

# Are Competitive Elections Good for Your Health?

## Evidence from the 1918 Flu and Covid-19 \*

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Do more electorally vulnerable government officials respond to public health emergencies more aggressively than officials in less competitive seats? Using novel data on local government responses to the 1918 influenza A (H1N1) “Spanish Flu” and 2020 Covid-19 pandemics in the United States, we study how the competitiveness of federal, state and local elections shapes the policy choices of incumbents. We find that, in 1918, vulnerable incumbents enacted more and longer nonpharmaceutical interventions (e.g. quarantines, closures), enforcing them more aggressively than in less-competitive jurisdictions. Their constituents subsequently experienced fewer influenza-related deaths and lower overall excess mortality. In 2020, more competitive constituencies similarly experienced lower rates of Covid-19 infection and death, but they implemented fewer nonpharmaceutical interventions and relied more on pharmaceutical measures. This policy substitution was feasible in part due to political geography: more competitive localities became more suburban, and more conducive to social distancing in the absence of government mandates.

JEL CLASSIFICATION: D72, H12, I18

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Why do some local jurisdictions adopt aggressive measures to slow the spread of infectious diseases while others do not? Public health emergencies raise fundamental questions on the public goods and civil liberties that citizens expect from their government. In a pandemic, protecting citizens' health and safety may require mass restrictions on public behavior.<sup>1</sup> Why are some incumbents more willing than others to impose such restrictions in the name of public health?

We test the general hypothesis that incumbents in more competitive political environments adopt more stringent containment measures, resulting in fewer infections and deaths.<sup>2</sup> This hypothesis synthesizes insights from public health (where researchers have demonstrated the effectiveness of containment measures) and political science (where researchers have revealed how electoral incentives shape policy). We test this hypothesis with data on local public health responses to the 1918 influenza and 2020 Covid-19 pandemics in the United States. We investigate how incentives for mitigation varied across jurisdictions with different levels of electoral competition, at the mayoral, gubernatorial and congressional levels.

Evidence from the 1918 pandemic supports this hypothesis; evidence from 2020 tells a more complicated story. In 1918, more politically competitive localities imposed more nonpharmaceutical interventions to reduce contact rates in the population, kept them in place longer, promoted and enforced them more aggressively. More competitive jurisdictions also saw lower case counts and fewer influenza-related deaths. These results hold across multiple competitiveness measures, multiple levels of elections, multiple measures of policy interventions and disease outcomes, and different estimation strategies.

These patterns are different from what transpired during the Covid-19 pandemic. In 2020-2021, authorities in more competitive constituencies were *less* likely to implement, promote and enforce nonpharmaceutical measures to slow the virus' spread. Yet much as in 1918, they still saw lower rates of infection and disease mortality. We consider several political and epidemiological explanations for this historical reversal, and highlight policy substitution as the most plausible one. In 2020, vulnerable incumbents favored pharmaceutical measures over nonpharmaceutical ones, in part due to an epidemiologically sig-

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<sup>1</sup>The term *epidemic* denotes a temporary local prevalence of a disease; a *pandemic* is an epidemic of continental or global reach.

<sup>2</sup>Our hypothesis and pre-analysis plan are pre-registered at doi.org/10.17605/OSF.IO/4NQGP.

nificant change in political geography: more competitive areas in 2020 were suburban, where social distancing was easier and community risk factors were less acute than in densely-populated city centers. The differences between the two eras stem, in part, from changes to the geography of political competition.

Although local governments play a crucial role in public health (Markel et al., 2007), we know surprisingly little about the politics that drive these governments to respond differently to epidemics. A sizeable literature has shown that democracy has positive impacts on public health outcomes (Besley and Kudamatsu, 2006; Fujiwara, 2015). A new and growing literature on the politics of Covid-19 has further highlighted the significance of electoral accountability for responses to public health emergencies (Stasavage, 2020). Early empirical studies on this topic have examined macro-level variation in political regime type across countries, yielding mixed evidence: some studies have found that democracies reacted more slowly to Covid-19 and experienced more per capita deaths than autocracies (Cheibub, Hong and Przeworski, 2020; Cepaluni, Dorsch and Branyiczki, 2020); others have found better outcomes in states with more accountable institutions (Bosancianu et al., 2020). Sub-national analyses within the United States, meanwhile, have focused mainly on partisan differences in Covid-19 responses (e.g. Allcott et al., 2020), rather than the impact of electoral vulnerability more generally. Very few studies have attempted a quantitative comparison of the politics of Covid-19 and other historical pandemics.<sup>3</sup> We extend this research by placing Covid-19 in a broader context and studying within-country variation in political competitiveness.

Our results provide insights into the dynamic forces behind governments' unequal protections of constituents' health and safety. They contribute to research on the politics of public health, by focusing on the political pressures that shape health emergency responses and by bringing unparalleled data to test these relationships. In American politics, research on political responsiveness to disasters is underdeveloped, and has traditionally neglected "black swan events" like global pandemics. Where such research does exist, it focuses on the downstream political consequences of disasters, not the political manipulation of public health responses (Malhotra and Kuo, 2008; Achen and Bartels, N.d.; Fowler and Hall, 2018; Aksoy, Eichengreen and Saka, 2020). Others focus on the politicization of federal spending, but not on its effects during disasters, when political identities may be more supple (Kriner and Reeves, 2012). In public

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<sup>3</sup>One exception is Napolio (2020), who compares federal spending during four recent pandemics.

health, past research on this subject has focused on assessing the toll of pandemics and the effectiveness of interventions (e.g. Johnson and Mueller, 2002; Barro, 2020). Our paper offers a unique contribution in connecting these interventions with the political forces driving public health decisions.

## I More Political Competition, Better Epidemic Response?

The spread and lethality of infectious diseases partly depend on government efforts to mitigate them. Nonpharmaceutical interventions (NPIs)—like quarantines, school closures, mask mandates, and bans on public gatherings—can reduce transmission rates and mortality by reducing pressure on healthcare systems and providing time for the development and distribution of vaccines and treatments (Bootsma and Ferguson, 2007; Markel et al., 2007). NPIs are also net-positive for economic growth, particularly if implemented early and assertively (Correia, Luck and Verner, 2020). NPIs are among the few mitigation tools policymakers have when pharmaceutical interventions (i.e. involving medical treatment) are unavailable on a mass scale. Despite these benefits, local governments vary substantially in their adoption and enforcement of NPIs. Of course, not all jurisdictions have the same capacity to act. Epidemics require competent public health experts, responsive bureaucracies, and the machinery to enforce regulations (Rourke, 1992). Yet even capable governments may fail to act. Why?

One potential explanation of why capable governments fail to act is that their leaders face relatively little political competition. When incumbents' risks of losing office are relatively low, their incentives to change policy to maximize constituents' wellbeing are also relatively low (Barro, 1973; Ferejohn, 1986). Political competition changes this calculus. The threat of losing office drives incumbents to more faithfully represent their constituents by implementing policies that are responsive to public demands. In competitive environments, challengers are likely to publicize actual or perceived missteps, making constituents more aware of failed policies (Gordon and Huber, 2007). To the extent that the marginal voter might punish incumbents for inaction, political competition should increase incentives to intervene with NPIs.

An alternative view is that political competition reduces incentives to intervene, so as to avoid alienating voters concerned about the short-term costs of NPIs. In the trade-off between closing the economy and mitigating disease, politicians and voters may have different time horizons on their preferences

(Nadeau and Lewis-Beck, 2001). NPIs are economically and socially disruptive in the short term, but their public health and economic benefits may not be realized until much later (Correia, Luck and Verner, 2020). Voters may be unaware of these future benefits (or may not attribute them to NPIs), but they certainly know that NPIs are costly to endure. Incumbents — especially those with a short time to re-election — may therefore prioritize minimizing economic and social disruptions, by adopting fewer NPIs.

Both perspectives rest on the assumption that public officials' accountability is higher in more competitive political environments than in less competitive ones. Accountability rests on constituents' ability to punish or reward political authorities for their actions (Przeworski et al., 1999), re-electing incumbents they credit with positive outcomes and ousting those they blame for negative ones (Fiorina, 1981). Accountability incentivizes officials to act in accord with constituents' preferences; that is, it incentivizes "good" government performance, where the meaning of "good" varies across constituencies with different preferences (Downs, 1957/1985; Fearon, 1999). Machine and one-party systems reduce accountability by making patronage, not open electoral competition, the source of political power (Trounstine, 2008).

When and where voters do have the power to replace political leaders, there is abundant evidence that this electoral pressure can inform policy. Research on local political responsiveness has shown that municipal authorities tend to reflect their constituents' preferences on spending (Palus, 2010), partisanship (de Benedictis-Kessner and Warshaw, 2016), and ideology (Tausanovitch and Warshaw, 2014). What drives voter punishment is variable. Some voters may prioritize local economic performance (Hopkins and Pettingill, 2018); others focus on the quality of public services (Burnett and Kogan, 2017).

Infectious disease outbreaks represent a potentially ambiguous focal point for political accountability. For citizens assessing government officials' performance, public health emergencies have the advantage of being isolated in time, with blame (or credit) potentially attributable to a specific set of incumbents (Healy and Malhotra, 2013). However, large-scale emergencies also tend to be relatively infrequent, leaving voters without a clear benchmark for evaluating incumbent performance. Assigning responsibility is also complicated by the fact that epidemic responses require coordinated efforts across multiple levels of federal, state, and local government (Arceneaux, 2006).

How these considerations shape local officials' epidemic responses is an open question. One possibil-

ity is that political competition generates incentives for incumbents to maximize constituents' wellbeing (or at least appear to do so) and, thus, to adopt NPIs and ultimately decrease mortality (Ansolabehere, Snyder Jr and Stewart III, 2001). Even if citizens do not hold the government responsible for a natural disaster or the outbreak of an infectious disease, they may expect officials to adequately prepare for such events, take mitigating steps, and provide timely and efficient relief (Malhotra and Kuo, 2008). Against this backdrop, officials may anticipate that inaction will invite voter punishment.

Another possibility is that political competition creates incentives to avoid implementing potentially unpopular or polarizing policies. Politically secure incumbents can more aggressively enforce health regulations (Brooke and Gans-Morse, 2016), more credibly offer future rewards to incentivize bureaucrats (Nath, 2015), and may be less willing to tolerate shirking by civil servants (Callen, Gulzar and Rezaee, 2020). To the extent that NPIs generate constituent opposition, we might expect less-accountable incumbents to adopt NPIs earlier and more frequently than more-accountable incumbents.

We hypothesize that the first possibility is more likely than the second because when faced with existential threats, people tend to prioritize health, safety, and security over economic prosperity (Maslow, 1981). But ultimately, this is an empirical question, which our data analyses will help adjudicate.

## 2 The Spanish Flu and Local Government Response

In the United States, the 1918 influenza A (H1N1) pandemic (“Spanish Flu”) unfolded in four main waves. The first coincided with the mobilization of troops for World War I. Due to limited knowledge of the disease, health officials missed many of these early cases, and H1N1 did not yet appear as a separate cause of death in public health reports (Centers for Disease Control and Prevention, 2018a). By the fall, a second, far more deadly wave of the influenza pandemic hit the United States. This wave began in Army and Navy installations on the eastern seaboard (Centers for Disease Control and Prevention, 2018b), with clusters first appearing among the civilian population near large military training facilities, and in areas with high exposure to transit, like shipyards. This wave reached peak fatalities in September and October, with West Coast cities peaking slightly later (Barry, 2020, 375). A third wave unfolded in winter of 1918 and spring of 1919, driven by the end of WWI (Centers for Disease Control and Prevention, 2018b). A final, fourth

wave coincided with the regular winter and spring flu season of 1919–1920.

Nearly all government responses to H1N1 in 1918–1919 occurred at the municipality level, and fell into three categories of nonpharmaceutical interventions (NPIs): closures and quarantines, public messaging, and mask mandates. Because there were no vaccines to prevent infection, and no antibiotics to treat secondary infections like pneumonia, control efforts were almost completely limited to NPIs (Barry, 2020).

The initiative behind NPIs came from local and regional networks of doctors, medical associations, and public health agencies (Bushel, 1966). At the federal level, the H1N1 response competed for resources with the war effort, and the latter took precedence until late 1918. No organized national resistance to NPIs emerged, although there were sporadic local protests, particularly over restrictions on church services, public gatherings and reduced business hours (UM Center for the History of Medicine, 2016).

## 2.1 Data and Measurement

If our hypothesis is valid, then more electorally competitive localities should have imposed NPIs earlier, lifted them later and enforced compliance more aggressively. More electorally competitive localities should also have experienced lower flu-related mortality rates. To assess the impact of political competition on local epidemic response, we use novel data on NPIs and the dynamics of the 1918 influenza pandemic across electoral constituencies within the U.S. We consider four types of outcomes:

1. *NPI events, including the timing of adoption, duration, and enforcement.* We extracted the dates and categories of these events from municipal-level public health bulletins, summaries and timelines in the Influenza Encyclopedia (UM Center for the History of Medicine, 2016), using natural language processing and deep learning methods.<sup>4</sup> These data are available at the daily level for 45 major U.S. cities in 25 states and Washington, DC, from September 1918 to March 1919. We matched these cities to pre-pandemic electoral constituency boundaries (Figure 1).<sup>5</sup>

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<sup>4</sup>To distinguish between enforcement episodes (e.g. arrests and fines for violations of public health guidelines) and other events in the timeline, we implemented long short-term memory (LSTM) models using the keras library in Python 3 (Chollet, 2015). See Appendix A1 for details.

<sup>5</sup>These constituencies are either 1916 congressional districts (congressional elections, Kollman et al. 2017)

2. *Daily reported flu deaths.* These are official influenza fatality statistics from local public health reports (UM Center for the History of Medicine, 2016). They have the same geographic and temporal coverage as our NPI data in Figure 1.
3. *Excess mortality.* To expand the geographic scope of this study beyond the 45 metropolitan areas for which we have official data on flu fatalities — and to compensate for the absence of the first wave from those numbers — we follow previous studies by estimating local excess mortality in 1918.<sup>6</sup> We constructed our estimates of excess deaths using county-level annual mortality data from Bailey et al. (2016).<sup>7</sup> These data cover 1350 counties in 20 states and D.C.
4. *Monthly pneumonia cases and deaths.* The most common secondary infection associated with influenza is bacterial pneumonia, which accounted for the majority of flu-related deaths in 1918 (Morens, Taubenberger and Fauci, 2008). We collected data on monthly pneumonia cases and deaths from van Panhuis, Cross and Burke (2018). These data cover 378 counties in 48 states and D.C., from January 1918 to December 1921 — a period that includes all four waves of the pandemic.

We use multiple measures of pandemic severity to compensate for the limitations of each individual statistic (Figure 1). If a consistent pattern emerges across these measures, we can be more confident that our results are not artifacts of idiosyncratic reporting, measurement strategies or spatio-temporal coverage.

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or 1910 counties (all other elections, Manson et al. 2021).

<sup>6</sup>Due to inconsistent reporting practices in the first wave of the pandemic, researchers have traditionally relied on excess deaths in lieu of reported flu deaths (Murray et al., 2006; Barro, 2020).

<sup>7</sup>Excess mortality is the difference between observed and predicted mortality (Serfling, 1963; Olson et al., 2005). We estimate predicted mortality with a model of county-specific linear trends for 1915–1925:

$$m_{it} = \beta_i + \gamma_i t + \epsilon_{it}$$

where  $m_{it}$  is all-cause mortality in county  $i$  and year  $t$  (we dropped 1918 from the sample so that mortality during the pandemic does not influence estimation). The parameters  $\beta_i$  and  $\gamma_i$  represent county-specific intercepts and time trends. We use this model to predict mortality at  $t = 1918$  for each county, and subtract that number from observed deaths to obtain an estimate of excess mortality:  $m_{i,1918} - \hat{m}_{i,1918}$ .

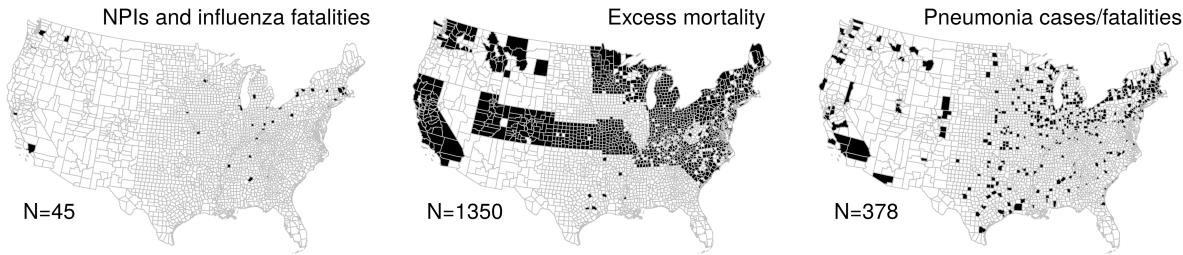


Figure 1: SPATIAL COVERAGE OF PUBLIC HEALTH OUTCOME DATA FOR 1918.

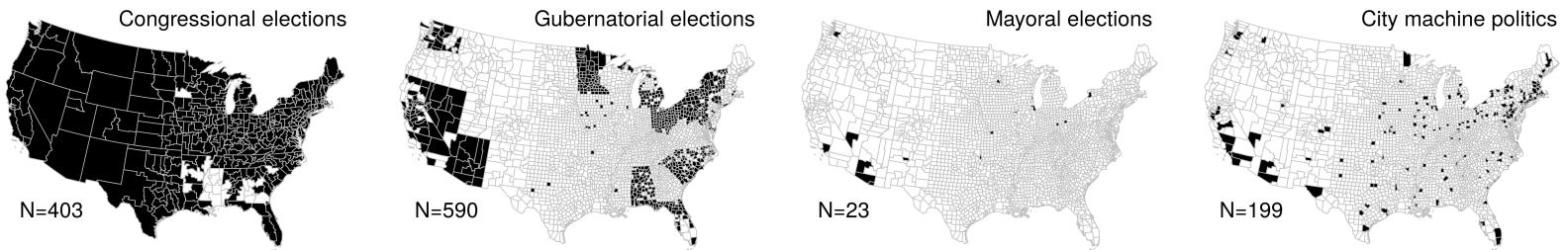


Figure 2: SPATIAL COVERAGE OF PRE-1918 ELECTORAL DATA.

We combine these data with information on local electoral competitiveness, based on the outcomes of pre-pandemic federal, state and local elections (Figure 2). Our data on *congressional elections* (House of Representatives) are from the University of Michigan’s Constituency-Level Elections Archive (Kollman et al., 2013). They include vote tallies for 403 constituencies (congressional districts) in 47 states in 1916.

Our data on *gubernatorial* and *mayoral elections* are from ourcampaigns.com, a crowd-sourced archive of historical election results, which draws primarily on Secretary of State reports, periodicals on state government (e.g. New York *Red Book*) and reference books (e.g. Kallenbach and Kallenbach 1977). We extracted the most recent available pre-pandemic vote tallies for 1912-1917, and aggregated them to the constituency level (counties in 1910 borders). The spatio-temporal extent covers 590 constituencies in 23 states for gubernatorial races, and 23 constituencies in 15 states for mayoral races. We also collected city-level information on political machines (Trounstine, 2008), covering 199 constituencies in 40 states.

For each set of elections, we constructed three measures of local political competitiveness, summarized in Table 1: top-one competitiveness, top-two competitiveness, and electoral contestation. For each measure, higher values represent more competitive elections, as measured by the vote share received by candidates other than the winner, the size of the vote margin separating the winner and “first loser,” and the vote margin between the incumbent and their nearest challenger (for details, see Gerring et al., 2018).

Variable	Measurement	Higher values
Top-1 competitiveness	100 – W's vote share	↑ competitive
Top-2 competitiveness	100 – (W's vote share – first L's vote share)	↑ competitive
Contestation	100 – (I's vote share – C's vote share)	↑ competitive

Table 1: ELECTORAL COMPETITION. I: incumbent, C: challenger, W: winner, L: loser.

We also created a binary measure, “no machine,” assigning a value of 1 to cities in which there was no evidence of a machine or reform monopoly in 1918, and 0 otherwise.

Political competition is not the only source of variation in these outcomes, and we account for several alternative explanations. We use U.S. Census data from 1910 to measure the size and density of the local population and its breakdown by race, sex, and age. We account for seasonality in disease transmission with monthly historical data on temperature and precipitation. Because WWI troop movements accelerated the pandemic, we include the distance from each constituency to the closest major U.S. Army mobilization base (US Army Center for Military History, 2017).

## 2.2 Regression Analysis

We consider three sets of statistical models. First, to examine the impact of electoral competitiveness on the timing of NPIs in a multivariate context, while accounting for shared regional risk factors, we fit a series of mixed effects Cox proportional hazard models.<sup>8</sup> Second, to assess how aggressively localities

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<sup>8</sup>We model the hazard of an event at time  $t$  as a function of electoral competitiveness measure  $c^{(k)}$ , where  $k \in \{\text{top-1, top-2, contestation}\}$ , along with a set of covariates and random effects:

$$h(t, c^{(k)}, X, Z) = h_0(t)e^{c^{(k)}\gamma + X\beta + Z\nu}, \quad \nu \sim N(0, \Sigma(\vartheta)) \quad (1)$$

where  $h_0(t)$  is the baseline hazard function,  $X, Z$  are design matrices for fixed and random effects,  $\gamma, \beta$  are fixed effects coefficients, and  $\nu$  is a vector of random effects with mean zero and variance-covariance matrix  $\Sigma(\vartheta)$  (Therneau, 2015). We consider two versions of this model: one that incorporates region-specific effects in  $Z$  (random intercepts for Midwest, Northeast, South, West) and another that incorporates them as additional fixed effects. The  $X$  matrix also includes covariates for distance to nearest army base (logged), urbanization, percent female, longitude and latitude. Hausman tests find no significant difference between  $\hat{\gamma}, \hat{\beta}$  estimates from the two models, suggesting that both are consistent, but fixed

pursued their NPIs over time, we employ mixed effects linear models, with monthly panel data.<sup>9</sup> Third, we use another set of linear mixed models to probe the relationship between electoral competition and the severity of the influenza outbreak, using both panel and cross-sectional data.<sup>10</sup>

We fit these models separately for each outcome, for each competitiveness measure in Table I, at each level of elections (federal, state, local), keeping the same covariates and group effects. Because this many estimates can be difficult to evaluate, we summarize our results using a meta-analytic random effects model (Berkey et al., 1995; Van Houwelingen, Arends and Stijnen, 2002).<sup>11</sup> This approach allows us to estimate a weighted mean effect of electoral competitiveness on each outcome, while accounting for potential het-

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effects is inefficient. We therefore report random effects estimates here (see Table A2.3 for fixed effects).

<sup>9</sup>We model the outcome (i.e. NPI enforcement events) for constituency  $i$  at month  $t$  as follows:

$$\log(\text{outcome}_{it} + 1) = c_i^{(k)}\gamma + X_{it}\beta + z_j + u_t + \varepsilon_{it}, \quad z_j \sim N(0, \Sigma(\vartheta)_z), \quad u_t \sim N(0, \Sigma(\vartheta)_u) \quad (2)$$

where  $z_j, u_t$  are random intercepts for region  $j$  and month  $t$ .  $X$  includes the same covariates as in eq. (1), augmented with monthly temperature and precipitation for  $i$  at  $t$ . Because NPI events are right-skewed, we use a logarithmic transformation on the outcome. As before, we also estimate a version of this model with regional and monthly fixed effects. Because Hausman tests fail to reject the null hypothesis of equality in coefficient estimates, we report the random effects results below (see Table A2.3 for fixed effects estimates). We weighted the observations by total census population in 1910.

<sup>10</sup>For panel data analyses (e.g. number of pneumonia cases per month), we use the same model specification as in eq. (2). For cross-sectional analyses with no time variation (e.g. excess mortality in 1918), we use a restricted version of the linear mixed model:

$$\log(\text{outcome}_{ij} + 1) = c_{ij}^{(k)}\gamma + X_{ij}\beta + z_j + \varepsilon_{ij}, \quad z_j \sim N(0, \Sigma(\vartheta)_z) \quad (3)$$

dropping the month-specific effects  $u_t$  and collapsing over the time index  $t$ .

<sup>11</sup>The meta-analytic random effects models take the form

$$\begin{aligned} \hat{\gamma}_k &= \omega_k + e_k, & e_k &\sim N(0, v_k) \\ \omega_k &= \mu + u_i, & u_i &\sim N(0, \tau^2) \end{aligned} \quad (4)$$

where  $\hat{\gamma}_k$  is the effect estimate for competitiveness measure  $k$ ,  $\omega_k$  is the corresponding unobserved true effect,  $e_k$  is the sampling error, and  $v_k$  is the sampling variance. Our goal is to estimate  $\mu$  (the mean true

erogeneity due to differences in sample characteristics and other factors.<sup>12</sup>

## TIMING OF NPIS

Table 2 reports preliminary results from all three sets of models. The top two rows (a,b) correspond to the mixed effects Cox regression models, examining how the competitiveness of most recent elections affected the timing of NPI imposition and termination during the 1918 influenza pandemic. Each black point in the figure represents the result from a different model, with estimated effect sizes (percent changes in the hazard of each event associated with a 1% increase in the predictor variable listed on the vertical axis) and confidence intervals on the horizontal axis.<sup>13</sup> The larger, white points represent weighted mean estimates from the meta-analytic random effects model. Estimates with confidence intervals not covering the vertical line at zero are statistically significant at the 95% (thin line) or 90% level (thick line). Scaled Schoenfeld residuals reveal no significant violations of the proportional hazards assumption for any model.

These results suggest that political competition influenced both the duration of NPIS and the timing of their initial implementation. As Table 2a shows, constituencies with more competitive pre-1918 elections implemented NPIS slightly earlier (higher hazard), on average, than constituencies with less competitive elections. According to our weighted mean (white dot, labeled “Z”), a one-percent increase in electoral competitiveness increased the hazard by .2%. However, this estimate is too uncertain to reach statistical significance, due in part to heterogeneity across measures of competitiveness: the competitiveness of gubernatorial races is predictive of earlier adoption, while competitiveness at other levels is not. Table 2b further indicates that more electorally competitive constituencies kept their NPIS in place longer. The hazard of NPI termination decreased by 1.7% with each one-percent increase in competitiveness. While the 95% confidence interval covers zero in several cases, this is in part due to small sample size ( $16 \leq N \leq 44$ ); the estimated point values are generally of the expected sign.

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effect) and  $\tau^2$  (the extent of heterogeneity among true effects).

<sup>12</sup>In Tables A3.5 and A3.6 we directly model this heterogeneity with an expanded mixed effects specification, to examine how measure-specific and election-specific factors influence the average true effect.

<sup>13</sup>The hazard is the probability of an event at  $t$ , given that the event had not occurred up to time  $t$ . Percent changes are calculated from the estimated hazard ratio as  $\Delta\% = (\exp(\hat{\gamma}) - 1) \times 100$ .

Table 2: POLITICAL COMPETITION AND THE 1918 INFLUENZA PANDEMIC IN U.S.

IMPACT OF ELECTORAL COMPETITION ON LOCAL PANDEMIC RESPONSE

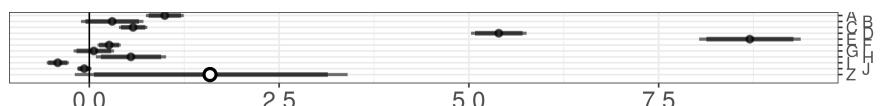
(a) Hazard of first NPI\*



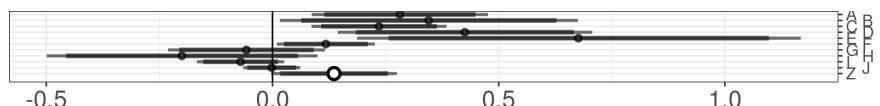
(b) Hazard of NPI termination\*



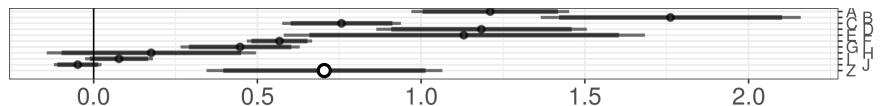
(c) New NPIs per month†



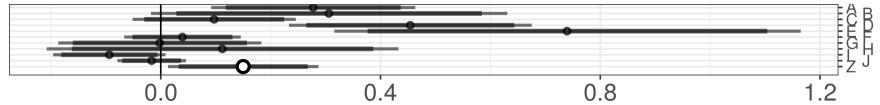
(d) Information releases per month†



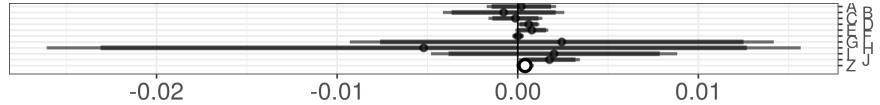
(e) NPI strengthening events per month†



(f) NPI enforcement events per month†

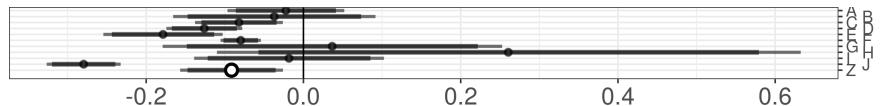


(g) Pharmaceutical interventions per month†

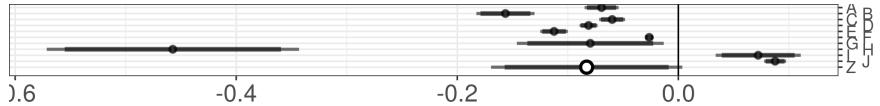


IMPACT OF ELECTORAL COMPETITION ON PUBLIC HEALTH OUTCOMES

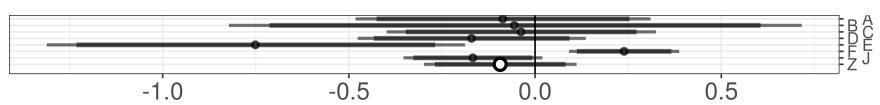
(h) Monthly influenza deaths (per 1000 people)†



(i) Monthly pneumonia cases (per 1000 people)†



(j) Excess mortality in 1918 (per 1000 people)‡



**INTERPRETATION:** Horizontal axis represents percent change in outcome, associated with a 1% increase in each measure of political competitiveness. Dots are point estimates; thin and thick lines are 95% and 90% confidence intervals. **MEASURES:** A: Top-2 Competitiveness (Congress), B: Top-1 Competitiveness (Congress), C: Contestation (Congress), D: Top-2 Competitiveness (Governor), E: Top-1 Competitiveness (Governor), F: Contestation (Governor), G: Top-2 Competitiveness (Mayor), H: Top-1 Competitiveness (Mayor), I: Contestation (Mayor), J: No Machine (City); Z: Weighted mean effect estimate. **MODELS:** \*: Mixed effects Cox regression, †, ‡: Linear mixed model. Models account for distance to nearest army base, percent urban, percent female, longitude, latitude, weather (†, ‡), region (\*, †, ‡) and month (†) random effects. Observations weighted by 1910 population. Units of analysis are 1916 congressional districts (A-C) and 1910 counties (D-J), September 1918–March 1919. See Table A2.3 for fixed effects model estimates.

## NPI INTENSITY AND ENFORCEMENT

The next four rows (c–f) of Table 2 report results from linear mixed models, examining how the competitiveness of pre-1918 elections affected the monthly frequency of several types of NPIs: imposition of any new NPIs (2c), public information announcements (2d), strengthening of the NPI regime through additional closures and public gathering bans (2e), and enforcement of compliance with NPIs through arrests and fines (2f). As before, we report estimates for mayoral, gubernatorial and congressional elections, each from a separate model (40 models in all), along with weighted means.

Although the uncertainty around these estimates varies, the general pattern is one of a positive relationship between the competitiveness of pre-pandemic elections and the intensity of local NPI regimes. For example, a one-percent increase in the competitiveness of elections yielded 1.6% additional NPIs per month, according to our weighted mean estimate (Table 2c). This relationship is strongest for gubernatorial elections, where estimates range from .3% to 8.7%, all significant at the 90% level or higher. It is more uncertain for congressional and mayoral elections.

The same pattern holds for the more specific NPI events: more electorally competitive constituencies were more likely to strengthen their NPI regimes (Table 2e), issued more information releases (Table 2d), made more arrests and levied more fines for noncompliance (Table 2f). There is, however, substantial heterogeneity across levels of elections, in that competitive congressional and gubernatorial races were generally associated with more aggressive responses, while mayoral competitiveness measures were not.

## LOCAL SEVERITY OF THE PANDEMIC

The bottom three rows of Table 2 (h–j) report another set of linear mixed model estimates for the impact of electoral competitiveness on several pandemic-related public health outcomes: monthly influenza deaths in 1918 (2h), monthly pneumonia cases (2i), and excess mortality in 1918 (2j). The first two sets of estimates employ constituency-month panel data, with random effects for region and month. The last set employs a cross-sectional design at the constituency level, with regional random effects.<sup>14</sup>

These results indicate that more electorally competitive constituencies not only had more stringent

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<sup>14</sup>Excess mortality estimates are not available at the monthly level.

NPIs, but they also experienced less severe disease outcomes. According to our weighted mean estimate, a one-percent increase in the competitiveness of elections is associated with a .1% decrease in monthly influenza deaths (per 1000 people) during the second and third waves of the pandemic (2h). A one-percent increase in competitiveness yielded a similar decrease in monthly pneumonia cases (per 1000), at -.1%, on average (2i). Eight of ten individual estimates for pneumonia cases were significant at the 5% level or better, with two deviant results (mayoral contestation and “no machine”). We observe similar patterns, but with more uncertainty, for the relationship between competitiveness and excess mortality in 1918 (2j).

### 3 Covid-19 and Local Government Response

To probe the generalizability of our 1918 results, we consider evidence from the Coronavirus disease 2019 (Covid-19) pandemic in the U.S.<sup>15</sup> There are striking parallels between the dynamics of the two pandemics and government responses to them. The symptoms of the two diseases are similar (e.g. fever, sore throat, headache, nasal congestion, coughing, fatigue, muscle pain). Like H1N1, severe acute respiratory syndrome coronavirus 2 (SARS-CoV-2) — the virus that causes Covid-19 — spreads through respiratory droplets, aerosols, contaminated objects and surfaces. The two viruses initially had comparable rates of transmission, with basic reproduction numbers (i.e. average number of secondary infections produced by a single infectious case) in the range 2-4 (Vynnycky, Trindall and Mangtani, 2007; Billah, Miah and Khan, 2020). These similar epidemiological characteristics call for similar methods of mitigation (e.g. social distancing, masking). During both pandemics in the U.S., NPI responses remained largely decentralized, with states, counties, and municipalities implementing and lifting local stay-at-home orders, mask mandates, business closures, and gathering restrictions (Gordon, Huberfeld and Jones, 2020).

There are also important differences. Unlike H1N1, SARS-CoV-2 became more transmissible over time, with reproduction numbers reaching 5-8 for the Delta variant in 2021.<sup>16</sup> Covid-19 also has a longer

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<sup>15</sup>Given the relative abundance of academic and general audience literature on Covid-19, we omit a detailed discussion of this pandemic’s history here. For background on the Covid-19 pandemic, see American Journal of Managed Care (2021); Taylor (2021).

<sup>16</sup><https://www.bbc.com/news/health-57431420>

period of pre-symptomatic and asymptomatic spread than the flu, further complicating efforts to slow transmission. While the 1918 flu affected many healthy young adults, Covid-19 is most severe in older patients with underlying medical conditions. The federal response was also more robust in 2020-21 — federal mask mandates at airports, national eviction moratoria, stimulus payments, and federalized vaccine rollouts are four examples among many (Centers for Disease Control and Prevention, 2020; Senate and Congress, 2020; Gordon, Huberfeld and Jones, 2020). There are also important differences in the political context (e.g. peacetime vs. wartime, presidential vs. midterm election year, diminished dominance of city machine politics), which may affect the incentives facing political incumbents.<sup>17</sup>

### 3.1 Data and Measurement

In extending our inquiry to Covid-19, we sought to closely replicate our measurement and estimation strategies from the 1918 pandemic. We consider three types of outcomes:

1. *NPI events, including the timing of adoption, duration, and enforcement.* We extracted the dates and categories of these events from the CoronaNet database (Cheng et al., 2020), which we geocoded and filtered to include only NPIs administered at the state, county or municipal level. The data version we used in our analysis spans from January 2020 to May 2021, which we truncated at October 2020 to cover the period when the ancestral strain of the virus — closest in transmissibility to H1N1 — was dominant.<sup>18</sup> We matched these events to pre-pandemic constituencies (Figure 3), using the same GIS data sources and geoprocessing routines as before.<sup>19</sup>
2. *Daily reported Covid-19 cases and deaths.* We obtained these statistics from the COVID-19 Data Repository at the Center for Systems Science and Engineering at Johns Hopkins University (Dong, Du and Gardner, 2020). We used county-level time series tables for the U.S. (January 2020-May

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<sup>17</sup>Local elections in the U.S. are still often subject to biases in representation and political turnout, often in favor of older, whiter, and wealthier residents — groups that experienced differentiated vulnerabilities to the SARS-CoV-2 virus (Trounstine, 2010).

<sup>18</sup>Expanding the observation period to May 2021 does not substantively change results (Table A2.2).

<sup>19</sup>We used 2014 congressional districts and 2010 counties as constituencies, depending on election level.

2021, truncated to October 2020), which we aggregated to the constituency level. As Figure 3 shows, these data have a much broader geographic scope than our mortality data for the 1918 flu.

3. *Excess mortality.* In line with our 1918 design, we obtained county-level estimates of excess mortality in 2020 (Ackley et al., 2021). These data cover 3114 counties in all 50 states and D.C. (Figure 3).

For information on pre-pandemic elections, we drew on the same sources as in the preceding analysis. Figure 4 shows the spatial extent of these data.

For each election, we constructed the same three measures of competitiveness described in Table 1. Due to the decline of city machine politics over the 20th Century, we did not create a “no machine” indicator. To account for other contributing and mitigating factors, we collected data on modern counterparts to some of the covariates we used for 1918, including census data on local population size, sex ratio and age distribution, and data on local population density (Schiavina, Freire and MacManus, 2019).

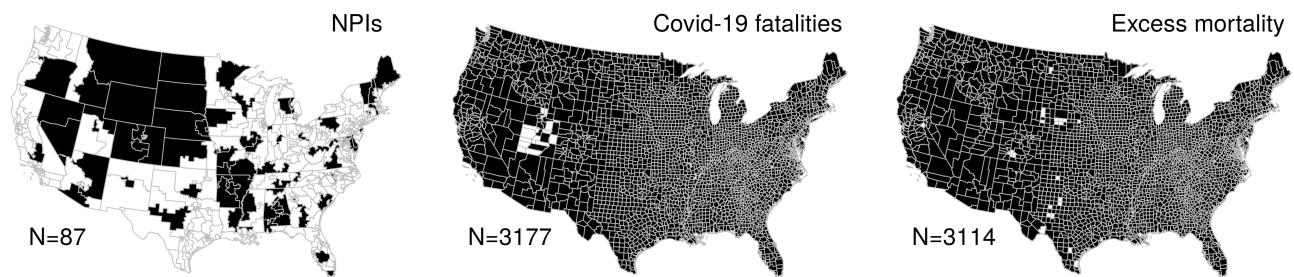


Figure 3: SPATIAL COVERAGE OF PUBLIC HEALTH OUTCOME DATA FOR COVID-19.  
Numbers include constituencies in Alaska and Hawaii.

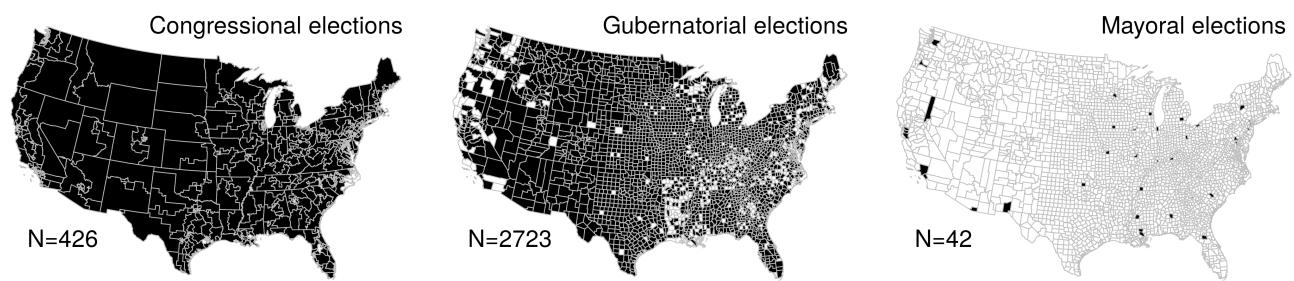


Figure 4: SPATIAL COVERAGE OF PRE-2020 ELECTORAL DATA.  
Numbers include constituencies in Alaska and Hawaii.

## 3.2 Regression Analysis

Our analysis for Covid-19 closely follows the estimation strategy described above: a mixed effects Cox regression to study the timing of NPIs, and linear mixed models to study the intensity of NPIs and severity of Covid-19 deaths over time, as well as excess mortality in 2020.<sup>20</sup> As before, we summarize the estimates across the competitiveness measures and election levels using a meta-analytic random effects model.

### TIMING OF NPIs

Table 3 reports estimates for all models and outcomes. The top two rows report percentage point changes in hazard of NPI implementation (3a) and termination (3b) associated with a 1% increase in each measure of competitiveness. Scaled Schoenfeld residuals indicate no significant changes in coefficients over time.

The results in 3a and 3b are, in some ways, the opposite of what we observed in the 1918 case: electoral competitiveness is *not* associated with faster or longer mitigation measures. If anything, the opposite seems more likely. According to our weighted mean estimate (labeled “Z”), a one-percent increase in competitiveness reduces the hazard of new NPIs (i.e. delaying imposition) by .8 percentage points on average. This delay is especially evident in the case of congressional elections, and is more uncertain at the state and local level. Meanwhile, competitiveness appears to have little discernible impact on the duration of NPIs: none of the estimate in 3b are statistically significant.

### NPI INTENSITY AND ENFORCEMENT

The results in Table 3c-f further reveal that, by 2020, the previously positive relationships between electoral competition and monthly NPI intensity had weakened and even reversed. Our weighted mean estimates show that a one-percent increase in competitiveness is associated with .1% fewer new NPIs (3c), .1% fewer strengthening events (3e), and .03% fewer enforcement events per month (3f). The weighted mean for information releases remains positive, but is smaller and noisier than before, at .05% (3d). There is substantial heterogeneity in these results — the negative relationship between competitiveness and NPIs

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<sup>20</sup>Because NPI and case data are less geographically sparse for Covid-19 than for 1918, we extend the mixed effects specifications to include state (rather than regional) random effects.

is strongest for congressional elections; in many cases, state and local competitiveness continue to be positively correlated with NPIs, albeit insignificantly so.

#### LOCAL SEVERITY OF THE PANDEMIC

Despite their clear hesitancy to impose NPIs, more politically competitive constituencies still saw generally better public health outcomes than less competitive constituencies. As rows h-i of Table 3 report, more competitive constituencies saw fewer Covid cases and deaths. Weighted mean estimates indicate that a 1-percent increase in competitiveness yields a .3% (.1%) decrease in monthly cases (deaths) per 1000 people. Point estimates for almost all individual competitiveness measures are negative, and most — except mayoral races — are significant at the 95% level. A similar picture emerges when we consider excess mortality ( $\beta_j$ ), a measure less sensitive to under-reporting. The weighted mean estimate here likewise indicates a .1% decline in excess deaths per 1000 people.

## 4 Explaining Differences between 1918 and 2020

We expected political leaders in more electorally competitive locations to have greater incentive to enact NPIs and reduce disease mortality. Evidence from 1918 aligns with this expectation, but evidence from Covid-19 does so only partially: more competitive constituencies indeed experienced fewer deaths, but they were also *less* likely to implement, strengthen or enforce NPIs. What explains this divergence? And why were cases and deaths so low in competitive areas despite a relative infrequency of NPIs?<sup>21</sup>

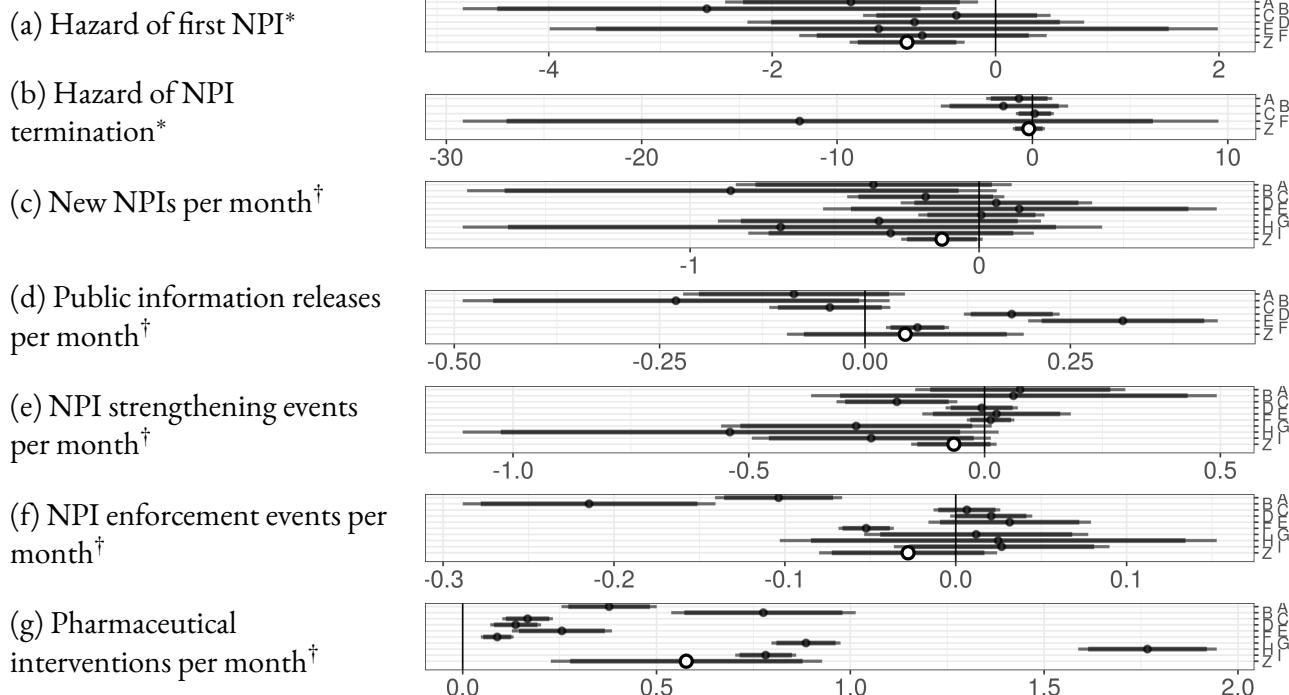
One potential answer is policy substitution. NPIs did not encompass the full range of public health measures available in 2020; competitive localities may have leaned more heavily on pharmaceutical interventions (PIs). This includes efforts to improve monitoring and early detection (e.g. providing free testing for educators and essential workers, opening community testing sites, requiring hospitals to report key disease metrics), efforts to improve medical treatment (e.g. hiring medical staff, addressing shortages of medical supplies, ventilators, protective and cleaning equipment) and, after December 2020, efforts to

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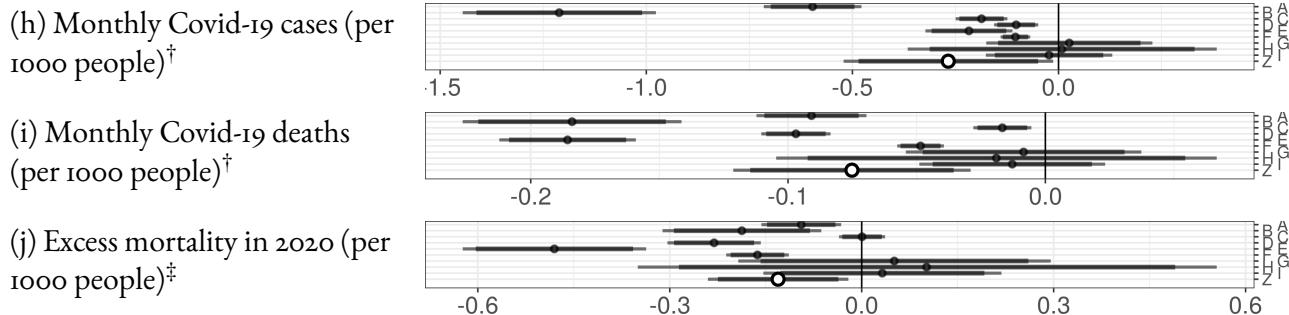
<sup>21</sup>Note that because the divergence between 1918 and 2020 was an unanticipated finding, the ancillary analyses in this section were not part of our pre-registered PAP ([doi.org/10.17605/OSF.IO/4NQGP](https://doi.org/10.17605/OSF.IO/4NQGP)).

Table 3: POLITICAL COMPETITION AND THE COVID-19 PANDEMIC IN U.S.

IMPACT OF ELECTORAL COMPETITION ON LOCAL PANDEMIC RESPONSE



IMPACT OF ELECTORAL COMPETITION ON PUBLIC HEALTH OUTCOMES



**INTERPRETATION:** Horizontal axis represents percent change in outcome, associated with a 1% increase in each measure of political competitiveness. Dots are point estimates; thin and thick lines are 95% and 90% confidence intervals. **MEASURES:** A: Top-2 Competitiveness (Congress), B: Top-1 Competitiveness (Congress), C: Contestation (Congress), D: Top-2 Competitiveness (Governor), E: Top-1 Competitiveness (Governor), F: Contestation (Governor), G: Top-2 Competitiveness (Mayor), H: Top-1 Competitiveness (Mayor), I: Contestation (Mayor), Z: Weighted mean effect estimate. **MODELS:** \*: Mixed effects Cox regression, †, ‡: Linear mixed model. Models account for population density, percent female, longitude, latitude, percent of population over 60, month (†) and region (\*, †, ‡) random effects. Observations weighted by 2010 population. Units of analysis are 2014 congressional districts (A-C) and 2010 counties (D-I). See Table A2.4 for fixed effects model estimates.

promote and distribute vaccines. This explanation is congruent with our hypothesis, in that PIs could be an appealing option for vulnerable incumbents, which was not as readily available in 1918 (Barry, 2020).

There is some empirical support for this view. Tables 2g and 3g report estimates from our main models, with monthly frequency of PIs as the dependent variable. In 1918 (2g), the relationship between competitiveness and PIs was on average positive but numerically small (0.0004%) and highly uncertain, in part due to the relative paucity of pharmaceutical measures available at the time. For 2020 (3g), the estimates were universally positive, numerically larger and statistically significant for all measure of competitiveness, at all levels of elections. According to the weighted mean, a 1 % increase in competitiveness yielded over a half-percentage point increase in local PIs per month.

The data further suggest that, in 2020, PIs were at least as effective as NPIs in reducing disease spread. To assess the efficacy of these measures, we regressed monthly differences in per capita cases and deaths on monthly differences in NPIs and PIs (lagged by one time period), with fixed effects for month.<sup>22</sup> We fit a separate model for PIs and each of the four types of NPIs we considered in Tables 2 and 3 (any new NPIs, information releases, strengthening events, enforcement events). The challenge in estimating the effect of policy interventions on cases and deaths is that authorities tend to intervene when case rates and deaths are already high. This selection problem inhibits identification of causal effects, and we make no claims of causality here. However, the direction of bias is almost certainly positive, attenuating estimates of a negative effect between increases in NPIs (or PIs) and subsequent changes in infections and deaths.<sup>23</sup>

The results, in Table 4, suggest that NPIs and PIs were both associated with reduced fatality and infection rates in 2020. During Covid-19, each additional PI was followed by 5 fewer cases per 1,000 people (4g), and 1 fewer death per 10,000 people (4h). In 1918, the impact of PIs was statistically indistinguishable

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<sup>22</sup>Our first difference models take the form

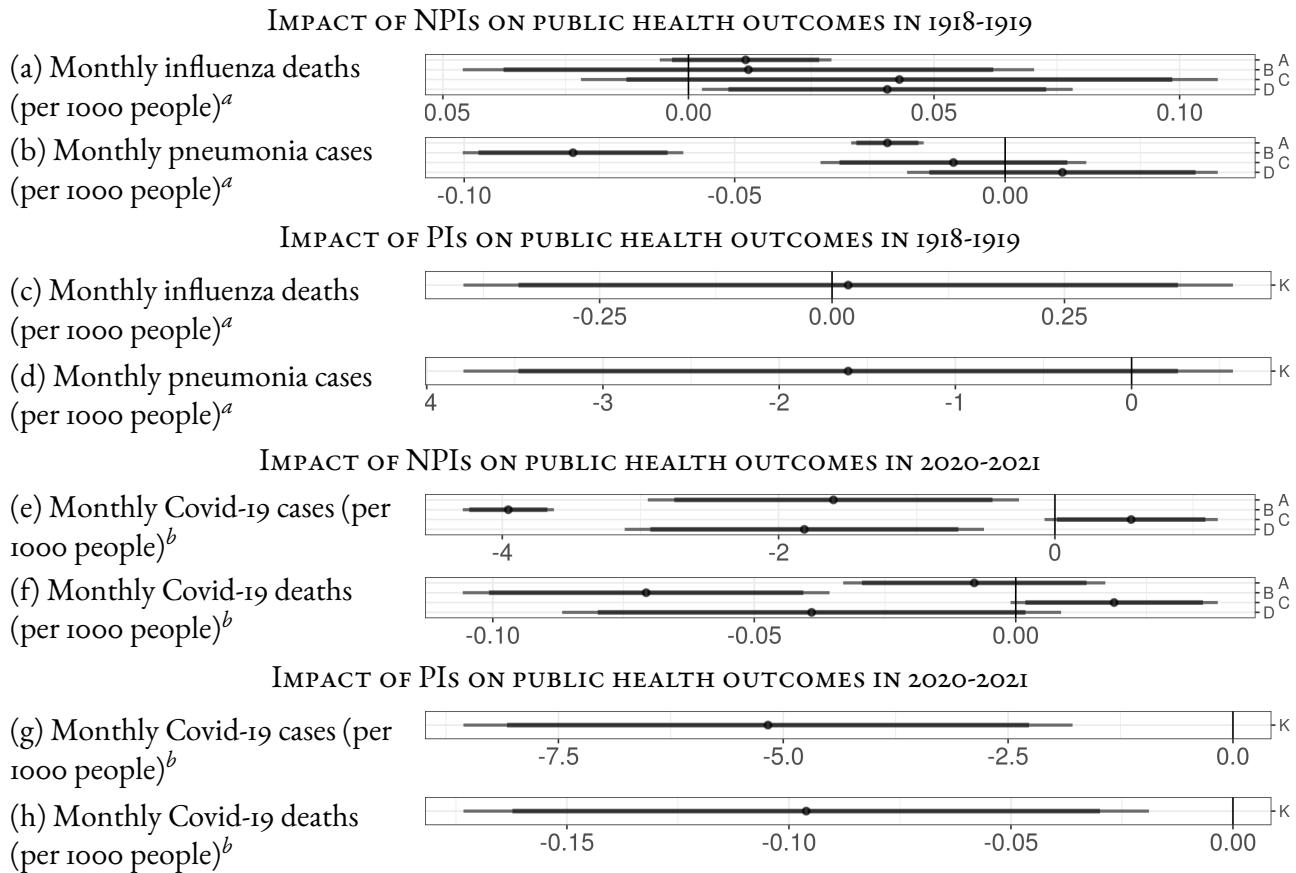
$$\Delta \text{outcome}_{it+1} = \Delta \text{NPI}_{it} \beta + u_t + \epsilon_{it} \quad (5)$$

where  $\Delta \text{NPI}_{it}$  is the current month's change in (N)PI intensity, and  $\Delta \text{outcome}_{it+1}$  is the next month's change in cases or deaths per 1000 people. Constituency fixed effects drop out due to first differencing.

<sup>23</sup>For example, the correlation between the raw number of NPIs and influenza deaths (per 1000) in 1918 — measured contemporaneously, without first differencing — is positive ( $\rho = .47$ ).

from zero. The impact of NPIs was more variable, but generally negative. In 1918, NPIs appeared to have little mitigating impact on monthly deaths — estimates for most measures are positive, suggesting that any associated reduction in deaths was too small to overcome selection effects. However, NPIs did precede significant decreases in severe illnesses, as measured by pneumonia cases (4c). In 2020 (4e,4f), similar declines followed months with more NPIs, more information releases, and more enforcement.

Table 4: EFFECTIVENESS OF NPIs AND PIs DURING 1918 INFLUENZA AND COVID-19 PANDEMICS



**INTERPRETATION:** Horizontal axis represents change in outcome associated with a unit increase in each type of NPI or PI measure. Dots are point estimates; thin and thick lines are 95% and 90% confidence intervals. Units of analysis are electoral constituencies (counties), September 1918–March 1919 and January 2020–May 2021. **MEASURES:** A: New NPIs, B: Information releases, C: NPI strengthening events, D: NPI enforcement events, K: PIs per month. **MODELS:** *a, b*: First difference models with time fixed effects. Observations weighted by 1910 (*a*) and 2010 (*b*) population.

Taken together, the evidence suggests that pharmaceutical interventions represented a potentially effective policy alternative in 2020, which can explain both the weaker relationship between competitiveness and NPIs, and the negative association between competitiveness and Covid-19 infections and mortality. This is a key distinction from 1918, when PIs were neither frequent nor effective.

By design, NPIs and PIs are complements, not substitutes. When confronting a novel virus, a key purpose of NPIs is to reduce the strain on hospitals until PIs become widely available. Yet many vulnerable incumbents did not follow this two-track approach in 2020, relying on PIs in lieu of NPIs. Why?

Political geography may help illuminate why this type of policy substitution was possible in 2020, but not 1918. Vulnerable incumbents may have reasoned that the community risk factors their constituents faced were relatively low, even early in the pandemic, by virtue of where they lived. One of the most significant changes to U.S. political geography since 1918 has been the rise of the suburb. These areas have some inherent advantages during an infectious disease outbreak: opportunities for physical contact outside the family are fewer where the population lives in single-unit homes, and relies on automobiles rather than public transit. If, as recent scholarship suggests, suburban areas have grown more diverse and politically competitive, occupying a middle ground in the urban-rural political divide (Scala and Johnson, 2017), then this geographic change may help explain both why vulnerable incumbents implemented fewer NPIs and why they saw fewer deaths — the former may have seemed unnecessary to prevent the latter.

The data offer some support for this proposition. First, more competitive localities in 2020 did tend to be more suburban. We estimated a series of semi-parametric Generalized Additive Models, regressing each competitiveness measure on a spline of urbanization and regional/state fixed effects (Table A4.15). Prior to 1918, when suburbs effectively did not exist, there was no consistent relationship between urbanization and competitiveness. By 2020 the relationship became more consistently concave, with higher competitiveness in localities with lower and intermediate population density (i.e. exurbs, suburbs), and lower competitiveness in densely-populated areas. The only exception was for mayoral races (more density, more competitiveness), albeit chiefly because we observe these election data only in major cities.

Second, the data confirm that populations within more competitive constituencies had fewer underlying socioeconomic risk factors. To probe this relationship, we regressed a series of social vulnerability measures from the COVID-19 Community Vulnerability Index (Smittenaar et al., 2021) on electoral competitiveness and the same set of covariates and random effects as in our baseline specification. We find a positive correlation between most measures of competitiveness and per capita income, and a negative correlation with the share of households with no motor vehicle, and percent of the population that is

uninsured, living in group homes, mobile homes or crowded conditions (Table A4.16). These patterns suggest, among other things, that populations in these areas could more easily adapt their daily routines to avoid exposure (e.g. work from home, rely less on public transit if they did need to commute, more easily socially distance), and obtain adequate treatment if they were exposed (Hong et al., 2021).

There is also evidence that — even without government mandates — people living in competitive localities were more likely to change their behavior in ways that mitigated disease risk. Using data from Apple’s Mobility Trend Reports, we found that more competitive localities experienced steeper early (i.e. before the first stay-at-home orders) declines in foot, personal vehicle and public transportation traffic than less competitive localities (Table A4.17). Furthermore, areas with steeper early declines in mobility subsequently experienced fewer cases and deaths (Table A4.18).

#### 4.1 Alternative Explanations

We considered multiple other potential explanations of the divergence between 1918 and 2020, including changes in the political context and epidemiological differences between H1N1 and Covid-19. While space constraints allow only a partial overview here (see Appendix A4 for details), available evidence permits us to exclude several of these explanations as empirically implausible. Others, however, warrant closer study.

1. ELECTORAL TIME HORIZONS (Appendix A4.1). There is little evidence that competitiveness increases NPIs only among incumbents with longer time horizons, or that the timing of reelection accounts for the differences between 1918 and 2020. The positive relationship between competitiveness and NPIs in 1918 was significantly *stronger* among incumbents facing reelection that year. In line with our policy substitution story for Covid-19, the impact of competitiveness on pharmaceutical interventions was also larger and more positive among incumbents up for reelection.
2. EXPANDED ROLE OF FEDERAL GOVERNMENT (Appendix A4.2). The data do not support the idea that more aggressive federal action in 2020 — or a larger federal public health footprint — created incentives for local incumbents to free-ride. The relationship between NPIs and electoral competitiveness remains negative and significant after conditioning on multiple local measures of federal public health capacity.

3. CHANGES IN THE ELECTORAL FRANCHISE (Appendix A4.3). We find no evidence that NPIs became more politically costly due to changes in the electorate, given the exclusion of most women and African Americans from voting in 1918. We also investigate whether state-by-state variation in voter eligibility confounds the positive relationship from 1918, particularly if more electorally competitive constituencies were in states that had already expanded the franchise to women.
4. CHANGES TO THE MEDIA ENVIRONMENT (Appendix A4.4). We cannot definitively rule out the possibility that the media landscape in 1918 (e.g. wartime censorship) suppressed anti-NPI mobilization. Using data on historical local and regional newspapers, we show that in 1918 authorities did implement fewer NPIs in more diverse media markets. However, competitiveness remains a strong positive predictor of NPIs, after accounting for characteristics of the local media market.
5. PARTISANSHIP AND POLARIZATION (Appendix A4.5). We find no evidence that the negative estimates in Table 3a-3f reflect the fact that Republicans controlled more state governments than Democrats in 2020. We also show that competitiveness affected Democratic and Republican incumbents in similar ways: inter-party heterogeneity in NPIs was no greater in 2020 than in 1918.
6. DIVIDED STATE GOVERNMENT (Appendix A4.6). We find no evidence that NPIs became harder to implement due to divided state government — where different parties control the legislature, governor's mansion, and/or senior executive offices like lieutenant governor and attorney general, creating opportunities for stonewalling.
7. (IN)EFFECTIVENESS OF NPIs (Appendix A4.7). We find no evidence that vulnerable incumbents avoided NPIs on efficacy grounds. As Table 4 shows, NPIs were no less effective at reducing cases and deaths in 2020 than in 1918.
8. DIFFERENCES IN DISEASE PATHOLOGY (Appendix A4.8). We find no evidence that the 1918 pandemic generated greater support for aggressive action because it affected a broader cross-section of society. Our results hold when we account for differences in age distributions of cases and deaths.

9. VACCINATION (Appendix A4.9). We find no evidence that the anticipation of a Covid-19 vaccine discouraged the implementation of NPIs, or that a vaccine-related phase shift is driving our results.
10. LOCAL PUBLIC HEALTH CAPACITY (Appendix A4.10). We find mixed evidence that more competitive constituencies implemented fewer NPIs because they had greater local public health capacity prior to Covid-19. While locations with competitive mayoral elections spent more per capita on public health, more competitive congressional districts spent less, and had fewer hospitals, beds, and emergency medical services than less competitive constituencies.

This list is not exhaustive, and our ancillary analyses cannot exclude all potential substantive explanations for divergent NPI responses. However, almost none of the explanations that we have considered here can account for both (a) why the relationship between electoral competitiveness and NPIs was weaker in 2020, and (b) why competitive localities nonetheless experienced more favorable public health outcomes. Available evidence suggests that policy substitution (competitive localities relying on pharmaceutical interventions) offers the most empirically plausible explanation for these observed phenomena.

## 5 Conclusion

More vulnerable incumbents face stronger political incentives to slow a virus' spread. How incumbents respond to these incentives, however, has changed in the last 100 years. Data from the 1918 H1N1 influenza outbreak suggest that more politically competitive constituencies imposed more nonpharmaceutical interventions, kept them in place longer, promoted them more aggressively and may have also enforced them more often. More competitive localities also saw fewer deaths directly attributable to the flu, fewer overall excess deaths in 1918, and fewer cases of pneumonia — the leading cause of influenza-related deaths. During the Covid-19 pandemic, more competitive localities also saw significantly lower death rates. Yet this time vulnerable leaders favored pharmaceutical interventions over NPIs, and the low death rates were at least partially due to an epidemiologically-significant change in political geography: more competitive areas in 2020 tended to be suburban, where social distancing was easier due to lower population density, and where community socioeconomic risk factors were less severe.

These results hold across multiple measures of political competitiveness, different levels of elections (congressional, gubernatorial, mayoral), multiple types of policy interventions, different data sources and measurement strategies, and data samples covering different geographic regions and periods of time. We also considered several alternative explanations, including changes in federal responses, the electoral franchise, partisanship, divided government, NPI effectiveness, disease pathology, and public health capacity.

Unanswered questions remain. First, our analyses treat past electoral outcomes as exogenous to epidemic response. While it is doubtful that expectations of emergency management during a once-in-a-century global pandemic was front of mind for voters in 1916 or 2018, it is certainly possible that the recent history of local responses to smaller health crises and natural disasters informed voter choice.

Another important question is whether the political geographic correlates of pre-2020 political competitiveness — especially as regards lower socioeconomic risk — are the result of self-selection (i.e. wealthier citizens moving to more competitive areas), or the reverse (i.e. citizens become wealthier due to competitiveness). The first possibility would run counter to recent research on migration into less competitive, more politically homogeneous areas (Frey, 2018), although it would be in line with trends of suburban population growth (Scala and Johnson, 2017).

Local governments vary substantially in their responses to public health emergencies. Some governments may fail to act due to a lack of capacity. Others seem to have this capacity, but still choose not to act. Understanding how political accountability affects these choices is important for advancing human welfare, given that epidemics can affect economic, social, and civic well-being as well as public health. Infectious diseases, like war and violence, interrupt many areas of life, including education, work, and family. Government actions that fail to protect citizens' health, or which knowingly make matters worse, deserve the same priority attention in political science as state actions that result in citizens' violent deaths.

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# Online Appendix

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## A1 NPI Event Classification

To measure NPI events, we employ a recurrent neural network (RNN) model with long short-term memory (LSTM) (Hochreiter and Schmidhuber, 1997). LSTMs are well-suited for learning problems related to sequential data, such as sequences of words of differential length, where the vocabulary is potentially large, and where the long-term context and dependencies between inputs are potentially informative for classification.<sup>1</sup> We employed a standard “vanilla LSTM” architecture (Graves and Schmidhuber, 2005), using the `keras` library in Python 3 (Chollet, 2015).<sup>2</sup>

<sup>1</sup>For an introduction to LSTMs, with applications to political science, see Chang and Masterson (2020).

<sup>2</sup>At the center of this architecture is a memory cell and non-linear gating units, which regulate information flow into and out of the cell. A “vanilla LSTM” block features three gates (input, forget, and output), block input, a single cell, and an output activation function. The block’s output recurrently connects back to the block input and all gates. Greff et al. (2017) demonstrated that this architecture performs well on a variety of classification tasks, and that common modifications do not significantly improve performance.

Our text corpus included 3,372 city timeline entries from the Influenza Encyclopedia (UM Center for the History of Medicine, 2016). We extracted a random sample of 800 entries (24%) to use as a training set, which we split between five human coders, including the co-authors and three research assistants. Each coder received 200 timeline entries, 50 of which were the same for each coder — to assess inter-coder reliability — and the remaining 150 of which were unique ( $50 + 150 \times 5 = 800$ ). We wrote a simple computer program, which displayed the text of an event description in a terminal window, and then prompted the coder to answer a series of yes/no questions about its content, particularly whether the event belonged to any of the 11 categories listed below:

```
[1] "EVENT DESCRIPTION: The City Board of Health issues an order requiring everyone to wear protective masks everywhere except at home and on the streets."  
[1] "-----"  
1 Tone of News Update? (+/-/0/?)  
1 First Reported Case? (Y/N/?)  
1 Nonpharmaceutical Intervention (NPI)? (Y/N/?)  
1 Punishment? (Y/N/?)  
1 Closures and Postponements? (Y/N/?)  
1 Public Information Statement? (Y/N/?)  
1 Pharmaceutical Intervention? (Y/N/?)  
1 Public Official Inaction? (Y/N/?)  
1 Public Debate? (Y/N/?)  
1 Civil Unrest or Protest? (Y/N/?)  
1 Reopening? (Y/N/?)  
1 Offers or Requests for Aid? (Y/N/?)  
1 Comments? (optional)  
1 Do you want to re-enter your responses? (Y/N)
```

To minimize misclassification errors at the training set stage, the program permitted coders to enter "?" for a short definition if they were unsure of what a particular category meant.<sup>3</sup> Coders also had the opportunity to re-enter their responses if they made a

---

<sup>3</sup>For the categories in Table 2, definitions were: *Nonpharmaceutical Intervention*: Included in this category are a wide range of public interventions into society and business to combat the pandemic. These statements should announce a new policy or intervention or mandate, not the existence of a continuing policy. Anything that is not directly related to a vaccine, salve, or specific medical treatment falls under this category of intervention. This can include increased messaging, mask-wearing, quarantine, impositions of penalties or fines, and closures. Statements of intention, like “debating a ban” or “intends to close” are not NPIs. *Closures and Postponements*: These are NPIs that specifically close businesses, public spaces, theaters, schools, etc. They may also call for the postponement or suspension of activities like meetings or practices like funerals. Do not infer a closure - look for explicit announcement of a

mistake, and to submit open-ended comments to flag hard calls and other issues. Reliability tests indicate a high level of inter-coder agreement, with Krippendorf's  $\alpha$  statistics ranging from a low of 0.48 ("Public Official Inaction") to a maximum of 0.92 ("Offers or Requests for Aid").<sup>4</sup> For the four categories we used in Table 2,  $\alpha$  statistics ranged from 0.65 ("Nonpharmaceutical Intervention") to 0.74 ("Enforcement").

To preprocess the text, we mapped each of the 3,372 timeline events into a real vector domain, with each word represented as an embedding vector of length 100.<sup>5</sup> We limited the total number of words used in modeling to the 5000 most frequent ones. Because the number of words per entry was variable, with a median of 22, we truncated longer event descriptions to a maximum of 50 words, and padded shorter entries with zero values. We used an LSTM layer with 100 memory units, and a dense output layer with a sigmoid activation function for binary predictions. We fit the model using the efficient ADAM optimization algorithm, with binary cross-entropy as the loss function. For most categories, the network achieved convergence at < 100 epochs, with median loss of 0.12 (minimum of 0.03, maximum of 0.48) and median training accuracy of 0.95 (0.80, 0.99).

Figure A1.2 show word clouds for event descriptions in each of the categories in the main analysis.<sup>6</sup> The font size is proportional to word frequencies in the LSTM-predicted test set, for events predicted as being most likely to belong to each category (99th percentile). The word frequencies generally align with our qualitative understanding of these categories, with terms such as "close... school" appearing in the closure category (Figure A1.2c) and "violat... arrest" appearing in the enforcement category (A1.2d).

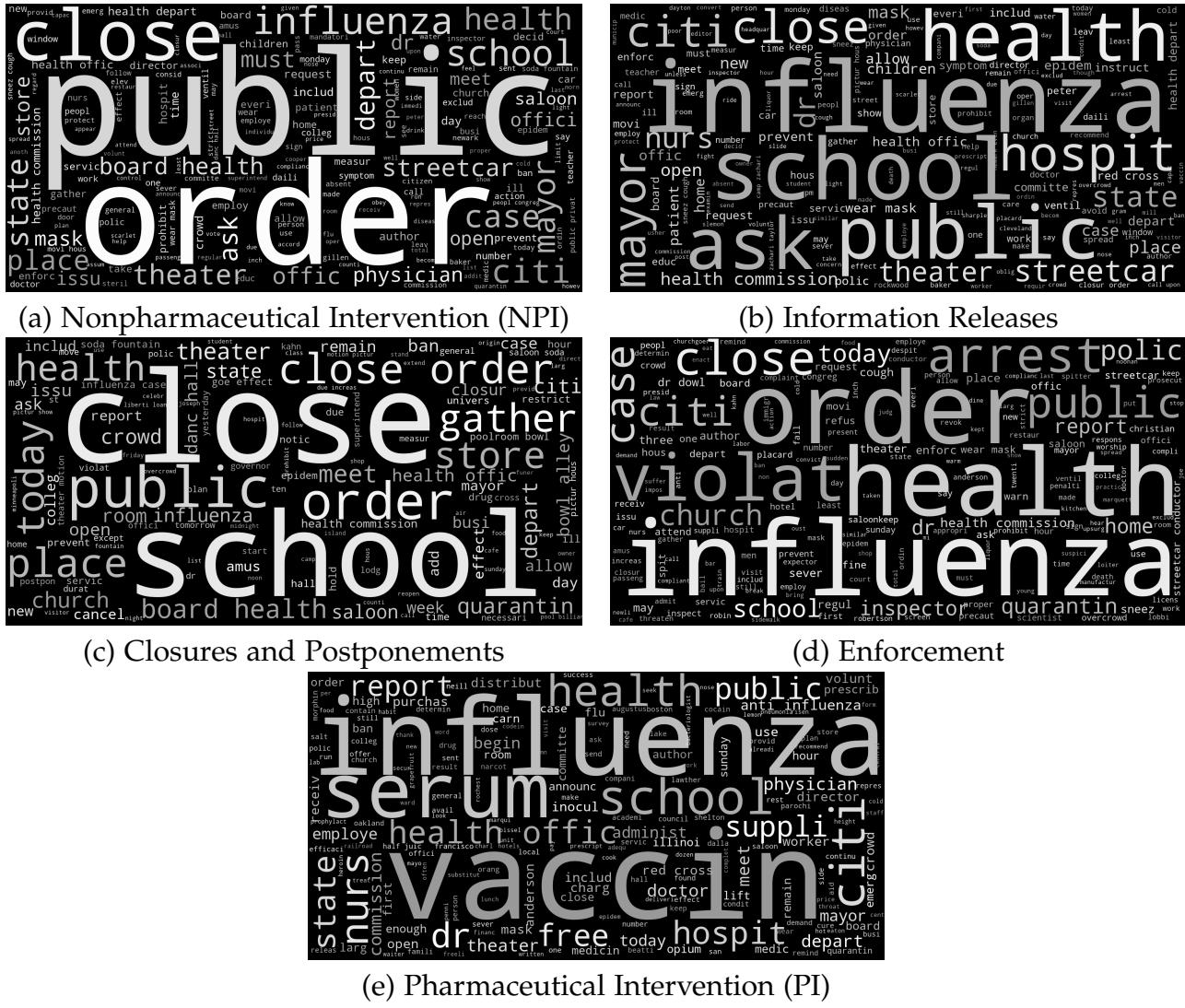
---

closure, or stopped activities. *Public Information Statement*: Messages, adverts, or other information campaigns intended to inform the public. *Enforcement*: These are NPIs which use a stick, not a carrot. They usually involve jail or fines for non-compliance. *Pharmaceutical Intervention*: These include any mention of testing, receipt, or use of vaccines, salves, serums, or medical treatments.

<sup>4</sup>Krippendorf's  $\alpha$  has a theoretical range from -1 to 1, with 1 indicating perfect reliability, negative values indicating systematic disagreement, and 0 indicating that the codings are statistically unrelated.

<sup>5</sup>The purpose of this step is to encode words as real-valued vectors in a high dimensional space, where words more similar in meaning appear closer in the vector space.

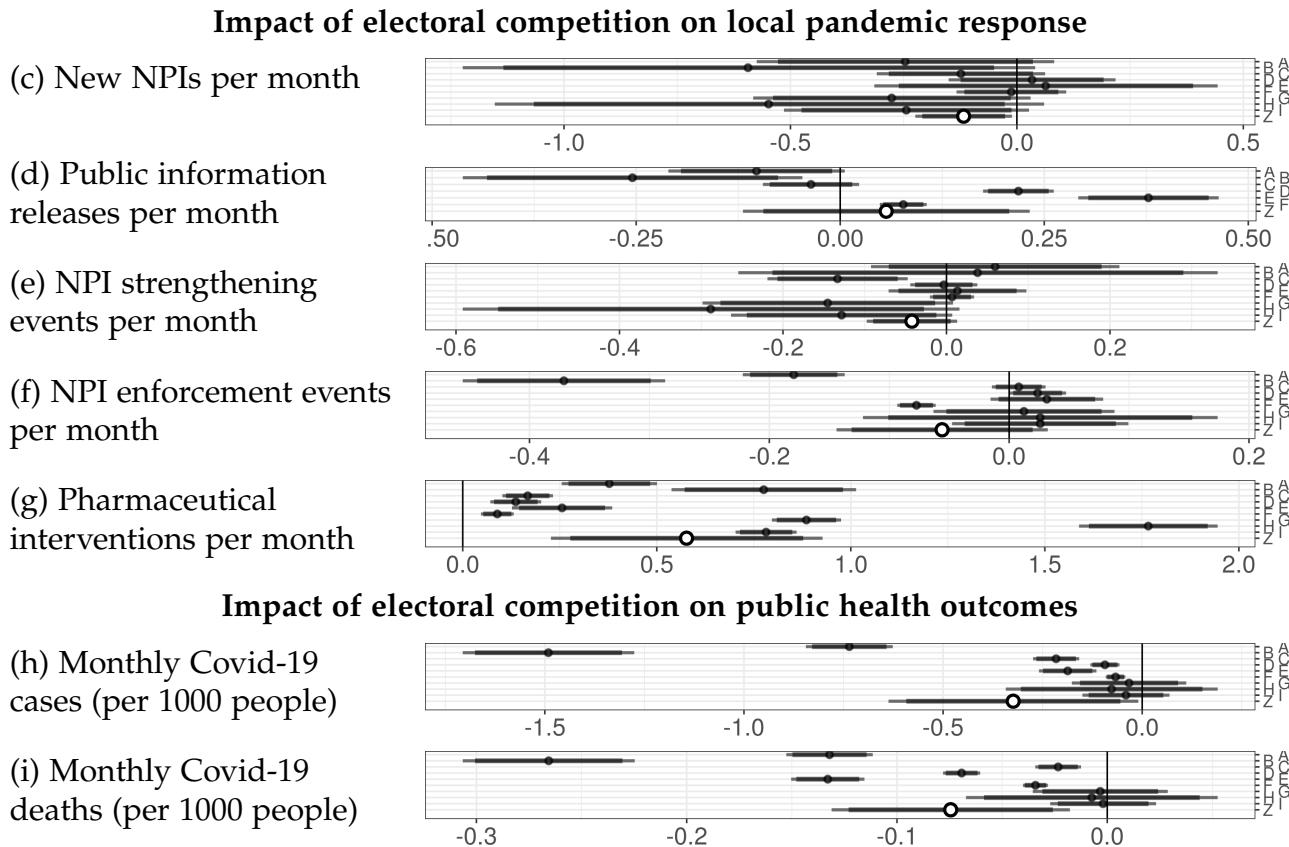
<sup>6</sup>To avoid double-counting measures that are already included in the "NPI" category, our "NPI strengthening events" category includes only closures and postponements that represent an escalation or extension of existing measures.



**Figure A1.2: Word clouds of LSTM NPI event classifications, test set.** Font size is proportional to word frequency. This figure presents events whose predicted value of belonging in each category is in the 99th percentile.

## A2 Sensitivity Analyses

Table A2.2: Re-estimation of models in Table 3 with date range extended to May 2021.

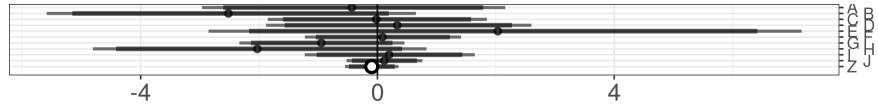


**Interpretation:** Horizontal axis represents percent change in outcome, associated with a 1% increase in each measure of political competitiveness. Dots are point estimates; thin and thick lines are 95% and 90% confidence intervals. **Measures:** A: Top-2 Competitiveness (Congress), B: Top-1 Competitiveness (Congress), C: Contestation (Congress), D: Top-2 Competitiveness (Governor), E: Top-1 Competitiveness (Governor), F: Contestation (Governor), G: Top-2 Competitiveness (Mayor), H: Top-1 Competitiveness (Mayor), I: Contestation (Mayor), Z: Weighted mean effect estimate. **Models:** Linear mixed effects model. Models account for population density, percent female, longitude, latitude, percent of population over 60, month and state random effects. Observations weighted by 2010 population. Units of analysis are 2014 congressional districts (A-C) and 2010 counties (D-I).

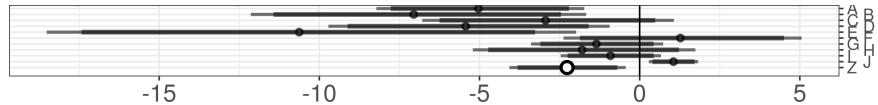
Table A2.3: Re-estimation of models in Table 2 with fixed effects

### Impact of electoral competition on local pandemic response

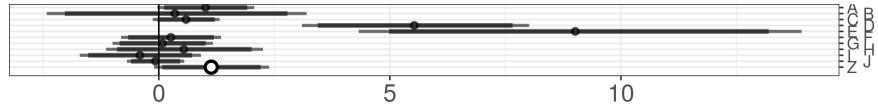
(a) Hazard of first NPI\*



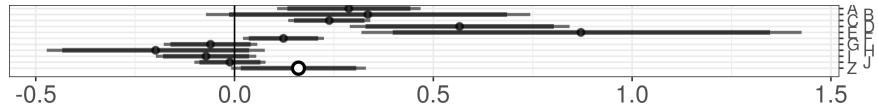
(b) Hazard of NPI termination\*



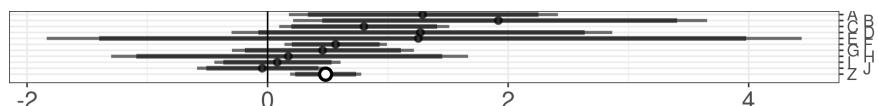
(c) New NPIs per month<sup>†</sup>



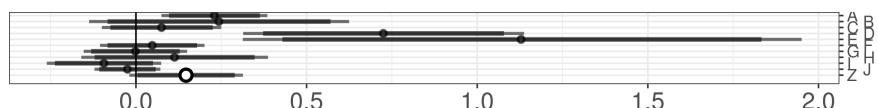
(d) Information releases per month<sup>†</sup>



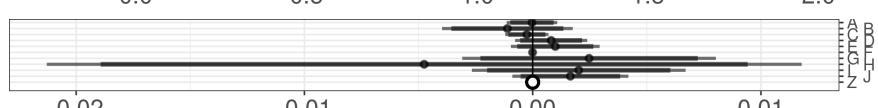
(e) NPI strengthening events per month<sup>†</sup>



(f) NPI enforcement events per month<sup>†</sup>

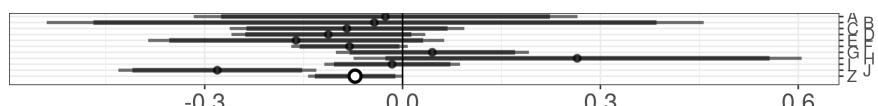


(g) Pharmaceutical interventions per month<sup>†</sup>

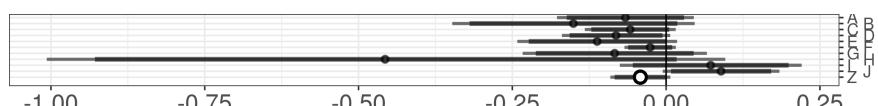


### Impact of electoral competition on public health outcomes

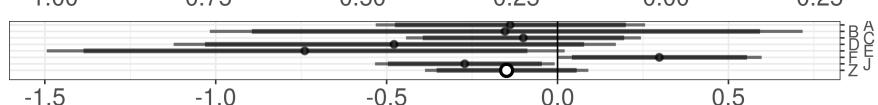
(g) Monthly influenza deaths (per 1000 people)<sup>†</sup>



(h) Monthly pneumonia cases (per 1000 people)<sup>†</sup>



(i) Excess mortality in 1918 (per 1000 people)<sup>‡</sup>



**Interpretation:** Horizontal axis represents percent change in outcome, associated with a 1% increase in each measure of political competitiveness. Dots are point estimates; thin and thick lines are 95% and 90% confidence intervals. **Measures:** A: Top-2 Competitiveness (Congress), B: Top-1 Competitiveness (Congress), C: Contestation (Congress), D: Top-2 Competitiveness (Governor), E: Top-1 Competitiveness (Governor), F: Contestation (Governor), G: Top-2 Competitiveness (Mayor), H: Top-1 Competitiveness (Mayor), I: Contestation (Mayor), J: No Machine (City); Z: Weighted mean effect estimate. **Models:** \*: Cox regression, †, ‡: Linear fixed effects model. Models account for distance to nearest army base, percent urban, percent female, longitude, latitude, weather (†, ‡), region (\*, †, ‡) and month (†) fixed effects. Observations weighted by 1910 population. Units of analysis are 1916 congressional districts (A-C) and 1910 counties (D-J), September 1918–March 1919

Table A2.4: Re-estimation of models in Table 3 with fixed effects

### Impact of electoral competition on local pandemic response

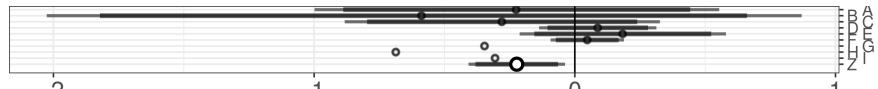
(a) Hazard of first NPI\*



(b) Hazard of NPI termination\*



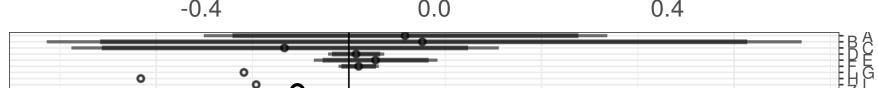
(c) New NPIs per month<sup>†</sup>



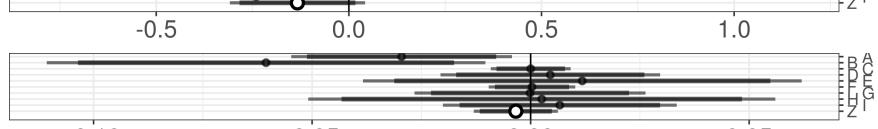
(d) Public information releases per month<sup>†</sup>



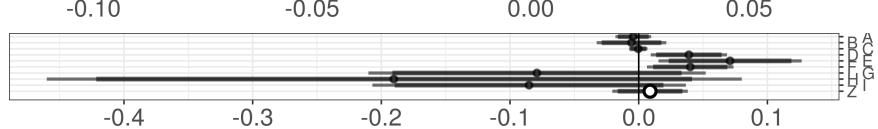
(e) NPI strengthening events per month<sup>†</sup>



(f) NPI enforcement events per month<sup>†</sup>

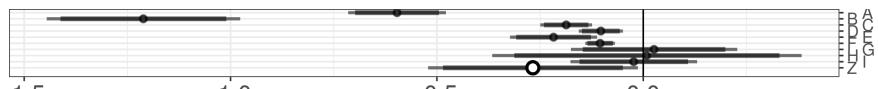


(g) Pharmaceutical interventions per month<sup>†</sup>



### Impact of electoral competition on public health outcomes

(h) Monthly Covid-19 cases (per 1000 people)<sup>†</sup>



(i) Monthly Covid-19 deaths (per 1000 people)<sup>†</sup>



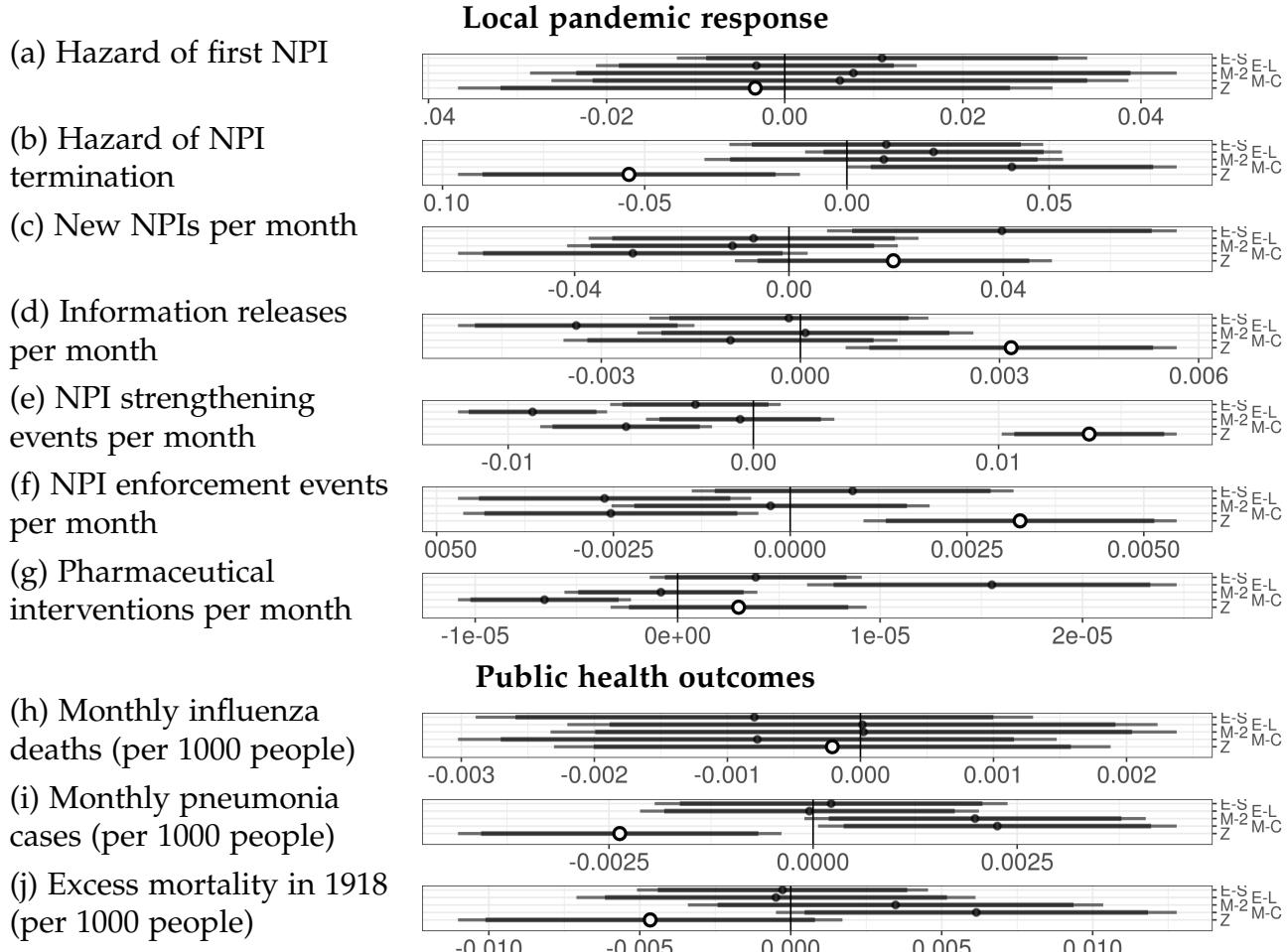
(j) Excess mortality in 2020 (per 1000 people)<sup>‡</sup>



**Interpretation:** Horizontal axis represents percent change in outcome, associated with a 1% increase in each measure of political competitiveness. Dots are point estimates; thin and thick lines are 95% and 90% confidence intervals. **Measures:** A: Top-2 Competitiveness (Congress), B: Top-1 Competitiveness (Congress), C: Contestation (Congress), D: Top-2 Competitiveness (Governor), E: Top-1 Competitiveness (Governor), F: Contestation (Governor), G: Top-2 Competitiveness (Mayor), H: Top-1 Competitiveness (Mayor), I: Contestation (Mayor), Z: Weighted mean effect estimate. **Models:** \*: Fixed effects Cox regression, †, ‡: Linear fixed effects model. Models account for population density, percent female, longitude, latitude, percent of population over 60, month (†) and state (\*, †, ‡) fixed effects. Observations weighted by 2010 population. Units of analysis are 2014 congressional districts (A-C) and 2010 counties (D-I).

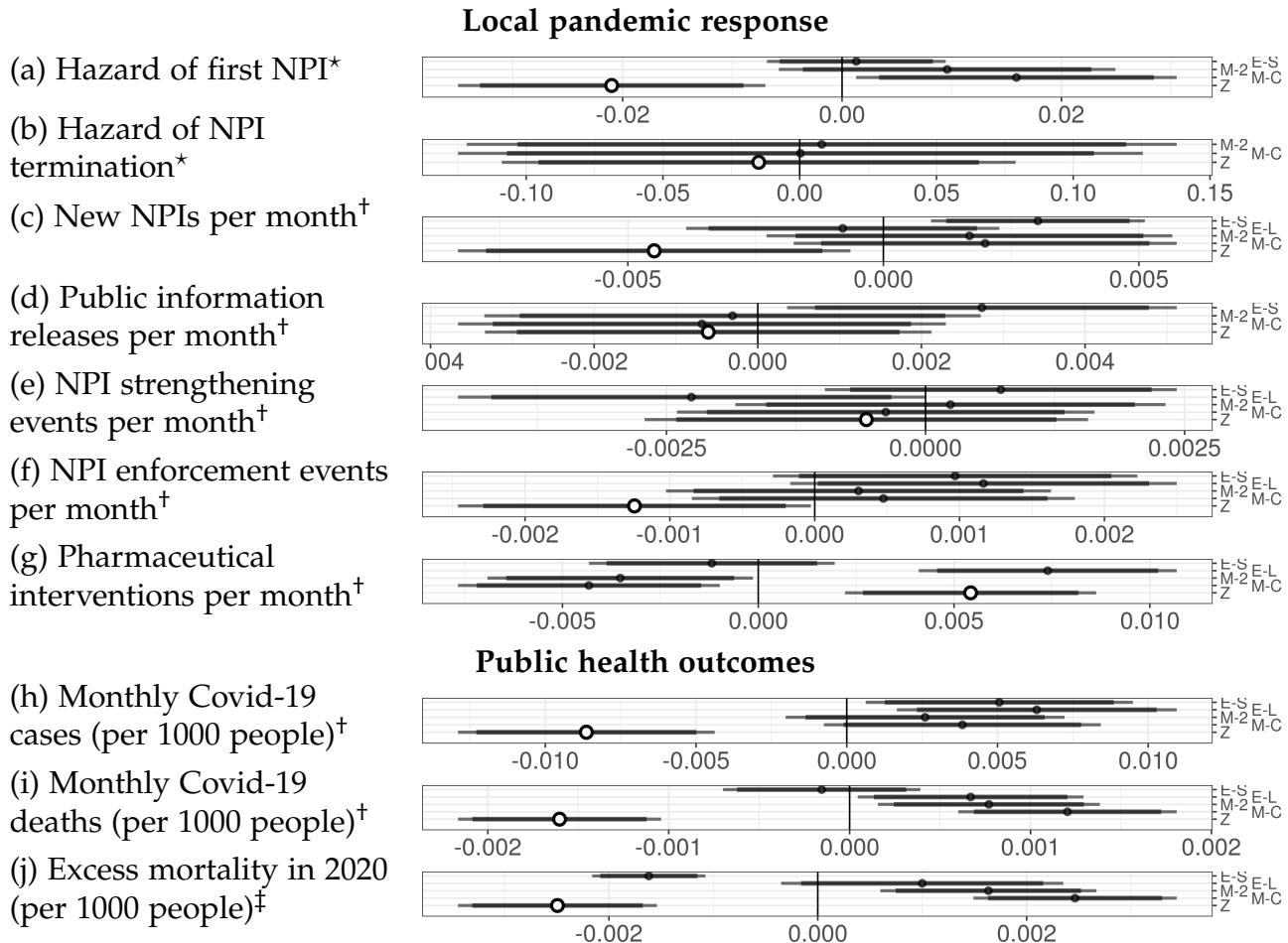
## A3 Meta Analytic Mixed Effects Models

Table A3.5: Meta-analytic mixed effects models (1918 Influenza pandemic in U.S.)



**Interpretation:** Horizontal axis represents change in weighted mean competitiveness effect estimate, associated with measure-specific and election-specific factors. Reference categories are federal elections and Top-1 Competitiveness. Dots are point estimates; thin and thick lines are 95% and 90% confidence intervals. **Measures:** E-S: State Elections (relative to Federal); E-L: Local Elections (relative to Federal); M-2: Top-2 Competitiveness (relative to Top-1); M-C: Contestation (relative to Top-1); M-N: No Machine (relative to Top-1); Z: Weighted mean effect estimate (intercept). **Models:** Meta-analytic mixed effects.

Table A3.6: Meta-analytic mixed effects models (Covid-19 pandemic in U.S.)



**Interpretation:** Horizontal axis represents change in weighted mean competitiveness effect estimate, associated with measure-specific and election-specific factors. Reference categories are federal elections and Top-1 Competitiveness. Dots are point estimates; thin and thick lines are 95% and 90% confidence intervals. **Measures:** E-S: State Elections (relative to Federal); E-L: Local Elections (relative to Federal); M-2: Top-2 Competitiveness (relative to Top-1); M-C: Contestation (relative to Top-1); Z: Weighted mean effect estimate (intercept). **Models:** Meta-analytic mixed effects.

## A4 Analysis of Differences Between 1918 and 2020

This discussion provides details on the ancillary analyses mentioned in Section 4.

### A4.1 Electoral Time Horizons

Part of the logic behind the main alternative hypothesis (“more competition, fewer NPIs”) is that incentives to implement these measures depend on how heavily politicians and voters discount the future benefits of these policies: incumbents who face

reelection before these benefits are realized (but after voters pay the short-term costs) are less likely to adopt NPIs. Both 1918 and 2020 were election years for the U.S. House of Representatives, although the timing of gubernatorial and mayoral elections was more variable. If more governors and mayors faced reelection in 2020 (a presidential election year) than in 1918, then the timing of reelection may explain differences in NPI policy.

We test this possibility by including in our linear mixed models an interaction between electoral competitiveness and a “reelection year” indicator for governors and mayors in 1918 and 2020 — the lower-order interaction term is unobserved for congressional races, since all incumbents faced reelection.<sup>7</sup>

The results of these analyses (Table A4.7) run counter to this view. Among incumbents with shorter time horizons (i.e. facing reelection, right pane), competitiveness had a stronger and larger positive impact on NPIs in 1918, and on PIs in 2020. For incumbents who were not up for reelection in 1918, the competitiveness effect was negative in some NPI categories (A4.7a, A4.7c) and null for others, suggesting that the positive relationships reported in Table 2 were driven overwhelming by incumbents facing reelection. This is the opposite of what we would expect if longer time horizons gave incumbents more latitude; the evidence here suggests that, if anything, longer time horizons reduced the urgency of disease mitigation in 1918. The patterns for Covid-19 are more variable in sign and magnitude. Although incumbents facing reelection in 2020 did appear more hesitant to implement NPIs, they were far more likely — by an order of magnitude — to implement PIs. These results are supportive of our main hypothesis and provide additional evidence of NPI-to-PI policy substitution in 2020.

## A4.2 Expanded Role of Federal Government

Vulnerable incumbents might have implemented fewer NPIs in 2020 because more aggressive federal action created incentives to free-ride. In 1918, there was effectively no unified federal response to the pandemic, and all initiative resided at the state and local level. In 2020, the federal government imposed travel restrictions, provided economic incentives to stay at home, and an eviction moratorium to lessen the pandemic’s impact on daily life (Centers for Disease Control and Prevention, 2020; Senate and Congress, 2020). Free-riding contradicts our hypothesis, since it assumes NPIs are so inherently politically costly that local leaders would rather pass the buck to the federal government.

To test for local free-riding, we expanded the linear mixed models in Table 3c-3f to

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<sup>7</sup>These linear mixed models take the form

$$\begin{aligned} \log(\text{outcome}_{it} + 1) &= c_i^{(k)}\gamma_1 + \text{Reelection}_i\gamma_2 + c_i^{(k)} \times \text{Reelection}_i\gamma_3 + X_{it}\beta + z_j + u_t + \epsilon_{it}, \\ z_j &\sim \mathcal{N}(0, \Sigma(\theta)_z), \quad u_t \sim \mathcal{N}(0, \Sigma(\theta)_u) \end{aligned} \quad (1)$$

The quantities in Table A4.7 correspond to  $(\exp(\hat{\gamma}_1) - 1) \times 100$  (left) and  $(\exp(\hat{\gamma}_1 + \hat{\gamma}_3) - 1) \times 100$  (right). We calculated standard errors for  $\hat{\gamma}_1 + \hat{\gamma}_3$  as  $\sqrt{\text{Var}(\hat{\gamma}_1) + \text{Var}(\hat{\gamma}_3) + 2\text{Cov}(\hat{\gamma}_1, \hat{\gamma}_3)}$ .

include several covariates measuring federal public health capacity. These include, in separate models, the proportion of a constituency's hospitals that are federally owned, the federal share of local hospital beds, and federal share of emergency medical stations (EMS). We assume that incentives for local free-riding should be greater where the federal government has a larger public health footprint.

The relationship between NPIs and electoral competitiveness remains negative and significant after conditioning on each of these variables (Table A4.8a-A4.8d). The relationship between NPIs and federal public health capacity (Table A4.8e-A4.8h) is more complex: constituencies with greater federal hospital capacity did see a lower rate of NPI enforcement, but those with a high federal share of EMS stations consistently saw *more* locally-imposed NPIs.

### A4.3 Changes in the Electoral Franchise

A third possibility — also at odds with our hypothesis — is that NPIs became more politically costly due to changes in the electorate, given the exclusion of most women and African Americans from voting in 1918. It is unclear why an expansion of the franchise would create incentives against NPIs. Existing research suggests that female voters are more supportive of welfare and health spending (Husted and Kenny, 1997; Lizzeri and Persico, 2004), and — during Covid-19 — were more likely to comply with public health orders and express concern over the virus (Galasso et al., 2020). Past studies have found similar policy preferences (Kidd et al., 2007) and NPI compliance (Gette et al., 2021; Hou et al., 2021) among African Americans.

A related possibility is that state-by-state variation in voter eligibility confounds the positive relationship from 1918, particularly if more electorally competitive constituencies were in states that had already expanded the franchise to women.<sup>8</sup> The data do not support this proposition: there is a generally positive relationship between NPIs and women's right to vote in 1916, but our results do not change when conditioning on pre-1918 women's enfranchisement (Table A4.9).

### A4.4 Media Environment

A fourth alternative explanation is that vulnerable incumbents implemented more NPIs in 1918 because they faced less scrutiny in the media. Wartime censorship in 1918 reduced the ability of anti-NPI activists to coordinate and protest (Taylor, 1980; Library of Congress, 2019). In 2020, the media landscape was structurally more conducive to anti-NPI mobilization, becoming at once more nationalized and more fragmented (Tucker,

<sup>8</sup>The most recent pre-1918 elections occurred during the peak period of the Suffragist movement, but before women won the right to vote at the federal level in 1920. State level franchise expansion was inconsistent: women had some right to vote in 12 states by 1916, and 21 states by 1918.

Guess, Barberá, Vaccari, Siegel, Sanovich, Stukal and Nyhan, 2018; Utych, 2021). Both developments should reduce coordination problems for anti-NPI activists, and raise barriers to NPI implementation. Like the first two explanations, this one assumes that competitiveness makes incumbents wary of adopting controversial policies.

This perspective is more difficult to empirically evaluate, but we can test one of its implications: that barriers to NPI implementation are greater where the media landscape is more diverse. To this end, we constructed a database of 547 local and regional U.S. newspapers active in 1918, using the Library of Congress' *Chronicling America* digital collection.<sup>9</sup> We used the addresses of newspapers' home bureaus to construct an index of local media fractionalization, which ranges from 0 to 1 and carries a simple interpretation: the probability that two random news articles about the flu were published by different newspapers.<sup>10</sup> If our 1918 results hold after conditioning on this variable, it would provide some evidence against media's role.

Competitiveness remains a strong predictor of NPIs, net of media fractionalization (Table A4.10). However, coefficients on our fractionalization index are generally negative. If we assume that scrutiny of local governments is increasing in the number of local news outlets, then these results do appear to suggest that NPIs were harder to implement where the media landscape was more dynamic and diverse.<sup>11</sup> That said, given how extensively the media environment has changed since 1918 — especially as regards the decline of local news, and the rise of social media — we consider this test suggestive, not conclusive (Tucker, Guess, Barberá, Vaccari, Siegel, Sanovich, Stukal and Nyhan, 2018).

## A4.5 Partisanship and Polarization

A fifth explanation is that the negative NPI estimates in Table 3 simply reflect the fact that Republicans controlled more state governments than Democrats in 2020. Political support for NPIs became divided along partisan lines, and vulnerable incumbents were reluctant to enact policies that alienate their base supporters. American politics experienced dramatic partisan polarization over the last century (Iyengar et al., 2019). Less-polarized party bases in 1918 may have left voters to prioritize administrative competence over ideological purity in their assessment of NPIs (Grossman et al., 2020; Weisel, 2021). In 2020, party elites regularly framed NPIs and compliance in ideological terms,

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<sup>9</sup>We limited our sample to newspapers that published at least one article on the flu in 1918-1920.

<sup>10</sup>Our media fractionalization index is a decreasing transformation of the Herfindahl concentration index:  $m_i = 1 - \sum_p s_{pi}^2$ , where  $s_{pi}$  represents the market share of newspaper  $p$  in constituency  $i$ . In the absence of circulation data, we calculated  $s_{pi}$  using the geocoded locations of newspapers' main bureaus, and the degree of overlap between tessellated polygons around these locations and the geographic boundaries of constituency  $i$ . We also constructed a dynamic version of this index,  $m_{it}$ , which varies according to which local newspapers were actively covering the pandemic each month.

<sup>11</sup>As an alternative measure, we also considered the total number of newspapers published in each constituency as a predictor of NPI frequency. Our results were unchanged.

with Republicans less supportive of these measures than Democrats (Utych, 2021). This explanation neither supports nor contradicts our hypothesis; it simply posits that incentives to implement vs. avoid NPIs will vary by political party.

The data do not support this view: competitiveness appears to have affected Democratic and Republican incumbents in very similar ways. Table A4.11 extends our models of monthly NPI intensity with an interaction between electoral competitiveness and incumbent party control in the most recent set of pre-pandemic elections (Democrats on the left, Republicans on the right).<sup>12</sup> While there are occasional differences — mostly in magnitude — between individual competitiveness effect estimates, the general patterns align with what we saw before: in 1918, competitiveness tended to increase NPIs; in 2020, it reduced them. The only instances where weighted mean estimates for Republicans and Democrats were not of the same sign were for information releases in 1918 and enforcement in 2020, although these estimates did not reach statistical significance. Furthermore, this inter-party heterogeneity seems no greater in 2020 than it was in 1918.

## A4.6 Divided State Government

A sixth explanation, which also presupposes partisan differences in political incentives, is that NPIs have become harder to implement due to divided government. Divided state government — where different parties control the legislature, governor’s mansion, and/or senior executive offices like lieutenant governor and attorney general — is more common in recent decades than in 1918 (Fiorina, 1994). This division creates opportunities to stonewall the enactment of NPIs (Calcagno and Escaleras, 2007).<sup>13</sup>

We tested this proposition by adding indicators for divided state government to our models of NPI intensity during Covid-19 — including both executive-legislative splits, and splits within the executive branch. Of these two, divided executive branches appear to have a stronger dampening effect on NPIs. Estimates for the competitiveness measures remain largely unchanged in magnitude, sign and precision (Table A4.12).

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<sup>12</sup>These linear mixed models take the form

$$\begin{aligned} \log(\text{outcome}_{it} + 1) &= c_i^{(k)}\gamma_1 + \text{Republican}_i\gamma_2 + c_i^{(k)} \times \text{Republican}_i\gamma_3 + X_{it}\beta + z_j + u_t + \epsilon_{it}, \\ z_j &\sim \mathcal{N}(0, \Sigma(\theta)_z), \quad u_t \sim \mathcal{N}(0, \Sigma(\theta)_u) \end{aligned} \quad (2)$$

The quantities in Table A4.11 correspond to  $(\exp(\hat{\gamma}_1) - 1) \times 100$  (left) and  $(\exp(\hat{\gamma}_1 + \hat{\gamma}_3) - 1) \times 100$  (right). We calculated standard errors for  $\hat{\gamma}_1 + \hat{\gamma}_3$  as  $\sqrt{\text{Var}(\hat{\gamma}_1) + \text{Var}(\hat{\gamma}_3) + 2\text{Cov}(\hat{\gamma}_1, \hat{\gamma}_3)}$ .

<sup>13</sup>In Michigan and Wisconsin, for example, Republican-controlled state legislatures blocked NPI policy directives of the Democratic governors during Covid-19.

## A4.7 Effectiveness of NPIs

One of the simplest explanations for the results in Table 3 is that NPIs were ineffective, perhaps even counterproductive, in slowing the spread of Covid-19, and vulnerable incumbents avoided them on efficacy grounds. This view is not inconsistent with our hypothesis, since it posits that competitive elections create incentives to avoid policies because they are ineffective, not because they are unpopular.

The results, in Table 4, do not support the view that NPIs were counterproductive in either pandemic. While not all types of NPIs were equally effective in slowing the rate of growth in cases and deaths, the only estimates that reached statistical significance were all negative. In 1918, NPIs appeared to have little impact on monthly deaths, but they did lead to a significant decrease in severe illnesses, as measured by pneumonia cases per 1000 people. Pneumonia cases declined most following increases in new NPIs and public information statements (4b). In 2020, a similar decline followed months with more new NPIs, more information releases, and more enforcement (4c-d). There is no evidence that NPIs were less effective in 2020 than in 1918 — the opposite is more likely.

## A4.8 Differences in Disease Pathology

An eighth alternative explanation for differences in policy responses to 1918 and 2020 is that severe Covid-19 cases were concentrated among older patients and those with pre-existing health conditions, while severe 1918 flu cases occurred among many otherwise healthy young adults. Because it affected a broader cross-section of society, the 1918 pandemic may have generated greater support for aggressive action.

There are several reasons to doubt this explanation. First, past research has shown that older voters are more politically mobilized and civically engaged than younger voters, and have an outsized voice in American politics (Verba, Schlozman and Brady, 1995) — all of which would imply greater pressure for NPIs in 2020. Second, our empirical models for Covid-19 already control for the local age structure of the population, and our results are robust to the inclusion of this variable.

## A4.9 Vaccine Anticipatory Behavior

A ninth explanation is that the anticipation of a Covid-19 vaccine might have discouraged political leaders and the public from implementing and adhering to NPIs, as the pandemic’s “end” appeared in sight. One example of this behavior is the rapid relaxation of mask mandates during the mass vaccination campaign of spring and summer 2021. By contrast, there was no vaccine and no such anticipatory action during the 1918 flu (Flaig, Houy and Michel, 2018; Andersson et al., 2020). This view is not entirely inconsistent with our hypothesis, if we assume that vulnerable incumbents view pharmaceutical interventions (PIs) as more effective than NPIs.

We find no evidence that a vaccine-related phase shift is driving our results. The analysis in Table 3 already accounts for this possibility, by truncating the Covid-19 data sample at October 2020, the last month before an early data release from the Pfizer-BioNTech vaccine’s Phase III trials. Our results also hold if we extend the period of observation to May 2021 (the last month for which we have data, Table A2.2), and if we interact competitiveness with a “post-November 2020” indicator (Table A4.13).

## A4.10 Local Public Health Capacity

Vulnerable incumbents may have implemented fewer NPIs in 2020 because they were in a strong position to manage the pandemic without them. If — in line with our hypothesis — competitive political environments create incentives to maximize constituents’ welfare, then we may expect such localities to have made greater pre-pandemic investments in public health infrastructure, such that a sudden surge in cases would not put excessive strain on local hospitals.

We collected data on several measures of local public health capacity, including health spending per capita (Smittenaar et al., 2021), and the number of hospitals, hospital beds and EMS facilities per 1,000 residents (Homeland Infrastructure Foundation-Level Data (HIFLD), 2020). We regressed each of these variables on our measures of competitiveness, along with the same covariates and random effects as those in Table 3h.<sup>14</sup>

The results show no evidence that more competitive constituencies had greater local public health capacity prior to Covid-19. The correlation is almost uniformly negative: more competitive constituencies had spent less per capita on public health, and had fewer hospitals, beds, and EMS than less competitive constituencies (Table A4.14).

## A4.11 Autonomous Social Distancing

People living in competitive localities may not have needed many government mandates to change their behavior in ways that mitigate disease risk. This explanation dovetails with our findings on lower socioeconomic risk in competitive areas (Section 4) and recent research showing that individuals with higher socioeconomic status tend to be less risk-seeking and more responsive to public health messaging (Durkin et al., 2018).

There is some support for this. Using data from Apple’s Mobility Trend Reports, we calculated the average decline in local driving, walking and transit traffic between January 13, 2020 and March 13, 2020, the latter date corresponding to President Donald J. Trump’s declaration of a National Emergency, five days before California issued the country’s first statewide stay-at-home order.<sup>15</sup> We then regressed the local decline

<sup>14</sup>Because these public health data are time-invariant (count data from January 1, 2020 to December 31, 2020), we fit the cross-sectional version of the model, with state random effects.

<sup>15</sup>January 13 is the reference date used by Apple in these data ([covid19.apple.com/mobility](https://covid19.apple.com/mobility)). We used a

in mobility on electoral competitiveness, using our baseline mixed effects specification. The data suggest that more competitive localities experienced a steeper decline in foot, personal vehicle and public transportation traffic than less competitive localities (Table A4.17). According to our weighted mean estimate, a one-percent increase in competitiveness corresponds to a .1 percentage point decrease in average local mobility. Further analysis shows that lower (higher) pre-lockdown mobility also correlates with lower (higher) excess deaths in 2020 (Table A4.18).

The Apple mobility data are not based on a representative or probability-weighted sample of Americans — it is based on a sample of users of the “Maps” application on Apple mobile devices, a wealthier subgroup. Nor do their patterns tell us why Apple users in competitive localities would act like this. Yet the strength and direction of the relationships between these variables do lend some support to the view that competitive localities were well-positioned to weather the pandemic without NPIs.

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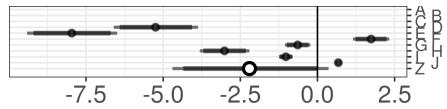
one-week rolling average to avoid day-of-the-week effects and reduce the influence of outliers, geocoded the locations, and employed area-weighted interpolation to map the original county- and city-level mobility numbers to the constituencies in our sample.

Table A4.7: Impact of political competition on NPIs, by timing of reelection

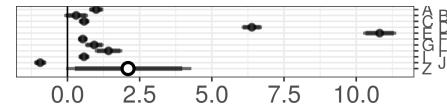
**1918 Influenza pandemic**

(a) New NPIs per month<sup>†</sup>

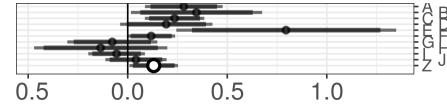
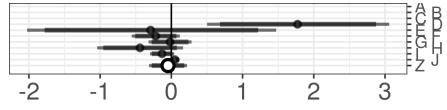
No reelection in 1918



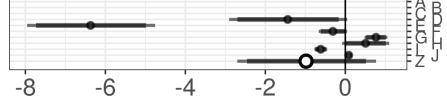
Reelection in 1918



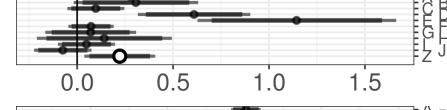
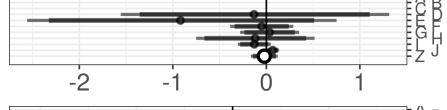
(b) Public information releases per month<sup>†</sup>



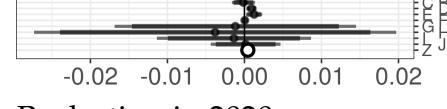
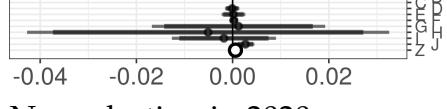
(c) NPI strengthening events per month<sup>†</sup>



(d) NPI enforcement events per month<sup>†</sup>



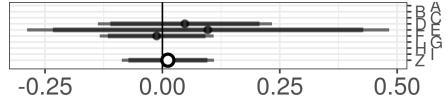
(e) Pharmaceutical interventions per month<sup>†</sup>



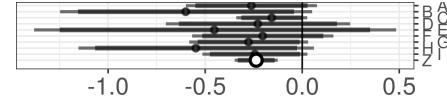
**Covid-19 pandemic**

(f) New NPIs per month<sup>‡</sup>

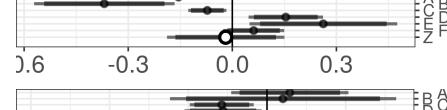
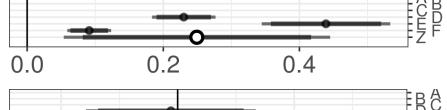
No reelection in 2020



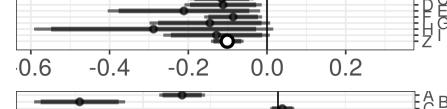
Reelection in 2020



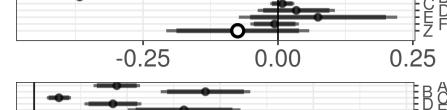
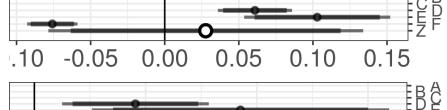
(g) Public information releases per month<sup>‡</sup>



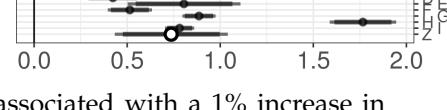
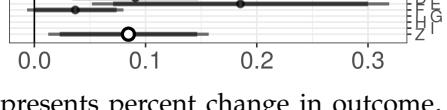
(h) NPI strengthening events per month<sup>‡</sup>



(i) NPI enforcement events per month<sup>‡</sup>



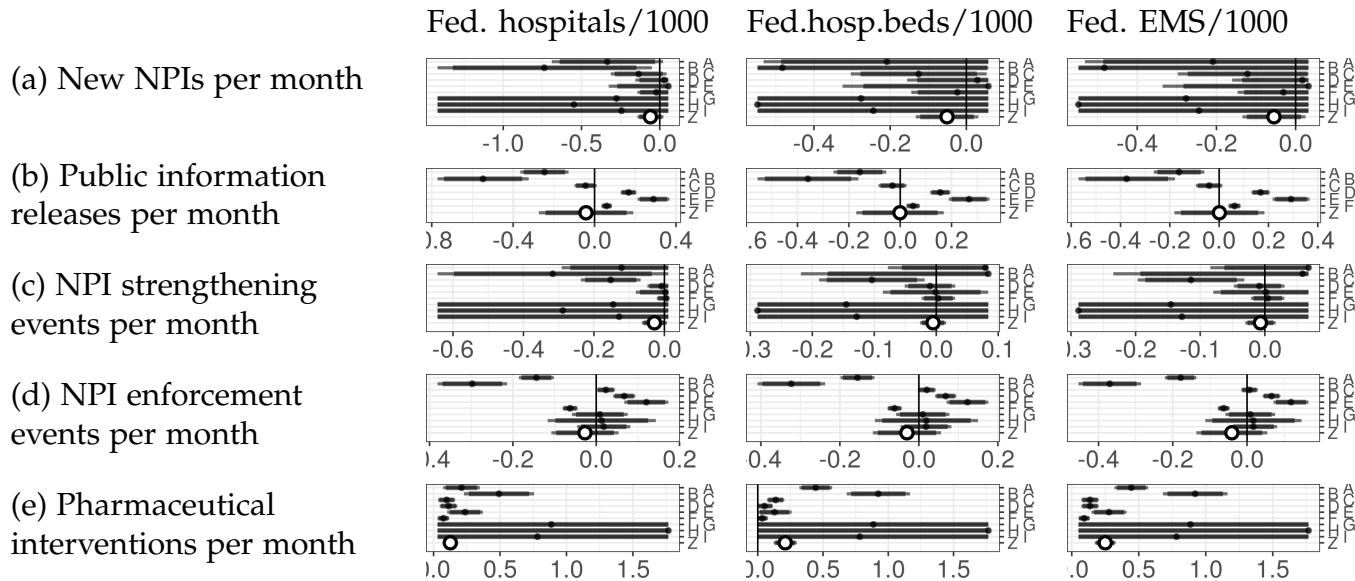
(j) Pharmaceutical interventions per month<sup>‡</sup>



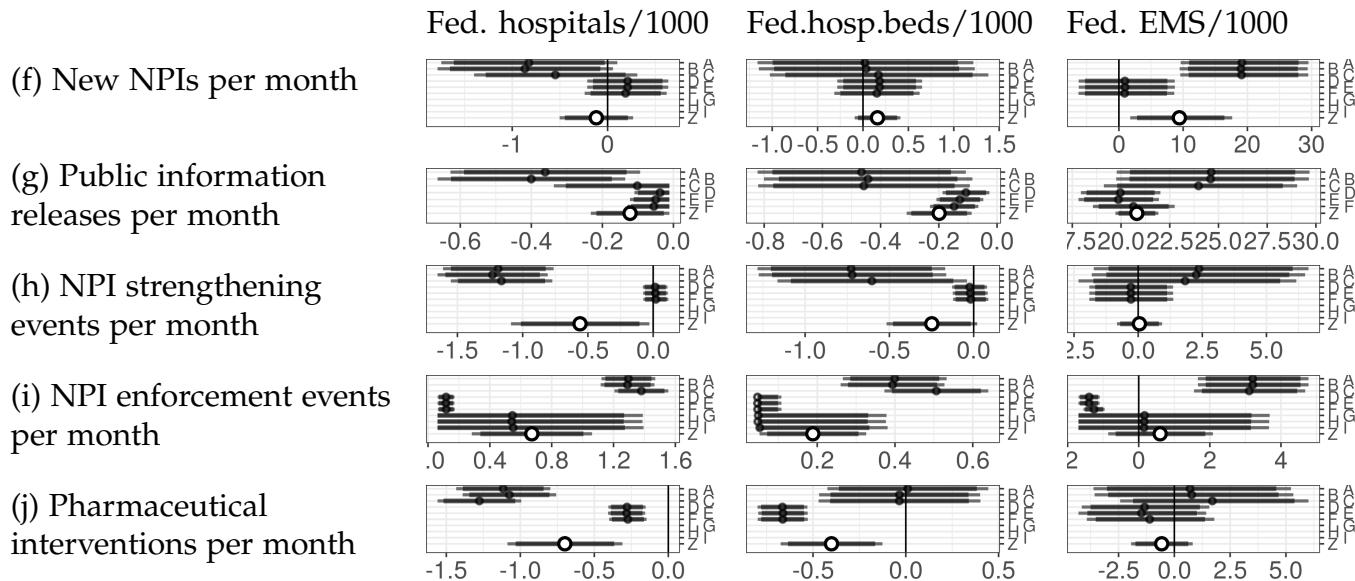
**Interpretation:** Horizontal axis represents percent change in outcome, associated with a 1% increase in each measure of political competitiveness in constituencies without (left) and with (right) a reelection campaign in 1918 or 2020. **Models:** Linear mixed models, accounting for urbanization (<sup>†</sup>), population density (<sup>‡</sup>), percent female, longitude, latitude, percent of population over 60 (<sup>‡</sup>), month, region (<sup>†</sup>) and/or state (<sup>‡</sup>) random effects. Population weights. See notes under Tables 2-3 for measurement labels.

Table A4.8: Impact of political competition on NPIs during Covid-19, controlling for federal public health capacity

**Impact of electoral competition on local pandemic response**



**Impact of federal capacity on local pandemic response**

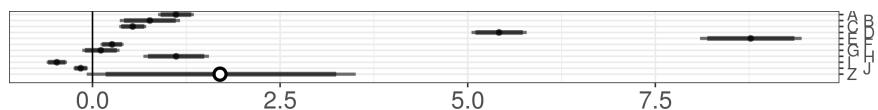


**Interpretation:** Horizontal axis represents percent change in outcome, associated with either: (a-e) a 1% increase in each electoral competitiveness measure A-I, while controlling for the federal public health capacity measure in the columns, or (f-j) a 1% increase in federal capacity, while controlling for electoral competitiveness. See note under Table 3 for measurement labels and model details.

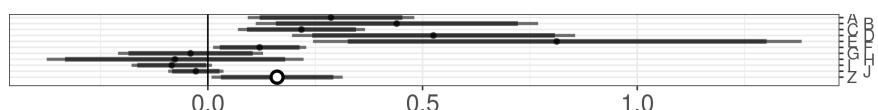
Table A4.9: Impact of political competition on NPIs during 1918 flu pandemic, controlling for women's enfranchisement in 1916

### Impact of electoral competition on local pandemic response

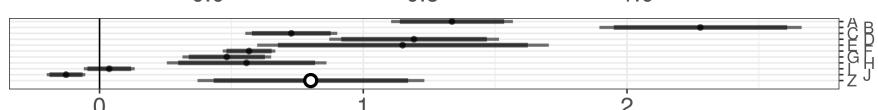
(a) New NPIs per month



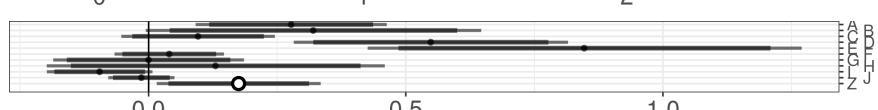
(b) Information releases per month



(c) NPI strengthening events per month



(d) NPI enforcement events per month



(e) Pharmaceutical interventions per month

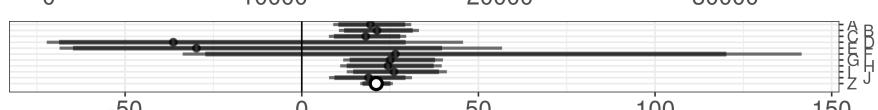


### Impact of women's enfranchisement on local pandemic response

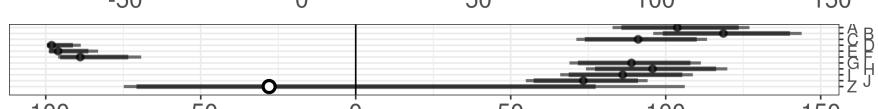
(f) New NPIs per month



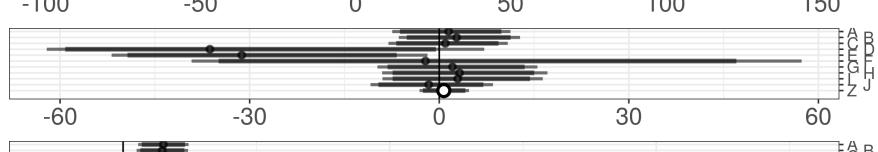
(g) Information releases per month



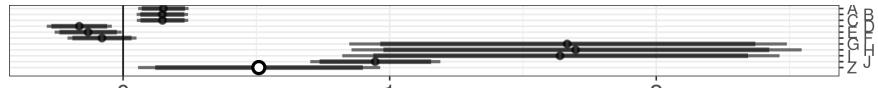
(h) NPI strengthening events per month



(i) NPI enforcement events per month

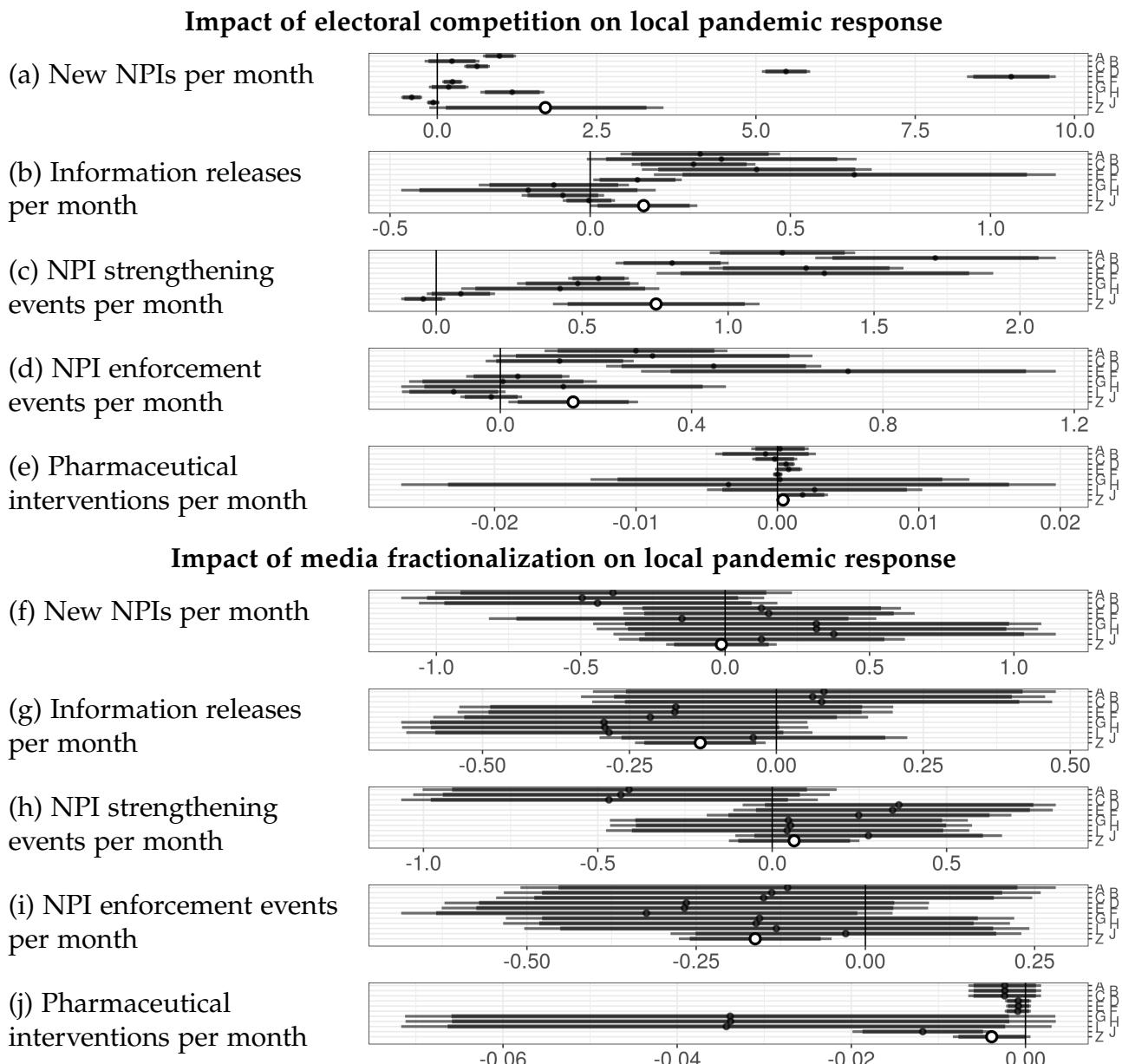


(j) Pharmaceutical interventions per month



**Interpretation:** Horizontal axis represents percent change in outcome, associated with (a-e) a 1% increase in each measure of political competitiveness, controlling for state-level women's enfranchisement in 1916, or (f-j) existence of women's enfranchisement in state, controlling for each measure of electoral competitiveness. See note under Table 2 for measurement labels and model details.

Table A4.10: Impact of political competition on NPIs during 1918 flu pandemic in U.S., controlling for media environment



**Interpretation:** Horizontal axis represents percent change in outcome, associated with (a-e) a 1% increase in each measure of political competitiveness, controlling for media fractionalization index, and (f-j) a 1% increase in media fractionalization, controlling for each measure of competitiveness. See note under Table 2 for measurement labels and model details.

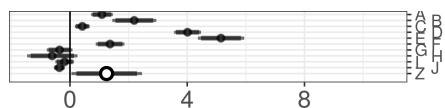
reelection

Table A4.11: Impact of political competition on NPIs, by incumbent party

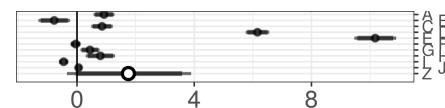
**1918 Influenza pandemic**

(a) New NPIs per month<sup>†</sup>

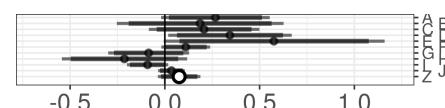
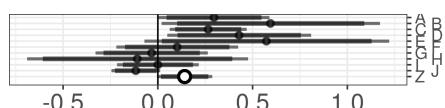
Democratic incumbent



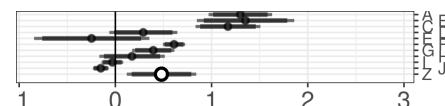
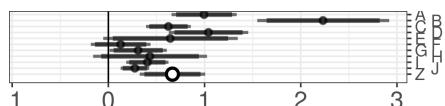
Republican incumbent



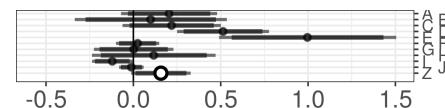
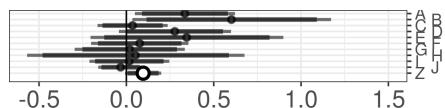
(b) Public information releases per month<sup>†</sup>



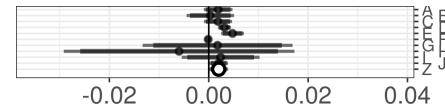
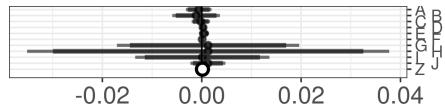
(c) NPI strengthening events per month<sup>†</sup>



(d) NPI enforcement events per month<sup>†</sup>



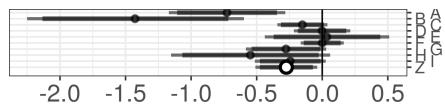
(e) Pharmaceutical interventions per month<sup>†</sup>



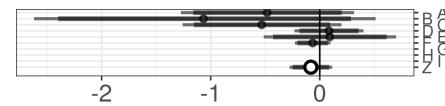
**Covid-19 pandemic**

(f) New NPIs per month<sup>‡</sup>

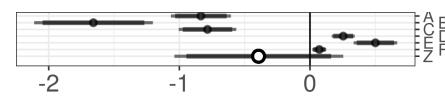
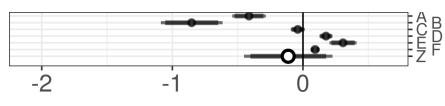
Democratic incumbent



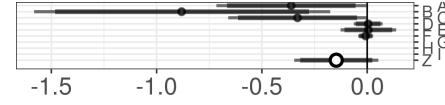
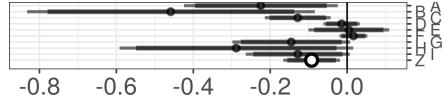
Republican incumbent



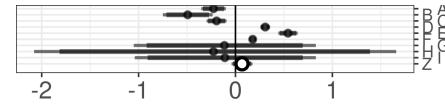
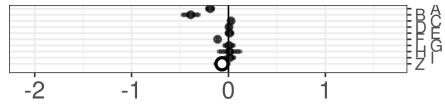
(g) Public information releases per month<sup>‡</sup>



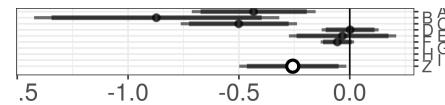
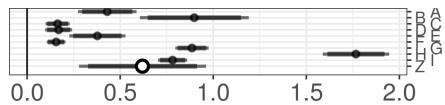
(h) NPI strengthening events per month<sup>‡</sup>



(i) NPI enforcement events per month<sup>‡</sup>



(j) Pharmaceutical interventions per month<sup>‡</sup>

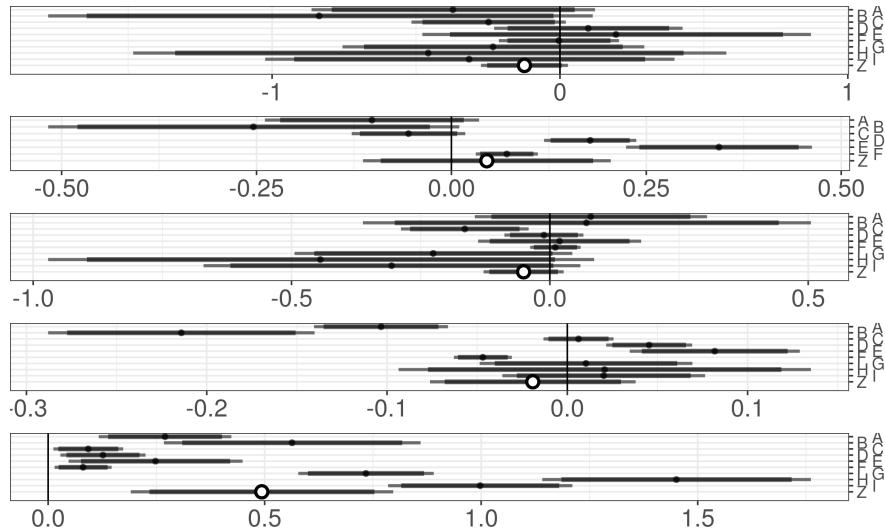


**Interpretation:** Horizontal axis represents percent change in outcome, associated with a 1% increase in each measure of political competitiveness in constituencies with Democratic (left) and Republican (right) incumbents. **Models:** Linear mixed models, accounting for urbanization (<sup>†</sup>), population density (<sup>‡</sup>), percent female, longitude, latitude, percent of population over 60 (<sup>‡</sup>), month, region (<sup>†</sup>) and/or state (<sup>‡</sup>) random effects. Population weights. See notes under Tables 2-3 for measurement labels.

Table A4.12: Political competition and the Covid-19 pandemic in U.S., controlling for divided state government

**Impact of electoral competition on local pandemic response**

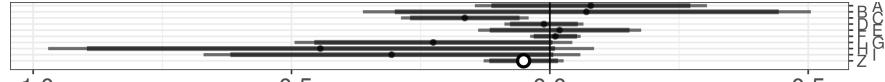
(a) New NPIs per month



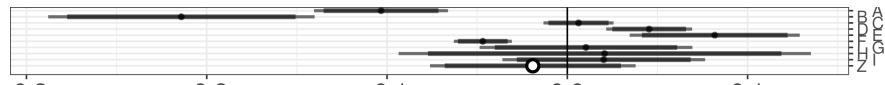
(b) Information releases per month



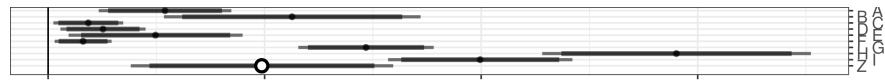
(c) NPI strengthening events per month



(d) NPI enforcement events per month



(e) Pharmaceutical interventions per month



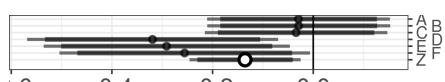
**Impact of divided state government on local pandemic response**

Executive-legislative split

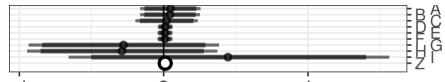
(f) New NPIs per month



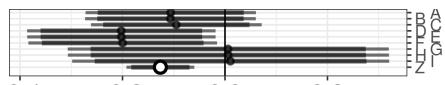
(g) Information releases per month



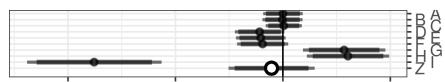
(h) NPI strengthening events per month



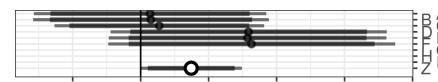
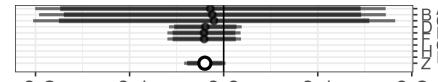
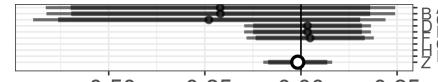
(i) NPI enforcement events per month



(j) Pharmaceutical interventions per month

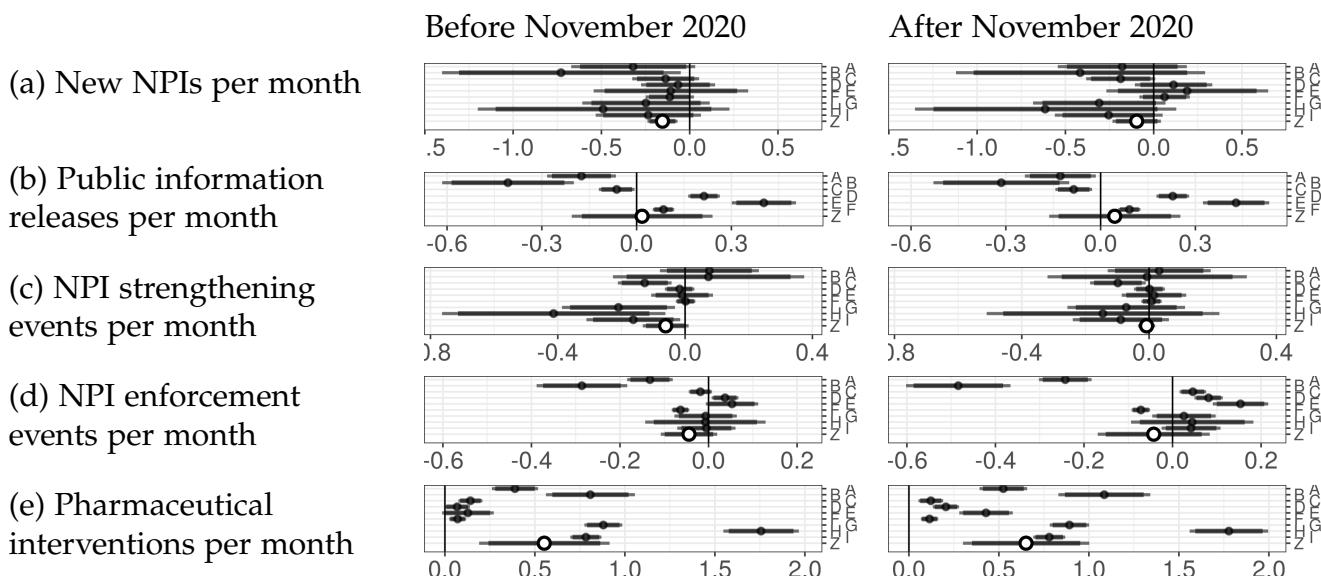


Within-executive split



**Interpretation:** Horizontal axis represents percent change in outcome, associated with (a-e) a 1% increase in each measure of political competitiveness, controlling for divided state government, and (f-j) divided state government, controlling for each measure of electoral competitiveness. See note under Table 3 for measurement labels and model details.

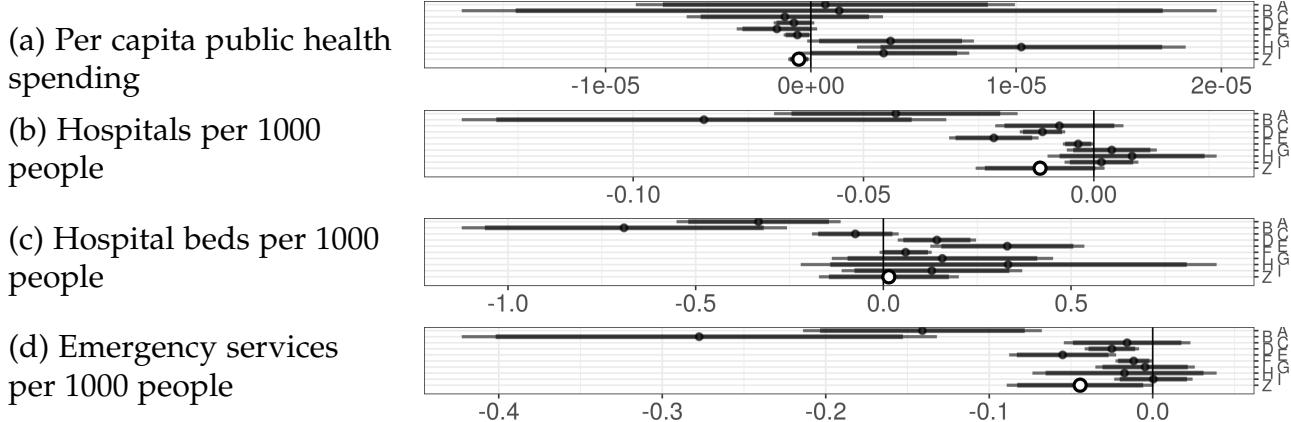
Table A4.13: Impact of political competition on NPIs, pre-/post-vaccine (Covid-19)



**Interpretation:** Horizontal axis represents percent change in outcome, associated with a 1% increase in each measure of political competitiveness before November 2020 (left) and in/after November 2020 (right). See note under Table 3 for measurement labels and model details.

Table A4.14: Political competition and pre-Covid local public health capacity

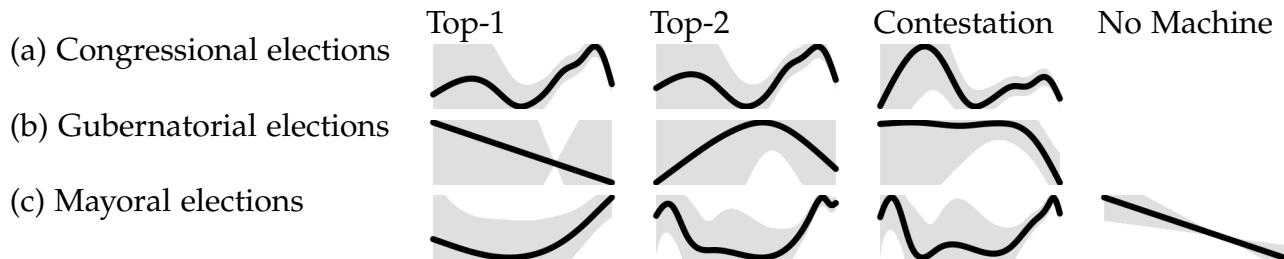
#### Impact of electoral competition on pre-pandemic public health spending and infrastructure



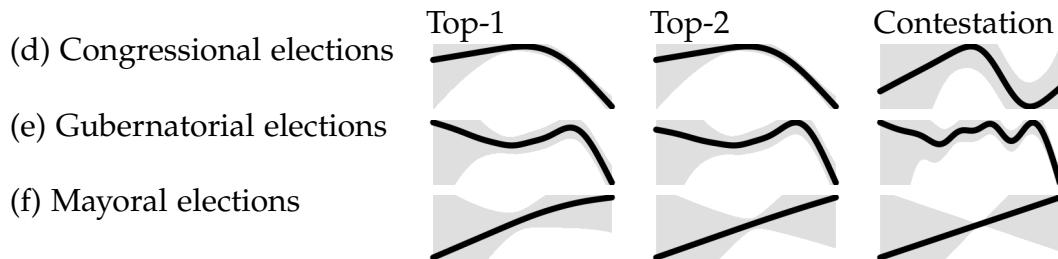
**Interpretation:** Horizontal axis represents percent change in outcome, associated with a 1% increase in each measure of political competitiveness. See note under Table 3 for measurement labels and model details.

Table A4.15: Population density and political competition

**Urbanization and electoral competitiveness pre-1918**



**Urbanization and electoral competitiveness pre-2020**

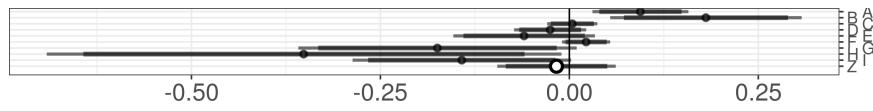


**Interpretation:** Horizontal axis represents (a-c) logged urbanization, or (d-f) logged population density. Vertical axis represents predicted value of each electoral competitiveness measures. Lines are point estimates; grey areas are standard errors. Units of analysis are electoral constituencies. **Models:** Generalized additive models, with thin-plate spline of urbanization (or population density), and regional (a-c) or state-level fixed effects (d-f). Observations weighted by population.

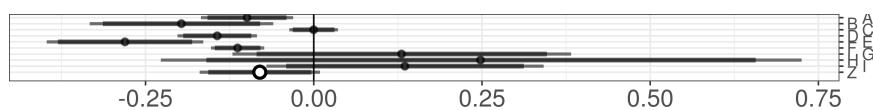
**Table A4.16: Political competition and pre-Covid socioeconomic risk factors**

**Impact of electoral competition on pre-pandemic socioeconomic outcomes**

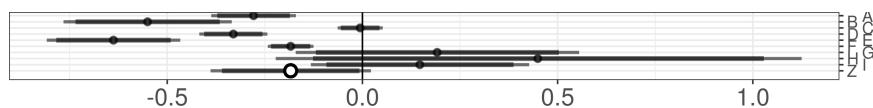
(a) Per capita income



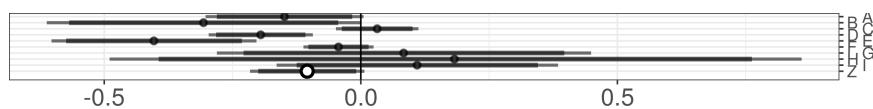
(b) % population uninsured



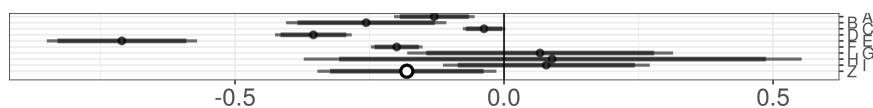
(c) % households with no vehicle



(d) % mobile homes



(e) % households with crowded living conditions



**Interpretation:** Horizontal axis represents percent change in outcome, associated with a 1% increase in each measure of political competitiveness. See note under Table 3 for measurement and model details.

**Table A4.17: Political competition and pre-NPI social distancing**

**Impact of electoral competition on local mobility (March 13, 2020)**

(a) % change in mobility from Jan. 13, 2020



**Interpretation:** Horizontal axis represents change in outcome, associated with a 1% increase in each measure of political competitiveness. See note under Table 3 for measurement labels and model details.

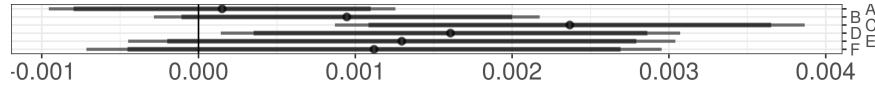
Table A4.18: Early social distancing and mortality during Covid-19

**Impact of local mobility changes relative to January 13, 2020**

(a) Reported Covid-19 deaths (per 1000 people)



(b) Excess mortality in 2020 (per 1000 people)



**Interpretation:** Horizontal axis represents change in outcome, associated with a 1% increase in physical mobility relative to January 13, 2020. **Measures:** A: Relative mobility on March 13, B: Relative mobility on March 13 (7 day rolling average), C: Relative mobility on March 17, D: Relative mobility on March 17 (7 day rolling average), E: Relative mobility on April 14, F: Relative mobility on April 14 (7 day rolling average). **Models:** Linear mixed model, estimated separately for each measure A-F. Models account for population density, percent female, longitude, latitude, percent of population over 60, and state random effects. Observations weighted by 2010 population. Units of analysis are 2010 counties.

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