Did COVID-19 cost Trump the election?

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Abstract

A common narrative is that COVID-19 cost Trump re-election. We do not find supporting evidence; if anything, the pandemic helped Trump. However, we find substantial evidence that voters abandoned Trump in counties with large increases in health insurance coverage since the Affordable Care Act, presumably fearing the roll-back of such expansion. Absent this effect, our estimates imply Trump would been on the precipice of re-election by winning Georgia, Arizona, Nevada, and only losing Wisconsin by a few thousand votes. Finally, while US trade war tariffs boosted Trump's support, foreign trade war tariffs and US agricultural subsidies had little effect.

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1 Introduction

The 2020 US Presidential election is one of the most controversial in modern history. A sizable minority of US voters questioned the widely acknowledged results amid suspicions over widespread voter fraud, resulting in pro-Trump supporters storming the Capitol during congressional certification of Joe Biden's victory. Additionally, many far-reaching policies and actions of the Trump administration were plausibly salient in voters' minds. Thus, understanding voting behavior in this election is fundamentally important.

The COVID-19 pandemic is perhaps the most common narrative explaining Trump's 2020 election defeat. According to the Washington Post, "[T]the president finally lost, aides and allies said, because of how he mismanaged the virus" (Dawsey et al. (2020)). Similarly, the BBC said "It was his botched handling of the crisis that contributed to his fall" (Bryant (2020)). And, TIME argued "his prospects for re-election were dragged down by... his reckless approach to a virus that landed him in the hospital at the peak of the campaign" (Bennett and Berenson (2020)).

Other common narratives focus on certain socioeconomic groups, particularly minority voters and suburban women. The Texas Tribune argued "In Democrats' bid to flip Texas, maximizing the Latino vote is key" (Carolan (2020)). Helping sink Trump, The Wall Steet Journal argued "Black Americans overwhelmingly backed President-elect Joe Biden in the 2020 election" (Belkin and Jamerson (2020)). And, the Associated Press News emphasized "Black women and suburban women, in particular, proved to be pillars of Biden's coalition" (Noveck (2020)).

Despite these narratives, the Trump administration also pursued various far-reaching policies that substantially affected wide segments of the American population. The Affordable Care Act (ACA), perhaps President Obama's lasting legacy, expanded health insurance coverage to millions of Americans. However, viewing it as government overreach, Republicans have pursued executive, congressional, and judicial avenues to repeal and undermine the ACA. Thus, its judicial and legislative foundation is not concrete. And, Trump's trade war left US protectionism at levels unseen since the infamous 1930 Smoot-Hawley tariffs despite the traditional Republican commitment to free trade.[1] Ultimately, Trump's wars on the ACA and trade are highly salient issues for many voters.

We analyze the county-level impacts of COVID-19, the post-ACA expansion of health insurance, and the trade war (US tariffs, foreign retaliatory tariffs, and US agricultural subsidies following these foreign tariffs) on the change in Trump's vote share between the

[1] For example, over 90% of Republican votes in the US House of Representatives were in favor of Free Trade Agreements over the 2003-2011 period compared to 37% of Democrat votes (Lake and Millimet (2016)).

2016 and 2020 US Presidential elections. Normalizing by population, our main measure of COVID-19 prevalence is deaths since the pandemic began but we also consider cases and deaths in various time windows. As a share of population, we compute health insurance coverage expansion between the 5-year American Community Survey (ACS) in 2018 (the first fully in the post-ACA period) and 2013 (the last fully in the pre-ACA period). Typical in the trade literature, we combine industry-level trade war tariffs (and agricultural subsidies) with county-by-industry employment composition to create county-level trade war exposure. We control for various county-level economic, demographic and health characteristics that could correlate with these salient issues and voting behavior. These include measures of social distancing and economic activity; health characteristics reflecting increased risk of COVID-19; measures of urbanization and population density; and the distribution of age, race, income and education.

Nevertheless, endogeneity concerns may remain. Thus, we pursue various instrumental variable (IV) strategies. For COVID-19, we use two alternative IVs: the population share of nursing home residents and, following Baccini et al. (2020), the employment share of meat-packing workers. The exclusion restrictions say that *conditional* on the composition of age, race, income and education as well as health characteristics of the county (and other controls), the change in Trump's vote share between 2016 and 2020 only depends on the instruments through their impact on COVID-19 deaths (or cases). We argue these are reasonable exclusion restrictions and the media have documented both as key sources of COVID-19 outbreaks.[2] Endogeneity concerns over the trade war and health insurance coverage expansion are more challenging. Indeed, instruments in the context of tariffs are notoriously problematic; Blanchard et al. (2019, p.3) state "We stop short of claiming causal identification... the 2018 tariffs were not orthogonal to future US political considerations...". Thus, we use the heteroskedasticity-based IV approach of Lewbel (2012). While less intuitive than a traditional IV approach, our Lewbel IV approach works well according to standard IV specification tests.

We do not find evidence that COVID-19 hurt Trump, let alone cost him re-election. Specifically, we do not find any negative and statistically significant point estimate for the effect of COVID-19 on the 2016-2020 change in Trump's vote share. Indeed, all of our COVID-19 measures are *positively* correlated with this change in Trump's vote share. And, although not always statistically significant, our regressions generally show a positive effect of COVID-19 on the change in Trump's vote share. One explanation is voters perceived Trump

[2] As of late November, The Wall Street Journal document nursing homes account for nearly 40% of US deaths (Kamp and Mathews (2020)) and USA Today document over 40,000 cases and 200 deaths among meat-packing workers (Chadde et al. (2020)).

as better at dealing with a COVID-ravaged economy.[3] However, our preferred interpretation is the absence of an effect given the varied results across specifications.

Our results regarding health insurance coverage expansion are robust and imply a crucial role in explaining Trump's loss. Interpreting this as proxying for the magnitude of voter anxiety over the ACA's fragile judicial and legislative existence, our results imply Trump would have won Georgia, Arizona, and Nevada in the absence of undermining the ACA. And, he would have only lost Wisconsin by a few thousand votes. This would have put him on the precipice of re-election, only needing one more state (e.g.Wisconsin) for re-election.

Overall, the trade war plays a limited role in the election outcome. Our IV results say foreign trade war tariffs and US agricultural subsidies have no effect on the 2016-2020 change in Trump's vote share. However, we find a robust positive effect of US trade war tariffs. While economic significance is modest, our results imply absence of US trade war tariffs would have pushed Georgia and Wisconsin out of recount territory and, hence, would have mattered with a slightly tighter election.

The two most closely related papers to ours are Baccini et al. (2020) and Blanchard et al. (2019). Baccini et al. (2020) is the only other paper we know that investigates how COVID-19 impacted the 2020 US Presidential election. Using the meat-packing worker instrument described above, their results imply Trump would have won re-election if COVID-19 cases had been 5% lower. However, we find no statistically significant impact of COVID-19 cases using the same instrument. Moreover, we find a statistically significant *positive* effect for Trump of COVID-19 cases when using our nursing home instrument. While the exclusion restriction appears reasonable for both instruments, the starkly different results highlight the difficulty of addressing COVID-19 endogeneity. An important difference between our analysis and Baccini et al. (2020) is that our sample has over 400 additional counties. At least in part, this stems from substantial portions of David Leip's Election Atlas county-level voting data being released in late-November after Baccini et al. (2020) completed their analysis.

Ours is the first paper we know that analyzes how health insurance expansion or the trade war impacted the 2020 US Presidential election. But, Blanchard et al. (2019) analyze these issues and the 2018 US congressional midterm elections.[4,5] Despite a similar theme, we argue the economic significance of our results about health insurance expansion is up to an order

[3] Despite a late-shift towards Biden, polls generally showed the economy as a clear issue advantage for Trump (see Burns and Martin (2020) in The New York Times).

[4] Additional recent papers on the trade war include Amiti et al. (2019), Cavallo et al. (2019), Fajgelbaum et al. (2020) and Handley et al. (2020).

[5] In the empirical political economy of trade policy literature, recent papers have looked at the electoral implications of the "China shock" (e.g. Che et al. (2016) and Autor et al. (2020)) and the determinants of legislative voting behavior on trade policy (e.g. Conconi et al. (2012, 2014) and Lake and Millimet (2016)).

of magnitude larger than Blanchard et al. (2019). They say the health insurance expansion issue accounts for half of the Democrat House majority following the 2018 midterms whereas our results say it essentially cost Trump the 2020 Presidential election. Moreover, the media proclaimed health insurance as a crucial issue before and after the 2018 midterms (Lowrey (2018), Scott (2018)). However, our results show its continued importance in the 2020 Presidential election despite the writing on the wall and other emergent issues including COVID-19, Trump's impeachment, and the Supreme Court.

In terms of the trade war, a self-acknowledged limitation of Blanchard et al. (2019) is that the trade war is likely endogenous. A contribution of our analysis to the political economy of trade policy literature is our use of the Lewbel (2012) heteroskedasticity-based IV approach. Indeed, we show these endogeneity concerns are well founded: only the statistical significance of US trade war tariffs survives this IV approach. This contrasts with Blanchard et al. (2019) where only foreign trade war tariffs are statistically significant.

2 Empirical model

Letting c index counties, our analysis revolves around the following specification:

$$\triangle V_c^{2020} = \beta_0 \triangle V_c^{2016} + \beta_1 COVID_c + \beta_2 \triangle HI_c + TW_c \beta_3 + X_c \beta_4 + \delta_s + \varepsilon_c. \tag{1}$$

$\triangle V_c^y$ is the change in the two-party Republican vote share between Presidential elections in year y and four years earlier. $COVID_c$ measures COVID-19 deaths or cases. $\triangle HI_c$ measures health insurance coverage expansion either side of the ACA. TW_c is a vector of trade war variables. X_c includes all other covariates. δ_s are state fixed effects. Following earlier literature (e.g. Autor et al. (2020), Blanchard et al. (2019)), we weight by total votes cast in the 2020 Presidential election and cluster standard errors by state.

We estimate (1) using OLS and IV specifications. While we use traditional IV estimation when instrumenting $COVID_c$, we use Lewbel (2012) heteroskedasticity-based IV estimation when instrumenting TW_c and $\triangle HI_c$ given the lack of obvious instruments.

The Lewbel approach "first-stage" regresses an endogenous variable r on the exogenous controls $\tilde{X} = [X \ \delta]$ from (1). For a subset of exogenous controls $Z_r \subseteq \tilde{X}$, he shows the model is identified assuming that $cov\left[Z_r, u_r^2\right] \neq 0$ and $cov\left[Z_r, \varepsilon u_r\right] = 0$. That is, Z_r is uncorrelated with the product of the first- and second-stage errors but Z_r drives heteroskedasticity of the first-stage errors. Lewbel shows these assumptions hold in, among others, situations with classical measurement error of the endogenous variable or situations with an unobserved common factor driving correlation between the first-stage and second-stage errors (e.g. local political activism in our context). Given the assumptions, $\tilde{Z}_r \equiv \left(Z_r - \bar{Z}_r\right) \hat{u}_r$ are valid instruments for the endogenous variable r (i.e. the sample-demeaned Z_r interacted with the first-stage residuals) when estimating (1) with standard IV techniques.[6]

Lewbel's approach allows the usual IV specification tests. This includes weak instrument and, when Z_r contains more than one variable, overidentification tests. Intuitively, strength of the instruments depends on heteroskedasticity of the first-stage errors. Thus, we use the Koenker (1981) Breusch-Pagan test for heteroskedasticity to identify $Z_r \subseteq \tilde{X}_c$ that are significantly related to the first-stage error variances.

3 Data

3.1 Voting data

We collect county-level voting data for the 2012, 2016 and 2020 US Presidential elections from David Leip's Election Atlas.[7] Reflecting Trump's 2016 triumph versus his 2020 demise, the mean change in the Republican vote share between the 2016 and 2020 elections, $\triangle V_c^{2020}$, is -0.55% points but the mean of $\triangle V_c^{2016}$ is 5.88% points (Table A1 in the online appendix contains all summary statistics). Panels A-B of Figure 1 show the geographic distributions of these variables. Although positively correlated, they differ notably.

3.2 COVID-19 variables and controls

Deaths and cases. Our COVID-19 data comes from COVID County Data which has merged with Covid Act Now. They obtain data from various sources with county-level dashboards most preferred.[8] Our baseline measure of COVID-19 prevalence is cumulative deaths per 10,000 population from January 1 to October 31, 2020. However, Section 4.2 explores cases and deaths in three time windows: (i) cumulative from January 1 to October 31, 2020, (ii) October daily average, and (iii) daily average in the county-specific window with the highest 14-day average.[9] Panels C-D of Figure 1 show the geographic incidence of COVID-19 cumulative deaths (mean 5.72 per 10,000 population) and cases (mean 28.29 per 1000 population) through October 31, 2020. While deaths are relatively higher than cases

[6] See, e.g., Arcand et al. (2015) and Millimet and Roy (2016) for applications of the Lewbel approach.

[7] Alaska and Kalawao county in Hawaii do not report county-level votes.

[8] The ordering of sources is county dashboards, state dashboards, COVID Tracking Project, department of HHS, USA Facts, New York Times, and CovidAtlas.

[9] Positive daily outliers and negative daily counts emerge from data dumps and revisions. For daily averages of cases (deaths), we (i) replace the highest three days (one day) with the daily average over the preceding seven days and (ii) replace negative daily counts with the maximum of zero and the three-day average including the negative middle day.

in the early-hit north-east, cases are relatively higher than deaths in the later-hit Dakotas and Minnesota. Figure A1 in the online appendix illustrates all of our COVID-19 measures.

Unfortunately, county-level data on hospitalizations or tests is not widely available. However, state fixed effects control for state-level differences in testing regimes.

Controls. Voting behavior and COVID-19 prevalence could be correlated with county-level social distancing and economic activity. To control for social distancing, we use the Mobility and Engagement Index (MEI) from the Federal Reserve Bank of Dallas (Atkinson et al. (2020)). The index varies daily based around cell phone data from SafeGraph. It is an inverse measure of social distancing, normalized so the nationwide daily average is 0 for January and February and -100 in the second week of April. We control for the daily average MEI using the time window that matches our measure of COVID-19. See Figure A2 in the online appendix for illustrations of the MEI data.

To control for economic activity, we use two county-level measures (see Figure A2 in the online appendix for illustrations of these data). First, we use the county-level change in the unemployment rate from October 2019 to October 2020 from the BLS Local Area Unemployment Statistics. Second, we collect monthly store-level visits from SafeGraph based on cell phone location data. We aggregate this business foot traffic data to the county-level and compute the growth in the number of visits between the period January-February 2020 and the period March-October 2020. To account for county-specific seasonality, our baseline control variable is this 2020 growth relative to the analogous growth in 2019. For deaths or cases in October (or the "peak" time windows), we adjust this measure so that growth in visits for 2020 or 2019 is just October (or the weighted average of months in the "peak" window) relative to January-February.

Relying heavily on Desmet and Wacziarg (2020), we control for broader correlates of county-level COVID-19 prevalence. First, using 5-year ACS samples, we control for the 2016 level and the change between 2012 and 2016 of measures related to ethnicity, poverty and density: population shares of (i) people where English is not spoken at home, (ii) foreign born people, (iii) naturalized citizens, and (iv) people living in poverty; population; share of multi-unit housing structures; and, the share of workers who commute by public transport. Additional density measures include effective density (Desmet and Wacziarg (2020)) and indicators for large metros, small and medium metros, and non-metros.[10] Second, given the importance of pre-existing conditions for COVID-19, we control for county-level health characteristics from Chetty et al. (2016): diabetes prevalence measures, 30-day mortality for

[10] Effective density differs from standard population density by using the spatial population distribution within a location.

pneumonia, 30-day mortality for heart failure, and the 30-day hospital mortality index.[11] Third, we control for social capital (Rupasingha et al. (2006)). Fourth, moving beyond Desmet and Wacziarg (2020), we control for the share of county employment that can work remotely.[12]

Instruments. Our county-level COVID-19 instruments are the 2016 population share of nursing home residents and the 2012-2016 employment share of meat-packing workers. Nursing home data comes from The Centers for Medicare & Medicaid Services (and population from the 5-year 2016 ACS). Following Baccini et al. (2020), we use 2012-2016 County Business Patterns (CBP) employment data to compute annual average employment of meat-packing workers (4-digit NAICS industry 3116 "Animal Slaughtering and Processing") as a share of annual average total employment.

Panels A-B of Figure 2 show the notably different geographic variation of these two instruments. 45% of counties have zero meat-packing workers and only 12% have an above-average share. But, 7% of counties have zero nursing home residents and 40% have an above-average share. Ultimately, meat-packing workers are concentrated in few counties while nursing home residents are dispersed nationwide.

3.3 Health insurance coverage expansion and trade war variables

Health insurance. ACA health exchanges became operational in January 2014. We measure health insurance coverage expansion as the change in the share of the civilian non-institutionalized population aged 19-64 years between the 2013 5-year ACS (last one completely in the pre-ACA period) and the 2018 5-year ACS (first one completely in the post-ACA period). The 3-year and 1-year ACS do not contain counties with population below 20,000 and 65,000 respectively, so the 5-year ACS maximizes county coverage.[13] Panel C of Figure 2 shows significant geographic variation around the mean expansion of 5.05% points. Numerous large counties around major cities in states that decided the 2020 Presidential election saw above-average expansion (including Georgia, Arizona and Nevada).

Trade war variables. In spring 2018, Trump began imposing tariffs on US trading partners and many partners retaliated (Table A2 in the online appendix provides background and source data). By fall 2019, the US was imposing tariffs of 10-25% on nearly 20% of US imports. With these tariffs focused on China, China was imposing tariffs of 5-35% on nearly

[11]The data can be downloaded from `https://healthinequality.org/data/`.

[12]Following Dingel and Neiman (2020), we classify whether an occupation can work remotely. To convert to county-level employment shares, we use the 5-year ACS microdata from IPUMS USA as well as a PUMA-to-county geographic concordance from the Missouri Census Data Center and an SOC occupation concordance (`https://usa.ipums.org/usa/volii/occsoc18.shtml`).

[13]See `https://www.census.gov/programs-surveys/acs/guidance/estimates.html`.

$100bn of US exports.

Closely following Blanchard et al. (2019), we construct county-level exposure to US and foreign retaliatory trade war tariffs.[14] TS_i^M and TS_i^X denote, respectively, the additional tariffs charged on US imports and US exports in 3-digit NAICS industry i. Using 2016 CBP employment data, we divide these additional tariffs by industry-level employment L_i and then aggregate across industries using county-industry employment shares $\frac{L_{ic}}{L_c}$.[15] This gives county-level exposure to, respectively, US and foreign retaliatory trade war tariffs measured in dollars per worker:

$$TS_c^{US} = \sum_i \frac{L_{ic}}{L_c} \frac{TS_i^M}{L_i}$$

$$TS_c^R = \sum_i \frac{L_{ic}}{L_c} \frac{TS_i^X}{L_i}.$$

Panels D-E of Figure 2 show exposure to US trade war tariffs (mean of $1030 worker) is concentrated around the Great Lakes and parts of the south while exposure to foreign retaliation (mean of $550 per worker) is concentrated along the Mississippi River, the lower Midwest and the Pacific North-West.

The Trump Administration implemented the Market Facilitation Program of agricultural subsidies in 2018 to help US farmers hurt by foreign retaliatory tariffs. We use county-level estimated payments from Blanchard et al. (2019) (mean of $430 per worker). Panel F of Figure 2 shows these are heavily concentrated in the central and upper Midwest and along the Mississippi River. Thus, they are only somewhat loosely related to exposure of foreign retaliation.

3.4 Other controls

In addition to the COVID-19 controls described in Section 3.2, we use a typical set of economic and demographic variables (using 5-year ACS data) to control for factors plausibly affecting voting preferences and health insurance coverage expansion or the trade war variables. First, we control for the 2013 pre-ACA level of health insurance coverage. Second, we control for the 2016 distributions, and the changes between 2012 and 2016 distributions, of age (six bins), household income (seven bins; and median household income), education (four bins), and race (five racial groups). Fourth, in 2016 levels and changes between 2012 and

[14] See online appendix A for more details.

[15] As Blanchard et al. (2019) explain in their Appendix A1, county-level CBP employment data is often a "flagged" range. We follow their interpolation method, replacing flagged employment ranges with imputed employment levels.

2016 levels, we control for industrial composition (shares of employment in manufacturing as well as agriculture and mining) and labor market tightness (population shares aged 16-plus that are unemployed and not in the labor force).

4 Results

4.1 Baseline results

Table 1 presents the baseline results. Column (1) only controls for the Republican vote share change between 2012 and 2016, $\triangle V_c^{2016}$. But, it shows a *positive* and statistically significant county-level relationship between cumulative COVID-19 deaths and the Republican vote share change between 2016 and 2020. Indeed, this column (1) point estimate is identical to that when not controlling for $\triangle V_c^{2016}$. Thus, Trump improved on his 2016 performance in counties with higher COVID-19 deaths. This relationship persists in column (2) when adding the social mobility (MEI) and economic activity controls (business foot traffic and the unemployment rate change).[16] But, it disappears when adding COVID controls (column (3)) and state fixed effects and non-COVID controls (column (4)).[17] The small and statistically insignificant point estimate in column (4) is essentially unchanged when introducing health insurance coverage expansion and the trade war variables in columns (5) and (6). Ultimately, these results provide little evidence for COVID-19 affecting the election results.

Columns (5)-(6) explore the election impact of non-COVID factors. These show the importance of health insurance coverage expansion. The estimate is somewhat more precise after controling for the trade war variables in column (6) with the $p-$value reducing from 0.104 to 0.090. The negative point estimate says Trump's vote share declined more from 2016 levels in counties experiencing stronger post-ACA health insurance coverage expansion. A natural interpretation is that expansion captures the extent of fears over Republican-led efforts to undermine and repeal the ACA.

Typical political economy of trade policy says US (foreign) trade war tariffs benefit (hurt) US producers of such goods. Similarly, US agricultural subsidies benefit US agricultural producers. Column (6) shows each trade war variable is statistically significant and reflects these forces: Trump's vote share increases (decreases) from 2016 levels in countries more exposed to US trade war tariffs and agricultural subsides (foreign trade war tariffs).

Some of the effects described above are economically significant. The point estimates from column (6) of Table 1 imply the mean county saw Trump's vote share increase by

[16] 106 observations are missing MEI data.

[17] 14 counties are missing COVID-controls data. Using state fixed effects loses an observation because Washington D.C. has no counties.

0.02%, 0.19% and 0.22% points respectively on account of COVID-19 deaths, US trade war tariffs and agricultural subsidies but decrease by 0.40% points and 0.11% points respectively on account of health insurance coverage expansion and foreign trade war tariffs. However, the effect for a mean county is potentially misleading regarding the impact on state-level electoral college outcomes. For example, the mean county effect understates the state-level electoral college impact of health insurance coverage expansion if large counties had the largest expansions of health insurance coverage.

Table 2 takes these county-level differences into account and illustrates economic significance in terms of state-level electoral college impact. For any variable of interest from Table 1, we use its county-specific value and its column (6) point estimate to compute counterfactual county-level vote shares for Trump and Biden in the absence of this variable. By construction, the mean counterfactual county-level effect matches that in the previous paragraph. At the county-level, multiplying counterfactual vote shares by total votes gives counterfactual vote tallies. Aggregating to state-level total votes, the implied state-level change in Trump's vote share could be more or less than the mean county change. Moreover, since a vote share increase for one candidate implies an equivalent vote share decrease for the other candidate, eliminating a winning candidates' vote share margin requires an offsetting impact of half this margin.

The key takeaway from panel A of Table 2 is that health insurance coverage expansion is easily the most economically significant variable and the effect of COVID-19 is essentially zero. Comparing columns (1)-(2), Trump's state-level vote share in the key states that decided the election does not move by more than 0.05% points in the absence of COVID-19. Conversely, removing the impact of health insurance coverage expansion in column (3) moves the Georgia and Arizona vote share margins in Trump's favor by 0.80-0.95% points. Rather than losing Georgia and Arizona by 0.24% points and 0.31% points respectively, Trump wins by 0.57% and 0.62% points. Additionally, Trump only loses Wisconsin by 0.13% points instead of the actual 0.64% points. With Georgia and Arizona's electoral college votes, Trump is only a few thousand votes in Wisconsin plus another one electoral college vote away from re-election.

Columns (4)-(6) of panel A in Table 2 illustrate economic significance of the trade war variables. Reflecting the narrow set of counties benefiting from agricultural subsidies, Trump's state-level vote share changes by no more than 0.06% points. Despite affecting more counties, removing the effects of foreign trade war tariffs changes Trump's state-level vote share by no more than 0.14% points. However, removing the effects of US tariffs roughly doubles Trump's loss both in Georgia to 0.50% points and, larger in absolute magnitude than the effect of health insurance coverage expansion, in Wisconsin to 1.19% points. This would prevent recounts in both states and could have swung the state electoral college outcomes if the election was only slighter tighter.

Columns (7)-(11) of Table 1 explore whether voting behavior is heterogeneous according to county-level competitiveness or partisanship. Columns (7)-(9) look at competitiveness: closely following Autor et al. (2020), competitive counties have a two-party Republican Presidential vote share between 45% and 55% in 2012 and 2016, but solidly Republican (Democrat) counties have vote shares above 55% (below 45%) in 2012 and 2016. While the point estimates are notably larger for solidly Democrat counties (column (8)) than other counties and the full sample, panel B of Table 2 shows these heterogeneities generally do not alter the economic significance of the effects on electoral college outcomes from panel A.

The major exception is Arizona because competitive counties account for over 80% of their 2020 votes. Thus, the effect of health insurance coverage expansion is notably smaller than the overall sample; Trump only wins by 0.02% points in panel B of Table 2 rather than 0.62% points in panel A. Additionally, although statistically insignificant ($p = 0.17$), the negative point estimate for COVID-19 deaths in competitive counties produces a counterfactual where Trump actually wins Arizona by 0.09% points in the absence of COVID-19. Moreover, driven by the statistically significant and positive COVID-19 deaths point estimate in solidly Democratic counties, panel B shows the effect of COVID-19 is similar to or stronger than the effect of US tariffs in Georgia, Pennsylvania and Nevada. Nevertheless, overall, the competitiveness of counties does not dramatically affect how the various issues impact electoral college outcomes.

Columns (10)-(11) of Table 1 explore heterogeneity in terms of partisanship as proxied by whether the county voted for Trump (column (10)) or Hillary Clinton (column (11)) in 2016. The effects are notably stronger in Clinton counties and this increases the economic significance of health insurance coverage expansion substantially. Absent the effects of health insurance coverage expansion, column (3) of panel C in Table 2 shows Trump's counterfactual winning margin in Georgia increases to 0.90% points and he now *wins* Nevada by 0.49% points. More than 1.3 million votes were cast in Nevada's largest two counties, Clarke and Washoe, which Clinton won in 2016 and experienced an expansion of health insurance coverage around twice the national average. More than 1.7 million votes were cast in the Atlanta suburb counties of Fulton, Gwinnett, Colb and DeKalb that Clinton won and experienced health insurance coverage expand more than the national average. Emphasizing the salience of health insurance coverage expansion, these counterfactual results say a 0.07% point loss in Wisconsin, less than 2500 votes, is all that prevents Trump's re-election.

4.2 Robustness

Alternative COVID-19 measures. Our analysis has focused on cumulative COVID-19 deaths. Panel A of Table 3 explores other measures of COVID-19 cases and deaths (Table A3 in the online appendix presents the full results from Panels A-C in Table 3). Only COVID-19 October deaths yield statistical significance at conventional levels, but the implied effect for the mean county only increases Trump's vote share by 0.08% points and is hence relatively small. Thus, alternative measures of COVID-19 do not alter the baseline result of little evidence for COVID-19 impacting the election outcome.

IV. Table 1 shows COVID-19 prevalence, health insurance coverage expansion, and the trade war variables are essentially uncorrelated. The COVID-19 point estimate from column (4) barely changes when adding health insurance coverage expansion and the trade war variables in columns (5)-(6). And, the health insurance coverage expansion point estimate in column (5) barely changes when adding the trade war variables in column (6). Thus, possible endogeneity concerns over, say, COVID-19 deaths are not a major concern for estimating the impacts of health insurance coverage expansion or the trade war. Nevertheless, we pursue various IV strategies.

Panels B-C of Table 3 present alternative IV strategies for COVID-19 prevalence: a county's employment share of meat-packing workers and a county's population share of nursing home residents. Conditional on our controls and fixed effects – including, among others, county-level distributions of age, income, education and race as well as county-level measures of health characteristics – the exclusion restriction says these IVs only affect voting behavior through a county's COVID-19 prevalence. Panel B shows the meat-packing instrument is generally weak. The Kleibergen-Paap weak instrument rk F-stat only exceeds 10 in column (2) for cumulative COVID-19 cases, but the point estimate is not close to statistically significant at conventional levels. Panel C shows the nursing home instrument is generally strong, but the point estimate remains positive and statistically significant in these cases. Indeed, the implied effect on Trump's vote share in the mean county ranges between 0.5% points and 2.57% points which is much larger than the baseline analysis. Nevertheless, while the exclusion restriction for the meat-packing and nursing home instruments both appear plausible, the vastly different results across the two IV approaches illustrate the difficulty of addressing potential endogeneity of COVID-19 prevalence.

Given the difficulty of finding traditional instruments for health insurance coverage expansion and the trade war variables, panel D uses the Lewbel (2012) heteroskedasticity-based IV approach. The control variables used to construct the Lewbel instruments for health insurance coverage expansion in column (1) are 2013 health insurance coverage, the percent diabetic with annual lipids test, the percent diabetic with annual hemoglobin test, and foot traffic cumulative relative growth. The control variables used to construct the Lewbel instruments for the trade war variables in column (2) are the 2016 manufacturing employment share, the 2016 share of naturalized citizens, the 2016 agricultural and mining employment share, and the MEI daily average (1/1/2020-10/31/2020). In both columns, we easily reject the null of homoskedasticity in the first stage ($p < .0001$). The Lewbel instruments also perform satisfactorily according to standard IV specification tests. In both columns, we reject the null that the model is underidentified ($p = .002$ and $p = .016$ respectively), the Kleibergen-Paap weak instrument rk F-stat exceeds 10 (48.67 and 10.95 respectively) and Hansen's J-test of overidentification fails to reject validity of the instruments ($p = 0.49$ and $p = 0.89$ respectively). Thus, the specifications in columns (1)-(2) appear well identified.

Column (1) reaffirms the crucial salience of health insurance coverage expansion. The point estimate is now statistically significant at the 5% level. And, relative to our baseline analysis, its value is closer to the larger value in Clinton counties than the smaller value in the overall sample.

Column (2) suggests that prior literature concerns about endogeneity of the trade war variables appear well founded. The US trade war tariffs point estimate remains statistically significant, although roughly one-third smaller. But, the foreign trade war tariff and the agricultural subsidies point estimates are statistically insignificant and, respectively, about 20% and 80% smaller.

Placebo specifications. One may still worry our results reflect pre-existing political trends. Thus, we pursue placebo specifications where the dependent variable is the change in Trump's vote share between the 2012 and 2016 elections and we remove the 2016-2020 change from the specification.

Table A4 in the online appendix shows the results. The point estimates are generally very imprecise, quite small, and sometimes differ in sign from the main text. This provides further evidence mitigating concerns about pre-existing political trends.

5 Conclusion

Understanding the political economy of the 2020 US Presidential election is important given the controversy surrounding the outcome itself and issues underlying voter decisions. Perhaps the most common narrative explaining Trump's loss is that he mishandled the COVID-19 pandemic. We do not find supporting evidence. If anything, our results say COVID-19 boosted Trump's vote share, perhaps because of his perceived strength on handling a post-

COVID ravaged economy.

Instead, our results suggest the issue that essentially cost Trump re-election was voter fears over rolling back expansion of health insurance coverage after the Affordable Care Act (ACA). Absent this issue, our main results imply Trump would have won Georgia, Arizona, Nevada, and would have lost Wisconsin by a few thousand votes. He would have only needed one more electoral college vote, e.g. Wisconsin, for re-election. Thus, despite writing on the wall for Republicans after the 2018 US midterm elections, their continued efforts to repeal and undermine the ACA leave health insurance coverage as a highly salient electoral issue.

When interpreting our results, one must remember that our analysis compares COVID-19 prevalence across counties. It essentially views variation of COVID-19 prevalence across counties as revealing county-level COVID-19 shocks. Thus, our analysis cannot address the impact of COVID-19 as a national shock even though voter views about Trump's handling of the pandemic may not depend on their county's COVID-19 prevalence. Nevertheless, our analysis pours cold water on the notion that voters penalized Trump more in counties subject to larger COVID-19 outbreaks.

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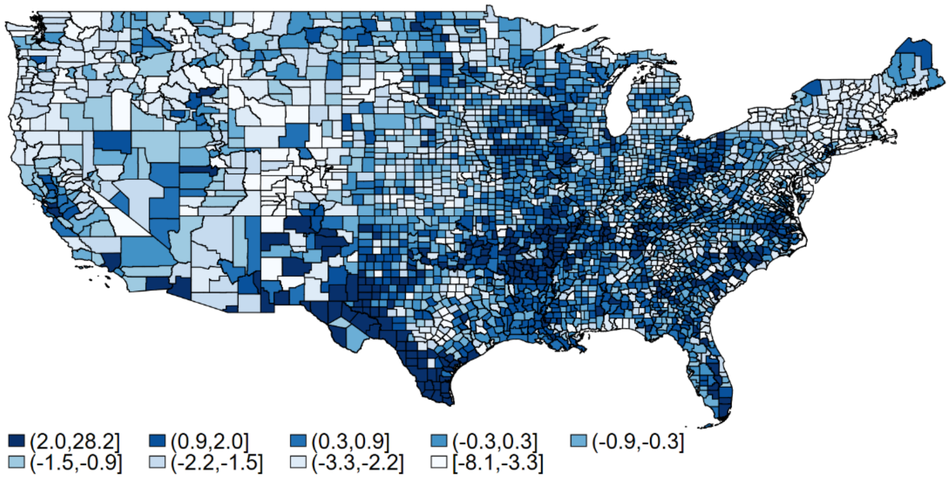
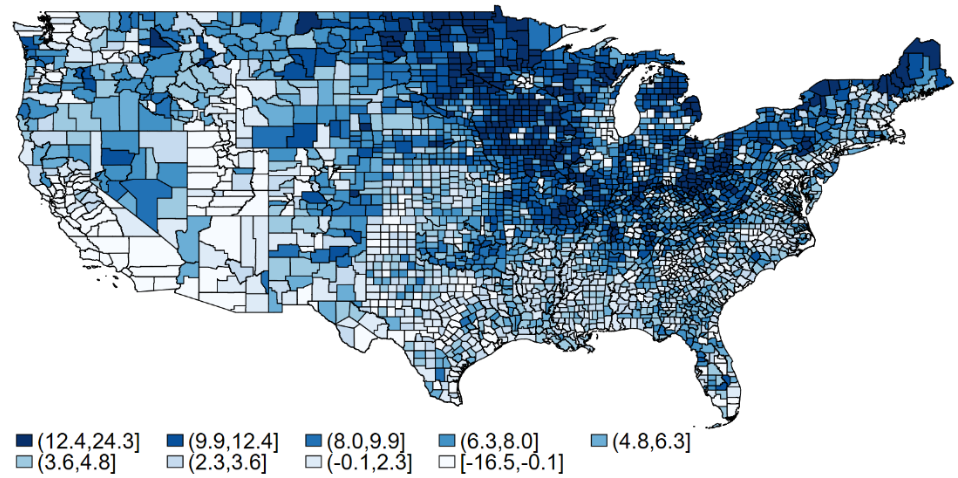
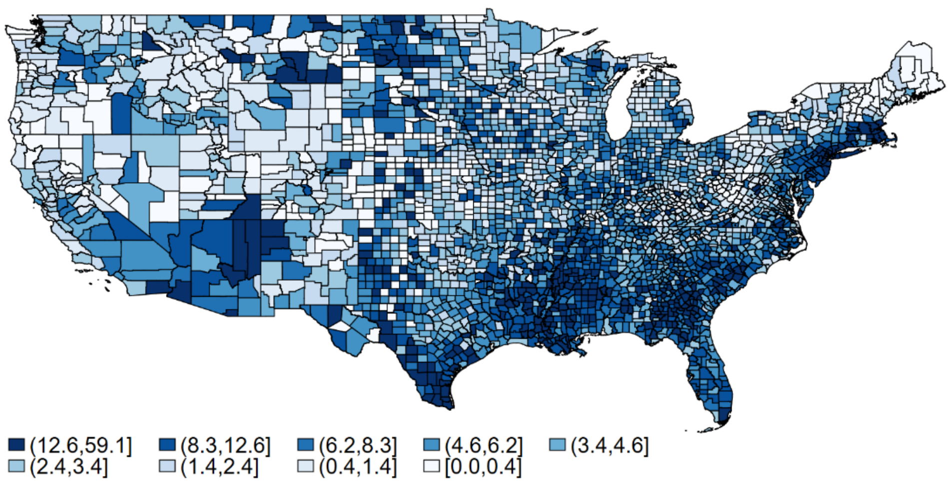


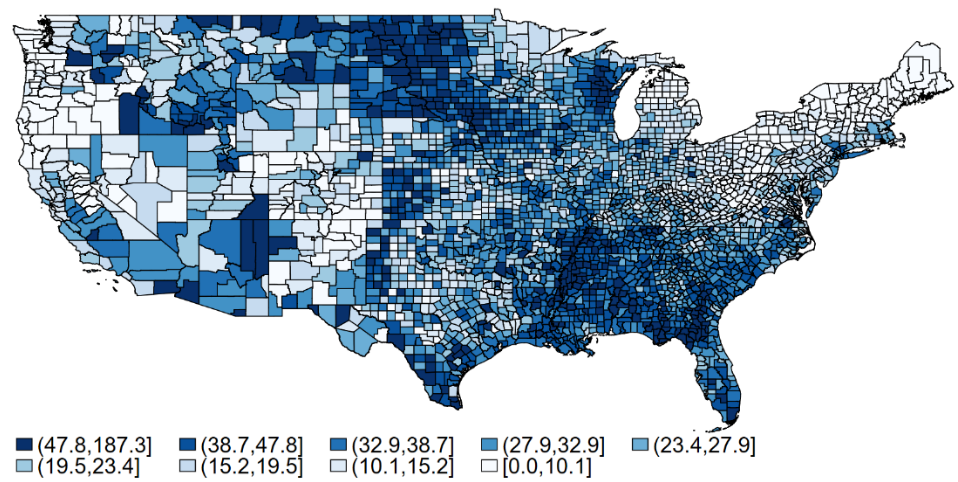
A. Change in 2-party Republican vote share 2016-2020 (% pts)



B. Change in 2-party Republican vote share 2012-2016 (% pts)



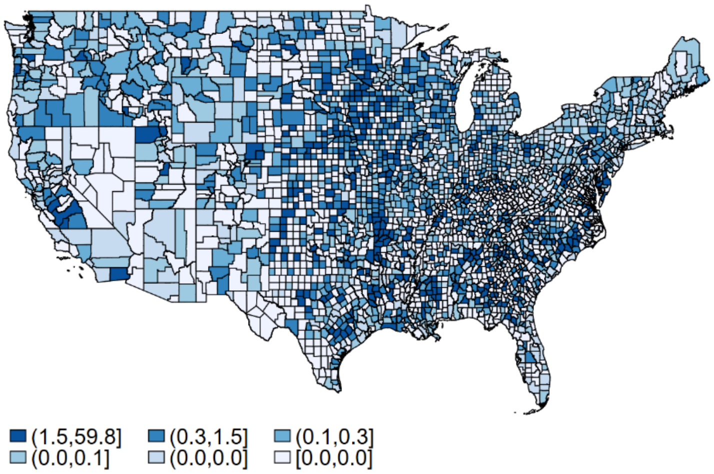
C. COVID-19 cumulative deaths (per 10,000 population)



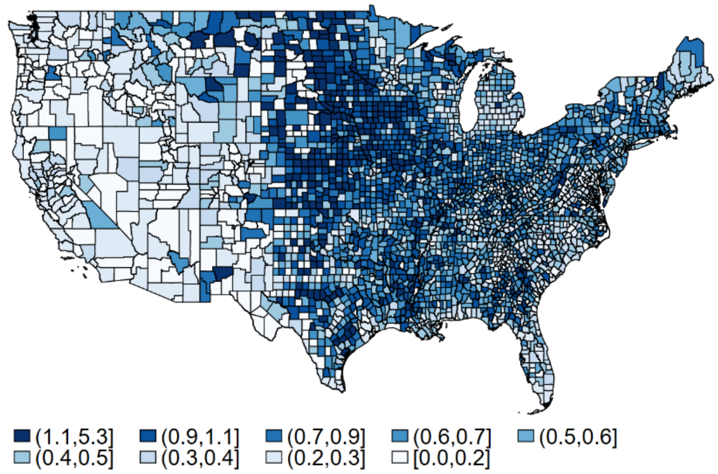
D. COVID-19 cumulative cases (per 1,000 population)

Figure 1: County-level presidential voting outcomes and COVID-19 prevalence

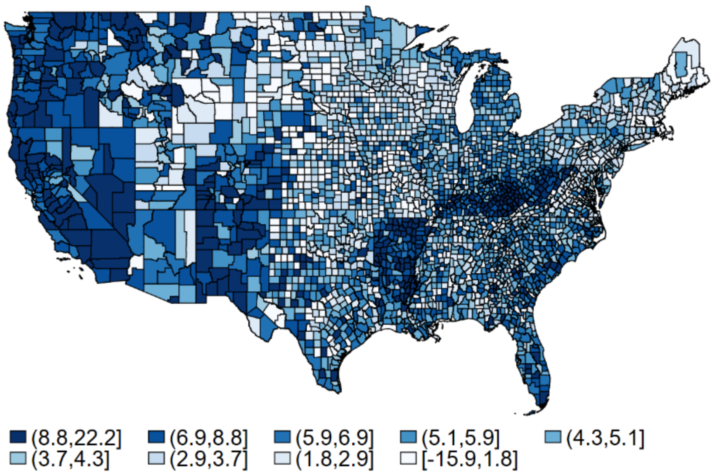
Notes: Maps represent the 3008 mainland US counties. Presidential voting data from David Leip's Election Atlas; 2020 election data is version 0.9 (official release of data for all states). Cumulative COVID-19 data is through October 31, 2020. COVID-19 data source is COVID Act Now (https://covidcountydata.org/). Population is 2018 population from 2018 5-year Census ACS. See main text for further details.



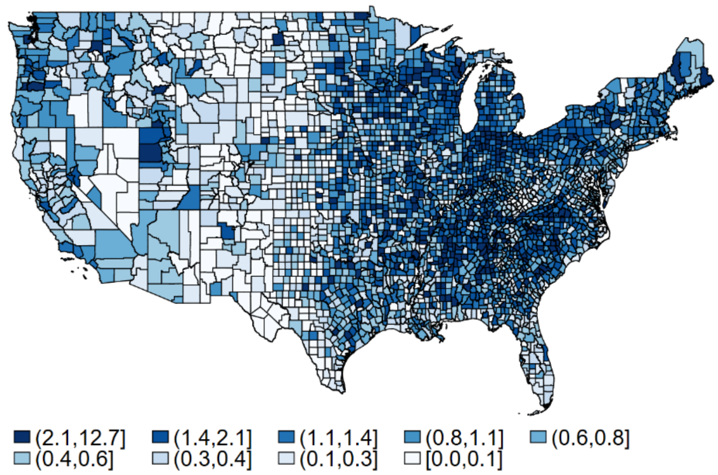
A. Meat-packing worker employment share (% pts)



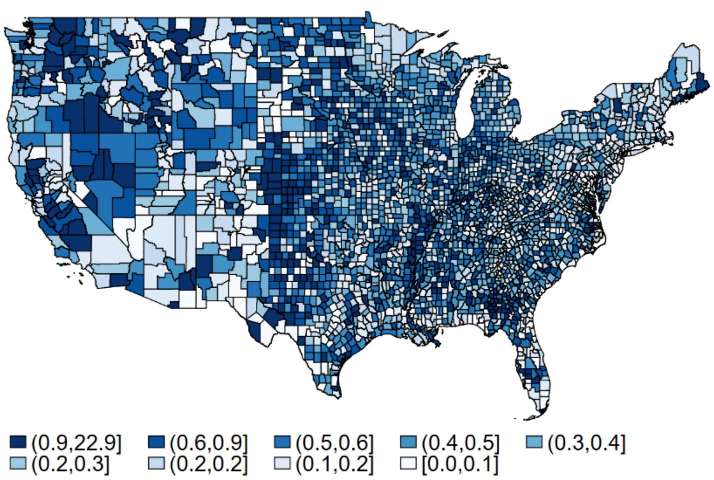
B. Nursing home resident population share (2016, % pts)



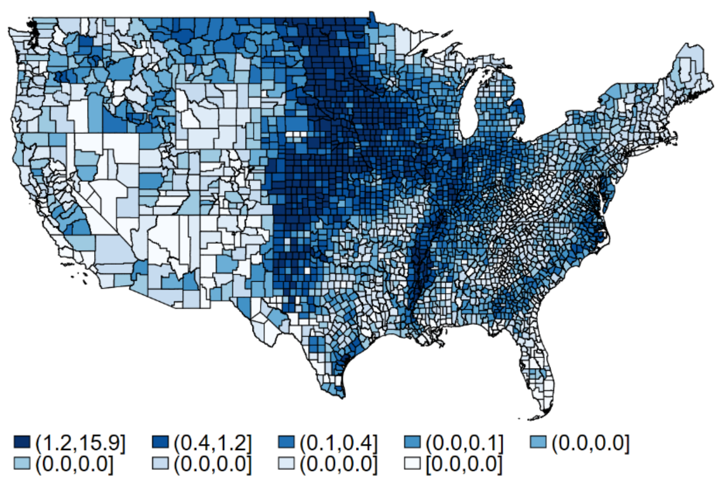
C. Health insurance coverage expansion (2013-2018, % pts)



D. US trade war tariff shock ($000s per worker)



E. Foreign retaliatory trade war tariff shock ($000s per worker)



F. Agricultural subsidies ($000s per worker)

Figure 2: COVID instruments, health insurance coverage expansion and trade war variables

Notes: Maps represent the 3008 mainland US counties. Meat packing worker employment share is the annual average over 2012-2016 using County Business Patterns data. Nursing home resident population share uses 2016 data from The Centers for Medicare & Medicaid Services and 2016 population data from 2016 5-year ACS. Health insurance coverage expansion is difference between coverage in 2018 and 2013 Census 5-year ACS. Table A2 in the online appendix describes data sources for trade war tariffs. Agricultural subsidies data from Blanchard et. al. (2019). See main text for further details.

Table 1. Baseline results

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
COVID-19 deaths (cumulative, per 10k pop)	0.169*	0.083#	-0.021	0.002	0.003	0.003	0.005	0.054^	-0.034	0.007	0.024
	(0.049)	(0.047)	(0.026)	(0.018)	(0.017)	(0.017)	(0.009)	(0.024)	(0.025)	(0.010)	(0.029)
Δ 2-party Rep. vote Share 2012-2016	0.037	0.100*	0.164^	0.222*	0.222*	0.217*	0.189*	0.298*	0.203*	0.191*	0.276*
	(0.063)	(0.037)	(0.065)	(0.032)	(0.032)	(0.032)	(0.033)	(0.071)	(0.036)	(0.026)	(0.045)
Δ Health insurance coverage					-0.080	-0.079#	-0.031	-0.124	-0.023	-0.034	-0.165#
					(0.048)	(0.046)	(0.030)	(0.076)	(0.053)	(0.030)	(0.092)
US tariff shock						0.186*	0.075#	0.602*	0.056	0.083^	0.382#
						(0.050)	(0.040)	(0.197)	(0.057)	(0.036)	(0.206)
Retalitory tariff shock						-0.193#	-0.017	-0.189	0.015	-0.057	-0.042
						(0.104)	(0.047)	(0.277)	(0.175)	(0.062)	(0.192)
Agricultural subsidies						0.501*	0.169^	1.219^	0.286	0.157	0.863#
						(0.129)	(0.081)	(0.489)	(0.299)	(0.106)	(0.465)
N	3112	3006	2992	2991	2991	2991	1981	305	694	2515	471
Social mobility & economic activity controls	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
COVID controls	N	N	Y	Y	Y	Y	Y	Y	Y	Y	Y
State FE	N	N	N	Y	Y	Y	Y	Y	Y	Y	Y
Non-COVID controls	N	N	N	Y	Y	Y	Y	Y	Y	Y	Y
Sample	All counties						Solid Republican	Solid Democrat	Competitive	Trump counties	Clinton counties

Notes: # p<0.10, ^ p<.05, * p<.01. Dependent variable is the change in the 2-party Republican vote share between the 2016 and 2020 US Presidential election. Estimation performed by fixed effects OLS. Standard errors clustered by state. See Table A1 in online appendix for list of COVID controls and non-COVID controls. 2013 level of health insurance coverage included from column (5) onwards. All specifications weighted by 2020 total Presidential votes cast. Competitive counties have 2012 and 2016 Republican 2-party Presidential vote share between 45% and 55%. Solid Republican (Democrat) counties have these vote shares above 55% (below 45%) in 2012 and 2016. Trump (Clinton) counties are counties that Trump (Clinton) won in 2016. See main text for further details.

Table 2. Counterfactual two-party vote share margin (% points)

	(1)	(2)	(3)	(4)	(5)	(6)
A. Baseline						
		Counterfactual: removing effects of …				
	Actual	COVID-19	Health insurance coverage expansion	US tariff shock	Retaliatory tariff shock	Agricultural subsidies
Nevada	-2.45	-2.48	-0.90	-2.57	-2.41	-2.45
Pennsylvania	-1.20	-1.24	-0.64	-1.53	-1.13	-1.21
Wisconsin	-0.64	-0.66	-0.13	-1.19	-0.50	-0.70
Arizona	-0.31	-0.37	0.62	-0.50	-0.25	-0.32
Georgia	-0.24	-0.28	0.57	-0.53	-0.15	-0.25
North Carolina	1.37	1.34	2.17	1.00	1.45	1.34
B. Heterogeneity by competitiveness						
		Counterfactual: removing effects of …				
	Actual	COVID-19	Health insurance coverage expansion	US tariff shock	Retaliatory tariff shock	Agricultural subsidies
Nevada	-2.45	-2.92	-0.62	-2.58	-2.44	-2.45
Pennsylvania	-1.20	-1.38	-0.75	-1.48	-1.19	-1.20
Wisconsin	-0.64	-0.65	-0.25	-1.13	-0.61	-0.68
Arizona	-0.31	0.09	0.02	-0.39	-0.31	-0.32
Georgia	-0.24	-0.41	0.40	-0.45	-0.22	-0.24
North Carolina	1.37	1.25	2.04	0.99	1.39	1.35
C. Heterogeneity by partisanship						
		Counterfactual: removing effects of …				
	Actual	COVID-19	Health insurance coverage expansion	US tariff shock	Retaliatory tariff shock	Agricultural subsidies
Nevada	-2.45	-2.72	0.49	-2.65	-2.44	-2.45
Pennsylvania	-1.20	-1.45	-0.46	-1.52	-1.18	-1.21
Wisconsin	-0.64	-0.72	-0.07	-1.08	-0.60	-0.67
Arizona	-0.31	-0.48	0.34	-0.45	-0.30	-0.32
Georgia	-0.24	-0.46	0.90	-0.49	-0.21	-0.24
North Carolina	1.37	1.25	2.33	1.04	1.39	1.35

Notes: Negative vote share margings indicate Trump loss. Each panel computes county-level predicted vote tallies for Trump and Biden using procedure described in main text and aggregates to state-level. Point estimates used are from Table 1: column (6) for Panel A, columns (7)-(9) for Panel B and columns (10)-(11) for Panel C.

Table 3. Robustness specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Alternative COVID-19 prevalence definitions (OLS)						
COVID-19	0.003	-0.007	0.299#	0.000	0.038	-0.001
	(0.017)	(0.005)	(0.164)	(0.004)	(0.057)	(0.003)
COVID-19 prevalence definition	Cumulative Deaths	Cumulative Cases	October Deaths	October Cases	Peak Deaths	Peak Cases
Panel B. Instrumenting COVID-19 prevalence - meat packing instrument						
COVID-19	-0.298	-0.040	-6.612	0.271	-1.932	-0.035
	(0.236)	(0.034)	(6.049)	(0.491)	(1.721)	(0.034)
COVID-19 prevalence definition	Cumulative Deaths	Cumulative Cases	October Deaths	October Cases	Peak Deaths	Peak Cases
Underidentification p-value	0.159	0.001	0.224	0.498	0.203	0.053
K-P weak instrument rk F-statistic	1.824	11.642	1.459	0.463	1.507	3.331
Panel C. Instrumenting COVID-19 prevalence - nursing home instrument						
COVID-19 deaths (cumulative per 10k pop)	0.101#	0.091#	2.669#	0.301	0.514#	0.064
	(0.052)	(0.052)	(1.338)	(0.422)	(0.280)	(0.040)
COVID-19 prevalence definition	Cumulative Deaths	Cumulative Cases	October Deaths	October Cases	Peak Deaths	Peak Cases
Underidentification p-value	0.000	0.003	0.001	0.413	0.000	0.010
K-P weak instrument rk F-statistic	55.542	13.199	15.426	0.615	53.495	7.280
Panel D. Instrumenting health insurance coverage expansion and trade war - Lewbel instruments						
COVID-19 deaths (cumulative per 10k pop)	-0.008	0.004				
	(0.016)	(0.017)				
Δ Health insurance coverage	-0.191^	-0.075#				
	(0.080)	(0.045)				
US tariff shock	0.178*	0.128#				
	(0.046)	(0.072)				
Retalitory tariff shock	-0.130	-0.156				
	(0.083)	(0.112)				
Agriculture subsidies	0.549*	0.093				
	(0.120)	(0.241)				
Endogenous	Health insurance coverage expansion	Trade war variables				
Underidentification p-value	0.002	0.016				
K-P weak instrument rk F-statistic	48.668	10.948				
Overidentification p-value	0.490	0.893				

Notes: Notes: # p<0.10, ^ p<.05, * p<.01. N=2991 in all specifications. Dependent variable is the change in the 2-party Republican vote share between the 2016 and 2020 US Presidential election. Estimation performed by fixed effects OLS in Panel A, IV in Panels B-C and IV-GMM in Panel D. Standard errors clustered by state. All specifications include full set of controls and fixed effects as in column (6) of Table 1, including health insurance coverage expansion and trade war variables. All specifications weighted by 2020 total Presidential votes cast. Peak deaths and cases in Panels A-C are county-level maximum 14-day rolling averages through October 31, 2020. Lewbel instruments in column (1) of Panel D created by demeaning and multiplying the following variables by the first stage residuals: 2013 health insurance coverage, percent diabetic with annual lipids test, percent diabetic with annual hemoglobin test, and foot traffic cumulative relative growth. Lewbel instruments in column (2) of Panel D created by demeaning and multiplying the following variables by the first stage residuals: 2016 manufacturing share of employment, 2016 population share of naturalized citizens, 2016 agricuture and mining share of employment, and the MEI daily average (1/1/2020-10/31/2020). See main text for further details.

Online Appendix
"Did COVID-19 cost Trump the election?"

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A County-level exposure to trade war tariffs

We closely follow Blanchard et al. (2019) in constructing county-level exposure to US trade war tariffs and foreign retaliatory trade war tariffs. Table A2 lists background information and source data for these tariffs.

We start with 2017 pre-trade war bilateral trade data between the US and the rest of the world. With 8-digit HS US import data and 6-digit HS US export data from the USITC, we multiply these bilateral trade flows by the relevant bilateral trade war tariff (i.e. US tariffs on US imports and foreign country tariffs on US exports). Following Blanchard et al. (2019), we focus on retaliatory tariffs by the four major US trade partners: China, Canada, Mexico and the EU. TS_h^m is the resulting additional tariffs charged on US imports form country m of HS8 product h and TS_h^x is the resulting additional tariffs charged on US exports to country x of HS6 product h. After aggregating these partner-product specific tariff shocks across US trade partners, we concord to NAICS 3-digit industries using the Feenstra et al. (2002) trade weights over the period 2002-2006. For each 3-digit NAICS industry i, this gives the total additional tariffs charged on US exports and US imports and are denoted by TS_i^X and TS_i^M respectively.

To aggregate these industry-level tariff shocks to county-level tariff shocks, we first divide by US employment in a given 3-digit NAICS industry to convert into a per worker measure using 2016 employment data from the County Business Patterns (CBP). Second, we aggregate across 3-digit NAICS industries using the CBP county-level composition of employment. As described by Blanchard et al. (2019) in their Appendix A1, county-level CBP employment data is often given by a "flagged" range rather than an actual number.

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Thus, we follow their interpolation method to replace the flagged employment range with an imputed employment level. Denoting employment by L, the US tariff shock and the foreign retaliatory tariff shock faced by county c due to the trade war are

$$TS_c^{US} = \sum_i \frac{L_{ic}}{L_c}\frac{TS_i^M}{L_i}$$
$$TS_c^{R} = \sum_i \frac{L_{ic}}{L_c}\frac{TS_i^X}{L_i}.$$

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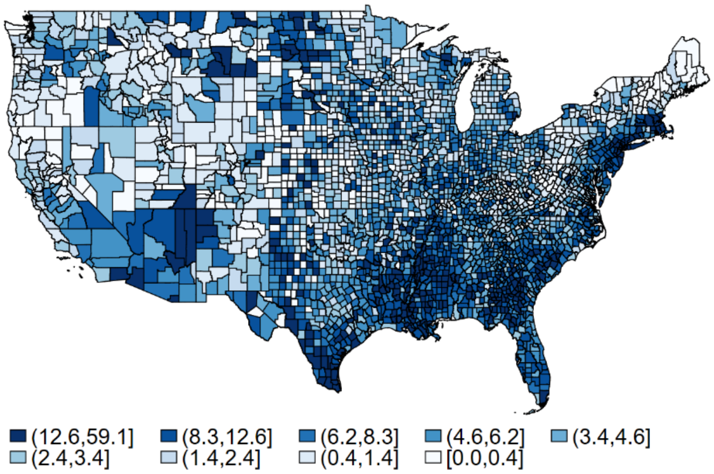
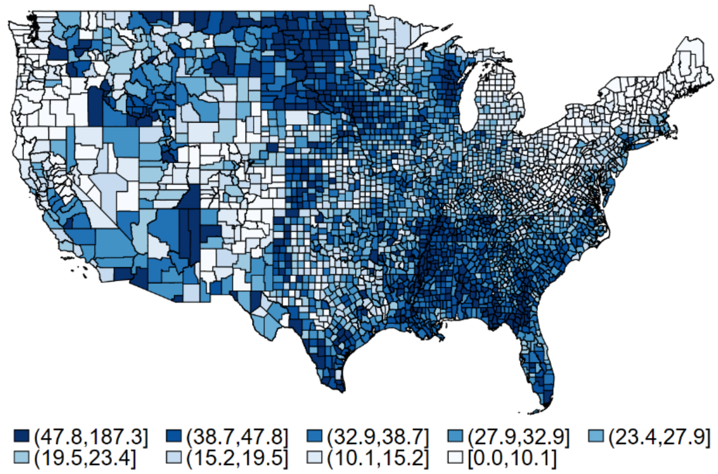
U.S. International Trade Commission, 2017a. Crystalline silicon photovoltaic cells (whether or not partially or fully assembled into other products): Investigation no. ta-201-75.

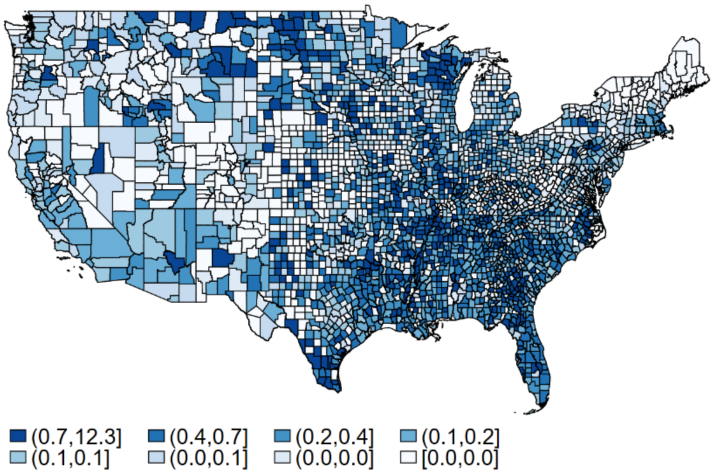
U.S. International Trade Commission, 2017b. Large residential washers: Investigation no. ta-201-076.



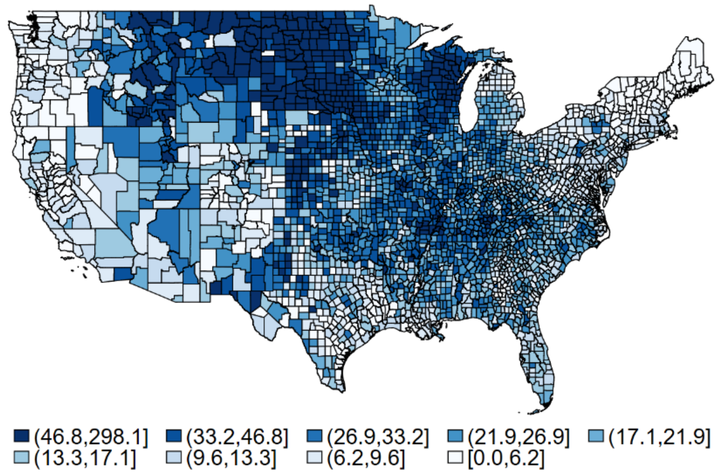
A. COVID-19 cumulative deaths (per 10,000 population)



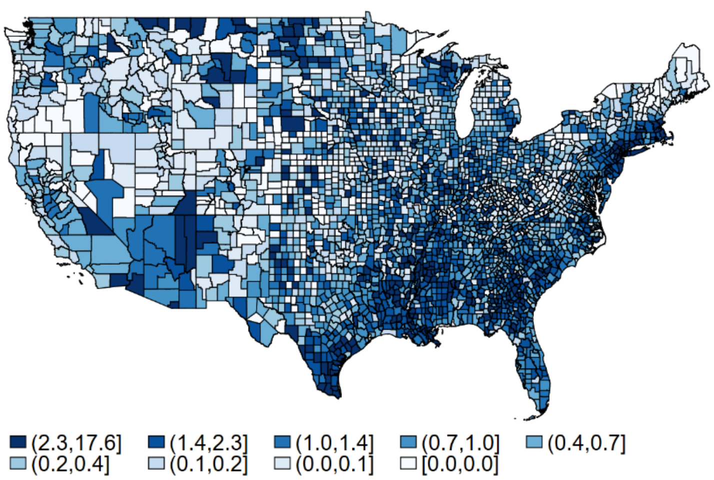
B. COVID-19 cumulative cases (per 1000 population)



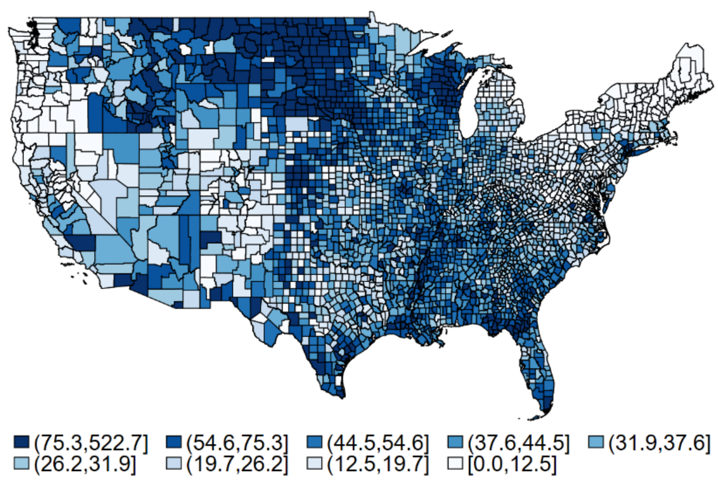
C. COVID-19 October deaths (daily average per 100,000 pop.)



D. COVID-19 October cases (daily average per 100,000 pop.)



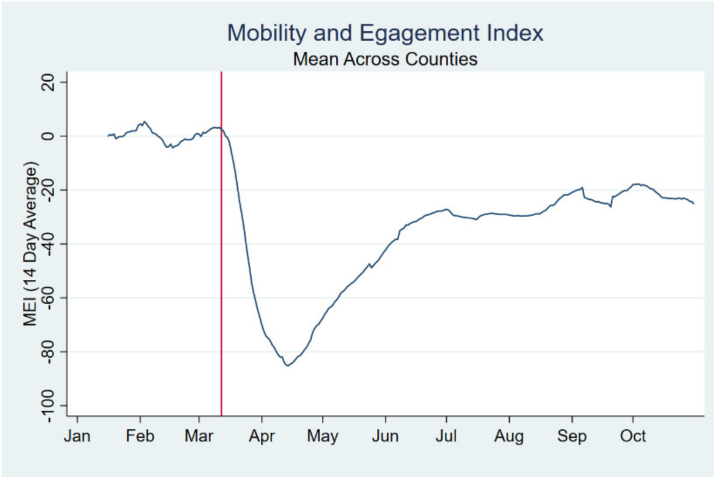
E. COVID-19 deaths (max 14-day average, per 100,000 pop.)



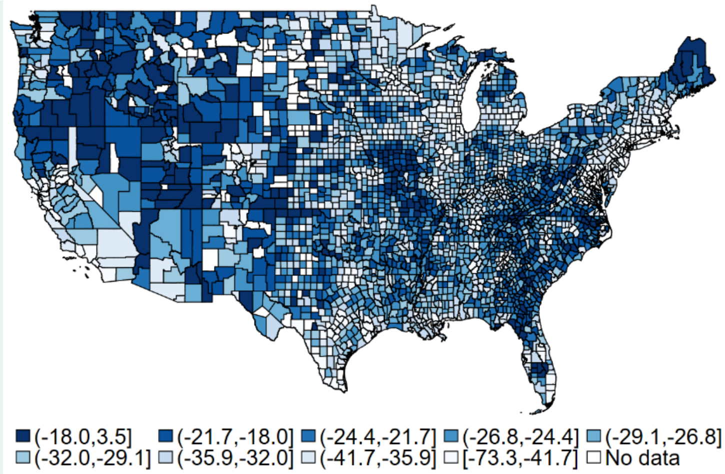
F. COVID-19 cases (max 14-day average, per 100,000 pop.)

Figure A1: Alternative measures of COVID-19 prevalence

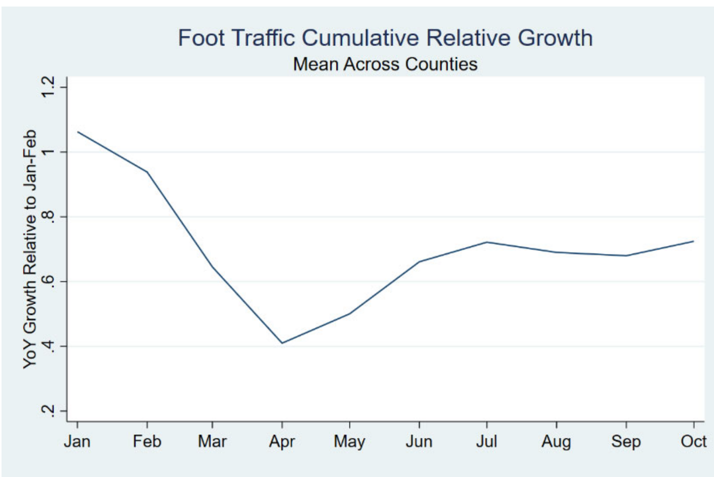
Notes: Maps represent the 3008 mainland US counties. COVID-19 data source is COVID Act Now (https://covidcountydata.org/). Population is 2018 population from 2018 5-year Census ACS. Panels A-B cumulative data is through October 31, 2020. Panels E-F are county-level maximum 14-day rolling averages through October 31, 2020. See main text for further details.



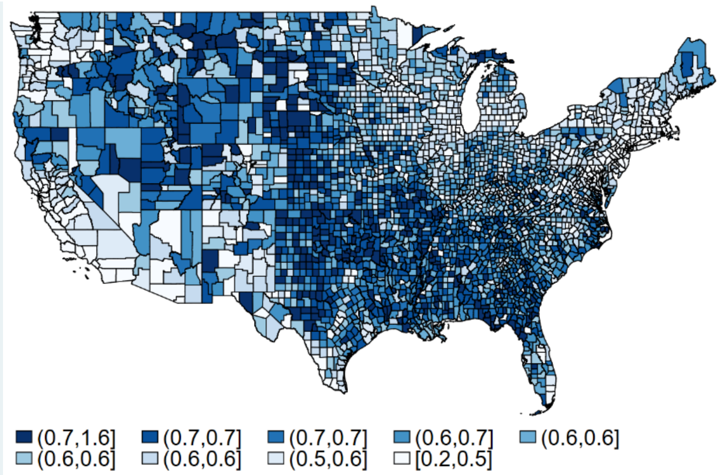
A. Daily MEI: 1/1/2020-10/31/2020



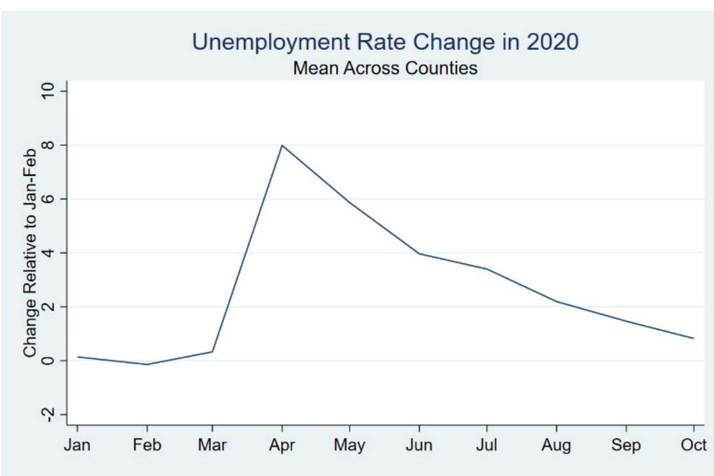
B. MEI daily average (1/1/2020-10/31/2020)



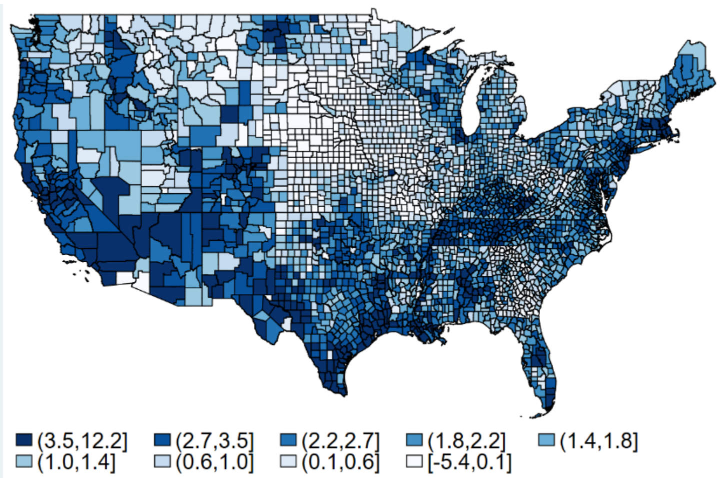
C. Foot traffic relative growth



D. Foot traffic cumulative relative growth



E. Change in unemployment rate



F. Unemployment rate change: October 2020 vs October 2019

Figure A2: Social distancing and economic activity controls

Notes: Maps represent the 3008 mainland US counties. MEI data from Federal Reserve Bank of Dallas (Atkinson et. al. 2020). Foot traffic data from SafeGraph. Unemployment rate data from BLS Local Area Unemployment Statistics. Red line in Panel A is date of National Emergency Declaration. Panel C shows 2020 foot traffic growth between January-February average and given later month, normalized relative to this same growth in 2019. Panel E shows the county mean of the change in unemployment rate between the January-February average and a given later month. See main text for more details.

Table A1. Summary statistics

	Mean	SD	Min	Max	N
Voting variables					
Change in 2-party Rep. Pres. Vote share (2016 to 2020)	-0.55	2.58	-8.08	28.16	3,112
Change in 2-party Rep. Pres. Vote share (2012 to 2016)	5.88	5.21	-16.52	24.29	3,112
COVID-19 variables					
Deaths cumulative (per 10k pop, through 10/31/2020)	5.72	6.01	0.00	59.14	3,112
Cases cumulative (per 1k pop, through 10/31/2020)	28.29	17.35	0.00	187.30	3,112
Deaths October (per 100k pop, per day)	0.28	0.55	0.00	12.26	3,112
Cases October (per 100k pop, per day)	24.73	21.65	0.00	298.09	3,112
Deaths (per 100k, max 14-day rolling daily average)	0.97	1.34	0.00	17.60	3,112
Cases (per 100k, max 14-day rolling daily average)	41.71	33.39	0.00	522.72	3,112
Unemployment rate change (Oct. 2019 to Oct. 2020)	1.77	1.62	-5.40	19.50	3,112
MEI daily average (1/1/2020 - 10/31/2020)	-29.28	10.50	-73.34	3.52	3,006
MEI October daily average (10/1/2020 - 10/31/2020)	-23.01	14.59	-79.74	31.08	3,006
MEI daily average over max 14-day death window	-30.18	27.05	-152.66	37.75	3,006
MEI daily average over max 14-day case window	-30.66	22.09	-162.99	24.55	3,006
Foot traffic cumulative relative growth	0.62	0.09	0.19	1.60	3,112
Foot traffic October relative growth	0.72	0.15	0.25	2.61	3,112
Foot traffic relative growth - max 14-day death window	0.66	0.18	0.14	2.61	3,112
Foot traffic relative growth - max 14-day case window	0.69	0.15	0.14	2.18	3,112
COVID-19 instruments					
Meat packing workers (employment share 2012-2016)	1.27	5.05	0.00	59.81	3,112
Nursing home residents (2016 population share)	0.64	0.47	0.00	5.28	3,112
Health insurance variables					
Change in health insurance coverage (2013 to 2018)	5.05	3.28	-15.90	22.20	3,112
Helath insurance coverage (2013)	84.95	5.59	52.70	97.60	3,112
Trade war variables					
US tariff shock ($000's per worker)	1.03	1.19	0.00	12.75	3,112
Retaliatory tariff shock ($000's per worker)	0.55	1.10	0.00	22.86	3,112
Agricultural subsidies ($000's per worker)	0.43	1.08	0.00	15.93	3,112
COVID-19 controls					
Population (2016)	102,128	326,630	76	10,100,000	3,112
Metro size: large (2013)	0.14	0.35	0.00	1.00	3,112
Metro size: medium or small (2013)	0.23	0.42	0.00	1.00	3,112
Share of multi-unit housing structures (2016)	12.54	9.29	0.00	98.26	3,112
Public transport commuters (2016, share of emp)	0.95	3.10	0.00	61.80	3,112
Effective population density	403.84	719.47	3.46	22,647	3,112
Foreign language at home (2016 pop share, age 5+)	9.29	11.61	0.00	96.10	3,112
Foreign born (2016 pop share)	4.62	5.63	0.00	52.20	3,112
Naturalized citizens (2016 pop share)	42.97	18.89	0.00	100.00	3,112

Table A1 (cont.). Summary statistics for main variables

	Mean	SD	Min	Max	N
Poverty (2016 pop share)	16.44	6.54	1.80	53.90	3,112
Social capital (2014)	0.00	1.26	(3.18)	21.81	3,112
% diabetic with annual eye test	66.08	7.60	31.37	90.00	3,058
% diabetic with annual lipids test	78.31	7.85	19.66	94.48	3,061
% diabetic with annual hemoglobin test	83.71	6.59	16.91	100.00	3,073
30-day mortality for pnuemonia	0.12	0.03	0.00	0.63	3,111
30-day mortality for heart failure	0.11	0.02	0.00	0.34	3,111
30-day hospital mortality rate index	0.46	1.21	(7.78)	8.47	3,110
Non-COVID controls					
Population Shares (2016)					
Age under 20	25.18	3.59	4.90	43.40	3,112
Age 20-24	6.40	2.48	0.40	32.50	3,112
Age 25-44	23.29	3.30	8.70	43.40	3,112
Age 45-64	27.50	3.03	9.00	47.40	3,112
Age 65-74	9.99	2.51	3.00	33.60	3,112
Age 75+	7.65	2.33	0.00	19.90	3,112
H/hold annual income below $25k	26.78	8.19	5.50	60.06	3,112
H/hold annual income $25k-$50k	26.20	4.00	8.11	41.68	3,112
H/hold annual income $50k-$75k	18.54	2.79	6.60	30.20	3,112
H/hold annual income $75k-$100k	11.67	2.71	1.30	32.43	3,112
H/hold annual income $100k-$150k	10.72	3.96	1.30	27.80	3,112
H/hold annual income $150k-$200k	3.26	2.16	0.00	16.30	3,112
H/hold annual income $200k plus	2.84	2.56	0.00	25.33	3,112
Female	49.98	2.33	21.50	58.50	3,112
Hispanic	9.62	13.28	0.64	95.49	3,112
Asian	1.82	3.02	0.20	60.93	3,112
Black	9.97	13.33	0.23	70.91	3,112
White (only)	76.44	17.80	3.57	97.01	3,112
Other	5.23	6.48	0.45	79.13	3,112
Less than high school	32.40	5.09	18.22	57.04	3,112
High school graduates	33.26	4.82	9.89	46.29	3,112
Some college	19.14	2.78	8.28	28.31	3,112
College graduates	15.20	5.82	5.59	59.09	3,112
Employment shares (2016)					
Employed in manufacturing	6.71	4.08	0.00	29.01	3,112
Employed in agric or mining	3.79	4.45	0.00	37.00	3,112
Population shares (age 16+; 2016)	4.01	1.65	0.00	18.80	3,112
Unemployed	41.29	7.90	19.60	85.50	3,112
Not in labor force					

Table A1 (cont.). Summary statistics for main variables

	Mean	SD	Min	Max	N
Other (2016)					
Median household income (real)	47,811	12,486	18,972	125,672	3,112
Change between 2012 and 2016					
Age under 20	-0.88	1.35	-15.10	12.70	3,112
Age 20-24	0.24	0.93	-7.40	7.20	3,112
Age 25-44	-0.43	1.46	-30.10	19.70	3,112
Age 45-64	-0.47	1.40	-23.40	16.20	3,112
Age 65-74	1.22	0.93	-8.70	19.10	3,112
Age 75+	0.31	0.76	-6.90	8.20	3,112
H/hold annual income below $25k	-1.38	3.11	-23.01	20.02	3,112
H/hold annual income $25k-$50k	-0.91	2.84	-18.34	13.18	3,112
H/hold annual income $50k-$75k	-0.24	2.47	-17.79	16.00	3,112
H/hold annual income $75k-$100k	0.25	2.07	-15.41	23.83	3,112
H/hold annual income $100k-$150k	1.13	1.90	-8.02	15.28	3,112
H/hold annual income $150k-$200k	0.56	0.96	-7.79	6.21	3,112
H/hold annual income $200k plus	0.59	1.00	-5.81	8.19	3,112
Female	-0.06	1.17	-12.30	23.90	3,112
Hispanic	0.62	2.35	-27.88	24.60	3,112
Asian	0.21	0.57	-8.70	5.83	3,112
Black	0.23	2.80	-29.62	31.64	3,112
White (only)	-1.14	4.11	-28.84	28.84	3,112
Other	0.14	2.53	-23.08	27.05	3,112
Less than high school	-1.91	1.85	-15.78	11.30	3,112
High school graduates	0.10	1.81	-9.00	15.39	3,112
Some college	0.75	1.27	-5.17	8.13	3,112
College graduates	1.06	1.99	-15.43	14.56	3,112
Employed in manufacturing	0.00	1.18	-7.00	5.89	3,112
Employed in agriculture or mining	-0.05	1.28	-16.08	11.09	3,112
Unemployed	-1.05	1.35	-10.40	9.00	3,112
Not in labor force	1.64	2.75	-18.90	27.80	3,112
Median household income (real)	2,321	3,448	-18,810	31,146	3,112

Notes: See main text for further details.

Table A2. Trade war tariffs

A. US tariffs

Tariff Type	Affected type of products	Source for HS8 products affected	Source for HS8 tariffs applied
Section 201 Safeguard Tariffs	Washing Machines & Solar Panels	USITC (2017a, b)	USITC (2017a, b)
Section 232 National Security Tariffs	Steel and Aluminum	US Dept. of Commerce (2018a, b)	US Dept. of Commerce (2018a, b)
Section 301 Unfair Trade Practices Tariffs	China Imports List 1: $34bn	Bown (2019a)	Bown (2019a)
	China Imports List 2: $16bn	Bown (2019a)	Bown (2019a)
	China Imports List 3: $200bn	Bown (2019a)	Bown (2020)
	China Imports List 4A: $121bn	Bown (2019a)	Bown (2020)

B. Foreign tariffs

Retaliation Tariff Type	Source for HS8 products affected	Source for HS8 tariffs applied
Canada Section 232	Bown et al (2018b)	Bown et al (2018b)
China Section 232	Lu & Schott (2018)	Lu & Schott (2018)
EU Section 232	Bown et al (2018a)	Bown et al (2018a)
Mexico Section 232	https://rb.gy/00bztl	https://rb.gy/00bztl
China List 1 -- Section 301	Bown et al (2018c)	Bown et al (2018c)
China List 2 -- Section 301	https://rb.gy/7t6rkq	https://rb.gy/7t6rkq
China List 3 -- Section 301	Bown et al (2018d)	Bown et al (2018d)
China List 4A -- Section 301	Bown (2019b)	Bown (2019b)

Notes: US Section 201 tariffs on solar panels are 30% and weighted average tariff for washing machine tariff rate quota is 42.8%. US Section 232 tariffs are 25% on steel and 10% on aluminum. US Section 301 tariffs are 25% for Lists 1, 2 and 3 but 15% for List 4. Section 232 foreign retaliatory tariffs are 10-25% for EU, 15-25% for China, 10-25% for Canada, and 5-25% for Mexico. Section 301 foreign retaliatory tariffs for China are 5-35%, their List 3 and 4A tariffs can increase earlier List 1 and 2 tariffs.

Table A3. Robustness specifications

	(1)	(2)	(3)	(4)	(5)	(6)
COVID-19 prevalence definition:	Cumulative Deaths	Cumulative Cases	October Deaths	October Cases	Peak Deaths	Peak Cases
Panel A. Alternative COVID-19 prevalence definitions (OLS)						
COVID-19	0.003	-0.007	0.299#	0.000	0.038	-0.001
	(0.017)	(0.005)	(0.164)	(0.004)	(0.057)	(0.003)
Δ Health insurance coverage	-0.079#	-0.077#	-0.080#	-0.080#	-0.077	-0.071
	(0.046)	(0.045)	(0.046)	(0.046)	(0.047)	(0.044)
US tariff shock	0.186*	0.179*	0.182*	0.179*	0.185*	0.177*
	(0.050)	(0.051)	(0.050)	(0.051)	(0.051)	(0.051)
Retalitory tariff shock	-0.193#	-0.188#	-0.180#	-0.184#	-0.171	-0.164
	(0.104)	(0.101)	(0.104)	(0.105)	(0.105)	(0.105)
Agricultural subsidies	0.501*	0.517*	0.498*	0.505*	0.473*	0.483*
	(0.129)	(0.129)	(0.131)	(0.128)	(0.132)	(0.133)
Panel B. Instrumenting COVID-19 prevalence - meat packing instrument						
COVID-19	-0.298	-0.040	-6.612	0.271	-1.932	-0.035
	(0.236)	(0.034)	(6.049)	(0.491)	(1.721)	(0.034)
Δ Health insurance coverage	-0.082	-0.066	-0.060	-0.209	-0.044	-0.033
	(0.051)	(0.043)	(0.056)	(0.270)	(0.068)	(0.053)
US tariff shock	0.113	0.152^	0.127	0.246	0.113	0.147^
	(0.085)	(0.063)	(0.091)	(0.158)	(0.089)	(0.064)
Retalitory tariff shock	-0.136	-0.169#	-0.275^	0.022	-0.142	-0.139
	(0.135)	(0.100)	(0.133)	(0.413)	(0.124)	(0.094)
Agricultural subsidies	0.694*	0.590*	0.668*	-0.186	0.898^	0.683*
	(0.223)	(0.132)	(0.226)	(1.317)	(0.440)	(0.224)
Underidentification p-value	0.159	0.001	0.224	0.498	0.203	0.053
K-P weak instrument rk F-statistic	1.824	11.642	1.459	0.463	1.507	3.331
Panel C. Instrumenting COVID-19 prevalence - nursing home instrument						
COVID-19	0.101#	0.091#	2.669#	0.301	0.514#	0.064
	(0.052)	(0.052)	(1.338)	(0.422)	(0.280)	(0.040)
Δ Health insurance coverage	-0.078#	-0.108#	-0.088#	-0.223	-0.085#	-0.144
	(0.046)	(0.063)	(0.048)	(0.281)	(0.046)	(0.093)
US tariff shock	0.209*	0.261*	0.200*	0.253	0.202*	0.235*
	(0.050)	(0.073)	(0.050)	(0.167)	(0.051)	(0.071)
Retalitory tariff shock	-0.211^	-0.246#	-0.147	0.045	-0.179#	-0.212
	(0.101)	(0.133)	(0.111)	(0.335)	(0.104)	(0.157)
Agricultural subsidies	0.437*	0.302#	0.439*	-0.261	0.370*	0.093
	(0.123)	(0.175)	(0.157)	(1.145)	(0.137)	(0.272)
Underidentification p-value	0.000	0.003	0.001	0.413	0.000	0.010
K-P weak instrument rk F-statistic	55.542	13.199	15.426	0.615	53.495	7.280

Notes: Notes: # p<0.10, ^ p<.05, * p<.01. Panels A-C present additional results from specifications in Panels A-C of Table 3 in main text. See notes to Table 3 for further details.

Table A4. Placebo specifications

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
COVID-19	-0.011	-0.084	-0.011	-0.018	-0.016	-0.005	-0.005
	(0.027)	(0.289)	(0.040)	(0.134)	(0.115)	(0.026)	(0.027)
Δ Health insurance coverage	0.023	0.023	0.023	0.027	0.029	0.024	0.042
	(0.060)	(0.061)	(0.061)	(0.059)	(0.065)	(0.061)	(0.119)
US tariff shock	-0.063	-0.08	-0.064	-0.069	-0.073	0.028	-0.062
	(0.080)	(0.100)	(0.086)	(0.081)	(0.122)	(0.116)	(0.072)
Retalitory tariff shock	-0.046	-0.032	-0.044	-0.041	-0.038	-0.174	0.011
	(0.073)	(0.091)	(0.078)	(0.078)	(0.101)	(0.160)	(0.077)
Agricultural subsidies	0.949*	0.994*	0.953*	0.964*	0.974*	1.437*	0.620^
	(0.289)	(0.352)	(0.281)	(0.303)	(0.323)	(0.533)	(0.269)
N	2991	2991	2991	2991	2991	2991	2991
COVID prevalence definition	Cum. Deaths	Cum. Deaths	Cum. Cases	Cum. Deaths	Cum. Cases	Cum. Deaths	Cum. Deaths
Endogenous variables	None	COVID-19				Health insurance coverage expansion	Trade war
Instruments		Meat packing emp. share		Nursing home pop. share		Lewbel	
Underidentification p-value		0.160	0.000	0.001	0.005	0.016	0.001
K-P weak instrument rk F-statistic		1.818	56.070	11.465	11.529	11.378	51.612
Overidentification p-value						0.477	0.005

Notes: # p<0.10, ^ p<.05, * p<.01. Dependent variable is the change in the 2-party Republican vote share between the 2012 and 2016 US Presidential election. Estimation performed by fixed effects OLS in column (1), IV in columns (2)-(5), and IV-GMM in columns (6)-(7). Standard errors clustered by state. Full set of control variables and fixed effects as in column (6) of Table 1 (except the 2012-2016 change in the Republican vote share). All specifications weighted by 2020 total Presidential votes cast. Lewbel instruments in columns (6)-(7) created using the same controls listed in notes to Table 3. See main text for further details.