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**Working Paper**

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CESifo Working Paper, No. 9669

**Provided in Cooperation with:**

Ifo Institute – Leibniz Institute for Economic Research at the University of Munich

*Suggested Citation:* Lake, James; Nie, Jun (2022) : The 2020 US Presidential Election and Trump's Trade War, CESifo Working Paper, No. 9669, Center for Economic Studies and Ifo Institute (CESifo), Munich

This Version is available at:

<https://hdl.handle.net/10419/260799>

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# The 2020 US Presidential Election and Trump's Trade War

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## **Impressum:**

CESifo Working Papers

ISSN 2364-1428 (electronic version)

Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH

The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute

Poschingerstr. 5, 81679 Munich, Germany

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Editor: Clemens Fuest

<https://www.cesifo.org/en/wp>

An electronic version of the paper may be downloaded

- from the SSRN website: [www.SSRN.com](http://www.SSRN.com)
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# The 2020 US Presidential Election and Trump's Trade War

## 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. These effects cross political and racial lines, suggesting the mechanism operates through the impact on local economies rather than political polarization. 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.

JEL-Codes: D720, F130, F140, I180.

Keywords: 2020 US Presidential election, Trump, Affordable Care Act, health insurance, trade war, tariffs, retaliation.

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This draft: March 25, 2022

First draft: December 1, 2020

We would like to thank Klaus Desmet, Mina Kim, Dan Millimet, Emanuel Ornelas and seminar participants at the Empirical Trade Online Seminar, SMU and Deakin University for helpful discussion and comments. This paper was previously circulated under the title "Did COVID-19 cost Trump the election?" and "The 2020 US Presidential election and Trump's wars on COVID-19, health insurance, and trade".

# 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.<sup>1</sup> 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 (Vaugh (2019); Flaaen and Pierce (2020)), and lower US exports via higher input tariffs (Handley et al. (2020)).<sup>2</sup> 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]he 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 have continued to pursue executive, congressional, and judicial avenues to repeal and undermine the ACA. These av-

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<sup>1</sup>See <https://trumpwhitehouse.archives.gov/trump-administration-accomplishments/>.

<sup>2</sup>See Fajgelbaum and Khandelwal (2021) for a recent survey on the economic effects of the trade war.

enues arguably 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 economic, socioeconomic, demographic, health, and political dimensions that could correlate with the salient issues discussed above.<sup>3</sup> 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. Our

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<sup>3</sup>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.

results imply the absence of US tariffs would have pushed Georgia and Wisconsin out of recount territory and, hence, would have been decisive in a slightly tighter election.

Motivated by the recent work of [Autor et al. \(2020\)](#) and [Che et al. \(2020\)](#), we investigate whether the mechanism driving the effect of trade war tariffs on voting behavior centered around political polarization or the local economic effects of Trump’s trade war.<sup>4</sup> We find no evidence of the trade war tariffs simultaneously making solidly “red” Republican counties (or that Trump won in 2016 or that have a majority white population) even “redder” and making 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.

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 economic, social, and political characteristics that both (i) correlate with how the political tariff formation process maps to county-level exposure and (ii) drive the change in voting behavior towards Trump between 2016 and 2020. 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.

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.

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

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<sup>4</sup>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 \(2021\)](#)).

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 that 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. \(2020\)](#), [Che et al. \(2020\)](#), and [Li et al. \(2020\)](#)).

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.<sup>5</sup> [Che et al. \(2020\)](#) argue the pro-Democrat effect of rising Chinese import competition in the 2000s reflected that Democrats typically voted against pro-trade congressional bills. [Conconi et al. \(2014\)](#) show that US politicians facing re-election risk are much more likely to vote against pro-trade congressional bills. When voting on Free Trade Agreements, [Lake and Millimet \(2016\)](#) show that US

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<sup>5</sup>A separate strand of the empirical literature emphasizes the importance of lobbying and campaign contributions on US trade policy. This literature goes back to at least the protection for sale literature (e.g. [Goldberg and Maggi \(1999\)](#); [Gawande and Bandyopadhyay \(2000\)](#); [Bombardini \(2008\)](#); [Gawande et al. \(2012\)](#)) and analyses looking at congressional voting behavior (e.g. [Baldwin and Magee \(2000\)](#); [Im and Sung \(2011\)](#); [Lake \(2015\)](#)). More recent papers have looked at the informational role of lobbying (e.g. [Ludema et al. \(2018\)](#)) and the contest nature of lobbying whereby lobbying expenditures are sunk before governments make trade policy decisions ([Cole et al. \(2021\)](#); [Blanga-Gubbay et al. \(2020\)](#)).



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.<sup>6</sup> 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. Thus, we cannot reject the null that the trade war tariffs are actually exogenous given our set of fixed effects as well as economic, socioeconomic, demographic, and political controls.<sup>7</sup> 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 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.

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<sup>6</sup>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.

<sup>7</sup>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.

## 2 Empirical strategy

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

$$\Delta V_c^{2020} = \beta_0 \Delta V_c^{2016} + TW_c \beta_1 + X_c \beta_2 + \delta_s + \varepsilon_c. \quad (1)$$

$\Delta V_c^y$  is the change in the two-party Republican vote share between Presidential elections in year  $y$  and year  $y - 4$ .  $TW_c$  is a vector of trade war variables.  $X_c$  includes all other covariates.  $\delta_s$  are state fixed effects. Following earlier literature (e.g. [Autor et al. \(2020\)](#), [Blanchard et al. \(2019\)](#)), we weight by total votes cast in the 2020 Presidential election and cluster standard errors by state.

The clear identification threat is omitted variable bias that leads to endogeneity of the trade war variables. This would require omitted variables that are correlated with our trade war variables *and* also drive the *change* in voting behavior between 2016 and 2020. Indeed, given the inclusion of  $\Delta V_c^{2016}$  in (1), these omitted variables would need to drive the change in voting behavior between 2016 and 2020 after conditioning on the change in voting behavior between 2012 and 2016.<sup>8</sup> Thus, omitted variables that drive permanent or long-run aspects of voting behavior do not pose an endogeneity problem. Nor do omitted variables that drive changes in voting behavior between 2016 and 2020 but were already driving changes in voting behavior between 2012 and 2016 as part of a trend in the evolution of local voting behavior. Thus, endogeneity concerns really revolve around omitted variables that explain *changes* to voting behavior towards Trump between 2016 and 2020.

These omitted variables could take two forms. The first form are emerging and salient electoral issues whose county-level importance happens to correlate with county-level trade war exposure. Issues at the top of the list would include the COVID-19 pandemic, health insurance coverage expansion, and race and immigration issues.

The second form of omitted variables are county-level shocks that influence how voter behavior changes between 2016 and 2020 and also drive how US or foreign governments choose trade war policies to target particular geographical areas of the US. The electoral college system for electing the US president makes each state a winner-take-all contest. In many states, the Democrat and Republican Presidential nominee need independent or swing voters to win the state. Thus, median voter theory suggests governments will use trade war policies to sway independent and swing voters in particular regions of the US (e.g. [Ma and McLaren \(2018\)](#)). Additionally, models revolving around the importance of raising campaign and lobbying contributions suggest that governments may also use trade

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

war policies to target very partisan regions of the US. In either case, trade war policies may target particular regions of the US in ways that depend on the region’s economic, social, and political characteristics. The trade war variables would be endogenous if we omit such characteristics that also happen to drive changing voter behavior between 2016 and 2020.

We take two approaches to deal with trade war variable endogeneity concerns stemming from omitted variable bias. First, we control for a host of county-level economic, socioeconomic, demographic, and political characteristics as well as state-level unobservables along these or other dimensions. These help control for factors that drive how US and foreign governments target trade policy to particular geographic areas of the US. To deal with the salience of the COVID-19 pandemic, these controls include various direct and indirect measures of county-level COVID-19 outbreaks and the resulting local economic and social impacts. To deal with the salience of race and immigration issues, these control include a host of county-level characteristics along race, ethnicity, and linguistic dimensions. To deal with the salience of health insurance coverage expansion, closely following [Blanchard et al. \(2019\)](#), these controls include the county-level post-ACA expansion in health insurance coverage and its initial level as well as county-level health characteristics.

Nevertheless, there may be still be omitted county-level social, economic, political or health characteristics that not only drive county-level changing voter behavior towards Trump between 2016 and 2020 but also help explain county-level exposure to the trade war. Thus, our second approach to dealing with endogeneity concerns instruments for these potentially endogenous trade war variables. Given the lack of obvious instruments, we use [Lewbel \(2012\)](#) heteroskedasticity-based IVs.

The Lewbel approach “first-stage” regresses an endogenous variable  $r$  on the exogenous controls  $\tilde{X} = [\Delta V^{2016} X \delta]$  from (1). For a subset of exogenous controls  $Z_r \subseteq \tilde{X}$ , he shows the identifying assumptions are  $cov[Z_r, u_r^2] \neq 0$  and  $cov[Z_r, \varepsilon u_r] = 0$  where  $u_r$  is the first-stage error term. Intuitively,  $cov[Z_r, u_r^2] \neq 0$  says heteroskedasticity of the first-stage errors  $u_r$  depends on  $Z_r$  but  $cov[Z_r, \varepsilon u_r] = 0$  says the correlation between the first-stage error  $u_r$  and structural error  $\varepsilon$  from (1) does not depend on  $Z_r$ . Lewbel shows these assumptions hold in, among others, situations with classical measurement error of the endogenous variable or situations with an unobserved common factor driving correlation between the first-stage and second-stage errors. An obvious example of a common factor in our context would be local political activism. Given the assumptions,  $\tilde{Z}_r \equiv (Z_r - \bar{Z}_r) \hat{u}_r$  are valid instruments for the endogenous variable  $r$  (i.e. the sample-demeaned  $Z_r$  interacted with the first-stage residuals) when estimating (1) with standard IV techniques.<sup>9</sup>

Lewbel’s approach allows the usual IV specification tests. This includes weak instru-

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

ment and, when  $Z_r$  contains more than one variable, overidentification tests. Intuitively, instrument strength depends on heteroskedasticity of the first-stage errors. Thus, we use the [Koenker \(1981\)](#) Breusch-Pagan test for heteroskedasticity to identify  $Z_r \subseteq \tilde{X}$  that are significantly related to the first-stage error variances.

## 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.<sup>10</sup> Reflecting Trump’s 2016 triumph versus his 2020 demise, the mean change in the Republican vote share between the 2016 and 2020 elections,  $\Delta V_c^{2020}$ , is  $-0.55\%$  points but the mean change between the 2012 and 2016 elections,  $\Delta V_c^{2016}$ , is  $5.88\%$  points (Appendix Table A1 contains all summary statistics).

Panels A-B of Figure 1 show the starkly different geographic distributions of these variables. Relative to the 2012 Republican Presidential nominee Mitt Romney, Panel 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.<sup>11</sup>

### 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.<sup>12</sup> The trade war began with the Trump administration imposing two types of MFN tariffs (i.e. applied to all US imports). In February 2018 came the Section 201 safeguard tariffs on around \$10bn of washing machine and solar panel imports. Then the Section 232 tariffs came in March 2018 on around \$40bn of steel and aluminum imports in the name of defending US national security. While the WTO allows safeguard tariffs, the national security tariffs created immediate and fierce claims of

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<sup>10</sup>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.

<sup>11</sup>The correlation is .264.

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

WTO illegality by US trading partners.<sup>13</sup> 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.

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 tariff imposed by country  $k$  on product  $h$  and country  $j$  as  $\tau_{h,j}^k$  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  $TS_{h,j}^{US} = \tau_{h,j}^{US} m_{h,j}$ . Denoting 2017 US exports by  $x$ , the additional retaliatory tariffs charged on US exports of HS8 product  $h$  to country  $j$  are  $TS_{h,j}^R = \tau_{h,US}^j x_{h,j}$ . Aggregating to the industry-level across US trade partners gives  $TS_h^{US} = \sum_j TS_{h,j}^{US}$  and  $TS_h^R = \sum_j TS_{h,j}^R$ . Finally, we concord to NAICS 3-digit industries using the 2002-2006 [Feenstra et al. \(2002\)](#) trade weights. This gives

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<sup>13</sup>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.

the additional tariffs charged on US imports,  $TS_i^{US}$ , and US exports,  $TS_i^R$ , 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.<sup>14</sup> We then use county-industry employment weights  $\frac{L_{ic}}{L_c}$  to compute the tariff shocks for county  $c$ :

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

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

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

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

### 3.3 Controls

As discussed in Section 2, endogeneity of our trade war variables is the key identification threat. Specifically, the concern is that county-level omitted social, economic, political and

<sup>14</sup>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.

<sup>15</sup>Their correlation is 0.075.

<sup>16</sup>Their correlation is .179. Further, the correlation between US tariff shocks and agricultural subsidies is -0.03.



health characteristics could correlate with county-level trade war exposure *and* with 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 control variables to mitigate these endogeneity concerns.

First, we start with a typical set of county-level economic, socioeconomic and demographic variables (using 5-year samples of ACS data) to control for factors that could affect voting preferences and trade war exposure. We control for the 2016 distributions, and the changes between 2012 and 2016 distributions, of age (six bins), gender, household income (seven bins; and median household income), and education (four bins). Similarly, both in 2016 levels and changes between 2012 and 2016 levels, we control for industrial composition (shares of employment in manufacturing as well as agriculture and mining) and labor market tightness (population shares aged 16-plus that are unemployed and not in the labor force).

Second, we control for the political salience of post-ACA health insurance coverage expansion by closely following [Blanchard et al. \(2019\)](#). The centerpiece of the ACA are the health exchanges that became operational in January 2014. Reflecting the ACA’s transformational nature, the uninsured population share fell from a stable 20% over the 2008-2013 period to 12% by 2016 and has remained stable thereafter ([Keisler-Starkey and Bunch \(2020\)](#)). 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).<sup>17</sup> Panel A of Figure 2 shows significant geographic variation around the mean expansion of 5.05% points (see Table A1). Numerous large counties around major cities in states that decided the 2020 Presidential election saw above-average expansion (including Georgia, Arizona and Nevada). Additionally, we control for the 2013 level of health insurance coverage and county-level health characteristics from [Chetty et al. \(2016\)](#) including diabetes prevalence measures, separate 30-day mortality rates for pneumonia and heart failure, and the 30-day hospital mortality index.<sup>18</sup>

Third, we control for the salience of race and immigration issues by using the 5-year ACS samples to control for the 2016 level and the change between 2012 and 2016 levels of measures related to race, ethnicity, poverty and density (we already controlled for race as described above). Specifically, these measures include the population shares of (i) race across five racial groups, (ii) people where English is not spoken at home, (iii) foreign born people, (iv) naturalized citizens, and (v) people living in poverty. The ACS based density measures

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<sup>17</sup>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. See <https://www.census.gov/programs-surveys/acs/guidance/estimates.html>.

<sup>18</sup>The data can be downloaded from <https://healthinequality.org/data/>.

include population, share of multi-unit housing structures, and the share of workers who commute by public transport. Additional non-ACS based density measures include effective density ([Desmet and Wacziarg \(2021\)](#)) and indicators for large metros, small and medium metros, and non-metros.<sup>19</sup> We also control for social capital ([Rupasingha et al. \(2006\)](#)).

Fourth, we control for the salience of county-level COVID-19 outbreaks using various direct and indirect measures of COVID-19 prevalence. For our direct measures, we collect data on COVID-19 prevalence from [COVID County Data](#) (which merged with [Covid Act Now](#)).<sup>20</sup> 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.<sup>21</sup> The possibility of voters caring about recent or “peak” COVID-19 outbreaks motivate the latter two windows. Panels B-C of Figure 2 show the geographic incidence of COVID-19 cumulative deaths and cases through October 31, 2020. While deaths are relatively higher than cases in the early-hit north-east, cases are relatively higher than deaths in the later-hit Dakotas and Minnesota. Figure A1 illustrates all of our COVID-19 measures.

Given that measuring COVID-19 prevalence can be quite challenging, we augment these direct measures of COVID-19 prevalence with a wide set of indirect measures. 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-

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<sup>19</sup>Effective density differs from standard population density by using the spatial population distribution within a location. Metro indicators can be downloaded from the [Urban-Rural Classification Scheme for Counties](#) of the National Center for Health Statistics (NCHS).

<sup>20</sup>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.

<sup>21</sup>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.



specific seasonality, divide by the analogous growth in 2019. Finally, we control for the share of county employment that can work remotely (Dingel and Neiman (2020)).<sup>22,23</sup>

## 4 Results

### 4.1 Baseline results

Table 2 presents the baseline results. Columns (1)-(3) successively add the three trade war variables: US tariff shock, retaliatory tariff shock, and agricultural subsidies. The only control here is the Republican vote share change between 2012 and 2016,  $\Delta V_c^{2016}$ . 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 2012 and 2016 and county-level trade war exposure is very important. Column (4) adds the trade war controls, flipping the sign and dramatically increasing the magnitude of the point estimates for US and retaliatory tariff shocks. Adding state fixed effects in column (5) and race and immigration controls in column (6) leaves all of the trade war variables highly statistically significant.<sup>24,25</sup> The positive point estimates for the US tariff shock and agricultural subsidies in column (6) say Trump’s county-level vote share was higher when the county had more exposure to US tariff shocks or received more agricultural subsidies.<sup>26</sup> The negative point estimate for the retaliatory tariff shock in column (6) 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 pro-

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<sup>22</sup>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/vol11/occsoc18.shtml>).

<sup>23</sup>Many of our controls motivated above through the need to control for county-level health characteristics or race and immigration characteristics are also shown by Desmet and Wacziarg (2021) to be important correlates of county-level COVID-19 cases and deaths.

<sup>24</sup>Note that the literature typically includes many of these measures as demographic and socioeconomic controls when studying the effects of international trade and international trade policy.

<sup>25</sup>We lose 1 observation in column (5) because state fixed effects lead to Washington D.C. being dropped from the estimation sample.

<sup>26</sup>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.

tection to local economies through tariffs or agricultural subsidies but was hurt politically when local economies suffered from retaliation in foreign markets.

Column (7) adds controls for health insurance coverage expansion.<sup>27</sup> Given the trade war point estimates are virtually unchanged from column (6), county-level health insurance coverage expansion is largely uncorrelated with county-level trade war exposure. So, again, any endogeneity problem for one of the explanatory variables does not spill over to other key explanatory variables.<sup>28</sup> 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.

Column (8) adds our main measure of COVID-19 prevalence – cumulative deaths through October 2020 per 10,000 population – and our COVID-19 control variables.<sup>29</sup> Continuing the theme developed above, the point estimates for the trade war (and health insurance coverage expansion) variables remain very stable in column (8) versus column (7) which says they are largely uncorrelated with COVID-19.<sup>30</sup> So, any potential endogeneity problems with COVID-19 prevalence are not major concerns for endogeneity of the other variables.

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 its 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 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.

Some of the effects described above are economically significant. The point estimates from column (8) of Table 2 imply the median county saw Trump’s 2020 vote share increase by 0.12% and .01% points respectively on account of US trade war tariffs and agricultural

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<sup>27</sup>We lose 60 observations in column (7) because these counties are missing health characteristics data that form part of our control variables for health insurance coverage expansion.

<sup>28</sup>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.

<sup>29</sup>We lose 60 observations in column (8) because these counties are missing MEI data.

<sup>30</sup>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.

subsidies deaths but decrease by 0.06% and 0.42% points respectively on account of retaliatory tariffs and health insurance coverage expansion. However, the effect for a median county is potentially misleading in terms of state-level electoral college outcomes. For example, the median county effect understates the state-level electoral college impact of the US trade war tariffs if large counties were the most exposed to these tariffs.

Table 3 takes these county-level differences into account and illustrates economic significance in terms of state-level electoral college impact. For any variable of interest from Table 2, we use its county-specific value and its column (8) point estimate to compute counterfactual county-level vote shares for Trump and Biden in the absence of this variable. At the county-level, multiplying counterfactual vote shares by total votes gives counterfactual vote tallies. Aggregating to state-level total votes, the implied state-level change in Trump's vote share could be more or less than the median county change. Moreover, since a vote share increase for one candidate implies an equivalent vote share decrease for the other candidate, eliminating a winning candidate's vote share margin requires an offsetting impact of half this margin.

The key takeaway from panel A of Table 3 is that the only economically significant variables are the US tariff shock and health insurance coverage expansion. Comparing column (1) with columns (2)-(4) of panel A in Table 3 reveals economic significance of the trade war variables. Reflecting the narrow set of counties benefiting from agricultural subsidies, Trump's state-level vote share changes by no more than 0.06% points between column (1) and column (4). Despite affecting more counties, removing the effects of foreign trade war tariffs changes Trump's state-level vote share by no more than 0.14% points. However, removing the effects of US tariffs roughly doubles Trump's loss both in Georgia to 0.51% points and in Wisconsin to 1.17% points. This would prevent recounts in both states and could have swung the state electoral college outcomes if the election was only slightly 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% points and 1.06% points respectively. Rather than losing Georgia and Arizona by 0.24% points and 0.31% points respectively, Trump wins by 0.69% points and 0.75% points. Additionally, Trump only loses Wisconsin by 0.05% points instead of the actual 0.64% points. With Georgia and Arizona's electoral 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 4 presents the IV results. For ease of comparison, column (1) presents the OLS results from column (8) of Table 2. Columns (2)-(4) treat one of the trade war variables as endogenous and column (5) treats all trade war variables as endogenous. Given the statistical and economic significance of our findings regarding health insurance coverage expansion in Section 4.1, 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 mostly at the  $p < 0.05$  level. The Kleibergen-Paap weak-instrument  $F$ -stats are in the 20-65 range when treating one variable as endogenous and still exceed the common rule-of-thumb-value of 10 when treating multiple variables as endogenous. And, based on Hansen’s  $J$ -test of overidentification, we always fail to reject the null that the instruments are exogenous with the  $p$ -values in the 0.43-0.83 range. These tests provide evidence that our instruments are strong and exogenous.

Indeed, there is notable evidence that our set of control variables actually contain the key county-level social, economic and political variables that remove endogeneity concerns over county-level exposure to US and foreign retaliatory tariffs. Specifically, based on comparing two Sargan-Hansen statistics, our test of endogeneity says we are far from conventional levels of statistical significance for rejecting the null that the US and foreign retaliatory tariff shocks are exogenous ( $p$ -values of .404 and .665 respectively). This provides support for the identification strategy in the broader trade literature of using county-level tariff exposure measures and controlling for endogeneity concerns using fixed effects and a wide set of county-level social, economic, and political variables.

Nevertheless, we now turn to the IV point estimates. The US tariff shock point estimate falls by around one-third in 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 4, Panel B of Table 3 shows removing its effect still roughly doubles Trump’s loss in Georgia from 0.24% to 0.43% points and increases his loss in Wisconsin by about one-half from 0.64% to 1.00% 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% point).

The point estimate for foreign retaliation falls somewhat in column (7) when treating all trade war variables and health insurance coverage as endogenous. While it remains

statistically significant, column (3) in panel B of Table 3 shows it also remains economically insignificant in affecting state-level electoral college outcomes of closely contested states.

Agricultural subsidies appear to be the trade war variable most susceptible to endogeneity. With  $p = .015$ , the endogeneity test rejects the null that they are exogenous at the  $p < 0.05$  level. And, treating them as endogenous reduces its point estimate from 0.501 in column (1) to 0.052 in column (5) and  $-0.109$  in column (7). This suggests an upward bias due to an omitted variable that is positively correlated with county-level agricultural subsidies and also drives changes in voter behavior towards Trump between 2016 and 2020. Intuitively, this fits closely with the idea that Trump used agricultural subsidies to target a narrow set of politically motivated counties.

If anything, the OLS estimate for health insurance coverage expansion appears downward biased: the IV point estimate in columns (6) and (7) is more than double its OLS value. Moreover, the endogeneity test rejects the null ( $p = .077$ ) that health insurance coverage expansion is exogenous at the  $p = 0.1$  level. As expected, the much larger IV point estimate dramatically increases the economic significance. Column (5) in Panel B of Table 3 says removing the effects of health insurance coverage expansion would now see Trump win Georgia, Arizona, Wisconsin, Pennsylvania and Nevada. Flipping all of these states would have won him re-election.

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

### 4.3 Robustness

**Alternative COVID-19 measures.** 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 5 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 social, 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. Thus, we pursue placebo specifications where the dependent variable is the change in Trump’s vote share between the 2012 and 2016 elections and we remove the 2016-2020 change from the specification.

Panel B of Table 5 presents the results. Column (1) shows the OLS results 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 4 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 ( $p > 0.55$ ). 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 provides a more conservative assessment of economic magnitudes. Third, the fact that our key explanatory variables are uncorrelated with each other means any endogeneity problem with one key explanatory 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. \(2020\)](#) 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 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 6 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.<sup>31</sup> Thus, the key issues we analyze did not simultaneously 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,

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<sup>31</sup>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 (6) is .101 and hence borderline statistically significant at the 10% level.

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 3 shows Trump’s counterfactual winning margin in Georgia increases to 0.90% points and he now *wins* Nevada by 0.52% 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% 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 economic effects of Trump’s policies on their local economies.

#### 4.4.2 Trade war heterogeneity

The trade war initiated by the Trump administration in spring 2018 was eventually dominated by the US-China piece of the trade war. Thus, one may wonder whether the prominence of the US-China trade war 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 6 isolates the effect of the US-China trade war. Here, the US and foreign retaliatory tariff shocks are defined *solely* by, respectively, US tariffs on China and Chinese tariffs on the US. The point estimates imply the median county saw Trump’s 2020 vote share increase by 0.074% points due to US tariffs on China and decrease by 0.056% points due to Chinese tariffs. These effects are somewhat lower than the 0.121% and 0.063% points in our baseline analysis. Indeed, according to column (2) in panel F of Table 3, the effects of US tariffs are sufficiently weaker that removing their pro-Trump effect would still leave Trump in recount territory in Georgia and Wisconsin. 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 allies like the EU, Canada and Mexico – and for various reasons – national security concerns over steel and aluminum imports and concerns over US intellectual prop-



erty rights in China. Thus, one may wonder whether voters paid less attention to subsequent rounds of the trade war through 2019. Alternatively, perhaps these later tariffs were fresher in voter minds in the 2020 Presidential election campaign.

Column (3) of panel B in Table 6 only looks at the tariffs imposed during 2018. The US and foreign retaliatory tariff shocks exclude the escalation in early summer 2019 and the new tariffs in fall 2019. The point estimates imply the median county saw Trump’s 2020 vote share increase by 0.091% points due to US tariffs on China and decrease by 0.056% 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 3 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 was 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 split counties into terciles of COVID-19 prevalence. The point estimates reveal no stark heterogeneities across the terciles. Panels A and H of Table 3 also show that taking this heterogeneity into account does not impact the economic significance of the issues in terms of electoral college outcomes.

## **5 Conclusion**

We analyze the impact of the Trump administration’s trade war on the 2020 US Presidential election. Our results emphasize the political salience of the trade war. 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.

Our results also emphasize the political salience of the post-ACA expansion of health insurance coverage. While the trade war literature has already established the political salience of this issue 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 effects of Trump's policies on local economic outcomes.

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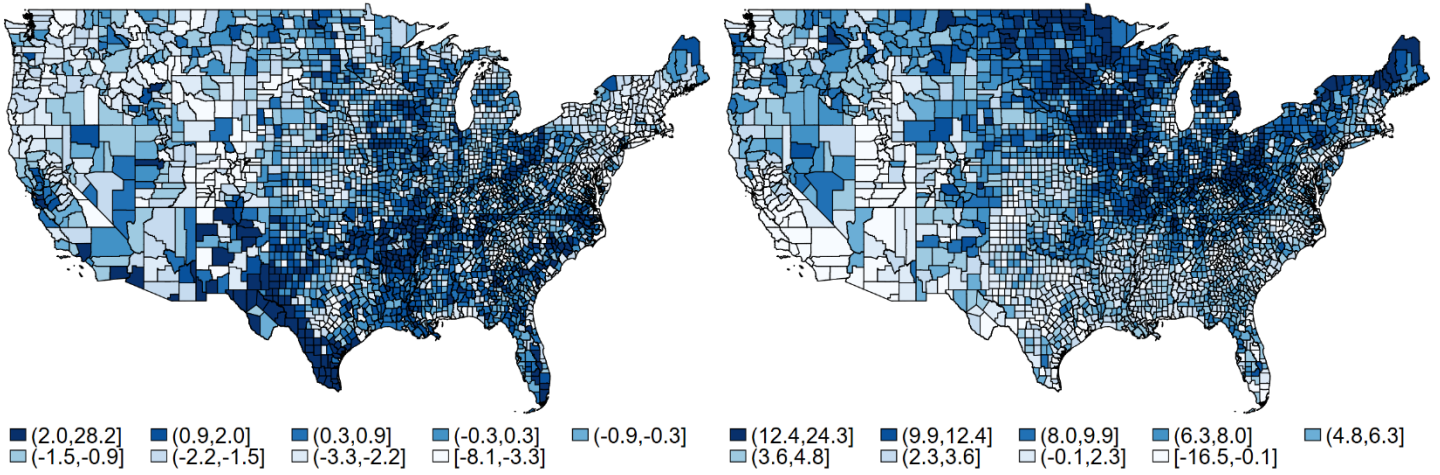
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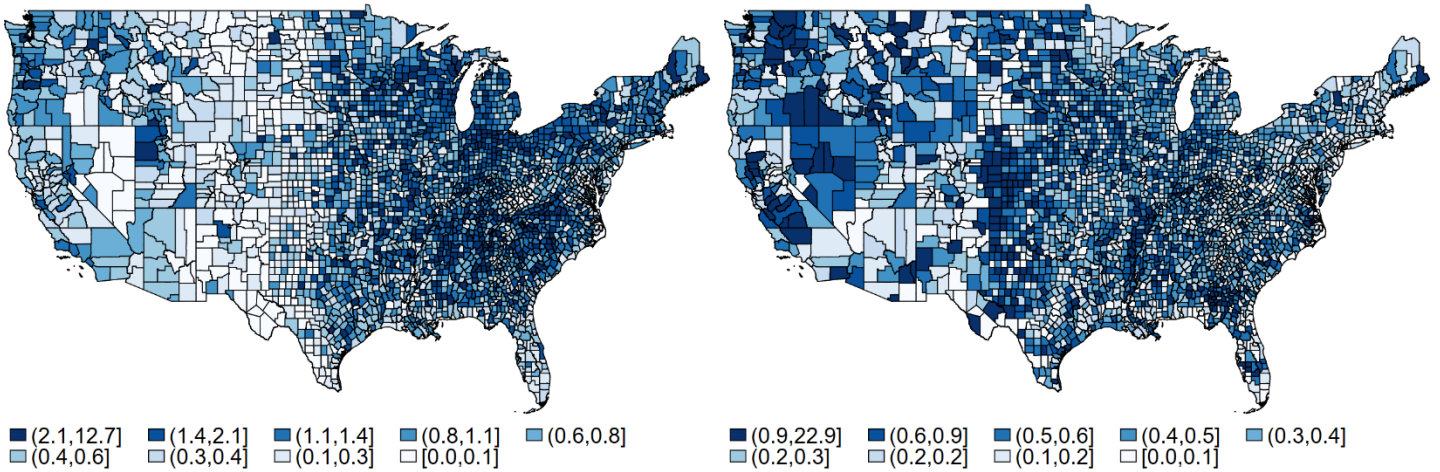
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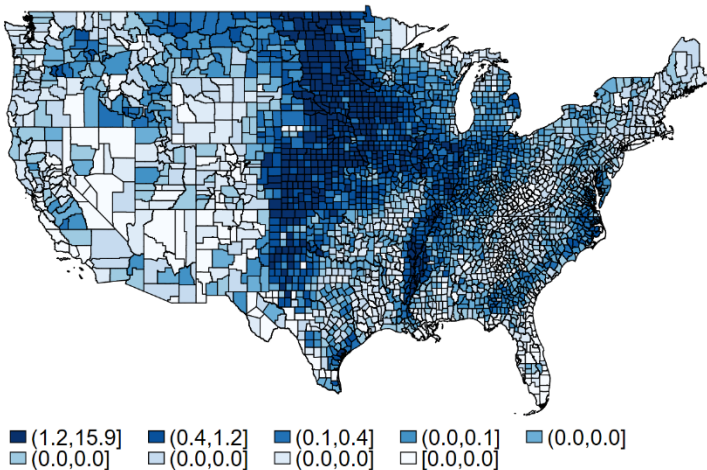
**A. Change in 2-party Republican vote share 2016-2020 (% pts)**

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



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

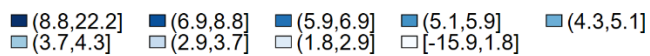
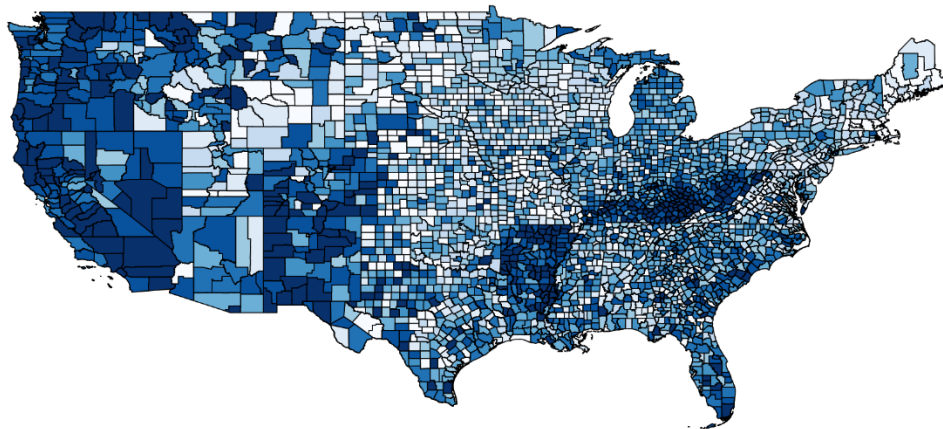
**D. Foreign retaliatory trade war tariff shock (\$000s per worker)**



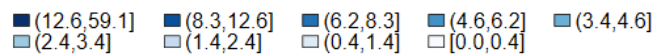
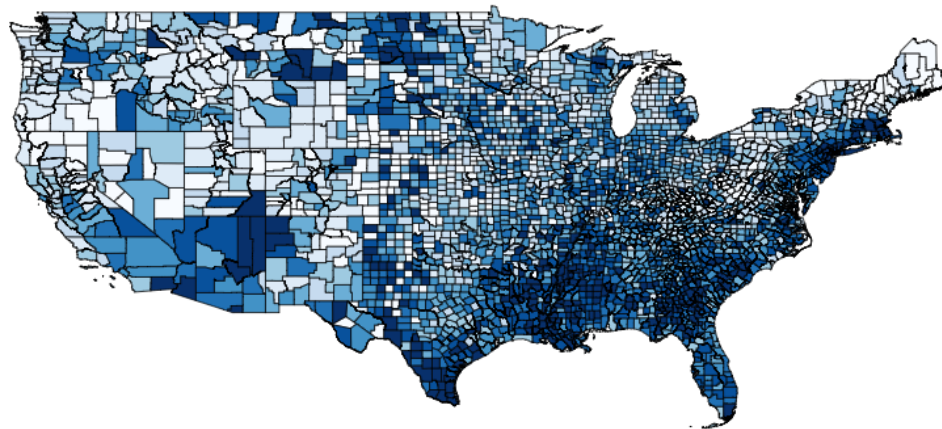
**E. Agricultural subsidies (\$000s per worker)**

**Figure 1: Presidential voting outcomes and trade war variables**

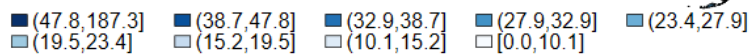
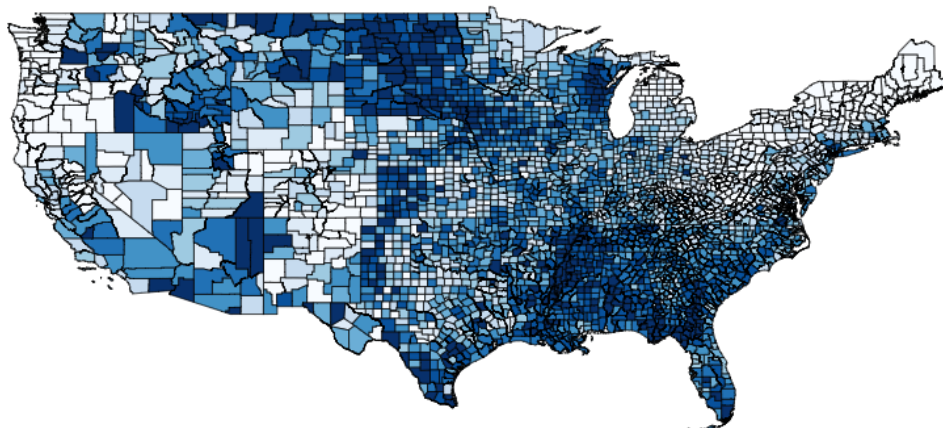
Notes: Maps represent the 3108 mainland US counties. Presidential voting data from David Leip's Election Atlas; 2020 election data is version 0.9 (official release of data for all states). Table 1 describes data sources for trade war tariffs. Agricultural subsidies data from Blanchard et. al. (2019). See main text for further details.



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



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

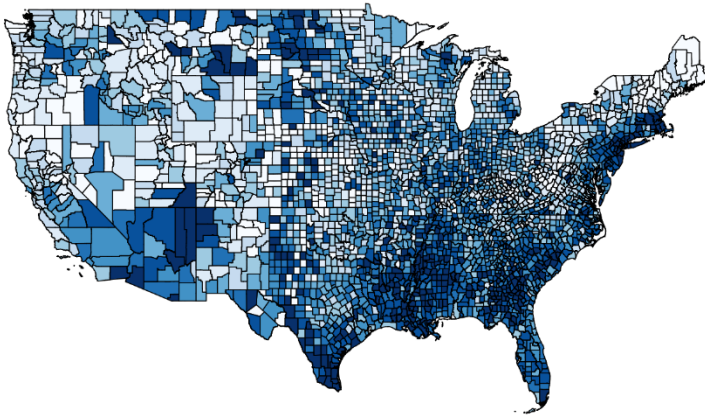


**C. COVID-19 cumulative cases (per 1,000 population)**

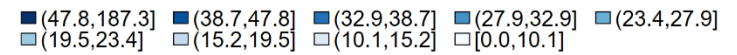
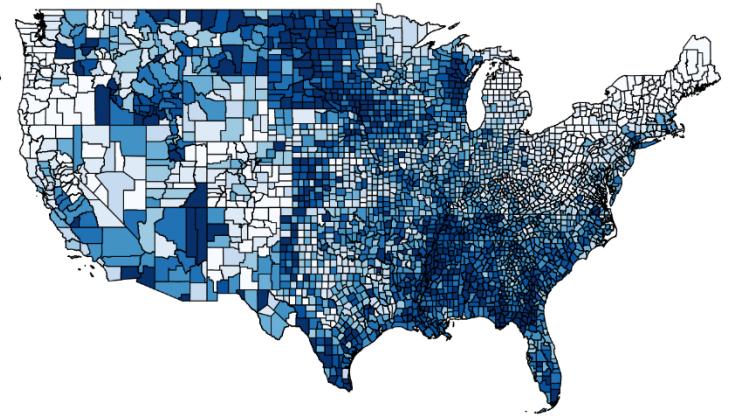
**Figure 2: Health insurance coverage prevalence and COVID-19 prevalence**

Notes: Maps represent the 3108 mainland US counties. Health insurance coverage expansion is difference between coverage shares in 2018 and 2013 Census 5-year ACS. 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.

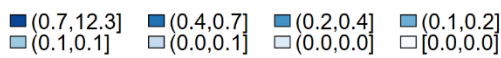
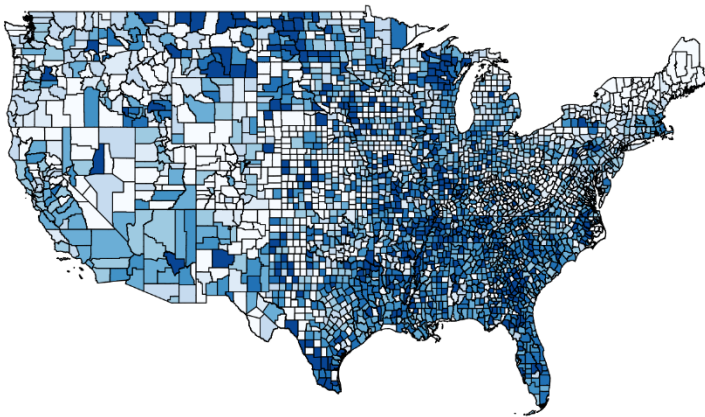




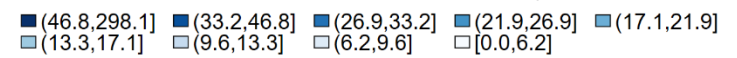
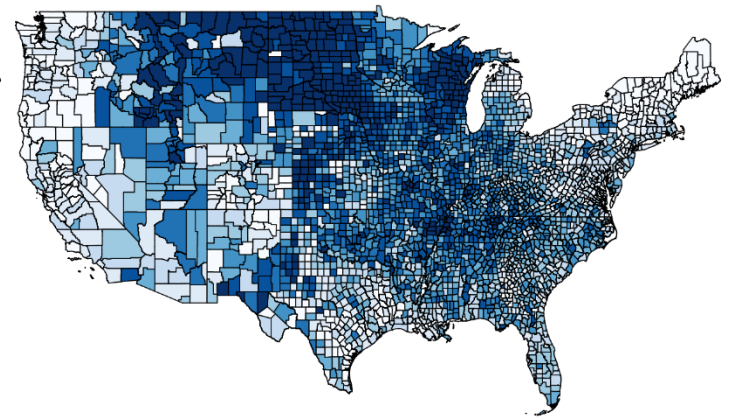
**A. COVID-19 cumulative deaths (per 10,000 population)**



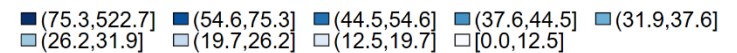
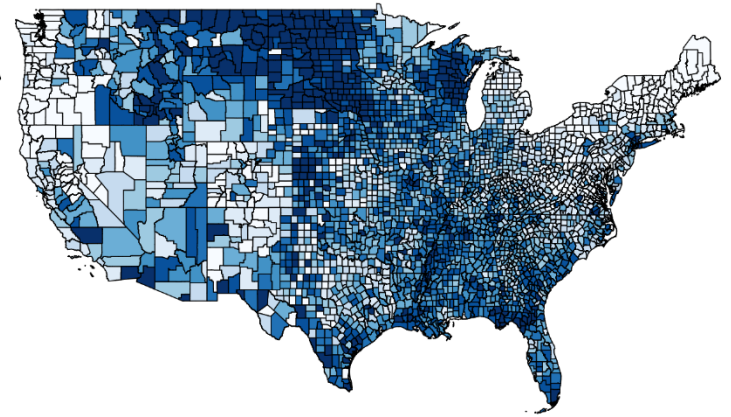
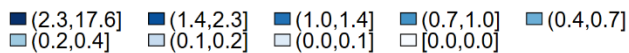
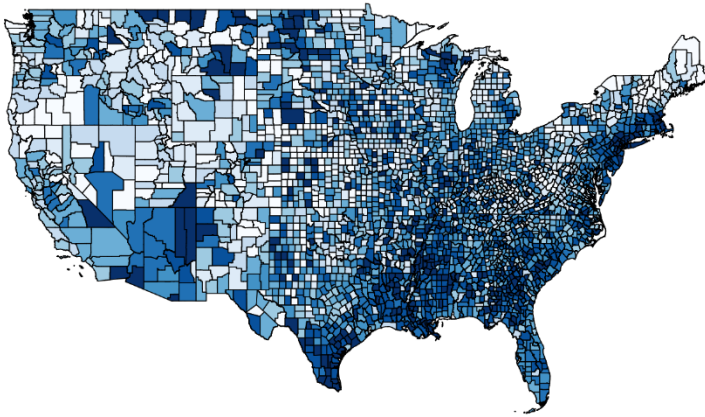
**B. COVID-19 cumulative cases (per 1000 population)**



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

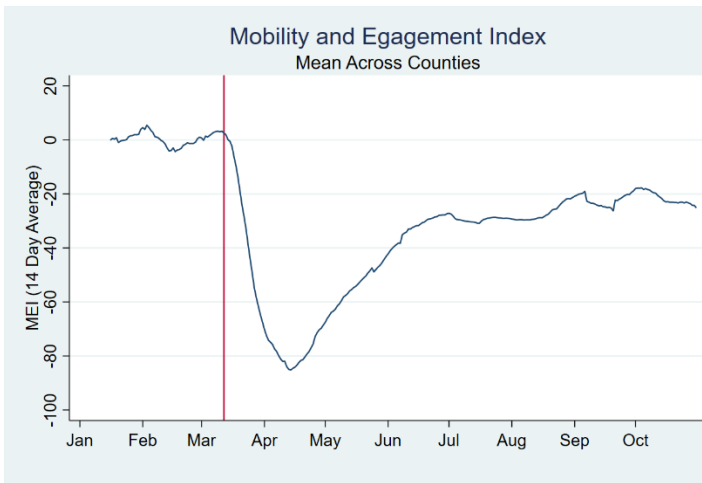


**F. COVID-19 cases (max 14-day average, per 100,000 pop.)**

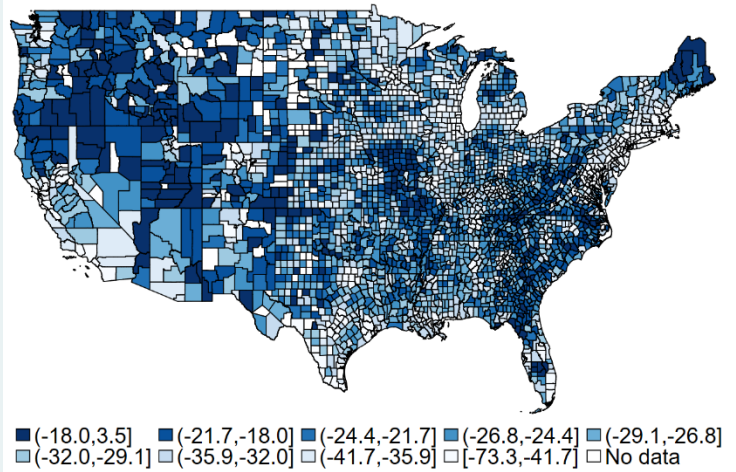


**Figure A1: Alternative measures of COVID-19 prevalence**

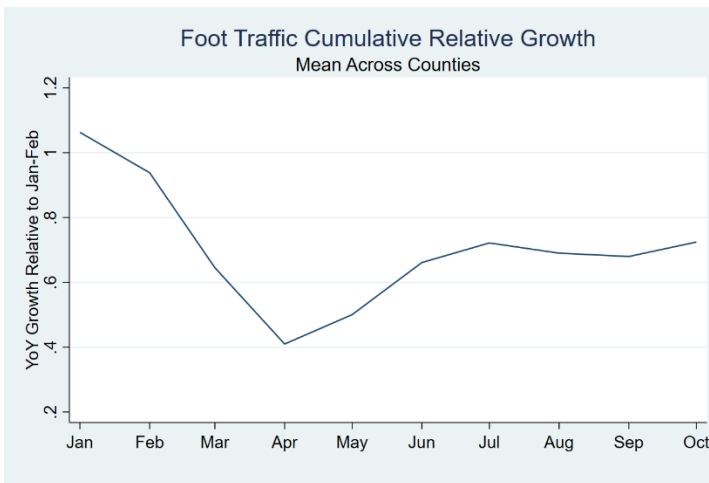
Notes: Maps represent the 3108 mainland US counties. COVID-19 data source is COVID County Data (<https://covidcountydata.org/>). Population is 2018 population from 2018 5-year Census ACS. Panels A-B cumulative data is through October 31, 2020. Panels E-F are county-level maximum 14-day rolling averages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



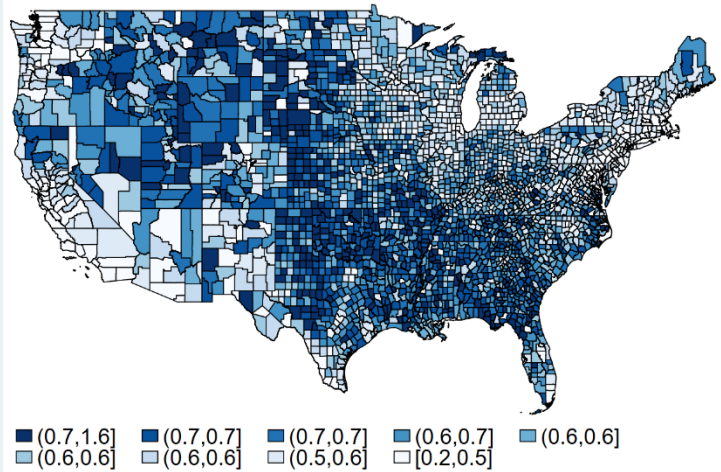
**A. Daily MEI: 1/1/2020-10/31/2020**



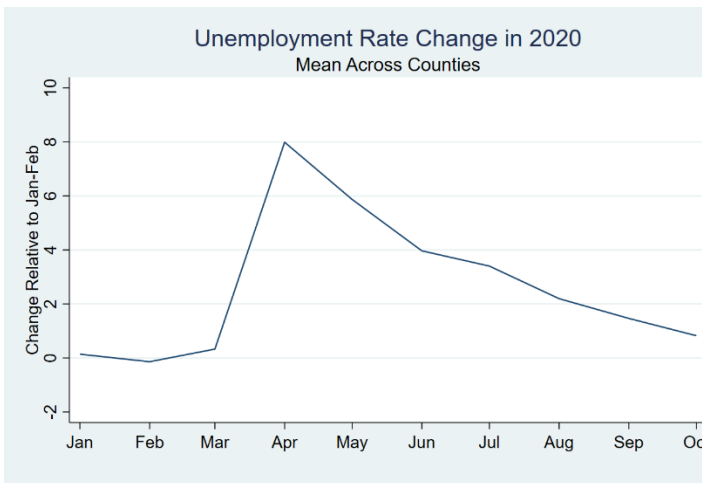
**B. MEI daily average (1/1/2020-10/31/2020)**



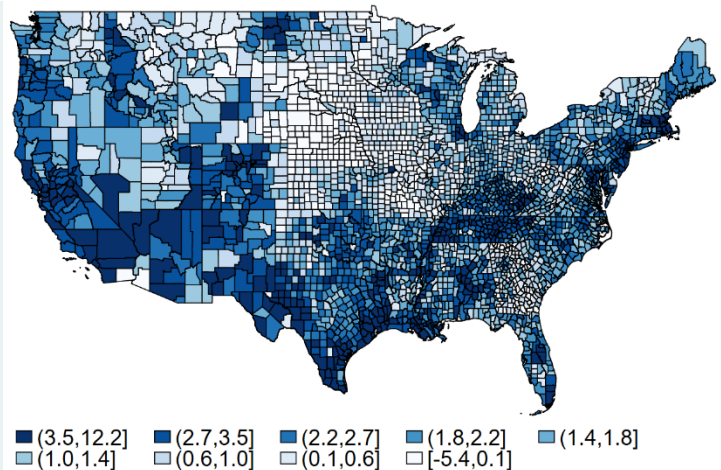
**C. Foot traffic relative growth**



**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	Tariffs
<b>A. US trade war tariffs</b>					
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>B. Foreign retaliatory trade war tariffs</b>					
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%	<a href="https://rb.gy/00bztI">https://rb.gy/00bztI</a>	<a href="https://rb.gy/00bztI">https://rb.gy/00bztI</a>
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%	<a href="https://rb.gy/7t6rkq">https://rb.gy/7t6rkq</a>	<a href="https://rb.gy/7t6rkq">https://rb.gy/7t6rkq</a>
China List 3 -- Section 301	September 2018		5-35%	Bown et al (2018d)	Bown et al (2018d)
China List 4A -- Section 301	September 2019		5-35%	Bown (2019b)	Bown (2019b)

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

**Table 2. Baseline results**

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ 2-party Rep. vote Share 2012-2016	0.038 (0.062)	0.036 (0.061)	0.032 (0.062)	0.08 (0.076)	0.162* (0.058)	0.213* (0.033)	0.213* (0.032)	0.218* (0.032)
US tariff shock	-0.048 (0.291)	-0.064 (0.289)	-0.064 (0.288)	0.175^ (0.083)	0.175* (0.064)	0.183* (0.050)	0.178* (0.051)	0.178* (0.050)
Retaliatory tariff shock		0.144 (0.160)	0.097 (0.160)	-0.226 (0.136)	-0.279 (0.181)	-0.246^ (0.120)	-0.218# (0.112)	-0.200# (0.100)
Agricultural subsidies			0.440^ (0.198)	0.427 (0.276)	0.640* (0.213)	0.404* (0.132)	0.420* (0.126)	0.501* (0.126)
Δ Health insurance coverage							-0.093# (0.054)	-0.091# (0.050)
COVID-19 deaths (cum., per 10k pop.)								0.001 (0.018)
N	3112	3112	3112	3112	3111	3111	3051	2991
R <sup>2</sup>	0.005	0.005	0.007	0.504	0.659	0.852	0.854	0.859
Trade war controls	N	N	N	Y	Y	Y	Y	Y
State FE	N	N	N	N	Y	Y	Y	Y
Race & immigration controls	N	N	N	N	N	Y	Y	Y
Health insurance controls	N	N	N	N	N	N	Y	Y
COVID controls	N	N	N	N	N	N	N	Y

Notes: # p<0.10, ^ p<.05, \* p<.01. Dependent variable is the change in the 2-party Republican vote share between the 2016 and 2020 US Presidential election. Estimation performed by fixed effects OLS. All specifications weighted by 2020 total Presidential votes cast. Standard errors clustered by state. See Appendix Table A1 for list of trade war, race and immigration, health insurance, and COVID controls. See main text for further details.

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

	(1)	(2)	(3)	(4)	(5)
<b>A. Baseline</b>					
		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	-2.41	-2.45	1.86
Pennsylvania	-1.20	-1.41	-1.13	-1.20	0.36
Wisconsin	-0.64	-1.00	-0.51	-0.63	0.79
Arizona	-0.31	-0.44	-0.26	-0.31	2.27
Georgia	-0.24	-0.43	-0.15	-0.24	2.02
North Carolina	1.37	1.13	1.44	1.37	3.59

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

	(1)	(2)	(3)	(4)	(5)
<b>E. Political heterogeneity: race</b>					
		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 margins indicate Trump loss. Each panel computes county-level predicted vote tallies for Trump and Biden using procedure described in main text and aggregates to state-level. Point estimates used are from: column (8) of Table 2 for Panel A, column (7) of Table 4 for Panel B, columns (2)-(4) from Panel A of Table 6 for Panel C, columns (5)-(6) from Panel A of Table 6 for Panel D, columns (7)-(8) from Panel A of Table 6 for Panel E, columns (2)-(3) from Panel B of Table 6 for Panels F-G, and columns (4)-(6) from Panel B of Table 6 for Panel H. See main text for more details.

**Table 4. Instrumental variables estimation**

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
US tariff shock	0.178* (0.050)	0.120# (0.071)	0.185* (0.048)	0.191* (0.043)	0.120# (0.070)	0.164* (0.047)	0.121# (0.068)
Retaliatory tariff shock	-0.200# (0.100)	-0.191# (0.101)	-0.220^ (0.093)	-0.179# (0.096)	-0.207^ (0.090)	-0.116 (0.075)	-0.175^ (0.069)
Agricultural subsidies	0.501* (0.126)	0.496* (0.124)	0.512* (0.123)	0.035 (0.166)	0.052 (0.154)	0.561* (0.121)	-0.109 (0.133)
Δ Health insurance coverage	-0.091# (0.050)	-0.094# (0.049)	-0.079^ (0.038)	-0.079 (0.048)	-0.069# (0.039)	-0.207^ (0.085)	-0.220* (0.066)
COVID-19 deaths (cum., per 10k pop.)	0.001 (0.018)	0.002 (0.018)	0.001 (0.018)	0.002 (0.018)	0.003 (0.018)	-0.009 (0.017)	-0.006 (0.017)
N	2991	2991	2991	2991	2991	2991	2991
Endogenous variables	None	US tariffs	Foreign tariffs	Agric. subsidies	Trade war variables	Health insurance	Trade war and health insurance
Instruments		Lewbel	Lewbel	Lewbel	Lewbel	Lewbel	Lewbel
Underidentification p-value		0.001	0.069	0.051	0.034	0.001	0.016
K-P weak instrument rk F-statistic		52.608	62.799	22.513	11.34	48.648	19.883
Overidentification p-value		0.797	0.707	0.808	0.832	0.428	0.740
Sargan-Hansen endogeneity p-value		0.404	0.665	0.015	0.04	0.077	0.005

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



**Table 5. Robustness specifications**

	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A. Alternative COVID-19 prevalence definitions</b>						
US tariff shock	0.178* (0.050)	0.172* (0.051)	0.175* (0.050)	0.172* (0.051)	0.178* (0.050)	0.170* (0.051)
Retaliatory tariff shock	-0.200# (0.100)	-0.196^ (0.097)	-0.187# (0.099)	-0.191# (0.100)	-0.178# (0.101)	-0.172# (0.100)
Agricultural subsidies	0.501* (0.126)	0.515* (0.127)	0.496* (0.129)	0.504* (0.126)	0.470* (0.129)	0.482* (0.134)
Δ Health insurance coverage	-0.091# (0.050)	-0.088# (0.049)	-0.091# (0.050)	-0.090# (0.050)	-0.086# (0.050)	-0.081# (0.047)
COVID-19	0.001 (0.018)	-0.007 (0.005)	0.293# (0.160)	0.000 (0.004)	0.031 (0.059)	-0.001 (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
<b>Panel B. Placebo specification</b>						
US tariff shock	-0.069 (0.079)	0.049 (0.100)				
Retaliatory tariff shock	-0.045 (0.072)	-0.144 (0.095)				
Agricultural subsidies	0.973* (0.295)	1.139* (0.387)				
Δ Health insurance coverage	0.025 (0.064)	0.069 (0.133)				
COVID-19 deaths (cum., per 10k pop.)	-0.01 (0.026)	0.005 (0.023)				
N	2991	2991				
Endogenous variables	None	Trade war Health insurance				
Instruments		Lewbel				
Underidentification p-value		0.012				
K-P weak instrument rk F-statistic		21.686				
Overidentification p-value		0.022				
Sargan-Hansen endogeneity p-value		0.553				

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 (8) of Table 2, regressions weighted by 2020 total Presidential votes cast, standard errors clustered by state. October deaths and cases in columns (3)-(4) of Panel A are daily October averages per 100,000 population. Peak deaths and cases in columns (5)-(6) of Panel A are county-level maximum 14-day rolling averages through October 31, 2020 per 100,000 population. Lewbel instruments in column (2) of Panel B are those from column (7) of Table 3. See main text for further details.



**Table 6. Heterogenous effects**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Panel A. Political heterogeneity</b>								
US tariff shock	0.178* (0.050)	0.077# (0.040)	0.555* (0.195)	0.044 (0.055)	0.085^ (0.037)	0.347 (0.207)	0.115^ (0.045)	0.346 (0.287)
Retaliatory tariff shock	-0.200# (0.100)	-0.021 (0.048)	-0.257 (0.309)	0.035 (0.184)	-0.054 (0.062)	-0.011 (0.209)	0.004 (0.067)	-0.576 (0.370)
Agricultural subsidies	0.501* (0.126)	0.156# (0.079)	1.138^ (0.466)	0.279 (0.287)	0.161 (0.104)	0.868 (0.526)	0.176 (0.134)	0.437 (0.388)
Δ Health insurance cov.	-0.091# (0.050)	-0.032 (0.031)	-0.142^ (0.065)	-0.033 (0.051)	-0.031 (0.031)	-0.167^ (0.068)	-0.025 (0.032)	-0.135 (0.099)
COVID-19	0.001 (0.018)	0.007 (0.009)	0.052# (0.028)	-0.034 (0.023)	0.004 (0.010)	0.014 (0.031)	-0.007 (0.017)	0.068^ (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
<b>Panel B. Heterogeneity by dimensions of trade war and COVID prevalence</b>								
US tariff shock	0.178* (0.050)	0.124^ (0.052)	0.339* (0.082)	0.106# (0.053)	0.088 (0.072)	0.193* (0.058)		
Retaliatory tariff shock	-0.200# (0.100)	-0.222^ (0.101)	-0.251^ (0.113)	-0.115 (0.104)	-0.276^ (0.103)	-0.083 (0.100)		
Agricultural subsidies	0.501* (0.126)	0.503* (0.127)	0.504* (0.125)	0.093 (0.121)	0.407^ (0.188)	0.388# (0.215)		
Δ Health insurance cov.	-0.091# (0.050)	-0.091# (0.050)	-0.093# (0.049)	-0.012 (0.035)	-0.013 (0.051)	-0.043 (0.040)		
COVID-19	0.001 (0.018)	0.001 (0.018)	0.002 (0.018)	-0.128 (0.086)	-0.018 (0.072)	-0.002 (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 (8) of Table 2, regressions weighted by 2020 total Presidential votes cast, standard errors clustered by state. In columns (2)-(4) of Panel A: competitive counties have 2012 and 2016 Republican 2-party Presidential vote share between 45% and 55%, and solid Republican (Democrat) counties have these vote shares above 55% (below 45%). In columns (5)-(6) of Panel A, Trump (Clinton) counties are counties that Trump (Clinton) won in 2016. In columns (7)-(8), majority white (non-white) have majority white non-Hispanic (non-white and hispanic) population in 2016. In column (2) of Panel B, US tariffs and foreign retaliatory tariff shocks computed based only on 2018 trade war tariffs. In column (3) of Panel B, US (foreign retaliatory) tariff shocks computed based only on US (China) tariffs on China (US). In columns (4)-(6), COVID-19 terciles based on cumulative COVID-19 deaths per 10,000 population.

**Table A1. Summary statistics**

	Mean	SD	Min	Max	N
<b>Voting variables</b>					
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
<b>Trade war variables</b>					
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
<b>Trade war controls</b>					
<i>Population Shares (2016)</i>					
Age under 20	25.18	3.59	4.90	43.40	3,112
Age 20-24	6.40	2.48	0.40	32.50	3,112
Age 25-44	23.29	3.30	8.70	43.40	3,112
Age 45-64	27.50	3.03	9.00	47.40	3,112
Age 65-74	9.99	2.51	3.00	33.60	3,112
Age 75+	7.65	2.33	0.00	19.90	3,112
H/hold annual income below \$25k	26.78	8.19	5.50	60.06	3,112
H/hold annual income \$25k-\$50k	26.20	4.00	8.11	41.68	3,112
H/hold annual income \$50k-\$75k	18.54	2.79	6.60	30.20	3,112
H/hold annual income \$75k-\$100k	11.67	2.71	1.30	32.43	3,112
H/hold annual income \$100k-\$150k	10.72	3.96	1.30	27.80	3,112
H/hold annual income \$150k-\$200k	3.26	2.16	0.00	16.30	3,112
H/hold annual income \$200k plus	2.84	2.56	0.00	25.33	3,112
Female	49.98	2.33	21.50	58.50	3,112
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
<i>Employment shares (2016)</i>					
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
<i>Population shares (age 16+; 2016)</i>					
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
<i>Other (2016)</i>					
Median household income (real)	47,811	12,486	18,972	125,672	3,112
<i>Change between 2012 and 2016</i>					
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

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

	Mean	SD	Min	Max	N
Age 25-44	-0.43	1.46	-30.10	19.70	3,112
Age 45-64	-0.47	1.40	-23.40	16.20	3,112
Age 65-74	1.22	0.93	-8.70	19.10	3,112
Age 75+	0.31	0.76	-6.90	8.20	3,112
H/hold annual income below \$25k	-1.38	3.11	-23.01	20.02	3,112
H/hold annual income \$25k-\$50k	-0.91	2.84	-18.34	13.18	3,112
H/hold annual income \$50k-\$75k	-0.24	2.47	-17.79	16.00	3,112
H/hold annual income \$75k-\$100k	0.25	2.07	-15.41	23.83	3,112
H/hold annual income \$100k-\$150k	1.13	1.90	-8.02	15.28	3,112
H/hold annual income \$150k-\$200k	0.56	0.96	-7.79	6.21	3,112
H/hold annual income \$200k plus	0.59	1.00	-5.81	8.19	3,112
Female	-0.06	1.17	-12.30	23.90	3,112
Less than high school	-1.91	1.85	-15.78	11.30	3,112
High school graduates	0.10	1.81	-9.00	15.39	3,112
Some college	0.75	1.27	-5.17	8.13	3,112
College graduates	1.06	1.99	-15.43	14.56	3,112
Employed in manufacturing	0.00	1.18	-7.00	5.89	3,112
Employed in agriculture or mining	-0.05	1.28	-16.08	11.09	3,112
Unemployed	-1.05	1.35	-10.40	9.00	3,112
Not in labor force	1.64	2.75	-18.90	27.80	3,112
Median household income (real)	2,321	3,448	-18,810	31,146	3,112
<b>Health insurance coverage expansion controls</b>					
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
% 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 pneumonia	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
<b>Race and immigration controls</b>					
<i>Population Shares (2016)</i>					
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
Poverty	16.44	6.54	1.80	53.90	3,112

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

	Mean	SD	Min	Max	N
<i>Other</i>					
Population (2016)	102,128	326,630	76	10,100,000	3,112
Metro size: large (2013)	0.14	0.35	0.00	1.00	3,112
Metro size: medium or small (2013)	0.23	0.42	0.00	1.00	3,112
Share of multi-unit housing structures (2016)	12.54	9.29	0.00	98.26	3,112
Public transport commuters (2016, share of emp)	0.95	3.10	0.00	61.80	3,112
Effective population density	403.84	719.47	3.46	22,647	3,112
Social capital (2014)	0.00	1.26	(3.18)	21.81	3,112
<i>Change between 2012 and 2016</i>					
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
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
Poverty	0.11	2.78	-20.10	15.00	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
Public transport commuters	-0.01	0.68	-16.50	13.70	3,112
<b>COVID-19 controls</b>					
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
Remote workers (2016, share of emp)	0.31	0.05	0.22	0.65	3,112

Notes: See main text for further details.