The 2020 US Presidential election and Trump's wars on trade and health insurance

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Abstract

The trade war initiated by the Trump administration is the largest since the US imposed the Smoot-Hawley tariffs in the 1930s and was still raging when he left office. We analyze how the trade war impacted the 2020 US Presidential election. Our results highlight the political salience of the trade war: US trade war tariffs boosted Trump's support but foreign retaliation hurt Trump. In particular, the pro-Trump effects of US trade war tariffs were crucial for Trump crossing the recount thresholds in Georgia and Wisconsin. Even more important politically, voters abandoned Trump in counties with large expansions of 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 of the trade war and health insurance coverage expansion cross political and racial lines, suggesting the mechanism operates through the impact on local economies rather than political polarization.

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1 Introduction

The trade war defined a key part of the Trump administration's economic policy agenda. It began as a temporary and small amount of WTO-allowed tariffs in early 2018 on imports of solar panels and washing machines. But spring 2018 soon brought Trump's much larger-scale tariffs on imports of steel and aluminum in the name of protecting US national security. His even larger-scale tariffs on China then began in summer 2018 in the name of protecting US intellectual property rights against the alleged "forced technology transfer" practices faced by US firms in China. By September 2019 the US was hitting about two-thirds of Chinese imports with an average tariff of roughly seven-fold that imposed by the US on the rest of the world. Naturally, US manufacturing producers and farmers soon faced retaliatory tariffs when exporting to each of the US major trading partners including Canada, Mexico, China and the European Union. Ultimately, the Trump administration's trade war is the largest since the trade war triggered by the US Smoot-Hawley tariffs in the 1930s and was still raging when Trump left office in January 2021.

When leaving office in January 2021, the Trump administration listed trade policy fourth on their list of achievements behind only the economy, tax reform, and deregulation.[1] Thus, unsurprisingly, an extensive literature has already studied the economic effects of the trade war on higher US consumer prices (Amiti et al. (2019); Fajgelbaum et al. (2020); Cavallo et al. (2021)), lower consumption and employment (Waugh (2019); Flaaen and Pierce (2020)), and lower US exports via higher input tariffs (Handley et al. (2020)).[2] And, media commentary openly discussed the political implications of the trade war for Republicans and the Trump administration 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)). 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.

Naturally, various issues other than the trade war may have impacted the 2020 US Presidential election. Perhaps none more so than the onset of the COVID-19 pandemic and the Trump administration's handling of it. Indeed, according to the Washington Post, "[T]the president finally lost, aides and allies said, because of how he mismanaged the virus" (Dawsey et al. (2020)). But other very important issues also helped define Trump's term in office. The Affordable Care Act (ACA) expanded health insurance coverage to millions of Americans after its implementation in 2014. However, Republicans continued to pursue executive, congressional, and judicial avenues to repeal and undermine the ACA. These avenues arguably

[1] See https://trumpwhitehouse.archives.gov/trump-administration-accomplishments/.

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

included the nomination of Amy Coney Barrett to the Supreme Court in the final days of Trump's term given the line of questioning during her confirmation hearings and upcoming cases on the Supreme Court's docket (e.g. Calamur (2020)). Indeed, media commentary (e.g. Lowrey (2018), Scott (2018)) and academic studies (Blanchard et al. (2019)) have documented the importance of the ACA in understanding the 2018 US midterm election results. Following immigration surges at the southern US border and high-profile police-involved deaths of African-Americans, the Trump administration's stance on race (e.g. Edsall (2020)) and immigration issues (e.g. Narea (2020)) were also much-discussed leading into the 2020 Presidential election.

The main question we ask in this paper is how the trade war impacted the 2020 US Presidential election. To do so, we analyze the county-level impacts of the trade war – US tariffs, foreign retaliatory tariffs, and US agricultural subsidies – on the change in Trump's vote share between the 2016 and 2020 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 control for a large set of county-level characteristics (and state-level unobservables) along demographic, socioeconomic, economic and political dimensions that could correlate with the salient issues discussed above.[3] To address remaining endogeneity concerns, we use the heteroskedasticity-based IV approach of Lewbel (2012) to instrument for our trade war variables. While less intuitive than a traditional IV approach, our Lewbel IV approach works well according to standard IV specification tests.

Our results highlight the political salience of the trade war for the 2020 US Presidential election. 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 had a meaningful impact on the election results.

[3] As we discuss in Section 4.1, we are hesitant to describe the point estimates for our measures of county-level COVID-19 prevalence as causal estimates of the effect that COVID-19 had on the 2020 US Presidential election. The main reason is that it is unclear whether county-level COVID-19 outbreaks are an important feature of the COVID-19 pandemic that factored into voter decisions as opposed to, for example, the Trump administration's national-level pandemic response. If one interprets our results causally, they say that COVID-19 had basically no effect on the election outcome. Prior to our paper, Baccini et al. (2021) is the only paper we know that investigates how COVID-19 impacted the 2020 US Presidential election. In prior working paper versions of this paper (Lake and Nie (2021)) we explored IV approaches to instrumenting for COVID-19 prevalence.

Trump's margin of defeat in Georgia and Wisconsin was below the thresholds enshrined in each state's law, 0.5 and 1 percentage point respectively, for a recount of votes.[4] Our results imply the absence of US tariffs would have pushed Trump's margin of defeat in Georgia and Wisconsin out of recount territory. Thus, US tariffs would have been decisive in a slightly tighter election.

In controlling for the county-level political salience of non-trade war issues, we find a robust and crucial role for health insurance coverage expansion in explaining Trump's loss. Closely following Blanchard et al. (2019), 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. 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.

Motivated by the recent work of Autor et al. (2020) and Che et al. (2022), we investigate whether the mechanism driving the effect of trade war tariffs and health insurance coverage expansion on voting behavior centered around political polarization or the impacts on local economies.[5] We find no evidence that these issues simultaneously made solidly "red" Republican counties (or that Trump won in 2016 or that have a majority white population) even "redder" and made solidly "blue" Democrat counties (or that Hillary Clinton won in 2016 or have a majority population of minorities) even "bluer". Indeed, the strongest pro-Trump effects of US tariffs are in solidly Democrat counties and counties that Hillary Clinton won in 2016. 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 and health insurance coverage expansion.

Our use of an IV approach is motivated by the trade policy literature clearly recognizing that politicians may endogenously choose tariffs based on various economic, social, and political factors (recent examples include Ma and McLaren (2018), Fajgelbaum et al. (2020) and Fetzer and Schwarz (2021)). An econometric endogeneity problem arises if we omit demographic, socioeconomic, economic, and political characteristics that both (i) drive the change in voting behavior towards Trump between 2016 and 2020 and (ii) correlate with how

[4] These recounts only led to very minor changes in vote tallies for Trump and did not come close to overturning the initial result. Each US state has its own rules regarding when recounts happen and only a handful do not allow recounts under any circumstances. For more explanation of the process in Georgia and Wisconsin respectively, see https://ballotpedia.org/Recount_laws_in_Georgia and https://ballotpedia.org/Recount_laws_in_Wisconsin.

[5] A large literature 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); Lake and Liu (2022)).

the political tariff formation process maps to county-level exposure or correlate with how the ACA differentially expanded health insurance coverage across counties. However, our host of control variables and fixed effects leaves the IV point estimates for the trade war tariffs only modestly smaller than the OLS point estimates. In turn, formal tests of endogeneity cannot reject the null hypothesis that the trade war tariffs are actually exogenous.

Our paper makes various contributions to the literature. First, ours is the first paper we know that analyzes the political salience of the trade war for the 2020 US Presidential election. In doing so, we show its political salience in this election and how its political salience can differ between Congressional midterm elections and Presidential elections. Both Blanchard et al. (2019) and Li et al. (2020) show the political salience of the trade war for the 2018 US Congressional midterm elections. While Blanchard et al. (2019) do not find statistically or economically significant effects of US tariffs, we find statistically and economically significant effects of these tariffs: US tariffs are an important reason why Trump earned recounts in Georgia and Wisconsin in 2020. However, while Blanchard et al. (2019) find that foreign retaliatory tariffs accounted for one-quarter of the Democrats 18 seat House majority, we show the counties penalizing Trump in the Presidential election for foreign retaliation were mostly in solidly Republican states. Thus, political salience of the trade war can depend on the different voting boundaries that define Congressional versus Presidential elections.

Second, our analysis contributes to the literature discussing whether trade is a salient electoral issue. Indeed, the electoral salience of trade has been questioned in the literature. Numerous papers suggest very low salience (e.g. Guisinger (2009), Blonigen (2011) and Cobb and Nance (2011)). However, our paper is especially well suited to address this issue by analyzing the US Presidential election at the height of the largest trade war in at least nearly 100 years and was initiated by the incumbent president. In doing so, our paper provides important additional support to the rapidly growing strand of the literature emphasizing the electoral salience of trade (e.g. Margalit (2011), Conconi et al. (2014), Lake and Millimet (2016), Jensen et al. (2017), Colantone and Stanig (2018), Blanchard et al. (2019), Nguyen (2019), Autor et al. (2020), Li et al. (2020) and Che et al. (2022)).

Third, unlike Blanchard et al. (2019) and Li et al. (2020), we present evidence on the mechanism behind the political salience of the trade war tariffs. In particular, 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. Indeed, past literature argues US trade policy reflects the resulting economic effects faced by voters.[6] Che et al. (2022) argue the pro-

[6] A separate strand of the empirical literature emphasizes the importance of lobbying and campaign

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.[7] 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. Nevertheless, our results show voter behavior regarding the trade war crossed political and racial lines in the 2020 US Presidential election.

Fourth, our IV approach offers an alternative IV strategy (the Lewbel heteroskedasticity-based IV approach) for the literature dealing with endogenous trade policy and shows that the US trade war tariffs are quite plausibly exogenous. 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. However, our Lewbel (2012) heteroskedasticity-based IV approach works well according to standard IV specification tests and produces very similar IV and OLS point estimates for the trade war tariffs. Thus, we cannot reject the null that the trade war tariffs are actually exogenous given our set of fixed effects as well as demographic, socioeconomic, economic, and political controls.[8] Especially given the important efficiency cost of the IV estimator over the OLS estimator (Wooldridge (2003, p.490)), our analysis suggests trade policy can be plausibly exogenous with an appropriate set of fixed effects and controls.

Fifth, our results highlight the crucial salience of health insurance coverage as an issue

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)).

[7] Additionally, Margalit (2011) shows how local job layoffs reduce but TAA compensation increases electoral support for the incumbent president. And, Jensen et al. (2017) show how local employment in low-skilled manufacturing reduces but local employment in high-skilled tradable services increases support for the incumbent president.

[8] 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.

underlying Trump's loss. Blanchard et al. (2019) find the health insurance coverage expansion issue accounted for half of the Democrat's 18 seat House majority following their sweeping 2018 US midterm election wins. However, 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.

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

2.1 Main empirical specification

Following prior literature (e.g. Blanchard et al. (2019), Autor et al. (2020) and Li et al. (2020)), we model the county-level change in the two-party Republican vote share between elections. In our paper, these are the 2016 and 2020 Presidential elections. Apart from consistency with prior literature, various reasons motivate modeling this change in voting behavior instead of modeling voting behavior in 2020. First, intuitively, persistent and partisan county-level voting behavior create difficulties when comparing vote shares across counties in a given election. Second, more formally, modeling the change in voting behavior flexibly controls for unobservable but time-invariant county-level factors that drive voting behavior and would otherwise fall in to the error term and cause potential endogeneity problems. This is especially valuable given the persistence in county-level voting behavior. Third, relatedly, modeling the change in voting behavior allows state-level fixed effects that flexibly control for any unobservable *trends* in state-level factors driving voting behavior.

Nevertheless, to motivate our ultimate specification, we show how it emerges from time-differencing a specification where the dependent variable is the Republican vote share in a given year. To this end, let c index counties and consider the following specification:

$$V_{ct} = \beta_0 V_{c,t-1} + TW_{ct}\beta_{1t} + HI_{ct}\beta_{2t} + COVID_{ct}\beta_{3t} + D_{c,t-1}\beta_{4t} + \eta_{st} + \epsilon_{ct} \tag{1}$$

where V_{ct} is the two-party Republican vote share in the year t Presidential election. This specification captures the idea that voting behavior can be persistent and hence depend on voting behavior in the last election through $V_{c,t-1}$ but will also likely depend on various current issues – e.g. the trade war (TW_{ct}), health insurance coverage (HI_{ct}), and the COVID-19 pandemic ($COVID_{ct}$) – and other start-of-period demographic, socioeconomic, and economic characteristics of the county ($D_{c,t-1}$).[9] Moreover, the effect of such issues and county-characteristics could vary over time. Thus, the coefficients β_{1t}, β_{2t}, β_{3t} and β_{4t} in equation (1) are time-varying. The fixed effects η_{st} capture time-varying state-level factors that influence county-level voting behavior across all counties within the state. Finally, ϵ_{ct} is an error term.

First-differencing equation (1) raises two important definition issues. First, the trade war and COVID-19 pandemic pre-date the 2016 election. Thus, $\beta_{1t} = \beta_{3t} = 0$ for $t \leq 2016$. Second, after allowing a few years for take-up, health insurance coverage expansion following ACA implementation is inherently a time-invariant variable. Moreover, pre-ACA health insurance coverage could be strongly correlated with post-ACA coverage expansion and voting behavior. Thus, we follow prior literature by controlling for pre-ACA coverage. In turn, we define HI_{ct} as the time-invariant vector $HI_c = [\Delta HI_c \; HI_{c,Pre}]$ where ΔHI_c is the post-ACA expansion of health insurance coverage and $HI_{c,Pre}$ is the pre-ACA coverage. Given these definitions, first-differencing equation (2) between the Presidential election years $t_2 = 2020$ and $t_1 = 2016$ (with $t_0 = 2012$) gives

$$\begin{aligned}\Delta V_{ct_2} =& \beta_0 \Delta V_{ct_1} + TW_{ct_2}\beta_{1t_2} + HI_c\left(\beta_{2t_2} - \beta_{2t_1}\right) + COVID_{ct_2}\beta_{3t_2} \\ &+ D_{ct_1}\beta_{4t_2} - D_{ct_0}\beta_{4t_1} + \Delta\eta_{st_2} + \Delta\epsilon_{ct_2}.\end{aligned} \tag{2}$$

To reach the final version of our estimating equation we make two more substitutions. First, we define the two-element coefficient vector $\beta_{2t_2} - \beta_{2t_1} \equiv [\alpha_t \; \theta_{2t}]$. Second, we re-write the terms involving time-varying county-level characteristics D_{ct} as

$$\begin{aligned}\beta_{4t_2} D_{ct_1} - \beta_{4t_1} D_{ct_0} &= \left(\beta_{4t_2} - \beta_{4t_1}\right) D_{ct_1} + \beta_{4t_1}\left(D_{ct_1} - D_{ct_0}\right) \\ &\equiv \theta_{4t_2} D_{ct_1} + \beta_{4t_1}\Delta D_{ct_1}.\end{aligned} \tag{3}$$

We now have the final version of our estimating equation:

$$\Delta V_{c,2020} = \beta_0 \Delta V_{c,2016} + TW_{c,2020}\beta_{1,2020} + \alpha_{2020}\Delta HI_c + X_{c,2020}\delta_{2020} + \gamma_s + \varepsilon_{c,2020} \tag{4}$$

where $X_{c,2020} = [HI_{c,Pre} \; COVID_{c,2020} \; \Delta D_{c,2016} \; D_{c,2016}]$ is a combined vector of county characteristics with a coefficient vector $\delta_{2020} = [\theta_{2,2020} \; \beta_{3,2020} \; \beta_{4,2016} \; \theta_{4,2020}]$. The state fixed effects $\gamma_s = \Delta\eta_{s,2020}$ capture state-level unobservables impacting the change in voting behavior between 2016 and 2020. And, $\varepsilon_c = \Delta\epsilon_{c,2020}$ is the error term. Following earlier literature

[9] Using start-of-period rather than end-of-period county-characteristics D_c helps mitigate endogeneity concerns. It is also consistent with prior literature, e.g. Blanchard et al. (2019) and Autor et al. (2020).

(e.g. Blanchard et al. (2019), Autor et al. (2020)), we weight by total votes cast in the 2020 Presidential election and cluster standard errors by state.

2.2 Identification

The clear identification threat is omitted variable bias that leads to endogeneity of the trade war variables or health insurance coverage expansion. Given the inclusion of $\triangle V_{c,t-1}$ in (4), endogeneity concerns really revolve around omitted variables that explain *changing* voting behavior between 2016 and 2020 rather than permanent or long-run aspects of voting behavior. Two forms of omitted variables naturally arise.

First, US and foreign government policy may disproportionately affect geographic regions of the US with certain characteristics that also directly affect changing voting behavior. For example, the Trump administration's aggressive and provocative stance on various international relations issues (e.g. the trade war, climate change, NATO, Russia etc.) may be inherently attractive to older white male voters. But, these voters may disproportionately live in the manufacturing-intensive and agricultural-intensive parts of the US that were more exposed to US and foreign trade war tariffs. Alternatively, the ACA may have led to disproportionate expansion of health insurance coverage in urban areas where a larger share of low income households may have been uninsured because they relied on part-time jobs without health insurance benefits. But, these areas may also be moving away from Trump due to a perceived notion that his tax policies favor the rich over the poor. These are just two of many plausible examples.

The second natural type of omitted variable bias in our setting is that the local salience for voters of various emerging issues may happen to correlate with the specific issues of interest in this paper. For example, COVID lockdowns may have been especially unpopular in manufacturing-intensive and agricultural-intensive parts of the US because these workers found it very difficult to work from home. In turn, this issue may push voters towards Trump in these parts of the US. But, these manufacturing-intensive and agricultural-intensive parts of the US are also the most exposed to US and foreign trade war tariffs. Alternatively, race and immigration issues could be more salient among voters in urban areas because of the larger share of minority voters and/or higher share of highly educated workers. In turn, urban voters could be moving away from Trump due his tough stance on race and immigration issues. However, these areas are also the least exposed to trade war tariffs because they tend to specialize in services rather than agriculture or manufacturing. And, health insurance coverage may have expanded more dramatically in these urban areas because minority workers may disproportionately hold jobs that do not provide health insurance.

We take two approaches to deal with endogeneity concerns of our trade war and health insurance expansion variables. First, we use a comprehensive set of controls and fixed effects. Section 3 describes these controls in detail. We use a host of county-level demographic, socioeconomic, economic, and political characteristics as well as state-level fixed effects that capture state-level factors along these or other dimensions. These help control for the factors that could both drive changes in county-level voter behavior and also explain why the US and foreign governments targeted certain counties or why health insurance coverage expanded differentially across counties. They also help control for the possibility that other salient issues including COVID-19 outbreaks or race and immigration issues may happen to correlate with local exposure to the trade war or expansion in health insurance coverage. To explicitly deal with the salience of the COVID-19 pandemic, we control for various direct and indirect measures of county-level COVID-19 outbreaks and the resulting local economic and social impacts.

Nevertheless, omitted county-level characteristics may still drive county-level changing voter behavior towards Trump between 2016 and 2020 *and* help explain county-level exposure to the trade war or health insurance coverage expansion. Thus, our second approach to dealing with endogeneity concerns uses instrumental variables (IV). However, given the lack of obvious instruments, we use Lewbel (2012) heteroskedasticity-based IVs.[10]

To understand the mechanics and intuition for the Lewbel (2012) heteroskedasticity-based IV approach, it is useful to do so in the context of the general IV framework. To this end, consider estimating the causal effect of a potentially endogenous variable W on an outcome Y. The set of exogenous controls, which includes any fixed effects, is X and the error term is ε_Y. Like all IV approaches, the Lewbel approach identifies the causal effect of W on Y by finding exogenous variation in W driven by a set of instruments Z. Thus, the Lewbel approach has the standard relevance assumption $\text{cov}(Z, W) > 0$ that ensures variation in Z reflects variation in the potentially endogenous variable W. It also has the standard validity assumption $\text{cov}(Z, \varepsilon_Y) = 0$ that ensures such variation in Z reflects an exogenous part of the variation in W. While typical IV approaches use instruments Z from outside the model, Lewbel (2012) shows the instruments Z can be generated from the exogenous controls X in the model.

Intuitively, using exogenous controls X in the model to generate an instrument Z goes a long way towards satisfying the validity assumption $\text{cov}(Z, \varepsilon_Y) = 0$. After all, the controls X are exogenous by assumption (i.e. $\text{cov}(X, \varepsilon_Y) = 0$). However, one cannot simply use X as an instrument for W given X is included in the model. Instead, Lewbel (2012) proposes using a

[10] Also see, e.g., Arcand et al. (2015), Millimet and Roy (2016) and Courtemanche et al. (2021) for applications of the Lewbel approach.

subset $\tilde{X} \subseteq X$ of the exogenous controls to construct the instrument set $Z = \left(\tilde{X} - \bar{\tilde{X}}\right) \varepsilon_W$ which is the sample-demeaned $\tilde{X}$ interacted with the error term ε_W from the "first-stage" regression of W on the full set of exogenous controls X. The generated instruments Z are known as heteroskedasticity-based IVs because the relevance assumption $\operatorname{cov}(Z, W) \neq 0$ reduces to $\operatorname{cov}\left(\tilde{X}, \varepsilon_W^2\right) \neq 0$: variation in the generated instruments Z reflect variation in the endogenous variable W only when the heteroskedasticity of W depends on the subset $\tilde{X} \subseteq X$ of exogenous controls. Indeed, the presence of heteroskedasticity can be tested empirically.

As usual, the key question empirically concerns the validity assumption. Given $Z = \left(\tilde{X} - \bar{\tilde{X}}\right) \varepsilon_W$, the validity assumption $\operatorname{cov}(Z, \varepsilon_Y) = 0$ reduces to $\operatorname{cov}\left(\tilde{X}\varepsilon_W, \varepsilon_Y\right) = \operatorname{cov}\left(\tilde{X}, \varepsilon_W \varepsilon_Y\right) = 0$. Intuitively, the subset of exogenous controls $\tilde{X} \subseteq X$ must be unrelated to $\operatorname{cov}(\varepsilon_Y, \varepsilon_W)$. Lewbel (2012) shows a common class of empirical situations that satisfy the validity assumption are situations where an unobserved factor U drives $\operatorname{cov}(\varepsilon_Y, \varepsilon_W) \neq 0$ and U is conditionally uncorrelated with any other components of ε_Y and ε_W. In this case, it follows immediately that $\tilde{X}$ is unrelated to $\operatorname{cov}(\varepsilon_Y, \varepsilon_W)$: (i) $\operatorname{cov}(\varepsilon_Y, \varepsilon_W) \neq 0$ stems solely from the unobserved factor U and (ii) $\operatorname{cov}\left(U, \tilde{X}\right) = 0$ because U is a part of ε_Y and the exogenity of $\tilde{X}$ says $\operatorname{cov}\left(\varepsilon_Y, \tilde{X}\right) = 0$.

Two specific examples of such an unobserved factor U naturally fit the context of our paper. First, conditional on observed county-level characteristics, U could be aspects of local political activism that directly affect changes in local voting behavior between 2016 and 2020 and also drove the Trump administration and/or foreign governments to target geographic areas of the US with trade war tariffs. This would generate $\operatorname{cov}(\varepsilon_Y, \varepsilon_W) \neq 0$ that is unrelated to $\tilde{X}$. Second, our trade war variables may suffer from classical measurement error given they are inherently proxy variables. In this case, U could represent the measurement error between our observed local trade war exposure variables W and their true unobserved value W^*. This measurement error would drive $\operatorname{cov}(\varepsilon_Y, \varepsilon_W) \neq 0$ but, given classical measurement error, would be unrelated to $\tilde{X}$. Thus, the validity assumption of the Lewbel approach appears plausible in our context.

Having discussed the mechanics and intuition behind the Lewbel approach, we briefly summarize the formalities of the Lewbel approach. The "first-stage" regresses a potentially endogenous variable on all exogenous controls including any fixed effects. In our context, we separately regress each of the potentially endogenous variables $[TW_{c,2020}\ \Delta HI_{c,2020}]$ on the exogenous controls (i.e. $X_{c,2020}$ and the fixed effects γ_s in equation (4)).[11] For a subset $\tilde{X}$ of exogenous controls that explain heteroskedasticity in a given endogenous variable,

[11]We do not include $\Delta V_{c,2016}$ as an exogenous control because of Nickell bias. See footnote 29 for more details.

identified using the Koenker (1981) Breusch-Pagan test for heteroskedasticity, we construct the instruments $Z = \left(\tilde{X} - \bar{\tilde{X}}\right) \hat{\varepsilon}_W$ where $\hat{\varepsilon}_W$ are the residuals from the first stage regression. The "second-stage" regression estimates equation (4) using the constructed instrument sets Z as IVs for the potentially endogenous variables $[TW_{c,2020}\ \Delta HI_{c,2020}]$. Lewbel's approach allows the usual IV specification tests including weak instrument and, when Z contains more than one instrument, overidentification tests.

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.[12] 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 A 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 B 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.[13]

Reflecting Trump's 2016 triumph versus his 2020 demise, Table A1 shows 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 percentage points (Appendix Table A1 contains all summary statistics). This dramatic change is emphasized by Panels B-C of Figure 1 which both use the Panel B cutoff values: the overwhelming majority of counties swung towards Trump in 2016 by an extent only seen in a small minority of counties in 2020.

3.2 Trade war

3.2.1 Evolution of the trade war

Table 1 summarizes the evolution of the trade war initiated by the Trump administration in 2018 and the source of our trade war data.[14] The trade war began with the Trump

[12] 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.

[13] The correlation is .264.

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

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.[15] 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%).

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. Due to foreign retaliation targeting US farmers, the Trump administration implemented the Market Facilitation Program of agricultural subsidies in 2018 to help US farmers hurt by foreign retaliatory tariffs. We use county-level estimated subsidy receipts from Blanchard et al. (2019).

[15] 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.

For county-level trade war tariff exposure, 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 the trade war tariff imposed by country k on product h and country j as $\tau^k_{h,j}$ and the associated 2017 US imports by $m_{h,j}$, the additional tariffs charged on US imports of HS8 product h from country j are $TS^{US}_{h,j} = \tau^{US}_{h,j} m_{h,j}$. Denoting 2017 US exports by x, the additional retaliatory tariffs charged on US exports of HS8 product h to country j are $TS^R_{h,j} = \tau^j_{h,US} x_{h,j}$. 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.[16] 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 2 describe the county-level trade war variables. Table A1 shows they have similar variability but different means. Panels A-C of Figure 1 emphasize the different geographic distribution of county-level exposure to the trade war.[17] Exposure to US trade war tariffs is concentrated around the Great Lakes and parts of the South in panel A. In contrast, exposure to foreign retaliation is concentrated along the Mississippi River, the lower Midwest and the far West in panel B. These different geographic distributions fit with the broad idea that US tariffs protected US manufacturing while foreign retaliation targeted US agriculture. Panel C shows the agricultural subsidies were 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.[18]

[16] 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.

[17] Their correlation is 0.075.

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

Across all US counties, the mean US tariff shock is \$1030 per worker while the mean county had a retaliatory tariff shock of \$550 per worker and agricultural subsidies of \$430 per worker. Panels A and D-E of Figure 2 illustrate the different means by all using the panel A cutoffs. They show a much broader set of counties significantly exposed to US trade war tariffs than to foreign retaliatory tariffs or agricultural subsidies. Indeed, the geographic concentration of agricultural subsidies in panel E is striking.

3.3 Health insurance coverage

The centerpiece of the ACA is the 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.

Closely following Blanchard et al. (2019), 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.[19] Panel F of Figure 2 shows significant geographic variation around the mean expansion of 5.05% points in 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 Controls

As discussed in Section 2, endogeneity of our trade war and health insurance coverage expansion variables is the key identification threat. Specifically, the concern is that omitted county-level characteristics could correlate with trade war exposure or health insurance coverage expansion *and* the *change* in voting behavior towards Trump between 2016 and 2020 (potentially through other emergent and salient electoral issues). Thus, we use a host of county-level control variables to mitigate these endogeneity concerns. Unless otherwise noted, all control variable data comes from the 5-year American Community Survey (ACS). For the ACS data, we use the 2016 and 2012 samples to construct controls in 2016 levels and changes between 2016 and 2012.

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

Table 2 summarizes the control variables and their source while Table A1 presents the descriptive statistics of all variables in our analysis. Panel A of Table 2 lists the demographic controls. This includes distributions of age (six bins including the omitted category), gender, and race (five bins). It also includes three population share variables related to ethnicity: naturalized citizens, foreign born, and foreign language spoken at home. Panel B lists the socioeconomic controls. This includes the distributions of household annual income (7 bins including the omitted category) and education (4 bins including the omitted category). It also includes median real household income and the population share of people in poverty. Finally, it includes a measure of social capital (Rupasingha et al. (2006)). Panel C lists the economic controls. It includes four measures capturing sectoral employment composition: manufacturing and agriculture/mining employment shares as well as population shares of unemployed and not in the labor force. It also includes six measures reflecting location size and density: population, metro size (two categories excluding the omitted non-metro category from the National Center for Health Statistics), share of multi-unit housing structures, employment share of workers commuting by public transport, and a measure of effective population density (Desmet and Wacziarg (2021)).[20]

Panel D of Table 2 lists the direct and indirect variables we use to control for the salience of COVID-19. For our direct measures, we collect data on COVID-19 prevalence from COVID County Data (which merged with Covid Act Now).[21] Our baseline measure of COVID-19 prevalence is cumulative deaths per 10,000 population from January 1 to October 31, 2020. However, we also explore 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.[22] The possibility of voters caring about recent or "peak" COVID-19 outbreaks motivate the latter two windows. Figure 3 shows 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.

Given the challenge of measuring COVID-19 prevalence, we augment these direct mea-

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

[21]They obtain data from various sources with county-level dashboards most preferred. The ordering of sources is county dashboards, state dashboards, COVID Tracking Project, department of HHS, USA Facts, New York Times, and CovidAtlas.

[22]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.

sures of COVID-19 prevalence with a wide set of indirect measures.[23] County-level social distancing and COVID-induced downturns in economic activity could proxy for COVID-19 prevalence (see Figure A2 for illustration). To measure social distancing, we use the Mobility and Engagement Index(MEI) from the Federal Reserve Bank of Dallas (Atkinson et al. (2020)). This index is an inverse measure of social distancing based on cell phone data from SafeGraph. We control for the daily average MEI using the time window that matches our measure of COVID-19. To control for economic activity, we use two county-level measures: (i) the change in the unemployment rate between October 2019 and October 2020 (BLS Local Area Unemployment Statistics) and (ii) the depressed growth in business foot traffic using county-by-store level cell phone data from SafeGraph. To measure the latter, we compute the growth in the number of store visits between the period January-February 2020 and the period March-October 2020 and, to account for county-specific seasonality, divide by the analogous growth in 2019. We also control for county-level health characteristics from Chetty et al. (2016) (including diabetes prevalence measures as well as separate 30-day mortality rates for pneumonia, heart failure, overall hospital mortality) and the share of county employment that can work remotely (Dingel and Neiman (2020)).[24,25]

4 Results

4.1 Baseline results

Table 3 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 factors that could influence both county-level changes in voting behavior towards Trump between 2016 and 2020 and county-level trade war exposure is very important.

[23]Many of our controls in Panels A-C of Table 2 and motivated above are also shown by Desmet and Wacziarg (2021) to be important correlates of county-level COVID-19 cases and deaths.

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

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

Column (4) adds the county-level demographic, socioeconomic, and economic controls listed in Panels A-C of Table 2. This flips the sign and dramatically increases the magnitude of the point estimates for US and retaliatory tariff shocks. Adding state fixed effects in column (5) further increases the economic magnitude of US and retaliatory tariff shocks and leaves all trade war variables highly statistically significant.[26] 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.[27] The negative point estimate for the retaliatory tariff 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 our main measure of COVID-19 prevalence – cumulative deaths through October 2020 per 10,000 population – and our other COVID-19 control variables listed in Panel D of Table 2.[28] Again, the point estimates for the trade war variables remain very stable relative to column (5) which says they are largely uncorrelated with COVID-19. So, any potential endogeneity problems with COVID-19 prevalence are not major concerns for endogeneity of the other variables.[29]

If one interpreted the COVID-19 point estimate causally, it would say that COVID-19 played no role in Trump's election loss. However, as we discussed in the introduction, we are quite hesitant in making this causal interpretation. The main reason is that it is unclear whether the extent of a county's COVID-19 outbreak is closely related to the pandemic-related factors that influenced voting behavior of the county's voters. Indeed, one could strongly argue that it was Trump's national-level policy response to the pandemic that influenced whether voters became more or less likely to vote for him. Thus, we ultimately

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

[27]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.

[28]We lose 120 observations in column (7) with missing COVID-19 controls data: 60 observations due to missing MEI data and another 60 observations due to missing health characteristics data.

[29]One may be worried about Nickell bias introduced through the lagged dependent variable in equation (4). However, any such bias is not materially biasing our key variables of interest. Dropping $\Delta V_{c,2016}$ from the specification in column (7) of Table 3 and re-estimating gives respective point estimates on US tariffs, foreign tariffs, agricultural subsidies and health insurance coverage expansion of 0.163, -0.210, .713 and -0.085. Only the point estimate for agricultural subsidies changes materially. However, our IV results in Section 4.2 suggest that agricultural subsidies are endogenous and hence one should not interpret the agricultural subsidy point estimates in Table 3 causally.

see our COVID-19 controls as merely controlling for the possibility that county-level COVID-19 outbreaks could have influenced voter behavior and could be correlated with county-level trade war exposure.

Finally, column (7) adds our health insurance coverage variables. Given the trade war point and COVID-19 point estimates are virtually unchanged from column (6), county-level health insurance coverage expansion is largely uncorrelated with county-level trade war exposure and COVID-19 outbreaks. So, again, any endogeneity problem for one of the explanatory variables does not spill over to other key explanatory variables.[30] 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.

Before moving on to the economic significance of the effects described above, we briefly describe the results from column (7) of Table 3 for our control variables. Appendix Table A2 shows the results from this specification for all control variables. Reflecting the complex way that these control variables factor into different political mechanisms, they present a nuanced picture. All else equal, counties with a larger share of younger and middle aged voters moved towards Trump but, conditional on the county's age distribution, counties that had growing shares of young and middle aged voters between 2012 and 2016 moved away from Trump. Similarly, counties with larger shares of educated people moved towards Trump but, conditional on the county's education distribution, counties with a growing share of educated workers between 2012 and 2016 moved away from Trump. And, while counties with larger shares of Asian and white people moved away from Trump, counties with larger shares of people speaking a foreign language at home moved towards Trump. More straightforwardly, lower-middle income households moved away from Trump. And, all measures of social distancing and the induced economic downturn (i.e. MEI, foot traffic, and the unemployment rate change) say that harder hit counties moved towards Trump.

Some of the effects regarding our main issues of interest – the trade war and health insurance coverage expansion – are economically significant. The point estimates from column (7) of Table 3 imply the median county saw Trump's 2020 vote share increase by 0.12 and .01 percentage points respectively on account of US trade war tariffs and agricultural subsi-

[30]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. 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.

dies but decrease by 0.06 and 0.42 percentage 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.[31]

Table 4 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 3, we use its county-specific value and its column (7) 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 candidate's vote share margin requires an offsetting impact of half this margin.

The key takeaway from panel A of Table 4 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 4 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 percentage 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 percentage points. However, removing the effects of US tariffs roughly doubles Trump's loss both in Georgia to 0.51 percentage points and in Wisconsin to 1.17 percentage 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 0.93 percentage points and 1.06 percentage points respectively. Rather than losing Georgia and Arizona by 0.24 percentage points and 0.31 percentage points respectively, Trump wins by 0.69 percentage points and 0.75 percentage points. Additionally, Trump only loses Wisconsin by 0.05 percentage points instead of the actual 0.64 percentage points. With Georgia and Arizona's electoral

[31] The system for electing the US President is known as the "electoral college system". Each US state is a winner-take-all contest with the winning candidate receiving a pre-determined number of "electoral college votes" for winning a state based on the state's share of the US population. The candidate with a majority of electoral college votes becomes President.

college votes, Trump is less than 2000 votes in Wisconsin plus another one electoral college vote away from re-election. Thus, health insurance coverage is a very politically salient issue.

4.2 IV results

Table 5 presents the IV results. For ease of comparison, column (1) presents the OLS results from column (7) of Table 3. 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. Finally, column (7) treats all trade war variables and health insurance coverage expansion as endogenous.

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 generally at the $p < 0.05$ level. The Kleibergen-Paap weak-instrument F-stats are in the $20 - 200$ 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 $0.29 - 0.88$ 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 demographic, socioeconomic, 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 .31 and .93 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 demographic, socioeconomic, 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 columns (2), (5) and (7) when treating it as endogenous. That said, the US tariff shock remains statistically and economically significant. Based on the column (7) point estimate from Table 5, Panel B of Table 4 shows removing its effect still roughly doubles Trump's loss in Georgia from 0.24 to 0.44 percentage points and increases his loss in Wisconsin by about one-half from 0.64 to 1.02 percentage points. These margins would still not prevent a Georgia recount and would be right on the threshold of a Wisconsin recount (respective recount thresholds of 0.5 and 1 percentage point).

The point estimate for foreign retaliation remains stable and statistically significant in columns (3), (5) and (7) when treating it as endogenous. Thus, column (3) in panel B of Table 4 shows it also remains economically insignificant in affecting state-level electoral college outcomes of closely contested states.

Agricultural subsidies appear to be the trade war variable most susceptible to endogeneity. With $p = .013$, the endogeneity test rejects the null that they are exogenous at the 5% level in column (4). And, treating them as endogenous reduces its point estimate from 0.501 in column (1) to 0.031, 0.033 and -0.151 in columns (4), (5) and (7) respectively. 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 rejects the null ($p = .002$) that health insurance coverage expansion is exogenous at the 1% level. As expected, the much larger IV point estimate dramatically increases the economic significance. Column (5) in Panel B of Table 4 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.

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 tariffs are not a threat to our identification strategy.

4.3 Robustness

Alternative COVID-19 measures. We have focused on cumulative COVID-19 deaths as our direct measure of COVID-19 prevalence. Given the inherent difficulties in controlling for COVID-19 prevalence, Panel A of Table 6 explores other measures of COVID-19 cases and deaths.

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.

Placebo specifications. Despite our attempts to control for county-level demographic, socioeconomic, economic, health and political characteristics and despite our IV approaches, one may still worry that our results reflect pre-existing county-level political trends that are correlated with county-level trade war exposure or health insurance coverage expansion. Thus, we pursue placebo specifications where the dependent variable is the change in Republican vote share between the 2016 and 2012 elections and we remove the 2020-2016 change from the specification.

Panel B of Table 6 presents the results. Column (1) shows the OLS results with point estimates that are generally very imprecise, quite small, and sometimes differ in sign from the main analysis. Column (2) uses the same Lewbel instruments as column (7) of Table 5 to instrument for the trade war variables and health insurance coverage expansion. The instruments appear strong. But, again, the Sargan-Hansen endogeneity test cannot reject the null that our potential endogenous variables are actually exogenous ($p > 0.66$). Overall, this provides further evidence mitigating concerns that our results merely reflect pre-existing political trends.

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 exogeneity for health insurance coverage expansion, the OLS point estimates were notably smaller than the IV point estimates. Thus, our OLS results provide 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 variable 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.

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. (2022) that voter behavior towards a politician reflects the economic impact of a politician's 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 a polarizing effect whereby the voting behavior of democrats or racial minorities penalize Trump on certain issues while the voting behavior of republicans or white voters reward Trump on the same or other issues.

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 and health insurance coverage expansion on voter behavior towards Trump are similar across political and racial lines or, instead, whether they polarize voters along these lines.

Panel A of Table 7 investigates this issue. Columns (2)-(4) proxy for political heterogeneity using 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. Columns (5)-(6) proxy for political heterogeneity by whether the county voted for Trump or Hillary Clinton in 2016. And, like Autor et al. (2020), columns (7)-(8) proxy for political heterogeneity by whether the county has a majority non-Hispanic white population or a majority population of minorities.

Ultimately, we see little evidence of political polarization: there is no key explanatory variable where the point estimates change sign across political lines and both are economically significant. Indeed, the US tariff shock point estimate is positive and statistically significant for both solidly Republican and solidly Democrat counties: both types of counties rewarded Trump for providing local protection. Moreover, although sometimes imprecise, the point estimates say that Trump was more strongly rewarded for providing local protection in counties that were solidly Democrat or won by Hillary Clinton in 2016 or had majority non-white populations than counties that were solidly Republican or won by Trump in 2016 or had majority white populations.[32] Thus, the key issues we analyze did not simultaneously

[32]While the point estimate for the US tariff shock is quite noisy for the majority non-white subsample in column (8), the p-value of the US tariff shock point estimate for the Clinton county subsample in column

solidify support for Trump among the groups already supporting Trump and also solidify support for Biden from groups already likely to support Biden.

An important result from our political heterogeneity analysis is the much stronger effect of health insurance coverage expansion in Clinton counties than Trump counties (and, similarly, in solidly Democrat versus solidly Republican counties and majority white versus majority non-white counties). This has strong implications for economic significance. Absent the effects of health insurance coverage expansion, column (5) of panel D in Table 4 shows Trump's counterfactual winning margin in Georgia increases to 0.90 percentage points and he now *wins* Nevada by 0.52 percentage 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 expansion more than the national average. Emphasizing the salience of health insurance coverage expansion, these counterfactual results say a 0.08 percentage point loss in Wisconsin, less than 3000 votes, is all that prevents Trump's re-election.

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 lines to the local economic effects of Trump's policies.

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 lead 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 7 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 0.074 percentage points due to US tariffs on China and decrease by 0.056 percentage points due to Chinese tariffs. These effects are somewhat lower than the 0.121 and 0.063 percentage points in our baseline analysis. Indeed, according to column (2) in panel F of Table 4, 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.

(6) is .101 and hence borderline statistically significant at the 10% level.

Nevertheless, 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 also 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 7 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 0.091 percentage points due to US tariffs on China and decrease by 0.056 percentage points due to Chinese tariffs. Again, these are somewhat lower than our baseline analysis. The effect of US tariffs is sufficiently lower than column (2) of panel G in Table 4 that removing its pro-Trump effect leaves Trump in recount territory in Georgia. Again, 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 trade war and health insurance coverage issues were systematically different in counties with greater prevalence of COVID-19. 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. Or, perhaps large county-level COVID-19 outbreaks dampened the focus of voters on trade and health insurance issues.

Columns (4)-(6) of panel B in Table 7 split counties into terciles of COVID-19 prevalence. The point estimates reveal no stark heterogeneities across the terciles. Panels A and H of Table 4 also show that taking this heterogeneity into account does not impact the economic significance of the issues in terms of electoral college outcomes.

4.5 Impact of COVID-19 on election outcome

As we have already discussed, we are quite hesitant to interpret the point estimates on our county-level direct measure of COVID-19 prevalence (deaths per 10,000 population in our main specification) as reflecting the causal effect of the pandemic on voter behavior. It is quite plausible that Trump's national-level policy response to the pandemic influenced voter behavior rather than the prevalence of outbreaks in the local community. Thus, we primarily view our broad set of COVID-19 variables as controls.

That said, we now focus on the possibility of causally estimating the impact of county-level COVID-19 prevalence on voting behavior. To this end, we ignore the trade war and health insurance coverage expansion. Table 8 presents the results and begins in column (1) by simply regressing $\Delta V_{c,2020}$ on county-level COVID-19 prevalence defined as deaths per 10,000 population. In the absence of any controls or fixed effects, the positive point estimate reflects a positive correlation in the data: counties with larger COVID-19 outbreaks moved towards Trump. Column (2) adds the set of controls used in our baseline specification (including county-level measures of social distancing and COVID-induced depressed economic activity) from column (7) of Table 3. The point estimate is now zero. The results from both specifications run contrary to the popular narrative that Trump's mishandling of the COVID-19 pandemic cost him the election.

Naturally, one may have concerns about endogeneity of county-level COVID-19 prevalence. Thus, columns (3)-(4) of Table 8 use two alternative IV strategies. Columns (3)-(4) instrument for COVID-19 prevalence using, respectively, the employment share of meat-packing workers (following Baccini et al. (2021)) and the population share of nursing home residents. 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 county-level vote share between 2016 and 2020 only depends on the instruments through their impact on county-level COVID-19 deaths. One could argue these are plausible exclusion restrictions and the media has documented both as key sources of COVID-19 outbreaks.[33]

Columns (3)-(4) do not provide notable evidence that local COVID-19 outbreaks pushed voters away from Trump. While the point estimate using the meat-packing worker instrument in column (3) is negative, the instrument is very weak with a Kleibergen-Paap F-stat of 3.05 and far below the rule-of-thumb of 10. Moreover, the point estimate using the nursing home instrument in column (4) is positive and statistically significant and the instrument is very strong (Kleibergen-Paap F-stat of 56.72). If anything, our results say that Trump benefited politically from more severe county-level COVID-19 outbreaks.[34] Ultimately, we interpret this overall set of results as suggesting that any meaningful and systematic impacts of COVID-19 on the election outcome likely arose through a national-level shock (e.g. Trump's

[33] 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)).

[34] These results are robust to using any of our COVID-19 prevalence measures from panel A of Table 6.

national response to the pandemic) rather than through the severity of local COVID-19 outbreaks.

5 Conclusion

We analyze the impact of the Trump administration's trade war and its attempts to undermine the ACA on the 2020 US Presidential election. Our results emphasize the political salience of both issues. 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. 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. While the trade war literature has already established the political salience of the ACA for the 2018 US midterm elections, our results highlight the issue was an order of magnitude larger in the 2020 US Presidential election. 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.

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? The literature has used this kind of mechanism to explain the political implications of rapid import growth from China in the 1990s and 2000s. However, our results say 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. Thus, our results suggest that voter behavior responded to the economic effects of Trump's policies.

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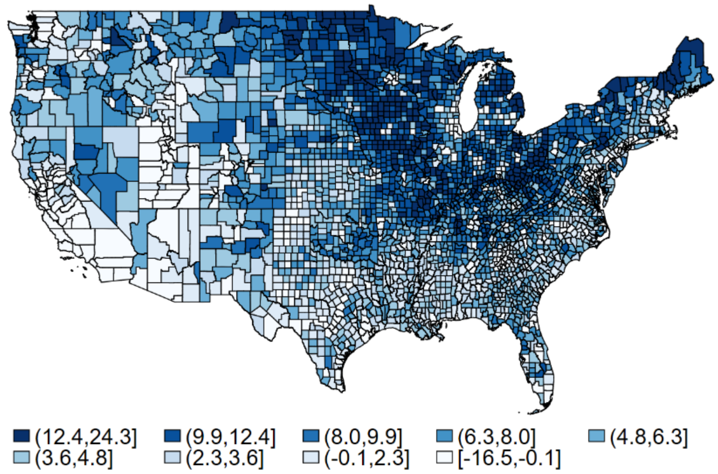
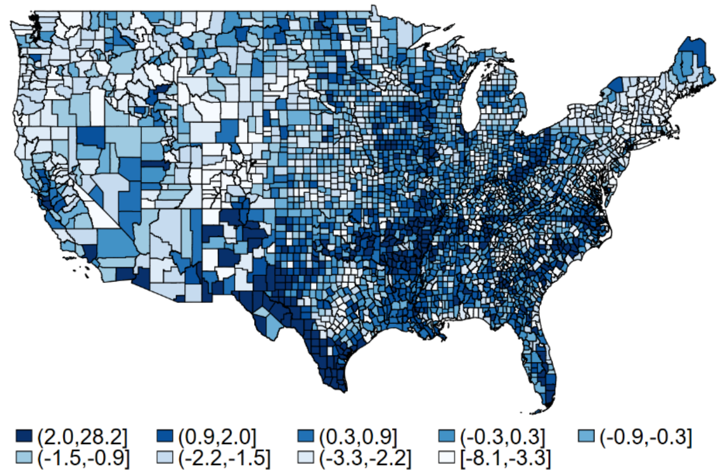
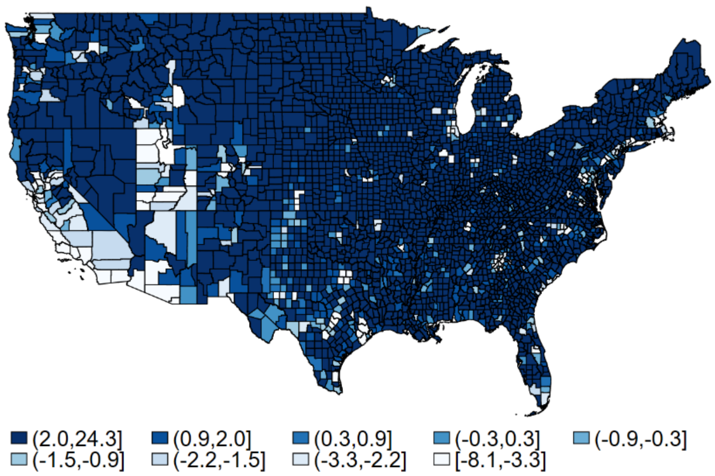


A. Change in 2-party Republican vote share 2012-2016 (% pts)



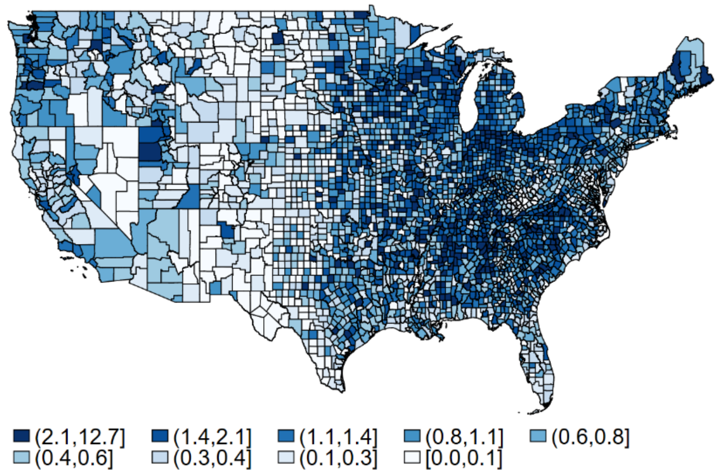
B. Change in 2-party Republican vote share 2016-2020 (% pts)



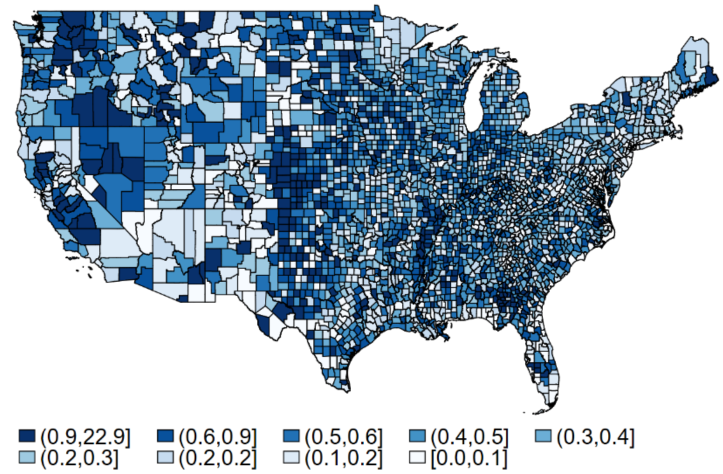
C. Change in 2-party Republican vote share 2012-2016 (% pts)

Figure 1: Presidential voting outcomes

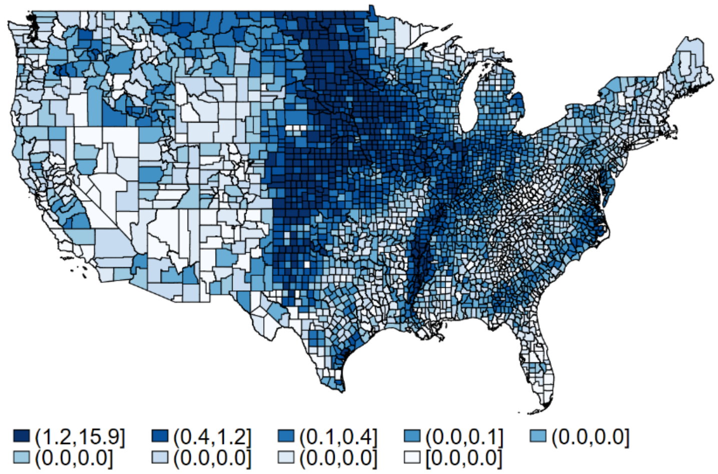
Notes: Maps represent the 3108 mainland US counties. Panel A-B cutoffs divide counties into equally sized bins. Panel C uses cutoffs from Panel B. Presidential voting data from David Leip's Election Atlas; 2020 election data is version 0.9 (official release of data for all states). See main text for further details.



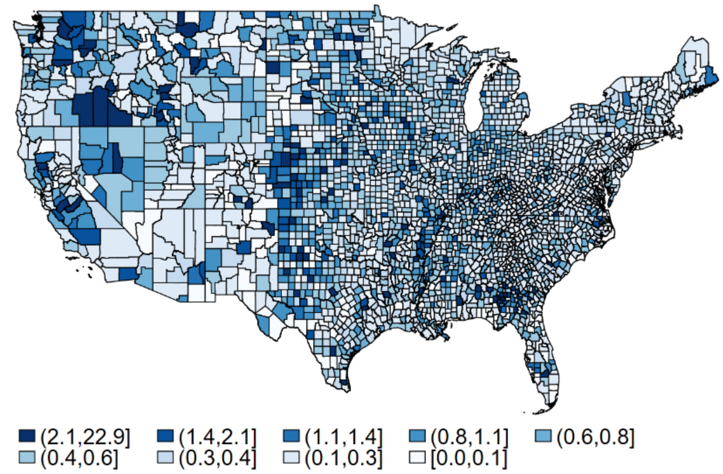
A. US trade war tariff shock ($000s per worker)



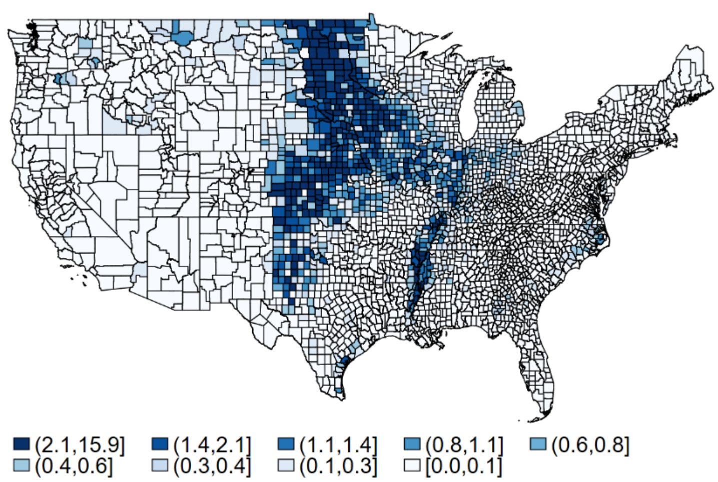
B. Foreign retaliatory trade war tariff shock ($000s per worker)



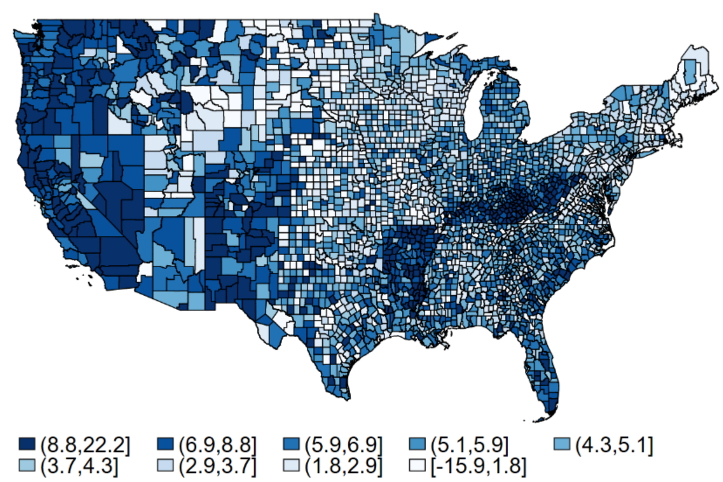
C. Agricultural subsidies ($000s per worker)



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



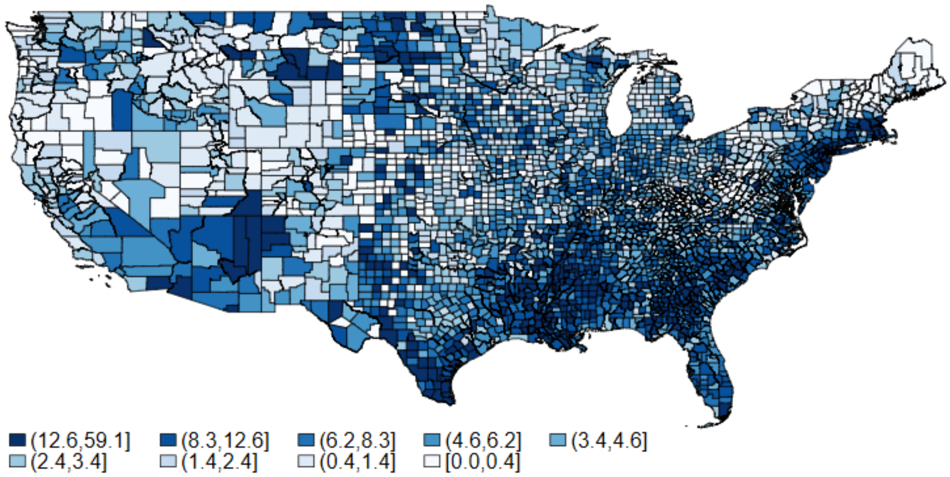
E. Agricultural subsidies ($000s per worker)



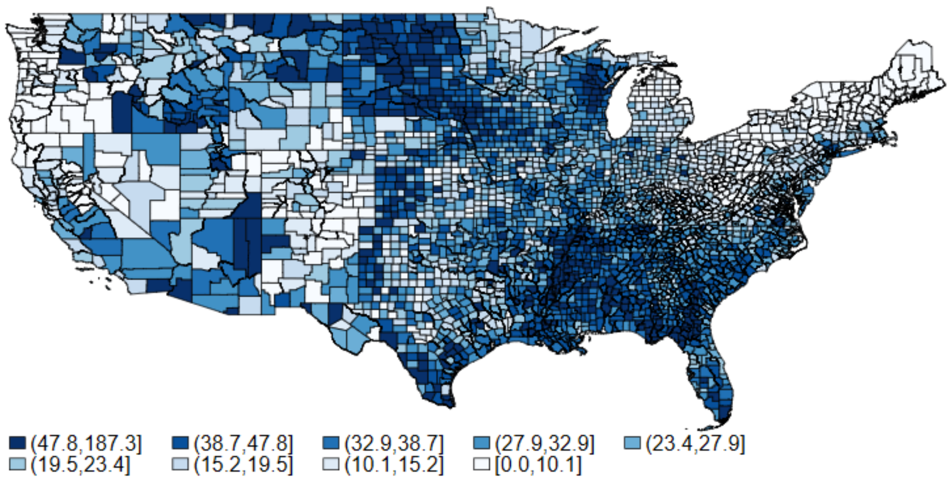
F. Health insurance coverage expansion (2013-2018, % pts)

Figure 2: Trade war and health insurance coverage expansion variables

Notes: Maps represent the 3108 mainland US counties. Panel A-C cutoffs divide counties into equally sized bins. Panel D-E cutoffs use cutoffs from Panel A. 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 shares in 2018 and 2013 Census 5-year ACS. See main text for further details.



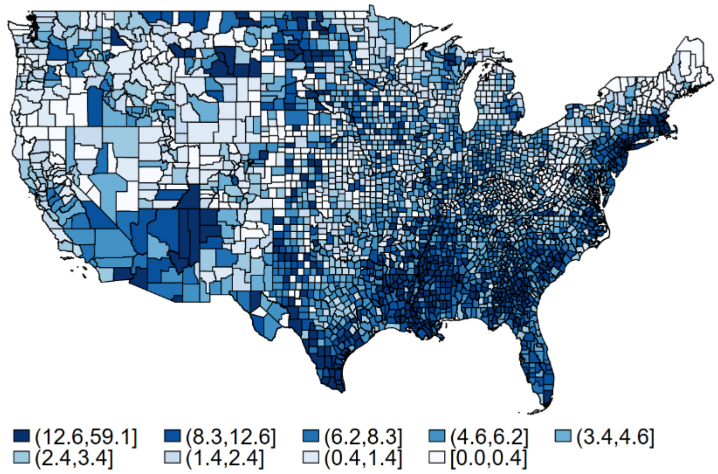
A. COVID-19 cumulative deaths (per 10,000 population)



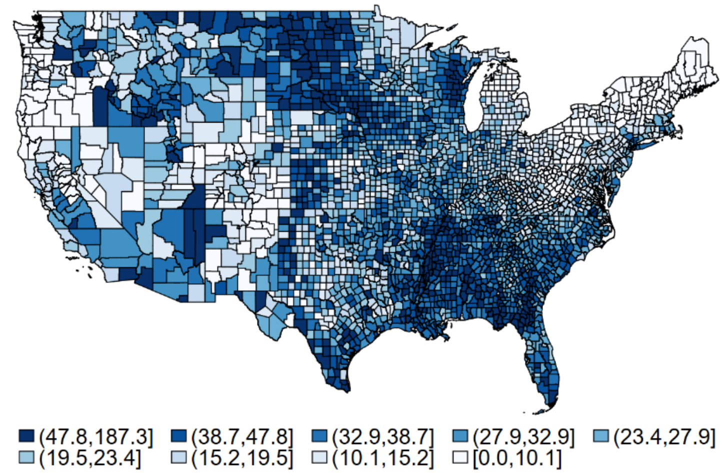
B. COVID-19 cumulative cases (per 1,000 population)

Figure 3: COVID-19 prevalence

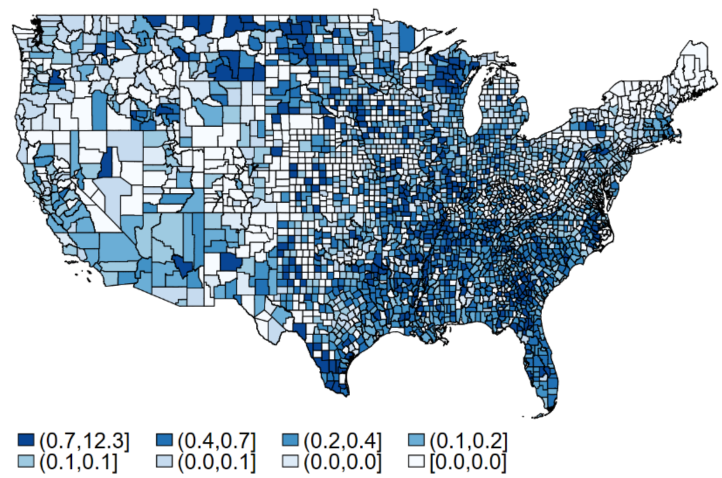
Notes: Maps represent the 3108 mainland US counties. COVID-19 data source is COVID County Data (https://covidcountydata.org/). Population is 2018 population from 2018 5-year Census ACS. See main text for further details.



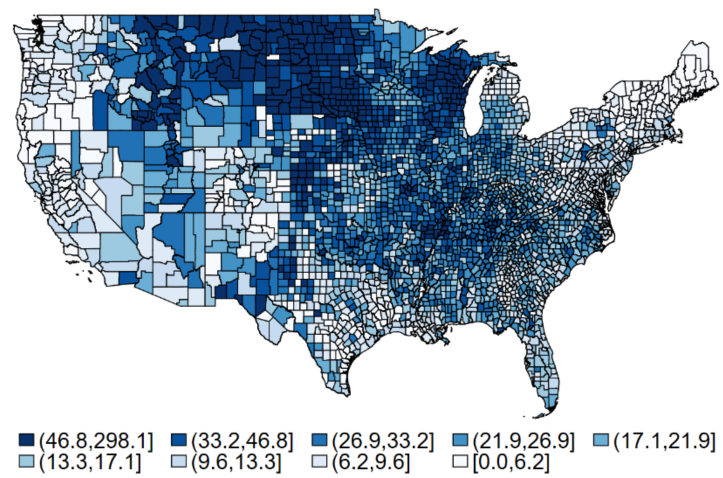
A. COVID-19 cumulative deaths (per 10,000 population)



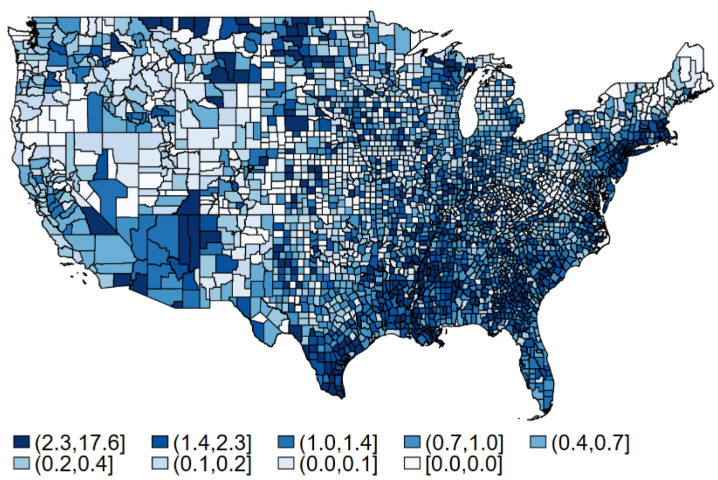
B. COVID-19 cumulative cases (per 1000 population)



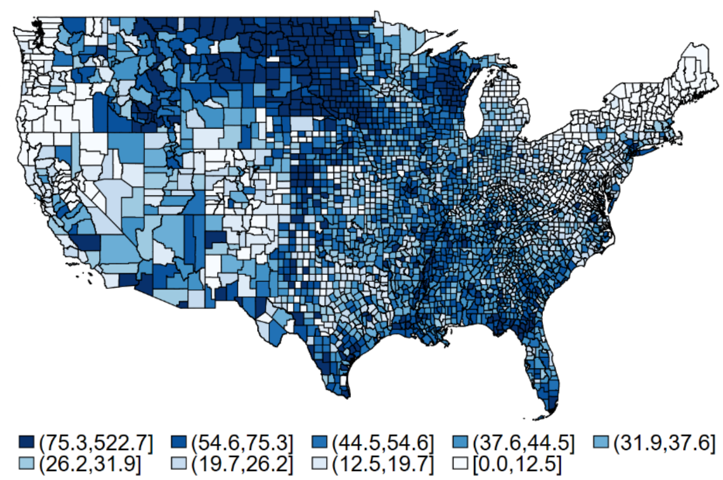
C. COVID-19 October deaths (daily average per 100,000 pop.)



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



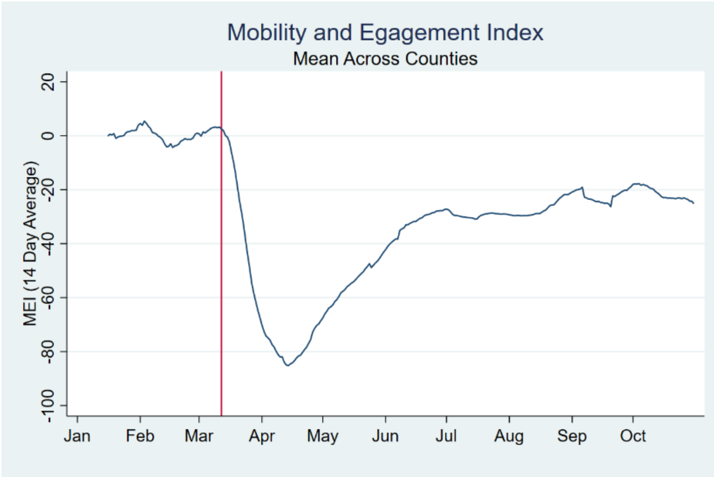
E. COVID-19 deaths (max 14-day average, per 100,000 pop.)

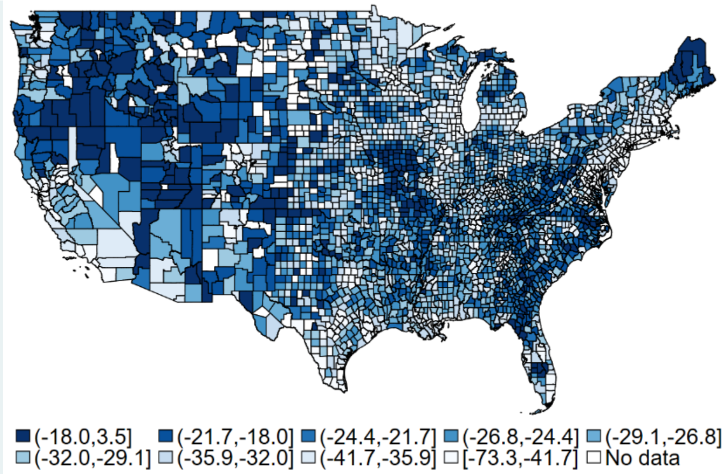


F. COVID-19 cases (max 14-day average, per 100,000 pop.)

Figure A1: Alternative measures of COVID-19 prevalence

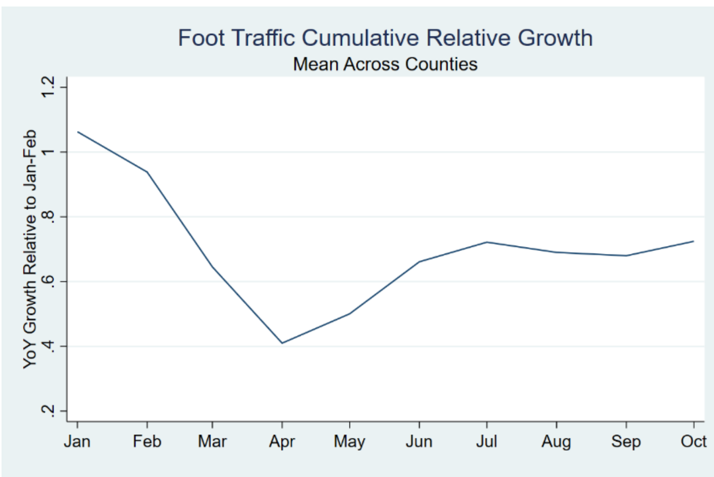
Notes: Maps represent the 3108 mainland US counties. COVID-19 data source is COVID County Data (https://covidcountydata.org/). Population is 2018 population from 2018 5-year Census ACS. Panels A-B cumulative data is through October 31, 2020. Panels E-F are county-level maximum 14-day rolling averages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

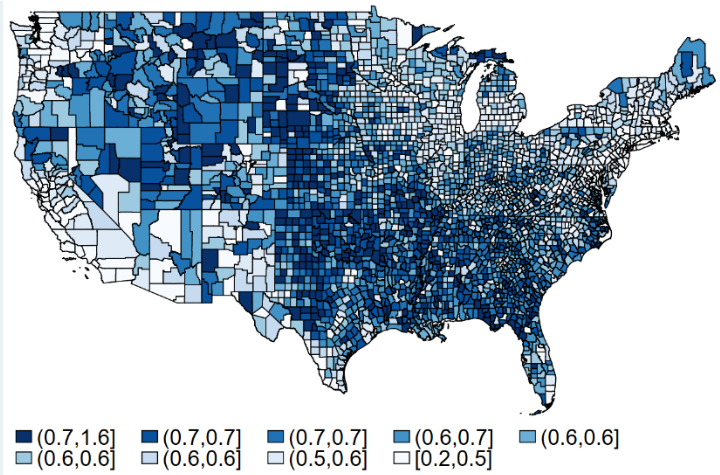




A. Daily MEI: 1/1/2020-10/31/2020

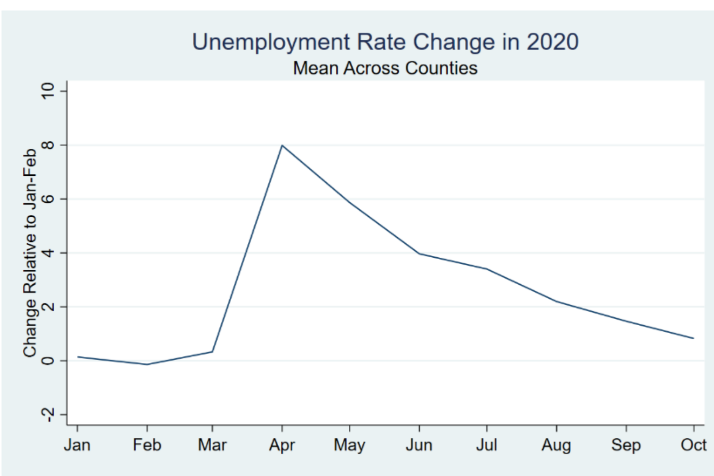
B. MEI daily average (1/1/2020-10/31/2020)

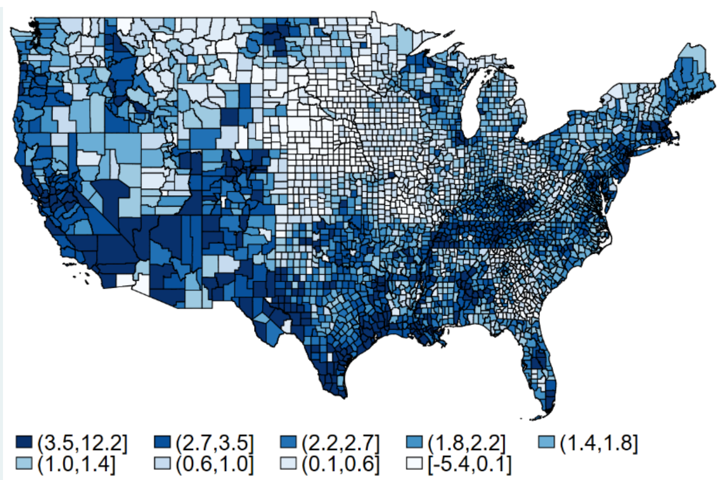




C. Foot traffic relative growth

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. Control variables

A. Demographic

- Age distribution (population shares)
  - Age 20-24
  - Age 25-44
  - Age 45-64
  - Age 65-74
  - Age 75+
- Gender distribution (population shares)
  - Female
- Race (population shares)
  - Hispanic
  - Asian
  - Black
  - White (only)
  - Other race
- Ethnicity (population shares)
  - Foreign language at home (age 5+)
  - Foreign born
  - Naturalized citizens

B. Socio-economic

- Income distribution (population shares)
  - H/hold annual income $25k-$50k
  - H/hold annual income $50k-$75k
  - H/hold annual income $75k-$100k
  - H/hold annual income $100k-$150k
  - H/hold annual income $150k-$200k
  - H/hold annual income $200k plus
- Median real household income
- Education distribution (population shares)
  - High school graduates
  - Some college
  - College graduates
- Poverty (population share)
- Social capital

C. Economic

- Sectoral employment composition
  - Manufacturing employment share
  - Agriculture & mining employment share
  - Unemployment rate (population share age 16+)

C. Economic (cont.)

  - Not in labor force (population share age 16+)
- Size and density
  - Population (2016)
  - Metro size: large (2013)
  - Metro size: medium or small (2013)
  - Share of multi-unit housing structures (2016)
  - Public transport commuters (2016, share of emp)
  - Effective population density

D. COVID-19

- Direct
  - Deaths
    - Cumulative (per 10k pop, through 10/31/2020)
    - October (per 100k pop, per day)
    - Peak (per 100k, max 14-day rolling daily avg)
  - Cases
    - Cumulative (per 10k pop, through 10/31/2020)
    - October (per 100k pop, per day)
    - Peak (per 100k, max 14-day rolling daily avg)
- Indirect
  - MEI
    - Daily average (1/1/2020 - 10/31/2020)
    - October daily average (10/1/2020 - 10/31/2020)
    - Daily avg over max 14-day death window
    - Daily avg over max 14-day case window
  - Foot traffic
    - Cumulative relative growth
    - October relative growth
    - Relative growth - max 14-day death window
    - Relative growth - max 14-day case window
  - Unemployment rate change (Oct 2019 to Oct 2020)
  - Health characteristics
    - % diabetic with annual eye test
    - % diabetic with annual lipids test
    - % diabetic with annual hemoglobin test
    - 30-day mortality for pnuemonia
    - 30-day mortality for heart failure
    - 30-day hospital mortality rate index
  - Remote workers (2016, share of emp)

Notes: Unless otherwise noted, controls in Panels A, B and C from 5-year ACS in 2016 level and 2016-2012 change. Time-invariant variables in Panels A-C: social capital (Rupsasingha et. al. (2006)), metro size (National Center for Health Statistics), effective population density (Desmet & Wacziarg (2021)). Panel D data sources: covidcountydata.org (COVID-19 data), Atkinson et. al. (2020) (MEI data), SafeGraph (foot traffic data), BLS (unemployment rate), Chetty et. al. (2016) (health data), Dingel & Nieman (2019) (remote workers).

Table 3. Baseline results

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ 2-party Rep. vote Share 2012-2016	0.038	0.036	0.032	0.088	0.213*	0.217*	0.218*
	(0.062)	(0.061)	(0.062)	(0.059)	(0.033)	(0.032)	(0.032)
US tariff shock	-0.048	-0.064	-0.064	0.129#	0.183*	0.180*	0.178*
	(0.291)	(0.289)	(0.288)	(0.074)	(0.050)	(0.051)	(0.050)
Retalitory tariff shock		0.144	0.097	-0.189	-0.246^	-0.211#	-0.200#
		(0.160)	(0.160)	(0.121)	(0.120)	(0.108)	(0.100)
Agricultural subsidies			0.440^	0.554*	0.404*	0.491*	0.501*
			(0.198)	(0.169)	(0.132)	(0.134)	(0.126)
COVID-19 deaths (cum., per 10k pop.)						0.001	0.001
						(0.018)	(0.018)
Δ Health insurance coverage							-0.091#
							(0.050)
N	3112	3112	3112	3112	3111	2991	2991
R^2	0.005	0.005	0.007	0.766	0.852	0.858	0.859
Non-COVID controls	N	N	N	Y	Y	Y	Y
State FE	N	N	N	N	Y	Y	Y
COVID controls	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 Table 2 for list of controls. See main text for further details.

Table 4. Counterfactual two-party vote share margin (% points)

	(1)	(2)	(3)	(4)	(5)
A. Baseline					
		Counterfactual: removing effects of …			
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion
Nevada	-2.45	-2.57	-2.40	-2.45	-0.67
Pennsylvania	-1.20	-1.51	-1.12	-1.21	-0.56
Wisconsin	-0.64	-1.17	-0.50	-0.70	-0.05
Arizona	-0.31	-0.50	-0.25	-0.32	0.75
Georgia	-0.24	-0.51	-0.14	-0.25	0.69
North Carolina	1.37	1.02	1.46	1.34	2.29
B. IV					
		Counterfactual: removing effects of …			
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion
Nevada	-2.45	-2.53	-2.40	-2.45	2.54
Pennsylvania	-1.20	-1.43	-1.12	-1.20	0.60
Wisconsin	-0.64	-1.02	-0.50	-0.62	1.01
Arizona	-0.31	-0.44	-0.25	-0.31	2.68
Georgia	-0.24	-0.44	-0.14	-0.24	2.37
North Carolina	1.37	1.12	1.46	1.38	3.94
C. Political heterogeneity: competitiveness					
		Counterfactual: removing effects of …			
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion
Nevada	-2.45	-2.57	-2.43	-2.45	-0.34
Pennsylvania	-1.20	-1.46	-1.19	-1.20	-0.68
Wisconsin	-0.64	-1.09	-0.61	-0.68	-0.18
Arizona	-0.31	-0.38	-0.31	-0.32	0.12
Georgia	-0.24	-0.44	-0.22	-0.24	0.50
North Carolina	1.37	1.01	1.39	1.35	2.13
D. Political heterogeneity: Trump vs Clinton counties					
		Counterfactual: removing effects of …			
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion
Nevada	-2.45	-2.63	-2.44	-2.45	0.52
Pennsylvania	-1.20	-1.50	-1.19	-1.21	-0.46
Wisconsin	-0.64	-1.06	-0.61	-0.67	-0.08
Arizona	-0.31	-0.44	-0.30	-0.32	0.32
Georgia	-0.24	-0.48	-0.22	-0.24	0.90
North Carolina	1.37	1.06	1.39	1.35	2.32

Table 4 (cont). Counterfactual two-party vote share margin (% points)

E. Political heterogeneity: race

	(1)	(2)	(3)	(4)	(5)
		Counterfactual: removing effects of …			
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion
Nevada	-2.45	-2.57	-2.42	-2.45	-0.48
Pennsylvania	-1.20	-1.41	-1.20	-1.20	-0.90
Wisconsin	-0.64	-0.98	-0.64	-0.66	-0.48
Arizona	-0.31	-0.44	-0.28	-0.32	0.06
Georgia	-0.24	-0.50	-0.19	-0.24	0.58
North Carolina	1.37	1.08	1.41	1.36	1.85

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
Nevada	-2.45	-2.52	-2.41	-2.45	-0.67
Pennsylvania	-1.20	-1.37	-1.14	-1.21	-0.56
Wisconsin	-0.64	-0.95	-0.52	-0.70	-0.05
Arizona	-0.31	-0.43	-0.25	-0.32	0.75
Georgia	-0.24	-0.42	-0.15	-0.25	0.69
North Carolina	1.37	1.15	1.45	1.34	2.29

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
Nevada	-2.45	-2.56	-2.41	-2.45	-0.63
Pennsylvania	-1.20	-1.51	-1.13	-1.21	-0.54
Wisconsin	-0.64	-1.15	-0.51	-0.70	-0.04
Arizona	-0.31	-0.48	-0.26	-0.32	0.78
Georgia	-0.24	-0.44	-0.15	-0.25	0.71
North Carolina	1.37	1.11	1.45	1.34	2.31

H. Heterogeneity by COVID prevalence

		Counterfactual: removing effects of …			
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion
Nevada	-2.45	-2.53	-2.41	-2.45	-0.67
Pennsylvania	-1.20	-1.45	-1.14	-1.21	-0.56
Wisconsin	-0.64	-0.98	-0.52	-0.70	-0.05
Arizona	-0.31	-0.50	-0.25	-0.32	0.75
Georgia	-0.24	-0.48	-0.15	-0.25	0.69
North Carolina	1.37	1.15	1.45	1.34	2.29

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 (7) of Table 3 for Panel A, column (7) of Table 5 for Panel B, columns (2)-(4) from Panel A of Table 7 for Panel C, columns (5)-(6) from Panel A of Table 7 for Panel D, columns (7)-(8) from Panel A of Table 7 for Panel E, columns (2)-(3) from Panel B of Table 7 for Panels F-G, and columns (4)-(6) from Panel B of Table 7 for Panel H. See main text for more details.

Table 5. Instrumental variables estimation

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
US tariff shock	0.178*	0.128#	0.182*	0.185*	0.147^	0.162*	0.128^
	(0.050)	(0.066)	(0.051)	(0.044)	(0.060)	(0.047)	(0.059)
Retalitory tariff shock	-0.200#	-0.191#	-0.214^	-0.178#	-0.191^	-0.155^	-0.202^
	(0.100)	(0.099)	(0.097)	(0.095)	(0.092)	(0.068)	(0.082)
Agricultural subsidies	0.501*	0.501*	0.519*	0.033	0.031	0.603*	-0.151
	(0.126)	(0.126)	(0.121)	(0.159)	(0.152)	(0.118)	(0.136)
Δ Health insurance coverage	-0.091#	-0.091#	-0.092#	-0.081#	-0.083#	-0.234*	-0.254*
	(0.050)	(0.050)	(0.050)	(0.048)	(0.047)	(0.084)	(0.072)
COVID-19 deaths	0.001	0.001	-0.001	0.002	0.000	-0.010	-0.014
(cum., per 10k pop.)	(0.018)	(0.018)	(0.018)	(0.018)	(0.017)	(0.017)	(0.015)
N	2991	2991	2991	2991	2991	2991	2991
Endogenous variables		US tariffs	Foreign tariffs	Agric. subsidies	Trade war variables	Health insurance	Trade war and health insurance
Underidentification p-value		0.001	0.093	0.049	0.088	0.002	0.054
K-P weak instrument rk F-statistic		89.471	188.23	23.437	10.616	55.243	12.986
Overidentification p-value		0.878	0.625	0.879	0.960	0.290	0.649
Sargan-Hansen endogeneity p-value		0.312	0.934	0.013	0.024	0.034	0.002

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) and IV-GMM in columns (2)-(7). In all specifications: full set of controls and fixed effects as in column (7) of Table 3, regressions weighted by 2020 total Presidential votes cast, standard errors clustered by state. Columns (2)-(7) use Lewbel instruments created by demeaning and multiplying the following variables by the first stage residuals: manufacturing employment share, and the share of households with annual household income between $25,000 and $50,000 in column (2); employment share in agricultural and mining, and the 2016-2012 change in the share of workers commuting by public transport 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, MEI daily average (1/1/2020-10/31/2020) 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). See main text for further details.

Table 6. Robustness specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Alternative COVID-19 prevalence definitions						
US tariff shock	0.178*	0.172*	0.175*	0.172*	0.178*	0.170*
	(0.050)	(0.051)	(0.050)	(0.051)	(0.050)	(0.051)
Retalitory tariff shock	-0.200#	-0.196^	-0.187#	-0.191#	-0.178#	-0.172#
	(0.100)	(0.097)	(0.099)	(0.100)	(0.101)	(0.100)
Agricultural subsidies	0.501*	0.515*	0.496*	0.504*	0.470*	0.482*
	(0.126)	(0.127)	(0.129)	(0.126)	(0.129)	(0.134)
Δ Health insurance coverage	-0.091#	-0.088#	-0.091#	-0.090#	-0.086#	-0.081#
	(0.050)	(0.049)	(0.050)	(0.050)	(0.050)	(0.047)
COVID-19	0.001	-0.007	0.293#	0.000	0.031	-0.001
	(0.018)	(0.005)	(0.160)	(0.004)	(0.059)	(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	-0.069	0.042				
	(0.079)	(0.100)				
Retalitory tariff shock	-0.045	-0.057				
	(0.072)	(0.057)				
Agricultural subsidies	0.973*	0.572^				
	(0.295)	(0.274)				
Δ Health insurance coverage	0.025	-0.028				
	(0.064)	(0.126)				
COVID-19 deaths (cum., per 10k pop.)	-0.01	-0.015				
	(0.026)	(0.021)				
N	2991	2991				
Endogenous variables	None	Trade war Health insurance				
Instruments		Lewbel				
Underidentification p-value		0.054				
K-P weak instrument rk F-statistic		13.585				
Overidentification p-value		0.091				
Sargan-Hansen endogeneity p-value		0.655				

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, and IV-GMM in column (2) of Panel B. In all specifications: full set of controls and fixed effects as in column (7) of Table 3, 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 5. See main text for further details.

Table 7. Heterogenous effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Political heterogeneity								
US tariff shock	0.178*	0.077#	0.555*	0.044	0.085^	0.347	0.115^	0.346
	(0.050)	(0.040)	(0.195)	(0.055)	(0.037)	(0.207)	(0.045)	(0.287)
Retalitory tariff shock	-0.200#	-0.021	-0.257	0.035	-0.054	-0.011	0.004	-0.576
	(0.100)	(0.048)	(0.309)	(0.184)	(0.062)	(0.209)	(0.067)	(0.370)
Agricultural subsidies	0.501*	0.156#	1.138^	0.279	0.161	0.868	0.176	0.437
	(0.126)	(0.079)	(0.466)	(0.287)	(0.104)	(0.526)	(0.134)	(0.388)
Δ Health insurance cov.	-0.091#	-0.032	-0.142^	-0.033	-0.031	-0.167^	-0.025	-0.135
	(0.050)	(0.031)	(0.065)	(0.051)	(0.031)	(0.068)	(0.032)	(0.099)
COVID-19	0.001	0.007	0.052#	-0.034	0.004	0.014	-0.007	0.068^
	(0.018)	(0.009)	(0.028)	(0.023)	(0.010)	(0.031)	(0.017)	(0.030)
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	0.178*	0.124^	0.339*	0.106#	0.088	0.193*
	(0.050)	(0.052)	(0.082)	(0.053)	(0.072)	(0.058)
Retalitory tariff shock	-0.200#	-0.222^	-0.251^	-0.115	-0.276^	-0.083
	(0.100)	(0.101)	(0.113)	(0.104)	(0.103)	(0.100)
Agricultural subsidies	0.501*	0.503*	0.504*	0.093	0.407^	0.388#
	(0.126)	(0.127)	(0.125)	(0.121)	(0.188)	(0.215)
Δ Health insurance cov.	-0.091#	-0.091#	-0.093#	-0.012	-0.013	-0.043
	(0.050)	(0.050)	(0.049)	(0.035)	(0.051)	(0.040)
COVID-19	0.001	0.001	0.002	-0.128	-0.018	-0.002
	(0.018)	(0.018)	(0.018)	(0.086)	(0.072)	(0.022)
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 (7) of Table 3, 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 or 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 8. Impact of COVID-19

Variable	**(1)**	**(2)**	**(3)**	**(4)**
COVID-19 deaths	0.169*	0	-0.338	0.121^
(cum., per 10k pop.)	(0.049)	(0.018)	(0.217)	(0.053)
MEI Daily average (1/1/20 - 10/31/20)		-0.021#	-0.055#	(0.008)
		(0.012)	(0.031)	(0.012)
Foot traffic cumulative relative growth		-1.667^	0.111	-2.299*
		(0.751)	(1.762)	(0.743)
Unemployment rate change (10/2019-10/2020)		0.201^	0.315^	0.161#
		(0.083)	(0.144)	(0.084)
N	3112	2991	2991	2991
Controls and fixed effects	N	Y	Y	Y
Instruments	None	None	Meat-packing	Nursing home
Underidentification p-value			0.071	0
K-P weak instrument rk F-statistic			3.05	56.718

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 OLS in columns (1)-(2) and IV in columns (3)-(4). In all specifications, regressions weighted by 2020 total Presidential votes cast and standard errors clustered by state. In columns (2)-(4), full set of controls and fixed effects as in column (7) of Table 3. See main text for further details.

Table A1. Summary statistics

	Mean	SD	Min	Max	N
Voting variables					
Change in 2-party Rep. Pres. Vote share (2016 to 2020)	-0.55	2.58	-8.08	28.16	3,112
Change in 2-party Rep. Pres. Vote share (2012 to 2016)	5.88	5.21	-16.52	24.29	3,112
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 coverage 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
Demographic variables					
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
Female	49.98	2.33	21.50	58.50	3,112
Hispanic	9.62	13.28	0.64	95.49	3,112
Asian	1.82	3.02	0.20	60.93	3,112
Black	9.97	13.33	0.23	70.91	3,112
White (only)	76.44	17.80	3.57	97.01	3,112
Other race	5.23	6.48	0.45	79.13	3,112
Foreign language at home (age 5+)	9.29	11.61	0.00	96.10	3,112
Foreign born	4.62	5.63	0.00	52.20	3,112
Naturalized citizens	42.97	18.89	0.00	100.00	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
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 race	0.14	2.53	-23.08	27.05	3,112

Table A1 (cont.). Summary statistics

	Mean	SD	Min	Max	N
Foreign language at home (age 5+)	0.19	1.81	-13.10	39.00	3,112
Foreign born	0.18	1.19	-8.10	20.20	3,112
Naturalized citizens	2.39	19.46	-100.00	100.00	3,112
Socioeconomic variables					
Population Shares (2016)					
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
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
Poverty	16.44	6.54	1.80	53.90	3,112
Other					
Median household income (real)	47,811	12,486	18,972	125,672	3,112
Social capital	0.00	1.26	(3.18)	21.81	3,112
Change between 2012 and 2016					
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
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
Poverty	0.11	2.78	-20.10	15.00	3,112
Median household income (real)	2,321	3,448	-18,810	31,146	3,112
Economic variables					
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
Public transport commuters	0.95	3.10	0.00	61.80	3,112

Table A1 (cont.). Summary statistics

	Mean	SD	Min	Max	N
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					
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
Effective population density	403.84	719.47	3.46	22,647	3,112
Change between 2012 and 2016					
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
Public transport commuters	-0.01	0.68	-16.50	13.70	3,112
Unemployed	-1.05	1.35	-10.40	9.00	3,112
Not in labor force	1.64	2.75	-18.90	27.80	3,112
Population	3020.51	14,389	-54,876	332,505	3,112
Share of multi-unit housing structures	0.23	1.66	-9.19	14.02	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
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
% 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
Remote workers (2016, share of emp)	0.31	0.05	0.22	0.65	3,112

Notes: See main text for further details.

Table A2. Baseline results for control variables

	Level	Change		Level	Change
Demographic			***Econmomic***		
Age 20-24	0.21 *	-0.11 *	Manufacturing employment share	0.07 *	-0.01
Age 25-44	0.12 ^	0.03 *	Agriculture & mining employment share	0.05	-0.04
Age 45-64	0.21 *	-0.10 ^	Unemployment rate	0.06	0.14
Age 65-74	-0.06	0.01 *	Not in labor force	0.06 *	0.02
Age 75+	0.07	0.69 *	Population	0.00	0.00
Female	0.08	-0.12	Metro size: large (2013)	-0.31	
Hispanic	-0.04	-0.07	Metro size: medium or small (2013)	-0.23 ^	
Asian	-0.11 #	0.12	Share of multi-unit housing structures	-0.01	0.02
Black	-0.04	-0.02	Public transport commuter	0.04	-0.13
White (only)	-0.09 #	0.09	Effective population density	0.00	
Other race	-0.09 ^	0.14 *			
Foreign language at home (age 5+)	0.15 *	-0.03	***COVID-19***		
Foreign born	0.00	-0.14	MEI Daily average (1/1/20 - 10/31/20)	-0.02 #	
Naturalized citizens	0.01 ^	0.00	Foot traffic cumulative relative growth	-1.67 ^	
			Unemployment rate change (10/2019-10/2020)	0.19 ^	
Socio-economic			% diabetic with annual hemoglobin test	0.00	
H/hold annual income $25k-$50k	-0.25 *	0.08	% diabetic with annual eye test	-0.01	
H/hold annual income $50k-$75k	-0.24 *	0.09	% diabetic with annual lipids test	0.03	
H/hold annual income $75k-$100k	-0.12 ^	0.01	30-day hospital mortality rate index	0.05	
H/hold annual income $100k-$150k	-0.16 *	0.18 #	30-day mortality for heart failure	-3.26	
H/hold annual income $150k-$200k	-0.44 *	0.22 #	30-day mortality for pnuemonia	-5.68	
H/hold annual income $200k plus	-0.15 ^	0.22 ^	Remote workers	-1.62	
Median real household income	0.00	0.00			
High school graduates	0.12 *	-0.08 #	***Health insurance***		
Some college	-0.08 ^	0.07	Health insurance share (2013)	-0.01	
College graduates	0.08 ^	-0.10 *			
Poverty	-0.04	0.02			
Social capital	0.16 ^				

Notes: # p<0.10, ^ p<.05, * p<.01. Table reports point estimates for controls in column (7) of Table 3. See Table 3 for more details on specification. See Table 2 for more details on control variables. Except as noted in Table 2 and the main text, "level" refers to 2016 level and "change" refers to the 2016-2012 change. See main text for additional details.