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Abstract

In 2020, many observers were surprised that the Covid-19 outbreak did not appear to have swung the election. Early returns showed little indication that harder-hit areas swung away from the incumbent GOP. In 1918, however, the United States also held an election in the middle of a devastating pandemic. Using county-level epidemiological, electoral, and documentary evidence from 1918-20 we find that flu mortality had a statistically-significant negative effect on the Congressional or gubernatorial vote. The swing, while precise however, was relatively small and not enough to determine the results. We find no effect from flu mortality on turnout rates or on the 1920 presidential election. Our results hold using overall mortality in 1917 and distance to military camps as instruments for 1918 flu deaths. They also withstand tests of coefficient stability and alternative specifications. Considering that the 1918 flu was much more severe than the 2020 Covid pandemic, the historical evidence implies that surprised observers of the 2020 election should not have been so surprised.

JEL Classification: N0

Keywords: Elections, Pandemics

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1 Introduction

In November 2020, many observers were surprised that the Covid-19 pandemic afflicting the nation appeared to have remarkably little effect on the electoral returns.[1] The most serious pandemic to hit the United States in a century, amid a widely-panned federal response, barely seemed to swing voters.

But should have observers been so surprised? A century earlier, between September and December 1918, the second wave of the Spanish flu pandemic carved a trench of death across the United States. It was possibly the second most destructive event in the history of the country. Excess mortality exceeded 600,000. That number of deaths surpassed the total number of deaths in either World War and roughly equaled the Civil War. The second wave started on September 8th, 1918, at Fort Devens, Massachusetts. From there it traversed the nation at breakneck speed. Monthly mortality rates rapidly tripled or quadrupled. Worse yet, in terms of the social impact, the Spanish flu disproportionately killed young adults between the ages of 20 and 40. Children lost their parents by the thousands; grandparents outlived their adult children. Contrary to the present-day popular belief, the press widely reported on the second wave.[2] Voters, however, did not need the newspapers to see the effects of the disease: morgues filled up, coffin production could not meet demand, and public services collapsed under the strain.

We find that voters punished incumbent politicians for letting local epidemics run rampant — but they did not punish them very much. Controlling for socioeconomic differences between counties, we find that the electorate punished the incumbent party in congressional and gubernatorial races, but the size of the swing was not enough to determine the election results. In Section 2, we examine the history of the flu and lay out evidence that all levels of government possessed tools to respond and that voters had reasonable expectations that their governments would respond. In section 3, we discuss retrospective voting in the context of American disease outbreaks from the 1790s through 1918. In Section 4, we exploit the geographical variability of the 1918 flu to tease out the effects of the incidence of disease casualties on voting. The swing, however, was small, ranging from 0.6 to 1.0 percentage points when local excess mortality doubled. By 1920, that small effect was gone for the presidential and congressional races. Democratic candidate James Cox lost the election but not because the Wilson Administration failed to contain the pandemic.

2 Background

2.1 The pandemic

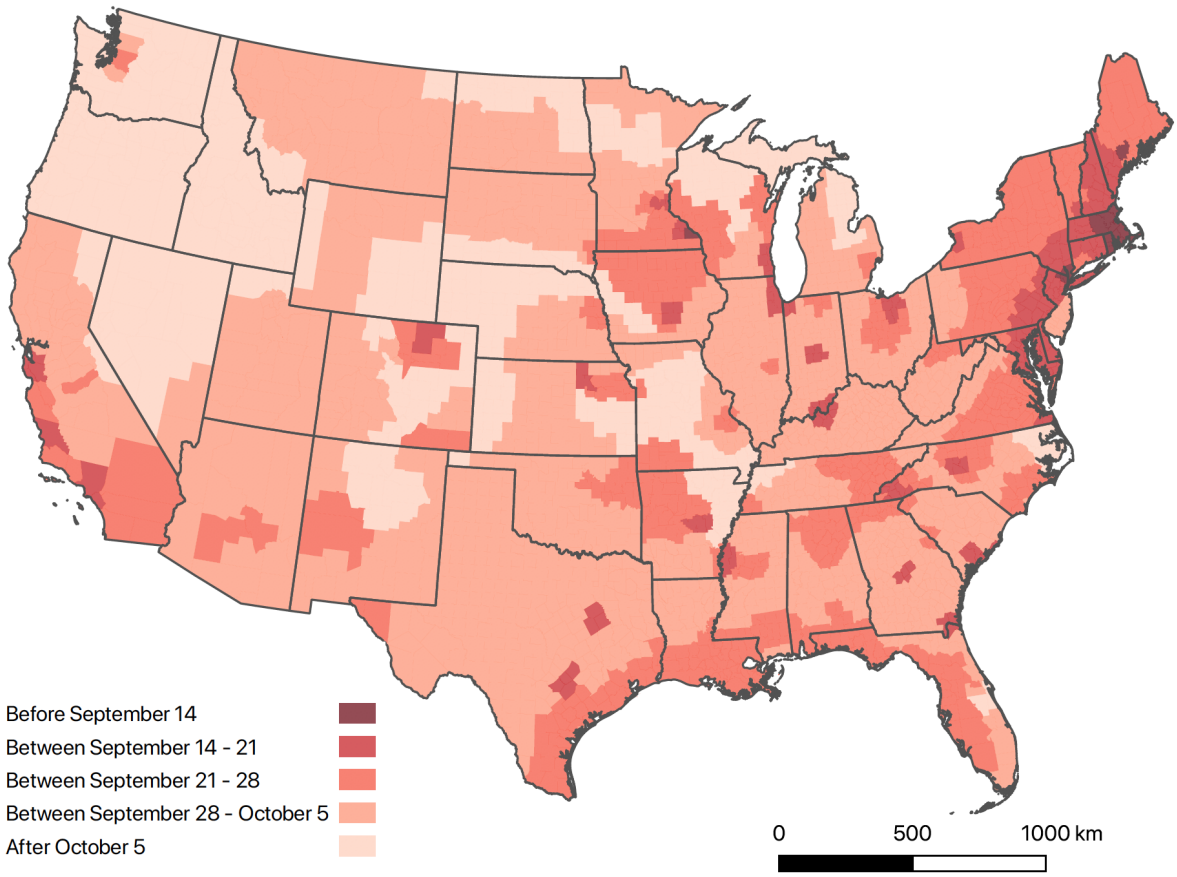
The 1918 flu pandemic appears to have begun at U.S. Army bases in Kansas in March 1918. The first wave of the pandemic killed relatively few people and appeared little worse than a typical seasonal flu. American soldiers spread the virus to Europe, where its virulence picked up. By the third week of May, the disease reached Spain. The Spanish press was not under wartime censorship and widely reported the epidemic, giving the name "Spanish flu" to a disease that most likely originated in the United States.

[1] These conclusions were based on simple uncontrolled correlations on limited samples. Simple scatterplots of the data showed a weak relationship between Covid fatalities and the Democratic vote swing even though the headlines on many pieces indicated the reverse. See, for example, McMinn and Stein (2020) and Tartar et al. (2020).

[2] In Section 2.1 we present statistical evidence about the prevalence of Spanish flu coverage in the media.

In September 1918, the virus re-emerged among demobilizing soldiers at Fort Devins, Massachusetts. Within a week of the initial reports the U.S. Public Health Service (USPHS) made influenza a reportable disease and began detailed tracking. The USPHS utilized weekly mortality reports from the Census Bureau, supplemented by reports from roughly 376 localities. The epidemic soon spread by road, rail, and ship up and down the East Coast (see Figure 1). It then spread out radially into the countryside, and by the middle of October had become a national pandemic (Treasury Department, 1919, 176). The case incidence was higher in children, but mortality spiked among adults aged 20 to 40, resulting in an atypical "W" distribution of mortality by age (Treasury Department, 1919, 179). The second wave generally hit rural counties later, but there was no pattern to its severity (Brainerd and Siegler, 2003).

Figure 1: Flu arrival



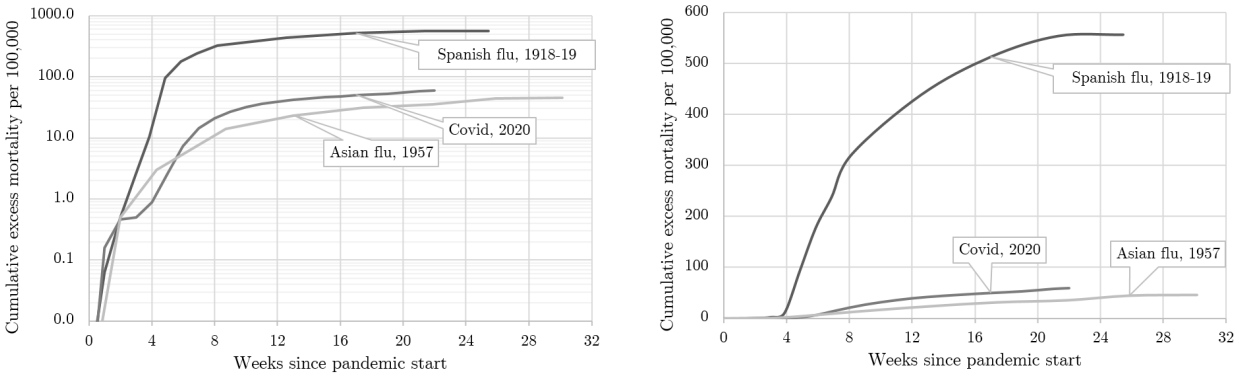
Source: Based on U.S. Navy (1920)

Despite rapid medical advances in the preceeding decades, there were few effective treatments for influenza. Antibiotics had not been developed and with no recourse to treat bacterial pneumonia, a common development after flu infection, hospitals quickly filled up. An inability to deal with bodies led to secondary infections. In some cities (but not all) garbage collection also broke down, further increasing the secondary impacts from the flu (Crosby, 2003, 326). Private and gov-

ernment laboratories attempted to develop vaccines. New York City developed and distributed (at taxpayer expense) a vaccine based on the theory that Pfeiffer's bacillus caused the disease; U.S. Steel distributed the vaccine to its 275,000 employees. The Mayo Clinic also developed a vaccine, which the City of Chicago put into production using its municipal laboratories. Chicago ultimately produced 500,000 doses to be distributed by the state government across Illinois (Robertson, 1919, 117). The problem was that the etiology of influenza was not well understood, and the vaccines did not address the cause of the disease (Eyler, 2010).

Ultimately, the Spanish flu killed approximately 617,000 people within the United States, measured as total excess mortality across the second and third wave, or 0.6 percent of the U.S. population. This estimate assumes that figures for cumulative excess mortality across what was called the "registration area" — a group of 32 states (plus Hawaii and the District of Columbia) and scattered cities across Alabama, Delaware, Florida, Georgia, Mississippi, Nebraska, North Dakota, Texas, and West Virginia — apply to the whole United States. The registration area covered 77.8 percent of the total U.S. population. Figure 2 shows excess mortality from the Spanish flu in comparative perspective, using comparable estimates from the Asian flu pandemic of 1957 and the current Covid-19 pandemic. Panel A shows death rates per 100,000 on a log scale; Panel B presents the same data linearly.

Figure 2: Excess mortality compared



Source: United States Public Health Service (1918)

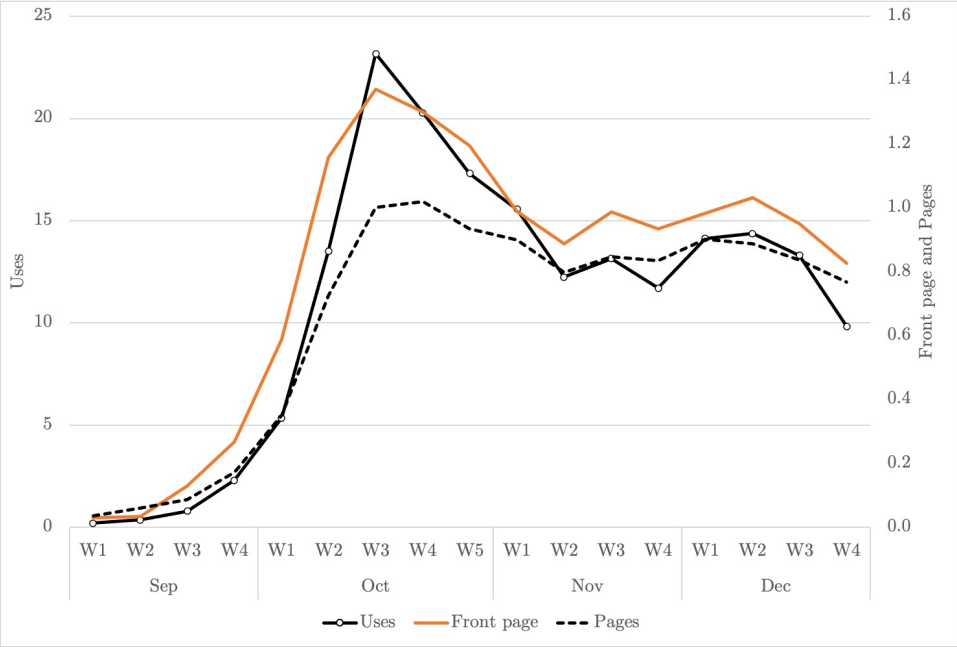
As it spread, the flu managed to push the World War off the front pages. We analyzed 495 newspapers across the United States for mentions of "flu" (and synonyms such as "grippe" or "influenza.") We measured mentions per issue in three ways: front page titles, total number of pages containing the word, and total uses of the word. We then scaled each measure against the equivalent for the word "war." Table 1 presents the average ratios across our sample period. Ratios changed over time, however (see Figure 3). The flu was barely mentioned at the outset of the second wave. Front page headlines about the disease surpassed war stories by the middle of October before declining slightly. Total pages and total mentions tracked the front pages and total mentions peaked at more than 23 times mentions of the war. Flu coverage continued intensely through the November 5th election.[3]

[3]The armistice ending World War I was signed on six days later, on November 11th.

Table 1: Newspapers statistics

	Mean	SD	N
Flu/War uses	0.59	0.81	495
Flu/War front page	0.69	0.47	481
Flu/War uses	9.46	7.32	481

Figure 3: Average flu / war uses



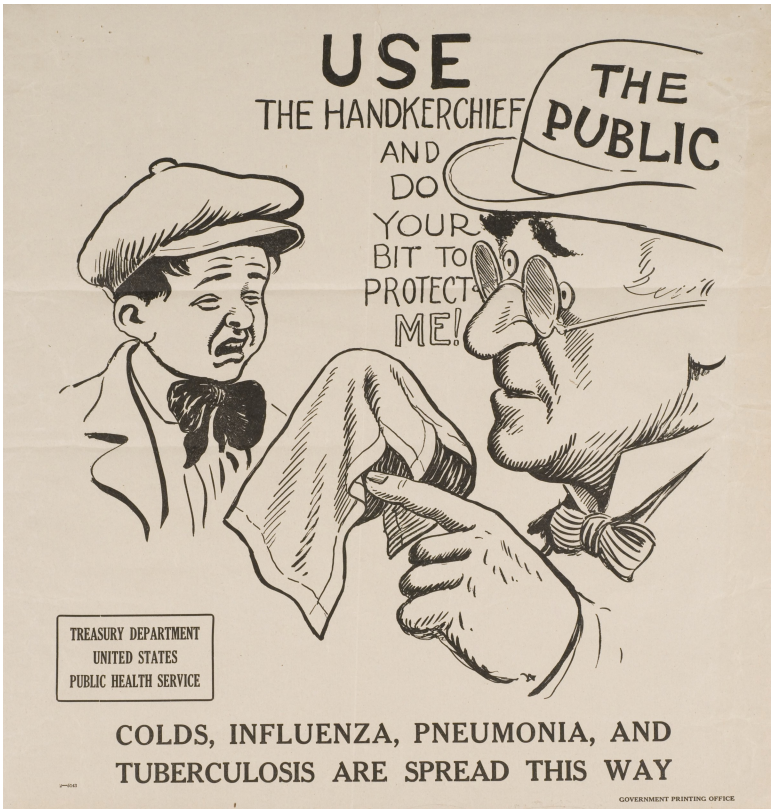
Source: Based on local newspapers

2.2 Official responses

The federal government responded fitfully and erratically to the epidemic. President Wilson never made any public statements. Surgeon General Rupert Blue initally tried to avoid involvement, advising only "bed rest" on September 13. Once the scope of fatalies in Massachusetts became undeniable, he strengthened his recommendations to include refraining from sneezing. Finally, after Philadelphia and Chicago succumbed to the pandemic, Blue put the federal machinery into operation. At Blue's request, on October 1st, Congress appropriated $1 million (the 2019 equivalent of $253 million as a share of GDP) to mobilize doctors into the U.S. Public Health Service (USPHS) and print advisories; Congress also put the Army and Navy's health departments at the USPHS's disposal to combat the disease (Blue, 1919, 3). Four days later, Blue advised local public health boards to ban public gatherings and close restaurants, bars, churches, theaters, and other facilities. Blue lacked the authority to make such recommendations mandatory, but he did print up six million pamphlets warning the public about the flu and used the Post Office and the Federal Railroad Administration to plaster the country with posters telling people to stay out of crowds, cover up coughs and sneezes, refrain from spitting on the sidewalks, and avoid common drinking cups (see

Figure 4) (Blue, 1919, 2). In the words of Alfred Crosby, "If influenza could have been smothered by paper, many lives would have been saved in 1918" (Crosby, 2003, 49).

Figure 4: USPHS pamphlet



Source: United States Public Health Service (1918)

State and local governments provided the locus of action against the pandemic. As already mentioned, sanitation and public health services collapsed under the strain of the epidemic in some cities, of which Philadelphia was the prime example. Other places, however, weathered the pandemic and used their civic machinery to combat the infection in very public ways. In Chicago, for example, the city government employed the fire department (and specially-purchased power hoses) to clean streets daily and upped refuse collection from bi-weekly to daily (Robertson, 1919, 113).

In most states, Progressive Era reforms had empowered the appointed Board of Health to enforce quarantines, ban public gatherings, close school and business, and take other actions necessary to protect public health. Most states also granted local government similar powers. As the flu spread, state and local health boards responded with various non-pharmaceutical interventions. After the USPHS issued its circular, these orders went national quickly, but local enforcement and cooperation varied. New York City and New York State, for example, refused to ban public gatherings. In California, the San Diego city council overturned a local Board of Health decision to close all public businesses and had to be overriden by the governor (Peterson, 1989, 96).[4] In Newark, New Jersey,

[4]The institutions closed by state order consisted of theaters, churches, and schools. Under state pressure, the city council extended that to include banks, bars, drugstores, groceries, and butchers. It also required masks be worn in all businesses that remained open. The council rescinded its closings after three days, however.

Mayor Charles Gillen subverted state orders by allowing bars to sell alcohol "out the side door," extending his partial exemption to churches, and finally declaring on October 21 that the state closing no longer applied to Newark. The state Board of Health immediately fired off a telegram telling Gillen that he had no such authority, but the mayor stonewalled and the state relented on October 26 (Marisam, 2007, 370-71).

2.3 The November elections

The November 5th midterm election occurred during the second wave. The entire House of Representatives was up for election, as were the governors of 32 states. 38 seats in the Senate were in play, but aside from Maine all of those Senators had either been appointed by their state legislatures prior to the passage of the 17th amendment, or had been appointed by the state governor when their predecessors died in office or resigned.[5]

The national election campaign was overshadowed by World War 1. The biggest issues cut across party lines: prohibition and women's suffrage divided the parties internally. In April 1918, President Wilson accused the Republican candidate in a Wisconsin special election of being unpatriotic. The Democrats went on to claim that a GOP congress "would be a source of comfort to the Kaiser." In October, Wilson explicitly tried to turn the election into a referendum on the Great War. For its part, the GOP was careful to avoid directly criticizing President Wilson's prosecution of the war. It did, however, mercilessly attack the Democrats for a series of wartime economic policy missteps, most predominantly a decision to fix wheat prices while letting cotton values spike (Busch, 1999, 88-89; Livermore, 1948).[6] Support for the emergency USPHS package also cut across party lines, but the Republicans managed to make it hard for them to criticize the package after the Senate minority leader, Boies Penrose (R-PA), came out against it (Journal, 1918). The federal government's failure to contain the pandemic did not come up directly in the national partisan fray, save indirectly when Massachusetts Lieutenant Governor Calvin Coolidge (R) called for federal help in September (Boston Post, 1918).

The pandemic played a bigger role in gubernatorial elections, with some incumbents campaigning on their response. In New Jersey, in a clever ploy, Governor Walter Edge (R) campaigned on his response by louding proclaiming that he was not campaigning: "The only campaigns I am converned with right now are the Liberty Loan campaign and the campaign to suppress the influenza epidemic" (Newark Evening News, 1918).

The epidemics cascading across the United States led to sea-changes in campaign techniques. Rallies and meetings were no longer possible. In response, campaigns began to deploy mass buys of newspaper ads, billboard space, and direct mail blasts to reach voters on heretofore unseen scales (Washington Post, 1918). The new campaigning style opened the way for modern political action committees. The first of these, the GOP-leaning National Security League, interviewed candidates for acceptability, coded legislators' votes on national security issues, and sent out mass mailings of its "Loyalty list" against its targets. It also placed the list in 1800 newspapers across the nation.[7]

[5] Both incumbent senators in New Jersey, and one senator Louisiana, Missouri, Nevada, New Hampshire, Oregon, and South Carolina fell into the latter category.

[6] When Congress finally fixed cotton prices in July 1918 the GOP derided the "high" price as a giveaway to cotton interests.

[7] Some of the positions that the NSL inquired of candidates wanting its support, like favoring income tax cuts for high earners and opposing railroad nationalization, had an unclear relationship to national security. The NSL eventually alienated enough congresspeople of both parties that it was hauled up before a Congressional committee and found in violation of the Federal Corrupt Practices Act. See Edwards (1978) and Livermore (1948, 35).

The epidemic also opened up opportunities for political gamesmanship. This became most pronounced in New York State, where the GOP-controlled state government openly encouraged upstate officials to "coincidentally" ban public gatherings every time the Democratic candidate, Al Smith, was scheduled to speak. An angry Smith proclaimed that GOP officials "want to prevent the spread of Democratic doctrine rather the spread of Spanish influenza" (New York Times, 1918b). Smith soon abandoned campaigning upstate (New York Times, 1918a).

Generally, the elections proceeded as normal, although there were haunting images from California of masked voters lining up at the polls. Newspapers reported few irregularities. New York State allowed women to vote for the first time in 1918. Some smaller towns and rural counties may have cancelled the election, but there is no evidence that cancellations were widespread. In her study of the influenza epidemic in Nebraska, Kristin Watkins found no evidence that Cherry County held elections, but the rest of the state carried off the polls as normal (Watkins, 2015, 61). In Shasta county, California, polls in the town of Ingot "were not opened at yesterday's election for the simple reason that there were not enough citizens who were well enough and willing to serve on the election board during the influenza epidemic" (Morning Union, 1918). The fact that the Ingot election made the statewide papers, despite the town having only 95 registered voters, implies that election cancellations were not common. See, for example, Oakland Tribune (1918).

Turnout nonetheless fell dramatically, from 50.4 percent in the 1914 midterms to only 39.9 percent. Many observers ascribed the drop to the Spanish flu, but causality is not as immediately obvious as it might seem. Midterm turnout had been falling almost monotonically since 1894, from a high of 67.4 percent, and it would continue falling in the 1922 and 1926 elections. Presidential turnout followed the same pattern, falling from 79.6 percent in 1896 to 61.8 percent in 1916 and 49.2 percent in 1920. It is true that the 19th Amendment provided for female suffrage, but that amendment did not take effect until the 1920 election; New York was the only state in which women voted for the first time in 1918.[8] Other factors likely depressed the vote. Progressive reforms intended to weaken political machines meant that the number of non-southern counties that required voters to register in-person (rather than allowing party machines to register them) jumped 72 percent between 1900 and 1930; similarly, many states adopted the Australian ballot. Both reforms tended to depress turnout. In addition, many war workers had changed counties at a time when absentee voting was not common outside of active-duty soldiers.

Incumbents did not do well in the 1918 elections. The national popular vote swung 4.1 percentage points in favor of the GOP, resulting in a 24-seat pick-up and a 22-seat Republican majority. Six governorships changed hands and incumbent parties lost vote shares in every state. The pattern repeated itself in the 1919 and 1920 gubernatorial elections, after the third wave of the virus. In New York State, the incumbent Republicans openly blamed their loss on the influenza epidemic. Governor Whitman took the Democrats to court over contested vote counts and flu-related polling irregularities and did not concede defeat until January 14 (New York Times, 1918c).

3 Retrospective voting in historical perspective

Who should voters blame? In his classic introduction to retrospective voting, Morris Fiorina defined retrospective voting as "whether responsible or not, does the administration prosper in good times

[8] Montana and Nevada allowed female suffrage for the first time in 1916, making three switching states between the 1914 and 1918 midterm elections. Kleppner (1982) found that the expansion of female suffrage was not responsible for the fall in turnout over the two decades before 1916.

and suffer in bad times" (Fiorina, 1978, 430). Much of the existing empirical literature on retrospective voting has focused on business cycles. Those studies have mostly shown that voters in the United States and elsewhere punish the incumbent party at the legislative and (in presidential systems) executive levels.[9] The presence of retrospective voting is a common finding among studies of voter behavior. Healy and Malhotra (2013) provide an excellent overview of the literature, but a few findings are worth noting. In 1978, Fiorina (1978) argued that changes in people's personal economic situation influenced their vote, albeit not their decision to turn out. McDermott and Jones (2003) presented evidence that voters punish the majority party in Congress for bad outcomes, not merely their individual representative. de Benedictis-Kessner and Warshaw (2020a) found that American voters punish voters congressional and gubernatorial incumbents for local economic conditions.

There is a literature that suggests that voters retrospectively punish politicians for non-economic bad events. For example, in *Democracy for Realists*, Achens and Bartels concluded that voters do engage in both "blind" and attentive retrospective voting (Achen and Bartels, 2017).[10] Indeed, in one spectacular case, they found that voters punished President Woodrow Wilson in 1916 for shark attacks off the coast of New Jersey.[11]

In addition, studies found that voters indeed blame governments for natural disasters, especially when they believe that governments had the ability to respond. Houston voters in 2001 blamed the incumbent mayor for the damage from Tropical Storm Allison (Arceneaux and Stein, 2006). Healy and Malhotra (2009, 2010) found that voters punished governments for the economic damage caused by tornados, but not blindly. Rather, they punished governments for their failure to respond adequately to the disaster, rather than for the disaster itself. Cole et al. (2012) found a similar result for weather events in India: voters punished governments for disasters but punished them less when they delivered substantial relief. Gasper and Reeves (2011) also found that voters punish politicians for weather damage, although Gallagher (2020) challenged their results on empirical grounds. Chang and Berdiev (2015) presented cross-country evidence that governments are more likely to be replaced after a natural disaster.

The conventional wisdom is American voters entirely ignored the pandemic in the run-up to the 1918 elections. In his history of the epidemic, Alfred Crosby evinced, "It would be tempting to suggest that the pandemic must have played a role in deciding such a close election," he wrote, but "there were so many close elections swayed by so many different factors that it would be arbitrary to assign flu a decisive influence" (Crosby, 2003, 174-175). He concluded only that one race, the Senate contest in New Mexico, had been affected by the flu and in that only inasmuch President Wilson's attacks on the GOP candidate, Albert Fall, backfired in the wake of public sympathy for the death of Fall's son and daughter in the pandemic. In 2004, Aachen and Bartels revisited the issue, using data from a cross-section of 16 states and a second cross-section of 29 cities within those states (Achen and Bartels, 2004). They recognized, however, the paucity of their data and reluctantly concluded that the 1918 Spanish flu might have been an exception to their general finding.

[9]For the United States, see for example Abramowitz and Segal (1986); Kramer (1971); Lewis-Beck and Rice (1992); Lewis-Beck and Stegmaier (2014); Elinder (2010); Fair (1978); Larsen et al. (2019); Simonovits et al. (2019) in addition to the other works cited in this discussion.

[10]Blind retrospective voting involves punishing politicians for bad events over which they had no control. Attentive retrospective voting means punishing politicians for failing to prevent or ameliorate bad events that they could be reasonably expected to control.

[11]The finding about shark attacks and the 1916 election has come under some debate recently. Fowler and Hall (2018) criticized it; Achen and Bartels (2018) provided a rebuttal. It is worth noting that President Wilson called a cabinet meeting to try to develop a federal response the attacks, which is not consistent with the conventional — and mistaken — wisdom that federal officials did not attempt to mitigate local disasters before the New Deal.

What should voters blame politicians for? Obviously, some events are clearly inside politicians' control. It is logical to vote against the president because she signed a bill that you oppose or because she vetoed one that you support is fairly clear. Other events are clearly outside the government's control. Is the incumbent government responsible for shark attacks? The answer to that question is a fairly clear "no." For epidemic disease, however, the answer to that question is not as clear as it might seem. Consider, for example, the 1793 yellow fever epidemic in Philadelphia. The etiology of the disease was entirely unknown at the time; no one knew that it was spread by mosquitos. In fact, no one knew whether it was transmitted person-to-person or simply arose out of the climatic "miasma." Doctors in 1918 might not have known what caused the "grippe," but they did at least have the germ theory of disease. It is completely unclear, therefore, what, if anything, a voter in 1793 should have expected the government to do to combat yellow fever.

None of that, however, stopped supporters of the nascent Federalist and Republican parties from blaming each other for the outbreak. Federalists claimed that the disease had arrived with white French refugees fleeing the Haitian revolution and pinned responsibility on the Republicans for admitting them to the country. Alexander Hamilton (a Federalist) then politicized the medical response (such as it was) by identifying the Republicans with the use of bleeding to treat the disease while getting an alternative treatment (consisting of warm baths in tree bark and glasses of wine) known as the "Federalist cure."[12] For their part, the Republicans blamed the spread of the disease on Philadelphia's filth, misgovernance, and bad morals (Pernick, 1972). In short, both sides blamed the other for the outbreak despite an almost total lack of medical knowledge.

In a big federal country like the United States, the problem of allocating blame carries an additional geographic component. Consider again the 1793 yellow fever outbreak and its 1798 follow-up. In 1793, it was the Mayor of Philadelphia, Matthew Clarkson, who expropriated buildings for hospital use, created and funded an orphanage, purchased (or seized) food, clothing and what passed for "medicine" to distribute to the sick, and arranged to bury corpses after the plague had passed. When a second outbreak hit Philadelphia in 1798, it was the state government that imposed special taxes and took out emergency loans worth $1.2 million in 2019 dollars (DeClue, 2001, 151).[13] In both outbreaks, politicians attempted to blame the federal government for allowing the disease into the country or failing to prevent (somehow) its spread. Should voters have blamed the federal government, the state government, or the city government for the disasters?[14] Or should they have given credit to the city or the state for handling the crisis, despite the high death toll? In addition, should they have attributed the responsibility to the individual officials in charge or were they right to brand the entire Federalist and Republican parties?

There is a modern literature that holds that voters should and do hold state and local political parties accountable for changes in economic conditions. This argument rests on two legs. First, the assumption that state and local officials set important policies that affect economic conditions and that these policies change after elections. (See, for example, Caughey et al. (2017), de Benedictis-Kessner and Warshaw (2016) and de Benedictis-Kessner and Warshaw (2020b).) Sec-

[12]We now know that the Federalists were right but for the wrong reasons: their cure was merely ineffectual, while bleeding actually killed people.

[13]The cash grants distributed by the city in 1793 were small by modern standards: roughly $24 per family in 2019 dollars measured by the CPI. See Powell (1949, 242-243). The 1798 state appropriations, however, adjusted for GDP growth were the equivalent of having the State of Pennsylvania organize an approximately $1.7 billion relief effort in 2019.

[14]Voters today have a problem with this distinction. Sances (2017), for example, found that voters blame the president's party for local property tax increases resulting from ballot initiatives.

ond, the assumption that voters associate policies with the political parties and not just individual politicians (Ebeid and Rodden, 2006). If both these conditions hold, then voters should engage in retrospective voting aimed at the political parties in charge when bad events happen. Even some level of blind retrospection, as Achen and Bartels put it, could be rational: voters cannot know whether the people in charge could have lessened the disaster, but they certainly should give the people in charge some strong incentive to try. The literature thus far, however, has yielded contradictory results about retrospective voting. It has also focused on voter reactions to economic conditions, with all the endogeneity problems such studies entail.[15]

In 1918, it was clear that American governments at all levels possessed tools that could have mitigated the impact of the flu, from quarantines to shutdowns to mask-mandates to accelerated street cleaning to simple cash relief. It was also clear that American state and local governments had responded to similar catastrophes in the past (albeit often to questionable effect) and that the federal government, in the midst of an unprecedented harnessing of national power to defeat the German Empire, could also do so if it wished. It was therefore reasonable to conclude that voters should have held their governments accountable for the pandemic. But just because economic historians and political scientists think something was reasonable does not mean that real voters felt the same way. We therefore turn to an exploration of the empirical evidence about voting patterns and the 1918 flu.

4 Empirics

4.1 Data

To test the impact of the Spanish flu on electoral outcomes, we built a county-level dataset for the period 1915-1921. This dataset includes electoral outcomes, mortality rates, excess mortality, demographic variables, and economic controls. For electoral outcomes, we relied on Clubb et al. (2006) for congressional and presidential elections and on the Inter-university Consortium for Political and Social Research (1999) for gubernatorial elections. In particular, we considered presidential, gubernatorial, and congressional vote shares and turnout by county for the different political parties for state and national elections between 1914 and 1921.

For gubernatorial elections, we first identified the governor's political party at the time of the election. We then identified the incumbent party for each state for the races before, during, and after the epidemic. We computed turnout rates for congressional races between 1914 and 1918. For our robustness checks, we calculated the change in vote shares between 1916 and 1918 for congressional races. For gubernatorial races, we calculated the change in vote shares between the first election prior to the start of the second wave of the flu and the next subsequent election.[16] For the 1920 presidential race, we estimated the vote share change (between 1916 and 1920) for the Democratic candidate (incumbent President Woodrow Wilson in 1916; Governor James Cox of Ohio in 1920).

We estimated overall excess mortality for each county to study the impact of the Spanish flu using mortality and population data from Bailey et al. (2018) and Department of Commerce (1923). Excess mortality, unlike flu mortality, captures the effect of the pandemic on the overall health sys-

[15] See, for example, Arnold and Carnes (2012), Ebeid and Rodden (2006), Hopkins and Pettingill (2018), Howell and McLean (2001), Howell and Perry (2004), Oliver and Ha (2007); and Wright (2012). de Benedictis-Kessner and Warshaw (2020b) use a large panel dataset and find strong evidence for retrospective economic voting at all levels of government, but their study, while strong, does not directly account for potential endogeneity. Hilt and Rain (2020) take an innovative approach to the question, analyzing voter reactions to changes in the price of liberty bonds. They

Table 2: Summary statistics

	Mean	SD	N
Electoral outcomes			
Incumbent governor vote share, first post-flu election, in %	46.13	19.56	951
Incumbent governor vote share, previous election, in %	51.54	17.90	951
Congressional Democratic vote share, 1918, in %	42.13	25.50	941
Congressional Democratic vote share, 1916, in %	44.71	18.20	949
Presidential Democratic vote share, 1920, in %	35.46	18.01	951
Presidential Democratic vote share, 1916, in %	49.58	12.57	949
Turnout, 1918, in %	44.50	19.06	941
Turnout, 1916, in %	60.69	17.64	951
Flu variables			
Excess mortality, 1918, per 100,000	384.95	370.99	951
Mortality rate, 1917, per 100,000	1303.09	618.70	951
Flu arrival date	3.57	0.96	951
Distance to nearest camp	171.75	119.32	950
Controls			
County casualties per 100,000	82.39	39.67	951
Population density, persons per sq. mile	261.62	3556.73	951
Urbanization rate, 1920	0.26	0.27	951
Share of Black population, 1920	0.07	0.15	951
Share of illiterate population, 1920	0.06	0.06	951
Manufacturing wages per worker	995.21	241.19	951

tem and deaths from flu-related diseases.[17] We then calculated excess mortality in 1918 with respect to different benchmarks depending on the specification and data availability. By georeferencing a map of the spread of the Spanish flu by U.S. Navy (1920) (see Figure 1), we added a categorical variable for each county in order to control for the estimated time of arrival at each location. This exercise yielded five periods ranging from 1 to 5: before September 14th, between September 14th and September 21st, between September 21st and September 28th, between September 28th and October 5th, and after October 5th.

We added county-level controls from the 1920 U.S. census compiled by Haines (2020). These controls included population density, urbanization, illiteracy, and the share of black population. Thanks to data kindly provided by Roberts and Burda (2018), we were also able to control for the number of war casualties by county, which impacted the vote for Democratic congressional candidates. We compiled a comprehensive list of the existing military camps from Crowder (1919), Bowen (1928), and U.S. Navy (1920). As potential vectors in disease spread, we estimated the distance to the nearest camp from the county centroid. Economic conditions could have affected voters' behavior, of course. In the absence of good county-level data on unemployment, we controlled for the average manufacturing wage as reported by the U.S. census.

In our sample, the average vote share for incumbent governors circa 1918 was 46 percent, the average congressional Democratic vote in 1918 was 42 percent, and the average vote share for the Democratic candidate in 1920 presidential races was 35 percent. Turnout was 44.5 and 60.7 percent in 1918 and 1916 respectively. Excess mortality ran around 385 deaths per 100,000 people (see Table 2). In terms of the progression of the epidemic in our sample, around 16 percent of the counties with 6 percent of the total population experienced its arrival after October 5th. For around 82 percent of the counties in our sample, the flu arrived between September 21st and October 5th, reaching around 86 percent of the population.

4.2 Empirical analysis

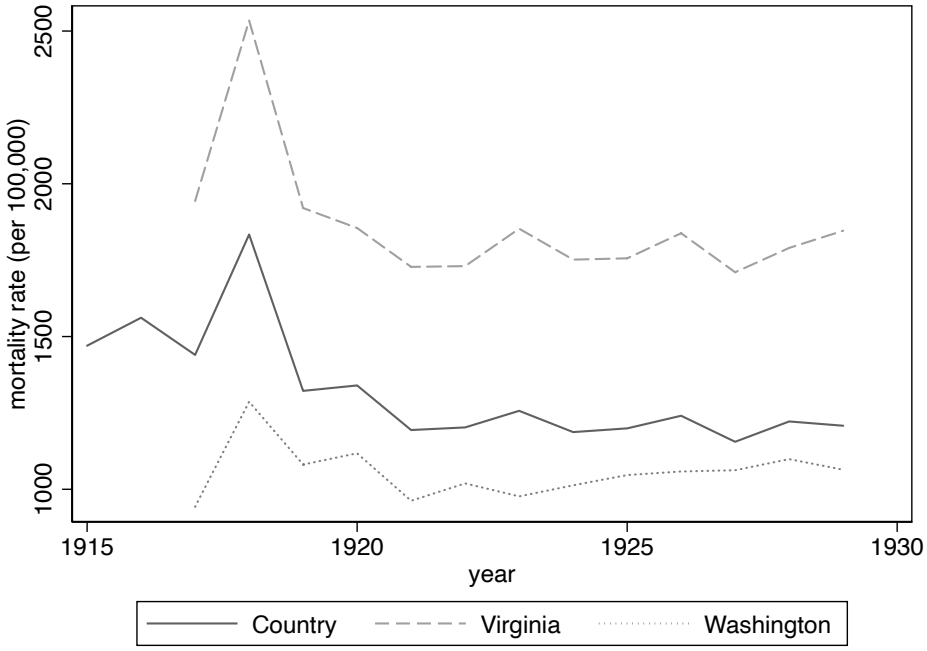
Our empirical strategy is to exploit the geographical variability of the incidence of the Spanish flu. Starting on the east coast, the flu moved rapidly westward and reached the entire country by early October. The incidence was far from uniform throughout the country. In our sample, 42 percent of counties had excess mortality rates over the mean of 385 deaths per 100,000. Over eight percent of the counties experienced excess death rates over twice the national mean, containing 22.4 percent of the total population of our sample. The top one percent of counties had a combined excess mortality rates over 4,000 per 100,000, covering nearly half a million people. Overall mortality ranged from 2,534 per 100,000 in Virginia while Washington state "only" registered a rate of 1,286 deaths per 100,000 (see Figure 5, for mortality rates). The hardest hit county was Henrico in Virginia with excess mortality of over 6,700 per 100,000. On the other end of the distribution, around 6 percent of all counties —covering also around 6 percent of the population— saw no change or improvement in excess mortality in 1918.

use an I.V. approach to deal with endogeneity in how many liberty bonds were purchased by voters in a specific county and treat bond prices as exogenous.

[16] Gubernatorial elections do not take place on a regular schedule. In the case that the race did not take place in 1918, we identified the first one after the second flu wave.

[17] In many districts, influenza was not a reportable disease until well into the second wave of the pandemic and calculating excess flu mortality–that is, flu mortality above that which would normally be expected–is possible for only a few jurisdictions.

Figure 5: Mortality rate



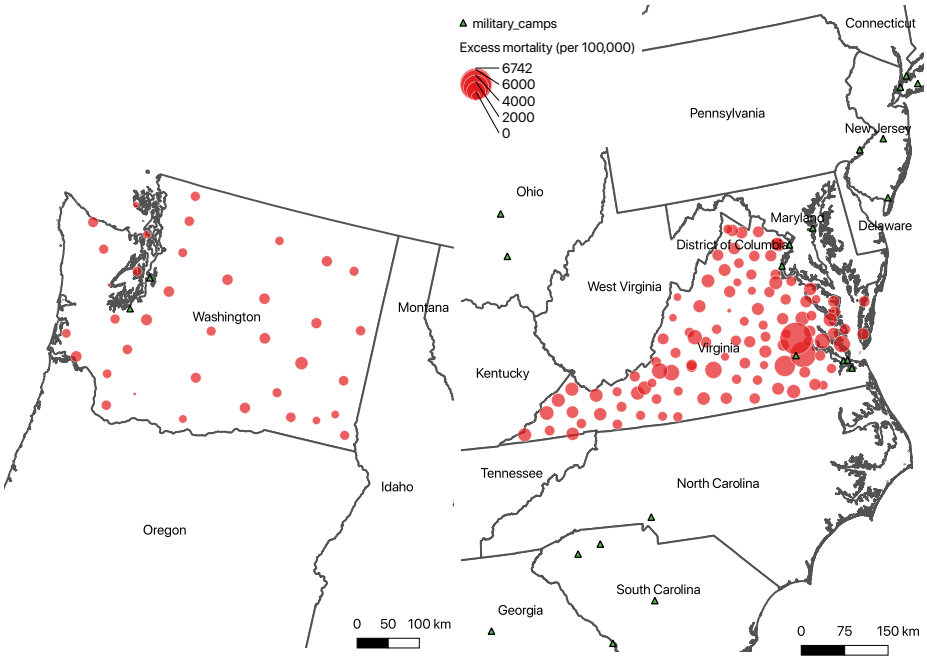
Some states had little spatial variation with overall low levels of excess mortality while others had high variation with high excess mortality. Consider the cases of the states of Washington and Virginia (see Figure 6). The mean excess mortality for Washington was over 50 percent lower than for the country and nearly 20 percent of all counties had either zero or negative excess mortality in 1918. In contrast, Virginia suffered a much more variable and deadly fate. The mean was 574 excess deaths per 100,000, almost 50 percent higher than the national average. Half of all Virginia counties exceeded the national average. Only six percent of all counties saw no change in excess mortality..

We are interested in identifying whether voters engaged in retrospective voting due to the pandemic, punishing incumbent governors (or their party) or the party in the control of the national government (the Democrats). In Section 3, however, we presented evidence that state and federal governments both possessed and used tools to mitigate the impact of the pandemic. It was therefore reasonable for an attentive retrospective voter to punish the relevant level of government for greater flu-related mortality. Our data, however, do not allow us to directly distinguish between blind and attentive retrospective voting.

We use the following electoral outcomes E for county i: the vote share of the governor's party ($Governor$), the vote share for congressional Democratic candidates ($Democrats$), the vote share for the Democratic presidential candidate in 1920 ($President$), and turnout ($Turnout$). To control for the voters' relative reaction to the Spanish flu, we also include the corresponding outcomes in the previous election ($PrevElection$).[18] Our main variables of interest are excess deaths defined as the difference between the mortality rate in 1918 (per 100,000) and the average mortality rates in

[18]Recent literature on accountability uses changes in vote shares, see for example Bartels (2008) and Healy and Lenz (2014). We use this alternative specification as a robustness check. Spoiler alert: the results don't change!

Figure 6: Excess mortality



1915, 1916, and 1920-24.[19] We include a categorical variable when the flu arrived in a given county. Controls include turnout for the previous election, urbanization, population density, share of black population, illiteracy rate, average manufacturing wage, and war casualty rates. All specifications include state fixed effects to take into account time-invariant differences between states.[20]

$$E_i = \alpha + \beta ExcessDeath_i + \gamma FluArrival_i + \tau PrevElection_i + \delta X_i' + \epsilon_i \quad (1)$$

The OLS baseline results suggest that voters punished the ruling party but the punishment was small. For the governor's party and congressional Democrats, a standard deviation increase in excess mortality from the Spanish flu decreased vote shares betwwen 0.41 to 1.38 percentage points in gubernatorial and congressional races (see column (1) in Table 3 in panels A and B). The coefficients are quite statistically significant. These results suggest a small but systematic effect of the incidence of the Spanish flu on partisan accountability. Voters do not, however, appear to have blamed the Democratic nominee in 1920 for the failures of the Democratic administration and Congress in 1918: our OLS results indicate a slight positive coefficient on excess mortality and the Democratic presidential vote share in 1920 (see Table 3, panel C). This result, however, appears to be regression to the mean; as we discuss below, it does not survive attempts to account for potential endogeneity.

A potential problem with our analysis is that the Spanish flu could have affected the number of people casting their votes. We therefore examined the effect of the flu on turnout rates in 1918

[19]Using future mortality rates as a benchmark to estimate mortality during a pandemic is a standard practice in epidemiological studies (see Ansart et al. (2009); Chowell et al. (2014); Dahal et al. (2018), among others).

[20]In particular, state fixed effects will pick up whether the state introduced female suffrage between the election period (as New York did) or whether the incumbent governor was running in the election.

Table 3: Electoral outcomes

A. Vote for Governor's party

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS
Excess mortality, 1918, per 100,000	-0.0011**	-0.0011**	-0.0020***	-0.0018***
	(0.0223)	(0.0219)	(0.0007)	(0.0005)
R-sq	0.89	0.89	0.89	0.89
Observations	951	951	951	951
F-test: excess mortality			204	214
Flu arrival?		✓		✓

B. Congresional vote for Democratic party

	(1)	(2)	(3)	(4)
Excess mortality, 1918, per 100,000	-0.0037***	-0.0037***	-0.0016*	-0.0019*
	(0.0020)	(0.0016)	(0.0850)	(0.0609)
R-sq	0.74	0.74	0.74	0.74
Observations	941	941	941	941
F-test: excess mortality			209	209
Flu arrival?		✓		✓

C. Vote for President's party

	(1)	(2)	(3)	(4)
Excess mortality, 1918, per 100,000	0.0011**	0.0011**	0.0007	0.0008
	(0.0467)	(0.0436)	(0.1599)	(0.1414)
R-sq	0.94	0.94	0.94	0.94
Observations	951	951	951	951
F-test: excess mortality			203	215
Flu arrival?		✓		✓

D. Turnout

	(1)	(2)	(3)	(4)
Excess mortality, 1918, per 100,000	-0.0020*	-0.0020*	-0.0006	-0.0008
	(0.0612)	(0.0595)	(0.4614)	(0.3538)
R-sq	0.89	0.89	0.89	0.89
Observations	966	966	966	966
F-test: excess mortality			178	187
Flu arrival?		✓		✓

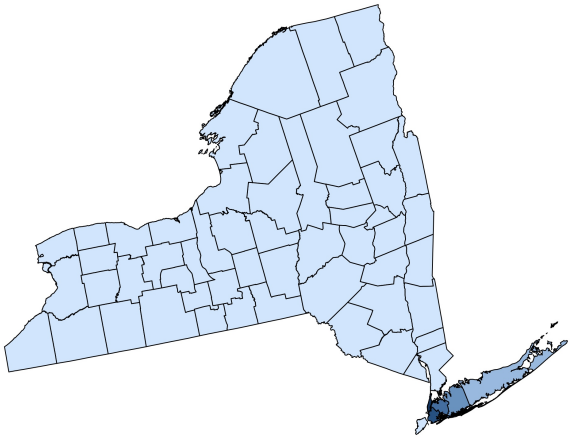
Notes: The table shows results from a fixed effects regression of *Vote for Governor's party, Congressional Democratic party, President's party, and Turnout.* Each specification includes state fixed effects, and controls for war casualty rates, previous elections, demographic, and economic variables. Specifications (2) and (4) include flu arrival as a control. See section 4.1 for a description of these variables. P-values in parentheses. Standard errors are clustered on state. *, **, and *** indicate significance at the 10, 5, and 1 percent levels, respectively.

controlling for turnout rates in the previous midterm election in 1914. We find a slightly significant effect in our OLS specifications, an increase of one standard deviation in excess mortality led to a decline of three-quarters of a percentage point in turnout in 1918 (see Table 3, panel D, columns (1) and (2)).

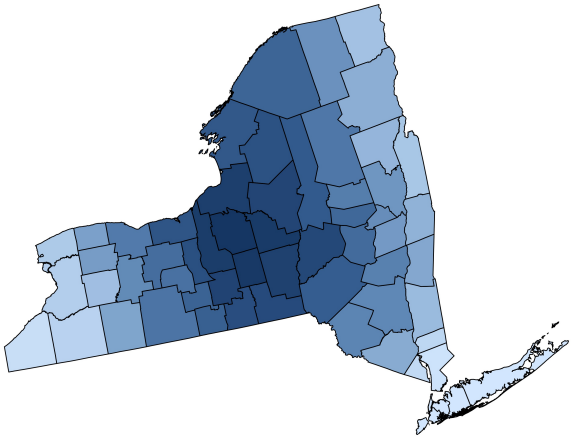
It is unlikely that voters' horizons stopped at county lines — voters likely cared not only about the impact of the Spanish flu in their county but in neighboring counties and beyond. As Tobler (1970, 234) First Law of Geography declared, "everything is related to everything else, but near things are more related than distant things." It is possible that our coefficients are failing to take into account vote swings generated not by changes in mortality in the country but changes in mortality in neighboring (or distant) counties as well. We therefore needed to tease out two effects: the direct effect of excess mortality of a given county and the indirect effect of neighboring counties. In order to do that we estimated a contiguity matrix to capture spatial interactions between counties, which allowed us to account for global spatial spillovers in order to estimate the effects of the pandemic on electoral outcomes.

Table 4: Spillover effects in Congressional Races – New York State

(a) Manhattan (New York county)



(b) Syracuse (Onondaga county)



How does this analysis work in practice? Consider New York State for example. Changes in excess mortality in Manhattan affected congressional Democratic vote shares big time in Brooklyn, quite a bit in Queens, noticeably in Nassau, and slightly in Suffolk ... but not at all anywhere else (see Figure 4, panel (a)). Nobody upstate appears to care about flu deaths in Manhattan. Conversely, in a result that will surprise precisely no one who knows anyone from New York, mortality in Syracuse affected vote shares across a broad swathe of the state but for voters in the five boroughs and Long Island it might as well have been Canada (see Figure 4, panel (b)). Note that if everything were indeed related to everything else — that is to say, if voters in Erie County cared as much about deaths in East Flatbush as they did about deaths in Lackawanna — then the entire state would be colored dark blue.

Across our sample, we find that spillover effects for gubernatorial races were fairly low. The coefficients barely changed compared to our core regressions (see columns (3) and (4) on Panel A of Table 5). In other words, voters reacted more to changes in mortality in their county and than they did to changes elsewhere. In congressional races, spillover effects were higher; however, the magnitude of the coefficient was remarkably unchanged (see columns (3) and (4) on Panel

B). This result is consistent with the view that voters were sending a message to the national government about its broader handling of the pandemic via their congressional vote, whereas they simply expected their governor to make things better for them.

Table 5: Spillover effects

A. Vote for Governor's party

	(1) OLS	(2) OLS	(3) SP	(4) SP
Excess mortality, 1918, per 100,000	-0.0011**	-0.0011**	-0.0011*	-0.0011*
	(0.0223)	(0.0219)	(0.0857)	(0.0787)
R-sq	0.89	0.89		
Observations	951	951	951	951
Spatial correlation p-value			0.5632	0.5755
Flu arrival?		✓		✓

B. Vote for Congressional Democratic party

	(1)	(2)	(3)	(4)
Excess mortality, 1918, per 100,000	-0.0037***	-0.0037***	-0.0037***	-0.0037***
	(0.0020)	(0.0016)	(0.0053)	(0.0043)
R-sq	0.74	0.74		
Observations	941	941	941	941
Spatial correlation p-value			0.0000	0.0000
Flu arrival?		✓		✓

Notes: The table shows results from a fixed effects regression of *Vote for Governor's part and Congressional Democratic party.* Each specification includes state fixed effects, and controls for war casualty rates, elections, demographic, and economic controls. See section 4.1 for a description of these variables. P-values in parentheses. Standard errors adjusted by spatial autocorrelation. *, **, and *** indicate significance at the 10, 5, and 1 percent levels, respectively.

A skeptical reader may think that our results are plagued by the bane of social science's existence: endogeneity. Our baseline OLS regressions control only for observable differences between counties. A number of unobservable factors could be driving these results. For example, one could claim that the use of non-pharmaceutical interventions (NPIs) had an effect on both excess mortality and voter behavior.[21] Inasmuch as state governments implement NPIs fixed effects will pick them up, but many restrictive initiatives were enacted at the local level.[22] Previous studies argued that while NPIs managed to reduced peak mortality, they did not reduce cumulative mortality. City-level evidence suggests that these interventions did not last long enough to make a dent in overall mortality (Barro et al., 2020). Nonetheless, they could have affected voter behavior. Voters might have rewarded politicians for action, or punished them because of the economic costs imposed by public gathering bans, school shut-downs, and restaurant and theater closures.

In order to address these concerns, we employed an I.V. approach. The underlying health environment affected how hard counties were hit by the flu in 1918. The slow-changing underlying health environment of the county did not, however, have a direct effect on the change in voter

[21] See Markel et al. (2007) for a discussion of these interventions.

[22] Moreover, state-level initiatives depended on local cooperation, as the example of Newark, New Jersey, makes clear. To our knowledge, there are no accounts or datasets of county-level NPIs or their implementation.

behavior between one election and another. We therefore instrumented excess mortality with death rates from the previous year. The first stage results, as revealed see the F-tests, indicate a strong instrument. Our main results held in the second stage. For gubernatorial races, the estimated effect was a decline of 0.7 percentage points from an increase of one standard deviation in excess mortality. For congressional races, the 2SLS coefficients imply a smaller impact of the flu on Democratic vote share; the estimated impact is a decline around 0.6-0.7 percentage points from an increase of one standard deviation of excess mortality. For the presidential race, the puzzling positive coefficient loses significance, implying that the positive correlation was not causal. For turnout, perhaps surprisingly, the 2SLS showed no significant decline in turnout due to the pandemic.

As a wise colleague told us, a credible and good instrument is the result of blood, sweat, tears, and luck. Therefore, as an additional check on the possibility that omitted variables are driving our results, we embarked on an exercise known as "selection on unobservables". Pioneered by Altonji et al. (2005) and Bellows and Miguel (2009), the idea is to figure out the potential influence of unobservables on our main variables of interest. On Figure 7, we present the main coefficients of interest: we looked into the effect of excess mortality on vote shares for gubernatorial and congressional races. We started with a restricted specification using only state fixed effects and controls for the previous election ("No controls") and added first demographic controls such as population density and the black population share, followed by illiteracy rates and manufacturing wages. We continued adding regressors sequentially, adding war casualty rates and the flu arrival date. We then compared the coefficients from the restricted specifications to the full specification with all the controls. We found that the attenuation of these coefficients for the congressional and gubernatorial races was fairly small, a difference of .00002 and 0.0009 respectively. Taking into account this difference, we estimated how large the unobservables had to be in order to explain away the effect of the Spanish flu.[23] The estimated ratios for both races were high (see Table 7), suggesting that additional controls would not erase the effect of the pandemic on electoral outcomes.

Table 6: Selection on unobservables

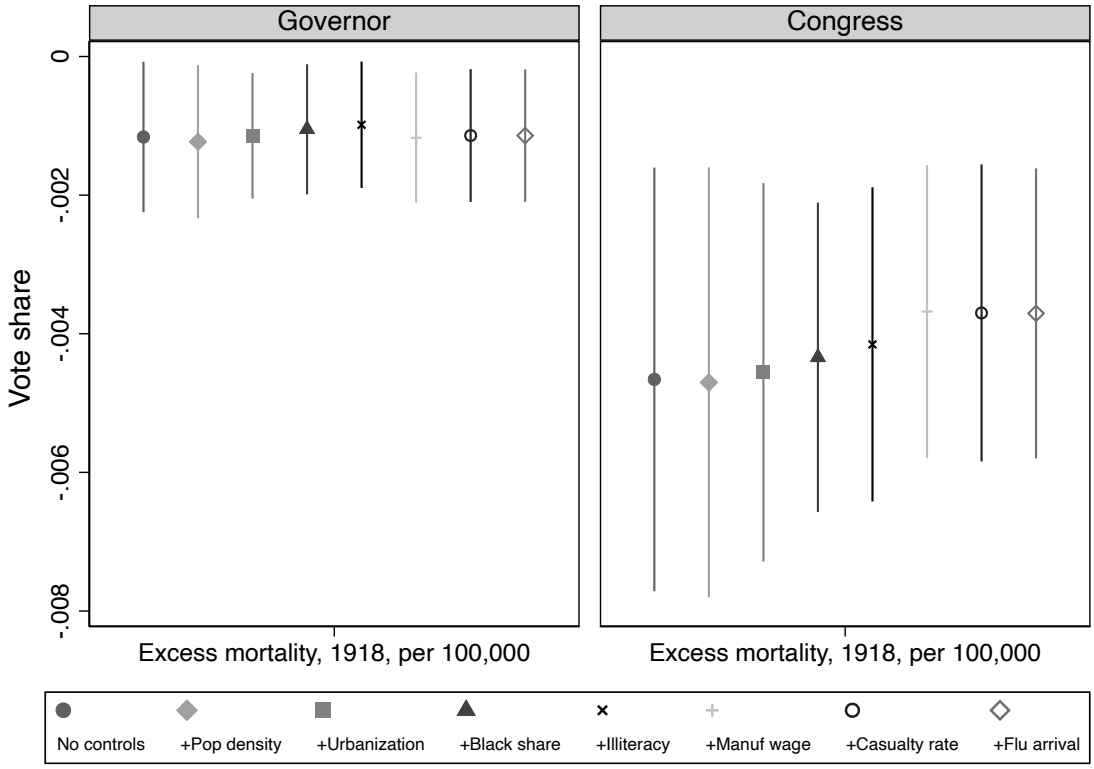
	Governor	Democrats
Population density	17.95	106.88
Urbanization	70.74	44.50
Black share	9.46	13.63
Illiteracy	5.59	8.23
Manufacturing wage	98.58	3.76
Casualty rate	53.02	3.86
Flu arrival	56.53	3.89

4.2.1 Robustness checks

A potential concern about our core results lies in factors that could have amplified or diminished excess mortality. From the contemporaneous accounts on the Spanish flu, the severity of the epidemic decreased as it traveled across the country (Sydenstricker, 1918). This implies that virulence was

[23] Mathematically, we computed the ratio as follows: $\beta_{full}/(\beta_{restricted} - \beta_{full})$ where $\beta_{restricted}$ is the coefficient for excess mortality with only controls for the previous election and β_{full} is the coefficient with different set of controls depending on the specification.

Figure 7: Selection on unobservables - coefficients



Notes: The graph shows coefficients of *excess mortality* from a fixed effects regression of *Vote for Governor's party* and *Congressional vote for Democrats*. Each specification includes state fixed effects with standard errors clustered on state. Controls are added cumulative starting with controls for the previous election to include population density, urbanization rate, black population share, illiteracy rate, average manufacturing wage, war casualty rate, and flu arrival date.

nonlinear with differential impact on localities. To capture this feature, we took advantage of our flu arrival indicator to create a flu virulence variable. This virulence variable decreases non-linearly from the earliest to latest hit counties. Another determinant of excess mortality often cited is the demobilization of military personnel after WWI. Indeed, Camp Devens in Massachusetts registered the first case of the second wave of Spanish flu. Crosby (2003) and Barry (2005) made the case that these camps provided early vectors of transmission.[24] To take into account this potential channel of transmission, we computed the distance as the crow flies from the centroid of each county to the nearest military camp.

We present these new estimates jointly on Table 7. For simplicity, we only include electoral outcomes for gubernatorial and congressional races. The baseline results (from Table 3) are on columns (1) and (3). Controlling for our proxy for flu virulence (columns (2) in panels A and B) does not change the results; the coefficients are remarkably stable and remain significant at similar

[24]The *Public Health Reports* of 1918 cast some doubts over the importance of military camps. Sydenstricker (1918, 2312) notes that influenza deaths came earlier from military camps; however, the timing may have been due to "greater promptness [reporting] in the camps".

Table 7: Robustness checks: Flu

A. Vote for Governor's party

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS
Excess mortality,	-0.0011**	-0.0011**	-0.0020***	-0.0019***
1918, per 100,000	(0.0223)	(0.0219)	(0.0007)	(0.0018)
R-sq	0.89	0.89	0.89	0.89
Observations	951	951	951	950
F-test - excluded instrument(s):			204	141
Flu virulence?		✓		
Distance to camps?				✓

B. Vote for Democratic party

	(1)	(2)	(3)	(4)
Excess mortality,	-0.0037***	-0.0037***	-0.0016*	-0.0019*
1918, per 100,000	(0.0020)	(0.0018)	(0.0850)	(0.0948)
R-sq	0.74	0.74	0.74	0.73
Observations	941	941	941	940
F-test - excluded instrument(s):			209	138
Flu virulence?		✓		
Distance to camps?				✓

Notes: The table shows results from a fixed effects regression of *Vote for Governor's party and Democratic party*. Each specification includes state fixed effects, and controls for war casualty rates, elections, demographic, economic variables. See section 4.1 for a description of these variables. P-values in parentheses. Standard errors are clustered on state. *, **, and *** indicate significance at the 10, 5, and 1 percent levels, respectively.

levels.[25] To gauge the importance of military camps, we used the distance to camps as second instrument to predict excess mortality in our two-stage-least squares estimation (see column (4) in panels A and B). This additional instrument worked for both races.

Gubernatorial and congressional races are different animals. The governor is elected by tallying the votes for the whole state; the results from individual counties do not count the same. Governors may engage in strategic campaigning or strategic policymaking to secure the votes from more populous locations. We add two additional robustness checks for gubernatorial races: population-weighted specifications and restricting the sample to counties with population of more than 10,000. The impact of the flu on these races was still significant and negative in all these specifications (see Table 8). The population-weighted coefficient is larger (see columns (3) and (4) relative to our core regressions in columns (1) and (2)). When looking at more populous counties, the results did not differ much.

Table 8: Robustness checks: Population

	Vote swing against Governor's party					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	OLS	OLS	OLS
Excess mortality,	-0.0011**	-0.0011**	-0.0031*	-0.0033*	-0.0013**	-0.0013**
1918, per 100,000	(0.0223)	(0.0219)	(0.0948)	(0.0798)	(0.0271)	(0.0252)
R-sq	0.89	0.89	0.94	0.94	0.90	0.90
Observations	951	951	973	973	813	813
Flu arrival?		✓		✓		✓
Population weighted?			✓	✓		
>10K population?					✓	✓

Notes: The table shows results from a fixed effects regression of *Vote for Governor's part and Democratic party.* Each specification includes state fixed effects, and controls for war casualty rates, elections, demographic, and economic variables. See section 4.1 for a description of these variables. P-values in parentheses. Standard errors are clustered on state. *, **, and *** indicate significance at the 10, 5, and 1 percent levels, respectively.

Our preferred specification presented on Table 3 used levels of our main electoral outcomes for the elections in 1918 (or circa 1918 for gubernatorial races), adjusting for the outcomes of the previous race. This specifications takes into account differences in the pre-existing electoral landscape at the county level and capturing the potential differential effect that previous elections had on the election of interest. As a robustness check, however, we consider the swing: i.e., the difference between shares with respect to the previous election. This specification forces equal weight of the electoral outcomes of the previous election to the election of interest. As shown in Table 9, all of our results are consistent with the findings in our core regressions. There were no major differences except that the coefficients were overall a tiny bit bigger in magnitude.[26]

[25] We also tried alternative non-linear constructions of the virulence variable, e.g. exponential on flu arrival date. The results did not change.

[26] This specification also yields similar results when subjected to the battery of robustness checks presented earlier in this section. Results are available upon request.

Table 9: Electoral outcomes

A. Vote swing against Governor's party

	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS
Excess mortality, 1918, per 100,000	-0.0015**	-0.0015**	-0.0029***	-0.0027***
	(0.0153)	(0.0115)	(0.0001)	(0.0002)
R-sq	0.87	0.88	0.87	0.88
Observations	951	951	951	951
F-test: excess mortality			206	216
Flu arrival?		✓		✓

B. Vote swing against Democratic party

	(1)	(2)	(3)	(4)
Excess mortality, 1918, per 100,000	-0.0040**	-0.0040**	-0.0036**	-0.0039**
	(0.0196)	(0.0190)	(0.0171)	(0.0151)
R-sq	0.27	0.27	0.27	0.27
Observations	941	941	941	941
F-test: excess mortality			203	213
Flu arrival?		✓		✓

C. Vote swing against President's party

	(1)	(2)	(3)	(4)
Excess mortality, 1918, per 100,000	0.0013*	0.0013*	0.0005	0.0006
	(0.0809)	(0.0773)	(0.3651)	(0.3248)
R-sq	0.75	0.75	0.75	0.75
Observations	951	951	951	951
F-test: excess mortality			206	216
Flu arrival?		✓		✓

D. Change in turnout

	(1)	(2)	(3)	(4)
Excess mortality, 1918, per 100,000	-0.0012*	-0.0012*	-0.0002	-0.0002
	(0.0677)	(0.0646)	(0.8311)	(0.7274)
R-sq	0.42	0.42	0.41	0.42
Observations	943	943	943	943
F-test: excess mortality			202	213
Flu arrival?		✓		✓

Notes: The table shows results from a fixed effects regression of *Vote swing against Governor's party, Democratic party, President's party, and Change in turnout.* Each specification includes state fixed effects, and controls for war casualty rates, elections, demographic and economic variables. Specifications (2) and (4) include flu arrival as a control. See section 4.1 for a description of these variables. P-values in parentheses. Standard errors are clustered on state. *, **, and *** indicate significance at the 10, 5, and 1 percent levels, respectively.

5 Conclusion

The devastation brought about by the Spanish flu was so deep that it depressed American life expectancy by more than 10 years (Grove and Hetzel, 1968). The Spanish flu presented political costs to incumbent politicians. Localities where excess casualties piled up swung against the ruling party in gubernatorial elections and the Democrats in congressional elections. A shift of one standard deviation implied a vote shift of about half a percentage point against the party in power. But those shifts were not enough to swing gubernatorial elections, with Maryland coming closest. Achen and Bartels (2017) were too quick to dismiss the presence of retrospective voting in 1918, but they were right to conclude that the Spanish flu did not determine the outcome. The presence of retrospective voting on one issue does not mean that other issues disappear.

In short, voters were attentively retrospective to flu casualties but the punishment they meted out to incumbents was small relative to other factors. The 1918 election was an anti-incumbent wave election. The Spanish flu contributed to that wave: voters swung more against the congressional Democrats and the incumbent governor when the flu was more severe. But the reaction to the flu was only a small part of why voters turfed so many incumbents out of office. A full analysis of the 2020 election remains to be written but voters facing Covid appear to have reacted in much the same way as their counterparts a century ago. With pandemic elections coming up across the world, death tolls will almost certainly influence the results — but they will likely be far less determinative than our instincts would suggest.

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