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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.

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

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[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".

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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 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 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; Bradford 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.

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

[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.

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., 2019; 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. 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.[7] Notice that the average county population decreases across the bins, reflecting the well-known pattern of stronger Republican support in less densely-populated areas.

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*, 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.

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

[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
1(Pres. Vote $\in [0, 0.4]$)	246	422,828	104,015,764	0.134	0.092	0.108
				(0.150)	(0.107)	(0.411)
1(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)
1(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)
1(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.

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

[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), using the crosswalk from Pierce and Schott (2009). 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).

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

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

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

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 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)

[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.

[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 relocating often incurs sunk costs.

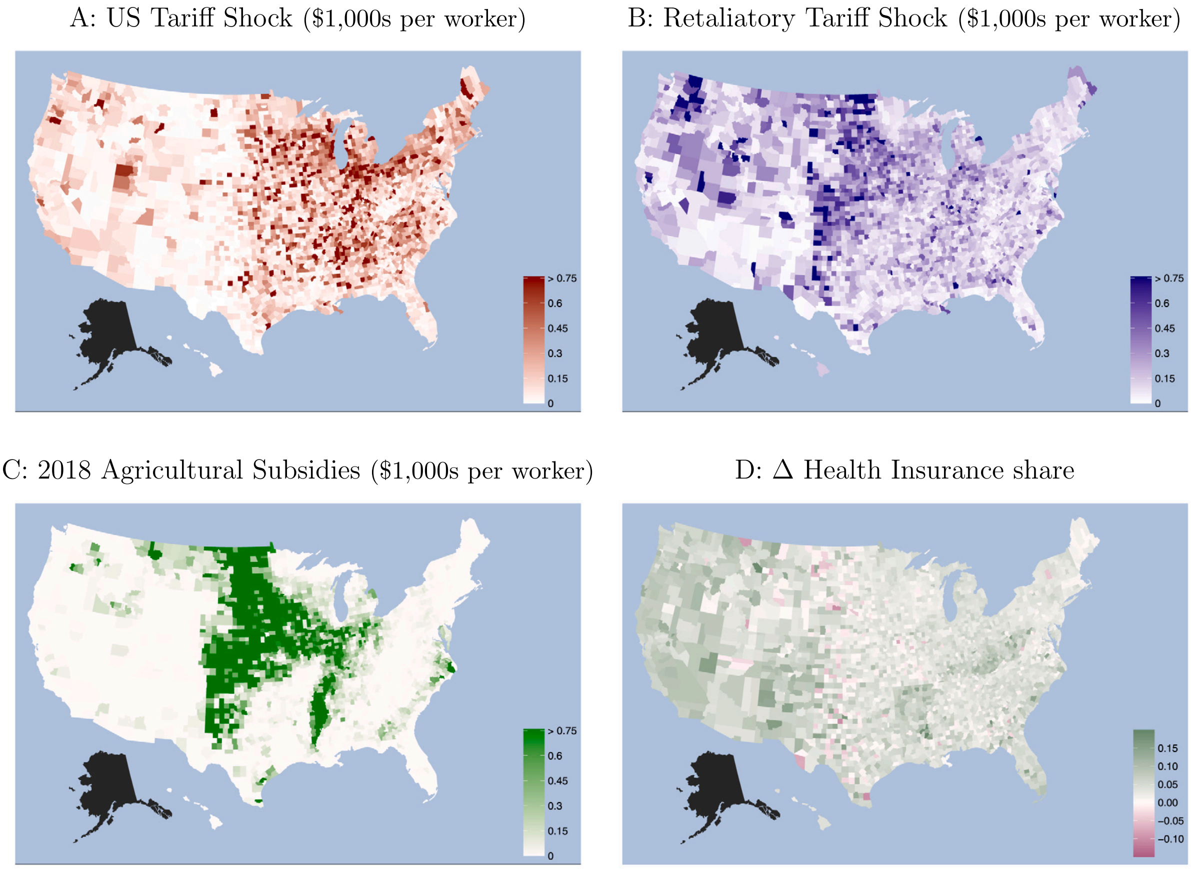

Fig. 1. Tariff shocks, agricultural subsidies, and health insurance coverage by county.

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.

Fig. 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 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.

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.

[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).

[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 Fig. 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 Fig. 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

[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).

as controls. In particular, for the demographic 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:

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

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. Eq. (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 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

[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.

[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 $\mathcal{W} = \{AgSubs_c, HInsur_c, SALT_c, R_c, \mathbf{1}(c \in B^b), X_c, D_s\}$.

pre-trends in shifts in Republican support prior to the lagged periods that we already explicitly 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). Towards 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.

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

[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).

Table 2
Tariff retaliation and voting patterns in the 2018 house elections.

Dep. variable: Δ Republican Vote Share	House				Pre-trend checks House	Pre-trend checks President
	'18–'16				'10–'08	'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.

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.

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

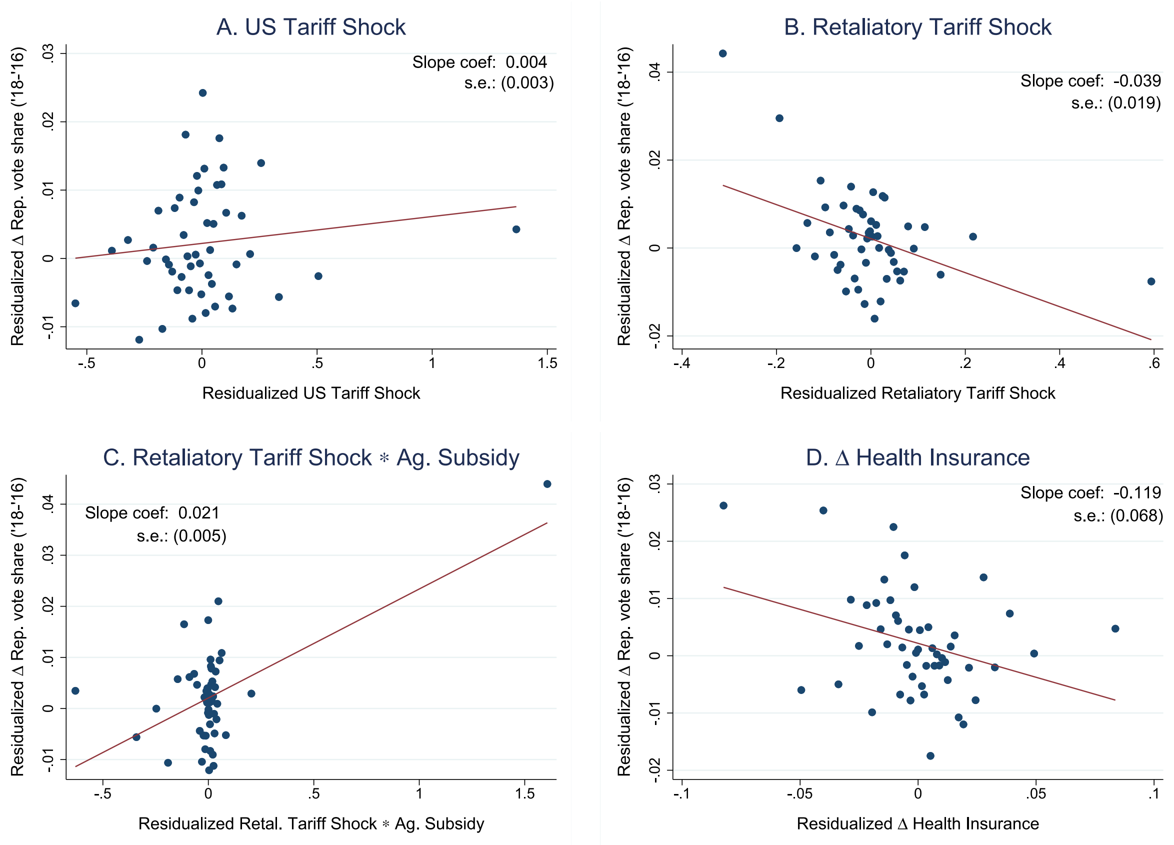

Fig. 2. Binned scatterplots.
Notes: Based on the regression specification in Column 4, Table 2. Each y- and x-variable is first residualized of variation that can be explained by the set of right-hand side variables (excluding the US Tariff Shock, Retaliatory Tariff Shock, Retaliatory Tariff Shock × 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.

subsidy amounts above $(0.0649/0.0192) \times \$1000 \approx \$3380$ 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.

Fig. 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

[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.

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]

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. Towards 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, 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 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]

[25] At the mean value of $AgSubs_c$ (0.429, from Table 1), the implied overall effect of TS_c^R is: $-0.065 + 0.429 \times 0.019 = -0.057$, which is significantly different from zero (p-value = 0.005).

[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.

[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 Méndez 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).

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 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):

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

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%. Eq. (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 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).

[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.

[30] Eq. (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.

[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 × **1**(Pres. Vote ∈ [0, 0.4])	0.058	0.057	0.057	0.057
	[0.050]	[0.051]	[0.051]	[0.051]
US Tariff Shock × **1**(Pres. Vote ∈ (0.4, 0.5])	0.008	0.009	0.008	0.010
	[0.038]	[0.038]	[0.038]	[0.038]
US Tariff Shock × **1**(Pres. Vote ∈ (0.5, 0.6])	0.030	0.030	0.030	0.030
	[0.021]	[0.021]	[0.021]	[0.021]
US Tariff Shock × **1**(Pres. Vote ∈ (0.6, 1])	−0.001	−0.001	−0.001	−0.001
	[0.007]	[0.007]	[0.007]	[0.007]
Retaliatory Tariff Shock × **1**(Pres. Vote ∈ [0, 0.4])	−0.166**	−0.168**	−0.169**	−0.180**
	[0.082]	[0.081]	[0.079]	[0.081]
Retaliatory Tariff Shock × **1**(Pres. Vote ∈ (0.4, 0.5])	−0.256***	−0.257***	−0.250***	−0.273***
	[0.090]	[0.090]	[0.091]	[0.086]
Retaliatory Tariff Shock × **1**(Pres. Vote ∈ (0.5, 0.6])	−0.021	−0.018	−0.022	−0.026
	[0.046]	[0.045]	[0.047]	[0.047]
Retaliatory Tariff Shock × **1**(Pres. Vote ∈ (0.6, 1])	−0.036*	−0.035	−0.031	−0.037
	[0.021]	[0.022]	[0.020]	[0.022]
Retaliatory Tariff Shock × Ag. Subsidy × **1**(Pres. Vote ∈ [0, 0.4])				0.240
				[0.162]
Retaliatory Tariff Shock × Ag. Subsidy × **1**(Pres. Vote ∈ (0.4, 0.5])				0.533*
				[0.284]
Retaliatory Tariff Shock × Ag. Subsidy × **1**(Pres. Vote ∈ (0.5, 0.6])				−0.024
				[0.018]
Retaliatory Tariff Shock × Ag. Subsidy × **1**(Pres. Vote ∈ (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]
1(SALT (2016) ∈ 4th Quintile)	−0.018**	−0.018*	−0.018**	−0.019**
	[0.009]	[0.009]	[0.009]	[0.009]
1(SALT (2016) ∈ 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: **1**(Pres. Vote ∈ (0.4, 0.5]), …	Y	Y	Y	Y
Double interactions: Ag. Subsidy × **1**(Pres. Vote ∈ [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 **1**(Pres. Vote ∈ (0.4, 0.5]), … , **1**(Pres. Vote ∈ (0.6, 1]), while Columns 3–4 include the double interaction terms in Ag. Subsidy × **1**(Pres. Vote ∈ [0, 0.4]), … , Ag. Subsidy × **1**(Pres. Vote ∈ (0.6, 1]). Unreported coefficients are available on request.

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.

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]
1(SALT (2016) ∈ 4th Quintile)	−0.018**	−0.018**	−0.018**	−0.018**	−0.018**
	[0.009]	[0.009]	[0.009]	[0.009]	[0.009]
1(SALT (2016) ∈ 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: **1**(Pres. Vote ∈ $(0.4, 0.5]$), …	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 **1**(Pres. Vote ∈ $(0.4, 0.5]$), …, **1**(Pres. Vote ∈ $(0.6, 1]$) 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.

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 under industry i. Eq. (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

[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.

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]
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]
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: **1**(Pres. Vote ∈ (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 **1**(Pres. Vote ∈ (0.4, 0.5]), … , **1**(Pres. Vote ∈ (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 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 Eq. (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 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.

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 Eq. (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 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.

[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.

[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.

[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).

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 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 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 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 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

[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.

[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.

[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.

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 1000 sets of Monte Carlo draws from the joint multivariate normal distribution of the Column 4, Table 3 coefficient estimates.

to 2016.[40] 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

[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).

[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.

(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 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 deductibility – 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 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. Towards 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

[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.

[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).

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 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).[45] 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.[46] 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.[47]

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 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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[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.

[45] In particular, information on the 2019 MFP subsidy rates by agricultural products is drawn from Government Accountability Office (2020).

[46] 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.

[47] 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.

Declaration of competing interest

All three authors individually attest to have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

https://data.mendeley.com/datasets/vm9hjcyddn/1.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jinteco.2024.103891.

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