House of Representatives and the Senate. We first collected every message sent from US MCs during the time period from January 2020–May 2022. The messages were then filtered using keyword searches pertaining to COVID-19. We used the keywords "covid", "corona", "sars-cov-2", "vaccine", "virus", "mask", "lockdown", and "social distanc".[1] The aim of the keyword search was simply to identify messages that explicitly concerned measures aimed at COVID-19 related policies. After the keyword search, there were 84,206 messages that were about COVID-19 and/or a policy aimed at reducing its impact.

The next step was to classify tweets according to whether they expressed opposition to COVID-19 related policies. For this, we fine-tuned a large language model (LLM) based on the BERT architecture (Devlin et al. 2018). The BERT transformer architecture allows for training and fine-tuning language models and achieves state-of-the-art performance on a number of different tasks. For our task, we used the BERTtweet model (Nguyen, Vu, and Tuan Nguyen

1. We used "social distanc" to capture both "social distance" and "social distancing." URLs were filtered out of messages before conducting the keyword searches.

2020), which was pre-trained on a corpus of 850 million English tweets, and then fine-tuned on an additional 23 million tweets related to COVID-19. Using this model as our base model, we then further fine-tuned the model on an additional 10,000 tweet sample from our dataset of COVID-19-related tweets. We hand-coded this sample of 10,000 tweets according to whether each message expressed opposition to a COVID-19 policy or procedure. We then used the fine-tuned model for inference on the remaining tweets. This method was impressively effective, and our model achieved an F1 score of 0.95 on a held-out test set. Additional evaluation criteria and validation for the model are provided in Appendix A.

One of the most important steps in creating a supervised language model for classification is ensuring that the data used to train the model are accurate. The tweets varied widely, and ranged from messages that recommended outright refusal to wear masks, to false claims about the intention of government policies aimed at curbing COVID-19. The vast majority of the tweets were easy to label; however, a few were more challenging. For instance, a clear example of a tweet from the dataset that did not express opposition to COVID-19 policies includes the following, sent from Rep. Rob Wittman on May 28, 2020:

> I will continue to lead our commonwealth as we navigate the federal, state, and local response to the health and economic impacts to the coronavirus. We must continue to practice social distancing to keep our nation and communities safe.

This instance clearly shows no indication of opposition to COVID-19 restrictions, and in fact encourages the public to take recommended precautions. The majority of messages about COVID-19 fell into this category. An example that we labelled as expressing opposition to COVID-19 measures includes the following, sent from Rep. Bob Good on July 28, 2021:

> The House has reinstated its draconian mask mandate once again. My amendment would end taxpayer funding for mask mandates in the Capitol complex. Follow the science – no more mask mandates!

From the message above, it is clear that Rep. Good opposes the mask mandates. This message was coded as expressing opposition.

We also provide an example of the type of messages that our model did not correctly classify. Although the model achieves a high level of accuracy, there are still some cases in which errors are made. To illustrate such an error, the following message from Thomas Massie in August, 2021 is an example of a tweet that our model incorrectly predicted as not expressing opposition:

> On the subject of mandatory vaccines imposed by private employers... if this is a matter of private contracts, and not a matter of criminal law, what's the penalty for providing false vaccine information to an employer? termination? the same as refusing to take the vaccine?

Although Rep. Massie does not explicitly challenge vaccine policies for private employers, he implies that individuals who find themselves in a situation in which they do not want to take the vaccine face the same risk (termination) regardless of whether they lie about taking the vaccine or outright refuse to take it. Implicit opposition to COVID-19 measures is harder to capture using text alone here, which is likely why the model misclassified the tweet and did not identify it as expressing opposition.

To add confidence in our classifications and to minimize the possibility that downstream statistical estimates are based on classification errors, we re-classified all the messages using GPT-3.5 – the model underlying OpenAI's ChatGPT (OpenAI 2022). Previous studies have found that GPT-3.5 out-performs human annotators on a number of different annotation tasks (Gilardi, Alizadeh, and Kubli 2023), which makes the model especially useful for our objectives. Using the OpenAI API, we used the model as a zero-shot classifier to classify each of the tweets in our dataset on the same opposition–support dimension as the BERTtweet model. We then compared the predictions of our fine-tuned BERTtweet model with the predictions of GPT-3.5. In all cases of discordance between GPT-3.5 and our model's predictions, we manually labelled the message. In total, 94.1% of the messages were similarly labelled by GPT-3.5 and our model.[2]

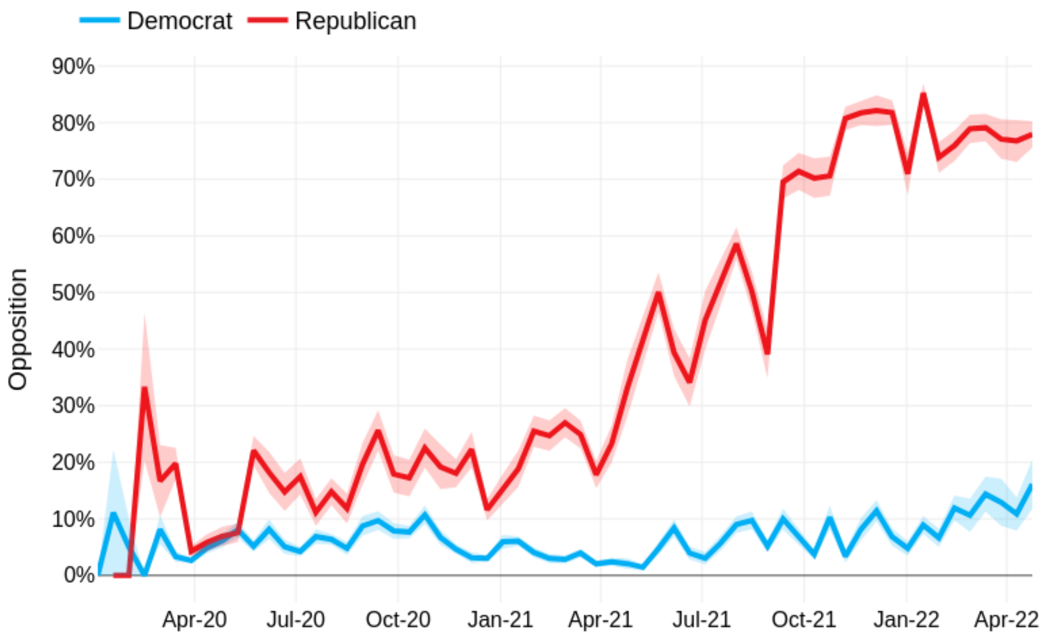
After each of the messages were classified according to whether they expressed opposition to COVID-related policies, we created a new dataset with bi-weekly measurements of tweets sent in opposition to COVID-19 restrictions, as well as bi-weekly measurements of the total number of tweets sent about COVID-19 in general. Measurements were made at the individual legislator level for every elected legislator who sent a tweet about a COVID-19 measure at any point between January 2020 and May 2022.

In order to provide a sense of our measure of opposition over time, Figure 2 presents dynamic

2. We provide full details of the classification procedures in Appendix A and descriptive statistics in Appendix B.

opposition to COVID-19 policies by political party. The figure indicates that opposition to COVID-19 policies has been relative stable for Democrats, but has increased dramatically for Republicans, especially around the time Joe Biden took office in January 2021.

**Figure 2:** Opposition to COVID-19 Policies by Political Party




**Note:** Figure displays the proportion of tweets that expressed opposition to COVID-19 policies by political party affiliation.

Figure 2 **Alt Text:** The figure shows the proportion of tweets that expressed opposition to COVID-19 policies by political party affiliation. The figure indicates that opposition to COVID-19 policies has been relative stable for Democrats, but has increased dramatically for Republicans. This increase is especially pronounced at the start of 2021.

Figure 2 highlights the fact that opposition to COVID-19 policies is highly partisan. This is not surprising given the highly polarized nature of the COVID-19 pandemic in the US (Gadarian, Goodman, and Pepinsky 2021). We return to further consider partisan asymmetries in opposition to COVID-19 policies in robustness checks in subsection 5.1.

## 3.2 Identification and Estimation Strategy

We adopt a staggered difference-in-differences design to recover an estimate for the effects of COVID-19 infection on legislators' expressed opposition to COVID-19 policies. We rely on variation in the timing of infection to identify the effects of COVID-19 infection. Specifically, we exploit sharp discontinuity in the four weeks right before and four weeks directly after COVID-19 infections. Our design therefore takes into consideration the possibility that legislators may have been infected with COVID-19 multiple times, while also still taking into consideration the dynamic nature of COVID-19 symptoms, which tend to have a duration of approximately 3-4 weeks (Mizrahi et al. 2020).

The primary identifying assumption of our research design that is required in order to recover a causal estimate is the parallel trends assumption. In order for there to be parallel trends in the outcome variable, the specific times (e.g. dates) at which legislators are infected with COVID-19 must be as-good-as random. In other words, legislators cannot select the dates on which they become infected with COVID-19. It is worth noting that our assumption is *not* that COVID-19 infections are exogenous, as infection is almost certainly correlated with attitudes about COVID-19. Moreover, we do not assume that the times of infection are exogenous in the wider population, which is also not likely to hold. Rather, we assume that conditional on being infected with COVID-19, expressed opposition to COVID-19 policies in the four weeks before infection should not systematically differ from opposition in the four weeks after infection, *in the absence of infection.* This assumption – also commonly referred to as the parallel trends assumption – is standard in difference-in-differences design (Card and Krueger 1993; Cunningham 2021), and we provide several pieces of evidence to support this crucial assumption.

First, we perform pre-treatment equivalence tests in order to test for trends in the pre-treatment periods (Liu, Wang, and Xu 2022). These tests examine the degree to which trends in the outcome variable (opposition) differs between the infected and not-yet-infected legislators in the pre-treatment period (i.e. before infection). We find no support for the hypothesis that pre-trend differences between the two groups exist. These results are presented in Appendix C. Second, we present coefficient estimates for the time period *before* COVID-19 infection, which allows for visual inspection of the pre-treatment trends in the outcome variable. In the case that the times at which infections occur was correlated with an unobserved variable, we would expect that the coefficient estimates could be statistically differentiable from zero in the time before the treatment. We find no evidence that the pre-treatment coefficients are statistically distinguishable from zero in the pre-infection period and present those estimates below in Figure 3.

### 3.2.1 Estimation

Our estimation strategy requires accommodating several features of the data. Namely, our data includes staggered treatment timing in which legislators can be infected with COVID-19 at different time periods. Additionally, legislators can be re-infected multiple times, and we expect that the effects of infection should be more apparent in the weeks following infection compared to much later. Traditional approaches to estimation in similar settings have relied on two-way fixed effects specifications in which time and unit effects are fixed. However, the recent econometrics literature has demonstrated that two-way fixed effects regressions can produce biased estimates in staggered settings (Sun and Abraham 2021; Callaway and Sant'Anna 2021; Baker, Larcker, and Wang 2022). We therefore rely on matrix completion methods to estimate the effects of infection (Athey et al. 2021).

Matrix completion methods are a class of causal estimators for panel data settings that estimate counterfactual outcomes for treated units by imputing missing values in the outcome variable. In our case, matrix completion methods estimate the counterfactual outcomes for each legislator in the weeks following infection (while under "treatment"). The difference between the observed and imputed values is then used to calculate an individualistic treatment effect. Each of these individual effects are then aggregated to produce estimates for the effect of the intervention – in our case, the average treatment effect on the treated for COVID-19 infection. Importantly, matrix completion methods provide more reliable causal estimates than two-way fixed effects models in the case of heterogeneous treatment effects or when there are potential time-varying confounders that are unobserved (Liu, Wang, and Xu 2022).[3]

Matrix completion is especially relevant for our research design because it accommodates staggered treatments, as well as multiple treatments that switch on and off for the same unit. Moreover, the method allows for the inclusion of unit and time fixed effects, which allow us to control for time-invariant characteristics of legislators such as partisan and gender differences.

In order to recover causal estimates from the matrix completion estimator and other counterfactual estimators like the synthetic control method (Abadie, Diamond, and Hainmueller 2010), treated units must not anticipate treatment. In other words, infected legislators must not anticipate infection. This is indeed a strong assumption to make in our observed data given that legislators may not report COVID-19 infection immediately after becoming aware. Should

3. For a full overview of this method, see Athey et al. (2021) or Liu, Wang, and Xu (2022).

this be the case, it would work *against* our hypothesis that COVID-19 infection reduces opposition by biasing our estimates toward zero. It is therefore possible that the effects of infection are greater than what we report.

In all estimations, we include unit and time fixed effects, and we report estimates with and without COVID-19 cases as a control variable (measured in the US state associated with the legislator's constituency). In all estimates, the treatment indicator variable is a binary variable that takes the value of 1 in the four weeks after each COVID-19 infection and is 0 in all other cases. Our estimand is therefore the average treatment effect on the treated (ATT) of COVID-19 infection on legislators' opposition to COVID-19 policies over the four weeks following infection. All estimations are performed using the `fect` library in R (Liu, Wang, and Xu 2022).

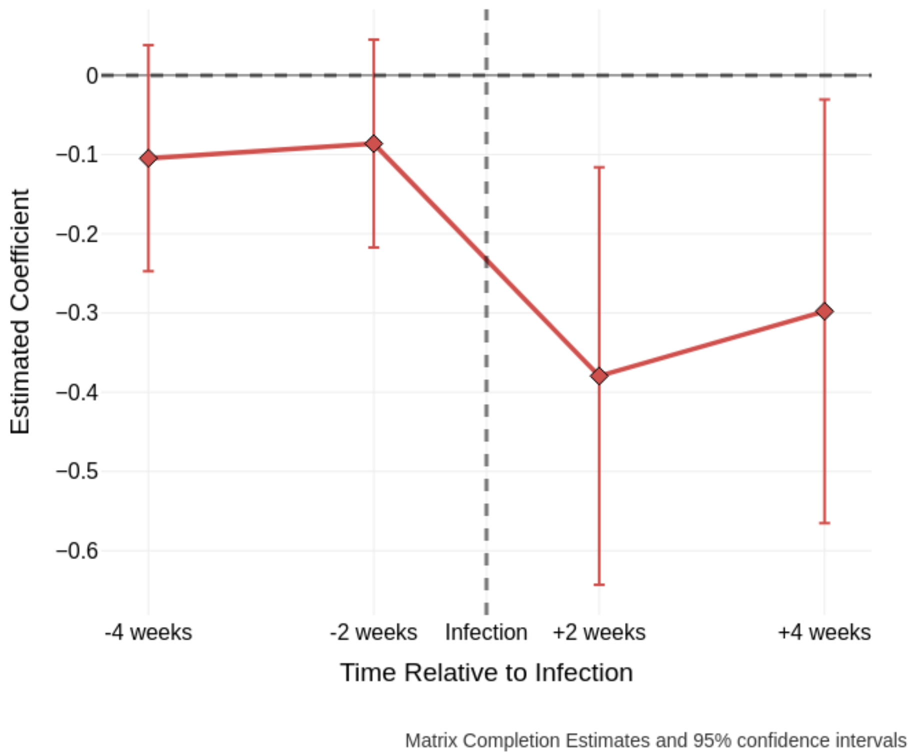
## 4 Results

Our theoretical expectations outlined above suggest that COVID-19 infection may lead legislators to reduce their expressed opposition to COVID-19 mitigation measures following personal experience with symptoms of the virus. Figure 3 presents the ATT estimates for the four weeks before and after infection. In the figure, the vertical line represents the date of infection. The first two coefficient estimates from left to right are the ATT estimates for four and two weeks before infection, respectively. These estimates are presented with 95% confidence intervals, and suggest that the level of expressed opposition in the four weeks before infection is relatively stable and is not statistically distinguishable from zero, which provides support for the parallel trends assumption.

The two coefficient estimates *after* infection are the ATT estimates for two and four weeks after infection, respectively, for the entire population of infections among lawmakers. In line with our theoretical expectations, these estimates are statistically distinguishable from zero, and suggest that expressed opposition to COVID-19 policies decreased by approximately 0.3–0.4 messages over the following four weeks. In other words, legislators who were infected with COVID-19 sent approximately 0.3–0.4 fewer messages expressing opposition to COVID-19 policies in the four weeks after infection than they did in the four weeks before infection.

We additionally consider the cumulative effects over the duration of infection. In Table 1, we present the cumulative effects of all infections on opposition to COVID-19 policies using

**Figure 3:** Effects of COVID-19 Infection on Opposition to COVID-19 Policies




**Note:** The figure displays the ATT estimates with 95% confidence intervals for the four weeks before and after COVID-19 infection. The specification includes unit and time fixed effects and an inverse hyperbolic sine transformation of the outcome variable, corresponding with Model 1 in Table 1.

Figure 3 **Alt Text:** The figure shows the ATT estimates with 95% confidence intervals for the four weeks before and after COVID-19 infection. The figure shows four coefficient estimates, with each representing a two week interval. The two estimates before infection are not statistically differentiable from zero, while the two estimates after infection are. The estimates suggest that expressed opposition to COVID-19 policies decreased by approximately 40% in the first two weeks after infection, and about 30% in weeks 3 and 4.

several different specifications. Model 1 uses an inverse hyperbolic sine transformation of the outcome variable, which is a common transformation used in count models. Model 2 uses the logarithmic transformation of the outcome variable, which is also a common transformation used in count models. Model 3 includes the number of COVID-19 cases per day in each legislator's constituency state. Model 4 includes the number of new cases and new deaths in each legislator's constituency state.

Across each of the specifications, the results are consistent with our theoretical expectations. In all models, the coefficient estimate for the ATT is negative and significant at the 95% confidence level. The coefficient estimates for the ATT range from -0.327 to -0.329, which suggests that legislators who were infected with COVID-19 sent approximately 32% fewer messages expressing opposition to COVID-19 policies in the four weeks after infection than they did in the four weeks before infection. The results also appear to be robust to the inclusion of COVID-19 cases and deaths in the legislator's constituency state.

**Table 1:** Cumulative effects (ATT) of COVID-19 Infection on Opposition to COVID-19 Policies

| | *DV: Opposition Messages* | | | |
|---|---|---|---|---|
| | IVHS | log+.1 | w/COVID-19 cases | w/cases and deaths |
| COVID-19 Infection (ATT) | -0.327** | -0.327** | -0.329** | -0.327** |
| Standard error | (0.118) | (0.122) | (0.126) | (0.112) |
| CI lower (2.5%) | -0.559 | -0.565 | -0.575 | -0.546 |
| CI upper (97.5%) | -0.095 | -0.088 | -0.082 | -0.108 |
| P-value | 0.006 | 0.007 | 0.009 | 0.003 |
| N. Obs. | 36,722 | 36,722 | 36,722 | 36,722 |
| COVID-19 cases | | | ✓ | ✓ |
| COVID-19 deaths | | | | ✓ |
| Time FE | ✓ | ✓ | ✓ | ✓ |
| Legislator FE | ✓ | ✓ | ✓ | ✓ |

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

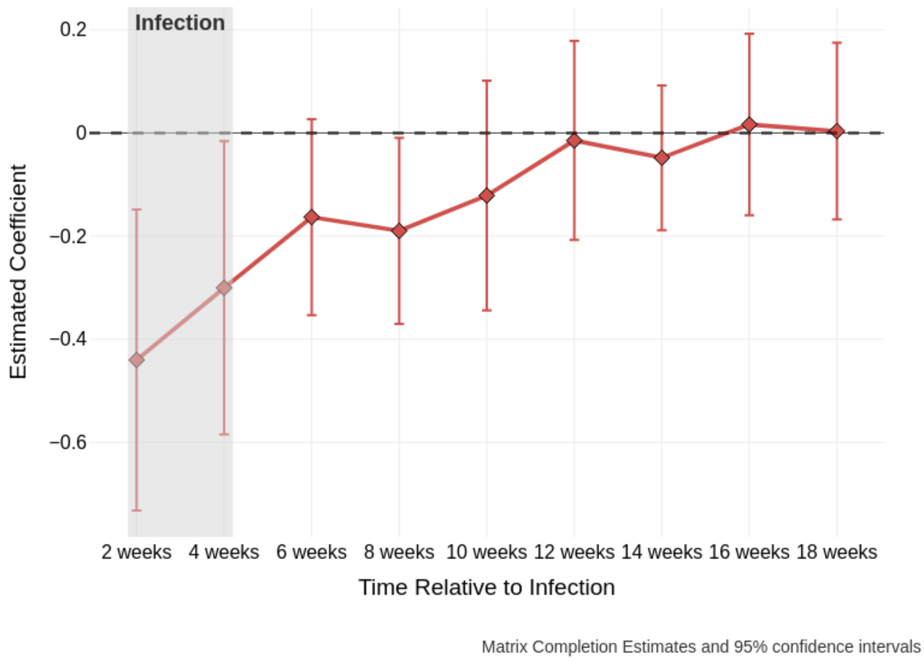
**Note:** Standard errors are presented in parentheses. All results presented use matrix completion methods and are estimated using the `Fect` library in R (Liu, Wang, and Xu 2022). Models 1 and 2 use an inverse hyperbolic sine transformation and log+.1 transformation, respectively. Model 3 includes the number of COVID-19 cases per day in each legislator's constituency state. Model 4 includes the number of new cases and new deaths in each legislator's constituency state.

## 4.1 How Long Do the Effects Last?

The results suggest that legislators respond to COVID-19 infection by reducing opposition to COVID-19 policies in the four weeks after infection. However, it is unclear how long the effects of infection on opposition last beyond the four week period. We therefore consider the duration of the effects by estimating the effects of infection after the four week duration following infection. Although we find limited evidence that the effect lasts after "exiting" the treatment (e.g. after recovering from COVID-19 following a 4-week infection), the figure provides suggestive evidence that lawmakers do not return right back to expressing the same level of opposition. Rather, the effect is still somewhat evident approximately two months after infection.

Nonetheless, the exit effects presented in Figure 4 suggest that infection reduces opposition for only a short period of time, with opposition returning fully to mean levels after approximately 12 weeks. This result indeed provides further support for the hypothesized effect of infection on opposition, but it also highlights the limitations of infection and the ability to create fundamental changes in lawmakers' policy positions toward COVID-19.

**Figure 4:** Carry-over Effects of COVID-19 Infection on Opposition to COVID-19 Policies




**Note:** Figure displays the ATT estimates with 95% confidence intervals for 18 weeks after COVID-19 infection. The specification includes unit and time fixed effects and an inverse hyperbolic sine transformation of the outcome variable, corresponding with Model 1 in Table 1.

Figure 4 **Alt Text:** The figure shows the ATT estimates with 95% confidence intervals for the four weeks after COVID-19 infection. The figure shows nine coefficient estimates, with each representing a two week interval. The estimates suggest that the effects of infection are completely undetectable after 12 weeks, with the coefficient estimate suggesting that opposition returns to mean relative levels.

# 5 Robustness Checks

Our findings throughout the analysis thus far suggest that COVID-19 infection reduces legislators' expressed opposition to COVID-19 policies. However, there are several alternative scenarios that may potentially explain the results we observe. In this section, we consider several alternative explanations for the results in order to test the robustness of our findings.

## 5.1 Party Differences

One potential scenario that may threaten our interpretation of the findings is that there are systematic differences in how legislators from different political parties reported COVID-19 infection. For instance, it may be possible that Republican lawmakers were more likely to hide their infections than Democratic lawmakers.

We therefore focus only on legislators who reported COVID-19 infection during the time period of analysis. The logic behind this check is that some legislators may hide their infection from the public. For instance, Republican lawmakers may feel pressured not to report that they were infected with COVID-19 if the party position is that COVID-19 is not a serious threat. Therefore, we subset the data to include *only* legislators who reported infection. We then used the subset to re-estimate each of the four primary specifications. The results, presented in Appendix E, confirm the substantive conclusions of the primary analysis and indicate nearly identical effect sizes when considering only legislators who reported infection as the control.

We additionally estimate the conditional average treatment effects (CATE) for Democratic and Republican legislators. The results, presented in Appendix D, suggest that the treatment effects are indeed greater for Republican lawmakers. These differences are statistically significant at conventional levels, suggesting that Republican legislators reduced their opposition to COVID-19 policies more so than Democratic legislators following infection. This is likely a result of the fact that Republicans were much more likely to express opposition to COVID-19 policies in the first place, which was illustrated in Figure 2.

## 5.2 Total Tweets

Another potential scenario that may threaten our interpretation of the findings is that legislators who were infected with COVID-19 may send fewer tweets in a more general sense when they are infected with COVID-19. For example, legislators may spend less time on Twitter while recovering from the virus, and therefore the reduction in opposition to COVID-19 policies that we observe may simply be a function of fewer total tweets sent. To test this possibility, we re-estimate each of the main specifications using the *total* number of tweets sent by each legislator as the dependent variable. The results, presented in Appendix F, actually suggest that legislators tend to increase their activity on Twitter in the four weeks after infection. Given these results, we conclude that it is unlikely that the reduction in opposition to COVID-19 policies that we observe is simply a function of fewer tweets sent.

## 5.3 Legislative Press Releases

As a fourth robustness check, we consider the possibility that our reliance on data from Twitter does not translate to other, more official forms of communication. For this check, we collected additional data on legislators' communication by referring to press releases during the same time period analyzed in the primary analysis. Specifically, we collected every press release sent by a US legislator via the ProPublica Recent Congressional Statements API (ProPublica 2022). The API provides access to press releases issued directly by members of Congress, and includes the date of the press release, the title, and the full text of the press release. After collecting nearly 200,000 press releases in total, we filtered the press releases according to the same COVID-19 related keywords employed in the main analysis. After narrowing to 17,255 COVID-19 related press releases, we effectively replicated the primary analysis using the press releases to measure opposition. The only exception to this procedure was that we used the GPT-3.5 API (OpenAI 2022) to classify all the press releases rather than training a new model ourselves to classify the messages according to whether they expressed opposition to COVID-19 measures.

The results of the analysis using the press releases and the same estimation strategy confirm the substantive conclusions drawn in the primary analysis with messages from Twitter. In press releases, legislators who were infected with COVID-19 reduce their opposition to COVID-19 policies by approximately 2% in the four weeks after infection compared to the four weeks before infection. The estimates are statistically significant across each of the specifications used in the primary analysis. We also demonstrate parallel trends in the pre-treatment period before infection in Appendix G.

Compared to the primary analysis using Twitter data, the effects sizes are much smaller in magnitude. This likely reflects the differing costs associated with distributing a press release compared to sending a tweet, as the amount of time and effort required to distribute a press release is greater in relation to sending a message on Twitter. In Appendix G, we present the full results and descriptive statistics from the press release analysis.

### 5.4 Alternative Estimators

We rely on matrix completion methods for estimation throughout the article. Despite recent studies that show that matrix completion methods outperform two-way fixed effects models and other methods that rely on assumptions of unconfoundedness (Athey et al. 2021), there are certain scenarios in which factor augmented models such as interactive fixed effects specifications may produce more reliable estimates (Liu, Wang, and Xu 2022). We therefore replicate the findings of the primary analysis and the robustness checks using interactive fixed effects models (e.g. factor-augmented models) (Bai 2009; Gobillon and Magnac 2016). The results, which are presented in Appendix H, confirm the substantive conclusions drawn in the primary analysis and the robustness checks.

Taken in total, the results from the robustness checks provide greater assurance that the results from the primary analysis are not driven by systematic party differences in reporting, in how infections may affect social media behavior (e.g. that the reduction in opposition we observe is not driven by a reduction in social media messages more generally), the medium of legislators' communication or the choice of estimation strategy. In each of the separate analyses, the results are consistent with the primary analysis, and suggest that legislators who were infected with COVID-19 reduce their expressed opposition to COVID-19 policies following infection.

## 6 Discussion

As the world must increasingly grapple with the effects of crises such as climate change and pandemics, the electorate, as well as our political leaders, must update their worldviews according to new information. As with climate change denial, skepticism and outright refusal of compliance with public health guidelines could be witnessed at nearly every position in government throughout the pandemic. Despite social and political pressure to take the virus more seriously, American legislators broadcast thousands of messages that expressed opposition to policies intended to slow the spread of COVID-19.

One explanation for why legislators downplayed the potential threat posed by the virus, we argued, is partisan motivated reasoning. Motivated reasoning plagues elites in the same way it might affect any other individual. Elites, like all of us, have a tendency to underestimate information perceived to be politically or personally damaging (Broockman and Skovron 2018).

Moreover, many of the most divisive issues become polarized in a similar fashion. Elites and more politically sophisticated individuals – both of whom are more polarized and more likely to reach conclusions that align with their partisan identities than the general electorate – articulate distinct conclusions about the same issue (Drummond and Fischhoff 2017; Bayes and Druckman 2021). As directions are set and in an effort to ensure that they are on the correct side of the argument, information that supports a certain viewpoint is amplified while conflicting information that challenges said viewpoint is simultaneously and systematically downplayed (Hart and Nisbet 2012; Broockman and Skovron 2018; Bayes and Druckman 2021).

The most recent and salient iteration of this top-down partisan process played out on the issue of COVID-19. From the time of the initial spread of COVID-19 in early 2020, then-President Donald Trump unleashed a number of false or misleading claims about the virus (Paz 2020; Wolfe and Dale 2020). At various points, Trump and fellow Republicans blamed Mexico, China, Democrats, immigrants, and the "mainstream media" for the spread of COVID-19 (Wolfe and Dale 2020; Paz 2020; LeBlanc 2020). When the buck could no longer be passed, Republicans, led by Trump, went to great lengths to downplay the threat of COVID-19, often sharing misinformation and conflicting public health advice from government officials (Cook and Choi 2020; Gollust, Nagler, and Fowler 2020). Trump's rhetoric and position on COVID-19 set the agenda for the broader Republican party, with many members emphasizing a similar sentiment (Cormack and Meidlinger 2021). With this in mind, Republicans in Congress had strong incentives to continue on the same path of emphasizing opposition to COVID-19 measures and downplaying the risks of the virus.

Moreover, at the time of the initial COVID-19 outbreak, Donald Trump and fellow Republicans were gearing up for the 2020 election with one of the lowest unemployment rates in nearly a century and a stock market that was at all time highs (Reserve 2022; MarketWatch 2022). Republicans likely imagined translating these two predictors of incumbent success, as well as other positive elements of the economy at the time, into a 2020 electoral victory. Consequently, when faced with the dire impact that the COVID-19 pandemic would bring to the economy, Republicans were deeply opposed to measures to contain its spread because they saw such measures as a threat to their electoral futures. Therefore, the chosen pathway forward was to deny the potential risk posed by the virus in order to maintain their favorable electoral position. Yet, denying the severity of a potential risk becomes harder once that risk is realized, which occurred when legislators were infected with COVID-19.

In conclusion, this article makes two key contributions to the literature on elites' responsiveness to policy shocks. First, our findings suggest that exogenous shocks that are personally realized can incentivize re-assessing a prior position. Moreover, this re-assessment process not only exists in an environment as polarized as the US, but for an issue as salient as COVID-19 and for individuals who are likely to be the most polarized – political elites in Congress. This finding is both encouraging and discouraging at the same time. While it is illuminating that elites change their views on salient matters when they are personally afflicted, it discouraging that infection with a deadly virus in this instance may be the point at which positions are re-assessed.

Second, the article speaks to the research on the role of exogenous events and their influence on political communication and behavior. Past studies have found conflicting results on the effect of extreme weather events and beliefs about climate change. Yet, there is evidence that climate events may change the political behavior of individuals (Baccini and Leemann 2021). Our study contributes to this research by showing that a behavioral change may occur even before individuals are willing to share that they have changed their attitudes. While our data do not allow us to explicitly test whether infection changed legislators' personal beliefs about the virus, there is an argument to be made that behavioral change in the form of a reduction in opposition is more important than changes in preferences.

While the evidence we present robustly demonstrates that infection reduced expressed opposition, our analysis also illustrates that the effects are relatively short lived, and legislators infected by the virus tend to return to their initial stances after about 2-3 months time. We argue that this is mainly due to in-partisan voters' skepticism about the pandemic measures. However, it is important to note that the persistence of the effect of personal contact with an external policy shock arguably depends heavily on the nature of the problem. For most people, COVID-19 infection does not lead to known long-term health consequences, and this might foster encouragement for skeptical views. However, personal contact with more persistent or more costly exogenous shocks, such as natural disasters, may induce a more enduring behavioral change among voters and political elites alike.

As with any study, there are several limitations to the findings of this research. First, the extent to which social media messages reflect the true preferences of elites is unclear. Although we additionally included the analysis of Congressmembers' press releases, it is still possible that elites may say one thing while believing another. With that said, such words have real world effects, so the extent to which they are genuine may be less important than the fact that they are said. Future research should continue to explore the extent to which elite communication—and communication on social media in particular—reflects the true preferences of elites.

Second, while we consider a number of potential mechanisms for the relationship between COVID-19 infection and reduced opposition to protection measures, we are unable to disentangle how these mechanisms collectively explain shifts in attitudes toward protection measures. Recognizing that personal risk assessment and motivated reasoning might influence opposition to COVID-19 measures differently for different legislators, we argue that distorted risk assessments likely interact with motivated reasoning and strategic position taking to shape legislators' public stance on and attitudes toward external policy shocks. More research on the cognitive distortions in elites should be welcomed. It is commonly accepted that elites are better informed about the nature of policy problems and hold a higher status in society than the wider public. These factors appear to paradoxically create a lower incentive for elites to update their views about the world (Tetlock 2017), and past studies indicate that elites' expertise acts as a constraint on their ability to adequately respond to changing conditions (Pereira and Öhberg 2020). With this in mind, greater focus on the ways in which cognitive biases and motivated reasoning may be remedied in elites is a challenging yet important path for future research.

## Data Availability

All materials required for replication of this analysis are available on GitHub.

## Declaration of Conflicting Interests

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

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# Part I
# Appendix

## Table of Contents


**A Classification Procedures** **34**
- A.1 BERT Model Evaluation . . . . . 34
- A.2 Model Usage . . . . . 36

**B Descriptive Statistics** **37**

**C Pre-Trend Equivalence Test** **38**

**D CATE Estimates** **39**

**E Estimation with Infected Legislators** **40**

**F Robustness Check – Total Tweets** **41**

**G Robustness Check – Congressional Press Releases** **42**

**H Alternative Estimators** **45**
- H.1 Primary Analysis . . . . . 45
- H.2 Robustness Check 1: Infected Legislators Only . . . . . 45
- H.3 Robustness Check 2: Total Tweets . . . . . 46
- H.4 Robustness Check 3: Press releases . . . . . 47

# A Classification Procedures

We rely on two methods to classify the tweets according to whether they expressed opposition to COVID-19. In the first method, we use a hand-coded dataset of 10k tweets to train a BERT model to classify the tweets. This method is explained in full in the main text and provides impressive accuracy (see below), but it is not perfect. We therefore re-classified each message using GPT-3.5 (OpenAI 2022). These classifications were then compared to the BERT classifications. The two models disagreed on 5,005 of the tweets (about 5.9%). We then manually reviewed the tweets that were classified differently by the two models and labelled them according to the correct label.

We used the following prompt when passing the messages to GPT-3.5 API:

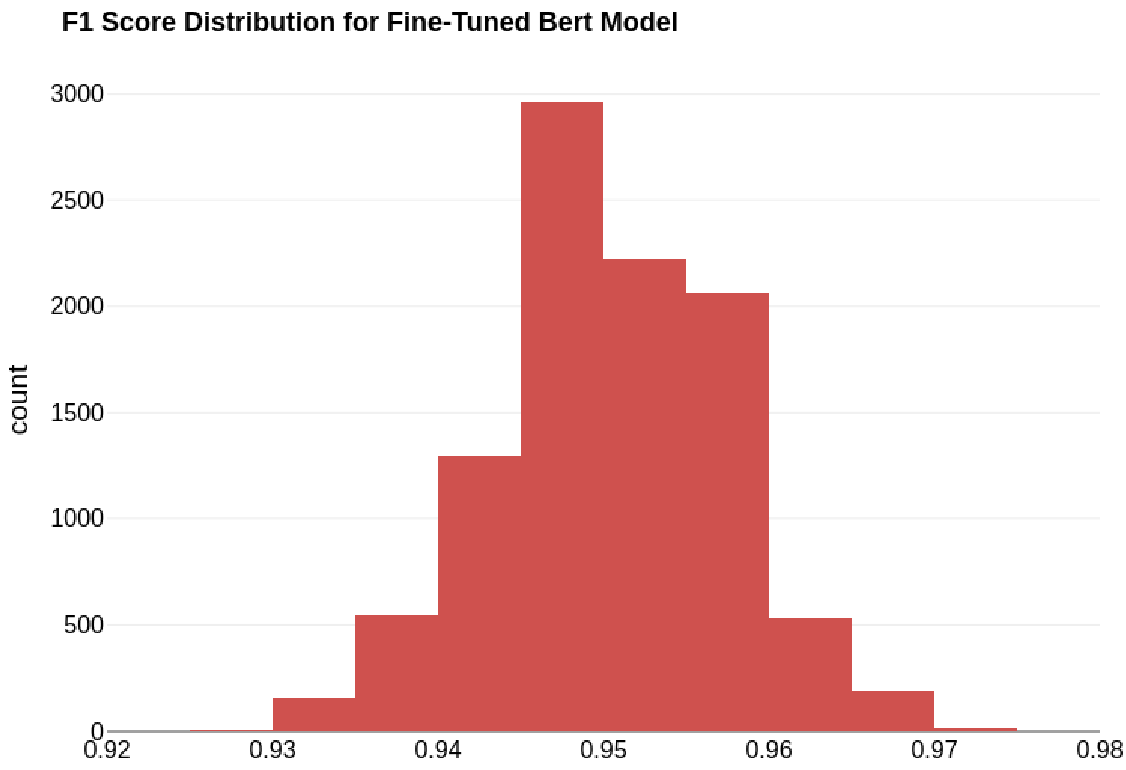
> I want you to act as a zero-shot classifier. I will give you tweet from a US politician regarding COVID-19 or a related policy. I want you to respond according to whether the message expresses opposition or support for a policy aimed at containing or reducing the spread of COVID-19. If the tweet only provides news about COVID-19, please assume the tweet expresses support. If the tweet has nothing to do with COVID-19 or related issues (i.e. vaccines, mask mandates, etc.), please respond with 'NA'. Please reply with only 'support', 'oppose' or 'NA'.

## A.1 BERT Model Evaluation

BERT Model: Upon cross-validation, our model achieves a precision score of 0.95 (true positive / (true positive + false positive) and a recall score of 0.95 (true positive / (true positive + false negative), which gives the model an F1 score of 0.95 (the weighted harmonic mean of the precision and recall scores).

For validation, we trained the model using 9,000, of the 10,000 hand coded tweets and performed validation metrics on the remaining 1,000. For robustness, we verified the F1 score by iterating through 10,000 random samples of 500 observations of the test set and taking the mean value of the scores. The main text cites the F1 score (0.95) that was calculated on all 1,000 values at a single point.

**Figure A1:** F1 Score




**Note:** F1 score using 10,000 samples of 500 from 1,000 annotated tweets the model had not seen

## A.2 Model Usage

The trained model is available **here** where you can find further details of the training process. The model can also be accessed directly from Python using the following code:

```python
# Load the necessary libraries
from transformers import BertTokenizer,
BertForSequenceClassification, pipeline

# Load the model and tokenizer
model_name = 'z-dickson/US_politicians_covid_skepticism'
tokenizer = BertTokenizer.from_pretrained(model_name)
model = BertForSequenceClassification.from_pretrained(model_name)

# Load the model into a pipeline
classifier = pipeline('sentiment-analysis',
                       model=model,
                       tokenizer=tokenizer)

# Example usage
classifier("I am skeptical about COVID-19 measures")

# Response
#[
#  [
#    {
#      "label": "LABEL_1",
#      "score": 0.9845899343490601
#    },
#    {
#      "label": "LABEL_0",
#      "score": 0.015410098247230053
#    }
#  ]
#]
```

# B Descriptive Statistics

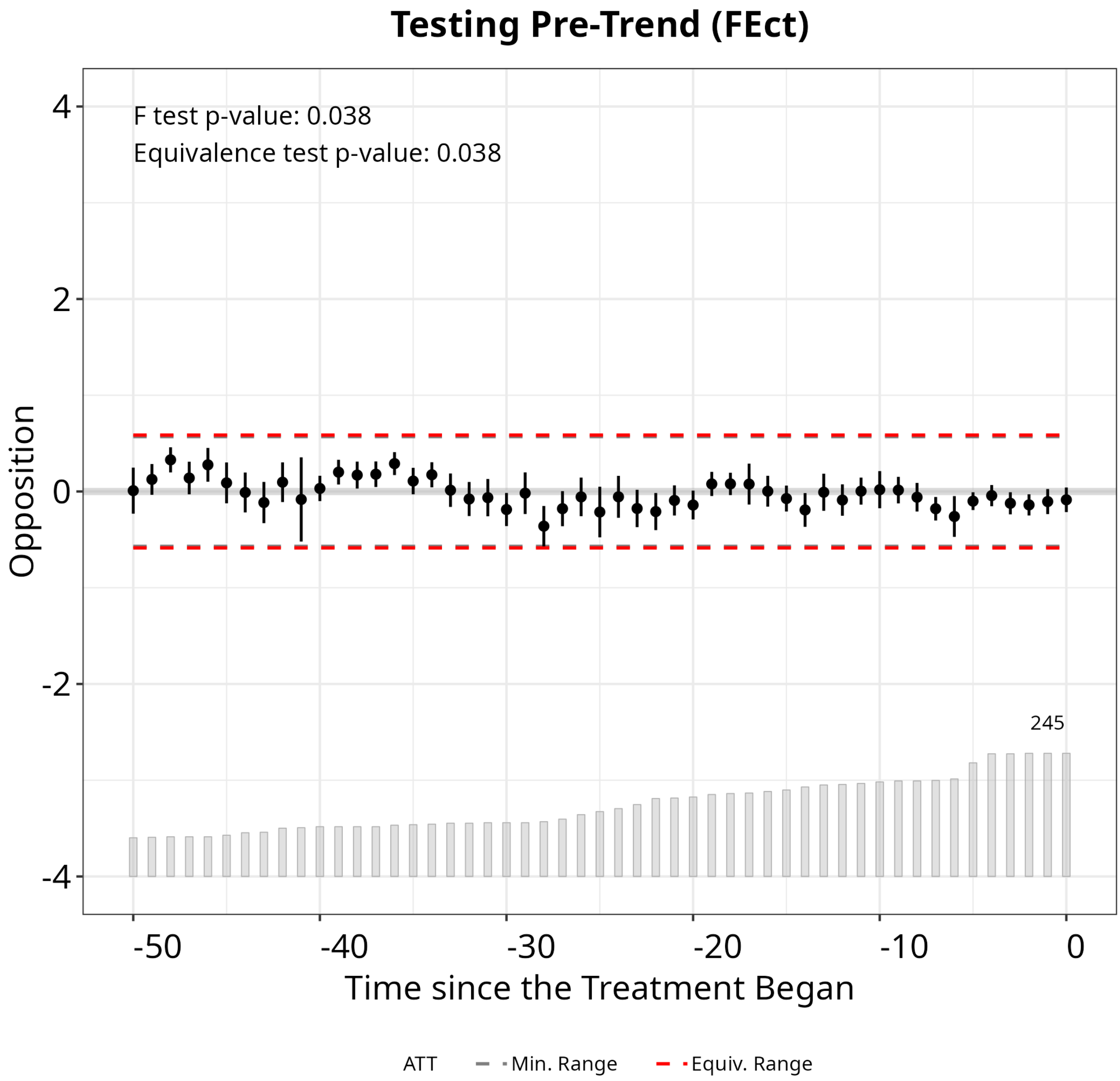
**Table A1:** Descriptive Statistics – Twitter Messages

| Date | Opposition Tweets | | Total Tweets | |
|---|---|---|---|---|
| | sum | mean | sum | mean |
| 2020-01-05 | 0.0 | 0.0 | 2.0 | 0.003 |
| 2020-01-19 | 1.0 | 0.002 | 12.0 | 0.020 |
| 2020-02-02 | 1.0 | 0.002 | 30.0 | 0.050 |
| 2020-02-16 | 6.0 | 0.010 | 34.0 | 0.056 |
| 2020-03-01 | 24.0 | 0.040 | 191.0 | 0.317 |
| 2020-03-15 | 118.0 | 0.196 | 989.0 | 1.643 |
| 2020-03-29 | 149.0 | 0.248 | 3077.0 | 5.111 |
| 2020-04-12 | 101.0 | 0.168 | 2067.0 | 3.434 |
| 2020-04-26 | 136.0 | 0.226 | 1491.0 | 2.477 |
| 2020-05-10 | 162.0 | 0.269 | 1428.0 | 2.372 |
| 2020-05-24 | 260.0 | 0.432 | 1449.0 | 2.407 |
| 2020-06-07 | 98.0 | 0.163 | 692.0 | 1.150 |
| 2020-06-21 | 131.0 | 0.218 | 991.0 | 1.646 |
| 2020-07-05 | 153.0 | 0.254 | 1825.0 | 3.032 |
| 2020-07-19 | 169.0 | 0.281 | 1717.0 | 2.852 |
| 2020-08-02 | 235.0 | 0.390 | 1985.0 | 3.297 |
| 2020-08-16 | 119.0 | 0.198 | 1159.0 | 1.925 |
| 2020-08-30 | 145.0 | 0.241 | 824.0 | 1.369 |
| 2020-09-13 | 185.0 | 0.307 | 1069.0 | 1.776 |
| 2020-09-27 | 171.0 | 0.284 | 1126.0 | 1.870 |
| 2020-10-11 | 207.0 | 0.344 | 1412.0 | 2.346 |
| 2020-10-25 | 213.0 | 0.354 | 1117.0 | 1.855 |
| 2020-11-08 | 105.0 | 0.174 | 991.0 | 1.646 |
| 2020-11-22 | 281.0 | 0.467 | 2151.0 | 3.573 |
| 2020-12-06 | 183.0 | 0.304 | 1393.0 | 2.314 |
| 2020-12-20 | 272.0 | 0.452 | 3224.0 | 5.355 |
| 2021-01-03 | 222.0 | 0.369 | 1410.0 | 2.342 |
| 2021-01-17 | 196.0 | 0.326 | 1883.0 | 3.128 |
| 2021-01-31 | 255.0 | 0.424 | 2197.0 | 3.650 |
| 2021-02-14 | 232.0 | 0.385 | 2455.0 | 4.078 |
| 2021-02-28 | 262.0 | 0.435 | 2459.0 | 4.085 |
| 2021-03-14 | 307.0 | 0.510 | 2951.0 | 4.902 |
| 2021-03-28 | 168.0 | 0.279 | 2178.0 | 3.618 |
| 2021-04-11 | 137.0 | 0.228 | 1623.0 | 2.696 |
| 2021-04-25 | 97.0 | 0.161 | 869.0 | 1.444 |
| 2021-05-09 | 122.0 | 0.203 | 1074.0 | 1.784 |
| 2021-05-23 | 373.0 | 0.620 | 1382.0 | 2.296 |
| 2021-06-06 | 236.0 | 0.392 | 1106.0 | 1.837 |
| 2021-06-20 | 156.0 | 0.259 | 928.0 | 1.542 |
| 2021-07-04 | 155.0 | 0.257 | 868.0 | 1.442 |
| 2021-07-18 | 320.0 | 0.532 | 1037.0 | 1.723 |
| 2021-08-01 | 1170.0 | 1.944 | 2628.0 | 4.365 |
| 2021-08-15 | 719.0 | 1.194 | 2015.0 | 3.347 |
| 2021-08-29 | 315.0 | 0.523 | 1541.0 | 2.560 |
| 2021-09-12 | 560.0 | 0.930 | 1382.0 | 2.296 |
| 2021-09-26 | 535.0 | 0.889 | 1260.0 | 2.093 |
| 2021-10-10 | 498.0 | 0.827 | 1269.0 | 2.108 |
| 2021-10-24 | 532.0 | 0.884 | 1128.0 | 1.874 |
| 2021-11-07 | 1104.0 | 1.834 | 1953.0 | 3.244 |
| 2021-11-21 | 847.0 | 1.407 | 1466.0 | 2.435 |
| 2021-12-05 | 665.0 | 1.105 | 1361.0 | 2.261 |
| 2021-12-19 | 954.0 | 1.585 | 1635.0 | 2.716 |
| 2022-01-02 | 465.0 | 0.772 | 1144.0 | 1.900 |
| 2022-01-16 | 1095.0 | 1.819 | 1922.0 | 3.193 |
| 2022-01-30 | 601.0 | 0.998 | 1252.0 | 2.080 |
| 2022-02-13 | 970.0 | 1.611 | 1529.0 | 2.540 |
| 2022-02-27 | 730.0 | 1.213 | 1042.0 | 1.731 |
| 2022-03-13 | 589.0 | 0.978 | 907.0 | 1.507 |
| 2022-03-27 | 386.0 | 0.641 | 546.0 | 0.907 |
| 2022-04-10 | 281.0 | 0.467 | 504.0 | 0.837 |
| 2022-04-24 | 564.0 | 0.937 | 856.0 | 1.422 |

# C Pre-Trend Equivalence Test

In the following figure, we plot the results for pre-trends equivalence tests outlined in Liu, Wang, and Xu (2022). Results provide evidence that the treatment and control groups follow parallel trends in the time leading up to infection.

**Figure A2:** Pre-Trends Tests



# D CATE Estimates

Table A2 present the CATE estimates for party, gender, age and total tweets. CATEs are estimated using Wager and Athey (2018)'s causal forest (`CRF`) with the best linear projection of the conditional average treatment effect with HC3 standard errors.

**Table A2:** CATE Estimates for the effect of Infection on Opposition

| | Opposition Tweets |
|---|---|
| Total Tweets | −0.181 57* |
| | (0.084 36) |
| Age | −0.004 27 |
| | (0.002 70) |
| Republican | −0.241 08* |
| | (0.115 70) |
| Male | −0.095 46 |
| | (0.075 95) |
| Num.Obs. | 36 722 |
| AIC | 259 110.3 |
| BIC | 571 613.6 |

# E Estimation with Infected Legislators

As a robustness test, we focus estimation efforts on only legislators who reported infection during the time period of analysis. The aim of this robustness test is to examine the extent to which potential differences in reporting of infection between Democrats and Republicans might account for the results we observe in the primary analysis. The results, presented in Table A3, are substantively consistent with the primary analysis.

**Table A3:** Estimation with Infected Legislators Only

| | *DV: Opposition Messages* | | | |
|---|---|---|---|---|
| | IVHS | log+1 | w/COVID-19 cases | w/cases and deaths |
| COVID-19 Infection (ATT) | -0.320* | -0.320* | -0.321* | -0.321* |
| Standard error | (0.132) | (0.132) | (0.133) | (0.132) |
| CI lower (2.5) | -0.579 | -0.579 | -0.581 | -0.579 |
| CI upper (97.5) | -0.061 | -0.061 | -0.060 | -0.062 |
| P-value | 0.015 | 0.015 | 0.016 | 0.015 |
| N. Obs. | 13,237 | 13,237 | 13,237 | 13,237 |
| COVID-19 cases | | | ✓ | ✓ |
| COVID-19 deaths | | | | ✓ |
| Time FE | ✓ | ✓ | ✓ | ✓ |
| Legislator FE | ✓ | ✓ | ✓ | ✓ |

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

**Note:** Standard errors presented in parentheses. All results presented use matrix completion methods and are estimated using the `Fect` library in R (Liu, Wang, and Xu 2022). Models 1 & 2 use an inverse hyperbolic sine transformation and log+1 transformation, respectively. Model 3 includes the number of COVID-19 cases per day in each legislator's constituency state. Model 4 includes the number of new cases and new deaths in each legislator's constituency state.

# F Robustness Check – Total Tweets

In Table A4, we present the results of a robustness check using the total number of tweets as the dependent variable.

**Table A4:** Effect of COVID-19 Infection on Total Tweets

| | *DV: Total Messages about COVID-19* | | | |
|---|---|---|---|---|
| | IVHS | log+1 | w/COVID-19 cases | w/cases and deaths |
| Total Tweets | 0.375 | 0.375 | 0.374 | 0.377 |
| Standard error | (0.224) | (0.224) | (0.224) | (0.224) |
| CI lower (2.5) | -0.065 | -0.065 | -0.065 | -0.062 |
| CI upper (97.5) | 0.815 | 0.815 | 0.814 | 0.816 |
| P-value | 0.095 | 0.095 | 0.095 | 0.092 |
| N. Obs. | 36,722 | 36,722 | 36,722 | 36,722 |
| COVID-19 cases | | | ✓ | ✓ |
| COVID-19 deaths | | | | ✓ |
| Time FE | ✓ | ✓ | ✓ | ✓ |
| Legislator FE | ✓ | ✓ | ✓ | ✓ |

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

**Note:** Standard errors presented in parentheses. All results presented use matrix completion methods and are estimated using the `Fect` library in R (Liu, Wang, and Xu 2022). Models 1 & 2 use an inverse hyperbolic sine transformation and log+1 transformation, respectively. Model 3 includes the number of COVID-19 cases per day in each legislator's constituency state. Model 4 includes the number of new cases and new deaths in each legislator's constituency state.

# G Robustness Check – Congressional Press Releases

To test the robustness of our results, we collected all available press releases from the ProPublica API (ProPublica 2022). We then used the press releases to replicate the primary analysis.

In Table A6, we present the descriptive statistics for the press releases. In Table A5, we present the results of the ATT estimates. In Figure A3, we present the estimates before and after infection to illustrate parallel trends. All results are consistent with the results presented in the main text.

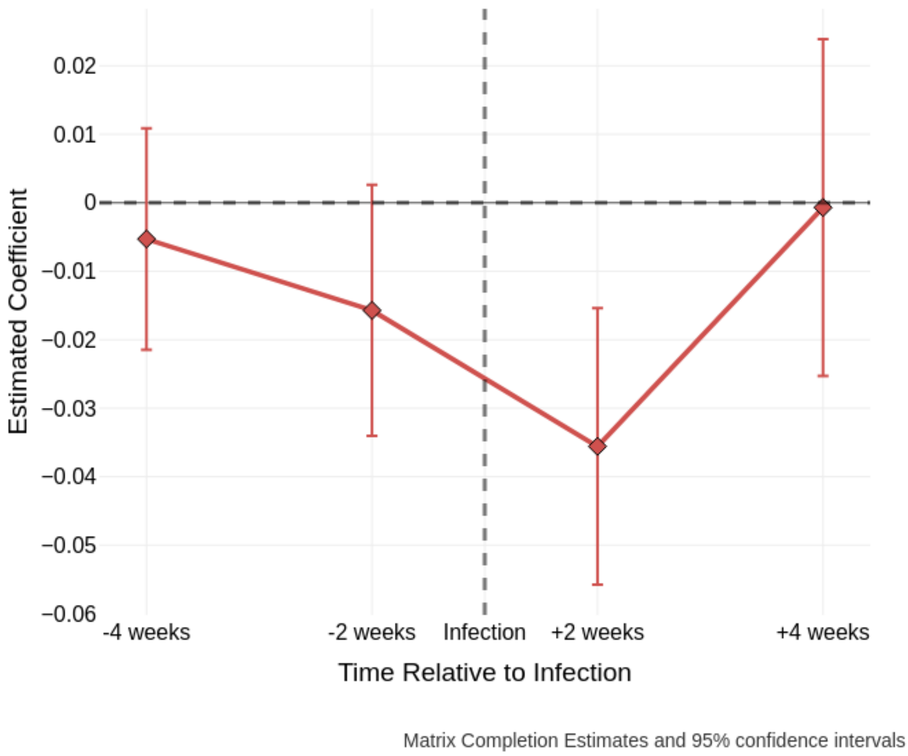
**Table A5:** ATT Estimates – Opposition Messages in Press Releases

| | *DV: Opposition Messages* | | | |
|---|---|---|---|---|
| | IVHS | log+1 | w/COVID-19 cases | w/cases and deaths |
| COVID-19 Infection (ATT) | -0.020* | -0.020* | -0.020* | -0.020* |
| Standard error | (0.009) | (0.009) | (0.009) | (0.009) |
| CI lower (2.5) | -0.037 | -0.037 | -0.037 | -0.037 |
| CI upper (97.5) | -0.002 | -0.002 | -0.003 | -0.003 |
| P-value | 0.025 | 0.025 | 0.024 | 0.024 |
| N. Obs. | 36,722 | 36,722 | 36,722 | 36,722 |
| COVID-19 cases | | | ✓ | ✓ |
| COVID-19 deaths | | | | ✓ |
| Time FE | ✓ | ✓ | ✓ | ✓ |
| Legislator FE | ✓ | ✓ | ✓ | ✓ |

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

**Note:** Standard errors presented in parentheses. All results presented use matrix completion methods and are estimated using the `Fect` library in R (Liu, Wang, and Xu 2022). Models 1 & 2 use an inverse hyperbolic sine transformation and log+1 transformation, respectively. Model 3 includes the number of COVID-19 cases per day in each legislator's constituency state. Model 4 includes the number of new cases and new deaths in each legislator's constituency state.

**Figure A3:** Effects of COVID-19 Infection on Opposition to COVID-19 Policies in Press Releases




**Note:** Figure displays the ATT estimates with 95% confidence intervals for the four weeks before and after COVID-19 infection. The specification includes unit and time fixed effects and an inverse hyperbolic sine transformation of the outcome variable, corresponding with Model 1 in Table A5.

Figure A3 **Alt Text:** The figure shows the ATT estimates with 95% confidence intervals for the four weeks before and after COVID-19 infection. The figure shows four coefficient estimates, with each representing a two week interval. The two estimates before infection are not statistically differentiable from zero, while the two estimates after infection are. The estimates suggests that expressed opposition to COVID-19 policies decreased by approximately 0.02 press releases over the following four weeks.

**Table A6:** Descriptive Statistics – Press Releases

| Date | Opposition Press Releases | | Total Press Releases | |
|---|---|---|---|---|
| | sum | mean | sum | mean |
| 2020-01-05 | 0.0 | 0.0 | 0.0 | 0.0 |
| 2020-01-19 | 0.0 | 0.0 | 1.0 | 0.002 |
| 2020-02-02 | 0.0 | 0.0 | 72.0 | 0.120 |
| 2020-02-16 | 0.0 | 0.0 | 80.0 | 0.133 |
| 2020-03-01 | 3.0 | 0.005 | 209.0 | 0.347 |
| 2020-03-15 | 7.0 | 0.012 | 1289.0 | 2.141 |
| 2020-03-29 | 19.0 | 0.032 | 1482.0 | 2.462 |
| 2020-04-12 | 9.0 | 0.015 | 1050.0 | 1.744 |
| 2020-04-26 | 20.0 | 0.033 | 1111.0 | 1.846 |
| 2020-05-10 | 24.0 | 0.040 | 999.0 | 1.659 |
| 2020-05-24 | 19.0 | 0.032 | 1140.0 | 1.894 |
| 2020-06-07 | 6.0 | 0.010 | 516.0 | 0.857 |
| 2020-06-21 | 10.0 | 0.017 | 504.0 | 0.837 |
| 2020-07-05 | 10.0 | 0.017 | 442.0 | 0.734 |
| 2020-07-19 | 6.0 | 0.010 | 429.0 | 0.713 |
| 2020-08-02 | 28.0 | 0.047 | 646.0 | 1.073 |
| 2020-08-16 | 14.0 | 0.023 | 472.0 | 0.784 |
| 2020-08-30 | 8.0 | 0.013 | 267.0 | 0.444 |
| 2020-09-13 | 18.0 | 0.030 | 272.0 | 0.452 |
| 2020-09-27 | 15.0 | 0.025 | 318.0 | 0.528 |
| 2020-10-11 | 27.0 | 0.045 | 365.0 | 0.606 |
| 2020-10-25 | 17.0 | 0.028 | 179.0 | 0.297 |
| 2020-11-08 | 4.0 | 0.007 | 106.0 | 0.176 |
| 2020-11-22 | 6.0 | 0.010 | 182.0 | 0.302 |
| 2020-12-06 | 2.0 | 0.003 | 150.0 | 0.249 |
| 2020-12-20 | 4.0 | 0.007 | 364.0 | 0.605 |
| 2021-01-03 | 15.0 | 0.025 | 451.0 | 0.749 |
| 2021-01-17 | 6.0 | 0.010 | 177.0 | 0.294 |
| 2021-01-31 | 6.0 | 0.010 | 299.0 | 0.497 |
| 2021-02-14 | 19.0 | 0.032 | 514.0 | 0.854 |
| 2021-02-28 | 31.0 | 0.051 | 406.0 | 0.674 |
| 2021-03-14 | 47.0 | 0.078 | 415.0 | 0.689 |
| 2021-03-28 | 7.0 | 0.012 | 194.0 | 0.322 |
| 2021-04-11 | 9.0 | 0.015 | 123.0 | 0.204 |
| 2021-04-25 | 3.0 | 0.005 | 129.0 | 0.214 |
| 2021-05-09 | 3.0 | 0.005 | 99.0 | 0.164 |
| 2021-05-23 | 5.0 | 0.008 | 101.0 | 0.168 |
| 2021-06-06 | 12.0 | 0.020 | 80.0 | 0.133 |
| 2021-06-20 | 3.0 | 0.005 | 69.0 | 0.115 |
| 2021-07-04 | 8.0 | 0.013 | 73.0 | 0.121 |
| 2021-07-18 | 8.0 | 0.013 | 41.0 | 0.068 |
| 2021-08-01 | 12.0 | 0.020 | 83.0 | 0.138 |
| 2021-08-15 | 7.0 | 0.012 | 48.0 | 0.080 |
| 2021-08-29 | 4.0 | 0.007 | 54.0 | 0.090 |
| 2021-09-12 | 24.0 | 0.040 | 79.0 | 0.131 |
| 2021-09-26 | 13.0 | 0.022 | 80.0 | 0.133 |
| 2021-10-10 | 21.0 | 0.035 | 67.0 | 0.111 |
| 2021-10-24 | 25.0 | 0.042 | 59.0 | 0.098 |
| 2021-11-07 | 73.0 | 0.121 | 139.0 | 0.231 |
| 2021-11-21 | 55.0 | 0.091 | 90.0 | 0.150 |
| 2021-12-05 | 21.0 | 0.035 | 63.0 | 0.105 |
| 2021-12-19 | 65.0 | 0.108 | 128.0 | 0.213 |
| 2022-01-02 | 10.0 | 0.017 | 62.0 | 0.103 |
| 2022-01-16 | 93.0 | 0.154 | 218.0 | 0.362 |
| 2022-01-30 | 15.0 | 0.025 | 63.0 | 0.105 |
| 2022-02-13 | 14.0 | 0.023 | 38.0 | 0.063 |
| 2022-02-27 | 12.0 | 0.020 | 40.0 | 0.066 |
| 2022-03-13 | 10.0 | 0.017 | 49.0 | 0.081 |
| 2022-03-27 | 5.0 | 0.008 | 22.0 | 0.037 |
| 2022-04-10 | 5.0 | 0.008 | 41.0 | 0.068 |
| 2022-04-24 | 1.0 | 0.002 | 16.0 | 0.027 |

# H Alternative Estimators

In this section, we replicate all the findings of the entire analysis (primary and robustness checks) using interactive fixed effects estimators. Interactive fixed effects is a counterfactual estimator similar to matrix completion. For a full overview of this estimator, see Gobillon and Magnac (2016) or Liu, Wang, and Xu (2022). We rely on the `fect` package in R for all estimations.

## H.1 Primary Analysis

The primary analysis uses a staggered difference-in-differences design to identify the effect of infection in the four weeks following infection. In the primary analysis we use matrix completion methods. Here, we use interactive fixed effects models. The results, presented below, suggest a slightly larger treatment effect (0.41 with IFE compared to 0.32 using MC). The results are substantively consistent with the primary analysis.

**Table A7:** Effects of COVID-19 Infection on Opposition to COVID – Full sample

| | IFE | IFE with controls |
|---|---|---|
| (ATT) | -0.416** | -0.416** |
| Standard error | 0.156 | 0.155 |
| CI lower (2.5) | -0.722 | -0.72 |
| CI upper (97.5) | -0.111 | -0.112 |
| P-value | 0.007 | 0.007 |
| N. Obs. | 36,722 | 36,722 |
| COVID-19 cases | | ✓ |
| COVID-19 deaths | | ✓ |
| Time FE | ✓ | ✓ |
| Legislator FE | ✓ | ✓ |

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

**Note:** Standard errors presented in parentheses. Model 1 uses interactive fixed effects without controls and Model 2 uses interactive fixed effects with controls. Estimations rely on the `fect` library in R.

## H.2 Robustness Check 1: Infected Legislators Only

The following robustness check uses interactive fixed effects to replicate the robustness check that examines the effect of COVID infection on opposition using only legislators who were infected with COVID-19 as the control group (e.g. not-yet infected legislators).

The results are consistent with the primary analysis and the primary robustness checks.

**Table A8:** Effects of Infection on Infected Legislators Only

| | IFE | IFE with controls |
|---|---|---|
| (ATT) | -0.388* | -0.390* |
| Standard error | 0.159 | 0.158 |
| CI lower (2.5) | -0.699 | -0.700 |
| CI upper (97.5) | -0.076 | -0.080 |
| P-value | 0.015 | 0.014 |
| N. Obs. | 36,722 | 36,722 |
| COVID-19 cases | | ✓ |
| COVID-19 deaths | | ✓ |
| Time FE | ✓ | ✓ |
| Legislator FE | ✓ | ✓ |

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

**Note:** Standard errors presented in parentheses. Model 1 uses interactive fixed effects without controls and Model 2 uses interactive fixed effects with controls. Estimations rely on the `Fect` library in R.

## H.3 Robustness Check 2: Total Tweets

The following robustness check uses interactive fixed effects to replicate the robustness check that examines the effect of COVID infection on the total number of tweets sent about COVID-19. These results suggest that legislators are likely to increase their Twitter activity in the time following COVID-19, which suggests that the reduction in opposition following infection that is observed in the primary analysis cannot be explained by a reduction in sending messages on Twitter more broadly.

**Table A9:** Effects of Infection on Total Number of Tweets

| | IFE | IFE with controls |
|---|---|---|
| (ATT) | 0.716 | 0.707 |
| Standard error | 0.397 | 0.395 |
| CI lower (2.5) | -0.062 | -0.067 |
| CI upper (97.5) | 1.494 | 1.481 |
| P-value | 0.071 | 0.073 |
| N. Obs. | 36,722 | 36,722 |
| COVID-19 cases | | ✓ |
| COVID-19 deaths | | ✓ |
| Time FE | ✓ | ✓ |
| Legislator FE | ✓ | ✓ |

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

**Note:** Standard errors presented in parentheses. Model 1 uses interactive fixed effects without controls and Model 2 uses interactive fixed effects with controls. Estimations rely on the `Fect` library in R.

## H.4 Robustness Check 3: Press releases

The following robustness check replicates the original robustness check that uses press releases to measure opposition. The results are nearly identical to the primary analysis using matrix completion.

**Table A10:** Effects of Infection on Opposition using Press releases

| | IFE | IFE with controls |
|---|---|---|
| (ATT) | -0.021* | -0.021* |
| Standard error | 0.009 | 0.009 |
| CI lower (2.5) | -0.039 | -0.039 |
| CI upper (97.5) | -0.003 | -0.003 |
| P-value | 0.020 | 0.023 |
| N. Obs. | 36,722 | 36,722 |
| COVID-19 cases | | ✓ |
| COVID-19 deaths | | ✓ |
| Time FE | ✓ | ✓ |
| Legislator FE | ✓ | ✓ |

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

**Note:** Standard errors presented in parentheses. Model 1 uses interactive fixed effects without controls and Model 2 uses interactive fixed effects with controls. Estimations rely on the `Fect` library in R.