The 2020 US Presidential election: Trump's wars on COVID-19, health insurance, and trade

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Abstract

A common narrative is that COVID-19 cost Trump re-election. But, we do not find supporting evidence. Rather, our results highlight the political salience of the trade war and health insurance coverage in the 2020 US Presidential election. US trade war tariffs boosted Trump's support and foreign retaliation hurt Trump. In particular, the pro-Trump effects of US trade war tariffs were crucial for Trump getting inside the recount thresholds in Georgia and Wisconsin. Even more important politically, 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 anti-Trump effect, Trump would have been on the precipice of re-election by winning Georgia, Arizona, Nevada, and only losing Wisconsin by a few thousand votes. These effects cross political and racial lines. Thus, our results suggest a mechanism based around the local economic impact of Trump administration policies rather than a mechanism of political polarization.

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1 Introduction

The COVID-19 pandemic is perhaps the most common narrative explaining President Trump's 2020 election defeat. According to the Washington Post, "[T]he 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)).

However, the Trump administration also pursued various far-reaching policies that substantially affected wide segments of the American population. 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] And countries around the world retaliated with their own tariffs. Indeed, the media openly discussed the possible adverse effects of this retaliation for Trump leading into the 2018 US midterm elections (Merica (2018)) and the broader role of the trade war in explaining the Democrat's sweeping victory in those elections (Bryan (2018)). Recent academic evidence finds important effects of the trade war through increasing US consumer prices (Amiti et al. (2019); Fajgelbaum et al. (2020); Cavallo et al. (2021)), decreasing consumption and employment (Waugh (2019); Flaaen and Pierce (2020)), and reducing US exports through higher input tariffs (Handley et al. (2020)).[2] Reflecting these various economic effects, Blanchard et al. (2019) and Li et al. (2020) confirm the political salience of the trade war in the 2018 US midterm elections.

More so than the trade war, the media proclaimed health insurance as a crucial issue before and after the 2018 midterms (Lowrey (2018), Scott (2018)). The Affordable Care Act (ACA), perhaps President Obama's lasting legacy, expanded health insurance coverage to millions of Americans in the early years after its implementation in 2014. 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 has not been concrete. Indeed, reflecting voter anxiety over Republican-led attempts to undermine the ACA, Blanchard et al. (2019) find it was a highly salient political issue in the 2018 midterms.

We analyze the county-level impacts of the trade war (US tariffs, foreign retaliatory tariffs, and US agricultural subsidies), the post-ACA expansion of health insurance coverage, and COVID-19 prevalence on the change in Trump's vote share between the 2016 and 2020

[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)).

[2] See Fajgelbaum and Khandelwal (2021) for a recent survey on the economic effects of the trade war.

US Presidential elections. 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 use US Census data to obtain the increased share of the population with health insurance coverage in the 5-year period after ACA implementation. Normalizing by population, we focus on COVID-19 prevalence as deaths since the pandemic began (we also consider cases and deaths in various time windows). We control for various county-level economic, demographic, health, and political characteristics that could correlate with these salient issues and voting behavior. These measures include the distribution of age, race, income and education; health and population density characteristics reflecting increased risk of COVID-19; and social distancing and COVID-depressed economic activity.

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. (2021), 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.[3] Endogeneity concerns over the trade war and health insurance coverage expansion are more challenging. Indeed, instruments for tariffs are notoriously problematic (Blanchard et al. (2019)). 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.

Against the common narrative, we do not find evidence that COVID-19 contributed to Trump's 2020 election defeat. 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 as better at dealing with a COVID-ravaged economy.[4] Nevertheless, our preferred interpretation is the absence of an effect given the varied results

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

[4] 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).

across specifications.

Rather than COVID-19, our results highlight the political salience of the trade war and health insurance coverage. We find robust evidence of a pro-Trump effect of US trade war tariffs: voters rewarded Trump for protecting their local economy. And, we find robust evidence of an anti-Trump effect of foreign retaliatory tariffs: voters penalized Trump when their local economy faced reduced access to foreign markets. In contrast, we do not find robust evidence for an effect of agricultural subsidies. Given the states that ultimately decided the Presidential election were not the agricultural heartland of the US that bore the brunt of foreign retaliation, only the US trade war tariffs have a meaningful impact on the election results. Our results imply the absence of US tariffs would have pushed Georgia and Wisconsin out of recount territory and, hence, would have been decisive in a slightly tighter election.

Motivated by the recent work of Autor et al. (2020) and Che et al. (2020), we investigate whether the mechanism driving the effect of trade war tariffs on voting behavior centered around political polarization or the pure economic effects of Trump's trade war on their local economies.[5] We find no evidence of the trade war tariffs leading counties that were solidly Republican (or that Trump won in 2016 or that have a majority white population) to become redder while simultaneously leading counties that were solidly Democrat (or that Hillary Clinton won in 2016 or have a majority population of minorities) to become bluer. Indeed, the strongest pro-Trump effects of US tariffs are in solidly Democrat counties and counties that Hillary Clinton won in 2016. Moreover, the effects of the trade war are statistically insignificant and economically small in competitive counties. Ultimately, our results are more consistent with a mechanism of economic incentives rather than political polarization driving voter behavior towards Trump over the trade war tariffs.

Our use of an IV approach is motivated by the trade policy literature clearly recognizing that politicians endogenously choose tariffs based on various economic, social, and political characteristics. An econometric endogeneity problem arises if we omit economic, social, and political characteristics that both (i) correlate with how the political tariff formation process maps to county-level exposure and (ii) drive the change in voting behavior towards Trump between 2016 and 2020. With our host of control variables and fixed effects, the IV point estimates for the trade war tariffs are only modestly smaller than the OLS point estimates. Based on these relatively small differences, formal tests of endogeneity cannot reject the null hypothesis that the trade war tariffs are actually exogenous. Given the important efficiency cost of the the IV estimator over the OLS estimator (Wooldridge (2003, p.490)), our analysis

[5] A large literature and influential shows how US trade policy has had large effects on US local labor market outcomes in recent decades (e.g. Autor et al. (2013); Hakobyan and McLaren (2016)).

provides support for treating the trade war tariffs as exogenous.

Politically more important than the trade war tariffs, we find a robust and crucial role for health insurance coverage expansion 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.

Ours is the first paper we know that analyzes how the trade war or health insurance coverage expansion impacted the 2020 US Presidential election. But, Blanchard et al. (2019) and Li et al. (2020) analyze how the trade war impacted the 2018 US congressional midterm elections. And, Blanchard et al. (2019) also analyze the effect of health insurance coverage expansion. These are the two most closely related papers to ours.

Blanchard et al. (2019) find an important role for the both the trade war and health insurance coverage expansion in explaining the Democrats sweeping victory in the 2018 US midterm elections that gave them an 18 seat House majority. They find that health insurance coverage expansion accounts for half of this majority and foreign trade war retaliation accounts for one-quarter. Our results reaffirm the persistent political salience of health insurance coverage. Indeed, our results say it essentially cost Trump the 2020 Presidential election which we would argue is an order of magnitude larger in terms of economic significance. In contrast to Blanchard et al. (2019), our results at the electoral college level for the Presidential election emphasize the political salience of US trade war tariffs rather than foreign retaliatory tariffs. Our IV approach, which appears to work well, also addresses the self-acknowledged endogeneity limitations of the trade war in Blanchard et al. (2019).

Our results on the political salience of US trade war tariffs and our use of IV to address potential endogeneity issues resemble key aspects of Li et al. (2020). In contrast to Blanchard et al. (2019), Li et al. (2020) focus on a *net* effect of the US-China trade war on the 2018 midterm elections that considers both US tariffs on China and Chinese retaliation. They find a pro-Republican effect of the trade war tariffs that is actually significantly stronger in solidly Democrat counties. This closely mirrors our result that the pro-Trump effect of US trade war tariffs is actually strongest in solidly Democrat counties or counties that Hillary Clinton won in 2016. Li et al. (2020) consider an IV strategy based around the idea that (i) the US tariffs on China reflected an attack on the products in China's "Made In China 2025" industrial policy but (ii) this industrial policy was unlikely to otherwise affect US voter behavior. Similar to our analysis, their IV results do not notably alter their key OLS results.[6]

Past literature has argued US trade policy reflects the economic effects that voters face from trade policy.[7] Che et al. (2020) argue the pro-Democrat effect of rising Chinese import competition in the 2000s reflected that Democrats typically voted against pro-trade congressional bills. Conconi et al. (2014) show that US politicians facing re-election risk are much more likely to vote against pro-trade congressional bills. When voting on Free Trade Agreements, Lake and Millimet (2016) show that US politicians facing re-election risk or representing constituents facing a lot of impending import competition are much more sensitive to the amount of Trade Adjustment Assistance their constituents receive. In contrast to these papers, Autor et al. (2020) argue that rising Chinese import competition led to political polarization by hollowing out the political center and by simultaneously pushing majority-white areas towards Republicans and majority-minority areas towards Democrats. Despite the polarizing nature of Trump, our analysis suggests voter behavior towards Trump in the 2020 US Presidential election reflected the economic effect of his policies on voters rather than his policies driving political polarization.

Recent empirical trade war papers discuss concerns about trade war tariffs reflecting a political calculus and creating econometric endogeneity issues. Fajgelbaum et al. (2020) document that 2018 US trade war tariffs protected swing counties. Fajgelbaum et al. (2020) and Fetzer and Schwarz (2021) show 2018 foreign retaliatory tariffs targeted Republican counties and counties that swung to Trump in 2016. Earlier theoretical work, e.g. Ma and McLaren (2018), rationalizes how politicians target swing states. Indeed, Blanchard et al. (2019, p.3) state "We stop short of claiming causal identification". To deal with endogeneity issues, Li et al. (2020) use an IV approach based around China's Made in China 2025 industrial policy. And, we use the Lewbel (2012) heteroskedasticity-based IV approach (which works well according to standard IV specification tests, including weak instrument and overidentification tests). However, given our host of controls and fixed effects, our IV and OLS point estimates are sufficiently close that we cannot reject the null that the trade war tariffs are actually exogenous. This suggests that a fairly typical set of fixed effects and economic, social and political characteristics can control for econometric endogeneity

[6]Although Li et al. (2020) do not perform similar tests, they also find very similar OLS and IV results. See their Table 2 and Table 5.

[7]A separate strand of the empirical literature emphasizes the importance of lobbying and campaign contributions on US trade policy. This literature goes back to at least the protection for sale literature (e.g. Goldberg and Maggi (1999); Gawande and Bandyopadhyay (2000); Bombardini (2008); Gawande et al. (2012)) and analyses looking at congressional voting behavior (e.g. Baldwin and Magee (2000); Im and Sung (2011); Lake (2015)). More recent papers have looked at the informational role of lobbying (e.g. Ludema et al. (2018)) and the contest nature of lobbying whereby lobbying expenditures are sunk before governments make trade policy decisions (Cole et al. (2021); Blanga-Gubbay et al. (2020)).

concerns.

Baccini et al. (2021) 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. Given the exclusion restriction for both instruments appear reasonable, we interpret our results as strong evidence against an anti-Trump effect of COVID-19. An important difference between our analysis and Baccini et al. (2021) 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. (2021) completed their analysis.

Our paper proceeds as follows. Section 2 presents our main empirical specification and discusses identification issues. Section 3 describes the data. Section 4 presents all of our results. Section 5 concludes.

2 Empirical strategy

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

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

$\triangle V_c^y$ is the change in the two-party Republican vote share between Presidential elections in year y and year $y-4$. TW_c is a vector of trade war variables. $\triangle HI_c$ measures health insurance coverage expansion either side of the ACA. $COVID_c$ is a measure of COVID-19 prevalence. 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.

2.1 Potential endogeneity concerns

The clear identification threat is omitted variable bias that leads to endogeneity of some of our key electoral issues: the trade war variables, health insurance coverage expansion, or COVID-19 prevalence. This would require omitted variables that are correlated with these issues and also drive the *change* in voting behavior between 2016 and 2020. Indeed, given the inclusion of $\triangle V_c^{2016}$ in (1), these omitted variables would need to drive the change in voting behavior between 2016 and 2020 *after* conditioning on the change in voting behavior between 2012 and 2016.[8] Thus, omitted variables that drive permanent or long-run aspects of voting behavior do not pose an endogeneity problem. Nor do omitted variables that drive changes in voting behavior between 2016 and 2020 but were already driving changes in voting behavior between 2012 and 2016 as part of a trend in the evolution of local voting behavior. Thus, endogeneity concerns really revolve around shocks to voting behavior towards Trump between 2016 and 2020.

Nevertheless, US or foreign governments may naturally choose trade war policies to influence voter behavior between 2016 and 2020 in particular geographical areas of the US. The electoral college system for electing the US president makes each state a winner-take-all contest. In many states, the Democrat and Republican Presidential nominee need independent or swing voters to win the state. Thus, median voter theory suggests governments will use trade war policies to sway independent and swing voters in particular regions of the US (e.g. Ma and McLaren (2018)). Additionally, models revolving around the importance of raising campaign and lobbying contributions suggest that governments may also use trade war policies to target very partisan regions of the US. In either case, trade war policies may target particular regions of the US in ways that depend on the regions' economic, social and political characteristics. The trade war variables would be endogenous if we omit such characteristics that also happen to drive voting behavior shocks between 2016 and 2020.

The link between health insurance coverage expansion and shocks to voting behavior between 2016 and 2020 is less direct than for the trade war variables. This is because, as discussed further in Section 3.3, the ACA health exchanges that underpinned the Obamacare-induced expansion of health insurance coverage became operational in January 2014. Thus, they pre-date Trump's 2016 victory. But, the social and economic characteristics driving the subsequent expansion of health insurance coverage may also be also important factors driving the change in voter behavior towards Trump between 2016 and 2020. If so, health insurance coverage expansion would be endogenous.

Only eight months before the 2020 US Presidential election, the onset of the COVID-19 pandemic was widely viewed as ushering a major turning point in Trump's political fortunes. Naturally, regional COVID-19 prevalence could be driven by regional economic, social, political and health characteristics. Our measure of COVID-19 prevalence will be endogenous if we omit such characteristics that also drive the change in voter behavior towards Trump between 2016 and 2020.

[8]Indeed, Fetzer and Schwarz (2021) find that retaliatory tariffs were targeted at areas where Trump's 2016 vote share improved over Romney's 2012 vote share.

2.2 Dealing with potential endogeneity problems

We take two approaches to deal with these endogeneity concerns. First, we control for a host of county-level social, economic, political and health characteristics. In addition, state fixed effects control for state-level unobservables along these dimensions. Nevertheless, there may be still be omitted county-level social, economic, political or health characteristics that not only drive county-level changing voter behavior towards Trump between 2016 and 2020 but also help explain county-level exposure to the trade war, health insurance coverage expansion, or COVID-19 prevalence.

Thus, our second approach instruments for these potentially endogenous variables. While we use traditional IVs for COVID-19 prevalence, we use Lewbel (2012) heteroskedasticity-based IVs for the trade war variables and health insurance coverage expansion given the lack of obvious instruments.

The Lewbel approach "first-stage" regresses an endogenous variable r on the exogenous controls $\tilde{X} = [\triangle V^{2016}\ X\ \delta]$ from (1). For a subset of exogenous controls $Z_r \subseteq \tilde{X}$, he shows the identifying assumptions are $cov\,[Z_r, u_r^2] \neq 0$ and $cov\,[Z_r, \varepsilon u_r] = 0$ where u_r is the first-stage error term. Intuitively, heteroskedasticity of the first-stage errors u_r depends on Z_r but the correlation between the first-stage error u_r and structural error ε from (1) does not depend on Z_r. 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. An obvious example of a common factor in our context would be local political activism. 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.[9]

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, instrument strength depends on heteroskedasticity of the first-stage errors. Thus, we use the Koenker (1981) Breusch-Pagan test for heteroskedasticity to identify $Z_r \subseteq \tilde{X}$ that are significantly related to the first-stage error variances.

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. These instruments have been highlighted in the US media as driving a large share of US COVID-19 deaths and an important factor seeding local outbreaks of COVID-19 cases. This clearly motivates the intuition for why these instruments could be strong.

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

The exclusion restriction for each instrument is that it is uncorrelated with the *change* in Trump's vote share between 2016 and 2020 *conditional* on the controls and fixed effects. Counties with high population shares of nursing home residents or high employment shares of meat-packing workers may have certain social, economic, and health characteristics. And, some of these characteristics could drive changes in voting behavior between 2016 and 2020. However, as described in Sections 3.4 and 3.5, we control for a county's distribution of age (including the population shares in the 65-74 and over 74 age brackets), education, and income; industrial and racial composition; and health characteristics (e.g. diabetes incidence and various mortality rates). Thus, we argue the exclusion restrictions appear plausible: conditional on these controls and state fixed effects, changes in voting behavior for Trump between 2016 and 2020 in a county with more nursing home residents or meat-packing workers comes through the effect of higher COVID-19 prevalence.

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.[10] 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 change between the 2012 and 2016 elections, $\triangle V_c^{2016}$, is 5.88% points (Appendix Table A1 contains all summary statistics).

Panels A-B of Figure 1 show the starkly different geographic distributions of these variables. Relative to the 2012 Republican Presidential nominee Mitt Romney, Panel B shows that Trump mostly increased his 2016 vote share in the Midwest and Northeast while only losing ground in barely 10% of counties. However, relative to his own 2016 vote share, Panel A shows that Trump mostly increased his 2020 vote share in the South while losing ground in nearly two-thirds of counties. Thus, these vote share changes differ notably and only have a weak positive correlation.[11]

[10] We use Version 0.9 from the Election Atlas. Alaska and Kalawao county in Hawaii do not report county-level votes. Thus, our sample has 3112 counties.

[11] The correlation is .264.

3.2 Trade war

3.2.1 Evolution of the trade war in 2018 and 2019

Table 1 summarizes the evolution of the trade war initiated by the Trump administration in 2018 and the source of our trade war data.[12] The trade war began with the Trump administration imposing two types of MFN tariffs (i.e. applied to all US imports). In February 2018 came the Section 201 safeguard tariffs on around $10bn of washing machine and solar panel imports. Then the Section 232 tariffs came in March 2018 on around $40bn of steel and aluminum imports in the name of defending US national security. While the WTO allows safeguard tariffs, the national security tariffs created immediate and fierce claims of WTO illegality by US trading partners.[13] Among others, the EU, Canada, China and Mexico retaliated quickly and proportionately with their own tariffs on the US.

Nevertheless, the trade war quickly developed into mostly a US-China trade war. At its center are the Section 301 tariffs imposed by the US. These were imposed in the name of unfair trade practices that revolved around alleged forced technology transfer from US firms by China. By September 2018, the US was imposing a 25% tariff on around $50bn of Chinese imports and a 10% tariff on around another $200bn of Chinese imports. This latter tariff increased to 25% in June 2019. And a 15% tariff on around $110bn more Chinese imports was imposed in September 2019. At that stage, the US was hitting about 65% of its Chinese imports with a trade-weighted average tariff of about 21% (compared to a trade-weighted average tariff on the rest of the world of around 3%).

China retaliated in a "tit-for-tat" manner. In summer 2018, it retaliated dollar-for-dollar by imposing tariffs on around $50bn of US exports. When China ran out of US exports to hit after the September 2018 US tariffs, they retaliated so that nearly 50% of US exports were hit with Chinese tariffs. Following the US tariff increase in June 2019, China increased tariffs on US exports already hit with tariffs. And China retaliated to the new US tariffs in September 2019 so that nearly 60% of US exports were hit with tariffs. At this stage, China's trade-weighted average tariff on US exports was around 22% (compared to their trade-weighted average tariff on the rest of the world of around 6%).

[12] See Bown and Kolb (2021) for an excellent interactive timeline of the trade war with links to various additional sources of information and analysis.

[13] Some exceptions were granted to the national security tariffs. Initially, the EU, Mexico, Canada, South Korea, Brazil, Argentina, and Australia were exempt. By summer 2018, the EU, Mexico and Canada were hit with the tariffs while tariff-rate quotas were imposed on South Korea, Brazil and Argentina. Australia remained exempt.

3.2.2 County-level exposure to trade war

We closely follow Blanchard et al. (2019) in constructing county-level exposure to US and foreign retaliatory trade war tariffs and county-level agricultural subsidy receipts.

We begin by defining industry-level trade war "tariff shocks" as the additional tariffs charged on (i) US imports from all countries and (ii) US exports to the four major US trading partners: China, Mexico, Canada and the EU. Denoting country k tariffs by τ^k and 2017 US imports by m, the additional tariffs charged on US imports of HS8 product h from country j are $TS^{US}_{h,j} = \tau^{US}_h m_j$. Denoting 2017 US exports by x, the additional retaliatory tariffs charged on US exports of HS8 product h to country j are $TS^{R}_{h,j} = \tau^{j}_h x_j$. Aggregating to the industry-level across US trade partners gives $TS^{US}_h = \sum_j TS^{US}_{h,j}$ and $TS^{R}_h = \sum_j TS^{R}_{h,j}$. Finally, we concord to NAICS 3-digit industries using the 2002-2006 Feenstra et al. (2002) trade weights. This gives the additional tariffs charged on US imports, TS^{US}_i, and US exports, TS^{R}_i, for each 3-digit NAICS industry i.

The last step is converting industry-level tariff shocks to county-level tariff shocks using 2016 US employment data from the County Business Patterns. Dividing the tariff shock for 3-digit NAICS industry i by its US employment L_i converts the industry-level tariff shock into a per worker measure.[14] We then use county-industry employment weights $\frac{L_{ic}}{L_c}$ to compute the tariff shocks for county c:

$$TS^{US}_c = \sum_i \frac{L_{ic}}{L_c}\frac{TS^{US}_i}{L_i}$$
$$TS^{R}_c = \sum_i \frac{L_{ic}}{L_c}\frac{TS^{R}_i}{L_i}.$$

Table A1 and Figure 1 describe the county-level tariff shocks. Across all US counties, Table A1 shows that the mean US tariff shock is \$1030 per worker and the mean retaliatory tariff shock is \$550 per worker. Panels C-D of Figure 1 emphasize the different geographic distribution of county-level exposure to US and foreign retaliatory tariff shocks.[15] Exposure to US trade war tariffs is concentrated around the Great Lakes and parts of the South. In contrast, exposure to foreign retaliation is concentrated along the Mississippi River, the lower Midwest and the far West. These different geographic distributions fit with the broad idea that US tariffs protected US manufacturing while foreign retaliation targeted US agriculture.

Due to foreign retaliation targeting US farmers, the Trump administration implemented

[14] 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. Thus, we follow their interpolation method to replace the flagged employment range with an imputed employment level.

[15] Their correlation is 0.075.

the Market Facilitation Program of agricultural subsidies in 2018 to help US farmers hurt by foreign retaliatory tariffs. We use county-level estimated subsidy receipts from Blanchard et al. (2019). Table A1 shows the mean county had per worker agricultural subsidies of $430. Panel E of Figure 1 shows these are heavily concentrated in the central and upper Midwest and along the Mississippi River. Perhaps surprisingly, but as noted by Blanchard et al. (2019), they are only loosely correlated with foreign retaliation.[16]

3.3 Health insurance

The centerpiece of the Affordable Care Act (ACA) are the ACA health exchanges that became operational in January 2014. US Census data shows a stable uninsured population share of around 20% over the 2008-2013 period that dropped to around 12% by 2016 and has remained stable thereafter (Keisler-Starkey and Bunch (2020)). This reflects how the ACA transformed the US health insurance marketplace and underpinned expansion of health insurance coverage to millions of Americans.

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.[17] Panel F of Figure 1 shows significant geographic variation around the mean expansion of 5.05% points (see Table A1). Numerous large counties around major cities in states that decided the 2020 Presidential election saw above-average expansion (including Georgia, Arizona and Nevada).

3.4 Non-COVID controls

As discussed in Section 2, endogeneity of our key explanatory variables – trade war variables, health insurance coverage expansion, and COVID-19 prevalence – is the key threat to identification. Specifically, the concern is that county-level omitted social, economic, political and health characteristics could not only drive the change in voting behavior towards Trump between 2012 and 2016 but also correlate with the key explanatory variables at the county-level. Thus, we use a host of control variables to mitigate these endogeneity concerns.

We start with a typical set of economic and demographic variables (using 5-year samples of ACS data) to control for factors plausibly affecting voting preferences and the trade war

[16] Their correlation is .179. Further, the correlation between US tariff shocks and agricultural subsidies is -0.03.

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

variables or health insurance coverage expansion. First, 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). Second, in 2016 levels and changes between 2012 and 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). Third, we control for the 2013 pre-ACA level of health insurance coverage. Fourth, some of our controls motivated by mitigating endogeneity of COVID-19 prevalence (see Section 3.5 below) could also help mitigate endogeneity concerns over the trade war variables or health insurance coverage expansion.

3.5 COVID-19 variables

COVID-19 prevalence. Our COVID-19 data comes from COVID County Data (which merged with Covid Act Now). They obtain data from various sources with county-level dashboards most preferred.[18] Our baseline measure of COVID-19 prevalence is cumulative deaths per 10,000 population from January 1 to October 31, 2020. However, Section 4.3 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.[19] The possibility of voters caring about recent or "peak" COVID-19 outbreaks motivate the latter two windows. Panels A-B of Figure 2 show the geographic incidence of COVID-19 cumulative deaths and cases through October 31, 2020. While deaths are relatively higher than cases in the early-hit north-east, cases are relatively higher than deaths in the later-hit Dakotas and Minnesota. Figure A1 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.

COVID-19 controls. County-level social distancing and COVID-induced downturns in economic activity could drive changes in voting behavior towards Trump and also correlate with county-level COVID-19 prevalence. 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

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

[19]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.

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. Figure A2 illustrates its geographic distribution and how the MEI national mean evolved during 2020.

To control for economic activity, we use two county-level measures. Figure A2 illustrates their geographic distributions and also how the national mean of underlying variables evolved during 2020. First, we use the county-level change in the unemployment rate between October 2019 and 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 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 (2021), we control for broader social, economic, political, and health correlates of county-level COVID-19 prevalence that could also drive changes in voting behavior towards Trump.[20] 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 (2021)) and indicators for large metros, small and medium metros, and non-metros.[21] 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, separate 30-day mortality rates for pneumonia and heart failure, and the 30-day hospital mortality index.[22] Third, we control for social capital (Rupasingha et al. (2006)). Fourth, moving beyond Desmet and Wacziarg (2021), we control for the share of county employment that can work remotely (Dingel and Neiman (2020)).[23]

[20] Also see Allcott et al. (2020).

[21] Effective density differs from standard population density by using the spatial population distribution within a location. Metro indicators can be downloaded from the Urban-Rural Classification Scheme for Counties of the National Center for Health Statistics (NCHS).

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

[23] 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

COVID-19 instruments. Our 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. (2021), 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 C-D 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.

4 Results

4.1 Baseline results

Table 2 presents the baseline results. Columns (1)-(3) successively add the three trade war variables: US tariff shock, retaliatory tariff shock, and agricultural subsidies. The only control here is the Republican vote share change between 2012 and 2016, $\triangle V_c^{2016}$. The fairly stable point estimates across these columns emphasize that, as discussed in Section 3.2, the trade war variables are largely uncorrelated between themselves. This is important because it notably mitigates concerns about endogeneity of one trade war variable spilling over to create endogeneity problems for other trade war variables.

As one may expect given our discussion of potential endogeneity problems in Section 2, controlling for county-level social, economic, and political variables as well as state fixed effects is important. Doing so in columns (4)-(5) makes the point estimates for US and retaliatory tariff shocks statistically significant and flips their sign.[24] The positive point estimates for the US tariff shock and agricultural subsidies in column (5) say Trump's county-level vote share was higher when the county had more exposure to US tariff shocks or received more agricultural subsidies.[25] The negative point estimate for the retaliatory tariff

(https://usa.ipums.org/usa/volii/occsoc18.shtml).

[24] We lose 1 observation in column (5) because state fixed effects lead to Washington D.C. being dropped from the estimation sample.

[25] When comparing across counties, it is important to remember the dependent variable is the change in Trump's vote share between 2016 and 2020. So, the positive point estimate for the US tariff shock also says counties more exposed to US tariffs had either a smaller decline in Trump's vote share from 2016 or a larger increase from 2016 than counties less exposed to US tariffs.

shock in column (5) says Trump's county-level vote share was lower when the county faced larger retaliatory tariff shocks. These signs are intuitive: Trump benefited politically from supplying greater protection to local economies through tariffs or agricultural subsidies but was hurt politically when local economies suffered from retaliation in foreign markets.

Column (6) adds health insurance coverage expansion. Given the trade war point estimates are virtually unchanged from column (5), county-level health insurance coverage expansion is largely uncorrelated with county-level trade war exposure. So, again, any endogeneity problem for one of the explanatory variables does not spill over to other key explanatory variables.[26] Moreover, the negative and statistically significant point estimate says Trump's county-level vote share was lower when the county experienced a greater post-ACA expansion of health insurance coverage. A natural interpretation is that larger health insurance coverage expansion translated into greater fears over Republican-led efforts to undermine and repeal the ACA. In turn, Trump was politically hurt by these efforts.

Columns (7)-(8) add our main measure of COVID-19 prevalence – cumulative deaths through October 2020 per 10,000 population – and our COVID-19 control variables.[27] Continuing the theme developed above, the point estimates for the trade war and health insurance coverage expansion variables remain very stable in column (7) versus column (6) which says they are largely uncorrelated with COVID-19.[28] So, any potential endogeneity problems with COVID-19 prevalence are not major concerns for endogeneity of the other variables. After adding the COVID-19 controls in column (8), the point estimate for COVID-19 prevalence is nowhere near statistical significance at conventional levels and very small economically. This result says Trump's political fortunes across counties in the 2020 US Presidential election cannot be explained by county-level differences in COVID-19 prevalence.

When interpreting our COVID-19 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 results from Table 1 do not support the notion that voters penalized Trump more in counties subject to larger COVID-19 outbreaks.

Some of the effects described above are economically significant. The point estimates

[26] The correlations between county-level health insurance coverage expansion and the trade war variables are -.032, .072 and -.146 for, respectively, US tariff shocks, retaliatory tariff shocks and agricultural subsidies.

[27] The estimation sample drops by 120 observations between columns (7) and (8) because 106 counties are missing MEI data and a further 14 counties are missing other COVID-controls data.

[28] The correlations between county-level COVID-19 prevalence and the trade war variables are -.014, .039 and .009 for, respectively, US tariff shocks, retaliatory tariff shocks and agricultural subsidies and .001 for health insurance coverage expansion.

from column (8) of Table 2 imply the median county saw Trump's 2020 vote share increase by 0.13%, .01% and .01% points respectively on account of US trade war tariffs, agricultural subsidies, and COVID-19 deaths but decrease by 0.06% and 0.37% points respectively on account of retaliatory tariffs and health insurance coverage expansion. However, the effect for a median county is potentially misleading in terms of state-level electoral college outcomes. For example, the median county effect understates the state-level electoral college impact of the US trade war tariffs if large counties were the most exposed to these tariffs.

Table 3 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 2, we use its county-specific value and its column (8) point estimate to compute counterfactual county-level vote shares for Trump and Biden in the absence of this variable. 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 median 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 3 is that the only economically significant variables are the US tariff shock and health insurance coverage expansion. Comparing column (1) with columns (2)-(4) of panel A in Table 3 reveals 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 between column (1) and column (4). 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 0.53% points and 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.

But, health insurance coverage expansion is easily the most economically significant variable. Column (5) shows removing the impact of health insurance coverage expansion 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. In contrast, column (6) shows Trump's state-level vote share in the key states that decided the election does not move by more than 0.06% points in the absence of COVID-19. Thus, at least in a cross-county sense, health insurance coverage is a very politically salient issue and COVID-19 prevalence is not.

4.2 IV results

Table 4 presents the IV results. For ease of comparison, column (1) presents the OLS results from column (8) of Table 2. Columns (2)-(4) treat one of the trade war variables as endogenous and column (5) treats all trade war variables as endogenous. Column (6) treats health insurance coverage expansion as endogenous. Column (7) treats all trade war variables and health insurance coverage expansion as endogenous. Columns (8)-(9) treat COVID-19 deaths as endogenous with column (8) using the nursing home resident instrument and column (9) using the meat-packing worker instrument.

Importantly, our Lewbel heteroskedasticity-based IV approach performs well according to standard IV specification tests in columns (2)-(7) when treating the trade war variables and/or health insurance coverage expansion as endogenous. We always reject the null of underidentification at the $p < 0.1$ level and mostly at the $p < 0.05$ level. The Kleibergen-Paap weak-instrument F-stats are in the 20-65 range when treating one variable as endogenous and still exceed the common rule-of-thumb-value of 10 when treating multiple variables as endogenous. And, based on Hansen's J-test of overidentification, we always fail to reject the null that the instruments are exogenous with the p-values in the 0.49-0.90 range. These tests provide evidence that our instruments are strong and exogenous.

Indeed, there is notable evidence that our set of control variables actually contain the key county-level social, economic and political variables that remove endogeneity concerns over county-level exposure to US and foreign retaliatory tariffs. Specifically, based on comparing two Sargan-Hansen statistics, our test of endogeneity says we are far from conventional levels of statistical significance for rejecting the null that the US and foreign retaliatory tariff shocks are exogenous (p-values of .458 and .884 respectively). This provides support for the identification strategy in the broader trade literature of using county-level tariff exposure measures and controlling for endogeneity concerns using fixed effects and a wide set of county-level social, economic, and political variables.

Nevertheless, we now turn to the IV point estimates. The US tariff shock point estimate falls by around one-third in column (2) when treating it as the only endogenous variable and by around one-quarter in column (7) when treating all trade war variables and health insurance coverage expansion as endogenous. That said, the US tariff shock remains statistically and economically significant. Based on the column (7) point estimate from Table 4, Panel B of Table 3 shows removing its effect still roughly doubles Trump's loss in Georgia from

0.24% to 0.46% points and his loss in Wisconsin from 0.64% to 1.06% points. These margins would still not prevent a Georgia recount but would avoid a Wisconsin recount (respective recount thresholds of 0.5% and 1% point).

The point estimate for foreign retaliation falls by around one-quarter in column (7) when treating all trade war variables and health insurance coverage as endogenous. While it remains statistically significant, column (3) in panel B of Table 3 shows it also remains economically insignificant in affecting state-level electoral college outcomes of closely contested states.

Agricultural subsides appear to be the trade war variable most susceptible to endogeneity. With $p = .093$, the endogeneity test rejects the null that they are exogenous at the $p = 0.1$ level. And, treating them as endogenous reduces its point estimate from 0.501 in column (1) to 0.010 in column (7). This suggests an upward bias due to an omitted variable that is positively correlated with county-level agricultural subsidies and also drives changes in voter behavior towards Trump between 2016 and 2020. Intuitively, this fits closely with the idea that Trump used agricultural subsidies to target a narrow set of politically motivated counties.

If anything, the OLS estimate for health insurance coverage expansion appears downward biased: the IV point estimate in columns (6) and (7) is more than double its OLS value. Moreover, the endogeneity test marginally rejects the null ($p = .099$) that health insurance coverage expansion is exogenous at the $p = 0.1$ level . As expected, the much larger IV point estimate dramatically increases the economic significance. Column (5) in Panel B of Table 3 says removing the effects of health insurance coverage expansion would now see Trump win Georgia, Arizona, Wisconsin, Pennsylvania and Nevada. Flipping all of these states would have won him re-election.

Columns (8)-(9) of Table 4 treat COVID-19 prevalence as endogenous. The IV specification tests suggests the meat-packing worker IV may not work well (and the point estimate is far from statistical significance at conventional levels). The instrument appears weak with a Kleibergen-Paap F-stat of 1.89, far below the rule-of-thumb of 10. And, we cannot reject the null that the model is underidentified at the $p = 0.1$ level. In contrast, these tests suggest the nursing home IV approach works well: the Kleibergen-Paap F-stat is 55.542 and we easily reject the null of underidentification at the $p < .001$ level. Thus, we focus on interpreting the nursing home IV point estimate.

Perhaps surprisingly, the COVID-19 point estimate in column (8) of Table 4 is *positive* and statistically significant. This says that, relative to his 2016 vote share, Trump did *better* in counties with higher cumulative per capita COVID-19 deaths. This clearly goes against the common narrative that COVID-19 was an important factor explaining Trump's 2020 defeat. Moreover, the IV point estimate of 0.101 is around 30 times larger than the OLS point estimate of .003. This is very large economically. Column (6) of Panel B in Table 3 says that removing the pro-Trump effects of COVID-19 would change the close defeats in Georgia and Arizona of less than around 0.3% points into substantial defeats by around 1.75-2% points. One potential explanation revolves around the economy as the key electoral issue where Trump had a long-lasting edge over Biden: voters may have trusted Trump to better handle a COVID-wrecked economy.

Ultimately, our IV results support our OLS results. Indeed, given our host of control variables – social, economic, political and health controls – and fixed effects, our results actually suggest that US and foreign retaliatory trade war are not endogenous.

4.3 Robustness

Alternative COVID-19 measures. Our analysis has focused on cumulative COVID-19 deaths. Panel A of Table 5 explores other measures of COVID-19 cases and deaths. Given the varying results regarding the political impact of COVID-19 between the two IV specifications as well as between the IV and OLS specifications, we focus here on OLS specifications. Table A2 presents the IV results and do not change our key conclusions.

The most obvious alternative measure of COVID-19 prevalence is cumulative cases (per 1000 population) in column (2). But, it could also be that recent COVID-19 prevalence is most important in voters minds when voting. Thus, columns (3)-(4) use daily average deaths and cases in October (per 100,000 population). Alternatively, perhaps most important in voters minds is the peak extent of the pandemic in their local area. Thus, columns (5)-(6) use the county-specific maximum of 14-day rolling average deaths and cases. As with cumulative deaths, the other measures of COVID-19 prevalence are also largely uncorrelated with our trade war variables or health insurance coverage expansion. Thus, our results regarding the trade war and health insurance remain essentially unchanged. Moreover, we still do not find any evidence that COVID-19 hurt Trump politically.

Placebo specifications. Despite our attempts to control for county-level social, economic, health and political characteristics and despite our IV approaches, one may still worry that 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.

Panel B of Table 5 presents the results. Column (1) shows the OLS results. Column (2) uses the same Lewbel instruments as column (7) of Table 4 to instrument for the trade war variables and health insurance coverage expansion. Columns (3)-(4) instrument for COVID-19 prevalence using the nursing home and meat-packing workers instruments respectively. The Lewbel and nursing home instruments appear strong. But, again, the Sargan-Hansen endogeneity test cannot reject the null that our potential endogenous variables are actually exogenous ($p > 0.74$). Moreover, the point estimates are generally very imprecise, quite small, and sometimes differ in sign from the main analysis. This provides further evidence mitigating concerns that our results merely reflect pre-existing political trends.

Direct and indirect effects of COVID-19 Our analysis has estimated the effect of COVID-19 on voting behavior *conditional* on county-level social distancing and economic activity. This is the *direct* effect of COVID-19. But, COVID-19 may also *indirectly* impact voting behavior by affecting social distancing and economic activity.

We address this issue in Table A3. There, Panel B breaks the total effect of COVID-19 into direct and indirect effects. Importantly, the indirect effects reinforce the direct effects.[29] That is, the total effects are the same sign and larger in absolute magnitude than the direct effects. Thus, the indirect effects do not alter the central themes of our earlier results about the direct effects of COVID-19 on the election outcome.

4.4 Heterogeneity

We now explore various dimensions of heterogeneity in the key results from our baseline analysis. Four reasons lead us to focus this heterogeneity analysis on OLS estimation. First, the Sargan-Hansen test of endogeneity strongly suggested that US and foreign retaliatory trade war tariffs were exogenous given our set of controls and fixed effects. Second, while we did not have strong evidence of exoegneity for health insurance coverage expansion and COVID-19 prevalence, the OLS point estimates were notably smaller than the IV point estimates. Thus, our OLS results provides a more conservative assessment of economic magnitudes. Third, the fact that our key explanatory variables are uncorrelated with each other means any endogeneity problem with one key explanatory variables does not spill over to create other endogeneity problems. Fourth, the Lewbel IV approach is based on in-sample heteroskedasticity. Thus, the Lewbel instruments are specific to the particular sample and/or set of explanatory and control variables.

[29] Columns (1)-(3) of Panel A in Table A3 show the direct effects of COVID-19. Columns (4)-(12) of Panel A show the effect of COVID-19 on economic activity (business foot traffic and unemployment rate) and social distancing (MEI). The indirect effect of COVID-19 on voting behavior through, e.g., social distancing equals the point estimate of COVID-19 on MEI in columns (10)-(12) multiplied by the point estimate of MEI on voting behavior in columns (1)-(3).

4.4.1 Political heterogeneity

Our baseline results showed that voters rewarded Trump for protecting their local economy through US trade war tariffs but penalized him for the costs of foreign retaliation and undermining the post-ACA expansion of health insurance coverage. This is consistent with theme of Che et al. (2020) that voter behavior towards a politician reflects the economic impact of a politicians' actions (they argue voters moved towards Democrats in the 2000s because Democrats were more likely to vote against pro-trade Congressional bills). But, our baseline result could mask that, e.g., Trump benefited from US trade tariff war tariffs in solidly Republican counties but was hurt by US trade war tariffs in solidly Democrat counties.

Indeed, Autor et al. (2020) argue that rising Chinese import competition drove political polarization during the 2000s and 2010s. Specifically, they argue this happened either through hollowing out the political center or by pushing majority-white areas towards Republicans and majority-minority areas towards Democrats. Thus, we investigate whether the impacts of the trade war on voter behavior towards Trump are similar across political and racial lines or, instead, whether they polarize voters along political and racial lines.

Columns (2)-(4) of Panel A in Table 6 investigate political heterogeneity according to county-level 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. Despite some political heterogeneity in the point estimates, panel C of Table 3 shows these heterogeneities generally do not alter economic significance of the effects from panel A of Table 3 at the electoral college level. Moreover, we do not find evidence of political polarization. In particular, the effects in competitive counties are statistically insignificant and generally very small economically. And, US trade war tariffs have pro-Trump effects in both solidly Republican and solidly Democrat counties that are actually much stronger in solidly Democrat counties. Thus, the key issues did not hollow out competitive counties or simultaneously solidify already solidly Republican and Democrat counties.

Columns (5)-(6) of Panel A in Table 6 proxy for political heterogeneity by whether the county voted for Trump or Hillary Clinton in 2016. Overall, panel D of Table 3 shows these heterogeneities generally do not alter the economic significance of the effects from panel A at at the electoral college level. Moreover, we do not find evidence of political polarization. Again, the sign of the point estimates for our key issues are the same for both Trump and Clinton counties. Thus, the key issues did not simultaneously push voters towards Trump in Trump counties but away from Trump in Clinton counties.

An important result from our political heterogeneity analysis is the much stronger effect of health insurance coverage expansion in Clinton counties than Trump counties. This has strong implications for economic significance. Absent the effects of health insurance coverage expansion, column (5) of panel D in Table 3 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, Cobb 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.

Finally, we look at political heterogeneity along racial lines. Like Autor et al. (2020), columns (7)-(8) in Panel A of Table 6 split our county sample depending on whether they have a majority non-Hispanic white population or a majority population of minorities. The closest evidence for a polarization mechanism is via foreign retaliation. Its effect is negative, statistically significant and economically large in majority minority counties but positive, although statistically insignificant and economically small, in majority white counties. However, Trump actually increased his 2020 vote share substantially in these majority minority counties relative to 2016 (a median increase of 0.58% points). So, these majority minority counties were not generally shifting towards Biden as suggested by a polarization story.

Ultimately, regardless of the way we look at political heterogeneity, we do not find evidence for political polarization as an underlying mechanism through which our key issues affect voter behavior. Our results instead suggest voters responded similarly across political and racial lines to the economic effects of Trump's policies on their local economies.

4.4.2 Trade war heterogeneity.

The trade war initiated by the Trump administration in spring 2018 was eventually dominated by the US-China piece of the trade war. Thus, one may wonder whether the prominence of the US-China trade war leads voters to focus less on other aspects of the trade war such as the national security tariffs on steel and aluminum.

Column (2) in panel B of Table 6 isolates the effect of the US-China trade war. Here, the US and foreign retaliatory tariff shocks are defined *solely* by, respectively, US tariffs on China and Chinese tariffs on the US. The point estimates imply the median county saw Trump's 2020 vote share increase by 0.07% points due to US tariffs on China and decrease by 0.054% points due to Chinese tariffs. These effects are somewhat lower than the 0.13% and 0.06% points in our baseline analysis. Indeed, according to column (2) in panel F of Table 3, the effects of US tariffs are sufficiently weaker that removing their pro-Trump effect would still leave Trump in recount territory in Georgia and Wisconsin. Thus, these results indicate the overall trade war, and not just the US-China trade war, impacted voter behavior.

Naturally, the trade war dominated media headlines throughout 2018 as Trump progressively ratcheted up tariffs. He was ratcheting up tariffs on various trading partners – not only China but allies like the EU, Canada and Mexico – and for various reasons – national security concerns over steel and aluminum imports and concerns over US intellectual property rights in China. Thus, one may wonder whether voters paid less attention to subsequent rounds of the trade war through 2019. Alternatively, perhaps these later tariffs were fresher in voter minds in the 2020 Presidential election campaign.

Column (3) of panel B in Table 6 only looks at the tariffs imposed during 2018. The US and foreign retaliatory tariff shocks exclude the escalation in early summer 2019 and the new tariffs in fall 2019. The point estimates imply the median county saw Trump's 2020 vote share increase by 0.09% points due to US tariffs on China and decrease by 0.053% points due to Chinese tariffs. Again, these are somewhat lower than our baseline analysis. The effect of US tariffs is sufficiently lower that column (2) of panel G in Table 3 says removing its pro-Trump effect leaves Trump in recount territory in Georgia. Thus, these results indicate the overall trade war, and not just the 2018 trade war, impacted voter behavior.

4.4.3 Heterogeneity by COVID-19 prevalence.

One may wonder whether the political salience of the issues – trade war, health insurance coverage, and COVID-19 – was higher in areas with greater prevalence of COVID-19. Perhaps, COVID-19 only had negative consequences for Trump in places that had severe COVID-19 outbreaks. Or, perhaps the anti-Trump effect of health insurance coverage expansion reflected particularly strong concerns over health insurance coverage among voters in areas that had large COVID-19 outbreaks.

Columns (4)-(6) of panel B split counties into terciles of COVID-19 prevalence. The point estimates reveal no stark heterogeneities across the terciles. Panels A and H of Table 3 also show that taking this heterogeneity into account does not impact the economic significance of the issues in terms of electoral college outcomes.

5 Conclusion

Understanding the political economy of the 2020 US Presidential election is important because the controversy surrounding the outcome itself often overshadows the substantial impact of various Trump administration policies on voters. 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 in handling a post-COVID ravaged economy.

Instead, our results emphasize the political salience of the trade war and the post-ACA expansion of health insurance coverage. Voters rewarded Trump for protecting their local economy via US trade war tariffs. But, they penalized Trump for foreign retaliation that hurt their local economy and for undermining the post-ACA expansion of local health insurance coverage. Absent the pro-Trump effect of US trade war tariffs, our results imply Trump would not have been close enough to force recounts in Georgia or Wisconsin. Absent the anti-Trump effects of health insurance coverage expansion, our results imply Trump would have won Georgia, Arizona, Nevada, and would have only lost Wisconsin by a few thousand votes. He would have needed just one more state, e.g. Wisconsin, for re-election. Thus, the trade war and health insurance coverage expansion had important effects on the election outcome.

Trump was undoubtedly a uniquely polarizing US President. This leads to a natural question: could the mechanism behind our results operate through a polarization channel whereby Trump's policies and actions hardened both Republican support for him and Democrat anger against him? Our results say the answer is no. They show that county-level voter behavior was not qualitatively different across political or racial lines in response to the US trade war tariffs or health insurance coverage expansion. Indeed, the pro-Trump effect of US tariffs was strongest in counties that were solidly Democrat and counties Hillary Clinton won in 2016. Instead, our results suggest that voter behavior responded to the effects of Trump's policies on local economic outcomes.

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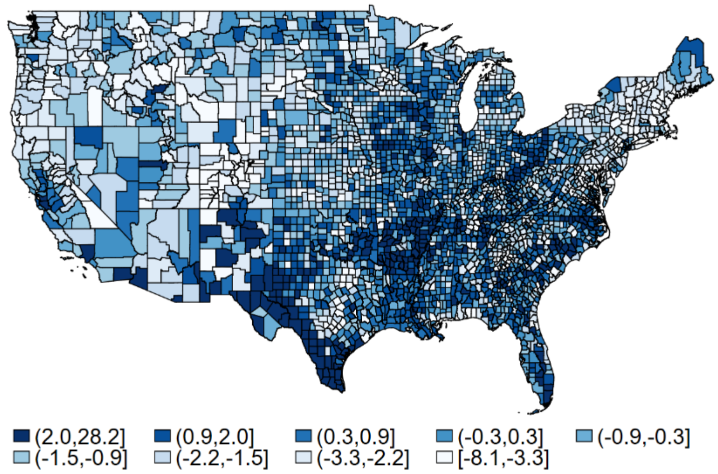
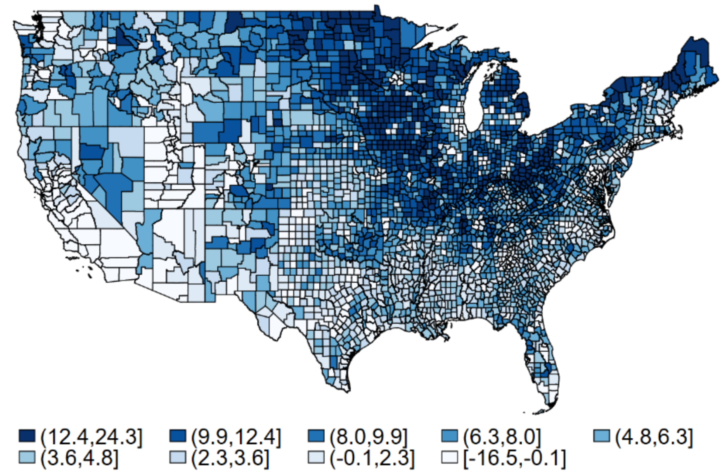
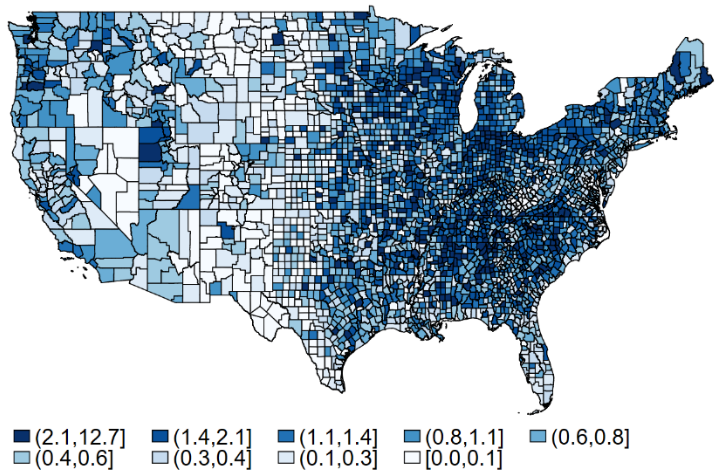


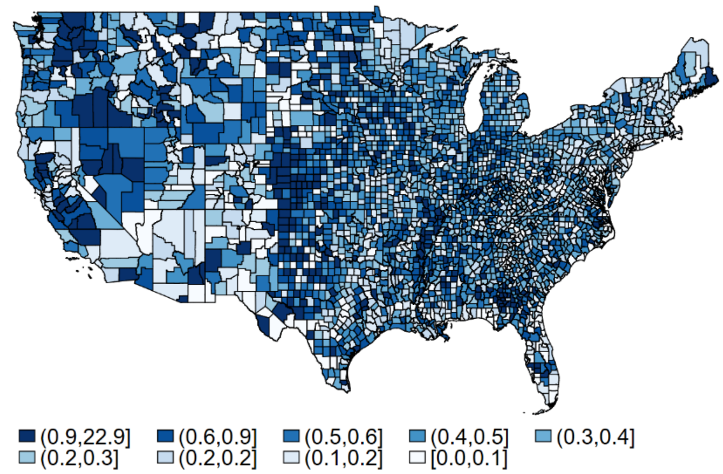
A. Change in 2-party Republican vote share 2016-2020 (% pts)



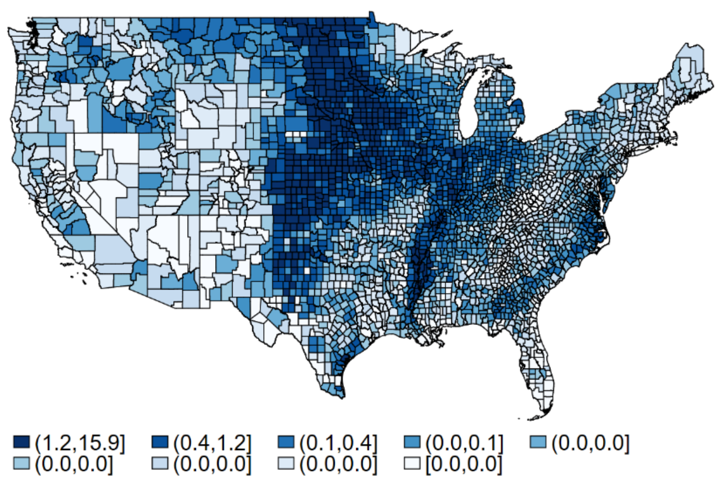
B. Change in 2-party Republican vote share 2012-2016 (% pts)



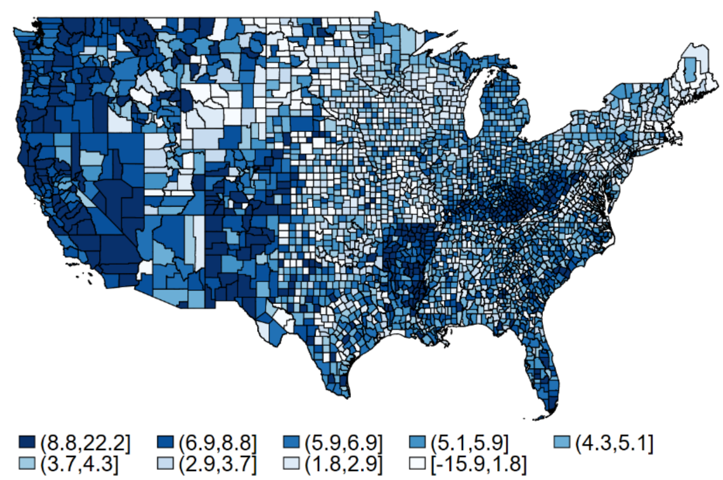
C. US trade war tariff shock ($000s per worker)



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



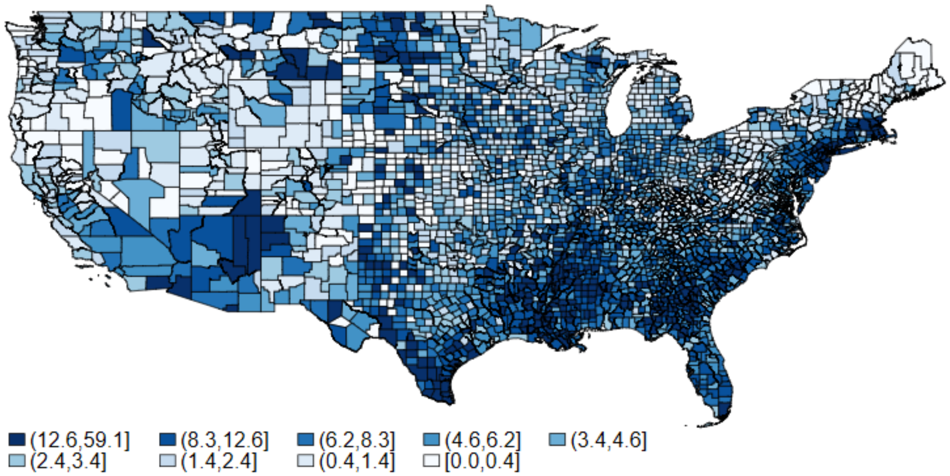
E. Agricultural subsidies ($000s per worker)



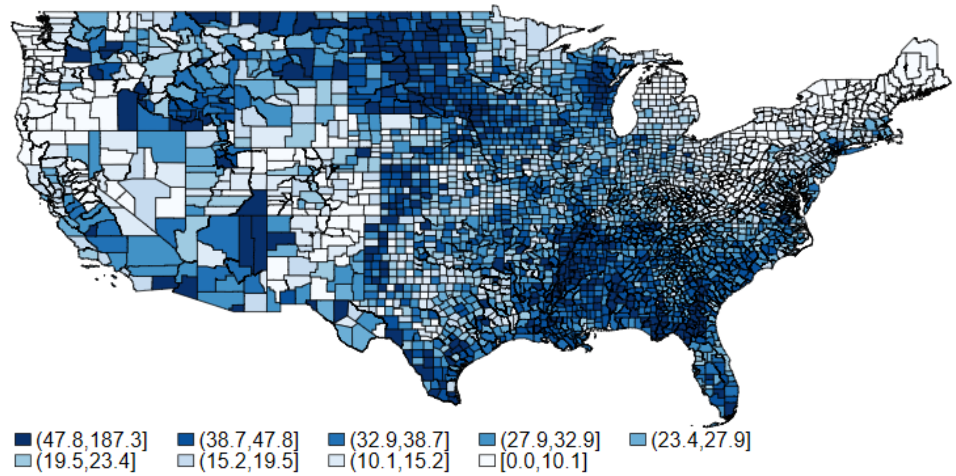
F. Health insurance coverage expansion (2013-2018, % pts)

Figure 1: Presidential voting outcomes, trade war variables, and health insurance coverage expansion

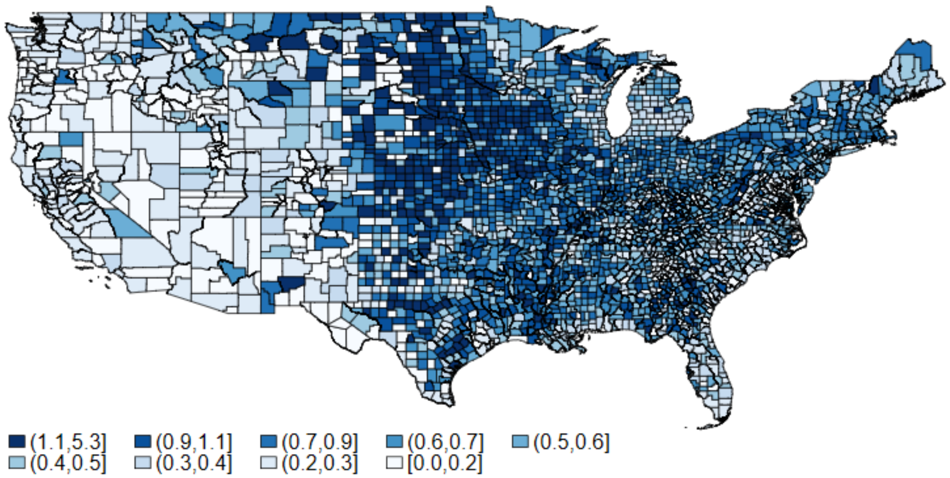
Notes: Maps represent the 3108 mainland US counties. Presidential voting data from David Leip's Election Atlas; 2020 election data is version 0.9 (official release of data for all states). Table 1 describes data sources for trade war tariffs. Agricultural subsidies data from Blanchard et. al. (2019). Health insurance coverage expansion is difference between coverage in 2018 and 2013 Census 5-year ACS. See main text for further details.



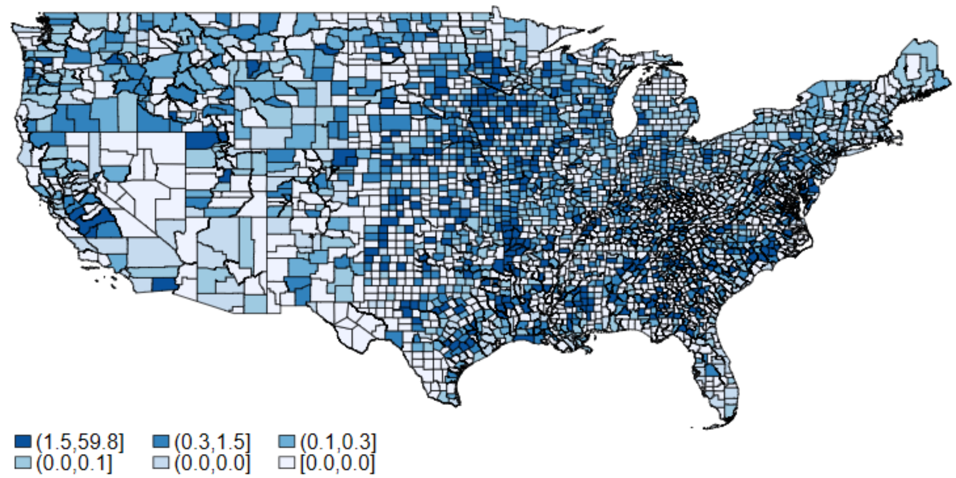
A. COVID-19 cumulative deaths (per 10,000 population)



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



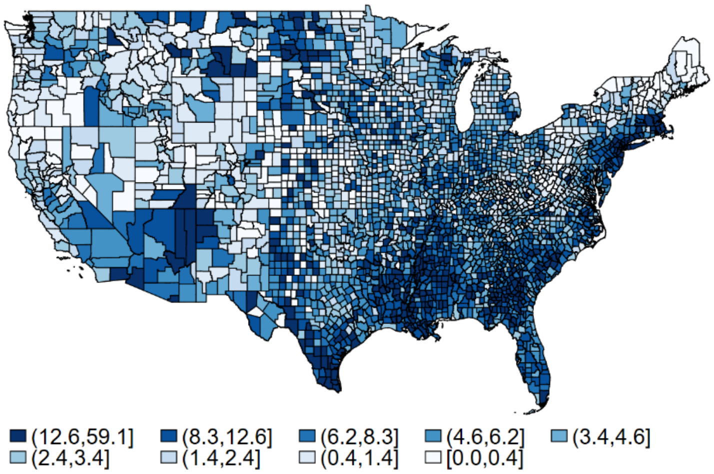
C. Nursing home resident population share (2016, % pts)



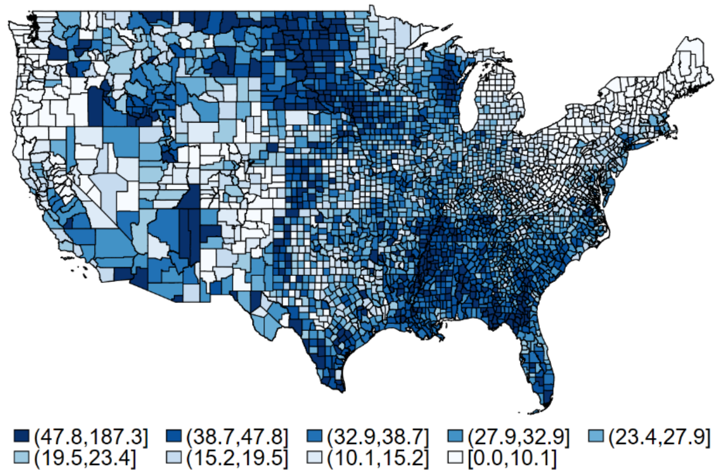
D. Meat-packing worker employment share (% pts)

Figure 2: COVID-19 prevalence and COVID-19 instruments

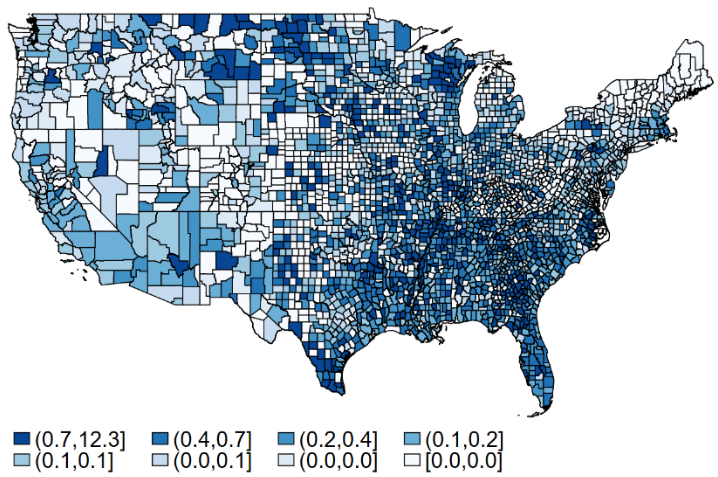
Notes: Maps represent the 3108 mainland US counties. COVID-19 data source is COVID County Data (https://covidcountydata.org/). Population is 2018 population from 2018 5-year Census ACS. Nursing home resident population share uses 2016 data from The Centers for Medicare & Medicaid Services and 2016 population data from 2016 5-year ACS. Meat-packing worker employment share is the annual average over 2012-2016 using County Business Patterns data. See main text for further details.



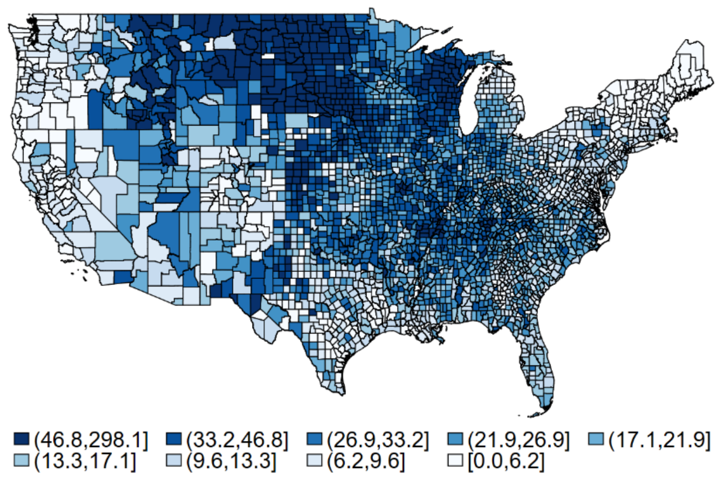
A. COVID-19 cumulative deaths (per 10,000 population)



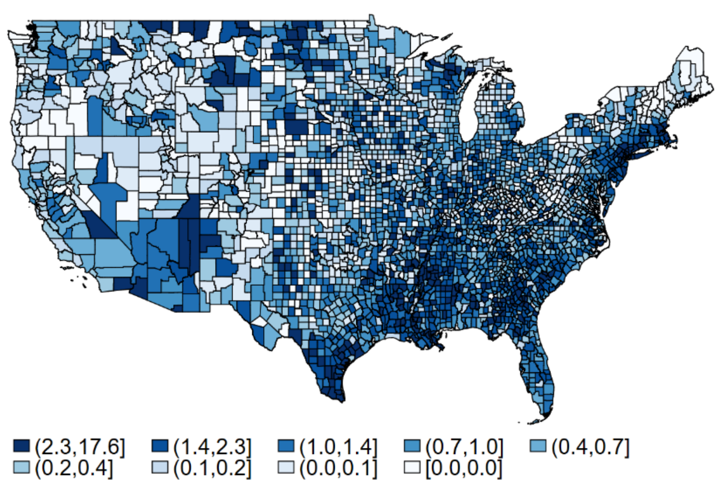
B. COVID-19 cumulative cases (per 1000 population)



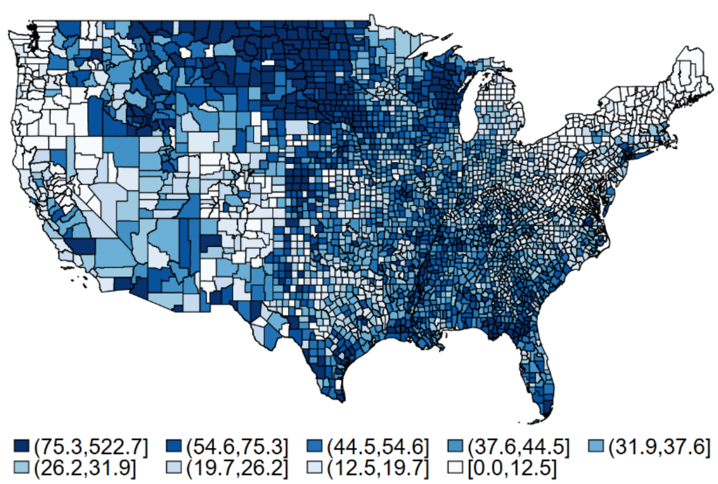
C. COVID-19 October deaths (daily average per 100,000 pop.)



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



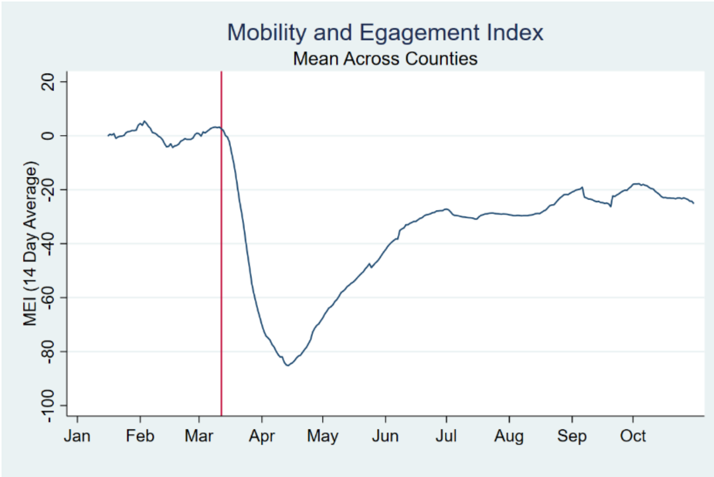
E. COVID-19 deaths (max 14-day average, per 100,000 pop.)



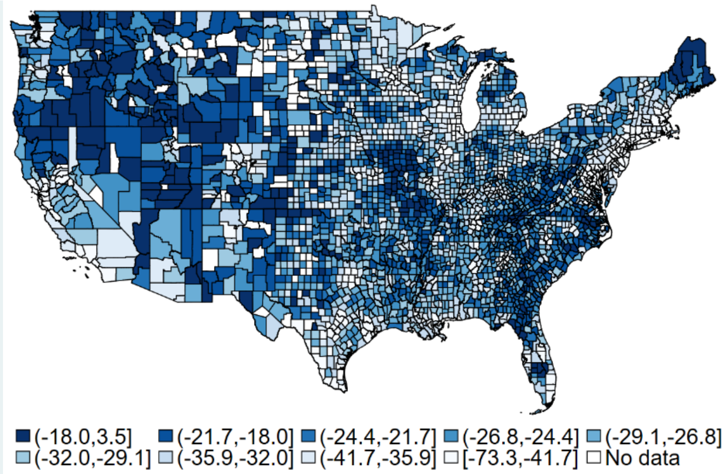
F. COVID-19 cases (max 14-day average, per 100,000 pop.)

Figure A1: Alternative measures of COVID-19 prevalence

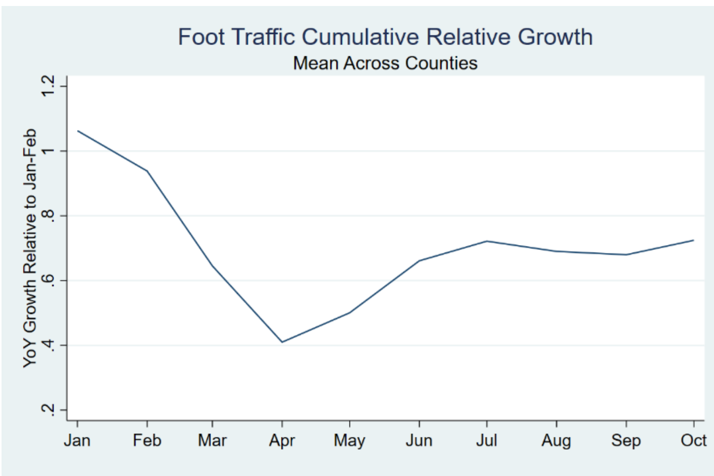
Notes: Maps represent the 3108 mainland US counties. COVID-19 data source is COVID County Data (https://covidcountydata.org/). Population is 2018 population from 2018 5-year Census ACS. Panels A-B cumulative data is through October 31, 2020. Panels E-F are county-level maximum 14-day rolling averages through October 31, 2020. See main text for further details.



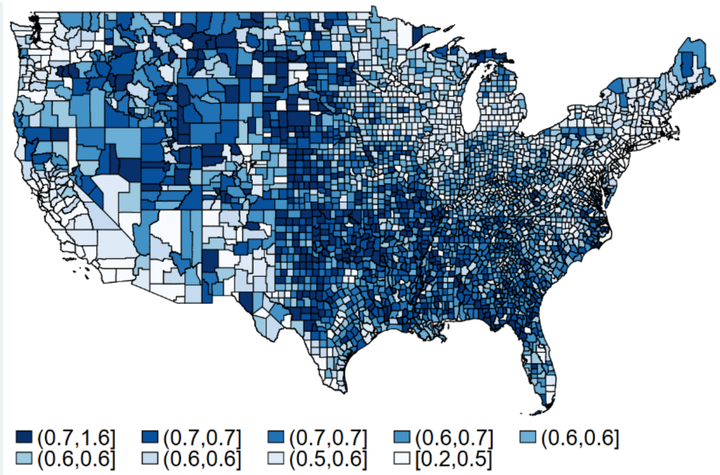
A. Daily MEI: 1/1/2020-10/31/2020



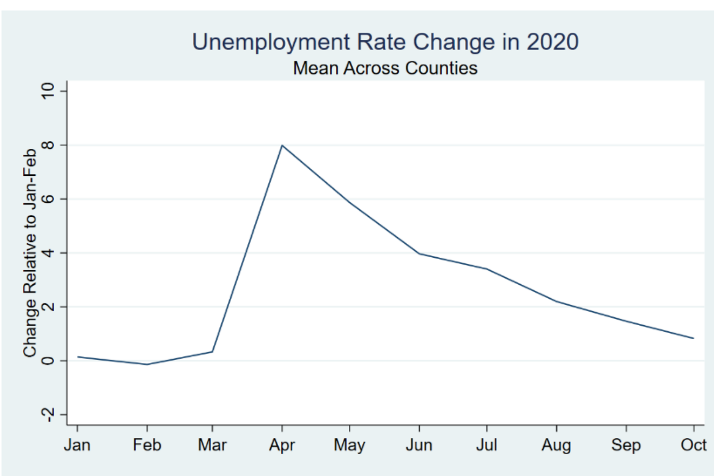
B. MEI daily average (1/1/2020-10/31/2020)



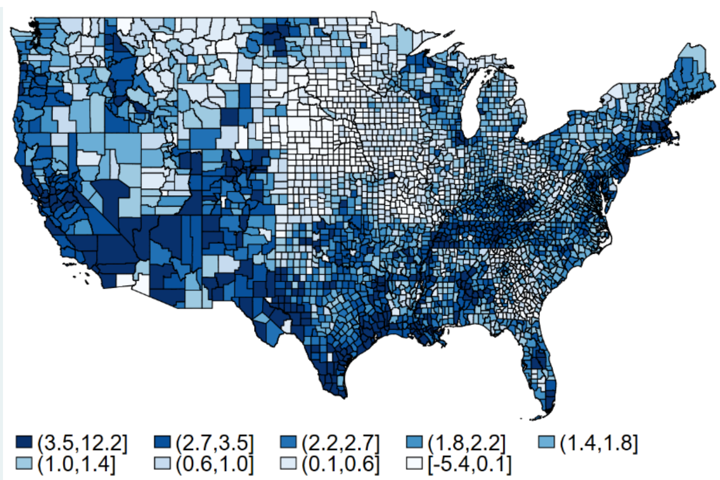
C. Foot traffic relative growth



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 3108 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. Vertical 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 1. Trade war tariffs

	Date Imposed	Affected products	Tariffs	Source: Products	Source: Tariffs
A. US trade war tariffs					
Section 201 Safeguard Tariffs	February 2018	Washing Machines & Solar Panels	30-42.8%	USITC (2017a, b)	USITC (2017a, b)
Section 232 National Security Tariffs	March 2018	Steel and Aluminum	10-25%	US Dept. of Commerce (2018a, b)	US Dept. of Commerce (2018a, b)
Section 301 Unfair Trade Practices Tariffs	July 2018	China Imports List 1: $34bn	25%	Bown (2019a)	Bown (2019a)
	August 2018	China Imports List 2: $16bn	25%	Bown (2019a)	Bown (2019a)
	September 2018	China Imports List 3: $200bn	25%	Bown (2019a)	Bown (2020)
	September 2019	China Imports List 4A: $121bn	15%	Bown (2019a)	Bown (2020)
B. Foreign retaliatory trade war tariffs					
China Section 232	April 2018		15-25%	Lu & Schott (2018)	Lu & Schott (2018)
EU Section 232	June 2018		10-25%	Bown et al (2018c)	Bown et al (2018c)
Canada Section 232	July 2018		10-25%	Bown et al (2018a)	Bown et al (2018a)
Mexico Section 232	July 2018		5-25%	https://rb.gy/00bztl	https://rb.gy/00bztl
China List 1 -- Section 301	July 2018		5-35%	Bown et al (2018b)	Bown et al (2018b)
China List 2 -- Section 301	August 2018		5-35%	https://rb.gy/7t6rkq	https://rb.gy/7t6rkq
China List 3 -- Section 301	September 2018		5-35%	Bown et al (2018d)	Bown et al (2018d)
China List 4A -- Section 301	September 2019		5-35%	Bown (2019b)	Bown (2019b)

Notes: US Section 201 weighted average tariff on washing machines is 42.8%. US Section 232 tariffs are 25% on steel and 10% on aluminum. US Section 301 tariffs China tariffs under List 3 were initially 10% in September 2018 but raised to 25% in June 2019 (we use the 25% tariff in our analysis). For Section 301 foreign retaliatory tariffs by China, their List 3 and 4A tariffs can increase earlier List 1 and 2 tariffs (in these cases, we use the List 3 and 4 tariff rates in our analysis).

Table 2. Baseline results

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ 2-party Rep. vote Share 2012-2016	0.038	0.036	0.032	0.097	0.205*	0.210*	0.209*	0.217*
	(0.062)	(0.061)	(0.062)	(0.063)	(0.035)	(0.035)	(0.035)	(0.032)
US tariff shock	-0.048	-0.064	-0.064	0.160#	0.203*	0.193*	0.205*	0.186*
	(0.291)	(0.289)	(0.288)	(0.084)	(0.054)	(0.052)	(0.052)	(0.050)
Retalitory tariff shock		0.144	0.097	-0.189	-0.282^	-0.248^	-0.268^	-0.193#
		(0.160)	(0.160)	(0.123)	(0.133)	(0.106)	(0.105)	(0.104)
Agricultural subsidies			0.440^	0.528*	0.356^	0.425*	0.401*	0.501*
			(0.198)	(0.189)	(0.155)	(0.131)	(0.126)	(0.129)
Δ Health insurance coverage						-0.114^	-0.106#	-0.079#
						(0.055)	(0.054)	(0.046)
COVID-19 deaths (cum., per 10k pop.)							0.054*	0.003
							(0.015)	(0.017)
N	3112	3112	3112	3112	3111	3111	3111	2991
Non-COVID controls	N	N	N	Y	Y	Y	Y	Y
State FE	N	N	N	N	Y	Y	Y	Y
COVID controls	N	N	N	N	N	N	N	Y

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. All specifications weighted by 2020 total Presidential votes cast. Standard errors clustered by state. See Appendix Table A1 for list of COVID controls and non-COVID controls. 2013 level of health insurance coverage included from column (6) onwards. See main text for further details.

Table 3. Counterfactual two-party vote share margin (% points)

	(1)	(2)	(3)	(4)	(5)	(6)
A. Baseline						
		Counterfactual: removing effects of …				
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion	COVID-19
Nevada	-2.45	-2.57	-2.41	-2.45	-0.90	-2.48
Pennsylvania	-1.20	-1.53	-1.13	-1.21	-0.64	-1.24
Wisconsin	-0.64	-1.19	-0.50	-0.70	-0.13	-0.66
Arizona	-0.31	-0.50	-0.25	-0.32	0.62	-0.37
Georgia	-0.24	-0.53	-0.15	-0.25	0.57	-0.28
North Carolina	1.37	1.00	1.45	1.34	2.17	1.34
B. IV						
		Counterfactual: removing effects of …				
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion	COVID-19
Nevada	-2.45	-2.54	-2.41	-2.45	1.45	-3.65
Pennsylvania	-1.20	-1.45	-1.14	-1.20	0.21	-2.60
Wisconsin	-0.64	-1.06	-0.53	-0.64	0.65	-1.33
Arizona	-0.31	-0.46	-0.27	-0.31	2.03	-2.04
Georgia	-0.24	-0.46	-0.17	-0.24	1.80	-1.74
North Carolina	1.37	1.09	1.43	1.37	3.38	0.55
C. Political heterogeneity: competitiveness						
		Counterfactual: removing effects of …				
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion	COVID-19
Nevada	-2.45	-2.58	-2.44	-2.45	-0.62	-2.92
Pennsylvania	-1.20	-1.48	-1.19	-1.20	-0.75	-1.38
Wisconsin	-0.64	-1.13	-0.61	-0.68	-0.25	-0.65
Arizona	-0.31	-0.39	-0.31	-0.32	0.02	0.09
Georgia	-0.24	-0.45	-0.22	-0.24	0.40	-0.41
North Carolina	1.37	0.99	1.39	1.35	2.04	1.25
D. Political heterogeneity: Trump vs Clinton counties						
		Counterfactual: removing effects of …				
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion	COVID-19
Nevada	-2.45	-2.65	-2.44	-2.45	0.49	-2.72
Pennsylvania	-1.20	-1.52	-1.18	-1.21	-0.46	-1.45
Wisconsin	-0.64	-1.08	-0.60	-0.67	-0.07	-0.72
Arizona	-0.31	-0.45	-0.30	-0.32	0.34	-0.48
Georgia	-0.24	-0.49	-0.21	-0.24	0.90	-0.46
North Carolina	1.37	1.04	1.39	1.35	2.33	1.25

Table 3 (cont). Counterfactual two-party vote share margin (% points)

	(1)	(2)	(3)	(4)	(5)	(6)
E. Political heterogeneity: race						
		Counterfactual: removing effects of …				
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion	COVID-19
Nevada	-2.45	-2.57	-2.42	-2.45	-1.10	-3.12
Pennsylvania	-1.20	-1.42	-1.20	-1.20	-0.98	-1.29
Wisconsin	-0.64	-1.00	-0.66	-0.66	-0.51	-0.58
Arizona	-0.31	-0.44	-0.29	-0.32	-0.03	-0.35
Georgia	-0.24	-0.51	-0.19	-0.24	0.33	-0.58
North Carolina	1.37	1.07	1.40	1.36	1.72	1.29
F. Trade war heterogeneity: China trade war only						
		Counterfactual: removing effects of …				
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion	COVID-19
Nevada	-2.45	-2.53	-2.41	-2.45	-0.91	-2.47
Pennsylvania	-1.20	-1.39	-1.14	-1.21	-0.65	-1.23
Wisconsin	-0.64	-0.97	-0.52	-0.70	-0.13	-0.65
Arizona	-0.31	-0.43	-0.26	-0.32	0.61	-0.35
Georgia	-0.24	-0.43	-0.15	-0.25	0.57	-0.27
North Carolina	1.37	1.13	1.44	1.34	2.16	1.35
G. Trade war heterogeneity: 2018 trade war only						
		Counterfactual: removing effects of …				
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion	COVID-19
Nevada	-2.45	-2.56	-2.41	-2.45	-0.86	-2.49
Pennsylvania	-1.20	-1.52	-1.13	-1.21	-0.63	-1.25
Wisconsin	-0.64	-1.17	-0.51	-0.70	-0.11	-0.66
Arizona	-0.31	-0.48	-0.26	-0.32	0.64	-0.37
Georgia	-0.24	-0.44	-0.15	-0.25	0.59	-0.29
North Carolina	1.37	1.10	1.44	1.34	2.19	1.34
H. Heterogeneity by COVID prevalence						
		Counterfactual: removing effects of …				
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion	COVID-19
Nevada	-2.45	-2.53	-2.41	-2.45	-0.91	-2.47
Pennsylvania	-1.20	-1.47	-1.15	-1.21	-0.65	-1.23
Wisconsin	-0.64	-0.99	-0.52	-0.70	-0.13	-0.65
Arizona	-0.31	-0.52	-0.26	-0.32	0.61	-0.35
Georgia	-0.24	-0.50	-0.15	-0.25	0.57	-0.27
North Carolina	1.37	1.14	1.44	1.34	2.16	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: column (8) of Table 1 for Panel A, column (7) of Table 4 for Panel B, columns (2)-(4) from Panel A of Table 6 for Panel C, columns (5)-(6) from Panel A of Table 6 for Panel D, columns (7)-(8) from Panel A of Table 6 for Panel E, columns (2)-(3) from Panel B of Table 6 for Panels F-G, and columns (4)-(6) from Panel B of Table 6 for Panel H. See main text for more details.

Table 4. Instrumental variables estimation

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
US tariff shock	0.186*	0.124#	0.183*	0.189*	0.126#	0.178*	0.142^	0.209*	0.113
	(0.050)	(0.073)	(0.050)	(0.045)	(0.072)	(0.046)	(0.069)	(0.050)	(0.085)
Retalitory tariff shock	-0.193#	-0.185#	-0.192^	-0.191#	-0.190^	-0.130	-0.149^	-0.211^	-0.136
	(0.104)	(0.106)	(0.093)	(0.101)	(0.090)	(0.083)	(0.072)	(0.101)	(0.135)
Agricultural subsidies	0.501*	0.489*	0.490*	0.122	0.167	0.549*	0.010	0.437*	0.694*
	(0.129)	(0.127)	(0.126)	(0.201)	(0.191)	(0.120)	(0.147)	(0.123)	(0.223)
Δ Health insurance coverage	-0.079#	-0.082#	-0.091^	-0.075#	-0.084^	-0.191^	-0.199*	-0.078#	-0.082
	(0.046)	(0.046)	(0.035)	(0.045)	(0.036)	(0.080)	(0.057)	(0.046)	(0.051)
COVID-19 deaths	0.003	0.003	0.004	0.005	0.006	-0.008	-0.003	0.101#	-0.298
(cum., per 10k pop.)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.016)	(0.016)	(0.052)	(0.236)
N	2991	2991	2991	2991	2991	2991	2991	2991	2991
Endogenous variables	None	US tariffs	Foreign tariffs	Agric. subsidies	Trade war variables	Health insurance	Trade war and health insurance	COVID-19 deaths	COVID-19 deaths
Instruments		Lewbel	Lewbel	Lewbel	Lewbel	Lewbel	Lewbel	Nursing Home	Meat Packing Workers
Underidentification p-value		0.001	0.069	0.045	0.035	0.002	0.081	0.000	0.159
K-P weak instrument rk F-statistic		52.078	61.129	22.663	10.795	48.668	13.283	55.542	1.824
Overidentification p-value		0.618	0.693	0.982	0.897	0.490	0.806		
Sargan-Hansen endogeneity p-value		0.458	0.884	0.093	0.217	0.099	0.02	0.049	0.321

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 in column (1), IV-GMM in columns (2)-(7) and IV in columns (8)-(9). All specifications weighted by 2020 total Presidential votes cast. Standard errors clustered by state. Lewbel instruments in columns (2)-(7) created by demeaning and multiplying the following variables by the first stage residuals: manufacturing employment share, and the change in the 2-party Republican vote share between the 2012 and 2016 US Presidential election in column (2); employment share in agricultural and mining, and 2016 population share of naturalized citizens in column (3); employment share in agricultural and mining, percent diabetic with annual eye test, and MEI daily average (1/1/2020-10/31/2020) in column (4); 2013 health insurance coverage, percent diabetic with annual lipids test, percent diabetic with annual hemoglobin test, and foot traffic cumulative relative growth in column (5); instruments from columns (2)-(4) in column (6); instruments from columns (2)-(5) in column (7). Nurshing home instrument used in column (8). Meat-packing worker instrument used in column (9). See main text for further details.

Table 5. Robustness specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Alternative COVID-19 prevalence definitions (OLS)						
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)
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)
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)
N	2991	2991	2991	2991	2991	2991
COVID-19 prevalence definition	Cumulative Deaths	Cumulative Cases	October Deaths	October Cases	Peak Deaths	Peak Cases
Panel B. Placebo specification						
US tariff shock	-0.063	0.036	-0.064	-0.08		
	(0.080)	(0.106)	(0.086)	(0.100)		
Retalitory tariff shock	-0.046	-0.097	-0.044	-0.032		
	(0.073)	(0.077)	(0.078)	(0.091)		
Agricultural subsidies	0.949*	0.999*	0.953*	0.994*		
	(0.289)	(0.343)	(0.281)	(0.352)		
Δ Health insurance coverage	0.023	0.025	0.023	0.023		
	(0.060)	(0.122)	(0.061)	(0.061)		
COVID-19 deaths	-0.011	0.007	-0.018	-0.084		
(cum., per 10k pop.)	(0.027)	(0.024)	(0.134)	(0.289)		
N	2991	2991	2991	2991		
Endogenous variables	None	Trade war Health insurance	COVID-19			
Instruments		Lewbel	Nursing home	Meat-packing workers		
Underidentification p-value		0.076	0.000	0.160		
K-P weak instrument rk F-statistic		14.437	56.070	1.818		
Overidentification p-value		0.022		0.000		
Sargan-Hansen endogeneity p-value		0.747	0.957	0.807		

Notes: # p<0.10, ^ p<.05, * p<.01. Dependent variable in Panel A is the change in the 2-party Republican vote share between the 2016 and 2020 US Presidential election. Dependent variable in Panel B 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 Panel A and column (1) of Panel B, IV-GMM in column (2) of Panel B, IV in columns (3)-(4) of Panel B. In all specifications: full set of controls and fixed effects as in column (8) of Table 1, regressions weighted by 2020 total Presidential votes cast, standard errors clustered by state. October deaths and cases in columns (3)-(4) of Panel A are daily October averages per 100,000 population. Peak deaths and cases in columns (5)-(6) of Panel A are county-level maximum 14-day rolling averages through October 31, 2020 per 100,000 population. Lewbel instruments in column (2) of Panel B are those from column (7) of Table 3. Nursing home instrument and meat-packing instrument used in columns (3)-(4) of Panel B respectively. See main text for further details.

Table 6. Heterogenous effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Political heterogeneity								
US tariff shock	0.186*	0.075#	0.602*	0.056	0.083^	0.382#	0.120^	0.354
	(0.050)	(0.040)	(0.197)	(0.057)	(0.036)	(0.206)	(0.046)	(0.258)
Retalitory tariff shock	-0.193#	-0.017	-0.189	0.015	-0.057	-0.042	0.027	-0.627#
	(0.104)	(0.047)	(0.277)	(0.175)	(0.062)	(0.192)	(0.068)	(0.349)
Agricultural subsidies	0.501*	0.169^	1.219^	0.286	0.157	0.863#	0.184	0.284
	(0.129)	(0.081)	(0.489)	(0.299)	(0.106)	(0.465)	(0.140)	(0.487)
Δ Health insurance cov.	-0.079#	-0.031	-0.124	-0.023	-0.034	-0.165#	-0.019	-0.091
	(0.046)	(0.030)	(0.076)	(0.053)	(0.030)	(0.092)	(0.032)	(0.096)
COVID-19	0.003	0.005	0.054^	-0.034	0.007	0.024	-0.009	0.070#
	(0.017)	(0.009)	(0.024)	(0.025)	(0.010)	(0.029)	(0.018)	(0.040)
N	2991	1981	305	694	2515	471	2702	281
Heterogeneity type		Competitiveness			2016 results		Racial	
Sample	All	Solid Republican	Solid Democrat	Competitive	Trump counties	Clinton counties	Majority white	Majority non-white

	(1)	(2)	(3)	(4)	(5)	(6)
Panel B. Heterogeneity by dimensions of trade war and COVID prevalence						
US tariff shock	0.186*	0.132^	0.349*	0.108#	0.09	0.215*
	(0.050)	(0.051)	(0.083)	(0.060)	(0.070)	(0.066)
Retalitory tariff shock	-0.193#	-0.215^	-0.242^	-0.114	-0.281^	-0.046
	(0.104)	(0.106)	(0.118)	(0.111)	(0.111)	(0.096)
Agricultural subsidies	0.501*	0.502*	0.502*	0.068	0.439^	0.36
	(0.129)	(0.130)	(0.128)	(0.130)	(0.194)	(0.215)
Δ Health insurance cov.	-0.079#	-0.078#	-0.081#	-0.029	0.002	-0.011
	(0.046)	(0.046)	(0.045)	(0.035)	(0.045)	(0.045)
COVID-19	0.003	0.002	0.003	-0.106	-0.006	0.001
	(0.017)	(0.017)	(0.017)	(0.097)	(0.068)	(0.021)
N	2991	2991	2991	919	984	1081
Heterogeneity type		Trade war		COVID-19 prevalence		
Sample	All	US-China trade war	2018 trade war	Bottom tercile	Middle tercile	Top tercile

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. In all specifications: full set of controls and fixed effects as in column (8) of Table 1, regressions weighted by 2020 total Presidential votes cast, standard errors clustered by state. In columns (2)-(4) of Panel A: competitive counties have 2012 and 2016 Republican 2-party Presidential vote share between 45% and 55%, and solid Republican (Democrat) counties have these vote shares above 55% (below 45%). In columns (5)-(6) of Panel A, Trump (Clinton) counties are counties that Trump (Clinton) won in 2016. In columns (7)-(8), majority white (non-white) have majority white non-Hispanic (non-white and hispanic) population in 2016. In column (2) of Panel B, US tariffs and foreign retaliatory tariff shocks computed based only on 2018 trade war tariffs. In column (3) of Panel B, US (foreign retaliatory) tariff shocks computed based only on US (China) tariffs on China (US). In columns (4)-(6), COVID-19 terciles based on cuimulative COVID-19 deaths per 10,000 population.

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
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
Health insurance variables					
Change in health insurance coverage (2013 to 2018)	5.05	3.28	-15.90	22.20	3,112
Health insurance coverage (2013)	84.95	5.59	52.70	97.60	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 peak (per 100k, max 14-day rolling daily average)	0.97	1.34	0.00	17.60	3,112
Cases peak (per 100k, max 14-day rolling daily average)	41.71	33.39	0.00	522.72	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
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

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

	Mean	SD	Min	Max	N
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)					
Unemployed	4.01	1.65	0.00	18.80	3,112
Not in labor force	41.29	7.90	19.60	85.50	3,112
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

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

	Mean	SD	Min	Max	N
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
COVID-19 controls					
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
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
Remote workers (2016, share of emp)	0.31	0.05	0.22	0.65	3,112
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

Notes: See main text for further details.

Table A2. 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. 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
Sargan-Hansen endogeneity p-value	0.049	0.036	0.056	0.040	0.078	0.042
Panel B. Meat packing worker 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
Sargan-Hansen endogeneity p-value	0.321	0.363	0.306	0.317	0.272	0.362

Notes: Notes: # p<0.10, ^ p<.05, * p<.01. Panels A-B report the IV results from the OLS specifications in Panel A of Table 5 in main text. See notes to Table 5 for further details.

Table A3. Direct and indirect effects of COVID-19 on voting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Direct and indirect channels of COVID-19												
	Dependent variable											
	Republican vote share change 2016-2020			Business foot traffic			Unemployment rate change			MEI		
COVID-19 deaths	0.003	-0.298	0.101#	0.001^	-0.005	0.002	0.027*	-0.598	0.079#	-0.168*	1.146	0.078
(cum., per 10k pop.)	(0.017)	(0.236)	(0.052)	0.000	(0.007)	(0.001)	(0.010)	(0.495)	(0.043)	(0.043)	(1.120)	(0.173)
Business foot traffic	-1.677^	-0.189	-2.164*									
	(0.01)	(0.04)	(0.01)									
Unemployment rate change	0.202^	0.305^	0.168^									
	(0.708)	(1.614)	(0.700)									
MEI	-0.022	-0.054	-0.011									
	(0.081)	(0.143)	(0.080)									
Underidentification p-value		0.159	0.000		0.166	0.000		0.222	0.000		0.200	0.000
K-P weak instrument rk F-statistic		1.824	55.542		1.764	55.870		1.357	56.209		1.506	47.704
Instrument		Meat Packing	Nursing Home		Meat Packing	Nursing Home		Meat Packing	Nursing Home		Meat Packing	Nursing Home

Panel B. Direct and indirect effects of COVID-19 on voting

	Estimation method		
	OLS	IV : Meat Packing	IV: Nursing Home
Direct effect of COVID	0.003	-0.298	0.101
Indirect effects of COVID via...			
Business foot traffic	-0.002	0.001	-0.004
Unemployment rate change	0.005	-0.182	0.013
MEI	0.004	-0.062	-0.001
Total effect of COVID	0.010	-0.541	0.109

Notes: # p<0.10, ^ p<.05, * p<.01. Estimation performed by fixed effects OLS in columns (1), (4), (7) and (9) of Panel A and by IV in all other columns of Panel A. In all specifications: N=2991, full set of controls and fixed effects as in column (8) of Table 1 (the dependent variable in columns (4)-(12) of Panel A are excluded as a control in these columns), regressions weighted by 2020 total Presidential votes cast, standard errors clustered by state. Business foot traffic is Foot traffic cumulative relative growth**.** MEI is MEI daily average (1/1/2020-10/31/2020). Direct effects of COVID in Panel B are the COVID-19 deaths point estimates from columns (1)-(3) of Panel A. Indirect effects of COVID in Panel B are the product of the COVID-19 deaths point estimate on the relevant variable in columns (4)-(12) of Panel A and the point estimate for the effect of this variable on voting from columns (1)-(3) of Panel A. See main text for further details.