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

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Abstract

We analyze the impact of three issues on the 2020 US Presidential election, each of which has been influentially shaped by the Trump administration – the COVID-19 pandemic, fragility of the massive health insurance coverage expansion after the Affordable Care Act, and the trade war. Evidence on the causal impact of the COVID-19 pandemic is mixed. If anything, higher COVID-19 deaths improved Trump's vote share, perhaps due to voter beliefs about his ability to repair the post-COVID economy. In contrast, we present strong causal evidence that the expansion of health insurance coverage hurt Trump's vote share, presumably due to voter fears about such expansion being rolled back. The point estimates imply that Trump would have won Georgia, Arizona, and possibly Nevada in the absence of this effect which would have put him on the precipice of re-election. While US tariffs and agricultural subsidies imposed during the trade war appear endogenous, foreign retaliation appears exogenous but has little electoral impact.

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1 Introduction

The 2020 US Presidential election is one of the most controversial in modern history. Not only has a sizable minority of the US population questioned the validity of the widely acknowledged election results amid suspicions over widespread voter fraud, but the election featured many far-reaching policies of the Trump administration that were plausibly salient in voters' minds. Among others, these issues include the trade war initiated by the Trump administration with China, the attempted repeal and consistent undermining of the Affordable Care Act that dramatically expanded health insurance coverage to millions of Americans, and the Trump administration's response to the COVID-19 pandemic. Thus, understanding the political economy determinants of voting behavior in the 2020 US Presidential election is fundamentally important to economists, social scientists generally, and society at-large.

A once-in-a-lifetime event that has utterly upended social and economic life, COVID-19 is perhaps the most obvious issue that could have influenced the 2020 US Presidential election. More specifically, the Trump administration's response has been particularly polarizing. Many people view the response as woefully inadequate and ignoring the clear public health reality of the pandemic with President Trump himself acting as if, and stating that, the pandemic would magically disappear overnight. Many other people see his "get on with business" attitude as reflecting a reality that we cannot lockdown the economy until a vaccine comes along. Coupled with a majority of Americans typically favoring Trump over the Democratic nominee Joe Biden in terms of handling the economy, COVID-19 is clearly an important issue to understand from a political economy perspective.[1]

However, prior to the pandemic, other issues were already setting up the 2020 Presidential election as a referendum on President Trump's key agenda items while in office. When thinking about issues that reflect how Trump has reshaped the Republican party, the trade war and its associated return to protectionism stands out. It is not an exaggeration to say the trade war has pushed the level of US protectionism back to levels unseen since the infamous Smoot-Hawley tariffs of the 1930s (see Bown and Zhang (2019)). This stands in stark contrast to over 90% of Republican votes in the US House of Representatives being in favor of Free Trade Agreements over the period 2003-2011 compared to 37% of Democrat votes (Lake and Millimet (2016)).

While the trade war may have redefined the Republican party, the Trump administra-

[1] While polls addressing voter opinions on whether they trusted Trump or Biden more on the economy closed dramatically leading up to the election, this was generally a clear issue advantage for Trump. Two weeks before the election, Burns and Martin (2020) state "The president has even lost his longstanding advantage on economic matters: Voters are now evenly split on whether they have more trust in him or Mr. Biden to manage the economy." in the New York Times.

tion's continual attempts to repeal and undermine the Affordable Care Act (ACA) continues previous Republican efforts during the Obama administration. The ACA may ultimately go down as Obama's lasting legacy, similar to the creation of Medicare by President Johnson in the 1960s and Social Security by President Roosevelt in the 1930s, by expanding health insurance coverage to millions of Americans. However, many Republicans view the ACA as government overreach and a Republican controlled executive and congress famously fell one vote short of repealing the ACA on the "thumbs down" vote of Republican congressman John McCain. Nevertheless, judicial avenues remain given what may go down as Trump's lasting legacy: a reformation of the US Supreme Court. What looked like becoming a clear 6-3 liberal majority in the aftermath of Justice Scalia's death late in the last year of the Obama administration is now a clear 6-3 conservative advantage with the successful confirmation of Judge Amy Coney Barrett in October 2020. The judicial and legislative foundation of the ACA is certainly not set in concrete yet.

We analyze the impacts of these three salient issues – the COVID-19 pandemic, the post-ACA expansion of health insurance, and the trade war – on the change in President Trump's county-level vote share between the 2016 and 2020 US Presidential election. Our three trade war variables reflect the multiple facets of the trade war: US trade war tariffs on China mostly but also the rest of the world sometimes too, foreign retaliatory tariffs by China and other countries, and agricultural subsidies paid to US farmers through the Market Facilitation Program aimed at mitigating the effects of foreign retaliation. Combining these industry-level variables with the industrial composition of county-level employment leads to county-level measures of trade war exposure. With ACA health exchanges fully certified and operational in January 2014, we use 5-year Census data from the American Community Survey (ACS) to compute the county-level increase in health insurance coverage between the first 5-year window after ACA implementation (the 2018 5-year ACS) and the last 5-year window before the ACA (the 2013 5 year ACS). We control for various economic and demographic factors that could correlate with these salient issues as well as voting behavior including the distribution of age, race, income and education in the 2016 5-year ACS. To control for pre-existing trends, we control for the change in these variables between the 5-year ACS in 2012 and 2016 as well as the change in the Republican Presidential vote share between the 2012 and 2016 elections and we also use state fixed effects.

While we ultimately use various measures and time windows that capture the county-level prevalence of COVID-19, our baseline analysis focuses on deaths (per 10,000 population) through October 31, 2020. Additionally, we control for a county-level index of social mobility (or, in other words, an inverse measure of social distancing) and county-level economic activity. The county-level measure of social mobility at the daily frequency is the Mobility and Engagement Index (MEI) from the Federal Reserve Bank of Dallas (see Atkinson et al. (2020)) and is based on cell phone location data from the company SafeGraph. To control for county-level economic activity, we aggregate business-level foot tariff data from SafeGraph (2020) and also use the change in county-level unemployment between August 2019 and August 2020. We additionally control for many county-level correlates of COVID-19 prevalence as summarized by Desmet and Wacziarg (2020). Beyond the county-level controls described above, these include measures related to urbanization (including population and indicators for metro versus non-metro areas), population density (including the share of workers who commute by public transport, the share of multi-unit housing structures, and an effective population density measure), and health characteristics (including diabetes prevalence and mortality measures pertaining to pneumonia and heart failure).

Naturally, the endogeneity of all three salient issues is a concern. While our main analysis focuses on fixed effects OLS specifications, we show that our main results are robust to using instrumental variables (IVs) for either the trade war variables, health insurance coverage expansion, or COVID-19 deaths (or other measures of COVID-19). To instrument for COVID-19 deaths (or cases), we use two alternative IV strategies. The first uses the county-level population share of nursing home residents as the instrument and the second borrows from Baccini et al. (2020) by using the employment share of meat packing workers as the instrument. The exclusion restrictions say that *conditional* on the composition of age, race, income and education as well as health characteristics of the county (and other controls), the change in Trump's vote share between 2016 and 2020 should only depend on the population share of nursing home residents or the employment share of meat packing workers through their impacts on COVID-19 deaths (or cases). We argue these are reasonable a priori exclusion restrictions. Moreover, these two instruments have been well documented in the media as key sources of county-level COVID-19 outbreaks and nursing homes have been documented academically as such by Desmet and Wacziarg (2020).[2]

Endogeneity concerns over the trade war and health insurance coverage expansion are more challenging. Indeed, instruments in the context of tariffs are a notorious problem in the empirical trade policy literature. For example, Amiti et al. (2019) and Blanchard et al. (2019) in the trade war literature do not use IV. The same is true for Hakobyan and McLaren (2016) in the trade and labor literature when it comes to NAFTA tariff cuts. Indeed, Blanchard et al. (2019, p.3) actually state "We stop short of claiming causal identification... the 2018 tariffs were not orthogonal to future US political considerations...". Thus, we construct IVs using

[2] In The Wall Street Journal on November 25, 2020, Kamp and Mathews (2020) document that nearly 40% of US deaths – over 100,000 deaths – had occurred in nursing homes. In the USA Today on November 20, 2020, Chadde et al. (2020) document that more than 40,000 meat packing workers had been infected with COVID-19 and more than 200 had died.

the heteroskedasticity-based approach of Lewbel (2012). A drawback of this approach is that identification is much less intuitive than a traditional IV approach such as our nursing home resident and meat packing worker instruments. However, we can construct enough instruments to use an overidentification test and we can also test the "first-stage" strength of the instruments. According to these tests, our Lewbel IV approach works well.

Our IV approaches support our OLS results for the impact of foreign retaliatory tariffs and health insurance coverage expansion. We first focus on these because the IV results do not support the OLS results for the impacts of US trade war tariffs and agricultural subsidies and the IV results are mixed for the impact of COVID-19 cases and deaths. Based on our within-state and across-county identification strategy, our results say the issue that had the most impact on the 2020 US Presidential election is health insurance coverage expansion. The baseline OLS specification says the point estimate is positive: stronger expansion (conditional on pre-ACA coverage) reduces the change in President Trump's vote share between 2016 and 2020. Interpreting this as proxying for the magnitude of county-level voter anxiety over the fragile judicial and legislative existence of the ACA, our results say President Trump would have won both Georgia and Arizona in the absence of undermining the ACA. When allowing heterogeneity of the point estimates for counties that Trump won in 2016 versus those that Hillary Clinton won, Trump would have additionally won Nevada in 2020 and only lost Wisconsin by 0.06% points or around 2000 votes. This would have put him on the precipice of an electoral college victory as he would have only needed one more state (e.g.Wisconsin) for re-election.

The economic significance of the impacts of foreign tariff retaliation are much smaller and do not display the crucial degree of heterogeneity just described. Consistent with existing results in the literature, Trump suffers from foreign retaliatory tariffs. This is also consistent with economic theory in that such tariffs should hurt US firms and workers in affected industries and, in turn, hurt counties more when more workers are concentrated in such industries. Absent the retaliatory trade war tariffs, the mean county change in Trump's vote share would have only been 0.11% points and, given the geographic distribution of county-level exposure to foreign retaliatory tariffs, it would not have changed the electoral college outcome of any state.

We find mixed evidence of the impact of COVID-19 on the 2020 US Presidential election. After controlling for typical socioeconomic and demographic characteristics as well as our COVID-19 controls described above and motivated by Desmet and Wacziarg (2020), our OLS results provide no evidence for any relationship between COVID-19 cases or deaths and the change in President Trump's vote share between the 2016 and 2020 elections. However, the alternative IV approaches tell a potentially different story.

Across six different measures of COVID-19 prevalence (cases and deaths across three different time period – cumulative since the pandemic began, October, and the "peak" period), the meat packing worker employment share is a very weak instrument except when looking at cumulative cases (per 1,000 population) since the pandemic began. But, this point estimate is still not close to conventional levels of statistical significance. In contrast, the nursing home resident instrument performs well according to standard IV specification tests for four of these six COVID-19 measures (including cumulative cases per 1,000 population and deaths per 10,000 population). Moreover, these point estimates are statistically significant and positive. That is, these IV results say increased COVID-19 prevalence actually increased Trump's 2020 vote share relative to his 2016 vote share. At mean levels of COVID-19 prevalence, the point estimates translate into a bump for Trump's vote share of between 0.8% and 4% points. While we suggest caution with these IV results given the vastly different results across our two IV approaches, the sign of these results are consistent with the positive raw correlation between COVID-19 prevalence the change in Trump's vote share between 2016 and 2020 and it is also consistent with Trump's perceived advantage in the minds of voters at dealing with the economy which has been ravaged by the pandemic.

The two most closely related papers to ours are Baccini et al. (2020) and Blanchard et al. (2019). The only other paper we know of that looks at the impact of COVID-19 on the 2020 US Presidential election is Baccini et al. (2020). Using a county's employment share of meat packing workers as an instrument for cumulative COVID-19 cases through October 22, 2020, they find a sizable negative effect of COVID-19 on the change Trump's vote share between 2016 and 2020. Indeed, they estimate that Trump would have won re-election if COVID-19 cases had been 5% lower. In our analysis, we find no statistically significant impact of COVID-19 cases on Trump's vote share when using a county's employment share of meat packing workers as an instrument. Moreover, we actually find a consistent positive effect when using a county's population share of nursing home residents as an instrument. While we argue that the exclusion restriction for both instruments appear reasonable, the starkly different results using the alternative instruments highlights the difficulty of using IV estimation to deal with endogeneity issues surrounding COVID-19. An important difference between our analysis and Baccini et al. (2020) is that we have nearly 3000 counties in our sample while they have a little under 2600 counties. At least in part, this stems from substantial portions of the county-level voting data from David Leip's Election Atlas being released in late-November.

While ours is the first paper we know of that looks at the impact of the trade war or health insurance expansion on the 2020 US Presidential election, Blanchard et al. (2019) analyze the impact of these two issues on the 2018 US midterm elections.[3,4] Similar to our results, they find that Republicans lost support on the issues of foreign retaliatory tariffs and health insurance coverage expansion. Their counter-factual estimates say that the trade war issue can account for five lost Republican seats in the US House of Representatives and the issue surrounding expansion of health insurance coverage can account for eight lost Republican seats. A self-acknowledged limitation of Blanchard et al. (2019) is that the trade war is likely endogenous. Thus, a main contribution of our analysis to the political economy of trade policy literature is to show how one can use the heteroskedasticity-based IV approach of Lewbel (2012) to address endogeneity concerns. Indeed, our analysis shows these concerns are well founded. Nevertheless, a second main contribution of ours to the political economy of trade policy literature is that the trade war had very little impact on electoral college outcomes in the 2020 US Presidential election. Relative to the results of Blanchard et al. (2019), this could at least in part stem from Presidential elections effectively being state level elections compared to midterm elections being congressional district level elections.

A literature in economics has already developed on the drivers of COVID-19 that we rely on heavily to control for the important correlates of COVID-19. Although coming from different perspectives, Desmet and Wacziarg (2020) and Allcott et al. (2020b) arrive at a similar key conclusion: the exogenous factors of population and population density are key drivers of COVID-19.[5] Interestingly, Desmet and Wacziarg (2020) look at whether COVID-19 prevalence is higher or lower in counties where Trump won in 2016. Controlling for the share of minority groups, they find Trump's vote share was higher in 2016 in counties with higher COVID-19 prevalence. We push beyond this question by analyzing how the prevalence of COVID-19 affected the change in Trump's vote share between the 2016 and 2020 elections.

A broader economics literature has also developed quickly on COVID-19 issues. Some papers address how to quantify social distancing and related policy interventions (see, e.g., Allcott et al. (2020a), Atkinson et al. (2020), Goolsbee and Syverson (2020) and Villas-Boas et al. (2020)). Others have looked at the how social distancing and policy interventions have impacted economic outcomes including consumer behavior (e.g. Alexander and Karger (2020), Baker et al. (2020)), small business performance (e.g. Bartik et al. (2020)), un-

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

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

[5] Desmet and Wacziarg (2020) conduct their analysis from the perspective of understanding whether the long-run spread of COVID-19 will equalize across US counties or whether county-characteristics will lead to different spread even in the long-run. Allcott et al. (2020b) conduct their analysis using an epidemiological model of disease transmission.

employment (e.g. Beland et al. (2020b), Kong and Prinz (2020)), international trade (e.g. Antràs et al. (2020)), other measures of economic activity (e.g. Chetty et al. (2020)), and optimal lockdown policies (e.g. Fajgelbaum et al. (2020a)). The impacts on public health have also been addressed (e.g. Beland et al. (2020a), Friedson et al. (2020) and Greenstone and Nigam (2020)). Our analysis contributes to this broad literature by focusing on election outcomes.

Our paper proceeds as follows. Section 2 describes the data. Section 3 describes our empirical methodology. Section 4 presents the results. Section 5 concludes.

2 Data

2.1 Voting data

Our voting data comes from David Leip's Election Atlas. We collect county-level voting data for the 2012, 2016 and 2020 US Presidential elections. This data has county-level votes for each candidate, including Republican and Democratic candidates as well as third-party candidates.[6] Consistent with prior literature, we compute the two-party vote share for the Republican and Democrat candidates (i.e. the denominator is total votes cast for Democratic and Republican candidates). Table 1 shows the mean change in Trump's vote share between the 2016 and 2020 Presidential election was -0.52% points, in stark contrast to the 5.85% point change for the Republican candidate between the 2012 and 2016 elections. Figure 1 shows the geographic distribution of the change in Trump's vote share differs notably across the two elections. While there is a positive correlation, the 2016 surge for Trump across the upper Midwest and Great Lakes region slows sharply in 2020.

2.2 Trade war and health insurance coverage expansion and controls

US tariffs and foreign retaliatory tariffs. Beginning in spring 2018, President Trump began using executive authority to impose tariffs on various US trading partners and many of those partners retaliated. Table A1 provides the background and source data for these trade war tariffs. By fall 2019, the US was imposing tariffs of 10-25% on around $425bn of US imports (about $375bn of these from China) which represent nearly 20% of US imports and is equivalent to over 2% of US real GDP. With China bearing the brunt of these US

[6] At the time of writing, version 0.6 is the latest data for county-level 2020 Presidential election results. This data is missing 14 counties in Illinois. Also, Alaska as well as Kalawao county in Hawaii do not report county-level vote tallies in any year.

trade war tariffs, they were the major retaliator and were imposing tariffs on 5-35% on nearly \$100bn US exports.

We closely follow Blanchard et al. (2019) in constructing county-level exposure to US trade war tariffs and foreign retaliatory trade war tariffs. We start with 2017 pre-trade war bilateral trade data between the US and the rest of the world. With 8-digit HS US import data and 6-digit HS US export data from the USITC, we multiply these bilateral trade flows by the relevant bilateral trade war tariff (i.e. US tariffs on US imports and foreign country tariffs on US exports).[7] TS_h^m is the resulting additional tariffs charged on US imports form county m of HS8 product h and TS_h^x is the resulting additional tariffs charged on US exports to county x of HS8 product h. After aggregating these partner-product specific tariff shocks across US trade partners, we concord to NAICS 3-digit industries using the Feenstra et al. (2002) trade weights over the period 2002-2006. For each 3-digit NAICS industry i, this gives the total additional tariffs charged on US exports and US imports and are denoted by TS_i^X and TS_i^M respectively.

To aggregate these industry-level tariff shocks to county-level tariff shocks, we first divide by US employment in a given 3-digit NAICS industry to convert into a per worker measure using 2016 employment data from the County Business Patterns (CBP). Second, we aggregate across 3-digit NAICS industries using the CBP county-level composition of employment.[8] Denoting employment by L, the US tariff shock and the foreign retaliatory tariff shock faced by county c due to the trade war are

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

Table 1 shows the mean county-level exposure is \$1030 per worker to US trade war tariffs and \$550 per worker for foreign retaliatory tariffs.

Figure 2 shows the geographic distribution of these tariff shocks. Exposure to US trade war tariffs is concentrated around the Great Lakes region (including Michigan, Wisconsin, Pennsylvania, Ohio and Indiana) as well as the parts of the south (including North Carolina, Tennessee, Alabama and Mississippi). In contrast, exposure to foreign retaliatory trade war tariffs is concentrated along the Mississippi River in Arkansas as well as the lower Midwest

[7]Following Blanchard et al. (2019), we focus on retaliatory tariffs by the four major US trade partners: China, Canada, Mexico and the EU.

[8]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.

(stretching from North Texas through Colorado, Kansas and Nebraska) and the Pacific North West (Idaho and Washington).

Agricultural subsidies. The Trump Administration implemented a Market Facilitation Program of agricultural subsidies in 2018 designed to help US farmers hurt by foreign retaliatory tariffs in the trade war. In 2018, about $12 billion was paid to farmers based on crop-specific subsidy rates (Blanchard et al. (2019)).[9] We use the 2018 county-level estimated payments computed by Blanchard et al. (2019). As documented by Blanchard et al. (2019), Figure 2 shows these are concentrated in the central and upper Midwest and along the Mississippi River. Thus, the mean agricultural subsidy of $430 per worker in Table 1 lies above the 75th percentile of the distribution and the top 5% of counties receive between $2400 and $15,900 per worker.

Health insurance coverage. Our data on health insurance coverage comes from the 5-year ACS. The ACA health exchanges were fully certified and operational in January 2014. We measure the change in health insurance coverage as the change in the share of people (civilian non-institutionalized aged between 19 and 64 years) between the 2013 5-year ACS and the 2018 5-year ACS. The 5-year 2013 ACS represents the last 5-year ACS that lies completely before the ACA exchanges are operational while the 5-year 2018 ACS is the first 5-year ACS that lies completely in the period where the ACA is operational. The 3-year and 1-year ACS does not contain counties where the population falls below 20,000 and 65,000 respectively, so using the 5-year ACS maximizes county coverage.[10]

While Table 1 shows the mean expansion is 5.05% points, Figure 2 shows some states see much larger expansions. Moving east to west, this includes West Virginia, Kentucky, Louisiana, Arkansas, Colorado, New Mexico, Nevada and the west coast. Moreover, numerous large counties around major cities in states that decided the 2020 Presidential election (including Georgia, Arizona and Nevada) saw above-average expansion.

Controls. We use a typical set of economic and demographic variables to control for factors that could plausibly affect voting preferences as well as the trade war variables and expansion of health insurance coverage. First, we control for the pre-ACA level of health insurance coverage using the 2013 5-year ACS as described above. Second, we collect numerous data from the 5-year ACS in 2016 and 2012 so we can include controls in their 2016 level and the change between 2012 and 2016. This allows us to compute the distribution of age across six age bins, the distribution of household income across seven income bins, median real household income, the distribution of education across four education bins, and

[9] In 2019, the program changed to pay subsidies based on acreage. See `https://www.fsa.usda.gov/programs-and-services/market-facilitation-program/index`.

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

the distribution of race across five racial groups. Third, reflecting local labor markets, we also collect the shares of the population aged 16 and above that are unemployed and not in the labor force. Fourth, reflecting local industrial composition, we also collect the share of employment in manufacturing and the share of employment in agriculture and mining. Finally, we collect the female share of the population. Table A2 presents the summary statistics for these variables.

2.3 COVID-19 variables and controls

Deaths and cases. Our data on COVID-19 cases and deaths comes from COVID County Data which has now officially merged with Covid Act Now.[11] They obtain data from various sources with county-level dashboards being the most preferred data source.[12] As shown in Table 1, we compute deaths and cases in three time windows. First, cumulative until October 31, 2020, in terms of deaths per 10,000 population and cases per 1,000 population. Second, the daily average during the month of October per 100,000 population. Third, the county-specific window that saw the highest 14-day rolling daily average per 100,000 population.[13] Unfortunately, there is no widely available county-level data on hospitalizations or tests; however, our use of state fixed effects will control for state-level differences is testing regimes.

Figure 3 illustrates the geographic incidence of COVID-19. In terms of prevalence since the pandemic began, the timing of different surges is apparent. Deaths are relatively higher than cases in the early hit north-east. But, cases are relatively higher than deaths in the recently hit Dakotas and Minnesota. This is emphasized both by the graphs for cases and deaths during October and also by the graphs showing the maximum 14-day rolling daily average of cases and deaths. In contrast, places hit during the late spring and summer sit in similarly high positions of the distribution for deaths and cases since the pandemic began (e.g. south-west Texas, Louisiana, Mississippi, Florida and Georgia).

Controls. COVID-19 cases and deaths could be correlated with the extent of county-level social distancing and economic activity. Moreover, each of these could be correlated with voting behavior. Thus, we control for measures of county-level social distancing and

[11] See `https://covidcountydata.org` and `https://apidocs.covidactnow.org/`.

[12] The ordering of the sources is county dashboards, state dashboards, COVID Tracking Project, department of health and human services, USA Facts, New York Time and CovidAtlas. See `https://covidcountydata.org/data/documentation`.

[13] Data dumps and revisions are not uncommon in the daily case and death data. This creates positive outliers of daily counts and even negative daily counts. To deal with positive outliers when computing daily averages of cases (deaths), we replace the highest three days (one day) with the daily average over the preceding seven days. To deal with negative outliers when computing daily averages of cases (deaths), we replace negative daily counts with the maximum of zero and the the three-day average across the negative day as well as the day either side of the negative day.

economic activity.

For social distancing, we use the Mobility and Engagement Index (MEI) created and updated by the Federal Reserve Bank of Dallas (Atkinson et al. (2020)).[14] This index varies at the daily frequency based on cell phone activity data from SafeGraph and is normalized so that the nationwide daily average index is 0 for January and February and -100 in the second week of April. Thus, this is an inverse measure of social distancing. When using cases or deaths since the pandemic began, our MEI measure averages over 2020 daily values between January 1 and October 31. When using a different time window for cases or deaths, the time window for our MEI measure adjusts accordingly. Because the index is only computed for counties where there are at least 100 mobile devices on each day in the sample, we are forced to drop 106 counties. Figure 4 shows the daily mean across counties for the county-level 14-day average MEI. The sharp increase in social distancing in the early spring tapered off relatively quickly during late spring and early summer and falls further in the fall.

For economic activity, we use two county-level measures. First, we collect monthly business foot traffic data from SafeGraph for 2019 and 2020. Based on cell phone location data, this data records foot traffic as store-level visits. We aggregate this to the county-level. For the window since the pandemic began, we compute the number of visits between the two periods March 2020 through October 2020 and January 2020 through February 2020. To account for different patterns of seasonality across counties, we compute the growth between these two periods in 2020 relative to the analogous growth in 2019. Thus, this measure proxies for the adverse impact of COVID-19 on county-level business foot traffic. For the time window of October, we adjust this measure so that the growth in visits for 2020 or 2019 is just October relative to January-February. Our second measure that proxies for the adverse impact of COVID-19 on local economic economic activity is the the county-level change in the unemployment rate from August 2019 to August 2020 from the BLS Local Area Unemployment Statistics.[15]

These two measures of local economic activity capture different phenomena. Looking at the mean across counties, Figure 4 shows (i) how our October measure of foot traffic defined above changes as we change the month of measurement from January through October and (ii) how the unemployment rate changes during 2020 relative to the (county-level) mean of January-February 2020. Foot traffic drops quicker than unemployment in March but has recovered slower than unemployment since June. Figure 4 also shows the geographic distribution and illustrates that the two are positively correlated but somewhat weakly. The

[14]See `https://www.dallasfed.org/research/mei`.

[15]See `https://www.bls.gov/lau` and `https://www.bls.gov/web/metro/laucntycur14.txt`. At the time of writing, August 2020 is the last month with non-preliminary unemployment data.

MEI is positively correlated with these measures of economic activity but again somewhat weakly.

In addition to measures capturing the change in social mobility and economic activity over the course of the pandemic, we also control for correlates of county-level COVID-19 prevalence. While some of these controls were described above in helping control for factors that could influence voting behavior as well as our trade war variables and health insurance coverage expansion, the correlates of COVID-19 are broader.

To this end, we rely heavily on the analysis of Desmet and Wacziarg (2020). First, we again use the 2016 5-year ACS to control for measures related to ethnicity, poverty and density. These measures include the population share where English is not spoken at home, the share of people that are foreign born, the share of people that are naturalized citizens, the share of people living in poverty, population, the share of multi-unit housing structures, and the share of workers who commute by public transport. Additional measures related to density include three binary variables related to rural versus urban areas (large metro, small and medium metro, non-metro) and effective density constructed by Desmet and Wacziarg (2020) that differs from the standard population density measure by taking into account the spatial distribution of the population within a location. Second, given the importance of pre-existing conditions for COVID-19, we control for county-level health characteristics from Chetty et al. (2016): measures of diabetes prevalence as well as the 30-day mortality for pneumonia, 30-day mortality for heart failure, and the 30-day hospital mortality index.[16] Third, we control for a measure of social capital from Rupasingha et al. (2006). Moving beyond Desmet and Wacziarg (2020), we also compute the share of county employment that can work remotely by using the occupational classification of Dingel and Neiman (2020) that classifies whether an occupation can work remotely.[17] Table A2 shows the summary statistics for all of these variables.

Instruments. Our instruments for COVID-19 cases and deaths are nursing home residents as a share of population for 2016 and meat packing workers as a share of employment over the period 2012-2016. Nursing home data comes from the nursing home data archive on `https://data.medicare.gov` and population from the 5-year ACS. Following Baccini et al. (2020), we define meat packing workers as workers in the 4-digit NAICS industry 3116 "Animal Slaughtering and Processing" and use CBP employment data to compute the county-level share of meat packing workers as the annual average number of such workers

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

[17] To convert to a county-level employment share, we use the 5-year ACS microdata from IPUMS USA (`https://usa.ipums.org/usa/`) as well as a PUMA to county geography concordance from the Missouri Census Data Center (`https://mcdc.missouri.edu/applications/geocorr2014.html`) and an SOC occupation concordance (`https://usa.ipums.org/usa/volii/occsoc18.shtml`).

across 2012-2016 divided by average annual total employment over the same time period.

Table 1 shows the summary statistics and Figure 5 shows the geographic distribution of these instruments. The mean meat packing worker share (1.27%) is about twice the mean population share of nursing home residents (0.64%). But, the median population share of nursing home residents is about 33 times the median meat packing worker employment share. This is because over 45% of counties have no meat packing workers and hence, as illustrated in Figure 5, meat packing workers are concentrated in a narrow set of counties. Indeed, only around 12% of counties have a share of meat packing workers above the median (often found in Texas, Arkansas, Missouri, Nebraska, Iowa, Georgia and Alabama). In contrast, Figure 5 shows the distribution of nursing home residents is more centered. Indeed, around 7% of counties have zero nursing home residents and over 40% of counties have a population share of nursing home residents above the mean. Ultimately, the variation in the data underlying these two instruments is very different.

3 Empirical model

Our baseline analysis revolves around the following specification:

$$\triangle V_c^{2020} = \triangle V_c^{2016} + \beta_1 TS_c^{US} + \beta_2 TS_c^{R} + \beta_3 AgSub_c + \beta_4 \triangle HI_c + \beta_5 COVID_c + \beta_6 X_c + \delta_s + \varepsilon_c. \quad (1)$$

All variables subscripted with c are county-level variables with c indexing counties. $\triangle V_c^{2020}$ is the change in the two-party Republican vote share between the 2016 and 2020 US Presidential elections. $\triangle V_c^{2016}$ is this variable but for the change between the 2012 and 2016 US Presidential elections. TS_c^{US}, TS_c^{R} and $AgSub_c$ are our trade war tariff shock and agricultural subsidy variables defined in Section 2.2. $\triangle HI_c$ is the expansion of health insurance coverage either side of the ACA defined in Section 2.2. $COVID_c$ is a measure of COVID-19 deaths or cases. X_c includes all of the controls described in Sections 2.2 and 2.3. δ_s are state fixed effects. Following earlier literature (e.g. Autor et al. (2020) and Blanchard et al. (2019)), we weight the regression by total votes cast in the 2020 Presidential election and cluster the standard errors by state. After exploring this baseline specification, we will also explore heterogeneity of the parameter estimates by splitting the overall sample into various sub-samples and estimating equation (1) on each sub-sample.

After estimating the fixed effects OLS specifications related to equation (1), we pursue various IV strategies. As discussed above, we use a traditional IV approach to instrument for $COVID_c$ by using either the population share of nursing home residents or the employment share of meat packing workers. However, in the absence of obvious instruments for our trade war variables and health insurance coverage expansion, we use heteroskedasticity-based IVs using the approach of Lewbel (2012).

The Lewbel approach works as follows. Consider a "first stage" regression for an endogenous variable r that regresses r on $\tilde{X}_c$ that includes all of the exogenous controls X_c and fixed effects δ_s in equation (1). As usual, equation (1) can be thought of as the "second stage". Lewbel shows that the model is identified if (i) the first-stage errors ζ_{rc} are heteroskedastic and (ii) some of the exogenous controls are correlated with the variances of these first-stage errors but are not correlated with the covariances between these first-stage errors and the second-stage error ε. Lewbel also shows these assumptions are satisfied if the covariance structure between the first-stage and second-stage errors comes from an unobserved common factor with factor loadings that can vary across all of the first-stage and second-stage equations. One plausible example of such a common factor in our context is local political activism. Formally, choosing a vector $z_{rc} \subseteq \tilde{X}_c$ such that $E\left[z'_{rc}\zeta^2_{rc}\right] \neq 0$ and $E\left[z'_{rc}\varepsilon_c\zeta_{rc}\right] = 0$ implies that the vector $\tilde{z}_{rc} \equiv \left(z_{rc} - \bar{z}_c\right)\zeta_{rc}$ (i.e. the demeaned z_{rc} interacted with the first-stage residuals ζ_{rc}) are valid instruments for an endogenous variable r. Having computed the vectors $\tilde{z}_{rc}$ for each endogenous variable r, one can then estimate equation (1) with standard IV techniques by using the vectors $\tilde{z}_{rc}$ as the IVs for the endogenous variables.[18]

Given standard IV estimation is ultimately performed with the Lewbel instruments $\tilde{z}_{rc}$, one can carry out the usual IV specification tests including weak instrument and overidentification tests. Intuitively, the strength of the Lewbel instruments are related to the heteroskedasticity of the first-stage errors. Thus, we use the Koenker (1981) version of the Breusch-Pagan test for heteroskedasticity to identify variables $z_{rc} \subseteq \tilde{X}_c$ that are significantly related to the first-stage error variances. For at least one endogenous variable r, we also let z_{rc} be a vector with at least two variables. This gives us more instruments than endogenous variables and thus allows us to perform the overidentification test.

4 Results

4.1 Baseline results

Table 2 presents the baseline results. Column (1) merely regresses the change in the Republican Presidential vote share between 2020 and 2016, ΔV_c^{2020}, on the trade war variables. Here, agricultural subsidies show a positive and statistically significant relationship with ΔV_c^{2020} but the other trade war variables are statistically insignificant. Column (2) adds

[18] See Millimet and Roy (2016) for more details, links to earlier work, and a practical application to the pollution haven hypothesis.

state fixed effects and leaves these results qualitatively unchanged.[19]

Column (3) adds the non-COVID control variables in their 2016 levels and the trade war variables become quite stable hereafter in terms of their point estimates and statistical significance. Column (4) adds the non-COVID controls in changes between 2012 and 2016. The US tariff shock and agricultural subsides remain statistically significant at the $p < .01$ level while the retaliatory tariff shock is statistically significant with a p-value between $p = .018$ and $p = .064$. The positive and statistically significant coefficient on US trade war tariffs says that Trump's vote share in the 2020 election relative to his vote share in the 2016 election is higher in counties more exposed to US trade war tariffs. Conversely, the negative and statistically significant coefficient on foreign retaliatory trade war tariffs says that Trump's vote share in the 2020 election relative to his vote share in the 2016 election is lower in counties facing higher foreign retaliatory trade war tariffs. At least partly offsetting this anti-Trump effect is the pro-Trump effect coming from the positive and statistically significant effect of agricultural subsidies. These voting effects are consistent the distributional implications of trade predicted by standard international trade theory: protected US firms and workers benefit from such protection but US firms and workers suffer when targeted by foreign retaliation.

Column (5) adds the health insurance variables, both the pre-ACA level of health insurance coverage in 2013 and the post-ACA expansion between 2013 and 2018. Conditional on the controls and state fixed effects already included, the health insurance variables are essentially uncorrelated with the trade war variables as indicated by the trade war variable point estimates barely moving from those in column (4). Like the trade war variables, the health insurance coverage expansion point estimate remains quite stable hereafter, its statistical significance ranges between the $p = .016$ and $p = .072$ levels. The negative point estimate says that Trump's 2020 vote share relative to his 2016 vote share is lower in counties where health insurance coverage expanded more. An obvious interpretation is that the millions of newly insured Americans are concerned about the legislative and judicial attempts by Republicans to roll back the post-ACA expansion of health insurance coverage and penalized President Trump at the voting booth.

Column (6) further controls for pre-existing trends by controlling for the change in Republican Presidential vote share between 2012 and 2016. The positive and statistically significant relationship says that Trump continued to improve his vote share in the 2020 election in counties where he improved in 2016 over the 2012 Republican candidate Mitt Romney. Nevertheless, this effect leaves the trade war and health insurance coverage expan-

[19] Since there are no counties within Washington D.C., state fixed effects mean Washington D.C. drops out of our analysis between columns (1) and (2).

sions variables essentially unchanged.

Naturally, there are many alternative measures that capture the prevalence of COVID-19. One could use cases, deaths or hospitalizations. And, one could focus on the cumulative impact since the pandemic began, the impact during October leading up to the election, or the period of peak impact which varies notably across different geographic areas of the US. Columns (7)-(8) of Table 2 focus on one particular measure of COVID-19 prevalence: cumulative COVID-19 deaths per 10,000 population through the end of October. Column (7) introduces this measure of deaths and county-level measures capturing the extent of social distancing (MEI) and our two local economic activity variables (business foot traffic and the change in the unemployment rate).[20] While deaths are positive and statistically significant in column (7), they are not statistically significant at conventional levels in column (8) that includes all of the COVID-19 controls. Importantly, the fact that the trade war and health insurance coverage expansion variables remain essentially unchanged in column (7) relative to column (6) says they are essentially uncorrelated with COVID-19 deaths as well as social distancing and local economic activity. However, the COVID-19 controls, which include county-level health characteristics, notably reduce the magnitude of the point estimate for health insurance coverage expansion. Hence, these are important controls for estimating the voting impact of health insurance coverage expansion.

Treating column (8) of Table 2 as the baseline specification, the trade war variables and health insurance coverage expansion are economically significant. Taking the county-level perspective in Table 2, the mean county saw Trump's vote share increase by 0.23% points on account of US trade war tariffs and 0.22% points due to agricultural subsidies. On the other hand, the mean county saw Trump's vote share fall by 0.11% points due to foreign retaliatory tariffs and by 0.41% points due to health insurance coverage expansion. Moreover, given a vote share increase (decrease) for Trump implies an equivalent vote share decrease (increase) for Biden, eliminating a winning candidates vote share margin requires an offsetting effect equal to half of this margin.

Nevertheless, the effects for the mean county mask two important dimensions of heterogeneity in assessing how these issues affected the electoral college outcome. First, as illustrated by Figure 2, some counties have been more exposed to the trade war and/or seen larger increases in health insurance coverage expansion. Second, larger counties matter more for influencing the winner of their state's electoral college votes. For example, the mean county effect will understate the ultimate impact on the electoral college outcome if

[20] As described in Section 2.3, 106 counties do not have MEI data and hence they are dropped between columns (6) and (7). Additionally, 14 counties are missing some of the COVID-19 controls and hence are dropped between columns (7) and (8).

large counties in states that decided the election were among the most exposed to the trade war or had some of the largest expansions of health insurance coverage (heterogeneity of the point estimates along various dimensions is a separate issue that we address in the next sub-section).

We can take these heterogeneities into account by computing county-by-county impacts and aggregating these results to the state level. Specifically, we can use the point estimates from column (8) of Table 2 together with the values of the trade war variables and the expansion of health insurance coverage for each county to compute the counterfactual county-level vote share for Trump and Biden in the absence of any trade war variable or health insurance coverage expansion. By construction, the mean counterfactual county-level vote share that results from this exercise matches that described in the previous paragraph. But, multiplying these counterfactual county-level vote shares by county-level total votes gives counterfactual vote tallies for Trump and Biden that we can aggregate to the state level. The implied state-level change in Trump's vote share could be more or less than the mean county change because, relative to the mean county, large counties may tend to be more or less exposed to the trade war and/or see more or less expansion of health insurance coverage.

Panel A of Table 3 shows the results. Absent US trade war tariffs in the states that ultimately decided the election, Trump would have lost Georgia by 0.60% points (instead of the actual 0.26% points), Arizona by 0.54% points (instead of the actual 0.31% points), Wisconsin by 1.3% points (instead of the actual 0.63% points), and Pennsylvania by 1.59% points (instead of the actual 1.2% points). While these are modest overall, they could have mattered if the election was only slighter tighter in these states than what it was and it would have precluded the recounts that took place in Georgia and Wisconsin. Further, Biden's margin of victory would be notably larger in Pennsylvania at 1.59% points than would be Trump's margin of 0.93% points in North Carolina. Nevertheless, except for the effect in Wisconsin, these effects are smaller than the effects in New Hampshire, Minnesota, Michigan, Ohio, Iowa and Indiana that generally represent traditional swing states or states where Trump has attempted to expand into traditionally Democratic states.

Foreign retaliatory tariffs and agricultural subsidies had notably smaller effects than US tariffs in the states that decided the election. This is especially true for agricultural subsidies which barely had any impact and is not surprising given the geographic distribution of these subsidies shown in Figure 2. The strongest foreign retaliatory retaliatory effects are concentrated in Idaho, Iowa, Washington, Arkansas, Kentucky, Alabama, Wisconsin and Wyoming. But while the agricultural subsidies can substantially negate these effects (e.g. Indiana, Arkansas, and Mississippi), the largest effects of the agricultural subsidies are generally in a different set of states. These states are Iowa, Kansas, Nebraska, South Dakota, and North Dakota and the effects reach up to 1.07% points in North Dakota. Ultimately, the voting effects of the agricultural subsidies are only somewhat loosely tied to the voting effects of the retaliatory tariffs.

Rather than the trade war variables, the expansion of health insurance coverage is the most salient issue in the states that decided the election. Indeed, Panel A of Table 3 shows the effect of health insurance coverage expansion on the vote share margin in Georgia and Arizona is around 0.8-0.95% points which is easily larger than the size of Trump's loss in these states. If one interprets this effect as proxying for the voting effect associated with the possibility of removing the ACA and its associated expansion of health insurance coverage, our results say that the undermining of the ACA by the Trump administration cost Trump the electoral college votes of Georgia and Arizona. Additionally, he would have only lost Wisconsin by 0.11% points and, reflecting the mean county-level expansion of health insurance coverage in Nevada being nearly twice the US county mean, he would have only lost Nevada by 0.86% points (instead of the actual 2.45% points). While these effects would not have lost Trump the election, it would have meant that Biden needed to win all of the blue-wall states that Trump won in 2016: Michigan, Wisconsin, and Pennsylvania. Given the myriad of lawsuits hanging over the election in the weeks following the election, this would have made for a very different level of calm in the Biden campaign.

4.2 Heterogeneity

We now look at heterogeneity of these point estimates to assess whether the point estimates are notably larger for some types of counties and whether this makes a sizable difference in terms of the overall electoral impact. We first look at heterogeneity by the competitiveness of the county following the spirit of Autor et al. (2020). A county is competitive if the two-party Republican Presidential vote share was between 45% and 55% in 2012 and 2016. But, a county is solidly Republican (Democrat) if this vote share was above 55% (below 45%) in both 2012 and 2016.

Table 4 shows the results. Overall, the baseline results, reproduced in column (1), appear largely driven by solidly Democratic counties in column (3). This shows itself most clearly for the US tariff shock and agricultural subsidies where the column (3) point estimates are statistically significant and notably larger than in the baseline results as well as in solidly Republican counties (column (2)) and competitive counties (column (4)). The point estimates for the foreign tariff shock and health insurance coverage expansion suggest a similar story, although the estimates are quite imprecise in solidly Democratic counties. In competitive counties, only the US tariff shock point estimate approaches its baseline value (0.171 versus 0.223) but is much more imprecise with a p-value of 0.112. In solidly Republican counties, only agricultural subsidies are statistically significant but the point estimate is less than one-third of its baseline value. Panel B of Table 3 shows the electoral impacts are more modest than the baseline results; for example, Trump still loses Arizona. Interestingly, COVID-19 deaths are positive and statistically significant in solidly Democratic counties which says Trump improved his vote share in solidly Democratic counties by more when such counties had more COVID-19 deaths.[21] In terms of economic significance, the point estimate implies the mean number of COVID-19 deaths increases Trump's vote share by about the same amount as the mean expansion of health insurance coverage decreases Trump's vote share.

We now look at heterogeneity according to partisanship as proxied by whether the county voted for Trump or Hillary Clinton in 2016. Column (6) shows that the impact of health insurance coverage expansion is substantially larger in Clinton counties (and now statistically significant at the $p < .05$ level) than Trump counties (column (5)) where the effect is small and statistically insignificant. This conforms with the notion of health insurance coverage as a partisan issue that can be especially important for Democrats when they win close races. Similar to our solidly Democratic counties, the US tariff shock and agricultural subsidies are notably larger (and statistically significant) in Clinton counties than Trump counties and the baseline results. The baseline point estimate for COVID-19 deaths is largely unchanged across Trump and Clinton counties, but is much more precisely estimated in Trump counties.

Panel C of Table 3 shows the partisan impact of health insurance coverage expansion has a substantive impact on the economic significance of health insurance coverage expansion. Removing these impacts, our point estimates imply Trump would win Georgia by 0.93% points and Nevada by 0.71% points which are notable changes from our baseline estimates. Similar to our baseline estimates, he would still win Arizona by 0.3% points and lose Wisconsin by 0.06% points. In terms of the underlying heterogeneity, more than 1.3 million votes were cast in Nevada's largest two counties, Clarke and Washoe, which Hillary Clinton won in 2016 and have experienced an expansion of health insurance coverage that is around twice the national average. More than 1.7 million votes were cast in the Atlanta suburb counties of Fulton, Gwinnett, Colb and DeKalb that Hillary Clinton won and have all experienced health insurance coverage expand more than the national average. In Arizona, over half a million votes were cast in Pima county that Hillary Clinton won and where health insurance coverage has expansion at the national average. Ultimately, the political threat of removing

[21] The raw correlation between our baseline measure of COVID-19 deaths and the change in Trump's vote share is 0.26 in the overall sample. This is similar at 0.22 in competitive counties but much stronger at 0.40 in solidly Democratic counties and much weaker at 0.16 in solidly Republican counties.

expanded coverage under the ACA appears to be a crucial issue for Republican politicians moving forward.

4.3 Robustness

4.3.1 Alternative measures of COVID-19

Our baseline analysis focused on a particular measure of COVID-19: cumulative deaths since the pandemic began per 10,000 population. While county-level hospitalization data is not widely available, we can use cumulative cases since the pandemic began. Although differences in COVID-19 testing capacity may appear to make deaths a much better measure, our state fixed effects absorb differences in state-level testing capacity and strategies. That said, given county-level testing data is not widely available, within-state differences in testing capacity is a potential concern of using cases as a proxy for COVID-19 prevalence. We can also use different time windows for measuring the county-level prevalence of COVID-19 that influences voting behavior. In particular, we now consider not only cumulative cases or deaths since the pandemic began but also (per 100,000 population) the daily average of cases or deaths in October as well as the maximum daily average of cases or deaths in any 14-day stretch since the pandemic began.

Tables 5-6 show the results. Table 5 shows the results without the COVID-19 controls in Panel B of Table A2. Column (1) reproduces column (6) from Table 2 which excludes any COVID-19 variables. The even columns of Table 5 alter this baseline specification by using a different measure of COVID-19 prevalence and odd columns add in our measures of social distancing and local economic activity. Given columns (2)-(3) use our baseline measure of COVID-19, cumulative deaths per 10,000 population, column (3) matches column (7) of Table 2. While the point estimates are generally imprecise for other measures of COVID-19 in later columns of Table 5, they remain positive which reflects the weak but positive raw positive correlation between the change in Trump's vote share and each measure of COVID-19 cases and deaths.[22]

Table 6 extends the odd columns from Table 5 by controlling for the COVID-19 controls in Panel B of Table A2. The only statistically significant COVID-19 variable is deaths in October, which is the time period for voters' most recent image of COVID-19. For the mean county with daily October deaths per 100,000 population of 0.28, the implied increase in Trump's vote share is 0.098% points which is smaller but roughly equivalent to the economic significance of the foreign tariff shock. Ultimately, Table 6 presents little evidence of a sizable

[22] Across these six measures of COVID-19 cases and deaths, the correlation with the change in Trump's vote share ranges between 0.04 and 0.26.

and robust influence of COVID-19 cases or deaths on election outcomes. Naturally, one may worry about the endogeneity of our COVID-19 measures of deaths and cases. We return to this issue in the next sub-section.

While one may be worried about the endogeneity of our COVID-19 measures of deaths and cases, these are essentially uncorrelated with our trade war variables and health insurance coverage expansion. We discussed this point in Section 4.1 when using deaths since the pandemic began as our measure of COVID-19 prevalence. Table 6 shows this is a very robust feature of our analysis. Column (2) of Table 6 is the baseline specification of column (8) from Table 2. Columns (3)-(7) of Table 6 show the point estimates and their precision barely move across the various measures of COVID-19 cases and deaths. Thus, possible endogeneity concerns over cases or deaths as measures of COVID-19 are not a major concern for estimating the impacts of our trade war variables or health insurance coverage expansion.

4.3.2 IV

Given natural concerns about endogeneity regarding COVID-19 prevalence as well as our variables reflecting the trade war and health insurance coverage expansion, we now implement various IV strategies. For our COVID-19 measures of cases and deaths, we use two alternative instruments: a county's employment share of meat packing workers and a county's population share of nursing home residents. Conditional on our controls and fixed effects – which include, among other things, the county-level distribution of age, income, education and race as well as county-level measures of health characteristics – the exclusion restriction says the only way that the employment share of meat packing workers and the population share of nursing home residents affect voting behavior is through their effect on a county's COVID-19 deaths and cases. Although much less intuitive, our IV approach for the trade war variables and health insurance coverage expansion relies on the heteroskedasticity-based IV approach of Lewbel (2012) given the difficulty of finding traditional IVs.

We start with our traditional IV approach for COVID-19 deaths and cases. Table 7 shows the results. Column (1) reproduces our baseline fixed effects OLS specification from column (8) of Table 2. Columns (2)-(7) use the employment share of meat packing workers as the instrument while columns (8)-(13) use the population share of nursing home residents. Overall, the evidence is mixed. With the meat packing worker instrument, the only measure of COVID-19 where the Kleibergen-Paap weak instrument rk F-stat exceeds the threshold value of 10 is for cumulative cases in column (3) (the null of underidentification is easily rejected). But, the point estimate is not close to statistical significance at conventional levels. In contrast, the nursing home instrument exceeds this weak instrument threshold for four of the six COVID-19 measures (the null of underidentification is easily rejected in these specifications): cumulative cases and deaths in columns (8) and (9), October deaths in column (10), and the maximum 14-day rolling daily average of deaths in column (12). In each case, the effect of COVID-19 cases or deaths is positive and statistically significant at the $p < .05$ level of significance. They are also economically significant: the mean county on each such measure of COVID-19 deaths sees Trump's vote share increase between 0.8% points and 1.16% points and the mean effect rises to around 4% points for cumulative cases.

While the exclusion restriction for the meat packing worker instrument and the nursing home resident instrument both appear plausible, the vastly different results across the two IV approaches illustrate the difficulty of addressing potential endogeneity of COVID-19 cases and deaths. Nevertheless, as we have already argued, this problem does not carry over to estimating the effects of the trade war and health insurance coverage expansion. Focusing on the IV specifications where the weak instrument F-stat exceeds 10, the point estimates and level of statistical significance are very stable for the effects of the trade war and health insurance coverage expansion.

We now use the heteroskedasticity-based IV approach of Lewbel (2012) to address endogeneity of the trade war variables and expansion of health insurance coverage. Table 8 shows the results. Column (1) reproduces the baseline specification from column (8) of Table 2. Column (2) instruments for the trade war variables. The control variables we use to construct the Lewbel instruments as described in Section 3 are the 2016 manufacturing employment share, the 2016 share of naturalized citizens, the 2016 agricultural and mining employment share, and the percent diabetic with annual eye test. We easily reject the null of homoskedasticity in the first stage at the $p < .0001$ level of significance. The Lewbel instruments also perform satisfactorily according to standard IV specification tests. We reject the null that the model is underidentified at the $p = .078$ level of significance. The Kleibergen-Paap weak instrument rk F-stat exceeds the threshold value of 10. And, Hansen's J test of overidentification fails to reject the validity of the instruments at the $p = 0.749$ level of significance. Thus, according to standard IV specification tests, the specification in column (2) appears well identified.

The results in column (2) imply that concerns in prior literature about endogeneity of the trade war variables appear well founded. While the point estimate for the foreign tariff shock remains statistically significant, although somewhat smaller in magnitude, the point estimates for the US tariff shock and agricultural subsidies are much smaller than the baseline and not statistically significant at conventional levels. Thus, we place notably more faith in a causal interpretation of the foreign tariff shock than the US tariff shock or agricultural subsidies. Treating the trade war variables as endogenous has essentially no effect on the magnitude and statistical significance of health insurance coverage expansion which is expected given the already established lack of correlation between these two types of variables.

Column (3) moves to addressing endogeneity of health insurance coverage expansion. The control variables we use to construct the Lewbel instruments are the 2016 population share (aged 5 and up) that speaks a foreign language at home, the 2016 share of households earning at least $200,000 annually, and the 2016 share of multi-unit housing structures. We easily reject the null of homoskedasticity in the first stage at the $p < .0001$ level of significance. The Lewbel instruments also perform satisfactorily according to standard IV specification tests. We reject the null that the model is underidentified at the $p < .001$ level of significance. The Kleibergen-Paap weak instrument rk F-stat exceeds the threshold value of 10. And, Hansen's J test of overidentification fails to reject the validity of the instruments at the $p = 0.161$ level of significance. Thus, according to standard IV specification tests, the specification in column (3) appears well identified.

Unlike any endogeneity concerns over the trade war variables which seem to reduce their magnitude and statistical significance, any endogeneity concerns over health insurance coverage expansion actually increase its magnitude and statistical significance. The point estimate is now four times larger than the baseline specification and statistically significant at the $p < .01$ level. As such, the economic magnitude of health insurance coverage expansion that we described earlier appears more like a lower bound of the causal effect. Ultimately, the IV results in column (3) provide evidence of the highly salient, politically crucial, and causal effect of health insurance coverage expansion on the electoral college outcomes in the 2020 US Presidential election.

4.4 Placebo specification

One may still be concerned that our results reflect pre-existing political trends despite our efforts to guard against this through various controls in both levels and changes, controlling for the change in Trump's vote share between the 2012 and 2016 elections, and using our IV approaches. Thus, we now present results from a placebo specification where we use the change in Trump's vote share between the 2012 and 2016 elections as the dependent variable and completely the change in his vote share between 2016 and 2020 from the specification.

Table 9 shows the results. Column (1) is analogous to the baseline specification from column (8) of Table 2. Columns (2)-(3) are analogous to our Lewbel IV specifications from columns (2)-(3) of Table 8. Columns (4)-(7) are analogous to columns (2)-(3) and (8)-(9) of Table 7. Overall, the results in Table 9 clearly show there is no meaningful relationship between our key variables of interest and the change in Trump's vote share between 2012 and 2016. This provides further evidence mitigating any concerns about pre-existing political trends. The only exception is for the agricultural subsidies variable: this point estimate is strongly positive and statistically significant except in column (2) which instruments for the trade war variables.

5 Conclusion

Understanding the political economy of the 2020 US Presidential election is important given the level of controversy surrounding both the issues underlying voter decisions and the outcome itself. Not only did the election take place in the shadow of the worst public health crisis in at least 100 years and an economic contraction that rivaled the Great Recession and the Great Depression, it also took place in the shadow of other crucial issues including Trump's trade war and a continued attempt by Republican's to undermine health insurance coverage expansion under the Affordable Care Act (ACA). Each of these three issues has dramatic economic consequences for certain groups of voters. Moreover, the two candidates – Donald Trump and Joe Biden – cast starkly different visions for dealing with these issues.

Our main result is that expansion of health insurance coverage was crucial to the electoral college outcome. Larger expansion of health insurance coverage in the post-ACA period reduced Trump's vote share. A plausible interpretation is that newly insured voters penalized Trump at the voting booth for continued Republican efforts to remove and undermine the ACA and its associated expansion of health insurance coverage. Absent the anti-Trump effect of health insurance coverage expansion, our baseline results imply Trump would have won Georgia and Arizona and narrowed his loss in Wisconsin to around 0.1% points. Taking into account heterogeneity of these effects according to partisanship (as proxied by whether a county voted for Hillary Clinton or Donald Trump in 2016), Trump would have additionally won Nevada and lost Wisconsin by 0.06% points or around 2000 votes. While Trump would have still lost the electoral college, he would have been on the precipice of re-election. He would have only needed to win Wisconsin or some other blue-wall state in order to win the electoral college. Using the Lewbel (2012) heteroskedasticity-based IV approach, we show these results are robust to dealing with endogeneity of health insurance coverage expansion.

In contrast, we find that concerns in prior literature about endogeneity of the trade war variables appear well founded. As such, we find little evidence for notable voting impacts of the trade war. Given the notorious difficulty of instrumenting for tariffs and especially trade war tariffs, we rely on the Lewbel (2012) heteroskedasticity-based IV approach which performs well according to standard IV specification tests. While our baseline OLS results point to a modest electoral impact of US trade war tariffs improving Trump's vote share, this is not robust to our IV strategy. And, despite being robust to our IV strategy, the electoral impact of foreign retaliatory tariffs is very small given counties most exposed to these tariffs were outside of the states that decided the election.

Perhaps surprisingly, we find mixed evidence on the voting impact of the COVID-19 pandemic. Our two IV approaches yield very different results. Using a county's employment share of meat packing workers as an instrument, we find no evidence that COVID-19 affected Trump's vote share. Conversely, we find evidence that COVID-19, especially COVID-19 deaths, actually increase Trump's vote share when using a county's population share of nursing home residents as an instrument. While the positive effect may be surprising, it is consistent with the idea that voters preferred Trump over Biden in dealing with the economy and an economy wrecked by COVID-19. It is also consistent with the raw positive correlation between COVID-19 deaths (or cases) and the change in Trump's vote share between the 2016 and 2020 elections.

When interpreting our results on the voting impact of the COVID-19 pandemic, one must remember that our analysis relies on comparing COVID-19 deaths and cases across counties. As such, it essentially views differential COVID-19 prevalence across counties as revealing county-level differences in the COVID-19 shock. Thus, addressing any nationwide shock that the COVID-19 pandemic had on voting behavior is beyond our analysis.

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Table 1. Summary statistics for main variables

	Mean	SD	Min	Max	N
Voting variables					
Change in 2-party Rep. Pres. Vote share (2016 to 2020)	-0.52	2.60	-11.45	28.16	3,098
Change in 2-party Rep. Pres. Vote share (2012 to 2016)	5.85	5.20	-16.52	24.29	3,098
Trade war variables					
US tariff shock ($000's per worker)	1.03	1.19	0.00	12.75	3,098
Retaliatory tariff shock ($000's per worker)	0.55	1.10	0.00	22.86	3,098
Agricultural subsidies ($000's per worker)	0.43	1.08	0.00	15.93	3,098
Health insurance variables					
Change in health insurance coverage (2013 to 2018)	5.05	3.28	-15.90	22.20	3,098
Helath insurance coverage (2013)	84.94	5.59	52.70	97.60	3,098
COVID-19 variables					
Deaths cumulative (per 10k pop, through 10/31/2020)	5.73	6.02	0.00	59.14	3,098
Cases cumulative (per 1k pop, through 10/31/2020)	28.32	17.38	0.00	187.30	3,098
Deaths October (per 100k pop, per day)	0.28	0.55	0.00	12.26	3,098
Cases October (per 100k pop, per day)	24.75	21.69	0.00	298.09	3,098
Deaths (per 100k, max 14-day rolling daily average)	0.98	1.34	0.00	17.60	3,098
Cases (per 100k, max 14-day rolling daily average)	41.76	33.45	0.00	522.72	3,098
Unemployment rate change (Aug. 2019 to Aug. 2020)	2.70	1.90	-5.00	18.60	3,098
MEI daily average (1/1/2020 - 10/31/2020)	-29.28	10.52	-73.34	3.52	2,992
MEI October daily average (10/1/2020 - 10/31/2020)	-23.03	14.61	-79.74	31.08	2,992
MEI daily average over max 14-day death window	-30.23	27.08	-152.66	37.75	2,992
MEI daily average over max 14-day case window	-30.69	22.13	-162.99	24.55	2,992
Foot traffic cumulative relative growth	0.62	0.09	0.19	1.60	3,098
Foot traffic October relative growth	0.72	0.15	0.25	2.61	3,098
Foot traffic relative growth - max 14-day death window	0.66	0.18	0.14	2.61	3,098
Foot traffic relative growth - max 14-day case window	0.69	0.15	0.14	2.18	3,098
Instruments					
Meat packing workers (employment share 2012-2016)	1.27	5.06	0.00	59.81	3098
Nursing home residents (2016 population share)	0.64	0.47	0.00	5.28	3098

Table 2. Baseline results

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
US tariff Shock	0.007	0.226	0.233*	0.234*	0.225*	0.230*	0.217*	0.223*
	(0.232)	(0.198)	(0.066)	(0.065)	(0.062)	(0.058)	(0.058)	(0.056)
Retalitory tariff shock	0.107	0.045	-0.308^	-0.272^	-0.239^	-0.230^	-0.223^	-0.202#
	(0.230)	(0.284)	(0.124)	(0.135)	(0.108)	(0.106)	(0.091)	(0.107)
Agricultural Subsidies	0.527^	1.107*	0.692*	0.538*	0.607*	0.434*	0.567*	0.528*
	(0.25)	(0.20)	(0.18)	(0.17)	(0.15)	(0.13)	(0.13)	(0.13)
Δ Health insurance coverage, 2013-2018					-0.112#	-0.116^	-0.122^	-0.081#
					(0.057)	(0.051)	(0.049)	(0.044)
Δ 2-party Rep. vote Share 2012-2016						0.223*	0.220*	0.224*
						(0.033)	(0.033)	(0.030)
COVID-19 deaths cumulative (per 10k pop)							0.050*	0.025
							(0.016)	(0.016)
Mobility and Engagement Index							-0.040*	-0.018
							(0.015)	(0.013)
Foot traffic cumulative relative growth							-0.825	-1.588^
							(1.041)	(0.774)
Unemployment rate change (Aug. 2019 to Aug. 2020)							0.182*	0.113^
							(0.035)	(0.055)
N	3098	3097	3097	3097	3097	3097	2991	2977
State FE	N	Y	Y	Y	Y	Y	Y	Y
Controls in levels	N	N	Y	Y	Y	Y	Y	Y
Controls in changes	N	N	N	Y	Y	Y	Y	Y
COVID Controls	N	N	N	N	N	N	N	Y

Notes: # p<0.10, ^ p<.05, * p<.01. Dependent variable is the change in the 2-party Republican vote share between the 2016 and 2020 US Presidential election. Estimation performed by fixed effects OLS. Standard errors clustered by state. Control variables in levels and changes are listed listed in Panel A of Table A2. COVID control variables listed in Panel B of Table A2. 2013 level of health insurance coverage included from column (5) onwards. All specifications weighted by 2020 total Presidential votes cast. See main text for further details.

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

A. Baseline

		Counterfactual: removing effects of …			
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion
Nevada	-2.45	-2.60	-2.40	-2.45	-0.86
Pennsylvania	-1.20	-1.59	-1.12	-1.21	-0.63
Wisconsin	-0.63	-1.30	-0.49	-0.69	-0.11
Arizona	-0.31	-0.54	-0.25	-0.32	0.64
Georgia	-0.26	-0.60	-0.16	-0.27	0.57
North Carolina	1.37	0.93	1.46	1.34	2.19

B. Heterogeneity by competitiveness

		Counterfactual: removing effects of …			
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion
Nevada	-2.45	-2.63	-2.43	-2.45	-1.11
Pennsylvania	-1.20	-1.56	-1.17	-1.20	-0.89
Wisconsin	-0.63	-1.33	-0.55	-0.67	-0.40
Arizona	-0.31	-0.49	-0.28	-0.32	-0.30
Georgia	-0.26	-0.50	-0.22	-0.26	0.22
North Carolina	1.37	0.94	1.41	1.35	1.89

C. Heterogeneity by partisanship

		Counterfactual: removing effects of …			
	Actual	US tariff shock	Retaliatory tariff shock	Agricultural subsidies	Health insurance coverage expansion
Nevada	-2.45	-2.71	-2.43	-2.45	0.71
Pennsylvania	-1.20	-1.60	-1.17	-1.20	-0.43
Wisconsin	-0.63	-1.19	-0.57	-0.66	-0.06
Arizona	-0.31	-0.48	-0.29	-0.32	0.30
Georgia	-0.26	-0.58	-0.21	-0.26	0.93
North Carolina	1.37	0.95	1.41	1.35	2.35

Notes: Negative vote share margings indicate Trump loss. Each panel computes county-level predicted vote tallies for Trump and Biden using procedure described in main text. Point estimates used are column (8) of Table 2 for Panel A, columns (2)-(4) of Table 4 for Panel B and columns (5)-(6) of Table 4 for Panel C. Predicted vote tallies then aggregated to state-level.

Table 4. Heterogeneity by county competitiveness and partisanship

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
US Tariff Shock	0.223*	0.065	0.659*	0.171	0.101^	0.497^
	(0.056)	(0.039)	(0.200)	(0.105)	(0.041)	(0.207)
Retalitory Tariff Shock	-0.202#	-0.022	-0.226	-0.129	-0.084	-0.099
	(0.107)	(0.046)	(0.295)	(0.167)	(0.063)	(0.227)
Agriculture Subsidies	0.528*	0.148#	1.058^	0.228	0.141	0.808#
	(0.128)	(0.086)	(0.399)	(0.283)	(0.114)	(0.454)
Δ Health insurance coverage, 2013-2018	-0.081#	-0.036	-0.098	0.011	-0.027	-0.179^
	(0.044)	(0.031)	(0.097)	(0.064)	(0.032)	(0.083)
COVID-19 deaths cumulative (per 10k pop)	0.025	0.007	0.076^	0.015	0.027#	0.030
	(0.016)	(0.009)	(0.029)	(0.028)	(0.014)	(0.030)
N	2977	1970	292	691	2501	471
Sample	Baseline	Solid Republican	Solid Democrat	Competitive	Trump counties	Clinton counties

Notes: # p<0.10, ^ p<.05, * p<.01. Dependent variable is the change in the 2-party Republican vote share between the 2016 and 2020 US Presidential election. Estimation performed by fixed effects OLS. Standard errors clustered by state. Full set of control variables and fixed effects as in column (8) of Table 2. Column (1) is column (8) from Table 2. Competitive counties have 2012 and 2016 Republican 2-party Presidential vote share between 45% and 55%. Solid Republican (Democrat) counties have these vote shares above 55% (below 45%) in 2012 and 2016. Trump (Clinton) are counties that Trump (Clinton) won in 2016. All specifications weighted by 2020 total Presidential votes cast. See main text for further details.

Table 5. Alternative measures of COVID-19 - without COVID-19 controls

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
US Tariff Shock	0.230*	0.239*	0.213*	0.244*	0.217*	0.231*	0.202*	0.233*	0.203*	0.239*	0.219*	0.237*	0.220*
	(0.058)	(0.062)	(0.059)	(0.059)	(0.058)	(0.058)	(0.057)	(0.058)	(0.057)	(0.060)	(0.059)	(0.061)	(0.060)
Retalitory Tariff Shock	-0.230^	-0.240^	-0.212^	-0.254^	-0.223^	-0.230^	-0.182#	-0.227^	-0.180#	-0.248^	-0.191^	-0.241^	-0.188#
	(0.106)	(0.109)	(0.095)	(0.103)	(0.091)	(0.105)	(0.091)	(0.104)	(0.091)	(0.106)	(0.092)	(0.110)	(0.097)
Agriculture Subsidies	0.434*	0.421*	0.590*	0.407*	0.567*	0.430*	0.598*	0.428*	0.596*	0.396*	0.510*	0.410*	0.518*
	(0.133)	(0.127)	(0.129)	(0.125)	(0.127)	(0.130)	(0.142)	(0.133)	(0.144)	(0.127)	(0.125)	(0.126)	(0.125)
Δ Health insurance coverage, 2013-2018	-0.116^	-0.118^	-0.132^	-0.106^	-0.122^	-0.117^	-0.129^	-0.117^	-0.128^	-0.109^	-0.117^	-0.120^	-0.128^
	(0.051)	(0.051)	(0.051)	(0.049)	(0.049)	(0.051)	(0.051)	(0.051)	(0.051)	(0.049)	(0.051)	(0.051)	(0.053)
COVID-19 deaths cumulative (per 10k pop)		0.066*	0.050*										
		(0.021)	(0.016)										
COVID-19 cases cumulative (per 1k pop)				0.009	0.009								
				(0.007)	(0.007)								
Deaths October (per 100k pop, per day)						0.342	0.328						
						(0.221)	(0.213)						
Cases October (per 100k pop, per day)								0.003	0.003				
								(0.003)	(0.004)				
Deaths (per 100k, max 14-day rolling daily average)										0.229*	0.213*		
										(0.084)	(0.067)		
Cases (per 100k, max 14-day rolling daily average)												0.006	0.006#
												(0.004)	(0.003)
Unemployment rate change (Aug. 2019 to Aug. 2020)			0.198*		0.182*		0.205*		0.205*		0.206*		0.218*
			(0.039)		(0.035)		(0.039)		(0.040)		(0.037)		(0.041)
MEI daily average (1/1/2020 - 10/31/2020)			-0.046*		-0.040*								
			(0.015)		(0.015)								
Foot traffic cumulative relative growth			-0.400		-0.825								
			(1.214)		(1.041)								
MEI October daily average (10/1/2020 - 10/31/2020)							-0.017#		-0.017#				
							(0.009)		(0.009)				
Foot traffic October relative growth							-2.087*		-2.090*				
							(0.665)		(0.662)				
MEI daily average over max 14-day death window											0.003		
											(0.003)		

Table 5 (cont.). Alternative measures of COVID-19 - without COVID-19 controls

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**	**(13)**
Foot traffic relative growth - max 14-day death window											-0.985		
											(0.602)		
MEI daily average over max 14-day case window													
													(0.005)
Foot traffic relative growth - max 14-day case window													-0.915
													(0.741)
N	3097	3097	2991	3097	2991	3097	2991	3097	2991	3097	2991	3097	2991

Notes: # p<0.10, ^ p<.05, * p<.01. Dependent variable is the change in the 2-party Republican vote share between the 2016 and 2020 US Presidential election. Estimation performed by fixed effects OLS. Standard errors clustered by state. Apart from COVID-19 controls in Panel B of Table A2, full set of remaining control variables from column (7) of Table 2 and state fixed effects included. Columns (1) and (3) are columns (6) and (7) from Table 2. All specifications weighted by 2020 total Presidential votes cast. See main text for further details.

Table 6. Alternative measures of COVID-19 - with COVID-19 controls

Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
US Tariff Shock	0.230*	0.223*	0.216*	0.214*	0.211*	0.218*	0.215*
	(0.058)	(0.056)	(0.058)	(0.056)	(0.056)	(0.056)	(0.057)
Retalitory Tariff Shock	-0.230^	-0.202#	-0.197#	-0.186#	-0.191#	-0.181	-0.180
	(0.106)	(0.107)	(0.106)	(0.108)	(0.109)	(0.109)	(0.109)
Agricultural Subsidies	0.434*	0.528*	0.548*	0.537*	0.547*	0.512*	0.525*
	(0.133)	(0.128)	(0.130)	(0.133)	(0.131)	(0.130)	(0.130)
Δ Health insurance coverage, 2013-2018	-0.116^	-0.081#	-0.081#	-0.083#	-0.082#	-0.081#	-0.078#
	(0.051)	(0.044)	(0.043)	(0.045)	(0.044)	(0.045)	(0.042)
COVID-19 deaths cumulative (per 10k pop)		0.025					
		(0.016)					
COVID-19 cases cumulative (per 1k pop)			-0.002				
			(0.005)				
Deaths October (per 100k pop, per day)				0.350^			
				(0.169)			
Cases October (per 100k pop, per day)					0.000		
					(0.004)		
Deaths (per 100k, max 14-day rolling daily average)						0.088	
						(0.062)	
Cases (per 100k, max 14-day rolling daily average)							0.000
							(0.003)
Unemployment rate change (Aug. 2019 to Aug. 2020)		0.113^	0.119^	0.127^	0.126^	0.137^	0.139^
		(0.055)	(0.057)	(0.054)	(0.053)	(0.054)	(0.055)
MEI daily average (1/1/2020 - 10/31/2020)		-0.018	-0.021				
		(0.013)	(0.013)				
Foot traffic cumulative relative growth		-1.588^	-1.444#				
		(0.774)	(0.831)				
MEI October daily average (10/1/2020 - 10/31/2020)				-0.005	-0.005		
				(0.007)	(0.008)		
Foot traffic October relative growth				-2.260*	-2.256*		
				(0.414)	(0.421)		
MEI daily average over max 14-day death window						0.007#	
						(0.003)	
Foot traffic relative growth - max 14-day death window						-1.197^	
						(0.465)	
MEI daily average over max 14-day case window							0.005
							(0.003)
Foot traffic relative growth - max 14-day case window							-0.945
							(0.596)
N	3097	2977	2977	2977	2977	2977	2977

Notes: # p<0.10, ^ p<.05, * p<.01. Dependent variable is the change in the 2-party Republican vote share between the 2016 and 2020 US Presidential election. Estimation performed by fixed effects OLS. Standard errors clustered by state. Full set of control variables and fixed effects as in column (8) of Table 2. Columns (1) and (2) are columns (6) and (8) from Table 2. All specifications weighted by 2020 total Presidential votes cast. See main text for further details.

Table 7. IV estimation for COVID-19 deaths and cases

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
US Tariff Shock	0.223*	0.159#	0.191*	0.170#	0.259^	0.157#	0.186*	0.256*	0.336*	0.242*	0.334	0.245*	0.300*
	(0.056)	(0.089)	(0.069)	(0.089)	(0.129)	(0.084)	(0.068)	(0.064)	(0.103)	(0.063)	(0.299)	(0.065)	(0.097)
Retalitory Tariff Shock	-0.202#	-0.156	-0.178#	-0.268#	-0.045	-0.16	-0.152	-0.226^	-0.290#	-0.133	0.181	-0.190#	-0.262
	(0.107)	(0.133)	(0.106)	(0.135)	(0.311)	(0.123)	(0.098)	(0.103)	(0.158)	(0.123)	(0.623)	(0.107)	(0.193)
Agriculture Subsidies	0.528*	0.707*	0.613*	0.685*	0.026	0.880^	0.726*	0.437*	0.234	0.443^	-0.784	0.349^	-0.064
	(0.128)	(0.200)	(0.133)	(0.214)	(1.024)	(0.382)	(0.232)	(0.125)	(0.218)	(0.188)	(2.284)	(0.148)	(0.345)
Δ Health insurance coverage, 2013-2018	-0.081#	-0.086#	-0.071#	-0.065	-0.188	-0.056	-0.038	-0.078#	-0.129#	-0.094#	-0.353	-0.092^	-0.195
	(0.044)	(0.049)	(0.041)	(0.052)	(0.230)	(0.060)	(0.055)	(0.045)	(0.074)	(0.047)	(0.562)	(0.044)	(0.122)
COVID-19 deaths cumulative (per 10k pop)	0.025	-0.235						0.156^					
	(0.016)	(0.228)						(0.067)					
COVID-19 cases cumulative (per 1k pop)			-0.032						0.145^				
			(0.035)						(0.070)				
Deaths October (per 100k pop, per day)				-5.578						4.131^			
				(6.025)						(2.000)			
Cases October (per 100k pop, per day)					0.212						0.542		
					(0.391)						(0.876)		
Deaths (per 100k, max 14-day rolling daily average)						-1.567						0.822^	
						(1.541)						(0.379)	
Cases (per 100k, max 14-day rolling daily average)							-0.035						0.101#
							(0.035)						(0.052)
N	2977	2977	2977	2977	2977	2977	2977	2977	2977	2977	2977	2977	2977
Instrument	None	Meat packing workers share						Nursing home residents population share					
Underidentification p-value	0	0.128	0.001	0.241	0.478	0.174	0.054	0.000	0.003	0.001	0.491	0.000	0.010
K-P weak instrument rk F-statistic	0	2.136	12.083	1.352	0.509	1.734	3.344	55.475	13.336	15.068	0.436	53.080	7.225

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 in columns (2)-(13). Standard errors clustered by state. Full set of control variables and fixed effects as in column (8) of Table 2. Columns (1) is column (8) from Table 2. All specifications weighted by 2020 total Presidential votes cast. See main text for further details.

Table 8. IV specifications for trade war and health insurance variables

Variable	(1)	(2)	(3)
US tariff Shock	0.223*	0.109	0.175*
	(0.056)	(0.073)	(0.049)
Retalitory tariff shock	-0.202#	-0.170#	-0.110
	(0.107)	(0.101)	(0.091)
Agricultural subsidies	0.528*	0.02	0.519*
	(0.128)	(0.263)	(0.125)
Δ Health insurance coverage, 2013-2018	-0.081#	-0.075#	-0.325*
	(0.044)	(0.044)	(0.106)
N	2977	2977	2977
Endogenous variables		US tariff Shock Retalitory tariff shock Agricultural subsidies	Δ Health insurance coverage
Underidentification p-value		0.078	0.005
K-P weak instrument rk F-statistic		10.972	13.988
Overidentification p-value		0.749	0.161

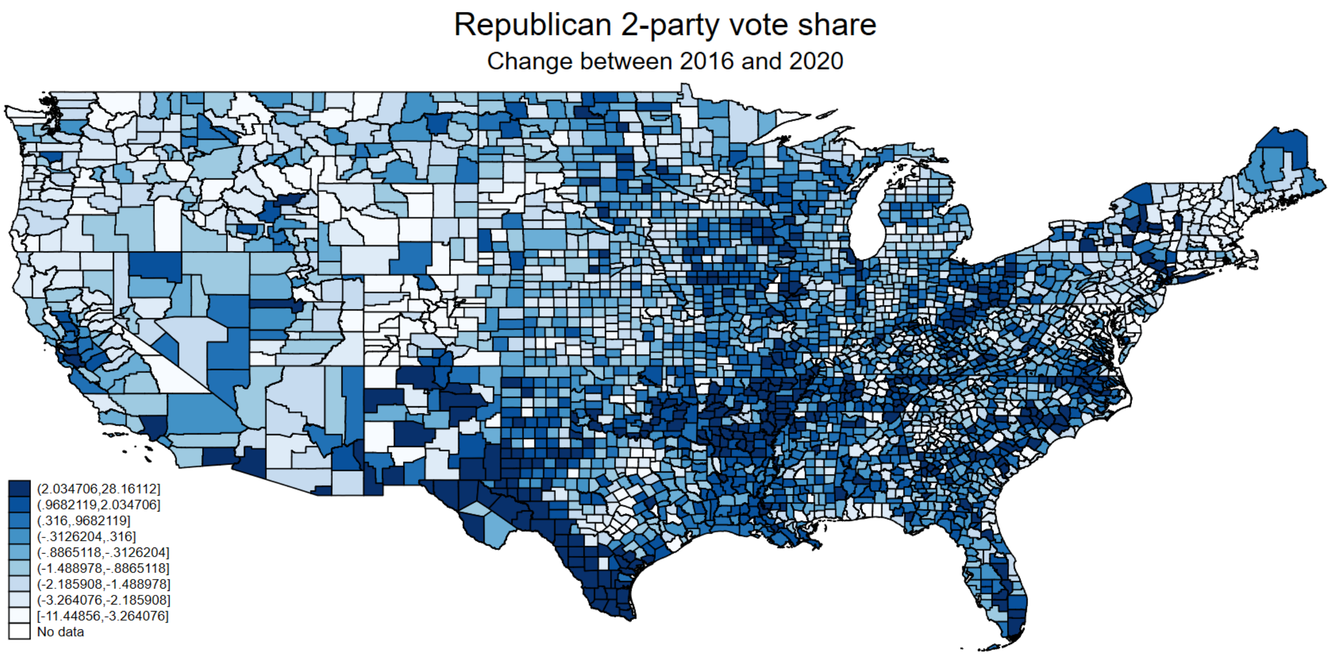
Notes: # p<0.10, ^ p<.05, * p<.01. Dependent variable is the change in the 2-party Republican vote share between the 2016 and 2020 US Presidential election. Estimation performed by IV-GMM. Standard errors clustered by state. Full set of control variables and fixed effects as in column (8) of Table 2. Column (1) is columns (8) from Table 2. All specifications weighted by 2020 total Presidential votes cast. Lewbel instruments in column (2) created by demeaning and multiplying the following variables the first stage residuals: manufacturing share of employment, population share of naturalized citizens, agricuture and mining share of employment, and the percent diabetic with annual eye exam. Lewbel instruments in column (3) created by demeaning and multiplying the following variables the first stage residuals: share of population aged 5+ that speaks a foreign language at home, share of households earning above $200,000 annually, and the share of multi-unit housing structures. See main text for further details.

Table 9. Placebo specification

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
US tariff Shock	-0.056	0.034	-0.085	-0.06	-0.056	-0.058	-0.07
	(0.081)	(0.111)	(0.078)	(0.100)	(0.083)	(0.087)	(0.124)
Retalitory tariff shock	-0.028	(0.076)	(0.011)	(0.025)	(0.028)	(0.026)	(0.016)
	(0.076)	(0.140)	(0.082)	(0.092)	(0.081)	(0.082)	(0.110)
Agricultural subsidies	0.931*	0.031	0.923*	0.942^	0.930*	0.938*	0.964*
	(0.282)	(0.323)	(0.285)	(0.354)	(0.299)	(0.272)	(0.316)
Δ Health insurance coverage, 2013-2018	0.026	0.037	-0.035	0.025	0.027	0.025	0.033
	(0.060)	(0.060)	(0.220)	(0.062)		(0.061)	
COVID-19 deaths cumulative (per 10k pop)	-0.014	-0.014	-0.015	0.048		0.048	
	(0.028)	(0.026)	(0.028)	(0.070)		(0.065)	
COVID-19 cases cumulative (per 1k pop)					-0.004		-0.022
					(0.04)		(0.12)
N	2977	2977	2977	2977	2977	2977	2977
Endogenous variables	None	US tariff Shock Retalitory tariff shock Agricultural subsidies	Δ HI coverage	Deaths	Cases	Deaths	Cases
Instruments		Lewbel	Lewbel	Meat packing emp. share		Nursing home pop. share	
Underidentification p-value		0.079	0.005	0.130	0.001	0.000	0.005
K-P weak instrument rk F-statistic		11.022	12.297	2.120	11.856	56.072	11.540
Overidentification p-value		0.743	0.183				

Notes: # p<0.10, ^ p<.05, * p<.01. Dependent variable is the change in the 2-party Republican vote share between the 2012 and 2016 US Presidential election. Estimation performed by fixed effects OLS in column (1), IV-GMM in columns (2)-(3) and IV in columns (4)-(7). Standard errors clustered by state. Full set of control variables and fixed effects as in column (8) of Table 2 (except the 2012-2016 change in the Republican vote share). All specifications weighted by 2020 total Presidential votes cast. Lewbel instruments in columns (2)-(3) created using the same controls listed in notes to Table 8. See main text for further details.

Figure 1: Change in 2-party Republican vote share



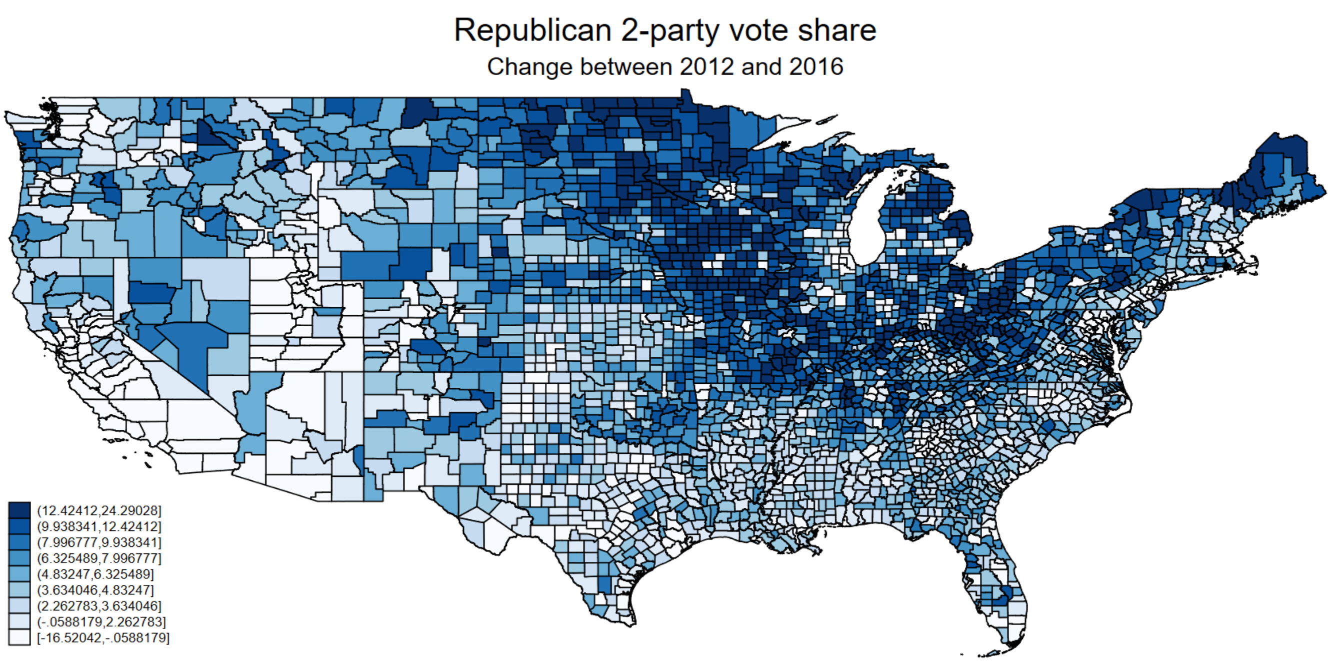
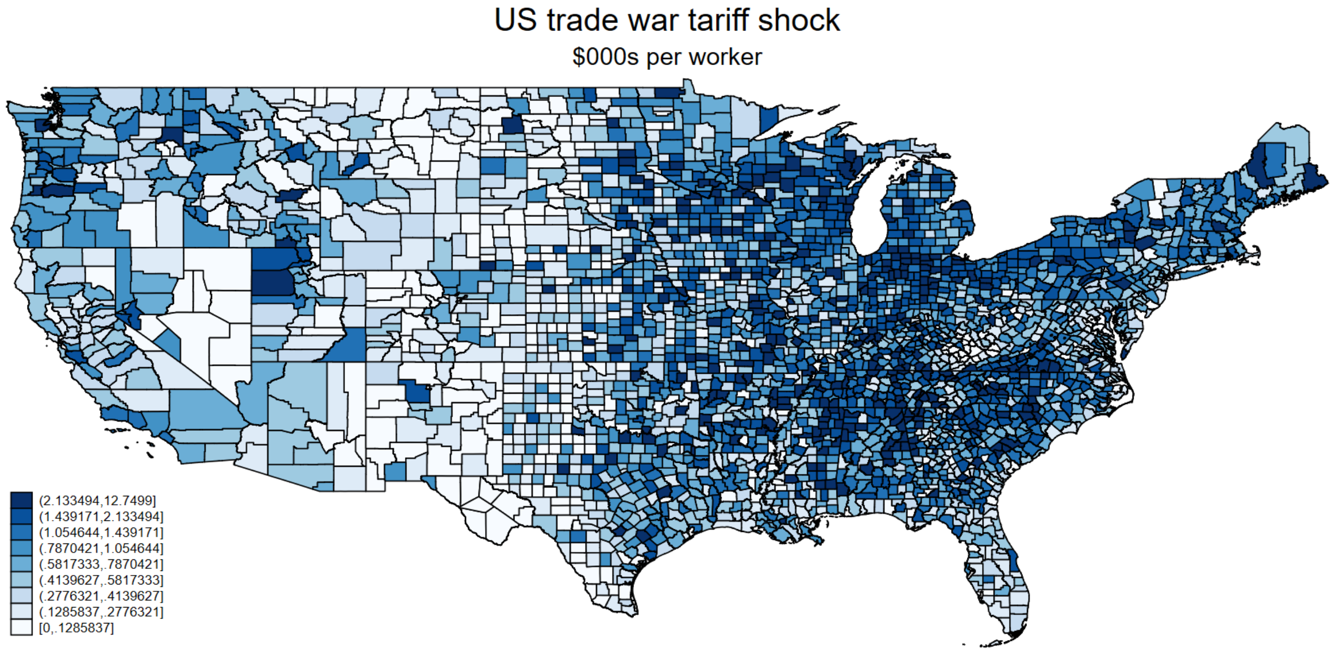


Figure 2: Trade war variables and health insurance coverage expansion



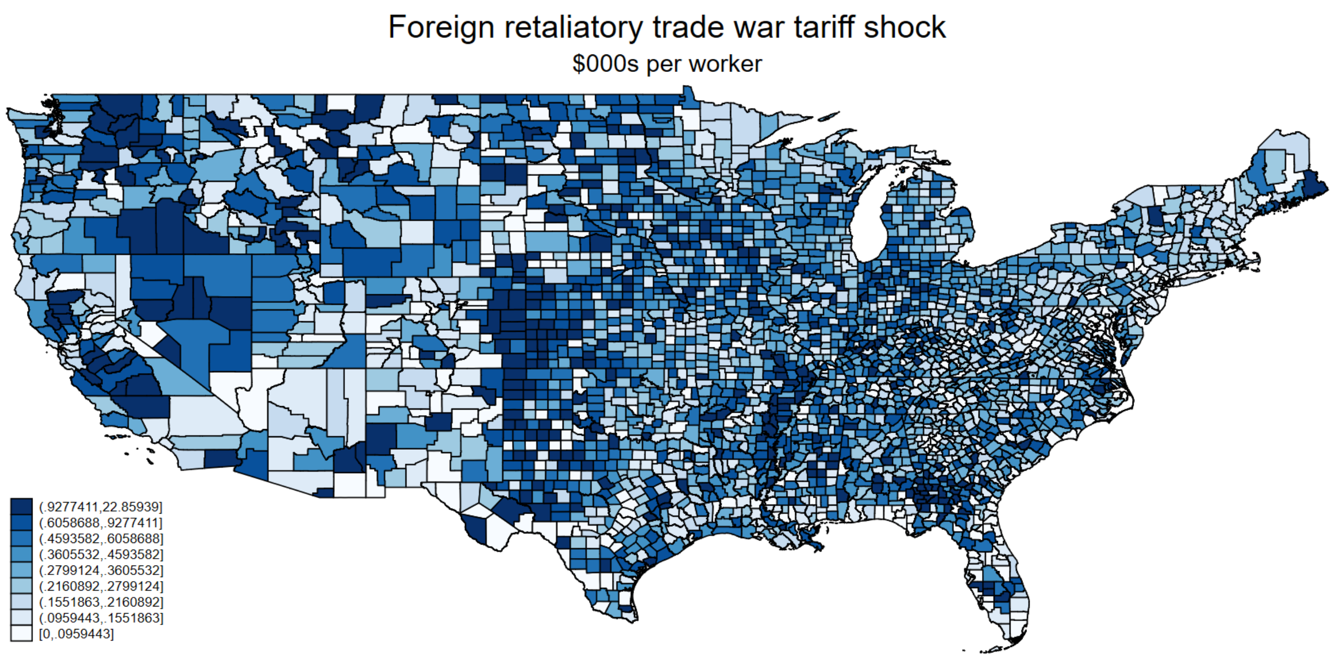
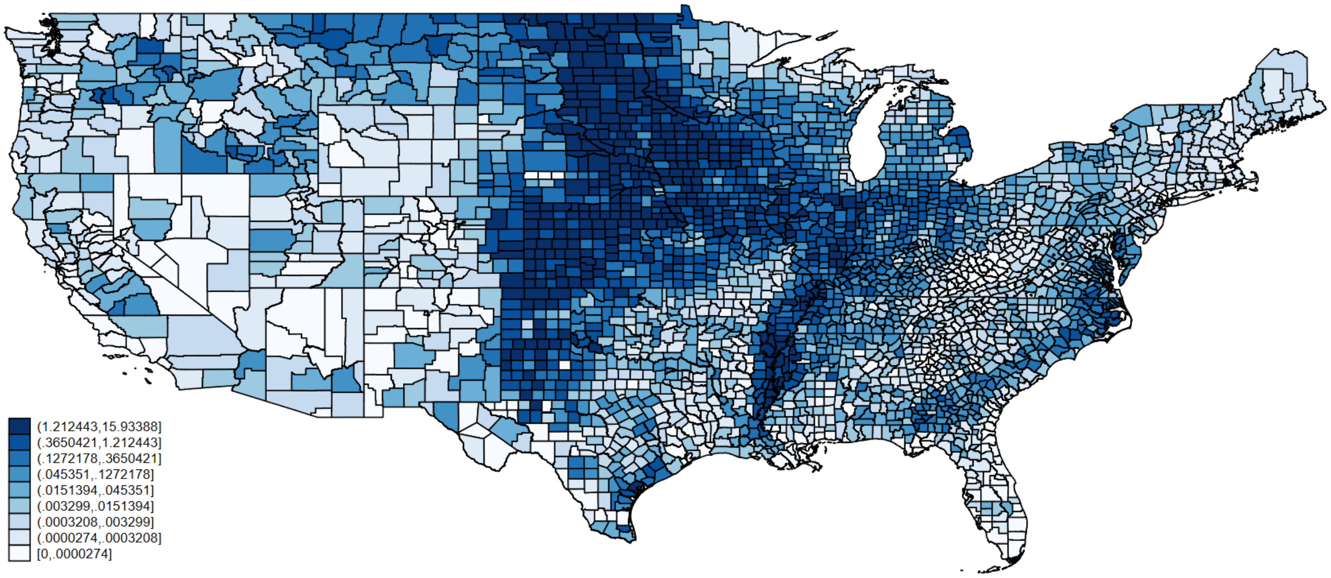


Figure 2 (cont.): Trade war variables and health insurance coverage expansion

Agricultural subsidies

$000s per worker



Health insurance coverage expansion

Pre-ACA to post-ACA

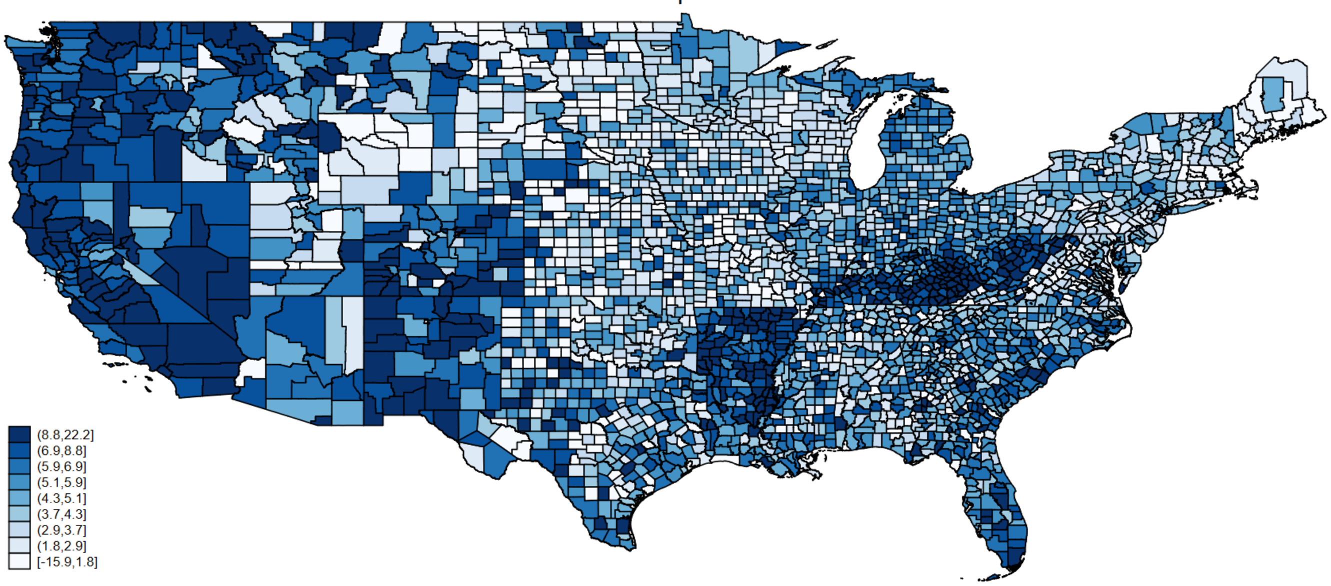


Figure 3: COVID cases and deaths

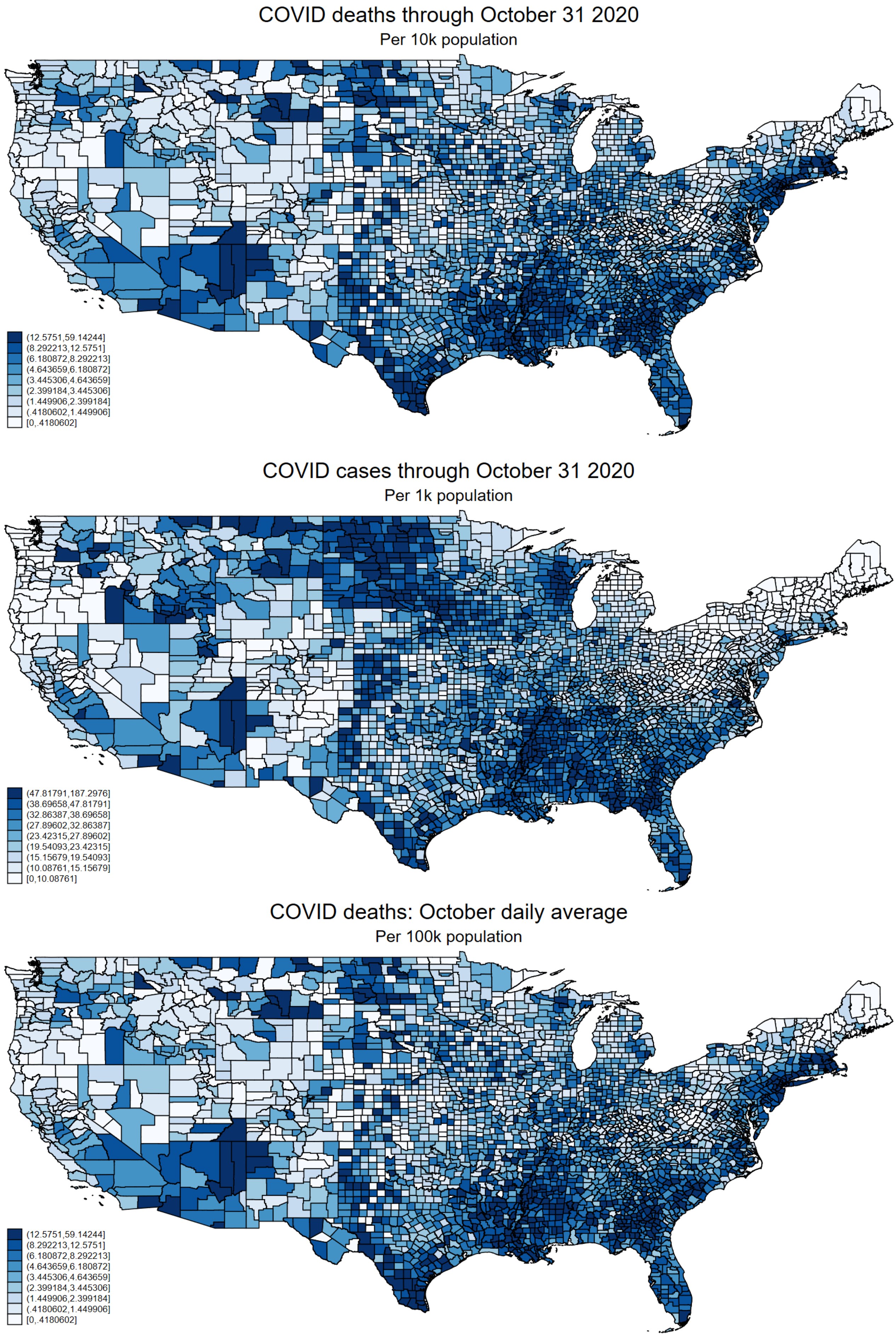
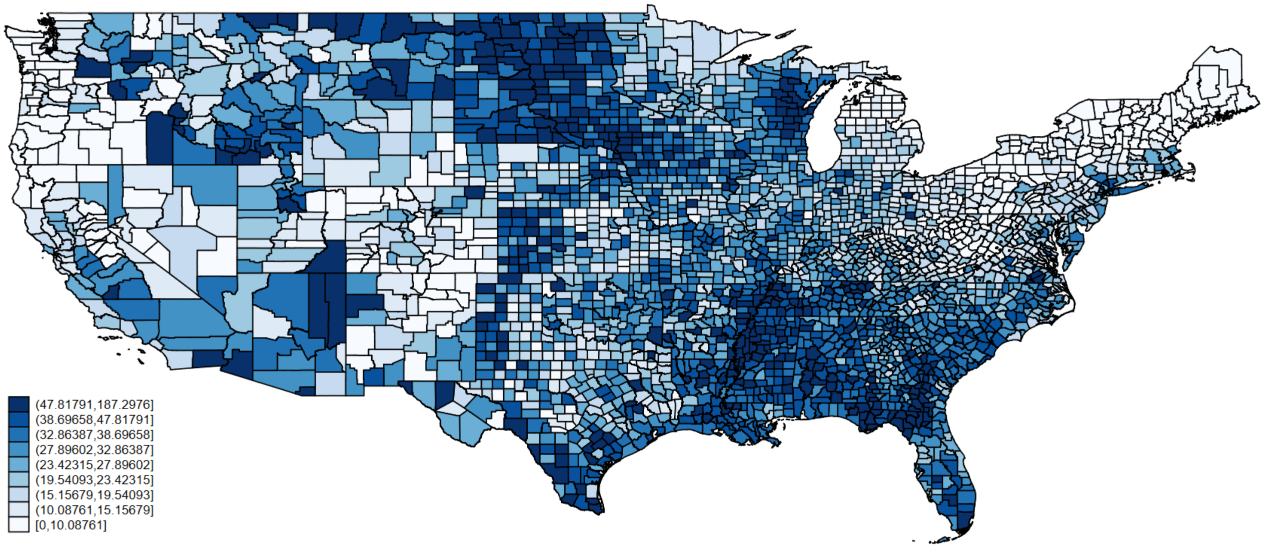


Figure 3 (cont.): COVID cases and deaths

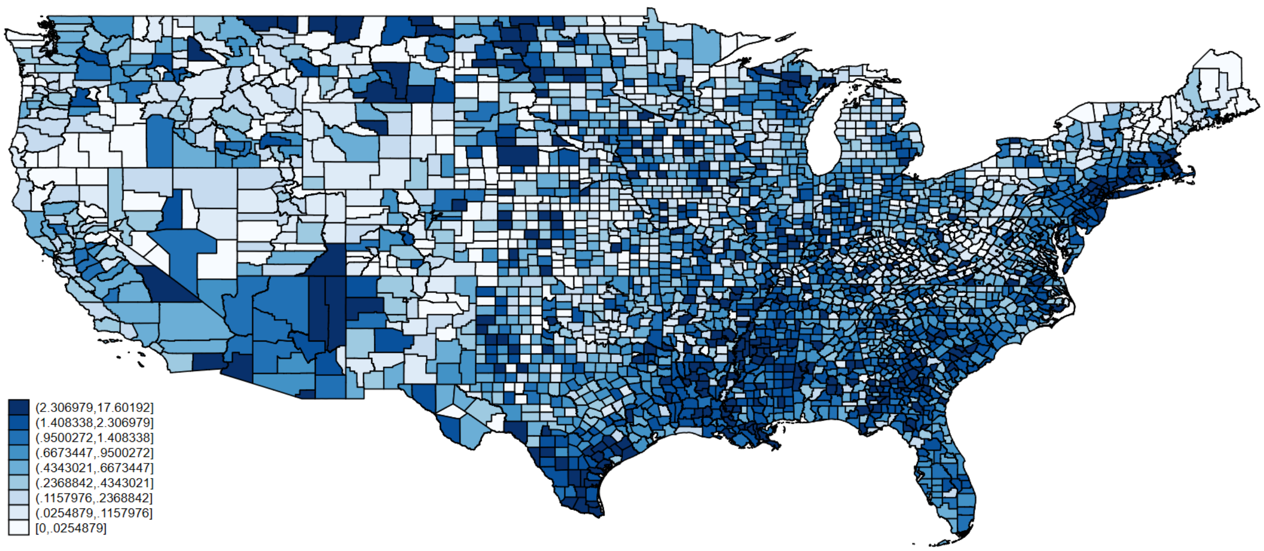
COVID cases: October daily average

Per 100k population



COVID deaths: max 14-day daily rolling average

Per 100k population



COVID cases: max 14-day daily rolling average

Per 100k population

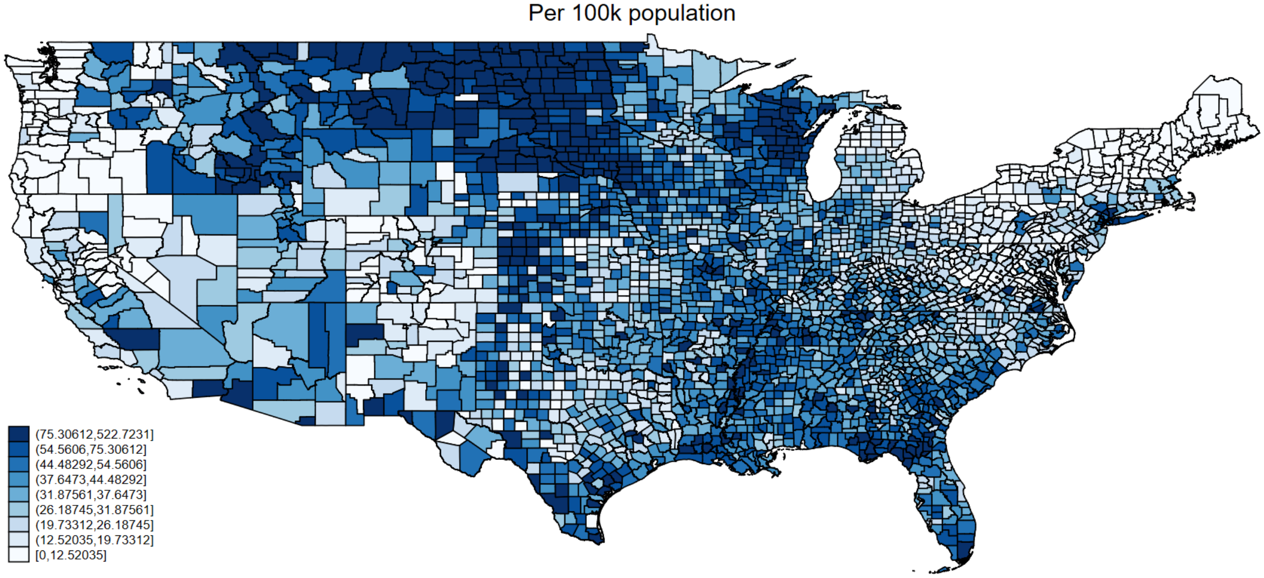


Figure 4: Social distancing and economic activity

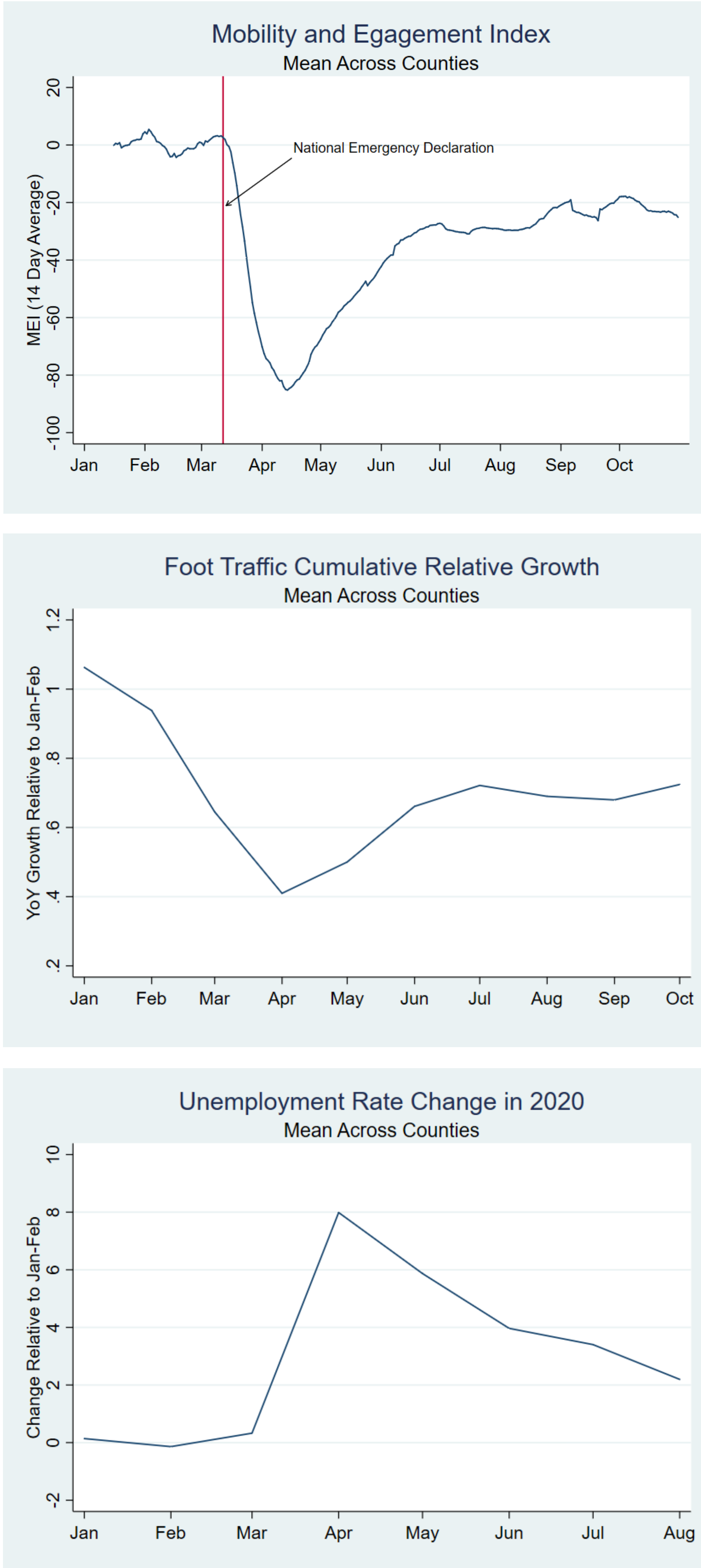


Figure 4 (cont.): Social distancing and economic activity

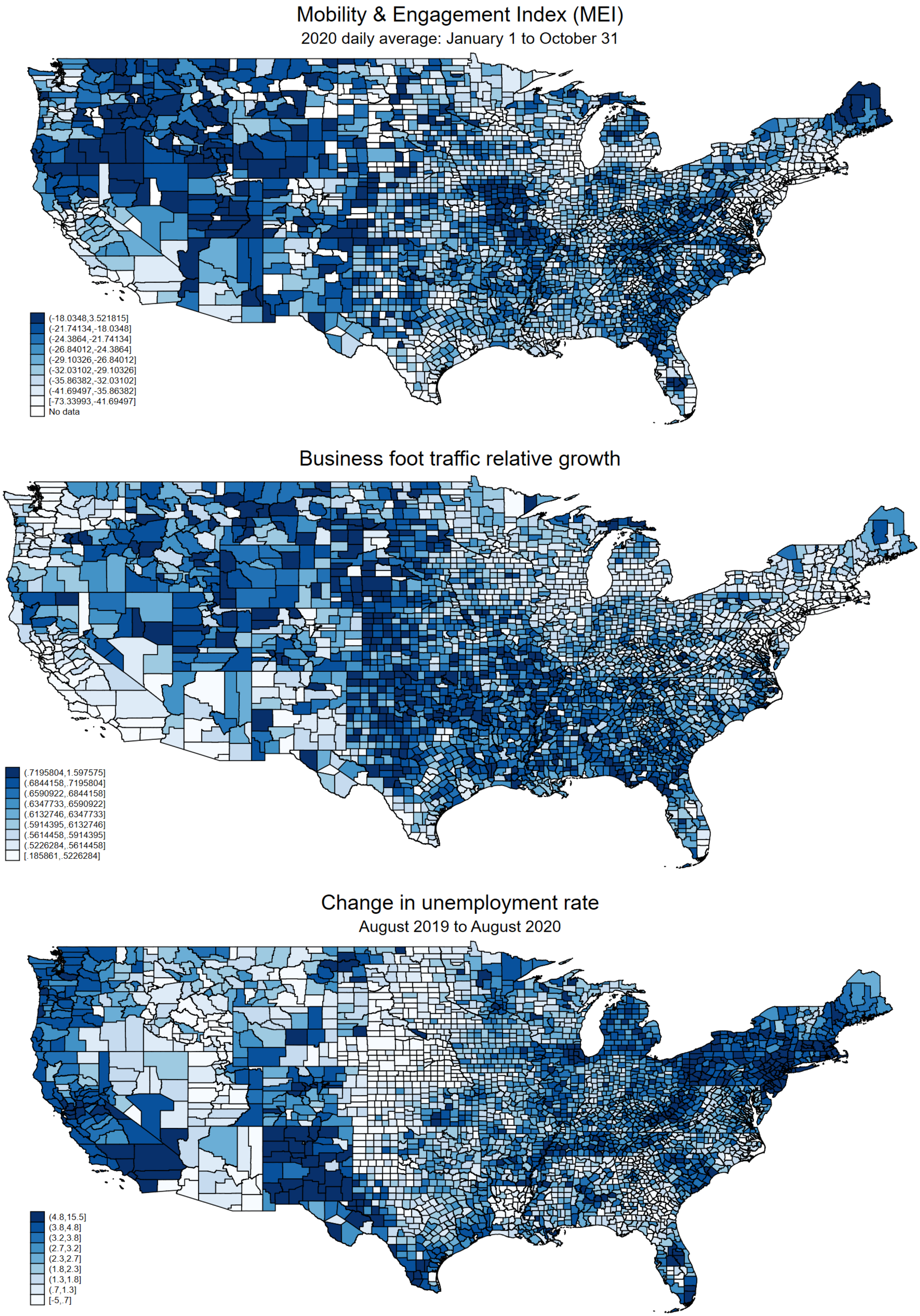
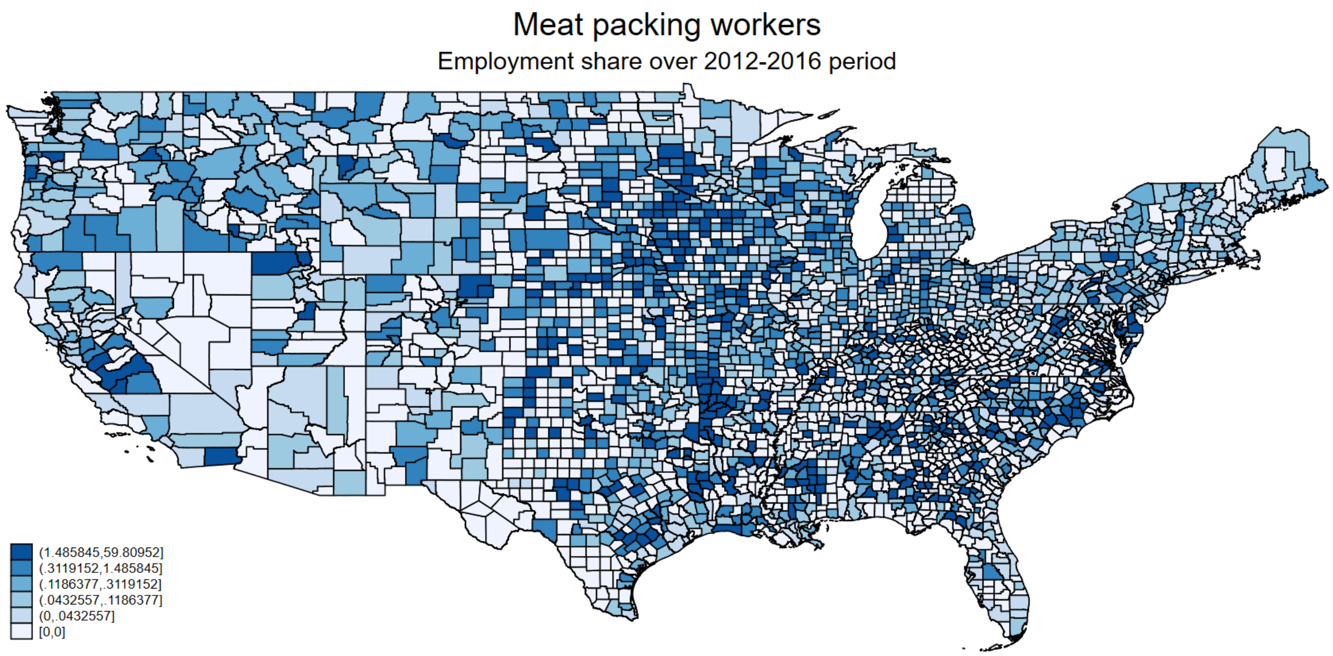


Figure 5: Nursing home and meat packing worker instrumental variables



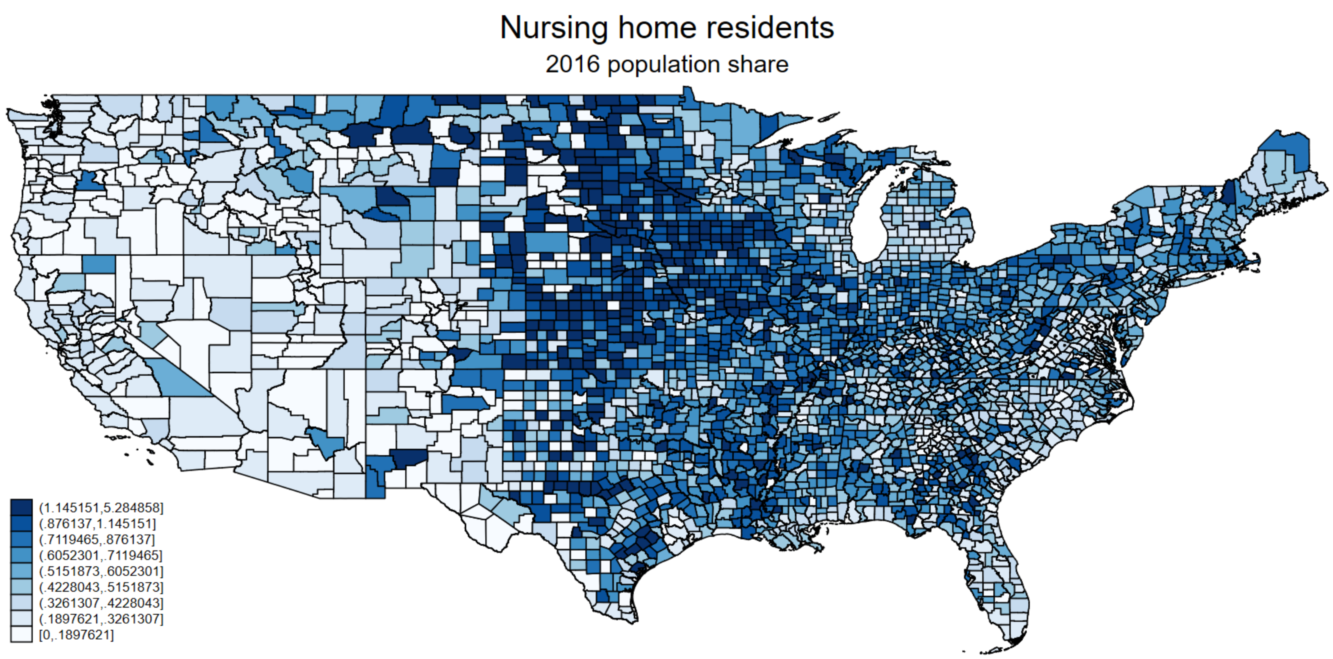


Table A1. Trade war tariffs

A. US tariffs

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

B. Foreign tariffs

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

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

Table A2. Summary statistics for control variables

	Mean	SD	Min	Max	N
A.1 Level in 2016					
Population shares					
Age under 20	25.19	3.59	4.90	43.40	3,098
Age 20-24	6.40	2.49	0.40	32.50	3,098
Age 25-44	23.30	3.30	8.70	43.40	3,098
Age 45-64	27.49	3.03	9.00	47.40	3,098
Age 65-74	9.98	2.51	3.00	33.60	3,098
Age 75+	7.64	2.33	0.00	19.90	3,098
H/hold annual income below $25k	26.77	8.19	5.50	60.06	3,098
H/hold annual income $25k-$50k	26.19	4.00	8.11	41.68	3,098
H/hold annual income $50k-$75k	18.54	2.79	6.60	30.20	3,098
H/hold annual income $75k-$100k	11.67	2.71	1.30	32.43	3,098
H/hold annual income $100k-$150k	10.73	3.97	1.30	27.80	3,098
H/hold annual income $150k-$200k	3.27	2.16	0.00	16.30	3,098
H/hold annual income $200k plus	2.84	2.56	0.00	25.33	3,098
Female	49.98	2.33	21.50	58.50	3,098
Hispanic	9.65	13.30	0.64	95.49	3,098
Asian	1.82	3.03	0.20	60.93	3,098
Black	9.99	13.36	0.23	70.91	3,098
White (only)	76.37	17.81	3.57	97.01	3,098
Other	5.25	6.49	0.45	79.13	3,098
Less than high school	32.41	5.10	18.22	57.04	3,098
High school graduates	33.25	4.82	9.89	46.29	3,098
Some college	19.12	2.78	8.28	28.31	3,098
College graduates	15.22	5.83	5.59	59.09	3,098
Employment shares					
Employed in manufacturing	6.71	4.08	0.00	29.01	3,098
Employed in agric or mining	3.79	4.45	0.00	37.00	3,098
Population shares (age 16+)					
Unemployed	4.01	1.65	0.00	18.80	3,098
Not in labor force	41.27	7.90	19.60	85.50	3,098
Other					
Median household income (real)	47,833	12,502	18972	125,672	3,098
A.2 Change between 2012 and 2016					
Age under 20	-0.89	1.33	-15.10	7.80	3,098
Age 20-24	0.25	0.93	-7.40	7.20	3,098
Age 25-44	-0.43	1.45	-30.10	19.70	3,098
Age 45-64	-0.47	1.40	-23.40	16.20	3,098
Age 65-74	1.22	0.93	-8.70	19.10	3,098
Age 75+	0.31	0.76	-6.90	8.20	3,098

Table A2 (cont.). Summary statistics for control variables

	Mean	SD	Min	Max	N
H/hold annual income below $25k	-1.38	3.11	-23.01	20.02	3,098
H/hold annual income $25k-$50k	-0.91	2.84	-18.34	13.18	3,098
H/hold annual income $50k-$75k	-0.23	2.47	-17.79	16.00	3,098
H/hold annual income $75k-$100k	0.24	2.07	-15.41	23.83	3,098
H/hold annual income $100k-$150k	1.13	1.90	-8.02	15.28	3,098
H/hold annual income $150k-$200k	0.56	0.96	-7.79	6.21	3,098
H/hold annual income $200k plus	0.59	1.00	-5.81	8.19	3,098
Female	-0.06	1.16	-12.30	23.90	3,098
Hispanic	0.62	2.35	-27.88	24.60	3,098
Asian	0.21	0.57	-8.70	5.83	3,098
Black	0.23	2.81	-29.62	31.64	3,098
White (only)	-1.14	4.12	-28.84	28.84	3,098
Other	0.14	2.54	-23.08	27.05	3,098
Less than high school	-1.91	1.85	-15.78	11.30	3,098
High school graduates	0.10	1.81	-9.00	15.39	3,098
Some college	0.76	1.27	-5.17	8.13	3,098
College graduates	1.06	1.99	-15.43	14.56	3,098
Employed in manufacturing	0.00	1.18	-7.00	5.89	3,098
Employed in agric or mining	-0.05	1.28	-16.08	11.09	3,098
Unemployed	-1.05	1.35	-10.40	9.00	3,098
Not in labor force	1.64	2.75	-18.90	27.80	3,098
Median household income (real)	2,319	3,449	-18,810	31,146	3,098
B. Additional COVID-19 controls					
Population (2016)	102,540	327,310	76.00	10,100,000	3098
Metro size: large (2013)	0.14	0.35	0.00	1.00	3098
Metro size: medium or small (2013)	0.23	0.42	0.00	1.00	3098
Share of multi-unit housing structures (2016)	12.56	9.30	0.00	98.26	3098
Public transport commuters (2016, share of emp)	0.95	3.11	0.00	61.80	3098
Effective population density	404.74	720.91	3.46	22,646.76	3098
Foreign language at home (2016 pop share, age 5+)	9.32	11.63	0.00	96.10	3098
Foreign born (2016 pop share)	4.63	5.64	0.00	52.20	3098
Naturalized citizens (2016 pop share)	42.90	18.82	0.00	100.00	3098
Poverty (2016 pop share)	16.44	6.54	1.80	53.90	3098
Social capital (2014)	0.00	1.26	-3.18	21.81	3098
% diabetic with annual eye test	66.11	7.59	31.37	90.00	3044
% diabetic with annual lipids test	78.31	7.86	19.66	94.48	3047
% diabetic with annual hemoglobin test	83.70	6.59	16.91	100.00	3059
30-day mortality for pnuemonia	0.12	0.03	0.00	0.63	3097
30-day mortality for heart failure	0.11	0.02	0.00	0.34	3097
30-day hospital mortality rate index	0.46	1.21	-7.78	8.47	3096