Help for the Heartland? The Labor Market 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, analyzing both short-run outcomes during the trade war and longer-run effects through 2023/24. US import tariffs did not significantly affect employment or earnings during or after the trade war. Retaliatory tariffs initially reduced employment—especially in agriculture—but harms were partially mitigated by agricultural subsidies and faded over time. Politically, the trade war benefited the Republican party. Residents in tariff-exposed areas showed increased support for tariffs, became more likely to identify as Republicans, and were more likely to vote for Donald Trump in 2020 and 2024. The pattern of tariff-induced Republican gains and limited local employment effects was especially pronounced in regions with greater prior exposure to Chinese import competition, where voters may have valued protectionist action even in the absence of tangible economic benefits.

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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. By tracing outcomes through 2023-24, we are able to assess not only the short-run implications of tariffs, but also their longer-run effects.

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), which also provides quarterly earnings data for the same regions. The QCEW data cover more than 95% of US employment and are well-suited for high frequency and high resolution spatial analysis of policies. We combine this data with detailed information on US import tariffs and foreign retaliatory tariffs, drawing both on widely used 2018 tariff measures as well as newly collected data on the 2019 tariff increases 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 earnings per capita based on QCEW data through 2023, and the later combine vote counts for presidential and congressional elections from Leip's Electoral Atlas with

[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.

survey evidence 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 employment rates or earnings in regions with newly-protected sectors during the trade war. Although favorable labor market effects of import tariffs could in principle materialize only with a lag—for example, if firms require time to expand manufacturing capacity—we also find no indication of deferred employment or earnings gains through 2023. Foreign retaliatory tariffs by contrast had modest but significantly estimated negative employment impacts during the trade war, and these harms were only partly mitigated by compensatory subsidies. The adverse impacts of the retaliatory tariffs however fade over the longer time horizon.

To provide context for the limited impact of US import tariffs on employment and earnings, 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. The local labor market analysis also indicates that import tariff protection did not produce gains in manufacturing employment especially in those industries that also experienced rising input costs due to the tariffs.

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, became more likely to identify as Republicans, more likely to vote to elect President Donald Trump in the 2020 and 2024 elections, and weakly more likely to elect Republicans to Congress. Foreign retaliatory tariffs only modestly reduced that electoral support.

In combination, these results indicate that the trade war failed to produce employment and earnings growth for US workers, but nevertheless generated electoral gains for Republicans in tariff-protected regions. While it is conceivable that residents of such regions misperceived the local economic benefits from tariffs, we do not find evidence for greater awareness of tariffs or greater self-reported income gains in these locations. Instead, an alternative explanation is that Republicans obtained support from voters who were skeptical about the beneficial economic effects of tariffs but appreciated the intention to confront foreign competition and protect US jobs. Consistent with that interpretation, Republican gains were concentrated in regions that had suffered from a rapid growth of Chinese import competition in the 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 tariffs' economic impacts, and documents changes in US trade volumes and goods prices that imply modest reductions in aggregate US welfare (Amiti et al., 2019, 2020a, 2021; Carter and Steinbach, 2020; Cavallo et al., 2021; Handley et al., 2020; Fajgelbaum et al., 2020; Flaaen et al., 2021; Feenstra and Hong, 2024). 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. Several papers evaluate the impact of the first waves of tariffs implemented in early phases of the trade war on employment through late 2018 or mid-2019. Flaaen and Pierce (2021) leveraged data from the Current Employment Statistics, a survey designed to produce very recent employment estimates, to provide the first results on tariffs' short-run employment effects across aggregate US manufacturing industries. They find small employment gains in industries that received impart tariff protection and larger negative effects in those facing retaliatory tariffs and rising input costs. Javorcik et al. (2022) address the related question of whether tariffs had an effect on the number of online job ads that were posted in tariff-exposed local labor markets, while Waugh (2019) and Benguria and Saffie (2020) provide results on local employment using aggregate versions of the QCEW data. These papers find a modest short-term increase in job creation due to US import tariffs and a more sizable reduction in jobs due to retaliatory tariffs. However, this previous literature (and the initial versions of the present paper) caution that the labor market impacts of tariffs may unfold only over a longer horizon, as firms need time both to recognize that the new tariff regime is permanent and to implement the necessary adjustments in production structures. We thus refine and extend prior research by providing the first longer-run analysis of the trade war's impact on local employment that spans not only the full period of the 2018-2019 trade war-including a substantial expansion of tariffs in the fall of 2019 that was not considered in the prior research-but also the subsequent years until 2023 in which longer-term effects of the tariffs could unfold. We complement the employment analysis by studying changes in earnings per working-age adult to assess whether local economic effect of the trade war emerge in earnings rather than employment levels.

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] Prior work on the short-term electoral effect of the trade war reaches uneven conclusions. Kim and Margalit (2021) and Blanchard et al. (2024) find that the early phase of the trade war caused Republican losses in the 2018 midterms, as a small gain in vote shares in regions experiencing import tariff protection was more than offset by declining vote shares in regions whose industries faced retaliatory tariffs. Conversely, Lake and Nie (2022) find that it generated Republican gains in the 2020 presidential election, driven by higher

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

electoral support in regions exposed to import tariffs.[3] We advance this work in three dimensions. First, we provide a longer-term perspective on the trade war's electoral consequences by studying congressional elections until 2020, prior to decennial redistricting, and presidential elections through 2024. The latter results allow to assess whether the trade policies enacted by his administration in 2018-2019 helped Donald Trump to return to the presidency several years later. Second, 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. Third, we combine the electoral analysis with survey evidence that gauges support for parties, support for tariff policies, and perceived economic gains in the locations most exposed to these policies. This complementary evidence helps to further discipline the interpretation of the electoral results.

Our analysis also provides several advances in data collection and measurement. We develop a novel fixed-point algorithm to address cases of data suppression in the QCEW data, and construct a monthly panel for employment by commuting zone and 6-digit industry. Relative to the County Business Patterns (CBP) data, which has been used to allocate trade and tariff exposure to local labor markets in most of the prior literature, the QCEW data has a broader coverage of sectors, a higher frequency, and less onerous data suppression in recent years. We derive measures of local tariff exposure from the canonical Eaton and Kortum (2002) gravity model of trade, and thus obtain theoretically grounded treatment variables that depend both on the magnitude of tariff hikes (unlike the simpler measures used in Kim and Margalit (2021) or Choi and Lim (2023)) and initial trade patterns (different from Waugh (2019), Benguria and Saffie (2020) or Lake and Nie (2022)). We quantify these tariff exposures by combining the 2018 tariff data that was widely studied in prior literature with newly collected data on the tariffs the US, China and other countries imposed through the end of 2019, before the trade war reached a stalemate. We complement these data with agricultural subsidy payments that the government provided to farmers in compensation for presumed losses due to foreign retaliatory tariffs. Different from prior literature, we are able to use data on actual local payments rather than relying on imputations based on local agricultural specialization. The full coverage of the 2018-2019 tariffs and subsidies 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

[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.

outcomes: awareness of tariff policies, support for tariff policies, identification with political parties, and electoral outcomes in presidential and congressional races. Section 8 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).[4] 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.[5] 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%.

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 from fall 2018 to early 2020 distributed more than $23 billion to farmers. Most of the subsidies went to producers of grains and oilseeds, whose compensatory payments were computed as the product of farming acreage times a county-specific rate that was based on the county's crop mix. The program led to highly uneven compensation to producers of the same crop in different parts of the country (United States Government Accountability Office, 2021).

[4] 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).

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

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. For the next five years, covering the Covid-19 pandemic and the presidency of Joseph Biden, the average tariffs that the US and China apply to each other's goods remained nearly unchanged at an average rate of about 20 percent (Bown, 2025). The return of Donald Trump to the presidency in February 2025, which lies outside our period of analysis, subsequently brought a drastic further increase of tariffs imposed on US-China trade, and new US tariffs for imports from other countries.

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. 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). We also draw on the QCEW to measure quarterly wage bills and average earnings per worker by CZ.

An important advantage of QCEW over alternative industry-by-county data from the County Business Patterns (CBP), which Autor et al. (2013) and many other studies 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 sizable foreign retaliatory tariffs on US agricultural crops to the locations that produce these crops.[6] It also provides a higher frequency of measurement compared to the annual CBP data.

The QCEW nearly always discloses a country's total monthly employment and total quarterly wage bill, which we use to construct the main labor market outcome variables in our analysis.[7] We additionally use the detailed industry structure of CZs prior to the trade war to measure regional exposure to industry-level tariffs. The QCEW discloses about 75 percent of private sector employment at the level of the most detailed NAICS six-digit industry-county cells (Table A10), while the remainder of the employment counts is disclosed at the level of more aggregate industry and/or geography units. These statistics are similar to the CBP data prior to 2017, while CBP uses more restrictive data suppression in more recent years. The QCEW always indicates the

[6] 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. Different from the CBP, the QCEW also includes the quantitatively important employment segment of the public sector.

[7] The analysis of quarterly earnings per working age adult omits 0.03% of the CZ-quarter cells for which earnings are missing.

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 A3. 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.[8] 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 other studies of local tariff impacts sometimes rely on just 29 three-digit industries (e.g., Waugh (2019), Benguria and Saffie (2020), Lake and Nie (2022)).[9]

Trade volumes and manufacturing production. Monthly trade data are from the US Census Bureau's Foreign Trade Statistics.[10] 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).[11] 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, wage bills, 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), manufacturing prices from the Bureau of Labor Statistics' Producer Price Index (BLS, 2022), as well as annual sales data for agriculture and mining sectors from the BEA Industry Economic Accounts (BEA, 2024).[12]

Tariff exposure. We measure US import tariffs by country and HS10 product and foreign tariffs on

[8] 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. For instance, Kim and Margalit (2021) use the imputation of Autor et al. (2013) and Javorcik et al. (2022) use the imputation of Eckert et al. (2020) to obtain employment by detailed industry and location. These techniques however can only be applied through 2016, after which the CBP started to suppress critical information. Appendix A3 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.

[9] 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.

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

[11] 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).

[12] 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 and wage bills, it does not indicate expenditures for contract 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.

US exports by country and HS6 product.[13] Pre-trade-war tariffs and US and retaliatory increases in tariffs through early 2019 are from Fajgelbaum et al. (2020). We extend these widely used data to December 2019, and thus incorporate an additional time period in which US tariffs on imports from China nearly doubled.[14] We also implement a host of data refinements to aggregate the product level tariff panel to NAICS 6-digit codes.[15] 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.[16]

Agricultural subsidies: The US Market Facilitation Program (MFP) paid farmers $5.3 billion in late 2018, $10.4 billion in 2019 and $3.8 billion in early 2020 as compensation for adverse effects of foreign retaliatory tariffs (EWG, 2022).[17] 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 MFP payments by county from the more recent EWG Farm Subsidy Database (EWG, 2022), which we normalize to monthly payments per working age population using 2017 population data from SEER (2022). MFP payments are reported by calendar year, but can approximately be allocated to the months of September 2018 to May 2019 (MFP-2018 program) and August 2019 to February 2020 (MFP-2019 program). We derive monthly payments for each month and CZ, and then compute cumulative MFP payments scaled by 2017 CZ population.[18] Due to its less precise

[13] 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.

[14] 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.

[15] 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.

[16] 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 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).

[17] The MFP-2018 program launched in fall 2018 paid subsidies based on commodity-specific payment rates per amount of eligible crop or milk produced or live hogs owned. For MFP-2019 launched in fall 2019, the list of eligible crops was expanded, while payments for non-specialty crops were set using county-specific per-acre rates ranging from $15 to $150 (Schnepf, 2019).

[18] Funds from the $8.6bn MFP-2018 scheme were disbursed on a rolling basis starting in September 2018 and were largely complete by May 2021 (Congressional Research Service, 2019a). Total payments made in 2018 and 2019 amounted to $5.3bn and $14.2bn, respectively, which implies that 23% of the payments in year 2019 came from the

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 through 2024, and congressional elections for CZ-by-congressional district cells through 2020 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 use the CCES to measure respondents' party identification, support for tariffs, and self-reported growth in household income across various years. We also document state-level internet search patterns for tariffs sourced from Google Trends.[19]

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

MFP-2018 scheme (=($8.6bn-$5.3bn)/$14.2bn). We distribute each CZ's 2018 payments equally across the months September to December, and assign 4.6% of total payments in 2019 (one fifth of 23%) to each month from January to May. Funds of the MFP-2019 scheme were paid out in three tranches starting in August 2019, November 2019, and February 2020, with the first tranche being about twice as large as the other two (Congressional Research Service, 2019b). For each CZ, we allocate 51.3% of the 2019 payments (two thirds of the 77% of 2019 payments that were due to MFP-2019) equally to each month from August to October, and 25.6% of the 2019 payments (one third of 77%) to November, consistent with government reports which state that MFP-2019 payments amounted to $5.9bn by mid-October (US Department of Agriculture, 2019) and to $10.2bn by late November (Congressional Research Service, 2019b). We assume that the third tranche of MFP-2019 was paid out in full in February 2020 (United States Government Accountability Office, 2021).

[19] 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.

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 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.[20]

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.[21] 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

[20] 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.

[21] 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.

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.[22]

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.[23] 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.

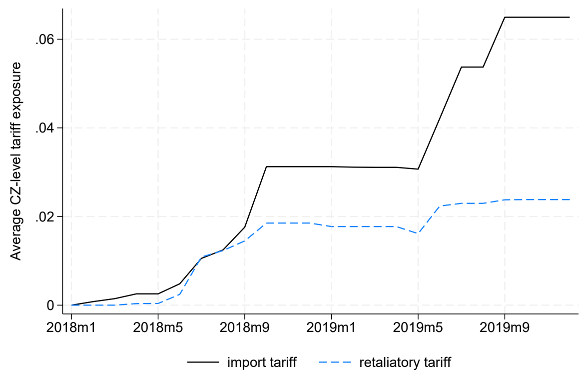
Panel A of the figure shows that tariff hikes commenced in February 2018 and accelerated in the summer and fall of the same year. While prior literature often considers only these 2018 tariffs, our analysis also covers another major escalation of tariffs during the second half of 2019. The mean effective 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 A7). 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

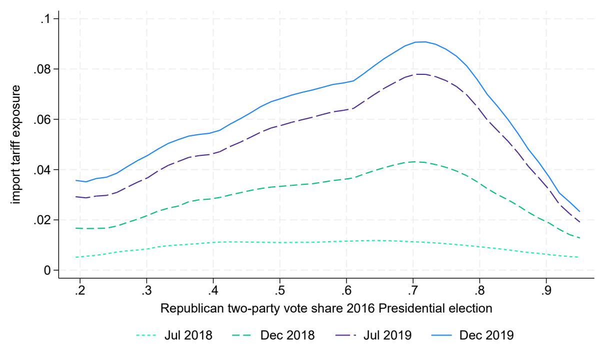
[22]For simplicity, we do not scale IMP_{it} and RET_{it} with the constant trade cost elasticity θ as in equation (1).
[23]The resulting exposure of CZ r to import and retaliatory tariffs are thus $\sum_i \frac{e_{ir}}{e_r} IMP_{it}$ and $\sum_i \frac{e_{ir}}{e_r} RET_{it}$.

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

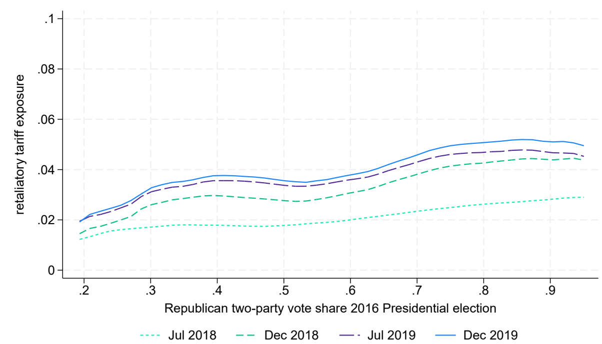
(a) CZ Import & Retaliatory Tariff Exposure Over Time



(b) Import Tariff Exposure by CZ Political Orientation



(c) Ret. Tariff Exposure by CZ Political Orientation



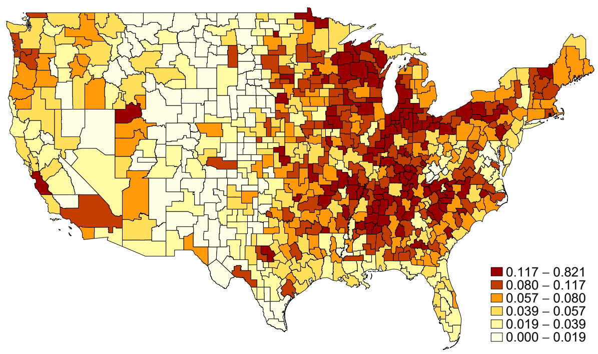
Notes: Panel a shows average CZ exposure to import tariffs (black line) and retaliatory tariffs (blue dashed line) from January 2018 to December 2019, weighted by 2012 CZ employment. Industry import and retaliatory tariff exposures are calculated according to equations (3) and (4) and mapped to CZs based 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.

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.

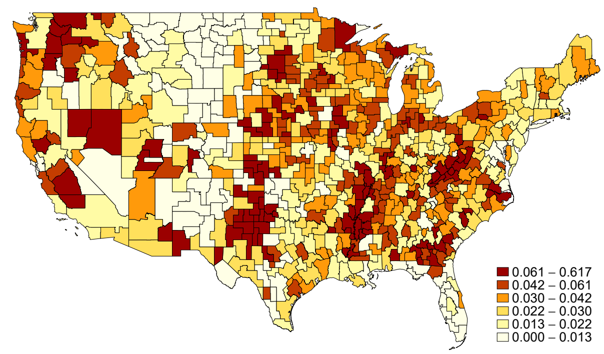
Figure 2 plots the spatial distribution of exposure to tariffs and agricultural subsidies. Panel A shows that import tariff protection concentrates in traditional industrial states (e.g., Pennsylvania, Ohio, Indiana and Michigan), as well as in manufacturing-oriented Southern states (e.g., Tennessee 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

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

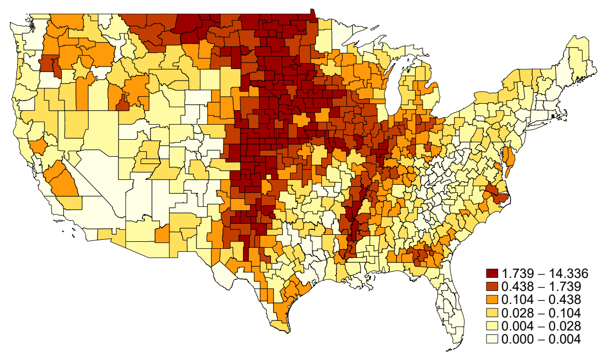
(a) Import tariff exposure



(b) Retaliatory tariff exposure



(c) Farm subsidies per capita



Notes: The figure depicts CZ exposure to import tariffs (Panel (A)), retaliatory tariffs (Panel (B)), and farm subsidies per working age population (Panel (C)) at their end-of-sample values (December 2019 for tariffs and February 2020 for farm subsidies). Industry import and retaliatory tariff exposures are calculated according to equations (3) and (4) and mapped to CZs based on equation (2).

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 subsectors 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 the markets for their crops.[24] Not surprisingly, CZs facing higher retaliatory tariffs received larger farm subsidies, though the correspondence is far from perfect ($\rho = 0.32$). A comparison between panels B and C of Figure 2 for instance shows that North Dakota and Montana received high per capita agricultural

[24]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)). 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).

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 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 been widely studied (e.g., Amiti et al. (2019, 2020b); Fajgelbaum et al. (2020, 2021)). Consistent with this prior research, we confirm in Table A8 of Appendix A2 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.[25]

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 total 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.[26] 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 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

[25] Appendix A2 provides further details on our analysis of tariffs' impact on monthly US imports and exports during the trade war period, which uses a regression design that closely resembles the one we introduce next for the analysis of industry-level sales.

[26] Data from the Annual Survey of Manufacturers is not available for post-pandemic years yet.

[27] Table A7 provides descriptive statistics for the industry-level tariff exposure measures. Yearly tariff exposure is computed as the average monthly exposure during a year.

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: 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	2.118	1.874	1.728	0.924	0.956
	(0.788)	(0.856)	(0.696)	(0.786)	(0.508)	(0.358)
retaliatory tariff exposure	-2.727	-3.041	1.905	1.598	-1.498	-2.338
	(3.222)	(3.172)	(2.068)	(2.313)	(1.447)	(1.102)

Panel B: expenditures for labor and capital inputs (manufacturing)

	log payroll		log cost contract work		log investment	
	(1)	(2)	(3)	(4)	(5)	(6)
import tariff exposure	0.315	0.470	4.636	4.452	6.460	6.407
	(0.442)	(0.552)	(4.721)	(4.521)	(3.131)	(3.112)
retaliatory tariff exposure	-0.177	-0.684	13.378	12.466	4.728	4.489
	(1.473)	(1.657)	(14.278)	(14.596)	(10.695)	(10.755)
sector * year FE	✓	✓	✓	✓	✓	✓
t * (Δ dep. var. in 2016-2017)		✓		✓		✓

Notes: 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=1,096 in columns (3) to (4) and N=1,220 in columns (5) to (6) of Panel B. Data on sales and expenditures is sourced from the Annual Survey of Manufacturers (ASM) 2016, 2018 and 2019 and Economic Census of Manufacturing in 2017 while data on producer prices in 2016 to 2019 is from the Bureau of Labor Statistics. The 2017 Economic Census does not report expenditures for contract work or investment 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 in 2017 (prior to indexing and applying the log transformation) are in Panel A: 2296.8 in columns (1) to (2) and 49,545.1 in (3) to (4), and in Panel B: 7,285.7 in (1) to (2), 636.8 in (3) to (4), and 1,520.8 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 standard errors are clustered by industry.

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

[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.

industry with average import tariff exposure in year 2019 gained 0.98 log points of sales relative to an industry with no such exposure, while an industry with average exposure to retaliatory tariffs lost -0.62 log points of sales.[29]

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 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 their expenditure for labor inputs in proportion to the expansion of sales. Columns 1 and 2 in Panel B of Table 1 however show that log payroll reacted only weakly and insignificantly to import 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 payroll. 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 of import tariff's impact on sales may be accounted for by higher prices (e.g., $0.956/1.728 = 0.55$ based on the column 4 and 6 estimates), while only the remainder 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 and services firms in reaction to demand shocks instead of employing workers directly. An increased labor input for production would in this case not be fully registered as a growth in manufacturing employment but partially 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. Our subsequent employment analysis at the level of local labor markets will capture such spillovers from tradeable

[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.

to nontradable sectors within local labor markets.

Another possible reaction of industries to import tariff protection is that firms could increase production by expanding capital rather than labor inputs.[30] Columns 5 and 6 in Panel B of Table 1 show that log capital investments react strongly and significantly to import tariff exposure. While capital investment can be a substitute for labor in the production, it is also conceivable that tariff-protected industries first need to invest into physical capacity building before they can hire more workers and expand production. The subsequent analysis of QCEW data through 2023 would allow to detect such deferred employment and earnings responses to the 2018-2019 tariffs.

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, during and after the trade war; we then present the main results for employment and split them into their sectoral components; and conclude with complementary results for earnings.

5.1 Preliminary analysis of employment trends

We estimate the effect of tariffs on cumulative employment changes in more versus less exposed CZs using the following simple event-study model:

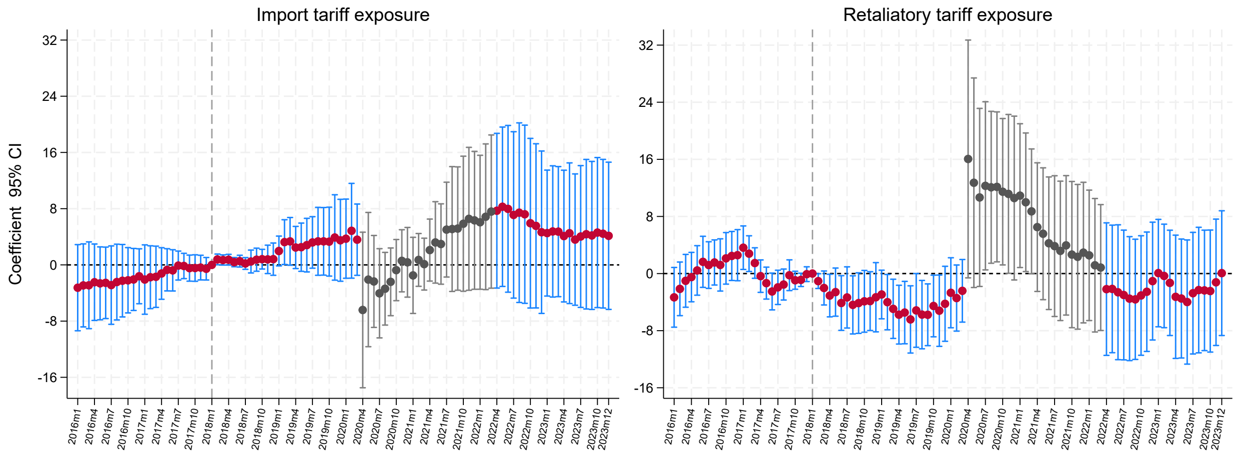
$$EPOP_{rt} - EPOP_{rJan18} = \beta_{1t} IMP_{rDec19} + \beta_{2t} RET_{rDec19} + \gamma_t + \lambda_{s_r,t} + \varepsilon_{rt}. \tag{6}$$

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 2023. 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. The control variables are a set of year-month indicators γ_t that absorb nonparametric time trends, and interactions $\lambda_{s_r,t}$ between time effects and initial employment shares in the two sectors of manufacturing and agriculture/mining, which absorb sector-specific trends.

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). CZs with greater exposure to the import tariffs experienced differential employment gains during the trade war period of 2018 and 2019, although a more favorable trend in employment is already visible prior to the onset of new tariffs in the years 2016 and 2017. CZs exposed to

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

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



Notes: The figure plots by-period coefficient estimates and 95% confidence intervals of the regression model in equation (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. Estimates from April 2020 through March 2022, corresponding to the period affected by the pandemic, are shown in gray. Regressions are weighted by 2012 CZ employment, and standard errors are clustered at the state level.

greater retaliatory tariffs had an uneven employment trend prior to the trade war, followed by relative declines in employment rates after the introduction of the new tariffs.

The onset of the Covid-19 pandemic marks a clear break in both time series of coefficients. From March to April 2020, the employment rate in the US dropped by an unprecedented 9%pts (see Panel A in Appendix Figure A1). The magnitude of the April 2020 employment decline varied across CZs in part due to their differential industrial composition. While this one-month change in employment was only modestly correlated with CZs' exposure to the trade war (a regression of the March to April 2020 change in employment rate on CZ import and retaliatory tariff exposure in December 2019 has an r^2 statistic of 0.09), the dramatic magnitude of the employment collapse at the start of the pandemic makes this effect salient in Figure 3 which indicates relatively larger employment declines in tariff-protected regions and weaker employment declines in CZs facing retaliatory tariffs.

Appendix Figure A1 shows that the recovery from the initial job loss of the pandemic took about two years. Panel A indicates that by early to mid 2022, the average employment rate in CZs had returned to the pre-pandemic levels of 2019 and early 2020. Panel B reports coefficient estimates from regressions that relate a CZ's change in employment rate from March 2020 to various endpoints to the one-month change in employment rate from March to April 2020. A CZ that experienced a one percentage point greater drop in employment rate from March to April 2020 had a 0.7 percentage point greater decline in employment rate over a three-month period from March 2020 to June 2020, and a 0.4 percentage point greater decline in employment rate over a six-month horizon from March 2020 to September 2020. However, the March to April 2020 employment decline is no longer predictive for the change in the employment rate from March 2020 to April 2022, suggesting that recovery from the pandemic's initial employment shock was largely complete after 24 months.

The plots in Figure 3 indicate the two-year window of the pandemic with gray color. In the post-pandemic period from April 2022 to December 2023, there no little evidence for a sustained differential growth of employment in CZs with greater exposure to import tariffs. For CZs with greater exposure to retaliatory tariffs, the decline in employment rate during the 2018-2019 trade war appears to be followed by a partial recovery after the pandemic.

5.2 Main employment results

To account for the staggered introduction of tariffs, the pre-trends detected above, and the temporary impact of the Covid-19 pandemic, 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, and the sector-specific declines in employment rates at the start of the pandemic in March to April 2020:[31]

$$\begin{aligned} EPOP_{rt} - EPOP_{rJan18} = {} & \beta_1 IMP_{rt} + \beta_2 RET_{rt} + \beta_3 SUB_{rt} \\ & + \gamma_t + \delta_{d(r),t} + \lambda_{s_r,t} + \phi(\bar{\Delta} EPOP_{r,2017} \times t) + \chi_{st}(\Delta EPOP_{rs,Mar20-Apr20}) + \varepsilon_{rt}, \end{aligned} \tag{7}$$

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 initial sectoral employment shares in manufacturing, agriculture and mining, and all other sectors ($\lambda_{s_r,t}$), and interactions between time effects and census division dummies ($\delta_{d(r),t}$). These variables respectively absorb overall, sector-specific, and division-specific time trends. The term $\phi(\bar{\Delta} EPOP_{r,2017} \times t)$ accounts for a linear CZ-specific pre-trend of the outcome variable in the year preceding the trade war. Finally, $\chi_{st}(\Delta EPOP_{rs,Mar20-Apr20})$ captures the decline in a commuting zone's employment-to-population ratio at the onset of the pandemic from March to April 2020, measured separately for each of the three broad sectors of manufacturing, agriculture and mining, and all other, and interacted with month-year indicators. These controls capture the large employment decline at the start of the pandemic and the subsequent recovery from that employment loss.

[31] We use the tariff exposure values of December 2019 for all subsequent months through 2023 since tariffs were largely stable over that period.

Table 2 presents results. Panel A first considers outcomes that cover the full trade war period until December 2019. 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. It indicates that both import and retaliatory tariffs are associated with a fall in employment-population rates. This simple specification does not account for the fact that CZ exposure to tariffs varies in part because of different initial employment shares in tradeable sectors that comprise the only industries that are directly exposed to tariffs. The column 2 model thus adds initial sectoral distribution by year-month interactions, and yields a positive coefficient for import tariff exposure and a negative one for retaliatory tariff exposure, consistent with the results in Figure 3. Columns 3 to 5 further control for census-division-by-year-month interactions in column 3, a one-year pre-trend in column 4, and farm subsidy exposure in column 5. With the addition of these controls, the estimated positive effect of import tariffs on employment rates becomes weaker and statistically insignificant. The estimated effect of retaliatory tariffs, however, remains negative and precisely estimated across specifications. As shown in column 5, farm subsidies have measurable employment impacts: a thousand dollar increase in cumulative farm subsidies per capita is estimated to raise employment rates by 0.30 percentage points (se = 0.12). Moreover, controlling for farm subsidies in the employment regressions increases the magnitude 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.

Panel B of Table 2 extends the time horizon of the analysis to cover the four additional years of 2020 to 2023. Since this period comprises the Covid-19 pandemic which temporarily caused differential employment losses in tariff-exposed regions (Figure 3), the specification in column 6 includes controls for sector-specific declines in employment rates at the onset of the pandemic which are interacted with month-year effects to capture recovery from the pandemic. For comparability, the last column of Panel A includes the same controls for the shorter outcome period through 2019. The results in column 6 of Panel B indicate an imprecisely estimated positive coefficient estimate for import tariffs, which is barely larger than the corresponding estimate in Panel A, suggesting that the tariffs did not spur substantial deferred employment growth in regions with protected industries. Conversely, the negative estimate for retaliatory tariffs is only about half as large and less precisely estimated over the longer horizon, suggesting some employment recovery in tariff-affected regions as also seen in Figure 3. The longer-term estimates also indicate larger job gains due to agricultural subsidies.[32]

[32] Appendix Table A2 provides robustness tests for the analyses through 2023 that use alternative strategies to account for pre-trends and the effects of the pandemic. Panel A of the table replaces the control for a one-year pre-trend in the outcome variable with a two-year pre-trend, while Panel B modifies the baseline specification by omitting the pandemic months of April 2020 to March 2022. In both cases, the column 6 estimates for the coefficient on import tariffs are nearly the same as in the baseline model of Table 2. The negative impact of retaliatory tariffs is larger but remains imprecisely measured in the alternative specifications, while the coefficient on agricultural subsidies remains similar across the alternative specifications.

Table 2: Impact of Tariff Exposure on CZ Employment

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: years 2016-2019						
import tariff exposure	-2.848	3.314	2.126	1.498	1.786	1.756
	(1.554)	(1.680)	(1.058)	(1.353)	(1.317)	(1.417)
retaliatory tariff exposure	-3.857	-6.606	-6.285	-4.387	-4.908	-4.625
	(1.953)	(2.005)	(2.215)	(1.766)	(1.761)	(1.495)
farm subsidies per capita					0.299	0.366
					(0.121)	(0.129)
Panel B: years 2016-2023						
import tariff exposure	-2.900	3.367	1.102	0.829	1.713	2.019
	(1.748)	(3.156)	(1.771)	(1.648)	(1.518)	(1.420)
retaliatory tariff exposure	-1.201	0.158	1.271	2.184	-0.387	-2.624
	(2.933)	(3.087)	(2.560)	(2.497)	(2.909)	(2.739)
farm subsidies per capita					0.583	0.531
					(0.140)	(0.147)
year-month FE	✓	(✓)	(✓)	(✓)	(✓)	(✓)
sector*year-month FE		✓	✓	✓	✓	✓
Census division*year-month FE			✓	✓	✓	✓
t * (pre-trend CZ outcome 2017)				✓	✓	✓
Δ emp$_{sector}$/pop Mar-Apr 2020*year-month FE						✓

Notes: N=34,656 (722 commuting zones x 48 months: Jan 2016 – Dec 2019) in Panel A, and N=69,312 (722 commuting zones x 96 months: Jan 2016 – Dec 2023) in Panel B. The dependent variable for all regression models is the seasonally-adjusted employment-to-population ratio, which is indexed to 0 in 2018m1 in each commuting zone. The mean (standard deviation) of the employment-to-population ratio prior to indexing is 65.71 (7.43) percentage points in 2018m1. Farm subsidies are denoted in units of 1,000s of 2018 dollars per working age population. Regressions in columns (2) to (6) 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 (6) also interact time fixed effects with indicators for the 9 geographic Census divisions. Regressions in columns (4) to (6) further control for the monthly change in a CZ's employment-to-population ratio from 2017m1 to 2018m1, interacted with a linear time trend (the count of months since 2018m1). Column (6) controls for interactions of time fixed effects with a CZ's March-to-April 2020 employment-to-population ratio change by sector (agriculture and mining, manufacturing, non-goods sector). Regressions are weighted by commuting zone employment in 2012, and standard errors are clustered by state.

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. For the outcome period through 2019, this yields an average +0.114%pt gain in the employment rate due to import tariffs and a nearly offsetting -0.111%pt decline due to retaliatory tariffs. The estimated employment impact of the agricultural subsidies is a +0.039%pt percentage point gain, which only partially compensates for the negative effect of retaliatory tariffs. Over the longer time period through December 2023, the same scaling exercise yields an employment gain of +0.131%pt due to import tariffs, and a now smaller decline of -0.063%pt due to retaliatory tariffs that is largely offset by a +0.061%pt gain from agricultural subsidies.

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 industries with greater exposure to inputs sourced from newly tariff-protected industries from all other manufacturing; and within the remaining sectors, distinguishing transportation and warehousing, and business services from the residual set.

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)
	total effect	crop prod	other	above-avg. supp. imp. exposure	below-avg. supp. imp. exposure	transport, ware-housing	business services	all other
Panel A: years 2016-2019								
import tariff exposure	1.756	0.207	0.135	-0.474	0.067	0.155	0.917	0.749
	(1.417)	(0.122)	(0.247)	(0.521)	(0.257)	(0.192)	(0.509)	(0.752)
retaliatory tariff exposure	-4.625	-0.984	-0.102	0.105	-0.408	-0.805	-1.212	-1.219
	(1.495)	(0.437)	(0.681)	(0.379)	(0.524)	(0.213)	(0.346)	(0.649)
farm subsidies per capita	0.366	0.039	0.012	0.021	0.077	-0.025	0.056	0.186
	(0.129)	(0.015)	(0.028)	(0.023)	(0.039)	(0.025)	(0.030)	(0.105)
Panel B: years 2016-2023								
import tariff exposure	2.019	0.109	-0.377	-0.750	0.076	-0.350	0.753	2.557
	(1.420)	(0.107)	(0.331)	(0.518)	(0.335)	(0.490)	(0.882)	(1.303)
retaliatory tariff exposure	-2.624	-0.845	1.551	0.915	-0.739	-1.190	-1.316	-1.000
	(2.739)	(0.585)	(1.251)	(0.543)	(0.845)	(0.576)	(0.827)	(1.694)
farm subsidies per capita	0.531	0.040	0.019	0.033	0.073	-0.046	0.141	0.271
	(0.147)	(0.016)	(0.027)	(0.031)	(0.027)	(0.034)	(0.036)	(0.123)
sector*year-month FE	✓	✓	✓	✓	✓	✓	✓	✓
Census division*year-month FE	✓	✓	✓	✓	✓	✓	✓	✓
t*(pre-trend CZ outcome 2017)	✓	✓	✓	✓	✓	✓	✓	✓
Δ emp$_{sector}$/pop Mar-Apr 2020*year-month FE	✓	✓	✓	✓	✓	✓	✓	✓
employment share in 2017	1.000	0.004	0.009	0.038	0.051	0.034	0.136	0.728

Notes: N=34,656 (722 commuting zones x 48 months: Jan 2016 – Dec 2019) in Panel A and N=69,312 (722 commuting zones x 96 months: Jan 2016 – Dec 2023) in Panel B. The dependent variable for all regression models is the seasonally-adjusted employment-to-population ratio in the indicated subsector, which is indexed to 0 in 2018m1 in each commuting zone. Farm subsidies are denoted in 1,000s of 2018 dollars per working age population. All regressions include the control vector of column 6 in Table 2. Columns (4) and (5) include all 3-digit manufacturing sectors with, respectively, above- and below-average supplier tariff exposure in December 2019, as defined in Appendix A2. Industries with above-average supplier import tariff exposure are fabricated metal products, machinery, transportation equipment, primary metal, furniture, and wood products. Regressions are weighted by commuting zone employment in 2012, and standard errors are clustered by state.

Applying equation (7) to these eight sectors, and using the full set of controls from column 6 in 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 tradable sector over both time horizons considered. Most notably, columns 4 and 5 indicate no evidence for job gains in manufacturing, the sector that was the intended beneficiary of the tariffs. Prior literature emphasizes that import tariffs may hinder employment growth in those manufacturing sectors that source many of their inputs from industries whose prices rise following the imposition of tariffs (Flaaen and Pierce, 2021; Javorcik et al., 2022). Since supplier and customer industries tend to collocate (Ellison et al., 2010), many of these spillovers along supply chains will take place within the CZs that have the largest direct exposure to import tariffs. In Appendix A2, we follow Acemoglu et al. (2016) to derive a measure of industries' exposure to suppliers that are protected by higher import tariffs, and Table 3 separately reports local employment outcomes across 3-digit manufacturing sectors that face above-average supplier import exposure (column 4) or below-average exposure (column 5).[33] The coefficient estimates in column 4 indicate that CZs with greater exposure to import tariffs experience larger employment losses in manufacturing industries that face higher input costs, while column 5 shows negligible job gains in other manufacturing sectors. These results barely change over the two time horizons in the Panel A and Panel B analyses, and thus refute the hypothesis that import tariffs generate job growth in manufacturing a few years after their implementation, once firms had time to make the necessary investments.[34]

Instead, the weak positive employment effects of import tariffs stem primarily from imprecisely estimated employment gains in business services (column 7) and the large residual sector (column 8), where growth in services appears consistent with the suggestive evidence on greater spending of tariff-protected industries on contract labor as seen in Table 1.

During the 2018-2019 trade war, exposure to retaliatory tariffs reduced 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. Over the longer time horizon considered in panel B of Table 3, the impacts of retaliatory tariffs on sectoral employment are measured with considerably lower precision, and they indicate slightly smaller job losses in crop production (column 2) combined with job gains in other primary sector industries (column 3), which result in a weaker aggregate negative employment effect seen in column 1.

Finally, farm subsidies significantly increased employment in crop production and weakly raised

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

[34]The CZ analysis captures any spillovers from directly tariff-exposed industries to other local industries but does not account for input-output linkages that operate nationally rather than locally. Appendix A2 presents a complementary industry-level employment analysis 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 corresponding results in Table A9 indicate that direct industry exposure to import tariffs variably has a weak positive or weak negative effect on employment depending on regression specifications, while exposure to retaliatory tariffs consistently has a negative and mostly significant impact. Accounting for indirect tariff exposure via input-output linkages does not substantially affect the estimates for industries' direct exposure, and the estimates for these supply chain spillover effects are imprecise and sensitive to pre-trends.

employment in most other sectors. The 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.

5.4 Earnings impacts

The 2018-2019 trade war took place at the end of a decade-long economic expansion when unemployment rates were at a historical low. While unemployment spiked dramatically during the pandemic, the labor market quickly became tight again when the economy recovered from the 2020 recession. If rising labor demand in tariff-protected regions met an inelastic labor supply, then its impact on the labor market may manifest primarily in higher earnings instead of expanded employment.

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 earnings per working-age adult.[35]

The results in Table 4 indicate that CZs with greater exposure to import tariffs did not experience sizable or significant earnings gains. Evaluated at the mean level of tariff exposure in December 2019, the coefficient estimate for import tariffs in column 5 of panel A translates to only a small 0.035%pt gain in per capita income. A corresponding calculation for retaliatory tariffs implies a nearly offsetting decline in earnings by -0.031%pt, while the positive effect of Agricultural subsidies translates to a +0.036%pt gain. While the sum of these effects is modestly positive at +0.040%pt, it should not be understood as an comprehensive measure of the welfare impact of the trade war. The tariff-induced restrictions to trade raised prices in the US, and Fajgelbaum and Khandelwal (2022) calculate a resulting aggregate -0.10%pt reduction in welfare over the period of the tariff war. Moreover, the stimulating effects of agricultural subsidies in targeted regions trade off with the eventual need to finance these transfers through additional taxation.

The panel B estimates for the extended time period through 2023 indicate more favorable earnings impacts overall. These however do not stem from a delayed positive impact of import tariffs on earnings. Indeed, the insignificant positive coefficient for import tariffs in column 5 of panel B is only half as large as the corresponding estimate in panel A. Instead, the effect of retaliatory tariffs is positive but imprecisely estimated over the longer time horizon, and the

[35] 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.

Table 4: Impact of Tariff Exposure on Earnings per Adult

	(1)	(2)	(3)	(4)	(5)
Panel A: years 2016-2019					
import tariff exposure	-3.906	1.890	-0.334	0.095	0.544
	(7.056)	(5.231)	(6.118)	(6.081)	(6.238)
retaliatory tariff exposure	-6.207	-4.915	-2.311	-3.095	-1.293
	(8.755)	(7.204)	(6.360)	(6.324)	(4.095)
farm subsidies per capita				0.439	0.365
				(0.220)	(0.231)
Panel B: years 2016-2023					
import tariff exposure	-0.318	-1.044	-2.478	-0.787	0.283
	(14.904)	(7.673)	(8.330)	(8.119)	(8.576)
retaliatory tariff exposure	24.643	14.892	15.969	11.000	7.136
	(11.481)	(7.557)	(7.360)	(7.981)	(6.233)
farm subsidies per capita				1.109	1.066
				(0.271)	(0.271)
sector*year-quarter FE	✓	✓	✓	✓	✓
Census division*year-quarter FE		✓	✓	✓	✓
t * (quarterly Δ dep. var. in 2017)			✓	✓	✓
Δ emp_{sector}/pop Mar-Apr 2020*year-month FE					✓

Notes: N=11,548 (722 commuting zones × 16 quarters in 2016-2019, minus 4 missing observations) in Panel A, and N=23,097 (722 commuting zones x 32 quarters in 2016-2023, minus 7 missing observations) in Panel B. The dependent variable is 100 times the quarterly percentage point change in earnings per working-age adult relative to the fourth quarter of 2017. The mean (standard deviation) of earnings per adult in the fourth quarter of 2017 is 9,730.49 (3,034.08). 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.

positive effect of agricultural subsidies also increases in magnitude. These results are qualitatively consistent with the results in Table 2 where the combination of retaliatory tariffs and farm subsidies has relatively more benign employment effect over the period through 2023 in comparison to the trade war period ending in 2019.

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, even years after the implementation 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, earnings and voting intersect at the level of detailed geography. Starting with a narrow focus and zooming outwards, we first assess 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. We finally contrast the impact of tariff exposure on electoral outcomes with our previous findings for employment, and discuss the contrast between these results.

6.1 Support for US tariff policies

To examine voter attitudes toward trade war policies in areas more exposed to tariffs, we draw on data from the Cooperative Congressional Election Study (CCES). In October 2019—shortly after the final major tariff hikes of the trade war had taken effect—the CCES asked respondents whether they supported or opposed two key elements of the Trump administration's trade policy: "Tariffs on 200 billion US dollars worth of goods imported from China"; and "25% tariffs on all imported steel and 10% on imported aluminum, including from Canada and Mexico." At the national level, tariffs on Chinese goods were supported by 50% of respondents and tariffs on imported steel and aluminum by 34%.[36]

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:

$$Y_{jr} = \beta_1 IMP_{rOct19} + \beta_2 RET_{rOct19} + \beta_3 SUB_{rOct19} + \lambda_{s_r} + \delta_{d(r)} + \boldsymbol{X_j'}\boldsymbol{\pi} + \kappa RVS_{r,16} + \varepsilon_{jr} \tag{8}$$

Here, the outcome variable is the percentage of the tariff policies that respondent j residing in CZ r expressed support for. Alongside the treatment variables (import and retaliatory tariff exposures, farm subsidies), all models include controls for initial CZ sectoral employment shares in manufacturing, agriculture and mining, and all other sectors (λ_{s_r}), while some further add a vector of Census division effects $\delta_{d(r)}$ and a vector of respondent characteristics $\boldsymbol{X_j}$, including a quadratic in age, as well as indicator variables for gender, race (non-Hispanic white vs. other), and education (at least some college education vs. high school or less). 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.[37] Regressions are weighted by CCES sampling weights, which are normalized

[36] The CCES does not regularly ask questions on tariffs, and corresponding information is thus not available either prior to the trade war or as a longer-run outcome after the pandemic.

[37] The two-party vote share measures the share of votes for the Republican presidential candidate as a fraction of all

to match CZ population in 2010. Standard errors are clustered at the state level.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
import tariff exposure	9.17	15.42	21.39	24.34	19.85	19.49	24.01	26.29
	(12.55)	(13.83)	(12.00)	(12.11)	(13.28)	(14.61)	(12.12)	(12.13)
retaliatory tariff exposure	-33.42	-35.78	-42.08	-51.92	-34.76	-31.63	-36.96	-44.92
	(22.05)	(21.50)	(24.74)	(26.42)	(21.71)	(21.84)	(25.31)	(26.41)
farm subsidies per capita				3.16				2.52
				(1.74)				(1.70)
2016 Repub. TPVS					0.24	0.20	0.21	0.20
					(0.03)	(0.05)	(0.05)	(0.05)
sector shares	✓	✓	✓	✓	✓	✓	✓	✓
Census division FE		✓	✓	✓		✓	✓	✓
demographic controls			✓	✓			✓	✓

Notes: N= 17,677 respondents (in 619 CZs). The sample size reduces to N=15,452 with the inclusion of demographic controls. The mean (standard deviation) for the outcome is 44.01 (41.99) percentage points. 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. Tariff and subsidy variables are measured in 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 $\text{pop}_{2010,\text{CZ}} \times \left(\frac{\text{CCESweight}_i}{\sum_{i=1}^{\text{CZ}} \text{CCESweight}_i}\right)$, and standard errors are clustered by state.

The results in columns 1 to 4 of Table 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.

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 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 only modest impacts on the coefficient estimates for the tariff and subsidy variables. Holding constant the Republican vote share in 2016, residents of CZs that subsequently

votes cast for the Republican or Democratic candidate while disregarding votes for third-party candidates.

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.[38]

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.

6.2 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 either the years 2016–2019 or 2016–2023, we study the consequences of trade policy on party identification at the level of detailed geography by estimating linear models of the form:

$$\begin{aligned} Y^p_{jrt} - \bar{Y}^p_{r,17} = {} & \beta_1 IMP_{rt} + \beta_2 RET_{rt} + \beta_3 SUB_{rt} + \gamma_t + \lambda_{s_r,t} + \delta_{d(r),t} \\ & + \boldsymbol{X_j'}\boldsymbol{\pi} + \phi(\Delta\bar{Y}^p_{r,16-17} \times t) + \rho\bar{Y}^p_{r,17} + \chi_{st}(\Delta EPOP_{rs,Mar20-Apr20}) + \varepsilon_{jrt}. \end{aligned} \tag{9}$$

The dependent variable in this equation comprises an indicator variable Y^p_{jrt} which is equal to one if voter j residing in CZ r in year t self-identifies as belonging to group $p \in \{$Republican, Demo-

[38] By averaging the answers of two tariff questions, we get a slightly larger sample size and higher precision for the analysis in Table 5. When considering the two questions separately in Table A3, 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.

crat, Independent}. This variable is demeaned relative to its CZ-wide average in 2017, so that the CZ-level average of the resulting outcome variable in other years captures a change in party identification relative to the pre-trade war base year. This setup is comparable to the employment and earnings analyses above, where CZ employment rates were measured relative to their baseline values in January 2018. All models include year fixed effects γ_t and interactions of these effects with initial employment shares in manufacturing and in agriculture and mining $\lambda_{s_r,t}$, and further estimates successively add Census division 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 further control for a one-year linear pre-trend in the CZ average level of party identification ($\Delta\bar{Y}^p_{r,16-17} \times t$). Since the outcomes for the longer time horizon through 2023 include survey data that was collected during the pandemic, some models further control for the same March to April 2020 employment declines considered in the labor market analyses. 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 CCES sampling weights scaled to CZ population, and standard errors are clustered at the state level.

Estimates of equation (9) for the 2016-2019 period are reported in columns 1 to 6 of Table 6. They consistently indicate that voters who were more exposed to import tariffs became more likely to identify as Republicans or Independents, and less likely to identify as Democrats. Conversely, voters in CZs exposed to retaliatory tariffs became more likely to identify as Democrats and less likely to identify as Republicans or Independents, though these effects are less precisely measured.

Column 7 and 8 provide additional estimates for the longer time horizon through 2023. These results indicate precisely estimated changes in the likelihood of survey respondents identifying as Republicans or Democrats, while there are now little impacts on identification as Independents. The estimates imply sizable effects on voter partisan identification. At the sample mean, we estimate from the column 8 model in Panel A that import tariffs increased the fraction of voters identifying as Republicans by 1.5%pt while retaliatory tariffs reduced that fraction by -1.0%pt. Agricultural subsidies had a small and imprecisely estimated effect which at the sample mean implies a 0.1%pt gain in Republican identification. The sum of these three effects implies a sizable 0.6%pt increase in the share of voters identifying as Republicans, while a corresponding calculation based on the coefficient estimates for Democrats in column 8 of Panel B yields a -0.9%pt decline in the share of voters identifying as Democrats (-1.7%pt due to import tariffs, +0.9%pt due to retaliatory tariffs, and -0.1%pt due to subsidies).

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

	2016-2019						2016-2023	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Identifies as Republican								
import tariff exposure	13.97	18.34	30.31	41.31	23.89	23.59	22.05	23.49
	(30.27)	(30.88)	(26.75)	(33.84)	(20.33)	(20.42)	(11.90)	(12.13)
retaliatory tariff exposure	-16.81	-12.70	-2.48	-29.21	-22.36	-21.71	-33.72	-41.22
	(28.77)	(29.56)	(32.12)	(33.62)	(19.80)	(19.74)	(12.23)	(11.96)
farm subsidies per capita						-0.36	1.46	0.78
						(2.93)	(0.78)	(0.79)
Panel B: Identifies as Democrat								
import tariff exposure	-45.41	-48.71	-64.77	-72.44	-54.36	-56.28	-24.05	-26.32
	(23.98)	(24.15)	(21.30)	(29.72)	(15.14)	(15.72)	(10.87)	(11.59)
retaliatory tariff exposure	35.40	27.90	24.87	38.61	33.52	37.79	29.99	39.52
	(24.34)	(24.06)	(25.67)	(28.32)	(17.76)	(18.96)	(12.54)	(13.19)
farm subsidies per capita						-2.34	-1.95	-1.07
						(2.71)	(0.87)	(0.77)
Panel C: Identifies as Independent								
import tariff exposure	31.45	30.37	34.47	36.72	30.89	32.80	2.59	3.51
	(14.26)	(16.05)	(14.75)	(16.34)	(13.02)	(12.61)	(6.15)	(6.36)
retaliatory tariff exposure	-18.59	-15.20	-22.39	-38.28	-19.10	-23.33	1.55	-0.69
	(23.13)	(20.12)	(20.20)	(24.32)	(12.25)	(13.21)	(9.74)	(9.47)
farm subsidies per capita						2.32	0.44	0.21
						(1.85)	(0.60)	(0.59)
sector*year FE	✓	✓	✓	✓	✓	✓	✓	✓
Census division*year FE		✓	✓	✓	✓	✓	✓	✓
Demographic controls			✓	✓	✓	✓	✓	✓
t*(pre-trend CZ avg. outcome 2016-17)				✓	✓	✓	✓	✓
CZ average outcome in 2017					✓	✓	✓	✓
Δ emp_{sector}/pop Mar-Apr 2020*year FE								✓

Notes: N=287,891 in column (1); the inclusion of demographic controls reduces the sample size to N=268,221, and the addition of the pre-trend as regressor further diminishes this number to 268,170. In columns (7) and (8), N=420,235. The dependent variable for all regression models is demeaned with its CZ-wide 2017 average. The mean (standard deviation) for the outcome in 2017 before demeaning is 36.03 (48.01) in Panel A, 46.10 (49.85) in Panel B, and 17.87 (38.31) in Panel C. Tariff exposure variables 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 their end-of-sample value (December 2019 for tariff exposure variables and February 2020 for farm subsidies) for survey years 2020-2023, 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). Column (8) further controls for a CZ's March-to-April 2020 employment-to-population ratio change by sector (agriculture and mining, manufacturing, non-goods sector) interacted with year indicators. Regressions are weighted by $\text{pop}_{2010,\text{CZ}} \times \left(\frac{\text{CCESweight}_i}{\sum_{i=1}^{\text{CZ}} \text{CCESweight}_i}\right)$, and standard errors are clustered by state.

6.3 Electoral outcomes

The tariff war was evidently successful in shifting voter identification away from the Democratic party and towards the Republican party. We next consider its impact 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 $t \in \{2012, 2016, 2020, 2024\}$:

$$RVS_{rt} - RVS_{r,16} = \beta_1 IMP_{rt} + \beta_2 RET_{rt} + \beta_3 SUB_{rt} + \gamma_t + \lambda_{s_r,t} + \delta_{d(r),t} \\ + \phi\left(\Delta RVS_{r,12-16} \times t\right) + \rho RVS_{r,16} + \chi_{st}(\Delta EPOP_{rs,Mar20-Apr20}) + \varepsilon_{rt} + \varepsilon_{jrt}. \quad (10)$$

As in the previous regression analyses, 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. Since the time-variant tariff and subsidy variables have values of zero prior to 2018, the regressions with outcomes through 2020 effectively estimate the impact of the trade war on the 2020 election, while those with outcomes until 2024 consider the effects on both the 2020 and 2024 elections. The regressions control for year main effects and sector-by-year interactions, and in successive specifications, for Census division-by-year interactions, the Republican two-party vote share in 2012–2016 interacted with a time trend to account for trends in partisan support, the 2016 Republican two-party vote share to absorb mean reversion, and sectoral declines in employment rates from March to April 2020 to absorb differential labor market shocks due to the pandemic.

Panel A of Table 7 indicates that import tariff exposure significantly increased support for the Republican candidate Trump in the 2020 election, while panel B shows an even larger Republican gain from import tariffs when the 2024 election is considered as well. Retaliatory tariffs had a modest and statistically insignificant negative effect on the Republican vote over either of the time horizons considered, while greater CZ exposure to farm subsidies generated more support for the Republican candidate especially over the longer horizon.[39]

Evaluated at the mean tariff exposure at the end of the trade war, the column 6 estimate in Panel B implies that import tariffs raised President Trump's two-party vote share by +0.88% following the trade war, while the implied effects of retaliatory tariffs (-0.10%) and subsidies (+0.11%) nearly cancel out. These results are qualitatively consistent and quantitatively comparable to the shifts of party identification in tariff-exposed CZs documented in Table 6.[40]

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. In order to observe election outcomes in consistently defined districts, we focus on the 2012-2020 period which is bracketed by the major redistricting that follows every decennial Census.[41]

[39] Since the first presidential election after the start of the trade war fell into the Covid-19 pandemic, the estimates in column 6 control for CZ-level employment declines from March to April 2020. These controls have little impact on coefficient estimates. Appendix Table A4 alternatively omits the 2020 election, and finds equally signed but slightly larger coefficients for each of the tariff and subsidy variables.

[40] Controlling forAs a robustness check, Table A4

[41] The analysis omits the 2020 electoral results for the state of North Carolina where redistricting took place between the 2018 and 2020 elections.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: years 2012-2020						
import tariff exposure	11.20	9.68	11.39	9.69	9.87	10.38
	(6.53)	(4.03)	(5.46)	(4.67)	(4.79)	(4.80)
retaliatory tariff exposure	1.50	6.71	-1.99	-3.34	-4.03	-3.30
	(4.66)	(4.41)	(5.47)	(4.42)	(4.92)	(4.71)
farm subsidies per capita					0.13	0.18
					(0.18)	(0.22)
Panel B: years 2012-2024						
import tariff exposure	16.19	13.86	14.69	12.58	13.36	13.49
	(8.94)	(5.81)	(6.37)	(5.52)	(5.79)	(5.82)
retaliatory tariff exposure	-0.96	5.03	0.84	-2.69	-5.73	-4.33
	(6.59)	(5.95)	(6.02)	(5.50)	(6.11)	(5.35)
farm subsidies per capita					0.59	0.59
					(0.21)	(0.26)
sector*year FE	✓	✓	✓	✓	✓	✓
Census division*year FE		✓	✓	✓	✓	✓
t * (trend pre-trend CZ outcome 2012-16)			✓	✓	✓	✓
2016 Repub. TPVS				✓	✓	✓
Δ emp$_{sector}$/pop Mar-Apr 2020*year FE						✓

Notes: N=2,166 (722 CZs x 3 presidential elections in the 2012-2020 period) in Panel A and N=2,888 (722 CZs × 4 presidential elections in the 2012-2024 period) in Panel B. The dependent variable in all panels is 100 times the Republican two-party vote share in presidential elections, indexed to 0 in 2016. The mean (standard deviation) of the outcome variable prior to indexing in 2016 is 49.00 (14.56) in both panels. Tariff exposure variables and cumulative farm subsidy payments are measured at their end-of-sample values (December 2019 and February 2020, respectively) for the 2020 and 2024 elections. Column (6) in all panels further includes controls for a CZ's March to April 2020 employment-to-population ratio change by sector (agriculture and mining, manufacturing, non-goods sector) interacted with time. Regressions are weighted by 2010 CZ population, and standard errors are clustered by state.

Panel A of Appendix Table A5 reports estimates, applying a specification similar to equation 10.[42] The results are qualitatively consistent with the Presidential analysis. They indicate vote share gains for Republicans in CZs with greater import tariff exposure accompanied by smaller negative effects of retaliatory tariffs, though all estimates are only marginally statistically significant at best.[43] It is noteworthy that the impacts of tariffs on party vote shares for Congressional elections

[42]Different from 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.

[43]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

in panel B are both larger but also much less precisely estimated than the results for presidential elections in panel A of Table 10. 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.

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 in the district. The corresponding results in Panel B of Appendix Table A5 point to imprecisely estimated Republican gains in tariff protected regions and losses in CZs exposed to retaliatory tariffs. The agricultural subsidies whose impact on party vote share is muted in Panel A have a significant positive effect on Republican victories in Panel B, which may suggest that some of the subsidies were regionally targeted for political gain.

7 Comparing the tariff war's impacts on jobs and elections

A comparison between the labor market results in section 5 and the political results in section 6 indicates consistency in the sign patterns of the tariffs and subsidy variables: The same variables that tend to have positive (negative) impacts on employment and earnings also have positive (negative) effects for support of the Trump tariffs and the Republican party. To provide a quantitative comparison of the effects in the labor market and political domains, Figure 4 plots the predicted employment effects of tariffs and subsidy variables based on Table 2 against their predicted impacts for the GOP vote share in presidential elections based on Table 7, with CZs aggregated into 40 bins based on their x-axis values.[44] Panel A of Figure 4 uses coefficient estimates for employment outcomes to 2019 and electoral outcomes to 2020. It shows 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 0.3% 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. Panel B repeats the same quantification with regression coefficient estimates for employment outcomes to 2023 and electoral outcomes to 2024. Over this longer time horizon, the predicted employment effects are

tariffs in 2019 (Mascaro, 2019).

[44]The predicted combined effect of import tariff, retaliatory tariff and farm subsidy exposure on employment in panel A of Figure 4 is based on each CZ's exposure in December 2019, scaled by the regression coefficients in column 6 of panel A in Table 2. The predicted combined effect of the trade war variables on the Republican vote share in presidential elections shown in panel A of Figure 4 scales the exposures at the end of the trade war with the regression coefficients in column 6 in panel A of Table 7. Panel B of Figure 4 scales CZ exposures at the end of the trade war with coefficient estimates from column 6 of panel B in Table 2 and column 6 in panel B of Table 7. Each bin comprises a set of CZs with approximately equal combined population size.

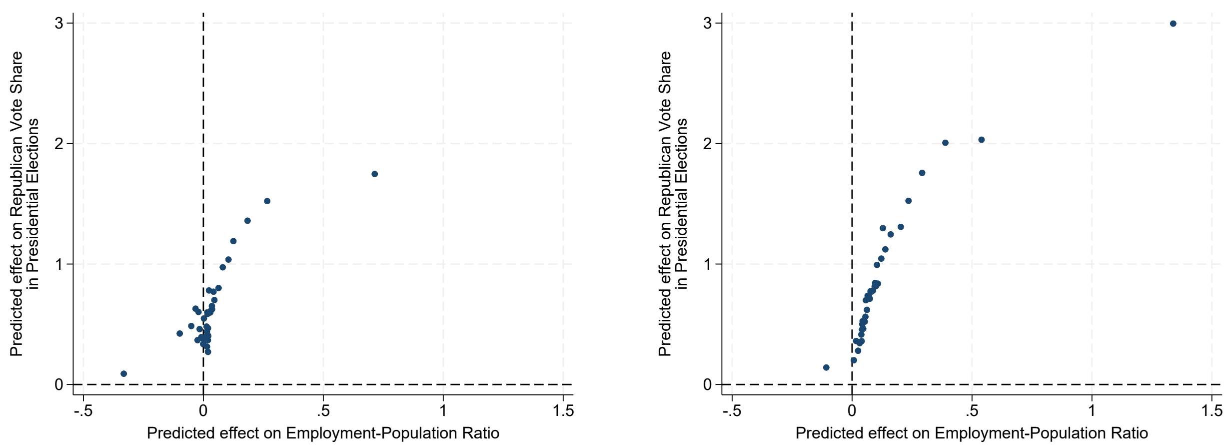
slightly more favorable, though the bulk of the effects remains close to zero, and there are also somewhat stronger predicted Republican vote share gains.

A striking pattern in both panels of Figure 4 is 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 6 of panel A or panel B in Table 2 indicate that two equally large increments in import tariff and retaliatory tariff exposure would result in a small 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 panel A or panel B in 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.

$\boxed{\text{Decide between long and short footnotes}}$

Figure 4: Predicted Effect of Tariff Exposure on Emp/Pop and Presidential Elections

(a) Outcomes to 2019/20 (b) Outcomes to 2023/24



Notes: Panel (a) scales CZ-level exposures to import tariffs, retaliatory tariffs and agricultural subsidies with coefficient estimates from column 6 in panel A of Table 2 to obtain predicted combined impacts of tariffs and subsidies on CZ employment-to-population ratios, and scales CZ exposures with coefficient estimates from column 6 in panel A of Table 7 to obtain predicted combined impacts on two-party vote shares in presidential elections. Panel (b) repeats the same analysis using column 6 estimates from panel B in Table 2 and Table 7. CZs are grouped into 40 bins with approximately equal population size based on predicted employment rate effects.

Why did the trade war benefit the party that instigated it even in regions whose labor markets show no signs of employment gains? Our results indicate that the tariff policies of the Trump government enjoyed greater support in regions whose industries received protection (Table 5), despite muted impacts of these tariffs on either employment rates (Table 2) or per capita earnings (Table 4). We discuss two potential explanations. The first suggests that residents in CZs with greater exposure to import tariffs had differential awareness of these policies, or misperceived their economic benefits. The second is that voters may support tariffs for reasons unrelated to direct economic gains.

7.1 Awareness of tariffs and misperception of economic gains

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). Voters may therefore have rewarded the government based on its claims about tariffs rather than their realized economic effects. Such political messaging may have resulted in greater support for the president in import-protected regions through two channels. If political communication was specifically targeted at those regions, then residents of these locations would have greater awareness of the tariff policies, and thus greater potential to reward the government for it. If instead awareness of the policy is pervasive, then the electoral reaction may still be stronger in tariff-protected regions when voters expect that the economic gains from tariffs concentrate in these locations. Moreover, research in political science shows that voters' perception of objective economic conditions is influenced by partisan bias (Bartels, 2002; Gerber and Huber, 2009), and the concentration of import tariff protection in Republican-leaning regions seen in Figure 1 may thus create a more favorable perception of economic growth in these areas.

To study the acquisition of information about tariffs, 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.[45] Panel A of Appendix Figure A2 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. It indicates that web search related to tariffs spiked in the periods when new tariffs were introduced, and the magnitude of search interest varied considerably across states. However, when we explore whether these cross-state differences in search intensity are systematically related to regional exposure to tariffs, there is no differential search activity across states with whose CZs faced greater or smaller exposure to import tariffs (panel B) or retaliatory tariffs (panel C). We interpret this result as indicating that tariffs were a subject of national rather than local interest.[46]

Even if voters in all parts of the country were equally aware of the tariffs and had the same propensity to overestimate their beneficial economic effects, differential electoral gains of Repub-

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

[46] Coverage of the tariff policies was also prevalent in major media outlets such as the *New York Times*, whose briefing section–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 (accessed via https://www.nytimes.com/search/?srchst=nyt).

licans in tariff-protected regions may result if voters are most likely to reward the government in those locations where the presumed labor market benefits of the tariffs accrue. To test for this possibility, one would ideally analyze data on voters' perception of the economic impacts of tariffs in their location. Absent such spatial data specifically linked to the economic effects of tariffs, we study a CCES survey question that more generally asks respondents whether their own household income has improved over the preceding year, using a regression specification similar to the analysis of tariff support in equation 8.[47]

Table A6 presents results. Residents in CZs with greater exposure to import tariffs were not more likely to report improvements in their household income, both unconditional or conditional on their support for Republicans prior to the trade war. Retaliatory tariffs had imprecise negative and farm subsidies weakly positive effects on perceived income growth. The null result for import tariffs is tightly estimated for the 2018-2023 period. Evaluated at the mean tariff exposure at the end of the trade war, the coefficient estimate for import tariffs in column 6 of Panel B implies a -0.02%pt smaller likelihood to report income growth. This lack of a differential self-reported income growth is consistent with the muted earnings effects of import tariffs that are observed using the QCEW register data in Table 4. The greater support for tariffs and for the Republican party in tariff-protected regions hence does not appear to be the result of a misperception of residents' own income growth.

7.2 Support for tariffs absent economic gains

An alternative 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 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]

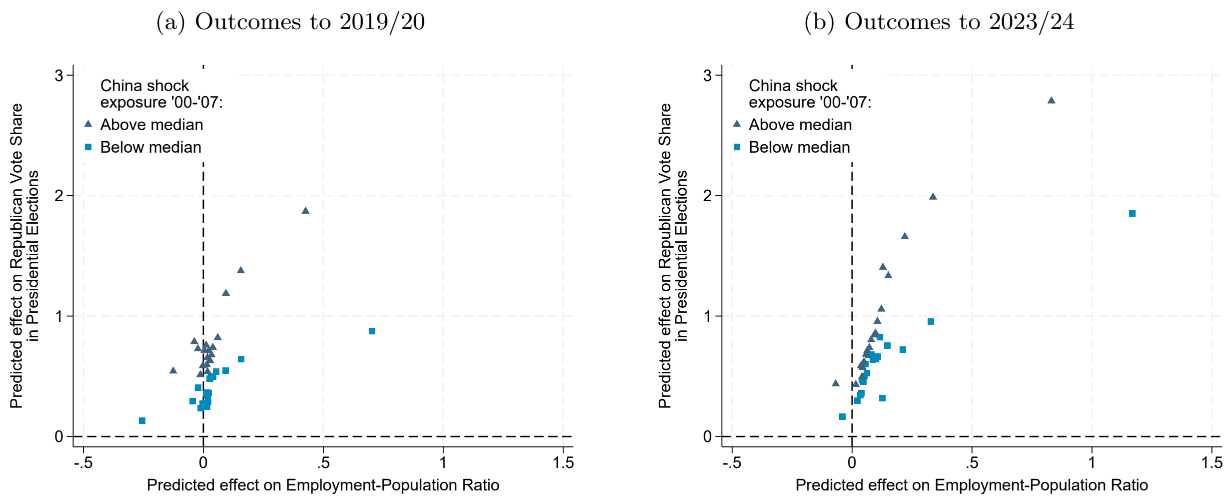
Prior research shows that skepticism about international trade and support for isolationist

[47] While Table 5 studies tariff support in the single year 2019, the CCES consistently asked about improvements in household income in every year since 2018. We study perceived household income growth for either the combined 2018-2019 or 2018-2023 waves of the CCES, and thus modify the regression setup of equation 8 by interacting the sector share, Census division indicator, and 2016 Republican vote share controls with year indicators, and in some specifications add controls for employment changes at the onset of the pandemic.

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

political parties is more widespread in regions and countries that were more exposed to the so-called 'China shock', a period of rapidly growing import competition from China in the 1990s and 2000s (Colantone and Stanig, 2018; Autor et al., 2020; Davenport et al., 2022). This import competition contributed to a sharp decline in US manufacturing employment (Autor et al., 2013, 2016; Acemoglu et al., 2016; Pierce and Schott, 2016), and affected regions continue to suffer from depressed employment rates up to the period of the tariff war (Autor et al., 2021, 2025). Analyzing the content of articles on China published in US regional newspapers, (Arezki et al., 2024) show that sentiment towards China continued to be significantly more negative in regions with prior exposure to the 'China shock' during the 2010s before the onset of the trade war.

Figure 5: Predicted Effect of Tariff Exposure on Emp/Pop and Presidential Elections



Notes: Panel (a) scales CZ-level exposures to import tariffs, retaliatory tariffs and agricultural subsidies with coefficient estimates from column 6 in panel A of Table 2 to obtain predicted combined impacts of tariffs and subsidies on CZ employment-to-population ratios, and scales CZ exposures with coefficient estimates from column 6 in panel A of Table 7 to obtain predicted combined impacts on two-party vote shares in presidential elections. Panel (b) repeats the same analysis using column 6 estimates from panel B in Table 2 and Table 7. CZs are grouped into 40 bins with approximately equal population size based on their predicted employment values. CZs with above- and below-median exposure to Chinese import competition in 2000–2007 are each grouped into 20 bins with approximately equal population size based on predicted employment rate effects.

We explore the differential impact of the tariff war on CZs that faced above-median versus below-median Chinese import competition from 2000 to 2007.[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 Figure 5, we plot the predicted employment and electoral effects of the tariff war as computed in Figure 4 separately for 20 bins of CZs with above median China shock

[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.

exposure and 20 bins of CZs with below median exposure. The two panels, which alternatively use regression estimates through 2019/2020 (panel A) or through 2023/24 (panel B), both indicate that there is little difference in predicted employment effects among more versus less China trade shocked-exposed CZs. However, for a given level of predicted employment changes, those with higher prior exposure to the China shock exhibit substantially larger predicted electoral gains for the Republican party (an average of 0.9% vs. 0.4% in CZs with high vs. low China shock exposure in panel A, and 1.2% vs. 0.5% in panel B). The overall result of a sizable increase in the Republican vote share even in absence of positive employment effects thus results to a considerably stronger extent in 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 and other trade partners even as economic gains from such government policy remained elusive.

8 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 largely a wash. US import tariffs had insignificantly positive employment effects; retaliatory tariffs had more significant negative employment impacts; and only a part of these adverse effects were offset by agricultural subsidies. Beyond their muted impact on jobs, import tariffs also failed to substantially raise earnings in tariff-protected regions. These muted impacts of tariffs on the labor market can be observed both during the trade war of 2018–2019 and also over a longer time horizon through 2023. We therefore do not find evidence for the notion that favorable labor market impacts of import tariffs emerge with a time lag after the economy was able to adjust to the new tariff regime.

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

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A1 Appendix Tables

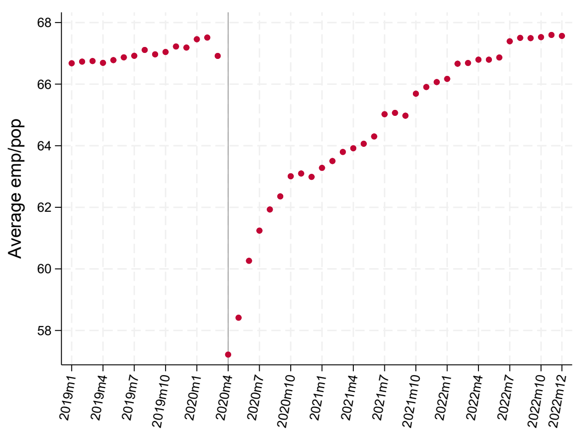
Table A1: CZ Exposure to Tariffs

	Commuting zone		
	import tariff exposure	retaliatory tariff exposure	farm subsidies
Exposure February '18 to December '19			
mean	0.031	0.017	0.046
sd	0.036	0.020	0.221
p25	0.007	0.006	0.000
p50	0.023	0.013	0.002
p75	0.043	0.021	0.013
Exposure Dec '19 (Feb '20 for farm subsidies)			
mean	0.065	0.024	0.115
sd	0.047	0.021	0.465
p25	0.039	0.014	0.001
p50	0.057	0.019	0.007
p75	0.079	0.029	0.038
Correlations (end of sample)			
w/imp. tariff	1.000	0.352	0.085
w/ret. tariff	0.352	1.000	0.319

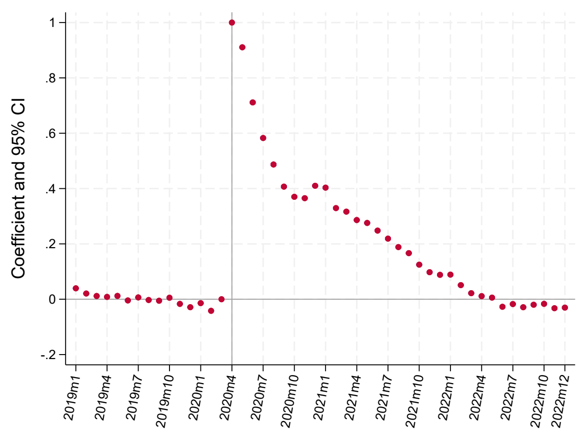
Notes: The top panel measures average import tariff exposure from February 2018 to December 2019, exposure to retaliatory tariffs from April 2018 to December 2019, and cumulative farm subsidy payments in $1000s of 2018 dollars per working age population from September 2018 to February 2020, for 722 commuting zones. In the second panel, tariff exposure variables and cumulative farm subsidy payments are measured at their end-of-sample values (December 2019 and February 2020, respectively), which corresponds to maximum exposure for most CZs. The same time period is used to compute correlations among the variables. Statistics are weighted by average commuting zone employment in 2012.

Figure A1: Effect of March-to-April 2020 Employment-to-Population drop on subsequent months

(a) CZ average Emp/Pop by month



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



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.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: years 2016-2023, 2-year pre-trend						
import tariff exposure	-2.900	3.367	1.102	0.833	1.737	2.199
	(1.748)	(3.156)	(1.771)	(1.601)	(1.469)	(1.436)
retaliatory tariff exposure	-1.201	0.158	1.271	0.264	-2.418	-4.587
	(2.933)	(3.087)	(2.560)	(2.676)	(3.039)	(2.929)
farm subsidies per capita					0.594	0.532
					(0.135)	(0.140)
year-month FE	✓	(✓)	(✓)	(✓)	(✓)	(✓)
sector*year-month FE		✓	✓	✓	✓	✓
Census division*year-month FE			✓	✓	✓	✓
t * (pre-trend CZ outcome 2016-2017)				✓	✓	✓
Δ emp$_{sector}$/pop Mar-Apr 2020*year-month FE						✓
Panel B: years 2016-2023 excluding pandemic						
import tariff exposure	-5.310	4.630	2.118	1.605	2.405	2.119
	(2.456)	(4.012)	(2.088)	(1.768)	(1.695)	(1.472)
retaliatory tariff exposure	-7.326	-4.201	-2.959	-1.359	-3.486	-3.390
	(3.941)	(2.714)	(2.562)	(2.206)	(2.486)	(2.653)
farm subsidies per capita					0.550	0.655
					(0.149)	(0.173)
year-month FE	✓	(✓)	(✓)	(✓)	(✓)	(✓)
sector*year-month FE		✓	✓	✓	✓	✓
Census division*year-month FE			✓	✓	✓	✓
t * (pre-trend CZ outcome 2017)				✓	✓	✓
Δ emp$_{sector}$/pop Mar-Apr 2020*year-month FE						✓

Notes: N=69,312 (722 commuting zones x 96 months: Jan 2016 – Dec 2023) in Panel A, and N = 51,984 (722 commuting zones x 72 months: Jan 2016 - Dec 2023, excluding Apr 2020 - Mar 2022) in Panel B. The dependent variable for all regression models is the seasonally-adjusted employment-to-population ratio, which is indexed to 0 in 2018m1 in each commuting zone. The mean (standard deviation) of the employment-to-population ratio prior to indexing is 65.71 (7.43) percentage points in 2018m1. Farm subsidies are denoted in units of 1,000s of 2018 dollars per working age population. Regressions in columns 2 to 6 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 6 also interact time fixed effects with indicators for the 9 geographic Census divisions. Regressions in columns 4 to 6 further control for pre-trends. In Panel A, this is done by interacting the average monthly change in a CZ's employment to population ratio from 2016m1 to 2018m1 with a linear time trend (the count of months since 2018m1), in Panel B, the pre-trend variable is constructed using the 2017m1-2018m1 period instead. Column 6 in both panels includes time fixed effects interacted with a CZ's March-to-April 2020 employment-to-population ratio change by sector (agriculture and mining, manufacturing, non-goods sector). Regressions are weighted by commuting zone employment in 2012, and standard errors are clustered by state.

Table A3: CZ Residents' Support for US Import Tariffs in 2019

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: support for Steel and Aluminium import tariff								
import tariff exposure	29.60	31.98	40.00	42.96	36.02	33.47	41.90	44.37
	(14.11)	(13.55)	(13.06)	(12.67)	(13.18)	(13.86)	(13.50)	(13.21)
retaliatory tariff exposure	-6.16	-5.50	-25.69	-35.69	-7.03	-4.04	-22.11	-30.84
	(30.21)	(31.44)	(30.72)	(32.76)	(30.95)	(31.38)	(31.22)	(32.87)
farm subsidies per capita				3.19				2.74
				(2.15)				(2.15)
2016 Repub. TPVS					0.15	0.07	0.15	0.14
					(0.04)	(0.06)	(0.06)	(0.06)
Panel B: support for China import tariff								
import tariff exposure	-8.45	1.91	6.77	9.81	6.77	8.77	10.28	12.44
	(23.00)	(26.54)	(22.73)	(23.48)	(25.27)	(26.72)	(22.24)	(22.75)
retaliatory tariff exposure	-60.25	-66.17	-59.31	-69.73	-62.37	-59.45	-52.72	-60.55
	(24.48)	(21.86)	(21.71)	(23.82)	(21.47)	(22.23)	(22.06)	(23.49)
farm subsidies per capita				3.45				2.54
				(1.70)				(1.64)
2016 Repub. TPVS					0.35	0.33	0.27	0.27
					(0.04)	(0.05)	(0.06)	(0.06)
sector shares	✓	✓	✓	✓	✓	✓	✓	✓
Census division FE		✓	✓	✓		✓	✓	✓
demographic controls			✓	✓			✓	✓

Notes: N=17,549 respondents (in 619 CZs) in Panel A and N= 17,616 respondents (in 618 CZs) in panel B. The sample size reduces to N=15,350 (Panel A) and N=15,404 (Panel B), respectively, with the inclusion of demographic controls. Panel A asks respondents whether they support the 25% tariffs on imported steel and 10% on imported aluminum, including from Canada & Mexico; Panel B asks respondents on the support for tariffs on 200 billion US dollars worth of goods imported from China in 2019. The outcome variable takes a value of 100 if the respondent is supportive of the tariff and zero if not. The mean (standard deviation) for the outcome is 35.6 (47.9) in Panel A and 52.3 (49.9) in Panel B. 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 $\text{pop}_{2010,\text{CZ}} \times \left(\frac{\text{CCESweight}_i}{\sum_{i=1}^{\text{CZ}} \text{CCESweight}_i}\right)$, and standard errors are clustered by state.

Table A4: Republican Two-Party Vote Share in Presidential Elections (excl. 2020)

	(1)	(2)	(3)	(4)	(5)	(6)
import tariff exposure	21.18	18.05	19.34	17.15	17.90	17.67
	(11.46)	(7.70)	(8.63)	(7.67)	(7.93)	(7.93)
retaliatory tariff exposure	-3.42	3.35	-3.13	-6.35	-9.23	-6.78
	(8.67)	(7.77)	(8.20)	(7.55)	(8.26)	(7.20)
farm subsidies per capita					0.56	0.63
					(0.28)	(0.35)
sector*year FE	✓	✓	✓	✓	✓	✓
Census division*year FE		✓	✓	✓	✓	✓
t * (trend pre-trend CZ outcome 2012-16)			✓	✓	✓	✓
2016 Repub. TPVS				✓	✓	✓
Δ emp$_{sector}$/pop Mar-Apr 2020*year FE						✓

Notes: N=2,166 (722 CZs x 3 presidential elections in the 2012-2024 period, excluding the 2020 election). The dependent variable is 100 times the Republican two-party vote share in presidential elections, indexed to 0 in 2016. The mean (standard deviation) of the outcome variable prior to indexing in 2016 is 49.00 (14.56) in both panels. Tariff exposure variables and cumulative farm subsidy payments are measured at their end-of-sample values (December 2019 and February 2020, respectively) for the 2024 election. Column (6) in all panels further includes controls for a CZ's March to April 2020 employment-to-population ratio change by sector (agriculture and mining, manufacturing, non-goods sector) interacted with time. Regressions are weighted by 2010 CZ population, and standard errors are clustered by state.

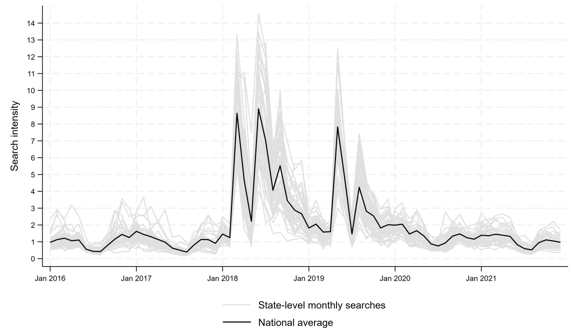
Table A5: Republican Vote Share and Victory Indicator in Congressional Elections

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Republican Two Party Vote Share						
import tariff exposure	64.93	58.84	54.56	42.06	43.24	44.51
	(35.66)	(34.99)	(35.60)	(29.71)	(30.25)	(28.30)
retaliatory tariff exposure	-17.00	-23.98	-32.97	-27.21	-29.87	-26.80
	(15.16)	(15.35)	(17.99)	(16.77)	(16.67)	(17.03)
farm subsidies per capita					0.86	0.52
					(0.93)	(0.90)
Panel B: Republican Victory Indicator						
import tariff exposure	41.88	15.33	12.27	6.72	14.71	15.91
	(22.73)	(23.78)	(22.63)	(21.27)	(21.26)	(21.04)
retaliatory tariff exposure	18.48	6.29	-0.13	2.42	-15.60	-13.22
	(23.12)	(21.17)	(22.75)	(23.34)	(25.00)	(25.11)
farm subsidies per capita					5.85	6.06
					(2.77)	(2.83)
sector*year	✓	✓	✓	✓	✓	✓
Census division*year		✓	✓	✓	✓	✓
t * (pre-trend outcome 2014-16)			✓	✓	✓	✓
2016 Repub. TPVS				✓	✓	✓
Δ emp$_{sector}$/pop Mar-Apr 2020*year FE						✓

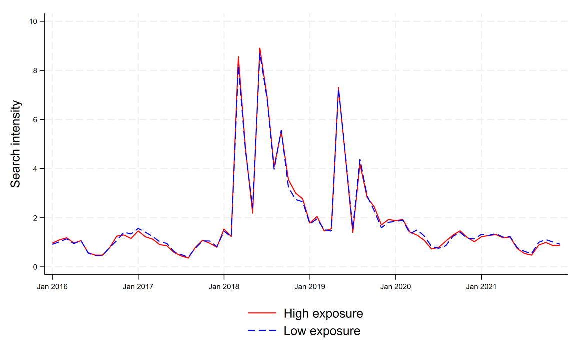
Notes: N=7,674 (1,546 CZ-congressional voting district cells $\times$ 5 congressional elections in 2012-2020 minus data for North Carolina in 2020) in both panels. The dependent variable in Panel A is 100 times the Republican two-party vote share in congressional elections, indexed to 0 in 2016. In Panel B, 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 49.97 (25.58) in Panel A and 55.47 (49.72) 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 final values in December 2019 or February 2020 for the 2020 elections. Column (6) in both panels further includes controls for a CZ's March to April 2020 employment-to-population ratio change by sector (agriculture and mining, manufacturing, non-goods sector) interacted with time. Regressions are weighted by 2010 CZ population, and standard errors are clustered by state.

Figure A2: Internet Searches for “Tariffs”

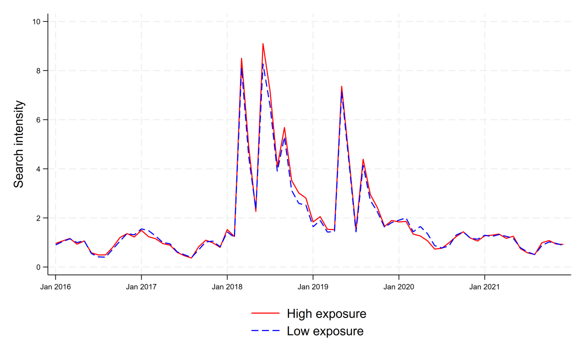
(a) all states



(b) by exposure to import tariffs



(b) by exposure to retaliatory tariffs



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

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

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: years 2018-2019						
import tariff exposure	-43.96	-0.34	3.26	3.29	3.65	2.96
	(29.77)	(11.12)	(13.04)	(13.06)	(13.18)	(12.37)
retaliatory tariff exposure	-31.37	-25.63	-25.45	-25.50	-25.39	-23.45
	(32.56)	(15.47)	(16.45)	(16.14)	(15.72)	(15.34)
farm subsidies per capita				0.03	-0.07	0.23
				(1.91)	(1.93)	(2.00)
Panel B: years 2018-2023						
import tariff exposure	-11.58	-0.36	-0.21	-0.10	-0.01	-0.20
	(7.07)	(2.73)	(3.09)	(3.11)	(3.15)	(2.96)
retaliatory tariff exposure	-11.84	-8.41	-8.82	-9.17	-9.13	-8.59
	(10.38)	(5.05)	(5.82)	(5.93)	(5.80)	(5.50)
farm subsidies per capita				0.08	0.07	0.09
				(0.20)	(0.20)	(0.21)
sector shares*year FE	✓	✓	✓	✓	✓	✓
Census division FE*year FE		✓	✓	✓	✓	✓
demographic controls			✓	✓	✓	✓
Repub. 2016 TPVS*year FE					✓	✓
Δ emp_{sector}/pop Mar-Apr 2020*year-month FE						✓

Notes: In Panel A, N=77,608 respondents (in 685 CZs) in columns 1 and 2, and N=75,356 in columns 3 to 6 after inclusion of demographic controls. In Panel B, N=246,753 respondents (in 712 CZs) in columns 1 and 2, and N=232,666 in columns 3 to 6 after inclusion of demographic controls. Respondents are asked about their perception of the household's income over the last year (HH income decreased a lot / somewhat / same / increased somewhat / a lot). The outcome variable takes the value of 100 if a respondent reports that household income increased somewhat or a lot, and 0 otherwise. The mean (standard deviation) for the outcome is 33.62 (47.24) in Panel A and 11.35 (31.49) in Panel B. Tariff and subsidy variables are measured in October 2018 and October 2019 for the corresponding survey years, and at their end-of-sample value (December 2019 for tariff and February 2020 for subsidy variables) for later years. Demographic controls include a quadratic in age, gender, race (non-Hispanic white vs all others) and education (at least some college education vs high school or less). Regressions are weighted by $\text{pop}_{2010,\text{CZ}} \times \left(\frac{\text{CCESweight}_i}{\sum_{i=1}^{\text{CZ}} \text{CCESweight}_i}\right)$, and standard errors are clustered by state.

A2 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 A7 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.[50] 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. 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 during the trade war. 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

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

Table A7: 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.

agriculture and mining. As with the CZ-level models, some specifications further control for a linear time trend in the observed change of the outcome variable in the year preceding the start of the trade war.

Table A8 presents results. Panel A indicates that exposure to import tariffs reduced import penetration for the protected U.S. industries, while exposure to retaliatory tariffs reduced the exports of U.S. industries. To assess the economic magnitude of the import tariff effect, we multiply the point estimate in the second column of Table A8 with the average industry-level tariff exposure in December 2019 from Table A7, and scale this product by the average level of industry-level import penetration prior to the trade war in January 2018. This calculation yields a meaningful -4.4% decrease in industry-level import penetration due to US import tariffs. An analogous exercise based on column 4 of Table A8 implies a -2.9% decrease in the export-to-shipment ratio due to retaliatory tariffs.

The subsequent panels B to E of Table A8 additively decompose the effects of tariffs on trade by groups of U.S. trade partners. These results provide some evidence for trade diversion. While industries protected by import tariffs imported less from China and from the other trade war coun-

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

	imports			exports		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Monthly Trade Total						
import tariff exposure	-1.543	-1.631	-2.051	-0.457	-0.336	-0.424
	(0.405)	(0.417)	(0.548)	(0.238)	(0.214)	(0.266)
retaliatory tariff exposure	-1.237	-1.111	-1.319	-1.998	-2.166	-2.362
	(0.756)	(0.766)	(0.913)	(0.668)	(0.720)	(0.820)
Panel B: Monthly Trade with China						
import tariff exposure	-2.099	-2.084	-2.127	0.178	0.124	0.088
	(0.611)	(0.624)	(0.648)	(0.074)	(0.070)	(0.069)
retaliatory tariff exposure	-0.219	-0.234	-0.273	-0.913	-0.828	-0.806
	(0.440)	(0.465)	(0.499)	(0.168)	(0.171)	(0.199)
Panel C: Monthly Trade with other Trade War Countries						
import tariff exposure	-0.220	-0.248	-0.311	-0.437	-0.348	-0.387
	(0.187)	(0.209)	(0.244)	(0.156)	(0.145)	(0.173)
retaliatory tariff exposure	-0.481	-0.440	-0.407	-0.634	-0.758	-0.843
	(0.315)	(0.310)	(0.360)	(0.472)	(0.495)	(0.580)
Panel D: Monthly Trade with other low-wage Asia						
import tariff exposure	0.400	0.381	0.354	-0.085	-0.078	-0.071
	(0.135)	(0.138)	(0.151)	(0.062)	(0.060)	(0.050)
retaliatory tariff exposure	-0.087	-0.065	-0.083	-0.006	-0.017	-0.036
	(0.157)	(0.154)	(0.179)	(0.056)	(0.059)	(0.059)
Panel E: Monthly Trade with Rest of the World						
import tariff exposure	0.375	0.320	0.204	-0.113	-0.034	-0.051
	(0.225)	(0.227)	(0.212)	(0.120)	(0.110)	(0.135)
retaliatory tariff exposure	-0.450	-0.372	-0.424	-0.445	-0.563	-0.644
	(0.249)	(0.258)	(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 26.91 (26.43) for Panel A), 6.87 (12.02) for Panel B), 12.31 (13.21) for Panel C), 1.86 (5.13) for Panel D) and 5.87 (8.31) for Panel E). The 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 19.01 (19.99) for Panel A), 1.31 (2.51) for Panel B), 11.44 (11.84) for Panel C), 0.65 (2.06) for Panel D) and 5.61 (7.89) for Panel E). 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.

tries (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.

We next study the impact of tariff exposure on employment in national tradable industries. Acemoglu et al. (2016) argue that CZ- and industry-level analyses are complements to each other which will capture slightly different employment effects of industry-level shocks. Effects at the CZ level combine the effect of tariffs on employment in directly exposed industries with local spillovers that can operate through local supply chain linkages, local consumption effects, and local labor reallocation effects. Such local spillovers are apparent in the results of Table 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.[51] To investigate both the direct effects of tariffs on industry employment and the indirect effects via national supply chain linkages between supplier and customer industries, we fit the regression

$$\begin{aligned}\ln E_{it} - \ln E_{iJan2018} = {} & \beta_1 IMP_{it} + \beta_2 RET_{it} + \mathbf{\hat{x}_{it}^{IO\prime}} \boldsymbol{\beta_3} \\ & + \gamma_t + \lambda_{s(i),t} + \phi(\bar{\Delta} \ln E_{i,2017} \times t) + \chi_t(\Delta \ln E_{i,Mar20-Apr20}) + \varepsilon_{rt} + \varepsilon_{it},\end{aligned} \tag{A4}$$

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

Column 1 of Table A9 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 over either the 2016-2019 period in panel A or the 2016-2023 period in panel B. 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.

[51] 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.

Moreover, and somewhat puzzlingly, we estimate that tariff protections applied to an industry's suppliers predict an increase in that industry's employment.[52]

Cognizant of the concern that inference on high-frequency monthly data may be particularly vulnerable to confounding trends, the next three columns of Table A9 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, with most regression specifications indicating insignificant negative effects. These results are consistent with the CZ-level evidence in Table 3, which indicates no employment gains in manufacturing over either time period considered. The employment effect of industries' indirect tariff exposure via suppliers and customers are mostly measured with little precision. These effects are also highly sensitive to the pre-trend control, and several one switch signs when moving from the column 3 estimates to the column 6 estimates.

To further explore the impact of pre-trends, the models in columns 7 to 9 fix the coefficient of the 2017 pre-trend variable to one. This setup is conceptually equivalent to a specification used in Flaaen and Pierce (2021), which defines the outcome of industry-level employment growth in deviations from pre-period growth. The results in column 9 indicate a small and insignificant positive effect of direct exposure to import tariffs and an insignificant negative effect of direct exposure to retaliatory tariffs over both the time period through 2019 (in panel A) and the period through 2023 (in panel B). All estimates for indirect exposures to tariffs are statistically insignificant, with the exception of a negative effect of supplier import exposure in panel A. While this specification thus points to a more prominent role of supplier import exposure, it is very restrictive as it assumes that absent tariff shocks, each industry would have continued to grow according to its 2017 growth trend. The more flexible regression specifications in column 4 to 6 however show that while past industry employment growth is predictive for subsequent growth, estimated coefficient values are considerably below unity, which indicates mean reversion: if an industry grew more than another industry in the recent past, it will subsequently continue to grow faster but between-industry gaps in growth rates diminish. Therefore, we prefer the flexible model in columns 4 to 6 over the restrictive specification in column 7 to 9.[53]

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

[53] In unreported results, we explored a version of the equation A4 model that uses as outcome variable the log change in industry employment relative to January 2016, and controls for the average monthly log change in industry employment during the year 2015. Applied to outcome data from the years 2014 to 2017, in which exposure to the trade war tariffs is zero, this regression yields a coefficient of 0.55 on the pre-trend variable, which is very close to the value in column 4 of panel A in Table A9 and suggests the presence of mean reversion in industry-level employment growth also outside the trade war period.

Table A9: : Impact of Tariff Exposure on Tradable Industry Employment

	no pretrend control			control for 2017 pretrend			outcome as dev. from trend		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: years 2016-2019									
import tariff exposure	-0.431	-0.678	-0.747	-0.057	-0.280	-0.303	0.261	0.064	0.079
	(0.352)	(0.358)	(0.340)	(0.293)	(0.296)	(0.296)	(0.318)	(0.303)	(0.316)
retaliatory tariff exposure	-0.923	-0.566	-0.579	-1.173	-0.851	-0.844	-1.387	-1.098	-1.071
	(0.403)	(0.398)	(0.394)	(0.353)	(0.310)	(0.308)	(0.418)	(0.343)	(0.358)
supplier import exposure			1.817			-0.071			-1.693
			(0.727)			(0.515)			(0.724)
customer import exposure			-0.472			-0.102			0.215
			(0.947)			(0.766)			(0.840)
supplier retaliatory exposure			-0.159			-0.443			-0.687
			(0.773)			(0.654)			(0.726)
customer retaliatory exposure			1.039			0.112			-0.684
			(1.022)			(0.993)			(1.243)
t * (monthly Δ ln(emp) in 2017)				0.540	0.537	0.538	1	1	1
				(0.052)	(0.045)	(0.047)	–	–	–
year-month FE	✓	(✓)	(✓)	✓	(✓)	(✓)	✓	(✓)	(✓)
sector*year-month FE		✓	✓		✓	✓		✓	✓
Panel B: years 2016-2023									
import tariff exposure	-0.284	-0.550	-0.619	-0.089	-0.345	-0.394	0.412	0.192	0.196
	(0.321)	(0.317)	(0.292)	(0.282)	(0.279)	(0.269)	(0.294)	(0.280)	(0.299)
retaliatory tariff exposure	-1.052	-0.666	-0.622	-1.181	-0.813	-0.759	-1.515	-1.197	-1.116
	(0.379)	(0.368)	(0.375)	(0.339)	(0.314)	(0.319)	(0.413)	(0.347)	(0.362)
supplier import exposure			2.818			1.763			-0.996
			(0.652)			(0.556)			(0.768)
customer import exposure			-0.988			-0.762			-0.170
			(0.882)			(0.770)			(0.828)
supplier retaliatory exposure			-0.436			-0.562			-0.892
			(0.761)			(0.681)			(0.741)
customer retaliatory exposure			0.679			0.231			-0.940
			(1.030)			(0.959)			(1.276)
t * (monthly Δ ln(emp) in 2017)				0.279	0.277	0.276	1	1	1
				(0.055)	(0.050)	(0.050)	–	–	–
year-month FE	✓	(✓)	(✓)	✓	(✓)	(✓)	✓	(✓)	(✓)
sector*year-month FE		✓	✓		✓	✓		✓	✓
Δ ln(emp) Mar-Apr 2020*year-month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: N=17,904 (373 tradable industries x 48 months: Jan 2016 – Dec 2019) in Panel A, and N=35,808 (373 tradable industries x 96 months: Jan 2016 - Dec 2023) in Panel B. The dependent variable for all regression model 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 1,110.8 (116.9) log points in 2018m1. Regressions in columns 4 to 6 control for the monthly change in log employment from 2017m1 to 2018m1, interacted with a linear time trend (the count of months since 2018m1); regressions in columns 7 to 9 constrain the coefficient associated with the pre-trend variable to be 1, which is equivalent to a specification that does not include a pre-trend control but defines the outcome variable as deviation from the pre-trend. The regressions in columns 2 to 3, 5 to 6, and 8 to 9 interact time fixed effects with indicators for three economic sectors (agriculture and mining, manufacturing, non-goods sector). All regressions in Panel B control for the decline in industry employment Regressions are weighted by industry employment in 2012, and standard errors are clustered by industry.

A3 Online 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 A10 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 A10: Data Sources for Regional US Industry Employment: QCEW vs. CBP

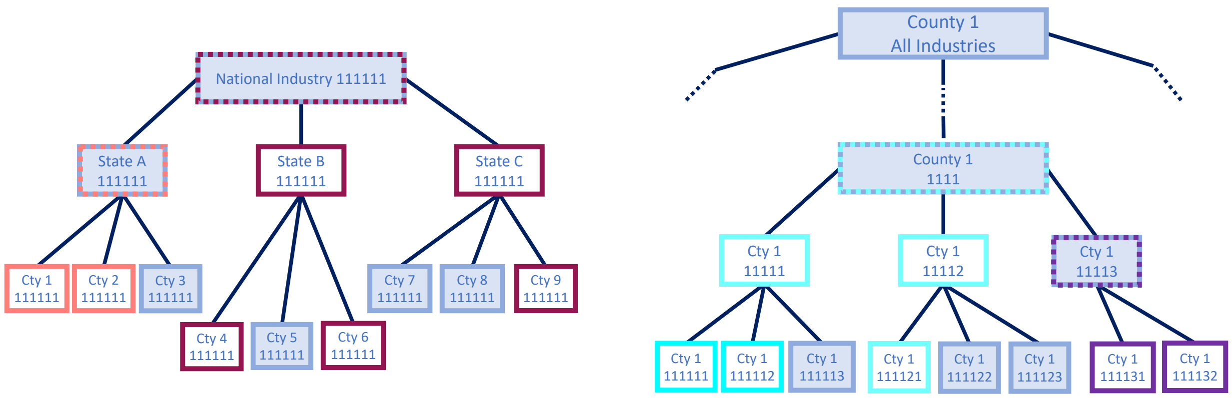
	Quarterly Census of Emp. and Wages (QCEW)	**County Business Patterns (CBP)**
	Frequency and Coverage of Employment Data	
Frequency	monthly (published quarterly)	annual
Sectors Covered	private non-farm private farm public sector	private non-farm
Total US Employment (2010)	126'228'228	111'970'095
	Disclosed 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 A3: 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 × 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 × 6-digit industry (as is the case for CBP data, see Table A10), we impute employment for the remaining county-industry cells.[54]

Key to our imputation algorithm is the observation that whenever employment counts are suppressed at the level of a detailed county × 6-digit industry cell, then employment will still be known at a higher level of geographic aggregation, and at a higher level of industry aggregation.[55] For instance, we may know the employment for a 6-digit industry at the state rather than county level, and we may know the employment in a county at the level of a 4-digit industry rather than 6-digit industry. Indeed, more than 99% of total private sector employment is disclosed at least at the level of state × 4-digit industry cell (Table A10). Our fixed-point algorithm distributes the known employment counts from more aggregate geography and industry levels to detailed county × 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

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

[55] 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).[56] Each suppressed cell is thus part of two clusters of suppressed cells: One that combines non-disclosed cells within an industry to the next higher geographic level (industry dimension); and one that combines cells within a geographic area to the next higher industry level (geographic dimension). Figure A3 provides a graphical illustration. The suppressed cell *county 1 - industry 111111* belongs to the cluster of cells that shares *state A - industry 111111* as the next higher disclosed cell in the industry dimension and the cell cluster that shares *county 1 - sector 1111* as the next higher disclosed cell in the geographic dimension.

2. **Calculation of distributable suppressed employment.** For each cell cluster, the sum of distributable suppressed industry (geographic) employment equals the total employment of the cluster minus the employment in disclosed cells that are part of the cluster. Using again the example in the left panel of Figure A3, one subtracts the disclosed employment for *county 3 - industry 111111* from the disclosed employment of *state A - industry 111111* to obtain the employment that has to be distributed from *state A - industry 111111* to *county 1 - industry 111111* and *county 2 - industry 111111*. At the same time, the right panel of Figure A3 shows that the employment of *county 1 - industry 1111* minus the employment of *county 1 - industry 111113*, *county 1 - industry 111122*, *county 1 - industry 111123* and *county 1 - industry 11113* yields an employment total that has to be distributed between cells *county 1 - industry 111111*, *county 1 - industry 111112* and *county 1 - industry 111121*.

3. **Preliminary Initialization.** We initialize the fixed-point algorithm by apportioning distributable industry employment to suppressed cells in proportion to each cell's fraction of the total establishment count in the cell cluster. This initialization is based on the assumption that establishments of the same detailed industry and aggregate geographic unit have the same average employment size per establishment across the different counties of the cluster. The resulting initial imputed employment counts for detailed county × 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

[56] 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

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

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

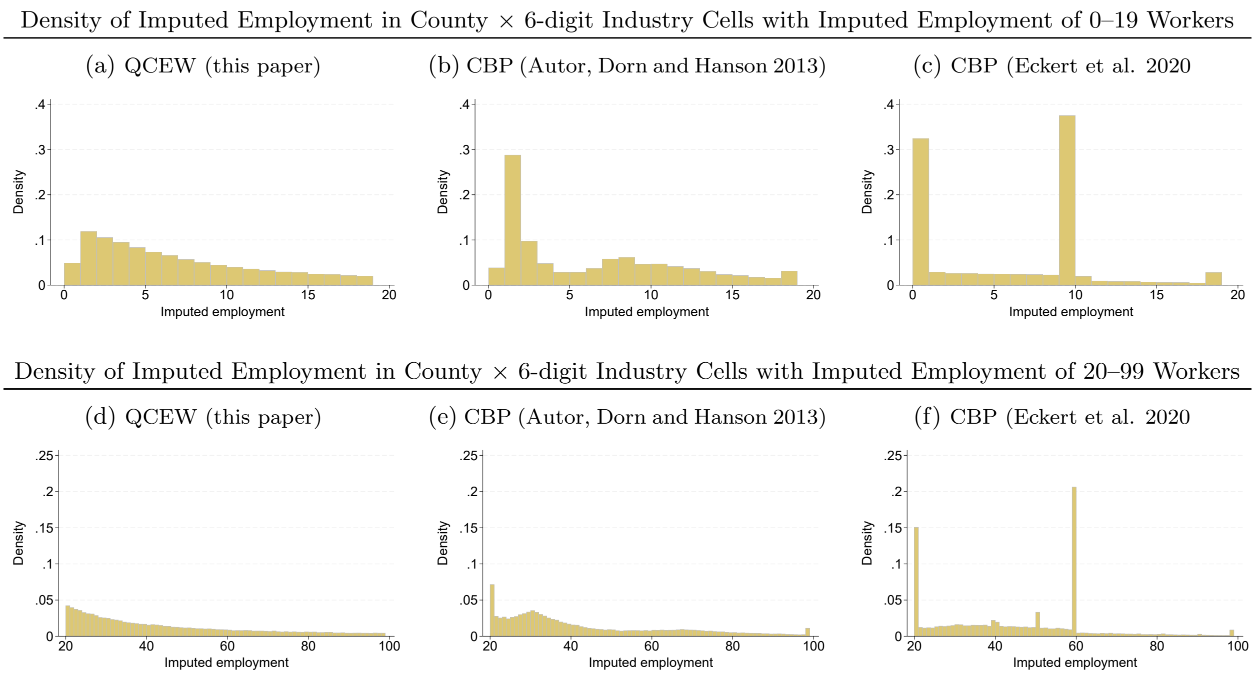
This initialization allocates to cell j a greater fraction of the distributable employment of cluster J not only when j accounts for a large fraction of all establishments in the cluster, but also when the initial iteration of the algorithm found that establishments of cell j had an unusually large employment size in the prior and subsequent month.[57]

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.

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

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



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

Second, we investigate in Table A11 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 A11 indicates the total deviations between sums of cell employment and disclosed county employment across all counties, expressed as a fraction of total national employment. The first two rows of the table similarly investigate whether cell employment correctly adds to national employment or state-level employment. In all cases, we are able to ascertain that the county × 6-digit industry employment counts in our final QCEW data sum exactly to known county, state or national employment. CBP data imputed by Autor et al. (2013) also displays very high consistency with known employment counts at aggregate geographic 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 A11 indicate that county × 6-digit industry employment in the QCEW also perfectly sums to known aggregate industry employment, while the CBP imputations by Autor et al. (2013) and Eckert et al. (2020) display 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 A11 indicates that imputed employment counts for county × 6-digit industry cells in the CBP are consistent with the cell-specific employment ranges. However, about 5% of the imputed values in Autor et al. (2013) and more than half of the imputed values in Eckert et al. (2020) are inconsistent with the employment ranges implied by the establishment-by-size counts. We conclude that our new county × 6-digit industry employment imputation in the QCEW both generates a more plausible employment distribution across cells and displays a greater consistency with disclosed data than extant imputations of CBP data. A key advantage of the QCEW is that its disclosed employment values are exact counts, whereas the CBP discloses employment with noise infusion (Table A10). 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 A11: 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.