Did Trump's Trade War Impact the 2018 Election?*

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Abstract

We uncover evidence that the US-China trade war was consequential for voting outcomes in the 2018 congressional midterm election. Republican House candidates lost support in counties more exposed to tariff retaliation, but saw no appreciable gains in counties that received more direct US tariff protection. The electoral losses were only modestly mitigated by the US agricultural subsidies announced in summer 2018. Republicans also fared worse in counties that had seen recent gains in health insurance coverage (where efforts to repeal the Affordable Care Act may have been more consequential), and where a new federal cap on state and local tax (SALT) deductions disadvantaged more taxpayers. Counterfactual calculations suggest that Republicans would have lost ten fewer House seats absent the trade war, in a similar range to either health care or SALT policies in the number of lost seats it can account for.

JEL Classifications: F13, F14.

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1 Introduction

In early 2018, President Donald Trump launched a series of unprecedented actions to raise tariffs against major US trading partners. By September 2018, these newly-introduced duties covered over 12% of US imports (Bown, 2021). These tariffs were met with swift retaliation against US exports by China, Canada, the European Union, Mexico, and others.[1] While the new US tariffs offered some protection for certain import-competing industries, retaliatory tariffs hurt other US producers. The export-dependent agricultural sector was especially hard-hit, prompting the Trump administration to announce a $12-billion subsidy program in summer 2018 to assist farmers.

These tariff-related events were exceptional in scope and scale, and by the eve of the November 2018 midterm elections, the potential economic repercussions of the trade war were widely publicized in both national and local media.[2] But did this ultimately affect how the electorate voted? This paper maps the geographic distribution of exposure to the 2018 tariff actions, as well as the subsequent agricultural subsidies, and evaluates whether this exposure may have influenced voting in the 2018 elections for the US House of Representatives.

We measure the extent to which US counties were protected by new US tariffs, the extent to which they were hit by foreign retaliatory tariffs, and the degree to which they stood to benefit from agricultural subsidies extended under the 2018 Market Facilitation Program (MFP). Our analysis further accounts for two other key issues that were important in the 2018 campaign. Given the central role of health care policy during this midterm election, we control for the extent of local health insurance coverage potentially vulnerable to repeal of the Affordable Care Act (ACA). The cap on state and local tax (SALT) deductions, introduced in the 2017 Tax and Job Cuts Act, was also reported to be a source of voter displeasure against Republican candidates; we control for the average SALT burden to capture the traction this issue may have had with local taxpayers. Our regression model combines these key explanatory variables with a rich set of initial county demographic and economic covariates, as well as lagged changes in the Republican vote share from prior election cycles. This allows us to study how changes in voting patterns between 2016-2018 were related to exposure to the changes in tariffs, while controlling extensively for potential pre-trends.

We uncover a set of robust relationships between local employment exposure to the 2018 trade war and support for Republican House candidates. Republican candidates lost ground in counties that were adversely affected by retaliatory tariffs, but saw no discernable gains in counties where workers received more protection from new US tariffs. The negative relationship between retaliatory tariffs and Republican support was concentrated in politically competitive counties where Trump narrowly lost the popular vote in 2016. The large set of county variables included in our regression model

[1] Bown (2021) offers a comprehensive timeline.

[2] For example, a headline from the *Des Moines Register* (dated 25 September 2018) read: "Iowa farming's $2.2 billion trade loss could ripple through state's economy", while on the eve of the midterm elections, a Reuters headline (dated 1 November 2018) surmised: "Trump's trade war looms over divided U.S. farm belt ahead of vote".

helps to mitigate concerns that a county's exposure to the tariffs might reflect selection on the basis of observables. We demonstrate that conditional on controls, our measures of US and retaliatory tariff shocks are uncorrelated with further lags in Republican vote share shifts, while being balanced with respect to other auxiliary county characteristics that we have not already controlled for; this helps to assuage concerns related to common pre-trends that might be driving both contemporaneous voting patterns and the propensity for a county to be the target of tariff policy. We further report several diagnostics following Oster (2019) that provide reassurance on the extent to which selection on unobservables might affect the stability of our key finding, specifically that the retaliatory tariffs had a negative effect on Republican support in the 2018 midterms. We also verify that there was little short-term mobility response to the tariffs in terms of migration across county borders, to validate the local shock approach adopted.

Our empirical analysis yields several additional findings. The 2018 agricultural subsidies offset some of the loss in Republican vote share; however, this mitigating effect was only consequential in a small number of counties that both experienced the most exposure to the retaliatory tariffs and received the largest per-worker MFP disbursements.[3] Republican support also fell systematically in counties where recent increases in health insurance coverage had been greatest, and where the state and local tax burden was high, underscoring how these controversial health care and tax policy issues worked against Republican House candidates during the 2018 election.

In further exploration, we uncover suggestive evidence that some of the tariffs' effects on voting may have been transmitted through domestic production linkages, even while holding constant a county's direct exposure to the US and retaliatory tariffs. We find a positive effect on the Republican vote share if downstream industries received more Section 301 tariff protection against imports from China; as a hypothetical example, this would be in line with US protection for the auto parts industry increasing the demand for domestic steel and thus raising pro-Republican sentiment in counties with concentrations of steel workers. Republican support also appeared to be higher if upstream agricultural industries were exposed to more tariff retaliation from China, which in principle would have lowered costs for buyers of these agricultural inputs (e.g., sorghum feed for hog farmers).

Returning to the direct impact of the tariffs (rather than that transmitted through production linkages), we translate our regression estimates to quantitative implications for voting outcomes. We find that the trade war – specifically, exposure to retaliatory tariffs – can account for about one-fifth of the observed nationwide decline in the Republican vote share in House races between 2016 and 2018. We also map our regression results into counterfactual Congressional election outcomes. Mindful of the complex US electoral geography, we consider several alternatives to apportion estimated county-level vote changes to congressional districts (CDs). The calculations we perform indicate that voters' response to the trade war can account for a net loss of ten Republican House seats in

[3]This finding that voters who receive compensatory payments may be less likely to hold politicians accountable is consistent with Leight et al. (2020).

2018. In comparison, concerns over health care coverage and the SALT deduction limit may have cost the Republican party eight and fifteen House seats respectively. We confirm through Monte Carlo simulations that these point estimates for lost seats are significantly different from zero at the 95% confidence level. The Monte Carlo-based confidence intervals moreover indicate that all three forces – the trade war, health care, and SALT – were substantively and comparably important in contributing to the 2018 'Blue Wave', in which Republicans lost a total of 40 House seats. We then conclude our analysis with a brief discussion of findings related to the potential impact of the trade war on the subsequent 2020 US elections.

Our paper builds on earlier work studying how economic openness, particularly US-China trade, has impacted US domestic politics. Prior studies have examined the effect of import competition on voting in elections (Margalit, 2011; Jensen et al., 2016; Che et al., 2016; Choi et al., 2021), roll-call behavior (Feigenbaum and Hall, 2015), and political polarization (Autor et al., 2020).[4] Following the literature, we construct our measures of local tariff exposure by combining detailed information on product-level tariffs with data on counties' initial industry employment mix.

Our work is related to the mounting evidence on the consequences of the US-China trade war for the US economy. Several studies have uncovered weaker employment outcomes, particularly in US locations more exposed to retaliatory tariffs (Flaaen and Pierce, 2019; Benguria and Saffie, 2020; Goswami, 2020). Our approach is consistent with these papers that have highlighted potential producer-side exposure to the tariffs via the employment composition of US counties; we in turn document how this might have affected voting. At the same time, US consumers have borne the brunt of higher prices from the new US tariffs (Amiti et al., 2019; Cavallo et al., 2021; Flaaen et al., 2020; Waugh, 2019). To the extent that voters also responded politically to the consumer-side impact of tariffs, or even the broader rhetorical influence of the trade war (Mansfield and Mutz, 2009), these would be captured (to an extent) by state fixed effects in our empirical specifications. Our estimates may therefore constitute a lower bound for the overall political impact of the trade war. Several other studies have looked into the link between the trade war and the 2018 elections (Kong, 2020; Fetzer and Schwarz, 2021; Chyzh and Urbatsch, 2021; Kim and Margalit, 2021; Li et al., 2022).[5] Relative to these papers, we find evidence of stronger voting responses in politically competitive counties, while also demonstrating the influence of tariffs, agricultural subsidies, health insurance, and SALT for voting patterns in a common empirical model. Our approach moreover allows us to characterize the consequences for both the Republican vote share and the number of House seats lost.

The paper proceeds as follows. Section 2 describes the key data sources, and the construction of our county-level measures of exposure to the tariffs. Section 3 presents our empirical specification.

[4]More broadly, the impact of openness to trade on domestic electoral outcomes has been studied in other country contexts, including: Dauth et al. (2014) and Dippel et al. (2022) for Germany, Colantone and Stanig (2018) for the UK Brexit vote, and Ogeda et al. (2021) for Brazil.

[5]Chyzh and Urbatsch (2021) in particular find a systematic pattern of Republican electoral losses in counties that produce more soybeans.

Section 4 then reports the regression findings and counterfactual implications. An online appendix documents further details on the data and additional checks.

2 Data

2.1 Elections

We adopt US counties as the unit of analysis, this being the most disaggregated geographic unit for which voting and socioeconomic data are readily available. The voting data are from David Leip's US Election Atlas. We construct the 'two-party vote share' at the county level, defined as the number of Republican votes divided by the total votes cast for Republican and Democratic candidates, for each of the US House and Presidential elections since 2008.[6] Our sample comprises all US counties outside Alaska, which does not report county-level election returns. While the majority of counties – 2,717 out of 3,108 in our sample – are located within a single congressional district (CD), the remaining 391 counties are split across multiple CDs; we return to the implications of these 'split counties' later.

Panel A of Table 1 reports summary statistics on these voting outcomes. Across counties, Republican House candidates lost 6.4 percentage points of vote share on average between 2016 and 2018. These losses unwound the Republican gains from the 2014-2016 and 2012-2014 election cycles, of 3.5 and 2.3 percentage points respectively. These 2016-2018 vote share changes exhibited considerable variation across counties: Republican candidates lost over 22 percentage points in the bottom decile of counties but gained nearly 3 percentage points in the top decile.

We further group the counties into four quantiles according to voting outcomes in the 2016 Presidential election, to capture how competitive the electoral landscape was leading into the 2018 midterms. Specifically, we bin the counties according to whether Trump garnered less than 40%, 40-50%, 50-60%, or over 60% of the vote in 2016. Panel C of Table 1 provides summary statistics for each 'competitiveness bin'. Close to two-fifths of the total US population resides in counties in the middle two bins (i.e., the 40-50% and 50-60% bins), which are the most electorally competitive according to this measure. Notice that the average county population decreases across the bins, reflecting the well-known pattern of stronger Republican support in less densely-populated areas.[7]

2.2 The 2018 Tariff Shock

Our county-level tariff shock measures seek to capture voters' potential exposure to the 2018 trade war through the industry composition of local employment. We construct: (i) the *US Tariff Shock*,

[6] We exclude Senate elections from our analysis, since these take place on a six-year cycle that would lead to a non-representative panel across states in any given election year.

[7] Figure A.1 in the appendix illustrates that these pivotal counties are geographically spread out across the US, albeit with fewer such counties present in the central plain states.

Table 1: Cross-County Summary Statistics

	Mean	Std. Dev.	10th pct.	50th pct.	90th pct.
A: Voting outcomes					
Republican House Vote Share (2018)	0.629	0.191	0.376	0.661	0.835
Republican House Vote Share (2016)	0.692	0.221	0.404	0.712	1.000
Δ Republican House Vote Share ('18-'16)	-0.064	0.125	-0.224	-0.043	0.026
Δ Republican House Vote Share ('16-'14)	0.035	0.148	-0.078	0.015	0.219
Δ Republican House Vote Share ('14-'12)	0.023	0.137	-0.112	0.035	0.130
Δ Republican House Vote Share ('12-'10)	0.001	0.133	-0.109	-0.018	0.155
Republican Presidential Vote Share (2016)	0.667	0.161	0.435	0.701	0.845
Δ Republican Presidential Vote Share ('16-'12)	0.059	0.052	-0.004	0.055	0.128
B: Tariff shocks and other explanatory variables					
US Tariff Shock	0.226	0.383	0.012	0.109	0.522
... non-Section 301	0.068	0.269	0.000	0.003	0.161
... Section 301	0.158	0.227	0.011	0.090	0.356
... of which, levied on Agricultural	0.003	0.018	0.000	0.001	0.004
... of which, levied on non-Agricultural	0.155	0.226	0.008	0.087	0.354
Retaliatory Tariff Shock	0.194	0.195	0.039	0.139	0.400
... of which, levied by China	0.155	0.170	0.028	0.105	0.332
... of which, on Agricultural	0.098	0.152	0.004	0.046	0.250
... of which, on non-Agricultural	0.058	0.080	0.006	0.037	0.126
... of which, levied by Canada, EU, Mexico	0.038	0.064	0.004	0.021	0.088
... of which, on Agricultural	0.002	0.003	0.000	0.001	0.005
... of which, on non-Agricultural	0.036	0.064	0.003	0.019	0.085
Upstream US Tariff Shock	0.107	0.136	0.014	0.067	0.240
Downstream US Tariff Shock	0.099	0.192	0.013	0.056	0.204
Upstream Retaliatory Tariff Shock	0.075	0.081	0.015	0.051	0.156
Downstream Retaliatory Tariff Shock	0.070	0.079	0.015	0.051	0.137
Estimated Ag. Subsidy per worker (2018)	0.429	1.080	0.000	0.027	1.345
Health Insurance Share (2013-17 avg.)	0.889	0.051	0.823	0.897	0.945
Δ Health Insurance Share (2013-17 minus 2008-12)	0.040	0.031	0.008	0.038	0.076
State & Local Taxes, 4th quintile (2016)	1.873	0.236	1.563	1.851	2.212
State & Local Taxes, 5th quintile (2016)	3.994	2.227	2.464	3.259	6.209

C: Counties by electoral competitiveness

By Republican Vote Share (2016 Pres.)	Number of counties	Avg. pop. (2016)	Total pop. (2016)	US Tariff Shock	Retaliatory Tariff Shock	Ag. Subsidy per worker
$\mathbf{1}(\text{Pres. Vote} \in [0, 0.4])$	246	422,828	104,015,764	0.134	0.092	0.108
				(0.150)	(0.107)	(0.411)
$\mathbf{1}(\text{Pres. Vote} \in (0.4, 0.5])$	243	299,096	72,680,235	0.190	0.125	0.127
				(0.192)	(0.147)	(0.531)
$\mathbf{1}(\text{Pres. Vote} \in (0.5, 0.6])$	395	132,167	52,205,954	0.248	0.173	0.205
				(0.309)	(0.153)	(0.666)
$\mathbf{1}(\text{Pres. Vote} \in (0.6, 1])$	2,224	41,503	92,303,572	0.236	0.216	0.537
				(0.425)	(0.207)	(1.208)

Notes: Summary statistics across $N = 3,108$ counties, excluding Alaska. Voting outcomes in Panel A are from the Election Atlas; the Republican vote share is the number of votes for the Rep. candidate out of total votes cast for the Dem. and Rep. candidates. For Panel B, the US Tariff Shock, Retaliatory Tariff Shock, Agricultural Subsidy, and State & Local Tax measures are in units of $1,000 per worker. The share of the civilian non-institutionalized population with health insurance is from the American Community Survey (five-year average series). Panel C provides descriptive statistics on counties by electoral competitiveness bins, based on the two-party Republican vote share in the 2016 Presidential election. For each bin, we report the number of counties, mean population per county, total population across all counties, mean US Tariff Shock, mean Retaliatory Tariff Shock, and mean estimated Ag. subsidy per worker; standard deviations are in parentheses.

defined as a county's average per-worker exposure to the increase in US tariffs on imports; and (ii) the *Retaliatory Tariff Shock*, defined as the corresponding per-worker exposure to the retaliatory tariffs levied against US exports.

We briefly describe the construction of these two Tariff Shock variables here; a more detailed description can be found in the appendix (see Section A.1). We use the HS 8-digit product-level data collected by Bown (2021) for the information on tariff increases that had come into force by October 2018 (i.e., just prior to the midterm elections). The US Tariff Shock incorporates the tariff actions against washing machines and solar panels (Section 201), steel and aluminum (Section 232), and a broad swath of imports from China (Section 301); for the Section 301 tariffs, this comprises the tariffs implemented in July and August 2018 covering \$50 billion of US imports, and the tariffs implemented in September 2018 on an additional \$200 billion of US imports. The Retaliatory Tariff Shock consists of the responses by the US' four largest trade partners, Canada, Mexico, China, and the EU; together, these countries accounted for about three-fifths of the US' total goods exports and two-thirds of the US' total goods imports in 2017.

To construct each Tariff Shock variable, we multiply the percentage-point increase in the tariff rate by initial bilateral trade values, which we then concord to NAICS industries. This yields measures of the tariff change in dollar terms in NAICS industry i, for US imports from country o, $TS_i^{o,US}$, as well as for US exports to country d, $TS_i^{US,d}$.[8] We then map these industry-i tariff shocks to individual US counties, indexed by c, by apportioning the national-level shock according to each county's share of national employment in industry i, $L_{i,c}/L_i$. We draw on the 2016 US County Business Patterns (CBP) data – specifically, the version processed and cleaned by Eckert et al. (2020) – for this information on employment; as the CBP does not cover farm establishments, we supplement this with estimates for county-level employment in farm-based agricultural industries that we construct from the US Census of Agriculture.[9] The final step aggregates the tariff shocks experienced by each county across industries and trade partner countries, and then divides this by total county population between ages

[8] As a baseline, we concord the HS 8-digit product-level tariff shocks to NAICS 3-digit industries for the non-farm agricultural sector (i.e., excluding NAICS 111 and 112). Our results are robust if we instead concord the product-level tariff shocks to more disaggregated 4- or 6-digit NAICS industries (see Table A.10 in the appendix).

[9] We use the 2012 and 2017 US Census of Agriculture to construct estimates of employment by county in thirteen farm-based agricultural industries that fall under NAICS codes 111 ("Crop production") and 112 ("Animal production and aquaculture"). We then linearly interpolate between 2012 and 2017, to obtain employment estimates for 2016. Please see Section A.1 in the appendix for the full list of these thirteen industries, which are roughly at the NAICS 4- or 5-digit level of disaggregation. This is more detailed than the agricultural employment data used in Kong (2020), who instead uses an aggregate for the entire farm-agriculture sector, which likely masks heterogeneity in tariff treatment across agricultural products. In their analysis, Li et al. (2022) appear to use the CBP exclusively as their source of employment data, and so are likely not incorporating information on farm employment.

15-64, $\bar{L}_c$. This yields our US and Retaliatory Tariff Shock measures, in dollar-per-worker terms:

$$TS_c^{US} = \sum_o \sum_i \frac{L_{i,c}}{L_i} \frac{TS_i^{o,US}}{\bar{L}_c}, \text{ and} \quad (1)$$

$$TS_c^{R} = \sum_d \sum_i \frac{L_{i,c}}{L_i} \frac{TS_i^{US,d}}{\bar{L}_c}. \quad (2)$$

The above measures capture counties' exposure to the tariff war through local employment in industries that are directly hit by the tariffs. To the extent that local labor market outcomes were among voters' relevant concerns in the 2018 election, we would expect a decline in the Republican vote share in counties more exposed to the Retaliatory Tariff Shock (and its adverse impact on workers), all else equal. Conversely, to the extent that new US tariffs protected American workers from foreign competition, as was the Trump administration's stated intention, we would expect Republican voter support to be positively correlated with the US Tariff Shock.[10] The approach in (1) and (2) – in which industry-level shocks in dollar-per-worker terms are apportioned to locations – can be justified in standard trade models; for example, in a multi-sector environment in which each tradable sector is monopolistically competitive and there are external trade imbalances, Autor et al. (2013) show how changes in local employment outcomes can be expressed as a function of trade or tariff shocks written in dollar-per-worker terms analogous to (1) and (2). If voters in turn respond to changes in local employment conditions, this then rationalizes our empirical approach of regressing changes in the Republican vote share against these tariff shock variables. Moreover, an advantage of TS_c^{US} and TS_c^R as constructed is that each can be decomposed additively into shocks attributable to different partner countries or products. That said, another common approach in the literature is to express a location's exposure to tariffs as a weighted-average of industry-level tariff rate changes, such as in Kovak (2013) and Dix-Carneiro and Kovak (2017). We will show later, specifically in Table A.9 in the appendix, that our key findings are robust when using tariff shock variables constructed in this alternative manner.

On a separate note, the empirical exercise here will be valid to the extent that the effects of the tariff shocks on voting outcomes were indeed localized, a premise that can be called into question if there was significant mobility across county borders. In our setting, we would argue that this is not likely to be a major concern given the short time frame between the onset of the tariffs (February 2018) and the midterm elections (November 2018), within which decisions to relocate and (where local laws require it) changes in voter registration would have had to be made in order to have a bearing on the election results.[11] More concretely, we will verify that the US and Retaliatory Tariff

[10]This would be in line with a body of empirical work that has found voters' economic self-interest to be relevant in shaping their preferences over trade policy (see for example, Scheve and Slaughter, 2001; Mayda and Rodrik, 2005; Fordham and Kleinberg, 2012).

[11]There was moreover a degree of uncertainty in the initial months of the trade war over how permanent the tariffs would be, which would in principle have delayed actual movements of individuals or households across counties, since

Shocks are indeed uncorrelated with contemporaneous measures of cross-county mobility drawn from the US Census Bureau's current population estimates, once pre-trends in these mobility variables are accounted for (see Section 3).

In addition to the effects of direct exposure of each county to the tariffs, we will later explore the potential impact of exposure that occurs indirectly, through tariff shocks to upstream or downstream industries that are then transmitted to the county's labor market via these production linkages (see Section 4.3). There are in principle further considerations that could offset these effects based on local labor market exposure. For example, higher US tariffs raise goods prices for consumers. On the other hand, voters may be willing to bear with the cost of retaliatory tariffs if they believe the trade war will eventually give the US leverage to improve market access or intellectual property protection; or Republican supporters may turn out to vote in larger numbers out of concern that the retaliatory tariffs could harm the electoral performance of the incumbent party. Such forces, if pertinent to voters, would generally bias the estimated political effects of TS_c^{US} and TS_c^R towards zero.

Table 1, Panel B, reports summary statistics for the US and Retaliatory Tariff Shocks across counties. On average, the county-level producer-side exposure to the US tariffs was \$226 per worker, slightly higher than the retaliatory tariff exposure at \$194 per worker. Table 1 confirms that the bulk of US import protection was rendered by the Section 301 tariffs on China, which mostly covered non-agricultural goods (specifically, manufacturing products). The retaliatory tariffs were more evenly balanced between agricultural and non-agricultural products; in particular, retaliation on agricultural products came primarily from China. (Recall that TS_c^{US} and TS_c^R can each be decomposed additively into component shocks by product and by partner country.)

Note further that the Retaliatory Tariff Shock is increasing across the political competitiveness bins, as ordered by the 2016 Republican Presidential vote share (Table 1, Panel C). On the other hand, the US Tariff Shock exhibits a non-monotonic relationship with 2016 voting patterns that peaks in the 50-60% bin. These broad patterns are also documented in Fajgelbaum et al. (2020) and Fetzer and Schwarz (2021), who use different constructions of county-level exposure to the tariffs.[12] The focus of our paper is not to explain these county-level tariff shocks, however, but to evaluate their consequences for voting outcomes in the 2018 House elections.

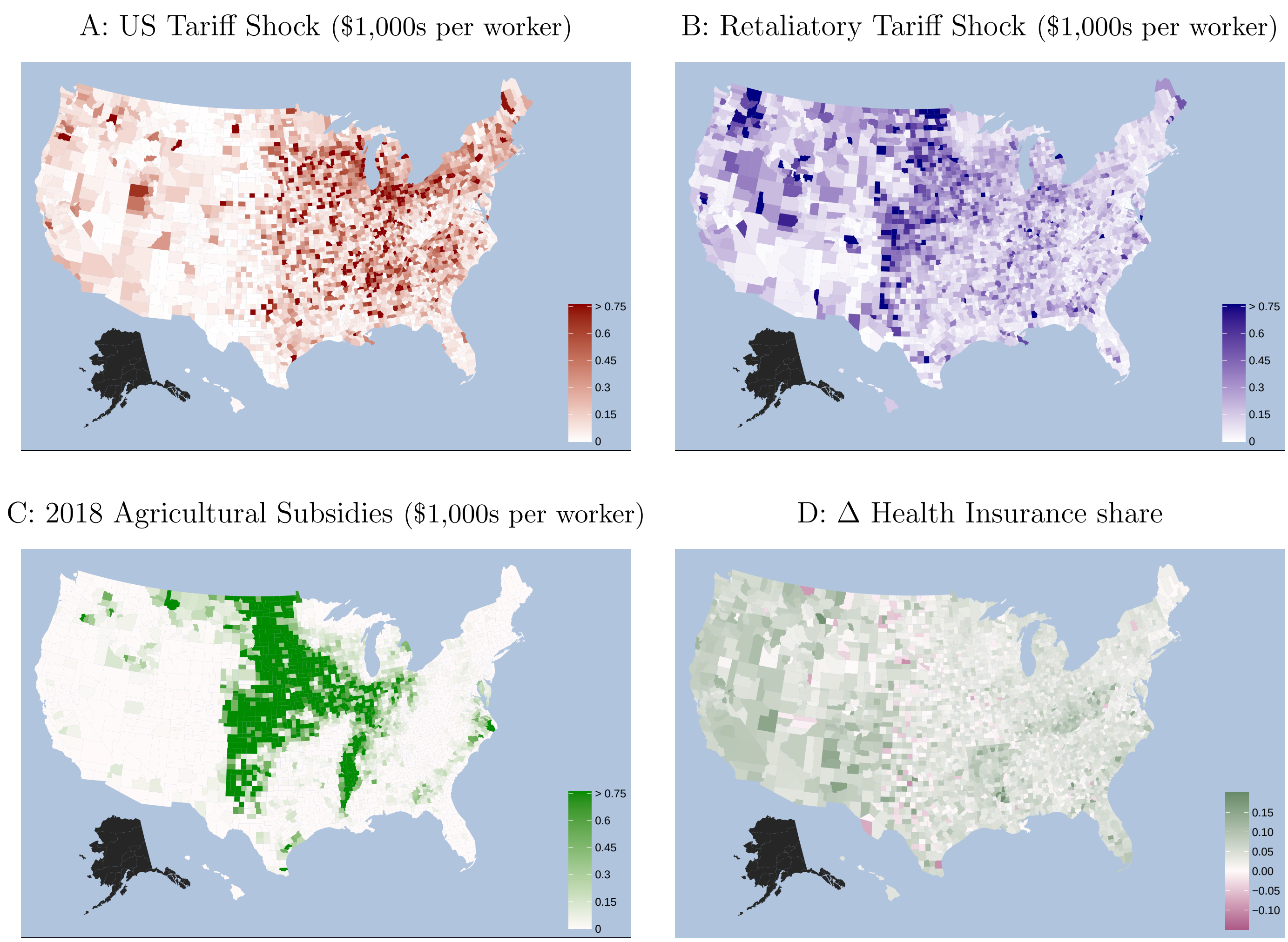
Figure 1 maps the US Tariff Shock (Panel A) and the Retaliatory Tariff Shock (Panel B) across counties. These shocks do not overlap neatly, though they are positively correlated (correlation

relocating often incurs sunk costs.

[12]The exposure measures in these two papers can be interpreted as county-level weighted-average tariff rates, whereas our TS_c^{US} and TS_c^R variables express the tariff impact in dollar-per-worker terms. As Figure A.2 in the appendix shows, we recover an inverted U-shaped relationship between our TS_c^{US} measure and the county-level Trump vote share in 2016, similar to Fajgelbaum et al. (2020). Likewise, the relationship between TS_c^R and the county-level Trump vote share is upward-sloping; this is largely due to the fact that agricultural commodities are a substantial share of China's imports from the US (close to 15% in 2017) and thus featured prominently as targets for tariff retaliation, while the rural counties where US agriculture is located tend both to have a high share of the local workforce in this sector and to vote strongly Republican. This relationship with the 2016 Trump vote share is less sharply monotonic when one considers the component of TS_c^R associated with non-agricultural products (available on request).

coefficient: 0.40).[13] Both shocks exhibit considerable geographic variation across the US, with a right tail of counties experiencing either a disproportionate amount of US tariff protection or costly tariff retaliation.

Figure 1: Tariff Shocks, Agricultural Subsidies, and Health Insurance Coverage by County



2.3 Agricultural Subsidies

In summer 2018, the US government announced a Market Facilitation Program (MFP) to cushion US farmers from the adverse effects of the retaliatory tariffs newly imposed by the US' trade partners. Administered by the US Department of Agriculture, the program consisted of roughly $12 billion in subsidies to be disbursed to farm owners. These large-scale subsidies were deployed within a matter of months, a promptness that speaks to the high level of concern within the Trump administration over how much China's retaliatory tariffs would hurt the US agricultural sector.

[13]Table A.2 in the appendix reports in more detail the pairwise correlation between the US and Retaliatory Tariff Shocks when these are broken down by trade partner country and by sector.

To construct estimates of the total MFP subsidies received in 2018 at the county level, we combine the announced subsidy rates for key commodities – namely, soybeans, hogs, cotton, sorghum, milk, wheat, and corn – with information on production or inventory from preceding years. (See Section A.1 in the appendix for further details.) This allows us to compute an estimate of the total subsidy received by each county c, which we then divide by the county's working-age population, $\bar{L}_c$, to obtain the variable $AgSubs_c$. As a working hypothesis, one might expect that a larger quantum of subsidies per worker extended by the Trump administration would better mitigate the impact of the retaliatory tariffs on agricultural workers, and thus shore up support for Republican House candidates. One might further hypothesize that this effect could be stronger in counties that saw a more severe Retaliatory Tariff Shock, as this could have raised the political salience of the tariff war for local voters.

The MFP subsidies were narrowly distributed. Across counties, the median per-worker subsidy was only \$27, even though the mean value was \$429 (Table 1, Panel B). The largest beneficiaries were rural counties that exhibited strong levels of Republican support in the 2016 Presidential election (Panel C). The limited geographic scope of the program is also evident from Panel C of Figure 1, with the main recipient counties located in the plains and central states. The cross-county correlation between the MFP subsidy per worker and the Retaliatory Tariff Shock is 0.49. Despite this positive correlation, it is useful to bear in mind that more products were targeted by tariff retaliation than the Trump administration made eligible for subsidies. Even within agriculture, the tariff retaliation had a broader reach, since the MFP omitted most fruits, nuts and fishery products.

2.4 Health Care

The potential overhaul of US federal health care policy was a central issue in many Congressional campaigns in 2018 (e.g., Lowrey, 2018). In early 2017, the Republican House leadership began to introduce controversial legislation that would have repealed the Affordable Care Act (ACA), or 'Obamacare'. Although the efforts were ultimately thwarted in the Senate by the late John McCain's deciding vote in July 2018, health care remained a galvanizing campaign issue in November 2018. Preserving access to health insurance was particularly important for Democratic-leaning voters according to survey data (Blendon et al., 2018), while health care policy dominated Democratic campaign advertising in October 2018 (Wesleyan Institute for Advertising Research, 2018).

We thus include two county-level variables from the American Community Survey (ACS) in our analysis: the share of the population with health insurance just prior to the 2018 elections, and the change in the share with health insurance in the years since the ACA was passed in 2010. We use specifically the ACS average between 2013-2017 (to reduce potential noise in yearly reporting) for the former, and the difference between the 2013-2017 and the 2008-2012 five-year averages for the latter. The first variable accommodates the possibility that the initial rate of health care coverage could have affected how important the preservation of the ACA was perceived to be at the county level. The second variable serves as a proxy for the share of the population whose health insurance coverage might be vulnerable had the ACA been repealed; following Hollingsworth et al. (2019), we expect greater gains in health insurance coverage to be negatively correlated with support for Republican House candidates in 2018.

County-level health insurance rates rose on average by about 4 percentage points in the years after the ACA was enacted (Table 1, Panel B).[14] The gain was below 1 percentage point for the 10th percentile county, but nearly 8 percentage points at the 90th percentile. Panel D of Figure 1 confirms that the increases in health insurance coverage were spread across the US; in comparison, the tariff shocks and (especially) the 2018 MFP subsidies were more narrow in geographic scope (Panels A-C).

2.5 Other Variables

State and Local Taxes: Another policy issue that weighed on Republican candidates' performance in the 2018 midterms concerned state and local taxes (SALT). The Trump administration's 2017 Tax Cuts and Jobs Act introduced a cap of $10,000 per household on SALT deductions that could be claimed on federal tax returns. This cap was reportedly unpopular among segments of voters, particularly high-income earners in high-tax locations; it has been argued that this can explain some of the Republican losses in districts with concentrations of such voters (Tankersley and Casselman, 2018). We draw on county-level tax statistics released by the US Internal Revenue Service, to compute the mean SALT amounts per tax return filed, in order to account for this election issue explicitly.

Other Controls: We include a broad set of county-level demographic and socioeconomic covariates, guided by the considerable empirical literature on determinants of US election outcomes.[15] To control for demographics, we include population shares by age group (25-34, 35-44, 45-54, 55-64, 65 and over), by gender, and by race (black, white non-Hispanic, Hispanic), as well as the share in urban areas, from the US Census. To control for differences in the composition of economic activity across counties, we include employment shares by sector (agriculture, mining, and manufacturing, with services as the omitted category), computed from the US County Business Patterns (c.f., Eckert et al., 2020) and the US Census of Agriculture.[16] We also include the unemployment rate, (log) mean household income, and population shares by educational attainment (for less than high school, and for some college and above), from the American Community Survey (ACS). For each of these variables, we include both pre-election levels and changes as controls. In particular, for the demographic

[14]This figure is comparable to the CBO (2017) estimate for the number of Americans – 17 million – that would no longer have held health insurance in 2018 had the repeal legislation passed.

[15]Flanigan et al. (2018) and Sabato and Kondik (2019) provide detailed treatments focusing on the 2018 midterms. Shafer and Wagner (2018) argue that the fundamental drivers of US voting patterns were largely unchanged in the 2018 election.

[16]We obtain very similar findings when these sectoral employment variables are expressed as a share of the working-age population (between 15-64), rather than as a share of total sectoral employment (see Column 3, Table A.7).

and sectoral employment variables, we control for their 2016 values for pre-election levels, and for the difference between their 2016 and 2013 values for pre-trends.[17] For variables drawn from the ACS, we control for the 2013-2017 average for initial levels, and for the difference between the five-year averages in 2013-2017 and 2008-2012 to account for pre-trends. The construction of these variables is detailed in the appendix (see Section A.1), with summary statistics reported in Table A.1.

3 Empirical Model

Our baseline regression specification is:

$$\begin{aligned}\Delta RHVoteSh_c^{18,16} \;&=\; \beta_1 TS_c^{US} + \beta_2 TS_c^{R} + \alpha_1 AgSubs_c \times TS_c^{R} + \alpha_2 AgSubs_c \\ &\quad + \eta HInsur_c + \lambda SALT_c + \rho R_c + \sum_{b=2}^{4} \gamma^b \mathbf{1}(c \in B^b) + \Gamma X_c + D_s + \epsilon_c. \quad (3)\end{aligned}$$

The dependent variable $\Delta RHVoteSh_c^{18,16}$ is the 2018 Republican House vote share in county c minus the corresponding share in 2016; this reflects the shift in support experienced by Republican candidates between these two House elections.

Our main explanatory variables are TS_c^{US} and TS_c^{R}, the measures of county-level exposure to the US and Retaliatory Tariff Shocks defined in (1) and (2) respectively. $AgSubs_c$ is the estimated county-level agricultural subsidy per worker received under the 2018 Market Facilitation Program; we also include the interaction between $AgSubs_c$ and TS_c^{R} to examine whether the subsidies may have had a bigger effect in counties that experienced a larger Retaliatory Tariff Shock. $HInsur_c$ is a vector that comprises the average health insurance coverage share in 2013-2017, and the change in this local coverage share since the passage of the ACA (relative to 2008-2012). $SALT_c$ is a set of dummy variables to capture the potential traction of the state and local tax deduction limit as a voter concern; to flexibly account for the high-tax locations where these deductions would have mattered more, we include indicators for the 4th and 5th county quintiles of SALT amounts per tax return in 2016, as well as for the 4th and 5th county quintiles of the change in SALT per tax return (relative to 2013).

Our regression model incorporates an extensive set of controls, including state fixed effects (D_s). These absorb any voting pattern differences arising from Senate or Gubernatorial races (or ballot initiatives) that may have spilled over to the House races. Equation (3) thus estimates the relationship between the 2018 tariff shocks and voting outcomes using within-state, cross-county variation.

There is a natural concern that stands in the way of a causal interpretation of the estimated tariff shock coefficients, β_1 and β_2 in (3): The extent to which a county receives US tariff protection

[17] The only exception is the population share in urban areas: We control only for the level of this variable in 2010, as data by county are not available for prior years.

or is hit by foreign retaliatory tariffs is likely to be shaped in part by underlying socioeconomic or political forces, which might themselves be correlated with shifts in voter preferences. For example, the patterns documented in Fajgelbaum et al. (2020) suggest that the Trump administration may have targeted US tariffs to counties in which the Trump vote share in 2016 was close to 50% to try to gain support in electorally competitive locations. On the other hand, foreign countries may have levied tariffs on agricultural goods, to try to dent support for the incumbent president's party in rural, farming-intensive districts (Fetzer and Schwarz, 2021). This is precisely why we augment the right-hand side of the regression with a comprehensive set of county-level control variables, to soak up forces that could be the basis for selection on observables (i.e., forces that could influence the magnitude of the US or Retaliatory Tariff Shocks).

Among these controls, the vector R_c comprises variables that directly seek to capture pre-trends in Republican support. We include here the lagged change in the Republican vote share for the three preceding House election cycles ($\Delta RHVoteSh_c^{16,14}$, $\Delta RHVoteSh_c^{14,12}$, and $\Delta RHVoteSh_c^{12,10}$). We also include the change in the Republican Presidential vote share between 2016 and 2012 ($RPVoteSh_c^{16,12}$), to control for the 2016 county-level swing in support towards Trump. Separately, the $\mathbf{1}(c \in B^b)$'s are a set of dummy variables for the competitiveness bins; specifically, B^b denotes the set of counties where the 2016 Trump vote share was 40-50%, 50-60%, and 60-100% for $b = 2, 3, 4$ respectively (the omitted category is the 0-40% bin). Our estimates therefore leverage variation across counties with a similar degree of competitiveness, as adjudged by how close the Trump vote share was in 2016. X_c is a vector of county-level initial characteristics – covering demographics, employment shares by sector, and economic conditions – and their pre-trends, as listed in Section 2.5. These variables help to control for any propensity to target tariffs towards voters of particular age or racial groups, or towards locations with particular concentrations of workers by sector.[18] Last but not least, X_c also includes: (i) a set of four dummy variables that equal 1 if the county was uncontested by either the Republican or Democratic party in 2016 or 2018, but contested in the other year (to capture what would otherwise show up as a large swing in vote share); and (ii) a dummy variable for counties that are split across multiple CDs.

Conditional on these controls, we posit that the residual variation in the Tariff Shock variables is no longer picking up underlying forces that could drive shifts in electoral support for the Republican party.[19] We perform several checks to address concerns on this front. First, we will show that conditional on the set of observables, the US and Retaliatory Tariff Shocks are uncorrelated with pre-trends in shifts in Republican support prior to the lagged periods that we already explicitly

[18] Recall that we control for the county-level employment shares in the agriculture, mining, and manufacturing sectors. Trends in the Republican vote share that are associated with the initial employment share in services – or the 'incomplete share' in the parlance of Borusyak et al. (2022) – are implicitly controlled for, since the shares together with this residual category sum to 1.

[19] More formally, this requires that $E[TS_c^{US}\epsilon_c|\mathcal{W}] = 0$ and $E[TS_c^{R}\epsilon_c|\mathcal{W}] = 0$, after conditioning on the set of observables $\mathcal{W} = \{AgSubs_c, HInsur_c, SALT_c, R_c, \mathbf{1}(c \in B^b), X_c, D_s\}$.

include on the right-hand side of (3). Next, we take guidance from the recent literature on shift-share empirical strategies and posit that identification stems in our context from the exogeneity of the shifters, conditional on the lagged variables and pre-trends that we control for in (3). Toward this end, we conduct a balance test in the spirit of Borusyak et al. (2022) to show that the US and Retaliatory tariff shifters are uncorrelated with exposure-weighted averages of various potentially relevant initial county characteristics that have not already been controlled for in the baseline regression, after all variables have been appropriately recast to the industry level (see Section A.3 in the appendix for formal details).[20] The results from this balance test are reported in the lower panel of Table A.3; we confirm in particular that conditional on the observables in (3), the US and Retaliatory tariff shocks are unrelated to longer pre-trends in voting patterns and in the sectoral employment shares, nor with trends that could be associated with county-level manufacturing wages.

While the inclusion of the large set of controls in (3) is useful, one cannot entirely rule out the possibility of omitted unobserved determinants of the county-level tariff shocks. To allay this concern, we will present diagnostics to assess the extent to which the estimated tariff shock coefficients are stable under the threat of selection on the basis of unobservables (c.f., Altonji et al., 2005; Oster, 2019).[21]

As discussed earlier, another key premise of the shift-share approach is that the counties can be viewed as independent spatial units with limited cross-county voter mobility. We provide more formal assurance on this front in Table A.4 in the appendix, by showing that the US and Retaliatory Tariff Shocks are unrelated to various measures of mobility over our period of interest, namely: the 2016-2019 change in county population, and the county-level net domestic migration rate in 2019 (for people movements relative to 2018), after controlling for pre-trends in these respective mobility variables (see Section A.3 for more details).[22]

Finally, note that we run our regressions in (3) weighting observations by county population (in 2016) to avoid systematically over-representing rural voters. We also cluster standard errors two-ways by state and by commuting zone to allow for correlated shocks in the ϵ_c residuals along these dimensions. We exclude from the analysis counties where the same party won the House race uncontested in both 2016 and 2018.

[20] This follows the approach in Borusyak et al. (2022), rather than that in Goldsmith-Pinkham et al. (2020) who base their identifying assumption on the plausible exogeneity of the initial share weights instead. Panel A of Table A.3 in the appendix reports summary statistics of the US and Retaliatory tariff shocks that have been recast to NAICS 3-digit industries in dollar-per-worker terms (i.e., the industry-level shifters).

[21] A separate conceptual issue is that the impact of a tariff on welfare in principle varies with the underlying market structure (e.g., perfect versus imperfect competition). In practice, the degree of market competition is likely to differ across industries and counties, and this could generate heterogeneity across counties in voters' responsiveness to the tariff shocks. The β_1 and β_2 tariff shock coefficients in (3) should thus be seen as effects that average across such potential heterogeneity in market structure (which we are unable to observe directly).

[22] The residualized binned scatterplots in Figure A.3 in the appendix further confirm that the US and Retaliatory Tariff Shocks are not associated in a significant way with cross-county movements of people. In the lower panel of Table A.4, we verify that our core findings on the effects of the tariffs on the change in the Republican House vote share are stable to including the lagged mobility measures among the X_c controls on the right-hand side of (3).

4 Results

4.1 Baseline Findings

Table 2 presents the main results from ordinary-least-squares estimation of (3). To limit table length, we report only the key coefficients of interest here; Table A.5 in the appendix reports the full set of coefficients for control variables and pre-trends. Column 1 reports a pared-down version of the estimating equation in which we exclude all terms related to agricultural subsidies (both $AgSubs_c$ and its interaction) and to health insurance ($HInsur_c$). Column 2 then introduces $HInsur_c$ to the right-hand side. Column 3 adds the $AgSubs_c$ variable in levels, while Column 4 is the full specification which includes the interaction term between $AgSubs_c$ and the Retaliatory Tariff Shock.

Across these four columns, the estimates indicate that Republican candidates lost vote share in the 2018 House election (relative to 2016) in counties where workers faced greater exposure to retaliatory tariffs. The coefficient of TS_c^R is moreover stable when the variables related to health insurance coverage and agricultural subsidies are introduced. Taking the Column 3 coefficient as a point of reference, a one standard deviation increase in exposure to retaliatory tariffs (0.195, from Table 1) is associated with a $0.058 \times 0.195 \approx 1.1$ percentage point loss in vote share; to put this in context, the mean cross-county drop in voter support for Republican House candidates was 6.4 percentage points. At the same time, while the coefficient of the US Tariff Shock variable exhibits the expected positive sign (consistent with these tariffs prompting a mild increase in Republican support), this effect is neither large nor statistically significant.

The health care variables are also systematically related to the observed shifts in voting patterns. The coefficient on the initial (2013-2017 average) share of health insurance coverage is positive (though not significant), suggesting that counties with greater coverage were more likely to support Republican candidates. Holding the level of coverage in 2013-2017 constant however, Republicans lost vote share in counties that saw larger increases in health insurance coverage following the passage of the ACA (relative to 2008-2012). As a gauge of the size of this effect, a one standard deviation greater increase in the share insured (0.031, from Table 1) is associated with a $0.189 \times 0.031 \approx 0.6$ percentage-point loss in Republican vote share. One plausible interpretation is that Republican support fell in response to the party's attempts to eliminate the ACA, which had contributed to the recent expansion in health insurance coverage. Separately, we find a significant erosion in the Republican vote share in counties with a high state and local tax burden. The swing against Republican House candidates was 1.8 and 2.5 percentage points respectively in the top 4th and 5th SALT quintiles, in line with contemporaneous reporting that portrayed the unpopularity of the SALT deduction cap among high-taxpaying voters. Importantly, while health insurance and the SALT deduction cap were policy issues that eroded the Republican vote share, including these in the regression model does not reduce the estimated impact of the Retaliatory Tariff Shock.

Table 2: Tariff Retaliation and Voting Patterns in the 2018 House Elections

Dep. Variable: Δ Republican Vote Share	House '18-'16				**Pre-trend checks** House '10-'08	**Pre-trend checks** President '12-'08
	(1)	(2)	(3)	(4)	(5)	(6)
US Tariff Shock	0.012	0.012	0.012	0.012	0.008	0.002
	[0.010]	[0.010]	[0.010]	[0.010]	[0.017]	[0.002]
Retaliatory Tariff Shock	-0.062***	-0.061***	-0.058***	-0.065***	-0.043	0.004
	[0.020]	[0.021]	[0.019]	[0.022]	[0.044]	[0.005]
Retaliatory Tariff Shock × Ag. Subsidy				0.019*	-0.009	-0.005
				[0.011]	[0.019]	[0.003]
Ag. Subsidy			-0.003	-0.012	-0.001	0.004
			[0.006]	[0.009]	[0.016]	[0.003]
Health Insurance Share (2013-17 avg.)		0.091	0.092	0.093	0.141	0.014
		[0.113]	[0.112]	[0.112]	[0.162]	[0.042]
Δ Health Insurance Share (2013-17 minus 2008-12)		-0.189**	-0.189**	-0.189**	0.159	0.002
		[0.092]	[0.092]	[0.092]	[0.192]	[0.028]
$\mathbf{1}$(SALT (2016) $\in$ 4th Quintile)	-0.019**	-0.018**	-0.018**	-0.018**	-0.004	-0.001
	[0.009]	[0.009]	[0.009]	[0.009]	[0.012]	[0.002]
$\mathbf{1}$(SALT (2016) $\in$ 5th Quintile)	-0.025**	-0.025**	-0.025**	-0.025**	-0.025	-0.004
	[0.012]	[0.012]	[0.012]	[0.012]	[0.016]	[0.003]
Lag Δ Rep. House Vote Share ('16-'14)	-0.597***	-0.595***	-0.595***	-0.595***	-0.165**	0.002
	[0.091]	[0.092]	[0.092]	[0.092]	[0.077]	[0.006]
Lag Δ Rep. House Vote Share ('14-'12)	-0.378***	-0.377***	-0.377***	-0.377***	-0.154*	0.012*
	[0.057]	[0.058]	[0.058]	[0.058]	[0.085]	[0.007]
Lag Δ Rep. House Vote Share ('12-'10)	-0.201***	-0.200***	-0.201***	-0.201***	-0.301***	-0.001
	[0.040]	[0.041]	[0.041]	[0.041]	[0.065]	[0.006]
Lag Δ Rep. Pres. Vote Share ('16-'12)	0.720***	0.704***	0.707***	0.709***	0.367*	-0.089**
	[0.112]	[0.111]	[0.113]	[0.113]	[0.186]	[0.035]
2016 Bins: $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y	Y
Observations	3,072	3,072	3,072	3,072	3,018	3,072
R^2	0.717	0.718	0.718	0.718	0.315	0.797
Oster (2019) test statistics:						
US Tariff Shock, β^*	0.032	0.030	0.030	0.030		
US Tariff Shock, δ	-1.118	-1.256	-1.290	-1.282		
Retaliatory Tariff Shock, β^*	-0.079	-0.080	-0.082	-0.169		
Retaliatory Tariff Shock, δ	8.278	11.451	2.540	2.871		
Retaliatory Tariff Shock × Ag. Subsidy, β^*				1.152		
Retaliatory Tariff Shock × Ag. Subsidy, δ				-0.986		

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in both 2016 and 2018. All columns control for: (i) county age-bin, gender, and race shares in 2016 (from the US Census), as well as pre-trends between 2013-2016; (ii) county urban population share in 2010 (from the US Census); (iii) county employment shares in agriculture, mining, and manufacturing in 2016 (from the Census of Agriculture and County Business Patterns), as well as pre-trends between 2013-2016; (iv) the county unemployment rate, log mean household income, share with less than high school, and share with some college education (or above) in 2013-2017 (from the American Community Survey), as well as pre-trends between 2008-2012 and 2013-2017; (v) indicator variables for the 4th and 5th quintiles of state and local taxes (SALT) per return in 2016, as well as indicators for the 4th and 5th quintiles of changes in SALT per return between 2013-2016; (vi) four indicator variables for counties contested by only one party in 2016 or 2018, but not both years; and (vii) an indicator for counties that are split across multiple congressional districts. The Oster (2019) β^* reported is the implied coefficient under the assumption of proportional selection on both observables and unobservables with equal weight ($\delta = 1$). The δ reported is the relative importance of selection on unobservables relative to observables that would lead to an implied coefficient point estimate of 0. Both β^* and δ are calculated assuming a maximum R^2 of 1, and that the lagged changes in vote shares and the 2016 presidential vote share bins are the controls with an unobserved component.

Agricultural subsidies appear to have played a more subtle role. In Columns 3 and 4, the $AgSub_c$ variable exhibits no significant relationship with voting patterns on its own. That said, the positive coefficient (0.019) on the interaction term indicates that the subsidy mitigated Republicans' electoral losses in counties more exposed to tariff retaliation. The point estimates indicate that the MFP would have fully offset the negative effect of the Retaliatory Tariff Shock for Republican candidates in counties that received subsidy amounts above $(0.0649/0.0192) \times \$1000 \approx \$3,380$ per worker. Note though that there were only 83 such counties, and these accounted for less than 0.1% of the total US population in 2016.[23]

Turning to the other variables reported in Columns 1-4, the negative coefficients on the Republican House vote share changes in the 2014-2016, 2012-2014, and 2010-2012 cycles suggest mean reversion: Republicans lost ground in counties where they had recorded gains in the prior three House elections. Of note, after conditioning on these pre-trends in House vote shares, there appears to have been a positive carry-over effect from the Presidential vote swing towards Trump in 2016 (relative to 2012) on the 2018 Republican performance in the House races. Several other county-level controls also exhibit familiar relationships with voting outcomes, as we report in full in Table A.5 in the appendix: Republican candidates continued to fare better in counties with older voters (specifically, aged 65 and up), with a higher employment share in the mining sector, with a lower urban population share, and with a higher mean household income.

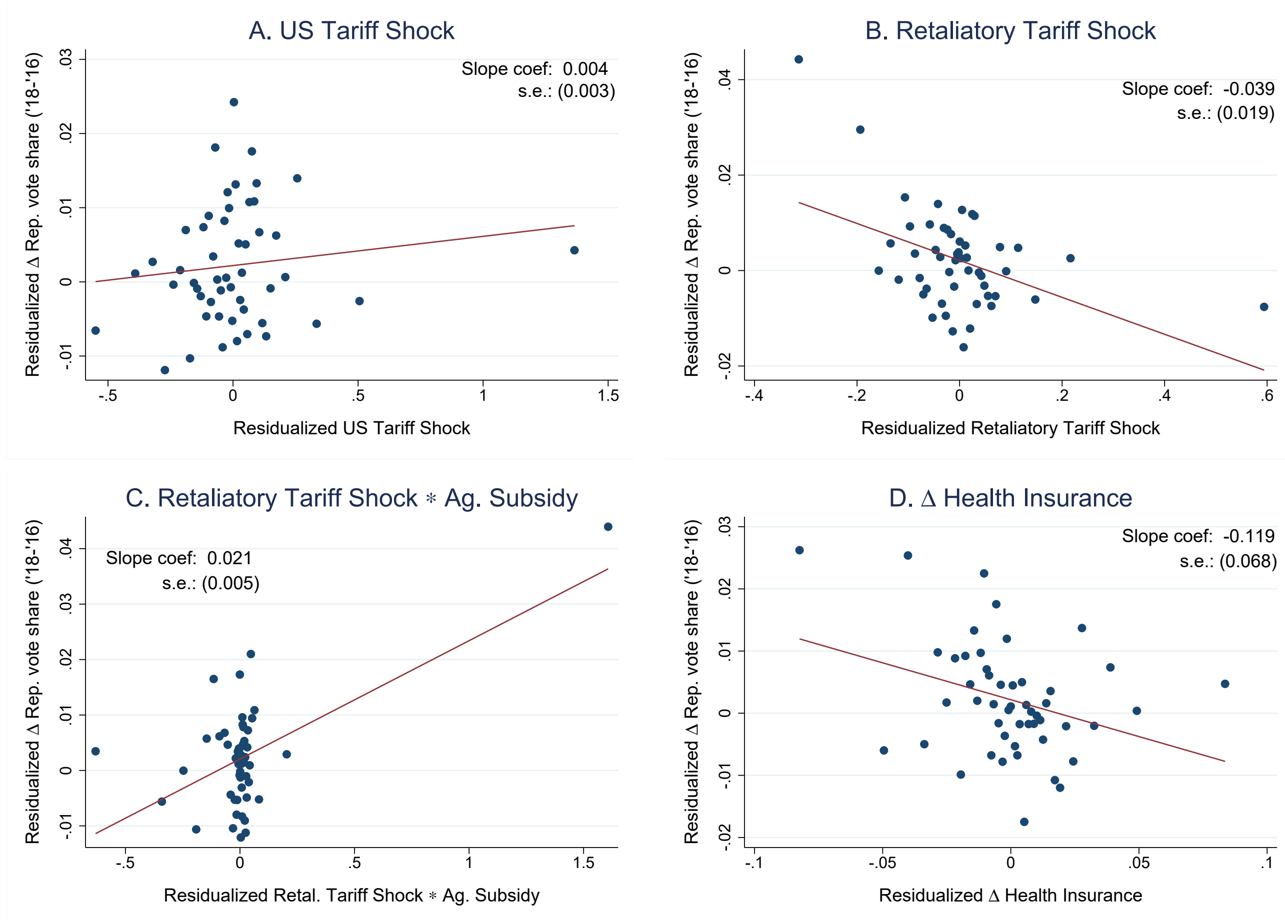
Figure 2 illustrates binned scatterplots of the key relations we have estimated, between the change in the Republican vote share and four explanatory variables: the US Tariff Shock, the Retaliatory Tariff Shock, the MFP subsidy per worker interacted with the Retaliatory Tariff Shock, and the change in health insurance coverage.[24] We observe a mild positive correlation with the US Tariff Shock, albeit one that is not statistically significant (Panel A). On the other hand, we obtain distinct downward-sloping relationships with the Retaliatory Tariff Shock (Panel B) and prior increases in health insurance coverage (Panel D), indicating the broad relevance of these variables for explaining the decline in Republican support. There are some grounds for caution in interpreting the effect of the agricultural subsidy interaction term, given that the topmost bin of counties – those most severely hit by retaliatory tariffs and that also received the most in subsidies – appears to be driving the positive slope in Panel C. Reassuringly though, the overall effect of the Retaliatory Tariff Shock that can be inferred from Column 4 of Table 2 – for example, when this is evaluated at the mean value of $AgSubs_c$ – remains negative and comparable in magnitude to the previous columns.[25]

[23]We have also run the Column 4 specification using MFP subsidies per worker restricted to each commodity in turn. This exercise indicates that the positive interaction effect with the Retaliatory Tariff Shock is driven by the subsidies to soybeans, corn, and cotton, these being the commodities which yield significant interaction coefficients at the 10% level (available on request). These three commodities account (according to our estimates) for about 85% of the MFP subsidies disbursed, with soybeans alone making up close to three-quarters of the total subsidy bill.

[24]We partial out the role of the other right-hand side variables in the regression model in (3) in these residualized binned scatterplots.

[25]At the mean value of $AgSubs_c$ (0.429, from Table 1), the implied overall effect of TS_c^R is: $-0.065 + 0.429 \times 0.019 =$

Figure 2: Binned Scatterplots



Notes: Based on the regression specification in Column 4, Table 2. Each y- and x-variable is first residualized of variation that can be explained by the set of right-hand side variables (excluding the US Tariff Shock, Retaliatory Tariff Shock, Retaliatory Tariff Shock × Ag. Subsidy, and Δ Health Insurance) in this Column 4 specification, while weighting by county 2016 population. The scatterplots are based on 50 bins of each x-variable, after computing the mean of the y- and x-variables within each bin. The slope coefficient of the best-fit line is reported, with robust standard errors.

As discussed in Section 3, we require that the Tariff Shock measures be uncorrelated with pre-existing forces that could drive changes in voting patterns over time, after conditioning on the extensive set of county-level observables. Toward this end, we verify in the remaining two columns of Table 2 that the US and Retaliatory Tariff Shock variables are uncorrelated with further lags in shifts in voter support for the Republican party, or in other words, that the 2018 tariff shocks do not "predict" voting patterns in earlier elections. We do so by re-running (3), replacing the dependent variable with the change in vote share for Republican House candidates in 2010 relative to 2008 (Column 5), and with the change in the Republican Presidential vote share in 2012 relative to 2008 (Column 6). Notwithstanding this, one might still be concerned about selection on the basis of unobservables. In the bottom of Table 2, we therefore report Oster (2019) diagnostics that speak to the extent to which selection on unobservables might constitute a plausible threat to our results,

−0.057, which is significantly different from zero (p-value= 0.005).

under a proportional selection assumption (i.e., that selection on observables is "proportional" to and therefore informative of the extent of selection on unobservables). Focusing on our key finding of a negative Retaliatory Tariff Shock effect, we find that selection on unobservables would have to be between 2.5 to 11.5 times as strong as selection on observables for the coefficient of TS_c^R to be nullified (δ statistic). Separately, under the assumption that selection on observables and selection on unobservables is equally important, the implied coefficient of TS_c^R would in fact be slightly larger in magnitude at -0.079 (β^* statistic).

Taking stock, we have found that the retaliatory tariffs exhibit a negative association with Republicans' performance in the 2018 midterms, whereas the US tariffs did not appear to exert a significant offsetting effect. This asymmetric response warrants discussion. One hypothesis is that the economic losses from the retaliatory tariffs may have weighed more heavily on voters' minds compared to the potential gains offered by protection against imports; this would be in line with a body of evidence on loss aversion in how the public perceives the gains and losses from trade (Freund and Ozden, 2008; Tovar, 2009). Arguably too, the negative economic consequences of the tariff retaliation were felt more immediately in the decline in prices for US agricultural commodities. For soybeans (the largest US export crop affected by volume), Adjemian et al. (2021) detect a structural break in the relative price of US to Brazilian soybeans in June 2018, around the time of China's tariff retaliation; they estimate that the retaliatory tariffs lowered US soybean export prices by 7.9% between June through November 2018, while rising demand for soybeans from other source countries raised Brazil's soybean prices by 9.7%.[26] Using price data from the Bureau of Labor Statistics, Cavallo et al. (2021) estimate that the 15% retaliatory tariffs leveled on many products prompted US exporters to lower their export prices by around 7%, with these decreases driven in large part by exports of nondifferentiated and agricultural products to China. This negative economic impact of the retaliatory tariffs was politically salient: Kim and Margalit (2021) document how Democratic House candidates in districts affected by the tariffs on US agriculture purposefully drew attention to the US-China trade war in their election campaign messaging to the general public. Using original survey data, they further report that individuals who were more exposed to the tariff retaliation were more likely to assign responsibility for the negative impact of these tariffs to the Republican party.

For the US tariffs on the other hand, Cavallo et al. (2021) find suggestive evidence that US retailers absorbed some of the price increases on imports from China. The swifter response of agricultural commodity prices could help explain why we find an adverse political reaction by voters in 2018 to the tariff retaliation, and not to the US tariffs. It may also have been the case that any improvements

[26] The 7.9% decrease in US soybean prices that Adjemian et al. (2021) find is in a similar ballpark to the price changes obtained in other studies that sought to perform model-based simulations or projections of the effects of the retaliatory tariffs at their outset (e.g., Taheripour and Tyner, 2018; Zheng et al., 2018; Sabala and Devadoss, 2019). Carter and Steinbach (2020) and Grant et al. (2021) corroborate this assessment that China's tariffs had an adverse impact on the US' agricultural exports and terms-of-trade; the latter also find little indication of a mitigating increase in US exports to third-country markets.

in US labor market outcomes from import protection needed more time to materialize. Along these lines, we will see later in Section 4.5 that the US Tariff Shock did indeed appear to gain more traction, drawing in more Republican support by the 2020 elections.[27]

Robustness: We briefly describe here a series of robustness checks on our baseline specification; these are documented in full in the appendix (see Section A.3). We first demonstrate that our key result – that the tariff retaliation appeared to hurt Republican House candidates in 2018 – obtains under different restrictions to the sample of counties (Table A.6). Our key finding is robust: (i) if we drop Pennsylvania, which saw significant redistricting in the leadup to the 2018 midterms; (ii) if we drop counties that are split across multiple congressional districts; (iii) if we drop counties in districts that were uncontested by either party in 2016 or 2018; (iv) if we drop open seats where an incumbent did not seek re-election; or (v) if we drop instances where there was a rematch in 2018 between the same Republican and Democratic candidates from the 2016 election.[28]

We have worked with alternative sets of control variables (Table A.7). Our results are preserved when we control additionally for a proxy for the capital-intensity of economic activity (both its initial level and pre-trend), constructed as the share of a county's employment that is in manufacturing industries with a high – above-median – real physical capital stock per worker (see Section A.3 for further details). We obtain estimates similar to our baseline when using longer lagged changes of the auxiliary controls to allow for the possibility of confounding trends over a more extended pre-period.[29] Separately, we verify that our results hold if we use the four-year change in the Republican vote share in House races (i.e., 2018 relative to 2014) as the dependent variable, for a more direct comparison of electoral performance relative to the preceding midterm year (Table A.8).

We also explore other constructions of the policy shock variables (Tables A.9, A.10), namely: (i) top-coding the US Tariff Shock, Retaliatory Tariff Shock, and Agricultural subsidy per worker at their 95th percentile values; (ii) using either sales or employment weights to apportion total state-wide employment in farm-agriculture industries to counties; and (iii) using a concordance from HS8 products to more disaggregated NAICS industries. We present findings when using US and

[27]While it would be useful to explore how important different channels were for voting outcomes – including the impact of trade exposure through labor market conditions (Autor et al., 2013; Feenstra et al., 2019), the capital returns of entrepreneurs (Xu, 2020; Unel, 2022), or costs of living – the data requirements for such an exercise are high. We are aware of relatively few papers that have jointly studied the roles of exposure through the labor market versus through consumption expenditures for explaining voter preferences. A recent example is Mendéz and Van Patten (2022) who use a range of administrative data on voting results and local characteristics at the sub-voting-center level to study the Costa Rica free trade agreement referendum; comparably detailed information for the US is not readily available. Such an exercise would also be complicated by the fact that what would matter for outcomes in the 2018 midterms is not just the realized impact of the tariffs on wages or costs of living, but also the perceived or expected impact in voters' minds (which we do not directly observe). This remains a fruitful avenue for future work.

[28]Our results are robust if we were to include an indicator variable for open seats or an indicator variable for rematches, instead of dropping these counties from the sample (available on request).

[29]Specifically, we control for changes over 2006-2016 in the demographic and sectoral employment share variables (instead of over 2013-2016). We control for changes between 2006-2010 and 2013-2017 (instead of between 2008-2012 and 2013-2017) for the socioeconomic controls from the American Community Survey's five-year estimates; note that the five-year estimates of most ACS data series commence in 2006-2010.

Retaliatory tariff shock measures that are constructed as a weighted-average of tariff rate changes (instead of in dollar-per-worker terms), where the industry-level tariff rate changes are aggregated to the county level using weights equal to the industry's share in county employment, $L_{i,c}/L_c$ (see Section A.1 for a more detailed description). Our results are robust under these various constructions. We further confirm that the county-level Retaliatory Tariff Shock remains relevant for explaining the decline in Republican vote share in 2018, even when we control for tariff shocks at the broader commuting zone (CZ) level; any potential effect on voting patterns of the CZ-level tariff shocks themselves are not precisely estimated.

4.2 By competitiveness bins

In a parallel set of regressions in Table 3, we examine whether the tariff shocks and agricultural subsidies exhibit heterogeneous effects on voting outcomes, depending on the competitiveness of the electoral landscape in each county. For this, we estimate a flexible triple-interaction specification that builds naturally on (3):

$$\begin{aligned}\Delta RHVoteSh_c^{18,16} \;=\; & \sum_{b=1}^{4}\beta_1^b\mathbf{1}(c\in B^b)\times TS_c^{US}+\sum_{b=1}^{4}\beta_2^b\mathbf{1}(c\in B^b)\times TS_c^{R} \\ & +\sum_{b=1}^{4}\alpha_1^b\mathbf{1}(c\in B^b)\times AgSubs_c\times TS_c^{R}+\sum_{b=1}^{4}\alpha_2^b\mathbf{1}(c\in B^b)\times AgSubs_c \\ & +\eta HInsur_c+\lambda SALT_c+\rho R_c+\sum_{b=2}^{4}\gamma^b\mathbf{1}(c\in B^b)+\Gamma X_c+D_s+\epsilon_c. \quad (4)\end{aligned}$$

As a reminder, $\mathbf{1}(c\in B^b)$ is a dummy variable equal to 1 when county c belongs to competitiveness bin B^b, where $b=1,\ldots,4$ refer respectively to the counties where the 2016 Trump vote share was 0-40%, 40-50%, 50-60%, and 60-100%. Equation (4) thus estimates a separate coefficient for the key explanatory variables – TS_c^{US}, TS_c^R, and $AgSubs_c\times TS_c^R$ – within each competitiveness bin.[30] The columns in Table 3 add progressively the variables related to health insurance, agricultural subsidies, and the relevant interaction terms with the Retaliatory Tariff Shock.

We find no statistically significant relationship between US tariff protection and shifts in the Republican vote share in any competitiveness bin. In contrast, across the four columns in Table 3, the estimated negative effect of the Retaliatory Tariff Shock is concentrated in counties where the 2016 Trump vote share was 0-40% and especially where Trump garnered between 40-50% of the vote. The magnitude of the coefficient for this 40-50% bin is economically meaningful: a one-standard-deviation increase in TS_c^R (0.192 among these counties, from Table 1, Panel C) is associated

[30] Equation (4) does not include a main effect term for TS_c^R, as this is already subsumed by the full set of interaction terms, $\mathbf{1}(c\in B^b)\times TS_c^R$, for $b=1,\ldots,4$. For an analogous reason, we do not spell out the main effect terms for TS_c^{US} and $AgSubs_c$, and the double interaction term for $AgSubs_c\times TS_c^R$, on the right-hand side.

with a $0.250 \times 0.192 \approx 4.8$ percentage point loss in the Republican House vote share (based on the Column 3 regression). The retaliatory tariffs thus appear to have hit Republican candidates' prospects particularly hard in locations where Trump narrowly lost the majority vote in 2016. That said, the agricultural subsidies displayed some mitigative effect in counties where the Retaliatory Tariff Shock was large and where the 2016 Trump vote share was in the 40-50% range (with a triple interaction coefficient significant at the 10% level).[31]

4.3 Upstream and downstream tariff shocks

The baseline tariff shock measures in (1) and (2) capture by construction the impact on the industries on which the tariffs are directly levied, before projecting these to county locations. In this subsection, we consider the possibility that there could be further impacts on local economic activity transmitted through production linkages across industries; as part of this exploration, we will also look into whether the tariffs' effects may have differed by trade partner or by sector. We re-examine the US tariffs along these dimensions first (Table 4), before turning to the retaliatory tariffs (Table 5).

The implications of the US tariffs on imports can be quite rich in the presence of cross-industry production linkages, as these in principle vary according to whether the tariffs' effects are transmitted from upstream or downstream industries. If US tariffs are raised on upstream industries, from which firms in county c tend to purchase inputs, one might expect the consequent increase in input prices to be detrimental for economic activity and hence workers in county c. To provide a hypothetical example to make this intuition more concrete, an increase in tariffs on aluminum would negatively impact counties with concentrations of canned beverage or beer factories that use aluminum inputs intensively. Conversely, if US tariffs are levied on imports that compete with downstream industries, the protection received might lead these downstream industries to increase their purchases of inputs from domestic suppliers. This would be the case if say a tariff increase on imports of auto parts raises production in the US auto parts industry; if demand for US steel were to rise as a result, this would benefit counties where the steel industry is located.

To accommodate both of these potential forces, we construct two distinct measures:

$$upTS_c^{US} = \sum_o \sum_j \frac{L_{j,c}}{L_j} \sum_i a_{ij} \frac{TS_i^{o,US}}{\bar{L}_c}, \text{ and} \tag{5}$$

$$dwTS_c^{US} = \sum_o \sum_i \frac{L_{i,c}}{L_i} \sum_j d_{ij} \frac{TS_j^{o,US}}{\bar{L}_c}. \tag{6}$$

Recall that $TS_i^{o,US}$ is industry i's exposure to the US tariffs imposed on imports from origin country o; more specifically, this is the estimated dollar value of these tariffs collected on products that fall

[31] In an analogous exercise, Table A.11 finds that the negative relationship between recent health insurance coverage gains and Republican support was concentrated too in an electorally competitive bin, specifically the 50-60% bin.

Table 3: Tariff Retaliation and Voting Patterns: By Electoral Competitiveness Bins

Dep. Variable: Δ Republican Vote Share	House, '18-'16			
	(1)	(2)	(3)	(4)
US Tariff Shock $\times$ $\mathbf{1}$(Pres. Vote $\in [0, 0.4]$)	0.058	0.057	0.057	0.057
	[0.050]	[0.051]	[0.051]	[0.051]
US Tariff Shock $\times$ $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$)	0.008	0.009	0.008	0.010
	[0.038]	[0.038]	[0.038]	[0.038]
US Tariff Shock $\times$ $\mathbf{1}$(Pres. Vote $\in (0.5, 0.6]$)	0.030	0.030	0.030	0.030
	[0.021]	[0.021]	[0.021]	[0.021]
US Tariff Shock $\times$ $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$)	-0.001	-0.001	-0.001	-0.001
	[0.007]	[0.007]	[0.007]	[0.007]
Retaliatory Tariff Shock $\times$ $\mathbf{1}$(Pres. Vote $\in [0, 0.4]$)	-0.166**	-0.168**	-0.169**	-0.180**
	[0.082]	[0.081]	[0.079]	[0.081]
Retaliatory Tariff Shock $\times$ $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$)	-0.256***	-0.257***	-0.250***	-0.273***
	[0.090]	[0.090]	[0.091]	[0.086]
Retaliatory Tariff Shock $\times$ $\mathbf{1}$(Pres. Vote $\in (0.5, 0.6]$)	-0.021	-0.018	-0.022	-0.026
	[0.046]	[0.045]	[0.047]	[0.047]
Retaliatory Tariff Shock $\times$ $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$)	-0.036*	-0.035	-0.031	-0.037
	[0.021]	[0.022]	[0.020]	[0.022]
Retaliatory Tariff Shock $\times$ Ag. Subsidy $\times$ $\mathbf{1}$(Pres. Vote $\in [0, 0.4]$)				0.240
				[0.162]
Retaliatory Tariff Shock $\times$ Ag. Subsidy $\times$ $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$)				0.533*
				[0.284]
Retaliatory Tariff Shock $\times$ Ag. Subsidy $\times$ $\mathbf{1}$(Pres. Vote $\in (0.5, 0.6]$)				-0.024
				[0.018]
Retaliatory Tariff Shock $\times$ Ag. Subsidy $\times$ $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$)				0.015
				[0.010]
Health Insurance Share (2013-17 avg.)		0.098	0.103	0.105
		[0.116]	[0.115]	[0.116]
Δ Health Insurance Share (2013-17 minus 2008-12)		-0.196**	-0.193**	-0.199**
		[0.090]	[0.089]	[0.090]
$\mathbf{1}$(SALT (2016) $\in$ 4th Quintile)	-0.018**	-0.018*	-0.018**	-0.019**
	[0.009]	[0.009]	[0.009]	[0.009]
$\mathbf{1}$(SALT (2016) $\in$ 5th Quintile)	-0.024**	-0.023*	-0.024**	-0.024**
	[0.012]	[0.012]	[0.012]	[0.012]
Lags: Δ Rep. House and Δ Rep. Pres. Vote Share	Y	Y	Y	Y
Main effects: $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y	Y
Double interactions: Ag. Subsidy $\times$ $\mathbf{1}$(Pres. Vote $\in [0, 0.4]$),...	N	N	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y	Y
State FEs	Y	Y	Y	Y
Observations	3,072	3,072	3,072	3,072
R^2	0.721	0.722	0.722	0.723

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in both 2016 and 2018. Electoral competitiveness bins are constructed on the basis of the two-party Republican vote share in the 2016 Presidential election. All columns control for: (i) lagged changes in the county-level Republican House vote share ('16-'14, '14-'12, '12-'10); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); and (iii) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies, as listed in the notes to Table 2. All columns include the main effects of $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),..., $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$), while Columns 3-4 include the double interaction terms in Ag. Subsidy $\times$ $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),..., Ag. Subsidy $\times$ $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$). Unreported coefficients are available on request.

under industry i. Equation (5) apportions a share of $TS_i^{o,US}$ in turn to each industry j that uses inputs from i according to the allocation coefficient, $a_{ij} = Z_{ij}/Y_i$, where Z_{ij} is the value of industry j's purchases from i and Y_i is industry i's gross output, drawn from the 2012 US Input-Output Tables.[32] Industry j's upstream exposure to $TS_i^{o,US}$ is thus larger, the greater is industry j's input purchases per dollar of i's gross output. This upstream exposure of industry j is then further apportioned to county c, using (as before) the county's share in industry-j employment, $L_{j,c}/L_j$. We then sum across all trade partner countries and industry pairs, and divide by the county working age population, $\bar{L}_c$, to obtain the overall exposure of county c to the US tariffs via upstream production linkages, $upTS_c^{US}$.

The impact via downstream linkages is computed in a similar manner, as defined in equation (6). We take the tariff shock experienced by industry j, $TS_j^{o,US}$, and apportion it across industries i that j purchases inputs from; we use here the direct requirements coefficient, $d_{ij} = Z_{ij}/Y_j$, as intuitively, the extent to which $TS_j^{o,US}$ affects input industries i would depend on how large input purchases from i are as a share of the size of industry j. The exposure of industry i to these downstream US tariffs is then mapped to individual counties, on the basis of the county's share in industry-i employment, $L_{i,c}/L_i$. Once again, summing across all target countries o and industry pairs, and dividing by $\bar{L}_c$, we obtain a per-worker measure of the county's exposure to the US tariffs as transmitted from downstream linkages, $dwTS_c^{US}$.

Figure A.4 in the appendix illustrates the intensity of these upstream and downstream US Tariff Shocks across counties. It will be useful for the analysis that follows to break down these measures into the components attributable to the non-Section 301 tariffs (on washing machines, solar panels, steel, and aluminum, from all countries), versus the Section 301 tariffs (targeting a broad set of products from China exclusively); as with the US Tariff Shock, both $upTS_c^{US}$ and $dwTS_c^{US}$ can be additively decomposed by tariff round or trade partner country. Looking first at the direct US Tariff Shocks, both the non-Section 301 and Section 301 tariffs are concentrated in the Midwest, Great Lakes, and (to some extent) the US Southeast. Note though that the Section 301 tariffs had a greater geographic reach in terms of counties affected, for the simple reason that these covered a wider range of imported products (Panels A and B). Turning to the upstream and downstream US Tariff Shocks, while these are smaller in dollar-per-worker terms for the average county than the direct US Tariff Shocks (see the summary statistics in Table A.2, Panel A), we nevertheless see that taking production linkages into account expands the set of counties to which the impact of the US tariffs is transmitted (Figure A.4, Panels C-F).[33]

In Table 4, we re-run the specification in (3), augmented with a more detailed set of US Tariff

[32] In practice, some of the inputs purchased by industry j from industry i may be drawn out of imports or inventories, rather than from i's gross output. We therefore follow Antràs et al. (2012) in applying a net exports and net inventories correction to Z_{ij} – based on a proportionality assumption about the input flows that are drawn from these additional sources – when computing both the upstream and downstream tariff exposure measures. See Section A.1 in the appendix for more details.

[33] Table A.2 in the appendix reports summary statistics and pairwise correlations among the various direct, upstream, and downstream county-level tariff shock measures.

Shocks on the right-hand side. Column 1 breaks down the US Tariff Shock into the non-Section 301 and Section 301 tariffs respectively. Neither of these two components yields a significant effect on voter support for Republican House candidates in the 2018 midterms. Column 2 further includes the upstream US Tariff Shock attributable to each of the non-Section 301 and the Section 301 tariff actions. Although neither coefficient is precisely estimated, the sign on the point estimates suggests that US tariffs on upstream industries had a negative producer-side impact in counties c where these inputs are used more intensively, consistent with the intuition articulated earlier.

Table 4: US Tariffs and Voting Patterns: Exploring Upstream and Downstream Effects

Dep. Variable: Δ Republican Vote Share	House, '18-'16				
	(1)	(2)	(3)	(4)	(5)
US Tariff Shock, non-Section 301	0.003	0.006	-0.016	-0.023	-0.022
	[0.012]	[0.039]	[0.020]	[0.044]	[0.044]
US Tariff Shock, Section 301	0.024	0.025	0.014	0.019	0.019
	[0.017]	[0.019]	[0.018]	[0.018]	[0.019]
Upstream US Tariff Shock, non-Section 301		-0.014		-0.010	-0.013
		[0.134]		[0.138]	[0.137]
Upstream US Tariff Shock, Section 301		-0.005		-0.092	-0.086
		[0.132]		[0.143]	[0.141]
Downstream US Tariff Shock, non-Section 301			-0.042	-0.053	-0.053
			[0.033]	[0.034]	[0.034]
Downstream US Tariff Shock, Section 301			0.172**	0.226**	0.226**
			[0.082]	[0.100]	[0.100]
Retaliatory Tariff Shock	-0.056**	-0.054**	-0.070***	-0.057**	-0.062**
	[0.022]	[0.024]	[0.025]	[0.025]	[0.026]
Retaliatory Tariff Shock × Ag. Subsidy					0.018
					[0.011]
Ag. Subsidy					-0.012
					[0.009]
Health Insurance Share (2013-17 avg.)	0.094	0.092	0.104	0.095	0.097
	[0.114]	[0.113]	[0.113]	[0.112]	[0.112]
Δ Health Insurance Share (2013-17 minus 2008-2012)	-0.187**	-0.187**	-0.186**	-0.186**	-0.187**
	[0.091]	[0.091]	[0.092]	[0.091]	[0.090]
$\mathbf{1}$(SALT (2016) $\in$ 4th Quintile)	-0.018**	-0.018**	-0.018**	-0.018**	-0.018**
	[0.009]	[0.009]	[0.009]	[0.009]	[0.009]
$\mathbf{1}$(SALT (2016) $\in$ 5th Quintile)	-0.025**	-0.025**	-0.024**	-0.024**	-0.025**
	[0.012]	[0.012]	[0.012]	[0.012]	[0.012]
Lags: Δ Rep. House and Δ Rep. Pres. Vote Share	Y	Y	Y	Y	Y
2016 Bins: $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y
Observations	3,072	3,072	3,072	3,072	3,072
R-squared	0.718	0.718	0.719	0.719	0.719

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in both 2016 and 2018. All columns control for: (i) lagged changes in the county-level Republican House vote share ('16-'14, '14-'12, '12-'10); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); (iii) the $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),..., $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$) indicator variables for the Republican vote share in the 2016 Presidential election; and (iv) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies, as listed in the notes to Table 2. Unreported coefficients are available on request.

Column 3 turns to the downstream US Tariff Shocks. Interestingly, we find that a greater degree of protection afforded to downstream industries – which would in principle raise these industries' demand for inputs – is indeed associated with an increase in the Republican vote share in counties where domestic suppliers tend to be located; this is especially the case for the Section 301 tariffs on China. These patterns from Columns 2 and 3 continue to hold when we simultaneously include both the upstream and downstream US Tariff Shocks (Column 4), and when we control further for MFP subsidies per worker and its interaction with the Retaliatory Tariff Shock (Column 5).[34] Note that these patterns do not detract from our baseline findings, that the retaliatory tariffs, concerns over health insurance, and the issue of SALT deduction limits in high-tax locations all weighed down on the Republican vote share; the coefficients on these key variables remain significant and stable in magnitude across all columns of the table.

In Table 5, we perform a parallel analysis for the retaliatory tariffs. We compute upstream and downstream Retaliatory Tariff Shocks as follows:

$$upTS_c^R = \sum_d \sum_j \frac{L_{j,c}}{L_j} \sum_i a_{ij} \frac{TS_i^{US,d}}{\bar{L}_c}, \text{ and} \tag{7}$$

$$dwTS_c^R = \sum_d \sum_i \frac{L_{i,c}}{L_i} \sum_j d_{ij} \frac{TS_j^{US,d}}{\bar{L}_c}. \tag{8}$$

These are analogous to (5) and (6), but are constructed using instead industries' exposure to the retaliatory tariffs imposed by destination country d; for example, the above expression for $upTS_c^R$ takes equation (5) and replaces $TS_i^{o,US}$ with $TS_i^{US,d}$. How the retaliatory tariffs might be expected to impact a given county through production linkages depends once again on whether it is upstream or downstream shocks that are being considered. As foreign retaliatory tariffs reduce demand for the goods on which they are levied, this would in principle lower US prices for these same goods. When retaliatory tariffs hit upstream industries, this could be beneficial to those firms – and hence county locations – that use these inputs intensively. On the other hand, when retaliatory tariffs hurt downstream industries, this can be expected to dampen their demand for inputs, and thus negatively impact those counties where key domestic suppliers are situated.

Figure A.5 in the appendix illustrates heat maps of exposure to the retaliatory tariffs. We present a breakdown of the direct Retaliatory Tariff Shock, as well as of $upTS_c^R$ and $dwTS_c^R$, according to whether the tariffs are imposed by China or non-China trade partner countries (which are Canada, Mexico, and the EU in our data); note that China in particular accounted for more than three-quarters of the direct Retaliatory Tariff Shock (see Table 1, Panel B). For the China tariffs, a further split into those levied on US agricultural versus non-agricultural exports will be of interest, given how

[34]Based on the Column 4 point estimate, a one-standard-deviation greater downstream exposure to the Section 301 tariffs (0.079, from Table A.2) yields an implied effect of a $0.226 \times 0.079 \approx 1.8$ percentage point vote share shift in favor of the Republican House candidate.

prominently the tariffs' impact on American farmers featured in the leadup to the 2018 midterms.[35] The retaliation by China against agricultural products hit the central plains and Northwest especially hard (Panel A, Figure A.5). The downstream and upstream transmission of this shock is also concentrated in these same regions (Panels D and G), likely reflecting the co-location of industries that use agricultural inputs or that supply inputs to the agricultural sector. On the other hand, exposure to the retaliatory tariffs on non-agricultural products was more evenly spread out across the US.

Column 1 in Table 5 first considers the effects of the tariff retaliation by different countries. The results indicate that the baseline negative effect of the Retaliatory Tariff Shock on support for Republican House candidates was not driven exclusively by either China or the US' largest non-China trade partners. Column 2 further disaggregates the China Retaliatory Tariff Shock into that levied on agricultural versus non-agricultural products. Though the coefficients here are imprecisely estimated, we will see in later columns that both of these margins of the China Retaliatory Tariff Shock have some explanatory power for voting outcomes.

In Column 3, we include in the regression the upstream Retaliatory Tariff Shock stemming respectively from China's tariffs on agriculture, China's tariffs on non-agricultural products, and the non-China (i.e., CAN/MEX/EU) tariffs. Taking the positive and significant upstream effect of China's retaliatory tariffs on agriculture at face value, this would be consistent with lower agricultural input costs (for example, sorghum feed for hogs) having a positive economic impact on counties where firms or farms use these inputs more. While one might expect retaliatory tariffs on downstream industries to have a converse negative impact, we do not obtain significant findings in Column 4 on any of the three components of the $dwTS_c^R$ measure. The upstream tariffs by China on agriculture remain the only significant production linkage effect when we include all upstream and downstream Retaliatory Tariff Shock measures (Column 5), or when we further control for an interaction effect of MFP subsidies (Column 6, with the direct Retaliatory Tariff Shock by China on US agriculture).[36]

In sum, when cross-industry production linkages are taken into account, we find several channels through which the Trump administration's tariffs or the foreign tariff retaliation may have worked in Republican House candidates' favor. These findings are consistent with an intuition grounded in how the economic interests of producers and workers in a county would be affected should the tariffs' effects be transmitted through domestic input-output linkages. That said, while the mechanisms that rationalize these patterns may align with economic intuition, these linkages are arguably subtle from

[35]We do not pursue an analogous breakdown of the non-China tariffs into an agricultural and a non-agricultural component, given that the average county-level exposure to the tariffs levied by Canada, Mexico, and the EU on agricultural products was small ($2 per worker, see Panel B of Table 1) relative to the overall non-China Retaliatory Tariff Shock ($38 per worker).

[36]In terms of magnitude, a one-standard-deviation larger upstream exposure to the China tariffs on agricultural products (0.034, from Table A.2) corresponds to a $0.606 \times 0.034 \approx 2.1$ percentage point positive effect on the change in the Republican House vote share (based on Column 5). However, this upstream retaliatory tariff shock variable exhibits a lot of right skew, with the standard deviation being more than four times its median value (0.008), so the implied vote share change should these upstream retaliatory tariffs be removed would be smaller for most counties.

the perspective of voters. There is little evidence of media reporting, for example, of voters recognizing that they were the beneficiaries of retaliatory tariffs being placed on upstream agricultural industries. By contrast, there was much more awareness in rural counties about the direct hit that farmers were absorbing from the tariffs levied on their products. Nonetheless, the findings here add to a growing body of work that has uncovered instances where the effects of trade policies have been transmitted

Table 5: Retaliatory Tariffs and Voting Patterns: Exploring Upstream and Downstream Effects

Dep. Variable: Δ Republican Vote Share	House, '18-'16					
	(1)	(2)	(3)	(4)	(5)	(6)
US Tariff Shock	0.018	0.018	0.023	0.017	0.021	0.021
	[0.012]	[0.012]	[0.017]	[0.012]	[0.018]	[0.018]
Retaliatory Tariff Shock, CHN	-0.051**					
	[0.023]					
Retaliatory Tariff Shock, CHN on Ag.		-0.050	-0.063**	-0.046	-0.055**	-0.061**
		[0.031]	[0.026]	[0.032]	[0.027]	[0.029]
Retaliatory Tariff Shock, CHN on non-Ag.		-0.054	-0.056	-0.126*	-0.149**	-0.147**
		[0.042]	[0.057]	[0.064]	[0.064]	[0.064]
Retaliatory Tariff Shock, CAN/EU/MEX	-0.122**	-0.122**	-0.116	-0.183***	-0.087	-0.089
	[0.057]	[0.057]	[0.074]	[0.063]	[0.078]	[0.078]
Upstream Retaliatory Tariff Shock, CHN on Ag.			0.326**		0.606***	0.592***
			[0.161]		[0.194]	[0.192]
Upstream Retaliatory Tariff Shock, CHN on non-Ag.			0.041		0.019	0.025
			[0.080]		[0.090]	[0.090]
Upstream Retaliatory Tariff Shock, CAN/EU/MEX			-0.171		-0.165	-0.152
			[0.308]		[0.308]	[0.307]
Downstream Retaliatory Tariff Shock, CHN on Ag.				-0.064	-0.195	-0.189
				[0.137]	[0.157]	[0.155]
Downstream Retaliatory Tariff Shock, CHN on non-Ag.				0.212	0.335	0.328
				[0.187]	[0.220]	[0.221]
Downstream Retaliatory Tariff Shock, CAN/EU/MEX				0.112	-0.227	-0.226
				[0.111]	[0.173]	[0.171]
Retaliatory Tariff Shock, CHN on Ag. × Ag. Subsidy						0.024*
						[0.012]
Ag. Subsidy						-0.013
						[0.009]
Health Insurance Share (2013-17 avg.)	0.093	0.093	0.093	0.093	0.099	0.103
	[0.113]	[0.111]	[0.113]	[0.110]	[0.112]	[0.111]
Δ Health Insurance Share (2013-17 minus 2008-12)	-0.187**	-0.187**	-0.182*	-0.186**	-0.176*	-0.177*
	[0.091]	[0.091]	[0.091]	[0.092]	[0.092]	[0.091]
$\mathbf{1}$(SALT (2016) ∈ 4th Quintile)	-0.018**	-0.018**	-0.018**	-0.018**	-0.018**	-0.018**
	[0.009]	[0.009]	[0.009]	[0.009]	[0.009]	[0.009]
$\mathbf{1}$(SALT (2016) ∈ 5th Quintile	-0.025**	-0.025**	-0.024**	-0.024**	-0.024**	-0.024**
	[0.012]	[0.012]	[0.012]	[0.012]	[0.012]	[0.012]
Lags: Δ Rep. House and Δ Rep. Pres. Vote Share	Y	Y	Y	Y	Y	Y
2016 Bins: $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y	Y
Observations	3,072	3,072	3,072	3,072	3,072	3,072
R-squared	0.718	0.718	0.719	0.718	0.719	0.719

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in both 2016 and 2018. All columns control for: (i) lagged changes in the county-level Republican House vote share ('16-'14, '14-'12, '12-'10); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); (iii) the $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),..., $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$) indicator variables for the Republican vote share in the 2016 Presidential election; and (iv) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies, as listed in the notes to Table 2. Unreported coefficients are available on request.

across industries via production linkages. There is now evidence showing that the US' temporary trade barriers (TTBs), such as anti-dumping duties, have had a negative impact on employment in downstream industries that use TTB-covered products as inputs (Barattieri and Cacciatore, 2023; Bown et al., 2023). On the US-China tariffs more specifically, Flaaen and Pierce (2019) find that US industries faced with higher imported input costs experienced a decline in domestic employment, while Handley et al. (2020) show that these industries' exports were hampered.[37]

A second caveat to bear in mind is that there is a strong positive correlation across several pairs of these more detailed tariff shock measures (see Table A.2). There is a lingering concern that multicollinearity could strain the reliability of statistical inference; for example, in Table 5, whether or not China's retaliatory tariffs on agriculture and on non-agricultural products had a significant impact of voting appears to depend on what other tariff shocks are included on the right-hand side. For this reason, we do not further include the measures of upstream and downstream exposure for both the US and retaliatory tariffs jointly in the same regression, nor do we consider these measures in the counterfactual exercises that follow below.

4.4 Counterfactuals

While the implied effects of the trade war discussed above are informative, measuring outcomes in terms of changes in average vote shares overlooks how the specific geographic incidence of the tariffs may have ultimately affected the number of congressional seats won by each party. In this subsection, we translate our regression results into counterfactual aggregate election outcomes, in order to address such questions as: How many more House seats would Republicans have won but for the estimated influence of the trade war?

We consider a series of counterfactual scenarios that focus on those explanatory variables that we found to have a statistically significant effect. Specifically, we ask how Republicans would have fared: (i) absent the trade war writ large (i.e., removing the effects of the direct Retaliatory Tariff Shock *and* agricultural subsidies); (ii) absent the agricultural subsidies *only* (but including the estimated political consequence of retaliatory tariffs); (iii) absent the political influence of recent health insurance coverage gains; and (iv) absent the potential role of the limit on SALT deductions as a voter concern.[38] In what follows, we will base our calculations on the point estimates from the full specification in Column 4 of Table 3 that allows for heterogeneous effects by 'competitiveness bins'. That means that in scenario (i), for example, we obtain the counterfactual vote shares by subtracting

[37] More broadly, Blanchard et al. (2016) study the determination of optimal tariffs when taking into account how policies applied to an industry would spill over through global value chain (GVC) linkages on other industries. This nascent literature on trade policy in GVC settings is surveyed in Section 6 of the handbook chapter by Antràs and Chor (2022).

[38] We do not consider a counterfactual involving the removal of US tariffs, since these did not have a statistically significant impact in Table 2. Also, given the caveats discussed at the end of Section 4.3, we refrain from performing counterfactuals related to the possible effects of indirect tariff shocks transmitted through production linkages.

the $\sum_{b=1}^{4} \beta_2^b \mathbf{1}(c \in B^b) \times TS_c^R$ and $\sum_{b=1}^{4} \alpha_1^b \mathbf{1}(c \in B^b) \times AgSubs_c \times TS_c^R$ terms from the actual 2018 Republican vote share for county c, whereas in scenario (ii), we remove only the main effect term $\sum_{b=1}^{4} \beta_2^b \mathbf{1}(c \in B^b) \times TS_c^R$.[39]

Table 6: Implied Effects of the Tariff War on 2018 Voting Outcomes

	Data	Counterfactuals			
		Remove retal. tariffs and Ag. subsidies	Remove Ag. subsidies only	Remove health insurance gains	Remove SALT effects
		A: Implied shift in Republican vote share			
National change: All counties	-0.050	-0.040	-0.050	-0.042	-0.032
		[-0.045,-0.035]	[-0.051,-0.049]	[-0.050,-0.035]	[-0.049,-0.017]
By competitiveness bins:					
$\mathbf{1}$(Pres. vote $\in [0, 0.4]$)	-0.031	-0.021	-0.031	-0.021	-0.009
		[-0.030,-0.011]	[-0.031,-0.031]	[-0.030,-0.013]	[-0.029,0.010]
$\mathbf{1}$(Pres. vote $\in (0.4, 0.5]$)	-0.050	-0.029	-0.049	-0.042	-0.029
		[-0.040,-0.017]	[-0.050,-0.049]	[-0.049,-0.034]	[-0.049,-0.010]
$\mathbf{1}$(Pres. vote $\in (0.5, 0.6]$)	-0.066	-0.064	-0.067	-0.058	-0.049
		[-0.074,-0.054]	[-0.068,-0.066]	[-0.066,-0.052]	[-0.065,-0.034]
$\mathbf{1}$(Pres. vote $\in (0.6, 1]$)	-0.063	-0.057	-0.062	-0.055	-0.051
		[-0.063,-0.050]	[-0.064,-0.060]	[-0.062,-0.049]	[-0.062,-0.040]
		B: Implied net gain of CDs for the Democratic party			
Actual swing:		Gain of 36 (excl. PA)			
Assumed county-by-CD weights:					
Uniform vote share within county	53	43	53	47	37
		[40,50]	[51,53]	[39,52]	[23,53]
Non-uniform, based on 2016	24	13	24	15	6
		[9,18]	[24,25]	[10,23]	[-2,23]
Non-uniform, based on 2018	36	26	36	28	21
		[20,33]	[36,38]	[23,36]	[5,35.5]

Notes: Implied effects are computed based on the coefficient estimates from Column 4, Table 3. The five columns report effects respectively: from the data; under a scenario where both the retaliatory tariff shock and agricultural subsidies are set to zero; where only the agricultural subsides are set to zero; where the five-year average gains in health insurance coverage are removed; and where the effects of being in a fourth or fifth quintile SALT county are removed. Panel A reports the implied change in the Republican vote share; the vote share changes at the county level are first computed, and then aggregated up to either the national level or by electoral competitiveness bins. Panel B reports the net gain in House seats for the Democratic party, namely the number of seats where the Republican two-party vote share was > 0.5 in 2016 but the predicted vote share dropped to < 0.5 in 2018, less the number of seats where the Republican two-party vote share was < 0.5 in 2016 but the predicted share was > 0.5 in 2018. The first row is computed on the assumption that the vote share received by the Republican party is uniform within each county across all constituent county-by-CD partitions. The remaining rows relax this uniformity assumption, using instead the reported share of Republican (respectively, Democratic) votes within a county accounted for by each county-by-CD to break up the Republican (respectively, Democratic) predicted vote at the county level, before aggregating to the CD level; the second row does this on the basis of the 2016 county-by-CD voting outcomes, while the third row uses the 2018 voting outcomes. The sample considered here excludes Pennsylvania due to redistricting; adding the net gain of 4 seats for the Democratic party in Pennsylvania would bring the actual total net gain to 40 seats. The 95% confidence intervals reported are based on 1,000 sets of Monte Carlo draws from the joint multivariate normal distribution of the Column 4, Table 3 coefficient estimates.

Table 6 summarizes our findings. The upper panel reports implications for the Republican vote share, aggregating over all House races to the national level. The first column reports the actual data as a benchmark, followed by results under each of the four scenarios. In the data, Republican House candidates saw a 5.0 percentage point decline in vote share nationwide, compared to 2016.[40]

[39] In scenario (iii), we remove the vote share effect explained by the change in health insurance coverage in 2013-2017 (relative to 2008-2012), while in scenario (iv), we remove that attributable to the two SALT quintile dummies. Throughout the counterfactuals reported, we hold constant the total number of votes cast in a county, while altering the Republican share of votes according to our regression estimates.

[40] Note that the average cross-county change of -6.4 percentage points reported in Table 1 is an unweighted mean across counties. Weighting the change in county-level vote shares by total county votes yields the -5.0 percentage point number just reported in the text (by definition).

Comparing this to our estimated counterfactuals across the first row, we find that the trade war (including remedial agricultural subsidies) can account for $(0.050 - 0.040) \times 100 = 1.0$ percentage point, or about one-fifth of the observed decline in Republicans' nationwide House vote share. In contrast, removing only the agricultural subsidies would have had a negligible effect on Republican support. Although the subsidies are important for the largest recipient counties, these are also counties with such small populations that there is little influence on nationwide vote shares. Health insurance accounts for $(0.050 - 0.042) \times 100 = 0.8$ percentage points of the erosion of Republican vote share, while considerations related to state and local taxes explain $(0.050 - 0.032) \times 100 = 1.8$ percentage points of the swing away from the party.

The lower panel translates the vote share changes into implied House seats. This exercise requires making assumptions about how to apportion the implied change in county-level voting when counties are split across multiple CDs, which we describe in more formal detail in Section A.4 in the appendix. Our first and simplest approach assumes that a county's Republican vote share is uniformly distributed across any CD with which it overlaps. Under this assumption, we divide the votes cast at the county level for each party in the 2018 House elections into each county-by-CD partition, in proportion to the total votes cast (summed over both parties) in each county-by-CD partition from the earlier 2016 House elections (from Election Atlas). Aggregating to the CD level, we can then count the implied number of seats won by each party. That exercise, reported in Column 1, yields a net swing to the Democratic party of 53 seats, which exceeds the actual swing of 36 seats observed (when excluding Pennsylvania).[41] This difference in the predicted versus actual seat swing can be interpreted as the number of additional seats that Republicans may have lost, absent the strategic gerrymandering of CD boundaries.

A second approach allows each county-by-CD partition to differ in importance to each party. Here, we divide the votes cast at the county level for the Republican party (respectively, Democratic party) using weights that are proportional to the Republican (respectively, Democratic) votes in each county-by-CD partition. Basing these weights on the county-by-CD figures from the 2016 House election, we obtain an under-estimate – a net swing of 24 seats – towards the Democrats. In contrast, using weights based on the 2018 county-by-CD figures yields exactly the net swing of 36 seats. This makes intuitive sense, as the 2018 data incorporate information about shifts in the importance of each county-by-CD partition for each parties' performance.

We compute the implied seat swing for the four hypothetical scenarios by converting the counterfactual county-level vote shares to CD-level race outcomes, under each approach. Focusing on the last row in Table 6, which adopts the more realistic (non-uniform) apportioning rule based on the 2018 party-specific county-by-CD weights, our results suggest that the trade war cost Republicans a net 36-26=10 House seats. We further report empirical confidence intervals based on 1,000 sets

[41] We exclude Pennsylvania from these counterfactual computations, since the redistricting in that state makes it infeasible to perform the apportionment of county votes to CDs.

of Monte Carlo draws from the associated joint distribution of the coefficient point estimates. This allows us to rule out in particular the null hypothesis that the trade war was irrelevant for explaining any Republican seat losses: absent the trade war, the 95% confidence interval for the number of seats lost, $[20, 33]$, is strictly below the actual swing of 36 seats. Under scenario (ii), where the retaliatory tariffs are in place, but no agricultural subsidies were extended, we find that the subsidies had a limited impact on the predicted number of House seats with an upside of at most two seats for Republican candidates on the basis of the upper bound of the 95% empirical confidence interval. The MFP thus appears to have had minimal bearing on race outcomes, likely due to the geographically-narrow impact of the subsidies.

Under scenario (iii), we find that the removal of health insurance as a policy issue can account for 36-28=8 Republican seats lost. Under the final scenario, it appears concerns over the cap on SALT deductions in high-tax locations can explain 36-21=15 lost seats, pointing to the unpopularity of this tax policy.[42] Note that the 95% confidence intervals for the counterfactual seat swing under each of scenarios (i), (iii) and (iv) have a good amount of overlap, which leads us to conclude that each of these three forces – the trade war, health care policy, concerns over SALT deductability – were similarly important in terms of the range of Republican seats lost they can account for.

4.5 Other Outcome Variables

We round off our analysis with a brief look at the impact of the US-China tariffs on several other outcome variables. These are not the primary focus of our study for a variety of reasons described below, but we present these nevertheless as the patterns are suggestive of a broader influence on other election outcomes of interest.

Turnout: The 2018 midterms were notable for its exceptionally high rate of voter turnout in a non-presidential election year. Based on the available data from the Election Atlas, 61.3% of all registered voters cast a ballot in 2018, much higher than the 45.7% in the previous midterm year in 2014. Did the Trump administration's stance on tariffs, or the tariff retaliation that followed, play any role in motivating voters to show up at the polls?

A key limitation here is the comprehensiveness of the data on voter turnout in US elections. The Election Atlas has undertaken a major effort in collecting this information from disparate state-level sources, but official turnout data is not reported by a handful of states; furthermore, about half the states do not document turnout by party registration. It is also tricky to compare turnout across states, given the myriad differences in voter registration rules. We therefore focus on a relatively basic measure of turnout, namely the votes cast as a share of total registered voters.

We explore the effects on turnout in Table A.12 in the appendix. There, we run a specification akin

[42] Appendix Figure A.6 presents a visualization of these counterfactual estimates of how much the trade war, health care policy, and SALT affected the Republican party's CD-level vote shares in the 2018 House elections.

to (3) but with the change in voter turnout and its lagged changes from prior election cycles replacing the change in the Republican vote share as the variable of interest. The results indicate that counties with more US tariff protection exhibited higher voter turnout in 2018 relative to either the 2016 elections (Columns 1-3) or the 2014 midterms (Columns 4-6). Conversely, the Retaliatory Tariff Shock is associated across all columns with lower turnout. While we are unable to control for other potential forces that could explain turnout (such as local political advertising or get-out-the-vote campaigns), these results nevertheless suggest that the Trump tariffs played a role in modestly raising voter participation and that the retaliatory tariffs instead dampened this propensity to vote. These effects roughly cancel out for the average county (i.e., when evaluated at the mean US and Retaliatory Tariff Shock values reported in Table 1): absent both these tariff actions, and using the Column 6 coefficients as a benchmark, the implied change in turnout would be $0.017 \times 0.226 - 0.039 \times 0.194 \approx -0.004$ or 0.4 percentage points lower.

The 2020 Elections: Last but not least, we briefly examine if there was any carry-over effect from these tariff actions to the 2020 elections. Toward this end, Table A.13 in the appendix explores whether there were effects on the Republican vote share in the 2020 House races relative to 2018 (Columns 1-3), as well as on the vote share garnered by Trump in the 2020 Presidential election relative to 2016 (Columns 4-6), using the same Tariff Shock and MFP subsidy measures constructed from the sequence of policy actions up until October 2018 (that we have been using in our baseline analysis).[43]

The results presented in Table A.13 indicate that the tariff war had no significant effect on Republican support in House races in 2020. Interestingly though, they appear to have had some bearing on the Presidential election: Holding all else constant, the Trump vote share was higher (relative to 2016) in counties with greater exposure to the US tariffs, suggesting a modest political dividend for the incumbent president from his pursuit of protectionist trade policies. However, the retaliatory tariffs that followed appeared to cost the incumbent some support, with the Democratic party's candidate (Biden) faring better in counties exposed to a larger Retaliatory Tariff Shock.[44] The impact uncovered here on voting outcomes for president echoes Lake and Nie (2023) and Beck et al. (2023); the size of the coefficient estimates in Columns 4-6 of Table A.13 suggests that a counterfactual removal of the trade war would imply a small change to the Trump vote share, but as Lake and Nie (2023) explore in detail, these may have been sufficient to move key states across the win-loss column given the thin margins of victory during the 2020 Presidential election. The lack of an impact of the tariffs on House races in 2020 is a new finding (to the best of our knowledge). This

[43]In all regressions in Table A.13, we control for four lagged changes in the Republican party's House vote share (2018-2016, 2016-2014, 2014-2012, 2012-2010) as well as the lagged change in its Presidential vote share (2016-2012). We have checked that a "no pre-trends" condition holds: Conditional on these controls, further lags of Republican vote share changes are uncorrelated with the tariff shock measures (see Table A.14 in the appendix, Columns 1-2).

[44]In Table A.14 in the appendix, specifically Columns 3-4, we have checked that these results are robust to using the four-year vote share change (relative to the last Presidential election year) instead.

is broadly in line with the observation that there were segments of the electorate who did not vote simply along party lines across all races on the ballot in 2020, which enabled the Republican party to register a gain of 14 seats on the Democratic House majority, even while losing the presidency.

We explore in Table A.15 in the appendix whether the tariffs and agricultural subsidies that were rolled out after the 2018 midterms had any further bearing on voting outcomes in the 2020 elections. To do so, we augment the specifications in Table A.13 with analogous measures of the US Tariff Shock, Retaliatory Tariff Shock, and MFP subsidy per worker for the respective policies implemented between November 2018 and November 2020 (see Section A.1 in the appendix for more details). Interestingly, we find that while the retaliatory measures prior to the 2018 midterms appeared to weigh down on Trump's performance in 2020, the tariff retaliation post-2018 had the converse effect on the Trump vote share (Columns 5-6, Table A.15). This could be due to differences in the underlying mix of products that were targeted pre- versus post-2018; for example, among the large US exports by value to China, minerals such as copper ores and chemical products were hit more in the post-2018 retaliation.[45] It is also possible that China's tariff retaliation post-2018 may have resonated more with the electorate given its proximity to the 2020 election, which may have rallied more support for the incumbent's re-election effort.[46]

Overall though, we would stress that the findings in Tables A.13 and A.15 should be taken with a proverbial grain of salt, given that many other issues – not least the Covid-19 pandemic – could have interacted in nontrivial ways with voters' appraisal of the incumbent administration's trade policies in ultimately shaping voting outcomes.

5 Conclusion

This paper contributes to a broader body of evidence demonstrating how trade policies – and their apparent consequences for the economic well-being of voters – can shape domestic politics and electoral outcomes. The extensive use of tariffs by the Trump administration, and the tariff retaliation that this triggered, provide the particular context for this study. We presented evidence that greater county-level exposure to foreign retaliatory tariffs was associated with a decline in the Republican vote share and a loss of seats in the 2018 midterm House races; this is consistent with the negative impact of these tariffs on targeted farms and firms contributing to the swing against the party of the incumbent president. These effects were moreover concentrated in competitive counties where

[45] The changes in US tariffs post-2018 were: (i) the additional 15% levied on List 3 products under Section 301 on 1 June 2019; and (ii) the tariffs levied on List 4 products on 1 September 2019. For the retaliatory tariffs, China made several adjustments to its tariff schedule post-2018, but the key event was in 1 September 2019 when China upped its retaliation on a range of products in response to the List 4 tariff action; while some of these retaliatory tariffs were eased off in February 2020 as part of the Phase 1 agreement with the US, these tariffs as they stood in November 2020 for most products (including soybeans) were still at or above their level in November 2018.

[46] Separately, see Choi and Lim (2021) for a more detailed study of the impact of the MFP agricultural subsidies on voting in the 2020 Presidential election.

Trump narrowly lost the popular vote in 2016. On the other hand, where US tariffs extended more protection against imports, this did not appear to aid the Republican party's cause in the 2018 midterms, though there are signs that this did eventually provide a modest boost to Trump's vote share in his 2020 re-election bid. The 2018 agricultural subsidies offset some of the Republican loss in vote share during the midterms, but this was likely immaterial to the swing in House seats due to the narrow set of counties that benefited from these funds.

These findings are unlikely to be driven by selection on the basis of observables, as conditional on a comprehensive set of controls, the county-level tariff shock measures exhibit no common pre-trends with further lagged changes in the Republican vote share. We also report diagnostics that provide some reassurance that the tariff shocks' effects are unlikely to be driven by selection on the basis of unobservables. The relevance of the tariff war for explaining electoral outcomes holds even while we control explicitly for the roles of health insurance coverage and SALT deductions as central policy issues during the 2018 midterms. In a series of counterfactual simulations in which we aggregate county-level vote shares to congressional seat outcomes, our estimates suggest that the trade war itself can explain about ten Republican House seats lost; this is in a similar range to the number of seats that health care and SALT policies can each account for in the 'Blue Wave' of 2018. Given that the use of tariffs has persisted past 2020, one might naturally ask how (if at all) these tariffs might continue to affect political support for the two major parties. Our findings also raise the question of whether foreign governments' trade policy actions can broadly succeed in influencing domestic political outcomes. We would caution against over-generalizing our findings, given that the strength of such effects is likely to depend for example on whether the domestic or foreign government is perceived by voters as bearing more responsibility. Clearly though, these are interesting open questions for future research.

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A Appendix

A.1 Data Details

Tariff Rates: The information on the tariffs introduced by the Trump administration, and in response by the US' major trade partners, was collected by Bown (2021). These were compiled for product codes at the tariff line level. We include in our dataset all tariff actions enacted prior to the US midterm elections in early November 2018. For the tariff increases levied by the US, this covers the following: (i) on washing machines and solar panels, under Section 201, implemented in February 2018; (ii) on steel and aluminum, under Section 232, implemented in March and June 2018; (iii) on China, under Section 301, three separate lists of increases implemented in July, August and September 2018. (The tariffs implemented in July and August 2018 on China were the so-called "$50 billion tariff list", i.e., covering $50 billion of US imports; the subsequent increases in September 2018 covered an additional $200 billion of US imports from China.) For the retaliatory tariffs, we include the tariff responses enacted by Canada, Mexico, China, and the EU. These trade partners are the four largest sources of US imports, accounting for about two-thirds of the total value of US imports in 2017. For a comprehensive timeline of the US and retaliatory tariff actions, see Bown (2021).

County-level Tariff Shocks: We construct the county-level tariff shocks from the raw product-level tariff rate increases as follows. Let $\Delta\tau_p^{o,US}$ denote the tariff rate change imposed by the US on imports from country o in product p (at the HS 8-digit level). As a convention, the first superscript denotes the origin country of the trade flows being considered, while the second superscript denotes the destination country. Thus, $\Delta\tau_p^{US,d}$ refers instead to the retaliatory tariff rate change on US exports to country d in product p. Next, define $X_p^{o,d}$ to be the value of product-p trade flows from country o to country d in an initial pre-tariff-war year. We use 2017 trade data from the World Bank WITS database for all countries, except for Canada where the most recent available year was 2016. The impact of the product-p tariff increase in dollar terms is then captured as: $TS_p^{o,d} = X_p^{o,d}\Delta\tau_p^{o,d}$, this being the magnitude of tariff revenues that would be raised holding trade flows constant at their initial level.

We map these product-level shocks to US counties in two steps. We first map the HS 8-digit tariff shocks to NAICS industries, as the US county-level data on industry composition is based on NAICS codes. As explained below, we work with NAICS 3-digit industries for the non-farm agricultural sector, and with industry groups that are roughly at the NAICS 4- to 5-digit level of aggregation for farm agricultural industries; we index these NAICS industries that we work with by i. The mapping from HS8 codes p to NAICS industries i is drawn from Pierce and Schott (2009); we use the crosswalk for import HS codes when constructing the US Tariff Shock, while using the crosswalk for export HS codes when constructing the Retaliatory Tariff Shock. If a given HS8 code p is missing from the crosswalk, we identify the set of all NAICS industries i that HS8 codes that share the same 7-digit root as p map to; if there are no HS8 codes that share the same 7-digit root as p in the Pierce and Schott (2009) crosswalk, we successively use the 6-digit root of p, and so on until we can identify a non-empty set of NAICS industries i that p maps to. Let $S(p)$ denote the set of NAICS codes i that product p maps to on the basis of the above procedure. In practice, the majority of HS8 codes map to a unique NAICS code i, i.e., $|S(p)| = 1$ for most products p. If a HS8 code is mapped to multiple NAICS industries, we apportion $TS_p^{o,d}$ equally across all the relevant NAICS codes. We then compute the tariff shock levied by country d on trade flows from country o in NAICS industry i as: $TS_i^{o,d} = \sum_{p\in H(i)} \frac{1}{|S(p)|} TS_p^{o,d}$, where $H(i)$ is the set of products p that map into NAICS industry i.

The second step is to map the above industry-level tariff shocks experienced at the national level to US counties. We do so using county-level employment weights. For all NAICS 3-digit industries other than NAICS 111 ("Crop production") and 112 ("Animal production and aquaculture"), we draw on employment data from the 2016 County Business Patterns (CBP); this covers in particular the manufacturing sector. We use the version of the CBP data processed by Eckert et al. (2020), who fill in confidentiality-suppressed cells in the CBP with values imputed via adding-up constraints. For NAICS 111 and 112, the CBP does not cover employment at farm establishments, so we instead draw on the 2012 and 2017 US Census of Agriculture. For these census years, we obtain state-wide total employment in the following thirteen NAICS industry groups: Oilseed and Grain Farming (1111); Vegetable and Melon Farming (1112); Fruit and Tree Nut Farming (1113); Greenhouse, Nursery, and Floriculture Production (1114); Tobacco Farming (11191); Cotton Farming (11192); Sugarcane Farming, Hay Farming, Sugar Beet Farming, Peanut Farming, All Other Miscellaneous Crop Farming (11193/4/9); Beef Cattle Ranching and Farming and Cattle Feedlots (11211); Dairy Cattle and Milk Production (11212); Hog and Pig Farming (1122); Poultry and Egg Production (1123); Sheep and Goat Farming (1124); Aquaculture and Other Animal Production (1125/9). We then apportion these to counties, using the county share in total state-level sales in that agricultural NAICS industry group as weights. (An alternative would be to use the county share in state-wide agricultural employment as weights, though this has the drawback that the county-level employment data in the US Census of Agriculture are not broken down by industry. We obtain similar findings if we were to instead use these employment weights in the construction of the county-level tariff shocks; see Table A.9.) We linearly interpolate between these 2012 and 2017 estimates of county-level agricultural industry employment, to obtain a corresponding estimate for 2016.

The above yields data on the total employment in industry i and county c, $L_{i,c}$, and hence also on the total employment in that industry in the US, L_i. The county-level tariff shocks arising from the US tariff action and from the retaliatory tariffs are then respectively constructed as:

$$TS_c^{US} = \sum_o \sum_i \frac{L_{i,c}}{L_i} \frac{TS_i^{o,US}}{\bar{L}_c}, \text{ and} \tag{A.1}$$

$$TS_c^{R} = \sum_d \sum_i \frac{L_{i,c}}{L_i} \frac{TS_i^{US,d}}{\bar{L}_c}. \tag{A.2}$$

This apportions the tariff shock at the NAICS industry level to each county, according to the county's share of US employment in NAICS industry i, and then sums the tariff shock across all industries i

and US trade partner countries (respectively, o and d) being considered. We further divide by $\bar{L}_c$ – the total workforce size in that county, proxied for by the population aged 15-64, from the US Census county-level estimates for 2016 – to arrive at a per-worker effect for county c.

Note that the tariff shock measures as constructed are additive across industries i and partner countries (o or d). For the purposes of Table 5, for example, the retaliatory tariff shocks by partner country are obtained by restricting the first summation in equation (A.2) to the relevant subset of countries d; the retaliatory tariff shock by country and industry is obtained by further restricting the second summation to the subset of NAICS industries i that are of interest.

We have further constructed the US and Retaliatory Tariff Shocks for tariff actions implemented after the conclusion of the 2018 midterms, for use in the exploration in Table A.15. These are based on equations (A.1) and (A.2), except that the product-level changes used are the respective changes in the US and retaliatory tariffs enacted between November 2018 and November 2020, as reported in Bown (2021). We continue to use the same initial data as in our baseline measures for the employment shares ($L_{i,c}/L_c$) and working-age population ($\bar{L}_c$), as well as the same initial trade data (for the $X_p^{o,d}$ terms). Concretely, the changes in US tariffs during the post-2018 period are: (i) the additional 15% tariff placed on Section 301, List 3 products on 1 June 2019; and (ii) the tariffs levied on List 4 products on 1 September 2019. For the retaliatory tariffs, China made several adjustments to its tariff schedule post-2018, but the key event was in 1 September 2019 when China upped its tariffs on a range of products in response to the Section 301, List 4 tariffs; while some of these retaliatory tariffs were eased off in February 2020 as part of the Phase 1 agreement with the US, it was still the case that the retaliatory tariffs as they stood in November 2020 were at or above their level in November 2018. As an example, the retaliatory tariff rate on yellow soybeans (HS 12019010, one of the largest US export products to China) was 25% on the eve of the 2018 midterms, was bumped up to 30% on 1 September 2019, but reduced slightly to 27.5% in February 2020. The post-2018 US and Retaliatory Tariff Shocks that we construct have non-negative values for all counties.

Apart from the above Tariff Shock measures in dollar-per-worker terms, we also constructed an alternative pair of measures as follows based on a weighted-average of industry tariff rates:

$$TS_c^{US,alt} = \sum_i \frac{L_{i,c}}{L_c} \Delta\tau_i^{US}, \text{ and} \tag{A.3}$$

$$TS_c^{R,alt} = \sum_i \frac{L_{i,c}}{L_c} \Delta\tau_i^{R}. \tag{A.4}$$

These are weighted-averages of (respectively) the changes in the US and retaliatory tariff rates across NAICS 3-digit industries (indexed by i). The US tariff rate change for industry i is computed as: $\Delta\tau_i^{US} = (\sum_o \sum_{p\in H(i)} \frac{1}{|S(p)|} X_p^{o,US} \Delta\tau_p^{o,US})/(\sum_o \sum_{p\in H(i)} \frac{1}{|S(p)|} X_p^{o,US})$, where recall that $H(i)$ is the set of products p that map into NAICS industry i, and $|S(p)|$ is the number of NAICS codes i that product p maps to (this assumes an equal apportionment in cases where p maps to multiple industry codes i). In words, for a given industry i, the numerator sums over all the tariff revenue $X_p^{o,US}\Delta\tau_p^{o,US}$

that would in principle be collected on trade in product p between origin o and the US under the changes in product-level tariff rates, $\Delta\tau_p^{o,US}$; we then divide this by the total volume of trade (summing $X_p^{o,US}$ over all products p that map to i), to obtain an average tariff rate for the NAICS industry. We construct the retaliatory tariff rate change for industry i in an analogous manner as: $\Delta\tau_i^R = (\sum_d \sum_{p\in H(i)} \frac{1}{|S(p)|} X_p^{US,d} \Delta\tau_p^{US,d})/(\sum_d \sum_{p\in H(i)} \frac{1}{|S(p)|} X_p^{US,d})$, where the relevant sums are over US exports to destination countries d. The $\Delta\tau_i^{US}$'s and $\Delta\tau_i^R$'s are then aggregated into a measure of county c's exposure to the tariffs by using weights, $L_{i,c}/L_c$, that are equal to the share of NAICS industry i in county-c employment. These alternative tariff shock measures are used in Table A.9.

Upstream and Downstream County-Level Tariff Shocks: We use the 2012 US Input-Output Tables – specifically, the Use tables after redefinitions – to compute the county-level tariff exposure that occurs via upstream (respectively, downstream) production linkages. We first convert the NAICS industry tariff shocks (whose construction was described above) to tariff shocks based on the IO industry classification system, using the concordance that accompanies the 2012 US Input-Output Tables. Note that each non-farm NAICS 3-digit industry i maps to a unique IO 3-digit industry, while each of the industry groups for farm agricultural industries (NAICS 1111 through 1125/9) also maps to a unique IO industry at the 4- or 5-digit level.[47] We can therefore cleanly compute tariff shocks at the level of these IO industry groups; with a slight abuse of notation, we continue to use i to index these IO industry groups (i.e., comprising the IO 3-digit industries for non-farm sectors and the IO 4- or 5-digit industries for farm agriculture).

For a county's exposure to the tariff shocks through upstream production linkages, we compute:

$$upTS_c^{US} = \sum_o \sum_j \frac{L_{j,c}}{L_j} \sum_i a_{ij} \frac{TS_i^{o,US}}{\bar{L}_c}, \text{ and} \tag{A.5}$$

$$upTS_c^{R} = \sum_d \sum_j \frac{L_{j,c}}{L_j} \sum_i a_{ij} \frac{TS_i^{US,d}}{\bar{L}_c}. \tag{A.6}$$

Here, $a_{ij} = Z_{ij}/Y_i$ is the value of industry i output that is purchased by industry j, Z_{ij}, expressed as a share of total industry i output, Y_i. When computing a_{ij}, we follow Antràs et al. (2012) and apply a net exports and net inventories correction to the input use value Z_{ij}. The Z_{ij} entries capture flows of input use that occur through domestic cross-industry transactions. In practice, there are also flows where the inputs are from foreign sources (i.e., imports) or are drawn from domestically-held inventories. Under a standard proportionality assumption, that these latter flows of inputs are apportioned across purchasing industries in a manner similar to domestic cross-industry transactions, Antràs et al. (2012) show that one can account for these latter flows of inputs by multiplying Z_{ij} by a correction term equal to $Y_i/(Y_i - NX_i - NI_i)$, where NX_i denotes net exports and NI_i denotes net outflows from inventories for industry i.

[47]Specifically: NAICS 1111 maps to IO 1111; NAICS 1112 to IO 1112; NAICS 1113 to IO 1113; NAICS 1114 to IO 1114; NAICS 11191, 11192, 11193/4/9 to IO 1119; NAICS 11211 to IO 1121A; NAICS 11212 to IO 11212; NAICS 1122, 1124, 1125/9 to IO 112A; NAICS 1123 to IO 1123.

In equations (A.5) and (A.6), we therefore take the US tariff shock (respectively, retaliatory tariff shock) experienced by IO industry i, and apportion the dollar value impact on IO industries j that purchase inputs from i on the basis of the "allocation" coefficient a_{ij}, which captures how important j is as a user of output generated by industry i. This shock that is transmitted to industry j via upstream production linkages is then apportioned to county c, using the county-c share of national employment in industry j, $L_{j,c}/L_j$. Summing across industry pairs and dividing by the working age population, $\bar{L}_c$, we obtain an expression for the per worker effect in county c. $upTS_c^{US}$ and $upTS_c^R$ thus capture the exposure to respectively the US tariffs on imports and the retaliatory tariffs on US exports that is transmitted to county c via upstream production linkages.

We construct a county's exposure to the tariff shocks through downstream production linkages in an analogous manner:

$$dwTS_c^{US} = \sum_o \sum_i \frac{L_{i,c}}{L_i} \sum_j d_{ij} \frac{TS_j^{o,US}}{\bar{L}_c}, \text{ and} \tag{A.7}$$

$$dwTS_c^{R} = \sum_d \sum_i \frac{L_{i,c}}{L_i} \sum_j d_{ij} \frac{TS_j^{US,d}}{\bar{L}_c}. \tag{A.8}$$

Here, $d_{ij} = Z_{ij}/Y_j$ is the value of input purchases from industry i by industry j, expressed as a share of the buying industry j's output; Z_{ij} is once again calculated incorporating the net exports and net inventories correction. Equations (A.7) and (A.8) therefore take the US tariff shock (respectively, retaliatory tariff shock) experienced in the downstream IO industry j, and apportion the dollar value impact on industries i on the basis of this "direct requirements" coefficient d_{ij}, which captures how large of a shock might be transmitted to industry i through j's purchases of inputs from industry i. This shock that is transmitted to industry i via downstream production linkages is then apportioned to county c, using the county-c share of national employment in industry i, $L_{i,c}/L_i$. We then sum across industries pairs and divide by the working age population, $\bar{L}_c$, to obtain an expression for the per worker effect in county c. $dwTS_c^{US}$ and $dwTS_c^R$ thus capture the exposure to respectively the US tariffs on imports and the retaliatory tariffs on US exports that is transmitted to county c via downstream production linkages.

As with the baseline county-level tariff shocks, the upstream and downstream county-level tariff shocks are also additive across industries (i) and partner countries (o or d).

Market Facilitation Program (MFP) Agricultural Subsidies: We estimate the total subsidies received by each county under the 2018 MFP, by combining information on: (i) the announced subsidy rates by commodity, and (ii) the initial output or inventory of each county by commodity. On (i), the subsidy rates are taken from the Congressional Research Service report on "Farm Policy: USDA's 2018 Trade Aid Package" (19 June 2019 update); see in particular Table 2 of the report. The set of commodities we consider and their associated subsidy rates are: Soybeans (\$1.65 per bushel), Hogs (\$8.00 per head of inventory), Cotton (\$0.06 per pound), Sorghum (\$0.86 per bushel), Milk

($0.12 per hundred pounds), Wheat ($0.14 per bushel), and Corn ($0.01 per bushel). On (ii), we use annual county-level crop output data from 2017, from the US Department of Agriculture's National Agricultural Statistics Service. The two exceptions are: Hogs, where we use annual inventory data from 2017, and Milk, where we use 2012 output data as that is the most recent available year. This covers all agricultural commodities included in the MFP, except the two smallest commodities by total output – Fresh sweet cherries and Shelled almonds – for which county-level output data are not available. The MFP subsidy per worker is the estimated total subsidy disbursed – summed across all commodities – to each county, divided by the county population between ages 15-64, $\bar{L}_c$ (from the US Census, 2016 population estimates).

We cross-check the validity of our MFP subsidy estimates against information on actual disbursements, from the Environmental Working Group's (EWG) Farm Subsidy Database. The EWG obtained information on MFP subsidy disbursements from the US Department of Agriculture through a Freedom of Information Act request, and has made publicly available a list of the largest beneficiaries from the 2018 MFP subsidies. Using the "Top Recipients" file as of October 2018, and aggregating the available data up to the commodity by state level, we find a positive correlation of 0.79 between the EWG data and the MFP subsidy estimates that we have computed.

For the extension in Tables A.13-A.15, we construct an analogous post-2018 MFP subsidy per worker variable for US counties using the 2019 MFP subsidy rates for key agricultural commodities reported in GAO (2020). The 2019 rates were more generous than in 2018 across the set of commodities: Soybeans ($2.05 per bushel), Hogs ($11.00 per head of inventory), Cotton ($0.26 per pound), Sorghum ($1.69 per bushel), Milk ($0.20 per hundred pounds), Wheat ($0.41 per bushel), and Corn ($0.14 per bushel). The MFP program was not extended into 2020.

Election Data: From David Leip's Atlas of U.S. Presidential Elections (available at the website: https://uselectionatlas.org), for the 2008, 2010, 2012, 2014, 2016, 2018, and 2020 House elections, as well as the 2008, 2012, 2016, and 2020 Presidential elections. This provides information on: (i) voting results at the county level, as well as at the county-by-CD level; and (ii) turnout at the county level (for the states where this is reported).

Employment shares: Employment data for non-farm agricultural sectors (i.e., excluding NAICS 111 and 112) are from the 2016 and 2013 County Business Patterns; we use the version of this data processed by Eckert et al. (2020). Employment data for the farm agricultural industries (i.e., industries under NAICS 111 and 112) are based on the 2012 and 2017 US Census of Agriculture. The construction of county-level estimates of employment across different farm industries was described above in the construction of the county-level tariff shocks. We obtain estimates for 2016 and 2013 by linearly interpolating between the 2017 and 2012 estimates. The agriculture, mining, and manufacturing sectors are defined respectively as the NAICS industries with leading digit 1, with leading digits 21, and with leading digit 3. In the regressions, the employment shares from 2016 are used as initial controls, while the corresponding changes in shares in 2016 relative to 2013 are used as pre-trend controls.

Demographics: From the US Census Bureau, 2016 and 2013 estimates of county population by characteristics. We use the population shares by age group (25-34, 35-44, 45-54, 55-64, 65 and over), by gender (female), and by race (black, white non-Hispanic, Hispanic). In the baseline regressions, the population shares from 2016 are used as initial controls, while the corresponding changes in shares in 2016 relative to 2013 are used as pre-trend controls. From the US Census Bureau, we also obtain county-level information on the population share located in urban areas, available for the year 2010. We include this in the regressions as an initial control.

Unemployment rate, Mean household income, Education, Health insurance coverage: From the American Community Survey, five-year averages for 2013-2017 and for 2008-2012. The education variables used are the share of the population aged ≥ 25 with respectively: (i) less than high school education; and (ii) some college and above. The health insurance variable used is the share of the civilian noninstitutionalized population with health insurance. In the baseline regressions, the five-year averages for 2013-2017 are used as initial controls, while the corresponding changes in five-year averages in 2013-2017 relative to 2008-2012 are used as pre-trend controls.

SALT: From the US Internal Revenue Service county-level tax statistics. Computed as the sum of state and local income taxes, sales taxes, and real estate taxes paid, divided by the number of returns filed with the IRS. In the regressions, we use the information from 2016 for our initial controls, while using the corresponding change in 2016 relative to 2013 for our pre-trend controls.

Cross-county mobility: From the US Census Bureau's county population estimates, three measures are obtained that speak to movements of people across county borders: (i) the change in population between 2016-2019; (ii) the domestic net migration rate in 2019; and (iii) the overall net migration rate in 2019. Note that the last two variables are components used in the computation of the Census Bureau's annual county population estimates, and are directly reported as part of those estimates; migration is defined as movements of people relative to the prior year. The net migration rate in a given year is calculated as the number of in-migrants minus the number of out-migrants, divided by the sum of the numbers of non-migrants and out-migrants. The domestic net migration rate is constructed using only counts of migrants who are moving across US locations.

A.2 Supplementary Figures

Figure A.1 below highlights the set of counties considered to be electorally competitive during the 2018 midterms, on the basis of the Republican vote share received in the 2016 Presidential election. Counties where the Trump vote share in 2016 was: (i) between 40-50%; and (ii) between 50-60%, are shaded in.

Figure A.1: Swing Counties

(2016 Republican Presidential Vote Share $\in [.4, .6]$)

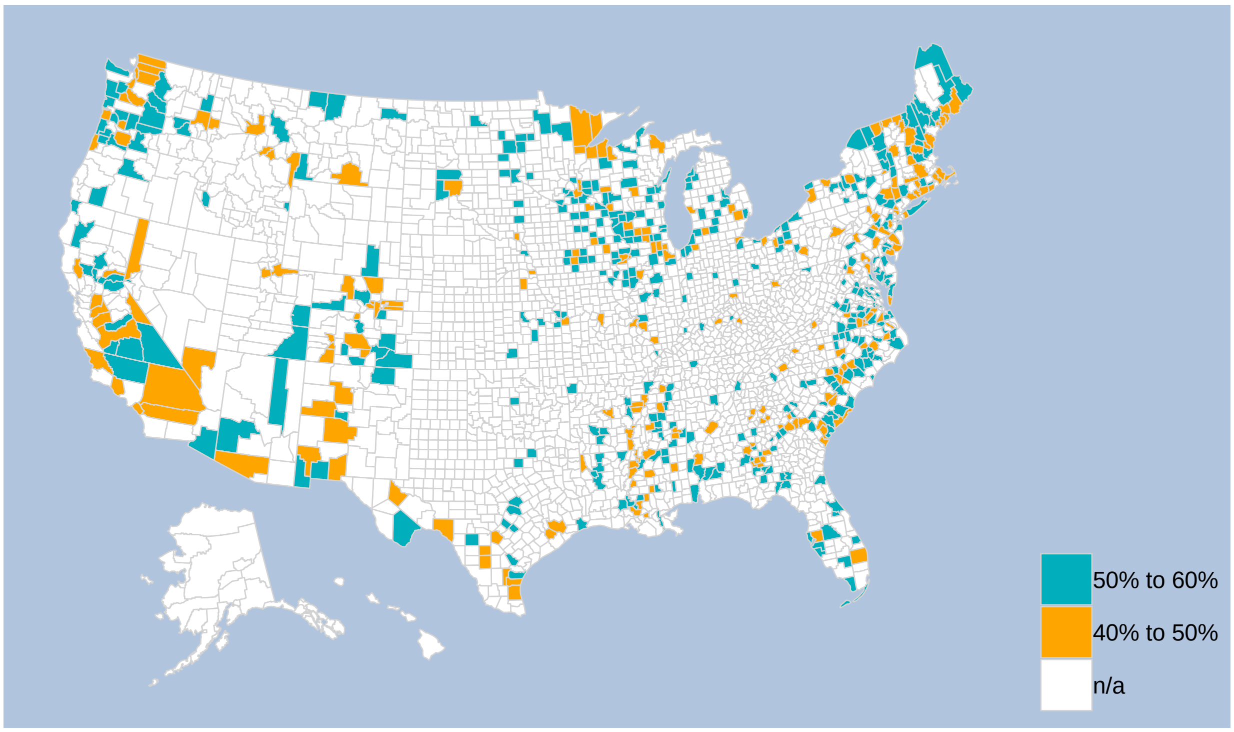
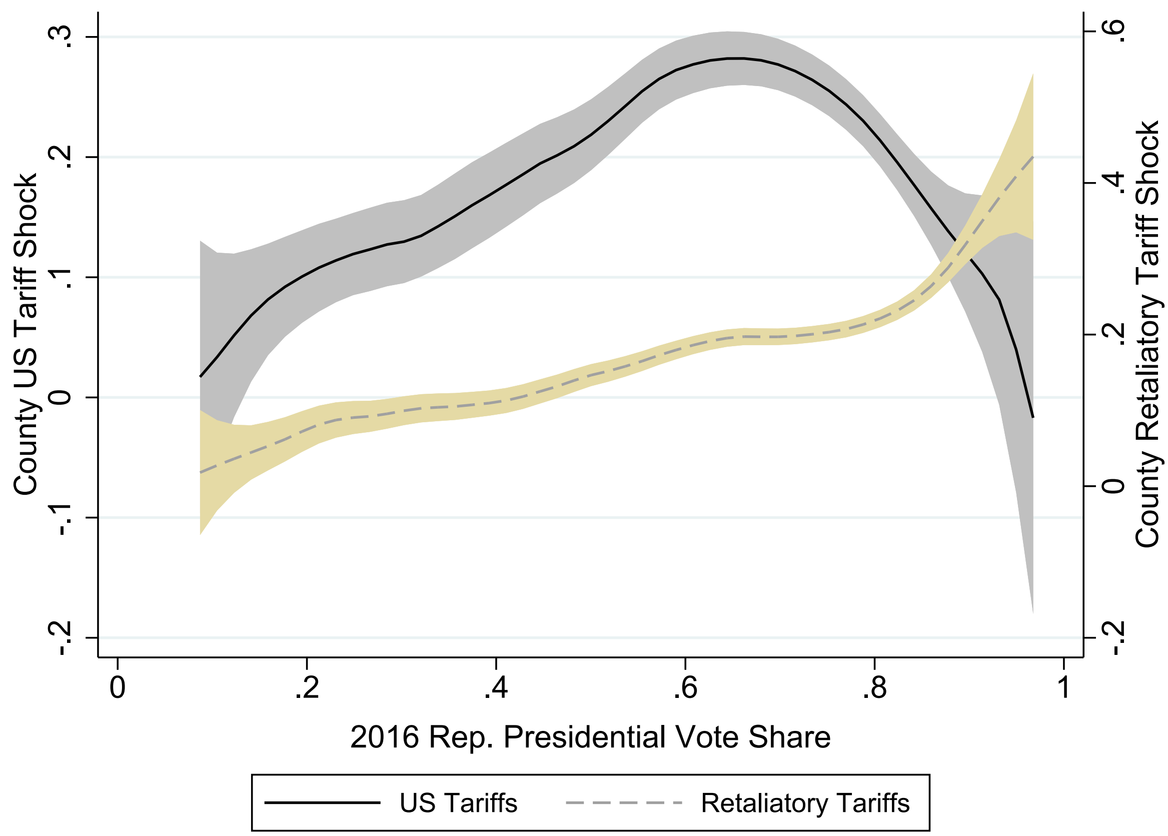


Figure A.2 illustrates how the county-level US Tariff Shock and the Retaliatory Tariff Shock, constructed as defined in equations (1) and (2), vary with the county-level vote share received by Trump in the 2016 Presidential election. The figure plots local polynomial regression estimates with their associated 95% confidence intervals. The qualitative patterns documented in Fajgelbaum et al. (2020) are replicated with the tariff shocks we have constructed in dollar-per-worker terms: The US Tariff Shock exhibits an inverted U-shaped relationship with the county share of votes received by Trump in 2016. On the other hand, the Retaliatory Tariff Shock increases monotonically with the 2016 Trump vote share.

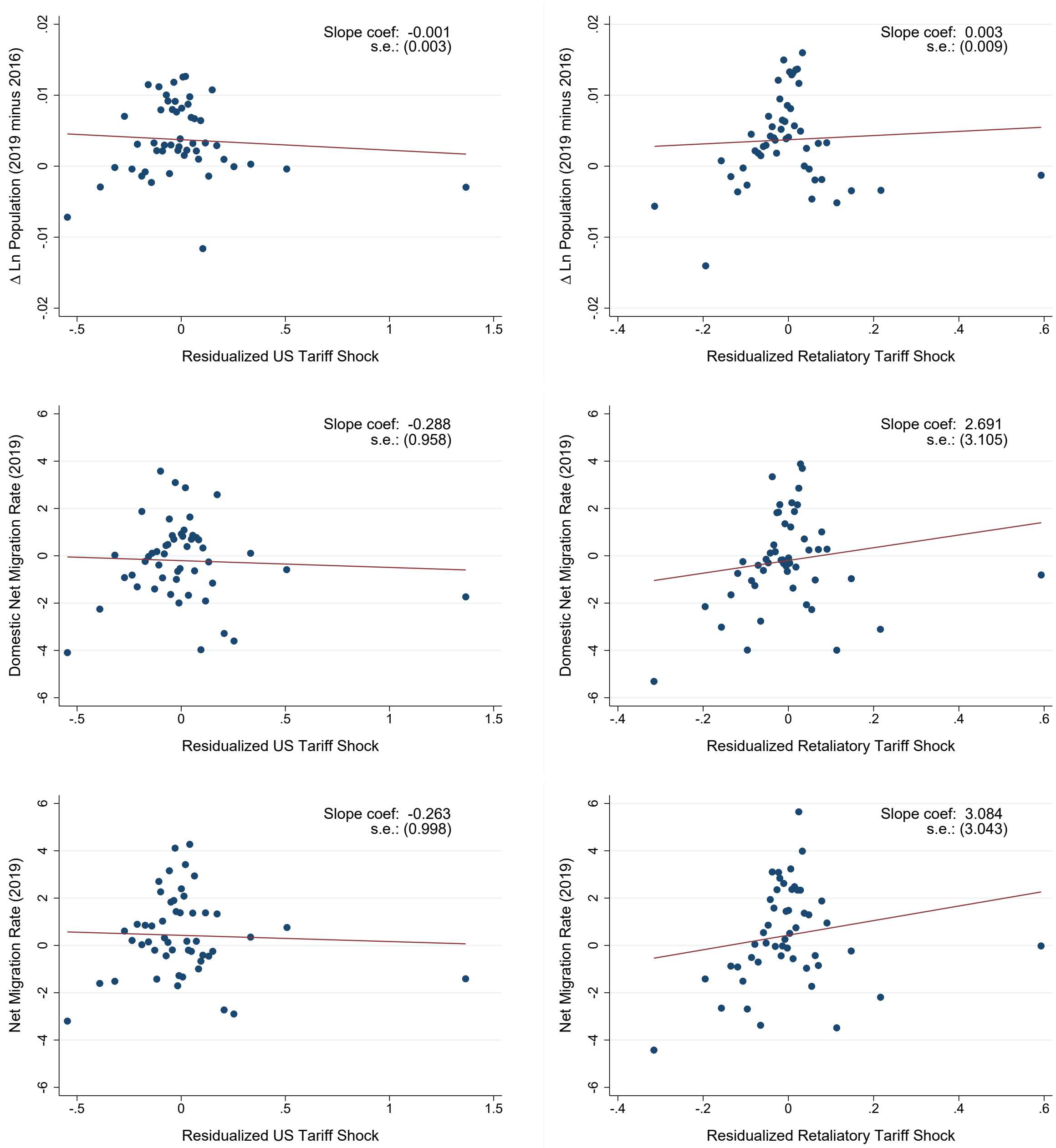
Figure A.2: Tariff Shocks and the 2016 US Presidential Election Vote Share



Notes: Local polynomial regression estimates are plotted with 95% confidence intervals, for the relationship between the US Tariff Shock (respectively, Retaliatory Tariff Shock) and the two-party Republican vote share in the 2016 Presidential election.

The residualized binned scatterplots in Figure A.3 illustrate that the US and Retaliatory Tariff Shocks are uncorrelated with contemporaneous measures of mobility across counties, namely: the log change in population between 2016-2019 (first row); the domestic net migration rate in 2019 (second row); and the net migration rate in 2019 (third row).

Figure A.3: Binned Scatterplots: Tariff Shocks and Cross-County Mobility Measures



Notes: Based on the regression specification in Column 4, Table 2. Each y- and x-variable is first residualized by the set of right-hand side variables (excluding the US Tariff Shock, Retaliatory Tariff Shock, and Retaliatory Tariff Shock $\times$ Ag. Subsidy interaction) in this Column 4 specification, as well as the respective lagged mobility variable, while weighting by 2016 county population. Based on 50 bins of each x-variable, after computing the mean of the y- and x-variables within each bin. The slope coefficient of the best-fit line is reported, with robust standard errors.

Figure A.4 illustrates heat maps for the US Tariff Shock. The first row presents these for the US Tariff Shock stemming respectively from the non-Section 301 and Section 301 tariffs. The corresponding upstream and downstream US Tariff Shock measures ($upTS_c^{US}$ and $dwTS_c^{US}$) are illustrated in the second and third rows respectively.

Figure A.4: Upstream and Downstream US Tariff Shocks by County ($1,000s per worker)

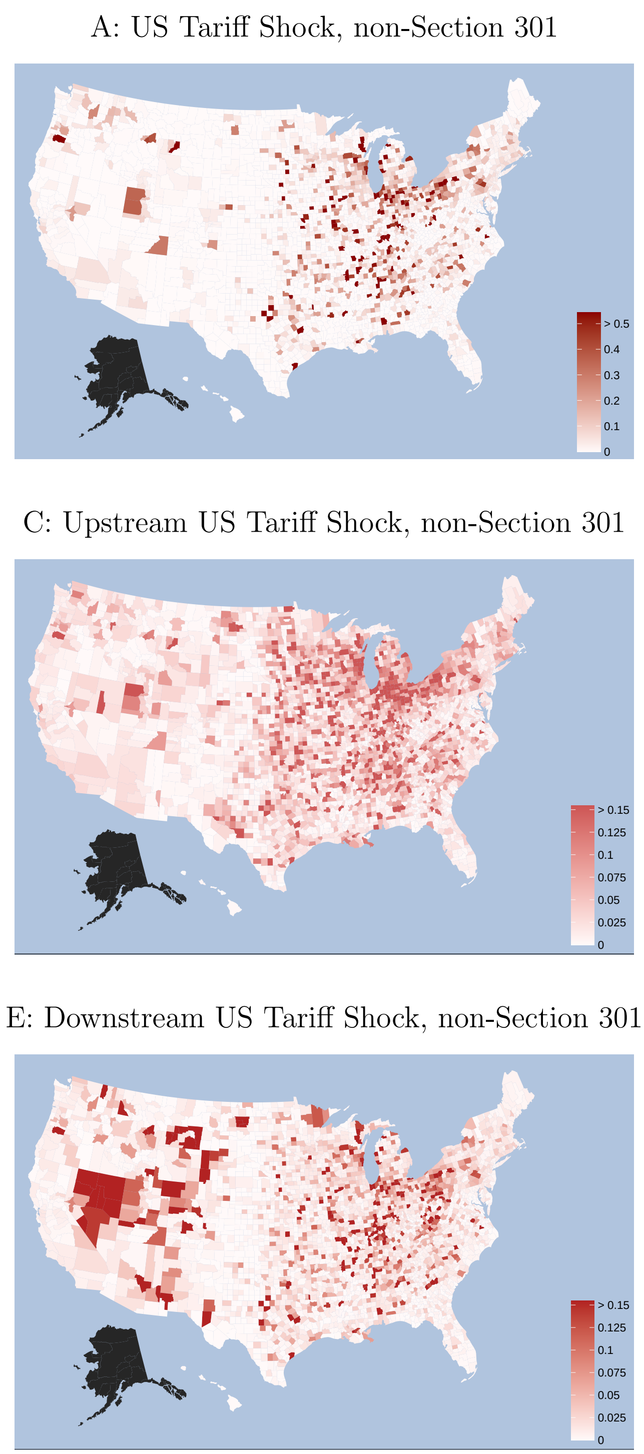
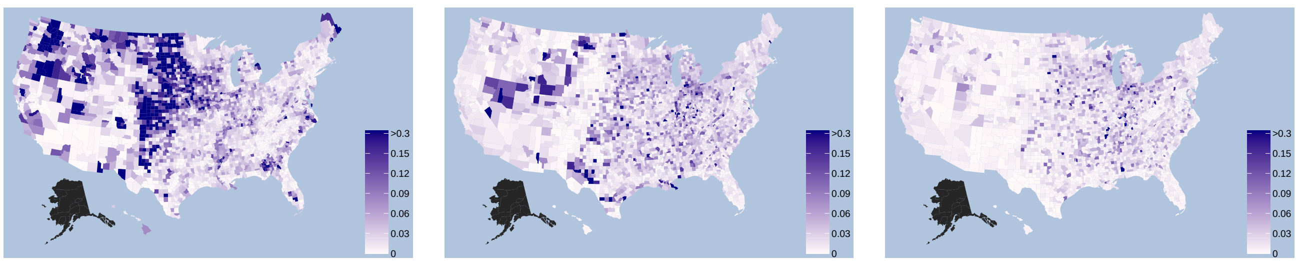


Figure A.5 illustrates heat maps for the Retaliatory Tariff Shock. The first row presents these for the Retaliatory Tariff Shock stemming respectively from China's tariffs on agricultural products, from China's tariffs on non-agricultural products, and from the non-China tariffs (i.e., levied by CAN/MEX/EU, on all products). The corresponding upstream and downstream Retaliatory Tariff Shock measures ($upTS_c^R$ and $dwTS_c^R$) are illustrated in the second and third rows respectively.

Figure A.5: Upstream and Downstream Retaliatory Tariff Shocks by County ($1,000s per worker)

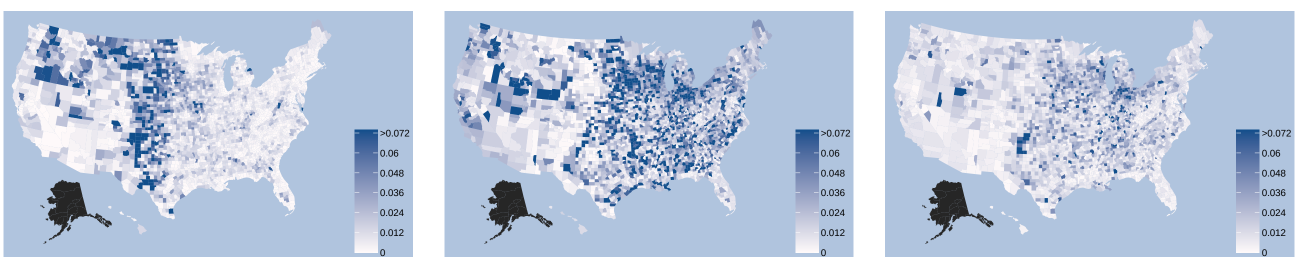
Retaliatory Tariff Shock:

A: CHN Ag. B: CHN non-Ag. C: CAN/EU/MEX



Upstream Retaliatory Tariff Shock:

D: CHN Ag. E: CHN non-Ag. F: CAN/EU/MEX



Downstream Retaliatory Tariff Shock:

G: CHN Ag. H: CHN non-Ag. I: CAN/EU/MEX

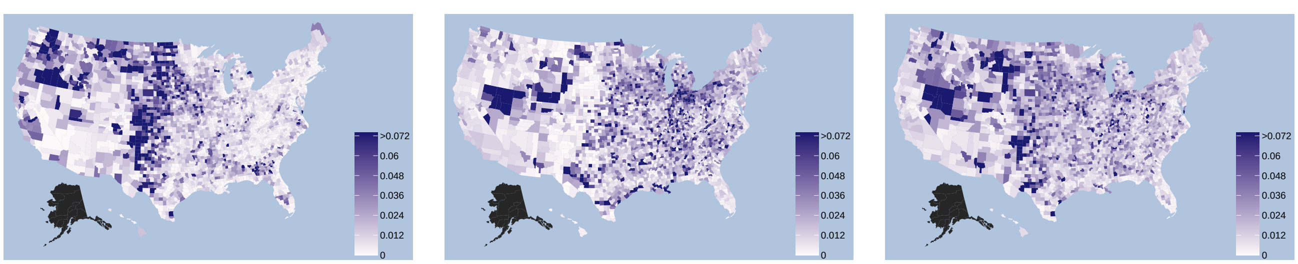
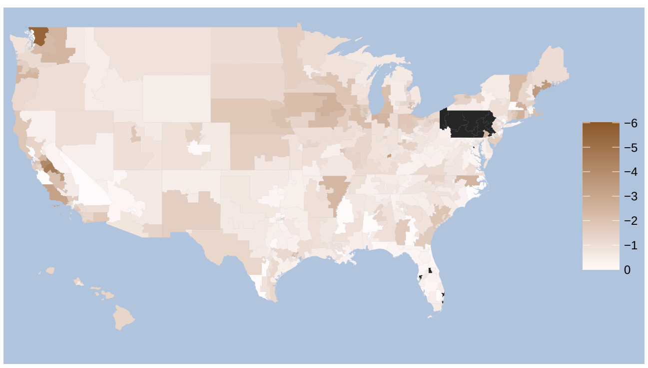


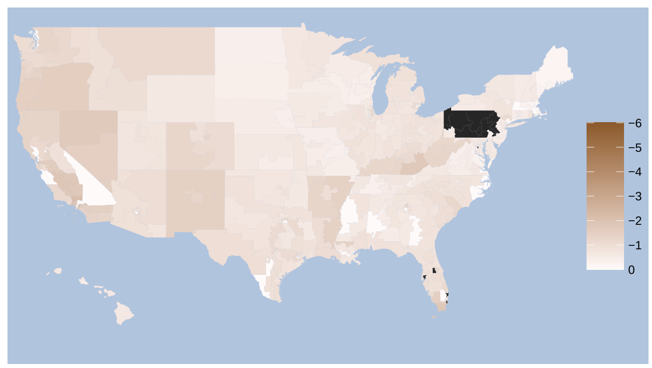
Figure A.6 is a Congressional District map that illustrates the predicted decrease in Republican House vote share between 2018 and 2016, under various counterfactual scenarios. These are computed from predicted county-level vote share changes, using the 'non-uniform' party-specific county-by-CD weights from 2018 (described in Section 4.4) to apportion the votes for counties that are split across multiple CDs. Panel A removes the effects related to both the retaliatory tariffs and agricultural subsidies; this is scenario (i) from Section 4.4. Panel B removes the effects related to gains in health insurance coverage, i.e., scenario (iii). Panel C removes the effects due to the SALT quintile dummies, i.e., scenario (iv). The map excludes Pennsylvania, where there was substantial redistricting of CD boundaries, and Hawaii, where both CDs remained firmly in the Democratic column.

Figure A.6: Counterfactual Vote Share Changes

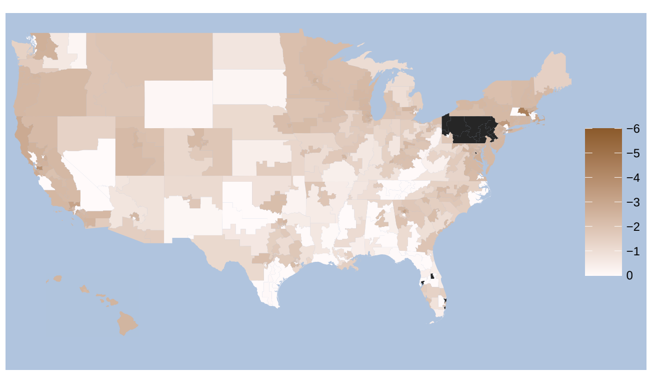
A: Remove Retal. tariffs & Ag. subsidies



B: Remove health insurance gains



C: Remove SALT effects



Notes: The CD-level Republican vote share changes between '18 and '16 are from the counterfactual exercise in the last row of Panel B of Table 6; the reported share of Republican (respectively, Democratic) votes within a county accounted for by each county-by-CD in 2018 is used to break up the Republican (respectively, Democratic) predicted vote at the county level, before aggregating to the CD level. The scenarios considered are: both the retaliatory tariff shock and agricultural subsidies are set to zero (Panel A); the five-year average gains in health insurance coverage are removed (Panel B); and the effects of being in a fourth or fifth quintile SALT county are removed (Panel C). The vertical scale in each panel reports percentage-point vote share changes. Uncontested CDs and CDs in Pennsylvania are shaded in black.

A.3 Further Details: Robustness

In this appendix section, we list the full set of appendix tables that have been included and elaborate on the robustness checks performed on our baseline analysis.

Table A.1 presents cross-county summary statistics for the full set of control variables used in the regression specification in (3).

In Table A.2, Panel A reports further summary statistics for the various additive components of the US and Retaliatory Tariff Shock measures (by tariff round, by trade partner country, or by broad sector). It also reports summary statistics of the corresponding tariff exposure measures arising from upstream and downstream production linkages. Panels B-D report on pairwise correlations across the various components of the US and Retaliatory Tariff Shock variables, as well as the corresponding upstream and downstream exposure measures.

Table A.3 performs a balance test following Borusyak et al. (2022), after recasting all variables to the NAICS 3-digit industry level. We construct US and Retaliatory tariff shocks for industry i as: $TS_i^{US} = (\sum_o \sum_{p \in H(i)} \frac{1}{|S(p)|} X_p^{o,US} \Delta\tau_p^{o,US})/L_i$ and $TS_i^R = (\sum_d \sum_{p \in H(i)} \frac{1}{|S(p)|} X_p^{US,d} \Delta\tau_p^{US,d})/L_i$; these are the industry analogues of the US and Retaliatory tariff shocks in dollar-per-worker terms.[48] Panel A of Table A.3 reports summary statistics for these industry-level shocks.

Panel B examines whether these shocks are balanced with respect to an exposure-weighted average of initial county-level characteristics (v_c's), after conditioning out variation that can be attributed to the observables already included in our baseline regression. To do so, we obtain the residuals $\tilde{v}_c$ from a cross-county regression of each v_c on the set of right-hand side variables in the Column 4, Table 2 specification (excluding the US Tariff Shock, Retaliatory Tariff Shock, Retaliatory Tariff Shock × Ag. Subsidy, and Ag. Subsidy), while weighting by 2016 county population. Next, define: $s_{i,c} = L_{i,c}/\bar{L}_c$ to be the exposure of county c to the industry-level shocks TS_i^{US} and TS_i^R; e_c to be the 2016 county population (the weights in the baseline regression); and $s_i = \sum_c e_c s_{i,c}$. Borusyak et al. (2022) show that the relevant exposure-weighted average of $\tilde{v}_c$ for the purposes of this balance test is: $s_i \phi_i$, where $\phi_i = \sum_c (e_c s_{i,c} \tilde{v}_c)/\sum_c (e_c s_{i,c})$. In practice, we perform a regression of ϕ_i against TS_i^{US} (respectively TS_i^R), using s_i as regression weights.

Panel B reports the estimated coefficients, with cluster-robust standard errors. For the v_c's, we consider a series of variables that could in principle affect the tariff protection or retaliation received by a county, but which are not already conditioned for in the estimating equation (3). We consider: (i) longer pre-trends in voting outcomes (the 2010-2012 change in the Republican House vote share, the 2008-2012 change in the Republican presidential vote share); and (ii) initial levels of voter support (the 2016 Republican House and presidential vote shares). We also look at: (iii) levels and pre-trends in a labor market outcome which we have not controlled for in Table 2, namely: the log average manufacturing weekly wage in 2016 and its change between 2013-2016.[49] Last but

[48]Recall that $H(i)$ is the set of products p that map into NAICS industry i, and $|S(p)|$ is the number of NAICS codes i that product p maps to; we assume an equal apportionment in cases where p maps to multiple industries i.

[49]This data are drawn from the BLS Quarterly Census of Employment and Wages.

not least, we also examine: (iv) longer-term changes between 2006-2016 in the employment shares in Agriculture, Mining, and Manufacturing; this is to guard against the possibility that the 2016 level and the 2013-2016 change in these variables (which we already control for) might not be adequate in capturing the role that county-level employment composition could play in influencing the extent of tariff protection or retaliation received. None of the estimated coefficients is significant at the 10% level, providing a degree of reassurance on the (conditional) exogeneity of the US and Retaliatory tariff shifters.

In Table A.4, we show that each of the three measures of cross-border mobility we consider are uncorrelated with the US and Retaliatory Tariff Shock measures. The three county-level measures are: (i) the change in population between 2016-2019; (ii) the domestic net migration rate in 2019; and (iii) the overall net migration rate in 2019. (Migration is defined as movements observed relative to the previous year.) Columns 1-3 run the specification in (3) using in turn each of these mobility measures as the dependent variable. These are indeed uncorrelated with the US and Retaliatory Tariff Shocks, conditional on the lagged variables and pre-trends that we control for in (3), as well as pre-trends in the respective mobility measures. (We control for mobility pre-trends with the change in county population between 2013-2016 for (i), and the respective net migration rates in 2016 for (ii) and (iii).) Columns 4-6 further verify that our baseline results on the effects of the Tariff Shocks on changes in the Republican House vote share in the 2018 midterms are robust to controlling for the respective lagged mobility measures (that are included as pre-trend controls in Columns 1-3).

Table A.5 reproduces Columns 1-4 from Table 2 in the main paper, while reporting the estimated coefficients for the full set of control variables.

Table A.6 examines the robustness of the baseline regression results – from Column 4 of Table 2 – under various county sample restrictions. Column 1 drops counties in the state of Pennsylvania, where the boundaries of congressional districts (CDs) were substantially redrawn in 2018. Column 2 drops all counties that are split across multiple CDs, since the Republican vote share in such counties aggregates across support for Republican candidates in more than one House race. Column 3 drops counties in CDs where either the 2016 or 2018 House race was uncontested by either party. Column 4 drops all counties in CDs in which there was an open seat vacated by the incumbent either due to outright retirement, a departure to seek higher office, resignation, or death. The information on open seats was taken from the Wikipedia page on "2018 United States House of Representatives elections", and cross-checked against The Atlantic's 2018 Congressional Retirement Tracker. Column 5 drops counties in which there were instances of a rematch between the same Republican and Democratic candidates as in the 2016 House race, since the forces that determine the popularity of candidates in such races could be different from that in a fresh match-up. Note that there were only 20 seats that saw an exact candidate rematch in 2018.

Across all five columns of Table A.6, we obtain a negative and significant effect of the Retaliatory Tariff Shock on the change in the Republican vote share. The interaction term with the agricultural subsidies variable is moreover positive and significant, except in Column 5 where this effect loses statistical significance marginally. The role of SALT deductability as an election issue also holds under each of these sample restrictions, particularly for counties in the highest SALT quintile. Interestingly, the change in five-year averaged health insurance coverage loses statistical significance in Columns 2 and 4. This suggests that health insurance policy was particularly relevant for explaining the shift against Republican candidates in split counties and open-seat counties, which are often viewed as being more electorally-competitive. (Counties that are not split across CDs are typically rural and hence Republican-leaning. Also, counties in which an incumbent is not running for re-election in principle provide a more level playing field for the contesting candidates.)

Table A.7 varies the set of control variables. Column 1 controls additionally for a proxy of the capital-intensity of county economic activity. To the best of our knowledge, county-level measures of the stock of physical capital (along the lines of that reported in national income accounts data) are not readily available. We instead pursue an alternative approach, to compute the share of workers in each county who are employed in industries that can be deemed to be of high capital-intensity. The underlying idea is that counties better endowed with physical capital would in principle attract more capital-intensive manufacturing activity, and so would feature a greater share of their local workforce being employed in high capital-intensity industries. To operationalize this, we use the NBER CES Manufacturing Industry Database to calculate a measure of capital-intensity – the real physical capital stock per worker between 2011-2015 – for NAICS 6-digit industries. We then merge this with the County Business Patterns data in Eckert et al. (2020), to compute the county share of manufacturing employment that is in industries with an above-median capital-intensity. Column 1 shows that our results are robust to controlling for the 2016 level and the 2013-2016 change in this county-level capital-intensity proxy.

Column 2 controls for longer pre-trends. For the demographic shares and sectoral employment shares, we control for the 2006-2016 changes. For socioeconomic characteristics obtained from the American Community Survey (ACS), including educational attainment shares, we control for the change between the 2006-2010 and 2013-2017 five-year estimates. (Note that the five-year estimates of most ACS data series commence in 2006-2010.) Our results are similar when using these longer pre-trend controls in the regressions (in lieu of the corresponding 2013-2016 changes, or the 2008-2012 to 2013-2017 five-year estimate changes). Separately, Column 3 verifies that our baseline results are robust when using sectoral employment share variables that are normalized by the county working age population, i.e., between 15-64, rather than by total sectoral employment.

Table A.8 uses the change in the Republican House vote share in 2018 relative to 2014 as the dependent variable. The vote share from the House elections in the previous midterm arguably provides a better point of comparison, as the Presidential election could have shaped down-ballot voting in 2016. We show that the baseline findings from Columns 1-4 in Table 2 are unchanged if we use the four-year vote share change as the outcome variable instead. (Note that in this specification, the lagged Republican House vote share changes that we control for are also four-year lagged changes.)

Table A.9 considers alternative constructions of the tariff shock and agricultural subsidy explanatory variables. A concern here is that our results might be driven by a small number of counties that experienced especially large exposure to the tariffs or agricultural subsidies. We therefore top-code the US Tariff Shock, Retaliatory Tariff Shock, and MFP Subsidy per worker variables at their 95th percentile values. As Column 1 confirms, this does not affect our key results.

On a separate note, our baseline estimates of employment by farm-agriculture industry, constructed from the US Census of Agriculture, used the county share in total state-level sales by industry to apportion that industry's total state-wide employment to individual counties. As discussed earlier in Section A.1, an alternative would be to use the county share in state-wide agricultural employment as weights, although this has the disadvantage that the county-level employment data are not broken down by farm industry. Using these alternative farm-agriculture industry employment estimates instead to construct the US and Retaliatory Tariff Shock measures, we obtain similar results in Column 2: The effect of exposure to the retaliatory tariffs is negative and strongly significant, though the interaction coefficient with agricultural subsidies is now marginally insignificant.

In Column 3, we show that our key conclusions are unchanged when using the weighted-average tariff rate change measures – defined in equations (A.3) and (A.4) in Section A.1 – in lieu of the Tariff Shocks that have been computed in dollar-per-worker terms. In Column 4, we consider an alternative "Full trade value" construction of the tariff shocks. Specifically, we replace the product-level tariff shock with $TS_p^{o,d} = X_p^{o,d}$ when constructing the US and Retaliatory Tariff Shock variables; this is in contrast to the baseline, where $TS_p^{o,d}$ was obtained by multiplying the initial value of trade by the tariff rate increase (i.e., $TS_p^{o,d} = X_p^{o,d}\Delta(\tau_p^{o,d})$). This approach takes the stance that the entirety of the value of trade flows could potentially be affected by the presence of the tariff, rather than scaling this by the imposed tariff rate change. The key finding on the negative impact of the Retaliatory Tariff Shock remains unaffected under this alternative construction.

Column 5 explores the potential for spillover effects – from tariff shocks experienced in other parts of a county's commuting zone (CZ) – to influence voting in the county. We construct the "Rest-of-CZ tariff shock" measure by aggregating over the US (respectively, Retaliatory) Tariff Shocks in the CZ, but outside the county in question. Specifically, we sum the $\frac{L_{i,c}}{L_i} TS_i^{o,d}$ terms in equations (1) and (2) across all counties within the CZ, except the county c in question; we then divide by the total working age population (between ages 15-64) located in the CZ, but outside the county c. (The mapping of counties to CZs is from: https://www.ddorn.net/data/cw_cty_czone.zip.) The relevance of the county-level Retaliatory Tariff Shock holds up even when we account for the tariff exposure of the rest of the CZ; the rest-of-CZ tariff shocks themselves do not display a significant relationship with voting outcomes.

Table A.10 works with versions of the tariff shock variables in which we use a more detailed HS8-to-NAICS industry concordance. Our baseline approach concords HS8 digit-product level tariff shocks to NAICS 3-digit industries. Constructing the measures with a concordance to more disaggregate NAICS industries is in principle useful since the 3-digit level of aggregation may obscure interesting variation. However, there is the potential drawback that the employment data for more detailed

NAICS industries may be subject to more measurement error as there are more confidentiality-suppressed cells in the County Business Patterns dataset that need to be inferred. We first reproduce the OLS specification in (3) using US and Retaliatory Tariff shock measures that are constructed respectively using concordances from HS8 products to NAICS 4-digit and NAICS 6-digit industries. (Note that we retain the same set of thirteen NAICS industry groups for farm agriculture throughout these exercises, and apply the more detailed concordances for non-farm NAICS industries only.) Columns 1 and 3 of Table A.10 suggest that there is some attenuation of the tariff shock coefficients toward zero. In Columns 2 and 4, we therefore instrument the US and Retaliatory Tariff Shocks in Columns 1 and 3 respectively using the baseline tariff shock variables (constructed using the concordance to NAICS 3-digit industries); this restores the statistical significance of our key findings.

Table A.11 examines if the role of health insurance gains in shaping the Republican vote share might be heterogeneous across the electoral competitiveness bins. We find here that the negative and significant effect from greater gains in health insurance coverage are concentrated in counties where the 2016 Trump vote share was in the 50-60% range, these being swing counties that Trump narrowly carried in 2016.

Table A.12 reports on the effects of the tariff war on turnout in the 2018 midterm elections. The turnout data is from the Election Atlas, but as discussed in Section 4.5 in the main paper, the available information on turnout is less comprehensive and subject to criticisms about comparability across states. The regressions here show that counties more exposed to the US tariffs saw an increase in turnout both relative to 2016 (Columns 1-3), and relative to 2014 (Columns 4-6). On the other hand, turnout was dampened in counties that experienced more tariff retaliation.

Table A.13 studies voting outcomes in the 2020 elections. Columns 1-3 show that the tariff and subsidy policies enacted prior to the 2018 midterms – i.e., the baseline US Tariff Shock, Retaliatory Tariff Shock, and MFP subsidy per worker measures – do not exhibit any significant relationship with the change in Republican House vote share between 2018 and 2020. Columns 4-6 however show that these variables were consequential for changes in the Trump vote share between 2016 and 2020: The US Tariff Shock was associated with a vote share swing in favor of Trump in the 2020 presidential elections, while the Retaliatory Tariff Shock had the opposite effect.

Table A.14 performs checks related to the analysis in Table A.13. Columns 1 and 2 confirm that conditional on the included controls, the tariff shocks are uncorrelated with further lags of Republican vote share changes, specifically for the House election in 2010 relative to 2008 and the Presidential election in 2012 relative to 2008. This provides reassurance that the tariff shocks are uncorrelated with further pre-trends in county-level support for the Republican party. Columns 3 and 4 demonstrate that the results in Table A.13 continue to hold when using four-year changes in the Republican vote share, instead of the two-year changes; here, note that we use four-year lagged changes for the control variables as well.

Table A.15 re-runs the specifications in Table A.13 further including the US Tariff Shock, Retaliatory Tariff Shock, and MFP subsidy per worker variables constructed for the policy changes enacted after the 2018 midterms. We continue to find that the policies implemented prior to the 2018 midterms did not affect the Republican vote share in the 2020 House races, but that there was a carry-over effect in the presidential race: Counties that had greater US protection from prior to the 2018 midterms delivered a larger increase in the Trump vote share, while the converse was true for counties that experienced more tariff retaliation. Moreover, the tariff retaliation post-2018 appears to have had a positive effect on the Republican presidential candidate's vote share in 2020, an effect which is significant in the regressions where we include the contemporaneous effect of the post-2018 MFP subsidies (Columns 5-6). Counties that were hit more by China's tariff retaliation in 2019 and 2020 thus appear to have swung more towards Trump in 2020, after accounting for their tariff exposure pre-2018.

A.4 Counterfactual Vote Shares and Seat Outcomes

In this part of the Appendix, we document formally how the counterfactual implications of the regression model in (4) for nationwide vote shares and CD-level race outcomes are calculated.

Let $RVote_c^{18}$ and $DVote_c^{18}$ denote the number of votes cast for Republican and Democratic candidates respectively in county c in the 2018 House elections, as reported in the Election Atlas. Let $TVote_c^{18} \equiv RVote_c^{18} + DVote_c^{18}$ be the total number of votes cast in county c for the two major parties. In turn, define $RHVoteSh_c^{18} \equiv RVote_c^{18}/TVote_c^{18}$ to be the 'two party vote share' received by Republican House candidates in county c.

In what follows, we will use 'hats' ($\widehat{x}$) to denote predicted or counterfactual values of a variable x. Section 4.4 considers four hypothetical scenarios (i)-(iv). The counterfactual 2018 Republican House vote share, $\widehat{RHVoteSh}_c^{18}$, is computed by subtracting from $RHVoteSh_c^{18}$ the terms on the right-hand side of (4) that capture the forces of interest in each scenario. Specifically:

- under scenario (i), where the effects of both the retaliatory tariffs and agricultural subsidies are removed, we compute the counterfactual Republican vote share as: $\widehat{RHVoteSh}_c^{18} = RHVoteSh_c^{18} - \sum_{b=1}^{4} \beta_2^b \mathbf{1}(c \in B^b) \times TS_c^R - \sum_{b=1}^{4} \alpha_1^b \mathbf{1}(c \in B^b) \times AgSubs_c \times TS_c^R$;

- under scenario (ii), where only the effect of the agricultural subsidies is removed, we instead have: $\widehat{RHVoteSh}_c^{18} = RHVoteSh_c^{18} - \sum_{b=1}^{4} \alpha_1^b \mathbf{1}(c \in B^b) \times AgSubs_c \times TS_c^R$;

- under scenario (iii), we compute $\widehat{RHVoteSh}_c^{18}$ as $RHVoteSh_c^{18}$ minus the effect of recent health insurance coverage gains (i.e., the increase in the county-c health insurance coverage share between 2008-2012 to 2013-2017 times its estimated regression coefficient); and

- under scenario (iv), we compute $\widehat{RHVoteSh}_c^{18}$ as $RHVoteSh_c^{18}$ minus the regression terms associated with the fourth and fifth SALT quintile dummies.

Note that we use the point estimates from the full specification in Column 4 of Table 3 to calculate the above counterfactual vote shares.

We next convert these county-level implications to nationwide vote share changes. For each scenario (i)-(iv), we compute the counterfactual number of votes received by Republican (respectively, Democratic) candidates in county c in 2018 as: $\widehat{RHVote}_c^{18} = \widehat{RHVoteSh}_c^{18} \times TVote_c^{18}$ and $\widehat{DHVote}_c^{18} = (1 - \widehat{RHVoteSh}_c^{18}) \times TVote_c^{18}$. Note that this calculation is made holding constant the total number of votes cast for the major party candidates ($TVote_c^{18}$) at the counts observed in the Election Atlas. Aggregating across the House races in all counties in our sample, we then obtain the counterfactual nationwide Republican vote share as: $\sum_c \widehat{RVote}_c^{18}/(\sum_c \widehat{RVote}_c^{18} + \sum_c \widehat{DVote}_c^{18})$. We subtract from this the Republican House vote share in 2016 aggregated over the counties in our sample (computed directly from the Election Atlas data), to arrive at the Republican vote share change under each of the four counterfactual scenarios. These are the results reported in the upper panel of Table 6.

We further infer the implications for the number of House seats won by each party under each of the four scenarios. Let H_c denote the set of CDs which overlap with county c. For any given set of counterfactual vote counts, these now need to be apportioned for each county c to the CDs $h \in H_c$ which overlap with the county. Let $\omega_{c,h}^R \in [0,1]$ denote the weights used to apportion the Republican votes cast in county c, $\widehat{RVote}_c^{18}$, to each of the CDs $h \in H_c$, where: $\sum_{h \in H_c} \omega_{c,h}^R = 1$. Let $\omega_{c,h}^D \in [0,1]$ denote the analogous weights for apportioning $\widehat{DVote}_c^{18}$, with $\sum_{h \in H_c} \omega_{c,h}^D = 1$.

Our baseline 'uniform vote share' assumption sets: $\omega_{c,h}^R = \omega_{c,h}^D = (RVote_{c,h}^{16} + DVote_{c,h}^{16})/(RVote_c^{16} + DVote_c^{16})$. Here, $RVote_{c,h}^{16}$ (respectively, $DVote_{c,h}^{16}$) is the number of Republican (respectively, Democratic) votes in the 2016 House election recorded in the county-by-CD partition where CD h overlaps with county c, as reported in the Election Atlas. At the same time, $RVote_c^{16}$ (respectively, $DVote_c^{16}$) is the number of Republican (respectively, Democratic) votes cast in the 2016 House election in all of county c. These weights assume that Republican (respectively, Democratic) voter support is distributed uniformly across the CDs within a county, so that the Republican (respectively, Democratic) vote share is identical within each county-by-CD partition; the $\omega_{c,h}^R$ and $\omega_{c,h}^D$ weights are thus identical for both parties and proportional to the total number of votes cast (summed over the two parties) in the county-by-CD partition.

Under the 'non-uniform vote share' assumption, we instead allow for each county-by-CD partition to differ in importance to each party. Specifically, we set: $\omega_{c,h}^R = RVote_{c,h}^y / RVote_c^y$ and $\omega_{c,h}^D = DVote_{c,h}^y / DVote_c^y$, where $y = 16$ or 18 depending respectively on whether we use 2016 or 2018 Election Atlas data to construct these weights. $\omega_{c,h}^R$ and $\omega_{c,h}^D$ would therefore split up the counterfactual county-level vote counts, $\widehat{RVote}_c^{18}$ and $\widehat{DVote}_c^{18}$, according to the share of Republican (respectively, Democratic) votes cast within each county-by-CD partition in either the 2016 or 2018 House elections.

We can now compute the counterfactual vote counts for each party at the CD level as follows. Let C_h denote the set of counties c that overlap with CD h. The counterfactual vote counts for CD h in the 2018 House elections are then given by: $\widehat{RVote}_h^{18} = \sum_{c \in C_h} \omega_{c,h}^R \widehat{RVote}_c^{18}$ and $\widehat{DVote}_h^{18} = \sum_{c \in C_h} \omega_{c,h}^D \widehat{DVote}_c^{18}$. This calculation can be performed for each of scenarios (i)-(iv), under each of the apportionment assumptions in turn. We then count up the number of House seats h for which: $\widehat{RVote}_h^{18}/(\widehat{RVote}_h^{18}+\widehat{DVote}_h^{18}) < 1/2$ and $RVote_h^{16}/(RVote_h^{16}+DVote_h^{16}) > 1/2$; we interpret these as seats that would have swung from the Republican to the Democratic win column from 2016 to 2018. Likewise, we count the number of House seats h for which: $\widehat{RVote}_h^{18}/(\widehat{RVote}_h^{18} + \widehat{DVote}_h^{18}) > 1/2$ and $RVote_h^{16}/(RVote_h^{16} + DVote_h^{16}) < 1/2$; these are seats that would have swung in the opposite direction, from the Democratic to the Republican party. We then take the difference between these two counts to infer the net swing in seats. The results for each of scenarios (i)-(iv), under each apportionment rule, are reported in the lower panel of Table 6.

For each of the counterfactual outcomes in Table 6, we have reported a Monte Carlo 95% confidence interval. This is computed by first taking 1,000 sets of Monte Carlo draws (indexed by n) from the joint distribution of the estimated coefficients from the regression model in (4). For each $n \in \{1, \ldots, 1,000\}$, we then compute the implied counterfactual vote shares and seat swing counts, under each scenario and apportionment rule. The confidence interval reported is bounded by the 2.5th and 97.5th percentile values for that outcome variable across all 1,000 sets of draws.

Table A.1: Cross-County Summary Statistics: All Controls and Other Variables

	Mean	Std. Dev.	10th pct.	50th pct.	90th pct.
Employment Shares by Sector					
Employment share, Agriculture (2016)	0.187	0.189	0.010	0.122	0.464
Employment share, Mining (2016)	0.015	0.046	0.000	0.001	0.037
Employment share, Manufacturing (2016)	0.123	0.103	0.012	0.099	0.269
Δ Employment share, Agriculture ('16-'13)	-0.002	0.039	-0.035	-0.001	0.032
Δ Employment share, Mining ('16-'13)	-0.003	0.027	-0.010	0.000	0.002
Δ Employment share, Manufacturing ('16-'13)	0.001	0.026	-0.021	0.000	0.025
Demographics					
Population share, Age 25-34 (2016)	0.118	0.021	0.095	0.115	0.145
Population share, Age 35-44 (2016)	0.114	0.014	0.098	0.115	0.131
Population share, Age 45-54 (2016)	0.130	0.014	0.112	0.130	0.146
Population share, Age 55-64 (2016)	0.142	0.021	0.116	0.142	0.167
Population share, Age 65 and over (2016)	0.184	0.045	0.131	0.181	0.243
Population share, Female (2016)	0.499	0.022	0.479	0.503	0.517
Population share, Black (2016)	0.093	0.145	0.005	0.024	0.305
Population share, White non-Hispanic (2016)	0.769	0.198	0.464	0.842	0.954
Population share, Hispanic (2016)	0.094	0.138	0.015	0.041	0.241
Urban Population share (2010)	0.415	0.314	0.000	0.405	0.867
Δ Population share, Age 25-34 ('16-'13)	0.002	0.005	-0.004	0.002	0.007
Δ Population share, Age 45-54 ('16-'13)	-0.002	0.005	-0.008	-0.002	0.004
Δ Population share, Age 35-44 ('16-'13)	-0.009	0.006	-0.017	-0.009	-0.003
Δ Population share, Age 55-64 ('16-'13)	0.004	0.005	-0.002	0.004	0.009
Δ Population share, Age 65 and over ('16-'13)	0.013	0.007	0.005	0.012	0.021
Δ Population share, Female ('16-'13)	-0.000	0.004	-0.004	-0.000	0.003
Δ Population share, Black ('16-'13)	0.001	0.004	-0.002	0.001	0.005
Δ Population share, White non-Hispanic ('16-'13)	-0.009	0.007	-0.018	-0.007	-0.002
Δ Population share, Hispanic ('16-'13)	0.005	0.006	0.001	0.003	0.012
Economic Conditions					
Unemployment rate (2013-17 avg.)	6.335	3.001	3.000	6.000	9.800
Log mean Household Income (2013-17 avg.)	11.058	0.223	10.801	11.040	11.329
Share with less than high school (2013-17 avg.)	0.138	0.065	0.068	0.124	0.226
Share with some college and above (2013-17 avg.)	0.517	0.107	0.381	0.516	0.653
State & Local Taxes, 4th quintile, $1,000s (2016)	1.873	0.236	1.563	1.851	2.212
State & Local Taxes, 5th quintile, $1,000s (2016)	3.994	2.227	2.464	3.259	6.209
Δ Unemployment rate (2013-17 minus 2008-12)	-2.288	2.686	-5.400	-2.200	0.700
Δ Log mean Household Income (2013-17 minus 2008-12)	0.103	0.079	0.017	0.100	0.194
Δ Share with less than high school (2013-17 minus 2008-12)	-0.021	0.024	-0.049	-0.018	0.003
Δ Share with some college and above (2013-17 minus 2008-12)	0.026	0.031	-0.007	0.026	0.061
Δ State & Local Taxes, 4th quintile, $1,000s ('16-'13)	0.124	0.028	0.088	0.121	0.165
Δ State & Local Taxes, 5th quintile, $1,000s ('16-'13)	0.409	0.295	0.203	0.327	0.696
Contested/Uncontested County indicators					
1(Uncontested by Dem. in 2016, Contested in 2018)	0.174	0.379	0.000	0.000	1.000
1(Uncontested by Rep. in 2016, Contested in 2018)	0.007	0.086	0.000	0.000	0.000
1(Contested in 2016, Uncontested by Dem. in 2018)	0.013	0.113	0.000	0.000	0.000
1(Contested in 2016, Uncontested by Rep. in 2018)	0.012	0.110	0.000	0.000	0.000
1(County split across multiple CDs)	0.126	0.332	0.000	0.000	1.000
Other Electoral Variables					
Δ Rep. House Vote Share ('20-'18)	0.053	0.106	-0.008	0.036	0.124
Δ Rep. Pres. Vote Share ('20-'16)	-0.006	0.026	-0.034	-0.006	0.021

Notes: Summary statistics across $N = 3,108$ counties, excluding Alaska.

Table A.2: Summary Statistics and Correlation Matrices of Tariff Shock Measures

Panel A:	Mean	Std. Dev.	10th pct.	50th pct.	90th pct.
US Tariff Shock	0.226	0.383	0.012	0.109	0.522
... non-Section 301	0.068	0.269	0.000	0.003	0.161
... Section 301	0.158	0.227	0.011	0.090	0.356
Upstream US Tariff Shock, non-Section 301	0.053	0.091	0.003	0.026	0.129
Upstream US Tariff Shock, Section 301	0.054	0.058	0.010	0.038	0.112
Downstream US Tariff Shock, non-Section 301	0.037	0.132	0.001	0.009	0.081
Downstream US Tariff Shock, Section 301	0.063	0.079	0.010	0.043	0.133
Retaliatory Tariff Shock	0.194	0.195	0.039	0.139	0.400
... Retaliatory, from CHN Ag.	0.098	0.152	0.004	0.046	0.250
... Retaliatory, from CHN non-Ag.	0.058	0.080	0.006	0.037	0.126
... Retaliatory, from CAN/EU/MEX	0.038	0.064	0.004	0.021	0.088
Upstream Retaliatory, from CHN Ag.	0.017	0.034	0.001	0.008	0.040
Upstream Retaliatory, from CHN non-Ag.	0.040	0.057	0.003	0.023	0.092
Upstream Retaliatory, from CAN/EU/MEX	0.018	0.021	0.003	0.012	0.039
Downstream Retaliatory, from CHN Ag.	0.021	0.035	0.001	0.010	0.051
Downstream Retaliatory, from CHN non-Ag.	0.025	0.033	0.002	0.016	0.054
Downstream Retaliatory, from CAN/EU/MEX	0.024	0.037	0.005	0.016	0.046

Panel B: Retal. Tariff by country & sector	US Tariff Shock	Retal. Tariff Shock	from CHN	from CHN Ag.	from CHN non-Ag.	from CAN/EU/MEX	from CAN/EU/MEX Ag.	from CAN/EU/MEX non-Ag.
US Tariff Shock	1.000							
Retal. Tariff Shock:	0.398	1.000						
from CHN	0.142	0.946	1.000					
from CHN Ag.	-0.041	0.787	0.884	1.000				
from CHN non-Ag.	0.381	0.522	0.451	-0.018	1.000			
from CAN/EU/MEX	0.826	0.517	0.212	0.037	0.384	1.000		
from CAN/EU/MEX Ag.	-0.047	0.765	0.859	0.968	-0.013	0.041	1.000	
from CAN/EU/MEX non-Ag.	0.829	0.481	0.172	-0.009	0.385	0.999	-0.007	1.000

Panel C: US Tariff, Up & Downstream	US Tariff Shock	non-Sec.301	Sec.301	Upst. US non-Sec.301	Upst. US Sec.301	Down. US non-Sec.301	Down. US Sec.301	Retal. Tariff Shock
US Tariff Shock:	1.000							
Non-section 301	0.814	1.000						
Section 301	0.725	0.190	1.000					
Upstream US, Non-section 301	0.896	0.853	0.502	1.000				
Upstream US, Section 301	0.600	0.244	0.725	0.656	1.000			
Downstream US, Non-section 301	0.443	0.568	0.075	0.513	0.319	1.000		
Downstream US, Section 301	0.870	0.808	0.512	0.895	0.661	0.637	1.000	
Retal. Tariff Shock	0.398	0.323	0.289	0.437	0.530	0.366	0.493	1.000

Panel D: Retal. Tariff, Up & Downstream	US	Retal. CHN Ag.	Retal. CHN non-Ag.	Retal. CAN/EU/MEX	Upst. Retal. CHN Ag.	Upst. Retal. CHN non-Ag.	Upst. Retal. CAN/EU/MEX	Down. Retal. CHN Ag.	Down. Retal. CHN non-Ag.	Down. Retal. CAN/EU/MEX
US Tariff Shock	1.000									
Retal., from CHN Ag.	-0.041	1.000								
Retal., from CHN non-Ag.	0.381	-0.018	1.000							
Retal., from CAN/EU/MEX	0.826	0.037	0.384	1.000						
Upst. Retal., from CHN Ag.	-0.076	0.565	-0.007	-0.000	1.000					
Upst. Retal., from CHN non-Ag.	0.237	0.029	0.538	0.457	0.030	1.000				
Upst. Retal., from CAN/EU/MEX	0.796	0.065	0.601	0.852	0.219	0.571	1.000			
Down. Retal., from CHN Ag.	-0.072	0.689	-0.020	0.014	0.826	0.037	0.153	1.000		
Down. Retal., from CHN non-Ag.	0.542	-0.055	0.867	0.562	-0.056	0.547	0.695	-0.066	1.000	
Down. Retal., from CAN/EU/MEX	0.369	0.271	0.509	0.487	0.560	0.326	0.613	0.441	0.616	1.000

Notes: Pairwise correlations computed across $N = 3,108$ counties, excluding Alaska.

Table A.3: NAICS Industry Shocks: Summary Statistics and Balance Tests

Panel A:	Mean	Std. Dev.	10th pct.	50th pct.	90th pct.
US Tariff Shock (NAICS level)	4.645	8.087	0.000	1.531	24.634
Retaliatory Tariff Shock (NAICS level)	3.144	10.174	0.003	0.979	4.731
Correlation:	0.548				

Panel B: Balance Test of Industry Shocks (wrt predetermined county characteristics)	Coef.	SE
US Tariff Shock		
Δ Republican House Vote Share ('12-'10)	0.0011	[0.0042]
Δ Republican Presidential Vote Share ('16-'12)	0.0023	[0.0016]
Republican House Vote Share (2016)	0.0064	[0.0064]
Republican Presidential Vote Share (2016)	-0.0053	[0.0071]
Δ Log average manufacturing weekly wages ('16-'13)	-0.0028	[0.0101]
Log average manufacturing weekly wages (2016)	0.1146	[0.0885]
Δ Employment share, Agriculture ('16-'06)	-0.0031	[0.0028]
Δ Employment share, Mining ('16-'06)	-0.0004	[0.0003]
Δ Employment share, Manufacturing ('16-'06)	-0.0067	[0.0067]
Retaliatory Tariff Shock		
Δ Republican House Vote Share ('12-'10)	-0.0439	[0.0430]
Δ Republican Presidential Vote Share ('16-'12)	0.0076	[0.0084]
Republican House Vote Share (2016)	0.0301	[0.0221]
Republican Presidential Vote Share (2016)	-0.0044	[0.0140]
Δ Log average manufacturing weekly wages ('16-'13)	-0.0175	[0.0218]
Log average manufacturing weekly wages (2016)	0.3619	[0.2611]
Δ Employment share, Agriculture ('16-'06)	-0.0074	[0.0077]
Δ Employment share, Mining ('16-'06)	-0.0069	[0.0064]
Δ Employment share, Manufacturing ('16-'06)	0.0251	[0.0184]

Notes: Panel A reports summary statistics for the US and Retaliatory Tariff Shocks at the NAICS 3-digit industry level (in units of $1,000 per worker). Panel B reports coefficients from regressions of industry-specific weighted averages of pre-determined county characteristics on the NAICS industry-level tariff shocks, as recommended by Borusyak et al. (2022); the regressions are weighted by the average NAICS tariff exposure across counties. Coefficients and standard errors are multiplied by 100 for readability; robust standard errors are reported, with none of the coefficients significant at the 10% level.

Table A.4: Checks on Cross-County Mobility and Migration

Dep. Variable: Mobility measure	County Pop., '19-'16	Domestic net migration rate, '19	Overall net migration rate, '19
	(1)	(2)	(3)
US Tariff Shock	-0.000	-0.089	-0.188
	[0.001]	[0.442]	[0.420]
Retaliatory Tariff Shock	-0.006	0.360	-0.150
	[0.004]	[1.662]	[1.635]
Lagged mobility measure	0.790***	0.574***	0.517***
	[0.024]	[0.042]	[0.036]
Observations	3,072	3,072	3,072
R-squared	0.868	0.758	0.732
Dep. Variable: Δ Republican Vote Share		House, '18-'16	
	(4)	(5)	(6)
US Tariff Shock	0.010	0.010	0.010
	[0.010]	[0.010]	[0.010]
Retaliatory Tariff Shock	-0.061***	-0.062***	-0.062***
	[0.019]	[0.019]	[0.019]
Lagged mobility measure	-0.320***	-0.001***	-0.001***
	[0.106]	[0.000]	[0.000]
Observations	3,072	3,072	3,072
R-squared	0.720	0.721	0.720
Included in all six columns:			
Lags: Δ Rep. House and Δ Rep. Pres. Vote Share	Y	Y	Y
2016 Bins: $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y
State FEs	Y	Y	Y
Other controls:		Ag. subsidy Health insurance share (levels and pre-trends) SALT dummies (levels and pre-trends)	

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in both 2016 and 2018. The dependent variable in Columns 1-3 are the respective county-level mobility measures listed in the column headings, namely: population change between 2016 and 2019, domestic net migration rate in 2019 relative to 2018, and overall net migration rate in 2019 relative to 2018. The dependent variable in Columns 4-6 is the change in the Republican House vote share between 2016 and 2018. All columns control for: (i) lagged changes in the county-level Republican House vote share ('16-'14, '14-'12, '12-'10); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); (iii) the $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),..., $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$) indicator variables for the Republican vote share in the 2016 Presidential election; (iv) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies (where applicable), as listed in the notes to Table 2; and (v) the county-level measures of MFP subsidies per worker, health insurance share (2013-2017 average), and change in health insurance share (between 2008-2012 and 2013-2017). Each column further includes the respective lagged mobility measure: population change between 2013 and 2016, domestic net migration rate in 2016 relative to 2015, and overall net migration rate in 2016 relative to 2015.

Table A.5: Tariff Retaliation and Voting Patterns: Full Table

Dep. Variable: Δ Republican Vote Share	House, '18-'16			
	(1)	(2)	(3)	(4)
US Tariff Shock	0.012	0.012	0.012	0.012
	[0.010]	[0.010]	[0.010]	[0.010]
Retaliatory Tariff Shock	-0.062***	-0.061***	-0.058***	-0.065***
	[0.020]	[0.021]	[0.019]	[0.022]
Retaliatory Tariff Shock × Ag. Subsidy				0.019*
				[0.011]
Ag. Subsidy			-0.003	-0.012
			[0.006]	[0.009]
Health Insurance Share (2013-17 avg.)		0.091	0.092	0.093
		[0.113]	[0.112]	[0.112]
Δ Health Insurance Share (2013-17 minus 2008-12)		-0.189**	-0.189**	-0.189**
		[0.092]	[0.092]	[0.092]
Lag Δ Rep. House Vote Share ('16-'14)	-0.597***	-0.595***	-0.595***	-0.595***
	[0.091]	[0.092]	[0.092]	[0.092]]
Lag Δ Rep. House Vote Share ('14-'12)	-0.378***	-0.377***	-0.377***	-0.377***
	[0.057]	[0.058]	[0.058]	[0.058]
Lag Δ Rep. House Vote Share ('12-'10)	-0.201***	-0.200***	-0.201***	-0.201***
	[0.040]	[0.041]	[0.041]	[0.041]
Lag Δ Rep. Pres. Vote Share ('16-'12)	0.720***	0.704***	0.707***	0.709***
	[0.112]	[0.111]	[0.113]	[0.113]
Pres. Vote Share ∈ [0.4, 0.5]	-0.015**	-0.016**	-0.016**	-0.016**
	[0.007]	[0.007]	[0.007]	[0.007]
Pres. Vote Share ∈ [0.5, 0.6]	-0.038***	-0.039***	-0.039***	-0.039***
	[0.010]	[0.010]	[0.010]	[0.010]
Pres. Vote Share ∈ [0.6, 1.0]	-0.025***	-0.026***	-0.026***	-0.026***
	[0.009]	[0.009]	[0.010]	[0.010]
Employment Share, Agriculture (2016)	-0.021	-0.025	-0.024	-0.019
	[0.027]	[0.029]	[0.029]	[0.030]
Employment Share, Mining (2016)	0.108*	0.105*	0.101*	0.105*
	[0.057]	[0.055]	[0.054]	[0.056]
Employment Share, Manufacturing (2016)	0.013	0.007	0.006	0.009
	[0.044]	[0.047]	[0.046]	[0.048]
Δ Employment Share, Agriculture ('16-'13)	0.021	0.026	0.029	0.028
	[0.058]	[0.059]	[0.059]	[0.059]
Δ Employment Share, Mining ('16-'13)	0.056	0.065	0.065	0.066
	[0.132]	[0.136]	[0.136]	[0.135]
Δ Employment Share, Manufacturing ('16-'13)	-0.037	-0.038	-0.038	-0.039
	[0.087]	[0.089]	[0.089]	[0.089]
Population Share, Age 25-34 (2016)	0.179	0.190	0.188	0.183
	[0.125]	[0.124]	[0.125]	[0.126]
Population Share, Age 35-44 (2016)	0.185	0.193	0.202	0.207
	[0.304]	[0.298]	[0.298]	[0.297]
Population Share, Age 45-54 (2016)	0.031	-0.039	-0.046	-0.054
	[0.398]	[0.418]	[0.417]	[0.420]
Population Share, Age 55-64 (2016)	-0.376	-0.242	-0.243	-0.242
	[0.563]	[0.564]	[0.565]	[0.567]
Population Share, Age 65 and over (2016)	0.359***	0.339***	0.342***	0.343***
	[0.110]	[0.114]	[0.116]	[0.116]
Population Share, Female (2016)	0.141	0.145	0.148	0.149
	[0.139]	[0.138]	[0.138]	[0.139]
Population Share, Black (2016)	0.001	-0.015	-0.013	-0.012
	[0.082]	[0.083]	[0.084]	[0.084]
Population Share, White non-Hispanic (2016)	-0.061	-0.078	-0.078	-0.078
	[0.092]	[0.090]	[0.090]	[0.090]
Population Share, Hispanic (2016)	0.083	0.079	0.080	0.081
	[0.071]	[0.070]	[0.070]	[0.070]
Urban Population Share (2010)	-0.060***	-0.061***	-0.062***	-0.062***
	[0.022]	[0.023]	[0.023]	[0.023]

(Cont...)

Tariff Retaliation and Voting Patterns: Full Table (cont.)

Dep. Variable: Δ Republican Vote Share	House, '18-'16			
	(1)	(2)	(3)	(4)
Δ Population Share, Age 25-34 ('16-'13)	0.626	0.524	0.526	0.519
	[0.779]	[0.800]	[0.803]	[0.804]
Δ Population Share, Age 35-44 ('16-'13)	0.100	0.022	0.012	0.019
	[1.176]	[1.201]	[1.201]	[1.204]
Δ Population Share, Age 45-54 ('16-'13)	-0.240	-0.015	-0.030	-0.041
	[0.931]	[0.921]	[0.919]	[0.918]
Δ Population Share, Age 55-64 ('16-'13)	-0.568	-0.502	-0.477	-0.473
	[0.609]	[0.597]	[0.607]	[0.606]
Δ Population Share, Age 65 and over ('16-'13)	0.018	-0.046	-0.080	-0.109
	[0.669]	[0.667]	[0.667]	[0.666]
Δ Population Share, Female ('16-'13)	0.799	0.862	0.868	0.865
	[1.010]	[1.018]	[1.020]	[1.022]
Δ Population Share, Black ('16-'13)	0.444	0.568	0.559	0.574
	[0.932]	[0.912]	[0.916]	[0.916]
Δ Population Share, White non-Hispanic ('16-'13)	0.381	0.530	0.522	0.535
	[0.713]	[0.687]	[0.689]	[0.690]
Δ Population Share, Hispanic ('16-'13)	0.007	0.168	0.158	0.152
	[0.749]	[0.742]	[0.742]	[0.740]
Unemployment rate (2013-17 avg.)	0.002	0.002	0.002	0.002
	[0.002]	[0.002]	[0.002]	[0.002]
Log mean Household Income (2013-17 avg.)	0.081***	0.078***	0.078***	0.078***
	[0.028]	[0.027]	[0.027]	[0.027]
Share with less than high school (2013-17 avg.)	-0.084	-0.058	-0.059	-0.061
	[0.128]	[0.139]	[0.139]	[0.139]
Share with some college and above (2013-17 avg.)	-0.087	-0.098	-0.098	-0.099
	[0.080]	[0.079]	[0.079]	[0.079]
1(SALT (2016) ∈ 4th Quintile)	-0.019**	-0.018**	-0.018**	-0.018**
	[0.009]	[0.009]	[0.009]	[0.009]
1(SALT (2016) ∈ 5th Quintile)	-0.025**	-0.025**	-0.025**	-0.025**
	[0.012]	[0.012]	[0.012]	[0.012]
Δ Unemployment rate (2013-17 minus 2008-12)	0.001	0.001	0.001	0.001
	[0.001]	[0.001]	[0.001]	[0.001]
Δ Log mean Household Income (2013-17 minus 2008-12)	0.031	0.030	0.030	0.029
	[0.047]	[0.047]	[0.047]	[0.047]
Δ Share with less than high school (2013-17 minus 2008-12)	-0.193	-0.205	-0.207	-0.206
	[0.139]	[0.142]	[0.143]	[0.144]
Δ Share with some college and above (2013-17 minus 2008-12)	-0.041	-0.027	-0.027	-0.025
	[0.115]	[0.117]	[0.117]	[0.117]
1(Δ SALT (2016) ∈ 4th Quintile)	-0.005	-0.004	-0.004	-0.005
	[0.006]	[0.006]	[0.006]	[0.006]
1(Δ SALT (2016) ∈ 5th Quintile)	0.000	0.001	0.001	0.001
	[0.009]	[0.009]	[0.009]	[0.009]
1(Uncontested by Dem. in 2016, Contested in 2018)	-0.108***	-0.108***	-0.108***	-0.108***
	[0.028]	[0.028]	[0.028]	[0.028]
1(Uncontested by Rep. in 2016, Contested in 2018)	0.077	0.075	0.075	0.075
	[0.072]	[0.072]	[0.072]	[0.072]
1(Contested in 2016, Uncontested by Dem. in 2018)	0.307***	0.307***	0.307***	0.307***
	[0.039]	[0.039]	[0.039]	[0.039]
1(Contested in 2016, Uncontested by Rep. in 2018)	-0.226***	-0.227***	-0.227***	-0.226***
	[0.032]	[0.032]	[0.032]	[0.032]
1(County split across multiple CDs)	-0.005	-0.005	-0.005	-0.005
	[0.007]	[0.007]	[0.007]	[0.007]
County controls: Initial levels and pre-trends	Y	Y	Y	Y
State FEs	Y	Y	Y	Y
Observations	3,072	3,072	3,072	3,072
R-squared	0.717	0.718	0.718	0.718

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in both 2016 and 2018.

Table A.6: Robustness: Different County Sample Restrictions

Dep. Variable: Δ Republican Vote Share	House, '18-'16				
	(1) Drop PA	(2) Drop split counties	(3) Drop uncontested	(4) Drop open seats	(5) Drop rematches
US Tariff Shock	0.016	-0.000	0.003	0.011	0.010
	[0.010]	[0.005]	[0.012]	[0.008]	[0.010]
Retaliatory Tariff Shock	-0.067***	-0.034**	-0.065**	-0.034*	-0.062***
	[0.021]	[0.015]	[0.030]	[0.018]	[0.023]
Retaliatory Tariff Shock × Ag. Subsidy	0.019*	0.012*	0.020*	0.027***	0.019
	[0.011]	[0.007]	[0.011]	[0.009]	[0.012]
Ag. Subsidy	-0.013	-0.007	-0.018	-0.015	-0.013
	[0.010]	[0.008]	[0.011]	[0.010]	[0.010]
Health Insurance Share (2013-17 avg.)	0.103	-0.103	0.091	0.065	0.036
	[0.116]	[0.118]	[0.102]	[0.121]	[0.119]
Δ Health Insurance Share (2013-17 minus 2008-12)	-0.145*	0.047	-0.211**	-0.090	-0.189*
	[0.080]	[0.087]	[0.089]	[0.114]	[0.096]
$\mathbf{1}$(SALT (2016) $\in$ 4th Quintile)	-0.018*	-0.010**	-0.019**	-0.007	-0.025**
	[0.009]	[0.004]	[0.009]	[0.007]	[0.010]
$\mathbf{1}$(SALT (2016) $\in$ 5th Quintile)	-0.026**	-0.027***	-0.023*	-0.027**	-0.034***
	[0.012]	[0.009]	[0.012]	[0.012]	[0.012]
Lags: Δ Rep. House and Δ Rep. Pres. Vote Share	Y	Y	Y	Y	Y
2016 Bins: $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y
Observations	3,005	2,682	2,438	2,511	2,917
R-squared	0.719	0.816	0.504	0.770	0.726

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in both 2016 and 2018. The sample in each column further drops counties as described in the respective column headings. All columns control for: (i) lagged changes in the county-level Republican House vote share ('16-'14, '14-'12, '12-'10); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); (iii) the $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),..., $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$) indicator variables for the Republican vote share in the 2016 Presidential election; and (iv) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies (where applicable), as listed in the notes to Table 2. Unreported coefficients are available on request.

Table A.7: Robustness: Alternative Controls

Dep. Variable: Δ Republican Vote Share	House, '18-'16		
	(1) Cap. int. proxies	(2) Longer lagged changes	(3) Alt. sector emp. shares
US Tariff Shock	0.012	0.012	0.014
	[0.010]	[0.010]	[0.010]
Retaliatory Tariff Shock	-0.077***	-0.066***	-0.088***
	[0.023]	[0.021]	[0.021]
Retaliatory Tariff Shock × Ag. Subsidy	0.020*	0.018*	0.021**
	[0.011]	[0.010]	[0.010]
Ag. Subsidy	-0.012	-0.011	-0.014
	[0.009]	[0.008]	[0.009]
Health Insurance Share (2013-17 avg.)	0.075	0.117	0.095
	[0.112]	[0.100]	[0.104]
Δ Health Insurance Share (2013-17 minus 2008-12)	-0.177*	-0.163*	-0.185**
	[0.092]	[0.082]	[0.089]
1(SALT (2016) ∈ 4th Quintile)	-0.019**	-0.019**	-0.018**
	[0.009]	[0.009]	[0.009]
1(SALT (2016) ∈ 5th Quintile)	-0.025**	-0.024**	-0.025**
	[0.012]	[0.011]	[0.012]
Lags: Δ Rep. House and Δ Rep. Pres. Vote Share	Y	Y	Y
2016 Bins: **1**(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y
State FEs	Y	Y	Y
Observations	3,072	3,072	3,072
R-squared	0.719	0.717	0.718

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in both 2016 and 2018. All columns control for: (i) lagged changes in the county-level Republican House vote share ('16-'14, '14-'12, '12-'10); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); (iii) the **1**(Pres. Vote $\in (0.4, 0.5]$),..., **1**(Pres. Vote $\in (0.6, 1]$) indicator variables for the Republican vote share in the 2016 Presidential election; and (iv) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies (where applicable), as listed in the notes to Table 2. Additional/Alternative controls included are: (i) in Column 1, the county employment share in high capital-intensity industries in 2016, and the lagged change ('16-'13) in this share; (ii) in Column 2, longer lagged changes for the sectoral employment shares, demographic shares, and economic characteristics ('16-'06, or '13 to '17 avg. relative to '06 to '10 avg.); and (iii) in Column 3, sectoral employment shares and their lagged changes constructed by normalizing by county working-age population (15-64) rather than total sectoral employment. Unreported coefficients are available on request.

Table A.8: Robustness: Four-year Vote Share Changes

Dep. Variable: Δ Republican Vote Share	House, '18-'14			
	(1)	(2)	(3)	(4)
US Tariff Shock	0.015	0.015	0.014	0.014
	[0.011]	[0.011]	[0.011]	[0.010]
Retaliatory Tariff Shock	-0.076**	-0.074**	-0.065**	-0.075**
	[0.029]	[0.029]	[0.026]	[0.030]
Retaliatory Tariff Shock $\times$ Ag. Subsidy				0.030**
				[0.012]
Ag. Subsidy			-0.012*	-0.027**
			[0.007]	[0.011]
Health Insurance Share (2013-17 avg.)		0.076	0.084	0.087
		[0.128]	[0.127]	[0.127]
Δ Health Insurance Share (2013-17 minus 2008-12)		-0.102	-0.101	-0.101
		[0.178]	[0.177]	[0.177]
$\mathbf{1}$(SALT (2016) $\in$ 4th Quintile)	-0.015*	-0.015*	-0.015*	-0.016*
	[0.009]	[0.008]	[0.008]	[0.008]
$\mathbf{1}$(SALT (2016) $\in$ 5th Quintile)	-0.020	-0.019	-0.020	-0.020
	[0.014]	[0.014]	[0.014]	[0.013]
Lag Δ Rep. House Vote Share ('16-'12)	0.106**	0.107**	0.107**	0.108**
	[0.049]	[0.048]	[0.048]	[0.048]
Lag Δ Rep. House Vote Share ('14-'10)	-0.426***	-0.425***	-0.426***	-0.426***
	[0.047]	[0.046]	[0.046]	[0.046]
Lag Δ Rep. House Vote Share ('12-'08)	0.103***	0.103***	0.103***	0.103***
	[0.027]	[0.027]	[0.027]	[0.027]
Lag Δ Rep. Pres. Vote Share ('16-'12)	0.765***	0.753***	0.763***	0.766***
	[0.154]	[0.151]	[0.151]	[0.150]
2016 Bins: $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y	Y
State FEs	Y	Y	Y	Y
Observations	2,964	2,964	2,964	2,964
R^2	0.713	0.713	0.713	0.714

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the change in the Republican two-party vote share between the '18 and '14 House elections. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in both 2014 and 2018. All columns control for: (i) lagged changes in the county-level Republican House vote share ('16-'12, '14-'10, '12-'08); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); (iii) the $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),..., $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$) indicator variables for the Republican vote share in the 2016 Presidential election; and (iv) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies (where applicable), as listed in the notes to Table 2. Unlike Table 2, the "uncontested" dummies are constructed as a set of four indicator variables for counties contested by only one party in the House elections in 2014 or 2018, but not both years. Unreported coefficients are available on request.

Table A.9: Robustness: Tariff Shock Measures I

Dep. Variable: Δ Republican Vote Share	House, '18-'16				
	(1)	(2)	(3)	(4)	(5)
Tariff Shock:	Top-coded 95th pct.	Ag. emp. weights	Tariff rate shock	Full trade value	Rest-of-CZ shocks
US Tariff Shock	0.012	0.014	2.758	0.002	0.013
	[0.020]	[0.010]	[1.917]	[0.002]	[0.009]
Retaliatory Tariff Shock	-0.092**	-0.082***	-1.053*	-0.011**	-0.073***
	[0.035]	[0.027]	[0.536]	[0.004]	[0.024]
Retaliatory Tariff Shock $\times$ Ag. Subsidy	0.050*	0.017	0.363*	0.003	0.020*
	[0.027]	[0.011]	[0.182]	[0.002]	[0.011]
Ag. Subsidy	-0.022**	-0.011	-0.015	-0.010	-0.014
	[0.011]	[0.008]	[0.009]	[0.009]	[0.010]
Rest-of-CZ US Tariff Shock					-0.016
					[0.015]
Rest-of-CZ Retaliatory Tariff Shock					0.038
					[0.026]
Health Insurance Share (2013-17 avg.)	0.094	0.084	0.107	0.096	0.080
	[0.113]	[0.110]	[0.110]	[0.113]	[0.113]
Δ Health Insurance Share (2013-17 minus 2008-12)	-0.186**	-0.188*	-0.191**	-0.188**	-0.168*
	[0.091]	[0.094]	[0.092]	[0.092]	[0.089]
$\mathbf{1}$(SALT (2016) $\in$ 4th Quintile)	-0.019**	-0.018**	-0.019**	-0.018**	-0.017**
	[0.009]	[0.009]	[0.009]	[0.009]	[0.008]
$\mathbf{1}$(SALT (2016) $\in$ 5th Quintile)	-0.025**	-0.025**	-0.025**	-0.025**	-0.023**
	[0.012]	[0.012]	[0.012]	[0.012]	[0.011]
Lags: Δ Rep. House and Δ Rep. Pres. Vote Share	Y	Y	Y	Y	Y
2016 Bins: $\mathbf{1}(\text{Pres. Vote} \in (0.4, 0.5])$,...	Y	Y	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y
Observations	3,072	3,072	3,072	3,072	3,018
R-squared	0.718	0.719	0.718	0.718	0.721

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. In Column 1, the US Tariff Shock, Retaliatory Tariff Shock, and Ag. Subsidy variables are top-coded at their 95th cross-county percentile values respectively. In Column 2, the US and Retaliatory Tariff Shocks are constructed using employment weights (rather than sales weights) to apportion state-by-agricultural-industry tariff shocks to the county level. In Column 3, the US and Retaliatory Tariff Shocks are constructed as weighted-average tariff rate changes (rather than in $1,000 per worker terms). In Column 4, the US and Retaliatory Tariff Shocks are constructed using the full value of trade flows affected, rather than the tariff rate multiplied by the value of trade. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in both 2016 and 2018. All columns control for: (i) lagged changes in the county-level Republican House vote share ('16-'14, '14-'12, '12-'10); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); (iii) the $\mathbf{1}(\text{Pres. Vote} \in (0.4, 0.5])$,..., $\mathbf{1}(\text{Pres. Vote} \in (0.6, 1])$ indicator variables for the Republican vote share in the 2016 Presidential election; and (iv) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies (where applicable), as listed in the notes to Table 2. Unreported coefficients are available on request.

Table A.10: Robustness: Tariff Shock Measures II

Dep. Variable: Δ Republican Vote Share	House, '18-'16			
Tariff Shock:	(1) NAICS4 OLS	(2) NAICS4 2SLS	(3) NAICS6 OLS	(4) NAICS6 2SLS
US Tariff Shock	0.008	0.012	0.002	0.008
	[0.007]	[0.008]	[0.006]	[0.008]
Retaliatory Tariff Shock	-0.027	-0.072***	-0.008*	-0.057**
	[0.016]	[0.026]	[0.005]	[0.022]
Retaliatory Tariff Shock × Ag. Subsidy	0.006	0.010*	0.004	0.013*
	[0.004]	[0.005]	[0.003]	[0.008]
Ag. Subsidy	-0.006	0.002	-0.009	-0.006
	[0.009]	[0.010]	[0.008]	[0.013]
Health Insurance Share (2013-17 avg.)	0.091	0.074	0.098	0.076
	[0.112]	[0.113]	[0.110]	[0.110]
Δ Health Insurance Share (2013-17 minus 2008-12)	-0.195**	-0.193**	-0.195**	-0.208**
	[0.092]	[0.091]	[0.091]	[0.087]
$\mathbf{1}$(SALT (2016) ∈ 4th Quintile)	-0.019**	-0.019**	-0.019**	-0.019**
	[0.009]	[0.009]	[0.009]	[0.009]
$\mathbf{1}$(SALT (2016) ∈ 5th Quintile)	-0.025**	-0.025**	-0.025**	-0.025**
	[0.012]	[0.012]	[0.012]	[0.012]
Lags: Δ Rep. House and Δ Rep. Pres. Vote Share	Y	Y	Y	Y
2016 Bins: $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y	Y
State FEs	Y	Y	Y	Y
Observations	3,072	3,072	3,072	3,072
R-squared	0.718	0.651	0.717	0.638

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. Columns 1-2 use US and Retaliatory Tariff Shock measures constructed using NAICS 4-digit employment weights for the non-farm industries, while Columns 3-4 use the corresponding measures constructed analogously using NAICS 6-digit level variation. Columns 1 and 3 report OLS estimates. In Columns 2 and 4, the US Tariff Shock, Retaliatory Tariff Shock and Retaliatory Tariff Shock × Ag. Subsidy variables constructed with NAICS 4- or 6-digit variation are instrumented using the baseline versions of these variables constructed using NAICS 3-digit level variation. In all columns, observations are weighted by 2016 county population; the sample excludes counties where the same party won uncontested in both 2016 and 2018. All columns control for: (i) lagged changes in the county-level Republican House vote share ('16-'14, '14-'12, '12-'10); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); (iii) the $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),..., $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$) indicator variables for the Republican vote share in the 2016 Presidential election; and (iv) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies (where applicable), as listed in the notes to Table 2. Unreported coefficients are available on request.

Table A.11: Change in Health Insurance Share and Voting Patterns: By Electoral Competitiveness Bins

Dep. Variable: Δ Republican Vote Share	House, '18-'16		
	(1)	(2)	(3)
US Tariff Shock	0.012	0.012	0.012
	[0.011]	[0.011]	[0.010]
Retaliatory Tariff Shock	-0.059***	-0.056***	-0.063***
	[0.020]	[0.019]	[0.021]
Retaliatory Tariff Shock $\times$ Ag. Subsidy			0.019*
			[0.011]
Ag. Subsidy		-0.003	-0.013
		[0.006]	[0.009]
Health Insurance Share (2013-17 avg.)	0.105	0.107	0.108
	[0.114]	[0.114]	[0.113]
Δ Health Insur. Share (2013-17 minus 2008-12) $\times$ $\mathbf{1}$(2016 Pres. Vote $\in$ [0.0, 0.4])	-0.064	-0.060	-0.059
	[0.240]	[0.241]	[0.241]
Δ Health Insur. Share (2013-17 minus 2008-12) $\times$ $\mathbf{1}$(2016 Pres. Vote $\in$ (0.4, 0.5])	-0.130	-0.121	-0.114
	[0.176]	[0.176]	[0.175]
Δ Health Insur. Share (2013-17 minus 2008-12) $\times$ $\mathbf{1}$(2016 Pres. Vote $\in$ (0.5, 0.6])	-0.670***	-0.670***	-0.668***
	[0.153]	[0.153]	[0.152]
Δ Health Insur. Share (2013-17 minus 2008-12) $\times$ $\mathbf{1}$(2016 Pres. Vote $\in$ (0.6, 1.0])	-0.116	-0.120	-0.123
	[0.119]	[0.119]	[0.119]
$\mathbf{1}$(SALT (2016) $\in$ 4th Quintile)	-0.018**	-0.018**	-0.018**
	[0.009]	[0.009]	[0.009]
$\mathbf{1}$(SALT (2016) $\in$ 5th Quintile)	-0.025**	-0.025**	-0.025**
	[0.011]	[0.011]	[0.011]
Lags: Δ Rep. House and Δ Rep. Pres. Vote Share	Y	Y	Y
2016 Bins: $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y
State FEs	Y	Y	Y
Observations	3,072	3,072	3,072
R-squared	0.719	0.719	0.720

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in both 2016 and 2018. Electoral competitiveness bins are constructed on the basis of the two-party Republican vote share in the 2016 Presidential election. All columns control for: (i) lagged changes in the county-level Republican House vote share ('16-'14, '14-'12, '12-'10); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); (iii) the $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),..., $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$) indicator variables for the Republican vote share in the 2016 Presidential election; and (iv) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies, as listed in the notes to Table 2. Unreported coefficients are available on request.

Table A.12: Tariff Shocks and Voter Turnout

Dep. Variable: Δ Turnout	House, '18-'16			House, '18-'14		
	(1)	(2)	(3)	(4)	(5)	(6)
US Tariff Shock	0.012***	0.013***	0.012***	0.017***	0.017***	0.017***
	[0.003]	[0.003]	[0.003]	[0.004]	[0.004]	[0.004]
Retaliatory Tariff Shock	-0.020***	-0.021***	-0.019**	-0.037***	-0.039***	-0.037***
	[0.007]	[0.007]	[0.008]	[0.013]	[0.013]	[0.014]
Retaliatory Tariff Shock × Ag. Subsidy			-0.007			-0.004
			[0.007]			[0.008]
Ag. Subsidy		0.002	0.006		0.002	0.004
		[0.003]	[0.006]		[0.003]	[0.006]
Health Insurance Share (2013-17 avg.)	0.034	0.033	0.032	0.021	0.020	0.019
	[0.046]	[0.046]	[0.046]	[0.078]	[0.078]	[0.078]
Δ Health Insurance Share (2013-17 minus 2008-12)	0.069	0.068	0.069	0.146	0.146	0.146
	[0.052]	[0.052]	[0.052]	[0.101]	[0.100]	[0.100]
$\mathbf{1}$(SALT (2016) ∈ 4th Quintile)	0.001	0.001	0.001	0.002	0.002	0.002
	[0.003]	[0.003]	[0.003]	[0.004]	[0.004]	[0.004]
$\mathbf{1}$(SALT (2016) ∈ 5th Quintile	0.002	0.002	0.002	0.006	0.006	0.006
	[0.005]	[0.005]	[0.005]	[0.006]	[0.006]	[0.006]
Lag Δ Turnout ('16-'14)	-0.385***	-0.386***	-0.386***			
	[0.033]	[0.034]	[0.034]			
Lag Δ Turnout ('14-'12)	-0.054	-0.055	-0.056			
	[0.044]	[0.044]	[0.044]			
Lag Δ Turnout ('12-'10)	-0.039	-0.038	-0.039			
	[0.035]	[0.035]	[0.035]			
Lag Δ Turnout ('16-'12)				0.370***	0.369***	0.369***
				[0.026]	[0.026]	[0.026]
Lag Δ Turnout ('14-'10)				-0.415***	-0.415***	-0.415***
				[0.049]	[0.049]	[0.049]
Lag Δ Turnout ('12-'08)				-0.007	-0.007	-0.007
				[0.046]	[0.046]	[0.046]
Lag Δ Rep. Pres. Vote Share ('16-'12)	-0.096**	-0.099**	-0.100**	-0.162**	-0.164**	-0.164**
	[0.047]	[0.047]	[0.047]	[0.064]	[0.064]	[0.064]
Lags: Δ Turnout	Y	Y	Y	Y	Y	Y
Lags: Δ Rep. House and Δ Rep. Pres. Vote Share	Y	Y	Y	Y	Y	Y
2016 Bins: $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y	Y
Observations	2,806	2,806	2,806	2,646	2,646	2,646
R-squared	0.768	0.768	0.768	0.863	0.863	0.863

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. Turnout is defined as the total votes cast divided by total registered voters. The dependent variable in Columns 1-3 is the change in turnout between '18 and '16, while that in Columns 4-6 is the four-year change in turnout between '18 and '14. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in the House elections in both 2016 and 2018 (Columns 1-2) or in both 2014 and 2018 (Columns 3-4). All columns control for: (i) lagged changes in the county-level Republican House vote share ('16-'14, '14-'12, '12-'10 in Columns 1-2, and '16-'12, '14-'10, '12-'08 in Columns 3-4); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); (iii) the $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),..., $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$) indicator variables for the Republican vote share in the 2016 Presidential election; and (iv) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies, as listed in the notes to Table 2. Unlike Table 2, the "uncontested" dummies are constructed in Columns 1-2 as a set of four indicator variables for counties contested by only one party in the House elections in 2016 or 2018, but not both years (analogously, in 2014 or 2018, but not both years, in Columns 3-4). Unreported coefficients are available on request.

Table A.13: Tariffs and Voting Patterns in the 2020 Elections

Dep. Variable: Δ Republican Vote Share	House, '20-'18			President, '20-'16		
	(1)	(2)	(3)	(4)	(5)	(6)
US Tariff Shock	-0.001	-0.000	-0.000	0.007***	0.007***	0.008***
	[0.008]	[0.008]	[0.008]	[0.002]	[0.002]	[0.002]
Retaliatory Tariff Shock	-0.011	-0.013	-0.013	-0.027***	-0.030***	-0.031***
	[0.016]	[0.018]	[0.018]	[0.005]	[0.005]	[0.005]
Retaliatory Tariff Shock $\times$ Ag. Subsidy			-0.001			0.002
			[0.009]			[0.003]
Ag. Subsidy		0.003	0.004		0.004***	0.003
		[0.005]	[0.008]		[0.001]	[0.002]
Health Insurance Share (2013-17 avg.)	0.015	0.013	0.013	0.011	0.009	0.009
	[0.130]	[0.130]	[0.130]	[0.056]	[0.055]	[0.055]
Δ Health Insurance Share (2013-17 minus 2008-12)	-0.215*	-0.215*	-0.215*	-0.063	-0.063	-0.063
	[0.108]	[0.108]	[0.108]	[0.041]	[0.041]	[0.041]
$\mathbf{1}$(SALT (2016) $\in$ 4th Quintile)	0.005	0.005	0.005	-0.008***	-0.008***	-0.008***
	[0.008]	[0.008]	[0.008]	[0.003]	[0.003]	[0.003]
$\mathbf{1}$(SALT (2016) $\in$ 5th Quintile)	-0.001	-0.000	-0.000	-0.009***	-0.009***	-0.009***
	[0.010]	[0.010]	[0.010]	[0.002]	[0.002]	[0.002]
Lag Δ Rep. House Vote Share ('18-'16)	-0.323***	-0.323***	-0.323***	0.041***	0.042***	0.042***
	[0.060]	[0.060]	[0.061]	[0.014]	[0.014]	[0.014]
Lag Δ Rep. House Vote Share ('16-'14)	-0.217***	-0.217***	-0.217***	0.046***	0.046***	0.046***
	[0.051]	[0.051]	[0.051]	[0.013]	[0.013]	[0.013]
Lag Δ Rep. House Vote Share ('14-'12)	-0.202***	-0.202***	-0.202***	0.027***	0.027***	0.027***
	[0.056]	[0.056]	[0.056]	[0.010]	[0.010]	[0.010]
Lag Δ Rep. House Vote Share ('12-'10)	-0.156***	-0.156***	-0.156***	0.022***	0.023***	0.023***
	[0.033]	[0.033]	[0.033]	[0.007]	[0.007]	[0.007]
Lag Δ Rep. Pres. Vote Share ('16-'12)	0.614***	0.612***	0.612***	0.191***	0.188***	0.188***
	[0.126]	[0.127]	[0.127]	[0.036]	[0.036]	[0.037]
Lags: Δ Rep. House and Δ Rep. Pres. Vote Share	Y	Y	Y	Y	Y	Y
2016 Bins: $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y	Y
Observations	3,073	3,073	3,073	3,074	3,074	3,074
R-squared	0.705	0.705	0.705	0.850	0.851	0.851

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the change in the Republican two-party vote share between the '20 and '18 House elections (Columns 1-3), and between the '20 and '16 Presidential elections (Columns 4-6), respectively. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in the House elections in both 2018 and 2020. All columns control for: (i) lagged changes in the county-level Republican House vote share ('18-'16, '16-'14, '14-'12, '12-'10); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); (iii) the $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),..., $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$) indicator variables for the Republican vote share in the 2016 Presidential election; and (iv) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies, as listed in the notes to Table 2. Unlike prior tables, the "uncontested" dummies are constructed as a set of four indicator variables for counties contested by only one party in the House elections in 2018 or 2020, but not both years. Unreported coefficients are available on request.

Table A.14: Tariffs and Voting Patterns in the 2020 Elections: Other Specifications

Dep. Variable: Lag Δ Rep. Vote Share	Pre-Trend checks		Four-year changes	
	House	Pres.	House	Pres.
	'10-'08	'12-'08	'20-'16	'20-'16
	(1)	(2)	(3)	(4)
US Tariff Shock	0.005	0.001	-0.003	0.006***
	[0.016]	[0.002]	[0.011]	[0.002]
Retaliatory Tariff Shock	-0.062	0.004	-0.011	-0.031***
	[0.041]	[0.005]	[0.035]	[0.006]
Retaliatory Tariff Shock $\times$ Ag. Subsidy	-0.004	-0.005	-0.001	0.002
	[0.019]	[0.003]	[0.011]	[0.003]
Ag. Subsidy	-0.006	0.004	0.004	0.003
	[0.016]	[0.003]	[0.009]	[0.002]
Health Insurance Share (2013-17 avg.)	0.108	0.013	0.091	0.004
	[0.160]	[0.043]	[0.131]	[0.055]
Δ Health Insurance Share (2013-17 minus 2008-2012)	0.127	0.002	-0.282	-0.071
	[0.197]	[0.028]	[0.170]	[0.043]
$\mathbf{1}$(SALT (2016) $\in$ 4th Quintile)	-0.005	-0.002	-0.007	-0.008***
	[0.010]	[0.002]	[0.010]	[0.003]
$\mathbf{1}$(SALT (2016) $\in$ 5th Quintile)	-0.028*	-0.004	-0.014	-0.009***
	[0.014]	[0.003]	[0.012]	[0.002]
Lag Δ Rep. House Vote Share ('18-'16)	-0.052	-0.004		
	[0.095]	[0.008]		
Lag Δ Rep. House Vote Share ('16-'14)	-0.212**	0.003		
	[0.094]	[0.008]		
Lag Δ Rep. House Vote Share ('14-'12)	-0.221***	0.013*		
	[0.078]	[0.007]		
Lag Δ Rep. House Vote Share ('12-'10)	-0.343***	0.000		
	[0.062]	[0.006]		
Lag Δ Rep. House Vote Share ('18-'14)			0.190***	0.038***
			[0.055]	[0.010]
Lag Δ Rep. House Vote Share ('16-'12)			-0.423***	0.005
			[0.080]	[0.006]
Lag Δ Rep. House Vote Share ('14-'10)			0.087*	0.019***
			[0.047]	[0.006]
Lag Δ Rep. House Vote Share ('12-'08)			-0.147***	0.002
			[0.037]	[0.005]
Lag Δ Rep. Pres. Vote Share ('16-'12)	0.425**	-0.092***	0.822***	0.193***
	[0.208]	[0.034]	[0.182]	[0.046]
Lags: Δ Rep. House and Δ Rep. Pres. Vote Share	Y	Y	Y	Y
2016 Bins: $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y	Y
State FEs	Y	Y	Y	Y
Observations	3,019	3,074	3,012	3,013
R-squared	0.329	0.796	0.716	0.852

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. Columns 1-2 perform pre-trend checks where the dependent variable is the change in the Republican two-party vote share between the '10 and '08 House elections, and between the '12 and '08 Presidential elections, respectively. Columns 3-4 use the four-year Republican vote share changes in the House and Presidential elections respectively as the dependent variable. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in the House elections in both 2018 and 2020 (Columns 1-2) or in both 2016 and 2020 (Columns 3-4). All columns control for: (i) lagged changes in the county-level Republican House vote share ('18-'16, '16-'14, '14-'12, '12-'10 in Columns 1-2, and '18-'14, '16-'12, '14-'10, '12-'08 in Columns 3-4); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); (iii) the $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),..., $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$) indicator variables for the Republican vote share in the 2016 Presidential election; and (iv) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies, as listed in the notes to Table 2. Unlike Table 2, the "uncontested" dummies are constructed in Columns 1-2 as a set of four indicator variables for counties contested by only one party in the House elections in 2018 or 2020, but not both years (analogously, in 2016 or 2020, but not both years, in Columns 3-4). Unreported coefficients are available on request.

Table A.15: Tariffs and Voting Patterns in the 2020 Elections: Additional Results with Post-2018 Exposure

Dep. Variable: Δ Republican Vote Share	House, '20-'18			President, '20-'16		
	(1)	(2)	(3)	(4)	(5)	(6)
US Tariff Shock	-0.000	-0.000	-0.000	0.006***	0.006***	0.006***
	[0.009]	[0.009]	[0.009]	[0.002]	[0.002]	[0.002]
Retaliatory Tariff Shock	-0.005	-0.009	-0.008	-0.039***	-0.046***	-0.047***
	[0.023]	[0.025]	[0.025]	[0.009]	[0.008]	[0.009]
Retaliatory Tariff Shock × Ag. Subsidy			-0.003			0.002
			[0.013]			[0.005]
Ag. Subsidy		0.004	0.005		0.002	0.001
		[0.009]	[0.013]		[0.002]	[0.002]
US Tariff Shock (post-2018)	0.001	0.001	0.001	0.000	0.000	0.000
	[0.003]	[0.003]	[0.003]	[0.001]	[0.001]	[0.001]
Retaliatory Tariff Shock (post-2018)	-0.016	-0.013	-0.015	0.023	0.030**	0.030*
	[0.041]	[0.042]	[0.044]	[0.016]	[0.015]	[0.016]
Retaliatory Tariff Shock (post-2018) × Ag. Subsidy (post-2018)			0.006			0.000
			[0.019]			[0.005]
Ag. Subsidy (post-2018)		-0.001	-0.002		0.003	0.003
		[0.007]	[0.007]		[0.002]	[0.002]
Health Insurance Share (2013-17 avg.)	0.015	0.014	0.014	0.015	0.012	0.013
	[0.127]	[0.127]	[0.127]	[0.056]	[0.055]	[0.055]
Δ Health Insurance Share (2013-17 minus 2008-12)	-0.215*	-0.215*	-0.215*	-0.063	-0.062	-0.062
	[0.108]	[0.109]	[0.109]	[0.041]	[0.041]	[0.041]
$\mathbf{1}$(SALT (2016) $\in$ 4th Quintile)	0.005	0.005	0.005	-0.008***	-0.008***	-0.008***
	[0.008]	[0.008]	[0.008]	[0.003]	[0.003]	[0.003]
$\mathbf{1}$(SALT (2016) $\in$ 5th Quintile)	-0.001	-0.000	-0.000	-0.009***	-0.009***	-0.009***
	[0.010]	[0.010]	[0.010]	[0.002]	[0.002]	[0.002]
Lag Δ Rep. House Vote Share ('18-'16)	-0.323***	-0.323***	-0.323***	0.041***	0.042***	0.042***
	[0.060]	[0.061]	[0.061]	[0.014]	[0.014]	[0.014]
Lag Δ Rep. House Vote Share ('16-'14)	-0.217***	-0.217***	-0.217***	0.047***	0.047***	0.047***
	[0.051]	[0.051]	[0.051]	[0.013]	[0.013]	[0.013]
Lag Δ Rep. House Vote Share ('14-'12)	-0.202***	-0.202***	-0.202***	0.027***	0.028***	0.028***
	[0.056]	[0.056]	[0.056]	[0.010]	[0.009]	[0.009]
Lag Δ Rep. House Vote Share ('12-'10)	-0.156***	-0.156***	-0.156***	0.022***	0.023***	0.023***
	[0.033]	[0.033]	[0.033]	[0.007]	[0.007]	[0.007]
Lag Δ Rep. Pres. Vote Share ('16-'12)	0.614***	0.612***	0.611***	0.191***	0.186***	0.187***
	[0.126]	[0.127]	[0.127]	[0.036]	[0.036]	[0.037]
Lags: Δ Rep. House and Δ Rep. Pres. Vote Share	Y	Y	Y	Y	Y	Y
2016 Bins: $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),...	Y	Y	Y	Y	Y	Y
County controls: Initial levels and pre-trends	Y	Y	Y	Y	Y	Y
State FEs	Y	Y	Y	Y	Y	Y
Observations	3,073	3,073	3,073	3,074	3,074	3,074
R-squared	0.705	0.705	0.705	0.851	0.852	0.852

Notes: Standard errors are two-way clustered by state and commuting zone; *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the change in the Republican two-party vote share between the '20 and '18 House elections (Columns 1-3), and between the '20 and '16 Presidential elections (Columns 4-6), respectively. All estimates are from OLS regressions, with observations weighted by 2016 county population; the sample excludes counties where the same party won uncontested in the House elections in both 2018 and 2020. All columns control for: (i) lagged changes in the county-level Republican House vote share ('18-'16, '16-'14, '14-'12, '12-'10); (ii) the lagged change in the county-level Republican Presidential vote share ('16-'12); (iii) the $\mathbf{1}$(Pres. Vote $\in (0.4, 0.5]$),..., $\mathbf{1}$(Pres. Vote $\in (0.6, 1]$) indicator variables for the Republican vote share in the 2016 Presidential election; and (iv) county characteristics in initial levels and pre-trends, and the county "uncontested" and "split" dummies, as listed in the notes to Table 2. Unlike prior tables, the "uncontested" dummies are constructed as a set of four indicator variables for counties contested by only one party in the House elections in 2018 or 2020, but not both years. Unreported coefficients are available on request.