Help for the Heartland? The Economic and Political Effects of the Trump Tariffs in US Local Labor Markets*

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Abstract

We study the economic and political consequences of the 2018–2019 US-China trade war at the level of local labor markets, where employment and voting intersect. Although the stated goal of the tariff policy was to bring back jobs to America, US import tariffs did not raise employment or earnings in tariff-protected regions through 2023, five years after the policy's introduction. Retaliatory tariffs reduced employment in the short run, especially in agriculture, but their effects faded over time and were partly offset by compensatory agricultural subsidies. Politically, however, the trade war benefited the Republican party. Residents of tariff-exposed regions expressed greater support for tariffs, became more likely to identify as Republicans, and were more likely to vote for Donald Trump in 2020 and 2024, with retaliatory tariff exposure only modestly dampening the electoral gains. The political-economic asymmetry was most pronounced in regions previously exposed to the China shock, where voters appear to have rewarded the Trump administration for confronting China through tariffs rather than for delivering local economic gain.

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1 Introduction

In the two decades following China's accession to the World Trade Organization in 2001, rapid expansion of world trade contributed to a sharp decline in US manufacturing employment (Autor et al., 2013; Pierce and Schott, 2016). Although majority public opinion did not turn against globalization (Davenport et al., 2022), skepticism about trade became increasingly politicized (Walter, 2021; Colantone et al., 2022). During the 2016 US presidential election, both GOP candidate Donald Trump and Democratic runner-up Bernie Sanders voiced opposition to trade and demanded greater protection for US manufacturing workers (Davenport, 2016).

In 2018, the US sharply reversed course, imposing substantial tariffs on Chinese imports and select goods from other countries. This protectionist turn triggered successive rounds of US import tariff hikes, retaliatory foreign tariffs, and US subsidies to affected sectors. By the end of 2019, the US and China had largely reversed the closer economic integration they had spent the 1980s and 1990s negotiating. Donald Trump's targeting of China with tariffs was both highly visible and aimed at places that had been subject to acute disruptions from globalization.

A stated goal of the first Trump administration's 2018–2019 tariff program was to "bring back American manufacturing" (The White House, 2019). A second, related but distinct objective was to win political support from voters affected by trade with China (Autor et al., 2020a). The political consequences of a trade war need not be tightly coupled with its pecuniary impacts: if tariffs convey solidarity with voters in import-competing locations, they may pay political dividends even absent measurable economic gains. This paper considers the political consequences of the US-China trade war and how they align with its economic impacts. We perform this analysis at the level of local labor markets (Commuting Zones), where employment and voting intersect.

The US-China trade war is a widely studied episode of US trade policy (Fajgelbaum and Khandelwal, 2022). Prior work has documented modest aggregate welfare losses from the tariffs and retaliation, while evidence on regional labor market effects has been mixed and confined to short post-tariff horizons. Findings on electoral consequences likewise vary across elections, with prior studies variously documenting GOP losses in the 2018 midterms and weak GOP gains in the 2020 presidential race. We review this literature in Section 2.

To examine regional economic and political outcomes of the US-China trade war in concert, we extend existing work along two dimensions. First, we study the spatial impacts of US tariffs, foreign tariffs, and US agricultural subsidies over a longer time span and at a more detailed industry level. We apply fixed-point imputation techniques to the Quarterly Census of Employment and Wages (QCEW) to create monthly data through 2023 on employment in US commuting zones for NAICS six-digit industries, spanning agriculture, manufacturing and services. We combine these data with detailed information on US import tariffs and foreign retaliatory tariffs, drawing on existing tariff measures and our extensions to these data, and government compensatory subsidies to agricultural sectors targeted by Chinese and other foreign retaliatory tariffs.

This sectoral and temporal granularity is valuable for establishing the robustness of our first result: the impact of tariffs on Commuting Zone (CZ) level employment is null for US duties at all time horizons, and for foreign counter-duties at longer time horizons. Much of the literature focuses on 2018 and 2019, likely to avoid Covid-era confounders.[1] By extending analysis through 2023, we address concerns that null US tariff effects reflect firms' incomplete adjustment to, or doubts about, the durability of the tariffs, while also revealing shorter-run dynamics that earlier truncation would mask. Whereas impacts on CZ employment over 2018 to 2019 are significantly negative for foreign tariffs and positive for US agricultural subsidies, over the full 2018 to 2023 period impacts of foreign tariffs approach zero while remaining strongly positive for farm subsidies.[2] In sum, exposure to the US-China trade war did not appear to noticeably affect US regional employment or income out to five years after its inception.

These null results do not extend to the political realm. Although the tariffs failed to deliver measurable economic gains, they nonetheless shifted regional politics in a manner that—given the electoral geography of affected places—meaningfully favored the GOP. Using survey responses from the Cooperative Congressional Election Study (CCES) in late 2019, we find that respondents in CZs whose industries were subject to US import protection expressed stronger support for US tariffs, while respondents in CZs whose industries faced foreign retaliation expressed weaker support. These cross-sectional patterns are mirrored in longitudinal shifts in party identification: over 2018 to 2023, CZs exposed to new US tariff protection saw increases in the share of respondents identifying with the Republican party and decreases in identification with the Democratic party, while CZs exposed to foreign counter-tariffs underwent the opposite shifts.

Changes in party identification translated asymmetrically into presidential voting. Using the Leip Electoral Atlas, we compare the two-party presidential vote share in 2020 (Trump's first reelection attempt) and 2024 (his second) against the last pre-trade-war election in 2016. GOP support rose differentially in CZs exposed to US import protection but was not substantially dampened by exposure to foreign retaliation. Because import-protected CZs were disproportionately located in swing states while CZs targeted by retaliatory tariffs were largely in GOP strongholds, these tariff-induced shifts likely benefited the GOP in federal contests. The political gains were particularly concentrated in CZs with the greatest prior exposure to Chinese import competition in the 2000s (Autor et al., 2013), even though employment in these places fared no better than elsewhere.

These findings raise the question of why the trade war paid off politically if not economically. One possibility is that residents of affected regions misperceived the local economic benefits of tariffs. We find no such evidence: residents of tariff-impact locations did not report more positive

[1] We document that disruptions to manufacturing during Covid-19 were substantial but short-lived.

[2] We detect related dynamics in trade war impacts on total local income—and its components labor income, capital income, and government transfers—which have not been examined in previous work.

income changes during the trade war. A second explanation is that Republicans drew support from voters who were skeptical of tariffs' economic benefits but valued the willingness to confront foreign competition and defend US jobs. The concentration of GOP gains in CZs with prior China-shock exposure is consistent with this view. The pattern echoes the political aftermath of the North American Free Trade Agreement (NAFTA): less-educated voters in manufacturing regions exposed to trade with Mexico reduced their support for Democrats after the agreement took effect in 1994 (Choi et al., 2024), apparently blaming the party for what they viewed as an abandonment of their economic interests. Voters in regions hurt by the China trade shock may similarly have rewarded Donald Trump for casting himself as an anti-globalization champion.

Section 2 reviews prior evidence on the trade war. After describing data sources and measurement in section 3, section 4 analyzes the impact of tariffs on employment and other outcomes at the level of local labor markets. Section 5 turns to political outcomes: awareness of and support for tariff policies; political party identification; and electoral outcomes in presidential races. Section considers potential explanations for the uneven effects of tariffs on economic and political outcomes. Section 7 concludes.

2 Prior Evidence on the Trade War

Three branches of work bear directly on our analysis: studies of prices and aggregate welfare, of labor market impacts, and of political and electoral consequences. We summarize each in turn and indicate how our analysis extends it.

A first branch examines the effects of tariffs on prices, trade flows, and aggregate welfare (Amiti et al., 2019, 2020a, 2021; Carter and Steinbach, 2020; Cavallo et al., 2021; Handley et al., 2020; Fajgelbaum et al., 2020; Flaaen et al., 2021; Feenstra and Hong, 2024). Substantial pass-through of US tariffs to border prices, coupled with aggressive foreign retaliation, supports the conclusion that the trade war lowered US welfare, albeit modestly. We do not analyze aggregate welfare directly, but our regional employment and income results bear on the labor market channels through which any local welfare effects would have propagated.

A second branch evaluates the trade war's labor market impacts. For national manufacturing sectors, Flaaen and Pierce (2021) find that modest gains from US import tariffs were offset by new foreign tariffs and tariff-induced increases in input costs. Benguria and Saffie (2020) extend the analysis to local labor markets, but find no measurable effects of import tariffs, retaliatory tariffs or input costs on local employment. Conversely, Waugh (2019) estimates a negative local employment effect of retaliatory tariffs, while Javorcik et al. (2022) provide complementary results using online job advertisements. Our analysis extends this work along three dimensions. First, we measure CZ exposure using NAICS six-digit industries, which substantially refines the three-digit aggregations used in most prior CZ-level studies. Second, we observe outcomes through 2023 rather than ending in 2018 or 2019, allowing both for fuller firm adjustment to the tariffs and for the unfolding of shorter-run dynamics that we document below. Third, we examine impacts on labor income, capital income, and government transfers, dimensions that prior work has not been able to study at the regional level due to data constraints (Fajgelbaum and Khandelwal, 2022).

A third branch of work documents the political geography of the trade war. Industries exposed to US import protection and to foreign retaliation tended to be clustered in Republican-leaning regions (Fajgelbaum et al., 2020), and Chinese counter-tariffs appear to have been targeted to inflict damage on the GOP (Fetzer and Schwarz, 2021; Kim and Margalit, 2021); voters, in turn, regarded foreign retaliation as a form of electoral interference (Brugter et al., 2023). Evidence on electoral consequences has nevertheless been mixed. Kim and Margalit (2021) and Blanchard et al. (2024) find that retaliatory tariffs in the early phase of the trade war contributed to sizable Republican losses in the 2018 midterm elections, while Lake and Nie (2022) find a modest positive effect of import tariffs on the GOP vote share in the 2020 presidential race that was largely but not fully offset by a negative impact of retaliatory tariffs.[3] The divergent findings reflect, in part, the different elections, time horizons, and tariff measures these studies employ. Our analysis brings these strands together by examining support for tariffs, party identification, and presidential vote shares in both 2020 and 2024 within a common framework. Because we study economic and political outcomes within a unified empirical design, we can also assess whether the trade war's political consequences align with its economic ones—an important question on mechanism that prior work, by studying one dimension or the other, has been unable to address directly.

3 Data and Measurement

This section describes our measures of regional exposure to the 2018-19 trade war and the economic and political outcomes we study. Between 2018 and 2019, average US tariffs on Chinese goods rose from 3.1% to 21.0%, and Chinese tariffs on US goods rose from 8.0% to 21.8% (Bown and Kolb, 2022; Fajgelbaum and Khandelwal, 2022).[4] Tariffs largely stabilized after early 2020 and changed little during the 2021-2025 Biden presidency (Bown, 2025); the renewed tariff escalation following Donald Trump's return to office in 2025 lies outside our period of analysis.

3.1 Trade and tariffs

We use US and foreign import values and tariff rates to measure US industry exposure to the US-China trade war. Monthly trade data are from the US Census Bureau's Foreign Trade Statis-

[3]With a narrower focus, Chyzh and Urbatsch (2020) and Choi and Lim (2023) document complementary electoral effects operating through tariffs and subsidies in the agricultural sector.

[4]While the US and China were at the center of the trade war, the first waves of US tariffs also targeted nations other than China, and the EU, Canada, Mexico, Turkey, and India followed suit with their own retaliatory tariffs. Our analysis accounts for these tariffs between the US and third countries.

tics.[5] Trade-war tariffs through early 2019 are from Fajgelbaum et al. (2020), which we extend to December 2019;[6] pre-trade war baseline tariffs for 2016 and 2017 are from Schott (2008).[7] We also use annual data for 2016 forward on industry sales in the manufacturing, agriculture and mining sectors, and manufacturing industry prices.[8]

Following a standard gravity trade model formulation, we calculate the exposure of US industry i to US tariffs on imports from countries k at time t as:

$$IMP_{it} = 100 \times \gamma_{iuu} \sum_{k} \rho_{iuk} \hat{\tau}_{iukt}, \tag{1}$$

where $\hat{\tau}_{iukt} \equiv \ln(1+\tau'_{iukt}) - \ln(1+\tau_{iuk,Jan18})$ is change in the US import tariff for industry i goods imported from country k from January 2018 (one month prior to the first increase in US tariffs) to a subsequent month t between February 2018 and December 2019 (when the trade truce between the US and China was announced), $\rho_{iuk} \equiv E_{iuk}/E_{iu}$ captures the pre-trade war fraction of imports from country k in total US expenditure for industry i goods, and $\gamma_{iuu} \equiv x_{iuu}/x_{iu}$ is the fraction of US industry i output sold domestically. Similarly, we calculate the exposure of US industry i to retaliatory tariffs by countries j at time t as:

$$RET_{it} = 100 \times \sum_{j} \gamma_{iju} \hat{\tau}_{ijut} \tag{2}$$

where $\hat{\tau}_{ijut} \equiv \ln(1+\tau'_{ijut}) - \ln(1+\tau_{iju,Jan18})$ is the tariff change by country j on US goods from industry i between January 2018 and month t, and $\gamma_{iju} \equiv x_{iju}/x_{iu}$ is the pre-trade war fraction of US industry i output sold to country j. In (1) and (2), US import tariffs are by country and HS10 product and foreign tariffs on US exports are by country and HS6 product.[9] To construct the shares used in (1) and (2), we first compute the fraction of US industry output sold domestically, the fraction of US industry output sold to each country, and the fraction of US industry spending on goods from each country using 2012 pre-trade war US output data by industry.[10]

[5] These data are obtained separately by trade partner country via USA Trade® Online.

[6] Our extensions update Fajgelbaum et al. (2020) through December 2019 to incorporate additional rounds of US-China tariffs, retaliatory tariffs by India and Turkey, exemptions for EU and NAFTA countries, and aggregation to time-consistent NAICS 6-digit codes (see Appendix A4 for the full procedure).

[7] Because various tariff exceptions made the *effective* average tariff rate that the US applied to imports from China slightly lower than the reported statutory tariff (Flaaen et al., 2021), our later empirical estimates should be interpreted as intention-to-treat effects. Trade patterns suggest that China further curtailed imports from the US by imposing non-tariff trade barriers, alongside increases in tariff rates (Chen et al., 2022).

[8] These data are from, respectively, the Annual Survey of Manufactures (U.S. Census Bureau, 2017, 2022a); the Economic Census of Manufacturing (U.S. Census Bureau, 2025); the Annual Integrated Economic Survey (U.S. Census Bureau, 2026); the Bureau of Labor Statistics' Producer Price Index (BLS, 2024); and BEA Industry Economic Accounts (BEA, 2025a).

[9] Import and export product codes share the same 6-digit root based on the Harmonized System (HS6). At HS8 or HS10 levels, import and export product codes sometimes do not coincide and detailed codes applied in different countries are not fully compatible (U.S. International Trade Administration, 2023). We thus rely on HS6 product codes for US exports to ensure comparability of retaliatory tariffs introduced by different countries.

[10] These data are from the US Economic Census of Manufacturing and Mining (U.S. Census Bureau, 2019a,b),

In (1) and (2), we account for all countries that were targeted by US tariffs and that retaliated in kind with tariffs on the US. Without question, China was the main US antagonist in the conflict. In our data, China-facing import tariffs accounted for 94.8% of employment-weighted industry tariff exposure as of December 2019. Similarly, Chinese retaliatory tariffs accounted for 82.5% of industry retaliatory tariff exposure as of December 2019.

3.2 Agricultural subsidies

US agriculture was heavily exposed to foreign retaliatory tariffs, due in part to the fact that the US has long been the largest exporter of agricultural goods to China. To mitigate adverse impacts of the trade war on the farm sector, the US launched the Market Facilitation Program (MFP), which from late 2018 to early 2020 distributed more than $23 billion to farmers. Most of the subsidies went to producers of grains and oilseeds, whose compensatory payments were computed as the product of farming acreage times a county-specific rate that was based on the county's crop mix. The program led to highly uneven compensation to producers of the same crop in different parts of the country (United States Government Accountability Office, 2021). The MFP paid farmers $5.3 billion in late 2018, $14.3 billion in 2019 and $3.8 billion in early 2020 as compensation for adverse effects of foreign retaliatory tariffs (EWG, 2022).[11] Building on prior research, which imputed subsidy payments to local labor markets (Blanchard et al., 2024; Javorcik et al., 2022), we draw on actual MFP payments by county from the EWG Farm Subsidy Database (EWG, 2022). The database reports annual subsidy payments by county, which we allocate to the months of September 2018 to February 2020 based on additional government documents that indicate the distribution of payments within years.[12] We normalize MFP monthly payments per working age population using 2017 population data from SEER (2022). Due to the less precise temporal resolution of the underlying data, we limit use of the farm subsidy variable to a subset of our regression specifications.

3.3 Economic outcomes and CZ exposure construction

To study economic outcomes, we aggregate county-level data to commuting zones (CZs) (Tolbert and Sizer, 1996; Autor and Dorn, 2013). We use the Quarterly Census of Employment and Wages (QCEW) to measure monthly employment by CZ and NAICS six-digit industry, and overall and sector-specific CZ employment-to-population ratios (with monthly population counts interpolated from annual data in SEER (2022)). Relative to County Business Patterns (CBP), which Autor

the US Census of Agriculture (USDA, 2014), and bilateral trade data from Schott (2008). Four manufacturing industries have suppressed output values in the 2012 Census. We impute these using 2011 data from the NBER-CES Manufacturing industry database (Becker et al., 2021). To ensure non-negative values of the fraction of US industry output sold domestically, we adjust trade flows for re-exported imports (BEA, 2021).

[11]MFP-2018 paid subsidies based on commodity-specific rates per acre of eligible crop planted, milk produced, or live hogs owned; MFP-2019 expanded the list of eligible crops and set payments using county-specific per-acre rates ranging from $15 to $150 (Schnepf, 2019).

[12]See Appendix A4 for the procedure of allocating annual MFP payments to months.

et al. (2013) and many other studies use to allocate industry shocks to local labor markets, the QCEW has three advantages: (1) it includes agriculture, which allows us to map foreign retaliatory tariffs on US farm crops to the regional level, (2) it provides data at a monthly rather than annual frequency, which allows us to explore tariff impacts at high temporal resolution, and (3) it is not subject to the highly restrictive data suppression procedures that were adopted by the CBP in 2017, which aids in imputing values for industry-county cells whose exact employment counts are not disclosed.[13] On the third issue, the QCEW discloses: county total monthly employment (nearly always), NAICS six-digit industry-county level employment for cells that comprise approximately 75 percent of private sector employment (see Table A11), and employment for more aggregate industry and geographic units that cover industry-county cells comprising the remainder of private sector totals. Even where employment counts are suppressed, the QCEW reports the number of establishments in each NAICS six-digit by industry-county cell, which is the basis for our fixed-point employment imputation procedure for suppressed cells (see Appendix A3).[14]

As a secondary economic outcome, we study annual income per capita at the CZ level using BEA Local Area Personal Income data (BEA, 2024), which includes time series of local income differentiated by income sources. We separately examine labor income obtained from wages and salaries; capital income obtained from proprietor's income, dividends, interests, and rents; and transfer income received from government benefit programs.[15]

In the empirical analysis, we project industry-level exposure to US import tariffs and foreign retaliatory tariffs on US exports in (1) and (2) to the CZ level using:

$$\hat{X}_{rt} = \sum_{i} \frac{e_{ir}}{e_r} \hat{X}_{it} \tag{3}$$

where $\hat{X}_{it} \in \{IMP_{it}, RET_{it}\}$, and e_{ir}/e_r is the share of industry i in total employment in r.[16] Our QCEW data allow us to measure CZ-level exposure to tariffs for 373 six-digit NAICS tradable industries, which provides higher resolution than prior studies of local tariff impacts that used employment in three-digit industries (e.g., Waugh, 2019; Benguria and Saffie, 2020; Lake and Nie, 2022).

Figure 1 plots the spatial distribution of exposure to tariffs and agricultural subsidies, while Appendix Table A1 provides further descriptive statistics. In Panel A of the figure, we see that US

[13] The Bureau of Labor Statistics compiles the QCEW data primarily from state-level unemployment insurance (UI) records and from records of the Unemployment Compensation for Federal Employees (UCFE), which together cover nearly all wage and salary workers in the US economy (including the public sector).

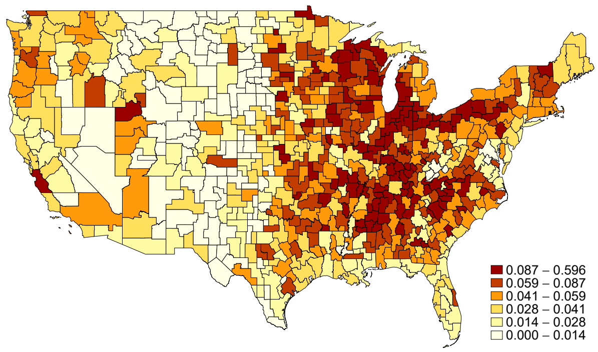
[14] Industry-county imputation is necessary in any study that allocates detailed industry shocks to local labor markets (see, e.g., Autor et al., 2013; Eckert et al., 2020; Kim and Margalit, 2021; Javorcik et al., 2022); we allocate employment from aggregate cells in proportion to each county's establishment share and adjust to ensure consistency with disclosed totals (see Appendix A3 for details and a comparison with CBP-based imputations).

[15] Dollar amounts are converted to \$2018 using the Personal Consumption Expenditure deflator (BEA, 2025b).

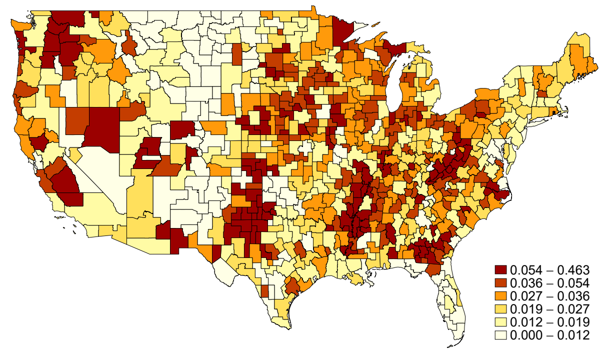
[16] To avoid simultaneity bias in measurement of shares and shocks, we use pre-treatment 2012 industry-by-CZ employment from the QCEW to map industry exposure to local labor markets.

Figure 1: CZ Exposure to Import and Retaliatory Tariffs and Farm Subsidies

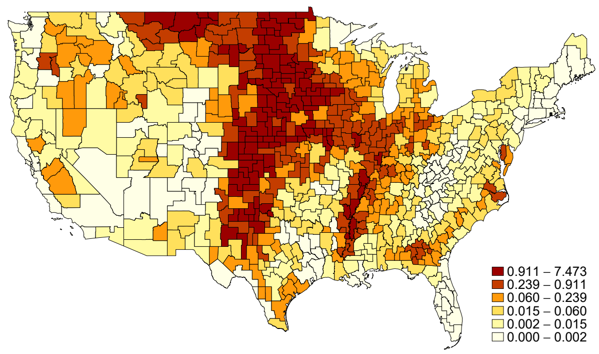
(a) Import tariff exposure



(b) Retaliatory tariff exposure



(c) Farm subsidies per capita



Notes: The figure depicts CZ exposure to import tariffs (Panel A), retaliatory tariffs (Panel B), and farm subsidies per working age population (Panel C), averaged over all months in 2019. Industry import and retaliatory tariff exposures are calculated according to equations (1) and (2) and mapped to CZs based on equation (3).

import tariff protection was concentrated in traditional industrial states in the Midwest (e.g., Ohio, Indiana, Michigan) and South (e.g., Tennessee, North Carolina, Alabama). Foreign retaliatory tariffs partly targeted the same industries—and hence the same regions—that received protection via US import tariffs. The cross-CZ correlation between CZ US import tariff and foreign retaliatory tariff exposure is 0.39 in 2019. Panel B of Figure 1 reveals that the incidence of retaliatory tariff exposure was also high in regions that were specialized in select agricultural sub-sectors. Heavily exposed regions included the Mississippi Delta (soybeans and cotton), northern Texas (cotton and sorghum), and California's San Joaquin Valley (cotton), all of which produce crops for which China is a top export market. Other major agricultural regions, such as North Dakota and northern Montana (wheat), and the Colorado-Kansas-Nebraska border region (corn), were less exposed to retaliatory tariffs based on differences in the foreign markets for their crops.[17] While CZs facing higher retaliatory tariffs received larger farm subsidies, the correspondence is surprisingly modest

[17]In 2017 and 2018, China was the most important destination for US exports of soybeans, sorghum, and pima cotton, and the second most important destination for upland cotton. It ranked outside the top 20 destination markets for US corn and wheat (U.S. Department of Agriculture, 2022).

(ρ = 0.25), perhaps indicating that subsidies were imperfectly regionally targeted. Comparing panels B and C of Figure 1, we see for instance that North Dakota and Montana received high per-capita agricultural subsidies despite limited exposure to retaliatory tariffs (United States Government Accountability Office, 2021).

3.4 Political outcomes

The political outcomes we study include voting in presidential elections by CZ in 2020 and 2024, based on data from Leip (2020). To analyze political attitudes, we use data from the Cooperative Congressional Election Study (CCES), a national stratified survey administered by YouGov which includes approximately 50k individuals in election years and 15k individuals in other years (Ansolabehere et al., 2020). CCES data allow us to measure respondent party identification, support for tariffs, and self-reported income across select years. As an additional political outcome, we measure state-level internet search related to tariffs using Google Trends.[18]

4 Impacts of Tariffs on Regional Employment and Income

This section evaluates the impact of exposure to US import tariffs, foreign retaliatory tariffs, and US agricultural subsidies on employment and income across US commuting zones over the course of the US-China trade war. Our main specifications regress regional outcomes on region-industry shift-share variables, as in equation (3). We first present results for overall employment rates; we then decompose these results by sector; we examine complementary results for earnings and non-labor income; and we conclude with additional industry-level evidence to assess alternative hypotheses on why import tariffs did not translate into worker gains.

4.1 Regional employment

We begin by estimating the effect of tariffs on cumulative employment changes in more versus less exposed CZs using the following regression model:

$$EPOP_{rt} - EPOP_{rJan18} = \beta_{1t} IMP_{rDec19} + \beta_{2t} RET_{rDec19} + \gamma_t + \lambda_{s(r),t} + \varepsilon_{rt}. \tag{4}$$

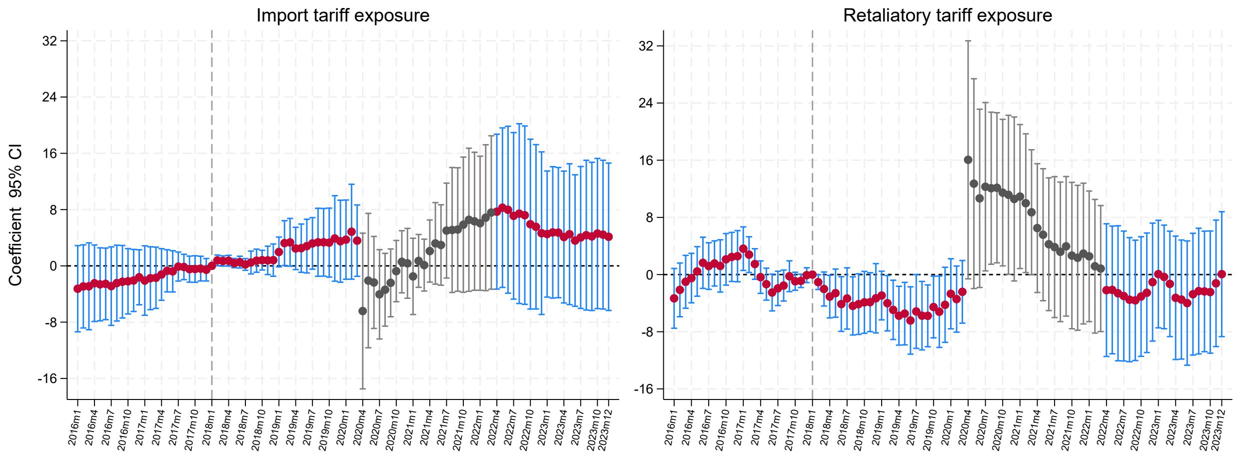
Here, $EPOP_{rt} - EPOP_{rJan18}$ is the change in the seasonally-adjusted employment to population ratio (in percentage points) in CZ r and in month t relative to its baseline value in January 2018 for a sample period that spans January 2016 to December 2023. The variables IMP_{rDec19} and RET_{rDec19} are import and retaliatory tariff exposure levels for CZ r at their peak values in December of 2019.[19]

[18] See Appendix A4.3 for data construction details.

[19] By fixing tariff exposure at a time-invariant value, the model in (4) follows the logic of the local projection approach in Jordà (2005). It estimates whether CZs that ultimately experienced greater tariff exposure exhibited differential employment-rate growth before, during, and after the trade war.

The control variables are a set of year-month indicators γ_t that absorb nonparametric time trends, and interactions $\lambda_{s(r),t}$ between time effects and initial employment shares in the two sectors of manufacturing and agriculture-mining, which absorb sector-specific trends.

Figure 2: Impact of Tariff Exposure on CZ Employment/Population Ratios



Notes: The figure plots by-period coefficient estimates and 95% confidence intervals of the regression model in equation (4). The dependent variable is the change in the seasonally-adjusted CZ employment to population ratio (in percentage points) relative to its January 2018 value. The left panel shows estimates for import tariff exposure, the right panel for retaliatory tariff exposure. Estimates from April 2020 through March 2022, corresponding to the period affected by the pandemic, are shown in gray. Regressions are weighted by 2012 CZ employment, and standard errors are clustered at the state level.

The two panels of Figure 2 report period-specific estimates of cumulative employment effects from import tariffs (left panel) and retaliatory tariffs (right panel) based on equation (4). CZs with eventually greater exposure to import tariffs experienced some employment gains during 2018–2019, with a mild positive pre-trend in 2016–2017; CZs more exposed to retaliatory tariffs showed a downward employment trajectory after the trade war began, with no systematic pattern beforehand. Unsurprisingly, the Covid-19 pandemic marks a clear break in both series.[20] Reflecting CZs' differential industrial composition, the April 2020 employment decline was steeper in CZs with higher import tariff exposure and shallower in CZs with higher retaliatory tariff exposure. In the post-pandemic period from April 2022 to December 2023, there is no evidence of sustained differential employment growth in CZs more exposed to import tariffs, while CZs exposed to retaliatory tariffs show a partial recovery from their 2018–2019 employment decline.

To account for the staggered introduction of tariffs, the dynamics detected above, and the temporary impact of the Covid-19 pandemic, we adjust the specification in (4) by regressing changes in employment-population rates relative to their January 2018 levels on CZs' time-varying exposure to import and retaliatory tariffs IMP_{rt} and RET_{rt}, as well as their exposure to agricultural subsidies

[20] The employment-to-population rate in the US fell by 9 percentage points just from March to April 2020 (see Panel A in Appendix Figure A1), and the recovery from initial pandemic job loss required two years (Panel B in Appendix Figure A1). In Figure 2, estimates from the period affected by the pandemic are shown in gray.

SUB_{rt}, using the following main model:[21]

$$
\begin{aligned}
EPOP_{rt} - EPOP_{rJan18} = {} & \beta_1 IMP_{rt} + \beta_2 RET_{rt} + \beta_3 SUB_{rt} \\
& + \gamma_t + \delta_{d(r),t} + \lambda_{s(r),t} + \phi(\bar{\Delta} EPOP_{r,2017} \times t) + \chi_{st}(\Delta EPOP_{s(r),Mar20-Apr20}) + \varepsilon_{rt}.
\end{aligned} \tag{5}
$$

The sequentially added control variables in this specification include a full set of year-by-month time effects (γ_t), interactions between time effects and initial sectoral employment shares in manufacturing, agriculture and mining, and all other sectors ($\lambda_{s(r),t}$), and interactions between time effects and census division dummies ($\delta_{d(r),t}$). These variables respectively absorb overall, sector-specific, and division-specific time trends. The term $\phi(\bar{\Delta} EPOP_{r,2017} \times t)$ accounts for a linear CZ-specific pre-trend of the outcome variable in the year preceding the trade war. Finally, $\chi_{st}(\Delta EPOP_{s(r),Mar20-Apr20})$ captures the decline in a commuting zone's employment-to-population ratio at the onset of the pandemic from March to April 2020, measured separately for each of the three broad sectors of manufacturing, agriculture and mining, and all other, and interacted with month-year indicators. These controls capture the large employment decline at the start of the pandemic and the subsequent recovery from that employment loss. Our analysis of CZ employment changes will capture the combination of the impact of tariffs on directly-exposed industries and possible local spillover effects, including through input-output linkages, local demand multipliers, and induced labor reallocation across sectors within CZs.

Table 1 presents estimates, focusing first in Panel A on outcomes that cover the trade war period of February 2018 to December 2019, prior to the onset of Covid. In a specification that includes only month-by-year fixed effects plus the tariff measures, we find that both import and retaliatory tariffs are associated with a fall in the employment-population rate. This simple specification may be confounded with sectoral composition: CZs that are highly exposed to tariffs have higher employment shares in tradable sectors, only a subset of which are tariff-protected. Column 2 addresses this confound by controlling for initial sectoral distribution by year-month interactions. This specification finds a positive coefficient for import tariff exposure and a negative coefficient for retaliatory tariff exposure. Both are consistent with the evidence in Figure 2. Subsequent columns additionally control for Census-division-by-year-month interactions (column 3), a one-year pre-trend (column 4), and farm subsidy exposure (column 5). The estimated positive effect of import tariffs on employment rates becomes weaker and statistically insignificant with the addition of these controls. The estimated effect of retaliatory tariffs, however, remains negative and precisely estimated across specifications. As shown in column 5, farm subsidies have a measurable positive

[21] By measuring changes in employment rates relative to January 2018, this specification absorbs level differences in the outcome variable just prior to the trade war, distinct from a specification with CZ fixed effects that would absorb level differences averaging over pre- and post-treatment periods. We use tariff exposure as of December 2019 for all later months through December 2023, since tariffs were largely stable over that period.

employment impact.

Table 1: Impact of Tariff Exposure on CZ Employment

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Years 2018-2019						
Import tariff exposure	-2.848	3.314	2.126	1.744	1.929	1.741
	(1.554)	(1.680)	(1.058)	(1.144)	(1.119)	(1.137)
Retaliatory tariff exposure	-3.857	-6.606	-6.285	-5.131	-5.554	-4.587
	(1.953)	(2.005)	(2.215)	(1.783)	(1.796)	(1.503)
Farm subsidies per capita					0.154	0.218
					(0.064)	(0.073)
Panel B: Years 2018-2023						
Import tariff exposure	-2.900	3.367	1.102	0.897	0.912	1.354
	(1.748)	(3.156)	(1.771)	(1.656)	(1.653)	(1.518)
Retaliatory tariff exposure	-1.201	0.158	1.271	1.956	1.901	-0.682
	(2.933)	(3.087)	(2.560)	(2.453)	(2.479)	(2.507)
Farm subsidies per capita					0.068	0.187
					(0.079)	(0.072)
Year-month FE	✓	(✓)	(✓)	(✓)	(✓)	(✓)
Sector × year-month FE		✓	✓	✓	✓	✓
Census division × year-month FE			✓	✓	✓	✓
t × (pre-trend CZ outcome 2017)				✓	✓	✓
Δ emp_{sector}/pop Mar-Apr 2020 × year-month FE						✓

Notes: N=16,606 (722 CZs x 23 months from Feb. 2018 to Dec. 2019) in Panel A, and N=51,262 (722 CZs x 71 months from Feb. 2018 to Dec. 2023) in Panel B. The dependent variable for all regression models is the seasonally-adjusted employment-to-population ratio, which is indexed to 0 in Jan. 2018 in each CZ. The mean (standard deviation) of the employment-to-population ratio prior to indexing is 65.71 (7.43) percentage points in Jan. 2018. Regressions in columns (2) to (6) interact time fixed effects with a CZ's sectoral employment shares (agriculture and mining, manufacturing, non-goods sector) in 2012, while columns (3) to (6) also interact time fixed effects with indicators for the 9 geographic Census divisions. Regressions in columns (4) to (6) further control for the monthly change in a CZ's employment-to-population ratio from Jan. 2017 to Jan. 2018, interacted with a linear time trend (the count of months since Jan. 2018). Column (6) controls for interactions of time fixed effects with a CZ's March-to-April 2020 employment-to-population ratio change by sector (agriculture and mining; manufacturing; and the non-goods sector). Regressions are weighted by CZ employment in 2012, and standard errors are clustered by state.

Panel B of Table 1 extends the time horizon to cover the years 2020 to 2023. Since this period comprises the Covid-19 pandemic, which temporarily caused differential employment changes in tariff-exposed regions (Figure 2), the specification in column 6 includes controls for sector-specific declines in employment rates at the onset of the pandemic, interacted with month-year effects to capture recovery from the pandemic. For comparability, the last column of Panel A includes the same controls for the shorter outcome period through 2019. The results in column 6 of Panel B indicate an imprecisely estimated positive coefficient estimate for import tariffs, which is close to the corresponding estimate in Panel A, suggesting that US tariffs did not spur substantial deferred employment growth in regions with protected industries. Conversely, the negative estimate for retaliatory tariffs is small and less precisely estimated over the longer horizon, suggesting employment recovery in tariff-affected regions, as seen in Figure 2. The longer-term estimates continue to show job gains due to agricultural subsidies.[22]

To quantify economic magnitudes, we multiply the point estimates in the final column of Table 1 by average CZ-level tariff and subsidy exposure during the outcome periods. For the period through 2019, this yields an average +0.05%pt gain in the employment rate due to import tariffs and an offsetting -0.07%pt decline due to retaliatory tariffs. The estimated employment average impact of the agricultural subsidies, +0.01%pt, offsets only a minor part of the average retaliatory tariff effect. Over the longer time period through December 2023, the same scaling exercise yields an employment gain of +0.07%pt due to import tariffs, and now smaller effects of -0.014%pt due to retaliatory tariffs and +0.003%pt due to agricultural subsidies.

4.2 Sector-level tariff impacts

To understand the sectoral contributions to these modest aggregate effects, Table 2 estimates how tariffs and subsidies affect CZ employment by broad sector. We decompose employment into seven categories: within the primary sector, crop production and other primary activities; within manufacturing, industries with above-average exposure to tariff-protected inputs and other manufacturing; and within all other sectors, transportation and warehousing, business services, and a residual category.

Applying equation (5) to these seven sectors, we estimate the impact of tariffs and subsidies on sector-specific employment rates. The first column of Table 2 repeats the final specification (column 6) of Table 1, while the subsequent columns decompose these point estimates into their sectoral components. Import tariffs have no precisely estimated employment impact in any tradable sector over either 2018–2019 (panel A) or 2018–2023 (panel B). Notably, columns 4 and 5 find no evidence for job gains in manufacturing, the intended beneficiary sector of import tariffs. To the degree that we detect any effect of protective tariffs on employment, this impact is found in business services (column 7) and in the large 'residual' sector (column 8).

Prior literature emphasizes that import tariffs may hinder employment growth in manufacturing sectors that source many of their inputs from tariff-protected sectors, since these sectors are themselves likely to raise prices (Flaaen and Pierce, 2021; Javorcik et al., 2022). Given that supplier and customer industries tend to collocate (Ellison et al., 2010), many such spillovers may take place within the CZs that have the largest direct exposure to import tariffs. In Appendix A2, we follow Acemoglu et al. (2016) to derive a measure of industries' exposure to suppliers that are protected by higher import tariffs. Table 2 separately reports local employment outcomes across 3-digit manufacturing sectors that face above-average supplier import exposure (column 4) and below-average

[22] Appendix Table A2 confirms these results under two alternative specifications: a two-year pre-trend control (Panel A) and exclusion of the pandemic months April 2020–March 2022 (Panel B). The import-tariff and farm-subsidy coefficients are nearly unchanged; the retaliatory-tariff estimate becomes larger but remains imprecise.

Table 2: Impact of Tariff Exposure on CZ Employment by Sector

	All	Primary sector		Manufacturing		Other sectors		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total effect	Crop prod	Other	> Avg supp imp exposure	< Avg supp imp exposure	Transport, ware-housing	Business svcs	All other
Panel A: Years 2018-2019								
Import tariff exposure	1.741	0.201	0.128	-0.468	0.060	0.163	0.913	0.744
	(1.137)	(0.127)	(0.232)	(0.494)	(0.264)	(0.189)	(0.493)	(0.626)
Retaliatory tariff exposure	-4.587	-0.984	-0.081	0.101	-0.411	-0.808	-1.202	-1.200
	(1.503)	(0.444)	(0.685)	(0.390)	(0.533)	(0.212)	(0.339)	(0.641)
Farm subsidies per capita	0.218	0.024	-0.001	0.016	0.048	-0.013	0.032	0.112
	(0.073)	(0.011)	(0.020)	(0.012)	(0.023)	(0.013)	(0.015)	(0.060)
Panel B: Years 2018-2023								
Import tariff exposure	1.354	0.059	-0.401	-0.792	-0.012	-0.287	0.570	2.218
	(1.518)	(0.093)	(0.312)	(0.521)	(0.351)	(0.478)	(0.876)	(1.252)
Retaliatory tariff exposure	-0.682	-0.703	1.633	1.038	-0.488	-1.363	-0.785	-0.014
	(2.507)	(0.574)	(1.188)	(0.572)	(0.796)	(0.560)	(0.879)	(1.527)
Farm subsidies per capita	0.187	0.018	-0.007	0.010	0.044	-0.009	0.027	0.104
	(0.072)	(0.007)	(0.022)	(0.011)	(0.023)	(0.011)	(0.014)	(0.060)
Sector × year-month FE	✓	✓	✓	✓	✓	✓	✓	✓
Census division × year-month FE	✓	✓	✓	✓	✓	✓	✓	✓
t × (pre-trend CZ outcome 2017)	✓	✓	✓	✓	✓	✓	✓	✓
Δ emp$_{sector}$/pop Mar-Apr 2020 × year-month FE	✓	✓	✓	✓	✓	✓	✓	✓
Employment share in 2017	1.000	0.004	0.009	0.038	0.051	0.034	0.136	0.728

Notes: N=16,606 (722 CZs x 23 months from Feb. 2018 to Dec 2019) in Panel A, and N=51,262 (722 CZs x 71 months from Feb. 2018 to Dec. 2023) in Panel B. The dependent variable for all regression models is the seasonally-adjusted employment-to-population ratio in the indicated subsector, which is indexed to 0 in Jan. 2018 in each CZ. All regressions include the control vector of column 6 in Table 1. Columns (4) and (5) include all 3-digit manufacturing sectors with, respectively, above- and below-average supplier tariff exposure in December 2019, as defined in Appendix A2. Sectors with above-average supplier import tariff exposure are fabricated metal products, machinery, transportation equipment, primary metal, furniture, and wood products. Regressions are weighted by CZ employment in 2012, and standard errors are clustered by state.

exposure (column 5).[23] The (imprecise) coefficient estimates in column 4 indicate that CZs with greater exposure to import tariffs experience larger employment losses in manufacturing industries that face higher input costs, while column 5 shows negligible job impacts in other manufacturing sectors. Counter to the narrative that protected sectors are likely to exhibit positive employment growth over the longer term, we see no positive movement in employment in tariff-protected sectors when comparing the shorter (panel A) to the longer (panel B) horizon.

We find negative employment impacts of retaliatory tariffs for the initial 2018-2019 period but not for the full 2018-2023 period. During the 2018-2019 trade war, exposure to retaliatory tariffs significantly reduced employment in crop production (a primary target sector), transportation and warehousing, and business services, which suggests that reductions in crop production may have adversely affected the local service sector. Over the longer time horizon in panel B of Table 2, impacts of retaliatory tariffs on sectoral employment are smaller in absolute terms and estimated

[23]The sectors with above-average supplier import exposure are fabricated metal products, machinery, transportation equipment, primary metal, furniture, and wood products.

with lower precision in most cases, so that we cannot reject the null of no impact of retaliatory tariffs on overall employment over 2018–2023. The last row of each panel shows that farm subsidies significantly raise employment in crop production, with positive spillovers to other sectors. This suggests that the subsidy income ultimately flowed from farm owners in agriculture to a broader set of goods and services.

4.3 Impacts on earnings and non-labor income

Although tariffs did not raise employment in exposed local labor markets, they may still have generated benefits in the form of higher income levels. We examine the effects of tariffs and subsidies on per-capita income in Appendix Table A3, using BEA local personal income data at the commuting-zone-by-year level.[24]

Tariff-protected CZs did experience modest per-capita income gains during the first two years of the trade war (Panel A, column 1 of Table A3), but these gains accrued primarily to capital rather than labor. Evaluated at average exposure levels in 2018–2019, import tariff protection predicts a modest increase of $157 in annual per-capita income, whereas retaliatory tariff exposure predicts a decline of $99. Roughly two-thirds of both the short-run gains from import tariffs and the short-run losses from retaliatory tariffs reflect changes in capital income, while labor income and transfer income respond only weakly. The immediate income effects of tariffs were thus largely absorbed by local business owners rather than workers.

Over the longer horizon through 2023, even these capital-income effects diminish, and Panel B of Table A3 no longer shows precisely estimated effects of tariffs on overall income. The effect of import tariffs on labor income remains muted throughout, providing no evidence that the tariffs generated delayed labor-market gains.

4.4 Why import tariffs failed to deliver worker gains

The results above show that US import tariffs did not generate substantial employment or earnings gains for workers in exposed local labor markets, even years after their introduction. Why did protective tariffs fail to deliver tangible worker benefits? Appendix A2 reports industry-level analyses that complement the CZ-level evidence and point to a clear mechanism.[25]

Tariffs did reach industries as policy intended. Using an industry-level version of the regression model in equation (5), we show that higher US import tariffs reduced US goods imports from targeted countries, with only a small share of this decline offset by increased imports from non-targeted countries (Appendix Table A8). Tariff-protected industries also saw significant short-run

[24] For this analysis, we use an annual rather than monthly version of the regression model in equation (5). In the annual specification, the outcome variable is normalized relative to 2017, the control variable for pre-trends uses per-capita income growth from 2016 to 2017, and tariff and subsidy exposure in each year is measured as the average monthly exposure over that year.

[25] See Appendix A2 for details on data sources, variable definitions, regression specifications, and discussion of results.

increases in sales during the trade war, with somewhat weaker gains over a longer outcome window through 2023 (columns 1-4 of Appendix Table A9). These findings are consistent with the broader literature reviewed by Fajgelbaum and Khandelwal (2022).

Sales gains did not translate into worker gains because firms responded along two non-labor margins. Consistent with earlier work (Amiti et al., 2019, 2020b; Fajgelbaum et al., 2020, 2021), we find that US manufacturers facing higher import tariffs raised prices (columns 5-6 of Appendix Table A9); half or more of the estimated sales increase reflects these price increases rather than higher production. Industries facing greater import tariffs also increased capital expenditure for machinery by considerably more than payroll, suggesting that the production expansion was partly driven by greater use of non-labor inputs rather than additional hiring (columns 7-10 of Appendix Table A9).

A residual concern is that tariff-protected industries might have expanded in locations where they had not been present before the trade war, in which case CZ-level analysis would understate true employment effects. Industry-level employment analysis rules this out: while Flaaen and Pierce (2021) do not detect significant short-run employment gains from import tariffs in broader manufacturing sectors, our results extend this finding to detailed tradable industries over both 2018–2019 and 2018–2023 (Appendix Table A10).[26]

5 Assessing the Political Spoils of the Trade War

Section 4 found little evidence that the trade war delivered on the Trump administration's stated goal of bringing jobs back to America. We now turn to its second, implicit goal: garnering political support. If tariffs conveyed solidarity with voters in import-competing locations, they may have paid political dividends even absent economic gains. We first assess local support for tariff policies, then quantify effects on party identification, and finally consider partisan vote shares in the 2020 and 2024 presidential elections.

In Figure 3, we document heterogeneity in tariff exposure across CZs according to political orientation. Panels A and B show tariff exposure over time as the trade war intensified for CZs based on their two-party Republican vote share in the 2016 presidential election, in which Donald Trump won (via an electoral college majority) with 48.9% of the two-party vote. In Panel A, we see that exposure to US import tariffs in July 2018 (early in the trade war) varied relatively little across CZs with different political orientations. As US import tariffs rose, they afforded greater protection to Republican-dominated areas. CZs with the strongest US trade protection from China in late

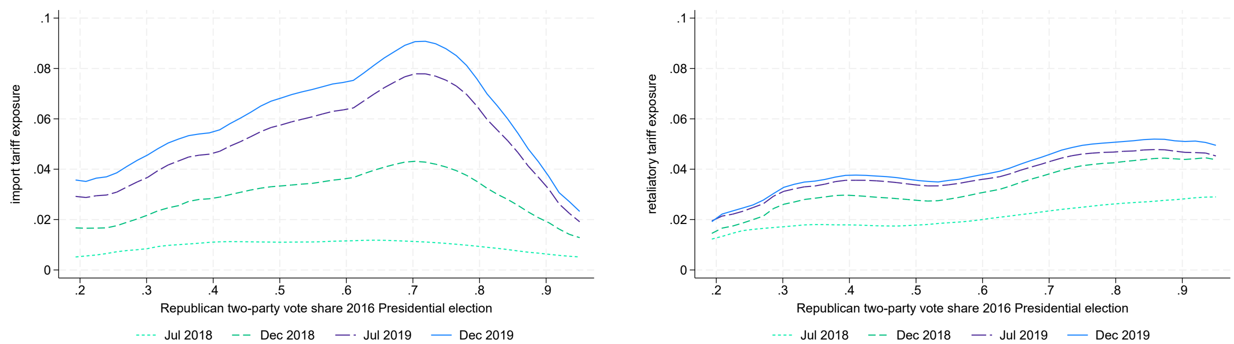
[26]The analysis in Appendix Table A10 also considers industries' indirect exposure to the trade war through supplier and customer industries facing import or retaliatory tariffs. While some results indicate imprecise negative employment effects for industries sourcing inputs from suppliers exposed to import tariffs, the estimates for such spillovers along supply chains are sensitive to specification choices, and including them has only modest effects on the estimated impact of tariffs on directly exposed industries.

2019 had GOP presidential vote shares of just over 70 percent. Exposure to retaliatory foreign tariffs, shown in panel B, was strongest in CZs with the highest GOP presidential vote shares. Agriculturally oriented CZs, which tend to vote heavily Republican, were those most targeted by foreign tariffs. These results are broadly in line with evidence for the early trade war period (Fajgelbaum et al., 2020; Fetzer and Schwarz, 2021; Kim and Margalit, 2021).

Figure 3: CZ Exposure to Import and Retaliatory Tariffs by CZ Political Orientation

(a) Import Tariff Exposure by CZ Political Orientation

(b) Ret. Tariff Exposure by CZ Political Orientation



Notes: Panels A and B show local polynominal smooth plots of CZ exposure to tariffs for CZs with different levels of the Republican two-party vote share in the 2016 presidential election. Industry import and retaliatory tariff exposures are calculated according to equations (1) and (2) and mapped to CZs based on equation (3).

5.1 Support for US tariff policies

To examine voter attitudes toward trade war policies in areas more exposed to tariffs, we draw on data from the Cooperative Congressional Election Study (CCES). In October 2019—shortly after the final major tariff hikes of the trade war had taken effect—the CCES asked respondents whether they supported or opposed two key elements of the Trump administration's trade policy: "Tariffs on 200 billion US dollars worth of goods imported from China"; and "25% tariffs on all imported steel and 10% on imported aluminum, including from Canada and Mexico." At the national level, tariffs on Chinese goods were supported by 50% of respondents and tariffs on imported steel and aluminum by 34%.[27]

In Table 3, we pool answers to the two tariff questions to assess whether respondent support varies systematically with geographic policy exposure by fitting a cross-sectional regression of the form:

$$Y_{jr} = \beta_1 IMP_{rOct19} + \beta_2 RET_{rOct19} + \beta_3 SUB_{rOct19} + \lambda_{s(r)} + \delta_{d(r)} + \boldsymbol{X_j'}\boldsymbol{\pi} + \kappa RVS_{r,16} + \varepsilon_{jr} \quad (6)$$

[27] Unfortunately, the CCES does not regularly ask questions on tariffs, which makes it infeasible to create a panel of responses that would contrast the 2019 data with respondents' views on tariffs prior to the trade war or several years after the trade war.

Here, the outcome variable is the percentage of the tariff policies that respondent j residing in CZ r expressed support for. Alongside the treatment variables (import and retaliatory tariff exposures, farm subsidies), all models include controls for initial CZ sectoral employment shares in manufacturing, agriculture and mining, and all other sectors ($\lambda_{s(r)}$). Some specifications further add a vector of Census division effects $\delta_{d(r)}$ and a vector of respondent characteristics $\boldsymbol{X_j}$, including a quadratic in age, as well as indicator variables for gender, race (non-Hispanic white vs. other), and education (at least some college education vs. high school or less). There is no baseline measure available to gauge the extent to which residents of a CZ supported protectionist policies prior to the trade war. In lieu of such a measure, we use a CZ's Republican two-party vote share in the 2016 Presidential election, $RVS_{r,16}$ as a lagged predictor for its subsequent support for the Trump tariffs.[28] Regressions are weighted by CCES sampling weights, which are normalized to match CZ population in 2010. Standard errors are clustered at the state level.

Table 3: CZ-Level Relationship Between Tariff Exposure and Support for US Import Tariff Policies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Import tariff exposure	9.17	15.42	21.39	24.09	19.85	19.49	24.01	26.00
	(12.55)	(13.83)	(12.00)	(12.25)	(13.28)	(14.61)	(12.12)	(12.24)
Retaliatory tariff exposure	-33.42	-35.78	-42.08	-53.28	-34.76	-31.63	-36.96	-45.61
	(22.05)	(21.50)	(24.74)	(26.90)	(21.71)	(21.84)	(25.31)	(26.93)
Farm subsidies per capita				2.06				1.57
				(1.16)				(1.12)
2016 Repub. TPVS					0.24	0.20	0.21	0.20
					(0.03)	(0.05)	(0.05)	(0.05)
Sector shares	✓	✓	✓	✓	✓	✓	✓	✓
Census division FE		✓	✓	✓		✓	✓	✓
Demographic controls			✓	✓			✓	✓

Notes: N= 17,677 respondents (in 619 CZs). The sample size reduces to N=15,452 with the inclusion of demographic controls. Respondents are asked about their support for tariffs on 200 billion US dollars worth of goods imported from China in 2019, and their support for the 25% tariffs on imported steel and 10% on imported aluminum, including from Canada & Mexico. The outcome variable measures the percentage of these questions that a respondent agreed with among the questions that the respondent answered. It has a mean (standard deviation) of 44.01 (41.99) percentage points. Tariff exposure variables and farm subsidy payments are measured as of October 2019. Demographic controls include a quadratic in age, gender, race/ethnicity (non-Hispanic white vs all other) and education (at least some college education vs high school or less). Regressions are weighted by $\text{pop}_{2010,\text{cz}} \times \left(\frac{\text{CCESweight}_i}{\sum_{i=1}^{\text{cz}} \text{CCESweight}_i}\right)$, and standard errors are clustered by state.

The results in columns 1 to 4 of Table 3 indicate that local support for tariff policies in 2019 corresponds closely with the presumptive local economic impacts of those policies. Residents of CZs receiving greater tariff protections were more supportive of import tariff policies, while residents of CZs facing higher retaliatory tariffs were less supportive. Similarly, support for tariff protections

[28] The two-party vote share measures the share of votes for the Republican presidential candidate as a fraction of all votes cast for the Republican or Democratic candidate while disregarding votes for third-party candidates.

appears higher in CZs receiving greater farm subsidies. While these estimates are not in all cases statistically significant, they are quite stable across specifications.

We documented in panel A of Figure 3 above that CZs that received greater effective import tariff protections during the trade war voted more heavily for Trump in the 2016 Presidential election, i.e., prior to the start of the trade war. To assess whether the greater support for import tariffs in these locations mainly reflects preexisting support for President Trump, we additionally control for the Republican two-party vote share in the 2016 presidential election in columns 5 through 8 of Table 3. Support for tariffs in 2019 was substantially higher in CZs that voted more heavily for Trump in 2016: the coefficient of 0.20 on $RVS_{r,16}$ in the final column of the table indicates that a 10 point higher Republican vote share in 2016 predicts an additional 2 points of support for the import tariffs imposed by the Trump administration. But including this control only modestly affects the coefficients of interest. Holding constant the Republican vote share in 2016, residents of CZs that subsequently faced greater exposure to import tariffs were more likely to support the Trump administration's tariff policies while residents with CZs facing higher retaliatory tariffs were less supportive.[29]

These public opinion results indicate that even if tariffs missed their economic targets, they reached their *political* targets: comparing two CZs, one at the 75th and the other at the 25th percentile of import tariff exposure in 2019, the estimate in column 8 of Table 3 predicts that support for tariffs would be about 1.22%pt higher in the more exposed CZ. Nor is voter support for tariff policies simply a proxy for earlier voter support for the President. Even accounting for the robust relationship between voter support for Trump in 2016 and support for his tariff policies in 2019, CZs that received greater tariff protections voiced more support while those that faced greater retaliatory tariffs voiced less support.

5.2 Political identification

Did public support for tariffs in tariff-protected local labor markets translate into subsequent electoral support for the Trump and the Republican party? The answer is not a foregone conclusion. Policy conflicts may increase the salience of political identity and cement group affiliations even when policies are not in the narrow economic interest of those supporting them (Shayo, 2009; Bonomi et al., 2021). This is particularly true for trade policy, a topic that divides affluent, college-educated consumers from blue collar workers and manufacturing-intensive communities (Davenport et al., 2022; Pierce and Schott, 2020; Grossman and Helpman, 2021). Reflecting this divide, a 2021

[29]By averaging the answers of two tariff questions, we get a slightly larger sample size and higher precision for the analysis in Table 3. When considering the two questions separately in Table A4, we obtain the same sign pattern for both tariff questions. While the point estimates suggest a stronger impact of import tariff exposure on support for metal tariffs and a stronger impact of retaliatory tariff exposure on support for China import tariffs, we cannot reject the null hypothesis that each type of tariff exposure has equally large impacts on the answers to both tariff questions.

Gallup poll found that 40% of voters with no more than a high school education viewed foreign imports as a threat to the economy versus only 25% of college graduates (Gallup Organization, 2021). This gap is actually larger than the pure partisan divide on trade policy: the majority of Republican voters (51%) view trade as a threat versus only 40% of Democratic voters.

To assess whether trade policy cements or erodes party identification, we again draw on the CCES, which provides a detailed window into party identification at annual frequency. Using pooled CCES data for years 2018–2019 or 2018–2023, we study how trade policy affects party identification by estimating linear probability models of the form:

$$\begin{aligned} Y^p_{jrt} - \bar{Y}^p_{r,17} = {} & \beta_1 IMP_{rt} + \beta_2 RET_{rt} + \beta_3 SUB_{rt} + \gamma_t + \lambda_{s(r),t} + \delta_{d(r),t} \\ & + \boldsymbol{X_j'}\boldsymbol{\pi} + \phi(\Delta\bar{Y}^p_{r,16-17} \times t) + \rho\bar{Y}^p_{r,17} + \chi_{st}(\Delta EPOP_{s(r),Mar20-Apr20}) + \varepsilon_{jrt}. \end{aligned} \tag{7}$$

The dependent variable in this equation is an indicator Y^p_{jrt} equal to 100 if voter j residing in CZ r in year t self-identifies as belonging to group $p \in$ {Republican, Democrat, Independent}. This variable is demeaned relative to its CZ-wide baseline level in 2017, so that the CZ-level average of the resulting outcome variable captures changes in party identification relative to the pre-trade war base year.[30] All models include year fixed effects γ_t and interactions of these effects with initial employment shares in manufacturing and in agriculture and mining $\lambda_{s(r),t}$. Further estimates successively add Census division effects interacted with year dummies $\delta_{d(r),t}$, as well as a vector $\boldsymbol{X_j}$ of detailed respondent characteristics: gender, race (non-Hispanic white vs. other), and education (at least some college education vs. high school or less), and a quadratic in age.

Some models further control for a one-year linear pre-trend in the CZ average level of party identification ($\Delta\bar{Y}^p_{r,16-17} \times t$) and for the 2017 CZ-level baseline of party identification ($\bar{Y}^p_{r,17}$), the latter to absorb mean reversion arising from CCES sampling variation in the modest CZ-level sample sizes. For specifications covering the longer time horizon through 2023, which include pandemic-era survey data, we also control for the same March to April 2020 employment declines considered in the labor market analyses. Regressions are weighted by CCES sampling weights scaled to CZ population, and standard errors are clustered at the state level.

Estimates of equation (7) for the 2018-2019 period reported in Table 4 consistently find that voters in more import tariff-exposed CZs became more likely to identify as Republicans or Independents, and less likely to identify as Democrats. Though less precisely estimated, retaliatory tariffs largely have effects of the opposite sign: voters in CZs more exposed to retaliatory tariffs are less likely to identify as Republicans or Independents and more likely to identify as Democrats. When fit to data for the longer time horizon through 2023 (columns 7 and 8), these models find enduring effects of both import tariffs and retaliatory tariffs on party identification with Republicans and

[30] This setup is comparable to the employment and earnings analyses above, where CZ employment rates were measured relative to their baseline values in January 2018.

Table 4: Probability of Voters Identifying as Republicans, Independents or Democrats

	2018-2019						2018-2023	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Identifies as Republican								
Import tariff exposure	13.97	18.34	30.31	10.21	12.93	14.39	13.89	16.12
	(30.27)	(30.88)	(26.75)	(15.32)	(14.42)	(14.15)	(7.79)	(8.32)
Retaliatory tariff exposure	-16.81	-12.70	-2.48	49.15	2.15	-1.63	-18.06	-27.65
	(28.77)	(29.56)	(32.12)	(31.39)	(19.93)	(19.62)	(13.32)	(12.68)
Farm subsidies per capita						1.02	0.99	0.11
						(1.49)	(1.50)	(1.41)
Panel B: Identifies as Democrat								
Import tariff exposure	-45.41	-48.71	-64.77	-49.60	-44.91	-47.80	-17.36	-20.54
	(23.98)	(24.15)	(21.30)	(10.04)	(9.56)	(9.57)	(6.21)	(7.09)
Retaliatory tariff exposure	35.40	27.90	24.87	-2.42	19.21	26.74	16.15	28.11
	(24.34)	(24.06)	(25.67)	(24.42)	(17.38)	(18.72)	(12.89)	(12.81)
Farm subsidies per capita						-2.01	-1.64	-0.83
						(1.48)	(1.47)	(1.39)
Panel C: Identifies as Independent								
Import tariff exposure	31.45	30.37	34.47	26.95	28.80	30.68	2.14	3.46
	(14.26)	(16.05)	(14.75)	(15.64)	(13.61)	(13.10)	(5.70)	(5.85)
Retaliatory tariff exposure	-18.59	-15.20	-22.39	16.74	-8.99	-13.73	6.59	3.19
	(23.13)	(20.12)	(20.20)	(14.67)	(11.93)	(13.00)	(9.38)	(9.12)
Farm subsidies per capita						1.31	0.77	0.76
						(0.66)	(0.73)	(0.76)
Sector × year FE	✓	✓	✓	✓	✓	✓	✓	✓
Census division × year FE		✓	✓	✓	✓	✓	✓	✓
Demographic controls			✓	✓	✓	✓	✓	✓
t × (pre-trend CZ avg. outcome 2016-17)				✓	✓	✓	✓	✓
CZ average outcome in 2017					✓	✓	✓	✓
Δ emp$_{sector}$/pop Mar-Apr 2020 × year FE								✓

Notes: N=74,660 in columns 1-2; the inclusion of demographic controls reduces the sample size to N=72,552 in columns 3-4, and the addition of the pre-trend control further diminishes this number to N=72,536 in columns 5-6, while N=224,601 in columns 7-8. The dependent variable for all regression models is demeaned with its CZ-wide 2017 average. The mean (standard deviation) for the outcome in 2017 before demeaning is 36.03 (48.01) in Panel A, 46.10 (49.85) in Panel B, and 17.87 (38.31) in Panel C. Tariff exposure variables are measured using their October 2018 and October 2019 values for the corresponding survey years, and using December 2019 values for later years. Farm subsidy payments are based on the amounts paid in October of each survey year. Demographic controls include a quadratic in age, gender, race/ethnicity (non-Hispanic white vs all other) and education (at least some college education vs high school or less). Column (8) further controls for a CZ's March-to-April 2020 employment-to-population ratio change by sector (agriculture and mining; manufacturing; and the non-goods sector) interacted with year indicators. Regressions are weighted by $\text{pop}_{2010,\text{cz}} \times \left(\frac{\text{CCESweight}_i}{\sum_{i=1}^{\text{cz}} \text{CCESweight}_i}\right)$, and standard errors are clustered by state.

Democrats, though not on support for Independents.

Evaluated at the sample mean, the column 8 model in Panel A implies that tariffs and subsidies increased the fraction of voters identifying as Republicans by 0.31%pt, as the positive impact of import tariffs (+0.97%pt) dominated the negative effect of retaliatory tariffs (-0.66%pt) and the minimal impact of agricultural subsidies (+0.003%pt). A corresponding calculation yields a -0.58%pt decline in the share of voters identifying as Democrats (-1.23%pt due to import tariffs,

+0.67%pt due to retaliatory tariffs, and -0.02%pt due to subsidies). Given that US presidential elections have in recent years turned on a small number of votes in a handful of states, these partisan realignments are potentially politically significant.

5.3 Electoral outcomes

To test whether shifts in party identification translated into votes, we estimate impacts on the Republican two-party vote share RVS_{rt} in presidential contests, where r denotes CZs and $t \in \{2020, 2024\}$ corresponds to election years. We fit the following model:

$$\begin{aligned} RVS_{rt} - RVS_{r,16} = {} & \beta_1 IMP_{rt} + \beta_2 RET_{rt} + \beta_3 SUB_{rt} + \gamma_t + \lambda_{s(r),t} + \delta_{d(r),t} \\ & + \phi\left(\Delta RVS_{r,12-16} \times \tau(t)\right) + \rho RVS_{r,16} + \chi_{st}(\Delta EPOP_{s(r),Mar20-Apr20}) + \varepsilon_{rt}. \end{aligned} \tag{8}$$

As per earlier models, the outcome variable is the change in the electoral outcome relative to the last pre trade-war period, which in this case is the 2016 election. The regressions control for year main effects and sector-by-year interactions, and in successive specifications, for Census division-by-year interactions, the Republican two-party vote share in 2012–2016 interacted with a time trend to account for trends in partisan support, the 2016 Republican two-party vote share to absorb mean reversion, and sectoral declines in employment rates from March to April 2020 to absorb differential labor market shocks due to the pandemic. Table 5 presents results.

Import tariff exposure significantly increased support for the Republican candidate Trump in the 2020 election (Panel A), with an even larger effect through 2024 (Panel B). Conversely, retaliatory tariffs had a modest and statistically insignificant negative effect on the Republican vote over both time horizons considered. Greater CZ exposure to farm subsidies generated more support for the Republican candidate. Evaluated at the mean values of tariff and subsidy exposure, the column 6 estimates in Panel B imply that import tariffs raised President Trump's two-party vote share by +0.81%pt following the trade war, while the small impacts of retaliatory tariffs (-0.07%pt) and agricultural subsidies (+0.04%pt) combined to a near-zero effect. These results are qualitatively consistent with—and quantitatively slightly larger than—the shifts in party identification documented in Table 4.[31]

The electoral effects of tariffs are further amplified by the spatial distribution of tariff exposure across states and by the Electoral College system. Appendix Table A5 reports counterfactual outcomes for the 2024 presidential election that adjust actual voting results for tariff-induced shifts in

[31] Our results differ from evidence on the short-run electoral impact of the 2018 tariffs, which finds that a unit increase in exposure to retaliatory tariffs reduced the GOP vote share by more than a corresponding increase in exposure to US import tariffs raised it (Kim and Margalit, 2021; Lake and Nie, 2022; Blanchard et al., 2024). Robustness tests in Blanchard et al. (2024) show that the estimated negative effect of retaliatory tariffs becomes weaker when exposure measures incorporate the substantial expansion of tariffs in 2019 and when tariffs are allocated to regions using local employment in 6-digit rather than 3-digit industries, as in our analysis.

Table 5: Republican Two-Party Vote Share in Presidential Elections

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2020 Election						
Import tariff exposure	11.20	9.68	9.79	8.02	8.91	9.37
	(6.54)	(4.03)	(4.08)	(3.57)	(3.92)	(3.92)
Retaliatory tariff exposure	1.50	6.71	6.13	1.09	-2.50	-2.30
	(4.66)	(4.41)	(4.31)	(4.50)	(5.33)	(4.83)
Farm subsidies per capita					4.74	4.46
					(1.32)	(1.44)
Panel B: 2020-2024 Elections						
Import tariff exposure	16.19	13.86	13.85	11.50	12.08	12.26
	(8.94)	(5.81)	(5.76)	(4.96)	(5.17)	(5.26)
Retaliatory tariff exposure	-0.96	5.03	5.09	-1.02	-3.46	-2.81
	(6.59)	(5.95)	(5.73)	(5.91)	(6.33)	(5.61)
Farm subsidies per capita					6.25	5.48
					(1.70)	(1.65)
Sector × year FE	✓	✓	✓	✓	✓	✓
Census division × year FE		✓	✓	✓	✓	✓
$\tau(t)$ × (pre-trend CZ outcome 2012-16)			✓	✓	✓	✓
2016 Repub. TPVS				✓	✓	✓
Δ emp_{sector}/pop Mar-Apr 2020 × year FE						✓

Notes: N=722 CZ in Panel A and N=1,444 (722 CZs × 2 elections in 2020 and 2024) in Panel B. The dependent variable in all panels is 100 times the Republican two-party vote share in presidential elections, indexed to 0 in 2016. The mean (standard deviation) of the outcome variable in 2016 prior to indexing is 49.00 (14.56). Tariff exposure variables are measured at their end-of-sample values (December 2019) for the 2020 and 2024 elections. Farm subsidy payments are measured as the average monthly amount received in the corresponding election year. Columns (4) to (6) control for a linear pre-trend interacted with the count of elections since 2016, $\tau(t)$. Column (6) in all panels further includes controls for a CZ's March to April 2020 employment-to-population ratio change by sector (agriculture and mining; manufacturing; and the non-goods sector) interacted with year indicators. Regressions are weighted by 2010 CZ population, and standard errors are clustered by state.

vote shares. These shifts are obtained by scaling each CZ's tariff exposure by the coefficients from column 6 of Panel B in Table 5.[32] While Donald Trump's national vote share falls by 0.77%pt in this counterfactual, the implied declines are substantially larger in several swing states, including Wisconsin (-1.41%pt), Michigan (-1.00%pt), and North Carolina (-1.28%pt). These states received substantial import-tariff protection, as shown in Panel A of Figure 1. The counterfactual shifts would have been large enough to flip Wisconsin and Michigan, awarding their 25 electoral

[32]For each CZ, we compute a counterfactual Republican two-party vote share, multiply it by total two-party votes in the CZ to obtain a counterfactual vote count, aggregate these counts to the state level, and then compute counterfactual statewide two-party vote shares. For CZs spanning multiple states, we allocate the CZ's counterfactual Republican vote count across states in proportion to each state's share of the CZ's total two-party votes. Using similar methodology, Autor et al. (2020b) and Lake and Nie (2022) provide counterfactual analyses for the impacts of trade shocks on earlier presidential elections.

votes—4.6% of the Electoral College—to the Democratic candidate. The election would therefore have been exceptionally close: the Republican candidate would still have won the tipping-point state, Pennsylvania, but with only 50.03% of the two-party vote, corresponding to a margin of roughly 4,000 votes and triggering an automatic recount.[33] At the same time, he would have narrowly lost the national popular vote, with a counterfactual two-party vote share of 49.98%.

6 The tariff war's unequal impacts on jobs and elections

A comparison of the labor market results in section 4 and the political results in section 5 indicates consistency in the sign patterns of the tariffs and subsidy variables: The variables that tend to have positive (negative) impacts on employment and earnings also have positive (negative) effects for support of the Trump tariffs and the Republican party. This similarity however does not extend to the magnitude of the predicted effects. Figure 4 illustrates this observation, plotting the distribution across CZs of predicted employment and electoral effects of tariffs and subsidies. Predicted employment effects are obtained by scaling each CZ's exposure with coefficients from column 6 of Panel B in Table 1; predicted electoral effects use coefficients from column 6 of Panel B in Table 5. Predicted effects are indicated for bins of 0.25%pt.

Panel A indicates that the estimated trade war impact on the CZ employment rates through 2023 is near zero (zero plus/minus 0.125%pt) in about 90% of all population-weighted CZs, and nearly always within a quarter of a percentage point of zero. In the average CZ, the predicted employment effect is essentially nil, amounting to a gain of just 0.06%pt gain. Conversely, the predicted effects of tariffs and subsidies on the presidential vote share of the Republican party through 2024 are much larger and range from 0.25%pt to 1.0%pt for most CZs. Local exposure to the trade war thus appears to have benefited the Republican party even in regions where the combined effect of tariffs and subsidies predicts no meaningful employment gain.

Why did the trade war benefit its instigating political party even where it did not produce tangible economic benefits? We consider two potential explanations. The first is that residents in CZs with greater exposure to import tariffs either had differential awareness of these policies or differentially misperceived their economic benefits. The second is that voters support tariffs for non-pecuniary reasons.

6.1 Awareness and misperceptions

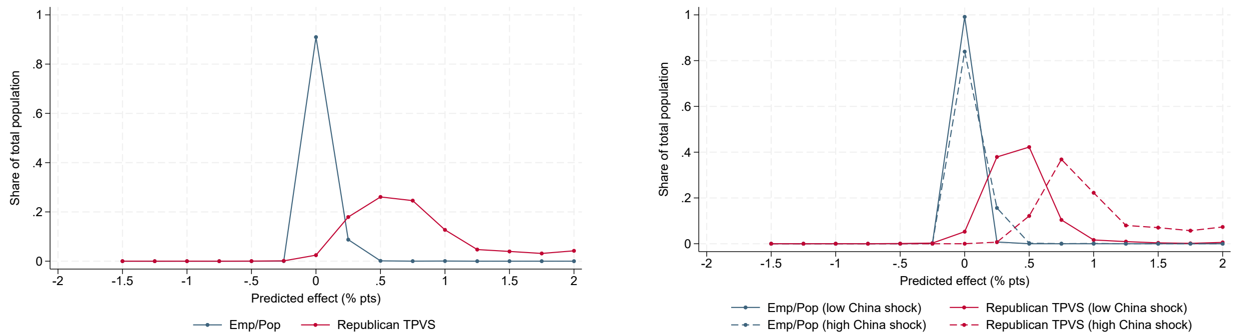
Voters in tariff-exposed regions may have rewarded Trump's tariff policies because they perceived these policies as locally beneficial, even though the economic evidence suggests otherwise. During his presidency, Donald Trump repeatedly claimed credit for job creation in manufacturing firms

[33] Pennsylvania requires a recount for statewide elections when the vote-share margin is half a percentage point or less (Verified Voting, 2026).

Figure 4: Predicted Effect of Tariff Exposure on Employment Rate and Presidential Elections

(a) Full sample

(b) Split by exposure to the 2000-2007 China shock



Notes: Panel (a) scales CZ-level exposures to import tariffs, retaliatory tariffs and agricultural subsidies with coefficient estimates from column 6 in panel B of Table 1 to obtain predicted combined impacts of tariffs and subsidies on CZ employment-to-population ratios; it then applies coefficient estimates from column 6 in Panel B of Table 5 to obtain predicted combined impacts on Republican two-party presidential vote shares. The figure shows the fraction of total population residing in CZs whose predicted values lie within ± 0.125%pt of the value shown on the x-axis, with the rightmost data point comprising all predicted effects >1.875%pt. Panel (b) repeats the same analysis separately for CZs with above- and below-median exposure to Chinese import competition in 2000–2007.

whose hiring decisions appeared to be only weakly linked to presidential actions (see e.g., Kessler, 2017; Timm, 2017; Lane, 2019; O'Neil, 2019; Jacobson, 2020). Such claims may have been especially resonant in tariff-exposed regions through two channels: greater local awareness of tariff policy (perhaps because political communication targeted these regions), or a stronger expectation of local benefit (perhaps reflecting the partisan bias in economic perceptions documented in the political-science literature (Bartels, 2002; Gerber and Huber, 2009)). The concentration of import tariff protection in Republican-leaning regions (Figure 3 above) makes both channels plausible. We test these in turn: differential awareness via Google Trends; and differential perception via CCES survey responses on household income.

Panel A of Appendix Figure A2 reports relative monthly searches for the term "tariffs" nationally (black line) and by state (gray lines), normalized to a national average of 1 in 2016–2017.[34] Web search related to tariffs spiked when new tariffs were introduced, with intensity varying considerably across states. However, cross-state differences in search intensity show no systematic relationship to tariff exposure (panels B and C). This suggests that tariffs were a subject of national rather than local interest.[35]

To test whether residents of tariff-protected regions perceived differential local benefit, we analyze responses to a CCES survey question that asked respondents whether their own household income improved over the preceding year. Using a regression specification similar to equation (6),

[34] Google Trends provides comprehensive data at the state level but not for smaller units such as CZs; see Appendix A4.3 for data construction. We assign to each state a population-weighted average of the tariff exposure in the constituent CZs, with each CZ mapped to the state that comprises all or most of its population.

[35] *New York Times* briefings used the word "tariffs" in 27% of items in 2018, up from 1% in 2017.

we find in Table A6 that residents in CZs with greater exposure to import tariffs were *not* more likely to report income improvements.[36] This null result does not support the hypothesis that tariff-protected CZs perceived differential economic benefits.

6.2 Support for tariffs absent economic gains

Many Republican voters voiced support for the Trump tariffs despite not perceiving them as economically beneficial. In a national poll of registered voters from August 2019 (Harvard Center for American Political Studies, 2019), most GOP voters (86%) agreed that it is necessary for the US to confront China over trade policy, and most (80%) voiced support for the Trump administration's tariffs. However, among these same Republican voters, a majority (60%) agreed that US consumers (rather than China) have to pay for the tariffs, and more than a third (37%) stated that the tariffs hurt the US more than China.[37]

Why might Republican voters support tariffs whose economic costs they themselves acknowledge? One explanation is past experience. Skepticism about international trade and support for isolationist political parties is more widespread in regions exposed to the so-called "China shock," a period of rapidly growing Chinese import competition in the 1990s and 2000s that produced a sharp decline in US manufacturing employment (Colantone and Stanig, 2018; Autor et al., 2020a; Davenport et al., 2022; Autor et al., 2013). Arezki et al. (2024) document that media sentiment toward China remained significantly more negative in China-shock regions through the 2010s, before the onset of the trade war.

Following this logic, we assess whether CZs that were more exposed to the China trade shock in the 2000s responded more positively to a given level of tariff protection. For the purposes of this analysis, we split CZs into those that faced above-median versus below-median Chinese import competition between 2000 and 2007.[38] CZs that experienced a greater China shock in the past had more than twice the import-tariff exposure of low-China-shock CZs (with an average exposure of 0.075 vs. 0.032) and higher higher retaliatory-tariff exposure (0.025 vs 0.018), while exposure to agricultural subsidies differed little (0.017 vs. 0.016).

Panel B of Figure 4 plots the predicted employment and electoral effects of the tariff war separately for CZs with above versus below median China shock exposure. The two subsamples

[36] While Table 3 studies tariff support in the single year 2019, the CCES consistently asked about improvements in household income in every year since 2018. We study perceived household income growth for either the combined 2018-2019 or 2018-2023 waves of the CCES, and thus modify the regression setup of equation (6) by interacting the sector share, Census division indicator, and 2016 Republican vote share controls with year indicators, and in some specifications add controls for employment changes at the onset of the pandemic.

[37] Among Democrats, support for a confrontational approach to China was also more widespread than the belief that tariffs are economically beneficial. A survey of Midwestern farmers (Qu et al., 2019) similarly found that less than a third (30%) of respondents opposed US tariff policy towards China, even though three quarters of them (76%) agreed that US farmers bear the brunt of the tariffs imposed by China, and nearly two thirds (62%) expected US agriculture to lose markets to competitors because of the trade war.

[38] Our metric for CZs exposure to Chinese import competition is taken from Autor et al. (2021). It measures industry-level growth of import penetration from China weighted by initial industry employment shares in a CZ.

differ little in terms of predicted employment effects, which are clustered around zero in either case (the mean predicted employment effect is 0.08%pt in CZs with high China shock exposure and 0.03%pt in those with low exposure). Conversely, the predicted impact of the tariff and subsidy variables on the Republican vote shares is much larger in CZs that had large previous exposure to the China shock (a gain of 1.07%pt in highly exposed CZs vs. 0.46%pt elsewhere). Voters in these regions appear to have rewarded the GOP for confronting China through tariffs, even when the policy delivered no measurable economic gain. This pattern aligns with the survey evidence above: voters value tariffs as a confrontational stance toward China rather than as an expected source of local economic benefit, and they do so most strongly where past China-shock exposure has been deepest.

7 Conclusion

We evaluate whether the tariff war between the United States, China and other US trade partners in 2018–2019 succeeded in meeting then-President Trump's stated goal of bringing back jobs to America, and in generating support for Trump and the Republican party. Since it may take time for the economy to adjust to a new regime of higher tariffs, we look beyond immediate impacts and study longer-term outcomes through 2023/24. We find a sharp contrast between economic and political effects. The net effect of import tariffs, retaliatory tariffs, and farm subsidies on employment in locations exposed to the trade war was largely a wash. US import tariffs had insignificantly positive employment effects; retaliatory tariffs had more significant negative employment impacts in the short run that faded over time; and only a small part of these adverse effects was offset by agricultural subsidies. Beyond their muted impact on jobs, import tariffs also failed to substantially raise earnings in tariff-protected regions, and our results show no evidence of more favorable labor market impacts emerging over a longer time horizon.

Conversely, the trade war appears to have been successful in strengthening support for the Republican party in both the short and longer run. Residents of tariff-protected locations voiced more support for the tariffs, became more likely to identify as Republicans, and were more likely to vote for President Trump in 2020 and 2024. Exposure to retaliatory tariffs only moderately reduced Republican support. Voters thus appear to have responded favorably to the extension of tariff protections to local industries despite their economic costs. Our analysis does not suggest that voters in tariff-protected regions had greater awareness of the tariff policy or overestimated any economic gains from the trade war. Instead, voters may have rewarded the government for its intention to confront China and protect US jobs. Although the goal of bringing back jobs to the heartland remained elusive, voters in regions that had borne the economic brunt of Chinese import competition in the 1990s and 2000s were particularly likely to reward the Trump administration for its tariff policy.

These findings illuminate the political logic of protectionist policy. If voters reward tariffs as confrontation with foreign competitors rather than as a path to local economic benefit, politicians face a persistent incentive to deploy tariffs even when their economic returns are modest or null. This dynamic is especially pronounced in regions whose past exposure to import competition has shaped longstanding dispositions toward trade, suggesting that protectionist coalitions may persist beyond the specific shocks that originally motivated them.

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A1 Appendix Tables and Figures

Table A1: CZ Exposure to Tariffs

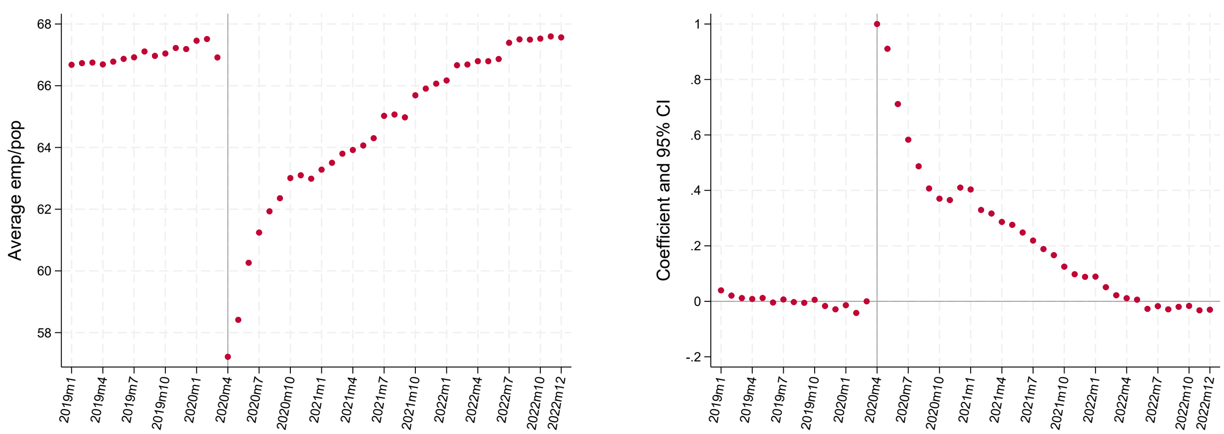
	Commuting zone		
	Import tariff exposure	Retaliatory tariff exposure	Farm subsidies
Exposure February ’18 to December ’19			
Mean	0.031	0.015	0.042
SD	0.036	0.019	0.273
P25	0.007	0.004	0.000
P50	0.023	0.011	0.000
P75	0.043	0.019	0.006
Exposure February ’18 to December ’23			
Mean	0.054	0.021	0.016
SD	0.046	0.021	0.179
P25	0.028	0.011	0.000
P50	0.046	0.018	0.000
P75	0.069	0.025	0.000
Correlations (2019)			
W/imp. tariff	1.000	0.387	0.095
W/ret. tariff	0.387	1.000	0.252

Notes: The top panel reports average tariff exposure, and farm subsidy payments measured in hundreds of 2018 dollars per working age population, from February 2018 to December 2019 across 722 CZs. The second panel extends the sample period through December 2023. Correlations are computed using monthly values from 2019. All statistics are weighted by average CZ employment in 2012.

Figure A1: March-to-April 2020 Decline in Employment-to-Population Ratio

(a) CZ average Emp/Pop by month

(b) Persistence of March-April 2020 decline in Emp/Pop



Notes: Panel (a) indicates the average CZ employment-to-population ratio from January 2019 to December 2022. Panel (b) reports coefficients obtained by regressing the change of the CZ employment-to-population ratio from the indicated month (prior to March 2020) to March 2020, or from March 2020 to the indicated month (after March 2020), on the March-to-April 2020 employment-to-population change.

Table A2: Impact of Tariff Exposure on CZ Employment: Alternative Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Years 2018-2023, 2-Year Pre-Trend						
Import tariff exposure	-2.900	3.367	1.102	0.881	0.898	1.501
	(1.748)	(3.156)	(1.771)	(1.613)	(1.611)	(1.531)
Retaliatory tariff exposure	-1.201	0.158	1.271	0.445	0.381	-2.326
	(2.933)	(3.087)	(2.560)	(2.667)	(2.689)	(2.683)
Farm subsidies per capita					0.077	0.193
					(0.079)	(0.070)
Year-month FE	✓	(✓)	(✓)	(✓)	(✓)	(✓)
Sector × year-month FE		✓	✓	✓	✓	✓
Census division × year-month FE			✓	✓	✓	✓
t × (pre-trend CZ outcome 2016-2017)				✓	✓	✓
Δ emp$_{sector}$/pop Mar-Apr 2020 × year-month FE						✓
Panel B: Years 2018-2023 Excluding Pandemic						
Import tariff exposure	-5.310	4.630	2.118	1.699	1.741	1.365
	(2.456)	(4.012)	(2.088)	(1.787)	(1.783)	(1.544)
Retaliatory tariff exposure	-7.326	-4.201	-2.959	-1.652	-1.793	-1.376
	(3.941)	(2.714)	(2.562)	(2.194)	(2.220)	(2.455)
Farm subsidies per capita					0.106	0.191
					(0.069)	(0.071)
Year-month FE	✓	(✓)	(✓)	(✓)	(✓)	(✓)
Sector × year-month FE		✓	✓	✓	✓	✓
Census division × year-month FE			✓	✓	✓	✓
t × (pre-trend CZ outcome 2017)				✓	✓	✓
Δ emp$_{sector}$/pop Mar-Apr 2020 × year-month FE						✓

Notes: N=51,262 (722 CZs x 71 months from Feb. 2018 to Dec. 2023) in Panel A, and N=33,934 (722 CZs x 47 months from Feb. 2018 to Dec. 2023, excluding Apr. 2020 to Mar. 2022) in Panel B. The dependent variable for all regression models is the seasonally-adjusted employment-to-population ratio, which is indexed to 0 in Jan. 2018 in each CZ. The mean (standard deviation) of the employment-to-population ratio prior to indexing is 65.71 (7.43) percentage points in Jan. 2018. The regressions in each column include the control vector of the corresponding column in Table 1, except that columns (4) to (6) in Panel A control for a 2-year pre-trend from Jan. 2016 to Jan. 2018 instead of a one-year pre-trend. Regressions are weighted by commuting zone employment in 2012, and standard errors are clustered by state.

Table A3: Impact of Tariff Exposure on Income per Capita

	(1) Total income	(2) Labor income	(3) Capital income	(4) Transfer income
Panel A: Years 2018-2019				
Import tariff exposure	5.079	1.259	3.321	0.499
	(2.572)	(1.344)	(1.689)	(0.459)
Retaliatory tariff exposure	-6.576	-2.098	-4.103	-0.375
	(2.608)	(1.566)	(1.761)	(0.366)
Farm subsidies per capita	0.488	0.269	0.130	0.090
	(0.308)	(0.131)	(0.230)	(0.047)
Panel B: Years 2018-2023				
Import tariff exposure	2.796	1.251	0.781	0.764
	(2.462)	(1.958)	(1.330)	(0.850)
Retaliatory tariff exposure	1.687	2.384	-2.977	2.280
	(4.031)	(2.817)	(2.514)	(0.736)
Farm subsidies per capita	0.562	0.245	0.203	0.114
	(0.344)	(0.156)	(0.266)	(0.058)
Sector × year FE	✓	✓	✓	✓
Census division × year FE	✓	✓	✓	✓
t × (pre-trend CZ total income 2016-17)	✓	✓	✓	✓
Δ emp_{sector}/pop Mar-Apr 2020 × year FE	✓	✓	✓	✓
Share of total income in 2017	1.000	0.448	0.294	0.258

Notes: N=2,888 (722 commuting zones × 4 years) in Panel A, and N=5,776 (722 commuting zones × 8 years) in Panel B. The dependent variable is annual income in $1,000 per capita, indexed to zero in 2017. Total income is the sum of labor income (wages and salaries), capital income (proprietor's income, dividends, interests, and rental income), and government transfer income. The mean of the outcome variable in 2017 prior to indexing is 50.8 for total income, 27.1 for labor income, 14.8 for capital income, and 8.8 for transfer income. Tariff exposure variables are measured based on their average monthly value for the years 2018 and 2019, and based on their end-of-sample value (Dec. 2019) for years 2020–2023. Farm subsidy payments are based on the average monthly amount paid in a given year. Regressions are weighted by CZ employment in 2012, and standard errors are clustered by state.

Table A4: CZ Residents' Support for US Import Tariffs in 2019

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Support for Steel and Aluminum Import Tariff								
Import tariff exposure	29.60	31.98	40.00	42.25	36.02	33.47	41.90	43.64
	(14.11)	(13.55)	(13.06)	(12.69)	(13.18)	(13.86)	(13.50)	(13.21)
Retaliatory tariff exposure	-6.16	-5.50	-25.69	-35.19	-7.03	-4.04	-22.11	-29.82
	(30.21)	(31.44)	(30.72)	(33.26)	(30.95)	(31.38)	(31.22)	(33.41)
Farm subsidies per capita				1.74				1.39
				(1.48)				(1.47)
2016 Repub. TPVS					0.15	0.07	0.15	0.14
					(0.04)	(0.06)	(0.06)	(0.06)
Panel B: Support for China Import Tariff								
Import tariff exposure	-8.45	1.91	6.77	9.96	6.77	8.77	10.28	12.54
	(23.00)	(26.54)	(22.73)	(23.76)	(25.27)	(26.72)	(22.24)	(23.01)
Retaliatory tariff exposure	-60.25	-66.17	-59.31	-72.98	-62.37	-59.45	-52.72	-62.94
	(24.48)	(21.86)	(21.71)	(24.81)	(21.47)	(22.23)	(22.06)	(24.48)
Farm subsidies per capita				2.57				1.89
				(1.23)				(1.16)
2016 Repub. TPVS					0.35	0.33	0.27	0.27
					(0.04)	(0.05)	(0.06)	(0.06)
Sector shares	✓	✓	✓	✓	✓	✓	✓	✓
Census division FE		✓	✓	✓		✓	✓	✓
Demographic controls			✓	✓			✓	✓

Notes: N=17,549 respondents (in 619 CZs) in Panel A and N= 17,616 respondents (in 618 CZs) in Panel B. The sample size reduces to N=15,350 (Panel A) and N=15,404 (Panel B), respectively, with the inclusion of demographic controls. Panel A asks respondents whether they support the 25% tariffs on imported steel and 10% on imported aluminum, including from Canada & Mexico; Panel B asks respondents on the support for tariffs on 200 billion US dollars worth of goods imported from China in 2019. The outcome variable takes a value of 100 if the respondent is supportive of the tariff and zero if not. The mean (standard deviation) for the outcome is 35.6 (47.9) in Panel A and 52.3 (49.9) in Panel B. Tariff exposure variables and farm subsidy payments are measured as of October 2019. Demographic controls include quadratic in age, gender, race/ethnicity (non-Hispanic white vs all other) and education (at least some college education vs high school or less). Regressions are weighted by $\text{pop}_{2010,\text{cz}} \times \left(\frac{\text{CCESweight}_i}{\sum_{i=1}^{\text{cz}} \text{CCESweight}_i}\right)$, and standard errors are clustered by state.

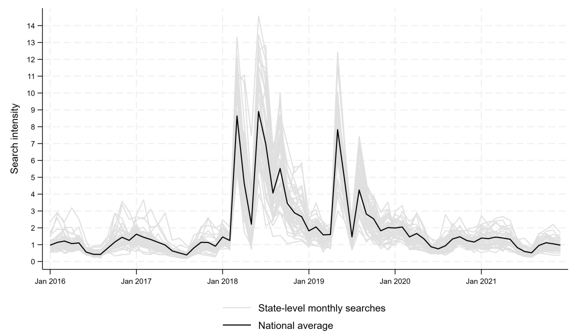
Table A5: Counterfactual Results for the 2024 Presidential Election

	Republican TPVS			Republican electoral votes	
State	Realized	Counterfactual	Difference	Realized	Counterfactual
Swing states	51.21%	50.29%	-0.91%	93	68
Arizona	52.79%	52.35%	-0.45%	11	11
North Carolina	51.63%	50.35%	-1.28%	16	16
Nevada	51.58%	51.27%	-0.31%	6	6
Georgia	51.10%	50.42%	-0.68%	16	16
Pennsylvania	50.86%	50.03%	-0.83%	19	19
Michigan	50.72%	49.72%	-1.00%	15	0
Wisconsin	50.44%	49.03%	-1.41%	10	0
Red states	59.63%	58.82%	-0.81%	219	219
Blue states	42.28%	41.61%	-0.67%	0	0
National	50.75%	49.98%	-0.77%	312	287

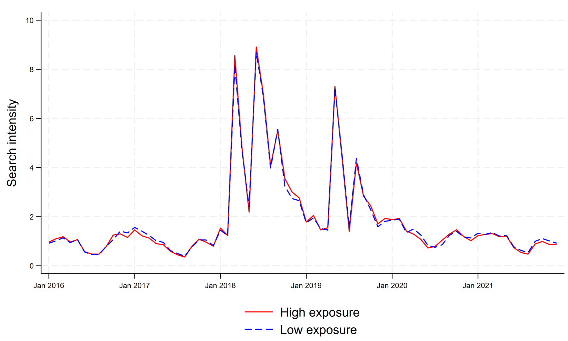
Notes: CZ exposures to import tariffs, retaliatory tariffs, and agricultural subsidies are scaled by the coefficient estimates from column 6 in Panel B of Table 5 to obtain the predicted combined impacts of tariffs and subsidies on Republican two-party presidential vote shares (TPVS). These predicted effects are then subtracted from realized Republican TPVS to obtain counterfactual TPVS from CZs. The counterfactual CZ-level results are then aggregated to counterfactual state-level results. Since the CZ regression analysis does not include Alaska and Hawaii, their vote shares are unchanged in the counterfactual. The seven swing states are those that were predicted to be most competitive prior to the election (Mongrain and Stegmaier, 2025), while red states and blue states are non-competitive states that were won (lost) by Donald Trump in both the 2020 and 2024 elections.

Figure A2: Internet Searches for "Tariffs"

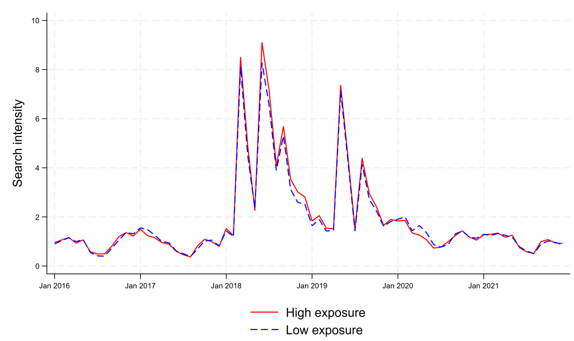
(a) all states



(b) by exposure to import tariffs



(b) by exposure to retaliatory tariffs



Notes: Grey lines in Panel (a) indicate monthly search intensities for the term "tariffs" according to Google Trends for each US state in the period of January 2016 to December 2021, while the black line indicates the population-weighted national average of the state-level series. The subsequent panels separately average the state-level series for states with above-average vs. below-average import tariff (Panel b) or retaliatory tariff exposure (Panel c), where a state's tariff exposure is measured as a population-weighted average of the December 2019 tariff exposure of all CZs whose population is mainly or entirely located in the state. All data series are scaled such that national search intensity has an average value of 1 in the pre-trade war period of January 2016 to January 2018.

Table A6: CZ Residents' Self-Reported Growth of Household Income

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Years 2018-2019						
Import tariff exposure	-43.96	-0.34	3.26	2.73	3.02	2.32
	(29.77)	(11.12)	(13.04)	(13.03)	(13.15)	(12.32)
Retaliatory tariff exposure	-31.37	-25.63	-25.45	-24.15	-23.79	-21.99
	(32.56)	(15.47)	(16.45)	(16.53)	(16.14)	(15.63)
Farm subsidies per capita				-0.35	-0.46	-0.34
				(0.99)	(0.98)	(1.03)
Panel B: Years 2018-2023						
Import tariff exposure	-11.58	-0.36	-0.21	-0.45	-0.37	-0.52
	(7.07)	(2.73)	(3.09)	(3.07)	(3.11)	(2.92)
Retaliatory tariff exposure	-11.84	-8.41	-8.82	-7.99	-7.86	-7.55
	(10.38)	(5.05)	(5.82)	(5.70)	(5.58)	(5.33)
Farm subsidies per capita				-0.71	-0.81	-0.67
				(0.99)	(0.98)	(1.03)
Sector shares × year FE	✓	✓	✓	✓	✓	✓
Census division × year FE		✓	✓	✓	✓	✓
Demographic controls			✓	✓	✓	✓
Repub 2016 TPVS × year FE					✓	✓
Δ emp$_{sector}$/pop Mar-Apr 2020 × year-month FE						✓

Notes: In Panel A, N=77,608 respondents (in 685 CZs) in columns 1-2, and N=75,356 in columns 3-6 after inclusion of demographic controls. In Panel B, N=246,753 respondents (in 712 CZs) in columns 1-2, and N=232,666 in columns 3-6 after inclusion of demographic controls. Respondents are asked about their perception of the household's income over the last year (HH income decreased a lot / somewhat / same / increased somewhat / a lot). The outcome variable takes the value of 100 if a respondent reports that household income increased somewhat or a lot, and 0 otherwise. The mean (standard deviation) for the outcome is 33.62 (47.24) in Panel A and 11.35 (31.49) in Panel B. Tariff exposure variables are measured using their October 2018 and October 2019 values for the corresponding survey years, and using December 2019 values for later years. Farm subsidy payments are based on the amounts paid in October of each survey year. Demographic controls include a quadratic in age, gender, race/ethnicity (non-Hispanic white vs all others) and education (at least some college education vs high school or less). Regressions are weighted by $\text{pop}_{2010,\text{cz}} \times \left(\frac{\text{CCESweight}_i}{\sum_{i=1}^{\text{cz}} \text{CCESweight}_i}\right)$, and standard errors are clustered by state.

ONLINE APPENDIX

A2 Industry-Level Analysis

A2.1 Tariff Exposure

Industries are directly exposed to U.S. import tariffs and foreign retaliatory tariffs according to equations (1) and (2). The tariff exposure of industries may propagate further along national supply chains, both downstream to the industries' customers and upstream to their suppliers. Using the harmonized U.S. 2012 input-output tables from BEA (2018), we account for these linkages, following the approach in Acemoglu et al. (2016). We calculate the downward propagation of a demand shock to a tariff-exposed supplier industry i to its customer industry g as,

$$\hat{x}_{gu}^{down} = \sum_i \frac{\delta_{gi}}{\delta_g} \hat{x}_{iu}, \tag{A1}$$

where δ_{gi}/δ_g is the share of inputs from industry i in total inputs purchased by industry g. Similarly, we calculate the upward propagation of the demand shock to a tariff-exposed customer industry i to its supplier industry h as,

$$\hat{x}_{hu}^{up} = \sum_i \frac{\delta_{ih}}{\sum_l \delta_{lh}} \hat{x}_{iu}, \tag{A2}$$

where $\delta_{ih}/\sum_l \delta_{lh}$ is the share of industry i in total sales of industry h. While the equations above describe the first-order linkages between customers and suppliers for simplicity, we use the full Leontief inverse of these relationships in the empirical analysis below.

Table A7 reports exposure to import and retaliatory tariffs across 373 tradable industries. Direct exposure to import tariffs is essentially uncorrelated with direct exposure to retaliatory tariffs, with a correlation coefficient of only 0.084. However, industries directly exposed to import tariffs tend to source inputs from, and sell outputs to, other industries that are themselves protected by import tariffs. Similarly, industries exposed to retaliatory tariffs tend to trade more with other industries exposed to retaliatory tariffs.

A2.2 Impacts of Tariffs on Industry-Level Trade, Sales, Prices, and Inputs

This section reports impact estimates for the impact of direct exposure to US and foreign tariffs on industry-level outcomes, including trade, sales, producer prices, and expenditures for inputs. Since the effect of the trade war on US imports and exports in targeted categories has already received extensive study, our analysis of these outcomes is purely confirmatory. We fit the specification

$$X_{it} - X_{iJan2018} = \beta_1 IMP_{it} + \beta_2 RET_{it} + \lambda_{s(i),t} + \phi(\bar{\Delta} X_{i,2017} \times t) + \varepsilon_{it}, \tag{A3}$$

Table A7: Industry Exposure to Tariffs

	Import tariff exposure	Retaliatory tariff exposure	Supplier import tariff exposure	Customer import tariff exposure	Supplier retaliatory tariff exposure	Customer retaliatory tariff exposure
Exposure February '18 to December '19						
Mean	0.303	0.148	0.243	0.141	0.236	0.105
SD	0.654	0.427	0.268	0.227	0.318	0.172
P25	0.000	0.000	0.053	0.008	0.034	0.004
P50	0.030	0.023	0.152	0.042	0.165	0.049
P75	0.318	0.142	0.339	0.173	0.290	0.137
Exposure February '18 to December '23						
Mean	0.528	0.206	0.317	0.226	0.301	0.134
SD	0.899	0.494	0.261	0.293	0.364	0.198
P25	0.006	0.005	0.125	0.022	0.109	0.017
P50	0.187	0.066	0.270	0.105	0.205	0.071
P75	0.738	0.210	0.424	0.309	0.318	0.190
Correlations (2019)						
W/imp. tariff	1.000	0.084	0.171	0.251	-0.169	-0.016
W/ret. tariff	0.084	1.000	-0.068	0.154	0.002	0.230

Notes: The table reports direct tariff exposures and exposures through input-output linkages for the 373 tradable industries. The top panel measures average import and retaliatory tariff exposure from February 2018 to December 2019; the second panel reports the same measures for the February 2018–December 2023 period. Correlations are computed using monthly values from 2019. Statistics are weighted by average industry employment in 2012.

where X_{it} is either an import penetration ratio or an export-to-shipment ratio for U.S. industry i in year-month t from February 2018 to December 2019, which we relate to an industry's direct exposure to import tariffs IMP_{it} and retaliatory tariffs RET_{it}. We use 2012 as a base year when computing the denominator of the two ratios. Controls include a full set of year-by-month effects γ_t, and in some specifications a complete set of interactions $\lambda_{s(i),t}$ between year-month and broad sectoral dummies for manufacturing, and agriculture and mining. As with the CZ-level models, some specifications further control for a linear time trend in the observed change of the outcome variable in the year preceding the start of the trade war.

Table A8 presents results. Panel A indicates that exposure to import tariffs reduced import penetration for the protected U.S. industries, while exposure to retaliatory tariffs reduced the exports of U.S. industries. To assess the economic magnitude of the import tariff effect, we multiply the point estimate in the second column of Table A8 with the average industry-level tariff exposure

Table A8: Impact of Tariff Exposure on Industry Imports and Exports

	Imports			Exports		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Monthly Trade Total						
Import tariff exposure	-1.543	-1.631	-1.527	-0.457	-0.336	-0.167
	(0.405)	(0.417)	(0.405)	(0.238)	(0.214)	(0.141)
Retaliatory tariff exposure	-1.237	-1.111	-1.060	-1.998	-2.166	-1.793
	(0.756)	(0.766)	(0.741)	(0.668)	(0.720)	(0.580)
Panel B: Monthly Trade with China						
Import tariff exposure	-2.099	-2.084	-1.725	0.178	0.124	0.126
	(0.611)	(0.624)	(0.375)	(0.074)	(0.070)	(0.067)
Retaliatory tariff exposure	-0.219	-0.234	0.088	-0.913	-0.828	-0.829
	(0.440)	(0.465)	(0.246)	(0.168)	(0.171)	(0.170)
Panel C: Monthly Trade with Other Trade War Countries						
Import tariff exposure	-0.220	-0.248	-0.233	-0.437	-0.348	-0.265
	(0.187)	(0.209)	(0.201)	(0.156)	(0.145)	(0.098)
Retaliatory tariff exposure	-0.481	-0.440	-0.448	-0.634	-0.758	-0.577
	(0.315)	(0.310)	(0.303)	(0.472)	(0.495)	(0.336)
Panel D: Monthly Trade with Other Low-Wage Asia						
Import tariff exposure	0.400	0.381	0.412	-0.085	-0.078	-0.072
	(0.135)	(0.138)	(0.140)	(0.062)	(0.060)	(0.048)
Retaliatory tariff exposure	-0.087	-0.065	-0.045	-0.006	-0.017	-0.032
	(0.157)	(0.154)	(0.130)	(0.056)	(0.059)	(0.064)
Panel E: Monthly Trade with Rest of the World						
Import tariff exposure	0.375	0.320	0.350	-0.113	-0.034	0.025
	(0.225)	(0.227)	(0.220)	(0.120)	(0.110)	(0.066)
Retaliatory tariff exposure	-0.450	-0.372	-0.359	-0.445	-0.563	-0.283
	(0.249)	(0.258)	(0.259)	(0.294)	(0.333)	(0.258)
Year-month FE	✓	(✓)	(✓)	✓	(✓)	(✓)
Sector $\times$ year-month FE		✓	✓		✓	✓
t $\times$ (monthly Δ dep. var. in 2017)			✓			✓

Notes: N=8,579 (373 tradable industries x 23 months from Feb. 2018 to Dec. 2019). The dependent variable is the seasonally-adjusted monthly import penetration ratio (columns 1-3) or export-to-shipment ratio (columns 4-6), which are indexed to 0 in Jan. 2018 in each industry. Panel A includes US trade with all other countries, Panel B includes only trade with China, Panel C includes trade with other trade war countries (Canada, EU, India, Mexico, Russia, Turkey, and the UK), Panel D includes trade with low-wage Asian economies (Bangladesh, Indonesia, Malaysia, Philippines, Thailand, and Vietnam), and Panel E includes trade with the rest of the world. The mean (standard deviation) of the import penetration ratio in Jan. 2018 prior to indexing is 26.91 (26.43) for Panel A, 6.87 (12.02) for Panel B, 12.31 (13.21) for Panel C, 1.86 (5.13) for Panel D and 5.87 (8.31) for Panel E. The mean (standard deviation) of the export-to-shipment ratio in Jan. 2018 prior to indexing is 19.01 (19.99) for Panel A, 1.31 (2.51) for Panel B, 11.44 (11.84) for Panel C, 0.65 (2.06) for Panel D and 5.61 (7.89) for Panel E. The regressions in columns 2 to 3 and 5 to 6 interact time fixed effects with indicators for the two tradable sectors (agriculture and mining, manufacturing). Regressions in columns 3 and 6 additionally control for the monthly change in the dependent variable from Jan. 2017 to Jan. 2018, interacted with a linear time trend (the count of months since Jan. 2018). Regressions are weighted by industry employment in 2012, and standard errors are clustered by industry.

in 2018-2019 from Table A7, and scale this product by the average level of industry-level import penetration prior to the trade war in January 2018. This calculation yields a meaningful -1.7%

decrease in industry-level import penetration due to US import tariffs. An analogous exercise based on column 4 of Table A8 implies a -1.4% decrease in the export-to-shipment ratio due to retaliatory tariffs.

The subsequent panels B to E of Table A8 additively decompose the effects of tariffs on trade by groups of U.S. trade partners. These results provide some evidence for trade diversion. While industries protected by import tariffs imported less from China and from the other trade war countries (i.e., countries that both faced U.S. import tariffs and imposed retaliatory tariffs) according to panels B and C, these industries instead imported more from non-trade war countries according to panels D and E. In particular, import tariff-protected U.S. industries responded by significantly expanding their imports from low-wage Asian countries other than China, thus partially compensating for reduced imports of Chinese goods.

We next extend the industry-level analysis to consider annual sales of tariff-protected and tariff-targeted industries. Sales data for six-digit manufacturing industries is reported in the Annual Survey of Manufacturers (ASM) in 2018-2021 (U.S. Census Bureau, 2022a), the Economic Census in 2022 (U.S. Census Bureau, 2025), and the Annual Integrated Economic Survey (AIES) in 2023 (U.S. Census Bureau, 2026), which we combine with annual sales for subsectors of agriculture and mining from the Industry Economic Accounts of the Bureau of Economic Analysis (BEA, 2025a).[39] Using data from 2018 through 2023, we fit the following specification,

$$\ln Y_{it} - \ln Y_{i,17} = \beta_1 IMP_{it} + \beta_2 RET_{it} + \lambda_{s(i),t} + \phi(\Delta \ln Y_{i,16-17} \times t) + \chi_t(\Delta \ln E_{i,Mar20-Apr20}) + \varepsilon_{it}. \tag{A4}$$

where the dependent variable is the log change in the outcome variable relative to its pre-trade-war 2017 value, and other variables are defined as in equation (A3), except that tariff exposure is averaged over all months of a year, and the pre-trend control is based on the 2016-2017 change of the outcome. For models with outcomes through 2023, we additionally control for the impact of the pandemic by including the March to April 2020 decline in log industry employment interacted with year indicators.

Table A9 presents estimates. Columns 1 and 2 of Panel A indicate that exposure to import tariffs significantly increased sales in protected industries while exposure to retaliatory tariffs had imprecisely estimated negative effects on sales during the trade war period of 2018-2019. The column 2 coefficient estimates in Panel A imply that an industry with average import tariff exposure in 2018-2019 gained 0.72 log points of sales relative to an industry with no such exposure, while an industry with average exposure to retaliatory tariffs lost -0.46 log points of sales.[40] Subsequent columns focus on the manufacturing sector, for which detailed outcomes are available from the

[39] We additionally obtain payroll and machinery capital expenditure data for manufacturing industries from the ASM in 2018-2021 and AIES in 2023, and impute missing data for 2022 under the assumption that the growth of the corresponding outcome variable in 2021-2022 was proportional to sales growth. Producer price data for manufacturing industries in 2018–2023 are obtained from the Bureau of Labor Statistics (BLS, 2024).

[40] Although the sales regressions in Table A9 consider nominal sales, the point estimates are invariant to deflation since these are log additive and hence absorbed by the time effects.

Table A9: Impact of Tariff Exposure on Industry Sales, Prices and Inputs

	log Sales (all tradable ind.)		log Sales (manuf.)		log Prices (manuf.)		log Payroll (manuf.)		log Machinery CapEx (manuf.)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Years 2018-2019										
import tariff exposure	2.413	2.377	1.996	1.973	0.911	0.916	0.182	0.193	8.616	8.676
	(0.922)	(0.929)	(0.838)	(0.850)	(0.521)	(0.380)	(0.473)	(0.481)	(3.462)	(3.446)
retaliatory tariff exposure	-3.032	-3.104	1.691	1.642	-1.422	-2.050	0.064	0.029	-4.959	-4.831
	(3.287)	(3.285)	(2.532)	(2.581)	(1.438)	(1.142)	(1.697)	(1.729)	(10.393)	(10.413)
sector × year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
t × (Δ dep. var. in 2016-2017)		✓		✓		✓		✓		✓
N	650		620		614		620		608	
Panel B: Years 2018-2023										
import tariff exposure	1.928	1.833	1.731	1.636	1.325	1.330	-0.507	-0.397	5.423	5.469
	(1.189)	(1.194)	(1.095)	(1.100)	(1.165)	(0.784)	(0.762)	(0.767)	(3.228)	(3.238)
retaliatory tariff exposure	-6.375	-6.545	-3.122	-3.249	-3.014	-3.533	-5.056	-5.341	-14.803	-14.746
	(4.228)	(4.165)	(4.801)	(4.814)	(2.766)	(1.968)	(2.642)	(2.616)	(7.863)	(7.889)
sector × year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
t × (Δ dep. var. in 2016-2017)		✓		✓		✓		✓		✓
Δ ln(emp) Mar-Apr 2020 × year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
N	1,942		1,852		1,842		1,852		1,792	

Notes: Regressions in columns 1-2 comprise data for 310 6-digit manufacturing industries and 15 aggregate sectors of agriculture and mining. Regressions in columns 3-10 comprise data for up to 310 6-digit manufacturing industries. The dependent variable is 100 times the difference between the log outcome in each year and the log outcome in the baseline year 2017. The means of the outcome variables in 2017 (prior to indexing and applying the log transformation) are USD 52.2 billion in columns 1-2, USD 49.5 billion in columns 3-4, USD 7.3 billion in columns 7-8, and USD 1.2 billion in columns 9-10. All regressions in Panel B control for year fixed effects and their interaction with the March-to-April 2020 change in industry employment; additionally, the first two columns of both panels interact these fixed effects with indicators for manufacturing and for agriculture and mining. Regressions are weighted by industry employment in 2012, and standard errors are clustered by industry.

Annual Survey of Manufacturers and related sources. Columns 3 and 4 in Panel A of Table A9 repeat the overall sales estimates (columns 1 and 2) for manufacturing. This yields estimates similar to columns 1 and 2 for the impact of import tariffs on sales. Instead, coefficients for retaliatory tariffs are now weakly positive but remain imprecisely measured.

Columns 5 and 6 of Panel A indicate that higher prices are an important contributor to increased sales volume, consistent with the literature linking Trump tariffs to higher US goods prices (Fajgelbaum and Khandelwal, 2022). A comparison of the coefficient estimates for prices with those for sales suggests that about half of import tariff's impact on sales is accounted for by higher prices (e.g., $0.916/1.973 = 0.46$ based on the column 4 and 6 estimates), with the remainder due to higher sales quantities.

Higher sales quantities however do not appear to have raised payroll, as shown in columns 7 and 8 of Panel A. Instead, tariff-protected industries significantly increased capital expenditure on machinery, as reported in columns 9 and 10 of Panel A.[41] These results suggests that tariff-protected industries are substituting towards capital and away from labor.

[41] Amiti et al. (2020a) show that tariff announcements lead to short-term declines in stock market valuations of exposed firms, which reduces returns to capital and may deter investments. Their analysis however does not study data on firms' expenditure for capital investment.

Panel B of Table A9 presents results for the same set of regression outcomes over the longer time horizon of 2018-2023. The results are less precisely estimated than those in Panel A but continue to show positive effects of import tariffs on sales and manufacturing prices. Higher prices now account for the majority of the increase in sales, and there continues to be little impact of import tariff protection on payroll. Higher retaliatory tariffs are associated with lower sales, prices, payroll and machinery expenditure, although most of these effects are imprecisely estimated.

A2.3 Impacts on Industry-Level Employment

We next study the impact of tariff exposure on employment in national tradable industries. Acemoglu et al. (2016) argue that CZ- and industry-level analyses are complements to each other which will capture slightly different employment effects of industry-level shocks. Effects at the CZ level combine the effect of tariffs on employment in directly exposed industries with local spillovers that can operate through local supply chain linkages, local consumption effects, and local labor reallocation effects. Such local spillovers are apparent in the results of Table 2, where tariff exposure affects employment not only in goods-producing sectors but also in service industries. The CZ analysis will however not generally capture national supply chain spillovers that extend beyond local labor market boundaries.[42] To investigate both the direct effects of tariffs on industry employment and the indirect effects via national supply chain linkages between supplier and customer industries, we fit the regression

$$\begin{aligned}\ln E_{it} - \ln E_{iJan2018} = {} & \beta_1 IMP_{it} + \beta_2 RET_{it} + \mathbf{\hat{x}_{it}^{IO}}' \boldsymbol{\beta_3} \\ & + \gamma_t + \lambda_{s(i),t} + \phi(\bar{\Delta} \ln E_{i,2017} \times t) + \chi_t(\Delta \ln E_{i,Mar20-Apr20}) + \varepsilon_{it},\end{aligned} \tag{A5}$$

where the dependent variable is log employment in tradable industry i in month-year t, measured as a deviation from the base period January 2018, IMP_{it} and RET_{it} are industry-level import and retaliatory tariffs, and $\mathbf{\hat{x}_{it}^{IO}}$ is a vector of four input-output terms corresponding to an industry's indirect exposures to import and retaliatory tariffs faced by its suppliers and customers (eqns. (A1) and (A2)). Control variables absorb year-month effects γ_t, year-month-sector effects $\lambda_{s(i),t}$, a linear one-year pre-trend of the outcome variable $(\bar{\Delta} \ln E_{i,2017} \times t)$, and the employment decline at the start of the pandemic interacted with time fixed effects $\chi_t(\Delta \ln E_{i,Mar20-Apr20})$.

Column 1 of Table A10 reports a bare bones version of equation (A5) with a specification that includes year-by-month main effects but excludes sector-by-month interactions, input-output linkages, and pre-trends. This model detects smaller negative effects of import tariffs and larger negative effects of retaliatory tariffs on employment in exposed industries over either the 2018-2019 period in Panel A or the 2018-2023 period in Panel B. When sector-by-month interactions are additionally included in column 2, the estimated negative employment effects increase for import

[42] The BEA input-output tables capture national linkages between industries. Since supply chains can be strongly localized (such that domestic trade takes place much more frequently between firms that are close than between firms of the same industries that are more distant), these tables may be ill-suited to explicitly measure supply chain spillovers in local markets, but they are appropriate for an analysis at the national industry level.

Table A10: Impact of Tariff Exposure on Tradable Industry Employment

	No pretrend control				Control for 2017 pretrend			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Years 2018-2019								
import tariff exposure	-0.431	-0.678	-0.760	-0.747	-0.195	-0.430	-0.463	-0.476
	(0.352)	(0.358)	(0.343)	(0.340)	(0.306)	(0.315)	(0.306)	(0.308)
retaliatory tariff exposure	-0.923	-0.566	-0.543	-0.579	-1.081	-0.744	-0.733	-0.740
	(0.403)	(0.398)	(0.401)	(0.394)	(0.355)	(0.329)	(0.331)	(0.325)
supplier import exposure			1.776	1.817			0.611	0.665
			(0.736)	(0.727)			(0.585)	(0.555)
customer import exposure				-0.472				-0.247
				(0.947)				(0.805)
supplier retaliatory exposure				-0.159				-0.333
				(0.773)				(0.677)
customer retaliatory exposure				1.039				0.473
				(1.022)				(0.967)
t × (monthly Δ ln(emp) in 2017)					0.340	0.335	0.328	0.328
					(0.068)	(0.063)	(0.065)	(0.066)
year-month FE	✓	(✓)	(✓)	(✓)	✓	(✓)	(✓)	(✓)
sector × year-month FE		✓	✓	✓		✓	✓	✓
Panel B: Years 2018-2023								
import tariff exposure	-0.946	-1.345	-1.343	-0.909	-0.429	-0.815	-0.754	-0.392
	(0.471)	(0.453)	(0.450)	(0.472)	(0.422)	(0.407)	(0.411)	(0.429)
retaliatory tariff exposure	-2.199	-1.528	-1.532	-1.347	-2.376	-1.743	-1.821	-1.615
	(0.666)	(0.679)	(0.684)	(0.712)	(0.606)	(0.588)	(0.589)	(0.593)
supplier import exposure			-0.195	1.562			-3.329	-1.643
			(1.765)	(1.773)			(1.735)	(1.743)
customer import exposure				-5.801				-5.112
				(1.651)				(1.542)
supplier retaliatory exposure				1.741				1.450
				(1.265)				(1.034)
customer retaliatory exposure				4.633				3.493
				(2.624)				(2.338)
t × (monthly Δ ln(emp) in 2017)					0.259	0.254	0.266	0.259
					(0.056)	(0.052)	(0.052)	(0.051)
year-month FE	✓	(✓)	(✓)	(✓)	✓	(✓)	(✓)	(✓)
sector × year-month FE		✓	✓	✓		✓	✓	✓
Δ ln(emp) Mar-Apr 2020 × year-month FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: N=8,579 (373 tradable industries x 23 months from Feb. 2018 to Dec. 2019) in Panel A, and N=26,483 (373 tradable industries x 71 months from Feb. 2018 to Dec. 2023) in Panel B. The dependent variable for the regression models is seasonally-adjusted log employment, which is indexed to 0 in Jan. 2018 in each industry. The mean (standard deviation) of log employment prior to indexing is 1,110.8 (116.9) log points in Jan. 2018. Regressions in columns 5 to 8 control for the monthly change in log employment from Jan. 2017 to Jan. 2018, interacted with a linear time trend (the count of months since Jan. 2018). The regressions in columns 2 to 4 and 6 to 8 interact time fixed effects with indicators for three economic sectors (agriculture and mining, manufacturing, non-goods sector). All regressions in Panel B control for the decline in industry employment from March to April 2020. Regressions are weighted by industry employment in 2012, and standard errors are clustered by industry.

tariffs and decrease for retaliatory tariffs. The estimates for import and retaliatory tariffs change relatively little in the third and forth column, which respectively include supplier import exposure, or all four input-output terms. The latter models indicate, somewhat puzzlingly, that tariff protections applied to an industry's suppliers predict an increase in that industry's employment over the 2018-19 period, while most of the remaining input-output terms are imprecisely estimated.[43]

Cognizant of the concern that inference on high-frequency monthly data may be particularly vulnerable to confounding trends, the final four columns of Table A10 control directly for pre-trends by including a linear time trend in industry-level employment growth between January 2017 and January 2018, which is interacted with the time elapsed since January 2018. Conditional on this trend variable, a clearer picture emerges of the industry-level impacts of the trade war. Retaliatory tariffs significantly depress employment in targeted industries, while import tariffs have smaller and insignificant negative effects. The employment effect of industries' indirect tariff exposure via suppliers and customers are mostly measured with little precision, including now negative effects of supplier import exposure over the longer outcome horizon in Panel B.

A3 Data Appendix: QCEW

The Quarterly Census of Employment and Wages (QCEW) provides information on monthly employment and quarterly establishment counts at different levels of geographic and industry aggregation. It relies primarily on administrative data collected through state unemployment insurance programs, which the Bureau of Labor Statistics supplements with information from additional sources.

The Bureau of Labor Statistics estimates that QCEW covers more than 95 percent of total U.S. employment (BLS, 2023). Table Table A11 shows that this coverage is broader than that of the Census Bureau's County Business Patterns (CBP) data, which following Autor et al. (2013) has been widely used in studies that allocate industry-level shocks to local labor markets. The coverage difference results primarily because the QCEW includes the agriculture and government sectors whereas the CBP does not. Excluded from the coverage of the QCEW are unincorporated self-employed workers, unpaid family members, and railroad workers who are covered by a separate unemployment insurance program.

QCEW reports data on private sector employment at three levels of spatial aggregation (nation, state and county), and six levels of industry aggregation (all industries and 2-/3-/4-/5-/6-digit NAICS industries), as well as for all intersections of geographic and industry levels. At the most granular level, it contains data for over 3,000,000 county $\times$ 6-digit NAICS industry cells. From 2004 onwards, QCEW reports quarterly establishment counts for each of these county-industry cells. However, employment counts are suppressed in detailed cells where the employment of individual firms could otherwise be easily inferred. While roughly 75% of total private sector employment is

[43] Supplier import tariff exposure might be expected to reduce competitive pressure in the supplying sector, thus raising input costs and depressing employment in customer industries.

reported at the most detailed level of county × 6-digit industry (as is the case for CBP data, see Table A11), we impute employment for the remaining county-industry cells.[44]

Table A11: Data Sources for Regional US Industry Employment: QCEW vs. CBP

	Quarterly Census of Emp. and Wages (QCEW)	**County Business Patterns (CBP)**
	Frequency and Coverage of Employment Data	
Frequency	monthly (published quarterly)	annual
Sectors Covered	private non-farm private farm public sector	private non-farm
Total US Employment (2010)	126'228'228	111'970'095
	Disclosed County × 6-digit Industry Cells	
Employment Share (2010)	74.45% of private sector employment	72.45% of private sector employment
Employment Information	actual counts	actual counts plus noise infusion (since 2007)
Establishment Information	actual counts	actual counts by establishment size class
	Undisclosed County × 6-digit Industry Cells	
Employment Share (2010)	25.55% of private sector employment	27.55% of private sector employment
Employment Information	flag for employment >0	flags for employment intervals
Establishment Information	actual counts	actual counts by establishment size class
	Undisclosed State × 4-digit Industry Cells	
Employment Share (2010)	0.55% of private sector employment	2.51% of private sector employment

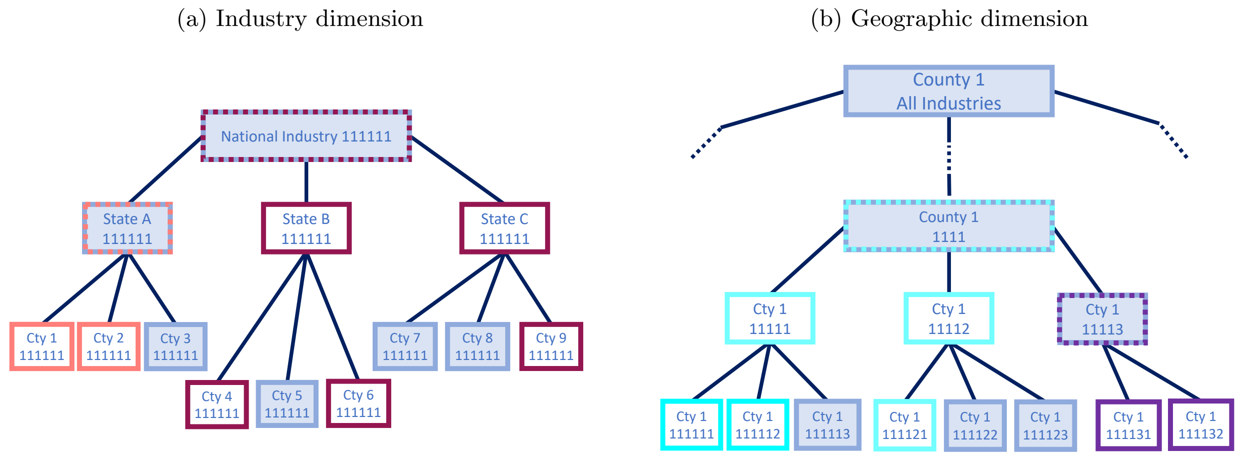
Notes: Employment statistics are based on monthly QCEW data for May 2010 and annual CBP data for 2010.

Key to our imputation algorithm is the observation that whenever employment counts are suppressed at the level of a detailed county × 6-digit industry cell, then employment will still be known at a higher level of geographic aggregation, and at a higher level of industry aggregation.[45] For instance, we may know the employment for a 6-digit industry at the state rather than county level, and we may know the employment in a county at the level of a 4-digit industry rather than 6-digit industry. Indeed, more than 99% of total private sector employment is disclosed at least at the level of state × 4-digit industry cell (Table A11). Our fixed-point algorithm distributes the known employment counts from more aggregate geography and industry levels to detailed county

[44] The QCEW also reports data on public sector employment by geographic unit and industry. Public sector employment is clustered heavily in a few non-tradable sectors such as public administration and education, and information at the most granular level of county × industry is often suppressed due to low numbers of distinct public employers. We treat public sector employment as a separate industry with no direct tariff exposure.

[45] Extant imputations of CBP data for detailed geography and industry cells by Autor et al. (2013) and Eckert et al. (2020) leverage similar aggregation properties as well as ranges of possible employment values that are indicated for each suppressed cell. We compare the results of our QCEW imputation to these prior CBP imputations below.

Figure A3: Illustration of QCEW Imputation Algorithm



Notes: This figure illustrates the structure of the QCEW data. The left panel shows how next upper level disclosed cells are identified in the industry dimension, the right panel provides an example for the geographic dimension. Observations with blue fill color are disclosed. Non-disclosed cells that belong to the same suppressed cell cluster have the same outline color. A dashed outline indicates the next higher disclosed cell for the cell cluster.

$\times$ industry cells while leveraging the fact that we know the exact establishment count for each detailed cell. The algorithm proceeds as follows:

1. **Identification of upper-level disclosed cells.** For each non-disclosed observation at the county $\times$ 6-digit industry level, we identify the next more aggregate geographic level at which information is disclosed (i.e., 6-digit industry employment at the state or national level) and the next more aggregate industry level at which information is disclosed (i.e., county employment in a 5-digit, 4-digit, 3-digit, or 2-digit industry, or for the entire private sector).[46] Each suppressed cell is thus part of two clusters of suppressed cells: One that combines non-disclosed cells within an industry to the next higher geographic level (industry dimension); and one that combines cells within a geographic area to the next higher industry level (geographic dimension). Figure A3 provides a graphical illustration. The suppressed cell *county 1 - industry 111111* belongs to the cluster of cells that shares *state A - industry 111111* as the next higher disclosed cell in the industry dimension and the cell cluster that shares *county 1 - sector 1111* as the next higher disclosed cell in the geographic dimension.

2. **Calculation of distributable suppressed employment.** For each cell cluster, the sum of distributable suppressed industry (geographic) employment equals the total employment of the cluster minus the employment in disclosed cells that are part of the cluster. Using again the example in the left panel of Figure A3, one subtracts the disclosed employment for

[46] In very rare cases, the QCEW data suppresses either total national employment in a few (often a pair of) small 6-digit industries, or total private sector employment in a few small counties. We impute total industry or total county employment in proportion to the number of establishments in the cells with non-disclosed employment while ascertaining that the resulting employment numbers add up to the disclosed employment counts at higher levels of industry or geographic aggregation.

county 3 - industry 111111 from the disclosed employment of *state A - industry 111111* to obtain the employment that has to be distributed from *state A - industry 111111* to *county 1 - industry 111111* and *county 2 - industry 111111*. At the same time, the right panel of Figure A3 shows that the employment of *county 1 - industry 1111* minus the employment of *county 1 - industry 111113*, *county 1 - industry 111122*, *county 1 - industry 111123* and *county 1 - industry 11113* yields an employment total that has to be distributed between cells *county 1 - industry 111111*, *county 1 - industry 111112* and *county 1 - industry 111121*.

3. **Preliminary Initialization.** We initialize the fixed-point algorithm by apportioning distributable industry employment to suppressed cells in proportion to each cell's fraction of the total establishment count in the cell cluster. This initialization is based on the assumption that establishments of the same detailed industry and aggregate geographic unit have the same average employment size per establishment across the different counties of the cluster. The resulting initial imputed employment counts for detailed county $\times$ 6-digit industry cells will by construction sum up correctly to the disclosed employment totals in the industry dimension, but they will not typically add up correctly in the geographic dimension.

4. **Iteration.** We continue with an iterative updating of the imputed employment counts that alternates between establishing consistency in the geographic dimension (such that imputed cell employment adds up exactly to disclosed employment for more aggregate industries in a county) and the industry dimension (such that imputed cell employment adds up exactly to disclosed employment for a 6-digit industry at a more aggregate geography level). In each case, the last imputed values are adjusted through multiplication with the ratio of suppressed distributable employment over the sum of imputed employment in the cell cluster.

5. **Convergence.** After each iteration, consisting of one correction in the geography dimension and one correction in the industry dimension, the average deviation (in absolute value) between imputed area cluster employment and suppressed area employment decreases. The imputation algorithm stops when the average deviation across all clusters reaches a threshold value smaller than 0.01%. This strategy implies that the imputed employment values for detailed cells sum precisely to disclosed 6-digit industry employment for more aggregate geographic units, while they sum nearly exactly to disclosed county employment at more aggregate industry levels.

6. **Revised Initialization.** The preliminary initialization of the algorithm is based on the assumption that establishments of the same detailed industry and aggregate geographic unit have the same average employment size per establishment across different counties. However, it is possible that an industry consistently has larger establishment sizes in one county than in another. To account for this possibility, we re-run the algorithm with a revised final initialization that takes into account the average establishment size for a cell that we initially computed for the previous month in the data. Let $emp^0_{j,t}$ be the employment for suppressed

cell j in month t that we computed with the first preliminary initialization of the algorithm, while $est_{j,t}$ is the known number of establishments in the cell and $emp^{dist}_{J,t}$ is the distributable employment of the cell cluster J to which cell j belongs. We newly implement the revised initialization of the fixed-point algorithm as

$$emp^{1,init}_{j,t} = emp^{dist}_{J,t} * \frac{x_{j,t}}{\sum_{i \in J} x_{i,t}}$$

$$\text{where } x_{j,t} = est_{j,t} * max(\frac{emp^{0}_{j,t-1}}{est_{j,t-1}}, 1)$$

This initialization allocates to cell j a greater fraction of the distributable employment of cluster J not only when j accounts for a large fraction of all establishments in the cluster, but also when the initial iteration of the algorithm found that establishments of cell j had an larger employment size in the prior month.[47]

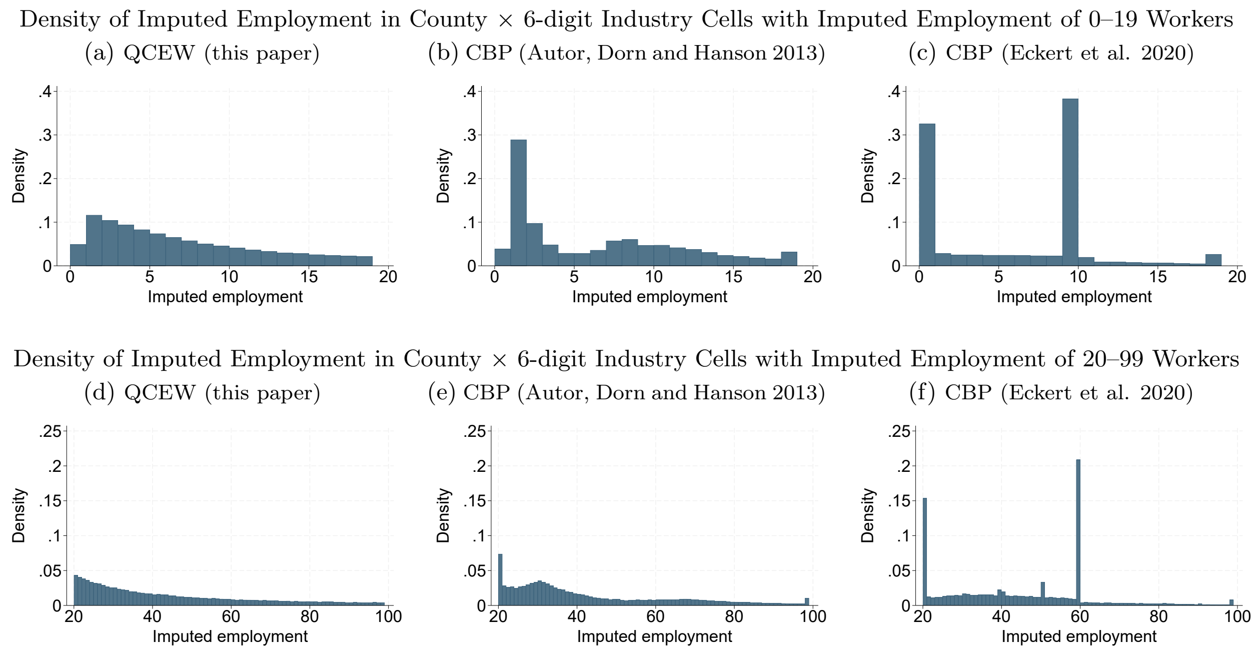
Our final dataset consists of monthly QCEW employment data from January 2004 onwards that is obtained using the fixed-point algorithm with the revised initialization as indicated above.

To assess the plausibility of our imputation results, we assess two properties of the imputed data. First, in Figure A4, we show the distribution of imputed employment counts for county × 6-digit industry cells when those counts are between 0 and 19 employees (top panel) or between 20 and 99 employees (bottom panel). Since firm size distributions are right-skewed, we would expect that imputed cell employment similarly displays a monotone right-skewed distribution. The left-hand panels of Figure A4 indicate that this distributional property is indeed present in our imputation of the QCEW data. By contrast, earlier imputations of CBP data by Autor et al. (2013) and Eckert et al. (2020) display non-monotone distributions with highly salient mass points.

Second, we investigate in Table A12 whether the final employment counts for county × 6-digit industry cells are consistent with the employment information that is disclosed in the source data. The upper panel of the table assesses the consistency of cell employment with disclosed employment numbers for more aggregate geography or industry levels. For instance, we sum up the employment of all county × 6-digit industry cells that belong to the same county and then compute the absolute deviation between this sum and the county employment totals that are disclosed in the source data. The third row in Table A12 indicates the total deviations between sums of cell employment and disclosed county employment across all counties, expressed as a fraction of total national employment. The first two rows of the table similarly investigate whether cell employment correctly adds to national employment or state-level employment. In all cases, we are able to ascertain that the county × 6-digit industry employment counts in our final QCEW data sum exactly to known county, state or national employment. CBP data imputed by Autor et al. (2013) also displays very high consistency with known employment counts at aggregate geographic

[47] If $est_{j,t-1} = 0$, then $x_{j,t}$ is set to $est_{j,t}$.

Figure A4: Distribution of Cell Sizes for Imputed County x 6-digit Industry Employment: QCEW vs. CBP



Notes: The figure indicates the frequency of imputed employment levels in county x 6-digit industry cells using our imputation for QCEW employment in May 2010, and using the imputation methods of Autor et al. (2013) and Eckert et al. (2020) for CBP employment in 2010.

levels, while cell-level employment from the CBP imputation by Eckert et al. (2020) modestly deviates from county, state and national employment levels. The fourth through sixth data row of Table A12 indicate that county × 6-digit industry employment in the QCEW also perfectly sums to known aggregate industry employment, while the CBP imputations by Autor et al. (2013) and Eckert et al. (2020) display small inconsistencies.[48]

We conclude that our new county × 6-digit industry employment imputation in the QCEW both generates a more plausible employment distribution across cells and displays a greater consistency with disclosed data than extant imputations of CBP data. A key advantage of the QCEW is that its disclosed employment values are exact counts, whereas the CBP discloses employment with noise infusion (Table A11). The QCEW data is thus more suitable for an imputation procedure that relies on the exact summation of employment in detailed cells to known employment counts in aggregate cells.

[48] One difference between the CBP and QCEW data is that the former indicates two ranges of possible employment values for each cell whose employment count is not disclosed. A set of data flags indicates that the suppressed employment of the cell falls into a specific employment range such as 20 to 49 employees. Moreover, a count of establishments by size class implies a second employment range. For instance, CBP data may indicate that a cell has two establishments with an employment of 1 to 4 workers, thus implying that employment in the cell lies in a range of 2 to 8 workers. The lower panel of Table A12 indicates that imputed employment counts for county × 6-digit industry cells in the CBP are consistent with the cell-specific employment ranges. However, about 5% of the imputed values in Autor et al. (2013) and more than half of the imputed values in Eckert et al. (2020) are inconsistent with the employment ranges implied by the establishment-by-size counts.

Table A12: Inconsistencies in County x 6-digit Industry Data: QCEW vs. CBP

	Quarterly Census of Emp. and Wages (QCEW)	County Business Patterns (CBP)	
Undisclosed Cells Imputed by	**This Paper**	**Autor, Dorn and Hanson (2013)**	**Eckert et al. (2020)**
	Inconsistencies between Post-Imputation Employment in County x 6-digit Industry Cells vs. Employment in Disclosed Aggregate Cells (in % of Total National Employment)		
County x 6-digit Industry Emp. vs.			
Total National Employment	0.00%	0.00%	0.12%
Total State Employment	0.00%	0.01%	0.12%
Total County Employment	0.00%	0.00%	0.12%
National 2-digit Employment	0.00%	0.01%	0.12%
National 4-digit Employment	0.00%	0.12%	0.12%
National 6-digit Employment	0.00%	0.24%	0.12%
	Inconsistencies between Post-Imputation Employment in County x 6-digit Industry Cells vs. Disclosed Cell-Specific Employment Ranges (in % of Cells with Non-Disclosed Employment)		
County x 6-digit Industry Emp. vs.			
Cell-Specific Employment Range	n/a	0.00%	0.00%
Cell-Specific Establ. Size Range	n/a	5.01%	55.61%

Notes: All statistics are reported for our imputation of QCEW data from May 2010, and imputations of annual CBP data from 2010 using either the imputation algorithm of Autor et al. (2013) or Eckert et al. (2020). The top panel of the table sums the employment counts of disclosed and imputed county x 6-digit industry cells to a more aggregate level of geography and/or industry. It computes the absolute deviations in employment counts between the summed cells and the actual employment at the aggregate geography and/or industry level that is disclosed in the source data. The reported statistic expresses the sum of these deviations as a fraction of total national employment that is disclosed at the corresponding level of aggregate geography and/or industry. The lower panel reports the fraction of county x 6-digit industry cells with non-disclosed employment count in the CBP data whose imputed employment is inconsistent with either the employment range or the number of establishments by establishment size ranges that CBP discloses for these cells.

A4 Other Data Construction Details

A4.1 Tariffs, products and industries

We extend the Fajgelbaum et al. (2020) tariff data through December 2019 to account for: (1) subsequent rounds of tariff increases between the US and China (USTR, 2018, 2019; Bown and Kolb, 2022); (2) increases in US tariffs on Turkey in August 2018; (3) increases in US tariffs on India and Turkey after their loss of status as beneficiary developing countries; (4) introduction of retaliatory tariffs by India and Turkey in May 2019; (5) exemptions of EU and NAFTA countries from US steel and aluminum tariffs in various periods; (6) Canada and Mexico relaxing their retaliatory tariffs in May 2019; (7) the removal of US steel and aluminum tariffs on Canada and Mexico in May 2019; and (8) reductions in US tariffs on solar panels and washing machines in February 2019.

To aggregate the product-level tariff panel to NAICS 6-digit codes, we refine the data by: (1) translating product codes to time-consistent HS10 import and HS6 export codes by combining information on changing goods classifications from Pierce and Schott (2012) (for import codes) and the Census Bureau Foreign Trade Department's list of obsolete Schedule B codes (for export codes) from U.S. Census Bureau (2020); (2) using crosswalks from the Census Bureau to aggregate 6-digit industry codes from the 2002 to 2017 NAICS classification to time-consistent NAICS industry codes, with the 2022 NAICS codes added via a weighted crosswalk; (3) augmenting U.S. Census Bureau concordances from HS product codes to industries to obtain a mapping from time-consistent HS product codes to time-consistent NAICS codes; and (4) combining some industries with industry neighbors to ensure that each consolidated tradable 6-digit industry is matched with at least one HS product code.

A4.2 MFP payment allocation

We allocate annual MFP payments by county to monthly periods between September 2018 and February 2020 based on government reports of the disbursement timing of the MFP-2018 and MFP-2019 schemes.

Funds from the \$8.6bn MFP-2018 scheme were disbursed on a rolling basis starting in September 2018 and were largely complete by May 2019 (Congressional Research Service, 2019a). Total payments made in 2018 and 2019 amounted to \$5.3bn and \$14.2bn, respectively, which implies that 23% of the payments in 2019 came from the MFP-2018 scheme (= (\$8.6bn − \$5.3bn)/\$14.2bn). We distribute each CZ's 2018 payments equally across September through December, and assign 4.6% of total 2019 payments (one-fifth of 23%) to each month from January through May.

MFP-2019 funds were paid out in three tranches starting in August 2019, November 2019, and February 2020, with the first tranche being approximately twice as large as the other two (Congressional Research Service, 2019b). For each CZ, we allocate 51.3% of 2019 payments (two-thirds of the 77% of 2019 payments due to MFP-2019) equally across August through October, and 25.6% (one-third of 77%) to November, consistent with government reports indicating MFP-2019 payments of \$5.9bn by mid-October (US Department of Agriculture, 2019) and \$10.2bn by late November (Congressional Research Service, 2019b). We assume the third MFP-2019 tranche was paid out in full in February 2020 (United States Government Accountability Office, 2021).

A4.3 Internet searches for tariffs

Google Trends provides time-series data on the frequency of web searches for specific keywords on Google's search platform. These data are consistently available at a monthly frequency for almost all U.S. states, but not for more detailed geographic units such as counties or commuting zones. For each state-level time series, Google Trends indexes search intensity to 100 in the month in which the search term reached its highest share of all Google searches in that state. Google Trends also permits cross-sectional comparisons of relative search intensity across states, indexing the measure to 100 for the state in which the keyword accounted for the highest fraction of total searches.

To construct a comparable measure of search intensity across states and over time, we first retrieved monthly search data for the keyword "tariffs" in California for the period 2016–2021. We then retrieved, for each month, the relative search intensity of "tariffs" across U.S. states and used these cross-sectional data to compute each state's deviation from California's time series. For some small states, monthly data were occasionally suppressed; in these cases, we computed the deviation from California at the quarterly level for the affected quarters. We combined the state-level time series to a national average that weights each state by its 2020 population, and re-standardized the state-level data series (initially indexed to 100 in the month greatest search intensity in California) to a new index base of 1 for the average national search intensity prior to the trade war in January 2016 to January 2018.