Help for the Heartland? The Employment and Electoral Effects of the Trump Tariffs in the United States*

David Autor
MIT and NBER

Anne Beck
World Bank

David Dorn
University of Zurich and CEPR

Gordon Hanson
Harvard University and NBER

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Abstract

We study the economic and political consequences of the 2018-2019 trade war between the United States, China, and other US trade partners at the detailed geographic level, exploiting new measures of local exposure to US import tariffs, foreign retaliatory tariffs, and US compensation programs. The trade war has not to date provided economic help to the US heartland: import tariffs on foreign goods neither raised nor lowered US employment; retaliatory tariffs had negative employment impacts notably in agriculture; and these harms were only partly mitigated by compensatory US agricultural subsidies. We also fail to detect positive effects of the trade war on local earnings. Nevertheless, consistent with expressive views of politics, the tariff war appears to have been a political success for the governing Republican party. Residents of regions more exposed to import tariffs voice more support for tariffs, became less likely to identify as Democrats, and more likely to vote to reelect Donald Trump in 2020. Foreign retaliatory tariffs only modestly weakened that support. Voters from regions whose industries were heavily exposed to Chinese import competition in prior decades rewarded President Trump the most for import tariff protection.

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"And the trade is so easy for me. It's so obvious what's happening when our companies are flocking out. We're going to fix our trade, we're going to bring jobs back to our country, including this area, right here, which has been devastated."
—Donald J. Trump, Florida campaign stop 10/25/2016, quoted in the Washington Post

1 Introduction

Two decades after establishing Permanent Normal Trade Relations with China and facilitating the country's accession to the World Trade Organization, the US in 2018 imposed substantial tariffs on Chinese imports and selective goods from other countries. This protectionist turn in US trade policy set in motion a trade war comprising successive rounds of US import tariff hikes, retaliatory foreign tariffs, and US subsidies to affected sectors. A stated goal of the Trump administration's trade policy was "to bring back jobs to America." A secondary goal of the policy was presumably to build political support in places hurt by trade with China. While these goals are nominally aligned, they are not mutually dependent. If the trade war conveyed political solidarity with voters in import-competing sectors and locations, its tangible consequences for jobs may be secondary to its political consequences. This paper jointly considers the labor market and political consequences of the trade war, focusing on impacts at the level of detailed geography, where employment and voting intersect.

To understand which locations were potentially affected by the trade war and what those effects were, we use monthly data on US employment by industry and commuting zone from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW). These data, also explored at a more aggregate level by Waugh (2019), cover more than 95% of US employment and are well-suited for high frequency and high resolution spatial analysis of policies. We combine the employment data with detailed information on US import tariffs and foreign retaliatory tariffs, drawing both on widely used 2018 tariff data and newly collected data on tariffs imposed in 2019 that substantially escalated the trade war. China featured centrally in this conflict: most US tariffs targeted Chinese goods, and China was responsible for most retaliatory tariffs.[1] Simultaneously, we account for government compensatory subsidies to agricultural sectors targeted by Chinese and other foreign retaliatory tariffs. We relate the differential exposure of commuting zones to tariffs and subsidies to labor market and political outcomes, where the former include monthly employment rates and quarterly wage bills based on QCEW data, and the later combine vote counts for presidential and congressional elections from Leip's Electoral Atlas with survey evidence

[1]In our data, China-facing import tariffs accounted for 94.8% of employment-weighted industry tariff exposure as of December 2019, and 93.8% on average over the entire sample period of February 2018 to December 2019. Similarly, Chinese retaliatory tariffs accounted for 82.5% of industry retaliatory tariff exposure as of December 2019, and 68.0% over the full period.

on support for tariffs and party identification from the Cooperative Congressional Election Study (CCES).

Our main results are as follows: import tariffs on Chinese and other foreign goods had neither a sizable nor significant effect on US employment rates or earnings in regions with newly-protected sectors. Foreign retaliatory tariffs by contrast had modest but significantly estimated negative employment and earnings impacts, and these harms were only partly mitigated by compensatory subsidies. To provide context for the limited impact of US import tariffs on employment, we confirm in a complementary industry-level analysis that these tariffs reduced US imports and increased US sales in protected industries. However, sales growth resulted primarily from higher sales prices and greater capital inputs rather than from an expansion of labor inputs.

Despite the trade war's failure to generate substantial labor market gains, it appears to have benefited the Republican party, consistent with expressive views of politics. Residents of regions more exposed to import tariffs voiced greater support for US tariffs, and became less likely to identify as Democrats, more likely to vote to reelect President Donald Trump in 2020, and weakly more likely to elect Republicans to Congress. Foreign retaliatory tariffs only modestly reduced that electoral support. Republican gains were concentrated in regions that had suffered from a rapid growth of Chinese import competition in the 1990s and 2000s, and whose residents appear to value US tariffs against China even absent tangible local economic gains.

Our work is related to two branches of the now expansive literature on the US-China trade war (Fajgelbaum and Khandelwal, 2022). A first branch assesses how the trade war has affected US trade volumes and goods prices, consistently finding that the effect on US welfare has been in net negative (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). Our empirical goal in this paper is narrower—measuring labor market rather than welfare effects—but, we believe, highly politically relevant: we ask whether the tariff policy achieved its explicit goal of bringing back jobs to America, which we interpret to mean jobs in the *places* where trade-war impacted industries reside.

The second tributary of work to which we contribute studies the political consequences of the trade war. Fajgelbaum et al. (2020) document that both import-tariff-exposed and retaliatory-tariff-exposed industries tend to be clustered in Republican-leaning regions, and Fetzer and Schwarz (2021) and Kim and Margalit (2021) show that Chinese counter-tariffs appear to have been carefully targeted to inflict damage on the Republican party.[2] Kim and Margalit (2021) and Blanchard et al. (2024) find that the trade war caused Republican losses in the 2018 midterms, while Lake and Nie (2022) conversely find that it generated Republican gains in the 2020 presidential election.[3]

Within the first branch of literature—studying economic impacts—our work is related to Flaaen

[2]Related work by Brugter et al. (2023) shows that both Republican and Democratic voters regarded retaliatory tariffs as a form of electoral interference.

[3]Chyzh and Urbatsch (2020) and Choi and Lim (2023) provide complementary evidence on the electoral consequences of the trade war's impact on agriculture.

and Pierce (2021), Javorcik et al. (2022) and Waugh (2019). Flaaen and Pierce (2021) leveraged data from the Current Employment Statistics, a survey designed to produce current employment estimates, to provide the first results on tariffs' impact on employment in aggregate US national manufacturing industries. They find positive employment effects of direct import protection and larger negative effects from rising input costs and retaliatory tariffs. Javorcik et al. (2022) address the related question of whether tariffs affected the number of online job ads that were posted in local labor markets in 2018, and find an attenuating effect of both rising input costs and retaliatory tariffs on job postings and no gains from import tariffs. Waugh (2019) focuses on retaliatory tariffs and their consequences for durable goods consumption, and shows that US counties more exposed to retaliatory tariffs had larger declines in auto sales and both tradeable and retail employment.[4] We complement and extend this research in two important dimensions. First, we provide the first analysis of the impact of tariffs on employment in all sectors of the US economy. To do so, we draw on detailed data that covers not only manufacturing (the primary target sector of US import tariffs) but also agriculture and mining (both important target sectors of foreign retaliatory tariffs), as well as services and government. Second, we complement this employment analysis by providing first results for the impacts of the tariff war on wage bills in local labor markets.[5]

Within the second stream of literature—political consequences—our analysis is most related to Kim and Margalit (2021), Blanchard et al. (2024) and Lake and Nie (2022), who study the impact of the 2018 US import and foreign retaliatory tariffs on Republican vote shares the 2018 congressional or 2020 presidential elections. While each of these analyses tends to find a positive effect of import tariffs and a negative one for retaliatory tariffs on GOP votes, the former effect appears to dominate in 2018 and the latter in 2020. We advance this work in two dimensions. First, we provide an integrated analysis of both economic and electoral outcomes in local labor markets. This allows us to assess whether changes in voting patterns are a reaction to tariff-induced local economic change, as some of the prior analyses had speculated, or whether voters react to local tariff exposure despite limited economic effects. Second, we explore a more comprehensive set of outcomes by studying the congressional elections of 2018 and 2020 and the presidential election of 2020, and combining these analyses with complementary survey evidence that gauges support for parties and tariff policies in the locations most exposed by these policies.

Our analysis also provides various advances in data collection and measurement. The extant early work on the employment or voting effects of the tariff war uses data on tariffs imposed until the end of 2018 or early 2019, and thus covers only the first half of the period of tariff escalation. This is also the reason why many studies analyze impacts of the trade war only through 2018 (e.g., Javorcik et al. (2022) and Blanchard et al. (2024)). We collect new data on the tariffs the US, China

[4] Flaaen and Pierce (2021) and Benguria and Saffie (2020) provide complementary evidence on a positive relationship between retaliatory tariffs and estimated unemployment rates.

[5] The literature review of Fajgelbaum and Khandelwal (2022) notes that "lack of data availability has so far prevented an analysis of actual wage changes across regions to the trade war" (p. 20).

and other countries imposed through the end of 2019 when the trade war reached a stalemate. The full coverage of the 2018-2019 tariffs makes our analysis suitable for studying the labor market and electoral impacts of the trade war over longer time horizons than previously available.

The next section summarizes the main events of the trade war that commenced in 2018. Section 3 describes data sources and measurement. Section 4 analyzes the impact of tariffs on industry-level trade, sales and production inputs, before section 5 analyzes the effects of the tariff war on employment at the commuting zone level. Section 6 explores a multifaceted set of political outcomes: awareness of tariff policies, support for tariff policies, identification with political parties, and electoral outcomes in presidential and congressional races. Section 7 concludes.

2 The Trump trade war

We review here the essential features of the trade war as relevant for our analysis, and refer the reader to detailed discussions in Bown and Kolb (2022) and Fajgelbaum and Khandelwal (2022) for more specifics.

World trade in goods expanded rapidly during the 1990s and 2000s, which contributed to a large decline in manufacturing employment in the United States (Autor et al., 2013; Pierce and Schott, 2016) and Europe (Dorn and Levell, 2024a,b).[6] Although majority public opinion did not turn against globalization in most countries (Davenport et al., 2022), skepticism against trade became increasingly politicized (Walter, 2021; Colantone et al., 2022). During the campaign for the 2016 US presidential election, both the eventual Republican nominee Donald Trump and the runner-up for the Democratic nomination Bernie Sanders voiced opposition to trade integration and demanded greater protections for US manufacturing workers (Davenport, 2016).

The initial tariff volleys were fired in early 2018: in February, the US introduced new safeguard tariffs on washing machines and solar panels; in March, the US imposed tariffs on steel and aluminum imports from most countries, invoking threats to national security; and in April and June, China and the European Union, respectively, responded with retaliatory actions against US exports, especially farm products.[7] The US soon escalated the trade conflict with China into a broader anti-China trade policy. In the summer and fall of 2018, the US imposed a 10 percent tariff on a wide range of Chinese imports, to which China reacted swiftly with sizable retaliatory tariffs on US exports. In the summer and fall of 2019, the US expanded Chinese imports subject to tariffs and raised levies from 10 percent to 25 percent. China again reacted with higher tariffs on US exports. In under two years, the average US tariff on Chinese goods jumped from 3.1% to 21.0%, while the average Chinese tariff on US goods increased from 8.0% to 21.8%.

[6] The expansion of world trade slowed in the 2010s, coinciding with a weakening of China's export growth which had been a major contributor to the preceding trade boom (Autor et al., 2016, 2021).

[7] Canada, Mexico, Turkey and India later followed suit with retaliatory tariffs of their own.

US agriculture was heavily exposed to foreign retaliatory tariffs because the US is a major exporter of agricultural products. To mitigate adverse impacts of the trade war on the farm sector, the US launched the Market Facilitation Program (MFP), which in 2018 and 2019 distributed $23 billion to farmers.[8] 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 escalation of the trade war ended in January 2020, when the US and China reached an agreement that left most tariffs in place but set goals for Chinese imports of US goods. Shortly thereafter, the US labor market was thrown into turmoil by the Covid-19 pandemic, which also severely disrupted international trade. Our analysis of the employment impacts of the tariff war thus focuses on the pre-pandemic period through December 2019.

3 Data and measurement

3.1 Data sources

Employment and Earnings. Throughout the analysis, we aggregate county-level data to the level of Commuting Zones (CZs), which approximate local labor markets (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, as well as quarterly wage bills by CZ. This allows to us to study changes in overall and sector-specific CZ employment-to-population ratios, with population counts interpolated from annual data in SEER (2022), as well as changes in aggregate CZ earnings.

We additionally harness QCEW data on the detailed industry employment structure of CZs prior to the trade war to measure regional exposure to industry-level tariffs. An important advantage of QCEW over alternative industry-by-county data from the County Business Patterns (CBP), which Autor et al. (2013) and others used to allocate industry shocks to regions, is that QCEW has a more comprehensive coverage of sectors. Most notably, the QCEW includes employment in agriculture, which allows us to map the sizeable foreign retaliatory tariffs on US agricultural crops to the locations that produce these crops.[9]

The QCEW always discloses a country's total monthly employment and total quarterly wage bill that we use to construct the main labor market outcome variables. We additionally use the

[8] $3.78bil of the total payments were made in a third tranche in 2020 (EWG, 2022).

[9] 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 across all sectors of the US economy.

detailed industry structure of CZs prior to the trade war to measure regional exposure to industry-level tariffs. As with the CBP prior to 2017, the QCEW discloses about 75 percent of private sector employment at the level of the most detailed NAICS six-digit industry-county cells (Table A6), while the remainder of the employment counts is disclosed at the level of more aggregate industry and/or geography units. However, the QCEW always indicates the number of establishments for each NAICS six-digit industry-county cell, which we use as the basis for a new fixed-point imputation procedure for employment in undisclosed cells that is detailed in Appendix A2. This algorithm first allocates industry employment counts from more aggregate cells to counties in proportion to each county's share of establishments in an industry, and subsequently corrects this allocation to ascertain that imputed cell employment always sums up exactly to disclosed employment numbers at higher levels of industry and geographic aggregation.[10] With our consolidated data, we are able to measure CZ exposure to tariffs based on the local employment shares of 385 consistently defined six-digit NAICS codes for tradeable industries, whereas prior work such as Waugh (2019) and Lake and Nie (2022) relied only on 29 three-digit industries.[11]

Trade volumes and manufacturing production. Monthly trade data are from the US Census Bureau's Foreign Trade Statistics.[12] We 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 from the US Economic Census of Manufacturing and Mining (U.S. Census Bureau, 2019a,b), the US Census of Agriculture (USDA, 2014), and bilateral trade data from Schott (2008).[13] To ensure non-negative values of the fraction of US industry output sold domestically, we adjust trade flows for re-exported imports (BEA, 2021). We also study annual data on sales, producer prices, employment, cost of contract work and capital expenditure in manufacturing industries from the 2016, 2018 and 2019 Annual Survey of Manufactures (U.S. Census Bureau, 2021) and the 2017 Economic Census of Manufacturing (U.S. Census Bureau, 2019c), as well as annual sales data for agriculture and mining sectors from the BEA Industry Economic Accounts (BEA, 2024).[14]

[10] Imputations are necessary in any study that seeks to allocate industry-level shocks to local labor markets based on detailed local industry employment structures, irrespective of the use of QCEW or CBP data. The imputation techniques that Autor et al. (2013) and Eckert et al. (2020) developed for CBP data however can only be applied through 2016, after which the CBP started to suppress critical information. Appendix A2 documents that the new QCEW imputation yields data with a slightly better internal consistency and a more plausible distribution of employment counts across industry-county cells than the existing imputation techniques for CBP data.

[11] Borusyak et al. (2022, 2024) argue that research designs which study regional exposure to industry shocks can be problematic if the number of industries is small. To ascertain that industry composition is not unduly dominated by a few large industries, they recommend to additionally report the inverse of the Herfindahl-Hirschmann index of national industry shares. The resulting 'effective number' of industries is 109.4 based on the six-digit industries used in our analysis, but it would only be 16.3 when using three-digit industries.

[12] US Census Bureau Foreign Trade Statistics by trade partner country are obtained via USA Trade® Online.

[13] 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).

[14] The Annual Survey of Manufactures does not take place in the years of the Economic Census. While the Census releases data on manufacturing industry sales, prices and employment, it does not indicate expenditures for contract

Tariff exposure. We measure US import tariffs by country and HS10 product and foreign tariffs on US exports by country and HS6 product.[15] Pre-trade-war tariffs and US and retaliatory increases in tariffs through early 2019 are from Fajgelbaum et al. (2020). We extend these data to December 2019, and thus incorporate an additional time period in which US tariffs on imports from China nearly doubled.[16] We also implement a host of data refinements to aggregate the product level tariff panel to NAICS 6-digit codes.[17] With these extensions and refinements in place, we compute average industry-by-country tariffs using 2016 and 2017 pre-trade war data on imports and exports by county and product from Schott (2008) as weights.[18]

Agricultural subsidies: The US Market Facilitation Program (MFP) paid farmers \$5.3 billion in 2018 and \$14.2 billion in 2019 as compensation for adverse effects of foreign retaliatory tariffs (EWG, 2022).[19] While prior research relied on estimates for local subsidy payments (Blanchard et al., 2024; Javorcik et al., 2022), we are able to draw on actual annual MFP payments by county from the EWG Farm Subsidy Database (EWG, 2022), which we normalize to monthly payments per working age population using 2017 population data from SEER (2022). The first payments were made in September 2018. Assuming uniform timing of 2018 payments from September to December of 2018, and uniform timing of 2019 payments across the 12 months of that year, we first distribute the annual payments that a CZ receives equally across the affected months, then

work or capital. We impute 2017 values of these variables as the product of sales in 2017 with the expenditure-to-sales ratio of 2016.

[15] Both import and export product codes share the same 6-digit root based on the Harmonized System (HS6). At greater levels of detail (HS8 or HS10), 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.

[16] Our extensions of the Fajgelbaum et al. (2020) tariff data 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.

[17] The refinements include: (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) building on crosswalks from the Census Bureau to aggregate 6-digit industry codes of different vintages of the NAICS classification to time-consistent NAICS industry codes, (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 neighbours to ensure that each consolidated tradable 6-digit industry is matched with at least one HS product code.

[18] Because the effective average tariff rate that the US applied to imports from China was slightly lower than the reported statutory tariff rate due to various exceptions from tariffs (Flaaen et al., 2021), our estimates should thus 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).

[19] Farm payments for MFP I launched in 2018 were based on commodity-specific payment rates per amount of eligible crop or milk produced or live hogs owned. For MFP II launched in 2019, the list of eligible crops was expanded, while payments for non-speciality crops were set using county-specific per-acre rates ranging from \$15 to \$150 (Schnepf, 2019). A third tranche of MFP II payments was made to farmers in 2020, totalling \$3.8 billion (United States Government Accountability Office, 2021; EWG, 2022).

compute the cumulative amount of farm subsidies payments made to a CZ for each month from September 2018 forward. Due to its low temporal resolution, the farm subsidy variable is included in only a limited subset of our regression specifications.

Political outcomes. We study voting in presidential elections for CZs, and congressional elections for CZ-by-congressional district cells using data from Leip (2020). For analyzing political attitudes, we draw on data from the Cooperative Congressional Election Study (CCES), a national stratified survey administered by YouGov that includes about 50k individuals in election years and 15k individuals in intermittent years (Ansolabehere et al., 2020). We also document state-level internet search patterns for tariffs sourced from Google Trends.[20]

3.2 Measuring tariff exposure

We motivate our measure of trade exposure using the canonical Eaton and Kortum (2002) gravity model of international trade. Total shipments x_{iu} of US industry i across destination markets j can be written as:

$$x_{iu} = \sum_j x_{iju} = \sum_j \frac{A_{iu}(\tau_{iju})^{-\theta}}{\sum_k A_{ik}(\tau_{ijk})^{-\theta}} E_{ij}.$$

In this expression, x_{iju} is sales by US industry i in country j, $A_{iu} \equiv T_{iu} w_{iu}^{-\theta}$ captures the supply conditions for US industry i (reflecting fundamental productivity, T_{iu}, and unit labor costs, w_{iu}), τ_{iju} is the iceberg trade cost for US industry i when shipping to country j, E_{ij} is total spending by country j on outputs from industry i, θ is the trade cost elasticity, and the denominator is the sum of the supply conditions for all other countries k that sell industry i goods. With this equation, we can express the effect of tariff changes on demand for US outputs by log differentiating and rearranging terms:

$$\hat{x}_{iu} = \theta \gamma_{iuu} \sum_k \rho_{iuk} \hat{\tau}_{iuk} - \theta \sum_j \gamma_{iju} \hat{\tau}_{iju} \tag{1}$$

where $\hat{x} \equiv \ln(x'/x)$ and we suppress changes in labor costs. The first term in this expression, $\theta \gamma_{iuu} \sum_k \rho_{iuk} \hat{\tau}_{iuk}$, captures the effect that rising US tariffs on imports of industry i goods from countries k have on the domestic demand for US industry i's output. This industry-level exposure

[20] To measure each state's search intensity on a comparable scale, we first retrieved the monthly search data for the state of California for the period 2016 to 2021, which Google Trends indexes to 100 in the month with highest search interest. We next retrieved for each month the relative search intensity across US states and used this to compute each state's deviation from California's time series (for some of the smallest states, monthly information was suppressed in certain months, in which case we computed the deviation from California at a quarterly level). We combined the state-level time series to a national average that weights each state by its population in the 2020 Census, and finally re-standardized all data series (thus far indexed to 100 in the month with most search interest in California) to a new index base of 1 for the average national search intensity in the pre-period of January 2016 to January 2018.

to US import tariffs is higher for an industry i if (i) there is a large increase in the tariff rate that is applied to imports of industry i goods from country k, $\hat{\tau}_{iuk}$, (ii) imports from country k account for a large fraction of total US expenditure on industry i goods, $\rho_{iuk} \equiv E_{iuk}/E_{iu}$, and (iii) US industry i sells a large fraction of its total output in the domestic market, $\gamma_{iuu} \equiv x_{iuu}/x_{iu}$, and thus stands more to benefit from a tariff protection of that market. The second term, $-\theta \sum_j \gamma_{iju}\hat{\tau}_{iju}$, captures the effect of retaliatory tariffs imposed by other countries j on the demand for goods supplied by US industry i. This exposure of industry i to retaliatory tariffs is high if (i) there is a large tariff rate increase for industry i goods that the US exports to country j, $\hat{\tau}_{iju}$, and (ii) exports to the foreign market j account for a large fraction of US industry i's total sales, $\gamma_{iju} \equiv x_{iju}/x_{iu}$.

To translate these industry-level outcomes to their local labor market analogues, we write the change in demand for outputs of US commuting zone r as

$$\hat{x}_r = \sum_i \hat{x}_{ir} = \sum_i \frac{x_{ir}}{x_r}\hat{x}_{iu} \approx \sum_i \frac{e_{ir}}{e_r}\hat{x}_{iu} \tag{2}$$

where x_{ir}/x_r is the share of industry i in total output of CZ r, and e_{ir}/e_r is the share of industry i in total employment in r, which in turn serves as a proxy for x_{ir}/x_r.[21]

To operationalize the import tariff exposure term in equation (1), we calculate the exposure of US industry i to US tariffs on imports from countries k at time t as:

$$IMP_{it} = 100 \times \gamma_{iuu} \sum_k \rho_{iuk}\hat{\tau}_{iukt}, \tag{3}$$

In this expression, $\hat{\tau}_{iukt} \equiv \ln(1+\tau'_{iukt}) - \ln(1+\tau_{iukJan18})$ is change in the US import tariff for industry i goods imported from country k from January 2018 to a subsequent month t between February 2018 and December 2019.[22] The variable $\rho_{iuk} \equiv E_{iuk}/E_{iu}$ 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:

$$RET_{it} = 100 \times \sum_j \gamma_{iju}\hat{\tau}_{ijut} \tag{4}$$

where $\hat{\tau}_{iju} \equiv \ln(1+\tau'_{ijut}) - \ln(1+\tau_{iju})$ is the tariff change by country j on US goods from industry i between January 2018 and a subsequent 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.[23]

[21] 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.

[22] Industry exposure IMP_{it} to the Trump tariffs is zero by construction for any month prior to the start of the trade war in February 2018.

[23] For simplicity, we do not scale IMP_{it} and RET_{it} with the constant trade cost elasticity θ as in equation (1).

In the empirical analysis, we construct values for equations (3) and (4), insert these into equation (1), and then apply equation (2) to measure CZ exposure to both changes in US import tariffs and to foreign retaliatory tariffs.[24] Our analysis of employment changes in local labor markets will capture the combination of the impact of tariffs on directly-exposed industries with several spillover effects: local spillovers across industries through input-output linkages, local demand multipliers, and induced labor reallocation across sectors within CZs.

3.3 Patterns of Tariff Exposure

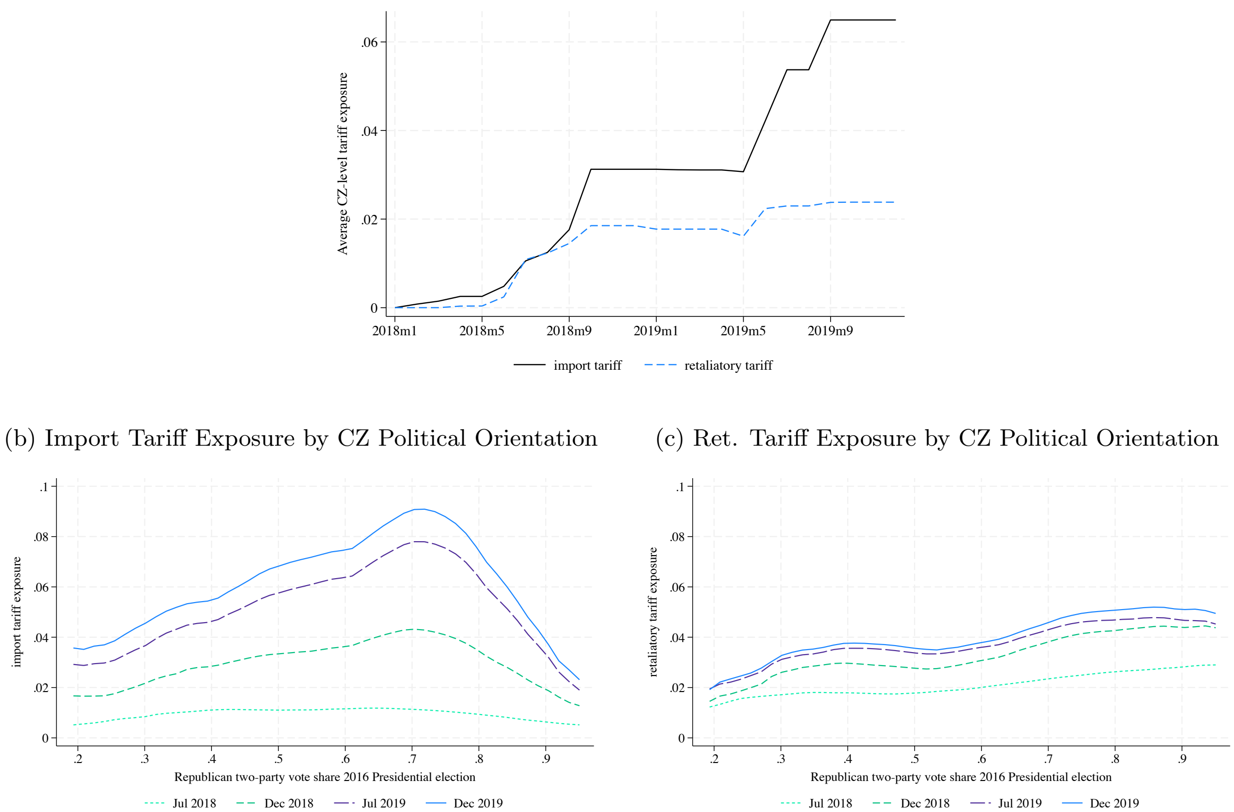
Figure 1 sets the stage for the main empirical analysis by reporting average CZ-level import and retaliatory tariff exposure during the trade-war period.

Figure 1: CZ Exposure to Import and Retaliatory Tariffs Over Time and by CZ Political Orientation

(a) CZ Import & Retaliatory Tariff Exposure Over Time

(b) Import Tariff Exposure by CZ Political Orientation

(c) Ret. Tariff Exposure by CZ Political Orientation



Notes: Panel a shows average CZ exposure to import tariffs (black line) and retaliatory tariffs (blue dashed line) from January 2018 to December 2019, weighted by 2012 CZ employment. Industry import and retaliatory tariff exposures are calculated according to equations (3) and (4) and mapped to CZs based on equation (2). Panels b and c show local polynominal smooth plots of CZ exposure to tariffs for CZs with different levels of Republican two-party vote share in the 2016 presidential election.

[24]The resulting exposure of CZ r to import and retaliatory tariffs are thus $\sum_i \frac{e_{ir}}{e_r} IMP_{it}$ and $\sum_i \frac{e_{ir}}{e_r} RET_{it}$.

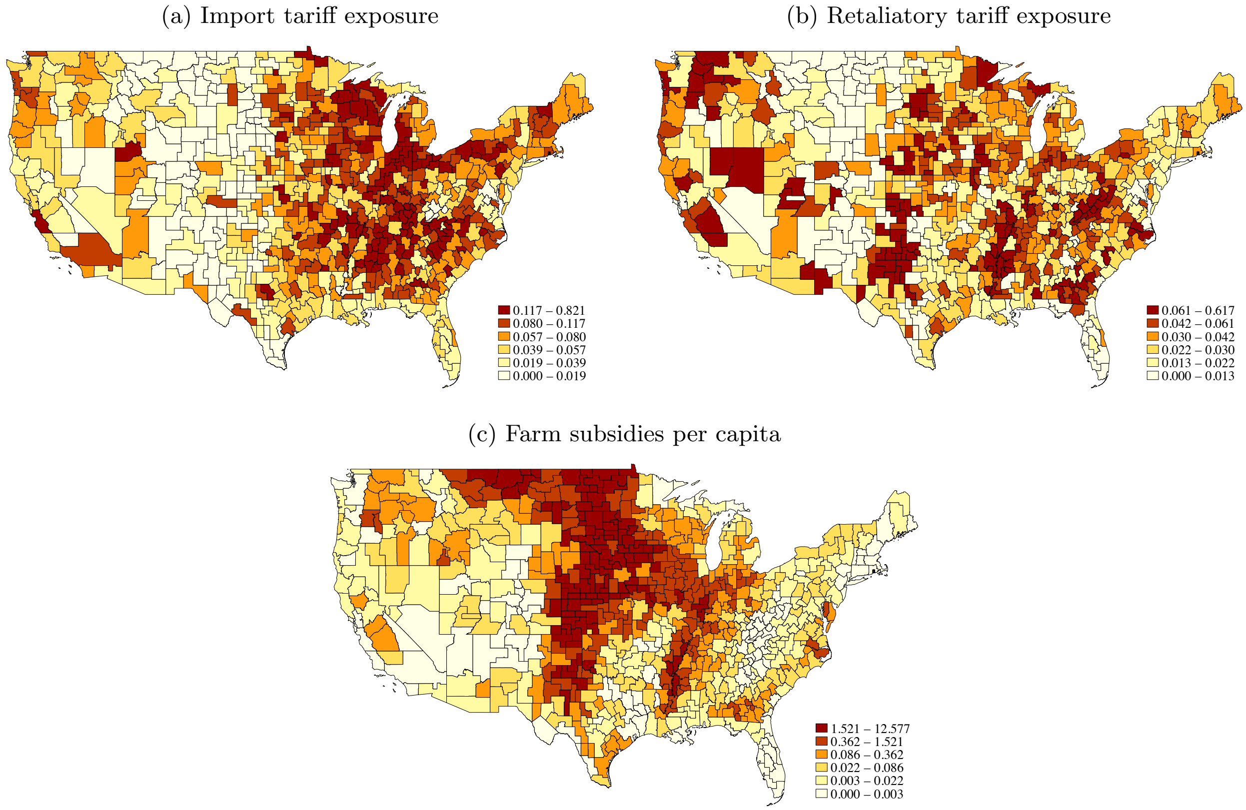
Panel A of the figure shows that tariff hikes commenced in February 2018 and escalated through the fall of 2019. The mean effective import tariff exposure statistic across all CZs averaged 0.065 by December 2019, with 25th, 50th, and 75th percentile values of 0.04, 0.06, and 0.08 (see Table A1). To interpret the magnitude of these exposures, consider an industry in which the US raised tariffs from 20% to 30% for countries whose exports to the US account for 10% of initial US spending on the industry's goods. If the tariffed US industry sells four fifths of its production domestically, then industry-level import tariff exposure according to equation (3) is $100 \times 0.8 \times 0.1 \times (\ln(1.3) - \ln(1.2)) = 0.64$, which matches the exposure of the average tradeable industry (see Table A8). A CZ that has 10% of its employment in that industry while the remainder is in non-traded sectors would have an import tariff exposure of $0.1 \times 0.64 = 0.064$, closely corresponding to the average CZ's tariff exposure at the end of 2019. Exposure to foreign retaliatory tariffs averaged about half the level of US import tariff exposure in 2018 and about one-third of the level of import tariff exposure in 2019 (Table A1).

Panels B and C of Figure 1 indicate considerable heterogeneity in tariff exposure depending on CZ's two-party Republican vote share in the 2016 presidential election. Whereas prior literature has studied the political targeting of tariffs through the fall of 2018 (Fajgelbaum et al., 2020; Fetzer and Schwarz, 2021; Kim and Margalit, 2021), our data cover a substantially longer time period through winter 2019 that includes large additional tariff hikes. Panel B of Figure 1 shows that early US import tariffs by July 2018 were highest in politically competitive CZs with Republican vote shares of about 40 to 65 percent. Subsequent tariff increases however raised import protection primarily in Republican-dominated areas. Retaliatory tariffs shown in panel C also became increasingly concentrated in CZs with Republican majorities.

Figure 2 plots the spatial distribution of exposure to tariffs and agricultural subsidies. Panel A shows that import tariff protection concentrates in traditional industrial states (e.g., Pennsylvania, Ohio, Indiana and Michigan), as well as in manufacturing-oriented Southern states (e.g., Tennessee and North Carolina). 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 import tariff and retaliatory tariff exposures is $\rho = 0.48$). However, panel B of Figure 2 shows that the incidence of retaliatory tariff exposure is also high in regions that were specialized in particular sub-sectors of agriculture. Heavily exposed regions include the lower Mississippi Valley (soybeans and cotton), Northern Texas (cotton and sorghum), and parts of California's Central 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 considerably less exposed to retaliatory tariffs based on their crops.[25]

[25] Our ability to observe the spatial distribution of agricultural employment by crop types provides a measurement improvement over previous work that mapped retaliatory tariffs in agriculture to regions based on total agricultural employment (e.g., Fajgelbaum et al. (2020)) or based on agricultural services that are partially covered in the County Business Patters data (e.g., Javorcik et al. (2022)). Our measurement also differs from earlier work that accounted

Figure 2: CZ Exposure to Import and Retaliatory Tariffs and Farm Subsidies



Notes: The figure depicts CZ exposure to import tariffs (Panel (A)), retaliatory tariffs (Panel (B)), and farm subsidies per working age population (Panel (C)) as of December 2019. Industry import and retaliatory tariff exposures are calculated according to equations (3) and (4) and mapped to CZs based on equation (2).

Not surprisingly, CZs facing higher retaliatory tariffs received larger farm subsidies, though the correspondence is far from perfect ($\rho = 0.29$). A comparison between panels B and C of Figure 2 for instance shows that North Dakota and Montana received high per capita agricultural subsidies despite limited exposure to retaliatory tariffs, consistent with a sometimes poor targeting of MFP compensation payments (United States Government Accountability Office, 2021).

4 Impacts of Tariffs on Industry-Level Trade, Sales and Inputs

Before advancing to our main analysis of the trade war's impact on labor market and political outcomes in CZs, we briefly present a set of results on trade, sales and production input variables that are observed at the industry but not regional level. To provide context for the interpretation

only for whether a US industry faced retaliatory tariffs from China, but not for whether China was an important export market for that industry (e.g., Kim and Margalit (2021); Blanchard et al. (2024)). In 2017/18, 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 export markets for corn and wheat (U.S. Department of Agriculture, 2022).

of subsequent null results for import tariffs' impact on employment, we ask whether these tariffs were important enough to affect trade flows and sales in tariff-protected US industries.

The impact of the trade war on US imports and exports in product categories and industries targeted by tariffs has already been widely studied (e.g., Amiti et al. (2019, 2020b); Fajgelbaum et al. (2020, 2021)). Consistent with this prior research, we confirm in Table A9 of Appendix A3 that industry exposure to US import tariffs (as defined in equation 3) caused a significant decline in monthly US imports, especially from China. Conversely, greater industry exposure to retaliatory tariffs (equation 4) reduced monthly US exports.[26]

We next ask whether US industries that gained protection from import tariffs were able to expand their sales as competition from imports declined, and whether exposure to foreign retaliatory tariffs reduced industry sales. Since there are no data on monthly sales by detailed industries, we draw on annual sales for six-digit manufacturing industries from the Annual Survey of Manufacturers and the Economic Census, which we combine with annual sales by subsectors of agriculture and mining from the Industry Economic Accounts of the Bureau of Economic Analysis. We use data from the years 2016 to 2019 to fit the specification

$$lnY_{it} - lnY_{i,17} = \beta_1 IMP_{it} + \beta_2 RET_{it} + \lambda_{s(i),t} + \phi(\Delta X_{i,16-17} \times t) + \varepsilon_{it}, \tag{5}$$

where an annual log outcome lnY_{it} (measured in log point units) is normalized with regard to its pre-period value in 2017. This specification absorbs level differences in the outcome variable just prior to the trade war, distinct from a specification with industry fixed effects that would absorb level differences averaging over pre and post-treatment periods. The variables IMP_{it} and RET_{it} measure tariff exposures as defined in equations 3 and 4.[27] Controls include a full set of interactions $\lambda_{s(i),t}$ between year indicators and indicators for the broad sectors of manufacturing, and agriculture and mining, which absorb non-parametric sector-specific time trends. Some specifications further control for an industry-specific linear pre-trend in the outcome variable during the period 2016-2017, $(\Delta X_{i,16-17} \times t)$.[28]

Table 1 presents results. Columns 1 and 2 in 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. The column 2 coefficient estimates imply that an industry with average import tariff exposure in year 2019 gained 0.85 log points of sales relative to an industry with no such exposure, while an industry with average exposure to retaliatory tariffs

[26] Appendix A3 provides further details on our analysis of tariffs' impact on monthly US imports and exports, which uses a regression design that closely resembles the one we introduce next for the analysis of industry-level sales.

[27] Table A8 provides descriptive statistics for the industry-level tariff exposure measures. Yearly tariff exposure is computed as the average monthly exposure during a year.

[28] The pre-trend control would have a coefficient estimate of $\hat{\phi} = 1$ if the outcome variable continued to evolve according to the 2016-2017 trend over the full outcome period through 2019.

lost -0.63 log points of sales.[29]

Table 1: Impact of Tariff Exposure on Industry Sales, Prices and Inputs

Panel A: sales and producer prices

	log sales (all tradable ind.)		log sales (manuf.)		log prices (manuf.)	
	(1)	(2)	(3)	(4)	(5)	(6)
import tariff exposure	2.275	1.851	1.874	1.480	0.924	0.956
	(0.788)	(0.868)	(0.696)	(0.799)	(0.508)	(0.358)
retaliatory tariff exposure	-2.727	-3.102	1.905	1.519	-1.498	-2.338
	(3.222)	(3.156)	(2.068)	(2.346)	(1.447)	(1.102)

Panel B: production inputs (manufacturing)

	log employment		log cost contract work		log capital expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)
import tariff exposure	0.029	0.205	4.330	4.385	6.474	6.463
	(0.439)	(0.625)	(5.169)	(5.219)	(3.572)	(3.581)
retaliatory tariff exposure	-1.211	-1.156	14.263	14.368	5.257	5.240
	(1.480)	(1.635)	(15.175)	(15.199)	(11.188)	(11.234)
(sector *) year FE	✓	✓	✓	✓	✓	✓
t * (Δ dep. var. in 2016-2017)		✓		✓		✓

N=1,300 in columns (1) to (2) of Panel A (325 industries x 4 years 2016-2019), and N=1,240 elsewhere except N=1,056 in columns (3) to (4) and and N=1,212 in columns (5) to (6) of Panel B. Data is sourced from the Annual Survey of Manufacturers (ASM) 2016, 2018 and 2019, the Economic Census of Manufacturing in 2017. The 2017 Economic Census does not report expenditures for contract work or capital and values are thus imputed by multiplying the corresponding expenditure-to-sales ratio of 2016 with observed sales in 2017. Regressions in columns (3) to (6) of Panel B exclude a small number of industries where the corresponding variables are not always reported in the ASM data. Regressions in columns (1) and (2) augment the ASM/Census data with BEA Industry Statistics data on sales in 15 sectors of agriculture and mining, and tariff exposures for these sectors are computed as import- or export-weighted averages of tariff exposure in the constituent 6-digit 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 prior to indexing are in Panel A: 2296.8 in columns (1) to (2) and 2290.5 in (3) to (4), and in Panel B: 999.0 in (1) to (2), 1817.8 in (3) to (4), 1934.2 in (5) to (6). The average of import (retaliatory) tariff exposure, weighted by industry employment in 2012, in 2019 is 0.461 (0.205). All regressions control for year fixed effects and the first two columns of Panel A interact these fixed effects with indicators for manufacturing, and for agriculture and mining. Regressions are weighted by industry employment in 2012, and std. errors are clustered by industry.

We next narrow our industry-level analysis to the manufacturing sector for which the Annual Survey of Manufacturers provides additional outcomes of interest. Columns 3 and 4 in Panel A of Table 1 first repeat the sales analysis for manufacturing industries only, which yields estimates similar to columns 1 and 2 for the impact of import tariffs on sales, whereas coefficients for retaliatory tariffs are now positive and remain very imprecisely measured. The manufacturing-only analysis is arguably more informative about the overall impact of import tariffs, which are concentrated in the manufacturing sector, than for retaliatory tariffs, which also targeted agriculture and mining. We

[29] The sales regressions in Panel A in Table 1 consider nominal sales, but coefficient estimates would be the same for CPI-adjusted sales since any inflation adjustment that is common across industries would be absorbed by the period fixed effects.

thus focus our discussion of the subsequent results in Table 1 on import tariffs while noting that all results for exposure to retaliatory tariffs within manufacturing are very imprecisely estimated.

Given the positive effect of import tariff protection on manufacturing industries' sales, one might hypothesize that these industries expanded production and labor in proportion to the expansion of sales. Columns 1 and 2 in Panel B of Table 1 however show that log employment barely reacted to the tariff protection of industries.

The remaining results of Table 1 address explanations for why a growth in sales could occur without a commensurate growth in employment. First, columns 5 and 6 in Panel A indicate that industries reacted to import tariff protections by significantly increasing their prices, which is consistent with a large prior literature that linked these tariffs to higher goods prices in the US (see Fajgelbaum and Khandelwal (2022) for a review). A comparison of the coefficient estimates for prices with those for sales suggests that about half to two thirds of import tariff's impact on sales may be accounted for by higher prices (e.g., $0.956/1.480 = 0.65$ based on the column 4 and 6 estimates), while only half or less of the sales expansion results from a greater volume of goods sold. A second reason why employment does not need to increase proportionally with sales is that firms may expand production by increasing inputs other than employed labor. Recent work by Atencio de Leon et al. (2024) notes that manufacturing firms increasingly use contract labor from staffing agencies in reaction to demand shocks instead of employing workers directly. An increased labor input for production would in this case not be registered as a growth in manufacturing employment but as growth in business services employment. Columns 3 and 4 in Panel B of Table 1 test whether tariff-protected industries increased their expenditure on contract labor. The coefficient estimates indeed suggest a highly elastic response of contract work to import tariff protection, though the results are imprecise. Another possible reaction of industries to import tariff protection could be to increase production by expanding capital rather than labor inputs. Columns 5 and 6 in Panel B of Table 1 confirm that log capital expenditures react strongly to import tariff exposure, where these effects are measured with t-statistics around 1.8. While the panel B estimates based on low-frequency annual manufacturing data lack high statistical precision, they do suggest that an analysis of tariffs' impact on employment based on manufacturing industries alone (like the pioneering work by Flaaen and Pierce (2021)) could miss potentially important employment spillovers outside the sector, for instance when tariff-exposed industries adjust their demand for local business services or their demand for transportation and construction services that can contribute to the provision of new capital. Our subsequent employment analysis will capture such spillovers from tradeable to nontradable sectors within local labor markets.

5 The Impacts of Tariffs on Regional Employment and Earnings

Our analysis of the local economic impacts of the US-China trade war proceeds in four steps: we first evaluate CZ employment trends before and during the trade war; we then present the main results for employment and split them into their sectoral components; we next provide complementary results for earnings; and conclude by briefly discussing further extensions.

5.1 Preliminary employment analysis

We estimate the effect of tariffs on cumulative employment changes in more versus less exposed CZs while accounting for pre-trends using the following event-study model:

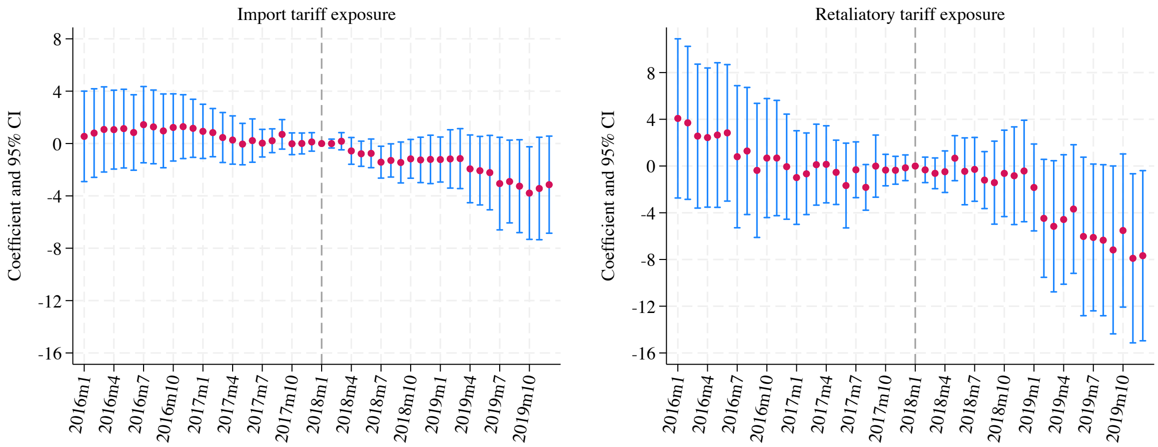
$$EPOP_{rt} - EPOP_{rJan18} = \delta_t + \beta_{1t} IMP_{rDec19} + \beta_{2t} RET_{rDec19} + \varepsilon_{rt}. \tag{6}$$

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 2019.[30] In this specification, IMP_{rDec19} and RET_{rDec19} are the import and retaliatory tariff exposure levels, respectively, of CZ r at their peak values in December of 2019, and δ_t is a set of year-month indicators that absorb nonparametric time trends. The coefficients β_{1t} and β_{2t} estimate whether employment-to-population rates evolved differently in CZs that eventually faced a large exposure to import or retaliatory tariffs. Observations are weighted by CZ-shares of aggregate employment in 2012, and standard errors are clustered at the state level.

The two panels of Figure 3 report period-specific estimates of the employment effects of exposure to cumulative trade-war changes in US tariffs (left panel) and retaliatory tariffs (right panel) based on equation (6). After the start of the trade war, employment-population rates weakly declined in CZs that were more exposed to import tariffs, while there were larger employment reductions in CZs facing retaliatory tariffs. Consistent with the timing of tariff levies depicted in Figure 1, these adverse effects do not ensue until mid-to-late 2018 when import and retaliatory tariffs started to escalate rapidly. Employment rates were, however, trending downward slightly in tariff-exposed CZs prior to the imposition of tariffs, suggesting some caution in drawing a causal interpretation of these patterns. Accordingly, we now fortify the bare-bones model in of equation 6 to better account for employment pre-trends and other potential confounds.

[30] The employment-to-population ratio is a popular metric of labor market health (Shierholz, 2014). We will later exploit the property that changes in employment-to-population ratios can be additively decomposed into changes in sectoral employment-to-population ratios. Section 5.4 provides supplementary evidence for changes in log employment.

Figure 3: Impact of Tariff Exposure on CZ Employment/Population Ratios



Notes: The figure plots by-period coefficient estimates and 95% confidence interval of the regression model described by equation (6). 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. Regressions are weighted by 2012 CZ employment, and standard errors are clustered at the state level.

5.2 Main employment results

To account for the pre-trends detected above and the staggered introduction of tariffs, we modify the specification in (6) by regressing changes in employment-population rates (measured in deviations from their baseline level in January 2018) on a CZ's time-varying exposure to import and retaliatory tariffs IMP_{rt} and RET_{rt}, as well as exposure to agricultural subsidies SUB_{rt}, while controlling in some specifications for a linear trend in the prevailing change of the average monthly CZ-level employment rate between January 2017 and January 2018:

$$\begin{aligned} EPOP_{rt} - EPOP_{rJan18} = {} & \beta_1 IMP_{rt} + \beta_2 RET_{rt} + \beta_3 SUB_{rt} \\ & + \gamma_t + \delta_{d(r),t} + \lambda_{s_r,t} + \phi(\bar{\Delta} EPOP_{r,2017} \times t) + \varepsilon_{rt}, \end{aligned} \tag{7}$$

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 census division dummies ($\delta_{d(r),t}$), and interactions between time effects and initial sectoral employment shares in manufacturing, agriculture and mining, and all other sectors ($\lambda_{s_r,t}$).

Table 2 presents results. The first model (column 1) relates changes in employment rates to local import and retaliatory tariff exposure, controlling only for month-by-year fixed effects. Consistent with the results in Figure 3, both import and retaliatory tariffs are associated with a fall in employment-population rates. Columns 2 through 4 add successively more complete controls, including initial sectoral distribution by year-month interactions in column 2, census-division-by-year-month interactions in column 3, and farm subsidy exposure in column 4. With the addition of these controls, the estimated effect of import tariffs on employment rates turns from negative to modestly positive, consistent with the presence of confounding pre-trends. Estimates, however, are noisy: in no specification do we reject the null of zero employment impacts of US import tariffs. The estimated employment effect of retaliatory tariffs, however, remains negative, stable, and precisely estimated across specifications. As shown in column 4, farm subsidies have measurable employment impacts: a thousand dollar increase in farm subsidies per capita is estimated to raise employment rates by 0.24 percentage points (se = 0.10). Moreover, controlling for farm subsidies in the employment regressions increases the magnitude and precision of the estimated adverse effect of retaliatory tariffs on employment. This result is expected since farm subsidies were targeted to local labor markets exposed to retaliatory tariffs.

Table 2: Impact of Tariff Exposure on CZ Employment

	no pretrend control				control for 2017 pretrend			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
import tariff exposure	-3.221	3.041	1.905	2.173	-1.993	1.769	1.362	1.682
	(1.828)	(1.702)	(1.083)	(1.078)	(1.445)	(1.343)	(1.379)	(1.334)
retaliatory tariff exposure	-4.518	-6.125	-6.202	-6.699	-5.456	-3.412	-4.217	-4.811
	(2.270)	(1.918)	(2.189)	(2.163)	(2.483)	(1.861)	(1.734)	(1.731)
farm subsidies per capita				0.237				0.284
				(0.103)				(0.117)
t * (monthly Δ emp/pop in 2017)					0.556	0.568	0.528	0.528
					(0.060)	(0.051)	(0.041)	(0.040)
year-month FE	✓	(✓)	(✓)	(✓)	✓	(✓)	(✓)	(✓)
sector*year-month FE		✓	✓	✓		✓	✓	✓
Census division*year-month FE			✓	✓			✓	✓

Notes: N=34,656 (722 commuting zones x 48 months: Jan 2016 – Dec 2019). The dependent variable for all regression models is the seasonally-adjusted employment-to-population ratio, which is indexed to 0 in 2018m1 in each commuting zone. The mean (standard deviation) of the employment-to-population ratio prior to indexing is 66.3 (7.8) percentage points in 2018m1. Farm subsidies are denoted in units of 1,000s of 2018 dollars per working age population. Regressions in columns 5 to 8 control for the monthly change in employment-to-population from 2017m1 to 2018m1, interacted with a linear time trend (the count of months since 2018m1). Regressions in columns 2 to 4 and 6 to 8 interact time fixed effects with a commuting zone's sectoral employment shares (agriculture and mining, manufacturing, non-goods sector) in 2012, while columns 3 to 4 and 7 to 8 also interact time fixed effects with indicators for the 9 geographic Census divisions. Regressions are weighted by commuting zone employment in 2012, and standard errors are clustered by state.

As a further check against pre-trends, columns 5 through 8 of Table 2 repeat the first four specifications while additionally controlling parametrically for the employment pre-trend prevailing in each CZ in the year prior to the start of the tariff war. This pre-trend variable is positive and precisely estimated in all cases but it has only modest effects on the coefficients of interest: import tariff protections predict a weakly positive increase in CZ employment rates in most regression specifications; retaliatory tariffs predict a robustly negative decline in employment; and farm subsidies appear to exert a countervailing effect on these declines.[31]

To assess the economic magnitudes of these tariff effects, we multiply the point estimates in the final column of Table 2 by average CZ-level tariff exposures in December 2019. This yields an average 0.110 percentage point gain in the employment rate due to import tariffs, a -0.115 percentage point decline due to retaliatory tariffs, and a +0.028 percentage point gain due to subsidies. The sum of these effects is thus near zero (+0.022), meaning that the implied effects of import and retaliatory tariffs are nearly offsetting. Although farm subsidies are estimated to boost employment (see columns 4 and 8), they fail to compensate for the negative effect of retaliatory tariffs.[32]

5.3 Sector-level tariff impacts

To better understand the sectoral contributions to these aggregate CZ-level employment effects, Table 3 leverages the detail of the QCEW data to consider how tariffs and subsidies affect employment by broad sector within CZs. We divide employment into primary, manufacturing, and all other sectors, then further divide these sectors as follows: within the primary sector, distinguishing crop production from other primary activities; within manufacturing, distinguishing the primary metal industry and the metal-using industries metal products, machinery, and automotive from all other manufacturing; and within the remaining sectors, distinguishing transportation and warehousing, and business services from the residual set.

Applying equation (7) to these eight sectors, and using the full set of controls from column 8 of Table 2, we obtain the impact of tariffs and subsidies on sector-specific employment rates. The sector-specific coefficient estimates in columns 2 to 9 of Table 3 sum up to the total employment effect across all sectors shown in column 1 of the table. Import tariffs do not have a precisely estimated impact on employment in any sector, including manufacturing (the intended beneficiary). We single out the primary metal sector in column 4 because it is the only 3-digit sector that obtained comprehensive tariff protection against imports from almost all countries and not just China, and major metal-using sectors in column 5 that may have faced higher input prices as a consequence of these tariffs. The coefficient estimates indicate an insignificant positive employment effect in primary metals and a larger negative effect in metal-using industries, which is qualitatively consistent with evidence on the employment effects of the earlier 2002-2003 US steel tariffs (Cox,

[31] Appendix Table A2 presents a further variant of pre-trend control with regression models that include a linear time trend in the CZ-level change in $EPOP$ from January 2016 to January 2018, thus controlling for a two-year instead of a one-year pre-trend prior to the start of the tariff war. Regressions with full controls again indicate an insignificant positive employment effect of import tariffs, combined with a significant negative effect of retaliatory tariffs, and a positive effect of agricultural subsidies.

[32] In the simplified regression model of column 5 that excludes the non-parametric interactions between time effects and both sectoral composition and Census divisions, we find that the combined effect of the two tariff variables is to reduce employment to population by -0.259 percentage points in a CZ with average exposure.

Table 3: Impact of Tariff Exposure on CZ Employment by Sector

	all	primary sector		manufacturing			other sectors		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	total effect	crop prod	other	primary metal	metal prod, machines, cars	other	transport, ware-housing	business services	all other
import tariff exposure	1.682	0.207	0.144	0.149	-0.560	-0.288	0.164	0.970	0.896
	(1.334)	(0.122)	(0.268)	(0.092)	(0.376)	(0.360)	(0.200)	(0.535)	(0.871)
retaliatory tariff exposure	-4.811	-1.038	-0.500	0.180	0.166	-0.167	-1.024	-1.177	-1.251
	(1.731)	(0.504)	(0.727)	(0.230)	(0.318)	(0.539)	(0.234)	(0.304)	(0.755)
farm subsidies per capita	0.284	0.038	0.011	0.007	0.024	0.079	-0.029	0.022	0.132
	(0.117)	(0.014)	(0.029)	(0.005)	(0.023)	(0.044)	(0.022)	(0.027)	(0.087)
t * (monthly Δ emp/pop in 2017)	0.528	0.002	0.102	-0.001	0.032	0.017	0.025	0.083	0.268
	(0.040)	(0.002)	(0.039)	(0.001)	(0.007)	(0.010)	(0.012)	(0.017)	(0.053)
year-month FE	(✓)	(✓)	(✓)	(✓)	(✓)	(✓)	(✓)	(✓)	(✓)
sector*year-month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Census division*year-month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
employment share in 2017	1.000	0.004	0.009	0.003	0.030	0.056	0.034	0.136	0.728

Notes: N=34,656 (722 commuting zones x 48 months: Jan 2016 – Dec 2019). 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 2018m1 in each commuting zone. Farm subsidies are denoted in 1,000s of 2018 dollars per working age population. All regressions include a control for the monthly change in CZ employment-to-population from 2017m1 to 2018m1, interacted with a linear time trend (the count of months since 2018m1). All regressions include time fixed effects interacted with a commuting zone's sectoral employment shares (agriculture and mining, manufacturing, non-goods sector) in 2012, and with indicators for the 9 geographic Census divisions. Regressions are weighted by commuting zone employment in 2012, and standard errors are clustered by state.

2022). Retaliatory tariffs significantly reduce employment in crop production (a primary target sector), as well as in transportation and warehousing and in business services. A plausible interpretation of this pattern is that crop producers purchase many of their non-material inputs from the local service sector, and a contraction in agricultural activity generates adverse spillovers on local service firms.[33] A third result from 3 is that farm subsidies significantly increased employment in crop production while modestly raising employment across other sectors. The farm subsidies were designed to compensate farm owners for foregone income rather than to avert a decline in agricultural production or employment. While it is plausible that some farm owners used the subsidy to maintain jobs at their farms, the subsidy may otherwise have operated as additional income that was spent across a broad range of goods and services.

[33]The coefficient estiamtes in Table 3 imply that a unit increase in retaliatory tariff exposure is associated with the loss of 2.5 jobs in nontradables for each job lost in tradeables (sum of coefficients in columns 7-9 divided by sum of coefficients in columns 2-6), whereas Moretti (2010) estimated that the creation of one additional job in the tradeable sector of a local labor market creates 1.7 additional jobs in the nontradable sector at a longer, decadal frequency. One reason contributing to the slightly higher ratio in our application could be an undercount of undocumented workers in crop production, who make up 44% of the sector's workforce according to the US Department of Labor's National Agricultural Workers Survey (Gold et al., 2022). If one assumes that the Table 3 estimates capture only 56% of the true employment adjustment in crop production, then the implied ratio of job loss between the nontradable and tradable sectors falls to 1.6.

5.4 Earnings

The finding that import tariffs did not generate sizable or significant employment gains doen not necessarily rule out local earnings gains, for instance if firms increased work hours and pay of extant employees. The QCEW data allows us to study CZ earnings, although different from the employment data, they are measured only at quarterly frequency and are not further broken down by sector. In Table 4, we use a quarterly version of the regression model of equation (7) to analyze the impact of tariffs on log quarterly wage bills in CZs, and also provide complementary results for log quarterly employment to facilitate a comparison between earnings and employment effects.[34]

Table 4: Impact of Tariff Exposure on Total Earnings and Employment in CZs

	log earnings				log quarterly employment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
import tariff exposure	-0.118	0.038	0.006	0.011	-0.076	0.043	0.038	0.037
	(0.046)	(0.062)	(0.068)	(0.067)	(0.029)	(0.025)	(0.021)	(0.021)
retaliatory tariff exposure	-0.072	-0.138	-0.062	-0.072	-0.134	-0.161	-0.082	-0.081
	(0.053)	(0.064)	(0.055)	(0.056)	(0.040)	(0.040)	(0.032)	(0.032)
farm subsidies per capita				0.004				-0.000
				(0.006)				(0.001)
t * (quarterly Δ dep. var. in 2017)			0.408	0.409			0.641	0.641
			(0.060)	(0.060)			(0.030)	(0.030)
year-quarter FE	✓	(✓)	(✓)	(✓)	✓	(✓)	(✓)	(✓)
Census division*year-quarter FE		✓	✓	✓		✓	✓	✓
sector*year-quarter FE		✓	✓	✓		✓	✓	✓

Notes: N=11,552 (722 commuting zones x 16 quarters in 2016-2019) for employment and N=11,548 for quarterly earnings (4 CZ-quarter observations are omitted due to suppressed earnings data). The dependent variable is either the quarterly log wage bill of a CZ in columns 1-4 or quarterly log employment in columns 5-8, where each of these variables is indexed to 0 in the fourth quarter of 2017 in each CZ. The mean of the outcome variables in the fourth quarter of 2017 prior to demeaning is 23.07 for log earnings and 13.53 for log employment. Farm subsidies are denoted in units of 1,000s of 2018 dollars per working age population. Regressions are weighted by commuting zone employment in 2012, and standard errors are clustered by state.

The first four columns of Table 4 indicate that import tariffs and farm subsidies have small positive effects on total CZ earnings in most regression specifications, while the impact of retaliatory tariffs is larger and consistently negative. The coefficient estimates for quarterly earnings in columns 1 to 4 of the table are qualitatively and quantitatively similar to those for quarterly employment in columns 5 to 8, although the earnings effects are measured with lower precision. This similarity implies that tariff-induced changes in total CZ earnings were roughly proportional to employment changes.[35]

[34] The quarterly version of equation (7) normalizes the outcome variable with regard to the fourth quarter of 2017, controls for pre-trends based on the average quarterly growth of the outcome variable between the fourth quarters of 2016 and 2017, and measures tariff exposure for each quarter as the average exposure of the three constituent months.

[35] The results for quarterly earnings and employment in Table 4 are also similar to those for monthly employment

To assess the economic magnitudes of the estimated earnings effects, we multiply the point estimates in the fourth column of Table 4 by average CZ-level tariff and subsidy exposures in December 2019. This yields a 0.07 log point gain in CZ quarterly earnings due to import tariffs, a -0.17 log point loss due to retaliatory tariffs, and a 0.04 log point gain due to subsidies, where these effects sum to a very small earnings decline of -0.06 log points. The earnings analysis thus supports our previous conclusion that CZ exposure to the trade war neither generated substantial labor market gains nor losses for local residents.

5.5 Additional margins of labor market adjustment

National Industry Spillovers. Our analysis of employment changes in local labor markets captures spillovers from directly trade-exposed industries to other local industries that operate through local input-output linkages, local demand multipliers, and induced labor reallocation across sectors within CZs. It may however fail to capture the effects of spillovers through industry input-output linkages that operate at the national rather than local level (Acemoglu et al., 2016). As a complement to our employment analysis in local labor markets, Appendix A3 develops and presents an analysis of QCEW industry employment data that uses national input-output tables to capture not only industries' direct exposure to import and retaliatory tariffs but also their indirect exposure to the tariffs faced by their customer and supplier industries. The results from this exercise (Table A10) are consistent with those of the local labor market analysis: direct industry exposure to import tariffs variably has a weak positive or weak negative effect on employment, while exposure to retaliatory tariffs consistently has a significant negative impact. Accounting for indirect tariff exposure via input-output linkages does not affect the estimates for industries' direct exposure, while the estimates for these supply chain spillover effects are imprecise and sensitive to pre-trends.[36]

Longer-Term Impacts. It remains an open question whether import protection that failed to generate substantial job gains during the trade war might spur job creation over longer time horizons. It may take both a sustained reduction in policy uncertainty (Handley and Limão, 2017; Alessandria et al., 2021; Handley and Limão, 2022), as well as physical capacity building, before worker headcounts rise in response to newly higher tariffs.[37] The positive impact of import tariffs

rates in Table 2, except that farm subsidies have a small but significant positive employment impact in Table 2 and no impact in the Table 4 analysis.

[36] Prior literature on the Trump tariffs (Flaaen and Pierce, 2021; Javorcik et al., 2022) and on earlier episodes of protective tariff policies (Cox, 2022; Bown et al., 2024) emphasizes an adverse employment effect of import tariff protection for supplier industries that may increase industries' input costs. The analysis in Table A10 also shows a negative employment impact of supplier import exposure for the subsample of tradeable industries, but the estimated effect is insignificant and small.

[37] It is of course also possible that over longer time horizons, imports from China will not be substituted by US manufactures but by increased imports from other countries in which the buildup of additional manufacturing capacity also takes time.

on capital expenditure observed in section 4, and the retention of the tariffs after the change of the Presidency from Donald Trump to Joseph Biden in 2020 suggest that these developments may already be under way. While the two-year outcome window of our employment analysis is longer than the time periods studied in prior work on the labor market effects of the tariff policies (Waugh, 2019; Flaaen and Pierce, 2021; Javorcik et al., 2022), it is restricted at its start by the beginning of the trade war and at its end by the confounding effects of the Covid-19 pandemic that followed in early 2020. The pandemic not only generated a labor market shock of historically unprecedented dimensions, but also dramatically disrupted international trade. Nevertheless, to probe for potential longer-term gains from trade exposure, we analyze in Table A3 the change in employment-population ratios between December 2019 and December 2021, where the former date marks the end of the tariff escalation and the latter date follows after a substantial (though at that time incomplete) recovery from the labor market impacts of the pandemic. Regressors are specified as in equation (7), with tariff and subsidy exposure measured at their December 2019 values. To account for the differential exposure of CZs to the pandemic, models control for the employment declines in manufacturing, agriculture and mining, and other sectors that each CZ suffered at the onset of the pandemic from March to April 2020. The Table A3 estimates indicate that neither exposure to import tariffs nor exposure to retaliatory tariffs had a sizable or significant impact on employment growth between the end of 2019 and 2021. Greater local payments of agricultural subsidies were associated with precisely estimated but small differential employment gains over this extended period.

6 Assessing the Political Spoils of the Trade War

While a stated goal of the Trump administration when instigating the trade war in 2018 was to "bring back jobs to America," a second (implicit) goal of the policy was surely to garner political support. The evidence above indicates that the first objective was not achieved, at least during the first years of the policy. But this does not preclude the possibility that the second objective was successful. If the trade war conveyed political solidarity with voters in import-competing locations, its tangible consequences for jobs might be secondary to its political consequences. We consider those consequences here by studying political and electoral outcomes where employment and voting intersect at the level of detailed geography. Starting with a narrow focus and zooming outwards, we first assess awareness of and support for tariff policies in the locations whose industries were exposed to tariffs; then quantify how tariffs affected voters' party identification in these locations; and subsequently consider the impact of the tariffs on partisan vote shares in presidential and congressional elections until 2020. We finally contrast the impact of tariff exposure on electoral outcomes with our previous findings for employment.

6.1 Awareness of tariff policies

A well-known result in the political economy literature is that voters reward incumbent governments for good economic performance (Fair, 1978). As long as voters can observe differential economic outcomes across regions, they may react to these economic outcomes without necessarily being aware of any underlying government policy (Bagues and Esteve-Volart, 2016).

Since we conclude that the 2018-2019 tariffs had little differential employment and earnings impacts across local labor markets, it is unlikely that voters could respond to the trade policy based only on the observation of its tangible local economic effects. Instead, voters would need to be aware of the tariffs in order to react to them. Such awareness seems plausible given that the tariff war was a very prominent topic in national news media. As an example, the briefing section of the *New York Times* which summarizes the most important news stories of the day or week used the word "tariffs" in 27% of all briefings in 2018, up from just 1% of all briefings in 2017. The term "tariffs" thus became more ubiquitous is the 2018 news than other frequently used economic terms such as "workers" or "dollar".[38]

To study the acquisition of information about tariffs more systematically and without reliance on single news outlet such as the *New York Times*, we draw on data from Google Trends. This database indicates the relative frequency of searches for a particular term on the Google search engine across time and space, and thus indicates when and where people sought to acquire information about a particular concept.[39] Figure 4 indicates monthly searches for the term "tariffs" nationally (black line) and by state (gray lines) using a common scaling for search intensity that is normalized to a national average of 1 in 2016-2017, prior to the trade war. A comparison of the Figure's Panel A with the time series of tariff exposure in Figure 1 shows that web search related to tariffs spiked in the periods when new tariffs were introduced: metal tariffs and first Chinese retaliatory tariffs in March-April of 2018, US import tariffs on Chinese goods and retaliatory tariffs by several other countries in June-September 2018, and additional tariffs between the US and China in May-August 2019.[40] The state-level data series in Panel A are qualitatively similar to the national series but indicate considerable heterogeneity in search intensities across states. We next explore whether these cross-state differences in search intensity are systematically related to regional exposure to tariffs. Panels B and C of Figure 4 separately report the average search intensity for states with an above-average or below-average exposure to import tariffs or retaliatory tariffs as of December 2019. They indicate no systematic differences in search patterns across states with high versus

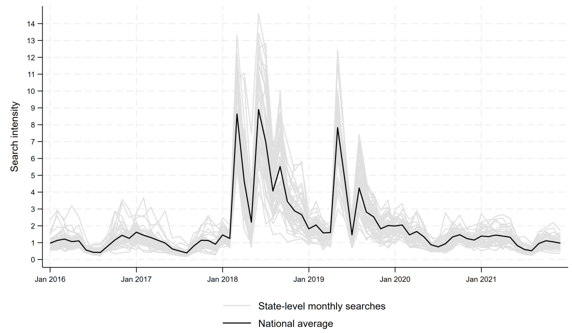
[38]The archive of the *New York Times* (https://www.nytimes.com/search/?srchst=nyt) indicates that the term "workers" appeared in 25% of the briefings in 2018 and 23% of those in 2017, whereas the term "dollar" was used in 16% of the briefings in 2018 and 13% of those in 2017.

[39]Google Trends provides comprehensive data at the level of states but not for smaller geographic units such as CZs.

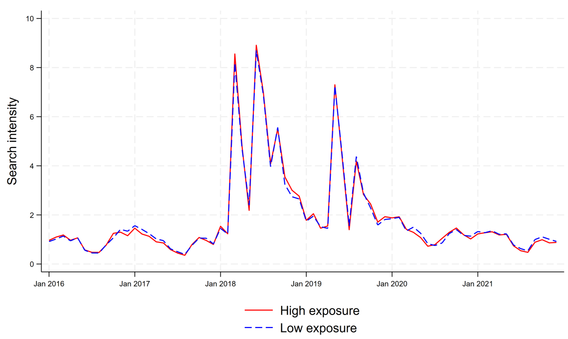
[40]The Google Trends data indicates the time of information acquisition rather than the time when people forgot information they had once acquired. The fact that web search for tariffs declines in 2020 is consistent with a lack of new developments in the trade war but does not imply that people were no longer aware of recently implemented tariffs.

Figure 4: Internet Searches for "Tariffs"

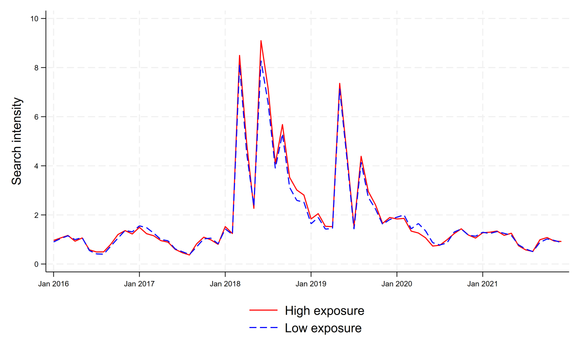
(a) all states



(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.

low local exposure to tariffs. We interpret this result as indicating that tariffs were a subject of national rather than local interest. However, even if voters in all regions had equal knowledge of the tariffs, it is still possible that those whose regions were directly targeted would voice different levels of support for the trade policies. We assess this next.

6.2 Support for US tariff policies

To study voter attitudes towards trade war policies in tariff-exposed locations, we use the Cooperative Congressional Election Study (CCES), which in October 2019 polled voters on whether they supported or opposed two planks 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%.

In Table 5, 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:

$$Y^p_{jr} = \alpha + \beta_1 IMP_{rOct19} + \beta_2 RET_{rOct19} + \beta_3 SUB_{rOct19} + \beta_4 RVS_{r,16} + \delta_{d(r)} + \lambda_{s_r} + \boldsymbol{X_j'}\boldsymbol{\pi} + \varepsilon_{jr} \quad (8)$$

Here, the outcome variable is the percentage of the tariff policies that respondent j residing in CZ r expressed support for.[41] Alongside the treatment variables (import and retaliatory tariff exposures, farm subsidies), some models additionally include a vector of Census division effects $\delta_{d(r)}$, and initial CZ sectoral employment shares in manufacturing, agriculture and mining, and all other sectors (λ_{s_r}). Some models further add 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). Since the tariff policies were not known or widely discussed prior to their implementation, there is no baseline measure available to gauge the extent to which residents of a CZ supported protectionist policies prior to the trade war. Instead, we control in some specifications for a CZ's Republican two-party vote share in the 2016 Presidential election, $RVS_{r,16}$, as a lagged predictor for the subsequent support for the tariffs of the Trump government. Regressions are weighted by CCES sampling weights, which are normalized to match CZ population in 2010. Standard errors are clustered at the state level.[42]

The results in columns 1 to 4 of Table 5 indicate that local support for tariff policies in 2019 corresponds closely with the 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 appears higher in CZs receiving greater farm subsidies, though these estimates are less precise.

We previously showed in panel B of Figure 1 that CZs with high exposure to import tariffs were leaning towards the Republican party already in 2016. It is thus possible that the positive relationship between import tariff exposure and support for the Trump government's tariff policies reflects a more favorable attitude towards President Trump that predates the tariff war. To assess this possibility, columns 5 to 8 of Table 5 control for the Republican two-party vote share in the 2016 presidential election. As expected, support for tariffs in 2019 was substantially higher in CZs

[41] By averaging the answers to the two tariff questions, we get a slightly larger sample size and higher precision. 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.

[42] Because the tariff support questions were asked in only one survey year, 2019, the analytic sample for Table 5 analysis is relatively small, with approximately 100 of 722 CZs not represented.

Table 5: CZ Residents' Support for US Import Tariffs in 2019

	no 2016 Repub TPVS control				control for 2016 Repub TPVS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
import tariff exposure	9.73	15.92	21.96	24.96	20.44	20.04	24.64	26.95
	(12.54)	(13.86)	(12.03)	(12.19)	(13.31)	(14.64)	(12.15)	(12.20)
retaliatory tariff exposure	-33.50	-35.86	-42.24	-52.10	-34.84	-31.69	-37.09	-45.09
	(22.05)	(21.50)	(24.74)	(26.49)	(21.72)	(21.84)	(25.31)	(26.48)
farm subsidies per capita				2.90				2.31
				(1.62)				(1.58)
2016 Repub. TPVS					0.25	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. The mean (standard deviation) for the outcome is 44.0 (42.0). 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. Trade tariff exposure variables are equal to CZ exposure in October 2019. Farm subsidies per working age population are in 1000 of US 2018 dollars and equal to the cumulative amount of farm subsidies paid as of October 2019. Demographic controls include a quadratic in age, gender, race (non-Hispanic white vs Not non-Hispanic or white) and education (at least some college education vs high school or less). Regressions are weighted by $pop_{2010,CZ} * (CCESweight_i / \sum_{i=1}^{CZ} CCESweight_i)$, and standard errors are clustered at state level.

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 government. The inclusion of the vote share control however has little impact on the coefficient estimates for the tariff and subsidy variables. 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 government's tariff policies while residents with CZs facing higher retaliatory tariffs were less supportive.

These public opinion results are meaningful in two respects. First, their magnitudes are non-trivial. Comparing a CZ at the 75th vs. 25th percentile of import tariff exposure in 2019, the estimate in column 8 of Table 5 predicts that support for these tariffs would be about 1.1 points higher in the more exposed CZ. Second, these estimates demonstrate that voter support for tariff policies is not simply a proxy for—or fully explained by—earlier voter support for the President.

6.3 Political identification

Public support for a particular policy is not equivalent to support for parties. Research highlights that 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; Grossman and Helpman, 2021). This observation appears particularly relevant for trade policy, which is fraught with perceived economic and class conflicts that pit affluent, highly-educated consumers against blue collar workers and manufacturing-intensive communities (Davenport et al., 2022; Pierce and Schott, 2020). For example, a 2021 Gallup poll found that 51% of Republicans and 40% of voters with no more than a high school education view foreign imports as a threat to the economy. By contrast, these fractions are only 18% and 25% among Democrats and college graduates, respectively (Gallup Organization, 2021). It is therefore possible that the Trump administration's polarizing trade policies would foster favorable partisan identification even if the tangible benefits of these policies were elusive.

We assess the effect of trade policy on party identification by again drawing on the CCES. Though national elections occur only once every two years, CCES data provide a detailed window into party identification at an annual frequency. Using pooled CCES data for the years 2016–2019, we study the consequences of trade policy on party identification at the level of detailed geography by estimating linear models of the form:

$$\begin{aligned} Y^p_{jrt} - \bar{Y}^p_{r,17} &= \beta_1 IMP_{rt} + \beta_2 RET_{rt} + \beta_3 SUB_{rt} \\ &\quad + \gamma_t + \lambda_{s_r,t} + \delta_{d(r),t} + \boldsymbol{X'_j \pi} + \phi(\Delta\bar{Y}^p_{r,16-17} \times t) + \rho\bar{Y}^p_{r,17} + \varepsilon_{jrt}. \end{aligned} \tag{9}$$

The dependent variable Y^p_{jrt} in this equation is an indicator variable equal to one if voter j residing in CZ r in year t self-identifies as belonging to group $p \in$ {Democrat, Republican, Independent}. This variable is demeaned relative to its CZ-wide average in 2017, so that the CZ-level average of the outcome in other years captures a change in party identification relative to the pre-trade war base year. This setup is comparable to our employment analysis above, where CZ employment rates were measured relative to their baseline values in January 2018. In addition to the year fixed effects γ_t included in all models, estimates successively add a full set of controls including CZ-level sectoral employment shares interacted with year dummies $\lambda_{s_r,t}$, geographic region effects interacted with year dummies $\delta_{d(r),t}$, and a vector $\boldsymbol{X_j}$ of the detailed respondent characteristics that comprises dummies for gender, race (non-Hispanic white vs. other), and education (at least some college education vs. high school or less) as well as a quadratic in age. Some models control for a one-year linear pre-trend in the CZ average level of party identification ($\Delta\bar{Y}^p_{r,16-17} \times t$), consistent with the pre-trend controls in the employment analysis above. One limitation of the relatively modest sample sizes by CZ in the CCES is that we may observe substantial mean reversion of the outcome. If for instance the CCES by chance sampled a disproportionate fraction of voters who identified as Republicans in a given CZ in 2017, we would expect to see relatively fewer Republican supporters in that CZ in other years compared to the high 2017 baseline value. To account for such mean reversion, some regression models control for the 2017 CZ average of party identification

$\bar{Y}^{p}_{r,17}$. Regressions are weighted by CCES sampling weights, as above. Standard errors are clustered at the state level.

Table 6: Probability of Voters identifying as Republicans, Independents or Democrats

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Identifies as Democrat							
import tariff exposure	-44.23	-33.23	-36.25	-50.40	-57.71	-39.41	-41.50
	(15.99)	(20.06)	(20.51)	(21.03)	(29.72)	(13.40)	(13.92)
retaliatory tariff exposure	-10.30	32.55	26.23	24.57	37.60	34.84	40.20
	(19.02)	(22.09)	(21.45)	(24.11)	(27.21)	(16.69)	(17.15)
farm subsidies per capita							-2.03
							(1.47)
Panel B: Identifies as Republican							
import tariff exposure	33.77	16.77	22.14	31.75	43.25	22.96	24.02
	(16.30)	(22.30)	(23.11)	(22.87)	(30.12)	(15.31)	(15.76)
retaliatory tariff exposure	13.95	-18.89	-16.20	-6.25	-30.71	-28.14	-30.88
	(22.35)	(29.68)	(29.62)	(32.14)	(33.11)	(17.93)	(17.47)
farm subsidies per capita							1.04
							(1.44)
Panel C: Identifies as Independent							
import tariff exposure	10.46	16.46	14.10	18.66	19.62	16.91	17.76
	(9.35)	(10.03)	(11.85)	(10.97)	(13.14)	(6.95)	(6.86)
retaliatory tariff exposure	-3.65	-13.66	-10.03	-18.32	-33.31	-12.89	-15.07
	(16.33)	(23.72)	(20.86)	(21.91)	(25.87)	(10.84)	(11.48)
farm subsidies per capita							0.83
							(0.77)
year FE	✓	(✓)	(✓)	(✓)	(✓)	(✓)	(✓)
sector*year FE		✓	✓	✓	✓	✓	✓
Census division*year FE			✓	✓	✓	✓	✓
demographic controls				✓	✓	✓	✓
t*Δ CZ avg outcome 2016-17					✓	✓	✓
CZ average outcome in 2017						✓	✓

Notes: N=346,034. The inclusion of demographic controls in column (4) reduces the sample size to N=321,545. 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 46.10 (49.85) in Panel A, 36.03 (48.01) in Panel B and 17.87 (38.31) in Panel C. Tariff exposure variables and cumulative farm subsidy payments (denoted in 1,000s of 2018 dollars per working age population) are measured according to their October 2018 and 2019 values for survey years 2018 and 2019, according to the final December 2019 value for survey year 2020, and set to zero in prior years. Demographic controls include a quadratic in age, gender, race (non-Hispanic white vs Not non-Hispanic or white) and education (at least some college education vs high school or less) Regressions are weighted by $\text{pop}_{2010,CZ} * (\text{CCESweight}_i / \sum_{i=1}^{CZ} \text{CCESweight}_i)$, and standard errors are clustered at state level.

Estimates of equation (9) reported in column 7 of Table 6 indicate that voters who were more exposed to import tariffs became less likely to identify as Democrats, and more likely to identify as either Republicans or Independents. Agricultural subsidies appear to move partisan identification in the same direction, but these effects are not precisely estimated. Conversely, voters in CZs exposed to retaliatory tariffs became more likely to identify as Democrats and less likely to identify as Republicans or Independents. These models imply meaningful effects on voter partisan identification. At the sample mean, we estimate from the column 7 model that import tariffs reduced the fraction of voters identifying as Democrats by 2.7%pts while raising the fraction identifying as Republicans and Independents by 1.6%pts and 1.1%pts respectively. Retaliatory tariffs worked in the opposite direction with a more modest impact, reducing Republican and Independent identification by 0.7%pts and 0.3%pts respectively while raising Democratic identification by 1.0%pts. Combining these effects and further accounting for the estimated impact of agricultural subsidies implies that the trade war reduced the fraction of voters identifying as Democrats by 1.9%pts and raised the fraction identifying as Republican and Independents by 0.9%pts and 1.0%pts respectively.

Comparing the estimates for party identification in Table 6 with those for employment in Table 2 underscores that while the political and labor market impacts of the tariff war were *directionally* highly comparable, there is an important difference: the political impacts appear to be substantially larger. We provide further comparison of the relative magnitudes of the trade war's impact on employment and political outcomes following the results for electoral outcomes below.

6.4 Electoral outcomes

The tariff war was evidently successful in shifting voter identification away from the Democratic party. Did it affect voting? We consider the impact of the tariff war on electoral outcomes, beginning in Table 7 with the Republican two-party vote share in presidential contests, measured at the CZ level. We estimate the following model for the Republican two-party vote share RVS_{rt} in commuting zone r in presidential election year $t \in \{2012, 2016, 2020\}$:

$$\begin{aligned} RVS_{rt} - RVS_{r,16} = {} & \beta_1 IMP_{rt} + \beta_2 RET_{rt} + \beta_3 SUB_{rt} \\ & + \gamma_t + \lambda_{s_r,t} + \delta_{d(r),t} + \phi\left(\Delta RVS_{r,12-16} \times t\right) + \rho RVS_{r,16} + \varepsilon_{jrt}. \end{aligned} \tag{10}$$

As in the previous regression analysis, 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 regression controls for year main effects and, in successive specifications, sector-by-year interactions, Census division-by-year interactions, the 2016 Republican two-party vote share to absorb mean reversion, and the Republican two-party vote share in 2012–2016 interacted with a time trend to account for trends in partisan support.

The estimates in Panel A of Table 7 indicate that import tariff exposure significantly increased support for the Republican candidate. Evaluated at the mean tariff exposure in December of 2019, the column 6 estimate implies that import tariffs raised President Trump's two-party vote share in 2020 by +0.67%. Retaliatory tariffs had a modest and statistically insignificant negative effect on the Republican vote, while farm subsidies had a weakly positive effect. These results are

Table 7: Republican Two-Party Vote Share in Presidential and Congressional Elections

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Presidential Election Republican Two Party Vote Share						
import tariff exposure	1.13	11.44	9.93	11.65	9.98	10.12
	(3.71)	(6.54)	(4.14)	(5.60)	(4.85)	(4.96)
retaliatory tariff exposure	2.25	1.72	6.67	-2.34	-3.66	-4.19
	(6.72)	(4.53)	(4.41)	(5.40)	(4.38)	(4.86)
farm subsidies per capita						0.12
						(0.21)
Panel B: Congressional Election Republican Two Party Vote Share						
import tariff exposure	21.49	62.89	58.94	54.55	42.01	43.16
	(15.98)	(36.05)	(34.93)	(35.50)	(29.62)	(30.16)
retaliatory tariff exposure	-49.25	-19.57	-23.94	-32.93	-27.17	-29.68
	(17.96)	(15.46)	(15.34)	(17.99)	(16.77)	(16.66)
farm subsidies per capita						0.98
						(1.09)
year FE	✓	(✓)	(✓)	(✓)	(✓)	(✓)
sector*year		✓	✓	✓	✓	✓
Census division*year			✓	✓	✓	✓
t*Δ lagged Repub share				✓	✓	✓
Repub. TPVS 2016					✓	✓

Notes: N= 2,166 (722 CZs x 3 presidential elections 2012-2020) in Panel A and N=7,674 (1,546 CZ-congressional voting district cells x 5 congressional elections 2012-2020 minus data for North Carolina in 2020) in Panel B. The dependent variable in Panel A and B is the Republican two-party vote share, indexed to 0 in 2016. The mean (standard deviation) of the outcome variable prior to indexing in 2016 is 49.00 (14.52) in Panel A, and 49.97 (25.58) in Panel B. Tariff exposure variables and cumulative farm subsidy payments are measured at their October 2018 values for the 2018 elections and at their end-of-sample December 2019 values for the 2020 elections. Regressions are weighted by 2010 CZ population, and standard errors are clustered by states.

qualitatively consistent with the shifts of party identification in tariff-exposed CZs documented in Table 6, although the magnitude of the Republican gain is somewhat smaller here.

A final set of results studies the impact of tariffs on Congressional elections. Following the research design in Autor et al. (2020), we subdivide CZs into their constituent overlaps with congressional voting districts. Panel B of Table 7 reports estimates, applying a specification similar to equation 10.[43] The results are qualitatively consistent with the Presidential analysis. The panel B estimates indicate vote share gains for Republicans in CZs with greater import tariff exposure accompanied by smaller negative effects of retaliatory tariffs and positive effects of farm subsidies, though these effects are only marginally statistically significant at best.[44] It is noteworthy that the

[43]Different from the notation in equation 10, vote shares are measured at the level of CZ-by-district cells and the control for pre-trend refers to the period 2014-2016.

[44]In our analysis through 2020, the positive effect of import tariffs on Republican votes in Congressional elections dominates the negative effect of retaliatory tariffs, whereas the latter effect was relatively stronger in the Blanchard et al. (2024) analysis of the 2018 election. One possible reason for this difference is that the favorable effect of import tariff protection on GOP votes in Congressional elections may have become stronger as an increasing number of representatives moved from opposition to the tariffs in 2018 (Caldwell, 2018; Frecking, 2018) to support for the tariffs in 2019 (Mascaro, 2019).

impacts of tariffs on party vote shares for Congressional elections in panel B are both larger but also much less precisely estimated than the results for presidential elections in panel A of Table 7. Party vote shares often fluctuate substantially over time within Congressional districts, where a dominant party may only occasionally face a promising opposition candidate. By contrast, party vote shares for presidential elections experience less fluctuation over time, which can allow for more precise estimates.[45]

6.5 Comparing the tariff war's impacts on employment and electoral outcomes

A comparison between our results for CZ employment rates in Table 2 and for Republican electoral success in Table 7 indicates consistency in the sign patterns of the tariffs and subsidy variables: The same variables that tend to have positive (negative) impacts on employment also have positive (negative) effects for Republicans. To provide a more direct comparison of employment and electoral outcomes, panel A of Figure 5 plots the predicted employment effects of tariffs and subsidy variables against their predicted impacts for the GOP vote share in the presidential election, with CZs aggregated into 40 bins based on their x-axis values.[46] The figure indicates that the combined predicted employment effect of import tariffs, retaliatory tariffs and agricultural subsidies is small, ranging from a 0.2% loss to a 0.2% gain in the employment rate in most CZs. Conversely, predicted Republican vote share gains are more sizable, reaching values of 0.5% to 1.0% for most CZs. There is a clear positive correlation between the two outcomes which suggests that Republican gains were largest in the regions where the tariff policies had the most favorable employment impacts. A striking pattern in Figure 5 is however that local exposure to the trade war appears to have benefited the Republican party even in regions where the combined effect of tariffs and subsidies predicts no employment gain or even a modest employment loss. This pattern results from the differential quantitative impacts of import versus retaliatory tariffs on employment and voting outcomes. The coefficients in column 8 of Table 2 indicate that two equally large increments in import tariff and retaliatory tariff exposure would result in a net employment loss, since the negative effect of retaliatory tariffs outweighs the smaller positive effect of import tariffs. Conversely, the results in column 6 of Table 7 imply that a balanced increase in tariff exposure would result in a net GOP vote share gain because the positive electoral impact of tariff protection dominates the negative impact of retaliatory tariffs.

Why did a trade war that failed to achieve the goal of raising employment rates or earnings

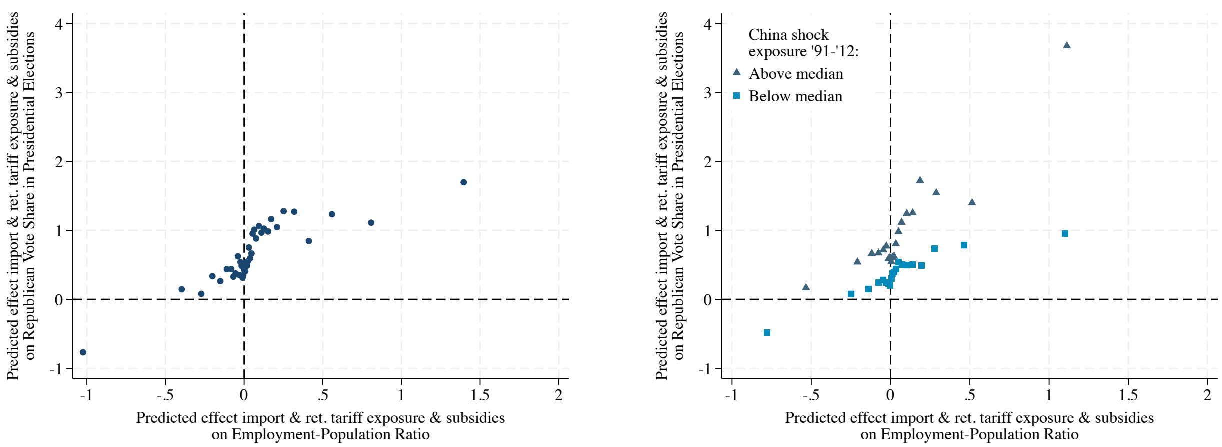
[45] As a complement to studying vote shares in congressional elections, our data structure of CZ-district-cells additionally allows estimates of the impact of the tariff war on the probability of a Republican election win. The corresponding results in Appendix Table A5 also point to imprecisely estimated Republican gains in tariff protected regions, along with a now significant positive effect of farm subsidies.

[46] The predicted combined effect of import tariff, retaliatory tariff and farm subsidy exposure on employment is based on each CZ's exposure in December 2019, scaled by the regression coefficients in column 8 of Table 2. The predicted combined effect of the trade war variables on the Republican vote share in presidential elections scales the same exposures with the regression coefficients in column 6 in panel A of Table 7.

Figure 5: Predicted Effect of Tariff Exposure on Employment-Population Ratio vs. Presidential Elections

(a) Pooled CZs

(b) CZs split by past China shock exposure



Notes: The predicted combined employment and electoral effects of import tariff, retaliatory tariff and farm subsidy exposure is based on each CZ's exposure in December 2019, scaled by the regression coefficients in column 8 of Table 2 for employment and column 6 in panel A of Table 7 for the Republican vote share in presidential elections. Panel a aggregates predicted effects for the 722 CZs to 40 bins based on CZs' predicted employment values. Panel b aggregates the same predicted effects to 20 bins for the one-half of CZs that had an above-median exposure to Chinese import competition in 1991-2012, and 20 bins for the one-half of CZs with a below-median exposure.

in tariff-protected regions nevertheless appear to benefit the party that instigated it? One explanation is that voters were misinformed about the employment impacts of the trade war. During his presidency, Donald Trump repeatedly claimed credit for job creation in manufacturing firms 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).[47] A second explanation is that the president may have garnered support from voters who were skeptical about the favorable economic consequences of tariffs, but who appreciated the president's intention to confront Chinese competition and protect US jobs. 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 tariffs of the Trump government. 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. Many Republican voters thus voiced support for the Trump tariffs despite not perceiving them as economically beneficial.[48]

[47] Our results on internet searches in section 6.1 do not suggest that residents of trade-exposed regions had a differential awareness of tariffs, and they provide no evidence for a spatially targeted electoral campaign messaging that would have led to higher search volumes in tariff-exposed regions in the run up to elections. However, when voters in all regions of the US overestimate the favorable impact of import tariffs on tariff-protected regions, then the GOP may be rewarded differentially by voters who are based in these protected regions.

[48] Among Democrats, support for a confrontational approach to China was also more widespread than the belief that

6.6 Differential impacts of the tariff war on regions affected by the 'China shock'

While we conclude that the tariff war was ineffective in raising employment and earnings in labor markets gaining protection, it is possible that the tariff measures were relatively more successful in bringing back jobs to those regions that lost employment due to trade pressures in prior decades. The strong focus of the US tariff measures on China may partly be a belated reaction to the so-called 'China shock', a period of rapidly growing import competition from China in the 1990s and 2000s that contributed to a sharp decline in US manufacturing employment during that period (Autor et al., 2013, 2016; Acemoglu et al., 2016; Pierce and Schott, 2016). CZs whose industries faced a particularly rapid rise in import competition in the 1990s and 2000s continued to suffer from depressed employment levels up to the period of the tariff war (Autor et al., 2021), and greater local exposure to the China shock also contributed to political polarization during the 2000s and raised support for Donald Trump in the in the 2016 presidential election (Autor et al., 2020).

We finally explore the differential impact of the tariff war on CZs that faced above-median versus below-median Chinese import competition in the 1991-2012 period.[49] The CZs which experienced a greater China shock in the past were nearly twice as exposed to the Trump government's import tariffs than CZs with low China shock (an average exposure of 0.075 vs. 0.039) while there was little difference in terms of retaliatory tariff exposure (0.024 vs. 0.023) and lower exposure to agricultural subsidies (0.074 vs. 0.158). In panel B of Figure 5, we plot the predicted employment and electoral effects of the tariff war separately for 20 bins of CZs with above median China shock exposure and 20 bins of CZs with below median exposure. The panel indicates that, while there is little difference in predicted employment effects among more versus less China trade shocked-exposed CZs, those with higher prior exposure to the China shock exhibit substantially larger predicted electoral gains for the Republican party (an average of 0.7% vs. 0.3% in CZs with high vs. low China shock exposure). The overall result of a sizable increase in the Republican vote share even in absence of positive employment effects is thus driven primarily by those regions that faced a large China shock in the past and whose industries received relatively large import tariff protection during the tariff war. Voters in these regions may have been particularly supportive of government action against China even as economic gains from such government policy remained elusive.

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.

[49] 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.

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. We find consistent evidence on both questions. The net effect of import tariffs, retaliatory tariffs, and farm subsidies on employment in locations exposed to the trade war was at best a wash, and it may have been mildly negative. US import tariffs had either insignificantly negative or insignificantly positive employment effects; retaliatory tariffs had a consistent and significant negative employment impact; and only a minor part of these adverse effects were offset by agricultural subsidies. Results for local earnings closely resemble those for local employment.

Conversely, the trade war appears to have been successful in strengthening support for the Republican party. Residents of tariff-protected locations voiced more support for the tariffs, became less likely to identify as Democrats and more likely to vote for President Trump. Although retaliatory tariffs were more effective in reducing *employment* than import tariffs were in boosting employment, retaliatory tariffs were less effective in reducing Republican *electoral support* than import tariffs were in boosting Republican electoral support. Voters thus appear to have responded favorably to the extension of tariff protections to local industries despite their economic cost. 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 government for its tariff policy.

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A1 Appendix Tables

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	0.017	0.053
sd	0.036	0.020	0.251
p25	0.007	0.006	0.000
p50	0.023	0.013	0.002
p75	0.043	0.021	0.016
Exposure Dec '19			
mean	0.065	0.024	0.098
sd	0.047	0.021	0.401
p25	0.039	0.014	0.001
p50	0.057	0.019	0.006
p75	0.079	0.029	0.033
Correlations			
w/imp. tariff	1.000	0.473	0.152
w/ret. tariff	0.473	1.000	0.263

Notes: The top panel measures average import tariff exposure from February 2018 to December 2019, exposure to retaliatory tariffs from April 2018 to December 2019, and cumulative farm subsidy payments in $1000s of 2018 dollars per working age population from September 2018 to December 2019, for 722 commuting zones. The second panel measures the exposure that a CZ had in December 2019 which corresponds to maximum exposure for most CZ. Statistics are weighted by average commuting zone employment in 2012.

Table A2: Impact of Tariff Exposure on CZ Employment: Alternative Pre-Trend Controls

	control for 2016-2017 pretrend			
	(1)	(2)	(3)	(4)
import tariff exposure	-2.414	2.235	2.013	2.358
	(1.468)	(1.277)	(1.258)	(1.227)
retaliatory tariff exposure	-2.818	-7.654	-7.617	-8.261
	(1.902)	(2.374)	(2.238)	(2.159)
farm subsidies per capita				0.305
				(0.111)
t * (monthly Δ emp/pop in 2016-2017)	0.769	0.784	0.766	0.768
	(0.078)	(0.070)	(0.062)	(0.060)
year-month FE	✓	(✓)	(✓)	(✓)
sector*year-month FE		✓	✓	✓
Census division*year-month FE			✓	✓

Notes: N=34,656 (722 commuting zones x 48 months: Jan 2016 – Dec 2019). The dependent variable for all regression models is the seasonally-adjusted employment-to-population ratio, which is indexed to 0 in 2018m1 in each commuting zone. The mean (standard deviation) of the employment-to-population ratio prior to indexing is 66.3 (7.8) percentage points in 2018m1. All regression models control for the monthly change in employment-to-population from 2016m1 to 2018m1, interacted with a linear time trend (the count of months since 2018m1). Farm subsidies are denoted in units of 1,000s of 2018 dollars per working age population. Regressions in columns 2 to 4 interact time fixed effects with a commuting zone's sectoral employment shares (agriculture and mining, manufacturing, non-goods sector) in 2012, while columns 3 to 4 also interact time fixed effects with indicators for the 9 geographic Census divisions. Regressions are weighted by commuting zone employment in 2012, and standard errors are clustered by state.

Table A3: Impact of Tariff Exposure on CZ Employment: Long term difference 2021m12 to 2019m12

	control for 2017 pretrend			
	(1)	(2)	(3)	(4)
import tariff exposure	-1.043	2.952	1.215	1.621
	(1.734)	(2.018)	(1.920)	(2.003)
retaliatory tariff exposure	-0.400	2.949	3.454	2.175
	(3.023)	(3.381)	(3.021)	(3.106)
farm subsidies per capita				0.330
				(0.103)
t * (monthly Δ emp/pop in 2017)	-0.105	-0.100	-0.171	-0.174
	(0.077)	(0.066)	(0.056)	(0.057)
Δ emp/pop by sector Mar to Apr 2020	✓	✓	✓	✓
sector share FE		✓	✓	✓
Census division FE			✓	✓

Notes: N=722 (722 commuting zones). The dependent variable for all regression models is the change in the employment-to-population ratio from December 2019 to December 2021. The average change is -1.29 with a standard deviation of 1.48. CZ exposure to tariffs and cumulative farm subsidy payments in 1,000s of 2018 dollars per working age population are measured at their 2019m12 values. All regression models control for the monthly change in employment-to-population from 2017m1 to 2018m1, scaled by 24 months. All regression models further include controls for a CZ's March to April 2020 employment-to-population ratio change by sector (agriculture and mining, manufacturing, non-goods sector). Regressions in columns 2 to 4 add controls for a commuting zone's sectoral employment shares (agriculture and mining, manufacturing, non-goods sector) in 2012, while columns 3 and 4 also include indicators for the 9 geographic Census divisions. Regressions are weighted by commuting zone employment in 2012, and standard errors are clustered by state.

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

	no 2016 Repub TPVS control				control for 2016 Repub TPVS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: support for Steel and Aluminum import tariff								
import tariff exposure	29.85	32.11	40.14	43.12	36.28	33.61	42.07	44.55
	(14.02)	(13.49)	(12.99)	(12.63)	(13.10)	(13.80)	(13.42)	(13.16)
retaliatory tariff exposure	-6.23	-5.55	-25.78	-35.75	-7.09	-4.09	-22.19	-30.89
	(30.21)	(31.44)	(30.71)	(32.81)	(30.95)	(31.38)	(31.21)	(32.93)
farm subsidies per capita				2.91				2.50
				(2.00)				(1.99)
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	-7.74	2.62	7.62	10.73	7.53	9.57	11.20	13.43
	(23.05)	(26.64)	(22.86)	(23.66)	(25.35)	(26.83)	(22.37)	(22.93)
retaliatory tariff exposure	-60.31	-66.24	-59.49	-70.05	-62.42	-59.49	-52.87	-60.83
	(24.49)	(21.87)	(21.72)	(23.90)	(21.50)	(22.25)	(22.08)	(23.57)
farm subsidies per capita				3.20				2.37
				(1.58)				(1.51)
2016 Repub. TPVS					0.35	0.34	0.27	0.27
					(0.04)	(0.05)	(0.06)	(0.06)
sector shares	✓	✓	✓	✓	✓	✓	✓	✓
Census division FE		✓	✓	✓		✓	✓	✓
demographic controls			✓	✓			✓	✓

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 and Panel B asks respondents on the support of 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. Trade tariff exposure variables are equal to CZ exposure in October 2019. Farm subsidies per working age population are in 1000 of US 2018 dollars and equal to the cumulative amount of farm subsidies paid as of October 2019. Demographic controls include quadratic in age, gender, race (non-Hispanic white vs Not non-Hispanic or white) and education (at least some college education vs high school or less). Regressions are weighted by $pop_{2010,CZ} * (CCESweight_i / \sum_{i=1}^{CZ} CCESweight_i)$, and standard errors are clustered at state level.

Table A5: Congressional Election Republican Victory Indicator

	(1)	(2)	(3)	(4)	(5)	(6)
import tariff exposure	39.34	29.39	15.88	12.75	7.19	15.29
	(21.10)	(26.88)	(23.73)	(22.56)	(21.24)	(21.34)
retaliatory tariff exposure	76.86	12.46	6.52	0.10	2.65	-15.02
	(31.15)	(24.25)	(21.16)	(22.73)	(23.31)	(25.06)
farm subsidies per capita						6.90
						(3.26)
year FE	✓	(✓)	(✓)	(✓)	(✓)	(✓)
sector*year		✓	✓	✓	✓	✓
Census division*year			✓	✓	✓	✓
t*Δ lagged Repub share				✓	✓	✓
Repub. TPVS 2016					✓	✓

Notes: N=7,674 (1,546 CZ-congressional voting district cells x 5 congressional elections 2012-2020 minus data for North Carolina in 2020). The dependent variable is 100 times an indicator for a Republican victory in a congressional district, indexed to 0 in 2016. The mean (standard deviation) of the outcome variable prior to indexing in 2016 is 55.47 (49.72). Tariff exposure variables and cumulative farm subsidy payments are measured at their October 2018 values for the 2018 elections and at their end-of-sample December 2019 values for the 2020 elections. Regressions are weighted by 2010 CZ population, and standard errors are clustered by states.

A2 Data Appendix: QCEW

The Quarterly Census of Employment and Wages (QCEW) provides information on monthly employment counts, quarterly establishment counts, and quarterly wage bills 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 A6 shows that this coverage is broader than that of the Census Bureau's County Business Patterns (CBP) data, most notably 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.

Table A6: Data Sources for Regional US Industry Employment: QCEW vs. CBP

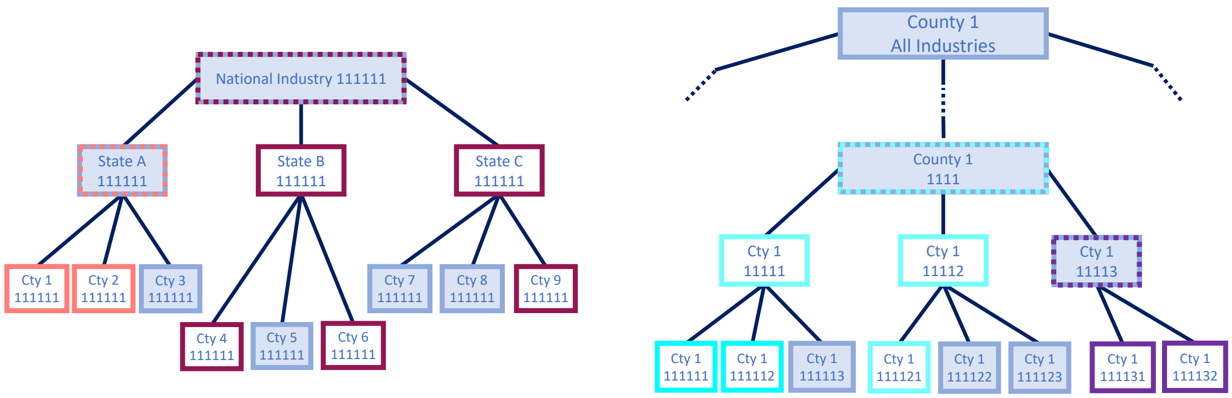
	Quarterly Census of Emp. and Wages (QCEW)	**County Business Patterns (CBP)**
	Frequency and Coverage of Employment Data	
Frequency	monthly (published quarterly)	annual
Sectors Covered	private non-farm private farm public sector	private non-farm
Total US Employment (2010)	126'228'228	111'970'095
	Disclosed 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.

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

Figure A1: Illustration of QCEW Imputation Algorithm

(a) Industry dimension (b) Geographic dimension



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.

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 reported at the most detailed level of county $\times$ 6-digit industry (as is the case for CBP data, see Table A6), we impute employment for the remaining county-industry cells.[50]

Key to our imputation algorithm is the observation that whenever employment counts are suppressed at the level of a detailed county $\times$ 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.[51] 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 $\times$ 4-digit industry cell (Table A6). Our fixed-point algorithm distributes the known employment counts from more aggregate geography and industry levels to detailed county $\times$ 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

[50] 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 $\times$ industry is often suppressed due to low numbers of distinct public employers. We treat public sector employment as a separate industry with no trade exposure.

[51] 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.

county × 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).[52] 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 A1 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 A1, one subtracts the disclosed employment for *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 A1 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 × 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

[52] 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.

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 and subsequent 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 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

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

$$\text{where } x_{j,t} = est_{j,t} * 0.5\left(\frac{max(1, emp^0_{j,t-1})}{est_{j,t-1}} + \frac{max(1, emp^0_{j,t+1})}{est_{j,t+1}}\right)$$

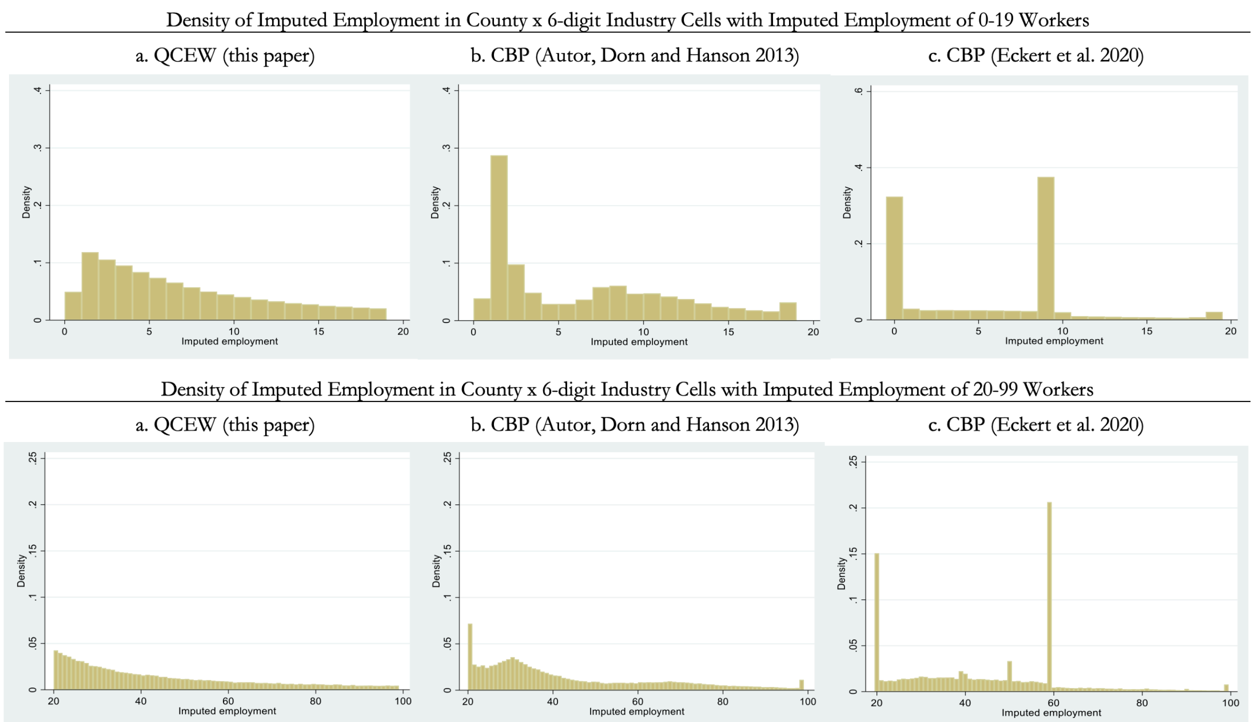
This initialization allocates to cell j a greater fraction of the distributable employment of cluster J not only when j accounts for a large fraction of all establishments in the cluster, but also when the initial iteration of the algorithm found that establishments of cell j had an unusually large employment size in the prior and subsequent month.[53]

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.

[53] The maximum operators in the formula for $emp^{1\text{-}init}_{j,t}$ ascertain that the initialization always assumes an establishment size of at least one employee. If cell j had zero establishments in either the previous or subsequent month, then $x_{j,t}$ is defined as $est_{j,t} * (\frac{max(1, emp^0_{j,t+1})}{est_{j,t+1}})$ or $est_{j,t} * (\frac{max(1, emp^0_{j,t-1})}{est_{j,t-1}})$, respectively.

To assess the plausibility of our imputation results, we assess two properties of the imputed data. First, in Figure A2, 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 to imputed cell employment similarly displays a monotone right-skewed distribution. The left-hand panels of Figure A2 indicate that this distributional property is indeed present in our impuation 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.

Figure A2: 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.

Second, we investigate in Table A7 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 A7 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 $\times$ 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 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 A7 indicate that county $\times$ 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 some inconsistencies. 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 0 to 19 employees or 20 to 99 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 A7 indicates that imputed employment counts for county $\times$ 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. We conclude that our new county $\times$ 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 A6). 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.

Table A7: 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.

A3 Industry-Level Analysis

Industries are directly exposed to U.S. import tariffs and foreign retaliatory tariffs according to equations (3) and (4). 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 A8: Industry Exposure to Tariffs

	A. Tradable industries		B. All industries					
	import tariff exposure	retaliatory tariff exposure	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	0.162	0.030	0.016	0.057	0.037	0.058	0.037
sd	0.654	0.445	0.226	0.149	0.116	0.093	0.131	0.084
p25	0.000	0.002	0.000	0.000	0.003	0.000	0.002	0.000
p50	0.030	0.033	0.000	0.000	0.025	0.005	0.024	0.006
p75	0.318	0.165	0.000	0.000	0.056	0.033	0.059	0.045
Exposure Dec '19								
mean	0.636	0.233	0.064	0.023	0.099	0.069	0.077	0.048
sd	0.979	0.522	0.364	0.179	0.135	0.139	0.157	0.100
p25	0.030	0.010	0.000	0.000	0.027	0.000	0.015	0.000
p50	0.247	0.074	0.000	0.000	0.065	0.015	0.036	0.017
p75	0.857	0.297	0.000	0.000	0.095	0.082	0.073	0.059
Correlations								
w/imp. tariff	1.000	0.145	1.000	0.262	0.375	0.395	0.163	0.169
w/ret. tariff	0.145	1.000	0.262	1.000	0.182	0.263	0.215	0.266

Notes: Panel A) reports direct tariff exposures for the 373 tradable industries. Panel B) shows direct exposures and exposures through input-output linkages for all 917 tradable and non-tradable industries. The top panel measures average import tariff exposure from February 2018 to December 2019 and exposure to retaliatory tariffs from April 2018 to December 2019. The second panel measures the exposure that an industry had in December 2019, which corresponds to maximum exposure for most industries. Statistics are weighted by average industry employment in 2012.

Table A8 indicates industry-level exposure to import and retaliatory tariffs. By construction, the mean tariff exposure among employment-weighted industries corresponds closely to the average tariff exposure that Table A1 reports for CZs.[54] Since the 373 goods-producing industries in manufacturing, agriculture and mining account for less than a quarter of U.S. employment, the 75th percentiles of employment-weighted industry-level import and retaliatory tariff exposures are zero in 2018 and 2019. By contrast, a majority of U.S. employment is in industries that are indirectly exposed to import and retaliatory tariffs based on their suppliers, and, to a lesser degree, on their customers.

We first investigate the impact of direct exposure to import and retaliatory tariffs on monthly trade flows. To this end, we fit the specification

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

where X_{it} is either an import penetration ratio or an export-to-shipment ratio for U.S. industry i in year-month t, which we relate to an industry's direct exposure to import tariffs IMP_{it} and retaliatory tariffs EXP_{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 A9 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 A9 with the average industry-level tariff exposure in December 2019 from Table A8, 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 -4.4% decrease in industry-level import penetration due to US import tariffs. An analogous exercise based on column 4 of Table A9 implies a -2.9% decrease in the export-to-shipment ratio due to retaliatory tariffs.

The subsequent panels B to E of Table A9 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 C and D, 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.

[54]The only minor difference is that employment in Alaska and Hawaii is excluded in the CZ sample.

Table A9: 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	-1.551	-1.637	-2.050	-0.457	-0.335	-0.423
	(0.406)	(0.417)	(0.547)	(0.238)	(0.214)	(0.265)
retaliatory tariff exposure	-1.240	-1.116	-1.319	-2.005	-2.172	-2.364
	(0.759)	(0.768)	(0.913)	(0.670)	(0.722)	(0.821)
Panel B: Monthly Trade with China						
import tariff exposure	-2.099	-2.083	-2.125	0.177	0.124	0.088
	(0.609)	(0.622)	(0.647)	(0.074)	(0.070)	(0.069)
retaliatory tariff exposure	-0.220	-0.235	-0.273	-0.912	-0.827	-0.804
	(0.440)	(0.465)	(0.498)	(0.167)	(0.170)	(0.199)
Panel C: Monthly Trade with other Trade War Countries						
import tariff exposure	-0.221	-0.249	-0.311	-0.437	-0.348	-0.386
	(0.186)	(0.208)	(0.243)	(0.156)	(0.145)	(0.172)
retaliatory tariff exposure	-0.480	-0.440	-0.407	-0.637	-0.761	-0.845
	(0.315)	(0.310)	(0.360)	(0.473)	(0.496)	(0.580)
Panel D: Monthly Trade with other low-wage Asia						
import tariff exposure	0.396	0.377	0.351	-0.085	-0.078	-0.071
	(0.134)	(0.138)	(0.151)	(0.062)	(0.060)	(0.050)
retaliatory tariff exposure	-0.088	-0.066	-0.083	-0.008	-0.019	-0.037
	(0.157)	(0.154)	(0.178)	(0.056)	(0.059)	(0.058)
Panel E: Monthly Trade with Rest of the World						
import tariff exposure	0.373	0.317	0.202	-0.112	-0.034	-0.051
	(0.224)	(0.226)	(0.211)	(0.120)	(0.109)	(0.134)
retaliatory tariff exposure	-0.452	-0.374	-0.425	-0.448	-0.566	-0.646
	(0.250)	(0.259)	(0.284)	(0.294)	(0.333)	(0.368)
year-month FE	✓	(✓)	(✓)	✓	(✓)	(✓)
Sector*year-month FE		✓	✓		✓	✓
t * (monthly Δ dep. var. in 2017)			✓			✓

Notes: N=17,904 (373 tradable industries x 48 months: Jan 2016 – Dec 2019). The dependent variable is the seasonally-adjusted monthly import penetration ratio (columns (1) to (3)) or export-to-shipment ratio (columns (4)-(6)), which is indexed to 0 in 2018m1 in each industry. Panel A) includes total imports or exports in the numerator of the ratios. Panel B) considers only imports or exports from China. Panel C) includes trade flows with other trade war countries (Canada, EU, India, Mexico, Russia, Turkey, and the UK). Panel D) focuses on trade with low-wage Asian economies (Bangladesh, Indonesia, Malaysia, Philippines, Thailand, and Vietnam). Panel E) includes trade with the rest of the world. The mean (standard deviation) of the import penetration ratio in 2018m1 prior to indexing is 26.83 (26.35) for Panel A), 6.85 (11.98) for Panel B), 12.27 (13.17) for Panel C), 1.86 (5.12) for Panel D) and 5.85 (8.29) for Panel E). The denominator is computed using 2012 trade flows and production. The mean (standard deviation) of the export-to-shipment ratio in 2018m1 prior to indexing is 18.96 (19.93) for Panel A), 1.31 (2.51) for Panel B), 11.40 (11.81) for Panel C), 0.65 (2.05) for Panel D) and 5.60 (7.86) for Panel E). All regressions include year-month fixed effects. 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 2017m1 to 2018m1, interacted with a linear time trend (the count of months since 2018m1). Regressions are weighted by industry employment in 2012, and standard errors are clustered by industry.

We next study the impact of tariff exposure on employment in national 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 3, 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.[55] 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

$$\begin{aligned} \ln E_{it} - \ln E_{iJan2018} = {} & \beta_1 IMP_{it} + \beta_2 RET_{it} \\ & + \hat{\mathbf{x}}_{\mathbf{it}}^{\mathbf{IO}'} \boldsymbol{\beta_3} + \gamma_t + \lambda_{s(i),t} + \phi(\bar{\Delta} \ln E_{i,2017} \times t) + \varepsilon_{it}, \end{aligned} \tag{A4}$$

where the dependent variable is log employment in 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 $\hat{\mathbf{x}}_{\mathbf{it}}^{\mathbf{IO}}$ 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 $\lambda_{s(i),t}$, and a linear one-year pre-trend of the outcome variable ($\bar{\Delta} \ln E_{i,2017} \times t$).

Column 1 of Table A10 reports a bare bones version of equation (A4) with a specification that includes year-by-month main effects but excludes sector-by-month interactions and input-output linkages. This model detects small, negative, and statistically insignificant effects of both import and retaliatory tariffs on employment in exposed industries. When sector-by-month interactions are additionally included in column 2, the estimated negative employment effects of both import and retaliatory tariffs increase. In the third column, where all four input-output terms are included, we estimate that both import and retaliatory tariffs significantly reduce employment in targeted industries. Moreover, and somewhat puzzlingly, we estimate that tariff protections applied to an industry's suppliers predict an increase in that industry's employment.[56]

[55]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.

[56]These tariffs might be expected to reduce competitive pressure in the supplying sector, thus raising input costs and depressing employment in customer industries.

Table A10: Impact of Tariff Exposure on Industry Employment

	no pretrend control			control for 2017 pretrend			Agrar, mining, mfg sectors		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
import tariff exposure	-0.245	-0.638	-0.895	0.084	-0.272	-0.397	0.028	-0.260	-0.273
	(0.308)	(0.372)	(0.360)	(0.268)	(0.299)	(0.325)	(0.288)	(0.296)	(0.294)
retaliatory tariff exposure	-0.674	-1.082	-0.979	-0.956	-0.910	-1.098	-1.111	-0.905	-0.927
	(0.441)	(0.448)	(0.445)	(0.388)	(0.335)	(0.398)	(0.367)	(0.339)	(0.342)
supplier import exposure			5.257			0.761			-0.156
			(2.137)			(1.031)			(0.515)
customer import exposure			0.754			-0.051			-0.225
			(1.361)			(1.134)			(0.804)
supplier retaliatory exposure			0.390			-0.656			-0.398
			(0.939)			(0.829)			(0.649)
customer retaliatory exposure			-1.545			3.512			0.746
			(2.904)			(3.269)			(0.996)
t * (monthly Δ ln(emp) in 2017)				0.536	0.538	0.540	0.525	0.556	0.557
				(0.058)	(0.060)	(0.057)	(0.033)	(0.033)	(0.033)
year-month FE	✓	(✓)	(✓)	✓	(✓)	(✓)	✓	(✓)	(✓)
sector*year-month FE		✓	✓		✓	✓		✓	✓

Notes: N=44,016 (917 industries x 48 months: Jan 2016 – Dec 2019) in columns (1)-(6), N=18,480 (385 industries in agriculture, mining and manufacturing sectors x 48 months: Jan 2016 – Dec 2019) in columns (7)-(9). The dependent variable for all regression models is seasonally-adjusted log employment, which is indexed to 0 in 2018m1 in each industry. The mean (standard deviation) of log employment prior to indexing is 1373.4 (207.7) log points in 2018m1. Regressions in columns 4 to 9 control for the monthly change in log employment from 2017m1 to 2018m1, interacted with a linear time trend (the count of months since 2018m1). The regressions in columns 2 to 3 and 5 to 6 and 8 to 9 interact time fixed effects with indicators for three economic sectors (agriculture and mining, manufacturing, non-goods sector). Regressions are weighted by industry employment in 2012, and standard errors are clustered by industry.

Cognizant of the concern that inference on high-frequency monthly data may be particularly vulnerable to confounding trends, the next three columns of Table A10 control directly for confounding trends by including a linear time trend in industry-level employment growth between January 2017 and 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, consistent with the CZ-level analysis in Tables 2 and 3. Import tariffs have elusive employment effects on targeted sectors, ranging from insignificantly negative to insignificantly positive, dependent upon covariates. The employment effect of industries' indirect tariff exposure via suppliers and customers are all measured with little precision. The final panel of Table A10 limits the sample to the 385 industries (out of a total of 917 industries) of the tradeable sectors agriculture, mining and manufacturing. The estimates are similar to the full industry sample, except that the employment effect of supplier industry exposure is now insignificantly negative, and thus has the same sign as in the Flaaen and Pierce (2021) analysis of broad manufacturing industries.